Pre-analysis Plan

Pricing and Allocative Efficiency of A New Agricultural Technology

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Abstract

Modern agricultural technologies can increase productivity and enhance resilience to climate shocks. The efficiency of subsidies to boost technology adoption is debatable. Subsidization of agricultural technologies could be justified in the presence of externalities (e.g., environmental or learning externalities) or other market failures. Nevertheless, higher prices could improve targeting through a screening effect. This research study aims to analyze the allocative efficiency of higher prices by focusing on a new agricultural technology – improved wheat seed – that has been recently introduced in Bangladesh. Using a randomized controlled trial (RCT), we examine whether charging higher prices causes farmers with lower actual returns to choose not to adopt. One innovation of the study is that we use a two-step experiment to estimate whether actual returns are lower among farmers that are unwilling to pay higher prices.

Timeline: The table below summarizes the timeline of the main activities.

Study Timeline

Activities	**Period**
Pilot survey	August-September 2021
Village census	September 2021
Market survey	September 2021
Baseline survey	October - November 2021
Treatment intervention	November 2021
First follow-up	May 2022
Second follow-up	April-May 2023

1 Introduction

Modern agricultural technologies represent an ideal case for examining the efficiency of price subsidies as a policy tool for increasing productivity. Expected economic gains from new agricultural technologies could make subsidization attractive for boosting adoption. For instance, Gollin et al. (2021) estimate that delaying the introduction of Green Revolution technologies by ten years would have resulted in a cumulative loss of $83 trillion to global GDP.[1] Nevertheless, agricultural subsidy programs are often criticized for their relatively high costs and questionable targeting of beneficiaries. Recent evidence from the second wave of input subsidy programs in Sub-Saharan Africa shows that agricultural subsidies can exhaust more than 25% of public spending on agriculture, and yet fail to target farmers who are expected to benefit from subsidy programs the most (Jayne et al. (2018)). This raises an important question: do lower agricultural subsidies induce self-targeting of farmers with higher marginal returns? That is, absent of precise eligibility criteria, are there allocative efficiency gains from lowering subsidies that can offset any losses from reduced adoption?

Two opposing forces are at play when policy makers decide whether to subsidize a new agricultural technology. On the one hand, there can be a strong case for subsidization due to positive externalities (both environmental and learning externalities) or constraints to adoption (such as credit, insurance, and information constraints).[2] On the other hand, subsidies represent a burden on public spending and are prone to waste and leakage (Pan and Christiaensen (2012)). A subsidy to a new agricultural technology could result in low average returns if farmers put less effort in using the subsidized technology, or if the subsidy is allocated to farmers with low comparative advantage.

In this paper we examine the allocative efficiency of subsidizing an agricultural technology by testing whether *actual returns* are lower for farmers who are unwilling to pay a randomly allocated price. The technology in this study is an improved wheat seed that is introduced in a context where farmers make simultaneous decisions on which crop (or crop combination) to grow and the type of seed variety to adopt. The new seed is expected to be profitable for farmers with a comparative advantage in growing wheat, since it is proven to be resistant to major crop diseases and can result in relatively higher yield compared to existing wheat varieties. However, farmers' actual returns to adopting the new seed depend not only on factors known to the farmer before planting but also on determinants of returns that farmers might not take into account (e.g., soil characteristics) as well as stochastic shocks. Hence, prices are expected to have a selection effect only if the determinants of returns that are known to the farmer ex-ante represent a large share of the variability of returns.

To test for this selection effect, we apply a two-stage experimental design that is executed in one agricultural season before planting starts. In the first stage, we randomly allocate villages to different subsidy levels, ranging from full subsidy to zero subsidy. A random sample of farmers in each village are offered to buy the seed at the village-level subsidy rate. In the second stage, we randomly allocate a subset of non-purchasing farmers from the first stage to receive the seed for free. We will estimate treatment outcomes from two rounds of follow-up surveys at the end of two consecutive wheat harvesting seasons.

The two-stage experiment allows us to estimate two parameters of interest. First, stage-one ran-

[1] Gollin et al. (2021) define the Green Revolution as the introduction of improved seed varieties as a result of advancements in modern crop-breeding techniques.

[2] Learning externalities from agricultural technology adoption have been documented in the literature (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, the time for making agricultural investment decisions (Field et al. (2013); Fink et al. (2020); Karlan and Mullainathan (2010)). This is in addition to the lack of complete insurance markets, which could hinder farmers from making optimal investments (Cole et al. (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 et al. (2021)).

domization gives the treatment effect over the whole population. Second, stage-two randomization gives the treatment effect among the farmers who decide not to buy the seed at the offer price. From these two parameters, we compute the treatment effect for the sample of buyers in stage one. Our two-stage experimental design thus allows us to compare the actual returns between farmers that are willing to pay for seeds and those that are not – at different subsidy levels.

Knowing whether selection into adoption is based on actual returns (vs. perceived returns or other factors) has important implications for subsidy policy. For example, subsidies may be inefficient if the farmers who choose not to buy the seed are those with relatively lower returns. On the other hand, ability to pay may be less than willingness to pay, causing self-selection to be less effective. Alternatively, learning over time could cause realized returns to differ from perceptions of expected returns pre-adoption. Thus, evaluating the allocative efficiency of agricultural subsidies requires analyzing how actual returns vary by purchasing decisions at different prices.

Besides, we will apply machine learning techniques to analyze heterogeneity in returns conditional on farmers' baseline characteristics. By training an algorithm on the sub-sample of villages that receive a 100% subsidy in stage one, we will predict conditional average treatment effects (CATE) for the entire population. We will use the predicted CATE to test for treatment effect heterogeneity using the procedure in Chernozhukov et al. (2018). We expect treatment effects to be heterogeneous across farmers for several reasons. The opportunity cost of a farmer adopting a new wheat seed include not only growing a different seed variety, but also growing other crops, or taking up seasonal or non-agricultural activities. Farmers access to and returns from different outside options would depend on their characteristics. CATE estimates will tell us whether observable characteristics can predict heterogeneity in farmers' returns. Importantly, we will examine whether farmers' purchasing decisions are correlated with the same characteristics that can predict their actual returns. That is, we will test whether farmers with higher CATE are more likely to adopt the improved seed at higher prices (i.e., lower subsidy rates). A test for positive selection using predicted CATE would reinforce our analysis using actual returns from the two-stage experiment. Predicted CATE resembles observable information available for farmers when estimating expected returns pre-adoption.

Subsidizing agricultural technology might have indirect effects. Two channels for spillovers are particularly important in our context: informal reallocation of the subsidized seeds and information diffusion. Treated farmers can transfer seeds to untreated farmers either before planting, or after harvesting by sharing part of their harvest with fellow farmers as seeds to be stored and planted in the next agricultural season. This informal reallocation might improve the allocative efficiency of untargeted subsidies, particularly if the subsidized seeds are reallocated to farmers with relatively high returns. Similarly, information could diffuse to neighbors of treated farmers by raising awareness of the existence of an improved seed variety or by providing signals on the expected returns to adoption. Thus, an analysis of the full impact of a subsidy will be incomplete if we neglected the potential for these spillovers. In our study, we will estimate spillover effects by comparing the outcomes for a random sample of untreated farmers in treatment villages to that of farmers in control villages. We will make this comparison across the different subsidy levels., using the subsidy as instrument for take up to examine whether higher take up induces greater spillover effects.

Our paper contributes to the literature on how prices allocate goods with potential externalities. Several studies have focused on subsidies for preventative health products and have shown mixed results. Evidence in support of a full subsidy were found in contexts where private gains are much lower than social benefits (Kremer and Miguel (2007)) or where price elasticity of demand is very high even at low prices (Cohen and Dupas (2010)). In contrast, other studies have shown that prices have a selective ability such that buyers with higher willingness-to-pay (WTP) are more likely to use the product (Ashraf et al. (2010)) and that the marginal benefits are increasing in the buyer's WTP (Berry et al. (2020)). We contribute to this literature by looking at the selective ability of prices in a context where the new technology is an agricultural input with potentially heterogeneous returns – an area where subsidies are widespread, but little is known about their allocative efficiency.

Self-selection as measured by willingness-to-pay (WTP) or willingness-to-accept (WTA) has also been analyzed in the context of agricultural investments. Farmers were found to self-select into loan take-up based on their returns to capital (Beaman et al. (2020)). Similarly, the benefits of a new agricultural technology such as laser land leveling were found to be increasing with farmers' WTP (Lybbert et al. (2017)). On the supply side, WTA as measured by reverse auctions has proven to be an effective mechanism for targeting conservation investments such as land-use subsidies and ecosystem service contracts (Jack (2013); Jack et al. (2009)). Our experiment uses a two-stage experimental design similar to that of Beaman et al. (2020) to explore whether lowering agricultural subsidies leads to efficient sorting.

Furthermore, our study contributes to the literature on adoption of agricultural technologies in developing countries. Several explanations for low adoption have been offered in the literature, including informational constraints (Ashraf et al. (2009); Carter et al. (2021); Hanna et al. (2014)), behavioral constraints (Duflo et al. (2011)), and heterogeneity in transaction costs (Suri (2011)). The paper that is closest to ours is that of Suri (2011), which uses panel data to show that heterogeneity in transportation costs affects farmers' comparative advantage in adopting fertilizers, implying that unadoption can be explained by low comparative advantage. We provide an experimental test of whether high prices act as a barrier preventing adoption by high-return farmers, or whether high prices allocate technology to farmers that will benefit from the technology the most.

Evidence on the impact of agricultural input subsidies from randomized controlled trials (RCT) are quite rare. A recent study by Carter et al. (2021) used an RCT for evaluating a one-off input subsidy package targeting "progressive" maize farmers in Mozambique. They find that the subsidy package increased agricultural technology adoption, improved maize yields, and had positive spillover effects on untreated "progressive" farmers in their sample frame. These findings are in line with the results from a similar RCT evaluating a targeted intervention package known as the Wheat Initiative in Ethiopia (Abate et al. (2018)). Giné and Ribeiro (2019) focus on equity-efficiency tradeoff in the targeting of subsidized fertilizers in Tanzania. They find that the significant impact of the subsidy on farmers' productivity disappears after accounting for ex-ante differences between targeted farmers and control farmers. We add to this literature by evaluating the allocative efficiency of an input subsidy on a general population of farmers, while taking into consideration potential spillover effects.

