Hybrid Gains and Tasty Grains: The Role of Agronomic and Consumption Traits in Technology Adoption

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

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Abstract

The dual role of farm households as both producers and consumers implies that adoption decisions for improved crop technologies depend not only on agronomic performance attributes such as yield or drought tolerance, but also on consumption attributes such as taste and ease of cooking. We examine how emphasizing different traits of an improved maize variety affects adoption behavior among smallholder farmers. In a randomized controlled trial in eastern Uganda, we implement two interventions: one highlighting production attributes through seed trial packs, and another emphasizing consumption attributes through cooking demonstrations and tasting sessions. Both interventions significantly improve farmers' knowledge and perceptions of the promoted variety and increase its subsequent use. However, closer examination reveals that the adoption response to seed trial packs is driven largely by the recycling of grain harvested from the trial as planting material. When adoption is defined using agronomic criteria that exclude recycled hybrid seed due to associated productivity losses, the seed trial pack intervention reduces the use of improved crop technologies, consistent with a crowding-out effect. We interpret these patterns as rational responses to informational, economic, and structural constraints faced by farmers. Overall, the results indicate that while both trial packs and cooking demonstrations facilitate learning about new technologies, trial packs may distort adoption incentives for non-reproducible

[*]Innovation Policy and Scaling Unit, International Food Policy Research Institute, Leuven, Belgium

[†]One Acre Fund, Jijna, Uganda

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

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

technologies such as hybrid seed by crowding out purchases of certified seed through recycling.

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JEL: Q16, O33, Q12, D83, C93

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 improved crop technologies, have been key to achieving higher yields and enhancing resilience to environmental challenges, such as drought, and emerging biotic stresses, such as pests and diseases (Evenson and Gollin, 2003). Traditionally, crop improvement efforts in breeding programs which serve smallholder farmers have focused on improving production-related agronomic 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 consumption-related traits, such as taste, color, texture, and ease of cooking (Timu et al., 2014; Thiele et al., 2021; Otieno et al., 2011). Integrating improvements in such traits alongside agronomic traits 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 trial 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 primary aim 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 seek to raise farmers' awareness of the consumption-related attributes of improved varieties 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 or variety that

[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).

was used during the demonstration (Reicks, Kocher, and Reeder, 2018).[2]

This paper examines how highlighting agronomic and consumption-related attributes of improved maize varieties affects their adoption by smallholder farmers, 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. The first intervention targets production traits by providing farmers with 1 kg trial packs of seed of an improved variety.[3] A second intervention 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 the same improved variety that was promoted in the seed trial pack intervention. Additionally, farmers in this treatment arm are provided with maize flour samples derived from the grain grown using the promoted improved variety to further explore consumption traits at home and potentially convince other household members of consumption related benefits.

The field experiment was conducted in four districts in eastern Uganda among a representative sample of about 1,500 farmers. The area, commonly known as the Busoga Kingdom, is predominantly populated by smallholder farmers who cultivate maize for both home consumption and sale. The study ran over two consecutive agricultural seasons in 2023, with endline data collected in early 2024 after the harvest of the second season. The variety we promoted in this study is a hybrid marketed as *Bazooka* (UH 5354), which is widely available from agro-dealers but has yet to see broad adoption among farmers. *Bazooka*'s primary notable production trait is its yield potential, which is over double that of popular open-pollinated varieties (OPVs) such as Longe 5 (Semalulu et al., 2022). However, it also offers important consumption attributes: *Bazooka* combines high starch content with mild sweetness and produces a very white porridge.

We find that farmers who received a trial pack are substantially more likely to plant *Bazooka* in the subsequent season. This increase, however, is driven almost entirely by the reuse of grain harvested from the trial pack rather than by purchases of fresh *Bazooka* seed. When we restrict attention to the use of fresh *Bazooka* seed, adoption rates are statistically indistinguishable between treatment and control farmers. Moreover, when we apply a broader but agronomically valid definition of seed of improved varieties that includes fresh hybrids and fresh or appropriately recycled OPVs (but exclude recycled hybrid grain), we find that the trial pack intervention actually *reduces* adoption of improved

[2]While seed trial packs may also enable farmers to discover consumption traits after they harvest the grain obtained from the seed trial pack, such learning is often not the primary focus of trial packs and may occur only if farmers taste the harvest from the trial 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 fresh maize hybrids and open-pollinated varieties (OPVs) that are either newly purchased or have been appropriately recycled (up to four seasons). This excludes farmer-saved seed or seed from informal 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 (Pixley and Banziger, 2004; Roos, 1980).

cropt technologies. This crowding-out effect appears to arise because treated farmers, having access to *Bazooka* grain at no monetary cost, are less likely to use fresh hybrid or use fresh or appropriately recycled OPV seed in the following season.

For the cooking demonstration and tasting intervention, we find suggestive evidence consistent with an increase in adoption of fresh *Bazooka* seed.[4] We find no evidence that either intervention influenced how the harvested maize in the next season was used—whether for own consumption, market sales, or saved to be used in the next season.

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 others are more sensitive to consumption traits, and the trial pack facilitated coordination—though the estimates are fairly noisy. 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, portion size, appearance, and ease of cooking. Furthermore, we find that farmers who received the seed trial 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, although the evidence is less convincing. Interestingly, the seed trial pack also positively affects how farmers think about consumption traits of improved varieties, suggesting farmers also consume at least part of the grain obtained from the trial pack and pay close attention to consumption attributes.

A key finding from our study is that the majority of farmers who received the seed trial pack continued using the distributed hybrid maize variety by recycling the harvest from the trial pack rather than purchasing fresh seed. This behavior does not seem to be merely the result of a lack of knowledge about the drawbacks of recycling hybrid seeds; rather, it may represent a rational response to the conditions farmers face: Even when farmers are aware of potential yield penalties associated with reusing hybrid seed, they may still find recycling preferable in light of high cost of seed, liquidity constraints, market risk, and distrust in agro-dealer networks. These findings suggest that exposure alone is not enough to ensure continued use of fresh improved seed, and that addressing the informational, economic, and structural barriers that shape farmers' decision environments is essential for sustained adoption.

Our work contributes to several strands of the literature on agricultural technology adoption. First, we add to the literature examining whether short-term input subsidies—whether provided in cash or in-kind—can stimulate longer-

[4]We deem this evidence as suggestive because, while false discovery rate–adjusted q-values point to significant effects, our prespecified analysis, which aggregates outcomes into indices across related measures, does not show significant effects.

term uptake of improved technologies (Macours, Mallia, and Rudder, 2025; Balew, Bulte, and Kassie, 2025; Gignoux et al., 2023; Fishman et al., 2022; Carter, Laajaj, and Yang, 2021). Specifically, despite the prevalent use of temporary subsidies as a policy instrument to spur agricultural technology adoption (Christinck, Diarra, and Horneber, 2014; Dorward and Kydd, 2005; Phiri et al., 2000), 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.[5] 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 trial packs to demonstration plots run by 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 trial pack provision. 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. Ragasa, Oyinbo, and Ma (2025) present results from Nigeria showing that seed trial packs substantially increase adoption of improved maize and cowpea OPVs.

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 their 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 try 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

[5] 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. Other studies similarly look at the effect of trial pack provision on other outcomes, such as willingness-to-pay for seeds (Morgan, Mason, and Maredia, 2020) or beliefs about the yields of improved varieties (Tjernstrom and Gars, 2019) .

Mozambique and Uganda.

The remainder of the paper is organized as follows. Section 2 outlines the research methodology, including the experimental design and empirical strategy. Section 3 describes the study context and data sources. Section 4 presents the main findings, focusing on the primary outcome of improved seed variety use and production outcomes as well as secondary outcomes related to use of harvest output, intra-household decision-making, food security, and overall well-being. Section 5 investigates potential mechanisms underlying the observed effects. Section 6 discusses possible explanations for some of the less intuitive findings from Section 4. Section 7 concludes.

