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Highlights

- A local REDD+ initiative in the Brazilian Amazon promoted win-win outcomes.
- Direct payments contributed to deforestation reduction and well-being improvement.
- Deforestation resumed after payments ended without retrieving avoided deforestation.
- Conservation gains induced from temporary payments were left intact.

Abstract

Rigorous impact evaluations of local REDD+ (reduced emissions from deforestation and forest degradation) initiatives have shown some positive outcomes for forests, while wellbeing impacts have been mixed. However, will REDD+ outcomes persist over time after interventions have ended? Using quasi-experimental methods, we investigated the effects of one REDD+ initiative in the Brazilian Amazon on deforestation and people's well-being, including intra-community spillover effects (leakage). We then evaluated to what extent outcomes persisted after the initiative ended (permanence). This initiative combined Payments for Environmental Services (PES) with sustainable livelihood alternatives to reduce smallholder deforestation. Data came from face-to-face surveys with 113 households (treatment: 52; non-participant from treatment communities: 35; control: 46) in a three-datapoint panel design (2010, 2014 and 2019). Results indicate the REDD+ initiative conserved an average of 7.8% to 10.3% of forest cover per household. It also increased the probability of improving enrollees' wellbeing by 27-44%. We found no evidence for significant intra-community leakage. After the initiative ended, forest loss rebounded and perceived wellbeing declined – yet, importantly, past saved forest was not cleared. Our results therefore confirm what the theory and stylized evidence envisioned for temporal payments on activity-reducing ('set-aside'): forest loss was successfully delayed, but not permanently eradicated.

Keywords: conservation incentives, emission reductions, additionality, climate change mitigation, impact assessment.

1. Introduction

REDD+, short for "Reducing Emissions from Deforestation and Forest Degradation and conservation, sustainable management and enhancement of carbon stocks", is a nature-based solution devised to mitigate greenhouse gas emissions while achieving co-benefits, mainly the improvement of human well-being and biodiversity conservation (Angelsen and McNeill, 2013). Launched 15 years ago, REDD+ initially generated widespread excitement as a fresh approach to forest conservation through promoting performance-based incentives at scale. However, global carbon markets, initially envisioned to finance REDD+, have expanded insufficiently, implementation proved more complex than expected, and so far tropical deforestation has thus continued (Angelsen, 2017; Angelsen et al., 2018). Notably, while REDD+ initially was conceived as a jurisdictional mechanism in the Paris Agreement, by far most of the action has remained in smaller-scale pilot projects (Wunder et al., 2020b).

To convincingly make conclusions about REDD+ performance, we need to focus our attention on rigorous impact evaluations vis-à-vis both REDD+ conservation and development outcomes. These evaluations allow attribution of observed outcomes to an intervention through construction of a credible counterfactual scenario (i.e., what would have happened in the absence of the REDD+ initiatives) (Sills et al., 2017; White, 2009). Despite the importance of REDD+ over the last decade, studies using a counterfactual approach have been scarce (Duchelle et al., 2018).

Of the few REDD+ studies using counterfactual approaches to assess deforestation reduction, most indicate moderately positive results (Simonet et al., 2018). For instance, a quasi-experimental assessment of 23 local REDD+ initiatives, based on different intervention mixes (e.g., restrictions on forest clearing and access, Integrated Conservation and Development Project (ICDP)-type activities, and Payments for Environmental Services (PES)), in Brazil, Peru, Tanzania, Cameron, Indonesia and Vietnam, showed that half reduced deforestation, although with moderate effect sizes (Bos et al., 2017). This study was undertaken as part of CIFOR's Global Comparative

Study on REDD+ (GCS)[1]. Likewise, a Randomized Control Trial (RCT) found a significant reduction in net tree losses in villages participating in an Ugandan PES-based carbon forestry initiative (tree cover declined 4.2% in treatment but 9.1% in control villages), which translated into an average of 5.5 ha of forestland saved per village (Jayachandran et al., 2017). Even more substantial were the outcomes of a mixed ICDP-PES initiative in the Brazilian Transamazon where participating households halved their yearly deforestation (Avg=4 ha/household) (Simonet et al., 2019). Finally, a unique quasi-experiment of Guyana's national REDD+ program based on synthetic controls found for 2010-15 a 35% reduction in tree-cover loss (5,800 ha/year) vis-à-vis a no-REDD+ baseline, with tree-cover loss increasing after the program ended (Roopsind et al., 2019).

Other studies typically found small (significant or insignificant) effects of REDD+ initiatives on deforestation. For example, a quasi-experimental assessment of REDD+ initiatives in Mexico's Yucatan Peninsula found no overall reduction in forest cover loss using difference-in-differences (DID) regression and propensity score matching (alternatively, using synthetic controls, effects were mixed) (Ellis et al., 2020). For Peru, a recent review found over periods of 4-6 years insignificant forest conservation effects from a public and a private REDD+ pilot program, respectively – both implemented in the Peruvian Amazon (Montoya-Zumaeta et al., 2021). Returning to Brazil, one quasi-experiment found insignificant REDD+ deforestation impacts in Mato Grosso State's Alta Floresta municipality. This is a REDD+ like initiative under the Amazon Fund, mixing environmental land registrations with PES-ICDP incentives: land registration increased attributably in project sites, yet deforestation effects were insignificant, presumably because of already very low rates of deforestation in the pre-intervention period (Correa et al., 2020). Quite similar is the situation for the large-scale Bolsa Floresta program in Amazonas State, holding also Brazil's oldest REDD+ program in the Juma Reserve. Bolsa Floresta combines PES with collective benefits (health, education, community organization) and ICDP investments in alternative livelihoods (Börner et al., 2013). An evaluation using matching techniques found insignificant forest conservation effects, given that most enrolled areas proved to be remote and little threatened in the

[1] As part of GCS REDD+, CIFOR and partners have assessed the outcomes of 23 local REDD+ initiatives in Brazil, Peru, Indonesia, Vietnam, Cameroon, and Tanzania. See more in: https://www.cifor.org/gcs/

first place (Cisneros, 2019). Finally, scrutinizing twelve REDD+ sites, all Amazon Fund projects spread over the Brazilian Amazon, a quasi-experiment with synthetic controls showed that only in four sites was deforestation reduced, whereas nil or negative impacts were observed in the remaining eight (West et al., 2020).

