Agriculture and Deforestation*

Ryan Abman
San Diego State University

Teevrat Garg
UC San Diego

Yao Pan
Aalto University

Saurabh Singhal
Lancaster University

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Abstract

Low land productivity and environmental degradation are common in much of the developing world. Improvements to agricultural productivity may raise household incomes and improve food security, but such improvements have ambiguous impacts on the environment. Increasing the relative value of natural land in agriculture may spurr land conversion, but factor market constraints paired with improvements in existing land productivity may delay or reduce the need to shift cultivation to new land. This paper studies this question using a large agricultural extension program in Uganda that increased availability of inputs and offered agricultural training programs. Leveraging a discontinuity in program eligibility, we find that forest loss during the program was significantly reduced in eligible villages compared to ineligible villages. Household level evidence indicates that the program led to intensification on existing agricultural land through practices such as increased irrigation, manure-use, crop rotation and inter-cropping. Overall, our results suggest that suitably designed, programs to improve agricultural productivity and improve ecological outcomes.

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1 Introduction

Low levels of agricultural productivity in developing countries remains one of the central challenges to economic development and barriers to exits from poverty (Gollin et al., 2014; Bustos et al., 2016; McArthur and McCord, 2017). As a result, governmental bodies and non-governmental agencies have emphasized technology adoption to improve agricultural productivity (Conley and Udry, 2010; Duflo et al., 2011). While concerns have been expressed over the ecological ramifications of programs designed to improve agricultural productivity, the evidence on their environmental impact remains mixed. One the one hand improving agricultural productivity can reduce pressure to clear forests for new agricultural land when farms are factor market constrained, a phenomenon referred to as Borlaug's hypothesis (Angelsen and Kaimowitz, 2001). On the other hand, improved agricultural productivity by increasing returns to agricultural land can increase pressures on land clearing. At the core of sustainable development is balancing these policies with ecological conservation (United Nations, 2015). In this paper, we estimate the effects of a large-scale extension program aimed at improving agricultural productivity on deforestation in Uganda.

Land-use change and deforestation is one of the most urgent global environmental concerns generating greenhouse gas emissions (IPCC, 2014; Jayachandran et al., 2017) and local health externalities (Bauch et al., 2015; Garg, 2019; Masuda et al., 2019). In fact, restoring deforested lands and protecting existing forests could deliver one-sixth of the emissions mitigation required to prevent runaway climate change by 2030 (IPCC, 2019). Between 2001-2018, Uganda lost approximately 780,000 hectares of forest cover and 90% of this loss is attributable to shifting cultivation (Curtis et al., 2018).

We estimate the impacts of a signature agricultural extension program implemented by BRAC Uganda that provided farmers with training on using new and improved techniques as well as providing access to better seeds. Leveraging a discontinuity in an eligibility rule that treated only those villages within 6 kilometers of a BRAC Uganda office, we find that the program reduced deforestation by 13% in barely eligible villages relative to those barely ineligible for the program. We find no discontinuity in baseline forest cover, or in the pre-treatment period. Using household survey data, we find that primary margin of adjustment is in the adoption of intensification technologies such as manure and irrigation, as well as land conservation practices such as inter-cropping and crop rotation. We find no evidence to suggest that there were any changes in firewood use, fertilizers or land under cultivation. Overall, we find that farm profits per acre increased substantially.

Our paper contributes to an important literature on the relationship between agricultural productivity and deforestation. Theoretical work on this "Borlaug's hypothesis" presents models with conflicting predictions rendering it largely an empirical question (Green et al., 2005; Takasaki, 2006; Balsdon, 2007; Phalan et al., 2016; Koch et al., 2019). While important and foundational empirical work has examined the relationship between agricultural productivity and deforestation (Rudel et al., 2009; Burney et al., 2010; Lambin and Meyfroidt, 2011; Stevenson et al., 2013) only two ongoing studies have addressed the endogeneity of agricultural productivity and deforestation, both using instrumental variables. Assunção et al. (2017) examine the effect of electrification on agricultural productivity and deforestation using geographic suitability of hydropower plants as an instrument. Abman and Carney (2019) use ethnic composition as an instrument for the allocation of fertilizer subsidies to study the effect on deforestation in Malawi. To the best of our knowledge, we are the only study that employs a regression discontinuity design relying on weaker assumptions of continuity in unobservable variables. Importantly, we provide household level evidence of the margins of adjustment that households undertake with an emphasis on land-saving technologies such as intercropping and crop rotation.

