Do Land Titles Deter Deforestation? Evidence from Brazil

Renan Chaves Yoshida*

April 9, 2024

Abstract

Illegal deforestation has emerged as a critical global concern, leading to the introduction of various countermeasures. A notable policy is the granting of formal property rights to indigenous communities living in endangered areas. Despite its increasing popularity, evidence regarding the effectiveness of this policy for curbing deforestation remains scant, particularly concerning the conditions under which it operates effectively. To address this gap, I investigate the staggered allocation of land titles to indigenous communities in the Brazilian Amazon. I employ a doubly-robust generalized difference-in-differences framework to assess the impact on deforestation. I find that titling reduces the probability of deforestation by 7 percentage points. The effect is driven by lands located in areas of high baseline deforestation and level of enforcement.

*Stanford University - e-mail: yoshida2@stanford.edu

1 Introduction

Forests such as the Amazon are crucial in supporting biodiversity and act as a carbon sink (Gatti et al., 2021). Illegal deforestation in these areas then poses a significant threat to climate change (Lawrence and Vandecar, 2014). The burden is particularly large for inhabitants of these regions. In many tropical countries like Brazil there are many indigenous communities living throughout the territory, with a large concentration in the rainforests. Deforestation often occurs inside their lands, threatening their way of life through land seizures, violence and environmental degradation. In response to these challenges, the Brazilian government started a program in 1988 to formalize property rights for lands traditionally occupied by indigenous peoples. Understanding if this policy indeed reduces deforestation is of crucial importance.

In this paper I investigate whether titling of lands traditionally occupied by indigenous communities in the Brazilian Amazon reduces deforestation. I exploit the staggered adoption of this policy and implement a generalized Difference-in-Differences framework from Callaway and Sant'Anna (2021). To alleviate concerns regarding parallel trends, I run a Doubly-Robust (Sant'Anna and Zhao, 2020) version with variables typically found in the literature to be predictors of deforestation. I also remove neighboring lands from the sample to eliminate spillover effects.

I find that titling reduces the frequency of deforestation by 7 percentage points. This effect is economically significant. A back-of-the-envelope calculation shows titling reduced deforested area by around 13 football fields, per year. I also show that areas with higher threat of deforestation have a higher impact of titling. In particular, titled lands located in the region of the Amazon where deforestation is high have 15% less deforestation than non-titled lands in that region.

This paper contributes to different strands of literature. First, there is an extensive literature on drivers of deforestation. It includes both correlation and causal analyses. Typical drivers are geological such as elevation or slope or economic like distance to markets, population and income (Araújo et al., 2023; Busch and Ferretti-Gallon, 2017; Chomitz and Thomas, 2003; Hargrave and Kis-Katos, 2013; Laurance et al., 2002), trade (Abman and Lundberg, 2020; Carreira et al., 2022), agricultural prices (Assunção et al., 2015), political incentives (Alesina et al., 2019; Balboni et al., 2021; Burgess et al., 2012; Cisneros et al., 2021; Marchand, 2016; Pailler, 2018), externalities (Balboni et al., 2023), enforcement (Assunção et al., 2023; Simonet et al., 2019) and land tenure (Andam et al., 2008; Araujo et al., 2009; Baragwanath and Bayi, 2020; BenYishay et al., 2017; Benzeev et al., 2023; Bonilla-Mejía and Higuera-Mendieta, 2019; Blackman et al., 2017; Liscow, 2013; Pfaff et al., 2015; Soares-Filho et al., 2010). Most similar to my work, BenYishay et al. (2017) found no significant effect of land demarcation on deforestation. However, I focus instead on the step where rights are officially recognized. Benzeev et al. (2023) applied an event study and a difference-in-differences strategy to analyze the impact of land rights in the Atlantic Forest. They use a similar empirical strategy and show improvement in forest outcomes when comparing formalized and non-formalized lands. However most of the area under threat is in the Amazon region (where also most of the titling happens) and they only use area as a control variable, which warrants concern about the validity of parallel trends in their setting. Baragwanath and Bayi (2020) study the Amazon and using an RDD strategy find a decrease in deforestation from titling. They exploit the land boundaries as the running variable, but since the demarcation of these lands is endogenous, the RDD assumptions are unlikely to hold. I add to the literature by improving the credibility of having obtained a causal estimate by using a combination of novel methods and exploiting the richness of regional data to aid identification. Additionally, this bulk of work has largely neglected heterogeneity. Part of the mixed evidence found (for instance, Liscow (2013) finds titling increases deforestation in Nicaragua) could be due to cross-country differences. I contribute by looking at how effects can be heterogeneous not only cross-country, but within.

