Agriculture and Deforestation

Ryan Abman, Teevrat Garg, Yao Pan and Saurabh Singhal*

This Version: April 2, 2021

Abstract

Although improving agricultural productivity is vital to anti-poverty and food security goals, its ecological effects are theoretically ambiguous. Increasing the relative value of agricultural land may spur deforestation, but factor market constraints paired with improvements in existing land productivity may reduce the demand for shifting cultivation. Leveraging the discontinuity in eligibility for a large agricultural extension program, we find that the program reduced deforestation by 13%. The program increased adoption of promoted practices such as manure-use and crop rotation resulting in higher productivity but no increase in cultivated area. Suitably designed programs improving agricultural productivity may also enable conservation.

*Abman: Department of Economics, San Diego State University (email: rabman@sdsu.edu). Garg: School of Global Policy and Strategy, University of California San Diego, CEGA and IZA (email: teevrat@ucsd.edu). Pan: Department of Economics, Aalto University (email: yao.pan@aalto.fi). Singhal: Department of Economics, Lancaster University, and IZA (email: s.singhal1@lancaster.ac.uk). We are grateful to Jennifer Alix-Garcia, Leah Bevis, Chris Barrett, Jennifer Burney, Brian Dillon, Yuta Masuda, participants at the AERE Summer Meetings 2020, OARES 2020 Seminar, and the 2020 PACDEV meetings, and seminar participants at University of British Columbia, University of California - San Diego and San Diego State University for valuable feedback. We thank Regan Kwan for excellent research assistance.

1 Introduction

Improving agricultural productivity remains one of the central goals of policy makers and researchers, not only to provide food security for a rising global population (Barrett, 2010; Godfray et al., 2010) but also to reduce poverty, foster economic development and enable the structural transformation of developing countries (Bustos et al., 2016; Gollin et al., 2014; McArthur and McCord, 2017). Such investments are increasingly made and warranted in regions with the last remaining stands of tropical forests, generating concerns about the ecological ramifications of improvements in agricultural productivity (Burney et al., 2010; Green et al., 2005). Although tropical 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), our understanding of the relationship between agricultural productivity and tropical deforestation remains incomplete.

In theory, improvements in agricultural productivity can have ambiguous effects on deforestation. "Boserup's hypothesis" suggests that improvements in land productivity would raise the value of agricultural land and thereby increase pressure on clearing forests for expansion of agriculture.[1] Conversely, under factor market constraints common in developing countries, improvements in productivity may instead spur intensification and spare land for nature, a notion referred to as "Borlaug's hypothesis". Investigating this empirical relationship is vital to enable conservation, especially in the face of regulatory failure to curb deforestation (Burgess et al., 2012).

We estimate this relationship in the context of a large-scale extension program aimed at improving agricultural productivity in Uganda. Such programs are being increasingly used around the developing world to improve agricultural yields.[2] The program, implemented by BRAC Uganda from 2008-2013, provided farmers with training on using new and improved techniques as well as providing access to better seeds. Coinciding with this time period, be-

[1] A related and more general class of phenomena where improvements in productivity or efficiency increase rather than reduce use is referred to as Jevon's Paradox.

[2] From 2005 to 2016, developing countries received approximately 91.7 billion USD in agricultural aid (with 1.9 billion specifically in the form of agricultural inputs) from NGOs, multilateral organizations, private sector institutions, and various other channels (OECD, 2016). Between 2011 and 2014, at least 10 African countries had implemented input subsidy programs to improve small-holder agriculture with annual spending ranging from 600 million to 1 billion USD (Jayne et al., 2018). The Food and Agriculture Organization of the United Nations allocated, in the fiscal year 2018-2019 budget, allocated approximately $200 million (19.6% of it's total budget) to making agriculture, forestry, and fisheries more productive and sustainable, this includes the target of doubling the agricultural productivity and incomes of small-scale food producers by 2030 (UNFAO, 2019).

tween 2001-2018, Uganda lost approximately 780,000 hectares of forest cover and 90% of this loss is attributable to shifting smallholder cultivation (Curtis et al., 2018).

To understand the effect of improvements in agricultural productivity on deforestation, we begin with a conceptual model on which we ground our empirical exercise. We adapt the framework of Assunção et al. (2017); in our setting farmers choose between farming on existing land or clearing forests to make way for new agricultural land. The extension program improves the productivity of all agricultural land but disproportionately so for land already under cultivation. Under factor market constraints, improvements in productivity lead to intensification, thereby decreasing pressure on clearing forests for agriculture. At the same time, we assume farmers differ only in their outside, non-agricultural option and increases in agricultural productivity induce more farmers to engage in agriculture thereby increasing pressure on land clearing. The net effect is ambiguous.

For the subsequent empirical exercise, we leverage a discontinuity in village eligibility – only villages within 6 kilometers of a BRAC Uganda office could participate – and find that the program reduced annual 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 forest loss prior to the program. Using household survey data, we report improvements in agricultural productivity but no increases in area under cultivation. We find the primary margin of adjustment is in the adoption of intensification technologies (e.g., manure-use, irrigation, intercropping, crop rotation and weeding) but find no increased use of purchased inputs (such as chemical fertilizers or improved seeds) nor in switching to perennial crops. While we find no evidence to suggest that the conservation effects were reversed or mitigated after the end of the program, research design limitations also prevent us from ruling out such effects. Under the most conservative scenario that program effects were entirely reversed at the conclusion of the program, the delay in forest loss provides 14% of the benefits of permanently avoiding forest loss.

We join a long-standing literature on the potential trade-offs and synergies between economic development and environmental conservation (Andreoni and Levinson, 2001; Antweiler et al., 2001; Arrow et al., 1996; Dasgupta et al., 2002; Den Butter and Verbruggen, 1994; Grossman and Krueger, 1995; Stern, 2004; Stern et al., 1996).[3] Within this literature, theoretical work

[3]On deforestation specifically, prior work has examined the effects of economic growth (Foster and Rosenzweig,

on the relationship between agricultural productivity and deforestation provide models with varying predictions rendering it largely an empirical question (Angelsen and Kaimowitz, 2001; Balsdon, 2007; Goldstein et al., 2012; Green et al., 2005; Phalan et al., 2016; Takasaki, 2006). While existing important and foundational empirical work examines this relationship between agricultural productivity and deforestation/land use at a macro scale (Burney et al., 2010; Lambin and Meyfroidt, 2011; Pelletier et al., 2020; Rudel et al., 2009; Stevenson et al., 2013), there is a notable dearth of well-identified empirical work at a subnational scale.[4] An important exception is (Assunção et al., 2017) who examine the impact of electrification in Brazil which improved the productivity of farming relative cattle ranching and reduced forest loss. Causal identification is complicated due to the non-random nature of variation in agricultural productivity, which is often correlated with land characteristics or existing land-use. We overcome this challenge by leveraging a spatial discontinuity in village-eligibility for a program aimed at improving agricultural productivity. Importantly, extension programs designed to improve productivity are ubiquitous thereby providing a potentially scalable policy that can deliver economic and conservation benefits.

