Community-Driven Deforestation? Experimental Evidence from a Rural Development Program in West African Drylands

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Community-Driven Deforestation? Experimental Evidence from a Rural Development Program in West African Drylands*

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Abstract: The effects of poverty reduction on the environment are the subject of an ongoing debate. In particular, it is unclear how deforestation is affected by large-scale development programs, which improve economic welfare and affect other channels that may also be related to deforestation. We study this issue in the context of a nationwide Community-Driven Development (CDD) program that was randomly assigned to villages in rural Gambia. We combine high-resolution satellite data with program implementation information and detailed household-level data to estimate the effect of the program on forest loss and to investigate underlying mechanisms. In areas with meaningful initial forest cover, we find that forest loss is substantially larger in program villages than in control villages. Our estimates imply that forest loss increased by around 11 percent in areas immediately surrounding program villages, over a five-year period after the program ended. If spatial spillovers into control villages are accounted for, we find even larger effects, suggesting that the CDD program accounts for more than a quarter of the forest loss in the years 2011 to 2015 around villages in our sample. This treatment effect is driven by villages with limited access to markets (as measured by poor road infrastructure). Our results also provide suggestive evidence for a relationship between agricultural productivity and deforestation. To further investigate possible channels at the household level, we examine whether the CDD program affected outcomes that could link the CDD to deforestation. Among the correlates of deforestation suggested by related literature, we only find moderate evidence for increases in economic welfare.

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1 Introduction

Forests are a key global public good. They absorb carbon dioxide from the atmosphere, are biodiversity hotspots, and supply water and oxygen (Baccini et al., 2012; Pan et al., 2011; Myers et al., 2000). At the same time, forests potentially suffer from the incompatibility of incentives that leads to the *tragedy of the commons* (Dietz et al., 2003), resulting in overexploitation. Indeed, deforestation has reached record levels over the last three decades (Hansen et al., 2013), and the depletion of common forest is particularly severe in poor areas (Barrett et al., 2011). In light of this, it is important to understand local determinants of deforestation, in particular in poor regions of the world. One link that remains unclear, but is of particular relevance, is the effect of improved living conditions in rural areas on environmental degradation (Alix-Garcia et al., 2013; World Bank, 2007). On the one hand, with improving welfare the demand for resource-intensive goods might increase, adding pressure to the exploitation of local forest and related environmental goods, particularly if access to markets is limited (Alix-Garcia et al., 2013; Baland et al., 2010). On the other hand, higher opportunity costs of extractive activities and increases in agricultural productivity might reduce the demand for new cultivation areas and forest resources (Cuaresma et al., 2017; Foster and Rosenzweig, 2003; Baland and Platteau, 1996).

We contribute to the understanding of deforestation by providing causal evidence regarding the relationship between rural development programs and deforestation in West Africa. To the best of our knowledge, this is the first experimental study of the unintended effects of a rural development program on forest loss using a large-scale randomized controlled trial.[1] The typical rural development program aims at improving living conditions. Because of the above-mentioned arguments regarding the effect of improvements in living conditions on deforestation, development programs may unintentionally affect deforestation. Using detailed satellite data, we analyze the impact of a nationwide

[1]The recent contributions by Jayachandran et al. (2017) and Wilebore et al. (2018) provide experimental evidence for programs that explicitly aim at reducing deforestation, while we study a broader development program. The existing experimental studies are also of a smaller scale.

Community-Driven Development (CDD) program on forest loss in rural villages in The Gambia that were randomly chosen as beneficiaries. We find that the average treatment effect is an increase in deforestation. Our most conservative estimates imply that 11% of the overall forest loss occurring within 1 km of the treatment villages resulted from the program (the number is only slightly larger, 12%, when we consider the 5 km radius around treatment villages).

A large body of literature has previously analyzed the relationship between economic development and environmental degradation. Initial work focused primarily on the *environmental Kuznets curve* (Stern, 2004; Grossman and Krueger, 1995). This hypothesis suggests a non-monotonic relationship in which income growth initially increases environmental degradation until a turning point is reached, at which the trend reverses. A different view is taken by the *poverty-environment hypothesis*, which suggests that environmental degradation is poverty induced, and therefore income growth at lower levels of income will imply environmental improvement (Baland and Platteau, 1996). The empirical evidence is mixed. Using historical data for rural India, Foster and Rosenzweig (2003) find that income growth increases demand for forest goods and is associated with growth of cultivated forests. Simorangkir (2017) uses matching methods to estimate the effect of a conditional cash transfer program in Indonesia and finds that recipient villages experienced less forest loss. These results contrast with Zwane (2007), who finds that income is positively correlated with land clearing in Peru, and Baland et al. (2010), who show that improvements of household living standards in Nepal is associated with increased demand for firewood. A meta-analysis of more than one hundred spatially explicit econometric studies about the determinants of deforestation concludes that the effect of a change in rural income is unclear (Busch and Ferretti-Gallon, 2017).

Given that household income and the use of forest are likely to be jointly determined, obtaining unbiased estimates was an issue for most previous studies. A recent study by Alix-Garcia et al. (2013) uses quasi-experimental techniques exploiting the discontinuity in the eligibility criteria of a large-scale conditional transfer cash program in Mexico (PROGRESA/Oportunidades) to deal with endogeneity. They show that poverty alleviation associated with this program causes an increase in forest loss. Our study is similar to Alix-Garcia et al. (2013), as we take the Gambia CDD program as an exogenous shock to provide causal estimates of the unintended secondary effects of rural development on forest loss. The benefit of our setting is that the program was implemented as a nationwide randomized controlled trial at the village level, and therefore we can provide well-identified estimates of the average treatment effect.

We find a positive treatment effect of the CDD program on forest loss in treatment villages located in areas with meaningful initial forest cover. The magnitude of the effect is large and economically and statistically significant, explaining approximately 11% of the overall forest loss around treatment village in the post-program period. If spillover effects from treated villages into neighboring villages are also accounted for, the estimated effect is even larger, suggesting that the CDD program is responsible for over one quarter of the overall forest loss occurring around the program villages after 2011. The effect is heterogeneous with respect to pre-program village characteristics. In their meta-analysis, Busch and Ferretti-Gallon (2017) identify poverty, population and transportation infrastructure as the main correlates of deforestation, and we use these dimensions to guide our analysis of heterogeneous effects. We find that program-induced deforestation within the immediate surroundings of the village was largest in treatment villages farther from roads. This result is in line with the heterogeneity described in the study by Alix-Garcia et al. (2013), which suggests that in areas with better infrastructure and better access to markets the environmental effect is spatially more dispersed.

Our study also relates to the literature on agricultural production and deforestation. The *Borlaug hypothesis* (Borlaug, 2007; Angelsen and Kaimowitz, 2001) suggests that increasing agricultural productivity, through modern production technologies, decreases the demand for cropland and thus deforestation. However, increased agricultural productivity could also have the opposite effect. New technologies may increase expected profits, create economies of scale, promote farming, and thus increase the demand for cropland. Only scarce empirical evidence exists about the relationship between agricultural productivity and deforestation. Abman and Carney (2018) find a reduction in deforestation caused by a fertilizer subsidy program in Malawi. Assunção et al. (2016) show evidence that an increase in productivity associated with rural electrification affects deforestation in Brazilian counties depending on initial farmland area. Given that several of the village-level projects implemented as part of the Gambia CDD program were related to agricultural production, we provide suggestive evidence on how these investments affect deforestation.[2] Treatment villages that spent a larger share of their budget on projects associated with increasing agricultural productivity, such as, the purchase of draft animals, fertilizer and non-mechanized tools, exhibit stronger deforestation in areas farther away from the village center. This suggests an expansion of agriculturally used land as one factor driving the observed forest loss.

We use post-program surveys to shed light on household channels underlying our results. These data are collected five to seven years after the program, and the estimates should be interpreted as medium-term treatment effects. We take outcome variables analyzed in previous studies on the determinants of deforestation and build indices to test hypotheses about the mechanisms through which the program may affect deforestation. The results suggest that treatment villages experienced modest improvements in economic welfare, which can be related to deforestation as discussed above. However, we find no evidence of a significant medium-term treatment effect of the CDD program on other variables identified in the literature as determinants of deforestation, such as consumption of resource-intensive goods and population increase. Even though the CDD program had the specific goal of changing village institutions and increasing social capital, we do not find evidence that these changes have taken place either.[3] Implicit in our study of channels is also a contribution to the literature on the effects on CDD programs more generally (e.g., Labonne and Chase, 2011; Casey et al., 2012; Mansuri and Rao, 2012; Wong, 2012).

[2] Given that the type of each particular village-level project was not randomly allocated, this part of our analysis must be regarded as circumstantial, rather than experimental, evidence. However, our difference-in-differences estimates control for several sources of endogeneity.

[3] In a related paper (Heß et al., 2018) we show that the networks of economic interactions in treatment villages differ from those in control villages half a decade after the program; however, there is no evidence that suggests this to be directly related to our findings regarding deforestation.

This paper also contributes to the literature by expanding the regional scope of previous studies. Thus far, most empirical studies have focused on specific cases of deforestation in the large rainforests of Brazil and Indonesia as well as in other countries in Latin America and Asia (Busch and Ferretti-Gallon, 2017). Ours is one of the first empirical studies of deforestation in the West African drylands. Dryland biomes are particularly exposed to climate change, as extended droughts and global warming increase the risk of land degradation (Bastin et al., 2017; Dietz et al., 2004). The Gambia, located at the frontier of desertification in the Sahel, is a very relevant study setting. Political actors in The Gambia increasingly recognize the importance of conservation efforts (FAO, 2011). The problems of forest degradation and desertification are identified as key issues in the Gambian national climate change adaptation strategy (UNDP, 2015). The Gambian government is a member of the *Great Green Wall of the Sahara and the Sahel Initiative*, a flagship initiative to combat climate change and desertification (UNCCD, 2016).

The rest of the paper is organized as follows. The next section presents background information about deforestation in The Gambia and about the CDD program. Section 3 describes the data used in the empirical analysis and Section 4 presents our empirical strategy, based on the experimental design of the Gambian CDD program, and a series of results and robustness checks for the treatment effect of the program on deforestation. In Section 5 we use post-treatment surveys to explore underlying mechanisms by analyzing household-level outcomes that could link the CDD program to deforestation. A final section concludes.

2 Background and Setting

2.1 Forest and Deforestation in The Gambia

The Gambian territory is part of the Sudano-Sahelian agro-ecological zone, characterized by a long dry season from October to June. Its biome is part of the West African drylands, one of the most degraded dryland areas in the world (Dietz et al., 2004). The National

Forest Assessment 2008-2010 (FAO, 2011) estimated that around 40% of the country was covered by forest, composed mainly of deciduous trees but also patches of evergreen forest and mangroves. Several sources report rapid deforestation in recent years. FAO (2011) estimates a reduction of 19% in forest and other wooded land between 1998 and 2010. Hansen et al. (2013) report a net decrease of 11,100 hectares of forest in The Gambia between 2001 and 2013, resulting in a reduction of around 11% with respect to the forest cover reported in 2000. In the case of the mangrove forest, a priority conservation area, Carney et al. (2014) report a 35% reduction between 1986 and 2010, mostly in the Southern border with Senegal's Casamance region.

FAO (2011) suggests that the main factors driving deforestation are agricultural expansion, bush fires (mainly due to clearing of new land), droughts and population expansion. Population expansion is particularly relevant given the reliance on firewood for cooking. Although the Gambian Government aims to attenuate this source of forest degradation by allowing only deadwood to be collected, these official regulations seem to have had limited success in preventing deforestation by individual actors. For instance, in the data from the Gambian Census of 2003, 98% of rural households declared using firewood as their main cooking fuel.

Even though the state is the owner of most of the forest in The Gambia (FAO, 2011), customary land tenure arrangements within and between villages primarily determine the access to and the use of forest (Schroeder, 1999; Freudenberger, 1993). Therefore, the village chief and other members of the founding families of each village have the *de facto* right of using the forest. Forest areas which lie between adjacent villages often lack clear territorial demarcations. Rather, there exist informal rules which cannot be perfectly enforced (Freudenberger, 1993). Since the 1990s, over 350 villages have implemented a Community Forest Management scheme, which comprises the step-wise transfer of legal rights over forest resources from the government to local communities (Camara et al., 2011). Those villages must explicitly define demarcations of forest customarily shared with their neighboring villages.

2.2 The Gambia Community-Driven Development (CDD) Program

The context of our study is a randomly allocated CDD program in small, rural villages of The Gambia. CDD programs are a major modality of the bottom-up approaches that involve local communities in project design and implementation, which international donors, multilateral organizations, and national governments have increasingly favored in the last two decades (Wong, 2012; Mansuri and Rao, 2012).

The Gambia CDD program was implemented between 2008 and 2010, and was mainly financed by the World Bank. It targeted a population estimated at 435,000 people or about 50 percent of the Gambian rural population (World Bank, 2006). The program was implemented in eligible villages belonging to 88 wards located in the six rural Local Government Areas (LGAs) of The Gambia.[4] Only communities with a population between 100 and 10,000 inhabitants (according to the 2003 National Census) were eligible for the project. As a way of improving the targeting of the project, village-level indicators of poverty were calculated using data from the Gambia Census 2003, and the two thirds of villages ranked the poorest in each ward were selected as eligible for the project. Within the group of eligible villages, around half of the villages (495) were randomly assigned to treatment—i.e., received funding for projects of their choice. The remaining eligible villages (435) did not receive funds for village-level projects and did not follow the project selection process. The random assignment was stratified at the ward level, and around half of the eligible villages within each ward were selected to receive the funds.

The project was demand driven. Villages in the treatment group were—subject to some restrictions—free to choose any type of project to invest the CDD funds in.[5] In order to select the final village-level project, each village had to follow a long decision-making process involving several local and external actors (GoTG, 2006). The budget allocated

[4]Wards are a smaller geographical division that tend to be homogeneous in geographical terms but heterogeneous in socio-cultural terms. Typical wards comprise 12 villages (25^{th} percentile) to 28 villages (75^{th} percentile) and 6 to 13 villages that were eligible for the CDD.

[5]In this aspect, the Gambian CDD program differs from the typical CDD modality, in which communities must decide whether to apply for a project and compete for resources.

to treatment villages was a base of US$10,000, plus an extra budget determined based on population and poverty levels. The average disbursement for the 495 treatment villages was around US$11,500 (current values). This translates into per-household allocations that are roughly equivalent to one-half of an annual per capita income in The Gambia. The villagers were expected to contribute at least 10% of the project costs in cash and/or in-kind. The most commonly implemented village-level projects were: farm implements and inputs, village-level infrastructure, water pumps, and milling machines (Table A8 provides more information about the village-level projects implemented in the Gambian CDD program). Though donors imposed some environmental safeguard policies regarding project choice, forest preservation was not among the explicitly stated objectives of the Gambian CDD program (World Bank, 2006).[6]

While the recipient villages were informed about their treatment status in 2008, the disbursements were often made much later. The administrative records do not report the exact disbursement dates, but they do report a village's project appraisal date for 90% of treatment villages. Project appraisals were carried out by the CDD program's administrative staff after the village collectively picked a shortlist of projects but before disbursements were made. All recorded appraisal dates fall after the onset of the rainy season in 2008. More than 50% indicate a date after the onset of the rainy season 2009. The earliest effects for the majority of villages should thus be expected from 2010 onwards.

