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Does REDD+ Complement Law Enforcement? Evaluating Impacts of an Incipient Initiative in Madre de Dios, Peru

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Subnational initiatives to Reduce Emissions from Deforestation and forest Degradation and enhance carbon stocks (REDD+) have been implemented across the tropics over the last decade. Such initiatives are often embedded within pre-existing conservation policies, such as forest law enforcement, making it challenging to disentangle attributable impacts. In this article, we analyze a new REDD+ project implemented in Brazil nut concessions in the southeastern Peruvian Amazon. Public law enforcement to verify compliance with Peru's Forest Law was already ongoing and intensified locally during our study period. Thus, we combine longitudinal data from remote sensing and household surveys of 197 concessionaires in a before–after control-intervention (BACI) study design to: a) evaluate the project's impacts during the 2012–2018 period on deforestation, forest degradation, and the participants' wellbeing and b) assess how the law-enforcing field inspections may have complemented the project effects. Our results show that the REDD+ initiative had insignificant effects on deforestation and forest degradation, but confirm the curbing effects of the field inspection measures on forest loss. The non-significance of the REDD+ effects may reflect delays in cash incentive payments to enrolled concessionaires, lack of careful alignment of benefit provision with project participants, and limited enforcement of project conditionalities. Most REDD+ participants reported a reduced subjective wellbeing, which may reflect the frustrated expectations associated with project implementation. We discuss the implications of our results and outline lessons for similar tropical forest conservation initiatives.

Keywords: environmental policy, impact evaluation, deforestation, forest degradation, perceived wellbeing, propensity score matching, policy-mix, difference-in-difference

INTRODUCTION

Conservation and sustainable management of tropical forests are key to mitigate greenhouse gases (GHG) emissions and therefore keep projected global warming below 1.5°C by 2030, as recognized in the Paris Climate Agreement. About 70% of land-use emissions in the tropics is caused by deforestation and forest degradation, contributing to over 8% of the net global GHG releases (Le Quéré et al., 2018). Over the last decade, initiatives aiming to reduce emissions from the forest sector have emerged across the tropics. These efforts range from national or subnational strategies and programs to localized projects (Duchelle et al., 2019). They form a part of global initiatives known as "REDD+"—Reducing Emissions from Deforestation and forest Degradation, and conservation, sustainable management of forests, and enhancement of forest carbon stocks in developing countries. These were conceived originally as a mechanism allowing countries to obtain payments from a global carbon market conditioned on the mitigation of GHG emissions within a multitier payments for environmental services (PES) scheme (Wertz-Kanounnikoff and Angelsen, 2009; Wunder et al., 2020b). However, nowadays, REDD+ represents an umbrella term referring to multi-objective policy mixes implemented at different scales, combining customized sets of incentives, disincentives (e.g. command-and-control measures), and enabling conditions (e.g., land tenure reforms) for responsible land stewardship mitigating climate change (Angelsen, 2017; Duchelle et al., 2018).

While REDD+ national and subnational programs have been considerably supported by multilateral and bilateral donors through result-oriented aid (Angelsen, 2017), many local initiatives emerged with the expectation of being funded with sales from carbon credits issued in voluntary markets. However, up to 2018, only one-third of initiatives participating in these markets have achieved any carbon sale at all, selling only 5% of the foreseen credit volume (Simonet et al., 2018a). Furthermore, frequently local REDD+ projects have emerged as outgrowths from the previous Integrated Conservation and Development Projects (ICDP), and only few of these have introduced incentives to the enrolled participants conditioned on their committed environmental achievements (Sunderlin and Sills, 2012). The real-world challenges of land-tenure insecurity and the unpredictable funding flows inhibited the originally envisioned multitier-PES model for REDD+ (Wunder et al., 2020b).

As mentioned above, on-the-ground implemented REDD+ initiatives typically pursue their objectives of increasing forest carbon stocks by introducing incentives, disincentives, and enabling settings to foster the behavioral change among agents (Sunderlin and Sills, 2012). Numerous projects of this type have been implemented in tropical countries over the last two decades, although very few of them had their forest–carbon and land-use outcomes subjected to rigorous impact evaluations (Duchelle et al., 2018). Bos et al. (2017) found a significant reduction in tree-cover loss at the village level in about half of 23 local REDD+ initiatives studied in six countries, when compared to the non-REDD+ control areas. Disappointing REDD+ performance is found in Amazon Fund-supported, Verified Carbon Standard (VCS) certified REDD+ projects in Brazil, mostly because of *ex ante* exaggerated baselines and *ex post* observed low forest pressures (West et al., 2020). Meanwhile, some reconfirming evidence of country-level REDD+ conservation impacts were found in Guyana (Roopsind et al., 2019).

In spite of the aforementioned difficulties, some REDD+ pilot schemes have used PES-type incentives to local people as one instrument for on-the-ground implementation, resulting in highly context-dependent impacts (Wunder et al., 2020a)[1]. For instance, Simonet et al. (2018b) scrutinized the "Sustainable Settlements in the Amazon" project, implemented during 2010–2014 to deter deforestation along Brazil's BR-230 Trans-Amazonian Highway, finding significant reductions in deforestation in a high-deforestation setting. Jayachandran et al. (2017) conducted a randomized control trial in Uganda, finding a 2-year PES trial there to decrease (previously high) tree-cover loss by 4.9% points. Meanwhile, Börner et al. (2013) and Cisneros et al. (2022) evaluated the tree-cover loss impacts of Brazil's Bolsa Floresta Program (Amazonas State), finding small but significant impacts in what already *ex ante* was a low-deforestation context. The same holds true for Peru's REDD+-like National Program for Forest Conservation, with pilots focused on Amazon indigenous lands (Börner et al., 2016b; Giudice et al., 2019). Conversely, Collins et al. (2022) found that a REDD+ readiness program on Pemba, Tanzania had produced negligible deforestation impacts after a decade of being implemented. Going beyond deforestation, forest degradation has been scarcely assessed. Mohebalian and Aguilar (2018), for example, reported a small but statistically significant reduction of disturbance in forested areas enrolled in Ecuador's public conservation-support program, Socio Bosque. Similarly, Sharma et al. (2020) found evidence indicating that a pilot REDD+ pilot project in Nepal has produced the same positive effects at 2 years of being implemented.

Regarding quasi-experimental evidence of REDD+ socioeconomic impacts, the results are of mixed nature. Sunderlin et al. (2017) did not find significant contributions of 22 local projects in six tropical countries on either incomes or wellbeing for the period 2010–2014. However, Larson et al. (2018), using a subset of such initiatives, found that women participating in REDD+ reported decreased subjective wellbeing. Jagger and Rana (2017) concluded that REDD+ activities had a positive impact on protecting the customary land rights, but negative influence on reported wellbeing of the participant villages for the 2008–2011 period in Kalimantan, Indonesia. Meanwhile, Solis et al. (2021) reported no-significant effect on the participant households' incomes in two local REDD+ projects in Peru for the period 2011–2014—one of these being the Madre de Dios REDD+ project analyzed below.

In the complex realities on the ground, REDD+ typically functions within contextualized policy and intervention mixes; carbon-based incentives interact with other site-level conservation instruments. Policy mixes are often motivated by multiple objectives, such as balancing conservation with livelihoods goals (Barton et al., 2017). Multi-instrumental REDD+ implementation can also be justified by multiple market failures (e.g., property rights, externalities, etc.), a complex ecological dynamic (e.g., conservation "tipping points"), or *ex ante* unknown behavioral reactions (Bouma et al., 2019). Some tradeoffs between components within the mix can also emerge (Börner et al., 2015b).

[1]For recent evidence, see Samii et al. (2014), Börner et al. (2016a), Snilsveit et al. (2019), and Wunder et al. (2020a).

