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Ph.D. Thesis

Effectiveness of Payments for Environmental Services in Curbing Deforestation in the Brazilian Amazon

by

Gabriela Demarchi

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À minha querida avó Sybilla

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Note to the reader

The three chapters of this thesis are self-contained research papers that can be read separately. They are preceded by a general introduction that summarizes the research presented in this thesis. Three additional peer-reviewed publications to which I contributed while working on this thesis can be found in the Appendix.

Abstract

Payments for Environmental Services (PES) have become a popular forest conservation tool in recent years, with numerous new schemes springing up around the world, particularly in developing countries. Nonetheless, despite an expanding body of academic literature on the subject, their environmental and economic impacts, both short and long term, are still poorly known. The three chapters in this thesis explore the effectiveness of PES programs in the Brazilian Amazon using the most advanced impact analysis methods, focusing on concrete and emblematic cases of public initiatives aimed at addressing the issue. of climate change in the tropics. The first and second chapters examine the performance of the Sustainable Settlements in the Amazon (SSA) program, which between 2013 and 2017 provided technical assistance and conditional payments to 350 households in the state of Para in exchange for maintaining forest cover on at least half of their land. The first chapter of the thesis, estimates the effectiveness of the SSA program on preserving natural forests using two publicly available remote sensing deforestation datasets. Using matching and panel data from a sample of farm holders both enrolled and not enrolled in the program, we estimated the loss of forest cover that would have occurred in the absence of the program. We find evidence that the program was successful in reducing deforestation during its early years of implementation. Nonetheless, at the end of the program, the participants resume their normal rate of deforestation, i.e. the one they would have had in the absence of payments. Despite this, we found no evidence of participants catching up on avoided deforestation by accelerating deforestation after payments were suspended, implying that the program's gains were still preserved – at least until the date of our analysis. We calculated the three-year delayed CO_2 emissions highlighted by the impact assessment and discovered that the benefits of the SSA program outweighed the costs. Our study adds to the existing literature by providing new evidence on the additionality, permanence and cost-effectiveness of forest conservation programs based on PES.

The second chapter complements the previous analysis by mobilizing additional data in order to determine which decisions relating to land use and agricultural activity have enabled participants in the SSA program to forgo cutting part of the

native forest on their farmland. Using counterfactual impact evaluation methods we assessed the impact of the project over 2013-2019, i.e., from its first year until two years after its end. Based on the Theory of Change, we focused on land use and socioeconomic outcomes likely to have been affected by changes in deforestation brought about by the initiative. The main findings suggest that the decrease in deforestation has occurred mainly at the expense of the slowdown in the expansion of pasture areas. Furthermore, our results indicate that the program had a positive impact on the intensification in cattle ranching activities, on farmers' incomes and on alternative livelihood production activities that require less area for production than extensive livestock farming and slash-and-burn agriculture. Altogether these results suggest that conservation programs that combine PES with technical assistance and support to farmers for the adoption of low-impact activities can be effective in the fight against climate change without jeopardizing the livelihood of local populations. This study adds to the existing literature by providing unique evidence on the permanence of the impacts of PES conservation programs on locals' livelihoods.

The third chapter provides the results of an experimental Becker-DeGroot-Marschak (BDM) auction implemented in western Brazilian Amazon among a sample of 300 landowners. The auction had the aim of eliciting farmers' willingness to accept (WTA) to enter two PES contracts with different forest conservation thresholds. The main findings suggest that the average WTA, whatever the PES proposed, is low relative to the social cost of carbon and that farmers tend to bid higher in a PES auction that offers contracts with the most difficult to achieve environmental targets. Moreover, using survey data collected from the participants, we found that certain types of farmers consistently bid higher than others, regardless of the contract offered. We also found that this type of farmer places greater value on flexibility in PES contracts. Although further research from the field is warranted to determine which type of PES contract is ultimately most cost-effective, these results suggest that measuring WTA provides a key input for shaping pricing policy and guiding the magnitude and targeting of forest conservation PES contracts in the Amazon.

Keywords: Impact evaluation, Climate policy, Deforestation, Payments for environmental services, Brazilian Amazon.

Résumé

Au cours des dernières années, les paiements pour services environnementaux (PSE) sont devenus un instrument de conservation des forêts très populaire, avec de nombreux nouveaux programmes mis en place dans le monde entier, en particulier dans les pays en développement. Néanmoins, malgré une littérature académique croissante sur le sujet, leurs impacts environnementaux mais aussi économiques, à court comme à long terme, sont encore mal connus. Les trois chapitres de cette thèse étudient l'efficacité des programmes de PSE en Amazonie brésilienne à l'aide des méthodes d'analyse d'impact les plus avancées, en se focalisant sur des cas concrets et emblématiques des initiatives publiques visant à résoudre la question du changement climatique dans les zones tropicales. Les deux premiers chapitres examinent la performance du programme Sustainable Settlements in the Amazon (SSA), qui, entre 2013 et 2017, a proposé une assistance technique et des paiements conditionnels à 350 ménages situés dans l'État du Para, en échange du maintien d'une couverture forestière sur au moins la moitié de leurs terres. Le premier chapitre de la thèse, évalue l'efficacité du programme SSA sur la préservation des forêts naturelles en utilisant deux bases de données de déforestation issues de la télédétection, publiquement disponibles. En utilisant des techniques d'appariement et des données de panel provenant d'un échantillon de propriétaires agricoles inscrits et non-inscrits au programme, nous avons reconstitué le scenario contrefactuel, c'est-à-dire la perte de couvert forestier qui aurait été observée en l'absence du programme. Nos résultats suggèrent que le programme aurait permis de réduire significativement la déforestation au cours de ses premières années de mise en œuvre. Néanmoins, à la fin du programme, les participants sont retournés à taux normal de déforestation, c'est-à-dire celui qu'ils auraient eu en l'absence de paiements. Malgré cela, nous n'avons trouvé aucune preuve que les participants avaient cherché à "rattraper la déforestation évitée" en accélérant la déforestation après la suspension des paiements, ce qui implique que les gains du programme ont été préservés - du moins jusqu'à la date de notre analyse. Nous avons calculé les émissions de CO_2 différées sur trois ans grâce au programme et mis en évidence que les bénéfices du programme SSA avaient surpassé les coûts. Notre étude contribue à la littérature existante en fournissant de nouvelles preuves de l'additionnalité, de

la permanence et de l'efficacité environnementale et économique des programmes de conservation des forêts s'appuyant sur les PSE.

Le deuxième chapitre complète l'analyse précédente en mobilisant des données supplémentaires, permettant de déterminer quelles décisions relatives à l'occupation des sols et aux pratiques agricoles ont permis aux participants au programme SSA de renoncer à couper une partie leur forêt. En utilisant des méthodes contrefactuelles, nous avons évalué l'impact du projet sur la période 2013-2019, c'est-à-dire de sa première année jusqu'à deux ans après sa clôture. En nous appuyant sur la théorie du changement, nous nous sommes concentrés sur l'occupation des sols et les résultats socio-économiques susceptibles d'avoir été affectés par les changements dans les trajectoires de déforestation induites par l'initiative. Les principaux résultats suggèrent que la diminution de la déforestation s'est produite principalement au détriment d'un ralentissement de l'expansion des zones de pâturage. En outre, nos résultats indiquent que le programme a eu un impact positif sur l'intensification des activités d'élevage bovine, sur les revenus des agriculteurs et sur les activités de production de moyens de subsistance alternatifs qui nécessitent moins de surface pour la production que l'élevage extensif et l'agriculture sur brûlis. Dans l'ensemble, ces résultats suggèrent que les programmes de conservation qui combinent les PSE avec une assistance technique et un soutien aux agriculteurs pour l'adoption d'activités à faible impact environnemental peuvent être efficaces dans la lutte contre le changement climatique, sans toutefois mettre en péril les moyens de subsistance des populations locales. Cette étude s'ajoute à la littérature existante en apportant des preuves nouvelles de la permanence des impacts des programmes de conservation basés sur les PSE.

Le troisième chapitre présente les résultats d'une enchère expérimentale de type Becker-DeGroot-Marschak (BDM), mise en œuvre dans l'ouest de l'Amazonie brésilienne auprès d'un échantillon de 300 propriétaires fonciers. L'enchère avait pour objectif d'évaluer le consentement-à-recevoir (CAR) des agriculteurs invités à participer à deux programmes de PSE, caractérisés par des objectifs de conservation différents. Les principaux résultats suggèrent que, quel que soit le contrat de PSE proposé, le CAR moyen est faible par rapport au coût social du carbone. Ils montrent aussi que les agriculteurs ont tendance à faire des offres plus élevées dans les enchères offrant des contrats de PSE dont les objectifs environnementaux sont plus difficiles à atteindre. De plus, en utilisant les données d'enquête recueillies auprès des participants, nous avons montré que certains types d'agriculteurs font systématiquement des offres plus élevées que les autres, quel que soit le contrat proposé. Nous avons également constaté que ce type d'agriculteur accorde une plus grande valeur aux contrats de PSE intégrant un cahier des charges plus flexible. Bien que d'autres recherches sur le terrain soient nécessaires pour déterminer quel type de contrat de PSE est finalement le plus efficace, ces résultats suggèrent

que la mesure du CAR est un élément clé pour l'élaboration des politiques de compensation, la détermination du montant et la définition du ciblage des contrats de PSE pour la conservation des forêts en Amazonie.

Mots-clés : Évaluation d'impact, Politique climatique, Déforestation, Paiements pour services environnementaux, Amazonie brésilienne.

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Introduction

Climate change is arguably the greatest challenge facing society today and in the coming decades (IPCC, 2022). The Agriculture, Forestry, and Other Land Use sector (AFOLU) is the second largest source of anthropogenic greenhouse gas (GHG) emissions, with deforestation[1] accounting for almost half of total AFOLU emissions (Van der Werf et al., 2009; IPCC, 2022). Yet the role of forests in climate change is twofold: they are both a cause of and a solution to the climate crisis. Plants and soils in terrestrial ecosystems currently absorb the equivalent of one-fifth of human-related GHG emissions, implying that forests have the potential to provide significant additional climate mitigation by combining enhanced carbon sinks with reduced emissions (Griscom et al., 2017).

According to the Intergovernmental Panel on Climate Change (IPCC), AFOLU mitigation options can result in large-scale GHG emission reductions and enhanced removals[2] (IPCC, 2022). The protection, improved management, and restoration of forests and other ecosystems account for the bulk of potential emission reductions, with reduced deforestation in tropical regions having the largest mitigation potential. In this context, reducing deforestation in the Brazilian Amazon could be a cost-effective way to reduce GHG emissions and mitigate climate change.

As the world's largest rainforest, the Amazon stores enormous amounts of carbon, which is released when trees are cut down and burned. According to Soares-Filho et al. (2006), Amazonian trees store between 90 and 140 billion tons of carbon. Brazil has the largest share of the Amazon, and according to the University of Maryland's Global Forest Watch data, the country leads the world in humid tropical primary forest loss, due to fires and clear-cutting. In 2020, Brazil lost 1.7 million hectares, more than three times the amount lost by the second-ranked country (the Democratic Republic of Congo).

For many years, the international community has been concerned about rising deforestation rates in the Brazilian Amazon. Although addressing the climate crisis is one of society's most urgent priorities, some worry that laws enacted to

[1] Deforestation is also a major driver of biodiversity loss (Tilman et al., 2017).

[2] According to the IPCC's 2022 report, the AFOLU sector's GHG mitigation potential between 2020 and 2050 ranges between 8 and 14 gigatons of carbon dioxide equivalent per year.

protect forests will stifle economic growth in countries that produce food and rely heavily on land use in their development plans. Indeed, reducing deforestation requires limiting agricultural expansion, as agriculture is still by far the leading cause of global deforestation (Pendrill et al., 2019).

In the current context of accelerated globalization and international commodity demand, and rising inequality and food insecurity, a variety of solutions have been considered to address escalating rates of forest loss in Brazil. Deforestation in the Amazon has been significantly reduced as a result of the implementation of command-and-control measures, the expansion of protected areas (PAs) and interventions in the soy and beef supply chains (Nepstad et al., 2014; Assunção, Gandour, and Rocha, 2015; Villoria et al., 2022). However, after decades of policies aimed primarily at larger farms, the debate over environmental policy is increasingly embracing incentive-based approaches to forest conservation. There is now agreement that new mechanisms aimed at small landowners are required to achieve additional reductions in Amazon rainforest deforestation (Börner et al., 2017; Godar et al., 2014).

Offering payments for ecosystem services (PES) contracts to small landowners has emerged as a potential strategy that may achieve both food security and forest conservation goals in Brazil. Forest conservation PES schemes are contracts between a landowner and the conservation buyer (typically the government or an NGO) through which the landowner receives a payment conditional to conserving the forest located on his land (Wunder et al., 2005; Engel, Pagiola, and Wunder, 2008). Introducing PES as a complementary policy measure can reduce income losses for those smallholders hit hardest by forest law enforcement. Several states in the Brazilian Amazon, including Acre and Amazonas, have passed legislation to make it easier to use public funds for PES-type arrangements, and a bill to establish a national PES program has already cleared several administrative hurdles (Börner, Marinho, and Wunder, 2015) .

PES schemes have gained a lot of attention in the global climate change agenda through the REDD+ mechanism[3]. The main idea of REDD+ is that rich countries pay developing countries, communities, and/or individuals to preserve their forests and reduce greenhouse gas emissions (Sills et al., 2014). Local REDD+ projects are often used as test-beds for larger-scale initiatives. Drawing lessons from REDD+ project experiences may thus aid in the design of jurisdictional programs, which is the focus of the Paris Agreement's REDD+ implementation (Wunder et al., 2020). Large-scale policies, if successful, are indeed likely to play an important role in the future (Basu, Meghani, and Siddiqi, 2017). In this perspective, the

[3]Acronym for reducing emissions from deforestation and forest degradation, conservation of existing forest carbon stocks, sustainable forest management and enhancement of forest carbon stocks

REDD+ mechanism is promising, at least in principle, but we need more evidence about if and how these pilot projects work on the ground. In particular, it is crucial to evaluate to what extent a program that succeeds in preserving native forest also guarantees sustainable livelihoods of local populations (Blundo-Canto et al., 2018).

According to Sunderlin et al. (2017), most REDD+ local projects are hybrids of the integrated conservation and development project (ICDP) approach and of newer forest conservation approaches such as PES schemes. Around half of the REDD+ projects implemented worldwide include a component of payment to local communities (Simonet et al., 2014). It is expected that this nature-based solution will promote climate change mitigation measures and bring environmental and social co-benefits such as poverty reduction, while conserving biodiversity and sustaining vital ecosystem services, thus reconciling global environmental goals with local development priorities specific to tropical regions. For many reasons, however, such initiatives are likely to fail (Wunder, 2007, 2008; Ferraro, 2008). What we need is more evidence on how REDD+ projects perform in practice, both economically and environmentally.

More than 15 years have now passed since REDD+ entered the climate policy arena, but evidence of its effectiveness remains scarce (Duchelle et al., 2018). More specifically, we still know little about the long-term effectiveness of REDD+ initiatives to mitigate climate change while safeguarding the peoples whose livelihoods depend upon tropical forests. Some of the few rigorous impact evaluations from the REDD+ literature suggest that initiatives are somehow effective in reducing deforestation in the short-term (Jayachandran et al., 2017; Simonet et al., 2018; Roopsind, Sohngen, and Brandt, 2019), while other studies find null or mixed impacts (Correa et al., 2020; West et al., 2020; Bos et al., 2017). One of the reasons for this lack of evidence is the high cost of accessing the longitudinal individual-level data needed for impact analysis (Ferraro et al., 2012; Blackman, 2013; Pagiola, Honey-Rosés, and Freire-González, 2016). In this thesis, I apply the most advanced impact analysis methods to data collected from different sources (both satellite imagery and surveys) to answer three important questions for which there is still little empirical evidence in the forest conservation literature in general and on the Amazon rainforest in particular:

1. What short and long term impacts do PES-based REDD+ programs have on forest cover loss and carbon sequestration?

2. What is the impact of REDD+ forest conservation programs on the economic development of program beneficiaries? Are the dual objectives of natural resource preservation and food security compatible?

3. Can we improve the effectiveness of PES programs by eliciting the so-called

individual willingness-to-accept (WTA) of the households targeted by these programs?

The first chapter of the thesis, "Using publicly available remote sensing products to evaluate REDD+ projects in Brazil", estimates the effectiveness of the Sustainable Settlements in the Amazon (SSA) program, a REDD+ program that offered 350 households in the state of Para technical assistance and conditional payments for maintaining forest cover on at least half of their land between 2013 and 2017. We used matching and panel data from a sample of farm holders both enrolled and not enrolled in the program to estimate counterfactuals for the outcomes of interest. Our study adds to the existing literature by providing new evidence on the additionality, permanence and cost-effectiveness of REDD+ programs, focusing on a region characterized by the world's highest annual forest loss, while previous studies have focused on areas where deforestation may appear less pressing (Cisneros et al., 2022). To the best of our knowledge, despite the abundance of REDD+ initiatives in the Brazilian Amazon, there are no studies that assess the long-term impacts of such projects combining ready-to-use remote sensing (RS) products and farm-level data. Our study aims to fill this gap. Furthermore, this is the first study that assesses the effectiveness of a Brazilian local REDD+ project conservation program using two different sources of remotely-sensed deforestation data to cross-validate impact assessment results.

Our results suggest that the local REDD+ initiative was effective in reducing deforestation during the early years of its implementation in the Transamazon region, an area with historically high deforestation rates. This result suggests that PES programs targeting smallholders in the Brazilian Amazon may well be effective, at least in the short-run. Moreover, we found evidence that non-enrolled farm-holds located close to enrolled ones were somehow impacted by the program, as they also decreased deforestation during the early years of program implementation. This suggests that PES programs may change the behavior of farmers who are not the primary beneficiaries of the program – although we are not able to determine through which channel this occurs. More importantly, we found that participants resumed their normal level of deforestation even before the program ended. Our data indicate that the SSA initiative did not have a long-term impact on farmers' deforestation decisions or instill more sustainable agriculture practices in later years. Despite this, we found no sign of participants catching up on prevented deforestation, suggesting that the program's effects were still sustained – at least until the date of our evaluation. We calculated the three-year delayed CO_2 emissions highlighted by the impact assessment and found that the SSA program benefits were greater than its costs.

The second chapter of the thesis, "Beyond reducing deforestation: the impacts of conservation programs on the livelihoods of rural households", completes the

first chapter by mobilizing additional data to determine which decisions relating to agricultural practices have enabled participants in the SSA program to forgo cutting part of the native forest on their farmlands. In this study, we look at the direct and indirect impact mechanisms of the SSA program, a REDD+ initiative that was found to be effective in decreasing deforestation in the Transamazon area during its early years of deployment. Based on available peer-reviewed literature, project documentation, survey data collected during fieldwork and remotely sensed data, we evaluated the impact of the SSA program on environmental outcomes, agricultural practices and livelihood indicators. Using counterfactual impact evaluation methods, we assessed the impact of the project from 2013-2019, i.e., from its first year until two years after its end. Based on the Theory of Change, we focused on land use and socioeconomic outcomes likely to have been affected by changes in deforestation brought about by the initiative.

The main findings indicate that the decrease in deforestation has happened mostly as a result of a slowing in the expansion of grazing lands. When we investigated whether this decrease in pasture expansion had a negative impact on cattle herds (one of the region's main sources of income), we found that the number of cattle per hectare increased on farms benefiting from the program, indicating that cattle ranching activities were intensified. Furthermore, our results indicate that the program had a positive impact on farmers' incomes and on alternative livelihood production activities that require less area for production than extensive livestock farming and slash-and-burn agriculture, the two primary causes of deforestation in the study region. Finally, the development activities under the project that are unconditional to forest conservation do not seem to have had a significant effect in the short term, but rather in the long term. This suggests that when program participants adopt non-conditional activities that require a greater mobilization of techniques, knowledge and resources, it takes time for their effects to become observable through the data. Altogether, these results suggest that local REDD+ programs that combine PES with technical assistance and support to farmers for the adoption of low-impact activities can be effective in the fight against climate change, without jeopardizing the livelihoods of local populations. This study adds to the existing literature by providing unique evidence on the permanence of the impacts of REDD+ programs on locals' livelihoods. Furthermore, it complements recently carried out analyses aimed at evaluating the effectiveness of the SSA program (Simonet et al., 2019; Carrilho et al., 2022). By combining never-before-used satellite data for this case study, covering all program participants and thousands of non-participants as a control group, and survey data collected from a small but extremely rich and precise sample, we were able not only to corroborate or amend the findings of previous studies but also to complete the story of the project, namely the mechanisms by which the objective of reducing

deforestation was achieved.

The third chapter of the thesis, "How much should farmers be paid not to cut down the forest? Experimental auctions to set payments for forest conservation in the Brazilian Amazon", provides the results of an experimental auction conducted in the state of Acre among a sample of 302 landowners, with the aim of eliciting the individual costs of bringing farmers into compliance with two PES schemes that differ in their requirements (one being more flexible than the other). We implemented an experimental auction using the Becker-DeGroot-Marschak (BDM) mechanism as an incentive-compatible procedure for eliciting landowners' willingness-to-accept (WTA) for two PES contracts (designated type A and type B) where landowners were offered compensation for forest conservation for a one-year period. Both types of contracts have as their common goal the preservation of the forest located on the farmhold. However, type A contracts include a zero deforestation target, which means that the farmer chooses to receive a payment for the full conservation of her forest cover. If any deforestation is observed, she receives zero compensation. In type B contracts, the farmer has a penalty coefficient applied to the payments when deforestation is observed. The full payment is received if no deforestation is observed. We also ran a survey from the same sample in order to collect data on participants' land use, agricultural activities and economic constraints and to highlight the main determinants of elicited WTA levels.

Our results of the elicited forest conservation WTA for each type of contract are consistent with theoretical expectations, with the WTA being higher on average for the more restrictive contract. According to our WTA estimates, the median landholder would require between 900 USD (contract A) and 700 USD (contract B) to preserve her forest cover for a one-year period. When we compared the estimates obtained with our BDM experiment of farmers' WTA and the benefits for avoiding CO_2 emissions, we found that private costs were lower than the social costs of carbon (SCC). Moreover, by combining revealed-preference WTA data with household survey variables, we found that a number of household characteristics, farm characteristics and socioeconomic variables are likely to drive the way participants bid their minimum WTA for forest conservation. Moreover, we found that two fairly precise farmer typologies emerged from the experiment: on the one hand, poor households practicing traditional (slash-and-burn) agriculture based on fallow land and subsistence farming, and on the other hand, wealthier households whose agricultural activities rely on highly deforestation-dependent land uses (e.g., extensive cattle-ranching). Although spread over quite a small area, the private costs of forest conversation of the two groups differ significantly. They have different WTAs, on average, regardless of the contract offered, with crop farmers who are less dependent on deforestation systematically exhibiting

lower WTA than cattle ranchers. In addition, and this is again consistent with the typology, the two groups value the contract types quite differently; the flexibility gap between the two contracts translates into a 50 percent higher bid by cattle ranchers than by crop farmers. This study makes three contributions to the literature. First, this paper adds to a small literature measuring the private costs of forest conservation through directly offering households customized PES contracts. Second, we investigate the impact of contract flexibility on farmers' demand for PES schemes. Third, we address the issue of hidden information of landowners by gathering information on observable landowner attributes likely to be correlated with opportunity costs. This could be a key input for the design of PES forest conservation contracts in the Amazon.

During the completion of this thesis, I also had the opportunity to collaborate on other projects that are related to the topic of this PhD. The manuscript entitled "Permanence of avoided deforestation in a Transamazon REDD+ initiative", is directly related to the analysis presented in Chapters 1 and 2 as it examines the effectiveness of the SSA program using panel survey data produced by CIFOR as part of its Global Comparative Study. The results of this article, whose main author is Cauê Carrilho, are in line with the results of my PhD thesis based on remotely sensed data. I was glad to participate to the development of the analysis framework, data analysis, and writing of this article, which summarizes the success story of the SSA program. The article entitled "Enforcement and inequality in collective PES to reduce tropical deforestation: Effectiveness, efficiency and equity implications", whose main author is Julia Naime, examines how different enforcement strategies can improve collective PES performance. I had the chance to supervise the data collection that took place in the state of Pará and to contribute to the writing of this publication, which brings new insights for the development of more effective collective conditional payment programs that are better adapted to the land tenure and socio-economic context of targeted populations. Finally, the article entitled "Farmers' preferences for water-saving strategies in Brazilian eucalypt plantations", is an article produced from my Master's thesis and finalized during my PhD thesis. It is not directly linked to the issue of deforestation in the Brazilian Amazon, but it is directly related to that of adaptation to climate change. It presents the results of a choice experiment that I carried out in the state of Minas Gerais, with eucalypt tree farmers faced with different strategies for adapting to climate change. From a methodological point of view, this study was very useful for my thesis work since it is based on original data collection and an experimental protocol dedicated to individual smallholders located in relatively isolated regions. The three publications can be found in the Appendix.

Chapter 1

Using publicly available remote sensing products to evaluate REDD+ projects in Brazil

This work is joint with Thibault Catry, Julie Subervie and Isabelle Tritsch

Abstract

Ensuring the perpetuity and improvement of REDD+ initiatives requires rigorous impact evaluation of their effectiveness in curbing deforestation. Today, a number of global and regional remote sensing (RS) products that detect changes in forest cover are publicly available. In this study, we assess the suitability of using these datasets to evaluate the impact of local REDD+ projects targeting smallholders in the Brazilian Amazon. Firstly, we reconstruct the forest loss of 21,492 farms located in the Transamazonian region for the period 2008 to 2018, using data from two RS products: Global Forest Change (GFC) and the Amazon Deforestation Monitoring Project (PRODES). Secondly, we evaluate the consistency between these two data sources and find that the deforestation estimates at the farm level vary considerably between datasets. Despite this difference, using micro-econometric techniques that use pre-treatment outcomes to construct counter-factual patterns of REDD+ program participants, we find that an average of about two (2) hectares of forest were saved on each of the 350 participating farms during the first years of the program, regardless of the data-source used. Moreover, we find that deforestation decreased on plots surrounding participating farms, suggesting that the program had a positive spillover effect on neighboring farms. Finally, we show that participants returned to the business-as-usual pattern at the end of the program; however, the environmental gain generated during the four years of the program was not offset by any "catch-up" behavior. By calculating the monetary gain of the delayed carbon dioxide emissions, we find that the program's benefits were ultimately greater than its costs.

Résumé

Pour améliorer et assurer la pérennité des initiatives REDD+, il est nécessaire d'évaluer de façon rigoureuse leur efficacité à diminuer la déforestation. Aujourd'hui, un certain nombre de bases de données mondiales et régionales issues de télédétection, permettant de détecter des changements fins dans la couverture forestière, sont disponibles publiquement. Dans cette étude, nous évaluons la pertinence de l'utilisation de ces bases de données pour évaluer l'impact des projets REDD+ locaux ciblant les petits agriculteurs en Amazonie brésilienne. Tout d'abord, nous reconstruisons la perte de forêt sur 21,492 exploitations agricoles situées dans la région de la Transamazonienne sur la période 2008 à 2018, en utilisant les données issues de de deux bases différentes : Global Forest Change (GFC) et le Amazon Deforestation Monitoring Project (PRODES). Dans un deuxième temps, nous évaluons la cohérence entre ces deux sources de données et constatons que les estimations de la déforestation à l'échelle de l'exploitation agricole varient considérablement d'une base de données à l'autre. Malgré cette différence, en appliquant à ces données des méthodes d'analyse permettant de reconstituer le scenario contrefactuel de déforestation des participants au programme, nous constatons qu'environ deux hectares de forêt ont été sauvés en moyenne sur chacune des 350 exploitations participantes au cours des premières années du programme, quelle que soit la source de données utilisée. De plus, nous mettons en évidence que la déforestation a diminué sur les parcelles entourant les fermes participantes, ce qui suggère que le programme a eu un effet de débordement positif sur les fermes voisines. Enfin, nous montrons que les participants sont retournés à leur taux de déforestation habituel (celui qu'ils auraient eu en l'absence de paiements) à la fin du programme. Cependant, le gain environnemental généré pendant les quatre années du programme n'a pas été compensé par un quelconque comportement de "rattrapage". En estimant la valeur monétaire des émissions de dioxyde de carbone retardées grâce aux PSE, nous constatons que les bénéfices du programme ont finalement été supérieurs à ses coûts.

1.1 Introduction

Forest cover change is a leading cause of Brazil's greenhouse gas emissions[1] (INPE, 2019). As a result, there has been a proliferation of sub-national initiatives financed by the REDD+ (Reducing Emissions from Deforestation and Forest Degradation) mechanism in the Brazilian Amazon in recent years (Sills et al., 2014). Brazil currently hosts about 50 REDD+ projects targeting smallholder farmers and financed by REDD+ funds or carbon markets (Simonet et al., 2014). The perpetuity and improvement of REDD+ projects require rigorous impact evaluation (Ollivier, 2012). Yet robust evidence on their effectiveness in reducing deforestation remains scarce (Jayachandran et al., 2017; Simonet et al., 2018; Roopsind, Sohngen, and Brandt, 2019). One of the reasons for this lack of evidence is the high cost of accessing the data needed for impact analysis (Ferraro et al., 2012; Blackman, 2013; Pagiola, Honey-Rosés, and Freire-González, 2016).

Over the past 20 years, however, remotely sensed data for detecting changes in land cover worldwide has evolved dramatically, which offers new opportunities for the evaluation of forest conservation programs. In particular, a number of derived and modelled remote sensing (RS) products that extract information from raw earth observation data have become publicly available (Kugler et al., 2019). Though these RS products present a great opportunity to measure large scale changes in forest cover, the suitability of these readily available datasets to perform proper impact evaluations of sub-national REDD+ initiatives has rarely been questioned (Bos et al., 2019; Neeff et al., 2020; Correa et al., 2020).

For the present study, we focus on two well-known datasets: the Global Forest Change (GFC) dataset, provided by the University of Maryland, and the PRODES dataset, provided by Brazil's National Institute for Space Research (INPE). GFC provides free of charge global historical records of annual tree-cover loss and has already been used to assess the effectiveness of forest conservation policies in Ecuador (Jones and Lewis, 2015; Jones et al., 2017) and French Guyana (Roopsind, Sohngen, and Brandt, 2019). In the Brazilian context, however, the most often used deforestation dataset is PRODES, an accessible and transparent RS product, also free of charge, which has been employed to study the effectiveness of protected areas on avoiding deforestation in the Amazon (Nolte et al., 2013; Herrera, Pfaff, and Robalino, 2019)

[1] Although the annual deforestation rate in Brazil fell by 70 percent between 2005 and 2013 (Nepstad et al., 2014), more than 9,700 square kilometers of the Brazilian Amazon were cleared between 2018 and 2019, representing an increase of 30 percent in the annual deforestation rate and the highest deforestation rate since 2008 (INPE, 2019). The reasons for this recent development have been widely documented in the literature (Harding, Herzberg, and Kuralbayeva, 2021; Moffette, Skidmore, and Gibbs, 2021; Mullan, Caviglia-Harris, and Sills, 2021; dos Reis et al., 2021; Pailler, 2018; BenYishay et al., 2017; Bowman, 2016; de Sá, Palmer, and di Falco, 2013).

In this study, we assess the applicability of these two RS datasets to evaluate, not the impact of protected areas, but that of REDD+ local projects targeting smallholders in the Brazilian Amazon. To do so, we concentrate on Brazil's REDD+ flagship project for curbing deforestation, the Sustainable Settlements in the Amazon (SSA) program, which offered technical assistance and conditional payments to 350 households for maintaining forest cover on at least half of their land between 2012 and 2016.

This project was previously evaluated using survey data collected at the early stages of the program (Simonet et al., 2019). The authors find that in 2014 an average of four (4) hectares of forest were saved on each participating farm and that this conservation came at the expense of pastures rather than croplands, which amounts to about a 50 percent decrease in the deforestation rate. A potentially important caveat in this study, however, is that the extent to which participants might have under-declared their actual deforestation (compared to non-participants) is unknown. Unlike survey data, RS data are not susceptible to such a problem. RS data also have at least three additional advantages over survey data. First, using RS data generally makes it possible to run an analysis from larger samples than those available from surveys, thus affording increased statistical power at a much lower access cost. In the case of the SSA project, this means that we can estimate the forest loss on the individual plots of the entire population of participants rather than on only a sample of them. Second, using RS data allows us to estimate with more precision what the forest loss on these farms would have been in the absence of any program (the so-called counter-factual situation). Indeed, to make valid inferences about participants, there must be a sufficient number of non-participants with a high potential of being selected as counterfactuals. Using RS data allows access to a larger pool of candidates for the selection procedure, which increases the probability of finding good matches[2]. Third, the use of RS data allows us to study the effects of the program several years after its end. It is very expensive to repeatedly collect information in the field over a long period and the analysis based on survey data generally does not provide evidence of the permanence of the effects of conservation programs. In contrast, RS data make it possible to study the long-term effects of conservation programs, from the early stages of implementation to the most recent time period, by highlighting any effects of attenuation, rebounds, or compensation that may arise after the program ends.

To date, a number of forest conservation and reforestation programs have been

[2]This is referred to as the common support assumption. Those non-participants who display a pattern identical to that of the participants during the period preceding the program typically have a high potential of being selected as counterfactuals in the matching procedure. The larger the pool of non-participants, the more likely that the common support hypothesis will hold (Hill and Su, 2013).

evaluated applying micro-econometric methods to RS data (see (Pattanayak, Wunder, and Ferraro, 2010; Samii et al., 2014; Alix-Garcia and Wolff, 2014; Börner et al., 2017) for reviews of this literature). Most studies have been conducted in Costa Rica (Sanchez-Azofeifa et al., 2007; Arriagada et al., 2012; Garbach, Lubell, and DeClerck, 2012; Robalino and Pfaff, 2013) and Mexico (Honey-Rosés, Baylis, and Ramirez, 2011; Scullion et al., 2011; Alix-Garcia, Shapiro, and Sims, 2012; Sims et al., 2014; Alix-Garcia, Sims, and Yañez-Pagans, 2015; Costedoat et al., 2015; Sims and Alix-Garcia, 2017)[3]. Recent REDD+ impact evaluations include a study in Uganda, using remote-sensed data developed from QuickBird satellite images (Jayachandran et al., 2017), in Ecuador, using remote-sensed data developed from Landsat TM images (Mohebalian and Aguilar, 2018) , and in Guyana, using the GFC dataset (Roopsind, Sohngen, and Brandt, 2019). Overall, the results of these studies suggest that the impact of REDD+ programs on forest loss may be significant. Indeed, Jayachandran et al. (2017) find that tree cover in Uganda declined in the treatment villages by 4.2 percent during the two-year period under study, compared to 9.1 percent in the control villages, thus indicating a 54 percent decrease in deforestation rates. Likewise, Roopsind, Sohngen, and Brandt (2019) estimate that the annual tree cover loss was 0.056 percent in Guyana compared to 0.087 percent in the counterfactual estimate, thus indicating a 36 percent decrease in annual deforestation rates as a result of the program.

Our study adds to the existing literature by providing new evidence on the effectiveness of REDD+ programs, focusing on a region characterized by the highest annual loss of forest in the world, while previous studies have focused on areas where deforestation may appear less pressing (Cisneros et al., 2022). To the best of our knowledge, despite the abundance of REDD+ initiatives in the Brazilian Amazon, there are no studies that assess the long-term impacts of such projects combining ready-to-use RS products and property-level data. Our study aims at filling this gap. Furthermore, this is the first study that assesses the effectiveness of a Brazilian local REDD+ project conservation program using two different sources of remotely-sensed deforestation data to cross validate impact assessment results.

The remainder of the paper proceeds as follows. Section 1.2 describes the different data-sets used in our empirical analysis. Section 1.3 describes the REDD+ case study. Section 1.4 presents the identification strategy for the impact assessment. Section 1.5 reports the results and robustness checks. Section 1.6 discusses the main results and Section 1.7 concludes.

[3]These programs typically offer participants conditional payments to reduce deforestation on their farmland. Overall, the results of these studies suggest that the impact of the programs on the average annual forest cover varies substantially across regions (Simonet et al., 2019).

1.2 Remote sensing datasets

1.2.1 The Amazon Deforestation Monitoring Project (PRODES)

PRODES was created in 1988 by Brazil's National Institute for Space Research (INPE), with the main objective to quantify and geolocalize deforestation in the Brazilian Legal Amazon[4] and help the Brazilian government to make informed decisions and establish environmental and development public policies for the region[5] (Câmara, Valeriano, and Soares, 2006). Annual rates are estimated from the deforestation increments identified in Landsat images[6]. PRODES uses the seasonal year, starting on August 1st, to calculate annual deforestation, so images are selected as near to this date as possible. Next, the images are masked to exclude non-forest, previous deforestation, and water, using the previous year's analysis. Finally, the identification of deforestation is done by photo-interpretation, where analysts delineate deforested polygons in the intact forest of the previous year. The patterns of clear cutting rely on three main observable elements present in the images: tone, texture, and context (see (INPE, 2019) for a more detailed description of PRODES methodology).

Like any RS product, PRODES has some technical limitations, since its minimum mapping unit is 6.25 hectares, smaller deforestation patches and forest degradation due to logging are not recorded in the dataset unless accumulated over several years. In addition, deforestation estimates only consider primary forests and do not account for secondary or regenerating forests and, since PRODES relies on optical imagery, constant cloud coverage prevents Landsat sensors from capturing land cover imagery.

1.2.2 The Global Forest Change dataset (GFC)

The most well-known global deforestation dataset currently available is the University of Maryland's Global Forest Change dataset (GFC), also called Hansen data. This dataset's objective is to produce annual globally consistent characterizations of tree cover loss (Hansen et al., 2013). GFC maps annual forest loss

[4]The Brazilian Legal Amazon occupies an area that corresponds to 59 percent of Brazil's territory.

[5]PRODES data are employed in (1) certification of agribusiness supply chains such as the Soy Moratorium; (2) national inventory reports on GHG emissions; and (3) conditional monetary donations from the Amazon Fund uses PRODES data as a reference for deforestation activity in the Legal Amazon.

[6]PRODES uses mainly Landsat series images, but when there's too much cloud coverage, analysts employ SENTINEL-2 and CBERS-4 images.

beginning in 2001. The maps produced by the GFC initiative are also based on Landsat satellite images, but the classification process is 100% automated. The classification process of cloud-free Landsat image mosaics is carried out using decision tree algorithms. As the classification is carried out pixel by pixel, the minimum area mapped by this product is 900 square meters (30 x 30 meters).

For this dataset, tree-cover is defined as all vegetation taller than 5 meters across a range of canopy densities (from 0 percent to 100 percent) for an area of approximately 0.1 hectares (equivalent to a Landsat pixel). Therefore, this layer can represent primary and secondary natural forests as well as tree plantations. In addition, this dataset requires users to choose a percentage threshold value to determine whether a pixel is considered forest or not. Forest loss is defined as the complete removal of tree cover canopy at the Landsat pixel scale (see (Hansen et al., 2013) for a complete methodological explanation).

While GFC represents major progress in the understanding and quantification of global forest change research and conservation planning, the dataset does have some limits. First, tree cover loss can be the result of human activities (e. g., plantation harvesting, selective logging, and clear-cut) as well as natural causes (e.g., disease, storm, and fire damage). Second, plantations, such as cocoa, palm oil, and eucalyptus, are included as forests (Tropek et al., 2014), although the Brazilian Forest Code does not classify them as such.

Table 1.1: Comparison between GFC and PRODES datasets

	PRODES	GFC
Data source	Mainly Landsat	Landsat
Resolution	30 meters	30 meters
Minimum patch size	6.25 hectares	0.09 hectares
Coverage	Brazilian Amazon	Global
Tree cover definition	Primary forest	Vegetation taller than 5 meters
Method	Image segmentation and analyst interpretation	Automated decision tree
Observation Period	August 1 to July 31	January 1 to December 31

1.3 Description of the REDD+ Case Study

The SSA project is a sub-national REDD+ initiative implemented by the Amazon Environmental Research Institute (IPAM in the Portuguese acronym), a Brazilian non-governmental organization involved in the design and implementation of several forest conservation programs in Brazil (Cromberg et al., 2014).

The SSA project started in 2012 and was financed by the Amazon Fund[7] until 2017. The program has offered a mix of interventions to reduce deforestation rates to smallholders living in settlements located in the Transamazon highway (Figure 3.1). According to IPAM, about 2700 families have benefited from the program through a series of interventions such as: i) Awareness-raising meetings on environmental legislation and tenure regularization that were held between 2013 and 2017, benefiting an unknown number of participants, since those were open to the local community; ii) Administrative support for registration under the Environmental Rural Registry (or CAR in the original Portuguese acronym)[8] to 1300 smallholders between 2012 and 2014; iii) Development of low deforestation activities (e.g., intensive cattle ranching, fish farming and horticulture) benefiting 650 families between 2014 to 2017; and iv) Payments for Ecosystem Services (PES) scheme to 350 smallholders.

Figure 1.1: Localization of the study zone



[7]The Amazon Fund is a REDD+ instrument designed to raise donations for non-reimbursable investments in efforts to curb deforestation as well as to promote sustainable use of resources in the Brazilian Amazon.

[8]The CAR is a mandatory and self-declaratory registry for all Brazilian rural properties. To obtain CAR, landholders must document georeferenced property boundaries, as well as within-property areas of native vegetation. The CAR is intended to reduce deforestation by facilitating monitoring and enforcement of environmental policies.

Our analysis focuses on the 350 farm-holders[9] that benefited from all the above-mentioned components. These families had participated in a previous PES federal program (Proambiente) from 2003 and 2006 (Simonet et al., 2018). The small landowners live in land reform settlements located in the municipalities of Anapu, Pacaja, and Senador Jose Porfirio. These three municipalities, located close to the Transamazon highway, figure in the ranking of the 10 ten critical municipalities for their deforestation rates[10].

The value accessed by the 350 households in the PES scheme was 1680 Brazilian reais (BRL) per year (about 626 USD[11]) from January 2014 to February 2017 (Pinto de Paulo Pedro, 2016). The payments offered to project participants were conditional on forest conservation and agricultural transition toward a fire-free production system. Thirty percent of the payment was conditional on conserving forest on at least 50 percent of the farm, another thirty percent of the payment was conditional on the maintenance of 15-meter-wide forest riparian zones and the remaining 40 percent of the payment relied on the adoption of fire-free practices. A minimum of 30 percent of forest cover was required to be eligible for payments, but only participants with at least 50 percent of forest cover received the full payments (see Simonet et al. (2018) for a detailed description of the SSA program). The payments were made every three months, according to the compliance to the established guidelines. The monitoring of the compliance was made annually by IPAM, based on analysis of PRODES data for the forest cover and on the performance of the low-deforestation activities certified by the project technical assistance team (Pinto de Paulo Pedro, 2016).

1.4 Material and Methods

1.4.1 Reconstructing forest loss on individual plots

We use property boundaries from the Environmental Rural Registry (CAR in the original Portuguese)[12]. We delineate a 80-kilometer buffer around the Transamazonian highway for the Altamira, Senador Jose Porfirio, Anapu and

[9]The map with the localization of the farm-holds enrolled in SSA program is publicly available at IPAM's website (http://www.pas-simpas.org.br/). Therefore, we used this available data to geolocalize the plots that belonged to the families who received payments conditional on forest conservation.

[10]The livelihoods of small landowners in this area depend on slash-and-burn agriculture and extensive cattle ranching, which are the two primary drivers of deforestation in the Brazilian Amazon (Smith et al., 1996; Soares-Filho et al., 2006).

[11]1,680 Reais converted to USD by applying the average conversion rate of Brazilian Real to American dollars in 2014

[12]The CAR is a mandatory and self-declaratory registry for all Brazilian rural properties. To obtain CAR, landholders must document georeferenced property boundaries, as well as

Pacaja municipalities in order to delimit the rural properties that would be included in our initial sample. Georeferenced deforestation data and registered private rural properties are overlapped to enable identification of patches cleared inside property boundaries. We use information from GFC and PRODES to determine the location of forest clearings on an annual basis. All geographical datasets are re-projected to a common spatial reference (SIRGAS 2000/UTM 22S). Our sample covers the 2008-2018 period. Since we did not have information about changes in property borders during our sample period, we assume that they were constant throughout this period. For this study we use a threshold of 75 percent of vegetation cover for GFC tree cover layer as a definition of the forest the average threshold used in studies in the Amazonian context (Baker and Spracklen, 2019; Gasparini et al., 2019).

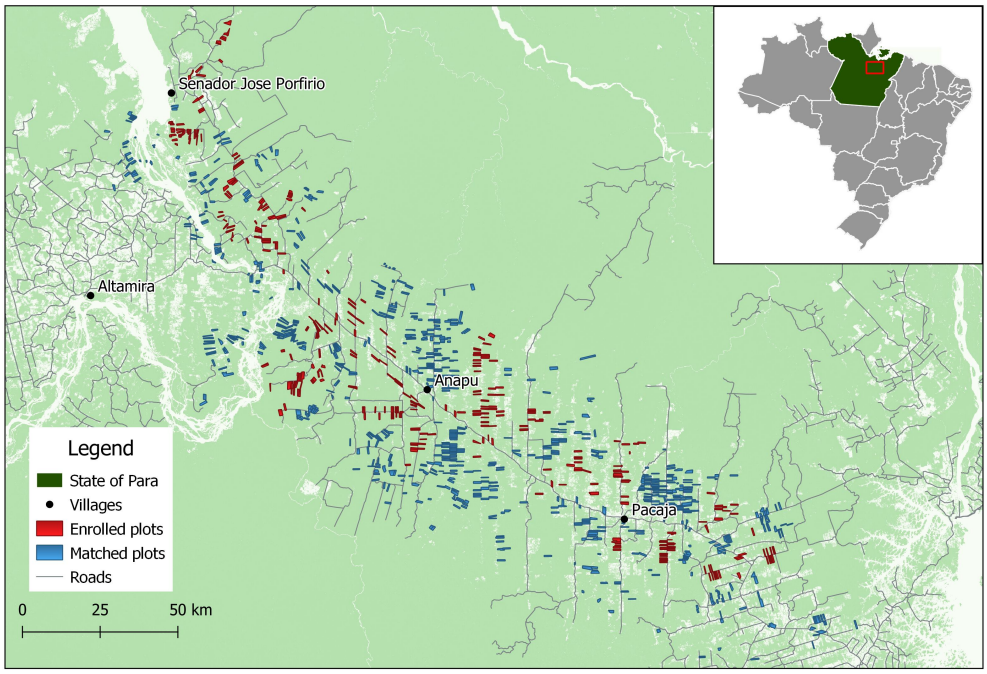
1.4.2 Identification strategy for the impact assessment

Estimating treatment effects using observational data brings the problematic of selection bias, since participants self-select into the program, the treated and untreated units may be different for many reasons other than the treatment itself. This bias occurs because some of the factors that influence the selection of participants also determine the outcomes of interest (forest loss in our case study). Observations of pre-treatment outcomes might help to correct for selection bias because they contain information on these confounding factors. Thus, matching treated and untreated groups on pre-treatment deforestation outcomes allows us to correct for selection bias (Abadie, Diamond, and Hainmueller, 2010).

In addition to pre-treatment outcomes, we select a number of covariates likely to drive both the participation in the SSA project and decisions regarding deforestation, in order to improve the matching procedure. These covariates include farm size, distance to the main road (Transamazon highway), distance from the main navigable river (the Xingu river), distance from the main market (Altamira city), and distance from the nearest small village (local market). We perform an estimation for the ATT applying the nearest neighbor matching (NNM) and the propensity score matching (PSM) estimators to our data to estimate the average effect of the SSA project on the deforestation rate of participants, using the comparison group to estimate the counterfactual level of deforestation.

within-property areas of native vegetation. The registry has a public consultation module where data from 21,492 households in our study area are available for download in vector format at http://www.car.gov.br/publico/municipios/downloads.

Figure 1.2: Localization of treated and matched plots used for estimating counterfactual levels of forest loss.

