Unconditional Transfers and Tropical Forest Conservation.

Evidence from a Randomized Control Trial in Sierra Leone

Abstract: Unconditional conservation payments are increasingly used by conservation non-governmental organizations to further their environmental objectives. One key objective in many conservation projects that use such unconditional payments schemes is the protection of tropical forest ecosystems in buffer zone areas around protected parks where the scope of instating mandatory restrictions is more limited. We use a randomized controlled trial to evaluate the impact of unconditional livelihood payments to local communities on land use outside a protected area – the Gola Rainforest National Park – which is a biodiversity hotspot on the border of Sierra Leone and Liberia. High resolution RapidEye satellite imagery from before and after the intervention was used to determine land use changes in treated and control villages. We find support for the hypothesis that unconditional payments, in this setting, increase land clearance in the short run. The study constitutes one of the first attempts to use evidence from a randomized control trial to evaluate the efficacy of conservation payments and provides insights for further research.

Key words: Africa; conservation; field experiments; land cover classification; randomized control trials; Sierra Leone; tropical deforestation; unconditional payments

JEL codes: Q56, Q1, Q12, Q15, Q2, Q23, Q5, D04, D13. C9, C93, I38

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Conservation payments to local communities have emerged as a prominent conservation tool (Ferraro 2001; Ferraro and Kiss 2002; Wunder 2007; Milne and Niesten 2009). Such payments are provided in different ways (e.g. as cash transfers or in kind payments) and with different degrees of conditionality and additionality (Engel 2015), reflecting a variation in policy preferences and contexts. Despite the prevalence of conservation payments there is a paucity of empirical evidence on their performance, especially using rigorous evaluation methods (Ferraro and Pattanayak 2006; Pattanayak, Wunder, and Ferraro 2010; Miteva, Pattanayak, and Ferraro 2012; Blackman 2012; Cowling 2014; Zheng et al. 2013; Alix-Garcia, Sims, and P. Yanez-Pagans 2015; Baylis et al. 2015; Puri et al. 2016; Samii et al. 2014; Börner et al. 2016, 2017; Ma et al. 2017).

We contribute to addressing this gap in the literature by using a randomized control trial framework to empirically evaluate the conservation impact of one particular type of conservation aid: the provision of livelihood support to local communities *without* specific conditions attached. Such 'unconditional conservation payments' or transfers can be contrasted with so-called 'payment for ecosystem service' (PES) schemes which, at least in principle, entail conditionality such that payments are only made conditional on specific conservation efforts or outcomes (Ferraro et al. 2012; Jack, Kousky, and Sims 2007). Recent reviews of conservation funding suggest that, in practice, unconditional payments are a popular conservation policy mechanism (e.g., Miller 2014; Figaj 2010; Hoeffler and Outram 2011; Hicks et al. 2010), especially for promoting conservation *outside* protected areas (such as in reserve buffer zones). This reflects the realities of conservation policy implementation, which may preclude the introduction of strict conditionality clauses due to problems with assigning property rights and ownership over resources, problems with enforcement, and especially problems with the political acceptability of strict conditionality requirements (Kaczan, Swallow, and Adamowicz 2013; Kaczan et al. 2017, Börner et al. 2017). Indeed, many purported PES-type programs in tropical regions are effectively ‘unconditional’ in the sense that violation of conditionality is often not penalized (OECD 2010, Engel 2015, and Honey-Roses et al. 2009). Unconditional payment schemes are also sometimes seen as potential precursors to eventual PES-type schemes (Caplow et al. 2011, Engel 2015).

The aim of unconditional payments typically is to promote behaviors that align with the goals of the contributing entity, often to relieve pressure on ecologically important habitats. For example, in the context of rural Sierra Leone which we consider here, unconditional conservation payments aim to promote the preservation (or at least the maintenance) and connectivity of remaining forest habitats outside of a protected area known as the Gola Rainforest National Park. In this case, the ultimate policy objective of the park is to avoid encroachment of agriculture in the buffer area around the park, and so the creation of an isolated nature reserve within an agricultural landscape. Such ‘island parks’ may provide fewer ecosystem services (less wildlife protection etc.) and are more vulnerable to encroachment compared with parks in a better-preserved landscape context. They could therefore represent an un-viable and cost-ineffective investment in the long term (e.g., Hansen and DeFries 2007; DeFries et al. 2005; Gascon, Williamson, and da Fonseca 2000; Pfeifer et al. 2012). However, while perhaps convenient for conservationists and popular among recipients, it is not evident that unconditional payments are an efficient or effective way to influence land use decisions. Such payments not only work through more indirect mechanisms compared with conditional payments, there are also concerns that they could affect land use via multiple, possibly offsetting, channels.

This article examines the short-term impacts of an unconditional payment scheme on land cover near the Gola Rainforest National Park (GRNP) in Eastern Sierra Leone. To our knowledge, this study constitutes one of the first randomized controlled trials (RCT) to analyze the impact of conservation payments in the context of (tropical) deforestation and land use change.[1] Beyond a project by Jayachandran et al. (2017) that was conducted concurrently to our study[2] all previous work relies on observational data to assess the impacts of conservation policies on land cover (e.g. see Blackman 2012 for a review). Despite advances in using observational methods to assess conservation policies, important challenges remain in terms of the formulation of a counterfactual scenario, and in overcoming selection bias. In contrast in RCTs, such as the one discussed in this article, enrolment in the transfer program is based on random assignment such that treatment status is orthogonal to community characteristics and other possibly confounding effects (Gerber and Green 2012). We use land cover data obtained from high-resolution RapidEye multispectral satellite imagery for the period before and after the intervention to obtain an independent measure of our main outcome variable of interest: land clearance for agriculture. Our study focuses on the short-term impacts of payments on land cover, for two reasons. First, it is practically difficult to run an RCT spanning the large number of years that would be needed to examine long-term impacts on land use change (Ferraro and Pattanayak 2006). Second, the evaluation was designed in close partnership with the GRNP authorities who wanted to learn about the more immediate positive or potential harmful impacts of their payments on land use.

The RCT randomly assigned 69 villages to treatment, where each village received an amount corresponding to US$15 (in vouchers) for each household, and 22 villages to the control group. Using detailed remote sensing data, we then compare the impact of the intervention on land use observed before and after the transfers were made. Our results suggest that for the case of Eastern Sierra Leone, with low population densities and relatively abundant land, unconditional payments lead to increased levels of land clearing for agriculture. This result is stable across a series of specifications and robustness checks. Importantly, we also show that increased land clearing is predominantly carried out on land with young vegetation regrowth. The rate of clearing mature forests (including within the GRNP) remained low and unchanged. We use survey data to probe into possible mechanisms linking unconditional payments to land clearing. We note that our experimental design was not set up to explicitly test these mechanisms, and as a result our discussion is suggestive. The data suggest that unconditional transfers invite additional land clearing for agriculture by likely crowding-in of additional labor. Plausible mechanisms of how this could materialize are discussed.

Conceptual Framework

The main features of the conservation payment scheme described in this study were determined by the local policy context and project partners that manage the activities in and around the Gola Rainforest National Park. In close collaboration, the research team designed an impact evaluation of this scheme, and worked to ensure the scientific validity of the experimental protocol and the sampling procedure. Here we describe these features and embed them in the relevant literature.

The conservation payment scheme evaluated here is akin to an unconditional transfer used extensively in many other areas of economic development policy (e.g., education, healthcare, labor projects). Empirical evidence suggests that unconditional transfers are a potentially cost-efficient and effective avenue for promoting policy objectives (see, for example, Kohler and Thornton 2011, Blattman, Fiala, and Martinez 2013). Several recent reports have shown how such payments are especially prevalent in sub-Saharan Africa (compared with Latin America and Asia (Davis, et al. 2016; Bastagli 2016; IDS 2009)). In practice a significant number of conservation payments are, either by design or due to lack of enforcement, *unconditional* (see Engel 2015, Honey-Roses et al. 2009). In our case, the NGO opted for an unconditional scheme for reasons related to the local policy context as well as practical considerations (see below).

Our specific unconditional payment scheme had some particular distinguishing features. First, our intervention has similarities to what is referred to by the literature as 'labelled unconditional transfers' or 'labelled cash transfers' (see Benhassine et al. 2015). These schemes fall somewhere between pure conditional- and pure unconditional-transfers. The framing or labelling of the aid package makes the policy objectives more salient and generates a type of 'endorsement effect' (akin to that induced by various framing nudges). In our case, the NGO gave an unconditional aid package with a statement highlighting the environmental objectives of the NGO and the importance of preserving the Gola forests.

Secondly, the intervention consisted of a one-off transfer, as opposed to recurrent payments. This design feature is also not unique to our case. Many unconditional transfers in real policy settings consist of one-off (or windfall) payments, provided once or over a very short period (Bastagli et al. 2016; Haushofer and Shapiro 2016). This can be explained by both logistical and budgetary reasons, but also reflects that these payments often aim to measure an initial short-term (trigger) reaction. Such one-off inducements also resemble behavioral 'nudges' that have received considerable attention in the last few years both within academic and policy circles (see Benhassine et al. 2015).

