

From Deforestation to Reforestation: The Role of Sanctions and Spillovers in Changing Farmers' Behavior*

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Abstract

We study how environmental sanctions and spillovers improve forest conservation in the Brazilian Amazon. Using a difference-in-differences framework and novel farm-level data, we show that sanctions curbed deforestation and promoted reforestation among punished farmers and their neighbors. Heterogeneities reveal that even sanctions with limited incapacitation potential elicited relevant behavioral changes. In particular, farmers' responsiveness to sanctions coincided with the government's commitment to enforcement. We did not find substantial evidence of spatial displacement or monitoring evasion. Overall, sanctions prevented 1.6 billion tons of CO_2 emissions between 2006 and 2019, equivalent to 31% of US emissions in 2021.

Keywords: Deforestation, Reforestation, Spillovers, Law Enforcement

JEL Codes: K42, Q23, Q28, Q58

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1 Introduction

Deforestation generates a range of negative externalities. Globally, it significantly contributes to climate change, accounting for 20% of annual greenhouse gas emissions (Gullison et al., 2007). Regionally, it reduces rainfall and negatively impacts agricultural production (Lawrence and Vandecar, 2015; Araujo, 2022). Locally, it threatens biodiversity and the livelihoods 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 hinders 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 the direct impacts of conservation policies on deforestation, such as payments for ecosystem services (Jayachandran et al., 2017), conditional rural credit (Assunção et al., 2020), land titling programs (Probst et al., 2020), and command-and-control actions (Assunção et al., 2022a). Nevertheless, our understanding of potential spillover effects and underlying mechanisms driving behavioral changes in response to these policies remains scarce.

This paper aims to fill this gap by examining the role of sanctions and spillovers in influencing farmers' forest change decisions. Specifically, we investigate the effects of environmental sanctions on curbing deforestation and promoting reforestation among punished farms and their neighbors in the Brazilian Amazon. The hypothesis is that farmers exposed to punishment reduce the demand for deforestation and deforested lands due to incapacitation through losses of deforestation-specific capital and deterrence through updates in the expected cost of violating forest laws. To investigate these mechanisms, we analyze the spatial spillovers, which help isolate the informational channel characterizing deterrence. Additionally, we explore the heterogeneous effects of sanctions with varying incapacitation potential and examine how farmers' responsiveness to sanctions varies with changes in the government's commitment to enforcing forest conservation laws.

The Brazilian Amazon provides a unique setting for studying this topic for several

reasons. Farms account for 53% of the Amazon’s deforestation, providing a large-scale context to observe individual behavioral responses.¹ Punishment primarily targets recurrent offenders with high and increasing deforestation rates but still possessing substantial forested areas. Moreover, it is important to note that strict conservation requirements render nearly all deforestation in the region illegal.²

In the mid-2000s, the Federal Government of Brazil implemented a series of policies to combat the escalating deforestation rates.³ On one hand, there was a remarkable 80% decrease in deforestation between 2004 and 2012 (see Figure A.1), with causal evidence indicating that the implementation of the System for Real-Time Detection of Deforestation (DETER),⁴ along with intensified actions to curb illegal deforestation⁵ 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, 2023), and 10% of fines were paid (Schmitt, 2015). In light of these observations, we hypothesize that sanctions and spillovers on neighboring farms help explain this apparent contradiction by changing the behavior of multiple agents in deforestation hot spots.

To estimate 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 year to the average evolution across all never-punished farms. These comparisons are similar to applying a canonical two-period/two-group DD estimator separately for each treatment cohort and year (Callaway and Sant’Anna, 2021). We use georeferenced punishment information from Brazil’s primary envi-

¹The remaining 47% of deforestation occurs in indigenous lands, protected areas, rural settlements, *quilombos*, military areas, and undesignated public forests where multiple actors contribute to forest changes, making it challenging to isolate direct and spillover punishment effects.

²Forest laws requiring at least 80% of forest cover on private properties have been in place since 1996.

³These policies were implemented under Brazil’s Action Plan for the Prevention and Control of Deforestation in the Legal Amazon (PPCDAm), launched in 2004.

⁴A near-real-time satellite monitoring system from Brazil’s National Institute for Space Research (INPE).

⁵The number of sanctions per deforested area increased approximately nine-fold from 2004 to the peak in 2009, as shown in Figure A.1.

ronmental agency, the Brazilian Institute for the Environment and Renewable Natural Resources (IBAMA, 2022), high-resolution forest change outcomes from the MapBiomass Project (MapBiomass, 2021), high-resolution carbon stock data from Global Forest Watch (GFW, 2022), and farm boundaries from the Atlas of Brazilian Agriculture published by the Institute of Forestry and Agricultural Management and Certification (Freitas et al., 2018).

Our analysis shows that environmental sanctions are effective in curbing deforestation and promoting reforestation. Punished farmers decrease deforestation by 49%, while adjacent neighbors decrease it by 21%. Furthermore, farmers increase reforestation by 13% and 7%, respectively. These effects persist for at least five years after punishment. In terms of the underlying mechanisms driving these changes, we find suggestive evidence that deterrence is relevant. The spillover effects suggest that sanctions increase the perceived risk of violating forest laws among farmers who witness the punishment of adjacent neighbors. The heterogeneity by type of sanction shows that even standalone fines with the lowest potential for incapacitation cause large behavioral changes. Additionally, heterogeneity over time shows that farmers' responsiveness to environmental sanctions decreases as the government's overall commitment to forest law enforcement deteriorates, indicating that farmers may not update their perceived risk of punishment when they encounter mixed signals regarding law enforcement efforts.

Next, we investigate whether farmers react strategically to circumvent forest law enforcement. To this end, we explore two potential strategies: attempts to evade satellite monitoring by deforesting below detection limits and spatial displacement to avoid targeted areas. Our findings show that all deforestation types reduce in response to a sanction, regardless of the monitoring degree. Furthermore, we expand the possible range of spillover effects by including non-adjacent neighbors within three distance rings (<10km, 10km-50km, 50km-200km). We find significant reductions in deforestation among adjacent and non-adjacent farms up to 10 kilometers away (84% of neighbor farms), no effects between 10-50 kilometers (15% of neighbor farms), and

a noisy increase between 50-200 kilometers (1% of neighbor farms). These results indicate that changes in deforestation patterns and spatial displacement are not significant strategic response margins in this context, increasing the aggregate effectiveness of command-and-control policies.

We perform several robustness checks to validate our results. To account for punishment selection based on rising pre-trends and relax the parallel trends assumption, we use three complementary strategies. First, we control for the pre-treatment lagged outcome dynamics to reduce the risk of selection bias and regression to the mean effects (Acemoglu et al., 2019; Dube et al., 2023). Second, we use linear extrapolations of the pre-trends as an alternative counterfactual trajectory, following (Rambachan and Roth, 2023) partial identification methods to conduct inference. Third, we use a within municipality-by-property-size groups estimator to avoid potential confounding effects from municipality- and property size-specific policies. We also check if forest scarcity generates mechanical results; analyze heterogeneities across property types; test alternative outcome transformations; and change the control group from never-treated to late-treated farms.

To assess the overall impact of sanctions, we construct a counterfactual scenario where no sanctions were issued between 2005-2018. Our findings indicate that farmers' deforestation would have increased by 48% relative to what was observed between 2006-2019, suggesting that the existence of sanctions saved 2.268 million hectares of forest and avoided 1.599 billion tons of CO_2 emissions, equivalent to 31% of US emissions in 2021 (Friedlingstein et al., 2022). These results provide insights into how sanctions and spillovers can be a powerful tool for improving forest conservation on a large scale, changing farmers' behavior, and overcoming Amazon's low punishment and fine collection rates.

Related Literature

First, we contribute to the literature on law enforcement and spillovers. There is growing recognition in the environmental policy literature of the importance of accounting for spillovers in policy evaluations (Pfaff and Robalino, 2017). In the crime

literature, there is an ongoing debate about whether 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 effectiveness.

We add to this literature by analyzing spillovers from field-based environmental sanctions in a developing country, observing illegal deforestation events across the entire Brazilian Amazon biome under the same regulation and monitoring system. We provide evidence against strategic reactions to enforcement by showing no spatial displacement of deforestation to less targeted areas among 99% of the farms. We also find no evidence of substitution towards less monitored types of deforestation.

Second, we add to the literature on conservation policies and farmers' forest change decisions. While previous studies have examined the direct effects of various policies on these decisions, including payments for ecosystem services (Jayachandran et al., 2017), conditional rural credit (Assunção et al., 2020), land titling programs (Probst et al., 2020), and command-and-control actions (Assunção et al., 2022a), there is limited understanding of the potential spillover effects and the specific mechanisms through which these policies influence behavior. Previous studies on the Brazilian Amazon find that enforcement effectively curbs deforestation at the municipal level but do not delve into the underlying mechanisms of deterrence and incapacitation (Assunção et al., 2022b,a). Studies at the pixel level improve data granularity but cannot identify individual behavioral responses regarding deforestation (Börner et al., 2015; Burgess et al., 2019; Ferreira, 2023) or reforestation (Assunção et al., 2019).

We add to this literature by being the first study, to the best of our knowledge, to ex-

amine the relationship between environmental law enforcement and forest change decisions at the farm level. This allows us to track and analyze the behavioral responses of individual farmers, including the examination of spillover effects on neighboring farms. By doing so, we gain a deeper understanding of the mechanisms driving these decisions and shed light on how environmental sanctions have effectively reduced deforestation, despite the low rates of punishment and fine payments observed in the Amazon region. Furthermore, our study distinguishes itself by utilizing more detailed forest change data from MapBiomas (2021). This richer dataset enables us to estimate the impacts of environmental sanctions on both deforestation and reforestation, even among small-scale farms. Additionally, we are able to examine potential strategic reactions across different types of deforestation with varying levels of monitoring. We also incorporate high-resolution carbon stock data to translate impacts on deforestation area into CO_2 emissions.

Roadmap

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 DD empirical strategy. Section 5 presents the results and discusses 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 envi-

ronmental laws aimed at conserving the forest,⁶ deforestation in the region has been a major issue, driven primarily by agricultural activities and illegal land grabbing (Gandour, 2021).⁷ In most cases, deforestation is considered an environmental crime,⁸ 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.⁹ The plan included 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 an 80% reduction in deforestation rates in Brazil within a decade.¹⁰

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, 2023), 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).

⁶They prohibit deforestation inside protected areas (conservation units and indigenous lands) and require the conservation of at least 80% of private property's native vegetation.

⁷Around two-thirds of deforested areas are converted to pasture for cattle grazing (Gandour, 2021).

⁸Azevedo et al. (2022b) estimate that more than 99% of the deforestation area was illegal between 2019 and 2021.

⁹For a more detailed overview of the plan, refer to Gandour (2018).

¹⁰For a summary of studies that provide evidence supporting the causal link between environmental policies and the decline in deforestation, see Gandour (2018).

