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Environmental effects of development programs: Experimental evidence from West African dryland forests☆

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ABSTRACT

Environmental effects of development programs are the subject of an ongoing debate. We contribute to this debate by studying effects of a randomly allocated, nationwide development program in The Gambia on deforestation, focusing on parts of the country with meaningful baseline forest cover. Our main finding is that the program caused significant increases in annual forest loss. Conservative benchmark estimates imply that 5.6% of all forest loss occurring within 1 km of treatment villages during the eight post-program years resulted from the program. Accounting for spillovers, we estimate that the program explains about one quarter of the forest loss around all villages. Looking at possible channels, we find moderate treatment effects of the development program on household wealth and livestock holdings. Further, villages with limited access to markets drive the effect of the program on deforestation.

1. Introduction

Deforestation has reached record levels over the last three decades (Hansen et al., 2013). The depletion of common forest is particularly severe in poor areas (Barrett et al., 2011) and the effect of increases in income per capita on forest cover is strongest in poor countries Cuaresma et al. (2017). Because of the effects on climate change, this deforestation has negative effects well beyond the country in which deforestation occurs. Recognizing this, a number of recent development initiatives attempt to reduce deforestation in developing countries, with the ultimate goal of mitigating climate change. Well known is in particular the UNFCCC's financial mechanism to reduce emissions from deforestation and forest degradation, REDD+ (Miles and Kapos, 2008). At the same time, development projects that do not specifically target deforestation also have the potential to affect deforestation in a number of ways, which are typically unintended side effects of programs that aim at increasing incomes, or at improving production techniques and living conditions more generally.

In the present study, we contribute to the understanding of the (unintended) effects of development projects on deforestation, by providing causal evidence from a nationwide randomized program in The Gambia, West Africa. Our spatial focus is on deforestation in dryland biomes, which span a large fraction of poor regions of the world and are particularly vulnerable to the negative effects of climate change (Bastin et al., 2017; Dietz et al., 2004). Despite this, they have received much less attention in studies of deforestation than rainforests in Latin America and Asia (Busch and Ferretti-Gallon, 2017; Probst et al., 2020; Assunção et al., 2020).

Using detailed satellite data, we analyze the impact of a nationwide Community-Driven Development (CDD) program on forest loss in rural villages that were randomly chosen as beneficiaries. Those villages received financial support to implement projects of their choice and mostly decided to use those funds for agricultural inputs, machinery, or investments in local infrastructure. The program is of significant magnitude, with average per-household funding of approximately US$140 (roughly equivalent to half the GDP per capita in The Gambia). The random allocation of the program allows for a straightforward identification of the causal effect of the development intervention on forest loss. Given that non-compliance to the treatment assignment was negligible, the estimates can be interpreted as average treatment effects of the program and the experimental design allows us to identify spillover effects from treated to neighboring villages. At the same time, we should note that the experimental nature of our analysis

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is limited by the fact that the ultimately implemented projects are quite heterogeneous and that they are endogenously chosen by the village. Therefore, the estimated program impact is the effect of being treated with the opportunity to implement a relatively wide range of development projects (with a budget determined by the size of the population of the community) rather than the effect of being treated with a particular type of project.

Our central finding is that the CDD program leads to forest loss in treatment villages. Our most conservative benchmark estimates indicate that forest loss occurring within 1 km of the treatment villages increased by 9.2%. An alternative way to illustrate the magnitude and ecological significance of our estimates, is that the estimates imply that 5.6% of all forest loss occurring around the treatment villages in the post-program period resulted from the program. If spillover effects from treated villages into other 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 program villages and neighboring villages after 2011. The estimated magnitudes of the effect of the project on deforestation are large and constitute a warning about possible unintended environmental consequences of development projects.

Based on the literature, two main mechanisms can be identified that potentially connect development projects, such as those implemented by CDD-funded villages, to deforestation.

First, projects may succeed in increasing incomes and that, in turn, may affect the environment. Initial work focused primarily on the so-called *environmental Kuznets curve* (Grossman and Krueger, 1995; Stern, 2004). 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. Under this hypothesis, income growth in poor regions implies environmental degradation.[2] A different view on possible effects of increases in income is taken by the *poverty-environment hypothesis*, which suggests that environmental degradation is poverty-induced, and therefore that increases in income in poor regions will lead to environmental improvement (Baland and Platteau, 1996). The empirical evidence is mixed and provides no clear support for these two contrasting hypotheses (Busch and Ferretti-Gallon, 2017). A problem with most extant literature is that household income and forest usage are likely to be jointly determined, implying that estimated effects based on observational data are likely biased.

A second potential mechanism is through changes in local production techniques. In this case, there are also two opposing views. On the one hand, the *Borlaug hypothesis* (Angelsen and Kaimowitz, 2001; Borlaug, 2007) suggests that increasing agricultural productivity, through modern production technologies, decreases the demand for cropland and thus deforestation. On the other hand, 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 (Morton et al., 2006). Extant empirical evidence on the relationship between agricultural productivity and deforestation is again inconclusive (Foster and Rosenzweig, 2003; Abman and Carney, 2020; Assunção et al., 2016).

To shed light on possible mechanisms, we first investigate the effect of the program on economic welfare in the medium run (3–5 years after the program). The results from post-program surveys suggest that treatment villages experienced modest improvements in economic welfare. Further, we test for heterogeneous effects and use additional post-program survey and census data and find some evidence that program-induced deforestation within the immediate surroundings of the village was largest in treatment villages farther from roads. Taken together, these findings are consistent with the existence of an environmental Kuznets curve. To investigate possible mechanisms further, we also classify implemented projects as either agricultural and non-agricultural, and find some evidence that implementing agricultural projects is associated with more forest loss in a 5 km radius around treatment villages. These results speak against the Borlaug hypothesis. Yet, because of endogenous project selection, unlike our other results, the results connecting project type and forest loss are not identified through experimental variation and should be interpreted with caution. Finally, we do not find 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, local institutions, and population growth (Baland et al., 2010; Klasen et al., 2010; Burgess et al., 2012).