Thirdly, the amount provided in our intervention may appear somewhat more modest than that offered in other prominent one-off payment schemes (such as GiveDirectly which gives amounts close to $1000/household). In reality, the magnitude of payment per household varies considerably in such schemes, with many interventions, including community-driven development (CDD) projects, offering less than $10 per capita (see Casey (2018) for a review). The magnitude of one-off payments has varying impacts on multiple facets of behavior (savings, consumption patterns, wellbeing etc.). In our case, the offered amount (worth a nominal minimum value of up to US$15/household) was chosen on the basis of both policy and practicality criteria. Our collaborating policy organizations were primarily interested in measuring the short-term reaction of households to a moderate but non-trivial transfer. Their interest was to see if such an amount would lead to a discernable behavioral change to inform the design of future conservation policies. Also, the amount offered was close in magnitude to transfers of potential future REDD+ projects (a policy option that park authorities were considering as a means to finance conservation of the national park in the future).

Finally, the scheme offered payments in the form of vouchers that could be exchanged for goods as opposed to a direct cash transfer (see details below). Vouchers are extensively used in the field for numerous reasons.[3] The relative significance of using in-kind vouchers versus cash transfers is subject to a limited but emerging empirical literature (e.g. Hidrobo et al. 2014; Aker 2017).

In sum, the intervention consisted of a labelled unconditional transfer scheme, made as a one-off payment, of a moderate but non-trivial size, which was offered in the form of vouchers. What is unique about our study is that we experimentally evaluate the use of unconditional payments in a conservation policy context. In the next section, we discuss some of the mechanisms through which these payments could impact behavior that may ultimately manifest in a discernable change in land cover.

Unconditional payments and conservation

While the impact of conditional payments (i.e. PES-type incentive schemes) on behavior and outcomes are well understood from a theoretical perspective (Wunder 2013; Engel, Pagiola, and Wunder 2008; Engel 2015; Persson and Alpízar 2012), the mechanisms through which unconditional payments impact on conservation objectives are more complex and potentially ambiguous.

Unconditional payments may promote conservation through various intermediary channels. First, payments could create a sense of ‘good will’ towards conservationists and park authorities, inviting reciprocal sentiments from local communities. As noted above, such payments are often made under a particular framing or labelling, normally with some reference to the donor's policy aims and the benefits of these aims. This in turn acts as a nudge or trigger of an 'endorsement effect' or 'community buy-in' effect (as in the study by Benhassine et al. 2015). This effect is similar to the 'winning the hearts and minds' argument for providing livelihood aid that has also been experimentally studied in several other contexts (McNeely 1993, Andrabi and Das 2016; Beath, Christia, and Enikolopov 2017).

Secondly, payments may also relax binding constraints, enabling communities to alter their land use practices or engage in off-farm employment. Such behavioral changes could potentially reduce pressure on marginal lands (Wunder, Engel, and Pagiola 2008; Angelsen and Kaimowitz 1999). For example, transfers may be used to improve agricultural efficiency through increased fertilizer use, reducing pressure at the "extensive margin" to grow food and thus allowing more land to be preserved for conservation (Phalan et al. 2011; Bationo et al. 2012; Louhichi & Gomez y Paloma 2014). Alternatively, households can be 'trapped' in sub-optimal labor allocation decisions, allocating more labor on farm (and exerting excessive pressure on natural resources) because of binding constraints that deter them from accessing more profitable off-farm labor opportunities. Conservation payments (even with very low levels of conditionality in practice) may relax these constraints (such as liquidity constraints) and allow rural households to break this cycle of poverty and environmental degradation (Uchida, Rozelle, and Jintao 2009; Groom et al. 2010). In addition, there could be an "income effect" associated with transfers, which could increase the demand for leisure. In the context of labor scarcity and imperfect labor markets, extra consumption of leisure could also relieve pressure on natural habitat.

However, the impact of transfers need not necessarily be benign, and they might backfire from a conservation perspective.[4] For example, transfers may alleviate constraints on land management practices that encourage additional land clearance (Angelsen and Kaimowitz 2001, Lybbert et al. 2011). In the case of Sierra Leone where shortage of local labor generally limits agricultural activity (Cartier & Bürge 2011; MAFFS, SSL, and IPA-SL 2012; Chenoune et al. 2016), and where the majority of hired labor is used for clearing vegetation for farming (MAFFS, SSL, and IPA-SL 2012), unconditional payments could be used to hire additional labor to convert more forest land to farm land. Moreover, one time (windfall) unconditional payments may trigger strategic behavior from recipients that could be detrimental to conservation. In particular, aid recipients may clear more land in order to better position themselves in an anticipated future negotiation setting (Harstad 2016).

Of course, it is also possible that payments are spent in ways that do not impact on land use practices in any discernible way. Further, it is likely that the impact of payment schemes will vary across communities, mediated by factors such as market access and agricultural suitability (Pfaff 1999; Kinnaird et al. 2003; Pfaff et al. 2009), or that large-scale transfer schemes have general equilibrium effects (affecting prices of factors and commodities) in the case that local economies are imperfectly integrated in regional economies (e.g. Angelsen et al. 2001). For example, if local demand for labor increases, wages are bid up which might invite an inflow of agricultural labor. Similarly, income transfers to the poor (especially if significant and recurrent) can increase demand for land intensive consumption goods which, under certain conditions, can lead to increases in deforestation. For example, Alix-Garcia et al. (2013) explore the impact of the Oportunidades program on deforestation in Mexico. The authors find a significant increase in deforestation and attribute this to a shift in the consumption of more land-intensive goods (such as meat and milk). Further, the adverse effect on deforestation is higher in communities that are isolated and have inferior access to markets so they cannot meet the increase demand from outside sources. Yet, as discussed by the authors, these results cannot be generalized to other areas with different market structures, and where an increase in income leads to higher demand for forest products which could in turn cause a reduction in deforestation (as in Foster and Rosenzweig 2003). Beyond local context, the frequency of payments, whether the extra source of income is earned or not earned, the stringency and nature of the conditions attached to aid funds, and the size of payments, all modulate impacts (Shively and Pagiola 2004).

Overall, theoretical predictions with respect to the conservation effects of unconditional payments are not easily predetermined. In our case, the one-off payments are hypothesized to have intermediate effects on incomes and expenditure (on both consumptive goods and inputs) as well as on attitudes ("hearts and minds"), which in turn may impact on land use decisions – the final outcome variable of interest. Assessing even the existence and direction, positive or negative, of such effects, regardless of the mechanism of impact, remains an empirical matter which to date has been sparingly and non-systematically explored.

Context of the study: Land use in Sierra Leone

Our empirical study is based in rural Sierra Leone, in an area surrounding the Gola Rainforest National Park (GRNP) on the international border with Liberia (see Panel (A) of figure 1). The GRNP is a 71,000 hectare remnant of upper Guinean moist tropical forest, and spans seven chiefdoms across the districts of Kailahun, Kenema and Pujehun. The forest was officially established as a national park in 2011, but its protection has been evolving over the last 20 years through efforts by external NGOs and local governments and conservation agencies. Protection of the GRNP derives foremost from restrictions on logging and extraction of plant, animal and mineral resources from within its boundaries. Most of the efforts and resources of the GRNP authorities are to compensate local communities for these restrictions, or to monitor and enforce them. Due to the establishment of the GRNP, compensation for direct losses of income from land usage restrictions have been provided to communities living within one mile of the GRNP boundary. Our study villages are located within a one to seven mile band from the GRNP boundary, and therefore they have not received direct conservation payments in the past. Satellite images and field observations suggests that the objectives of protecting forest cover *inside* the reserve itself have been largely met (Gola Rainforest Conservation LG. 2013).

<< Insert figure 1 about here >>

In recent years, the GRNP authorities have emphasized the promotion of sustainable land management in forest areas *beyond* the reserve boundaries, where legal restrictions on resource-use enacted to protect the GRNP do not apply.