2.2 Environmental Law Enforcement

In Brazil, environmental law enforcement is a shared responsibility among municipal, state, and federal governments. However, 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 must know where the infractions happen. 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 primary tropical forests. Hence, it improved IBAMA's detection capacity and allowed faster and better-targeted responses (Assunção et al., 2022a).

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).¹¹

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 increase the punishment severity in cases of recidivism

¹¹See Schmitt (2015) for a more detailed description of the sanctioning administrative process and IBAMA's actions.

and lead to credit restrictions through Central Bank’s Resolution 3,545, which added environmental requirements for lending rural credit. The financial losses can be 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 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 information from 18 official sources based on a cross-section of 2018.¹² We focus on farms as the unit of analysis because they have a single individual responsible for the land. This allows us to track agents’ behavioral responses over time by looking at the changes occurring inside the property.¹³

We extract the farms’ boundary, area, size (small, medium, or large),¹⁴ 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-

¹²The compilation covers 82.6% of the country (Freitas et al., 2018) and uses a hierarchical approach to deal with spatial overlaps across sources.

¹³In public areas, multiple agents are responsible for a single land, and changes due to outsiders are more common than in private properties.

¹⁴Size 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.

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 main analysis. Moffette et al. (2023) provide evidence in favor of this assumption by showing that only 0.51% of properties between 2019-2020 were transacted. 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 (MapBiomias, 2021).¹⁵ 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 years.¹⁶ 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.

¹⁵MapBiomias 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>.

¹⁶For 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).

We extract the total deforestation area of primary forests¹⁷ 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. We also extract the primary forest area from 2000 to 2019.

To translate deforestation area to CO_2 emissions, 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* group, based on the first year a farm receives any sanction; the *Adjacent* group, based

¹⁷Primary forest is a forest with no previous deforestation, at least since 1987.

on the first year an adjacent neighbor receives any sanction; and the *Direct & Adjacent* group, based on the first year a farm and 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 77.5% of the farms are directly punished only once, while 16% are punished twice, 4% three times, and 2.5% at least four times.

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 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 26,671 farms with less than 10% of primary forest coverage in 2005 to avoid the mechanical effects of reducing deforestation due to a lack of forest. Third, we drop 3,752 farms with no deforestation before the first punishment to guarantee that we are capturing punishments motivated by deforestation. Fourth, we drop 2,047 farms exposed to punishment before 2005 to focus on sanction effects after the monitoring system implementation. Finally, we split the remaining 167,959 farms into three treatment and two control groups: 3,551 farms are in the *Direct* treatment group (first direct punishment between 2005-2018), 28,495 in the *Adjacent* treatment group (first adjacent neighbor punishment between 2005-2018), 7,297 in the *Direct & Adjacent* treatment group (direct and adjacent neighbor punishment between 2005-2018); 2,566 in the late-treated control group (first direct or adjacent neighbor punishment between 2019-2021); and 126,050 farms in the never-treated control group (no direct or adjacent

neighbor punishment). Figure A.2 shows the farms' spatial distribution by groups.

3.5 Descriptive Statistics

Table 1 presents the descriptive statistics for the pre-treatment period, providing an overview of the different groups. Treated farms exhibit, on average, higher levels of deforestation area, deforestation recurrence, reforestation area, CO_2 emissions, and property area compared to the control farms. It also shows that, for all groups, the majority of the property area was covered by forest in 2005. The recurrence of deforestation and the prevalence of forest cover among treated farms even in the year of punishment suggest that, in the absence of sanctions, many forests would be at risk of future deforestation. This indicates that we should not expect mechanical effects due to forest scarcity or single deforestation events driving the results.

Figure 1 presents the deforestation trajectories for each treatment cohort by type, along with the two control groups. We see a clear trend reversal across all cohorts with direct punishment, which is absent in the control groups. This reversal provides initial 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.¹⁸

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.

¹⁸Given the correlation between punishment and deforestation dynamics, it is important to consider potential reverse causality biases. The significant increase in deforestation observed after 2016 among the late-treated farms cannot be considered a credible counterfactual for the early-treated farms. To mitigate this issue, we select the never-treated farms as the preferred control group and only incorporate the late-treated farms in robustness exercises, restricting the sample until 2016 to avoid reverse causality bias.

To address these pre-existing differences in levels and control for common shocks, we use a staggered DD framework that leverages the timing and location of the environmental sanctions between 2000-2021. Following the Callaway and Sant’Anna (2021) methodology,¹⁹ we estimate average treatment effects on the treated ($ATT^{type}(g, t)$) for each cohort g , year t , and treatment $type$ ²⁰ 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. In robustness exercises (Section A.3), we relax this assumption in three ways: conditioning on lagged outcome dynamics to reduce the potential for selection bias and regression to the mean effects; considering a linear extension of the pre-trends as an alternative counterfactual; and using a within municipality-by-property-size groups estimator to further address potential confounding effects.

4.1 Estimation and Aggregation

Let $i \in \{1, 2, \dots, N\}$ be farms, $t \in \{2000, 2001, \dots, 2019\}$ years, $G_i^{type} = g \in \{2005, 2006, \dots, 2018\}$ treatment cohorts of each $type \in \{\text{Direct \& Adjacent}, \text{Direct}, \text{Adjacent}\}$, $C_i = 1$ 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 $Y \in \{\text{IHS(deforestation area)}, \text{IHS(reforestation area)}\}$ 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:

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

This estimator is equivalent to a two-period/two-group DD estimator that com-

¹⁹We 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).

²⁰We focus on three treatment types: farms that are punished directly (*Direct*), farms that are not punished but witness the punishment of an adjacent neighbor (*Adjacent*), and farms that are punished and witness the punishment of an adjacent neighbor (*Direct & Adjacent*).

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 \times 13 cohorts \times 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 magnitudes, 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 increasing differential trends because IBAMA priori-

tizes punishing farmers with accelerating deforestation, such that assuming parallel trends can be conservative relative to using the pre-trends linear extrapolation as the counterfactual or can exaggerate the punishment effects relative to comparing farms with similar deforestation trajectories pre-punishment due to mean reversion effects.

We conduct three robustness exercises to account for differential trends and relax the parallel trends assumption. First, we control for the pre-treatment lagged outcome dynamics to reduce the potential for selection bias and regression to the mean effects among punished farms (Acemoglu et al., 2019; Dube et al., 2023). Second, we use linear extrapolations of the pre-trends as an alternative counterfactual trajectory, following (Rambachan and Roth, 2023) partial identification methods to conduct inference. Third, we use an outcome regression (OR) estimator conditioning on the municipality-by-property-size groups 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

For the main results, we rely on the multiplier bootstrap procedure suggested by Callaway and Sant’Anna (2021) to conduct inference. We compute simultaneous confidence intervals robust to multiple hypothesis testing in the event study and cohort aggregations. 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.²¹ In Panels *Direct & Adjacent* and *Direct*, we see that punishment changes farmers’ behavior, leading to

²¹In the appendix, we also present the full event study with all relative years in Figure A.3.

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 22%, 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 suggest that when farmers 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 sanctions, which characterizes the deterrence mechanism.

The sanctions also increase reforestation by 22%, 13%, and 7.2% across one to five years of exposure among farmers with *Direct & Adjacent*, *Direct*, and *Adjacent* treatments, 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 through forest conservation. We complement them by directly measuring enforcement actions and observing responses at the decision-maker level. The impact on reforestation is also consistent with a 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 the case of an embargo punishment, there is an explicit goal of preventing further damage and allowing forest regrowth by prohibiting any activity in the specified area and increasing punishment severity in case of recidivism.

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 expected given that reforestation was 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.

However, for cases with direct punishment, there is a rising differential trend be-

cause IBAMA targets farms with accelerating deforestation. On the one hand, as the pre-trends evolve almost linearly in the opposite direction of the post-treatment effects, assuming parallel trends might give us conservative magnitudes relative to using the pre-trends linear extrapolation as the counterfactual. On the other hand, the difference in pre-trends could also exaggerate the punishment effects relative to comparing farms with similar trajectories pre-punishment due to mean reversion effects. In practice, we conduct robustness exercises for both scenarios to bound the direct effects, adjusting: for a linear extrapolation of the pre-trends and the pre-treatment lagged outcome dynamics. See Section A.3 for more details on each exercise.

Figure 3 plots the baseline event study and the two robustness estimates. For the *Direct & Adjacent* and *Direct* treatments with deforestation as the outcome, we see that adjusting for the linear trend makes the effects even larger in magnitude, increasing from 39% to 63% and 49% to 65%, respectively. While adjusting for the lagged outcome dynamics makes the effects smaller in magnitude, decreasing from 39% to 33% and 49% to 44%. For the other treatment-outcome combinations where the differential pre-trends are small, the adjustments have less impact on the effect sizes. Overall, these robustness exercises provide additional confidence in the baseline results by accounting for the pre-trends while maintaining similar post-treatment effects.

5.2 Mechanism: Deterrence x Incapacitation

The spillover effects suggest that deterrence is a relevant mechanism because there is no apparent direct impact channel. However, for direct treatment effects, 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. An embargo punishment aims to prevent further damage and allow forest regrowth by prohibiting any activity inside the embargoed area, increasing the punishment severity in cases of recidivism, and restricting credit availability. Therefore, embargoes have a higher potential for incapacitation through credit restrictions but also a higher potential for deterrence through the threat of recurrent and more severe sanctions. In theory, seizure and equipment destruction present the highest potential for incapacitation because they target deforestation-specific capital. In practice, there are many cases of seizure where the offender retains possession of the equipment in the role of trustee, drastically reducing the incapacitation potential.

Table 2 and Figure A.4 show that even standalone fines, with the lowest potential for incapacitation and pre-trends close to zero, produce one of the largest reductions in deforestation, suggesting that deterrence can also play a role among directly punished farmers. Moreover, punishments with embargoes are prevalent and produce one of the largest impacts on deforestation and reforestation, showing the relevance of a component that combines deterrence and incapacitation for forest conservation. Finally, seizures present noisier estimates for direct impact and significant conservation impacts for adjacent neighbors.²²

5.3 Local Effects and Overall Commitment to Law Enforcement

Next, we analyze how the effects of sanctions vary over time. Figure 4 show each cohort's effects with one year of exposure. We see a clear trend of decreasing magnitudes on deforestation, especially for the *Adjacent* treatment, going from -21.9% (2005-2012) to -2.56% (2013-2018).²³ As discussed in Section 2, starting in 2004, the Federal Govern-

²²It is important to note that comparisons across punishment types should be interpreted cautiously, as the selection of the specific punishment combination is endogenous. For example, there are large variations in terms of the average of the dependent variable in the year prior to punishment. However, the effects of adjacent punishments, where exposure to the punishment is more exogenous, demonstrate an intuitive pattern, with a greater number of punishments leading to larger effects. We also include a robustness estimator controlling for the pre-treatment lagged outcome dynamics that show very similar estimates in the event-study plot.

