From Deforestation to Reforestation:

The Role of General Deterrence in Changing Farmers' Behavior*

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April 11, 2023

Abstract

We investigate the role of general deterrence in improving forest law enforcement in the Brazilian Amazon. Using a difference-in-differences strategy and novel farm-level data, we find that sanctions curbed deforestation and promoted reforestation among punished farmers and their neighbors. Heterogeneities reveal that even sanctions lacking incapacitation components lead to substantial behavioral changes and that farmers' responsiveness to sanctions coincides with the government's commitment to enforcement. We find no evidence of significant strategic responses regarding spatial displacement or monitoring evasion. Overall, sanctions prevented 1.6 billion tonnes of CO_2 emissions between 2006-2019, equivalent to 31% of US emissions in 2021.

Keywords: Deforestation, Reforestation, General Deterrence, Law Enforcement
JEL Codes: K42, Q23, Q28, Q58

*We thank Edson Severnini, Eduardo Souza-Rodrigues, and audiences at PUC-Rio for helpful comments and suggestions. Any errors are our own.

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1 Introduction

Deforestation generates a range of negative externalities. At the global level, it is a significant contributor to climate change, accounting for 20% of annual greenhouse gas emissions (Gullison et al., 2007). At the regional level, it reduces rainfall and harms agricultural production (Lawrence and Vandecar, 2015; Araujo, 2022). At the local level, it threatens the biodiversity and livelihood of local communities (Gandour, 2021). Governments rely on conservation policies to align agents' deforestation decisions with their social costs and combat excessive extraction. However, weak state capacity often undermines these efforts, particularly in developing countries where deforestation is more prevalent (Jayachandran, 2022; Balboni et al., 2022). There is a growing body of knowledge on how specific policies affect deforestation, such as payments for ecosystem services (Jayachandran et al., 2017), rural credit conditional on conservation requirements (Assunção et al., 2020), land titling programs (Probst et al., 2020), and command-and-control actions (Assunção et al., 2022a), but an understanding of the mechanisms mediating the effects on behavioral changes remains scarce.

This paper aims to fill this gap by examining the role of general deterrence as a key mechanism in changing farmers' forest change decisions.[1] Specifically, we explore environmental sanctions' direct and spillover effects on curbing deforestation and promoting reforestation in the Brazilian Amazon. The hypothesis is that farmers exposed to punishment update their perceived risk of violating forest laws and reduce the demand for deforestation and deforested lands as the expected cost of engaging in illegal activities increases. For spillover effects, we look at farms only exposed to punishment through an adjacent neighbor, isolating the informational channel that characterizes general deterrence. For direct effects, general deterrence can be even stronger, but incapacitation through losses of deforestation-specific capital can also be relevant. Hence, we provide complementary evidence to disentangle these mechanisms. We explore the heterogeneous effects of sanctions with varying incapacitation potentials and analyze how farmers' responsiveness to sanctions varies with changes in the government's overall commitment to law enforcement.

The Brazilian Amazon provides a unique setting to study this topic for several reasons. Farms concentrate 53% of the Amazon's deforestation, allowing us to track individual behavioral responses at scale.[2] There are strict conservation requirements,

[1]The relevance of the general deterrence mechanism has been highlighted in other contexts, such as in urban crimes (Chalfin and McCrary, 2017), financial crimes (Huttunen et al., 2022), workplace safety and health violations (Johnson, 2020), and water pollution violations (Shimshack and Ward, 2005).

[2]The remaining 47% of deforestation occurs in indigenous lands, protected areas, rural settlements,

such that almost all deforestation is illegal.[3] In the mid-2000s, the Federal Government implemented a series of policies to curb the rise in deforestation.[4] On the one hand, deforestation reduced by 80% between 2004-2012 (see Figure A.1), with causal evidence that implementing the System for Real-Time Detection of Deforestation (DETER),[5] and increasing targeted actions to punish illegal deforestation[6] were key drivers in this process (Gandour, 2021). On the other hand, only 7% of farms with deforestation received any punishment, 13% of deforested areas received a fine in the same year (Ferreira, 2022), and 10% of fines were paid (Schmitt, 2015). We hypothesize that general deterrence helps explain this apparent contradiction, as each sanction may persistently change the behavior of multiple agents.

To identify the average treatment effects for each group of treated farms, we combine novel spatial data at the farm-year level with a staggered difference-in-differences (DD) framework that exploits the timing and location of environmental sanctions between 2000-2021. Specifically, we compare the average outcome evolution for each treatment cohort in a given year to the average evolution across all never-punished farms in the same year. These comparisons are equivalent to separately applying a two-period/two-group DD canonical estimator for each treatment cohort and year (Callaway and Sant'Anna, 2021). We use geo-referenced punishment information from Brazil's primary environmental agency, the Brazilian Institute for the Environment and Renewable Natural Resources (IBAMA, 2022); high-resolution forest change outcomes from the MapBiomas Project (MapBiomas, 2021a); high-resolution carbon stock data from Global Forest Watch (GFW, 2022); and farm boundaries from the Institute of Forestry and Agricultural Management and Certification (Imaflora) 's Atlas of Brazilian Agriculture (Freitas et al., 2018).

Our analysis shows that environmental sanctions can effectively curb deforestation and promote reforestation. Punished farmers decrease deforestation by 49%, while adjacent farmers decrease by 21%. Additionally, farmers increase reforestation by 13% and 6.9%, respectively. These effects persist for at least five years after punishment. The relevance of spillover effects supports the general deterrence mechanism by showing that sanctions increase the perceived risk of violating forest laws among farmers who only witness the punishment of adjacent neighbors. Complementary evidence

quilombos, military areas, and undesignated public forests where multiple agents are responsible for the forest changes, such that we cannot entirely separate direct and spillover punishment effects.

[3] Forest laws requiring at least 80% of forest cover on private properties have existed since 1996.

[4] The policies were implemented under the umbrella of Brazil's Action Plan for the Prevention and Control of Deforestation in the Legal Amazon (PPCDAm), launched in 2004.

[5] A near-real-time satellite monitoring system from Brazil's National Institute for Space Research (INPE).

[6] The number of sanctions per deforested area increased approximately nine-fold from 2004 to the peak in 2009, as shown in Figure A.1.

also supports general deterrence as a key driver of the direct effects. Heterogeneity by type of sanction shows that even standalone fines with the lowest potential for incapacitation cause large behavioral changes. Furthermore, heterogeneity over time shows that farmers' responsiveness to environmental sanctions decreases as the government's overall commitment to forest law enforcement deteriorates, suggesting that farmers may not readjust their perceived risk of punishment in response to sanctions' exposure when there are mixed enforcement signals.

Next, we investigate whether farmers react strategically to circumvent forest law enforcement. To this end, we explore two potential strategies: attempts to evade monitoring deforesting below detection limits and spatial displacement to avoid targeted areas. Our findings show that deforestation reduction occurs across all categories, regardless of the monitoring degree. Furthermore, we expand the possible range of spillover effects by including non-adjacent neighbors into three distance rings (<10km, 10km-50km, 50km-200km). We find significant reductions on deforestation among adjacent and non-adjacent farms until 10 kilometers (84% of farms), no effects between 10-50 kilometers (15% of farms) and a noisy increase between 50-200 kilometers (1% of farms). These results indicate that changes in deforestation patterns and spatial displacement are not significant strategic response margins in this context, increasing aggregate policy effectiveness.

We perform various robustness checks to corroborate our results. First, to account for differential pre-trends, we use linear extrapolations of the observed differences as an alternative counterfactual trajectory and rely on the partial identification methods from Rambachan and Roth (2023) to conduct inference. Second, we test alternative estimators, conditioning on the pre-trends and the municipality by property size groups. Third, we split the sample by punishment recurrence. Fourth, we check whether the lack of forests due to past deforestation drives the results. Fifth, we analyze heterogeneities in property types. Sixth, we test alternative outcome transformations. Seventh, we change the control group to late-treated farms instead of never-treated ones.

To assess the sanctions' overall impact, we construct a counterfactual scenario in which we assume no sanctions were issued between 2005-2018. We find that farmers' deforestation would have increased by 48% relative to what we observed between 2006-2019, indicating that the existence of sanctions saved 2.266 million hectares of forest and avoided 1.604 billion tonnes of CO_2, equivalent to 31% of US emissions in 2021 (Friedlingstein et al., 2022). These findings provide insights into how general deterrence made environmental sanctions a powerful tool in improving forest conservation at scale in the Amazon by changing farmers' behavior and overcoming the region's low punishment and fine collection rates.

Our work contributes to three areas of the existing literature. We add to the literature on farmers' forest change decisions. Previous studies have identified a range of factors that influence farmers' decisions, such as payments for ecosystem services (Jayachandran et al., 2017), rural credit conditional on conservation requirements (Assunção et al., 2020), land titling programs (Probst et al., 2020), and command-and-control actions (Assunção et al., 2022a), but less is known about the specific channels mediating the policy effects. In this study, we highlight the role of general deterrence as a key mechanism for the impact of environmental sanctions on deforestation and reforestation in targeted farms and surrounding areas. Complementing studies that highlight the general deterrence in other contexts, such as in urban crimes (Chalfin and McCrary, 2017), financial crimes (Huttunen et al., 2022), workplace safety and health violations (Johnson, 2020), and water pollution violations (Shimshack and Ward, 2005).

Next, we contribute to the literature on law enforcement and spillovers. There is growing recognition in the environmental policy literature on the importance of accounting for spillovers in policy evaluations (Pfaff and Robalino, 2017). In the crime literature, there is an ongoing debate about the extent to which targeted law enforcement deters or displaces crime. On the one hand, a review by Braga et al. (2019) highlights that most studies find reductions in crime both in targeted areas and their surrounding areas. On the other hand, Blattman et al. (2021) argue that there is mixed evidence on the direction of spillovers, with many studies suffering from low statistical power. Also, their large-scale randomized controlled trial shows that intensifying state presence has modest direct effects on crime and leads to crime displacement to nearby streets. In other contexts, Banerjee et al. (2019) and Gonzalez-Lira and Mobarak (2021) have shown that agents can learn and react strategically to targeted enforcement, reducing its effectiveness. We add to this literature by analyzing spillovers from field-based environmental sanctions in a developing country while observing the universe of illegal deforestation events in the whole biome subject to the same regulation and monitoring system. We provide evidence against strategic reactions to enforcement by showing diffusion of general deterrence prevailing over displacement and no increase in deforestation under varying monitoring levels.

Finally, our study contributes to the literature on environmental law enforcement and forest change in the Brazilian Amazon. To the best of our knowledge, this is the first study to examine this relationship at the farm level, which allows us to track individual behavioral responses, highlight the general deterrence mechanism, and better understand how environmental sanctions significantly reduced deforestation despite low rates of punishment and fine payments. Additionally, we are the first to use more detailed forest change data from MapBiomas (2021a), enabling us to estimate deforestation and reforestation impacts even among small farms and examine potential strategic reactions across deforestation types with varying monitoring levels. We also incorporate high-resolution carbon stock data to translate impacts on deforestation area to deforestation CO_2 emissions. Previous studies at the municipal level find that enforcement effectively curbs deforestation but cannot distinguish between general deterrence and incapacitation mechanisms (Assunção et al., 2022b,a). Studies at the pixel level improve the granularity of the data but cannot identify individual behavioral responses on deforestation (Börner et al., 2015; Burgess et al., 2019; Ferreira, 2022) or reforestation (Assunção et al., 2019).

The remainder of the paper proceeds as follows. Section 2 discusses the institutional context, focusing on deforestation and law enforcement characteristics in the Brazilian Amazon. Section 3 describes the data and presents descriptive statistics. Section 4 details the staggered difference-in-differences empirical strategy. Section 5 presents the results and discuss mechanisms. Section 6 presents the counterfactual exercise to assess the aggregate impact. Section 7 concludes with the main takeaways and policy implications.

2 Institutional Context

2.1 Deforestation in the Brazilian Amazon

The Brazilian Amazon is one of the world's most important forests in terms of its biodiversity and its role in regulating the global climate. Despite the stringent environmental laws aimed at conserving the forest,[7] deforestation in the region has been a major issue, driven primarily by agricultural activities and illegal land grabbing (Gandour, 2021).[8] In most cases, deforestation is considered an environmental crime,[9] but offenders often remain in the area to collect benefits, hoping not to be punished.

In 2004, Brazil's Federal Government launched PPCDAm, an integrated plan aimed at improving forest law enforcement and curbing the rise in deforestation.[10] The plan in-

[7]They prohibit deforestation inside protected areas (conservation units and indigenous lands) and require the conservation of at least 80% of private property's native vegetation.
[8]Around two-thirds of deforested areas are converted to pasture for cattle grazing (Gandour, 2021).
[9]Azevedo et al. (2022b) estimate that more than 99% of the deforestation area was illegal between 2019 and 2021.
[10]For a more detailed overview of the plan, refer to Gandour (2018).

cluded several key components, such as creating a near-real-time satellite monitoring system (DETER), using new enforcement sanctions, adding conservation requirements for rural credit, targeting actions with a list of priority municipalities, and expanding protected areas. These efforts resulted in a 80% reduction in deforestation rates in Brazil within a decade.[11]

However, a significant fraction of deforestation was not punished, and the political momentum pro-conservation was short-lived. For instance, only 7% of farms with deforestation received any punishment, 13% of deforested areas received a fine in the same year (Ferreira, 2022), and 10% of fines were paid (Schmitt, 2015). Furthermore, deforestation rates reversed in 2012 and started increasing again, coinciding with an economic crisis and weakening environmental efforts under political pressure (Burgess et al., 2019).

In 2012, the revision of the Forest Code resulted in an amnesty of past illegal deforestation for 90% of private properties (Burgess et al., 2019). There were also cuts in the leading environmental agency with reductions in the overall budget (20% between 2014-2020), the operational expenditures in the Amazon (40% between 2014-2020), the number of enforcement officers (Burgess et al., 2019), and the number of sanctions per deforested area (56% between 2012-2019, see Figure A.1).

2.2 Environmental Law Enforcement

In Brazil, environmental law enforcement is a shared responsibility among municipal, state, and federal governments. However, in practice, IBAMA has taken on most of the responsibility since its creation in 1989, particularly regarding monitoring, inspecting, and punishing deforestation in the Amazon. IBAMA is also responsible for enforcing other environmental laws related to pollution, animal trafficking, and predatory fishing.

To carry out its law enforcement duties, IBAMA needs to know where the infractions are happening. The agency uses multiple sources of information such as anonymous complaints, intelligence reports, patrolling, and checkpoints (Schmitt, 2015). DETER was a game changer in providing information because it issues georeferenced deforestation alerts in near-real time and covers the full extent of the Brazilian Amazon. Thus improving IBAMA's detection capacity and allowing faster and better-targeted responses (Assunção et al., 2022a).

[11] For a summary of studies that provide evidence supporting the causal link between environmental policies and the decline in deforestation, see Gandour (2018).

After detecting potential infractions, IBAMA relies on field operations with support from other actors, such as the federal and state police, to inspect and punish the offenders. When there is evidence of illegal deforestation, an officer writes an infraction notice identifying the offender, describing the violation, specifying the legal basis, and suggesting a fine value. The infraction notice is only a communication for the offender that the State will open an administrative process against him. After the notice, the competent judging authority analyzes the process and decides whether or not to maintain the fine. To prevent further deforestation and enable reforestation, the officer may impose additional penalties such as embargoes in designated areas and seizure or destruction of equipment or products related to the illegal activity (Schmitt, 2015).[12]

These administrative sanctions increase the cost of deforestation for offenders. Even if the offender does not pay the fine, he must still go through the administrative process, which can be time-consuming and may require hiring a lawyer. Additionally, having embargoed areas can lead to credit restrictions through Central Bank's Resolution 3,545, which added environmental requirements for lending rural credit. The financial losses are significant and immediate with the seizure and destruction of equipment and products. The offenders can also be criminally investigated and prosecuted.