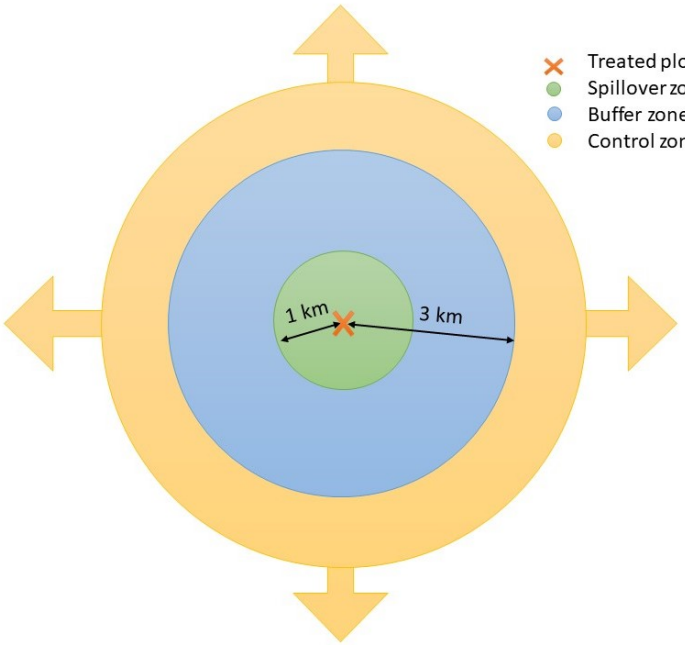


Note: We matched each treated household (red plots) to two of the most similar control households (blue plots).

1.4.3 The stable unit of treatment value assumption

All estimators used in our analyses rely on the Stable Unit of Treatment Value Assumption (SUTVA). This assumption states that there are no treatment diffusion effects, that is, that an individual's outcome is only influenced by her own treatment status. For SUTVA to be valid, the fact that one farmer participates in the program (or not) should not impact another farmer's outcome. To deal with the SUTVA, we exclude from the untreated group those plots that were less than three (3) kilometers distant from a treated plot, thus creating a buffer zone between treated farmholds and potential control ones (Figure 1.3). By doing so, we end up with 11,897 observations.

Figure 1.3: Study design to address the Stable Unit Treatment Value Assumption (SUTVA) and spillovers



Note: To deal with SUTVA, we build a buffer zone around treated units (3 km) and excluded the farmholds located in this zone from the donor pool. When testing for spillovers, we included the farmholds that would be more likely to experience spillovers due to proximity (less than 1 km from a treated unit).

1.4.4 Strategy for addressing spillovers

To test for spillover effects, we assumed that deforestation leakage would occur on farms that were not enrolled in the PES program, but were located near the beneficiary farms as they could be indirectly impacted by the program due to proximity. If we detected an increase or decrease in deforestation rates on farmholds located near those enrolled in the program during the time-frame of the SSA program (2013 to 2017) compared to the period before the program (2008 to 2012), this could be interpreted as evidence of deforestation leakage. In practice, we again create a buffer zone between the potential spillover zone and controls, by excluding from the sample of untreated plots those farm-holds located between one (1) and three (3) kilometers from an enrolled plot (see Figure 1.3).

1.4.5 Cost effectiveness of avoided CO2 emissions

Following Jayachandran et al. (2017) and Simonet et al. (2019), we calculate the value of the carbon benefit over the 2013 to 2018 period, using the social cost of carbon (SSC), and compare this value to the program's costs. The SCC is a metric that helps quantify the costs of climate change related to carbon emissions in terms of dollars per metric ton of carbon dioxide (CO_2) emitted. It can also be used to quantify the benefits of reducing CO_2 emissions. Policy recommendations for the SCC ranged from 11 to 116 USD/ton of CO_2 over the 2013 to 2018 period (Group, 2016). The benefits of the SSA project are computed using the estimates of the additionality in terms of hectares of forest saved as a result of the program.

1.5 Results

1.5.1 Statistical and spatial consistency

The purposes and methodologies of PRODES and GFC are quite divergent (Table 1.1). Firstly, GFC records forest changes in every type of vegetation greater than 5 m in height, while PRODES only captures primary forest loss. Secondly, GFC is a global dataset, while PRODES only focuses on the Brazilian Amazon. Finally, PRODES's methodology is based on contextual classification (i. e., image segmentation and analyst interpretation), while GFC's methodology is grounded on pixel-based classifications (i. e., automated decision tree). As such, there is no a priori reason for forest-loss-related information provided by the two products to coincide. To check this assumption, we run a paired t-test of annual differences in deforestation as measured in the two datasets. The results reveal significant differences between the PRODES and GFC data for the years 2008 to 2018 (Table 1.2). Furthermore, GFC detected higher rates of deforestation than PRODES in all years except 2010 and 2011.

Also, several spatial differences emerge when comparing the GFC and PRODES data. To highlight these differences, we aggregate deforestation pixels for the 2008-2018 period into binary raster layers. To measure the spatial consistency between the two RS products, we overlap both layers and perform validation samples of the areas of consistency and inconsistency (Figure 1.4). We find that the datasets have a 39 percent concordance at the municipality scale, a 36 percent concordance at the settlement scale and a 27 percent concordance at the SSA plot scale.

1.5.2 Additionality of the REDD+ initiative

The low spatial agreement between the two products suggests that one or both may not be suitable for monitoring individual-level deforestation under REDD+

Table 1.2: Paired t-tests on the equality of deforestation means (ha)

Year	GFC	Std. Dev.	PRODES	Std. Dev.	ND	t	p-value
2008	3.47	7.89	2.50	7.57	0.09	18.54	0.00
2009	2.37	5.69	1.42	5.02	0.13	21.86	0.00
2010	1.56	4.32	2.04	6.08	0.06	-12.07	0.00
2011	1.54	3.85	2.71	6.76	0.15	-25.22	0.00
2012	2.85	6.25	0.63	2.74	0.32	52.65	0.00
2013	1.97	4.59	0.77	3.23	0.21	41.80	0.00
2014	3.07	6.75	0.83	3.64	0.29	55.49	0.00
2015	1.71	5.68	1.44	5.43	0.03	7.29	0.00
2016	3.05	7.11	1.53	5.86	0.17	39.99	0.00
2017	3.97	7.70	1.33	5.14	0.29	62.64	0.00
2018	2.66	6.49	1.17	4.59	0.19	41.51	0.00

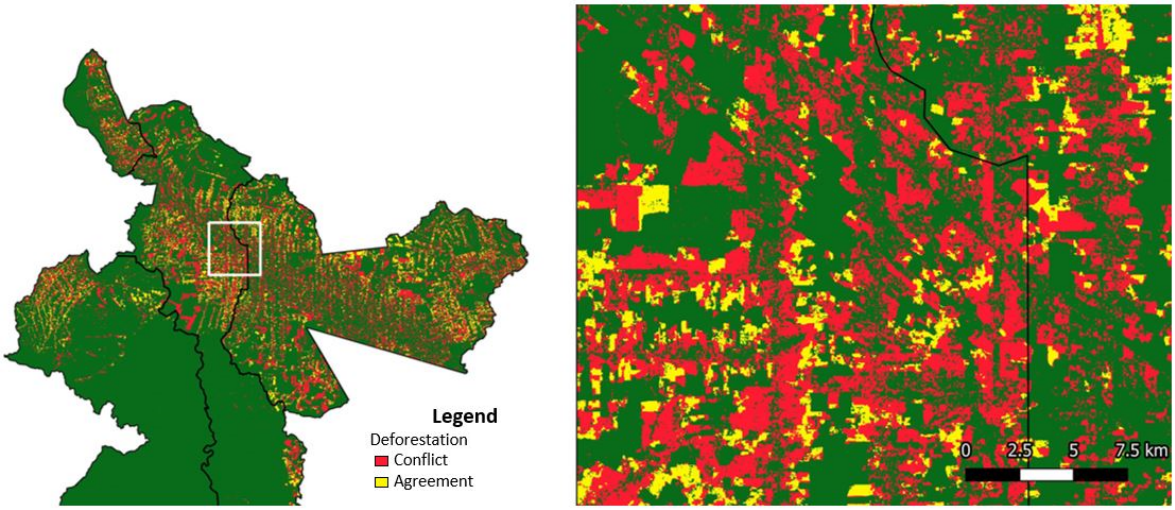
Note: Mean forest loss (in hectares) of GFC and PRODES yearly estimates for individual farm-holds (n=21,492). A paired t-test of annual differences in deforestation revealed significant difference between the PRODES and GFC estimates for the period 2008-2018. Furthermore, GFC detects systematically higher rates of deforestation than PRODES (except for 2010 and 2011).

programs. However, this does not necessarily call into question the interest of these products for evaluating the effectiveness of REDD+ programs in curbing deforestation. Indeed, it is possible that the precision level of forest loss measurement is sufficient to detect any significant differences between the participating and control plots. In other words, it is possible to detect a statistically significant difference in forest loss between participating farms and control farms, even though the level of forest loss itself is imprecisely measured on each farm.

To identify the causal effect of the SSA program, referred to as the Average Treatment effect on the Treated (ATT), we use a matching approach that uses pre-treatment outcomes to construct a valid control group from non-participating plots (Imbens and Wooldridge, 2009). This econometric method, widely used in economics, allows to distinguish the effects of enrolling some specific farms in a REDD+ program from the effects of the program itself (Millimet and Alix-Garcia, 2021).

The results of the ATT estimates are displayed in Table 1.3 and balancing tests are presented in the Appendix in Tables 1.5 and 1.6. The results show that, before matching, the participant group was significantly different from the non-participants for most covariates and that after matching, these differences dropped below 0.25 standard deviations, suggesting that the matching procedure performed well. According to GFC data, the ATT ranges between -0.4 and -0.6 hectares for

Figure 1.4: Spatial consistency between PRODES and GFC datasets



Note: The pixels in yellow (red) indicate the areas where the two databases agree (disagree) with the deforestation estimates for the 2008-2018 period

the year 2013, between -0.8 and -1.1 ha for the year 2014, and between -0.3 and -0.6 ha for the year 2015. This indicates that the SSA project prevented on average 1.9 hectares of forest from being cleared on each participating farm during the first three years of the program. Similarly, we find significant negative point estimates using PRODES data. The ATT ranges between -0.33 and -0.61 ha for the year 2014, between -0.34 and -1.13 ha for the year 2015, and between -0.43 and -0.72 ha for the year 2016, implying that the program prevented on average 1.8 hectares of forest from being cleared on each enrolled plot during these three years.

The main results for the estimations are shown graphically on Figure 1.5. Regardless of the data source used, we can visually confirm that participants and controls have nearly identical pre-treatment deforestation rates (i.e., the parallel trend assumption holds). According to the GFC dataset, the participant group deviates significantly from the trajectory of the control group from 2012 until 2015. The same phenomenon is demonstrated with the PRODES data, with a one-year lag, due to the difference in the observation period (Table 1.1). Under the hypotheses that we made when constructing the control groups, this clear break in the deforestation trend among participants can be attributed to the SSA program.

Table 1.3: Long-run impact of the SSA program on avoided deforestation

Year	estimator	ATT PRODES		SE	ATT GFC		SE
2013	NNM (4X)	-0.153		0.095	-0.553	***	0.161
	NNM (2X)	-0.174		0.113	-0.603	***	0.203
	NNM (1X)	-0.169		0.149	-0.474	**	0.224
	PSM (4N)	-0.005		0.073	-0.399	***	0.142
	PSM (2N)	0.011		0.078	-0.488	***	0.168
	PSM (1N)	0.082		0.076	-0.528	***	0.198
2014	NNM (4X)	-0.401	***	0.128	-1.044	***	0.266
	NNM (2X)	-0.402	**	0.157	-0.931	***	0.306
	NNM (1X)	-0.613	**	0.243	-0.835	**	0.335
	PSM (4N)	-0.328	***	0.113	-1.043	***	0.235
	PSM (2N)	-0.471	***	0.144	-1.046	***	0.266
	PSM (1N)	-0.575	***	0.192	-1.058	***	0.324
2015	NNM (4X)	-1.134	***	0.260	-0.635	***	0.224
	NNM (2X)	-1.061	***	0.285	-0.307	*	0.180
	NNM (1X)	-1.043	***	0.373	-0.402	*	0.240
	PSM (4N)	-0.760	***	0.214	-0.557	***	0.145
	PSM (2N)	-0.642	**	0.251	-0.504	***	0.184
	PSM (1N)	-0.338		0.267	-0.417	*	0.216
2016	NNM (4X)	-0.430	**	0.174	-0.521	*	0.287
	NNM (2X)	-0.491	**	0.208	-0.405		0.325
	NNM (1X)	-0.458	*	0.264	-0.187		0.320
	PSM (4N)	-0.659	***	0.166	-0.359		0.242
	PSM (2N)	-0.573	***	0.180	-0.176		0.252
	PSM (1N)	-0.721	***	0.231	-0.094		0.294
2017	NNM (4X)	-0.328		0.212	-0.690	**	0.324
	NNM (2X)	-0.240		0.223	-0.858	**	0.387
	NNM (1X)	-0.332		0.270	-0.605		0.441
	PSM (4N)	-0.514	***	0.188	-0.321		0.280
	PSM (2N)	-0.375	*	0.209	-0.307		0.307
	PSM (1N)	0.016		0.208	-0.227		0.346
2018	NNM (4X)	-0.033		0.152	-0.024		0.185
	NNM (2X)	-0.106		0.175	-0.142		0.203
	NNM (1X)	-0.044		0.217	-0.245		0.240
	PSM (4N)	-0.122		0.145	0.126		0.187
	PSM (2N)	-0.167		0.171	0.216		0.201
	PSM (1N)	-0.138		0.197	0.223		0.231

Note: The average treatment effect on the treated (ATT) is the mean difference in forest loss (hectares) between participants and control group. ***, **, and * denote rejection of the null hypothesis of no impact at the 1%, 5% and 10% level. NNM(4X) (resp. 2X and 1X) refers to the nearest neighbor estimator using 4 (resp. 2 and 1) matched observations as controls. PSM(4N) (resp. 2N and 1N) refers to the propensity score matching estimator using 4 (resp. 2 and 1) matched observations as controls.

1.5.3 Permanence of the effects of the REDD+ initiative

We fail to detect a significant impact of the program for the years 2016, 2017 and 2018, using GFC data, and for the years of 2017 and 2018, using PRODES data (Table 1.3). The fact that the ATT becomes non-significant as the program ends indicates that the treated group, whose trajectory had diverged from that of the control group, goes back to their previous behavior after three years (Figure 1.5).

These results suggest that the gains achieved by the program until 2018 represent a three-year delay in the deforestation that would have occurred in the absence of the program. This means that the program participants consented to modifying their behavior for the duration of the program, only to return to their business-as-usual behavior after the end of the program, suggesting that the intervention was not sufficient to trigger long-lasting changes in farmers' behavior.

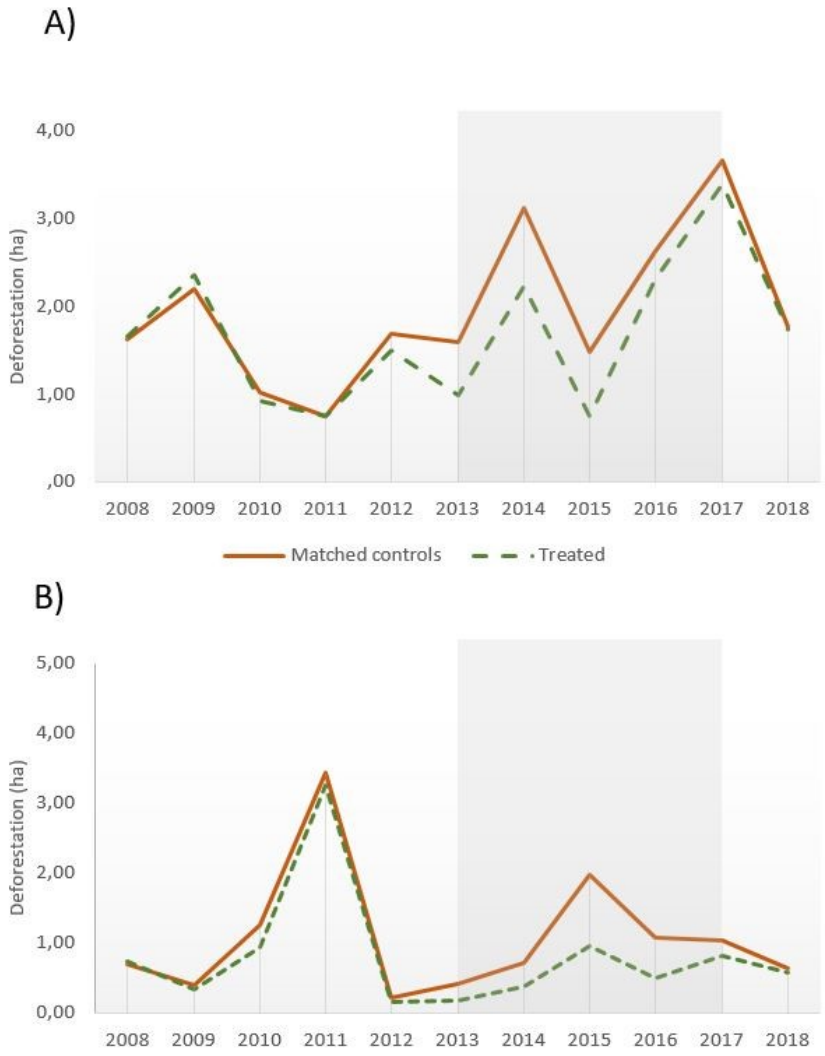
Nevertheless, it is important to mention that we do not detect a higher rate of deforestation by the participating group than by the control group after the end of the program, meaning that participants did not "catch up" on their postponed deforestation. Thus, the environmental gain generated during the first three years of the program is not subsequently lost, but lasts at least until 2018 (when our analysis ends).

1.5.4 Spillover effects of the REDD+ initiative

In any conservation program targeting individuals, the possible presence of spillover effects, i.e., an impact of the program on individuals who were not initially targeted by the program but were exposed to it, must be considered. In the case of the SSA program, these individuals are farmers who did not sign any PES contracts but who were able to benefit from some of the program's non-financial components, such as information campaigns or free registration in the rural registry. Moreover, the development of low deforestation activities among participants in the PES program may have benefited surrounding families on the adoption of new practices, access to inputs, and increasing labor demand.

To test for the presence of such spillovers, we focus on any deforestation that may have occurred in the plots surrounding participating farms. We assess the impact of the program on those 2,125 farm-holds located less than one (1) kilometer from an enrolled farm (Figure 1.3). We apply the same identification strategy to these potential program beneficiaries as that applied to those with treated plots (Table 1.4). We find that tree cover loss on plots surrounding participating farms was lower than forest loss on control plots, suggesting that the program indeed had a positive spillover effect of avoiding, on average, between 0.25 and 0.96 hectare of deforestation on each surrounding plot during the first year of the program. This effect subsequently disappears.

Figure 1.5: Deforestation on enrolled and non-enrolled farms for the 2008–2018 period



Note: (A) Nearest neighbor matching (NNM) estimates using PRODES dataset. (B) NNM estimates using GFC dataset. The SSA REDD+ program was implemented from 2013 to 2017 (grey panel). The difference between treated and controls is significant for the early years of program implementation.

1.5.5 Cost effectiveness of the REDD+ initiative

We scale up yearly point estimates to the 350 farmholds enrolled in the project, assessing that a total of 647 hectares of forest were saved as a result of the project.

Next, we compute the average stock of carbon in our study area, using the estimates of biomass provided by the World Resources Institute, i.e., 116 tons of carbon (C) above-ground per hectare of forest with at least 50 percent tree cover (Simonet et al., 2019). We then calculate the impact of the forestland conserved in tons of CO_2. Since one CO_2 molecule weighs 3.67 times as much as a carbon atom, this means that 426 metric tons of CO_2 are stored in one hectare of land covered by the PES contracts. Therefore, we find that the program avoided between 275,628 (PRODES) and 293,659 (GFC) tons of CO_2 emissions.

The costs of the SSA project's PES component were computed using the amount disbursed to participants from 2014 to 2017 (626 USD per participant), which came to 838,849 USD of discounted costs (using a 3 percent discount rate). By relating this expenditure to the emissions avoided, we find that the cost of the project was between 2.86 (GFC) and 3.04 (PRODES) USD per ton of CO_2 emissions avoided. By then computing the value of the carbon benefit using the SSC, we find that the discounted benefit of the avoided emissions was between 1,111,795 (PRODES) and 1,503,154 (GFC) USD, which gives us a benefit/cost ratio that ranges between 1.33 (PRODES) and 1.79 (GFC) USD, meaning that, on average, one dollar invested in the program translates into a dollar and a half of environmental gain.

Table 1.4: Spillover effect of the SSA program on avoided deforestation on neighboring farms

Year	estimator	ATT PRODES		SE	ATT GFC		SE
2013	NNM (4X)	-0.094	*	0.051	-0.312	***	0.093
	NNM (2X)	-0.113	**	0.057	-0.262	**	0.103
	NNM (1X)	-0.150	**	0.067	-0.293	**	0.119
	PSM (4N)	-0.019		0.045	-0.184	**	0.079
	PSM (2N)	0.001		0.044	-0.207	**	0.090
	PSM (1N)	0.028		0.047	-0.145		0.110
2014	NNM (4X)	-0.452	***	0.095	-0.369	*	0.205
	NNM (2X)	-0.466	***	0.104	-0.361		0.232
	NNM (1X)	-0.486	***	0.122	-0.314		0.263
	PSM (4N)	-0.405	***	0.089	-0.038		0.182
	PSM (2N)	-0.412	***	0.105	-0.027		0.195
	PSM (1N)	-0.410	***	0.121	-0.028		0.226
2015	NNM (4X)	-0.673	***	0.193	-0.253		0.181
	NNM (2X)	-0.653	***	0.206	-0.167		0.205
	NNM (1X)	-0.732	***	0.238	-0.234		0.233
	PSM (4N)	-0.300	**	0.121	-0.157		0.113
	PSM (2N)	-0.358	**	0.152	-0.153		0.127
	PSM (1N)	-0.414	**	0.184	-0.133		0.147
2016	NNM (4X)	-0.149		0.120	-0.145		0.206
	NNM (2X)	-0.139		0.141	-0.162		0.225
	NNM (1X)	-0.167		0.170	-0.078		0.241
	PSM (4N)	-0.102		0.106	-0.001		0.176
	PSM (2N)	-0.143		0.123	-0.129		0.202
	PSM (1N)	-0.113		0.069	0.063		0.220
2017	NNM (4X)	-0.235		0.176	-0.357		0.220
	NNM (2X)	-0.240		0.184	-0.576	**	0.253
	NNM (1X)	-0.306		0.199	-0.659	**	0.296
	PSM (4N)	-0.077		0.115	-0.242		0.239
	PSM (2N)	-0.115		0.121	-0.276		0.225
	PSM (1N)	-0.089		0.139	-0.129		0.254
2018	NNM (4X)	0.060		0.080	-0.235	*	0.142
	NNM (2X)	0.046		0.093	-0.334	*	0.174
	NNM (1X)	0.025		0.109	-0.435	**	0.203
	PSM (4N)	-0.057		0.064	-0.039		0.134
	PSM (2N)	-0.084		0.086	-0.158		0.146
	PSM (1N)	-0.086		0.100	-0.142		0.159

Note: The average treatment effect on the treated (ATT) is the mean difference in forest loss (hectares) between neighboring farms and control group. ***, **, and * denote rejection of the null hypothesis of no impact at the 1%, 5% and 10% level. ATT is the Average Treatment effect on the Treated. NNM(4X) (resp. 2X and 1X) refers to the nearest neighbor estimator using 4 (resp. 2 and 1) matched observations as controls. PSM(4N) (resp. 2N and 1N) refers to the propensity score matching estimator using 4 (resp. 2 and 1) matched observations as controls.

1.6 Discussion

1.6.1 Remote-Sensing products

The divergence that we find between the two RS datasets can be partially explained by methodological differences in the construction of the products. First, small levels of deforestation are not incorporated within PRODES estimates (because deforestation activities are only reported if they accumulate beyond the 6.25-hectare threshold), while these can be detected more easily with GFC. Second, GFC estimates detect secondary forest clearings while PRODES does not. Third, some of the cover loss reported in the GFC dataset may be due to forest degradation (e.g., forest fires and selective logging), something that is less likely to be captured with PRODES estimates. All this may explain why GFC often detects higher rates of deforestation than PRODES.

This result calls for at least two comments. The first is that neither of the two products seems to completely outperform the other. PRODES estimates are validated in the field but are not able to account for small deforestation operations. GFC, on the other hand, can detect small patches of deforestation, but sometimes does so when an area is not, in fact, strictly deforestation. This should encourage REDD+ project evaluators to cross-validate their results using multiple datasets when available.

The second comment relates to the suitability of PRODES and GFC products for the evaluation of REDD+ local projects. Incontestably, any RS deforestation dataset may contain classification errors and biased deforestation area estimates. Does this mean that these products are of no use in properly estimating the impact of PES programs targeting smallholders? Although these products do not seem suitable for the fine monitoring of this type of program, our analysis nevertheless suggests that they make it possible to assess the effectiveness of a PES program. This can be done provided that (i) the impact of the program is large enough to be detected despite the lack of precision of the RS products, and (ii) the noise caused by the imprecision of the RS product estimates is distributed between the treated and control plots so that it is netted out through the comparison between the two groups. There is no reason, a priori, for the second condition not to be fulfilled, since inaccuracies can appear on a participating farm as well as on a farm in the control group.

1.6.2 Local REDD+ effectiveness

We assess the impact of an SSA project over 2013-2018 and estimate that on each participating farm an average of about 2 hectares of forest were saved during the early years of the project, regardless of the source of deforestation data used.

Even though the size of the estimated effect is smaller than that estimated using survey data by Simonet et al. (2019), it is quite similar in magnitude to those found for other PES-based forest conservation programs run in Latin America (Robalino and Pfaff, 2013; Alix-Garcia, Shapiro, and Sims, 2012).

We fail to detect a positive impact of the program during its last year (the difference between participants and the counter-factual group vanishes even before the end of the program), suggesting that the program's effects were primarily realized in its initial years. Evidence thus suggests that the SSA project may have failed to prompt a transition to more sustainable agricultural practices in the years following. Similarly, Giudice et al. (2019) and Fiorini et al. (2020) find that Peru's Natural Forest Conservation Program and Water and Forest Producers program succeeded in increasing forest cover only in the program's first years. One possible explanation for this is that the opportunity costs of complying with the program requirements increased during the period when the program was in place, leading farmers to return to their business-as-usual practices even before the end of the program. As we can see in Figure 1.6, the price of cattle sharply increased from 97 BRL per arroba[13] in January 2013 to 144 BRL in January 2015. This abrupt increase in cattle prices may have played a determining role on the decision to deforest, by increasing the relative profitability of expanding pastures compared to that of complying with the program requirements (Caviglia-Harris, 2018).

Finally, it should be stressed that the SSA program had several components, other than the PES, which may have played a role in the effects we estimated. We are unable to disentangle the effects of each of these components on the detected impact on forest cover among participant plots. However, our results, which show that deforestation decreased on farm-holds surrounding treated plots between, suggest that non-monetary components of the program may also have been at play, at least in the first years of the program.

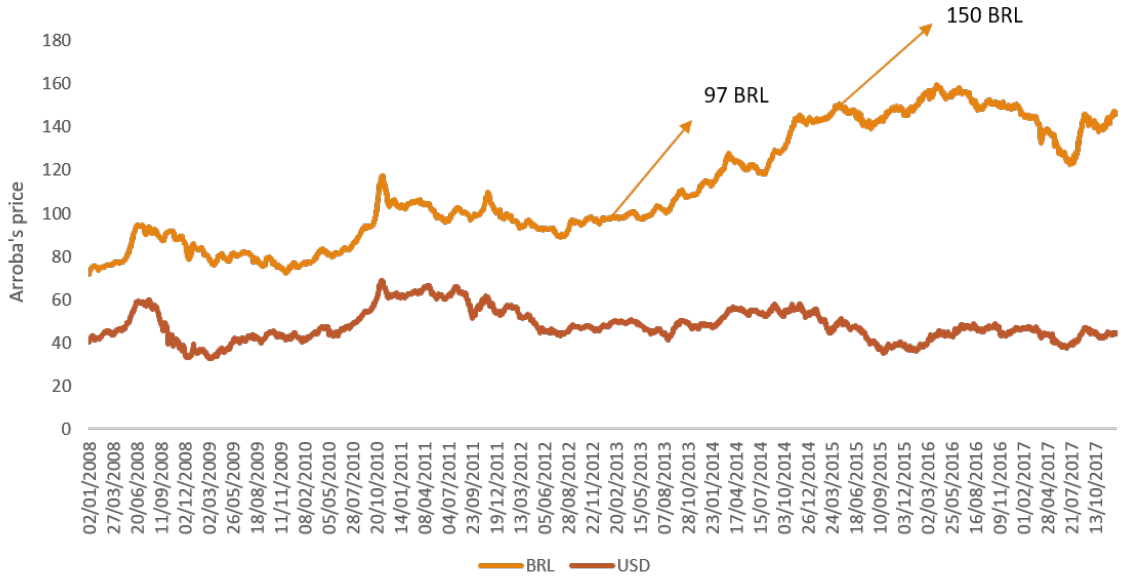
1.7 Conclusion

To summarize, the four key messages one can take away from this analysis relate to (i) the suitability of RS products for evaluating conservation program effectiveness, (ii) the likely short- and long-term effectiveness of PES programs, (iii) the likely spillover effects of such programs and ultimately (iv) their cost-effectiveness.

Overall, our findings suggest that, despite the disagreement between GFC and PRODES on forest cover loss estimates at the individual plot-level, such datasets represent a valuable source of data to evaluate forest conservation projects.

[13]The metric arroba is an unit of measurement used in Brazil and is defined as 15 kilograms (33 lb)

Figure 1.6: Evolution of cattle price in Brazil



Source: Centro de Estudos Avancados em Economia Aplicada (CEPEA)

We find evidence that the local REDD+ initiative was effective in reducing deforestation during its early years of implementation in the Transamazon region, an area with historically high deforestation rates. This suggests that PES programs targeting smallholders in the Brazilian Amazon may well be effective, at least in the short-run. Moreover, we find evidence that non-enrolled farm-holds located close to enrolled ones were somehow impacted by the program, as they also decreased deforestation during the early years of program implementation. This suggests that REDD+ projects may change the behavior of farmers who are not the primary beneficiaries of the program - although we are not able to determine through which channel.

In addition, we find that the participants resumed their normal rate of deforestation even before the end of the program. Our findings suggest that the SSA project failed to generate a permanent effect on farmers' decisions about deforestation or to induce more sustainable agricultural practices in years subsequent to the program. Despite this, we value the three-year delayed CO_2 emissions highlighted by the impact assessment and find that the SSA program benefits were greater than its costs.

1.8 Appendix to Chapter 1

Balancing tests

The goal of matching is to make the covariate distributions of participants and non-participants similar. After we compare the extent of balancing between the participant and comparison groups before and after the matching procedure. We calculate the normalized difference between these two groups for the pre-treatment covariates. The normalized difference is the most commonly accepted diagnostic used to assess covariate balance (Rosenbaum and Rubin, 1985).

The normalized difference is considered negligible when it is below of 0.25 standard deviations. Column 6 of Tables 1.5 and 1.6 shows that, before matching, the participant group (column 3) differs significantly from the comparison group (column 2) in terms of distance from nearest village, distance from Altamira, distance from Transamazon highway and distance from Xingu river. Column 7 of Tables 1.5 and 1.6 reports the normalized mean differences between participants and the constructed matched group. All normalized differences are below 0.25 standard deviations, which indicates that the matching procedure was successful in constructing a valid control group.

Table 1.5: Balancing test using PRODES data

	Means		Variances		Stand. differences	
Covariates	Control	Treated	Control	Treated	Raw	Matched
Total area	97.38	77.21	6053.72	1369.13	-0.33	0.07
Distance from nearest village	29329.13	23091.53	2.17E+08	1.26E+08	-0.48	-0.02
Distance from Altamira	164568.60	122600.50	9.64E+09	3.22E+09	-0.52	-0.01
Distance from Transamazon	20130.35	13875.05	1.92E+08	1.17E+08	-0.50	-0.04
Distance from Xingu river	111879.40	63760.82	7.07E+09	2.79E+09	-0.69	-0.01
Forest loss PRODES 2008	1.94	0.55	49.73	5.27	-0.27	0.03
Forest loss PRODES 2009	0.97	0.27	17.30	2.18	-0.23	0.01
Forest loss PRODES 2010	1.97	0.93	36.91	9.92	-0.22	0.02
Forest loss PRODES 2011	2.37	3.36	36.57	43.59	0.16	0.05
Forest loss PRODES 2012	0.40	0.17	4.21	2.26	-0.13	0.01

Table 1.6: Balancing test using GFC data

	Means		Variances		Stand. differences	
Covariates	Control	Treated	Control	Treated	Raw	Matched
Total area	97.38	77.21	6053.72	1369.13	-0.33	0.10
Distance from nearest village	29329.13	23091.53	2.17E+08	1.26E+08	-0.48	-0.04
Distance from Altamira	164568.60	122600.50	9.64E+09	3.22E+09	-0.52	-0.04
Distance from Transamazon	20130.35	13875.05	1.92E+08	1.17E+08	-0.50	-0.04
Distance from Xingu river	111879.40	63760.82	7.07E+09	2.79E+09	-0.69	-0.04
Forest loss GFC 2008	3.26	2.13	58.49	20.72	-0.27	0.11
Forest loss GFC 2009	1.89	2.75	24.54	23.29	0.03	0.11
Forest loss GFC 2010	1.54	0.94	16.62	6.87	-0.15	0.07
Forest loss GFC 2011	1.54	0.86	14.19	4.70	-0.24	0.06
Forest loss GFC 2012	2.39	1.78	32.33	12.88	-0.20	0.05

Chapter 2

Beyond reducing deforestation: Impacts of conservation programs on household livelihoods

This work is joint with Stibniati Atmadja, Cauê Carrilho, Thibault Catry and Julie Subervie

Abstract

Understanding why forest conservation initiatives succeed or fail is essential to designing cost-effective programs at scale. In this study, we investigate direct and indirect impact mechanisms of a REDD+ project that was shown to be effective in reducing deforestation during the early years of its implementation in the Transamazon region, an area with historically high deforestation rates. Using counterfactual impact evaluation methods applied to survey and remote-sensing data, we assess the impact of the project over 2013-2019, i.e., from its first year until two years after its end. Based on the Theory of Change, we focus on land use and socioeconomic outcomes likely to have been affected by changes in deforestation brought about by the initiative. Our findings highlight that forest conservation came at the expense of pastures rather than cropland and that the project induced statistically greater agrobiodiversity on participating farms. Moreover, we find that the project encouraged the development of alternative livelihood activities that required less area for production and generated increased income. These results suggest that conservation programs, that combine payments conditional on forest conservation with technical assistance and support to farmers for the adoption of low-impact activities, can manage to slow down deforestation in the short term are likely to induce profound changes in production systems, which can be expected to have lasting effects.

Résumé

Comprendre pourquoi les initiatives de conservation des forêts aboutissent ou échouent est essentiel pour concevoir des programmes efficaces à grande échelle. Dans cette étude, nous examinons les mécanismes d'impact directs et indirects d'un projet REDD+ qui s'est avéré efficace pour réduire la déforestation au cours des premières années de sa mise en œuvre dans la région Transamazonienne, une zone avec des taux de déforestation historiquement élevés. En appliquant des méthodes contrefactuelles à des données d'enquête et de télédétection, nous évaluons l'impact du projet sur la période 2013-2019, c'est-à-dire de sa première année jusqu'à deux ans après sa clôture. En nous appuyant sur la théorie du changement, nous nous concentrons sur l'occupation des sols et les résultats socio-économiques susceptibles d'avoir été affectés par les changements dans les trajectoires de déforestation induites par l'initiative. Nos résultats soulignent que la conservation des forêts s'est faite au détriment des pâturages plutôt que des terres cultivées et que le projet a eu un impact positif sur l'agrobiodiversité des exploitations participantes au programme. En outre, nous constatons que le projet a encouragé le développement d'activités de subsistance alternatives nécessitant moins de surface de production et générant des revenus plus élevés. Ces résultats suggèrent que les programmes de conservation, qui combinent des paiements conditionnés à la conservation des forêts avec une assistance technique et un soutien aux agriculteurs pour l'adoption d'activités à faible impact environnemental, peuvent freiner la déforestation à court terme et sont également susceptibles d'induire des changements plus profonds dans les systèmes de production, dont les effets sont potentiellement plus durables dans le temps.

2.1 Introduction

More than 15 years have passed since REDD+ entered the climate policy arena, but evidence about the effectiveness of this promising mechanism remains scarce (Duchelle et al., 2018). Indeed, we still know little about the ability of these initiatives to mitigate climate change while also safeguarding the people whose livelihoods depend upon tropical forests. Of the few rigorous impact evaluations from the REDD+ literature, some suggest that the initiatives are effective in reducing deforestation (Jayachandran et al., 2017; Simonet et al., 2018; Roopsind, Sohngen, and Brandt, 2019; Coutiño et al., 2022), while others find null or mixed impacts (Correa et al., 2020; West et al., 2020). However, most evaluations of REDD+ local initiatives focus on environmental impacts and do not measure their effects on other land use activities or socioeconomic outcomes (Jack and Santos, 2017), which does not allow conclusions to be drawn as to the reasons for the success or failure of these programs.

Even in cases where the primary environmental objective has been achieved, it is important to understand how and to consider the implications for other land uses and for the livelihoods of program participants who agreed to reduce their rate of deforestation. In some cases, it is crucial to evaluate to what extent a program that succeeds in preserving the native forest also guarantees sustainable livelihoods of local populations (Blundo-Canto et al., 2018), because even when conservation programs are voluntary and offer financial compensation to participants in exchange for forest conservation, certain unanticipated exogenous shocks may ultimately reduce the profitability of participation. Evaluating the direct and indirect effects of conservation programs can also tell us about the potential sustainability of the results of these programs once they end. There is thus a need for rigorous assessment of all the likely impacts beyond forest outcomes of local REDD+ initiatives (Sills et al., 2017). Understanding why REDD+ pilot initiatives succeed or fail is essential both for the improvement of this mechanism to fight climate change and for designing upscale cost-effective programs in the future (Wunder et al., 2020).

In this study, we carried out a comprehensive evaluation of the impacts of the Sustainable Settlements in the Amazon (SSA) program, a REDD+ initiative whose objective was to reduce deforestation in the Brazilian Amazon by improving local agricultural systems. The smallholders who entered the program benefited from a mixed approach of Payments for Environmental Services (PES) and Integrated Conservation and Development Projects (ICDP). This study builds on Chapter 1 and on previous analyses that have evaluated the effectiveness of the SSA program (Simonet et al., 2018; Carrilho et al., 2022). In particular, Chapter 1 showed that the SSA project prevented, on average, almost two hectares of forest from being cleared on each participating farm during the first three years of the

program. The results also suggest that the participants resumed their normal rate of deforestation thereafter, without catching up on their postponed deforestation, thus maintaining a significant gap with non-participants in the program, even after its end. In this study, we aim at understanding how this reduction in deforestation was achieved and how the program impacted participants. We also examine whether other project objectives, initially considered secondary, were achieved, in what time frame, and with what chance of having lasting effects over time.

Using the available literature and project documentation, survey data collected from a sample of participants and non-participants, and remotely-sensed data on the land-use of all the participants and those of their non-participating neighbors, we evaluated the impact of the SSA program on a series of environmental outcomes, agricultural practices, and livelihood indicators. As much as possible, we used the same analytical tools used in previous studies that highlight the the SSA project's environmental performance. We applied matching estimators to panel data on participating and non-participating farms, using matched non-participants to establish counterfactual participant levels.

Results suggest that the decrease in deforestation occurred mainly at the expense of the slowdown in the extensions of pasture areas. Moreover, we investigated whether the number of cattle per hectare increased on the farms benefiting from the program, and our findings suggest that there was in fact an intensification in cattle ranching activities. Furthermore, our results indicate that the program had a positive impact on farmers' gross income and on alternative livelihood production activities that require less area for production than extensive livestock farming and slash-and-burn agriculture, the two main drivers of deforestation in the study region. These findings demonstrate that REDD+ projects that combine PES with technical assistance and support for the adoption of low-impact activities can be effective in the fight against climate change, without jeopardizing the livelihood of local populations. They also suggest that the effort made to curb deforestation mechanically resulted in a lower extension of pastures (not herds) but did not prevent the participants from simultaneously developing new agricultural activities, which can be expected to have lasting effects, even if deforestation returns to a normal rate.

The remainder of the paper proceeds as follows. Section 2.2 describes the content of the SSA project and the Theory of Change that frames our empirical analysis. Section 2.3 describes the data used in the analysis. Section 2.4 presents the identification strategy. Section 2.5 reports the results and robustness checks. Section 2.6 discusses the main results and Section 2.7 summarizes the key messages of the study.

2.2 The SSA program

REDD+ was envisioned and designed to be implemented by governments at national and jurisdictional levels (Wunder et al., 2020). However, most initiatives that have been implemented and are subject to detailed evaluations today have been undertaken at the local scale by non-governmental organizations (NGOs) and the private sector (Sills et al., 2014; Simonet et al., 2014). Although PES schemes were originally envisioned as the first choice of intervention in local REDD+ programs, non-conditional incentives to adopt sustainable livelihood alternatives have been adopted more frequently by proponents of local initiatives (Duchelle et al., 2017). These local projects aim to implement a mix of interventions to reduce deforestation and promote alternative production activities that require less land to achieve a given production/income level. This is the case of the SSA program which was implemented by the *Instituto de Pesquisa Ambiental da Amazonia* (IPAM), a Brazilian NGO dedicated to environmental research which has played an important role in designing and implementing REDD+ in Brazil (Gebara et al., 2014). IPAM started operating the program in 2012 and ended it in 2017, after its refinancing request was denied by the Amazon Fund[1] (Carrilho et al., 2022).

2.2.1 Program content to support alternative agricultural production

The primary goal of SSA was to reduce deforestation rates, mainly by promoting alternative livelihood activities, which were expected to generate better profits than traditional land-use, while being associated to lower deforestation practices. On aggregate, IPAM targeted approximately 2,700 smallholders from the western part of the Pará state (Brazil) (IPAM, 2016). The main economic activities of target smallholders were slash-and-burn agriculture and extensive cattle ranching (Cromberg et al., 2014). In this study, we focused on 350 smallholders who benefited from the whole package of interventions offered by IPAM, including PES.

Alternative livelihood activities were defined by IPAM's technicians together with farmholders in customized property management plans (Simonet et al., 2018). IPAM then made a selection of the activities they wanted to promote, based on a market study that identified the agricultural products with the greatest commercialization potential in the nearest larger cities (IPAM, 2017; Souza et al., 2020). The objective was to implement activities that require less area for production but provide higher economic returns. These included new livelihood activities (e.g., fish-farming, horticulture, fruit pulp and cocoa production) as well as alternative

[1] A results-based funding program created in 2008 that allocated international REDD+ donations to Brazilian projects (Correa, van der Hoff, and Rajão, 2019).

practices for current agricultural production - in this case, the transition from extensive to more intensive cattle ranching, and from slash-and-burn to mechanized agriculture (IPAM, 2017; Stella et al., 2020; Carrilho et al., 2022).

To promote these new/alternative activities, IPAM offered technical assistance and free agricultural inputs in addition to PES (Carrilho et al., 2022). The PES component was designed to provide participants additional income (up to 1,600 BRL per year) until the new livelihood activities took off. The payments were conditional on preserving forest on at least 50 percent of the farm[2], the preservation of riparian forests along water courses, and the adoption of a fire-free production system (IPAM, 2016; Simonet et al., 2018). In addition, IPAM provided support with transportation and market infrastructure to farmers selling vegetables in cities (Carrilho, 2021).

Participants also benefited from information meetings designed to raise awareness about the Brazilian Forest Code, which requires farmholds to retain a legal reserve of forest[3] and permanent preservation areas (PPA) along streams and rivers and around water springs. Participants also benefited from administrative support to register their proprieties under the Rural Environmental Registry (*Cadastro Ambiental Rural* or CAR) (Simonet et al., 2018). It is worth mentioning that CAR is a mandatory digital registration for all Brazilian rural properties in which landholders must document their property's boundaries, including the location of all native vegetation that must be protected according to the Brazilian Forest Code.

2.2.2 Expected and unexpected outcomes (Theory of Change)

To understand how the interventions proposed by IPAM could achieve both expected and unexpected outcomes on forests and livelihoods, we built a Theory of Change (ToC) of the SSA program, as represented in Figure 2.1. The diagram is divided into three parts: program inputs, program outcomes (which can also be seen as intermediary impacts), and (final) program impacts. The color code corresponds to the sign of the expected effect, when it is possible to envisage it, at least theoretically. Since we are interested in the 350 participants who received all the interventions offered by IPAM, and decisions about deforestation and land use are simultaneous, it is difficult to establish a priori a causal link between a given intervention and a given outcome. However, in order to organize the description of the ToC in a simple way, we first describe the interventions that were designed by IPAM, mainly with the objective of curbing deforestation in the short term. We then present the interventions, whose main objective was to develop sustainable

[2]A minimum of 30 percent of forest cover was required to be eligible for payments, but only participants with at least 50 percent of forest cover received the full payments

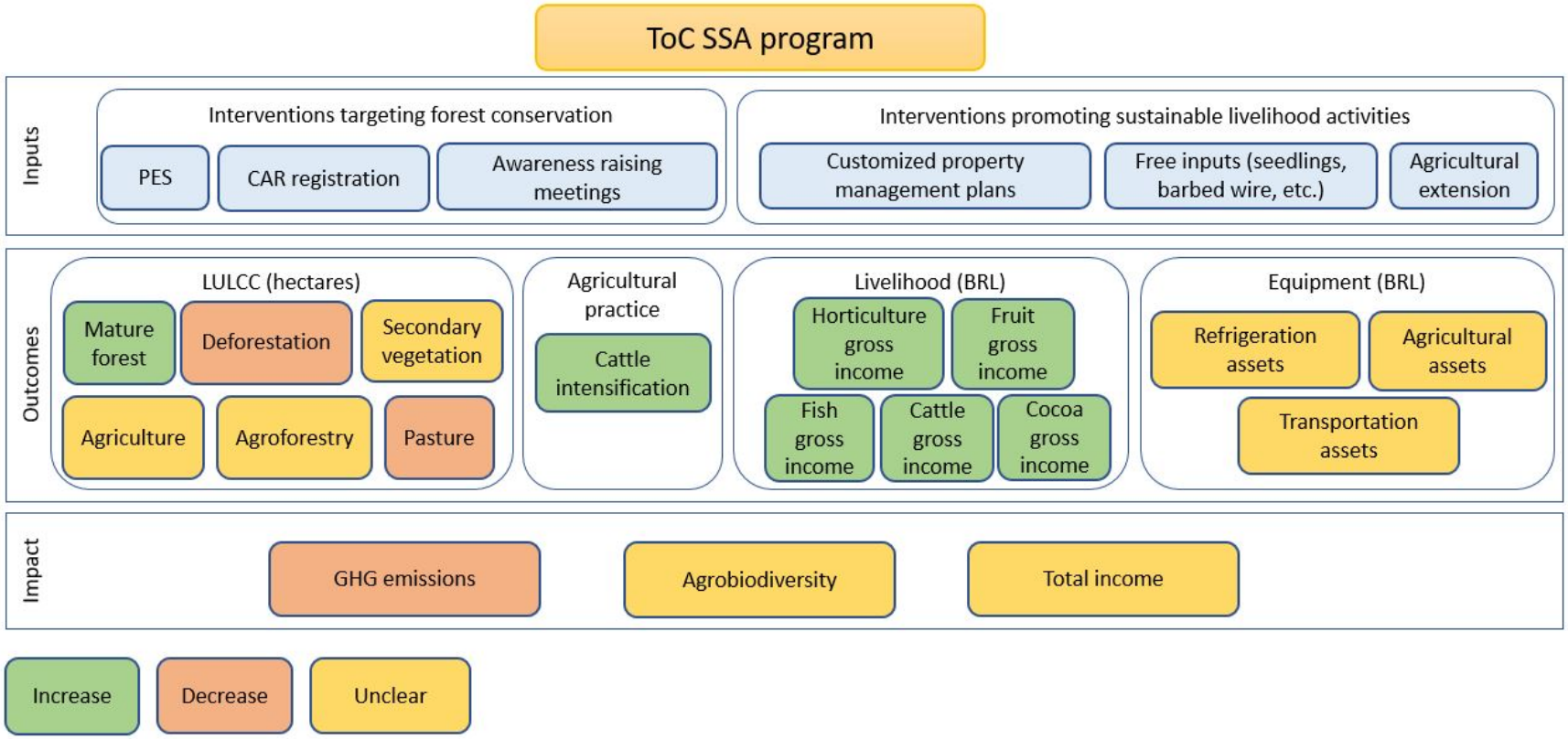
[3]The legal reserve consists in a fixed proportion of land covered with native vegetation that varies between 50% and 80% of the farmhold in the Amazon biome.

alternative production systems over time in order to reduce deforestation in the long term after the PES ended.