2 Research Design

2.1 Study Context

Our experiment takes place in Bangladesh, the fifth largest wheat importer in the world. The country's annual wheat imports are in the range of six million tons (USDA (2021)). Recent growth in Bangladesh wheat imports reflects an increase in demand, due to increases in domestic demand as well as increases in wheat-based exports, accompanied with a reduction in wheat production.[3] Over the past few years, Bangladesh has faced a drop in wheat area due to a number of factors including the emergence of a devastating crop disease referred to as wheat blast.

The wheat blast is a fungal seed disease that first emerged in Brazil in the 1980s, and has spread to several 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

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

(Figure 1). Reported blast-related losses reached 51% of total field output CIMMYT (2019)). The fact that wheat blast can spread through wind-blown spores makes it highly contagious. Once the blast attacks a plant, it can deform the grain in less than a week from the first symptoms. 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. A short-term policy response by Bangladesh Ministry of Agriculture was to discourage farmers from cultivating wheat in blast-prone districts to limit blast spread.[4] As a result, wheat area in the affected districts has dropped sharply (Figure 2).

As part of the Bangladesh government's support for the production of improved seed varieties, the Bangladesh Agricultural Research Institute (BARI) collaborated with the International Maize and Wheat Improvement Center (CIMMYT) to produce a blast-resistant wheat seed called BARI Gom 33. BARI Gom 33 is proven to be resistant to wheat blast based on field trials in Bangladesh and Bolivia, and greenhouse tests by the US Department of Agriculture (CIMMYT (2021)). Even in the absence of a blast outbreak, BARI Gom 33 is also resistant to other major diseases, such as leaf blight and leaf rust, and is expected to result in 5-8% higher yield relative to other varieties (Mottaleb et al. (2019)). In addition, the new seed is fortified with zinc, an important micronutrient given the high levels of zinc deficiency in Bangladesh (Akhtar (2013)). BARI Gom 33 was first released in the fall of 2017. Nevertheless, the seed is still at early stages of dissemination. A short market survey that was carried out at the sub-districts in our sample showed that merely 8% of the retailers are selling BARI Gom 33 seeds.[5]

Although Bari Gom 33 is expected to have positive impacts on wheat productivity, not all farmers have the same comparative advantage in growing wheat. Alternative crops that farmers could grow during winter season (the dry season in Bangladesh) include Boro rice, maize, onion, mustard, lentil, etc. Figure B.2 in the Appendix shows the share of different dry-season crops in our sample at baseline for each sub-district. A number of factors could affect the degree of substitution between these alternative crops. Limited access to irrigation could prevent farmers from growing irrigation-intensive crops such as rice or sugar cane. Credit or liquidity constraints can limit farmers' ability to grow cash crops due to their high seed costs. Also, information constraints could result in limited knowledge about farm management techniques for different crops. The drop in total wheat area shown in Figure 2 suggests that farmers are self-selecting whether to continue growing wheat on a specific plot or to switch to other crops or other income generating activities.

Subsidization of improved seeds represents an interesting setting for studying allocative efficiency. Expected returns from the improved wheat variety depend on a number of attributes such as plot characteristics, access to capital, and farmer's skills. A profit maximizing farmer would base their adoption decision on their evaluation of the expected returns from adopting the improved seed relative to an idiosyncratic set of outside options (e.g., growing a replacement crop, fallowing or renting out the plot, or taking up a seasonal or non-agricultural activity). This relative profitability is what determines a farmer's comparative advantage in growing the new seed. Prices (or price subsidies) are said to be allocatively efficient if farmers self-select into adoption at the given price based on their comparative advantage. This study aims to examine the allocative efficiency of price subsidies in the context of an improved wheat seed variety in Bangladesh.

[4] A similar policy was followed 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)).

[5] Baseline data shows that only 1% of sampled farmers who grew wheat at baseline reported using BARI Gom 33 seeds. This is expected given the limited supply of the new seed variety in our study area.

2.2 Experimental Design

The field experiment is implemented in seven districts in Bangladesh with different degrees of exposure to wheat blast.[6] The total sample size is 5,500 farmers from 220 villages. Villages were randomly selected from twelve upazilas (sub-districts), while targeting regions that commonly grow wheat. We first did a village census at the beginning of September 2021. The village census covered around twenty thousand farmers from the 220 villages. This census data was used to randomly select 25 farmers per village.

We use a two-stage experimental design similar to Beaman et al. (2020). In the first stage, villages are split into three sub-samples of control, high subsidy, and medium-low subsidy villages. Treatment randomization is stratified by: (a) sub-districts and (b) village-level intensity of wheat cultivation.[7] As shown in Figure 3, farmers in the control villages receive no intervention. In the high subsidy villages, sampled farmers were offered a standard seed package with either a full subsidy or a 50% subsidy.[8] Subsidy rates were randomly allocated at the village level, and were carefully chosen to reflect a full range of prices for estimating the demand curve.[9] In the medium-low subsidy villages, sampled farmers were offered to buy the seed package at the village-level price. Village-level subsidies ranged from 25% to 40% in the medium-subsidy villages, and from 0% to 20% in the low-subsidy village. Demand by each farmer was elicited independently in a take-it-or-leave-it design at stage one.

In the second stage, medium-low subsidy villages were 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.[10] The implementation of stage two took place a few days after the completion of stage one in the stage-two treatment villages. Farmers in stage-two treatment villages who initially choose not to buy the seed package at stage one received the seeds for free in the second stage. Stage-two control villages, on the other hand, did not receive any further intervention at the second stage. Table 2 shows the number of villages at each subsidy level for stage-two treatment and stage-two control groups.

We took several safeguards to make sure that farmers are blind of stage-two treatment during the implementation of stage one. First, enumerators did not know about stage-two treatment until stage one was completed at the upazila (sub-district) level. Second, stage two took place after all villages within the upazila have completed stage one. This approach avoids any contamination in stage-one results that is due to farmers' knowledge of their stage-two treatment status. The distance between villages in our sample that belong to different upazilas is large enough to make us confident that the information on stage-two treatment did not simply spread from one upazila to another.

In order to ensure fairness in the implementation of the two-stage experiment, the message communicated with farmers at stage two is that a surplus in the seeds used for this research study will be freely distributed to a sub-sample of farmers based on a lottery. In the two-stage-experiment villages

[6] The seven districts are: Faridpur (Dhaka Division); Choudanga, Jashore, and Jhenaidah (Khulna Division); Naogaon, Pabna, and Rajshahi (Rajshahi Division).

[7] Intensity of wheat cultivation was calculated using the village census data on the last year the farmer cultivated wheat. We 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 B.1 shows the intensity of wheat cultivation across all villages in our sample.

[8] Standard seed packages weighted 15 kg each. The size of the seed package was determined based on findings from the pilot survey 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.

[9] The market price of the standard seed package is 600 BDT (40 BDT for 15 kgs). For reference, the average daily wage of farmers in the study area is about 500 BDT. We deliberately choose not to offer a price subsidy of more than 50% because the pilot results showed very inelastic demand at higher subsidy rates. Average take-up at subsidies of more than 50% during the pilot was 93%.

[10] Figure 4 shows that seeds take up at a 50% subsidy rate (offer price = 20 BDT/kg) was around 90%. This is close to the 100% take up in free-distribution 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 all treatment farmers in the same villages are expected to receive an equal treatment. However, we choose not to introduce any repayments in stage-two control villages.

We will use two rounds of follow-up surveys to estimate treatment effects as well as spillovers. The follow-up surveys will take place at the end of harvesting for two consecutive wheat seasons. Although we will not execute any intervention in the following wheat season, treatment effects may last or even increase in succeeding seasons as a result of seed multiplication and potential learning effects. The follow-up surveys will cover treatment and control farmers, in addition to a random sample of 900 untreated farmers in treatment villages (i.e., within treatment controls). This sample of within treatment controls will be randomly selected from the village census data to sample 5 untreated farmers in each of the 180 treatment villages. A comparison between the outcomes of untreated farmers in treatment villages and that of farmers in control villages should indicate whether the subsidized seeds had a significant spillover effect. A priori we would expect stronger spillovers in villages with higher take up of the subsidized seed. However, since take-up decisions are endogenous, we will instrument for take up using the randomly allocated subsidy levels.

2.3 Objectives and Hypotheses

Our main research question is whether subsidized prices lead to inefficient allocation of a new agricultural technology – improved wheat seeds. One innovation of this study is that our two-stage experimental design allows us to estimate actual returns of farmers who initially select out of buying the improved seeds at different prices – including market price. If the higher prices lead to positive selection, then we would expect farmers who choose not to buy at stage one to have lower returns than the average farmer in the population. We should be able to test for positive selection by comparing the average returns in the free-distribution villages (no selection) to the average returns of non-buyers in stage-two treatment villages (free distribution conditional on non-purchasing at stage one). We can make this comparison across different subsidy levels to test whether a higher subsidy triggers inefficient sorting.

Since the subsidy might have opposing effects on different beneficiaries, we are interested in analyzing average treatment effects as well as the heterogeneity in treatment effects. We will look at the distributional effects of the subsidy using quantile and distribution regressions to examine whether farmers at the higher or lower end of agricultural returns benefited more from the subsidized seeds. Besides, we will use machine learning techniques to analyze conditional average treatment effect (CATE) and group average treatment effects (GATES) based on a set of baseline covariates. The main advantage of relying on machine learning algorithms is the ability to identify the set of variables that predict heterogeneity. We can then use these predictions in a classification analysis to contrast the characteristics of the farmers who benefited the most versus those who benefited the least from the subsidized seeds (i.e., compare the characteristics of the farmers with the highest versus those with the lowest GATES).

We acknowledge that the subsidy may have substantial spillover effects. In the presence of externalities the social benefits of the subsidy could exceed the private benefits to treated farmers in our sample. Environmental externalities from the improved wheat seed include limiting the spread of crop diseases across wheat plots. The size of this externality would depend on the ratio of the farmers growing the improved seed, the likelihood of crop damage from the outbreak of crop diseases, and the expected size of the damage absent of the improved seed. In addition, the potential for seed re-allocation or multiplication by treated farmers, means that spillover effects may last or even surge in succeeding seasons. Similarly, a learning externality that makes farmers update their expectations on the returns to adoption would depend on the distribution of the farmers' prior expectations and the signal they receive from adopting farmers. In this study, we will provide an empirical test of spillover effects by comparing the outcomes of a random sample of untreated farmers in treatment villages (i.e., within treatment controls) to that of pure control farmers. There is also a potential for calibrating a model to estimate the net social gains of the subsidy from a social planner's perspective.