Lastly, it contributes to the literature on difference-in-differences (DiD). Most papers rely on the assumption of parallel trends and often rely on multiple time periods to 'test' for pre-trends using event-study equations or other sources of identification such as a placebo group. However, recent work has highlighted tests for parallel trends using pre-periods are often under-powered (Roth, 2022) and other issues especially in staggered treatment settings (Borusyak et al., 2024; Callaway and Sant'Anna, 2021; Sun and Abraham, 2021; Wooldridge, 2022). This recent work has been mostly theoretical, and whether these methods can be successfully used in applications is still an open question.[1] I contribute by applying in a real world setting a new method that circumvents common issues with DiD with staggered treatment, in particular showing how covariates can be included in a flexible and efficient way when there is concern about parallel trends not holding exactly.[2]

The rest of the paper goes as follows. Section 2 describes the empirical setting. Section 3 presents the data and basic descriptive statistics. Section 4 delineates my empirical strategy. Section 5 shows the main results on deforestation. Section 6 examines heterogeneity. Section 7 discusses the results and policy implications, and Section 8 concludes.

[1]See Berman and Israeli (2022); Benzeev et al. (2023); Braghieri et al. (2022) for some examples of applied work. Neither uses covariates.

[2]Most similar to my paper, Kline and Moretti (2014) also uses covariates to assuage concerns about parallel trends. They do not use the Doubly-Robust approach I employ here, so their method is more sensitive to model misspecification.

2 Background

Deforestation in Brazil. Our setting is Brazil, home of the most diverse biomes in the world. For instance, the Amazon covers over 4 million square kilometers and most of its area is situated within Brazilian national borders. It constitutes over half of the remaining tropical rainforests on Earth. Given its size, the forest is key for regulating the carbon cycle and keeping global warming in check (Gatti et al., 2021; Lawrence and Vandecar, 2014). In particular, it absorbs one-fourth of the CO2 absorbed by all the land on Earth. It is therefore a vital carbon reserve.

The Amazon has attracted a lot of attention in the media in recent years given the increase in deforestation in the region. Since 1985, 15% of its forests have been entirely lost (Mapbiomas, 2023a). This is concerning given its negative effect on the local wildlife, the carbon stock the forest provides and communities natives to the region. Outsiders find land and take down the trees to make room for either planting crops, ranching of cattle or mining. These lands are often inhabited, and clearing of these lands by outsiders often involves killing.

Indigenous Communities. Hundreds of communities have been living in the region for thousands of years. They live mostly of their own subsistence. Their nutrition is usually obtained from hunting, fishing or subsistence farming. They vary in size, ethnicity, and language spoken. Most tribes have members that speak Portuguese, but there are isolated tribes. Their culture has been mostly preserved and the relationship to nature is a key part of it.

Indigenous Lands and Titling. Indigenous lands are the ones considered to be traditionally occupied by one or more indigenous community or village. Following the fall of the dictatorship, Brazil enacted a new constitution in 1988 that establishes 'ancestral right' over those lands, i.e., considers the indigenous people to be the first and natural owners of those lands. For a piece of land to be officially declared as belonging to the natives, there is a protocol to be followed: First, the National Indigenous Foundation (FUNAI) starts a study to identify boundaries. After FUNAI concludes this process it is sent to the Minister of Justice, who has 30 days to approve it. Lastly, it requires a final approval by presidential decree. The entire process can take over ten years. Titling is meant to gives locals an assurance their land will not be expropriated or used for commercial purposes, either by the government or by squatters. There are a few ways in which this can affect deforestation. It could deter outsider incursions through a fear of fines or incarceration, and it could empower the locals to fight against invaders. Both of these would reduce deforestation.[3]

[3] Another mechanism discussed in the literature on property rights is that having more security increases

3 Data and Descriptive Statistics

I assembled a rich dataset by combining information from several sources. In this section I describe the main variables obtained, their source and descriptive statistics.