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 Deforestation in Uganda

Forest cover in Uganda has shrunk rapidly from 24% of total land area in 1990 to just 9% in 2015. Forest conservation in Uganda is uniquely challenging as a majority of the forested land is privately owned (approximately 70%).[5] Under the Land Act (Amendment) of 2010, private land

2003), transportation infrastructure (Asher et al., 2020), place-based economic policies (Garg and Shenoy, 2020) and cash transfers (Alix-Garcia et al., 2013; Ferraro and Simorangkir, 2020; Wilebore et al., 2019).

[4]Relatedly, Abman and Carney (2020) use ethnic favoritism as an instrument for subsidized agricultural inputs and find a resulting reduction in forest loss in Malawi. Caviglia-Harris (2018) and Koch et al. (2019) demonstrate that conservation policies can induce agricultural intensification by limiting the ability to expand into natural lands, however, the question of interest in this paper is whether productivity improvements increase or decrease forest clearing.

[5]Forest tenure typically takes the form of freehold, leasehold, or mailo.

owners are allowed to convert forest lands for agricultural and other development purposes. Consequently, most of the deforestation in Uganda has occurred on private forest lands that fall outside designated protected areas. It is estimated that between 1990 and 2015 almost half of the private forested land was lost (Uganda Ministry of Water and Environment, 2017). The main drivers of deforestation during this period have been conversion of land for agriculture (Curtis et al., 2018).

2.2 The BRAC Uganda Extension Program

BRAC Uganda launched an agricultural extension program in 2008 with the objective of increasing productivity of small, low-income female farmers through the adoption of modern cultivation techniques. The program included two complementary arms. In the first, "model farmers" were selected and trained in modern cultivation techniques such as crop rotation, inter-cropping, manure, irrigation, weeding and pest control. 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 high yielding variety (HYV) seeds (at approximately a 10% discount) to sell in their villages. There were no restrictions on the selling price as the objectives were to increase the availability of HYV seeds in the village and the entrepreneurial skills of the CAP. In most cases, these seeds were sold at market prices.

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.[6] The program was rolled out across 39 branches in 2008. Program activities were officially ceased in 2013, though some branches participated in a staggered phaseout program.[7] 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.

[6] 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 selected for the pilot was later incorporated arbitrarily into the agricultural extension program and implemented regardless of geography or population density. In Appendix Figure A.1 we show that key geographic parameters such as elevation, distance to nearest road and distance to nearest water source are continuous at the 6 km threshold.

[7] Unfortunately, the nature of the phaseout, the concurrent introduction of new agricultural programs and anecdotal evidence of post-program spillovers makes it difficult to estimate causal effects from the end of the program. We discuss this in more detail in Section 5.2.

2.3 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 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.[8]

We adapt Assunção et al. (2017) to build a simple model to understand how the extension program could affect land-use change. We begin with an economy where there is a continuum of agents who are identical in every regard except their outside non-agricultural options such that changes in returns to the agricultural sector change the number of agents (n) who select into farming. Farmers choosing to engage in agricultural livelihoods are faced with the decision to allocate their household labor between farming on existing agricultural land or on new agricultural land derived by clearing forests. For simplicity, we limit household labor allocation to farming on new or existing land. Reallocation of labor towards and away from agriculture is captured instead by allowing number of farmers in the agricultural sector to vary. Let γ denote household labor allocated to farming in new land; we normalize total household labor to 1 and therefore labor allocated to farming in existing land is $1 - \gamma$. The production functions for new and existing land are $f(\cdot)$ and $g(\cdot)$ respectively, with usual properties of monotonicity and concavity in inputs. Importantly, we assume that the extension program (Λ) improves the productivity of all agricultural land but disproportionately so for existing land. That is,

$$\frac{\partial g(l)}{\partial \Lambda} \geq \frac{\partial f(l)}{\partial \Lambda} \geq 0 \quad \forall\, l \tag{1}$$

This is a reasonable assumption and one we can empirically validate. First, BRAC promoted several practices that help conserve soil fertility such as inter-cropping and crop rotation which improve the relative soil quality and hence productivity of existing land relative to newly cleared land. Second, the overall objective of the program was to increase food security

[8] There can be other circumstances, such as inelastic demand for agricultural outputs, that may disincentivize extensification when land productivity increases. For the sake of simplicity, we highlight factor market constraints that are rather ubiquitous in developing countries.

by improving land productivity across the board. Indeed, in the empirical section that follows, we find that the program increased adoption of soil conservation techniques and productivity enhancing technologies.

Finally, we assume that farmers are factor market constrained and are unable to hire labor beyond household labor, a reasonable assumption in our context (Conning and Udry, 2007). We denote n^* as the total number of farming units (individuals or households) engaging in agriculture in equilibrium for whom the returns in the agricultural sector exceed their individual-specific outside option. Total deforestation (D^*) in equilibrium is given by new land cleared for agriculture. For simplicity, we assume a linear function:

$$D^* = n^* \cdot \gamma^* \tag{2}$$

Differentiating equation (2) with respect to the program Λ, we get

$$\frac{\partial D^*}{\partial \Lambda} = \underbrace{n^* \frac{\partial \gamma^*}{\partial \Lambda}}_{\leq 0} + \underbrace{\gamma^* \frac{\partial n^*}{\partial \Lambda}}_{\geq 0} \tag{3}$$

Equation (3) shows that the effect of the program on deforestation is ambiguous. It is decomposed into two terms. The first term is the effect of the program on the share of labor each farmer allocates to new land versus existing land. This is the intensive margin effect and is unambiguously negative. This follows from Equation (1) and the assumption on factor market constraints. Since the program makes existing land more valuable and farmers are constrained in labor inputs, they reallocate labor from new land to existing land reducing demand for new land. The second term is the effect of the program on the number of farmers in agriculture. This is the extensive margin effect and is unambiguously positive. Since all agricultural land becomes more productive with the program, agricultural returns increase and more agents choose to farm. The net effect of these two countervailing forces is ambiguous and we test this effect in the empirical exercise that follows.

3 Data

In the absence of detailed, spatially explicit official records of forest cover and deforestation, we rely on standardized, publicly available, high-resolution time series of forest cover. Our primary analysis employs a commonly-used product, Global Forest Change (GFC) that provides baseline forest cover in 2000 and year-to-year forest loss at the 30 meter resolution derived from Landsat imagery (Hansen et al., 2013). The high resolution of GFC is ideal for our research design that relies on a spatial discontinuity at the 6 km point and in this context is preferable to lower resolution products such as Vegetation Continuous Fields (VCF) used in other contexts (Asher et al., 2020). However, one disadvantage of GFC is that it only tracks forest loss but not forest gain rendering it less than ideal in contexts where forest cover is increasing over time. However, our period of study saw large scale declines in forest cover in Uganda, allowing us to capture overall forest cover change suitably through GFC.

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.[9] We keep all unique villages that lie within 12 km of a BRAC center that have some forest cover at baseline. 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.

[9] According to reports 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, poverty levels etc.), so we are unable to account for any such factors in our analysis. However, in the regression discontinuity design we employ, interpreting our estimates as casual requires only that these indicators be continuous at the 6 km threshold from BRAC Uganda centers.