2.4 Specific Outcomes

We expect the introduction of a new wheat variety to have an impact on a number of outcomes. Table 1 lists the primary and secondary outcomes and the variables used in estimating each of these outcomes.

We note that farmers selection of the farm plot for growing the improved wheat seeds is endogenous. That is why we collected baseline data on farmer's ranking of their plots' suitability for growing wheat. Figure B.3 in the Appendix shows the distribution of the dry season crops grown at baseline for all farmers' plots versus the one plot that the farmer ranked as the "top plot" in terms of suitability for wheat cultivation. In addition, we also collected data on plot characteristics at baseline (e.g., plot area, plot tenure, distance from farmer's home, and cropping cycle over the past three seasons). We will use this baseline data to predict the likelihood that the farmer will select a specific plot for growing the improved seeds. We will follow the same plot selection criteria across treatment and control farmers.

3 Empirical Strategy

We will proceed with our empirical analysis as follows. First, we will estimate the average returns to the improved seeds from stage-one treatment. Using an encouragement design, we will test whether average treatment effects vary with the subsidy rate. Second, we will make use of the two-stage experimental design to test for positive selection. That is, we will test whether farmers with lower (higher) actual returns are more likely to select out of (into) buying the seeds at stage one. Third, we will use quantile regressions to analyze the effects of the subsidy along the distribution of agricultural returns. Fourth, we will apply machine learning techniques to estimate conditional average treatment effects (CATE) and test for treatment heterogeneity. We will also use the machine learning analysis to examine whether farmers with higher CATE are more likely to select into buying the seeds. Thus, the CATE analysis will reinforce the two-stage experiment analysis by testing for positive selection using a different approach. We will extend the machine learning analysis to estimate group average treatment effects (GATES) in order to identify the characteristics of the farmers that are likely to benefit from the subsidy the most. Finally, we will provide empirical estimates for the spillover effects by comparing the outcomes of a random sample of untreated farmers in treatment villages to that of farmers in control villages.

3.1 Intent-To-Treat Effects of the New Technology

3.1.1 Encouragement Design

We will first use the random assignment of prices in an encouragement design. That is, we use an instrumental variable approach such that the random subsidy is used as an instrument for adoption. For this analysis, we limit our sample to the pure control, high-subsidy, and the stage-two control villages in the medium- and low-subsidy arm. We will exclude stage-two treatment villages from this analysis to avoid potential biases from the intervention in stage two. We chose to retain a pure control group in this encouragement design to increase power. Pre-intervention, we cannot be certain of the demand elasticity. If demand is inelastic, a first-stage regression with only the treatment villages will be weak and the encouragement design will lack power. In this case, the comparison between pure control villages and all treatment (excluding stage-two treatment) villages would still allow for an estimation of average treatment effect.

The reduced-form regression is:

$$Y_{ivs} = \beta_1 Subsidy^{High}_{vs} + \beta_2 Subsidy^{Medium}_{vs} + \beta_3 Subsidy^{Low}_{vs} + \delta X_{ivs} + \alpha_s + \epsilon_{ivs} \quad (1)$$

where Y_{ivs} represents the outcome of interest (see Section 2.4 for a complete list of outcomes) for farmer i in village v and strata s. $Subsidy^{High}_{vs}$, $Subsidy^{Medium}_{vs}$, and $Subsidy^{Low}_{vs}$ represent villages receiving high, medium, and low subsidy levels, respectively. Table 2 shows the number of villages and the subsidy rates in each of these three groups. X_{ivs} is a vector of baseline characteristics, including baseline value of the outcome variable. α_s represents strata fixed effects and ϵ_{ivs} is a random error term.[11] Standard errors will be clustered at the village level in all regressions. The β coefficients represent the difference in outcomes across the different price levels, and the omitted category is the pure control villages.

This specification allows us to estimate the average returns to the new seed variety. We will begin by testing the null hypothesis of no treatment effect; that is, $H_o : \beta_1 = \beta_2 = \beta_3 = 0$. The reduced-form regression also allows as to test whether the average returns vary by subsidy level, which is important for our research question on the allocative efficiency of prices. If the average returns are increasing in uptake regardless of self-selection, then we would expect $\beta_1 > \beta_2$ and $\beta_2 > \beta_3$, particularly if the first-stage regression shows significantly higher takeup at the higher subsidy levels. We can explicitly test for these hypotheses using the regression specification in Equation 1.

Our choice of the control variables to include in the vector X_{ivs} will follow the "post-double-selection" method proposed by Belloni et al. (2014). First, using a large list of potential predictors, we will estimate separate LASSO regressions to select the best predictors for each of our outcome variables as well as the treatment dummy.[12] Then, the list of controls that will be included in our specification will include both the best predictors for the outcome variable of interest and the best predictors for the treatment dummy. We will force the baseline values of the outcome variable as well as the strata fixed effects to be included as a control in each regression.

The specification in Equation 1 can be extended to include stage-two treatment villages. Although, the free distribution of seeds (and repayment of seed prices) at stage two could justify including stage-two treatment villages under the group of high subsidy villages, one limitation to this approach is that the repayment to initial buyers in stage-two treatment villages might introduce a re-budgeting effect (i.e., farmers who are re-paid funds initially budgeted for seeds could spend that money on buying more inputs). For this reason, we prefer to add stage-two treatment villages as a separate group as follows:

$$Y_{ivs} = \beta_1 Subsidy^{High}_{vs} + \beta_2 StageTwo^{Control}_{vs} + \beta_3 StageTwo^{Treat}_{vs} + \delta X_{ivs} + \alpha_s + \epsilon_{ivs} \quad (2)$$

where $Subsidy^{High}_{vs}$ represents the villages that receive the seeds for free or at a relatively high subsidy of 50% in stage one. $StageTwo^{Control}_{vs}$ is essentially the same as pooling the stage-two control villages in $Subsidy^{Medium}_{vs}$, and $Subsidy^{Low}_{vs}$ villages in Equation 1 since stage two randomization was among

[11] As explained in Section 2.2, we stratify treatment by upazila and 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. We end up with 18 strata for the 12 upazilas in our sample. This is because three of the 12 upazilas did not have enough variation in the indicator for wheat intensity and had to be merged with other upazilas. Appendix Figure B.1 shows the density of the number of wheat farmers within the villages in the sample.

[12] See Appendix Table B.1 for a full list of potential predictors that we will include in the double LASSO regression.

the medium and low subsidy levels only. We can then 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, had a substantially different treatment effects relative to an unconditional distribution at a highly subsidized rate.[13] A test for $\beta_2 = \beta_3$ can reveal whether the difference in outcomes for stage-two treatment and stage-two control villages is statistically significant. This later comparison will shed the light on the difference in ITT between villages that received the same set of randomly allocated prices at stage-one (i.e., controlling for a pure price effect), whereas take-up in stage-two treatment villages is pushed to nearly 100% by the exogenous shock of stage two treatment.[14]

3.1.2 First Stage for the Encouragement Design

Since the encouragement design uses randomly allocated subsidy rates as an instrument for adoption, we will verify the validity of this instrument by estimating the sensitivity of demand to the different subsidy rates (i.e., the first stage specification). The exclusion restriction is assumed to hold from the random allocation of treatment.

The first-stage regression in the encouragement design is as follows:

$$Demand_{vs} = \theta_1 Subsidy_{vs}^{High} + \theta_2 Subsidy_{vs}^{Medium} + \alpha_s + \varepsilon_{ivs} \tag{3}$$

where $Demand_{vs}$ is a farm-level measure of uptake at stage-one of the experiment, and rest of the variables are defined as in Equation 1. The omitted category is the villages that received a low subsidy rate. Since this is a first stage for the reduced form regression presented in Equation 1, we will use the same sample and drop the stage-two treatment villages from this regression. However, we can also estimate demand from all the treatment villages as a robustness check. Demand estimates are based on the results from stage-one treatment, which should be the same for all the treatment villages regardless of their stage-two treatment status.

As shown on the study timeline, the implementation of the experiment took place in November 2021. The authors could not stay blinded of the seeds demand data for two reasons. First, the data on seeds demand from stage one is an essential input for the implementation of stage two. Second, the number of non-buyers in stage-two treatment villages is needed for the updated power calculations presented in Section 4.2.

Table 3 shows the results from estimating Equation 3 for the entire sample, as well as the sub-sample of villages that did not receive further treatment at stage two.[15] As shown on Table 3, average demand is very low at the low subsidy rates (0-20%). Out of 25 treated farmers, on average merely one farmer takes up the seeds at the low subsidy rate. The medium subsidy rates (25-40%) result in an increase in demand of 28 percentage points. With the high subsidy (50-100%), take-up goes up by 89 percentage points. Figure 4 shows the results at each price level for the entire sample.

3.2 Is Self-Selection Based on Actual Returns?

The two-stage experimental design allows us to answer the question of whether farmers facing positive prices self-select into buying the seeds based on their actual returns. We focus on stage-two of the

[13] As a robustness check we can disaggregate the group of high subsidy villages into two sub-groups of free-distribution and 50% subsidy villages.

[14] Merely 11 farmers out of 1,400 farmers refused to take the seed for free during stage-two treatment.

[15] During implementation, four villages where excluded from the random sample of stage-two treatment. For three villages, the reason for exclusion was 100% take-up of the seeds at stage one. For the fourth village, the reason for exclusion is that the farmers refused to cooperate with enumerators during the seed sales intervention at stage one.

experiment and define stage-two treatment farmers as those who received the seed for free in stage two after deciding not to purchase the seeds in stage one. Similarly, stage-two control farmers are the non-buyers from stage one who did not receive any further intervention at stage two. By comparing the average returns among stage-two treatment farmers to the average returns among farmers in the free-distribution villages, we can test whether there was positive selection at stage one. The specific regression specification is:

$$\begin{aligned} Y_{ivs} = \gamma_1 Free_{vs} + \gamma_2 StageOne^{NonPurchase}_{ivs} * StageTwo^{Treat}_{vs} \\ + \gamma_3 StageOne^{NonPurchase}_{ivs} * StageTwo^{Control}_{vs} + \alpha_s + \epsilon_{ivs} \end{aligned} \tag{4}$$

For this specification we will use data from the pure control villages (omitted category), stage-one free-distribution villages ($Free_{vs}$), non-buyers in stage-two treatment villages ($StageOne^{NonPurchase}_{ivs} * StageTwo^{Treat}_{vs}$), and non-buyers in stage-two control villages ($StageOne^{NonPurchase}_{ivs} * StageTwo^{Control}_{vs}$). The terms α_s and ϵ_{ivs} represent strata fixed effects and a random error term, respectively. To correct for nonrandom sampling of non-buyers, we can include probability weights in the specification above. The probability 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 will ensure that stage-two treatment and control villages with different proportions of non-purchasing farmers at stage one are equally represented.