Regarding well-being and livelihoods impacts, evaluations of REDD+ initiatives most frequently indicate small, mixed (positive or negative) effects. For instance, a quasi-experiment based on publicly-available social and spatial data at 18 REDD+ sites in Indonesia suggested the strengthening of local land rights, but potentially negative effects on other welfare indicators (Jagger and Rana, 2017). The aforementioned carbon-focused project in Uganda found a significant rise in the non-food, but no impact in the food consumption of target-households monetary income (Jayachandran et al., 2017). In turn, Solis et al. (2021) found for two REDD+ projects in the Peruvian Amazon (Madre de Dios and Ucayali) insignificant income effects, using matched DID estimations. Likewise, a comparison evaluating 22 local REDD+ initiatives in CIFOR's GCS on REDD+ did not observe any significant impact whatsoever (neither positive, nor negative) on household and village-level perceptions of wellbeing and income sufficiency (Sunderlin et al., 2017). Finally, another quasi-experiment targeting 17 of these 22 GCS REDD+ initiatives showed that impacts on subjective wellbeing of REDD+ participants depended on the composition of REDD+ interventions: a predominance of disincentives (e.g., law enforcement) negatively affected households' perceived well-being, but this negative effect was alleviated when incentives (e.g., PES, ICDP) were added (Duchelle et al., 2017).

Hence, the accumulated evidence suggests local REDD+ initiatives are struggling to achieve strong win-win outcomes in terms of simultaneously delivering sizeable deforestation reductions and well-being improvements. So far, REDD+ is on average achieving moderate conservation effects, typically in a welfare-neutral manner. However, still few counterfactual evaluations exist and practically only REDD+ projects, rather than jurisdictional programs, have been evaluated (Duchelle et al., 2018). More assessments will be needed to increase our confidence about if, when, and how REDD+ interventions are working.

Even less studied is whether REDD+ achievements persist after initiatives are suspended. Since most of the above reviewed initiatives were evaluated only at early stages (e.g. 2-3 years after start for Bos et al. (2017), Jayachandran et al. (2017), and Simonet et al. (2019)), there is an understandable knowledge gap about their longer-term effects.

Theoretically, we should expect that if forest conversion paid off better than forest conservation *ex ante*, this will likely persist post-payment, so that deforestation should pick up again after payments stopped. Yet, under some scenarios substantial post-payment permanence could still happen. First, REDD+ initiatives might explicitly manage to achieve the lasting adoption of more benign land uses with sustained economic returns (e.g. establishing agroforestry systems). Second, REDD+ benefit transfers could have had a motivational 'crowding-in' effect, boosting landowners' altruistic motives for forest conservation – not the most common, but still a possible scenario (Ezzine-de-Blas et al., 2019). Finally, REDD+ might just have successfully bought time for the external environment to have substantially adapted in its own right (e.g. falling commodity prices, alternative employment options, or political changes), thus exogenously reducing the opportunity costs of conservation.

However, these exceptions apart, as default scenario we should rather expect the original environmental externality problem to persist: once REDD+ initiatives have ended, the incremental conversion to alternative land uses (e.g., extensive cattle ranching, swidden agriculture) will continue. Forests that were temporarily spared by the initiatives would thus likely be converted after REDD+ ends (non-permanence) (Dutschke and Angelsen, 2008). If so, REDD+ initiatives would postpone deforestation, instead of permanently reducing it (Angelsen and Wertz-Kanounnikoff, 2008). On the livelihoods side, when REDD+ on-the-ground incentive flows (e.g., PES) end, welfare impacts may also fade, unless longer-lasting welfare-enhancing assets had been constructed from those accumulated REDD+ transferred benefits.

Indeed, the two published evaluations of REDD+ outcomes' permanence that we are aware of found support for our default scenario. The first is the aforementioned quasi-experiment showing that Guyana's national REDD+ program, funded by Norway through performance-based payments for reducing deforestation, decreased tree cover loss during the implementation period (2010-2015) (Roopsind et al., 2019). Yet, right after Norway's payments ceased, in 2016, the authors detected an increase in tree cover loss, which suggested forest protection was not guaranteed without continued payments. The second is a follow-up evaluation of the forest outcomes from the carbon-focused project in Uganda. Implemented between 2011 to 2013, the program had reduced deforestation substantially (Jayachandran et al. 2017). However, using satellite imagery from 2016, World Bank (2018) showed the previous PES recipients resumed forest clearing at similar rates to control group households after payments had ended; yet, importantly, without 'catching up' with the initial curve, i.e. leaving the temporary, payment-induced conservation gains intact.

The results of these two studies, therefore, would suggest REDD+ initiatives are struggling to induce self-sustained deforestation reductions. However, we cannot make sweeping generalizations from only two cases. Fortunately, drawing also on the broader family of forest carbon-focused PES interventions may help us to get a clearer picture (Wunder et al., 2020a), and they present some more optimistic evidence. One example is a natural experiment in PES permanence that occurred in Cuyabeno, Ecuador, within Socio Bosque – the national PES-like program for forest conservation. The program had attributably reduced annual deforestation on enrolled plots by annually 0.4–0.5% points during 2011-14 (Jones et al., 2017). However, when public funds run dry during 2015–17, Socio Bosque payments were suspended for some recipients. Even without pay, they continued to clear less forest than similar non-participating landowners, unless though they held plots close to roads or oil wells: in these plots with higher deforestation threat and presumably larger conservation opportunity costs, forest loss reverted back to rates similar to what applied to unenrolled properties (Etchart et al., 2020).

In the case of using PES as an adoption subsidy for environmentally more benign practices that pay off for land stewards ('asset-building' PES), the prospects for permanence should be better. At least a few case studies seem to also support this empirically, in particular from the World Bank-supported trinational Regional Integrated Silvo-pastoral Ecosystem Management Project (RISEMP). This carbon- and biodiversity-focused PES program paid landowners between 2003 and 2008 for the introduction of silvopastoral practices. In Quindío (Colombia), four years after payments ended, these systems had been widely retained (Pagiola et al., 2016). Similar permanence results were reconfirmed again in 2016 (Calle, 2020). The sister silvopastoral program implemented simultaneously in Nicaragua was assessed also in 2012, showing also widespread permanence, although the evidence here is based only on a before-after comparison of various interventions, without a no-intervention control group (Pagiola et al., 2020). The third rollout was in Costa Rica (2002-07), and also here there is about a decade after (2016) evidence of a high degree of permanence (Rasch et al., 2021). The corollary of their findings is that inducing the lasting adoption of more benign land uses is feasible. In the REDD+ context, this could, in turn, self-sustain the deforestation reduction reached by the initiatives.

On aggregate, permanence is clearly important for the effectiveness of any conservation intervention. It is true that conservation-focused payment programs will typically strive to make conditional contracts and payment flows renewable, but we cannot guarantee funding streams will last forever. Yet, our knowledge about the degree of, and factors influencing permanence remains quite limited – not only for REDD+, but also for other types of conservation payments. Even in the developed Global North, most empirical assessments of post-intervention permanence or 'persistence' are derived from stated landowner intentions, not de facto behavior (Swann and Richards, 2016). It seems likely that permanence is higher for incentives that are linked to asset-building, rather than activity-restricting conservation action (Dayer et al., 2018), but surely the socio-institutional context will also matter at different scales (Rasch et al., 2021).