2 Methods

2.1 Experimental Design

We use a cluster randomized control trial (RCT) to evaluate the effectiveness of two interventions, using a 2x2 factorial design, with treatments assigned at the village level to mitigate potential concerns about spillover effects. The first factor in the factorial design is the seed trial pack treatment: sampled farmers in the treatment villages received a complimentary sample of a hybrid maize variety (*Bazooka*), whereas those in the control villages did not. The second factor involved a cooking demonstration and tasting session; in treatment villages, sampled farmers were invited to a session where they observed the preparation of maize flour made from the *Bazooka* hybrid variety, participated in a tasting session, and received a free sample of *Bazooka*-derived maize flour to try at home. No such activities were conducted in control villages.[6]

2.2 Treatments

The first intervention involves providing a seed trial pack to the household member responsible for most maize cultivation decisions, who in the study context is typically the male co-head (Van Campenhout, Lecoutere, and Spielman, 2023). This trial 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.[7] 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).[8] 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

[6]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.

[7]At baseline, only about 8 percent of sampled farmers reported growing *Bazooka*—See Table 1 in Section 3.3 below.

[8]The primary production trait of *Bazooka* is its high yield, underscored by the package slogan, "yield explosion."

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, organized once in each village assigned to the cooking demonstration treatment. Within these villages, all sampled households were invited to attend a facilitated meeting at a central location (half of whom were cross-randomized to receive a seed trial pack under the factorial design). As for the cooking demonstration and tasting intervention, invitations were addressed to the household member responsible for maize production decisions. Invitees were explicitly encouraged to attend together with another household member involved in food preparation, in order to ensure that both production and consumption perspectives were represented during the session.

During the event, a facilitator started by asking the group to mention the most commonly grown maize varieties by farmers in their village. These varieties are grouped into "improved varieties" and "local varieties" on a flip chart.[9] 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 staple 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 corresponded 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 again, voting on which of the two samples is superior for each attribute by a show of hands.

The rating results were then 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. As the results in Section 5.2.1 will show, improved seed varieties are generally perceived favorably with respect to consump-

[9] 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.

tion traits, yet these positive perceptions are far from universal, leaving scope for further improvement. At the end of the session, participating households received a 2 kg take-home sample of maize flour produced from the *Bazooka* variety, sufficient for preparing at least one family meal, to facilitate post-session experimentation at home.

In both treatments, facilitators followed a standardized script and were instructed to avoid promotional language, focusing instead on eliciting participants' own assessments and facilitating discussion rather than advocating for a specific variety. The objective of both interventions was to study learning about improved crop technologies in general, rather than to promote adoption of a particular seed. Accordingly, discussions in both treatments were kept deliberately broad, emphasizing generic attributes of improved maize varieties, such as yield potential and consumption traits, rather than the merits of *Bazooka* per se.

At the same time, the underlying technology used to illustrate these attributes was held constant across interventions. In the seed trial pack treatment, the distributed seed was clearly labeled as *Bazooka*, while in the cooking demonstration, the maize flour used for preparation was explicitly identified as being derived from *Bazooka* seed. By maintaining transparency about the source technology while keeping the framing comparable across treatments, we aimed to ensure that differences in outcomes across interventions cannot be attributed to differences in the underlying technology itself, but rather to the mode of learning emphasized by each intervention.

The two interventions deliberately differ in how exposure within the household was structured. The seed trial pack intervention targets the household member responsible for maize production decisions, while the cooking demonstration and tasting intervention was designed to involve both production- and consumption-relevant decision-makers by encouraging joint attendance. This design aligns each intervention with the margin it is intended to affect: production-oriented learning through agronomic experimentation on the one hand, and consumption-oriented learning through sensory experience on the other. By structuring exposure in this way, the design leverages existing household decision-making roles rather than confounding treatment effects with arbitrary differences in which household members were exposed.

2.3 Estimation and Inference

We estimate intent-to-treat (ITT) effects using linear regression models that compare mean outcomes across treatment and control groups. Given that randomization was conducted at the village level, we will estimate ITTs using 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 equals one if village j was randomly assigned to the seed trial pack intervention (and zero otherwise), and T_j^D is an indicator variable that equals 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 allow for an interaction effect between the two interventions, and we control for baseline outcomes $\left(Y_{ij}^{B}\right)$ to improve precision. We 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 main specification is the fully interacted model 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 (Colen et al., 2024). For conciseness, we include results from these pooled specifications, which are qualitatively similar to the results from the fully interacted model, in Online Appendix A1 rather than the main text.

While our main outcome of interest is whether individuals adopt improved varieties the subsequent season, we also look at additional outcomes related to productivity, use of harvest output, intrahousehold decision-making, and household welfare. 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 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

The experiment spans two consecutive agricultural seasons in order to accommodate the inherently different durations of the two interventions while measuring outcomes at a common point in time. For the seed trial pack treatment, the outcome of interest is not (the first stage result) whether farmers plant the trial pack itself, but whether the experimentation enabled by planting the trial pack in one season leads to continued use of the variety in the subsequent season. This necessarily requires observing outcomes after an additional planting cycle.

In contrast, the cooking demonstration and tasting session is a one-time informational intervention that does not require a full growing season to imple-

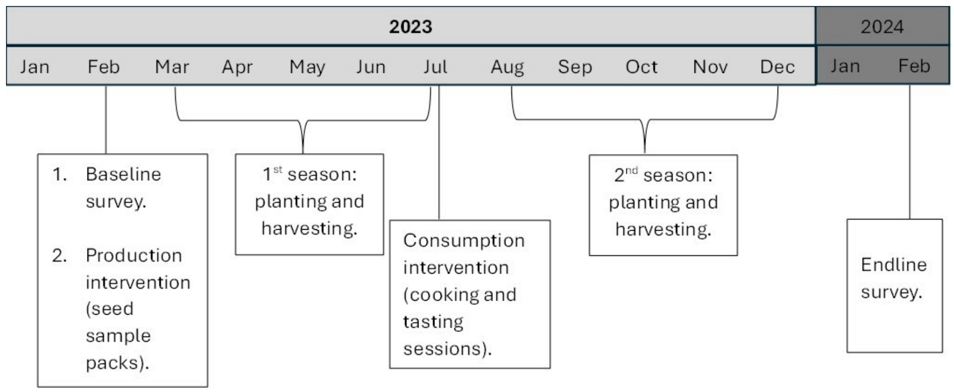

Figure 1: Timeline

ment. To ensure comparability, this intervention was therefore administered at the beginning of the second experimental season, such that outcomes for both interventions are measured after the same agricultural season.

The study area experiences two maize growing seasons per calendar year. The first season, locally known as *Entoigo*, runs from March/April to June/July, while the second season, *Nsambya*, extends from August/September to November/December. *Bazooka* can be cultivated in either season. As illustrated in Figure 1, seed trial packs were distributed alongside baseline data collection prior to the start of *Entoigo* 2023, allowing farmers to plant the trial pack during that season. Cooking demonstrations and tasting sessions were conducted after the completion of *Entoigo* and before the onset of *Nsambya*. Endline data were collected in February 2024, following the conclusion of *Nsambya*.

Importantly, while the two interventions differ in their duration of exposure—season-long agronomic experimentation versus a short informational session—all outcomes are evaluated at the same point in the agricultural cycle, ensuring that estimated treatment effects are not driven by differences in outcome measurement timing.

3 Study Sample and Data

3.1 Sample Size Calculation

Sample size was determined through a series of power simulations detailed in the pre-registered pre-analysis plan (Colen et al., 2024). 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 the prompted seed 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 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 simulations resulted in a sample design of 148 villages, with 10 households sampled per village, assigned evenly across the four cells of the 2×2 factorial design. Specifically, 37 villages were assigned to each of the following groups: pure control, seed trial pack only, cooking demonstration only, and both interventions. This implies that, in total, 74 villages received the seed trial pack (those assigned to the seed-only and both-treatment groups), and 74 villages were exposed to the cooking demonstration and tasting sessions (those assigned to the demo-only and both-treatment groups). The resulting sample therefore comprises 1,480 households across 148 villages. 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.

3.2 Sampling Strategy

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,* despite a well-established network of agro-input dealers.