3 Data

To conduct the empirical analysis, we construct a panel dataset at the farm-year level, covering all private properties in the Brazilian Amazon from 2000 to 2019. Our primary data sources include novel spatial information on deforestation, reforestation, and forest carbon stock. We also rely on administrative records of environmental sanctions from IBAMA, Brazil's leading agency responsible for enforcing environmental laws. All of the data we used are publicly available.

3.1 Unit of Analysis: Farms

Data on private properties comes from the Institute of Forestry and Agricultural Management and Certification (Imaflora)'s Atlas of Brazilian Agriculture (v.1812) (Freitas et al., 2018), which gathers and harmonizes the most up-to-date land tenure informa-

[12] See Schmitt (2015) for a more detailed description of the sanctioning administrative process and IBAMA's actions.

tion from 18 official sources based on a cross-section of 2018.[13] We focus on farms as the unit of analysis because they have a single individual responsible for the land, allowing us to track agents' behavioral responses over time by the changes occurring inside the property.[14]

We extract the farms' boundary, area, size (small, medium, or large),[15] and type (registered in the National Institute for Colonization and Agrarian Reform (INCRA), self-declared in the Environmental Rural Registry (CAR), or regularized in the Terra-Legal program). We view self-declared farms as essential to avoid observing only those with formal titling that may be less engaged in environmental crimes. In total, there are 365,682 farms, occupying 96 million hectares (23% of the Amazon's area) and responsible for 53% of the Amazon's deforestation.

A limitation of the data is the lack of occupation dates, which prevents us from tracking possible changes in tenure or ownership. Therefore, we assume tenure stability during our sample period for the principal analysis. In robustness exercises, we separate properties by size, type, and intersection with undesignated public forests to check if a subset of the farms, more subject to tenure changes, drives the effects. For example, the regularization process in the Terra Legal program began in 2009, although there was a requirement for active occupation before 2004. Moreover, an overlap with undesignated public forests indicates potential cases of illegal land grabbing.

3.2 Outcomes: Deforestation and Reforestation

Data on deforestation and reforestation comes from Mapbiomas, which generates land use and land cover annual maps with 30m pixel resolution from 1985 to 2020 (MapBiomas, 2021a).[16] A deforestation event occurs when a pixel changes its classification from a Natural category to an Anthropic one, and a reforestation event occurs when an Anthropic category changes to a Natural one. Spatial and persistence criteria are used to avoid false positives, removing transitions smaller than 1 hectare and initial/final

[13] The compilation covers 82.6% of the country (Freitas et al., 2018) and uses a hierarchical approach to deal with spatial overlaps across sources.

[14] In public areas, multiple agents are responsible for a single land, and changes due to outsiders are more common.

[15] Size categories are defined based on the official metric of fiscal modules that vary across municipalities. A fiscal module is a minimum area needed to ensure the economic viability of exploring a rural establishment in a Brazilian municipality (Assunção et al., 2017). Small farms have less than four fiscal modules, medium 4-15, and large more than 15.

[16] MapBiomas Project - is a multi-institutional initiative to generate annual land use and cover maps based on automatic classification processes applied to satellite images. The complete project description can be found at http://brasil.mapbiomas.org.

years.[17] We also include an additional filter removing non-tropical forest areas (INPE, 2017). We observe the universe of tropical forest change events measured by an independent initiative, which allows us to avoid the usual reporting issues in measuring illegal behaviors.

We extract the total deforestation area of primary forests[18] by farm-year from 2000 to 2019, and the total reforestation area from 2000 to 2018. We also divide the deforestation area into different categories based on the monitoring degree: small polygons below 3 ha, which are never monitored; medium polygons between 3-25 ha, which are monitored after 2015; large polygons above 25 ha, which have been monitored since 2005; and secondary vegetation polygons, which are never monitored or measured by the official systems.

To translate deforestation area to CO_2 emission, we incorporate high-resolution aboveground biomass data from Global Forest Watch (GFW, 2022). We spatially match the biomass density data with the deforestation polygons and transform to CO_2 emissions by multiplying by the polygon area, dividing by half (carbon stock represents 50% of the biomass), and multiplying by 3.67 (carbon dioxide (CO_2) mass is equivalent to 3.67 of carbon (C) mass).

To account for variations in property size and include responses from both the extensive and intensive margins, we normalize the raw measures using the inverse hyperbolic sine (IHS) transformation. We also explore alternative measures, such as an indicator for the occurrence of the event (extensive margin), the log transformation removing observations with zero areas (intensive margin), the division by property areas (alternative normalization), and the raw measures with no normalization.

3.3 Treatment: Environmental Sanctions

IBAMA's public administrative records provide data on environmental sanctions, including fines, embargoes, and seizures (IBAMA, 2022). Among directly punished farmers: 70% receive a fine plus an embargo, 14% only a fine, 9% all sanctions, and 7% a fine plus a seizure.

We combine and aggregate all deforestation-related sanctions at the farm-year level from 2000 to 2021, constructing three mutually exclusive treatment groups: the direct

[17]For deforestation, the pixel has to persist as Natural at least two years before the change and persist as Anthropic at least one year after the conversion (1987-2019). For reforestation, the pixel has to persist as Anthropic at least two years before and as Natural at least three years after (1987-2018).

[18]Primary forest is defined as a forest with no previous deforestation, at least since 1987.

group, based on the first year a farm receives any sanction; the adjacent group, based on the first year an adjacent neighbor receives any sanction; and the direct and adjacent group, based on the first year a farm or an adjacent neighbor receive any sanction. We also separate farms with different combinations of sanctions in a heterogeneity exercise. We define the cohorts by the first year of exposure because 78% of the farms are directly punished only once.[19]

At the beginning of the sample period, some farms lack precise spatial coordinates, as shown in Table A.1. This absence restricts our ability to match sanctions to farms for these years and can attenuate our estimates because we may have control units with non-observed treatment. Therefore, we use data from 2000 to 2004 to identify punishments before the satellite monitoring began but only consider treatment cohorts starting in 2005 after the start of PPCDAm and when the number of farms was larger.

3.4 Covariates

Data on covariates come from multiple sources. From MapBiomas Project, we extract the primary forest coverage in 2000 and the average number of cloudless images available to classify the land use pixels from 2000 to 2019 (MapBiomas, 2021b,c). From Climate Prediction Center (CPC), we extract the total precipitation and average temperature from 2004 to 2019 (NOAA-CPC, 2020a,b). From DETER/INPE, we extract the average cloud coverage that blocks the monitoring system from 2004 to 2019 (INPE, 2021).

3.5 Sample Selection

We start with all the 365,682 farms in the Brazilian Amazon and use the following criteria to select our sample for the analysis. First, we drop 165,253 farms with no deforestation between 2000-2019 because they are not available for punishment. Second, we drop 6,659 farms with less than 10% of primary forest coverage in 2000 to avoid mechanical effects of reducing deforestation due to a lack of forest. Third, we drop 3,636 farms with no deforestation before the first punishment to guarantee that we are capturing punishments motivated by deforestation. Fourth, we drop 2,289 farms exposed to punishment before 2005 to focus on sanction effects after the monitoring system implementation. Finally, we split the remaining 187,853 farms into three treatment and two control groups: 3,754 farms are in the direct treatment group (first

[19] 16% are punished twice, 4% three times, 2% at least four times.

direct punishment between 2005-2018), 30,340 in the adjacent treatment group (first adjacent neighbor punishment between 2005-2018), 7,430 in the direct and adjacent treatment group (direct and adjacent neighbor punishment between 2005-2018); 2,680 in the late-treated control group (first direct or adjacent neighbor punishment between 2019-2021); and 143,441 farms in the never-treated control group (no direct or adjacent neighbor punishment).

3.6 Descriptive Statistics

Table 1 presents the descriptive statistics for the pre-treatment period, showing the breakdown by group. Treated farms exhibit, on average, higher levels of deforestation, reforestation, CO_2 emission, precipitation, property area, and forest coverage in the year 2000, as well as lower levels of cloud coverage compared to the control farms.

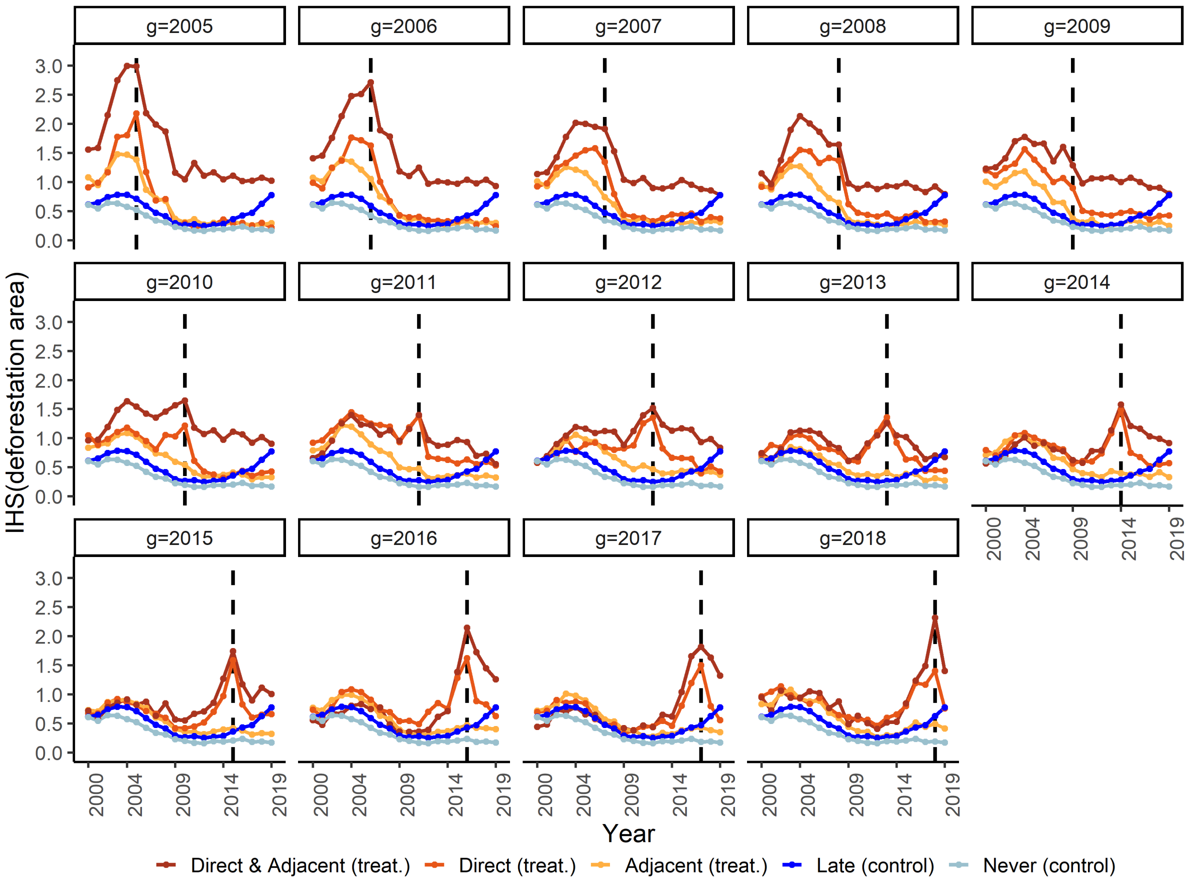
Table 1: Descriptive Statistics By Treatment Group

	Treatment			Control	
	Direct & Adjacent	Direct	Adjacent	Late	Never
Deforestation (ha)	27.3	10.9	8.0	3.4	1.4
	(85.64)	(30.71)	(24.95)	(7.16)	(4.2)
Reforestation (ha)	4.9	3.4	2.1	1.4	1.1
	(20.97)	(17.08)	(7.73)	(4.82)	(5.52)
CO2 Emissions (1,000 t)	11.6	4.8	3.2	1.6	0.6
	(37.23)	(14.03)	(9.89)	(3.38)	(1.72)
Quality (# images)	13.0	12.3	12.4	12.1	11.8
	(3.53)	(3.58)	(3.85)	(3.85)	(3.97)
Precipitation (1,000 mm)	2.9	2.8	2.8	2.2	2.3
	(0.55)	(0.58)	(0.55)	(0.22)	(0.37)
Temperature (celsius)	26.9	27.0	27.0	27.2	27.3
	(0.64)	(0.75)	(0.73)	(0.66)	(0.74)
Property Area (1,000 ha)	1.8	0.9	0.6	0.5	0.2
	(6.64)	(3.58)	(1.8)	(1.7)	(2.11)
Property with Forest (%)	83.3	71.7	72.2	75.6	60.4
	(20.03)	(24.65)	(24.64)	(23.75)	(26.84)
Property under Cloud (%)	41.5	44.0	43.7	49.5	51.4
	(11.03)	(13.29)	(13.06)	(12.55)	(13.24)
Farms (#)	7430	3754	30340	2680	143441

Notes: This table presents descriptive statistics for each treatment and control group at the farm level. For treatment groups, the averages and standard deviations (in parenthesis) are from 2000 until the treatment year, while for the control groups, they cover the whole sample period 2000-2019. The only exception is 'Property with Forest (%)' which is fixed in year 2000. The sample includes all farms in the Brazilian Amazon with any deforestation between 2000-2019, with more than 10% of primary forest coverage in 2000, with any deforestation before the first punishment, and with no punishment or with the first punishment after 2005 (following the description from Section 3.5). Direct & Adjacent: farms with first direct and adjacent neighbor punishment between 2005-2018. Direct: farms with first direct punishment between 2005-2018. Adjacent: farms with first adjacent neighbor punishment between 2005-2018. Late: farms with first direct or adjacent neighbor punishment between 2019-2021. Never: Farms with no direct or adjacent neighbor punishment between 2000-2021.
Data Sources: (Freitas et al., 2018; NOAA-CPC, 2020a,b; INPE, 2021; MapBiomas, 2021a,b,c; IBAMA, 2022)

Figure 1 presents the deforestation trajectories for each treatment cohort by type and the two control groups. We see a clear trend reversal across all cohorts with direct punishment, which is not present in the control groups. This reversal is our first descriptive evidence that sanctions may effectively reduce deforestation. Additionally, the data shows that IBAMA targets farms with higher levels of deforestation and increasing deforestation rates.[20]

Figure 1: Deforestation Evolution by Punishment Type and Cohort



Notes: The figure plots the deforestation trajectories for each treatment cohort g by type and the two control groups (late- and never-treated). It also highlights the punishment year of each treated cohort with dashed vertical lines. The deforestation area is normalized using the IHS transformation. Direct & Adjacent (treat.): farms with first direct and adjacent neighbor punishment between 2005-2018. Direct (treat.): farms with first direct punishment between 2005-2018. Adjacent (treat.): farms with first adjacent neighbor punishment between 2005-2018. Late (control): farms with first direct or adjacent neighbor punishment between 2019-2021. Never (control): Farms with no direct or adjacent neighbor punishment between 2000-2021.
Data Sources: (MapBiomas, 2021a; IBAMA, 2022).

[20]This targeting also occurs in late-treated farms, generating a divergence relative to the never-treated ones, mainly after 2016. So, we only use them as a control group in robustness exercises, restricting the sample until 2016 to avoid reverse causality bias.

4 Empirical Strategy

Our empirical strategy aims to identify the impacts of environmental sanctions on farmers' behavior related to deforestation and reforestation. The challenge is that law enforcement targets farms with high levels of deforestation. Hence, a simple comparison of post-punishment averages between punished and non-punished farmers can have the opposite sign of the causal effect because of selection bias.