3 Data

3.1 Forest Cover and Forest Change Data

Our forest-related outcome measures are based on the Global Forest Change Database 1.3 (GFCD henceforth), which contains worldwide information about forest cover in 2000 and forest change between 2001 and 2015. These data were first described by Hansen

[6]For instance, the acquisition of chainsaws was not allowed within the CDD program.

et al. (2013) and have been updated since the first version.[7] The data are based on images from the Landsat satellites. Images captured during tree growing season for each region are used to generate pixel-level data at a 1 arc-second resolution, which corresponds to a relatively high resolution with a pixel size of approximately 30×30 meters at the equator.[8] Trees are defined in the GFCD as "all vegetation taller than 5 m in height" and forest loss as "a stand-replacement disturbance". The latter implies that a pixel of forest is considered to be lost when there is a complete removal of the tree canopy.[9] These data have a superior resolution to most other alternative sources of georeferenced forest change data (such as the FAO data described by Keenan et al. 2015), and have the advantage of providing a series of annual forest change indicators.

Several recent econometric studies have used GFCD to analyze the relationship between deforestation and socioeconomic factors, either in specific countries (Burgess et al., 2017; Abman and Carney, 2018; Burgess et al., 2012) or in cross-country analyses (Cuaresma et al., 2017; Leblois et al., 2017). These studies use these data aggregated at large administrative units, thus not taking full advantage of the high resolution of the data. Fewer studies use the GFCD data in comparatively smaller units. BenYishay et al. (2016) take cells of $5\,\text{km} \times 5\,\text{km}$ to analyze the impact of Chinese development projects in Tanzania and Cambodia and Alix-Garcia et al. (2015) take Thiessen polygons[10] around localities that implemented payments for ecosystem services in Mexico.

In the present study we take advantage of the high resolution of the data by aggregating the data in different ways. First, we aggregate pixels in buffers of 1 km and 5 km radii around each village to obtain village-level forest and deforestation measures. As a second method, we use the villages' Thiessen polygons as the unit of aggregation. Figure 1a shows the 1 km buffers around all villages in The Gambia (indicating villages that were

[7] The data are publicly available at University of Maryland's earthenginepartners.appspot.com and at globalforestwatch.org.

[8] In the case of The Gambia, the area of each pixel is 755 square meters or 0.075 hectares.

[9] More details about the data can be found in the supplementary materials of Hansen et al. (2013).

[10] Thiessen polygons (also called Voronoi tessallation) partition the map into regions of varying size, assigning each point on the map to the nearest village centroid. This process achieves a complete partitioning of the map, where polygons regions with more villages are smaller and polygons in regions where villagers are further apart are comparatively large.

eligible for the CDD program) and Figure 1b shows the Gambian territory divided into Thiessen polygons.

An advantage of using buffers is that the size of the covered area is constant across villages. A disadvantage is however that buffers can overlap, which complicates estimating aggregate effects. Due to the overlaps, estimated magnitudes based on the buffers are not comparable to other estimates of deforestation, as overlapping areas will be counted for multiple villages (i.e., the sum of all buffer areas does not correspond to the size of the country). It is thus important to always relate estimated effect sizes to the magnitude of deforestation within the same unit of aggregation for the control group. Larger and smaller buffers have different advantages. When using the smaller 1 km buffers, there is little spatial overlap with other villages compared to the 5 km buffers, though in rural, remote villages, the 1 km buffers may not large enough to capture the entire range of influence of a village. This is important because the agriculturally used lands are usually found in a circular area around the villages that can easily exceed the 1 km radius. Deforestation resulting from more extensive agriculture is thus best captured in the 5 km buffer. Thiessen polygons on the other hand do not overlap, but have the disadvantage of being highly heterogeneous in size in The Gambia.[11] Estimates for aggregate deforestation based on Thiessen polygons count each pixel once (for the nearest village) and are thus comparable to other measures of aggregate forest loss. Yet, the heterogeneity in polygon area makes the average treatment effect interpretation in our main specification less intuitive.

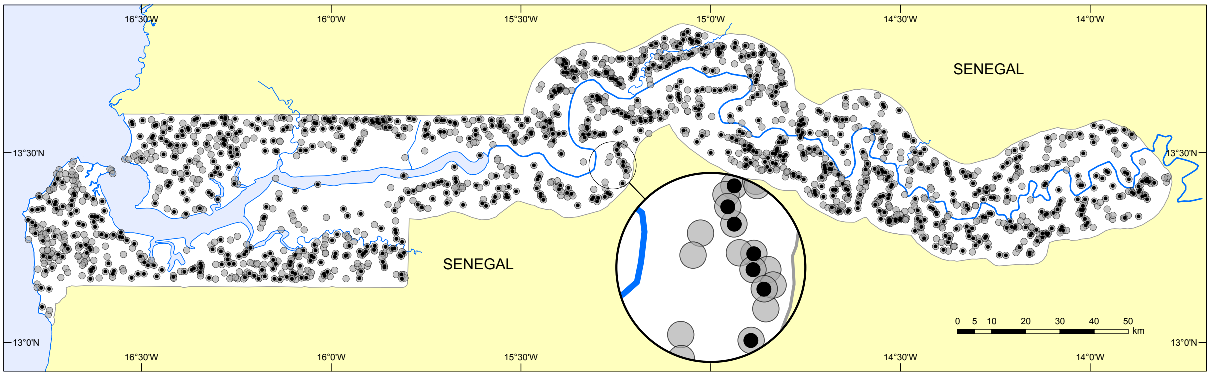
In our analysis we thus always present results for both buffer sizes and the Thiessen polygons. For identifying the average treatment effect on forest loss in the immediate surroundings of individual villages, we consider the 1 km buffer the most reliable measure. The larger 5 km buffers provide the benefit of capturing forest further away at the cost of increased noise and overlapping buffers, which renders the calculation of aggregate effects

[11] The distribution of Thiessen polygon sizes in our high forest cover sample varies from 44 ha. to 5674 ha., with the 25^{th} percentile being 302 ha. and the 75^{th} percentile being 857 ha.

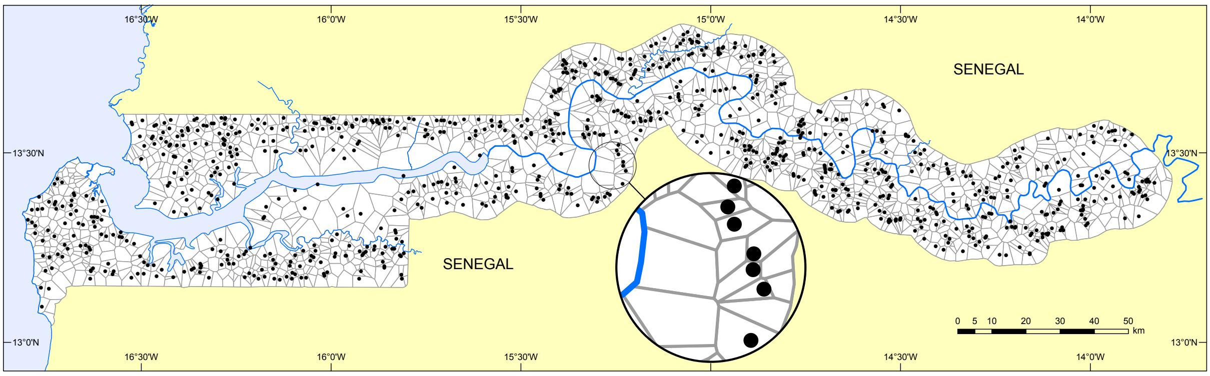
based on this specification less intuitive. The non-overlapping polygons provide the best means for estimating spillovers and quantifying the aggregate treatment effect.

Figure 1: Aggregation Levels of Forest Cover and Forest Change Used in the Empirical Analysis

(a) 1 km buffers



(b) Thiessen polygons



Notes: Dots represent villages that were eligible for the CDD program. Empty buffers/polygons represent settlements that were not eligible for the CDD program.

The high resolution of the GFCD data allows us to also capture forest at low densities. This is of fundamental importance in semi-arid drylands, such as most of The Gambia, where most forest cannot be considered dense by international standards, yet carries ecological importance (Bastin et al., 2017). However, the GFCD has been recently criticized by Bastin et al. (2017) for underestimating forest cover in dryland biomes. In order to check the accuracy of the GFCD in the area of study, we compare these data with manually coded tree cover densities from Bastin et al. (2017). To this end, we extract data on 188 0.5-hectare plots within a 350 km × 90 km rectangle centered on The Gambia from the Bastin et al. (2017) data set and plot these tree cover measures against the forest density recorded by the GFCD for those locations (see Appendix Figure A4).

There are good reasons to expect some differences between the forest cover assessments in these two data sets. First, the GFCD uses satellite imagery from 15 years before the images underlying the data by Bastin et al. (2017). Second, there is spatial uncertainty, as we could only match the plots from Bastin et al. (2017) to the GFCD-pixels with an accuracy of about 50-100 m. Nonetheless, the baseline forest data from the GFCD correlates reasonably well with with the manually coded canopy densities from Bastin et al. (2017). Additionally, this manually coded plot-level data contains a binary classification into forest and non-forest land use. In the 106 sample plots that Bastin et al. (2017) coded as non-forest, the median forest density according to the GFCD is 5.57%, while in the 82 plots that are coded as forest the median forest density in the GFCD is 11.76%. This is clear evidence that the GFCD data captures relevant aspects of the forest density variation we intend to measure. However, the GFCD systematically records much lower densities. Therefore, we consider the 2000 forest cover reported by GFCD as a lower bound of the true forest cover.

In order to further investigate the accuracy of the GFCD data, we also verified some of the data in the field, by visiting a small number of villages where recent forest loss has been recorded.[12] Our interviews with villagers tended to confirm the accuracy of GFCD data.

According to the GFCD, during the 2001-2015 period there was an average annual forest loss of 1,200 hectares in The Gambia. Appendix Figure A5 shows that behind this mean lies a volatile rate of forest loss that varies substantially across years, ranging from 250 hectares lost in 2003 to more than 3,500 hectares in 2002. This volatility is likely to be driven by changes in local conditions. For instance, the peak in forest loss in 2002 is mostly explained by a large fire in one national park (Kiang West National Park) at the end of 2001 (Sonko et al., 2002).

[12]For these field tests we visited four villages in the West Coast Region and the Lower River Division in October 2016. We identified prominent features from the GFCD in these villages, such as large areas of recent forest loss, and inquired about them with villagers knowledgeable about the local forests (such as the village chief or representatives from local forestry groups). We first asked villagers to identify significant forest losses before revealing our data. All significant losses identified by the villagers also appear in GFCD, though the exact timing of events was not always clear. In almost all cases such losses were attributable to bushland and loose forest being cleared for cultivation.

The sample used in the empirical analysis is based on the villages eligible for the CDD program. There are 930 eligible villages in total, 495 treatment villages and 435 control villages. In order to focus on rural locations, we exclude 81 villages located in the peri-urban zone of the Kombo districts (indicated in Figure 2) and around Gambia's capital Banjul, the most populated area in the country, as well as seven other towns defined as urban settlements according to the Census 2003. By not including these villages, we relate to the literature on the determinants of deforestation in rural areas, which differ from urban locations in many ways.[13] There are 22 villages (11 control and 11 treatment villages) that are not included in the sample because we do not have precise information about their geographical location.[14] Therefore, our *full* data consist of a sample of 820 eligible villages, 433 treatment villages and 387 control villages.

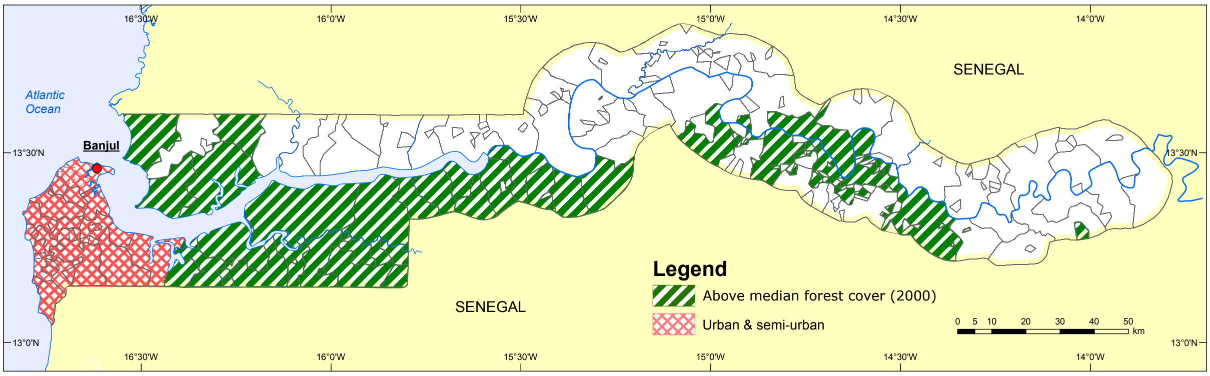
Even though the GFCD reports that almost 80% of The Gambia is covered with at least some forest, densities are very low. About 97% of all pixels have a forest cover of 20% or less, and half of the pixels have less than 10% forest cover. Mechanically, with little initial forest, there is little scope for further forest loss. Indeed, most forest loss in our data takes place in wards with relatively high initial forest cover. This is evidenced by the fact that the 402 villages (211 treatment and 191 control) located in wards with above-median forest cover at baseline (the median is 7.6% forest cover) are responsible for 85% of the country-wide forest loss in our data. Accordingly, our empirical analysis focuses on this *high forest cover* sub-sample. The location of these wards is indicated in Figure 2.

The characteristics of the GFCD data in the 402 villages that we use for our main empirical analysis are described in Table 1, where mean values are shown for the treatment and control group. There are no statically significant differences in the baseline forest cover between the treatment and the control group. The mean value of the baseline forest

[13]The 46 treatment villages in the Kombo districts chose a very different set of projects than those in rural areas: 62% of them implemented non-agricultural projects and only 8% invested in agricultural inputs. Column 1 Table A9 shows that the effect of the CDD program on deforestation in Kombo districts is the opposite to the rest of the country.

[14]Column 2 of Table A9 shows that the main results are not affected if the wards in which villages lie for which coordinates are not available are removed from the sample.

Figure 2: Baseline Forest Cover in The Gambia (2000)



Notes: Own calculation based on GFCD data. The area in green is for wards with above-median baseline forest cover (2000) outside of the urban and semi-urban areas.

cover (i.e., for the year 2000) in the 1 km and 5 km buffers and the Thiessen polygons are all very similar, at around 9% to 10%. The 1 km buffers contain very similar shares of forest cover as the 5 km buffers, suggesting relatively small shares of area covered by the settlements in our sample.[15]

Table 1 also shows the average forest loss during the pre-CDD program period (2001-2007) recorded by the GFCD. Given the skewness of the forest loss data, we use a logarithmized dependent variable specification for our empirical analysis. To calculate the logarithm in spite of some observations with zero forest loss, we add a very small constant (the area of a single 30 m $\times$ 30 m pixel). This approach is discussed in much greater detail in Section 4.1. In Appendix B we show that our main results do not depend on this transformation and remain qualitatively comparable when other alternatives are used, such as the inverse hyperbolic sine transformation or the untransformed loss. As indicated in Table 1, in control villages during the seven years preceding the program, logarithmized forest loss in the 1 km buffers is on average 0.53 (corresponding to $\exp(0.53) \approx$ 1.7 hectares, i.e., 0.5% of the buffer area). This number rises to 0.96 (2.6 hectares $\approx$ 0.4% of the average polygon area) for the polygons and to 3.97 (53 hectares $\approx$ 0.7% of the buffer area) for the 5 km buffers. The pre-treatment difference in forest loss between treatment and control villages is never statistically significant.