To obtain a representative random sample of farmers in the Busoga Kingdom, villages were selected with probability proportional to the number of households within each village, and then randomly assigned to one of the four study arms (control, trial pack only, consumption demonstration only, both trial pack and consumption demonstration). Within each sampled village, 10 households were randomly chosen to participate in the study from the list of farm households maintained by the village council.

3.3 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 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.

In terms of variables that will be used to assess impact at endline, we first inquired whether farmers had used *Bazooka*, the hybrid seed variety that is also utilized in our experiment. At baseline, only about 8 percent of farmers in the control group reported having used *Bazooka* seed on at least one plot in the previous season (*Nsambya* of 2022). We also asked a less specific question where we asked if "quality maize seed, such as an OPV or hybrid seed" was used on any of their plots during the previous season. Approximately 42 percent of control group households answered affirmatively to this question at baseline.

We also asked where the seed used on a randomly selected plot was obtained from.[10] 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 reports baseline mean differences between the trial pack group and the control group obtained from the fully interacted specification in Equation 1 (column 2, β_S), between the cooking demo group and the control group (column 3, β_D), and the interaction term capturing the additional effect of receiving both treatments beyond the sum of individual effects (column 4, β_I). Overall, the results suggest good baseline balance, as none of the treatment–control differences are statistically significant.

3.4 Attrition

Attrition was minimal: during endline, we successfully located all but four of the 1,560 households interviewed at baseline. Moreover, only 18 of the remaining 1,556 households 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, or 98.6% of the baseline sample. Attrition was balanced across treatment arms.

4 Results

We now turn to the main results, with subsections for first stage results, adoption of improved crop technologies, use of harvest, women's empowerment, and

[10]During both baseline and endline, detailed plot-level questions (such as seed source, price, etc.) were collected for a single, randomly selected plot per household to limit respondent burden. If trial pack seed is systematically planted on particular plots (for example, smaller plots), random plot selection leads to misclassification of plot-level treatment status for some treated households. This attenuates estimated effects toward zero relative to the effect on the plot that actually received the trial pack. We therefore interpret plot-level results as conservative lower bounds on effects at the plot that actually receives the trial seed. However, because both treatment assignment and plot selection are randomized, the estimate remains unbiased for the intent-to-treat effect on a randomly selected plot.

Table 1: Baseline balance

	control mean	ITT trial pack	ITT cooking demo	ITT 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	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 the promoted seed (*Bazooka*) on any plot in last season (1=yes)	0.08	0.04	0.03	-0.07^{+}
	(0.27)	(0.03)	(0.03)	(0.04)
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)
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 d 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 intent-to-treat effect of the seed trial packs; column (3) reports the estimate of the intent-to-treat 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).

Table 2: First Stage Results

	Crtl	Seed trial pack
At endline, household recalls being given seed trial pack (%)	8.6	98
Household received seed trial pack (%)	–	100
Trial pack planted in *Entoigo* 2023 (%)	–	99
Median area planted with trial pack (acres)	–	0.25
	Crtl	Cooking demo
At endline, household recalls being invited for cooking demo (%)	4	92
Household attended cooking demo and tasting session (%)	–	89
Both male and female co-heads attended (%)	–	48
Used maize flour (1=yes) (%)	–	99

welfare and food security.

4.1 Compliance and Treatment Recall

Self-reported recall of the treatments was high for both interventions (Table 2). During the endline survey, we asked all farmers whether they recalled receiving the interventions, regardless of assignment. This provides useful information on failure to reach intended beneficiaries and on spillovers or exposure among non-targeted households. For the trial pack treatment, 98 percent of farmers in the treatment group indicated that they received a seed trial 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 invitation.

Administrative records from the seed trial pack distribution, which happened concurrently with baseline data collection, show that no households that were assigned to this treatment refused the seed. Consistent with this, nearly all households who recalled receiving the trial pack reported planting it during the first experimental season (*Entoigo* 2023). Among households that planted the trial pack, the median area planted was 0.25 acres, approximately twice the nominal one-eighth acre coverage implied by the size of the trial pack.

Households that were assigned to the cooking demonstration and tasting sessions received an invitation; attendance was voluntary. Administrative attendance records indicate that just under 90 percent of households assigned to the cooking demonstration intervention also attended. Among attendees, approximately half participated jointly as male and female co-heads. Endline survey responses further indicate that nearly all households who reported attending the demonstration also reported using the maize flour provided at the end of the session to prepare a meal at home, rather than selling or otherwise disposing of it. Together, these figures indicate high intervention fidelity and substantial treatment uptake, supporting an intent-to-treat interpretation of the estimated effects.

4.2 Adoption

Adoption of the promoted seed in the season following the intervention is the primary outcome of interest in this study. Accordingly, Table 3 examines whether farmers used the promoted variety at all, while Table 4 reports more disaggregated outcomes on seed use and management practices measured on a single randomly selected maize plot within each household. As described in Section 2.3, related outcomes are aggregated into summary indices following Anderson (2008).

The structure of both tables mirrors that of Table 1. Column (1) reports control group means at endline, while columns (2) to (4) present intent-to-treat estimates from the fully interacted specification in Equation 1, corresponding to the trial pack intervention, the cooking demonstration intervention, and their interaction, respectively.

Turning to the summary adoption index in Table 3, we find a positive and statistically significant intent-to-treat effect of the trial pack intervention. In contrast, the tasting session intervention has no detectable effect on the index. We also find no evidence of an interaction effect between the two treatments, suggesting that their combined provision does not generate additional adoption beyond the effect of the trial pack alone.

We next consider the individual outcome components underlying the adoption index. We first look at whether households planted the promoted variety, *Bazooka*, on at least one maize plot. In the control group, approximately 10 percent of farmers report having grown *Bazooka* in the season that followed the season where they could experiment with the seed from the trial pack. Receipt of a trial pack increases this likelihood by 67 percentage points, corresponding to an adoption rate of roughly four out of five farmers in the treatment group. The point estimate for the cooking demonstration and tasting session is also positive (p=0.051), but it is not statistically significant after adjusting for multiple hypothesis testing using false discovery rate corrections (q=0.110). We likewise find no evidence of interaction effects between the two interventions.

Moving on to measures of intensity of adoption of the promoted variety, we find that the trial pack intervention increases not only the likelihood of planting *Bazooka* but also the scale of its use. Households receiving a trial pack plant *Bazooka* on more plots, allocate a larger share of maize plots to the variety, and devote more land area to its cultivation, both in absolute terms and as a share of total maize area. All of these effects are positive and statistically significant. For the orthogonal treatment, coefficient estimates are also positive, but they again do not survive adjustment for multiple outcomes. Interaction effects are absent.

Because recycled hybrid grain is not agronomically recommended, as it does not retain the genetic characteristics of the original hybrid and typically entails substantial yield penalties in subsequent generations, we also consider a narrower adoption measure that defines adoption as the use of fresh *Bazooka* seed

Table 3: Adoption

	control mean	ITT trial pack	ITT cooking demo	ITT interaction	nobs
Adoption Index	0.00	0.37**	0.09	-0.08	1495
	(0.81)	(0.07)	(0.08)	(0.10)	
Has used *Bazooka* on any plot in last season?† (1=yes)	0.10	0.67**	0.07	-0.08	1495
	(0.31)	(0.04)	(0.04)	(0.06)	
Number of plots planted with *Bazooka*	0.15	0.84**	0.07	-0.13	1495
	(0.48)	(0.06)	(0.05)	(0.09)	
Number of plots planted with *Bazooka* as share of total number of maize plots	0.10	0.65**	0.07	-0.11	1495
	(0.29)	(0.04)	(0.04)	(0.06)	
Area planted with *Bazooka* (acres)	0.13	0.75**	0.08	-0.15	1495
	(0.44)	(0.08)	(0.06)	(0.12)	
Area planted with *Bazooka* as a share of total maize cultivation area	0.10	0.65**	0.07	-0.11	1495
	(0.29)	(0.04)	(0.04)	(0.06)	
Has used fresh *Bazooka* on any plot in last season?† (1=yes)	0.05	0.02	0.05*	-0.01	1495
	(0.22)	(0.02)	(0.02)	(0.03)	
Has use fresh hybrid or OPV?† (1=yes)	0.30	-0.14**	0.03	0.02	1488
	(0.46)	(0.03)	(0.03)	(0.05)	

Note: Column (1) reports control group means at baseline (and standard deviations below); column (2) reports the estimate of the intent-to-treat effect of the seed trial packs; column (3) reports the estimate of the intent-to-treat 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.

sourced from a trusted supplier (such as an agro-input dealer, the government extension system, or non-governmental organizations). In the control group, only about 5 percent of farmers report using fresh *Bazooka* seed on at least one plot. In contrast to when we allowed for recycling, we now find no evidence that receipt of a seed trial pack increases this share. Interestingly, under this narrower and agronomically grounded definition of adoption of the promoted variety, exposure to the cooking demonstration and tasting session is associated with a statistically significant increase in the likelihood that farmers use fresh *Bazooka* seed in the subsequent season, an effect that was not detectable under a definition that allowed for recycling of the promoted variety.