Our paper contributes to the literature on unintended effects of development programs on the environment. Several authors have previously exploited quasi-experimental setups to investigate unintended effects.[3] Alix-Garcia et al. (2013) study the effect of the well-known Progresa/Oportunidades conditional cash transfer program on forest cover. Exploiting the discontinuity in program implementation based on a marginality index, they show that the program increased deforestation. Ferraro and Simorangkir (2020) study a conditional cash transfer program in Indonesia, exploiting the phasing-in of the program to estimate its effect on the environment. Their findings suggest that the program significantly reduced deforestation. Garg and Shenoy (2021) consider a program that provided tax benefits for industrial development in an Indian state and explicitly excluded environmentally damaging industries. Using a spatial difference-in-discontinuities design, these authors show that programs that significantly increase economic activity can be implemented without an impact on deforestation. Abman and Carney (2020) show that a large fertilizer and seed subsidy program in Malawi lead to a decrease in deforestation, using instrumental variables as identification strategy. Hanna and Oliva (2015) exploit an experimental setup in which assets were randomly distributed to very poor households in West Bengal, India, leading to a statistically and economically significant increase in economic well-being. The authors explore additional effects on fuel consumption. On the one hand, they find that fuel consumption increased, yet they also find a decrease in the use of wood as fuel. Thus, although the effect on forests was not explicitly studied, these findings suggest a channel—changes in fuel consumption patterns—through which increases in incomes may lead to a reduction in deforestation. In sum, while there is a recent empirical literature that exploits experimental and quasi-experimental designs to study the unintended effects of development programs on the environment, the emerging picture is far from clear. While some of the papers suggest negative effects (e.g., Alix-Garcia et al., 2013), others find positive effects (e.g., Ferraro and Simorangkir, 2020; Abman and Carney, 2020) or show—possibly because of an explicit consideration of the environment and the implementation of pro-environmental measures—that programs can promote economic activity without harming the environment (e.g., Garg and Shenoy, 2021). Against this background, Alpízar and Ferraro (2020) call for more experimental studies linking anti-poverty programs to environmental outcomes. Our present study is intended to shed further empirical light on this open question, exploiting an experimental design and focusing directly on the unintended effects of a development program on deforestation.

[2] However, alternative explanations for the environmental Kuznets curve exist that do not relate the inverse-U shape to income growth, e.g., Andreoni and Levinson (2001).

[3] There are also a number of experimental studies in the economic development literature that consider *intended* effects of programs on the environment (such as Jayachandran et al., 2017; Wilebore et al., 2019; Duflo et al., 2018). There are also quasi-experimental studies on intended effects, as Alix-Garcia et al. (2015) that use matching combined with fixed effects panel regressions to show the environmental and welfare effects of a Payments for Ecosystem Services Program in Mexico.

The following section introduces our data. Section 3 discusses our empirical strategy and explains how the experimental design allows us to identify direct effects as well as spillover effects of the program. Main results and further results regarding possible mechanisms based on supplementary data are discussed in Section 4. Section 5 concludes.

2. Background and data

2.1. 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). Donors have particularly targeted participatory CDD programs in their strategy for climate change mitigation and adaptation (Arnold et al., 2014).

The Gambia CDD program was rolled out between 2008 and 2010, and was mainly financed by the World Bank, and co-financed by the Government of Japan (World Bank, 2006b). It targeted a population estimated at 435,000 people or about 50 percent of the Gambian rural population (World Bank, 2006b). 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.[5] The remaining eligible villages (435) did not receive funds for village-level projects.[6] The random assignment was blocked at the ward level, i.e., around half of the eligible villages *within each ward* were selected to receive the funds. This low-level spatial blocking gives us the opportunity to restrict the deforestation analysis to areas with meaningful forest cover, while retaining the randomized blocked structure (as explained below).

The implementation of the program was to a large extent managed by local authorities. This is central to the CDD approach, because the program's stated objective was to plan, implement and maintain local social and economic investment priorities jointly with Local Government Authorities (World Bank, 2006b) and because one of the project's central components was to strengthen local government capacities. In particular, the Gambian Department of State for Local Government and Lands managed the project's implementation in cooperation with specifically hired field staff and jointly with representatives from recipient villages (World Bank, 2006b).

On the village-side, project choice 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.[7] 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 (see World Bank, 2006a). As a first step, a village-level development committee was established to organize a series of village meetings and consultations with community-based organizations and other representatives of the community. These meetings and consultations were used to define village-level development priorities and a list of sub-projects for which the CDD funds were to be used. Sub-projects were presented to all the villagers for the final selection of sub-projects to be financed by the CDD program. Also after project selection, community members were involved in the implementation and maintenance of the investments The budget allocated 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). With about 60 households in an average village, this translates into per-household allocations that are roughly equivalent to one-half of an annual per capita income in The Gambia. Rural villages in The Gambia, as in our sample, have on average only 40 households and thus even higher per-household allocations. 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. Appendix Table C.13 provides more information about the village-level projects implemented in the Gambian CDD program. Though donors imposed some environmental safeguard policies regarding project choice,[8] forest preservation was not among the explicitly stated objectives of the Gambian CDD program (World Bank, 2006b).

2.2. Data

Our forest-related outcome measures are based on the Global Forest Change Database 1.6 (GFCD henceforth), which contains worldwide information about forest cover in 2000 and forest change between 2001 and 2018 (Hansen et al., 2013). The data are based on images from Landsat satellites. Images captured during tree growing season for each region are used to generate high-resolution pixel-level data at a 1 arc-second resolution, which corresponds to a pixel size of less than 30 m × 30 m.