To address these pre-existing differences in levels and control for common shocks, we use a staggered difference-in-differences (DD) framework that leverages the timing and location of the environmental sanctions between 2000-2021. Following the Callaway and Sant'Anna (2021) methodology,[21] we estimate average treatment effects on the treated ($ATT^{type}(g,t)$) for each cohort g, year t, and treatment $type$[22] by comparing the outcome evolution between punished and never-punished farmers, under the hypothesis that in the absence of treatment, the trends would be parallel.[23]

4.1 Estimation and Aggregation

Let $i \in \{1, 2, ..., N\}$ be farms, $t \in \{2000, 2001, ..., 2019\}$ years, $G_i^{type} = g \in \{2005, 2006, ..., 2018\}$ treatment cohorts of each $type \in$ {direct & adjacent, direct, adjacent}, $C_i = 1$ the control group of never-treated farms, and $\Delta Y_{ig-1,t} \equiv Y_{i,t} - Y_{i,g-1}$ the evolution of outcome $Y \in$ {IHS(deforestation area), IHS(reforestation area)} in a given year t relative to the year before treatment $g-1$.

For a given treatment $type$, Callaway and Sant'Anna (2021) propose an unconditional estimator for the average treatment effect of environmental sanctions for cohort g at year $t \geq g$ given by:

$$\widehat{ATT}^{type}(g,t) = \frac{\sum_i \Delta Y_{ig-1,t} 1\left\{G_i^{type} = g\right\}}{\sum_i 1\left\{G_i^{type} = g\right\}} - \frac{\sum_i \Delta Y_{ig-1,t} C_i}{\sum_i C_i} \tag{1}$$

[21]We do not rely on the usual two-way fixed effect regression because it can introduce bias in contexts with multiple periods, treatment timing variation, and dynamic heterogeneous effects (see Roth et al. (2022), De Chaisemartin and D'Haultfoeuille (2022), and Baker et al. (2022) for recent surveys of this literature).

[22]We focus on three treatment types: farms that are punished directly, farms that are not punished but witness the punishment of an adjacent neighbor, and farms that are punished and witness the punishment of an adjacent neighbor.

[23]In a robustness exercise (Section A.3.1) we relax this assumption by considering a linear extension of the observed trends difference across time as an alternative counterfactual for the post-punishment differences. Following Rambachan and Roth (2023)'s smoothness restriction assumption to conduct inference.

This estimator is equivalent to a two-period/two-group DD estimator that compares the average outcome evolution of the treated group in year t, post-treatment, relative to year $g-1$, pre-treatment, with the average outcome evolution of the control group across the same periods.

After estimating each $\widehat{ATT}^{type}(g,t)$, we have 780 parameters (20 years x 13 cohorts x 3 treatment types) to summarize, considering deforestation as the outcome. We present the main results in an event-study aggregation, which combines the estimates by relative time since the treatment year ($e = t - g \in \{-5 : 5\}$). We focus on cohorts treated between 2005 and 2014 to observe at least five years of exposure and avoid changes in sample composition across relative time. To evaluate magnitude, we combine the post-treatment estimates ($e = \in \{1 : 5\}$ into a single measure. Next, we aggregate by treatment cohort ($g \in \{2005 : 2018\}$), focusing on the impact one year after ($e = 1$) to evaluate heterogeneity over time. Finally, we aggregate by calendar year ($t \in \{2006 : 2019\}$) to include all effects and construct a counterfactual scenario without sanction effects. We weigh all aggregations by the share of treated farms.

4.2 Identification

The estimator in Equation 1 relies on three assumptions for identification: (1) absorbing treatment, meaning that each farm belongs to a unique treatment cohort and changes its status only once; (2) no anticipation, meaning that treatment effects are null before any punishment occurs; and (3) parallel trends, meaning that in the absence of punishment, the outcome evolution between $g-1$ and t for treatment cohort g would be the same as the evolution of the control group $C_i = 1$.

The key assumption of parallel trends establishes that the control group trends act as the counterfactual for the treatment group trends in the post-period. This assumption is reasonable when we have reforestation as the outcome because reforestation measurements were not available to influence the allocation of sanctions at that time. For the adjacent treatment, it is also reasonable because witnessing the punishment of a neighbor is arguably exogenous to the farmers' behavior. For cases with direct punishment, we should expect differential trends going in the opposite direction of reducing deforestation because IBAMA prioritizes punishing farmers with accelerating deforestation, such that assuming parallel trends can be conservative.

To account for differential trends and relax the parallel trends assumption, we conduct three robustness exercises. First, we use the linear extrapolation of the differential pre-trends as an alternative counterfactual for the post period, following (Rambachan and Roth, 2023) partial identification methods to conduct inference. Second, we use a doubly robust (DR) estimator conditioning on the pre-trends to avoid potential bias from regression to the mean effects among punished farms. Third, we use an outcome regression (OR) estimator conditioning on the municipality-by-property size group to avoid potential bias from municipality-specific policies and differences based on the property size. See appendix Section A.3 for more details on each exercise.

4.3 Inference

To conduct inference we rely on the multiplier bootstrap procedure suggested by Callaway and Sant'Anna (2021). In the event-study baseline aggregation, we compute simultaneous confidence intervals robust to multiple hypothesis testing. In all cases, we cluster the standard errors at the farm level to allow for heteroskedasticity and serial correlation within a farm.

5 Results

5.1 Sanction Effects on Deforestation and Reforestation

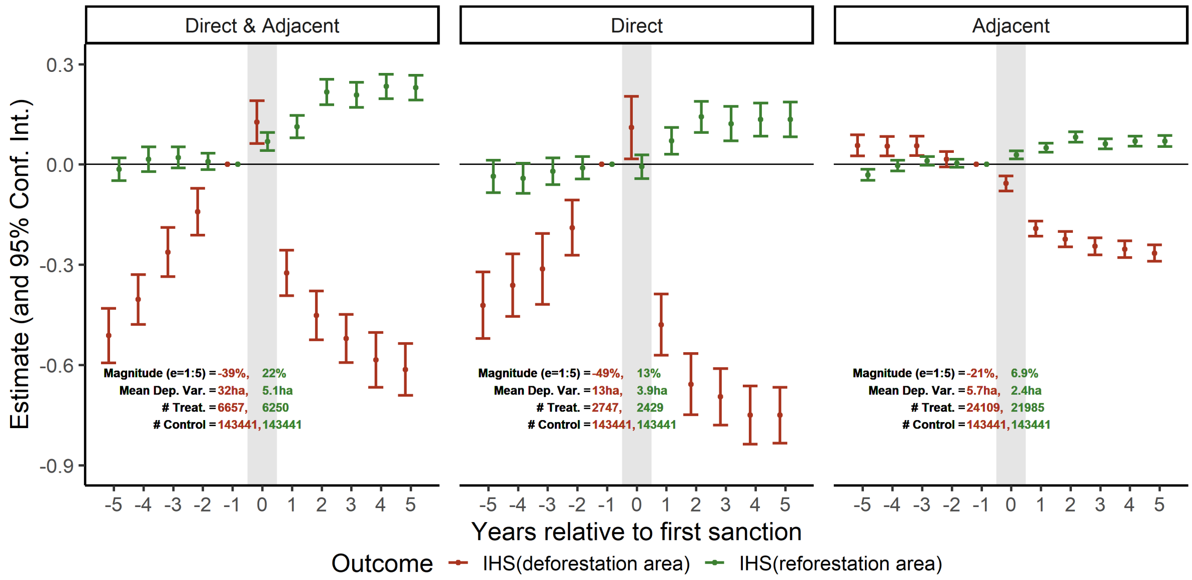
Figure 2 presents the balanced event-study aggregation of the environmental sanction effects on deforestation and reforestation for each type of treatment.[24] In Panels Direct & Adjacent and Direct, we see that punishment changes farmers' behavior, leading to a reversal in deforestation trends and a reduction of 39% and 49% respectively, on average, across one to five years of exposure. In Panel Adjacent, we see that farmers exposed to the punishment of an adjacent neighbor decrease deforestation by 21%, showing the relevance of spillover effects. The magnitude of the adjacent exposure is smaller than the other two treatment types, but the number of impacted farms is 2.5 times larger than both combined. These spillovers support general deterrence as the key mechanism behind behavioral changes. The idea is that when farmers suffer or witness a punishment, they update their beliefs about the risk of violating the forest laws and reduce their demand for deforestation because of the increase in the expected costs of engaging in illegal activity.

The sanctions also increase reforestation by 22%, 13%, and 6.9% across one to five years of exposure among farmers with direct and adjacent, direct, and adjacent treatments,

[24] In the appendix, we also present the full event-study with all relative years in Figure A.3).

respectively. These results are relevant because they corroborate Assunção et al. (2019) findings that command-and-control policies may impact social welfare more than previously thought. We complement them by directly measuring enforcement actions and observing responses at the decision-maker level. The impact on reforestation is also relevant as supporting evidence for the general deterrence mechanism. Using areas deforested without permission is also illegal, so an increase in the perceived risk of violating forest laws can also reduce the demand for illegally deforested lands, leading to the abandonment of these areas and allowing the forest to regrow naturally. In fact, an embargo punishment aims to prevent further damage by prohibiting any activity and increasing the potential for punishment in the specified area.

Figure 2: Sanction Effects on Deforestation and Reforestation



Notes: The figure plots the balanced event-study aggregation, which combines the estimates from Equation 1 by relative time since the treatment year ($e = t - g \in \{-5 : 5\}$), of the environmental sanction effects on deforestation and reforestation for each type of treatment. The effects are relative to the year before the first sanction. The dependent variables are normalized using the IHS transformation. The grey shaded area indicates the treatment year. Samples include cohorts treated between 2005 and 2014 to observe at least five years of exposure and avoid changes in sample composition across relative time. For reforestation, the last cohort is 2013 because the data ends in 2018 instead of 2019. Magnitude (e=1:5): is the average estimate from one to five years of exposure transformed to a percentage interpretation by $100 * (\exp(\text{average estimate}) - 1)$. Mean Dep. Var.: is the average dependent variable in the year before treatment for the treated group. Control group: farms with no direct or adjacent neighbor punishment between 2000-2021. Direct & Adjacent: farms exposed to direct and adjacent neighbor punishment. Direct: farms only exposed to direct punishment. Adjacent: farms only exposed to adjacent neighbor punishment. Bands are uniform 95% confidence intervals based on standard errors from a multiplier bootstrap procedure clustered by farm, as suggested by Callaway and Sant'Anna (2021).
Data Sources: (MapBiomas, 2021a; IBAMA, 2022).

The event-study estimates also present the pre-trends, which can act as an indirect test of the parallel trends assumption. For reforestation, the pre-trend differences are close to zero, which is not surprising because reforestation was essentially an invisible phenomenon at the time, so it could not influence law enforcement decisions. For the adjacent treatment, it is also small because witnessing the punishment of a neighbor is arguably exogenous to the farmers' behavior. For cases with direct punishment, there is a rising differential trend because IBAMA targets farms accelerating deforestation. As the pre-trends evolve almost linearly in the opposite direction of the post-treatment effects, assuming parallel trends might give us conservative magnitudes.[25]

5.2 Mechanism: General Deterrence or Incapacitation?

For the adjacent treatment, it is more apparent that general deterrence is a key mechanism because there is no direct channel of impact. However, for treatment effects on deforestation with direct punishment, an alternative mechanism that could also play a critical role is incapacitation. Here we provide additional evidence by exploring different punishments, varying the potential for incapacitation effects.

As explained in Section 2, there are four punishment combinations: *fines*, *fines + embargoes*, *fines + seizures*, and *fines + embargoes + seizures*. A standalone fine acts more as a communication that the State will open an administrative process against the offender, which usually takes a long time. Even when the fine is confirmed, it is not paid 90% of the time (Schmitt, 2015). Hence, fines by themselves have a low potential for incapacitation. Embargoes, in theory, prohibit any economic activity inside the specified area; in practice, they can lead to credit restriction because of Resolution 3,545 of the Central Bank, which added environmental requirements for lending rural credit. Therefore, embargoes have a higher potential for incapacitation through credit restrictions. Seizures or destruction of equipment presents the highest potential for incapacitation because they target deforestation-specific capital. In practice, there are many cases of seizure where the offender remains in possession of the equipment in the role of trustee, which cancels any incapacitation potential. We also consider fines not related to deforestation as a placebo treatment.

Table 2 shows that standalone fines significantly reduce deforestation. We argue that punishment with low incapacitating effects generating one of the largest magnitudes across all combinations supports general deterrence as the key mechanism. Moreover, punishments with embargoes produce the largest impacts on deforestation and reforestation, while seizures do not present an additional impact relative to standalone fines. Finally, fines non-related to deforestation do not produce significant effects, except for the adjacent treatment with deforestation as the outcome.

[25] To judge how these differences can affect the magnitudes, we compare them with additional results that allow for violations in the parallel trends assumption, using the linear continuation of the differential pre-trends as an alternative counterfactual, as described in Section A.3.1.

Table 2: Heterogeneity by Type of Sanction

	IHS(deforestation area)					IHS(reforestation area)				
	Fine	Fine + Embargo	Fine + Seizure	Fine + Embargo + Seizure	Fine Non-Deforest.	Fine	Fine + Embargo	Fine + Seizure	Fine + Embargo + Seizure	Fine Non-Deforest.
Treat: Direct & Adjacent										
Agg. Coef. (e=1:5)	-0.52**	-0.61***	-0.27*	-0.37***	-0.12	0.03	0.21***	0.03	0.2***	0.28
	(0.24)	(0.03)	(0.16)	(0.05)	(0.41)	(0.1)	(0.01)	(0.13)	(0.03)	(0.34)
Magnitude (e=1:5)	-40%	-46%	-24%	-31%	-12%	3.1%	23%	3.4%	22%	33%
Mean Dep. Var. (e=-1)	24ha	27ha	15ha	49ha	3.3ha	4.5ha	3.1ha	7.9ha	8.3ha	28ha
# Treated Farms	48	3220	89	1651	11	42	2948	81	1537	11
# Control Farms	142304	142304	142304	142304	142304	142304	142304	142304	142304	142304
Treat: Direct										
Agg. Coef. (e=1:5)	-0.67***	-0.8***	-0.23***	-0.66***	-0.09	0.08**	0.16***	0.09*	0.17**	0.07
	(0.09)	(0.04)	(0.08)	(0.11)	(0.13)	(0.04)	(0.02)	(0.05)	(0.07)	(0.09)
Magnitude (e=1:5)	-49%	-55%	-20%	-48%	-9%	8.7%	17%	9.4%	18%	7.6%
Mean Dep. Var. (e=-1)	14ha	19ha	6.9ha	17ha	3ha	3.9ha	3.6ha	8.4ha	4.6ha	6.8ha
# Treated Farms	363	2231	296	238	80	325	1994	274	221	74
# Control Farms	142304	142304	142304	142304	142304	142304	142304	142304	142304	142304
Treat: Adjacent										
Agg. Coef. (e=1:5)	-0.19***	-0.23***	-0.18***	-0.31***	-0.15***	0.02	0.07***	0.07***	0.1***	-0.01
	(0.02)	(0.01)	(0.02)	(0.02)	(0.04)	(0.02)	(0.01)	(0.02)	(0.01)	(0.04)
Magnitude (e=1:5)	-17%	-21%	-16%	-27%	-14%	2.4%	7.2%	7.7%	11%	-0.76%
Mean Dep. Var. (e=-1)	4.5ha	6.1ha	4.2ha	10ha	3ha	2.2ha	2.2ha	3.4ha	3.6ha	4.3ha
# Treated Farms	2241	16594	2022	4340	556	2041	15051	1887	4124	528
# Control Farms	142304	142304	142304	142304	142304	142304	142304	142304	142304	142304

Notes: The table presents the averages across one to five years of exposure from the balanced event-study aggregation of Equation 1 estimates for each treatment type and varying the type of sanction. The dependent variables are normalized using the IHS transformation. Samples include cohorts treated between 2005 and 2014 to observe at least five years of exposure and avoid changes in sample composition across relative time. For reforestation, the last cohort is 2013 because the data ends in 2018 instead of 2019. Control group: farms with no direct or adjacent neighbor receiving any sanction between 2000-2021. Magnitude (e=1:5): is the average estimate from one to five years of exposure transformed to a percentage interpretation by $100*(\exp(\text{average estimate})-1)$. Mean Dep. Var. (e=-1): is the average of the dependent variable in the year before treatment for the treated group. Direct & Adjacent: farms exposed to direct and adjacent neighbor punishment. Direct: farms only exposed to direct punishment. Adjacent: farms only exposed to adjacent neighbor punishment. Standard errors are from a multiplier bootstrap procedure clustered by farm, as suggested by Callaway and Sant'Anna (2021). Significance: *** p<0.01, ** p<0.05, * p<0.10.
Data Sources: (MapBiomas, 2021a; IBAMA, 2022).