Analytically, policy mixes make it challenging to attribute impacts to singular instruments (Wunder et al., 2020a). However, this might be achieved whenever different components of the treatment mix are, deliberately or accidentally, being rolled out non-simultaneously in time and/or space. For instance, for the watershed-based Moyobamba initiative in the Peruvian High Amazon, PES and command-and-control protection, rolled out in only partially overlapping areas; both separately contributed to conservation impacts in attributable portions (Montoya-Zumaeta et al., 2019). Meanwhile, both PES and protected areas national-level policies in Mexico were shown to have enhanced forest conservation, yet only PES also significantly contributed to poverty alleviation (Sims and Alix-Garcia, 2017). Furthermore, the interactions between policies are seldom being tested or discussed in the conservation impact literature. Yet, Robalino et al. (2015) found that both protected areas and PES in Costa Rica achieved statistically significant conservation impacts when applied individually, but such effects vanished when implementation was simultaneous. Nevertheless, significant complementarities between the same policies, PES and protected areas were found in villages settled close to the borders of protected areas in Mexico, but not in villages whose territories are entirely located within these (Sims and Alix-Garcia, 2017). In the same way, some authors sought to disentangle contributions from distinct policies within the multifaceted strategy to slow down deforestation in the Brazilian Amazon, as implemented since the early 2000s. Assunção et al. (2015) estimated that conservation policies were responsible for 56% of avoided deforestation across the region over the time period 2005–2009, while the rest could be attributed to decreasing prices of agricultural outputs. Recently, Harding et al. (2021) found that between 2009 and 2013, blacklisting illegally deforesting municipalities was more effective than conservation policies and the soy moratorium.

In this article, we focus primarily on the local impacts of a REDD+ initiative involving Brazil nut concessionaires in the department of Madre de Dios in southeastern Peru, in the triangular border area with Acre, Brazil and Pando, Bolivia. Notably, recent new road infrastructure here has raised environmental threats from deforestation and illegal logging (Chávez Michaelsen et al., 2013). We aim to inspect to what extent the REDD+ project in Madre de Dios has mitigated deforestation and forest degradation while improving the participant's wellbeing, and scrutinize any complementarities with law-enforcing field inspections. To do so, we combine the matching and difference-in-difference (DiD) methods, using longitudinal remote-sensing and household surveys data from 197 concessionaires in a before–after control-intervention (BACI) design. Our results confirm the curbing effect of command-and-control measures on forest loss, while REDD+ so far has been ineffective in significantly reducing deforestation and forest degradation. We also find evidence that the households participating in REDD+ reported decrease on their perceived wellbeing in comparison with the non-participants, reflecting their frustrated expectations associated to uncompliance of some project promises.

We contribute to the existing literature on the impacts of forest conservation strategies in at least three ways. First, we provide one more case study to the still scarce literature about rigorously evaluated REDD+ impacts. Second, we address not only deforestation but also forest degradation effects—a widely neglected issue in evaluating the conservation impacts. Third, we empirically incorporate policies that are not explicitly REDD+ aligned into the analysis, as Miteva et al. (2012) had asked for. We have structured our arguments as follows: Section The Context: Brazil Nut Concessions in Madre de Dios, Peru contextualizes the impact assessment scenario. In Section Methods, we explain our methods. Section Results presents our results while in Section Discussion and Conclusion, we discuss the lessons learned and the broader perspectives.

THE CONTEXT: BRAZIL NUT CONCESSIONS IN MADRE DE DIOS, PERU

Peru has the fourth largest extent of tropical forests worldwide; over 60% of its territory is forested (Keenan et al., 2015). Despite the amount of natural forests in the country, economic revenues from formal forestry activities reach 1.1% of its Gross Domestic Product (GDP) (Che Piu and Menton, 2013)[2]. During 2001–2020, on average over 131,000 ha of forestlands were annually converted, or 3.7% of Peru's Amazonian rainforest (MINAM, 2017). Deforestation and forest degradation were mainly driven by shifting agriculture, cattle ranching, artisanal mining, commercial and illegal crops, and logging. It is estimated that around half of GHG emissions nationwide comes from deforestation and forest degradation, which in 2012 represented 86,742 $MtCO_{2e}$ released to the atmosphere (MINAM, 2016).

The Department of Madre de Dios (MdD), located at southeastern of Peru in the Amazon region (**Figure 1**), has an area of 85,301 km^2. It harbors Peru's lowest population density−1.65 inhabitants per km^2 (INEI, 2018)—alongside vast tropical forests of global priority for biodiversity (Myers et al., 2000; Catenazzi et al., 2013) and carbon stocks (Asner et al., 2010). However, construction of the Inter-Oceanic Highway (IOH) has facilitated access to the remote interior (Naughton-Treves, 2004; Perz et al., 2008; Chávez Michaelsen et al., 2013) during the last two decades. Hence, land clearing for crops (e.g., papaya, cocoa) and cattle ranching, illegal logging, and notably, informal artisanal gold mining have become important drivers of deforestation and forest degradation (Finer et al., 2017; Nicolau et al., 2019)[3].

Furthermore, extractivism is also an important economic activity in MdD. Collected from the *Bertholletia excelsa* tree, Brazil nut (or *castaña,* as it is widely known in MdD) is a prominent non-timber forest product (NTFP) with well-established markets worldwide. Some 15% of MdD's population

[2]In Chile and Ecuador, two of the leading countries for the sector in the region, forestry revenues amounted to 3.3% and 2.3% of their GDP in 2011, respectively (FAO, 2014).

[3]According to official data (PNCBMCC, 2017), during 2017 Madre de Dios reached a historic deforestation peak with 23,669 ha of cleared lands (15.18% of the total nationwide), surpassed only by Ucayali in that year.

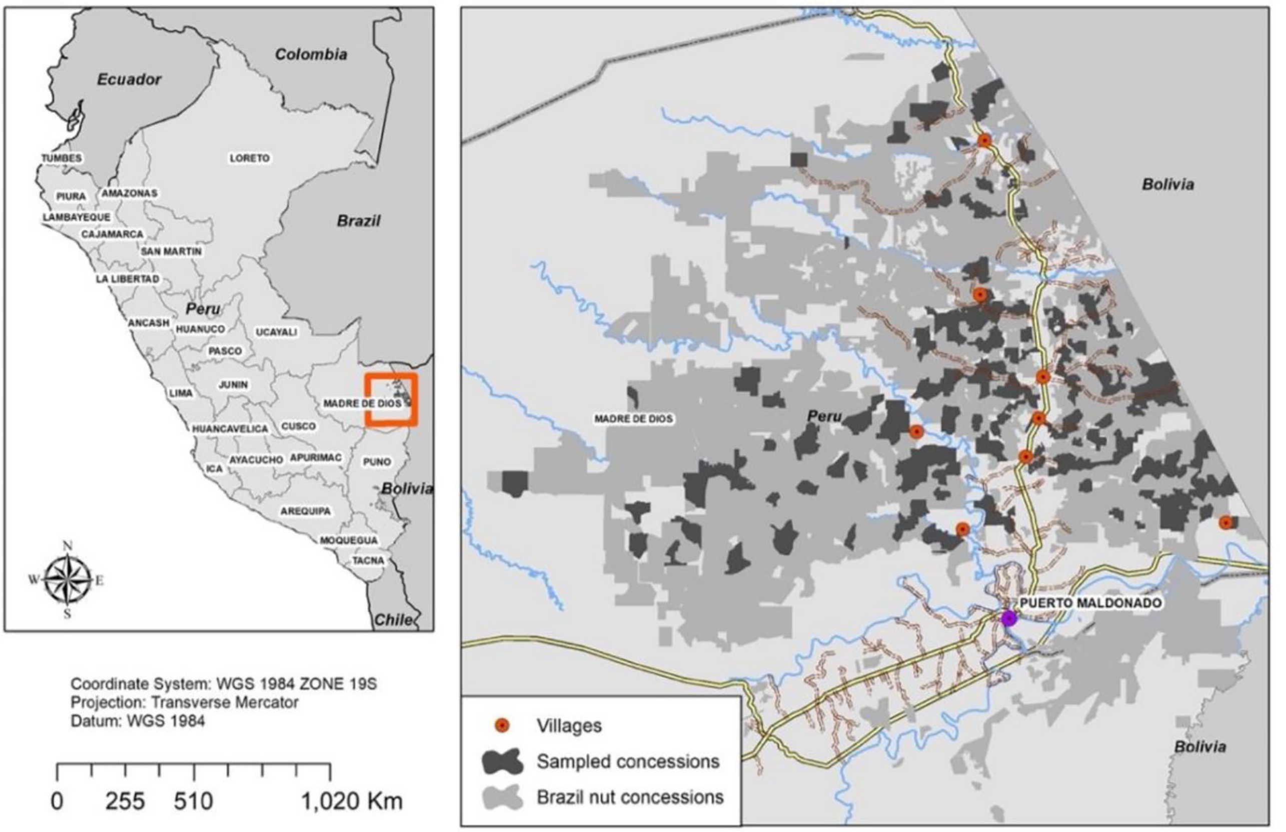

FIGURE 1 | Location of Madre de Dios and Brazil nut concessions.

is directly involved in its production chain (Escobal and Aldana, 2003; Cossío-Solano et al., 2011). Brazil nuts generate local employment and incomes among the forest-reliant people, while adding value to the mature tropical forests. This has raised enthusiasm from observers looking for "conservation-through-use" solutions (Duchelle et al., 2012; Nunes et al., 2012; Guariguata et al., 2017).

