Pricing and Allocation of New Agricultural Technologies *

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Abstract

This study uses a two-stage experiment to examine whether lower prices allocate new agricultural technologies to farmers with lower returns. In stage one, I randomize a price subsidy, ranging from full to zero subsidies, for a new wheat seed variety. In stage two, I randomize free distribution across the self-selected sample of non-buyers from stage one. This design allows me to compare treatment effects across the entire population with treatment effects among the sample choosing not to buy the seed. If higher prices screen out farmers with low willingness to adopt, then the effect of stage-two free distribution on adoption by non-buyers should be trivial. Instead, I find that the stage-two free distribution increases adoption and wheat cultivation by an amount almost equal to the effect from stage one. In addition, farmers choosing not to buy in stage one do *not* realize lower returns to adoption – despite there being substantial heterogeneity in returns across the sample. A potential mechanism for explaining the results is that binding credit constraints prevent some farmers from buying in stage one. Free distribution in stage two selects in farmers who are credit constrained but do not have systematically lower returns to adoption. Taken together, these findings imply that policy makers who aim to increase dissemination of agricultural technologies cannot rely on market prices as a mechanism for targeting high return farmers.

Keywords: pricing, subsidies, self-selection, technology adoption, agriculture, field experiments.

JEL Codes: C93, D61, M31, O12, O13, Q12, Q16

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1 Introduction

Prices play an important role in the allocation of goods. A subsidy is a key policy tool for lowering prices and increasing take-up of goods with high expected benefits. The prevalence of subsidies in settings such as agricultural production and public health can be justified by market frictions (e.g., information imperfections as in Carter, Laajaj, and Yang 2021; or large positive externalities as in Cohen and Dupas, 2010; and in Kremer and Miguel, 2007). However, there is little evidence on how prices allocate agricultural technologies across farmers with potentially heterogeneous returns. This paper provides the first experimental evidence to address the question: do higher agricultural input prices screen out farmers with low returns to adoption?

Subsidies can have an ambiguous effect on the allocation of agricultural technologies. On the one hand, if willingness-to-pay (WTP) for a new technology is influenced solely by farmers' returns, then lower prices will induce take-up by farmers who value the technology less and realize lower returns to adoption. On the other hand, demand may lie below the social optimum due to factors such as positive externalities or constraints to adoption.[1] The correlation between adoption constraints and farmers' returns is unknown. If adoption constraints are positively correlated with returns, then lower prices will induce take-up by farmers who are most constrained and have high returns to adoption.

In this paper, I examine the consequences of lowering the price of a new seed variety on take-up and allocative efficiency. I experiment with an improved wheat seed that is introduced in a setting in which farmers make simultaneous decisions on what crops to grow and which seed varieties to use. Agronomic studies show that the improved seed is resistant to a contagious crop disease called wheat blast. In addition, the new seed can result in higher yields compared to existing wheat varieties. However, wheat is only one crop in the farmers' choice set. Factors contributing to heterogeneity in farmers' returns include soil quality, weather shocks, and market access, among others.[2] Some sources of heterogeneity may be known to the farmer ex-ante when they make seed purchase decisions, while others may not.

I use a two-stage randomized controlled trial (RCT) to test whether switching from a partial subsidy to a full subsidy differentially allocates the new seed variety to farmers with higher or lower realized returns. In the first stage, I randomly allocate 220 villages to different subsidy levels for the improved seed, ranging from zero subsidy (i.e., official price) to full subsidy (free distribution). I also preserve a group of villages to serve as a pure control group that does not receive any intervention. I divide the subsidy levels into three categories of high (50-100%), medium (25-40%), and low (0-20%) subsidy. In the second stage, I randomly allocate villages in the medium- and low-subsidy categories into stage-two treatment and stage-two control. In stage-two treatment villages, farmers who did *not* buy the seeds in stage one are offered the same seed package for free before they start planting. Stage-two control villages, on the other hand, do not receive any further intervention after receiving stage one treatment. That is, stage-two treatment randomizes free distribution across non-buyers from stage one. The two stages of the experiment are implemented in one agricultural season before planting.

The two-stage experimental design allows me to compare the outcomes of non-buyers with the outcomes of the farmers who receive the seeds for free at random. First, I exploit the randomization of free distribution to a subset of villages in stage one to estimate treatment effects over the entire

[1] Learning externalities from agricultural technology adoption have been documented in the literature (e.g., Conley and Udry, 2010; Foster and Rosenzweig, 1995; Munshi, 2004). Credit constraints can be particularly problematic for farmers who are short of liquidity right before planting, when most agricultural investment decisions are made (Field et al., 2013; Fink, Jack, and Masiye, 2020; Karlan and Mullainathan, 2010). In addition, the lack of complete insurance markets could hinder farmers from making optimal investments (Cole, Stein, and Tobacman, 2014; Emerick et al., 2016; Karlan et al., 2014). Imperfect information could also result in low adoption due to underestimation of the expected returns from a new agricultural technology (Carter, Laajaj, and Yang, 2021).

[2] See Suri and Udry (2022) for a detailed discussion on the sources of heterogeneity in farmers returns.

population. Second, I make use of stage-two randomization to estimate treatment effects among the farmers who choose not to buy the seed at stage one. Using these estimates, I examine whether the realized returns of the farmers who decline to buy the seeds are different from the realized returns of the average farmer.

The first stage of the experiment shows that farmers are highly responsive to the subsidy. As the subsidy rate decreases from a high subsidy level to a low subsidy level, demand decreases from 94% to 6%. The immediate question is whether farmers who do not buy the seed do so because they expect low returns to adoption or because some constraints prevent them from buying the seed.[3] The results on actual adoption among seed buyers suggest that the subsidy does not sort farmers based on their likelihood of planting the seeds. Unexpectedly, the likelihood of planting the seeds is similar across farmers who took up the seeds at different subsidy levels.

The second stage of the experiment shows that farmers who decline to buy the seeds in stage one are willing to plant the seed and increase their wheat cultivation when offered the seed for free.[4] Stage-two free distribution to non-buyers causes a net increase in adoption of 32 percentage points, compared to a 41-percentage point increase in adoption in stage-one free-distribution villages. The similarity in treatment effects on adoption between non-buyers and the average farmer persists one year after the intervention. Not only do farmers use the distributed seeds to replace existing wheat seeds, they also change their cropping pattern and increase wheat cultivation at the extensive and intensive margins. The treatment effect of stage-two free distribution on wheat cultivation by non-buyers (21 percentage point increase) is on par with the treatment effect of stage-one free-distribution on wheat cultivation by the average farmer (28 percentage-point increase). These findings suggest that modest subsidy levels in stage one prevent farmers who are willing to adopt the new seed – or almost as willing to adopt as the average farmer in the population – from buying.

A comparison between the returns of the average farmer and the returns of self-selected non-buyers should reveal whether prices have a selection effect. A positive selection effect occurs when farmers with lower returns to adoption select out of buying the seed as the price increases (i.e., the subsidy level decreases). I measure farmers' returns using data on profits (after subtracting the subsidy) at the plot level. Results on plot profits show that returns to adoption are low for the entire population. Low average returns can be explained by the finding that farmers substitute away from relatively more lucrative crops to increase wheat cultivation. Yet, importantly, I find that the profits of the average farmer in stage-one free-distribution villages are similar to the average profit of all non-buyers. This comparison shows that, for the entire sample of non-buyers, higher prices do not screen out farmers with lower returns.

The selection effect of prices may depend on the subsidy level, especially with an elastic demand curve. I separately examine the selection effect among non-buyers in the sub-sample of villages that receive a medium versus a low subsidy level in stage one. I find that the effect of stage-two free distribution on adoption is similar for non-buyers in the medium-subsidy and low-subsidy villages. At the same time, the realized revenues and profits of non-buyers in the medium-subsidy villages are lower than that of non-buyers in the low-subsidy villages. The two-stage experimental design allows me to identify the average returns of the farmers who would be induced to take up the seed if the subsidy level increased (i.e., would-be buyers). I show that would-be buyers at the medium subsidy level have relatively high returns. Therefore, an increase in the subsidy level in the study setting does not distort allocation to lower return farmers. If anything, the medium subsidy level increases take-up by relatively high return farmers for whom a low subsidy level would prevent them from buying.

[3] The size of the seed package offered to treated farmers should be enough for an average plot size of around 0.3 acres. The package size was determined based on results of a pilot study and seeding rate recommendations from agronomists.

[4] The reported uses of the distributed seeds by treated farmers are: planting the seeds on one of the farmer's plots, passing the seeds to another farmer to be planted on the other farmer's plot, using wheat seeds for food, or other uses.

Several mechanisms may explain the finding that non-buyers do not realize lower returns to adoption compared to the average farmer. First, factors other than returns may influence farmers' purchase decisions. For instance, the presence of a binding credit or liquidity constraint could mean that farmers' purchase decisions are driven by their ability to pay rather than their willingness-to-pay. Second, the (post-harvest) realized returns of the average farmer can be similar to those of non-buyers, even if their (pre-planting) expected returns are different. While I do not attempt to isolate one mechanism as an exclusive explanation for the main results, I do test for the hypothesis that factors other than expected returns influence farmers' purchase decisions.

I apply a data-driven approach to analyze the heterogeneity in treatment effects across farmers based on a large set of baseline covariates. I use machine learning (ML) methods to examine the heterogeneity in predicted treatment effects for the sub-sample of farmers who received stage-one free-distribution treatment on the one hand, and the sub-sample of non-buyers who received stage-two free-distribution treatment on the other hand. I find strong evidence of heterogeneity in treatment effects for both sub-samples. This finding reinforces the presumption that farmers have heterogeneous returns to adoption.

I examine whether covariates that can serve as indicators for market frictions (e.g., credit market failures) distinguish farmers with the highest from those with the lowest predicted treatment effects. As for the predicted treatment effects on growing wheat, non-buyers with the highest treatment effects are more likely to face constraints to obtaining credit compared to non-buyers with the lowest treatment effects. For the predicted treatment effects on profits, non-buyers with the highest and lowest predicted treatment effects differ minimally with respect to the likelihood of reporting constraints to borrowing.

Therefore, the heterogeneity analysis results suggest that free distribution in stage two selects in farmers who are credit constrained but do not have systematically lower returns to adoption. This finding is consistent with a model in which a binding credit constraint creates a wedge between farmers' WTP for agricultural inputs and their expected marginal returns. In this case, a full subsidy to non-buyers can alleviate a binding credit constraint without distorting allocation to low return farmers.

My paper builds on a strand of the literature that analyzes self-selection into agricultural investments. On the demand side, farmers are found to self-select into loan take-up based on their returns to capital (Beaman et al., 2023). In addition, farmers with higher WTP realize higher benefits from a sophisticated agricultural technique such as laser land leveling (Lybbert et al., 2017). On the supply side, willingness-to-accept as measured by reverse auctions has proven to be an effective mechanism for targeting conservation investments (e.g., Jack, 2013; Jack, Leimona, and Ferraro, 2009). My study rather focuses on self-selection into purchasing an agricultural input that has multiple (productive as well as unproductive) uses and heterogeneous returns. I contribute to this literature by using an experimental design to examine the actual returns of non-buyers in comparison with the returns of the average farmer.

The literature on how pricing and subsidy decisions affect the allocation of goods has shown mixed results. In the case of preventative health products, a full subsidy is found to increase both take-up and usage in contexts where private benefits are lower than social benefits (e.g., deworming: Kremer and Miguel, 2007) or where price elasticity of demand is very high even at low prices (e.g., antimalarial bednets: Cohen and Dupas, 2010). In contrast, other studies have shown that prices for some goods have a selection effect such that buyers with higher WTP are more likely to use the product (see Ashraf, Berry, and Shapiro, 2010, for the case of water chlorination) and that marginal benefits increase with buyers' WTP (e.g., Berry, Fischer, and Guiteras, 2020, for the case of water filters). I contribute to this literature by looking at the selection effect of prices in a context where the new technology is an agricultural input – an area where subsidies are widespread, yet little is known about the allocative efficiency impact of increasing price subsidies.

Furthermore, I contribute to the literature on the adoption of agricultural technologies in developing countries. Several explanations for low adoption have been offered in the literature, including informational constraints (Ashraf, Gine, and Karlan (2009); Carter, Laajaj, and Yang (2021); Hanna, Mullainathan, and Schwartzstein (2014)), behavioral constraints (Duflo, Kremer, and Robinson, 2011), and heterogeneity in comparative advantage (Suri, 2011). The paper that is closest to mine is that of Suri (2011), which uses panel data to show that heterogeneity in net benefits (i.e., marginal benefits after accounting for transportation costs) can explain low adoption of hybrid maize in Kenya. My paper provides experimental evidence that higher prices can act as a barrier preventing take up by farmers who are otherwise willing to experiment with a new seed variety.

Evidence for the impact of agricultural input subsidies from RCTs is quite rare. Carter, Laajaj, and Yang (2021) used an RCT for evaluating a one-off input subsidy program in Mozambique targeting "progressive" maize farmers (i.e., farmers subjectively selected by extension agents as having high potential). A similar RCT evaluated a targeted intervention package known as the Wheat Initiative in Ethiopia (Abate et al., 2018). Giné et al. (2022), on the other hand, focus on equity-efficiency tradeoff in the targeting of agricultural input subsidies in Tanzania. They find that local committees are effective in targeting productive farmers. I add to this literature by evaluating the allocative efficiency of an input subsidy on a general population of farmers. I examine the selection effect of prices in the absence of an explicit targeting mechanism.

The policy implications of my findings are multifaceted. An important policy question is whether prices can serve as a mechanism for screening farmers with high returns to adoption of new technologies. Using causal evidence from a randomized field experiment, I show that higher prices do **not** screen out low-return farmers. On the contrary, I show that lowering the price, by increasing the subsidy level from low to medium, is expected to induce take-up by farmers with higher than average returns. In practice, agricultural input markets are heavily regulated. In my study setting, the government sets an official price for new seed varieties and attempts to increase dissemination by distributing seeds for free to a (subjectively) selected sample of farmers. Alternative policy options include reducing the official price to all farmers (since this study shows that lower prices do not distort allocation to low-return farmers), or finding a new mechanism for targeting high-return farmers using objective measures. Further research is needed for evaluating new mechanisms for targeting the dissemination of new agricultural technologies to high-return farmers.

The rest of the paper is organized as follows: Section 2 introduces the study setting and presents the two-stage experimental design. Section 3 outlines the timeline for data collection and describes the data. Section 4 presents results on demand, adoption, and returns to adoption. Section 5 explores some potential mechanisms that can explain the key findings. Section 6 concludes.

2 Research Design

2.1 Study Setting

The subsidized agricultural technology in my experiment is an improved wheat seed variety called "*BARI Gom 33*". The new seed was developed by the International Maize and Wheat Improvement Center (CIMMYT) in collaboration with the Bangladesh Agricultural Research Institute (BARI) as a rapid response to a newly emerged crop disease called wheat blast. The wheat blast is a fungal seed disease that first emerged in Brazil, and has spread to other countries including Bangladesh and Zambia through international grain trade. In Bangladesh, the wheat blast first appeared in the 2015-2016 winter season and has spread rapidly across districts (see Figure A.1). Reported blast-related losses reached 51% of the affected farms' harvest (CIMMYT, 2019). Farmers in my sample are aware of the risk of a blast outbreak. At baseline, one third of farmers listed wheat blast as one of the most important diseases affecting dry season crops in their village. When asked about the likelihood of a blast outbreak over the next season, 54% of the respondents perceived the likelihood of a balst outbreak to be more than fifty percent. The fact that wheat blast can spread through wind-blown spores renders it highly contagious. Early attempts to fight wheat blast with fungicides were not successful since fungicides provide only partial defense and are not cost-effective for smallholder farmers. Before the introduction of *BARI Gom 33*, a short-term policy response to limit the spread of wheat blast was to discourage farmers from cultivating wheat in blast-prone districts.[5]

BARI Gom 33 was locally tested by CIMMYT in Bangladesh and is expected to have a number of benefits. First, *BARI Gom 33* is resistant to wheat blast, which means that the seed has an implicit insurance feature. In the event of a blast outbreak, farmers growing *BARI Gom 33* seeds are insured against blast-related losses. Second, *BARI Gom 33* has a yield advantage of 5-8% relative to existing wheat varieties. Third, *BARI Gom 33* is biofortified with zinc, an important micronutrient given the high levels of zinc deficiency in Bangladesh (Mottaleb et al., 2019). The new seed is still in early stages of dissemination as it was first released in the fall of 2017. A short market survey carried out as part of the baseline data collection shows that 8% of the retailers in the sampled districts were selling *BARI Gom 33* seeds.

The Bangladesh government is keen on increasing dissemination of *BARI Gom 33* seeds because of the potential for environmental as well as pecuniary externalities. First, the environmental externality of blast resistance implies that the social benefits of the improved seed may exceed the private benefits to the farmer. Farmers may undervalue losses averted due to the blast-resistant seed. Second, the potential for increasing domestic wheat production and decreasing reliance on wheat imports can have a pecuniary externality. Indeed, Bangladesh is the fifth largest wheat importer in the world. The country's annual wheat imports are in the range of six million tons (USDA, 2021).[6] The risk of dependence on wheat imports has become more patent in the aftermath of the war in Ukraine (Mamun, Glauber, and Laborde, 2022). To increase dissemination of a new seed variety, a common policy by the Ministry of Agriculture is to distribute improved seeds for free to a selective sample of farmers. In this study, I use a randomized controlled trial to examine the implications for allocative efficiency of moving along a spectrum of prices going from the official price to a full subsidy.

Improved wheat seeds suit my research question on the allocation efficiency of price subsidies for two reasons. First, although *Bari Gom 33* is expected to have positive impacts on wheat productivity, not all farmers have the same returns to growing wheat. Factors such as farm management skills, and agroclimatic conditions (e.g., flash floods or a short winter season) can affect farmers' returns. Second, farmers choose not only their seed variety, but also the type of crop to grow in a given season. A potential source of heterogeneity is the substitute crops in a farmer's choice set. For instance, infrastructure constraints such as limited access to irrigation could prevent some farmers from growing water-intensive crops such as rice or sugar cane. This is particularly relevant for wheat cultivation in Bangladesh since wheat is grown during the dry season. Moreover, credit or liquidity constraints can limit farmers' ability to grow cash crops due to high input costs.

[5]A similar policy was adopted by West Bengal government in India to avoid the spread of the wheat blast across borders. In 2017, wheat cultivation in West Bengal was banned within 5 kilometers of Bangladesh border (CIMMYT, 2021).

[6]Wheat exports are prohibited in Bangladesh per the government's export policy.

2.2 Two-Stage Experimental Design

I use a two-stage experimental design as summarized in Figure 1.[7] In this section, I present the randomization procedures for the two stages of the experiment. I show how the experimental designs allows me to estimate treatment effects among farmers who choose not to buy the seeds. Also, I explain how the experimental design allows me to infer potential profits among farmers who would have bought the seeds had they been offered a positive price.

Treatment randomization followed two steps. First, I randomly allocated a sample of 220 villages from 12 sub-districts to three treatment arms: pure control, high subsidy, and medium-low subsidy villages.[8] Second, I randomly selected 25 farmers from each village using a village census collected primarily for this research. The targeted sample size is 5,500 farmers.[9] Treatment randomization is stratified by: (a) sub-district and (b) village-level intensity of wheat cultivation pre-intervention.[10]

In the first stage of the experiment, treated farmers are offered a standard seed package at a randomly assigned subsidy rate.[11] Subsidy rates are randomized at the village level, and are carefully chosen to reflect a full range of prices for estimating the demand curve. In the high subsidy villages, treated farmers are offered the seed package at either a full subsidy or a 50% subsidy rate.[12] In the medium-subsidy villages, subsidy rates range from 25% to 40%, while in the low-subsidy villages subsidy rates range from 0% to 20%. Demand by each farmer is elicited individually in a take-it-or-leave-it design. Farmers in the pure control villages are surveyed without receiving any intervention.

In the second stage of the experiment, medium- and low- subsidy villages are randomized into stage-two treatment and stage-two control. High-subsidy villages are excluded from stage-two randomization due to high take up at stage one.[13] In stage-two treatment villages, farmers who initially choose not to buy the seed package at stage one receive the seeds for free. Stage-two control villages, on the other hand, do not receive any further intervention at the second stage. The implementation of stage two took place within a few weeks after the completion of stage one. Both stages of the experiment were completed before the beginning of the planting season. The top panel of Table 1 shows the number of villages at each subsidy level for stage-two treatment and stage-two control groups.

The implementation of the two-stage experiment was carefully managed to avoid any contamination that may occur if farmers had prior knowledge of their stage-two treatment status. Each enumerator

[7]A number of studies have applied two-stage experimental designs to analyze treatment effects conditional on willingness-to-pay (e.g., Berry, Fischer, and Guiteras, 2020; Cohen and Dupas, 2010; Karlan and Zinman, 2009). The study that is closest to mine is that of Beaman et al. (2023) that examines farmers' returns to capital conditional on their selection into obtaining credit.

[8]The Districts covered in my sample are: Faridpur (Dhaka Division); Choudanga, Jashore, and Jhenaidah (Khulna Division); Naogaon, Pabna, and Rajshahi (Rajshahi Division).

[9]The sample size was determined based on power calculations targeting a minimum detectable effect of 15 percentage points increase in plot profits for a simple comparison between treatment and control farmers. The target sample was expanded at follow-up to test for spillover effects. In each of the 180 treatment villages, I randomly selected 8 control farmers to construct a sample of 1,440 within-treatment controls. With this additional sample, the total sample size became 6,940 farmers. The size of the within-treatment control sample was constrained by the survey budget.

[10]Intensity of wheat cultivation was calculated using the village census data on the last year the farmer cultivated wheat. I classified villages into high and low wheat intensity based on whether more than 50% of the farmers in the village reported cultivating wheat at least once over the past four years. Appendix Figure A.2 shows the intensity of wheat cultivation across all villages in my sample.

[11]Standard seed packages weighted 15 kg each. The size of the seed package was determined based on findings from a pilot study that the average wheat plot size is 0.30 acres, which requires around 15 kg of wheat seeds. Each sampled farmer was offered only one seed package at the offer price.

[12] Market survey data shows that the official price of *BARI Gom 33* seeds is similar to the price of other wheat seeds in the retail market. The price of the standard seed package is 600 BDT (40 BDT for 15 kgs). For reference, the average daily wage of farmers in the sampled villages is about 500 BDT. I deliberately choose not to offer a subsidy of more than 50% because the pilot study results showed very inelastic demand at higher subsidy rates. Average take-up at subsidies of more than 50% during the pilot was 93%.

[13]Figure A.3 shows that seeds take up at a 50% subsidy rate (offer price = 20 BDT/kg) was around 90%.

team had no knowledge about stage-two treatment until they completed stage-one implementation in the sub-district assigned to them. Stage two took place after all villages within the same sub-district had completed stage one. The message communicated with farmers at stage two was that a surplus in the seeds used for this research study would be freely distributed to a sub-sample of farmers based on a lottery. In the stage-two treatment villages only, farmers who paid a positive price for the seed package in stage one got their money back in stage two. The rationale for this repayment is that fairness required all treated farmers in the same villages to receive an equal treatment. I did not introduce any repayments in stage-two control villages.

The two-stage experimental design allows me to compare the outcomes of farmers who declined to buy the seeds with the outcomes of a random sample of farmers who received the seeds for free. The randomization of free distribution in stage one provides an estimate of the treatment effect of the seeds over the entire population. The randomization of free distribution among non-buyers in stage two provides an estimate of the treatment effect among the self-selected non-buyers. Upon comparing these two estimates, I examine whether the returns of the farmers who declined to buy the seeds are higher or lower than the returns of the average farmer. It is noteworthy that non-buyers in stage-two treatment villages as well as treated farmers in stage-one free-distribution villages both received the seeds for free. This should alleviate any concerns that the difference in outcomes could be driven by the behavioral implications of free distribution, since these implications would apply to both groups.[14]

As illustrated in Beaman et al. (2023), the two-stage randomization also allows for an inference of treatment effects among the farmers in stage-one free-distribution villages who would have bought the seeds had they been offered the seed at a positive price (i.e., the would-be buyers). The marginal distribution of potential profits among farmers in stage-one free-distribution villages can be broken down as follows:

$$F(Q^{seeds}) = P(buy = 1)F(Q^{seeds} \mid buy = 1) + (1 - P(buy = 1))F(Q^{seeds} \mid buy = 0) \tag{1}$$

where Q^{seeds} refers to the potential profits from growing *BARI Gom 33* seeds, and $P(buy = 1)$ is the probability of buying the seeds. Demand elicitation at stage one of the experiment provides an estimate of $P(buy = 1)$.[15] $F(Q^{seeds})$ can be estimated using the randomization of free distribution at stage one (as shown by the solid rectangle in the top-right of Figure 1). $F(Q^{seeds} \mid buy = 0)$ can be estimated using the randomization of free distribution among non-buyers in stage two (as shown by the dashed rectangle in the bottom-left of Figure 1). Together, these estimates allow me to infer:

$$F(Q^{seeds} \mid buy = 1) = \frac{F(Q^{seeds}) - (1 - P(buy = 1))F(Q^{seeds} \mid buy = 0)}{P(buy = 1)} \tag{2}$$

Section 4.2 presents the empirical strategy for estimating treatment effects among non-buyers as well as the would-be buyers.

Furthermore, the randomization of stage-two treatment among villages receiving different subsidy levels in stage one allows me to infer the potential profits of farmers who would have bought the seeds had the subsidy level been increased from the low to medium level (i.e., the would-be buyers at the medium subsidy). This is a novel feature of the two-stage design in my experiment, which addresses

[14] Two potential biases might result from free distribution. The first is the perception of freely distributed goods to be of low quality. This false signal hardly applies to my context since farmers in Bangladesh are acquainted with ad-hoc free distribution of certified seeds by the Department of Agricultural Extension. I use follow-up data on farmers' perceptions of the distributed seeds' quality to test for potential biases in perceptions across treatment arms. A second source of bias is wastage of freely distributed seeds. For example, farmers might exert less effort in planting free seeds. This concern should not affect the interpretation of my results. My empirical test essentially compares the outcomes for farmers who received the seeds for free at stage-one to that of farmers who received the seeds for free in stage two conditional on refusing to buy the seeds at stage-one.

[15] Note that the randomization of the subsidy at stage one implies that $P(buy = 1 \mid subsidy \neq 100\%) = P(buy = 1 \mid subsidy = 100\%) = P(buy = 1)$.

an important policy question: what is the counterfactual of increasing the subsidy level from low to medium?

The marginal distribution of potential profits among non-buyers at the low subsidy level can be presented as a weighted average of the marginal distribution of potential profits of the farmers who do not buy at the medium (as well as the low) subsidy level and farmers who would only buy at the medium (but not at the low) subsidy level:

$$\begin{aligned} F(Q^{seeds} \mid buy_{low} = 0) = P(buy_{med} = 0)F(Q^{seeds} \mid buy_{low} = 0 \ \& \ buy_{med} = 0) \\ +(1 - P(buy_{med} = 0))F(Q^{seeds} \mid buy_{low} = 0 \ \& \ buy_{med} = 1) \end{aligned} \tag{3}$$

where $P(buy_{med} = 0)$ is the probability of not buying the seed at the medium subsidy level. Stage-two randomization at the *low* subsidy level provides an estimate of $F(Q^{seeds} \mid buy_{low} = 0)$. Stage-two randomization at the *medium* subsidy level provides an estimate of $F(Q^{seeds} \mid buy_{low} = 0 \ \& \ buy_{med} = 0)$, since not buying at the medium subsidy also implies not buying at the low subsidy level. Then, I can estimate potential profits among the would-be buyers at the medium subsidy as follows:

$$\begin{aligned} &F(Q^{seeds} \mid buy_{low} = 0 \ \& \ buy_{med} = 1) = \\ &\qquad \frac{F(Q^{seeds} \mid buy_{low} = 0) - P(buy_{med} = 0)F(Q^{seeds} \mid buy_{low} = 0 \ \& \ buy_{med} = 0)}{(1 - P(buy_{med} = 0))} \end{aligned} \tag{4}$$

Section 4.4 presents the empirical strategy for estimating the treatment effects among non-buyers at different subsidy levels.

3 Data and Descriptive Statistics

3.1 Data Collection

Figure 2 summarizes the timeline of project implementation and primary data collection. Village census data provides sampling frame for a random selection of farmers. In addition, the village census includes data on the last year each farmer cultivated wheat. I use this data to stratify treatment by intensity of wheat cultivation (i.e., number of farmers growing wheat pre-intervention) at the village level. A short market survey was collected in parallel with the village census. The market survey includes data on wheat seed varieties sold in the market, the average price of wheat seeds, and the time of the year during which wheat seeds are available in retail markets.