Second, our paper contributes to a broader literature on the effect of development programs on deforestation (Bulte et al., 2007; Alix-Garcia et al., 2013; Assunção et al., 2019; Asher et al., 2020). In this literature, we provide evidence that a scalable, and relatively inexpensive training program can substantially reduce deforestation. By extension we link to a broader literature examining the relationship between economic development and environmental quality, a relationship often referred to as the "Environmental Kuznet's Curve" (Arrow et al., 1995; Grossman and Krueger, 1995; Stern et al., 1996; Andreoni and Levinson, 2001; Foster and Rosenzweig, 2003; Dasgupta, 2007).

The rest of the paper is organized as follows. In Section 2 we provide background on the BRAC extension program as well as the conceptual framework. Section 3 describes the data while Section 4 details the research design. In Section 5 we discuss the results and in Section 6 we offer concluding remarks.

2 Background and Conceptual Framework

2.1 The BRAC Extension Program

BRAC's agricultural extension program in Uganda was launched in 2008 with the objective of increasing productivity of small, low-income female farmers through the adoption of modern cultivation techniques. The program had two complementary arms. In the first, "model farmers" were selected and trained in modern cultivation techniques such as inter-cropping, weeding, pest control, and adoption of new crop varieties. They were then required to set up a demonstration plot in their villages and pass on that training to others in the village. In the second, Community Agriculture Promoters (CAP) were selected from the same villages and provided subsidized HYV seeds (at approximately a 10% discount) to sell in their villages. There were no restrictions on the selling price, and objective was simply to increase the availability of HYV seeds in the village and the entrepreneurial skills of the CAP.

A key feature of the agricultural extension program was that it was limited to villages lying within an arbitrarily chosen distance of 6 km from each BRAC branch office.[1] The program was rolled out across 39 branches in 2008, with a further 20 branches added during 2009-2011. Program activities were officially ceased in 2013. Pan et al. (2018) find that the program was effective in increasing the adoption of modern cultivation techniques and inputs that require minimal upfront monetary investment such as inter-cropping, crop rotation and the use of manure, and significantly improved food security. Furthermore, the program also reduced prevalence of malaria, likely driven by the increase in income and the resulting ability to purchase bednets (Pan and Singhal, 2019).

2.2 Agriculture and Land-Use Change

Agricultural extension programs, when successful, can change agricultural practices such as input use and adoption of crop rotation thereby improving agricultural productivity. The effects of such programs on deforestation, however, are theoretically ambiguous. By improving

[1]BRAC chose the 6km limit in the pilot phase with the objective of balancing the need to reach as many villages as possible and the transportation costs for BRAC trainers. This threshold was later incorporated into the actual agricultural extension program and implemented regardless of geography or population density.

the returns to agricultural land, extension programs can increase pressures on land clearing by expanding agricultural land. On the other hand, if farmers are factor market constrained as they commonly are in developing countries (Conning and Udry, 2007), then intensification of agricultural production could alleviate pressures on clearing forest land for agriculture.

To understand how the extension program could affect land-use change on the intensive margin, we build a simple model of the farmer's optimization problem. The farmer has to choose to spend his wealth endowment (M) on agricultural inputs (a) that result in intensification (e.g., manure, crop rotation, inter-cropping, irrigation etc.) on existing (fixed) land (K) or expend resources to clear forested land (D) for agriculture at a constant unit cost (θ) or both. We assume that the farmer is constrained in factor markets. For the purposes of this model, we think of these constraints as credit constraints - the farmer is unable to borrow to finance inputs or agricultural land expansion. This is a reasonable assumption given well documented evidence of factor market constraints in developing countries, particularly in agricultural settings (Conning and Udry, 2007). The farmer faces decreasing returns to scale in intensification inputs on existing land and decreasing returns to scale in new land. This assumption is functionally equivalent to farmers being constrained in other factors of production such as labor. We model the extension program treatment (Λ) improving the productivity of intensification inputs. This is most easily thought of as farmer's learning how to better use these inputs. We normalize output prices to one for simplicity and without loss of generality. The farmer's problem is:

$$\begin{aligned}
&\max_{a,D} f(a, D; \Lambda) \\
&s.t \\
&\theta D + pa \leq M \qquad (1)
\end{aligned}$$

We derive two testable propositions from this model with proofs in Appendix A.1.

- The effect of the program on forest land cleared for agriculture is negative, that is, $\frac{\delta D}{\delta \Lambda} \leq 0$
- The effect of the program on adoption of intensification inputs is unambiguously positive, that is, $\frac{\delta a}{\delta \Lambda} \geq 0$

We can directly test both these theoretical predictions using our regression discontinuity design.

3 Data

3.1 Forest Cover

We use data on forest loss from the Global Forest Change (GFC) dataset from Hansen et al. (2013). This dataset provides spatially-explicit estimates of forest cover and annual forest loss derived from Landsat imagery. The high spatial resolution of the data (30 meters) makes it ideal for the present paper.

We obtain data on village locations from the Uganda Bureau of Statistics (2012). These data provide the latitude and longitude coordinates for over 5,500 villages across Uganda.[2] We keep all unique villages that lie within 12 KM of a BRAC center that have some forest cover at baseline.[3] This results in a sample of 807 villages. We attribute forest data pixels to villages if they lie with 400 meters of the village latitude and longitude coordinates. We choose 400 meters as our primary specification because that is the median household distance to village center in our household survey data described below and we report estimates using varying village radii in the appendix.

We calculate average baseline forest cover by averaging year 2000 forest cover percent over all village pixels. To obtain our measures of forest loss, we fit a two-way fixed effects model (using village and year fixed effects) to the inverse hyperbolic sine of the count of pixels reported as deforested in a given year. We average the residuals from this model for each village across the pre-program period (2001 - 2007) and the period in which the program operated (2008 - 2012). These residualized forest loss measures are the primary outcomes used in the regression discontinuity estimation.

3.2 Household Surveys

We also use data from BRAC's agricultural survey, conducted in 2011, in order to investigate the mechanisms under the effects on deforestation. The survey employed a two-stage cluster

[2] According to numbers from the Ugandan Electoral Commission (2015), these data only cover a subsample of all Ugandan villages. Furthermore, these data offer no additional information on villages (such as population), so we are unable to control for any such factors in our analysis.

[3] We use the the 39 branched that rolled out the program in 2008.

sampling process. First, for each of the 39 branches that rolled out the program in 2008, 17 villages were randomly picked from the list of villages in a radius of 9 km around a branch. Next, in each of the selected villages, 25 households were randomly chosen for the survey. The survey successfully collected demographic information and detailed agricultural practices records for the last two cropping seasons (July 2010 - June 2011) for 7206 households residing in 417 villages households.

4 Research Design

We estimate the impact of access the agricultural extension program on forest loss using a regression discontinuity (RD) design. While access to BRAC's program was intended for villages lying within a radius of 6 km of each BRAC branch office, we do not have information on actual implementation.[4] For this reason we use a sharp RD design to estimate intent-to-treat (ITT) effects.

We estimate the ITT effects using the non-parametric approach suggested by Hahn et al. (2001). We use local linear regressions to estimate the left and right limits of the discontinuity, and the difference between the two is the estimated treatment effect. Thus, the ITT effect can be identified as:

$$\beta = lim_{z\uparrow 0} E[Y|z_i = z] - lim_{z\downarrow 0} E[Y|z_i = z] \quad (2)$$

where the running variable, z_i, is defined as distance in meters from the cutoff point of 6 km, which is normalized to zero. The choice of the bandwidth can play an important role in RD analysis. Because the optimal bandwidth differs by outcomes, we choose a 2 km as our preferred specification to ensure consistent samples across outcomes in our analysis. For all our main regressions, we show sensitivity of our results to a variety of bandwidths including the optimal bandwidth according to Calonico et al. (2014). Finally, we use a triangular kernel to give higher weights to points nearer to the threshold (Imbens and Lemieux, 2008).