To attain the sustainable forest management, as underlying the 2011 Forestry and Wildlife Law (Law No. 29763), since 2002 the State has concessioned over 1 M ha of forest to MdD Brazil nut harvesters (Willem et al., 2019), thus granting usufruct rights to concessionaries for 40 years through renewable contracts. Originally, concessions were limited to Brazil nut extraction, although it was quite common for concessionaires to complement such activity with logging of large timber volumes; however, afterward only small-scale timber extraction (<5 m^3/ha during 40-year rotation cycles) was allowed in these areas (Cossío-Solano et al., 2011). The Organism for Supervision of Forestry Resources and Wildlife (OSINFOR), an autonomous office attached since 2008 to the Presidency of the Council of Ministers (Peru, 2008), is in charge of monitoring and enforcing the lawful extraction of NTFPs and timber, and is thus the key public organ implementing environmental command-and-control policies. Concessionaries need to pre-declare planned extraction volumes of Brazil nut and timber to the MdD Regional Government, and pay corresponding fees to obtain permits that are monitored by OSINFOR through field inspections, penalizing unauthorized forest clearance and timber over-extraction (OSINFOR, 2018)[4]. Sanctions imposed *vis-à-vis* cases of detected non-compliance range from fines to reversion of concessionaires' granted rights and forced forest restoration (OSINFOR, 2016). Moreover, OSINFOR also promotes legal activities of sustainable use, and sometimes, these could actually imply further deforestation (Brandt et al., 2016). Nevertheless, we hypothesize that in-field public law enforcement did induce concessionaires to clear and degrade less forest within the concessions they manage, as they would have perceived higher risks of being detected and sanctioned than without field enforcement.

Furthermore, in 2009, the private company Bosques Amazónicos S.A.C. (BAM), in partnership with the Federation of Brazil nut producers of MdD ("Federation"), launched a REDD+ project within Brazil nuts concessions of its partners, having as objectives to conserve forests while promoting local livelihoods (Garrish et al., 2014). In that year, through an agreement with

[4]The number of OSINFOR inspections on Brazil nut concessions sampled for our study by year is shown in **Supplementary Material S1**.

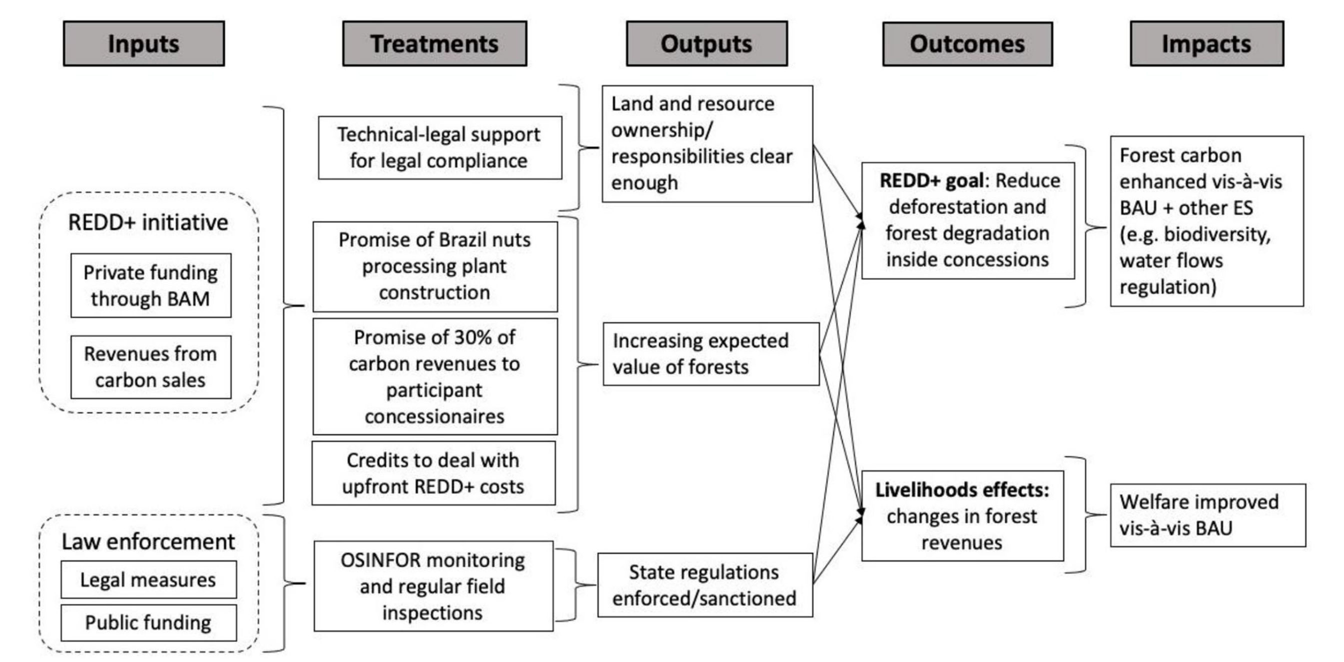

FIGURE 2 | Theory of Change of Madre de Dios REDD+ with simultaneous law enforcement. Own elaboration based on implementer interviews and project documents.