[15]We do not have precise information about the area covered by the village settlements. In a sub-sample of 60 villages where we were able to measure it, the median area is 0.88 km^2 (Jaimovich, 2015), which is 28% of the area within the 1 km buffer.

Table 1: Balance of Pre-Treatment Village Characteristics

	mean		difference		
	(1) control	(2) treated	(3) raw	(4) cond.	(5) p-value
Panel A: Forest Characteristics					
% forest cover in 2000 within 1 km	9.29	9.17	−0.126	−0.193	0.40
% forest cover in 2000 within 5 km	10.36	10.23	−0.127	−0.204	0.22
% forest cover within polygons	10.34	10.14	−0.199	−0.276	0.30
log(forest loss (ha.) in 2001-2007 within 1 km)	0.53	0.40	−0.130	−0.121	0.26
log(forest loss (ha.) in 2001-2007 within 5 km)	3.97	3.91	−0.058	−0.049	0.45
log(forest loss (ha.) in 2001-2007 in polygon)	0.96	0.85	−0.108	−0.106	0.42
Panel B: Geographic Characteristics					
distance to road (km)	4.24	4.69	0.446	0.299	0.29
distance to river (km)	10.18	10.03	−0.150	−0.147	0.61
villages within 1 km	0.82	0.85	0.031	0.044	0.65
CDD eligible villages within 1 km	0.35	0.43	0.076	0.084	0.20
villages within 5 km	13.80	13.59	−0.208	−0.121	0.80
CDD eligible villages within 5 km	6.74	6.62	−0.127	−0.039	0.89
area of the polygon (ha.)	681.25	671.70	−9.554	−22.708	0.66
Panel C: Census 2003 Characteristics					
population	329.22	346.17	16.946	13.436	0.64
poverty index	0.66	0.67	0.006	0.007	0.47
ethno-linguistic fractionalization	0.24	0.27	0.031	0.036	0.06
share Fula	0.21	0.25	0.045	0.039	0.20
share Mandinka	0.51	0.43	−0.081	−0.074	0.03
share Wollof	0.07	0.07	−0.004	−0.004	0.82
share Jola	0.12	0.15	0.026	0.020	0.07
share born in different village	0.13	0.14	0.003	0.003	0.81

Notes: Columns 1 and 2 display the means of each variable in the respective treatment group. Sample sizes are 191 and 211 communities respectively. Column 3 shows the raw difference in means, while column 4 shows the conditional difference after controlling for ward fixed effects. Columns 5 shows the p-value of a test for no difference in means, controlling for ward fixed effects. The data underlying Panel A stems from the GFC database. Panel B uses data from the Gambian Census 2003. Panel C is based on our own calculations.

3.2 Additional Data

We have extensive information about the implementation of the CDD program. In addition to the treatment status of all program villages, we have information about the village-level projects implemented by each treatment village, including the types of projects, year and amount of the related disbursement and the contribution from the villagers. We were able to corroborate these data in the field for around 10% of the program villages and found that the administrative records are highly accurate.[16]

[16] For a related project (Heß et al., 2018), we visited around 80 treatment and control communities in 2014, for data collection and piloting of questionnaires.

In order to match the data related to the implementation of the CDD program, as well as other data relevant for the analysis, with the data from GFCD, we put together a geo-referenced dataset of all settlements in The Gambia. Our main source for village centroid coordinates is a dataset collected by JICA (2003). These data contain the coordinates of all villages registered in the Gambian Census 2003, which we were able to match to our data using village names and districts. In ambiguous cases we additionally relied on GPS coordinates taken during various surveys to complement our database, including our own fieldwork and the Integrated Household Survey 2015 (IHS 2015 henceforth). Through this process we reliably identified coordinates of 95% of villages listed in the Gambian Census 2003. Among villages which were eligible for the CDD program this rate is 97%.[17]

From these village-level geodata we derive further geographic characteristics of the Gambian villages for our analysis (described in Table 1, Panel B). Control villages in the sample used for the empirical analysis are located on average 4.2 km from a paved road and 10.2 km from the Gambia River. We also calculated the number of nearby villages for each village. While within the 1 km buffer there are on average 0.35 neighboring villages eligible for the CDD program, this increases to 6.74 eligible villages within the 5 km buffers. This variable has a large dispersion, as some villages are relatively isolated while others are clustered together. This is reflected in the large variation in the size of the Thiessen polygons, which have an average area of 633 hectares and a standard deviation of 595 hectares, with some polygons being as large as 5,674 hectares. Treatment and control villages have no significant difference in means in any of these variables (Table 1, column 5).

The main source for additional pre-CDD program village-level data is the Gambia National Census 2003. This was also the source used to identify eligible villages for the program and to implement the randomization of treatment. Table 1, Panel C describes variables from this source for the high forest cover sample. Despite the fact that only

[17]In some cases we found discrepancies between the information from different sources. 12% of the centroids reported by JICA (2003) where located at more than 1 km from the centroids calculated using the IHS 2015 data. Column 3 of Table A9 shows that the main results are similar, and even larger in magnitude, when we drop districts from the sample that have more than two villages for which the distance between centroids from the two datasets exceeds 1 km.

the poorest villages in each ward were eligible for the program, we still observe large heterogeneity in the poverty index.[18] Despite its small size, The Gambia exhibits large variation in the fractionalization of ethnic groups within small geographical units (which makes the country different from others in West Africa, as described by Arcand and Jaimovich 2014). The ethno-linguistic fractionalization (ELF) index of the villages ranges from zero (complete homogeneity) to 0.80, with a mean of 0.24. The average shares of the main ethnic groups in the villages (Mandinka, Fula, Wollof and Jola) are close to the national shares in rural areas. The composition of the village groups change slowly over time, as only an average of 13% of people were born outside the village. In most variables from the Census 2003, the difference in means between the control and the treatment group is not statistically significant at conventional levels, with the exception of the ELF and some ethnicity shares. We have also observed this imbalance in other sub-samples of treatment villages (Heß et al., 2018), and our results hold irrespective of whether or not we control for these.

In order to test some of the channels through which the CDD program may affect deforestation we will use some additional data sources for post-program characteristics, like the Census 2013 and the IHS 2015, which are further described in Section 5.

4 Empirical Strategy and Results

As described in Section 1, previous studies have shown that rural development programs can decrease or increase the rate of deforestation, and the overall effect is a priori ambiguous. Therefore, the existence and sign of an effect of the CDD program remains an empirical question. We take advantage of the experimental design of the Gambia CDD program to explore its potential effects on forest change. We interpret the results as average treatment effects, because compliance to treatment was very high. According to

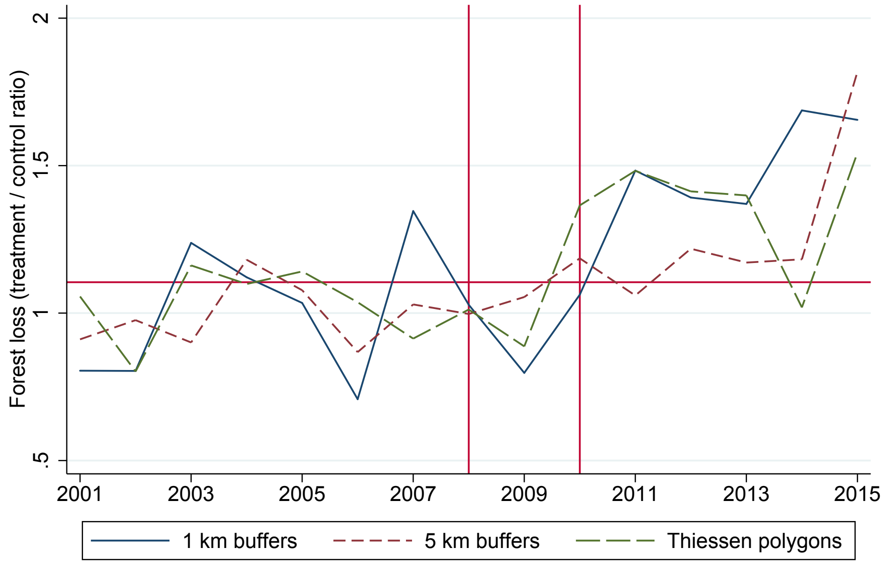
[18]The poverty index is the average of four variables: the share of villagers who do not know how to read and write; the share of villagers without access to electricity (either directly or through a generator); the share of villagers without access to private toilets; and the share of villagers without access to an improved source of water.

the project's disbursement records, only 15 villages in the full sample were assigned to treatment and did not implement the CDD program (this is 3% of treatment villages) and only two control villages did implement it according to records. As forest preservation, or any related outcome, was never an explicit goal of the CDD program, all results must be interpreted as secondary unintended effects.

In Figure 3 we use the raw data from the high forest cover sample to plot the ratio of total annual forest loss in treatment and control villages in the 1 km buffers, 5 km buffers and the Thiessen polygons. For comparison, the straight horizontal line depicts the ratio of the number of treatment relative to control villages, which is slightly above one because the randomization procedure assigned one more village to the treatment than to the control group in wards with an odd number of eligible villages. A visual inspection of the changes in the forest loss ratios provides first graphical evidence of the treatment effect. Before 2010, the ratios fluctuate around the horizontal line, implying that pre-treatment forest loss does not systematically differ between the control and treatment group, supporting the evidence presented in Table 1. After all treatment villages implemented the program, i.e., after 2010, the ratios drift upwards and lie almost entirely above the horizontal line, suggesting that deforestation is larger in treatment villages than in control villages.[19] For example, the ratio for the 1 km buffers after 2010 is on average 1.5, which implies that $\frac{1.5}{1.5+1} = 60\%$ of all deforestation inside 1 km buffers around eligible villages occurred in treatment villages, in spite of treated villages only making up 52% of the sample. The following section will provide estimates of forest loss based on a difference-in-differences analysis that exploits the experimental setup further.

[19] Appendix Figure A6 shows that the changes in the ratios after treatment are also present if the full sample of 820 eligible villages is considered.

Figure 3: Ratio of Forest Loss in Treatment and Control Villages



Notes: The straight horizontal line represents the ratio of the number of treatment communities relative to the number of control communities. The three plotted lines indicate the ratios of the total area deforested around treatment communities over the total area deforested around control communities. Each line represents this ratio using a different village-level aggregate: deforestation within the 1 km buffer, the 5 km buffer, and deforestation in the Thiessen polygon. The two vertical lines indicate the start and the end date of the CDD program implementation phase (2008-2010).

4.1 Difference-in-Differences Estimates for the Village-Level Average Treatment Effect

We base our empirical strategy on the following difference-in-differences specification:

$$
\begin{aligned}
\log(\text{loss}_{vwt}) = &\beta_1 \times \text{treatment}_v \times \mathbb{1}(t \in [2008, 2010]) \\
&+\beta_2 \times \text{treatment}_v \times \mathbb{1}(t \in [2011, 2015]) \\
&+\alpha_v + \delta_{wt} + \varepsilon_{vwt}, \qquad (1)
\end{aligned}
$$

where $\log(\text{loss}_{vwt})$ is the logarithm of forest loss in year t and village v located in ward w, derived from the GFCD, and treatment_v is a binary indicator for the CDD treatment. We distinguish between the implementation period (2008-2010) and post-program period (2011-2015).[20] Consequently, estimates for β_1 and β_2 can be interpreted as semi-

[20] As discussed in Section 2.2, villages received the first program disbursement at different times within the 2008-2010 period. Nevertheless, the CDD program implementation process included a series of pre-disbursements activities (meetings, debates, project selection, etc.) which started in 2008. Accordingly, in the baseline specification we consider the years 2008 to 2010 as the implementation period and the following years as the post-program period.

elasticities.[21] We also include village fixed effects, α_v, which control for any unobserved time-invariant pre-treatment imbalance, and ward-year fixed effects, δ_{wt}, which account for the stratification of the randomization and for time-variant unobserved shocks at the ward level (such as bushfires, rainfall, prices, etc.).[22] [23]

In order to obtain the logarithm in spite of observations with zero loss, we add a small constant of 0.075 hectares (the area of a single pixel), which is a natural choice as it is the smallest increment for forest loss measures derived from the GFCD. The frequency of village-years with zero forest loss for the 1 km buffer, 5 km buffer, and the Thiessen polygons are 67%, 20%, and 60% respectively. Given that our data straightforwardly provides us with a default choice for the magnitude of a small constant to be added, using the logarithmized dependent variable is our preferred specification. In Appendix B we show that our results remain qualitatively comparable when using the inverse hyperbolic sine (Table A11) or the untransformed area of forest loss (Table A12) as the dependent variable.[24]

The standard errors are estimated allowing for correlation of the regression model error at the village level. In Table A13 we show that this inference method is the most conservative out of a large set of alternative inference methods, including inference based on

[21] In our empirical analysis we concentrate on the combination of intensive and extensive margins of deforestation. While it is possible that the project could affect the propensity to deforest, we find no significant treatment effects at the extensive margin, as shown in column 6 of Table A9.

[22] In the Census 2013, 47 villages were assigned ward identifiers that are different from those registered in the CDD program administrative data (usually belonging to neighboring wards). Given that the stratification of the treatment assignment was done using the latter data, we always use the ward identifiers from the CDD program data for our empirical analysis. As a robustness check, in column 4 of Table A9 we show that the main results do not change if wards containing more than two villages with contradictory identifiers are excluded from the sample.

[23] One possible concern here is the large number of estimands in our regression, implied by the two sets of fixed effects (15 years $\times$ 36 ward fixed effects, and 402 village fixed effects). However, our point estimates remain remarkably stable, and are in fact slightly larger, if we use the post-double-LASSO method (Belloni et al., 2014) to reduce the number of parameters by selecting which fixed effects to include (see Table A10).

[24] The results based on the inverse hyperbolic sine transformation and the untransformed variable are in fact statistically stronger. Note, however, that using the inverse hyperbolic sine transformation also does not solve the conceptual problem of elasticities being undefined at zero. In addition, for the inverse hyperbolic sine transformation the scale of the outcome matters and there is no natural default for this. But both the inverse hyperbolic sine transformation and the $\log(y + c)$ avoid dropping all observations with zero forest loss. Thus, we prefer to stick to the specification using $\log(y + c)$, since our context provides us with a natural choice for c and since in our case this specification seems more conservative in terms of statistical significance.

ward-level cluster-robust standard errors, randomization inference, modeling the spatial dependence using Conley inference, and various bootstrapping alternatives. For convenience, Tables 2 and 3 each show p-values based on one suitable alternative inference method — ward-level cluster-robust standard errors in the case of Table 2 and randomization inference for Table 3.