While the promoted variety is *Bazooka*, the objective of the intervention and of this study is not limited to the adoption of a single seed variety, but more broadly to the uptake of improved crop technologies, including both hybrid seed and open-pollinated varieties (OPVs). Accordingly, we also consider a broader, agronomically valid definition of adoption of seed of improved varieties that includes the use of fresh hybrid seed and fresh or appropriately recycled OPVs (but excluding recycled hybrid grain). Under this broader definition, we find that the trial pack intervention actually *reduces* adoption of improved crop technologies. This pattern is consistent with a crowding-out effect whereby treated framers become less likely to use fresh hybrid seed or fresh or appropriately recycled OPV seed, most likely because Bazooka is considered a free and strong contender for fresh hybrids and fresh or recycled OPVs.

Turning to Table 4 on outcomes related to adoption on a randomly selected plot, we see similar results. Judged by the adoption index, we find a large and positive effect from the seed trial pack. We now also see a positive effect from the cooking demo and tasting sessions, albeit only at the 10 percent significance level. There are again no signs of interaction effects.

Looking into the components of the index, we find that about 10 percent of farmers in the control group used the promoted seed variety on the randomly sampled plot. Adoption is substantially higher among farmers who received the seed trial pack, with an increase of about 64 percentage points relative to the control group. Similar to what we found for adoption on any plot, for farmers invited to the cooking demonstration and tasting session, the estimated effect is positive (p=0.049), but it is not statistically significant after adjusting for multiple hypothesis testing using false discovery rate corrections (q=0.150).

We next examine the intensive margin, and find that the seed trial pack leads to substantial increases in the quantity of Bazooka seed used on the randomly selected plot, both in absolute terms and per acre. These effects suggest that treated farmers not only adopt the promoted variety but also allocate larger quantities of seed to its cultivation. We find no comparable effects for the cooking demonstration and tasting session, nor evidence of complementarities between the two interventions.

Despite these sizable effects of the seed trial pack intervemtion on adoption and the intensity of use, we do not detect corresponding impacts on maize production or productivity on the randomly selected plot in the fully interacted specification. Estimated effects on log production and log productivity are small and statistically indistinguishable from zero across treatment arms. There is, however, some evidence that the cooking demonstration and tasting session increases maize yields. This effect is not precisely estimated in the fully interacted model, but becomes statistically significant when pooling across the orthogonal treatment assignment, suggesting that limited statistical power rather than the absence of an effect may explain the null result in the main specification (see Appendix Table A3).

As in Table 3, we also examine impacts on alternative measures of adoption that are grounded in agronomic principles. Consistent with the broader adoption results, we find no effect of the seed trial pack on the use of fresh Bazooka seed; increased use of the promoted variety in this treatment arm appears to be driven primarily by recycling grain harvested from the trial pack. In contrast, for farmers invited to the cooking demonstration and tasting session, increases in Bazooka use, though imprecisely estimated, are driven by purchases of fresh seed. This distinction is also reflected in productivity outcomes: households that recycle seed in the trial pack treatment do not experience yield gains, whereas farmers who adopt through the purchase of fresh seed in the cooking demonstration and tasting treatment exhibit higher maize productivity.

Finally, and again as in Table 3, we find a decline in the use of seed of improved varieties, which, along with the insignificant effect of the trial pack on fresh *Bazooka* purchases, suggests that (appropriately recycled) OPVs and other (fresh, non-*Bazooka*) hybrid 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 fresh 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 recent OPVs. However, they tended to opt for cheaper alternatives rather than purchasing fresh *Bazooka* seed, which is more expensive than OPVs, which are more popular but lower yielding compared to hybrid varieties. We come back to this in Section 6.

4.3 Use of Harvest

If the interventions affect how farmers perceive and use the promoted variety, they may also influence how harvested maize is allocated across competing uses. In particular, a focus on the use of commercial seed instead of replanting grain mar affect how much of the harvest is retained for seed, while exposure to consumption attributes may increase the share allocated to own consumption or sale. We thus 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.

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

Table 4: Adoption on random plot

	control mean	ITT trial pack	ITT cooking demo	ITT interaction	nobs
Adoption Index	-0.01	0.22**	0.09+	-0.01	1345
	(0.71)	(0.05)	(0.05)	(0.07)	
Has used *Bazooka* on randomly selected plot in last season (yes=1)	0.10	0.64**	0.07	-0.08	1495
	(0.29)	(0.04)	(0.03)	(0.06)	
Quantity of *Bazooka* used on randomly selected plot (kg)	0.55	3.18**	0.22	-0.34	1437
	(1.88)	(0.36)	(0.23)	(0.49)	
Quantity of *Bazooka* used on randomly selected plot (kg/acre)	0.64	4.28**	0.19	-0.38	1427
	(2.15)	(0.32)	(0.22)	(0.50)	
Maize production (log)	5.39	-0.09	0.09	0.04	1410
	(1.02)	(0.08)	(0.09)	(0.12)	
Maize productivity (log)	5.61	-0.01	0.11	0.04	1375
	(0.91)	(0.08)	(0.07)	(0.11)	
Has used fresh *Bazooka* on randomly selected plot in last season (yes=1)	0.04	0.02	0.05*	-0.02	1495
	(0.20)	(0.02)	(0.02)	(0.03)	
Has used fresh hybrid or OPV on randomly selected plot in last season (yes=1)	0.27	-0.13**	0.02	0.02	1495
	(0.45)	(0.03)	(0.03)	(0.05)	

Note: Column (1) reports control group means at baseline (and standard deviations below); column (2) reports the estimate of the intent-to-treat effect of the seed trial packs; column (3) reports the estimate of the intent-to-treat 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: Use of harvest

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

Note: Column (1) reports control group means at baseline (and standard deviations below); column (2) reports the estimate of the intent-to-treat effect of the seed trial packs; column (3) reports the estimate of the intent-to-treat 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).

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 Women's Empowerment

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 economies with norms around gendered division of labor, men typically handle production and marketing decisions, while women are more often involved in domestic responsibilities such as cooking (Van Campenhout, Lecoutere, and Spielman, 2023). The cooking demonstration and tasting session might increase men's awareness that factors beyond yield, such as consumption attributes, may be important in choosing which variates to produce, potentially creating opportunities for women, who often make decisions in the consumption/cooking domain, to participate more in production related decision-making processes.

To explore this, we asked about who participates in household decision-making processes. Specifically, we inquired about who decided which maize variety 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).[12]

Table 6 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's involvement in agricultural decisions.

4.5 Well-being and Food Security

Increasing the adoption of improved seed varieties is a means to an end; farmers ultimately aim to improve their households' food security and overall well-being. While it's unlikely that the small changes in yield that we observe for the cooking demonstration and tasting sessions would lead to meaningful changes in

[12]We also tested stricter definitions of women's involvement in decision-making, such as requiring that the decision was made solely by the woman or jointly with her husband, or even only unilaterally by the woman. These alternative definitions yielded similar (null) results.