A baseline survey was completed before the two-stage intervention. The baseline data includes a list of demographic characteristics, asset ownership, access to insurance and credit, time preferences, access to agricultural extension services, attitudes towards agricultural risks, stated willingness to pay for an improved wheat seed, and agricultural production in the previous cropping cycle.

Seed distribution survey data was collected during stage one and stage two of seed distribution. In stage one, the seed distribution survey includes each farmer's take-up decision, and the main reasons for buying or refusing to buy the seed. In stage two, the seed distribution survey includes the farmer's take-up decision as well as intended use of the free seed package.

Data on treatment outcomes is drawn from two rounds of follow-up surveys. The first follow-up survey was collected at the end of the harvesting season after the two-stage intervention. The second follow-up survey was collected at the end of the following wheat harvesting seasons. The follow-up surveys cover the sample of 5,500 farmers who form the baseline survey, in addition to a random sample of 1,440 control farmers in treatment villages. The data on control farmers within treatment villages is used for estimating spillover effects. Both follow-up surveys include agricultural input and output data as well as data on crop and seed varieties grown on three main plots for each farmer. If a farmer was cultivating the same farm plots they used to cultivate at baseline, the data for the same plots was collected at follow-up.

The survey data allows me to estimate treatment outcomes at the farm level as well as the plot level. Revenue and profit outcomes, in particular, are estimated at the plot level. This is because the treatment intervention is not expected to have strong effects on farm level revenues or profits. The treatment intervention offers farmers a 15-kg package of wheat seeds at the randomized offer price. The seed package is expected to be sufficient for one average sized plot, as per agronomic recommendations of seeding rates. At baseline, I identify a reference plot for plot-level analysis as follows. First, I collected baseline data on each farmer's three "main plots". The definition of a main plot for the purpose of the baseline survey is a farm plot that is most likely to be used for wheat cultivation. Second, I asked each farmer to rank their three main plots in terms of the plot's suitability for growing wheat. I used the plot that the farmer ranks as the most suitable for wheat cultivation as the reference plot in the plot-level analysis. I follow the same strategy for farmers in treatment and control villages. Indeed, follow-up data shows that plots ranked as most suitable for wheat cultivation at baseline are 10 percentage points more likely to be used for wheat cultivation at follow-up compared to the rest of the plots in the sample.

3.2 Descriptive Statistics and Characteristics of Seed Buyers

Appendix Table A.1 presents summary statistics and verifies randomization balance for most of the baseline variables. I check for balance across treatment arms for both stage-one and stage-two randomization. The sample is balanced across treatment arms with very few exceptions. For example, farmers in the high subsidy villages are more likely to grow Boro rice (a direct substitute for growing wheat or other dry season crops) at baseline relative to farmers in the pure control villages. I deal with unintended imbalances in two ways. First, I control for the unbalanced baseline characteristics in all of the regressions. Second, I follow the post-double LASSO approach (which is the approach I specified in the pre-analysis plan) for selecting baseline controls, while making sure that the unbalanced variables are included in the list of controls. The main results stay the same whether I control for unbalanced characteristics, or I control for LASSO selected controls, or both. In the results section, I show the results with and without controlling for LASSO selected controls.

Table 2 presents a comparison between the characteristics of seed buyers and non-buyers in the medium-subsidy and low-subsidy villages separately. A common observation in both groups of villages is that seed buyers are more likely to have access to non-farm sources of income and are cultivating relatively bigger farms, as measured by farm area and the number of plots cultivated pre-intervention. These descriptive results suggest that buyers may be more open to experimenting with a new seed variety for two reasons. First, access to non-farm income may provide opportunities for hedging risks from farm income. Second, cultivating large farms or more farm plots could mean that experimenting with a new seed variety on one plot represents a smaller share of the farmer's total farm income and, hence, a relatively smaller risk compared to the case of farmers cultivating a smaller number of plots. At the same time, farmers' degree of risk aversion (measured by baseline survey module on attitudes toward risk) does not differ significantly between buyers and non-buyers.

It is notable that some characteristics that should reveal farmers' preferences for growing wheat do not distinguish buyers from non-buyers. For example, buyers are *not* significantly more likely to grow wheat at baseline compared to non-buyers. Also, unexpectedly, non-buyers stated a relatively higher willingness to pay (WTP) for an improved wheat seed at baseline than seed buyers. However, for all farmers, the stated WTP is substantially lower than the official price of 40 BDT/kg.

4 Results: Demand and Actual Returns to Adoption

This section presents my four main results. First, the randomization of the subsidy rate shows that farmers are highly responsive to a price subsidy. Second, the randomization of free distribution among non-buyers shows that non-buyers are willing to adopt the new seed upon receiving it for free. I find that the non-buyers' response to a full subsidy in stage two is on par with the average farmer's response to a full subsidy in stage one. Third, the realized returns of non-buyers are *not* significantly different from the realized returns of farmers in stage-one free-distribution villages. Fourth, a distinction between self-selection at the medium versus low subsidy levels reveals that non-buyers who select out of buying at the low subsidy have higher returns than the average farmer. I show that the counterfactual of increasing subsidies from a low to a medium subsidy level is expected to induce take up by farmers with higher than average returns. Taken together, these results imply that agricultural input subsidies do not induce take-up by lower return farmers.

4.1 Demand is Highly Sensitive to Prices

I find two descriptive facts about the demand for *BARI Gom 33* seed. First, demand is highly sensitive to prices. Second, conditional on buying the improved seed, the likelihood of planting the seed does not depend on the price paid. This lack of correlation between price and usage provides first suggestive evidence that the seed price does not sort farmers based on the likelihood of planting the seeds.

Demand for the new seed is highly sensitive to the subsidy rate. The inverse demand curve in Panel A of Figure 3 shows the share of treated farmers who took up *BARI Gom-33* (i.e., paid the randomized offer price) at the high (50 or 100%), medium (25, 30, or 40%), and low (0, 10, or 20%) subsidy levels. In the high-subsidy villages, 94% of treated farmers took up the seeds. Demand drops sharply as the subsidy rate decreases, such that the share of treated farmers buying the seeds at the low-subsidy level is merely 6%. Appendix Figure A.3 shows a detailed version of the inverse demand curve with the full set of prices.

The likelihood of a purchasing farmer planting the seeds on their farm does *not* vary significantly by the subsidy rate (in addition to planting the seeds, farmers may eat the wheat seeds or pass it to other farmers as explained below).[16] If prices sort farmers based on the likelihood of planting the seeds, then farmers paying a higher price would be more likely to plant the seeds. However, Figure 3 Panel B shows that the planting rate curve is relatively flat across all subsidy levels. If anything, farmers who bought at higher prices are slightly less likely to plant the purchased seeds. The average planting rate among buyers in low-subsidy villages is 41% compared to an average planting rate of 49% for the buyers in the medium- and low-subsidy villages. This result provides suggestive evidence that prices do *not* sort farmers based the likelihood of planting the improved seed.

It should be noted that the treatment intervention did not prevent farmers from using the subsidized seeds as they choose. The common uses of the improved wheat seeds are: planting the seeds, passing the seeds to another farmer, or using the wheat seeds for food. Appendix Table A.2 shows that the share of farmers passing the subsidized seeds to fellow farmers is around 13%.[17] The majority of farmers either planted the seeds or used wheat seeds as food. One explanation for the low seed-planting rate is that many farmers in the study area have switched away from growing wheat due to several factors including crop diseases. Among the treated farmers who did not plant the distributed seeds,

[16] By purchasing farmer (or seed buyer) I refer to treated farmers who took up the seeds at stage one of the experiment. This term is inclusive of treated farmers in free-distribution villages who took up the seeds for free in stage one, but exclusive of non-buyers in stage-two treatment villages that received the seeds for free in stage two.

[17] The low likelihood of a treated farmer passing the seed to fellow farmers is consistent with the weak spillover effects found in Appendix D. Trading frictions in secondary markets may explain this finding Emerick (2018).

only 3% grew any wheat on their farms during the same season.

Appendix Table A.2 shows that the likelihood of a treated farmer planting the distributed seeds does not vary across farmers who took up the seeds at different prices and across different treatment arms. This result holds for the other uses of the seeds. The only exception is that farmers who bought the seeds in the stage-two treatment villages are more likely to pass the seeds to other farmers and less likely to use the seeds for food. This finding suggests that stage-two treatment might have created slightly higher demand for the distributed seeds in a secondary market.[18]

To sum up, an elastic demand curve shows that farmers do respond to lower prices by increasing take-up. At the same time, a flat usage rate suggests that increasing the price does not increase the likelihood that seed buyers plant the seed on their farms. Also, redistribution of the subsidized seeds by treated farmers is quite limited. In the following sections, I first analyze treatment effects on adopting the improved seed and increasing wheat cultivation. I then move to examining whether the returns of the farmers who select out of buying the improved seeds are any different from the returns of the average farmer.

4.2 Non-Buyers Increase Adoption Upon Receiving a Full Subsidy

How does free distribution to self-selected non-buyers affect actual adoption of the improved seed? To answer this question, I use as a benchmark the free-distribution treatment arm in stage one randomization. Relative to this benchmark, it is not obvious whether free distribution to self-selected non-buyers would result in higher, lower, or similar increase in adoption. If farmers choose not to buy due to low expected returns, then free distribution to non-buyers might not increase adoption. However, if farmers choose not to buy due to market frictions or adoption constraints, then the net effect of free distribution among non-buyers would depend on whether a full subsidy alleviates adoption constraints. For example, some adoption constraints, such as information constraints, may persist even with a full subsidy. I examine the treatment effects on adoption as well as the extensive and intensive margins of wheat cultivation.

I start by presenting results on the intent-to-treat effects using the following specification:

$$Y_{ijs} = \beta_1 Free_{js} + \beta_2 Subsidy^{50}_{js} + \beta_3 StageTwo^{Treat}_{ks} + \beta_4 StageTwo^{Control}_{js} + \delta X_{ijs} + \alpha_s + \epsilon_{ijs} \quad (5)$$

where Y_{ijs} represents the outcome of interest (e.g., adoption, wheat cultivation) for farmer i in village j and strata s. $Free_{js}$ represents the villages that received the seeds for free in stage one. $Subsidy^{50}_{js}$ are villages that received 50% subsidy in stage one and are excluded from stage-two randomization. $StageTwo^{Treat}_{js}$ pools all the stage-two treatment villages that received either a medium or low subsidy in stage-one (recall that stage-two randomization was among both the medium- and the low-subsidy villages). Similarly, $StageTwo^{Control}_{js}$ represents the pool of stage-two control villages that received either a medium or a low subsidy in stage one. The pure control villages represent the omitted category. I test for $\beta_1 = \beta_3$ to examine whether the free distribution of seeds in stage two, which followed an incentivized demand elicitation in stage one, has a substantially different treatment effects relative to the free distribution in stage one.[19] X_{ijs} is an optional vector of baseline characteristics, including

[18] Table A.3 shows that stage-one buyers in stage-two treatment villages are more likely to perceive the yield of distributed seeds as relatively higher than the yield of existing varieties, compared to the seed buyers in the high-subsidy villages. This result may explain why stage-one buyers in stage-two treatment villages are more likely to pass the distributed seeds to other farmers than use the seeds for food.

[19] Two factors might explain a potential difference in treatment effects between a free-distribution treatment at stage one versus stage two. First, stage-one buyers in stage-two treatment villages are re-paid funds initially budgeted for seeds and, hence, might spend that money on buying more inputs (i.e., a re-budgeting effect). Second, the potential for a secondary market for the distributed seeds is relatively higher in stage-two treatment villages due to the repayment of

baseline value of the outcome variable.[20] The term α_s represents strata fixed effects and ϵ_{ijs} is a random error term.[21] Standard errors are clustered at the village level in all regressions.

Next, I exploit the two-stage experimental design to compare the outcomes of self-selected non-buyers with the outcomes of the farmers who received the seeds for free in stage one. With reference to stage-two randomization, I distinguish between non-buyers in stage-two treatment villages (i.e., farmers who received the seed for free in stage two after choosing not to buy the seeds in stage one) and non-buyers in stage-two control villages (i.e., farmers who declined to buy the seeds in stage one and did not receive any further intervention in stage two). The regression specification is:

$$\begin{aligned} Y_{ijs} = \gamma_1 Free_{js} + \gamma_2 StageOne_{ijs}^{NonBuyer} * StageTwo_{js}^{Treat} \\ + \gamma_3 StageOne_{ijs}^{NonBuyer} * StageTwo_{js}^{Control} + \delta X_{ijs} + \alpha_s + \epsilon_{ijs} \end{aligned} \tag{6}$$

For this specification, I use data from the pure control villages (omitted category), stage-one free-distribution villages ($Free_{js}$), non-buyers in stage-two treatment villages ($StageOne_{ijs}^{NonBuyer} * StageTwo_{js}^{Treat}$), and non-buyers in stage-two control villages ($StageOne_{ijs}^{NonBuyer} * StageTwo_{js}^{Control}$). I use inverse probability weights to account for the non-random selection of farmers into the sub-sample of non-buyers.[22] The main results stay the same when weights are not included.

I use the specification in Equation 6 to test for a selection effect of prices as follows. The coefficient γ_1 gives the treatment effects of free distribution relative to the pure control group (the two sub-samples highlighted by the solid rectangle in Figure 1). The difference between γ_2 and γ_3 shows the treatment effect amongst the self-selected sample of non-buyers (the two sub-samples highlighted by the dashed rectangle in Figure 1). Thus, a test for $\gamma_1 = \gamma_2 - \gamma_3$ indicates whether self-selected non-buyers realized higher, lower, or similar outcomes relative to the average farmer.

In addition, I use the two-stage experimental design to infer treatment effects among farmers in stage-one free-distribution villages who would have bought the seeds had they been asked to pay a positive price corresponding to a medium-low subsidy level. As explained in Section 2.2 (Equation 2), I can infer treatment effects among the would-be buyers by calculating $(\frac{\gamma_1 - (0.82)*(\gamma_2 - \gamma_3)}{0.18})$, where 0.18 is the probability of purchasing the seeds at stage one in the medium-low subsidy villages. For completeness, I also extend the specification in Equation 6 to include self-selected buyers in stage-two treatment and stage-two control villages.[23] However, I note that the realized outcomes of self-selected buyers do not isolate the treatment effects of *BARI Gom 33* seeds from selection effects.

the seed price to farmers who bought the seeds at stage-one and free distribution to farmers who choose not to buy the seeds at stage one.

[20] The choice of the control variables follows the "post-double-selection" method proposed by Belloni, Chernozhukov, and Hansen (2014), while forcing the strata fixed effects to be included as controls in each regression. As a robustness check, I tried including the following as unpenalized controls in post-double-lasso: (1) baseline values of the outcome variable; and/or (2) baseline covariates that did not balance across treatment arms. In all cases, the main results are qualitatively similar with and without including the selected control variables.

[21] As explained in Section 2.2, I stratify treatment by sub-districts as well as village-level wheat cultivation intensity, where wheat intensity is measured by an indicator of whether more than 50% of the farmers cultivated wheat at least once over the past four years prior to the intervention. I end up with 18 strata for the 12 sub-districts in my sample. This is because three of the 12 sub-districts did not have enough variation in the indicator for wheat intensity and had to be merged with other sub-districts. Appendix Figure A.2 shows the density of my measure of wheat cultivation intensity for the entire sample.

[22] The weight for a stage-one non-buyer is equal to (# of sampled farmers in the village)/(# of non-buyers in that specific village at stage one). For farmers in the free-distribution and control villages, the probability weight is equal to one. These probability weights ensure that stage-two treatment and control villages with different proportions of non-purchasing farmers are equally represented. As shown in the second panel of Table 1, without correcting for non-random sampling my sample would over represent non-buyers from low-subsidy villages and under represent non-buyers from medium-subsidy villages.

[23] This extension was not included in the pre-analysis plan. However, it does not diverge dramatically from the main specification in Equation 6. The probability weights for a stage-one buyer is equal to (# of sampled farmers in the village)/(# of buyers in that specific village at stage one).

Table 3 shows how the intervention increases adoption across all treatment arms relative to the pure control group.[24] On average, 2% of the farmers in control villages adopted the improved seed variety.[25] Column (1) shows that this share significantly increased by 41, 36, and 7 percentage points in the free-distribution, stage-two treatment, and stage-two control villages, respectively. Column (2) breaks down stage-two treatment and stage-two control villages into self-selected samples of stage-one buyers and non-buyers. I find that stage-two treatment is effective in increasing adoption among non-buyers by 32 percentage points (which corresponds to $\gamma_2 - \gamma_3$ in Equation 6). The difference between the treatment effects of stage-one free distribution (41-percentage point increase in adoption) versus stage-two free distribution (32-percentage point increase in adoption) is marginally significant. The p-value for the test $\gamma_1 = \gamma_2 - \gamma_3$ is 0.07.

The significant treatment effect on adoption by non-buyers is notable. If farmers decide not to buy the seeds in stage one due to low expected returns, then we should not see an effect of stage-two treatment on adoption by non-buyers; particularly, since the time gap between stage one and stage two interventions in each village is less than two weeks. This result suggest that switching from a modest subsidy in stage one to a full subsidy in stage two allocates the seed to farmers who are interested in planting the seed upon receiving it for free.

The results on adoption by self-selected buyers suggest that seed buyers are not necessarily the farmers who are most likely to adopt the seeds. For the self-selected buyers, buying the seeds is associated with an increase in adoption of 39 and 32 percentage points in stage-two treatment and stage-two control villages, respectively. In contrast, the implied effect on adoption among farmers who would have bought the seeds upon receiving a modest subsidy (i.e., the estimated treatment effect among *would-be* buyers using Equation 2) is an increase in adoption of 85 percentage points. These findings suggest that self-selection into buying the seeds biases downwards the results on adoption by self-selected buyers. This is not surprising given the results on seed usage. As shown in Section 4.1, among seed buyers, the average rate of seed adoption is less than 50 percent.

The intervention caused an increase in wheat cultivation at both the extensive and intensive margins.[26] Columns (3) and (5) of Table 3 show significant positive effects on the likelihood of growing wheat and the farm-level share of area devoted to wheat, particularly in the free-distribution, 50% subsidy, and stage-two treatment villages. Columns (4) and (6) show how stage-two treatment effects on wheat cultivation among non-buyers is not significantly different from the treatment effects among treated farmers in stage-one free-distribution villages.

The point estimates on the share of wheat area, columns (5) and (6) of Table 3, highlight the extent to which farmers in the sample allocate a relatively small share of their farms to wheat. On average, farmers in control villages devote 6% of their farm to wheat. The treatment intervention increased this share by at most 9 percentage points in the free-distribution villages. In fact, I did not expect the intervention, which offered treated farmers one seed package of 15 kg, to have strong farm-level effects. Instead, before introducing the intervention, I collected baseline data that allows me to compare plot-level outcomes across treatment and control farmers, as explained in Section 3. I use baseline data on the farmers' ranking of their plots suitability for growing wheat to estimate plot-level data for the plot that the farmers rank as the most suitable for growing wheat. I follow the same strategy for farmers in treatment and control villages.

Plot-level results on growing wheat, columns (7) and (8) of Table 3, lead to similar conclusions

[24]Appendix Table A.4 replicates the regressions in Table 3 with the inclusion of LASSO selected controls. The results are similar after the inclusion of these controls.

[25]This can be explained by the low supply of a new seed variety that is still in early stages of dissemination.

[26]Appendix Table A.5 shows that the treatment has partially crowded out other wheat seeds. However, the positive effect on the extensive margin of wheat cultivation is stronger than the negative effect on planting other wheat seeds by treated farmers.

as the farm-level results in columns (3) and (4).[27] Treatment effects on growing wheat are positive and significant. The effect of stage-two free distribution on wheat cultivation by non-buyers is *not* statistically different from the effect for farmers in stage-one free-distribution villages. Appendix Table A.6 and Table A.7 confirm that the plot ranked by farmers pre-intervention as the most suitable for wheat has experienced the strongest effect on adoption and wheat cultivation across all treatment arms.[28]

In summary, the results show that farmers who select out of buying the seed when offered a modest subsidy in stage one are responsive to a subsequent offer of a full subsidy in stage two. Indeed, the non-buyers' response to a full subsidy in stage two is on par with the average farmer's response to a full subsidy in stage one. This is true whether we are looking at farmers' response in terms of adopting the improved seed or in terms of increasing wheat cultivation. I investigate potential mechanisms for explaining this finding in Section 5. In the next section, I focus on examining treatment effects on revenues and profits.

4.3 The Realized Returns of Non-Buyers Are Similar to the Returns of the Average Farmer

After showing that the two-stage intervention significantly increases adoption and wheat cultivation even by non-buyers, the next step is to investigate whether non-buyers have different returns from those of the average farmer. The intuition is that relatively *lower returns* by non-buyers would suggest *positive selection* out of buying the improved seed (i.e., non-buyers select out of buying due to their low returns). On the contrary, relatively *higher returns* by non-buyers would suggest *negative selection* (i.e., non-buyers select out of buying the seeds in spite of their high returns). I measure farmers' returns using follow-up data on plot-level revenues and profits.[29] I find that the improved wheat seed caused farmers to substitute away from more profitable crops, which results in low treatment effects on revenues and profits across all treatment arms. Yet, importantly, the returns of self-selected non-buyers are similar to those of farmers in stage-one free-distribution villages.

The increased adoption of the improved wheat seed does not increase revenues or profits. Table 4 shows that stage-one free distribution results in lower revenues relative to the pure control group. The

[27]The data allows me to measure farm-level outcomes in two ways. First, I collected detailed data on crop cultivation for up to three main plots for each farmer. I define farm-level outcomes as the aggregate of the outcomes of these *three main plots*. Second, I included at the end of the follow-up surveys explicit questions on adoption, wheat cultivation, and total wheat area across *all plots* grown by the farmer during the last season. Each of these measures has its pros and cons. The measure using aggregated plot-level data is less prone to over-reporting, particularly by treatment farmers, and is my preferred measure. On the other hand, the measure using explicit farm-level questions is less prone to underestimation of farm-level effects given that the average farmer in the sample cultivated 5 or more plots. In the paper, I present farm-level results using the first measure. Results using the second measure of farm-level outcomes show similar results and are available upon request.

[28]One drawback of restricting the sample to the top-ranked wheat plot is the loss of observations since attrition at the plot-level was slightly higher than attrition at the farmer-level (i.e., some farmers are no longer cultivating the same plot they used to cultivate at baseline). Overall, the attrition at the first follow-up was trivial: 3% attrition at the farmer level and around 8% at the plot level.

[29]As explained in Section 4.2, plot-level data is restricted to the plot ranked by farmers at baseline as the "most suitable" plot for wheat cultivation. Revenues are measured as the output per unit area (i.e., yield) multiplied by the farm-gate price of output as reported by farmers. Profits are measured as total revenues net of all input costs. Both revenues and costs are measured per unit area. I include the full cost of seeds for treated farmers who received subsidized seeds. Also, I include the opportunity cost of family labor in the profit calculation. I follow the rule of thumb given by Agness et al. (2022) in valuing family labor at 60% of the average market wage. The unit of measurement for revenues and profits in levels is Bangaldeshi Takas (BDT) per acre. The results are qualitatively similar whether revenues and profits are measured in levels or logs. However, the results in logs should be interpreted with caution as they exclude plots that had non-positive profits at follow-up. Table A.11 presents results on plot profits in levels as well as the likelihood of negative profits.

impact on profits is not statistically different from zero. The confidence interval on the coefficient of stage-one free distribution villages rules out large positive effects on profits. A potential explanation for the negative treatment effects on revenues is that the intervention did replaced not only inferior wheat seeds, but also other crops that are more lucrative than wheat. Indeed, Table A.8 shows that the treatment causes farmers to substitute common dry-season crops with wheat.[30] The summary statistics in Table A.10 shows that, although wheat revenues and profits increased at follow-up relative to baseline, wheat is less profitable than common dry-season crops in the sample.[31]

Results from the wheat plots in the sample alone suggest that *BARI Gom 33* seeds have a yield advantage over existing wheat varieties. Table A.12 shows that, among the wheat plots grown by the same farmer, the yield of plots growing *BARI Gom 33* seeds is 10 percent higher than that of plots growing other wheat varieties. This result is in line with the findings of agronomic trials. Another important feature of *BARI Gom 33* seeds is that they represent an insurance device against wheat blast, as explained in Section 2.1. Since there was not a blast outbreak in the sampled villages post intervention, the insurance feature of the improved seed did not pay out.[32]

Despite the noisy effects on profits, self-selection results in columns (3) and (7) of Table 4 provide no evidence that prices have a selection effect for the entire sample of non-buyers. Stage-one free distribution resulted in a decrease in revenues and profits of 12 and 11 percent, respectively. Similarly, stage-two free distribution to self-selected non-buyers resulted in a net decrease in revenues and profits of 10 percent. Adding a list of LASSO selected controls to the main specification – columns (4) and (8) – slightly improves precision, but does not change the main finding.[33] That is, the treatment effect on revenues and profits in the free-distribution villages is not statistically distinguishable from the net effect of free distribution among non-buyers. Appendix Table A.11 shows that this conclusion also holds when measuring plot profits in levels, rather than logs.[34] Figure 4 summarizes the self-selection results on plot revenues and plot profits. Overall, for the entire sample of non-buyers, the shift from moderate price subsidies in stage one to a full subsidy in stage two does not distort allocation to lower return farmers.

Given the significant effects on adoption and wheat cultivation, the negative treatment effects on revenues and profits raises the question of what motivates farmers to grow wheat in the first place. One way to answer this question is to mention a few limitations of the cross-sectional results on realized returns. First, plot-level outcomes do not fully capture potential farm-level considerations such as choosing to grow a relatively low-profit crop on one plot for the sake of risk diversification.[35] The results on plot-level returns do no account for risk-return tradeoffs. Second, farmers in my sample report growing staple crops for their own consumption as one of the primary motives affecting their crop choice. For instance, growing wheat on a farm plot is associated with a 15 percent increase

[30] Table A.8 shows results on the plot-level substitution between wheat and other crops. I choose to show plot-level results here to be consistent with the plot-level results on profits and revenues. Farm-level outcomes show similar treatment effects on the substitution between wheat and other crops.

[31] The data on profits and revenues at baseline should be interpreted with caution due to the long recall period between the (pre-planting) baseline survey and the reported outcomes for the previous year's dry season's harvest.

[32] Indeed, at the research design stage, blast outbreak was a likely event for the sampled sub-districts. In the pre-analysis plan (Mahmoud, 2022), power calculations accounted for the possibility of a blast outbreak. The estimated minimum detectable effects (MDEs) for the test on selection effects were in the range of 0.2 - 0.3 standard deviations. These MDEs are reasonable in the event of a blast outbreak such that the difference between the profits of farmers in the treatment and control groups would also account for difference in blast-related losses.

[33] Given that adding controls does not significantly reduce the standard errors, this suggests that there are wide variations even within farmers of the same group (i.e., within-group uncertainty matters).

[34] The results on plot profits in logs should be interpreted with caution. When taking logs, the results show the treatment effects conditional on making positive profits. If anything, column (6) of Table A.11 show that the likelihood of making non-positive profits is higher among non-buyers in stage-two treatment villages.

[35] As shown in Table A.1, the average farmer in the sample grew 5 plots at baseline. The survey data allows me to calculate revenues for three main plots only. Results on total revenues for the three main plots show that treatment effects on total revenues are not statistically different from zero. These results are available upon request.

in the share of the harvest used for the farmer's own consumption. This finding may reflect some market imperfections that prevent specialization and trade, which can also prevent separation between production and consumption decisions. Third, the one-year results on farmers returns are noisy, which suggests that actual returns provide noisy signals for estimating expected returns. Farmers may take time to learn about their expected returns from a new technology. That is why I collected a second-round of follow-up data, as presented in Section 4.5.

The key takeaway, so far, is that the returns of self-selected non-buyers are similar to the returns of the average farmer. A caveat, though, is the relatively wide confidence intervals on the test for selection effects (i.e., the test for $\gamma_1 = \gamma_2 - \gamma_3$ in Table 4). A plausible interpretation of the wide confidence intervals is that actual returns are uncertain, which makes it hard for farmers to estimate their returns ex-ante. If farmers can hardly estimate their expected returns at the time of sale, then price subsidies are unlikely to crowd in lower-return farmers.

4.4 A Medium Subsidy Level Induces Take-Up by Farmers with Higher Returns

The difference in demand across different subsidy levels, as shown in panel A of Figure 3, suggests that selection effects of price subsidies may different by the subsidy level. I find that stage-two treatment effects on adoption are similar across both the medium and low subsidy levels. However, the results on revenues and profits suggest that non-buyers at the medium subsidy level have lower returns compared to non-buyers at the low-subsidy level. I use these results to infer the potential profits among farmers who would have bought the seeds upon increasing the subsidy rate from a low to a medium subsidy level. I find that *would-be* buyers at the medium subsidy level have large and positive treatment effects on profits.