Table 2: Difference-in-Differences – Estimation Results

	(1) $\log(\text{loss}^{1km})$	(2) $\log(\text{loss}^{5km})$	(3) $\log(\text{loss}^{poly})$
implementation (2008-10) × treatment	-0.011	0.028	-0.003
	(0.85)	(0.66)	(0.96)
	[0.82]	[0.56]	[0.96]
post–program (2011-15) × treatment	0.111	0.117	0.079
	(0.07)*	(0.06)*	(0.28)
	[0.04]**	[0.02]**	[0.33]
village fixed effects	✓	✓	✓
ward × year fixed effects	✓	✓	✓
observations	6030	6030	6030
implementation-phase (2008-10) control mean annual loss (ha.)	0.625	11.05	1.084
post-program (2011-15) control mean annual loss (ha.)	0.266	8.393	0.683
total CDD-attributable loss (ha.) 2011-15 in area under consideration (1 km, 5 km, or polygons)	44.2	1189.2	62.6
total loss in treatment villages (ha.) 2011-15	378.8	9654.5	832.3
total loss in all villages (ha.) 2011-15	632.8	17669.9	1484.8

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. p-values in parentheses are based on cluster-robust standard errors, allowing for clustering of the model error at the village level (villages are the unit of treatment assignment). Standard errors underlying the p-values in square brackets allow for clustering at the ward-level (wards were the stratum for the treatment randomization). Our sample comprises 36 wards. The reference period is 2001-2007. Units of observation are village-years between 2001 and 2015. The dependent variable is the logarithm of the area of forest loss per year plus a very small constant (the area of a single 30 m × 30 m pixel) to deal with observations where the area of forest loss is zero. Ward-year and village fixed effects are included in all specifications.

The resulting difference-in-differences estimates are shown in Table 2. The average treatment effect is large, statistically significant and concentrated in the post-program period (confirming the graphical evidence from Figure 3). The estimates for the 1 km and 5 km buffers (column 1 and 2) indicate that deforestation in treatment villages is 11%-12% larger than in control villages during this period.[25] The treatment effect estimate in the specification using the Thiessen polygons is slightly smaller at 8%, and statistically insignificant.

[25]The results are not driven by a particular ward or village, as the coefficient estimates change very little compared to Table 2, when iteratively excluding individual wards from the sample. Additionally, Table A9 shows that the results are robust to excluding wards with missing or imprecise information.

To calculate the total program-induced forest loss in hectares we need to take into account that the estimation is based on a log-level model specification. We implement an approach that accounts for the transformed dependent variable and produces an estimate of the total area of forest loss attributable to the CDD, as is explained in detail in Appendix C.[26] Our estimates using the 1 km buffers indicate that in total over the years 2011-2015, about 44.2 hectares were deforested as the result of the CDD program (Table 2, column 1).[27] As discussed in Section 3.1, some of the 1 km buffers are overlapping and thus this number cannot be interpreted in isolation but has to be put in relation to the total amount of lost forest summed across these buffers. This comparison suggests $\frac{44.2}{378.8} \approx 12\%$ of forest loss within the 1 km buffers of treatment villages in high forest wards is due to the CDD program.

In Appendix B we document the robustness of our findings to various alternative specifications, such as (i) using loss in levels or the inverse hyperbolic sine of the dependent variable instead of the logarithm (Table A11 and Table A12), (ii) using pre- and post-treatment aggregates instead of annual data (Table A14), and (iii) using different variants of cluster-robust standard errors, randomization inference, or the bootstrap (Table A13). Furthermore, in Table A15 we show that the point estimate of the average treatment effect in the full sample, i.e., including wards with low levels of initial forest cover, is positive but imprecisely estimated and not statistically significant. For the three outcome measures, forest loss within 1 km, 5 km and the Thiessen polygons, the point estimate implies an increase in deforestation of around 4%, 5% and 1% respectively in the full sample.

The fact that sizable treatment effects are visible only for the years after the end of the CDD program is the result of multiple factors. First, during the implementation period many villages had not yet received the funds or were just beginning to set up the project

[26] Another benefit of this method is that it straightforwardly extends model specifications that are non-linear in the covariates, such as the spillover effect model which we explore in Section 4.2 where treatment does not only affect the treated observation itself but also its neighbors.

[27] This compares to an estimate of 31.1 hectares lost that would be calculated in a naïve approach that does not take into account that the model uses a logarithmized dependent variable: The point estimate of our model implies that treatment villages deforest 11% more than control villages. The average control village deforests 0.266 ha. per year. Summing up $11\% \cdot 0.266$ over 211 villages and 5 years yields the naïve result of 31.1 ha. Appendix C shows that Jensen's inequality implies that this estimate of deforestation is incorrect and likely smaller than the actual deforestation.

sites.[28] Second, even after the disbursements are made, effects may take several years to materialize because deforestation takes time. A third factor is measurement. According to the definition of forest loss in the GFCD, a pixel is considered as deforested only once all trees are removed and since annual satellite images from the growing season are used for the GFCD, post-growing season tree removal would be recorded in the following year. Therefore, even if deforestation started to increase with the implementation of the program, the eventual forest loss will only be recorded in the data some years later. This measurement effect is exacerbated by the fact that the algorithm used in the GFCD is different for the periods 2001-2010 and 2011-2015, as for the latter the methodology is more precise. While the difference in the algorithm is not a problem for our empirical strategy, as the change is captured by the ward-year fixed effects, it might imply that deforestation in the years around the CDD program implementation are more likely to be registered after 2011. The timing of the treatment effect is confirmed in the results presented in Table A16, which shows the estimates of Equation (1) using interactions of the binary treatment variable with a dummy for each year after the beginning of the CDD program. At this high level of disaggregation, almost all coefficients are individually insignificant, however for years between 2011 and 2015 they tend to be larger and are consistently positive, while coefficients for earlier years are smaller and of mixed signs.

As a way to provide evidence in support of the parallel trends assumption underlying the difference-in-differences model, we expand the results by including the interaction of treatment with dummies for the years before the CDD program was implemented, and using the initial year (2001) as the reference. Therefore, the interactions with years 2002 to 2007 are *placebo tests*. The pre-treatment coefficients in Table A17 are smaller than coefficients for the post-program period, mostly insignificant, and of mixed signs.

[28]Recall that based on the administrative records, the earliest effects for the majority of villages should be expected from 2010 onwards, as discussed in Section 2.2.

4.2 Spillover Effects

The experimental design of the CDD program also allows us to identify spillover effects from treated villages to other villages. This is because the number of neighboring treatment villages is independent of unobservable characteristics, as long as the number of neighboring CDD-eligible villages is controlled for. The type of spillovers we estimate here are not necessarily deliberate interactions between villages or externalities across villages. Rather, we define as spillovers any effect a neighboring village's treatment has on deforestation within the 1 km buffer, 5 km buffer, or Thiessen polygon.

The existence of spillover effects is tested using an extension of the difference-in-differences model with the following specification:

$$\begin{aligned}
\log(\text{loss}_{vwt}) = {} & \beta_1 \times \text{treatment}_v \times \mathbb{1}(t \in [2008, 2010]) \; + \; \beta_2 \times \text{treatment}_v \times \mathbb{1}(t \in [2011, 2015]) \\
& + \gamma_1 \times N^{2\,km}_{Treat,v} \times \mathbb{1}(t \in [2008, 2010]) \; + \; \gamma_3 \times N^{2\,km}_{Treat,v} \times \mathbb{1}(t \in [2011, 2015]) \\
& + \gamma_2 \times N^{2\,km-5\,km}_{Treat,v} \times \mathbb{1}(t \in [2008, 2010]) \; + \; \gamma_4 \times N^{2\,km-5\,km}_{Treat,v} \times \mathbb{1}(t \in [2011, 2015]) \\
& + \eta_1 \times N^{2\,km}_{v} \times \mathbb{1}(t \in [2008, 2010]) \; + \; \eta_3 \times N^{2\,km}_{v} \times \mathbb{1}(t \in [2011, 2015]) \\
& + \eta_2 \times N^{2\,km-5\,km}_{v} \times \mathbb{1}(t \in [2008, 2010]) \; + \; \eta_4 \times N^{2\,km-5\,km}_{v} \times \mathbb{1}(t \in [2011, 2015]) \\
& + \alpha_v + \delta_{wt} + \varepsilon_{vwt}, \qquad (2)
\end{aligned}$$

where $N^d_{Treat,v}$ counts the treatment villages within distance d around village v, so that γ captures the spillover effects. Additionally we control for N^d_v, which counts all villages that were eligible for the CDD program within distance d. We consider spillovers from neighboring villages located within 2 km and those located more than 2 km and less than 5 km away. Since multiple villages can share the same neighboring villages, the model error in the above equation must be assumed to be spatially correlated. Clustering standard errors at the village level is thus not appropriate. We implement Conley inference, taking 10 km as a cutoff (note that villages which are farther apart cannot share a third village within their 5 km perimeter). Alternatively, we compute p-values using randomization inference, which also accounts for the cross-village dependence structure of $N^d_{Treat,v}$.[29]

Table 3: Spillover Effects

	(1) $\log(\text{loss}^{1km})$	(2) $\log(\text{loss}^{5km})$	(3) $\log(\text{loss}^{poly})$
implementation (2008-10) × treatment	0.010	0.028	0.002
	(0.87)	(0.68)	(0.98)
	[0.88]	[0.75]	[0.98]
implementation (2008-10) × N^{2km}_{Treat}	0.078	0.001	0.076
	(0.18)	(0.99)	(0.18)
	[0.28]	[1.00]	[0.31]
implementation (2008-10) × $N^{2km-5km}_{\text{Treat}}$	0.078	0.016	0.024
	(0.03)**	(0.78)	(0.53)
	[0.06]*	[0.83]	[0.58]
post–program (2011-15) × treatment	0.126	0.148	0.110
	(0.04)**	(0.02)**	(0.15)
	[0.07]*	[0.06]*	[0.17]
post–program (2011-15) × N^{2km}_{Treat}	0.083	0.118	0.076
	(0.17)	(0.11)	(0.29)
	[0.20]	[0.17]	[0.31]
post–program (2011-15) × $N^{2km-5km}_{\text{Treat}}$	0.050	0.121	0.111
	(0.23)	(0.02)**	(0.02)**
	[0.26]	[0.04]**	[0.02]**
observations	6030	6030	6030
control mean annual loss (ha.) post-program	0.266	8.393	0.683
mean N^{2km}_{Treat}	0.852	0.852	0.852
mean $N^{2km-5km}_{\text{Treat}}$	2.69	2.69	2.69
total CDD-attributable loss (ha.) 2011-15 in area under consideration (1 km, 5 km, or polygons)	167.0	7179.8	430.4
total loss in all villages (ha.) 2011-15	632.8	17669.9	1484.8

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. p-values in parenthesis are based on standard error estimates allowing for spatial correlation and auto-correlation of the model error. We implement Conley inference, taking 10 km as spatial cutoff, while leaving the temporal autocorrelation unrestricted. We chose 10 km because two villages that are farther apart than 10 km cannot have a common third village within their 5 km perimeter. p-values in square brackets are based on randomization inference as described in Footnote 29 and in Heß (2017). The reference period is 2001-2007. Units of observation are village-years between 2001 and 2015. The dependent variable is the logarithm of the area of forest loss per year plus a very small constant (the area of a single 30 m × 30 m pixel) to deal with observations where the area of forest loss is zero. Estimated coefficients are based on Equation (2). Ward-year and village fixed effects are included in all specifications.

Table 3 displays the result of the estimation of spillover effects based on Equation (2). The results indicate that there are treatment spillover effects, as the estimates for γ are

[29] The vector of treatment assignment enters the regression equation in three ways: each village's individual treatment status, treatment$_v$, and the number of treated villages within the 2 km radius, $N^{2\,km}_{Treat,v}$, and 2-5 km ring, $N^{2\,km-5\,km}_{Treat,v}$. The number of treated villages within certain radii is correlated for villages that are close enough to each other. When conducting randomization inference we can compute these count variables for each re-drawn alternative treatment assignment and thereby automatically account for the design-based spatial correlation between villages.

large and in several cases statistically significant, especially for the post-program period. Mechanically, spillover effects are more likely to be relevant for the larger buffers or the polygons, which is also confirmed by the results. The results for the 5 km buffers, for example, indicate that in addition to the direct effect of the treatment (15% increase in deforestation), there is a 12% increase (p-value=0.11) in deforestation for each treatment village located within 2 km from the centroid and a 12% increase in deforestation for each treatment village located in the 2-5 km ring.

While the estimation sample only includes villages that were eligible for the CDD program, treatment effects can also spill over into non-eligible neighboring villages. The aggregated spillover effect estimates should thus be considered lower bounds, as only spillover effects into villages that are in our sample are captured.

Thus we find that the total effect of the CDD program on deforestation is even larger when spillover effects are considered. This is because in addition to the direct treatment effect on each village itself, this specification captures the treatment effect on pixels that deforested because they are close to neighboring treatment villages. Estimates for the total loss due to the CDD program are reported at the bottom of Table 3. Overall, the total estimated forest loss that is attributable to the CDD program exceeds 25% of the total forest lost that occurred in eligible villages between 2011 and 2015, in all specifications.

4.3 Heterogeneous Effects by Pre-Treatment Characteristics

In Section 3 we describe how eligible villages are a heterogeneous group in many aspects. In this section we study if the impact of the CDD program on deforestation differs with the pre-treatment characteristics described in Table 1. We focus on the variables identified as correlates of deforestation in the meta-analysis of Busch and Ferretti-Gallon (2017), namely population, poverty and transportation costs. To measure poverty we use the poverty index used by the CDD program as eligibility criterion. We follow Alix-Garcia et al. (2013) and using distance to roads as a proxy for transportation costs.[30] As Burgess et al. (2012) and Alesina et al. (2014) have highlighted the importance of ethnic diversity in the context of deforestation, we also explore heterogeneous effects according to the village-level ethno-linguistic fractionalization (ELF).

We test for treatment effect heterogeneity using village-level pre-program characteristics based on the Census 2003 and geographic features. We focus only on the estimates using the 1 km buffers around the village centroid (results for other outcomes are in Table A18). Estimation is based on the following specification:

$$\begin{aligned}\log(\text{loss}_{vwt}) = &\beta_1 \times \text{treatment}_v \times \mathbb{1}(t \in [2008, 2010]) + \beta_2 \times \text{treatment}_v \times \mathbb{1}(t \in [2008, 2010]) \times high_v \\ &+\beta_3 \times \text{treatment}_v \times \mathbb{1}(t \in [2011, 2015]) + \beta_4 \times \text{treatment}_v \times \mathbb{1}(t \in [2011, 2015]) \times high_v \\ &+\alpha_v + \delta_{wt} + \varepsilon_{vwt}, \end{aligned} \tag{3}$$

where $high_v$ is a binary median-split indicator for various village-level characteristics (i.e., the indicator is equal to one if the value is above the median for that variable), so that β_2 and β_4 capture the differential treatment effects for villages with different characteristics. α_v and δ_{wt} are village and ward-year fixed effects.

The results are presented in Table 4. The only heterogeneous effect that is statistically significant is with respect to distance to roads. In villages that have worse transportation infrastructure, the treatment effect is large and positive, while it is small and insignificant for villages closer to roads. This result is in line with the findings of Alix-Garcia et al. (2013), suggesting that more connected villages, which are likely to have better access to markets, are able to mitigate the pressure on land and forest resources resulting from development programs.[31]

[30] Our measure of distance to road considers the distance to the two main paved roads connecting the country along the northern and southern riverbanks and a few other major paved roads through The Gambia that mainly connect cities in Senegal.

[31] Column 5 of Table A9 shows that our main results are not only driven by villages that are isolated (without neighbors in 1 km), but rather because they are located in areas with poor infrastructure.