In order to investigate the mechanisms underlying the effects on deforestation at the household-level, we also use data from BRAC's agricultural survey, conducted in 2011. The survey employed a two-stage cluster 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 around the branch. Next, in each of the selected villages, 25 households were randomly chosen for the survey. After cleaning the GPS coordinates and restricting the sample to villages within a radius of 12 km of a branch we get a sample of 7,781 households residing in 451 villages. The survey successfully collected demographic information and detailed agricultural practices records for the last two cropping seasons (July 2010 - June 2011).

4 Research Design

We estimate the impact of access to the agricultural extension program on forest loss using a regression discontinuity (RD) design. The program was designed to reach all villages within a 6 km radius from a BRAC Uganda Office. We estimate the intent-to-treat (ITT) effects using the non-parametric approach following Hahn et al. (2001).[10] We use local linear regressions to estimate the left and right limits of the discontinuity, and the difference between the two is the estimated ITT effect. Thus, the ITT effect can be identified as:

$$\beta = lim_{z \uparrow 0} E[Y|z_i = z] - lim_{z \downarrow 0} E[Y|z_i = z] \tag{4}$$

where the running variable, z_i, is defined as distance of the village (in meters) from the cutoff point of 6 km, which is normalized to zero, and $z \leq 0$ implies that the village had access to the program. As the choice of the bandwidth can play an important role in regression discontinuity estimates, we test the sensitivity of the RD estimates of each of our main outcome variables to various bandwidth choices including the optimal bandwidth as proposed by Calonico et al. (2014). However, the optimal bandwidth differs by outcomes; therefore, we choose a 2 km bandwidth for our preferred specification to ensure consistent samples across outcomes in our

[10] Unfortunately, we do not have administrative data on village-level treatment designation, and thereby are unable to employ a fuzzy discontinuity design. However, Pan et al. (2018) and Pan and Singhal (2019) use data from a household survey conducted three years after the inception of the program and find a spatial discontinuity in program coverage in villages sampled.

analysis. We use a triangular kernel to give higher weights to points nearer to the discontinuity threshold (Imbens and Lemieux, 2008). We cluster our standard errors at the BRAC branch level.

Our primary outcome of interest is forest loss. Since forest loss can be zero in a village in a given year, we use an inverse hyperbolic sine transformation of forest loss, residualized using two-way village and year fixed effects. The use of satellite-based measures of forest cover allows us to also test for the presence of a discontinuity on baseline forest cover (available for the year 2000) and pre-program forest loss (2001-2007) in order to support our research design. The identifying assumption behind the 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 6 km for program eligibility. We provide evidence in support of our identifying assumptions finding no changes in baseline forest cover or pre-program forest loss at the 6 km boundary. In the appendix, we provide evidence of continuity in geographic characteristics.[11] 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$=0.46$).

To uncover the mechanisms through which the program affects forest loss, we compliment our village-level forest outcome results with similar regression discontinuity models on the household survey data. As distance from village centers to the nearest BRAC branch was not directly reported, we computed each household's distance from the nearest BRAC branch using its GPS coordinates and then used the median household's distance as a proxy for the distance of the village from the nearest BRAC branch (Pan and Singhal, 2019; Pan et al., 2018). For comparability, we use the 2 km bandwidth for our preferred specification with results from alternative bandwidths relegated to the appendix. As in the case of the specifications on forest cover, we cluster standard errors at BRAC branch level.

[11] In Figure A.1, we show continuity at the eligibility boundary for distance to nearest road, distance to nearest lake or river, and elevation.

5 Results

We report three principal findings. First, the program reduced forest loss in treatment relative to control villages by 13%. Second, the evidence is consistent with an increase in intensification through changes in inputs (e.g., manure use and irrigation) as well as changes in cultivation practices (e.g., intercropping and crop rotation). Finally, we find that revenue and profits per acre increased with no changes in total land area under cultivation.

We begin with visual evidence of the impact of access to the agricultural extension program on deforestation. Figures 3a and 3b 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 on the running variable (left-hand side of zero) 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 treatment period.

Figure 3b presents the same relationships using the average residuals over the pre-treatment period. Unlike in 3a, 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. If our main findings were 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 3c) or village density (Figure 3d) at the eligibility boundary (Cattaneo et al., 2018, 2019).

We present corresponding estimates in Table 1. The first panel presents our RD estimates for residualized inverse hyperbolic sine of annual forest loss for the treatment period. Our estimates indicate program eligibility reduced annual forest loss by 12 - 13 percent during the program period. Our estimates are similar in magnitude and precision across various bandwidth choices including the optimal bandwidth as proposed by Calonico et al. (2014).

The second and third panels of Table 1 present our RD estimates for residualized inverse hyperbolic sine of annual forest loss prior to the treatment program as well as percentage of baseline (year 2000) 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. The coefficients in each case are small and statistically insignificant.

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 (Appendix Table A.1). We also estimate a variety of placebo boundaries, finding the 6 km to be unique in both magnitude and significance (Appendix Table A.2).

5.1 Mechanisms

In this section, we investigate the mechanisms through which the program increased agricultural productivity and reduced forest loss. In Table 2 we test the effect of the program on promoted agricultural practices that are consistent with sustaining soil nutrients and/or intensification investments.[12] 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. Consistent with Pan et al. (2018), we find significant increases in manure use (Column 1: 9.8 percentage points), intercropping (Column 2: 5.9 percentage points), and crop rotation (Column 3: 7.4 percentage points) for households in villages eligible for the program. These practices all address the issue of nutrient depletion in the soil on existing agricultural land that encourages farmers to shift agriculture to new land. We also find increases in irrigation (Column 4: 3.3 percentage points) , another practice intended to increase productivity of existing agricultural land. We find no evidence of increases in use of chemical fertilizer (Column 5), pesticides (Column 6) or high-yielding varieties of seeds (Column 7).[13]

[12] As we do not have plot level information on the use of agricultural practices, we are estimating the combined effects of agricultural intensification and extensification. However, since we do not find any evidence of extensification (Table 3), we believe that changes in these practices reflect agricultural intensification.

[13] These estimates are also robust to choice of bandwidth (Appendix Table A.6).

These changes in practices correspond with 32.5% increase in revenue per acre (Table 3, Column 1). Under the reasonable assumption that prices do not change discontinuously at the 6 km threshold, revenue per acre serves as a reasonable proxy for yields, implying that the program did improve agricultural productivity. We find no evidence that program eligibility led to an increase in area under cultivation (Table 3, Column 2) suggesting that factor market constraints bind the expansion of overall agricultural land. Additionally, we find no evidence of an increase in participation in agriculture at either the individual or household level (Table 3, Columns 3-4) implying that the effect of the program on land-use change at the extensive margin was largely muted.

We consider two alternative mechanisms – reduced demand for fuel wood and adoption of perennial crops – that could explain the forest conserving effect of the agricultural program. In both cases, we find evidence inconsistent with these mechanisms.

