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ABSTRACT

How Sustainable Are Benefits from Extension for Smallholder Farmers? Evidence from a Randomized Phase-Out of the BRAC Program in Uganda[*]

Many development programs are based on short-term interventions, either because of external funding constraints or because it is assumed that impacts persist post program termination ("sustainability"). Using a novel randomized phase-out research method, we provide experimental tests of the effects of program phase-out in the context of a large-scale agricultural input subsidy and extension program operated by the NGO BRAC to increase the use of improved seed varieties and basic farming practices among women smallholders in Uganda. We find that while supply of improved seeds through local, BRAC trained women declined, demand does not diminish, and farmers shift purchases from BRAC to market sources, indicating a persistent learning effect. We also find no evidence of declines in the practice of improved and less costly cultivation techniques taught by the program. These results have implications for both efficient program design and for models of technology adoption.

JEL Classification: O13, O33, I32, Q12

Keywords: agricultural extension, agricultural technology adoption, food security, supply chain, subsidies, randomized phase-out, Uganda

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1. Introduction

Economic development programs are often based on short-term interventions. In some cases programs are designed on the premise that such 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 leaves a major gap in our understanding of optimal program duration; and underlines the need for empirical methods to test hypotheses addressing continuation, termination, and phase-outs of programs and policies.

This paper reports novel estimates of the impacts of program phase-out in the context of 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 many developing countries, some of which consist of temporary efforts and others are essentially permanent (see e.g. Anderson and Feder, 2007). After several years of implementation, the Uganda program was terminated in a randomized subsample of the treatment population, allowing us to estimate the causal impacts of phase-out.

The resulting estimates provide important insights into the post phase-out impacts of the program itself.[1] Moreover, our results also differ from (the rather few) existing post phase-out evaluations and contribute to the literature in a fundamental way. Rather than comparing post phase-out program outcomes to a counterfactual scenario in which no intervention took place, we compare outcomes to a counterfactual in which the program is continued. This new approach thus helps answer a distinct, if related, question that has important lessons for policy design: when, if at all, should development interventions be terminated? For example, finding that a program's impacts persist post phase-out does not determine whether continued implementation would result in further increases (or declines) in impacts, and whether continuation is cost-ineffective. Similarly, finding that impacts diminish post phase-out does not allow one to determine whether a continued

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[1] Post phase-out impact evaluations remain scarce, particularly in the context of agriculture. Recent notable exceptions include Carter et al. (2016) in agriculture, and in health, Baird et al. (2016), on deworming, and Maluccio et al. (2009), on early childhood nutrition supplements.

implementation would maintain the original impacts. Another difference between our approach and that of standard post phase-out comparisons is the possibility that the latter are influenced by time varying factors, particularly negative or positive spillovers, making results challenging to interpret.

It is far from obvious whether or when to terminate an apparently successful intervention, from either a theoretical or practical point of view. In the agricultural context, permanent government extension programs and interventions in input supply chains are common in both developing and industrialized countries, although they are sometimes claimed to be wasteful and inefficient. From a theoretical point of view, the impact of phasing out the supply of subsidized inputs on farmers' input use depends on whether these inputs are normally unused because they are unprofitable (Suri, 2011), or because of limited access to information, credit or insurance (for example, see Foster and Rosenzweig, 2010; Bardhan and Mookherjee, 2011; Duflo et al., 2011; Karlan et al., 2014; Emerick et al., 2016). Similarly, the impact of phasing out extension and demonstration programs of improved practices depends on farmers' learning processes (Hanna et al., 2012). For example, high variability in yields 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 (see Munshi, 2004).

The agricultural extension program we examine was implemented in Uganda by BRAC[2], a large, highly rated NGO. The program was focused on basic practices and on the use of improved seeds, which remains very low in Uganda and much of Sub Saharan Africa.[3] Low usage of improved seeds is a simultaneous problem of low demand and low supply. To address these problems, BRAC followed a two pronged approach: stimulating demand by demonstrating the benefits of improved seeds in the plots of model farmers, training farmers and distributing free samples; and stimulating supply by creating local semi-informal supply chains within villages. Our experimental design allows us to test each of those dimensions separately, because only one of those components was phased out in each of the two treatment arms in the first two seasons of the randomized phase-out.[4]

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[2] For details on BRAC, see Smillie (2009).

[3] 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).

[4] In earlier conference versions of this paper we used a different term, the reverse-Randomized Control Trial, but use the Randomized Phase-out term here, as it is more specific to our application.

In what sense can the impacts of these interventions be considered to persist post program termination? Consider the important example of improved seeds. From the supply side, a successful experience with selling seeds could encourage informal village suppliers to continue to work as (for-profit) distribution agents. From the demand side, a positive experience with improved seeds could permanently increase demand for such seeds by local farmers.

Our results suggest that the semi-informal supply of improved seeds declined as a result of phase-out: without BRAC involvement the number of suppliers and amounts sold have declined in phase-out villages (evidence suggests transportation costs have played a major role). Results for the demand side are more positive. While seed purchases from formal and informal BRAC sources decline in phase-out villages, we find evidence that farmers switch to purchasing improved seeds from local market sources, so that after a brief period of adjustment the total impact on improved seed usage from all sources remained nearly unchanged. Similarly, our results indicate that additional agricultural practices taught by BRAC tend to be retained in phased-out villages, suggesting knowledge transfer has "sustainable" effects as well.

Our methodology allows for a clean interpretation of lack of post phase-out impacts when the original intervention is also experimentally designed. However, the original BRAC intervention was not randomized in the same sample in which our own study is conducted (although it was randomized in a different sample, see below). A general challenge for interpreting the results of randomized phase-out when the original intervention was not experimentally implemented is that without other evidence, the absence of post phase-out impacts could be interpreted both as evidence of persistence and as a reflection of the absence of any impacts of the program itself. In this paper we address this challenge with two strategies. First, we compare our data with results from a new experimental evaluation of the BRAC program in a different region in Uganda, which finds significant impacts on improved seeds purchases (among other results). Second, PSM analysis using comparable but never-treated households in the phase-out study region shows significant differences in outcomes corresponding to the BRAC program interventions. Based on these findings, as well as those reported in previous literature, we conclude that the sum of the evidence supports our interpretation that the experimental phase-out results indicate the program had positive impacts that "survive" program termination, in particular on improved seed use, adoption of better basic methods including crop rotation and line-sowing, increased hired labor, and a larger number of crops grown. Ideally, future applications of randomized phase-out evaluation research can be conducted for samples from interventions that are also randomized from the onset. This was not possible in the context of the specific intervention studied here - and is unlikely to be feasible in many such applications - but this does not deter from the merit of the methodology itself; and several of our specific findings on persistence appear robust to this limitation.

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 our experimental phase-out results, on (i) practices, (ii) supply of and (iii) demand for BRAC seeds. Section 5 describes the evidence of initial program impacts through results from a new RCT of the BRAC agriculture program in Southwest Uganda, and from a comparison of initially treated and never treated villages in our randomized phase-out sample; this section also presents further counterfactual sensitivity analysis of sustainability. Section 6 concludes.

2. Overview of the Intervention

BRAC's agriculture program in Uganda was begun in 2009. The program has many features in common with other extension programs[5], and seeks to improve agricultural productivity and incomes, as well as food security of smallholders through increased productivity by promoting the usage of high yielding variety (HYV) seeds and to some extent other inputs, and through improved basic farming methods.

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 by giving free samples of improved seeds in trainings[6]; and stimulating supply by creating local semi-

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[5] For background on agricultural extension and similar programs see Barrett (2002), Anderson and Feder (2007), Barrett et al. (2010), and Davis et al. (2012). There have been a limited number of studies attempting to measure the impact of related agricultural extension programs, among them Owens et al. (2003), Feder et al. (2004), Godtland et al. (2004), Dercon et al. (2009), Davis et al. (2012), and Larsen and Lilleør (2014). See also Evenson (2001). Despite many similarities, a more unusual feature of BRAC's program is that only women can participate.

[6] Previous research in the literature has demonstrated that programs to increase farmer knowledge of basic farming practices, crop choices, and input technologies can matter in principle, because many farmers lack basic information, and benefit from learning in a variety of developing country settings; see e.g. Foster and Rosenzweig (1995), Munshi

informal supply chains within village.

The program, only available to women, consists of two components: the Community Agriculture Promoter (CAP) and the Model Farmer (MF). CAPs serve as an input supplier, selling advanced agricultural inputs in the villages, mainly HYV seeds (Barua, 2011).[7] MFs provide training in agricultural practices to other farmers in the village to raise efficiency.[8] Both CAPs and MFs were selected from female farmers in the village between 25 and 60 years of age who were willing to attend training sessions and meetings. Another qualification for Model Farmers was ownership of a plot of at least 1 acre, which could be used for demonstration purposes. Each season, CAPs and MFs receive week-long training from BRAC staff, who also monitored program implementation throughout the season. Both CAPs and MFs also received a small allowance[9] as reimbursement for travel and other costs of their participation in the training sessions.

Model farmers received six days of training in crop production techniques, adoption of new crop varieties and pest control, and follow-up refresher courses. A requirement for MFs to assume this role was to set up a demonstration plot using learned techniques; each season they received a small compensation of 10 kg of HYV seeds, to be used for demonstration purposes. In turn, the MFs trained about 10 to 12 farmers in the villages each season since the start of program implementation.[10] 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.[11] The MFs are expected

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(2004), Bandiera and Rasul (2006), Conley and Udry (2010).

[7] 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."

[8] Similar peer model farmer programs have been found beneficial in other studies; for example, Krishnan and Patnam (2014) found that learning from neighboring farmers has a longer lasting effect over time than learning from conventional extension agents; and BenYishay and Mobarak (2015) report that "farmers find communicators who face agricultural conditions and constraints most comparable to themselves to be the most persuasive."

[9] In our study period, this was 7000 Ugandan shillings, approximately 2 US dollars.

[10] There are two agricultural (planting through harvesting) seasons per year in the areas in Uganda covered by the program.

[11] BRAC indicated that at least in some smaller villages in which the program had been active since 2009 most if not all farmers who wanted training had already received it at least once.

to demonstrate to general farmers a variety of agricultural practices including crop rotation, line sowing and intercropping; and to explain the benefits of inputs such as improved seed and organic fertilizer. In addition, the MFs were also required to maintain their demonstration plots, and to distribute for free a small quantity of improved seed (provided to them for this purpose by BRAC) during training.

CAPs were able to buy improved maize seed from BRAC at a modest discount, and then sell it to interested farmers in their village. The ability to realize profits on seed sales was meant to create incentives for entrepreneurship based on market principles, increasing the potential for longer-term sustainability.[12] 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, we can think of BRAC's intentionality of sustainability as an effort to establish better equilibria: first, by improving long run farm-level production functions; and second, by overcoming market or coordination failures to establish otherwise profitable missing markets (examined in Section 4.6).[13]

Post-phase-out, CAPs were no longer visited by BRAC staff or offered incentives to participate in the program. However, CAPs did not have to cease their activity as input dealers. To the extent they found the activity worthwhile, they could continue procuring seed from BRAC and reselling it to village farmers. Unlike previously, however, the initiative lies fully with the CAPs themselves and they receive no incentive from BRAC, monetary or otherwise, to continue their activities. Moreover, if CAPs choose to continue their work as intermediaries, the price they charge farmers for improved seeds would be expected to rise, as BRAC no longer discounts its sales of seeds to CAPs. In addition, transport costs are now the CAPs' full responsibility and likely to be passed on to buyers of seed.

One of the aims of the intervention was to increase farmers' access to improved seeds, at which it was apparently successful (for program impact on seed use see Section 5). Improved access

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[12] The average CAPs markup above what they paid to BRAC was apparently quite small; anecdotal evidence suggests that this was possibly due in part to peer pressure by fellow villagers.

[13] 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).

may have occurred through at least three mechanisms. First, as a result of the intervention, the average cost of improved maize seeds would decrease generally, making this input more attractive to farmers.

Second, the CAPs, who were among the main suppliers of seed to the treated villages, were able to offer the seeds at a lower price than other market suppliers, such as input dealers, vendors and general stores, since BRAC sold the seeds to the CAPs at a slight discount (up to 20% relative to market sources).[14]

Third, BRAC transported seeds to the CAPs, so transport costs were reduced - both explicit monetary costs and opportunity costs of time required of farmers to travel to the main trading center to buy inputs.

