Can Agricultural Extension and Input Support Be Discontinued? Evidence from a Randomized Phaseout in Uganda

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Abstract

Many development programs that attempt to disseminate improved technologies are limited in duration, either because of external funding constraints or an assumption of impact sustainability, but there is limited evidence on whether and when terminating such programs is efficient. We provide novel experimental evidence on the impacts of a randomized phase-out of an agricultural extension and subsidy program that promotes improved inputs and cultivation practices among smallholder women farmers in Uganda. We find that phaseout does not diminish the use of either practices or inputs, as farmers shift purchases from NGO-sponsored village-based supply networks to market sources. These results indicate short-term interventions can suffice to trigger persistent effects, consistent with models of technology adoption that emphasize learning from experience.

Keywords: Agricultural Extension; Agricultural Technology Adoption; Food Security; Supply Chain; Subsidies; Randomized Phaseout; High-Yielding Varieties; Randomized Controlled Trial; Uganda.

JEL Classification: O13; O33; I32; Q12

1. Introduction

Economic development programs are often based on short-term interventions, even though the conceptual or empirical basis for limiting their duration is little discussed. In some cases, the underlying premise is that interventions can trigger changes that persist post program termination; in others, duration is arbitrarily determined by external funding cycles. However, there is at best thin evidence by which the premise of persistence can be tested, let alone the causal effects of program termination.

This paper presents a novel approach to estimating the impacts of program phaseout. The context of the study is a large-scale agricultural development program designed to improve cultivation methods of smallholder women farmers in Uganda. The program's general features, such as extension and input subsidies, are widespread in the numerous agricultural development programs employed to alleviate rural poverty in developing countries, some of which consist of temporary efforts and others are essentially permanent (see e.g. Anderson and Feder, 2007; Bardhan and Mookherjee, 2011; Jayne et al., 2018; Carter et al., 2020). [1] After several years of implementation, the Uganda program was first terminated in a randomized subsample of the treatment population, allowing us to estimate the causal impacts of phaseout.

Our estimates add to the relatively few existing post phaseout impact evaluations, particularly in the context of agriculture (Carter et al., 2019, 2020; Bird et al., 2020),[2] but also make a more fundamental contribution. Rather than comparing outcomes between the initial treatment and control group after a program is phased out, we compare outcomes between a random sub-sample of the treatment group in which the program is phased-out to the remaining treatment group, in which treatment is continued. This allows us to estimate the causal impacts of phaseout, and answer a distinct, if related, question that has important lessons for policy design but cannot be answered in

[1]It is estimated that public extension services, offered by most governments in the world, employ close to a million extension personnel, the vast majority of which are in developing countries (Anderson and Feder, 2004).

[2]Outside of agriculture, recent growth in long-term post-phaseout evaluations include, for example, Maluccio et al. (2009) on early childhood nutrition supplements, Baird et al. (2016) on deworming, Attanasio et al. (2017) on vocational training and Blattman et al. (2020) on grants.

the existing approach: when, if at all, should development interventions be terminated? For example, finding that a program's impacts persist post phaseout does not determine whether continued implementation would have resulted in further increases (or declines) in impacts, and whether continuation is cost-ineffective. Similarly, finding that impacts diminish post phaseout does not allow one to determine whether a continued implementation would have maintained the original impacts. Existing approaches also face problems of assigning causality: changes in differences between treated and control groups that occur after phaseout may result from other time varying factors, such as spillovers or external circumstances (Rosenzweig and Udry, 2019), that are unrelated to the phaseout itself .

From a theoretical point of view, it is not obvious whether or when an apparently successful intervention should be terminated. Phasing out a subsidy program need not reduce usage if informational barriers impede adoption and subsidies enable learning about the value of a good (Kremer and Miguel, 2007; Dupas, 2014; Carter et al., 2020). However, if such goods are normally unused because they are in fact unprofitable at market prices (Suri, 2011); because of liquidity or risk barriers (Karlan et al., 2014; Emerick et al., 2016); or because of behavioral factors (Duflo et al., 2011), then phasing out may lead to a decline in usage. Similarly, the impact of phasing out extension and demonstrations of improved practices after a certain duration, depends on whether lack of awareness inhibits adoption and on farmers' learning processes (Foster and Rosenzweig, 2010; Hanna et al., 2012). For example, high variability in outcomes resulting from other factors may make it difficult for farmers to learn about the profitability of a technology on the basis of just a few years' observations (Munshi, 2004; Cai et al., 2020).

The agricultural extension program we examine was implemented in Uganda by BRAC, a large NGO.[3] The program focused on both basic practices, and on the use of improved seeds (Barua, 2011), which remains very low in Uganda, as in much of Sub Saharan Africa.[4] Low usage of

[3]For details on BRAC, see Smillie (2009). BRAC Uganda has been the subject of prior research, not only on agriculture (Pan et al., 2018), but also in other programs addressing rural women (Bandiera et al., 2017).

[4]For information on low seed use in Sub-Saharan Africa, see World Bank (2007) and Sheahan and Barrett (2016). For the role of lack of knowledge of improved farming practices in the region, see Davis et al. (2012).

improved seeds is a simultaneous problem of low demand and low supply, and the BRAC program attempted to stimulate both by engaging women farmers to conduct demonstrations and training as well as to sell subsidized improved seeds. The focus on women farmers is motivated by pervasive gender differences in productivity and access to inputs (Croppenstedt et al., 2013).

These interventions may be considered to persist post program termination from the supply side if a successful experience with selling seeds encourages informal village suppliers to continue to work as (for-profit) distribution agents; and from the demand side if a positive experience with improved seeds and practices permanently increases demand for such seeds and farming practices by local farmers.

Our results suggest that the semi-informal supply of improved seeds, as well as training activities, declines markedly as a result of phaseout. At the same time, while seed purchases from BRAC sources decline in phaseout villages, we find strong evidence that farmers switch to purchasing improved seeds from market sources, so that after a brief period of adjustment, the total usage of improved seed usage from all sources combined remained nearly unchanged. We also find no evidence of declines in the use of agricultural practices taught by BRAC in phased-out villages These results are observed consistently across three repeated household surveys that are conducted up to three years (six seasons) post-phaseout, providing unusual confidence in their "temporal external validity" (Rosenzweig and Udry, 2019).

Our findings are consistent with a model in which the financial, risk-related or behavioral costs of experimentation prevent farmers from learning about unfamiliar (but profitable) technologies. An intervention that reduces these costs temporarily can therefore facilitate learning and result in sustained adoption. That the findings apply to both costly (seeds) and less costly (practices) technologies suggests that barriers to self experimentation may not be entirely financial.

A general challenge for interpreting null results of a randomized phaseout is that without other evidence, the absence of post phaseout impacts could be interpreted both as evidence of persistence or as a reflection of the absence of any impacts of the program itself. The methodology we introduce in this paper can therefore be ideally applied to interventions and samples that were themselves experimentally designed from the onset, and for which initial impacts would therefore be known. As is often the case, the particular program studied here was not randomized in the same sample in which our own study is conducted. To improve the interpretation of the results, we use two lines of additional evidence on initial impacts. First, we refer to results from a new experimental evaluation of the same BRAC program conducted in a different region in Uganda, which finds significant impacts on improved seed purchases (among other results). Second, we compare our sample to a matched sample of comparable but never-treated households in the phaseout study region, and find them to be significantly less likely to use the improved seeds and practices promoted by BRAC. The sum of the evidence supports our interpretation of the experimental phaseout results as indicating the program had positive impacts on the use of improved seeds and practices that are sustained after program termination.

The remainder of the paper is structured as follows. Section 2 presents further background on the BRAC program and the Ugandan context. Section 3 lays out the experimental design. Section 4 describes the estimated impacts of phaseout on the use of improved seeds and practices. Section 5 describes the evidence for initial program impacts . Section 6 concludes.

2. Overview of the Intervention

BRAC's agriculture program in Uganda began in 2008-2009. The program has many features in common with other extension programs[5] and seeks to improve the agricultural productivity, incomes and food security of smallholder women farmers by promoting the usage of improved farming practices and inputs, especially high yielding variety (HYV) seeds, also called improved seeds. [6] Improved seed varieties are bred by agricultural research organisations to provide, under suitable conditions, higher yields than "traditional" varieties, or "local seeds" typically obtained by farmers from previous harvests. The low rate of usage of improved seeds is widely considered to be one of the principal causes of stagnant yields in Sub-Saharan Africa (World Bank, 2007). The

[5]For background on agricultural extension and similar programs see Barrett (2002), Anderson and Feder (2007) and Barrett et al. (2010).

[6]Broadly similar programs aiming to diffuse improved seeds are described by Carter et al. (2020); Bird et al. (2020)

BRAC program, and therefore this study, is mostly focused on maize, the dominant crop grown by farmers in the area.

The failure to use improved seeds is a simultaneous problem of low demand and low supply. To address these problems, BRAC followed a two-pronged approach: stimulating demand through demonstrations, training and distributions of free samples of improved seeds[7]; and stimulating supply by creating semi-informal supply chains within villages. These parallel functions are implemented through two types of agents that were recruited from local communities and engaged by BRAC: the Community Agriculture Promoter (CAP) [8] and the Model Farmer (MF).

CAPs were engaged as for-profit distribution agents to provide BRAC's improved seeds (as well as other inputs) in their villages at reasonable costs (Barua, 2011).[9] MFs were engaged to demonstrate improved practices and inputs in their own farms and to train other farmers in the village in their application.[10] Both CAPs and MFs were selected from female farmers in the village between 25 and 60 years of age who own a plot of at least 1 acre in size, which could be used for demonstration purposes, and were willing to attend training sessions and meetings. Each season, CAPs and MFs received week-long training in improved farming practices by BRAC program staff, who also monitored their activities throughout the season. Both CAPs and MFs received modest compensation for their time and travel costs.

MFs were expected to implement a variety of improved practices in their farms, including crop rotation, line sowing and intercropping, and the use of of improved seeds and fertilizer. They

[7]Previous studies of the impacts of agricultural extension include Feder et al. (1987); Owens et al. (2003); Feder et al. (2004); Godtland et al. (2004); Dercon et al. (2009); Cole and Fernando (2012); Davis et al. (2012); Larsen and Lilleør (2014); Beaman et al. (2015). See also reviews by Birkhaeuser et al. (1991), Evenson (2001) and Anderson and Feder (2007). Studies of learning between farmers include Foster and Rosenzweig (1995), Munshi (2004), Bandiera and Rasul (2006), and Conley and Udry (2010). Also see Jack (2013).

[8]A terms commonly used to describe such agents is *Village Based Agents* (VBAs) but we use the BRAC term in the remainder of the paper.

[9]In addition, as Barua (2011, page 5) notes, the CAPs' "role is to provide general farmers and their local communities with farm inputs at a reasonable price. These inputs include seeds (such as high yielding varieties of maize, rice, beans, groundnuts, cabbage, tomatoes, and eggplants), tools (such as hoes and pangas), and inorganic fertilizers."

[10]The idea of using model farmers in agricultural extension programs has a long history. Recent studies of these models include Krishnan and Patnam (2014), who find that learning from neighboring farmers has a longer lasting effect than learning from conventional extension agents; and BenYishay and Mobarak (2015) who report that "farmers find communicators who face agricultural conditions and constraints most comparable to themselves to be the most persuasive."

were also expected to hold regular training sessions for about 10-12 farmers per season, in which they would demonstrate and explain the benefits of these improved practices and inputs.[11] Each season, the MFs also received a small amount of improved seeds for use in their own farms and for distribution as samples during the trainings.

CAPs were encouraged to buy improved seeds (and to some extent, also chemical fertilizers) from BRAC and to sell it to farmers. BRAC provided seeds to CAPs at a discounted rate (up to 20% relative to market sources) and transported it to their villages, providing them with advantages over conventional input dealers located in towns and market centers who might with to sell to these villagers. The ability to generate profits from seed sales was meant to incentivize CAPs and increase the potential for longer-term sustainability past BRAC phaseout. While CAPs did not provide formal training in agricultural practices to the farmers, they were encouraged to give advice based on knowledge gained through their own participation in BRAC training sessions.

Taken together, one can think of BRAC's strategy to achieve sustainability as an effort to establish a new equilibrium, first generating demand by making farmers aware through direct experience of the benefits of improved seeds; and second generating supply by overcoming market or coordination and information failures to establish otherwise profitable missing markets (see Section 4.3).[12]

Post phaseout, CAPs and MFs stopped receiving financial incentives to carry out their activities. CAPs ceased to be visited or encouraged by BRAC to sell seeds. They could continue to procure seed from BRAC (or other sellers) and resell it to farmers, but they would have to take the initiative and they no longer received the same discounted prices or BRAC's assistance with transport.

[11] There was no restriction on the number of times a farmer could participate in the training sessions, and some farmers received training two or more times. BRAC indicated that in some villages in which the program had been active since 2009, most if not all farmers who were interested in training had received it at least once.

[12] In this light, if an intervention is unsustainable, it acts as a temporary shock, after which the agricultural household returns to its earlier equilibrium (for a framework comparing types of farm household equilibria shifts, see Kwak and Smith, 2013). BRAC certified seeds might help overcome a "lemons" problem of poor (if not counterfeit) HYV seed quality (Bold et al., 2017).