In this article, we investigate the effects of a local REDD+ initiative in the Brazilian Amazon on deforestation and people's well-being, including intra-community spillover effects (leakage), scrutinizing in particular to what extent outcomes persisted after the initiative and associated 'treatments' ended, thus helping to fill a knowledge gap on REDD+ permanence. The remainder of the paper is organized as follows. In Section 2, we present the study site plus all applied REDD+ interventions. Section 3 accounts for the methods for our sampling and panel data collection. In Section 4, we present our quasi-experimental approach to estimate both short and long-term impacts of the REDD+ initiative. We then show the longitudinal effects of the REDD+ initiative on both deforestation and people's well-being (Section 5). In Section 6, we explore possible explanations for our estimation results, and in the last section, we present conclusions.

2. The Transamazon REDD+ initiative

As part of CIFOR's GCS REDD+, we scrutinized the *Projeto Assentamentos Sustentáveis na Amazônia* (PAS), a REDD+ initiative implemented by a Brazilian non-governmental organization (*Instituto de Pesquisa Ambiental da Amazôni*a – IPAM). PAS started in 2012 but it was suspended in 2017, after IPAM had its refinancing request denied by the Amazon Fund.

Approximately 2,700 households from the western part of the Pará state (Brazil) participated in PAS (IPAM, 2016). However, our study only focused on the 350 households for which IPAM offered PES (see all interventions' description below). They lived in twelve communities located in the municipalities of Anapu, Pacajá and Senador José Porfírio, near the Transamazon highway. This is a high-deforestation area dominated by smallholders (properties up to 100 ha), mostly colonists from the northeast part of the country (Godar et al., 2012; Stella et al., 2020). The 350 households had participated in a previous PES-ICDP mixed federal program (*Proambiente*) from 2003 and 2006, where actual payments were delivered only for six months though (Bartels et al., 2010; Simonet et al., 2019).

Households' main economic activities were cattle ranching and swidden agriculture. Part of the production was sold (e.g., rice, cassava, cocoa), despite poor transportation infrastructure. Secondarily, households depended on forest resources, collected mainly for auto-consumption, such as firewood for cooking, fruits, fish and bushmeat, in addition to monetary income from other sources, especially from government transfers, such as retirement pensions and the *Bolsa Família* conditional cash transfer program (Cromberg et al., 2014b).

The central goal of PAS was to reduce deforestation rates, mainly by increasing profitability in pasture and agricultural plots. To do that, IPAM relied on a ICDP-PES mixed approach, with project activities divided into the following four main components (Simonet et al., 2019).

a. Awareness-raising meetings on environmental legislation and tenure regularization

Between 2013 and 2017, IPAM conducted farmer meetings, explaining the processes involved in land tenure regularization (many local properties were not titled) and Brazilian environmental legislation, especially the Forest Code (Law 12.651/2012), which requires for rural properties in the Amazon to retain a fixed proportion of 50-80% of land covered with native vegetation (Legal Reserve). Failing to comply, landowners could be fined by environmental agencies, such as the federal Brazilian Institute for the Environment and Renewable Natural Resources (*Instituto Brasileiro do Meio Ambiente e dos Recursos Renováveis* – IBAMA).

b. Properties environmental registration

The Forest Code also requires rural landowners to register their properties in the Rural Environmental Registry (*Cadastro Ambiental Rural* – CAR), a public document with information on property boundaries and the location of its areas set aside for protection (Legal Reserve and Permanently Protected Area). During 2012-14, IPAM provided administrative support for CAR registration, as a main instrument for forest monitoring in Brazil. It is legally required when rural landowners want to sell cattle or to access rural credit.

c. Payments for Environmental Services

IPAM offered direct cash payments for households whose properties had at least 30% of forest cover (IPAM, 2016). Contracts were signed in 2013, and quarterly payments were provided from 2014 to 2017. Households could receive up to 1,600 BRL/year (725 USD/year, as of 07/01/2014), depending on the level of compliance with the following conditionalities (Cromberg et al., 2014a): (i) 30% of the payment was contingent upon the conservation of at least 50% of the property as forest (Legal Reserve); (ii) another 30% was contingent upon the conservation of 15 meters-wide forest riparian zones (the Permanently Protected Areas); (iii) the 40% left was based on the adoption of fire-free land management (Simonet et al., 2019).

d. Sustainable livelihood alternatives (ICDP component)

IPAM promoted sustainable livelihood alternatives in the project area, such as cattle ranching intensification, agroforestry (e.g., cacao – *Theobroma cacao*, *açaí* – *Euterpe oleracea*, babassu – *Attalea speciosa*), vegetables (e.g., lettuce – *Lactuca sativa*, cabbage – *Brassica oleracea var. capitata*) and black pepper (*Piper nigrum*). Between 2013 and 2016, IPAM developed customized property management plans together with household heads to decide which economic activities to develop. From 2014 to 2017, IPAM offered technical assistance through regular visits, and free inputs (e.g., wire for fences, fertilizers). According to IPAM, each household could choose a list of inputs valued at up to 5,000 Brazilian reais (BRL) (2,267 USD, as of 07/01/2014) for the planned activities.

Figure 1 synthetizes the timeline of PAS activities, including data collection.