Table 6: Impact on women co-head involvement

	control mean	ITT trial pack	ITT cooking demo	ITT interaction	nobs
Women's Empowerment Index	0.00	0.05	-0.18	0.15	1098
	(0.95)	(0.10)	(0.11)	(0.14)	
Woman involved in decision what to plant (1=yes)	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 (1=yes)	0.87	0.02	-0.07	0.05	1098
	(0.34)	(0.04)	(0.04)	(0.05)	

Note: Column (1) reports control group means at baseline (and standard deviations below); column (2) reports the estimate of the intent-to-treat effect of the seed trial packs; column (3) reports the estimate of the intent-to-treat 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.

short-term welfare, we still evaluate the interventions' effects on key welfare indicators, including subjective well-being assessments, food security indicators, and a consumption expenditure aggregate. Results, reported in Online Appendix A2 show no impact.

5 Mechanisms

In this section, we examine intermediary outcomes that help connect the interventions to the adoption effects documented above. Specifically, we document changes in awareness of, and general attitudes toward, improved varieties, followed by changes in perceptions related to their consumption and production attributes.

5.1 Awareness About and General Attitudes Toward Improved Varieties

One reason farmers may have altered their planting decisions is that the interventions affected their awareness of, and general attitudes toward, the promoted seed. We explore this hypothesis using the family of outcomes presented in Table 7, which includes measures of varietal awareness, perception of riskiness, and general sentiment toward improved varieties. When summarizing these outcomes into an index in the top row of this table, we observe a significant positive impact of the seed trial 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*. Table 7 suggest an impact on varietal knowledge resulting from both interventions. On average, control group farmers are aware of approximately 2 to 3 different maize varieties. Both interventions led 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 trial pack intervention increases the probability that farmers report knowing about *Bazooka* by about 30 percentage points compared to non-trial pack group farmers. In contrast, the cooking and tasting demonstration shows a much smaller and insignificant 8 percentage point effect.

To capture more general attitudes toward improved varieties, we examine farmers' perceptions of downside risk associated with their use. Specifically, farmers were asked to assess how likely they believed it was that improved varieties would yield less than local varieties, a margin that is directly relevant for policies that distribute seed trial packs to lower perceived adoption risk. Responses were recorded on a four-point Likert scale ranging from "very likely" to "very unlikely." An analogous question was asked for the promoted variety, Bazooka, among farmers who reported familiarity with it. We construct an indicator for perceived downside risk that equals one if respondents selected either "very likely" or "somewhat likely." Overall, perceived downside risk is low, and we find no statistically significant effects of either intervention on risk perceptions, either for improved varieties in general or for the promoted variety specifically.

A complementary indicator of positive sentiment toward a variety is whether farmers recommend it to others. Peer recommendations are of particular interest given evidence that social learning plays an important role in the diffusion of agricultural technologies and motivates policies that subsidize or temporarily provide seed at no cost (Conley and Udry, 2010; Van Campenhout, 2021). To assess whether the interventions affected peer learning, we asked farmers whether they recommended any improved varieties to others, and, among those familiar with *Bazooka*, whether they recommended *Bazooka* specifically. A substantial share of farmers in the control group (43 percent) reported recommending improved varieties. Receipt of a seed trial pack increased the likelihood of recommending improved varieties by 29 percentage points. Focusing specifically on *Bazooka*, the effect is even larger: farmers who received a seed trial pack were 44 percentage points more likely to recommend the promoted variety.

Finally, we examine farmers' stated intentions regarding future use of improved varieties as an additional indicator of general attitudes. In the control group, 80 percent of farmers report an intention to use improved varieties in the future, while 25 percent report an intention to use Bazooka specifically. We find no treatment effects on stated intentions to use improved varieties in general. However, farmers who received a seed trial pack are 20 percentage points more likely to report an intention to plant Bazooka in the future. This pattern suggests that trial packs primarily shape intentions toward the promoted variety rather than toward improved technologies more broadly, consistent with the observed concentration of adoption responses on 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 trial 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

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

	control mean	ITT trial pack	ITT cooking demo	ITT interaction	nobs
Awareness and Attitudes Index	0.26	0.16**	0.07	-0.03	1156
	(0.37)	(0.05)	(0.05)	(0.06)	
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 varieties are risky (1=yes)	0.08	0.00	0.01	-0.01	1447
	(0.28)	(0.03)	(0.03)	(0.04)	
Thinks *Bazooka* is risky (1=yes)	0.13	0.03	0.01	-0.05	1207
	(0.33)	(0.05)	(0.06)	(0.08)	
Recommended improved varieties to others (1=yes)	0.43	0.29**	0.02	0.01	1538
	(0.50)	(0.06)	(0.05)	(0.07)	
Recommended *Bazooka* to others (1=yes)	0.34	0.44**	0.11	-0.11	1260
	(0.48)	(0.06)	(0.07)	(0.08)	
Will use improved varieties in the future (1=yes)	0.80	0.05	0.01	0.00	1461
	(0.40)	(0.05)	(0.05)	(0.07)	
Will use *Bazooka* in the future (1=yes)	0.25	0.20**	0.05	-0.07	1503
	(0.43)	(0.06)	(0.05)	(0.09)	

Note: Column (1) reports control group means at baseline (and standard deviations below); column (2) reports the estimate of the intent-to-treat effect of the seed trial packs; column (3) reports the estimate of the intent-to-treat 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.

varieties to maize from improved varieties (such as *Longe5*, a popular OPV, or *Bazooka*) 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 trial 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 perceptions in favor of improved seed varieties. This suggests that trial pack interventions may, in some cases, be sufficient to encourage farmers to learn about the consumption traits of new varieties.

5.2.2 Perceptions of Production Traits

The seed trial 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 maize varieties might offer higher yields but were less resistant to fall armyworm (*Spodoptera frugiperda*), a highly destructive invasive pest that has spread rapidly across East Africa in recent years. 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 trial 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 improved maize varieties' production traits, we also include a module in the questionnaire where we ask farmers to compare seed of an improved variety to local seed on five agronomic traits—yield, abiotic stress resistance such as drought or heat resistance, biotic stress resistance such as pests and weed resistance, time to maturity, and seed 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 trial 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 perceptions of consumption traits of maize grain obtained from improved seed varieties versus maize grain obtained from local seed varieties

	control mean	ITT trial pack	ITT cooking demo	ITT interaction	nobs
Consumption Trait Perceptions Index	0.06	0.29**	0.26**	-0.12	1248
	(0.71)	(0.09)	(0.09)	(0.11)	
Improved variety grain tastes better (1=yes)	0.51	0.27**	0.15*	-0.11^{+}	1424
	(0.50)	(0.06)	(0.06)	(0.08)	
Portions obtained from improved variety grain are larger (1=yes)	0.67	0.14**	0.15**	-0.08^{+}	1360
	(0.47)	(0.04)	(0.05)	(0.06)	
Improved variety grain had better appearance (1=yes)	0.77	0.06	0.10*	-0.02	1421
	(0.42)	(0.05)	(0.04)	(0.05)	
Improved variety grain is easier to cook (1=yes)	0.54	0.19**	0.18*	-0.10	1316
	(0.50)	(0.06)	(0.06)	(0.08)	

Note: Column (1) reports control group means at baseline (and standard deviations below); column (2) reports the estimate of the intent-to-treat effect of the seed trial packs; column (3) reports the estimate of the intent-to-treat 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 perceptions of production traits of improved seed varieties versus local seed

	control mean	ITT trial pack	ITT cooking demo	ITT interaction	nobs
Production Trait Perceptions Index	0.02	0.13^{+}	0.05	-0.04	1273
	(0.60)	(0.08)	(0.07)	(0.10)	
Improved seed yields more (1=yes)	0.90	0.03	0.01	-0.02	1464
	(0.29)	(0.03)	(0.03)	(0.04)	
Improved seed is more restistant to abiotic stresses (1=yes)	0.69	0.10	0.00	-0.06	1336
	(0.47)	(0.05)	(0.06)	(0.08)	
Improved seed is more restistant to biotic stresses (1=yes)	0.52	0.12	0.02	-0.05	1391
	(0.50)	(0.06)	(0.06)	(0.09)	
Improve seed matures faster (1=yes)	0.94	-0.01	-0.02	0.05	1446
	(0.24)	(0.03)	(0.02)	(0.04)	
Improved seed has higher germination rates (1=yes)	0.77	0.09	0.06	-0.02	1439
	(0.42)	(0.05)	(0.04)	(0.06)	

Note: Column (1) reports control group means at baseline (and standard deviations below); column (2) reports the estimate of the intent-to-treat effect of the seed trial packs; column (3) reports the estimate of the intent-to-treat 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 Adoption Dynamics Following Seed Trial Packs

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 seed as recommended. In fact, the intervention appears to have partially crowded out the use improved seed varieties (defined as fresh hybrids or appropriately recycled OPVs): some farmers who might have otherwise used improved seed opted to recycle the seed obtained through the trial pack. In this section, we explore possible explanations for this behavior, including limited knowledge about hybrid seed degeneration, perceived profitability of recycling and aversion to market price risks, trust and quality perceptions, and liquidity constraints.

