ABSTRACT

DAVID BRAULIO SOLIS CHAVEZ. Quasi-experimental impact evaluation: applications to timber concessions and REDD+. (Under the direction of Erin O. Sills)

Conservation of tropical forests is a concern to both national governments and the international community, because the ecosystem services provided by those forests benefit society at multiple scales from local to global. The laws of many tropical countries protect forests, but the lack of enforcement and incentives has meant that those laws are widely ignored. In this dissertation, I examine two potential remedies: (i) on-the-ground inspections to verify compliance with the law (essay 1), and (ii) conditional incentives for reducing forest carbon emissions, or REDD+ (essays 2 and 3).

Illegal logging is a major concern throughout the tropics and is challenging to control in vast, remote, and effectively open-access tropical forests. Many governments have therefore focused enforcement efforts on the transport of logs, which in turn creates a new black market for the required transport permits. In many countries, timber harvest (and therefore transport) is authorized primarily through a system of concessions. In Peru, the government issued timber concessions in over 762,222 square kilometers of tropical forest in 2003 and 2004.

In the first essay of this dissertation, I assess the incidence and seek to understand the economic rationale for violations of national forestry law in these concessions, including its relation to the black market in permits for transport of timber. Using field reports from inspections of the concessions from 2009 to 2014, I characterize the violations, quantify their extent, develop a conceptual framework that predicts where they are most likely to occur, estimate a probability model of which concessions are breaking and which are abiding by the rules, and assess whether inspections effectively reduce future violations and therefore the supply of transport permits to the black market. I implement a single difference strategy to estimate the Average Treatment Effect on the Treated (ATET), i.e. the average effect of inspections on mis-reporting of harvested timber. Using nearest neighbor and kernel matching with propensity scores, I find that field inspection of a concession reduced future

misreported timber harvest by 1,874 cubic meters (e.g. equivalent to 89% of the average amount of timber mis-reported by a timber concession in the year of the study). This implies an equivalent reduction in black-market permits for transport of timber. These results are statistically significant and robust.

Over the past decade, numerous sub-national REDD+ projects have been implemented in order to demonstrate, test, and start generating carbon credits for an international REDD+ system. Evaluating the impacts of these projects is challenging, both because they are non-randomly allocated on the landscape and across households and because of the short time-period in which to observe impacts, but possible with the right design and methods. While in many cases it may be too early to observe concrete effects in terms of changing patterns of deforestation and forest degradation, we should be able to observe whether projects are effectively creating incentives to reduce deforestation and degradation. In the second essay in this dissertation, I use multiple approaches to evaluate whether two sub-national REDD+ projects in Peru have significantly changed annual forest income for participating families. The data are from CIFOR's Global Comparative Study of REDD+, including panel data from household surveys in 2010-11 and 2013-14. Using a quasi-experimental approach (Difference in Difference with Propensity Score Matching), I find no evidence that REDD+ initiatives in Peru have affected the forest income of households in the intervention area or of households that participated in specific REDD+ interventions.

One specific reason for piloting REDD+ was to learn about its impacts on disadvantaged groups, including women. Clearly the women who are part of households in the intervention area are affected by impacts (or lack therefore) on household income. However, women may consider other types of impacts more important for their well-being. Drawing on focus groups with women conducted by CIFOR's Global Comparative Study of REDD+, I demonstrate the importance of gender-responsive analysis in my third essay in this dissertation. Comparative research on wellbeing was conducted in 62 villages participating in 22 REDD+ initiatives in 6 countries and 61 control villages selected by pre-matching on village characteristics. Focus groups with villagers (68% male) and women (100% female) permit a gendered comparison of definitions of wellbeing and outcomes of initiatives.

Definitions of well-being overlapped between the two groups, but almost half of the women's focus groups thought having their own source of income was important for women's wellbeing. Women perceive they have not fared as well as villagers as a whole perceive that they have fared, during the first years of REDD+ implementation. DiD and regression analysis both point to net detrimental impacts of REDD+ interventions on women's wellbeing, but no significant impact on village wellbeing as a whole. Comparative analysis can help identify vulnerabilities of such initiatives and potential ways forward.

Quasi-experimental impact evaluation: applications to timber concessions and REDD+.

by

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DEDICATION

To the woman who helped me to find my source of confidence and energy, my mother.

BIOGRAPHY

David Solis was born in the Huanuco region of Peru in 1978. He earned a bachelor's degree in economics at the Universidad Nacional Hermilio Valdizan in Peru. He started his professional career in 2003 as a Research Assistant at GRADE (a Peruvian think tank), where he developed his skills as a development and environmental researcher. In 2005, he earned a master's degree in economics from the Universidad de Chile.

From 2006 to 2008, he worked at the Canadian International Development Agency in the PERCAN project, during which time he co-published a guide to elaborate baseline study for informal mining. He also worked for two years in the Alternative Development Program founded by the U.S. International Cooperation Agency (USAID), assessing the impact of productive agricultural projects like cacao, coffee and palm for oil on household incomes in rural communities in the Peruvian Amazon. In 2011, he worked for the Environmental Ministry's National Conservation Forest Program. He was involved in designing the impact evaluation of economic incentives (cash transfers) used to promote forest conservation in indigenous communities that voluntarily decide to preserve forests in their territories.

In 2012, he joined the Economics Graduate Program at NCSU and started working with optimization and dynamic programming models for environmental resources. He earned a Master of Economics degree in 2014. He then began the PhD program in the Department of Forestry & Environmental Resources at NCSU. In 2016, he was recognized in the XVI annual research contest promoted by the Social and Economic Research Consortium (CIES) for research related to the impact of field inspections on compliance with Peruvian forestry laws.

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I also want to thank Dr. Cronkleton from the Center for International Forestry Research (CIFOR), who helped me to precisely define the level of analysis in the impact evaluation of REDD+ initiatives in Peru. Dr. Cronkleton is my co-author in the second chapter. Likewise, I am very grateful to Dr. Larson (from CIFOR), who helped me navigate softly in the gender topic. Dr. Larson is my co-author in the third chapter. Moreover, I am indebted to my committee members—Dr. Abt, Dr. Cubbage, and Dr. Renkow—for relevant comments that helped me to improve this research.

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INTRODUCTION

My dissertation includes three chapters about timber concessions in Peru, REDD+ in Peru, and REDD+ initiatives across the tropics.

In the first two chapters, I employ quasi-experimental methods to estimate the impacts of field inspections and REDD+ initiatives. In the first chapter, I assess the impact of field inspections on compliance with Peruvian forestry law using the single difference method with propensity score matching (PSM). In the second chapter, I assess the impact of REDD+ initiatives on forest-related household income in Peru using the difference in difference (DiD) method with PSM. In these chapters, I explore counterfactual thinking and the application of the quasi-experimental framework to evaluate interventions, or "treatments," when selection into treatment is not random. Further, I consider how to assess the ability of these methods to replicate randomization conditions, or to control for confounders, thereby obtaining robust estimates of treatment effects. In both cases, I pre-process the sample using PSM, demonstrating the role of matching in building a robust control group to construct the counterfactual.

The third chapter analyzes women's perceptions of wellbeing change in the context of REDD+ implementation using data from 123 villages in 23 REDD+ initiatives in six countries (Brazil, Peru, Tanzania, Cameroon, Indonesia, and Tanzania) across the tropics. The REDD+ initiatives considered in this chapter include the two in Peru evaluated in the second chapter, but the outcomes analyzed are different. These two chapters provided a broad view of how interventions aimed at reducing deforestation and improving social development can affect different population groups in the same area in different ways.

CHAPTER 1

IMPACT OF INSPECTIONS ON COMPLIANCE WITH PERUVIAN FORESTRY LAW[1]

1.1 INTRODUCTION

The Amazon holds a vast stock of timber resources that is part of the national wealth of the nations of this region. National governments have sought to manage this resource and the forestry sector through command and control policy. However, forest degradation due to selective illegal logging is widespread across the Amazonian region, including in the Peruvian forestry sector (Sears & Pinedo-Vasquez, 2011).

From 2003 to 2015, Peruvian forest policy was guided by the national Forestry Law N° 27308. Under this law, the government issued 609 forest concessions through public auctions in 2003 - 2004. OSINFOR (Organismo de Supervisión de los Recursos Forestales y de Fauna Silvestre) was given responsibility for enforcement of the law, and in 2009, OSINFOR became fully independent and autonomous. One of OSINFOR's tools for enforcing the law is to conduct field inspections in the areas authorized for timber harvest, including the 609 concessions. However, such inspections are costly, raising the question of whether they improve compliance with the law, e.g. by reducing the availability of black market permits for transport of illegally harvested timber. We argue that the primary mechanism through which inspections could improve compliance is deterrence, and we therefore ask whether inspections reduce the probability and size of future violations. We approach this question by first providing background information on the policy, and then in the next section developing a theoretical framework. Following that, we review the literature, explain the methodology, and present the results. In the last section, we conclude and recommend how to improve the system of field inspections of forest concessions.

Reports by both governments and civil society have found that illegal logging is widespread in Peru and have alleged that timber concession holders are helping to "launder"

[1] Co-author: Erin Sills

illegally harvested timber (Finer et al. 2014). The World Bank estimated that illegal loggers made between US$45 and US$72 million in 2006, while the legal timber industry earned only US$31.7 million (Pautrat and Lucich, 2006). Key reasons for illegal logging include its high profitability, weak forestry law enforcement, and the vast extent and poor connectivity of the Peruvian Amazon (Smith et al., 2006).

In 2012, the Environmental Investigation Agency (EIA) identified illegal practices among forestry companies in Peru. Specifically, the EIA tracked shipments of Spanish cedar (Cedrela odorata) and mahogany (Swietenia macrophylla)—tree species listed in CITES[2] Appendix II and traded between Peru and the United States—and found 100 illegal exportation shipments between 2008 and 2010, representing around 35% of total exports of CITES species to the United States. The most important Peruvian timber company, Maderera Bozovich, was found to be involved in illegal timber trafficking. EIA estimated that 45% of Bozovich's shipments in the analyzed period came from illegal sources (EIA, 2012).

In 2014, a joint operation carried out by OSINFOR, the Peruvian tax agency SUNAT (Superintendencia de Administración Tributaria), and INTERPOL detected 213,982 cubic meters of illegal timber valued commercially around US$25.4 million (OSINFOR, 2015). This operation found that 67% of authorized loggers (concessionaires, native communities, and farmers) had submitted false information about logging activities: field inspections of the approved harvest areas did not find any evidence (e.g. stumps) of around 8,200 trees reported to have been harvested. A similar operation in 2015 found even more illegal activity, detecting 432,764 cubic meters of illegal timber with a commercial value of around US$51 million (OSINFOR, 2016).

Finally, in 2016, the Interagency Committee on Trade and Timber Products of the United States released a report on a particular case of illegal timber from Peru being imported into the US. Using official information obtained from OSINFOR and other Peruvian agencies, they concluded that most of a shipment of timber from Peru to the United States in January 2015—which had been seized in the port of Houston—came from illegal sources. This report also highlighted how the Regional Forestry Authority in Loreto (under

[2] Convention on International Trade in Endangered Species of Wild Fauna and Flora.

the Gobierno Regional) had delayed providing documents requested by OSINFOR, which in turn delayed on-site supervision.

The Peruvian government's response to illegal logging has been shaped since 2003 by Forestry Law N° 27308.[3] This law created OSINFOR as a forestry supervisory agency in order to separate forestry promotion and supervisory activities. Before 2000, the Agriculture Ministry had overseen all forestry activities, which generated rampant corruption because the same officials who approved timber activities were in charge of monitoring those activities. Even though the forestry law ordered that OSINFOR be an independent and autonomous agency, OSINFOR was actually established as a minor office operating under the supervision of INRENA (an agency created for promoting forestry activities). The 2009 free trade agreement between Peru and the United States included an Annex on Forest Sector Governance that required the Peruvian government to establish OSINFOR as an independent and separate agency, as originally required by the forestry law, within 18 months of the date of the agreement.

Since 2009, OSINFOR has been in charge of enforcing the Forestry Law for anyone (companies, communities, or farmers) granted the right to extract timber from natural forest. This includes 609 forest concessions that the government allocated through public auctions between 2003 and 2004. OSINFOR's primary responsibility is to verify that the Annual Operation Plan (AOP) is carried out as it was approved. The AOP contains information about each tree approved for harvesting and detailed plans for timber harvest in the authorized harvest area. Inspecting authorized harvest areas is costly because it requires that OSINFOR officials and assistants (forestry assistant, local guide, helper to open access in the forest, and cooks) travel to the forest concessions. In the Amazonian region, boat is the only way to access many concessions. After a field inspector makes his report, OSINFOR's legal team starts the administrative process in response to any infractions of the Forestry Law.

OSINFOR (2015) reported that 564,00 cubic meters of illegal timber was detected on forest concessions between 2009 and 2014. This illegal timber is classified by OSINFOR as over-reporting which is the Allowable Sale Quantity (ASQ) based in the AOP that

[3] This forestry law was in effect until October 2015, when a new forestry law was passed.

concessionaire may opt not to harvest but still request transport permits for the full amount of ASQ. The difference between non-harvested ASQ and the total ASQ is illegal timber (over-reporting).

The other means that the Peruvian government has for controlling illegal logging is to regulate the transport of logs by road and river. In the interregional roads, the Government installed check points to supervise all timber trucks. In the river network, the Government implemented check points to supervise timber shipments. The law enforcement in these control sites is carried out by ecological police and forester engineers. These official agents check whether the timber in transit has the official transport format and whether the information is accurate. Ending by noting that the effectiveness of the inspection system depends entirely on the integrity of the permit system, which in turn depends on the actions of agents who have been granted the legal right to harvest timber, including the concession holders

This research attempts to quantify the benefits of OSINFOR's enforcement activities, particularly the impact of field inspections of forest concessions from 2009 to 2014 on the generation of black market permits for the transport of illegal timber. We selected 2009 as beginning of our analysis because it coincides with the year that OSINFOR became an independent and autonomous agency. We proxy for the supply of transport permits to the black market by the number of trees and total volume that was claimed but not substantiated by field inspections.

This research was possible because OSINFOR provided access to all field inspection reports from 2009 to 2014 that had reached the final legal stage (i.e., the process had been closed by 2016). This was made possible by a Memorandum of Cooperation entered into by OSINFOR and North Carolina State University in 2015. OSINFOR has used standardized guides[4] for field inspections of forest concessions since 2005, which helped ensure uniform collection of data on standardized indicators during on-site inspection across all years of our analysis.

[4] The inspection procedures where further standardized in August 2015 by Resolución Gerencial Nº 005–2005–INRENA–OSINFOR.

Native communities are immersed in the illegal timber problem because around 10.6 million of natural forests, 20% of total Peruvian forest, are inside their territories (MINAM, 2008). This large area is managed by 1,786 native communities with a total population of around 332,975 inhabitants (representing 1% of the total Peruvian population) (INEI, 2012). However, we exclude native communities from this study because timber activities on these lands require special permits that are issued at the regional level and thus not accessible through our MOC with OSINFOR. Additionally, we exclude other legal mechanisms for harvesting timber from natural tropical forest such as local contracts for non-native communities and authorization to harvest dry forest in the coastal area.

1.2 THEORETICAL FRAMEWORK

Becker (1968) developed a theoretical framework to study law enforcement, and Stiglitz (1987) continued to develop the framework through an Agent-Principal model. We draw on these perspectives to develop a theoretical model of the behavior of forest concessionaires with regards to Peruvian forestry law.

We assume that there are two participants in the forestry sector: an agent (in our case, the forest concessionaire) who has an incentive to break the law because it generates individual economic benefits for him; however, this behavior imposes costs on society (enabling illegal logging that causes forest degradation or, in the extreme, extinction of a specific tree species). The second participant is the principal (in our case, the forestry supervising agency, OSINFOR), who is in charge, on behalf of society, of enforcing forestry law in order to maximize social welfare through compliance.

The agent is rational, in an economic sense, and his decision whether to not abide by the forestry law depends on the economic benefit of breaking the law and the expected cost of being caught by OSINFOR (the probability of being caught multiplied by the penalty, which may be fines or, in the extreme, loss of timber rights). Therefore, the agent maximizes the following expected benefits equation:

$$Max\ E(B) = (1 - p)\ economic\ benefits - (p)\ cost\ of\ being\ caught$$

In this equation, the economic benefits are the benefits derived from additional transport permits generated by claiming to harvest more timber than actually harvested, potentially differentiated by species and region. The cost of being caught includes fines and the potential loss of authorization for future timber harvests. The probability p represents the agent's perceived probability of being caught by OSINFOR. This probability plays an important role in the agent's decision whether to break the law. For instance, assume that the benefits and costs are the same. When the probability of OSINFOR detecting the infraction is less than 50%, the expected benefits will be positive and the agent will be encouraged to break the law. The converse happens when the probability is greater than 50%. The agent's perception of the probability of being caught is shaped by both stated policy and the actions of OSINFOR, i.e. OSINFOR's actions may have a deterrence effect.

The maximum expected benefits that can be obtained by the agent (forest concessionaire) may fall in one of three regions: (1) if Max E(B) > 0, the agent will break the law, (2) if Max E(B) are around zero, the agent will be indifferent, and (3) if Max E(B) < 0, the agent will not break the law. Thus, the principal must consider the costs of constraining the agent's Max E(B) to less than zero, by making choices that influence both p (e.g., the fraction of concessions to inspect every year) and the cost of being caught (e.g., the size of the fine). Thus, in addition to the objective of catching and fining companies for infractions, inspections are intended to increase the perceive probability of future inspections. Infractions that are considered minor and therefore incur only small fines may continue in concessions even if they have been inspected repeatedly. For more serious infractions, it should be possible to observe lower rates in concessions with higher perceived probabilities of inspection.

In the environmental enforcement literature, the deterrence effect is classified as either specific or general (Gray and Shimshack, 2011). The specific deterrence effect is defined as the impact of enforcement actions on compliance in the inspected plants, sites, or agents (concessions, in this case). The general deterrence effect is defined as the impact of environmental enforcement on compliance in non-inspected plants, sites, or agents located nearby or belonging to the same industry, sector, or jurisdiction on the inspected unit. Thus, the general deterrence effect is the spillover effect on non-inspected units. Our analytical framework does not explicitly consider the possibility of a general deterrence effect, but we return to this point in the discussion, drawing on in-depth interviews with actors in the Peruvian forestry sector conducted in May – June 2016.

It is also important to note that our Agent-Principal model does not consider corruption among Principals (OSINFOR inspectors), which could modify the expected benefit equation. In that case, the probability of being caught would depend of the level of an inspector's corruption. Likewise, our model does not considering the time that elapses between field inspection and the effective economic sanction, which occurs after an administrative process. In order to account for this, the cost of being caught should be computed as a present value considering the average amount of time between detection and sanction.

1.3 LITERATURE REVIEW

Several studies performed in the United States, Canada, and Europe have found deterrence effects from environment law enforcement. Shimshack and Ward (2005) measured the specific and general deterrence effect of monetary sanctions (fines) on the water sector in the United States. They evaluated the effect of sanctions on water pollution by both the specific sanctioned plants and by the water sector overall. According to their results, a single fine to an individual plant increased the credibility of the regulator thereby amplifying the effect on the whole sector. Other plants in the same regulatory jurisdiction responded similarly to the fined plant. From a policy perspective, better water quality can be achieved from a small increase in enforcement investment. On average, the spillover effect meant that a marginal fine induced a two-thirds reduction in the statewide violation rate of pollution in the year following the fine.

Gray and Shadbegian (2007) studied the general deterrence effect of enforcement of air pollution regulations in the context of the specific spatial locations of plants. They found significant positive spatial correlations of compliance between inspected plants and non-inspected nearby plants. Nearby plants tended to have similar compliance rates, as long as they were in the same state. Deily and Gray (2007) studied the relationship between law enforcement and compliance with air pollution regulations in the steel sector and found that steel plants were more likely to comply with air pollution regulation when they had been inspected. Specifically, steel plants that had been inspected two years ago had a 33% increased probability of compliance compared to plants that had not been inspected.

Telle (2013) evaluated the specific deterrence effect of several enforcement strategies of the Norway Environmental Protection Agency using data from a natural field experiment that allowed different enforcement strategies to be compared in a statistically robust way. On-site audits by the EPA reduced the likelihood of environmental violations in the next year by 68%, while self-audits reduced the likelihood of environmental violations by 29%. The study found no statistical evidence that announcing higher frequency audits improved the compliance behavior of firms.

In the forestry sector, Ramcilovic-Souminen and Epstein (2015) studied the impact of deterrence, social norms, and legitimacy on forestry law compliance in Ghana. These authors modeled individual compliance with three different rules: (1) the tree-felling rule (a prohibition against cutting trees on farmland for commercial purposes), (2) the farming rule (a prohibition on growing crops in forest reserves), and (3) the bushfire prevention rule (legal requirements for using fire in farming activities). The individual compliance information was gathered from self-reported responses. These authors used a logistic model to measure the probability of compliance with each rule and found that the deterrence variable (measured as perceived individual probability of being detected[5] multiplied by the sanctions expected if detected[6]) was only statistically significant in the model for the tree-felling rule. Compliance with the tree-felling rule increased by 37.5% when an individual believed that detection was very likely and expected to be sanctioned, compared to 22% for individuals that do not expect to be sanctioned. Peer behavior and perceived fairness of rules were not statistically significant in the model of compliance with the tree-felling rule. The exact opposite results were found for compliance with the farming rule and the bushfire prevention rule.

There is limited access to data on field inspections in the forestry sector in Latin America, because many countries, including Peru, consider this information to be legally

[5] This is a discrete variable: 1 very unlikely, 2 somewhat unlikely, 3 somewhat likely, and 4 very likely
[6] Dummy variable: 1 sanctions expected if detected or 0 no sanctions expected if detected.

sensitive. However, descriptive studies have explained the nature, modus operandi, and strategies used by illegal timber traffickers.

Finer et al. (2014) pointed out that the concession system in Peru facilitates the expansion of illegal timber extraction. In a simple scheme called a laundering strategy, forest concessionaires over-report the number of trees in their concessions and then ask for formal approval to extract the nonexistent trees. After approval, transport forms (guias de transporte) are used to transport and sell timber that comes from unauthorized areas, generally protected areas.

Even the free trade agreement between Peru and the United States, which included a forestry annex intended to improve Peruvian forestry governance, was unable to stop or reduce illegal timber trafficking in the Peruvian Amazon (Finer et al., 2014). In fact, 79.4% of forest concessionaires in the Loreto region did not implement their Forestry Management Plan and 63.7% of forest concessionaires reported false information, mostly nonexistent trees, in their AOP. At the same time, 57.8% of forest concessionaires had extracted timber outside of their concession. These infractions have resulted in cancelation of a majority (~70%) of forest concession rights.

By using OSINFOR's field inspection reports, we are able to fill a gap in the literature on law enforcement and the deterrence effect in the forestry sector in developing countries (Gray and Shimshack, 2011). More specifically, we assess whether field inspections – which are a common but expensive enforcement strategy – have a specific deterrence effect on the inspected concessions.

1.4 METHODOLOGY

This research poses two challenges. First, field inspection data are only available for inspected forest concessions; we do not have data for non-inspected concessions. In order to obtain a control group, we exploit time series data on field inspections. Field inspection occurs at the end or very near the end of logging operations, and therefore checks on activities performed during the most recent logging operations. According to the OSINFOR Supervision Manual, the inspector's main duty is to verify the status (standing, fallen, or harvested with only stump remaining) of a sample of trees in the annual harvesting area inside concessions. Any effect of this inspection on the behavior of the forest concessionaire should be detected in future field inspections. This allows us to build a control group using forest concessions that were inspected for the first time in 2013 or 2014. This control group represents the behavior of forest concessionaires before any inspection, i.e. at some baseline level of the expected probability of inspection. The treatment group are forest concessionaires that were also inspected in 2013 and 2014, but in addition, had already been inspected at least once between 2009 and 2012. This treatment group represents the behavior of forest concessionaires after being inspected (see Figure 1.1).

The second challenge to overcome is the selection bias generated by OSINFOR's choice of which concessions to inspect early (before 2012). Note that by limiting our study sample to only concessions that have been inspected at least once in 2013 or 2014, we control for any unobservables that result in inspections of some concessions and not others. Thus, we focus on identifying and controlling for any confounders that may influence both whether a concession was inspected early (2009 – 2012) (the treatment) and the probability of compliance with the forestry law in that concession (the outcome).

In a 2012 presentation, the Excutive President of OSINFOR stated that concessions in areas with the following characteristics are prioritized for inspection (OSINFOR 2012):

1. Buffer zone of protected area(s);
2. Presence of CITES species;
3. Evidence of inaccurate self-reported and increasing timber extraction;
4. High concentrations of timber concessions;
5. Presence of illegal mining;
6. Allegations of illegal activity.

These criteria could also be related to the probability of breaking the forestry law. Thus, we control for these characteristics when comparing treatment and control groups in order to avoid potential selection bias, using approaches developed in the quasi-experimental impact evaluation literature.

Quasi-experimental methods have been developed to evaluate policies and programs when selection to treatment is not random, either because of administrative selection or because of self-selection. In this study, we consider inspections initiated by OSINFOR (not requested by the concessionaire or native communities in order to resolve conflicts). Concessionaires do not have the option of refusing these inspections after they have been selected, so we focus on eliminating or mitigating bias introduced through the OSINFOR process of selecting concessions. Specifically, we constructed a balanced sample of treated and control concessions using propensity score matching with covariates suggested by OSINFOR's selection process.

To facilitate the exposition, we define the outcome as the average illegal timber volume, $\mathrm{D_i}$, of inspected concessionaires. Suppose that there are n forest concessions, i of which are inspected by OSINFOR prior to 2012. We expect that forest concessionaires who are inspected will reduce or (in the best case scenario) eliminate illegal timber volume in subsequent years. Thus, the treatment is prior inspection and the impact of that treatment is the difference between the average amount of illegal timber that they actually harvest $\mathrm{D_{1i}}$ and the average amount they would have harvested if they had not previously been inspected, denoted by $\mathrm{D_{0i}}$. Forest concessions in the treatment group are identified as $\mathrm{T} = 1$, and concessions in the control group are identified as $\mathrm{T} = 0$. Our objective is to estimate the Average Treatment Effect on the Treated (ATET):

$$ATET = [D_{1i} - D_{0i}|T = 1] \tag{1}$$

The estimation of ATET would be trivial if we could simultaneously observe the same forest concessions in the treatment (previously inspected) and control (never previously inspected) groups, but we know that this is impossible because each forest concession can belong to only to one group (control or treatment). With the observable information, we could instead estimate:

$$Diff = E[D_{1i}|T = 1] - E[D_{0i}|T = 0] \tag{2}$$

By combining equations 1 and 2, including the unobserved outcome $E[D_{0i}|T = 1]$, we obtain:

$$ATET = E[D_{1i} - D_{0i}|T = 1] + \{E[D_{0i}|T = 1] - E[D_{0i}|T = 0]\} \quad (3)$$

The expression in curly brackets on the right side of equation 3 represents the selection bias. Quasi-experimental methodologies try to neutralize the effect of selection bias in the ATET estimation through a variety of approaches[7]. Conceptually, we need to find a control group such that

$$E[D_{0i}|T = 1] = E[D_{0i}|T = 0] \quad (4)$$

To accomplish this, we examine single differences in a sample balanced on observables X (equation 5). As explained by Ferraro and Miranda (2014), "conditional on the observable variables, the expected outcome of the control group represents the expected outcome of the treated group in absence of treatment" (p. 347). This is based on a well-known assumption called "selection on observables." Thus, we replace equation 1 with

$$ATET(X) = E[D_{1i}|X, T = 1] - E[D_{0i}|X, T = 0] \quad (5)$$

and assume that

$$E[D_{0i}|X, T = 1] = E[D_{0i}|X, T = 0] \quad (6)$$

Equation 5 represents an unbiased estimator of ATET, assuming that equation 6 holds. There are several approaches to conditioning on observables, and we use the matching method, which helps build statistically comparable control and treatment groups on observable characteristics. In the matched sample, we then use a single difference to compute the Average Treatment Effect over Treated (ATET). To evaluate the consistency of our

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[7] Approaches include Instrumental Variables (IV), Discontinuity Regression (DR), Difference in Difference (DiD), and Single Difference (SD). The IV methodology requires at least one variable that explains the participation decision but does not directly influence the outcome. The DR methodology exploits the discontinuity in some relevant variable that is required for treatment and compares treatment and control groups around this threshold. The DiD methodology exploits panel data before and after the intervention to get the ATE and assumes that the effects of time-variant, unobservable variables are the same for the treatment and control groups (that is, parallel trend assumption over unobservable variables).

results, we estimate a kernel matching with Epanechnikov distribution and bandwidth of 0.06 using the STATA 13 "psmatch" routine. Additionally, we estimate Ordinary Least Square (OLS) to test the robustness of our results.