In our main specification, we aggregate pixels in buffers of 1 km radii around each village centroid to obtain village-level forest and deforestation measures. An advantage of using buffers is that they provide village-level proxies of deforestation for an area of fixed size. As an extension to these results, we also use larger radii as well as villages' Thiessen polygons as the unit of aggregation. Fig. 1a shows the 1 km buffers around all villages in The Gambia (black dots indicate villages that were eligible for the CDD program).

The richness of the GFCD 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 the standard of forest-rich countries, yet carries high ecological importance (Bastin et al., 2017). In Appendix C.1 we compare the GFCD data with other available data and confirm that they provide a useful measure for forest in the area under investigation. In particular, we show that variation in GFCD-based tree cover estimates for our study region is qualitatively comparable to variation in a manually coded dataset by Bastin et al. (2017), that is designed for improved detection of tree-cover in dryland biomes. According to the GFCD, during 2001–2013, forest loss in The Gambia amounted to 11,000 ha., i.e. over 1% of the country's area (Hansen et al., 2013).

Despite our ability to measure forest at low density, we need to consider implications of low forest cover for the empirical work. Mechanically, there is little deforestation potential in areas with very little

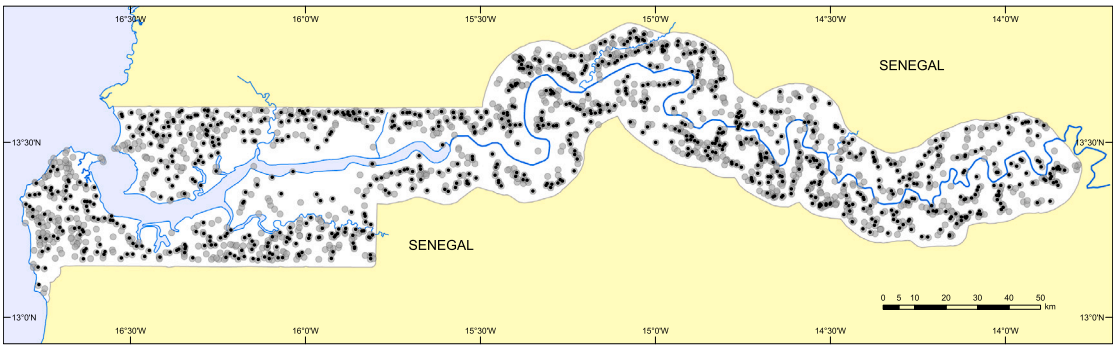
[4] Wards are a smaller geographical division that tend to be homogeneous in geographical terms but heterogeneous in socio-cultural terms. Typical rural wards comprise 11 villages (10th percentile) to 36 villages (90th percentile) and 6 to 18 villages that were eligible for the CDD program.

[5] The randomization was performed as part of the program implementation by the World Bank program staff.

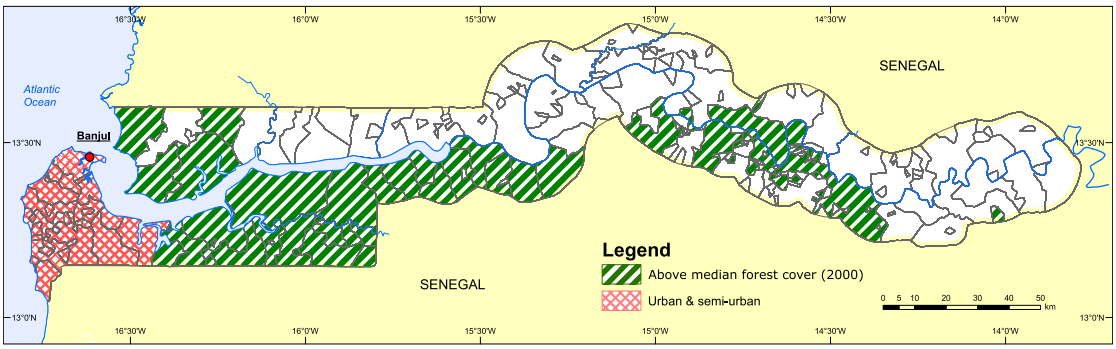
[6] Appendix Figure A.1 illustrates the sample selection process.

[7] 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.

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



(a) 1 km buffers



(b) Baseline forest cover in The Gambia (2000)

Fig. 1. Aggregation levels of forest cover and forest change used in the empirical analysis.
Notes: In Figure (a), dots represent settlements that were eligible for the CDD program. Figure (b) is based on calculations from the GFCD. The green areas indicate wards with above-median baseline forest cover (2000) outside of the urban and semi-urban areas.

or no forest cover to begin with (and there is no forest gain in The Gambia during the sample period; see Hansen et al., 2013, Supplementary Materials). We thus restrict our analysis to rural areas with meaningful initial forest cover. For this, we make use of the block-randomized treatment assignment, which stratified treatment geographically using wards (typically comprising 6–18 eligible villages). We exclude entire wards, which allows us to conserve the experimental treatment-control balance in our estimation sample. To focus on rural areas, we exclude urban and peri-urban wards, specifically Banjul and the Kombo area in the West Coast Region. To define areas with meaningful initial forest cover, we use the ward-level tree cover density as the criterion to select wards. In related work, forests are commonly defined as areas with more than 10% tree cover (FAO, 2018), yet this cutoff is too restrictive to be applied to the ward-level tree cover density aggregate we use. Using a tree cover of 10% as the ward-level cutoff would imply dropping 79% of all rural villages, including wards that are partly covered in dense forests. Instead, we use a less restrictive cutoff. Specifically, we exclude wards with initial forest cover below the sample median, which is 7.6% tree cover. Because wards do not all contain the same number of villages, the wards that remain in the sample do not contain exactly 50%, but only 49.3% of all rural villages. These villages are responsible for more than 80% of all rural forest loss in the GFCD. Appendix Figure A.1 provides details on how the sample is obtained. After applying these sample restrictions, we obtain a sample of 790 villages, namely 211 treatment villages, 191 control villages and another 388 ineligible villages that can only be affected indirectly, through spillovers. The location of the high-forest wards is indicated in Fig. 1b. All results discussed in the main text are based on this sample, unless explicitly stated otherwise. In Appendix Tables A.2 to A.4 we explore how the main results change when we apply alternative cutoffs to restrict the sample to locations with meaningful forest cover.