Interventions targeting forest conservation

At the top left of Figure 2.1, there are three interventions whose primary objective was to reduce deforestation in the short term, i.e., as soon as the household had signed its commitment to comply with the program requirements, namely the PES, the registration at the CAR, and attendance of the information meetings. Starting with PES, the rationale is that direct payments are expected to induce forest conservation by providing landholders higher economic returns from conserving forests than they would receive from deforestation (World Bank, 2018). In other words, by making standing forests more profitable for landowners than economic activities that generate deforestation (e.g., slash-and-burn agriculture, extensive cattle ranching), payments should induce them to cooperate with forest conservation (Phelps et al., 2013). Therefore, to effectively attract participants to the program, payments should at least offset the opportunity cost of deforestation, i.e., the yields lost by abandoning business-as-usual land uses (Wunder, 2008). In the case of the SSA program, according to IPAM, payments were defined based on the local yields from cattle ranching and agricultural activities (Pinto et al., 2020).

Figure 2.1: Representation of the SSA program Theory of Change



Note: This is a representation of the simplified theory of change linking the SSA intervention with intermediate outcomes and impact indicators. The color code corresponds to the hypothesized direction of the effect.

In small-scale PES-based pilot programs, participants' conservation actions are generally well monitored, and free-riding behaviors can easily be avoided (Pagiola, Honey-Rosés, and Freire-González, 2020). However, once farmers lose the financial incentive not to clear forests, business-as-usual deforestation becomes more economically attractive again (Swart, 2003). Thus, like payments offered in the SSA program, the PES are temporary by design (Pagiola, Honey-Rosés, and Freire-González, 2016) and expected to result in only temporary deforestation reduction (Wunder, 2008; World Bank, 2018). At least three recent studies have been able to highlight the change in deforestation that was induced by the PES offered as part of the SSA program (see Chapter 1, Simonet et al. (2018); Carrilho et al. (2022). On aggregate, their findings show that the program conserved, on average, between 2.24 to 8.45 hectares per farm while payments were ongoing. Moreover, PES was also probably the SSA intervention responsible for improving beneficiaries' perceived well-being during the program's initial years (Carrilho et al., 2022). Yet as predicted by theory, deforestation resumed after the temporary PES program ended. Still, according to Chapter 1 and Carrilho et al. (2022), the program left a permanent environmental gain, since deforestation reduction achieved during the program was not offset by any catch-up behavior thereafter.

In the SSA ToC, both CAR and public information meetings were also expected to have a direct impact on deforestation reduction. These interventions were designed by IPAM with the goal of raising farmers' compliance with the Brazilian Forest Code. Indeed, CAR is one of the most important forest monitoring instruments of Brazilian environmental agencies such as IBAMA, and previous evidence indicates that registering properties on CAR might result in deforestation reduction (e.g., Alix-Garcia et al. (2018); Costa et al. (2018)). In addition, in the most isolated regions where households are not always well informed about forest conservation regulations, meetings such as those offered by IPAM can fill the information gap. In the present case, the information collected in the field suggests that the inhabitants of the study area (both in the treated and control communities) were generally well aware of the legal obligations of retaining a certain portion of native vegetation, the existence of forest monitoring by environmental agencies, and the risk of possible sanctions for non-compliance. In any case, it is important to emphasize that even if there were an impact of the CAR or the public information meetings on deforestation decisions, our empirical analysis framework would not allow us to highlight it, because in our data, both the participants and non-participants used as counterfactuals had a CAR and the meetings were open to the local community (while non-participants used as counterfactuals didn't receive PES, technical assistance nor free inputs).

To understand the broader picture of land-use changes, it is also important to analyze possible indirect impacts of the SSA program on other types of vegetation,

which include not only mature forest, but also secondary forests and fallow vegetation. These outcomes are represented in the central left part of Figure 2.1. As farmers in the study area usually deforest mature and secondary forests to grow crops and raise cattle, it seems reasonable to expect that avoided deforestation will come at the expense of pasture or cropland (Simonet et al., 2018). One might also expect that some of the participants displace part or all of their deforestation from mature forests to secondary forests and do not leave fallow vegetation aside to regenerate. If this were the case, participants would remain eligible for the payment, but there would be a trade-off between curbing deforestation and inducing forest regeneration. Thus, by sparing mature forests, PES are also likely to induce a negative impact on forest regeneration and, consequently, on carbon sequestration. According to Chapter 1, the SSA program reached the REDD+ goals of reducing deforestation and avoiding carbon emissions by 309,746 tCO2. However, this estimate does not take into account the likely impact of the SSA program on secondary vegetation. In this paper, we attempt to understand if avoided deforestation emissions were somehow negatively compensated by a reduction of secondary forests.

Interventions targeting household livelihoods

At the top right of Figure 2.1 is a representation of the package of non-conditional incentives designed to promote alternative livelihoods (i.e., customized property management plans, technical assistance, and free inputs) whose expected outcome are land uses less dependent on deforestation. This approach, not new, is based on the so-called Integrated Conservation and Development Project (ICDP) principle, which provides upfront subsidies and assistance to boost livelihoods likely to achieve the dual objective of poverty reduction and environmental conservation (Sanjayan, Jansen, and Shen, 1997). The assumption behind this strategy is that more environmentally-friendly land uses can provide higher economic returns than current, less sustainable, practices. Therefore, as long as the program incentives overcome obstacles to their adoption (e.g., startup costs, farmers' lack of technological knowledge, social approval and acceptance), one can expect that beneficiary farmers will switch from business-as-usual to sustainable practices over the long term (Pagiola, Honey-Rosés, and Freire-González, 2020). Such interventions are expected to support deforestation reduction even after the end of conditional payments.

For various reasons, however, the transition from a system dependent on deforestation to a sustainable one may not occur (Wright et al., 2016). First of all, the returns from the sustainable activities may actually be lower than those from current practices, at least for some of the beneficiaries. Remember that rural households are not homogeneous and are thus expected to vary in many aspects, in-

cluding the opportunity costs of adopting more environmentally-friendly land uses (Piñeiro et al., 2020). If the proposed land uses are less profitable than business-as-usual activities, participants will not adopt them or, when adopting, will abandon the new activities after receiving frustrating results (Pagiola, Honey-Rosés, and Freire-González, 2020). Second, since these incentives are non-conditional, i.e., there are no requirements associated with receiving the package, certain diversion behaviors of the inputs offered may be observed (Pagiola, Honey-Rosés, and Freire-González, 2016). Typically, beneficiaries could use the free inputs to invest in business-as-usual activities instead of in sustainable land uses. Finally, the beneficiaries may adopt new activities without abandoning business-as-usual practices (Barrett, Reardon, and Webb, 2001). This combination of new and old activities could be possible, for example, (i) by reallocating time devoted to production activities among household members (Allison and Ellis, 2001), (ii) by reducing household members' leisure time (Epstein et al., 2022), and (iii) by distributing over the year the dedication to multiple activities in order to maximize economic returns, based on product seasonality and the variation of market prices (Van Vliet, 2010). Thus, whether ICDP-type incentives can be effective in reducing deforestation over the long term is difficult to anticipate. Previous evaluation of the impact of ICDP programs on conservation outcomes provides evidence of disappointing results (Roe et al., 2015).

In the case of the SSA program, however, the context appeared particularly favorable, since the sustainable activities were previously agreed upon between IPAM and the household heads themselves. According to Carrilho (2021), 48% of the sampled households self-declared that have implemented sustainable activities between 2014, when IPAM began providing technical assistance and free inputs, and 2019. When comparing this number to how many matched non-participants adopted alternative activities in the same period, the authors show the SSA program increased by approximately 40% the probability of households adopting new livelihood activities. Notably, the authors also show that participants who adopted new sustainable activities continued to have more self-declared forest cover than matched non-participants, even after the program ended. Yet the results suggest that this was insufficient to promote long-term deforestation avoidance in average terms. Moreover, the authors show that the SSA program increased beneficiaries' agricultural productivity and annual farm income (approximately 3,200 BRL per cultivated hectare more in the participating farms than in their matched counterparts). However, the authors do not investigate if the program positively impacted the agricultural production generated by the sustainable activities, which we, therefore, address in this article. We also investigate potential SSA impacts on households' physical assets necessary to agricultural production and transportation of the products for sale. We posit that beneficiaries might

have used REDD+ transfers to accumulate longer-lasting assets, which could have contributed to maintaining the alternative activities and enhancing participants' well-being.

The bottom right of Figure 2.1 shows two outcomes assumed to be affected by the package of incentives to adopt sustainable production systems. First is total gross income, a measure of household well-being. We estimated SSA impacts not only on total income, but also on the income from salary and family business, taken separately. Despite Carrilho (2021) finding improvements in farm income, it would still be possible that beneficiaries faced some trade-off between income sources. As household members were supposed to dedicate time to the sustainable activities IPAM wanted to promote, they might have had to reduce time devoted to other economic activities. For instance, in the Transamazon region, it is common for farmers to do some daily-wage work on neighbor farms, in addition to working on their own property. Time devoted to these daily-wage jobs may have been reallocated to the new activities. In this case, if the returns from the new activities did not overcome those from the abdicated daily-wage work, contrary to what the participants had hoped, the SSA program might have led to negative impacts on total income. This could help explain why Carrilho et al. (2022) detected that participants' perceived that their well-being declined after the program ended.

The second final outcome that we can assume will be affected by the SSA program is a measure of agrobiodiversity on the farm. By diversifying livelihoods, the SSA program may have promoted unplanned increases in the agrobiodiversity of participant farms, which could have potential benefits to farmers' food access. On the other hand, since the payments were conditional on activities not dependent on deforestation, it is quite possible that landholders decreased the production of cassava, corn, banana, beans, and other crops (that are dependent on deforestation). Therefore, on the one hand, incentives to adopt new subsistence crops may have increased agrobiodiversity, while on the other hand, the impediment to clearing new areas may have decreased the number of crops that were commonly cultivated in the area prior to the program.

2.3 Data sources and variables

2.3.1 Land Use and Land Cover (LULC) remote-sensing data

The map with the localization of the farmholds enrolled in the SSA program is publicly available on IPAM's website (http://www.pas-simpas.org.br/). We used it to geolocalize the boundaries of the farms enrolled in the program. In order to build a control group, we also used property boundaries from the CAR of 11,457

farmholds in our study area. Land Use and Land Cover (LULC) annual maps were obtained from the MapBiomas project. These maps are produced based on the classification of Landsat imagery mosaics. The mosaics are then used to produce a map with land cover classes (forest, agriculture, pasture, urban area, water, etc.) using the random forest algorithm. All data are publicly available at the MapBiomas website (https://mapbiomas.org). Detailed information on the processing and validation of this dataset is provided in Souza and Azevedo (2017).

The spatial resolution of the dataset did not allow us to assess land use classes that cover small areas (< 1 ha). In our case, this means that MapBiomas does not typically provide data on the area covered by crops and agroforestry. Hence, we focused our analysis on pasture cover, mature forest, and secondary forest. We computed the surface of each LULC class for each farmhold by multiplying the number of pixels classified by the pixel area (0.09 hectares). At the time of the analyses, we only had access to data on pasture and mature forest cover up to 2019 and on secondary forest cover ending in 2017. The forest land use class includes both primary and secondary forest together. To be consistent with Chapter 1, we built LULC maps starting in 2008. Therefore, we ended up with five observations for the period prior to the program start (2008-2012), five observations for secondary forest cover (2013-2017), and seven observations for pasture and forest (2013-2019) after the program began.

2.3.2 Socioeconomic survey data

We used household-level survey data from the Center for International Forestry Research (CIFOR)'s Global Comparative Study (GCS) on REDD+. Data were collected in eight communities (four intervention and four comparison) in three periods, one pre-treatment in 2010 (the baseline) and two post-treatment: 2014 (one year after the program began) and 2019 (two years after the program ended). Intervention communities were randomly chosen from a pool of twelve communities in which IPAM had planned to implement the SSA program. In turn, the comparison communities were selected based on a pre-matching procedure with another pool of fifteen communities located in the Transamazon region. The pre-matching procedure was to identify communities with a balanced distribution of characteristics that could influence the selection of SSA's target areas, e.g., forest cover, deforestation pressures, and market accessibility (Sunderlin et al., 2016).

A total of 240 households (30 in each community) were randomly selected for face-to-face interviews during the baseline period. There was considerable attrition of households between the three survey rounds. This includes households that moved, passed away, were traveling, or no longer wanted to participate in the study. The final sample of households for which we could obtain information from the three survey rounds thus includes 98 households: 52 treated farms (i.e.,

program participants) and 46 comparison farms (non-participants likely to be used as matched counterparts). Besides land use information, the GCS dataset includes socioeconomic characteristics of the households[4] (e.g., demographic data, sources of income, assets).

When looking at the alternative livelihoods, we focused on four variables measuring the farmer's involvement in sustainable activities: i) gross income from cocoa farming (*Theobroma cacao*); ii) gross income from horticulture (e.g., carrots - *Daucus carota*, parsley - *Petroselinum crispum*, lettuce - *Lactuca sativa*), iii) gross income from fruit pulp manufacturing (e.g., from açaí - *Euterpe oleracea*, cupuaçu - *Theobroma grandiflorum*, passion fruit - *Passiflora edulis*), and iv) gross income from fish farming. We also looked at cattle production and cattle intensification, measured as the change in the cattle stocking rate, i.e., the ratio between the number of adult cattle per hectare of pasture. The gross income of cattle, as well as the production of the five alternative livelihoods, were estimated as the product of the production volume (consumed and traded) from the twelve months prior to the survey and the market prices.

In addition, we examined three asset categories: i) agricultural equipment (e.g., tractor, plow, water pump, wheelbarrow), ii) refrigeration equipment (i.e., refrigerator/freezer) since we consider them essential to store fish from psiculture and frozen fruit pulp, and iii) transportation equipment (e.g., automobile, truck). Survey data also allowed us to measure the total income of the households in the sample, i.e., all household yields obtained in the 12 months prior to the survey. This included the yields from farm activities (crop and animal production) and environmental income (i.e., income from products obtained from low or no management forest and non-forest areas), both for consumption and trade, added to the income from salary, wages, family business, government transfers, and other possible income sources (e.g., the renting out of land, remittances from relatives, inheritance, etc.). We also looked at income from salary, wages, and family business separately, since we suspected that households could have reduced these activities to invest time in the new livelihoods. Finally, we used survey data to construct a variable of crop richness, measured by the number of crops grown on the farm, divided by the total farm area. We used this as a proxy for agrobiodiversity.

[4]GCS data also involve a number of interviews with IPAM and other key informants, such as government officials, local NGOs, and community associations. We used this information to better understand the SSA program and its target areas, to build the ToC, and to interpret the results of the impact analysis.

2.4 Identification strategy

We estimated the impact of the SSA program on a series of variables that included LULC, agrobiodiversity, livelihood, and socioeconomic outcomes. To do so, we estimated the difference between the change in the level of the outcome observed on participating farmholds and that which would have been observed in those same farmholds if they had not been enrolled in the REDD+ initiative (i.e., the counterfactual scenario). This is the Average Treatment Effect on the Treated (ATT), $ATT = E(y^1 - y^0|D = 1)$, where y^1 denotes the change in the level of the outcome variable under the treatment, y^0 is the same variable in the absence of treatment, and D is a dummy that takes the value of one when the household has been treated and zero otherwise. Since we cannot observe y^0 when D equals 1, the counterfactual scenario has to be estimated (Ferraro, 2009).

This is not a straightforward task, since the intervention was not randomly assigned. Participation in the SSA program, like in most REDD+ projects, was indeed voluntary. Therefore, one can expect that farmers who chose to participate have different characteristics than those who declined (e.g., social preferences, environmental motivations, human and natural capital). If these pre-existing differences between participants and non-participants were correlated to the outcomes of interest, comparing the two groups directly would yield biased estimates of the program's impact (White and Raitzer, 2017). However, it is reasonable to assume that in comparison communities it is possible to find a number of farmholders who would have participated in the program, had they been offered to do so. Therefore, we used similar farmholders as matched counterparts of participants.

We used a Difference-in-Difference (DID) approach combined with a matching procedure, using a series of pre-treatment observable characteristics likely to affect both a farmer's decision to participate in the program and the outcomes of interest (Imbens and Wooldridge, 2009). We used the Nearest Neighbor matching (NNM) and the Propensity Score matching (PSM) estimators, which matches each treated farmhold to the most similar non-participant farmhold from the comparison group (Abadie et al., 2004). Insofar as the sample of farms for which we have LULC remote-sensing panel data is different and much larger than that for which we have survey panel data (more than 11,000 farmholds in the first case versus 98 farmholds in the second case), the vector of covariates used for the matching procedure was different in the two cases.

For the sample of 11,299 farmholds for which we have panel data on pasture areas, forest areas, and secondary forest areas, the set of covariates used for the matching procedure also included the farm size, the distance from the farm to: i) the main road, ii) the main navigable river, iii) the main market, and iv) the nearest village. Summary statistics and balancing tests are presented in the Appendix in Table 2.5. The results show that, before matching, the participant group was

significantly different from the non-participants for most covariates and that after matching, these differences dropped below 0.25 standard deviations, suggesting that the matching procedure performed well. To deal with the Stable Unit Treatment Value Assumption (SUTVA), we excluded from the untreated group those farmholds that were less than three (3) kilometers distant from a treated plot, thus creating a so-called buffer zone between treated plots and potential control ones.

For the sample of 98 farms for which we have survey panel data on livelihoods and socioeconomic and agrobiodiversity outcomes, the set of covariates used for the matching procedure included five variables only extracted from the baseline survey[5]: i) the number of members in the family, ii) the total area of the farm, iii) the share of the farm covered by forest, iv) the share of the farm under pasture, and v) the total household income. Summary statistics and balancing tests are presented in the Appendix in Table 2.6. The results show that before matching, the participant group was significantly different from the non-participant group and all normalized differences of the baseline covariates, except for household members, were higher than 0.25 standard deviations, while after matching, these differences dropped below 0.25 standard deviations, indicating that selection bias decreased and, therefore, a valid control group was constructed from non-participating households.

2.5 Results

2.5.1 Impacts on LULC outcomes

Table 2.1 displays the estimates of the impact of the SSA program on forest cover, each year, over the period 2013-2019, using six different matching estimators. In most cases, we failed to demonstrate a significant effect of the program over its first year of implementation. On the other hand, the results tend to show a positive impact of the program, i.e., a statistically larger forest area on the treated farms than on the control farms, each year from 2014 until 2019. The last row of Table 2.1 gives the average forest area in the treated group. The numbers show that the forest cover of the treated farms decreased every year, but it decreased less than in the control group, which is why the ATT is always positive. The ATT indeed ranged from 1.1 ha to 5.4 ha in 2014 and increased steadily every year, eventually ranging between 4.4 ha and 8.1 ha in 2019. This indicates that the

[5]Since we aimed to assess the SSA effects on multiple outcomes, including the pre-treatment values of each of the outcomes in the matching procedure would create different control groups for each estimation. However, including all of the pre-treatment values would have complicated the matching procedure, given the sample size. We thus chose to match on the same set of baseline covariates, without controlling for pre-treatment outcomes.

effect of the program continued even two years after its end (2017). At the time the study was conducted, it had saved more than 4 ha of forest, on average, per farm (taking the smallest estimate).

Table 2.1: Impact of the REDD+ project on forest cover each year between 2013 and 2019

Estimator	2013	2014	2015	2016	2017	2018	2019
NNM (4X)	0.310	1.125**	1.947***	2.596***	3.330***	3.700***	4.409***
	0.435	0.499	0.562	0.633	0.710	0.790	0.815
NNM (2X)	0.295	1.095**	2.082***	2.844***	3.680***	4.309***	5.004***
	0.397	0.503	0.588	0.677	0.759	0.833	0.877
NNM (1X)	0.539	1.531**	2.796***	3.592***	4.589***	5.296***	5.964***
	0.515	0.623	0.730	0.816	0.900	0.947	0.981
PSM (4N)	1.472	2.308	3.024**	3.642**	4.480***	4.822***	5.831***
	1.462	1.521	1.519	1.510	1.483	1.441	1.307
PSM (2N)	2.658**	3.531**	4.087***	4.708***	5.528***	5.559***	6.405***
	1.281	1.446	1.505	1.530	1.542	1.511	1.327
PSM (1N)	4.497**	5.378***	5.889***	6.399***	7.239***	7.224***	8.123***
	1.791	1.893	1.920	1.922	1.939	1.915	1.687
Mean in treated	49.479	48.575	47.655	46.275	45.611	44.111	40.992

Notes: This table displays the average treatment effect (ATT) on forest area in hectares. NNM(4X) (resp. 2X and 1X) refers to the DID nearest neighbor estimator using 4 (resp. 2 and 1) matched observations as controls. PSM(4N) (resp. 2N and 1N) refers to the DID propensity score matching estimator using 4 (resp. 2 and 1) matched observations as controls. ***, ** and * indicate that the estimated coefficients are statistically significant at the 1%, 5%, and 10% levels, respectively. Standard errors are given in parentheses.

Similarly, Table 2.2 displays the estimates of the impact of the SSA program on pasture area, each year, over the same period, using the same estimators. Again, results do not show any significant impact of the program in 2013. In most estimates, they also show no significant impact in the second year of the program (2014). From 2015, however, the results tend to show a negative impact of the program, i.e., a statistically smaller pasture area on the treated farms than on the control farms. The last row of Table 2.2 shows that the pasture cover of the treated farms increased every year on treated farms, but it increased less than in the control group. Thus, the ATT ranged from -3 ha to -1.6 ha in 2015 and (its absolute value) increased steadily every year, until 2019 when it ranged between -7.1 ha and -3.3 ha. This suggests that almost seven years after its launch, the program had prevented the establishment of more than 3 ha of pasture, on average, on each enrolled farm (taking the smallest estimate).

Finally, Table 2.3 displays the results of the estimates of the impact of the SSA program on secondary forest area, using the same identification strategy. Results show a quite clear impact of the program from 2015 to 2017 (our analysis stops in

2017). The secondary forest area of the treated farms slightly decreased between 2015 and 2017, but it decreased less than in the control group, which is why the ATT is positive: it represented more than half a hectare in 2015 and almost a hectare in 2017 (taking the smallest estimates). This suggests that there was actually not a trade-off between deforestation reduction and forest regeneration. Program participants therefore did not offset the reduction in mature forest cutting by an increase in secondary forest cutting or by impeding fallow regeneration. Quite the contrary, it would seem that they made an effort both on the mature forest and on the secondary forest. One possible explanation for this is the need for rural properties to comply with the Brazilian Forest Code and recover permanent preservation areas (PPA) in order to receive the PES. To comply with the law, the farmers indeed had to delimit the PPA along streams and rivers and around water springs, isolating those areas with natural fences (namely trees) or physical barriers to avoid the entry of animals and human activity.

Table 2.2: Impact of the REDD+ project on pasture cover each year between 2013 and 2019

Estimator	2013	2014	2015	2016	2017	2018	2019
NNM (4X)	-0.232	-0.928**	-1.599***	-1.620***	-2.214***	-2.736***	-3.284***
	(-0.336)	(0.445)	(0.517)	(0.609)	(0.711)	(0.819)	(0.911)
NNM (2X)	-0.303	-1.433***	-2.185***	-2.431***	-3.253***	-3.902***	-4.657***
	(0.305)	(0.442)	(0.544)	(0.640)	(0.744)	(0.862)	(0.981)
NNM (1X)	-0.364	-1.497***	-2.479***	-2.552***	-3.339***	-4.032***	-4.673***
	(0.299)	(0.491)	(0.595)	(0.730)	(0.853)	(0.999)	(1.116)
PSM (4N)	-0.781	-1.824	-2.621	-3.133***	-4.155***	-4.590***	-5.795***
	(1.470)	(1.539)	(1.532)	(1.440)	(1.439)	(1.443)	(1.549)
PSM (2N)	-1.060	-2.070	-2.739***	-3.167***	-4.250***	-4.678***	-5.862***
	(1.287)	(1.255)	(1.210)	(1.107)	(1.094)	(1.130)	(1.382)
PSM (1N)	-1.304	-2.453	-3.008	-3.644***	-4.735***	-5.487***	-7.091***
	(1.557)	(1.606)	(1.580)	(1.460)	(1.475)	(1.541)	(1.821)
Mean in treated	28.352	29.300	30.254	31.705	32.458	34.055	37.196

Notes: This table displays the average treatment effect (ATT) on pasture area in hectares. NNM(4X) (resp. 2X and 1X) refers to the DID nearest neighbor estimator using 4 (resp. 2 and 1) matched observations as controls. PSM(4N) (resp. 2N and 1N) refers to the DID propensity score matching estimator using 4 (resp. 2 and 1) matched observations as controls. ***, ** and * indicate that the estimated coefficients are statistically significant at the 1%, 5%, and 10% levels, respectively. Standard errors are given in parentheses.

Taken all together, these results suggest that the program was effective in curbing deforestation among participants one or two years after the start of the program and up to two years after its end. The magnitude of the estimated effects quite clearly suggests that the conservation effort made on the forest cover from

Table 2.3: Impact of the REDD+ project on secondary forest cover each year between 2013 and 2017

Estimator	2013	2014	2015	2016	2017
NNM (4X)	0.318**	0.364*	0.674**	0.773**	0.951**
	(0.126)	(0.201)	(0.269)	(0.321)	(0.379)
NNM (2X)	0.325**	0.348	0.639**	0.827**	1.097***
	(0.129)	(0.213)	(0.292)	(0.357)	(0.414)
NNM (1X)	0.337**	0.386	0.746**	0.992**	1.382***
	(0.161)	(0.256)	(0.367)	(0.433)	(0.510)
PSM (4N)	0.826	0.876	1.213*	1.190*	1.262*
	(0.652)	(0.670)	(0.660)	(0.644)	(0.650)
PSM (2N)	0.900	1.025	1.350*	1.361*	1.451**
	(0.738)	(0.749)	(0.738)	(0.705)	(0.698)
PSM (1N)	1.182	1.209	1.646*	1.584*	1.757**
	(0.877)	(0.875)	(0.853)	(0.811)	(0.805)
Mean in treated	10.364	10.436	10.709	10.209	10.178

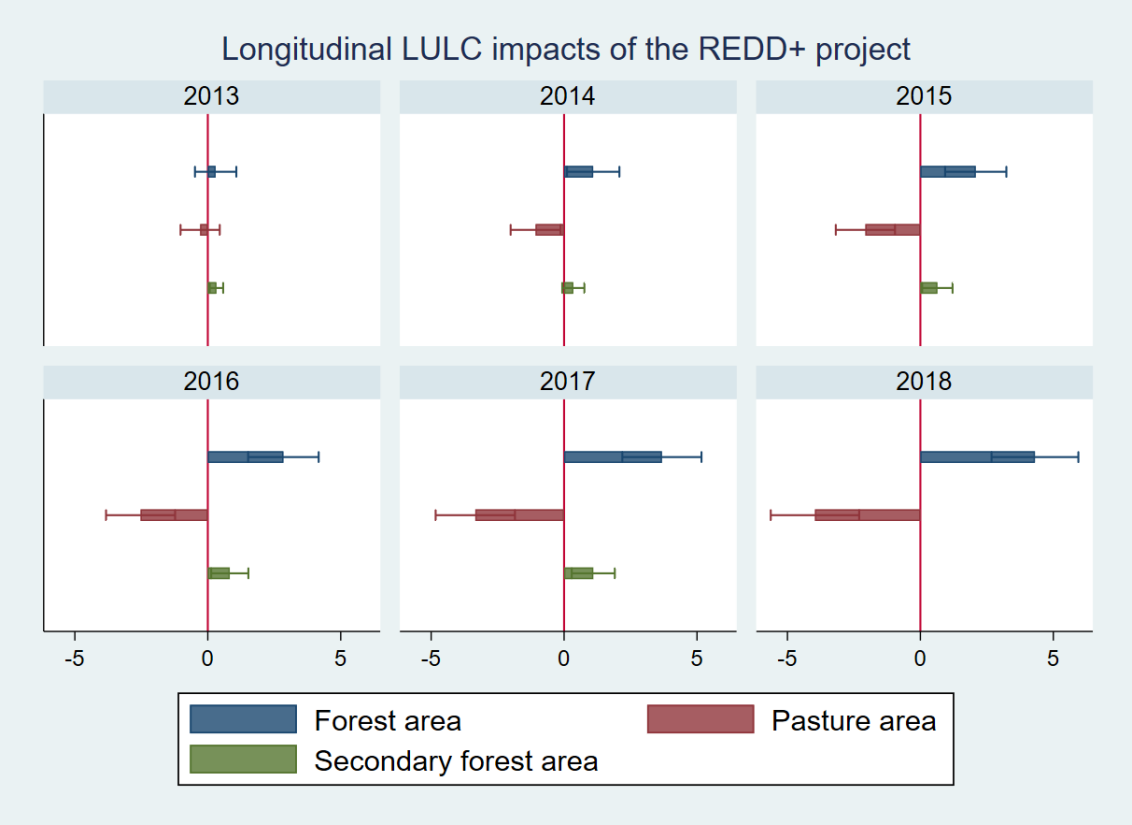
Notes: This table displays the average treatment effect (ATT) on secondary forest area in hectares. NNM(4X) (resp. 2X and 1X) refers to the DID nearest neighbor estimator using 4 (resp. 2 and 1) matched observations as controls. PSM(4N) (resp. 2N and 1N) refers to the DID propensity score matching estimator using 4 (resp. 2 and 1) matched observations as controls. ***, ** and * indicate that the estimated coefficients are statistically significant at the 1%, 5%, and 10% levels, respectively. Standard errors are given in parentheses.

2015 resulted almost mechanically in a lesser increase in pasture. This result is illustrated by Figure 2.2, which shows the estimated ATT, positive for forests and negative of comparable magnitude for pasture.

2.5.2 Impact on livelihood outcomes

Main findings on program impacts on variables measuring livelihoods from CIFOR surveys are displayed in Table 2.4 (robustness checks are presented in the Appendix in Tables 2.7–2.10). The first column of results presents the estimates made for the variables measured in 2014 (the short-term impact). The second column of results presents the estimates made for the variables measured in 2019 (the long-term impact). In general, the results regarding the adoption of more sustainable activities suggest that the program indeed boosted the production of alternative livelihoods, but only over the long run. No statistically significant impacts were found in the early project stage. However, looking at 2019, i.e., six years after the project began and two years after its end, we found significant

Figure 2.2: Impact of the REDD+ project on LULC outcomes



Note: This graph displays the ATT estimates for the three LULC outcomes. Bars represent the ATT, estimated using the DID nearest-neighbor estimator that matches 4 observations as controls. Brackets represent 95 percent confidence intervals. Forest area includes both primary and secondary forests. Data for secondary forests is only available until 2017.

impacts on the production of three of the four activities under study: i) fish production (almost 5,000 BRL more annual income among the treated than among the controls), ii) horticulture production (almost 1,900 BRL more), and iii) fruit pulp production (1,200 BRL more). Yet, we failed to detect significant impacts on cocoa income, whether short- or long-term. One possible explanation for such a result is that the cocoa tree takes a long time to produce, meaning that, even if the project had indeed triggered the adoption of this activity, the results would only be detectable over a longer time horizon.

Results also indicate that the project somehow contributed to the transition from extensive to more intensive cattle ranching systems. We indeed found a

positive gap in the cattle stocking rate (i.e., the number of adult cattle per pasture area) between the two groups, which equaled 0.4 in the short run and 0.69 in the long run. Since our results on LULC outcomes indicate that the project had a negative impact on pasture expansion, this means that farmers simply raised more cattle using less pasture area because of the project. In the long run, we also found that the increase in the cattle stocking rate was followed by an increase of almost 22,000 BRL in annual gross income from cattle production.

The increase in the production of cattle, fish, horticulture, and fruit pulp was not followed by significant impacts on households' assets, except for refrigeration equipment (in the long run). Participating households were expected to use at least part of the payment received to accumulate the equipment required for new agricultural production and transportation. However, we found no evidence of this, whether in the short or long term. As for refrigeration assets, we found an increase of about 1,000 BRL, which could be related to the expansion of fish and fruit pulp production.

Looking at total household income, we found a negative impact (almost 1,500 BRL) on the income from salary, wages, and family business, in the short run only. As mentioned in the ToC section, the likely explanation is that households may have invested less time in business-as-usual activities to invest more time in the new activities promoted by the project. Moreover, this early negative impact seems to have then been compensated by positive impacts on the total income in the long run, since we found that participation in the project increased total income by an average of more than 40,000 BRL by 2019.

Finally, our results also suggest that the project had a positive impact on farms' agrobiodiversity. We found an increase in crop richness, as measured by the number of crops grown on the farm divided by the farm's total area that reached 0.08 in the short run and 0.09 in the long run. Considering that the average land area of the treatment group is about 80 ha (see summary statistics in Table 2.6), this indicates an average increase in the number of cultivated crops of 6.3 in the short run and 7.4 in the long run.

2.6 Discussion

2.6.1 No catch-up of postponed deforestation after end of payments

Our main results on the impact of the SSA project on forest cover are in line with and complement the findings of previous studies that evaluated the same program using different data. Using survey panel data on forest cover, Simonet et al. (2019) estimated that, as of 2014, the program had saved, on average, about

Table 2.4: Short- and long-term effects of the REDD+ project on livelihood outcomes

Alternative livelihoods	Short-run impact	Long-run impact
Cocoa gross income	517.53	7407.36
	(1393.54)	(8479.02)
Fish gross income	-1232.64	4597.091*
	(1781.98)	(2657.53)
Horticulture gross income	403.65	1876.85***
	(365.16)	(708.43)
Fruit pulp gross income	321.18	1200.23***
	(250.26)	(430.08)
Equipment		
Refrigeration assets	194.59	1059.1**
	(390.24)	(480.49)
Agricultural assets	320.47	133.21
	(526.14)	(502.42)
Transportation assets	1892.86	3429.12
	(1920.99)	(2319.32)
Cattle production		
Cattle stocking rate	0.4*	0.69**
	(0.21)	(0.29)
Cattle gross income	-3589.74	21974.23**
	(5614.88)	(10458.55)
Agrobiodiversity		
Crop richness	0.079**	0.092***
	(0.03)	(0.03)
Income		
Total income	-7816.63	40279.07**
	(13557.19)	(17096.00)
Salary + Business income	-1436.67**	-1254.58
	(557.98)	(3103.65)

Notes: This table displays the ATT of the SSA project on short-run (2014) and long-run (2019) outcomes obtained with the nearest neighbor estimator (NNM) using 2 matched observations as controls. The cattle stocking rate is expressed as the herd size divided by the pasture area. Crop richness is expressed as the number of different crops divided by the total area. ***, ** and * indicate that the estimated coefficients are statistically significant at the 1%, 5%, and 10% levels, respectively. Standard errors are given in parentheses.

4 hectares of forest on each participating farm, which is also found by Carrilho et al. (2022) using the same survey data. Using remote sensing panel data on annual forest loss, Chapter 1 highlighted that the declarative data relating to deforestation practices may have somewhat overestimated the impact found by Simonet et al. (2019) and that the program had more likely saved 2 hectares per farm, on average, during the first three years of the project.

Moreover, we showed in Chapter 1 that this reduction in deforestation stopped before the end of the payments (or else became too low from 2017 to be detected using public data), meaning that the participants had resumed their usual rhythm of annual cutting. However, we provided evidence that no catch-up of postponed deforestation was observed thereafter. Using a satellite dataset different from that used in Chapter 1, the present study corroborates this absence of catching-up, since it highlights the persistence of a gap in forest cover between the treated and control groups two years after the end of payments. Taken end to end, these works thus support the idea that the SSA program did indeed reduce deforestation during the period of PES payments but not beyond and that the environmental gain generated during this short period was not subsequently canceled – at least until 2019, when our analysis ends.

2.6.2 The interplay of deforestation and the intensification of cattle ranching

Our results are also in line with previous findings from Simonet et al. (2019) that used survey data to show that the decrease in deforestation occurred mainly at the expense of the slowdown in the expansion of pasture areas. We came to the same conclusion using satellite panel data on pasture areas of the whole population of participants. We investigated whether this decrease in pasture expansion had a negative impact on cattle herds and found that the number of cattle per hectare had increased on the farms benefiting from the program, suggesting that there had in fact been an intensification in cattle ranching activities. These findings add to the knowledge on the SSA project by pointing out that one of the mechanisms through which the conservation of primary and secondary forest was achieved was the intensification of cattle ranching.

A number of scholars have advocated that encouraging cattle ranching intensification in Brazil could decline greenhouse gas emissions by sparing land from deforestation (Nepstad et al., 2014; Cohn et al., 2014; Garrett et al., 2018). The idea is that intensification of cattle ranching could help ranchers use the already deforested land more efficiently and prevent them from clearing more land. More recently, however, the likely effects of land-use intensification on deforestation have been debated in the literature. Müller-Hansen et al. (2019) developed an agent-

based model to study the interplay of deforestation and the intensification of cattle ranching in the Brazilian Amazon. The model shows that intensification can lower deforestation rates under certain conditions only, when the local cattle market is saturated. Indeed the model shows that in most scenarios intensification would not reduce deforestation rates and sometimes would even increase them. An evaluation of the SSA program in a few years would provide an empirical contribution to the debate.

2.6.3 The coexistence of sustainable and non-sustainable systems

Our results suggest that the implementation of sustainable activities under the SSA project seem to have created new means of subsistence for the participants and thus new sources of income. These effects, however, are noticeable only two years after the end of the program (2019). This suggests that when program participants voluntarily adopt new practices which require a greater mobilization of techniques, knowledge, and resources, it takes time for their effects to become observable through the data. Our results also show that participants simultaneously continued with more conventional and environmentally damaging systems and that cattle ranching continues to be one of their main sources of income.

Promoting a structural change in agricultural practices by stimulating the adoption of more sustainable activities and keeping deforestation rates permanently low at the same time, proves to be quite challenging. The relatively higher profitability of cattle ranching seems to be the most obvious explanation. However, recent studies have shown that livestock production yields the lowest per hectare incomes and still remains the most prevalent land use in remote areas of the Brazilian Amazon (Garrett et al., 2017). The literature presents several explanations as to why changes in agricultural practices are difficult to achieve, ranging from social preferences, the lack of technical assistance and rural extension services, the absence of clear land tenure, and lack of access to credit.

In the case of the SSA program, most of these bottlenecks were solved, or at least temporarily alleviated, but still, there was a relatively low uptake of the alternative livelihood activities (Carrilho, 2021). Qualitative data collected from the field suggest that one of the biggest obstacles to the adoption of alternative agricultural activities is poor access via unpaved roads, which gets worse every year during the rainy season, making it difficult not only for people to move around, but also for the outflow of agricultural production. According to several farmers' narratives, the lack of access makes it impossible to market the agricultural production most of the year (typically between November and May). This would be one of the main reasons why farmers continue to focus on cattle ranching, as cattle

buyers come directly to the farms to buy the animals.

2.7 Conclusion

This study complements a series of recently carried out analyses aimed at evaluating the effectiveness of one of the first pilot PES programs implemented in the Brazilian Amazon. By combining satellite data never before used for this case study, covering all program participants (and thousands of non-participants who could be used as a control group), and survey data collected from a small but extremely rich and precise sample, we were able not only to corroborate or amend the findings of previous studies but also to complete the story of the project, namely the mechanisms by which the objective of reducing deforestation was achieved.

The key messages one can take away from this analysis relate to the likely short- and long-term effectiveness of REDD+ projects that aim at improving both forest conservation and household livelihoods. Overall, our findings suggest that the decrease in deforestation occurred mainly at the expense of the slowdown in the expansion of pasture areas. When we investigated whether this decrease in pasture expansion had a negative impact on cattle herds, we found that the number of cattle per hectare had increased on the farms benefiting from the program, suggesting that there was in fact an intensification in cattle ranching activities, something that can be observed in the short run. Our results further show that the program had a positive impact on the adoption of alternative production activities that require less area for production than extensive livestock farming and slash-and-burn agriculture, the two main drivers of deforestation in the region. The development of such activities, however, is not statistically observable in the short term, while they are designed to have lasting impacts, contrary to PES. Altogether, these results suggest that local REDD+ programs that combine PES with technical assistance and support to farmers for the adoption of sustainable activities can be effective in reducing deforestation in the short run, at least as long as the PES last, without jeopardizing the standard of living of participants. They also show that a number of households are ready to adopt new agricultural practices, while maintaining their traditional ones. The question of whether the coexistence of both types of production systems is sustainable over time or not remains open. In any case, the transmission of technical knowledge necessary for the development of environmentally sustainable activities was effective and it cannot be ruled out that the participants who have acquired this new knowledge during the program will use it in the future.

2.8 Appendix to Chapter 2

Table 2.5: Summary statistics of LULC variables for participants and comparison groups

	Participants (n=348)		Comparison (n=10,950)		N.D.	
Pre-treatment variables	Mean	Std. Dev.	Mean	Std. Dev.	Raw	Matched
Total area (ha)	77.209	37.002	97.410	77.828	-0.335	0.210
Distance from nearest village (km)	23.092	11.245	29.327	14.736	-0.478	-0.039
Distance from Altamira (km)	122.601	56.746	164.546	98.194	-0.502	-0.085
Distance from Transamazon highway (km)	13.875	10.821	20.133	13.863	-0.690	-0.074
Distance from Xingu river (km)	63.761	52.797	111.857	84.054	-0.527	-0.072
Pasture area in 2008 (ha)	28.158	21.386	46.530	53.312	-0.454	0.174
Pasture area in 2009 (ha)	29.116	21.749	47.605	53.587	-0.453	0.175
Pasture area in 2010 (ha)	29.326	21.797	49.057	53.686	-0.483	0.172
Pasture area in 2011 (ha)	29.706	21.775	49.532	53.446	-0.488	0.173
Pasture area in 2012 (ha)	28.207	21.451	48.460	52.725	-0.505	0.162
Forest area in 2008 (ha)	49.906	29.043	51.513	54.012	-0.048	0.129
Forest area in 2009 (ha)	48.946	29.000	50.432	53.175	-0.045	0.125
Forest area in 2010 (ha)	48.730	29.004	48.985	52.281	-0.017	0.128
Forest area in 2011 (ha)	48.361	29.275	48.478	51.938	-0.013	0.128
Forest area in 2012 (ha)	49.853	29.730	49.509	52.436	-0.002	0.137
Secondary forest area in 2008 (ha)	6.433	9.127	6.406	10.226	0.002	0.138
Secondary forest area in 2009 (ha)	7.006	9.422	6.860	10.633	0.013	0.149
Secondary forest area in 2010 (ha)	7.204	9.685	7.107	10.944	0.008	0.146
Secondary forest area in 2011 (ha)	8.062	10.199	7.444	11.419	0.055	0.164
Secondary forest area in 2012 (ha)	8.594	10.678	8.123	11.960	0.040	0.167
Post-treatment variables						
Pasture area in 2013 (ha)	28.327	21.928	49.623	53.052		
Pasture area in 2014 (ha)	29.241	22.118	50.384	52.984		
Pasture area in 2015 (ha)	30.148	22.076	51.479	52.729		
Pasture area in 2016 (ha)	31.522	22.232	52.695	52.871		
Pasture area in 2017 (ha)	32.177	22.179	53.452	52.594		
Pasture area in 2018 (ha)	33.680	22.366	55.247	52.665		
Pasture area in 2019 (ha)	36.822	23.725	57.843	53.407		
Forest area in 2013 (ha)	49.479	29.790	48.453	52.445		
Forest area in 2014 (ha)	48.575	29.428	47.590	52.316		
Forest area in 2015 (ha)	47.655	28.790	46.382	51.612		
Forest area in 2016 (ha)	46.275	28.787	44.754	50.835		
Forest area in 2017 (ha)	45.611	29.065	43.904	50.512		
Forest area in 2018 (ha)	44.111	29.094	42.215	49.684		
Forest area in 2019 (ha)	40.992	28.100	40.286	48.300		
Secondary forest area in 2013 (ha)	10.330	11.356	9.265	12.954		
Secondary forest area in 2014 (ha)	10.397	11.170	9.406	13.323		
Secondary forest area in 2015 (ha)	10.666	11.592	9.987	13.835		
Secondary forest area in 2016 (ha)	10.162	11.177	9.701	13.855		
Secondary forest area in 2017 (ha)	10.125	11.287	9.712	13.975		

Notes: N.D.: normalized differences between the two groups. Forest area includes mature and secondary forests area.

Table 2.6: Summary statistics for participants and comparison groups from GCS survey dataset

Variables	Participants (n=52)		Comparison (n=46)		N.D.	
Pre-treatment variables	Mean	Std dev.	Mean	Std dev.	Raw	Matched
Household head age in 2010 (years)	48.73	11.42	53.91	11.42	-0.45	-0.07
Household members in 2010 (number)	5.59	2.45	5.33	2.63	0.10	0.03
Total area in 2010 (ha)	80.02	35.29	91.61	54.39	-0.25	-0.02
Forest cover (%)	69.62	15.80	0.59	0.22	0.55	0.09
Pasture cover (%)	20.09	15.75	0.33	0.23	-0.66	-0.16
Total income in 2010 (BRL)	49546.02	37734.36	61086.10	44547.24	-0.28	0.07
Salary + Business income in 2010 (BRL)	9502.25	14806.32	3049.56	5277.59		
Cocoa gross income in 2010 (BRL)	6392.20	9189.18	5087.53	13894.69		
Fish gross income in 2010 (BRL)	579.56	3685.89	43.94	298.02		
Cattle gross income in 2010 (BRL)	15458.57	20658.29	31836.80	31077.65		
Horticulture gross income in 2010 (BRL)	255.43	557.93	274.50	393.58		
Fruit pulp gross income in 2010 (BRL)	295.03	889.39	154.11	324.30		
Refrigeration assets in 2010 (BRL)	1143.47	1017.43	963.26	956.97		
Agricultural assets in 2010 (BRL)	1249.53	2048.27	853.89	979.94		
Cattle stocking rate in 2010	0.50	0.56	0.91	0.81		
Transportation assets in 2010 (BRL)	5925.98	12295.62	6501.63	11567.66		
Crop richness in 2010	0.17	0.11	0.17	0.11		
Post-treatment variables						
Total income in 2014 (BRL)	75227.36	59943.44	99891.39	120464.00		
Total income in 2019 (BRL)	111688.90	101528.30	92466.84	85108.62		
Salary + Business income in 2014 (BRL)	7167.41	11168.19	2300.24	3980.81		
Salary + Business income in 2019 (BRL)	8637.71	16915.89	3047.57	6469.20		
Cocoa gross income in 2014 (BRL)	6807.00	10137.32	4416.73	10281.47		
Cocoa gross income in 2019 (BRL)	16698.63	60282.37	4079.65	10411.12		
Fish gross income in 2014 (BRL)	1244.94	4909.81	757.57	5058.78		
Fish gross income in 2019 (BRL)	7162.55	18506.81	3084.35	15151.92		
Cattle gross income in 2014 (BRL)	23878.09	30616.69	43955.45	46145.65		
Cattle gross income in 2019 (BRL)	45955.12	58378.01	47383.80	60036.40		
Horticulture gross income in 2014 (BRL)	1756.95	2270.51	1168.04	2063.74		
Horticulture gross income in 2019 (BRL)	2385.83	5241.03	405.33	660.45		
Fruit pulp gross income in 2014 (BRL)	609.53	1498.88	173.12	281.08		
Fruit pulp gross income in 2019 (BRL)	1429.16	3222.57	181.99	349.03		
Refrigeration assets in 2014 (BRL)	1712.64	1539.57	1253.66	1494.76		
Refrigeration assets in 2019 (BRL)	2257.35	2595.53	1088.22	854.81		
Agricultural assets in 2014 (BRL)	2011.45	2616.53	1162.43	1089.75		
Agricultural assets in 2019 (BRL)	1890.69	1992.62	1185.00	1337.15		
Transportation assets in 2014 (BRL)	8517.18	15594.03	5862.49	7456.84		
Transportation assets in 2019 (BRL)	11721.57	21243.73	7734.78	11140.42		
Cattle stocking rate in 2014	0.51	0.52	0.69	0.82		
Cattle stocking rate in 2019	0.60	0.64	0.50	0.47		
Crop richness in 2014	0.27	0.19	0.19	0.11		
Crop richness in 2019	0.23	0.16	0.15	0.11		

Notes: N.D.: normalized differences between the two groups. Cattle stocking rate is expressed by the herd size divided by the pasture area. Crop richness is expressed by the number of different crops divided by the total area.