6.1 Limited Knowledge

A first plausible explanation is that farmers may be unaware that hybrid seed, such as *Bazooka*, is not intended for recycling.[13] 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*), which have likely gone through successive cycles of reuse, averaged 323 kilograms per acre, while those who recycled *Bazooka* seed just once from the seed trial pack obtained intermediate yields of around 375 kilograms per acre.

[13] 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 trial pack 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.

6.2 Profitability and Aversion to Market Price Risk

Seed adoption decisions differ from those for standard variable inputs because seed can be reproduced, making replacement choices intertemporal and forward-looking. Classic models of seed replacement emphasize that farmers optimally balance expected yield deterioration of retained seed against the costs and uncertainty associated with purchasing new seed, implying that seed recycling can be a rational response even when improved varieties are available (Heisey and Brennan, 1991).

Figure 2 illustrates estimated profits per acre of maize as a function of the sales price per 100 kg bag, computed by combining the yield estimates reported above with observed price and cost parameters, for three seed types: fresh Bazooka hybrid (535 kg/acre), recycled Bazooka (375 kg/acre), 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 selling 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 was UGX 60,000 per bag, recycling the harvest from the trial pack for a single season appears to be a profit-maximizing response for the majority of farmers. With its low upfront investment, recycling would also maximize expected utility for a risk averse farmer concerned about any production or price risk that may exist.

Over time, as farmers continue to recycle *Bazooka* seed across multiple seasons, the associated yield (and thus the profit (long-dash blue) line in Figure 2) can be expected to converge toward that of local seed (the short-dash 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 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.

6.3 Quality Issues

Another possible explanation for the observed differences in recycling rates across treatment groups is that farmers may place greater trust in seed provided

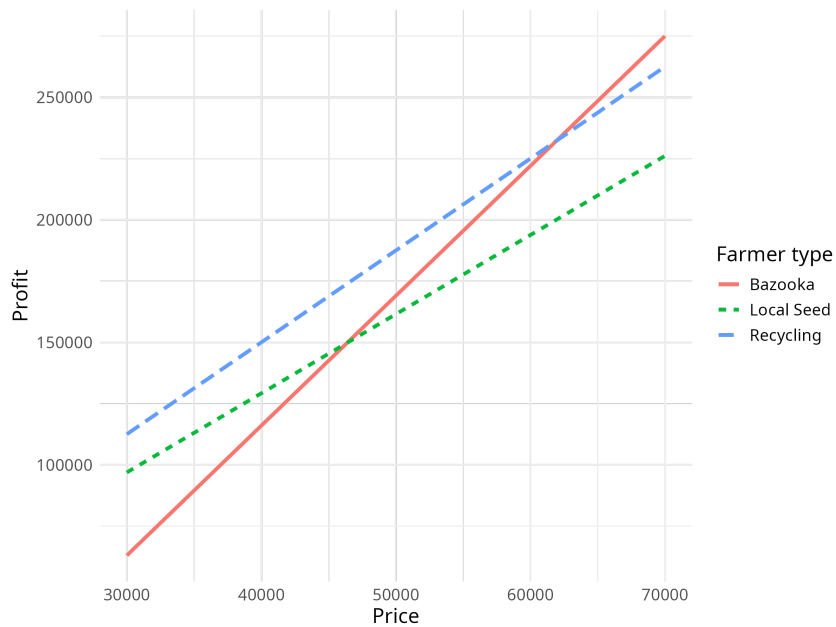

Figure 2: Profits as a function of sales price

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 that 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 its 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 pack. Satisfaction declines somewhat among farmers who recycled *Bazooka* from the trial pack, but even recycled seed is generally rated more favorably than fresh *Bazooka* seed purchased from agro-dealers.

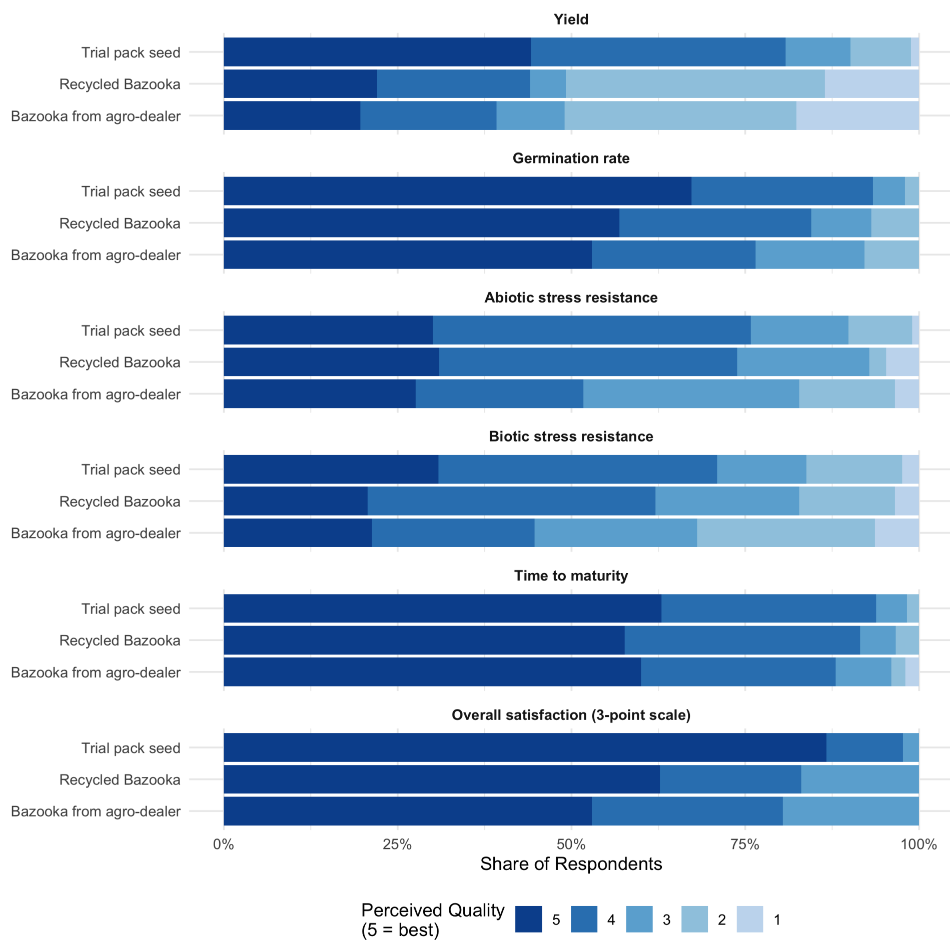

Figure 3: Perceptions of quality of *Bazooka* from agro-dealer vs seed trial pack