Although we use data from 2009 to 2014 to define treatment, our outcome is based on reports from inspections in 2013 and 2014 only. This allows us to make a consistent comparison, controlling for exogenous macroeconomic factors that change slowly over time and that could affect the behavior of forest concessionaires in relation to illegal timber trafficking. However, we do not differentiate by time elapsed between the first and second field inspections (four or five years), which could affect the formation of expectations about the probability of new inspections. Another potential complication is "general deterrence," or spillover effects, which could affect concessionaires in the control group. Even though this effect is desirable from a public policy perspective, from an impact evaluation perspective, it could bias the estimated impact of treatment downward.

1.5 DATA

Between 2009 and 2014, OSINFOR carried out 444 field inspections of forest concessions. Table 1.1 lists the number of inspections carried out, the number completely resolved with the administrative process now closed, and the number of inspection reports available for this analysis. OSINFOR provided reports that reached final resolution in the administrative process (PAU) and reports that did not go to the administrative process (because there was no evidence of forestry law infraction). We have access to most of the reports in each year, with the exception of 2009 (less than 25%). We do not have access to all reports for several reasons. First, many inspections in 2009 and 2010 were not carried out properly and are therefore incomplete. For instance, some reports only show the AOP information because the inspector did not access the extraction balance or official transportation form, meaning that the inspector was unable to conclude anything about illegal timber in the concession. OSINFOR considers these inspection reports to be invalid. Second, some reports of inspections carried out between 2011 and 2014 were not provided because the legal process resulting from the inspection is still ongoing. Omission of these reports does not introduce a systematic bias because the legal process could be decided in favor of the concessionaires (reports are declared invalid) or in favor of OSINFOR (reports are declared valid).

Using information from 337 reports on field inspections carried out from 2009 to 2014, we identify forest concessions to be included in the treatment group (see Table 1.2). Concessions in this group were first inspected between 2009 and 2012 and inspected a second time in 2013 or 2014. This treatment group includes 29 forest concessions. Although this group appears small, it includes 181 observations on particular tree species in particular concessions (on average, OSINFOR assigns an inspector in an inspected concession).

The control group is made up of forest concessions that received their first inspection in 2013 or 2014 (see Table 1.3). We exclude field inspections requested by forest concessionaires in response to invasion by third parties (one report in 2013) or requested by native communities affected by the logging activities of forest concessionaires (one report in 2014). Similarly, we do not include field inspections required by the Regional Forestry Authorities to determine the source of illegal timber seized by police (five reports between 2013 and 2014). Incomplete field inspection reports are also not included (ten reports between 2013 and 2014).

Figure 1.2 presents a map of the control and treatment groups. This map also shows the distribution of forest concessions with respect to Protected Areas (PA) and their buffer zones. Buffer zones of varying width are defined by the Peruvian national authority of protected areas. Several concessions in both the control and treatment groups are close to PA buffer zones, and nine forest concessions are located inside PA buffer zones. This is one of OSINFOR's criteria for selecting concessions for inspection, because of the concern that concessionaires located close to or inside PA buffer zones could more easily harvest trees from protected areas. Therefore, this variable is included as a dummy variable (1= inside buffer zone) in the specification of the propensity score in the matching process between the control and treatment groups.

After identifying the treatment and control groups, we created a database by entering data from hard copies of field inspection reports. In the process, we became intimately familiar with the types of information included in those reports.

Figure 1.3 displays the size distribution of the Annual Harvesting Parcel (AHP), which is the area where field inspections take place. The area of the AHP may be an important confounder because forest concessionaires ask for authorization to harvest trees in this area and OSINFOR assigns samples of trees to inspect in this area. However, the AHP changes from year to year, and thus our primary specification uses the area of the entire concession as a matching covariate. When the logs harvested in the AHP are ready to be transported to the market, concessionaires ask for transportation permits from the Regional Forestry Authorities. Each transportation permit is registered in an extraction balance after the timber passes a forestry checkpoint on a road or river. At the end of the harvesting operation in the AHP, concessionaires are required to present execution AOP reports, which contain information about the status (harvested, standing, fallen, other) of each tree that had been approved for harvesting. Finally, a sample of those approved trees are inspected using all of the available information about the concession (AOP, extraction balance, transportation permits, and—in some cases—execution AOP report). Appendix 1 summarizes this regulatory process.

One variable that could affect decisions about which concessions to inspect is distance from each concession to the nearest OSINFOR regional office. Salonen et al. (2012) compared several distance measures in the northeastern Peruvian Amazon and found that the mean Euclidean distance from riverine villages to Iquitos City (the capital of Loreto) is 267 km while the mean distance using the riverine network is 758 km. They recommend using the distance that takes into account the riverine transportation network, because that more accurately represents the distance traveled by people living there. We follow their recommendation to compute distance to each concession. Figure 1.4 shows distance in kilometers and hours between OSINFOR regional offices and the forest concessions in our study groups. Distance in hours was taken from field inspection reports and distance in kilometers was estimated from the reported route taken by the OSINFOR team using Google Earth (see Appendix 2 for an example). This distance represents the best option for accessing a concession and is the most accurate estimate of the distance traveled by the OSINFOR team.

We also sought indicators of the quality of field inspections. In the forestry sector, these could be inferred from the number of days of fieldwork (not including travel days) and the number of trees inspected. However, we can see in Figure 1.5 that there is no clear correlation between these two variables. Occasionally, inspectors did not find any trees when doing fieldwork in the AHP; these cases are shown as a zero mark on the x-axis (inspected trees) and positive values along the y-axis (days of fieldwork). Many (if not all) of these cases are inspections of concessions that reported false information (nonexistent trees) in the AOP. Before 2015, it was possible to get a 100% false AOP approved because of a provision of administrative law (Ley Nº 27444 "Ley de Procedimiento Administrativo General") that meant no visual inspection of the information reported in the AOP was required. Illegal loggers used this loophole in the approval process to get access to official documents to transport illegally harvested timber to both local markets and ports for export. Field inspection reports show extreme cases where an AOP was approved in wetlands, which are not ecologically suitable for any of the trees species harvested for timber in Peru.

Figure 1.6 shows the percentage of inspected trees by the total number of trees approved for harvest by species for each inspected concession. The heterogeneity in the percentage of inspected trees by species reflects the guidance in OSINFOR Supervision Manual (2013), which requires 100% of CITES species to be supervised. During the time period of this study, the only two Peruvian trees listed in Appendix II of the CITES convention were mahogany (*Swietenia macrophylla*) and Spanish cedar (*Cedrela Odarata*). Additionally, 25% of tree species meeting the following criteria are inspected:

1. Species with high demand in the timber markets.
2. Species with high harvested volume according to Extraction Balance reported by the forest concessionaire.
3. Species included in the Decreto Supremo N° 043–2006–AG, which lists all endangered species in the Peruvian forestry sector.
4. Species with high approved volume.

The OSINFOR supervisor selects 25% of total non-CITES species using these four criteria mentioned in the previous paragraph. It means this selection is not random. The sample size by each selected species is computed using the formulation for random sample for finite population. Although the process of determining the sample size is detailed in the OSINFOR Supervision Manual, there are no instructions on how to distribute the sample in the AHP. The Manual only states that "trees to be inspected have to be distributed throughout the AHP which will allow more representativeness" (OSINFOR, 2013, p. 4).

The lack of a clear methodology for sampling trees in the AHP undermines the statistical representativeness of the sample, which therefore cannot be used to infer the status of the total population of trees by species in the AHP. Specifically, it means that inspectors cannot estimate the total illegal timber volume (called unjustified timber volume by OSINFOR) in inspected forest concessions. The illegal timber volume reported by an inspector is a lower-bound estimate because it is just the difference between harvested timber volume reported by the concessionaire to the government and the harvested timber volume confirmed by inspection in the inspected sample. According to OSINFOR procedures, inspectors cannot make any inferences about non-inspected trees. An exception to this rule applies when there is no evidence of any timber harvesting operation in the AHP; in this case the inspector can infer that all reported timber is illegal, i.e. was not actually harvested but was instead used to generate illicit transportation permits.

1.6 RESULTS

1.6.1 Descriptive results

Figure 1.7 shows the relationship between harvested timber volumes reported by the concessionaires in the extraction balance and harvested timber volume confirmed by inspectors.[8] The blue line identifies forest concessions in which reported and inspected timber volumes are the same. Any point over the blue line identifies concessions that over-reported timber volume (in order to obtain additional transportation permits to launder illegal timber). In the last five years, over-reporting has been the main problem in Peruvian timber

[8] To improve the visual presentation in Graph 5, we dropped nine observations in which harvested timber volume reported by concessionaires was greater than 2,000 cubic meters.

concessions. There is a special case in Figure 1.7 in which the x-axis value (inspected) is zero and the y-axis has positive values (reported). In these cases, inspectors did not find any of the trees in their assigned sample in the AHP even though the concessionaires had reported harvested timber volume. These are cases of 100% false information.

Figure 1.8 replicates Figure 1.7 with information on specific species. The study database contains eighty-five species of harvested trees, but here we only show the species that are most prevalent in the concessions. The amounts authorized and actually harvested most often coincide (fall on the blue line) for spanish cedar and mahogany, implying that at least some concessionaires have internalized the fact that 100% of the trees of these species are inspected in concessions selected for inspection, deterring them from over- or under-reporting. The situation is totally different for species such as tornillo, shihuahuaco, cumala, copaiba, and lupuna, which have more cases of over-reporting by concessionaires.

This discrepancy between the harvested volume reported in the extraction balance and the volume identified in field inspections is the main source of information to quantify the amount of illegal timber. However, other variables—such as sample size by tree species and the estimation range margin stated in the OSINFOR Supervision Manual—play a role in determining the final volume of illegal timber. For mahogany and Spanish cedar, the difference between timber harvested volume reported in the extraction balance and inspected is the total illegal timber volume because these tree species are supervised at 100%. For species with tree sample for inspections, the inspector estimates the illegal timber volume only for inspected tree species. In case that concessionaire report 100% of authorized volume as harvested, inspector uses the difference between reported and inspected as illegal timber. In case that concessionaire reports less than 100% of authorized volume as reported, the inspector takes as a reference the remaining timber volume that was not declared as harvested to compute illegal timber. For instance, whether there are100 authorized trees with 500 cubic meters of timber for the inspected specie, and the concessionaire reports timber harvested for 400 cubic meters in the extraction balance. Then, the inspector supervises a sample of 60 trees and he find that 30 are standing and they accounts for150 cubic meters of timber. The illegal timber computed for this specific case will be only 50 cubic meters (the difference between 150 cubic meters confirmed in standing trees by inspection and 100 cubic meters remaining from authorized trees). The inspector assumes that 40 non-inspected trees were harvested which favors to concessionaire.   The OSINFOR Supervision Manual does not include a procedure for estimating illegal timber volume, so this estimate is made using individual inspectors' criteria.

After finishing a field inspection, the OSINFOR inspector prepares a report concluding one of the following: (1) reported and supervised timber volume are similar, which means timber volume is justified; (2) reported harvested timber volume is greater than inspected (observed) timber volume, which means that timber volume has been over-reported; or (3) reported harvested timber volume is lower than inspected timber volume, which means that volume has been under-reported. OSINFOR assumes that over-reported timber volume is illegal because the concessionaire could not prove that the timber came from the AHP inside the concession, yet transportation permits were issued. When OSINFOR identifies under-reporting, this is considered to be "non-reported harvested timber," which is not illegal timber. OSINFOR considers that the concessionaire was in process to update the extraction balance if the total harvested timber volume is not greater than authorized timber volume. When the field inspection report includes the second or third cases, the report is sent to the Dirección de Supervisión de Concesiones Forestales y de Fauna Silvestre (Forest Concession and Wildlife Supervision Unit) to start the administrative process that determines what infractions have incurred and the associated pecuniary or nonpecuniary sanctions.

Figure 1.9 displays illegal (over-reported) timber volume claimed by OSINFOR, aggregated at the concessionaire level. In our sample, there is one concessionaire with two concessions. In this case, we averaged the variables related to concessions characteristics (area, AHP, and distance to concessions) and variables related to timber volume activity (authorized, reported, and inspected). Many concessionaires in the control group have high illegal timber volume. At the species level, 68% of inspected species include illegal timber that was not justified by over-reporting, 30% of cases include justified timber volume (reported and inspected are the same), and 2% of cases that were not justified by under-reporting.

Thus, these data confirm that over-reporting to access official documents in order to launder illegal timber is widespread among timber concessionaires in Peru. Through interviews, we confirmed the existence of a black market for transportation permits in the Ucayali and Loreto regions. These two regions are highly interconnected, and almost all timber harvested in these regions is transported to Pucallpa (Ucayali's capital) via river and then via road to Lima.

Table 1.4 summarizes descriptive statistics for variables at the concessionaire level. The average AHP in the treatment group is greater than the average AHP in the control group by around 210 hectares. In contrast, approved timber volume is greater in the control group than in the treatment group by around 1,600 cubic meters. The reported harvested timber volume is higher in the control group than in treatment group (by around 400 cubic meters). Similarly, the control group has been fined for larger amounts (around US$10,000) and more frequently than the treatment group for infractions of forestry law.

The combination of smaller harvesting area and higher approved timber volume could indicate that concessionaires in the control group inflate the number of trees when they submit their AOP to the Regional Forestry Authority in order to get more transportation forms. The higher and more frequent fines are consistent with this suggestion. However, these patterns could also reflect other systematic differences between the treatment and control groups. The standardized differences in means (Austin, 2011) suggest that almost all variables are substantively different in the treatment and control groups, with only two exceptions (presence of CITES species and reported harvested timber volume).

Table 1.5 presents descriptive statistics for each inspected species. Illegal timber claimed by OSINFOR is greater in the control than in the treatment group, and these averages are systematically different (absolute Standardized Difference in Mean greater than 0.10). Nevertheless, the difference in illegal timber between the control and treatment groups is only around 100 cubic meters. Given that the control and treatment groups were not chosen using an experimental design, we cannot draw any conclusions about the effect of treatment from this information.

1.6.2 Impact evaluation results

We apply Single Difference (SD) methodology to estimate the Average Treatment Effect over Treated (ATET). We need to build a robust counterfactual control group to get an unbiased estimator of ATET. We construct this control group by matching on observable variables, which also helps control for selection bias. There are various algorithms for matching, including propensity score matching (PSM), stratification of propensity score, trimming matching, inverse probability weighting, and kernel. Rosenbaum and Rubin (1983, 1985) provided theoretical justification for the popular PSM method, which we adopt.

The PSM algorithm is popular because it helps reduce a multidimensional problem to a one-dimensional problem. This technique assumes that observable variables affect the probability of being selected into treatment group and that their combined contribution to the probability can be summarized in a "propensity score." After estimating the propensity score, the next step is to define a metric to compare propensity scores and define sub-sets of treatment and control observations that are statistically similar (e.g., nearest neighbor or kernel matching).

Given that the propensity score is the probability of being treated, it is estimated with a discreet choice model (Caliendo and Kopeinig, 2008). We estimate a probit model of the probability of "early inspection" between 2009 and 2012. Given that our sample of concessionaires was all inspected in 2013 or 2014, this is equivalent to estimating the probability of being inspected at least twice during the full study period. Thus, the dependent variable is binary (1 if concessionaire was inspected twice). The independent variables are as follows:

1. Area of Annual Harvesting Parcel (AHP) in hectares;
2. Area of forest concession in hectares;
3. Access to AHP (distance from OSINFOR regional offices) in kilometers;
4. Presence of CITES species in the concession area, dummy variable;
5. Timber volume approved by the Regional Forestry Authority, in cubic meters, in 2013 or 2014;

6. Harvested timber volume reported by the concessionaire to the government, in cubic meters, in 2013 or 2014;
7. Location at least partly inside buffer zone of protected area(s).

Table 1.6 shows the estimation results for the propensity score model. We include two probability models. My primary model is model 1 which includes concession area as a dependent variable. We chose this model because it results in the lowest average percentage bias across covariates, with only distance remaining unbalanced. Given that distance is an important potential confounder, we check an alternative specification (model 2) adding AHP to the model.

While the primary purpose of the propensity score model is to identify balanced sub-samples of treated and control concessions for further statistical analysis, the estimated coefficients reported in Table 1.6 also provide insight on the factors influencing selection of concessions for inspection. As expected, forest concessions further away from OSINFOR regional offices were less likely to be inspected between 2009 and 2012. Early inspection efforts were concentrated closer to regional offices, whether for cost or safety reasons. Concessions inside the buffer zones of protected areas were less likely to be inspected between 2009 and 2012. Even though OSINFOR claimed (in 2012) that location in a buffer zone was a reason to select a concession for inspection. Finally, the presence of CITES species in a concession was not relevant to the inspection decision between 2009 and 2012, again counter to OSINFOR's description of its process for selecting concessions for inspection.

Table 1.7 presents balance diagnostics for treated and untreated concessionaires for model 1 (primary model). Almost all variables have Standardized Difference in Mean (SDM[9]) of less than 0.11 in absolute value, with the exception of the covariate related to distance traveled, with an SDM of around 0.22 in absolute value. Even though no general consensus exists about the threshold that indicates balance, Austin (2011) suggests that

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[9] SDM formulae for continuous variable: $= \frac{(\overline{x}_{treatment} - \overline{x}_{control})}{\sqrt{\frac{s^2_{treatment} + s^2_{control}}{2}}}$

values less than 0.1 (in absolute value) indicate negligible differences in the means between treatment and control groups. The alternative model (see Table 1.8) presents more unbalanced variables than our primary model.

Using the propensity scores to control for selection bias, we estimate the impact of early inspection on illegal timber volume detected in the same concessionaires by later OSINFOR inspections, i.e. the ATET. We report results from two matching algorithms: (1) nearest neighbor matching based on the propensity score (implemented using the package "teffects" in STATA13) and (2) kernel matching based on the same covariates. For the first, we select the single nearest neighbor (i.e. the control concession with the closest propensity score) of every treated concession . We conduct this matching with replacement, so that any particular concession in the control group can be used more than once if it is the nearest neighbor for more than one treated concession. Likewise, we use a single nearest neighbor, which means that each concession in the treatment group will get only one control concession. Instead, kernel matching requires matching all control individuals. It implies that the full sample is used to compute the impact estimation. Kernel matches, also, on observable characteristics but, additionally, generates weights for each matched pair according to similarity. Then, these weights are used to compute a weighted DiD.

Table 1.9 summarizes the estimated ATET using nearest neighbor matching, with both the primary and the alternative specification of the propensity score. The outcome is "illegal timber," or the volume of timber over-reported, as detected in field inspections in 2013 or 2014. This outcome does not include fifteen under-reporting cases because they reflect a different decision making process. Our preferred specification (model 1 which has the best balance) indicates that inspections reduce illegal timber in future years by an average of 1,874 cubic meters, compared to an average of 2,206 cubic meters of illegal timber in control group. This impact estimate is significantly different from zero at the 95% significance level. The magnitude – but not the statistical significance – is robust to changes in the specification of the propensity score (model 2). In Appendix 3 and 4, we present the histograms of probability distributions for model 1 and model 2. From these two models, we observe that model 1 has a better overlap between probability distributions of control and treatment groups. In Appendix 5, we include the ATET estimation including the fifteen under-reported cases. Our impact estimates are approximately the same and model 1 is statistically significant at 95%.

Table 1.10 summarizes our ATET estimation by kernel matching for the same probit models used in the PSM. Our primary model shows that the ATET estimation is not statistically significant at 95% confidence level, even the magnitude of the impact is lower (947 cubic meters) than ATET by PSM algorithm. Our alternative model presents an ATET estimation of 1,512 cubic meters and is statistical significant at 95%. These results suggest that ATET estimation is not robust to matching algorithm.

Finally, as a robustness check of the previous result, we use Ordinary Least Squares (OLS) to regress illegal timber volume on a dummy variable for treatment (prior inspection), controlling for all of the same covariates as in the propensity score model. Likewise, we ran the linear regression only for matched sample. Table 1.11 summarizes the results for model 1 (primary) and model 2 (alternative) and shows that the coefficient related to the treatment variable is consistently negative and statistically significant at the 95% significance level. Our primary estimation results suggest that field inspection reduces future illegal timber harvest in 1,821 cubic meters. Our alternative model shows lower impact estimation (830 cubic meters) but it is not statistically significant. The results for our primary model confirm the positive impact of field inspections have on reducing illegal timber activities in forest concessions in Peru.

The estimated coefficients on several other variables are notable. First, reported harvested timber volume is statistically significant at 95% in the two analyzed models. In model 1, tthe positive coefficient indicates that illegal timber volume increases by 0.43 cubic meters for each cubic meter of harvested timber volume reported in the extraction balance by the concessionaire to the government. Second, presence of CITES species inside a forest concession is also statically significant at 95% (model 1) and 99% (model 2). In model 1, the negative coefficient suggests that forest concessions with CITES species have around 1,230 cubic meters less illegal timber. These results suggest that concessionaires with CITES species recognize that their concessions are more likely to be inspected, reducing their incentive to over-report their harvest volume in order to obtain illicit transportation permits. Third, travel distance is statically significant at 99% (model 1) and 95% (model 2). In model

1, the positive coefficient indicates that an additional kilometer distance to concessions, illegal timber increases by 7.71 cubic meters.

1.7 DISCUSSION

In Peru, most common violation of the law in timber concessions is over-reporting of harvested volume in order to access official documents and launder illegal timber. Two profit-maximizing strategies could drive the decision to over-report timber volume. The first is a cost-minimizing strategy. In this case, concessionaires harvest trees from unauthorized sites such as Protected Areas, Native Community lands, and special reserves, which are close to roads, mills, or markets. The second is a profit-maximizing strategy, in which concessionaires harvest highly commercially valuable trees from distant areas. This strategy can be profitable even when paying high transportation costs. This strategy implies that concessionaries can switch permits, using transportation forms issued for less commercially valuable species to transport highly commercially valuable species. The lack of specialized workers at forestry checkpoints makes this strategy possible. Similarly, a degree of corruption at those checkpoints makes it possible to use the same transportation form more than once. This problem is not unique to Peru; in Costa Rica, officials are bribed extensively to not stamp transportation permits, which can then be reused several times (Miller, 2001). The lack of an integrated forestry control system between checkpoints and central server also facilitates the transportation of illegal timber.

Even though our results indicate that field inspections increase compliance with the law in forest concessions, potentially decreasing illegal timber trafficking, this does not imply that field inspections eliminate illegal activities in forest concessions. We have only found that misreporting is reduced by inspections. Misreporting may continue despite field inspections for various reasons, including involuntary mistakes in the harvesting process (i.e., failure by new employees to harvest approved trees) or high demand (and therefore high profit margins) for official documentation that allows transport and trade of illegally harvested trees.

While field inspections attenuate illegal timber activities in Peru, there is evidence that illegal timber trafficking nonetheless increased between 2009 and 2014. One possible explanation is that there are not enough inspections. Another is that illegal timber traffickers have found other means to obtain the official documents required to launder illegal timber—such as permits for logging in native communities, on private lands, and in local forest. The last is a special permit that allows non-native local communities along rivers to harvest trees in the surrounding area. This instrument has been used extensively by illegal timber traffickers in recent years because it allows a community to harvest timber with limited threat of inspection (because specific loggers inside communities cannot be identified). According to forestry regulation (Resolución Jefatural Nº 042-2003-INRENA), management of local forest requires that specific authorization be issued to each member of a community who wants to harvest timber; however, this is not happening and local leaders (and their partners) have access to official documents on behalf of community. This is the perfect situation for illegal loggers to access official documents at low cost and low risk.

In 2015, OSINFOR discovered that six nonexistent communities had received local forest contracts. These communities exist only on paper and according to official records they harvested, transported, and sold timber products overseas. Likewise, OSINFOR inspectors could not find 61.5% of harvested trees reported by communities with local forest contracts, suggesting that this type of permit is becoming more popular to access to official transport form.

Forestry Law Nº 29763, which has been in place since October 2015, could change the situation. According to the new law, forest management contracts in local forests will be granted to local governments (municipalities), which will then issue permits to specific persons inside those communities. OSINFOR officials expect this new regulation to discourage illegal loggers from using these permits.

As a part of this study, we performed confidential interviews with forest concessionaires, OSINFOR inspectors, and forester engineers in the Loreto, Ucayali, and Madre de Dios regions in Peru during Summer 2016. Among concessionaires there is a general belief that OSINFOR is a punitive agency whose main objective is to close and put forest concessions out of business. Likewise, they perceive OSINFOR as an authoritarian agency that does not solicit concessionaires for suggestions to improve the field inspection process. This belief was reinforced when a new OSINFOR Supervision Manual was published in June 2016 without any prior consultation with forest concessionaires or Regional Forestry Authorities. The concessionaires' main concerns about field inspections are that

1. There is lack of knowledge among OSINFOR inspectors about tree species identification, which has generated a lot of economic sanctions for forest concessions stemming from mis-identification of tree species.
2. Economic sanctions are not graduated. Forest concessionaires are penalized the first time that an infraction is detected, which has resulting in the suspension of many concessions.
3. There is no mechanism for controlling concession areas that are suspended or canceled by OSINFOR. Illegal loggers could potentially invade these areas, out-competing legal concessionaires in the timber market.
4. OSINFOR provides little advance notice of inspections (typically fifteen days), making it difficult to organize a team to coordinate field inspections with OSINFOR and verify that they are carried out properly.
5. There are no economic or non-economic rewards or incentives for concessionaires who are found to be in compliance with Forestry Law.

Most OSINFOR inspectors for forest concessions are men between 25 and 40 years old. Field inspections are exhausting, particularly in Amazonia. Inspectors are trained for four months every year (during the Amazonian rainy season, from December to March). During that time, the inspectors also plan field inspections for the upcoming season based on the available budget. Inspectors' main perceptions about field inspections are that

1. There is no uniform criterion for estimating illegal timber in the field inspection process. Some believe that this could be solved by inspecting 100% of trees of most commercial valuable species (in addition to mahogany and Spanish cedar). In the new

Supervision Manual, all Forestry Management Plans classified as “lower intensity” will inspect 100% of trees.

2. The detected illegal timber in concessions is the minimum illegal timber harvest (lower bound) that the concessionaire could not justify by any means. The concessionaire’s interests are always favored in cases where doubt exists.
3. The most contentious issue in field inspections is tree species identification.
4. Current documents (Annual Operation Plan, extraction balance, transportation form) are insufficient to detect all illegal timber in forest concessions because the extraction balance and transportation forms have aggregate timber volume information that are not comparable to the data on individual trees in the AOP. They think that the AOP execution report is be the most important document to carry out field inspections because it has the final status information about each approved tree in the AOP after the timber operation has ended. However, this document is not submitted by the majority of forest concessionaires.
5. Cases of false AOP with no existing trees could be mitigated if the Regional Forestry Authority (which approves AOP) inspected forest concessions before approving the AOPs. Until 2015, AOPs were approved without field inspection.
6. Budget has not been a constraint on performing field inspections, even when the forest concession is far away. However, inspectors complained about the lack of a means of communication from isolated areas and think that satellite cellphones would aid communication in case of accidents, emergencies, or risky situations.