Table 2.7: Short- and long-term effects on alternative livelihood outcomes

Estimator	Cocoa gross income		Fish gross income		Horticulture gross income		Fruit pulp gross income	
	Short run	Long run	Short run	Long run	Short run	Long run	Short run	Long run
NNM (2X)	517.53	7407.36	-1232.64	4597.09*	403.65	1876.85***	321.18	1200.23***
	(1393.54)	(8479.02)	(1781.98)	(2657.53)	(365.16)	(708.43)	(250.26)	(430.08)
NNM (1X)	16.67	6681.80	-1049.74	4187.32	89.04	1813.00**	371.55	1244.46***
	(1356.82)	(8379.15)	(1668.03)	(3187.27)	(443.32)	(704.81)	(261.75)	(433.77)
PSM (2N)	2002.45	13468.73	391.61	4900.29**	434.64	1884.20***	322.44	1150.67***
	(2776.67)	(9448.38)	(485.59)	(2197.70)	(388.21)	(699.52)	(226.68)	(408.83)
PSM (1N)	2195.89	14977.77	-11.70	6043.38***	828.06**	1897.64***	391.68 *	1253.23***
	(3142.01)	(9956.69)	(764.74)	(1920.88)	(386.20)	(695.74)	(234.23)	(408.98)

Notes: This table displays the average treatment effect (ATT) on alternative livelihood outcomes. NNM refers to the DID nearest neighbor estimator using 2 (1) matched observations as controls. PSM refers to the DID propensity score matching estimator using 2 (1) matched observations as controls. ***, ** and * indicate that the estimated coefficients are statistically significant at the 1%, 5%, and 10% levels, respectively. Standard errors are given in parentheses.

Table 2.8: Short- and long-term effects on cattle production and agrobiodiversity

Estimator	Cattle gross income		Cattle stocking rate		Crop richness	
	Short run	Long run	Short run	Long run	Short run	Long run
NNM (2X)	-3589.74	21974.23**	0.26*	0.81***	0.08**	0.09***
	(5614.88)	(10458.55)	(0.21)	(0.29)	(0.03)	(0.03)
NNM (1X)	-5726.41	18888.24	0.35*	0.77***	0.09**	0.10***
	(5849.01)	(11658.40)	(0.19)	(0.27)	(0.04)	(0.03)
PSM (2N)	-6179.31	26136.31***	0.35*	0.47*	0.07*	0.08***
	(4964.67)	(8049.49)	(0.18)	(0.25)	(0.04)	(0.03)
PSM (1N)	-3255.55	27556.42***	0.38*	0.25	0.07*	0.11***
	(5856.31)	(8357.34)	(0.20)	(0.24)	(0.04)	(0.03)

Notes: This table displays the average treatment effect (ATT) on cattle production and agrobiodiversity. NNM refers to the DID nearest neighbor estimator using 2 (1) matched observations as controls. PSM refers to the DID propensity score matching estimator using 2 (1) matched observations as controls. ***, ** and * indicate that the estimated coefficients are statistically significant at the 1%, 5%, and 10% levels, respectively. Standard errors are given in parentheses.

Table 2.9: Short- and long-term effects on physical assets

Estimator	Refrigeration equipment		Agricultural equipment		Motorized vehicles	
	Short run	Long run	Short run	Long run	Short run	Long run
NNM (2X)	194.59	1059.10**	320.47	133.21	1892.86	3429.12
	(390.24)	(480.49)	(526.14)	(502.42)	(1920.99)	(2319.32)
NNM (1X)	-25.37	1097.27**	30.84	-100.25	2796.88	3523.40
	(469.50)	(512.34)	(556.35)	(502.31)	(1935.96)	(2721.13)
PSM (2N)	215.27	1191.67***	406.36	371.08	1342.70	1571.32
	(325.12)	(395.38)	(533.65)	(416.91)	(1842.10)	(3138.64)
PSM (1N)	217.17	1246.08***	532.95	651.32	2186.71	1651.96
	(345.15)	(393.34)	(600.89)	(462.70)	(1859.30)	(3486.67)

Notes: This table displays the average treatment effect (ATT) on physical assets value. NNM refers to the DID nearest neighbor estimator using 2 (1) matched observations as controls. PSM refers to the DID propensity score matching estimator using 2 (1) matched observations as controls. ***, ** and * indicate that the estimated coefficients are statistically significant at the 1%, 5%, and 10% levels, respectively. Standard errors are given in parentheses.

Table 2.10: Short- and long-term effects on gross income

Estimator	Off farm income		Salary + Business income		Total income	
	Short run	Long run	Short run	Long run	Short run	Long run
NNM (2X)	96.60	-604.33	-1436.67**	-1254.58	-7816.63	40279.07**
	(5399.18)	(3296.25)	(557.98)	(3103.65)	(13557.19)	(17096.00)
NNM (1X)	-2240.59	363.80	-1229.07**	-1163.37	-20810.26	36737.23**
	(6100.79)	(3320.66)	(582.50)	(3100.28)	(17612.01)	(18531.58)
PSM (2N)	1406.47	-576.09	-1510.50***	-2182.00	-5927.572	50886.33***
	(4742.16)	(3906.19)	(554.17)	(3067.00)	(12337.14)	(13761.76)
PSM (1N)	2405.97	1519.51	-1593.89***	-107.64	3311.78	55908.98***
	(5120.92)	(3825.88)	(569.55)	(3020.74)	(11174.23)	(13873.16)

Notes: This table displays the average treatment effect (ATT) on income. NNM refers to the DID nearest neighbor estimator using 2 (1) matched observations as controls. PSM refers to the DID propensity score matching estimator using 2 (1) matched observations as controls. ***, ** and * indicate that the estimated coefficients are statistically significant at the 1%, 5%, and 10% levels, respectively. Standard errors are given in parentheses.

Chapter 3

Experimental auctions to set forest conservation payments in the Brazilian Amazon

Abstract

Payments for Environmental Services (PES) have become a popular forest conservation tool in recent years, with numerous new schemes springing up around the world, particularly in developing countries. However, more often than not, the effectiveness of PES contracts is minimal, because conservation buyers know less than landowners do about the costs of contractual compliance. Auction mechanisms can be used to make *ex ante* estimates of the payments needed to save the forests. This study reports results from an experimental auction which uses the Becker-DeGroot-Marschak (BDM) mechanism to estimate Brazilian Amazon forestland owners' willingness to accept (WTA) PES contracts at different forest conservation thresholds. First, we found that the average WTA is low, relative to the social cost of carbon. As one might expect, we also found evidence that farmers tend to bid higher in a PES auction that offers contracts with the most difficult to achieve environmental targets. Moreover, using survey data collected from the participants, we found that certain types of farmers consistently bid higher than others, regardless of the contract offered. We also found that this type of farmer places greater value on flexibility in PES contracts. Although further research from the field is warranted to determine which type of PES contract is ultimately most cost-effective, these results suggest that measuring WTA provides a key input for shaping pricing policy and guiding the magnitude and targeting of forest conservation PES contracts in the Amazon.

Résumé

Au cours des dernières années, les paiements pour services environnementaux (PSE) sont devenus un instrument de conservation des forêts très populaire, avec de nombreux nouveaux programmes mis en place dans le monde entier, en particulier dans les pays en développement. Cependant, le plus souvent, l'efficacité des contrats de PSE est plus faible que ce qui était escompté, car les acheteurs de services environnementaux (les pouvoirs publics typiquement) ignorent le coût réel pour les propriétaires fonciers de respecter les termes du contrat de conservation des forêts. Des mécanismes d'enchères peuvent être utilisés pour estimer *ex ante* le montant des PES nécessaires pour sauver les forêts. Cette étude restitue les résultats d'une enchère expérimentale qui utilise le mécanisme Becker-DeGroot-Marschak (BDM) pour révéler le consentement à recevoir (CAR) d'un échantillon de propriétaires de forêts en Amazonie brésilienne, pour des contrats de PSE incluant différents objectifs de conservation. Les principaux résultats suggèrent que le CAR moyen est faible par rapport au coût social du carbone, quel que soit le type de contrat de PSE considéré. De plus, comme on pouvait s'y attendre, les agriculteurs ont tendance à faire des offres plus élevées dans les enchères de PSE proposant des contrats dont les objectifs environnementaux sont plus difficiles à atteindre. De plus, en utilisant les données d'enquête recueillies auprès des participants, nous avons montré que certains types d'agriculteurs ont un CAR systématiquement plus élevées que les autres, quel que soit le contrat proposé. Nous avons également constaté que ce type d'agriculteurs accorde une plus grande valeur aux contrats de PSE intégrant un cahier des charges plus flexible. Bien que d'autres recherches sur le terrain soient nécessaires pour déterminer quel type de contrat de PSE est finalement le plus efficace, ces résultats suggèrent que la mesure du CAR est un élément clé pour l'élaboration des politiques de compensation, la détermination du montant et la définition du ciblage des contrats de PSE pour la conservation des forêts en Amazonie.

3.1 Introduction

Payments for environmental services (PES) use direct incentives to improve the environmental impacts of farmers' conservation decisions (Ferraro, 2008). In recent years, the expected effects of using financial incentives to achieve conservation objectives has played an important role in the increase of the popularity of PES-like programs (Jack, Leimona, and Ferraro, 2009). Despite the incentives they offer, however, forest conservation PES programs may ultimately not be effective in curbing deforestation rates, because PES contractual relationships are subject to asymmetric information between landowners and conservation buyers (Ferraro, 2008). In PES schemes where all landowners receive the same payment regardless of their particular WTA for contractual compliance (i.e., the minimum price they require for forest conservation), farmers who do not need to change their behavior to comply with the contract to receive payment (i.e., farmers who would preserve their forest anyway) are the most likely to enter a PES program (Jack and Jayachandran, 2019). As a result, the PES program may end up paying some farmers for doing nothing different from what they would have done in the absence of any payment. In such cases, the additionality of the program may be quite limited. On the other hand, payments below a farmer's minimum reservation price would reduce participation in the program and/or increase the probability that the contract would eventually be terminated. Thus, in an optimal PES contract, payments would be adjusted to each farmer's WTA for forest conservation. As such, WTA estimates can provide important information on the design of the most efficient PES programs. However, measuring demand for PES in developing countries is difficult because revealed-preference tools, such as hedonic valuation or compensating differentials, rely on strong assumptions of complete markets (Berry, Fischer, and Guiteras, 2020; Greenstone and Jack, 2015).

In recent decades, experimental auctions have become a popular method to estimate individuals' private values in order to provide private and public goods (Corrigan et al., 2009; Shogren, 2005). In particular, a number of studies have used experimental auctions in laboratory settings to explore the behavior underlying environmental public schemes (Cherry, Kroll, and Shogren, 2007; Shogren, Parkhurst, and Hudson, 2010). Although environmental valuation is an increasingly invested-in field of research (Shogren, 2005), field experiments are still very rare. To our knowledge, only a few experimental auctions have been run in low-income countries in the context of PES (Jack, Leimona, and Ferraro, 2009; Jack, 2013; Jindal et al., 2013; Narloch, Drucker, and Pascual, 2017; Khalumba et al., 2014). Moreover, none of them provide estimates of landowners' WTA payments for avoided deforestation. This study presents the results of an experimental auction protocol run in the field among a sample of 302 landowners living in the Brazilian Amazon, a region that leads the world in humid tropical primary forest

loss due to fires and clear-cutting driven mainly by agricultural expansion.

We implemented an experimental auction using the Becker-DeGroot-Marschak (BDM) mechanism as an incentive-compatible procedure for eliciting landowners' reservation prices for forest conservation (Becker, DeGroot, and Marschak, 1964). In BDM, an individual states her bid for an item (a PES contract in our case). Then a random price is drawn. If the random price is less than her bid, she does not sign the PES contract, since the payment offered is insufficient. If the random price is greater than or equal to her bid, she signs the PES contract but receives the random price draw rather than her stated bid. Because the subject's stated WTA affects only whether she signs the contract and not the price she receives, BDM is incentive-compatible, which means that the subject's dominant strategy is to bid her true minimum WTA.

In order to be able to cross-validate our estimates, as well as to better take into account the reality on the ground, we tested two different PES contracts. In practice, each participant was offered the option to bid for the two contracts, but only one of the two, chosen at random, was finally implemented in real life if the participant won the BDM lottery. In both PES contracts (type A and type B) landowners were offered a one-time payment in compensation for preserving their forest for a one-year period. While both contract types have as their common goal the preservation of the forest located on the farmhold, type A contracts include a zero deforestation target, which means that the farmer chooses to receive a payment for the full conservation of her forest cover. If any deforestation is observed, she receives zero compensation. In type B contracts, a penalty coefficient is applied to the payments when any deforestation is observed, and the full payment is received if no deforestation is observed. In summary, contract A has significant forest conservation potential but is not flexible, making the risk of default higher, while contract B, which includes several thresholds, has, in theory, lower conservation potential but is associated to a lower risk of default. We also ran a survey from the same sample in order to collect data on participants' land use, agricultural activities, and economic constraints, with the aim of identifying what observable characteristics help predict landholders' bids.

This study makes three contributions. First, we measure the private costs of forest conservation in a population that clears forest every year in order to extend their pastures for cattle and to cultivate staple crops for self-sustaining purposes. Precise revealed-preference WTA data is key input for the design of PES in areas where landowners' livelihoods depend on production systems that are significant drivers of deforestation. This paper adds to a small literature measuring the private costs of forest conservation through directly offering households customized PES contracts.

Second, we explore the impact of introducing some flexibility into PES con-

tracts, a feature that is likely to improve the cost-effectiveness of PES programs. The question of cost-effectiveness goes beyond the scope of this paper, but it is nevertheless important to verify beforehand that the participants in an environmental auction are able to understand the difference between contract A (zero-deforestation) and contract B (threshold) and that their revealed WTA reflects their understanding of the consequences of their choice for either contract. Our results of the elicited WTA for forest conservation for each type of contract are consistent with theoretical expectations, with the WTA being higher, on average, for the more restrictive (zero-deforestation) contract.

Third, we address the issue of hidden information of landowners by gathering information on observable landowner attributes likely to be correlated with opportunity costs. Indeed, if it is possible to highlight strong correlations, eligibility requirements for receiving a given contract type and price could be implemented (Ferraro, 2008). This could be a key input for the design of PES forest conservation contracts in the Amazon. By combining revealed-preference WTA data with household survey variables, we find that a number of household characteristics, farm characteristics, and socioeconomic variables are likely to drive the way participants bid their minimum WTA for forest conservation. Moreover, we find that two fairly precise farmer typologies emerge from the experiment: on the one hand, low-income households practicing traditional (slash-and-burn) agriculture based on fallow land and subsistence farming, and on the other hand, wealthier households whose agricultural activities rely on highly deforestation-dependent land uses (e.g., extensive cattle-ranching).

The rest of the paper proceeds as follows. Section 3.2 presents the main results of the literature on environmental valuation of forest conservation. Section 3.3 describes the experimental setting and design. Section 3.4 presents the results on revealed-preference WTA data. Section 3.5 presents bid determinants and farmer typology. Section 3.6 discusses a number of implications for the design of future PES programs and perspectives for further research.

3.2 Assessing the private costs of forest conservation

A major challenge when designing a PES scheme is to estimate the true costs of environmental service provision. Services providers know more about these costs than do service buyers (Ferraro, 2008). When it comes to forest conservation PES schemes, landowners must forego the monetary (and non-monetary) value generated by alternative land uses in order to conserve their forests (e.g., livestock, agriculture). Such value frequently relies on unobservable (or difficult-to-observe)

economic agent characteristics such as time and risk preferences, cultural and option values, and subjective beliefs (Jack, Leimona, and Ferraro, 2009; Parks, 1995). To estimate the true costs of environmental service provision, a diversity of methods has been proposed in the environmental economics literature.

Modeling approaches that rely on observables can provide insight into the design of conservation payment schemes and explore the sensitivity of such costs under different socioeconomic scenarios. However, when heterogeneities in private costs are driven by unobservables, these types of estimates may be inaccurate (Ferraro, 2008). Some authors have developed models to set up forest conservation payments in the Brazilian context. Engel et al. (2015) develop a model of optimal land use change for farmers in the State of Mato Grosso, where forest conservation payments could either be indexed to returns from soybean production or to the carbon market. The model estimates that an average payment that ranges between 2,339 and 2,583 dollars per hectare of forest is needed to avoid deforestation for a 30-year period. However, such estimates do not take into account space and time heterogeneities. Building upon the model of conservation payments by Engel et al. (2015), Palmer, Taschini, and Laing (2017) estimate the returns from three alternative land uses (cattle, corn, and coffee) in the State of Acre in order to compute the opportunity cost of avoiding deforestation for the "average" landowner within each municipality. Their model is used to simulate the level of incentive needed to ensure that the landowner continues to postpone the decision to switch from forest to agriculture for a 5-year period. The authors find that an average payment ranging from 9.17 to 9.94 dollars per hectare of forest is needed to avoid deforestation for 5 years and that in the majority of municipalities cattle pasture generates the highest returns. One caveat of modeling approaches is that rural areas of the Brazilian Amazon are often imperfectly connected to markets, and much of the agricultural production associated with deforestation is destined to household subsistence, so making accurate estimates from observable characteristics is a major challenge. A second caveat is that, although modeling approaches estimate opportunity costs as forgone profits from alternative land uses, landowners' WTA for PES contracts is often composed of much more than just loss of earnings (Jack, Leimona, and Ferraro, 2009).

Another strand of literature proposes stated preference methods, such as choice experiments and contingent valuation (CV), as an alternative approach to capture hard-to-measure components of landowners' opportunity costs. There have been several stated-preference studies to investigate potential designs of PES contracts in developing countries (Kaczan, Swallow et al., 2013; Costedoat et al., 2016; Raes, Speelman, and Aguirre, 2017; Mohebalian and Aguilar, 2018; Poudyal et al., 2018). To our knowledge, only one study has implemented a stated preference assessment to estimate WTA for forest conservation in the Brazilian Amazon context. In the

State of Amazonas, Kim-Bakkegaard et al. (2017) performed two CVs with different framings applied to obtain farmers' WTA for forest conservation contracts. Their findings show that auction framing resulted in lower mean WTA than standard framing - with the average WTA ranging from 162 Brazilian reals (BRL) per hectare and month for the auction framing and 362 BRL/hectare/month for the standard framing. The authors also compared WTA results from the CV with household survey-based agricultural profit measures and found that estimated agricultural profits often exceed the declared WTA. One caveat of the stated preference methods is that they rely on hypothetical markets to obtain opportunity cost values. Because respondents have no financial incentive to reveal private information or invest any cognitive effort in estimating their opportunity costs in such hypothetical markets, estimations may be biased (Jack, Leimona, and Ferraro, 2009).

In contrast to stated preference methods, revealed-preference approaches to economic valuation use data on observed market decisions to capture components of decision makers' preferences (Jack, Leimona, and Ferraro, 2009; Adamowicz et al., 1997). The only problem is that markets don't exist for most environmental services. One method to induce farmers to reveal their true costs could be through real auctions that create temporary markets for conservation contracts (Latacz-Lohmann and Schilizzi, 2005). Procurement auctions create incentives for landholders to reveal information about their true WTA for providing environmental services (Jack, 2013). Combining PES initiatives with procurement auctions is intended to facilitate program targeting to the landholders with the lowest opportunity costs (Ferraro, 2008). Auctions are intended to incentivize bidders to reduce rent seeking and submit bids that are closer to the true costs of their (potential) service provision (Wünscher and Wunder, 2017).

In uniform procurement auctions, such as Vickrey[1] auctions and nth-price auctions, farmers are paid the same amount, usually the second lowest accepted bid for a given budget (Berry, Fischer, and Guiteras, 2020). By contrast, in discriminatory procurement auctions, such as the BDM, participants are paid according to their bids, where a lower bid increases the probability of being awarded (Lundberg et al., 2018). Lately we have seen various field experimental auctions in low-income countries; however, to the best of our knowledge, there have been no applications in the Brazilian Amazon or in the context of deforestation.

In Indonesia, Jack, Leimona, and Ferraro (2009) conducted a multiple round Vickrey auction to reveal the opportunity costs of coffee farmers supplying environmental services by adopting soil-erosion control measures. The study results pointed out that participants used test rounds to learn about the temporary mar-

[1] A Vickrey auction is a type of sealed-bid auction where participants submit their bids without knowing the bids of others. The lowest bidder wins, but the price paid is the second lowest bid. This type of auction gives bidders an incentive to bid their true value.

ket. Data on contract compliance suggests that multiple rounds of bidding improved auction performance over implementation with binding bids in the first round. In Malawi, Jack (2013) compared the performance of two allocation mechanisms: a Vickrey auction and a posted offer market, on farmers' provision of environmental services. The WTA results were very different for the two mechanisms, which are, in theory, strategically equivalent. For the same clearing price (86 USD), there was a 38% enrollment rate in the auction treatment group, while 99.5% of landholders in the posted bid treatment group accepted a tree planting contract at that price. The impact analyses indicate that significantly more trees survived via the contract allocated by the auction mechanism. The auction treatment was estimated to provide a 30% cost savings per surviving tree. Altogether, the results suggest that the auction mechanism achieved better landowner targeting.

In Peru and Bolivia, Narloch, Drucker, and Pascual (2011, 2017) examined the effectiveness of collective PES auctions for agrobiodiversity conservation. The bids differed significantly between Bolivia and Peru and, depending on the chosen criteria, the auctions yielded different results that suggest that group payment increases accountability among farmers and reduces transaction costs. In Tanzania, Jindal et al. (2013) also used a Vickrey auction to elicit farmers' WTA for planting trees in their fields. Data on the tree survival rate suggest that performance was not related to household poverty status or farmers' WTA. The authors ran simulations using the data obtained from the auctions and the household survey data, and the results show large trade-offs between the goal of cost effectiveness and that of maximizing the participation of poor households. In a pilot study in Kenya, Khalumba et al. (2014) performed a non-incentive-compatible discriminatory price auction with repeated rounds to elicit WTA for tree planting. The authors compared outcomes with a control group that enrolled in a fixed payment scheme, but the trials were too small to deliver conclusive statistical evidence regarding seedling survival rates between both treatments.

Our study differs from previous experimental PES auctions in developing countries in that we apply a BDM design, which is an incentive-compatible mechanism that allows for discriminatory payments. The experimental auction literature has highlighted three major appealing features of the BDM mechanism: i) accuracy in revealing each respondent's WTA; ii) random variation in treatment status conditional on the WTA; and iii) random variation in the price paid, conditional on the WTA[2]. In the present study we will only make use of the first point in order to estimate the demand for PES contracts. However, we intend to explore points

[2]This means that two participants bidding the exact same value can have different treatment status and payoffs and that this difference will be determined purely by chance. This feature allows for estimating heterogeneous treatment

ii and iii in future work.

3.3 Experimental setting and design

3.3.1 Study zone and village selection

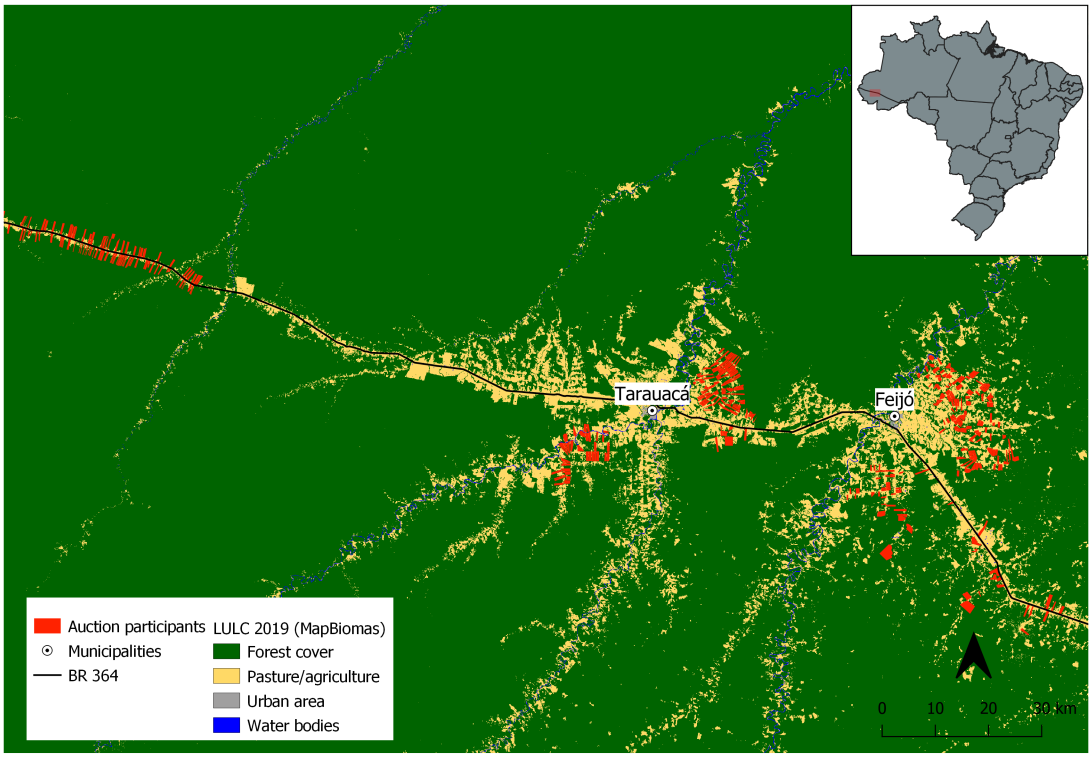
The experiment was carried out in the western region of the Brazilian Amazon, in the state of Acre. Our study zone is located in the Taraucá-Evira watershed, and the participating households are concentrated in the area along highway BR-364, which is located within the municipalities of Tarauacá and Feijó (Figure 3.1). This territory is largely occupied by private smallholders who own an average area of less than 100 hectares, mostly covered by forests (57 percent, on average) and pastures (42 percent, on average), according to data from Mapbiomas[3]. The two municipalities are located in a frontier zone of human occupation deforestation expansion.

In our study region, cattle livestock production is integrated with subsistence slash-and-burn rotational crops (maize, manioc, banana, vegetables). There is a marked dry and wet season and one agricultural cycle per year. To establish new annual crops and expand pasture areas, forest area is cleared and burned during the dry season. Clearing occurs mostly between May and July and burning between August and October. There are two types of complementary clearing practices that are employed in the region: i) Primary forest is cleared using machetes (for small vegetation) and chainsaws (for trees with greater diameters), usually in the beginning of the dry season (May) so that the larger vegetation has enough time to dry in order to be set on fire during the fire season; ii) Secondary vegetation has smaller diameters and is mainly cleared using machetes. The clearing of secondary vegetation occurs between June and July. Once the cut vegetation has time to dry, controlled fires are then set to clear the area for planting. Cleared land generally remains under cultivation for 1–2 years until the soil nutrients are worn-out. After this, the land is either sown with grass and converted into pasture for cattle or is left fallow for 1–4 years, during which time young secondary forest will establish and regenerate soil nutrients for the next agricultural cycle. Some farmers use the same cleared area for up to 4 cycles of slash-and-burn rotational crops, followed by an uncultivated period.

To select study villages, we used Global Forest Change (GFC) deforestation data (Hansen et al., 2013) to identify settlements in the study zone with recent deforestation activity. We also chose a region with relatively easy access (roads and navigable rivers) and a high enrollment of farmholds in the Brazilian rural en-

[3]MapBiomas Project- Collection 5 of the Annual Series of Land Use and Land Cover Maps of Brazil, accessed on September 30, 2020 through the link: https://mapbiomas.org/en/download

Figure 3.1: Map of the farms participating in the auction experiment



vironmental registry (designated by the acronym CAR in Portuguese) to facilitate the monitoring of the compliance with the contract specifications. We selected 6 villages for the study and then conducted door-to-door visits to the eligible farms to present the program and enroll the farmers interested in participating. To be eligible to participate in the pilot, a farmer had to: i) own a rural property with less than 500 hectares, ii) be registered in the CAR, iii) exercise a productive activity such as livestock or agriculture on the property, and iv) have been actively clearing forest on their land in recent years (characterized by the detection of deforestation activities at least twice between 2017 and 2020).

3.3.2 Data collection and experimental design

The experiment was carried out in three stages: the enrollment of eligible voluntary families (May-November 2021), the pretest and design refinement phase of the PES contracts (August 2021), and the data collection (September-December 2021), which included a survey and the BDM itself. For logistical reasons, the enrollment and data collection phases occurred sequentially in each selected village.

Enrollment of volunteer families

The preliminary phase of the experiment, namely the enrollment of participants, took place between May and November 2021. In each of the 6 selected villages, with each eligible household that the team was able to reach, we conducted an initial individual meeting, during which we provided a description of the objectives of the PES program and the course of each step for those who agreed in principle to participate. In particular, we clarified that we were registering the families interested in participating in the project but that only a part of them would be randomly selected to participate in the auction. Indeed, the BDM presented in this paper is embedded in a randomized controlled trial (RCT), which will end in December 2022 and should allow the impact of the PES contracts offered to be assessed. The RCT includes one control group and two treatment groups of about 150 families each[4]. People in the control group were not offered to participate in the auction and thus did not have the opportunity to sign any contract. In this paper, we focus on the 302 families who were assigned to a treatment group and therefore participated in the auction. Figure 3.2 shows a partial map of the farms assigned to the three RCT groups.

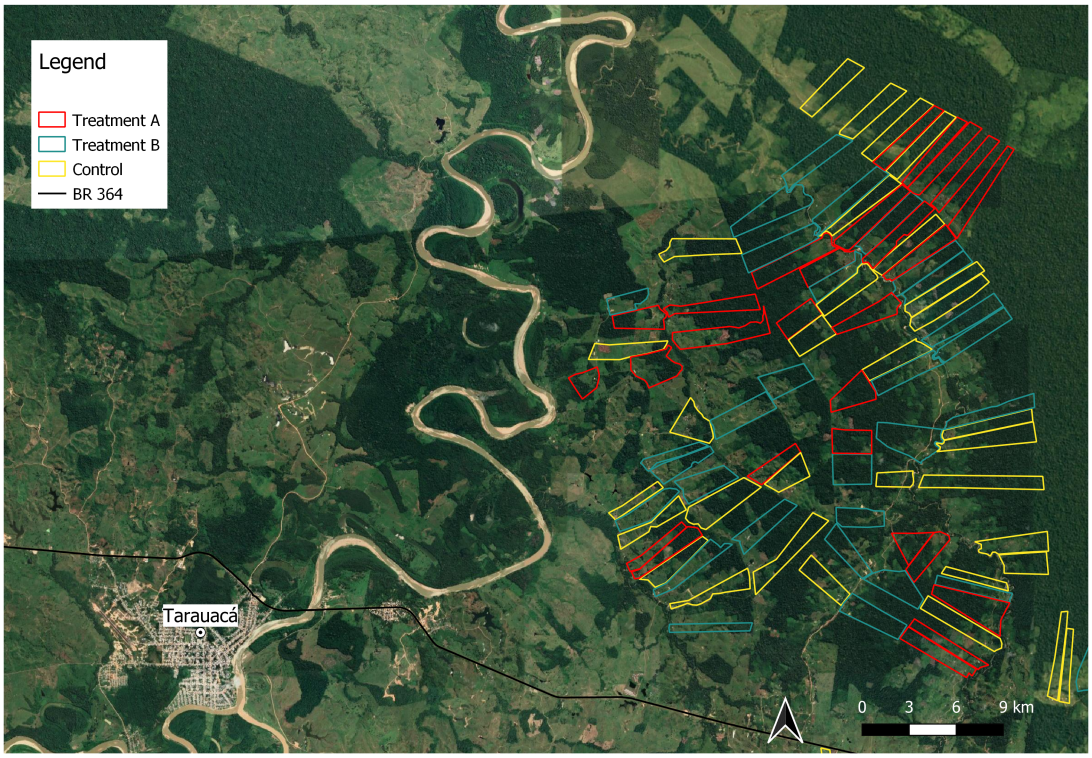
During the face-to-face interviews, we also pointed out that not all families selected for the auction phase would receive payments at the end. We explained that only those participants winning the auction would actually sign a PES contract for forest conservation. Each willing participant was asked to sign the Free Prior and Informed Consent (FPIC) form stating that they understood that their participation in the pilot program did not guarantee that they would receive a conditional payment at the end, as the pilot included two randomization phases (one as part of the RCT and one as part of the auction). Additionally, it was specified that even if they did not ultimately sign a PES contract, deforestation data on their farm would still be collected as part of the study. We instructed the farmers that we would contact them in the following weeks to inform them if they were selected for the second phase of the pilot and schedule a second visit to perform the auction activity. Farmers were encouraged to discuss with their families what they were willing to accept for the PES contract.

Finally, the last part of the interview was devoted to the verification or, if necessary, the registration of the boundaries of the farm in the rural registry (CAR). Since the CAR contained a lot of geometry errors (overlaps, displacements, etc), the field team corrected the property boundaries of willing project participants along with their help. The team was trained to use a GIS[5] software containing the

[4]In practice, we used stratified randomization based on geographical location (village), in order to obtain balanced groups in each of the areas visited during the enrollment phase.

[5]Each group of two enumerators had at least one member with previous experience using GIS software.

Figure 3.2: Map of treatment groups (partial picture)



boundaries overlaid with high-resolution image mosaics. The map of the enrolled farms is displayed in (Figure 3.1).

Pretesting and PES contract design

In August 2021, a draft version of the protocol was first tested in the field with twenty volunteer families. The contracts offered included different conservation thresholds and terms that were often misunderstood by participants. Several adjustments were therefore made at the end of this pretest and the BDM was reframed accordingly. The final experimental protocol was then registered at the American Economic Association's registry for randomized controlled trials, under RCT ID AEARCTR-0008193. The experimental protocol was implemented in the field between September and December 2021. Participants were offered two types of PES contracts that included different forest conservation thresholds: contract A with a zero deforestation target and contract B with a threshold penalty. The two contracts (in Portuguese) can be found in the Appendix.

With contract A, the holder receives a payment conditional on the full conser-

vation of her forest cover for a one year period. If any deforestation is detected through satellite imagery, she receives zero. Farmers may generally decide to exit from PES contracts. This can occur, for instance, if there is a boom in commodity prices that makes contract breach more profitable. One can expect that, with contract A, if there is an increase in commodity prices, landholders are likely to exit the contract.

With contract B, the farmer receives a penalty coefficient applied to the total payment when deforestation is observed above certain thresholds[6]: i) a 20% penalty will be applied to the total payment if a deforestation patch between 0.1 to 0.5 hectares is detected through satellite imagery; ii) a 50% penalty will be applied to the total payment if a deforestation patch between 0.6 to 1 hectares is detected; and iii) if a deforestation patch greater than 1 hectare is detected, she receives zero. The full payment is received if no deforestation is observed. If there is a boom in commodity, with contract B, farmers might not opt out but rather increase their deforestation levels (at the cost of a larger penalty).

Household survey and BDM auction

We implemented a simplified version of BDM, in which a surveyor presents an individual participant with a range of conditional payments in exchange for not cutting down the forest for a period of twelve months. Since the BDM mechanism is incentive-compatible, the bidding behavior should reveal the participant's true opportunity cost, i.e., the money they would lose by suspending activities that drive deforestation for one year, slash-and-burn agriculture and raising livestock, in the present case. Opportunity costs are also expected to cover other private costs the landholder incurs as a result of entering the program, such as monitoring the forests and changing her social status, minus the benefits she's getting through warm glow and social payoffs. Individual opportunity costs are thus expected to be highly heterogeneous, whether because individuals have different farming activities and constraints or because they have different time, risk, and social preferences and different market price expectations.

In practice, the auction was held face-to-face with each participant individually. Landowners who were randomly chosen to participate in the BDM were contacted by phone, and a second visit was scheduled. During this meeting, the surveyors made a detailed presentation of the two types of contracts and explained the BDM procedure. Participants were informed that they were going to bid twice, i.e., they were going to make an offer for each of the two contracts, but that only one of these bids would then be considered for the BDM draw. During the BDM phase, we collected testimonials in which most participants explained the

[6]The deforestation thresholds were defined based on previous analyses of average deforestation rates on eligible farmholds (about 1.5 hectares per year).

reasoning behind their bids with an example of how much money they would lose by not expanding their pasture or how much it would cost to buy corn or cassava from their neighbors. Before beginning the bidding exercise, each participant was administered a survey covering a number of socioeconomic characteristics such as income, assets, and land-use. The survey questionnaire (in Portuguese) can be found in the Appendix.

At the end of the survey, the surveyors collected the bids on each of the two contracts, each time following the same procedure. After the participant confirmed her final bid for each of the two contracts[7], the surveyor revealed what kind of contract was randomly assigned to her[8] and the participant drew a token from the urn corresponding to this contract. If the price written on the token was lower than the price requested by the participant, she was not offered a contract but received an in-kind compensation for her time spent with the surveyors[9]. If the price written on the token was greater than or equal to the price requested by the participant, she was offered a PES contract for the amount written on the token. The tokens in urn A ranged from 4500 to 6100 BRL, in increments of 100 BRL, while in urn B tokens ranged from 3900 to 5500, also in increments of 100 BRL.[10] The auction script can be found in the Appendix.

Because the landowner's bid affects only whether she signs the contract and not the value of the contract, BDM is incentive-compatible: it would not be a good strategy for the landowner to make a bid above the true opportunity cost, because if the amount drawn from the box is lower than the bid but higher or equal to the true opportunity cost, she will lose the opportunity to be offered a suitable contract. Therefore, the player's dominant strategy is to bid her true minimum WTA. Note that incentive-compatibility no longer holds when participants play multiple rounds, because they can learn about the frequency distribution of prices. In our research design, however, although participants played two rounds, the random price was not revealed before the end of the bidding exercise. Therefore, participants didn't get to learn about the interval on which the draw was made. They were also informed that they would only have one chance to bid for each contract and that they could not change their bid after drawing the token from the urn.

[7]The order in which contracts were offered to participants in the BDM was random to avoid an order effect.

[8]This randomization was done beforehand in order to have balanced groups. Neither the enumerators or the participants knew which contract had been drawn before playing the BDM.

[9]Participants were provided in-kind food bundles worth 80 BRL as compensation for their time spent participating in the trial.

[10]The price range shown on the tokens placed in the urn was not known to the players.

3.3.3 Descriptive statistics of respondent characteristics

Key characteristics of participants are given in Table 3.1, which breaks the full sample into three groups: i) landholders who answered that growing crops was their main source of income (from here on we will refer to them as *crop farmers*); ii) landholders who answered that cattle livestock was their main source of income (we will thus refer to them subsequently as *cattle ranchers* and iii) landholders, whose main source of income was other livelihood activities (we'll now refer to them as *diversified farmers*). Note that the three groups have similar livelihood activities, such as livestock farming and agriculture. What differentiates them is which activity represents the largest share of their income. The sample includes a total number of 302 participants: 158 crop farmers, 132 cattle ranchers, and 12 diversified farmers.

The first column of Table 3.1 ('Full sample') shows that the average respondent is most often a man around 50 who has spent less than 4 years in school and is the head of a family of 6 people, including 4 or 5 who work on the farm. He has been living in the village for 20 years and his annual gross income is around 45,000 BRL. He has about sixty hectares, the majority of which are covered with mature forest. Over the past three years, he has cut one and a half hectares of forest per year.

Comparing Columns 2 ('Crop farmers'), 3 ('Cattle ranchers), and 4 ('Diversified farmers') highlights some interesting differences between the three groups. Almost a quarter of the sample is made up of women, but they are more present in the crop farmers subgroup (28 percent). Diversified farmers are the most highly educated, followed by cattle ranchers. Crop farmers have larger families (7 members, on average) and the majority of family members are working on-farm (5 members, on average). Diversified farmers have the highest annual total gross income (61,590 BRL on average), followed by cattle ranchers (51,455 BRL) and crop farmers (39,387 BRL). Cattle ranchers have larger farmholds (68 hectares) and less mature forest area (35 hectares) than crop farmers (55 hectares of total area and 38 hectares of forest area). This is because cattle ranching is more extensive and requires the clearing of very large areas. Crop farmers also have larger fallow vegetation areas (4.8 hectares) than cattle ranchers do (3.6 hectares), suggesting that crop farmers have traditional slash and burn systems where a portion of the land is typically kept fallow. Cattle ranchers have the highest rate of deforestation (1.55 hectares per year). Note that the deforestation rate of crop farmers includes clearing of fallow areas that are left to regrow in the next agricultural season.

Table 3.1: Descriptive statistics of participant characteristics

	Full sample			Crop farmers			Cattle ranchers			Diversified farmers		
Variable	Obs	Mean	Std. Dev.	Obs	Mean	Std. Dev.	Obs	Mean	Std. Dev.	Obs	Mean	Std. Dev.
Age	302	48.56	12.96	158	47.42	11.98	132	49.78	13.82	12	50.00	15.39
Gender	302	0.23	0.42	158	0.28	0.45	132	0.17	0.38	12	0.25	0.45
Education	302	3.69	4.18	158	3.03	3.64	132	4.17	4.38	12	7.08	6.27
Family size	302	5.99	3.93	158	7.07	4.25	132	4.73	3.16	12	5.50	3.18
Family members working on farm	302	4.62	2.91	158	5.33	3.25	132	3.81	2.31	12	4.25	1.76
Family members working off farm	302	1.43	1.52	158	1.62	1.60	132	1.19	1.30	12	1.58	2.27
Village years	302	20.79	12.55	158	21.76	11.86	132	19.89	13.24	12	17.89	13.53
Total income (BRL)	302	45543.93	39425.43	158	39387.03	39595.28	132	51454.74	37786.98	12	61590.75	43673.36
Crop income (BRL)	302	9799.54	26243.58	158	15920.10	34313.61	132	3299.88	8705.27	12	708.33	1814.82
Cattle income (BRL)	302	13234.40	23943.24	158	3257.24	6777.87	132	24656.41	30929.75	12	18958.33	24645.17
Other livestock income (BRL)	302	3235.63	7418.47	158	2055.98	5988.79	132	3998.07	7200.02	12	10380.83	17146.17
Forest income (BRL)	302	395.70	1831.32	158	439.84	2257.39	132	374.29	1257.95	12	50.00	173.21
Business income (BRL)	302	889.40	4478.56	158	1146.42	5565.22	132	634.58	2956.92	12	308.33	757.34
Retirement (BRL)	302	8334.67	11818.64	158	7354.56	11182.48	132	9397.36	12326.28	12	9550.00	14106.38
Government transfer (BRL)	302	3674.05	3925.93	158	4520.82	3925.65	132	2689.97	3583.29	12	3349.83	5182.08
Agricultural subsidy (BRL)	302	83.53	537.06	158	71.03	469.63	132	90.92	607.89	12	166.75	577.32
Other source of income (BRL)	302	5897.01	10371.57	158	4621.04	7785.65	132	6313.27	10235.54	12	18118.33	25342.77
Total area (hectares)	299	61.44	40.05	157	54.96	31.28	130	67.60	47.81	12	79.53	36.89
Mature forest area (hectares)	291	37.47	32.38	153	38.03	28.48	126	35.31	36.57	12	53.13	30.27
Fallow vegetation area (hectares)	294	4.23	5.83	154	4.84	6.14	128	3.60	5.52	12	3.04	4.27
Crop area (hectares)	297	2.30	2.45	155	2.43	2.40	130	2.12	2.56	12	2.63	1.73
Pasture area (hectares)	295	15.91	16.59	155	8.37	8.58	128	25.01	19.47	12	16.25	13.56
Other LULC area (hectares)	295	0.79	1.29	155	0.65	1.21	128	0.93	1.38	12	1.30	1.24
2018-2021 average deforestation	282	1.43	0.91	148	1.35	0.77	123	1.55	1.07	11	1.05	0.72
Cattle herd size	302	25.18	32.03	158	9.23	12.57	132	44.2197	37.7493	12	25.67	29.03

3.4 Elicited WTA forest conservation contracts

3.4.1 Descriptive statistics of auction bids

According to the principle of free, prior, and informed consent, participants can withdraw from participating at any time without explanation. Of the 305 volunteers who were randomly assigned to the BDM treatment arm, only three ultimately declined to participate in the auction. As each participant posted one bid per contract, we ended up with a balanced panel dataset of 604 observations. Table 3.2 presents the summary statistics of the two auction rounds. The distribution of bids in both contracts is skewed to the right, with the mean bid greater than the respective median bid in both cases. The mean (median) bid for contract A is 9,284 BRL (4,500 BRL), which is roughly equivalent to eight (four) times the minimum wage in the country in 2021. The mean (median) bid for contract B is significantly lower, at 6,692 BRL (3,500 BRL). Note that the highest bids amount to several thousand Brazilian reals (100K BRL for contract A and 60K BRL for contract B). Although one cannot formally exclude that this is the true opportunity cost of the respondents, an alternative explanation could be that these individuals knowingly made an excessively high bid, in order to be certain not to win the auction (i.e., protest bidders). Finally, Table 3.2 highlights a difference between the offers of the crop farmers and those of the cattle ranchers. The mean (median) bid of crop farmers for contract A indeed amounts to 7,492 BRL (4,000 BRL), while that of cattle ranchers reaches 11,054 BRL (4,550 BRL).

Figure 3.3 displays the BDM demand curves for each of the two PES contracts. The bids are log transformed for ease of viewing. Regardless of the percentile group considered, we can observe that the offer for contract B (more flexible) is always lower than the offer for contract A. For example, 75 percent of the sample requested less than 5,000 BRL for contract B and 9,500 BRL for contract A.

Table 3.3 presents the results of two tests that compare the distributions of the BDM sub-treatments, using both the Wilcoxon-Mann-Whitney rank-sum and Kolmogorov-Smirnov tests. In both cases, we find that the distribution under the contract A treatment is significantly different from the contract B treatment at the 1% level. This is in line with theoretical expectations, since the more difficult contract (contract A) is expected to have a higher cost of compliance.