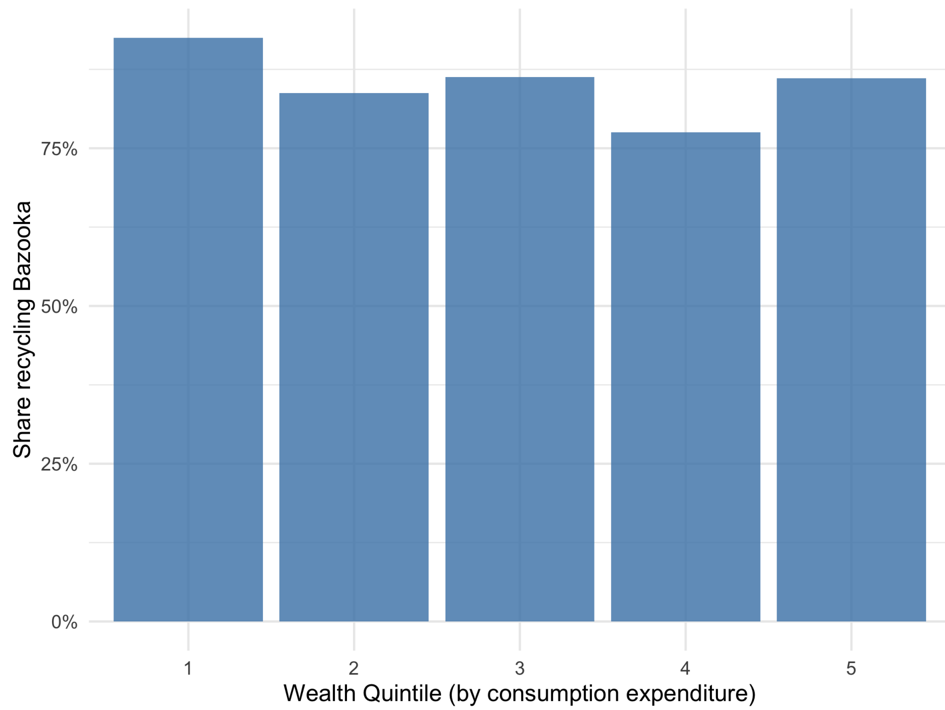

Figure 4: Wealth and Recycling

6.4 Liquidity Constraints

Cash or credit constraints may also induce recycling. If a farmer wants to plant *Bazooka*, treatment farmers can fall back on recycling as a cost-minimizing strategy, while control farmers likely must purchase seeds. As a result, liquidity constraints may 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 (Figure 4). Recycling of *Bazooka* seed is common across all wealth groups. In the lowest quintile, more than 90 percent of farmers recycled, compared to roughly 84–86 percent in the middle quintiles and 77 percent in the fourth quintile. Overall, the data suggest only modest variation in recycling behavior by wealth, with slightly lower recycling rates observed among relatively wealthier farmers.

7 Conclusion

In this study, we evaluate two commonly used extension strategies aimed at promoting the adoption of an improved maize variety among smallholder farmers in eastern Uganda: seed trial packs emphasizing production attributes and cooking demonstrations emphasizing consumption attributes. Using a cluster-randomized field experiment with a 2×2 factorial design, we show that both interventions shape farmers' perceptions and use of the promoted variety, but through distinct pathways and with different implications for productivity.

The seed trial pack substantially increased farmers' awareness and appreciation of both production- and consumption-related attributes and led to a marked increase in subsequent use of the promoted variety. However, this adoption response was driven primarily by the recycling of harvested grain as seed. For hybrid varieties like the variety we used in the study, such recycling entails a yield penalty, and consistent with this, we find no evidence of yield gains despite increased use. In contrast, cooking demonstrations and tasting sessions primarily influenced perceptions of consumption attributes. While their effects on adoption were more modest, farmers exposed to this intervention were more likely to use fresh seed, and we find suggestive evidence of associated yield gains.

Taken together, these findings highlight the importance of aligning extension strategies with the technological characteristics of the promoted seed. Trial-based extension appears well suited to technologies that are compatible with farmer-led reproduction, such as open-pollinated varieties, but may generate unintended consequences when applied to non-reproducible technologies like hybrids. In such settings, trial packs can crowd out purchases of certified seed by encouraging recycling, even when farmers recognize the agronomic superiority of the variety.

More broadly, our results underscore that observed adoption patterns reflect rational responses to economic and structural constraints rather than simple information failures. For the average farmer in our sample, recycling seed appears to be a reasonable choice given modest yield penalties, high seed costs relative to grain prices, unreliable input quality, and limited liquidity. These findings suggest that efforts to promote hybrid seed adoption may need to be complemented by interventions that relax liquidity constraints, improve access to affordable high-quality seed, or strengthen output market incentives that reward productivity gains.

Finally, while consumption traits clearly matter for varietal choice, our results indicate that farmers can learn about these attributes through direct experience with harvested grain, potentially reducing the need for costly demand-creation interventions. Future research should further explore how different modes of experiential learning interact with technology characteristics and market conditions to shape adoption dynamics. Understanding these interactions is crucial for designing extension strategies that promote not only uptake, but also sustained, productivity-enhancing use of improved crop technologies.

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Online Appendix

A1 Pooled models

In this Online Appendix, we provide results for the same set of outcomes and in similar table as in the main manuscript, but now estimated using an alternative empirical specification. 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):

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

where now T_i^M is a dummy for the main treatment (the seed trial 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 trial 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 trial pack treatment of Equation 1 and the effect of the seed trial pack in the presence of the cooking demonstration and testing treatment.

Tables A1 through A8 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 trial 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 trial 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)

	control mean	ITT trial pack	control mean	ITT 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 (1-yes)	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 the promoted seed (*Bazooka*) on any plot in the last season (1=yes)	0.09	0.01	0.10	-0.01
	(0.29)	(0.02)	(0.30)	(0.02)
Has used quality maize seed on any plot in last season (1=yes)	0.42	-0.04	0.41	-0.02
	(0.49)	(0.03)	(0.49)	(0.03)
Seed on random plot was obtained from formal seed source (1=yes)	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 (1=yes)	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 group means at baseline (and standard deviations below); column (2) reports the estimate of the intent-to-treat effect of the seed trial packs; column (3) reports the estimate of the intent-to-treat 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).

Table A2: Adoption (pooled)

	control mean	ITT trial pack	control mean	ITT cooking demo	nobs
Adoption Index	0.05	0.33**	0.18	0.05	1495
	(0.84)	(0.05)	(0.75)	(0.05)	
Has used *Bazooka* on any plot in last season in last season† (1=yes)	0.14	0.63**	0.44	0.03	1495
	(0.35)	(0.03)	(0.50)	(0.03)	
Number of plots planted with improved varieties	0.18	0.78**	0.56	0.01	1495
	(0.49)	(0.04)	(0.74)	(0.04)	
Number of plots with improved varieties as share of total number of plots	0.13	0.60**	0.42	0.01	1495
	(0.33)	(0.03)	(0.49)	(0.03)	
Area planted with improved varieties (acres)	0.17	0.67**	0.50	0.01	1495
	(0.49)	(0.06)	(0.85)	(0.06)	
Area planted with improved varieties as a share of total maize cultivation area	0.13	0.60**	0.42	0.01	1495
	(0.33)	(0.03)	(0.49)	(0.03)	
Has used fresh *Bazooka* on any plot in last season in last season† (1=yes)	0.08	0.02	0.06	0.05**	1495
	(0.27)	(0.02)	(0.25)	(0.02)	
Has used seed of an improved variety on any plot in last season† (1=yes)	0.31	-0.13**	0.23	0.04+	1488
	(0.46)	(0.03)	(0.42)	(0.02)	

Note: Column (1) reports control group means at baseline (and standard deviations below); column (2) reports the estimate of the intent-to-treat effect of the seed trial packs; column (3) reports the estimate of the intent-to-treat 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.