3. Experimental Design and Data

3.1. Experimental Design

BRAC operations, particularly seed distribution and training, are organized around groups of villages called *branches*. While the program was implemented in a broader area, this study is based on a sample of farmers from 15 branches in Eastern Uganda. The unit of analysis in this study is a *village cluster*, one or at most two villages no further than 2 KMs apart, that have one active CAP and one active MF. Overall, 99 clusters were included in the phaseout experiment, of which 18 consisted of two villages and the rest consisted of a single village.

An important point to keep in mind when designing a experimental phaseout study of this kind, is whether the sample chosen for the study is restricted to sites in which the program is still active at the time of phaseout. This distinction is important to take into account in interpreting the resulting estimates, as being conditional on the program being active or not, even though both estimates would be internally consistent. In our particular context, this distinction is not relevant, as BRAC's policy was to replace any ineffective or inactive CAPs or MFs in the same clusters, and the program had not been withdrawn from any cluster prior to the initiation of this study.

Within each branch, village clusters were randomly assigned to three treatment arms: Continuation (no change), CAP-1st phaseout and MF-1st phaseout. Village clusters that were assigned to either of the phaseout groups had that particular component of the program discontinued in early 2013. One year (or two agricultural seasons) later the remaining component was discontinued as well, meaning that differences between the two phaseout groups were only in effect for the first year of the experiment. Overall, 32 clusters were assigned to Continuation, 34 assigned to CAP-1st phaseout and 33 to MF-1st phaseout (the clusters are mapped in Figure I).

3.2. Data Collection and Summary Statistics

We surveyed a sample of randomly selected farmers from amongst those who had received training from MFs in either of the two seasons preceding the start of the phaseout. The annual cropping calendar in the region of study consists of two maize growing seasons: the first lasts from approximately March to July/August and the second from approximately September to December/January. We conducted a pre-phaseout baseline at the end of the 2013 first season harvest (February 2013); and three follow-up surveys in September 2013, September 2014 and February 2016, i.e. at the end of 1, 3 and 6 seasons following the phaseout. Each survey collected detailed information about the cropping practices of the concluding season and the prior season (with the exception of the second follow-up survey). An overview of the timing and scope of each survey is offered in Figure II.

Attrition rates were 15.2% after one season and 17.8% after three seasons and were uncorrelated with treatment status or baseline characteristics (see Appendix Tables A.I and A.II). The final sample of households observed in all surveys consists of 1124 households, of which 405 are in the Continuation arm, 352 in are in the CAP-1st phaseout arm, and 367 in the MF-1st phaseout arm.

Table I reports mean values of key baseline characteristics at each of the three experimental groups. Columns 1-3 report the difference between the Continuation and the combined phaseout sample, and Columns 4-7 report differences between the Continuation and each of the two phaseout groups separately. For the most part, baseline attributes and use of improved practices and inputs are balanced across the three groups, with significant but small differences in the use of line sowing, weeding and organic fertilizer between the Continuation and one of the phaseout groups.[13]

A majority of the respondents (82%) report using local maize seeds, and about 59% of the respondents report making use of improved seeds (the two are not mutually exclusive since farmers may use a combination of improved and local seeds on different portions of their land). Of these, 32% report obtaining these seeds from market sources (including input suppliers, general shops, local markets and moving vendors) and 24% report obtaining them from BRAC sources (MFs, CAPs, and other BRAC sources combined). More than 70% of the farmers practice line sowing and intercropping, and 55% practice crop rotation at baseline (i.e. prior to phaseout).

[TABLE I HERE]

[13]Six of 161 coefficients, or 3.7%, are statistically significant at the 5% level, about as many as could be expected by chance.

3.3. *Empirical Specification*

Our primary empirical specification follows directly from the experimental design. We estimate regressions of the form:

$$y^s_{i,v,b} = \rho PH_v + \beta \cdot X_i + \gamma_b + \epsilon_{i,v,b} \tag{1}$$

where y^s is the outcome of interest, observed $s = 1, 3, 5, 6$ seasons after phaseout (see Figure II) for household i in village v in branch b. We estimate impacts in each season through a separate regression. The vector X contains the value of the outcome variable at baseline, and baseline attributes that were imbalanced across treatment groups (see below). The regression also includes branch fixed effects γ_b. The errors $\epsilon_{i,v,b}$ are clustered at the village level.

The variable PH is a binary indicator of phaseout and ρ is the coefficient of interest. In our primary specification, we bundle the two phaseout treatment arms together into a single phaseout arm. We also report the separate impacts of each of the phaseout arms (CAP-1st and MF-1st) in Appendix B.

4. Effects of Program Phaseout

We now turn to our main analysis, i.e. the impact of the phaseout on farmers' use of improved seeds and farming practices. We present results for each of the survey rounds for which the relevant data was collected, i.e. up to three years, or six cropping seasons after the first component of treatment was discontinued in the two phaseout groups.

4.1. *Use of Improved and Local Seeds*

We begin by examining the impacts of program phaseout on improved seed usage and its procurement from various sources.

4.1.1. *Overall Usage*

Table II (top panel) reports the estimated effect (and 95% confidence intervals) of program phaseout on a binary indicator of improved seed use (i.e. the extensive margin). We find no indication of a negative effect, with estimates that are statistically indistinguishable from zero and of modest size in each of the four seasons for which we have data, extending up to three years post phaseout. We are able to bound the negative effect in season 6 to be no higher than about 6 percentage points with 95% confidence.

We note that while MFs distributed small samples of free seeds during the training, almost all farmers who make use of improved seeds purchase at least some of them, i.e. binary indicators of use and purchase of improved seeds are nearly identical. Moreover, quantities of used seeds exceed the small amounts distributed by MFs in virtually all observations. We therefore focus on indicators of usage throughout.

[TABLE II HERE]

Table II also reports (second panel) estimated effects on the *quantity* of improved seeds use (measured in kg per acre), for which we have data only in seasons 3 and 5 (Columns 2-3). Here too, we find no indication of any negative effect, although the estimates are somewhat noisier (and shift sign between seasons) with wider confidence intervals. [14]

As a further check, we also examine effects on the use of local seeds (both binary indicators and quantity), which would be expected to increase in tandem with any reduction in the use of improved seeds. The results, reported in the bottom half of Table II, also fail to reject the null hypothesis of no effect, with estimates that are statistically insignificant and small in size, both in the case of the binary indicator and the amount of seeds per acre. Here too, we can bound the potential increase in local seeds use to about 6 percentage points with 95% confidence. The sum of the evidence suggests that the phaseout of BRAC activities did not lead to a decline in improved seed use. In section 5, we will situate these estimates in the context of initial program impacts.

We also explore the possibility that there were heterogeneous effects of phaseout on the use of improved seeds, based on the number of times farmers were trained, how recently they were last

[14] The estimated levels of seed use suggest that farmers who use local seed use about 10 kg/acre, whereas those who use improved seed use about 5 kg/acre, suggesting about half of total seeds used are improved.

trained, the size of their cultivated area and other indicators of wealth. However, we find no such indications (results omitted for brevity).

4.1.2. Use of Improved Seeds from Various Sources

To better understand why the phaseout did not seem to affect the use of improved seeds, we next examine the procurement of improved seeds from various sources. Our surveys asked improved seed users to indicate their sources, which we group into three categories: BRAC program sources (MFs and CAPs), other BRAC sources (mainly direct procurement from BRAC branch offices), and market sources (including all types of commercial vendors not related to BRAC, such as input dealers and local shops in regional trading centers). Estimates of the effect of the phaseout on the probabilities of obtaining improved seeds from these three sources are reported in Table III and summarized in Figure III.

The phaseout led to a reduction of purchases from CAPs and MFs of about 5 percentage points (p.p.) after one season, 6 p.p after three seasons, and 10 p.p. after six seasons (see Table III, Column 1), amounting to a decline of between 50% to almost 100% of the mean level of usage in the continuation group. This decline is consistent with the decline in CAP activity in the villages reported above.

However, the decline in purchases from CAP and MF sources is accompanied by a parallel increase in purchases from market sources, starting from about a 2 p.p. increase after one season, to 6 p.p. after 3 seasons and 11 p.p. after 6 seasons (Table III, Column 2). These effects are also sizable, amounting eventually to around 40% of usage in the continuation group. There is also an initial, but smaller increase in purchases from other BRAC sources - mostly direct purchases from BRAC branch offices - of 1-2 p.p. which tends to disappear six seasons post phaseout (Table III, Column 3).

Results obtained by limiting the sample to farmers who use improved seeds at baseline follow a very similar pattern but are larger in magnitude: In the first season after phaseout, procurement from CAP and MFs declines by 12 p.p., whereas procurement from market sources does not display a significant increase. Later on, procurement from CAP and MFs continues to decline by 16 p.p after 3 seasons and 31 p.p. after 6 seasons, while procurement from market sources increases by 13 p.p. and 32 p.p. in the corresponding time periods.[15]

These results suggest that farmers facing reduced supply from CAPs and MFs, rather than fully shifting to the use of unimproved local seeds, turn to alternative sources of improved seeds which initially include other BRAC sources but eventually consist entirely of market sources. The estimates suggest that despite the greater effort (and cost) required to obtain seeds from other sources, the substitution is essentially complete. Moreover, the shift does not appear to take place immediately, suggesting a delayed response, which further highlights the need for long-term monitoring in impact evaluations of this type.

Results obtained by separating the effect of the initial CAP-1st and MF-1st phaseouts (see Appendix B Tables) do not display significant differences. Point estimates are somewhat larger and more precise for the MF-1st phaseout after 3 seasons (though statistically indistinguishable), but after 6 seasons, results become very similar in magnitude, and are both statistically significant.

[TABLE III HERE]

4.2. Cultivation Practices and Inputs

BRAC's training and dissemination activities, conducted through the MFs, included the practices of crop rotation, intercropping, line sowing, weeding, pest control, and the use of organic and chemical fertilizers. Among practices, crop rotation and line sowing received the greater emphasis. In Table IV, we report estimates of the effects of phaseout on binary indicators of the application of these practices, observed three and six seasons post phaseout. Overall, we do not find evidence of any negative impacts of the phaseout, with most estimates being of small size, of mixed sign, and statistically insignificant, with the exception of a positive impact on pest management. The potential declines in the practices most emphasized in the BRAC trainings, line sowing and crop rotation, can be bound at 6-9 p.p. with 95% confidence, compared to mean values in the Continuation group of around 60-70 p.p..

[15]Results omitted for brevity but available upon request.

[TABLE IV HERE]

An analysis of the effects of phaseout on the use of other inputs (e.g. hired labor and pesticides) and tools (plows and pesticide pumps), crop diversification, maize cultivation or yields, revenue and profits also does not reveal evidence of negative impacts (Appendix Tables A.III-A.VI).

4.3. Phase-out effects on improved seed sales by CAPs

To investigate whether the program was successful in developing a sustainable input supply chain, we conducted a survey of CAPs. Appendix Table A.VII reports results of OLS regressions for three outcome variables: (1) whether CAPs continue selling BRAC seeds; and if so, (2) in what quantities and (3) at what price. The explanatory variable is a binary indicator of phaseout. After six seasons, phased out CAPs are 25 percentage points less likely to sell any seeds (Column 1). Among those still selling seeds, point estimates suggest the phaseout increased prices and reduced quantities (estimates are usually statistically insignificant, likely due to small sample size). Among potential causes of the rise in CAP prices post-phaseout, the share of respondents who point to transport costs is 31% in the Continuation group and 51% in the phased-out groups, a statistically significant difference.

These findings are consistent with results from farmer surveys, collected six seasons after phaseout, which show that farmers in phaseout clusters were less likely to report seeking advice or training from the CAP in their village (by 5 p.p., $p<0.01$) or having purchased seeds from CAPs in the previous year (by 10 p.p., $p<0.01$).

These results indicate that in the absence of a subsidized supply of improved seeds to CAPs, CAP operations largely cease. This may not be surprising, since without a price differential and strong economies of scale, there may be little advantage that CAPs can have over conventional market sources.

5. Initial Program Impacts

In the previous section, we presented a range of results indicating that phasing out the BRAC program did not lead to a reduction in improved seeds use, as farmers turned to market sources to substitute for the diminishing supply from BRAC. We interpret these results as evidence of persistent program effects, deriving from farmers' positive experience with improved seeds and farming practices. This interpretation rests on the assumption that the BRAC program itself originally increased the use of improved seeds and practices.

This assumption is consistent with data from the endline survey (six seasons post phaseout) which indicates large proportions of farmers expressed positive opinions about the comparative effectiveness of the improved seeds and practices offered through the program. Between 65%-70% of respondents report BRAC seeds are better quality, provide higher yields and are more profitable, and approximately 80% of respondents felt BRAC's cultivation practices were better and result in larger yields and profits (Appendix Table A.VIII).

Direct experimental tests of the assumption of initial impacts are not possible, as the BRAC program was not randomly implemented in our own study area. Instead, in this section we report two other sources of evidence on of initial program impacts. First, we report results from a recent experimental evaluation of the BRAC program impacts in a different region of Uganda. Second, we compare our study villages to a matched contemporaneous sample in the same region, but in which the program was never implemented. Taken together, the combined evidence suggests the BRAC program had a substantial initial impact on the outcomes of interest.