In our study region (figure 1, panel A), agricultural practices are typified by subsistence slash-and-burn rotational cropping of annual crops (upland rice, cassava, vegetables). There are also plantations of cash crops such as coffee, cacao and oil palm. The use of fertilizer in the region is very low, as most communities do not have access to the necessary markets, and transportation costs are prohibitive for most farmers (Cartier & Bürge 2011; Casaburi, Glennerster, and Suri 2013). There is a marked dry and wet season in Sierra Leone, and as such there is one agricultural cycle per year. To establish a new upland farm, natural vegetation is cleared ("brushed") using machetes, and after allowing the cut vegetation time to dry, controlled fires are then set to clear the area ready for planting. Clearing occurs at the beginning of the year, mostly in January and February, and burning in March and April. Afterwards the land is ploughed and sown, and harvests are typically reaped in September – December. Many rice fields are intercropped with crops such as cassava, which leads to a longer harvest period. Despite this, there is still a marked 'lean season' in June and July, before the first harvests of the year. Large work groups of laborers are especially advantageous due to the critical timing of certain agricultural activities, primarily burning. Dry weather is needed between vegetation clearance and burning to ensure a good burn on the field, which maximizes the nutrients released to the soil, minimizes weeds and pests later in the season, and ultimately determines the yields that can be achieved (Richards 1986). Although burning may only take half a day, it can require larger groups of workers to control the fire and ensure it does not spread to neighboring fields. Cleared land generally remains under cultivation for 2 – 3 years, until the soil nutrients are depleted or the weed and pest load becomes too great. After this, the land is left fallow for 6 – 10 years (Bulte et al. 2013), during which time scrub vegetation and young secondary forest will establish, known as 'farmbush', and in doing so regenerate soil nutrients for the next agricultural cycle. Mature forest in our context is forested land that has never been farmed, or that has been left fallow for more than 25 years. These forests contain larger trees and represent a more valuable habitat from a conservation perspective, being a more structurally and species diverse habitat than farmbush and a larger store of aboveground carbon. Farms established on land cleared from mature forests are typically higher yielding and allow for shorter fallow periods for the first few agricultural cycles than farms cleared from farmbush, due to higher concentrations of soil nutrients and fewer weeds. However, despite being potentially lucrative, clearance of the large trees in mature forest is strenuous for farmers in Sierra Leone as most farmers clear land by hand using low-grade machetes. Establishing a farm from mature forest therefore requires more labor (initially), compared with farms established from farmbush.

Labor is a significant limiting factor for agricultural activity in Sierra Leone. Survey responses to the national Agricultural Household Tracking Survey (AHTS) for Sierra Leone (MAFFS, SSL, and IPA-SL 2012) confirm that most households (80%) in eastern Sierra Leone hire external labor at some point in the agricultural cycle, and for all agricultural activities more than half of households reported a shortage of labor. The peak demand for hired labor occurs between January and March, when land is being cleared and burned to establish new farms. Land clearance is also the agricultural activity for which labor shortage is reported as the most acute. According to the responses in the household survey of the AHTS for upland farms, approximately 1/3rd of annual labor requirements on farms in Sierra Leone come from hired labor, with the rest from reciprocal labor agreements within the community, and from household labor. The mean reported wage for general agricultural labor in the AHTS for eastern Sierra Leone is 6822 Leones per day (1.7 USD/day), including the value of in-kind payments and meals.

Agricultural expansion and habitat fragmentation is believed to be the primary threat to mature forests both within, and surrounding, the national park (Gola Rainforest Conservation LG 2013). The extent of agricultural land and forest fragmentation can be seen in the land cover map shown in figure 2. The figure clearly shows the extent of slash and burn agriculture depicted in the yellow and light green areas that are overwhelmingly surrounding the park, and relatively few areas of mature forest remain in the zone outside the national park boundary. Since the national park is not one contiguous area of forest cover, but rather split into blocks (two large and two small), the maintenance of mature forest patches outside of the national park is particularly important to enhance the connectivity of habitat between the national park blocks. Forest patches such as these have been hypothesized to act as stepping stones for the movement of animal and plant species across the landscape (Saura, Bodin, and Fortin 2014). For this reason, GRNP authorities have explored means for providing incentives to lessen potential pressures for forest conversion outside the protected area. Since conditional payments were regarded as unpractical (on both logistical and "political acceptability" grounds), unconditional payments have been used in this study as the preferred policy mechanism.[5, 6]

Experimental design

We use data from a field experiment in which one-off payments were made to randomly selected communities in 2011. The experiment centers around the so-called Livelihood

Community Development scheme involving 91 villages within 1 – 7 miles of the GRNP boundary (figure 1, panel A). In total, 69 villages received an unconditional aid transfer and 22 did not, with random selection of treatment villages stratified at the chiefdom level.[7] Under this scheme, each of the 69 treated villages were given vouchers worth up to 60,000 Leones (SLL), approximately 15 USD, for each household in the village. This amount per household is cash equivalent to nine days wages for unskilled labor (MAFFS, SSL, and IPA-SL 2012). The total value of payments to treatment villages therefore varied between villages, dependent on the number of households in the village (mean household number in our study villages was just below 40). Payments were made by the NGO and described as a gesture of goodwill to promote sustainable management of forests, and to improve livelihoods. Treated villages were not given any instructions as to what to do with their aid, nor were any conditions attached for receiving it (see Annex 3 for a representative MoU between GRNP and a local village and Annex 4 for a representative consent form).

The NGO had hypothesized possible indirect channels through which aid could trigger land use changes with implications for conservation, but they purposefully left the set-up of the aid distribution flexible.[8] The research aim was not to test any particular mechanism but to explore if (in principle) the payments could trigger a discernable short term impact on land use decisions. These could be plausibly through intermediate effects on incomes and expenditure (of both consumptive goods and inputs) as well as on attitudes ('community buy-in'). No cash was distributed in the program. Instead, treated communities could use the vouchers to order goods from a pre-specified list (see Annex 2).[9] This list included 41 consumption, investment and public goods such as food, tools, agricultural inputs, and building materials. Prices on the goods menu reflected local market prices in Kenema, however since the payments were made in-kind the actual value of the aid package is higher than the nominal value of the vouchers as the transportation costs associated with obtaining such goods and materials is nontrivial in these remote communities.[10] The goods list contained not only prices and item descriptions, but also pictures to facilitate comprehension by illiterate respondents. Vouchers came in increments of 10,000 Leones, and each had a unique identification code. The vouchers were unique to each village and only valid within the village of origin, and for the duration of the intervention. Households could choose to spend vouchers individually, to bundle them and spend them with other households, or even pool them at the village level. Of course, how the aid was actually utilized is not directly observable, nor were implications in terms of fungibility of the aid. In order to gain some insights as to where and how aid was spent follow-up survey data were collected with the village chiefs (when aid was initially distributed) and with the household recipients of the vouchers (at end-line).

The funds were transferred to treatment villages between the end of April 2011, and beginning of June 2011, with the majority of payments delivered in May 2011. The timing of our payments came after the peak labor demand in 2011, when new farms had already been established for that year. The next substantial demand for hired agricultural labor would be at the beginning of the agricultural cycle in February 2012, and farms established by clearing land during this period would bring land into active management for 2-3 years from that date. Our satellite observations were taken in January 2011 (prior to the intervention) and December 2012/January 2013, after the full agricultural cycle of 2012 and before clearing had begun for the agricultural cycle in 2013. Any differences in land cover between our control and treatment villages are therefore most likely to have come as a result of activities during 2012 agricultural cycle.

Description of land cover data

Our main outcome or dependent variable is land cover change, classified from satellite observations. A particular strength of this study is that our dependent variable is more objectively and accurately measured than outcome variables from a survey module on stated and recalled land use. For the land cover data, we use high resolution RapidEye multispectral satellite imagery at 5x5 m pixel resolution from before (dated 13 January 2011) and after (dated 6 December 2012 and 21 January 2013) the intervention (see panels B and C in figure 1).[11] We classified the satellite data into the categories of bare soil, farmbush (including all non-forest vegetation), mature forest and water. Definitions of these types of land cover are provided in table 1 (see also Annex 5 for details of the classification method).[12] Water pixels (0.33%) in either 2011 or 2012/13 were excluded from the analysis.

The difference in land cover classifications between the 2011 and 2012/13 images is summarized in figure 2. Since the study period was rather short, it is not surprising that land cover changes are generally small in magnitude. In this study, we define land clearance as land that was classified as farmbush or mature forest in 2011, and classified as bare in 2013. As might be expected, within the GRNP boundary very little land clearance took place.[13] For our analyses we only consider land cover outside the GRNP where the land is owned and managed by communities.[14] Within the study area, a little under 3700 ha of vegetation was cleared (forest or farmbush vegetation in 2011 to bare land in 2012/2013), representing 8.62% of the total area, and 10.6% of all land that could potentially be used for agriculture (farmbush or forest in 2011). At the same time, just over 4470 ha of land transitioned from bare soil to vegetation, representing 10.4% of total land. Contiguous patches of clearance from farmbush (median size = 100m^2, interquartile range = 25m^2 to 375m^2) are generally larger than those cleared from mature forest (median size = 75m^2, interquartile range = 25m^2 to 375 m^2). In total, 321,100 patches of farmbush were cleared in the study area, compared with just 69,830 patches of mature forest.

Due to cloud cover, the RapidEye images do not cover 12 out of the study 91 villages. Our final analysis sample comprises of 79 villages of which 62 are treated villages and 17 are control. These missing villages are all in the North East of the study area, and all but one originate from the same chiefdom. Due to our block randomization at the chiefdom level, dummies in the analysis soak up the different treatment propensities within each block. Dropping the block with most missing data from the analysis does not alter the results (see Annex 1, table A1 and A2).

<< Insert table 1 about here >>

As is common in this region, precise boundaries of land holdings per village are unknown. However, we have data on total estimated land-holdings from socio-economic surveys implemented in 2011, and so attributed land to villages via weighted Voronoi polygons. The polygons rest on the initial assumption that the farmer for any given plot of land is from the village with the shortest Euclidean distance to that land. We then expand or contract our polygons using land-holding data from our surveys to improve the estimation of land boundaries, validated against 98 GPS locations of known village boundaries collected in the field. For more information on this procedure see Wilebore and Coomes (2016).[15] In total, the Voronoi polygons of the 79 study villages cover 42,866 ha, with a median village size of 470 ha (interquartile range 279 ha – 655 ha).