The parameter γ_1 gives the average treatment effect across the entire population relative to the pure control group (the two sub-samples highlighted by the red rectangle on Figure 3). 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 green rectangle on Figure 3). Thus, a test for $\gamma_1 = \gamma_2 - \gamma_3$ should indicate whether the self-selected farmers obtained higher or lower returns. It is noteworthy that treatment farmers in stage-two treatment villages and treatment farmers in the free-distribution villages both received the seeds for free. This alleviates any concerns that the difference in outcomes could be driven by some behavioral implications of free distribution, since these implications would apply to both groups.[16]

The analysis in Equation 4 can be extended in several ways. First, we have pooled all the medium- and low-subsidy villages together in Equation 4 to gain power. However, we can also separate them by a subsidy rate threshold of 25% to estimate the extent to which higher prices lead to more efficient sorting at the first stage. Moreover, we can control for the date of the free seed distribution at stage one (in free-distribution villages) and stage two (in stage-two treatment villages) to account for any changes in outcomes that are merely due to the timing of the seed delivery. The extended specification is:

$$\begin{aligned} Y_{ivs} = \gamma_1 Free_{vs} + \gamma_2 StageTwo^{Treat}_{vs} * Subsidy^{Medium}_{vs} + \gamma_3 StageTwo^{Control}_{vs} * Subsidy^{Medium}_{vs} \\ + \gamma_4 StageTwo^{Treat}_{vs} * Subsidy^{Low}_{vs} + \gamma_5 StageTwo^{Control}_{vs} * Subsidy^{Low}_{vs} + \alpha_s + \epsilon_{ivs} \end{aligned} \tag{5}$$

where, for simplicity of exposition, we use $StageTwo^{Treat}_{vs}$ and $StageTwo^{Control}_{vs}$ to refer to non-buyers in stage-two treatment and control villages, respectively. $Subsidy^{Medium}_{vs}$, and $Subsidy^{Low}_{vs}$ are indicators for whether the village received a medium or a low subsidy rate at stage one. Medium subsidy

[16]Two potential biases that 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 our context since farmers in Bangladesh are acquainted with ad hoc free distribution of certified seeds by the Department of Agricultural Extension. 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 our results. Our test for positive selection 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 conditional on non-purchasing at stage-one.

rates are in the range of 25-40% and low subsidy rates are in the of 0-20%. We excluded the 50% subsidy villages from the two-stage experiment due to the very high take-up in these villages at stage one.[17] The term α_t represents time fixed effects that accounts for the day of the free seeds delivery. The rest of the variables are as defined before. If positive selection is stronger at higher prices (i.e., lower subsidies), then we would expect to find $\gamma_2 - \gamma_3 > \gamma_4 - \gamma_5$.

3.3 Distributional Effects

As explained in Section 2.1, the choice set of crops to grow during dry season may vary across farmers due to infrastructural or capital constraints. Baseline data on profits and revenues, summarized in Appendix Table B.3, shows substantial variation in the profitability of different crops. This implies that the opportunity cost of growing an improved wheat seed may not the same across farmers at different quantiles of the profit distribution. For this reason, we will re-run the analysis in Equation 1 and Equation 4 using quantile regressions, particularly when profits or revenues are used as the outcome variable.

Technically, a quantile regression estimates coefficients that minimize the median absolute deviation at each quantile, q. For Equation 1, for example, $\beta(q)$ coefficients will be estimated to minimize the following:

$$MAD = \frac{1}{n}\sum_{i=1}^{n} \theta_q \left| Y_{ivs} - (\beta_1(q) Subsidy_{vs}^{H} + \beta_2(q) Subsidy_{vs}^{M} + \beta_3(q) Subsidy_{vs}^{L} + \delta(q) X_{ivs} + \alpha_s) \right| \quad (6)$$

where θ_q is a function of asymmetric weights that takes the form:

$$\theta_q = \begin{cases} q & if \quad \epsilon_{ivs} > 0 \\ (1-q) & if \quad \epsilon_{ivs} \leq 0 \end{cases}$$

ϵ_{ivs} represents the random error term as before. Similar steps can be applied in estimating the quantile regression coefficients for Equation 3.

An alternative approach for analyzing distributional effects is to use distribution regression as discussed in Chernozhukov et al. (2013). This involves estimating the same regression specifications in Equation 1 and Equation 4, while the dependent variable becomes the probability that the outcome variable is greater than a threshold (i.e., $P(Y_{ivs} > y)$). This threshold, y, would move to cover all points in the support of the outcome variable Y. That is, the distribution regression for Equation 1 becomes:

$$F(y) = \beta_1(y) Subsidy_{vs}^{H} + \beta_2(y) Subsidy_{vs}^{M} + \beta_3(y) Subsidy_{vs}^{L} + \delta(y) X_{ivs} + \alpha_s + \epsilon_{ivs} \quad \forall \quad y \in \mathcal{Y} \quad (7)$$

such that the slope coefficients $\beta(y)$ are allowed to vary with the threshold y. Distribution regressions would be preferred over quantile regressions if the distribution of the outcome variable Y does not have a smooth density.

3.4 Is There Heterogeneity in Returns Based on Observable Characteristics?

In Section 3.2, we presented a test for positive selection based on actual returns. However, one might argue that farmers make their adoption decisions based on expected returns, which could be

[17] See footnote 9 for details on the choice of offer prices.

different from realized returns due to uncertainty in agricultural production. In this section, we present an alternative approach to analyze self-selection. We will apply machine learning algorithms to predict conditional average treatment effects (CATE), conditional on baseline covariates. Then, we will examine whether farmers' adoption decisions are correlated with their CATE.

One algorithm that can be used for this analysis is the causal random forests approach in Wager and Athey (2018). The advantages of causal forests include allowing for a large set of predictors and accounting for potential non-linearities in the relationship between treatment effect and predictors. Causal forests build on Athey and Imbens (2016) approach in using causal trees to estimate average treatment effects within subspaces referred to as terminal leaves. The criteria for splitting the data into terminal leaves are based on maximizing heterogeneity in treatment effects across leaves while minimizing the variance in estimates within each leaf. CATE is estimated as the difference between outcomes for treatment and control observations within each leaf. The causal forest algorithm of Wager and Athey (2018) improves predictive power by applying a random forest approach to causal trees. A further improvement over Wager and Athey (2018) method is the generalized causal forests introduced by Athey et al. (2019). Generalized causal forests use a set of kernel-based weights to estimate CATE, instead of letting each tree estimate its own treatment effect. Thus, our preferred method will be the generalized causal forests. Alternative algorithms, such as those presented in Knaus et al. (2021), can also be applied.

We will use the free-distribution and pure control villages as the training sample for the machine learning algorithm to predict CATE for the entire sample based on baseline characteristics. We are primarily interested in examining whether there is a positive correlation between predicted CATE and seeds' demand, and whether this correlation is higher or lower at higher subsidy rates.

Using the predicted CATE, we can formally test for treatment heterogeneity using the approach proposed by Chernozhukov et al. (2018) for estimating the best linear predictor (BLP). Following the notation of Chernozhukov et al. (2018), we will estimate the following weighted linear projection:

$$Y = \alpha' X_1 + \beta_1(D - p(Z)) + \beta_2(D - p(Z))(S - ES) + \varepsilon \tag{8}$$

where Z is a vector of baseline covariates, X_1 is a vector of (optional) controls such that $X_1 := X_1(Z)$, D is an indicator for treatment, $p(Z)$ is a probability of random treatment assignment that depends on a sub-vector of stratifying variables Z_1 in Z, and $S := S(Z)$ is the predicted CATE. The main identification assumption for Equation 8 is $E[w(Z)\varepsilon X] = 0$, where $w(Z) = \{p(Z)(1 - p(Z)\}^{-1}$, $X := (X_1, X_2)$, $X_1 := X_1(Z)$, and $X_2 := (D, (D - p(Z))S(Z))$. β_1 captures the average treatment effect (ATE), while β_2 is the heterogeneity loading parameter. Thus, a rejection of the hypothesis $H_0 : \beta_2 = 0$ implies that there is heterogeneity in treatment and the machine learning predictor $S(Z)$ is able to capture this heterogeneity.

In addition, we can also split the data into groups based on the predicted CATE to analyze group average treatment effects (GATES). Formally, the parameter:

$$E[s_0(Z) \mid G_k]$$

is constructed such that the data is split into G_k groups to explain as much variation in the predicted CATE, $s_0(Z)$, as possible. This way a test for:

$$E[s_0(Z) \mid G_1] = ... = E[s_0(Z) \mid G_k]$$

provides an alternative way to test for heterogeneous treatment effects.

If the results from the BLP and GATES analyses showed substantial heterogeneity, we can further extend this exercise to examine the characteristics of the farmers with the highest and the lowest predicted GATES. This is essentially the classification analysis (CLAN) approach put forth by Chernozhukov et al. (2018).

Going back to the question of whether farmers' adoption decisions are correlated with their CATE, we can test for this formally using the specification:

$$Purchase_{ivs} = \theta_1 Subsidy_{vs} + \theta_2 s_0(Z)_{ivs} + \theta_3 Subsidy_{vs} * s_0(Z)_{ivs} + \alpha_s + \varepsilon_{ivs} \tag{9}$$

where $Purchase_{ivs}$ represents an individual farmer's decision to buy the seed at stage one, $Subsidy_{vs}$ is the randomly allocated subsidy rate at the village level, and $s_0(Z)_{ivs}$ is the predicted CATE. The coefficient θ_2 should indicate whether farmers' self-selection into purchasing the seed is associated with CATE. That is, whether farmers' observable characteristics that predict returns could also predict demand. Besides, θ_3 should indicate whether the association between predicted returns and demand is stronger at higher or lower subsidy rates. We can also modify this specification to account for nonlinearities in demand by using dummy variables for different subsidy levels.