Farmers in the sample grow more than 30 different crops[15], of which the main crop grown by a majority of the farmers is maize, followed by beans (see Figure I). The BRAC-provided seeds that were distributed by Model Farmers and available for purchase through CAPs are for the most part improved maize seeds, accounting for more than 80% of BRAC's total seed distribution; the rest are vegetable and bean seeds.

[FIGURE I HERE]

3. Experimental Design

BRAC operations are organized within branches; seed distribution and training, as well as other activities, are organized at the branch level. Randomization was therefore stratified by branch. While the program was implemented in a broader area, the experiment in this study is based on a sample of farmers from 15 branches in Eastern Uganda. As noted previously, there are three treatment arms - CAP Phase-out, MF Phase-out, and the Continuation arm. BRAC research staff selected villages to include in the sample used to generate the random phase-out; a criterion was that villages had at least one active component of the program (CAP or MF) at the time the phase-out started. The level of randomization was a village cluster, i.e. a unit in which both of these

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[14] This could put downward pressure on prices charged by other seed distributors as well.

[15] Note that the survey collected data only on each farmer's five most important crops, so the total number of crops across all respondents may in fact be even higher.

components existed. A village with both a CAP and an MF formed a cluster, which was the case for a majority of villages. In other cases, villages with only a single component were combined to form a village cluster if they were no more than 2km away from each other. In total, there are 99 village clusters in the randomized sample: 32 in the Continuation group, 34 in CAP Phase-out and 33 in MF Phase-out (see the map in Appendix B). Out of the 99 clusters, 18 are composed of two villages, while the rest represent a single village. The original sample of farmers in the study consisted of those that had received training from MFs in either of the two seasons prior to the start of the phase-out.

Village clusters that were randomly assigned to either Phase-out group saw a discontinuation of one part of the intervention (MF in the MF Phase-out arm and CAP in the CAP Phase-out arm) in early 2013. One year or two agricultural seasons later, the remaining treatment (CAP in MF Phase-out and MF in CAP Phase-out) was discontinued as well.

Our data includes a pre-phase-out baseline (February 2013) and two follow-up surveys (September 2013 and September 2014). For each of the treatment groups, Table I presents the means of program outcomes and other observable characteristics at pre-phase-out baseline. For the most part, the three experimental treatment arms are balanced, with slight significant differences between them only for line sowing, weeding and organic fertilizer use among the outcome variables (in each case significant for only one of the phaseout groups). Household characteristics generally are not significantly different between groups, with exceptions (for one of the phaseout groups only) in owning two or more sets of clothes, ownership of cows and oxen, and having a formal title to the farmland.[16] Survey attrition rates across the three experimental groups are not significantly related to treatment status[17] and there is no evidence that households who attrited from the panel differed in terms of baseline improved seed use or other observables (see Appendix A, Tables A.I and A.II).

[TABLE I HERE]

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[16] All but one of these are significant only at the 10% level.

[17] The attrition rate is 15.2% after one season and 17.8% after three seasons.

4. Effects of Program Phase-out

In this section we test for the effect of the phase-out of the program six months (one cropping season) as well as a year and a half (i.e., three cropping seasons) after the first component of treatment was discontinued in the two phase-out groups. We have a total of 1124 observations, of which 405 are in the continuation group, 352 in are in the CAP phase-out, and 367 in the MF phase-out.[18]

4.1. Impact of the phase-out on improved seed use

In this section, we examine how both the decreased availability of improved seed through CAPs, and the discontinuation of training by MFs, affected overall improved seed use. Then, we turn to an analysis of how the phasing out of the intervention affected the sources from which the farmers obtained those seeds.

4.1.1. Use of improved seeds

The first question is whether, once the program is phased out, farmers might cease or at least reduce their use of improved seed. The second question is whether farmers change their supplier while nevertheless continuing to use improved seeds; even if CAPs are still operating in the phased out villages, they might no longer be as competitive on the price of improved seed compared with regular agricultural dealers.

We first compare the use of improved seeds in the Continuation villages with that in the two phased-out groups. The estimates in this section are obtained using OLS, with errors clustered at the village cluster level (the village cluster also being the unit at which randomization was conducted). Our outcome variable is a binary indicator of change in whether the farmer made any use of improved seeds (i.e., effect on the extensive margin). We control for relevant covariates that were statistically significantly different at baseline between any of the three groups - namely, line sowing, weeding, use of organic fertilizer and the ownership of a title to the agricultural land used for production (see Table I above).

The results (see Table II.a) show that there is no statistically significant reduction in the use of improved seed as a result of the phase-out, either one season or three seasons after the discontin-

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[18] We also have a separate sample of 697 households who received no treatment, examined in Section 5.

uation of treatment. This result is essentially the same whether the two phase-out arms are pooled together (top panel) or the phase-out groups are examined separately (bottom panel). Analyzing the effect jointly for the two treatment arms enables us to account for potential spillovers between the two treatment types - for example, a still-active CAP in a village which has seen its model farmer phased out might respond differently from a CAP in a village where the MF is also still active. We also present 95% confidence intervals for each estimate.[19]

[TABLE II.a HERE]

Moreover, as seen in Table II.b, at the intensive margin farmers do not reduce the amount of seeds they use, either for overall quantities of seed used or quantity per acre of cultivated land (note that data on quantities of seeds used was not collected as part of the baseline and first follow-up surveys, so the effect of phase-out on quantities is only analyzed at three seasons post-phase-out).

[TABLE II.b HERE]

As a further check, we note that if there had been a reduction in improved seed use due to discontinuation of the intervention, we would expect to see increased use of traditional local seeds. As results in Tables III.a and III.b suggest, the phase-out does not seem to have had a significant positive impact on purchases of local seeds, consistent with our earlier conclusion that improved seed use overall has not dropped in the phase-out groups.

[TABLE III.a HERE]
[TABLE III.b HERE]

4.1.2. Sources of improved seeds

CAPs continue to sell improved seeds in at least 21 of the phased out villages; some 10% of farmers in the sample report having purchased seeds from that source after phase-out. As a result of the phase-out farmers reduce purchases from BRAC sources - CAPs and Model Farmers - decreasing purchases from those sources by around 5 percentage points after one season and 6

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[19] These intervals indicate at most a small decline even at the lower bounds. We return to discuss these intervals in Sections 5.2 and 5.3 where we examine initial program impacts.

percentage points after three seasons (Table IV, column 3).[20] On the one hand, with this declining trend, it remains unclear whether the CAP program is sustainable, even in part. On the other hand, overall phaseout and CAP phaseout declines are statistically significant only at the 10% level; and the magnitude of declines after 3 seasons is about 6 percentage points (or about a 20 percent decline). Future research will be needed to determine whether some CAPs will be able to maintain viable businesses in the longer term.

After phase-out, a significant number of farmers turn to conventional market sources, among which the main ones are input dealers, followed by local markets or large trading centers. However, the replacement of CAPs and MFs with market sources is not immediate. There is a statistically insignificant coefficient on the phase-out dummy in column 1 after one season; but after three seasons the coefficient becomes statistically significant and economically meaningful, with an impact of about six percentage points. This result suggests that there is a lag between discontinuation of the program and farmers connecting to alternative sellers of seed. This is one reason that it takes time to see if important aspects of the program impact were sustainable - it is not just that some practices may hold on longer than others before being dropped. In addition, the transition itself may take time, in this case possibly resulting in a U-shape response as input use falls until the farmer finds a viable alternative source. There is also some difference between treatment arms: among those whose MF program was phased out earlier (MF Phase-out arm) there is a statistically significant switch to conventional market sources (Table IV, column 2); but the positive coefficient on CAP phase-out is not statistically significant.

[TABLE IV HERE]

In addition, after three seasons farmers also turn from CAPs directly to BRAC branch offices for the purchase of seeds - specifically, as a result of the phase-out, the use of that source rises by 2 percentage points (Table IV, column 5). The fact that they choose to buy directly from BRAC instead of buying from a conventional market source could be an indication of the farmers' view of the relative quality of the seeds.

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[20] One reason for the drop may be that the average price at which CAPs sell seeds in phased-out villages is higher than that charged by CAPs in Continuation villages; for further details see Section 4.6.

If we narrow this analysis only to those farmers who used improved seed, then 3 seasons after phase-out we see a stronger and statistically more significant shift away from CAP and Model Farmer sources (-15.4 percentage points), which are replaced instead by market sources (increase by 12.6 percentage points) and, to a lesser extent, other BRAC sources with an increase by 5.1 percentage points.[21] Again, this shift occurs after a lag - while a drop in CAP and Model Farmer sources (-11.7 percentage points) is recorded the first season after phase-out; purchases from market sources do not see a statistically significant increase until the later period.

4.2. Phase-out effects on other inputs

The use of most inputs other than improved seed - hired labor, pesticides, hand or mechanized plows, transport or fuel - do not appear to be negatively and statistically significantly affected by the phase-outs. In Table V the outcome variables are dummies indicating whether or not farmers used a given input. The one negative and significant effect (at the 10% level) on inorganic fertilizer use in the CAP Phase-out group may be explained by the fact that in some villages, CAPs supplied fertilizer in addition to seeds and other inputs (Barua, 2011); and such CAP fertilizer sales may have decreased on both the intensive and extensive margins.[22]

[TABLE V HERE]

4.3. Phase-out effects on cultivation practices

Cultivation practices that were part of the Model Farmers' training program include crop rotation, intercropping, line sowing, zero tillage, weeding, irrigation, pest and disease management, and post-harvest management.[23] The phase-out led to no statistically significant reduction in the degree to which these practices are applied in phase-out and Continuation areas (Table VI).

[TABLE VI HERE]

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[21] Tables omitted for brevity but available from the authors upon request.

[22] The effect on organic fertilizer is similar in magnitude though statistically insignificant

[23] Note that these practices have been demonstrated in general to improve yields in the short run; they also promote conservation in the long run and thus may show added benefits in coming years as climate change adaptations - already used as such by farmers in Ethiopia - become more important (Malik and Smith, 2012). This may be a useful setting for future research on climate change adaptation.

4.4. Phase-out effects on crop yields

In this subsection we examine the potential effects of discontinuation on crop yields.[24]

The phase-out does not seem to have had a negative effect on yields (Table VII); this is consistent with the finding that use of most inputs and cultivation practices do not change in response to phase-out. Overall yields do not respond to the discontinuation of treatment after three seasons.

Maize is both the most commonly grown crop and the seed most widely distributed by BRAC. Examined separately the point estimates imply a modest increase in maize productivity among the combined sample of phase-out groups as compared with the continuation group, though this is only marginally significant at the 10% level. Taken together, we conclude that after three growing seasons there has been no significant effect of the phase-out on farm yields.[25]

[TABLE VII HERE]

Moreover, consistent with the lack of impact on inputs and yields, we found also that the phase-out had no statistically significant effect on overall revenues and profits from agriculture reported by the farmers (details are found in Appendix Table A.III).

4.5. Phase-out effects on crop diversification

Part of BRAC's training focused on teaching farmers about new crops, including vegetables, and techniques such as intercropping.[26] Accordingly, we examine whether discontinuation had any

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[24] Total yield (in kilograms per acre) for each farmer is calculated by adding up data on total output, in kilograms, for all crops grown, and dividing by the total cultivated area for that farmer. For specific crops we analyze, i.e. maize and beans, output of the given crop in kilograms is divided by the number of acres on which that particular crop is grown.

[25] Clearly there has been no statistically significant negative effect on yields thus far. When treatment arms are combined, the point estimate actually indicates a 11% yield increase, although this is significant only at the 10% level. Also, farmers may substitute into other crops from land that was marginal for growing maize, thus raising average maize yields on the remaining maize plots; this is a topic of ongoing research. Further research will consider additional specific crops and may lead us to modify our conclusion or interpretations; at present, we emphasize the potential impact on total yields, and again conclude that after three seasons there was no net effect of the phase-out on farm yields.

[26] Note that, on the consumption side, food variety is often considered an indicator of food security.

effect on the variety of crops farmers grow (Table VIII, columns 1-4). While negative, the coefficients on the phase-out dummies are statistically insignificant indicating no reduction in crop diversification through the third season after extension services were phased-out. Although improved maize seeds were a key component of the program, there is no statistically significant difference in the share of farmers who grow maize in either season between Continuation and Phase-out groups (Table VIII, columns 5-8).