5.3 Local Effects and Overall Commitment to Law Enforcement

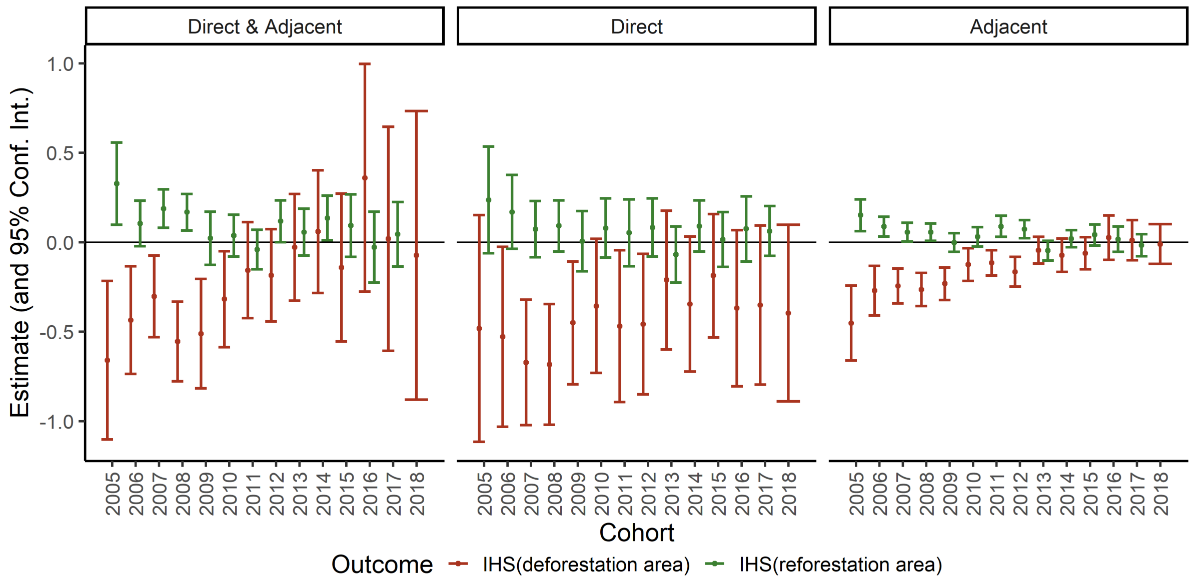
Next, we analyze how the effects of sanctions vary over time. Figure 3 show each cohort's effects with one year of exposure. We see a clear trend of decreasing magnitudes, especially for the adjacent treatment, going from -20.9% (2005-2012) to -2.6% (2013-2018) in the case of deforestation. As discussed in Section 2, starting in 2004, the Federal Government increased the efforts to curb deforestation in the Amazon through more robust law enforcement, but the momentum was relatively short-lived. After 2012, the commitment to forest law enforcement waned under political pressure, and there was a reversal in the overall deforestation trend (Burgess et al., 2019).[26] Therefore, the timing of the political reversal coincides with the changes in the local effects of sanctions.

This result complements the findings of Burgess et al. (2019). They document how changes in deforestation at the Brazilian international borders follow the degree of

[26] There was also a reversal in the number of sanctions per deforested area in 2009, as shown in Figure A.1.

commitment to environmental regulation by the Federal Government. They highlight, as examples of the commitment deterioration, the 2012 revision of the Forest Code that pardoned 90% of farmers for past deforestation and the reductions in the number of enforcement officers, IBAMA's budget, and operational expenditures in the Amazon. Here, we show that the overall commitment may have significant repercussions for the effectiveness of local law enforcement actions. Given the signals that illegal deforestation will be pardoned and that enforcement is losing momentum, farmers may stop perceiving current sanctions as a signal of increased risk to engage in future forest law violations, diminishing the general deterrence effects.

Figure 3: Sanctions Effects Over Time



Notes: The figure plots the treatment cohort effects, focusing on each type's impact one year after treatment ($e = 1$). The dependent variables are normalized using the IHS transformation. Samples include cohorts treated between 2005 and 2018 for deforestation and until 2017 for reforestation because the data ends in 2018 instead of 2019. Control group: farms with no direct or adjacent neighbor punishment between 2000-2021. Direct: farms only exposed to direct punishment. Direct & Adjacent: farms exposed to direct and adjacent neighbor punishment. Adjacent: farms only exposed to adjacent neighbor punishment. Bands are uniform 95% confidence intervals based on standard errors from a multiplier bootstrap procedure clustered by farm, as suggested by Callaway and Sant'Anna (2021).
Data Sources: (MapBiomas, 2021a; IBAMA, 2022).

5.4 Do Farmers React Strategically to Avoid Punishment?

The main goal of enforcing forest regulations is to improve conservation through changes in farmers' actions. However, farmers can react strategically to avoid punishment rather than changing their behavior as intended by the regulators. We explore two potential ways that farmers could use to circumvent law enforcement.

First, we examine whether farmers avoid the satellite monitoring system by changing their deforestation patterns to smaller polygons below the detection limit. Previous studies provide descriptive evidence of this trend after DETER's implementation (Assunção et al., 2017; Kalamandeen et al., 2018). Assunção et al. (2017) show that the rise in small-scale deforestation is present among all property sizes. They use property-level data from two Amazon States (Mato Grosso and Pará) and argue that this is suggestive - albeit not causal - evidence of strategic behavior to elude monitoring and does not reflect only a change in the type of deforesting agents. Another possible explanation for this trend is a reverse causality story. As the enforcement targets properties with large polygons, it will curb this type of deforestation such that even if farmers decide to keep their relative deforestation pattern fixed, the proportion of aggregate small-scale deforestation will increase.

We provide more robust evidence relative to the previous analyses by combining our causal identification framework at the property level with more detailed data on deforestation, including the period after an improvement in DETER's monitoring capacity. The data allow us to categorize deforestation into four types based on the degree of monitoring: large (monitored since 2004), medium (monitored since 2015), small (never monitored), and secondary vegetation (never monitored). Table 3 shows that all types of deforestation decrease after any punishment exposure. The largest magnitudes come from large and medium types, followed by small. Secondary vegetation presents negative coefficients in all cases but with the smallest magnitudes and non-significant for the adjacent treatment.

We interpret these results as evidence against the farmers' strategic response explanation because there is no increase in non-monitored deforestation. Hence, the contribution to the rise in the proportion of small-scale deforestation from monitoring and law enforcement comes more from the targeting criteria and heterogeneous effects. There are at least two possible explanations for this lack of strategic response. Farmers could take time to learn about the system's limitations as they change over time, despite being public information. Moreover, there can be gains of scale in the size of the deforestation polygon, making monitored and non-monitored polygons poor substitutes.

Table 3: Sanction Effects on Deforestation by Monitoring Degree

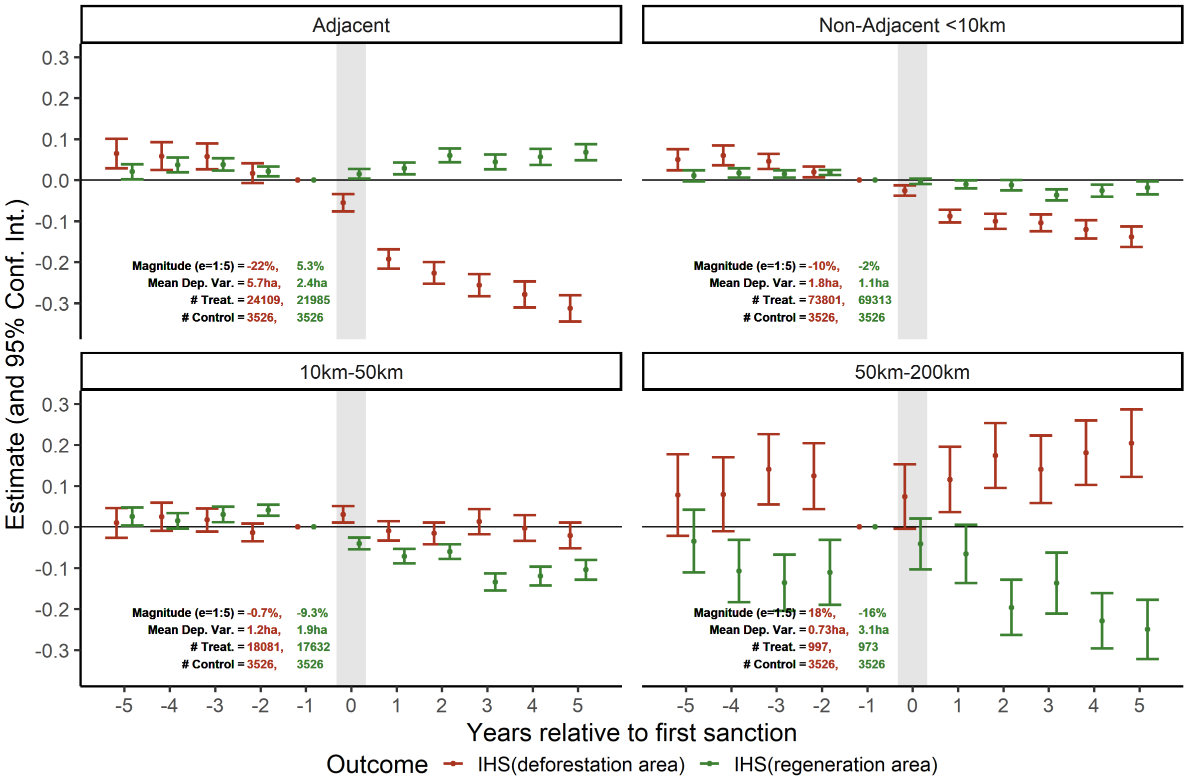
	IHS(deforestation type area)			
	Large	Medium	Small	Sec. Veg.
Treatment: Direct & Adjacent				
Agg. Coef. (e=1:5)	-0.346***	-0.294***	-0.143***	-0.016*
	(0.021)	(0.018)	(0.012)	(0.01)
Magnitude (e=1:5)	-29.3%	-25.5%	-13.3%	-1.61%
Mean Dep. Var. (e=-1)	21ha	7.6ha	3.4ha	2ha
# Treated Farms	6657	6657	6657	6657
# Control Farms	143441	143441	143441	143441
Treatment: Direct				
Agg. Coef. (e=1:5)	-0.321***	-0.383***	-0.171***	-0.092***
	(0.026)	(0.026)	(0.015)	(0.014)
Magnitude (e=1:5)	-27.5%	-31.8%	-15.7%	-8.74%
Mean Dep. Var. (e=-1)	8.2ha	3.2ha	1.3ha	1.4ha
# Treated Farms	2747	2747	2747	2747
# Control Farms	143441	143441	143441	143441
Treatment: Adjacent				
Agg. Coef. (e=1:5)	-0.086***	-0.131***	-0.097***	-0.004
	(0.005)	(0.006)	(0.004)	(0.004)
Magnitude (e=1:5)	-8.25%	-12.3%	-9.28%	-0.354%
Mean Dep. Var. (e=-1)	3.3ha	1.6ha	0.8ha	0.64ha
# Treated Farms	24109	24109	24109	24109
# Control Farms	143441	143441	143441	143441

Notes: The table presents the averages across one to five years of exposure from the balanced event-study aggregation of Equation 1 estimates for each treatment type and varying the type of deforestation as the dependent variable. The dependent variables are normalized using the IHS transformation. Large: polygons larger than 25 hectares, monitored since 2004. Medium: polygons between 3-25ha, monitored since 2015. Small: polygons smaller than 3ha, never monitored. Sec. Veg.: polygons of deforestation of secondary vegetation, never monitored by official systems. Samples include cohorts treated between 2005 and 2014 to observe at least five years of exposure and avoid changes in sample composition across relative time. For reforestation, the last cohort is 2013 because the data ends in 2018 instead of 2019. Control group: farms with no direct or adjacent neighbor punishment between 2000-2021. Direct & Adjacent: farms exposed to direct and adjacent neighbor punishment. Direct: farms only exposed to direct punishment. Adjacent: farms only exposed to adjacent neighbor punishment. Standard errors are from a multiplier bootstrap procedure clustered by farm, as suggested by Callaway and Sant'Anna (2021). Significance: *** p<0.01, ** p<0.05, * p<0.10.
Data Sources: (MapBiomas, 2021a; IBAMA, 2022).

Second, we investigate the possibility of spatial displacement. There is an ongoing debate in the crime literature about the direction of spillover effects from targeted law enforcement: evaluating if they generate broad general deterrence (Braga et al., 2019), or displace crime to non-targeted areas (Blattman et al., 2021). To evaluate the spillover effects beyond adjacent neighbors, we expand the range of potentially exposed farmers, including three other distance rings: non-adjacent farms less than 10 kilometers, between 10 and 50 kilometers, and between 50 and 200 kilometers. This exercise also accounts for possible violations of the "Stable Unit Treatment Value Assumption" (SUTVA), restricting the control group to 393 never-treated farms more than 200 kilometers away from any punished farm and 3,244 late-treated farms with a neighbor punished only after 2019.

Figure 4 shows significant reductions in deforestation among adjacent and non-adjacent farms until 10 kilometers (84% of farms), no effects between 10-50 kilometers (15% of farms) and a noisy increase, similar to the pre-punishment coefficients, between 50-200 kilometers (1% of farms). These results suggest a spatial diffusion of general deterrence prevailing over displacement effects for most farms in our context with decreasing intensity as the distance increase. For reforestation, the spatial diffusion is more limited, increasing only in adjacent farms and decreasing in all other groups. There are at least three possible explanations for the lack of a clear spatial displacement of deforestation. First, deforestation in the Amazon is usually motivated by future land use (e.g., agricultural production or illegal land grabbing), thus requiring spatial permanence to collect the benefits. Second, the whole biome is under surveillance with DETER, and the conservation requirements in non-private areas are even more strict. Third, moving to a new location might require rapid and extensive initial deforestation, which can draw much attention from IBAMA.

Figure 4: Sanction Effects by Neighbor Distance



Notes: The figure plots the balanced event-study aggregation, which combines the estimates from Equation 1 by relative time since the treatment year ($e = t - g \in \{-5 : 5\}$), of the environmental sanction effects on deforestation and reforestation for each type of treatment. The effects are relative to the year before the first sanction. The dependent variables are normalized using the IHS transformation. The grey shaded area indicates the treatment year. Samples include cohorts treated between 2005 and 2014 to observe at least five years of exposure and avoid changes in sample composition across relative time. For reforestation, the last cohort is 2013 because the data ends in 2018 instead of 2019. Magnitude (e=1:5): is the average estimate from one to five years of exposure transformed to a percentage interpretation by $100 * (\exp(\text{average estimate}) - 1)$. Mean Dep. Var.: is the average dependent variable in the year before treatment for the treated group. Control group: never-treated farms restricted to more than 200km away from any punished farm and late-treated farms with a neighbor punished only after 2019. Adjacent: farms only exposed to adjacent neighbor punishment. Non-Adjacent <10km: farms only exposed to non-adjacent and less than 10km away neighbor punishment. 10km-50km: farms only exposed to neighbor punishment between 10 and 50 kilometers. 50km-200km: farms only exposed to neighbor punishment between 50 and 200 kilometers. Bands are uniform 95% confidence intervals based on standard errors from a multiplier bootstrap procedure clustered by farm, as suggested by Callaway and Sant'Anna (2021).
Data Sources: (MapBiomas, 2021a; IBAMA, 2022).