²³The large increase in georeferenced fines between 2005-2011 cannot explain this result. A lower share of georeferenced fines means that punished farms with no georeferenced fine will be misclassified as

ment 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).²⁴ 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 commitment to environmental regulation by the Federal Government. As examples of the commitment deterioration, they highlight the 2012 revision of the Forest Code that pardoned 90% of the farmers for past deforestation and the reductions in the number of enforcement officers, IBAMA's budget, and operational expenditures in the Amazon. Here, 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 enforcement is losing momentum, farmers may stop perceiving current sanctions as a signal of increased risk to engage in future forest law violations, diminishing deterrence effects.

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 limits. 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 never-treated, generating a bias towards a null result. Hence, as the share of georeferenced fines increases over time, we should expect this potential bias to reduce, which goes in the opposite direction of the observed effects. In practice, the bias should be small given that there are more than three times never-treated farms relative to all treated farms combined.

²⁴There was also a reversal in the number of sanctions per deforested area in 2009, as shown in Figure A.1.

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 and secondary vegetation.

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 the 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.

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 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 67 never-treated farms more than 200 kilometers away from any punished farm and 2,944 late-treated farms, with a neighbor punished only after 2019.²⁵

Figure 5 shows significant reductions in deforestation among adjacent and non-adjacent farms until 10 kilometers (84% of neighbor farms), no effects between 10-50 kilometers (15% of neighbor farms), and a noisy increase between 50-200 kilometers (1% of neighbor farms). These results suggest there was no spatial displacement of deforestation to less targeted areas among 99% of the farms in our context and that deterrence intensity decay over space. For reforestation, we see an increase among adjacent farms, null effects among non-adjacent farms until 10 kilometers, and reductions among farms between 10 and 200 kilometers, suggesting no spatial displacement among 84% of the farms in our context and a faster deterrence decay over space.

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 entire 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. In the case of reforestation, secondary forests were not monitored at the time, reducing the cost of

²⁵To increase power we follow Butts (2023), that adapts Gardner (2022) two-stage DD estimator. We remove farms with direct punishment from the sample and incorporate spillovers by restricting the observations of the first-stage ($Y_{it} = \mu_i + \lambda_t + u_{it}$) to units in the control group with no spillover exposure and by including a set of distance ring indicators in the second-stage (regressing the residuals of the first stage, including all observations with no direct punishment, on indicators by relative treatment time and distance ring). Identification becomes stronger in this case because farms of each distance ring must have the same parallel trends.

displacement.

Overall, we find no evidence of relevant 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 (Pfaff and Robalino, 2017).

5.5 Robustness

In this section, we conduct several robustness checks to increase confidence in our results. We use an alternative estimator, split the sample based on forest cover and property group, test alternative outcome transformations, and select a different control group. We reproduce the balanced event study plot for the alternative estimator, forest cover, and control group robustness. For the property group and alternative outcomes, we present a single coefficient, aggregated from the balanced event study, for each treatment, data subset, and outcome.

First, we consider an alternative estimator for dealing with potential confounding factors, as detailed in Section A.3. The outcome regression (OR) estimator 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. Figure A.5 shows post-treatment effects smaller but close to the baseline event-study (Figure 2).²⁶

Next, we investigate whether the availability of forests drives our results. To do this, we divide the farms into six bins based on the percentage of the property covered by forest in 2005. Table A.2 shows significant effects relevant in magnitude across all bins, reinforcing that punishment effects reducing deforestation are not driven by

²⁶The number of treated and control farms in the OR regression is smaller than in the baseline event-study because we remove farms in groups without at least one treated and one control farm.

forest scarcity. Figure A.6 shows that for all bins below 70% the pre-trends are close to null, while the post-treatment effects are still significant and large. Hence, for subgroups with better evidence supporting the parallel trends assumption, we have effects in the same direction and similar in size.

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.3 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.4 shows that the normalization choice does not affect our results' significance and that both the extensive and intensive margins are relevant. The baseline IHS estimates are even more conservative regarding magnitude than 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). However, 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.7 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 ($\exp(\text{estimate}) - 1$), 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 2.267 million hectares of deforestation. In terms of CO_2 emissions, there would be an increase of 71%, indicating that sanctions avoided 1.599 billion tons 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 3.3% smaller than observed between 2006-2018, indicating that sanctions promoted 0.158 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 due to the lack of georeferencing. 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

²⁷Figure A.8 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.

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

7 Conclusion

Our study provides new evidence on how sanctions and spillovers can be a powerful tool for improving forest conservation at scale. It also suggests that deterrence plays a relevant role in shaping farmers' behavior by altering their perceived risk of violating forest laws after exposure to environmental sanctions.

These findings have important policy implications. First, sanctions generate persistent reductions in deforestation, avoiding the emission of 1.599 billion tons of CO_2 , equivalent to 31% of US emissions in 2021 (Friedlingstein et al., 2022). Second, targeted sanctions and spillovers help 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 law enforcement targeting, 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, potentially boosting deterrence. However, dramatically increasing 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 note that 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 on 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 the sanction effects interact with each feature to improve the understanding of the external validity.

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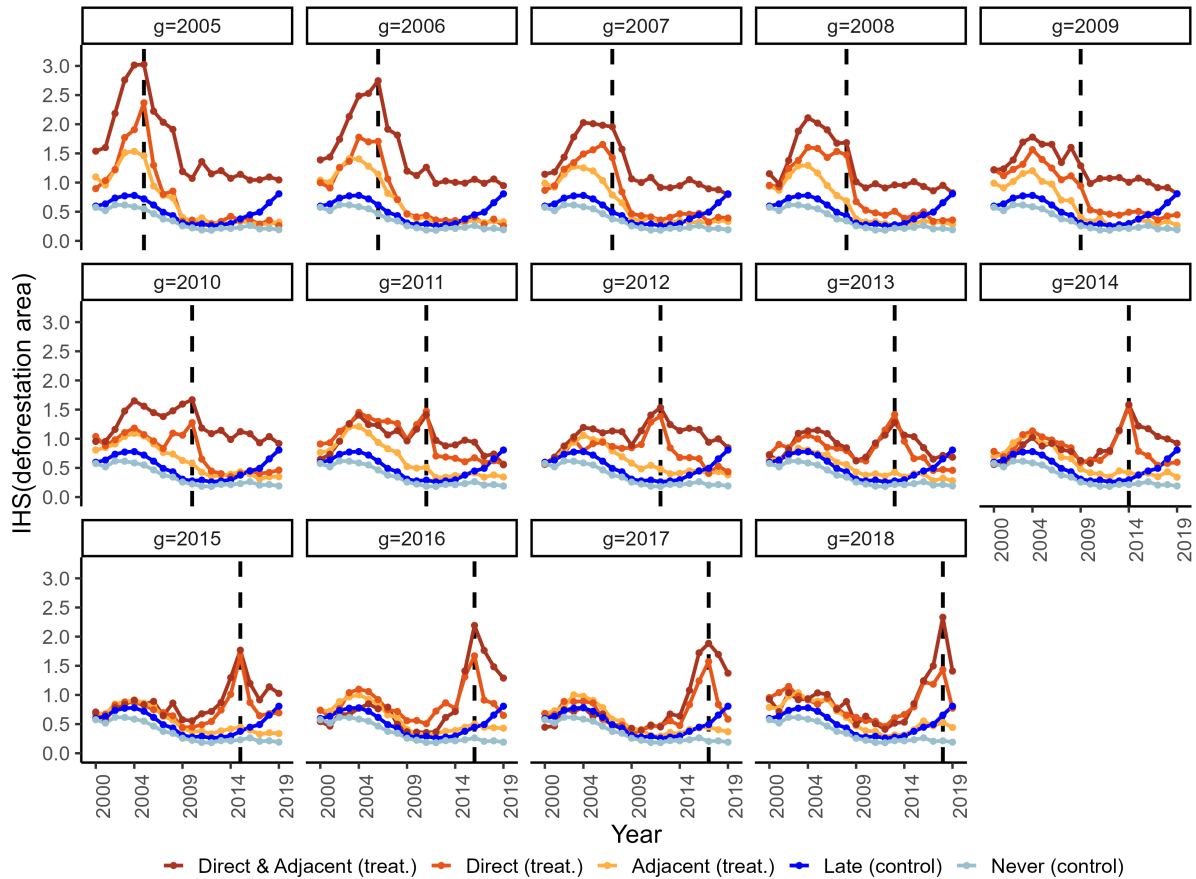
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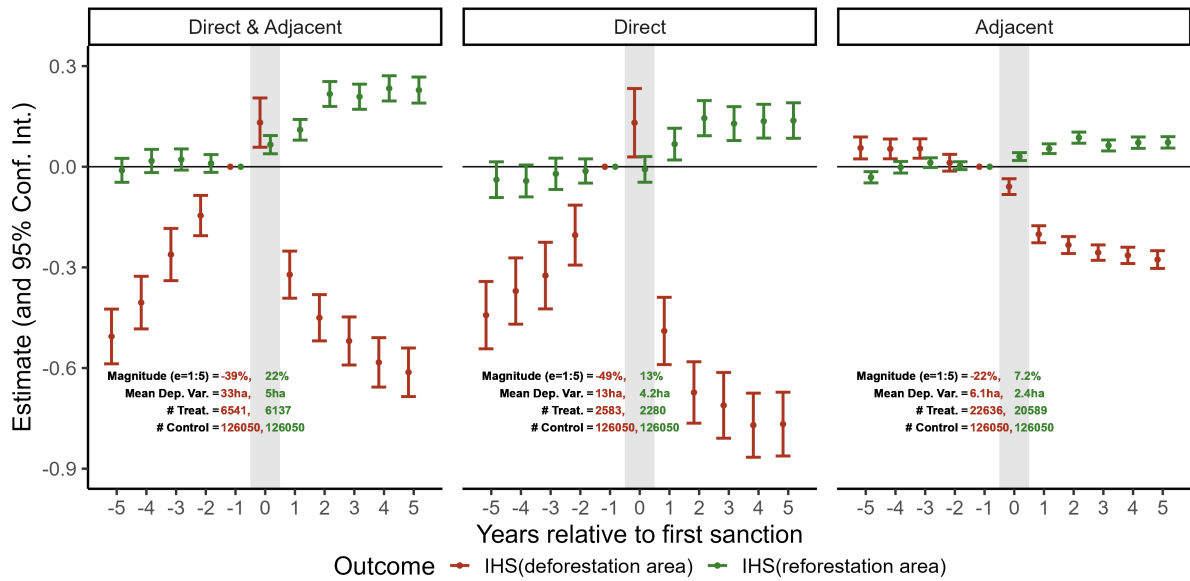
8 Figures