OSINFOR has taken the following measures to avoid corruption:

1. Inspectors are rotated every two years to different regions and to work with different client groups (concessionaires, native communities, and other local communities).
2. OSINFOR directors of regional offices are rotated every two years to different regions.
3. Some inspectors are sent directly from headquarters (Lima) to field inspections; they are not under the supervision of OSINFOR regional offices.
4. All inspectors are concentrated in headquarters (Lima) three months of every year for training and to reduce their dependence on the regional office.

Professional foresters have long worked as consultants for concessionaires, native communities, and mills. Prior to 2015, they were not liable for any illegal activity inside forest concessions or the illegal use of official documentation. However, under the new forestry law, they will be "regentes forestales," which means they will be legally accountable for all logging activities and official documents related to the concession (including AOP, extraction balance, transportation permits, and AOP execution report) to which they provide professional service. The new law has generated uncertainty for some foresters, who are waiting for more information before applying to be "regentes forestales." Their main observations about field inspections are:

1. In general, the estimated volume in a standing tree differs from the estimated volume actually harvested. This is true partly because of the technical challenges of estimating volume, but when it results in authorization for a greater volume than actually harvested, that creates an opportunity and an incentive to become involved in illegal timber trafficking.
2. The OSINFOR tree shape factor (0.65) used to estimate commercial timber volume is not suitable for all tree species. To estimate commercial timber volume, trees are assumed to have cylindrical shape with uniform diameter from base to the top. Under this assumption, the total volume is computed multiplying the tree height (below canopy) and DAP (diameter breast height). Given that this estimation is not realistic, OSINFOR uses a standard factor to adjust the estimate. This factor is the tree shape factor. Concessionaires consider that this factor should be estimated by specie because for some particular species the shape factor is greater than the standard.
3. Some species—such as tornillo (*Cedrelinga catenaeformis*) and shihuahuaco *(Coumarouna odorata)*— often have holes in the center of the tree, reducing the volume of commercial timber available. This problem is only detected when trees are harvested. Again, this creates an opportunity and an incentive to become involved in illegal timber trafficking in order to use the total authorized timber volume.
4. Mills should also be inspected because all timber is sent to mills (in the regional capital) to produce sawn timber before going to final markets.

5. The AOP execution report should be used as the main input in field inspections. Concessionaires should be allowed to correct any mistakes in the AOP when writing the execution report.

Finally, based on these interviews and observations of the process, we recommend:

1. Starting a technical and legal discussion about the inclusion of scientific samples in the supervision process, which will allow sample results to be inferred over populations and reduce inspectors' discretion in estimating illegal timber volume.
2. Using AOP execution reports as the main input to perform field inspections given that this report contains tree-level information. The current use of AOP, extraction balance, and transportation forms is not enough to detect all illegal timber volume inside concessions because these documents include different, non-comparable types information.
3. Including concessionaires and Regional Forestry Authorities in future discussions to update the OSINFOR Supervision Manual in order to reach general agreements about field inspection processes with all involved stakeholders in the Peruvian forestry sector.
4. Taking action to define which public institutions will control concession areas that have been cancelled. This vacuum in responsibility could allow illegal loggers to invade these areas.

1.8 CONCLUSIONS

This research quantifies the impact of field inspections on compliance with forestry law in timber concessions. This study was made possible by cooperation with OSINFOR, which provided field inspection reports from 2009 to 2014.

Misreporting timber harvest is a long-standing problem in forest concessions in Peru. Field inspection reports found that 68% of inspected tree species had been over-reported, meaning that concessionaires reported more harvested timber volume than they had actually extracted from the forest concession. Their motivation is to obtain the official documents needed to transport timber, which are used to launder illegal timber harvested from unauthorized areas.

We modeled the probability of prior inspection through a probit model. Our results indicate that a larger area of AHP (model 2) was more likely to be supervised between 2009 and 2012, suggesting that OSINFOR concentrated its initial efforts on bigger concessions because they have more timber volume and are more likely to break the law. Concessions distant from OSINFOR regional offices were less likely to be inspected in the same period, possibly because the presence of the Peruvian government in remote sites was limited in this period and the risk of performing field inspections was high. Likewise, concessions with areas inside buffer zones of protected areas were less likely to be inspected. Even though OSINFOR claimed in 2012 that location inside a buffer zone was a criterion to select concession to supervision, the results indicate that prior to this year, location in a buffer zone reduced the probability of being selected for inspection.

Given data restrictions, we implemented a single difference strategy to estimate the Average Treatment Effect on the Treated (ATET). Using propensity score matching to the single nearest neighbor (with replacement) and linear regression model for our primary model, we found that field inspection reduces misreported timber harvest (which could enable illegal timber trafficking through availability of paperwork to launder illegal timber) by 1,850 cubic meters on average which represents 17% of average total authorized harvest per inspected concessions (10,934 m3). These results are statistically significant and robust (i.e., confirmed in a linear regression with the matching covariates as controls).

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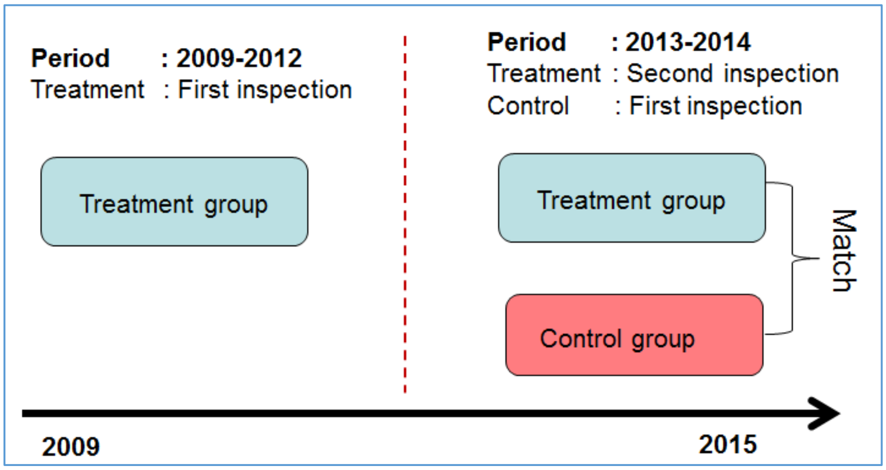

Figure 1.1: Treatment and control groups

A. Sample of concessions	B. Location of concessions relative to Protected Areas and buffer zones

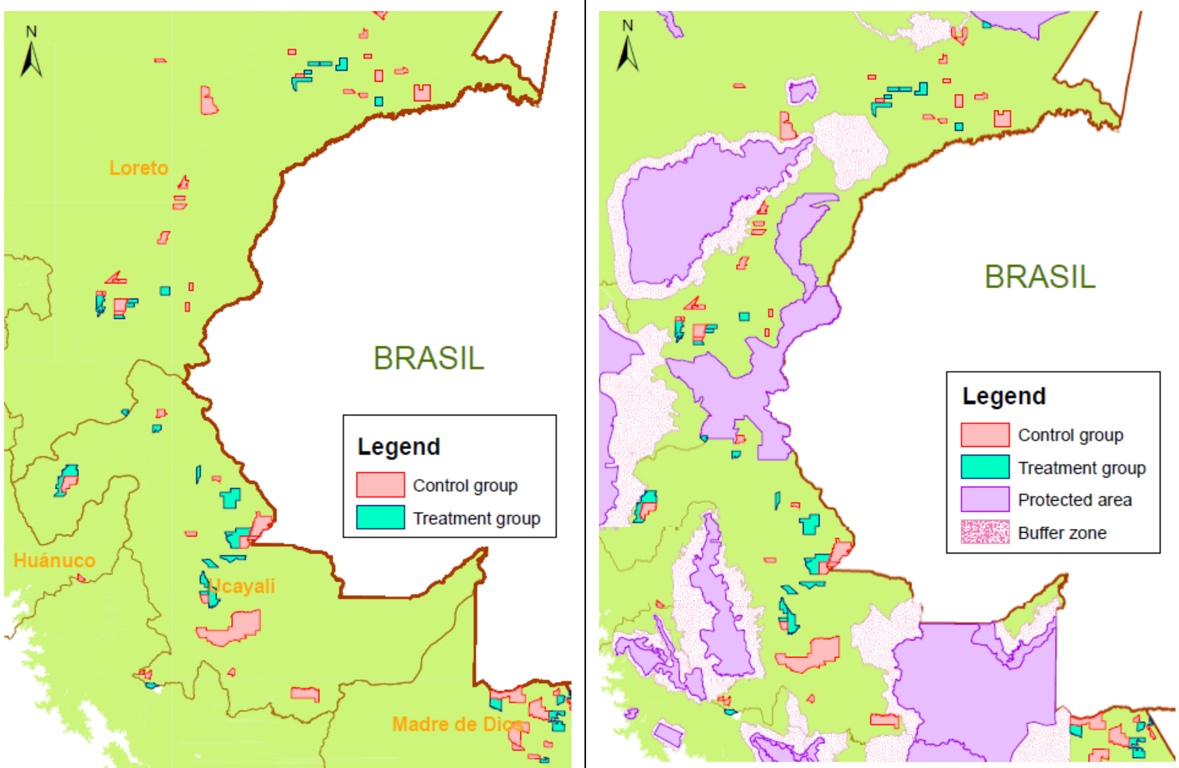

Figure 1.2: Spatial distribution of forest concessions in the study sample

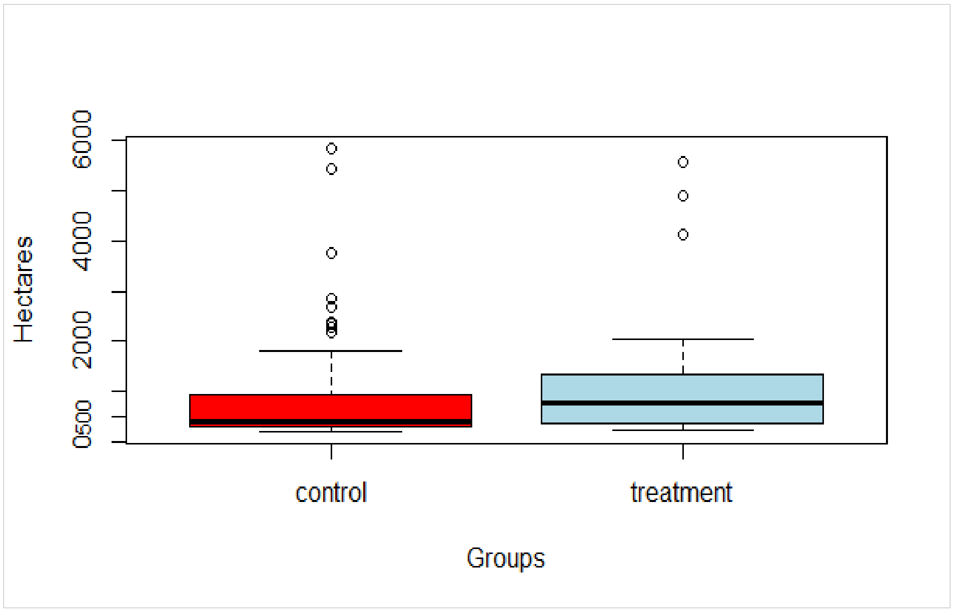

Figure 1.3: Size of AHP in control and treated concessions.

Description: the bold line represents the median, the box represents the lower quartile (25% of data less than this value) and the upper quartile (25% of data greater than this value). The outer bars represent the minimum least and maximum greatest values, excluding outliers. And, the dots represent the outliers more than and lower than 3/2 times of upper quartile or lower quartile.

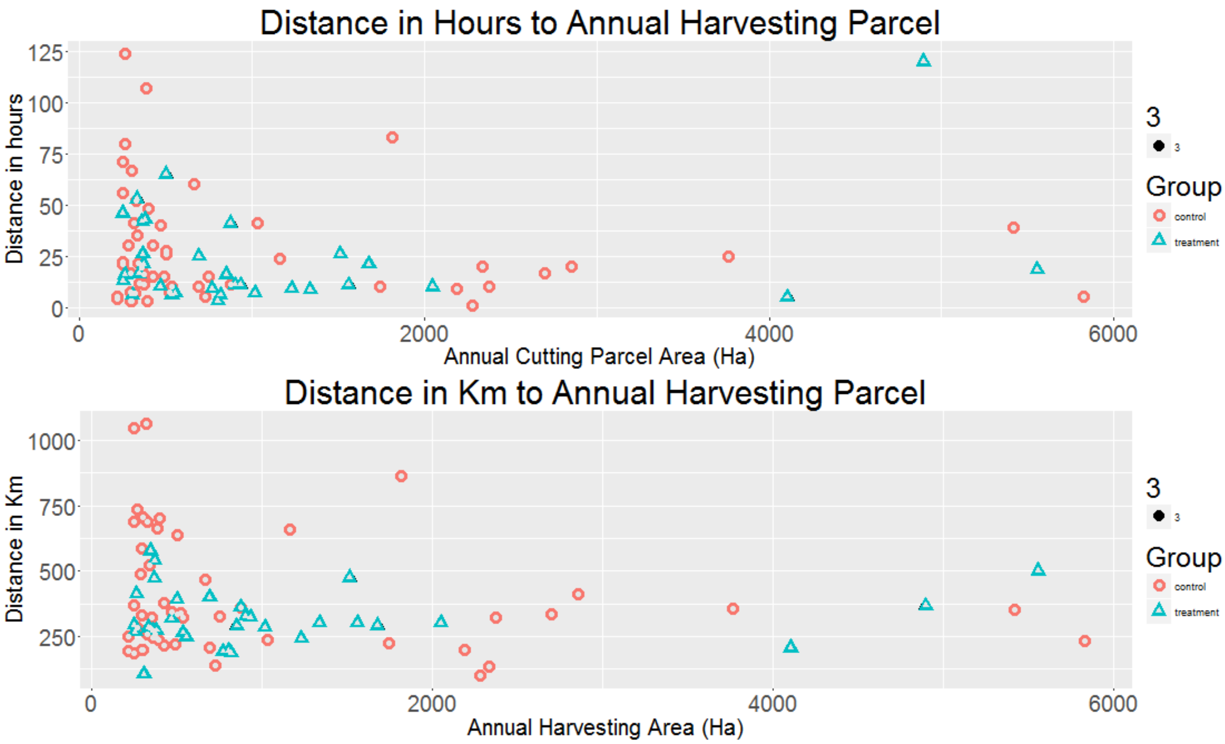

Figure 1.4: Distance from nearest OSINFOR regional office to each inspected concession

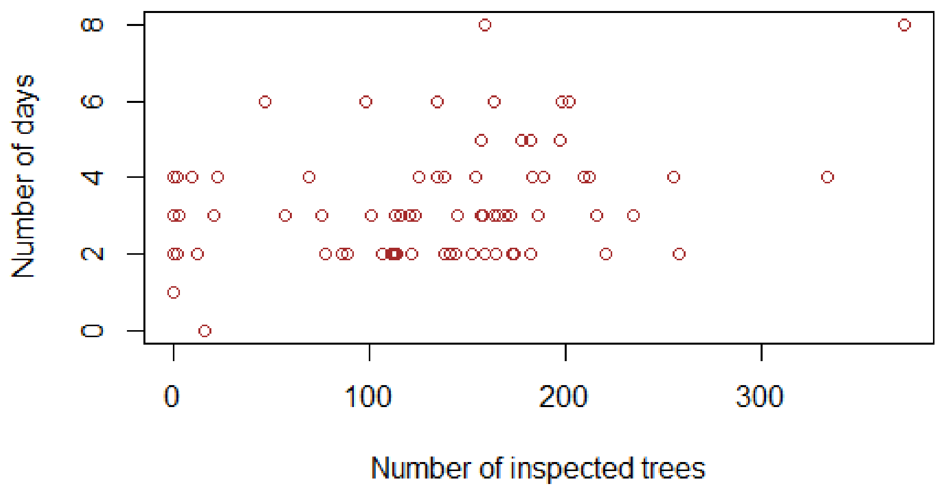

Figure 1.5: Number of days vs. number of inspected trees in inspected concessions

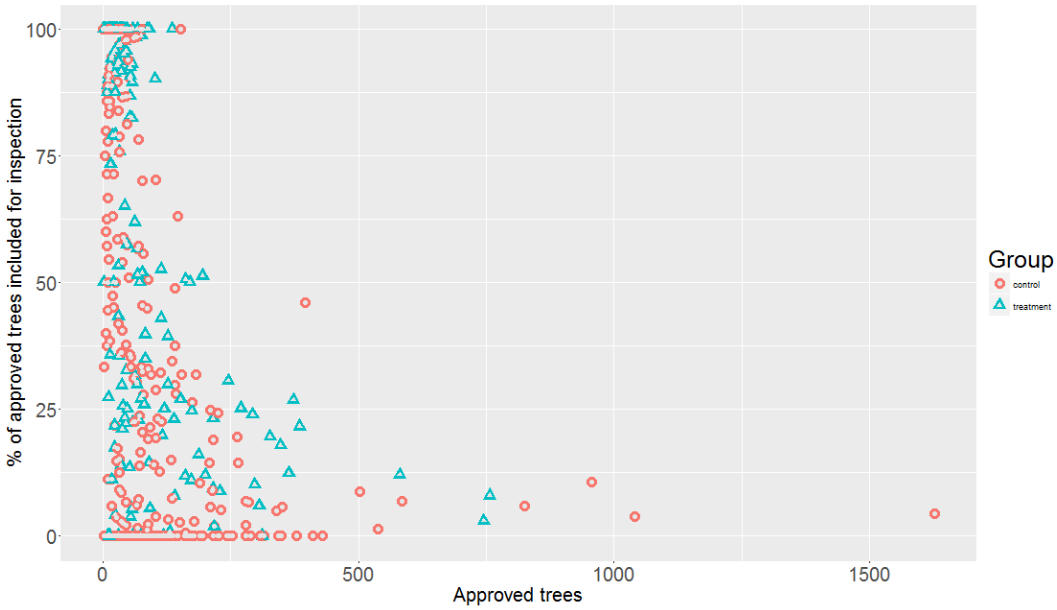

Figure 1.6: Percentage of inspected trees by total number of approved trees

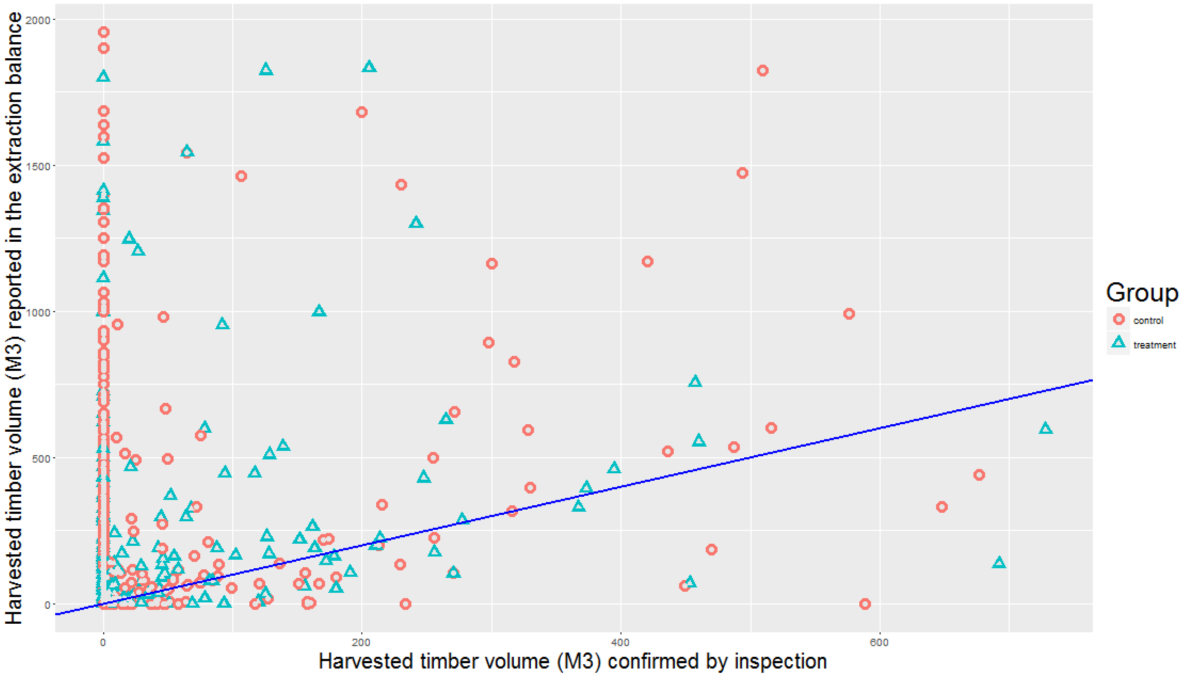

Figure 1.7: Harvested timber volume (in M^3) reported by concessionaire and inspector

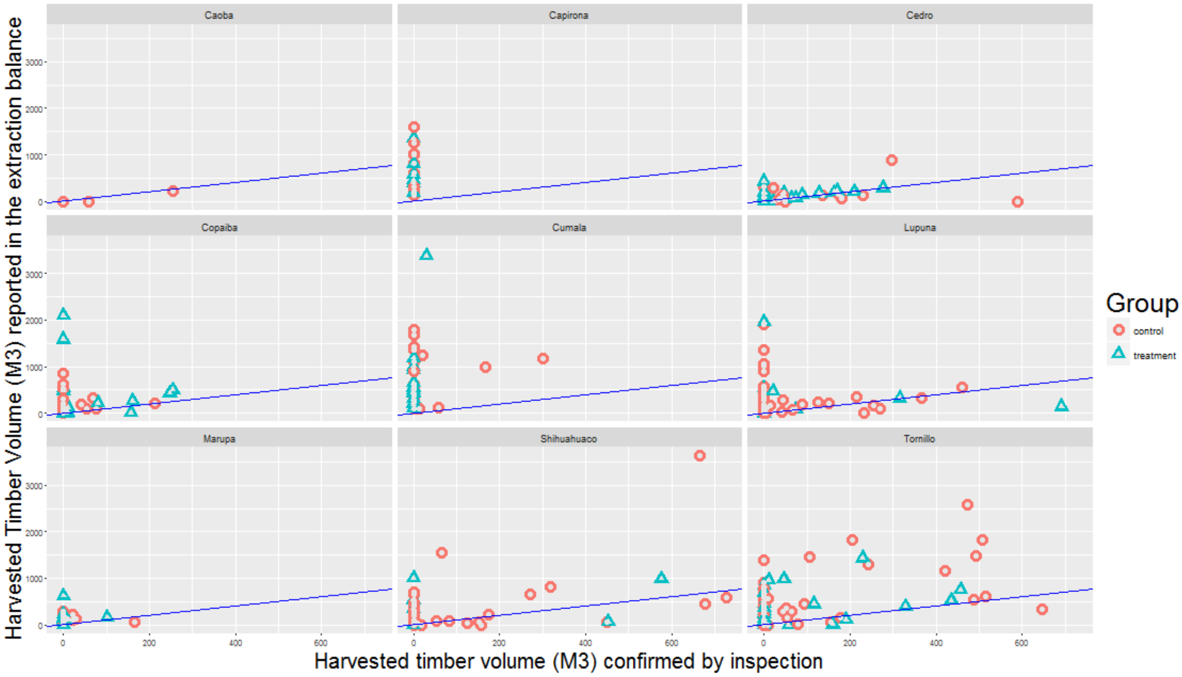

Figure 1.8: Harvested timber volume reported and inspected by species

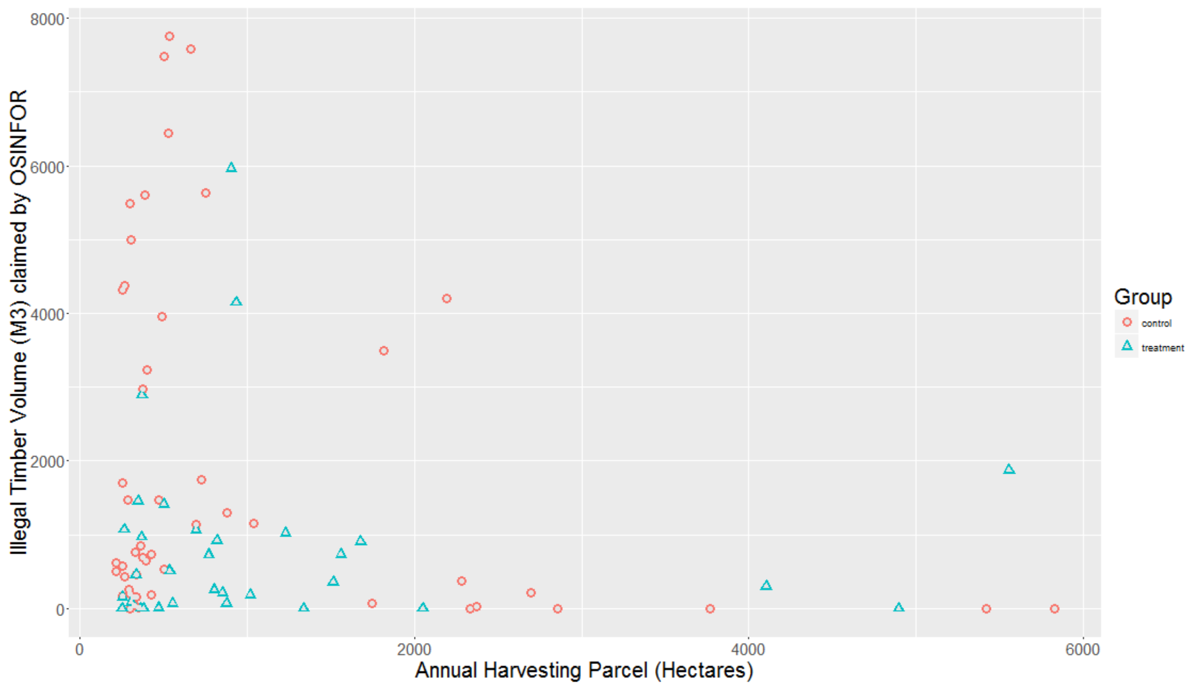

Figure 1.9: Illegal timber volume claimed by OSINFOR by size of AHP

Table 1.1: Distribution of field inspections from 2009 to 2014

Year	OSINFOR Activities		Available Data	
	Inspections*	Final resolution**	Inspection reports	% from total
Year 2009	51	11	11	21.6%
Year 2010	138	86	88	63.8%
Year 2011	74	52	66	89.2%
Year 2012	64	45	63	98.4%
Year 2013	47	32	44	93.6%
Year 2014	70	57	65	92.9%
Total	**444**	**283**	**337**	

Sources: * OSINFOR web page ** Unique Administrative Process (PAU) database.

Inspection reports are classified as follows: (1) reports with evidence of forest inspection and (2) reports with no evidence of forest inspection. The first group goes to administrative process (PAU) and the second group not. The inspection reports column includes reports with PAU in final resolution and reports that did not go to PAU. The difference between inspection reports column and total inspections in the period 2011-2014 is the number of inspection that did not reach the final resolution of PAU.