5.1. Experimental Estimates of BRAC Program Impacts in Southwest Uganda

We first present estimates from an ongoing experimental evaluation of the BRAC program in a different part of Uganda (the Southwest Kabale region). The evaluation, for which one of this paper's coauthors (Sulaiman) is co-PI, is intended to study the combined effects of BRAC's agricultural and microfinance interventions, using a sample of 230 villages randomly selected into one of the four possible combinations of these two treatments. Here, we report estimated impacts of the agricultural intervention on the agricultural outcomes that are directly relevant to our study. These estimates are based on data collected only two seasons after the intervention began, while our data were collected post phaseout, which occurred up to eight seasons after the initial roll-out of the program. One might therefore expect the estimated impacts to be smaller than in our sample.

Also, we note that the comparability of the survey instruments of the two studies is substantial, but imperfect.

Table V reports two sets of estimates. In Column 1, the treated sample consists of all villages that received the agricultural intervention, and the control group consists of all villages that did not, irrespective of whether they also received the microfinance treatment. In Column 2, the treated group consists of all villages that received the agricultural intervention alone (without microfinance), and the control group consists of all villages that received neither the agriculture nor the microfinance intervention. Our preferred estimates are from Column 1, largely because some households in the phaseout experimental sample also benefit from BRAC's microfinance program, and the larger sample allows for more precise estimates.

Results from both samples show statistically significant impacts on the purchase of improved seeds in general, and from BRAC sources in particular. The impact of the BRAC program on improved seed use is estimated to be 7 p.p., which is a substantial effect to observe one year after the program is introduced. The estimated effects on the practices in question are mostly positive, but imprecise.

[TABLE V HERE]

5.2. The No-Treatment Comparison Group for the Randomized Phaseout

We next turn to an estimation of initial program impacts derived from a comparison group of villages in which the BRAC program was never implemented. These "No Treatment" (NT) villages were chosen, prior to the start of the phaseout, from the areas of the same branches that constitute our main sample and were included in the same household surveys. It is worth keeping in mind that given that several years have elapsed since the initiation of the BRAC program, estimates of its impacts through a comparison of the NT villages to those in our main sample may be (likely downward) biased due to spillovers.

BRAC's official policy is to implement its agricultural program in villages located up to 6 kms away from its branch offices (Pan et al., 2018). In Table VI, we present comparisons of characteristics that are unlikely to have been impacted by the BRAC program, between the NT and continuation samples. We find that the NT villages are, accordingly, located farther away from BRAC branch offices (see Figure I) and have lower membership in BRAC microfinance groups; but that they are similar to the villages in our main sample in terms of education, age and land and asset ownership. Branch offices are generally located in the main town or trading center of the branch area, meaning that NT farmers are likely to be more distant from input suppliers and from markets, potentially impacting the likelihood of purchases of improved inputs. Access to microfinance could also affect NT farmers' ability to invest in costly inputs, including improved seed, although, as we show below, we do not find evidence for such an association, which is unsurprising given that the microfinance program provided loans designed for non-farm micro-enterprise activities.

[TABLE VI HERE]

In comparing outcomes between the NT sample and villages in our main sample, we follow two approaches. The first consists of OLS regressions that control for distance to the branch office and for administrative indicators of the presence of a BRAC microfinance group in the village. The second approach utilizes matched sets of households from the NT sample that are selected through a number of different matching methods - inverse probability weighted (IPW) matching (Hirano and Imbens, 2001), minimum-biased inverse-probability weighted matching (MB-IPW, following Millimet and Tchernis, 2013), and coarsened exact matching (CEM, see Iacus et al., 2012). We match households on the basis of attributes that are unlikely to have been impacted by the BRAC program but are likely to have affected treatment status, namely distance to the branch office and a woman's age and education level. We report the OLS and the three matching estimates obtained by comparing the NT sample to the Continuation group. [16]

The IPW approach weights each treatment observation with the probability of being treated, and each control observation with the probability of not being treated. The probability of treatment

[16] It is also possible to compare the NT group to the entire main sample, but this has the drawback of possibly diluted impacts in the phaseout groups. Results obtained in this way are nevertheless similar to those reported here and are available upon request.

is the inverse of the propensity score, which is estimated using a logit model with errors clustered at the village cluster level. The MB-IPW estimator then minimizes the bias that arises from the violation of the conditional independence assumption by trimming the sample around the bias-minimizing propensity score. CEM uses a nonparametric procedure to divide the sample into strata based on the selected coarsened [17] covariates. For each treated observation, a control observation is selected from the same stratum.

It is worth noting that neither of these approaches allow us to address the possibility of bias resulting from differences between the NT and treatment villages in unobserved characteristics. A particular concern is that villages were selected into treatment on the basis of unobserved attributes that are also predictive of outcomes. For example, inspection of treatment assignment shows that compliance with the 6 KM rule is only partial, potentially reflecting selective treatment, but also likely resulting from geographical constraints related to the hilly terrain. We acknowledge this limitation of the initial program impact estimation, and proceed to present the best evidence available to us.

5.2.1. Program Impacts on the Use of Improved and Local Seeds

Comparisons of improved seed use and purchase between the NT group and the continuation group, based on data from the second follow-up survey (3 seasons post phaseout) are reported in Table VII (top and middle panels) and suggest that the program has led to substantial increases of 15-17 p.p. in improved seed use, with estimates that are statistically significant and similar in magnitude regardless of whether they are obtained using OLS or any of the matching methods (Columns 1-4). Effects of this size are highly unlikely to be explained by the distribution of free seeds by MFs, since almost every farmer that reports using improved seeds in the main sample also reported purchasing some of them. In fact, the effect on improved seed purchase is very similar (14-16 p.p., columns 5-8). Note that access to microfinance does not appear to be correlated with

[17] Variables are coarsened by recoding continuous or categorical variables into a smaller number of bins. In this case, the distance variable was coarsened by creating 5 categories (up to 0.5km, 0.5-2km, 2-4km, 4-6km, 6-10km, and over 10km). Similarly, 6 categories were used for the education and age variables. Different reasonable numbers of categories were also tested to ensure the robustness of the results (available upon request).

improved seed use, while being farther away from branch offices seems to reduce it by about 1.5 p.p. per km (Appendix Table A.IX). Using data from the other survey rounds yields very similar results (Appendix Table A.IX). The results are also maintained if access to microfinance is indicated by self-reported survey data rather than administrative data (Appendix Table A.X).

Data from the last survey round provide further indications that differences in improved seed use between the NT group and the main sample are related to the BRAC program. First, amongst farmers who reported using improved seeds in the NT sample, only 6% reported having been exposed to it by BRAC, as compared to 69% in the Continuation group. Second, the maize seed promoted by BRAC belongs to the Longe5 open pollinated variety. Other common varieties in the area include the Longe4 variety and hybrid varieties such as Longe6H, Longe7H and Longe10H. The proportion of farmers who report being familiar with the Longe5 variety was 22 p.p. higher in the main sample than in the NT sample ($p<0.01$). The corresponding difference for the Longe4 variety was also significant, but much lower at only 6 p.p. There was no statistically significant difference in the case hybrid seeds.

[TABLE VII HERE]

As noted in Section 4.1, increases in improved seed use are likely to be accompanied by declines in the use of local seeds (even though farmers may well use both types of seeds in tandem). Table VII also reports estimated BRAC program impacts on the use of those types of seed (bottom panel). The results are indicative of a decline of 3-6 p.p. in local seed use, but are imprecise in early seasons.

Appendix Figure A.I displays the proportion of farmers reporting procuring improved seeds from BRAC program (MF and CAP), Market sources and other BRAC sources. There is hardly any procurement from BRAC sources in the NT sample.

5.2.2. Program Impacts on Cultivation Practices and Inputs

Table VIII reports comparisons of the prevalence of the practices and inputs promoted by BRAC between the Continuation group and the NT sample. The results suggest that the program had a positive impact on most practices, with especially large, significant and robust effects for the two practices that received the greatest emphasis, i.e. crop rotation and line sowing. Line sowing is estimated to have increased by 13 p.p. using OLS, and by 18-21 p.p. using the matching estimators. Crop rotation is estimated to have increased by 10-12 p.p.. We also find significant and large impacts on the use of chemical fertilizer, a costly input, for which, like improved seed, we did not find evidence of reduced use after the phaseout. Chemical fertilizers were also sold by CAPs to some extent, and these results suggest the program also had persistent impacts on the use of this input.

As for improved seeds, additional data from the last survey round provides further indications that differences in the use of crop rotation and line sowing are related to the BRAC program. Among those farmers in the NT sample who were familiar with these practices, only 2% said they learned line sowing and 1% said they learned crop rotation from BRAC. In the Continuation sample, those rates were 68% and 56%, respectively.[18]

Estimates of program impacts on crop diversification and on additional inputs that were not explicitly encouraged in the training are presented in Appendix Tables A.XI and A.XII.

5.3. Comparisons of Initial Program Impacts and the Effects of Phaseout

Figure IV presents a comparative summary of the estimated impacts of the phaseout (three seasons after phaseout) on key outcomes, vis-a-vis the estimated impacts of the program itself on the same outcomes, presented earlier in this section. Phaseout impacts are represented by circles with error bars capturing their 95% confidence intervals. Program impacts are reversed in sign, and represented by symbols of various shapes (OLS, IPW, MB-IPW and CEM). These estimates represent what the effects of the phaseout would have been in a scenario of complete reversal (zero persistence) of all program impacts (for ease of visualization, confidence intervals for program impacts are omitted). Figure IV shows that for most outcomes, the likely (95%) range of the actual impact of the phaseout does not include the (inverse of the) estimated impact of the program itself, suggesting against the hypothesis of complete reversal. The corresponding estimates, obtained six seasons after phaseout, are plotted in Figure A.II, and show similar patterns. In Figure V, we only

[18]These responses reflect data from 6 seasons post-phaseout; similar results were found in earlier survey rounds.

report the OLS estimates but plot their confidence intervals as well. For most key outcomes, the two confidence intervals have little or no overlap. [19]

[FIGURE IV HERE]

[FIGURE V HERE]

6. Concluding Remarks

This paper addresses a basic question for development programs: when is program termination efficient? It does so using a novel method of a randomized phaseout designed to identify the causal impact of the removal of some or all components of the intervention.

In low-income countries it is common for both government and NGO programs to be initiated, show some apparent progress, and then be terminated, often due to lack of funding. Such discontinuations are sometimes accompanied by a statement that the program has “become sustainable”. However, evidence (Kremer and Miguel, 2007) and anecdotal reports suggest that impacts often prove unsustainable after funding of rural development programs ends, even for high-return interventions. Randomized phaseouts provide a new research strategy to help and study the effects of program phase out and termination.

We study an agricultural development program implemented by the NGO BRAC in Uganda that sought to disseminate improved inputs and practices among women smallholders. Loss of funding prompted BRAC to phase-out the program in 2013 in a random sub-sample of locations. Six growing seasons later, we find no evidence that phaseout led to a decline in the usage of improved practices or inputs. While usage of the main input, improved seed varieties, is unaffected, farmers shifted purchases from BRAC sources to market sources in a manner which is consistent with a model - representing a major hypothesized justification for temporary interventions - in which initial learning increases farmers’ willingness to pay for the input.

[19] Comparable plots for the OLS, IPW and CEM estimates and their confidence intervals are displayed in Appendix Figure A.III.

Our results also suggest that this response gradually develops and becomes more emphasized post program termination with a substantial lag, highlighting the importance of long-term monitoring (in our case, three follow up surveys) for impact evaluation in general, and for program termination in particular. It may take time to determine whether a program has "sustainable" outcomes - either because some practices may hold on longer than others before farmers discontinue them; or on the other hand, because a transition to other sources itself may take time, possibly resulting in a U-shape response.

A general challenge for our randomized phaseout design is that absence of post phaseout impacts can be reliably interpreted as persistence only to the extent that initial program impacts are identified. Our sample was not originally randomized into program treatment, requiring us to address the question through non-experimental methods. The ideal research design would involve experimental assignment to both initial treatment and termination, although realistically this may be unlikely in most cases.

A randomized phaseout can provide several insights not otherwise obtainable through alternative methods. If an intervention is discontinued entirely, the counterfactual of continuation cannot be observed: even if gains from the program are retained among former participants, we do not know if those gains would have been even greater had the program continued; or if gains were lost, it is impossible to tell whether this would have happened even with program continuance.[20] Opportunities to carry out randomized phaseouts may present themselves whenever programs need to be scaled down, but funds are available to continue them in a smaller population.