We estimate the regression discontinuity model on our outcome of interest (residualized

[4]Using information on program activities in the six months preceding the BRAC household survey, Pan et al. (2018) and Pan and Singhal (2019) find discontinuity in the coverage of the program.

forest loss during the program) but also on two additional outcomes that allow us to test our identifying assumptions, residualized forest loss prior to the program and baseline forest cover. The identifying assumption behind our spatial regression discontinuity model is that the eligibility distance was not chosen to coincide with other, unobserved factors that may also drive deforestation. Significant differences in baseline forest cover inside compared to outside the eligibility boundary or pre-program differences in forest loss in eligible vs ineligible villages prior to the start of the program may indicate previous forest-clearing activity led to the choice of 6KM for program eligibility. In the results below, we provide evidence in support of our identifying assumptions. We also verify that there is no difference in village density at the eligibility boundary using the method proposed by Cattaneo et al. (2019) and Cattaneo et al. (2018). We fail to reject the null hypothesis of no difference in village density at the boundary (p-value= 0.456).

We compliment our village-level forest outcome results with similar regression discontinuity models on the household data described above. Following Pan et al. (2018) and Pan and Singhal (2019), we assign households the running variable of distance of their village to the BRAC branch, with program eligibility for households whose village center lies within the 6KM boundary. For comparability, we use the 2KM bandwidth for our main household results (with alternative bandwidth results provided in the appendix).

5 Results

We begin with visual evidence of the impact of access to the agricultural extension program on deforestation. Figures 1a and 1b present average residuals of village-level forest loss by distance to the 6 km eligibility boundary for the treatment period and pre-treatment period respectively. Negative values indicate that a village lies within the eligibility cutoff. We overlay local linear regression lines and 95% confidence intervals, estimated separately for each side of the boundary. In the treatment period, we see average residuals systematically trending downward as the distance approaches the boundary for eligible villages while average residuals stay near zero for untreated villages. This figure presents the main finding of this paper, notably that annual forest loss was reduced in eligible villages during the agricultural extension period.

Figure 1b presents the same relationships using the average residuals over the pre-treatment period. Unlike in 1a, there is no notable difference in the regression lines to the left and right of the eligibility boundary. The absence of any systematic relationship at the boundary provides important validation of our underlying regression discontinuity assumptions. Were our main findings driven by underlying differences in technology, agricultural practices, or other unobservable factors that may also influence annual forest loss, we would expect to see differences in forest loss at the boundary prior to the treatment period. Furthermore, we find no differences in baseline forest cover (Figure 1c) or village density (Figure 1d) at the eligibility boundary.

We present our main results in Table 1. The first panel presents our RD estimates for residualized annual forest loss for the treatment period. Our estimates indicate program eligibility reduced annual forest loss by 12 - 13 percent during the program period. Estimates are significant at the 5 percent level for 2 km, 2.5 km, and 3 km bandwidths (the p-value for the CCT bandwidth is 0.052). The narrow 1.5 km badwidth estimate is of a similar magnitude, but is less precise.

The second and third panels of Table 1 present our RD estimates for annual forest loss prior to the treatment program as well as baseline forest cover. Our coefficient estimates are consistent with the visual evidence discussed above. Across all bandwidths, we find no evidence of significant differences in annual forest loss prior to the extension program and baseline forest cover is nearly identical in villages on either side of the eligibility boundary at baseline.

We turn to our household results in Tables 2 and 3. We present results in program-promoted agricultural practices that are consistent with sustaining soil nutrients and/or intensification investments. We use indicators of whether a household practices a particular agricultural technique and estimate the regression discontinuity model on those practices as a function of village location. We find significant increases in manure use, intercropping, and crop rotation for households in villages eligible for the program with intent-to-treat effects between 6 and 10 percentage points. These practices all address the issue of nutrient depletion in the soil that leads to individuals to shift agriculture to new land. We also find increases in irrigation investments, another practice intended to increase productivity of existing agricultural land.