Overall, we find evidence of broad general deterrence effects prevailing over potential strategic responses such as deforestation pattern change and spatial displacement. These findings help to explain sanctions' effectiveness in improving forest law enforcement at scale, even in a context with low punishment rates. This lack of strategic responses also contrasts with evidence of relevant changes in behavior to avoid targeted enforcement in other contexts (Banerjee et al., 2019; Blattman et al., 2021; Gonzalez-Lira and Mobarak, 2021), and highlights the importance of accounting for spillovers in policy evaluations in different contexts (Pfaff and Robalino, 2017).

5.5 Robustness

In this section, we conduct several robustness checks to increase confidence in our results. We relax the parallel trends assumption, use alternative estimators, split the sample based on punishment recurrence, forest cover, and property group, test alternative outcome transformations, and select a different control group. For the parallel trends, punishment recurrence and control group robustness, we reproduce the balanced event-study plot. For the alternative estimators, we present aggregated cohort-specific coefficients. For the forest cover and property group subsets and alternative outcome transformations, we present a single coefficient, aggregated from the balanced event-study, for each treatment, data subset, and outcome.

First, we relax the parallel trends assumption. Figure A.4 plots the baseline event-study estimates (solid lines), the baseline estimates net of the linear predicted trend (dashed lines), and the 95% confidence intervals (shaded areas) under the weaker assumption of linear violations of the parallel trends using the smoothness restriction from Rambachan and Roth (2023), detailed in Section A.3.1. For reforestation, the differential pre-trends are small and go in the same direction as the treatment effects, so the magnitudes become smaller but remain significant at the 5% level. For deforestation, the effects vary more in magnitude. Among farms with direct punishment (Panels Direct & Adjacent and Direct), the differential pre-trends are significant and go in the opposite direction of the post-treatment estimates, so the net effects become even larger in magnitude, going from 39% to 63% and 49% to 64%, respectively. For adjacent farms, the differential pre-trend goes in the same direction as the treatment effects but is small, so the linear extrapolation slightly reduces the magnitude from 21% to 17%.

Second, we consider two alternative estimators to account for the differential trends and potential confounding factors, as discussed in Section A.3. The first is the doubly-robust (DR) estimator, which controls for the pre-treatment trends to mitigate regression to the mean bias. The second is the outcome regression (OR) estimator, which controls for the municipality and property size group to ensure that any municipality-specific policies or differential treatment based on property size do not confound the results. Table A.2 presents the estimates aggregated by cohort from the baseline unconditional (UN) estimator and the alternative DR and OR estimators. The DR and OR estimates are similar to the UN, with slight variations in magnitude until 2011. However, from 2012 until 2018, the estimates are mixed in sign, reflecting the decreasing magnitude of the UN estimates over time.

Moreover, we relax the treatment definition based on the first year of exposure splitting the farmers with direct punishment by the number of years with punishment during the balanced event-study period. Figure A.5 shows similar patterns across farmers who are directly punished once, twice, or at least thrice. The main difference is the increase in the confidence interval as the treatment sample shrinks with a higher recurrence level.

Next, we investigate whether the availability of forests drives our results. To do this, we divide the farms into five bins based on the percentage of the property covered by forest in 2000. Table A.3 shows that the effects are even stronger for farms with more forest at the baseline. This heterogeneity suggests that the reductions in deforestation are not due to a lack of forests.

We also examine the effects across different property groups. We divide the properties by size (small, medium, and large), as a proxy for different types of farmers, by registration (registered, self-reported, terra-legal), as a proxy for tenure stability, and by intersection with public forests, as a proxy for illegal land grabbing. Table A.4 shows that the effects are similar across all property groups, indicating that no single group drives our results and minimizing concerns about tenure mismeasurement biasing the results.

Next, we test different outcome transformations, including the raw area measure and an alternative normalization by property area. We also distinguish between extensive and intensive margins, using a dummy for the occurrence of any deforestation in a given year and the log of non-zero outcomes, respectively. Table A.5 shows that the significance of our results is not affected by the normalization choice and that both the extensive and intensive margins are relevant. The baseline IHS estimates are even more conservative in terms of magnitude relative to the alternatives.

Finally, we modify the comparison group from never-treated to late-treated farms. Late-treated farms are more similar to early-treated farms than never-treated farms (as shown in Table 1), but since they are punished based on previous deforestation, using these years may introduce reverse causality bias. To minimize this issue, we restrict the sample to 2016 and use farms punished between 2019 and 2021 as the control group. We also adjust the balanced event-study, including farms punished between 2005 and 2011 in the treatment group. Figure A.6 shows almost identical results to the baseline estimates in Figure 2, despite all the changes in the sample for analysis.

6 Counterfactual Analysis: Shutting Down Sanctions

To assess the overall impact of environmental sanctions on deforestation, we construct a counterfactual scenario in which we shut down all sanctions issued between 2005 and 2018. To embrace heterogeneous effects, we use the disaggregated $ATT^{type}(g,t)$ cohort-year-treatment estimates from Equation 1. We transform the coefficients to percentages ($\exp{(estimate)}-1$), then to deforestation area, multiplying the percentages by the average deforestation in hectares one year before punishment and by the number of farms for each cohort and treatment type. Next, we aggregate to the annual level, summing all transformed estimates with at least one year of exposure, and calculate the counterfactual deforestation area by adding the annual increments of each treatment type to the observed deforestation within farms. We repeat this same exercise using deforestation CO_2 emissions and reforestation as outcomes.[27]

Table 4 shows that in the counterfactual scenario, deforestation would increase by 48% relative to the observed area between 2006-2019, indicating that the existence of sanctions prevented 2.266 million hectares of deforestation. In terms of CO_2 emissions, there would be an increase of 71%, indicating that sanctions avoided 1.604 billion tonnes of CO_2 between 2006-2019, equivalent to 31% of US emissions in 2021 (Friedlingstein et al., 2022). For reforestation, the counterfactual area would be 3.4% smaller than observed between 2006-2018, indicating that sanctions promoted 0.161 million hectares of reforestation.

The estimates may understate the total impact of sanctions as they do not include sanctions prior to 2005, and we do not observe all sanctions at the farm level between 2005 and 2010. In addition, our framework identifies only local causal effects. However, strengthening command and control may have an aggregate deterrence impact on all farms that is not causally identifiable in our empirical strategy.[28]

[27] Figure A.7 reproduces the balanced event-study estimates using the deforestation CO_2 emissions as the outcome, showing even large magnitudes as we incorporate spatial heterogeneity of carbon stock.

[28] For example, (Burgess et al., 2019) show evidence of large discontinuities in deforestation at the Brazilian international border disappearing after PPCDAm's introduction.

Table 4: Shutting Down Sanctions

Year	Deforestation Area (million hectares)		Deforestation Emissions (billion tonnes CO2)		Reforestation Area (million hectares)	
	Observed	Full Shutdown	Observed	Full Shutdown	Observed	Full Shutdown
2006	0.765	0.802	0.348	0.373	0.311	0.309
2007	0.550	0.621	0.254	0.300	0.340	0.336
2008	0.479	0.578	0.214	0.284	0.374	0.367
2009	0.240	0.402	0.114	0.224	0.331	0.320
2010	0.211	0.386	0.102	0.222	0.412	0.399
2011	0.268	0.429	0.121	0.234	0.403	0.392
2012	0.192	0.376	0.093	0.218	0.492	0.475
2013	0.238	0.427	0.113	0.247	0.449	0.428
2014	0.235	0.427	0.115	0.251	0.318	0.304
2015	0.283	0.471	0.139	0.275	0.328	0.312
2016	0.333	0.532	0.167	0.311	0.337	0.319
2017	0.284	0.487	0.141	0.288	0.333	0.318
2018	0.334	0.533	0.167	0.313	0.338	0.327
2019	0.316	0.524	0.160	0.311	NA	NA
2006-2019	4.728	6.994	2.248	3.851	4.767	4.605

Notes: This table presents the annual deforestation area, deforestation CO_2 emission, and reforestation area within farms from 2006 through 2019 and the sum across all years (Observed). It also presents the counterfactual annual values for each measure, considering a scenario with no effects from sanctions issued between 2005 and 2018 (Full Shutdown).
Data Sources: (MapBiomas, 2021a; GFW, 2022; IBAMA, 2022).

7 Conclusion

Our study provides new evidence on the mechanisms mediating the effects of conservation policies on farmers' forest change decisions. We find that general deterrence plays an essential role in changing farmers' behavior by altering their perceived risk of violating forest laws after being directly or indirectly exposed to environmental sanctions in the Brazilian Amazon.

These findings have important policy implications. First, sanctions generate persistent reductions in deforestation, avoiding the emission of 1.586 billion tonnes of CO_2, equivalent to 31% of US emissions in 2021 (Friedlingstein et al., 2022). Second, general deterrence helps to reconcile the apparent contradiction between the considerable deforestation reduction observed between 2004-2012 and the low punishment and fine collection rates by showing how one sanction can change the behavior of multiple potential offenders. Third, documenting spillover effects has implications regarding cost-effectiveness and optimal targeting of law enforcement, as previously demonstrated by Assunção et al. (2022b), at the municipality level. Fourth, some states in the Brazilian Amazon have recently begun using remote punishment systems, applying embargoes based on existing satellite images without field-based inspection (Azevedo et al., 2022a). This strategy provides an innovative way to increase punishment rates with faster responses at lower costs, which can boost general deterrence. However, a dramatic increase in punishment rates can also generate political backlash (Browne et al., 2023). Future research can utilize our empirical framework to test these hypotheses and evaluate the effectiveness of remote punishment.

Finally, it is important to notice that the environmental sanction effects do not occur in a vacuum. As described in Section 2, Brazil implemented a series of policies during the analysis period with the potential for interactions. The near-real-time satellite monitoring system allows for timely and targeted sanctions. The rural credit restriction in embargoed areas increases the cost of being punished even with no fine payment. The priority municipalities list concentrates efforts at high deforestation municipalities. The strict conservation requirements leave almost no room for legal deforestation. Future work could exploit existing geographic discontinuities and the timing of these policies to identify how sanction effects interact with each one of them to understand better how they influence general deterrence.

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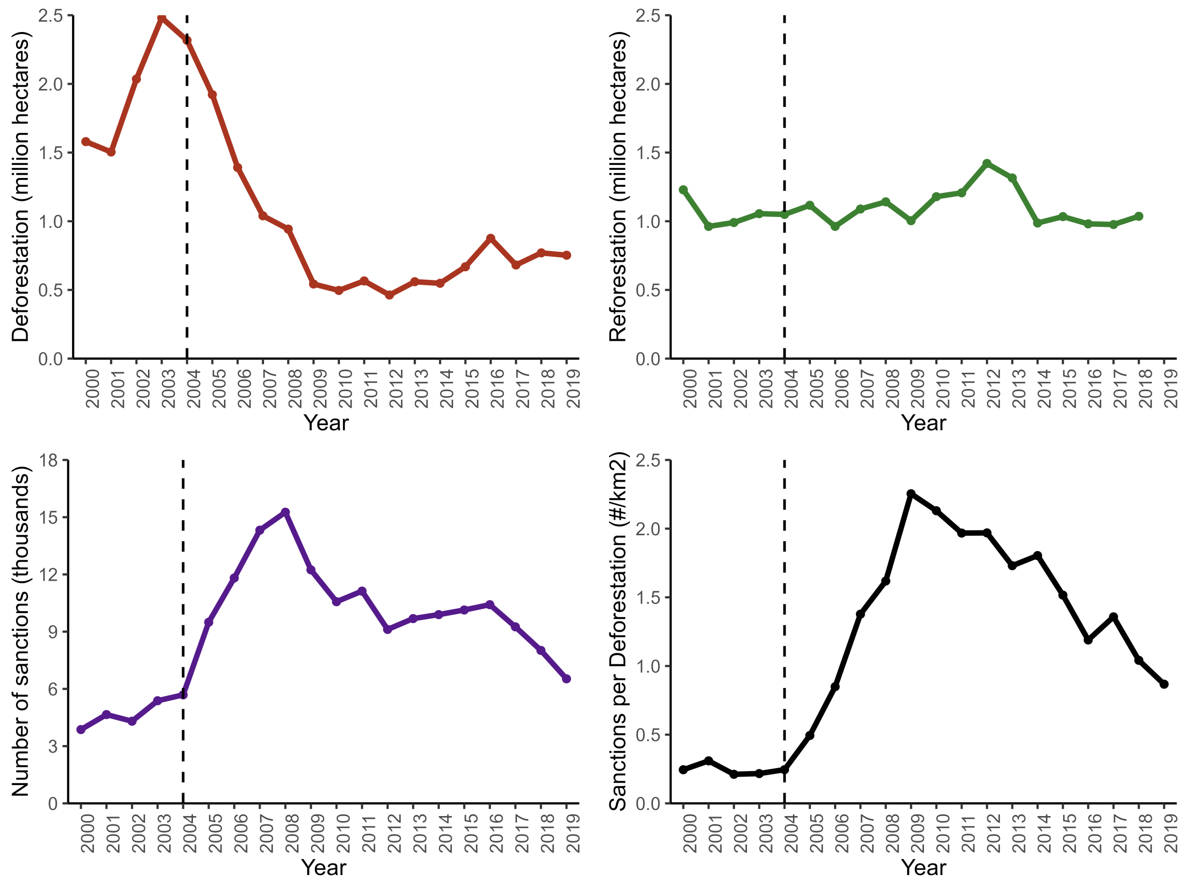
A Appendix

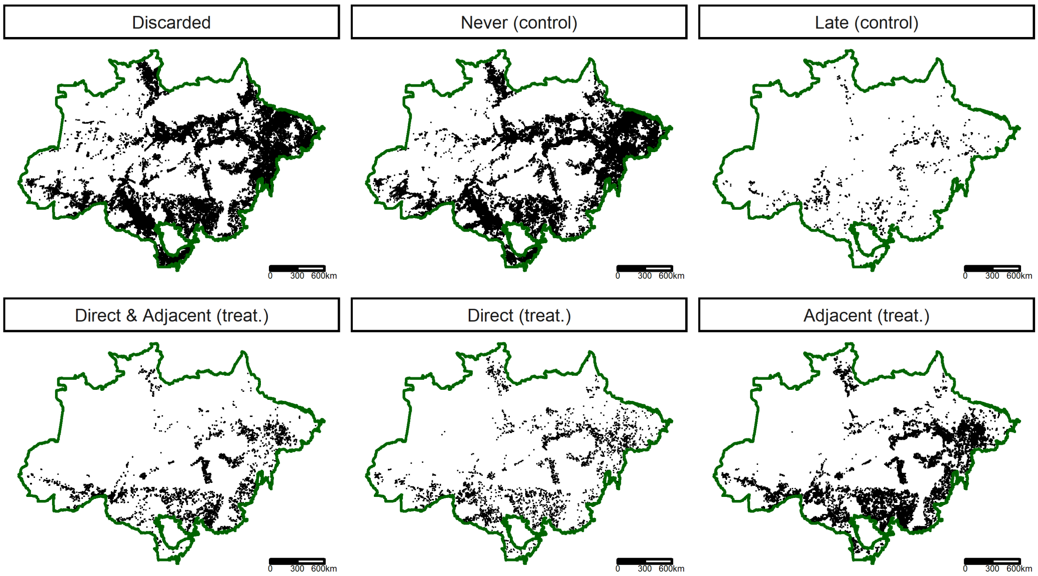
A.1 Additional Figures

Figure A.1: Deforestation, Reforestation, and Environmental Sanctions



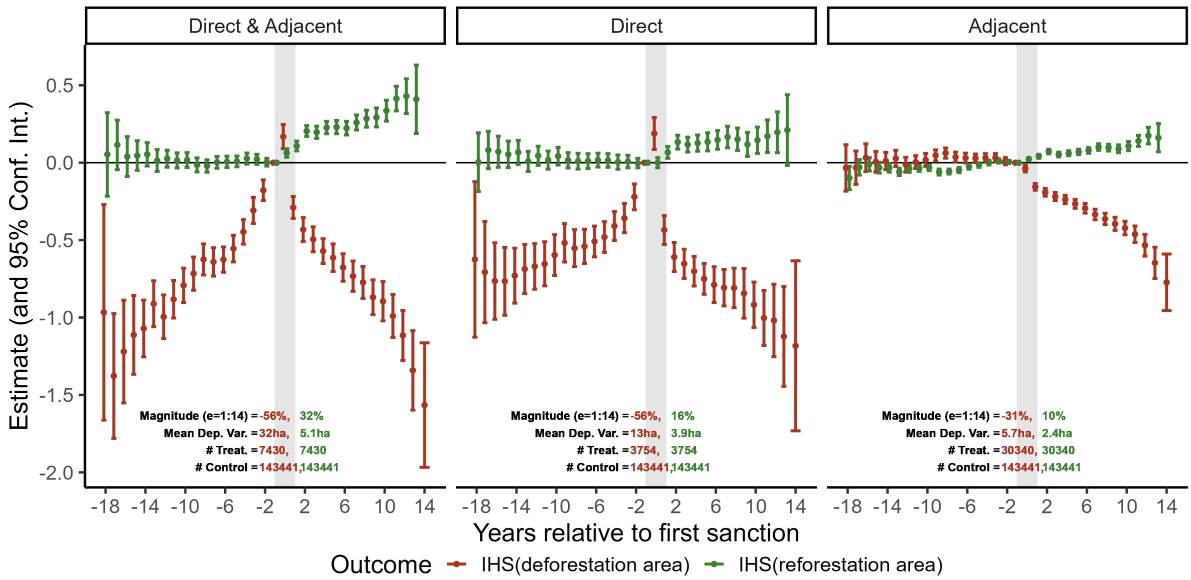
Notes: The figure plots the total area of deforestation (in 1,000,000 hectares), the total area of reforestation (in 1,000,000 hectares), the number of sanctions (in thousands) issued by IBAMA in the Brazilian Amazon biome, and the number of sanctions divided by the deforested area (in square kilometers). It also highlights 2004, the initial year of the Federal Government's plan to curb deforestation in the Amazon (PPCDAm), with a dashed vertical line. The number of sanctions includes flora-related fines, embargoes, and seizures.
Data Sources: (MapBiomas, 2021a; IBAMA, 2022).