To test for selection effects at different subsidy levels, I extend the specification in Equation 6 as follows:

$$\begin{aligned} Y_{ijs} = \lambda_1 Free_{js} + \lambda_2 Subsidy_{js}^{Medium} * StageOne_{ijs}^{NonBuyer} * StageTwo_{js}^{Treat} \\ +\lambda_3 Subsidy_{js}^{Medium} * StageOne_{ijs}^{NonBuyer} * StageTwo_{js}^{Control} \\ +\lambda_4 Subsidy_{js}^{Low} * StageOne_{ijs}^{NonBuyer} * StageTwo_{js}^{Treat} \\ +\lambda_5 Subsidy_{js}^{Low} * StageOne_{ijs}^{NonBuyer} * StageTwo_{js}^{Control} \\ +\delta X_{ijs} + \alpha_s + \epsilon_{ijs} \end{aligned} \tag{7}$$

where, $Subsidy_{js}^{Medium}$, and $Subsidy_{js}^{Low}$ are indicators for whether the village received a medium or a low subsidy rate at stage one. Recall that medium subsidy rates are in the range of 25-40% and low subsidy rates are in the range of 0-20%. The rest of the variables are as defined above. A test for $\lambda_1 = \lambda_2 - \lambda_3$ evaluates the difference between the treatment effects in stage-one free-distribution villages and the net treatment effects among the self-selected non-buyers who received a medium subsidy level in stage-one. A test for $\lambda_1 = \lambda_4 - \lambda_5$ represents a similar comparison but for the self-selected non-buyers who received a low subsidy level in stage-one. I also test for $\lambda_2 - \lambda_3 = \lambda_4 - \lambda_5$ to compare the outcomes of non-buyers who selected out of buying at the medium-subsidy level versus at the low-subsidy level.

As explained in Section 2.2, I also infer the treatment effects among farmers in the low-subsidy villages who would have bought the seeds upon increasing the subsidy rate to a medium subsidy level. By noting that non-buyers in the medium-subsidy villages would also not buy the seeds at any lower subsidy level, the treatment effects among non-buyers in the low-subsidy villages can be considered a weighted average of the treatment effects among non-buyers at the medium subsidy level and the treatment effects among the *would-be* buyers at the medium subsidy level. Thus, using regression results from Equation 7, treatment effects among the *would-be* buyers at the medium subsidy level is calculated as : $\frac{(\lambda_4-\lambda_5)-(0.67)*(\lambda_2-\lambda_3)}{0.33}$, where 0.33 is the probability of buying the seeds at stage one in the medium-subsidy villages.

To compare treatment effects on adoption, I examine the farm-level outcome of planting *BARI Gom 33* seeds. Columns (1) and (2) of Table 5 show that treatment effects on adoption by non-buyers are very similar for non-buyers in the medium-subsidy and low-subsidy villages. For both groups of non-buyers, the net effect of stage-two treatment is an increase in the likelihood of adopting the improved seed by 31 percentage points, as shown in column (2). The difference between the treatment effects of stage-one free-distribution and the effect of stage-two free-distribution to non-buyers is at most 11 percentage points for both sub-groups of non-buyers. Hence, results on adoption do not provide evidence of differential selection effects at different subsidy levels.

I turn to plot-level outcomes to analyze treatment effects on plot revenues and profits. Given that the summary statistics in Table A.10 show noticeable differences in the profitability of different crops, I start by showing plot-level results on the treatment effects on growing wheat on the reference plot. Columns (3) and (4) of Table 5 show that stage-two treatment effect on the likelihood of growing wheat is higher among non-buyers in the low-subsidy villages relative to non-buyers in the medium-subsidy villages. However, the test for the difference between stage-two treatment effects on non-buyers in the medium-subsidy versus the low-subsidy villages is not statistically significant (p-value of 0.27 in column (4)). Appendix Table A.9 shows results on the substitution between growing wheat and growing common dry-season crops on the reference plot. These results reveal that, relative to the average farmer, non-buyers in the low-subsidy villages are more likely to substitute Boro rice with wheat, and are less likely to substitute onion with wheat as a result of stage-two treatment. The differential treatment effects on crop substitution may contribute to the difference in treatment effects on revenues and profits.

The results on revenues and profits show that the realized returns of non-buyers in the medium-subsidy villages are lower than the realized returns of non-buyers in the low-subsidy villages. The results are presented in columns (5) to (8) of Table 5 and summarized in Figure 5. As a benchmark, columns (6) and (8) show that stage-one free distribution resulted in a decrease in revenues and profits by 13 and 12 percent, respectively. For the non-buyers in the medium subsidy villages, the net effect of stage-two free distribution (i.e., $\lambda_2 - \lambda_3$ in Equation 7) is a decrease in revenues and profits by 18 and 25 percent, respectively. In contrast, for the non-buyers in the low-subsidy villages, the net effect on revenues and profits is not statistically different from zero; point estimates are negative 5 and positive 7 percent for revenues and profits, respectively. The difference between the net effects on the revenues and profits of non-buyers in the medium-subsidy versus the low-subsidy villages (i.e., the test for $\lambda_2 - \lambda_3 = \lambda_4 - \lambda_5$) is marginally significant.

Table 6 complements the analysis on the differential treatment effects on plot profits by showing the results on profits in levels as well as the likelihood of making non-positive profits. Although measuring outcome variables in logs (as in Table 5) facilitates the interpretation of the point estimates, log measurements by construction exclude from the analysis plots with non-positive profits, which represent around 8% of the sampled plots at follow-up. Column (3) of Table 6 suggests that non-buyers in the medium-subsidy villages are more likely to have non-positive plot profits. The results using plot profits in levels reinforce the conclusion that non-buyers in the medium-subsidy villages have significantly lower treatment effects on profits than non-buyers in the low-subsidy villages. Moreover, column (2) of Table 6 shows that the realized profits of non-buyers in the low-subsidy villages are higher than the realized profits of the average farmer. The test for $\lambda_1 = \lambda_4 - \lambda_5$ in Equation 7 is marginally significant at the 10 percent level. The results on plot profits suggest that there is a negative selection out of buying the improved seed at the low subsidy level.

The implied treatment effects among *would-be* buyers at the medium subsidy level suggest that the low subsidy level crowds out farmers with both a high likelihood of adoption and higher than average returns to adoption. Column (2) of Table 5 shows that, for the farmers who would have bought the seeds upon increasing the subsidy to a medium level, the inferred treatment effect on adoption is a significant increase of 30 percentage points. The results on plot profits in Table 5 and Table 6 show that the inferred treatment effects on the profits of *would-be* buyers at the medium subsidy level are large and positive. Therefore, moving from a low subsidy to a medium subsidy induces farmers with relatively high returns to buy the seeds. The medium subsidy level is good enough to screen out farmers with lower returns and induce take-up by farmers with higher returns. This is another piece of evidence that an increase in the agricultural input subsidy does not distort allocation to low-return farmers.

To summarize, the results on demand at stage one of the experiment show that demand decreases dramatically at the low subsidy level. The results on adoption show that non-buyers in the low-subsidy villages are as responsive to stage-two free distribution as non-buyers in the medium-subsidy villages. At the same time, plot-level outcomes show that the realized returns of non-buyers in the low-subsidy villages are relatively high. I interpret the results on realized returns by showing that the counterfactual of switching from a low subsidy level to a medium subsidy level would induce take up by farmers with higher than average returns. Therefore, increasing subsidies does not distort allocation to low-return farmers.

4.5 One Year Later... Partially Persistent Treatment Effects on Adoption

The purpose of following the same farmers for a subsequent wheat season is to observe farmers' outcomes after removal of the subsidy. Farmers may take time to learn about their expected returns. This makes it critical to examine whether farmers continue to adopt or dis-adopt the improved wheat seed in the absence of follow-up interventions. I find that similar treatment effects on adoption between stage-one free-distribution treatment and stage-two free-distribution treatment persist for a consecutive year. However, dis-adoption rates in year 2 are substantial.

Table 7 shows adoption effects one year after the intervention. I present treatment effects on adoption in year 2 in general. Then, I distinguish between persistent adopters (i.e., farmers who adopt the seeds in year 1 and year 2), new adopters in year 2, and dis-adopters in year 2. Column (1) shows that the treatment effect of stage-one free distribution on adoption in year 2 is similar to the treatment effect of stage-two free distribution among non-buyers. In year 2, the adoption rate by farmers in the pure control villages is 9%. Stage-one free distribution increases adoption rate by 7 percentage points, while stage-two free distribution to non-buyers increases adoption rate by 9 percentage points. The difference between the two point estimates (i.e., the test for $\gamma_1 = \gamma_2 - \gamma_3$) is not statistically significant. Stage-two treatment continues to have a positive and significant effect on adoption in the subsequent wheat season.

One channel through which treatment effects on adoption may last for a succeeding season is through seed multiplication by treated farmers. That is, treated farmers who adopted the seed in the first year can store part of their harvest and use it as seeds in the following year. Indeed, column (2) shows significant effects on persistent adoption for a second year that is similar among stage-one free-distribution treatment and stage-two free-distribution treatment. Nevertheless, the likelihood of persistent adoption in year 2 is low compared to the treatment effects on adoption in year 1. This is due to the high disadoption rates, as shown in column (4). At the same time, treatment effects on new adoption in year 2 are low and even negative in stage-one free-distribution villages, as shown in column (3).

Year 2 results show that the share of non-buyers who are persistent in adopting the improved seed for two years is similar to the share of persistent adopters in the entire population. The high disadoption rates in year 2 are expected given the results on realized profits shown in Section 4.3. One of the main benefits of adopting the improved seed is to prevent losses from contagious crop disease such as wheat blast. The fact that there was not a blast outbreak during the study period means that farmers did not get a chance to learn about the blast-resistant feature of the seed.

5 Mechanisms

Several mechanisms could explain the finding that non-buyers do not realize lower returns to adoption compared to the average farmer. First, factors associated with market frictions, such as credit limitations and uninsured risks, might explain why self-selected non-buyers can realize returns comparable to or even higher than those of the average farmer. Second, agricultural returns are uncertain. There is a chance that non-buyers underestimated their expected returns when they made their purchase decisions. The free-distribution treatment in stage two may cause non-buyers to upgrade their expected returns (i.e., a flypaper effect of stage-two free distribution).

In this section, I focus on the first potential mechanism: the possibility that market frictions, particularly credit or risk market failures, may explain the low WTP revealed by non-buyers. To empirically test for this hypothesis, I use machine learning methods to analyze heterogeneity in farmers' response to the free-distribution treatment conditional on a high-dimensional set of baseline observations. I find that credit constrained non-buyers are more responsive to stage-two free-distribution. I show that this finding is consistent with a model on agricultural investments in the presence of credit market failures. In Appendix B, I show that the empirical results do not support the hypothesis that stage-two treatment cause farmers to upgrade their expected returns.

5.1 Evidence on Heterogeneity in Treatment Effects

I test for heterogeneity in treatment effects based on observable characteristics, using machine learning (ML) techniques. I find strong evidence of heterogeneity in treatment effects. For all primary outcomes, the difference between the predicted treatment effects for the most affected versus the least affected groups is economically and statistically significant. This result raises questions as to which farmer characteristics are important predictors of this heterogeneity. The next section answers this question by comparing the most affected versus the least affected groups based on a set of baseline .

I follow the approach of Chernozhukov et al. (2020) in testing for heterogeneity in treatment effects. First, I estimate the conditional average treatment effects (CATE) (i.e., predicted treatment effects conditional on baseline characteristics) for the primary outcomes using ML algorithms.[36] Second, I estimate the best linear predictor (BLP) of the CATE, which is defined as:

$$BLP := \beta_1 + \beta_2(S(Z) - ES(Z)) \tag{8}$$

where $S(Z)$ is an estimator for the CATE. β_2 represents the heterogeneity loading parameter. Z is a set of baseline covariates. Rejecting the hypothesis $\beta_2 = 0$ implies that there is heterogeneity and $S(Z)$ is its relevant predictor.

[36] ML algorithms applied in this analysis are: random forest, elastic net, support vector machine, and boosted trees. These algorithms use a training sample to separately train predictors for $E[Y|D = 1, Z]$ in the treatment group and $E[Y|D = 0, Z]$ in the control group. Then, using the main sample, they predict potential outcomes for each individual. The predicted CATE is the difference between the two predictions for each individual. In Table 8 and Table 9, I am presenting the results from the "best" ML algorithm (i.e., the algorithm that minimizes prediction errors). Results from the other three ML algorithms are available upon request.

Table 8 shows strong evidence of heterogeneity in treatment effects. I am separately analyzing treatment effect heterogeneity for the sub-sample of farmers who received full subsidy in stage one (panel A) and the sub-sample of self-selected non-buyers who received full subsidy in stage two after declining to buy the seed at modest subsidy levels in stage one (panel B). The comparison group for the former is farmers in the pure control villages, while the comparison group for the latter is non-buyers in stage-two control villages. For the three main outcomes –namely, the likelihood of growing wheat, plot-level revenues, and plot-level profits– there is significant heterogeneity in the predicted treatment effects. This is the case for both sub-samples.

Next, I examine the magnitude of the heterogeneity in treatment effects by following Chernozhukov et al. (2020) in estimating sorted group average treatment effects (GATES). I sort observations in each sub-sample into five groups in ascending order based on the predicted CATE. That is, the first group represents 20% of the observations in the respective sub-sample with the lowest predicted CATE. Likewise, the fifth group represents 20% of the observations in the respective sub-sample with the highest predicted CATE.

The magnitudes of the heterogeneity in treatment effects shown in Table 9 are considerable. I estimate the GATES parameters for each outcome once for the sub-sample that received a full subsidy in stage one, and once for the sub-sample of non-buyers who were randomized into a full subsidy in stage two. For each outcome in both sub-samples, the difference between the predicted treatment effects for the most affected versus the least affected groups is economically and statistically significant. For example, the magnitude of the heterogeneity in treatment effects for plot revenues is more than 39,000 BDT per acre for both sub-samples. Similarly, both sub-samples show heterogeneity in the treatment effects on profits of a magnitude greater than 20,000 BDT per acre.

5.2 Credit Constraints Can Explain Differential Response to the Seed Subsidy

Given that baseline characteristics can explain heterogeneity in treatment effects, the next step is to compare the characteristics of farmers with the highest treatment effects to the characteristics of those with the lowest treatment effects. In particular, I am interested in baseline covariates that can serve as indicators for market frictions such as risk aversion and constraints to credit. I focus on heterogeneity in farmers' responses to the seed subsidy by increasing the likelihood of growing wheat.[37] The results show that, among the sub-sample of non-buyers, farmers who are most responsive to stage-two free distribution are significantly more likely to report constraints to additional borrowing at baseline.

Figure 6 compares heterogeneity in treatment effects for two sub-samples. The first sub-sample consists of farmers in stage-one free-distribution villages, for which the relevant control group is farmers in the pure control villages. The second sub-sample consists of self-selected non-buyers in stage-two treatment villages, for which the relevant control group is non-buyers in the stage-two control villages. The three predictors presented in the Figure 6 are: 1) whether the farmer obtained formal credit at baseline; 2) whether the farmer stated facing constraints to additional borrowing; 3) farmers' score on a risk aversion index score measured using a baseline survey module on attitudes towards agricultural risks.

Panel A of Figure 6 shows that, for the random sample of treated farmers in stage-one free-distribution villages, farmers with the highest treatment effect on growing wheat are less likely to have obtained formal credit at baseline. However, the survey data alone does not allow me to conclude

[37] Ideally, I should analyze heterogeneity in treatment effects on seed adoption. However, due to the low adoption levels in the control group, I am not able to use machine learning algorithms to estimate heterogeneity in treatment effects on adoption.

whether these farmers are less likely to obtain credit because they face credit constraints at the extensive margin or because they are better off without credit. It is not surprising that the most affected farmers who are less likely to have obtained credit are also less likely to report constraints to obtaining *more* credit. Farmers need to have access to credit at the extensive margin to report constraints to credit at the intensive margin. In addition, the most affected farmers in stage-one free-distribution villages score significantly lower on a risk-aversion index compared to the least affected farmers. The risk associated with experimenting with a new seed as well as the risk of incurring losses in case the seed did not fully protect farmers from crop diseases, may explain this heterogeneity in risk aversion between the most and least affected farmers in the stage-one free-distribution sub-sample.

Panel B of Figure 6 shows a different story for the self-selected sample of non-buyers. The most affected and the least affected non-buyers are equally likely to have obtained formal credit at baseline. However, non-buyers with the highest treatment effect on wheat cultivation are more likely to face constraints in obtaining additional credit. Risk aversion does not distinguish non-buyers with the highest from those with the lowest treatment effects on growing wheat. Therefore, the likelihood of facing constraints to additional borrowing is one of the key characteristics that distinguish non-buyers with the highest treatment effects on wheat cultivation. At the same time, Appendix Figure A.5 shows that constraints to additional borrowing does not distinguish farmers with the highest from those with the lowest treatment effects on plot profits. These results suggest that stage-two free distribution selects in farmers who are credit constrained but do not have systematically lower profits.

The finding that the most affected non-buyers are more likely to face constraints to additional borrowing is consistent with a model on agricultural investments in the presence of credit market frictions. The model is presented in Section 5.3. A key takeaway from the model is that a binding credit constraint can shift farmers' WTP for agricultural inputs downward, which results in a wedge between farmers' WTP and their expected marginal returns.

Finally, to test for status quo bias, Figure 7 compares the most affected and least affected farmers in terms of the likelihood of growing wheat at baseline. Panel A of Figure 7 shows that farmers in stage-one free-distribution villages with the highest treatment effects on wheat cultivation are significantly more likely to have grown wheat at baseline. In contrast, Panel B shows that non-buyers with the highest and lowest treatment effects have similar likelihood of growing wheat at baseline. This result suggests that status quo bias is more relevant for stage-one free-distribution treatment. However, status quo bias cannot explain non-buyers' response to free distribution in stage two.[38]

All in all, a data-driven heterogeneity analysis shows that the characteristics of the farmers with the highest response to a full subsidy in stage one differ from those of self-selected non-buyers who are most responsive to a full subsidy in stage two. For the farmers who received a full subsidy in stage one, status quo bias and risk aversion partly explain the heterogeneity in their response to the full subsidy. For the self-selected sample of non-buyers, constraints to additional borrowing partly explain the heterogeneity in their response to a full subsidy in stage two. These findings are consistent with a model in which farmers with relatively high expected marginal returns may reveal relatively low WTP for agricultural inputs due to a binding credit constraint as shown in the next section.

[38] Appendix Figure A.6 compares the most and least affected farmers in terms of the likelihood of growing common dry season crops other than wheat at baseline. The figure shows that the pattern of results is similar for the sub-sample of stage-one free-distribution treatment and stage-two free-distribution treatment for all common crops, except for lentil. This result suggests that stage-two free distribution may have induced farmers who were on the margin of growing wheat to switch from lentil to wheat cultivation.

5.3 A Model of Agricultural Investments in the Presence of Credit or Risk Market Failures

In an environment with complete markets and full separation between production and consumption, farmers' WTP for an agricultural technology should reflect their expected marginal returns from that technology. This study shows experimental evidence that farmers who reveal a relatively low WTP for the improved wheat seed realize returns to adoption that are not lower than the returns of the average farmer. In this section, I present a model on farmers' investment decisions in the presence of credit and/ or risk market failures following Karlan et al. (2014). I show that a binding credit constraint can shift farmers' WTP for agricultural inputs downward, and results in a gap between WTP and expected marginal productivity.

5.3.1 Base Model

I start with a simple two-period model in which farmers make investment decisions in the first period and realize a state-contingent output in the second period. The expected output from agricultural investments is given by:

$$E[Y(s_\kappa)] = \sum_{s_\kappa} P(s_\kappa)Y(s_\kappa) \tag{9}$$

where s_κ represents a crop-specific state of the world, κ is the crop produced, and $P(s_\kappa)$ is the probability that s_κ is realized. For simplicity of notation, I am dropping the i subscript for farmer-specific output. However, I note that the production function varies across farmers. A farmer's production function is given by:

$$Y(s_\kappa) = A_\kappa f_s(\mathbf{x}_\kappa) \tag{10}$$

where A_κ is a productivity term that reflects the farmer's comparative advantage in growing crop κ. $\mathbf{x}_\kappa$ is a bundle of agricultural inputs used to produce crop κ. $f_s(.)$ is concave, and $f_s(0) = 0$ $\forall s$.[39] For simplicity, assume that there are only two states of the world for each crop: $s_\kappa \in \{H_\kappa, L_\kappa\}$. Production is such that state H_κ results in strictly higher output: $f_H(\mathbf{x}_\kappa) > f_L(\mathbf{x}_\kappa)$ $\forall\kappa$. Also, the marginal productivity of inputs in state H_κ is higher, such that: $f'_H(\mathbf{x}_\kappa) > f'_L(\mathbf{x}_\kappa)$ $\forall\kappa$.

Farmers choose between investing in agricultural production and buying (or selling) a risk-free asset a that earns (or pays) a flat interest rate R. There is perfect risk pooling such that a farmer's consumption in the second period is the expected value of total income in any realized second-period state. The farmer's problem is to maximize an intertemporal utility of consumption subject to a set of constraints. That is:

$$\max_{\mathbf{x}_\kappa, a} u(c^0) + \delta \sum_{s_\kappa} P(s_\kappa)u(c^1) \tag{11}$$

s.t.

$$\begin{aligned} c^0 &= I - \sum_{\kappa} \psi(\mathbf{x}_\kappa) - a \\ c^1_H = c^1_L = c^1 &= \sum_{s_\kappa} P(s_\kappa)(Y_{s\kappa} + Ra) \\ \mathbf{x}_\kappa &\geq 0 \end{aligned} \tag{12}$$

where $u(.)$ is a concave utility function. The concavity of $u(.)$ depends on the farmer's degree of risk aversion. δ is a discount factor that is inversely related to the flat interest rate R. For simplicity, let $\delta = \frac{1}{R}$. I is an initial endowment of income. $\psi(\mathbf{x}_\kappa)$ is a cost function that is increasing in $\mathbf{x}_\kappa$. The

[39] This setup implicitly assumes a perfect agricultural land market. There are no restrictions on the farm land that the farmer can use.

shape of the cost function depends on the economies of scale, which varies across farmers. Agricultural output prices are normalized to 1. The farmer chooses to invest in a such that:

$$\frac{u'(c^0)}{u'(c^1)} = R\delta = 1 \tag{13}$$

and chooses $\mathbf{x}_\kappa$ such that:

$$\begin{cases} \psi'(\mathbf{x}^*_\kappa) = \delta \sum_s P(s_\kappa) A_\kappa f'_s(\mathbf{x}^*_\kappa), & \text{if } \mathbf{x}^*_\kappa > 0 \\ \psi'(\mathbf{x}^*_\kappa) > \delta \sum_s P(s_\kappa) A_\kappa f'_s(\mathbf{x}^*_\kappa), & \text{if } \mathbf{x}^*_\kappa = 0 \end{cases} \tag{14}$$

Thus, with perfect credit markets and complete risk pooling, farmers invest in agricultural production up to the point where the marginal cost of inputs is equal to their expected marginal productivity. A farmer's WTP for agricultural inputs depends on the likelihood of each state of the world, the characteristics of the production function, the farmer's comparative advantage in producing crop κ, and a given discount factor (that is determined by the supply and demand of risk-free assets).

A farmer is expected to select out of growing one crop, $\mathbf{x}^*_\kappa = 0$, if the farmer has low comparative advantage in growing that crop. That is, if A_κ is sufficiently low. A subsidy on a crop-specific input, lowers the threshold for growing the subsidized crop on both the extensive and intensive margins. Thus, the subsidy is expected to induce farmers with low expected returns to grow the subsidized crop.

5.3.2 Credit Constraints

In the presence of credit market frictions, there is an additional constraint to the utility maximization problem, which is $a \geq 0$. That is, borrowing is not possible. Suppose that the constraint is binding, but there is complete risk pooling. That is, $c^1_H = c^1_L = c^1$ still holds. Then, the first order conditions become:

$$\frac{u'(c^0)}{u'(c^1)} > 1 \tag{15}$$

and

$$\begin{cases} \psi'(\mathbf{x}^*_\kappa) = \delta \frac{u'(c^1)}{u'(c^0)} \sum_s P(s_\kappa) A_\kappa f'_s(\mathbf{x}^*_\kappa) < \delta \sum_s P(s_\kappa) A_\kappa f'_s(\mathbf{x}^*_\kappa), & \text{if } \mathbf{x}^*_\kappa > 0 \\ \psi'(\mathbf{x}^*_\kappa) > \delta \frac{u'(c^1)}{u'(c^0)} \sum_s P(s_\kappa) A_\kappa f'_s(\mathbf{x}^*_\kappa), & \text{if } \mathbf{x}^*_\kappa = 0 \end{cases} \tag{16}$$

Therefore, a binding credit constraint creates a wedge between farmers' WTP for agricultural inputs and expected marginal productivity. Credit-constrained farmers are expected to have lower WTP for agricultural inputs compared to unconstrained farmers with similar marginal productivity. In addition, a concave production function implies that credit-constrained farmers who start at relatively low levels of $\mathbf{x}^*$ may have relatively high marginal returns to investment in additional inputs.

When the credit constraint is binding, a subsidy on a crop-specific input releases part of the farmer's budget allocated to that input. As a result, the farmer may increase investment in other inputs and end up with relatively high returns. Therefore, credit market frictions imply that farmers who are induced to grow a certain crop as a result of a crop-specific subsidy are not necessarily those who have low expected returns.

5.3.3 Uninsured Risks

In the absence of insurance against risks, whether formal insurance or informal risk pooling, consumption in the second period depends on the realized state of the world. In this case, the constraints in

Equation 12 become:

$$
\begin{aligned}
c^0 &= I - \sum_{\kappa} \psi(\mathbf{x}_\kappa) - a \\
c^1_H &= \sum_{\kappa} P(H_\kappa)(Y(H_\kappa) + Ra) \\
c^1_L &= \sum_{\kappa} P(L_\kappa)(Y(L_\kappa) + Ra) \\
\mathbf{x}_\kappa &\geq 0 \\
a &\geq 0
\end{aligned}
\tag{17}
$$

the farmer chooses a such that

$$
\begin{cases}
u'(c^0) = \sum_{s_\kappa} P(s_\kappa) u'(c^1_s), & \text{if } a^* > 0 \\
u'(c^0) > \sum_{s_\kappa} P(s_\kappa) u'(c^1_s), & \text{if } a* = 0
\end{cases}
\tag{18}
$$

and chooses $\mathbf{x}_\kappa$ such that:

$$
\begin{cases}
\psi'(\mathbf{x}^*_\kappa) = \delta \sum_{s_\kappa} P(s_\kappa) \frac{u'(c^1_s)}{u'(c^0)} A_\kappa f'_s(\mathbf{x}^*_\kappa), & \text{if } \mathbf{x}^*_\kappa > 0 \\
\psi'(\mathbf{x}^*_\kappa) > \delta \sum_{s_\kappa} P(s_\kappa) \frac{u'(c^1_s)}{u'(c^0)} A_\kappa f'_s(\mathbf{x}^*_\kappa), & \text{if } \mathbf{x}^*_\kappa = 0
\end{cases}
\tag{19}
$$

Compared to the case with perfect markets in Equation 14, a farmer's WTP for agricultural inputs becomes a weighted sum of their expected returns in each state of the world. The weights depend on the marginal utility of future consumption relative to the marginal utility of current consumption. With a concave utility function, it follows that the marginal utility is higher at lower levels of consumption: $u'(c^1_L) > u'(c^1_H)$ if $c^1_L < c^1_H$. In addition, it is given that marginal productivity is higher at the high state: $f'_H(\mathbf{x}_\kappa) > f'_L(\mathbf{x}_\kappa)$. Without loss of generality, let $c^1_L < c^0 < c^1_H$, then $\frac{u'(c^1_L)}{u'(c^0)} > 1 > \frac{u'(c^1_H)}{u'(c^0)}$. Thus, risk-averse farmers overweight their expected marginal returns in the low state and underweight their expected marginal returns in the high state. The higher the degree of risk aversion, the lower the farmer's WTP for agricultural inputs, regardless of whether the credit constraint binds or not. Therefore, in the presence of uninsured risks, a subsidy on a crop-specific input does not necessarily induce adoption by farmers with low expected returns. At the same time, more risk-averse farmers can be less responsive to input subsidies compared to less risk-averse farmers.