First, positive income effects from the program (Pan and Singhal, 2019; Pan et al., 2018), may induce households to move up the "energy ladder", switching fuel use away from firewood (Hanna and Oliva, 2015). Indeed, local demand for firewood and charcoal is an important, although not the dominant factor underlying deforestation in Uganda (Curtis et al., 2018). We test whether changes in firewood use, as opposed to agricultural practices, may be driving the effect of the program on reductions in forest loss we consider indicators of firewood use for light and for cooking in the household. We find no differences in these extensive margin measures of firewood use. As shown in Appendix Table A.7, the coefficients are negligible in magnitude and are statistically insignificant.[14]

Second, the BRAC program may have induced adoption of perennial crops thereby reducing the need to leave land fallow and shift cultivation to forested land. We test for this possibility by constructing an indicator variable that takes the value 1 if the household grows any perennial crops and 0 otherwise. We find no evidence that the program induced the take-up on perennial as opposed to seasonal crops (Table 2, column 8).

[14]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.

5.2 Delayed or avoided deforestation?

Some elements of the BRAC Uganda extension program were transitory in their nature (provision of BRAC HYV seeds) while other elements may have more persistent effects (agricultural training modules). Whether the ecological benefits found in this paper should persist beyond the end of the program is an open, empirical question. Although our forest loss data extend past 2013, the setting makes it difficult to test this for a number of reasons. First, although the program ended in 2013, a number of BRAC branches participated in a phase-out program - continuing some program features beyond the end date. 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 6 km 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. Lastly, diffusion of the techniques to originally ineligible villages overtime could lead to underestimates of the persistence of program effects measured at the original eligibility boundary.

With these caveats in mind, we estimate the effect of program treatment on residualized village-level forest loss post-2013 (Appendix Table A.8). We limit the sample to villages near branches that did not participate in the phaseout (222 villages within the 2 km bandwidth). While we do not find continued reduction in annual forest loss at the eligibility boundary, the magnitude of the estimates suggests that previous reductions were, at a minimum, not offset at the end of the program. We note that the lack of precision in our estimates and lack of household data after 2013 limit the conclusions that can be drawn from this analysis.

Even if the reductions in forest loss found from the program are not permanent, there are still sizeable economic benefits from delaying deforestation. To illustrate, under the conservative scenario that forest loss was merely postponed by 5 years and subsequently undone, the present value of this delay in associated CO_2 emissions, under a time discount rate of 3 percent, is approximately 14 percent of the benefits of permanently avoiding the forest loss. This scenario of a 5-year delay would arise if our estimates on post-program forest loss (presented in Appendix Table A.8) were positive and of the same magnitude as the estimates in our main findings. Thus, forest not cleared in 2008 would be cleared in 2013, forest not cleared in 2009 would be cleared in 2014, etc. As the post program estimates are smaller in magnitude and not statistically distinguishable from zero, we believe this to be overly conservative with the realized environmental benefits in excess of 14 percent of those associated with permanently avoided deforestation.

6 Conclusion

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 have 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 and earn greater profits per acre of land cultivated.

While these findings are optimistic for achievement of the dual objectives underlying sustainable development, there are two caveats that warrant discussion and may offer areas for future research. First, it is not clear if reductions in forest loss would occur under a similar program in a location with fewer labor market constraints. If households could easily hire labor, improvements in agricultural productivity may encourage both intensification and extensification leading to increased forest loss. Without spatial variation in labor market constraints in our setting, we are unable to test this hypothesis. Second, the improvements in agricultural income could lead to certain general equilibrium effects that we are not able to capture in our regression discontinuity approach. Improving household incomes via improved yields might stimulate local demand for land-intensive goods leading to a higher rate of forest loss. While some insights on these effects can be gleaned from the literature on cash transfers and forest loss (Alix-Garcia et al., 2013; Ferraro and Simorangkir, 2020; Wilebore et al., 2019), findings in this literature tend to vary by setting and program.

Our research underscores the importance of evaluating ecological outcomes in the context of interventions aimed at improving agricultural productivity. Many of these interventions, including the one we evaluate here, can potentially be scaled up to deliver to "win-win" scenarios. Future research on these issues will be important to help craft agricultural interventions that also conserve nature.

References

Abman, R. and Carney, C. (2020). Agricultural productivity and deforestation: Evidence from input subsidies and ethnic favoritism in malawi. *Journal of Environmental Economics and Management*, page 102342.

Alix-Garcia, J., McIntosh, C., Sims, K. R., and Welch, J. R. (2013). The ecological footprint of poverty alleviation: evidence from mexico's oportunidades program. *Review of Economics and Statistics*, 95(2):417–435.

Andreoni, J. and Levinson, A. (2001). The simple analytics of the environmental Kuznets curve. *Journal of Public Economics*, 80(2):269–286.

Angelsen, A. and Kaimowitz, D. (2001). *Agricultural technologies and tropical deforestation*. CABi.

Antweiler, W., Copeland, B. R., and Taylor, M. S. (2001). Is free trade good for the environment? *American Economic Review*, 91(4):877–908.

Arrow, K., Bolin, B., Costanza, R., Dasgupta, P., Folke, C., Holling, C., Jansson, B.-O., Levin, S., Miller, K.-G., Perrings, C., and Pimentel, D. (1996). The environment, carrying capacity and economic growth. *Ecological Applications*, 6(1):17–19.

Asher, S., Garg, T., and Novosad, P. (2020). The ecological impact of transportation infrastructure. *The Economic Journal*, 130(629):1173–1199.

Assunção, J., Lipscomb, M., Mobarak, A. M., and Szerman, D. (2017). Agricultural productivity and deforestation in brazil. Mimeo.

Balsdon, E. M. (2007). Poverty and the management of natural resources: A model of shifting cultivation. *Structural Change and Economic Dynamics*, 18(3):333–347.

Barrett, C. B. (2010). Measuring food insecurity. *Science*, 327(5967):825–828.

Bauch, S. C., Birkenbach, A. M., Pattanayak, S. K., and Sills, E. O. (2015). Public health impacts of ecosystem change in the Brazilian Amazon. *Proceedings of the National Academy of Sciences*, 112(24):7414–7419.

Burgess, R., Hansen, M., Olken, B. A., Potapov, P., and Sieber, S. (2012). The political economy of deforestation in the tropics. *The Quarterly Journal of Economics*, 127(4):1707–1754.

Burney, J. A., Davis, S. J., and Lobell, D. B. (2010). Greenhouse gas mitigation by agricultural intensification. *Proceedings of the national Academy of Sciences*, 107(26):12052–12057.

Bustos, P., Caprettini, B., and Ponticelli, J. (2016). Agricultural productivity and structural transformation: Evidence from Brazil. *American Economic Review*, 106(6):1320–65.

Calonico, S., Cattaneo, M. D., and Titiunik, R. (2014). Robust nonparametric confidence intervals for regression-discontinuity designs. *Econometrica*, 82(6):2295–2326.

Calonico, S., Cattaneo, M. D., and Titiunik, R. (2015). Optimal data-driven regression discontinuity plots. *Journal of the American Statistical Association*, 110(512):1753–1769.

Cattaneo, M. D., Jansson, M., and Ma, X. (2018). Manipulation testing based on density discontinuity. *The Stata Journal*, 18(1):234–261.