[TABLE VIII HERE]

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

We now turn to examining results from a survey of 76 CAPs. By studying the CAP data we attempt to test whether the program was successful in developing and supporting a sustainable input supply chain that would continue to operate after the program is discontinued. Table IX presents the results of OLS regressions of seed sales three seasons after phase-out on phase-out status with alternative dependent variables: (1) whether or not CAPs continue to sell BRAC seeds, and if so, (2) in what quantities and (3) at what price, using a dummy for CAPs who were phased out as the explanatory variable. While the coefficients largely show that among CAPs who continue to sell seeds, quantities sold fall and prices rise, none are statistically significant. However, the lack of statistical significance may be due in part to the small sample size of CAPs (n=76).

[TABLE IX HERE]

Among the potential causes of the rise in CAP prices after the phase-out, the available data points to transport costs as an important factor: The share of respondents who say that transport costs have caused them not to sell seeds to customers is 31% in the Continuation group and 51% in the phased-out groups, a statistically significant difference.

5. Did the BRAC program have impacts to begin with?

In this section, we present estimates of the impacts of the BRAC agriculture program using two new sources of data: a recent RCT from the Kabale region in Southwest Uganda; and a matched contemporaneous sample in the phaseout region, using data from our most recent household survey round.

5.1. RCT estimates of program impact in Southwest Uganda

In this subsection we report the initial results from a new RCT study of the impact of the BRAC agriculture program in the Kabale region of Southwest Uganda. The sample consists of 230 villages randomly selected into one of four treatment arms.

RCT estimates in this section are obtained from data collected just two seasons after the start of the intervention, while in our data phase-out began up to eight seasons after initial rollout.[27] Table X reports results for all impact variables for which data are available; there is substantial though not exact overlap with questions on our surveys. We provide two sets of comparisons, over a broader and narrower sample frame. In the broader sample in column A, our treatment group consists of households that had potential access either to the agriculture program alone, or the agriculture program plus a microcredit program; the control group consists of the remaining households in the full sample, which includes all those who did not have access to the agriculture program, though they may have been eligible for a standalone BRAC microcredit access program. The narrower sample in column B compares only those who received the agriculture program - omitting any household eligible for BRAC microcredit access in addition to the agriculture program - against a control group who were not eligible for either BRAC program.

Our preferred estimates are from the broader sample, which best represents the way the program works in practice, with some agriculture program participants having access to the credit program and others not; at the same time, irrespective of BRAC credit program availability, overall access to credit is balanced between credit only and credit-plus-agriculture, and between agriculture-only and

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[27] A major purpose of the Southwest Uganda RCT is to examine the marginal benefit of adding a credit program conditional on the presence of the agriculture program, and the marginal benefit of the agriculture program conditional on presence of a credit program; but analysis of that aspect of the experiment is beyond the scope of this paper and may be addressed in some future research.

the pure control. In addition, it allows for more precise estimates with an approximately doubled sample size. On the other hand, it is also informative to isolate the strictly agriculture training and market development activities from credit access activities which affects some of the participants; and results can be compared in Table X.

Results for the broader sample show statistically significant impacts on 7 of the 15 outcome variables: number of acres cultivated; number of crops, purchase of improved seeds, purchased improved seeds from BRAC sources, total production, whether received revenue from crop sales, and crop sales revenues. The narrower sample has statistically significant impacts for purchase of improved seeds, purchased improved seeds from BRAC sources, and crop sales revenues; the signs are also positive and of broadly similar magnitude for the other four variables, but the coefficients drop below significance levels.

The BRAC agriculture program impact on improved seed use is not only significant at the 1% level; it is already quite substantial in magnitude at about 7 percentage points in both broad and narrow household samples, just one year after program implementation. Cash revenue impact is also very substantial in magnitude in both samples at over 29,000 Ugandan shillings. Moreover, point estimates are similar for the other variables for which impacts are statistically significant in the broader sample but not in the narrower sample. Taken as a whole, the RCT estimates of the program already show substantial impacts after a relatively short period of implementation.

[TABLE X HERE]

5.2. The No-Treatment comparison group for the randomized phase-out

The comparison, or No Treatment villages in our sample were chosen from the same BRAC area branches as the village clusters for randomized phase-out, and are similar in terms of agricultural activity, with comparable levels of land ownership, cultivation, and household wealth (see Table XI). Although these villages never received any BRAC agricultural program, they were not randomly chosen prior to the start of the intervention, but only after the program was implemented in other villages, though prior to the start of the randomized phase-out. Thus, we use quasi-experimental techniques to approximate as closely as possible as-good-as random identification.[28] Some of the knowledge imparted to BRAC program villages may have spilled over to our No Treatment comparison villages by the time of our survey, and we have no clean way to overcome this challenge; to the extent of such spillovers, we will underestimate program impact. It is possible that BRAC sought to work in villages where implementation was easier; or that, as its early written materials may be read to imply, "negative selection" may have been at work, with BRAC operating in villages with deeper poverty. We use propensity score matching to address these potential selection problems.

[TABLE XI HERE]

Despite many similarities, there are two observable differences between villages that were part of the program and the comparison (No Treatment) villages. First, BRAC's written policy is to implement the program only in areas within a 6km radius from the branch office. Branch offices are generally located in the main town or trading center of the branch area, which means that the No Treatment group farmers are likely to be more distant on average from markets for inputs as well as for their produce. A map of all the villages and branch offices (given in the Appendix) shows this general pattern, although a number of treated villages are at least as far away from branch offices as No Treatment comparison villages. From the data in Table XI the distance for the No Treatment group is higher than in the Continuation group, by about 2.4 km on average.

Second, a larger fraction of farmers in the treated arms are members of BRAC microfinance groups than in the No Treatment group, which could have implications for their access to credit. That, in turn, may matter for the degree to which farmers are likely to adopt some of the more costly inputs that are part of the intervention - improved seed in particular. There are two sources of data for microfinance membership - self-reported from the survey and BRAC administrative village-level data, which gives information on which villages have a microfinance group, but does not have

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[28] However, these results, particularly when considered together with the previous RDD and D-in-D studies of the program (Pan et al., 2015), provide confidence that we can analyze the randomized phase-out on the basis that the program originally had positive impacts. Again, this is a concern in principle only when no deterioration is found in the phase-out sample.

individual membership lists. In the regression analysis below the latter variable is used[29], but we examined the former specification as a robustness check, with results reported in the Appendix (Table A.IV). The analysis that follows uses both propensity score matching (PSM) and OLS with controls to identify impacts of the original intervention.

In obtaining estimates of initial impact using PSM, we match households based on variables unlikely to have been affected by the intervention - distance to BRAC branch office, farmers' age and educational level (but potentially affecting treatment status). Distance to branch offices is likely to have affected treatment status given that BRAC targeted villages that were closer to those offices; at the same time, the offices are generally located in the main town in each branch, the distance from which might be related to our outcomes of interest. Women's educational level and age are likely to have had some impact on the decision to participate in the program and on outcomes of interest.[30] Propensity scores are estimated using a logit model with errors clustered at the village cluster level; and matching is done within branches.[31]

PSM results shown in the regression tables in this section use nearest-neighbor matching within caliper; but alternative matching specifications were tested as robustness checks (see Appendix, Table C.II). To account for clustering, standard errors are obtained by block bootstrapping at the village cluster level. For both the OLS and PSM models, in each case we compare two specifications. First, we regress outcome variables on a dummy variable (Treated) that assigns a value of 1 to those in any of the three treated groups and a 0 to those in the No Treatment group. Second, we regress outcomes on a dummy for those in the Continuation group, dropping those in the phased-out groups.[32]

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[29] We feature this specification because it is a measure of credit access beyond current use, eliminates risk of misreporting, and accounts for spillovers; but it may also overstate credit access somewhat.

[30] Household asset indicators are not included because they may have been impacted by the program.

[31] See Appendix C for details of the propensity score matching procedure.

[32] These alternatives reflect a tradeoff: The former has a larger sample size and allows for treatment effects on phased-out as well as continuation households; the latter focuses on the program impact without potential dilution from skill "depreciation" among those in villages that were dropped from the program.

5.2.1. Improved seed use

Results in Table XII show the program led to substantial increases in improved seed use.[33] Specifically, farmers in treated groups overall are 14.8 percentage points (column 1) more likely to use improved seeds compared with farmers in the No Treatment group; this is significant at the 99% level. When we compare only No Treatment and Continuation groups (column 2), the results hold; improved seed use is 15.5 percentage points higher. As noted earlier, small quantities of improved seeds were distributed for free to farmers during their training sessions; but the estimates imply a far greater increase than can be explained by farmers receiving these free distributions; far more substantial quantities of improved seeds were available for purchase through CAPs, as well as other sources. PSM results in columns 3 and 4 indicate that a 13 percentage point effect of treatment when all treated groups are combined. Dropping phased-out farmers from the sample, results show a 17 percentage point increase for the Continuation group. The program's persistent success in raising improved seed use is further confirmed by the fact that the phasing out of the treatment, as shown earlier, had no negative effect on the use of these seeds. Specifically, confidence intervals for the phase-out impacts in Table II.a (referenced in Section 4.1.1) have a lower bound of -0.059; the initial impact estimates found in the first four columns of Table XII are substantially greater (in absolute magnitude) than the estimate of maximum decline in improved seed use.

Focusing instead on improved seed purchases excluding farmers who report getting improved seeds for free - a strong test because these farmers may also purchase additional seeds to supplement the small quantity of free distributions - the result is quite similar to that for total improved seed use, with a 13 percentage point increase as a result of the intervention for all treated groups combined (Table XII, column 5), and a 14 percentage point increase for the Continuation subgroup (column 6). While consistent with these results, estimates obtained from propensity score matching are slightly lower when comparing all treated groups (10 percentage points statistically significant at 15%, column 7), and similar to OLS and statistically significant when dropping the phase-out sample and defining the treated as only those in the Continuation group (column 8).

In addition to using data from the most recent survey, that is, three seasons after phase-out (t+3),

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[33] Use of improved seeds is of special importance also because it was stressed in the program; this is strongly associated with improved yields (estimates are reported in Appendix Table A.VI).

we also analyze program impact on improved seed use based on data from pre-phase-out baseline (t-1) and one season after phase-out (t+1), and compare the No Treatment and Continuation groups (results are reported in the Appendix, Table A.V). The effect is strongly statistically significant no matter which season data are used.

These results imply that improved seed use is not related to BRAC microfinance group membership, implying that that particular source of credit does not matter for farmers' ability to purchase this input. This is unsurprising given that the microfinance program provided loans for non-farm microenterprise activities. Distance from BRAC branch office has a small negative effect - an additional kilometer reduces the probability of improved seed use by around one percentage point.

[TABLE XII HERE]

As noted in Section 4.2, changes in improved seed use are likely to be accompanied by changes in the opposite direction in the use of the alternative (local) type of seed, as farmers substitute between the two. Estimates from both OLS with controls and PSM indicate a negative impact of the program on local seed use among treated farmers (see Table XIII, columns 1 and 3 respectively). The upper bound of the phaseout impact for local seeds (0.026, see Table III.a referenced in Section 4.1.1) is significantly smaller than these program impact estimates, suggesting that the decrease in (unimproved) local seed use is persistent. Comparing only the No Treatment and the Continuation groups (columns 2 and 4), however, there is no statistically significant difference in local seed use. Some farmers use a combination of improved seeds on some plots and traditional seeds on others; and farmers may respond to the program by expanding the areas they cultivate or utilizing their cultivated acreage more fully, so that expanded seed use could be additive at least in part.

The effect of distance, in the case of local seeds, is exactly the opposite from improved seeds - the farther away a village is, the more likely farmers are to use local seeds. The magnitude of this effect corresponds to that for improved seeds (around 1 percentage point).

[TABLE XIII HERE]

5.2.2. *Effects on other inputs*

The training also encouraged the use of organic fertilizer (manure), but as seen in Table XIV there is no evidence of impact in this dimension.

[TABLE XIV HERE]

Impacts on some inputs that were not explicitly encouraged in the training are presented in Table XV. OLS results indicate increases in the use of chemical fertilizer, hired labor and use of both hand plows and animals for plowing. Propensity score matching confirms significant increases in hiring labor and hand plow use (14 and 8 percentage point increases, respectively), but not in chemical fertilizer use or plowing with animal power. While the program intended to facilitate sale of fertilizers by CAPs, only a fraction of them did so in practice; we cannot determine whether this was a result of continued low fertilizer demand by farmers, or low supply on the part of the CAPs. Also, the positive impact on hiring labor is substantially larger in magnitude than the lower bound on the estimated phaseout impact (-0.035, see Table V). However, hand plow use has a lower bound of -0.092, which exceeds in magnitude the estimated program impact of 0.0811. This is one of only two cases in which the sustainability interpretation of the phaseout estimates is questionable at the 95 percent confidence level.