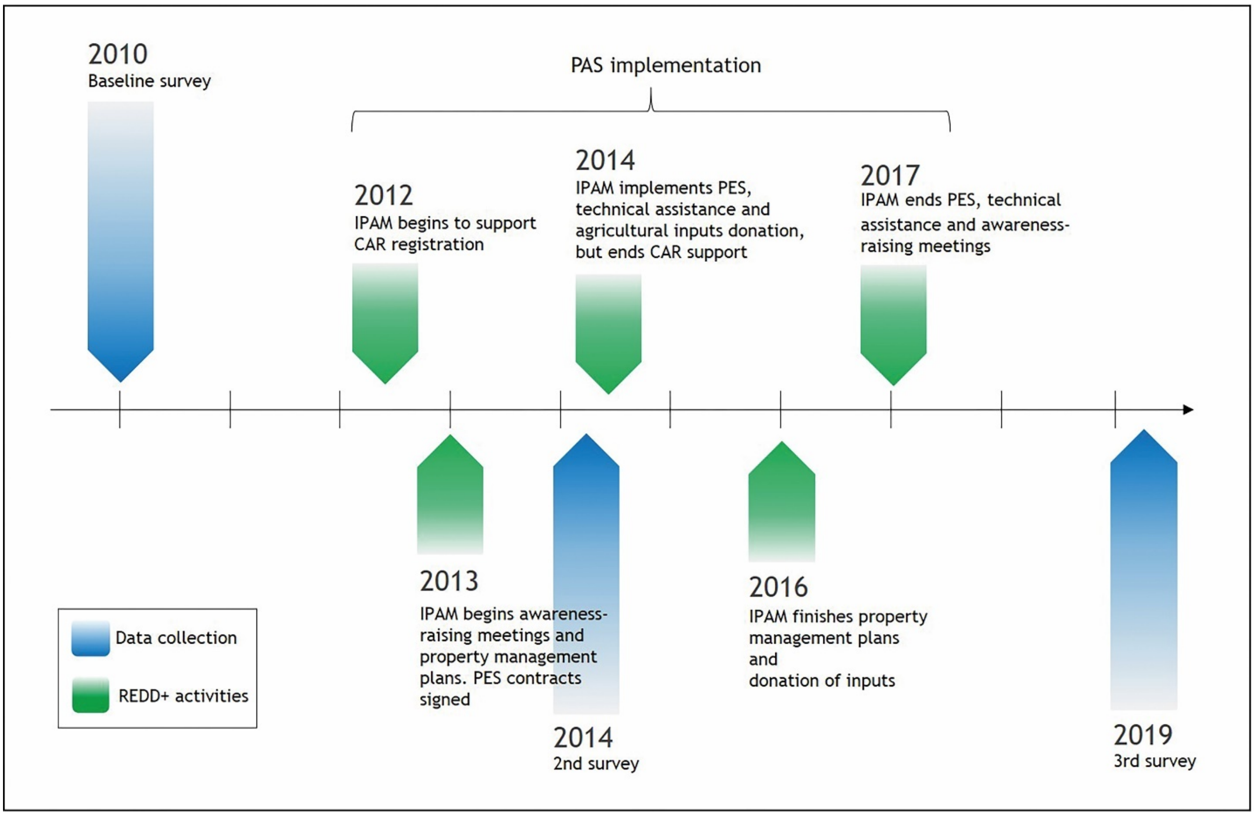

Figure 1 – PAS implementation and CIFOR-GCS data collection timeline. Source: own elaboration, based on Cromberg et al. (2014b) and IPAM (2016).

3. Sampling and data collection

Identical survey instruments were employed through face-to-face interviews in four treatment and four control communities (Figure 2) in three time periods: June-July 2010 (baseline period), February-March 2014 (~2 years after the REDD+ initiative's onset) and March-May 2019 (~7 years after onset and 2 years after the initiative ended).

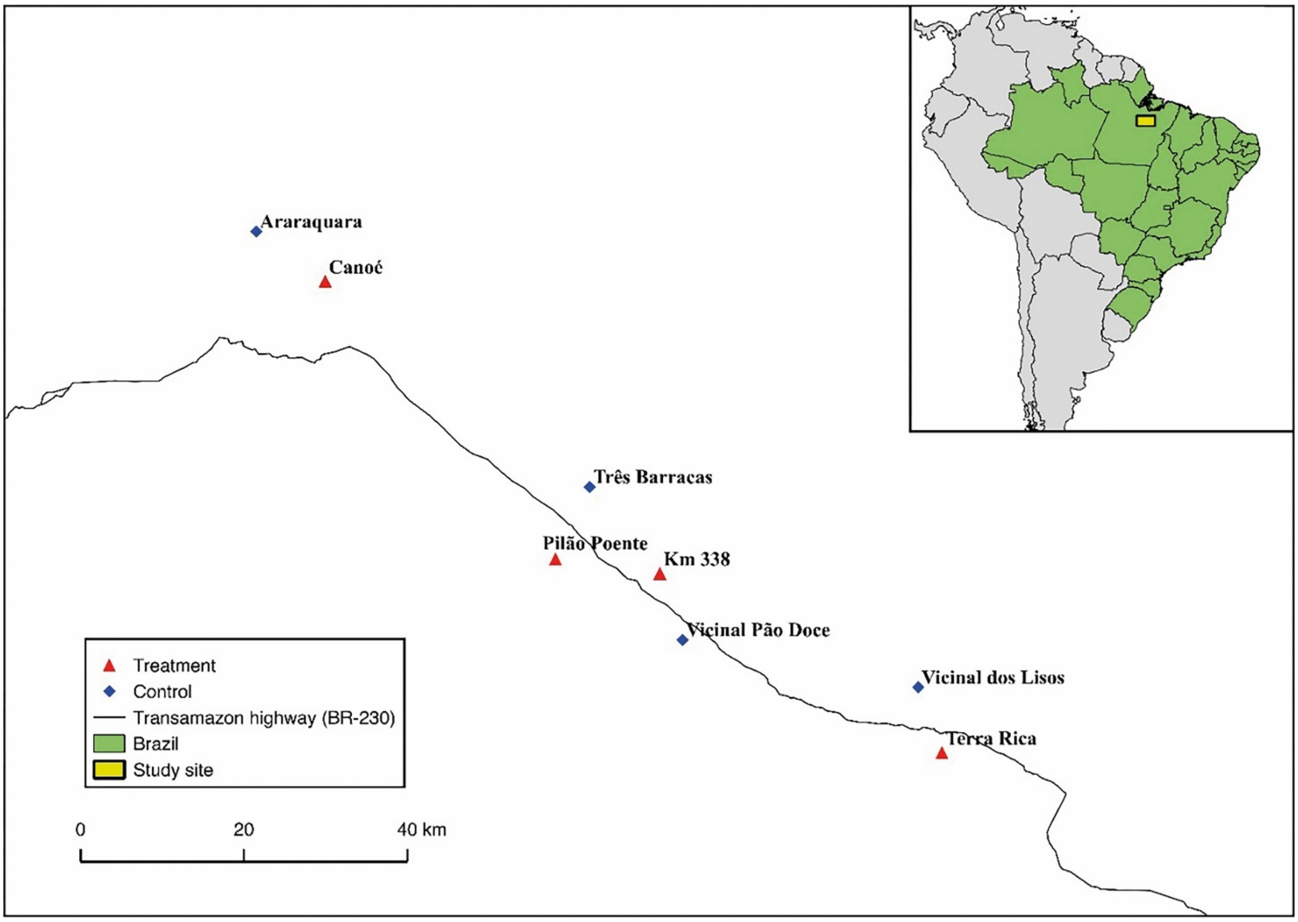

Figure 2 -Location of both treatment and control communities along the Transamazon highway (Brazil).