The characteristics of the GFCD data in the sample of CDD-eligible villages that we use for our main empirical analysis are described in Appendix C.2. There are no statistically significant differences in the baseline forest cover between the treatment and the control group.

3. Empirical strategy

We estimate three different specifications (as well as a battery of robustness checks shown in Appendix A) for the post-program treatment effect (i.e., the effect in the period 2011–2018). Specification 1, which serves as our benchmark specification, is a difference-in-differences specification relying on experimental variation in direct exposure to treatment and is estimated on the subsample of CDD-eligible villages. Specification 2 extends Specification 1 by additionally exploiting experimental variation in indirect exposure to treatment (i.e., through treatment of neighboring villages). This specification is estimated on a significantly larger sample, which includes ineligible villages. In total, this sample includes all 790 villages in the above-median forest cover wards, i.e. almost half of all rural villages in The Gambia.

To help us understand possible mechanisms, we perform an analysis of heterogeneity of the main treatment effect (Specification 3). Further, we use supplementary data that we obtain from surveys that were conducted after the program to study effects on household-level outcomes. This auxiliary analysis is presented in Section 4.4. Since this analysis is based on cross-sectional post-program data and we are able to exploit experimental variation, the empirical strategy for this part is straightforward and is not further discussed in this section.

Our panel-data analysis distinguishes between the pre-program period (2001–2007), the implementation period (2008–2010), and the post-program period (2011–2018). We do so, because we hypothesize that effects of the program appear only after 2010 for several reasons. To begin, our data do not record the exact disbursement dates, but projects had to be appraised before payments were made. According to administrative data, most project proposals were appraised between late 2008 and mid 2010. Implementation likely began later than that.

In the absence of more precise data on actual project starting dates, we infer from these appraisal dates that the majority of projects were not implemented before 2009 and likely started operating during or after 2010. This assessment is also consistent with aggregate disbursement data (World Bank, 2012, p. 3). Moreover, even after the disbursements are made, effects may take several years to materialize because deforestation takes time. The way deforestation is measured, likely adds further lag. According to the definition of forest loss in the GFCD, a pixel is considered as deforested only after the full removal of all forest cover in a pixel, 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 during the following years.

Thus, we do not expect major treatment effects on deforestation during the implementation period from 2008 to 2010. Yet, villages were informed about the treatment earlier and the CDD program implementation process included a series of pre-disbursements activities (meetings, project selection, etc.), which also started in 2008. Consequently, treatment villages do neither have a clear treatment status nor control status during the implementation period, and therefore we separate the implementation period from the pre-treatment period in all our specifications to allow for an early effect.

Specification 1: Direct average treatment effect

The average treatment effect estimate of the CDD program is based on the following difference-in-differences specification (Specification 1):

$$\log(\text{loss}_{vwt}) = \sum_{p\in\{[2008,2010],[2011,2018]\}} \beta_p \cdot \mathbb{1}_{(t\in p)} \cdot \text{treatment}_v + \alpha_v + \delta_{wt} + \varepsilon_{vwt}, \tag{1}$$

where loss_{vwt} measures hectares of forest loss in village v in ward w during year t. The indicator $\mathbb{1}_{(t\in p)}$ indicates whether year t falls into period p and treatment_v is a binary treatment indicator. Thus, the specification allows for the treatment effect to vary by period. As discussed above, we expect at most a small effect during the implementation period (2008–2010).

Our specification allows for village fixed effects, α_v, which control for any unobserved time-invariant differences, 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.). In this benchmark specification fixed effects are selected via a post double-selection procedure based on the LASSO (Ahrens et al., 2018; Belloni et al., 2014). More precisely, we always control for program-period-ward fixed effects, as the strata of the randomization (Bugni et al., 2019). The post-double-LASSO procedure selects which additional ward-year and village fixed effects to control for, in order to maximize statistical power. Our results are qualitatively robust to using OLS instead (see Appendix Table A.1). For the sake of simplicity, Specifications 2 and 3 and all robustness checks are estimated using OLS with fixed effects.

Due to the logarithmized dependent variable, estimates for β_p can be interpreted as semi-elasticities. To deal with the skewness of the distribution of forest loss while keeping observations with zero loss, we explore several alternative approaches. First, in our main specification, we compute the logarithm after adding a small constant of 0.075 ha. (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. Second, in Appendix A we show that our results remain qualitatively comparable when using the inverse hyperbolic sine (Appendix Table A.5), or the untransformed area of forest loss (Appendix Table A.6) as dependent variables. For those variations, results are in fact stronger in terms of statistical significance.

We interpret the estimates as average treatment effects, because non-compliance to the treatment assignment was negligible.[9]

Specification 2: Spillover effects

In Specification 2, we exploit the fact that the experimental design of the CDD program also allows us to identify spillover effects from treated villages to neighboring villages. This is because, conditional on the number of neighboring *CDD-eligible* villages, the number of neighboring *treatment* villages is random and thus independent of village characteristics. The type of spillovers we estimate here are not necessarily interactions between villages or externalities across villages. For example, estimated spillovers may also be due to plots de facto belonging to one village but located within the buffer of another village.

Spillover effects are not restricted to CDD-eligible villages. Thus, while the direct treatment effect can only be identified from the comparison of CDD-eligible treated and control villages, the spillover effects are identified from variation among all villages that have CDD-eligible neighbors. Consequently, our estimates for the spillover effects are estimated on and identified from the sample of all settlements in wards with above median forest cover.