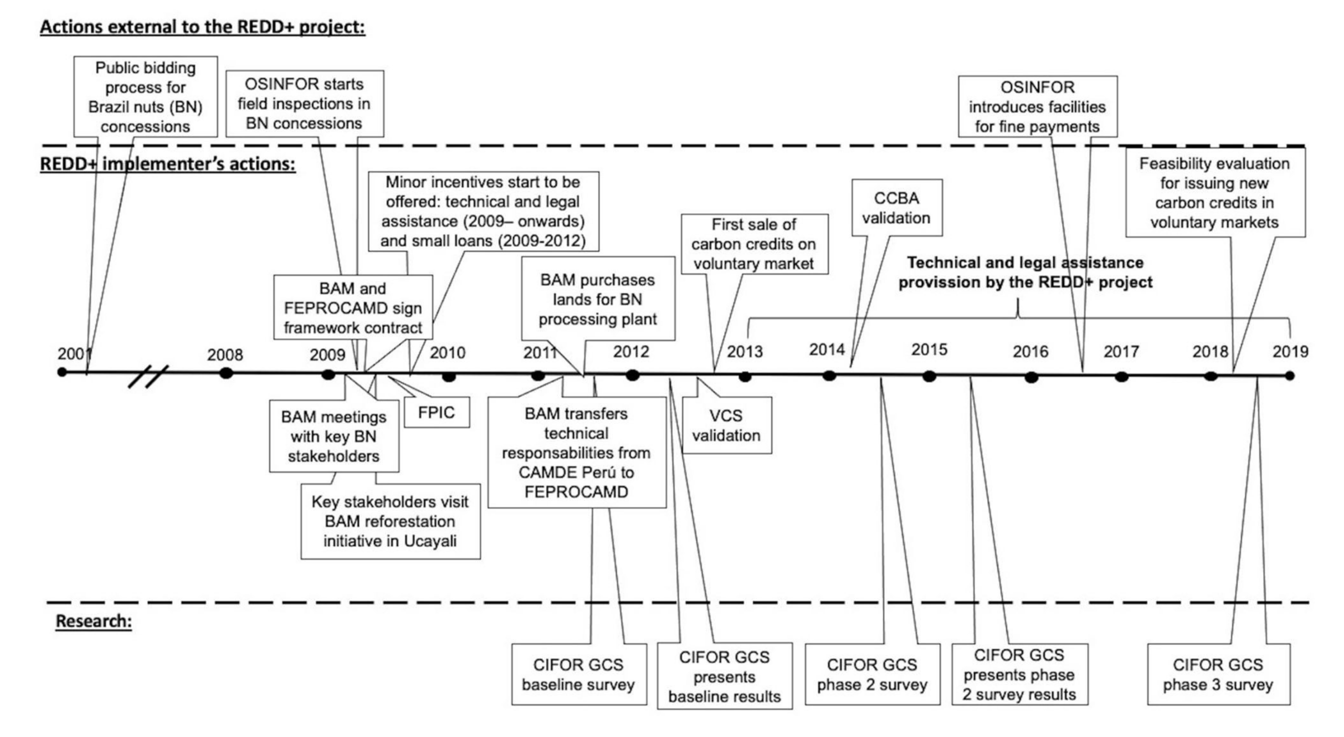

FIGURE 3 | Timeline of Madre de Dios REDD+ initiative and related conservation efforts. Garrish et al. (2014), OSINFOR (2018), and own compilation.

BAM, the "Federation" ceded its carbon commercialization rights to BAM. To access carbon markets, the project was certified using the Verified Carbon Standard in 2012, and the Climate, Community and Biodiversity Alliance standard in 2014. The Federation served as intermediary between participating Brazil nut concessionaires and BAM, the official REDD+

proponent (Garrish et al., 2014). Through contracts signed with the Federation, participant concessionaires committed to "reduce the deforestation in the forest of Brazil nuts concessions in Madre de Dios, and protect the existing biodiversity" in their concessions for 31 years—for carbon monitoring and accountability matters, starting retrospectively from 01 January 2010 (BAM, 2014). In exchange, concessionaires were promised the following benefits: 1) A new local Brazil nut processing plant; 2) 30% of the carbon credit revenues from voluntary markets; 3) credit access to cover upfront REDD+ costs; and 4) legal and technical assistance with land-tenure paperwork and harvesting permits (Garrish et al., 2014).

However, REDD+ implementation notably did not proceed as planned, because the sales of carbon credits lagged behind. As of February 2022, construction of the Brazil-nut processing plant had still not been initiated, and the benefit sharing of carbon revenues was still under discussion. So far, only the following two on-the-ground benefits were actually delivered to REDD+ participants: Legal–technical assistance with harvesting permits and small upfront loans. During our survey evaluation period, i.e., between years 2012 and 2018, no direct cash payments from selling carbon credit, as stipulated in the REDD+ contracts, had yet been delivered to participant concessionaires. With the elements described in this section, we developed a Theory of Change (ToC) (**Figure 2**), capturing the rationales behind the interventions, which is also underlying our impact evaluation below. However, in doing so, we need to cope with the strong gap between "intended" and *de facto* interventions, given that REDD+ treatment intensity lagged the initial expectations severely.

As for the REDD+ initiative, the delayed rollout is also visible in the implementation timeline (**Figure 3**). For context, we also included here some external, potentially influential events (e.g., *vis-à-vis* law enforcement), as well as the timing of the accompanying research efforts.

We thus aim to answer the following four research questions:

1. What impacts had the REDD+ project on deforestation and forest degradation in MdD Brazil nut concessions during the analyzed period (2012–2018)?
2. What were the corresponding REDD+ impacts on the participant households' wellbeing?
3. What were the effects of OSINFOR field inspections on forest conservation and wellbeing, respectively?
4. Which interaction effects, if any, between REDD+ and forest law enforcement could be found?

METHODS

Data Sources and Sampling

Our main source of socioeconomic and demographic information was the Global Comparative Study on REDD+ (GCS–REDD+) database, built by the Center for International Forestry Research (CIFOR), using a before-after-control-intervention (BACI) approach (Sills et al., 2017). CIFOR's GCS database comprises specific datasets that include household-level data collected from the study area in the following three different time periods: 2011–2012, 2014, and 2018. The first author of this article (Montoya-Zumaeta) coordinated the most recent survey collection, carried out from August to November 2018. This was executed following the ethics protocols approved by the Human Research Ethics Committee of the Australian National University (Human Ethics Protocol 438/2018), which included the explicitly documented free prior informed consent of all participants and measures to maintain the confidentiality of provided information.

TABLE 1 | Cartographic data.

Data	Format	Period	Resolution	Source
Humid forest loss in Peru's Amazon region	Raster	2001–2018	30 m	MINAM, 2017
Forest degradation	Raster	2005–2011, 2012–2018	30 m	Own estimation, based on Langner et al. (2018)
Road map, including principal and secondary roads	Vector	2019	n.a.	MTC, 2019
Digital elevation map and slope	Raster	2018	90 m	CGIAR-CSI, 2018
Villages and populated centers	Vector	2009	n.a.	INEI, 2009
Boundaries of Brazil nut concessions	Vector	2012	n.a.	SERFOR, 2017

We built the dataset used for this analysis with pre-treatment socioeconomic data gathered between October 2011 and January 2012, prior to VCS certification in June 2012, and 2018 data. Prior to the first round of data collection in 2011, BAM was asked to list villages with forthcoming REDD+ activities, among which CIFOR selected four for closer "treatment" scrutiny (**Figure 1**). Then, using secondary data and statistical matching techniques, another similar four villages located in the Tambopata province where the initiative is being implemented were selected to serve as controls in respective to their comparable market integration, deforestation trends, and socioeconomic features (Sunderlin et al., 2016). In each village, at least 30 concessionaire households were randomly selected and surveyed. We complemented the dataset with publicly available information about OSINFOR inspections (OSINFOR, n.d.).

Spatial Data and Remote Sensing

For statistical analyses, we also included concession-level spatial variables (parcel size, land-cover changes, distances to village centers, markets, rivers, roads, etc.), estimated using remote-sensing tools based on cartographic information from multiple sources (**Table 1**).

Boundaries at village, district, province, and departmental levels are from the Statistics and Informatics National Institute (INEI, 2009); nationwide roads data is from the Ministry of Transportation and Communications (MTC, 2019). Data from NFCP's Geobosques platform (MINAM, 2017) was used to estimate deforestation at concession-level, using 30-m resolution Landsat imagery in the following two periods: 2005–2011 (prior to REDD+ project implementation) and 2012–2018 (REDD+ implementation period). To estimate concession-level degraded forest areas, we followed Langner et al. (2018) in using a Google Earth Engine script. This method allowed us to calculate estimates of forest degradation in selected periods, rather than disturbance just at one particular time point as evaluated previously in Peru (Miranda et al., 2016; Blackman et al., 2017). Both deforestation and forest degradation outcomes were thus consistently estimated from the same Landsat satellite source. Concession boundaries were obtained using the National Forestry Service (SERFOR)'s GeoSerfor platform (SERFOR, 2017). For the distance estimations, through the CGIAR–CSI GeoPortal (CGIAR-CSI, 2018) we accessed slope and digital elevations from the Shuttle Radar Topography Mission model (version 4.1), which provides accurate information for tropical zones. All cartographic information was centralized and processed with the software QGIS 3.6.3.

We set 2012 as the starting year of REDD+ implementation, corresponding to when the project issued and sold its first generated carbon credits in the voluntary market (**Figure 2**), boosting the participant expectations though not resulting in full and immediate on-the-ground project implementation. We complemented the MdD REDD+ GCS dataset with a BACI design, with spatial variables estimated for both before and after the initiative, which was launched in 2012. To calculate deforested and degraded forest areas, we used geospatial data corresponding to the start and final years of the period, estimating land-cover changes per concession. Although forest cover in concessions predominantly decreased over time (denoted by a negative sign), our approach allowed us to incorporate positive changes also (i.e., forest regrowth) in our evaluated outcomes. Taking such consideration in mind, it is important to note that estimating a positive forest-cover effect should be interpreted as a reduction in net deforested or degraded forest area.