3.5 Does the Subsidy Have Positive 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 our 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. We do not aim to disentangle the effect of each spillover channel separately. However, we will provide empirical test for spillover effects using a random sample of untreated farmers in treatment villages. Starting with a basic regression, we will run the following specification to test for any spillover effects:

$$Y_{ivs} = \phi_1 Treatment_{vs} * T_{ivs} + \phi_2 Treatment_{vs} + \delta X_{ivs} + \alpha_s + \epsilon_{ivs} \tag{10}$$

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 wheat; whether the farmer adopted the improved wheat variety; and whether the farmer has discussed this improved seed with other farmers.[18] $Treatment_{vs}$ is a dummy variable for treatment villages, while control villages are the omitted category. T_{iv} is an indicator for a randomly selected treatment farmer in a treatment village. X_{ivs} is a vector of time-invariant controls. The set of control variables available for this regression is quite limited since we have not collected baseline data for the within treatment control farmers. A potential control variable for this specification is the distance between the control and treatment farmers' plots or the ratio of treatment farmers within, say, a 500 meter radius of the control farmer. Testing for $\phi_2 = 0$ should indicate if there is a spillover effects across all treatment villages.

We can extend this regression specification to test for differential spillover effects in two ways. First, the specification below extends the analysis in Equation 1 using the subsidy rate as an instrument for take up to test for whether spillover effects are increasing in take up.

[18] We note that we might not be able to use plot-level outcomes in our analysis of spillover effects since we do not have baseline data for the sub-sample of within-treatment control farmers. The baseline data included farmers' ranking of their plots suitability for growing wheat. We can try to get around the endogeneity of the farmer's plot choice by using plot-level characteristics to predict which plot is most likely to be selected by the farmer for growing the improved seeds. Alternatively, we can stick to farm-level outcomes. However, we would expect the impact on farm-level profits to be weaker than the impact on plot-level profits due to the limited amount of seeds offered to each treatment farmer.

$$
\begin{aligned}
Y_{ivs} = \beta_1 Subsidy_{vs}^{High} * T_{ivs} + \beta_2 Subsidy_{vs}^{Medium} * T_{ivs} + \beta_3 Subsidy_{vs}^{Low} * T_{ivs} \\
+\beta_4 Subsidy_{vs}^{High} + \beta_5 Subsidy_{vs}^{Medium} + \beta_6 Subsidy_{vs}^{Low} + \delta X_{ivs} + \alpha_s + \epsilon_{ivs}
\end{aligned} \tag{11}
$$

The encouragement design allows us to test whether spillovers vary by subsidy level. That is, we can test whether $\beta_4 > \beta_5 > \beta_6$. We can also examine whether the intent-to-treat (ITT) effect is significantly different from the spillover effect by testing for $H_o : \beta_1 = \beta_4$; $H_o : \beta_2 = \beta_5$; and $H_o : \beta_3 = \beta_6$.

Second, we can extend the regression specification in Equation 2 to test for difference in spillover effects between stage-two treatment and stage-two control villages as follows:

$$
\begin{aligned}
Y_{ivs} = \beta_1 Subsidy_{vs}^{High} * T_{ivs} + \beta_2 StageTwo_{vs}^{Control} * T_{ivs} + \beta_3 StageTwo_{vs}^{Treat} * T_{ivs} \\
+\beta_4 Subsidy_{vs}^{High} + \beta_5 StageTwo_{vs}^{Control} + \beta_6 StageTwo_{vs}^{Treat} + \delta X_{ivs} + \alpha_s + \epsilon_{ivs}
\end{aligned} \tag{12}
$$

Given the difference in the intervention received by stage-two treatment and stage-two control villages, we would 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 $\beta_4 = \beta_6$ 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 $\beta_5 = \beta_6$ will should indicate if there is a difference in spillover effects across villages that received the same offer prices at stage one, while at stage two only stage-two treatment villages were shocked by free distribution of seeds.

3.6 Multiple Hypotheses Testing

As mentioned in Section 2.4, our primary outcomes are centered around three main variables; namely, farm-level wheat area, plot-level profits, and plot-level revenues. However, our secondary outcomes include groups of multiple outcomes such as impact on input use for a number of inputs. For these outcomes, we will report adjusted p-values that correct for multiple inference following the free step-down procedure of Westfall and Young (1993). Specifically, we will control for the Family-Wise Error Rate by applying the free step-down resampling using 10,000 or more bootstraps, as explained in Anderson (2008), for each family of outcomes.

4 Data

4.1 Data Collection and Processes

The key data source is primary survey data collected from the following questionnaires:

- **Village listing survey** *(September 2021)*: covers all farmers in the 220 villages in the sample. This survey includes farmer's contact information, and the last year the farmer cultivated wheat, if applicable.
- **Market survey** *(September 2021)*: covers a random sample of inputs market in each of the upazilas in the sample. This survey includes type of seed varieties sold in the market, average price of wheat seeds, and start and end time for selling wheat seeds (in weeks).

- **Baseline survey** *(October-November 2021)*: covers sampled farmers in the 220 villages. This survey includes farmer's demographic characteristics, household asset ownership, and baseline values for all outcome variables with reference to the last year's cropping cycle. In addition, the baseline survey had modules on farmer's time-preferences, risk aversion, and preferences towards wheat cultivation. Appendix XX provides a brief description of the baseline survey modules.
- **Seed distribution survey** *(November 2021)*: was collected during stage one and stage two of the seed distribution. In stage one, the seed distribution survey includes farmer's buying/ 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, and the farmer's intended use of the free seed package.
- **Midline survey** *(May-June 2022 after harvesting ends)*: will include updated farmer's contact information, which seed varieties were grown on which plots (including *Bari Gom 33*), and all of the outcome variables.
- **Endline survey** *(May-June 2023 after harvesting of the following season)*: will be similar to the midline survey.

4.2 Balance Check and Power Analysis

We check for balance across treatment arms for both stage-one and stage-two randomization using baseline data. We also use the baseline data to re-evaluate our power calculations after estimating intera-clusteral correlations (ICC) for the primary outcomes at baseline.

Table 4 shows the balance tests for each treatment arm.[19] Balance at stage one of the experiment is checked by comparing the means of key parameters across control, high-subsidy and medium-low-subsidy villages. Similarly, balance at stage two is checked by comparing sample means for the sub-samples of stage-two control and stage-two treatment villages. The sample is fairly balanced across treatment arms with very few exceptions. For example, farmers in the pure control villages are slightly older than farmers in other treatment arms. For those farmers who had any amount of outstanding debt at baseline, the average amount of money owed is higher for farmers in the high-subsidy villages. Also, farmers in stage-two treatment villages are more likely to have grown wheat on any plot over the past three years. We can control for these three parameters in our regression specifications.

Table 5 presents the power analysis for the regression specifications in Equation 1 and Equation 4. Using baseline data, we estimate the ICC for the primary outcomes and simulate power calculations under different values for the correlation coefficient between the outcome variable and a set of control variables including baseline values of the outcome and strata fixed effects. We show the results under the assumption that this correlation coefficient, r, is 0.25, 0.5, or 0.75. We believe this is a reasonable range, which corresponds to an R-squared of 0.05, 0.25, or 0.56, respectively. The baseline data shows that regressing the baseline value of each of outcome variable on strata fixed effects alone results in an R-squared in the range of 0.05 to 0.28. We present the minimum detectable effect (MDE) for different values of r with one round of follow-up data as well as a panel of two rounds of follow up.

For Equation 1, we estimate MDE for the three primary outcomes, namely farm-level wheat area, plot-level profits, and plot-level revenues. Given that high-subsidy villages have, expectedly, the highest take up rate, we focus our power calculations on the regression coefficient β_1, which corresponds to high-subsidy villages. The power analysis shows that, with two rounds of follow up, we are powered to detect a moderate increase in profits ranging from 0.10 to 0.15 standard deviations, depending on

[19] Table B.2 in the Appendix shows summary statistics for the entire sample.

the value of r. However, due to a high ICC at baseline, we are only powered to detect an increase in wheat area or revenues that is greater than 0.21 or 0.18 standard deviations, respectively.

For Equation 4, we are concerned with comparing the returns of farmers who selected out of buying the seeds at stage one to the returns of the average population of farmers. This corresponds to a test for $\gamma_1 = \gamma_2 - \gamma_3$, following the notation in Equation 4. The primary outcome for this comparison is plot-level profits. MDE in this case is sensitive to the value of r and whether the comparison is done with cross-sectional data of one follow up versus a panel of two rounds of follow up. With two rounds of follow up, the MDE is between 0.17 and 0.25 standard deviations, depending on the value of the correlation between the outcome variable and the set of controls.