Table 4: Heterogeneous Effects by Pre-Treatment Variables

village-level split:	population	poverty	distance to road	ELF
	(1)	(2)	(3)	(4)
	log(loss^{1km})	log(loss^{1km})	log(loss^{1km})	log(loss^{1km})
implementation (2008-10) × treatment	-0.023	-0.088	-0.018	0.022
	(0.78)	(0.28)	(0.84)	(0.77)
implementation (2008-10) × treatment × $high_v$	0.039	0.156	0.023	-0.071
	(0.73)	(0.16)	(0.84)	(0.52)
post–program (2011-15) × treatment	0.092	0.126	-0.027	0.067
	(0.30)	(0.14)	(0.76)	(0.42)
post–program (2011-15) × treatment × $high_v$	0.042	-0.030	0.274	0.090
	(0.74)	(0.80)	(0.03)**	(0.45)
split indicator×period	✓	✓	✓	✓
observations	6030	6030	6030	6030
control mean annual loss (ha.) (low)	0.227	0.193	0.235	0.261
control mean annual loss (ha.) (high)	0.309	0.339	0.299	0.271
mean of village-level var. (low)	0.16	0.58	0.91	0.057
mean of village-level var. (high)	0.52	0.74	6.15	0.45

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. p-values in parentheses are based on cluster-robust standard errors, allowing for clustering of the model error at the village level. The reference period is 2001-2007. Units of observation are village-years between 2001 and 2015. The dependent variable is the logarithm of the area of forest loss per year plus a very small constant (the area of a single $30\,\mathrm{m} \times 30\,\mathrm{m}$ pixel) to deal with observations where the area of forest loss is zero. The results show the interaction of the difference-in-differences interaction terms with a binary indicator dividing the sample according to the median of each pre-treatment village-level variable. Ward-year and village fixed effects are included in all specifications.

4.4 Project Choice and Deforestation

The binary CDD program treatment indicator hides substantial heterogeneity in the types of projects implemented by each village. Through the participatory selection process, most villages selected two or three projects in which to invest the program's resources (in a few cases there were up to seven projects per village).

Among the treatment villages in our sample of high forest cover wards, the average village invested three quarters of the budget in agriculture-related projects, but again there is heterogeneity between villages.[32] Table A8 lists the different types of implemented projects and how we classify them. Most villages with agriculture-related projects used the funds to acquire tools, inputs or draft animals. Other common uses for the funds were milling machines or animals for fattening. Non-agricultural projects are generally very heterogeneous but fall broadly into three groups. First, infrastructure projects used the funds to build physical structures such as roads, bridges or buildings. A second group

[32] The sample contains 211 treatment villages, including 4 villages for which we do not have information about implemented projects.

of projects used the funds to pay for maintaining or creating water points, such as taps or hand pumps. Lastly, in several remaining projects, smaller mobile assets, such as boats, carts, or furniture were bought.

Different types of village-level projects are likely to have different impacts on deforestation. In order to study how deforestation differs between villages that chose different types of projects, we expand the difference-in-differences estimation strategy in the following way:

$$\begin{aligned}\log(\text{loss}_{vwt}) = &\beta_1 \times \mathbb{1}(t \in [2008, 2010]) \times \text{share}_v^{\text{agricultural}} \\ &+\beta_2 \times \mathbb{1}(t \in [2008, 2010]) \times \text{share}_v^{\text{non-agricultural}} \\ &+\beta_3 \times \mathbb{1}(t \in [2011, 2015]) \times \text{share}_v^{\text{agricultural}} \\ &+\beta_4 \times \mathbb{1}(t \in [2011, 2015]) \times \text{share}_v^{\text{non-agricultural}} \\ &+\alpha_v + \delta_{wt} + \varepsilon_{vwt},\end{aligned} \tag{4}$$

where $\text{share}_v^{\text{agricultural}}$ and $\text{share}_v^{\text{non-agricultural}}$ are the CDD budget shares village v allocated to agricultural and non-agricultural projects respectively. For treatment villages these share variables sum up to one, for control villages both are zero.

The village-level projects themselves were not randomly assigned, but were chosen through a long decision-making process in each village. Hence, the estimates cannot be interpreted as causal treatment effect estimates but as correlations. Nonetheless, our specification allows us to account for some potential sources of bias. The village fixed effects, α_v, account for any time-invariant village-level unobserved heterogeneity and the ward-year fixed effects, δ_{wt}, for unobserved heterogeneity at that level (e.g., prices or weather shocks). Still, there might be some within-ward time-variant unobserved heterogeneity that could be correlated with project choice.

The estimates of the treatment effect by project type are shown in Table 5. The results suggest that the different types of projects have different effects on deforestation. Non-agricultural projects are correlated with post-program deforestation in the 1 km buffer,

Table 5: Difference-in-Differences Estimates by Village-Level Project Types

	(1) log(loss^{1km})	(2) log(loss^{5km})	(3) log(loss^{poly})
implementation (2008-10) × share agricultural	-0.037	0.089	0.023
	(0.56)	(0.21)	(0.75)
implementation (2008-10) × share non-agricultural	0.038	-0.137	-0.143
	(0.70)	(0.20)	(0.28)
post–program (2011-15) × share agricultural	0.044	0.151	0.069
	(0.49)	(0.03)**	(0.38)
post–program (2011-15) × share non-agricultural	0.296	0.007	0.057
	(0.03)**	(0.96)	(0.73)
observations	6030	6030	6030
control mean annual loss (ha.) post-program	0.359	9.151	0.789

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. p-values in parentheses are based on cluster-robust standard errors, allowing for clustering of the model error at the village level. The reference period is 2001-2007. Units of observation are village-years between 2001 and 2015. The dependent variable is the logarithm of the area of forest loss per year plus a very small constant (the area of a single $30\,\text{m} \times 30\,\text{m}$ pixel) to deal with observations where the area of forest loss is zero. The results show the interaction of the implementation phase and post-program indicators with a variable indicating the CDD budget shares allocated by each village to agricultural and non-agricultural projects. Ward-year and village fixed effects are included in all specifications.

while agricultural projects correlate with post-program deforestation in the 5 km buffers. This is likely to be driven by the fact that a good share of the agricultural land is typically located outside the immediate surroundings of the village. The strong deforestation in the larger buffers around villages that chose agricultural projects is consistent with villages expanding the area of agriculturally used land in response to treatment. This would be in contradiction with the *Borlaug hypothesis*, which predicts that increasing agricultural productivity decreases the demand for cropland. Such an effect could be the result of villages clearing land to take advantage of higher expected profits or of potential economies of scale associated with the new inputs.[33]

5 Possible Household Channels

In order to shed light on potential mechanisms as they are hypothesized in the literature, we test whether the CDD has effects on outcomes that possibly connect the program to

[33]In order to further explore what kinds of projects correlate with the treatment effects on deforestation, in Table A19 we further divide projects into finer sub-categories. We find no clear evidence suggesting that any of those various sub-categories drives the results for agricultural and non-agricultural projects. Moreover, given the high degree of disaggregation and lack of exogenous variation in project choice, we do not want to focus on these results in our interpretation of the findings.

deforestation. To this end, we use data collected in Gambian villages after the end of the CDD program, but unrelated to the program. In particular, we use the Gambia Census 2013 and the IHS 2015, a comprehensive household survey. For the empirical analysis we again take advantage of the experimental design of the CDD program, estimating the following model:

$$\mathrm{Y}_{hvw} = \beta \cdot \text{treatment}_v + X_v \cdot \delta + \alpha_w + \varepsilon_{hvw}, \tag{5}$$

where Y_{hvw} is an outcome variable for household h, treatment_v a binary indicator for the treatment status of village v, X_v a vector of village-level controls, and α_w are ward fixed effects. We control for the poverty index and village population before the beginning of the program (using data from the Gambia Census 2003) because the budget assigned to each village is a function of these variables (as described in Section 2). As we are using data collected up to five years after the implementation of the CDD program, β should be considered an estimate of the medium-term average treatment effect of the program.

For statistical inference we rely on cluster-robust standard errors, clustered at the village level. Observations are weighted with the inverse village size, so that results are representative at the village level and larger and smaller villages have equal weights.[34]

5.1 Census-Based Results

The Gambia Census 2013 provides information for all households in the country, but only includes a small number of potential outcome measures. We were able to match the Census 2013 data for most of the 820 villages in the sample used for the empirical analysis, except for 20 villages (11 treatment and 9 control villages) for which the match was not possible given the lack of unique village-level identifiers. With these data, we test a set of hypotheses stemming from previous studies.

[34]Results are similar when no weights are used. One exception is that in the unweighted regression the point estimate for livestock is substantially smaller, suggesting that a potential effect is driven by smaller villages.

Hypothesis 1 (H1): The CDD program increases general economic welfare.

One of the goals of the CDD program was to increase general economic welfare. If this goal was reached, the literature related to the environmental Kuznets curve, described in Section 1, predicts an increase in environmental degradation. From the Census 2013 we build an asset index, which we take as a proxy for a household's wealth.[35] The index is scaled to have mean zero and variance one in the control group. The results in Table 6, Panel B indicate that the CDD program had a positive and statistically significant effect on the asset index in the full sample (p-value=0.07). In the sub-sample of high forest cover villages the estimate is even larger in magnitude but marginally insignificant (p-value=0.11). We take this result as an indication that the CDD program had a modest positive impact on wealth. This result is consistent with the results reported in Heß et al. (2018), where we find that the program led to modest increases in economic welfare in a subset of treatment villages for which more detailed data is available.[36]

Table 6: Household Channels: Results Using Data from Census 2013

	(H1)	(H2)	(H3)	(H4)		
	(1) assets (PCA)	(2) livestock (PCA)	(3) firewood	(4) share migrant	(5) #children	(6) village size
Panel A: Villages with Above Median Forest Cover						
treatment	0.116	0.074	-0.002	0.014	0.391	4.526
	(0.11)	(0.10)	(0.80)	(0.23)	(0.13)	(0.71)
observations	15585	15593	15598	15561	15595	389
control mean d.v.	0.000	0.000	0.982	0.219	10.411	404.134
Panel B: All Villages						
treatment	0.086	0.040	-0.002	0.011	0.165	7.626
	(0.07)*	(0.21)	(0.74)	(0.13)	(0.39)	(0.40)
observations	32355	32377	32386	32259	32383	800
control mean d.v.	0.000	0.000	0.984	0.187	11.230	456.984

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. p-values in parentheses are based on cluster-robust standard errors, allowing for clustering of the model error at the village level. Dependent variables are taken from Census 2013. In columns 1 through 5 the unit of observation is the household and each observation is weighted by the inverse village population. In column 6 the unit of observation is the village and no weights are applied. All specifications include ward fixed effects as well as the poverty index and village population in 2003.

[35] The assets index is calculated using a principal component analysis (PCA) and includes ownership of vehicles, electronic devices and other assets.

[36] The result is also similar to the findings of Casey et al. (2012), who analyze the effects of a very similar CDD program in Sierra Leone.

Hypothesis 2 (H2): The CDD program affects livestock ownership.

Alix-Garcia et al. (2013) point out one specific channel through which increasing wealth can affect deforestation: the increase in livestock ownership. We build a livestock PCA-index based on indicator variables for owning any of the livestock covered by the Census questionnaire (cattle, goats, sheep and poultry). The results in column 2 of Table 6 show no strong effect of the program on livestock ownership. Yet the results for the high forest cover sample suggests that the livestock PCA-index is slightly larger in the treatment group (p-value=0.1). This result is much weaker when the number or monetary value of livestock is used for the index instead of the binary indicators.

Hypothesis 3 (H3): The CDD program affects consumption of resource-intensive goods.

Households in treatment villages may have increased deforestation due to changes in the consumption of forest resources and land-intensive goods. To test this hypothesis we follow Baland et al. (2010) and Foster and Rosenzweig (2003) considering the consumption of firewood for fuel. Almost every household located in control villages (98%) relies on firewood as the main source for fuel, so there is little room for an increase in treatment villages. Column 3 shows indeed no significant effects.

Hypothesis 4 (H4): The CDD program affects village population.

Busch and Ferretti-Gallon (2017) indicate that population size is a correlate of deforestation and Klasen et al. (2010) show that immigration is a relevant factor to explain deforestation in Indonesia. If the size of treatment villages changed due to the CDD program, this could impact deforestation. We estimate the treatment effect on three outcomes related to *H4*. Column 4 presents results for a variable indicating for each household the share of members not born in the village, as a proxy for immigration. The results indicate that the CDD program did not affect immigration. In column 5, results indicate that the CDD program does not increase the number of children per household. The last column of Table 6 is based on data at the village level, and the results indicate that there is no significant difference in the number of inhabitants between treatment and control villages. Therefore, we do not find evidence that the CDD program induced any change in the village population that could explain the increase in deforestation.

5.2 *Integrated Household Survey*-Based Results

A second source of data we use for testing potential channels is the IHS 2015, an extensive survey conducted by The Gambia Bureau of Statistics with the support of several external donors. The IHS 2015 includes 680 settlements distributed across all districts of The Gambia, with data for close to 13,000 households and 105,000 individuals. The survey follows the Living Standards Measurement Study structure, with additional detailed information about agricultural production as well as political and environmental attitudes.

About one-third of the villages in the sample used in our empirical analysis are covered in the IHS 2015. This represents a total of 266 villages, 133 treatment and 133 control, covering 69 out of the 72 wards of our main sample. We exclude wards from the analysis with only control or only treated villages, resulting in a sample of 244 villages in 58 wards covering 4,462 households and 39,305 individuals.[37] When only high forest cover wards are used, 64 treatment and 62 control villages remain, covering 18,495 individuals in 2,225 households. As households were randomly drawn from enumeration areas based on population, they are not equally distributed across villages. We weight each observation in our empirical analysis by the inverse of the number of sampled households per village.[38]

The IHS 2015 is a very comprehensive survey that provides a large number of potential variables to measure the impact of the CDD program. To avoid "cherry picking" only

[37] The treatment effect of the CDD program on deforestation in this sub-sample is not statistically different from the average treatment effect reported in the main empirical analysis.

[38] The IHS 2015 sampling design made use of enumeration areas (EAs) from the Census 2013, which divide the country into groups to facilitate the division of tasks between census enumerators. EAs were delineated targeting an average group size of around 500 persons per EA while following village demarcations whenever possible. Very small villages, however, were grouped into single EAs, while larger villages, especially in urban locations, were divided into multiple EAs. Overall, 89% of EAs contain persons from a single village (including villages that are spread over more than one EA) and 70% of EAs directly correspond to villages. The IHS 2015 randomly sampled EAs with a probability proportional to population and within each EA targeted 20 randomly selected households for interview.

individual statistically significant estimates among those indicators, we aggregate the household-level variables into indices related to the four hypotheses discussed above, and add two additional hypotheses that can be tested with these data (but not with the data from the Census 2013). To create the indices we use the method proposed by Anderson (2008), following Casey et al. (2012).

The variables entering into the computation of the indices are listed in Table A20. This table also reveals that, among the many individual outcomes, some have statistically significant differences between the treatment and control group. While individually significant differences might suggest that the CDD program affected outcomes related to the above stated hypotheses, the results in Table 7 do not support this. Table 7 shows the estimation results of the treatment effect on the indices that summarize the hypotheses. None of the program treatment effect estimates for any of the indices is statistically significant at conventional levels and magnitudes are consistently below 0.1 standard deviations. Treatment effect estimates in wards with above-median baseline forest cover are also statistically insignificant throughout (Table 7, Panel A).

In conclusion, we cannot reject that the CDD program had no long-lasting effect on the outcomes studied in *H1-H4* that could explain the increased deforestation in treatment villages. Considered jointly with the census-based results, the results for the IHS 2015 suggest that there was at most a modest increase in general economic welfare. Estimates related to *H1* and *H2* appeared marginally significant in the Census 2013, but were insignificant using data collected two years later in the IHS 2015. This may indicate that the effects were larger in years closer to the project and dissipated over time.