Estimates of the degree to which phasing out a program affects its long-term impacts can help inform optimal program duration. Consider a program that has been in place for T time units (say, seasons or years). Assume that the additional costs of another year of implementation are C_T, and the difference in (present discounted) impacts the program will have had because of this extension are B_T. It is efficient to continue the implementation as long as $B_T > C_T$. B_0 would be the impact of implementing a program for one year, and could be estimated through a standard program

[20]For example, gains could have been lost as a result of general factors in the wider economy.

evaluation with randomly selected treatment and control groups. Estimating B_1 would require randomly phasing out the program for some of the initially treated group and retaining it for the rest. Estimating B_2 would require randomly phasing out the program for some of this latter group, and so on. The analysis in this paper provides only a single component of a systematic assessment of this kind. In our case, the evidence suggests program impacts are little affected, so that terminating the program at the time of the phaseout, or potentially earlier, would have been more efficient than continuing it (although an important caveat is that we do not observe all relevant outcomes). In general, a more systematic estimation of B_T could result in several patterns. For example, if costs are stable but marginal benefits of continuation decline, the benefit cost ratio of the program could exhibit an inverted-U relation to its duration. In any case, carrying out such an estimation can be a difficult but worthy objective for future studies.

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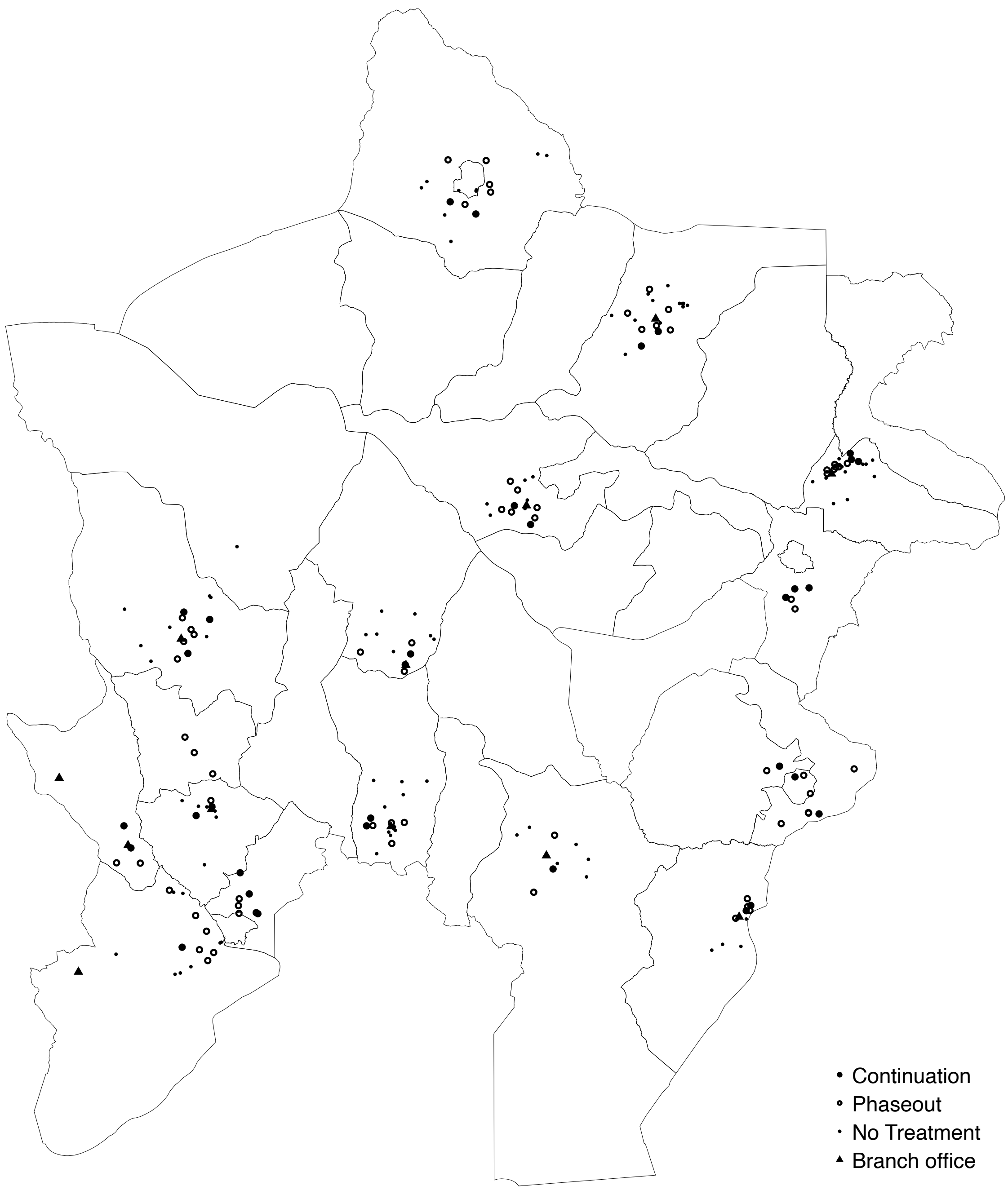
Figures

Figure I: Locations of Sample Households (Eastern District, Uganda)



Note: Locations of the Continuation (solid circles) and Phaseout (hollow circles) clusters, “No Treatment” villages (points), and branch offices (triangles).

Figure II: Timeline

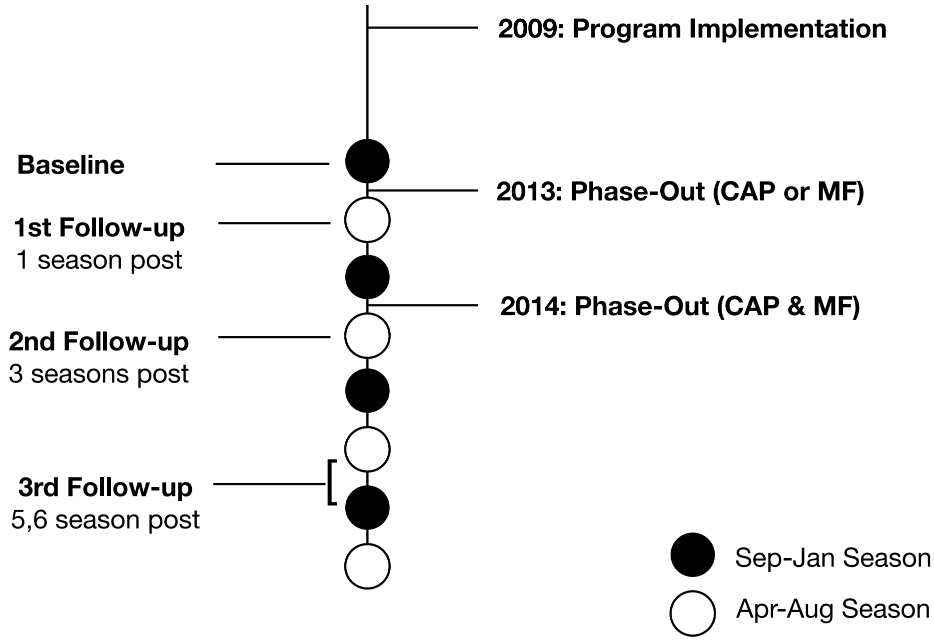
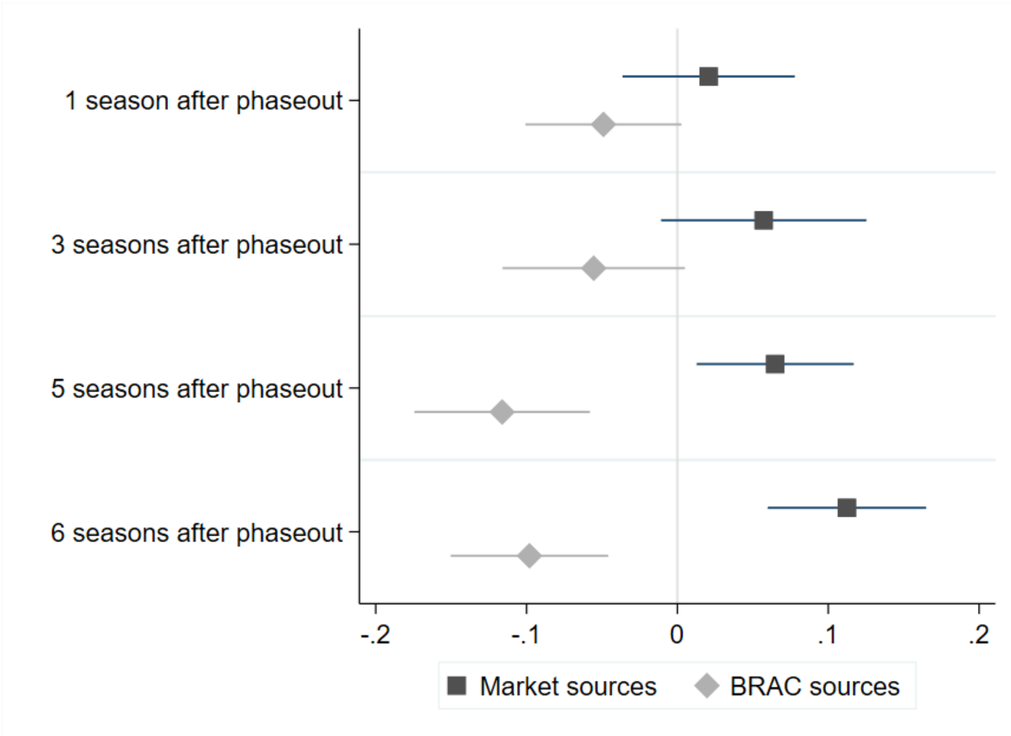
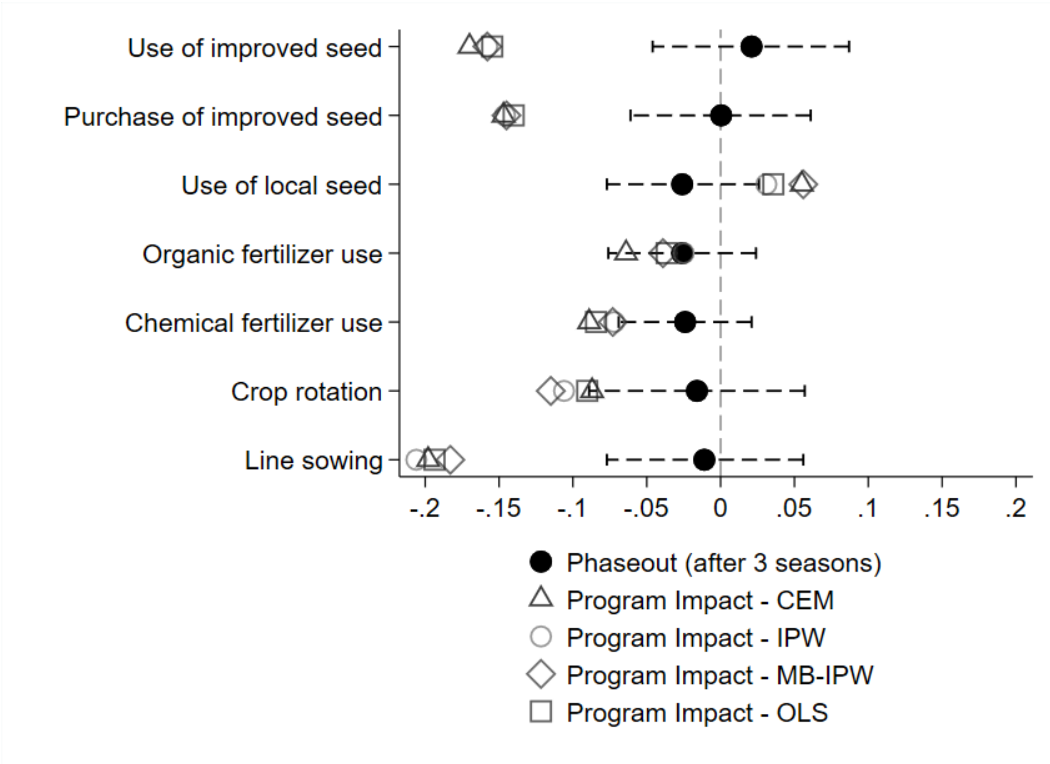


Figure III: Phaseout Impacts on Improved Seed Procurement From BRAC and Market Sources Over Time



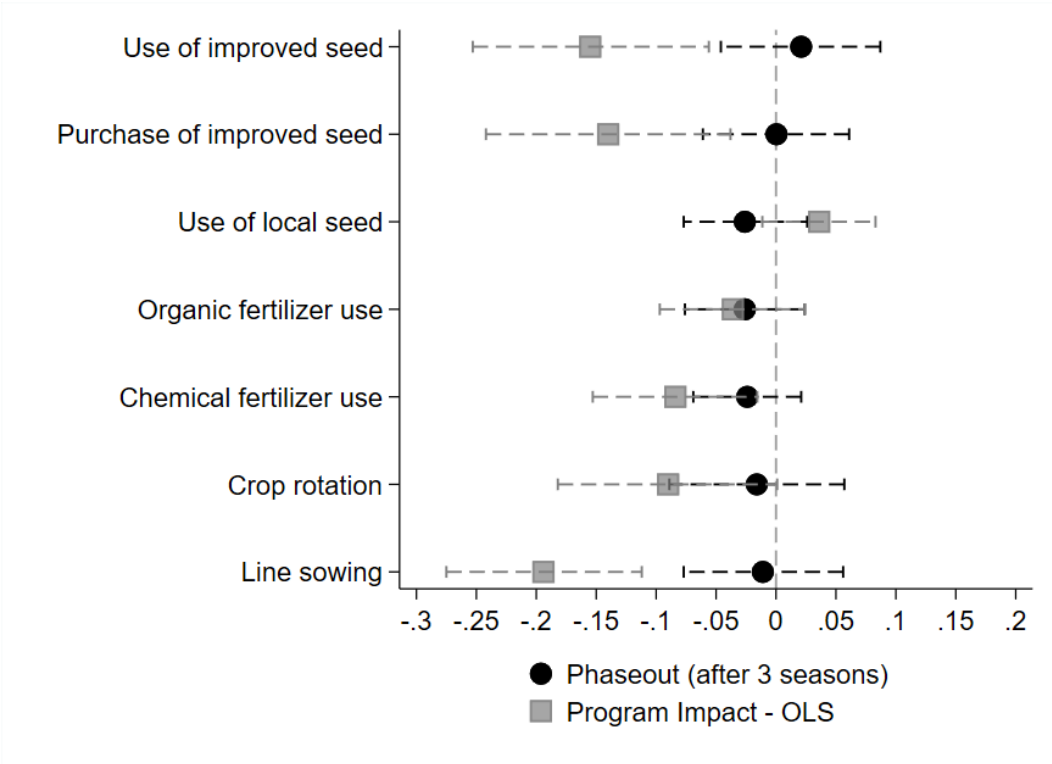
Note: Estimated impacts of phaseout on binary indicators of improved seed procurement from BRAC(MF/CAP) and market sources (see Table III notes for details). Error bars denote 95% confidence intervals.