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Figures and Tables

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

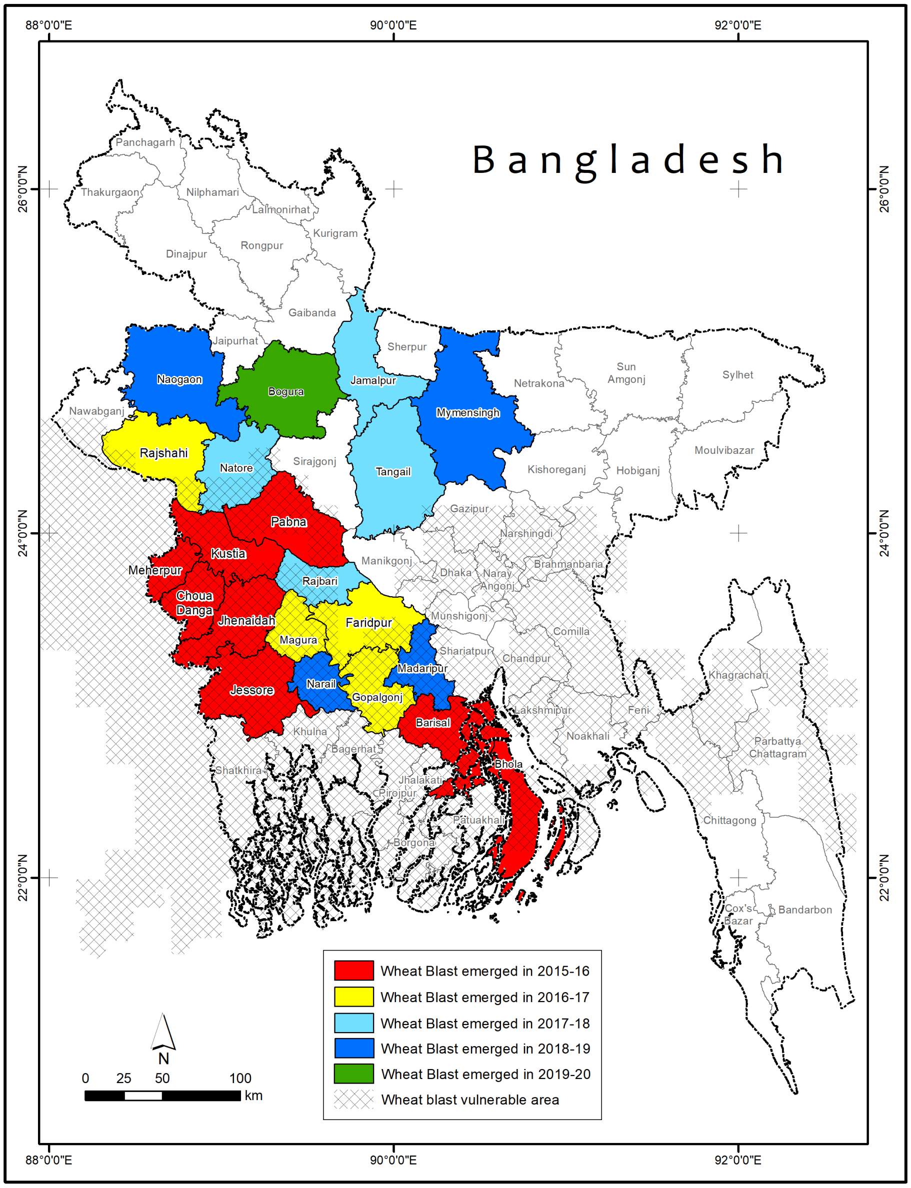


Figure 2: Changes in wheat area in blast-affected districts (2013-2020)

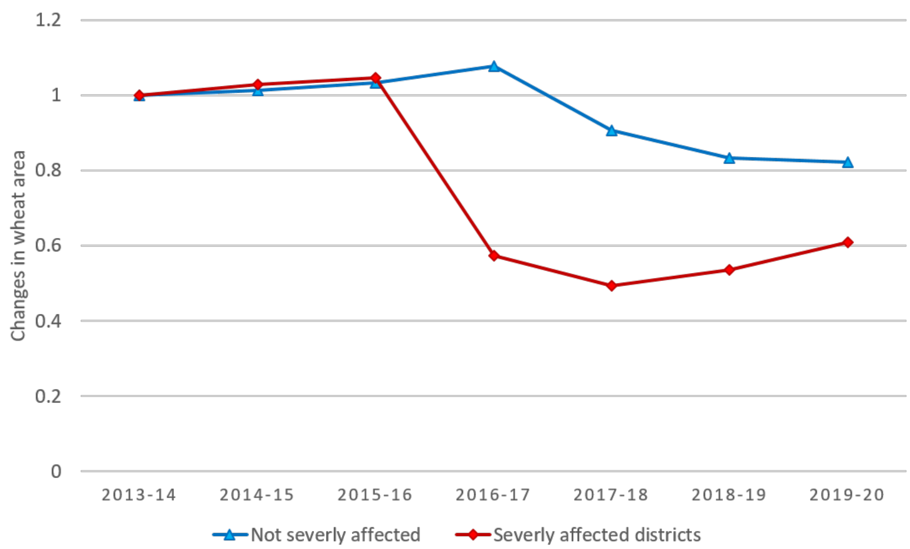
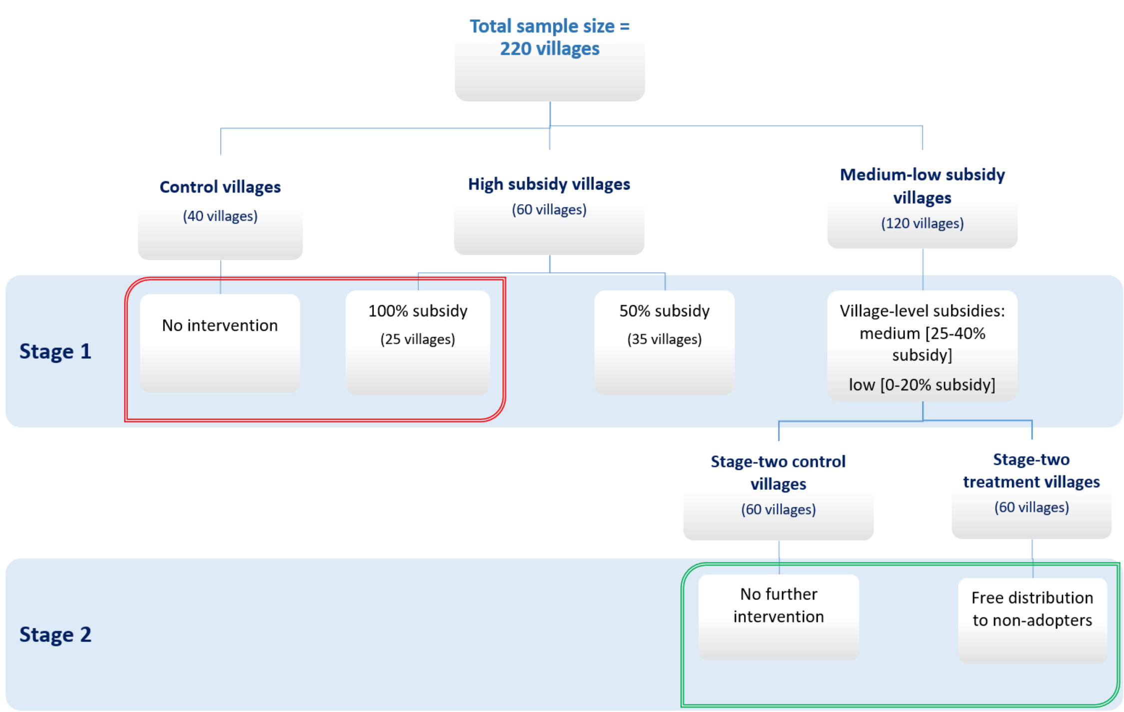


Figure 3: Experimental design



This figure illustrates the two-stage experimental design. The red and green rectangles highlight the comparison between the free-distribution and control villages at stage one versus the free-distribution and control villages at stage two. This comparison represents our primary test for positive selection by farmers as explained in Section 3.2

Figure 4: Seeds demand at stage-one

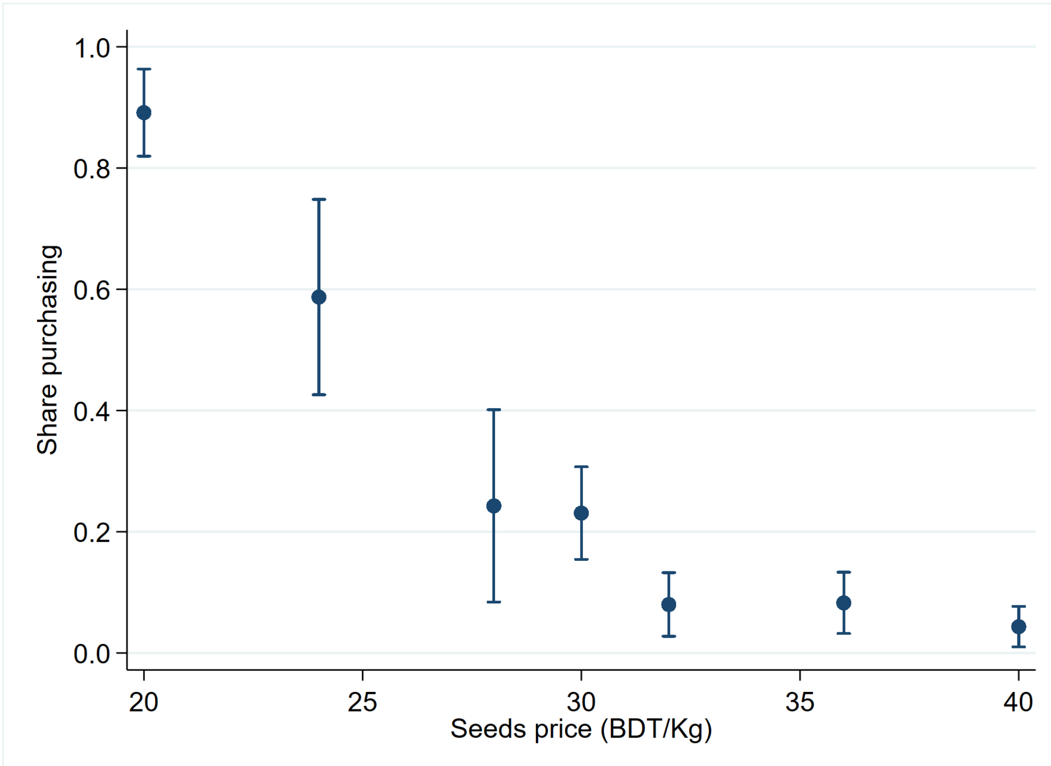


Table 1: Primary and secondary outcomes

Primary Outcomes	
Farm-level wheat area	An indicator of whether the farmer has grown wheat on any plot during the winter season. Total area allocated to wheat planting by the farmer during the winter season.
Plot-level profits	Total farm revenue less total input costs. Total revenues are calculated as the total output multiplied by the output price for each crop. Total costs include: land rental expenses, seed expenses, chemical input costs (e.g., fertilizers, pesticides, etc.), irrigation costs, hired labor expenses, and family labor opportunity costs (imputed from village-level average wage and following Agness et al. (2022) rule of thumb of valuing family labor at 60% of the market wage). Profit as an outcome variable can be presented in terms of "total profits" or "profit per acre" or "log profit per acre". In all cases, outliers will be trimmed at the top and bottom 1%.
Plot-level revenue	Similar to profit, the revenue variable can be presented in terms of "total revenues" or "revenues per acre" or "log revenues per acre". Outliers will be trimmed at the top and bottom 1%.
Secondary Outcomes	
Plot-level input use	Fertilizers in kilograms per acre, pesticides in kilograms per acre, herbicide in kilograms per acre, hired labor hours by activity (planting, weeding, harvesting), family labor hours by activity, irrigation time.
Cropping pattern	Mix of crops cultivated during Rabi/ Boro and the following season, measured by dummy variables for the primary crops grown during Aman season, Rabi/ Boro season, and an optional third season.
Seasonal or non-farm work during wheat season	An indicator for whether the farmer took-up seasonal or non-farm work during Rabi season. A measure of household income from non-farm activities during Rabi season.