Figure 1: Deforestation Evolution by Punishment Type and Cohort



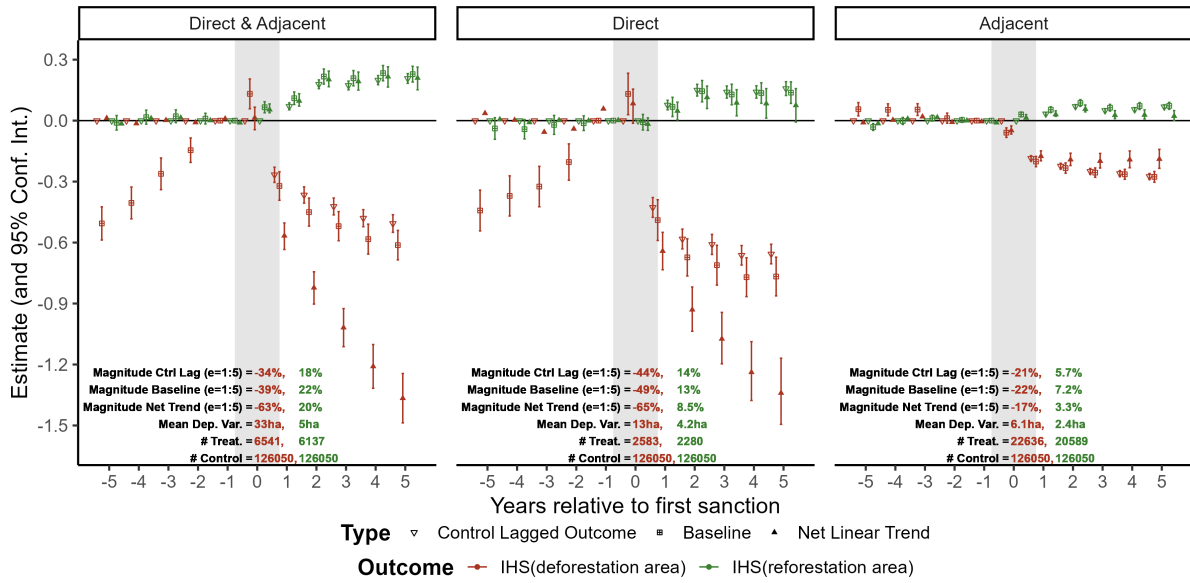
Notes: The figure plots the deforestation trajectories for each treatment cohort g by type and the two control groups (late- and never-treated). It also highlights the punishment year of each treated cohort with dashed vertical lines. The deforestation area is normalized using the inverse hyperbolic sine 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:* (Freitas et al., 2018; MapBiomass, 2021; IBAMA, 2022).

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 inverse hyperbolic sine transformation. The grey shaded area indicates the treatment year. The sample includes cohorts treated between 2005 and 2014 to observe at least five years of exposure and avoid changes in 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: (Freitas et al., 2018; MapBiomas, 2021; IBAMA, 2022).

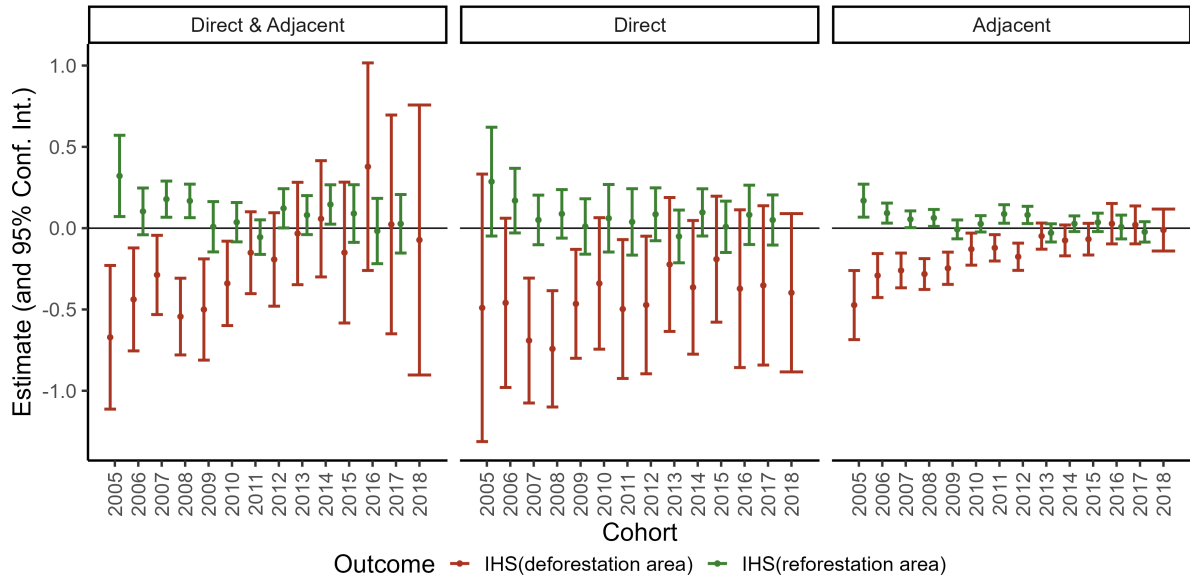
Figure 3: Robustness to Parallel Trends Violation



Notes: The figure plots the baseline event-study estimates from Figure 2, along with two robustness: one controlling for lagged outcomes ($e=-5:-1$) using the Local Projection based difference-in-differences approach as stated in Equation A.1 (Dube et al., 2023); and the other subtracting the predicted linear pre-trend, detailed in Section A.3.2. The effects are relative to the year before the first sanction for the baseline and net linear trends approaches and the year of the first sanction for the lagged outcome approach. The sample includes cohorts treated between 2005 and 2014 to observe at least five years of exposure and avoid changes in composition across relative time. For reforestation, the last cohort is 2013 because the data ends in 2018 instead of 2019. *Magnitude Ctrl Lag (e=1:5)*: is the average estimate from one to five years of exposure transformed to a percentage interpretation by $100 * (\exp(\text{average estimate}) - 1)$, controlling for lagged outcomes. *Magnitude Baseline (e=1:5)*: is the average baseline estimate from one to five years of exposure transformed into a percentage interpretation. *Magnitude Net Trend (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. For the lagged outcome estimates, bands are 95% confidence intervals based on standard errors clustered by farm. For the baseline estimates, 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). For the net linear trend estimates, bands 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: (Freitas et al., 2018; MapBiomass, 2021; IBAMA, 2022).

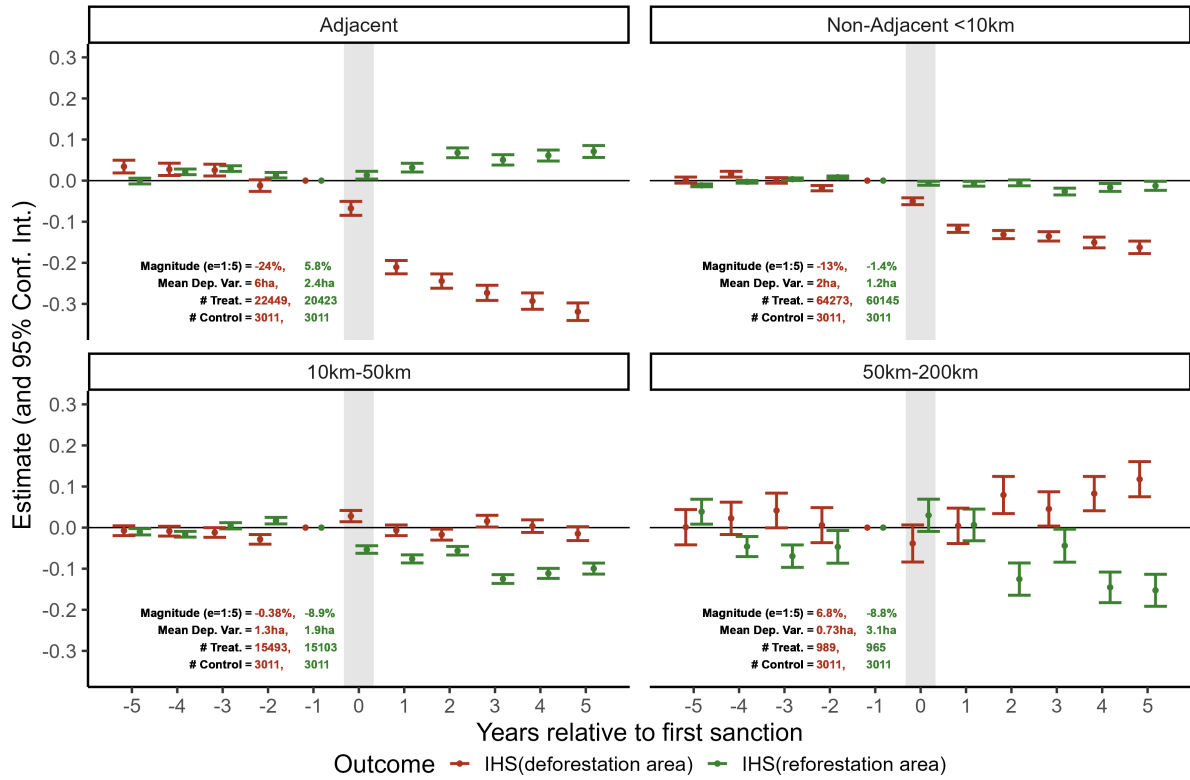
Figure 4: Sanctions Effects Over Time



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 inverse hyperbolic sine transformation. The sample includes cohorts treated between 2005 and 2014 to observe at least five years of exposure and avoid changes in 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. 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: (Freitas et al., 2018; MapBiomas, 2021; IBAMA, 2022).

Figure 5: Sanction Effects by Neighbor Distance



Notes: The figure plots the balanced event-study estimates of the environmental sanction spillover effects on deforestation and reforestation by distance ring, following Butts (2023), that adapts Gardner (2022) two-stage difference-in-differences estimator. We remove farms with direct punishment from the sample and incorporate spillovers by restricting the observations of the first-stage ($Y_{it} = \mu_i + \lambda_t + u_{it}$) to units in the control group with no spillover exposure and by including a set of distance ring indicators in the second-stage (regressing the residuals of the first stage, including all observations with no direct punishment, on indicators by relative treatment time and distance ring). The relative time goes from -5 until 5 years since the neighbor punishment, with -1 being the reference. The distance rings are: farms adjacent to a punished farm (*Adjacent*); farms non-adjacent and within 10 kilometers from a punished farm (*Non-Adjacent <10km*); farms between 10 and 50 kilometers from a punished farm (*10km-50km*); and farms between 50 and 200 kilometers from a punished farm (*50km-200km*). *Control group*: farms more than 200 kilometers from any punished farm (*never-treated*) or with a neighbor punished within 200 kilometers only after 2019 (*late-treated*). The dependent variables are normalized using the inverse hyperbolic sine transformation. The grey shaded area indicates the neighbor punishment year. The sample includes farm cohorts exposed to neighbor punishment 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. Bands are 95% confidence intervals based on standard errors clustered at the farm level, estimated using the two-stage GMM procedure suggested by Gardner (2022).

Data Sources: (Freitas et al., 2018; MapBiomass, 2021; IBAMA, 2022).