Figure A.2: Farms Spatial Distribution



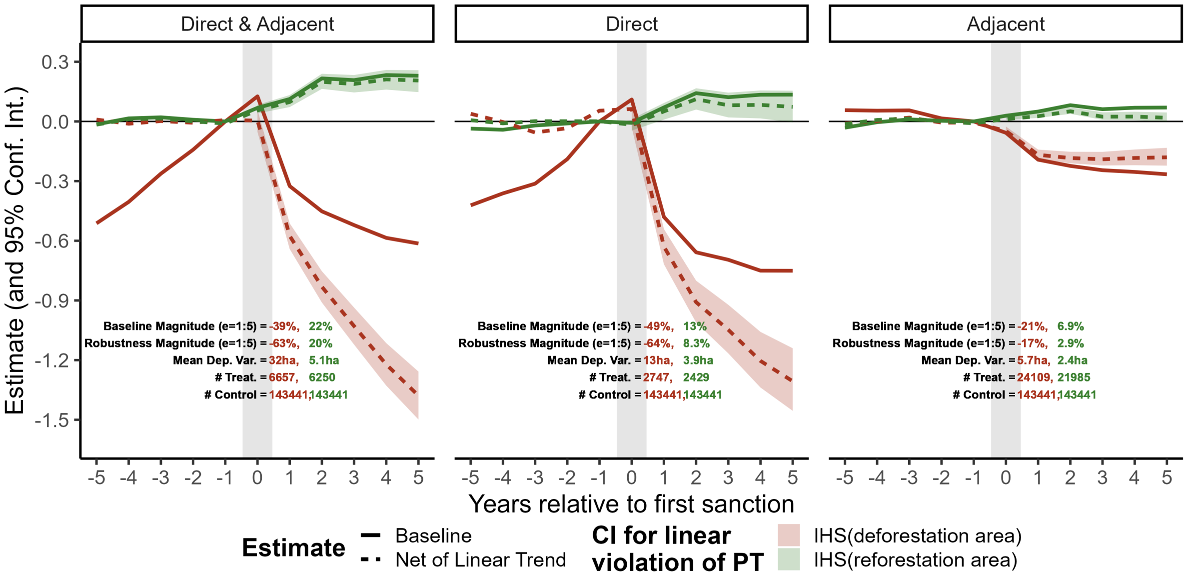
Notes: The figure plots a map of the Brazilian Amazon biome with the spatial distribution of farms faceted by group, following the description from Section 3.5. Discarded: farms with no deforestation between 2000-2019 or less than 10% of primary forest coverage in 2000 or with no deforestation before the first punishment or with the first punishment before 2005. Direct & Adjacent (treat.): farms with first direct and adjacent neighbor punishment between 2005-2018. Direct (treat.): farms with first direct punishment between 2005-2018. Adjacent (treat.): farms with first adjacent neighbor punishment between 2005-2018. Late (control): farms with first direct or adjacent neighbor punishment between 2019-2021. Never (control): Farms with no direct or adjacent neighbor punishment between 2000-2021.
Data Sources: (Freitas et al., 2018; MapBiomas, 2021a; IBAMA, 2022).

Figure A.3: Full Event-Study Estimates



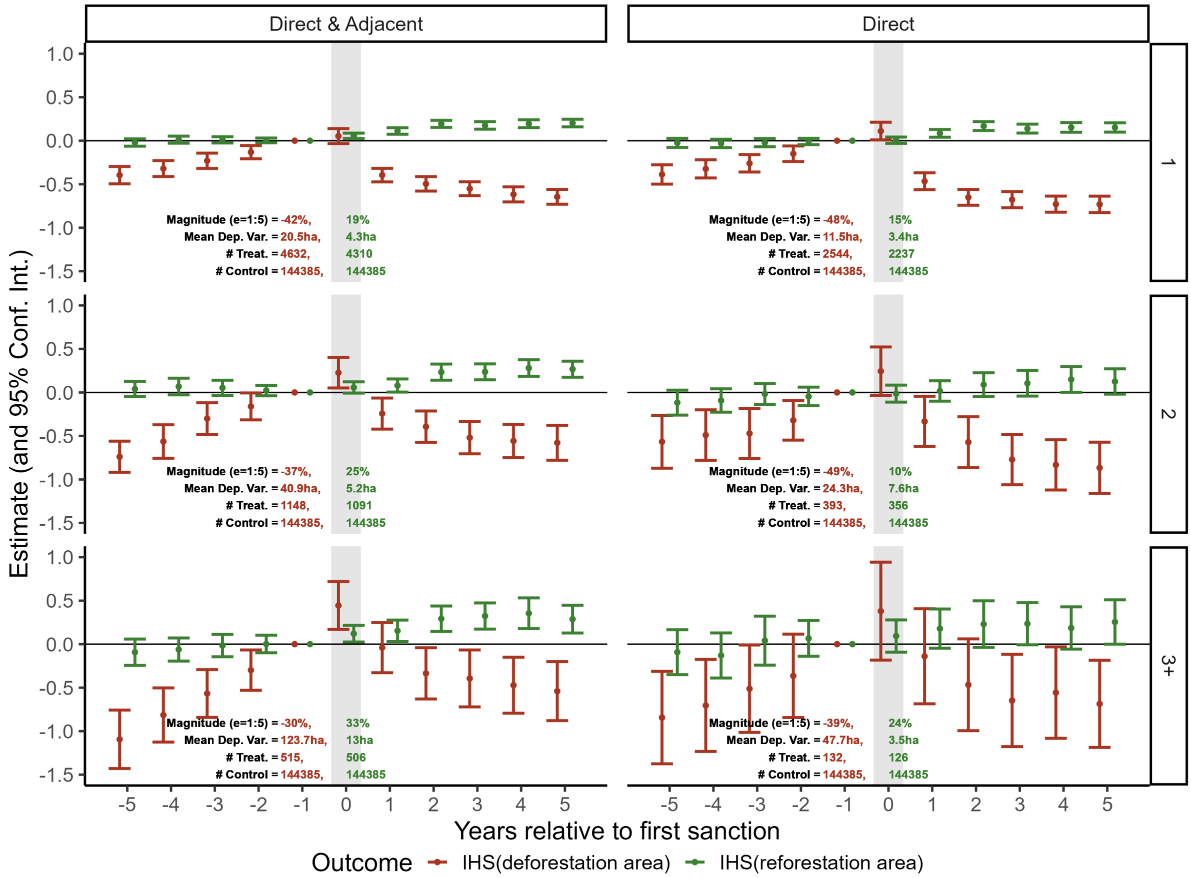
Notes: The figure plots the full event-study aggregation, which combines the estimates from Equation 1 by relative time since the treatment year ($e = t - g \in \{-18 : 14\}$), of the environmental sanction effects on deforestation and reforestation for each type of treatment. The dependent variables are normalized using the IHS transformation. The grey shaded area indicates the treatment year. Samples include all cohorts treated between 2005 and 2018. For reforestation, the last cohort is 2017 because the data ends in 2018 instead of 2019. Magnitude (e=1:14): is the average estimate from one to fourteen years of exposure transformed to a percentage interpretation by $100 * (\exp(\text{average estimate}) - 1)$. Mean Dep. Var.: is the average dependent variable in the year before treatment for the treated group. Control group: farms with no direct or adjacent neighbor punishment between 2000-2021. Direct & Adjacent: farms exposed to direct and adjacent neighbor punishment. Direct: farms only exposed to direct punishment. Adjacent: farms only exposed to adjacent neighbor punishment. Bands are uniform 95% confidence intervals based on standard errors from a multiplier bootstrap procedure clustered by farm, as suggested by Callaway and Sant'Anna (2021).
Data Sources: (MapBiomas, 2021a; IBAMA, 2022).

Figure A.4: Robustness to Parallel Trends Violation



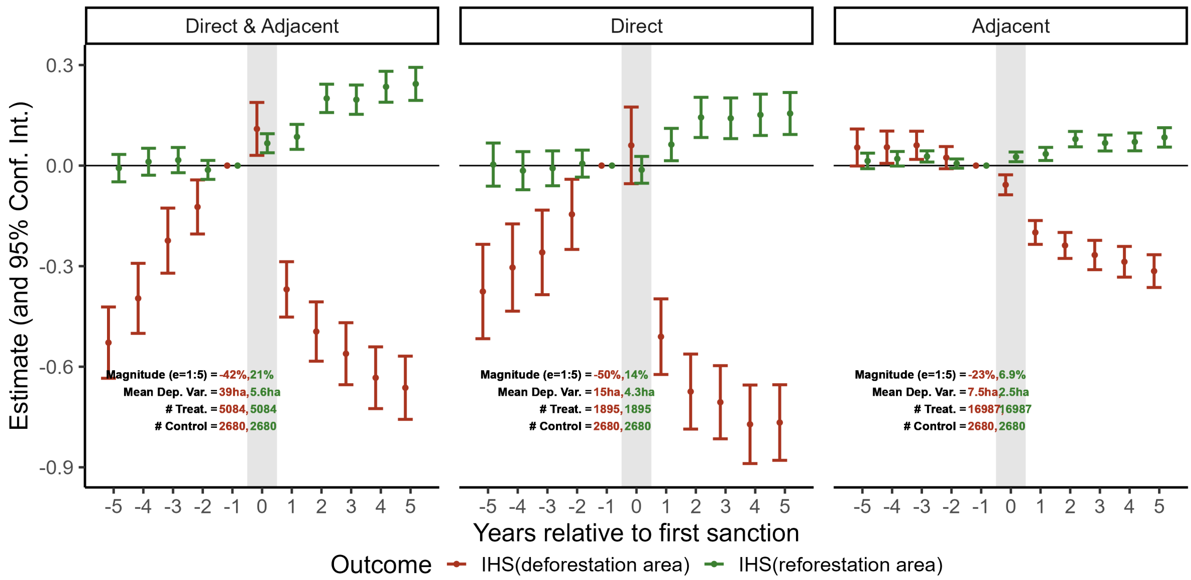
Notes: The figure plots the baseline event-study estimates from Figure 2 (solid lines), and the robustness estimates net of the linear predicted trend (dashed lines). The effects are relative to the year before the first sanction. Samples include cohorts treated between 2005 and 2014 to observe at least five years of exposure and avoid changes in sample composition across relative time. For reforestation, the last cohort is 2013 because the data ends in 2018 instead of 2019. Baseline Magnitude (e=1:5): is the average baseline estimate from one to five years of exposure transformed to a percentage interpretation by $100 * (\exp(\text{average estimate}) - 1)$. Robustness Magnitude (e=1:5): is the average baseline estimate net of the linear trend from one to five years of exposure transformed to a percentage interpretation. Control group: farms with no direct or adjacent neighbor punishment between 2000-2021. Direct & Adjacent: farms exposed to direct and adjacent neighbor punishment. Direct: farms only exposed to direct punishment. Adjacent: farms only exposed to adjacent neighbor punishment. The non-grey shaded areas are the 95% confidence interval constructed under the weaker assumption of linear violations of the parallel trends using the smoothness restriction from Rambachan and Roth (2023).
Data Sources: (MapBiomas, 2021a; IBAMA, 2022).

Figure A.5: Heterogeneity by Punishment Recurrence



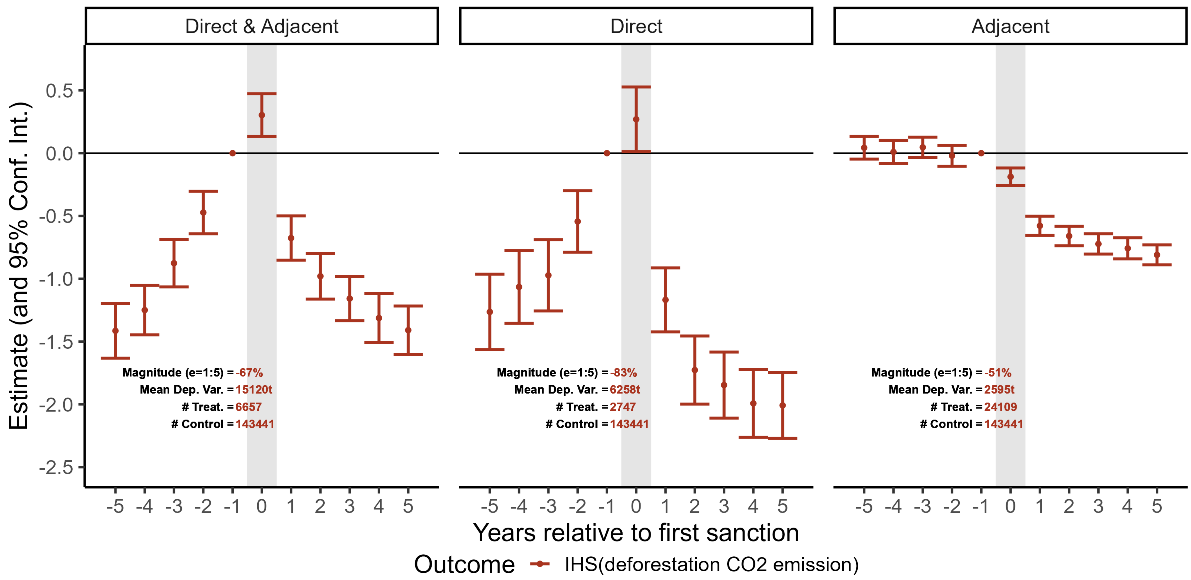
Notes: The figure plots the balanced event-study aggregation, which combines the estimates from Equation 1 by relative time since the treatment year ($e = t - g \in \{-5 : 5\}$), of the environmental sanction effects on deforestation and reforestation for each type of treatment and splitting the sample by punishment recurrence. The dependent variables are normalized using the IHS transformation. The grey shaded area indicates the treatment year. Samples include cohorts treated between 2005 and 2014 to observe at least five years of exposure and avoid changes in sample composition across relative time. Magnitude (e=1:5): is the average estimate from one to five years of exposure transformed to a percentage interpretation by $100 * (\exp(\text{average estimate}) - 1)$. Mean Dep. Var.: is the average dependent variable in the year before treatment for the treated group. Control group: farms with no direct or adjacent neighbor punishment between 2000-2021. Direct: farms only exposed to direct punishment. Direct & Adjacent: farms exposed to direct and adjacent neighbor punishment. 1: farms punished only once during the sample. 2: farms punished twice during the sample. 3+: farms punished three or more times during the sample. Bands are uniform 95% confidence intervals based on standard errors from a multiplier bootstrap procedure clustered by farm, as suggested by Callaway and Sant'Anna (2021).
Data Sources: (MapBiomas, 2021a; IBAMA, 2022).