[TABLE XV HERE]

5.2.3. Effects on cultivation practices

The Model Farmer component of the extension program included the teaching of modern cultivation practices. As seen in Table VI in the previous section, there are wide differences in the degree to which cultivation practices are applied outside the program areas. For example, weeding was widely applied, at least by the time of the control survey, with more than 90% of farmers reporting weeding their fields, but only 2% of farmers report irrigating[34] their crops.

Based on comparisons with our Continuation and No Treatment groups,[35] the success of the intervention on rates of use of various practices is mixed (see Table XVI). The statistically significant increases in crop rotation and line sowing are robust to estimation method. For crop rotation, the PSM estimate is a 15 percentage point gain; the OLS estimate is a 10 percentage point increase. For

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[34] The most common methods of irrigation in this context are flooding or spraying; this category does not extend to watering individual plants by hand (referred to as “lifting water”).

[35] We are reporting here only comparisons with the Continuation sample to save space. Results for alternative comparisons yield qualitatively similar results and are available from the authors.

for line sowing, the estimates are 18 and 13 percentage points for PSM and OLS and, respectively. Comparing these impact estimates with the lower bounds on phase-out impact estimates given in Table VI (-0.089 for crop rotation and -0.077 for line sowing), we confirm our program persistence interpretation that farmers continue to apply these practices. Other cultivation practices do not seem to have been affected by the program as measured by differences between our Continuation and No Treatment groups.

The program had a positive impact on crop diversification, as seen in Table XVII. The mean value of the number of crops grown by farmers in the No Treatment group is 3.27, while in the Continuation group it is significantly higher - by 0.38 if OLS is used, or 0.47 using PSM.[36] However, these estimates fall within the confidence intervals of phaseout effects on crop diversification given in Table VIII, making it less clear that these program impacts are sustainable.

[TABLE XVI HERE]

[TABLE XVII HERE]

Finally, as reported in Table XVIII, unlike the Southwest Uganda RCT results, in our comparison sample the program effects on total revenues from agriculture and the total value of agricultural production is not statistically significantly different from zero.

[TABLE XVIII HERE]

5.3. Confidence Intervals

As a final check, we compare estimates of initial impact with confidence intervals around the estimated phase-out effect. Figure II presents phase-out estimates (circles) with their confidence intervals (impact estimates three seasons after phase-out), together with program impact estimates using OLS and PSM (red and gray squares, respectively). The latter are depicted as negative values to highlight how large a decrease we would have had to see as a result of phase-out for a complete return to the pre-treatment state. As Figure II shows, for most outcome measures the initial impact estimate does not overlap with the confidence intervals, consistent with the interpretation of persistence or sustainability of improved practices and increased input usages.

[FIGURE II HERE]

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[36] This result closely matches that from the RCT study in reported in Section 5.1.

6. Concluding Remarks

This paper has addressed a basic question for rural development and poverty alleviation: how sustainable are benefits from agricultural extension programs for smallholder women farmers? In doing so it introduced a novel research method, a randomized phase-out (or reverse-randomized control trial), designed to identify the causal impact of the removal of some or all components of an intervention. The context is the phase-out of an agricultural extension program for smallholder women farmers operated in Uganda since 2009 by the NGO BRAC. The program featured two components, broadly targeting farmer knowledge and input market development, particularly for improved maize seeds. BRAC stimulated demand for improved seeds by providing free samples provided in model farmer (MF) trainings. BRAC stimulated supply by appointing and training community agricultural promoters (CAPs), who sold BRAC's improved seeds in villages. MFs taught improved farming practices. Using data collected from a specially constructed control group, and from a new RCT of the BRAC agriculture program from a different part of Uganda, we present evidence that this program had a number of positive impacts; for example, participating smallholders adopted better farming practices.

Due to loss of funding BRAC scheduled this program to be phased-out from early 2013.[37] The sustainability (or persistence) of the program structures, and program impacts, were tested through a randomized phase-out of the program. In early 2013, villages were randomly assigned to one of three arms: to continue in the program (the control group); or to be part of one of two program phase-out groups initially through discontinuation of one or the other component of the intervention (sponsorship of MFs in the MF phase-out arm, and sponsorship of CAPs in the CAP phase-out arm). For the Continuation (control) group the program continued without changes. One year later, the remaining treatments in the phase-out arms (sponsorship of CAPs in the MF phase-out, and sponsorship of MFs in the CAP phase-out) were discontinued. In this way, we examine also whether one of the components, or the sequence of phase-outs, matters more for sustainability.

After three growing seasons improved practices continue: farmers in the phase-out villages showed no statistically significant impacts on the use of crop rotation, intercropping, line sowing, zero tillage, weeding, irrigation, pest and disease management, or post-harvest management.

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[37] Fortunately, limited funding was available for the RCT program in Southwest Uganda and other research activities.

Effects of phase-out on improved seed use are more complex. In phase-out villages fewer CAPs sold seeds; and among CAPs who carried on despite loss of BRAC sponsorship quantities sold fell and sale prices rose. Evidence points to CAPs' post-phase-out transport costs as a key reason for the decline in their activity. On the other hand, purchases from local input dealers rose substantially in the phase-out groups. Moreover, direct purchase of seeds at BRAC area offices also rose modestly, more commonly among farmers who considered these seeds superior.

Results suggest that there is a lag between discontinuation of the program and farmers connecting to alternative sellers of seed. This is one reason why it takes time to determine whether a program has been sustainable - some practices may hold on longer than others before farmers stop using them; and in addition, the transition itself may take time, possibly resulting in a U-shape response as input use falls until a farmer finds a viable alternative source.

Our method provides a straightforward interpretation whenever lack of persistence is identified, even if no estimates of initial impact are available. A general challenge for randomized phase-out experimental design is that absence of post phase-out 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 participation status; we addressed this challenge with two strategies. First, we present results of a new RCT of the BRAC agriculture program from a different part of Uganda. Second, we surveyed households in villages neighboring those in the phaseout area never treated by BRAC that were comparable on observables to our continuation and phase-out samples. In both cases, we found significant differences in agricultural practices that correspond to program interventions.

For the Southwest Uganda RCT, findings include substantial and statistically significant impacts on purchase of improved seeds, crop sales revenues, and number of crops grown, plus some impact on other inputs and outputs after a short period of implementation. For the neighboring village comparison group, results indicate that the intervention was successful in substantially raising the use of improved seeds, while decreasing use of unimproved seeds. Use of hand plows and hired labor also increased significantly, as did practices such as crop rotation and line sowing. Evidence on increased crop diversification is positive but more mixed, possibly due to more general diffusion of these practices in the intervening years.[38] In sum, while impact outcomes were not uniformly significant, taken together these findings support a persistence interpretation of our experimental results for most of the outcomes for which phase-out did not lead to deterioration of practices.

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, sustainability even for high-return activities initiated in NGO and other programs is far from certain (Kremer and Miguel, 2007). Anecdotal reports that impacts prove unsustainable after funding ends is a recurrent theme in discussions of rural development programs. Randomized phase-outs provide a new research strategy to identify effects (such as farming practices and household outcomes) of program phase out and termination.

More generally, a randomized phase-out may be helpful in several circumstances. If an intervention is discontinued entirely, the counterfactual of continuation cannot be observed: 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.[39] For example, when a government budgetary crisis forces the closure of an agricultural extension or other program, if funds can be found to continue to program for a small randomly selected continuation group and for follow up surveys, estimates of the effects can help guide later policy. Similarly, if an NGO has reached a preliminary decision to discontinue a program - due to stringent budget constraints say, or an expectation that a new program would work better (or provide better funding) - a randomized phase-out can be informative for leaders making the decision; or, having decided to terminate a program, it can nonetheless be continued for a minimal number of randomly selected participants whose outcomes are then compared with a randomly sample of those discontinued. A randomized phase-out could also be used to determine the effects of rules that individuals ongoing participation

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[38] We did not find statistically significant impacts on organic fertilizer use, although other research on the program did. Finally, although the RCT study and other prior research found a statistically significant impact on revenues and total agricultural production value, we did not find robust evidence of a significant initial effect on these variables in the comparison group analysis.

[39] This might result from general factors in the wider economy.

in programs be dependent on individual outcome variables.

More expansively, randomization may help estimate impacts of alternative phase-out designs, such as duration and phase-out of program components. The method could reveal program and participant characteristics associated with sustainability of impacts potentially offering insight into targeting design. Randomized phase-out research may clarify tradeoffs from a program sustainability perspective; given budget constraints, often a decision must be made on whether to include more participants in a shorter-duration program or fewer participants in a longer-duration program. Moreover, randomization research on program phase-outs could inform other aspects of new program design, identifying which program components are most vital to sustainability

Funding for program implementers including NGOs may be directed only or primarily to new programs - and perhaps only after NGOs have declared their previous program to be sustainable. The randomized phase-out or “reverse-RCT” approach offers a research method to examine whether this approach makes sense within a given context.

Appendix A. Additional Tables

Table A.I: Attrition by treatment group

	Continuation	**Difference wrt Continuation**	
	Attrition rate	**CAP Phaseout**	**Model Farmer 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: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Standard errors are in parentheses.

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	0.019	0.037	0.013	-0.026	-0.059	0.028
attritors and non-attritors	(0.038)	(0.042)	(0.034)	(0.033)	(0.032)*	(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	-0.038	-0.053	-0.000	0.014	-0.05	0.033
attritors and non-attritors	(0.035)	(0.046)	(0.000)	(0.009)	(0.064)	(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	-0.000	-0.010	0.035	0.034
attritors and non-attritors	(0.011)	(0.007)	(0.030)	(0.034)
N	1529	1658	1560	1534

Note: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Standard errors are in parentheses.

Table A.III: Phase-out effects on revenues and profits, three seasons after phase-out

	Revenues, in UGX				**Profits**, in UGX			
	after 3 seasons		after 1 season		after 3 seasons		after 1 season	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Phaseout	-7668.1		-46987.7		-47377.4		-572043.3	
	(35350.4)		(32056.7)		(64768.9)		(421554.6)	
CAP Phaseout		-49050.2		-51316.5		-46684.3		-584099.2
		(34995.9)		(36068.9)		(78417.7)		(479153.8)
MF Phaseout		30397.2		-42980.5		-48101.5		-559264.9
		(44638.1)		(39494.3)		(65028.0)		(397793.3)
Controls	yes	yes	yes	yes	yes	yes	yes	yes
R^2	0.069	0.074	0.201	0.201	0.036	0.036	0.031	0.031
N	626	626	554	554	970	970	570	570
Mean value in Continuation	419823.3		383712.5		191508		976772.3	

Note: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Regressions are OLS models with standard errors (in parentheses) clustered at the village cluster level. Regressions include branch fixed effects. Controls are binary indicators for the use of agricultural practices that differ at baseline (weeding, line sowing and organic fertilizer use) and land title.

Table A.IV: Program impact estimate on improved seed use using alternative measure of BRAC microfinance group membership

	Improved seed use		**Improved seed purchases**	
	OLS		OLS	
	(1)	(2)	(3)	(4)
Treated	0.1078***		0.0926***	
	(0.0285)		(0.0290)	
Continuation		0.1005**		0.0903*
		(0.0463)		(0.0468)
BRAC microfinance member	0.1837***	0.2254***	0.1708***	0.2223***
	(0.0280)	(0.0370)	(0.0281)	(0.0378)
Distance to BRAC office	-0.0118***	-0.0142***	-0.0095**	-0.0107**
	(0.0039)	(0.0044)	(0.0038)	(0.0043)
R^2	0.209	0.245	0.210	0.242
N	1781	1075	1779	1074
Mean value in No Treatment	0.240		0.227	

Note: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Standard errors (in parentheses) are clustered at the village cluster level. Dependent variables are binary indicators of seed use or purchases. OLS 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).

Table A.V: Impact on improved seed use, by season

	Improved seed use		
	Pre-phaseout - t-1	Post-phaseout - t+1	Post-phaseout - t+3
	(1)	(2)	(3)
Continuation	0.160**	0.127**	0.155***
	(0.0653)	(0.0544)	(0.0498)
BRAC microfinance membership	-0.0907*	-0.0542	-0.0189
	(0.0526)	(0.0537)	(0.0420)
Distance to BRAC office	-0.0194***	-0.00837	-0.0162***
	(0.00659)	(0.00613)	(0.00488)
R^2	0.257	0.164	0.221
N	1006	1003	1079

Note: *** p<0.01, ** p<0.05, * p<0.1. Regressions are OLS models with standard errors (in parentheses) clustered at the village cluster level. Dependent variables are binary indicators of seed use. 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).