Specification 2 is described by the following regression model:

$$\log(\text{loss}_{vwt}) = \sum_{p\in\{[2008,2010],[2011,2018]\}} \mathbb{1}_{(t\in p)} \cdot \Big(\beta_p \cdot \text{treatment}_v + \theta_p \cdot \text{eligible}_v + \sum_{d\in\{2\text{ km},2\text{–}5\text{ km}\}} \gamma_p^d \cdot N^d_{\text{Treat},v} + \eta_p^d \cdot N^d_{\text{Elig},v}\Big) + \alpha_v + \delta_{wt} + \varepsilon_{vwt}, \tag{2}$$

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 $N^d_{\text{Elig},v}$, 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.

When this specification is estimated on the sample of all village, including settlements that were not eligible for the CDD program, the coefficients capturing the direct treatment effect, β_p, are still solely identified from variation within the sample eligible of villages. Any differences between eligible and non-eligible villages will be captured in θ_p. The coefficients capturing the spillover effects, γ_p^d and η_p^d, are identified from variation within eligible *and* ineligible villages. Thus, the estimated effects are average treatment effects across eligible and ineligible villages. Appendix Table A.8 shows that estimating the spillover specification on the subsample of eligible villages yields almost identical point estimates for all effects, suggesting that the effects are comparable in eligible and ineligible villages. The estimates shown in Appendix Table A.8, which are based on the subsample of eligible villages, naturally have larger standard errors, as they are estimated on a smaller sample.

Specification 3: Treatment effect heterogeneity

To investigate possible channels, Specification 3 extends the difference-in-differences approach of Specification 1 to study if the

[9] The program's administrative disbursement data indicate high compliance. The records lack disbursement information on only four rural villages in the high forest sample that were assigned to treatment. This amounts to less than 2% of the assigned villages (13 villages, or 3%, in the full rural sample that includes low-forest wards). Only two rural control villages appear in the program's disbursement records, both located in high-forest wards. This amounts to 1% of the villages in the high-forest sample or 0.5% of the full sample.

impact of the CDD program on deforestation differs by pre-treatment village characteristics. To this end, we focus on the characteristics identified as correlates of deforestation in previous studies, namely transportation costs, population, poverty, and ethno-linguistic fractionalization (ELF) (Busch and Ferretti-Gallon, 2017; Burgess et al., 2012). For each of the variables used in the analysis of heterogeneous effects we estimate separate effects for villages with high values (i.e., above the median value) and low values of the respective variable. Treatment effect heterogeneity is tested using the following extension of the difference-in-differences approach of Specification 1:

$$\log(\text{loss}_{vwt}) = \sum_{p\in\{[2008,2010],[2011,2018]\}} \mathbb{1}_{(t\in p)}\text{treatment}_v \left(\beta_p^{\text{high}}\text{high}_v + \beta_p^{\text{low}}\text{low}_v\right) + \alpha_v + \delta_{wt} + \varepsilon_{vwt}, \quad (3)$$

where high_v and low_v is a binary median-split indicators for the various village-level characteristics (i.e., the indicator high [low] is equal to one if the value is above [below] the median for that variable and zero otherwise), so that β_p^{high} captures the treatment effects for villages with a high value for the respective characteristics. Conversely, estimates for β_p^{low} captures the treatment effect estimate for villages with a low value. α_v and δ_{wt} are village and ward-year fixed effects. Panel C of Appendix Table C.14 provides evidence that these sample split variables are balanced between the treatment and the control group.

Inference

Standard errors for Specifications 1 and 3 are estimated allowing for heteroscedasticity and autocorrelation of the regression model error at the village level. In Appendix Table A.7 we show that this inference method is the most conservative out of a large set of alternative inference methods, including inference based on ward-level cluster-robust standard errors, randomization inference, modeling the spatial dependence using Conley inference, and bootstrap-based alternatives. Standard errors for the spillover regressions (Specification 2) have to account for the spatial correlation of the model error. This is because spatially proximate villages may have common neighboring villages, which induces spatial correlation in the number of treated neighbors and thus in the model error. We implement Conley inference, using 10 km as a cutoff (note that villages that are farther apart cannot share a third village within their 5 km perimeter). As additional robustness checks, several tables show p-values based on suitable alternative inference methods were appropriate: E.g., ward-level cluster-robust standard errors in the case of Appendix Table A.1 and randomization inference for columns 4–6 in Table 1, which also accounts for the cross-village dependence structure of $N^d_{\text{Treat},v}$.[10]

4. Results

4.1. Pre-treatment trends and balance

Before moving to the regression results, we discuss balance. Appendix Table C.14 presents evidence supporting cross-sectional pre-treatment balance along a number of important dimensions: pre-period forest density and loss, geographic characteristics, the village-level characteristics used for Specification 3, as well as several village-level characteristics based on the census 2003. The tests presented in this table confirm the successful randomization and that treatment and control group are comparable. Yet, differences in levels would in any case be accounted for in the difference-in-differences specification, through the village fixed effects. Differences in trends, due to an exceptionally unfortunate draw in the treatment assignment, would be a more serious concern if they were present. Since we have seven years of pre-treatment data, we can test for differential time trends between the treatment and control group using a specification that is analogue to Specification 1, i.e., that accounts for village fixed effects and ward-year fixed effects. These results, presented in Appendix Table C.15, show that there was no significant difference in pre-treatment deforestation trends between treatment and control villages.