5.4 Discussion

The model suggests that, in the presence of credit and/ or risk market frictions, farmers with similar expected returns may show different levels of WTP for agricultural inputs. One reason for that is some farmers face binding credit constraints and some do not. In addition, some farmers may be more risk-averse than others. The results presented in Section 4.3 and Section 4.4 show that self-selected non-buyers do not realize lower returns compared to the average farmer. In addition, the results from a data-driven heterogeneity analysis presented in Section 5.2 show that non-buyers who are most responsive to the full subsidy in stage two are significantly more likely to face constraints on additional borrowing. Put together, these findings suggest that credit or liquidity constraints may explain the low WTP revealed by non-buyers. However, there is also a possibility that other mechanisms may explain the main results. Providing suggestive evidence that credit constraints is a plausible mechanism for explaining the main results does not eliminate the possibility that other mechanisms may be plausible too.

6 Conclusion

This paper examines whether lower prices distort allocative efficiency for goods with heterogeneous marginal benefits. I take this research question to the context of agricultural production where price subsidies are common, and the factors contributing to heterogeneity in farmers' returns are numerous. The subsidized agricultural input in this study is an improved wheat seed that has an environmental externality due to its resistance to contagious crop disease. Subsidizing the improved seed may improve allocative efficiency by reaching farmers who have high returns but are facing constraints to adoption. Alternatively, subsidization may distort allocative efficiency by allocating the seed to farmers who either waste the subsidized seed or have low returns to adoption.

Using a two-stage experimental design, I compare the outcomes of farmers who receive the seed for free in the first stage with the outcomes of farmers who receive the seed for free in the second stage after selecting out of buying the seed in stage one. I find that free distribution increases not only take up but also the likelihood of adoption (i.e., planting the seed rather than consuming wheat seeds for food). The effect of stage-one free distribution treatment on adoption by the average farmer is on par with the effect of stage-two free distribution treatment on adoption by self-selected non-buyers. Thus, non-buyers in stage one show relatively high willingness to adopt conditional on receiving the seed for free in stage two.

A comparison between the returns of the average farmer and the returns of non-buyers does not suggest that non-buyers select out of buying the seed due to lower returns. On the contrary, farmers who select out of buying the improved seed at a low subsidy level realize slightly higher returns compared to the average farmer. The two-stage randomization allows me to infer the average returns of the farmers who are induced to buy the seed as the subsidy level increases from low to medium (i.e., would-be buyers at the medium subsidy). I show that the would-be buyers at the medium subsidy level have higher than average returns. Thus, lowering agricultural input prices in the study setting does not distort allocation to lower return farmers.

Several mechanisms may explain why non-buyers select out of buying the improved seed if their actual returns are not lower than the returns of the average farmer. While I do not attempt to isolate one mechanism as an exclusive explanation for the main results, I do provide suggestive evidence that binding credit constraints may provide a plausible explanation for non-buyers' outcomes. Empirically, I find that non-buyers who are most responsive to stage-two free distribution treatment are more likely to report binding credit constraints at baseline compared to the least affected non-buyers. Theoretically, the credit constraints mechanism is consistent with a model in which farmers' WTP for agricultural inputs is shifted downward when the credit constraint binds. That is, a binding credit constraint can result in a gap between revealed WTP and expected marginal returns.

In conclusion, this paper provides the first experimental evidence that agricultural input prices do **not** screen farmers based on their actual returns. This implies that policy makers who aim to increase the dissemination of agricultural technologies cannot rely on market prices as a mechanism for targeting high return farmers. Nevertheless, this finding does not imply that a universal subsidy is an optimal policy solution either. An alternative policy might be to use a targeting mechanism independent of prices. For example, the second wave of agricultural input subsidies in Sub-Saharan Africa relies on agricultural extension agents to target farmers with high expected returns. This policy might be justified when prices fail to sort farmers based on their returns. However, relying on the subjective judgement of extension agents entails its own problems. Further research is needed to evaluate new mechanisms for targeting farmers with high marginal returns to adoption of modern agricultural technologies.

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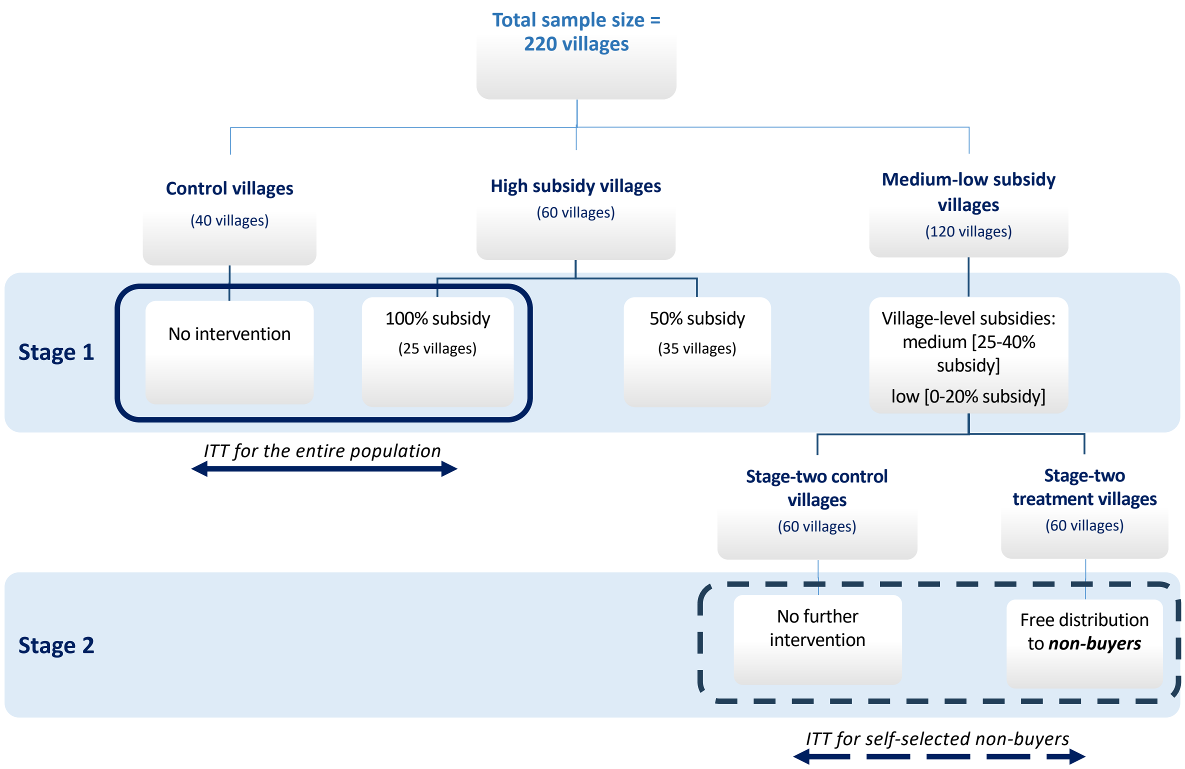
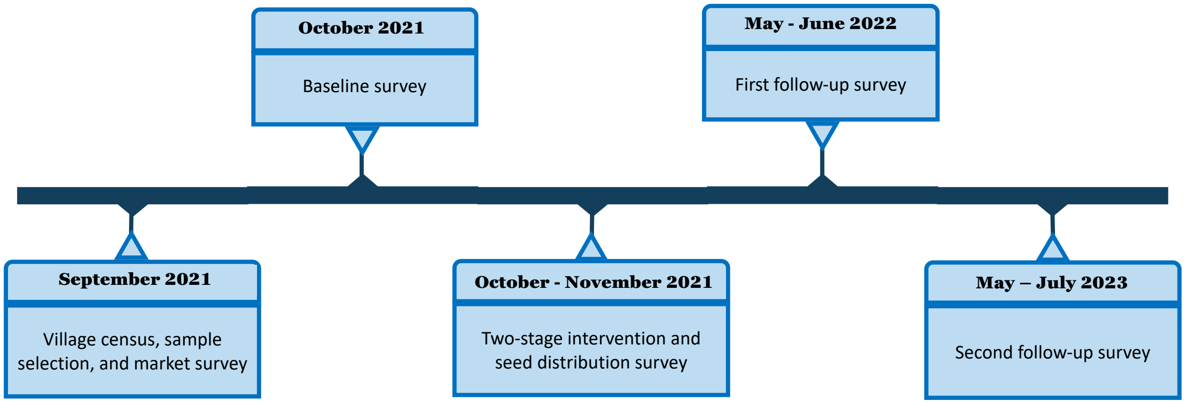
Figures and Tables

Figure 1: Experimental design



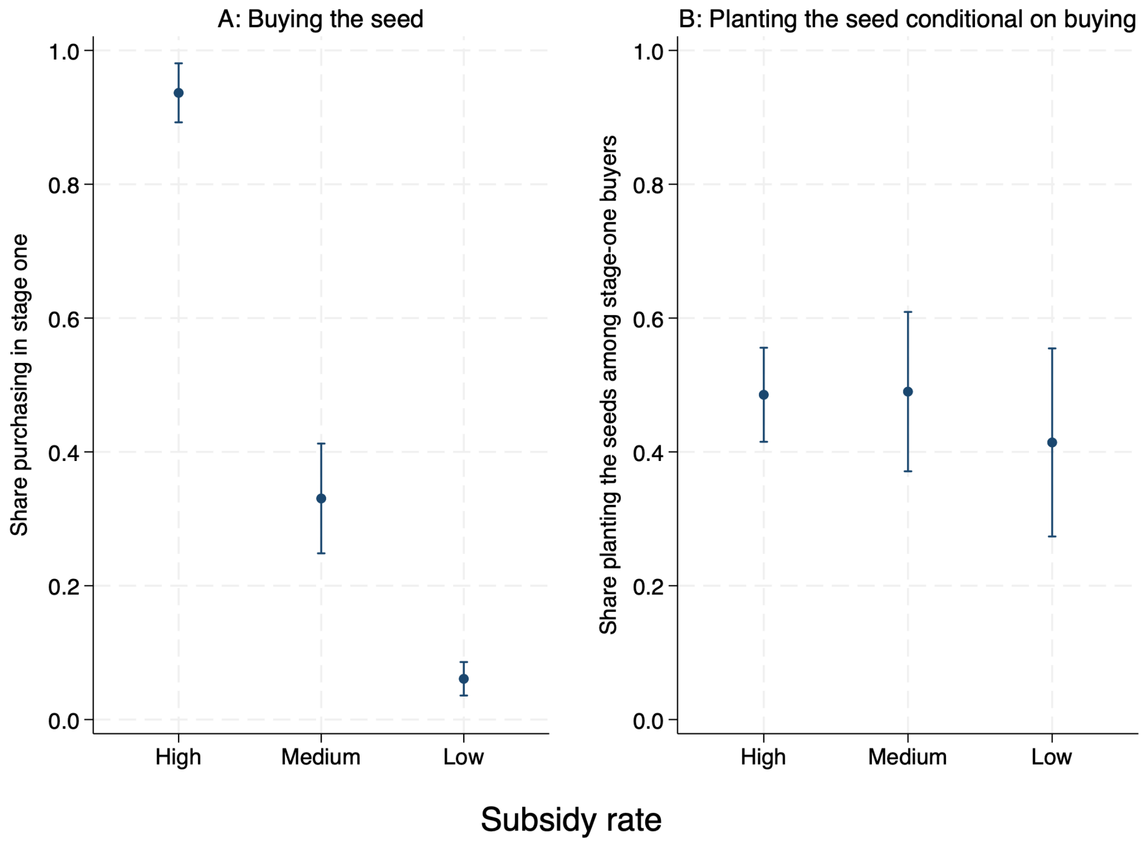
Notes: This figure illustrates the two-stage experimental design. In the first stage, villages are randomized into a pure control arm and two treatment arms. One treatment arm receives a high (100% - 50%) subsidy level, and the other treatment arm receives a medium-low subsidy levels (40% - 0%) for an improved wheat seed. Farmers in treatment villages are offered to buy the improved seed at the village-level subsidy rate in a take-it-or-leave-it design. In the second stage of the experiment, villages in medium-low subsidy arm are randomized into stage-two treatment and stage-two control. *Non-buyers* in stage-two treatment villages are offered the same improved seed for free within two weeks after the implementation of stage-one. The solid rectangle in the top-right corner highlights the comparison between stage-one free-distribution villages and the pure control villages, which is used to estimate treatment effects for the entire population. The dashed rectangle in the bottom-left corner highlights the comparison between *non-buyers* in stage-two treatment and stage-two control villages, which is used to estimate treatment effects among non-buyers.

Figure 2: Timeline of implementation and data collection



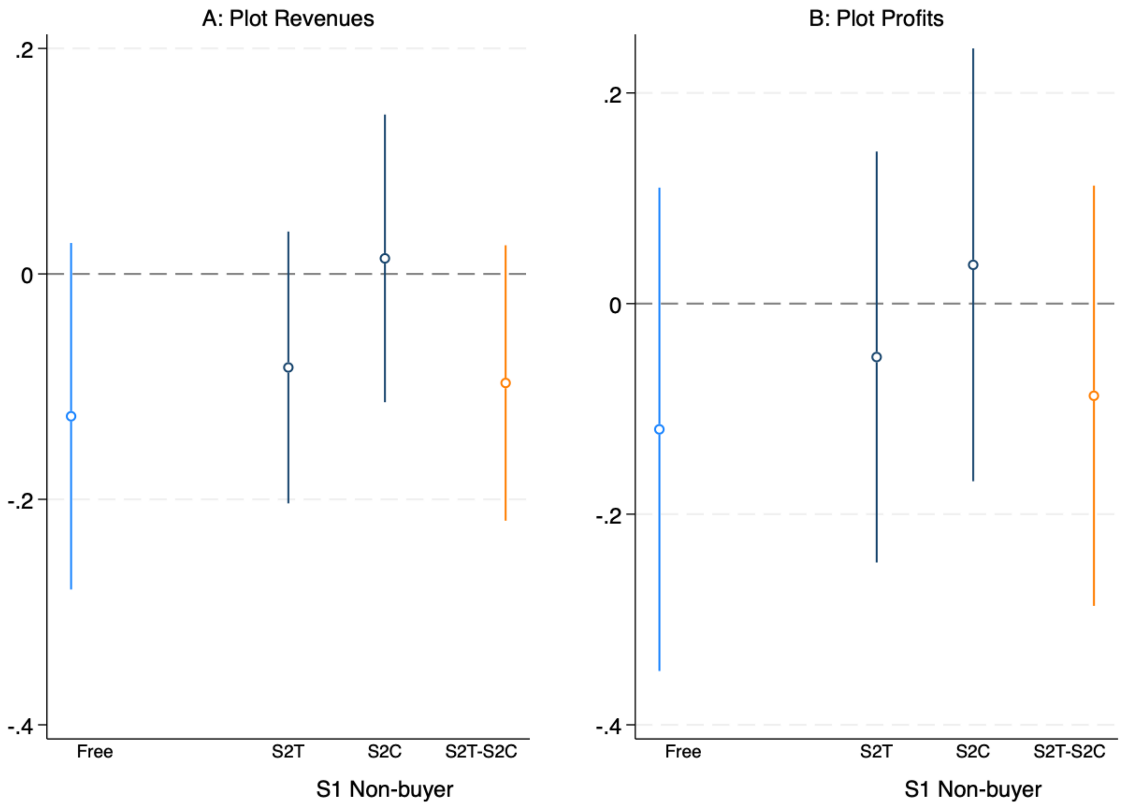
Notes: This figure summarizes the timeline for implementation and data collection. The baseline survey was completed before starting the intervention. The two-stages of the intervention were completed at least one week before the start of the planting season. The first follow-up survey was collected at the end of wheat harvesting season. The second follow-up survey was collected at the end of wheat harvesting season the following year.

Figure 3: Demand vs. usage of distributed seeds



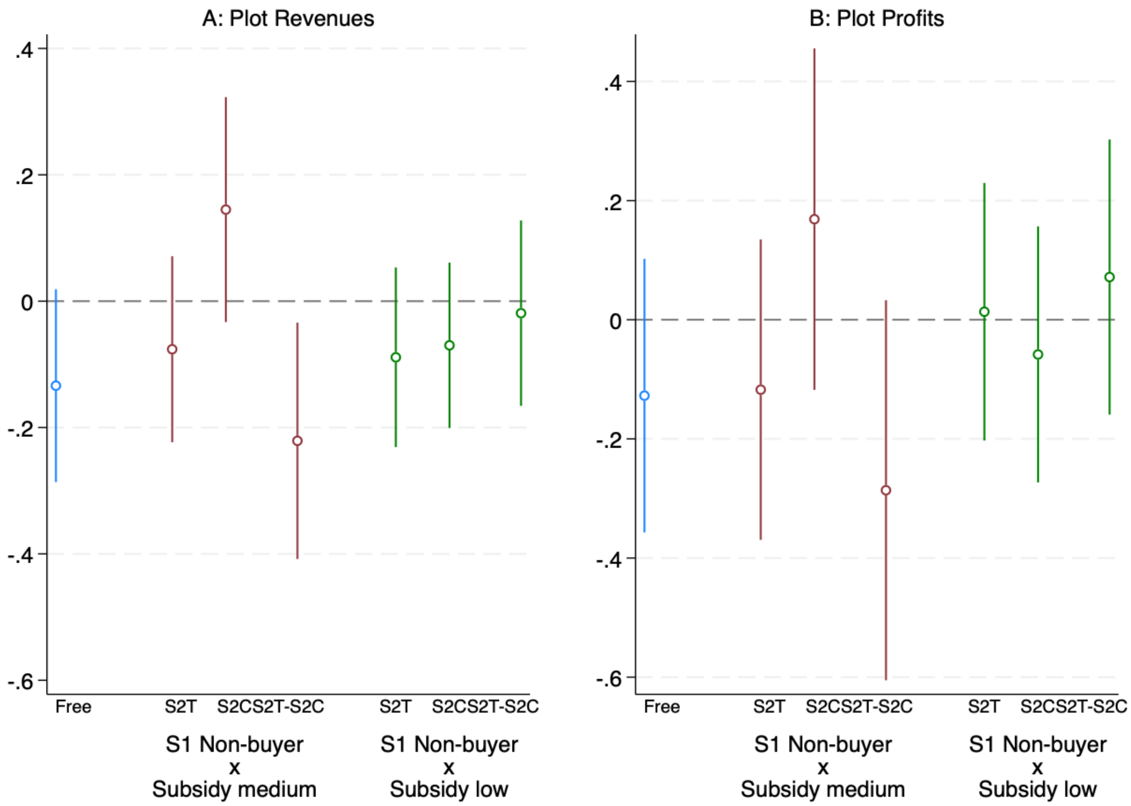
Notes: The subsidy rates on the horizontal axis refer to high (50-100%), medium (25-40%), and low (0-20%) subsidy. Panel A shows the share of treated farmers who took up the improved seed at the village-level subsidy rate in stage one of the experiment. Panel B shows the share of farmers who planted the seeds among the self-selected sample of farmers who purchased the seeds in stage one. The data used to plot this figure is the entire sample of treatment villages. The results are similar when stage-two treatment villages are excluded from the sample. Confidence intervals are at the 95% level.

Figure 4: Selection effects on plot revenues and plot profits



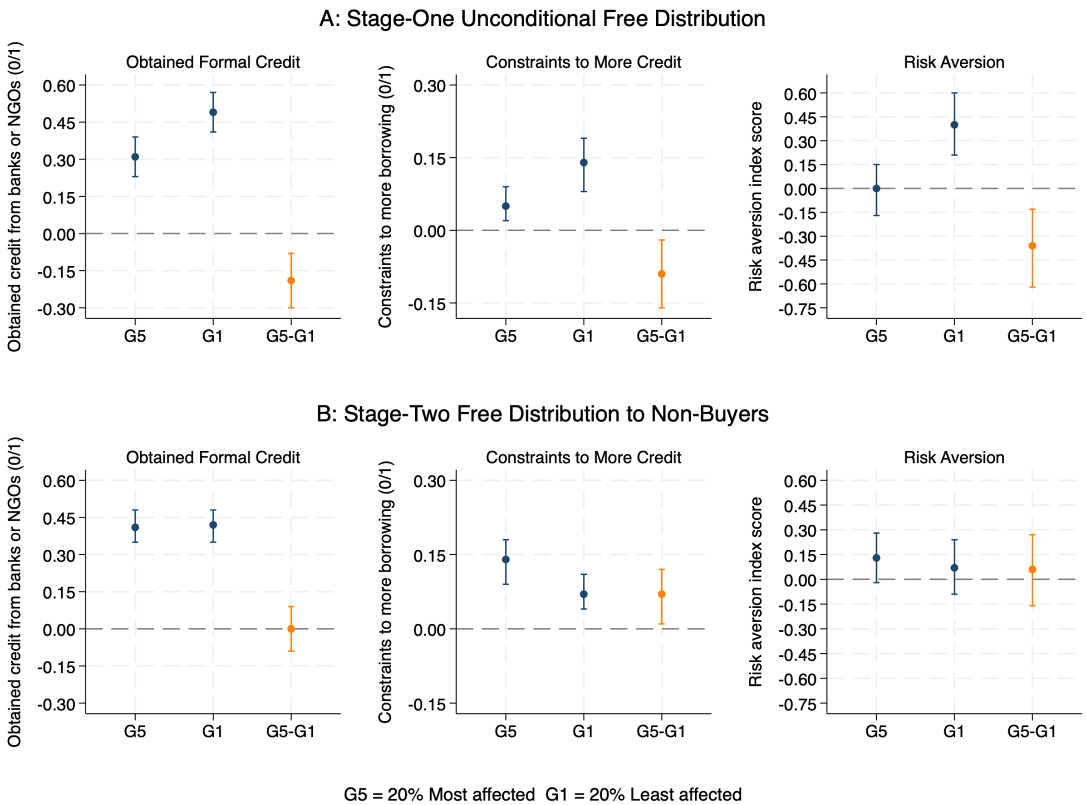
Notes: This figure summarizes regression results using the specification in equation (6). The outcome variable in panel A is plot revenues (in logs), and the outcome variable in panel B is plot profits (in logs). Confidence intervals are at the 95% level.

Figure 5: Selection effects: Distinction between self-selection in medium-subsidy vs low-subsidy villages



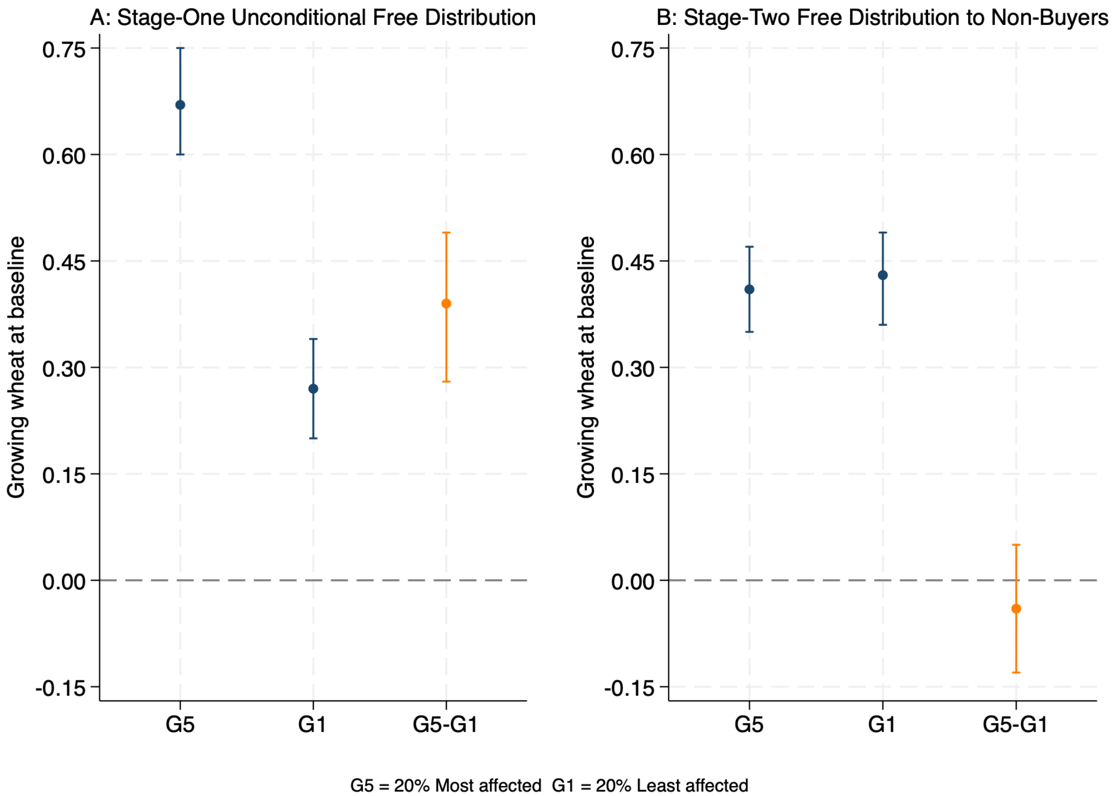
Notes: This figure summarizes regression results using the specification in equation (7) that interacts the coefficients on stage-one non-buyers with the randomized subsidy level that was received at stage one. The outcome variable in panel A is plot revenues (in logs), and the outcome variable in panel B is plot profits (in logs). Confidence intervals are at the 95% level.

Figure 6: Distinction between farmers with the highest and lowest treatment effects on growing wheat: Risk aversion and credit constraints



Notes: This figure compares farmers with the highest versus the lowest treatment effects on growing wheat in terms of the likelihood of obtaining formal credit at baseline, the likelihood of reporting constraints to borrowing more money at the given rates, and their degree of risk aversion (measured using baseline survey module on attitudes towards risk). The sample in panel A is the treated farmers in the free-distribution villages and surveyed farmers in the pure control villages. The sample in panel B is the self-selected non-buyers in the stage-two treatment and stage-two control villages. Confidence intervals are at the 90% level.

Figure 7: Distinction between farmers with the highest and lowest treatment effects on growing wheat: The likelihood of growing wheat at baseline



Notes: This figure compares farmers with the highest versus the lowest treatment effects on growing wheat in terms of the likelihood of growing wheat on a surveyed plot at baseline. The sample in panel A is the treated farmers in the free-distribution villages and surveyed farmers in the pure control villages. The sample in panel B is the self-selected non-buyers in the stage-two treatment and stage-two control villages. Confidence intervals are at the 90% level.

Table 1: Sample size by subsidy rate and self-selection into buying at stage one

Subsidy Rate	A. Village-level Randomization	
	S2 Control Villages	S2 Treatment Villages
High subsidy (50-100%)		
Free distribution	25	- -
50% subsidy	35	- -
Medium subsidy (25-40%)	25	30
Low subsidy (0-20%)	35	30
Pure control	40	- -
Villages Total	160	60

	B. Farmers' Self-Selection into Buying							
	S2 Control Villages				S2 Treatment Villages			
	S1 Buyer		S1 Non-Buyer		S1 Buyer		S1 Non-Buyer	
	N	%	N	%	N	%	N	%
Medium subsidy (25-40%)	163	82	459	35	290	82	459	41
Low subsidy (0-20%)	36	18	839	65	63	18	662	59
Self-Selection Total	199	100%	1,298	100%	353	100%	1,121	100%

Notes: This table summarizes the sample size based on village-level randomization (panel A) and farmers' self-selection into buying the seeds at stage one (panel B). The village-level randomization is relevant for an analysis of the intent-to-treat effects. The self-selected sample is relevant for analyses that examine whether farmers select out of buying the improved seed based on realized returns. A random sample of 25 farmers is treated in each of the treatment villages. One village was excluded from stage-two treatment during implementation because farmers refused to cooperate during seed sales intervention at stage one.

