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Improved agricultural input delivery systems for enhancing technology adoption: evidence from a field experiment in Ethiopia

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Abstract

In this study, we test the hypothesis that small-scale testing can reduce the risk and uncertainty of trying new technologies. We conducted a field experiment, in a cluster randomised control trial setting, to examine whether the availability of divisible packages of seeds influences smallholder farmers' decisions to try a new wheat variety. Our results show that the adoption of the newly introduced wheat variety was higher in the villages where small seed packages were introduced. We find that smallholder farmers tend to experiment on the newly introduced variety on their farmland and are less likely to adopt the new variety as a coping mechanism for risk exposure at the stage of experimentation. The results from treatment heterogeneity reveal that supplying seed in small bags had differential causal effects on individual farmers. The intervention which made small seed bags available impacted relatively younger and poorer farmers the most. This finding provides an insight into the significance of seed delivery in small bags to improve the use of seeds of new varieties by smallholders.

Keywords: cluster randomised control trial, technology adoption, imperfect input market, divisible seed packages, Ethiopia

JEL classification: O1, Q1, D83, O13, C99

1. Introduction

New crop varieties are responsible for about 50–90 per cent of the increase in world crop yield (Bruins, 2009). Cognisant of its importance, donors and governments have given considerable attention to the agricultural research agenda and the Consultative Group on International Agricultural Research which made available a large number of new crop varieties that are well suited

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to smallholder farmers in developing countries. However, their adoption rates by smallholder farmers are quite low (Lantican *et al.*, 2016; Bold *et al.*, 2017). Particularly, in sub-Saharan Africa (SSA), the low agricultural productivity compared to other regions in the world has been partly attributed to the low levels of adoption of improved agricultural technologies (Walker and Alwang, 2015; Yitayew *et al.*, 2021) and low levels of agricultural input use in the region (Sheahan and Barrett, 2017), which ultimately affect progress towards food security (Issahaku and Abdulai, 2020b).

Learning reduces uncertainty related to unobserved exogenous factors and inherent features of a new technology. Information about new technologies is normally obtained through farmers experimenting on small portions of their land or learning from their neighbours' experience with the technology and via information technologies (Krishnan and Patnam, 2013; Ogutu *et al.*, 2018; Dzanku *et al.*, 2020; Abdul-Mumin and Abdulai, 2021). It is important to note that because modern inputs have been available for many decades and supported by extension services in Africa, many authors have argued that learning may not be the important driver of imperfect adoption decisions observed in the region (Suri, 2011). However, experimenting with new varieties tends to influence the learning process, particularly when available or supplied in quantities or packages that enable smallholder farmers to test new technologies at a small scale (Pannell *et al.*, 2006). Accordingly, if seeds of improved varieties are not marketed in desirable quantities or packages and at affordable prices for smallholder farmers, adoption may be low. Inefficient input supply systems are partly responsible for the low adoption rates of improved agricultural technologies in SSA (Wandulu, 2004). Large public or parastatal companies hold the lion's share of the seed market, with low contribution from the small-scale private input suppliers to the marketing of improved seeds in the region, averaging only about 10–15 per cent (ECA, 2010). Thus, seeds are often marketed in a one-size-fits-all fashion often in one or utmost in two large sizes, making it difficult for smallholder farmers to find inputs in suitable quantities that they intend and can afford to buy.

Many studies show that relaxing liquidity constraints through credit availability for smallholders increases the adoption of new technologies (e.g. Karlan *et al.*, 2014; Shahzad and Abdulai, 2021). Similarly, relaxing liquidity constraints through supplying seed in small bags might encourage small farmers to try new varieties, because they can afford to spend less on the inputs when supplied in small and less expensive bags. Therefore, the availability of small seed packages may contribute to the increased use of a new variety. For example, farmers might prefer paying the cost of the seeds immediately by purchasing the small bags than paying later for the larger ones. This is consistent with the theory of present-biased preferences. O'Donoghue and Rabin (1999) argue that when two time horizons are considered, bias in favour of the present provides a relatively stronger weight to the option of obtaining the opportunity or dealing with uncertainty at the earlier time horizon.

However, there is a lack of rigorous evidence on the impact of input supply in divisible packages on the adoption of new technologies in SSA, especially using randomised control trials (RCTs). Previous adoption studies using RCTs focus on the impact of input subsidies on technology adoption (e.g. Carter, Laajaj and Yang, 2014) and relaxing credit constraints on decisions of smallholder farmers (e.g. Karlan *et al.*, 2014). In this study, we conduct a cluster RCT experiment to examine the impacts of supplying seed in divisible quantities on smallholder farmers' decisions to grow a new wheat variety (*Kingbird*). In this field experiment, we hypothesise that by supplying seeds with divisible packages, we can minimise farmers' cost of experimenting, thereby enhancing their ability to try a new variety in the first period. We specifically test the hypothesis that the adoption of *Kingbird* will be higher in villages (locally called *got's*) where divisible seed packages are available for farmers to purchase than in the villages where the markets provide only one large-sized seed package. Both the concept of stages of experimentation proposed by Rogers (1962) and the target-input model in new technology explained in Bardhan and Udry (1999) motivate this hypothesis. Specifically, the target-input model, which indicates testing a new variety, given the random effect of the quality of land on the yield of the variety, plays a significant role in learning about the optimal input levels. The availability of the new variety in smaller packages will encourage farmers to try the new variety at a smaller scale, thereby learning-by-doing, and ultimately fully replace their old varieties.

The present study contributes to the literature on the impacts of input marketing strategies on technology adoption in SSA. The findings from this study can provide useful insights for policymakers and other stakeholders to develop better strategies for targeting and enhancing adoption by making shelved agricultural technologies available to smallholder farmers and as such contribute to improving agricultural productivity and food security. The study also contributes to the empirical literature in this regard by underlining the issue of treatment effect heterogeneity. We account for heterogeneity in treatment effects in this study, using generalised random forests (GRFs). Our findings show that smallholder farmers exposed to the *Kingbird* variety in small bags have a higher tendency to try the new variety. The seed supply in small bags has generally differential causal effects on individual farmers' adoption decisions.

The rest of the article is structured as follows. In the next section, we describe the experimental design and the sampling framework used in the study. This is followed by a discussion on the conceptual and empirical framework employed in the analyses and the empirical results. In the final section, conclusions and policy implications are presented.

1.1. Context

In Ethiopia, 4.6 million farm households are directly dependent on wheat farming for their livelihoods (Shiferaw *et al.*, 2014). That is why improving wheat productivity and production is a key agenda for policymakers and other stakeholders to ensure food security. Although wheat is among the five most important high-value commodities (ATA, 2014), accounting for about 16 per cent of the total area under cereals in the country (CSA, 2011), productivity is still very low. The national average productivity is about 2.1 tons/ha (Abate *et al.*, 2018), which is 13 per cent below the average in SSA (FAO, 2014). This is mainly because the adoption of improved crop varieties to boost productivity remains low (Yigezu *et al.*, 2018). The average per capita consumption of wheat is about 32 kg/year (Minot *et al.*, 2015), accounting for 15 per cent of the total caloric intake in Ethiopia (FAO, 2015). The country covers 25–35 per cent of its national wheat demand through imports (Minot *et al.*, 2015).

Expanding the use of improved varieties among farmers is one of the strategies pursued by the government to boost agricultural productivity. However, seed supply remains limited, especially for smallholder farmers (Alemu and Spielman, 2006). Input supply in the Ethiopian seed system is highly dominated by the public seed enterprise, with limited private sector involvement, especially in the wheat seed market. Ninety per cent of modern agricultural inputs are supplied to smallholder farmers by the government through rural cooperatives (Tadesse, Abate and Ergano, 2018). In recent years, the integrated seed sector development programme has been coordinating international, national and local seed sectors to provide certified seeds to farmers in Ethiopia (ISSD, 2013). Improved seeds and chemical fertilisers are normally available to farmers in standardised packages. The Federal Ministry of Agriculture, based on a 0.5-ha average landholding, defines the current packaging size (see Alemu, Rashid and Tripp, 2010). In addition, both public and private seed companies prefer selling in large package sizes. These large standard packages provide little opportunity for farmers to experiment and learn about new technologies and to adopt them (Spielman *et al.*, 2010). Including improved wheat seeds, all cereal crops improved seeds, except improved maize seeds,[1] are supplied to farmers only in 50-kg packages, even though many, if not most, farmers grow crops such as wheat on farm plots, which require seeds less than 50 kg. In our study, the smallest sizes of farm plots that are allocated for wheat production in the previous year are used as a benchmark to set the smallest size of the seed bags.