Description of survey data

Baseline and endline surveys were collected. In each village, approximately 30 households were randomly selected. These surveys provide a series of baseline characteristics so as to compare our villages and evaluate the randomization process. Table 2 presents summary statistics of variables from these surveys at baseline, as well as a balance test between treatment and control villages. Differences are small and 30 out of the 33 variables have a p-value >0.1, suggesting that our experimental groups are well balanced across a wide range of geographical, socio-economic, and institutional variables. The three variables that show some imbalance are: perceived quality of the village chief at baseline (which proxies for institutional quality; see Voors et al. 2017), average age and proximity to main road (which captures access to markets). We control for these variables by including them as covariates in the regressions below.

<< Insert table 2 about here >>

Estimation strategy

Our main identification strategy is simple and rests on the fact that, by design, treatment assignment is exogenous to land use. To assess how unconditional payments affect clearance of new land for farming, we simply calculate the area of land per village (in hectares) that was classified as vegetation (as either mature forest or farmbush vegetation) in the first period (2011) and transitioned to a classification of 'bare soil' in the second period (2013). This represents the land that has been actively cleared during the period between the two observations, which is the main response variable of interest in our study. We then run an OLS model such that:

(1) $$Y_j = \alpha_k + \beta T_j + \eta C_j + \varepsilon_j$$

where Y_j captures change in land cover class (measured in hectares) between baseline (2011) and end line (2013) for village j, (with j = 1, ..., 79). T_j is a binary treatment variable where T=1 indicates that the village received a transfer payment, and ε_j is the usual village-level error term. The parameter β is our outcome of interest, the amount of land cover change due to the transfer payment. We run a set of three models with different definitions of Y_j, in particular: (i) where we look at changes from any vegetated land (either 'mature forest' or 'farmbush') at baseline to 'bare' soil at end line; (ii) changes from 'farmbush' in baseline to ‘bare’ in end line, and; (iii) changes from ‘mature forest’ in baseline to ‘bare’ in end line. Separating by type of land clearance is important from a policy perspective as we would like to examine whether the transfer program affects the type of vegetation that is targeted for clearing. For example, if payments induce a shift from the clearing of mature forest towards the clearing of farmbush, this could be interpreted a conservation success, even in the absence of any reduction in overall gross amount of cleared land. Alternatively, if payments lead to higher levels of cleared land, then we would like to know if these new farms are being established from land that was previously farmbush, or whether they are being established through the clearance of mature forest and leading to a net reduction in mature forest outside of the GRNP. All base models include chiefdom fixed effects α_k, since randomization occurred within chiefdoms, and variables that control for any unbalanced baseline characteristics between treated and control villages C_j.

As a robustness check we estimate the effect of transfers on the probability of clearing individual pixels. We use a set of about 4500 randomly selected points from across the study area, with a minimum sampling distance of 100m between points to reduce the chance of sampling the same farm more than once, and to avoid spatial autocorrelation (see similar methods used in Gaveau et al. 2009; Pfaff et al. 2009; Nelson & Chomitz 2011). Specifically, we estimate,

$$(2)\quad Y_{ij} = \alpha_k + \beta T_j + \eta C_{ij} + \varepsilon_{ij}$$

where our outcome variable now represents Y_{ij} a dummy for pixel *i* in village *j*, which captures changes in land cover class between baseline (2011) and end line (2013) at a specific point location. The term ε_{ij} are standard errors clustered at the village level, to capture that intra-village pixels are not independent.

The benefit of this pixel level approach is that it allows us to capture topographical and locational variables of each point location, rather than averaged out across all land in the village. Variables included (as suggested by relevant literature e.g. Pfaff et al. 2009; Deininger and Minten 2002; Li et al. 2014; Busch and Ferretti-Gallon 2017) are distance to GRNP boundary, slope (derived from remotely sensed 1 arc second SRTM Digital Terrain Elevation Data, and reflecting potential agricultural productivity of each plot), as well as village-polygon (area) size.[16]

In Annex 1, we also present robustness analysis focusing on dropping a randomization block with most missing data, transformations of the main dependent variable, heterogeneous treatment effects, and analysis not based on remote sensing data but on survey-based measures capturing agricultural activity.

Results

First, we report the results of land cover change detected from satellite imagery, and then we use survey data to probe our main findings and document plausible mechanisms.

Land change impacts: Table 3 and 4 present our main results. In table 3, Columns (1) and (2) assess program impacts on the change (in hectares) of vegetated land to bare land between 2011 and 2013. Column (1) presents the results from the base model (Eq.1). Column (2) adds controls to account for imbalance in baseline variables. The main result is that unconditional transfers increase the conversion of land from vegetation to bare soil, compared with control villages. The coefficient is significant with $p<0.05$ in all model specifications. The base model suggests that on average 19.1 ha more land is cleared in treated villages than in controls, (approx. 3.5% of the mean area size of the typical village in our study).

Assessing the type of vegetation that is being cleared during the study period, we see that new farms are predominantly established from younger vegetation (‘farm bush’), see columns (3) and (4). Columns (5) and (6) assess if clearance of mature forest is higher in treated villages than controls. This is not the case. The treatment coefficient is insignificant and small suggesting that mature forest areas were not targeted in this additional (program induced) land clearance.

<< Insert Tables 3, 4, >>

Table 4 reports on Eq.2 (the pixel level models). The main findings with respect to the treatment effect on land clearance are similar to those reported in table 3. The intervention significantly increases the amount of total vegetation clearance, and again does not have a significant effect on mature forest. We thus see that this additional bare land has been established through the clearance of farmbush. The size of the treatment effect in the base model - column 1 in table 4 - (i.e. about 3.4%) is similar to that of table 3 (i.e. ATE = 19 ha, mean village size within our sample is 542 ha, 19/542 = 3.5%). With controls – column 2 in table 4 - this is increased slightly to 4 percentage points.

The interpretation of the added control variables in the pixel level model is in accord with expectations. There is less land clearance further from roads (signaling that land clearance is correlated with access to output and input (labor) markets) and on steeper land (signaling that have steeper slopes are potentially less suitable for agriculture, or harder to access). For example, several studies documented that plots with shallower slopes and low elevations are favored for agricultural expansion due to their higher agricultural returns (Kinnaird et al. 2003; Nakakaawa, Vedeld, and Aune 2011). Similarly, proximity to roads tends to increase the probability of a plot being deforested as roads increase accessibility to previously remote sites (Chomitz & Gray 1996; Nelson & Hellerstein 1997; Pfaff 1999; Alves 2002; Dalle, Pulido, and De Blois 2011) and distance to both settlements and roads can be used as proxies for market access (Pfaff 1999; Cropper, Puri, and Griffiths 2001), although we note that this is not always the case (Kinnaird et al. 2003).

Additional robustness checks are presented in Annex 1 in Tables A1 and A2, dropping the randomization block with (most of the) missing data from the analysis. We also re-run Equation 1, transforming the main dependent variable in logs and taking proportions, see table A3. All the results from the robustness analysis are highly similar to the results presented in tables 3 and 4.

Survey data: We asked program recipients directly how the vouchers affected their land use and labor input. Table 5 presents summary statistics of the responses. Approximately 29% of the treatment group stated that they converted *more* land as a result of the aid treatment, and a similar 30% of them stated that they *increased* labor inputs to agricultural, logging and other activities as a result of the vouchers. This suggests that vouchers (or the fungibility of this aid modality) relaxed a binding constraint on land clearing.

<<<Insert table 5>>>>>

We also included follow up questions to probe into the reasons behind the responses of table 5. These were asked as open-ended questions that provided a wealth of qualitative information. We examined over 2800 individual responses and classified them into response categories. The results are in Tables A4a-c and A5a-c in the Annex 1. In table A4c (reasons for farming more), we see that approximately 55% of treated households stated that they hired additional labor (either from outside or inside their own household) to work more and expand their farm. Another 8% stated that aid was used to acquire tools in order to clear land. Another 30% used the aid to buy other (non-labor) agricultural inputs (predominately more seeds). This could increase yields in the year when aid was delivered, which could provide the means for hiring labor for clearing more land in the following year.

Table A5 summarizes responses to open ended qualitative questions on the reasons why households changed the amount they worked as a result of the aid. We see that, for respondents that worked more as a result of aid, 82% did so on farm-related activities (only 2.5% used the aid to work more off-farm). In many of these responses households stated that the aid was mostly helpful to expand their farm (through hiring labor) and that is was then easier to increase their own labor for the subsequent farming activities (such as harvesting). This qualitative data is, of course, only suggestive of how the aid could have brought about the observed result. Still, given the clear trend in the responses obtained it appears that the ability to hire labor for clearing land for farming played a significant part.