Table 1.2: Treatment group distribution by year

		Second inspection		
		Year 2013	Year 2014	**Total**
First	Year 2009	1	2	**3**
	Year 2010	5	5	**10**
	Year 2011	2	2	**4**
	Year 2012	6	6	**12**
	Total	**14**	**15**	**29**

Source: OSINFOR field inspection reports

Table 1.3: Control group distribution by year

	First inspection		**Total**
	Year 2013	Year 2014	
Total control	14	38	52

Source: OSINFOR field inspection reports

Table 1.4: Descriptive statistics of forest concessions in the study groups

Variable	Group	Obs	Mean	Standard Deviation	SDM*	Ratio of variances (T/C)
Concession area (has)	Control	52	20,037.7	30,841.3	-0.120	0.195
	Treatment	29	17,174.3	13,614.0		
Annual Harvesting Parcel (has)	Control	52	981.8	1,250.3	0.166	1.190
	Treatment	29	1,199.4	1,364.0		
Distance (hours)	Control	52	28.0	27.2	-0.194	0.804
	Treatment	29	23.0	24.4		
Distance (km)	Control	52	394.1	227.1	-0.375	0.231
	Treatment	29	327.2	109.2		
Inspection days (fieldwork only) in 2013 or 2014	Control	52	3.0	1.4	0.367	1.146
	Treatment	29	3.6	1.5		
Approved timber volume (m^3) in 2013 or 2014	Control	52	11,528.8	16,106.3	-0.131	0.243
	Treatment	29	9,866.5	7,940.1		
Reported harvested timber volume (m^3) in 2013 or 2014	Control	52	4,223.7	4,641.5	-0.097	0.738
	Treatment	29	3,802.1	3,988.3		
Approved timber volume not reported harvested (m^3) in 2013 or 2014	Control	52	7,305.1	13,793.2	-0.117	0.192
	Treatment	29	6,064.4	6,043.1		
Fines (US$) accumulated until 2013 or 2014	Control	52	50,640.6	91,778.8	-0.105	1.557
	Treatment	29	39,724.6	114,522.9		
Concession inside buffer zone of protected area(s)	Control	52	0.15	0.36	-0.429	0.279
	Treatment	29	0.03	0.19		
Concession with CITES species	Control	52	0.40	0.50	-0.041	0.960
	Treatment	29	0.38	0.49		

*Standardized Difference in Mean

Source: OSINFOR field inspection reports

Table 1.5: Species-level descriptive statistics in the study groups

Doc	Variable	Unit	Group	Obs	Mean	Standard Deviation	SDM*	Ratio of variances (T/C)
Information from Annual Operation Plan	Approved by Regional Forestry Authority	Tree	control	454	84.7	131.7	-0.037	0.894
		Tree	treatment	181	80.1	115.4		
		Volume	control	454	580.2	1214.6	0.029	1.111
		Volume	treatment	181	611.5	923.7		
	Reported harvested timber	Volume	control	454	357.7	596.4	-0.068	0.800
		Volume	treatment	181	319.8	510.2		
Information from field inspection reports	Inspected timber in standing trees	Tree	control	454	6.1	11.6	0.678	7.312
		Tree	treatment	181	16.4	18.1		
		Volume	control	454	47.5	128.3	0.550	6.997
		Volume	treatment	181	125.5	154.4		
	Inspected timber in stumps	Tree	control	454	3.2	9.4	0.283	3.672
		Tree	treatment	181	6.2	11.4		
		Volume	control	453	28.7	85.5	0.251	3.440
		Volume	treatment	181	53.3	108.8		
	Inspected timber lying in the ground	Tree	control	454	1.7	7.2	-0.049	0.691
		Tree	treatment	181	1.4	3.7		
		Volume	control	454	9.9	40.3	0.064	1.563
		Volume	treatment	181	12.4	36.5		
	Illegal timber	Volume	control	454	254.2	344.6	-0.311	0.364
		Volume	treatment	181	153.3	303.4		

* Standardized Difference in Mean

Source: OSINFOR field inspection reports

Table 1.6: Probability of prior inspection

Variable	Unit	Model 1	Model 2
Annual Harvesting Area (AHA)	Has		0.30411
			[0.00014]*
Concession area	Has	0.01268	0.00511
		[0.00001]	[0.00001]
Traveled distance from office	Km	-0.00193	-0.00202
		[0.00078]*	[0.00080]*
Presence of CITES specie	Dummy	-0.08677	-0.09978
		[0.30897]	[0.30313]
Approved timber volume	M3	-0.03717	-0.04994
		[0.00003]	[0.00004]
Reported harvested timber volume	M3	0.03790	0.05091
		[0.00006]	[0.00006]
Concession area inside buffer zone of protected area	Dummy	-1.21292	-1.21040
		[0.61714]*	[0.65188]+
Pseudo R-squared		0.07600	0.10300
Observations		**81**	**81**

+ p<0.1; * p<0.05; ** p<0.01

Note: Standard errors in square brackets and númeric variables scalated by 1,000

Table 1.7: Balance test for propensity specification of model 1

Variable	Group	Obs	Mean	Standard Deviation	SDM* Model 2	Ratio of variances (T/C)
Concession area (Has)	Control	18	16,442.8	15,742.0	0.050	0.748
	Treatment	29	17,174.3	13,614.0		
Distance in Km	Control	18	304.2	93.7	0.227	1.359
	Treatment	29	327.2	109.2		
Approved timber volume	Control	18	10,278.7	6,964.8	-0.055	1.300
	Treatment	29	9,866.5	7,940.1		
Reported harvested timber volume	Control	18	4,183.9	3,500.1	-0.102	1.298
	Treatment	29	3,802.1	3,988.3		
Concessions inside protected areas buffer zone	Control	18	0.06	0.24	-0.102	0.621
	Treatment	29	0.03	0.19		
Concession with CITES species	Control	18	0.39	0.50	-0.020	0.969
	Treatment	29	0.38	0.49		

* Standardized Difference in Mean

Table 1.8: Balance test for propensity specification of model 2

Variable	Group	Obs	Mean	Standard Deviation	SDM* Model 3	Ratio of variances (T/C)
Concession area (Has)	Control	16	16,753.7	16,542.7	0.028	0.677
	Treatment	29	17,174.3	13,614.0		
Annual Harvesting Parcel (Has)	Control	16	844.6	848.8	0.312	2.582
	Treatment	29	1,199.4	1,364.0		
Distance in Km	Control	16	340.6	138.0	-0.108	0.626
	Treatment	29	327.2	109.2		
Approved timber volume	Control	16	9,926.1	7,304.8	-0.008	1.182
	Treatment	29	9,866.5	7,940.1		
Reported harvested timber volume	Control	16	4,887.51	5,405.31	-0.229	0.544
	Treatment	29	3,802.11	3,988.30		
Concessions inside protected areas buffer zone	Control	16	0.06	0.25	-0.131	0.552
	Treatment	29	0.03	0.19		
Concession with CITES species	Control	16	0.25	0.45	0.281	1.219
	Treatment	29	0.38	0.49		

* Standardized Difference in Mean

Table 1.9: Average Treatment Effect over Treated – PSM with nearest neighbor

Propensity Model	ATET	Standard Error	P-value	Lower bound on CI	Upper bound on CI	Matched control obs
Model 1	**-1,874.7**	803.2	0.02	-3,448.9	-300.5	18
Model 2	**-1,808.2**	1,372.9	0.19	-4,499.0	882.6	16

Table 1. 10: Average Treatment Effect over Treated – Kernel matching

Propensity Model	ATET	Standard Error	P-value	Lower bound on CI	Upper bound on CI
Model 1	**-947.8**	711.3	0.09	-2,343.5	447.8
Model 2	**-1,512.8**	714.8	0.02	-2,915.2	-110.5

Table 1.11: Ordinary Least Square results

		Model 1	Model 2
Variable	**Unit**	**Illegal timber volume (M3)**	
Treatment (prior inspection)	Dummy	-1,821.05	-830.18
		[455.26248]**	[527.37906]
Annual Harvesting Area	Has		-0.17
			[0.25857]
Concession area	Has	-0.02	0.03
		[0.02630]	[0.03979]
Traveled distance	Km	7.71	9.71
		[2.10480]**	[3.67935]*
Presence of CITES species	Dummy	-1,230.91	-1,740.00
		[482.70461]*	[633.66146]**
Approved timber volume	M3	-0.05	-0.16
		[0.05325]	[0.11480]
Reported harvested timber volume	M3	0.43	0.73
		[0.14889]**	[0.16451]**
Concession with areas inside buffer zone of protected area	Dummy	-588.63	-1,615.34
		[2,003.37712]	[2,761.78930]
R-squared		0.61	0.73
Observations		**58**	**58**

+ *p*<0.1; * *p*<0.05; ** *p*<0.01

Note: Standard errors in square brackets

CHAPTER 2

ASSESSING THE IMPACT OF REDD+ INTERVENTIONS ON HOUSEHOLD INCOME IN PERU[10]

2.1 INTRODUCTION

This research assesses the impact of REDD+ initiatives on household forest incomes in the Peruvian Amazon. We focus our analysis on two subnational REDD+ initiatives in Ucayali and Madre de Dios, Peru that are part of CIFOR's Global Comparative Study of REDD (GCS-REDD).

We draw on GCS-REDD for survey data from before and 2 years after the launch of the two initiatives. Because of this short time horizon, we focus on whether the initiatives have accomplished their immediate goal of increasing income from sustainable forest management, in order to promote both forest conservation and improved well-being over the long-run. Thus, we model changes in household income from the forest sector, which is assumed to influence behaviors leading to deforestation and forest degradation. This means that we are not evaluating whether the REDD+ initiatives had an impact on deforestation or forest degradation but instead whether the mechanisms introduced by proponents are generating noticeable incentives for participating households to reduce deforestation and forest degradation. We estimate the effects of the initiatives on income by combining difference-in-difference and propensity score matching methods.

This paper is organized as follows. In the background section, we present the general idea about REDD+ initiatives, typical limitations for assessing the impact of this type of projects, the framework of the quasi-experimental approach used to assess the impact of REDD+ initiatives on household incomes related to forest, and a brief description of the initiatives that we are evaluating are presented in logic models in Appendix 6 (Ucayali) and Appendix 7 (Madre de Dios). In the Methods section, we present our step by step procedure for implementing Difference in Difference (DiD) with Propensity Score Matching (PSM).

[10] Co-authors: Peter Cronkleton and Erin Sills

We also describe the alternative quasi-experimental approach used to check the robustness of our findings.

In the results section, we present our estimates of the impacts of the REDD+ initiatives on household income derived from the forest. First, we used the DiD method to assess the impact of REDD+ on forest income, using the full sample. Then, we estimate the same DiD models in a matched sample of households constructed with PSM. Finally, using DiD with PSM, we estimate the impacts of specific components of the REDD+ initiatives. We close the paper with discussion and conclusions.

Our main finding is that the REDD+ initiatives have not affected household forest income in the Peruvian sites. We confirm that this is robust to our definition of treatment: neither participation in the REDD+ initiative as a whole nor participation in specific components of the initiative has had any detectable impact on household income from the forest. Robustness checks confirm our results. We discuss implications in the discussion and conclusions.

2.2 BACKGROUND

In theory, REDD+ initiatives provide incentives (and disincentives) to encourage forest owner to lower deforestation and forest degradation to limit carbon emissions and sequester carbon (Angelsen, et al., 2012). To reach these goals, REDD+ projects use multiple approaches including conditional payments (or carbon payments), forest law enforcement, development interventions, and capacity building (Petkova et al., 2010; Sills et al., 2014; Luttrell and Betteridge, 2017). All these approaches could generate co-benefits such as increased forest income as an incentive to avoid deforestation and degradation.

The goal of REDD is to reduce greenhouse gases emissions from forests in developing countries to mitigate climate change. In recent years, a plus sign was added (REDD+) to include conservation of forest carbon stocks, sustainable management of forest, and enhancement of forest carbon stocks.

Although the countries party to the UNFCCC agreed on the basics of the REDD+ mechanism at the Paris COP in 2015, the specifics are still under development, including how to safeguard indigenous and other local communities, and how to set carbon reference levels. In the last years three conceptualizations of REDD+ were developed such as (1) carbon-centered where REDD+ aims, mainly, to mitigate climate change, (2) co-benefit centered where REDD+ aims to deliver, additionally to conserve forest, environmental and social benefits, and (3) landscape-centered where REDD+ is expected to be part of an integrated sustainable land-use approach (Thurnout et al., 2017). the solution of drivers of deforestation. The further development of REDD+ should be informed by the experience to date with the mechanism. This includes sub-national projects implemented by Non-Governmental Organizations (NGO) and private companies, to pilot REDD+ demonstration activities. Most of these projects have been third party certified by the Climate, Community and Biodiversity Standard (CCBS) and the Verified Carbon Standard (VCS). This third-party certification is a necessary step for negotiating a carbon trade in international voluntary markets.

Most REDD+ projects around the world are at an early stage, many less than a decade old. Hence, they can only provide evidence on the short-term effects of REDD+ as a mechanism for reducing carbon emissions from deforestation or degradation. However, if the REDD+ activities are working as planned, we should be able to observe measurable responses to the incentives (i.e. increased incomes) offered to participants. Caplow et al. (2010) reviewed several papers related to impact evaluation of REDD+ (or the earlier label of “avoided deforestation”) interventions around the world, and they found that most use unscientific methods, specifically, they lack valid counterfactual scenarios for establishing an impact.

Bos et al. (2017) studied the impact of REDD+ in reducing deforestation. This study was carried out for 23 REDD+ projects included in the GCS and used Global Forest Change Data (Hansen et al. 2013). It included two levels of analysis: (1) meso-level assessment (REDD+ initiative boundaries) and (2) micro-level assessment (intervention village boundaries). Their findings show an overall minimal impact of REDD+ in reducing deforestation; however, micro-level assessment shows better results.

Evaluating impacts of subnational REDD+ initiatives is a challenge because in many cases researchers do not have baseline data on both households that will be affected by the REDD+ activities and similar households that are not participating and that will not be affected. This makes it difficult to construct the counterfactual. With these data limitations, it is difficult to know if observed change is caused by treatment or other contextual factors.

When baseline data are available, researchers can employ Difference in Difference (DiD) methods. Did is a quasi-experimental approach that compares individuals from intervention and control groups both before and after an intervention to get an unbiased measure of the treatment effect, called the Average Treatment Effect on Treated (ATET). In other words, it compares the difference in outcomes for participants before and after a treatment with the difference in outcomes for others that did not participate in the treatment. DiD was developed to evaluate social and economic phenomena where selection of participants was not random. The main goal of such a quasi-experimental approach is to estimate the ATET, which is a statistical measure of the quantitative impact or change due to the intervention, program, project or public policy. In theory, the ATET is computed as follows:

Equation 1:

$$\mathrm{ATET\ DiD} = \mathrm{E}\left[\mathrm{D}_{1i}^{t=1} - \mathrm{D}_{1i}^{t=0} \middle| \mathrm{T} = 1\right] - \mathrm{E}\left[\mathrm{D}_{0i}^{t=1} - \mathrm{D}_{0i}^{t=0} \middle| \mathrm{T} = 1\right]$$

Where D_{1i} represents the outcome for household i if it participates in the intervention and D_{0i} the outcome if does not participate. Households in the treatment group are identified as $\mathrm{T} = 1$. Likewise, t indicates period of outcome (0 before and 1 after).

Estimating the ATET (equation 1) would be trivial if we could observe the same household simultaneously participating in the treatment and not participating in the treatment. However, it is impossible for a household to belong simultaneously to the treatment group and the control. This means that we cannot observe the second term on the right side of equation 1. One solution is to identify a control group similar to the treatment group, but that does not participate in the treatment. We can then compare the original treatment group to this control group to observe how they change over the same period of time, before and after the intervention. In other words, the control serves as the best counterfactual of what would have happened if the treated group had skipped the intervention.

The DiD design entails one key assumption to identify an impact. This is that selection bias is constant over time. For example, if some internal village agreements encourage some villagers to participate and other not, then these agreements must remain the same during the base line and evaluation year, in order to have time-invariant selection bias. This implies that the potential outcomes for the treated and control group follow parallel trends so that the outcome of the control group reflects what the outcome for the treatment group would have been if they had not participated in the intervention (i.e. the untreated potential outcome of the treated). Mathematically, this is represented by the following equation.

Equation 2:

$$E\left[D_{0i}^{t=1} - D_{0i}^{t=0} \middle| T = 1\right] = \mathrm{E}\left[D_{0i}^{t=1} - D_{0i}^{t=0} \middle| T = 0\right]$$

This assumption means that the ATET represented in equation 1 becomes:

Equation 3:

$$\mathrm{ATET\ DiD} = \mathrm{E}\left[\mathrm{D}_{1\mathrm{i}}^{\mathrm{t}=1} - \mathrm{D}_{1\mathrm{i}}^{\mathrm{t}=0} \middle| \mathrm{T} = 1\right] - \mathrm{E}\left[\mathrm{D}_{0\mathrm{i}}^{\mathrm{t}=1} - \mathrm{D}_{0\mathrm{i}}^{\mathrm{t}=0} \middle| \mathrm{T} = 0\right]$$

The DiD method eliminates selection bias from time-invariant non-observable variables. For instance, the proponent may select villages for REDD+ intervention based on their relationship with leaders. This affinity is difficult to observe or measure, particularly after the fact when collecting data to evaluate the project. The researcher does not know how such factors affect the outcome but can sweep out their influence using DiD. Likewise, non-observable factors related to institutional agreements, arrangements inside communities, past interventions and familiarity of villagers with interventions, expectations of benefits or socio-economic or racial prejudice (i.e. selection of participants that are more like the socio-economic or cultural background of technicians) could drive participation by some members instead of others.

There are two possible sources of selection bias in this study, bias by the researcher/proponent and bias by the participants/informants. The first source of bias occurs when proponents (or the project implementer) select communities, household or individuals to participate in the intervention using particular criteria. The second source of bias results from decisions by targeted people to participate in a treatment or not. These two sources of bias, whether introduced by the proponent or by potential participants, are different and must be controlled to get an unbiased estimation of the ATET.

The steps for computing the ATET using the DiD method (equation 3) start with data collection of before and after observations of the control and treatment groups. With the data organized into four groups -- treatment before, treatment after, control before, and control after--compute the base line, which is the difference between the treatment and control group means before the intervention. Then compute the difference between the treatment group and control group means after the intervention. Finally, calculate the difference between the outcomes prior to the intervention minus the observed difference between outcomes after the intervention. These two continuous differences eliminate the selection bias, which is assumed constant over time. The result is the Difference in Difference, which represents the impact of the intervention (if it exists).

It is possible to add an additional level of precision to the DiD method by comparing similar individuals in the intervention and control groups. This also helps make the assumption of parallel trends more credible. In such a case, we would use other information we have about sampled individuals to match similar households in the intervention and control groups. We use Propensity Score Matching (PSM) to determine which individuals should be paired using observable characteristics before computing DiD. Matching is a multi-dimensional problem because there are a variety of observed characteristics that we can use to compare individuals in the treatment and control groups. Rosenbaum and Rubin (1983) proposed using the propensity score to summarize all information about the observed characteristics. The propensity score is the likelihood of participation in the treatment. This is computed for treated and control individuals using pre-treatment observable variables.

Then, the individual probability values to participate in the treatment are used to compare individuals from treatment and control groups. This requires that the probability distributions of the likelihood to participate for treated and control groups overlap. We used a logit model to estimate the probability of participating in the REDD+ projects.

The matching process is implemented with 'single nearest neighbor matching with replacement'. This method is an algorithm that matches each individual from the treatment group to the individual in the control group with the nearest propensity score. It selects only the best match for each treated unit. Individuals from the control group can be re-used, i.e. matched with multiple individuals in the treated group. Several studies recommend 'replacement', or not withdrawing units once they are matched, because it allows better matches (Caliendo 2008; Austin 2011).

PSM builds a robust control group that is statistically similar to the treated group. This is a powerful technique because it selects the best match for each treated individual from the universe of potential individuals in the control group. However, PSM can only control for observable characteristics. By combining with DiD, unobserved and time-invariants characteristics are also swept out. In this case, the ATET estimation is:

Equation 4:

$$ATET\ DID = E\left[D_{1i}^{t=1} - D_{1i}^{t=0} \middle| X, T = 1\right] - \mathrm{E}\left[D_{0i}^{t=1} - D_{0i}^{t=0} \middle| X, T = 0\right]$$

An alternative approach is Kernel Matching. Like PSM, Kernel matching judges similarity based on observable characteristics. The key difference from the PSM method is that kernel matching uses all control individuals. It implies that the full sample is used to compute the impact estimation, with different weights place on each control observation.

2.2.1 GCS-REDD and the BACI model

We used DiD method to evaluate the impact of REDD interventions in the Peruvian Amazon, using data generated by CIFOR's GCS-REDD project. We use annual forest income as the outcome in evaluation, which is computed as the addition of each harvested forest product (consumed or sold) valued at local price available at the time of interview.

CIFOR’s GCS design implemented a BACI approach (Before, After, Control, Intervention). This approach track interventions over time (before and after) to monitor impact and make a comparison on intervention communities with similar control communities to isolate effects. This approach is theoretically a strong research design because it helps to model the selection mechanism; it allows disentangling the effects of intervention from contemporaneous policy, markets, social, economic, and environmental changes; and it eliminates bias from time invariant factors even when these factors are not observable (Sills et al., 2017).

In 2010, CIFOR initiated a Global Comparative Study (GCS) to evaluate impacts on 23 sub-national REDD+ projects in six countries (Perú. Brazil, Cameroon, Tanzania, Indonesia, and Vietnam). This is a multi-country comparative research project to monitor and evaluate implementation covering 150 communities (participants and non-participants in REDD+), and around 4,000 households

In the two REDD+ projects that we are evaluating, the AIDER and BAM proponents did not randomly select households from the rural households in each region, but instead selected groupings of households using criteria and affinities that we could not observe. Given that selection of participants was not systematically random, it is not known how bias may have influenced selection of participants in these two projects. To measure whether the REDD+ interventions were providing significantly different benefits, it was necessary for CIFOR’s GCS team to select similar control communities to provide a counterfactual case for observing impact.

According to GCS technical documentation (Sunderlin et al. 2013), intervention and control villages were selected through a matching process. “For intervention villages, 16 REDD+ project villages were identified where the proponent has specific intervention targets and approaches. In case that the REDD+ project had more than 16 villages the list was reduced by selecting those villages that had a level of deforestation and degradation that was higher than average or average compared to the mean for all project villages in the last five years. Then, a village appraisal form was filled out to collect information to be used in the matching process. For control villages, 16 villages were selected outside of project boundaries, but there are some exceptions where candidate control villages where located inside project boundaries. Candidate control villages were required to: 1) they will never be subjected to REDD+ interventions, and 2) they should be far enough from intervention villages to avoid spillover effect. The same village appraisal form was carried out for each candidate control villages. Using information from village appraisal form, a covariate matching technique (Mahalonobis) was carried out to match intervention and control villages including the following covariates: deforestation pressures, NGO presence, strength of forest tenure, number of active community groups, population size, extent of forest cover around village, high forest dependence, and distance to main road. Finally, four intervention and control villages were identified considering that index[11] values were closest as measured by balance in mean values of each of the eight included variables".

In Peru, the GCS of REDD+ project has conducted two rounds of data collection on two sub-national REDD+ projects in 2012 and 2014. One of these sub-national projects was initiated by the NGO Asociación para la Investigación y Desarrollo Integral (AIDER) in collaboration with seven native communities in the Ucayali region of the Peruvian amazon. This project covers 127,004 hectares of community forest. AIDER has been actively working in these communities since 2000 assisting them with a forest management program by providing technical support for the preparation of Community Forest Management Plan and the associated Annual Operation Plans. Additionally, in 2006, four of these communities participated in FSC certification process. The AIDER project built on earlier community forestry initiatives with the intention to increase household forest income from timber and non-timber forest products to reduce deforestation. The AIDER project included a number of distinct interventions such as the establishment of timber plantations using the fast growing pioneer species bolaina (Guazuma crinite) plantation in 2013, and programs for the sustainable management of NTFP including tananoni (Thevetia peruviana), aguaje (Mauritia flexuosa) and huayruro (Ormosia coccinea). It also included a capacity-building program to

[11] The index is a weighted average of the eight variables for every site, where the weights were the inverse of the elements of variance-covariance matrix of this set of eight variables. The index is called the Mahalonobis metric.

train villagers in sustainable forestry management practices. An eventual goal of the AIDER REDD+ project is to distribute carbon revenues among community residents as incentive to avoid deforestation and forest degradation but at the time of the study, this component was only a plan and no carbon benefit-sharing arrangement is operational.

CIFOR's GCS project gathered data from four of the participating native communities and identified four similar indigenous communities that are not participating in the AIDER initiative. In total, 204 households were interviewed in base line and follow-up survey (109 treated and 95 controls).

The second case is a REDD+ project organized by a private forestry company Bosques Amazónicos (BAM) in the southern Amazonian region, Madre de Dios. This project was jointly implemented with Federation of Brazil Nut (BN) producers in Madre de Dios. Up to December 2015, 405 BN concessionaires had joined this project with concession areas totaling approximately 308,738 hectares of forest. The participating BN concessionaires commit to avoid deforestation within their BN concessions and transfer the related Greenhouse gas emission reductions rights to the Federation. Conversely, the BAM REDD+ project provided technical assistance to participants relating to the preparation of concession management documents (including the Forest Management Plans, the Annual Operation Plan, and Complementary Plans for timber harvests) and other legal assistance. They also assist in delineating concession boundaries. As part of the agreement, the Federation transferred the rights to hold and commercialize carbon credits to BAM. BAM pledged to invest at least US$1 million in the REDD+ project and to the construction of a BN processing facility. Additionally, after reaching the break-even point of the REDD+ project BAM commits to distribute 30% of net revenues of carbon credits to participating BN concessionaires. The main objective of this REDD+ project is to maintain the current forest cover in BN concessions through improvement land tenure, compliance with forestry law regulation, and increasing price of BN by adding aggregate value. Moreover, the proponent expects that carbon payments to household will be a component of the incentive structure eventually. However, up to date this has not happened.

The GCS project gathered data from four REDD+ participant communities and four non-participants communities. Different from Madre de Dios site, these communities are not indigenous; most of community members are migrants from southern Peruvian Andean regions. In total, 207 households were interviewed in base line and follow-up survey (113 treated and 104 controls).

2.3 METHODS

To evaluate whether the selected REDD+ projects were having significant impact on participating households, we evaluated whether the forest income of participating households increased. For each project, we first compared control and intervention sites using the GCS-REDD+ BACI approach using a DiD method. Here, we just compared mean outcome between treated and control households which have information before and after intervention (panel data). We then used the same data set to compare matched samples using DiD with PSM. Here, we implemented single nearest neighbor matching (1:1 matching) with replacement. We then conducted the same types of matched comparisons but focused on subsets of informants from intervention communities that reported participating in specific interventions. This final test concentrated on households that were apparently most engaged with project activities and therefore were likely to have benefited if any households had. We implement the impact estimations with "psmatch2" routine in STATA 13.

We used a single nearest neighbor matching algorithm (1:1 matching) with replacement following these steps. First, we computed the propensity score (probability to be treated) for the entire samples from Ucayali and MDD (repeated for each site). Second, we sorted the propensity score from 0 to 1 for all individuals in the sample. Third, we then matched the propensity score of first individual in the treated group to their counterpart in the control group with the closest propensity score. After finding the best match, we continued with the next individual from the treated group, repeating the process so on down the list. Because matched individuals from the control group can be replaced in the selection pool, they could be considered for matching with multiple individuals from the treated group. In the end, only a subset from the control group was paired. That sub-group is the final matched control group with the best match.

However, it is necessary to test whether the new control sub-group accurately reflects the treated group, in other words, whether it is the best counterfactual for the treated group. To do this, we implemented a balance test to verify that there is no systematic difference between the control and treated groups in the means for variables included in the propensity model. Heckman, Ichimura and Todd (1997) suggest that pre-treatment variables that influence simultaneously the participation decision and the outcome variable should be included in the propensity model (probability to be treated). To know which variables complies these criteria we have to use the economic theory, previous research findings, and specific knowledge about the institutional settings in the study area. We used the Standardized Difference in Mean[12] (Austin, 2011) to evaluate the difference between the groups. If there is no systematic difference in pre-treatment variables, the variables are considered 'balanced'. To determine whether there is a systematic difference in means between the two groups, we use a value greater than 0.10, in absolute value, as suggested by Austin (2011). If the pre-treatment variables were not balanced in the first matching algorithm implementation, we changed the specification of the propensity model (probability to be treated) using a different set of pre-treatment variables as well as artificial variables[13]. We repeated the balance test of pre-treatment variables (excluding the artificial variables) until there was balance.

It is typical when doing impact evaluation to do a robustness check. Therefore, as a robustness check of the ATET estimation by DiD with PSM, we used a Kernel matching method with Epanechnikov distribution and bandwidth of 0.06. Similarly to PSM, Kernel matching procedure matches individuals from treatment and control groups based on similarity in their propensity scores. In this case, each member of the control group is paired to one member of the treated group based on the closeness of their propensity scores. Then, Kernel matching generates a 'weight' for each matched pair, which represents the closeness of the propensity score of the control subject with respect to the propensity score of the

[12] SDM is robust to sample variation, which provides a consistent comparison between treated and control groups. Likewise, this estimator is not subject to an assumed parametric functional form, for example, T test uses a T distribution to evaluate the significance of an estimator

[13] These are pre-treatment variables powered to quadratic or cubic. Likewise, we included some interactions between variables.

participant. Finally, the ATET estimation is computed as weighted average of the individual difference in the outcome variable between each matched pair.