We consider a variety of alternative explanations in Table 3. To see whether changes in firewood use may be driving this finding (rather than agricultural practices) we look at indicators of firewood use for light and for cooking in the household. We find no significant differences in these extensive margin measures of firewood use.[5] We also examine whether the access to purchased inputs may have driven the changes we observe. We find no significant differences in household use of fertilizer or high-yield-variety seeds and a small reduction in pesticide use. Program eligibility did not lead to differential sizes in cultivated area in eligible villages and households in ineligible villages. Finally, we find some evidence that the program increased agricultural productivity by looking at log profit per acre (column (7)) and log revenue per acre (column (8)). These estimates do become noisy and only the profit per acre is significant at the 10% level. Taken together, these household results suggest that the extension program prompted households to engage in agricultural practices that reduced the rate of nutrient depletion on the soil and increased productivity, yet did not change the area under cultivation or the use of firewood.

We undertake several robustness checks to confirm that our estimates are not sensitive to the specification choices and present these in the appendix of this paper. We vary the radius used to attribute spatial forest data to the village and find statistically significant results for 200 meter to 600 meter radii and consistent magnitudes but larger standard errors with an 800 meter radius. We estimate a variety of placebo boundaries, finding the 6 KM to be unique in both magnitude and significance. We show all household results are robust to choice of bandwidth.

6 Final Remarks

In this paper we provide evidence that improvements in agricultural productivity may also have ecological benefits via reduced pressure to expand into forest lands. Although improvements in agricultural productivity has the potential to increase the relative value of land in agriculture to standing forest, the presence of factor market constraints may lead farmers to continue to work on cleared land rather than clear and shift cultivation so long as soil productivity can be sustained. Empirically, we demonstrate a significant reduction in annual forest loss for villages eligible for the extension program during the program period that is not explained by earlier differences in forest loss nor differences in baseline forest cover. Households in eligible villages practice more techniques associated with nutrient preservation and intensification

[5]Unfortunately, the household survey does not include any questions that allow us to examine the intensive margin of firewood use. The survey asks about the total value of all fuel used, but it does not separate firewood from other forms of fuel.

and earn greater profits per acre of land cultivated.

While we show this particular agricultural extension program reduced forest loss while it operated, the lasting impacts of this program are an open question. Our deforestation data continues past the end of the program, but empirical estimates of the lasting effects are complicated by a number of factors. First, a subset of the branches phased out different aspects of the program over time. Second, in correspondence with BRAC officials, we learned that upon the conclusion of the program under study, BRAC did launch new agricultural outreach programs that did not utilize the same 6KM bandwidth. The follow-up program may have sought to include villages on the other side of the original eligibility boundary that did not previously have access to these services. Finally, diffusion of agricultural practices would attenuate any estimated effect over time.

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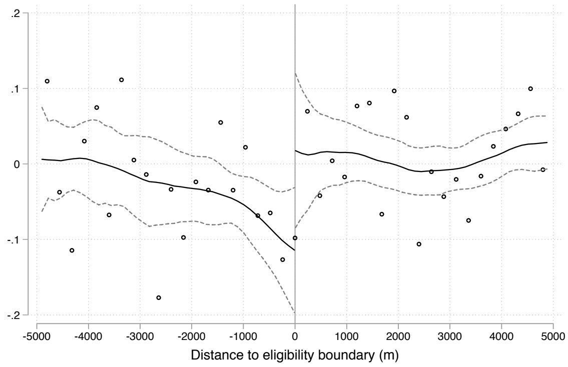
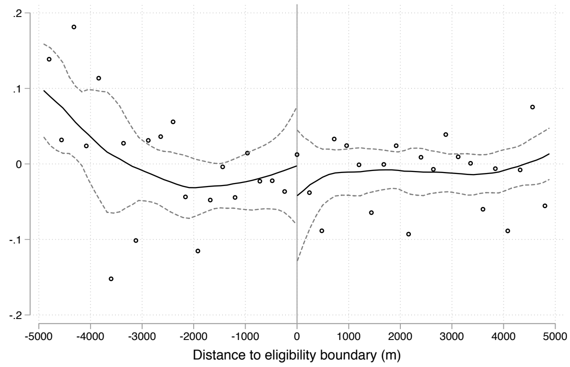
Figures