Future research should evaluate exactly which constraint on land use was lifted by our intervention. Our data are not sufficiently fine-grained to do this (in particular because we lacked baseline data on agricultural activity). We did collect some income variables (such as farm labor, selling produce and savings) during the endline, enabling us to compare behavior of households in the treated and control villages. In many cases, we found no difference between treatment and control villages, possibly due to the coarse or imprecise nature of the estimates (see table A6). We do, however, have access to baseline and endline values for the number of bushels of rice harvested by each household. This variable also suffers from imprecise measurement, but should give a good indication of agricultural production during the 2012 agricultural cycle. Specifically, we estimate:

$$h_{jt2}=\alpha+ \beta_1 T+ \beta_2(T\cdot h_{jt1}) +\beta_3 h_{jt1} +\varepsilon_j \qquad (4)$$

where h_{jt2} is the bushels of rice harvested at endline and h_{jt1} is the corresponding variable at baseline. Table 6 presents the results, showing that the interaction term is positive and significant (at the 5% level). This suggests that treated farmers with larger initial yields increased agricultural outputs (harvested rice).[17] We cannot say if this was due to increased land under production, or improved yields from existing farm land. Only the former would explain our observations of increased land clearance in treated villages. In support of this argument, we note that in larger treatment villages more land was cleared (see table A7).

<<<Insert table 6>>>>>

Our empirical results potentially have important implications for our perception of rural communities at the forest edge in Sierra Leone. Our findings suggest that farmers increased land clearance within two years after the receipt of the unconditional transfer. Under the socio-economic context of the study region, the observed additional land clearance would have been made possible primarily through utilizing additional labor inputs and secondarily by acquiring additional smaller scale farming tools. Accessing or employing larger land clearing machinery would be unlikely in our case, as this is rare in the area.

There could be more than one mechanism through which transfers lead to additional land clearing inputs and explain the observed treatment effect. The finding that a one-off transfer increases farm size suggests farms at baseline were considered “too small” – below conventional equilibrium levels dictated by marginal benefits and costs of effort. In a world with perfect capital markets, a one-off transfer should not affect optimal farm size. Farmers would borrow funds to finance an expansion to the optimal size. In the absence of the transfer, why did farmers not cultivate more extensive areas if this is privately optimal in the sense of equating marginal benefits and costs of effort? The literature provides two possible explanations.

First, smallholders may be extremely resource-constrained and cannot afford to increase their farm size over time – not even marginally (or if they do, they approach the optimal farm size very slowly and are currently “out of equilibrium”). This possibility might be relevant for many of the farmers in our study region, who are rationed out of credit markets and have very imperfect socially-mediated access to capital for productive purposes. In a recent study, Chenoune et al. (2017) explore consumption and production decisions for agricultural households in Sierra Leone and emphasize the importance of rice seed. In general, rice seed is self-produced from the previous year's harvest, and not purchased on the market. Farmers must decide how much rice to consume, and how much to save for planting the following year – not an easy decision in our study area, which is the most food insecure region of the country with a marked 'hungry season'. Farmers are constrained in how much seed they can store for the following agricultural year.

Further, a large determinant of yield in upland rice farming is the density of seed that is sown. Chenoune et al. (2016) find that the predominant factor affecting seeding density, and therefore production in the current year, is the amount of rice that has been stored as seed from the previous year. The timing of the intervention payments in our study - after clearing but at the time of sowing - suggest that farmers who spent their cash transfer on rice seed, or on goods that they would otherwise have needed to use rice to pay for (rice is known to be used as an effective currency in our study area) are likely to have been able to achieve higher sowing densities as a result of the intervention. We hypothesize that this would have increased yields in the 2011 harvest, and therefore acted as a form of value transfer to the following February, where larger rice harvests would increase the resource available for hiring labor. We do not have yield data on the 2011 agricultural cycle, so we cannot test this hypothesis directly. However, Chenoune et al. (2016; 2017) and interview evidence from our study area collected since, suggest that this seems to be a likely mechanism. Also, over 60% of the response to our open-ended questions on the reasons why treated villagers "farmed more" are in line with this plausible mechanism (shown in Tables A4c), while 30% of these responses explicitly stating that they used rice seeds to increase yields.

The second explanation for why a one-off transfer may affect farm size eventuates when production is characterized by locally increasing returns and multiple equilibria. If the transfer enables households to "switch" to an alternative production process that generates higher returns but that requires a minimum project size to be profitable (such as felling of forest by labor teams), then one-off transfers could invite an escape from a poverty trap to an alternative equilibrium with higher income and less forest (Carter and Barrett 2006). This may happen when households cannot finance the cost of the "switch" by (strategic) borrowing – a condition discussed above. Farmers should also be forward-looking for this strategy to work, and they should be sufficiently patient to save the transfer through the lean season (as transfers were paid in May and land clearing did not commence until January the next year). Carter and Barrett (2006) discuss such dynamics, based on temporary "tightening of the belt" in the context of asset-based poverty analysis. However, we realize that saving liquid assets in an environment of poverty and need, characterized by an informal sharing imperative, is far from easy for many households. Future work should establish whether seasonal savings are indeed feasible, enabling rural households to exit from one equilibrium (poverty trap) to another.

Lastly, using data obtained from the Agricultural Household Tracking Survey (AHTS) for Sierra Leone (MAFFS, SSL, and IPA-SL 2012), we apply a plausibility test for the magnitude of the treatment effect that we observe. In the village level models, we find a treatment effect of approximately 19 additional hectares of land cleared in treated compared with untreated villages. The average village in our study has just under 40 households (SD 26) and, at an average labor wage of $1.7 per day (AHTS), this transfer would equate up to approximately nine days of general agricultural labor per household, or up to 360 days of labor for the village. From Johnny, Karimu, and Richards (1981), the clearing of new agricultural land requires approximately 10 days of labor per ha, so for an average treatment effect of 19 ha, this is equivalent to 190 days of labor per village. This is within the value of the aid that was delivered by our treatment (360 days), and would require a at least 50% of the payments to be spent on labor. In Annex 6, we present a more elaborate analysis under various assumptions on the percentage of aid used for labor and over different productivity ranges. The magnitude of the treatment effect was found to be plausible under a range of assumptions with respect to key productivity parameters. These figures are, of course, rough estimates and designed only to give a back-of-the-envelope indication of the plausibility for our results. It is also worth noting that since the payments were given to the villages in kind, rather than as cash, the value of the payments was greater than their nominal amount due to the additional value of sourcing and transporting goods to these remote locations, which can be substantial. This further strengthens the possibility that sufficient labor could be mobilized by the value of aid in this study to explain the treatment effect that we observe.

Discussion and conclusions

Conservation organizations increasingly use unconditional transfers to promote the conservation of natural habitats. Such transfers are uncontroversial and popular among recipients, easy to deliver and scale up (provided sufficient funding is available) and hold the promise of killing multiple birds with one stone; promoting conservation and improving the livelihoods of some of the world's poorest people. Further, these instruments are particularly attractive outside protected areas where the scope of instating conditionality on land use is more limited. However, to our knowledge this is one on the few studies that uses a randomized controlled trial to evaluate such policies. The use of an RCT approach (as opposed to observational data) has the potential advantage of leveraging identification from variation in transfers that is exogenous by design.[18]

By combining our RCT with high-resolution satellite data we find that, in our case, such unconditional payments significantly increase the clearance of land in a slash and burn agricultural system over short time scales. Yet, we do not find evidence that this clearance is targeted towards mature forests. Rather, clearance of vegetation comes from farmbush (non-mature fallowed land) that is already in the agricultural cycle. Our survey data could not illuminate the intermediary variables that caused the observed impact on the final outcome variable, nor could we decisively show the exact mechanisms. The results from Tables 6 and the associated qualitative responses from households suggest that the additional land clearance that we observe in treated villages could have plausibly been achieved through hiring additional labor. This finding is consistent with research from the same study area (see Mokuwa et al. 2011) that shows that agricultural labor demand is much higher for initial land clearing (mostly undertaken by men in the early months of the year) as compared to subsequent farming activities such as weeding, plowing, or harvesting (mostly undertaken by women). The treatment could have (at least in the short run) alleviated this land clearing constraint, with the additional (male) labor likely being hired in from the community (working more than usual) or from external so called ‘labor teams’ found in rural parts of Sierra Leone (see Peeters et al. 2009). However, the channels through which additional labor for land clearing could have been utilized are not readily tractable from the data. Taking into consideration the socio-economic context of our study site, as well as the timing of the intervention within the agricultural production cycle, we have proposed a few plausible explanations. Further empirical testing is warranted to explore how aid would impact labor and land allocation decisions if it were delivered at different times during the agricultural cycle.