To make a consistent comparison between nominal monetary values from different years, we used the consumer price index from World Bank to correct for inflation effect in forest income, which is the outcome variable we evaluated from the REDD+ projects.

We repeated the DiD with PSM with specific interventions that were components of the REDD+ initiatives implemented by the proponents. As the residents self-reported participation in these interventions, we expect that these individuals were likely more engaged and thus more likely to have benefited from the intervention. In Ucayali, we selected participation in a Forest Management Training course which is part of the REDD+ project implemented by AIDER. In this case, we evaluated the change in timber income because this specific activity was intended to increase household timber income in the treated communities. In Madre de Dios, we examined the impact of BAM administrative assistance, which involved legal and technical support to help Brazil nut gatherers comply with forestry regulations and delimit the concession boundaries. In that case, we evaluated the impact of this specific activity on household Brazil nut income. Here, we changed the outcomes in evaluation in both sites because the specific interventions were design to increase the specific incomes related to forest as incentive to avoid deforestation or forest degradation.

2.4 RESULTS

2.4.1 DiD analysis

First, we performed DiD analysis with GCS BACI data. Figure 2.1 shows the annual average forest income for each group in the base line and assessment year. Among the control communities in Ucayali, during 2012 baseline survey, the average forest income was US$11,411 while in the intervention communities it was US$7,739. In the 2014 survey, the average incomes were US$ 1,824 and US$ 2,392 respectively. These averages show a drastic decline (more than 60%) in forest income between base line and follow-up survey. Without any analysis, it appears that the control group suffered a sharper decline in comparison to treated group. However, this begs the question, is the difference between the sites significant, was there less of a decline in income in the intervention communities?

In the Ucayali case, at the baseline survey, the difference showed that the treated group average was US$3,672 lower than the control group. In the second survey, the averages were lower, but the treated group's average was US$568 more than the control group. The impact estimation of REDD+ project is around US$ 4,241 but it is not statistical significant (P-value 0.4). The difference in averages between control and treatment group before and after intervention shows that the forest incomes in participating communities did not decline as much as control group. We observed that the positive ATET estimation of REDD+ project came for helping to the intervention villages to not lose the same magnitude of income than the control group.

In the Madre de Dios case, at the baseline survey, the difference showed that the treated group average was US$ 1,991 lower than the control group. In the second survey, the averages were lower, but the treated group's average was US$ 1,047 more than the control group (see Figure 2.2). The difference between the base line and second survey generated an impact estimation of REDD+ project around US$ 3,039. However, applying a statistical test shows that it is not statistically significant (P-value 0.286). We observe the same trend before and after intervention in forest incomes for both groups in Madre de Dios and in Ucayali sites.

2.4.2 DiD with PSM analysis

2.4.3 Evaluating the Ucayali REDD+ project

In this section, we implemented DiD method with PSM to evaluate the REDD+ projects. We first needed to carry out the matching exercise using variable related to observed characteristics from the sampled population. The pre-treatment variables used for matching in Ucayali sites included variables related to characteristics of the household head (such as age, years of education, years in village, and birthplace) and variables characterizing the households (such as size, years of household existence, household utility index, number of adult equivalent[14], asset value, total land, total annual income, annual farming income, annual forest income, and annual timber income) (Variables are listed in Appendix 8). A raw estimation of means with full sample showed that two variables were unbalanced using Standardized Difference in Mean (SDM). These variables were 'years of household existence', and the 'household utility index' (which is a composite measure of household access to utilities such as potable water, electricity or plumbing)[15]. For both variables, the control group means were higher, indicating that these households were older and had better access to utility services. The remaining variables were balanced.

We ran a logit model to estimate the propensity score (or probability to be treated) for each informant in the treated and control groups. Our first logit model attempted to use a full set of variables characterizing the household heads and households but we could not produce probability distributions of the propensity scores that overlapped. As a result, we cut down the set of variables leaving only the following 'age', 'year of education', 'birthplace', 'asset value', 'house condition index', 'utility index', 'total land', and 'total annual farming income' as well as some artificial[16] variables to produce better balance.

Using the propensity scores from the best probability model, we applied a 'nearest neighbor matching algorithm with replacement' to build a new control group using the individuals that best matched individuals in the treated group.

The balance test shows that there is no systematic difference in each pre-treatment variables included in the analysis between treated and matched control groups (See Table 2.1). These results show that PSM efficiently removed the systematic difference in two pre-treatment variables. As a result, the new matched control group included 52 indigenous

[14] We are using the OECD scale where adult equivalent formula is AE=1+0.7(Number of adults – 1) + 0.5*Number of Children. It has the following equivalence: first adult =1, further adults = 0.7, all members <16 years =0.5

[15] Utility index represents household access to utilities (water, toilet and electricity). Each variable indicates the following relative value: water (stream, river, pond, common faucet or well = low, own well or reservoir= medium and piped water =high); toilet (no latrine or shared latrine = low, own latrine = medium, own flush toilet with piped water = high); electricity (no electricity = low, through unpaid connection to grid or village system = medium, and through paid connection to grid or own generator = high). The relative values are 1=low, 2=medium, and 3=high. This gives an index with minimum value 3 (low) and maximum value 9 (high).

[16] Artificial variables were generated from the original individual variables such as years of education power to two, years of education power to three, and interaction between birthplace and years of education.

informants from the control villages, some of them matched to multiple individuals in the group of 105 informants from the treated indigenous villages. We ended up with two groups of statistical similar informants, one group of indigenous households that participated in the REDD+ project and another formed of similar counterpart households from control villages.

After building the matched control group for REDD+, we could computed the Average Treatment Effect over Treated (ATET) with PSM for annual forest income. In the Ucayali site, at the baseline survey, the difference showed that the treated group average was US$ 5,309 lower than the control group (See Table 2.2). In the second survey, the averages were lower, but the treated group's average was US$ 628 more than the control group The ATET estimation of REDD+ project is around US$ 5,309. The ATET estimate indicates that the decline in mean annual forest income was less for the treatment communities; however the difference is not statistically different (P-value 0.20). This result suggests that at the time of the second survey, there was no impact in terms of increased forest income among participants in the AIDER REDD+ project.

As a robustness check of the results, we used Kernel Matching to estimate the difference using a different matching algorithm. Because kernel matching uses all available control observations when estimating ATET, in this case the matched control group included all 90 subjects. With this test, the ATET for the REDD+ project is a little less US$ 5,614 but it still is not statistically significant (T-statistic 0.75). This result confirms our previous ATET estimation using DiD with PSM. Moreover, this result suggests that the impact estimation is robust to matching algorithm used to build the counterfactual of the treated group. These findings suggest that the REDD+ project did not significantly impact forest income in the targeted households by the AIDER project in Ucayali.

2.4.4 Evaluating the MDD REDD+ project

To match the informant in the MDD case, we used the same set of variables related to household head characteristics and variables characterizing the household than Ucayali site (a full list of variables is included in Appendix 9). Additionally, we included pre-treatment variables such as 'distance to road', 'gender', and 'number of homes'. The firs variable measures the distance in kilometers from the household in the village to the near road. Gender identifies female head of household. Number of homes indicates the number of homes out village owned by Brazil nut concessionaires. The distance variable and number of homes could have affected the decision to engage in REDD+ activities which usually were organized on the Brazil nut federation head quarter at capital city of Madre de Dios. We considered that including gender variable in Madre de Dios site was important because we have a significant female heads of household (20% in treatment group) and it is likely that this household behaves different from household with male head in brazil nut activity.

A raw estimation of means using the Standardized Difference in Mean (SDM) with full sample showed that there were seven unbalanced variables. The control group concentrates higher values in five variables such as 'distance to road', 'number of homes', 'total land area', 'total annual income', and 'total annual farming income'. For two variables ('years in village' and 'birthplace') higher values were concentrated in the treated group.

To generate the matched samples, we initially ran a logit model with the full sixteen variables and then ran the balance test to confirm that there was no systematic difference between the means in the treated and new control groups. After several tries, we ended up with an optimal logit model that minimized the unbalanced variables.

There were 104 potential informants in the control communities, slightly lower than the 111 informants in the treated communities. The balance test is presented in Table 2.3. The results show that the 'birthplace', 'total land area', and 'total annual incomes' were slightly unbalanced with values less than 0.20 (in absolute value). This imbalance does not affect the ATET estimation. Instead, we observed that the 'number of homes' variable appears unbalanced (SDM value of 0.30). However, when exploring the average of this variable for both groups, we realized that this difference is negligible: treated (0.5 homes units) and control (0.7 homes units). Moreover, the ratio of variances for this variable is around 1.1, which indicates that both groups have approximately the same variances. The final matched control group was composed of 55 castaneros from the control communities

The impact estimation is summarized in Table 2.2. In the MDD site, at the baseline survey, the difference showed that the treated group average was US$ 1,525 lower than the control group. In the second survey, the averages were lower, but the treated group's average was US$ 949 more than the control group. Similar to the Ucayali site, we observed a sharp decline in forest income between base line and assessment year with more drastic income declining in the control group. The ATET measurement for the REDD+ project shows a positive impact around US$ 2,474, but, it is not statistically significant (P-value 0.37). Although we do not observe increased forest incomes after REDD+ intervention, we observed that treated communities experienced less reduction in forest income after REDD+ intervention in comparison to control group. Moreover, the confidence interval shows a huge variation (-3,004 to 7,953) which suggest that individual differences are highly heterogeneous. This result indicates that there is no impact of REDD+ project on forest income on targeted household by BAM

We ran a robustness check of these results using a different matching algorithm (Kernel matching). Our results show a negative ATET of US$ 465.8 but not statistically significant (t-statistics -0.12). This robustness check confirms our previous results that there is no significant impact on household´s forest income from a REDD+ project implemented by BAM in Madre de Dios.

2.4.5 Impact evaluation of specific interventions

Given that the ATET estimated by DiD with PSM for both the Ucayali and the MDD REDD+ project showed no significant impacts on forest income, we decided to do an additional round of evaluations for specific interventions from each site that played important roles in the design of the REDD+ projects. We expect that focusing on informants that have reported direct participation with the specific intervention in the REDD+ project helps to focus analysis on informants who may be likely to have experienced a significant impact. In the Ucayali case, we evaluated the impact of a Forest Management Training course that was part of the development assistance provided by AIDER as part of the REDD+ project. In the MDD example, we examined the administrative assistance offered by BAM to help Brazil nut gatherers to comply with forestry regulations.

We selected these two interventions because households reported direct participation in these interventions in the evaluation survey. This information is crucial to identify precise impact over households which were involved in the REDD+ activities. If there is any impact (positive or negative) from REDD+ activities, this should appear for this group of the households. This requires that the treated group will be adjusted including only households that reported direct participation in the specific intervention. Additionally, we focus our assessment in the incomes influenced by these specific interventions: timber income in Ucayali and Brazil nut income in Madre de Dios.

2.4.6 Ucayali Forest Management Training intervention

In the Ucayali case, the treated group consisted of the 60 indigenous household heads that reported direct participation in the Forest Management Training course offered by AIDER. This was far less than the 109 indigenous households sampled in the treated villages. In this part of the analysis, we included variables related to household head (such as age, years of education, years in village, and birthplace) and variables characterizing the households (such as size, years of household existence, household utility index, number of adult equivalent, asset value, total land, total annual income, annual farming income, annual forest income, and annual timber income).

Our best logit model for participation in the treatment did not balance all the variables included in the analysis (See Table 2.4). Specifically, three household variables were not balanced: 'number of adult equivalent', 'utility index', and 'total annual farming income'. Adult equivalent and total annual farming income have a SDM value slightly over 0.10 (in absolute value) which means that they are close to being systematic similar. For instance, the average value for adult equivalent is 3.6 for the control group and 3.7 for the treated group. Likewise, the ratio of variances is around 1.004, which means the two groups have similar variances. Instead, the variable related to utility index has a SDM value around 0.29 (in absolute value) which appear to be a systematic difference between treated and control groups. However, the individual average shows the insignificant difference: treated (4.6) and control (4.3). It is important to highlight that this index was created from a discrete answer for household reporting access to the utility. It means that using SDM formula for continuous variables may not be convenient for this type of index. Finally, it is reasonable to think that access to utility does not affect timber incomes in native communities in Ucayali. After implementing the single nearest neighbor matching algorithm (1:1 matching) with replacement, we ended up with 38 matched control individuals.

Our impact estimation for the specific intervention in Ucayali site is summarized in Table 2.5. In this case, at the baseline survey; the difference showed that the treated group average was US$ 1,091 lower than the control group. In the second survey, the averages were lower, but the treated group's average was US$ 58 more than the control group. These results show a huge decrease in timber income between 2012 and 2014. These timber income reduction follows the same trend than forest income described in the DiD analysis with the control group experiencing a worse off situation. For the sample of villagers reporting direct participation in the Forest Management Training, the ATET estimation was US$ 1,149 annually. This result is not statistically different from zero (P-value 0.26) which suggests that there is no impact from the Forest Management Training on household´s timber income (See Table 2.5). The robustness check with Kernel Matching confirms this finding, the impact is positive (US$ 233), but it is not statistically significant (T-statistics 0.53).

2.4.7 Madre de Dios administrative assistance intervention

As mentioned in the background section, one facet of the BAM project was to include administrative assistance for legal compliance. The administrative assistance provided by BAM in Madre de Dios included, mainly, technical and legal assistance to prepare Forest Management documents required by Peruvian Forestry Law and technical assistance for delimiting Brazil nut concession boundaries. Informants that reported participation in the treated group includes 65 Brazil nut concessionaires from treated village which reported receiving this administrative assistance. In this analysis, we used variables related to household head and household characteristics (See Table 2.6 for a description).

The optimal logit model for participation in the specific intervention and balance test show that there are four unbalanced variables: 'distance to road', 'house condition index', 'birthplace', and the 'years of household existence' (See Table 2.6). All these variables have an SDM less than 0.20 in absolute value, which means that the difference is not significant.

Moreover, we think that variables such as ‘birthplace’, ‘years of household existence’, and ‘house condition index’[17] does not affect Brazil nut incomes. Even though we think that ‘distance to road’ could affect Brazil nut income, the lower number in the SDM (0.18 in absolute value) indicates the difference is not significant and, therefore, it will not affect the ATET estimation. For instance, the treated households are located, on average, 32.4 kilometers away from the road and the control group 27.2 kilometers from the road. After implementing the single nearest neighbor matching algorithm (1:1 matching) with replacement, the matched control group consisted of 33 castaneros from control villages.

Finally, the ATET estimation for the administrative assistance in Madre de Dios is summarized in Table 2.5. The outcome in evaluation is the annual Brazil nut income. In this case, at the baseline survey; the difference showed that the treated group average was US$ 258 greater than the control group. In the second survey, the averages were lower, but the treated group’s average was US$ 93 lower than the control group. We observed that the same general decline of forest incomes between 2012 and 2014 affected also Brazil nut income.. The ATET estimations indicated that participants had a negative impact (on average US$ 352 less income), but the results are not statistically significant, which means we cannot distinguish a difference between the two groups incomes. In this specific case, even when the treated group started, on average, with higher Brazil nut income, after the REDD+ specific intervention the income was lower than control group. This is a surprising result because it suggests that households which were involved in the administrative assistance were negatively affected on their average Brazil nut incomes. Our ATET estimation using Kernel Matching shows a negative impact of US$ 461 for participants in the specific interventions but is not statistical significant (T-statistics -0.3). These results are consistent with our DiD with PSM estimation.

[17] Aggregate index which represents house conditions of roof, wall, and floor. Each variable indicates the relative value (on a village scale) of the main material used in the construction of the roof, walls or floor. The relative values are: 1=low, 2=medium and 3=high. This gives an index with minimum value 3 (low) and maximum value 9 (high).

2.5 DISCUSSION

The data show that there was a sharp decline in forest income between Phase 1 and Phase 2 in Peru. The same result occurs for timber income in Ucayali and Brazil nut income in Madre de Dios. Likewise, total income from all sources shows the same significant reduction from Phase 1 and Phase 2. The BACI research design was important to identify this general decline, which occurred in both groups and different locations in the Peruvian Amazon (Ucayali and Madre de Dios). If there had not been this sort of quasi-experimental design, observer could have concluded that the REDD+ initiatives were disasters. The reasons for this income decline are unknown. During this period (2012-2014) there were no economic shocks, changes to legal framework, policy changes or natural disasters in the Amazonian area where communities under study are located. However, In 2013, indigenous communities in Ucayali site reported field inspections from OSINFOR (Peruvian Forestry Supervising Agency). This initial law enforcement actions in indigenous communities could be a possible factor that influenced the sharp decline in forest income that we see specifically from timber sales. We will address this issue in further research.

Our results, based on multiple variations of impact assessment techniques allow us to conclude that there was no impact from the REDD+ projects on forest income in Peruvian sites. Analysis on individuals who reported direct participation in specific activities implemented by REDD+ proponents indicates that there was no significant difference between the intervention and control groups. In summary, the assessment tried multiple approaches to identify some impact; however, all confirm the same results.

What are the implications of our findings? REDD+ interventions should redefine their strategies if they want to generate economic impacts on communities. Even tough economic incentive could be a powerful instrument to promote human behavior towards activities to improve forest quality, others non-economic incentives could be explored to promote the same behavior.

Several factors could explain the lack of impact of these REDD+ projects on forest income. First, it could be possible that REDD+ activities implemented by proponents were intended to improve forest quality (for instance, activities to lower damage forest from extraction) which probably had little effect on income or possibly lowered income. Second, it is possible that initial REDD+ activities were designed to comply with international standards to qualify for carbon credits such as Verified Carbon Standard (VCS) or Climate, Community & Biodiversity Standard (CCBS). These international standards request, in the beginning, compliance with forestry law, ensure effective land tenure, and the implementation of Monitoring, Verification, and Reporting (MRV) system. Finally, carbon credits were part of incentives to communities considered in the REDD+ design; however, until 2014, communities did not receive any cash transfer from carbon credits even when one of the proponents sold carbon credits.

2.6 CONCLUSIONS

In this paper, we used several methods to control participant selection bias and minimize unbalanced variables. Using the DID method with PSM, we were able to identify two statistically similar groups of informants, one group of households that participated in the REDD+ project and another formed of similar counterpart households from control villages. We implemented two levels of analysis. First, we estimated the impact of REDD+ projects on household forest income considering the intervention status reported by the proponents (i.e. households intended to treat). In the second layer of analysis, we evaluated direct beneficiaries of two important interventions in the REDD+ projects.

DiD analysis demonstrate a sharp decline in forest income for participants in the Ucayali and Madre de Dios control and treated sites. In both sites, treated groups had lower forest incomes than the control groups in the baseline survey (phase 1) while treated groups had slightly higher forest incomes than the control groups in the second surveys (phase 2). In addition, the ATET estimate indicates that the decline in mean annual forest income from Phase 1 to Phase 2 was less for the treated communities but this changed when we analyzed specific interventions at the sites. Treated participants in the forest management intervention of Ucayali had a slightly higher income than the control group in Phase 2 but treated participants in administrative and technical assistance in Madre de Dios had a slightly lower income from brazil nuts than the control group in Phase 2.

In summary, our findings suggest that there is no evidence that REDD+ interventions affected household forest income in Peruvian sites. It means there is no impact of living in REDD+ intervention area, there is no impact of participating in improved forest management training in Ucayali site, and there is no impact of technical and legal assistance with Brazil nut concessions in Madre de Dios.

Finally, impact estimations of Ucayali REDD+ project may be affected by long-standing relationship between proponent and treated communities. In the Ucayali site, AIDER had been working on improved forest management practices within the treated communities since 2005. In a sense, REDD+ activities were perceived as a continuation of AIDER activities within these communities.

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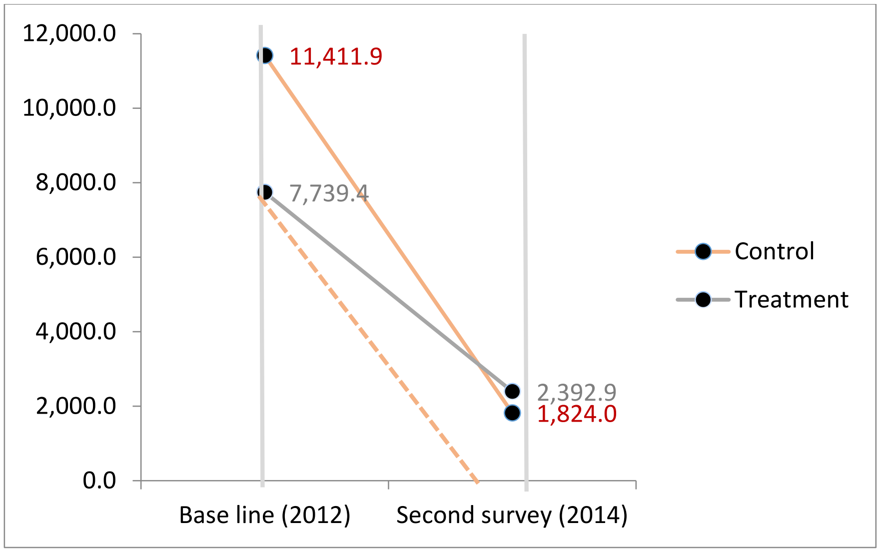

Figure 2.1: Change in annual forest income Ucayali site

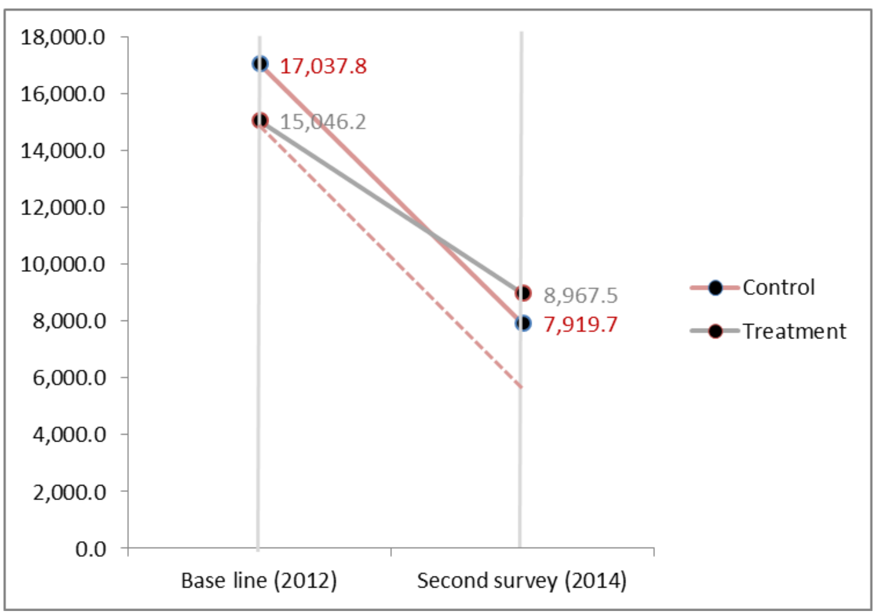

Figure 2.2: Change in annual forest income Madre de Dios site

Table 2.1: Balance test of pre-treatment variables for REDD+ project – Ucayali

Type	Variable	Group	Obs	Mean	Standard Deviation	SDM*	Ratio of variances (T/C)
Head of household	Age	Control	52	43.4	11.9	-0.058	1.162
		Treated	105	42.7	12.9		
	Years of education	Control	52	8.9	4.0	0.005	0.789
		Treated	105	8.9	3.5		
	Years in village	Control	52	28.6	16.5	-0.010	0.894
		Treated	105	28.5	15.6		
	Birthplace (%)	Control	52	0.4	0.5	0.030	0.996
		Treated	105	0.5	0.5		
Household information	Size	Control	52	5.8	2.1	-0.069	0.997
		Treated	105	5.7	2.1		
	Adult equivalent	Control	52	3.8	1.2	-0.070	1.006
		Treated	105	3.7	1.2		
	Years of existence	Control	52	18.6	11.7	-0.079	0.967
		Treated	105	17.7	11.5		
	Asset value (US$)	Control	52	920.5	904.8	-0.026	1.500
		Treated	105	894.6	1,108.1		
	House condition index	Control	52	6.5	1.6	-0.024	0.639
		Treated	105	6.5	1.2		
	Utilities index	Control	52	4.4	0.7	-0.078	2.422
		Treated	105	4.4	1.1		
	Total land (Has)	Control	52	4.4	4.3	-0.086	1.311
		Treated	105	4.0	4.9		
	Annual farming income (US$)	Control	52	2,448.2	2,896.9	-0.083	1.301
		Treated	105	2,190.1	3,304.0		

Table 2.2: ATET estimations for REDD+ projects

Intervention	ATET	Standard Error	P-value	Lower bound on CI	Upper bound on CI	Matched control obs
Ucayali	**5,937.8**	4,650.2	0.202	-3,176.5	15,052.0	52 out of 95
Madre de Dios	**2,474.7**	2,795.3	0.376	-3,004.0	7,953.4	55 out of 104

Table 2.3: Balance test of pre-treatment variables for REDD+ project – Madre de Dios

Type	Variable	Group	Obs	Mean	Standard Deviation	SDM*	Ratio of variances (T/C)
Head of household	Gender (%)	Control	55	0.2	0.4	-0.052	0.905
		Treated	111	0.2	0.4		
	Age	Control	55	55.4	14.6	-0.036	0.747
		Treated	111	54.9	12.7		
	Years of education	Control	55	7.1	4.2	0.031	0.811
		Treated	111	7.2	3.8		
	Years in village	Control	55	34.9	17.1	0.016	1.140
		Treated	111	35.1	18.2		
	Birthplace (%)	Control	55	0.22	0.4	0.121	1.146
		Treated	111	0.27	0.4		
Household information	Size	Control	55	4.0	2.0	0.000	1.270
		Treated	111	4.0	2.3		
	Adult equivalent	Control	55	2.9	1.2	0.004	1.162
		Treated	111	2.9	1.3		
	Years formed	Control	55	23.4	14.9	-0.006	0.947
		Treated	111	23.3	14.5		
	Asset value (US$)	Control	55	9,587.2	18,389.7	-0.004	0.252
		Treated	111	9,534.3	9,240.0		
	House condition index	Control	55	7.0	1.1	-0.007	1.265
		Treated	111	7.0	1.2		
	Utilities index	Control	55	6.3	2.1	0.106	0.919
		Treated	111	6.5	2.0		
	Distance from home to near road (km)	Control	55	33.8	24.2	-0.094	1.883
		Treated	111	31.1	33.3		
	Number of homes own out of the village	Control	55	0.7	0.6	-0.300	1.144
		Treated	111	0.5	0.7		
	Total land (Has)	Control	55	1,134.5	2,415.1	-0.184	0.049
		Treated	111	812.9	533.8		
	Total annual income (US$)	Control	55	22,343.8	37,104.0	-0.140	0.161
		Treated	111	18,393.6	14,896.8		
	Annual farming income (US$)	Control	55	1,168.4	1,809.0	-0.103	1.169
		Treated	111	974.1	1,956.3		

Table 2.4: Balance test of pre-treatment variables for the specific intervention – Ucayali

Type	Variable	Group	Obs	Mean	Standard Deviation	SDM*	Ratio of variances (T/C)
Head of household	Age	Control	38	42.3	12.3	0.047	1.043
		Treated	60	42.9	12.5		
	Years of education	Control	38	9.4	4.4	-0.043	0.622
		Treated	60	9.3	3.5		
	Years in village	Control	38	30.0	13.2	0.004	1.099
		Treated	60	30.1	13.8		
	Birthplace (%)	Control	38	0.5	0.5	-0.033	0.989
		Treated	60	0.5	0.5		
Household information	Size	Control	38	5.5	2.0	0.106	0.907
		Treated	60	5.7	1.9		
	Adult equivalent	Control	38	3.6	1.1	0.131	1.004
		Treated	60	3.7	1.1		
	Years formed	Control	36	18.9	11.7	0.063	0.936
		Treated	58	19.6	11.4		
	Asset value (US$)	Control	38	994.6	999.1	-0.009	1.769
		Treated	60	984.5	1,328.7		
	House condition index	Control	38	6.7	1.4	-0.089	0.747
		Treated	60	6.6	1.2		
	Utilities index	Control	38	4.6	0.6	-0.291	2.158
		Treated	60	4.3	0.9		
	Total land (Has)	Control	38	4.6	4.5	-0.043	1.173
		Treated	60	4.4	4.8		
	Annual farming income (US$)	Control	38	1,765.4	2,114.7	0.112	1.285
		Treated	60	2,019.6	2,396.9		

Table 2.5: ATET estimations for specific interventions

Intervention	ATET	Standard Error	P-value	Lower bound on CI	Upper bound on CI	Matched control obs
Ucayali	**1,149.7**	1,035.3	0.267	-879.3	3,178.8	38 out of 95
Madre de Dios	**-352.0**	918.3	0.701	-2,151.9	1,447.9	33 out of 104

Table 2.6: Balance test of pre-treatment variables - specific intervention Madre de Dios

Type	Variable	Group	Obs	Mean	Standard Deviation	SDM*	Ratio of variances (T/C)
Head of household	Gender (%)	Control	33	0.2	0.4	-0.095	0.868
		Treated	64	0.2	0.4		
	Age	Control	33	55.6	14.6	-0.096	0.736
		Treated	64	54.3	12.5		
	Years of education	Control	33	7.0	4.1	0.103	0.752
		Treated	64	7.4	3.5		
	Years in village	Control	33	36.5	17.1	-0.005	1.144
		Treated	64	36.4	18.3		
	Birthplace (%)	Control	33	0.24	0.4	0.157	1.152
		Treated	64	0.31	0.5		
Household information	Size	Control	33	3.8	2.2	-0.025	0.860
		Treated	64	3.7	2.0		
	Adult equivalent	Control	33	2.7	1.4	0.024	0.878
		Treated	64	2.8	1.3		
	Years of existence	Control	33	25.8	14.3	-0.164	1.059
		Treated	64	23.4	14.7		
	Asset value (US$)	Control	33	8,101.8	5,030.1	0.105	2.763
		Treated	64	8,822.9	8,361.1		
	House condition index	Control	33	6.8	1.0	0.175	1.627
		Treated	64	7.0	1.3		
	Utilities index	Control	33	6.5	2.0	0.048	0.971
		Treated	64	6.6	1.9		
	Distance from home to near road (km)	Control	33	32.4	23.8	-0.186	1.769
		Treated	64	27.2	31.7		
	Number of homes own out of the village	Control	33	0.5	0.6	0.050	1.481
		Treated	64	0.5	0.7		
	Total land (Has)	Control	33	800.8	495.0	-0.032	1.144
		Treated	64	784.4	529.5		
	Total annual income (US$)	Control	33	16,659.8	12,499.3	0.033	1.003
		Treated	64	17,073.3	12,520.9		
	Annual farming income (US$)	Control	33	1,086.4	1,454.0	0.005	2.714
		Treated	64	1,096.6	2,395.2		

CHAPTER 3

GENDER LESSONS FOR CLIMATE INITIATIVES: A COMPARATIVE STUDY OF REDD+ IMPACTS ON WELLBEING[18]

3.1 INTRODUCTION

Reducing Emissions from Deforestation and Degradation (REDD+) is one approach to implementing the Paris Accords to mitigate climate change. As with almost any emerging policy approach, there is much to learn from the experience of REDD+ initiatives that is relevant for future efforts. This is particularly true because of the resemblance of such initiatives, in the end, to prior phases, or perhaps fads (Angelsen et al. forthcoming), that came before – and the hope that future endeavors can at least avoid similar errors and, ideally, break productive new ground.