2. Experimental design and intervention

We implemented a single treatment factor in a two-level structure (see Figure 1).[2] First, we randomly selected 96 villages for the study from four

1 Maize seed market is quite different from other seed markets, because farmers have to buy certified maize seeds for each production season, since stored maize seed causes huge yield losses. After a myriad of efforts that were made by national and international organisations, the government opened the maize seed market to some extent to the private sector (see Alemu and Spielman, 2006). Currently, public (through cooperatives) and private sectors supply maize seed to farmers in small bag sizes, while for other improved seeds including chemical fertiliser, cooperatives are still the only channel for them.

2 It is useful to mention that we organised workshops to consult with stakeholders at the national level about how to improve agricultural technology adoption and input delivery system and at the regional level about the implementation of the experiment. This was conducted before the onset of the experiment in November 2016 and June 2017. Stakeholders that participated in the first workshop are the Federal Ministry of Agriculture, Regional Bureau of Agriculture, Bahir Dar University (Bahir Dar, Ethiopia), Boku University (Vienna, Austria), International Center of Agricultural Research for Dry Areas (ICARDA), Ethiopian Institute of Agricultural Research (EIAR),

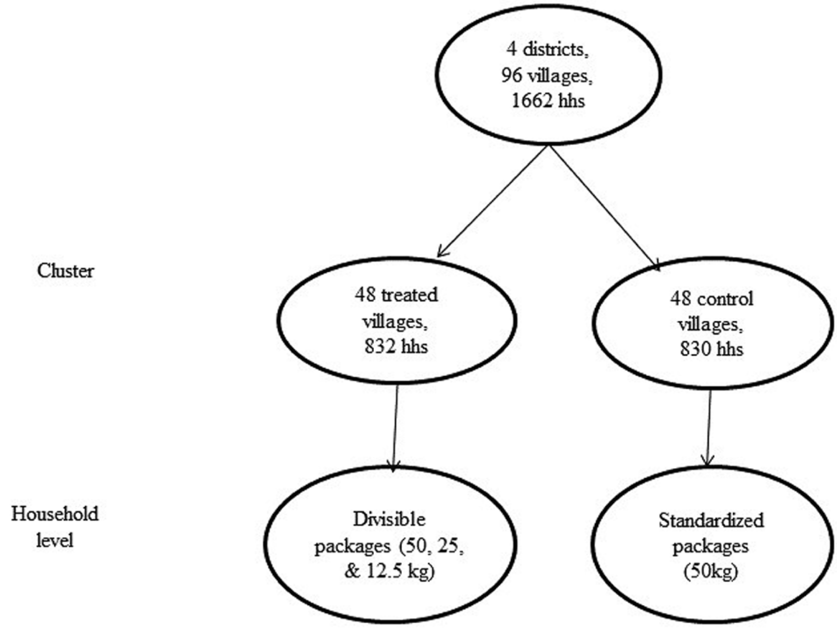

Fig. 1. Experimental design.
Notes: The figure shows that 96 villages drawn from four adjacent districts were randomly assigned into treatment and control groups. Each group consists of 48 villages. Farm households in the treatment villages had access to purchase the new variety in divisible (50, 25 and 12.5 kg) packages, while farmers in the control villages could purchase seeds of the new variety in the standardised packages (50-kg bags). The baseline information was collected on treatment and control groups from 832 and 830 farm households, respectively.

adjacent districts.[3] We then randomly assigned each village into treatment and control groups—leading to 48 villages in the treatment group and another 48 in the control group. Grouping villages into strata based on particular observable and unobservable characteristics that are believed to be correlated with the outcomes of interest can improve the balance in observable and unobservable characteristics (Bruhn and McKenzie, 2009). A cluster RCT is ideal for reducing spillover effects (Small, Ten Have and Rosenbaum, 2008), although farmers in the same cluster are exposed to the same shocks (Duflo, Glennerster and Kremer, 2008). As noted by Donner and Klar (2000), the design effect of the experiment should account for the variation within clusters, particularly to adjust the required sample size in cluster sampling.

Amhara Region Agricultural Research Institute (ARARI) and farmers from the East Gojjam Zone of the Amhara region. The second workshop participants are the Regional Bureau of Agriculture, Zonal Bureau of Agriculture, District Office of Agriculture, District Office of Cooperatives and *Gozamen* Multipurpose Farmers' Cooperative Union.

3 We determine the number of villages (clusters) based on power calculation that specifically accounts for the design effect of cluster randomisation.

The procedure we used to determine the sample size is as follows. First, we compute the sample size required for randomisation at the individual level to be 786. To do so, the minimum detectable effect (MDE) for adoption between 5- and 15-percentage points, 5 per cent significance level, 80 per cent statistical power, 0.5 standard deviations for a medium MDE and an equal proportion of treatment and control groups are taken into account. We then correct the sample size for the design effect of cluster random assignment, using the intracluster correlation coefficient (ICC).[4] Given the ICC value of 0.06 and the cluster size of 17,[5] we determine the sample size required at the cluster level, with an expected attrition rate of 2 per cent, to be 1,662.[6] We then randomly drew farm households from each of the 96 villages for the estimation of treatment effects. All the 96 villages selected from four adjacent districts[7] are wheat growing. A baseline survey, which included 832 households in the treatment group and 830 households in the control group, was then conducted before the start of the field experiment. As indicated in Figure 1, farm households in the control villages can purchase seeds of the new variety, but only in 50-kg packages, while farmers in the treatment villages have access to the new variety in divisible (50, 25 and 12.5 kg) packages.

We use the new *Kingbird* variety registered in 2015 by the national variety release committee (see MoANR, 2016) as a vehicle to test our hypothesis. *Kingbird* was newly introduced into the study area by the research project, and thus, none of the farmers previously planted it in this area. Next to yield potentials, the level of rust resistance is crucial to farmers in the varietal choice decision-making process (Jaleta *et al.*, 2019).[8] The new variety (*Kingbird*) and the most dominant old variety (*Kakaba*) grown in the study area are improved wheat varieties. The two varieties have comparable average yield potentials, but with different variances (see MoA, 2010; MoANR, 2016). The yield potential of *Kakaba* has a wider range compared to that of *Kingbird*, making *Kingbird* more advantageous in risk management. Moreover, both are generally categorised as moderate in their resistance to rust but normally differ in terms of their resistance to specific races of rust—thereby

4 The rationale for using ICC to correct the sample size for the design effect is that accounting for the correlation between the outcomes of the individuals from the same group in sample size determination contributes to power gain in the estimation.

5 We compute the cluster size, based on Van Breukelen and Candel (2012), given the ICC value and the cost associated with per additional sample and per cluster. Accordingly, the cluster size is about 17 households per village.

6 We use STATA commands '*sampsi*' to compute the sample size required for randomisation at the individual level and '*sampclus*' to correct the sample size for the design effect of cluster randomisation.

7 These study districts are Deber Elias, Baso-liben, Machakel and Gozamen in the East Gojjam zone of the Amhara region. In these districts, out of 123 villages, 107 villages are wheat growing and 96 villages are randomly selected for the study. The main wheat-growing season in the area is from April to December.

8 The important point here is that these information (attributes of the new variety) generated by breeders are blanket, because it relies in practice on few testing (or experimental) sites and represents broad agro-climatic zones and soil types (see also Ayalew, Chamberlin and Newman, 2021). Thus, the information about the parameters of a new variety is less likely to be based on farmers' particular context.

leading to differences in the yield loss that occurs in case of rust outbreak.[9] Besides reducing yield losses during favourable disease conditions, the cultivation of rust-resistant varieties has a 29–41 per cent yield advantage (Jaleta *et al.*, 2019).