9 Tables

Table 1: Descriptive Statistics By Treatment Group

	Treatment			Control	
	Direct & Adjacent	Direct	Adjacent	Late	Never
Deforest. Area (ha)	27.4 (85.9)	10.9 (29.74)	8.1 (24.65)	3.5 (7.2)	1.4 (4.32)
Deforest. Recurrence (#)	4.6 (2.74)	4.7 (3.12)	3.8 (2.64)	4.5 (3.33)	3.6 (2.9)
Reforest. Area (ha)	4.8 (20.86)	3.4 (16.42)	2.1 (7.91)	1.4 (4.9)	1.2 (4.77)
CO2 Emissions (1,000 t)	11.7 (37.35)	4.8 (13.83)	3.3 (9.95)	1.6 (3.41)	0.6 (1.78)
Property Area (1,000 ha)	1.8 (6.67)	0.9 (3.75)	0.6 (1.85)	0.5 (1.74)	0.2 (2.24)
Forest in 2005 (%)	69.0 (23.91)	60.6 (26)	57.5 (26.22)	65.0 (26.98)	50.1 (27.29)
Forest at Punishment (%)	55.0 (25.25)	46.7 (25.3)	46.3 (26.03)	48.3 (28.69)	(NA)
Farms (#)	7297	3551	28495	2566	126050

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 exceptions are: *Forest in 2005 (%)*, which is measured in 2005; and *Forest at Punishment (%)*, which is measured at the year of the first punishment exposure for treatment groups, and 2019 for the late-treated group (last year available). The sample includes all farms in the Brazilian Amazon with any deforestation between 2000-2019, with more than 10% of primary forest coverage in 2005, with any deforestation before the first punishment, and with no punishment or with the first punishment after 2005, as detailed in Section 3.4. *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; MapBiomass, 2021; IBAMA, 2022)

Table 2: Heterogeneity by Type of Sanction

	IHS(deforestation area)				IHS(reforestation area)			
	Fine	Fine + Embargo	Fine + Seizure	Fine + Embargo + Seizure	Fine	Fine + Embargo	Fine + Seizure	Fine + Embargo + Seizure
Treat: Direct & Adjacent								
Agg. Coef. (e=1:5)	-0.669*** (0.133)	-0.557*** (0.029)	-0.19* (0.114)	-0.385*** (0.043)	0.021 (0.059)	0.199*** (0.013)	0.134* (0.073)	0.22*** (0.023)
Magnitude (e=1:5)	-49%	-43%	-17%	-32%	2.1%	22%	14%	25%
Mean Dep. Var. (e=-1)	24ha	27ha	15ha	48ha	5.8ha	3.4ha	10ha	7.8ha
# Treated Farms	151	4236	187	1967	145	3962	179	1851
# Control Farms	126050	126050	126050	126050	126050	126050	126050	126050
Treat: Direct								
Agg. Coef. (e=1:5)	-0.59*** (0.086)	-0.765*** (0.04)	-0.203** (0.087)	-0.618*** (0.119)	0.105** (0.044)	0.131*** (0.019)	0.097* (0.053)	0.105 (0.072)
Magnitude (e=1:5)	-45%	-53%	-18%	-46%	11%	14%	10%	11%
Mean Dep. Var. (e=-1)	10ha	14ha	5.8ha	17ha	3.1ha	4ha	6.4ha	4ha
# Treated Farms	292	1858	239	194	254	1632	217	177
# Control Farms	126050	126050	126050	126050	126050	126050	126050	126050
Treat: Adjacent								
Agg. Coef. (e=1:5)	-0.195*** (0.028)	-0.243*** (0.01)	-0.192*** (0.024)	-0.313*** (0.022)	0.028* (0.016)	0.068*** (0.006)	0.076*** (0.017)	0.095*** (0.012)
Magnitude (e=1:5)	-18%	-22%	-17%	-27%	2.8%	7%	7.9%	10%
Mean Dep. Var. (e=-1)	4.7ha	5.9ha	4.2ha	8.3ha	2.2ha	2.1ha	3ha	3.4ha
# Treated Farms	2021	15004	1833	3778	1828	13493	1697	3571
# Control Farms	126050	126050	126050	126050	126050	126050	126050	126050

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 inverse hyperbolic sine transformation. The sample includes cohorts treated between 2005 and 2014 to observe at least five years of exposure and avoid changes in 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: (Freitas et al., 2018; MapBiomass, 2021; IBAMA, 2022).

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)	-0.347*** (0.022)	-0.294*** (0.019)	-0.141*** (0.013)	-0.018* (0.009)
Magnitude (e=1:5)	-29.3%	-25.5%	-13.2%	-1.79%
Mean Dep. Var. (e=-1)	22ha	7.7ha	3.5ha	2.1ha
# Treated Farms	6541	6541	6541	6541
# Control Farms	126050	126050	126050	126050
Treatment: Direct				
Agg. Coef. (e=1:5)	-0.334*** (0.027)	-0.389*** (0.026)	-0.173*** (0.016)	-0.087*** (0.015)
Magnitude (e=1:5)	-28.4%	-32.2%	-15.9%	-8.32%
Mean Dep. Var. (e=-1)	8.5ha	3.3ha	1.3ha	1.3ha
# Treated Farms	2583	2583	2583	2583
# Control Farms	126050	126050	126050	126050
Treatment: Adjacent				
Agg. Coef. (e=1:5)	-0.09*** (0.006)	-0.137*** (0.006)	-0.101*** (0.005)	-0.008** (0.004)
Magnitude (e=1:5)	-8.61%	-12.8%	-9.62%	-0.791%
Mean Dep. Var. (e=-1)	3.5ha	1.7ha	0.84ha	0.64ha
# Treated Farms	22636	22636	22636	22636
# Control Farms	126050	126050	126050	126050

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 inverse hyperbolic sine 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. The sample includes cohorts treated between 2005 and 2014 to observe at least five years of exposure and avoid changes in 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: (Freitas et al., 2018; MapBiomass, 2021; IBAMA, 2022).

Table 4: Shutting Down Sanctions

Year	Deforestation Area (million hectares)		Deforestation Emission (billion tonnes CO ₂)		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	0.620	0.254	0.300	0.340	0.337
2008	0.479	0.577	0.214	0.284	0.374	0.367
2009	0.240	0.403	0.114	0.224	0.331	0.320
2010	0.211	0.386	0.102	0.222	0.412	0.399
2011	0.268	0.428	0.121	0.233	0.403	0.393
2012	0.192	0.375	0.093	0.218	0.492	0.476
2013	0.238	0.427	0.113	0.246	0.449	0.428
2014	0.235	0.428	0.115	0.251	0.318	0.304
2015	0.283	0.472	0.139	0.274	0.328	0.311
2016	0.333	0.533	0.167	0.312	0.337	0.319
2017	0.284	0.487	0.141	0.287	0.333	0.318
2018	0.334	0.533	0.167	0.312	0.338	0.328
2019	0.316	0.524	0.160	0.310	NA	NA
2006-2019	4.728	6.996	2.248	3.848	4.767	4.609

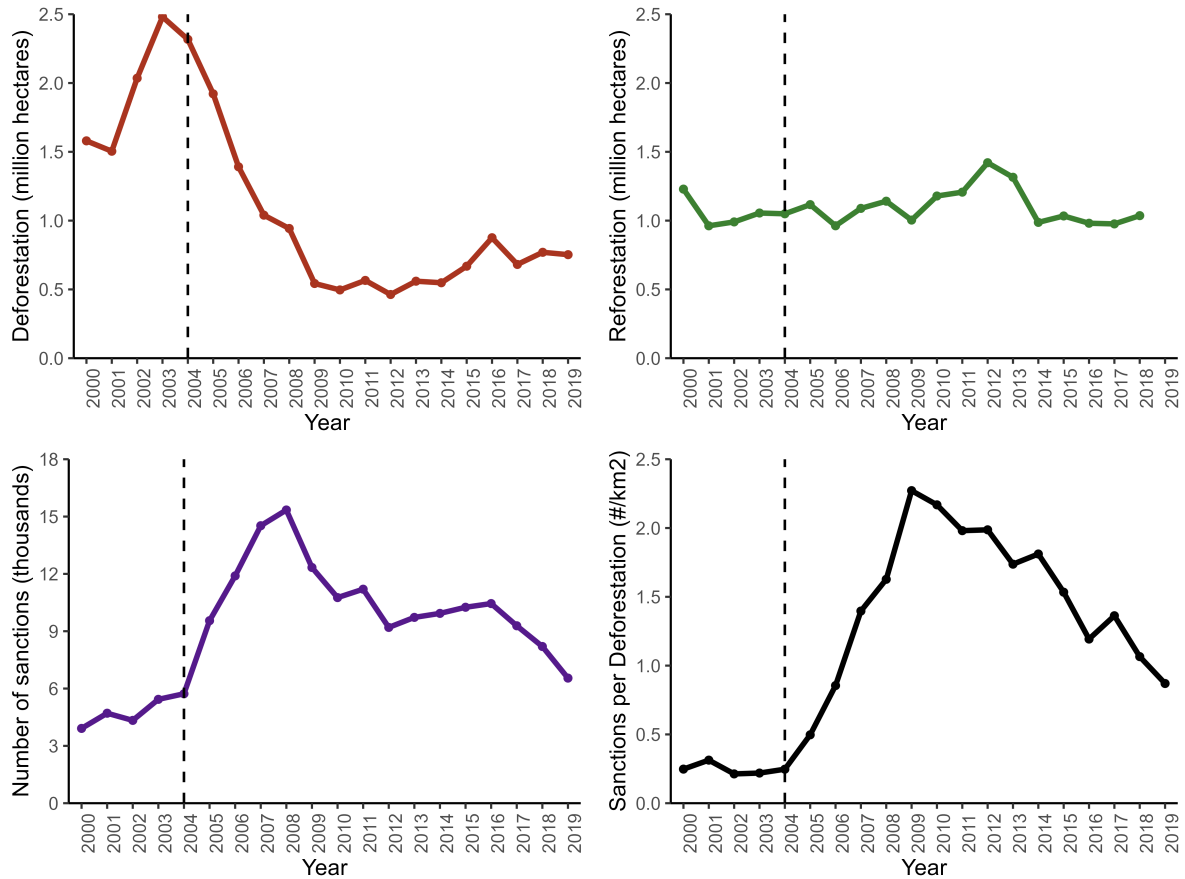
Notes: This table presents the annual deforestation area, deforestation CO₂ emission, and reforestation area within farms from 2006 through 2019 and the sum across all years (Observed). It also presents the counterfactual annual values for each measure, considering a scenario with no effects from sanctions issued between 2005 and 2018 (Full Shutdown).

Data Sources: (Freitas et al., 2018; MapBiomass, 2021; GFW, 2022; IBAMA, 2022).