Table A.VI: Agricultural production function

	Total yield, kg per acre	**Maize yield, kg per acre**
	log	log
	(1)	(2)
Treatment dummy	-0.0253	0.0119
	(0.0257)	(0.0199)
Cultivated land, own	-0.0963***	
in acres	(0.0201)	
Cultivated land, rented	-0.156***	
in acres	(0.0316)	
Cultivated land, total#		-0.00498
in acres		(0.0159)
Dummy variables:		
Improved seed	0.155**	0.153***
	(0.0642)	(0.0521)
Organic fertilizer	0.145*	-0.102
	(0.0796)	(0.0762)
Chemical fertilizer	0.0878	0.161**
	(0.0916)	(0.0656)
Pesticide, herbicide	0.0876	0.103**
	(0.0661)	(0.0519)
Hired labor	0.0772	0.0613
	(0.0546)	(0.0481)
Animals for plowing	0.206**	0.235***
	(0.0928)	(0.0687)

Table A.VI – *Continued from previous page*

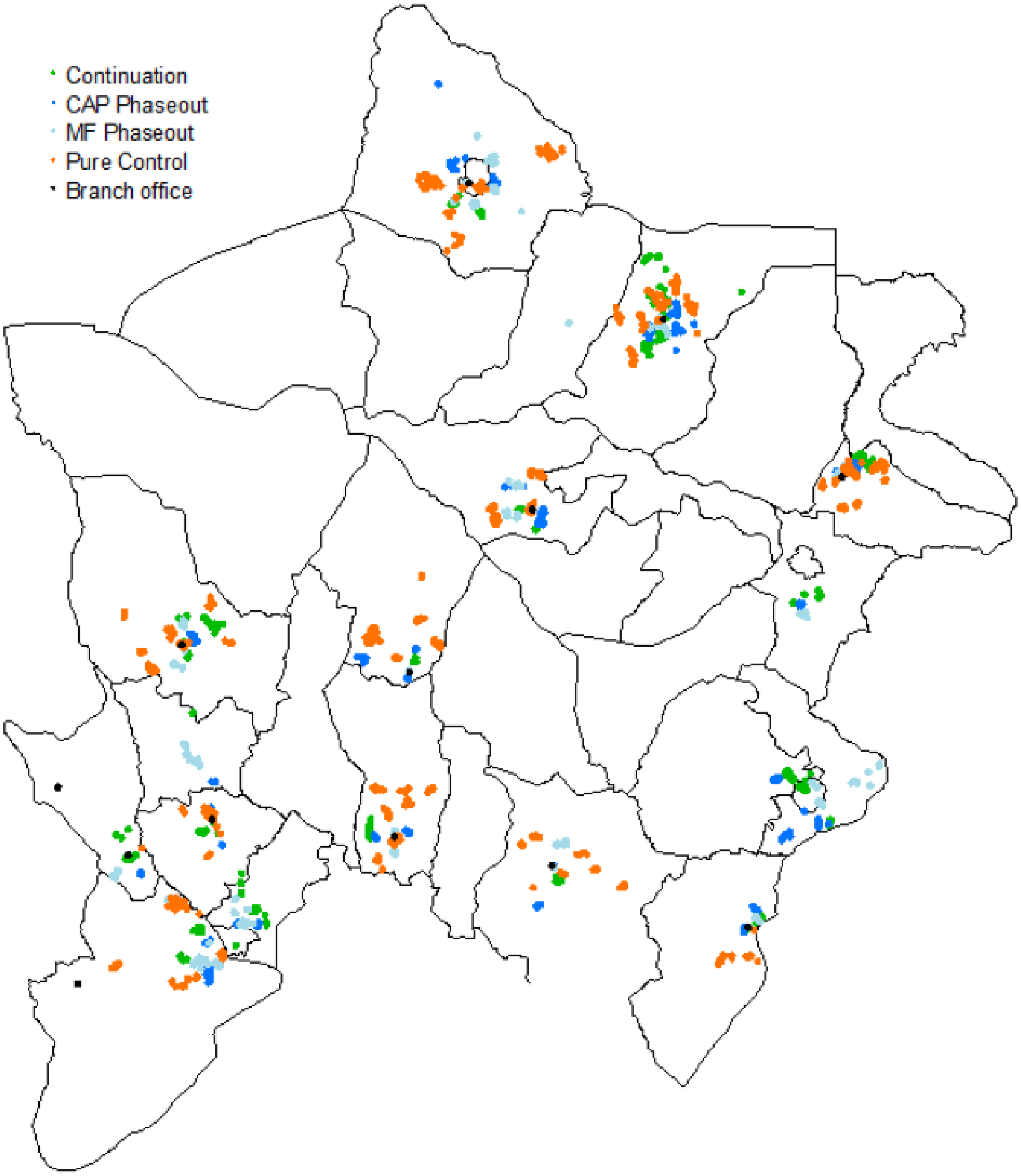
	Total yield, kg per acre	**Maize yield, kg per acre**
	log	log
	(1)	(2)
Hand plow	-0.0116	-0.189**
	(0.102)	(0.0884)
Mechanized plow	0.148	0.578***
	(0.127)	(0.121)
Crop rotation	0.304***	0.0910*
	(0.0603)	(0.0468)
Intercropping	0.0813	-0.181***
	(0.0598)	(0.0578)
Mixed cropping	0.0377	-0.0931
	(0.0610)	(0.0564)
Line sowing	0.180**	0.0233
	(0.0732)	(0.0650)
Weeding	-0.107	0.0280
	(0.103)	(0.0904)
Irrigation	0.0305	0.0314
	(0.153)	(0.160)
Education level	0.00136	0.00204
	(0.00625)	(0.00553)
R^2	0.217	0.204
N	1355	1305

Note: *** p<0.01, ** p<0.05, * p<0.1. Regressions are OLS models with standard errors (in parentheses) clustered at the village cluster level. Includes branch fixed effects

Data on own vs. rented land was only gathered for the plot size overall, but not for cultivation areas of individual crops.

Total yield (kg per acre) for each farmer is calculated using information on output of all crops grown (in kilograms) and the total cultivated area; maize yield is calculated as total maize output in kg divided by the area used for maize cultivation.

Appendix B. Map of treated and untreated households - Eastern District, Uganda



Appendix C. Propensity score matching

Figure C.I: Propensity score distribution

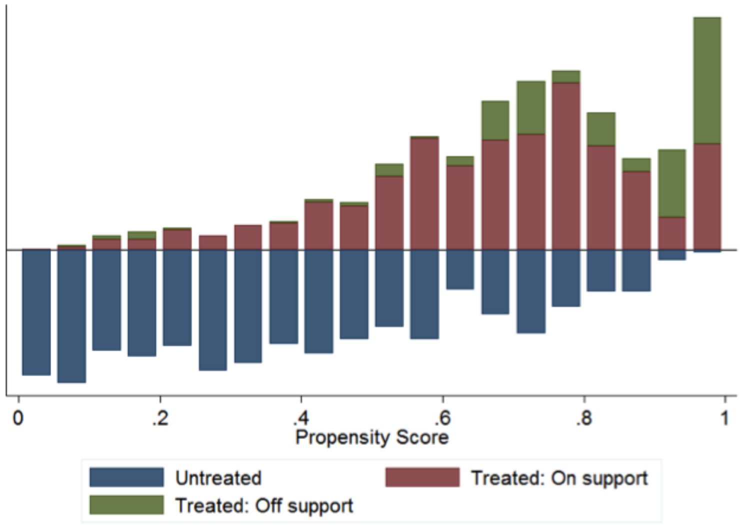


Table C.I: Sample comparison pre- and post-matching - improved seed use

Sample	Pseudo R^2	LR chi^2	p $>chi^2$	MeanBias	MedBias	B	R	%Var
Unmatched	0.110	226.26	0	34.5	18.9	80.5*	0.47*	67
Matched	0.003	5.27	0.153	5.6	3.8	12.7	0.96	67

Table C.II: Alternative PSM methods - improved seed use (No Treatment vs. all treated groups)

Matching method	Caliper=0.01	Caliper=0.025	Caliper=0.05	Radius with caliper = 0.025	Kernel bandwidth=0.05
Estimated coefficient	0.1266**	0.1253**	0.1170**	0.1237**	0.1146**
SE	(0.0577)	(0.0627)	(0.0564)	(0.0615)	(0.0514)

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Tables

Table I: Balance of Sample Characteristics at Pre-Phase-out Baseline, by Treatment Group

	(1) Continuation	(2) Combined Phase Out	(3) Columns (1) - (2)	(4) CAP Phase Out	(5) Columns (1) - (4)	(6) MF Phase Out	(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	0.0239	0.005
	(0.021)	(0.016)	(0.027)	(0.024)	(0.032)	(0.022)	(0.031)
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)
Mixed cropping	0.335	0.345	-0.010	0.376	-0.041	0.314	0.021
	(0.024)	(0.018)	(0.030)	(0.026)	(0.035)	(0.024)	(0.034)
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)
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)

Table I – *Continued from previous page*

	(1) **Continuation**	**(2)** **Combined Phase Out**	**(3)** **Columns (1) - (2)**	**(4)** **CAP Phase Out**	**(5)** **Columns (1) - (4)**	**(6)** **MF Phase Out**	**(7)** **Columns (1) - (6)**
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: Standard errors are in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table II.a: Phase-out effect on improved seed use, binary indicator

	Improved seed use - after 3 seasons			**Improved seed use - after 1 season**		
	(1)	(2)	(3)	(4)	(5)	(6)
Phaseout combined	0.0106	0.0082	0.0207	0.0157	0.0148	0.0083
	(0.0341)	(0.0331)	(0.0336)	(0.0363)	(0.0364)	(0.0334)
95% CI	[-0.0593 0.0757]	[-0.0575 0.0739]	[-0.0460 0.0874]	[-0.0589 0.0858]	[-0.0575 0.0871]	[-0.0581 0.0746]
Controls - baseline values						
Improved seed use		0.1442***	0.1336***		0.0286	0.0138
		(0.0357)	(0.0358)		(0.0363)	(0.0369)
Line sowing			0.0513			0.1010***
			(0.0428)			(0.0319)
Weeding			-0.0314			0.1101**
			(0.0553)			(0.0545)
Organic fertilizer			0.1291**			-0.0406
			(0.0535)			(0.0403)
Land title			-0.0058			-0.0426
			(0.0361)			(0.0326)
R^2	0.160	0.176	0.174	0.167	0.167	0.180
N	1118	1105	1032	1123	1110	1038
Mean value in Continuation		0.386			0.427	

	Improved seed use - after 3 seasons			**Improved seed use - after 1 season**		
	(1)	(2)	(3)	(4)	(5)	(6)
CAP Phaseout	-0.0125	-0.0152	-0.0101	0.0140	0.0130	0.0072
	(0.0379)	(0.0378)	(0.0390)	(0.0389)	(0.0390)	(0.0370)
95% CI	[-0.0878 0.0628]	[-0.0902 0.0599]	[-0.0875 0.0673]	[-0.0631 0.0912]	[-0.0643 0.0903]	[-0.0662 0.0806]
MF Phaseout	0.0287	0.0313	0.0524	0.0128	0.0165	0.0094
	(0.0418)	(0.0400)	(0.0391)	(0.0423)	(0.0422)	(0.0389)
95% CI	[-0.0542 0.1117]	[-0.0481 0.1106]	[-0.0253 0.1300]	[-0.0711 0.0967]	[-0.0673 0.1004]	[-0.0678 0.0865]
Controls - baseline values						
Improved seed use		0.1450***	0.1341***		0.0287	0.0139
		(0.0360)	(0.0360)		(0.0363)	(0.0369)
Line sowing			0.0486			0.1010***
			(0.0428)			(0.0317)
Weeding			-0.0237			0.1103**
			(0.0533)			(0.0542)
Organic fertilizer			0.1299**			-0.0406
			(0.0534)			(0.0404)
Land title			-0.0068			-0.0426
			(0.0359)			(0.0325)
R^2	0.161	0.177	0.177	0.167	0.167	0.180
N	1118	1105	1032	1123	1110	1037
Mean value in Continuation		0.386			0.427	

Note:*** $p<0.01$, ** $p<0.05$, * $p<0.1$. Regressions are OLS models with standard errors (in parentheses) clustered at the village cluster level. Regressions include branch fixed effects. All variables are binary indicators.