Treatment communities were randomly selected among the twelve communities in which IPAM intended to implement the initiative. Control communities were selected from a pool of fifteen other Transamazon communities, based on a pre-matching procedure to find communities with similar characteristics likely to influence both initiative placement and land use/well-being outcomes (e.g., forest cover, deforestation pressures, and distance to the main road) (Sunderlin et al., 2016).

For our analysis, the key unit is the household, defined here as the group of people, usually family members, living under the same roof and pooling resources (Sunderlin et al., 2016). We chose this level because households make the deforestation decisions, mainly driven by farming, and received REDD+ incentives (e.g., PES, sustainable livelihood alternatives).

In each of the four treatment and four control communities, 30 households (240 in total) were randomly selected for interviews in the baseline period. The sample in treatment communities was further stratified to include both PAS participants and non-participants, a procedure adopted to investigate intra-community spillover effects. A total of 113 households were interviewed in the three survey points (2010, 2014, 2019), comprising three different groups: 46 control households; 52 treatment households (i.e. participated in REDD+ initiative, receiving all interventions described in Section 2); and 35 non-participant households (i.e. did not participate in REDD+, but inhabited treatment communities).

4. Empirical strategy

4.1. Assessing short-term outcomes

Our first goal was to assess the outcomes of the REDD+ initiative on deforestation and household wellbeing, respectively. To do so, we adopted a DID approach also known as Before-After-Control-Impact (BACI) evaluation. Thus, we assumed that changes in outcome variables from pre- to post-intervention in the control group represented what would have happened to the treatment group without the intervention (counterfactual scenario) (Fredriksson and Oliveira, 2019). For this to be credible, both groups should follow the same trend in pre-treatment outcomes (parallel trend assumption). This assumption implies that, in the absence of the treatment, outcomes for the treatment and control groups would change at the same rate (Ryan et al., 2019). Indeed, the parallel trend assumption was confirmed using a placebo test over a pre-treatment period (2008-2010) in which no effects were detected (Appendix A).

Accordingly, we estimated the impact of the intervention on treatment households by calculating the difference between the changes in outcomes over time (between 2010 – the baseline – and 2014 – two years post the REDD+ indicative's launch) from the control and treatment groups. This is the so-called Average Treatment Effect on the Treated (ATT): ATT = $E\,(y^1 - y^0|D = 1)$, where

y^1 denotes the result variable under the treatment, y^0 the same variable in the absence of treatment, and D is a dummy that takes on the value of one when the household has been treated, and zero otherwise. We adopted two outcome variables (Table 1): (i) forest cover (% of primary and secondary forest in the household property), as self-stated by household respondents, and selectively validated through remote-sensing data (cf. discussion below), as our proxy for deforestation; (ii) perceived wellbeing (self-declared by household respondents in interviews, compared to previous years: 1 = improved; 0 = not improved).

Table 1 – Summary statistics for treatment and control groups.

Variables	Treatment group		Control group		ND	
Pre-treatment variables	Mean	Std dev.	Mean	Std dev.	Raw	Matched
Forest cover in 2008 (% of land area)*	0.75	0.16	0.64	0.23	0.55	0.09
Forest cover in 2010 (% of land area)*	0.70	0.16	0.60	0.23	0.52	0.04
Total land area in 2010 (ha)*	79.45	35.19	91.61	54.39	-0.26	-0.01
Total income in 2010 (BRL)*	27,931.35	21,525.78	34,906.54	25,455.53	-0.29	0.06
Household head age in 2010 (years)*	48.48	11.45	53.91	11.42	0.47	0.12
Household members in 2010 (number)*	5.54	2.45	5.33	2.63	0.08	0.07
Perceived well-being in 2010 (1 = improved; 0 = not improved)	0.65	0.48	0.59	0.50	-	-
Post-treatment variables						
Forest cover in 2014 (% of land area)	0.65	0.19	0.50	0.24	-	-
Forest cover in 2019 (% of land area)	0.54	0.23	0.45	0.24	-	-
Perceived well-being in 2014 (1 = improved; 0 = not improved)	0.79	0.41	0.46	0.50	-	-
Perceived well-being in 2019 (1 = improved; 0 = not improved)	0.58	0.50	0.35	0.48	-	-
Total land area in 2014 (ha)	79.45	35.19	91.61	54.39	-	-

Note: *matching covariates. Treatment group: 52 households that participated in the REDD+ initiative. Control group: 46 households living in the control communities. ND: normalized differences between the two groups. Forest cover was estimated for 2008 as a recall period from the 2010 survey.

As the intervention was not randomly attributed, confounders could be unevenly distributed between treatment and control groups (Ferraro and Pattanayak, 2006). Therefore, besides using DID to control for time-invariant unobserved confounders (Deschenes and Meng, 2018), we minimized selection bias combining DID with matching to preprocess the data and control for observable confounders (Ho et al., 2007). This means we applied the DID estimator to those households from treatment and control groups that were statistically similar in pre-treatment observable characteristics (baseline observable covariates – Table 1) (Imbens, 2004). We adopted the nearest-neighbor matching estimator, by matching each treated household to two of the most similar control households (Abadie et al., 2004), as well as two robustness checks. For the first, we again used the nearest-neighbor, but this time matching each treated household to four of the most similar controls. The second was the kernel-based propensity score matching, by which we compared households with the closest probability of being treated given the same set of observable covariates we used before (Rosenbaum and Rubin, 1983).

Baseline values of observable covariates before matching were significantly different between treatment and control groups (Table 1). All normalized differences of the matching covariates, except for household members, were higher than 0.25 standard deviations. After matching, however, normalized differences dropped below 0.25, demonstrating that a plausible counterfactual was created from the matching procedure (Imbens and Wooldridge, 2009).

Finally, following Börner et al. (2020) and Wunder et al. (2020), we calculated normalized effect sizes (Cohen's *d*) as indicators of the magnitude of the initiative's impact on both forest cover and perceived well-being. The effect size is the standardized mean difference of the outcome variables between control and treatment groups (Coe, 2002). The formula is $d = M_1 - M_0$ / SDpooled, where M_1 denotes the mean of the treatment group, M_0 denotes the mean of the control group, and SDpooled = $\sqrt{} SD_1^2 (N_1 - 1) + SD_0^2 (N_0 - 1) / N_0 - N_1 - 2$, where SD_1^2 and SD_0^2 are the square of the respective standard deviations, and N_1 and N_0 are the respective sample sizes (Acock, 2014).