A Appendix

A.1 Additional Figures

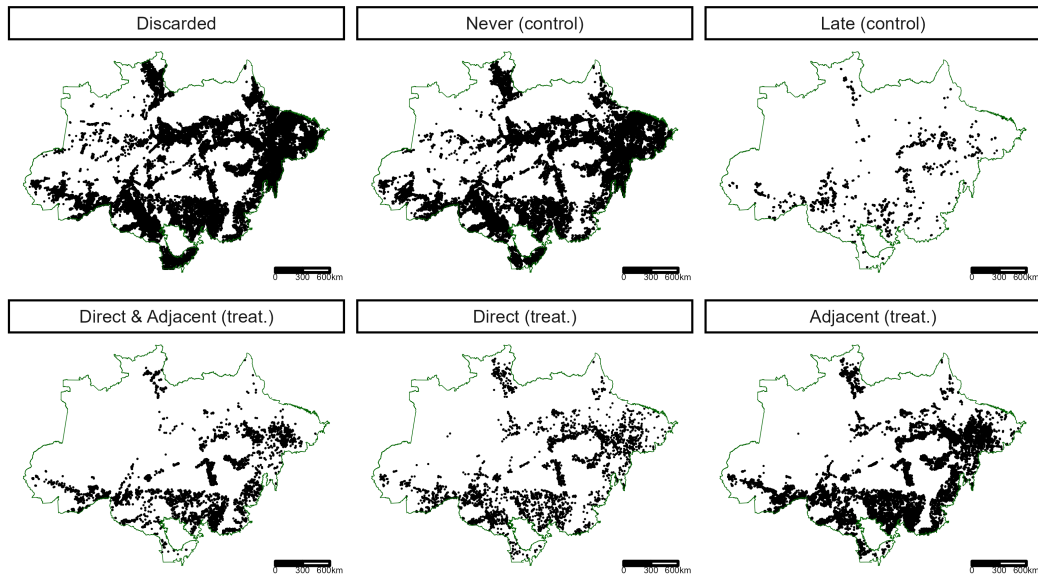
Figure A.1: Deforestation, Reforestation, and Environmental Sanctions



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, 2021; IBAMA, 2022).

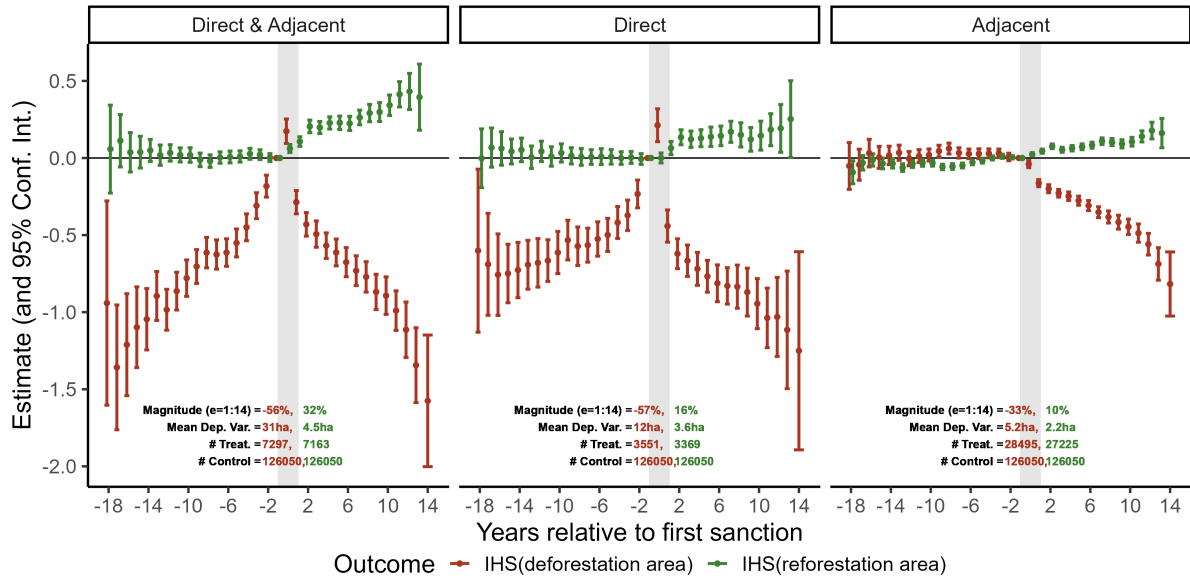
Figure A.2: Farms Spatial Distribution



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 3.4. *Discarded:* farms with no deforestation between 2000-2019 or less than 10% of primary forest coverage in 2005 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.

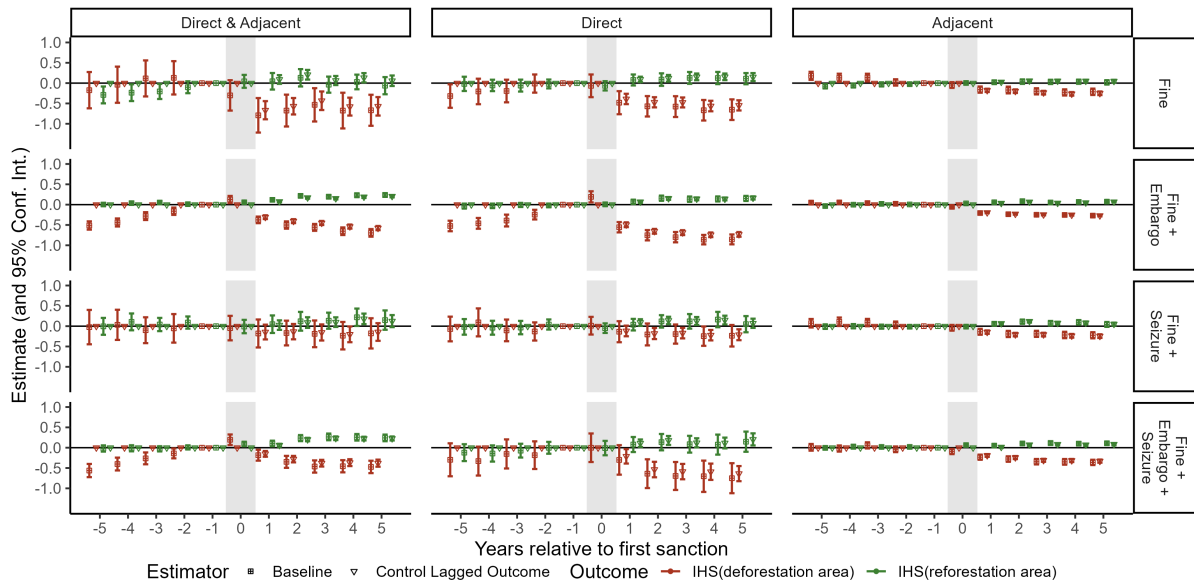
Data Sources: (Freitas et al., 2018; MapBiomias, 2021; IBAMA, 2022).

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 effects are relative to the year before the first sanction. The dependent variables are normalized using the inverse hyperbolic sine transformation. The grey shaded area indicates the treatment year. The sample includes 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:* (Freitas et al., 2018; MapBiomass, 2021; IBAMA, 2022).

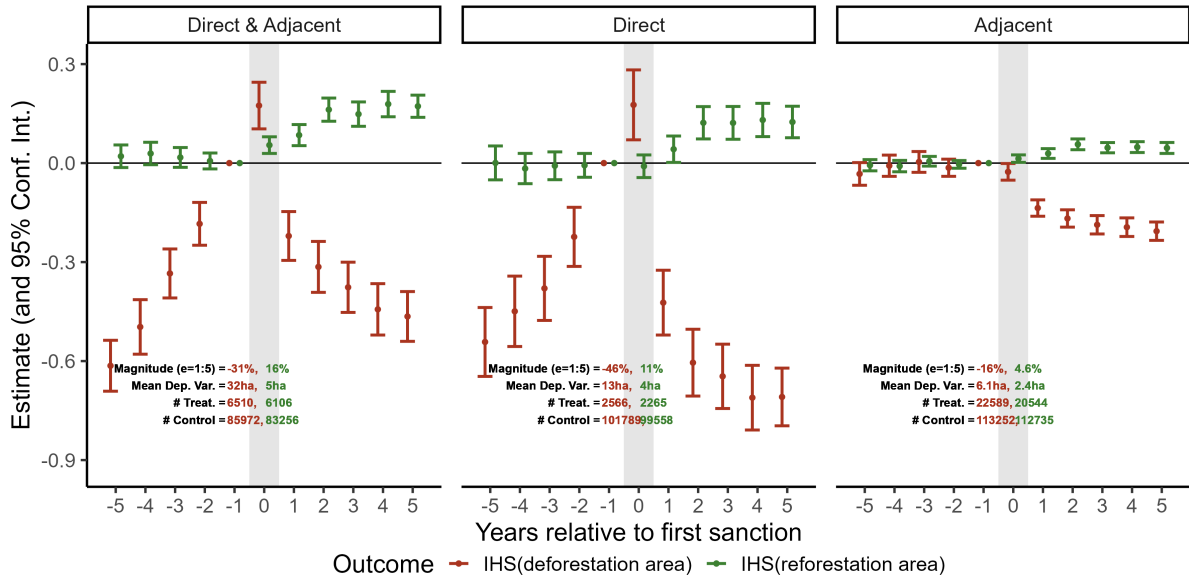
Figure A.4: Heterogeneity by Type of Sanction



Notes: The figure plots the baseline 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 treatment type and varying the type of sanction. It also includes a robustness controlling for lagged outcomes ($e = -5 : -1$) using the Local Projection based difference-in-differences approach as stated in Equation A.1. The effects are relative to the year before the first sanction for the baseline estimator and to the year of punishment for the lagged outcome approach. The dependent variables are normalized using the inverse hyperbolic sine transformation. The sample includes cohorts treated between 2005 and 2014 to observe at least five years of exposure and avoid changes in 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. *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. For the lagged outcome estimates, bands are 95% confidence intervals based on standard errors clustered by farm. For the baseline estimates, 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).

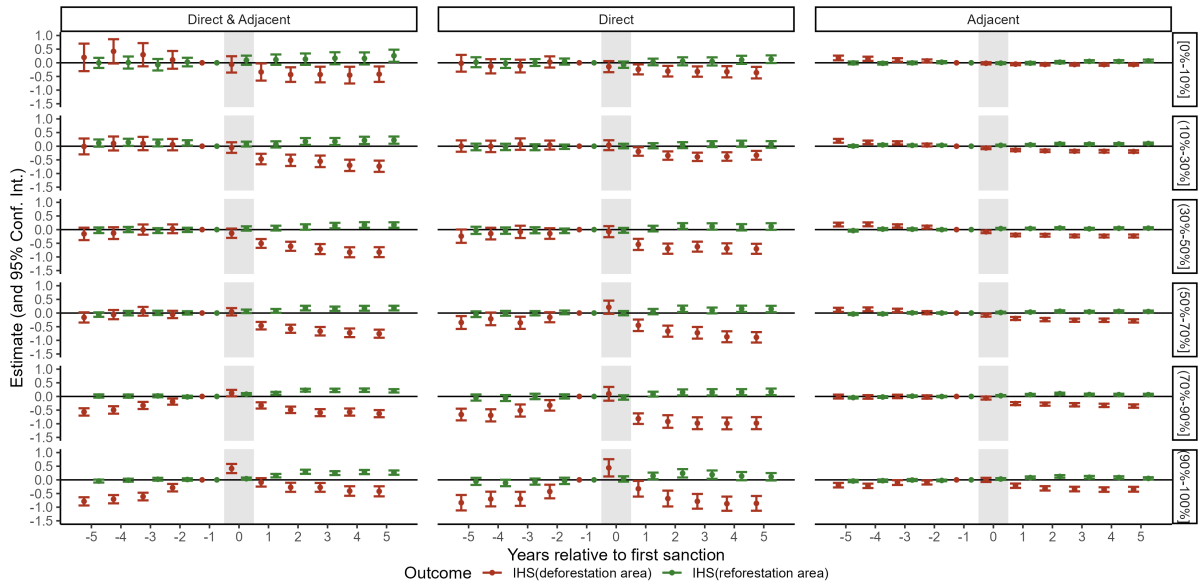
Data Sources: (Freitas et al., 2018; MapBiomass, 2021; IBAMA, 2022).