3.1 Data

Deforestation and Land Use. I collect yearly deforestation data for the period 1988 to 2019 from *Mapbiomas Brasil*. The data is constructed from pixel-level satellite imagery and processing algorithms from Google Earth Engine. Each $30m^2$ land pixel is tracked and classified every year into one of multiple uses ranging from virgin forest to anthropic modes like farming, pasture or mining. A pixel is considered deforested in a given year if in that year the pixel classification changed from forest to an anthropic mode. The sum of the total area of pixels deforested by year and land is then calculated.[4]

Indigenous Lands. The primary dataset used in this study includes the geolocation of each indigenous land in Brazil and the year land rights were granted by Presidential executive order starting in 1988. This information is available on the website of the National Indigenous Foundation (FUNAI), a Brazilian ministry tasked with protecting the indigenous population.[5]This dataset also includes the area of each land.

Market Access Variables. I obtain data on the geolocation of paved roads, railroads and ports from Brazil's Ministry of Transportation. I use the earliest data point available, 1990. I also obtained data on navigable rivers from the National Water Agency (ANA) and urban centers[6] is obtained from the Brazilian Institute of Geography and Statistics (IBGE).

Geological Variables. Elevation and slope data were obtained from a Digital Elevation Model compiled by NASA's Shuttle Radar Topography Mission (SRTM).

3.2 Descriptive Statistics

Deforestation. Figure 1 plots the fraction of lands that had any deforestation in a given year. There is much variation from year to year. What explains this variation? Assunção et al. (2015) and Burgess et al. (2019) show that agricultural prices and State policies tend

the likelihood people will invest in the land, which would increase deforestation. However, this applies mostly when the land is used for economic purposes. Indigenous people in Brazil extract what they need for subsistence from the land and what they deforest is very little and this amount is not affected by titling. In the unlikely scenario this channel is present, our estimate will be conservative.

[4] It is possible that a given pixel is reclassified as forest when there is regeneration/reforestation. Throughout the paper I only consider deforestation of the original forest.

[5] https://www.gov.br/funai/pt-br/acesso-a-informacao/dados-abertos-1

[6] Urban centers are defined as agglomerations or isolated cities with more than 350000 people calculated using the 2000 Census.

to be the main drivers of the time trend in deforestation. I will discuss how this affects the empirical strategy in the next section. Figure 1 shows deforestation in the extensive margin only[7]. To have a sense of the area deforested, average deforestation per year is around 450 km^2. To put things in perspective, one football field has an area of .004 km^2.

Indigenous Lands. Figure 2 displays the map of the Brazilian Amazon overlaid with the indigenous lands. In blue are the lands that have been granted rights, and in red are the ones that had not been granted as of 2022. Most of the lands are blue, but I will explore the timing of titling in the empirical strategy. Also many of the lands are concentrated next to each other in clusters. In Section 4 I will discuss how this can be an issue and how I address it. Most of the indigenous lands are concentrated in the Amazon region, but there are also lands along the coast in the Southwest or on the Eastern coast (not shown). According to FUNAI, indigenous lands in the whole of Brazil make up 13.75% of the national territory.

Figure 4 shows the titling of indigenous lands per year. It varies year by year, with no discernible trend. One important confounder is the current government attention on indigenous peoples. As discussed in previous work such as Baragwanath and Bayi (2020), there is often differing views. For instance, President Jair Bolsonaro (2019-2022 term) publicly stated he did not believe indigenous communities and deforestation to be important, which resulted in no land being granted in that period. In contrast, President Fernando Collor (1990-1992 term) made an attempt to follow the demarcation requirement set by the recent Constitution. Some of the years have very few titled lands. I remove those with less than five titled lands from the empirical analysis, since they lead to imprecision in the estimation stage.

Market Access Variables. Figures 5-8 show maps of the rivers, ports, roads, railroads and urban centers used in the paper. Rivers, roads and railroads are usual means of transporting agricultural crops and cattle to markets across the country. Ports are used for exports to foreign markets. Urban centers have the biggest consumers so it is also included.