To minimize the risk of bias in our household self-reported forest data, we cross-checked the estimates with remotely sensed data from the Brazilian Annual Land Use and Land Cover Mapping Project (MapBiomas) (www.mapbiomas.org). IPAM shared property boundaries from 43 of the 52 treated households in our sample. Through MapBiomas forest cover and total land area data, we analyzed changes in percent forest cover from 2010 to 2018 for these 43 overlapping households. We found that estimates matched well to those derived from self-reported data. Paired t-test and f-test of annual differences in self-reported and observed forest cover revealed that they are not statistically significantly different in the means, as well as in standard deviations (see Appendix B).

4.2. *Evaluating outcomes after the initiative ended*

Our second goal was to evaluate to what extent potential outcomes persisted after the initiative ended. To this end, we also used the DID-matching estimators to compare differences in the same outcome variables (Table 1) over 2014 (during the initiative's implementation) and 2019 (two years after its end) between treatment and control groups. If the outcomes were sustained, we would expect similar results in this analysis than those with the comparison between 2010 (baseline) and 2014.

4.3. *Intra-community spillover*

Finally, to check for intra-community spillover effects, we reused the DID matching estimators to test for outcome variables differences between the control and the non-participant group, for 2010-14 and 2014-19, respectively. We first confirmed the parallel trend assumption for control and non-participant groups running another placebo test (Appendix A). We also compared the normalized differences of the matching covariates before and after matching, concluding once more that a plausible counterfactual had been constructed (Appendix C).

5. Results

5.1. *Longitudinal impact assessment*

Our results show that the REDD+ initiative achieved positive outcomes in terms of both forest conservation and perceived well-being improvement (Table 2). Aligned with the results of the earlier study on the effectiveness of PAS in reducing deforestation (Simonet et al., 2019), ATT for forest cover was significant for all DID matching estimators in the first period (2010-14). This implies an average of 7.80% to 10.32% of forest cover was saved by the initiative, or the equivalent of 6.20 to 8.20 ha per farm, given that the average land area of the treatment group was 79.45 ha in 2014 (Table 1). Moreover, ATT for perceived wellbeing was also positively significant in this first period: the REDD+ initiative increased the probability of improved enrollee perception of wellbeing by 27% to 44%. Cohens' *d* for forest cover (0.32 – Table 3) and perceived wellbeing (0.37) were numerically similar, both indicating small-to-medium effect sizes according to Sawilowsky (2009) (0.01 = very small; 0.2 = small; 0.5 = medium; 0.8 = large; 1.2 = very large; 2.0 = huge).

Table 2 - Longitudinal forest and wellbeing impacts of the REDD+ initiative on the treatment group.

DID-matching estimator	Forest cover (%)		Perceived well-being (1 = improved; 0 = not improved)	
	2010-2014 Coeff[a] (S.E.)[b]	2014-2019 Coeff (S.E.)	2010-2014 Coeff (S.E.)	2014-2019 Coeff (S.E.)
NNM(2X)	7.80* (4.36)	-1.75 (4.67)	0.45** (0.19)	-0.31** (0.15)
NNM(4X)	8.08* (4.57)	-1.73 (5.29)	0.44** (0.18)	-0.28* (0.15)
PSM(kernel)	10.32** (4.00)	-3.65 (4.87)	0.27* (0.16)	-0.17*** (0.15)

Note: Significance level: *=10%; **=5%; ***= 1%. DID-matching estimators: nearest neighbor using four matched observations as controls (NNM(4X)) and using two matched observations (NNM(2X)); kernel-based propensity score matching (PSM(kernel)). [a] Coefficient represents ATT (treated are the treatment group: 52 households that participated in the REDD+ initiative). [b] Standard errors in parentheses.

Table 3 - Normalized effect sizes (Cohen's *d*) of the outcomes.

Variables	Cohens' *d*	Treatment group		Matched control households	
		Mean	Std dev.	Mean	Std dev.
Δ Forest cover between 2010-2014 (% of land area)	0.32	0.05	0.11	-0.10	0.20
Δ Perceived well-being between 2010-2014 (1 = improved; 0 = not improved)	0.37	0.13	0.63	-0.13	0.79

Note: Treated households: 52 that had participated in the REDD+ initiative. Matched control households: 40 that matched to treated households using nearest neighbor estimator with two matched observations as controls, from the total of 46 households living in the control communities.

However, according to our estimates, deforestation resumed after the initiative ended (Table 2). Indeed, for the second period (2014-2019), we found that ATT from all DID matching estimators were non-significant for forest cover. We also detected a significant negative impact on perceived well-being for the same period.

The results for forest cover change from the nearest neighbor estimator, using two matched observations as controls, are provided in Figure 3. We observe that forest cover continued to decrease from 2008 to 2019 in both treatment and control groups. However, there is a break in the forest loss trend between 2010 and 2014 in the treatment group. Despite that, from 2014 and 2019, the treatment group lost forests again in a similar trend to the control group.

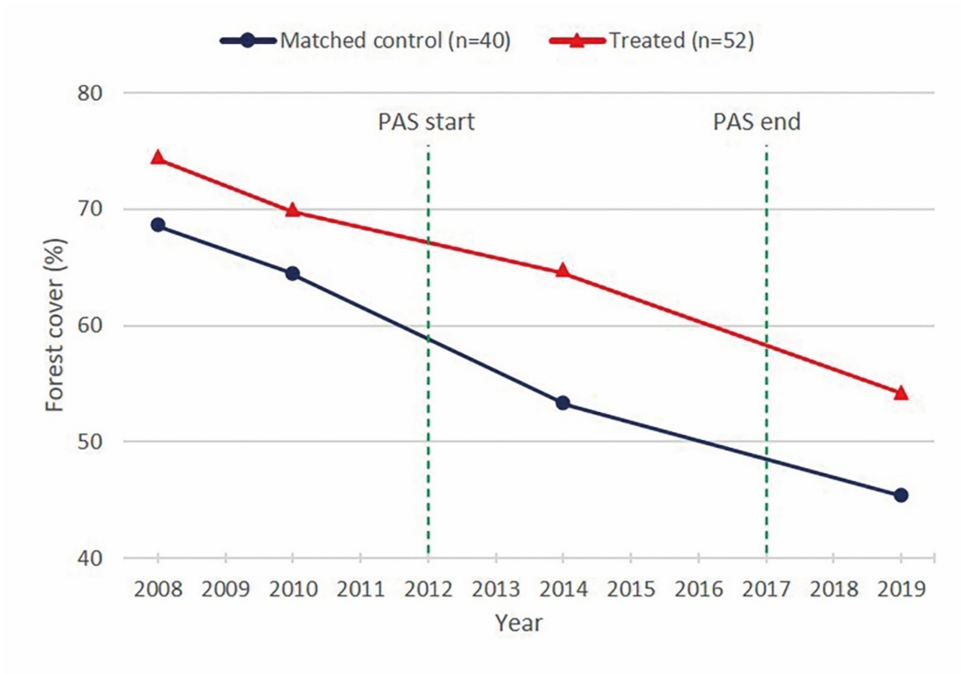

Figure 3 – Forest cover (% of forest of the household property) change among treated and their matched control households.