3.4.2 Auction results

Table 3.4 presents the results of the auction. As mentioned before, each participant made an offer for each contract but the choice of the contract for which the BDM would be played in real life was random so that we ended up with a balanced number of subjects for each type of contract (150 for contract A and 152

Table 3.2: Summary statistics of bids (contract A and contract B)

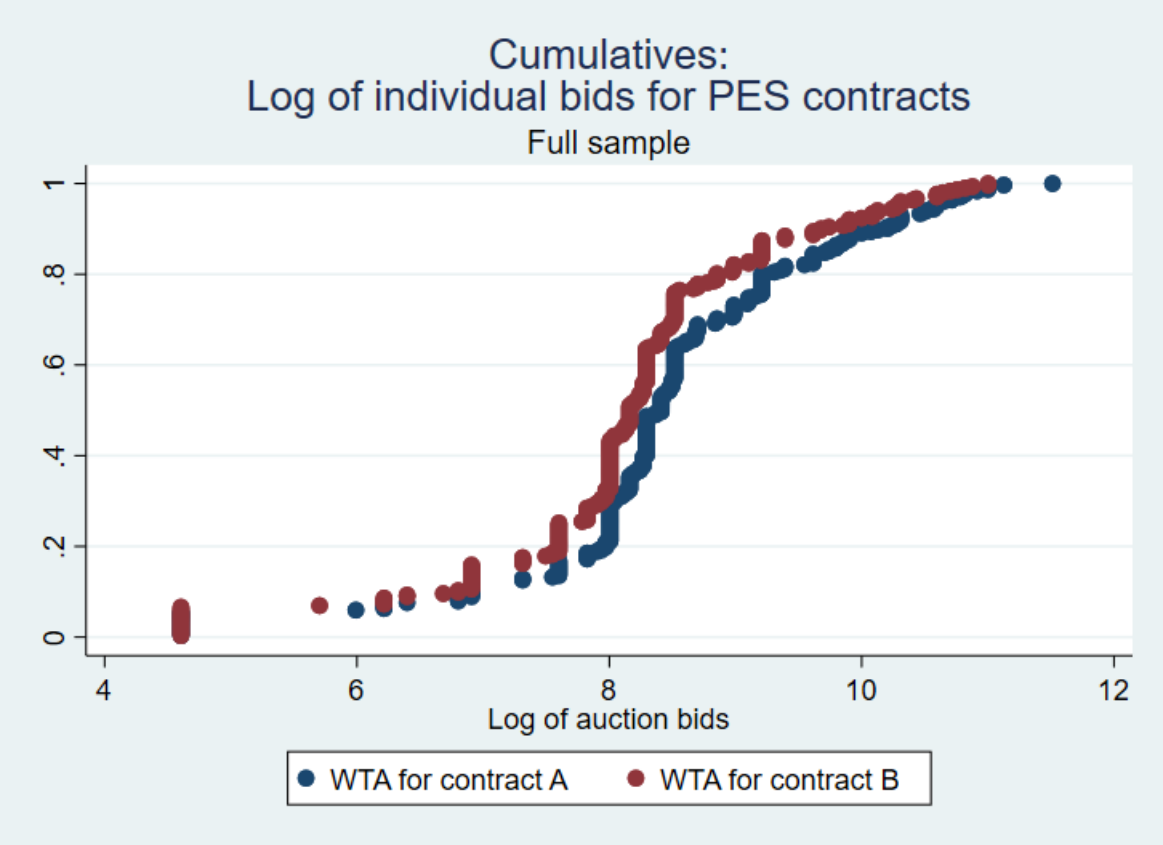
	Contract type	
	A (Zero-deforestation)	B (Threshold)
Full sample		
Number of participants	302	302
Mean bid (BRL)	9,284	6,692
Median bid (BRL)	4,500	3,500
Std. Dev. (BRL)	13,176	9,850
Minimum bid (BRL)	100	100
Maximum bid (BRL)	100,000	60,000
Cattle ranchers		
Number of participants	132	132
Mean bid (BRL)	11,054	7,639
Median bid (BRL)	4,550	3,600
Std. Dev. (BRL)	15,472	10,849
Crop farmers		
Number of participants	158	158
Mean bid (BRL)	7,492	5,548
Median bid (BRL)	4,000	3,500
Std. Dev. (BRL)	15,472	10,849
Diversified farmers		
Number of participants	12	12
Mean bid (BRL)	13,417	11,342
Median bid (BRL)	5,050	4,500
Std. Dev. (BRL)	17,989	16,559

Table 3.3: Test of equality of bid distributions (contract A versus contract B)

Kolmogorov-Smirnov		
D-statistic	P-value	Num. Obs.
-0.162	0.000	302

Wilcoxon rank-sum		
Z-statistic	P-value	Num. Obs.
3.903	0.000	302

Figure 3.3: Cumulative density function of log auction bids



for contract B). We calibrated the tokens in the urn in such a way as to respect our budgetary constraint, that is to say, around 150K euros in total for all the payments.[11] Thus, participants who placed high bids had less chance of winning the auction. On average, more than 70 percent of players won a PES contract, whether type A or type B. The median value of the payment allocated for contract A was 5,300 BRL (5,000 for contract B). The highest contract awarded was 6,100 for contract A (5,500 for Contract B). Interestingly, crop farmers had the highest awarding rate (78.5 percent contracts won), followed by cattle ranchers (65.1 percent) and diversified farmers (58.3 percent). We explore this heterogeneity further in what follows.

[11] We were able to do so by calculating the minimum detectable effect of the PES.

Table 3.4: Auction results

	Contract type	
Bids/contracts awarded	A (Zero-deforestation)	B (Threshold)
Full sample	150/108	152/109
Cattle ranchers	65/41	67/45
Crop farmers	78/62	80/62
Diversified farmers	7/5	5/2
Mean contract value (BRL)	5345	4831
Median contract value (BRL)	5300	5000
Std. Dev. (BRL)	422	477
Minimum (BRL)	4500	3900
Maximum (BRL)	6100	5500

3.5 Bid determinants and farmer heterogeneity

Following Jack, Leimona, and Ferraro (2009) and Jindal et al. (2013), we used an ordinary least squares (OLS) pooled regression to examine whether observable characteristics could predict farmers' bids[12]. We regressed the bid price on the participant characteristics that are likely to drive private opportunity costs, according to the standard non-separable farm household model. An individual's bid was therefore modeled as a function of observed characteristics:

$$bid_{ic} = X_i\beta + D_c + \epsilon_{ic} \tag{3.1}$$

where individual i's specific bid_i is a function of labor, land-use, and household characteristics. The r represents the auction round, with the dummy variable D_r taking the value 1 for bidding on the Type B contract and 0 for bidding on the Type A contract. We reran the same model separately for the subset of crop farmers and for the subset of cattle ranchers. Table 3.5 reports the results for the three samples[13]. The results show that bidding behavior predictors of cattle ranchers are quite different from those of crop farmers.

Looking at the contract type variable, results are in line with theory, since bidding for a type B contract (rather than contract A) reduced the bid by 2,353 BRL, on average, (Column 1). This result is statistically significant and has the same

[12]An alternative model is a random effect regression. In our case, it gives results extremely similar to those of the OLS pooled-regression.

[13]In all cases, household characteristics explain very little of the variation in WTA (R^2 is around 0.3), which is consistent with the consumer-behavior literature (Berry, Fischer, and Guiteras, 2020) and indicates that part of heterogeneity across landowners remains unexplained. In fact, our model does not include farmers' price expectations, which can vary considerably from one individual to another.

Table 3.5: Determinants of auction bids

	(1)	(2)	(3)
	Full sample	Crop farmers	Cattle ranchers
Type B contract (0/1)	-2352.84***	-1906.76***	-2930.08**
	(722.92)	(736.50)	(1190.44)
Age of the participant	-45.56	-31.60	-102.08*
	(32.83)	(33.47)	(57.36)
Female participant (0/1)	-1923.71***	-1822.03***	-1544.40
	(731.88)	(675.47)	(1377.40)
Education	148.42	-61.10	8.29
	(129.71)	(109.27)	(196.48)
Family size	-744.72***	-1061.20***	-591.55
	(228.66)	(300.71)	(479.03)
Family members working on farm	893.61***	1640.96***	725.54
	(283.29)	(377.36)	(657.19)
Family members working off farm	-246.73	-743.03**	-161.44
	(277.22)	(328.13)	(516.31)
Total income (BRL)	0.05	-0.02	-0.03
	(0.04)	(0.03)	(0.05)
Crop income (BRL)	0.02	0.12**	0.06
	(0.06)	(0.05)	(0.07)
Cattle income (BRL)	-0.10*	-0.04	-0.02
	(0.05)	(0.08)	(0.05)
Other livestock income (BRL)	0.13	-0.20***	0.17
	(0.11)	(0.07)	(0.17)
Business income (BRL)	0.26	0.84***	-0.25*
	(0.21)	(0.28)	(0.13)
Total area (hectares)	-99.58**	19.55	-133.54**
	(45.81)	(55.38)	(62.26)
Forest area (hectares)	115.42**	-15.21	127.63*
	(52.25)	(58.55)	(70.74)
Crop area (hectares)	421.06**	517.58**	257.88
	(202.37)	(238.39)	(354.11)
Pasture area (hectares)	232.24***	25.04	309.68***
	(54.61)	(61.80)	(69.07)
2018-2021 average deforestation (hectares)	2348.60***	3466.53***	2223.20**
	(619.18)	(1000.98)	(861.68)
Constant	3373.67	1788.54	9028.97**
	(2061.53)	(2009.16)	(3904.96)
Number of observations	564	296	246
Prob > F	0.000	0.000	0.000
R2	0.261	0.326	0.330

Notes: ***, ** and * indicate that the estimated coefficients are statistically significant at the 1%, 5%, and 10% levels, respectively. Robust standard errors are given in parentheses.

sign across all three samples, but the magnitude is higher for cattle ranchers (Column 3). Farmer-specific variables that seem to correlate with the value of the bid also include the age of the bidder (for cattle ranchers) and his gender (for crop

farmers). On average, male crop farmers bid higher than women crop farmers by 1,822 BRL (Column 2). Family size and labor seem to play a significant role for crop farmers but not for cattle ranchers. This makes sense since cattle ranching is much less labor intensive. Results from the crop farmer sample indicate that, on average, one additional family member working on farm increased the value of the bid by 1,641 BRL (Column 2), while one additional family member working off farm would decrease the bid by 743 BRL. Nothing similar could be found for cattle ranchers.

Income variables that tend to explain bidding choices include the 'crop and other livestock income' variable (which includes chicken, pork, and fish production), when it comes to crop farmers, and business income more generally. Looking at the whole sample, all land-use variables included in the model appear to play a significant role in the bidding decision (Column 1), but only average deforestation is significant across all three models. All other things being equal, one additional hectare of average deforestation increases the value of the bid by about 2,349 BRL. This result is statistically significant and has the same sign across all models, but the magnitude is higher for crop farmers (column 3). Looking at the two groups separately, one can observe that the weight of the total area, the area in forest and that in pasture, seems driven by the group of cattle ranchers, while the cultivated area, as one might expect, plays a significant role primarily in the group of crop farmers. Overall, the signs on the coefficients are consistent with theoretical expectations, which suggests that participants understood the BDM exercise well.

3.6 Discussion and next steps

3.6.1 WTA estimates and the social cost of carbon

The social cost of carbon (SCC) is a metric that helps quantify the costs of climate change related to our carbon emissions in terms of dollars per metric ton of carbon dioxide (CO_2) emitted. It can also be used to quantify the benefits of reducing CO_2 emissions. Current policy recommendations for the SCC range from 52 USD (IWG, 2021) to 202 USD (UmweltBundesamt, 2019).

To compare the WTA revealed by the participants and the benefits from avoiding CO_2 emissions, we take into account the average above-ground biomass (AGB)[14] stored in forests in our study zone (315 metric tons), provided by Harris

[14]We ignored carbon stocks in the below-ground biomass (BGB) that is composed of roots, litter, dead wood, and soil. The reason we take only the AGB into account is because this is the portion of the total biomass most rapidly emitted after forests are cleared and set on fire. The carbon contained in BGB takes longer to be emitted, and part of this carbon is fixed again by pasture and crops.

et al. (2021), and assume that 48% of biomass is in the form of carbon (Carvalho Jr et al., 1995). We find that, on average, one hectare covered in forests stores 151 tons of carbon above-ground. Since one CO_2 molecule weighs 3.67 times as much as a carbon atom, this means that 554 metric tons of CO_2 are stored in one hectare of land covered by the PES contracts.

According to our WTA estimates, the median landholder[15] would require between 900 USD[16] (contract A) and 700 USD (contract B) to preserve her forest cover during a one-year period. If we take into account the average annual deforestation rate of the respondents (1.43 hectares), we find that farmers' WTA ranges from 490 to 630 USD/hectare. If we apply an interest rate of 3 percent to the SSC estimated by IWG (2021) and UmweltBundesamt (2019), the expected benefits from avoided CO_2 emissions ranges between 839 and 3260 USD/hectares. Therefore, the estimates obtained with our BDM experiment of farmers' private costs for avoiding CO_2 emissions is lower than the SCC.

It is also worth-mentioning that the revealed-preference WTA data we computed are much higher than those found in previous studies. As mentioned in Section 3.2, Palmer, Taschini, and Laing (2017) found that, on average, a payment of 9.5 USD per hectare of forest is needed to avoid deforestation during a 5-year period in the State of Acre. Thus, if we take the average forest area of surveyed landholders (37 ha), we arrive at an average payment of 70 USD per year. However, according to our estimates, the median landholder would require between 700 USD and 900 USD to preserve her forest cover during a one-year period. This is due, in part, to the choice of targeting the program to a deforestation-front area. As our study area is near the main road and local markets, the incentives to clear forests are higher than in more remote areas of Acre.

3.6.2 A typology of private landowners

Another interesting result of this experiment is the emergence of a typology of farmers who agreed to participate in the auction. On the one hand, the so-called 'crop farmers', who are low-income households practicing traditional agriculture based on fallow land and subsistence farming and who have a relatively low opportunity cost of conserving the forest located in their farmhold. On the other hand, there are wealthier households, more specialized in cattle farming – the reason we call them 'cattle ranchers' in the study – and highly dependent on deforestation, because they do not use the fallow and rotation system for their pasture areas. Their agricultural activity indeed depends on less sustainable systems. Although

[15]We opted to use the median value rather than the mean value, because the distribution of bids is highly skewed to the right.

[16]Results in BRL were converted to USD by applying a conversion rate of 0.2 (average conversion rate of Brazilian real to U.S. dollar in May 2022).

spread over quite a small area, the private costs of forest conversation of the two groups differ significantly. They have different WTAs, on average, regardless of the contract offered, with crop farmers who are less dependent on deforestation systematically exhibiting lower WTA than cattle ranchers. In addition, and this is again consistent with the typology, the two groups value the contract types quite differently; the flexibility gap between the two contracts translates into a 50 percent higher bid by cattle ranchers than by crop farmers.

3.6.3 Next steps

Main results show that the participants in an environmental auction are well able to understand the difference between a threshold contract and a zero-deforestation one and that their revealed WTA reflects their understanding of the consequences of their choice for either contract. This is likely to play an important role in the cost-effectiveness of each of the contracts. Other aspects also come into play, for which it is difficult a priori to anticipate the effects on the cost-effectiveness of PES: the probability that the participant will break the contract before its end, for example. In fact, the question of whether contract A (high conservation potential but high default risk) outperforms contract B (lower conservation potential but lower default risk) is difficult to address theoretically. The next stage of our work will therefore consist in evaluating the cost-effectiveness of the PES tested in the light of the results of the RCT set up a few months ago and still in progress at the time of this study.

Besides being incentive-compatible, the BDM mechanism creates a random variation in the actual contract value offered to landholders with identical WTA. That is, two participants bidding the exact same value, can receive different actual payoffs at the end of the contract duration, and this difference will be determined purely by chance. This random variation will allow us to isolate the causal effect of the PES on forest conservation outcomes (Guiteras and Jack, 2018). It will also allow us to estimate heterogeneous treatment effects, i.e., checking whether those who provide the most environmental services are also those with the highest WTA (Berry, Fischer, and Guiteras, 2020).

3.7 Appendix to Chapter 3

INRAE-USP-CIRAD F4F Program		**Interview date:** ____/____/2021 **Start:** ____:____ **End:** ____:____
1	**Experimental Auction**	**Household code:** |__||__||__| *HH number* |__| *Municipality* |__||__| - |__|__| *Surveyors Initials*
Municipality: **Village:** **Respondent:** **Approx. forest area (ha):**		
HH Coordinates		**UTM X :** **UTM Y :**

(Note: The statements in italics are instructions for the surveyors and should not be read to the participants).

Now I will ask you whether you will be willing to participate on the two forest conservation contracts mentioned earlier for several different contract values. But remember that only one kind of contract will be offered to you.

The auction will occur as follows.

The first step will be to ask you the minimum amount of money that you are willing to accept to conserve the forest on your farmhold for one year, according to the requirements of each of the two contracts presented earlier. In the second part, we will reveal what kind of contract was randomly assigned to you. And finally, you will draw a token from an urn. The urn will contain several tokens, once you have drawn one token, two things can happen.

1. If the token value is greater than or equal to the value you requested, **we will offer you a forest conservation contract.** In this contract, we will commit to pay you the same amount you drew from the urn, even if it is higher than the amount you previously requested, but only if the requirements of the contract are met during a period of twelve months from today's date. During these twelve months we will monitor your forest with satellite images. **If you follow the requirements of the contract regarding the conservation of your forest, you will be paid the value written on the token.** SOS Amazônia will make the payment up to 2 (two) months after the end of the 12 (twelve) month period established in the contract. If you do not have a bank account, you will have 6 (six) months to open one and inform SOS Amazônia. **If you do not open a bank account within 6 (six) months after signing the contract, the contract will be cancelled.** If you do not follow the requirements of the contract you signed, you will not receive the payment. You can choose whether or not to sign the contract with SOS Amazônia. If you choose not to sign, we will not make any payment to you and we will thank you for your participation in the study.

2. The second thing that can happen is that the amount drawn is smaller than you requested. In that case **you will not be given a forest conservation contract and you will not have a chance to change your mind**. We will thank you for participating in our study, and we will give you a food parcel as compensation for your time spent on the study. **Only participants who sign a PES contract and follow the conditions of the contract**, i.e. conserve their forests for a period of twelve (12) months as required by the contract, **will receive payment at the end of fourteen (14) months.**

Please note: It is in your best interest to actually state the lowest amount you are willing to accept for each forest conservation contract. Otherwise, there is a risk that you will not be able to sign the contract, even though you would be interested in signing the contract for the drawn amount. In other words, you should not ask for less than the minimum you would be willing to receive to conserve your forests according to the requirements of the contact. But if you ask for a higher value, you risk losing the opportunity to sign a contract that would be advantageous to you.

The bids will be posted separately for each of the two contacts, because each one of them has different rules. After the bids are posted, we will let you know which contract was randomly picked to be offered to you.

Before we continue, do you have any questions?

Do you wish to proceed?

a) Yes
b) No

Remember that we will calculate the forest area on your property by satellite images based on the boundaries of your property provided by you when signing up for the program, and we will monitor the forest conservation activity on your property to make sure that you follow the contract. Is this clear?

c) Yes
d) No

Instructions for the surveyor:

1- *Ask the participant what is the minimum amount of money he/she would be willing to accept to fulfill the PES contract. If he/she doesn't know what to answer, use the selected question randomly picked before the interview.*
2- *Imagine the respondent selected (or you randomly picked) the **option 17** as the opening question.*
    a. *If the respondent says yes to this value, go halfway to the lower bound (option 1), i.e., to **option 9** (17/2=8.5). Round up it is not a whole number.*
    b. *If the respondent says no to this value, go halfway to the upper bound (option 31), i.e., to **option 24** [(31-17)/2]+17= 24.*
    c. *Keep choosing between the midpoint of the two previous options, or between the last option and the extreme, until you identify the shift options.*
3- *The procedure should be the quickest way to identify the shift options, that is, two options for which the respondent alternates between 'yes' and 'no'. Remember that you need to write down these two options, and that the two options should be neighbors.*

Type A Contract

1. What is the minimum price that you are willing to accept to conserve the forest on your property for one year? Note that in the **type A contract**, if you do not conserve all the forest on your property for 12 months, you will not receive any financial compensation.

2. For each of the following values, confirm whether you would be willing to sign a contract for forest conservation:

	Question:	Answer (Yes/No)	
1	If you pick 3000 BRL, would you accept this payment for your PES contract?		
2	If you pick 3100 BRL, would you accept this payment for your PES contract?		
3	If you pick 3200 BRL, would you accept this payment for your PES contract?		
4	If you pick 3300 BRL, would you accept this payment for your PES contract?		
5	If you pick 3400 BRL, would you accept this payment for your PES contract?		
6	If you pick 3500 BRL, would you accept this payment for your PES contract?		
7	If you pick 3600 BRL, would you accept this payment for your PES contract?		
8	If you pick 3700 BRL, would you accept this payment for your PES contract?		
9	If you pick 3800 BRL, would you accept this payment for your PES contract?		
10	If you pick 3900 BRL, would you accept this payment for your PES contract?		
11	If you pick 4000 BRL, would you accept this payment for your PES contract?		
12	If you pick 4100 BRL, would you accept this payment for your PES contract?		
13	If you pick 4200 BRL, would you accept this payment for your PES contract?		
14	If you pick4,300 BRL, would you accept this payment for your PES contract?		
15	If you pick 4400 BRL, would you accept this payment for your PES contract?		
16	If you pick 4500 BRL, would you accept this payment for your PES contract?		
17	If you pick 4600 BRL, would you accept this payment for your PES contract?		

18	If you pick 4700 BRL, would you accept this payment for your PES contract?		
19	If you pick 4800 BRL, would you accept this payment for your PES contract?		
20	If you pick 4900 BRL, would you accept this payment for your PES contract?		
21	If you pick 5000 BRL, would you accept this payment for your PES contract?		
22	If you pick 5100 BRL, would you accept this payment for your PES contract?		
23	If you pick 5200 BRL, would you accept this payment for your PES contract?		
24	If you pick 5300 BRL, would you accept this payment for your PES contract?		
25	If you pick 5400 BRL, would you accept this payment for your PES contract?		
26	If you pick 5500 BRL, would you accept this payment for your PES contract?		
27	If you pick 5600 BRL, would you accept this payment for your PES contract?		
28	If you pick 5700 BRL, would you accept this payment for your PES contract?		
29	If you pick 5800 BRL, would you accept this payment for your PES contract?		
30	If you pick 5900 BRL, would you accept this payment for your PES contract?		
31	If you pick 6000 BRL, would you accept this payment for your PES contract?		

Type B Contract

1. What is the minimum price that you are willing to accept to conserve the forest on your property for one year? Note that if any portion of your forest area is found to have been deforested, penalties will apply as follows: i) a **20% penalty** will be applied to the total payment if a deforestation patch **between 0.1 to 0.5 hectare** is detected through satellite imagery; ii) a **50% penalty** will be applied to the total payment if a deforestation patch **between 0.6 to 1 hectare** is detected through satellite imagery; and iii) **you will not receive any financial compensation** if a deforestation patch **greater than 1 hectare** is detected through satellite imagery.

3. For each of the following values, confirm whether you would be willing to sign a contract for forest conservation:

	Question:	Answer (Yes/No)	
1	If you pick 3000 BRL, would you accept this payment for your PES contract?		
2	If you pick 3100 BRL, would you accept this payment for your PES contract?		
3	If you pick 3200 BRL, would you accept this payment for your PES contract?		
4	If you pick 3300 BRL, would you accept this payment for your PES contract?		
5	If you pick 3400 BRL, would you accept this payment for your PES contract?		
6	If you pick 3500 BRL, would you accept this payment for your PES contract?		
7	If you pick 3600 BRL, would you accept this payment for your PES contract?		
8	If you pick 3700 BRL, would you accept this payment for your PES contract?		
9	If you pick 3800 BRL, would you accept this payment for your PES contract?		
10	If you pick 3900 BRL, would you accept this payment for your PES contract?		
11	If you pick 4000 BRL, would you accept this payment for your PES contract?		
12	If you pick 4100 BRL, would you accept this payment for your PES contract?		
13	If you pick 4200 BRL, would you accept this payment for your PES contract?		
14	If you pick4,300 BRL, would you accept this payment for your PES contract?		
15	If you pick 4400 BRL, would you accept this payment for your PES contract?		
16	If you pick 4500 BRL, would you accept this payment for your PS contract?		

17	If you pick 4600 BRL, would you accept this payment for your PES contract?		
18	If you pick 4700 BRL, would you accept this payment for your PES contract?		
19	If you pick 4800 BRL, would you accept this payment for your PES contract?		
20	If you pick 4900 BRL, would you accept this payment for your PES contract?		
21	If you pick 5000 BRL, would you accept this payment for your PES contract?		
22	If you pick 5100 BRL, would you accept this payment for your PES contract?		
23	If you pick 5200 BRL, would you accept this payment for your PES contract?		
24	If you pick 5300 BRL, would you accept this payment for your PES contract?		
25	If you pick 5400 BRL, would you accept this payment for your PES contract?		
26	If you pick 5500 BRL, would you accept this payment for your PES contract?		
27	If you pick 5600 BRL, would you accept this payment for your PES contract?		
28	If you pick 5700 BRL, would you accept this payment for your PES contract?		
29	If you pick 5800 BRL, would you accept this payment for your PES contract?		
30	If you pick 5900 BRL, would you accept this payment for your PES contract?		
31	If you pick 6000 BRL, would you accept this payment for your PES contract?		

Auction Answers

	Question:	*Code:*	*Answer:*
1	*What kind of contract was drawn?*	A=1 B=2	
2	*What was the amount drawn from the urn?*	BRL	
3	*Did the participant say he/she would accept a contract with that value?*	Yes=1 No=0	
4	*If yes, read: You said you would accept a payment of <<contract value>>, so we will offer you a contract for PES for that amount.*		

INRAE-USP-CIRAD A Renda da Floresta		**Dia da semana**: _____ **Data de entrevista:** ____/____/2021 **Início:** ____:____ hs **Término:** ____:____ hs
1	**Questionário de Unidades Familiares**	**Registro da Unidade Amostral (UA):** |__||__| |__| |__||__| |__|__| *UA* *Município* *Iniciais Entrevistadores*
Município :_____ **Comunidade:**_____		**Entrevistado:** _____ _____
Coordenadas da UA		UTM X : UTM Y :

(Nota: As declarações em itálico são instruções para os entrevistadores e não devem ser lidas para os participantes).

1. INFORMAÇÕES BÁSICAS SOBRE O/A CHEFE/A DA UNIDADE FAMILIAR
1.1. Sexo do entrevistado: a) Masculino b) Feminino
1.2. Quantos anos o(a) senhor(a) tem?
1.3. Onde o(a) senhor(a) nasceu?
1.3.1 Comunidade:
1.3.2 Cidade:
1.3.3 Estado:
1.4. Até qual série da escola o(a) senhor(a) completou? *(Calcular total de anos de estudo depois, antes de entrar os dados)*
1.5. Há quanto tempo o(a) senhor(a) vive nessa comunidade? *(anos/meses)*

1.6. Qual a ocupação principal do(a) senhor(a)?

a) Produtor rural
b) Diarista (motosserra, roçadeira, terçado, etc.)
c) Dona de casa
d) Outro (especifique):

1.7. Há quanto tempo o(a) senhor(a) trabalha com isso? *(anos/meses)*

1.8. O(a) senhor(a) participa de alguma organização ou associação ou sindicato?

a) Não
b) Sim
c) Qual? *(pode ser mais de um)*

Por organização, associação ou sindicato, queremos saber se ele(a) é associado(a), por exemplo, ao(à): i) associação da comunidade/assentamento; ii) sindicato dos trabalhadores rurais ou outro sindicato; iii) organizações diversas (ex. ONG, conselho de meio ambiente/saúde).

1.9. O(a) senhor(a) já participou de algum programa ambiental proposto por uma ONG ou pelo governo?

a) Não
b) Sim
c) Qual?

Por programa ambiental, queremos dizer programas voltados estritamente à conservação das florestas (ex. PSA prévio, programa de regeneração florestal), ou mesmo aqueles com objetivos múltiplos ligados à melhoria da produção (através, p. ex., de assistência técnica e doação de insumos agropecuários), mas com algum componente de conservação (p. ex., com o objetivo de melhorar a produção gerando menos desmatamento).

1.9.1. A sua experiência com este programa ambiental foi:

a) Muito positiva
b) Positiva
c) Neutra
d) Negativa
e) Muito negativa

1.9.2. Por que?

2. INFORMAÇÕES BÁSICAS SOBRE A UNIDADE FAMILIAR

Identifique e preencha as informações sobre os membros da unidade familiar. Considerar, aqui, a definição de "agregado familiar" ou "unidade doméstica". Ou seja, incluir pessoas que, mesmo que não morem na mesma casa, morem na mesma propriedade e compartilhem recursos, como trabalho, alimentos e dinheiro. Isso inclui até, eventualmente, pessoas que não sejam membros da mesma família.

2.1.	2.1.1. Primeiro nome *(preencher a primeira linha com as informações do entrevistado obtidas nas perguntas anteriores)*	2.1.2. Gênero (F/M)	2.1.3. Idade	2.1.4. Trabalha na propriedade (*Não/Sim*)	2.1.5. Trabalha fora da propriedade (*Não/Sim*)	2.1.6. Ocupação fora da propriedade a) trabalho formal (ex. professor, agente de saúde, vendedor) b) diárias (trabalho rural) c) ambos acima d) outro *(especificar*)
1						
2						
3						
4						
5						
6						
7						
8						
9						
10						
11						
12						

13						
14						
15						

2.2. Existem funcionários ou trabalhadores por diária na propriedade?

a) Não
b) Sim

2.2.1. Se sim, quantos?

2.3. Qual é a renda mensal média da família aproximadamente? (*Soma de todos os ingressos*)

a) Até 500 reais
b) Entre 500 e 1000 reais
c) Entre 1000 e 1500 reais
d) Entre 1500 e 2000 reais
e) Entre 2000 e 2500 reais
f) Entre 2500 e 3000 reais
g) Acima de 3000 reais

Incluir a renda proveniente de todos os membros do agregado familiar/unidade doméstica identificados na pergunta 2.1. Incluir: salários/diárias, transferências governamentais, renda monetária proveniente da produção agropecuária, da venda de madeira e produtos florestais não madeireiros, negócios próprios e qualquer outra fonte de renda.

2.4. Qual o uso da terra gera mais renda para a família?

a) Gado
b) Agricultura
c) Outro (especifique):

3. RENDA MISTA NOS ÚLTIMOS 12 MESES

3.1. Agricultura: _____ reais.

3.2. Criação de gado: _____ reais.

3.3. Criação de outros animais *(especifique)*: _____ reais.
3.4. Produtos da floresta (ex: açaí, andiroba, patuá, etc.) : _____ reais.
3.5. Negócio familiar *(especifique):* _____ reais.
3.6. Aposentadoria: _____ reais.
3.7. Apoio do governo (ex: bolsa família, auxílio emergencial, etc.): _____ reais.
3.8. Programa do governo para a agricultura familiar (ex: PAA, PNAE.): _____ reais.
3.9. Outro *(especifique):* _____ reais.

4. FINANCIAMENTO AGRÍCOLA
4.1. O(a) senhor(a) já fez um financiamento agrícola?
4.2. Qual foi o propósito do financiamento? a) Comprar terra b) Comprar gado c) Comprar maquinário d) Outros, especifique:

5. INFORMAÇÕES BÁSICAS SOBRE A PROPRIEDADE RURAL

5.1. O(a) senhor(a) é proprietário da área?

a) Não
b) Sim

5.1.2. Se não, quem é?

Por proprietário da terra, entenda quem é responsável por ela e toma decisões sobre seu uso. Não necessariamente precisa ter o título da propriedade.

5.2. Essa propriedade foi:

a) Comprada
b) Doada pelo governo (ex: INCRA)
c) Posse
d) Herdada
e) Outros, especifique:

5.3. O(a) senhor(a), ou o proprietário da terra (*caso ele tenha respondido "não" na 5.1.*), possui documento da propriedade?

a) Não
b) Sim

5.4. Tamanho da propriedade? (hectares)

a) Mata bruta *(floresta primária):*

b) Capoeira *(floresta secundária):*

c) Roçado (ex: mandioca, milho, feijão, banana) *(agricultura de subsistência e eventuais vendas)*

d) Pasto:

e) Outro uso do solo (ex: açude, plantio de açaí, graviola, etc.):

5.5. Quantidade de cabeças de gado: (unidades)

5.5.1. Quantidade de bezerros que nasceram no último ano:

5.5.2. Quantidade de cabeças de gado compradas no último ano:

5.5.3. Quantidade de cabeças de gado vendidas no último ano:

6. PRODUÇÃO E CUSTOS DE PRODUÇÃO AGRÍCOLA NOS ÚLTIMOS 12 MESES

6.1. Qual o custo médio para brocar um hectare de floresta?
Este custo inclui o custo de diárias para brocar, para derrubar, combustível, óleo, etc.

6.2. Qual o custo médio para formar um hectare de pasto?

Este custo inclui o custo de adquirir sementes, diárias, combustível, etc.

6.3. Qual o custo médio para formar um hectare de roçado?
Este custo inclui sementes, diárias, etc.

7. MUDANÇAS NA COBERTURA FLORESTAL

7.1. Com qual frequência o(a) senhor(a) derruba a mata bruta seja para fazer roçado, pasto ou outra finalidade?

a) Todo ano
b) A cada dois anos
c) A cada três anos
d) A cada quatro anos
e) Outro:

7.2. Quantas vezes você faz o roçado em uma área de derrubada?

a) Uma vez
b) Duas vezes

c) Três vezes
d) Quatro vezes
e) Outro:

7.3. Qual o intervalo de tempo antes de fazer o roçado na mesma área de novo?

a) Um ano
b) Dois anos
c) Três anos
d) Quatro anos
e) Outro:

7.4. O que acontece com a área após a colheita do roçado?

a) Vira capoeira
b) Vira pasto
c) Outro:

7.5. Qual foi a área total de derrubada nos últimos 4 anos e qual o foi o principal propósito da derrubada? (número de hectares derrubados por mês)

7.5.1 2018		7.5.2 2019		7.5.3 2020		7.5.4 2021	
Abril	_____ha	Abril	_____ha	Abril	_____ha	Abril	_____ha
Maio	_____ha	Maio	_____ha	Maio	_____ha	Maio	_____ha
Junho	_____ha	Junho	_____ha	Junho	_____ha	Junho	_____ha
Julho	_____ha	Julho	_____ha	Julho	_____ha	Julho	_____ha
Agosto	_____ha	Agosto	_____ha	Agosto	_____ha	Agosto	_____ha
Setembro	_____ha	Setembro	_____ha	Setembro	_____ha	Setembro	_____ha
Outubro	_____ha	Outubro	_____ha	Outubro	_____ha	Outubro	_____ha

7.5.1.1 Qual foi o principal propósito da derrubada? a) Roçado b) Pastagem c) Outros, especifique (ex: plantar açaí, café, açude):	7.5.2.1 Qual foi o principal propósito da derrubada? a) Roçado b) Pastagem c) Outros, especifique (ex: plantar açaí, café, açude):	7.5.3.1 Qual foi o principal propósito da derrubada? a) Roçado b) Pastagem c) Outros, especifique (ex: plantar açaí, café, açude):	7.5.4.1 Qual foi o principal propósito da derrubada? d) Roçado e) Pastagem f) Outros, especifique (ex: plantar açaí, café, açude):

8. CRENÇAS SOCIAIS, NORMATIVAS E DE CONTROLE	
8.1.	Na opinião do(a) senhor(a) quais são as vantagens, ou pontos positivos, em brocar a floresta da sua colônia? a) Fazer roçado para alimentar a minha família b) Fazer roçado para alimentar a criação de animais c) Aumentar a minha área de pasto d) Valorizar o preço da minha terra e) Abertura de açude f) Outro (especifique):
8.2.	Na opinião do(a) senhor(a) quais são as desvantagens, ou pontos negativos, em brocar a floresta da sua colônia? a) Risco de multa b) Poluição do ar pelas queimadas c) Calor d) Seca e) Diminuição dos animais de caça f) Diminuição da água nos igarapés g) Outro (especifique)
8.3.	Na percepção do(a) senhor(a), quem aprova, ou acha boa a sua decisão de derrubar a floresta? Por exemplo membros da sua família, vizinhos, governo, etc. a) Mais velhos da família b) Mais jovens da família c) Produtores vizinhos d) Amigos e) Comprador de gado

f) Governo federal
g) Agronegócio
h) Outro (especifique)

8.4. Na percepção do(a) senhor(a), quem desaprova, ou acha ruim a sua decisão de derrubar a floresta? Por exemplo membros da sua família, vizinhos, governo, etc.

a) Mais velhos da família
b) Mais jovens da família
c) Produtores vizinhos
d) Órgão de assistência técnica (SEPA, EMATER, Secretaria de Agricultura, etc.)
e) Órgão ambiental (IMAC, Secretaria do Meio Ambiente, etc.)
f) Governo regional
g) Outro:

8.5. Na sua opinião, quais são as coisas que impedem ou dificultam o(a) senhor(a) usar as áreas já derrubadas por mais vezes?

a) Falta de dinheiro para investir
b) Falta de mão de obra
c) Falta de crédito do governo
d) Preço do gado
e) Falta de estradas de acesso
f) Falta de maquinários
g) Outro:

8.6. Na sua opinião, quais são as coisas que facilitam ou iriam facilitar o(a) senhor(a) usar as áreas já derrubadas por mais vezes?

a) Maquinário para arar a terra
b) Maquinário para construir açudes
c) Disponibilidade de crédito do governo
d) Disponibilidade de insumos (ex: arame, mudas, adubo)
e) Assistência técnica qualificada
f) Outro:

8.7. Quais são as preocupações do(a) senhor(a) em relação à sua colônia? Por exemplo, tem algo que preocupa o(a) senhor(a) com relação à sua produção, como: secas, enchentes, doenças do gado, pragas do roçado, etc.? Ou tem alguma outra preocupação que o senhor tenha com relação à segurança em permanecer na propriedade ou em relação à sua segurança pessoal e de seus familiares?

a) Enchentes
b) Pragas da lavoura
c) Doença do gado
d) Seca
e) Grilagem e especulação de terras
f) Criminalidade (assalto ou roubo)
g) Perda de produtividade do solo
h) Outro:

9. HISTÓRICO DE CONFLITOS POR TERRA

9.1. A terra do(a) senhor(a) já passou por algum tipo de disputa ou conflito? Por exemplo, a terra já foi invadida ou houve divergência com o vizinho sobre os limites das propriedades?

a) Não
b) Sim

9.2. Qual foi o principal motivo?

a) Disputa com o vizinho sobre a terra (ex: limites das parcelas que se sobrepõem)
b) Invasão de pessoas de fora da comunidade (ex: madeireiros)
c) Outro, especifique:

10. HISTÓRICO DE FISCALIZACAO

10.1. Em seu conhecimento, de que forma os órgãos de fiscalização monitoram o desmatamento aqui na região?

a) Imagens de satélite
b) Visitas de campo
c) Denúncias
d) Outros, especifique:

10.2. O(a) senhor(a) já foi fiscalizado por algum órgão ambiental do governo aqui na região?

a) Não
b) Sim

10.2.1. Se sim, quando foi a última vez? *(mês/ano)*

10.3. Qual órgão?

a) IBAMA
b) IMAC
c) Outros, especifique:

10.4. Qual foi o principal motivo?

a) Derrubada (corte raso)
b) Queimada
c) Caça de animais silvestres
d) Outros, especifique:

Via 1/2 - Participante

A RENDA DA FLORESTA

TERMO DE CONSENTIMENTO LIVRE E ESCLARECIDO

1. TERMO DE ESCLARECIMENTO

Gostaríamos de convidar você a participar da segunda fase do estudo chamado “A Renda da Floresta”. Como parte do estudo, será implementado um programa piloto de pagamentos por serviços ambientais. Este programa irá recompensar proprietários de terra pelos esforços de conservar suas florestas e será implementado pela SOS Amazônia em parceiria com o Instituto Nacional Francês de Pesquisa em Agricultura, Alimentação e Meio Ambiente (INRAE), e a Universidade de São Paulo (USP) na região da rodovia BR 364, nos municípios de Tarauacá e Feijó.

Neste documento, nós explicaremos como vai funcionar a segunda fase do projeto. Sempre que necessário, nós vamos explicar e tirar todas as suas dúvidas. Você pode escolher agora se quer continuar participando ou não. Mas poderá também desistir de participar a qualquer momento sem explicar o motivo. Nós fizemos duas cópias deste documento. Caso você concorde em participar, pediremos que assine as duas cópias. Uma das cópias ficará com você e a outra cópia será guardada pelo pesquisador responsável.

Por que eu fui convidado(a) a participar da segunda fase?

Você fez um cadastro para participar do programa piloto A Renda da Floresta e foi sorteado(a) para participar da segunda fase do estudo. Hoje nós estamos convidando você para participar de uma dinâmica/sorteio experimental e responder um questionário socioeconômico.

O que acontece se eu participar da segunda fase?

A primeira etapa da atividade é explicar os dois tipos de contratos de Pagamento por Serviços Ambientais que poderão ser oferecidos a você:

Contrato Fixo: Por meio deste contrato, o proprietário da terra concorda em receber, ao final do contrato, um pagamento **pela preservação integral da área de floresta** existente em sua propriedade. A SOS Amazônia apenas pagará a compensação financeira caso toda a sua área de floresta seja preservada até o final do contrato. Ou seja, se for constatado que o proprietário da terra desmatou parte da área de floresta, mesmo que mínima, o pagamento não será realizado.

Contrato Flexível: Por meio deste acordo, o proprietário da terra concorda em receber, ao final do contrato, um pagamento também **pela preservação integral da área de floresta** existente em sua

propriedade. Porém, caso seja constatado desmatamento de alguma porção de sua área de floresta, penalidades serão aplicadas da seguinte forma:

- O valor a ser pago por hectare terá uma redução de 20% caso a área de floresta tenha de 0,1 a 0,5 hectare de desmatamento.
- O valor a ser pago por hectare terá uma redução de 50% caso a área de floresta tenha de 0,6 a 1 hectare de desmatamento.
- Nenhuma compensação será paga caso a área de floresta tenha ultrapassado 1 hectare de desmatamento.

Na segunda parte da atividade, pediremos que você nos diga o valor que gostaria de receber por hectare de floresta para cada um dos dois tipos de contratos de PSA mostrados anteriormente. Este valor representa o valor mais baixo que você está disposto a aceitar para cada tipo de contrato. Entre os dois contratos apresentados, **sortearemos um contrato a ser oferecido a você**. Lembrando que você pode declarar um valor diferente para cada tipo de contrato.

Na terceira parte da atividade, você vai sortear um valor de uma urna de forma aleatoria. Uma vez sorteado o valor, duas coisas podem acontecer:

1. **Se o valor sorteado for maior ou igual ao valor que você solicitou, iremos oferecer um contrato de pagamentos por serviços ambientais**. Neste contrato, nos comprometeremos a pagar o mesmo valor que o(a) senhor(a) tirou na urna, mesmo que ele seja maior que o valor que senhor(a) solicitou anteriormente, mas somente se as exigências do contrato forem cumpridas durante um período de doze meses, ou seja, de dozes meses a partir da data de hoje. Durante esses 12 (doze) meses, verificaremos sua floresta com imagens de satélite e faremos uma comparação com a situação atual. Se o(a) senhor(a) tiver seguido as exigências do contrato referentes à conservação de sua floresta, será pago o valor sorteado na urna. No caso, a SOS Amazônia irá realizar o pagamento até 2 (dois) meses após o término do período de 12 (doze) meses estabelecido no contrato. Se o(a) senhor(a) não tiver uma conta bancária em seu nome, terá 6 (seis) meses para abrir uma e informar a SOS Amazônia. Se o(a) senhor(a) não abrir uma conta bancária dentro de 6 (seis) meses após a assinatura do contrato, o contrato será cancelado. Se o(a) senhor(a) não seguir os requisitos do contrato que assinou, não receberá o pagamento. O(a) senhor(a) pode optar por assinar ou não o contrato com a SOS Amazônia. Se o(a) senhor(a) optar por não assinar, não faremos qualquer pagamento e lhe agradeceremos por sua participação no estudo.

2. A segunda coisa que pode acontecer é **o valor sorteado ser inferior ao que o(a) senhor(a) solicitou. Nesse caso, não o(a) convidaremos a assinar um contrato de Pagamento por Serviço Ambiental**. Assim sendo, lhe agradeceremos por participar de nosso estudo e lhe daremos uma cesta de alimentos em compensação pelo tempo dedicado ao estudo.

Todos os participantes, sorteados ou não para receber um contrato de pagamentos por serviços ambientais, farão parte do estudo. É importante entender que a participação no estudo não garante que as famílias receberão um pagamento ao final, pois o projeto possui duas fases de sorteio e os pagamentos são condicionais ao cumprimento das exigências dos contratos de Pagamentos por Serviços Ambientais.

Ou seja, apenas os participantes do estudo que assinarem um contrato de pagamentos por serviços ambientais com a gente e seguirem os compromissos do contrato referentes à conservação de suas florestas no período de doze meses, irão receber um pagamento ao final do estudo.

Quais são os benefícios esperados caso eu participe?

Nós apresentaremos os resultados do nosso estudo para todos os participantes, tenham eles assinado ou não um contrato de Pagamentos por Serviços Ambientais com a gente. Acreditamos que este estudo será bom para a região, pois ajudará a entender se Pagamentos por Serviços Ambientais seriam uma boa estratégia para ajudar a conservar a floresta dessa região, se são bem aceitos pelos moradores daqui e se tem alguma questão neles que pode ser melhorada.

Além disso, caso você assine um contrato de Pagamentos por Serviços Ambientais com a gente e, ao final de doze meses, nós tenhamos confirmado, por meio de imagens de satélite, que sua floresta foi conservada conforme as exigências do contrato, você receberá um pagamento em dinheiro em troca do seu esforço feito para conservar a floresta, como explicamos antes.

Quais são os riscos e desconfortos para a minha participação?

Os riscos para a sua participação no estudo são mínimos, pois todas as etapas do estudo foram preparadas para evitar qualquer constrangimento ou inconveniente para os participantes. Você poderá decidir se quer participar ou não do estudo. Poderá fazer perguntas para a gente a qualquer momento. Poderá também se retirar do estudo a qualquer momento sem precisar explicar o motivo. Caso seja oferecido um contrato de pagamentos por serviços ambientais, você poderá decidir se quer ou não assinar o contrato. Mesmo assinando o contrato, poderá decidir ou não cumprir com o que solicitaremos no contrato, ou seja, conservar a floresta localizada em sua propriedade (mas, lembrando que, nesse caso, não receberá nenhum pagamento em dinheiro no final, ou receberá algum pagamento, mas com os descontos estabelecidos no contrato do tipo Flexível). Seu nome e outros dados pessoais não serão divulgados fora da equipe do estudo, em nenhuma etapa do estudo, ou mesmo no futuro. Os dados

serão analisados somente pela equipe do estudo. A equipe do estudo transformará os nomes em números, de modo que só sejam identificáveis pela própria equipe.

Sua participação é essencial para este estudo e queremos lembrar que não existe resposta certa para nenhuma pergunta que iremos fazer. Vamos sempre respeitar sua opinião, costumes, crenças, visão de mundo e sua vivência.

Se, durante o desenvolvimento do estudo, você ou sua propriedade forem filmados e fotografados, você:

☐ concorda que elas poderão ser apresentadas em publicações científicas, aulas, palestras e materiais de divulgação do projeto.

☐ não concorda que elas sejam incluídas em qualquer tipo de publicação ou material de divulgação.

E se eu precisar falar com alguém sobre esse estudo?

Para dúvidas, você pode ligar ou enviar um e-mail à nossa equipe. Telefones para contato: (68) 3223-1036 e (68) 9 9281 4417. E-mail: arendadafloresta@sosamazonia.org.br.

2. TERMO DE CONSENTIMENTO LIVRE, APÓS ESCLARECIMENTO

Eu,_____, declaro que concordo em participar voluntariamente deste estudo e que fui esclarecido sobre a natureza da pesquisa, seus objetivos, métodos, benefícios previstos e potenciais riscos. Afirmo que aceitei participar por minha própria vontade, sem sofrer qualquer tipo de coação ou constrangimento, sem receber qualquer incentivo prévio financeiro, e sem ter qualquer ônus, com a finalidade de colaborar com o sucesso do estudo. Fui ainda informada(o) de que posso me retirar desse estudo/pesquisa a qualquer momento, sem precisar esclarecer meus motivos, sem qualquer prejuízo ou sofrer quaisquer sanções ou constrangimentos, e de que posso entrar em contato com as/os pesquisadores responsáveis pelo estudo a qualquer momento pelos telefones e e-mails que constam neste documento.