Although wellbeing is not an explicit goal of REDD+ initiatives, much of the framework surrounding REDD+ not only promotes but also requires attention to community wellbeing. For example, the Cancun safeguard (e) refers to social benefits, and under the UNFCCC, countries will be required to have a national social and environmental monitoring system in place, and regularly report on impacts, in order to be eligible for funds (UNFCCC 2014, Duchelle et al 2017).

Women's wellbeing, in particular, has been emphasized in recent climate agreements, through the emphasis on a gender-responsive climate policy, including in the Paris accord[19], and the 2016 Decision 21/CP.22 on Gender and Climate Change.[20] In addition, goal 5 of the Sustainable Development Goals is to "achieve gender equity and empower all women and girls".

[18] Co-authors: Anne Larson, Amy Duchelle, Abdon Awono, Stibniati Atmadja, Therese Dokken, and Daju Resosudarmo
[19] http://unfccc.int/gender_and_climate_change/items/9619.php
[20] http://unfccc.int/files/gender_and_climate_change/application/pdf/pages_17-20_from_10a02.pdf

These broad commitments remind us that gender should be an integral part of any global initiative, and not an afterthought. Unfortunately, gender responsiveness has been late in coming to REDD+ initiatives, and this may be one of the causes of the results presented in this article.

Research on subnational REDD+ initiatives demonstrates the importance of considering local REDD+ implementation and community wellbeing from a gender perspective. The findings presented here are based on the Center for International Forestry Research (CIFOR) Global Comparative Study on REDD+ (GCS-REDD; http://www.cifor.org/gcs/). It is based primarily on the analysis of wellbeing change over time, comparing "village" focus groups, which were 68% male, with women's focus groups (100% female) in 62 REDD+ intervention villages and 61 "control" (without REDD+ initiatives) villages, at two different moments in time. The research used a before-after-control-intervention research design, which permits clearer attribution of results to the intervention. Wellbeing, for the purpose of this part of the research, is measured by perception, and is based on definitions of wellbeing developed at the time by the focus group participants themselves.

Analysis of the definitions of wellbeing and its components, such as good health, education and sufficient food to eat, suggest important overlap between the two types of focus groups, but there are also a few important differences, such as the importance of women's empowerment and income, as well as "unity" and "harmony", specifically to the women's focus groups.

The most striking results, however, emerge from the assessment of changes in wellbeing over time. We find, in REDD+ initiative sites, that on average both focus groups see people as worse off three years after the REDD+ intervention. In contrast, the control sites see no net change in wellbeing over the same period. Also, a larger number of women's FGs see women overall as worse off in comparison to the village FGs' perception of household wellbeing. A regression model based on household surveys finds that REDD+ interventions are significantly associated with the decline in women's wellbeing. On average, REDD+ interventions have had a relatively negative impact on women.

3.2 GENDER RESPONSIVENESS AND CLIMATE POLICY

Since 2007, when REDD was adopted in the Bali Action Plan, increasing attention has been given to gender in climate policy, related negotiations and other important global commitments. By 2015, the COP21 Paris agreement included the statement that "Parties should when taking action to address climate change, respect, promote and consider their respective obligations on … gender equality, empowerment of women…." The specific emphasis of this policy was on participation in UNFCCC processes and "increasing awareness and support for the development and effective implementation of gender-responsive climate policy at the regional, national and local levels."[21]

The UNFCCC Women and Gender Constituency has built a strong coalition to support women's rights.[22] The Lima work programme on gender, established at COP20, was extended for three years at COP22 in Marrakech, "noting that gender-responsive climate policy still requires further strengthening in all activities concerning adaptation, mitigation and related means of implementation (finance, technology development and transfer and capacity-building) as well as decisionmaking on the implementation of climate policies."[23]

Gender responsiveness goes beyond being gender sensitive, or the "do no harm" principle, rather seeking to "substantially help to overcome historical gender biases…." (Aguilar 2016). With regard to climate and forests, "gender-responsive planned actions … should integrate measures for promoting gender equality and women's empowerment, foster women's inclusion and provide equal opportunities for women and men to derive social and economic benefits" (Aguilar 2016).[24] Similarly, Kabeer (2010: 108) explains that "gender-blind" projects do not take gender issues into account, and "gender-aware" projects use gender information to try to "avoid reinforcing constraints"; in constrast, "gender-transformative" interventions go beyond this "to ensure that women capture meaningful benefits and are empowered by the intervention process."

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[21] http://unfccc.int/gender_and_climate_change/items/9619.php
[22] see http://womengenderclimate.org/
[23] http://unfccc.int/files/gender_and_climate_change/application/pdf/pages_17-20_from_10a02.pdf
[24] http://genderandenvironment.org/2015/08/stop-being-so-sensitive-the-shift-from-gender-sensitive-to-gender-responsive-action/ ; also Aguilar, L. Foreward in Colfer et al. 2016 *Gender and Forests*.

Arguably, as a global project aimed at climate mitigation in the 21st century, REDD+ should be contributing to the transformational change advocated by the Sustainable Development Goals (called "Transforming Our World") and affirmed by the Paris agreement. Yet REDD+ advocates appear to have repeated the mistakes of prior development and conservation-development interventions. There are not many gender analyses of REDD+ initiatives to date, but so far the results are not heartening.

In the study of REDD+ in three countries in the Congo Basin (Cameroon, Democratic Republic of Congo and Central African Republic), Peach Brown (2011) found that women had had little participation in discussions on climate change or REDD+, such as the development of initial policy documents. Nevertheless, there was mention in some later documents, such as the DRC's Readiness Plan, to assure that gender dimensions would be addressed in the future regarding community forest management and benefit distribution. Also in DRC, however, Stiem and Krause (2016) found REDD+ impacts on gender had not been sufficiently addressed, in spite of high levels of rural gender inequality. They found that women spend as much time in the forests as men but that men's activities are much more highly valued. "This systematic devaluation of women's work, and their knowledge about the forest, legitimizes men's dominance in forest governance" (Stiem and Krause 2016).

In Burkina Faso, Westholm and Arora (2015) found that REDD+ appeared "to be perpetuating gendered divisions of labour, as formal environmental decision-making moves upwards; and responsibility and the burden of actual environmental labour shifts further down in particularly gendered ways." Women were targeted in projects aimed at forest conservation through the promotion of trade in non-timber forest products, based on "essentialist assumptions about what men and women do" (Westholm 2016). In Kenya, a comparative analysis of three conservation schemes found that the REDD+ project fared better than two PES projects: the REDD+ scheme used gender targeting and mainstreamed "minimum standards" (p.444), although none of the three had an "explicit gender project" (p.437), and all failed to address underlying inequalities (Kariuki and Birner 2016).

In their study of REDD+ in Nepal, Kahka et al. (2014) found that government and project implementers had neither responsibilities nor strategies for applying gender equitable initiatives; also, explicit inclusion of women in REDD+ program discussions was insufficient for addressing underlying power dynamics and, hence, gender imbalances. Pham et al. (2016) studied the factors that influence women's participation in national decision making processes on REDD+ in Vietnam. They found that large numbers of women participated in REDD+ meetings, yet they were rarely in leadership positions or involved in REDD+ working groups. The authors argued that there was little capacity to implement gender strategies or even concern for gender issues among the national organizations working on REDD+.

In a comparative study of 20 early REDD+ projects (the same projects studied three years later in this article), Larson et al. (2015) found that women were much less informed and knowledgeable about REDD+ and the projects starting up in their villages than the men in the same villages. For example, only 41% of women's focus groups demonstrated a basic understanding of REDD+ compared to 67% of village (male-dominated) groups. This was true even in villages where women believed they had a strong voice in village decisions, when they used forest resources as much or more than men, and when the projects had explicitly noted gender as a concern or women as a specific target group. The authors warned that the failure to address gender imbalances at the outset might inadvertently perpetuate them.

By the time of a second round of research on these early REDD+ initiatives three years later, they appeared to have rectified some aspects of women's participation. For example, focus groups with women (91%) were now found to be equally aware of the REDD+ projects in their villages as male-dominated focus groups (92%) (Larson et al forthcoming). Nevertheless, past research suggests that the gains may be limited. An analysis of integrated conservation and development projects in Asia and Africa found that "costly mistakes could have been avoided if gender issues had been better understood and considered during project design and before implementation had begun" (Flintan 2003). Although some projects planned to rectify this by redoing feasibility studies later, the author argued that "'adding on' a gender component is not likely to provide as positive results as would have been achieved by integrating gender from the very beginning."

It is disappointing yet not particularly surprising to find that REDD+ initiatives have failed to address gender adequately. A review of 200 references of PES schemes found that "less than 5% dealt with gender-related aspects or impacts of PES" (Ravnborg et al. 2007:17). As summarized by IUCN (2012) "Despite the introduction of tools for gender and forestry analysis in the 1990s, it is rare today to find evidence of clear strategies linking gender and forest management for decisionmakers…. [T]here is generally an institutional 'gender blindness' that renders women's participation and contributions invisible and allows forest management to be incorrectly treated as 'gender neutral.'" The authors conclude that "gender equality and women's empowerment must be at the heart of REDD+ policy design and implementation."

Nevertheless, in many REDD+ initiatives, "as in many other development contexts before, gender issues are depoliticized (c.f. Baden and Goetz, 1998) and become a tiresome add on" (Westhom and Arora-Jonnson 2015); approaches to gender are "simplistic" (Westholm 2016). Empowerment cannot be a technocratic exercise, as genuine empowerment involves changing power relations; participation requires not only opportunities but also assets (Esquivel 2015; Reddy and Harvold Kvangraven 2015; Chant and Sweetman 2015). "The goal of gender-responsive development is … to ensure that men and women … have control over important assets that they can use to improve livelihoods, well-being, and bargaining power within their households and communities" (Meinzen Dick et al. 2011). Silverman (2015), for example, argues that REDD+ must consider women's land tenure security.

In addition, Kabeer (2010) argues that there are five 'critical moments' in a project's life cycle that define the "ideas, values, assumptions and information drawn on." The failure to acknowledge gender inequities at any of these moments will have a profound influence on the extent to which it will be able to bring about change. Those moments are: the conceptualization of the intervention; the translation of the goals and objectives into the concrete design of the intervention; project implementation; monitoring and evaluation; and the extent to which there are feedback mechanisms that allow changes to practice (Kabeer 2010). It is possible that the failure to genuinely address gender at earlier critical moments in some of the interventions studied here may be partly responsible for the results presented below.

3.3 DATA AND METHODS

As a part of GCS-REDD, data were collected during focus group (FG) interviews in 87 villages participating in 22 subnational REDD+ initiatives in Brazil, Cameroon, Indonesia, Peru, Tanzania and Vietnam, at two periods of time: early in REDD+ implementation (Phase 1, 2010-11) and three years later (Phase 2, 2013-14). We call these “intervention villages”. Likewise, data were collected in 63 control villages (without REDD+ initiatives) for comparison purposes, for a total of 150 villages.

For the analysis in this article we eliminated 25 intervention villages from the total dataset, which were part of less-intensive research that did not use control villages. This eliminates potential bias for before-after-control-intervention, or “difference in difference,” analysis, that could result from having a larger intervention group. Likewise, we dropped two control villages from the analysis because either phase 1 or phase 2 were lacking data. Therefore, this paper concentrates the analysis on 62 intervention and 61 control villages that are part of the intensive GCS-REDD research sites[25]. Table 3.1 summarizes the number of villages by country. Information about REDD+ initiatives involved in the study is presented in Appendix 10.

3.3.1 Well-being data

We focus on two important sets of data comparing responses from mixed-gender village FGs (on average, 72% male in Phase 1 and 68% in Phase 2) and women’s FGs (100% female), exploring definitions of wellbeing and changes in wellbeing over the three-year intervention period.

[25] Intensive research involved in-depth field research over a period of three months at each site.

The FGs implementation (mixed-gender and women) was carried out by the same team that executed the household survey. FGs participants were selected through a voluntary participation. The field work team leader contacted to community leaders to talk about the objectives of both FGs and asked to invite community members to participate. Community leaders invited community members through loudspeakers or in communal meetings. The minimum size of FGs were set out in 10 participants. In case that the minimum size of FG was not reached, community leaders invited specific households in the village to complete the minimum participants. However, there are some cases where FG size was less than the minimum. In Phase 2, there are nine mixed-gender village FGs which had between 5 to 9 participants and there are 13 women FGs which had between 3 to 7 participants. In term of representativeness, For Phase 2, the women FG size represents on average 3% of total population and only four women FG size represents around 10.5% of population. For mixed-gender village FG, participants represent on average 4.4% of total population and 13 mixed-gender FG size represent between 10% and 20%.

For definitions of wellbeing, the FGs were asked an open–ended question to define the characteristics of wellbeing. The specific question in Phase 2 for the women´s FG was: In this village, what are the characteristics of a woman who has high wellbeing? The specific question in Phase 2 for the mixed-gender FG was: In this village, what are the characteristics of a household with high wellbeing?[26] On average, each group provided five to six answers to these questions, which were coded into 133 distinct responses.

For wellbeing change over time, in both Phase 1 and Phase 2, each focus group was asked to evaluate its perception of wellbeing change over the past two years. The specific question for the women´s FG was: In comparison to two years ago, what proportion of women in the village have experienced overall improvement in their wellbeing, what proportion are the same, and what proportion are worse off? The specific question for the mixed gender FG was: In comparison to two years ago, what portion of households in the village have experienced overall improvement in their wellbeing, what portion are the same, and what portion are overall worse off?

[26] In phase 1 the questions were stated as “better than average wellbeing”.

The informants in the FGs were asked to name the proportion of households or women in the village whose wellbeing had improved, stayed the same or declined. The proportions were pre-coded in the following ranges: 0-20% (none or very few), 21-40% (some), 41-60% (about half), 61-80% (many) and 81-100% (very many to all). It was expected that the sum of these categories would be approximately 100% to allow comparison across the focus groups and over time.[27] In this paper, we concentrated our analysis only on the improvement category because we wanted to know how the REDD+ intervention had improved women´s wellbeing. Figure 3.1 summarizes the improvement category answers for intervention and control villages for the women's focus group for phase 1. The largest number of villages falls in the proportion 81-100% and the second, largest, 0-20%.

In phase 2, villages concentrate in the 0-20% proportion, folowed by 61-80% (See Figure 3.2). These Figures show changes in the distribution of women´s wellbeing improvement perception between the two phases, but we need to know which villages move up (increase proportion), move down (reduce proportion), or stayed the same (no movements). We use a transition matrix, which is presented in the subsection on methods below, to track each village's individual trajectory between phases, for both women's and village FGs.

3.3.2 Village data

We use village data, as well as household data, to model wellbeing perception. According to Austin (2009), a Standardized Difference in Mean (SDM) value greater than an absolute value of 0.10 could potentially indicate a systematic difference in mean between two groups. An exploratory analysis of village data show that variables related to the percentage of villages with health centers, the percent of villages with elementary and secondary schools, distance to market, and daily female wage are not systematically different between

[27] Interviewers were instructed to pay attention to this, and inconsistencies were only found in 7 villages, which were resolved through consultation with the researcher and/or by proportionally adjusting the numbers.

control and intervention villages using an SDM estimator (See Table 3.2). Around 50% of both control and intervention villages have a health center, and 80% have elementary schools. Secondary schools are available in less than 40% of villages in the sample. Average daily wage female is around US$ 8 in control and intervention villages and the variances are not different (ratio of variance around 0.81).

Variables related to population, size, availability of cellphone service in the village, and distance to the nearest road from the village are systematically different between the two groups. For example, on average, control villages are larger than intervention villages in both size and population. Surprisingly, cellphone service is available in 60% of intervention and 77% of control villages. Intervention villages, on average, are further from roads than control villages.

Household data

We present descriptive statistics for household variables, taken from household survey data[28], in Table 3.3. These variables were averaged for each village, then a new average was computed for control and intervention groups. The table shows that only a few variables are systematically different between control and intervention groups: annual off farm income, land use area, annual Payment for Environmental Service (PES) income and annual government support income. For example, control villages concentrate more households with higher off-farm income, for a difference of about US$ 1,500 per year.

Most household variables, however, are similar in mean between the two groups. For example, the annual area of forest cleared by household is approximately 0.4 hectares with no statistical difference between groups. Control villages have households with a higher forest land area (in average, 20 hectares more than intervention villages), but this difference is not significant.

[28] Total households were 3,960, divided in 1,842 in control villages and 2,118 in interventions villages.

3.3.3 Methods for data analysis

In this paper, we identify the perceived changes in wellbeing after REDD+ interventions. We constructed a transition matrix (see Figure 3.3) to track the proportion of women (women's FG), or of households (village FG), in each improvement category between the two research phases for each village. This method allows us to identify which villages experienced positive, negative or no wellbeing movement. Villages experiencing a positive movement are those reporting that a higher proportion (of women or households) were better off in Phase 2 than in Phase 1 (light gray area in Figure 3.3). Conversely, villages experiencing a negative movement are those with a lower proportion better off in Phase 2 compared to Phase 1 (dark grey area in Figure 3.3). Villages experiencing no wellbeing movement are those with the same proportion in the two phases (white area in Figure 3.3).

Thus each village has a unique trajectory in the transition between Phase 1 and Phase 2. We used this information to compare changes between control and intervention villages and between women's and village FGs. (A detailed description of the construction of the transition matrix is presented in Appendix 11.)

To understand the reasons behind changes in wellbeing perception, we analyzed responses from FG interviews that specifically asked each group to identify the three main reasons for wellbeing improvement.

As a complement to this analysis, we model the relationship between changes in women´s perceived wellbeing improvement and variables related to village context, characteristics of the household and household head, and focus group characteristics.

The statistical model was implemented at the village level. Hence, to include household variables, we calculated the mean of continuous variables and percentage for dummy variables at the village level. For instance, we computed the mean of household size for each village. Then, we use this average in the regression model as an independent variable. Likewise, we computed the percentage for female heads of household at village level and used this variable as an independent variable.

The dependent variable is the proportion of women in each village who are in the wellbeing improvement category. This variable was gathered in five intervals from 0% to 100% (See Figure 3.1). Given this particular feature of the dependent variable, we use an interval regression approach. This approach helps to deal with our particular dependent variable which has to observations: (1) lower bound and (2) upper bound. For instance, the first interval has two values: 0% and 20%. The interval regression follows the same assumptions that the typical Ordinary Least Square (OLS) regression. The model is set up as follows:

$$(WP_{LB}, WP_{UB}) = \alpha_0 + \beta T + \sum_{i=1}^{n} \alpha_i Village\ variable_i + \sum_{j=1}^{n} \beta_j HH\ variable_j + \sum_{k=1}^{n} \gamma_k\ FG\ variable_k\ + e$$

Where WP is the proportion of women in the wellbeing improvement category. In the right-hand side, village variables include population, area, cellphone service, health center, elementary school, secondary school, distance from the village to the road, and distance from the village to the market. HH stands for household and it includes the following group of variables: (1) head of household characteristics, (2) household income by different economic activities, (3) household income from government or NGO, (4) lands managed by households, (5) home characteristics (house condition index[29]), and (6) access to utilities (utility index[30]) . Table 3.2 lists all household variables used in the regression. Finally, FG means Focus Group and includes two variables: (1) average age in the focus group and (2) size of focus group. Finally, e is the error term. All the variables included in the model were computed at the village level.

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[29] This index represents household access to utilities (water, toilet and electricity). Each variable indicates the following relative value: water (stream, river, pond, common faucet or well = low, own well or reservoir= medium and piped water =high); toilet (stream, river, pond, field, or shared latrine = low, own latrine = medium, own flush toilet with piped water = high); electricity (no electricity = low, through unpaid connection to grid or village system = medium, and through paid connection to grid or own generator = high). The relative value are 1=low, 2=medium, and 3=high. This gives an index with minimum value 3 (low) and maximum value 9 (high).

[30] This index represents house conditions of roof, wall, and floor. Each variable indicates the relative value (on a village scale) of the main material used in the construction of the roof, walls or floor. The relative values are: 1=low, 2=medium and 3=high. This gives an index with minimum value 3 (low) and maximum value 9 (high).

The interval regression model was run only for Phase 2 for the sample under study (62 treated villages and 61 control villages) [31]. Villages (10) with missing information at least in one variable were dropped from the model estimation. We did not make imputations for missing values. We included a fixed effect by country to control for idiosyncratic country differences. Likewise, this model used white's variance-covariance estimator. The same model was then run for the village FG data for comparison purposes.

3.4 RESULTS

3.4.1 Definitions of wellbeing

Table 3.4 presents the most frequently mentioned responses by the focus groups when asked to define the characteristics of a woman, or a household, with high wellbeing. We include all of those that have at least 10 responses in one definition description. They are presented roughly in descending order by frequency.

The results are used to present tendencies and not statistical differences, but they suggest both similarities and differences between the two groups. The most common responses for both groups were good health, good education, and sufficient food to eat (top 3 for both), as well as good quality house construction material (top 4 for village and top 5 for women). Another important characteristic equally important for both was a peaceful or happy life, which fell in the top 6 for women and the top 8 for the village FG.

The fourth most common response for the women's FG was for women to have their own source of income. This was not mentioned in the village FG, and it is striking that 70 women's focus groups (57%), mentioned this response. Other responses that were more important for women than for the village FG, but with 30 or fewer mentions, were: union in the family, a husband who provides for the family, union between people in the community, religious faith and having many children.

[31] This statistical model was run in STATA 13 with the routine "intreg"

The village FGs much more frequently mentioned ownership of transport items and self-sufficiency (5th and 7th most common responses), as well as ownership of livestock (with only 15 mentions), and somewhat more frequently, electricity (6th most common for the village groups and 7th for women). A small number of village FGs (10 to 16) mentioned a series of characteristics 2-3 times as frequently as the women's FGs, such as communication, owning more land for cultivation and community organization.

3.4.2 Perceived changes in wellbeing improvement: comparison of control and intervention sites

We computed the trajectory for each village studied using the transition matrix method. Then, we aggregated these individual trajectories into three groups: villages that moved up (more women better off), moved down (more women worse off) and stayed the same (no movement between phases). In Figure 3.4, we present these results expressed as a percentage respect to the sample study. The results show that for women, those in the REDD+ intervention sites perceive that more women are worse off (27 villages, or 43.5%) and fewer women are better off (16 villages, 25.8%) in comparison to the same groups in the control sites (23 villages, 37.7% in both cases); 16 intervention and 15 control villages stayed the same.

For comparison purposes, we included in Figure 3.4 the same information for village FG, which is labeled as "Village". Village intervention sites also show that fewer villages are better off and more are worse off than in the control sites. More important for the gendered analysis in this article, however, is the comparison of the women and village FGs in the intervention sites. In this case, again, more women are worse off (27 villages) and fewer women are better off (16 villages) in comparison to the village FGs (24, or 38.7%, and 18, or 29%, respectively); in both intervention groups about one third stayed the same.

The net change between better off and worse off status for each group is shown in Figure 3.5. The results show an overall improvement in wellbeing for control groups and overall worsening in REDD+ intervention sites. Among the four categories of villages, the village FGs in control sites appears to perceive the most positive perception of their wellbeing. In fact they are the group with the only net positive change. Women FGs in the intervention sites voiced the least positive perception. Women in the control sites perceive no net change over time.

The results suggest that women in REDD+ intervention villages perceive that they are faring more poorly than do women in villages without REDD+ intervention, and more poorly than the village as a whole in REDD+ intervention sites. We explore the possible reasons for these findings in the next section, after first examining the data by country.

As a part of the analysis, we present disaggregated results by country. Table 3.5 shows the number of villages in each wellbeing improvement category, in control and intervention sites, based on the transition matrix. The final column presents the net positive change in wellbeing (number of villages better off minus worse off) by country.

The results show strong net negative changes in Brazil and Peru for intervention villages but also net negatives in control villages. There are no changes, on average, for women in intervention villages in Tanzania, Indonesia and Vietnam, compared to positive net changes in the control sites. Only in Cameroon is there a similar, net positive change in both intervention and control villages.

In Table 3.6, we present the same results for the village FG. Similar to the women´s FG, Peru and Brazil have more intervention villages that are worse off; control villages are also worse off overall in Peru but there is a net positive results in Brazil. Tanzania and Indonesia have net positive results for both control and intervention villages. And in Cameroon net intervention results are positive and control results negative, whereas the reverse is true in Vietnam.