Empirical Strategy

Our main empirical challenge is that REDD+ participation was not randomly allocated. Hence, simple outcome comparisons between the participant and the non-participant observations might be biased. For instance, participation in the REDD+ project would likely have been disproportionally higher among the remotely located precompliant concessions, i.e., with the already lower deforestation and timber extraction levels. Hence, we need to control for this potential self-selection bias, as it could point toward risks of underlying endogeneity between treatment and outcomes (cf. subsection below for discussion).

Matched Difference-in-Difference

We used matching to control for potential self-selection bias in the REDD+ project to be able to estimate reliably its medium-term effects. We took advantage of the BACI design with socioeconomic survey data, allowing us to build a dataset containing information from 197 households who voluntarily participated in the GCS–REDD+ first and third phases. We complemented this dataset with geographic and biophysical features from the Brazil nut concessions they manage. Due to the legal restrictions, each household is allowed to manage only one concession. Location of Brazil nut concessions included in our dataset are shown in **Figure 1**. To identify which concessions and corresponding managing households were REDD+ treated, we used the original list of the participants who were formally enrolled into the initiative, and therefore were prioritized to receive the incentives listed in Section The Context: Brazil Nut Concessions in Madre de Dios, Peru[5]. Likewise, we included a dichotomous variable to identify households whose concession was at least once field-inspected by OSINFOR during the 2012–2018 period, based on information collected from its website (OSINFOR, n.d.).

Using this approach, we aim to estimate the average treatment effect on the treated (ATT) of the MdD REDD+ project, for land-use and wellbeing outcomes, respectively. We used the following difference-in-difference (DiD) estimator (Solis et al., 2021):

$$ATT = E\left[\left(Y_{1i}^{p=1} - Y_{1i}^{p=0} \middle| D = 1\right)\right] - E\left[\left(Y_{0i}^{p=1} - Y_{oi}^{p=0} \middle| D = 1\right)\right] \ldots \quad (1)$$

where Y_{1i} represents the outcome for the household i if it participates in the intervention, while Y_{0i} denotes the outcome for same household if it would not participate. The p in the superscript represents time period (0 before REDD+ implementation, and 1 after the initiative started). Given the dichotomy of households either participating or not, we cannot *a priori* estimate the second term of Equation (1). Therefore, we need to identify an adequate control group, replicating treatment group characteristics with the sole difference of not participating in REDD+. Then, we can estimate the REDD+ participation effect by comparing the original treatment to the control group. This procedure allows us to construct a "counterfactual," i.e., a reference scenario reflecting what would have happened had the evaluated initiative not been implemented (Ferraro, 2009).

The validity of the counterfactual scenario relies on the assumption that the potential outcomes for the control and treatment groups follow parallel trends; thus, indicating that any selection bias is to remain constant over time. The assumption can be represented as follows:

$$E\left[\left(Y_{0i}^{p=1} - Y_{0i}^{p=0} \middle| D = 1\right)\right] = E\left[\left(Y_{0i}^{p=1} - Y_{0i}^{p=0} \middle| D = 0\right)\right] \ldots \quad (2)$$

Assuming the condition holds, we can replace Equation (2) with Equation (1), obtaining Equation (3):

$$ATT = E\left[\left(Y_{1i}^{p=1} - Y_{1i}^{p=0} \middle| D = 1\right)\right] - E\left[\left(Y_{0i}^{p=1} - Y_{oi}^{p=0} \middle| D = 0\right)\right] \ldots \quad (3)$$

[5]In the pre-treatment period (2009-12), some training and loans were offered in some villages (specifically the four of alleged investments focus), prior to selling carbon credits (cf. **Figure 3**). However, not all concessionaires in these villages eventually signed legal REDD+ agreements. Furthermore, few concessionaires from the other sampled four villages signed these agreements, and hence were considered as treated in this analysis.

This simple DiD estimator contributes to mitigate the selection bias due to time-invariant non-observable variables, yet some baseline differences between the treated and control groups may still significantly influence outcomes. To control for these, we estimate the DiD estimator on a matched sample with similar distribution of group characteristics at baseline. The matching procedure, explained in more detail below, was implemented to make the parallel trends assumption more plausible, allowing us to construct a robust control group that is similar to the treatment group, and hence estimate the ATT using the following equation:

$$ATT = E\left[\left(Y_{1i}^{p=1} - Y_{1i}^{p=0}\middle|X,\, D=1\right)\right] - E\left[\left(Y_{0i}^{p=1} - Y_{oi}^{p=0}\middle|X, D=0\right)\right]\ldots \quad (4)$$

where X represents a vector containing variables representing observed characteristics measured at baseline. These were selected due to their expected influence on both the treatment variable and the evaluated outcomes (Ferraro and Hanauer, 2014). Following the ToC shown in the **Figure 2**, we expect the MdD REDD+ initiative to have had effects on the following outcomes:

1. Concession-level avoided deforestation;
2. Concession-level avoided forest degradation;
3. Household environmental income (timber and Brazil nut revenues); and
4. Self-reported changes in wellbeing[6].

As mentioned in Section Spatial data and remote sensing, using the available geospatial data, we were able to measure the first two outcomes listed. We estimated REDD+ effects on the difference between land-cover change, comparing pre-treatment (2005–2011) to MdD REDD+ treatment (2012–2018) periods, for both treatment and control groups. Following Miteva et al. (2015), this can be conceived as a triple-difference procedure, as the estimated effects indicate the relative change of outcomes between periods larger than a year. The main advantage that this estimation offers is that while controlling for observable characteristics, it not only considers systematically different time-invariant covariates but also allows to incorporate unobserved time trends (Ravallion, 2007; Miteva et al., 2015). **Figure 4** shows that *a priori* concession-level land-cover outcomes trend in both REDD+ treated and control groups quite similarly. For wellbeing outcomes, we used a more standarized DiD procedure that allows us to estimate REDD+ effects on outcomes measured pre-implementation (2012) and during it (2018).

We apply propensity score matching as a pre-processing technique to reduce initial significant differences between the treated and the non-treated groups—in contrast to the randomized control trials in which the characteristics of treated and untreated units are expectedly similar (Ho et al., 2007). Through this matching procedure, we aim to mimic randomization in assigning the treatment by reweighting the observations' probability to be treated based on their observable characteristics (Ferraro and Hanauer, 2014). In addition to the above-mentioned village-level matching procedure that was performed to identify control villages within the frame of GCS–REDD+ (Section Data sources and sampling), we also perform Kernel-based Propensity Score (K-PS) matching with 0.05 caliper algorithm to match the REDD+ participant households with households in the control group, using the Stata command *psmatch2* (Leuven and Sianesi, 2003). This procedure also allowed us to statistically test differences in outcome means between the treatment and the control groups, producing preliminary estimates of intervention effects[7]. To check for acceptable post-matching balance, we used as criterium that standardized percentage differences between groups (% $St.\ Dif. = \frac{X_T - X_c}{\sqrt{\frac{(S_T^2 + S_C^2)}{2}}}$, where T and C represent treated and non-treated groups, respectively) at each covariate should not exceed a 25% threshold (Stuart, 2010).

To perform this household-level matching procedure, we used 19 covariates measured at baseline, counting eight geographic and land dynamics concession-level characteristics (area, distances to village, river, secondary roads, and to the Inter-Oceanic Highway (IOH); baseline non-disturbed forest, and average annual deforestation and forest degradation in the 2005–2011 pre-treatment period); 10 household-level socioeconomic baseline variables (members number; assets value; head's age and education years; agriculture, livestock, environmental, and other incomes; and two discrete variables reflecting households' perceived wellbeing answers before the REDD+ project was implemented). Finally, we also included as matching covariate a dichotomous variable taking the value of one if OSINFOR field-inspected the concession previous to implementing the REDD+ project, and zero otherwise.