Table II.b: Phase-out effect on improved seed use, quantities

	Improved seed quantities, total - after 3 seasons		**Improved seed quantities, per acre - after 3 seasons**	
	(1)	(2)	(3)	(4)
Phaseout combined	0.3655	0.5304	0.3761	0.2890
	(0.5087)	(0.5033)	(0.2509)	(0.2267)
95% CI	[-0.6440 1.3751]	[-0.4684 1.5291]	[-0.1219 0.8741]	[-0.1609 0.7389]
Controls - baseline values				
Line sowing		0.1907		-0.1590
		(0.9543)		(0.4242)
Weeding		0.0203		0.0952
		(0.9788)		(0.4021)
Organic fertilizer		1.4803*		0.3942
		(0.8114)		(0.3688)
Land title		1.2140*		0.3825
		(0.6617)		(0.3364)
R^2	0.084	0.092	0.073	0.085
N	1113	1029	1078	998
Mean value in Continuation	3.48		1.85	

	Improved seed quantities, total - after 3 seasons		**Improved seed quantities, per acre - after 3 seasons**	
	(1)	(2)	(3)	(4)
CAP Phaseout	0.0626	0.1834	-0.0155	-0.0725
	(0.5250)	(0.5550)	(0.2355)	(0.2331)
95% CI	[-0.9792 1.1044]	[-0.9179 1.2847]	[-0.4828 0.4518]	[-0.5350 0.3901]
Model Farmer Phaseout	0.6402	0.8879	0.7797**	0.6720*
	(0.7019)	(0.6785)	(0.3682)	(0.3111)
95% CI	[-0.7256 2.0600]	[-0.4586 2.2344]	[0.0489 1.5105]	[0.0546 1.2895]
Controls - baseline values				
Line sowing		0.1623		-0.1919
		(0.9615)		(0.4184)
Weeding		0.1087		0.1877
		(0.9567)		(0.3778)
Organic fertilizer		1.4874*		0.4057
		(0.8127)		(0.3600)
Land title		1.2043*		0.3730
		(0.6590)		(0.3353)
R^2	0.085	0.093	0.078	0.090
N	1113	1029	1078	998
Mean value in Continuation	3.48		1.85	

Note:*** $p<0.01$, ** $p<0.05$, * $p<0.1$. Regressions are OLS models with standard errors (in parentheses) clustered at the village cluster level. Regressions include branch fixed effects. Quantities of improved seeds are in kilograms; all other variables are binary indicators.

Table III.a: Phase-out effect on local seed use, binary indicator

	Local seed use - after 3 seasons			**Local seed use - after 1 season**		
	(1)	(2)	(3)	(4)	(5)	(6)
Phaseout combined	-0.0249	-0.0279	-0.0258	-0.0228	-0.0207	-0.0158
	(0.0258)	(0.0259)	(0.0260)	(0.0224)	(0.0223)	(0.0227)
95% CI	[-0.0761 0.0262]	[-0.0792 0.0234]	[-0.0774 0.0258]	[-0.0672 0.0217]	[-0.0650 0.0235]	[-0.0609 0.0292]
Controls - baseline values						
Local seed use		-0.0344	-0.0607**		0.0224	0.0065
		(0.0271)	(0.0294)		(0.0322)	(0.0343)
Line sowing			0.0015			0.0038
			(0.0254)			(0.0253)
Weeding			0.0045			0.0211
			(0.0385)			(0.0320)
Organic fertilizer			0.0447			0.0056
			(0.0366)			(0.0305)
Land title			-0.0456			0.0041
			(0.0275)			(0.0216)
R^2	0.071	0.073	0.090	0.184	0.185	0.189
N	1116	1116	1031	1123	1123	1038
Mean value in Continuation		0.878			0.869	

	Local seed use - after 3 seasons			**Local seed use - after 1 season**		
	(1)	(2)	(3)	(4)	(5)	(6)
CAP Phaseout	0.0027	0.0002	-0.0043	-0.0319	-0.0303	-0.0319
	(0.0265)	(0.0264)	(0.0277)	(0.0270)	(0.0267)	(0.0279)
95% CI	[-0.0498 0.0552]	[-0.0521 0.0525]	[-0.0592 0.0506]	[-0.0855 0.0216]	[-0.0833 0.0227]	[-0.0873 0.0235]
MF Phaseout	-0.0523	-0.0563*	-0.0480	-0.0136	-0.0110	0.0008
	(0.0319)	(0.0322)	(0.0320)	(0.0247)	(0.0247)	(0.0257)
95% CI	[-0.1155 0.0110]	[-0.1201 0.0076]	[-0.1116 0.0156]	[-0.0626 0.0353]	[-0.0560 0.0379]	[-0.0501 0.0518]
Controls - baseline values						
Local seed use		-0.0366	-0.0620**		0.0237	0.0075
		(0.0262)	(0.0287)		(0.0323)	(0.0344)
Line sowing			0.0035			0.0025
			(0.0251)			(0.0249)
Weeding			-0.0007			0.0250
			(0.0400)			(0.0330)
Organic fertilizer			0.0442			0.0058
			(0.0365)			(0.0301)
Land title			-0.0449			0.0037
			(0.0276)			(0.0215)
R^2	0.075	0.077	0.093	0.185	0.185	0.190
N	1116	1132	1031	1123	1123	1038
Mean value in Continuation		0.878			0.869	

Note:*** $p<0.01$, ** $p<0.05$, * $p<0.1$. Regressions are OLS models with standard errors (in parentheses) clustered at the village cluster level. Regressions include branch fixed effects. All variables are binary indicators.

Table III.b: Phase-out effect on local seed use, quantities

	Local seed quantities, total - after 3 seasons		Local seed quantities, per acre - after 3 seasons	
	(1)	(2)	(3)	(4)
Phaseout combined	0.6441	1.0340	-0.8387	-0.8154
	(1.7459)	(1.7809)	(1.2050)	(1.2825)
95% CI	[-2.8206 4.1088]	[-2.5002 4.5681]	[-3.2302 1.5528]	[-3.3608 1.7301]
Controls - baseline values				
Line sowing		3.8275*		-0.0166
		(2.0307)		(1.0877)
Weeding		0.7699		2.263
		(2.6660))		(2.417)
Organic fertilizer		2.1712		-2.511**
		(2.2742)		(1.1709)
Land title		-0.1989		-0.3936
		(1.9744)		(1.4454)
R^2	0.102	0.119	0.073	0.076
N	1092	1009	1057	978
Mean value in Continuation	17.5		11.9	

	Local seed quantities, total - after 3 seasons		Local seed quantities, per acre - after 3 seasons	
	(1)	(2)	(3)	(4)
CAP Phaseout	1.7731	2.0567	-0.4434	-0.5613
	(1.9359)	(2.0496)	(1.4584)	(1.5440)
95% CI	[-2.0687 5.6148]	[-2.0106 6.1241]	[-3.3379 2.4510]	[-3.6257 2.5030]
Model Farmer Phaseout	-0.4847	-0.0297	-1.2484	-1.0874
	(2.0709)	(2.1304)	(1.3284)	(1.4731)
95% CI	[-4.5943 3.6250]	[-4.25746 4.1980]	[-3.8850 1.3881]	[-4.0110 1.8363]
Controls - baseline values				
Line sowing		3.9213*		0.0093
		(2.0469)		(1.1034)
Weeding		0.4821		2.1902
		(2.8093)		(2.5036)
Organic fertilizer		2.1380		-2.5228**
		(2.2273)		(1.1678)
Land title		-0.1778		-0.3892
		(1.9777)		(1.4501)
R^2	0.103	0.12	0.073	0.076
N	1092	1009	1057	978
Mean value in Continuation	17.5		11.9	

Note:*** $p<0.01$, ** $p<0.05$, * $p<0.1$. Regressions are OLS models with standard errors (in parentheses) clustered at the village cluster level. Regressions include branch fixed effects. Quantities of local seeds are in kilograms; all other variables are binary indicators.

Table IV: Phase-out effect on sources of improved seed

3 seasons after phaseout

	Market sources		**CAP and Model Farmer**		**Other BRAC sources**	
	(yes/no)		(yes/no)		(yes/no)	
	(1)	(2)	(3)	(4)	(5)	(6)
Phaseout combined	0.0572*		-0.0550*		0.0174***	
	(0.0343)		(0.0303)		(0.0065)	
CAP Phaseout		0.0305		-0.0344*		0.0136
		(0.0394)		(0.0321)		(0.0095)
Model Farmer Phaseout		0.0847**		-0.0762**		0.0234**
		(0.0388)		(0.0326)		(0.0093)
Controls	yes	yes	yes	yes	yes	yes
R^2	0.15	0.152	0.110	0.115	0.029	0.037
N	1032	1032	1032	1032	1032	1032
Mean value in Continuation	0.256		0.102		0.002	

1 season after phaseout

	Market sources		**CAP and Model Farmer**		**Other BRAC sources**	
	(yes/no)		(yes/no)		(yes/no)	
	(1)	(2)	(3)	(4)	(5)	(6)
Phaseout combined	0.0207		-0.0484*		0.0120*	
	(0.0288)		(0.0258)		(0.0068)	
CAP Phaseout		0.0288		-0.0461*		0.0062
		(0.0336)		(0.0267)		(0.0079)
Model Farmer Phaseout		0.0124		-0.0509*		0.0180*
		(0.0345)		(0.0301)		(0.0095)
Controls	yes	yes	yes	yes	yes	yes
R^2	0.172	0.172	0.077	0.077	0.069	0.071
N	1037	1037	1037	1037	1015	1015
Mean value in Continuation	0.271		0.096		0.005	

Note: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Regressions are OLS models with standard errors (in parentheses) clustered at the village cluster level. Regressions include branch fixed effects. Dependent variables are binary indicators of whether farmers used seeds from market, CAP/Model farmer or Other BRAC sources; independent variables are binary indicators of treatment status. Controls are binary indicators for outcome at pre-phase-out baseline, the use of agricultural practices that differ at baseline (weeding, line sowing and organic fertilizer use) and land title.

Table V: Phase-out effect on other inputs and tools, binary indicator - 3 seasons after phase-out

	Hired labor	**Organic fertilizer**	**Inorganic fertilizer**	**Pesticide**	**Hand plow**	**Mechanized plow**	**Fuel**	**Transport**
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Phaseout combined	0.0321	-0.0261	-0.0242	0.0298	-0.0322	0.0208**	0.0047	0.0105
	(0.0339)	(0.0252)	(0.0226)	(0.0275)	(0.0302)	(0.0096)	(0.0068)	(0.0306)
95% CI	[-0.0353 0.0995]	[-0.0760 0.0238]	[-0.0692 0.0207]	[-0.0248 0.0845]	[-0.0921 0.0277]	[0.0017 0.0398]	[-0.0087 0.0182]	[-0.0502 0.0712]
Controls	yes	yes	yes	yes	yes	yes	yes	yes
R^2	0.102	0.119	0.098	0.103	0.653	0.669	0.028	0.139
N	1033	1030	1029	1030	1007	1003	999	1008
Mean value in Continuation	0.602	0.145	0.120	0.211	0.344	0.055	0.010	0.244
CAP Phaseout	0.0309	-0.0444	-0.0417*	0.0054	-0.0263	0.0207*	-0.0027	0.0223
	(0.0411)	(0.0271)	(0.0242)	(0.0337)	(0.0335)	(0.0106)	(0.0059)	(0.0344)
95% CI	[-0.0507 0.1124]	[-0.0981 .0093]	[-0.0897 0.0062]	[-0.0616 0.0723]	[-0.0928 0.0402]	[-0.0004 0.0418]	[-0.0143 0.0089]	[-0.0460 0.0907]
MF Phaseout	0.0334	-0.0073	-0.0060	0.0550	-0.0384	0.0208*	0.0127	-0.0019
	(0.0369)	(0.0325)	(0.0272)	(0.0336)	(0.0361)	(0.0113)	(0.098)	(0.0340)
95% CI	[-0.0398 0.1066]	[-0.0718 0.0571]	[-0.0599 0.0479]	[-0.0117 0.1217]	[-0.1099 0.0332]	[-0.0016 0.0432]	[-0.0069 0.0322]	[-0.0695 0.0657]
Controls	yes	yes	yes	yes	yes	yes	yes	yes
R^2	0.102	0.120	0.100	0.105	0.653	0.669	0.031	0.140
N	1033	1030	1029	1030	1007	1003	999	1008
Mean value in Continuation	0.602	0.145	0.120	0.211	0.344	0.055	0.010	0.244

Note: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Regressions are OLS models with standard errors (in parentheses) clustered at the village cluster level. Regressions include branch fixed effects. Controls are binary indicators for outcome at pre-phase-out baseline, the use of agricultural practices that differ at baseline (weeding, line sowing and organic fertilizer use) and land title.