3.4.3 Accounting for wellbeing declines

(1) Stated Reasons for change

In this section, we explore the reasons behind the change in women´s wellbeing perception. FGs were asked to give up to three reasons for wellbeing improvements. Table 3.7 shows the top five reasons given for wellbeing improvement in the women and village FG for intervention and control villages. It is important to highlight that the direct reported reasons were coded in 199 reasons (107 better off reasons and 92 worse off reasons) which are used to generate these tables.

From Table 3.7, we observe that having an improvement or stable agriculture income is the most frequent reason mentioned by both intervention groups and even more frequently by the village FG. For women, the other, equally important reason is gender equity or women's empowerment. The next three refer to government services, new housing or improved house conditions and increased or secure income or savings. For the village focus groups, the top five include these same three plus improved utilities.

The emphasis in the control groups is quite different, with the introduction of or improved infrastructure as the most frequent answer for women and new employment and work opportunities for the village FG. The top five overlapped for all four groups on agriculture income, and for women, on gender equity and empowerment. But infrastructure, income from animal husbandry, new work or income opportunities and improved economic or business conditions were in the top five for the control but not the intervention groups.

Table 3.8 shows the reasons stated for people being worse off for the same villages under study. Reduced work productivity is a key reason for reduced wellbeing in all groups but is the top reason for the intervention villages, tied with absence of or decrease in safety or cohesion in the community for the women's FG. In this case the top five are the same across the two intervention groups. For the women's control group, illness in the family and insufficient or unstable income or savings are tied for first place; for the village control group, low or decreased job or income opportunities ties with low or decreased agricultural product prices for the most frequent response.

Additionally, the women and village FG facilitators were asked to identify if any reasons for improvement or worsening could be related to the REDD+ project in the intervention villages. The specific indication for interviewers was: "Check [box] if respondents volunteer that the reason as at least partly related to the REDD+ initiative". In the women's data, only seven reasons were checked as related to REDD+ initiative. These are improvements related to economic incentives and training to start a business, additional income from REDD+ projects and incentives for women to be involved in agricultural activities (vegetable garden). For village data, thirteen reasons were checked as related to

REDD+ initiatives. These reasons are related to increasing income from the improvement of agriculture productivity, more participation in REDD+ activities, and direct payment from REDD+ projects.

(2) Women's Wellbeing perception model

Table 3.9 shows the results of the interval regression for women's wellbeing improvement perception. The dependent variable is the proportion of women in the wellbeing improvement category in Phase 2. Independent variables related to income and village area were divided by 1,000 to avoid the scale effect in the coefficients. We include in Table 3.9 only the variables that turned out to be statistically significant.

According to our results, the treatment variable – being in a REDD+ intervention site – has a negative coefficient and is statistically significant at 90%. It means that the share of women that have improved wellbeing in the last two years is about 9% lower than in non-REDD+ villages.

Similar negative results are observed for variables related to household characteristics averaged at the village level. Hence, if the village mean of the head of household years of education is one unit higher, the share of women in the village with improved wellbeing is around 5% lower. This is a counterintuitive result because it is expected that on average, a more educated head of household could help to improve the wellbeing of their household's members.

The variable related to forest land under control of household shows a negative coefficient and is statically significant at 99%. The result implies that one additional hectare of forest land in the village mean reduces the improvement of wellbeing perception in around -0.26%. This result goes in opposite direction to the main objective of REDD+ projects, which aim to conserve the forest. Cellphone service and total village area are also significantly associated with negative wellbeing perception.

In Appendix 12, we present the model with full variables. It is important to highlight that variables related to income generation (farming, off farm or forest) are not statistically significant to explain women's wellbeing perception. The same happens with external income from the Government or PES (Payment for Environmental Service). This may indicate that other non-observable variables, which were not gathered in the survey or focus group, could affect women's wellbeing perception.

For comparison purposes, we ran the same interval regression model for the village focus group. The dependent variable is the proportion of households in the wellbeing improvement category in Phase 2. Similar to women´s FG, the independent variables related to income and village area are included in thousands. Table 3.10 shows the result of the village wellbeing perception model for statistically significant variables. In contrast to women's results, the treatment variable is not statistically significant for the village focus groups. It means on average village members were not affected, positively or negatively, in their wellbeing perception by the REDD+ projects.

On the other hand, infrastructure for education affects wellbeing perception. Hence, the availability of elementary schools in the village increases the wellbeing perception around 17.8%. Curiously, the reverse happens with a secondary school in the village which reduced wellbeing improvement perception in around 14.9%.

Another relevant result is associated with distance to the market from the village. It indicates that villages located in remote areas which are further away from the markets have lower wellbeing improvement perception. Our result indicates that one additional kilometer of distance from the villages to the markets will reduce the wellbeing improvement perception by 0.08% points. For instance, if the village is located 100km away from the market, it will reduce the wellbeing perception by 8%.

Another interesting correlation is that an additional annual thousand dollars in the village mean related to off farm income increase the wellbeing perception in 3.75%. Instead, agriculture and forest incomes are not affecting, not statistically significant, the improvement of wellbeing perception. These results suggest that off farm income affects men's wellbeing improvement perception, but it does not affect women's wellbeing perception.

Variables related to house condition index and utility index are positively correlated to wellbeing improvement perception of the village focus groups. Hence, when house condition index increases in one point in the village mean, the wellbeing improvement perception increases in 6.3%. The effect of the utility index goes in the same direction, increasing the wellbeing improvement perception by 11.3%.

Another striking result for the village focus group is related to hectares of forest cleared by household. It has positive coefficient and is statistically significant at 99%. This result indicates that one additional hectare of cleared forest in the village mean will increase the wellbeing perception of improvement in around 24.1%. It implies that wellbeing perception of village members are in the opposite direction of REDD+ project objectives.

Finally, another result that differs from the women's focus group is the variable related to PES (Payment for Environmental Service). An increment of one thousand dollars in the village mean related to an annual transfer from PES scheme to the household will increase the wellbeing improvement perception in around 13% in the village. It means that transferring less than $100 per month to each household in the village will have a significant effect on the wellbeing improvement. Appendix 12 presents the model with full covariates included in the analysis.

3.5 DISCUSSION

The results presented here suggest some reasons for concern regarding REDD+ interventions and gender. Changes in wellbeing, as perceived by women's focus groups in intervention villages, show improvements in only 26% of villages, declines in 44% and no change in 31%. In comparison, village focus groups also perceive more declines (39%) than improvements (29%) in household wellbeing, but on average their perceptions are better than women's. In the control groups, women perceive no net change overall whereas village FGs perceive net improvements. The BACI analysis shows that REDD+ villages have fared worse with regard to wellbeing perceptions during the same period than control villages.

The difference-in-difference method, which follows similar control villages over the same time period to establish a counterfactual (what would have happened without the intervention), is designed to permit a greater ability to attribute changes over time of the change. Nevertheless, the matching is not perfect, and confounding factors could be important in influencing outcomes (Sills et al. 2017), in addition to or instead of the REDD+ initiative.

The regression model presented here was designed to explore this question systematically for women, as well as for the village FGs for comparison. The regression model for women found that the REDD+ initiative was a significant variable negatively affecting perception of wellbeing change across the sample. Although there was a similar large difference in village wellbeing in intervention compared to control villages, the REDD+ initiative was not found to be significant for the village sample.

Although the results suggest that REDD+ projects may be partly responsible for the decline in women's perception of wellbeing relative to the control groups, it is important both to explore other potential explanations as well as to try to understand what factors might improve the gendered outcomes of REDD+ interventions. We consider, in turn, definitions of wellbeing in relation to the stated reasons for improvements and decline; other important variables in the regression models; and, finally, a discussion of the REDD+ interventions.

It is important to highlight that our analysis did not control any spillover effect or learning effects between phases. For instance, Hawthorne effect (i.e. effect of being observed, and being asked to participate in women's only focus group) could play a role driving the answers in the women FGs. Likewise, learning effect could be a significant problem for villages which have long standing relationship with proponents. It is very likely that these villages were exposed to similar strategies to collect data for several projects.

3.5.1 Wellbeing and wellbeing change

Is it possible that women have different priorities than the majority male village focus groups? Perhaps REDD+ interventions are more likely to provide what men are looking for and neglect the factors that women consider important for wellbeing.

The analysis of definitions of wellbeing suggests important overlap between the two types of focus groups: the top three for both groups were good health, good education and sufficient food to eat. Nevertheless, in fourth place for the women's FGs was "own source of income." Women also placed more emphasis on unity in the family and in the community. The village focus group put more emphasis on owning transport vehicles, livestock and land.

When asked specifically about reasons for wellbeing improvements and declines, responses were very similar for the two groups, with the exception of equity and empowerment for women as one of the two top most frequent responses for women (tied with increased income, the top response for the village FG). Almost twice as many village FGs (13 versus 7) noted that improvements were due to REDD+ initiatives.

Some notable differences in the control groups regarding reasons for improvement in wellbeing are the much greater reference to infrastructure improvements and job opportunities for both groups (women and mixed-gender FG) and improved economic or business conditions, particularly for women. Gender equity and empowerment is a frequent reason for both groups of women.

Although the definitions of wellbeing, being more general conditions, such as good health, are not fully comparable to the reasons for improvement, the results still suggest some general conclusions. First, wellbeing improvements for women more likely need to be tied to specific interventions that support women's employment, economic conditions and empowerment. Second, it is possible that interventions that bring some economic benefit to the village, or even to their spouses, are likely to be seen more critically by women, for example if they increase tensions or conflict in the home or the community.

3.5.2 The regression models

In the women's regression model, not only the REDD+ intervention but also several other variables were statistically significant in affecting women's perception of wellbeing improvement. They include: education level of the household head, household forest area, the presence of cellphone service, and the size (total area) of the village. Although one would normally expect improved wellbeing associated with a higher level of education, the relationship in this case was the opposite; nevertheless, since the dependent variable is wellbeing perception, it is possible that expectations of wellbeing are higher based on education level. Village size and cellphone service seem inexplicable, but apparently contribute to disharmony from the perspective of women.

As stated previously, area of forest as associated with a decline in wellbeing perception flies in the face of REDD+ goals, but is not a surprising result; it parallels the result regarding forest clearing in the village regression model. Greater forest area is associated with lower wellbeing for women and more forest clearing is associated with higher wellbeing for the village FG probably due to the belief that clearing forest in order to produce more food instead will lead to more welllbeing.

Many more variables turned out to be significant in affecting the village FG's perception of wellbeing improvement, relative to the women's model. These include household condition, distance to markets, two income variables (off farm and PES), access to utilities and agricultural land area, among others.

Perhaps most relevant for this article is to understand why none of these variables were found to be significant in the model of women's wellbeing perception. This may represent women's lower market participation, less control over agricultural land, lower opportunities for off farm income and less control over income from PES. Perhaps the results would have been different if it were possible to include a variable specifically focused on women's income apart from household income, based on the women FGs' emphasis in wellbeing definitions and reasons for improvement on women's equity, empowerment and own income sources.

3.6 CONCLUSION

It is possible that the failure to genuinely address gender at earlier critical moments in some of the interventions studied may be partly responsible for the results presented here. The findings suggest that REDD+ initiatives are not reducing – and in some cases may be widening – gender gaps.

The results highlight the importance of asking gender-differentiated questions related to wellbeing and demonstrate gendered impacts of REDD+ interventions. There is broad concern across the climate change community regarding the importance of fully engaging women in climate decisions, initiatives and goals and promoting gender-responsive solutions that support SDG goal #5, to achieve gender equity and empower women and girls.

Gender-responsive analysis is crucial to understanding both risks and opportunities associated with such initiatives, and comparative research data can help identify vulnerabilities and potential ways forward. The results in this article are driven by a relatively small number of villages, and the results vary even among the villages in the same project sites. This suggests that village-specific conditions may affect outcomes, highlighting even further the importance of detailed, local-level gendered analysis for any project or initiative.

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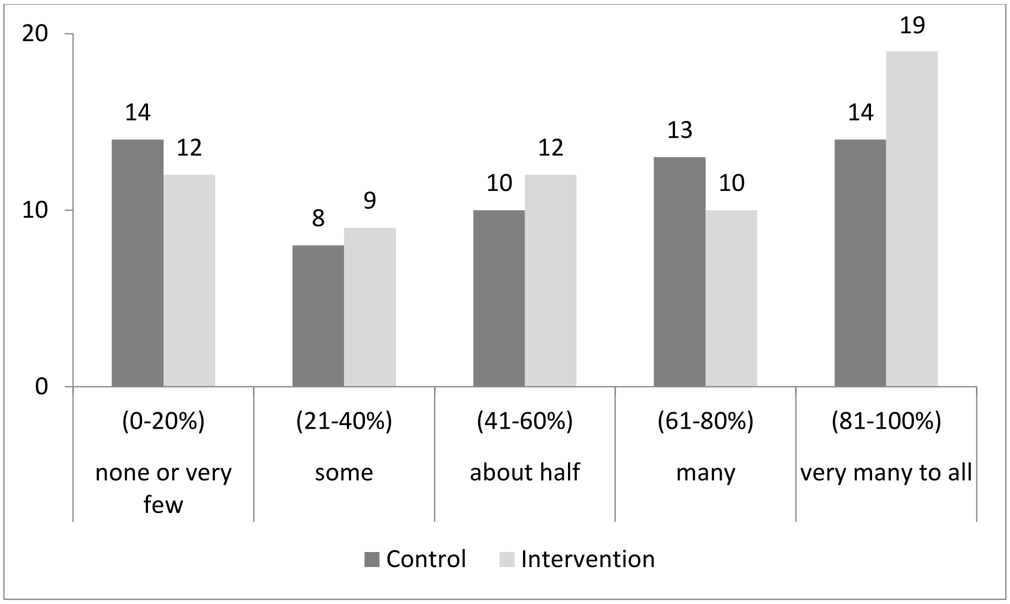


Figure 3.1: Distribution of improved wellbeing perception - Phase 1

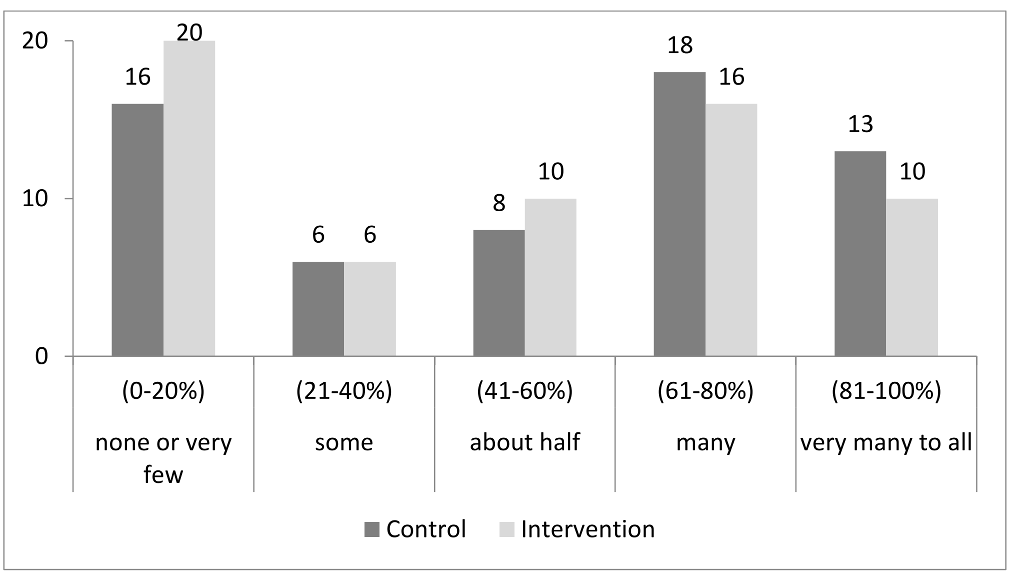


Figure 3.2: Distribution of improved wellbeing perception – Phase 2

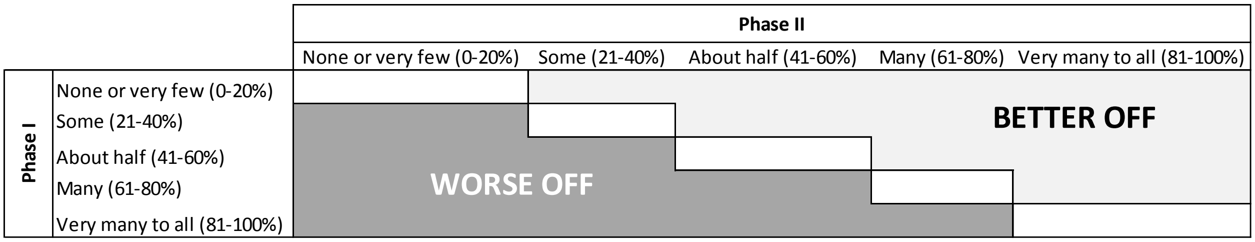


Figure 3.3: Change in wellbeing perception between phases

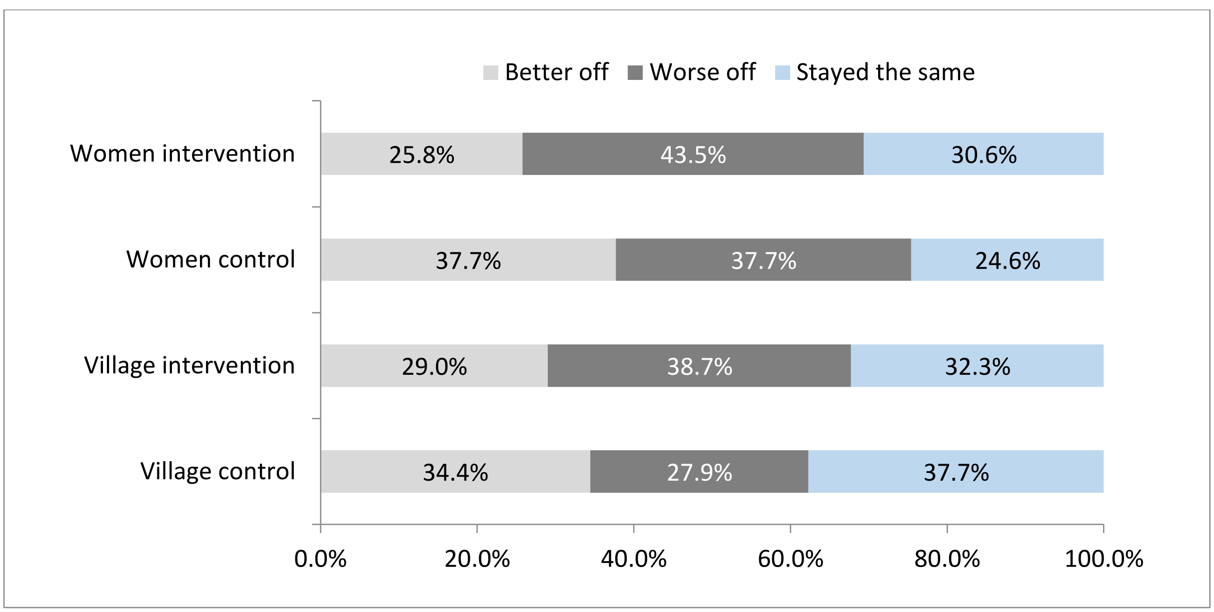


Figure 3.4: Perceived changes in wellbeing between phase 1 and phase 2, for women and village FGs in control and intervention sites

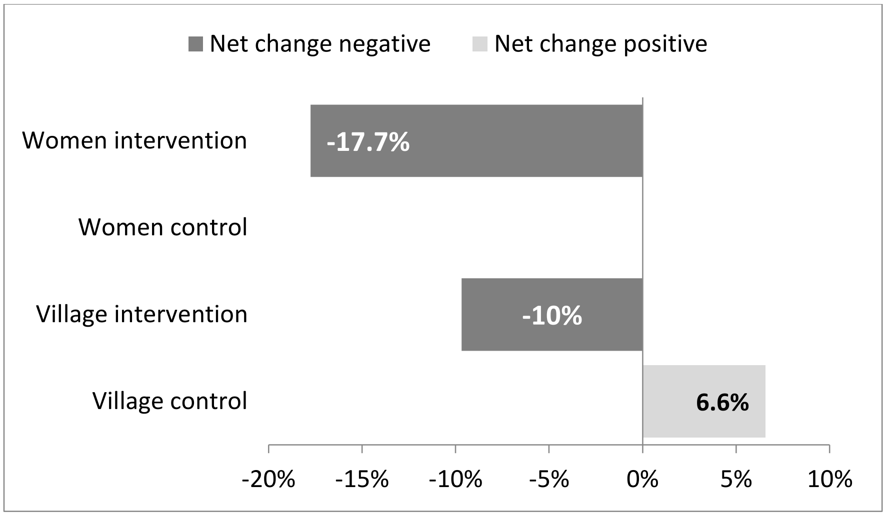


Figure 3.5: Net change in wellbeing of improvement category by type of focus group

Table 3.1: Sample by country

Country	Control	Intervention	TOTAL
Brazil	16	16	32
Peru	8	8	16
Cameroon	7	6	13
Tanzania	6	7	13
Indonesia	20	21	41
Vietnam	4	4	8
TOTAL	**61**	**62**	**123**

Source: GCS REDD+

Table 3.2: Descriptive Statistics at Village level – Phase 2

Variable	Type	Freq	Mean	Standard Deviation	SDM*	Ratio of variances (I/C)
Population	Control	56	1,280	1,434	-0.348	0.342
	Intervention	57	872	839		
Total area (has)	Control	61	33,281	114,276	-0.157	0.108
	Intervention	62	19,931	37,480		
Cellphone service	Control	61	0.77	0.42	-0.377	1.360
	Intervention	62	0.60	0.49		
Health center	Control	61	0.52	0.50	0.047	0.993
	Intervention	62	0.55	0.50		
Elementary shool	Control	61	0.85	0.36	-0.122	1.241
	Intervention	62	0.81	0.40		
Secondary School	Control	61	0.38	0.49	-0.012	0.993
	Intervention	62	0.37	0.49		
Distance to road (Km)	Control	59	5.6	14.4	0.190	9.079
	Intervention	62	11.8	43.4		
Distance to market (Km)	Control	60	34.2	68.4	0.033	0.393
	Intervention	62	36.1	42.9		
Wage female (US$ daily)	Control	61	8.8	7.2	-0.077	0.818
	Intervention	62	8.3	6.5		

Source: GCS REDD+ study

* Standardized Difference in Mean

Table 3.3: Descriptive Statistics at Household level – Phase 2

Variable	Type	Freq	Mean	Standard Deviation	SDM*	Ratio of variances (I/C)
HH gender (1=female)	Control	61	0.11	0.07	-0.045	1.287
	Intervention	62	0.11	0.09		
HH years of education	Control	61	5.7	2.2	-0.113	0.628
	Intervention	62	5.5	1.8		
Size	Control	61	4.9	1.1	-0.050	0.938
	Intervention	62	4.9	1.0		
Ethnic (1=yes)	Control	61	0.81	0.28	-0.056	1.009
	Intervention	62	0.79	0.29		
Forest income (US$ annual)	Control	61	1,186	2,282	-0.047	0.893
	Intervention	62	1,083	2,156		
Off farm income (US$ annual)	Control	61	3,490	3,579	-0.148	0.741
	Intervention	62	2,997	3,080		
Farming income (US$ annual)	Control	61	2,834	4,079	0.066	1.206
	Intervention	62	3,115	4,480		
Wealth (US$)	Control	61	9,660	17,295	-0.096	0.458
	Intervention	62	8,235	11,710		
Asset value (US$)	Control	61	4,315	6,194	-0.123	0.434
	Intervention	62	3,672	4,081		
Agriculture land (Has)	Control	61	10.7	16.5	0.051	0.801
	Intervention	62	11.5	14.7		
Forest land (Has)	Control	61	90.1	277.8	-0.076	0.621
	Intervention	62	71.2	218.9		
Land use (Has)	Control	61	1.2	1.9	-0.203	0.212
	Intervention	62	0.9	0.9		
HH clear (Has)	Control	61	0.4	0.4	-0.055	0.807
	Intervention	62	0.4	0.3		
PES income (US$ annual)	Control	61	9.0	31.5	-0.201	0.136
	Intervention	62	4.2	11.6		
NGO support income (US$ annual)	Control	61	6.7	30.2	-0.031	1.001
	Intervention	62	5.8	30.3		
Government support income (US$ annual)	Control	61	406.6	704.3	-0.231	0.371
	Intervention	62	272.0	428.9		

Source: GCS REDD+ study
* Standardized Difference in Mean

Table 3.4: Definitions of Wellbeing (frequency of mentions)

Definition description	Women		Village	
	Intervention	Control	Intervention	Control
good health	36	43	36	39
good education	36	34	43	39
sufficient food to eat	28	30	26	27
good quality house construction material	26	21	25	22
Own source of income	24	29		
ownership of transport items (boats, motorbikes, cars)	3	5	18	16
access to electricity	17	11	24	16
Tranquil / peaceful / harmonious / happy life	16	13	14	17
Union in family	13	16	8	8
Husband who provides for the family	9	10		
Union between people in community	7	10	2	6
self-sufficiency (no need for outside employment)			15	13

Source: Focus Group data

Table 3.5: Perceived changes (frequency) in women's wellbeing by country

Country	Better off		Worse off		Stayed the same		Total	
	Control	Intervention	Control	Intervention	Control	Intervention	Control	Intervention
Brazil	4	2	6	8	6	6	16	16
Peru	1	1	6	7	1	0	8	8
Cameroon	3	2	2	1	2	3	7	6
Tanzania	4	3	2	3	0	1	6	7
Indonesia	9	7	7	7	4	7	20	21
Vietnam	2	1	0	1	2	2	4	4
TOTAL	**23**	**16**	**23**	**27**	**15**	**19**	**61**	**62**

Source: GCS REDD+

Table 3.6: Perceived changes (frequency) in village wellbeing by country

Country	Better off		Worse off		Stayed the same		TOTAL	
	Control	Intervention	Control	Intervention	Control	Intervention	Control	Intervention
Brazil	5	3	4	7	7	6	16	16
Peru	1	0	5	8	2	0	8	8
Cameroon	2	3	5	1	0	2	7	6
Tanzania	3	5	1	1	2	1	6	7
Indonesia	9	6	2	4	9	11	20	21
Vietnam	1	1	0	3	3	0	4	4
TOTAL	**21**	**18**	**17**	**24**	**23**	**20**	**61**	**62**

Source: GCS REDD+

Table 3.7: Better off reasons related to wellbeing improvement (frequency of mentions)

Reason description (Top five for each group)	Women		Village	
	Intervention	Control	Intervention	Control
Good/increased/stable income from agriculture	13	13	22	12
Gender equity/women's empowerment	13	11	0	1
Good/increased service or support from government	9	10	10	6
Able to buy/build own house or improve condition of house or housing	9	5	12	8
Income, assets, savings, capital are adequate or increased or secure	9	3	11	9
Good/improved economic/business conditions	7	11	8	8
Introduction of or improved utilities (water, electricity, gas)	7	6	10	5
Got (new/additional/different) employment or income/increased work opportunities	6	11	7	16
Good/increased/stable income from animal husbandry	3	6	6	10
Introduction of or improved infrastructure (roads, bridges, waterways, irrigation)	3	15	7	12

Source: GCS REDD+ data

Note: dark grey shading highlights the most frequent response by group; light grey highlights the others in the top five.

Table 3.8: Worse off reasons related to village sample under study (frequency of mentions)

Reason description (Top five for each group)	Women		Village	
	Intervention	Control	Intervention	Control
Absence of/decreased safety, security, stability, morality, and cohesion in the community	8	3	6	6
Old age and reduced productivity	8	9	7	6
Insufficient/decreased/unstable income, assets, or savings	6	10	6	6
Absent/inadequate/worsening infrastructure (roads, bridges, waterways, irrigation)	5	2	6	4
Low/decreased/unstable income from agriculture	5	7	6	6
Illness in family	4	10	1	3
Low/decreased employment/income opportunities	3	4	4	7
Low/decreased/unstable prices of agricultural product(s)	1	0	2	7

Source: GCS REDD+ data

Note: dark grey shading highlights the most frequent response by group; light grey highlights the others in the top five.