Table 2: Sample size at each subsidy rate

Subsidy Rate	**Number of Villages in Stage-Two Control**	**Number of Villages in Stage-Two Treatment**
Low subsidy (0-20%)	35	30
Medium subsidy (25-40%)	25	30
High subsidy (50-100%)	60	0

Table 3: Stage-one demand

	(1) All villages	(2) Excluding stage-two treatment villages
High Subsidy [50-100%]	0.89***	0.89***
	(0.03)	(0.03)
Medium Subsidy [25-40%]	0.28***	0.28***
	(0.05)	(0.07)
Strata FE	Yes	Yes
F-statistic	526.19	470.42
R-squared	0.792	0.825
Low-Subsidy Villages' Mean	0.06	0.04
Number of farmers per village	25	25
Number of villages	180	123

Dependent variable is the share of treated farmers taking up the seed package at the offer price. Standard errors clustered at the village level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 4: Balance check

	Stage-one Randomization						Stage-two Randomization		
Variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Control	High subsidy	Med-low subsidy	p-val (1)-(2)	p-val (2)-(3)	p-val (1)-(3)	Stage-two control	Stage-two treat-ment	p-val (7) - (8)
Farmer's age	46.51	45.27	44.22	0.03	0.21	0.00	43.96	44.53	0.48
Farmer's years of schooling	4.81	4.77	4.78	0.81	0.98	0.95	4.75	4.82	0.41
Household size	4.67	4.63	4.65	0.98	0.67	0.71	4.58	4.74	0.12
Access to non-farm income (0/1)	0.32	0.29	0.28	0.56	0.41	0.25	0.28	0.29	0.98
Access to credit from banks or NGOs (0/1)	0.41	0.42	0.43	0.85	0.34	0.38	0.44	0.41	0.64
Total amount of outstanding loans	54,378.61	58,035.62	48,469.47	0.68	0.07	0.15	47,831.96	49,358.56	0.76
Size of agricultural land owned (decimals)	95.48	88.31	88.13	0.30	0.50	0.19	87.32	89.20	1.00
Total value of livestock owned ('000)	119.58	118.85	114.98	0.87	0.78	0.70	113.31	117.02	0.64
Area of land cultivated last dry season (decimals)	159.27	156.63	147.58	0.56	0.33	0.16	146.80	148.69	0.50
Farmer grew wheat in the past 3 years	0.56	0.50	0.57	0.45	0.68	0.69	0.54	0.61	0.01
Wheat area to total farm area in 2020-21 winter season	0.18	0.17	0.19	0.86	0.60	0.92	0.20	0.19	0.85
Farmer received extension in the past 12 months	0.39	0.40	0.39	0.61	0.89	0.75	0.34	0.45	0.24
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	0.80
Primary plot area (decimals)	33.45	31.84	31.96	0.32	0.89	0.31	31.33	32.71	0.09
No. of times primary plot is irrigated in dry season	8.77	10.68	9.70	0.31	0.66	0.06	9.78	9.61	0.34
Plot-level revenues (BDT/acre)	62,836.63	68,211.92	68,341.63	0.11	0.40	0.24	67,784.81	68,868.91	0.50
Plot-level profits (BDT/acre)	26,738.48	29,489.65	26,131.89	0.36	0.20	0.85	27,319.00	24,717.88	0.20
Sample size	1,000	1,500	3,000				1,600	1,400	

Columns (1)-(3) and (7)-(8) show sample means of the listed covariates for the each arm in stages one and two of the experiment, respectively. Columns (4)-(6) and column (9) are estimated by regressing 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 5: Minimum detectable effects for the primary outcomes

Outcome variable	MDE for one round of follow-up			MDE for two rounds of follow-up		
	$r^2 = 0.05$	$r^2 = 0.25$	$r^2 = 0.56$	$r^2 = 0.05$	$r^2 = 0.25$	$r^2 = 0.56$
	A. Impact on high-subsidy villages: β_1					
Farm-level wheat area	0.31	0.28	0.21	0.30	0.27	0.21
Plot-level profits	0.17	0.16	0.11	0.15	0.14	0.10
Plot-level revenues	0.27	0.24	0.18	0.26	0.23	0.18
	B. Evidence for positive selection: $(\gamma_1 > \gamma_2 - \gamma_3)$					
Plot-level profits	0.30	0.27	0.21	0.25	0.22	0.17

MDEs are in standard deviation terms. $alpha = 0.05$, $power \geq 0.8$. Intera-clusteral correlation (ICC) is estimated using baseline data as 0.39, 0.07, and 0.28 for farm-level wheat area, plot-level profits, and plot-level revenues, respectively. The correlation coefficient, r, represents the correlation between the outcome variable and baseline values of the outcome or other predictive covariates.

A Survey Modules

Table A.1: Baseline Survey Modules

Module	Brief description
Consent	Informed consent form
A. General household characteristics	Farmer's contact information, household demographics, non-farm income, total farm size
B.1. Farm overview	Total number of plots cultivated by the farmer. *For the three main plots* (including main wheat plots): plot area, distance from home, primary crops cultivated in each season (usually Aman and Boro seasons, or Aman and Rabi seasons), planting and harvesting month for each season, output price and quantity for each season
B.2. Ranking of plots	Ranking of plots suitability for growing wheat, ranking of plots suitability for growing boro rice
C. Plot details	Plot tenure, main crops cultivated during Rabi or Boro seasons, seed amount, seed variety (for wheat only), labor hours by task for hired and family labor separately, total expenditure on hired or contract labor by task, other input (e.g., fertilizers, pesticides, herbicides) quantities
D. Farm-level expenditures	Total expenditures on fertilizers, herbicides, seeds (by crop), contract labor
E. Landholder questions	*Extension:* Year of last extension visit, main crops discussed in extension training *Wheat area:* farm-level wheat area for the last three years *Abandoned crop:* crop that the farmer no longer cultivates (if any), year in which the farmer stopped cultivating that crop, main reason(s) for not cultivating that crop any more *Crop diseases:* farmer's perception of the main crop disease, likelihood of wheat blast (if blast is mentioned as a major disease), possibility of protecting harvest from blast (if blast is mentioned as a major disease)
F.1. Time preferences	Elicitation of hypothetical discount rate
F.2. Risk and ambiguity aversion (qualitative)	5-point Likert scale evaluation for normative statements on farmer's attitude towards risk and uncertainty
F.3. Risk and ambiguity aversion (quantitative)	Farmer's choice between different seeds that have different outputs depending on the amount of rain
G. Wheat seed preferences	Elicitation of farmer's willingness to pay for a blast-resistant wheat seed
H. Crop insurance and access to credit	Access to crop insurance or formal credit. Identifying any potential credit constraints
I. Asset ownership	Land assets, livestock, productive capital, and household assets
Plot location GPS	GPS location of the main (wheat) plot during dry season