Cattaneo, M. D., Jansson, M., and Ma, X. (2019). Simple local polynomial density estimators. *Journal of the American Statistical Association*, (just-accepted):1–11.

Caviglia-Harris, J. L. (2018). Agricultural innovation and climate change policy in the brazilian amazon: Intensification practices and the derived demand for pasture. *Journal of Environmental Economics and Management*, 90:232–248.

Conning, J. and Udry, C. (2007). Rural financial markets in developing countries. *Handbook of agricultural economics*, 3:2857–2908.

Curtis, P. G., Slay, C. M., Harris, N. L., Tyukavina, A., and Hansen, M. C. (2018). Classifying drivers of global forest loss. *Science*, 361(6407):1108–1111.

Dasgupta, S., Laplante, B., Wang, H., and Wheeler, D. (2002). Confronting the environmental Kuznets curve. *Journal of Economic Perspectives*, 16(1):147–168.

Den Butter, F. A. and Verbruggen, H. (1994). Measuring the trade-off between economic growth and a clean environment. *Environmental & Resource Economics*, 4(2):187–208.

Ferraro, P. J. and Simorangkir, R. (2020). Conditional cash transfers to alleviate poverty also reduced deforestation in Indonesia. *Science Advances*, 6(24):eaaz1298.

Foster, A. D. and Rosenzweig, M. R. (2003). Economic growth and the rise of forests. *The Quarterly Journal of Economics*, 118(2):601–637.

Garg, T. (2019). Ecosystems and human health: The local benefits of forest cover in indonesia. *Journal of Environmental Economics and Management*, page 102271.

Garg, T. and Shenoy, A. (2020). The ecological impact of place-based economic policies.

Godfray, H. C. J., Beddington, J. R., Crute, I. R., Haddad, L., Lawrence, D., Muir, J. F., Pretty, J., Robinson, S., Thomas, S. M., and Toulmin, C. (2010). Food security: the challenge of feeding 9 billion people. *science*, 327(5967):812–818.

Goldstein, J. H., Caldarone, G., Duarte, T. K., Ennaanay, D., Hannahs, N., Mendoza, G., Polasky, S., Wolny, S., and Daily, G. C. (2012). Integrating ecosystem-service tradeoffs into land-use decisions. *Proceedings of the National Academy of Sciences*, 109(19):7565–7570.

Gollin, D., Lagakos, D., and Waugh, M. E. (2014). The agricultural productivity gap. *The Quarterly Journal of Economics*, 129(2):939–993.

Green, R. E., Cornell, S. J., Scharlemann, J. P., and Balmford, A. (2005). Farming and the fate of wild nature. *science*, 307(5709):550–555.

Grossman, G. M. and Krueger, A. B. (1995). Economic Growth and the Environment. *The Quarterly Journal of Economics*, 110(2):353–377.

Hahn, J., Todd, P., and Van der Klaauw, W. (2001). Identification and estimation of treatment effects with a regression-discontinuity design. *Econometrica*, 69(1):201–209.

Hanna, R. and Oliva, P. (2015). Moving up the energy ladder: the effect of an increase in economic well-being on the fuel consumption choices of the poor in india. *American Economic Review*, 105(5):242–46.

Hansen, M. C., Potapov, P. V., Moore, R., Hancher, M., Turubanova, S., Tyukavina, A., Thau, D., Stehman, S., Goetz, S., Loveland, T. R., et al. (2013). High-resolution global maps of 21st-century forest cover change. *Science*, 342(6160):850–853.

Imbens, G. W. and Lemieux, T. (2008). Regression discontinuity designs: A guide to practice. *Journal of econometrics*, 142(2):615–635.

IPCC (2014). Climate change 2014: Synthesis report. Technical report, Intergovernmental Panel on Climate Change.

Jayachandran, S., de Laat, J., Lambin, E. F., Stanton, C. Y., Audy, R., and Thomas, N. E. (2017). Cash for carbon: A randomized trial of payments for ecosystem services to reduce deforestation. *Science*, 357(6348):267–273.

Jayne, T. S., Mason, N. M., Burke, W. J., and Ariga, J. (2018). Taking stock of Africa's second-generation agricultural input subsidy programs. *Food Policy*, 75:1–14.

Koch, N., zu Ermgassen, E. K., Wehkamp, J., Oliveira Filho, F. J., and Schwerhoff, G. (2019). Agricultural productivity and forest conservation: evidence from the brazilian amazon. *American Journal of Agricultural Economics*, 101(3):919–940.

Lambin, E. F. and Meyfroidt, P. (2011). Global land use change, economic globalization, and the looming land scarcity. *Proceedings of the National Academy of Sciences*, 108(9):3465–3472.

Masuda, Y. J., Castro, B., Aggraeni, I., Wolff, N. H., Ebi, K., Garg, T., Game, E. T., Krenz, J., and Spector, J. (2019). How are healthy, working populations affected by increasing temperatures in the tropics? implications for climate change adaptation policies. *Global Environmental Change*, 56:29–40.

McArthur, J. W. and McCord, G. C. (2017). Fertilizing growth: Agricultural inputs and their effects in economic development. *Journal of Development Economics*, 127:133–152.

OECD (2016). Statistical tables on aid to food and agriculture. Available at: `http://www.oecd.org/dac/stats/agriculture.htm`, Accessed on 9/11/2020.

Pan, Y. and Singhal, S. (2019). Agricultural extension, intra-household allocation and malaria. *Journal of Development Economics*, 139:157–170.

Pan, Y., Smith, S. C., and Sulaiman, M. (2018). Agricultural extension and technology adoption for food security: evidence from uganda. *American Journal of Agricultural Economics*, 100(4):1012–1031.

Pelletier, J., Ngoma, H., Mason, N. M., and Barrett, C. B. (2020). Does smallholder maize intensification reduce deforestation? evidence from zambia. *Global Environmental Change*, 63:102127.

Phalan, B., Green, R. E., Dicks, L. V., Dotta, G., Feniuk, C., Lamb, A., Strassburg, B. B., Williams, D. R., Zu Ermgassen, E. K., and Balmford, A. (2016). How can higher-yield farming help to spare nature? *Science*, 351(6272):450–451.

Rudel, T. K., Schneider, L., Uriarte, M., Turner, B. L., DeFries, R., Lawrence, D., Geoghegan, J., Hecht, S., Ickowitz, A., Lambin, E. F., et al. (2009). Agricultural intensification and changes in cultivated areas, 1970–2005. *Proceedings of the National Academy of Sciences*, 106(49):20675–20680.

Stern, D. (2004). The Rise and Fall of the Environmental Kuznets Curve. *World Development*, 32(8):1419–1439.

Stern, D., Michael, C. S., and Edward, B. B. (1996). Economic Growth and Environmental Degradation: The Environmental Kuznets Curve and Sustainable Development. *World Development*, 24(7):1151–1160.

Stevenson, J. R., Villoria, N., Byerlee, D., Kelley, T., and Maredia, M. (2013). Green revolution research saved an estimated 18 to 27 million hectares from being brought into agricultural production. *Proceedings of the National Academy of Sciences*, 110(21):8363–8368.

Takasaki, Y. (2006). A model of shifting cultivation: can soil conservation reduce deforestation? *Agricultural Economics*, 35(2):193–201.