These covariates were identified and selected based on an existing impact evaluation literature (e.g., Arriagada et al., 2012; Jones et al., 2016; Simonet et al., 2018a), and the semi-structured field interviews conducted by the first author in 2018. Post-matching covariate balance and standardized percentage differences between groups are presented in columns 3 and 4 of **Table 2**, respectively. We check robustness of post-matching mean difference estimates by calculating the associated Rosenbaum bound (Γ) for the cases in which a statistically significant difference of at least 10% was found. This parameter is frequently used in observational studies to assess the unobserved heterogeneity that could potentially undermine the statistical significance of the evaluated treatment (Rosenbaum, 2005).

Once matching was performed, we estimated the ATT of the REDD+ initiative through the DiD approach by performing post-matching multivariable regressions using same matching covariates and incorporating propensity score of observations as inverse weights (Hirano et al., 2003; Austin, 2011)[8]. Such a procedure is recommended to remove any remaining bias in

[6]Households responding to the question: "Overall, how is the wellbeing of your household today, compared with the situation two years ago?" (Possible answers are "improved," "worsened," or "the same").

[7]*Vis-à-vis* concerns about the precision of the *psmatch2* command to estimate effects from the REDD+ initiative, we estimate similar mean comparison results using the command *teffects psmatch*. This technique produces more reliable standard errors (Appendix S5.2).

[8]To estimate inverse weights used in post-matching regressions we use the following formula (Hirano et al., 2003): $w_i = D_i + (1 - D_i)\frac{\hat{e}_i}{1-\hat{e}_i}$, where is w is the estimated weight for observation i, D denotes whether the observation is treated ($D = 1$) or non-treated ($D = 0$), and $\hat{e}$ the estimated propensity score.

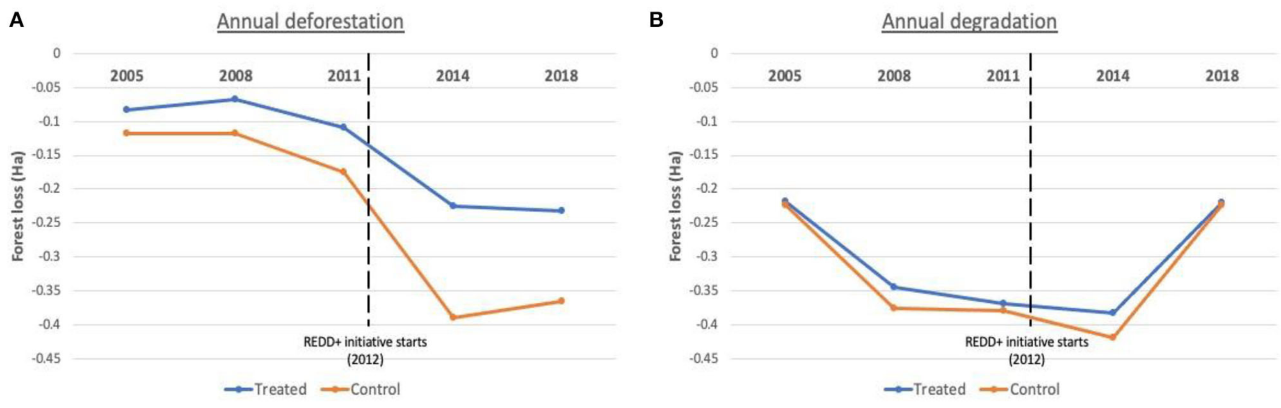

FIGURE 4 | Trends in land-cover related outcomes at concession-level by groups. **(A)** Corresponds to concession-level average annual deforestation in control and treated groups, and **(B)** to concession-level average annual degradation area in both groups.

observational studies (Rubin, 1979; Jones and Lewis, 2015). Our post-matching regressions took the following form:

$$Y_i = \alpha + \delta D_i + \varphi O_i + \zeta I_i + \phi X_i + \varepsilon \ldots. \quad (5)$$

where Y is the evaluated outcome in observation i; D represents whether or not the observation was REDD+ treated; δ can be interpreted as the bias-adjusted matched DiD estimator of ATT of participating in REDD+; O represents whether or not the observation was inspected by OSINFOR during the evaluated period; I is a variable representing the interaction between the REDD+ and the OSINFOR policies; X is the same vector of covariates used for matching; φ, ζ, *and* ϕ are other parameters of interest to be estimated through the regression, and ε the stochastic term. We estimate Equation (5) using linear specifications for the case of continuous dependent variables, and a multinominal logit regression to model households' answers *vis-à-vis* self-reported wellbeing. To address the concerns on the potential endogeneity issue between the OSINFOR inspections variable and the deforestation outcome, we implement the two-stage endogenous model proposed by Heckman (1978). The description of this approach and results from implementing it as part of our empirical strategy are included in the (**Supplementary Appendix S5.1**).

Note that the econometric specification used to estimate our main results does not consider village-level autocorrelation, since we found that its influence on evaluated outcomes is low, and statistically insignificant ($p > 0.1$ in all cases). We confirmed this by estimating the intra-cluster correlations for each response variable (**Supplementary Appendix S3**). This finding is not surprising given that concessions are relatively distant from each other, and the most concessionaires have migrated from the nearby regions. Furthermore, since villages are not officially delimited by government, it is unclear by which criteria REDD+ implementers assigned concessionaires to villages. Complementarily, we use a matching algorithm, a semi-parametric procedure, and the previously mentioned two-stages endogenous specification to confirm the robustness of our estimates (see **Supplementary Appendix S5**).

RESULTS

Post-matching Sample Balance

Column 2 of **Table 2** shows that prior to matching, the households in the REDD+ treated group were statistically different from those in the non-treated group in terms of household heads' age, livestock income, likelihood to have improved their perceived wellbeing in 2012, and propensity to pre-treatment OSINFOR inspections. Through the matching procedure, we were able to considerably control for initial selection biases before evaluating the impacts of REDD+ interventions using the matched DiD approach (Column 4, **Table 2**); none of the post-matching standardized mean differences of each covariate exceeds the conventional 25% threshold (Stuart, 2010). Notably, in most cases, matching allowed us to diminish considerably the pre-matching biases (see **Supplementary Appendix S2.1**).

Effects on Land-Cover Changes

The land-cover change estimates are shown in **Table 3**. First, we report the MdD REDD+ project's effects on concession-level, which avoided deforested and degraded areas for the period 2012–2018 using DiD estimators. By applying Equation (3), the simplest DiD estimator, that assumes no statistical intergroup differences at baseline, calculates that per concession REDD+ avoided during the period 2012–2018, 0.33 and 0.04 ha on average of deforestation and degradation, respectively; but any such estimates is statistically non-significant. Post-matching mean comparisons that operationalize Equation (4), and linear regressions ran on the matched sample whose form is represented by Equation (5), indicate that the REDD+ effects remain negligible after controlling for selection biases. Nevertheless, the post-matching regression detects that OSINFOR inspections would have

TABLE 2 | Summary statistics before and after matching.