Table 2: Characteristics of Buyers versus Non-Buyers

	Medium-Subsidy Villages			Low-Subsidy Villages		
	(1)	(2)	(3)	(4)	(5)	(6)
	Buyers	Non-Buyers	Difference	Buyers	Non-Buyers	Difference
A. Demographics						
Farmer's age	44.81	44.05	0.81	44.83	44.15	0.22
	(12.41)	(12.45)	(1.05)	(12.44)	(12.84)	(1.40)
Farmer's years of schooling	4.88	4.44	0.54*	5.03	4.93	0.14
	(3.90)	(3.92)	(0.28)	(4.48)	(4.10)	(0.54)
HH members available for farm work	1.72	1.70	0.02	1.94	1.56	0.23**
	(0.90)	(0.90)	(0.07)	(0.89)	(0.75)	(0.10)
B. Non-farm income, risk, and credit						
Access to non-farm income (0/1)	0.34	0.25	0.09***	0.31	0.28	0.04
	(0.48)	(0.43)	(0.03)	(0.47)	(0.45)	(0.05)
Risk aversion index [-5, 5]	0.17	-0.12	0.18	0.19	0.16	-0.08
	(1.02)	(1.12)	(0.12)	(1.04)	(1.17)	(0.12)
Access to credit from banks or NGOs (0/1)	0.44	0.40	0.02	0.43	0.43	0.01
	(0.50)	(0.49)	(0.03)	(0.50)	(0.50)	(0.06)
HH's outstanding loans ('000 BDT')	28.26	19.99	9.33**	28.98	23.45	5.72
	(58.20)	(42.74)	(4.19)	(53.89)	(44.05)	(5.75)
HH faces credit limitations	0.11	0.10	-0.01	0.12	0.13	-0.00
	(0.31)	(0.30)	(0.02)	(0.33)	(0.33)	(0.03)
C. Agriculture						
Area of land cultivated at baseline (acres)	1.60	1.35	0.25***	1.74	1.51	0.23
	(1.24)	(1.20)	(0.09)	(1.56)	(1.50)	(0.17)
Number of plots cultivated at baseline	5.44	4.87	0.43	5.66	4.98	0.80**
	(2.91)	(2.98)	(0.26)	(3.21)	(2.91)	(0.31)
Value of livestock owned ('000 BDT)	123.54	123.07	3.82	110.21	108.10	-9.99
	(131.00)	(138.28)	(8.90)	(135.91)	(130.08)	(14.57)
Farmer grew wheat at baseline	0.41	0.38	-0.01	0.36	0.34	0.02
	(0.49)	(0.49)	(0.04)	(0.48)	(0.48)	(0.05)
Stated WTP for improved wheat seed (BDT/kg)	16.09	18.82	-3.44*	14.60	16.21	-1.51
	(12.90)	(15.66)	(1.81)	(10.99)	(13.70)	(0.90)
Plot revenues ('000 BDT/acre)	75.23	71.16	7.79	71.01	65.83	1.63
	(93.93)	(60.01)	(6.69)	(64.47)	(49.65)	(6.39)
Plot profits ('000 BDT/acre)	22.74	21.21	2.83	29.66	18.02	6.45
	(84.10)	(56.38)	(5.91)	(47.56)	(132.09)	(5.49)

Notes: This table compares baseline characteristics of seed buyers versus non-buyers in the medium-subsidy and low-subsidy villages separately. Columns (3) and (6) are estimated by regressing each of the listed covariates on a dummy variable for the corresponding comparison. All regressions use strata fixed effects and cluster standard errors at the village level.

Table 3: Impact on adoption and wheat cultivation

	Adoption (farm-level)		Growing Wheat (farm-level)		Farm-level Share of Wheat Area		Growing Wheat (plot-level)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Free distribution village (γ_1)	0.41***	0.41***	0.29***	0.28***	0.09***	0.09***	0.18***	0.18***
	(0.04)	(0.04)	(0.04)	(0.04)	(0.02)	(0.02)	(0.04)	(0.04)
50% Subsidy village	0.29***		0.19***		0.06***		0.12***	
	(0.04)		(0.04)		(0.02)		(0.03)	
Stage 2 treatment village	0.36***		0.24***		0.08***		0.15***	
	(0.03)		(0.03)		(0.01)		(0.03)	
Stage 2 control village	0.06***		0.04		0.02		0.03	
	(0.02)		(0.03)		(0.02)		(0.03)	
S1 Non-buyer x S2 Treat (γ_2)		0.35***		0.23***		0.07***		0.13***
		(0.04)		(0.04)		(0.02)		(0.03)
S1 Non-buyer x S2 Control (γ_3)		0.03		0.02		0.01		0.01
		(0.03)		(0.04)		(0.02)		(0.03)
S1 Buyer x S2 Treat		0.39***		0.27***		0.09***		0.20***
		(0.05)		(0.05)		(0.02)		(0.05)
S1 Buyer x S2 Control		0.32***		0.23***		0.08***		0.14**
		(0.06)		(0.06)		(0.02)		(0.05)
Impact among *would-be* buyers		0.85***		0.60**		0.21		0.45
		(0.28)		(0.30)		(0.14)		(0.28)
p-value Free = S2T	0.22		0.29		0.52		0.43	
p-value $\gamma_1 = \gamma_2 - \gamma_3$		0.07		0.23		0.30		0.26
CI: $\gamma_1 - \gamma_2 + \gamma_3$		(-0.01, 0.20)		(-0.05, 0.18)		(-0.03, 0.08)		(-0.04, 0.17)
p-val $S2T_{non\_buyer} = S2C_{non\_buyer}$		0.00		0.00		0.00		0.00
p-value $S2T_{buyer} = S2C_{buyer}$		0.29		0.55		0.69		0.32
Strata FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.209	0.265	0.147	0.192	0.097	0.118	0.103	0.127
Control villages' mean	0.02	0.02	0.15	0.15	0.06	0.06	0.10	0.10
Number of observations	5,489	4,611	5,489	4,611	5,489	4,611	5,054	4,234

Notes: Farm-level outcomes refer to variables measured using survey data on the farmer's three main plots. Plot-level outcomes are measured using the top-ranked plot for each farmer. A top-ranked plot is defined as the farm plot ranked by the farmer at baseline as the most suitable plot for growing wheat. The outcome variable in columns (1) and (2) is whether the farmer adopted the improved seed on a surveyed plot. The outcome variable in columns (3) and (4) is whether the farmer cultivated wheat on a surveyed plot (extensive margin). The outcome variable in columns (5) and (6) is the share of the farm area allocated to wheat (intensive margin). Finally, the outcome variable in columns (7) and (8) is whether the farmer cultivated wheat on the top-ranked plot. Impact among *would-be* buyers is calculated as: $\frac{\gamma_1-(0.82)*(\gamma_2-\gamma_3)}{0.18}$, where 0.18 is the probability of buying the seeds at stage one in the medium-low subsidy villages. Standard errors in parentheses are clustered at the village level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 4: Impact on plot revenues and profits

	Plot Revenues (log)				Plot Profits (log)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Free distribution village (γ_1)	-0.12	-0.11*	-0.12	-0.12*	-0.11	-0.10	-0.11	-0.11
	(0.08)	(0.07)	(0.08)	(0.07)	(0.12)	(0.12)	(0.12)	(0.12)
50% Subsidy village	-0.02	-0.02			-0.01	0.03		
	(0.07)	(0.06)			(0.11)	(0.10)		
Stage-two treatment village	-0.09	-0.09*			-0.06	-0.04		
	(0.06)	(0.05)			(0.09)	(0.09)		
Stage 2 control village	-0.00	-0.01			0.03	0.02		
	(0.06)	(0.05)			(0.10)	(0.09)		
S1 Non-buyer x S2 Treat (γ_2)			-0.08	-0.08			-0.04	-0.02
			(0.06)	(0.05)			(0.10)	(0.10)
S1 Non-buyer x S2 Control (γ_3)			0.02	0.01			0.06	0.05
			(0.07)	(0.06)			(0.10)	(0.10)
S1 Buyer x S2 Treat			-0.20***	-0.19***			-0.38**	-0.36**
			(0.07)	(0.07)			(0.16)	(0.16)
S1 Buyer x S2 Control			-0.16**	-0.16**			-0.13	-0.15
			(0.08)	(0.08)			(0.12)	(0.12)
Impact among *would-be* buyers			-0.22	-0.24			-0.14	-0.25
			(0.53)	(0.46)			(0.80)	(0.77)
p-value Free = S2T	0.61	0.66			0.65	0.54		
p-value $\gamma_1 = \gamma_2 - \gamma_3$			0.83	0.78			0.96	0.83
CI: $\gamma_1 - \gamma_2 + \gamma_3$			(-0.22, 0.18)	(-0.20, 0.15)			(-0.31, 0.30)	(-0.32, 0.26)
p-value S2T$_{non_buyer} = S2C_{non_buyer}$			0.11	0.08			0.33	0.43
p-value S2T$_{buyer} = S2C_{buyer}$			0.63	0.64			0.13	0.19
Strata FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
LASSO selected controls	No	Yes	No	Yes	No	Yes	No	Yes
R-squared	0.149	0.210	0.143	0.192	0.056	0.084	0.080	0.099
Control villages' mean ('000 BDT/acre)	94.84	94.84	94.84	94.84	44.42	44.42	44.42	44.42
Number of observations	4,815	4,815	4,022	4,022	4,396	4,396	3,659	3,659

Notes: The outcome variable in columns (1) and (4) is the log of plot revenues. Revenues are measured as the output per unit area (i.e., yield) multiplied by the farm-gate price. The outcome variable in columns (5) and (8) is the log of plot profits. Profits are measured as total revenues net of all input costs, where both revenues and costs are measured per unit area. I follow Agness et al. (2022) rule of thumb of valuing the opportunity cost of family labor at 60% of the average market wage. Impact among *would-be* buyers is calculated as: $\frac{\gamma_1-(0.82)*(\gamma_2-\gamma_3)}{0.18}$, where 0.18 is the probability of buying the seeds at stage one in the medium-low subsidy villages. Standard errors in parentheses are clustered at the village level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 5: Distinction between self-selection in medium-subsidy vs low-subsidy villages

	Adoption (farm-level)		Growing Wheat (plot-level)		Plot Revenues (log)		Plot Profits (log)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Free distribution village (λ_1)	0.41***	0.42***	0.18***	0.18***	-0.13*	-0.13*	-0.11	-0.12
	(0.04)	(0.04)	(0.04)	(0.04)	(0.08)	(0.07)	(0.12)	(0.12)
Subsidy Med x S1 Buyer	0.36***	0.37***	0.14***	0.14***	-0.11	-0.08	-0.19	-0.17
	(0.04)	(0.04)	(0.04)	(0.04)	(0.07)	(0.06)	(0.13)	(0.13)
Subsidy Med x S1 Non-Buyer x S2 Treat (λ_2)	0.36***	0.36***	0.08*	0.08*	-0.07	-0.06	-0.10	-0.08
	(0.05)	(0.05)	(0.05)	(0.04)	(0.07)	(0.07)	(0.13)	(0.13)
Subsidy Med x S1 Non-Buyer x S2 Control (λ_3)	0.04	0.05	-0.01	0.01	0.15	0.12	0.18	0.17
	(0.03)	(0.03)	(0.04)	(0.04)	(0.09)	(0.08)	(0.15)	(0.14)
Subsidy Low x S1 Buyer	0.34***	0.35***	0.22***	0.24***	-0.30***	-0.31***	-0.39**	-0.41**
	(0.06)	(0.06)	(0.07)	(0.06)	(0.08)	(0.08)	(0.17)	(0.16)
Subsidy Low x S1 Non-Buyer x S2 Treat (λ_4)	0.34***	0.34***	0.17***	0.18***	-0.09	-0.11*	0.03	0.03
	(0.04)	(0.04)	(0.04)	(0.04)	(0.07)	(0.06)	(0.11)	(0.11)
Subsidy Low x S1 Non-Buyer x S2 Control (λ_5)	0.03	0.03	0.03	0.03	-0.06	-0.06	-0.03	-0.04
	(0.03)	(0.03)	(0.04)	(0.03)	(0.07)	(0.06)	(0.11)	(0.10)
Impact among *would-be* buyers at subsidy med	0.29*	0.30*	0.22	0.29*	0.38	0.23	0.76	0.70
	(0.16)	(0.16)	(0.17)	(0.16)	(0.29)	(0.26)	(0.47)	(0.44)
p-value S2T$_{med} = S2C_{med}$	0.00	0.00	0.08	0.13	0.02	0.03	0.08	0.12
p-value Free = S2T$_{med} - S2C_{med}$	0.18	0.15	0.19	0.09	0.47	0.63	0.40	0.51
p-value S2T$_{low} = S2C_{low}$	0.00	0.00	0.00	0.00	0.77	0.50	0.62	0.55
p-value Free = S2T$_{low} - S2C_{low}$	0.06	0.05	0.43	0.50	0.31	0.37	0.29	0.23
p-value $S2T_{med} - S2C_{med} = S2T_{low} - S2C_{low}$	0.86	0.93	0.53	0.27	0.09	0.19	0.08	0.09
Strata FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
LASSO selected controls	No	Yes	No	Yes	No	Yes	No	Yes
R-squared	0.264	0.272	0.131	0.162	0.156	0.206	0.081	0.102
Control villages' mean	0.02	0.02	0.10	0.10	94.84	94.84	44.42	44.42
Number of observations	4,611	4,611	4,234	4,234	4,022	4,022	3,659	3,659

Notes: The outcome variable in columns (1)-(2) is whether the farmer adopted the improved seed. The outcome variable in columns (3)-(4) is whether the farmer cultivated wheat on the reference plot (extensive margin). The outcome variable in column (5)-(6) is the log of plot revenues. The outcome variable in column (7)-(8) is the log of plot profits. Impact among *would-be* buyers at medium subsidy is calculated as: $\frac{(\lambda_4-\lambda_5)-(0.67)*(\lambda_2-\lambda_3)}{0.33}$, where 0.33 is the probability of buying the seeds at stage one in the medium-subsidy villages. Standard errors in parentheses are clustered at the village level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 6: Impact on plot profits: Distinction between self-selection in medium- vs low-subsidy villages

	Plot Profits ('000 BDT/acre)		Non-positive Plot Profits
	(1)	(2)	(3)
Free distribution village (λ_1)	-6.60	-6.53	0.00
	(5.55)	(5.22)	(0.02)
Subsidy Med x S1 Buyer	-6.86	-6.09	0.03
	(5.45)	(5.28)	(0.03)
Subsidy Med x S1 Non-buyer x S2 Treat (λ_2)	-9.76	-8.53	0.07*
	(6.00)	(5.88)	(0.04)
Subsidy Med x S1 Non-buyer x S2 control (λ_3)	9.52	8.73	-0.01
	(9.25)	(8.97)	(0.03)
Subsidy Low x S1 Buyer	-17.59**	-18.55**	0.08
	(7.31)	(7.30)	(0.05)
Subsidy Low x S1 Non-buyer x S2 Treat (λ_4)	-0.42	-0.34	0.04
	(5.81)	(5.56)	(0.03)
Subsidy Low x S1 Non-buyer x S2 control (λ_5)	-5.33	-5.57	0.03
	(5.35)	(4.99)	(0.03)
Impact among *would-be* buyers at subsidy med	54.03**	50.90**	-0.13
	(24.14)	(22.83)	(0.12)
p-value S2T$_{med} = S2C_{med}$	0.03	0.05	0.07
p-value Free = S2T$_{med} - S2C_{med}$	0.24	0.31	0.14
p-value S2T$_{low} = S2C_{low}$	0.37	0.30	0.75
p-value Free = S2T$_{low} - S2C_{low}$	0.13	0.10	0.88
p-value S2T$_{med} - S2C_{med} = S2T_{low} - S2C_{low}$	0.02	0.02	0.20
Strata FE	Yes	Yes	Yes
LASSO selected controls	No	Yes	No
R-squared	0.095	0.116	0.057
Control villages' mean	44.42	44.42	0.07
Number of observations	4,024	4,024	4,024

Notes: The outcome variable in columns (1)-(2) is plot profits measured in thousands of Bangladeshi Takas per acre. The outcome variable in column (3) is the likelihood of making non-positive plot profits. The results in column (3) do not include LASSO selected controls since post-double-lasso did not select any controls for this outcome variable beyond the (non-penalized) strata fixed effects. Impact among *would-be* buyers at medium subsidy is calculated as: $\frac{(\lambda_4-\lambda_5)-(0.67)*(\lambda_2-\lambda_3)}{0.33}$, where 0.33 is the probability of buying the seeds at stage one in the medium-subsidy villages. Standard errors in parentheses are clustered at the village level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 7: Impacts on adoption and disadoption in Year 2

	(1) Year 2 Any Adoption (farm-level)	(2) Year 2 Persistent Adoption (farm-level)	(3) Year 2 New Adoption (farm-level)	(4) Year 2 Disadoption (farm-level)
Free distribution village (γ_1)	0.07*	0.12***	-0.05**	0.30***
	(0.04)	(0.03)	(0.02)	(0.03)
S1 Non-buyer x S2 Treat (γ_2)	0.09***	0.10***	-0.01	0.25***
	(0.03)	(0.02)	(0.02)	(0.03)
S1 Non-buyer x S2 Control (γ_3)	0.00	0.01	-0.01	0.02
	(0.02)	(0.02)	(0.02)	(0.02)
S1 Buyer x S2 Treat	0.17***	0.13***	0.03	0.26***
	(0.04)	(0.03)	(0.03)	(0.04)
S1 Buyer x S2 Control	0.08	0.11***	-0.03	0.21***
	(0.05)	(0.04)	(0.03)	(0.05)
Impact among *would-be* buyers	-0.04	0.23	-0.26*	0.61***
	(0.25)	(0.21)	(0.15)	(0.20)
p-value $\gamma_1 = \gamma_2 - \gamma_3$	0.64	0.54	0.11	0.07
CI: $\gamma_1 - \gamma_2 + \gamma_3$	(-0.12, 0.07)	(-0.06, 0.11)	(-0.11, 0.01)	(-0.01, 0.15)
p-value S2T$_{non_buyer} = S2C_{non_buyer}$	0.00	0.00	0.91	0.00
p-value S2T$_{buyer} = S2C_{buyer}$	0.13	0.62	0.04	0.44
Strata FE	Yes	Yes	Yes	Yes
R-squared	0.186	0.109	0.125	0.181
Control villages' mean	0.09	0.00	0.09	0.01
Number of observations	4,601	4,594	4,594	4,594

Notes: The outcome variable in column (1) is whether the farmer adopted the improved seed in year 2. The outcome variable in column (2) is whether the farmer adopted the improved seed in both years 1 and 2. The outcome variable in column (3) is whether the farmer adopted the improved seed in year 2 but not in year 1. The outcome variable in column (4) is whether the farmer adopted the improved seed in year 1 but dis-adopted in year 2. Impact among *would-be* buyers is calculated as: $\frac{\gamma_1-(0.82)*(\gamma_2-\gamma_3)}{0.18}$, where 0.18 is the probability of buying the seeds at stage one in the medium-low subsidy villages. Standard errors in parentheses are clustered at the village level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 8: Heterogeneity in predicted average treatment effects

	(1) Growing Wheat	(2) Log Revenues	(3) Revenues ('000 BDT/acre)	(4) Log Profits	(5) Profits ('000 BDT/acre)
Panel A: Unconditional free distribution at stage one					
ATE (β_1)	0.19	-0.15	-12.71	-0.17	-7.65
	(0.11, 0.25)	(-0.26, -0.04)	(-23.64, -1.82)	(-0.36, 0.02)	(-17.06, 1.81)
	[0.000]	[0.005]	[0.024]	[0.083]	[0.117]
HET (β_2)	0.92	0.79	0.57	0.69	0.43
	(0.52, 1.29)	(0.36, 1.12)	(0.14, 1.03)	(0.17, 1.17)	(-0.04, 0.91)
	[0.000]	[0.000]	[0.009]	[0.010]	[0.070]
Best ML method	Elastic Net	Elastic Net	Elastic Net	Elastic Net	Elastic Net
Panel B: Free distribution among non-buyers at stage two					
ATE (β_1)	0.13	-0.09	-8.02	-0.07	-2.86
	(0.07, 0.19)	(-0.18, -0.01)	(-18.07, 1.76)	(-0.24, 0.09)	(-10.84, 4.89)
	[0.000]	[0.065]	[0.107]	[0.400]	[0.481]
HET (β_2)	0.81	0.74	0.87	0.56	0.86
	(0.30, 1.33)	(0.25, 1.13)	(0.38, 1.35)	(-0.01, 1.11)	(0.29, 1.38)
	[0.002]	[0.003]	[0.000]	[0.040]	[0.002]
Best ML method	Random Forest	Random Forest	Random Forest	Random Forest	Random Forest

Notes: This table presents results on estimating the best linear predictor (BLP) of the conditional average treatment effects (CATE) following the approach of Chernozhukov et al. (2020). The sample in panel A is the treated farmers in the free-distribution villages and surveyed farmers in the pure control villages. The sample in panel B is the self-selected non-buyers in the stage-two treatment and stage-two control villages. Results from the best machine learning method (i.e., the algorithm with the highest prediction power) for each outcome are reported. Reported values are the median of predicted treatment effects over 100 splits. 90% confidence interval in parenthesis. P-values for the hypothesis that the parameter is equal to zero in brackets.

Table 9: Group average treatment effects of the least and most affected groups

	(1) Growing Wheat	(2) Log Revenues	(3) Revenues ('000 BDT/acre)	(4) Log Profits	(5) Profits ('000 BDT/acre)
Panel A: Unconditional free distribution at stage one					
Group 5 (20% most affected)	0.49	0.02	3.23	0.01	2.51
	(0.32, 0.68)	(-0.14, 0.21)	(-12.55, 19.52)	(-0.30, 0.32)	(-10.52, 15.53)
	[0.000]	[0.766]	[0.708]	[0.937]	[0.700]
Group 1 (20% least affected)	0.14	-0.51	-36.31	-0.66	-19.71
	(0.01, 0.28)	(-0.80, -0.22)	(-66.88, -6.69)	(-1.21, -0.07)	(-40.96, 1.26)
	[0.029]	[0.001]	[0.016]	[0.029]	[0.069]
Group 5 - Group 1	0.35	0.54	39.13	0.66	21.78
	(0.15, 0.57)	(0.21, 0.86)	(4.81, 73.65)	(0.01, 1.31)	(-3.19, 47.05)
	[0.001]	[0.002]	[0.025]	[0.047]	[0.086]
Best ML method	Elastic Net	Elastic Net	Elastic Net	Elastic Net	Elastic Net
Panel B: Free distribution among non-buyers at stage two					
Group 5 (20% most affected)	0.27	0.10	14.15	0.13	9.06
	(0.14, 0.40)	(-0.11, 0.30)	(-5.22, 33.47)	(-0.18, 0.46)	(-5.21, 22.90)
	[0.000]	[0.343]	[0.142]	[0.383]	[0.205]
Group 1 (20% least affected)	0.05	-0.21	-25.81	-0.28	-18.89
	(-0.06, 0.16)	(-0.36, -0.06)	(-44.06, -8.13)	(-0.61, 0.03)	(-35.59, -2.25)
	[0.401]	[0.007]	[0.005]	[0.091]	[0.028]
Group 5 - Group 1	0.23	0.32	39.70	0.45	27.64
	(0.07, 0.39)	(0.07, 0.55)	(16.43, 63.21)	(0.00, 0.89)	(8.03, 46.56)
	[0.004]	[0.009]	[0.001]	[0.060]	[0.004]
Best ML method	Random Forest	Random Forest	Random Forest	Random Forest	Random Forest

Notes: This table presents results on estimating ordered group average treatment effects (GATES) following the approach of Chernozhukov et al. (2020). The sample in panel A is the treated farmers in the free-distribution villages and surveyed farmers in the pure control villages. The sample in panel B is the self-selected non-buyers in the stage-two treatment and stage-two control villages. Results from the best machine learning method (i.e., the algorithm with the highest prediction power) for each outcome are reported. Reported values are the median of predicted treatment effects over 100 splits. 90% confidence interval in parenthesis. P-values for the hypothesis that the parameter is equal to zero in brackets.

A Additional Figures and Tables

Figure A.1: Wheat blast vulnerability by district during 2016-2019

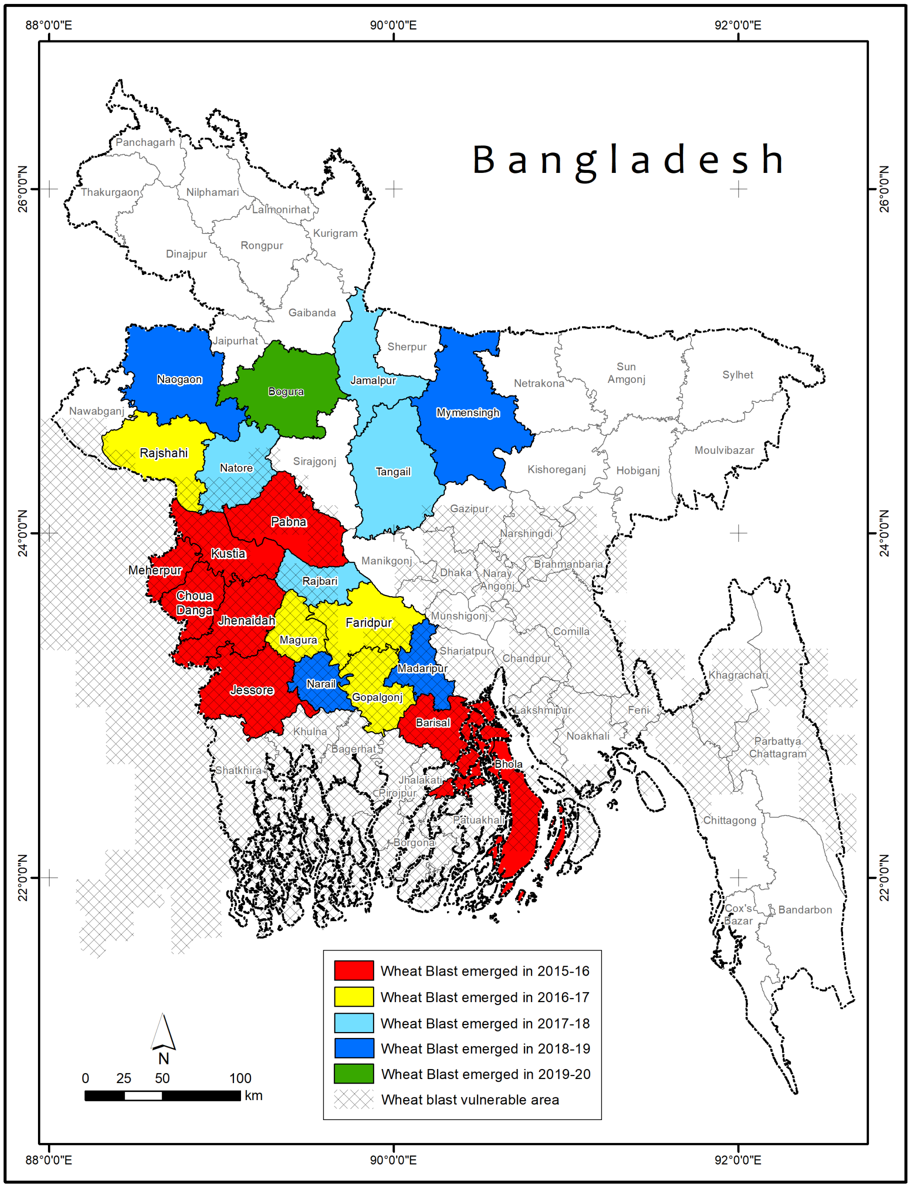
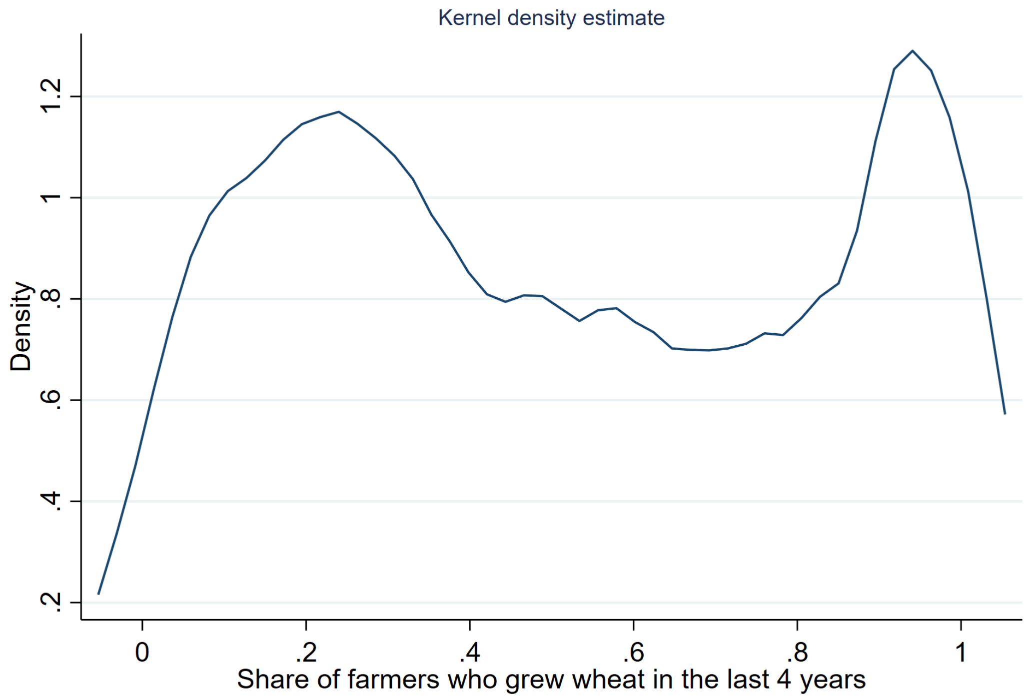
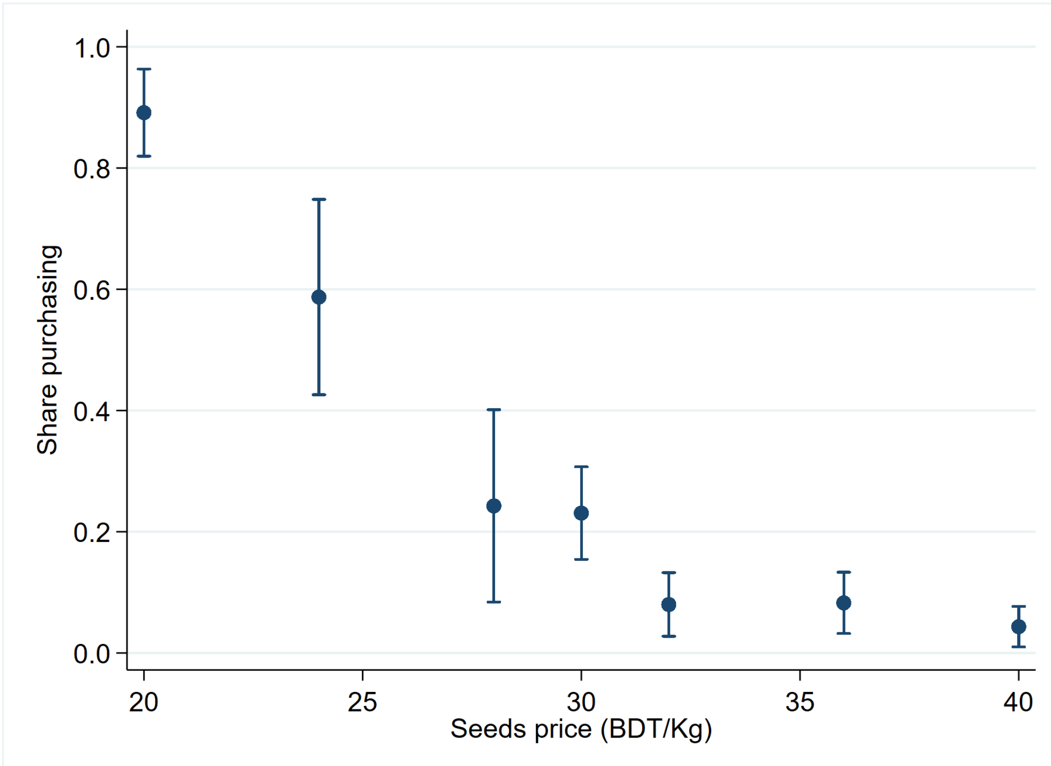