Figure A.5: Within Municipality and Property Size Estimator



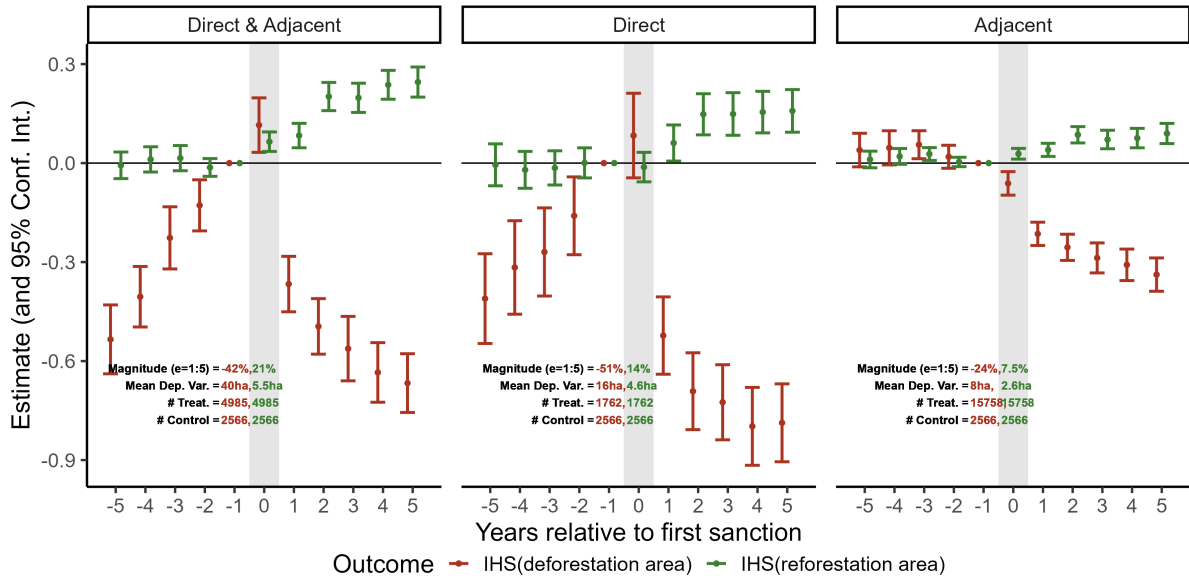
Notes: The figure plots the balanced event-study aggregation by relative time since the treatment year ($e = t - g \in \{-5 : 5\}$), of the environmental sanction effects on deforestation and reforestation for each type of treatment, using the within municipality and property size outcome regression approach (Equation A.3). The effects are relative to the year before the first sanction. The dependent variables are normalized using the inverse hyperbolic sine transformation. The grey shaded area indicates the treatment year. The sample includes cohorts treated between 2005 and 2014 to observe at least five years of exposure and avoid changes in 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*: (Freitas et al., 2018; MapBiomass, 2021; IBAMA, 2022).

Figure A.6: Heterogeneity by Forest Cover



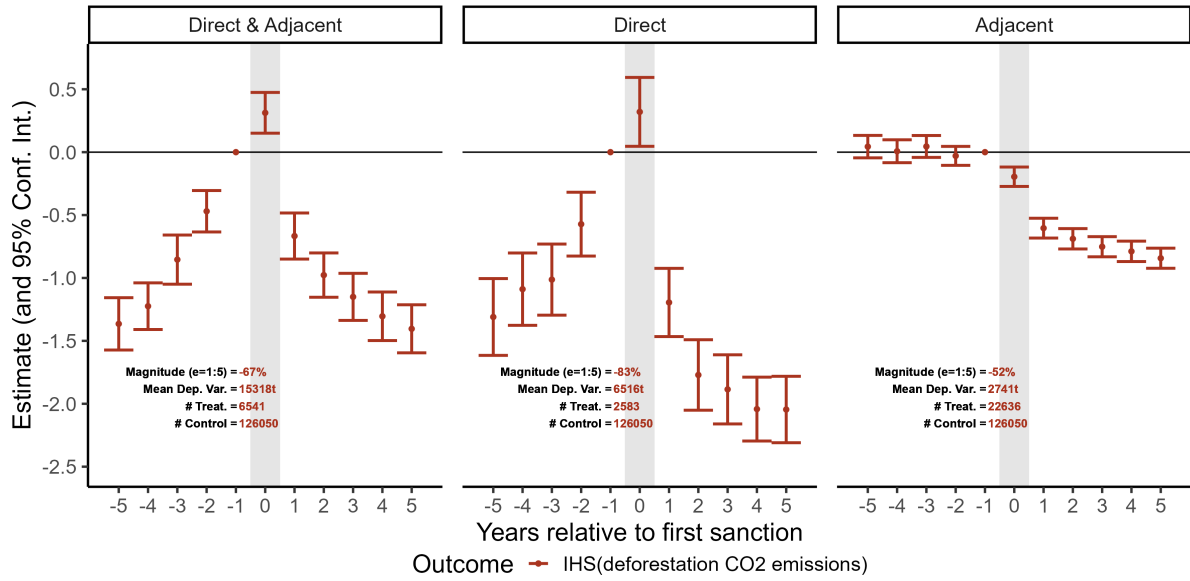
Notes: The figure plots the balanced event-study aggregation, which combines the estimates from Equation 1 by relative time since the treatment year ($e = t - g \in \{-5 : 5\}$), of the environmental sanction effects on deforestation and reforestation for each type of treatment and varying the bin of forest cover in 2005. The effects are relative to the year before the first sanction. The dependent variables are normalized using the inverse hyperbolic sine transformation. The sample includes cohorts treated between 2005 and 2014 to observe at least five years of exposure and avoid changes in 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 within the forest cover bin. *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:* (Freitas et al., 2018; MapBiomass, 2021; IBAMA, 2022).

Figure A.7: 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 effects are relative to the year before the first sanction. The dependent variables are normalized using the inverse hyperbolic sine transformation. The grey shaded area indicates the treatment year. The sample includes 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 & 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*: (Freitas et al., 2018; MapBiomass, 2021; IBAMA, 2022).

Figure A.8: 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 CO_2 emissions for each type of treatment. The effects are relative to the year before the first sanction. The dependent variable is normalized using the inverse hyperbolic sine transformation. The grey shaded area indicates the treatment year. The sample includes cohorts treated between 2005 and 2014 to observe at least five years of exposure and avoid changes in 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: (Freitas et al., 2018; MapBiomas, 2021; 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	9	21	109	0.01	0.14	0.11	0.10	0.09	0.10	1.42
2001	16	53	259	0.02	0.16	0.16	0.15	0.51	0.33	1.89
2002	24	102	300	0.02	0.54	0.41	0.37	1.34	0.37	3.27
2003	54	213	563	0.08	1.11	0.79	0.52	2.93	1.24	5.80
2004	60	356	999	0.06	2.73	1.52	0.24	4.45	2.24	9.73
2005	142	530	1503	0.26	2.60	1.72	0.76	5.44	2.61	10.87
2006	253	834	2791	0.39	3.50	2.35	0.66	6.34	3.20	31.15
2007	404	1270	4214	0.36	3.04	3.67	0.76	6.55	3.77	55.05
2008	372	1054	3856	0.46	2.62	3.31	1.21	4.42	2.50	54.42
2009	291	540	2963	0.30	1.19	2.10	0.68	4.19	1.84	52.28
2010	250	660	2539	0.29	0.99	1.90	1.08	4.48	2.25	70.86
2011	292	779	2808	0.37	0.69	1.62	1.23	3.79	2.70	100.00
2012	277	750	3057	0.20	0.75	1.48	1.07	5.51	2.62	100.00
2013	269	543	2667	0.18	0.46	0.92	1.74	3.26	1.84	100.00
2014	321	468	2408	0.27	0.41	1.10	1.50	2.30	1.07	100.00
2015	349	337	2384	0.25	0.26	1.19	2.17	1.92	1.57	100.00
2016	251	209	1436	0.50	0.15	0.85	1.16	2.25	1.25	100.00
2017	254	186	1671	0.19	0.19	0.84	1.24	2.34	0.88	100.00
2018	194	160	1536	0.18	0.41	1.05	1.07	1.98	1.03	100.00
2019	191	112	1047	0.12	0.16	0.59	1.87	1.42	1.32	99.97
2020	110	42	670	0.08	0.05	0.40	NA	NA	NA	100.00
2021	117	31	590	0.12	0.02	0.38	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, 2021; IBAMA, 2022).