Figure A.6: Late-treated as Control Group



Notes: The figure plots the balanced event-study aggregation, which combines the estimates from Equation 1 by relative time since the treatment year ($e = t - g \in \{-5 : 5\}$), of the environmental sanction effects on deforestation and reforestation for each type of treatment. The dependent variables are normalized using the IHS transformation. The grey shaded area indicates the treatment year. Samples include cohorts treated between 2005 and 2011 to observe at least five years of exposure and avoid changes in sample composition across relative time. Magnitude (e=1:5): is the average estimate from one to five years of exposure transformed to a percentage interpretation by $100 * (\exp(\text{average estimate}) - 1)$. Mean Dep. Var.: is the average dependent variable in the year before treatment for the treated group. Control group: farms with direct or adjacent neighbor punishment between 2019-2021. Direct: farms only exposed to direct punishment. Direct & Adjacent: farms exposed to direct and adjacent neighbor punishment. Adjacent: farms only exposed to adjacent neighbor punishment. Bands are uniform 95% confidence intervals based on standard errors from a multiplier bootstrap procedure clustered by farm, as suggested by Callaway and Sant'Anna (2021).
Data Sources: (MapBiomas, 2021a; IBAMA, 2022).

Figure A.7: Sanction Effects on Deforestation CO_2 Emissions



Notes: The figure plots the balanced event-study aggregation, which combines the estimates from Equation 1 by relative time since the treatment year ($e = t - g \in \{-5 : 5\}$), of the environmental sanction effects on deforestation CO2 emission for each type of treatment. The effects are relative to the year before the first sanction. The dependent variable is normalized using the IHS transformation. The grey shaded area indicates the treatment year. Samples include cohorts treated between 2005 and 2014 to observe at least five years of exposure and avoid changes in sample composition across relative time. For reforestation, the last cohort is 2013 because the data ends in 2018 instead of 2019. Magnitude (e=1:5): is the average estimate from one to five years of exposure transformed to a percentage interpretation by $100 * (\exp(\text{average estimate}) - 1)$. Mean Dep. Var.: is the average dependent variable in the year before treatment for the treated group. Control group: farms with no direct or adjacent neighbor punishment between 2000-2021. Direct & Adjacent: farms exposed to direct and adjacent neighbor punishment. Direct: farms only exposed to direct punishment. Adjacent: farms only exposed to adjacent neighbor punishment. Bands are uniform 95% confidence intervals based on standard errors from a multiplier bootstrap procedure clustered by farm, as suggested by Callaway and Sant'Anna (2021).
Data Sources: (MapBiomas, 2021a; GFW, 2022; IBAMA, 2022).

A.2 Additional Tables

Table A.1: Number of Farms by Punishment Year

Year	# Farms			% of total property area			% of annual deforestation			% of fines
	Direct & Adjacent	Direct	Adjacent	Direct & Adjacent	Direct	Adjacent	Direct & Adjacent	Direct	Adjacent	geo-referenced
2000	8	16	44	0.01	0.15	0.05	0.10	0.09	0.10	1.41
2001	15	39	134	0.03	0.17	0.10	0.15	0.52	0.33	1.91
2002	24	78	166	0.03	0.55	0.34	0.37	1.35	0.37	3.29
2003	50	185	405	0.09	1.19	0.73	0.52	2.95	1.25	5.81
2004	56	317	752	0.07	2.98	1.42	0.23	4.49	2.26	9.72
2005	136	469	1143	0.29	2.80	1.71	0.76	5.48	2.63	10.84
2006	233	769	2177	0.43	3.85	2.32	0.66	6.39	3.23	31.34
2007	375	1171	3333	0.38	3.29	3.35	0.77	6.57	3.80	54.84
2008	337	980	3184	0.41	2.92	3.30	1.23	4.60	2.54	54.39
2009	269	508	2455	0.31	1.29	2.04	0.70	4.31	1.89	52.07
2010	224	622	2144	0.27	1.12	1.89	1.11	4.62	2.31	71.19
2011	269	745	2423	0.40	0.75	1.63	1.26	3.91	2.78	100.00
2012	261	713	2678	0.18	0.83	1.54	1.10	5.65	2.70	100.00
2013	254	514	2278	0.17	0.50	0.94	1.82	3.42	1.93	100.00
2014	308	438	2103	0.30	0.43	1.13	1.59	2.45	1.13	100.00
2015	326	320	2104	0.26	0.28	1.22	2.21	2.06	1.67	100.00
2016	238	194	1232	0.55	0.16	0.84	1.26	2.43	1.35	100.00
2017	244	175	1501	0.21	0.20	0.89	1.35	2.56	0.97	100.00
2018	178	156	1343	0.20	0.46	1.07	1.16	2.15	1.12	100.00
2019	184	110	959	0.13	0.17	0.62	2.02	1.54	1.43	99.97
2020	104	42	599	0.08	0.05	0.37	NA	NA	NA	100.00
2021	111	28	543	0.11	0.02	0.36	NA	NA	NA	100.00

Notes: This table presents the number of farms, percentage of the property area, percentage of deforestation, and percentage of fines with geo-referenced information by punishment year for each treatment type. Direct & Adjacent: farms with direct and adjacent neighbor punishment. Direct: farms only with direct punishment. Adjacent: farms with adjacent neighbor punishment.
Data Sources: (Freitas et al., 2018; MapBiomas, 2021a; IBAMA, 2022).

Table A.2: Cohort-Specific Effects: Alternative Estimators

	IHS(deforestation area)									IHS(reforestation area)								
	Direct & Adjacent			Direct			Adjacent			Direct & Adjacent			Direct			Adjacent		
	UN	DR	OR	UN	DR	OR	UN	DR	OR	UN	DR	OR	UN	DR	OR	UN	DR	OR
Agg. Coef. (g=2005)	-1.38***	-0.72***	-0.8***	-1.02***	-0.68***	-0.73***	-0.71***	-0.54***	-0.37***	0.52***	0.46***	0.45***	0.33***	0.3***	0.27***	0.18***	0.17***	0.12***
	(0.11)	(0.09)	(0.11)	(0.17)	(0.12)	(0.16)	(0.06)	(0.04)	(0.06)	(0.06)	(0.05)	(0.05)	(0.06)	(0.06)	(0.06)	(0.02)	(0.02)	(0.02)
Agg. Coef. (g=2006)	-1.04***	-0.72***	-0.73***	-1.04***	-0.73***	-0.84***	-0.53***	-0.54***	-0.35***	0.39***	0.37***	0.3***	0.24***	0.22***	0.18***	0.21***	0.21***	0.13***
	(0.08)	(0.07)	(0.08)	(0.12)	(0.09)	(0.11)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.05)	(0.05)	(0.05)	(0.02)	(0.01)	(0.01)
Agg. Coef. (g=2007)	-0.73***	-0.59***	-0.52***	-0.97***	-0.77***	-0.87***	-0.41***	-0.44***	-0.3***	0.26***	0.25***	0.21***	0.12***	0.11***	0.1***	0.08***	0.08***	0.05***
	(0.06)	(0.05)	(0.06)	(0.08)	(0.06)	(0.08)	(0.03)	(0.02)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.04)	(0.03)	(0.01)	(0.01)	(0.01)
Agg. Coef. (g=2008)	-0.6***	-0.67***	-0.48***	-0.84***	-0.77***	-0.75***	-0.28***	-0.36***	-0.21***	0.23***	0.23***	0.18***	0.2***	0.2***	0.18***	0.07***	0.07***	0.04***
	(0.06)	(0.05)	(0.06)	(0.1)	(0.06)	(0.09)	(0.02)	(0.02)	(0.02)	(0.03)	(0.03)	(0.03)	(0.04)	(0.04)	(0.04)	(0.01)	(0.01)	(0.01)
Agg. Coef. (g=2009)	-0.5***	-0.36***	-0.38***	-0.5***	-0.52***	-0.45***	-0.24***	-0.28***	-0.21***	0.06	0.05	0.03	0.06	0.06	0.02	0.01	0.02	0
	(0.09)	(0.06)	(0.08)	(0.08)	(0.06)	(0.08)	(0.02)	(0.02)	(0.02)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.01)	(0.01)	(0.02)
Agg. Coef. (g=2010)	-0.52***	-0.33***	-0.51***	-0.54***	-0.47***	-0.56***	-0.19***	-0.22***	-0.19***	0.08**	0.06*	0.06**	0.16***	0.15***	0.16***	0.05***	0.05***	0.06***
	(0.08)	(0.06)	(0.07)	(0.09)	(0.07)	(0.09)	(0.02)	(0.02)	(0.02)	(0.03)	(0.03)	(0.03)	(0.05)	(0.05)	(0.05)	(0.01)	(0.01)	(0.01)
Agg. Coef. (g=2011)	-0.37***	-0.18***	-0.41***	-0.49***	-0.4***	-0.48***	-0.12***	-0.16***	-0.1***	0.08***	0.05*	0.03	0.07	0.06	0.02	0.05***	0.05***	0
	(0.06)	(0.04)	(0.06)	(0.1)	(0.06)	(0.09)	(0.02)	(0.02)	(0.02)	(0.03)	(0.03)	(0.03)	(0.04)	(0.05)	(0.04)	(0.01)	(0.01)	(0.01)
Agg. Coef. (g=2012)	-0.37***	-0.04	-0.32***	-0.71***	-0.26***	-0.66***	-0.15***	-0.12***	-0.11***	0.11***	0.05*	0.06**	0.13***	0.05	0.1**	0.03**	0.02*	-0.02*
	(0.07)	(0.06)	(0.07)	(0.09)	(0.07)	(0.09)	(0.02)	(0.01)	(0.02)	(0.03)	(0.03)	(0.03)	(0.04)	(0.04)	(0.04)	(0.01)	(0.01)	(0.01)
Agg. Coef. (g=2013)	-0.25***	-0.03	-0.28***	-0.52***	-0.21***	-0.54***	-0.05***	-0.06***	-0.07***	0.12***	0.09***	0.06*	0	-0.03	0.07*	-0.03*	-0.02*	0.01
	(0.08)	(0.05)	(0.08)	(0.09)	(0.06)	(0.08)	(0.02)	(0.01)	(0.02)	(0.04)	(0.04)	(0.03)	(0.04)	(0.04)	(0.04)	(0.01)	(0.01)	(0.01)
Agg. Coef. (g=2014)	-0.1	0.25***	-0.12	-0.4***	-0.05	-0.42***	-0.07***	-0.03**	-0.08***	0.14***	0.1***	0.13***	0.09***	0.06*	0.1***	0.02	0.01	0.01
	(0.09)	(0.07)	(0.09)	(0.08)	(0.06)	(0.09)	(0.02)	(0.02)	(0.02)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.01)	(0.01)	(0.01)
Agg. Coef. (g=2015)	-0.32***	0.14*	-0.33***	-0.25***	0.09	-0.25***	-0.08***	-0.05***	-0.06***	0.07	0.04	0.04	0.1***	0.08**	0.1***	0.02	0.02	0
	(0.11)	(0.08)	(0.11)	(0.09)	(0.06)	(0.09)	(0.02)	(0.02)	(0.02)	(0.05)	(0.05)	(0.04)	(0.04)	(0.04)	(0.04)	(0.01)	(0.01)	(0.01)
Agg. Coef. (g=2016)	0.11	0.76***	0.06	-0.45***	0.03	-0.48***	0.02	0.06**	-0.01	0	-0.05	0.03	0.09*	0.05	0.12**	0.01	0	0.02
	(0.16)	(0.13)	(0.17)	(0.11)	(0.09)	(0.11)	(0.03)	(0.02)	(0.03)	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)	(0.02)	(0.02)	(0.02)
Agg. Coef. (g=2017)	-0.12	0.57***	-0.14	-0.36***	0.01	-0.39***	0.01	0.03	-0.03	0.04	-0.09**	0.06	0.07*	0	0.08**	-0.02	-0.02	0.01
	(0.17)	(0.14)	(0.16)	(0.11)	(0.08)	(0.11)	(0.03)	(0.02)	(0.03)	(0.05)	(0.05)	(0.05)	(0.04)	(0.04)	(0.03)	(0.02)	(0.02)	(0.01)
Agg. Coef. (g=2018)	-0.09	0.33	-0.1	-0.28**	-0.07	-0.29**	-0.02	0.01	-0.02	NA	NA	NA	NA	NA	NA	NA	NA	NA
	(0.23)	(0.21)	(0.22)	(0.13)	(0.13)	(0.14)	(0.03)	(0.03)	(0.03)	NA	NA	NA	NA	NA	NA	NA	NA	NA
Magnitude (g=2005:2012)	-50%	-36%	-40%	-53%	-44%	-49%	-28%	-28%	-21%	24%	21%	18%	18%	15%	14%	8.7%	8.7%	5%
Magnitude (g=2013:2018)	-12%	40%	-14%	-31%	-3.3%	-33%	-3.1%	-0.7%	-4.4%	7.7%	1.9%	6.4%	7.5%	3.3%	10%	-0.0093%	-0.21%	1.1%
Mean Dep. Var. (e=-1)	33ha	16ha	33ha	15ha	8.7ha	15ha	5.7ha	1.9ha	5.7ha	5.2ha	1.2ha	5.2ha	4ha	1.2ha	4ha	2.3ha	1.5ha	2.3ha
# Treated Farms	7040	7040	7040	4193	4193	4193	29574	29574	29574	6907	6907	6907	3991	3991	3991	28252	28252	28252
# Control Farms	144385	144385	144385	144385	144385	144385	144385	144385	144385	144385	144385	144385	144385	144385	144385	144385	144385	144385

Notes: The table presents the averages across the punishment subsequent year ($e = 1$) until the last sample year ($e = 2019 - g$) for each cohort varying the outcome, treatment, and estimator. The dependent variables are normalized using the IHS transformation. For reforestation, the last sample year is 2018 instead of 2019 so the last cohort is 2017 instead of 2018. Magnitude (g=2005:2012): is the average estimate from cohort punished 2005 until 2012 transformed to a percentage interpretation by $100 * (\exp(\text{average estimate}) - 1)$. Magnitude (g=2013:2018): is the average estimate from cohort punished 2013 until 2018 transformed to a percentage interpretation. Mean Dep. Var. (e=-1): is the average dependent variable in the year before treatment for the treated group. Control group: farms with no direct or adjacent neighbor punishment between 2000-2021. UN: is the unconditional estimator from Equation 1. DR: is the doubly robust estimator conditional on the pre-trend from Equation 2. OR: is the outcome regression estimator within municipality and property size group from Equation 4. Direct & Adjacent: farms exposed to direct and adjacent neighbor punishment. Direct: farms only exposed to direct punishment. Adjacent: farms only exposed to adjacent neighbor punishment. Standard errors are from a multiplier bootstrap procedure clustered by farm, as suggested by Callaway and Sant'Anna (2021).

Data Sources: (MapBiomas, 2021a; IBAMA, 2022).