What predicts deforestation? Multiple studies have shown how geological or market access variables affect deforestation in different regions (Assunção et al., 2023; Busch and Ferretti-Gallon, 2017; Laurance et al., 2002). Table 1 shows the results of a logistic regression of deforestation on the following list of variables: Elevation, slope, distance from roads, railroads, rivers, ports and urban centers. The purpose is to check if the results from the

[7]This is the measure I use throughout the paper for a few reasons. First, using deforested area lowers the precision of the estimates because the data becomes very skewed. Given the data has many zeroes a log type transformation is not suitable here. Other common data transformations also tend to estimate a combination of intensive and extensive margins (Chen and Roth, 2022). Second, this is the relevant economic estimate for the mechanism being studied: the paper seeks to understand whether titling deters outsiders from encroaching lands for deforestation.

above studies replicate at the indigenous land level and in the time frame considered in this paper. Consistent with Busch and Ferretti-Gallon (2017) and Laurance et al. (2002), I find deforestation to be higher in places that are less elevated, closer to roads, ports and rails. This is also consistent with Assunção et al. (2023).[8] Busch and Ferretti-Gallon (2017) finds the evidence on rivers to be mixed. One reason suggested by Chomitz and Thomas (2003) is that despite areas closer to rivers being more accessible, in the Amazon they tend to have lower agricultural output and therefore could have less deforestation.[9] Another reason is that most of the deforestation in indigenous lands is devoted to cattle ranching (Mapbiomas, 2023b), which does not benefit from wet soil as opposed to agriculture. On that note, I find places further from rivers have more deforestation. Distance from urban centers is also ambiguous a priori since despite being more accessible, places closer to urban centers could mean a higher likelihood of getting caught. I also find distance to urban centers increases deforestation. The result on slope is puzzling given that the literature has generally found higher slope deters deforestation. However, this will not be a big concern since treatment groups are balanced in this variable as can be seen in Table 2 (and I also include it as a control variable).

Are titled and non-titled lands different? Table 2 displays the average value of a list of variables for 3 different groups: lands titled before 2000 (early treated), after 2000 (later treated) and lands that have not been titled as of 2020 (never treated). I include means and a p-value of a difference in means between group 1 and the others. Table 2 shows that compared to the later treated, the earlier treated lands have higher elevation, are further away from rivers and closer to roads. Compared to the never treated group, earlier treated are further from rails and urban centers. Combining this with the effect on deforestation as seen in Table 1, the sign of the ommited variable bias is unclear. Distance to roads and urban centers could be biasing the effect upward while elevation and distance to rails leads to downward bias. I will address this in the next section.

4 Empirical Strategy

4.1 Identification

The goal of the paper is to estimate the causal effect of titling on deforestation. A simple difference in means would not be suitable given endogeneity concerns. For instance, a reverse causality issue would occur if titled lands have more deforestation (decision makers could

[8]Despite not looking at each component individually, they compute a market access measure that includes ports and railroads and show through simulations that it increases deforestation.

[9]This may be counter-intuitive, but it reflects more nuanced factors like soil suitability.

have chosen lands under more threat of deforestation, for example) or an omitted variable bias if lands closer to roads are both titled more and have more deforestation (squatters settle in places easier to transport their output to markets). The descriptive statistics in Section 3.2 does indeed show treated units (in particular earlier treated) are closer to roads which relates to more deforestation. However as discussed above the tables also show that the earlier treated have higher elevation, so the bias could go in the other direction. I will address these concerns by exploiting the staggered roll-out of treatment using a generalized difference-in-differences (DiD) approach, where I compare the difference in deforestation between the treatment and control group before and after rights were granted.

In a panel data setting with staggered treatment timing, the literature often estimates an equation with the two-way fixed-effect (TWFE) form:

$$y_{t,i} = \alpha_i + \gamma_t + \beta D_{it} + \epsilon_{t,i} \tag{1}$$

where $y_{t,i}$ is the outcome variable for year t and land i, D_{it} is an indicator that equals 1 if land i is titled by year t, α_i indicates land fixed effects and γ_t year fixed effects.

This specification rules out various concerns, such as time-invariant factors (lands closer to roads have higher deforestation on average and are selected into titling) and factors that vary in time but affect lands similarly (a surge in agricultural prices could induce deforestation and coincide with years more lands were titled).