Our findings should not be misconstrued as an argument against payments for ecosystem services (PES) schemes. A crucial difference between such schemes and the one we study here is that, in our case, there was no quid pro quo requirement; receiving communities were not required to alter their behavior in return for the payment, and so any impact of the treatment had to come from indirect channels (e.g., income effects, goodwill or reciprocity, the purchase of land-saving technology, “general equilibrium effects” discouraging local deforestation). Provided conditionality is enforced, theory predicts that PES should be more effective in curbing deforestation than unconditional payments by directly paying people to supply ecosystem services (Ferraro 2001, Persson and Alpízar 2012, Pagiola and Platais 2007). Recent empirical evidence from an RCT by Jayachandran et al. (2017) in Uganda on a PES scheme with a higher degree of conditionality, but which similarly to our case entailed a one-off payment (of comparable size) within a 2 – 3 year time span, shows that payments do lead to enhanced levels of avoided deforestation compared to a control group. These payments *directly* compensate treated villagers to deliver a particular ecosystem service. In contrast, our RCT study aimed at assessing whether such unconditional payments could trigger a short term behavioral response via *indirect* channels. Observation of a negative statistically significant impact on the amount of land brought under cultivation would be indicative that these channels can be relied upon if more sustained levels of funding were provided. Our study finds that unconditional payments have the opposite result (significant positive effect on additional amount of land cleared), suggesting that these payments could have an eroding effect outside protected areas as they lead to higher levels of agricultural activity and land clearance (albeit from farmbush, which is not of high conservation value).[19] Although we do also find that mature forest areas were not targeted, we should be cautious in interpreting this as an unambiguous pro-conservation outcome. Villagers could have refrained from targeting mature forests as a gesture of goodwill towards the donor (in accordance with the ‘winning the hearts and mind’ or endorsement effect hypotheses of labeled unconditional transfers, as in Benhassine et al. 2015), though it is questionable whether the potentially more productive mature forest lands would not have been targeted if the aid could have provided access to expensive tree-felling machinery. In the longer term, or with higher value payments, it is possible that mature forests could be targeted (as shown in the study by Alix-Garcia et al. 2013).

Our findings also do not argue against unconditional transfer poverty alleviation schemes more generally. The indirect channels via which poverty alleviation transfers might impact on land use suggest the net conservation effect will vary from one context to the next. For example, the effect will depend on market integration, as this will determine the extent to which extra labor can be hired (affecting the balance between income and substitution effects at the household level), or the extent to which general equilibrium effects may be expected to occur. The availability of (land- or labor-saving) production technologies will also matter, as may the degree to which sustainable forest use is compatible with rural livelihoods. For these reasons we emphasize that the findings from the labor-scarce and land-abundant forest edge in rural Sierra Leone need not spill over to other contexts.

Overcoming the reasons that NGOs opt for unconditional payments may in the future become more feasible. This is perhaps more realistically achievable by overcoming practical obstacles towards conditionality (for example new technologies can make monitoring and enforcement of conditions much more economical). Yet, there are deeper and persistent political and social reasons that make using conditions in conservation payments undesirable or infeasible. The realities of conservation programs suggest that compensation policies will, in many cases and contexts, continue to entail low levels of conditionality, in essence reflecting unconditional schemes. Extensive experience and case study examples from development economics suggest that such unconditional programs are more prevalent than realized (Haushofer and Shapiro 2016, Engel 2015, Honey-Rosés et al. 2009). It is therefore important that the key design elements of such programs are subjected to experimentation, including the degrees of conditionality, the institutions delivering the payments, the framing or labeling used in their provision, their frequency and their magnitude. The study presented in this article shows how RCTs can be used towards addressing such questions. Finally, our experience with undertaking one of the first RCTs applied to a conservation policy context, suggests that large scale field experiments can be more informative if they are conducted as part of mixed methods approach that incorporates both quantitative and qualitative inputs and research approaches.

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Tables and Figures

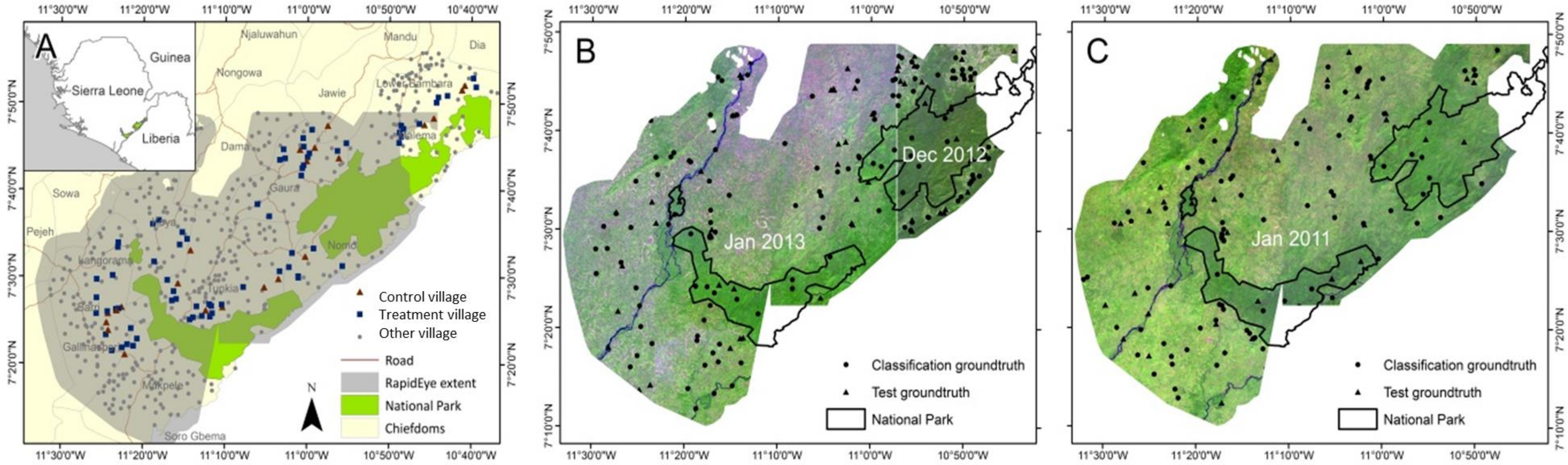

Figure 1. Study area: Gola Rainforest National Park, Sierra Leone

(A) Location of all villages in the seven chiefdoms of the Gola Rainforest National Park, and the extent of Rapideye imagery used for this study. (B) RapidEye imagery dated 13 January 2011 with ground truth pixels used for classification. (C) RapidEye imagery from 21 January 2013 and 06 December 2012 with ground truth pixels used for classification.

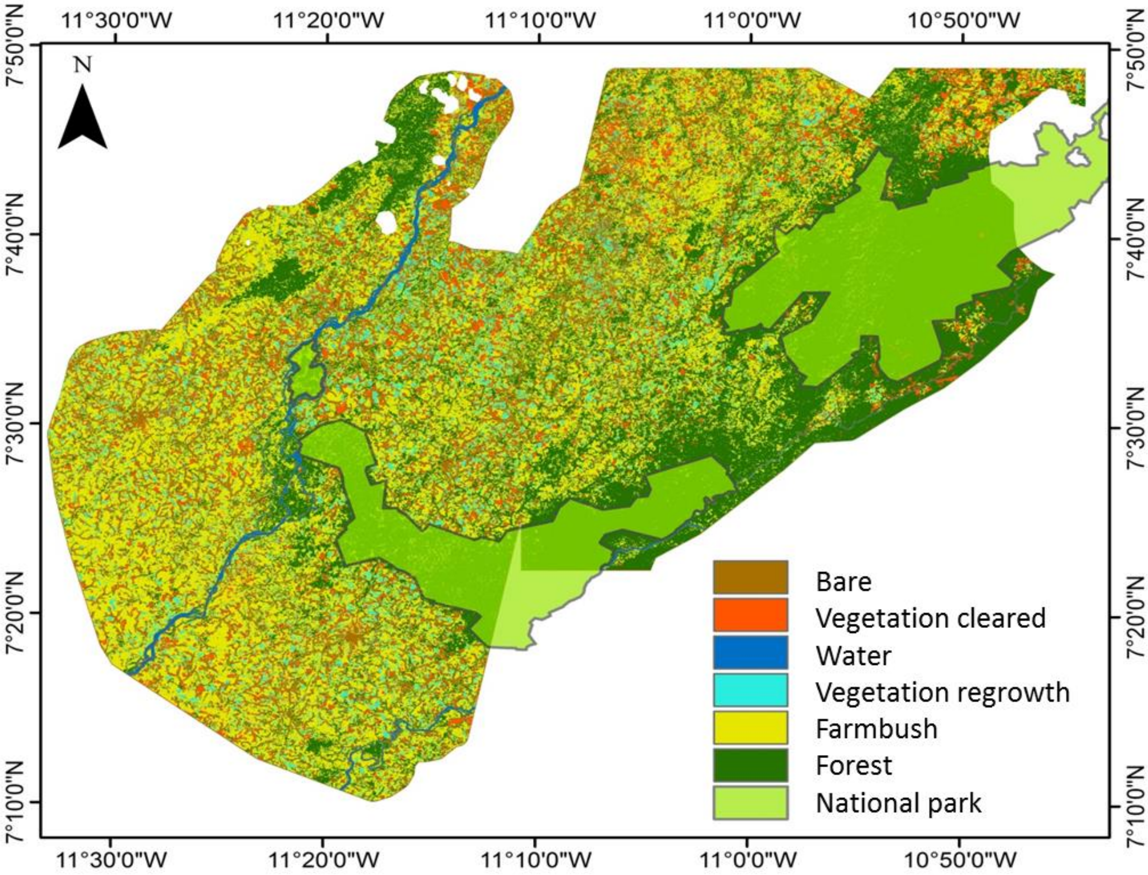

Figure 2. Land cover change January 2011 – Dec 2012/January 2013

Change in land cover derived from post-classification differences between two land cover maps dated 13 January 2011 and 06 December 2012/21 January 2013. Each map was classified into four land cover classes (mature forest, 'farmbush', bare soil, water), resulting in 16 possible combinations between the two time periods. Here we have aggregated those combinations into six classes of land cover change for illustrative purposes: (i) land that was classified as bare soil in both 2011 and 2013 (bare); (ii) land that changed from either farmbush or mature forest in 2011 to bare in 2013 (vegetation cleared); (iii) water; (iv) land that changed from bare in 2011 to farmbush in 2013 (vegetation regrowth); (v) land that was classified as farmbush in both 2011 and 2013 (farmbush); and (vi) land that was mature forest in both 2011 and 2013 (forest).