Figure A.2: Village-level wheat intensity in the sample



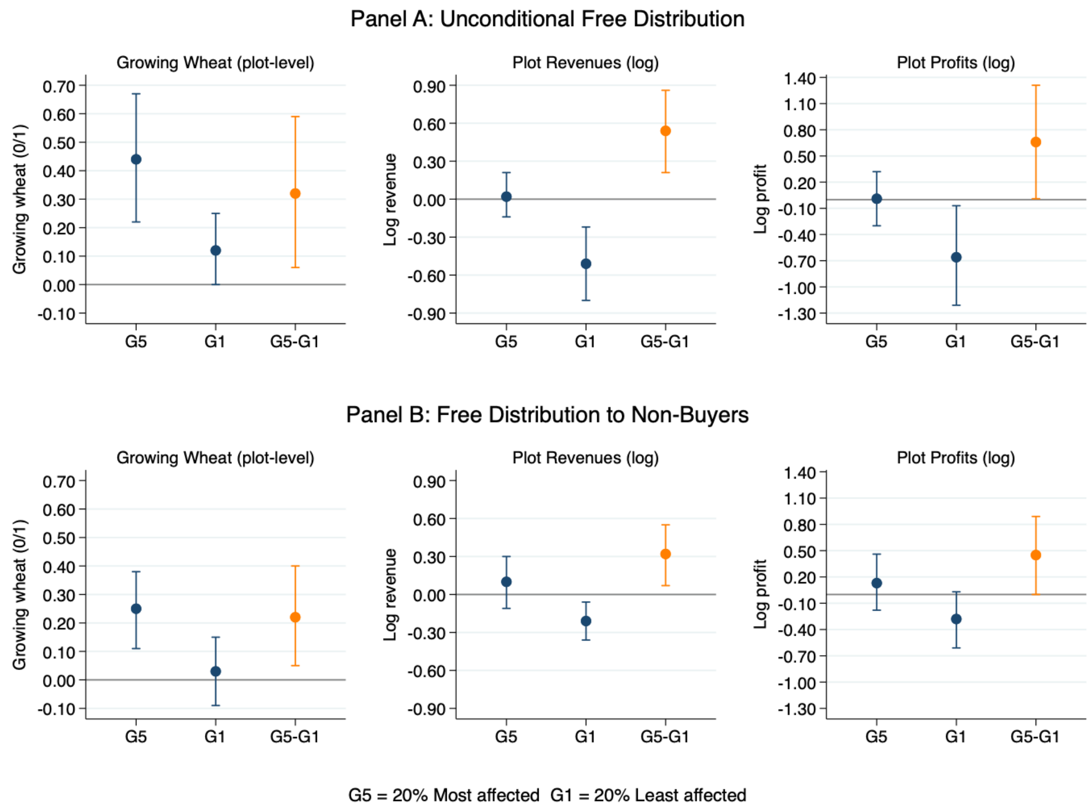
Notes: This figure shows kernel density estimate for the share of farmers in the sampled villages who grew wheat at least once over the past four years. Data on the history of wheat cultivation by all farmers in the village was collected as part of the village census. By construction, villages in which less than 10% of the farmers have ever cultivated wheat in the past few years were excluded from the sample.

Figure A.3: Seeds demand at stage-one



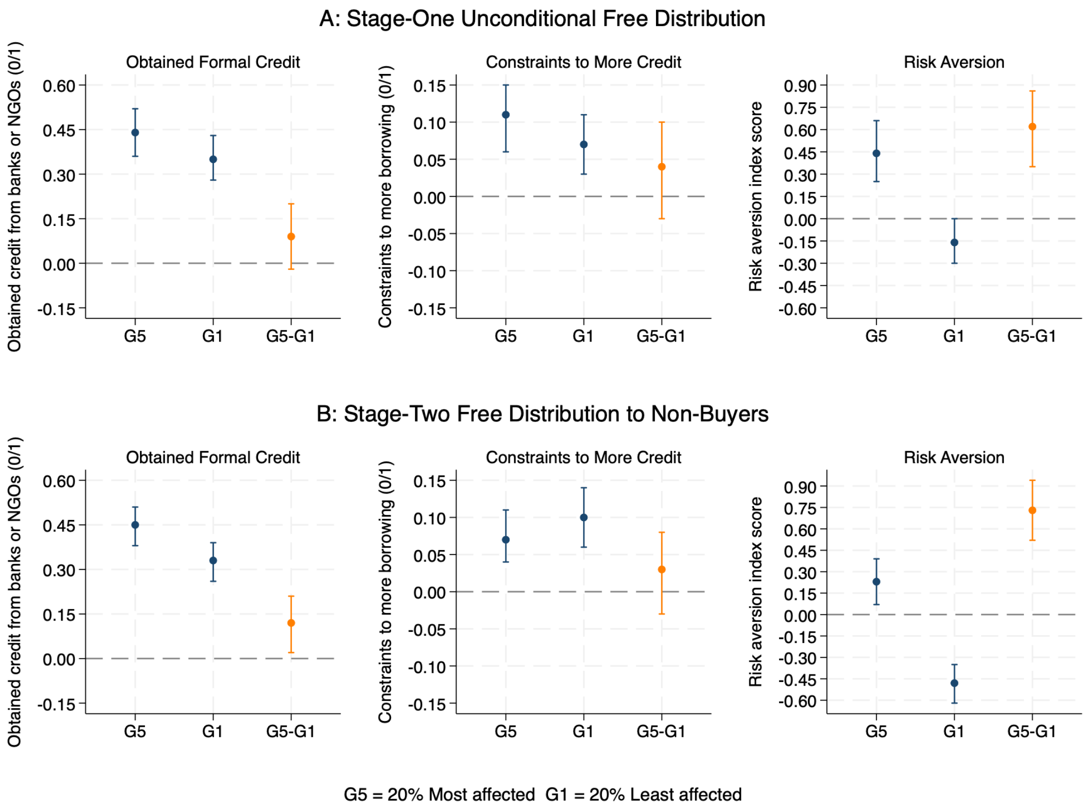
Notes: The seed price is the village-level offer price that was randomly allocated at the first stage of the experiment. Offer prices were chosen to correspond to subsidy rates ranging from 50% to 0% relative to the official price of 40 BDT/kg. Confidence intervals are at the 95% level.

Figure A.4: Sorted Group Average Treatment Effects (GATES) for the main outcomes



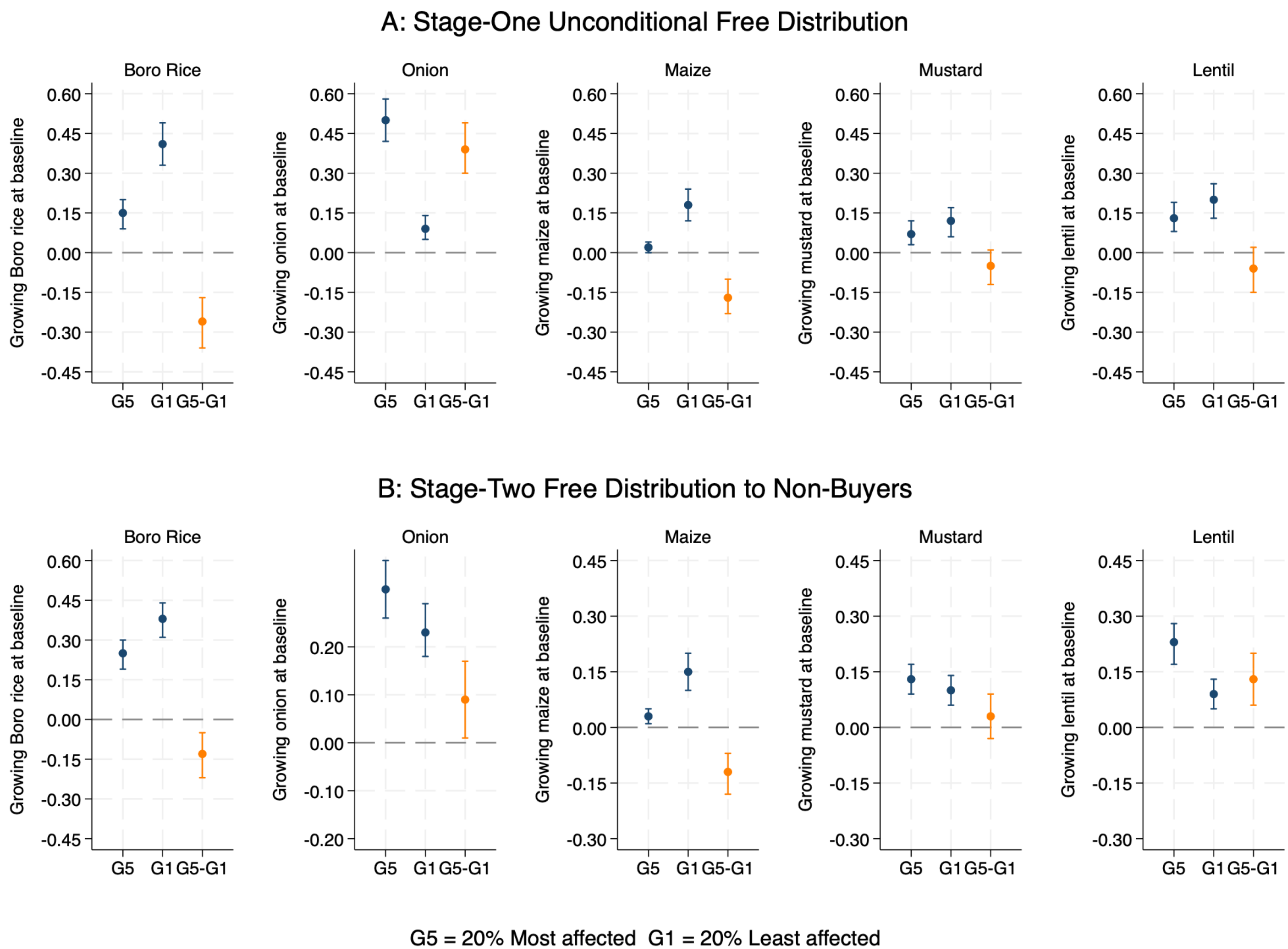
Notes: This figure summarizes results on estimating ordered group average treatment effects (GATES) for the main outcomes, as presented in Table 9. The sample in panel A is the treated farmers in stage-one free-distribution villages and surveyed farmers in the pure control villages. The sample in panel B is the self-selected non-buyers in the stage-two treatment and stage-two control villages. Confidence intervals are at the 90% level.

Figure A.5: Distinction between farmers with the highest and lowest treatment effects on profits: Risk aversion and credit constraints



Notes: This figure compares farmers with the highest versus the lowest treatment effects on plot profits in terms of the likelihood of obtaining formal credit at baseline, the likelihood of reporting constraints to borrowing more money at the given rates, and their degree of risk aversion (measured using baseline survey module on attitudes towards risk). The sample in panel A is the treated farmers in the free-distribution villages and surveyed farmers in the pure control villages. The sample in panel B is the self-selected non-buyers in the stage-two treatment and stage-two control villages. Confidence intervals are at the 90% level.

Figure A.6: Distinction between farmers with the highest and lowest treatment effects on growing wheat: The likelihood of growing common dry-season crops at baseline



Notes: This figure compares farmers with the highest versus lowest treatment effects on growing wheat in terms of the likelihood of growing a common dry-season crop on a surveyed plot at baseline. The sample in panel A is the treated farmers in the free-distribution villages and the surveyed farmers in the pure control villages. The sample in panel B is the self-selected non-buyers in the stage-two treatment and stage-two control villages. Confidence intervals are at the 90% level.

Table A.1: Balance table

Variable	Stage-one Randomization						Stage-two Randomization		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Pure control	High subsidy	Med-low subsidy	p-val (1)-(2)	p-val (2)-(3)	p-val (1)-(3)	Stage-two control	Stage-two treatment	p-val (7) - (8)
Farmer's age	46.51	45.27	44.22	0.03	0.21	0.00	43.81	44.64	0.30
Farmer's years of schooling	4.81	4.77	4.78	0.81	0.98	0.95	4.75	4.81	0.46
HH members available for farm work	1.71	1.62	1.64	0.66	0.67	0.20	1.60	1.67	0.81
Access to non-farm income (0/1)	0.32	0.29	0.28	0.56	0.41	0.25	0.28	0.28	0.90
Area of land cultivated last season (acres)	1.59	1.57	1.48	0.55	0.33	0.16	1.46	1.50	0.20
Number of plots cultivated by farmer last season	5.14	5.17	5.04	0.96	0.95	0.74	4.82	5.25	0.05
Wheat area to total farm area	0.18	0.17	0.19	0.86	0.60	0.92	0.20	0.19	0.72
Farmer grew wheat on a main plot	0.36	0.33	0.37	0.86	0.38	0.60	0.36	0.37	0.30
Farmer grew Boro rice on a main plot	0.36	0.47	0.39	0.04	0.27	0.16	0.38	0.40	0.68
Primary plot is owned by the farmer (0/1)	0.67	0.64	0.64	0.55	0.25	0.21	0.62	0.66	0.59
Primary plot's area (acres)	0.34	0.32	0.32	0.33	0.99	0.26	0.32	0.32	0.14
Plot revenues ('000 BDT/acre)	63.33	68.38	68.99	0.12	0.47	0.23	69.34	68.65	0.46
Plot profits ('000 BDT/acre)	21.23	23.37	19.97	0.49	0.22	0.72	22.58	17.38	0.09
Sample size	1,000	1,500	3,000				1,500	1,500	

Notes: This table presents summary statistics and tests for randomization balance. All variables are from baseline survey data. Plot-level characteristics refer to the plot ranked by farmer at baseline as most-suitable for wheat. Plot-level as well as farm-level outcomes refer to the most recent dry season outcomes pre-intervention. Columns (1)-(3) and (7)-(8) show sample means of the listed covariates for each arm in stages one and two of the experiment, respectively. Columns (4)-(6) and column (9) are estimated by regressing each of the listed covariates on a dummy variable for the corresponding comparison. For example, column (4) shows the p-values from regressing each covariate on an indicator for a high-subsidy treatment versus a control village. All regressions use strata fixed effects and cluster standard errors at the village level.

Table A.2: Uses of the distributed seeds

	Planted the seeds		Passed it on		Used it as food	
	(1)	(2)	(3)	(4)	(5)	(6)
	Excluding S2T	All T villages	Excluding S2T	All T villages	Excluding S2T	All T villages
Medium Subsidy [25-40%]	-0.05		0.06		0.03	
	(0.07)		(0.06)		(0.07)	
Low Subsidy [0-20%]	0.04		-0.03		-0.02	
	(0.09)		(0.05)		(0.09)	
S1 Buyer x S2 Control		-0.02		-0.01		0.04
		(0.06)		(0.04)		(0.06)
S1 Buyer x S2 Treat		0.01		0.07*		-0.08
		(0.06)		(0.04)		(0.06)
S1 Non-buyer x S2 Treat		0.00		-0.00		0.00
		(0.04)		(0.03)		(0.04)
Strata FE	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.209	0.179	0.052	0.080	0.270	0.202
p-value S2T$_{buyer} = S2C_{buyer}$		0.71		0.14		0.12
High-Subsidy Villages' Mean	0.49	0.49	0.13	0.13	0.36	0.36
Number of observations	1,599	3,046	1,599	3,046	1,599	3,046

Notes: This table presents results on the uses of the distributed seeds by treated farmers. The outcome in columns (1) and (2) is whether the treated farmer themselves planted the distributed seeds on any farm plot. The outcome in columns (3) and (4) is whether the treated farmer passed the seeds to another farmer. The outcome in columns (5) and (6) is whether the treated farmer used the distributed wheat seeds for food. Standard errors in parentheses are clustered at the village level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.3: Farmers perceive the distributed seed's yield as higher than that of other seed varieties

	(1) Excluding S2T	(2) All T villages
Medium Subsidy [25-40%]	-0.02	
	(0.06)	
Low Subsidy [0-20%]	-0.04	
	(0.07)	
S1 Buyer x S2 Control		0.02
		(0.06)
S1 Buyer x S2 Treat		0.07*
		(0.04)
S1 Non-buyer x S2 Treat		-0.01
		(0.04)
Strata FE	Yes	Yes
R-squared	0.167	0.102
p-value $\text{S2T}_{buyer} = S2C_{buyer}$		0.41
p-value $\text{S2T}_{buyer} = S2T_{non\_buyer}$		0.02
High-Subsidy Villages' Mean	0.80	0.80
Number of observations	1,601	3,074

Notes: The outcome variable is whether the farmer perceives the distributed seeds' yield as higher than the yield of other wheat seed varieties. The alternative to this outcome is that the farmer perceives the yield of the distributed seeds to be similar or lower than that of other varieties or that the farmer does not know. Standard errors in parentheses are clustered at the village level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.4: Impact on adoption and wheat cultivation

	Adoption (farm-level)		Growing wheat (farm-level)		Farm-level share of wheat area		Growing wheat (plot-level)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Free distribution village (γ_1)	0.41***	0.41***	0.29***	0.29***	0.09***	0.09***	0.18***	0.18***
	(0.04)	(0.04)	(0.04)	(0.04)	(0.02)	(0.02)	(0.04)	(0.04)
50% Subsidy village	0.29***		0.18***		0.06***		0.12***	
	(0.04)		(0.04)		(0.02)		(0.03)	
Stage 2 treatment village	0.36***		0.24***		0.08***		0.16***	
	(0.03)		(0.03)		(0.01)		(0.03)	
Stage 2 control village	0.07***		0.06*		0.02		0.05*	
	(0.02)		(0.03)		(0.01)		(0.03)	
S1 Non-buyer x S2 Treat (γ_2)		0.35***		0.23***		0.07***		0.13***
		(0.04)		(0.04)		(0.01)		(0.03)
S1 Non-buyer x S2 Control (γ_2)		0.04		0.02		0.01		0.02
		(0.03)		(0.03)		(0.01)		(0.03)
S1 Buyer x S2 Treat		0.40***		0.28***		0.09***		0.21***
		(0.05)		(0.05)		(0.02)		(0.05)
S1 Buyer x S2 Control		0.31***		0.24***		0.07***		0.15***
		(0.05)		(0.05)		(0.02)		(0.05)
Impact among *would-be* buyers		0.87***		0.64**		0.22*		0.47*
		(0.28)		(0.30)		(0.13)		(0.27)
p-value Free = S2T	0.30		0.35		0.56		0.50	
p-value $\gamma_1 = \gamma_2 - \gamma_3$		0.06		0.19		0.30		0.19
CI: $\gamma_1 - \gamma_2 + \gamma_3$		(-0.00, 0.20)		(-0.04, 0.19)		(-0.02, 0.07)		(-0.03, 0.17)
p-val S2T$_{non_buyer}$ = $S2C_{non\_buyer}$		0.00		0.00		0.00		0.00
p-value S2T$_{buyer}$ = $S2C_{buyer}$		0.18		0.47		0.56		0.29
Strata FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
LASSO controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.214	0.274	0.168	0.219	0.128	0.163	0.127	0.157
Control Villages' Mean	0.02	0.02	0.15	0.15	0.06	0.06	0.10	0.10
Number of observations	5,489	4,611	5,489	4,611	5,489	4,611	5,054	4,234

Notes: Farm-level outcomes refer to variables measured using all surveyed plots for each farmer. Plot-level outcomes, in contrast, are measured using the top-ranked plot for each farmer. A top-ranked plot is defined as the farm plot ranked by the farmer at baseline as the most suitable plot for growing wheat. The outcome variable in columns (1) and (2) is whether the farmer adopted the improved seed on a surveyed plot. The outcome variable in columns (3) and (4) is whether the farmer cultivated wheat on a surveyed plot (extensive margin). The outcome variable in columns (5) and (6) is the share of the farm area allocated to wheat (intensive margin). Finally, the outcome variable in columns (7) and (8) is whether the farmer cultivated wheat on the top-ranked plot. Impact among *would-be* buyers is calculated as: $\frac{\gamma_1-(0.82)*(\gamma_2-\gamma_3)}{0.18}$, where 0.18 is the probability of buying the seeds at stage one in the medium-low subsidy villages. Standard errors in parentheses are clustered at the village level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.5: Crowding out other wheat seeds

	Planted other wheat seeds (*non BARI Gom 33*)		Adopted *BARI Gom 33* seeds		Growing wheat (farm-level)	
	(1)	(2)	(3)	(4)	(5)	(6)
Free distribution village (γ_1)	-0.09***	-0.10***	0.41***	0.41***	0.29***	0.28***
	(0.02)	(0.03)	(0.04)	(0.04)	(0.04)	(0.04)
50% Subsidy village	-0.09***		0.29***		0.18***	
	(0.02)		(0.04)		(0.04)	
Stage 2 treatment village	-0.10***		0.36***		0.24***	
	(0.02)		(0.03)		(0.03)	
Stage 2 control village	-0.01		0.06***		0.04	
	(0.03)		(0.02)		(0.03)	
S1 Non-buyer x S2 Treat (γ_2)		-0.11***		0.35***		0.23***
		(0.02)		(0.04)		(0.04)
S1 Non-buyer x S2 Control (γ_3)		-0.02		0.03		0.02
		(0.03)		(0.03)		(0.04)
S1 Buyer x S2 Treat		-0.09***		0.39***		0.27***
		(0.02)		(0.05)		(0.05)
S1 Buyer x S2 Control		-0.07**		0.32***		0.23***
		(0.03)		(0.06)		(0.06)
Impact among *would-be* buyers		-0.15		0.85***		0.60**
		(0.19)		(0.28)		(0.30)
p-value Free = S2T	0.71		0.22		0.29	
p-value $\gamma_1 = \gamma_2 - \gamma_3$		0.76		0.07		0.23
CI: $\gamma_1 - \gamma_2 + \gamma_3$		(-0.08, 0.06)		(-0.01, 0.20)		(-0.05, 0.19)
p-value $\text{S2T}_{non\_buyer} = S2C_{non\_buyer}$		0.00		0.00		0.00
p-value $\text{S2T}_{buyer} = S2C_{buyer}$		0.44		0.29		0.55
Strata FE	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.102	0.110	0.209	0.265	0.147	0.192
Control Villages' Mean	0.14	0.14	0.02	0.02	0.15	0.15
Number of observations	5,489	4,611	5,489	4,611	5,489	4,611

Notes: The outcome variable in columns (1) and (2) is whether the farmer grew other wheat seeds (i.e., other than *BARI Gom 33* seeds) on a surveyed plot. The outcome variable in columns (3) and (4) is whether the farmer planted *BARI Gom 33* seeds on a surveyed plot. The outcome variable in columns (5) and (6) is whether the farmer cultivated wheat on a surveyed plot. Impact among *would-be* buyers is calculated as: $\frac{\gamma_1-(0.82)*(\gamma_2-\gamma_3)}{0.18}$, where 0.18 is the probability of buying the seeds at stage one in the medium-low subsidy villages. Standard errors in parentheses are clustered at the village level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.6: Plot-level impacts on adoption

	Top-Wheat Plot		Non-Top-Wheat Plot		All Sampled Plots	
	(1)	(2)	(3)	(4)	(5)	(6)
Free distribution village (γ_1)	0.25***	0.25***	0.09***	0.09***	0.15***	0.15***
	(0.04)	(0.04)	(0.01)	(0.01)	(0.02)	(0.02)
50% Subsidy village	0.18***		0.07***		0.11***	
	(0.03)		(0.01)		(0.02)	
Stage 2 treatment village	0.21***		0.08***		0.13***	
	(0.03)		(0.01)		(0.01)	
Stage 2 control village	0.04**		0.01*		0.02**	
	(0.02)		(0.01)		(0.01)	
S1 Non-buyer x S2 Treat (γ_2)		0.20***		0.09***		0.13***
		(0.03)		(0.01)		(0.01)
S1 Non-buyer x S2 Control (γ_3)		0.02		0.00		0.01
		(0.02)		(0.01)		(0.01)
S1 Buyer x S2 Treat		0.26***		0.07***		0.14***
		(0.04)		(0.01)		(0.02)
S1 Buyer x S2 Control		0.16***		0.10***		0.13***
		(0.04)		(0.03)		(0.03)
Impact among *would-be* buyers		0.58***		0.13		0.29**
		(0.25)		(0.09)		(0.11)
p-value Free = S2T	0.36		0.59		0.35	
p-value $\gamma_1 = \gamma_2 - \gamma_3$		0.14		0.66		0.16
CI: $\gamma_1 - \gamma_2 + \gamma_3$		(-0.02, 0.16)		(-0.03, 0.04)		(-0.01, 0.07)
p-value S2T$_{non_buyer} = S2C_{non_buyer}$		0.00		0.00		0.00
p-value S2T$_{buyer} = S2C_{buyer}$		0.10		0.29		0.63
Baseline plot ranking dummies	No	No	No	No	Yes	Yes
Strata FE	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.131	0.170	0.042	0.057	0.088	0.108
Control Villages' Mean	0.01	0.01	0.01	0.01	0.01	0.01
Number of observations	5,054	4,234	8,384	6,993	13,436	11,225

Notes:

1. The outcome variable is an indicator for adoption of *BARI Gom 33* seeds at the plot level. Columns (1) and (2) present the results for the top-ranked plot. A top-ranked plot is defined as the farm plot ranked by the farmer at baseline as the most suitable plot for growing wheat. Columns (3) and (4) present results for all plots in the sample other than the top-ranked plot. Finally, columns (5) and (6) pool all the plots in the sample while controlling for the baseline ranking of the plot's suitability for growing wheat. The sample includes a maximum of three main plots per surveyed farmer.

2. Impact among *would-be* buyers is calculated as: $\frac{\gamma_1-(0.82)*(\gamma_2-\gamma_3)}{0.18}$, where 0.18 is the probability of buying the seeds at stage one in the medium-low subsidy villages.