Seeds of the *Kingbird* variety were new in the seed value chains of the study region. As part of the process, we first consulted with the Regional Bureau of Agriculture to introduce the new improved seeds in the input supply chain of the region and then with the main actors of seed distribution in the public sector at the grassroots level, such as the District Office of Agriculture, District Office of Cooperatives and Gozamen Multipurpose Farmers' Cooperative Union, to develop a *Kingbird* seed distribution protocol. In line with the distribution protocol and treatment assignment, rural cooperatives located in the study villages were used to channel seeds of *Kingbird* to all interested farmers. Through this channelling, except for differences in the sizes of packaging in treated and control villages, *Kingbird* seeds were made available to all farmers in all the 96 villages. Using a similar approach applied to other certified seeds, which is based on the rapid assessment of farmers' demand for improved seeds annually conducted by the District Office of Agriculture, sufficient amounts of certified seed of *Kingbird* were made available to all farmers interested to purchase from cooperatives in all the 96 villages. A total of 161.1 tons of *Kingbird* seeds were made available to farmers in the study area. From this total amount, about 86.1 tons of *Kingbird* seeds were available in 50-kg bags to farmers in the treatment and control groups, while an additional 65 tons in 25-kg bags and 10 tons in 12.5-kg bags were made available to only farmers in the treatment villages.[10] About 149.8 tons of *Kingbird* seeds were sold, constituting 93 per cent of the total seed supply, specifically 45 per cent in small while 55 per cent in large bag sizes.[11]

Cooperatives strictly follow government directives to channel agricultural inputs to farmers. Hence, the supply of *Kingbird* seeds through cooperatives contributes to the availability of small- and large-sized packages to farmers according to their assignment. The possible source of contamination of the experiment is differences in price per kilogram between the treated and control groups. To avoid this possible contamination of the experiment, the project covered the costs of the bags to ensure that the price per kilogram of the seed

9 Wheat rusts are a fungal disease, distinguished as stem, yellow and leaf rusts. Much focus is given to stem and yellow rusts, because they incur the highest yield loss. As pointed out by Vergara-Diaz *et al.* (2015), *Kingbird* is resistant to stem rust—an emerging race potentially causing complete yield loss if conditions are favourable to disease, while *Kakaba* is resistant to yellow rust, causing up to 70 per cent yield losses under favourable conditions.

10 It is important to mention that based on a rapid assessment of farmers' demand, the total supply of the seed packages to cooperatives was initially fixed across the treatment and control groups. However, in effect, the distribution of 50-kg bags was made to be flexible regardless of treatment assignment, and therefore, it reallocated from cooperatives that had low demand to those that had high demand.

11 In terms of leftover seeds, 6.3, 1.95 and 3.05 tons of *Kingbird* seeds in 25, 12.5 and 50-kg bag sizes were unsold, respectively.

Table 1. Descriptive statistics for the outcome variables

Outcome variable	Mean	SD
Farmers share *Kingbird* seeds with others	0.001	0.04
Farmers grow *Kingbird* variety	0.10	0.30
Farmers grow *Kingbird* variety only	0.02	0.15
Farmers grow both *Kingbird* and traditional varieties	0.08	0.27
Farmers grow more than one variety	0.11	0.32
Farmers self-reported wheat rust outbreaks	0.08	0.27
Number of plots allocated to *Kingbird* variety	0.11	0.01
Farmers purchased *Kingbird* seeds in 50 kg	0.06	0.23
Farmers purchased *Kingbird* seeds in 25 kg	0.04	0.19
Farmers purchased *Kingbird* seeds in 12.5 kg	0.01	0.09

Notes: The difference in mean in the area allocated to the *Kingbird* variety between the treated and control farmers is statically significant, which is about 0.04. SD denotes standard deviation.

is the same across the study villages.[12] As a result, both treated and control farmers purchased *Kingbird* seeds at a uniform price of ETB 14.73/kg. Another possible contamination scenario would be if farmers pool financial resources to divide *Kingbird* seeds in 50-kg bag size into smaller-sized seed packages. We collected the data on the bag sizes that individual farmers purchase and the amount of *Kingbird* seeds used. The information is provided in Table 1. In Section 6.5, we test whether any experiment contamination is likely to influence the impact of the treatment.

3. Conceptual framework

The primary goal of the experiment is to test whether the adoption of the new technology is higher in villages where divisible seed packages that provide options to farmers are available than in villages where only a standard large-sized package with no other option is available. The notion is that the availability of small packages encourages farmers to adopt the new variety at a smaller scale, thereby learning-by-doing (Foster and Rosenzweig, 1995). In this section, we present a simple framework to guide our empirical analysis. In particular, we show how input divisibility helps farmers to experiment with smaller quantities—thereby leading to increased adoption of the new variety. As argued by Foster and Rosenzweig (2010), farmers make decisions on whether to adopt the new variety when they have sufficient information about the benefits and risks of adopting. Input divisibility in this regard allows farmers to test the new variety on small plots of land to generate site-specific information about the parameters of the variety that ultimately reduce the

12 It is important to mention that the project covered the costs of the bags used only to package the seeds in 12.5- and 25-kg bags. Otherwise, we maintained the existing seed supply system, particularly standard packaging, channelling improved seeds through rural cooperatives and assessment of farmers' demand for improved seeds. Note that the difference in the price of seeds of *Kingbird* and *Kakaba* varieties was very small, actually less than ETB 1/kg. The approximate exchange rate in June 2017 was from ETB 22.4137 to USD 1, ETB = Ethiopian Birr.

risks and uncertainties associated with the newly introduced variety parameters. Thus, we employ the simple target-input model presented in Bardhan and Udry (1999) to illustrate these relationships. In line with this framework, we assume that the farmer updates his prior beliefs about the parameters of a new variety.[13] Thus, the parameters of the new variety and the farmer's prior knowledge are taken into account in the model. The traits of the new variety that are related to soil characteristics are generally assumed to vary across fields. Hence, although the farmer knows the underlying production technology, the target input is unknown. The target-input model is presented as follows:

$$q_{it} = 1 - (k_{it} - \kappa_{it})^2 \tag{1}$$

where q_{it} and k_{it} are the yield obtained and the input used by farmer i in period t, respectively, and κ_{it} is the target input. However, farmer i is uncertain about the target input level at the time the input is chosen. He updates his prior beliefs only after using the input and observing the output level and then obtains better information on the optimal target κ^* on average. The target input is therefore expressed as follows:

$$\kappa_{it} = \kappa^* + \mu_{it} \tag{2}$$

where μ_{it} is an i.i.d. shock with mean zero and variance σ_u^2. In period t, farmer i does not know κ_{it}, but he has beliefs about κ^*, which is assumed to be distributed as $\mathrm{N}(\kappa^*, \sigma^2_{\kappa_{it}})$. Farmer i, who learns from his trials, updates his beliefs in period t about the variance of κ^*, after observing k_{it}, by applying Baye's rule. The Bayesian learning yields a posterior belief:

$$\sigma^2_{\kappa_{it}} = \frac{1}{\frac{1}{\sigma^2_{\kappa_{it}}} + \frac{1}{\sigma^2_u}} \tag{3}$$

Let's define $\rho_0 = 1/-\sigma_u^2$ as the precision of the information generated by observations from his trial, and $\rho_{i0} = 1\Big/-\sigma^2_{\kappa_{i0}}$ as farmer i's initial beliefs about the true value of κ^*. Equation (3) can be rewritten as follows:

$$\sigma^2_{\kappa_{it}} = \frac{1}{\rho_{i0} + I_{t-1}\rho_0} \tag{3'}$$

where I_{t-1} is the number of trials i has observed the new variety on his plot between period 0 and period $t-1$. As noted by Bandiera and Rasul (2006), there are no returns to experimentation, because the information generated by i's trials is independent of the amount of the input used, implying that farmers experiment with the new variety anticipating future profits. Given this, farmers are more likely to try the new variety in the first period to increase their

13 As shown in Table A1 (Appendix in supplementary data at ERAE online), we use the male gender, because male-headed households (92 per cent) are dominant in the study area.

knowledge about the actual target input, with a minimum cost of experimentation.[14] Thus, we model the farmer's decision to try the new *Kingbird* variety to learn about the parameters of the new variety from his trials, when the *Kingbird* seeds are available in divisible packages. The package sizes in which *Kingbird* seeds are made available to farmers are hypothesised to influence farmers' decisions to try the new variety on a small plot of land, as the cost of trying the new variety declines with farm size. If the availability of input supply in small bags enables farmers to experiment with the new variety, adoption among treated farmers should be higher than control farmers.