Figure 1: Discontinuity plots

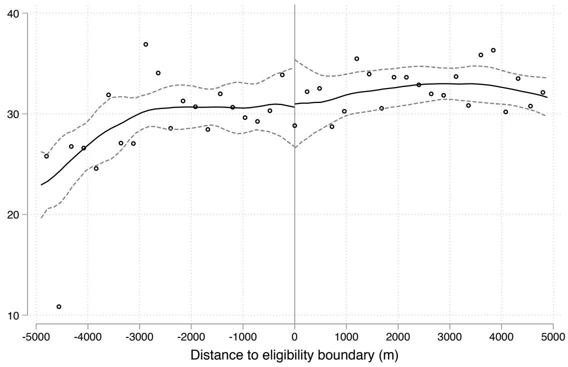
(a) Residualized Forest Loss - Treatment period



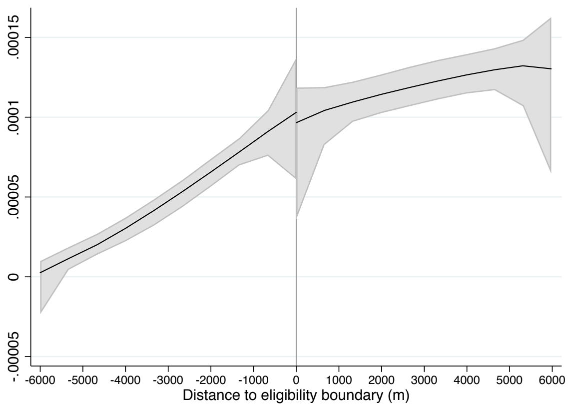
(b) Residualized Forest Loss - Pre-treatment period



(c) Baseline treecover



(d) Village location density



This figure presents the distribution of outcomes of interest plotted against the running variable of distance to the 6KM eligibility boundary in meters. Panel (a) plots average residuals from a two-way fixed-effects model of the inverse hyperbolic sine transformation of annual forest loss during the program period (2008 - 2012) and panel (b) plots these same average residuals for the pre-program period (2001 - 2007). Panel (c) plots average baseline (year 2000) forest cover and panel (d) plots the density test from Cattaneo et al. (2019). Plots (a) - (c) are constructed according to Calonico et al. (2015) using a fourth-order polynomial for fit.

Tables

Table 1: Regression discontinuity estimates of program eligibility on village-level forest outcomes

Bandwidth:	**2 km**	**CCT**	**1.5 km**	**2.5 km**	**3 km**
	(1)	(2)	(3)	(4)	(5)
	Forest Loss (IHS)				
Program Eligible	-0.133**	-0.133**	-0.123*	-0.128**	-0.126**
	(0.0629)	(0.0632)	(0.0733)	(0.0558)	(0.0523)
Mean loss in control (ha/yr)	0.0678	0.0682	0.0629	0.0674	0.0645
Obs	308	306	223	390	447
	Pre-treatment Forest Loss (IHS)				
Program Eligible	0.0396	0.0340	0.0555	0.0300	0.0228
	(0.0697)	(0.0643)	(0.0844)	(0.0602)	(0.0548)
Mean loss in control (ha/yr)	0.0578	0.0565	0.0512	0.0540	0.0565
Obs	308	354	223	390	447
	Year 2000 Treecover (%)				
Program Eligible	-0.314	-0.176	-0.908	-0.135	-0.301
	(3.554)	(3.428)	(3.903)	(3.414)	(3.313)
Control Average (%)	31.70	32.38	31.96	32.22	32.38
Obs	308	379	223	390	447