B Appendix Figures and Tables

Figure B.1: Village-level wheat intensity in the sample

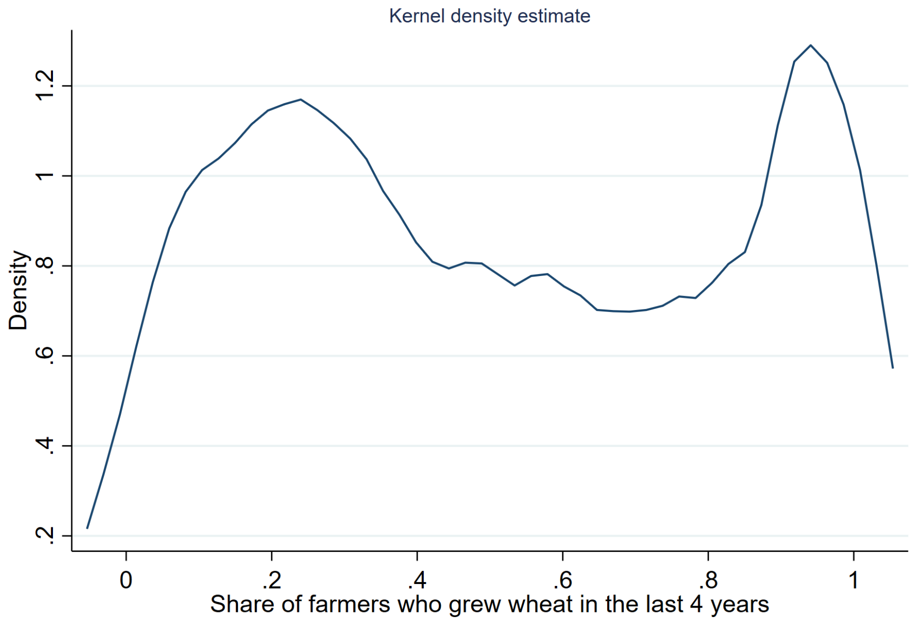


Figure B.2: Crops cultivated during dry season by upazila

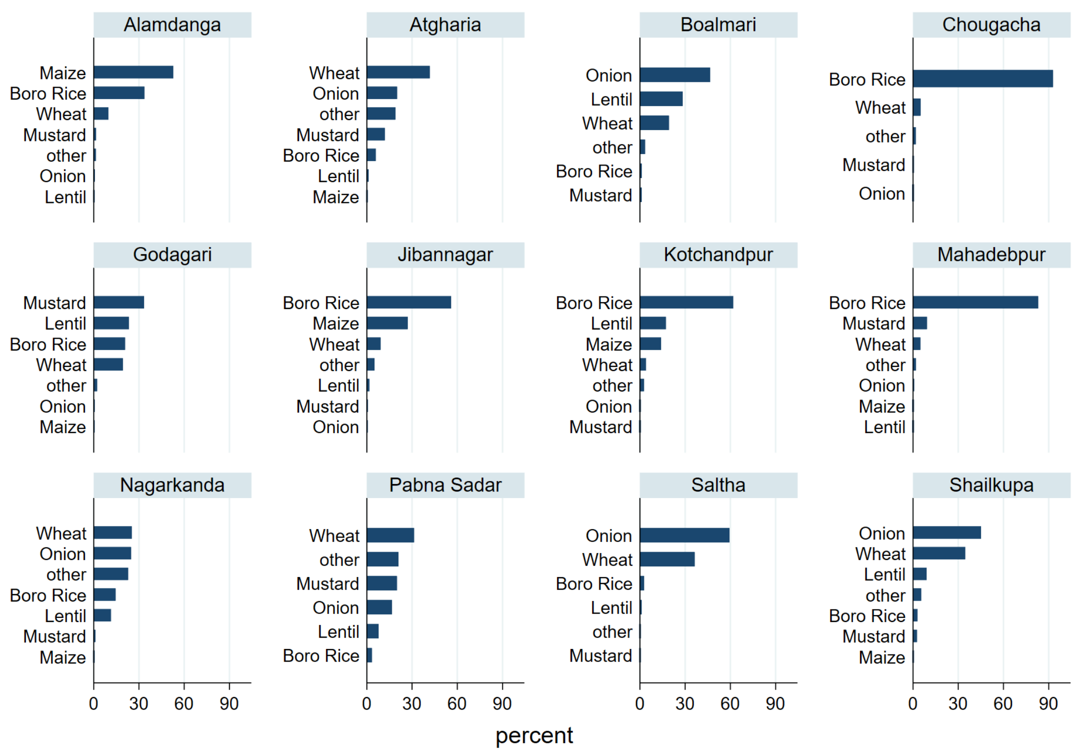


Figure B.3: Crops cultivated on the plot during dry season

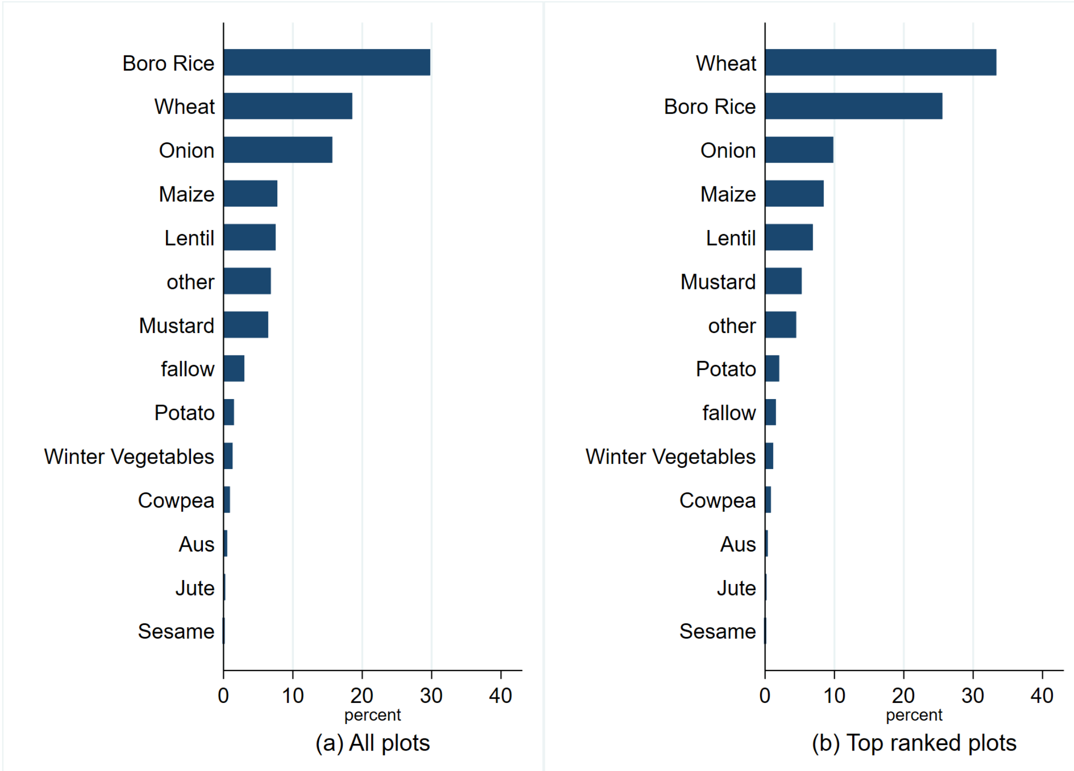


Table B.1: List of potential controls

Household and farm characteristics	
Demographics	Farmer's age, literacy, years of education, household size, number or household members above the age of 16, number of household members available for farm work
Non-farm income	Indicator for any source of non-farm income, specific source of non-farm income
Asset Ownership	Agricultural landholdings, livestock ownership, ownership of any irrigation equipment (shallow machine, power pump, deep tube well, etc.), ownership of power tiller, ownership of a fishpond, house size, ownership of means of transportation (bike, motorcycle, etc.)
Access to credit and insurance	Access to any kind of crop insurance, access to formal credit over the past 12 months, reason for not obtaining more credit
Access to agricultural information	Year of the last extension visit
Preferences towards wheat cultivation	Indicator for discussing "wheat" during the last extension visit, farm-level wheat area in 2021, farm-level wheat area in 2020, farm-level wheat area in 2019, indicator for whether the farmer has stopped wheat cultivation, reason for abandoning wheat, farmer's perceptions of the likelihood of wheat blast, indicator for whether the farmer believes it is possible to protect a wheat farm from the blast, hypothetical WTP for a blast-resistant wheat seed
Time preferences	Estimated discount rate
Risk aversion (qualitative)	Estimated risk aversion index based on 5-point likert scale evaluation for normative statements on farmer's attitude towards risk and uncertainty
Farm and (main) plot characteristics	Farm size (total cultivated area during 2020-21 winter season), plot area, plot tenure, walking distance to the plot from home, number of primary crops grown on the plot during the previous winter season, type of wheat seed used during the previous winter season Estimates for plot (or village) elevation from GPS data
Baseline values of outcome variables [20]	
Baseline profits	Plot-level profits at baseline
Baseline revenues	Farm-level and plot-level revenues at baseline
Baseline input use	Input use on the main wheat plot at baseline
Baseline cropping pattern	Mix of crops cultivated during the dry season at baseline

[20] See Table 1 for a full list of outcome variables.

Table B.2: Summary statistics from baseline data

	Mean	SD	N
Farmer's age (years)	44.92	12.68	5,500
Farmer's years of formal schooling	4.78	4.12	5,500
Household size	4.65	1.74	5,500
Number of household members available for farm-work	1.64	1.56	5,500
Access to non-farm income (0/1)	0.29	0.45	5,500
Access to any kind of crop insurance (0/1)	0.01	0.11	5,500
Access to credit from banks or NGOs (0/1)	0.42	0.49	5,500
Farmer wanted to borrow more money at the usual rates (0/1)	0.27	0.45	2,397
Total amount of outstanding loans (BDT)	52,120.87	51,639.55	2,391
Size of agricultural land owned (in decimals)	89.51	101.64	5,446
Total value of livestock owned by the household ('000)	116.87	134.96	5,500
Household owns a power pump or deep tube well (0/1)	0.29	0.45	5,500
Household owns a threshing machine (0/1)	0.12	0.32	5,500
Household owns a power tiller (0/1)	0.07	0.26	5,500
Area of land cultivated during 2020-21 winter season (in decimals)	152.17	139.08	5,500
Farmer grew wheat on any plot in the past 3 years (0/1)	0.55	0.50	5,500
Farmer grew wheat on any plot in 2020-21 winter season (0/1)	0.36	0.48	5,500
Wheat area to total farm area in 2020-21 winter season	0.19	0.30	5,500
Hypothetical WTP for blast-resistant wheat seed (BDT/kg)	17.44	14.33	5,500
Farmer received extension visit over the past 12 months (0/1)	0.39	0.49	5,500
Wheat was one of the crops discussed during the last extension visit	0.22	0.41	4,848
Primary plot is owned by the farmer (0/1)	0.65	0.48	5,500
Primary plot area (in decimals)	32.20	22.05	5,500
Number of times the primary plot is irrigated during dry season	9.80	14.16	5,500
Plot-level revenues (BDT/acre)	67,303.64	57,403.56	5,260
Plot-level profits (BDT/acre)	27,157.78	82,184.35	5,260

Table B.3: Summary statistics by main dry season crops

		Mean/SD	Median	N
Wheat	Yield (kg/acre)	1,310.11	1,230.77	1,797
		(363.76)		
	Revenues (BDT/acre)	32,441.28	31,250.00	1,787
		(11,066.62)		
	Profits (BDT/acre) *including* family labor costs	-6,788.68	-5,596.00	1,787
		(20,558.40)		
	Profits (BDT/acre) *excluding* family labor costs	1,283.90	1,715.15	1,787
		(15,917.92)		
Boro Rice	Yield (kg/acre)	2,677.14	2,666.67	1,378
		(466.28)		
	Revenues (BDT/acre)	67,972.95	67,941.18	1,372
		(15,820.62)		
	Profits (BDT/acre) *including* family labor costs	23,986.84	25,060.27	1,372
		(22,416.55)		
	Profits (BDT/acre) *excluding* family labor costs	31,990.09	32,810.79	1,372
		(19,675.67)		
Onion	Yield (kg/ acre)	6,565.94	6,400.00	526
		(1,672.75)		
	Revenues (BDT/ acre)	186,908.76	184,615.39	516
		(53,826.51)		
	Profits (BDT/acre) *including* family labor costs	71,379.54	76,068.46	516
		(59,750.92)		
	Profits (BDT/acre) *excluding* family labor costs	103,100.27	109,414.52	516
		(55,616.62)		
Maize	Yield (kg/ acre)	4,790.07	4,800.00	447
		(1,085.01)		
	Revenues (BDT/ acre)	83,166.46	84,848.48	443
		(17,809.23)		
	Profits (BDT/acre) *including* family labor costs	34,672.82	35,403.02	443
		(20,817.94)		
	Profits (BDT/acre) *excluding* family labor costs	41,558.88	43,404.65	443
		(19,551.73)		
Lentil	Yield (kg/ acre)	700.42	727.27	369
		(177.53)		
	Revenues (BDT/ acre)	49,055.66	50,000.00	359
		(13,794.27)		
	Profits (BDT/acre) *including* family labor costs	19,668.54	24,775.76	359
		(18,244.39)		
	Profits (BDT/acre) *excluding* family labor costs	27,240.44	30,787.88	359
		(15,405.14)		

Administrative Information

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Institutional Review Board: We obtained approval from Tufts Social, Behavioral & Educational Research Institutional Review Board (Tufts SBER IRB).

Declaration of interest: Mai Mahmoud has no conflicts of interest to declare. Khondoker Mottaleb is a scientist at the socioeconomics program of International Maize and Wheat Improvement Center (CIMMYT). His performance is not evaluated based on the impact evaluation results that would come out of this study.