Covariates	(1) All samples (n = 197)	(2) Pre-matching mean comparison[a]		(3) Post-matching mean comparison[a]		(4) Standardized difference (%)
		REDD+ treated (n = 102)	Control (n = 95)	REDD+ treated (n = 95)	Non-treated (n = 95)	
Outcomes:						
Concession-level avoided deforested area for 2012–2018 period (ha)	−1.278 (2.329)	−1.121 (1.896)	−1.447 (2.718)	-	-	-
Concession-level avoided forest degraded area for 2012–2018 period (ha)	−1.497 (1.105)	−1.478 (1.0566)	−1.5175 (1.594)	-	-	-
2018 HH environmental income (PEN)	−5 220 (38.884)	−2 688 (37.298)	−7 939 (40.538)	-	-	-
2018 perceived HH wellbeing (*vis-à-vis* 2 years ago)[b]						
Better	0.18 (0.38)	0.15 (0.35)	0.21 (0.41)	-	-	-
Worse	0.22 (0.41)	0.28** (0.45)	0.15 (0.36)	-	-	-
Covariates used for analyses:						
Distance to secondary road (km)	9.0577 (10.855)	9.235 (12.8)	8.867 (8.3423)	9.293	8.208	10.0
Distance to river (km)	9.8526 (5.5904)	9.987 (5.5)	9.709 (5.711)	10.015	10.171	−2.8
Distance to IOH (km)	15.578 (11.516)	15.625 (12.645)	15.527 (10.234)	15.77	14.86	7.9
Distance to village (km)	24.531 (16.033)	25.307 (19.405)	23.698 (11.404)	25.468	23.696	11.1
Concession area (ha)	778.97 (524.52)	779.61 (583.1)	778.29 (456.43)	782.18	772.75	1.8
Growth forest in 2012 (ha)	742.33 (510.96)	742.17 (570.35)	742.51 (441.42)	746.73	737.18	1.9
Accumulated deforested area, 2005–2011 (ha)	−0.8366 (1.377)	−0.6962 (0.9778)	−0.9873 (1.697)	−0.734	−0.682	3.8
Accumulated degraded forest, 2005–2011(ha)	−1.5425 (1.553)	−1.5606 (1.279)	−1.523 (1.8079)	−1.604	−1.61	−0.4
Baseline assets value (PEN)	24 066 (31 387)	25 402 (23 858)	22 632 (37 929)	22 736	21 759	3.1
Baseline HH members	4.14 (2.21)	4.26 (2.46)	4.02 (1.92)	4.1263	4.1941	−3.1
Baseline HH head age (years)	53.02 (12.30)	54.52* (10.97)	51.41 (13.46)	53.96	53.67	2.4
Baseline HH head education (years)	7.27 (3.68)	7.09 (3.44)	7.47 (3.93)	7.05	7.02	0.9
Baseline agricultural income (PEN)	2 669 (5 893)	2 223 (3 232)	3 148 (7 793)	2 197	2 079	2.0
Baseline livestock income (PEN)	7 863 (20 557)	4 710** (15 508)	11 249 (24 503)	4 647	5 981	−6.5
Baseline environmental income (PEN)	36 882 (53 653)	33 246 (33 719)	40 787 (68 927)	32 997	30 568	4.5
Other HH incomes at baseline (PEN)	10 491 (17 057)	9 491 (13 544)	11 564 (20 182)	9 552	8 692	5.0
OSINFOR insp. before 2012 (y/n)	0.25 (0.43)	0.33*** (0.47)	0.16 (0.37)	0.305	0.306	−0.1
Better self-declared HH wellbeing at 2012	0.47 (0.5)	0.52* (0.05)	0.41 (0.05)	0.505	0.492	2.7
Worse self-declared HH wellbeing at 2012	0.16 (0.37)	0.19 (0.39)	0.13 (0.34)	0.179	0.201	−6.0
OSINFOR inspections 2012–2018 (y/n) [c]	0.41 (0.49)	0.54 (0.5)	0.44 (0.5)	-	-	-
OSINFOR village-level intensity 2008–2011 (%)[d]	24.9 (15.09)	32.9*** (14.64)	16.26 (9.99)	-	-	-
Distance to Puerto Maldonado (km)[d]	57.76 (21.69)	63.98*** (24.9)	51.09 (15.1)	-	-	-

*HH – household; y/n – yes/no; PEN – Peruvian soles. Standard errors reported between parenthesis. [a]In columns 2 and 4, statistical differences between treated and non-treated groups in each case are represented using *for 10%, **for 5%, and ***for 1% significance. We used t-test for continuous and Chi-squared for categorical variables. [b]Responses from 188 households, being 96 of these REDD+ treated. [c]Used as regressor in post-matching regressions. [d]Covariate used only in the two-stage endogenous model (**Supplementary Appendix S5.1**).*

avoided 0.78 ha of deforestation over the evaluation period ($p < 0.05$). Robustness of this statistically significant OSINFOR impact on comcession-level deforestation was confirmed by using the alternative two-stage endogenous linear specification (cf. **Supplementary Appendix S5.1**); the estimated OSINFOR average effect here is 2.1 ha. ($p < 0.05$). The complete results from post-matching regressions models are presented in **Supplementary Appendix S4**.

In summary, we identify three relevant findings. First, participation in the REDD+ initiative alone achieved so far only negligible effects on both deforestation and forest degradation; estimated effects using matching procedures are not statistically different from nil. Second, we detect statistically significant effects from OSINFOR actions on avoiding concession-level deforestation, but not forest degradation. This brings the relevant insights about the role of government-led field inspections on disincentivizing concessionaires from extracting timber at larger scales where road construction and logging encampments would cause significant deforestation (OSINFOR, 2018). Finally, interaction terms in neither of our econometric approaches was

TABLE 3 | Singular and combined effects of the Madre de Dios REDD+ project and OSINFOR inspections on land-cover changes, 2012–2018.

Covariate	Outcomes (at concession-level)[a]	
	2012–2018 avoided deforested area (ha)	2012–2018 avoided forest degraded area (ha)
DiD estimators		
Simple DiD on unmatched sample[b]	0.326 (0.332)	0.0392 (0.158)
DiD on matched sample[c]	0.0249 (0.358)	0.0533 (0.189)
Rosenbaum bound (Γ)[d]	-	-
Marginal effects after post-matching regressions:[e]		
REDD+ participation – bias-adjusted matched DiD estimator	−0.33 (0.337)	0.0419 (0.133)
OSINFOR inspections	0.777** (0.306)	0.085 (0.184)
REDD+*OSINFOR interaction term	0.436 (0.394)	−0.009 (0.24)

***p < 0.05, *p < 0.1. Robust standard errors are reported between parenthesis. [a]A positive estimate sign means that avoided deforestation or forest degradation was achieved; negative signs indicate forest loss effects. [b]Statistical significance was estimated using t-test mean comparisons. Such estimations operationalize Equation (3). [c]Estimated using mean comparisons on matched sample. Estimations here operationalize Equation (4). [d]Rosenbaum bound reported only when significant statistical differences at 10% are estimated through post-matching mean comparison. [e]Estimations here operationalize Equation (5).*

statistically significant; the hoped-for positive interplay between both interventions could not be substantiated.

Wellbeing Effects

The wellbeing effects from both interventions are shown in **Table 4**. Using the simplest DiD estimator on the unmatched sample, we found that the households participating in REDD+ earned in 2018 similar environmental income (composed mainly of annual earnings from selling timber and Brazil nut) as the non-participants; the difference is statistically insignificant. Applying post-matching mean comparisons does not change the picture. Similarly, OSINFOR impacts, and the interaction variable with REDD+, are also both statistically insignificant; none of our treatments mattered for environmental income. In turn, our results suggest that the participation in the evaluated REDD+ project in MdD has negatively influenced the households' perceived wellbeing over the last 2 years. The results from our matched DiD estimators indicate that the REDD+ participants were between 13 and 16% more likely to report that their perceived wellbeing had worsened recently.

DISCUSSION AND CONCLUSION

We have evaluated the local impacts of an incipient REDD+ project involving Brazil nut harvesters in the Peruvian department of Madre de Dios (MdD), close to the borders with Brazil and Bolivia. REDD+ adds a "carrot" to an environmental policy mix already containing the "stick" of public law enforcement actions, applied at variable intensity over time. We found only negligible effects from the incipient REDD+ project actions. In turn, the intensified command-and-control efforts from the national agency OSINFOR significantly enhanced forest conservation by preventing deforesting land-cover changes in MdD Brazil nut concessions, although not reducing forest degradation. The traditional regulatory disincentive measures thus proved environmentally effective, avoiding a loss of around 170 ha of forest in the sampled concessions over 7 years (24 ha annually)[9]. On the recipient welfare side, all income effects from our treatments were insignificant, but REDD+ participants reported reduced subjectively wellbeing recently.

What should we make of these results? Allegedly, environmental command-and-control policies ("sticks") are worldwide currently not sufficiently effective. Supposedly, REDD+ should be sweetening the conservation deal for landholders, and achieve an additional conservation while providing the compensation for the losses suffered from forest law enforcement. There are several reasons why things have worked out differently in our case. Greater intensity in the regulatory efforts may have achieved what REDD+ has not (yet) managed to tangibly benefit forest conservation. In turn, REDD+ "carrot" effects have been restricted from operating effectively in at least three respects.