Note: Treated households: 52 that had participated in the REDD+ initiative. Matched control households: 40 that matched to treated households using nearest neighbor estimator with two matched observations as controls, from the total of 46 households living in the control communities. Forest cover was estimated in 2008 as a recall period from the 2010 survey. The REDD+ initiative started in 2012 and ended in 2017 (see Figure 1).

5.2. Intra-community spillover

We found no evidence for intra-community spillover effects for the REDD+ initiative (Appendix D). ATT from all DID matching estimators were insignificant for forest cover and perceived well-being in both periods (2010-2014; 2014-2019).

6. Discussion

6.1. Short-term effects on forests and well-being

Our results indicate the REDD+ initiative investigated here for the group of ICDP-cum-PES treated households achieved win-win outcomes in terms of reducing deforestation and improving well-being while it was being implemented. These findings therefore add to the emerging evidence that REDD+ initiatives have demonstrated statistically significant success in reducing deforestation (Simonet et al., 2018). Moreover, our findings highlight positive impacts on perceived well-being, which differ from observations in other REDD+ initiatives that evidenced insignificant impacts (Sunderlin et al., 2017).

Notably, we found no evidence that deforestation reduction reached by the REDD+ initiative was offset by intra-community spillover. This means that, according to our estimations, reducing deforestation in REDD+ participant properties did not lead to an increase in deforestation in non-participant properties. This could have occurred, for instance, if participants had shifted their deforestation activities to non-enrolled plots, which would have reduced REDD+ net impacts as a consequence (Pfaff and Robalino, 2017).

As this REDD+ initiative was based on a mix of on-the-ground interventions (see Section 2), we might want to also discuss the likely efficiency across different components. We conjecture the original deforestation reduction likely resulted more from PES than from the ICDP-type sustainable livelihood alternatives or CAR registration, for two reasons. First, in 2014, households were surveyed when the REDD+ initiative was still beginning (Figure 1), therefore before the main livelihood alternatives (technical assistance, free agricultural inputs) were delivered. Although the first conditional payment also occurred soon after the 2014 survey, PES contracts were signed earlier (beginning of 2013). It is therefore reasonable to suppose that households could have reduced deforestation in the dry season of 2013 (when they usually convert forest to pasture/crops) in anticipation of conditional payments that would begin around March 2014. Similarly, perceived wellbeing improvements could have resulted from an expectation of the payments that would start soon. Second, besides IPAM, several other organizations offered administrative support in the Transamazonian region for registering households' properties under CAR. As a result, most of the control households received the same intervention from elsewhere, as already noted by Simonet et al. (2019).

If the initial outcomes did result mostly from PES, as we believe, our findings corroborate the majority of studies employing counterfactual designs, showing the potential effectiveness of PES in forest conservation (e.g., Alix-Garcia et al., 2012; Costedoat et al., 2015; Jayachandran et al., 2017; Montoya-Zumaeta et al., 2019; Robalino and Pfaff, 2013; Scullion et al., 2011), and in delivering small but often significant wellbeing improvements (e.g., income, assets) (e.g., Duan et al., 2015; Hegde and Bull, 2011; Jack and Cardona Santos, 2017; Sims and Alix-Garcia, 2017;

Uchida et al., 2007), despite fewer studies finding null impacts (e.g., Arriagada et al., 2018, 2009; Sánchez-Azofeifa et al., 2007; Wiik et al., 2019). The moderately positive outlook on PES for delivering these win-win outcomes is supported by more recent meta-studies and systematic reviews (Snilsveit et al., 2019; Wunder et al., 2020a).

Our findings also align with the emerging evidence showing the effect size of PES on forest conservation outcomes is limited, even if our estimated effect sizes (0.32 – Table 3) were above average. Based on data from 19 studies measuring PES outcomes for forest conservation, Wunder et al. (2020a) showed Cohens' *d* effect size ranged from 0.0 to ~0.5, with an average value around 0.2. According to Sawilowsky's (2009) interpretation of Cohens' *d*, this means PES effect size varies in reviewed studies from very small to medium, being small on average. However, to make a fair evaluation, PES must be compared with other forest conservation interventions (e.g., protected areas, certification, decentralized forest management) and they all exhibit small effect sizes in general (Börner et al., 2020). In fact, PES has larger impacts than these other interventions, though differences are small (Wunder et al., 2020a).

6.2. Long-term effects of the REDD+ initiative

In our study, deforestation reductions and wellbeing improvements were more temporary than permanent – both improving trends reversed post-treatment. However, the REDD+ initiative still left a lasting gain for the environment: the treatment group cleared forests again as quickly as the control group, but without exceeding it to 'catch up' on the earlier mitigations, meaning there was a net forest gain over time (Skutsch and Trines, 2010). Our findings therefore match very well with the aforementioned evaluation of the permanence outcomes from the PES RCT in Uganda (see Section 1) (World Bank, 2018). The study showed deforestation resumed among former PES recipients once payments ended, but without exceeding the reference scenario; thus, leaving the initial conservation gains intact.

Notably, the literature on PES permanence suggested that asset-building PES ('active establishment') may have better chances of locking in forest gains than activity-reducing ('passive conservation') PES, to the extent the former manage to lastingly boost the adoption of environmentally desirable land uses established (e.g. agroforestry systems) (see Section 1). In fact, this was the main goal of the REDD+ initiative investigated here when providing the ICDP investments in alternative livelihoods (see Section 2). Following this logic, one possible explanation for why deforestation reduction was not sustained after the REDD+ initiative ended could be that the alternative livelihood activities promoted did not take off, leastways at a desirable scale. Notably, during the 3rd data collection phase, 27 treated interviewees (i.e., 52%) declared they did not adopt alternative livelihood activities between 2014 (when the main ICDP-type support started) and 2019. Studies accessing conservation outcomes of ICDP-type programs indeed most indicate failures rather than successes (Hughes and Flintan, 2001; Roe et al., 2015). This may be because programs often rely on upfront subsidies instead of conditional payments to promote alternative livelihood activities, which increases the risk of non-compliance (Pagiola et al., 2020). Therefore, despite that targeted households had received technical assistance and free agricultural inputs, a significant part of them may have not adopted sustainable livelihood alternatives. Otherwise, lasting deforestation reduction outcomes might have been possible.