3. Standard errors in parentheses are clustered at the village level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.7: Plot-level impacts on growing wheat

	Top-Wheat Plot		Non-Top-Wheat Plot		All Sampled Plots	
	(1)	(2)	(3)	(4)	(5)	(6)
Free distribution village (γ_1)	0.18***	0.18***	0.07***	0.07***	0.11***	0.11***
	(0.04)	(0.04)	(0.02)	(0.02)	(0.02)	(0.02)
50% Subsidy village	0.11***		0.05**		0.07***	
	(0.03)		(0.02)		(0.02)	
Stage 2 treatment village	0.15***		0.05***		0.09***	
	(0.03)		(0.01)		(0.02)	
Stage 2 control village	0.03		0.01		0.02	
	(0.03)		(0.02)		(0.02)	
S1 Non-buyer x S2 Treat (γ_2)		0.13***		0.05***		0.08***
		(0.03)		(0.02)		(0.02)
S1 Non-buyer x S2 Control (γ_3)		0.01		-0.01		0.00
		(0.03)		(0.01)		(0.02)
S1 Buyer x S2 Treat		0.20***		0.04**		0.10***
		(0.05)		(0.02)		(0.02)
S1 Buyer x S2 Control		0.14***		0.08**		0.10***
		(0.05)		(0.03)		(0.03)
Impact among *would-be* buyers		0.45		0.09		0.22
		(0.28)		(0.13)		(0.14)
p-value Free = S2T	0.43		0.38		0.32	
p-value $\gamma_1 = \gamma_2 - \gamma_3$		0.26		0.79		0.36
CI: $\gamma_1 - \gamma_2 + \gamma_3$		(-0.04, 0.17)		(-0.04, 0.05)		(-0.03, 0.08)
p-value S2T$_{non_buyer} = S2C_{non_buyer}$		0.00		0.00		0.00
p-value S2T$_{buyer} = S2C_{buyer}$		0.32		0.30		0.90
Baseline plot ranking dummies	No	No	No	No	Yes	Yes
Strata FE	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.103	0.127	0.033	0.048	0.076	0.094
Control Villages' Mean	0.10	0.10	0.06	0.06	0.07	0.07
Number of observations	5,054	4,234	8,384	6,993	13,436	11,225

Notes:

1. Columns (1) and (2) replicate columns (7) and (8) in Table 3, which present the results for the top-ranked plot. A top-ranked plot is defined as the farm plot ranked by the farmer at baseline as the most suitable plot for growing wheat. Columns (3) and (4) present results for all plots in the sample other than the top-ranked plot. Finally, columns (5) and (6) pool all the plots in the sample while controlling for the baseline ranking of the plot's suitability for growing wheat. The sample includes a maximum of three main plots per surveyed farmer.

2. Impact among *would-be* buyers is calculated as: $\frac{\gamma_1-(0.82)*(\gamma_2-\gamma_3)}{0.18}$, where 0.18 is the probability of buying the seeds at stage one in the medium-low subsidy villages.

3. Standard errors in parentheses are clustered at the village level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.8: Plot-level Impact on Growing Common Dry-Season Crops

	(1) Wheat plot	(2) Boro Rice plot	(3) Onion plot	(4) Maize plot	(5) Mustard plot	(6) Lentil plot
Free distribution village (γ_1)	0.18***	-0.01	-0.09*	-0.01	-0.04	-0.03
	(0.04)	(0.04)	(0.05)	(0.02)	(0.03)	(0.03)
S1 Non-buyer x S2 Treat (γ_2)	0.13***	-0.04	-0.04	-0.03*	0.01	-0.05**
	(0.03)	(0.04)	(0.04)	(0.02)	(0.02)	(0.02)
S1 Non-buyer x S2 Control (γ_3)	0.01	0.00	0.00	-0.01	-0.00	-0.00
	(0.03)	(0.03)	(0.04)	(0.02)	(0.02)	(0.02)
S1 Buyer x S2 Treat	0.20***	-0.05	-0.12***	-0.04***	0.06*	-0.06**
	(0.05)	(0.04)	(0.04)	(0.02)	(0.03)	(0.03)
S1 Buyer x S2 Control	0.14**	-0.07	-0.03	-0.03	0.04	-0.04
	(0.05)	(0.05)	(0.06)	(0.03)	(0.04)	(0.03)
Impact among *would-be* buyers	0.45	0.17	-0.31	0.07	-0.23	0.07
	(0.28)	(0.30)	(0.34)	(0.16)	(0.19)	(0.18)
p-value $\gamma_1 = \gamma_2 - \gamma_3$	0.26	0.52	0.46	0.60	0.26	0.50
CI: $\gamma_1 - \gamma_2 + \gamma_3$	(-0.04, 0.17)	(-0.08, 0.15)	(-0.18, 0.08)	(-0.04, 0.08)	(-0.12, 0.03)	(-0.04, 0.09)
p-value $\text{S2T}_{non\_buyer} = S2C_{non\_buyer}$	0.00	0.27	0.32	0.28	0.80	0.00
p-val $\text{S2T}_{buyer} = S2C_{buyer}$	0.32	0.70	0.10	0.44	0.77	0.46
Strata FE	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.127	0.300	0.295	0.274	0.209	0.069
Control Villages' Mean	0.10	0.34	0.21	0.09	0.07	0.08
Number of observations	4,234	4,234	4,234	4,234	4,234	4,234

Notes:

1. The outcome variable is whether the farmer grew the crop in question on a top-ranked plot. A top-ranked plot is defined as the farm plot ranked by the farmer at baseline as the most suitable plot for growing wheat.

2. Impact among *would-be* buyers is calculated as: $\frac{\gamma_1-(0.82)*(\gamma_2-\gamma_3)}{0.18}$, where 0.18 is the probability of buying the seeds at stage one in the medium-low subsidy villages.

3. Standard errors in parentheses are clustered at the village level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.9: Plot-level Impact on Growing Common Dry-Season Crops: Distinction between selection in medium- vs low-subsidy villages

	(1) Wheat plot	(2) Boro Rice plot	(3) Onion plot	(4) Maize plot	(5) Mustard plot	(6) Lentil plot
Free distribution village (λ_1)	0.18***	-0.01	-0.09**	-0.01	-0.04	-0.03
	(0.04)	(0.04)	(0.05)	(0.02)	(0.03)	(0.03)
Subsidy Med x S1 Buyer	0.14***	-0.04	-0.07*	-0.02	0.03	-0.05**
	(0.04)	(0.04)	(0.04)	(0.02)	(0.02)	(0.02)
Subsidy Med x S1 Non-Buyer x S2 Treat (λ_2)	0.08*	0.03	-0.05	-0.04***	-0.00	-0.06**
	(0.05)	(0.05)	(0.06)	(0.01)	(0.02)	(0.03)
Subsidy Med x S1 Non-Buyer x S2 Control (λ_3)	-0.01	-0.02	0.07	-0.01	0.03	-0.04
	(0.04)	(0.04)	(0.06)	(0.01)	(0.03)	(0.02)
Subsidy Low x S1 Buyer	0.22***	-0.08	-0.09*	-0.06**	0.08	-0.06*
	(0.07)	(0.05)	(0.05)	(0.02)	(0.05)	(0.03)
Subsidy Low x S1 Non-Buyer x S2 Treat (λ_4)	0.17***	-0.10**	-0.04	-0.02	0.01	-0.04*
	(0.04)	(0.05)	(0.05)	(0.02)	(0.03)	(0.02)
Subsidy Low x S1 Non-Buyer x S2 Control (λ_5)	0.03	0.02	-0.05	-0.01	-0.02	0.02
	(0.04)	(0.04)	(0.05)	(0.03)	(0.03)	(0.03)
Impact among *would-be* buyers at subsidy med	0.22	-0.44**	0.26	0.01	0.14	-0.13
	(0.17)	(0.20)	(0.20)	(0.10)	(0.13)	(0.08)
p-value S2T$_{med}$ $= S2C_{med}$	0.08	0.45	0.12	0.00	0.36	0.06
p-value Free = S2T$_{med}$ $- S2C_{med}$	0.19	0.48	0.84	0.40	0.84	0.99
p-value S2T$_{low}$ $= S2C_{low}$	0.00	0.03	0.85	0.67	0.42	0.01
p-value Free = S2T$_{low}$ $- S2C_{low}$	0.43	0.11	0.13	0.84	0.17	0.37
p-val S2T$_{med}$ $- S2C_{med} = S2T_{low} - S2C_{low}$	0.53	0.05	0.16	0.70	0.25	0.25
Strata FE	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.131	0.304	0.296	0.276	0.213	0.073
Control Villages' Mean	0.10	0.34	0.21	0.07	0.09	0.08
Number of observations	4,234	4,234	4,234	4,234	4,234	4,234

Notes: The outcome variable is whether the farmer grew the crop in question on a top-ranked plot. A top-ranked plot is defined as the farm plot ranked by the farmer at baseline as the most suitable plot for growing wheat. Impact among *would-be* buyers at medium subsidy is calculated as: $\frac{(\lambda_4-\lambda_5)-(0.67)*(\lambda_2-\lambda_3)}{0.33}$, where 0.33 is the probability of buying the seeds at stage one in the medium-subsidy villages. Standard errors in parentheses are clustered at the village level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.10: Summary statistics for revenues and profits of common dry season crops

		Follow-up		Baseline	
		Mean/SD	N	Mean/SD	N
Wheat	Revenues ('000 BDT/acre)	45.52	893	32.84	1,670
		(14.26)		(10.00)	
	Profits ('000 BDT/acre)	14.29	893	-4.20	1,670
		(15.39)		(16.93)	
Boro Rice	Revenues ('000 BDT/acre)	76.27	1,642	68.68	1,335
		(16.19)		(12.32)	
	Profits ('000 BDT/acre)	29.19	1,642	27.45	1,335
		(19.26)		(16.90)	
Onion	Revenues ('000 BDT/acre)	158.52	930	187.14	493
		(53.07)		(56.57)	
	Profits ('000 BDT/acre)	75.28	930	92.12	493
		(54.38)		(61.40)	
Maize	Revenues ('000 BDT/acre)	158.92	391	82.71	434
		(31.90)		(17.64)	
	Profits ('000 BDT/acre)	102.34	391	34.82	434
		(30.07)		(22.55)	
Mustard	Revenues ('000 BDT/acre)	44.36	366	39.82	264
		(17.10)		(16.05)	
	Profits ('000 BDT/acre)	23.33	366	10.47	264
		(14.22)		(21.91)	
Lentil	Revenues ('000 BDT/acre)	51.75	236	48.33	328
		(20.10)		(14.01)	
	Profits ('000 BDT/acre)	27.92	236	20.08	328
		(19.24)		(16.72)	

Table A.11: Impact on plot profits

	Plot Profits ('000 BDT/acre)				Non-positive Plot Profits	
	(1)	(2)	(3)	(4)	(5)	(6)
Free distribution village (γ_1)	-6.24	-5.96	-6.43	-6.49	0.00	0.00
	(5.83)	(5.44)	(5.50)	(5.17)	(0.02)	(0.02)
50% Subsidy village	-2.47	-0.88			0.02	
	(5.86)	(5.51)			(0.02)	
Stage-two treatment village	-4.29	-3.39			0.05**	
	(5.06)	(4.81)			(0.02)	
Stage 2 control village	-0.72	-0.97			0.01	
	(5.66)	(5.33)			(0.02)	
S1 Non-buyer x S2 Treat (γ_2)			-4.73	-4.13		0.05*
			(5.19)	(5.00)		(0.03)
S1 Non-buyer x S2 Control (γ_3)			0.41	-0.08		0.02
			(5.91)	(5.63)		(0.02)
S1 Buyer x S2 Treat			-10.16**	-9.38*		0.04
			(5.09)	(4.92)		(0.03)
S1 Buyer x S2 Control			-12.47*	-13.31**		0.07
			(7.15)	(7.05)		(0.05)
Impact among *would-be* buyers			-12.34	-17.61		-0.15
			(39.12)	(37.25)		(0.19)
p-value Free = S2T	0.69	0.56			0.04	
p-value $\gamma_1 = \gamma_2 - \gamma_3$			0.87	0.74		0.37
CI: $\gamma_1 - \gamma_2 + \gamma_3$			(-16.41, 13.81)	(-16.84, 11.96)		(-0.11, 0.04)
p-val S2T$_{non_buyer} = S2C_{non_buyer}$			0.32	0.41		0.17
p-value S2T$_{buyer} = S2C_{buyer}$			0.72	0.54		0.50
Strata FE	Yes	Yes	Yes	Yes	Yes	Yes
LASSO selected controls	No	Yes	No	Yes	No	No
R-squared	0.095	0.130	0.084	0.106	0.029	0.055
Control Villages' Mean	44.42	44.42	44.42	44.42	0.07	0.07
Number of observations	4,817	4,817	4,024	4,024	4,817	4,024

Notes: The outcome variable in columns (1) - (4) is plot profits. Profits are measured as total revenues net of all input costs, where both revenues and costs are measured per unit area. I follow Agness et al. (2022) rule of thumb of valuing the opportunity cost of family labor at 60% of the average market wage. The outcome variable in columns (5) - (6) is an indicator of whether plot profits were non-positive (i.e., zero or negative profits). Impact among *would-be* buyers is calculated as: $\frac{\gamma_1-(0.82)*(\gamma_2-\gamma_3)}{0.18}$, where 0.18 is the probability of buying the seeds at stage one in the medium-low subsidy villages. Standard errors in parentheses are clustered at the village level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.12: Impact of *BARI Gom 33* seeds on wheat yield

	Log Yield			
	(1)	(2)	(3)	(4)
BARI Gom 33 plot	0.04	0.04	0.11*	0.10**
	(0.03)	(0.03)	(0.05)	(0.04)
Baseline yield (kg/acre)		0.00*		-0.00
		(0.00)		(0.00)
Farmer FE	No	No	Yes	Yes
Strata FE	Yes	Yes	No	No
Baseline plot ranking dummies	Yes	Yes	Yes	Yes
R-squared	0.128	0.130	0.592	0.637
Average wheat yield (kg/acre)	1,559	1,559	1,559	1,559
Number of observations	1,577	1,528	385	363

Notes: This table presents results on the impact of *BARI Gom 33* seeds on the yield of wheat plots in the sample. Columns (1) and (2) present results across all wheat plots. Columns (3) and (4) control for farmer fixed effects to compare the yield of wheat plots grown by the same farmer using different wheat seed varieties. Standard errors in parentheses are clustered at the village level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

B Alternative Mechanism: Did Stage-Two Treatment Cause an Update in Farmers' Expectations?

Under the updated expectations mechanism, farmers base their purchase decision in stage one on expected returns, but the free distribution in stage two causes farmers to update their expected returns. The net effect of stage-two treatment on farmers' expectations is ambiguous. On the one hand, farmers might perceive the free distribution at stage two as a kind of endorsement or a nudge to use a high quality seed. On the other hand, distributing seeds for free at stage two, after offering the same seeds for a positive price at stage one, might cause farmers to downgrade their expectations of the seed quality.

I investigate the updated expectations hypothesis in three ways. First, I compare the outcomes of stage-one buyers in stage-two treatment and stage-two control villages, particularly outcomes related to adoption and crop choice. These two groups self-selected into buying the seeds at similar offer prices. The main difference is that stage-two treatment increases take-up by non-buyers. Such intervention may increase spillover effects through higher treatment intensity or update farmers expectations through repeated treatment. However, Table 3 shows that the outcomes of stage-one buyers in both treatment arms is statistically similar.

Second, I show results on farmers' perceptions of whether the distributed seeds' yield is higher than that of other wheat seeds. Table A.3 shows results on perceptions for the sub-sample of farmers who took up the improved seed during intervention. In particular, I note that the difference between the coefficients on stage-one buyers in stage-two treatment and stage-two control villages is not statistically different from zero. That is, buyers in stage-two treatment villages are as likely to perceive the distributed seeds' yield to be relatively high as buyers in stage-two control villages. Moreover, a comparison between the coefficients on non-buyers and buyers in stage-two treatment villages shows that buyers in stage-two treatment villages are significantly more likely to perceive the distributed seeds' yield to be relatively high. Thus, if stage-two treatment has an impact on changing farmers' perceptions of the seed productivity, this impact is noticeable among stage-one buyers only and cannot explain the increase in adoption by non-buyers in stage-two treatment villages.

Finally, Table B.1 shows that stage-two treatment does not cause treated farmers (neither buyers nor non-buyers) to increase their input use. If farmers in stage-two treatment villages had updated their expected returns from the distributed seeds, I would expect those farmers to increase their investments in complementary agricultural inputs such as hired labor, fertilizers, and irrigation. However, Table B.1 suggests that this was not the case. On the contrary, I find that farmers in stage-two treatment villages tend to use less fertilizers and hired labor than their peers in stage-two control villages.

A caveat on the input use results is that crop-specific input requirements can differ across different crops in the sample (e.g., wheat, Boro rice, and onion). Table B.2 presents a set of regression results showing that, even after controlling for farmer fixed effects, spending on hired labor and fertilizers is significantly lower for wheat plots relative to other plots. At the same time, the realized revenues on wheat plots are significantly lower than the revenues on other plots. Hence, it is not clear whether farmers who grow wheat spend less on agricultural inputs due to low expected returns, or whether liquidity constrained farmers are more likely to grow wheat due to the low cost of growing wheat. In all cases, the hypothesis that stage-one non-buyers updated their expected returns upon receiving the new seed for free in stage two is not supported by the data.

Table B.1: Plot-level impact on input use and input spending

	(1) Hired labor (person days/acre)	(2) Hired labor spending (log)	(3) Fertilizers amount (kg/acre)	(4) Fertilizer spending (log)	(5) Irrigation times	(6) Irrigation spending (log)
Free distribution village (γ_1)	-6.91	-0.22*	20.56	0.04	0.07	-0.03
	(4.19)	(0.12)	(21.70)	(0.07)	(1.36)	(0.06)
S1 Non-buyer x S2 Treat (γ_2)	-4.34	-0.16*	-7.80	-0.05	-0.07	-0.02
	(3.58)	(0.09)	(17.05)	(0.06)	(1.07)	(0.06)
S1 Non-buyer x S2 Control (γ_3)	-0.20	-0.02	15.88	0.06	1.64	0.01
	(3.34)	(0.08)	(17.77)	(0.06)	(1.03)	(0.05)
S1 Buyer x S2 Treat	-13.26***	-0.31***	-12.45	-0.04	-0.61	-0.09
	(3.48)	(0.11)	(18.75)	(0.06)	(1.19)	(0.08)
S1 Buyer x S2 Control	-4.50	-0.16	20.74	0.06	-0.30	-0.11
	(4.49)	(0.13)	(30.26)	(0.09)	(1.31)	(0.08)
Baseline value of the outcome	0.09***	0.00*	0.13***	0.00***	0.18***	0.00***
	(0.03)	(0.00)	(0.02)	(0.00)	(0.06)	(0.00)
p-value $\gamma_1 = \gamma_2 - \gamma_3$	0.61	0.57	0.10	0.09	0.32	0.98
CI: $\gamma_1 - \gamma_2 + \gamma_3$	(-13.60, 8.06)	(-0.38, 0.21)	(-8.48, 96.96)	(-0.03, 0.33)	(-1.73, 5.28)	(-0.17, 0.18)
p-value $\text{S2T}_{non\_buyer} = S2C_{non\_buyer}$	0.24	0.13	0.13	0.05	0.14	0.56
p-value $\text{S2T}_{buyer} = S2C_{buyer}$	0.05	0.25	0.25	0.28	0.83	0.83
Strata FE	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.215	0.263	0.255	0.265	0.342	0.230
Control Villages' Mean	44.20	21,478.01	337.60	9,003.63	11.39	5,970.82
Number of observations	4,117	3,968	4,117	4,103	4,117	3,817

Notes: The outcome variables in odd columns measure input use. The outcome variables in even columns is the log of input spending, where input spending is measured in Bangladeshi Takas per acre. In columns (1) the outcome variable measures hired labor use in terms of person days per acre. In column (2) the outcome variable is the log of spending on hired labor. In column (3) the outcome variable measures fertilizer use in Kgs per acre. In column (4) the outcome variable is the log of fertilizer spending. In column (5) the outcome variable is the number of times the plot was irrigated during dry season. In column (6) the outcome variable is the log of irrigation spending. Standard errors in parentheses are clustered at the village level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table B.2: Crop-specific effects on input spending, revenues, and profits

	Hired labor spending (log)		Fertilizer spending (log)		Irrigation spending (log)		Revenues (log)		Profits (log)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Wheat plot regressions	-0.85***	-0.72***	-0.18***	-0.16***	-0.35***	-0.33***	-0.74***	-0.65***	-0.93***	-0.82***
	(0.05)	(0.04)	(0.04)	(0.03)	(0.04)	(0.04)	(0.04)	(0.04)	(0.08)	(0.08)
Boro Rice plot regressions	0.33***	0.25***	0.07	0.01	0.77***	0.83***	0.04	-0.07	-0.32***	-0.48***
	(0.05)	(0.03)	(0.04)	(0.04)	(0.05)	(0.04)	(0.04)	(0.05)	(0.07)	(0.09)
Onion plot regressions	1.19***	0.97***	0.40***	0.42***	0.22***	0.14***	1.02***	0.89***	1.20***	1.03***
	(0.05)	(0.04)	(0.04)	(0.03)	(0.04)	(0.03)	(0.05)	(0.04)	(0.09)	(0.10)
Maize plot regressions	0.07	0.06***	0.66***	0.53***	-0.44***	-0.53***	0.85***	0.80***	1.57***	1.58***
	(0.05)	(0.02)	(0.05)	(0.05)	(0.06)	(0.05)	(0.03)	(0.03)	(0.06)	(0.08)
Baseline value of the outcome	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No
Baseline plot ranking dummies	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Farmer FE	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Strata FE	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No
Mean of the outcome ('000 BDT/acre)	20.83	20.83	9.16	9.16	6.37	6.37	93.45	93.45	40.87	40.87
Number of observations	4,722	11,449	4,916	12,057	4,586	10,976	4,769	11,742	4,358	10,421

Notes: Each row of this table reports the coefficients from separate regressions of the outcome variable shown in the column heading on a dummy variable indicating whether the plot was planted with wheat, Boro rice, onion, or maize, respectively. These four crops are the most common dry season crops in the sample. Given that all the plots in the sample are mono-cropped (i.e., growing one crop at a time), the indicator variables in the row headings can be treated as mutually exclusive groups. The specifications in the odd columns limit the sample to one plot per farmer (namely, the top ranked plot for growing wheat) and control for the baseline value of the outcome as well as strata fixed effects. The specifications in the even columns include all sampled plots and control for baseline ranking of the plot's suitability for growing wheat as well as farmer fixed effects. Each of the outcome variables in the column headings was first measured in BDT per acre terms before taking logs. Standard errors in parentheses are clustered at the village level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

C Distributional Effects

In this Appendix, I examine whether the free-distribution treatment shifts particular portions of the distribution of revenues and profits. In addition, I test for the difference in distributional effects between non-buyers and average farmers. I do so by running the specification in equation (6) using quantile regressions and distribution regressions. I do not find evidence that the treatment resulted in a shift in the distribution of revenues nor profits.

Technically, a quantile regression estimates coefficients that minimize the median absolute deviation at each quantile, q. Distribution regression, on the other hand, provides an alternative approach for analyzing distributional effects even when the distribution of the outcome variable does not have a smooth density (Chernozhukov, Fernandez-Val, and Melly, 2013). This involves estimating the same regression specification of interest, while replacing the dependent variable with the probability that the outcome variable is greater than a threshold (i.e., $P(Y_{ijs} > y)$). The threshold, y, moves to cover all points in the support of the outcome variable, Y.

The quantile treatment effects on revenues do not show dramatic shifts in the distribution of revenues, neither for treated farmers in the free-distribution villages nor for non-buyers who received free seeds at stage two of the experiment.[40] Results for quantile treatment effects on plot revenues are presented in Table C.1 and summarized in panel A of Figure C.1. Treatment effects are consistently negative throughout the revenue's distribution for both groups. For non-buyers, the treatment effects on revenues are slightly lower (more negative) for farmers at lower quantiles of the revenues distribution. The distribution regression results presented in panel A of Figure C.2 show a similar trend.

Similarly, the results on plot profits do not show significant changes in the distribution of profits for both treated farmers in the free-distribution villages and non-buyers who received free seeds at stage two. This is true whether we look at the results of quantile regressions in Table C.2 and panel B of Figure C.1 or the results of distribution regressions in Figure C.2.

A distinction between treatment effects for non-buyers in the medium-subsidy versus low-subsidy villages shows two key findings. First, the distribution effects on revenues follow opposite trends for non-buyers in the medium-subsidy villages compared to non-buyers in the low-subsidy villages. Second, the distributional effects on profits for non-buyers in the medium-subsidy villages are consistently lower than that of non-buyers in the low-subsidy villages across all quantiles. For revenues, panel A of Figure C.3 shows that treatment effects among non-buyers in the medium subsidy villages reach a maximum right before the median, while for non-buyers in the low-subsidy villages treatment effects reach a minimum right at the median of the distribution. These opposite treatment effects tend to cancel each other out when looking at the treatment effects on revenues among all non-buyers. For profits, panel B of Figure C.3 shows that the treatment effects of non-buyers in the medium-subsidy villages are everywhere lower than those of non-buyers in the low-subsidy villages. At the median of the profit distribution, the difference between the treatment effects for the medium-subsidy versus the low-subsidy groups is significant at the 10-percent level (see Table C.3). This is consistent with the findings in section 4.4.

Therefore, the analysis on the distributional effects of the free-distribution treatment confirms the finding that non-buyers who received a medium-subsidy in stage one make substantially lower profits compared to non-buyers who received a low-subsidy in stage one.

[40] As explained in section 4.2, for treated farmers in the free-distribution villages the reference group is farmers in the control villages. For the non-buyers in stage-two treatment villages, the reference group is non-buyers in stage-two control villages.