4.2. Treatment effects

The estimates of the magnitude of the effect of the development program on forest loss are based on a difference-in-differences analysis that exploits the experimental setup further. The estimation results of Specification 1 and Specification 2 are presented in Table 1. The estimate of the average treatment effect in the 1 km buffers around village centroids of CDD-eligible villages is large. The estimates in column 1 indicate that deforestation in treatment villages is 9.2% (p-value = 0.08) larger than in control villages during this period. We use the fitted model to predict estimates for forest loss in the absence of the program (Appendix B discusses the estimation of these predicted values). The model fit implies that a total of 26.3 ha. forest loss within the 1 km buffers of treatment villages are due to the CDD program. This area amounts to 5.6% of all forest loss occurring around the 211 treatment villages in the post-program period. We also use Specification 1 to estimate the average treatment effect based on 5 km buffers and Thiessen polygons. For 5 km buffers we find that deforestation around treatment villages is 16.4% (p-value = 0.01) larger than in control villages. For Thiessen polygons the effect is estimated to be 5.2% (p-value = 0.42).

The results of Specification 2, accounting for spillover effects, qualitatively corroborate these results regarding the direct treatment effect, but provide additional insights on indirect effects. The results in column 4–6 of Table 1 imply that, in addition to a direct treatment effect of a 11.4% increase in forest loss (p-value = 0.02), the treatment leads to deforestation because of spillovers. The additional effect is a 7.3% increase (p-value = 0.08) in deforestation for each treatment village located within 2 km from the centroid and also a 7.3% increase (p-value <0.01) in deforestation for each treatment village located in the wider 2–5 km ring.[11] Again, we use the fitted model to predict estimates for forest loss in the absence of the program, this time also accounting for the indirect spillover effects. For the 1 km buffers, these estimates suggest that one quarter of the total forest loss in all 790 villages of our sample after 2011, is due to the CDD program. For forest loss in different areas around the village centroid, the estimates vary, but are of a similar magnitude (see footer of Table 1, column 4–6).

4.3. Mechanisms: Treatment effect heterogeneity

To investigate possible mechanisms behind the findings so far, we investigate heterogeneity of the treatment effect. Specification 3 extends the difference-in-differences approach of Specification 1 to study if the impact of the CDD program on deforestation differs by pre-treatment village characteristics. For each baseline variable that we consider possibly relevant for deforestation, based on previous work by Busch and Ferretti-Gallon (2017) and Burgess et al. (2012), we estimate separate effects for villages with high and low values of the respective variable. The results for the 1 km buffers are shown in

[10] 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, $N^{2\,\text{km}}_{\text{Treat},v}$, and 2–5 km ring, $N^{2\,\text{km}-5\,\text{km}}_{\text{Treat},v}$. The number of treated villages within certain radii is correlated for villages that are close enough to each other. When conducting randomization inference following Heß (2017), 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.

[11] A distance of 2 km was chosen, as it constitutes the smallest distance at which two villages' 1 km buffers are not overlapping.

Table 1
Average annual treatment effect on deforestation.

	Direct effects (Eligible villages, Specification 1)			Spillover effects (All villages, Specification 2)		
	(1) $\log(\text{loss}^{1\,\text{km}})$	(2) $\log(\text{loss}^{5\,\text{km}})$	(3) $\log(\text{loss}^{poly})$	(4) $\log(\text{loss}^{1\,\text{km}})$	(5) $\log(\text{loss}^{5\,\text{km}})$	(6) $\log(\text{loss}^{poly})$
post–program (2011–18) × treatment	**0.092**	**0.164**	**0.052**	**0.114**	**0.211**	**0.085**
	(0.08)*	(0.01)**	(0.42)	(0.02)**	(0.00)***	(0.17)
				[0.07]*	[0.00]***	[0.24]
post–program (2011–18) × $N^{2\,\text{km}}_{\text{Treat}}$				**0.073**	**0.153**	**0.020**
				(0.08)*	(0.01)**	(0.65)
				[0.21]	[0.03]**	[0.73]
post–program (2011–18) × $N^{2\,\text{km}-5\,\text{km}}_{\text{Treat}}$				**0.073**	**0.125**	**0.089**
				(0.00)***	(0.00)***	(0.00)***
				[0.06]*	[0.02]**	[0.01]**
implementation (2008–10) × treatment	−0.003	0.024	−0.009	−0.001	0.026	−0.004
	(0.95)	(0.69)	(0.88)	(0.99)	(0.69)	(0.95)
				[0.99]	[0.76]	[0.96]
implementation (2008–10) × $N^{2\,\text{km}}_{\text{Treat}}$				0.011	−0.014	−0.007
				(0.85)	(0.84)	(0.90)
				[0.87]	[0.90]	[0.92]
implementation (2008–10) × $N^{2\,\text{km}-5\,\text{km}}_{\text{Treat}}$				0.025	0.008	0.001
				(0.35)	(0.85)	(0.98)
				[0.49]	[0.91]	[0.98]
eligibility × period indicators				✓	✓	✓
Observations	7236	7236	7236	14 220	14 220	14 220
Villages	402	402	402	790	790	790
Mean $N^{2\,\text{km}}_{\text{Treat}}$				0.851	0.851	0.851
Mean $N^{2\,\text{km}-5\,\text{km}}_{\text{Treat}}$				2.747	2.747	2.747
Total post-program loss in ...						
... CDD villages (ha.)	467.0	11 624.9	970.6			
... all villages (ha.)				1660.3	44 086.7	3707.7
Part that is due to CDD	26.3	830.9	63.2	453.8	19 172.1	747.3

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. For columns 1–3, p-values in parentheses are based on cluster-robust standard errors, allowing for clustering of the model error at the village level. For Specification 2, in columns 4–6, 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, which implies that neither of the three variables relating to treatment can be spatially correlated beyond this distance. p-values in square brackets are based on randomization inference as described in Footnote 10 and in Heß (2017). All regressions control for the treatment indicator and ward-period indicators, to account for the fact that wards constitute the strata of the treatment randomization. Additional controls are village fixed effects and ward-year fixed effects. For the estimation of Specification 1 in columns 1–3, a post double-LASSO approach (Ahrens et al., 2018; Belloni et al., 2014) is used to select which ward-year fixed effects to control for. Specification 2 in columns 4–6 is estimated using OLS. Control means in the table footer are computed using the unlogarithmized forest loss variables. The estimated total loss due to the CDD projects is, estimates the counterfactual outcome for treatment villages using the procedure described in Appendix B.