Table 1. Description of land Cover Classes used in RapidEye Classification

Land cover class	Description
Bare ground	Exposed soil substrate with little vegetation representing recently cleared and burnt land, as well as settlements and roads. Also includes active agricultural land that is managed through weeding and harvesting.
Mature forest	Tree dominated vegetation that has not been clear cut within the past 25 years. Includes historically selectively logged forests, and sacred groves and forested burial grounds that are never used for agriculture.
Farmbush	Vegetated land cover that is not mature forest, typically fallowed land which may include tree-dominated land covers such as young secondary woodland; also herbaceous vegetation.
Water	Rivers, lakes

Table 2. Village Characteristics at Baseline and Balance Test between Experimental Groups

Variable	Baseline Control mean	T-C at baseline	T-C Standard error	p-value	N
Altitude (m above sea level)	133	-0.63	9.77	0.95	79
Total village land area (hectares)	515.01	52.42	90.45	0.56	79
Chief quality index. Village average to household question "Is your chief a good chief? (=1 if yes)	0.87	0.05	0.03	0.09	79
Average age of household heads (years)	40.62	-1.53	0.86	0.07	79
% male	0.62	-0.03	0.04	0.47	79
% of households with tin roof	0.34	0.01	0.06	0.81	79
Average amount of rice harvested (in bushels)	5.6	-0.39	0.66	0.55	79
Distance to Gola Rainforest National Park (km)	5.65	-0.44	0.55	0.42	79
Distance to Liberia (km)	18.95	1.71	1.16	0.14	79
Distance to nearest major road (km)	4.11	-1.11	0.65	0.09	79
Population in 2010	117.64	-27.86	17.98	0.12	79
Average slope (degrees)	6.04	-0.01	0.5	0.98	79
Number of families that can stand for chief	2.5	-0.02	0.44	0.96	79
Vegetation cover in 2011 (hectares)	421.78	34.48	79.98	0.67	79
Mature forest cover in 2011 (hectares)	120.73	-29.1	44.03	0.51	79
Education level of chief (years of formal education)	6.59	-0.16	1.15	0.89	79
Size of land farmed by chief (hectares)	40.14	-15.86	28.62	0.58	79
Number of wives chief has	2.00	-0.13	0.3	0.67	79
Chief years in power	18.94	-2.4	3.79	0.53	79
Number of village forest management bylaws	3.71	-0.61	0.43	0.15	79
Bylaw for logging (=1 if yes)	0.71	0.03	0.13	0.83	79
Bylaw for hunting (=1 if yes)	0.47	-0.06	0.12	0.66	79
Bylaw for mining (=1 if yes)	0.82	-0.13	0.12	0.27	79

Villagers log commercially outside GRNP (=1 if yes)	0.24	0.07	0.11	0.50	79
Villagers hunt outside GRNP (=1 if yes)	0.59	0.19	0.12	0.12	79
Villagers mine in forest outside GRNP? (=1 if yes)	0.06	0.07	0.07	0.32	79
Village experienced drought within five years from baseline (=1 if yes)	0.65	-0.02	0.12	0.86	79
Village experienced crop disease within five years from baseline (=1 if yes)	0.94	0.06	0.06	0.31	79

Note: Number of villages in control n = 17, treatment = 62, column (5) presents robust p-values from OLS regression controlling for randomization blocks (chiefdom). Missing values imputed at treatment arm mean.

Table 3. Village Level Analysis of Program Impacts on Land Use Change

	(1)	(2)	(3)	(4)	(5)	(6)
	Any vegetation to bare	Any vegetation to bare	Farmbush to bare	Farmbush to bare	Mature forest to bare	Mature forest to bare
Treatment	19.111**	20.599**	18.886**	19.391**	0.225	1.208
	(8.267)	(8.022)	(7.726)	(7.444)	(1.329)	(1.231)
Chief is good (std)		-4.448		-3.691		-0.756
		(3.425)		(3.137)		(0.729)
Average age (std)		6.728		6.711*		0.017
		(4.180)		(3.844)		(0.757)
Distance to road (std)		-8.504		-9.830*		1.326
		(6.039)		(5.730)		(0.950)
Constant	42.697***	42.258***	39.673***	40.111***	3.024***	2.147*
	(10.097)	(10.199)	(9.664)	(9.750)	(1.073)	(1.101)
Observations	79	79	79	79	79	79

Note: OLS regressions including randomization blocks (chiefdom). The dependent variables are the hectares that transition from any type of vegetated land in 2011 to 'bare' soil in 2013; 'farmbush' land to 'bare' soil and 'mature forest' land to 'bare' soil. Missing values for controls are imputed at treatment arm mean and standardized. Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 4. Pixel Level Analysis of Program Impacts on Land Use Change

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Any vegetation to bare	Any vegetation to bare	Any vegetation to bare	Farmbush to bare	Farmbush to bare	Farmbush to bare	Mature forest to bare	Mature forest to bare	Mature forest to bare
Treatment	0.034	0.041**	0.039**	0.037*	0.043**	0.042***	-0.003	-0.002	-0.002
	(0.021)	(0.018)	(0.017)	(0.019)	(0.016)	(0.015)	(0.006)	(0.005)	(0.005)
Total village land area (std)		-0.023**	-0.019***		-0.022**	-0.019***		-0.001	-0.000
		(0.010)	(0.006)		(0.010)	(0.006)		(0.002)	(0.002)
Chief is good (std)			0.002			-0.003			0.006**
			(0.006)			(0.006)			(0.002)
Average age (std)			0.020**			0.020**			0.001
			(0.009)			(0.008)			(0.003)
Distance to road (std)			-0.023***			-0.026***			0.003
			(0.006)			(0.005)			(0.002)
Distance to GRNP (std)			0.004			0.005			-0.001
			(0.008)			(0.007)			(0.002)
Slope			-0.011**			-0.010**			-0.002
			(0.004)			(0.004)			(0.002)
Constant	0.083***	0.083***	0.071***	0.076***	0.077***	0.069***	0.007	0.007	0.003
	(0.022)	(0.019)	(0.012)	(0.021)	(0.018)	(0.012)	(0.005)	(0.005)	(0.006)
Observations	3375	3375	3375	3343	3343	3343	3064	3064	3064
# Clusters	79	79	79	79	79	79	79	79	79

Note: Regressions include chiefdom level fixed effects. Dependent variable are pixels that transition from any type of vegetated land in 2011 to 'bare' soil in 2013; 'farmbush' land to 'bare' soil and 'mature forest' land to 'bare' soil. Missing values for controls are imputed at treatment arm mean and standardized. Standard errors in parentheses clustered at village level. * p < 0.10, ** p < 0.05, *** p < 0.01

Table 5. Stated Land and Labor Behavioral Changes in Treatment Group

Changes in land farmed/converted as a result of aid	**Freq.**	**Percent**
Farm/convert less land	130	10.07
Farm/convert same land	792	61.35
Farm/convert more land	369	28.58
Total	1291	100

Changes to labor inputs in farming/ logging and other activities as a result of aid	**Freq.**	**Percent**
Work less	150	11.75
Work equal	739	57.87
Work more	388	30.38
Total	1277	100

Table 6. Stated Harvest Amount at EL (Household Level)

	(1)	(2)
	Bushels harvested at EL	**Bushels harvested at EL**
Treatment	-1.178	-1.132
	(0.949)	(0.865)
*T*Bushels harvested at BL*	0.236**	0.229**
	(0.117)	(0.110)
Bushes harvested at BL	0.021	0.022
	(0.094)	(0.090)
Good Chief (std)		-0.093
		(0.313)
Average age (std)		0.263
		(0.309)
Distance to road (std)		-0.072
		(0.256)
Constant	8.485***	8.497***
	(0.878)	(0.868)
Observations	1942	1942