Uganda Bureau of Statistics (2012). Villages: Uganda, 2009. Available at: http://purl.stanford.edu/vx912bz3835.

Uganda Ministry of Water and Environment (2017). State of uganda's forestry.

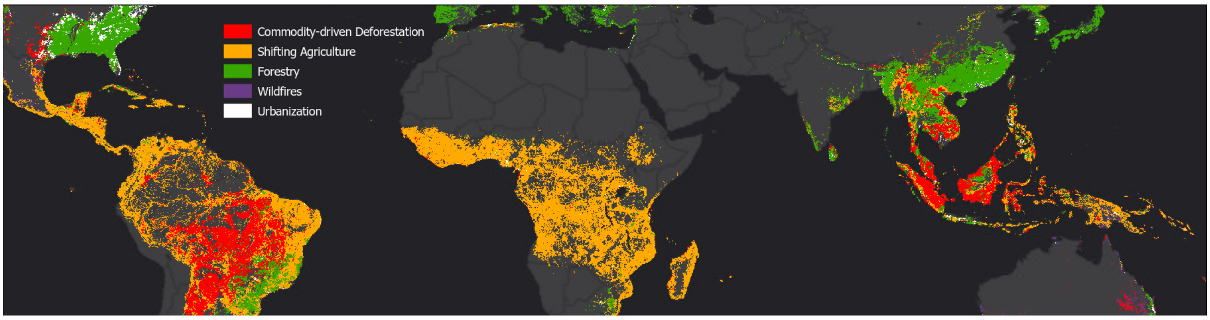
Ugandan Electoral Commission (2015). 2016 general election statistics. Available at: https://www.ec.or.ug/?q=content/2016-general-elections-statistics, Accessed on 11/11/2019.

UNFAO (2019). The Director-General's Medium Term Plan 2018-21 (reviewed) and Programme of Work and Budget 2020-21. Technical report, Food and Agriculture Organization of the United Nations. Available at: http://www.fao.org/3/my734en/my734en.pdf, Accessed on 9/11/2020.

Wilebore, B., Voors, M., Bulte, E. H., Coomes, D., and Kontoleon, A. (2019). Unconditional transfers and tropical forest conservation: evidence from a randomized control trial in sierra leone. *American Journal of Agricultural Economics*, 101(3):894–918.

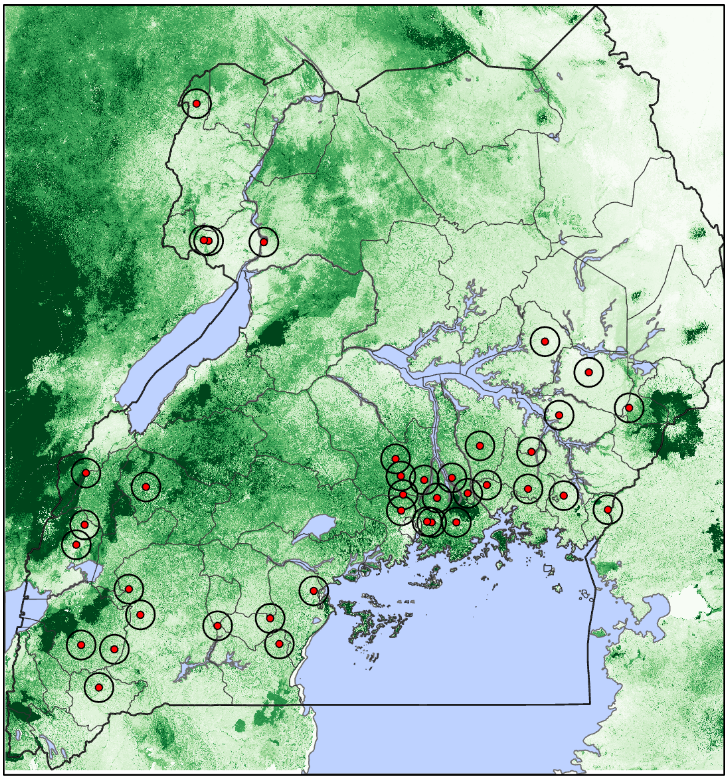
Figures

Figure 1: Global forest loss by dominant cause



Notes: This figure shows the dominant driver of forest loss in each 10 km X 10 km grid cell globally. The dominant driver of forest loss in Uganda, our country of study, is shifting agriculture. Data are obtained from Curtis et al. (2018).

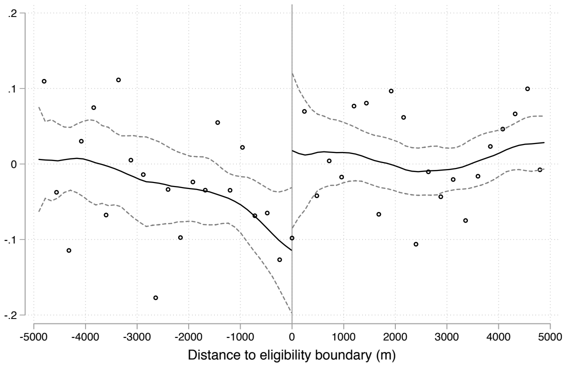
Figure 2: Brac branch locations in Uganda



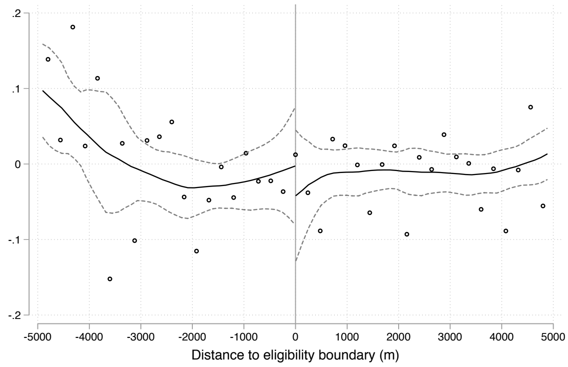
Notes: This figure shows the location of the Brac branches (red dots), the 12 km buffers covering the villages used in the analysis, as well as the baseline forest cover percentage (shade of green).