Table 2 (detailed estimation results for 5 km buffers and polygons are presented in Appendix Tables A.9 and A.10). The only heterogeneous effect that is statistically significant is distance to roads, a proxy for transportation costs (Alix-Garcia et al., 2013). 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. The coefficient estimates in Table 2 imply that treatment villages that have above median distance to roads, i.e., worse transportation infrastructure, have a large and positive effect, a 21.2% increase in forest loss. The treatment effect is small and insignificant for villages closer to roads. This difference is robust to controlling for baseline forest cover, population density and the interaction of these two variables with treatment and period indicators (Appendix Table A.11), suggesting that road access is not capturing the effect other dimensions of remoteness. Instead, villages with better road access are likely to have better access to markets as well, and may thus be able to mitigate the pressure on land and forest resources resulting from development programs.[12]

Further, we study the correlation between the type of project chosen by a village and deforestation. We observe that treatment villages invested their funds in a heterogeneous set of projects (see Appendix Table C.13) and that this spending behavior is correlated with forest loss. Several villages chose non-agricultural investments, but most villages invested at least part of their budget in projects related to agriculture. In Appendix Table A.12, we summarize findings on how post-program forest loss developed differently depending on how the villages allocated funds between agricultural an non-agricultural projects. In particular, we find that villages that used larger shares of their funds for agricultural projects experienced more forest loss farther away from the village center. In contrast, villages that used more of their funds for non-agricultural projects, experienced more forest loss in the 1 km buffer. Clearly, project choice is endogenous and villages with and without agricultural projects likely differ in relevant characteristics. Thus, the observed difference in forest loss cannot be interpreted as evidence for a causal link between project choice and deforestation in different distances to the village, but it is consistent with such hypotheses.

4.4. Mechanisms: Household-level channels

In order to shed light on further potential mechanisms as they are hypothesized in the literature, we test whether the CDD has effects on outcomes that possibly connect the program to 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 to gain insights into how the CDD program affected economic welfare, livestock ownership, consumption of forest-intensive goods, and village population growth. These results are discussed in this section. We also use data from the Integrated Household Survey 2015 (IHS 2015 henceforth), a comprehensive household-level survey. The results based on these data are presented in greater detail in Appendix C.3.

[12] In line with this interpretation, we find suggestive evidence that villagers' expectations for land prices increased in treatment villages with limited road access compared to control villages with limited road access (Appendix C.4).

Table 2
Heterogeneous effects by pre-treatment variables for 1 km buffers (Specification 3)

Village-level split:	Distance to road	Population	Poverty	ELF
	(1) $\log(\text{loss}^{1\,\text{km}})$	(2) $\log(\text{loss}^{1\,\text{km}})$	(3) $\log(\text{loss}^{1\,\text{km}})$	(4) $\log(\text{loss}^{1\,\text{km}})$
low × **post–program (2011–18) × treatment**	**−0.040** (0.61)	**0.068** (0.39)	**0.102** (0.18)	**0.069** (0.34)
high × **post–program (2011–18) × treatment**	**0.222** (0.00)***	**0.115** (0.13)	**0.083** (0.30)	**0.118** (0.15)
low × implementation (2008–10) × treatment	−0.015 (0.87)	−0.018 (0.83)	−0.076 (0.35)	0.031 (0.69)
high × implementation (2008–10) × treatment	0.016 (0.83)	0.023 (0.76)	0.071 (0.36)	−0.043 (0.60)
split indicator × period	✓	✓	✓	✓
Observations	7236	7236	7236	7236
Villages	402	402	402	402
p-value $\beta^{\text{high}} = \beta^{\text{low}}$ (post-program coefficients identical)	0.018	0.216	0.236	0.225

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 2018. 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 interaction terms with binary indicators dividing the sample according to the median of each pre-treatment village-level variable. The coefficients can thus be interpreted as treatment effect estimates for the sub-group defined by the sample split. Ward-year and village fixed effects are included in all specifications.

For the empirical analysis we again take advantage of the experimental design of the CDD program, estimating the following specification:

$$Y_{hvw} = \beta \cdot \text{treatment}_v + X_v \cdot \delta + \alpha_w + \varepsilon_{hvw}, \tag{4}$$

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 2.1). 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.[13]

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.

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 the introduction, predicts an increase in environmental degradation. From the Census 2013 we build an asset z-score, which we take as a proxy for a household's wealth.[14] The index is scaled to have mean zero and variance one in the control group. The results in Table 3, Panel B indicate that the CDD program had a positive and statistically significant effect on the asset index in the full sample (p-value = 0.075). In the sub-sample of high forest cover villages (Panel A) the estimate is even larger (p-value = 0.048). 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. (2021), 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.[15]

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 z-score, composed of variables capturing ownership of different types of livestock covered by the Census questionnaire (cattle, goats, sheep and poultry). The results in column 2 of Table 3 show no strong effect of the program on livestock ownership for the full sample. Yet the results for the high forest cover sample suggests that the livestock PCA-index is slightly larger in the treatment group (p-value = 0.097).[16]

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

[13] 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.

[14] The asset z-score combines indicator variables for ownership of vehicles, electronic devices and other assets.

[15] 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.

[16] This result is weaker when binary indicators or the imputed monetary value of livestock are used to compute the index instead of the z-score.

Table 3
Household channels: Results using data from Census 2013.