Figure IV: Phaseout Impacts vs. Program Impacts



Note: Comparisons of the effects of phaseout, estimated experimentally from regression 1 (point estimates represented by circles, error bars denote 95% confidence intervals), and the (negative of the) effects of the program itself, as estimated through OLS and several other matching approaches (CEM, IPW and MB-IPW) that compare the "No Treatment" group with the Continuation group. Effects are estimated three seasons after phaseout.

Figure V: Phaseout Impacts vs. Program Impacts



Note: Comparisons of the effects of phaseout, estimated experimentally from regression 1 (circles represent point estimates, error bars represent 95% confidence intervals), and the (negative of the) effects of the program itself, as estimated through OLS (squares represent point estimates, error bars represent 95% confidence intervals) that compares the "No Treatment" group with the Continuation group. Effects are estimated three seasons after phaseout.

Tables

Table I: Balance of Pre-Phaseout Baseline Characteristics

	(1) Continuation	(2) Combined Phaseout	(3) Columns (1) - (2)	(4) CAP-1st Phaseout	(5) Columns (1) - (4)	(6) MF-1st Phaseout	(7) Columns (1) - (6)
Program components - inputs (binary indicators)							
Improved seed use	0.594	0.545	0.048	0.550	0.043	0.540	0.054
	(0.025)	(0.019)	(0.031)	(0.027)	(0.036)	(0.026)	(0.036)
BRAC seed use	0.244	0.252	-0.007	0.265	-0.021	0.239	0.005
	(0.021)	(0.016)	(0.027)	(0.024)	(0.032)	(0.022)	(0.031)
Market seed use	0.317	0.271	0.045	0.266	0.051	0.275	0.042
	(0.023)	(0.017)	(0.030)	(0.024)	(0.033)	(0.023)	(0.033)
Organic fertilizer use	0.138	0.109	0.029	0.130	0.007	0.088	0.049**
	(0.017)	(0.012)	(0.020)	(0.018)	(0.025)	(0.015)	(0.023)
Program components - practices (binary indicators)							
Crop rotation	0.548	0.585	-0.037	0.578	-0.030	0.592	-0.043
	(0.025)	(0.019)	(0.031)	(0.027)	(0.036)	(0.026)	(0.036)
Intercropping	0.707	0.666	0.041	0.657	0.050	0.674	0.032
	(0.023)	(0.018)	(0.029)	(0.026)	(0.034)	(0.026)	(0.033)
Line sowing	0.718	0.678	0.041	0.638	0.081**	0.716	0.003
	(0.022)	(0.018)	(0.028)	(0.026)	(0.034)	(0.024)	(0.033)
Weeding	0.891	0.842	0.049**	0.879	0.012	0.807	0.083**
	(0.016)	(0.014)	(0.021)	(0.018)	(0.023)	(0.021)	(0.026)
Zero tillage	0.088	0.072	0.016	0.069	0.019	0.074	0.014
	(0.0142)	(0.010)	(0.017)	(0.014)	(0.020)	(0.014)	(0.020)
Pest&disease mgmt	0.472	0.452	0.020	0.427	0.046	0.476	-0.042
	(0.025)	(0.019)	(0.031)	(0.027)	(0.037)	(0.026)	(0.036)
Household characteristics							
Farmer age	39.87	39.63	0.234	39.22	0.644	40.03	-0.162
	(0.587)	(0.438)	0.733	(0.592)	(0.834)	(0.643)	(0.871)
Cultivated land	2.473	2.397	0.0758	2.454	0.019	2.342	0.131
in acres	(0.080)	(0.060)	(0.100)	(0.089)	(0.120)	(0.080)	(0.113)
Own ag. land	2.088	2.131	-0.043	2.159	-0.071	2.105	-0.016
in acres	(0.073)	(0.058)	(0.093)	(0.083)	(0.110)	(0.082)	(0.110)
Formal title to land	0.558	0.509	0.049	0.488	0.070*	0.530	0.029
yes/no	(0.025)	(0.019)	(0.032)	(0.028)	(0.037)	(0.027)	(0.037)
# of rooms in house	2.691	2.623	0.069	2.638	0.054	2.609	0.083
	(0.077)	(0.055)	(0.095)	(0.081)	(0.112)	(0.074)	(0.107)
At least 2 sets clothes	0.968	0.941	0.027**	0.952	0.015	0.931	0.037**
yes/no	(0.009)	(0.009)	(0.013)	(0.012)	(0.015)	(0.014)	(0.016)

Table I – *Continued from previous page*

	(1) Continuation	(2) Combined Phaseout	(3) Columns (1) - (2)	(4) CAP-1st Phaseout	(5) Columns (1) - (4)	(6) MF-1st Phaseout	(7) Columns (1) - (6)
At least 2 pairs shoes	0.781	0.791	-0.009	0.820	-0.038	0.764	0.017
yes/no	(0.021)	(0.016)	(0.027)	(0.022)	(0.030)	(0.023)	(0.031)
Mobile phone	0.764	0.870	-0.106	0.892	-0.128	0.849	-0.085
number owned by HH	(0.055)	(0.042)	(0.069)	(0.062)	(0.083)	(0.056)	(0.079)
HH appliances	1.873	1.977	-0.104	2.058	-0.185	1.903	-0.030
number owned by HH	(0.134)	(0.114)	(0.181)	(0.150)	(0.201)	(0.171)	(0.218)
Poultry	5.631	6.174	-0.542	6.650	-1.019	5.711	-0.079
number owned by HH	(0.379)	(0.395)	(0.593)	(0.561)	0.677	(0.555)	(0.672)
Livestock, small	2.424	2.313	0.112	2.606	0.181	2.030	0.395
number owned by HH	(0.191)	(0.214)	(0.316)	(0.348)	(0.381)	(0.254)	(0.313)
Livestock, large	1.188	1.255	-0.067	1.434	-0.246*	1.086	0.102
number owned by HH	(0.081)	(0.072)	(0.112)	(0.107)	(0.134)	(0.095)	(0.125)
N	405	719		352		367	

Note: Mean values of key baseline characteristics at each of the three experimental groups. Columns 1-3 report the difference between the Continuation and the combined phaseout sample, and Columns 4-7 report differences between the Continuation and each of the two phaseout groups (Cap-1st and MF-1st) separately. Standard errors in parentheses. Stars indicate statistical significance: * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

Table II: The Effect of Phaseout on Seed Use

	(1)	(2)	(3)	(4)
Seasons after Phaseout	**1**	**3**	**5**	**6**
Improved Seed Use (Y/N)	0.008	0.021	-0.042	0.020
	(0.033)	(0.034)	(0.042)	(0.040)
95% CI	[-0.058 0.075]	[-0.046 0.087]	[-0.125 0.041]	[-0.060 0.100]
R^2	0.18	0.17	0.10	0.09
N	1037	1032	921	925
Mean Value in Continuation	0.43	0.39	0.43	0.32
Improved Seed Use (kg/acre)		0.289	-0.049	
		(0.227)	(0.289)	
95% CI		[-0.161 0.739]	[-0.622 0.525]	
R^2		0.09	0.09	
N		998	878	
Mean Value in Continuation		1.85	1.96	
Local Seed Use (Y/N)	-0.016	-0.026	-0.006	0.002
	(0.023)	(0.026)	(0.029)	(0.030)
95% CI	[-0.061 0.029]	[-0.077 0.026]	[-0.064 0.052]	[-0.057 0.060]
R^2	0.19	0.09	0.07	0.07
N	1038	1031	930	934
Mean Value in Continuation	0.87	0.88	0.86	0.83
Local Seed Use (kg/acre)		-0.815	0.832	
		(1.283)	(0.817)	
95% CI		[-3.361 1.730]	[-0.789 2.453]	
R^2		0.08	0.19	
N		978	922	
Mean Value in Continuation		11.9	8.0	

Note: OLS regression estimates of the impact of phaseout on a binary indicator of improved and local seed use, and on quantities of improved and local seed use (in kg/acre). Outcome variables are indicated on the leftmost column. The explanatory variable is a binary indicator of phaseout. Regressions also include branch fixed effects, and control for the outcome variable at pre-phaseout baseline, as well as the use of agricultural practices that differ at baseline (weeding, line sowing and organic fertilizer use) and land title. Standard errors, clustered by village cluster, in parentheses. Stars indicate statistical significance: * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

Table III: The Effect of Phaseout on Procurement of Improved Seeds, by Source

	BRAC (CAP and MF)	Market Sources	Other BRAC Sources
	(1)	(2)	(3)
1 Season After Phaseout	-0.049*	0.021	0.012*
	(0.026)	(0.029)	(0.007)
R^2	0.08	0.17	0.07
N	1037	1037	1015
Mean Value in Continuation	0.10	0.27	0.01
3 Season After Phaseout	-0.055*	0.057*	0.017***
	(0.030)	(0.034)	(0.007)
R^2	0.11	0.15	0.03
N	1032	1032	1032
Mean Value in Continuation	0.10	0.26	0.00
5 Season After Phaseout	-0.123***	0.061**	0.005
	(0.031)	(0.027)	(0.009)
R^2	0.12	0.09	0.03
N	907	907	907
Mean Value in Continuation	0.14	0.23	0.02
6 Season After Phaseout	-0.103***	0.113***	-0.009
	(0.028)	(0.027)	(0.010)
R^2	0.12	0.08	0.04
N	911	911	911
Mean Value in Continuation	0.12	0.14	0.02

Note: OLS regression estimates of the impact of phaseout on binary indicators of improved seed procurement from various sources, observed 1-6 seasons post phaseout (as indicated on the leftmost column). Regressions include branch fixed effects, and control for the baseline value of the outcome variable, the use of agricultural practices that differ at baseline (weeding, line sowing and organic fertilizer use) and land title. Standard errors, clustered by village cluster, in parentheses. Stars indicate statistical significance: * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

Table IV: The Effect of Phaseout on Cultivation Practices and Inputs

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Crop Rotation	**Inter-Cropping**	**Line Sowing**	**Organic Fertilizer**	**Chemical Fertilizer**	**Proper Weeding**	**Pest Control**
				Three Seasons After Phaseout			
Phaseout (Combined)	-0.016	-0.019	-0.011	-0.026	-0.024	-0.005	0.058*
	(0.037)	(0.024)	(0.034)	(0.025)	(0.023)	(0.018)	(0.030)
95% CI	[-0.089 0.057]	[-0.066 0.028]	[-0.077 0.056]	[-0.076 0.024]	[-0.069 0.021]	[-0.040 0.031]	[-0.001 0.118]
R^2	0.12	0.34	0.26	0.12	0.10	0.04	0.10
N	1029	1029	1031	1030	1029	1033	1021
Mean value in Continuation	0.69	0.59	0.73	0.145	0.120	0.94	0.21
				Six Seasons After Phaseout			
Phaseout (Combined)	-0.002	0.002	0.056	0.007	0.024	-0.003	0.009
	(0.042)	(0.044)	(0.039)	(0.033)	(0.040)	(0.022)	(0.046)
95% CI	[-0.087 0.082]	[-0.085 0.089]	[-0.022 0.134]	[-0.058 0.072]	[-0.055 0.103]	[-0.048 0.041]	[-0.082 0.099]
R^2	0.19	0.14	0.27	0.15	0.14	0.05	0.10
N	926	927	925	855	853	927	918
Mean value in Continuation	0.51	0.43	0.59	0.199	0.076	0.87	0.23

Note: OLS regression estimates of the impact of phaseout on binary indicators of improved practices. Regressions include branch fixed effects, and control for baseline value of the outcome, the use of agricultural practices that differ at baseline (weeding, line sowing and organic fertilizer use) and land title. Standard errors, clustered by village cluster, in parentheses. Stars indicate statistical significance: * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