Table VI: Phase-out effect on cultivation practices, binary indicator - 3 seasons after phase-out

	Crop rotation (1)	**Intercropping** (2)	**Line sowing** (3)	**Irrigation** (4)	**Proper weeding** (5)	**Zero tillage** (6)	**Pest and disease management** (7)	**Post-harvest management** (8)
Phaseout combined	-0.0164	-0.0188	-0.0108	0.0069	-0.0046	-0.0142	0.0585*	0.0219
	(0.0368)	(0.0236)	(0.0335)	(0.0067)	(0.0180)	(0.0120)	(0.0299)	(0.0329)
95% CI	[-0.0894 0.0566]	[-0.0656 0.0281]	[-0.0773 0.0558]	[-0.00640 0.0202]	[-0.0404 0.0311]	[-0.0380 0.0097]	[-0.0008 0.11770]	[-0.0433 0.0871]
Controls	yes	yes	yes	yes	yes	yes	yes	yes
R^2	0.118	0.342	0.259	0.041	0.044	0.164	0.103	0.232
N	1029	1029	1031	1030	1033	1028	1021	1027
Mean value in Continuation	0.685	0.594	0.730	0.018	0.938	0.060	0.214	0.609
CAP Phaseout	0.0165	-0.0084	0.0063	0.0101	-0.0014	-0.0242	0.0253	0.0616*
	(0.0423)	(0.0257)	(0.0343)	(0.0080)	(0.0203)	(0.0131)	(0.0371)	(0.0324)
95% CI	[-0.0676 0.1005]	[-0.0594 0.0426]	[-0.0617 0.0743]	[-0.0057 0.0259]	[-0.0418 0.0390]	[-0.0501 0.0017]	[-0.0484 0.0990]	[-0.0026 0.1259]
MF Phaseout	-0.0504	-0.0294	-0.0284	0.0031	-0.0079	-0.0038	0.0927**	-0.0193
	(0.0450)	(0.0313)	(0.0437)	(0.0079)	(0.0215)	(0.0134)	(0.0355)	(0.0426)
95% CI	[-0.1397 0.0389]	[-0.0915 0.0326]	[-0.1152 0.0584]	[-0.0121 0.0192]	[-0.0506 0.0347]	[-0.0304 0.0229]	[0.0222 0.1632]	[-0.1038 0.0652]
Controls	yes	yes	yes	yes	yes	yes	yes	yes
R^2	0.121	0.342	0.260	0.043	0.044	0.165	0.106	0.236
N	1029	1029	1031	1030	1033	1028	1021	1027
Mean value in Continuation	0.685	0.594	0.730	0.018	0.938	0.060	0.214	0.609

Note: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Regressions are OLS models with standard errors (in parentheses) clustered at the village cluster level. Regressions include branch fixed effects. Controls are binary indicators for outcome at pre-phase-out baseline, the use of agricultural practices that differ at baseline (weeding, line sowing and organic fertilizer use) and land title.

Table VII: Phase-out effects on crop yields

	Maize yield		**Overall yield**		**Maize yield**		**Overall yield**	
	log kg		log kg		log kg		log kg	
	after 3 seasons				**after 1 season**			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Phaseout combined	0.1069*		-0.0051		0.0192		-0.0754	
	(0.0642)		(0.0620)		(0.0688)		(0.0812)	
95% CI	[-0.0206 0.2344]		[-0.1282 0.1180]		[-0.1174 0.1557]		[-0.2365 0.0858]	
CAP Phaseout		0.1147		0.0297		0.0083		-0.1567
		(0.0728)		(0.0664)		(0.0909)		(0.0940)
95% CI		[-0.0298 0.2593]		[-0.1022 0.1615]		[-0.1720 0.1887]		[-0.3431 0.0298]
MF Phaseout		0.0978		-0.0445		0.0303		-0.0052
		(0.0747)		(0.0801)		(0.0799)		(0.0957)
95% CI		[-0.0505 0.2461]		[-0.2035 0.1146]		[-0.1283 0.1889]		[-0.1846 0.1951]
Controls	yes	yes	yes	yes	yes	yes	yes	yes
R^2	0.149	0.149	0.114	0.115	0.203	0.203	0.316	0.320
N	706	706	807	807	773	773	819	819
Mean value in Continuation	5.608		5.761		5.198		4.669	

Note: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Regressions are OLS models with standard errors (in parentheses) clustered at the village cluster level. Regressions include branch fixed effects. Controls are binary indicators for outcome at pre-phase-out baseline, the use of agricultural practices that differ at baseline (weeding, line sowing and organic fertilizer use) and land title. Dependent variables are in logarithms. Maize yield is calculated as kilograms of maize per acre of land on which maize is cultivated; total yields are calculated as the total output from all crops in kilograms per acre of total cultivated land.

Table VIII: Phase-out effect on crop diversification

	Number of crops				Cultivated maize			
					(yes/no)		(yes/no)	
	after 3 seasons		after 1 season		after 3 seasons		after 1 season	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Phaseout combined	-0.1191		-0.1021		0.0057		-0.0349	
	(0.1888)		(0.3416)		(0.0242)		(0.0242)	
95% CI	[-0.4938 0.2556]		[-0.7801 0.5760]		[-0.0431 0.0534]		[-0.0629 0.0316]	
CAP Phaseout		-0.1006		0.0197		0.0135		-0.0283
		(0.2497)		(0.03623)		(0.0257)		(0.0240)
95% CI		[-0.5960 0.3949]		[-0.6995 0.7388]		[-0.0385 0.06410]		[-0.0591 0.0496]
MF Phaseout		-0.1383		-0.2416		-0.0023		-0.0417
		(0.2024)		(0.4109)		(0.0324)		(0.0323)
95% CI		[-0.5400 0.2635]		[-1.0571 0.5738]		[-0.0669 0.0613]		[-0.0862 0.0324]
Controls	yes	yes	yes	yes	yes	yes	yes	yes
R^2	0.064	0.064	0.272	0.273	0.202	0.203	0.194	0.195
N	1051	1051	857	857	1034	1034	1038	1038
Mean value in Continuation	3.569		4.485		0.881		0.753	

Note: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Regressions are OLS models with standard errors (in parentheses) clustered at the village cluster level. Regressions include branch fixed effects. Controls are binary indicators for outcome at pre-phase-out baseline, the use of agricultural practices that differ at baseline (weeding, line sowing and organic fertilizer use) and land title.

Table IX: Phase-out effects on CAP seed sales

	Sale of BRAC seed		**Maize seed sold - quantity**		**Maize seed sold - price**	
	dummy		log kg		log UGX/kg	
	(1)	(2)	(3)	(4)	(5)	(6)
Phaseout combined	0.0329		-0.218		0.124	
	(0.101)		(0.534)		(0.154)	
CAP Phaseout		0.0525		-0.408		-0.0604
		(0.130)		(0.676)		(0.174)
MF Phaseout		0.0125		-0.0199		0.289
		(0.113)		(0.588)		(0.228)
R^2	0.358	0.359	0.661	0.670	0.533	0.591
N	76	76	34	34	34	34
Mean value in Continuation	0.436		4.081		7.945	

Note: *** p<0.01, ** p<0.05, * p<0.1. Regressions are OLS models with standard errors (in parentheses) clustered at the village cluster level. Includes branch fixed effects.

Table X: Program impact estimates from RCT in Kabale, SW Uganda

		A **Ag only + Ag and MF vs. MF + None**	B **Ag only vs. None**
Number of acres cultivated	Coefficient	0.090	0.062
	SE	(0.047)*	(0.071)
	N	6,007	3,049
Number of crops produced	Coefficient	0.205	0.134
	SE	(0.100)**	(0.145)
	N	6,230	3,156
Purchased any improved seeds	Coefficient	0.061	0.077
	SE	(0.013)***	(0.018)***
	N	6,105	3,094
Purchased seeds from BRAC sources	Coefficient	0.062	0.066
	SE	(0.010)***	(0.014)***
	N	6,105	3,094
Adopted crop rotation	Coefficient	0.022	-0.009
	SE	(0.025)	(0.036)
	N	6,229	3,156
Adopted inter cropping	Coefficient	-0.008	-0.005
	SE	(0.005)	(0.008)
	N	6,226	3,154
Adopted line sowing	Coefficient	0.012	0.026
	SE	(0.014)	(0.020)
	N	6,227	3,153
Adopted proper weeding	Coefficient	0.005	-0.008
	SE	(0.018)	(0.025)
	N	6,229	3,156

Table X – *Continued from previous page*

		A **Ag only + Ag and MF** **vs. MF + None**	B **Ag only** **vs. None**
Spent money on pesticide	Coefficient	0.008	0.003
	SE	(0.010)	(0.015)
	N	6,230	3,156
Spent money on manure	Coefficient	-0.001	-0.001
	SE	(0.001)	(0.001)
	N	6,230	3,156
Spent money on hiring labour	Coefficient	0.008	0.014
	SE	(0.017)	(0.026)
	N	6,230	3,156
Total agriculture production	Coefficient	81.143	50.304
(in ’000 UGX)	SE	(31.596)**	(46.556)
	N	6,167	3,126
Production per acre	Coefficient	17.941	7.113
(in ’000 UGX)	SE	(16.938)	(23.643)
	N	5,725	2,915
Received cash revenue	Coefficient	0.060	0.060
from crop sales	SE	(0.025)**	(0.037)
	N	6,230	3,156
Cash revenue from crop sales	Coefficient	29.218	29.263
(in ’000 UGX)	SE	(11.228)***	(16.213)*
	N	6,167	3,124

Note: *** p<0.01, ** p<0.05, * p<0.1. Regressions are OLS models with standard errors clustered at the village cluster level. Regressions include branch fixed effects. Baseline values of dependent variables are included as controls.

Table XI: Descriptive statistics of No Treatment vs. Continuation groups

	No Treatment	Continuation	Difference
Farmer age	42.18	41.93	0.251
	(0.538)	(0.603)	(0.824)
Education level, highest grade completed	5.344	5.569	-0.225
	(0.151)	(0.201)	(0.251)
Cultivated land, in acres	1.978	2.155	-0.177
	(0.070)	(0.088)	(0.113)
Own agricultural land, in acres	2.314	2.369	-0.055
	(0.169)	(0.110)	(0.232)
Formal title to land	0.556	0.604	-0.048
	(0.020)	(0.024)	(0.032)
At least two sets of clothes	0.887	0.906	-0.019
	(0.012)	(0.014)	(0.019)
At least two sets of shoes	0.645	0.665	-0.02
	(0.018)	(0.023)	(0.030)
Livestock, large	1.154	1.169	-0.015
	(0.092)	(0.102)	(0.143)
Livestock, small	1.368	1.23	0.138
	(0.091)	(0.092)	(0.137)
Microfinance member	0.238	0.655	-0.417
	(0.016)	(0.023)	(0.027)***
Distance to BRAC branch office	6.497	4.065	2.432
	(0.142)	(0.114)	(0.203)***

Note: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Standard errors in parentheses.

Table XII: Program impact on improved seed use

	Improved seed use				**Improved seed purchases**			
	OLS		PSM		OLS		PSM	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treated	0.1481***		0.1253**		0.1291***		0.0992	
	(0.0314)		(0.0609)		(0.0322)		(0.0663)	
Continuation		0.1549***		01703**		0.1405***		0.1485*
		(0.0500)		(0.0816)		(0.0515)		(0.0794)
BRAC microfinance	0.0307	-0.0188			0.0362	-0.0029		
member	(0.0320)	(0.0420)			(0.0308)	(0.0434)		
Distance to	-0.0104**	-0.0162***			-0.0079*	-0.0120**		
BRAC office	(0.0043)	(0.0049)			(0.0041)	(0.0048)		
R^2	0.188	0.219			0.191	0.211		
N	1781	1073	1437	863	1779	1072	1435	862
Mean value in No Treatment	0.240				0.227			

Note: *** p<0.01, ** p<0.05, * p<0.1. Standard errors (in parentheses) are clustered at the village cluster level. Dependent variables are binary indicators of seed use or purchases. OLS 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). PSM uses k-nearest neighbor matching (k=1, caliper 0.025) to find matches within branches only. Results restricted to households on the common support.