Figure 3: Regression discontinuity plots

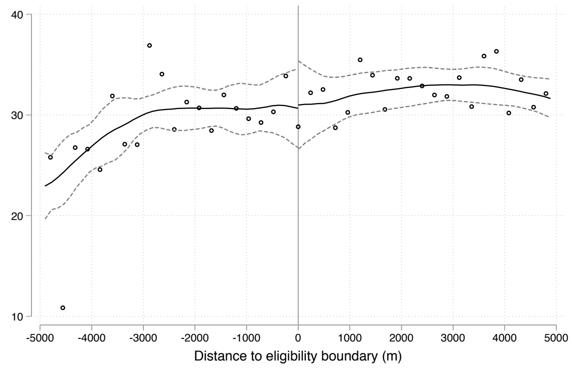
(a) Residualized Forest Loss - Treatment period



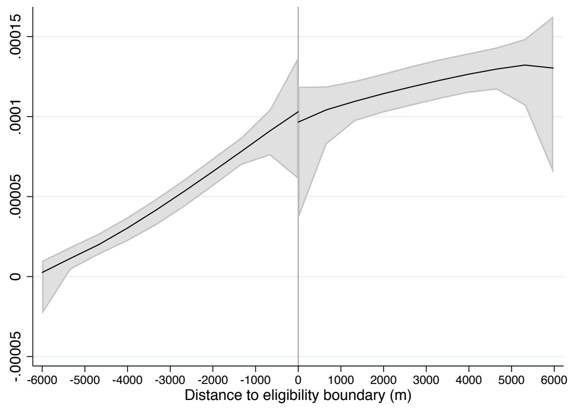
(b) Residualized Forest Loss - Pre-treatment period



(c) Baseline treecover



(d) Village location density



Notes: This figure presents the distribution of outcomes of interest plotted against the running variable of distance to the 6 km 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**
	(0.0685)	(0.0688)	(0.0792)	(0.0602)	(0.0556)
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	0.0335	0.0555	0.0300	0.0228
	(0.0689)	(0.0631)	(0.0825)	(0.0601)	(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	-0.147	-0.908	-0.135	-0.301
	(3.800)	(3.692)	(4.003)	(3.706)	(3.607)
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 inverse hyperbolic sine of annual forest loss during the program. The middle panel presents estimates on the average residualized inverse hyperbolic sine 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). Standard errors are clustered by BRAC branch. 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) Intercrop	(3) Crop Rotation	(4) Irrigation	(5) Weeding	(6) Fertilizer Use	(7) HYV Seeds	(8) Perennial crops
Program Eligible	0.0977***	0.0590*	0.0737***	0.0326***	0.0644**	-0.0171	-0.0440	-0.0264
	(0.0249)	(0.0308)	(0.0251)	(0.00829)	(0.0310)	(0.0161)	(0.0323)	(0.0351)
Obs	2912	2912	2912	2912	2912	2912	2912	2912
Control mean	0.0731	0.796	0.797	0.0266	0.693	0.0725	0.356	0.351

Notes: Presented are regression discontinuity estimates of program eligibility on the adoption of agricultural practices. Outcomes are indicator variables taking value of 1 if the household reports engaging in the particular activity or using the particular input. Columns (1) - (8) correspond to using manure, practicing intercropping, practicing crop rotation, using irrigation, practicing weeding, using purchased chemical fertilizer, using high yield variety seeds and growing perennial crops, respectively. 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: *** $p<0.01$, ** $p<0.05$, *$p<0.1$. Despite the different bandwidth choice and regression specification, these results are qualitatively similar to those reported in Pan et al. (2018), Table 3.

Table 3: Regression discontinuity estimates of program eligibility on agricultural incomes, land in cultivation, and agricultural engagement

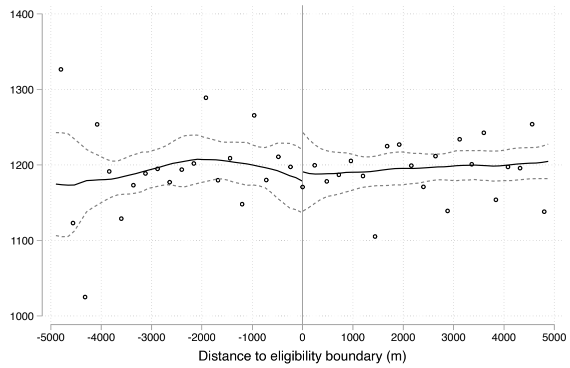
Outcome	(1) Revenue per acre	(2) IHS Cultiv. ag area	(3) Engage in ag (All)	(4) Engage in ag (Any)
Program Eligible	0.325*	0.0262	-0.0252	-0.0244
	(0.189)	(0.0514)	(0.0376)	(0.0340)
Obs	2843	2907	15186	3235
Control mean	11.59	1.414	0.666	0.686

Notes: Presented are regression discontinuity estimates of program eligibility on household agricultural outcomes. Columns (1) uses the inverse hyperbolic sine of revenue per acre as the outcome variable. Column (2) estimates the effect of program eligibility on the inverse hyperbolic sine of cultivated area. The mean of cultivated agricultural area is in acres. Columns (3) and (4) use indicators for whether someone reported working on household plots over the past 7 days. Column (3) uses all individuals (aged 5 and above) sampled while Column (4) uses household-level indicators taking a value of 1 if anyone in the household reports working in agriculture. 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.

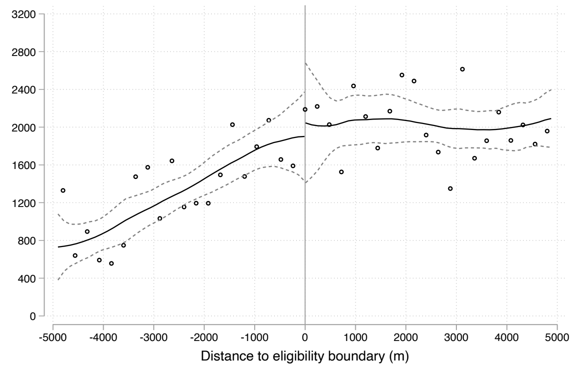
A Appendix - Additional Tables and Figures

Figure A.1: Geographic continuity by village distance

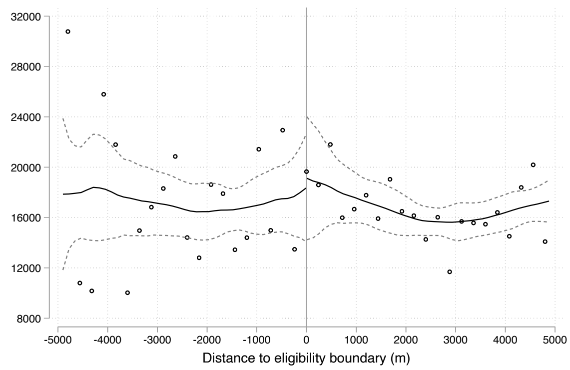
(a) Elevation (meters)



(b) Distance to nearest road (meters)



(c) Distance to nearest water source (meters)



Notes: This figure presents the distribution of geographic characteristics at village locations plotted against the running variable of distance to the 6 km eligibility boundary in meters. Panel (a) plots average village elevation in meters, panel (b) plots average distance to nearest road in meters, and panel (c) plots average distance to nearest water body (river or lake) in meters.