Note: Regressions include chiefdom level fixed effects. Missing values for controls are imputed at treatment arm mean and standardized. Standard errors are clustered by village. The dependent variables is bushels of rice harvested at end line. Standard errors in parentheses. * p < 0.10, ** p < 0.05, *** p < 0.01

Table 6. Stated harvest amount at EL

	(1)	(2)
	Bushels harvested at EL	Bushels harvested at EL
Treatment	-1.178	-1.132
	(0.949)	(0.865)
T*Bushels harvested at BL	0.236**	0.229**
	(0.117)	(0.110)
Bushes harvested at BL	0.021	0.022
	(0.094)	(0.090)
Good Chief (std)		-0.093
		(0.313)
Average age (std)		0.263
		(0.309)
Distance to road (std)		-0.072
		(0.256)
Constant	8.485***	8.497***
	(0.878)	(0.868)
Observations	1942	1942
Adjusted R^2	0.023	0.022

Note: Regressions include chiefdom level fixed effects. Missing values for controls are imputed at treatment arm mean and standardized. Standard errors are clustered by village. The dependent variables is bushels of rice harvested at end line. Standard errors in parentheses. * p < 0.10, ** p < 0.05, *** p < 0.01

Endnotes

[1] Field experiments have been used in the context of environmental issues and land use. For example, Jack (2013) analyses how auctions can help reveal private information about the performance of landowners under different incentive schemes. See also Greenstone and Jack (2015) and Curzon and Kontoleon (2016) about the potential of experimental methods for improving environmental and resource economics analyses in the context of developing countries.

[2] The empirical support for the effects of PES schemes on deforestation is summarized in Samii et al. (2014) and Börner et al. (2016, 2017). They make a strong case for using RCTs to evaluate impacts. Jayachandran et al. aims to assess the efficacy of a PES program in Uganda that pays households to conserve private mature forest lands. The authors are assessing a scheme that has higher levels of conditionality to ours. There are several other key contextual and design differences between the two studies that makes a direct comparison of the relative efficacy of conservation payment schemes with different degrees of conditionality difficulty. Yet, the Jayachandran et al. (2017) study has some useful similarities to ours, for example they also focus on assessing short term impacts on land cover and provide payments that are comparable in magnitude to ours. This allows for a useful comparison of our corresponding results. There are two more relevant projects underway both taking place in Bolivia that also use an RCT framework to assess the impacts of conservation payments. The first looks at payments for reducing deforestation in order to deliver downstream watershed services. The second compares the relative performance of direct payments vs. offering capacity building for improving grazing practices. Results from these projects are not currently published (but are referred to in Grillos 2017).

[3] For example, they allow for better targeting of aid, they could be less distortionary on local prices than cash, they could avoid crowding out of certain desirable behaviors, and they offer protection of recipients against rampant price inflation (Hidrobo et al. 2014; Aker 2017).

[4] A different set of concerns have been voiced for conditional PES schemes. For example, PES schemes may crowd out pro conservation behavior or lead to leakage effects where conversion is intensified outsides areas covered by the PES (Engel, 2015).

[5] Attaching conditions on conservation payments is a hard sell in many conservation settings, and particularly when working outside protected areas. For our specific case the NGO was concerned that the local communities may perceive the introduction of conditions as a covert form of 'land grabbing'. Another challenge had to do with the property right structure in the region which was not particularly suitable for introducing a *quid pro quo* compensation scheme. Lastly, the cost of enforcing conditionality was rendered to be well beyond the budgetary resources and capabilities of the Gola authorities. Some of these obstacles are more easily resolved than others. The land grabbing fears (that go hand-in-hand with fears over loss of food security) are still a serious concern and can only be overcome with the deepening of institutions in Sierra Leone. Practical considerations could be overcome with more local community participation in monitoring as well as with the introduction of the new enforcement technologies which can significantly reduce the operating costs of introducing conditionality. The experiment is part of a larger research program to understand how institutions and (unconditional) distribution modalities affect conservation behavior and livelihoods. These treatment arms are beyond the scope of this paper and have all been collapsed.

[6] Before receiving their vouchers, communities were told that*: "We are here representing the Gola Forest Programme and the Government of Sierra Leone. We want to help your village by providing you with livelihood aid to show our mutual support for conserving the Gola Forest."* The full protocol is available from the authors.

[7] The pool of eligible villages was determined by the NGO. Our research team then randomly selected treated and control groups within this set of villages.

[8] This decision was taken by the NGO as they felt it reflected the realities of any future stream of payments which, due to the specific socio-institutional setting, would have to be necessarily flexible and 'light touch' in terms of detailed conditions.

[9] Beyond the economics reason in favor of vouchers noted by the literature, in our case the use of vouchers rather than cash arose due to specific regulations set by the collaborating policy organizations. Due to past experience, the GRNP moved away from making cash transfers to in-kind transfers. One concern had to do with the safety of field staff in transporting large sums of cash in remote areas. It is likely that whether cash versus in-kind payments are used, or whether another dimension of the compensation mechanism is altered (for example the magnitude, frequency or duration payments), could impact behavior (Aker, 2017). The impact of such design elements for conservation payments constitutes a significant area of ongoing research (Engel 2015).

[10] GDP per capita in Sierra Leone in 2011 stood at an average of $374 (WDI, 2015). This is likely much lower in rural areas (for example poverty headcount in Kenema district was 62% in 2011). The grant is valued at central market prices in Kenema. This implies that the total value of the project in each village is substantially higher as the NGO incurred the transportation costs, which constitute a significant amount in these remote areas.

[11] Clearing and burning of land for agriculture occurs in this area between January and March. Our observations cover two agricultural cycles: clearance between January and March 2011 prior to payments being made, and one subsequent clearance cycle between Jan and March 2012, after payments were made.

[12] Classification was undertaken using a supervised pixel-based approach and maximum likelihood classifier, including optical bands, texture metrics calculated at two window sizes (5 x 5 and 21 x 21) and vegetation indices). Ground truth data gathered from field observation and google earth imagery. The classification resulted in an overall classification accuracy of 97.7% for 2011, 99.9% for 2012, and 95.0% for 2013. Post classification analysis was conducted in ArcGIS 10.3. Full details of the classification procedure are in Annex 5.

[13] Most clearance within the park boundary occurred in the north of the study site: Gola South = 0.02% pixels changed from forest to bare, 0.03% farmbush to bare; Gola Central = 0.09% pixels changed from forest to bare, 0.09% farmbush to bare; Gola North = 0.18% pixels changed from forest to bare, 0.52% farmbush to bare.

[14] We could not explore the impact of payments on individual (as opposed to village level) land use behavior because attributing pixels to individual households was not possible. This does not in any way diminish the usefulness of our analysis as our main aim is to ultimately evaluate impacts at the village level.

[15] It is common in studies such as these to use unweighted Voronoi polygons or circular buffers to assign land clearance to individual villages. By using both spatial information and survey data from villages in multiplicatively weighted Voronoi polygons we were able to increase the correlation coefficient between surveyed village area and mapped areas from 0.18 to 0.68 compared with unweighted Voronoi polygons. Further information on the exact validation procedure, and as well the method used to generate the polygons is detailed in (Wilebore and Coomes, 2016).

[16] We also explored the impact of elevation in addition to slope but the as the two variables were highly collinear we did not include elevation in the final analysis. Replacing slope with elevation produces the same type of results.

[17] Using survey data, we explore whether specific variables evaluated at their baseline levels were associated with heterogeneous treatment effects within the treatment group. We chose variables for which (a) we have baseline values and that (b) have some theoretical or policy relevant reason as to why treatment may have heterogeneous effects. In particular, we consider whether the effects of the intervention varied across the following dimensions: distance to roads and markets, the quality of local governance, amount of (mature forest) land and population. In essence we run models as specified in Eq1 but now with the inclusion of these variables interacted with the treatment dummy variable. Results are in tables A7 in Annex 1. None of these interaction terms entered significantly at conventional significance levels except and as expected in village level models total land area (p=0.06) and population size (p=0.11). Do note that our analysis is likely underpowered.

[18] We acknowledged that all such RCT studies when applied to real social policies potentially suffer from the added noise that they cannot be truly blind (so participants may know which experimental group they are assigned to) while often participants (in both treated and control groups) may alter their behaviour when they know they are part of an experimental study and their actions monitored (e.g. leading to various behavioural distortions such as the "Hawthorne" and "John Henry" effects; see Chassang, Padro i Miquel, and Snowberg 2012; Duflo, Glennerster, and Kremer 2007). In our current study, participants did not know that their endline land clearance behaviour would be observed (they could not have known of the existence of our remote sensing data) nor were they told that we would revisit their village for a follow up (end-line) survey to ask them questions related to land use. Hence, such "observer effects" should have been muted. Of course, although we did not inform either of the two experimental groups (treated and control) of the existence of the other group, we cannot rule out that this information was found out in some villages (especially for villages that are located relatively nearby to each other).

[19] Though comparisons with the study by Jayachandran et al. (2017) are informative, the two studies differ in several key parameters to make for a direct credible comparison between conditional and unconditional conservation payment schemes. Such comparisons are best served by undertaking experimental studies with multiple treatment arms within the same study region and policy context.