Table XIII: Program impact on local seed use

	Local seed use			
	OLS		PSM	
	(1)	(2)	(3)	(4)
Treated	-0.0384**		-0.0670**	
	(0.0177)		(0.0274)	
Continuation		-0.0358		-0.0526
		(0.0239)		(0.0396)
BRAC microfinance member	0.0202	0.0101		
	(0.0204)	(0.0275)		
Distance to BRAC office	0.0110***	0.0080***		
	(0.0029)	(0.0030)		
R^2	0.068	0.077		
N	1782	1074	1438	864
Mean value in No Treatment	0.922			

Note: *** p<0.01, ** p<0.05, * p<0.1. Standard errors (in parentheses) are clustered at the village cluster level. Dependent variables are binary indicators of seed use or purchases. OLS 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). PSM uses k-nearest neighbor matching (k=1, caliper 0.025) to find matches within branches only. Results restricted to households on the common support.

Table XIV: Program impact on organic fertilizer use

	Organic fertilizer use			
	OLS		PSM	
	(1)	(2)	(3)	(4)
Treated	0.0258		-0.0741	
	(0.0192)		(0.0808)	
Continuation		0.0416		0.0132
		(0.0297)		(0.0581)
BRAC microfinance member	0.0170	0.0151		
	(0.0205)	(0.0271)		
Distance to BRAC office	-0.0025	-0.0049		
	(0.0028)	(0.0032)		
R^2	0.094	0.112		
N	1793	1087	1444	872
Mean value in No Treatment	0.059			

Note: *** p<0.01, ** p<0.05, * p<0.1. Standard errors (in parentheses) are clustered at the village cluster level. Dependent variables are binary indicators of organic fertilizer use. OLS 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). PSM uses k-nearest neighbor matching (k=1, caliper 0.025) to find matches within branches only. Results restricted to households on the common support.

Table XV: Program impact on other inputs

	Chemical fertilizer		Pesticide		Hired labor		Mechanized plow	
	OLS	PSM	OLS	PSM	OLS	PSM	OLS	PSM
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Continuation dummy	0.0844**	0.0696	0.0508	0.0565	0.1025*	0.1370**	-0.0077	0.0351
	(0.0348)	(0.00533)	(0.0339)	(0.0538)	(0.0526)	(0.0681)	(0.0217)	(0.0369)
BRAC microfinance	-0.0455		-0.0425		0.0533		0.0765***	
member	(0.0317)		(0.0362)		(0.0418)		(0.0275)	
Distance to	-0.0037		0.0028		0.0005		-0.0017	
BRAC office	(0.0032)		(0.0044)		(0.0055)		(0.0020)	
R^2	0.139		0.076		0.103		0.426	
N	1089	874	1090	875	1090	875	1069	860
Mean value in No Treatment	0.065		0.17		0.454		0.080	

	Animals for plowing		Hand plow		Fuel		Transport	
	OLS	PSM	OLS	PSM	OLS	PSM	OLS	PSM
	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
Continuation dummy	0.1067**	0.0680	0.0801**	0.0811**	0.0014	0.0044	-0.0039	0.0393
	(0.0399)	(0.0.0551)	(0.0351)	(0.0400)	(0.0044)	(0.0077)	(0.0549)	(0.0716)
BRAC microfinance	-0.0616*		-0.0157		-0.0050		0.0553	
member	(0.0353)		(0.0300)		(0.0059)		(0.0472)	
Distance to	-0.0005		0.0054		-0.0010**		-0.0019	
BRAC office	(0.0060)		(0.0037)		(0.0005)		(0.0051)	
R^2	0.544		0.690		0.028		0.156	
N	1069	862	1069	859	1070	860	1071	861
Mean value in No Treatment	0.376		0.265		0.003		0.214	

Note: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Standard errors (in parentheses) are clustered at the village cluster level. Dependent variables are binary indicators of input use. OLS 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). PSM uses k-nearest neighbor matching (k=1, caliper 0.025) to find matches within branches only. Results restricted to households on the common support.

Table XVI: Program impact on cultivation practices

	Crop rotation		Intercropping		Line sowing		Irrigation	
	OLS	PSM	OLS	PSM	OLS	PSM	OLS	PSM
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Continuation dummy	0.1037**	0.1472*	-0.0475	0.0131	0.1295***	0.1856**	-0.0147	0.0044
	(0.0404)	(0.0841)	(0.0361)	(0.0591)	(0.0397)	(0.0808)	(0.0093)	(0.0128)
BRAC microfinance member	-0.0024		0.0076		-0.0478		0.0099	
	(0.0456)		(0.0341)		(0.0097)		(0.0278)	
Distance to BRAC office	0.0098*		0.0028		-0.0001		0.0050	
	(0.0056)		(0.0039)		(0.0011)		(0.0040)	
R^2	0.096		0.377		0.033		0.066	
N	1011	876	1018	866	874	866	1026	868
Mean value in No Treatment	0.685		0.588		0.753		0.015	
	Proper weeding		Zero tillage		Pest and disease management		Post-harvest storage	
	OLS	PSM	OLS	PSM	OLS	PSM	OLS	PSM
	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
Continuation dummy	0.0096	0.0261	0.0153	0.0175	0.0453	0.0437	0.0327	0.1026
	(0.0245)	(0.0490)	(0.0184)	(0.0206)	(0.0401)	(0.0580)	(0.0532)	(0.0951)
BRAC microfinance member	0.0361		-0.0003		-0.0645		-0.0595	
	(0.0392)		(0.0196)		(0.0425)		(0.0478)	
Distance to BRAC office	0.0036		0.0002		0.0028		-0.0011	
	(0.0051)		(0.0025)		(0.0046)		(0.0054)	
R^2	0.175		0.134		0.079		0.236	
N	986	866	693	866	949	866	855	866
Mean value in No Treatment	0.922		0.060		0.177		0.777	

Note: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Standard errors (in parentheses) are clustered at the village cluster level. Dependent variables are binary indicators of whether a practice is used. OLS 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). PSM uses k-nearest neighbor matching (k=1, caliper 0.025) to find matches within branches only. Results restricted to households on the common support.

Table XVII: Program impact on crop diversification

	Number of crops grown		**Cultivated maize**	
	OLS	PSM	OLS	PSM
	(1)	(2)	(3)	(4)
Continuation dummy	0.3758**	0.4738*	0.0709***	0.0480
	(0.1618)	(0.2700)	(0.0243)	(0.0463)
BRAC microfinance member	-0.2331		-0.0067	
	(0.1413)		(0.0269)	
Distance to BRAC office	0.0278		-0.0028	
	(0.0307)		(0.0037)	
R^2	0.091		0.262	
N	1098	868	1077	866
Mean value in No Treatment	3.274		0.786	

Note: *** p<0.01, ** p<0.05, * p<0.1. Standard errors (in parentheses) are clustered at the village cluster level. The dependent variable in columns 3 and 4 is a binary indicator of maize cultivation. OLS 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). PSM uses k-nearest neighbor matching (k=1, caliper 0.025) to find matches within branches only. Results restricted to households on the common support.

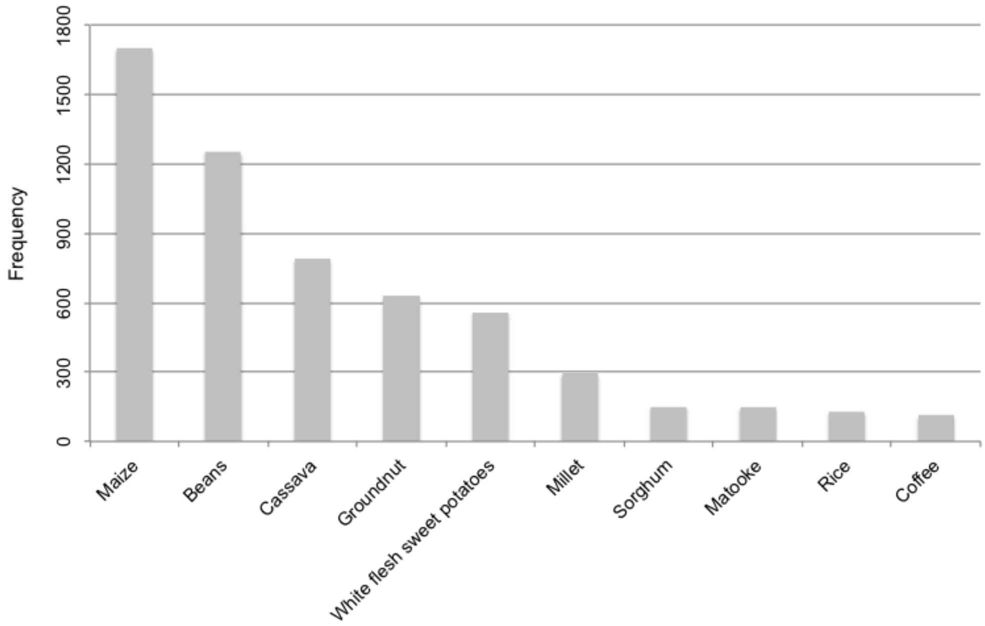
Table XVIII: Program impact on revenues and production value

	Revenues		**Total production value**	
	log UGX		log UGX	
	OLS	PSM	OLS	PSM
	(1)	(2)	(3)	(4)
Continuation	0.1025	0.0896	0.0916	0.1215
	(0.0963)	(0.1858)	(0.1131)	(0.1605)
BRAC microfinance member	-0.1616		-0.1292	
	(0.0940)		(0.1710)	
Distance to BRAC office	-0.0140		-0.0230	
	(0.0110)		(0.0148)	
R^2	0.145		0.208	
N	668	549	1020	825
Mean value in No Treatment	12.469		12.579	

Note: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Standard errors (in parentheses) are clustered at the village cluster level. Dependent variables are natural logarithms of UGX values. OLS 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). PSM uses k-nearest neighbor matching (k=1, caliper 0.025) to find matches within branches only. Results restricted to households on the common support.

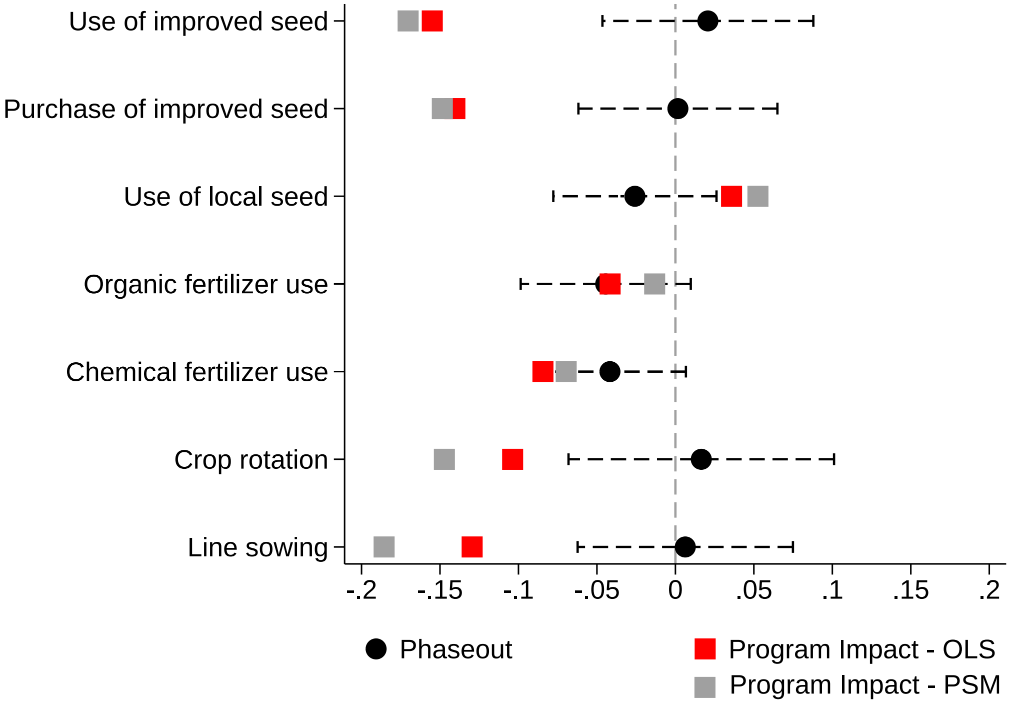
Figures

Figure I: Top 10 crops



Note: Data collected in second follow-up survey (September - October 2014).

Figure II: Phase-out impact confidence intervals



Note: Error bars denote 95% confidence intervals. OLS and PSM point estimates are from regressions using No Treatment and Continuation groups only.