In addition to the four hypotheses described above, the data in IHS 2015 allow us to test additional hypotheses that relate specifically to the Gambia CDD program, in particular to agricultural production and village institutions.

Hypothesis 5 (H5): The CDD program affects agricultural production.

Every household in the IHS 2015 sample cultivates some kind of crop. As a large share of the CDD program sub-projects focused on agriculture, deforestation in treatment

Table 7: Household Channels: Indices for Six Hypotheses

	(H1) welfare	(H2) livestock	(H3) land-intensive goods	(H4) population	(H5) agric. production	(H6) social capital
Panel A: Villages with Above Median Forest Cover						
treatment	0.089	0.027	-0.031	-0.072	0.023	-0.056
	(0.23)	(0.67)	(0.68)	(0.35)	(0.78)	(0.33)
observations	2224	2218	2222	2225	2218	2225
Panel B: All Villages						
treatment	0.020	0.067	-0.038	-0.040	0.015	0.000
	(0.69)	(0.11)	(0.45)	(0.44)	(0.79)	(1.00)
observations	4460	4447	4455	4462	4446	4458

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. p-values in parentheses are based on cluster-robust standard errors, allowing for clustering of the model error at the village level. The unit of observation is the household. Dependent variables are indices for each of the six hypotheses stated in the text. The index is created using the inverse covariance weighting as proposed by Anderson (2008) and it is standardized to have mean zero and standard deviation one in the control group. All specifications include ward fixed effects as well as the village-level poverty index and village population in 2003.

villages may be related to an expansion of agricultural production. We build an index for agricultural production considering inputs (plot size and use of fertilizer), grain processing and indicators for each type of crop (groundnut, rice, millet, maize and vegetables). The estimates of the treatment effect on the *H5* index in column 5 of Table 7 are small and insignificant, though larger in wards with high baseline forest cover. We do not find evidence for a medium-term treatment effect on agricultural production. However, we cannot rule out short-run changes that had dissipated by the time the IHS 2015 was conducted.

Hypothesis 6 (H6): The CDD program affects institutions and social capital.

An aspect that distinguishes the Gambian CDD program from other development programs is that it does not only target the provision of goods and services to the village, but also attempts to influence local institutions and decision-making processes. These are defined as “software” outcomes by Casey et al. (2012). Changes in “software” can affect deforestation directly (e.g., empowering forest management groups) and indirectly (e.g., affecting the willingness to contribute to public goods provision). We build an index for this hypothesis based on household-level indicators for participation in projects at the ward-level, voting in the last local elections, participating in a village forestry group, contributing to village’s tree planting, contributing to building a buffer to protect the forest from bush fires, and listing the environment as one of the main village problems. The estimates for the treatment effect on the *H6* index in Table 7 are close to zero and statistically insignificant. Therefore, we do not find any evidence that there were changes in institutions and social capital brought about by the CDD program that can explain its effects on deforestation.[39]

Overall, our results indicate that the CDD program has a modest impact on economic welfare (wealth and livestock in the sample of high forest cover wards), which could be related to the increase of deforestation in treatment villages. Nonetheless, we do not find evidence that this effect is driven by channels described in previous literature, such as an increase in the consumption of resource-intensive goods or an increase in village population. Nor do we find evidence that suggests a channel specific to the CDD program such as changes in agricultural production and villages institutions played an active role in deforestation.

6 Conclusion

The present study takes advantage of the experimental design of a nationwide CDD program in The Gambia to provide causal evidence about the relationship between rural development and deforestation. We find that the average treatment effect is an increase in deforestation in the immediate area surrounding treatment villages. This finding is in contrast to the poverty-environment hypothesis, which predicts that poverty reduction at low levels of income implies environmental improvements (Foster and Rosenzweig, 2003; Baland and Platteau, 1996), but is consistent with the existence of an environmental Kuznets curve and with the non-experimental evidence presented by Baland et al. (2010) and Alix-Garcia et al. (2013).

The treatment effect is larger in areas with meaningful levels of initial forest cover and limited access to markets (poor road infrastructure). The effect also varies with

[39] In Heß et al. (2018) we use detailed data for a sub-set of eligible villages to show that the CDD program is likely to have induced internal disputes related to unequal benefits and failed sub-projects. An increase in within-village disputes may imply a reduction in the coordination for the management of common resources such as the forest. We do not have data to directly test this hypothesis on the full set of villages.

the kind of project implemented by a village as part of the CDD program. Treatment villages choosing projects related to agriculture have higher levels of forest loss in a wide area around the village centroid, while villages with non-agricultural projects lose more forest cover close to the village center. Our results suggest that households in treatment villages exhibit modest improvements in economic welfare, but we do not find evidence supporting that other channels at the household level described in the extant literature are important in the Gambian context.

We do not find evidence that the increase in forest loss in treatment villages relates to the particular features of CDD programs, namely a participatory approach and the goal to influence local institutions and decision-making processes. Therefore, community-driven deforestation takes place mainly as a secondary effect of the projects chosen as part of the program. Nonetheless, this suggests environmental protection should play a special role in the participatory process, particularly as donors have targeted CDD in their strategy for climate change mitigation and adaptation (Arnold et al., 2014). However, this is rarely done. For instance, the Gambian CDD program did not actively involve the forestry groups, which exist in many villages, in program implementation. This is not only harmful from an environmental point of view, but possibly decreases the efficiency of the program, as community-based natural resource management groups tend to have experience in dealing with the problem of collective action.

CDD-like interventions should adapt to the type of social dilemmas that communities face. While environmental aspects may be secondary in post-conflict settings where several CDD programs have been implemented (Fearon et al., 2015; Casey et al., 2012), they should receive particular attention in countries where no major armed conflicts exist, such as The Gambia. If development projects deliver short-term economic benefits at the cost of deforestation and desertification that will negatively impact future generations, they are not sustainable. Community-based interventions that pay appropriate attention to social and environmental externalities are key to successfully implementing initiatives such as the *Great Green Wall of the Sahara and the Sahel* (UNCCD, 2016) and to mitigating effects of anthropogenic climate change.

Our results contribute to the understanding of the link between development programs, increases in welfare and environmental impacts, yet there are several directions for future research. First, drylands are an important habitat, covering over 40% of the Earth's land surface (Bastin et al., 2017), yet comparatively little economic research focuses deforestation in this particular ecosystem. Understanding how our findings compare to effects of similar programs in other drylands is one possible avenue of future research. Beyond this, it is also important to understand the extent to which our findings are specific to dryland ecosystems or whether they translate into other parts of the world, especially those with denser forests. Second, we have documented that the CDD program has effects that depend on community characteristics. The understanding of the mechanisms underlying these heterogeneous effects is fundamental for future policy design. For instance, program evaluations that take into account baseline integration to markets can help us to shed light on why rural development projects affect deforestation more in areas with poor transportation infrastructure. Third, only scarce evidence exists for whether rural development programs promoting the use of modern technologies of production decrease the demand for cropland and forest resources, as suggested by the Borlaug hypothesis (Abman and Carney, 2018; Assunção et al., 2016; Angelsen and Kaimowitz, 2001). Fourth, Jayachandran et al. (2017) and Alix-Garcia et al. (2015) have shown that individual-level conditional cash transfers for forest conservation seem to be effective, but potentially create community conflicts. Thus, one avenue for further research might be to compare those individual-based programs with community-based initiatives to explore if CDD could be a better alternative when preservation and poverty reduction are considered jointly. Fifth, the provision of unconditional payments is an alternative to conditional payments for environmental protection, but might bring about unintended consequences. Our results suggest that increased economic welfare has increased deforestation, and therefore unconditional payments may not reach the expected goal. More evidence on unconditional payments for conservation is necessary, in the spirit of the recent experimental study by Wilebore et al. (2018).

Over the past years, CDD programs have been implemented across the world. However, many of their promises remain unfulfilled (Mansuri and Rao, 2012). We have shown that, under their current design, CDD programs can additionally be harmful for the environment. Nevertheless, proper implementation that considers local social dilemmas and includes environmental provisions could prove beneficial to recipients and may even play a role in forest protection.

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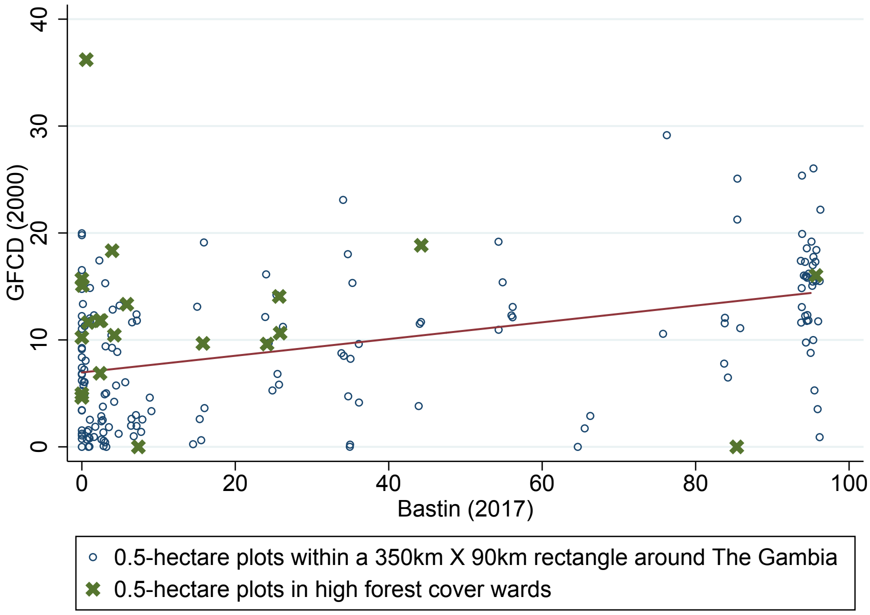
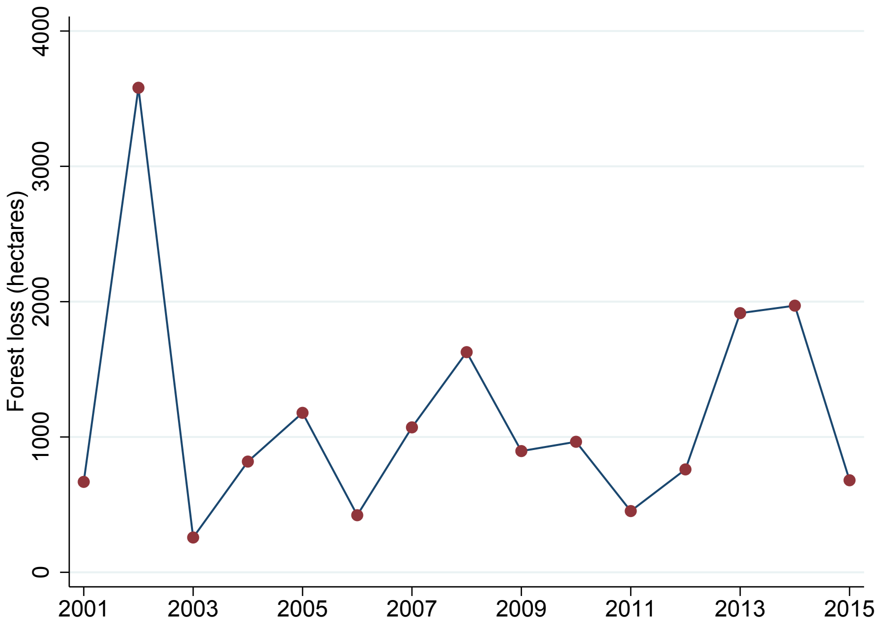
A Appendix – Additional Figures

Figure A4: Comparison of the Percentage of Forest Cover in the GFCD Data for the Year 2000 and the Bastin et al. (2017) Data for the Year 2015



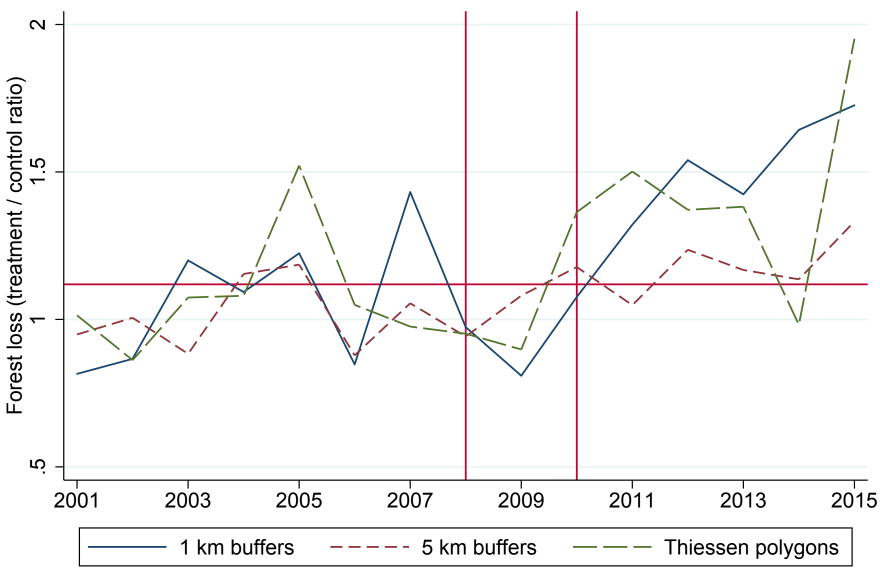
Based on 188 0.5-hectare plots of the data from Bastin et al. (2017) that are within a 350 km×90 km rectangle centered on The Gambia. The 22 points that fall into the areas corresponding to our high forest cover sample are marked by an X. Among those there are two obvious outliers. The outlier at the bottom right can be identified as peritidal mudflats on the banks of a tributary of the Gambia River and seems to be misclassified in the Bastin et al. (2017) data (13.440444, -16.196639; goo.gl/maps/ZsjtuMhkvD42). The outlier at the top left corner seems to be fallow land, around 2 km from the nearest village, which might have been cleared after 2000 (13.216167, -16.322389; goo.gl/maps/Z4vskABFdn12). In both cases visual inspection based on Google Earth's historical satellite imagery from February 2004 suggests that the GCFD data is accurate.

Figure A5: Forest Loss by Year in The Gambia



Notes: Based on authors' calculation using the GFCD data for The Gambia.

Figure A6: Ratio of Deforestation in Treatment and Control Villages in the Full Sample



Notes: Based on the sample comprising villages located in wards with low as well as high baseline forest cover. The straight horizontal line represents the ratio of the number of treatment communities relative to the number of control communities. The three plotted lines indicate the ratios of the total area deforested around treatment communities over the total area deforested around control communities. Each line represents this ratio using a different village-level aggregate: deforestation within the 1 km buffer, the 5 km buffer, and deforestation in the Thiessen polygon. The two vertical lines indicate the start and the end date of the CDD program implementation phase (2008-2010).