Table V: RCT Estimates of Program Impacts

	(1)	(2)
Purchased any improved seeds	0.061***	0.077***
	(0.013)	(0.018)
Purchased seeds from BRAC sources	0.062***	0.066***
	(0.010)	(0.014)
Adopted crop rotation	0.022	-0.009
	(0.025)	(0.036)
Adopted inter-cropping	-0.008	-0.005
	(0.005)	(0.008)
Adopted line sowing	0.012	0.026
	(0.014)	(0.020)
Adopted proper weeding	0.005	-0.008
	(0.018)	(0.025)
N	6,229	3,156

Note: Intent-to-Treat experimental estimates of the impact of the BRAC program in 4 BRAC branches in South West Uganda (outcome variables measured at endline). Each row reports a regression in which the outcome variable is indicated on the left hand side. The study encompasses 230 village clusters. Regressions control for baseline values of the outcome variables and branch fixed effects. Column 1 reports results that use the entire sample. Column 2 restricts the sample to those not receiving the microfinance intervention. Standard errors, clustered by village cluster, in parentheses. Stars indicate statistical significance: * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

Table VI: Comparison of the "No Treatment" and Continuation Groups

	No Treatment	**Continuation**	**Difference**
Farmer age	41.99	42.20	-0.21
	(0.53)	(0.62)	(0.82)
Education level, highest grade completed	5.31	5.16	0.14
	(0.15)	(0.19)	(0.25)
Cultivated land, in acres	1.98	2.17	-0.19
	(0.07)	(0.09)	(0.11)
Own agricultural land, in acres	1.81	1.75	0.06
	(0.07)	(0.07)	(0.10)
Formal title to land	0.56	0.61	-0.05
	(0.02)	(0.02)	(0.03)
At least two sets of clothes	0.89	0.90	-0.02
	(0.01)	(0.01)	(0.02)
At least two sets of shoes	0.65	0.66	-0.02
	(0.02)	(0.02)	(0.03)
Livestock, large	1.15	1.20	-0.05
	(0.09)	(0.11)	(0.14)
Livestock, small	1.37	1.25	0.12
	(0.09)	(0.10)	(0.13)
Microfinance member	0.24	0.65	-0.42***
	(0.02)	(0.02)	(0.03)
Distance to BRAC branch office	6.50	4.11	2.39***
	(0.14)	(0.12)	(0.18)

Note: Mean values of the indicated variables in the "No Treatment" (Column 1) and Continuation (Column 2) groups, and their differences (Column 3). Standard errors in parentheses. Stars indicate statistical significance: * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

Table VII: Program Impacts on Seed Use

	OLS (1)	IPW (2)	MB-IPW (3)	CEM (4)
Improved Seed Use (Y/N)	0.155***	0.158***	0.158***	0.170***
	(0.050)	(0.035)	(0.045)	(0.052)
R^2	0.221			
N	1079	979	979	788
Mean Value in "No Treatment"		0.240		
Improved Seed Purchase (Y/N)	0.140***	0.146***	0.145***	0.147***
	(0.051)	(0.031)	(0.044)	(0.053)
R^2	0.218			
N	1078	978	978	787
Mean Value in "No Treatment"		0.227		
Local Seed Use (Y/N)	-0.036	-0.031	-0.056*	-0.055*
	(0.024)	(0.020)	(0.033)	(0.030)
R^2	0.077			
N	1080	980	980	789
Mean Value in "No Treatment"		0.922		

Note: Regression estimates of the impact of the BRAC program on binary indicators of improved seed use and purchase, and on a binary indicator of local seed use. Column 1 reports estimates of OLS regressions that include branch fixed effects and control for micro-finance membership (binary indicator denoting existence of a BRAC micro-finance group in the village) and distance to the BRAC branch office (measured in kilometers). Columns 2-4 report matching estimates that are restricted to households on the common support. Column 2 reports Inverse Probability Weighted (IPW) estimators (Hirano and Imbens, 2001). Column 3 reports Minimum Biased Inverse Probability Weighted (MB-IPW) estimators, following Millimet and Tchernis (2013). Column 4 reports Coarsened Exact Matching (CEM) estimators, following Iacus et al. (2012). All matching estimators use distance from BRAC branch office, education and age of the farmer as matching variables. Estimates based on data from the second follow-up survey (3 seasons after phaseout). Standard errors, clustered by village cluster, in parentheses. Stars indicate statistical significance: * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

Table VIII: Program Impacts on Cultivation Practices and Inputs

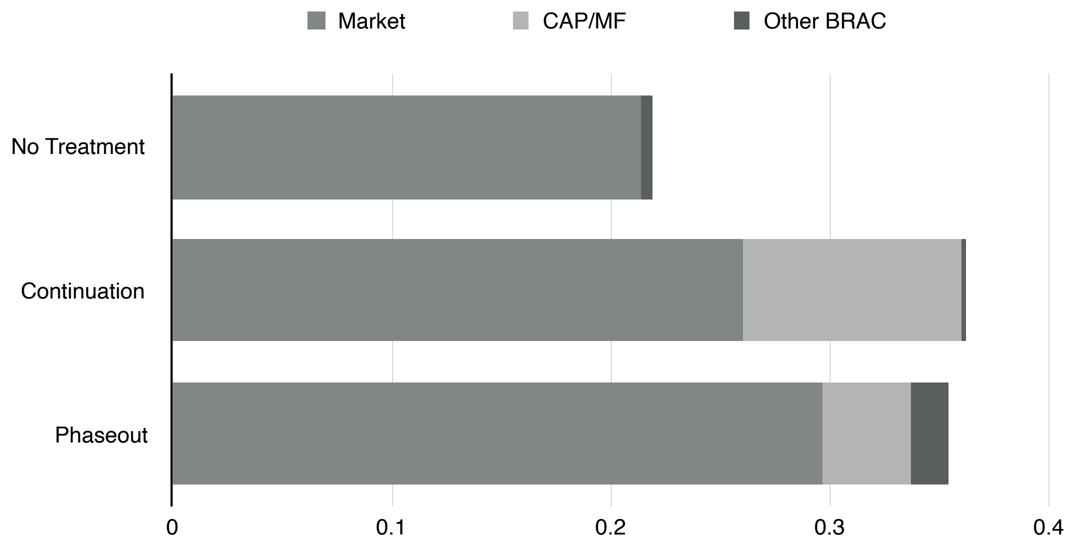
	Crop Rotation				Inter-cropping				Line Sowing				Organic Fertilizer			
	OLS	IPW	MB-IPW	CEM	OLS	IPW	MB-IPW	CEM	OLS	IPW	MB-IPW	CEM	OLS	IPW	MB-IPW	CEM
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
Continuation dummy	0.090*	0.106***	0.115***	0.087	-0.030	-0.035	-0.022	-0.062	0.194***	0.206***	0.183***	0.198***	0.036	0.025	0.039	0.064**
	(0.046)	(0.034)	(0.040)	(0.056)	(0.034)	(0.034)	(0.038)	(0.046)	(0.041)	(0.024)	(0.024)	(0.050)	(0.031)	(0.016)	(0.036)	(0.027)
R^2	0.089				0.351				0.172				0.110			
N	1081	981	981	787	1081	981	981	787	1082	982	982	788	1093	988	988	797
Mean value in No Treatment	0.685				0.588				0.753				0.059			

	Chemical Fertilizer				Proper Weeding				Pest Control			
	OLS	IPW	MB-IPW	CEM	OLS	IPW	MB-IPW	CEM	OLS	IPW	MB-IPW	CEM
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Continuation dummy	0.084**	0.072***	0.073**	0.089**	0.020	0.045*	0.047	-0.028	0.056	0.055*	0.048	0.079*
	(0.035)	(0.018)	(0.024)	(0.035)	(0.028)	(0.021)	(0.035)	(0.024)	(0.037)	(0.033)	(0.033)	(0.036)
R^2	0.139				0.173				0.082			
N	1095	990	990	799	1084	984	984	790	1082	942	942	788
Mean value in No Treatment	0.065				0.922				0.177			

Note: Regression estimates of the impact of the BRAC program on binary indicators of improved cultivation practices. OLS regressions that include branch fixed effects and control for micro-finance membership (binary indicator denoting existence of a BRAC micro-finance group in the village) and distance to the BRAC branch office (measured in kilometers). Inverse Probability Weighted (IPW) estimators (Hirano and Imbens, 2001), Minimum Biased Inverse Probability Weighted (MB-IPW) estimators Millimet and Tchernis (2013) and Coarsened Exact Matching (CEM) estimators (Iacus et al., 2012) use distance from BRAC branch office, education and age of the farmer as matching variables. Estimates based on data from the second follow-up survey (3 seasons after phaseout). Standard errors, clustered by village cluster, in parentheses. Stars indicate statistical significance: * p<0.1, ** p<0.05, *** p<0.01.

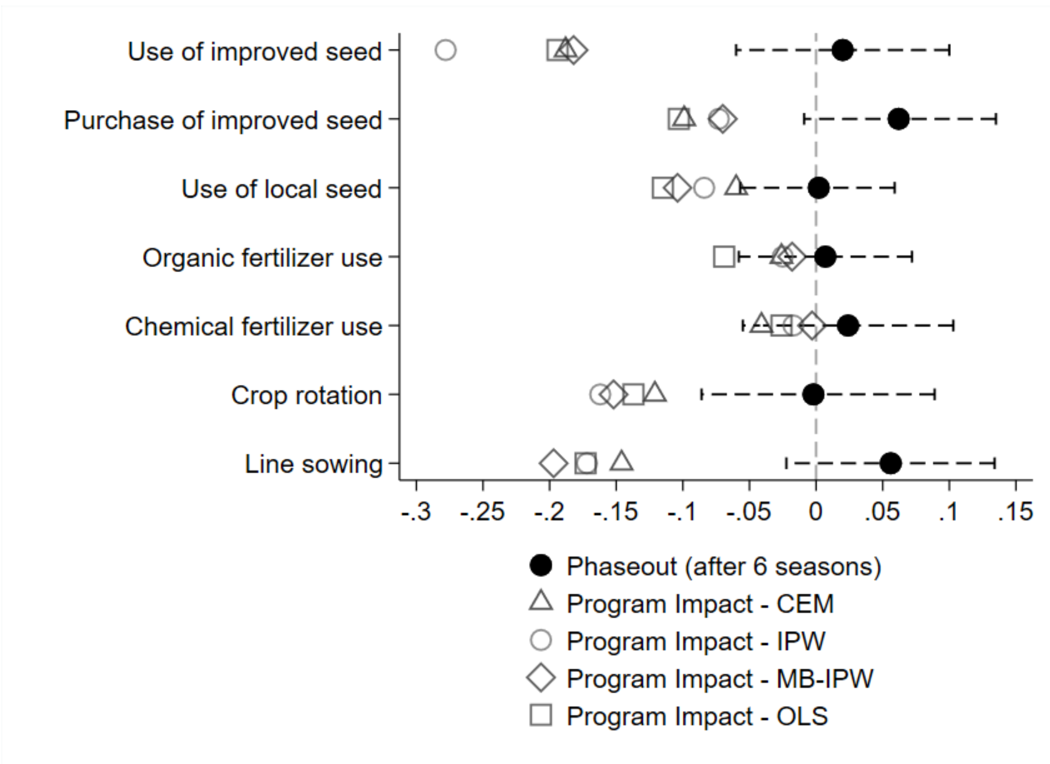
Appendix A. Additional Tables and Figures

Figure A.I: Procurement of Improved Seeds From Various Sources



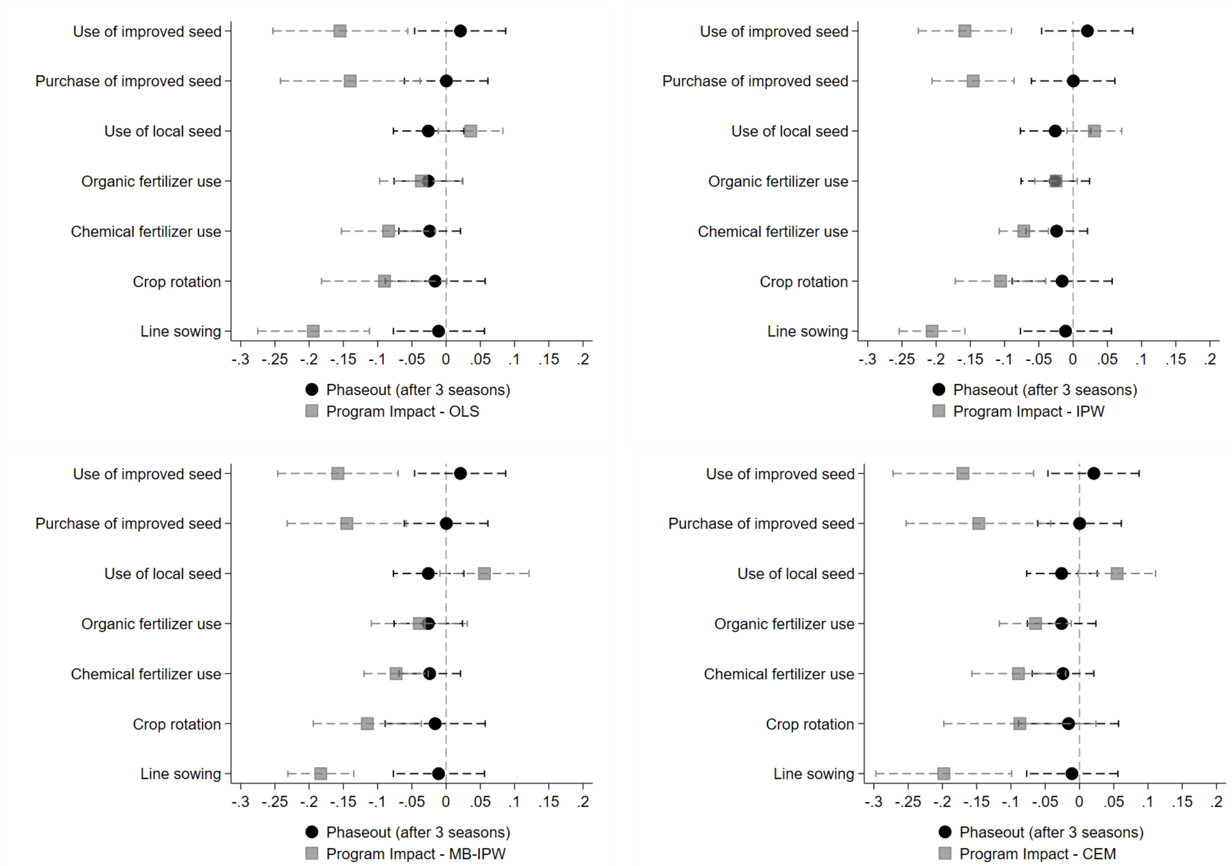
Note: Sources are observed three seasons after phaseout.