Confirmo que recebi uma cópia assinada deste Termo de Consentimento Livre e Esclarecido.

Local: _____Data _____/_____/_____.

Assinatura do participante: _____

Assinatura do pesquisador: _____

Via 1/2 - Participante

CONTRATO PSA/SOS AMAZÔNIA/ARDF Nº ____ /2021

A RENDA DA FLORESTA

CONTRATO DE PAGAMENTO POR SERVIÇOS AMBIENTAIS (PSA)

CONTRATO TIPO FIXO

Por meio deste acordo particular:

A **ASSOCIAÇÃO SOS AMAZONIA**, pessoa jurídica de Direito Privado inscrito no CNPJ/MF sob o nº 14.364.434/0001-85, situada na Rua Pará, nº 61, Bairro Habitasa na cidade de Rio Branco-Acre, neste ato representado por seu Secretário Técnico **Alisson Sobrinho Maranho,** brasileiro, inscrito no CPF/MF nº 938.646.042-49, residente e domiciliado na rua José Maria Rios, nº 412, apto 01, bairro Santa Quitéria, Rio Branco Acre e, por sua Secretária Administrativa **Gabriela Silva de Souza**, brasileira, inscrita no CPF/MF nº 895.043.482-20, residente e domiciliado na Rua Casa Nova I, 20, Baixa da Colina, 69.901-319, Rio Branco Acre, neste contrato denominada "SOS AMAZÔNIA",

O **INSTITUTO NACIONAL FRANCÊS DE PESQUISA EM AGRICULTURA, ALIMENTAÇÃO E MEIO AMBIENTE (INRAE)**, pessoa jurídica de Direito Público, inscrita no VAT FR 57 180 070 039 – APE 7219Z, localizada em Centre Occitanie-Montpellier, 2 Place Pierre Viala, 34060 Montpellier Cedex 2, França, representado por sua Diretora de Pesquisa, **Julie Subervie**, neste contrato denominado "INRAE", e

_____, devidamente inscrita(o) no CPF sob nº _____ e no RG nº _____ (órgão de expedição _____), com endereço na/no _____, no município de _____ no Estado do Acre, CEP _____, doravante denominado "PROPRIETÁRIO DA TERRA"

Firmam entre si o presente **CONTRATO DE PAGAMENTO POR SERVIÇOS AMBIENTAIS (PSA)**, que será regido pelas seguintes Cláusulas e Condições:

CLÁUSULA PRIMEIRA – DO OBJETO DO CONTRATO

O presente Contrato tem por objeto a conservação da floresta existente na propriedade do provedor de serviços ambientais (PROPRIETÁRIO DA TERRA), de forma voluntária, conforme os objetivos do programa piloto "A Renda da Floresta", implementado pela SOS AMAZÔNIA, em parceria com o Instituto Nacional Francês de Pesquisa em Agricultura, Alimentação e Meio Ambiente (INRAE).

A Renda da Floresta é um projeto de pesquisa, experimental, que tem o objetivo de implementar um programa piloto de Pagamento por Serviços Ambientais (PSA) na Amazônia brasileira. Esse Programa compensará proprietários de terra pelos esforços de conservar suas florestas. Por meio

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desse estudo, será analisado se os participantes estão dispostos a receber dinheiro em troca da conservação da floresta e se esse incentivo financeiro trará resultados efetivos na região.

Parágrafo único: O Termo de Consentimento Livre e Esclarecido assinado pelo PROPRIETÁRIO DA TERRA, confirmando seu interesse em participar do estudo, é parte integrante deste Contrato.

CLÁUSULA SEGUNDA – DA ÁREA OBJETO DO CONTRATO

As partes, em comum acordo, definem o modo como será definido o perímetro da área que será objeto do contrato de Pagamento de Serviços Ambientais (PSA):

Parágrafo primeiro: A definição da área objeto do contrato será realizada na visita de assinatura do contrato de Pagamento de Serviços Ambientais (PSA), ocasião em que o proprietário da terra em conjunto com um membro integrante da SOS AMAZÔNIA definirão os limites da propriedade rural através do uso do software de mapeamento QGis, assim como os pontos limítrofes (coordenadas geográficas) através de uma ata. Tal delimitação tomará como base, inicialmente, os limites estabelecidos no Cadastro Ambiental Rural (CAR) da propriedade, porém com possíveis alterações desses limites para que o mapeamento da área do contrato seja condizente com a real delimitação da propriedade, conforme relatado pelo proprietário.

Parágrafo segundo: A definição da área será realizada mediante assinatura de ata, contendo as coordenadas geográficas definidas para a área mapeada, com assinatura das partes envolvidas no presente contrato.

Parágrafo terceiro: Não poderá ser objeto de contestação a definição da área, após assinatura da ata e do respectivo contrato.

CLÁUSULA TERCEIRA – DA PREVISÃO LEGAL

O Pagamento por Serviços Ambientais (PSA) está legalmente previsto na Lei Federal nº 14.119/2021, a qual institui a Política Nacional de Pagamento por Serviços Ambientais (PNPSA).

CLÁUSULA QUARTA – DO PRAZO DO CONTRATO

Este acordo terá a duração de 14 (quatorze) meses, contados da data de assinatura, sendo 12 (doze) meses o período em que o PROPRIETÁRIO DA TERRA deverá cumprir o compromisso voluntário de proteger sua parcela de floresta, e, após esse período, 2 (dois) meses de prazo para que a SOS AMAZÔNIA efetue o pagamento (PSA) do valor acordado, caso verificado o cumprimento.

CLÁUSULA QUINTA – DO DIREITO DE PROPRIEDADE

Este acordo não implica em nenhum direito de propriedade ou expropriação de terra por parte da SOS AMAZÔNIA, do INRAE ou qualquer de seus parceiros do Programa A Renda da Floresta sobre a(o) PROPRIETÁRIO DA TERRA que presta o Serviço Ambiental, sendo o seu direito de propriedade inviolável.

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Via 1/2 - Participante

CLÁUSULA SEXTA – DA COMPENSAÇÃO PELA CONSERVAÇÃO DA FLORESTA

Por meio deste acordo, o PROPRIETÁRIO DA TERRA concorda em receber, ao final do contrato, o valor de R$ _____ (_____) pela preservação integral da área de floresta existente em sua propriedade.

Parágrafo primeiro: A SOS AMAZÔNIA penas pagará a compensação financeira caso toda a sua área de floresta seja preservada até o final do Contrato. Ou seja, se for constatado que o PROPRIETÁRIO DA TERRA desmatou parte da área de floresta, mesmo que mínima, o pagamento não será realizado.

Parágrafo segundo: A propriedade rural com a área de floresta a ser preservada durante o estudo está localizada em: _____

Parágrafo terceiro: A área total de floresta existente na propriedade do PROPRIETÁRIO DA TERRA será estimada, pelo INRAE, por imagens de satélite com base no perímetro da área que será objeto do contrato, definida em conjunto entre um membro integrante da SOS AMAZÔNIA e o PROPRIETÁRIO DA TERRA e registrada em ata, conforme CLÁUSULA SEGUNDA.

Parágrafo quarto: A área florestal será monitorada pelo INRAE por meio de imagens de satélite, o que servirá de base para a verificação do cumprimento dos compromissos voluntários do PROPRIETÁRIO DA TERRA. Ou seja, ao final do Contrato, será feita uma análise comparativa da situação da cobertura de floresta da propriedade no decorrer de 12 (doze) meses, para constatar se o Pagamento por Serviços Ambientais será pago e em quais condições.

CLÁUSULA SÉTIMA – DOS COMPROMISSOS VOLUNTÁRIOS DO PROPRIETÁRIO DA TERRA

O PROPRIETÁRIO DA TERRA é a parte provedora de serviços ambientais e deverá cumprir, pelo período de 12 (doze) meses, os seguintes compromissos voluntários para conservar sua área de floresta e, ao final do contrato, poder receber o Pagamento por Serviços Ambientais (conforme definido na CLÁUSULA QUINTA):

1. Tomar as medidas que estejam ao seu alcance para proteger a integridade da área de floresta;

2. Não cortar a área de floresta a ser protegida para o desenvolvimento de atividades econômicas, como agricultura e pecuária;

3. Não fazer uso de fogo na área de floresta a ser protegida, bem como prevenir contra possíveis incêndios florestais; e

4. Informar seus dados bancários e pessoais necessários para que seja possível realizar o pagamento da compensação ao final do Contrato, se for o caso. Se o PROPRIETÁRIO DA TERRA ainda não possuir uma conta bancária em seu nome no momento da assinatura, deverá informar à SOS AMAZÔNIA no prazo de 6 (seis) meses, por e-mail ou telefone.

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Nome do titular da conta bancária: _____
CPF: _____ Banco: _____
Agência: _____ Conta: □ Corrente □ Poupança _____

Parágrafo único: O PROPRIETÁRIO DA TERRA poderá continuar desenvolvendo atividades que não geram a derrubada de árvores, como, por exemplo, a coleta de produtos florestais não madeireiros.

CLÁUSULA OITAVA – DAS OBRIGAÇÕES DA SOS AMAZÔNIA

Neste acordo, a SOS AMAZÔNIA é a parte pagadora de serviços ambientais e possui as seguintes obrigações:

1. Realizar o pagamento da compensação financeira à título de Pagamento por Serviço Ambiental no prazo de 2 (dois) meses a contar do término do período de 12 (doze) meses em que o PROPRIETÁRIO DA TERRA deverá preservar sua área de floresta; e

2. Estar à disposição sempre que o PROPRIETÁRIO DA TERRA desejar tirar dúvidas acerca do acordo ou do estudo sendo implementado.

CLÁUSULA NONA – DAS OBRIGAÇÕES DO INRAE

Neste acordo, o INRAE é a instituição de pesquisa parceira na implementação do Programa e possui as seguintes obrigações:

1. Realizar análises por meio de imagens de satélite para estimar a área de floresta existente na propriedade e monitorar a área durante a vigência do contrato;

2. Estar disponível para esclarecer ao PROPRIETÁRIO DA TERRA, sempre que necessário, sobre o andamento do estudo e quaisquer dúvidas que surjam durante a implementação do contrato, reiterando que o vínculo estabelecido pelo presente Contrato se trata de uma pesquisa, de modo a fortalecer a participação, seja por telefone, e-mail ou outro meio de comunicação direto (conforme o Termo de Consentimento Livre e Esclarecido); e

3. Enviar relatórios quando solicitado pelo PROPRIETÁRIO DA TERRA para informar sobre os dados de monitoramento de sua propriedade, onde devem constar as evidências de cumprimento ou não cumprimento dos compromissos voluntários quanto à conservação da floresta.

Parágrafo único: Os dados coletados referentes à área florestal do PROPRIETÁRIO DA TERRA serão usados exclusivamente para subsidiar a pesquisa e este Contrato, sendo mantidos de forma sigilosa, e nenhuma informação será compartilhada com terceiros. Nenhum dos dados coletados e analisados durante o estudo se destinam ao compartilhamento com órgãos de fiscalização ambiental.

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Via 1/2 - Participante

CLÁUSULA DÉCIMA – DO NÃO CUMPRIMENTO E DA RESCISÃO DO CONTRATO

1. Caso o PROPRIETÁRIO DA TERRA não realize o serviço ambiental desejado neste Contrato, ou seja, se não cumprir com seu compromisso voluntário de conservar a floresta, não receberá nenhuma compensação financeira.

2. O PROPRIETÁRIO DA TERRA não sofrerá nenhuma penalidade contratual nem terá de arcar com nenhum ônus em razão do descumprimento de seu compromisso, pois este acordo é voluntário e não compulsório. Caso não cumpra o acordo, a depender do descumprimento, apenas não receberá parte ou a totalidade do valor oferecido, conforme a CLÁUSULA QUINTA.

3. Assim, o PROPRIETÁRIO DA TERRA, se desejar, poderá rescindir este Contrato a qualquer momento. Neste caso, a SOS AMAZÔNIA solicita que seja informada por e-mail ou telefone, preferencialmente, em até 7 (sete) dias após a decisão ser tomada.

CLÁUSULA DÉCIMA PRIMEIRA – DA PROTEÇÃO DE DADOS

A SOS AMAZÔNIA e o INRAE, por si e por seus colaboradores, obrigam-se a atuar neste Contrato e no bojo do estudo em conformidade com a Legislação vigente sobre Proteção de Dados Pessoais e as determinações de órgãos reguladores/fiscalizadores sobre a matéria, em especial a Lei 13.709/2018, além das demais normas e políticas de proteção de dados de cada país onde houver qualquer tipo de tratamento dos dados do PROPRIETÁRIO DA TERRA. O Termo de Consentimento para Coleta e Uso de Dados deve ser assinado pelo participante e é parte integrante deste Contrato.

CLÁUSULA DÉCIMA SEGUNDA– DAS DISPOSIÇÕES GERAIS

Tendo em vista que este acordo consiste em um projeto de pesquisa experimental, sendo todas as obrigações dele provenientes meramente voluntárias, sem contraprestação e por tempo determinado, não há qualquer vínculo entre as partes para fins da legislação trabalhista e tributária, bem como não há uma prestação de serviço propriamente dita, mas somente o compromisso voluntário de não desmatar a área de floresta, com o objetivo de promover serviços ecossistêmicos.

E assim, as partes assinam o Contrato em duas vias, na presença de duas testemunhas, para que produza efeitos, almejando que este acordo traga benefícios para ambas as partes, bons resultados para o estudo e leve mais consciência ambiental para todos que o conhecerem ou dele se beneficiarem.

_____ -AC, _____ de _____ de 2021

INRAE SOS AMAZÔNIA A RENDA DA FLORESTA

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General Conclusion

This dissertation examines the effectiveness of PES programs in the Brazilian Amazon using impact analysis methods, focusing on initiatives aimed at addressing the issue of tropical deforestation. The first two chapters of this dissertation examine the effectiveness of a Brazilian PES-based REDD+ project: the Sustainable Settlements in the Amazon (SSA) program. The third chapter, presents the results of an experimental auction that had the aim of eliciting the individual costs of bringing farmers into compliance with two PES schemes that differ in their requirements.

The results of the first chapter suggest that the SSA program was effective in reducing deforestation during the early years of its implementation suggesting that PES-based REDD+ programs targeting smallholders in the Brazilian Amazon may well be effective, at least in the short-run. Moreover, evidence suggests that farm-holds located close to participants were somehow impacted by the program, as they also decreased deforestation during the early years of program implementation. This suggests that PES programs may change the behavior of farmers who are not the primary beneficiaries of the program – although we are not able to determine through which channel this occurs. More importantly, we found that participants resumed their normal level of deforestation at the end of the program, suggesting that the REDD+ initiative did not have a long-term impact on farmers' deforestation decisions. Despite this, we found no sign of participants catching up on prevented deforestation, suggesting that the program's effects were still sustained – at least until the date of our evaluation. Our findings therefore show that even when forest loss resumes at post-intervention, a net environmental gain will still exist as long as the beneficiaries do not start to deforest more than the reference scenario. Finally, we calculated the delayed CO_2 emissions highlighted by the impact assessment and found that the SSA program climate benefits were greater than its costs.

Given the ongoing debate about exaggerated REDD+ projects baselines, it is known that even effective REDD+ projects tend to have inflated estimations of additionality and are likely to generate "phantom carbon credits" that do not represent genuine carbon reductions. Indeed, the additionality estimates found in

our analysis are significantly lower than those reported by the project implementer, which are based on a before and after comparison. This is consistent with the findings of West et al. (2020), which demonstrate that REDD+ projects have exaggerated baselines, which can result in hot air being traded in offset markets. However, this is not the case in the present study, as the SSA project was funded by the Amazon Fund and did not sell carbon credits in voluntary carbon markets. Nevertheless, since long-term counterfactual evaluations are rare, we still need more assessments to increase our confidence about how much these findings on net environmental gain of PES-based REDD+ can be generalized to other programs.

The main findings of Chapter 2 suggest that the decrease in deforestation has happened mostly as a result of a slowing in the expansion of grazing lands. When we investigated whether this decrease in pasture expansion had a negative impact on cattle herds, one of the region's main sources of income, we found that the cattle stocking rate has actually increased on farms benefiting from the program, indicating that farmers adapted to the new constraints by intensifying cattle ranching activities. Furthermore, our results indicate that the program had a positive impact on farmers' incomes and on alternative livelihood production activities that require less area for production than extensive livestock farming and slash-and-burn agriculture. Finally, according to our results, the development alternative activities does not seem to have had a significant effect in the short term, but rather in the long term. This suggests that when program participants adopt activities that require a greater mobilization of techniques, knowledge and resources, it takes time for their effects to become observable through the data but these can be expected to have lasting effects..

Regarding the debate over the impact of REDD+ projects on people's livelihoods (Sills et al., 2017; Blundo-Canto et al., 2018), our findings suggest that local REDD+ programs that combine PES with technical assistance and support to farmers for the adoption of low-impact activities can be effective in the fight against climate change, without jeopardizing the livelihoods of local populations. Our findings also show that a number of households are ready to adopt new agricultural practices, while maintaining their traditional ones. The question of whether the coexistence of both types of production systems is sustainable over time or not remains open. In any case, the transmission of technical knowledge necessary for the development of environmentally sustainable activities was effective and it cannot be ruled out that the participants who have acquired this new knowledge during the program will use it in the future.

The third chapter of the thesis provides the results of an experimental auction that aimed at eliciting the individual costs of bringing farmers into compliance with two PES schemes that differ in their requirements - one being more flexible than the other. Our results of the elicited forest conservation WTA for each type of

contract are consistent with theoretical expectations, with the WTA being higher on average for the more restrictive contract. When we compared the estimates obtained with our experiment of farmers' WTA and the benefits for avoiding CO_2 emissions, we found that private costs were lower than the social costs of carbon (SCC). Moreover, by combining revealed-preference WTA data with household survey variables, we found that a number of household characteristics, farm characteristics and socioeconomic variables are likely to drive the way participants bid their minimum WTA for forest conservation. Moreover, we found that two fairly precise farmer typologies emerged from the experiment: on the one hand, poor households practicing traditional agriculture based on fallow land and subsistence farming, and on the other hand, wealthier households whose agricultural activities rely on highly deforestation-dependent land uses (e.g., extensive cattle-ranching). Although spread over quite a small area, the private costs of forest conversation of the two groups differ significantly. They have different WTAs, on average, regardless of the contract offered, with subsistence crop farmers, who are less dependent on deforestation, systematically exhibiting lower WTA than cattle ranchers. In addition, and this is again consistent with the typology, the two groups value the contract types quite differently; the flexibility gap between the two contracts translates into a 50 percent higher bid by cattle ranchers than by crop farmers.

Although the design of environmental policy allocation mechanisms has received considerable academic attention in the literature (Jack, Leimona, and Ferraro, 2009; Jack, 2013), this is the first experimental auction to investigate the allocation problem in a forest conservation environmental market. Our findings could be used to help design PES forest conservation contracts in the Amazon, since our results suggest that measuring WTA provides a key in-put for shaping pricing policy and guiding the magnitude and targeting of forest conservation PES contracts in the Amazon. Furthermore, although further research from the field is warranted to determine which type of PES contract is ultimately most cost-effective, our ex-ante cost-benefit estimates can help policymakers compare the PES program's cost to other options for reducing global carbon emissions.

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Appendix

During the completion of this thesis, I also had the chance to participate on other projects relating to the theme of this PhD. The following manuscripts are enclosed in this section:

1. ***Permanence of avoided deforestation in a Transamazon REDD+ initiative**, by Cauê D. Carrilho, Gabriela Demarchi, Amy E. Duchelle, Sven Wunder, and Carla Morsello*

2. ***Enforcement and inequality in collective PES to reduce tropical deforestation: Effectiveness, efficiency and equity implications**, by Julia Naime, Arild Angelsen, Adriana Molina-Garzon, Cauê D. Carrilho, Vivi Selviana, Gabriela Demarchi, Amy E. Duchelle, and Christopher Martius*

3. ***Farmers' preferences for water-saving strategies in Brazilian eucalypt plantations**, by Gabriela Demarchi, Julie Subervie, Fernando Palha Leite, and Jean-Paul Laclau*

Permanence of avoided deforestation in a Transamazon REDD+ initiative (Pará, Brazil)

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Highlights

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

Abstract

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

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

1. Introduction

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

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

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

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

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

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

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

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

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

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

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

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

Indeed, the two published evaluations of REDD+ outcomes' permanence that we are aware of found support for our default scenario. The first is the aforementioned quasi-experiment showing that Guyana's national REDD+ program, funded by Norway through performance-based payments

for reducing deforestation, decreased tree cover loss during the implementation period (2010-2015) (Roopsind et al., 2019). Yet, right after Norway's payments ceased, in 2016, the authors detected an increase in tree cover loss, which suggested forest protection was not guaranteed without continued payments. The second is a follow-up evaluation of the forest outcomes from the carbon-focused project in Uganda. Implemented between 2011 to 2013, the program had reduced deforestation substantially (Jayachandran et al. 2017). However, using satellite imagery from 2016, World Bank (2018) showed the previous PES recipients resumed forest clearing at similar rates to control group households after payments had ended; yet, importantly, without 'catching up' with the initial curve, i.e. leaving the temporary, payment-induced conservation gains intact.

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

In the case of using PES as an adoption subsidy for environmentally more benign practices that pay off for land stewards ('asset-building' PES), the prospects for permanence should be better. At least a few case studies seem to also support this empirically, in particular from the World Bank-supported trinational Regional Integrated Silvo-pastoral Ecosystem Management Project (RISEMP). This carbon- and biodiversity-focused PES program paid landowners between 2003 and 2008 for

the introduction of silvopastoral practices. In Quindío (Colombia), four years after payments ended, these systems had been widely retained (Pagiola et al., 2016). Similar permanence results were reconfirmed again in 2016 (Calle, 2020). The sister silvopastoral program implemented simultaneously in Nicaragua was assessed also in 2012, showing also widespread permanence, although the evidence here is based only on a before-after comparison of various interventions, without a no-intervention control group (Pagiola et al., 2020). The third rollout was in Costa Rica (2002-07), and also here there is about a decade after (2016) evidence of a high degree of permanence (Rasch et al., 2021). The corollary of their findings is that inducing the lasting adoption of more benign land uses is feasible. In the REDD+ context, this could, in turn, self-sustain the deforestation reduction reached by the initiatives.

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

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

of the REDD+ initiative. We then show the longitudinal effects of the REDD+ initiative on both deforestation and people's well-being (Section 5). In Section 6, we explore possible explanations for our estimation results, and in the last section, we present conclusions.

2. The Transamazon REDD+ initiative

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

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

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

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

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

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

b. Properties environmental registration

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

c. Payments for Environmental Services

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

d. Sustainable livelihood alternatives (ICDP component)

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

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

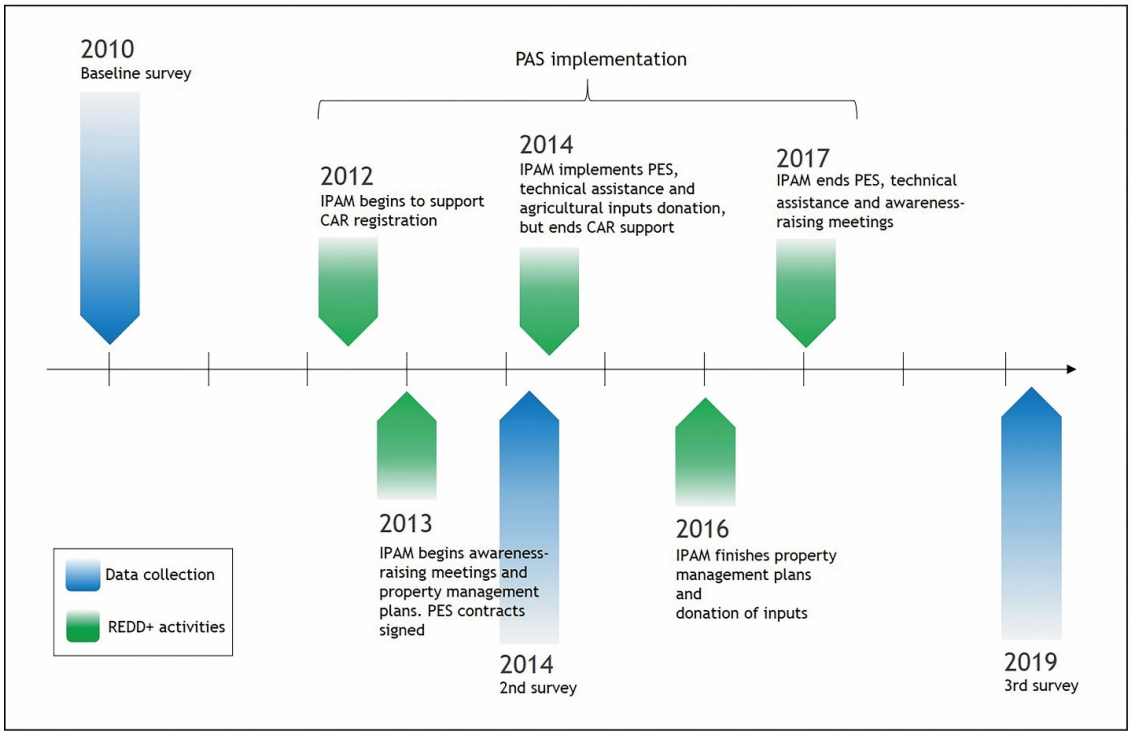

Figure 1 – PAS implementation and CIFOR-GCS data collection timeline. Source: own elaboration, based on Cromberg et al. (2014b) and IPAM (2016).

3. Sampling and data collection

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

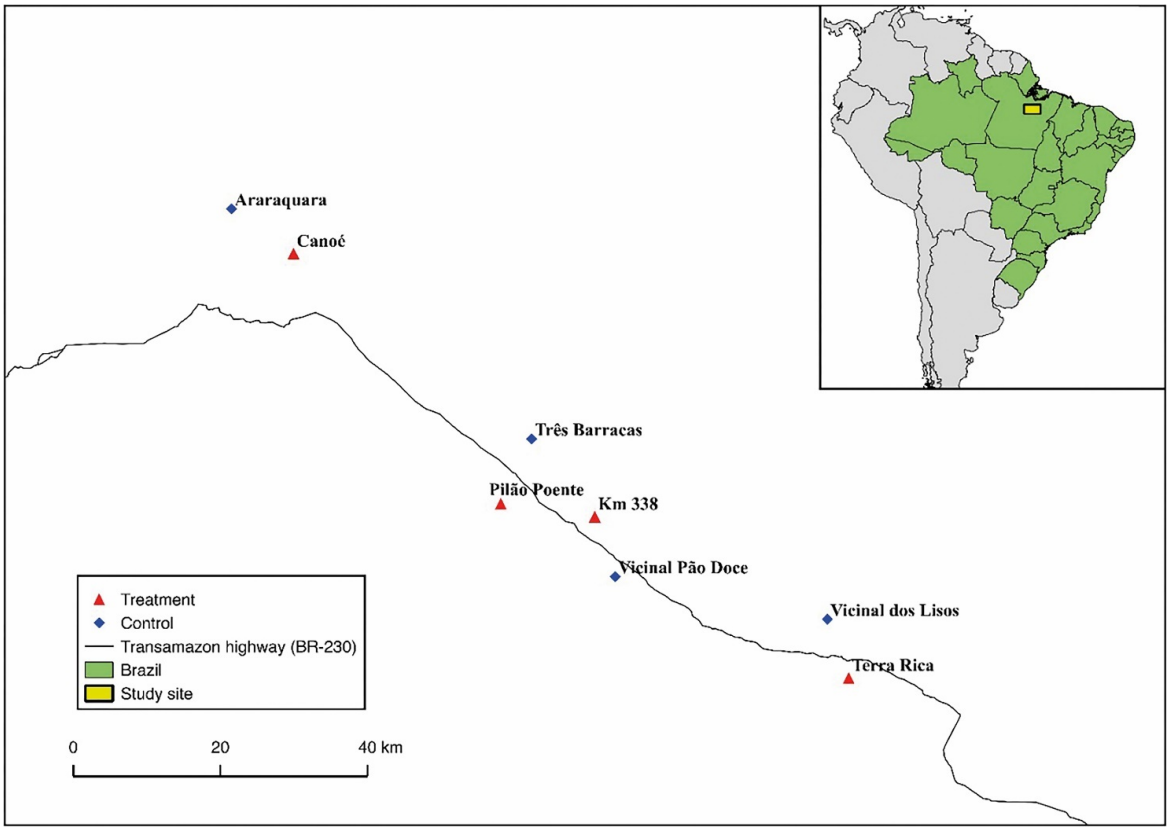

Figure 2 -Location of both treatment and control communities along the Transamazon highway (Brazil).

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

For our analysis, the key unit is the household, defined here as the group of people, usually family members, living under the same roof and pooling resources (Sunderlin et al., 2016). We

chose this level because households make the deforestation decisions, mainly driven by farming, and received REDD+ incentives (e.g., PES, sustainable livelihood alternatives).

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

4. Empirical strategy

4.1. Assessing short-term outcomes

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

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

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

Table 1 – Summary statistics for treatment and control groups.

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

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

As the intervention was not randomly attributed, confounders could be unevenly distributed between treatment and control groups (Ferraro and Pattanayak, 2006). Therefore, besides using DID to control for time-invariant unobserved confounders (Deschenes and Meng, 2018), we minimized selection bias combining DID with matching to preprocess the data and control for observable confounders (Ho et al., 2007). This means we applied the DID estimator to those households from treatment and control groups that were statistically similar in pre-treatment observable characteristics (baseline observable covariates – Table 1) (Imbens, 2004). We adopted the nearest-

neighbor matching estimator, by matching each treated household to two of the most similar control households (Abadie et al., 2004), as well as two robustness checks. For the first, we again used the nearest-neighbor, but this time matching each treated household to four of the most similar controls. The second was the kernel-based propensity score matching, by which we compared households with the closest probability of being treated given the same set of observable covariates we used before (Rosenbaum and Rubin, 1983).

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

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

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

4.2. Evaluating outcomes after the initiative ended

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

4.3. Intra-community spillover

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

5. Results

5.1. Longitudinal impact assessment

Our results show that the REDD+ initiative achieved positive outcomes in terms of both forest conservation and perceived well-being improvement (Table 2). Aligned with the results of the earlier study on the effectiveness of PAS in reducing deforestation (Simonet et al., 2019), ATT for forest cover was significant for all DID matching estimators in the first period (2010-14). This implies an average of 7.80% to 10.32% of forest cover was saved by the initiative, or the equivalent of 6.20 to 8.20 ha per farm, given that the average land area of the treatment group was 79.45 ha in 2014 (Table 1). Moreover, ATT for perceived wellbeing was also positively significant in this first period: the REDD+ initiative increased the probability of improved enrollee perception of wellbeing

by 27% to 44%. Cohens' *d* for forest cover (0.32 – Table 3) and perceived wellbeing (0.37) were numerically similar, both indicating small-to-medium effect sizes according to Sawilowsky (2009) (0.01 = very small; 0.2 = small; 0.5 = medium; 0.8 = large; 1.2 = very large; 2.0 = huge).

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

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

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

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

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

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

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

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

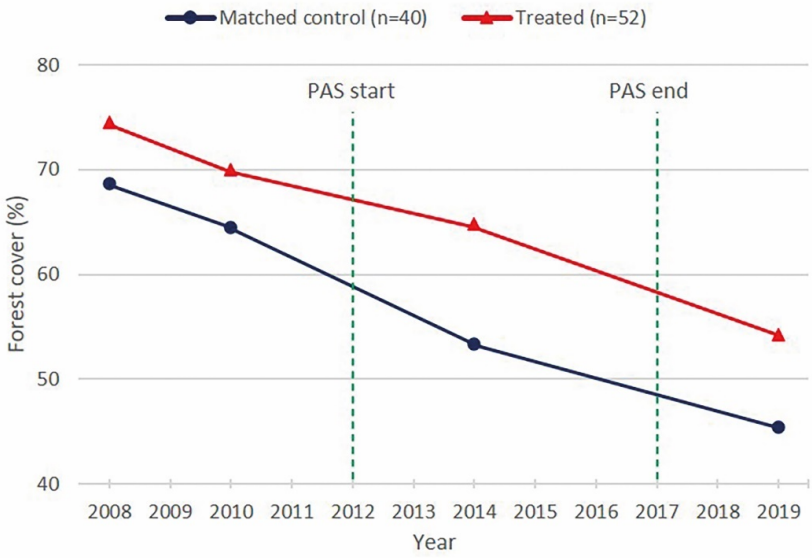

Figure 3 – Forest cover (% of forest of the household property) change among treated and their matched control households.

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

5.2. Intra-community spillover

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

6. Discussion

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

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

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

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

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

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

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

6.2. Long-term effects of the REDD+ initiative

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

Notably, the literature on PES permanence suggested that asset-building PES ('active establishment') may have better chances of locking in forest gains than activity-reducing ('passive

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

7. Concluding remarks

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

As long as the basic environmental externality persists, i.e. that standing forests privately cannot compete with the yields from alternative land uses, we should not expect the miracle that a temporary payment would permanently change the logic of the productive system. ICDP type of

investments try to achieve exactly this type of change, but have typically been little successful, as also seems the case, at first sight, in the PAS initiative we analyzed. PES used as adoption subsidies for environmentally beneficial land uses seem to have a somewhat better record (see Section 1).

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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Enforcement and inequality in collective PES to reduce tropical deforestation: Effectiveness, efficiency and equity implications

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ABSTRACT

Collective Payments for Ecosystem Services (PES), where forest users receive compensation conditional on group rather than individual performance, are an increasingly used policy instrument to reduce tropical deforestation. However, implementing effective, (cost) efficient and equitable (3E) collective PES is challenging because individuals have an incentive to free ride on others' conservation actions. Few comparative studies exist on how different enforcement strategies can improve collective PES performance. We conducted a framed field experiment in Brazil, Indonesia and Peru to evaluate how three different strategies to contain the local free-rider problem perform in terms of the 3Es: (i) Public monitoring of individual deforestation, (ii) internal, peer-to-peer sanctions (Community enforcement) and (iii) external sanctions (Government enforcement). We also examined how inequality in wealth, framed as differences in deforestation capacity, affects policy performance. We find that introducing individual level sanctions can improve the effectiveness, efficiency and equity of collective PES, but there is no silver bullet that consistently improves all 3Es across country sites. Public monitoring reduced deforestation and improved the equity of the program in sites with stronger history of collective action. External sanctions provided the strongest and most robust improvement in the 3Es. While internal, peer enforcement can significantly reduce free riding, it does not improve the program's efficiency, and thus participants' earnings. The sanctioning mechanisms failed to systematically improve the equitable distribution of benefits due to the ineffectiveness of punishments to target the largest free-riders. Inequality in wealth increased group deforestation and reduced the efficiency of Community enforcement in Indonesia but had no effect in the other two country sites. Factors explaining differences across country sites include the history of collective action and land tenure systems.

1. Introduction

Tropical deforestation is the largest source of carbon emissions from Agriculture, Forestry and Other Land Use (AFOLU) activities (IPCC, 2019), also driving biodiversity loss (Gibson et al., 2011) and threatening the livelihoods of local communities (Angelsen et al., 2014). To meet the global climate, biodiversity and sustainable development goals, adequate policies for reducing deforestation need to be implemented at regional and local scales (Ostrom, 2010). Among the set of policy options to reduce deforestation are positive incentives ("carrots"), which aim to increase the welfare of forest users by incentivizing or rewarding their conservation activities, and disincentives ("sticks"), which aim to deter deforestation activities by punishing or increasing the cost of non-environmentally friendly behaviour (Börner et al., 2020).

Payments for Ecosystem Services (PES) programs are positive incentives that reward forest users conditional on conservation performance. They consist on voluntary agreements at the individual or group level, under which the providers agree to supply ecosystem services in exchange for payments (Wunder, 2015). PES are a commonly used tool in the efforts to reduce deforestation (Min-Venditti et al., 2017; Salzman

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et al., 2018) and a key component of Reducing Emissions from Deforestation and forest Degradation (REDD+) initiatives worldwide. Collective PES are characterized by assigning the payment to a group instead of an individual, based on their collective performance (Hayes et al., 2019; Pfaff et al., 2019). Collective PES are preferred when land is managed under collective ownership, when individual actions are hard to identify, or when spatial coordination of conservation activities is particularly important, such as in watershed or biodiversity management (Engel, 2016).

Although collective PES help solve the *global* collective action problem of forest conservation, they face a number of challenges to provide effective, efficient (i.e., cost-effective) and equitable outcomes (3E) (Angelsen and Wertz-Kanounnikoff, 2008) at the local level. First, they create a *local* collective action problem: the individual compensation from collective PES is only partly conditioned on individual behaviour (Hayes et al., 2019). Participants have an incentive to free ride on others' conservation actions, which can decrease the overall effectiveness of the policy as compared to an individual based PES (Gatiso et al., 2018; Hayes et al., 2019; Kerr et al., 2012; Midler et al., 2015; Narloch et al., 2012; Ngoma et al., 2020). Second, a related challenge is to balance conservation costs and benefits in a way that is equitable among program participants (Hayes et al., 2019; Hayes and Murtinho, 2018). Collective PES are likely to be implemented in communities with heterogenous participants in terms of household labour, capital and physical access to forests, which can in turn affect policy performance as well as exacerbate existing inequalities (Andersson et al., 2018b).

Stronger monitoring and enforcement – introducing individual "sticks" with the collective "carrots" – can help navigate these interrelated challenges as it reduces the incentives to free ride. However, strong monitoring and enforcement involves additional implementation costs (Börner et al., 2014). Thus, higher program effectiveness and equity might reduce economic efficiency (Pascual et al., 2010; Wu and Yu, 2017), yet there are few empirical evaluations of such trade-offs . In this article, we compare how different monitoring and enforcement strategies perform in terms of the 3Es in a collective PES. We define *effectiveness* as the degree to which deforestation is reduced from a baseline level. *Efficiency* is the degree to which the monitoring and enforcement achieve conservation outcomes for the least cost, from the perspective of the community members. *Equity* has both a distributional and procedural dimension, and thus includes the distribution of earnings amongst PES participants as well as their fairness perceptions (Lliso et al., 2021; Loft et al., 2017; Pascual et al., 2010).

We conducted a framed field experiment (FFE) in three countries with high forest cover but different local governance contexts: Brazil, Indonesia and Peru. We compare three strategies to reduce the free rider problem in a collective PES: (i) Public monitoring of individual deforestation, (ii) monitoring with peer sanctions (Community enforcement) and (iii) monitoring with external sanctions (Government enforcement). We also evaluate whether inequality in wealth, framed as differences in *deforestation capacity*, affect the performance of a collective PES. Recent research suggests inequality might affect program and institutional performance (De Geest and Kingsley, 2021; Nockur et al., 2021). Even though a number of economic experiments have examined the effects of economic inequality on cooperation (De Geest and Kingsley, 2019; Hauser et al., 2019; Kingsley, 2016; Tavoni et al., 2011), few have tested it with actual natural resource users (Loft et al., 2020; Narloch et al., 2012; Vorlaufer et al., 2017), and none have examined the question across multiple countries.

2. Theoretical background

2.1. Reducing the free-rider problem

Collective PES programs in which it is hard to exclude community members from the benefits of the collective payment are similar to the common-pool resource (CPR) problem; the benefit individuals receive from the group compensation is not proportional to the individual conservation actions (Hayes et al., 2019; Martin et al., 2014). To maximize own net earnings individuals can free ride by appropriating the common pool resource (i.e., deforesting), creating a negative externality on the rest of the group by reducing the collective payment.

A central strategy to reduce free riding is to increase its cost by introducing sanctions. The first type of sanction that we evaluate is the non-monetary sanction of publicly revealing individual deforestation decisions, which can induce guilt or pride (Masclet et al. 2003; Lopez et al. 2012). We also consider two monetary sanctions that can be classified at the opposite sides of a governance spectrum: (i) a centralized, external sanctioning institution, and (ii) a decentralized, internal sanctioning institution in which community members sanction their peers. The experimental literature indicates that in general, when faced with the threat of an external, centralized sanction, participants significantly increase cooperation (Cardenas, 2004; Gelcich et al., 2013; Lopez et al., 2012; Rodriguez-Sickert et al., 2008; Velez et al., 2010; Vollan et al., 2019). This is consistent with non-experimental evidence showing how law enforcement by authorities provides effective results to reduce tropical deforestation (Busch and Ferretti-Gallon, 2017; Tacconi et al., 2019). Even though the expected net benefit of free-riding decreases as the probability of the external sanction increases, experiments show that the probability of the sanctions does not greatly affect their overall effectiveness (Cardenas, 2004; Lopez et al., 2012).

Likewise, experimental studies on CPRs, pioneered by Ostrom et al. (1992), show how peer punishment enhances cooperation (e.g., Cason and Gangadharan, 2015; Chaudhuri, 2011; Kosfeld et al., 2009), also in the context of collective PES (Kaczan et al., 2017). The impact of the punishment depends on the cost of the punishment (Chaudhuri, 2011; Sutter et al., 2010), and the type of punishment – monetary or non-monetary. Social, non-monetary sanctions such as the public revelation of individual decisions can increase cooperation, as it might induce guilt or shame (Lopez et al., 2012; Masclet et al., 2003; Noussair and Tucker, 2005; Pfaff et al., 2019). The experimental studies align with observational studies pointing out the capacity of communities to regulate CPR use (Chhatre and Agrawal, 2008; Ostrom, 1990; Rustagi et al., 2010). Additional factors that increase peer-punishment impact in experiments are communication (Koch et al., 2021; Ostrom et al., 1992), and previous trust and experience (Gelcich et al., 2013; Pfaff et al., 2019).

Both monetary sanctioning strategies have potential shortfalls. External sanctions might undermine the legitimacy and liberty of participating communities, potentially crowding out motivations for cooperative behaviour (Cardenas et al., 2000; Kube and Traxler, 2011; Lopez et al., 2012). Furthermore, in many situations, external regulations and sanctioning are hard to implement, because of costly monitoring, lack of political interest, or corruption (Karsenty and Ongolo, 2012; Sundström, 2015). In turn, when individuals must regulate common-pool resource use on their own, they incur monitoring and enforcement costs. If these costs are too high, they erode the benefits of more cooperation (Ostrom et al., 1992). The effectiveness of each sanctioning strategy has been evaluated in the context of homogenous populations in experimental games (see Vollan et al., 2019), but there is no research evaluating how they perform relative to each other in terms of the 3Es and with heterogenous populations.

2.2. The effect of economic inequality in management of the commons

It has for long been recognized that agent heterogeneity and inequality affects the level of cooperation in social dilemmas, but in ambiguous ways (Agrawal, 2001; Baland and Platteau, 1999). Broadly, three types of inequalities can affect collective action: inequality in wealth or endowments, inequality in interests or incentives, and inequality in identity (Baland and Platteau, 1996)[1]. Critical factors that determine the effect of inequality on commons outcomes include the incentive structure facing the participants (e.g., individual endowments) and the characteristics of the public good, such as whether it creates positive or negative externalities, or whether it offers the same returns to all participants (Dayton-Johnson and Bardhan, 2002).

Inequality has positive effects on collective action if the wealthiest agents face stronger incentives to cooperate, for example, by receiving a larger share of the benefits from the common pool. In such cases, the elite has higher interests in collective action, and thus involve themselves more actively in setting rules and enforcing them (Baland and Platteau, 1999). Similarly, inequality in opportunity costs of conservation of a CPR (i.e., the returns to the best outside option) increases cooperation, as players with more valuable external options put less pressure on the common resource (Cardenas et al., 2002). Further, an increase in wealth inequality leads to reduced deforestation when the demand for the common resource is increasing at a decreasing rate with wealth (Alix-Garcia, 2008). In this case, more inequality entails less overall deforestation because the poor reduce their deforestation more than what the wealthy increase it.

Other evidence suggests that economic heterogeneity has negative effects on the commons. For example, there is less collective action in groups with unequal landholdings (Adhikari and Lovett, 2006; Varughese and Ostrom, 2001), and more deforestation in countries with higher inequality (Ceddia, 2019; Koop and Tole, 2001). Fairness and equity considerations are important determinants of people's behaviours and affect cooperation rates (Almås et al., 2010; Fehr and Schmidt, 1999). In experimental games, inequality in endowments or returns from the public good creates trade-offs between an efficient and an equitable distribution of benefits (Kingsley, 2016; Koch et al., 2021; Nikiforakis et al., 2012). Participants with higher endowments place higher value in efficiency while those with lower returns prioritize equity (Nikiforakis et al., 2012). Inequality in endowments also has negative effects on cooperation by creating distinct social identities (Martinangeli and Martinsson, 2020), decreasing levels of trust or social preferences amongst group members (Andersson and Agrawal, 2011), or reducing the positive effects of communication (Cardenas, 2003; Gangadharan et al., 2017).

In sum, the impact of inequality on the commons greatly depends on the type of inequality, the degree of inequality, the preferences and characteristics of the group, and the broader socioeconomic and institutional context. In observational studies, the effect of economic inequality on commons outcomes is hard to identify, because different types of inequalities interact simultaneously. For example, inequality in endowment coupled with inequality in the marginal benefits from the public good can have positive effects on cooperation, but negative effects when only one type of inequality is present (Hauser et al., 2019; Naidu, 2009). Experimental methods reduce such potential sources of bias. In this paper, we use experimental data to focus on how inequality in wealth, framed as the 'capacity to deforest' affects participation in a collective PES.

[1] Dayton-Johnson and Bardhan (2002) further distinguish between four types of economic inequalities that are relevant in a user group: (i) inequality in wealth or income, (ii) inequalities in the sacrifices that community members make in cooperative arrangements, (iii) inequalities in the benefits they derive from public good, and (iv) inequalities from outside "exit" opportunities.

3. Methods

3.1. Framed field experiments and the study sites

Framed field experiments (FFEs) engage real stakeholders who have experience with the problem at hand. They recreate the decision-making situation in a controlled, hypothetical setting but with real (cash or in-kind) incentives, thus serving as a testbed of alternative real-world policy interventions (Shreedar et al., 2020). Participants bring their own experiences and values, which increases the external validity of the results (Anderies et al., 2011; Cardenas and Carpenter, 2008; Finkbeiner et al., 2018; Gelcich et al., 2013). FFEs never fully capture all the nuances of the actual field settings, but they offer the advantage of manipulation and random assignment of treatments in a controlled setting (Ostrom, 2006), and allow for replication and direct comparison among different groups or samples. While it is impossible to capture the precise magnitudes of the treatments that could be observed in natural environments, the significance and direction of the effects in field experiments are relevant to capture (Kessler and Vesterlund, 2015). Simplified experimental games help identify general principles and patterns of behavior.

An important question of collective PES is how they perform in different local governance contexts (Hayes et al. 2019). The three sites selected for the study in Pará (Brazil), Central Kalimantan (Indonesia) and Ucayali (Peru), have characteristics that make them relevant for a comparison of the effects of a collective PES under different sanctioning institutions. At the country level, the selected villages share similar socioeconomic and institutional characteristics, such as drivers of deforestation and poverty levels (Sills et al., 2017). However, the country sites show differences in local reliance on forests and land tenure systems. Forests are owned communally in the Peruvian site, in the Indonesian site the land is owned by the state, while at the site in Brazil land is owned individually by colonist farmers. In the Peruvian and Indonesian sites, households have community level institutions for collective decision-making, while in Brazil there are no such institutions. Households control, on average, an area of ~2.0 ha for subsistence and commercial agriculture in the Peruvian and Indonesian sites, while in the Brazilian site, households control, on average, an area of 44.8 ha of forest and 38.7 ha of agricultural land, mostly pastures. In Brazil and Peru land tenure is in most cases considered secure, in the sense that collective and individual boundaries of properties are legally recognized. On the contrary, tenure is considered weak in the Indonesian site because village and households do not have legal recognition of the land they manage and forest access is based on local customary laws, which give individuals land claim when they have invested on that land (e.g., planting, clearing land) (Sills et al., 2014). Furthermore, deforestation activities by smallholders serve different economic purposes. In Indonesia, the production is mostly for subsistence consumption, while in Peru, and even more so in Brazil, it is conducted for market purposes. Average household deforestation is higher in Brazil (1.8 ha yr^{-1}) than in Peru (0.43 ha yr^{-1}) and Indonesia (0.04 ha yr^{-1}). Agricultural income share is higher in Peru (20.3%) than in Brazil (16.2%) and Indonesia (9.7%), while the livestock income share is much higher in the Brazilian site (47.4%) than in the Peruvian (6.4%) and Indonesian (4.7%) sites. Income inequality is highest in Brazil, while inequality in assets and land is highest in Indonesia (see Supplementary Information (SI), section B4 for a detailed description of the study sites).