B Appendix – Additional Tables

Table A8: List of Village-Level CDD Projects, Classifications and Descriptive Statistics

project description	classification	subclassification	frequency		median budget share	
			full sample	high forest cover sample	full sample	high forest cover sample
farm implements & inputs: unspec.	agric	agritool	96	22	49%	70%
farm implements: planting equip..	"	agritool	93	39	32%	28%
" : animals	"	agritool	65	31	53%	47%
" : tools	"	agritool	50	29	52%	53%
" : tools & animals	"	agritool	38	29	86%	75%
" : tools & planting equip.	"	agritool	13	4	51%	75%
" : animals & planting equip.	"	agritool	8	7	100%	100%
" : tools & animals & planting equip.	"	agritool	1	0	100%	—
" : tools & power tiller	"	agritool	1	1	100%	100%
" : tractor	"	tractor	36	21	100%	100%
" : power tiller	"	tractor	7	4	59%	78%
ram fattening	"	animals	25	2	20%	19%
cattle fattening	"	animals	6	3	28%	28%
small ruminants	"	animals	3	3	28%	28%
seed store/cereal banking	"	cerbank	60	17	27%	37%
vegetable gardens	"	garden	35	23	50%	50%
orchards	"	garden	1	1	48%	48%
milling machine: coos	"	milmach	81	43	44%	44%
" : unspec.	"	milmach	50	15	33%	39%
" : rice	"	milmach	11	7	45%	52%
" : multipurpose	"	milmach	11	3	50%	50%
rice cultivation	"	other (agric)	6	3	42%	68%
access road to rice field	"	other (agric)	1	1	18%	18%
solar electrification	nonagric	infrastructure	17	17	97%	97%
market stalls	"	infrastructure	10	10	68%	68%
schools	"	infrastructure	9	5	52%	52%
latrines	"	infrastructure	7	6	13%	24%
feeder road rehab./construction	"	infrastructure	7	5	26%	22%
PHC centre	"	infrastructure	6	4	79%	98%
skills centre	"	infrastructure	5	3	38%	62%
consumer shops	"	infrastructure	3	2	30%	40%
bio-gas	"	infrastructure	3	1	45%	5%
recreation centres	"	infrastructure	3	3	57%	57%
waiting sheds	"	infrastructure	2	2	20%	20%
salt processing center	"	infrastructure	2	2	50%	50%
erosion control	"	infrastructure	2	0	60%	—
video hall	"	infrastructure	1	1	100%	100%
fishing equip.	"	other (nonagric)	3	2	76%	48%
horse cart ambulance	"	other (nonagric)	2	1	17%	27%
plastic chairs & tents	"	other (nonagric)	2	1	21%	39%
metal boats	"	other (nonagric)	2	1	23%	25%
vehicle	"	other (nonagric)	2	2	64%	64%
sanitary equip.	"	other (nonagric)	1	0	5%	—
speed boat	"	other (nonagric)	1	1	22%	22%
hand pump wells	"	water	81	39	60%	61%
stand pipes	"	water	9	6	84%	87%
repair of borehole	"	water	4	1	36%	16%
open wells	"	water	4	3	58%	61%
water & sanitation	"	water	1	0	39%	—

Notes: Project descriptions in column 1 are taken from the official CDD program records. Classification and subclassification in columns 2 and 3 was done by the authors. Frequencies of projects are listed for the *full* sample and the *high forest cover* sample. The median budgets for each project type are computed based on the total project budget, including potential village contributions.

Table A9: Additional Specifications

	(1) $\log(\text{loss}^{1km})$	(2) $\log(\text{loss}^{1km})$	(3) $\log(\text{loss}^{1km})$	(4) $\log(\text{loss}^{1km})$	(5) $\log(\text{loss}^{1km})$	(6) $\mathbb{1}(\text{loss}^{1km} > 0)$
implementation (2008-10) × treatment	-0.011	-0.006	-0.006	-0.059	0.034	0.003
	(0.85)	(0.93)	(0.94)	(0.38)	(0.58)	(0.90)
post–program (2011-15) × treatment	0.111	0.113	0.140	0.132	0.142	0.025
	(0.07)*	(0.10)	(0.08)*	(0.08)*	(0.06)*	(0.31)
implementation (2008-10) × treatment × kombo	-0.297					
	(0.05)*					
post–program (2011-15) × treatment × kombo	-0.284					
	(0.09)*					
implementation (2008-10) × treatment × isol1km					-0.096	
					(0.22)	
post–program (2011-15) × treatment × isol1km					-0.067	
					(0.50)	
observations	7065	4575	3945	4035	6030	6030
control mean annual loss (ha.) post-program	0.431	-1.735	-1.761	-1.804	-1.743	0.398

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. p-values in parentheses are based on cluster-robust standard errors, allowing for clustering of the model error at the village level. The reference period is 2001-2007. The dependent variable is the logarithm of the area of forest loss per year plus a very small constant (the area of a single 30 m × 30 m pixel) to deal with observations where the area of forest loss is zero. Units of observation are village-years between 2001 and 2015. Estimated coefficients are based on Equation (1). Ward-year and village fixed effects are included in all specifications.
In column 1 the sample incorporates communities of the peri-urban areas in the Kombo districts. In column 2 wards in which at least one eligible village has missing geocoded data are excluded from the sample. In column 3 districts that have more than two villages for which the distance between centroids from different datasets is more than 1km are excluded from the sample. In column 4 wards in which more than 3 villages were assigned different ward identifier in the Census 2013 are excluded from the sample. Column 5 shows the additional treatment effect of villages without neighbours in the 1 km buffer. Column 6 shows results for the extensive margin of deforestation. The dependent variable is a variable taking a value of one if any deforestation took place in the 1 km buffer around the village centroid in a given year, zero otherwise.

Table A10: Difference-in-Differences: Forest Loss, Post-Double-LASSO

	(1) $\log(\text{loss}^{1km})$	(2) $\log(\text{loss}^{5km})$	(3) $\log(\text{loss}^{poly})$
implementation (2008-10) × treatment	-0.018	0.041	-0.002
	(0.78)	(0.59)	(0.98)
post–program (2011-15) × treatment	0.128	0.125	0.091
	(0.04)**	(0.07)*	(0.24)
uninteracted treatment & period indicators	✓	✓	✓
observations	6030	6030	6030
post-double-LASSO reduced the model to the following numbers of parameters:			
year fixed effects (of 15)	8	10	8
ward fixed effects (of 36)	9	10	7
village fixed effects (of 402)	71	53	36
ward-year fixed effects (of 36×15=540)	133	198	117

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. p-values in parentheses are based on cluster-robust standard errors, allowing for clustering of the model error at the village level. The reference period is 2001-2007. The dependent variable is the logarithm of the area of forest loss per year plus a very small constant (the area of a single 30 m × 30 m pixel) to deal with observations where the area of forest loss is zero. Units of observation are village-years between 2001 and 2015. Estimated coefficients are based on Equation (1). Ward-year and village fixed effects are included in all specifications, but selectively excluded using the post-double-LASSO as described in Belloni et al. (2014) and implemented by Ahrens et al. (2018).

Table A11: Difference-in-Differences: Forest Loss Using the Inverse Hyperbolic Sine Transformation Instead of the Logarithms

	(1) $\sinh^{-1}(\text{loss}^{1km}$ in ha)	(2) $\sinh^{-1}(\text{loss}^{5km}$ in ha)	(3) $\sinh^{-1}(\text{loss}^{poly}$ in ha)
implementation (2008-10) × treatment	-0.013	0.020	0.006
	(0.62)	(0.63)	(0.87)
post–program (2011-15) × treatment	0.057	0.092	0.061
	(0.04)**	(0.05)*	(0.12)
village fixed effects	✓	✓	✓
ward × year fixed effects	✓	✓	✓
observations	6030	6030	6030
implementation-phase (2008-10) control mean annual loss (ha.)	0.625	11.05	1.084
post-program (2011-15) control mean annual loss (ha.)	0.266	8.393	0.683
total CDD-attributable loss (ha.) 2011-15 in area under consideration (1 km, 5 km, or polygons)	70.5	896.5	92.6
total loss in treatment villages (ha.) 2011-15	378.8	9654.5	832.3
total loss in all villages (ha.) 2011-15	632.8	17669.9	1484.8

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. p-values in parentheses are based on cluster-robust standard errors, allowing for clustering of the model error at the village level. The reference period is 2001-2007. Units of observation are village-years between 2001 and 2015. The dependent variable is the inverse hyperbolic sine of the area of forest loss per year. Estimated coefficients are based on Equation (1). Ward-year and village fixed effects are included in all specifications.

Table A12: Difference-in-Differences: Forest Loss Using Levels Instead of Logarithms

	(1) loss^{1km}	(2) loss^{5km}	(3) loss^{poly}
implementation (2008-10) × treatment	-0.003	0.623	0.170
	(0.97)	(0.39)	(0.34)
post–program (2011-15) × treatment	0.192	1.907	0.342
	(0.02)**	(0.02)**	(0.05)*
village fixed effects	✓	✓	✓
ward × year fixed effects	✓	✓	✓
observations	6030	6030	6030
implementation-phase (2008-10) control mean annual loss (ha.)	0.625	11.05	1.084
post-program (2011-15) control mean annual loss (ha.)	0.266	8.393	0.683
total CDD-attributable loss (ha.) 2011-15 in area under consideration (1 km, 5 km, or polygons)	202.8	2011.5	360.6
total loss in treatment villages (ha.) 2011-15	378.8	9654.5	832.3
total loss in all villages (ha.) 2011-15	632.8	17669.9	1484.8

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. p-values in parentheses are based on cluster-robust standard errors, allowing for clustering of the model error at the village level. The reference period is 2001-2007. Units of observation are village-years between 2001 and 2015. The dependent variable is the area of forest loss in hectares per year. Estimated coefficients are based on Equation (1). Ward-year and village fixed effects are included in all specifications.

Table A13: Difference-in-Differences: Alternative Inference Methods for Coefficient Estimates from Equation (1) and Table 2

	(1) log(loss^{1km})	(2) log(loss^{5km})	(3) log(loss^{poly})
implementation (2008-10) × treatment	-0.011	0.028	-0.003
	p^{vill}=0.85	p^{vill}=0.66	p^{vill}=0.96
	p^{ward}=0.82	p^{ward}=0.56	p^{ward}=0.96
	p^{ri}=0.84	p^{ri}=0.66	p^{ri}=0.96
	$p^{\text{Conley}_{10\,\text{km}}}$=0.85	$p^{\text{Conley}_{10\,\text{km}}}$=0.65	$p^{\text{Conley}_{10\,\text{km}}}$=0.96
	p^{bs-v}=0.84	p^{bs-v}=0.68	p^{bs-v}=0.96
	p^{bs-w}=0.81	p^{bs-w}=0.55	p^{bs-w}=0.96
	p^{wbs-w}=0.82	p^{wbs-w}=0.59	p^{wbs-w}=0.95
post–program (2011-15) × treatment	0.111	0.117	0.079
	p^{vill}=0.07*	p^{vill}=0.06*	p^{vill}=0.28
	p^{ward}=0.04**	p^{ward}=0.02**	p^{ward}=0.33
	p^{ri}=0.07*	p^{ri}=0.05*	p^{ri}=0.27
	$p^{\text{Conley}_{10\,\text{km}}}$=0.05*	$p^{\text{Conley}_{10\,\text{km}}}$=0.04**	$p^{\text{Conley}_{10\,\text{km}}}$=0.28
	p^{bs-v}=0.01**	p^{bs-v}=0.07*	p^{bs-v}=0.26
	p^{bs-w}=0.04**	p^{bs-w}=0.02**	p^{bs-w}=0.33
	p^{wbs-w}=0.03**	p^{wbs-w}=0.03**	p^{wbs-w}=0.32
observations	6030	6030	6030
implementation-phase (2008-10) control mean annual loss (ha.)	0.625	11.05	1.084
post-program (2011-15) control mean annual loss (ha.)	0.266	8.393	0.683

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. The table shows p-values based on several alternative inference methods. p^{vill} is based on village-level cluster robust standard errors. Unless specifically indicated otherwise, this is our method of choice. Among all alternative methods this empirically yields the most conservative p-values, as can be seen from the results above. p^{ward} is based on ward-level cluster robust standard errors (our sample comprises 36 wards). p^{ri} is based on randomization inference using the treatment effect estimate as test-statistic, as described in Heß (2017). $p^{\text{Conley}_{10\,\text{km}}}$ is based on Conley inference allowing for spatial and temporal correlation of the model error. In particular we allow for spatial correlation within 10 km and impose no restriction on temporal auto-correlation of the error term. p^{bs-v} is based on standard cluster-bootstrap, resampling villages, stratified by wards. p^{bs-w} is based on standard cluster-bootstrap, resampling entire wards. p^{wbs-w} is based on the wild bootstrap, resampling wards. The reference period is 2001-2007. Units of observation are village-years between 2001 and 2015. The dependent variable is the logarithm of the area of forest loss per year plus a very small constant (the area of a single 30 m × 30 m pixel) to deal with observations where the area of forest loss is zero. Estimated coefficients are based on Equation (1). Ward-year and village fixed effects are included in all specifications.

Table A14: Difference-in-Differences: Total Forest Loss per Period

	(1) log(loss^{1km})	(2) log(loss^{5km})	(3) log(loss^{poly})
implementation (2008-10) × treatment	0.011	-0.060	0.049
	(0.92)	(0.39)	(0.66)
post–program (2011-15) × treatment	0.311	0.086	0.292
	(0.02)**	(0.25)	(0.03)**
observations	1206	1206	1206
implementation-phase (2008-10) control mean annual loss (ha.)	1.876	33.16	3.252
post-program (2011-15) control mean annual loss (ha.)	1.330	41.97	3.417

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. p-values in parentheses are based on cluster-robust standard errors, allowing for clustering of the model error at the village level. The reference period is 2001-2007. The dependent variable is the logarithm of the area of forest loss per period (2001-2007, 2008-2010, and 2011-2015) taken from the GFCD. Units of observation are village-periods. Estimated coefficients are based on Equation (1). Ward-year and village fixed effects are included in all specifications.

Table A15: Difference-in-Differences: Forest Loss Including Wards with Low Forest Cover

	(1) $\log(\text{loss}^{1km})$	(2) $\log(\text{loss}^{5km})$	(3) $\log(\text{loss}^{poly})$
implementation (2008-10) × treatment	-0.040	-0.005	-0.023
	(0.22)	(0.90)	(0.57)
post–program (2011-15) × treatment	0.041	0.049	0.012
	(0.23)	(0.24)	(0.78)
observations	12300	12300	12300
implementation-phase (2008-10) control mean annual loss (ha.)	0.625	11.05	1.084
post-program (2011-15) control mean annual loss (ha.)	0.266	8.393	0.683

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. p-values in parentheses are based on cluster-robust standard errors, allowing for clustering of the model error at the village level. The reference period is 2001-2007. Units of observation are village-years between 2001 and 2015. The dependent variable is the logarithm of the area of forest loss per year plus a very small constant (the area of a single $30\,\text{m} \times 30\,\text{m}$ pixel) to deal with observations where the area of forest loss is zero. Estimated coefficients are based on Equation (1). Ward-year and village fixed effects are included in all specifications. This sample is different to the one in Table 2 because villages located in wards with below-median forest cover in 2000 are included as well.