4. Empirical analysis

In this section, we describe how we test the predictions from the conceptual framework. We also discuss complementary empirical models to ascertain the effect of our intervention. To examine the impact of our intervention on farmers' decision to try the new variety, we first use ordinary least squares (OLS) to estimate the intent-to-treat (ITT). OLS is suitable because our RCT solves the selection problem for the estimation of the mean outcome difference associated with assignment to treatment (Duflo, Glennerster and Kremer, 2008). We use two indicators of farmers' decision to try the new variety. These include a binary choice of adoption or non-adoption and the intensity of adoption, which is measured as the percentage share of *Kingbird* to the total wheat area cultivated by the household. We use the ITT for estimating the propensity and intensity of adoption, given that *Kingbird* is an entirely new wheat variety to the study areas. We specify the ITT in a reduced-form regression as follows:

$$Y_i = \alpha_1 + \beta_1 D_i + \mu_1 X_i + \eta_j + \varepsilon_{i1} \tag{4}$$

where Y_i represents the observed outcome variables—in our case, the adoption of the new wheat variety, which takes a value of 1 if a farmer adopts *Kingbird* and 0 otherwise, and the share of land allocated to the variety. D_i is a household level indicator that equals 1 if the household is from the treatment village and 0 otherwise. β_1 is the parameter of interest, indicating the difference-in-mean

14 As rightly noted by an anonymous reviewer, since the variety we used to test our hypothesis is rust-resistant, farmers' anticipation might go beyond future profits (or yield gains). Given the risk reduction property of the variety, farmers might opt for changing the traditional variety with one which is resistant to a specific race of rust and/or for increasing the number of varieties grown (for diversification). On the other hand, given the varietal release modality discussed earlier, farmers know the attributes of the new variety such as yield potential and level of resistance to rust. This enables farmers to handle varietal choice carefully, and specifically, they at least need to ensure comparable yield with that of the existing varieties. Moreover, in line with the assumptions of the target input model, the target input level is random and ex-ante unknown, irrespective of whether the new variety is resistant to rust or not. Therefore, unless farmers know the specific features of that new variety, they are less likely to use the newly introduced variety primarily to replace the existing variety and/or to diversify varieties grown to reduce their risk exposure to wheat rust outbreaks (Di Falco and Chavas, 2009; Foster and Rosenzweig, 2010). Despite this, examining this issue by using the second and third moments of yield distribution might provide additional insights into farmers' strategies for reducing risk exposure, particularly during the experimentation stage.

between the treatment and control groups, and ε_{i1} is an unobserved individual-level shock. We adjust the covariates X_i and location fixed effects η_j to improve the efficiency of the estimates (Duflo, Glennerster and Kremer, 2008).

Given that potential mechanisms through which seeds in divisible bags may influence farmers' decisions to test the new variety, we use causal mediation analysis.[15] Thus, we employ a linear structural equation model with an interaction term (see Imai, Keele and Tingley, 2010) to estimate the causal mediation effect. In addition to Equation (4), we thus specify the following equations:

$$M_i = \alpha_2 + \beta_2 D_i + \mu_2 X_i + \eta_j + \varepsilon_{i2} \tag{5}$$

$$Y_i = \alpha_3 + \beta_3 D_i + \gamma M_i \ + \kappa D_j M_i + \mu_3 X_i + \eta_j + \varepsilon \tag{6}$$

where M_i is the intermediate variables (causal mechanisms or mediators), such that the number of varieties grown, liquidity constraint status, which is equal to 1 if farmers are liquidity-constrained to purchase *Kingbird* seed supplied in 50-kg package and 0 otherwise, and the choice of payment for seeds, which is equal to 1 if farmers paid immediately and 0 otherwise. β_2, γ and κ are the parameters of interest, indicating whether an average mediation effect exists. If all these parameters are statistically different from 0, we can conclude the presence of the mediation effect. We estimate each linear equation by OLS and use the product of coefficients ($\beta_2\gamma$) as the mediation effect.

We also use OLS to estimate the effect of the treatment on wheat yields, using the specification in Equation (4). This estimate specifically is important to validate our conceptual framework, because yield gains are not expected in the experimentation period.[16] Furthermore, as indicated earlier, the variety we used for testing our hypothesis has a risk reduction parameter, since farmers may choose the variety to reduce their risk exposure, besides learning about the random and ex-ante unknown parameters of the variety (Issahaku and Abdulai, 2020a). To examine the risk reduction role of *Kingbird* adoption, we employ the moment-based approach[17] suggested by Antle (1983) and used in other studies (e.g. Di Falco and Chavas, 2009; Issahaku and Abdulai, 2020a). Specifically, we compute the yield variance and skewness distributions, which are approximated by the second and third moments of crop yield distribution. While the variance captures yield variability, the skewness tends to capture farmers' exposure to downside risk. The moments of crop yields are estimated through a sequential approach. First, we obtain an estimate of the mean effect by regressing yields on farm inputs and farmers' socio-economic characteristics, farm-level and location fixed effects, after which the residuals are retrieved. The variance or second moment of yields is estimated by squaring the residuals, while the third moment or skewness is obtained by

15 We thank an anonymous referee for suggesting the inclusion of mediation analysis.

16 Given this, the study is powered by targeting the propensity of adoption, which is indicated in Section 2.

17 We thank an anonymous referee for suggesting the inclusion of this approach.

utilising the estimated errors raised to the third power. The estimated variance and skewness of crop yields are used as outcome variables and regressed on the same set of explanatory variables to examine the impact of the adoption of *Kingbird* on risk exposure.

Due to the fact that the ITT, as shown in Equation (4), estimates the average treatment effects (ATEs), it is not able to reveal the most and the least beneficiary of seed delivery in small bags. In Section 6.3, we briefly review the methodological development of heterogeneity analysis and use the machine learning methods to analyse heterogeneity effects. In the interest of brevity, we present the equations for machine learning methods in the Appendix in supplementary data at ERAE online.

5. Data and tests

5.1. Data collection

A balanced panel data was generated through two farm household surveys, one before and another after the 2017 growing season, during which the experiment was conducted. The survey instruments we used for both the first and second surveys were structured and pre-tested questionnaires. In the first-round survey, we collected detailed information on household demographics, asset holdings, affiliation to institutions, income and wheat yields. The baseline data are significant in verifying the balance of covariates in treatment and control groups, improving the precision of the estimates and estimating the heterogeneous treatment effects. For the main outcome of this study, in the follow-up survey, we interviewed the same households in the baseline about the number and respective areas of wheat fields they cultivated that year, the variety they used in each field, their source of seed, if they purchased certified seed, the size of the seed package, inputs of production and yields.

5.2. Balance test

Due to the fact that treatment and control groups are randomly assigned, we expect no systematic differences between the two groups at the baseline. Nevertheless, we conducted a balancing test and reported the results in Table A1 (Appendix in supplementary data at ERAE online). Columns (1) and (2) report the mean of control and treated groups, while columns (3) and (4) report mean differences between the control and treated groups and *p*-values of the mean difference. In column (5), we report normalised differences, which are useful for assessing the sensitivity of the estimates of the ATE to alternative model specifications and outliers (Imbens and Rubin, 2015). More importantly, we examine the distribution of propensity score matching of treated and control groups, conditional on observed characteristics of households, targeting robust estimates of conditional ATE (CATE) (Imbens and Rubin, 2015).

The results in column (4) show a good degree of balance in most of the observed characteristics across the treated and control groups. Moreover, with values of less than one-quarter, which Imbens and Rubin (2015) set as a rule of thumb for measuring the sensitivity of linear regression to model specification, the computed normalised differences in column (5) indicate that the average covariate values in the treated and control groups are not substantially different. Besides assessing balance in the univariate distributions, we examine the overall balance in the covariate distributions using the propensity score (see Imbens and Rubin, 2015). Figure A1 (Appendix in supplementary data at ERAE online) displays overlap in the covariate distributions. The figure shows a strong overlap in covariate distribution, and hence, the distributions of measured baseline covariates in the two groups are similar. Given this overall balance of covariates, we include covariates in the estimation to account for the significant mean differences of some covariates such as the age of household head and household size between the two groups to improve the precision of the estimates.[18] The covariates are also used to examine treatment effect heterogeneity, as well as to identify the most and least affected households by the intervention. Our balancing tests also indicate that the status of farmers' credit constraints and the prevalence of wheat rust are balanced across the two groups. On the outcome of interest, the number of varieties grown is balanced between the treated and control groups.

5.3. Attrition bias

We collected the main outcome variables during the end-line survey from the same households included in the baseline. During the end-line survey, only 1,485 households were interviewed, indicating that not all households in the baseline survey appeared in the second wave. The attrition rate was 11.5 per cent in the treatment group and 9.5 per cent in the control group. We therefore test whether there was systematic attrition bias between the treatment and control groups, and whether this attrition rate is related to the pre-treatment values of the variables. This enables us to test whether attrition had compromised the internal validity of the results. We estimated a probit model (Fitzgerald, Gottschalk and Moffitt, 1998) and treatment-effect bounds for non-random sample selection to examine the correlation between attrition and treatment status (Lee, 2009).