Table 3.9: Women's Wellbeing Perception model – Interval regression

Variable	Units	Women
Intervention	Dummy (REDD+ village=1)	-8.656
		(5.196)*
Village total area	Has	-0.089
		(0.020)***
Cellphone service in the village	Dummy (available=1)	-13.576
		(7.861)*
Years of education of household head	Years (mean at village)	-4.591
		(2.756)*
Forest land of Household	Has (mean at village)	-0.265
		(0.099)***
Observations		**110**

* $p<0.1$; ** $p<0.05$; *** $p<0.01$

Table 3.10: Village Wellbeing Perception model – Interval regression

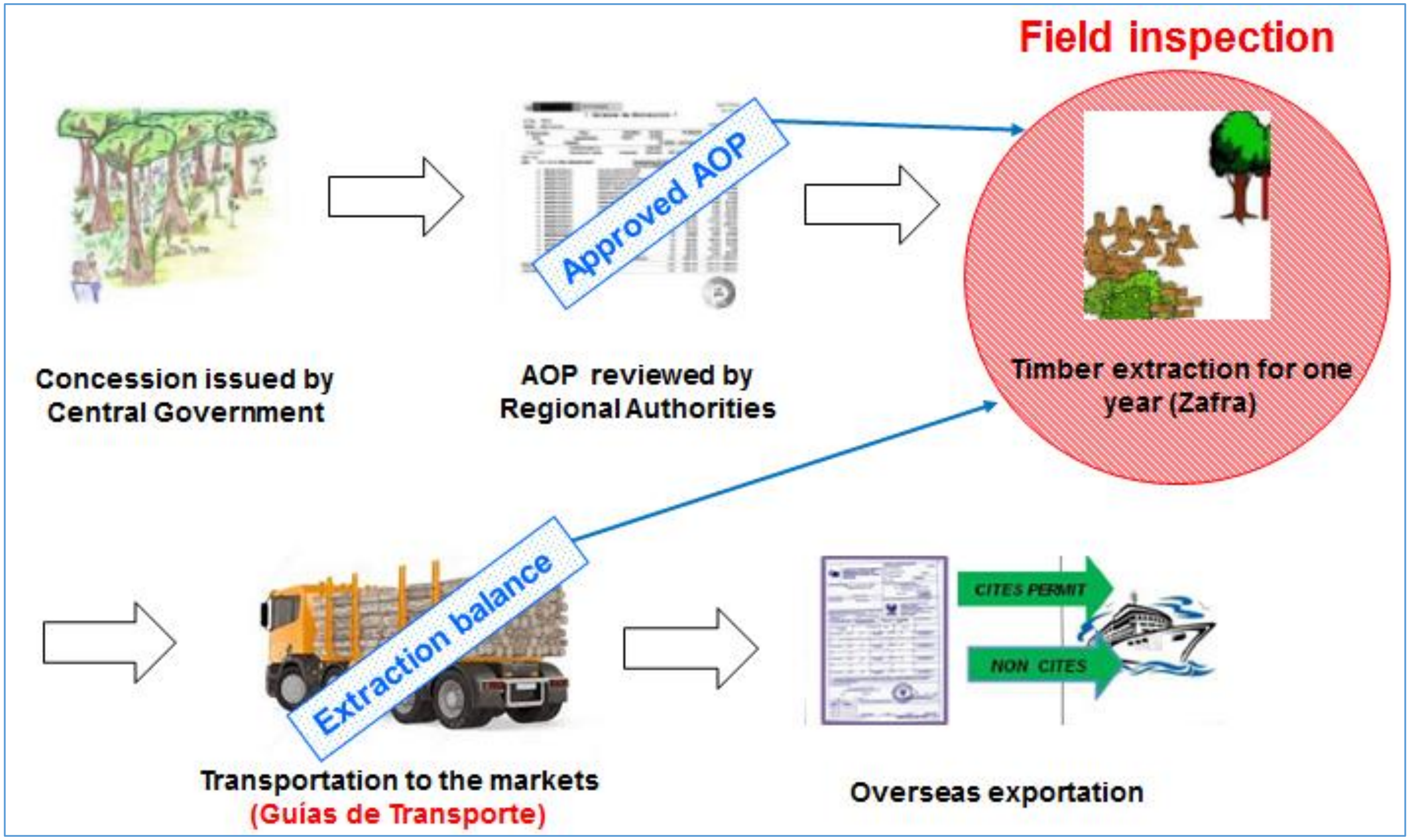
Variable	**Units**	**Village**
Intervention	Dummy (REDD+ village=1)	-1.122
		(4.299)
Elementary school	Dummy (Available=1)	17.837
		(9.934)*
Secondary school	Dummy (Available=1)	-14.96
		(5.386)***
Distance to road	Km	0.182
		(0.049)***
Distance to market	Km	-0.083
		(0.028)***
Off farm income	US $ (mean at village)	3.751
		(2.260)*
Agriculture land	Has (mean at village)	0.566
		(0.343)*
Forest cleared by HH	Has (mean at village)	24.181
		(8.162)***
Payment for Environmental Service income	US $ (mean at village)	123.047
		(70.905)*
Utility Index	Index (mean at village)	11.297
		(2.700)***
House condition index	Index (mean at village)	6.371
		(3.355)*
Observations		110

* p<0.1; ** p<0.05; *** p<0.01

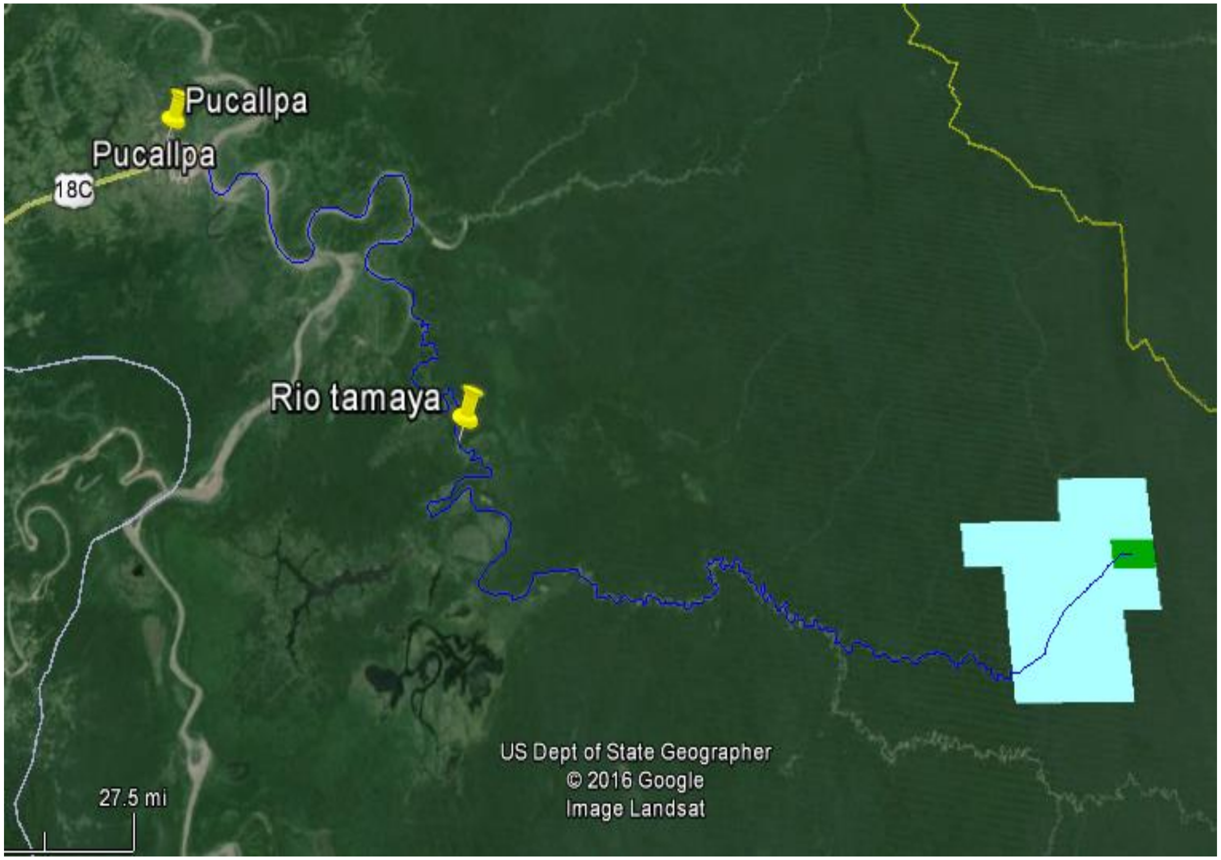
Note: Standard errors in parenthesis

APENDICES

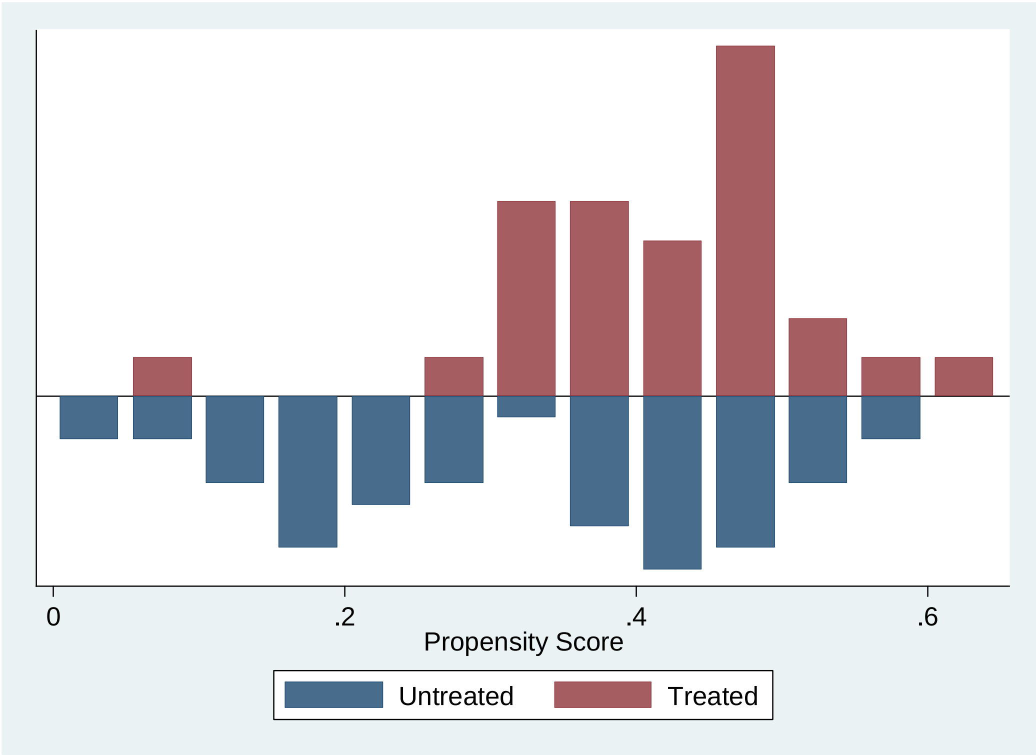
Appendix 1: Forest concession regulation in Peru



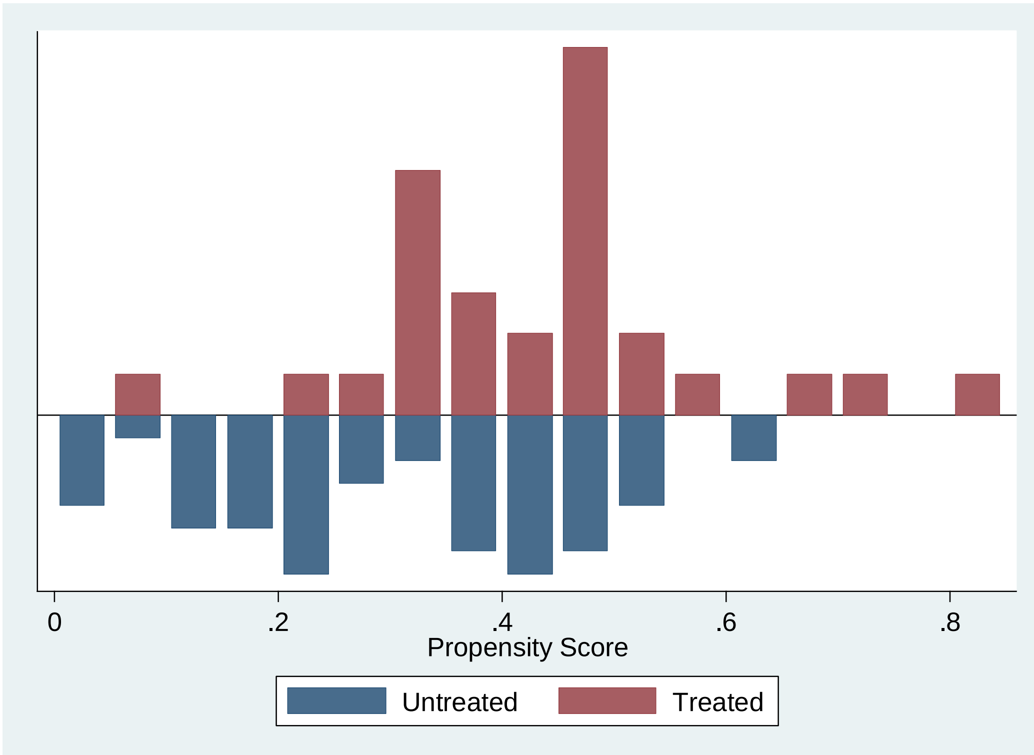
Appendix 2: Example of estimated distance traveled following the field inspection report



Appendix 3: Histogram of probability distribution of propensity score model 1



Appendix 4: Histogram of probability distribution of propensity score model 2



Appendix 5: ATET estimates, including under-reported cases

Propensity Model	ATET	Standard Error	P-value	Lower bound on CI	Upper bound on CI	Matched control obs
Model 1	**-1,885.1**	803.7	0.02	-3,460.3	-309.9	18
Model 2	**-1,829.1**	1,372.4	0.18	-4,518.9	860.8	16

Appendix 6: Logic Model of Ucayali REDD+ project

Inputs	Activities	Outputs	Outcomes Short term (1 or 2 years)	 Medium term (3 to 5 years)
Collaborative participation with international organization which provide funding to implement REDD+ projects	1.- Improved forest management training			
	1.1 Capacity-building for local community members to participate in forest inventories and mapping	1.1 Local labor trained in forest inventory and mapping	1.1 Decreased cost (because no longer need to hire external professionals)	
	1.2 FSC Forest Management Certification	1.2 Certified timber	1.2 Increased timber prices (because certified)	
	2.- Construction of sawmill and warehouse for lumber	2.- Production of sawn timber	2. Increased revenues from wood products	
	3.- Seeds for reforestation of tanoni trees which produce seeds used for artisanal products	3.- Increased production of NTFPs		3.- Increase quantity of seeds for handicraft
	4.- Inputs and technical assistant for establishment of bolaina (Guazuma crinite) timber plantations	4.- Availability of short rotation timber		4.- Increase quantity of timber
	5.- Training in improved management of paiche (Arapaima gigas) fish	5.- Repopulating and marketing the paiche fish		5.- Increase quantity of fish

Appendix 7: Logic Model of Madre de Dios REDD+ project

Inputs	Activities	Outputs	Outcomes: Short term (1 or 2 years)	Outcomes: Medium term (3 to 5 years)
Funding from carbon credits sales through voluntary markets	1.- Technical support and legal assistance			
	1.1 technical support to prepare forest management plan for brazil nut, annual operation plan for brazil nut, and complementary plan to harvest timber	1.1 Concessions have all management documents required by law		1.1 Ensure the right to access to brazil nut and timber
	1.2 technical support to implement forest inventories	1.2 Concessionaires trained in forest inventory	1.1 Decreased cost (because no longer need to hire external professionals)	
	1.3 technical and legal assistance to delineate concession boundaries	1.3 Effective control of brazil nut concessions		1.1 Ensure control over concession to access to brazil nut and timber
	2.- Credit line for working capital in brazil nut activity	2.- Reduce informal credits from brazil nut buyers which generates sales binding contract with lower prices	2.- Increase the price of brazil nut	
	3.- Workshops on harvesting and post-harvesting activities of brazil nut	3.- Reduce losses on storage of brazil nut	3.- Reduce losses on storage (because humidity)	
	4.- Collection services (with tractors) of brazil nut	4.- Provide collection service of brazil nut	4.- Reduce cost of transportation of brazil nut	

Appendix 8: Descriptive statistics Ucayali Site

Type	Variable	Group	Obs	Mean	Standard Deviation	SDM*	RV** (T/C)
Head of household	Age	Control	95	44.7	12.8	-0.146	0.966
		Treated	60	42.9	12.5		
	Years of education	Control	94	8.5	3.9	0.191	0.807
		Treated	60	9.3	3.5		
	Years living at village	Control	95	30.5	15.9	-0.031	0.756
		Treated	60	30.1	13.8		
	Born at Village (%)	Control	95	0.5	0.5	0.040	1.011
		Treated	60	0.5	0.5		
Household information	Size	Control	95	5.5	2.2	0.066	0.769
		Treated	60	5.7	1.9		
	Adult equivalent	Control	95	3.6	1.2	0.059	0.794
		Treated	60	3.7	1.1		
	Years formed	Control	91	20.0	12.3	-0.028	0.847
		Treated	58	19.6	11.4		
	Asset value (US$)	Control	95	846.6	843.5	0.124	2.481
		Treated	60	984.5	1,328.7		
	House condition index	Control	95	6.6	1.6	0.041	0.608
		Treated	60	6.6	1.2		
	Utilities index	Control	95	4.4	0.7	-0.137	1.784
		Treated	60	4.3	0.9		
	Total land (Has)	Control	95	4.4	4.1	0.006	1.382
		Treated	60	4.4	4.8		
	Total annual income (US$)	Control	95	13,689.5	47,592.8	-0.093	0.066
		Treated	60	10,454.4	12,236.5		
	Annual farming income (US$)	Control	95	2,426.5	2,932.9	-0.152	0.668
		Treated	60	2,019.6	2,396.9		
	Annual off-farming income (US $)	Control	95	1,144.7	2,212.3	-0.024	0.950
		Treated	60	1,092.7	2,156.3		
	Annual forest income (US$)	Control	95	10,010.4	46,566.0	-0.080	0.053
		Treated	60	7,293.1	10,738.0		
	Annual timber income (US$)	Control	95	512.9	1,989.3	0.082	1.916
		Treated	60	709.9	2,753.5		
	Agriculture land (Has)	Control	95	2.2	2.4	-0.091	1.458
		Treated	60	2.0	3.0		
	Forest land (Has)	Control	95	2.1	2.1	0.113	1.584
		Treated	60	2.4	2.7		

* Standardized Difference in Mean, ** Ratio of Variances

Appendix 9: Descriptive statistics Madre de Dios Site

	Variable	Group	Obs	Mean	Standard Deviation	SDM*	RV** (T/C)
Head of household	Gender (%)	Control	104	0.16	0.4	0.133	1.243
		Treated	65	0.22	0.4		
	Age	Control	104	53.4	14.7	0.050	0.724
		Treated	65	54.1	12.5		
	Years of education	Control	104	7.0	4.3	0.097	0.676
		Treated	65	7.4	3.5		
	Years living at village	Control	104	32.5	17.2	0.199	1.152
		Treated	65	36.0	18.5		
	Born at Village (%)	Control	104	0.21	0.4	0.221	1.285
		Treated	65	0.31	0.5		
Household information	Size	Control	104	3.9	1.9	-0.064	1.163
		Treated	65	3.8	2.0		
	Adult equivalent	Control	104	2.8	1.2	-0.046	1.179
		Treated	65	2.8	1.3		
	Years formed	Control	104	23.1	14.1	0.025	1.083
		Treated	64	23.4	14.7		
	Asset value (US$)	Control	104	9,645.0	16,470.5	-0.069	0.255
		Treated	65	8,741.2	8,321.6		
	House condition index	Control	104	6.9	1.1	0.130	1.372
		Treated	65	7.0	1.2		
	Utilities index	Control	104	6.3	2.0	0.161	0.899
		Treated	65	6.6	1.9		
	Distance from home to near road (km)	Control	104	35.7	24.2	-0.307	1.686
		Treated	65	27.1	31.4		
	Number of homes own out of the village	Control	104	0.88	0.8	-0.476	0.801
		Treated	65	0.54	0.7		
	Total land (Has)	Control	104	1,009.3	1,794.3	-0.177	0.087
		Treated	65	775.4	530.3		
	Total annual income (US$)	Control	104	21,838.0	29,150.5	-0.220	0.184
		Treated	65	16,902.2	12,499.0		
	Annual farming income (US$)	Control	104	2,329.2	4,714.7	-0.328	0.254
		Treated	65	1,105.3	2,377.4		
	Annual off-farming income (US $)	Control	104	4,550.8	8,851.9	-0.050	0.494
		Treated	65	4,165.1	6,220.1		
	Annual forest income (US$)	Control	104	14,945.4	26,362.0	-0.167	0.136
		Treated	65	11,631.7	9,716.3		
	Annual brazil nut income (US$)	Control	104	11,090.4	15,307.1	-0.213	0.228
		Treated	65	8,538.3	7,313.4		
	Agriculture land (Has)	Control	104	13.5	19.2	0.124	3.343
		Treated	65	17.0	35.0		
	Forest land (Has)	Control	104	995.8	1,793.6	-0.179	0.088
		Treated	65	758.5	533.4		

* Standardized Difference in Mean, ** Ratio of Variances

Appendix 10: REDD+ interventions involved in the sample study

REDD+ project	Control	Intervention	TOTAL
Brazil			
Acre	4	4	8
Cotriguacu	4	4	8
Transamazon	4	4	8
SFX	4	4	8
Peru			
Madre de Dios	4	4	8
Ucayali	4	4	8
Cameroon			
SE Cameroon	4	2	6
Mt. Cameroon	3	4	7
Tanzania			
Shinyanga	4	4	8
KILOSA	2	3	5
Indonesia			
Ulu Masen	4	4	8
KCCP	4	4	8
KFCP	4	4	8
Katingan	4	4	8
TNC within BFCP	4	5	9
Vietnam			
Cat Tien	4	4	8
TOTAL	**61**	**62**	**123**

Source: GCS REDD+

Appendix 11: Transition matrix description process

The women's wellbeing perception transition matrix aims to identify the individual perception movement of each women's focus group from Phase 1 and Phase 2.

Step 1: Constructing panel data

The first step is identifying the women's focus groups in Phase 1 and Phase 2. Given that the focus group was carried out at the village level, we search for the same village names in Phase 1 and Phase 2. Therefore, only villages with information in both phases will be included in the analysis.

Step 2: Generating the transition matrix

For each panel village identified in step 1, we identify the portion of women in Phase 1 and Phase 2 who indicated wellbeing improvement. Given that this a two-way table, each women's focus group has only one entry (it will be one frequency) in one of 25 possible combinations. The table below provides a graphic illustration:

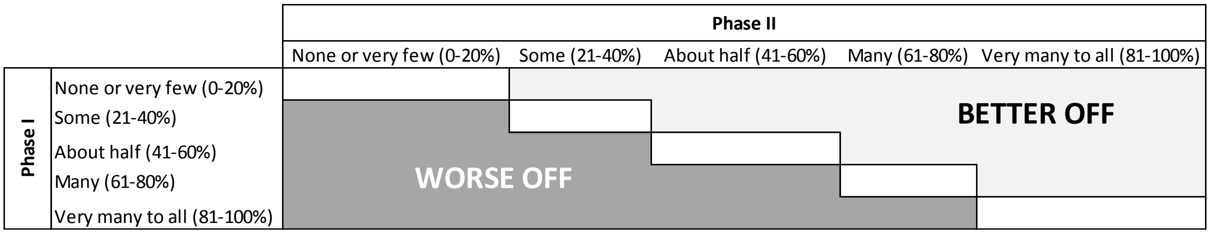
		Phase II				
		None or very few (0-20%)	Some (21-40%)	About half (41-60%)	Many (61-80%)	Very many to all (81-100%)
Phase I	None or very few (0-20%)					
	Some (21-40%)					
	About half (41-60%)					
	Many (61-80%)					
	Very many to all (81-100%)					

Step 3: Identifying transition movements

To define the transition movements, we note the direction of movement (up or down). Hence, for wellbeing improvement, we define each movement from a lower position in Phase 1 to a higher position in Phase 2 to be positive (better off) and each movement from a higher position In Phase 1 to lower position in Phase 2 to be a negative movement (worse off).

It is important to highlight that instances that are invariant between Phase 1 and Phase 2 (elements in the diagonal) will not be considered as a transition movement. Likewise, the answers that respondents do not know (RDNK) will not be included. The illustration below shows an example of this step for wellbeing improvement.

Example: Wellbeing improvement



Step 4: Aggregating transition movements

Finally, we aggregate the frequencies at global, country and site level for each transition movement (better off or worse off). Given that elements in the diagonal (same status between periods) and answer (respondent does not Know) will not be considered in the transition movements, the addition of better off and worse off will not equal 100%.

Appendix 12: Interval regression results for wellbeing improvement perception (Full variables)

Type	Variable	Unit	Women	Village
Village information	Treatment	Dummy (REDD+ = yes)	-8.656	-1.122
			(5.196)*	(4.299)
	Population		0.004	0.004
			(0.003)	(0.003)
	Village area	Has	-0.089	0.003
			(0.020)***	(0.011)
	Cellphone	Dummy (1=yes)	-13.576	5.388
			(7.861)*	(4.759)
	Health center	Dummy (1=yes)	-1.363	-8.624
			(7.934)	(6.041)
	Elementary school	Dummy (1=yes)	-0.571	17.837
			(11.376)	(9.934)*
	Secondary school	Dummy (1=yes)	-6.932	-14.96
			(7.618)	(5.386)***
	Distance to the road	Average	0.017	0.182
			(0.096)	(0.049)***
	Distance to market	Average	0.056	-0.083
			(0.035)	(0.028)***
	Female wage (daily)	Average	-0.025	-0.628
			(1.237)	(0.746)
Household information	Gender of head	Dummy (1=female)	20.177	-15.366
			(35.059)	(33.849)
	Years of education of head	Average	-4.591	-4.1
			(2.756)*	(3.05)
	Size	Average	-0.045	0.038
			(3.784)	(3.46)
	Ethnic of head	Dummy (1=local)	-0.415	-24.992
			(19.207)	(17.63)
	Forest income (annual)	Average (US $)	-6.085	6.148
			(8.823)	-4.274
	Off farming income (annual)	Average (US $)	3.84	3.751
			(3.04)	(2.260)*
	Farming income (annual)	Average (US $)	0.487	0.711
			(1.363)	(1.106)
	Wealth	Average (US $)	0.355	-0.039
			(0.886)	(0.65)
	Assests	Average (US $)	0	-0.001
			(0.002)	(0.002)
	Agriculture land	Has	-0.632	0.566
			(0.558)	(0.343)*
	Forest land	Has	-0.265	-0.101
			(0.099)***	(0.089)
	Land used by Household	Has	1.565	-0.761
			(1.561)	(1.271)
	Land cleared by household	Has	18.803	24.181
			(12.956)	(8.162)***
	PES income (annual)	Average (US $)	-149.188	123.047
			(152.006)	(70.905)*
	NGSO support income (annual)	Average (US $)	-0.029	-0.024
			(0.059)	(0.045)
	Government support income (annual)	Average (US $)	-0.006	0.006
			(0.007)	(0.004)
	Utilities index		3.91	11.297
			(3.377)	(2.700)***
	House condition index		6.814	6.371
FG information			(4.729)	(3.355)*
	Age		-0.826	0.663
			(0.617)	(0.484)
	Size		-0.086	-0.157
			(0.387)	(0.24)
	Observations		110	110

* p <0.1; ** p <0.05; *** p <0.01

Note: Standard errors in parenthesis