Table A.2: Heterogeneity by Forest Cover

	IHS(deforestation area)						IHS(reforestation area)					
	[0%-10%]	(10%-30%]	(30%-50%]	(50%-70%]	(70%-90%]	(90%-100%]	[0%-10%]	(10%-30%]	(30%-50%]	(50%-70%]	(70%-90%]	(90%-100%]
Treatment: Direct & Adjacent												
Agg. Coef. (e=1:5)	-0.414*** (0.121)	-0.595*** (0.067)	-0.697*** (0.061)	-0.637*** (0.047)	-0.523*** (0.04)	-0.293*** (0.053)	0.167** (0.076)	0.17*** (0.038)	0.128*** (0.031)	0.154*** (0.025)	0.201*** (0.018)	0.25*** (0.023)
Magnitude (e=1:5)	-33.9%	-44.9%	-50.2%	-47.1%	-40.7%	-25.4%	18.2%	18.5%	13.6%	16.7%	22.3%	28.4%
Mean Dep. Var. (e=-1)	7.7ha	28ha	38ha	36ha	32ha	29ha	5.8ha	7.7ha	7.7ha	6.8ha	3.2ha	2.2ha
# Treated Farms	129	565	976	1435	2084	1481	123	542	934	1364	1952	1345
# Control Farms	23463	38802	29127	23059	20880	14182	23463	38802	29127	23059	20880	14182
Treatment: Direct												
Agg. Coef. (e=1:5)	-0.312*** (0.072)	-0.328*** (0.057)	-0.651*** (0.063)	-0.719*** (0.064)	-0.935*** (0.073)	-0.706*** (0.098)	0.075* (0.044)	0.058 (0.036)	0.097*** (0.035)	0.121*** (0.035)	0.142*** (0.037)	0.165*** (0.043)
Magnitude (e=1:5)	-26.8%	-27.9%	-47.9%	-51.3%	-60.7%	-50.6%	7.81%	5.95%	10.2%	12.9%	15.3%	18%
Mean Dep. Var. (e=-1)	4.9ha	7.1ha	11ha	12ha	19ha	15ha	4.3ha	7.5ha	3.4ha	4.3ha	2.9ha	1.6ha
# Treated Farms	220	453	527	596	624	383	202	417	468	537	545	313
# Control Farms	23463	38802	29127	23059	20880	14182	23463	38802	29127	23059	20880	14182
Treatment: Adjacent												
Agg. Coef. (e=1:5)	-0.061*** (0.016)	-0.174*** (0.015)	-0.221*** (0.017)	-0.251*** (0.019)	-0.305*** (0.019)	-0.312*** (0.024)	0.036*** (0.013)	0.079*** (0.01)	0.045*** (0.011)	0.052*** (0.011)	0.074*** (0.009)	0.089*** (0.014)
Magnitude (e=1:5)	-5.91%	-16%	-19.8%	-22.2%	-26.3%	-26.8%	3.66%	8.25%	4.62%	5.39%	7.66%	9.28%
Mean Dep. Var. (e=-1)	0.8ha	4.2ha	5.8ha	7.3ha	6.9ha	5.8ha	2.4ha	2.9ha	3.1ha	2.6ha	1.7ha	1.1ha
# Treated Farms	1816	4375	4971	5027	5260	3003	1722	4040	4540	4540	4793	2676
# Control Farms	23463	38802	29127	23059	20880	14182	23463	38802	29127	23059	20880	14182

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 2005. The dependent variables are normalized using the inverse hyperbolic sine transformation. The sample includes cohorts treated between 2005 and 2014 to observe at least five years of exposure and avoid changes in 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 within the forest cover bin. *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: (Freitas et al., 2018; MapBiomass, 2021; IBAMA, 2022).

Table A.3: 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)	-0.5*** (0.05)	-0.49*** (0.04)	-0.39*** (0.04)	-0.38*** (0.03)	-0.41*** (0.05)	-0.5*** (0.05)	-0.4*** (0.04)	-0.55*** (0.03)
Magnitude (e=1:5)	-40%	-39%	-33%	-31%	-34%	-40%	-33%	-42%
Mean Dep. Var. (e=-1)	76ha	19ha	9.1ha	7.7ha	22ha	90ha	16ha	41ha
# Treated Farms	1832	3171	1538	3446	1333	1762	2253	4288
# Control Farms	9814	79854	36382	112474	9503	4073	30584	95466
Treatment: Direct								
Agg. Coef. (e=1:5)	-0.69*** (0.08)	-0.64*** (0.04)	-0.73*** (0.07)	-0.63*** (0.04)	-0.67*** (0.07)	-0.67*** (0.1)	-0.72*** (0.06)	-0.67*** (0.04)
Magnitude (e=1:5)	-50%	-47%	-52%	-47%	-49%	-49%	-51%	-49%
Mean Dep. Var. (e=-1)	23ha	12ha	8.2ha	6.9ha	14ha	38ha	13ha	13ha
# Treated Farms	487	1554	542	1671	504	408	778	1805
# Control Farms	9814	79854	36382	112474	9503	4073	30584	95466
Treatment: Adjacent								
Agg. Coef. (e=1:5)	-0.26*** (0.02)	-0.24*** (0.01)	-0.21*** (0.02)	-0.19*** (0.01)	-0.28*** (0.03)	-0.28*** (0.03)	-0.3*** (0.02)	-0.23*** (0.01)
Magnitude (e=1:5)	-23%	-21%	-19%	-17%	-24%	-25%	-26%	-20%
Mean Dep. Var. (e=-1)	15ha	4.3ha	2.8ha	2.5ha	9ha	24ha	4.6ha	6.6ha
# Treated Farms	4319	12554	5763	16412	3579	2645	6032	16604
# Control Farms	9814	79854	36382	112474	9503	4073	30584	95466

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 group. The dependent variables are normalized using the inverse hyperbolic sine 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. The sample includes cohorts treated between 2005 and 2014 to observe at least five years of exposure and avoid changes in composition across relative time. *Control group:* farms with no direct or adjacent neighbor receiving any sanction between 2000-2021 within the property group. *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: (SFB, 2017; Freitas et al., 2018; MapBiomias, 2021; IBAMA, 2022).

Table A.4: Varying Outcome Transformation

	Deforestation					Reforestation				
	IHS	% Prop.	Area	Dummy	Log	IHS	% Prop.	Area	Dummy	Log
Treat: Direct & Adjacent										
Agg. Coef. (e=1:5)	-0.497*** (0.023)	-1.332*** (0.126)	-16.748*** (1.793)	-0.098*** (0.006)	-0.202*** (0.03)	0.2*** (0.011)	0.115*** (0.016)	2.146*** (0.342)	0.046*** (0.005)	0.237*** (0.03)
Magnitude (e=1:5)	-39.2%	-37.1%	-51.1%	-18.1%	-18.3%	22.1%	40.4%	42.9%	6.61%	26.8%
Mean Dep. Var. (e=-1)	33ha	3.6%	33ha	0.54	33ha	5ha	0.28%	5ha	0.7	5ha
# Treated Farms	6541	6541	6541	6541	6541	6137	6137	6137	6137	6137
# Control Farms	126050	126050	126050	126050	126050	126050	126050	126050	126050	126050
Treat: Direct										
Agg. Coef. (e=1:5)	-0.682*** (0.033)	-2.423*** (0.194)	-9.212*** (0.917)	-0.178*** (0.01)	-0.526*** (0.047)	0.123*** (0.016)	0.113*** (0.02)	0.305 (0.614)	0.031*** (0.007)	0.166*** (0.043)
Magnitude (e=1:5)	-49.5%	-66.9%	-70%	-37.3%	-40.9%	13.1%	25.9%	7.35%	3.82%	18.1%
Mean Dep. Var. (e=-1)	13ha	3.6%	13ha	0.48	13ha	4.2ha	0.44%	4.2ha	0.81	4.2ha
# Treated Farms	2583	2583	2583	2583	2583	2280	2280	2280	2280	2280
# Control Farms	126050	126050	126050	126050	126050	126050	126050	126050	126050	126050
Treat: Adjacent										
Agg. Coef. (e=1:5)	-0.246*** (0.008)	-0.808*** (0.054)	-3.564*** (0.275)	-0.08*** (0.003)	-0.135*** (0.017)	0.07*** (0.005)	0.067*** (0.009)	0.414*** (0.136)	0.023*** (0.002)	0.121*** (0.015)
Magnitude (e=1:5)	-21.8%	-40.1%	-58.8%	-25.3%	-12.7%	7.22%	13.7%	17%	2.95%	12.9%
Mean Dep. Var. (e=-1)	6.1ha	2%	6.1ha	0.31	6.1ha	2.4ha	0.48%	2.4ha	0.77	2.4ha
# Treated Farms	22636	22636	22636	22636	22636	20589	20589	20589	20589	20589
# Control Farms	126050	126050	126050	126050	126050	126050	126050	126050	126050	126050

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 inverse hyperbolic sine 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. The sample includes cohorts treated between 2005 and 2014 to observe at least five years of exposure and avoid changes in 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*: (Freitas et al., 2018; MapBiomass, 2021; IBAMA, 2022).

A.3 Relaxing the Parallel Trends Assumption

A.3.1 Conditioning on Lagged Outcomes (Local Projection Estimator)

To address concerns about the parallel trends assumption, we conduct a robustness exercise by controlling for the pre-treatment lagged outcome dynamics. This approach helps to mitigate the risk of selection bias and regression to the mean effects.^{A.1} 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 future outcome trends.^{A.2}

We use the local projection DD estimator proposed by Dube et al. (2023) to estimate the treatment effects while conditioning on the pre-treatment lagged outcomes. Specifically, we estimate the following specification separately for each outcome-treatment combination:

$$\begin{aligned}
 y_{i,t+h} - y_{i,t} = & \beta_h^{type} \Delta D_{it}^{type} \text{ (treatment type indicator)} \\
 & + \sum_{k=1}^5 \gamma_k^h \Delta y_{i,t-k} \text{ (outcome lags)} \\
 & + \delta_t^h \text{ (time effects)} \\
 & + e_{it}^h \text{ (error term)},
 \end{aligned} \tag{A.1}$$

where $y_{i,t+h} - y_{i,t}$ represents the change in the outcome variable for farm i at time $t + h$ compared to time t , ΔD_{it}^{type} is the first difference of the treatment type indicator, $\Delta y_{i,t-k}$ represents the lagged outcome variables, β_h^{type} and γ_k^h are the corresponding coefficients of interest, δ_t^h captures time fixed effects, and e_{it}^h is the error term.

To ensure the robustness of the estimates, we restrict the sample to observations that are either newly treated or clean control. In other words, we include farms that were newly treated ($\Delta D_{it}^{type} = 1$) or farms that did not receive any treatment ($D_{i,t+h}^{type} = 0$)

^{A.1}As IBAMA targets farms based on recent deforestation, an exceptional year with a large increase in deforestation average could trigger punishment among farmers more subject to mechanically reducing deforestation afterward, regressing to their mean.

^{A.2}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 punished since we do not observe the universe of punishments before 2011. 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.

for all treatment types) throughout the analysis period. We also re-weight the regression to generate the average treatment effect on the treated (ATT) rather than variance-weighted ATT

The local projection estimator with lagged outcomes allows us to examine the treatment effects while controlling for the pre-treatment trajectory of the outcome variable. By comparing the the balanced event study for each outcome-treatment combination of this estimator with the baseline estimator, we can assess the sensitivity of the treatment effects to the assumption of parallel trends. See Dube et al. (2023) for more details about the method.

A.3.2 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 for linear violations ($M=0$).

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.

We use the outcome regression approach from Sant'Anna and Zhao (2020) with two steps. First, we estimate the change in outcomes among never-treated farms ($C_i = 1$) for each municipality-by-property size group ($X(i) = M(i) \times S(i)$):^{A.3}

$$\Delta \hat{\mu}_{g-1,t}(x) = \frac{\sum_i (\Delta Y_{ig-1,t}) 1\{C_i = 1\} 1\{X(i) = x\}}{1\{C_i = 1\} 1\{X(i) = x\}} \quad (\text{A.2})$$

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

$$\widehat{ATT}_{OR}^{type}(g, t) = \frac{\sum_i ((\Delta Y_{ig-1,t}) - \Delta \hat{\mu}_{g-1,t}(x)) 1\{G_i^{type} = g\}}{\sum_i 1\{G_i^{type} = g\}} \quad (\text{A.3})$$

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 perform the balanced event-study aggregation using the same multiplier bootstrap procedure for inference.

^{A.3}We remove farms in $X(i)$ groups without at least one treated and one control farm.