Figure A.II: Phaseout Impacts vs. Program Impacts, Six Seasons After Phaseout



Note: Comparisons of the effects of phaseout, estimated experimentally from regression 1 (point estimates represented by circles, error bars denote 95% confidence intervals), and the (negative of the) effects of the program itself, as estimated through OLS and several other matching approaches (CEM, IPW and MB-IPW) that compare the "No Treatment" group with the Continuation group. Effects are estimated six seasons after phaseout.

Figure A.III: Phaseout Impacts vs. Program Impacts



Note: Comparisons of the effects of phaseout, estimated experimentally from regression 1 (circles represent point estimates, error bars represent 95% confidence intervals), and the (negative of the) effects of the program itself, as estimated through OLS, IPW and CEM matching estimators (squares represent point estimates, error bars represent 95% confidence intervals) that compare the "No Treatment" group with the continuation group. Effects are estimated three seasons after phaseout.

Table A.I: Attrition rates by treatment group

	Continuation	**Difference wrt Continuation**	
	Attrition rate	**CAP-1st Phaseout**	**MF-1st Phaseout**
1 season after phaseout	0.152	0.029	0.055
		(0.036)	(0.048)
3 seasons after phaseout	0.178	0.036	0.065
		(0.045)	(0.042)

Note: Column 1 reports the attrition rate in the Continuation group. Columns 2 and 3 report the differences in attrition rates between the CAP-1st and MF-1st groups and the Continuation group. Standard errors in parentheses. Stars indicate statistical significance: * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

Table A.II: Baseline characteristics of attritors and non-attritors

	Improved seed use (yes/no)	**Organic fertilizer use** (yes/no)	**Crop rotation** (yes/no)	**Inter-cropping** (yes/no)	**Line sowing** (yes/no)	**Mixed cropping** (yes/no)
Difference between attritors and non-attritors	0.019 (0.038)	0.037 (0.042)	0.013 (0.034)	-0.026 (0.033)	-0.059* (0.032)	0.028 (0.031)
N	1628	1616	1639	1640	1639	1641

	Weeding (yes/no)	**Zero tillage** (yes/no)	**Farmer age**	**Farmer literacy** (yes/no)	**At least 2 sets of clothes** (yes/no)	**At least 2 pairs of shoes** (yes/no)
Difference between attritors and non-attritors	-0.038 (0.035)	-0.053 (0.046)	-0.000 (0.000)	0.014 (0.009)	-0.05 (0.064)	0.033 (0.039)
N	1638	1631	1131	1129	1506	1518

	# rooms in main house	**Cultivated land** in acres	**Land title** (yes/no)	**Mobile phone** (yes/no)
Difference between attritors and non-attritors	-0.000 (0.011)	-0.010 (0.007)	0.035 (0.030)	0.034 (0.034)
N	1529	1658	1560	1534

Note: Mean values of key baseline outcomes (inputs, practices, and household/farmer characteristics) for attritors and non-attritors. Standard errors are in parentheses. Stars indicate statistical significance: * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

Table A.III: Phaseout effect on other inputs and tools, binary indicator

3 seasons after phaseout

	Hired labor (1)	**Pesticide** (2)	**Hand plow** (3)	**Mechanized plow** (4)	**Pump for pesticides** (5)
Phaseout combined	0.032	0.030	-0.032	0.021**	0.032
	(0.034)	(0.028)	(0.030)	(0.010)	(0.030)
95% CI	[-0.035 0.100]	[-0.025 0.085]	[-0.092 0.028]	[0.002 0.040]	[-0.028 0.091]
R^2	0.102	0.103	0.653	0.669	0.090
N	1033	1030	1007	1003	1030
Mean value in Continuation	0.602	0.211	0.344	0.055	0.190

6 seasons after phaseout

	Hired labor (1)	**Pesticide** (2)	**Hand plow** (3)	**Mechanized plow** (4)	**Pump for pesticides** (5)
Phaseout combined	0.000	0.024	0.028	0.004	0.026
	(0.049)	(0.040)	(0.027)	(0.012)	(0.038)
95% CI	[-0.096 0.097]	[-0.055 0.103]	[-0.025 0.081]	[-0.020 0.028]	[-0.049 0.102]
R^2	0.081	0.140	0.175	0.124	0.082
N	857	853	838	834	855
Mean value in Continuation	0.466	0.199	0.026	0.026	0.150

Note: OLS regression estimates of the impact of phaseout on binary indicators of input and tool use. Regressions include branch fixed effects, and control for the outcome at pre-phaseout baseline, as well as the use of agricultural practices that differ at baseline (weeding, line sowing and organic fertilizer use) and land title. Standard errors (in parentheses) are clustered at the village cluster level. Stars indicate statistical significance: * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

Table A.IV: Phaseout effect on crop diversification

	Number of crops			**Cultivated maize** (yes/no)		
	1 season post-phaseout	3 seasons post-phaseout	5 seasons post-phaseout	1 season post-phaseout	3 seasons post-phaseout	5 seasons post-phaseout
	(1)	(2)	(3)	(4)	(5)	(6)
Phaseout combined	-0.102	-0.119	-0.211	-0.016	0.005	-0.011
	(0.342)	(0.189)	(0.131)	(0.024)	(0.024)	(0.024)
95% CI	[-0.780 0.576]	[-0.494 0.256]	[-0.471 0.049]	[-0.063 0.032]	[-0.043 0.053]	[-0.060 0.037]
R^2	0.272	0.064	0.278	0.243	0.203	0.271
N	857	1031	857	1039	1034	857
Mean value in Cont.	4.485	3.57	2.85	0.88	0.75	0.77

Note: OLS regression estimates of the impact of phaseout on the number of crops and a binary indicator for maize cultivation. Regressions include branch fixed effects, and control for the outcome at pre-phaseout baseline, as well as the use of agricultural practices that differ at baseline (weeding, line sowing and organic fertilizer use) and land title. Standard errors (in parentheses) are clustered at the village cluster level. Stars indicate statistical significance: * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

Table A.V: Phaseout effects on crop yields

	Maize yield, log kg					
	1 season post-phaseout		3 seasons post-phaseout		5 seasons post-phaseout	
	(1)	(2)	(3)	(4)	(5)	(6)
Phaseout combined	0.019		0.1069		0.0918	
	(0.069)		(0.0642)		(0.0937)	
95% CI	[-0.117 0.156]		[-0.0206 0.2344]		[-0.0942 0.2777]	
CAP-1st phaseout		0.0083		0.1147		0.0144
		(0.0909)		(0.0728)		(0.1044)
95% CI		[-0.1720 0.1887]		[-0.0298 0.2593]		[-0.1927 0.2216]
MF-1st phaseout		0.0303		0.0978		0.1705
		(0.0799)		(0.0747)		(0.1108)
95% CI		[-0.1283 0.1889]		[-0.0505 0.2461]		[-0.0494 0.3904]
R^2	0.203	0.203	0.149	0.149	0.124	0.127
N	773	773	706	706	688	688
Mean value in Continuation	5.608		5.198		5.339	

Note: OLS regression estimates of the impact of phaseout on crop yields. Dependent variables are in logarithms. Maize yield is calculated as kilograms of maize per acre of land on which maize is cultivated. Regressions include branch fixed effects, and control for the use of agricultural practices that differ at baseline (weeding, line sowing and organic fertilizer use) and land title. Standard errors (in parentheses) are clustered at the village cluster level. Stars indicate statistical significance: * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

Table A.VI: Phaseout effects on revenues and profits

	Revenues, in UGX						**Profits**, in UGX					
	after 1 season		after 3 seasons		after 5 seasons		after 1 season		after 3 seasons		after 5 seasons	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Phaseout	-46987.7		-7668.1		-51699.0		-572043.3		-47377.4		-75143.6	
	(32056.7)		(35350.4)		(53228.3)		(421554.6)		(64768.9)		(65574.6)	
CAP-1st phaseout		-51316.5		-49050.2		-45384.6		-584099.2		-46684.3		-83546.9
		(36068.9)		(34995.9)		(60725.1)		(479153.8)		(78417.7)		(66458.4)
MF-1st phaseout		-42980.5		30397.2		-58243.7		-559264.9		-48101.5		-66191.0
		(39494.3)		(44638.1)		(60544.9)		(397793.3)		(65028.0)		(75093.2)
R^2	0.201	0.201	0.069	0.074	0.127	0.127	0.031	0.031	0.036	0.036	0.067	0.067
N	554	554	626	626	700	700	570	570	970	970	917	917
Mean value in Continuation	383712.5		419823.3		455971.9		191508		976772.3			

Note: OLS regression estimates of the impact of phaseout on revenues and profits from agriculture (in Ugandan shillings). Regressions include branch fixed effects, and control for the use of agricultural practices that differ at baseline (weeding, line sowing and organic fertilizer use) and land title. Standard errors (in parentheses) are clustered at the village cluster level. Stars indicate statistical significance: * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

Table A.VII: Phaseout effects on CAP seed sales

3 seasons after phaseout

	Sale of BRAC seed dummy (1)	Maize seed sold - quantity log kg (2)	Maize seed sold - price log UGX/kg (3)
Phaseout combined	0.033	-0.156	0.124
	(0.101)	(0.504)	(0.154)
R^2	0.358	0.607	0.533
N	78	34	36
Mean value in Continuation	0.436	4.081	7.945

6 seasons after phaseout

	Sale of BRAC seed dummy (1)	Maize seed sold - quantity log kg (2)	Maize seed sold - price log UGX/kg (3)
Phaseout combined	-0.245**	-0.687	0.397
	(0.106)	(0.570)	(0.496)
R^2	0.411	0.590	0.278
N	77	21	22
Mean value in Continuation	0.444	3.313	7.695

Note: OLS regression estimates of the impact of phaseout on a binary indicator of BRAC seed sales (column 1), quantity of maize seed sold (log Kgs, column 2) and price of maize seed sold (log Ugandan shillings per Kg, column 3). Regressions include branch fixed effects. Standard errors (in parentheses) are clustered at the village cluster level. Stars indicate statistical significance: * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

Table A.VIII: Farmers' views on CAP and MF activity

	Continuation	**MF-1st Phaseout**	**CAP-1st Phaseout**
	(1)	(2)	(3)
CAPs sell seeds of better quality than other seeds available on the local market.	0.714	0.643	0.686
	(0.452)	(0.480)	(0.465)
You can get higher yields planting CAP seeds.	0.695	0.650	0.685
	(0.461)	(0.478)	(0.465)
You can get higher profits planting CAP seeds.	0.685	0.643	0.684
	(0.465)	(0.480)	(0.466)
MFs teach cultivation practices that are new or different from those used before.	0.825	0.793	0.844
	(0.380)	(0.406)	(0.363)
MFs teach cultivation practices that are better than the ones used before.	0.813	0.776	0.834
	(0.390)	(0.418)	(0.373)
You can get higher yields using practices taught by MFs.	0.846	0.816	0.858
	(0.361)	(0.388)	(0.350)
You can get higher profits using practices taught by MFs.	0.840	0.809	0.847
	(0.367)	(0.394)	(0.360)
N	338	294	295

Note: Values refer to the share of farmers in each treatment arm agreeing or strongly agreeing with the statements in the left column. Data was collected in the last follow-up survey.