Table A.1: Estimates varying village radius

	(1)	(2)	(3)	(4)
Program Eligible	-0.0586	-0.133*	-0.158*	-0.137
	(0.0369)	(0.0685)	(0.0953)	(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. Standard errors are clustered at the BRAC branch level. Statistical significance denoted as follows: *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table A.2: 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
	(0.0834)	(0.0766)	(0.0758)	(0.0685)	(0.0673)	(0.0684)	(0.0627)
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: The outcome variable is the average residualized inverse hyperbolic sine of annual forest loss during the program. 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.Standard errors are clustered at the BRAC branch level. Statistical significance denoted as follows: *** p<0.01, ** p<0.05, * p<0.1

Table A.3: Regression discontinuity estimates by land suitable for Coffee

	(1) Full Sample	(2) Above Median	(3) Below Median
Program Eligible	-0.133*	-0.120	-0.125
	(0.0685)	(0.0901)	(0.0960)
Mean loss in control (ha/yr)	0.0678	0.0765	0.0594
Obs	308	162	146

Notes: Presented are non-parametric regression discontinuity estimates of program eligibility on residualized forest loss. The first column includes villages for all Brac branch locations. The second column limits the sample to villages near Brac branch locations more suitability for coffee and the third column limits the sample to villages near Brac branch locations that are less suitable for coffee. More vs less suitable is determined by being above or below the median. Standard errors are clustered at the BRAC branch level. Statistical significance denoted as follows: *** p<0.01, ** p<0.05, * p<0.1

Table A.4: Regression discontinuity sensitivity to omitting rainfall abnormalities

	(1) Full Sample	(2) Excl. Drought	(3) Excl. Low Rainfall
Program Eligible	-0.133*	-0.153**	-0.159**
	(0.0683)	(0.0715)	(0.0783)
Obs	1,540	1,408	1,254

Notes: Presented are non-parametric regression discontinuity estimates of program eligibility on annual residualized forest loss. Residuals are not averaged prior to estimating the discontunity, thus leaving us with village-by-year observations (instead of village averages). The first column includes villages-year observations with a bandwidth of 2 km and is analogous to our main result. We calculate annual rainfall anomalies for all 12 km buffers around Brac branch locations for 2000 - 2018 and classify drought years as those in which annual rainfall was 1.5 standard deviations below the local average and low rainfall years as those in which annual rainfall was 1 standard deviation below the local mean. We estimate our main effect omitting village-year observations with droughts (column 2) and omitting village-year observations with low rainfall (column 3). Standard errors are clustered at the BRAC branch level. The use of annual observations allows for standard clustering rather than nearest-neighbor clustering as in the pooled regressions. Statistical significance denoted as follows: *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table A.5: Cultivated land area as a function of household size

	(1) First Quartile (1-4 members)	(2) Second Quartile (5-6 members)	(3) Third Quartile (7-8 members)	(4) Fourth Quartile (9+ members)
Program Eligible	0.0129	0.0419	0.00328	0.275***
	(0.0826)	(0.0956)	(0.0634)	(0.0713)
Obs	805	877	640	585
Control mean	1.350	1.370	1.474	1.500

Notes: Presented are regression discontinuity estimates of program eligibility on the inverse hyperbolic sine of area cultivated. The full estimation sample is split by quartiles of household size with Column (1) representing the smallest quartile of household sizes and Column (4) the largest. 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 A.6: Regression discontinuity estimates of agricultural practices across multiple bandwidths

Bandwidth:	**2KM** (1)	**CCT** (2)	**1.5KM** (3)	**2.5KM** (4)	**3KM** (5)
	Manure Use				
Program Eligible	0.0977***	0.109***	0.111***	0.0846***	0.0726***
	(0.0249)	(0.0234)	(0.0232)	(0.0235)	(0.0207)
Obs	2912	2329	2231	3388	4054
	Intercropping				
Program Eligible	0.0590*	0.108***	0.0814**	0.0519*	0.0565**
	(0.0308)	(0.0395)	(0.0349)	(0.0269)	(0.0231)
Obs	2912	1561	2231	3388	4054
	Crop Rotation				
Program Eligible	0.0737***	0.0708**	0.0703**	0.0776***	0.0771***
	(0.0251)	(0.0286)	(0.0283)	(0.0239)	(0.0237)
Obs	2912	2126	2231	3388	4054
	Irrigation				
Program Eligible	0.0326***	0.0238***	0.0335***	0.0318***	0.0325***
	(0.00829)	(0.00805)	(0.00736)	(0.0101)	(0.0107)
Obs	2912	1091	2231	3388	4054
	Weeding				
Program Eligible	0.0644**	0.0692*	0.0682*	0.0659**	0.0635**
	(0.0310)	(0.0366)	(0.0378)	(0.0278)	(0.0258)
Obs	2912	2287	2231	3388	4054
	Chemical Fertilizer Use				
Program Eligible	-0.0172	-0.0187	-0.0236	-0.0191	-0.0192
	(0.0161)	(0.0165)	(0.0144)	(0.0165)	(0.0160)
Obs	2912	3367	2231	3388	4054
	HYV Seeds Use				
Program Eligible	-0.0441	-0.0466	-0.0433	-0.0322	-0.0236
	(0.0323)	(0.0336)	(0.0347)	(0.0311)	(0.0294)
Obs	2912	2749	2231	3388	4054
	Perennial Crops				
Program Eligible	-0.0264	-0.0278	-0.0292	-0.0299	-0.0199
	(0.0351)	(0.0346)	(0.0415)	(0.0314)	(0.0275)
Obs	2912	3076	2231	3388	4054

Notes: This table presents household level non-parametric RD estimates for each of agricultural practices reported in Table 2. CCT refers to the optimal bandwidth as proposed by Calonico et al. (2014). We include BRAC branch fixed effects and cluster standard errors at the BRAC branch level. Statistical significance denoted as follows: *** p<0.01, ** p<0.05, * p<0.1

Table A.7: Regression discontinuity estimates of household firewood use across multiple bandwidths

Bandwidth:	**2KM**	**CCT**	**1.5KM**	**2.5KM**	**3KM**
	(1)	(2)	(3)	(4)	(5)
			Firewood used for light		
Program Eligible	0.00261	0.00180	0.00206	0.00311	0.00329
	(0.00221)	(0.00265)	(0.00262)	(0.00206)	(0.00203)
Obs	3213	2371	2471	3719	4440
			Firewood used for cooking		
Program Eligible	-0.0491	-0.0499	-0.0460	-0.0503	-0.0405
	(0.0343)	(0.0322)	(0.0341)	(0.0316)	(0.0289)
Obs	3210	3603	2468	3717	4437

Notes: This table presents household level non-parametric RD estimates for each firewood variable across a variety of different bandwidths. CCT refers to the optimal bandwidth as proposed by Calonico et al. (2014). We include BRAC branch fixed effects and cluster standard errors at the BRAC branch level. Statistical significance denoted as follows: *** p<0.01, ** p<0.05, * p<0.1

Table A.8: Regression discontinuity estimates of program eligibility on post program forest loss (2013 - 2016)

Bandwidth:	**2 km**	**CCT**	**1.5 km**	**2.5 km**	**3 km**
	(1)	(2)	(3)	(4)	(5)
Program Eligible	0.0522	0.0442	0.0147	0.0729	0.107
	(0.199)	(0.212)	(0.221)	(0.176)	(0.162)
Mean loss in control (ha/yr)	0.248	0.268	0.286	0.283	0.274
Obs	222	188	163	278	322

Notes: Presented are non-parametric regression discontinuity estimates of program eligibility across different bandwidths on forest loss after the BRAC program ended. The outcome is average residualized asinh of annual forest loss after the end of the program (2013 - 2016). The sample is limited to BRAC Branches that did not participate in the staggered phaseout program. Standard errors are clustered at the BRAC branch level. Statistical significance denoted as follows: *** $p<0.01$, ** $p<0.05$, * $p<0.1$