3.2. The basic experimental set-up

The FFE was implemented with 720 participants in 24 villages between October 2019 and January 2020, equally split between the three country sites. Five experimental sessions were conducted in each village, summing up to 30 participants per village (see SI, section B4). The average age of the participants was 44 years, and 52% of them were men.

In the experiment, a group of six forest users shared access to a forest under a collective PES. In each round the participants simultaneously chose how many forest plots they would transform to agricultural land (croplands and pastures). Individual earnings depended on how many plots each participant had deforested and on how many forest plots were left standing once all participants had made their decisions. This framing is relevant for how collective PES operate on the ground: in many cases, benefits are distributed equally amongst participants, while cooperation and willingness to join varies amongst them (Hayes et al. 2019). We introduced the collective PES in the baseline stage, therefore we did not evaluate the additionality of the collective PES as compared to a pure open-access situation, as the topic has been well explored in other experimental studies (Andersson et al., 2018a; Handberg and Angelsen, 2019; Kaczan et al., 2017; Moros et al., 2019; Ngoma et al., 2020). Rather, we focused on identifying and comparing strategies to mitigate the local free-rider problem identified in collective agreements.

The experiment consisted of four stages with six rounds each. In the first stage, we introduced the baseline with the collective action problem. With a total stock of forest plots equal to S, and given the maximum allowed number of plots to deforest $\overline{x_i}$, the monetary pay-off during the baseline stage for participant i in round t was:

$$\pi_{it} = x_{it} + \delta(S - x_{it} - \sum x_{-it}) \; ; \; x_i \leq \overline{x_i} \tag{1}$$

The two conditions necessary for creating a social dilemma are that: (i) the return of deforestation of forest land x_{it} is higher than the individual return of the collective PES ($\delta < 1$), and (ii) the individual return from deforestation is lower than the group benefits from the collective PES ($n\delta > 1$), with n being the number of forest users. Thus, the parameters must satisfy the condition $\delta < 1 < n\,\delta$. The levels of the parameters were set at $S = 60$, and $\delta = 0.4$. We specified that each forest plot was equivalent to 0.5 ha. Considering individual pay-off maximizing users, the Nash Equilibrium, defined as the set of strategies where no one has an incentive to change their behaviour, occurs when everyone maximizes deforestation. However, from the perspective of the group, the best strategy is when there is no deforestation at all, as it yields higher returns than the Nash equilibrium. Thus, self-maximizing individual strategies lead to outcomes that are not socially optimal and lower individual earnings.

Inequality in wealth, or in the "capacity to deforest", was introduced by modifying the maximum number of forest plots that a participant could convert to agricultural land. Our inequality treatment was framed in terms of household's differences in capital needed to establish agricultural plots. In half of the experimental sessions, the *Unequal* groups, three randomly chosen "low capacity" participants could deforest a maximum of four plots (equivalent to 2 ha), and three "high capacity" participants could deforest up to eight plots (4 ha). In the *Equal* groups, all participants had a "medium capacity" to deforest six plots (3 ha). To strictly focus on the effects of inequality in wealth (i.e., individual endowments), the same aggregate deforestation capacity was maintained in Equal and Unequal groups. Further, the marginal benefits of deforestation were kept constant and equal across participants. Hence, the cooperation incentives were the same for every participant.

A major rationale for implementing collective PES is that it allows to reduce the monitoring and enforcement costs as compared to individual PES. We thus assumed that the group deforestation was perfectly monitored, and PES was fully enforced at the group level. This also allowed to make the experiment more easily understood by participants. Throughout the experiment the PES payment was distributed equally among participants, as communities with collective PES often distribute the earnings based on an individual basis and on egalitarian principles, not based on individual contributions (Hayes et al., 2019; Robinson et al., 2016). Although payments can be subject to elite capture (Andersson et al., 2018b; Persha and Andersson, 2014), we retain the same return to be able to identify the effect of unequal wealth distribution.

The stock of forestland was reset in every round, to avoid effects due to accumulated forest loss. Each plot of agricultural land was worth 10 points, while each plot of forest gave 24 points to the group, equivalent to 4 points to each player. In other words, the collective PES covers for the opportunity costs of conservation at the collective level, but not at the individual level, creating the social dilemma.

In all sessions, each participant had a payoff table indicating his/her earnings as a function of his/her and others' decisions. Visual support was provided to explain the collective action dilemma, using a cardboard with 60 green squares. Each square represented a forest plot, and showed the group payoff of 24 points, and the individual payoff of 4 points. Whenever deforestation took place, yellow paper stickers indicating the individual payoff of 10 points replaced the green squares. Before the baseline stage started, the structure and procedures of the common-pool resource were carefully explained, and any questions raised were addressed (see section B6 of SI for the script).

Participants knew who the other members of the group were, thus bringing their expectations and relationships with each other to the experiment. Individual actions remained anonymous to avoid post-experimental effects, such as retaliation, and to better capture individual preferences without the confounder of social pressure. While some individual deforestation decisions in real life can be visible to neighbours and authorities, operating in an anonymous environment is relevant as some decisions are not fully open: for example, when farmers try to "hide" their deforestation by converting forest far from the forest edge.

To conserve anonymity and reduce spillovers throughout the stages, each participant was represented by a letter of the alphabet, only known to the participant and the experimenter, and the letter was changed in each stage. No verbal communication between participants was allowed for multiple reasons. First, communication cannot be assumed *a priori* in our research sites: the study sites do not have the same local institutions that allow to discuss and collaborate. Further, verbal communication is a well-researched treatment found to increase cooperation in experiments (Chaudhuri, 2011; Ostrom, 2006), also in the context of collective agreements (Midler et al., 2015; Rodriguez et al., 2021). Experiments are most useful when they incorporate prior knowledge (Ludwig et al., 2011). The comparative impacts of increasing monitoring and enforcement in collective PES is less explored. Without communication we were able to clearly identify individual motivations to respond to different types of sanctions. Finally, no verbal communication reduced the risk of losing anonymity during the experiment by revealing own decisions or deforestation capacity.

3.3. The monitoring and enforcement treatments

Our treatments were implemented sequentially: in the second stage, after the baseline, we introduced Public monitoring. During this stage, once participants had chosen how many forest plots to deforest, the number of plots deforested by each was publicly revealed using their secret letter. The Public monitoring treatment allowed to explicitly separate the effect of two key elements of environmental governance that are often merged in experimental games: monitoring and sanctioning (Andersson et al., 2014). This allowed to evaluate whether there is an effect of just increasing the amount of information available to players through announcing individual conversion. One of the central mechanisms by which communication affects cooperation is by filling gaps in knowledge about future intentions of others and allowing participants to adjust their expectations (Cardenas et al., 2004). In that sense, the individual level monitoring introduced in stage two (and kept throughout the following stages), served as non-verbal communication, as participants could adjust expectations after seeing others' individual decision and not just the aggregate.

For the third and fourth stages, we alternated between first introducing Community enforcement, followed by Government enforcement, or vice-versa (see Fig. S5 in SI). This allowed to control for spill-over or

learning effects from the two treatments. The Community enforcement treatment recreated a self-enforced collective PES, in which community members themselves could choose to sanction each other. This treatment captures the individual motivations to engage in self-enforcement. Self-enforcement involves some individual-level costs, that can be monetary or non-monetary, such as the time spent on monitoring activities, to report a non-cooperative individual, or the cost of bringing it up in a community assembly.

The Community enforcement stage consisted of two steps. The first step was identical to the Public monitoring stage. In the second step, each participant chose whether or not to assign a punishment to other participants. Assigning a punishment had a cost of 10 points for the punisher but it subtracted 30 points to the punished participant. This punishment-cost ratio (3:1) follows common practice in experimental games (Chaudhuri, 2011; Vollan et al., 2019). To avoid excessive punishment, the maximum number of allowed punishments in each round was limited to three, and each punishment had to be assigned to a different participant. Information about the punisher and punished participants in each round were made public by using their secret letters. This procedure allowed retaliation and reputation building, while maintaining anonymity.

The Government enforcement treatment recreated a policy-mix scenario, in which a collective PES is implemented along with an external enforcer who randomly monitors individuals and assigns sanctions to those who deforest. The treatment allows to identify the benefits of a 'hybrid approach' to forest conservation (Lambin et al., 2014). Individual level enforcement can operate even if PES benefits are provided at the collective level, but it is likely to be more costly than the aggregate level, and thus not fully enforced. During this stage, a probabilistic exposure to a third-party sanction was introduced, representing imperfect government enforcement (Cardenas et al., 2000; Velez et al., 2010). This is considered to be a better representation of the weak and costly forest enforcement that exists in most tropical forest countries (Robinson et al., 2010). The inspection probability for each participant was 1/3, and if inspected, for each plot deforested they lost 15 points. The sanction was non-deterrent as the expected benefit of deforestation was still higher than the one from conservation (i.e., it did not change the optimal strategy for a risk neutral participant). Government enforcement was costless to participants because in real-world scenarios smallholders cannot decide on the stringency and provision of government enforcement. For a detailed description of the payoff functions and optimal strategies in each stage, see SI (section B1).

3.4. Hypotheses

Given that non-monetary considerations can motivate cooperative behaviour (Lopez et al., 2012; Masclet et al., 2003), and that cooperation is often conditional on others' actions (Rustagi et al., 2010), at least two effects of the Public monitoring treatment are conceivable: (i) the display of own non-cooperative behaviour might induce some guilt and reduce the conversion in the following rounds; (ii) the conditional cooperators might reduce the willingness to cooperate, seeing some non-cooperative members (high converters), and thus increase deforestation.

We expect monetary sanctions to further increase cooperation, but the relative effectiveness of each enforcement strategy is difficult to predict *a priori*. Government enforcement is likely to be more effective and efficient than Community enforcement because it imposes a norm of zero deforestation by punishing any deforestation if inspected, and it incurs no cost to participants. Community enforcement offers, however, the opportunity to better target the largest free-riders (compared to random sanctioning by Government) and participants can be punished more than once. We conjecture that the effects of enforcement will differ across sites, given the difference in land tenure regimes and history of collective governance. These differences are particularly relevant for peer punishment, which is dependent on cultural and social norms (Bruhin et al., 2020; Eriksson et al., 2017; Herrmann et al., 2008).

The second category of hypotheses relates to the effect of inequality in wealth. Evidence from lab experiments suggests that without sanctions, inequality in individual endowment does not affect average cooperation when the aggregate endowment is the same between equal and unequal groups, as participants will move towards the non-cooperative outcome (Kingsley, 2016; Nockur et al., 2021; Reuben and Riedl, 2013). Once sanctions are introduced, participants with the highest capacity to deforest are expected to reduce their deforestation the most (Kingsley, 2016; Vollan et al., 2019). Thus, the introduction of monitoring and sanctioning should have heterogenous effects depending on the individuals' capacity to deforest. Inequality in endowments can in addition attenuate the positive effects of punishments or increase their frequency (Bernhard et al., 2006; Kingsley, 2016), increase risk taking attitudes (Payne et al., 2017), as well as reduce the preferences for internal enforcement institutions as compared to external (De Geest and Kingsley, 2019). Thus, we expect inequality in deforestation capacity to decrease the positive effects of the enforcement mechanisms, in particular efficiency.

3.5. Data analysis

We operationalized the 3E outcomes as follows. To evaluate *effectiveness*, we used the group and individual deforestation levels. For *efficiency*, following Cason and Gangadharan (2015), we calculated an index based on the realized earnings of participant i in each round t (π_{it}), the self-maximizing (Nash) strategy of the baseline stage (π_{NE}) and the socially optimal payoff (π_{SO}), such that:

$$Efficiency = \frac{\pi_{it} - \pi_{NE}}{\pi_{SO} - \pi_{NE}} \quad (2)$$

The realized earnings π_{it} has three components: the agricultural income from forest conversion, the payment from the standing forest (the same for all group members), and the costs of received sanctions and assigned punishments during the Community and Government stages. Under the Nash strategy participants convert their maximum, and it gives the minimum payoff for the group (π_{NE}). Under the socially optimal payoff, conversion is zero and the group outcome is maximized (π_{SO}). Both of the latter indicators are constant across rounds and stages. The efficiency of each treatment compares individuals' realized payoffs π_{it} to the socially optimal outcome π_{SO}. Higher earnings indicate higher efficiency. Our definition of efficiency considers only the enforcement costs and assumes no monitoring costs for the aggregate forest outcome. This is a reasonable assumption in the case that the PES implementer is shouldering those costs.

To measure *equity* at the group level and for each stage, we calculated a Gini coefficient of individual earnings (Cowell, 2011) and the perceived fairness of each enforcement strategy using a post-experiment questionnaire (see SI, section B2).

We used Wald tests, Friedman tests, and repeated measures ANOVA tests to compare group averages, and multilevel linear mixed effects models to evaluate individual level effects. We included random effects across participants and sessions in all regression models (Rabe-Hesketh and Skrondal, 2008) to control for the dependence of observations within experimental sessions and individuals across rounds. We present our main results as linear models, as they produce unbiased predictions in public good games data and their interpretation is more straightforward than probit and tobit models (Ai and Norton, 2003; Kent, 2020), but use ordered probit models as a robustness check (Moffatt, 2015). To control for potential learning effects and temporal trends, the order of enforcement (whether Community or Government enforcement was played first), the experimental round within stages (from 1 to 6), and a dummy (from 1 to 5) indicating the order of the experimental session within a village were included in all the models. Likewise, to control for behavioral preferences across participants, we included variables measuring risk (Binswanger, 1981), social preferences (Fehr et al., 2013), see SI section B2 for a detailed description of elicitation methods.

We also measure and include trust as a control, given the empirical evidence indicating how trust shapes experimental outcomes (Andersson et al., 2018a; Pfaff et al., 2019). The distribution of covariates is balanced across treatments except for risk and social preferences, which are included as control in all subsequent analyses (see SI, section B3).

4. Results

4.1. Effectiveness

Overall, the results lend support to the hypotheses that Public monitoring works as a social sanctioning mechanism and reduces deforestation, and that introducing monetary sanctions further increases PES effectiveness (Fig. 1). Group deforestation was high in the baseline stage: on average 15.9 and 16.8 forest plots were deforested in Equal and Unequal groups, out of a maximum of 36. Public monitoring significantly decreased group deforestation by 1.2 units in both the Equal ($p < 0.04$) and Unequal groups ($p < 0.03$), equivalent to 7.5% and 7.1% reduction respectively. In turn, Community enforcement decreased deforestation by 4.9 units or 30.8% ($p < 0.001$) in the Equal groups and by 5.7 units or 33.9% ($p < 0.001$) in the Unequal groups compared to the baseline. Government enforcement was the most effective, decreasing deforestation by 8 units or 50.3% ($p < 0.001$) in the Equal groups and by 7.5 units or 44.6% ($p < 0.001$) in the Unequal groups compared to baseline. Although group deforestation is higher in Unequal than Equal groups, the difference is not significant in any of the stages (SI, Table S1).

There are, however, important differences between the countries (Table 1). In Indonesia we observe no differences between the treatment effects of the Community and Government enforcement (Wald test, p = 0.59), and Public monitoring had no significant effects in Brazil (Wald test, p = 0.82). Furthermore, while inequality in deforestation capacity had no effect in Brazil or Peru, it significantly increased group deforestation in Indonesia by 0.4 units or 10%. We further examined whether the effectiveness of the enforcement mechanisms depends on (i) the inequality treatment and (ii) the order of the enforcement. Contrary to what was hypothesized, we found no significant interactions with inequality (SI, Table S4). Thus in our study sites inequality in deforestation capacity arising from wealth differences do not affect the overall effectiveness of the free-riding mitigation measures. We find, however, that the order of enforcement matters. When Community sanctions are introduced after Government enforcement, their effectiveness increases

Table 1
Treatment effects on individual deforestation decisions, by country sites.

	(1) Total sample	(2) Brazilian site	(3) Indonesian site	(4) Peruvian site
Treatment				
Public monitoring	-0.20*** (0.05)	0.02 (0.07)	-0.45*** (0.09)	-0.16** (0.08)
Community	-0.88*** (0.07)	-0.77*** (0.12)	-1.12*** (0.11)	-0.76*** (0.13)
Government	-1.29*** (0.07)	-1.42*** (0.13)	-1.09*** (0.12)	-1.36*** (0.13)
Inequality	0.03 (0.12)	-0.01 (0.14)	0.40*** (0.13)	-0.35 (0.24)
Constant	3.70*** (0.39)	3.80*** (0.41)	3.89*** (0.46)	2.69*** (0.58)
Village fixed effects	Yes	Yes	Yes	Yes
Individual covariates	Yes	Yes	Yes	Yes
Observations	17280	5760	5760	5760
Log-likelihood	-30542.95	-10806.46	-9582.39	-9863.98
AIC	61181.90	21676.91	19226.79	19791.95
p-value	0.000	0.000	0.000	0.000

Note: Coefficients from multilevel mixed effects linear models of deforestation, with random effects at the experimental session and individual level. Clustered standard errors at the experimental session level in parenthesis. P-values * p < 0.1, ** p < 0.05, *** p<0.01.

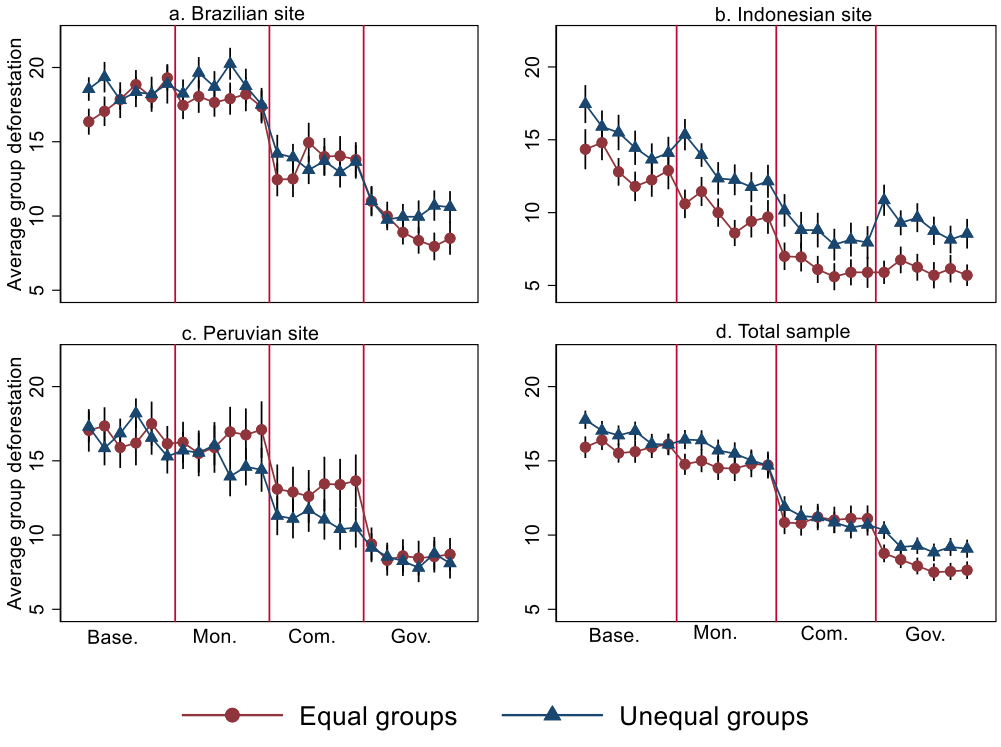

Fig. 1. Aggregate group deforestation (number of plots) per round, per country. The Community and Government stages were played randomly in either rounds 13–18 or rounds 19–24. Vertical lines represent 95% confidence intervals.

(SI, Table S5). In other words, previous exposure to external enforcement increases the effectiveness of internal sanctions.

Further decomposing the treatment effects by participant type (i.e., deforestation capacity) reveals that overall participants with a high (low) deforestation capacity deforested more (less) than their medium-capacity counterparts (Table 2). Importantly, there are heterogenous responses to treatment depending on the participant type. For example, the Public monitoring effect in Peru is dominated by the response of wealthy participants (Table 2, column 4). In general, wealthy participants responded more to the Community and Government enforcement, while the behavioural response from participants with low deforestation capacity was in general weaker. As a result, there were no significant differences in predicted deforestation levels among participant types during the Community and Government enforcement stages in any country (Fig. 2.). The introduction of sanctions equalized individual deforestation levels.

We further examined the proportion of forest plots deforested from the maximum allowed (instead of the absolute number of plots) and found no significance in the interaction terms (SI, Table S6). Thus, the heterogenous effects by participant type manifest in absolute changes in deforestation, not in relative changes. Country differences are again observed, and participants in Indonesia with low deforestation capacity converted a higher proportion than their medium-capacity counterparts, which explains why there are no significant differences in absolute deforestation levels between the two groups levels (Table 2, column 3).

Table 2
Treatment effects on individual deforestation interacted with deforestation capacity, by country.

	(1) Total sample	(2) Brazilian site	(3) Indonesian site	(4) Peruvian site
Treatment				
Public monitoring	-0.20***	-0.02 (0.09)	-0.53***	-0.05 (0.11)
	(0.07)		(0.13)	
Community	-0.82***	-0.71***	-1.15***	-0.58***
	(0.10)	(0.17)	(0.16)	(0.19)
Government	-1.33***	-1.46***	-1.18***	-1.34***
	(0.10)	(0.15)	(0.17)	(0.20)
Deforestation capacity				
Low capacity (LC)	-0.52***	-0.76***	-0.17 (0.18)	-0.70**
	(0.13)	(0.20)		(0.28)
High capacity (HC)	0.60***	0.68***	0.78***	0.29 (0.30)
	(0.16)	(0.23)	(0.27)	
Interaction terms				
Public monitoring*LC	0.13 (0.09)	0.19 (0.12)	0.32* (0.17)	-0.11 (0.14)
Community*LC	0.25* (0.13)	0.40* (0.22)	0.32 (0.20)	0.04 (0.22)
Government*LC	0.48***	0.54**	0.53**	0.37 (0.26)
	(0.14)	(0.23)	(0.21)	
Public monitoring*HC	-0.12 (0.13)	-0.04 (0.20)	0.00 (0.25)	-0.34*
				(0.19)
Community*HC	-0.53***	-0.62**	-0.20 (0.29)	-0.76**
	(0.17)	(0.29)		(0.32)
Government*HC	-0.32*	-0.35 (0.34)	-0.16 (0.34)	-0.45 (0.30)
	(0.19)			
Constant	3.60***	3.70***	3.81***	2.64***
	(0.39)	(0.42)	(0.50)	(0.57)
Village fixed effects	Yes	Yes	Yes	Yes
Individual covariates	Yes	Yes	Yes	Yes
Observations	17280	5760	5760	5760
Log-likelihood	-30448.05	-10765.64	-9557.87	-9821.92
AIC	61006.11	21609.28	19191.74	19721.83
p-value	0.00	0.00	0.00	0.00

Note: Coefficients from multilevel mixed effects linear models of deforestation, with random effects at the experimental session and individual level. Clustered standard errors at the experimental session level in parentheses. P-values * $p < 0.1$, ** $p < 0.05$, *** $p<0.01$.

4.2. Efficiency

Recall that the efficiency index is individuals' realized payoffs relative to the socially optimal outcome, cf. Eq. (2). Public monitoring of individual deforestation increased efficiency in Indonesia and Peru. Government enforcement was the most efficient treatment in all countries (Table 3). Community enforcement, on the other hand, did not increase efficiency compared to the baseline stage, in any of the country sites (Table 3). Thus, the benefits of the disciplining effect of peer punishment were not sufficient to outweigh its cost. This result is not only contingent on the fact that Government enforcement had no costs to participants during the experiment. Artificially introducing a cost to Government enforcement that resembles the cost of Community enforcement at the group level finds that Government enforcements remains more efficient as compared to Community enforcement (see SI, Table S7). Moreover, in Unequal groups in Indonesia and for the total sample, Community enforcement *decreased* efficiency and thus participants' earnings (Table 3, columns 2 and 6). The lower efficiency observed in the Unequal groups during the Community stage is explained by the higher frequency of costly punishment in Unequal groups (16.9 per session on average) as compared to the Equal groups (11.7 per session on average), a statistically significant difference (SI, Table S8).

4.3. Equity and fairness

Overall, inequality decreased with the introduction of Public monitoring and Government enforcement, but not with the introduction of Community enforcement, as indicated by the Gini coefficients (Table 4). But there are differences across groups and sites. Both Public Monitoring and Government enforcement decreased inequality in earnings in the Equal groups, while in the Unequal groups only Public monitoring had a significant effect in reducing inequality (Fig. 3). Across sites, in Brazil none of the enforcement strategies reduced inequalities. In Peru only Public monitoring reduced inequality. In Indonesia, Community enforcement *increased* inequality in Unequal groups, and in Equal groups both Government enforcement and Public monitoring reduced inequality (Fig. 3).

Why did the treatments not reduce inequalities significantly, despite deforestation rates being equalized across participant types? If we calculate the Gini coefficient of earnings without including the punishment costs, there are significant reductions in inequalities (Table S10 and Fig. S3, see SI). Thus, it is the punishment behaviour during the Community enforcement, as well as the random nature of sanctioning from the part of Government which inhibits positive distributional effects of enforcement.

Participants perceived Government enforcement as fairer than Community enforcement. Half (51.1%) thought that Government enforcement was fairer than Community enforcement, while 24.6% favored Community over Government enforcement. The remaining participants considered both enforcements to be equally fair (21.3%) or that neither institutional arrangement was fair (3%). In Peru participants were more likely to mention that both types of enforcement were equally fair (41%), while in Indonesia and Brazil most participants thought Government enforcement was fairer, with 64 % and 54 % of the participants, respectively. The probability of choosing either Government or Community enforcement as fairer was independent of being a participant with high, medium or low deforestation capacity (see Table S11, in SI).

5. Discussion

5.1. Solving the free-rider problem

Collective payments for forest conservation create a local collective action problem, as individual forest users have incentives to free ride on

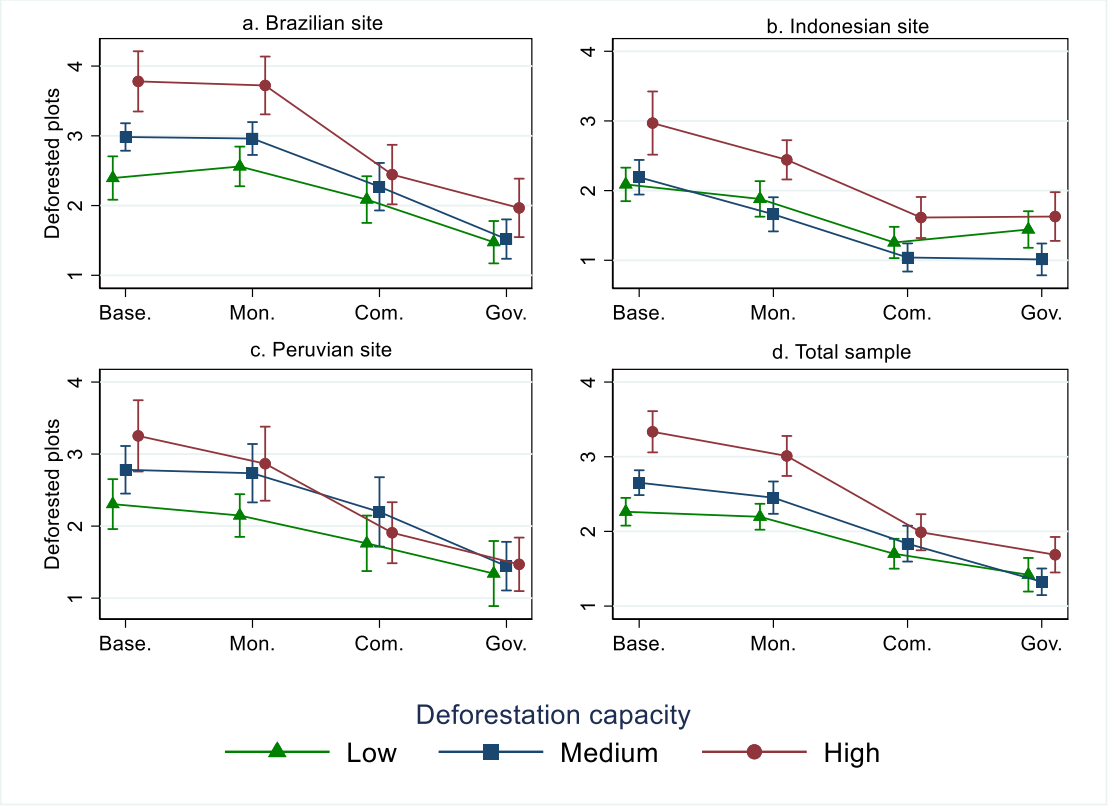

Fig. 2. Predicted deforestation depending on participant's deforestation capacity, by treatment and country site. Vertical lines represent 95% confidence intervals.

Table 3
Treatment effects on efficiency, by country.

	Total sample		Brazilian site		Indonesian site		Peruvian site	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Public monitoring	0.03*** (0.01)	0.03*** (0.01)	-0.00 (0.01)	0.00 (0.01)	0.07*** (0.02)	0.09*** (0.02)	0.03** (0.01)	0.01 (0.02)
Community	-0.05*** (0.02)	-0.02 (0.02)	-0.03 (0.03)	-0.01 (0.04)	-0.11*** (0.04)	-0.04 (0.05)	-0.02 (0.03)	-0.01 (0.03)
Government	0.13*** (0.01)	0.14*** (0.02)	0.14*** (0.03)	0.16*** (0.03)	0.10*** (0.02)	0.14*** (0.03)	0.14*** (0.03)	0.14*** (0.04)
Inequality	-0.04* (0.02)	-0.02 (0.02)	-0.04 (0.03)	-0.02 (0.03)	-0.12*** (0.03)	-0.06** (0.03)	0.02 (0.04)	0.02 (0.04)
Interaction terms								
Public monitoring*Inequality		-0.00 (0.02)		-0.02 (0.02)		-0.03 (0.03)		0.04 (0.02)
Community*Inequality		-0.06* (0.04)		-0.03 (0.06)		-0.14* (0.08)		-0.02 (0.05)
Government*Inequality		-0.03 (0.03)		-0.04 (0.05)		-0.07 (0.05)		-0.00 (0.05)
Constant	0.35*** (0.07)	0.34*** (0.07)	0.37*** (0.08)	0.36*** (0.08)	0.20** (0.08)	0.17** (0.09)	0.54*** (0.10)	0.54*** (0.10)
Village fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	17280	17280	5760	5760	5760	5760	5760	5760
Log likelihood	-1317.56	-1295.59	-530.90	-528.95	-642.77	-613.74	69.41	75.08
AIC	2731.12	2693.17	1125.79	1127.91	1347.54	1295.48	-74.82	-80.17
p-value	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00

Note: Coefficients from multilevel mixed effects linear models of deforestation, with random effects at the experimental session and individual level. Clustered standard errors at the experimental session level in parentheses. P-values * $p < 0.1$, ** $p < 0.05$, *** $p<0.01$.

others' conservation actions. Introducing individual level sanctions can improve the effectiveness, efficiency and equity of collective PES, but there is no strategy that simultaneously and consistently improves the 3E outcomes across country sites and inequality contexts.

Public monitoring of individual deforestation had a positive, albeit modest effect on group deforestation. This is consistent with studies showing that monitoring activities can increase PES effectiveness (Martin et al., 2014) and forest protection in general (Slough et al., 2021a), but also that they are far from being sufficient to ensure perfect compliance (Wunder et al., 2018). In our study, the effect was significant only in the country sites which have history of local collective action in terms of forest management and rule setting (Peruvian and Indonesian sites). This suggests that previous experience with collective agreements is an essential ingredient for getting a positive conservation impact of individual monitoring. The experimental literature has also demonstrated how previous communication or successful cooperation positively influences collective outcomes (Gangadharan et al., 2017; Rodriguez et al., 2019). While in our experiment the individual monitoring was anonymised, non-anonymised reporting, where the identity of the individuals is revealed, could have yielded even stronger effects. For example, public disclosure has stronger effects when non-cooperating individuals are singled out (Spraggon et al., 2015). Our

Table 4
Average Gini coefficient in Equal and Unequal groups, by stage[a,b].

Gini coefficient	Baseline	Monitoring	Community	Government
Equal groups	0.041 (0.01)	0.038 (0.02)	0.043 (0.03)	0.034 (0.02)
Unequal groups	0.045 (0.02)	0.041 (0.02)	0.052 (0.03)	0.040 (0.02)

[a] Gini coefficients are in general low because the collective benefits were large. We chose to have a high base collective payment for ethical reasons.
[b] Standard deviations in parenthesis. Friedman tests indicate significant differences between Gini coefficients of each stage in the Equal ($p=0.007$) and Unequal groups ($p<0.001$).

results are thus a lower bound of cooperative dynamics under a collective PES system reinforced with different sanctioning mechanisms.

Government enforcement is the most robust policy to increase the effectiveness and efficiency of the collective PES and was effective in all country sites and inequality contexts. In addition, previous exposure to external sanction increased the effectiveness of Community enforcement. Introducing external sanctions allows to coordinate on particular norms that can serve as focal-points (Gelcich et al., 2013; Nikiforakis et al., 2012). Moreover, we show that the random targeting of largest free-riders inhibits the positive distributional effects of enforcement. Accurately identifying the largest free-riders is therefore necessary to strengthen the positive equity effect of external enforcement. An impartial, strong external enforcement might be difficult to implement in situations of weak governance and corruption, where private interests or lack of funding might conflict with the provision of the public goods (Karsenty and Ongolo, 2012; Sundström, 2015). This is still a major challenge for effective environmental regulation. Nonetheless, most participants perceived Government enforcement as being fair, which indicates that effectiveness and efficiency considerations do not contradict equity and fairness ones. Emphasizing the potential win–win outcomes of external sanctions is particularly important considering that enforcement and sanctioning of PES non-compliance often lacks political support (Wunder et al., 2018).

Community enforcement can deliver on conservation outcomes but potentially entails a significant cost to community members. Results from the Indonesian site show that, compared to the baseline stage, introducing costly peer punishment creates significant trade-offs between effectiveness on the one hand, and efficiency and equity on the other. One of the reasons for lower effectiveness and efficiency of peer punishment is the existence of antisocial and retaliatory punishments (Bruhin et al., 2020; Nikiforakis, 2008; Vollan et al., 2019). Indeed, the uneven distribution of costs and benefits of collective agreements can lead to within community conflicts (Hayes et al., 2019). Community enforcement effectiveness and efficiency could be improved if collective PES implementers facilitate communication and increase social capital amongst PES participants (Koch et al., 2021). A large body of experimental evidence has shown the positive effects of communication on cooperation (Cardenas et al., 2002; Chaudhuri, 2011; Gangadharan et al., 2017; Hackett et al., 1994; Tavoni et al., 2011). But while communication typically increases effectiveness, it has limited positive distributional effects (Rodriguez et al. 2021). Given that strong community governance remains a major challenge (Dokken et al., 2014; Murtinho and Hayes, 2017) our study highlights the need to guarantee

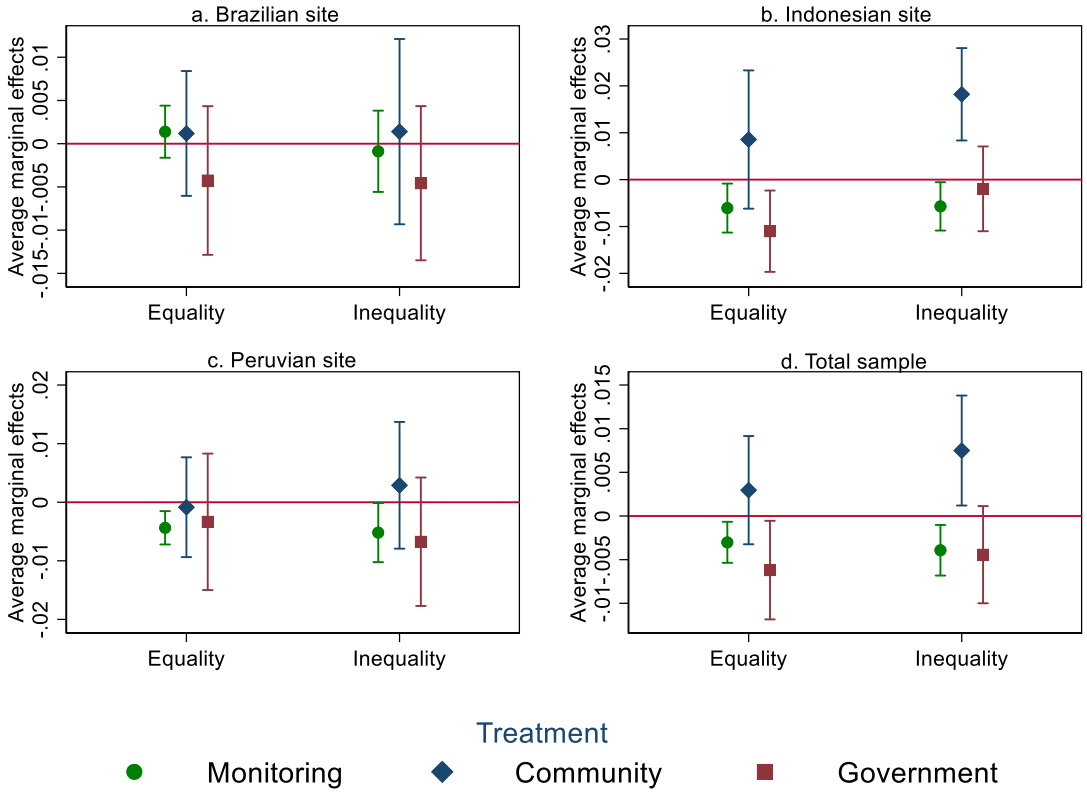

Fig. 3. Average marginal treatment effects of Public monitoring, Community and Government enforcement on the Gini coefficient, for Equal and Unequal groups and by country. See SI (Table S8) for full model specification and regression results. Vertical lines represent 95% confidence intervals of the coefficients.

that communities have an arena to discuss strategies and define their monitoring and sanctioning rules in the implementation of collective PES. Non-experimental studies suggest stakeholder involvement and external support from intermediaries such as NGOs facilitate participation and cooperation in PES in general (Izquierdo-Tort et al., 2021; Murtinho and Hayes, 2017; Pham et al., 2010), and can reduce elite capture (Persha and Andersson, 2014).

5.2. The effect of inequality

Our study provides new evidence of how wealth inequality, understood as differences in the capacity to engage in deforestation, can negatively affect the effectiveness and efficiency impacts of environmental regulations. The effect of wealth inequality cannot, however, be generalized across study sites: it was only significant in Indonesia, where it both increased deforestation as well as reduced efficiency. Considering that Indonesia has lower tenure security compared to the other sites, our results are consistent with the theory of collective action: one of the eight design principles for successful management of the commons is to have clearly defined boundaries (Ostrom, 1993). Inequality, when coupled with insecure tenure, has negative effects on cooperation, but does not have significant effects in sites with clear land tenure (communal or individual). Other factors explaining the strong inequality effect in the Indonesia site include higher pre-existing inequality in landholdings and assets compared to the other two sites, and stronger customary rules of forest management. These factors also explain why there were no differences in the effectiveness of external and internal enforcement in this country site, coinciding with a similar experiment conducted in Namibia (Vollan et al., 2019). While the impact of inequality seems to depend on the country site, future research could examine how this effect is mediated by factors such as levels of trust and social preferences amongst participants. The heterogenous findings across sites highlight the importance of considering different populations in inequality studies.

A result generalizable across country sites is that wealthy participants with high deforestation capacity tended to be more responsive to (the threat of) sanctions than their poorer counterparts. This result is particularly interesting considering that all participants faced the same incentives to cooperate and the same sanctioning costs. The lower responsiveness of poorer participants to sanctioning is consistent with being more averse to disadvantageous inequality than to advantageous inequality (Fehr and Schmidt, 1999). Evaluations of collective PES also show that wealthier residents are more likely to change their behaviours (Hayes et al., 2017).

5.3. Policy implications and limitations

Two important considerations for the external validity and policy implications of our results should be noted. First, that the endowment inequality was created exogenously. Different results could be expected with endogenous inequality (i.e., with a real effort task), as the origin of wealth differences affects fairness perceptions (Almås et al., 2010). Future inequality studies could evaluate what happens when wealth inequality (i.e., differences in endowment) are stronger or when they are interacted with other sources of inequality, such as the returns of collective PES or of the private good (e.g., Vorlaufer et al., 2017). Second, the experiment simulated a best-case scenario of perfect and costless monitoring conditions: PES was perfectly monitored, and everyone could observe others' deforestation and could punish all players at the same cost (Community stage) or with the same probability (Government stage). Arguably, conditions in the field are different; it might be costly to track individual deforestation, or power relations can modify enforcement costs amongst community members. Experimental evidence shows that external enforcement and collective PES maintains strong effects even with lower sanctioning probabilities than in this study (Andersson et al., 2018a; Lopez et al., 2012; Vollan et al., 2019), or when the sanctions are provided only at the collective rather than individual level (Cason and Gangadharan, 2013). On the other hand, under imperfect monitoring, the effectiveness and efficiency of peer punishments decreases (Boosey and Isaac, 2016; Grechenig et al., 2010; Shreedar et al., 2020), as do the acceptability and preference for a decentralized institution (De Geest and Kingsley, 2019). These findings – along with our results – point to the advantages of external enforcement as compared to internal enforcement mechanisms when implementing collective PES. Given the known positive effects of community monitoring in the management of common-pool resources (Buntaine and Daniels, 2020; Slough et al., 2021b), a combination of bottom-up monitoring with higher-level sanctioning could be a promising strategy to increase individual compliance in collective agreements. Yet, it could potentially decrease the economic efficiency (earnings) as the PES participants incur the monitoring costs.

Overall, we showed how different sites respond to increased monitoring and enforcement in collective PES. The fact that we find heterogenous responses to the treatments lends support to the external validity of our results; the sites with less history of collective action are less responsive to peer punishment and individual monitoring. Our findings are useful to policymakers and PES implementers as they consider options for designing more effective, efficient and equitable interventions, in particular, the potential benefits of increasing monitoring and enforcement. Relevant criteria affecting the impacts of enforcement mechanisms include tenure regimes, histories of collective action, and previous exposure to centralized enforcement.

6. Conclusion

Collective payments are a promising conservation policy to reduce global deforestation, but their effectiveness is jeopardized by the fact that they entail incentives for individual free riding. As collective PES gain traction, policy makers and practitioners should consider strategies that can help solve the free-riding problem intrinsic to such payments and thus deliver effective, efficient and equitable (3E) outcomes. Our study is the first to show the implications of different monetary and non-monetary sanctioning strategies to limit free-riding, and to link these outcomes to different land tenure and institutional contexts. Compared to a situation of collective PES without any individual monitoring and enforcement, we show that introducing monitoring and enforcement allows to significantly increase the benefits of collective PES.

Public monitoring of individual decisions has limited effectiveness as compared to the introduction of monetary sanctions, and a significant effect is only observed in sites with a stronger history of collective action. Community enforcement (internal, peer-to-peer sanction) increases effectiveness but can reduce the efficiency and equity of collective PES, especially when implemented in communities with unequal access to resources. We find important variations in impacts; for example, in Indonesia the reduction in deforestation from Community enforcement is higher than in the other two sites, and inequality in the access to forest resources significantly increases group deforestation. However, across the sites, external, Government enforcement provides the strongest and most robust results in terms of effectiveness and efficiency outcomes. Further, punishment that does not effectively target free-riders hampers the positive distributional effects of both enforcement strategies.

Finally, we find that implementing collective PES in groups with inequality in wealth can have negative effects on conservation and exacerbate the trade-offs between effectiveness, efficiency and equity outcomes. In addition to individual free riding, a challenge in designing and implementing PES is to manage such trade-offs, and our results suggest that these are particularly pronounced – and thus PES implementation more challenging – in contexts with unequal forest access. The results are relevant for both collective PES schemes as well as group-based incentive schemes in general.

7. Author statement

J.N., A.A., A.M., A.E.D., and C.M., conceived of the project, A.A., J. N., A.M., designed the experiments, C.C., J.N., V.S., and G.D. conducted the experiments, J.N., A.A. and A.M. developed the analysis approach, J. N. analysed the data, J.N., A.A. and A.M., wrote the paper, and A.E.D., C. M., C.C., V.S. and G.D revised and edited it.

Declaration of Competing Interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

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Appendix A. Supplementary data

Supplementary data to this article can be found online at https://doi.org/10.1016/j.gloenvcha.2022.102520.

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Farmers' preferences for water-saving strategies in Brazilian eucalypt plantations

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ABSTRACT

In a climate change context, changing temperature and precipitation patterns are expected to have strong impacts on Brazilian eucalypt plantations. Implementing adaptive water-efficient management practices is thus becoming necessary to maintain high levels of productivity while preserving the water resources. This paper investigates the ability of eucalypt farmers to modify their current silvicultural practices in order to adapt to drought in the near future. We ran a choice experiment in the state of Minas Gerais, among 80 eucalypt tree farmers, who were asked to choose from several management options associated with various financial supports. The results show that adaptation by reducing the length of the eucalypt rotation proves to be by far the preferred option, despite the associated costs. On the contrary, reducing density appears to be the least chosen option by the respondents, which may suggest that they underestimate the benefits of this strategy. We moreover find a clear and relevant segmentation of farmers' choice behavior, the general preference for reducing the length of the eucalypt rotation being driven by the most vulnerable farmers of the sample.

1. Introduction

The biophysical effects of climate change on natural and managed systems, agricultural productivity and food security are increasingly well-understood (IPCC, 2014; Moore et al., 2017). In many areas, management options for adaptation to climate change have already been developed. These adaptation measures include, for example, using scarce water resources more efficiently, developing drought-tolerant crops and choosing tree species and forestry practices that reduce vulnerability to storms and fires. Adaptation to climate change, however, requires the incorporation of this knowledge into management decisions (Keenan, 2015). Several management options for adaptation in agriculture exist, but farmers differ in their individual preferences for time and risk as well as in the constraints they face. Adaptation in agriculture may therefore vary significantly across regions, depending on climatic, social, economic and institutional factors (Khanal et al., 2018; Below et al., 2012; Deressa et al., 2009). How will farmers adapt to the effects of climate change in the near future thus remains hard to predict.