Our results, which are presented in Table A2 (Appendix in supplementary data at ERAE online), show that the attrition rate has no significant relationship with household treatment status, their observed characteristics as well as their interaction. Specifically, only the variable indicating the nearness of a district to the main road shows a significant (at 10 per cent level) relationship

18 Thus, we control for covariates in the empirical analysis. The covariates included are the gender of household head, age of household head, schooling of household head, household size, cultivated farm size, annual household income, access to extension services, credit constraints status and cooperative membership. It is useful to mention particularly how farmers are classified as credit-constrained and non-constrained. We classified farmers as credit-constrained if they sought for, but were unable to obtain credit or they obtained, but the amount was inadequate, while farmers who were able to obtain sufficient credit or did not seek credit at all are not credit-constrained (Abdulai and Huffman, 2014).

with the attrition rate. The results from the test for attrition bias using the probit model are consistent with the estimates of the treatment-effect bounds. The point estimates of our outcome variables statistically fall within the true treatment effect bounds (Table A3 in the Appendix in supplementary data at ERAE online). Since we do not find systematic differences in the values of the outcome and covariate variables between absentee and attendee households across treatments, we concluded that our study is unlikely to suffer from attrition bias.

We also estimate the ICC for the main outcome variables. The estimates, which are presented in Table B1 (Appendix in supplementary data at ERAE online), show that the ICC values obtained after the end-line survey are close to 0, indicating within-cluster variations and similarities across clusters, and as such contribute to power gain in the estimation. Although the values of ICC are small, we account for the correlation between outcomes of individuals from the same village in the estimation. A point noteworthy is the fact that the standard methods for cluster-robust inferences perform well when the clusters are fairly homogeneous in terms of the number of observations and the characteristics of the regressors and disturbance (MacKinnon, 2019). Thus, we conducted a robustness check on the performance of the standard methods in contrast to the emerging methods in statistical inferences such as randomisation inference and wild cluster bootstrap and presented the results in Table B2 (Appendix in supplementary data at ERAE online). The results indicate that clustering the standard errors at the village level provides robust inference. We therefore reported heteroscedasticity-robust standard errors clustered at the village level in all inferences. We also presented the descriptive statistics of the outcome variables collected in the end-line survey in Table 1. The information in Table 1 shows that 10 per cent of farmers grow the new *Kingbird* variety, with about 2 per cent growing exclusively the *Kingbird* variety and 8 per cent of them growing both *Kingbir*d and traditional varieties. About 11 per cent of farmers grow more than one variety. About 8 per cent of farmers reported the prevalence of wheat rust in the 2018 growing season, which is lower than 20 per cent in the previous growing season.

6. Results and discussion

6.1. Propensity of adoption

Table 2 presents the estimates of the impact of input supply in divisible packages on smallholder farmers' decisions to try *Kingbird* at the early stage of its introduction. The results show that access to divisible seed packages has a positive and statistically significant impact on the decision to try the new variety. Smallholder farmers with access to the *Kingbird* variety in divisible packages are more likely to try it than those without access by 4.2-percentage points. As indicated earlier, farmers interested in the new variety may want to try it on smaller portions of their land, before scaling up to cover large portions. Thus, farmers appear to prefer a piecemeal approach rather than replacing the old variety with the new one at once. During the end-line survey, we collected information on wheat varieties grown in the area. The information in Table 1

Table 2. Treatment effects on the propensity of adoption

	Coeff.	SE
Treatment effect	0.042**	0.019
Gender of household head	0.057***	0.018
Age of household head	−0.000	0.001
Schooling of household head	0.003	0.003
Household size	0.005	0.004
Annual household income	0.000	0.000
Cultivated farm size	0.043	0.030
Extension services	0.049***	0.017
Credit constraints	−0.005	0.016
Cooperative membership	0.020	0.021
Constant	−0.094***	0.032
Observation	1,485	
R^2	0.032	

Notes: We control for covariates and location fixed effects in both estimations. The covariates included are the gender of household head, age of household head, schooling of household head, household size, cultivated farm size, annual household income, access to extension services, credit constraints status and cooperative membership. SE denotes robust standard errors clustered at the village level.
***$p < 0.01$,
**$p < 0.05$

reveals that many *Kingbird* adopter farmers' (8 per cent) grow both the new and the traditional varieties. The decisions by smallholder farmers to try the new variety on smaller portions of their land are consistent with the notion of learning about the parameters of a new variety from their trials.

We also estimate the proportion of the treatment effect on adoption mediated by the purchase of *Kingbird seeds* in divisible package sizes, using specifications in Equations (4)–(6).[19] Table 3 presents the estimates of the causal mediation analysis. The results in column (2) reveal that the effect of the treatment on adoption is mediated by the availability of *Kingbird* seeds in 25-kg packages. This finding is consistent with our hypothesis that the availability of improved seeds in divisible packages will enable farmers to try the new variety. However, as shown in columns (1) and (3), the treatment effect is not mediated by the purchase of *Kingbird* seeds in 12.5- and 50-kg package sizes. In addition to the availability of seeds in divisible packages, the possible causal mechanisms that might explain the adoption of *Kingbird* are discussed in Section 6.4. Here, we also estimate treatment effect heterogeneity in package sizes. For brevity, the results are presented in Table C1 (Appendix in supplementary data at ERAE online). We find that the difference in package size results in treatment effect heterogeneity. In particular, the treatment effects among farmers who purchase the seeds in standard 50-kg packages compared to those who purchase the seeds in the sizes of 25- and 12.5-kg packages are statistically different. A noteworthy point is that not only package sizes but also observed

19 We use the R package for causal mediation analysis (most up-to-date '*mediation*' package) (see Imai *et al.*, 2019).

Table 3. Treatment effects on the adoption of *Kingbird* mediated by purchased package sizes

	Purchased *Kingbird* seeds		
	12.5-kg bags	25-kg bags	50-kg bags
	(1)	(2)	(3)
Average mediation effect	0.004	0.051***	−0.015
	[−0.004, 0.01]	[0.033, 0.07]	[−0.039, 0.01]
Average direct effect	0.039**	−0.008	0.057***
	[0.008, 0.07]	[−0.036, 0.02]	[0.037, 0.08]
Total effect	0.043**	0.042***	0.043**
	[0.010, 0.07]	[0.011, 0.07]	[0.012, 0.08]
Proportion mediated	0.085	1.18***	−0.34
	[−0.155, 0.38]	[0.716, 3.47]	[−2.346, 0.14]

Notes: Columns (1), (2) and (3) report the proportion of treatment effects on adoption mediated by the purchase of 50-, 25- and 12.5-kg package sizes, respectively. We control for covariates and location fixed effects in all estimations. The covariates included are the gender of household head, age of household head, schooling of household head, household size, cultivated farm size, annual household income, access to extension services, credit constraints status and cooperative membership. Figures in squared brackets are the confidence intervals.
***$p < 0.01$,
**$p < 0.05$

characteristics of farmers might result in treatment heterogeneity. We analyse the most and the least impacted households, given their observed characteristics by the intervention using machine learning methods. The estimates are presented in Section 6.3.

6.2. Intensity of adoption and wheat yields

In Table 4, column (1) reports the estimates for the ITT effects of divisible input supply on the share of *Kingbird* to the total wheat areas using the specification in Equation (4). The estimates of ITT effects of our intervention on the intensity of adoption of the *Kingbird* variety are consistent with the estimates for the propensity of adoption. The results reveal that our intervention positively influenced farmers' decisions to allocate more land to the *Kingbird* variety. In particular, the ITT estimates show that when smallholder farmers have access to divisible seed packages of *Kingbird*, the share of *Kingbird* to the total wheat areas allocated increases by 2.9-percentage points. Moreover, the number of farm fields allocated to *Kingbird* provides some insights into the significance of improved input marketing in enhancing the diffusion of new agricultural technologies. The mean farm field allocated to *Kingbird* in the treated villages is greater than the control villages by 0.04 units (see Table 1, *Notes*).