Table A.3: Heterogeneity by Forest Cover

	IHS(deforestation area)				
	[10%-30%]	(30%-50%]	(50%-70%]	(70%-90%]	(90%-100%]
Treatment: Direct & Adjacent					
Agg. Coef. (e=1:5)	-0.357***	-0.37***	-0.503***	-0.554***	-0.529***
	(0.084)	(0.079)	(0.062)	(0.05)	(0.032)
Magnitude (e=1:5)	-30%	-30.9%	-39.5%	-42.6%	-41.1%
Mean Dep. Var. (e=-1)	7.1ha	20ha	29ha	33ha	36ha
# Treated Farms	187	414	763	1603	3690
# Control Farms	143441	143441	143441	143441	143441
Treatment: Direct					
Agg. Coef. (e=1:5)	-0.235***	-0.355***	-0.6***	-0.874***	-0.805***
	(0.06)	(0.063)	(0.066)	(0.065)	(0.064)
Magnitude (e=1:5)	-21%	-29.9%	-45.1%	-58.3%	-55.3%
Mean Dep. Var. (e=-1)	3.2ha	6.3ha	8.7ha	19ha	15ha
# Treated Farms	255	380	503	746	863
# Control Farms	143441	143441	143441	143441	143441
Treatment: Adjacent					
Agg. Coef. (e=1:5)	-0.079***	-0.129***	-0.187***	-0.241***	-0.354***
	(0.016)	(0.017)	(0.016)	(0.016)	(0.015)
Magnitude (e=1:5)	-7.63%	-12.1%	-17.1%	-21.4%	-29.8%
Mean Dep. Var. (e=-1)	1ha	2.6ha	4.5ha	6.7ha	8.1ha
# Treated Farms	1960	3113	4443	6454	8139
# Control Farms	143441	143441	143441	143441	143441

Notes: The table presents the averages across one to five years of exposure from the balanced event-study aggregation of Equation 1 estimates for each treatment type and varying the bin of forest cover in 2000 ([10%-30%],...,(90%-100%]). The dependent variables are normalized using the IHS transformation. Samples include cohorts treated between 2005 and 2014 to observe at least five years of exposure and avoid changes in sample composition across relative time. Control group: farms with no direct or adjacent neighbor punishment between 2000-2021. Direct & Adjacent: farms exposed to direct and adjacent neighbor punishment. Direct: farms only exposed to direct punishment. Adjacent: farms only exposed to adjacent neighbor punishment. Standard errors are from a multiplier bootstrap procedure clustered by farm, as suggested by Callaway and Sant'Anna (2021).
Data Sources: (MapBiomas, 2021a; IBAMA, 2022).

Table A.4: Heterogeneity by Property Group

	IHS(deforestation area)							
	Type: Registered	Type: Self-reported	Type: Terra-Legal	Size: Small	Size: Medium	Size: Large	Public Forest: Inside	Public Forest: Outside
Treatment: Direct & Adjacent								
Agg. Coef. (e=1:5)	-0.52***	-0.49***	-0.41***	-0.39***	-0.42***	-0.5***	-0.43***	-0.56***
	(0.05)	(0.03)	(0.04)	(0.03)	(0.06)	(0.05)	(0.04)	(0.03)
Magnitude (e=1:5)	-41%	-39%	-34%	-32%	-34%	-39%	-35%	-43%
Mean Dep. Var. (e=-1)	75ha	19ha	9.4ha	7.8ha	21ha	90ha	16ha	42ha
# Treated Farms	1768	3078	1449	3326	1287	1682	2260	4035
# Control Farms	144385	144385	144385	144385	144385	144385	144385	144385
Treatment: Direct								
Agg. Coef. (e=1:5)	-0.67***	-0.61***	-0.66***	-0.59***	-0.62***	-0.66***	-0.7***	-0.63***
	(0.08)	(0.04)	(0.06)	(0.03)	(0.07)	(0.09)	(0.05)	(0.04)
Magnitude (e=1:5)	-49%	-46%	-48%	-44%	-46%	-48%	-50%	-47%
Mean Dep. Var. (e=-1)	30ha	12ha	8.1ha	6.7ha	14ha	45ha	12ha	16ha
# Treated Farms	626	1788	655	1945	589	535	908	2161
# Control Farms	144385	144385	144385	144385	144385	144385	144385	144385
Treatment: Adjacent								
Agg. Coef. (e=1:5)	-0.25***	-0.22***	-0.2***	-0.18***	-0.27***	-0.26***	-0.28***	-0.22***
	(0.02)	(0.01)	(0.01)	(0.01)	(0.02)	(0.03)	(0.02)	(0.01)
Magnitude (e=1:5)	-22%	-20%	-18%	-16%	-23%	-23%	-25%	-19%
Mean Dep. Var. (e=-1)	14ha	4ha	2.7ha	2.4ha	8.6ha	23ha	4.3ha	6.1ha
# Treated Farms	4439	13175	5888	17271	3646	2585	6241	17261
# Control Farms	144385	144385	144385	144385	144385	144385	144385	144385

Notes: The table presents the averages across one to five years of exposure from the balanced event-study aggregation of Equation 1 estimates for each treatment type and varying the property grou´p. The dependent variables are normalized using the IHS transformation. Type Registered: registered in the National Institute for Colonization and Agrarian Reform (INCRA). Type Self-declared: self-declared in the Environmental Rural Registry (CAR). Type Regularized: regularized in the Terra-Legal program. Size Small: farms with less than four fiscal modules (an official metric that vary by municipality). Size Medium: farms between 4-15 fiscal modules. Size Large: farms with more than 15 fiscal modules. Public Forest Inside: farms overlapping with undesignated public forests, potentially including cases of illegal land grabbing. Public Forest Outside: farms not overlapping with undesignated public forests. Samples include cohorts treated between 2005 and 2014 to observe at least five years of exposure and avoid changes in sample composition across relative time. For reforestation, the last cohort is 2013 because the data ends in 2018 instead of 2019. Control group: farms with no direct or adjacent neighbor punishment between 2000-2021. Direct: farms only exposed to direct punishment. Direct & Adjacent: farms exposed to direct and adjacent neighbor punishment. Adjacent: farms only exposed to adjacent neighbor punishment. Standard errors are from a multiplier bootstrap procedure clustered by farm, as suggested by Callaway and Sant'Anna (2021).
Data Sources: (Freitas et al., 2018; MapBiomas, 2021a; IBAMA, 2022).

Table A.5: Varying Outcome Transformation

	Deforestation					Reforestation				
	IHS	% Prop.	Area	Dummy	Log	IHS	% Prop.	Area	Dummy	Log
Treat: Direct & Adjacent										
Agg. Coef. (e=1:5)	-0.511***	-1.365***	-16.874***	-0.102***	-0.206***	0.193***	0.108***	1.935***	0.047***	0.223***
	(0.024)	(0.129)	(1.776)	(0.007)	(0.031)	(0.012)	(0.017)	(0.364)	(0.005)	(0.032)
Magnitude (e=1:5)	-40%	-74.5%	-100%	-9.74%	-18.6%	21.3%	11.4%	592%	4.76%	24.9%
Mean Dep. Var. (e=-1)	33ha	3.6%	33ha	0.54	33ha	5ha	0.29%	5ha	0.7	5ha
# Treated Farms	6295	6295	6295	6295	6295	5907	5907	5907	5907	5907
# Control Farms	144385	144385	144385	144385	144385	144385	144385	144385	144385	144385
Treat: Direct										
Agg. Coef. (e=1:5)	-0.647***	-2.253***	-9.707***	-0.165***	-0.486***	0.138***	0.13***	0.905*	0.031***	0.187***
	(0.031)	(0.199)	(1.002)	(0.009)	(0.046)	(0.015)	(0.019)	(0.524)	(0.007)	(0.037)
Magnitude (e=1:5)	-47.7%	-89.5%	-100%	-15.2%	-38.5%	14.8%	13.9%	147%	3.16%	20.6%
Mean Dep. Var. (e=-1)	15ha	3.6%	15ha	0.46	15ha	3.8ha	0.41%	3.8ha	0.79	3.8ha
# Treated Farms	3069	3069	3069	3069	3069	2719	2719	2719	2719	2719
# Control Farms	144385	144385	144385	144385	144385	144385	144385	144385	144385	144385
Treat: Adjacent										
Agg. Coef. (e=1:5)	-0.233***	-0.862***	-3.331***	-0.075***	-0.138***	0.063***	0.06***	0.378***	0.022***	0.11***
	(0.008)	(0.073)	(0.271)	(0.003)	(0.016)	(0.005)	(0.009)	(0.129)	(0.002)	(0.015)
Magnitude (e=1:5)	-20.8%	-57.8%	-96.4%	-7.25%	-12.9%	6.55%	6.18%	46%	2.26%	11.7%
Mean Dep. Var. (e=-1)	5.7ha	2.1%	5.7ha	0.3	5.7ha	2.3ha	0.48%	2.3ha	0.76	2.3ha
# Treated Farms	23502	23502	23502	23502	23502	21433	21433	21433	21433	21433
# Control Farms	144385	144385	144385	144385	144385	144385	144385	144385	144385	144385

Notes: The table presents the averages across one to five years of exposure from the balanced event-study aggregation of Equation 1 estimates for each treatment type and varying the dependent variable normalization. IHS: the baseline normalization using the IHS transformation. % Prop: percentage of the property area. Area: the raw area measure. Dummy: the extensive margin equals one if the area is larger than zero. Log: intensive margin, excludes observations with zero areas. Samples include cohorts treated between 2005 and 2014 to observe at least five years of exposure and avoid changes in sample composition across relative time. For reforestation, the last cohort is 2013 because the data ends in 2018 instead of 2019. Control group: farms with no direct or adjacent neighbor punishment between 2000-2021. Magnitude (e=1:5): is the average estimate from one to five years of exposure transformed to a percentage interpretation by $100 * (\exp(\text{average estimate}) - 1)$ if the outcome is IHS or Log, and by $100 * (\frac{\text{(average estimate)}}{\text{Mean Dep. Var.}}$ otherwise. Mean Dep. Var. (e=-1): is the average of the dependent variable in the year before treatment for the treated group. Direct: farms only exposed to direct punishment. Direct & Adjacent: farms exposed to direct and adjacent neighbor punishment. Adjacent: farms only exposed to adjacent neighbor punishment. Standard errors are from a multiplier bootstrap procedure clustered by farm, as suggested by Callaway and Sant'Anna (2021).
Data Sources: (MapBiomas, 2021a; IBAMA, 2022).

A.3 Relaxing the Parallel Trends Assumption

A.3.1 Allowing for Linear Violations of Parallel Trends

To deal with the differential trends, we complement our baseline event-study estimates with additional ones under a weaker assumption that allows for linear violations of the parallel trends. The idea is that, in the absence of treatment, we should not expect sudden changes in the observed pre-trends pattern relative to the counterfactual post-trends pattern. So we can use the linear extrapolation of the differential pre-trends as an alternative counterfactual for the post-period.

In practice, we fit a linear function of the event-study estimates pre-treatment on the relative time since the treatment year ($e \in \{-5 : -1\}$). Then, we calculate the predicted values for each event year e from the linear trend. Finally, we calculate the difference between the baseline estimates and the predicted values. This procedure exacerbates (reduces) the baseline effects when the differential pre-trends evolve in the opposite (same) direction of the post-treatment effects and generates similar results to the baseline when the pre-trends difference is close to zero.

To conduct inference, we apply the partial identification methods from Rambachan and Roth (2023) under the smoothness restriction assumption allowing linear violation (M=0).

A.3.2 Conditioning on Pre-Trends (Doubly Robust Estimator)

The doubly robust (DR) estimator conditions on the pre-trends and avoids potential bias from regression to the mean effects among punished farms,[1] because it compares farms with extreme outcome values in both groups.[2] Also, by conditioning on the trend, the residual decision to punish might be more influenced by idiosyncratic factors, such as clouds blocking the satellite monitoring visibility, rather than substantial differences that could correlate with the outcome trends.[3]

To condition on the pre-trends ($D^{type}_{g,i}$), we use the doubly robust estimator from Sant'Anna

[1]As IBAMA targets farms based on recent deforestation, an exceptional year with deforestation higher than average might trigger punishment among farmers more subject to mechanically reducing deforestation afterward, regressing to their mean.

[2]Daw and Hatfield (2018) show that when the treatment correlates with pre-trends, matching on the pre-trend might reduce bias although it does not fully correct the violation in their simulation.

[3]In our setup, when we condition on the deforestation trend, we also increase the probability of selecting a farm in the never punished group that was actually punished since we do not observe the universe of punishments before 2013. In this case, we would generate a bias toward a null result, so observing a significant effect improves, even more, our confidence in the results.

and Zhao (2020).[4] First, we estimate the logit propensity score for being in cohort g of treatment type $type$ as a function of $D_{g,i}^{type}$, denoted as $\hat{p}_{i,g}^{type}$. We also estimate the predicted outcome evolution among never-treated farms resulting from a regression of $Y_{i,t} - Y_{i,g-1}$ on $D_{g,i}^{type}$, denoted as $\Delta\hat{\mu}_{ig-1,t}^{type}$. The doubly robust estimator is then defined as:

$$\widehat{ATT}_{DR}^{type}(g,t) = \frac{\sum_i 1\left\{G_i^{type} = g\right\}\left(\Delta Y_{ig-1,t} - \Delta\hat{\mu}_{ig-1,t}^{type}\right)}{\sum_i 1\left\{G_i^{type} = g\right\}} - \frac{\sum_i \frac{\hat{p}_{i,g}^{type} C_i}{1-\hat{p}_{i,g}^{type}}\left(\Delta Y_{ig-1,t} - \Delta\hat{\mu}_{ig-1,t}^{type}\right)}{\sum_i \frac{\hat{p}_{i,g}^{type} C_i}{1-\hat{p}_{i,g}^{type}}} \tag{2}$$

This approach creates a comparison trend through the combination of observed outcome changes for farms with similar pre-trends (via propensity score reweighting) and predicted outcome changes for a given pre-trend (via outcome regression). The estimator is doubly robust because only one of the two methods needs to be correctly specified in order to recover the parameters of interest. Identification is similar to the estimator in Equation 1, just changing the unconditional parallel trends to a conditional version based on farms with the same average pre-treatment deforestation trend ($D_{g,i}^{type}$). To include a cohort-specific covariate, we did the estimations separately for each outcome-treatment-cohort group. To compare with the unconditional estimator we aggregate all estimates by cohort using the same multiplier bootstrap procedure for inference.

A.3.3 Within Municipality and Property Size (Outcome Regression Estimator)

The outcome regression (OR) estimator conditions on the municipality-property size group and avoids potential bias from municipality-specific policies, such as the priority municipalities list, and differences based on the property size, such as the rural credit restriction being less binding for small properties (Assunção et al., 2020). These differences could be confounders as they potentially correlate with sanction targeting and outcome trends. More details about the OR estimator are in the appendix (Section A.3.3).

We use the outcome regression approach from Sant'Anna and Zhao (2020) that has two steps. First, we estimate the change in outcomes among never-treated farms ($C_i = 1$) for each municipality-by-property size group ($X(i) = M(i) \times S(i)$):

[4] We calculate the pre-trends for each outcome-treatment-cohort group between five, four, three and two years before treatment relative to one year before, then take the average.

$$\Delta\hat{\mu}_{g-1,t}(x) = \frac{\sum_i(\Delta Y_{ig-1,t})1\left\{C_i = 1\right\}1\{X(i) = x\}}{1\left\{C_i = 1\right\}1\{X(i) = x\}} \tag{3}$$

Second, the conditional estimator for a given treatment $type$ is:

$$\widehat{ATT}^{type}_{OR}(g,t) = \frac{\sum_i\left((\Delta Y_{ig-1,t}) - \Delta\hat{\mu}_{g-1,t}(x)\right)1\left\{G_i^{type} = g\right\}}{\sum_i 1\left\{G_i^{type} = g\right\}} \tag{4}$$

Identification is similar to the estimator in Equation 1, just changing the unconditional parallel trends by a conditional version based on farms within the same municipality and property size group $X(i)$. To compare with the unconditional estimator we aggregate all estimates by cohort using the same multiplier bootstrap procedure for inference.