NOTES: Presented are non-parametric regression discontinuity estimates of program eligibility across different bandwidths. The top panel presents estimates on average residualized asinh of annual forest loss during the program. The middle panel presents estimates on the average residualized asinh of annual forest loss prior to the program and the final panel presents estimates on percent forest cover at baseline. CCT refers to the optimal bandwidth as proposed by Calonico et al. (2014). Statistical significance denoted as follows: *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 2: Regression discontinuity estimates of program eligibility on household-level agricultural practices

Outcome	(1) Manure Use	(2) Intercropping	(3) Crop Rotation	(4) Irrigation
Program Eligible	0.0977***	0.0590*	0.0737***	0.0326***
	(0.0249)	(0.0308)	(0.0251)	(0.00829)
Obs	2912	2912	2912	2912
Control mean	0.105	0.822	0.851	0.0127

Notes: Presented are regression discontinuity estimates of program eligibility on the adoption of agricultural practices. All models are estimated using the 2 KM bandwidth, with branch fixed effects and clustered standard errors at the branch level. Statistical significance denoted as follows: *** $p<0.01$, ** $p<0.05$, *$p<0.1$.

Table 3: Regression discontinuity estimates of program eligibility on household-level firewood use, purchased agricultural input use, and cultivated area

Outcome	(1) Firewood for Light	(2) Firewood to Cook	(3) Fertilizer	(4) Pesticide	(5) HYV Seeds	(6) IHS Cultiv. Ag Area	(7) Profit per Acre	(8) Revenue per Acre
Program Eligible	0.00261	-0.0492	-0.0171	-0.0371*	-0.0440	0.0262	0.160*	0.118
	(0.00221)	(0.0343)	(0.0161)	(0.0194)	(0.0323)	(0.0514)	(0.0896)	(0.0919)
Obs	3213	3210	2912	2912	2912	2907	2376	2671
Control mean	0.00505	0.841	0.0669	0.137	0.357	2.749	11.54	0.822

Notes: Presented are regression discontinuity estimates of program eligibility on firewood use, purchased agricultural input, and asinh of cultivated area. Outcomes in (1) - (5) are binary variables. The mean of cultivated agricultural area is in acres. All models are estimated using the 2 KM bandwidth, with branch fixed effects and clustered standard errors at the branch level. Statistical significance denoted as follows: *** p<0.01, ** p<0.05, *p<0.1.

A Appendix - Additional Tables and Figures

Table A1: Estimates varying village radius

	(1)	(2)	(3)	(4)
Program Eligible	-0.0586*	-0.133**	-0.158*	-0.137
	(0.0337)	(0.0629)	(0.0952)	(0.106)
Village Radius (m)	200	400	600	800
Mean loss in control (ha/yr)	0.0181	0.0678	0.148	0.279
Obs	308	308	308	308

Notes: This table presents estimates of our main result while varying the radius used to relate forest loss data to village coordinates. Column (2) presents estimates using the 400 meter radius in our main specification while column (1) uses a shorter 200 meter radius and columns (3) and (4) use 600 and 800 meter radii, respectively. All models use a bandwidth of 2 KM.

Table A2: Placebo eligibility boundary test

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Program Eligible	0.0852	0.112	0.0554	-0.133**	-0.00420	-0.0862	0.0924
	(0.0750)	(0.0731)	(0.0714)	(0.0629)	(0.0664)	(0.0654)	(0.0655)
Placebo Dist (km)	**4.5**	**5**	**5.5**	**6**	**6.5**	**7**	**7.5**
Mean loss in control (HA)	0.0678	0.0441	0.0629	0.0678	0.0674	0.0645	0.0647
Obs	229	269	282	308	331	340	359

Notes: This table presents placebo tests varying the radius of the cutoff for the regression discontinuity. The actual eligibility distance was 6 KM, which corresponds to Column (4). Other columns estimate placebo models by moving the estimated eligibility thresholds by 500 meters towards or away from the BRAC center. All models are estimated using the 2 KM bandwidth.