It is well argued that if the expected net benefits of adopting the new variety are higher than not adopting, farmers replace their old variety with the new variety. We thus examine yield heterogeneity of *Kingbird* and *Kakaba* varieties prior to estimating the impact of our intervention. The estimates on yield heterogeneity are presented in Table C2 (Appendix in supplementary data at

Table 4. Estimates of the intensity of adoption, wheat yields and farm net returns

	Intensity of adoption		Wheat yields: ITT		Wheat yields: LATE		Farm net returns	
	(1)		(2)		(3)		(4)	
	Coeff.	SE	Coeff.	SE	Coeff.	SE	Coeff.	SE
Treatment effect	2.90***	0.916	0.011	0.033	0.232	0.709	0.021	0.059
Gender of household head	3.83**	0.721	0.089	0.048	0.074	0.067	−0.047	0.079
Age of household head	−0.020	0.035	−0.004**	0.002	−0.003	0.002	0.001	0.001
Schooling of household head	0.260	0.191	0.013**	0.006	0.012	0.006	−0.003	0.010
Household size	0.067	0.169	0.003	0.009	0.002	0.010	−0.011	0.011
Annual household income	−0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Cultivated farm size	0.510	0.967	−0.012	0.043	−0.021	0.045	0.055	0.041
Extension services	2.09*	1.14	0.058	0.037	0.046	0.050	−0.010	0.048
Credit constraints	−0.596	0.898	0.013	0.032	0.015	0.033	0.028	0.034
Cooperative membership	0.068	1.41	0.076**	0.032	0.072	0.033	0.029	0.052
Constant	−3.38**	1.59	1.19***	0.099	1.21	0.111	8.60	0.117
Observation	1,485		1,318		1,318		1,304	
R^2	0.024		0.030			0.016	0.005	

Notes: Column (1) reports the share of *Kingbird* to the total wheat areas. Columns (2) and (3) report the ITT and treatment on the treated estimates of wheat yields, respectively. We use OLS and instrumental variable methods to estimate the ITT and LATE, respectively. In particular, we estimate LATE by instrumenting the adoption of the *Kingbird* variety with the randomised assignment to treatment. Column (4) reports the log form of farm net returns of wheat production measured in tons per acre, computed by deducting variable costs from wheat gross revenue. We control for covariates and location fixed effects in all estimations. The covariates included are the gender of household head, age of household head, schooling of household head, household size, cultivated farm size, annual household income, access to extension services, credit constraints status and cooperative membership. SE denotes robust standard errors clustered at the village level.
***$p < 0.01$,
**$p < 0.05$,
*$p < 0.1$.

ERAE online). In particular, the interaction effects indicate that yield heterogeneity does not appear in growing *Kingbird* and *Kakaba* varieties. In Table 4, column (2) presents the ITT estimates of the impacts of divisible input supply on wheat yields using the OLS specification in Equation (4). In column (3), we report the local ATE (LATE) estimates of wheat yields.[20] Although the results of LATE are higher than ITT estimates as expected, both estimates are not statistically significant. We find no evidence that the effect of additional adoption due to divisible seed packaging leads to increased wheat yields.[21] We also estimate the impact of the adoption of *Kingbird* on farmers' net returns[22] in column (4). The estimates reveal that the adoption of the *Kingbird* variety has a positive, but statistically insignificant impact on farmers' net returns, which is expected during the stage of technology experimentation (see also Rogers, 1962).

Table 5 presents the variability of wheat yields and the downside risk of wheat production.[23] In column (1), we find that the adoption of *Kingbird* has a positive and statistically significant effect on the variance of wheat yields. Moreover, due to the fact that the second moment of yield distribution does not enable us to identify the unexpected good and bad outcomes, we report the effect of the adoption of *Kingbird* on the skewness of wheat yields or downside risk in column (2), as smallholders tend to avoid the unexpected bad outcomes. The results show that the adoption of *Kingbird* has no significant role in reducing exposure to downward risk at least in the first period. The regression results of variance and skewness functions are consistent with our conceptual framework, indicating that farmers are more likely to opt for the new variety to experiment on their plots in the first period in anticipation of future profits by increasing their knowledge about the actual target input (see Foster and Rosenzweig, 2010).

6.3. Heterogeneous treatment effects

Besides the ATEs, policymakers and analysts have also been interested in heterogeneous treatment effects. Many studies have examined heterogeneous treatment effects by using subgroup analysis in which the treatment effects

20 We estimate the LATE by instrumenting the adoption of the *Kingbird* variety by the randomised assignment of treatment, using two-stage least squares (2SLS) regression. For brevity, we explain this and the model specification used to estimate LATE in Appendix D in supplementary data at *ERAE* online.

21 Although it is intuitive to expect a significant increase in farmers' yield in treated villages, given the significant number of *Kingbird* adopters in the treated villages, we find no evidence of yield increase due to new improved seeds supply in divisible bags. Many factors can affect the yield of the new variety such as the attributes of the variety and farmers' knowledge to optimally manage the variety. For example, under conditions favourable for diseases as stated earlier, the *Kingbird* variety provides higher yields than the traditional one (*Kakaba*); otherwise, both varieties provide on average the same yields. However, it is also relevant to note that the rate of take-up (the first stage) and attrition affects the estimates of the yield outcomes (the second stage).

22 We compute farm net returns by subtracting variable costs from wheat gross revenue per acre.

23 The full estimated results, including the mean wheat yields estimated, are available in Table E1 (Appendix in supplementary data at ERAE online).

Table 5. Estimation of variance and skewness of yield

	Variance of yield	Skewness of yield
	(1)	(2)
Adoption of *Kingbird*	0.019*** (.008)	0.006 (0.006)
Inputs of production	Yes	Yes
Covariates	Yes	Yes
Fixed effect	Yes	Yes
Constant	−0.074 (0.029)	−0.062 (0.061)
Observation	1,318	1,318
R^2	0.059	0.024

Notes: Columns (1) and (2) report the variance and skewness of wheat yield, respectively. We control for inputs, covariates and location fixed effect in all estimations. The inputs included are the log of labour, log of farm size, log of NPS, log of UREA, log of herbicides, dummy of herbicide use and the use of machinery for threshing. The covariates included are the gender of household head, age of household head, schooling of household head, household size, annual household income, access to extension services, credit constraints status and cooperative membership. Figures in the parentheses are the robust standard errors clustered at the village level.
***$p < 0.01$

are estimated for each group, such as gender, education or age. However, as pointed out by Cook, Gebski and Keech (2004), a potential problem with this approach is that analysts may intentionally choose subgroups with higher treatment effects or report only extreme effects. Chernozhukov *et al.* (2018) also argue that choosing a large number of subgroups could result in overfitting in the model. For estimating heterogeneous treatment effects, classical non-parametric methods such as nearest neighbour matching method, kernel method and series estimation perform well in applications with a small number of covariates (Wager and Athey, 2018), while machine learning methods outperform in the case of large numbers of covariates (Chernozhukov *et al.*, 2018).

Random forests are widely used machine learning methods and perform well in practice for prediction but do not outperform in terms of statistical properties (Athey and Imbens, 2017). Thus, we focus on forest-based machine learning methods that allow for valid asymmetric theory and statistical inference. We first employ GRF proposed by Athey, Tibshirani and Wager (2019) to examine whether our intervention results in heterogeneous treatment effects on the propensity of farmers' adoption decisions. GRF predicts the CATE for subgroups as the weighted mean difference between the treated and control groups.[24] The results, which are presented in Figure 2,[25] show that the

24 It is useful to mention that GRF helps to account for relatively small sample size, explained in detail in Figure 2, *Notes*. For brevity, the equations of GRF and CLAN discussed below are presented in Appendix F in supplementary data at ERAE online.