Figure C.1: Quantile regressions

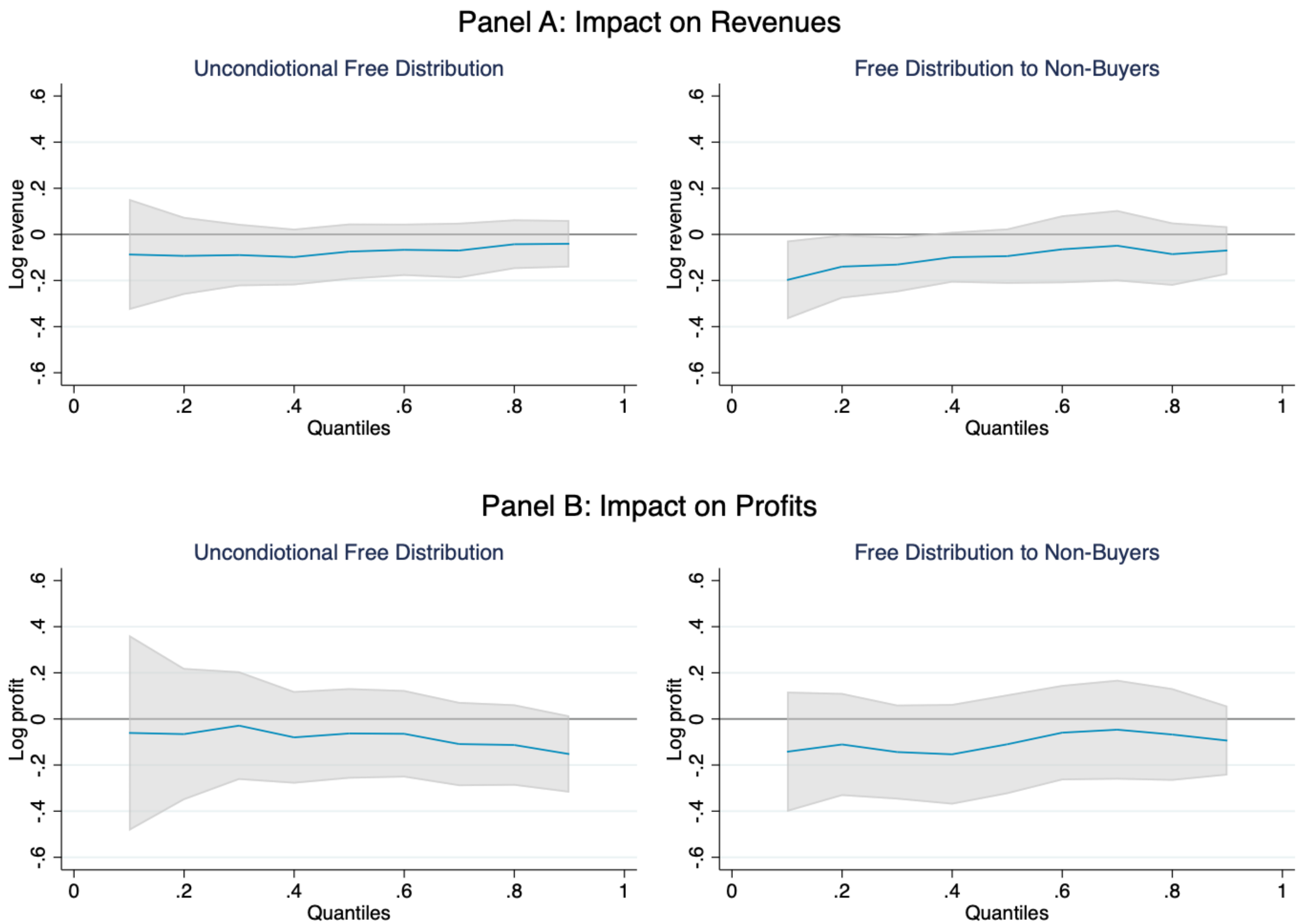


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Figure C.2: Distribution regressions

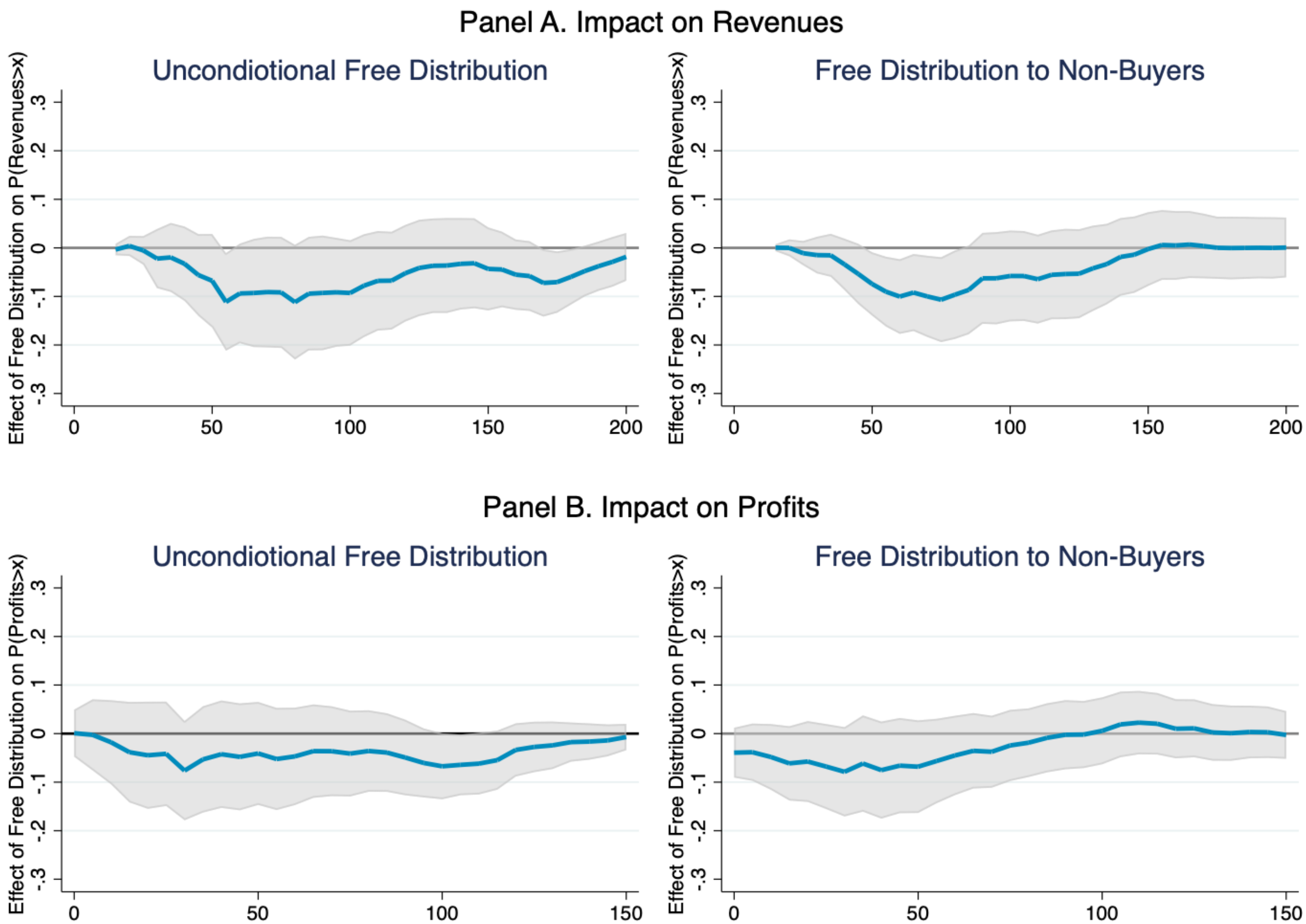

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Figure C.3: Quantile regressions: distinction between self-selection in medium- vs low-subsidy villages

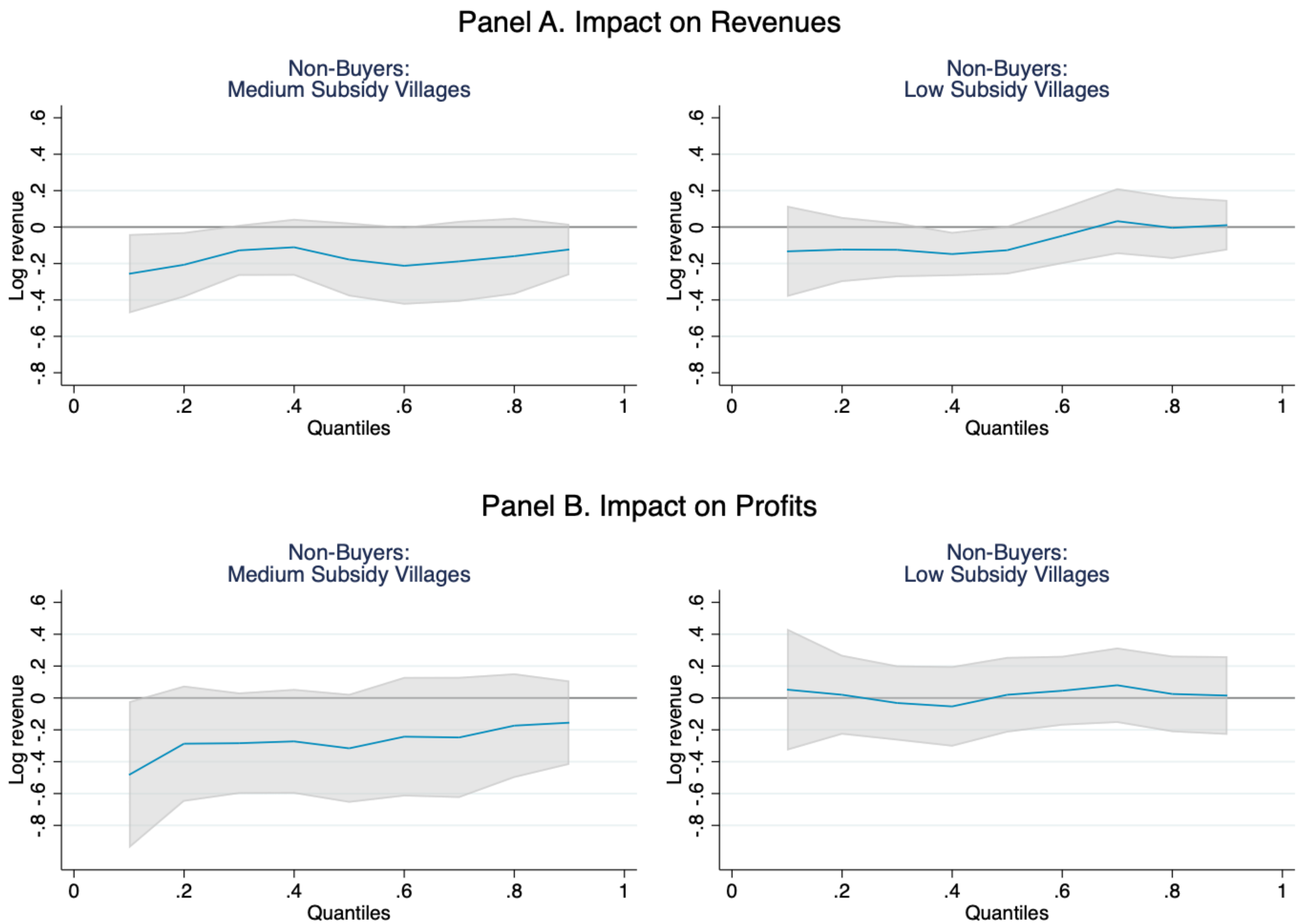

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Figure C.4: Distribution regressions: distinction between self-selection in medium- vs low-subsidy villages

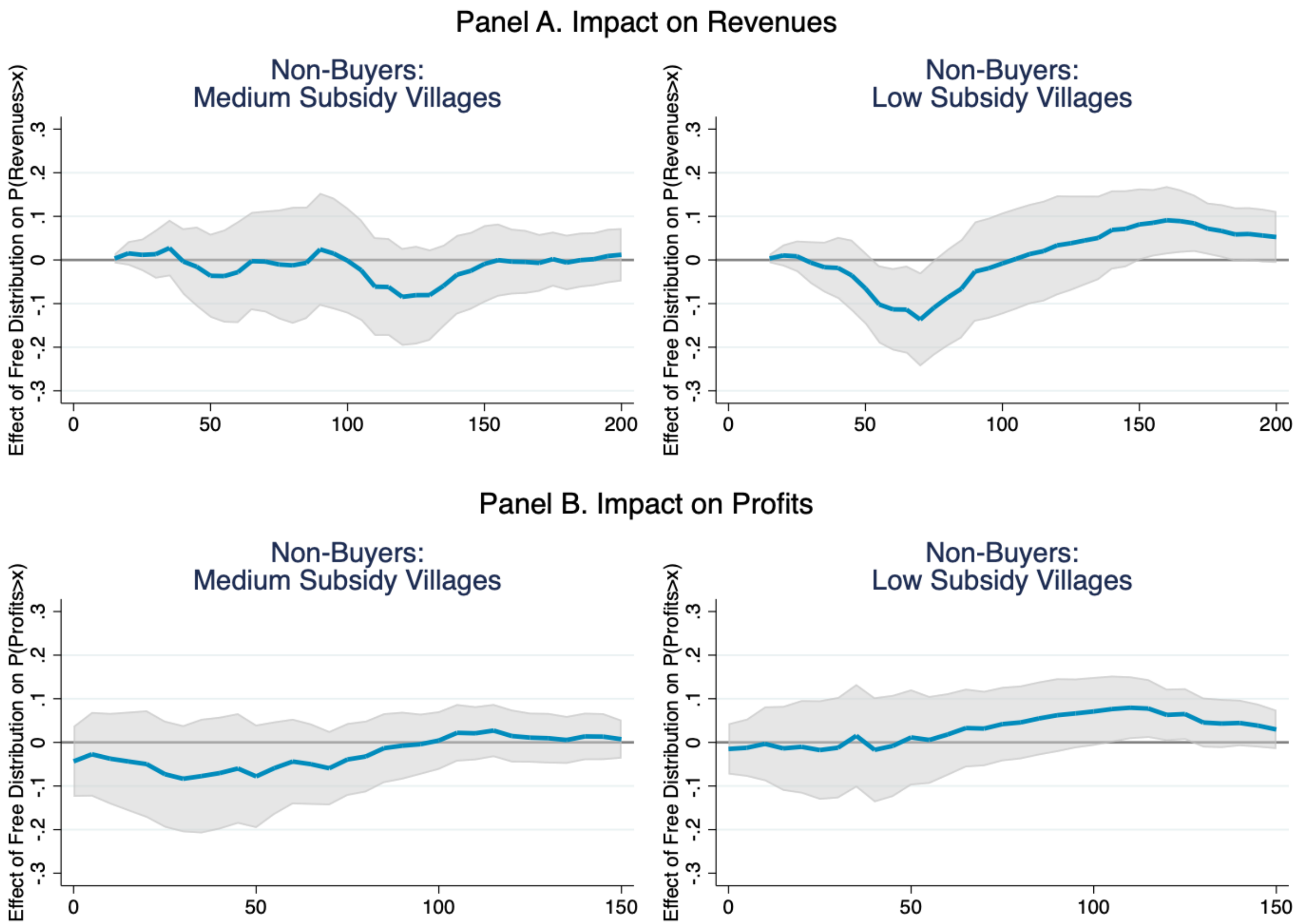

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Table C.1: Quantile regressions: Impact on plot revenues

	(1) Q10	(2) Q25	(3) Q50	(4) Q75	(5) Q90
Free distribution village	-0.09	-0.10	-0.07	-0.05	-0.04
	(0.11)	(0.07)	(0.07)	(0.07)	(0.06)
S1 Non-buyer x S2 Treat	-0.15	-0.09	-0.06	-0.02	-0.01
	(0.10)	(0.06)	(0.06)	(0.07)	(0.06)
S1 Non-buyer x S2 Control	0.05	0.03	0.03	0.04	0.06
	(0.10)	(0.07)	(0.06)	(0.07)	(0.06)
S1 Buyer x S2 Treat	-0.19*	-0.22**	-0.15**	-0.13	-0.07
	(0.10)	(0.09)	(0.07)	(0.09)	(0.08)
S1 Buyer x S2 Control	-0.15	-0.17**	-0.13	-0.05	0.07
	(0.12)	(0.08)	(0.10)	(0.09)	(0.08)
Strata FE	Yes	Yes	Yes	Yes	Yes
p-value Free = S2T$_{non_buyer} - S2C_{non_buyer}$	0.45	0.90	0.83	0.90	0.76
CI: Free - S2T$_{non_buyer} + S2C_{non_buyer}$	(-0.18, 0.40)	(-0.20, 0.22)	(-0.17, 0.21)	(-0.20, 0.23)	(-0.15, 0.21)
p-value S2T$_{non_buyer} = S2C_{non_buyer}$	0.02	0.07	0.10	0.39	0.19
p-value S2T$_{buyer} = S2C_{buyer}$	0.69	0.64	0.88	0.41	0.10
Observations	4,022	4,022	4,022	4,022	4,022

Notes: These regressions are estimated using STATA command *qrprocess*.

Bootstrapped standard errors in parentheses are clustered at the village level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table C.2: Quantile regressions: Impact on plot profits

	(1) Q10	(2) Q25	(3) Q50	(4) Q75	(5) Q90
Free distribution village	-0.06	-0.02	-0.06	-0.09	-0.15
	(0.19)	(0.14)	(0.11)	(0.10)	(0.10)
S1 Non-buyer x S2 Treat	-0.04	-0.06	-0.06	-0.01	-0.06
	(0.16)	(0.11)	(0.10)	(0.11)	(0.08)
S1 Non-buyer x S2 Control	0.10	0.06	0.05	0.08	0.03
	(0.16)	(0.11)	(0.10)	(0.12)	(0.08)
S1 Buyer x S2 Treat	-0.83**	-0.32	-0.17	-0.11	-0.19
	(0.40)	(0.29)	(0.17)	(0.16)	(0.12)
S1 Buyer x S2 Control	0.28	-0.14	-0.22	-0.12	-0.01
	(0.18)	(0.12)	(0.15)	(0.23)	(0.12)
Strata FE	Yes	Yes	Yes	Yes	Yes
p-value Free = S2T$_{non_buyer} - S2C_{non_buyer}$	0.75	0.53	0.75	1.00	0.63
CI: Free - S2T$_{non_buyer} + S2C_{non_buyer}$	(-0.42, 0.59)	(-0.19, 0.38)	(-0.25, 0.34)	(-0.32, 0.32)	(-0.30, 0.18)
p-value S2T$_{non_buyer} = S2C_{non_buyer}$	0.37	0,31	0.34	0.46	0.23
p-value S2T$_{buyer} = S2C_{buyer}$	0.00	0.54	0.78	0.97	0.18
Observations	3,659	3,659	3,659	3,659	3,659

Notes: These regressions are estimated using STATA command *qrprocess*.

Bootstrapped standard errors in parentheses are clustered at the village level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table C.3: Quantile regressions: Impact on plot revenues with distinction between self-selection in medium- vs low-subsidy villages

	(1) Q10	(2) Q25	(3) Q50	(4) Q75	(5) Q90
Free distribution village	-0.09	-0.11	-0.08	-0.07	-0.06
	(0.12)	(0.07)	(0.07)	(0.08)	(0.07)
Subsidy Med x S1 Buyer	-0.18*	-0.20**	-0.07	0.04	0.02
	(0.09)	(0.08)	(0.07)	(0.09)	(0.08)
Subsidy Med x S1 Non-buyer x S2 Treat	-0.14	-0.07	-0.01	-0.03	-0.03
	(0.11)	(0.07)	(0.07)	(0.09)	(0.08)
Subsidy Med x S1 Non-buyer x S2 control	0.11	0.11	0.16	0.18	0.10
	(0.12)	(0.09)	(0.11)	(0.12)	(0.09)
Subsidy Low x S1 Buyer	-0.20**	-0.20**	-0.23***	-0.27**	-0.21*
	(0.13)	(0.10)	(0.08)	(0.12)	(0.11)
Subsidy Low x S1 Non-buyer x S2 Treat	-0.16	-0.10	-0.14*	-0.02	0.00
	(0.10)	(0.07)	(0.07)	(0.09)	(0.07)
Subsidy Low x S1 Non-buyer x S2 control	-0.02	-0.00	-0.01	-0.07	-0.01
	(0.11)	(0.07)	(0.06)	(0.09)	(0.07)
Strata FE	Yes	Yes	Yes	Yes	Yes
p-value Free = $\mathrm{S2T}_{med} - S2C_{med}$	0.32	0.56	0.50	0.35	0.54
CI: Free - $\mathrm{S2T}_{med} + S2C_{med}$	(-0.17, 0.50)	(-0.18, 0.33)	(-0.18, 0.37)	(-0.16, 0.45)	(-0.15, 0.29)
p-value Free = $\mathrm{S2T}_{low} - S2C_{low}$	0.80	0.91	0.66	0.35	0.49
CI: Free - $\mathrm{S2T}_{low} + S2C_{low}$	(-0.31, 0.41)	(-0.22, 0.20)	(-0.15, 0.23)	(-0.36, 0.13)	(-0.25, 0.12)
p-value $\mathrm{S2T}_{med} - S2C_{med} = S2T_{low} - S2C_{low}$	0.46	0.48	0.71	0.08	0.24
Observations	4,022	4,022	4,022	4,022	4,022

Notes: These regressions are estimated using STATA command *qrprocess*.

Bootstrapped standard errors in parentheses are clustered at the village level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table C.4: Quantile regressions: Impact on plot profits with distinction between self-selection in medium- vs low-subsidy villages

	(1) Q10	(2) Q25	(3) Q50	(4) Q75	(5) Q90
Free distribution village	-0.09	-0.02	-0.07	-0.11	-0.14
	(0.22)	(0.13)	(0.11)	(0.11)	(0.09)
Subsidy Med x S1 Buyer	-0.18	-0.22	-0.17	0.01	0.01
	(0.21)	(0.15)	(0.13)	(0.15)	(0.11)
Subsidy Med x S1 Non-buyer x S2 Treat	-0.16	-0.10	-0.11	-0.00	-0.08
	(0.20)	(0.16)	(0.14)	(0.15)	(0.10)
Subsidy Med x S1 Non-buyer x S2 control	0.32	0.15	0.20	0.24	0.08
	(0.22)	(0.14)	(0.15)	(0.17)	(0.11)
Subsidy Low x S1 Buyer	-0.35	-0.25	-0.22	-0.41***	-0.18
	(0.49)	(0.20)	(0.16)	(0.15)	(0.16)
Subsidy Low x S1 Non-buyer x S2 Treat	0.05	0.01	-0.03	-0.00	0.00
	(0.17)	(0.12)	(0.13)	(0.12)	(0.09)
Subsidy Low x S1 Non-buyer: S2 control	-0.01	0.01	-0.05	-0.06	-0.01
	(0.20)	(0.14)	(0.10)	(0.13)	(0.11)
Strata FE	Yes	Yes	Yes	Yes	Yes
p-value Free = $\text{S2T}_{med} - S2C_{med}$	0.20	0.29	0.20	0.53	0.93
CI: Free - $\text{S2T}_{med} + S2C_{med}$	(-0.20, 0.99)	(-0.19, 0.65)	(-0.14, 0.63)	(-0.29, 0.56)	(-0.27, 0.29)
p-value Free = $\text{S2T}_{low} - S2C_{low}$	0.62	0.92	0.52	0.30	0.18
CI: Free - $\text{S2T}_{low} + S2C_{low}$	(-0.71, 0.42)	(-0.38, 0.35)	(-0.36, 0.18)	(-0.47, 0.14)	(-0.39, 0.07)
p-value $\text{S2T}_{med} - S2C_{med} = S2T_{low} - S2C_{low}$	0.10	0.25	0.10	0.17	0.26
Observations	3,659	3,659	3,659	3,659	3,659

Notes: These regressions are estimated using STATA command *qrprocess*.

Bootstrapped standard errors in parentheses are clustered at the village level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

D Spillover Effects

An analysis of the efficiency gains or losses from a subsidy would be incomplete if we ignored spillover effects on untreated farmers...

In this study context, there is a potential for three spillover channels. First, the subsidized technology is an improved seed variety that is expected to have a positive environmental externality by limiting the spread of a contagious crop disease. Second, the subsidized seeds can easily be reallocated or multiplied by treated farmers and shared with other farmers. Third, there is a potential for social learning through information diffusion or direct observations by neighbors of treated farmers. I do not aim to disentangle the effect of each spillover channel separately. Instead, I provide an empirical test for any spillover effects using a random sample of untreated farmers in treatment villages.[41] I start with a basic regression specification as follows:

$$Y_{ivs} = \phi_1 Treatment_{vs} * T_{ivs} + \phi_2 Treatment_{vs} + \alpha_s + \epsilon_{ivs} \tag{20}$$

where Y_{ivs} represents the outcome of interest for farmer i in village v and strata s. The primary outcomes for the analysis of spillover effects are whether the farmer grew any variety of wheat; whether the farmer adopted the improved wheat variety; whether the farmer shared wheat seeds with other farmers.[42] $Treatment_{vs}$ is a dummy variable for treatment villages, while pure control villages are the omitted category. T_{iv} is an indicator for a randomly selected treatment farmer in a treatment village. The term α_s represents strata fixed effects and ϵ_{ijs} is a random error term.[43] Standard errors are clustered at the village level. Testing for $\phi_2 = 0$ should indicate if there are spillover effects across all treatment villages.

Next, I test for differential spillover effects across villages that received different treatment interventions by extending the ITT specification in equation (5) as follows:

$$\begin{aligned} Y_{ijs} = {} & \beta_1 Subsidy^{High}_{js} * T_{ijs} + \beta_2 Subsidy^{50}_{js} * T_{ijs} + \beta_3 StageTwo^{Treat}_{js} * T_{ijs} + \beta_4 StageTwo^{Control}_{js} * T_{ijs} \\ & + \beta_5 Subsidy^{High}_{js} + \beta_6 Subsidy^{50}_{js} + \beta_7 StageTwo^{Treat}_{js} + \beta_8 StageTwo^{Control}_{js} + \alpha_s + \epsilon_{ijs} \end{aligned} \tag{21}$$

Given the difference in the intervention received by stage-two treatment and stage-two control villages, I expect the spillover effects in these villages to differ as well. For example, the reallocation channel might be stronger in stage-two treatment villages, since a portion of the treated farmers received free seeds after deliberately choosing not to buy the seeds at stage one. Re-allocation could take place among treated farmers as well as between treatment and control farmers in treatment villages. As before, T_{ivs} is an indicator for a randomly selected treatment farmer, regardless of the farmer's seed purchasing decision at stage one. A test for $\beta_5 = \beta_7$ should indicate if there is a difference in spillover effects across villages with similar take-up rates but different interventions. On the other hand, a test for $\beta_7 = \beta_8$ should indicate if there is a difference in spillover effects across villages that received similar offer prices at stage one.

Table D.1 presents results on spillover effects on adoption and the extensive margin of growing wheat in years 1 and 2. In year 1, column (1) shows that the spillover effects on adoption are weak. The

[41] As explianed in Section 2.2, a random sample of 8 un-treated farmers was selected in each of the 180 treatment villages. This makes a within-treatment controls sample of 1,440 farmers. In addition to the initial sample size of 5,500 farmers, the total sample size including the within treatment controls is 6,940 farmers. The size of the within-treatment control sample was constrained by the survey budget.

[42] I do not aim to use plot-level outcomes in the analysis of spillover effects since it was not possible to collect baseline data for the sub-sample of within-treatment control farmers. The baseline data included farmers' ranking of their plots' suitability for growing wheat, which was used to select the reference plot for plot-level outcomes as explained in Section 4.2.

[43] See Section 2.2 for an explanation of treatment stratification.

only treatment arm that has significant spillover effects on adoption is the 50-percent subsidy villages, as shown in column (3). Interestingly, column (5) shows strong spillover effects on the likelihood of growing wheat in year 1. This spillover effect is driven by significant spillover effects in stage-two treatment villages, as shown in column (7). One potential explanation for the significant spillover effects on wheat cultivation but not on adoption is through farmers' coordination of the type of crop to grow on neighboring plots –a common phenomenon in settings characterized by fragmented farm plots as in the study setting.

Table D.1 and Table D.2 show a significantly negative spillover effect on adoption in year 2. Columns (3) and (4) of Table D.2 show that the negative spillover effect on adoption in year 2 is driven by a negative effect on new adoption in year 2 by the within-treatment control farmers that is consistent across all treatment arms. At the same time, year 2 shows significant spillover effects on disadoption that is driven by the spillover effect in the 50-percent subsidy villages. Therefore, the positive spillover effects on adoption in the 50-percent subsidy villages in year 1 is reversed through significant spillover effects on disadoption in year 2.

The negative spillover effects on adoption in year 2 can be explained by social learning. The results in Section 4.3 show that the average returns to adopting the improved wheat seed in the sample are low, which could explain the negative spillover effects on adoption in year 2 as well as the significant disadoption in year 2.

Table D.1: Spillover Effects on Adoption and Wheat Cultivation

	Adoption (farm-level)				Growing wheat (farm-level)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Year 1	Year 2	Year 1	Year2	Year 1	Year 2	Year 1	Year 2
Treatment village x Treated farmer	0.23***	0.13***			0.11***	0.02		
	(0.02)	(0.02)			(0.02)	(0.01)		
Treatment village	0.03	-0.07***			0.06**	0.03		
	(0.02)	(0.02)			(0.03)	(0.03)		
Free distribution x Treated farmer			0.37***	0.13***			0.21***	0.02
			(0.05)	(0.04)			(0.05)	(0.03)
Free distribution village			0.03	-0.07***			0.08	0.02
			(0.03)	(0.02)			(0.05)	(0.05)
50% Subsidy x Treated farmer			0.23***	0.11***			0.14***	0.00
			(0.04)	(0.04)			(0.04)	(0.03)
50% Subsidy village			0.06**	-0.04			0.05	0.05
			(0.03)	(0.03)			(0.05)	(0.04)
S2 Treat x Treated Farmer			0.35***	0.16***			0.14***	0.03
			(0.03)	(0.03)			(0.03)	(0.02)
Stage 2 treatment village			0.01	-0.07***			0.11***	0.05
			(0.02)	(0.02)			(0.04)	(0.04)
S2 Control x Treated farmer			0.05***	0.10***			0.03	0.01
			(0.02)	(0.02)			(0.02)	(0.01)
Stage 2 control village			0.02	-0.09***			0.03	0.00
			(0.02)	(0.02)			(0.04)	(0.04)
Strata FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.136	0.160	0.218	0.167	0.111	0.182	0.140	0.185
p-value Free = S2T			0.49	0.89			0.54	0.63
p-value S2T = S2C			0.92	0.49			0.05	0.27
Control Villages' Mean	0.02	0.09	0.02	0.09	0.15	0.21	0.15	0.21
Number of observations	6,929	6,916	6,929	6,916	6,929	6,916	6,929	6,916

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table D.2: Spillover Effects on Adoption and Wheat Cultivation

	Year 2 Persistent Adoption		Year 2 New Adoption		Year 2 Disadoption	
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment village x Treated farmer	0.07***		0.05***		0.15***	
	(0.01)		(0.01)		(0.01)	
Treatment village	0.00		-0.07***		0.03*	
	(0.01)		(0.02)		(0.01)	
Free distribution x Treated Farmer		0.11***		0.02**		0.26***
		(0.04)		(0.01)		(0.04)
Free distribution village		0.00		-0.07***		0.03
		(0.01)		(0.02)		(0.02)
50% Subsidy x Treated Farmer		0.08***		0.03*		0.15***
		(0.03)		(0.02)		(0.02)
50% Subsidy village		0.01		-0.05**		0.05**
		(0.02)		(0.02)		(0.02)
S2 Treat x Treated Farmer		0.10***		0.06***		0.25***
		(0.02)		(0.02)		(0.03)
Stage 2 treatment village		-0.00		-0.07***		0.02
		(0.01)		(0.02)		(0.02)
S2 Control x Treated Farmer		0.03***		0.07***		0.02*
		(0.01)		(0.02)		(0.01)
Stage 2 control village		-0.00		-0.08***		0.02
		(0.01)		(0.02)		(0.02)
Strata FE	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.081	0.097	0.103	0.105	0.086	0.146
p-value Free = S2T		0.69		0.95		0.52
p-value S2T = S2C		0.96		0.40		0.88
Control Villages' Mean	0.00	0.00	0.09	0.09	0.01	0.01
Number of observations	6,908	6,908	6,908	6,908	6,908	6,908

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$