1. Low REDD+ intensity: First and foremost, the limited sale of credits on voluntary carbon markets seriously reduced funding for implementing REDD+ incentives; carbon revenues over the first 10 project years only reached one-third of the projected earnings[10]. The treatment intensity thus remained very low; the promised Brazil nut processing plant was not constructed, and delivery of cash from carbon sales to REDD+ contract signatories households has not yet happened. Conversely, REDD+ was also not a nil-treatment; legal–technical assistance and small upfront loans were provided, and concessionaires were contractually bound by the REDD+ obligations they signed up for (even though incentives lagged behind). Also, the land stewards often adjust their behavior in anticipation of the future treatments. However, since so far only a small subset of the promised suite of REDD+ actions has actually been implemented, the incentive-based treatment has barely been tested; truly, the REDD+ rubber has not yet hit the road.
2. Ill benefit alignment: Although at the beginning of the project legal–technical assistance and small-scale credits were actually delivered, these were not exclusively allocated to the participants enrolled in the initiative, but provided to all villagers *en bloc*. As an intended *ad hoc* strategy to increase the project acceptance by non-enrolled concessionaires, benefits were allowed to spill over to them. Thus far obviously, this

[9]Using the estimate of OSINFOR deterrent effect on concesion-level deforestation obtained through the two-stage endogenous regression (**Supplementary Appendix S5.1**). Annual deforestation rates in sampled inspected concessions was 0.013% *vs.* 0.033% in non-inspected concessions (62% less) during the evaluation period.

[10]Personal communication from BAM representatives, November 2020.

TABLE 4 | Singular and combined effects of the Madre de Dios REDD+ project and OSINFOR inspections on household wellbeing.

Covariate	Outcomes (at concession-level)		
	2018 household income (S/.)	2018 perceived wellbeing	
		Improved	Worsened
DiD estimators			
Simple DiD on unmatched sample[a]	−2,819 (7,458)	−0.061 (0.055)	0.129** (0.059)
DiD on matched sample[b]	3,108 (9,340)	−0.056 (0.064)	0.156** (0.066)
Rosenbaum bound (Γ)[c]	-	-	1.6
Marginal effects after post-matching regressions:[d]			
REDD+ participation – bias-adjusted matched DiD estimator	1,522 (6,144)	−0.083 (0.061)	0.127* (0.074)
OSINFOR inspections	−556 (8,280)	0.073 (0.061)	0.05 (0.077)
REDD+*OSINFOR (interaction term)	1,146 (11,743)	0.077 (0.081)	−0.183 (0.113)

***p < 0.05, *p < 0.1. Robust standard errors are reported between parenthesis. [a]Statistical significance was estimated using t-test and proportion mean comparisons for both income and perceived wellbeing outcomes. Estimations here operationalize Equation (3). [b]Estimated using mean comparisons on the matched sample. Estimations here operationalize Equation (4). [c]Rosenbaum bound reported only when significant statistical differences at 10% are estimated through post-matching mean comparison. [d]Both perceived wellbeing outcomes were estimated using a multinominal logit regression. Estimations here operationalize Equation (5).*

benefit dilution also diminished the credibility of the REDD+ contract's performance-based reward element.

3. Ignoring land-use non-compliance: Even for those benefits that truly came through to contract-signing households, the implementers (BAM and the Federation) did not implement incentives as truly conditional, i.e., delivering rewards only to those contracted the participants who had complied with their environmental commitments. The heavily delayed delivery of multiple REDD+ benefits had generated a strained relationship between the implementers and REDD+ participating concessionaires, thus making it difficult for the former to enforce a contract compliance based on their reduced basket of benefits. Notably, the non-sanctioning of contract incompliance is a common pitfall in the implementation of conditional incentives (Wunder et al., 2020b). Overall, the land-use restrictions were seldom monitored (beyond some certifier verifications visits), and environmental compliance was weakly enforced.

Counterintuitively, REDD+ recipients reported declines in their subjective wellbeing. However, since the main promised incentives have not yet been delivered, this is not surprising. Probably, the participants issued a "vote of anti-project protest," observed also elsewhere in Moyobamba, Peru (Montoya-Zumaeta et al., 2019), and in a carbon project in north-eastern Bolivia (Asquith et al., 2002). This reaction of deception may be applicable to many REDD+ projects worldwide, since less than 5% of carbon credits issued in voluntary markets have actually been sold (Simonet et al., 2018a).

Notably, the setting of our study is characterized by low baseline deforestation pressures; within Brazil nut concessions, pre-project (2005–2011) deforestation was at 0.032%. Intuitively and empirically, the positive quantitative conservation impacts are harder to detect when the pre-treatment pressures are already low (Börner et al., 2020). Enrolling large swaths of low-threatened forests can ultimately undermine the economic feasibility of conservation initiatives (Giudice and Börner, 2021). Hence, our mostly non-significant impacts should also be seen in this light.

Our law enforcement findings are in line with other research from the tropics confirming that intensified "sticks" constitute an, at least in isolation, effective anti-deforestation strategy. It is notably coherent with confirmatory evidence for similar government-led enforcement policies in Brazil (Cisneros et al., 2015; Assunção and Rocha, 2019; Koch et al., 2019). For instance, the field inspections helped to conserve 4.0 and 9.9 ha of forest in Mato Grosso and Pará, respectively (Börner et al., 2015a). For MdD, Anderson et al. (2019) had found that fines imposed on Brazil nuts concessionaires, used as a proxy of government enforcement, did not significantly reduce deforestation in the subsequent year 2011. However, this different finding from ours may occur because we evaluated a different treatment and longer period (7 years vs. 1 year).

We also found no significant interaction effects between REDD+ and OSINFOR enforcement. This may not only be due to the weak intensity of REDD+ treatments. Based on our satellite imagery, interviews, and some suggestive evidence from elsewhere [e.g., Pearson et al. (2014), Brandt et al. (2016)], we can argue that the technical–legal assistance to REDD+ recipients came to reduce transaction costs of their timber harvesting permits; thus, unintentionally subsidizing more timber harvesting. The hoped-for pro-conservation synergies of REDD+ incentives and OSINFOR disincentives were thus also jeopardized on that account.

Our results may also highlight some future avenues for how to more satisfactorily attain both environmental and socioeconomic objectives. First, given the current outlook in carbon markets, REDD+ projects of this type may need to look for more diversified funding sources to be able to "intensify treatment," delivering on their promises to local stakeholders. Second, best-practice design of conditional incentives (Engel, 2016; Wunder et al., 2018; McWherter et al., 2022) might also make the MdD REDD+ project more effective. Beyond carbon certifier audits, this refers to adopting a timely monitoring system into the frame of the REDD+ initiative, e.g., based on low-cost remote-sensing technologies (Blackman, 2013). It could help linking up with broader initiatives with similar environmental objectives, led by government or non-governmental actors. Third, spatial targeting measures can assist in better addressing the local heterogeneity of deforestation risk, using proxies to focus actions more on those forest areas that are at greatest risk (Alix-Garcia et al., 2008; Wünscher et al., 2008).

Finally, although we found that the MdD REDD+ initiative has so far not been effective in attributably reducing deforestation and forest degradation, its implementation brings still relevant lessons about forest conservation initiatives under similar contexts in Peru and elsewhere. Beyond a general proof of the REDD+ concept in the Peruvian Amazon, compiling robust evidence and constructing a credible counterfactual is essential for elucidating to what extent global efforts to mitigate climate change are making progress on the ground.

DATA AVAILABILITY STATEMENT

The raw data supporting the conclusions of this article will be made available by the authors, without undue reservation.

ETHICS STATEMENT

The studies involving human participants were reviewed and approved by Human Research Ethics Committee - Australian National University. The patients/participants provided their written informed consent to participate in this study.

AUTHOR CONTRIBUTIONS

JMZ, SW, and AD designed the study. JMZ collected information and wrote the original manuscript. JMZ and ER analyzed collected data. SW and AD supplemented original manuscript. All authors contributed to the article and approved the submitted version.

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SUPPLEMENTARY MATERIAL

The Supplementary Material for this article can be found online at: https://www.frontiersin.org/articles/10.3389/ffgc.2022.870450/full#supplementary-material

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