7. Concluding remarks

We presented one of the first evaluations of the permanence of a local REDD+ initiative's outcomes. We found that the PAS initiative reduced deforestation in the Transamazon region and improved the perceived well-being of treated households, while being actively implemented. Post-intervention, deforestation resumed at a similar pace as the control group, yet without 'catching up' on the temporary forest gains made. Our results, therefore, suggest that temporary performance-based REDD+ benefit flows may effectively delay, though not permanently eradicate deforestation. Still, they can be important in mitigating the climate change challenge the world is facing.

As long as the basic environmental externality persists, i.e. that standing forests privately cannot compete with the yields from alternative land uses, we should not expect the miracle that a temporary payment would permanently change the logic of the productive system. ICDP type of investments try to achieve exactly this type of change, but have typically been little successful, as also seems the case, at first sight, in the PAS initiative we analyzed. PES used as adoption subsidies for environmentally beneficial land uses seem to have a somewhat better record (see Section 1).

As for perceived wellbeing impacts, we failed to detect permanence of improvements; perceptions clearly improved during REDD+ implementation but were then negatively impacted after the initiative ended. Does that mean the project left no permanent welfare gains behind? We would be cautious to press such an extreme interpretation, given that ex-post stated negative subjective wellbeing can also sometimes be seen as a vote of protest against the sudden withdrawal of benefits that were expected to be steadily provided anew – and still go perfectly hand in hand with lastingly higher incomes (e.g. Montoya-Zumaeta et al., 2019).

We close by highlighting that the external validity of our study must be approached with two cautions. First, our analysis relied on a subsample of households that were targeted for PES payments; thus from this we cannot extrapolate to the entire PAS project (see Section 2). Second, REDD+ as implemented on the ground is a basket of interventions, which includes incentives (direct payments and alternative livelihood activities), disincentives (e.g., law enforcement) and enabling measures (e.g., tenure clarification) (Duchelle et al., 2017). Thus, any generalization to other REDD+ sites must consider, besides the local context, the type of interventions applied.

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Appendix A. Impact on treatment and non-participant groups over 2008 and 2010 (placebo test).

DID-matching estimator	Forest cover (%) Treatment group Coeff[a] (S.E.)[b]	Forest cover (%) Non-participant group Coeff (S.E.)
NNM(4X)	0.00 (0.00)	0.00 (0.00)
NNM(2X)	0.00 (0.00)	0.00 (0.00)
PSM (kernel)	-0.71 (0.01)	-0.45 (0.01)

Note: Treatment group: 52 households that had participated in the REDD+ initiative. Non-participant group: 35 households from treatment communities that had not participated in the REDD+ initiative. No statistically significant effects were detected to both groups in comparison to the control group over the pre-treatment period (2008-2010), which confirms the parallel trend assumption. [a] Coefficient represents ATT. [b] Standard errors in parentheses. Forest cover was estimated in 2008 as a recall period from the 2010 survey.

Appendix B. Paired tests on the equality of forest cover means and standard deviations.

	Forest cover (%)							
	Mapbiomas data		Self-reported data		Paired t-test		Paired f-test	
Year	Mean	Std dev.	Mean	Std dev.	t	p-value	f	p-value
2009/2010	0.67	0.17	0.70	0.15	-1.33	0.19	1.39	0.29
2013/2014	0.65	0.16	0.65	0.17	-0.12	0.91	0.87	0.66
2018/2019	0.59	0.18	0.55	0.21	1.33	0.19	0.76	0.39

Note: Mean forest cover was estimated based on 43 households' properties from our treatment group (N=52). The paired t-test of annual differences and the f-test of equality of variances revealed that self-reported and observed forest cover were not statistically significantly different both in the means and in standard deviations, confirming the validity of our self-reported data. We used MapBiomas data from the preceding years to our interview surveys because the Amazon dry season, when households usually convert forest to pasture/crops, runs between May-September, but interviews were conducted before this period or in its beginning (see Section 2).

Appendix C. Summary statistics for non-participants and control group.

Variable	Non-participant group		Control group		ND	
Pre-treatment variables	Mean	Std dev.	Mean	Std dev.	Raw	Matched
Forest cover in 2008 (% of land area)*	0.72	0.20	0.64	0.23	0.41	-0.04
Forest cover in 2010 (% of land area)*	0.68	0.19	0.60	0.23	0.38	-0.08
Total land area in 2010 (ha)*	117.70	101.30	91.61	54.39	0.32	0.17
Total income in 2010 (BRL)*	43,224.72	74,367.73	34,906.54	25,455.53	0.16	0.20
Household head age in 2010 (years)*	50.91	13.48	53.91	11.42	-0.29	-0.10
Household members in 2010 (number)*	5.06	2.38	5.33	2.63	-0.13	-0.10
Perceived well-being in 2010 (1 = improved; 0 = not improved)	0.59	0.50	0.59	0.50	-	-
Post-treatment variables						
Forest cover in 2014 (% of land area)	0.61	0.21	0.50	0.24	-	-
Forest cover in 2019 (% of land area)	0.55	0.22	0.45	0.24	-	-
Perceived well -being in 2014 (1 = improved; 0 = not improved)	0.62	0.49	0.46	0.50	-	-
Perceived well -being in 2019 (1 = improved; 0 = not improved)	0.53	0.51	0.35	0.48	-	-
Total land area in 2014 (ha)	118.22	102.78	91.61	54.39	-	-

Note: *matching covariates. Non-participant group: 35 households from treatment communities that had not participated in the REDD+ initiative. Control group: 46 households living in the control communities. ND: normalized differences for the two groups. Forest cover was estimated in 2008 as a recall period from the 2010 survey.

Appendix D. Longitudinal impact of the REDD+ initiative on the non-participant group.

DID-matching estimator	Forest cover (%)		Perceived well-being (1=improved; 0=not improved)	
	2010-2014 Coeff[a] (S.E.)[b]	2014-2019 Coeff (S.E.)	2014-2019 Coeff (S.E.)	2014-2019 Coeff (S.E.)
NNM(4X)	3.77 (4.21)	2.21 (5.53)	0.16 (0.21)	0.11 (0.16)
NNM(2X)	5.36 (4.47)	2.03 (5.87)	0.10 (0.20)	0.11 (0.17)
PSM (kernel)	7.93 (4.57)	-3.21 (5.01)	0.13 (0.18)	- 0.00 (0.17)

Note: DID-matching estimators: nearest neighbor using four matched observations as controls (NNM(4X)) and using two matched observations (NNM(2X)); kernel-based propensity score matching (PSM(kernel)). [a] Coefficient represents ATT (treated are the non-participant group: 35 households from treatment communities that had not participated in the REDD+ initiative). [b] Standard errors in parentheses.

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