Table A.IX: Program Impacts on Improved Seed Use, by Season

	Improved Seed Use				
Seasons After Phaseout	-1	1	3	5	6
	(1)	(2)	(3)	(4)	(5)
Continuation	0.160**	0.127**	0.155***	0.228***	0.194***
	(0.065)	(0.054)	(0.050)	(0.052)	(0.044)
BRAC Microfinance	-0.091*	-0.054	-0.019	-0.075	-0.048
Membership	(0.053)	(0.054)	(0.042)	(0.049)	(0.039)
Distance to BRAC Office	-0.019***	-0.008	-0.016***	-0.009	0.000
	(0.007)	(0.006)	(0.005)	(0.006)	(0.005)
R^2	0.257	0.164	0.221	0.179	0.121
N	1006	1003	1079	902	902

Note: OLS regression estimates of the impact of the BRAC program (being in the continuation group vs. in the NT group) on binary indicators of improved seed use, using data from pre-phaseout baseline (Column 1), and from one, three, five and six seasons after phaseout (Columns 2-5). Regressions include branch fixed effects and controls for microfinance membership (binary indicator denoting existence of a BRAC microfinance group in the village) and distance to BRAC branch office (measured in kilometers). Standard errors (in parentheses) are clustered at the village cluster level. Stars indicate statistical significance: * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

Table A.X: Program impact on improved seed use, alternative measure of microfinance membership

	Improved seed use		Improved seed purchases	
	OLS		OLS	
	(1)	(2)	(3)	(4)
Treated	0.163**		0.140***	
	(0.033)		(0.034)	
Continuation		0.153***		0.136**
		(0.056)		(0.057)
BRAC microfinance member	0.0.096***	0.140***	0.103***	0.146***
	(0.033)	(0.045)	(0.034)	(0.047)
Distance to BRAC office	-0.011***	-0.013***	-0.008**	-0.009*
	(0.004)	(0.005)	(0.004)	(0.005)
R^2	0.197	0.230	0.201	0.227
N	1471	889	1470	889
Mean value in No Treatment	0.240		0.227	

Note: OLS regression estimates of the impact of the BRAC program on binary indicators of improved seed use and purchase. Regressions include branch fixed effects and controls for microfinance membership (binary indicator denoting self-reported BRAC microfinance group membership) and distance to BRAC branch office (measured in kilometers). Standard errors (in parentheses) are clustered at the village cluster level. Estimates based on data from the second follow-up survey (3 seasons after phaseout). Stars indicate statistical significance: * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

Table A.XI: Program impact on crop diversification

	Number of crops grown				Cultivated maize			
	OLS	IPW	MB-IPW	CEM	OLS	IPW	MB-IPW	CEM
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Continuation dummy	0.368**	0.262**	0.275**	0.445**	0.070***	0.073***	0.072*	0.058**
	(0.171)	(0.132)	(0.132)	(0.190)	(0.024)	(0.024)	(0.038)	(0.027)
R^2	0.092				0.260			
N	1084	984	984	790	1083	983	983	792
Mean value in No Treatment	3.274				0.786			

Note: Regression estimates of the impact of the BRAC program on the number of crops grown and on a binary indicator of maize cultivation. OLS regressions include branch fixed effects and control for micro-finance membership (binary indicator denoting existence of a BRAC micro-finance group in the village) and distance to the BRAC branch office (measured in kilometers). Inverse Probability Weighted (IPW) estimators (Hirano and Imbens, 2001), Minimum Biased Inverse Probability Weighted (MB-IPW) estimators Millimet and Tchernis (2013) and Coarsened Exact Matching (CEM) estimators (Iacus et al., 2012) use distance from BRAC branch office, education and age of the farmer as matching variables. Standard errors, clustered by village cluster, in parentheses. Estimates based on data from the second follow-up survey (3 seasons after phaseout). Stars indicate statistical significance: * p<0.1, ** p<0.05, *** p<0.01.

Table A.XII: Program impacts on other inputs

	Hired labor				Pesticide				Hand plow			
	OLS	IPW	MB-IPW	CEM	OLS	IPW	MB-IPW	CEM	OLS	IPW	MB-IPW	CEM
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Continuation dummy	0.106**	0.107**	0.090***	0.102*	0.055	0.056	0.059	0.081**	0.080**	0.045*	0.041	0.083**
	(0.052)	(0.034)	(0.021)	(0.053)	(0.033)	(0.033)	(0.041)	(0.034)	(0.035)	(0.026)	(0.041)	(0.033)
R^2	0.101				0.074				0.694			
N	1096	991	991	800	1096	991	991	800	1075	975	975	786
Mean value in No Treatment	0.454				0.170				0.265			

	Mechanized plow				Pump for pesticide			
	OLS	IPW	MB-IPW	CEM	OLS	IPW	MB-IPW	CEM
	(13)	(14)	(15)	(16)	(17)	(18)	(19)	(20)
Continuation dummy	-0.008	0.012	0.018	-0.011	0.036	0.043	0.045	0.065
	(0.022)	(0.014)	(0.042)	(0.016)	(0.036)	(0.037)	(0.044)	(0.035)
R^2	0.426				0.069			
N	1075	975	975	785	1075	991	991	785
Mean value in No Treatment	0.080				0.167			

Note: Regression estimates of the impact of the BRAC program on binary indicators of input and tool use. Columns 1, 5, 9, 13 and 17 report estimates of OLS regressions that include branch fixed effects and controls for microfinance membership (binary indicator denoting existence of a BRAC microfinance group in the village) and distance to BRAC branch office (measured in kilometers). Columns 2-4, 6-8, 10-12, 14-16 and 18-20 report matching estimates that are restricted to households on the common support. IPW reports the Inverse Probability Weighted estimator (Hirano and Imbens, 2001). MB-IPW reports the Minimum Biased Inverse Probability Weighted estimator, following Millimet and Tchernis (2013). CEM reports the Coarsened Exact Matching estimator, following Iacus et al. (2012). All matching estimators use distance from BRAC branch office, education and age of the farmer as matching variables. Standard errors (in parentheses) are clustered at the village cluster level. Estimates based on data from the second follow-up survey (3 seasons after phaseout). Stars indicate statistical significance: * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

Appendix B. Separate Impacts of the CAP-1st and MF-1st Phaseout

Table B.I: Phaseout effect on improved seed use, binary indicator

	Improved seed use			
Seasons Post-Phaseout	1	3	5	6
	(1)	(2)	(3)	(4)
CAP-1st phaseout	0.0072	-0.0101	-0.0248	0.0067
	(0.0370)	(0.0390)	(0.0483)	(0.0468)
95% CI	[-0.0662 0.0806]	[-0.0875 0.0673]	[-0.1205 0.0710]	[-0.0862 0.0996]
MF-1st phaseout	0.0094	0.0524	-0.0577	0.0255
	(0.0389)	(0.0391)	(0.0429)	(0.0400)
95% CI	[-0.0678 0.0865]	[-0.0253 0.1300]	[-0.1428 0.0274]	[-0.0539 0.1049]
R^2	0.180	0.177	0.099	0.082
N	1037	1032	921	925
Mean value in Continuation	0.427	0.386	0.424	0.317

Note: OLS regression estimates of the impact of phaseout on a binary indicator of improved seed use. Regressions include branch fixed effects, and control for the outcome variable at pre-phaseout baseline, as well as the use of agricultural practices that differ at baseline (weeding, line sowing and organic fertilizer use) and land title. Standard errors, clustered by village cluster, in parentheses. Stars indicate statistical significance: * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

Table B.II: Phaseout effect on improved seed use, quantities

	Improved seed quantities, total		Improved seed quantities, per acre	
Seasons Post-Phaseout	3	5	3	5
	(1)	(2)	(3)	(4)
CAP-1st phaseout	0.1834	-0.2038	-0.0725	-0.0095
	(0.5550)	(0.5275)	(0.2331)	(0.3219)
95% CI	[-0.9179 1.2847]	[-1.2506 0.8429]	[-0.5350 0.3901]	[-0.6483 0.6293]
MF-1st phaseout	0.8879	-0.2292	0.6720*	-0.0424
	(0.6785)	(0.5576)	(0.3111)	(0.03395)
95% CI	[-0.4586 2.2344]	[-1.3356 0.8771]	[0.0546 1.2895]	[-0.7160 0.6312]
R^2	0.093	0.086	0.090	0.099
N	1029	926	998	878
Mean value in Continuation	3.48	3.19	1.85	1.95

Note: OLS regression estimates of the impact of phaseout on quantities of improved seed use (in Kg). Regressions include branch fixed effects, and control for the use of agricultural practices that differ at baseline (weeding, line sowing and organic fertilizer use) and land title. Standard errors, clustered by village cluster, in parentheses. Stars indicate statistical significance: * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

Table B.III: Phaseout effect on local seed use, binary indicator

	Local seed use			
Seasons Post-Phaseout	1	3	5	6
	(1)	(2)	(3)	(4)
CAP-1st phaseout	-0.0319	-0.0043	-0.0220	-0.0158
	(0.0279)	(0.0277)	(0.0357)	(0.0360)
95% CI	[-0.0873 0.0235]	[-0.0592 0.0506]	[-0.0928 0.0488]	[-0.0874 0.0558]
MF-1st phaseout	0.0008	-0.0480	0.0111	0.0153
	(0.0257)	(0.0320)	(0.0324)	(0.0317)
95% CI	[-0.0501 0.0518]	[-0.1116 0.0156]	[-0.0531 0.0753]	[-0.0476 0.0782]
R^2	0.190	0.093	0.057	0.062
N	1038	1031	930	934
Mean value in Continuation	0.878	0.869	0.859	0.836

Note: OLS regression estimates of the impact of phaseout on a binary indicator of local seed use. Regressions include branch fixed effects, and control for the outcome variable at pre-phaseout baseline, as well as the use of agricultural practices that differ at baseline (weeding, line sowing and organic fertilizer use) and land title. Standard errors, clustered by village cluster, in parentheses. Stars indicate statistical significance: * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

Table B.IV: Phaseout effect on local seed use, quantities

	Local seed quantities, total		**Local seed quantities, per acre**	
Seasons Post-Phaseout	3	5	3	5
	(1)	(2)	(3)	(4)
CAP-1st phaseout	2.0567	-1.1625	-0.5613	-0.0393
	(2.0496)	(1.8236)	(1.5440)	(0.8868)
95% CI	[-2.0106 6.1241]	[-4.7808 2.4559]	[-3.6257 2.5030]	[-1.7991 1.7204]
MF-1st phaseout	-0.0297	-0.6663	-1.0874	0.9919
	(2.1304)	(2.1000)	(1.4731)	(1.0311)
95% CI	[-4.25746 4.1980]	[-4.8331 3.5005]	[-4.0110 1.8363]	[-1.0540 3.0378]
R^2	0.120	0.201	0.076	0.176
N	1009	925	978	922
Mean value in Continuation	17.5	15.9	11.9	9.4

Note: OLS regression estimates of the impact of phaseout on quantities of local seed use (in Kg). Regressions include branch fixed effects, and control for the use of agricultural practices that differ at baseline (weeding, line sowing and organic fertilizer use) and land title. Standard errors, clustered by village cluster, in parentheses. Stars indicate statistical significance: * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

Table B.V: Phaseout Effect on Procurement of Improved Seed, by Source

	CAP and MF	Market Sources	Other BRAC
	(1)	(2)	(3)
		1 Season After Phaseout	
CAP-1st phaseout	-0.0465*	0.0288	0.0062
	(0.0269)	(0.0336)	(0.0079)
MF-1st phaseout	-0.0517*	0.0124	0.0180*
	(0.0303)	(0.0345)	(0.0095)
R^2	0.075	0.172	0.071
N	1037	1037	1015
Mean value in Continuation	0.096	0.271	0.005
		3 Season After Phaseout	
CAP-1st phaseout	-0.0347	0.0305	0.0136
	(0.0322)	(0.0394)	(0.0095)
MF-1st phaseout	-0.0768**	0.0847**	0.0234**
	(0.0327)	(0.0388)	(0.0093)
R^2	0.114	0.152	0.037
N	1032	1032	1032
Mean value in Continuation	0.102	0.256	0.002
		5 Season After Phaseout	
CAP-1st phaseout	-0.112***	0.0756**	0.0007
	(0.0306)	(0.0371)	(0.0107)
MF-1st phaseout	-0.123***	0.0538*	-0.0013
	(0.0302)	(0.0292)	(0.0106)
R^2	0.113	0.084	0.021
N	907	907	907
Mean value in Continuation	0.142	0.234	0.018
		6 Season After Phaseout	
CAP-1st phaseout	-0.0965***	0.117***	-0.0050
	(0.0264)	(0.0371)	(0.0137)
MF-1st phaseout	-0.102***	0.113***	-0.0205**
	(0.0288)	(0.0284)	(0.0091)
R^2	0.107	0.074	0.037
N	911	911	911
Mean value in Continuation	0.117	0.139	0.021

Note: OLS regression estimates of the impact of phaseout on binary indicators of improved seed procurement from various sources. Regressions include branch fixed effects, and control for the baseline value of the outcome variable, the use of agricultural practices that differ at baseline (weeding, line sowing and organic fertilizer use) and land title. Standard errors, clustered by village cluster, in parentheses. Stars indicate statistical significance: * $p<0.1$, ** $p<0.05$, *** $p<0.01$.