Table A3: Regression discontinuity estimates of program eligibility on household-level crop choice

	(1) Millet	(2) Maize	(3) Rice	(4) Groundnut	(5) Bean	(6) Coffee
Program Eligible	-0.0143	0.00468	-0.00548	-0.0131	-0.0517*	0.0512**
	(0.0224)	(0.0356)	(0.00972)	(0.0291)	(0.0300)	(0.0259)
Obs	3254	3254	3254	3254	3254	3254

Notes: Presented are regression discontinuity estimates of program eligibility household crop choice. Outcome variables are indicators equal to 1 if the household grows the particular crop. All models are estimated using the 2 KM bandwidth, with branch fixed effects and clustered standard errors at the branch level. Statistical significance denoted as follows: *** p<0.01, ** p<0.05, *p<0.1.

Table A4: Regression discontinuity estimates of household firewood use across multiple bandwidths

Bandwidth:	**CCT**	**1.5KM**	**2KM**	**2.5KM**	**3KM**
	(1)	(2)	(3)	(4)	(5)
	Firewood used for light				
Program Eligible	-0.00254**	0.00206	0.00261	0.00311	0.00329
	(0.00118)	(0.00262)	(0.00221)	(0.00206)	(0.00203)
Obs	1085	2471	3213	3719	4440
	Firewood used for cooking				
Program Eligible	-0.0501	-0.0460	-0.0491	-0.0503	-0.0405
	(0.0362)	(0.0341)	(0.0343)	(0.0316)	(0.0289)
Obs	2787	2468	3210	3717	4437

Notes: This table presents household level non-parametric RD estimates for each of our three firewood variables across a variety of different bandwidths. CCT refers to the optimal bandwidth as proposed by Calonico et al. (2014). Statistical significance denoted as follows: *** p<0.01, ** p<0.05, * p<0.1

Table A5: Regression discontinuity estimates of household firewood use across multiple bandwidths

Bandwidth:	**CCT**	**1.5KM**	**2KM**	**2.5KM**	**3KM**
	(1)	(2)	(3)	(4)	(5)
			Manure Use		
Program Eligible	0.105***	0.111***	0.0977***	0.0846***	0.0726***
	(0.0237)	(0.0232)	(0.0249)	(0.0235)	(0.0207)
Obs	2484	2231	2912	3388	4054
			Intercropping		
Program Eligible	0.124***	0.0814**	0.0590*	0.0519*	0.0565**
	(0.0427)	(0.0349)	(0.0308)	(0.0269)	(0.0231)
Obs	1429	2231	2912	3388	4054
			Crop Rotation		
Program Eligible	0.0709**	0.0703**	0.0737***	0.0776***	0.0771***
	(0.0285)	(0.0283)	(0.0251)	(0.0239)	(0.0237)
Obs	2174	2231	2912	3388	4054
			Irrigation		
Program Eligible	0.0246***	0.0335***	0.0326***	0.0318***	0.0325***
	(0.00799)	(0.00736)	(0.00829)	(0.0101)	(0.0107)
Obs	1499	2231	2912	3388	4054
			Weeding		
Program Eligible	0.0697**	0.0682*	0.0644**	0.0659**	0.0635**
	(0.0355)	(0.0378)	(0.0310)	(0.0278)	(0.0258)
Obs	2464	2231	2912	3388	4054

Notes: This table presents household level non-parametric RD estimates for each of agricultural practices for which we find significant effects at the 2KM bandwidth across a variety of different bandwidths. CCT refers to the optimal bandwidth as proposed by Calonico et al. (2014). Statistical significance denoted as follows: *** p<0.01, ** p<0.05, * p<0.1

A.1 Appendix - Analytical Proofs

$$\max_{a,D} f(a) + g(D)$$
$$s.t$$
$$\theta D + (1 - \Lambda)a \leq M$$

For ease of the analytical model, we will make some functional form assumptions, namely $f(a) = \sqrt{(a)}$ and $g(D) = \sqrt{(D)}$.

$$Ł = \sqrt{(a)} + \sqrt{(D)} - \eta \left[M - \theta D - (1 - \Lambda)a\right]$$