Several studies have explored the steps that farmers' can take in adapting to climate change(Chen et al., 2014; Deressa et al., 2009; Seo and Mendelsohn, 2008; Alam et al., 2016; Alauddin and Sarker, 2014; Ngigi et al., 2017). Most have analysed the determinants of adaptation decisions by comparing the characteristics of adapters and non-adapters. For example, Deressa et al. (2009) found that household characteristics and access to agricultural extension and credit can influence farmers' adaptation decisions in the Ethiopian context. A number of studies have analysed the impact of adaptation on crop yields (Deressa and Hassan, 2009; Di Falco et al., 2011; Di Falco et al., 2012; Huang et al., 2015; Khanal et al., 2018). Although a growing number of studies employ choice experiments to estimate farmers' willingness to provide ecosystem services (see Kaczan et al. (2013) and references therein), there exist few ex-ante evaluations of the ability of farmers to adapt to climate change. Such evaluations would assess farmers' willingness to adopt new agricultural strategies that sometimes require drastic changes in forest or crop management. This study aims to fill this gap by

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conducting a choice experiment on a sample of Brazilian managers of eucalypt plantations who were asked to choose among several climate change adaptation strategies on eucalypt plantations.[1]

Eucalypt is a prime source of low-cost woody biomass, which explains its popularity among both industrial firms and smallholders. Like many plants, however, drought is a major risk for eucalypt plantations in a context of climate change. In the Brazilian state of Minas Gerais, recent droughts have caused significant loss of yields and tree mortality in highly productive eucalypt plantations (Goncalves et al., 2017). The sustainability of eucalypt plantations is now threatened by high water demand[2] and the absorptive capacity of the fast-growing genotypes that are increasingly used. In some areas, silvicultural practices also affect the availability of water, and consequently tree growth (Gonçalves et al., 2008; Goncalves et al., 2017). In this study, we investigate what would be the adaptation strategies chosen by smallholder farmers who would have to deal with the consequences of climate change. Governments may intervene in this choice for at least two reasons. First, eucalypt plantations are intensive fast-wood plantations and the way they are managed may have negative effects on water resources. As in any standard negative externality problem, the social planner may want to subsidize the most water efficient strategies. For that reason, improving water use efficiency by adopting appropriate forest management practices has become a key challenge in ensuring ecologically sustainable levels of productivity (Booth, 2013). Second, eucalypt plantations have an important role in the Brazilian economy. Since climate change threatens the development of this sector, the government may want to support farmers' adaptation to climate change by subsidizing the switch to the most profitable adaptation strategies. Our study aims at studying farmers' preferences for water-saving strategies in Brazilian eucalypt plantations to avoid mortality during prolonged droughts. We ran a choice experiment (CE) among 80 eucalypt growers who were asked to choose from several hypothetical adaptation strategies, defined as a combination of attributes. Each strategy was represented as a forest management option in which they received monetary compensation for implementing specific practices on their farm. We examined growers' preferences for five different options that have been identified in the literature as promising strategies for reducing the susceptibility of trees to drought while maintaining either the same or slightly diminished yields. These options include: reducing the cutting cycle, adopting new hybrid plants, reducing tree density, reducing the use of fertilizers, and coppicing. In our framework, a respondent who chooses to adapt to climate change opts for one of these forest management options, may also receive (in addition to a financial support) free technical assistance to help him implement the proposed system and a weather insurance subsidized at 50%.

The way in which farmers choose between several different adaptation strategies, each with varying levels of individual attributes, is used to quantify their preferences for these attributes, as well as to quantify overall willingness to accept (WTA) values, i.e. the amount of money an average grower would require in order to adapt to climate change. We analyse the data following the same approach as Gevrek and Uyduranoglu (2015); Lienhoop and Brouwer (2015); Broch et al. (2013) and other recent studies. We use a mixed logit model, which allows for heterogeneity in growers' tastes. We moreover study the extent to which the socioeconomic and structural farm characteristics of the respondents may influence their answers using a latent class model.

Altogether, our results indicate that respondents tend to overvalue both free technical assistance and subsidized insurance. Moreover, the strategy based on the reduction of the cutting cycle appears to be by far the preferred option, while the reduction of plantation density is the least selected option. In between these two options, the respondents appear to equally value the reduction of fertilizer, the introduction of hybrid plants and coppicing. We also find that growers' preferences are highly heterogeneous and that the strongest preferences for reducing the cutting cycle are held by the most vulnerable farmers in the sample.

The remainder of this article is organized as follows. We first provide background information on the Brazilian eucalypt plantations and the possible management options for adaptation to climate change in Section 2. We present the methodology and the data used in Section 3. Thereafter we present the results of the analysis in Section 4 and discuss them in Section 5. Section 6 concludes.

2. Brazilian eucalypt plantations and climate change

2.1. Background

Cultivation of eucalypt trees began in the 19th century and spread throughout the next century as the most planted genus of broadleaf trees in the world. Today eucalypt plantations are spread over more than 20 million hectares around the world (Booth, 2013). Extensive cultivation of this genus beyond its natural range began in the early 20th century in Brazil (FAO, 2011). Over the past decades, it has expanded rapidly in Brazil, mainly replacing degraded pastures (Smethurst et al., 2015). Nowadays, eucalypt is the primary and most productive planted forest in Brazil, covering around 5.6 million hectares (IBA, 2016). Of all eucalypt plantations in Brazil, more than one-third belongs to companies in the pulp and paper sector. Independent farmers and farmers in outgrower schemes[3] hold the second largest share of planted forests in Brazil (IBA, 2016).

Short rotation eucalypt crops are a significant source of raw material for the pulp and paper industry in Brazil, and these plantations have been mostly established in areas where the climate favours high yields (GonÃ§alves et al. Goncalves et al., 2013). In addition to their private plantations, timber-based companies encourage the establishment of new plantations through outgrower programs. In these programs, companies typically provide seedlings, cuttings, and other inputs in exchange for being given priority when purchasing wood after the harvest (Rode et al., 2014). These contracts encourage farmers to consider cultivation of eucalypt as a complement to their agricultural income.

2.2. Plantation management options for adaptation to drought

Eucalypt plantations are predominantly clonally propagated due to the ability of the plant to adapt to regions with low to moderate water scarcity and low fertility soils (Gonçalves et al., 2008). The largest Brazilian plantations are found in the Central-West and Southeastern regions of the country, particularly in the states of Minas Gerais (containing 24% of the total area of planted eucalypt), Sao Paulo (17%) and Mato Grosso do Sul (15%) (IBA, 2016). These clonal forests have been largely established on sites with water and nutrient restrictions, where they out-perform conventional seed-based silviculture. In a climate change scenario, however, the sustainability of these plantations is threatened. In this study, we focus on several potential management options, which are built based on results of a number of recent studies (Christina et al., 2017; Battie-Laclau et al., 2016; White et al., 2014;

[1] Eucalypt plantations are not forests in the sense in which it is understood in temperate zones, since the average rotation is around 6–7 years. It should be seen as closer to the cultivation of permanent crops in tropical zones, like coffee for example.

[2] Previous studies have shown that water use by eucalypt plantations depends on the particular territory, environmental conditions and land-use practices employed (Poore and Fries, 1985; Cornish, 1993; Calder, 1998; Almeida and Soares, 2003; Hubbard et al., 2010; Ferraz et al., 2013; de Barros Ferraz et al., 2019).

[3] Outgrower schemes, also known as contract farming, are broadly defined as binding arrangements through which a firm ensures its supply of agricultural products by individual or groups of farmers (Race and Desmond, 2001).

Matusick et al., 2013). We explored the agronomic literature in order to determine the most relevant adaptation strategies in the Brazilian context. We then inserted these strategies into an socio-economic framework allowing us to assess not only farmers' preferences for these strategies, but also the potential levers to guide these choices (monetary and non-monetary rewards), and the factors likely to explain the heterogeneity of these choices (the characteristics of the farmers themselves). In order to reduce the risks of tree susceptibility to drought resulting from climate change, several silvicultural systems for eucalypt production are currently under development. Management options to reduce the risk of tree mortality during exceptional droughts, however, have some disadvantages compared to current silvicultural practices. In particular, a loss of productivity may occur compared to the most productive clone, planted with a high stocking density and highly fertilized. Five silvicultural systems appear promising for reducing the risk of tree susceptibility to drought while either maintaining or slightly diminishing yields.

The Short Rotation (SR) option: This production system consists in reducing the length of the cutting cycle from 7 years (the current practice in our study area) to 4 years. Previous studies indeed show that an increase in the frequency of clearcutting would make it possible to store water in deeper soil layers over a greater proportion of the rotation (Christina et al., 2017). As Stape et al. (2010) showed, reducing the cutting cycle from 6 years to 4 years would not affect the mean annual increment (MAI). The main constraint to the farmers under this system is the higher frequency of harvesting operations, which implies increased total harvesting and replanting costs.

The New Hybrid (NH) option: This option consists in adopting new hybrid eucalypt trees that are more tolerant to drought, instead of the highly productive clones currently used. Although these hybrids are less productive, they have a greater water use efficiency rate that reduces water use and tree mortality (Booth, 2013). Therefore, the loss of MAI in this system compared to the most productive clones currently planted will depend on the expected risk of mortality during an exceptional drought period. *The Reduced Density (RD) option:*This option consists in reducing the density of trees planted, switching from one tree per area of 3 m by 3 m to one tree per area of 3 m by 4 m. This would decrease tree stand evapotranspiration and competition for water resources (White et al., 2009, 2014). However, a decrease in leaf area is needed to reduce tree stand transpiration, which may slightly decrease the productivity.

The Reduced Fertilization (RF) option: This option consists in a reduction of fertilizer doses. Previous studies have pointed out that this strategy can diminish tree mortality risk in the event of extreme droughts, as a consequence of lower leaf areas (Battie-Laclau et al., 2016). Additionally, the water stored in the deep soil layers during the rainy season is withdrawn early in the dry season in fertilized plantations, leading to greater water deficit, while unfertilized stands use the water more slowly, making more water available during the rest of the season (Christina et al., 2018; White et al., 2014).

The Coppice Management (CM) option: This option consists in coppicing, which enables already-established roots to access to water at great depths (Laclau et al., 2013; Germon et al., 2019). Coppicing after the first rotation is a common option in Brazilian eucalypt plantations. An average loss of 5% on MAI is expected under this option (de Souza et al., 2016). In the field experiment presented below, these five management options are included in the strategies used for climate change adaptation.

3. Methodology and materials

3.1. Statistical models

We use the framework provided by Revelt and Train (1998), in which a sample of N respondents have the choice of J alternatives (strategies for climate change adaptation here) on T choice occasions. A farmer is assumed to choose an adaptation strategy if the utility from choosing that alternative is greater than choosing either no adaptation or any of the competing choices. The utility that farmer n gets from choosing alternative j is given by $U_{nj} = \beta_n' x_{nj} + \varepsilon_{nj}$, where β_n is a vector of individual-specific coefficients, x_{nj} is a vector of observed attributes relating to individual n and alternative j, and ε_{nj} is a random term. The probability that farmer n chooses alternative k is:

$$P_{nk} = P\left(U_{nk} > U_{nj}\right) = P\left(\varepsilon_{nk} - \varepsilon_{nj} < \beta_n' x_{nk} - \beta_n' x_{nj}\right) \forall k \neq j$$

Different discrete choice models are obtained from different assumptions about the distribution of the random terms ε. We first use a mixed logit model.[4] We assume that all the parameters, except the monetary attribute, follow a normal distribution. Our models also include an alternative specific constant (ASC) taking the value of one if the *status quo* alternative describing the current situation is chosen and zero otherwise (Adamowicz et al., 1998; Scarpa et al., 2005). As β_n is unknown, the unconditional probability for a sequence of choices d can be expressed by integrating over all values of β weighted by the density of its distribution, denoted $f(\beta|\theta)$, where θ are the parameters of the distribution:

$$S_n^{\mathrm{MXL}} = \int \prod_{t=1}^{T} \prod_{j=1}^{J} \left[\frac{exp\left(x_{njt}'\beta\right)}{\sum_{j=1}^{J} exp\left(x_{njt}'\beta\right)} \right]^{y_{njt}} f(\beta|\theta) d\beta$$

where $y_{njt} = 1$ if the respondent chooses j in situation t and zero otherwise. The log likelihood for the model is given by $LL(\theta) = \sum_{n=1}^{N} \ln P_n(\theta)$. This expression cannot be solved analytically, and it is therefore approximated using simulation methods. We estimate this model by employing maximum simulated likelihood using 500 Halton draws (Hole, 2007). Since the monetary attribute is assumed to be a fixed parameter in our model, we have the convenient result that the willingness-to-accept (WTA) attribute k, i.e. the average value the respondents put on attribute k, all other things being equal, is:

$$E\left(\mathrm{WTA}^k\right) = -\frac{E(\beta^k)}{\beta^{\mathrm{money}}}$$

where β^{money} is the coefficient of the monetary attribute.

We then use a latent class model in order to provide some insights regarding the heterogeneity of farmers' preferences – if there is indeed any according to the results of the mixed logit model – and the importance of their characteristics in the decision-making process regarding climate adaptation practices. In this case, each respondent is assumed to belong to a class q, where preferences vary across, but not within classes. In this case, the probability of a particular sequence of choices is:

$$S_n^{\mathrm{LC}} = \sum_{q=1}^{Q} H_{nq} \prod_{t=1}^{T} \prod_{j=1}^{J} \left[\frac{exp\left(x_{njt}'\beta\right)}{\sum_{j=1}^{J} exp\left(x_{njt}'\beta\right)} \right]^{y_{njt}}$$

where H_{nq} is the probability of belonging to class q. The log-likelihood for this model is $LL = \sum_{n=1}^{N} \ln S_n$. We maximize this expression using the expectation-maximization algorithm.

3.2. Design of the choice experiment

The selection of attributes for the study was based on a review of existing relevant literature on current agricultural and environmental

[4] The mixed logit model overcomes three drawbacks of the standard logit model by allowing for heterogeneity in tastes, correlation in unobserved factors over repeated choices made by each individual, and complete relaxation of the independence of irrelevant alternatives (IIA) assumption (Train, 1998; Greene and Hensher, 2003).

Table 1
Description of attributes and levels.

Attribute	Levels	Description
Silvicultural management	Density (reference)	Reduction of plantation density (833 trees per hectare instead of 1111)
	Fertilizer	Reduction of the dose of fertilizers (30% of the current dose)
	Hybrid	Adoption of drought-tolerant hybrids
	Cycle	Reduction of the cutting cycle (4 years instead of 7 years)
	Coppice	Adoption of coppice management
Technical assistance	Yes; No	Free technical assistance for implementing the management option
Subsidized weather insurance	Yes; No	Subsidy of 50% for the insurance
Compensation (BRL)	100; 200; 300; 400; 500	Financial support to adapt to climate change

policies and discussion groups involving scientists as well as Brazilian forest managers who participated in the project. The four attributes and their corresponding levels are presented in Table 1. The adaptation strategies are characterized by four attributes: a silvicultural management option, some level of monetary compensation, a weather insurance scheme that is 50% subsidized and the provision of free technical assistance to help the farmer implement the management option proposed. The silvicultural management attribute consists of five levels, namely the five management options designed to reduce the risks of tree susceptibility to drought in a context of climate change: reducing the cutting cycle, adopting new hybrid plants, reducing the tree density, reducing of the use of fertilizers, and coppicing the trees (see Section 2.2). In our model, reducing tree density is the reference level of the silvicultural management attribute.

The level of monetary compensation, the attribute used to estimate the implicit values of the other attributes, was defined so as to be realistic for respondents. The starting point here was identifying a payment level that was in line with expected wood production losses when adopting one of the water-efficient management systems. We calculated that a loss in productivity of three cubic meters per hectare per year would cost about 100 Brazilian reais (BRL) The design moreover includes two non-cash reward attributes: the provision of a free technical assistance and a 50% subsidized weather insurance. It is generally assumed that the presence of a technical expert – also called agricultural extension in the literature – is likely to promote significant changes in farming practices and more effectively in the early stages of the process dissemination of the new technology, new practice or new system sought to be adopted (Anderson and Feder, 2007). This is the reason why a number of recent studies from the farmer choice experiment literature include technical assistance as an attribute of the options offered to respondents and generally show a marked preference of farmers for this type of non-cash rewards (Abebe et al. (2013); Andow et al. (2017); Minten et al. (2009); Kuhfuss et al. (2016); Vignola et al. (2012) to name just a few examples). Private climate insurance has also been seen as having the potential to reduce vulnerability of agricultural systems to climate-related risks to farm-level production, infrastructure and income (Smit and Skinner, 2002). This instrument seems less often offered than technical assistance as an attribute in the choice experiment literature, probably because it is not available in all contexts. However, a number of recent papers in the choice experiment literature have considered it as one of the more viable income smoothing strategies (Prasada (2020) and references herein).

We followed a D-efficient design approach to construct the choice sets, using prior information we had about the sign and relative values of the design attributes.[5] We used secondary data to construct prior values for the true parameters of the model. The value chosen for technical assistance was 600 BRL, which is the average price for hiring a specialist for one day in Minas Gerais region. The value chosen for weather insurance was 175 BRL for insuring one hectare of eucalypt plantation (after the 50% subsidy) and 35 BRL for the value of the equivalent of one meter cubic of wood.

The design was generated with the software package Ngene in order to produce 10 choice sets per respondent. In our study, a choice set consists of two alternative adaptation strategies and an option to decline both strategies (the *status quo* option). An example of a choice set is displayed in Fig. 2.

3.3. Data

We collected original data from a total of 80 out-grower farmers[6] living in the state of Minas Gerais. In practice, we had access to the registry database of all the farmers enrolled in the contract-farming scheme of CENIBRA, an eucalypt pulp and paper company located in Ipatinga, on the eastern part of Rio Doce basin (Fig. 1). There were several hundreds of farmers likely to participate in the experiment. Given the financial constraints of the research project, we targeted 200 of them, focusing on their geographic location so that we have respondents from all the different zones of the study area. After contacting them by phone, we ended up with 80 farmers who agreed to participate in the experiment.

In order to ensure that respondents would fully understand the questions and concepts used in the CE, the questionnaire was pre-tested with technical assistants from CENIBRA, who interacted frequently with the out-grower farmers.[7] The respondents had a 30-min information session regarding the attributes and levels before beginning the survey. The monetary compensation levels were not mentioned, since it could result in anchoring at the highest offer.[8] A brief description of the choice

[5] Efficient experimental designs can reduce confidence intervals for parameters of interest in choice models, or alternatively reduce required sample sizes. Informed priors can then be useful when trying to make strong inferences from small amounts of data, since these priors capture any assumptions the researcher makes about model parameters before observing the data (Kruschke and Liddell, 2017).

[6] According to the FAO, a contractual partnership between growers or landholders and a company for the production of commercial forest products. Out-grower schemes or partnerships vary considerably in the extent to which inputs, costs, risks and benefits are shared between growers/landholders and companies. Partnerships may be short or long-term (eg. 40 years), and may offer growers only financial benefits or a wider range of benefits. Also, growers may act individually or as a group in partnership with a company, and use private or communal land. Out-grower schemes are usually prescribed in formal contracts.

[7] CENIBRA did not participate in the design of the choice experiment. But they provided access to the pool of potential participants and guided the sampling so that it is representative in terms of geographic areas.

[8] The monetary compensation levels were not revealed during the 30-min briefing that took place prior to the choice experiment, but of course they did appear explicitly on the choice cards. More precisely, the attribute of monetary compensation was presented during the information session through the following sentence: *The third proposed compensation consists of the remuneration of an amount per hectare that takes into account losses in productivity without mentioning the values per hectare.* The participants discovered the values only when starting the choice experiment.

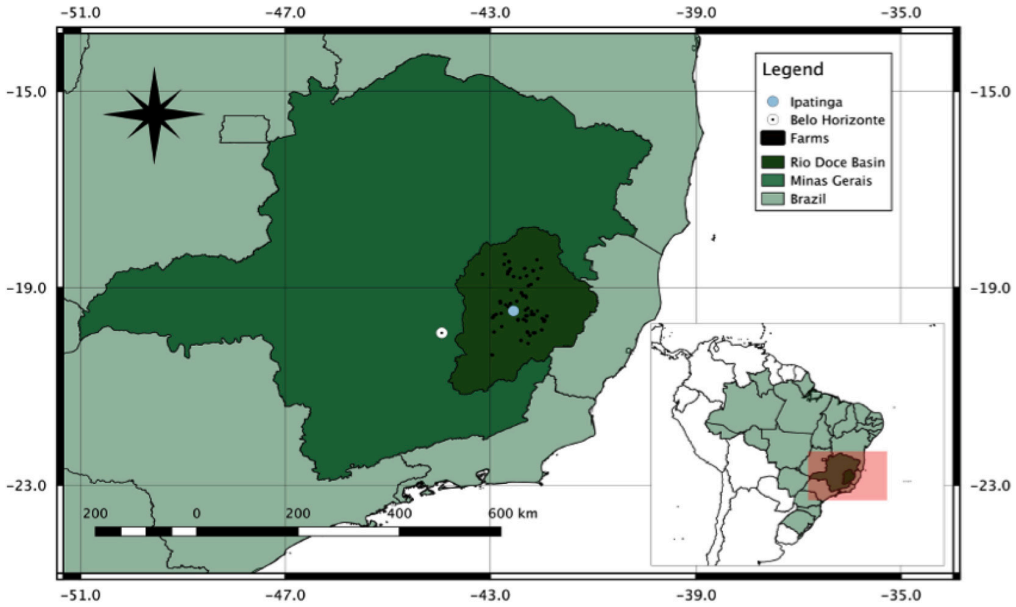

Fig. 1. Location of the study area.

		Option A	Option B
Eucalypt management		4 years	4 m x 3 m
Compensation		**No** technical assistance	**With** technical assistance
		With weather insurance	**No** weather insurance
		R$ 200,00/ha/year	R$ 400,00/ha/year
Choice	☐ **Option A**	☐ **Option B**	☐ **None**

Fig. 2. Example of choice card.

task was provided to each respondent before each choice set. We moreover provided plausible values for average annual incremental loss for each scenario, based on expert estimates.[9] The participants first answered survey questions about themselves, their farm and their environmental perceptions, and then participated in the CE. Data collection took place between March and April 2017 through face-to-face interviews.

Descriptive statistics of the farms owned or managed by the survey respondents as well as their main socioeconomic characteristics are shown in Table 2. The sample is mainly composed of male growers, who have on average three household members, a secondary education and less than 15 years' worth of experience growing eucalypt. The majority of farmers interviewed grew eucalypt as a complementary source of revenue (less than 30% of their income). Less than 7% of the sample has a plantation insurance that covers for fire and other weather-related damages. The average farm size is around 200 ha and the mean area of eucalypt plantations is around 90 ha (the median is 65 ha).

We find, however, that these figures mask a high level of heterogeneity. The interviewed farmers are spatially distributed into four distinct geographic zones (see Table 3). The zone near the municipality of Belo Oriente is characterized by the greatest climatic constraints and can be considered the zone that is most vulnerable to climate-change in our study. The main constraints in this region are: higher water deficit,

[9] When presenting the choice experiment, we informed the participants various consequences of climate change, including loss of yields and tree mortality, through simple diagrams showing the relative advantages of the different adaptation strategies proposed. In particular, we ranked the proposed management options according to the likely loss in MAI: 1) SR option (no loss), 2) CM option, 3) RD and RF options, and 4) NH option (highest loss). We also clearly specified that the performance of these strategies was based solely on expert opinion and could not correspond to the reality that the farmer will face in the future.

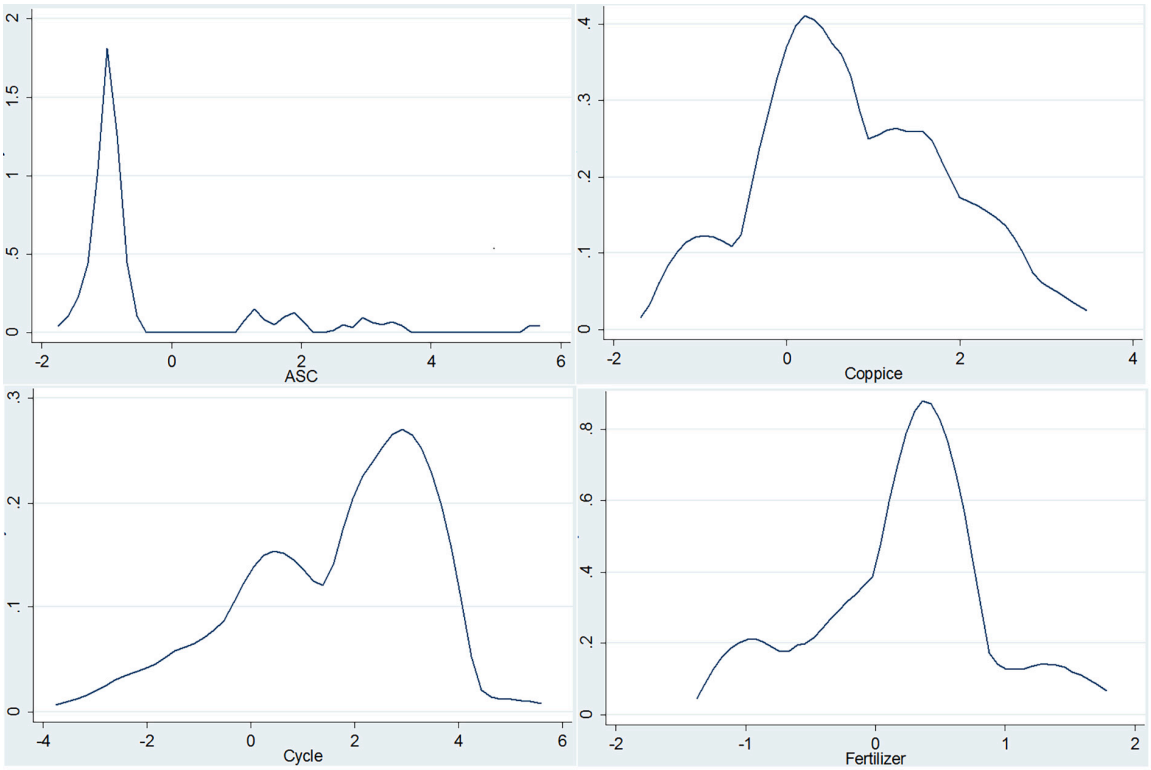

Fig. 3. Distribution of the individual-level coefficients.
Note: These graphs display the Epanechnikov kernel density estimates for the individual-level coefficients of the mixed logit model.

lower altitude and smaller precipitation volumes, compared to other zones.[10] These characteristics explain the lower MAI among the sample in this region. Since these plantations are located in the vicinity of CENIBRA's pulp mill, however, growing eucalypt in these areas remains economically viable for the time being. In contrast, plantations located in the area near the municipality of Cocais and Pecanha are characterized by the highest MAI rates, as a result of the greater annual precipitation levels, a higher altitude and soils with suitable properties to grow eucalypt trees. Despite the fact that Pecanha is one of the most distantly located plantations, eucalypt cultivation in this area remains viable. Caratinga is characterized by a moderate MAI due to its climatic and topographical conditions and is located on an intermediate distance from CENIBRA pulp mill. Although participants in the study were not randomly selected, our sample is quite representative of CENIBRA outgrowers' dispersion in the country.[11]

[10] The precipitation pattern in the basin is characterized by two distinct periods. The rainy period extends from October to March and precipitation in this period varies from 800 to 1300 mm. The dry period extends from April to September, with the most severe droughts occurring from June to August. Precipitation during the dry period ranges between 150 and 250 mm (Agência Nacional de Águas, 2010).

[11] We do not have quantitative data on those farmers who ultimately did not participate in the choice experiment. However, qualitative (informal) information collected from CENIBRA when the sample was drawn up suggests that the sample is representative in terms of zoning (the four geographical areas of CENIBRA farms are represented), but not in terms of farm size (participants in the survey are presumably larger on average than non-participants, according to CENIBRA staff). Moreover, as in any survey where respondents voluntarily choose to participate or not, we cannot exclude that participants differ from non-participants in terms of their allocation of leisure time or perhaps in terms of awareness of the likely impacts of climate change.

4. Results

In this section, we first provide the estimates of the mixed logit model parameters, along with the WTA estimates, and we discuss the apparent heterogeneity in farmers' preferences. We then turn to a latent class (LC) model in order to investigate to what extent heterogeneity in preferences is correlated with farmers' characteristics.

4.1. Mixed logit model

Our main results are displayed in Table 4. Consistent with economic theory, all reward-type attributes significantly increase the probability that farmers adapt to climate change. The coefficients of the cash payment variable, the free technical assistance variable and the subsidized insurance variable are statistically different from zero at the 1% level. From these coefficients, we calculate that 96% of farmers prefer adaptation strategies that provide free technical assistance and/or prefer subsidized insurance. These figures are given by $100 * \Phi(\beta_k/s_k)$ where Φ is the cumulative standard normal distribution, and β_k and s_k are the mean (Column 1) and standard deviation (Column 2), respectively, of the kth coefficient of the model.

Following Sheremet et al. (2018), we moreover computed the simulated probabilities of adapting to climate change for the different management options (Fig. 4). Although the levels shown in these graphs cannot be interpreted as such, especially because the adoption probabilities reported in choice experiments are generally overestimated (List and Gallet, 2001), it is nonetheless interesting to note that an adaptation strategy that would include both technical assistance and weather insurance was the most likely to be accepted for any management option and cash payment, while a strategy that would offer none of these two non-cash rewards has the lowest chances to be chosen.

Regarding preferences about management options, our results suggest that farmers prefer adopting coppice management or shortening the

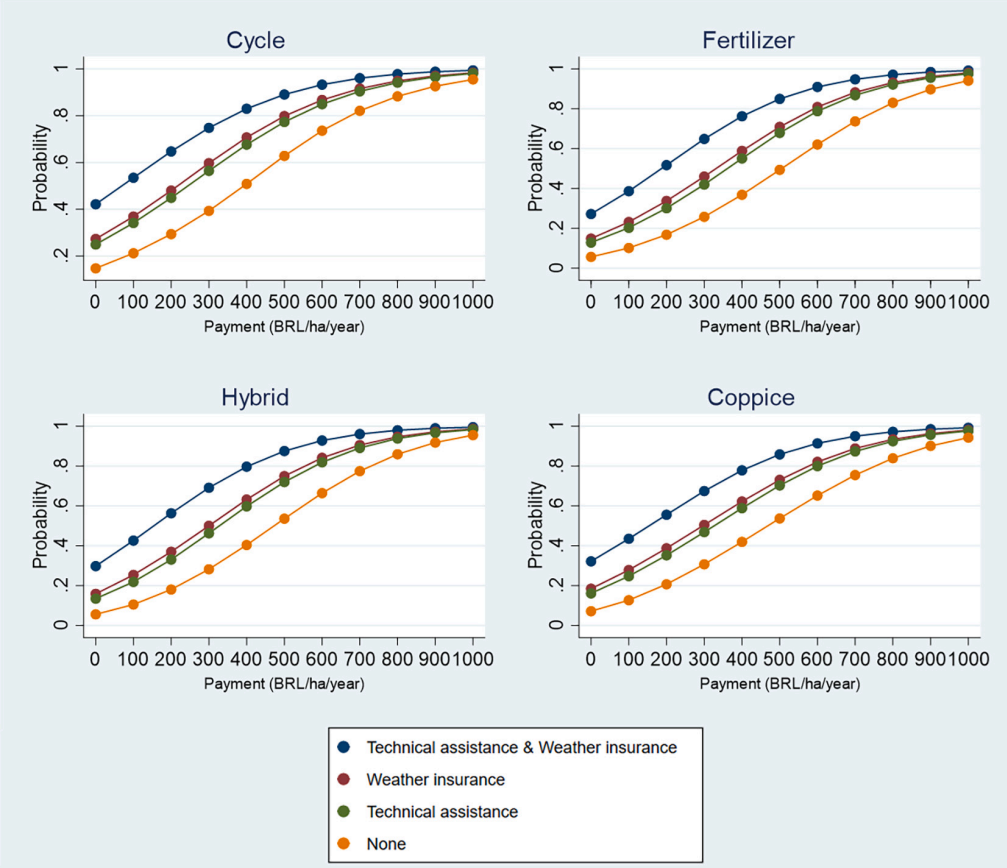

Fig. 4. Simulated probabilities of adapting to climate change for different management options.
Note: These diagrams show that an adaptation strategy that includes both technical assistance and weather insurance is the most likely to be accepted for any management option and cash payment, while a strategy that offers none of these two non-cash rewards has the lowest chances to be chosen.

cutting cycle rather than diminishing the plantation density (which is the reference management option in our model). We calculate from the estimated coefficients that 75% of farmers prefer adaptation strategies that involve a reduced cutting cycle and 69% prefer those that require coppice management (when confronting with diminishing the plantation density). Nevertheless, the WTA (Col 3) indicates that the farmers value reduced cutting cycles almost twice as much as coppicing: the average respondent is willing to receive 175 BRL to implement the density option instead of the cycle option, while he only requires 86 BRL to implement the density option instead of the coppice option. Finally, the results do not indicate any stronger (or lower) preference for the reduction of fertilizers or the adoption of drought-tolerant hybrids compared to the reference option.[12]

Column 2 of Table 4 moreover suggests that there is significant heterogeneity in respondents' preferences for nearly all attributes of the proposed adaptation strategies. This is particularly the case for the two preferred management options - coppice and reduced cutting cycles - and for the ASC (alternative specific constant). As for the reduction of fertilizers, which is not significant on average, we find on the contrary that the standard deviation is statistically significant, suggesting that at least some respondents have stronger or lower preference for this management option.[13]

In order to present this result about heterogeneity graphically, we estimate the individual-level coefficients for each attribute using the approach suggested by Revelt and Train (2000) and using Stata software. The distribution of the individual-level coefficients associated with each management option for which β_k and/or s_k appear significant in Table 4 is displayed in Fig. 3. In some cases, as for reduced cutting cycles and coppicing, the shape of the distribution of the coefficients suggests that we might have two different classes of farmers in our sample, which we investigate using a latent class model.

4.2. Latent class model

The LC model provides an alternative approach to describing our data, in which farmers are expected to have different motivations and purposes for their respective choices regarding adaptation to climate

[12] For the present study we opted for the practical solution to simply maximize the sample size given the research budget at hand, i.e., trying to overpower the study as much as possible (de Bekker-Grob et al., 2015). However, a limited sample size, like the one in our study, prevents detection of small effects. This is possibly the reason why we were not able to detect any effect for two adaptation options using the mixed logit model (the New Hybrid strategy and the Reduced Fertilization strategy). A consequence of this is that no conclusions should be drawn from insignificant results.

[13] The fact that the standard errors are significant for most variables indicates that the Mixed logit model fits well with the presented data. As a robustness check we however ran the most standard Conditional logit model. Same results hold on the average estimates. They are displayed in supplementary material, as well as alternative mixed logit models, assuming the price to be a random parameter and using different probability density functions for the distribution of coefficients.

Table 2
Sample descriptive statistics.

Variable		Unit	Mean	S.d.	Min	Max
Age		Year	52.34	14.16	23	86
Sex		1 = male; 0 = female	0.86	0.35	0	1
Education	Elementary	1 = yes; 0 = no	0.31	0.46	0	1
	Secondary	1 = yes; 0 = no	0.35	0.48	0	1
	Tertiary	1 = yes; 0 = no	0.34	0.47	0	1
Profession	Farmers	1 = yes; 0 = no	0.44	0.496	0	1
	Other profession	1 = yes; 0 = no	0.56	0.496	0	1
Household size		Number	3.42	1.37	1	7
Is eucalypt the main source of income?		1 = yes; 0 = no	0.24	0.43	0	1
Contribution of eucalypt on income	Less than 10%	1 = yes; 0 = no	0.37	0.48	0	1
	Between 10 and 30%	1 = yes; 0 = no	0.29	0.45	0	1
	Between 30 and 60%	1 = yes; 0 = no	0.24	0.43	0	1
	Between 60 and 90%	1 = yes; 0 = no	0.06	0.24	0	1
	More than 90%	1 = yes; 0 = no	0.04	0.19	0	1
Is livestock the main source of income?		1 = yes; 0 = no	0.19	0.39	0	1
Are there employees working all year long with the eucalypt crop?		1 = yes; 0 = no	0.42	0.49	0	1
Experience in eucalypt growing		1 = less than 15 yr; 0 = more than 15 yr	0.67	0.47	0	1
Do farmers have crop insurance?		1 = yes; 0 = no	0.062	0.24	0	1
Land area		Hectares	202.98	214.23	10	920
Eucalypt area		Hectares	89.28	84.9	7.5	400
Productivity (m3/ha/y)	Less than 35	1 = yes; 0 = no	0.15	0.36	0	1
	Between 35 and 40	1 = yes; 0 = no	0.34	0.47	0	1
	Between 40 and 45	1 = yes; 0 = no	0.3	0.46	0	1
	More than 45	1 = yes; 0 = no	0.46	0.5	0	1
	Not yet harvested	1 = yes; 0 = no	0.025	0.16	0	1
Zone	Belo Oriente	1 = yes; 0 = no	0.31	0.46	0	1
	Caratinga	1 = yes; 0 = no	0.29	0.45	0	1
	Cocais	1 = yes; 0 = no	0.2	0.4	0	1
	Pecanha	1 = yes; 0 = no	0.2	0.4	0	1
Cutting cycle		Years	7.02	0.81	5	9
Cost of implementation		BRL/ha	2856.21	2357.52	800	10,800

Table 3
Description of geographic zones in the study area.

Zone	Precipitation	Water deficit	Altitude	Major agronomic crops
Belo Oriente	1094 mm	459 mm	220 m	Pasture (mostly overgrazed)
Cocais	1348 mm	137 mm	791 m	Eucalypt
Caratinga	1175 mm	324 mm	578 m	Coffee and Pasture
Pecanha	1171 mm	209 mm	780 m	Eucalypt and Pasture

Source: CENIBRA.

change. To explore this possibility, the model assigns farmers to groups based on their preferences and other (latent) individual-specific variables. The LC model combines characteristics of the individual, such as socioeconomic characteristics with the stated behavior in the choice sets (Beck et al., 2013). Preferences are presumed to be homogeneous within each latent class but different between classes (Colombo et al., 2009). In this model, we focus on a selection of individual characteristics about the farmers and their farm (Wilson, 1997; Vanslembrouck et al., 2002; Horne et al., 2006; Ruto and Garrod, 2009), as well as geographical features (Espinosa-Goded et al., 2010; Broch et al., 2013).

We select the optimal number of latent classes in a model using the Bayesian information criterion (BIC) and the Akaike information criterion (AIC), both of which pointing to a two-class model (Table 5). Our main results are displayed in Table 6. The smaller class (21% of the sample) is mainly comprised of male farmers who engage in cattle ranching activity as their main source of revenue and possess large eucalypt plantations. These farmers are heavily dependent on the income from these plantations and are located in a region considered to be more sensitive to climate change (Table 3).

Results displayed in the upper part of Table 6 call for four comments. First, while both groups exhibit a strong (and comparable in magnitude) preference for the technical assistance attribute, insurance does not seem to play a decisive role anywhere other than in Class 1 (though this

Table 4
Mixed logit model estimates.

Attribute	Mean		Std.Dev.		WTA
Money	0.009	***			
	(0.001)				
Assistance	1.298	***	0.758	**	−149
	(0.227)		(0.313)		(−95; −175)
Insurance	1.890	***	1.053	**	−217
	(0.706)		(0.485)		(−25; −274)
Fertilizer	0.187		1.050	***	
	(0.671)		(0.355)		
Hybrid	0.460		−0.281		
	(0.649)		(0.934)		
Cycle	1.529	***	2.306	***	−175
	(0.475)		(0.492)		(−51; −279)
Coppice	0.750	***	1.482	***	−86
	(0.263)		(0.343)		(−30; −135)
ASC	−0.471		2.255	***	
	(0.796)		(0.519)		
Log-likelihood	−475.60				
LR χ^2	81.95				
P-value	0.00				
Nb. of observations	2400				

Notes: ***, ** and * indicate that the estimated coefficients are statistically significant at the 1%, 5%, and 10% levels, respectively. Standard errors are given in parentheses. Last column gives the willingness-to-accept (WTA) estimates. Confidence intervals of WTA are given in parentheses.

Table 5
Criteria for determining the optimal number of classes.

Classes	LLF	CAIC	BIC
2	−488.4	1095.2	1073.2
3	−460.8	1115.4	1079.4
4	−446.7	1162.5	1112.5
5	−434.2	1212.9	1148.9

Table 6
Latent class model estimates.

Attribute	Class 1		Class 2	
Money	0.005	***	0.007	***
	(0.002)		(0.001)	
Assistance	0.876	**	1.002	***
	(0.432)		(0.181)	
Insurance	0.951	⋄	0.459	
	(0.619)		(0.813)	
Fertilizer	1.308	⋄	1.066	
	(0.884)		(0.840)	
Hybrid	1.242	⋄	1.282	⋄
	(0.872)		(0.843)	
Cycle	3.626	***	0.257	
	(0.833)		(0.254)	
Coppice	2.162	***	0.430	**
	(0.670)		(0.189)	
ASC	2.826	***	-0.639	
	(0.965)		(0.541)	
Share	0.211		0.789	
Class membership				
Gender	18.785		0.000	
(male = 1)	(496.59)			
Eucalypt as main income	7.128	**	0.000	
(1 if larger than 30%)	(3.32)			
Eucalypt area	0.037	**	0.000	
(ha)	(0.017)			
Location	9.382	**	0.000	
(1 if vulnerable zone)	(3.983)			
Livestock as main income	4.678	*	0.000	
(yes = 1)	(2.472)			
Constant	−32.707		0.000	
	(496.59)			

Notes: ***, **, * and ⋄ indicate that the estimated coefficients are statistically significant at the 1%, 5%, 10% and 15% levels, respectively. Standard errors are given in parentheses.

result lacks precision). Second, although preferences for the coppice option appears significant in both groups, they are stronger in Class 1. Third, the reduced cutting cycle option now appears to be the preferred option for Class 1 only and not Class 2. Finally, the significant ASC in Class 1 suggests that a *status quo* effect occurs, the positive sign of the coefficient indicating that moving away from the current situation may have a negative effect on respondents' decisions to opt for adaptation in Class 1.[14] Such an effect does not appear in Class 2.[15]

5. Discussion

Although the results of this study cannot be generalized, our findings enable us to identify three main takeaways.

5.1. Farmers' valuation of reward attributes

First, we found that farmers particularly value adaptation strategies based on free technical assistance and/or subsidized weather insurance. This result is consistent with results provided in previous studies conducted in other contexts (Abebe et al., 2013; Andow et al., 2017; Minten et al., 2009; Kuhfuss et al., 2016; Vignola et al., 2012; Prasada, 2020). When confronting the values that respondents place on technical assistance and subsidized weather insurance with market prices, we can conclude that farmers tend to overestimate the value of both of these attributes. Indeed, a subsidized weather insurance incentive has a market value of approximately 175 BRL per hectare per year, which is approximately 42 BRL lower than the WTA of the farmers who participated in this study (217 BRL). We also find that growers value technical assistance at nearly 149 BRL per hectare per year, while the average cost of technical assistance, considering the average 7 year cycle, is about 36 BRL per hectare per year (Rode et al., 2014). This result makes sense given that Brazilian eucalypt producers tend to be unaware of the existence of this sort of insurance; in our sample for example, less than 7% of farmers had an insurance policy (Table 2). Note also that respondents are not irrational when they value technical assistance and insurance. The analysis of the choice experience simply indicates that they place a monetary value on these attributes that is higher than the real cost which would be incurred by the implementation of these instruments in reality.

5.2. Farmers' valuation of management options

Second, we found that farmers highly value adaptation strategies that bring them money today rather than in the future (shortening the cutting cycle, typically). This result is consistent with the standard hypothesis that economic agents value the present more than the future (Samuelson, 1937). It is also in line with findings from the behavioural economics literature, which suggests that individuals may have present-biased preferences (Laibson, 1997; Cohen et al., 2020). Moreover, if the rotation is shorter, then the exposure to the risk of drought is reduced. Moreover, if the rotation is shorter, then the exposure to the risk of drought is reduced. Such preferences, however, could have environmental as well as economic consequences. First, increasing the frequency of clearcuts could result in soil compaction and an increase in nutrient exports since nutrient remobilizations decrease the concentrations in stemwood throughout the rotation (Sette et al., 2013). Harvesting young trees could therefore increase soil nutrient depletion and the need for fertilizers to maintain high yields. Second, the quality of the wood obtained from young trees wood may not be optimal for cellulose production because wood density increases with tree aging (Sette et al., 2012). Additionally, increasing the frequency of harvesting operations could raise the final cost of a meter cubic of wood.

The marked preference for the coppice strategy (the second most preferred option of respondents) can be explained by the fact that a number of farmers already know this management practice and possibly employ it at least partially.[16] In such a case, adopting this strategy could be done without a significant decrease in income. Additionally, the possibility of multiple earnings from more rotations combined with the smaller cultivation costs can also make the eucalypt coppice system more economically attractive than alternative options (Ribeiro and Graca, 1996).

Another important finding of our study is the reluctance of farmers to decrease the plantation density to cope with prolonged drought periods, while this option is often highlighted by researchers in ecophysiology and silviculture (Booth, 2013; Goncalves et al., 2017). One plausible reason for this is that switching from one tree per area of 3 m by 3 m to one tree per area of 3 m by 4 m would mean replacing the usual 1111 trees per hectare by 833 trees per hectare – a loss that farmers would overestimate. Moreover, this option has some drawbacks, like the time needed to reach canopy closure, which would increase the need for weeding during the early growth stage.

5.3. Heterogeneity in farmers' preferences

Third, we found that farmers' preferences for adaptation strategies are heterogenous. Our results from the mixed logit model indeed suggest that respondents' preferences are heterogeneous for a number of attributes, which include not only the rewards but also the adaptation

[14] None of the 80 farmers who agreed to participate in the experiment opted for the status quo option in all cards.

[15] These results can be found also using a mixed logit model that interacts farmers' characteristics with attributes. Such a model is displayed in the supplementary material.

[16] Such information was collected in the field as part of the informal discussion with farmers.

strategies themselves. Our results from the latent class model moreover suggest that adaptation to climate change remains a major concern mainly for the most vulnerable out-growers (named Class 1), while other farmers who are less dependent on income from eucalypt plantations exhibit much less concern about it (named Class 2). These results are consistent with that highlighted by previous choice experiments involving farmers exposed to climate change in other regions of the world (Nthambi et al., 2021; Khanal et al., 2019; Schaafsma et al., 2019) just to cite some recent ones). Our findings moreover suggest that adaptation policies should target the most vulnerable smallholders first (Donatti et al., 2018), whether with the objective of supporting the standard of living of households that depend on the cultivation of eucalypt or with the objective of preserving water resources. This is because our analysis shows that this category of the population displays more marked preferences for the proposed adaptation strategies.

6. Conclusion

This paper reports the results of a CE study that investigates how eucalypt farmers arbitrate between changes in silvicultural management practices and the monetary compensation offered in exchange for adopting these practices. Our approach compares several innovative silvicultural strategies, as well as a variety of "rewards" (or support) for undertaking these strategies.

A mixed logit model and latent class model were used to analyse the CE data. When we analyse farmers' preferences as a whole, we find that adaptation to climate change is more likely to occur by reducing the eucalypt cutting cycle, since the majority of farmers have a predilection for this adaptation strategy. However, this practice could entail negative environmental and economic impacts. Furthermore, the farmers in our survey were shown to be extremely averse to reducing the density of eucalypt trees on their plantations. Since reducing tree density could be a more sustainable strategy than reducing the length of the cutting cycle, governmental, non-governmental or private bodies may want to consider supporting this practice. This, however, this can only be done at a high cost.

When we analyse heterogeneity in farmers' preferences for adaptation strategies, a two-class model explained the observed choices and provided a clear segmentation between farmer types. We detected that both groups are likely to adapt to the upcoming global changes, but not in the same way. In particular, a small group of the most vulnerable farmers appear to drive the result on the preference to adapt to climate change by reducing the cutting cycle. These results suggest that a customized approach to payments for ecosystem services[17] would make sense in this context.

Lastly, this study is only looking at one part of the problem (that of Eucalypt farmers) and not that of society as a whole (which would then include water consumers or other types of farmers). It gives us clues about farmers' willingness to adopt the proposed silvicultural changes and provides information regarding the order of magnitude, in monetary terms, with which farmers value each strategy. Overall, our results suggest that adaptation may not require complex or expensive changes. However, management should consider the maintenance and provision of environmental services across the landscape. One direction for further research is to better understand the divergence between private and social optima and define strategies for climate change adaptation that assure long-term sustainability for the planted forest sector.

[17] PES have been generally defined as transfers of resources between stakeholders in order to encourage the agreement between individual and/or collective land use with the public interest in the management of natural resources (Muradian et al., 2010).

Declaration of Competing Interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

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Appendix A. Supplementary data

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