Table A16: Difference-in-Differences: Forest Loss by Year

	(1) $\log(\text{loss}^{1km})$	(2) $\log(\text{loss}^{5km})$	(3) $\log(\text{loss}^{poly})$
2008 × treatment	0.047	-0.127	0.034
	(0.55)	(0.12)	(0.70)
2009 × treatment	-0.088	0.101	-0.097
	(0.28)	(0.27)	(0.30)
2010 × treatment	0.010	0.109	0.053
	(0.91)	(0.24)	(0.57)
2011 × treatment	0.084	0.107	0.048
	(0.35)	(0.20)	(0.61)
2012 × treatment	0.096	0.022	0.036
	(0.19)	(0.80)	(0.65)
2013 × treatment	0.176	0.126	0.200
	(0.10)	(0.10)	(0.09)*
2014 × treatment	0.099	0.146	0.088
	(0.35)	(0.18)	(0.52)
2015 × treatment	0.100	0.186	0.023
	(0.18)	(0.12)	(0.82)
observations	6030	6030	6030
control mean annual loss (ha.) 2008-15	0.401	9.390	0.834

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. p-values in parentheses are based on cluster-robust standard errors, allowing for clustering of the model error at the village level. The reference period is 2001-2007. The dependent variable is the logarithm of the area of forest loss per year plus a very small constant (the area of a single $30\,\text{m} \times 30\,\text{m}$ pixel) to deal with observations where the area of forest loss is zero. Units of observation are village-years between 2001 and 2015. Estimated coefficients are based on Equation (1). Ward-year and village fixed effects are included in all specifications.

Table A17: Difference-in-Differences: Forest Loss by Year, Including Years Before the CDD Program

	(1) $\log(\text{loss}^{1km})$	(2) $\log(\text{loss}^{5km})$	(3) $\log(\text{loss}^{poly})$
2002 × treatment	0.066	0.155	-0.120
	(0.61)	(0.06)*	(0.40)
2003 × treatment	0.038	0.012	-0.095
	(0.72)	(0.90)	(0.43)
2004 × treatment	0.078	0.158	0.000
	(0.51)	(0.07)*	(1.00)
2005 × treatment	0.103	0.089	0.083
	(0.35)	(0.30)	(0.50)
2006 × treatment	-0.027	-0.005	-0.088
	(0.81)	(0.95)	(0.44)
2007 × treatment	0.176	0.065	0.134
	(0.12)	(0.47)	(0.30)
2008 × treatment	0.109	-0.059	0.022
	(0.36)	(0.56)	(0.86)
2009 × treatment	-0.026	0.169	-0.109
	(0.82)	(0.10)	(0.41)
2010 × treatment	0.072	0.177	0.041
	(0.56)	(0.09)*	(0.76)
2011 × treatment	0.146	0.175	0.036
	(0.22)	(0.09)*	(0.78)
2012 × treatment	0.158	0.089	0.024
	(0.15)	(0.38)	(0.84)
2013 × treatment	0.238	0.194	0.188
	(0.08)*	(0.05)*	(0.22)
2014 × treatment	0.161	0.214	0.075
	(0.23)	(0.10)	(0.65)
2015 × treatment	0.162	0.254	0.010
	(0.16)	(0.08)*	(0.94)
observations	6030	6030	6030
control mean annual loss (ha.) 2008-15	0.401	9.390	0.834

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. p-values in parentheses are based on cluster-robust standard errors, allowing for clustering of the model error at the village level. The reference period is 2001. The dependent variable is the logarithm of the area of forest loss per year plus a very small constant (the area of a single 30 m × 30 m pixel) to deal with observations where the area of forest loss is zero. Units of observation are village-years between 2001 and 2015. Estimated coefficients are based on Equation (1). Ward-year and village fixed effects are included in all specifications.

Table A18: Heterogeneous Effects by Pre-Treatment Variables (5 km Buffers and Polygons)

village-level split:	population	poverty	distance to road	ELF	population	poverty	distance to road	ELF
	(1) $\log(\text{loss}^{5km})$	(2) $\log(\text{loss}^{5km})$	(3) $\log(\text{loss}^{5km})$	(4) $\log(\text{loss}^{5km})$	(5) $\log(\text{loss}^{poly})$	(6) $\log(\text{loss}^{poly})$	(7) $\log(\text{loss}^{poly})$	(8) $\log(\text{loss}^{poly})$
implementation (2008-10) × treatment	-0.055	-0.023	-0.006	-0.039	0.000	-0.143	-0.064	-0.087
	(0.60)	(0.75)	(0.95)	(0.68)	(1.00)	(0.12)	(0.50)	(0.38)
implementation (2008-10) × treatment × $high_v$	0.170	0.106	0.081	0.126	-0.002	0.283	0.130	0.160
	(0.19)	(0.42)	(0.54)	(0.34)	(0.99)	(0.03)**	(0.34)	(0.23)
post–program (2011-15) × treatment	0.020	0.119	0.008	0.075	0.033	0.020	-0.057	-0.124
	(0.81)	(0.19)	(0.92)	(0.41)	(0.74)	(0.85)	(0.55)	(0.22)
post–program (2011-15) × treatment × $high_v$	0.184	-0.002	0.215	0.076	0.089	0.118	0.264	0.403
	(0.15)	(0.99)	(0.09)*	(0.55)	(0.56)	(0.42)	(0.09)*	(0.01)**
split indicator×period	✓	✓	✓	✓	✓	✓	✓	✓
observations	6030	6030	6030	6030	6030	6030	6030	6030
control mean annual loss (ha.) (low)	7.695	7.164	8.080	7.608	0.476	0.480	0.328	0.777
control mean annual loss (ha.) (high)	9.176	9.635	8.723	9.238	0.916	0.889	1.058	0.582
mean of village-level var. (low)	0.16	0.58	0.91	0.057	0.16	0.58	0.91	0.057
mean of village-level var. (high)	0.52	0.74	6.15	0.45	0.52	0.74	6.15	0.45

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. p-values in parentheses are based on cluster-robust standard errors, allowing for clustering of the model error at the village level. The reference period is 2001-2007. Units of observation are village-years between 2001 and 2015. The dependent variable is the logarithm of the area of forest loss per year plus a very small constant (the area of a single 30 m × 30 m pixel) to deal with observations where the area of forest loss is zero. The results show the interaction of the Difference-in-Differences terms with a binary indicator dividing the sample according to the median of each pre-treatment village-level variable. Ward-year and village fixed effects are included in all specifications.

Table A19: Difference-in-Differences Estimates by Village-Level Project Subclassification

	(1) log(loss^{1km})	(2) log(loss^{5km})	(3) log(loss^{poly})
implementation (2008-10) × agric: agritool share	-0.021	0.153	-0.032
	(0.79)	(0.13)	(0.73)
implementation (2008-10) × agric: animals share	-0.097	-0.474	-0.110
	(0.69)	(0.21)	(0.81)
implementation (2008-10) × agric: cerbank share	-0.150	-0.737	-0.010
	(0.46)	(0.10)	(0.98)
implementation (2008-10) × agric: garden share	0.431	0.523	0.290
	(0.15)	(0.06)*	(0.46)
implementation (2008-10) × agric: milmach share	-0.233	-0.110	0.037
	(0.08)*	(0.30)	(0.80)
implementation (2008-10) × agric: other agric share	-0.138	1.189	0.407
	(0.88)	(0.39)	(0.42)
implementation (2008-10) × agric: tractor share	0.023	0.096	0.154
	(0.87)	(0.42)	(0.34)
implementation (2008-10) × nonagric: infrastructure share	0.040	0.052	0.060
	(0.80)	(0.65)	(0.70)
implementation (2008-10) × nonagric: other nonagric share	0.100	0.326	-0.535
	(0.68)	(0.44)	(0.41)
implementation (2008-10) × nonagric: water share	0.028	-0.352	-0.284
	(0.80)	(0.03)**	(0.12)
post–program (2011-15) × agric: agritool share	0.029	0.134	0.008
	(0.70)	(0.14)	(0.93)
post–program (2011-15) × agric: animals share	0.374	0.072	0.368
	(0.10)	(0.84)	(0.34)
post–program (2011-15) × agric: cerbank share	0.212	-0.339	-0.232
	(0.51)	(0.50)	(0.53)
post–program (2011-15) × agric: garden share	0.440	0.023	0.171
	(0.19)	(0.94)	(0.60)
post–program (2011-15) × agric: milmach share	-0.074	0.179	0.069
	(0.74)	(0.32)	(0.72)
post–program (2011-15) × agric: other agric share	0.002	1.700	0.234
	(0.99)	(0.00)***	(0.74)
post–program (2011-15) × agric: tractor share	0.028	0.237	0.349
	(0.89)	(0.19)	(0.23)
post–program (2011-15) × nonagric: infrastructure share	0.262	-0.101	0.150
	(0.21)	(0.52)	(0.47)
post–program (2011-15) × nonagric: other nonagric share	1.233	1.704	0.934
	(0.14)	(0.00)***	(0.39)
post–program (2011-15) × nonagric: water share	0.198	-0.071	-0.127
	(0.23)	(0.63)	(0.56)
observations	6030	6030	6030
control mean annual loss (ha.) post-program	0.359	9.151	0.789

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. p-values in parentheses are based on cluster-robust standard errors, allowing for clustering of the model error at the village level. The reference period is 2001-2007. Units of observation are village-years between 2001 and 2015. The dependent variable is the logarithm of the area of forest loss per year plus a very small constant (the area of a single $30\,\text{m} \times 30\,\text{m}$ pixel) to deal with observations where the area of forest loss is zero. The results show the interaction of the implementation phase and post-program indicators with a variable indicating the CDD budget shares allocated to each project subclass. Project subclasses are built as listed in Table A8. Ward-year and village fixed effects are included in all specifications.

Table A20: Variables Entering into the Indices Used in Table 7

hypothesis	variable	variable type/unit	weight in index		TE (high forest cover)		
			high forest cover sample	full sample	estimate	p-value (CRSE)	p-value (RI)
H1	assets	PCA	0.26	0.28	0.01	0.48	0.50
	annual income	log(income in GMD + 1)	0.10	0.09	0.34	0.07*	0.06*
	paid job	dummy	0.27	0.29	0.05	0.07*	0.10*
	food spending	log(food spending/week in GMD + 1)	0.25	0.23	-0.03	0.68	0.71
	non-food spending	log(non-food spending/year in GMD + 1)	0.11	0.11	0.00	0.98	0.98
H2	cattle	count	0.06	0.11	0.55	0.33	0.52
	oxen	count	0.22	0.17	0.02	0.75	0.77
	goats	count	-0.01	0.02	-0.08	0.73	0.79
	sheep	count	0.02	0.01	0.37	0.16	0.23
	donkey	count	0.15	0.21	-0.17	0.01***	0.01**
	any cattle	dummy	0.13	0.12	-0.04	0.19	0.26
	any goat	dummy	0.20	0.15	0.01	0.76	0.78
	any sheep	dummy	0.16	0.13	0.06	0.03**	0.07*
	any donkey	dummy	0.06	0.09	-0.09	0.00***	0.00***
H3	fuel consumption	dummy	0.23	0.23	-0.01	0.69	0.76
	firewood cooking	dummy	0.29	0.28	-0.01	0.48	0.58
	rooms	count	0.11	0.12	-0.09	0.66	0.71
	beef	dummy	0.13	0.12	-0.03	0.21	0.35
	other meat	dummy	0.13	0.13	0.03	0.08*	0.16
	milk	dummy	0.11	0.11	0.04	0.21	0.35
H4	born outside village	dummy	0.39	0.39	0.03	0.32	0.40
	household size	count	0.15	0.10	-0.47	0.26	0.37
	children	count	0.15	0.20	-0.29	0.30	0.39
	any births past year	dummy	0.32	0.31	0.00	0.88	0.86
H5	plot size	hectares	0.06	0.08	-0.07	0.89	0.88
	fertilizer	dummy	0.09	0.08	-0.03	0.45	0.55
	processes grain	dummy	0.14	0.12	-0.06	0.18	0.25
	groundnut	dummy	0.09	0.09	-0.06	0.06*	0.12
	rice	dummy	0.20	0.20	0.04	0.42	0.47
	millet	dummy	0.05	0.08	-0.03	0.40	0.52
	maize	dummy	0.15	0.14	0.03	0.44	0.54
	vegetables	dummy	0.23	0.22	0.08	0.05**	0.14
H6	participate in ward proj.	dummy	0.20	0.19	-0.04	0.04**	0.10*
	voted in elec.	dummy	0.22	0.22	0.01	0.56	0.57
	forestry group	dummy	0.14	0.14	-0.02	0.52	0.64
	tree planting activity	dummy	0.12	0.11	-0.01	0.67	0.70
	environmental concerns	dummy	0.22	0.20	0.00	0.98	0.98
	buffer creation activity	dummy	0.11	0.12	-0.02	0.52	0.56

Notes: This table contains a comprehensive list of all variables used to compute the indices used in Table 7. The weight of each variable in the final index is based on the variance-covariance matrix in the control group of that specific sample of all variables included in the same index (following Anderson, 2008). The weights are computed from and applied to the normalized variables (mean 0, variance 1). Treatment effect estimates and p-values are based on Equation (5). "Buffer creation activities" refers participation in the collective clearing of land in the village surroundings for the purpose of preventing wildfires.

C Estimating the Total Treatment Effect in Hectares

To calculate the total program-induced forest loss in hectares we need to take into account that the estimation is based on a log-level model. In a level-level model one would simply multiply the average treatment effect estimate with the number of observed years times the number of treated villages. In a log-linear model, as used for Table 2, this is not possible due to two reasons: First, because the sum of two logarithms does not equal the logarithm of the sum. This could be solved by computing the loss in hectares for each village separately and summing them up in a second step. The second, more important problem is that the logarithm of the expected value is not identical to the expected value of the logarithm. Formally, the model estimates $E[\log(\text{loss}_{vwt})|\text{treatment}_v, v, w, t]$, while the total program-induced forest loss is given by:

$$\sum_v \sum_t \left(\mathbb{E}\left[\text{loss}_{vwt}|\text{treatment}_v, v, w, t\right] - \mathbb{E}\left[\text{loss}_{vwt}|\text{treatment}_v = 0, v, w, t\right]\right), \tag{6}$$

and there is no direct correspondence between these two expressions. More precisely, it follows from Jensen's inequality that $\log(\mathbb{E}[\text{loss}]) \geq \mathbb{E}[\log(\text{loss})]$, which implies that plugging in $\exp\left(\mathbb{E}\left[\log(\text{loss}_{vwt})|\text{treatment}_v, v, w, t\right]\right)$ for $\mathbb{E}\left[\text{loss}_{vwt}|\text{treatment}_v, v, w, t\right]$ would produce an incorrect estimate of the program-induced forest loss. If this bias is stronger for the first expectation in the expression than for the second, the overall effect will be and underestimation of the program-induced forest loss. We thus use the following method to calculate the total program-induced forest loss based on the estimated log-linear model:

1. For each village-year, we use the estimated model coefficients to obtain fitted values for the logarithmized forest loss, once for the actual treatment assignment and once assuming no village was treated. For each of these fitted values we draw 100 realizations by adding random regression error, bootstrapped from the fitted model. In this bootstrap procedure we draw model residuals for all 15 years grouped by village, so that potential temporal auto-correlation is accounted for. I.e. in each

draw we take a series of the 15 regression residuals from one village and add it to the fitted value for another village.

2. These draws of logarithmized forest loss are transformed via the exponential function (or, $\sinh(\cdot)$ when the inverse hyperbolic sine was used instead of the logarithm) to obtain measures for the forest loss in hectares at the village-year level and then averaged separately for the 'actual treatment' variant and the 'no treatment' variant. This yields estimates for the two expected values in Equation (6).

3. These means are summed up over all years 2011-2015, and all villages. Finally, the number reported in Table 2 is the difference between the 'actual treatment' and the 'no treatment' result, as indicated in Equation (6).

For a linear model specification without a transformed dependent variable this procedure yields the same estimate as multiplying the average treatment effect estimate with the number of observed years times the number of treated villages (see Table A12).