	(H1)	(H2)	(H3)	(H4)		
	(1) Asset z-score	(2) Livestock z-score	(3) Firewood	(4) Share migrant	(5) #children	(6) Village size
Panel A: Villages with above median forest cover						
treatment	0.139	0.075	−0.002	0.014	0.391	3.769
	(0.048)**	(0.097)*	(0.796)	(0.233)	(0.125)	(0.756)
Observations	15 585	15 593	15 598	15 561	15 595	389
Control mean d.v.	0.000	0.000	0.982	0.219	10.411	403.210
Panel B: All villages						
treatment	0.086	0.033	−0.002	0.011	0.165	6.852
	(0.075)*	(0.305)	(0.739)	(0.135)	(0.393)	(0.450)
Observations	32 355	32 377	32 386	32 259	32 383	800
Control mean d.v.	0.000	0.000	0.984	0.187	11.230	456.370

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. 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 to make results representative at the village-level. The asset z-score combines indicators for owning a radio, a TV, a mobile, a bicycle and for having access to electricity and to water. To compute the animal z-score, for each animal category, we first create a variable containing the decile the household is in (to reduce the severe skewness of that data) and aggregate these into the z-score. Animal categories are cattle, goats, sheep, and poultry. 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.

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 3 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.

We also use data from the IHS 2015, a comprehensive household-level survey. About one third of the villages in the sample used above are covered by the IHS 2015. Results based on these data are discussed in Appendix C.3 and suggest that there was at most a modest increase in general economic welfare. For this analysis, we aggregate several household-level variables into indices for each hypothesis. Estimates for treatment effects on indices related to H1 and H2 are positive but insignificant using these data, which were collected five years after the program. This may indicate that the effects were larger in years closer to the project (when the Census data were collected) and dissipated over time. Alternatively, this may indicate that the analysis based on IHS 2015-data does not have enough statistical power to detect small effects. In addition to the four hypotheses described above, the data from the IHS 2015 allow us to test additional hypotheses that relate specifically to the Gambia CDD program, in particular to agricultural production and village institutions. We find no evidence for any these channels playing a role here.

Overall, we find that the CDD program has a modest impact on economic welfare, as measured by household wealth and livestock holdings. This is consistent with increases in welfare as a link between the program and deforestation. Yet, we do not find evidence that the effect on deforestation is driven by other channels described in previous literature, such as an increase in the consumption of resource-intensive goods (beyond the apparent increase in livestock holding) or an increase in village population. We also do not find evidence pointing towards a channel specific to the community-driven aspects of the program. In particular, using survey-based indices we find no significant differences between treatment and control villages regarding the villages' institutions.

5. Conclusion

The effect of human development on the environment is the subject of an ongoing debate. While the theoretical predictions are unclear, recent evidence tends to support the idea that development interventions can feature environmental degradation as an unintended side effect (Alix-Garcia et al., 2013; Asher et al., 2020). In particular negative effects on forest cover are a reason for concern in light of climate change. In the context of our study, a development program in The Gambia, we note that forest loss and desertification are identified as key issues in the national climate change adaptation strategy (UNDP, 2015). The role of forests in our semi-arid context is also evidenced by the existence of global collaborations such as the *Great Green Wall of the Sahara and the Sahel Initiative*, a flagship initiative to combat climate change and desertification (UNCCD, 2016). Yet, causal evidence on drivers of deforestation is scarce, particularly for semi-arid biomes such as The Gambia.

In this paper, we take advantage of the experimental design of a nationwide community-driven development program in The Gambia to provide causal evidence about the relationship between rural development interventions and deforestation. We find that deforestation in treatment villages is significantly larger than in control villages in post-program period. Additionally, there is strong evidence for indirect spillover effects from treatment villages on forest loss around neighboring villages. Combining estimates for direct effects and indirect spillover effects, our main results is that about one quarter of the total forest loss around sample villages within an eight year period after the implementation of the CDD program can be attributed to the program.

Our investigation of possible household-level channels considers increases in income. We find that households in treatment villages exhibit modest improvements in economic welfare in the medium run (3–5 years after the program). To the extent to which increased welfare is responsible for the increased deforestation, this 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). Instead, this is consistent with the existence of an environmental Kuznets curve. It is also in line with more recent non-experimental and quasi-experimental evidence (Baland et al., 2010; Alix-Garcia et al., 2013; Cuaresma et al., 2017). We further find that the treatment effect is larger in areas with limited access to markets, as measured by poor road infrastructure. This result, too, is consistent with the hypothesis that improved economic conditions are driving the deforestation results. This is the case as villages located in areas where markets are available will be less reliant on forests for the supply of goods if, as a result of the project, there are changes in consumption patterns (e.g., more use of wood for housing or a more meat-intensive and thus land-intensive nutrition).

The second broad channel discussed in the introduction is structural change in agriculture and production techniques. While we do not find evidence for a significant treatment effect on a survey-based index for modernized agricultural production, many projects in treatment villages directly targeted agricultural productivity. Classifying implemented projects into two categories, we find that forest loss varies

between treatment villages that invested greater shares of their funds in agricultural projects and treatment villages that invested more in non-agricultural projects. While this difference does not provide an experimentally identified estimate for heterogeneous treatment effects, it is consistent with the hypothesis that agricultural projects facilitate an expansion of agriculturally used lands, which results in deforestation, and with empirical evidence reported in Foster and Rosenzweig (2003).

We do not find evidence that the increase in forest loss in treatment villages relates to the particular features of the development program that we study, the CDD, which takes a participatory approach and aims also at influencing decision-making processes and local institutions. Therefore, we interpret the documented deforestation mainly as a secondary effect of a program that intends to increase welfare and affect economic activity.

As the area of study is located at the frontier of desertification in the Sahel, our findings are relevant for informing development policy and environmental policy in some of the areas of the world that are most affected by the climate crisis. The findings are a reminder that the design of development interventions needs to incorporate possible environmental impacts in cost-benefit considerations. This is especially important for development projects that relate to agricultural production, given the importance of land use—in particular forestry—for global climate change mitigation (IPCC, 2019).

Appendices and supplementary data

Appendices A-C and supplementary data to this article can be found online at https://doi.org/10.1016/j.jdeveco.2021.102737.

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