This strategy relies on the assumption of parallel trends (PT): the trend in the outcome for the treatment group had it not been treated is the same as the trend for the control. Combined with the assumptions of treatment effects being homogeneous across treated units and over time, no anticipation and no spillovers, the coefficient β is unbiased for the average treatment effect on the treated (ATT). I will address these assumptions in turn.[10]

Homogeneous effects. Goodman-Bacon (2021) shows that the ATT estimate obtained from a TWFE model is a weighted average of all $2x2$ DiD comparisons between groups of units treated at different times. In a staggered setting with heterogeneous effects across groups or time, some of these weights are negative and the TWFE estimator is not consistent for the ATT.[11] There are strong reasons to believe the homogeneity assumption is not true in my setting. For example, titling could have an effect only when enforcement is present and the latter might vary in time. Some recent papers have proposed estimators to fix this problem. For instance, Callaway and Sant'Anna (2021)(henceforth CS) propose a method that estimates group-time specific treatment effects and aggregates them using appropriate

[10]I address anticipation in the appendix.

[11]In essence, this happens since the estimator uses already treated units as controls for units treated later on.

weights, yielding a consistent estimate of the ATT given the same assumptions above but relaxing the homogeneity assumption. I will use their approach.[12]

Parallel trends. There could be concerns over the plausibility of the parallel trends assumption in my setting. For instance, lands treated in a given year could have been on different trends than control units. This could happen if lands were selected based not on deforestation levels, but trends. I will address this in two ways:

First, I will estimate a fully dynamic version of the CS estimator (i.e., an event-study plot) and check for pretrends. This is a common approach in the applied literature using DiD methods.[13] Second, I will include covariates. I will describe the choice of variables and how they ameliorate the issue in Section 4.3.

Spillovers. Given the spatial distribution of lands as seen in Figure 2, there might be concerns that titling makes squatters move to neighboring untitled lands, which would affect its deforestation path and therefore violate SUTVA. To address this I run specifications where neighbors are removed from the sample.[14]

4.2 Staggered DiD

The approach of Callaway and Sant'Anna (2021) goes as follows. Under the staggered versions of the parallel trends and no-anticipation assumptions, the ATT at time t for a given cohort treated at time g can be identified by comparing the difference between the expected change in outcome for cohort g between periods $g-1$ and t and that change for a control group not-yet treated at period t, $G_{comp} = \{g^{'} : g^{'} > t\}$:

$$ATT(g,t) = E[Y_{i,t} - Y_{i,g-1} | G_i = g] - E[Y_{i,t} - Y_{i,g-1} | G_i \in G_{comp}] \tag{2}$$

The $ATT(g,t)$ can then be estimated by replacing expectations with their sample analogs:

[12] Other methods include de Chaisemartin and D'Haultfœuille (2022), Sun and Abraham (2021), Borusyak et al. (2024) and Wooldridge (2022). The first two have a similar approach but aggregate group-time ATTs using different weights. Borusyak et al. (2024) uses an imputation approach while Wooldridge (2022) saturates the TWFE model with various interactive terms. They all rely on slightly different assumptions, and a discussion of the pros and cons of each method is beyond the scope of this paper. I chose CS for a few reasons: its simplicity, computational efficiency and the ability to incorporate covariates in a transparent manner and use an efficient estimator (Sant'Anna and Zhao, 2020). In a previous version of the paper I ran all of the above methods and found similar results.

[13] See Braghieri et al. (2022) for a recent example also in a staggered setting.

[14] i.e., for all treated lands starting with the earliest ones I remove lands that share a border with them (in practice I set a 500 meter bandwidth). I choose this approach instead of a higher distance threshold for a few reasons: there are many clusters of lands so this already drops around 20% of the sample; lands that do not share a border tend to be far away from each other or have a river between them, so spillovers are unlikely.

$$\widehat{ATT}(g,t) = \frac{1}{N_g} \sum_{i:G_i=g} [Y_{i,t} - Y_{i,g-1}] - \frac{1}{N_{comp}} \sum_{i:G_i \in G_{comp}} [Y_{i,t} - Y_{i,g-1}]. \tag{3}$$

With many periods and treatment years reporting all $ATT(g,t)$ may be cumbersome and each one imprecisely estimated. One can instead choose to aggregate them in many ways, such as by calendar year, time since treatment (also called event-study) or into an overall ATT estimate.