25 As noted by Wager and Athey (2018), although honest forest training options reduce bias in tree predictions, it performs less when the data set is small, because it further cuts the subsample that is already small in half and does not give enough information to choose quality splits. Note that the subsample is used for determining tree splits. We mitigate this limitation using a special honest forest training option that allows increasing the fraction of samples used in selecting splits (i.e. *honesty.fraction* in R package). The honesty fraction we used is about 0.7, which directs

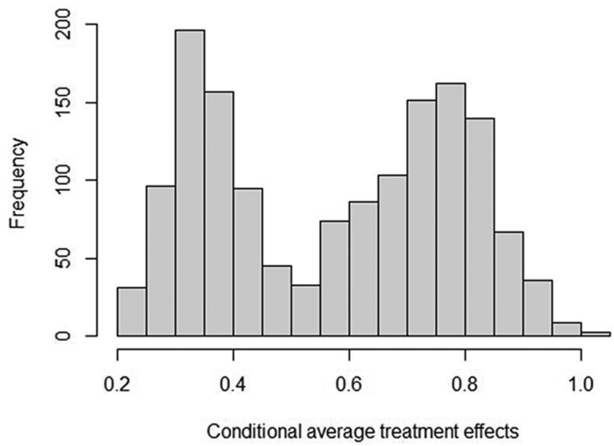

Fig. 2. Histogram of CATEs.
Notes: We examine treatment heterogeneity using *grf* algorithm developed by Tibshirani *et al*. (2019). In doing so, we use 50 per cent of the data for training, while the remaining is for testing, with 4,000 trees and leaves ranging from 215 to 311. In the data training process, we allow for covariates that induce more than 10 per cent of the treatment effect heterogeneity.

differences in the subpopulation treatment effects provide evidence of heterogeneity in treatment effects on the propensity of adoption for the observed variables. It is evident from Figure 2 that treatment effects are positively distributed.

Thus, we further analyse who are most impacted by the intervention, given the observed characteristics. We use the classification analysis (CLAN) of generic machine learning developed by Chernozhukov *et al*. (2018) to identify the average characteristics of the most and least affected households from the intervention.[26] CLAN shows a significant difference between the most affected and least affected households presented in Table 6, column (3). The estimates show that although the majority of sampled households are male-headed, only about 11 per cent of them are among the most impacted by the seed delivery intervention. Interestingly, the results reveal that the most impacted farm households by the intervention are relatively young and poor, indicating that besides experimenting with the new variety in the first period, seed delivery in small bags may enhance the use of improved seeds in subsequent years by smallholders, ultimately increasing agricultural productivity. We also find that farmers who are credit-constrained and a member of rural

GRF to use 70 per cent of the tree subsample for splitting. The covariates considered are the gender of household head, age of household head, schooling of household head, household size, cultivated farm size, annual household income, access to extension services, credit constraints status and cooperative membership.

26 We thank the anonymous referee for suggesting the inclusion of CLAN .

Table 6. CLAN of seed delivery in small bags

	Most affected	Least affected	Difference
	(1)	(2)	(3)
Gender of household head	0.981 (0.959, 1.00)	0.876 (0.854, 0.897)	0.105 (0.075, 0.136) [0.000]
Age of household head	42.57 (41.45, 43.70)	45.27 (44.15, 46.40)	−2.77 (−4.35, −1.20) [0.008]
Schooling of household head	1.81 (1.59, 2.02)	1.44 (1.22, 1.67)	0.379 (0.061, 0.695) [0.100]
Household size	5.52 (5.35, 5.69)	5.48 (5.31, 5.66)	0.035 (−0.207, 0.276) [1.00]
Annual household income	25,125 (22,229, 27,709)	29,909 (27,062, 32,454)	−4789 (−8,565, −982.9) [0.081]
Cultivated farm size	0.447 (0.418, 0.471)	0.478 (0.451, 0.506)	−0.033 (−0.072, 0.004) [0.283]
Extension services	0.833 (0.800, 0.866)	0.795 (0.761, 0.829)	0.038 (−0.010, 0.086) [0.399]
Credit constraints	0.523 (0.480, 0.566)	0.416 (0.374, 0.459)	0.105 (0.045, 0.165) [0.008]
Cooperative membership	0.941 (0.911, 0.970)	0.776 (0.747, 0.805)	0.156 (0.116, 0.198) [0.000]

Notes: Columns (1) and (2) report the median level of the covariate in the most and least affected groups. The differences between columns (1) and (2) are reported in column (3). 90 per cent confidence interval in parentheses. Figures in squared brackets are *p*-values for the hypothesis that the parameter is equal to 0.

cooperatives are most affected by the intervention.[27] The result implies that supplying seed in small bags relaxes liquidity constraints, and hence farmers' credit constraint status is likely to be changed. This finding is consistent with the previous studies (see Karlan *et al.*, 2014; Shahzad and Abdulai, 2021) that indicates increased access to credit can help farmers surmount short-run liquidity constraints and directly influence the adoption of new agricultural technologies.

27 As discussed above, our results in column (2) of Table 2 are in line with our assumption, specifically supplying seed in small bags might relax liquidity constraints and made a change in the credit constraint status of farmers. This hypothesis is further supported by the CLAN by accounting for credit constraint status during the baseline year.

6.4. Mechanisms for adoption

Although our estimates show some evidence of the positive impact of divisibility of seed bags on adoption, it does not explicitly indicate how much effect arises. To this effect, we examine potential mechanisms that might explain the adoption that we observe in the analysis. The potential mechanisms we examine are related to learning, liquidity and preferences (behavioural). The first mechanism we examine relates to the uncertainty of the new variety. Farmers who are uncertain about the new variety and have access to seeds in divisible bags are interested in testing the new variety on small plots of land and will tend to plant the new one in addition to the traditional varieties. The second mechanism we examine is the extent to which seed divisibility can encourage liquidity-constrained farmers to adopt the new variety, because our intervention potentially relaxes farmers' liquidity constraints, by allowing them to invest small amounts of money on small seed quantities. In particular, where credit constraints bind, because of the inadequate amount of credit to purchase seeds in the standard package size, seed divisibility can encourage liquidity-constrained farmers to grow the new variety. A third mechanism we examine is the role of present bias preferences in explaining adoption. Besides the need to overcome liquidity constraints, farmers may prefer paying the cost of the seed immediately by purchasing small bags. Farmers normally purchase improved seeds by ready funds (immediate payment) or seed vouchers (later payment). Thus, the present-biased preferences might contribute to farmers purchasing the small bags.

The estimates of the mediation analysis are presented in Table 7, using specifications in Equations (4)–(6). We present the differences in the number of planted wheat varieties by treated and control farmers in column (1). The average mediation effect is statistically significant, constituting about 47 per cent of the total effects. This finding is consistent with the result of skewness of wheat variety in Table 5, column (2), suggesting that farmers planted the new variety for the first period in addition to the traditional varieties for experimenting. In Table 7, column (2), the estimates of seed divisibility on liquidity constraints are presented. The estimates reveal no statistically significant difference in liquidity constraints between the treatment and control groups. The estimates of the present bias preferences, which are presented in column (3), show that there is no significant difference between the treated and control farmers regarding the choice of payment for seeds. Our findings support the notion that a larger number of planted wheat varieties is the causal mechanism through which the divisibility of seed packages causes farmers to plant the new wheat variety. In contrast, liquidity constraints and present bias preferences do not explain the adoption of the new variety.

6.5. Robustness checks

In this section, we investigate the robustness of our results. Thus, we conduct checks to ensure that the validity of the high adoption rate of the improved *Kingbird* variety is indeed a consequence of the intervention. Some of the

Table 7. Estimates of mechanisms for adoption

	Wheat varieties	Liquidity constraints	Present bias preferences
	(1)	(2)	(3)
Average mediation effect	0.020**	−0.0004	−0.001
	[0.004, 0.04]	[−0.002, 0.00]	[−0.005, 0.00]
Average direct effect	0.022*	0.042***	0.044***
	[−0.004, 0.05]	[0.010, 0.07]	[0.011, 0.07]
Total effect	0.042***	0.042***	0.042***
	[0.011, 0.07]	[0.009, 0.07]	[0.010, 0.07]
Proportion mediated	0.472**	−0.005	−0.030
	[0.111, 1.35]	[−0.092, 0.030]	[−0.196, 0.03]

Notes: Columns (1), (2) and (3) report potential mechanisms that explain the positive adoption of the new variety, particularly, learning, liquidity constraints and present bias preference mechanisms, respectively. Column (1) reports the total number of wheat varieties grown between the treated and control farmers. In column (2), we classified farmers as liquidity-constrained, if they are not able to purchase *Kingbird* seed supplied in the standardised package by ready funds, or 0 otherwise. In column (3), we consider the choice of the time horizon in which farmers prefer to pay the cost of the seed, denoting payment choice as immediate if farmers made immediate payment for the cost of the seed, and 0 otherwise. We control for covariates and location fixed effects in all estimations. The covariates included are the gender of household head, age of household head, schooling of household head, household size, cultivated farm size, annual household income, access to extension services, credit constraints status and cooperative membership. Figures in squared brackets are the confidence intervals.
***$p<0.01$,
**$p<0.05$,
*$p<0.1$.