Table A3: Adoption on random plot (pooled)

	control mean	ITT trial pack	control mean	ITT cooking demo	nobs
Adoption Index	0.05	0.22**	0.10	0.08*	1345
	(0.70)	(0.04)	(0.64)	(0.04)	
Has used *Bazooka* on randomly selected plot in last season (yes=1)	0.13	0.60**	0.42	0.03	1495
	(0.34)	(0.03)	(0.49)	(0.03)	
Quantity of improved variety used on randomly selected plot (kg)	0.74	3.01**	2.11	0.05	1437
	(2.44)	(0.25)	(3.44)	(0.25)	
Quantity of improved variety used on randomly selected plot (kg/acre)	0.79	4.09**	2.72	0.00	1427
	(2.38)	(0.25)	(4.06)	(0.25)	
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)	
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)	
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)	

Note: Column (1) reports control group means at baseline (and standard deviations below); column (2) reports the estimate of the intent-to-treat effect of the seed trial packs; column (3) reports the estimate of the intent-to-treat 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 A4: Use of harvest (pooled)

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

Note: Column (1) reports control group means at baseline (and standard deviations below); column (2) reports the estimate of the intent-to-treat effect of the seed trial packs; column (3) reports the estimate of the intent-to-treat 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 A5: Impact on women co-head involvement (pooled)

	control mean	ITT trial pack	control mean	ITT cooking demo	nobs
Women's Empowerment Index	-0.09	0.12^{+}	0.03	-0.11	1098
	(1.03)	(0.07)	(0.90)	(0.07)	
Woman involved in decision what to plant (1=yes)	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 (1=yes)	0.83	0.05	0.88	-0.04	1098
	(0.37)	(0.03)	(0.33)	(0.03)	

Note: Column (1) reports control group means at baseline (and standard deviations below); column (2) reports the estimate of the intent-to-treat effect of the seed trial packs; column (3) reports the estimate of the intent-to-treat 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 A6: Knowledge, risk, social learning, and intentions (pooled)

	control mean	ITT trial pack	control mean	ITT cooking demo	nobs
Awareness and Attitudes Index	0.30	0.14**	0.36	0.05+	1156
	(0.36)	(0.03)	(0.38)	(0.03)	
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 varieties are risky (1=yes)	0.09	0.00	0.08	0.00	1447
	(0.28)	(0.02)	(0.28)	(0.02)	
Thinks *Bazooka* is risky (1=yes)	0.13	0.01	0.14	-0.02	1207
	(0.34)	(0.04)	(0.35)	(0.04)	
Recommended improved varieties to others (1=yes)	0.44	0.29**	0.58	0.02	1538
	(0.50)	(0.04)	(0.49)	(0.04)	
Recommended *Bazooka* to others (1=yes)	0.40	0.38**	0.60	0.06	1260
	(0.49)	(0.04)	(0.49)	(0.04)	
Will use improved varieties in the future (1=yes)	0.80	0.05	0.82	0.01	1461
	(0.40)	(0.03)	(0.38)	(0.03)	
Will use *Bazooka* in the future (1=yes)	0.28	0.16**	0.35	0.02	1503
	(0.45)	(0.04)	(0.48)	(0.04)	

Note: Column (1) reports control group means at baseline (and standard deviations below); column (2) reports the estimate of the intent-to-treat effect of the seed trial packs; column (3) reports the estimate of the intent-to-treat 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.

Table A7: Impact on perceptions of consumption traits of maize grain obtained from improved seed varieties versus maize grain obtained from local seed varieties (pooled)

	control mean	ITT trial pack	control mean	ITT cooking demo	nobs
Consumption Trait Perception Index	0.20	0.23**	0.23	0.20**	1248
	(0.65)	(0.05)	(0.66)	(0.05)	
Improved variety grain tastes better (1=yes)	0.59	0.21**	0.65	0.09**	1424
	(0.49)	(0.04)	(0.48)	(0.04)	
Portions obtained from improved variety grain are larger (1=yes)	0.75	0.10**	0.75	0.10**	1360
	(0.43)	(0.03)	(0.43)	(0.03)	
Improved variety grain had better appearance (1=yes)	0.83	0.05*	0.80	0.09**	1421
	(0.38)	(0.03)	(0.40)	(0.03)	
Improved variety grain is easier to cook (1=yes)	0.64	0.14**	0.65	0.13**	1316
	(0.48)	(0.04)	(0.48)	(0.04)	

Note: Column (1) reports control group means at baseline (and standard deviations below); column (2) reports the estimate of the intent-to-treat effect of the seed trial packs; column (3) reports the estimate of the intent-to-treat 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 A8: Impact on perceptions of production traits of improved seed varieties versus local seed (pooled)

	control mean	ITT trial pack	control mean	ITT cooking demo	nobs
Production Trait Perceptions Index	0.04	0.11*	0.09	0.03	1273
	(0.60)	(0.05)	(0.58)	(0.05)	
Improved seed yields more (1=yes)	0.91	0.03	0.92	0.00	1464
	(0.29)	(0.02)	(0.27)	(0.02)	
Improved seed is more restistant to abiotic stresses (1=yes)	0.69	0.06	0.73	-0.03	1336
	(0.46)	(0.04)	(0.44)	(0.04)	
Improved seed is more restistant to biotic stresses (1=yes)	0.53	0.09	0.59	-0.01	1391
	(0.50)	(0.04)	(0.49)	(0.04)	
Improve seed matures faster (1=yes)	0.93	0.01	0.93	0.00	1446
	(0.26)	(0.02)	(0.25)	(0.02)	
Improved seed has higher germination rates (1=yes)	0.80	0.08+	0.82	0.05	1439
	(0.40)	(0.03)	(0.39)	(0.03)	

Note: Column (1) reports control group means at baseline (and standard deviations below); column (2) reports the estimate of the intent-to-treat effect of the seed trial packs; column (3) reports the estimate of the intent-to-treat 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).

A2 Well-being and Food Security

This section provides more details on the impact of seed trial packs and the cooking and tasting session on food security and welfare indicators. Results for the fully interacted models are in Table A9, while results for the pooled models are in Table A10.

Judging by the index values, we indeed 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 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 trial 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 trial pack group are more likely to feel that their well-being improved over time compared to those who did not receive a seed trial 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 trial pack, either through real or perceived harvest increases (despite yields not showing a significant increase on average, as detailed in Table 4).

Next, we use two questions from the The Household Food Insecurity Access Scale (HFIAS), including 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 "reductions in food 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).[14] Table A9 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

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

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.

Table A9: Welfare and food security

	control mean	ITT trial pack	ITT cooking demo	ITT interaction	nobs
Welfare and Food Security Index	0.01	0.00	-0.08	0.13	1471
	(0.58)	(0.06)	(0.06)	(0.09)	
HH is better off than average of village? (1=yes)	0.35	0.00	-0.02	0.05	1504
	(0.48)	(0.05)	(0.05)	(0.07)	
HH is better off than 6 months ago? (1=yes)	0.38	0.10	-0.05	0.06	1531
	(0.49)	(0.05)	(0.05)	(0.07)	
HH always eat what they want (1=yes)	0.52	-0.07	-0.07	0.21	1538
	(0.50)	(0.07)	(0.07)	(0.10)	
HH can always eat quantity needed (1=yes)	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)	

Note: Column (1) reports control group means at baseline (and standard deviations below); column (2) reports the estimate of the intent-to-treat effect of the seed trial packs; column (3) reports the estimate of the intent-to-treat 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 A10: Welfare and food security (pooled)

	control mean	ITT trial pack	control mean	ITT cooking demo	nobs
Welfare and Food Security Index	0.01	0.06	0.01	-0.01	1471
	(0.59)	(0.04)	(0.57)	(0.04)	
HH is better off than average of village (1=yes)	0.35	0.02	0.35	0.00	1504
	(0.47)	(0.03)	(0.48)	(0.03)	
HH is better off than 6 months ago (1=yes)	0.38	0.12**	0.38	-0.02	1531
	(0.48)	(0.03)	(0.50)	(0.03)	
HH can always eat what they want (1=yes)	0.52	0.03	0.52	0.03	1538
	(0.50)	(0.05)	(0.50)	(0.05)	
HH can always eat quantity needed (1=yes)	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)	

Note: Column (1) reports control group means at baseline (and standard deviations below); column (2) reports the estimate of the intent-to-treat effect of the seed trial packs; column (3) reports the estimate of the intent-to-treat 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).