caveats in cluster RCTs that require special attention are selection bias and programme placement. We thus examine whether the ITT estimates are influenced by selection bias that could be associated with unobservable factors such as farmers' innate skills of adopting new technologies. Although RCTs can avoid the problem of selection bias, the efficacy depends on the design and implementation of the experiment. For instance, cluster RCTs are more vulnerable to selection bias than individually RCTs (Bolzern *et al.*, 2018). However, due to the advantage of cluster RCTs regarding treatment contaminations, rather than randomising the treatment at the individual level, we executed randomisation over groups at the village level. To check the sensitivity of the baseline covariates to selection bias, we therefore examine the covariate balance before and after propensity score matching.[28] The standardised bias for all covariates is small, even before propensity score matching. It is below the threshold of 10 per cent (Morgan, 2017), indicating that the randomisation balanced the baseline covariates sufficiently (see Figure G1 in the Appendix in supplementary data at ERAE online). In this case, the matching estimator could be a poorly effective method to estimate the ATE, since the variance of the estimates

28 We use propensity score matching to create a sample in which treatment groups are balanced on baseline covariates and assess the quality of covariate balance based on the standardized bias, since standardised bias, unlike the t-statistics, is not influenced by the sample size (Austin, 2011).

increases due to a reduction in effective sample size and as such contributes to the problem of over-fitting (Golinelli *et al.*, 2012). As a result, estimates of the ATE of the matched sample of propensity and intensity of adoption are larger than the original sample (see results in Table G1 in the Appendix in supplementary data at ERAE online).

We also examine whether there is bias related to programme placement. The ITT estimates provide unbiased estimates of treatment effects if there is no programme placement bias. In our randomised experiment, the cluster randomisation and public-lead input supply chain play an essential role in reducing treatment contamination by ensuring that members of the control group do not buy seeds of the *Kingbird* variety in 25- and/or 12.5-kg packages. However, our end-line survey data revealed that the proportion of members of the control group that bought *Kingbird* seeds in 25 and/or 12.5-kg packages is about 1.5 per cent. Thus, it is important to ensure that the results are consistent in terms of magnitude and statistical significance. Following the procedure applied in Omotilewa *et al.* (2018), we tested for attenuation by dropping contaminated households in the control group. Overall, the results, which are presented in Table G2 (Appendix in supplementary data at ERAE online), are consistent with the results of the original estimates in Tables 2 and 3. Although the magnitudes of the estimates are expected to increase slightly when contaminated households are dropped from the control group, our results show that the estimates are consistent with the original results.

Moreover, we conduct sensitivity analyses to examine whether the results in the mediation analysis are likely influenced by unobserved factors. A point noteworthy is the fact that although the intervention in our field experiment is randomly assigned, the mediation analysis needs additional assumptions to identify the causal mediation effects, particularly the mediator should be ignorable, given the observed treatment status and pretreatment variables (see Imai, Keele and Tingley, 2010). However, the relationship between the outcome and mediator variables might be confounded by unobserved factors. As noted by Imai, Keele and Yamamto (2010), it is therefore useful to quantify the degree to which the assumption must be violated to reverse the original conclusion. We graphically illustrate this by plotting the estimated average mediation effect against different values of sensitivity parameter ρ—a correlation between the error terms in Equations (5) and (6). The results of the sensitivity analysis are presented in Figure H1 (Appendix in supplementary data at ERAE online). The figure shows that the original conclusion about the average mediation effect is relatively robust to violation of the assumption, except the ρ-value is greater than 0.5. This finding compares with other mediation analyses. For example, Imai, Keele and Tingley (2010) found a ρ-value of 0.48 in which mediation effects are assumed to be 0.

7. Cost-effectiveness

In this section, we examine the cost-effectiveness of divisible seed delivery in enhancing technology adoption to provide some insights into the significance of this approach for similar future interventions. In doing so, we consider only the cost of the small-sized bags; thus, the cost-effectiveness of divisible seed delivery is estimated on the basis of marginal cost-effectiveness analysis, assuming that the costs that are incurred are only because of the intervention. In estimating the cost-effectiveness, the marginal cost of packaging is scaled by the percentage point increase of *Kingbird* adoption. Because of the lack of rigorous impact measurement on the input delivery system to contrast our estimates, we also provide information on the per kilogram costs of the small-sized bags.

We summarise the cost estimates and the cost-effectiveness of divisible seed delivery on the adoption of the new *Kingbird* variety in Table I1 (Appendix in supplementary data at ERAE online). In the table, the cost of the bag in different sizes in terms of per kilogram is computed by dividing the cost of the bag by the size of the bag used to pack the seeds (in 12.5-, 25- or 50-kg bag size). The differences in the cost of the bag per kilogram were found to be below ETB 1, which is very small.[29] To estimate the marginal cost of the intervention, we considered the amount of *Kingbird* seeds supplied in 12.5- and 25-kg bags, which is actually 75 tons (for details, see Table I1 notes in the Appendix in supplementary data at ERAE online). We then scale this marginal cost by the number of *Kingbird* adopting households to obtain a result of ETB 65.41. Based on the marginal cost-effectiveness analysis, we find the cost of the intervention for each percentage point increase in *Kingbird* adoption to be about ETB 1,557.27, indicating that this amount of investment is required to achieve a 1-percentage increase in the propensity of *Kingbird* adoption.

8. Conclusions and policy implications

Inefficient input delivery system in many SSA countries remains a constraint to the adoption of new agricultural technologies. In this study, we hypothesise that input supply with small bags enhances farmers' ability to try the new technology to reduce the risk of adopting the new technology, by increasing their knowledge about the parameters of the technology in a particular context. In particular, we used an RCT to examine the impact of an improved input supply system on the adoption of a new wheat variety in Ethiopia. Our study has two important findings. The main finding of this study is that farmers treated with divisible input supply involving 50-, 25- and 12.5-kg seed packages had a much greater tendency to try the new variety. The other important finding is the feature of divisible input supply on different farmers. Divisible input supply exerts differential causal effects on farmers with varying characteristics.

29 Specifically, the costs of 12.5-, 25-, and 50-kg bags in terms of per kilogram are about ETB 1.08, 0.384 and 0.333, respectively. The costs of ordering and bagging do not vary on the scale of production, i.e. the cost of a 12.5-kg bag is the same whether the order is to produce 100 or 1,000 of such bags (no scale economies).

The overall impact of divisible input supply revealed that a significant number of farmers in the treated villages got motivated to try out the new variety, in anticipation of future profits.

In the first year of the introduction of the improved variety, the rate of adoption of the improved variety by farmers in the treated villages was 4.2-percentage points higher than those in the control villages. This finding is consistent with the results from adoption studies in the region and even more far-reaching in terms of adoption periods. For example, Diagne and Demott (2007) found that the rate of adoption of different rice varieties in Cote d'Ivoire from 1996 to 2000 was only 4 per cent. The positive impact of divisibility of seed bags on adoption was explained by the farmers' use of more than one wheat variety. The mediation analysis we used for mechanisms of adoption revealed that growing more than one wheat variety explains about 47 per cent of the total effects of the intervention. However, we did not find evidence at the stage of experimentation that farmers adopt the new varieties as a coping mechanism for risk exposure. Given that the costs of trying new technologies tend to decline with farm size, the availability of inputs in smaller seed packages tends to encourage farmers to try the variety on small portions of their land.

The findings of heterogeneous treatment effects showed that seed supply in different-sized bags exerts differential causal effects on individual farmers' adoption decisions, with the treatment effects being positively distributed. The results of CLAN revealed that relatively younger and poorer farmers are the most impacted by the intervention. Therefore, besides experimenting with the new variety in the first period, seed delivery in small bags may enhance the use of improved seeds in subsequent years by smallholders. The results of this study provide insight into the significance of divisible seed bags with smaller options in scaling up the use of improved seeds by smallholder farmers. In particular, female-headed and subsistence farmers are more likely to benefit from further interventions on agricultural input delivery.

Moreover, the impact of divisible input supply on the intensity of adoption has important implications for the accumulation and spillover of knowledge about new technologies in society. In particular, to the extent that allocating a larger number of farm fields represents wider agro-ecological and soil conditions, the spillover effects would be more likely in the treated villages, since farmers could obtain information that is competitive with one's own learning. Thus, the adoption of the new *Kingbird* variety is likely to increase in the subsequent periods, provided the net benefits from the variety are higher than the benefits obtained from the traditional varieties.

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Supplementary data

Supplementary data are available at *ERAE* online.

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