4.3 Doubly-Robust DiD

As discussed in Section 3.2, Table 2 shows there are some differences in baseline variables between treated and non-treated units or earlier and later treated units. A DiD approach addresses the issue of potential differences in deforestation levels between treated and control groups, but not differences in trends. For instance, it could be the case that an increase in agricultural prices rises deforestation but only in places close to roads, where transporting agricultural output is viable. This shock then leads to differing trends between areas with varying distance to roads. Since I have data on distance to roads, I can control for it in the estimation.

The identification assumption then becomes conditional PT (Callaway and Sant'Anna, 2021), i.e. trends need to be the same conditional on the covariates. In order to estimate the ATT, I employ a Doubly-Robust Difference-in-Differences (DRDID) approach from Sant'Anna and Zhao (2020). DR methods are more robust to functional form misspecification.[15] There is an extensive literature on predictors of deforestation (e.g., see Busch and Ferretti-Gallon (2017); Laurance et al. (2002) for meta-studies of such papers). Variables are often divided into 3 types: biophysical characteristics such as elevation and slope; market access/infrastructure measures such as distance to roads, rivers, ports and urban centers; and demographic/socioeconomic characteristics like population, income and agricultural prices. I include all covariates with the exception of the latter type, given its endogenous and time-varying nature. Inference is done using the multiplier bootstrap procedure of CS.

[15] DR methods work by combining the propensity score approach (which estimates the probability of treatment using the covariates and then weights observations by the inverse of that probability) and an outcome regression approach. Either one by itself yields an unbiased estimate of the ATT under unconfoundness but it has been shown they can suffer from bias in finite samples if the model is misspecified. The new approach is doubly-robust in the sense that by combining both, only one needs to be properly specified for the bias to disappear. I use logistic regression to estimate the propensity score and OLS for the outcome regression.

5 Baseline Results

Figure 9 plots the point estimate and 95% confidence interval of the ATT of titling on deforestation for various specifications. The first specification is CS with the full sample and no covariates. The second and fourth have neighbors removed from the sample and the third and fourth include controls. The point estimates are all negative, suggesting a negative effect of titling on deforestation across all models. Confidence intervals are large, but contain mostly negative values.

The estimates become more negative when controls are included, suggesting we had an upward bias (e.g., places closer to roads with deforestation increasing faster than in other areas). There is also a decrease in the coefficient when neighbors are removed. This is due to the staggered timing of treatment. In a static setting, spillovers are likely to affect the path of the control groups, leading to an upward bias. However, in my setting the spillovers can go to units who are later treated leading to a downward bias. In essence the later the units affected are treated, the 'more downward' the bias. The coefficient being lower when neighbors are removed is evidence that the affected units are treated sooner rather than later, which is intuitive given Figure 4: the number of treated lands decreases over time. I use the specification with neighbors removed and with controls for the remainder of the paper.

In order to shed more light in the temporal pattern of effects, Figures 10, 11 and 12 show the effects aggregated by years since treatment, treatment year and calendar year respectively. As expected from the temporal pattern of the policy displayed in Figure 4, estimates tend to be noisy so confidence intervals include zero. It is however reassuring that in Figure 10 there is no evidence of pre-trends and estimates become more negative post-treatment. As shown in the next section, the relevant decomposition of effects is spatial as opposed to temporal.

6 Heterogeneity

It is paramount to understand not only whether the policy had an overall impact, but whether the decrease in deforestation is restricted to or is higher in certain areas. In this section, I do a heterogeneity analysis on distance to markets. Titling may have a stronger impact in areas where transportation infrastructure is more accessible or the threat of deforestation is high. Figure 13a (a)-(e) shows, for a list of variables, the ATT estimated separately for subsamples split by the median. The list includes distance to roads, rivers, ports, rails and urban centers.[16] The effect is significantly stronger in places further from ports. To understand this result, it is useful to dig deeper into the geography. Looking at the map in Figure 5, the area furthest from ports is the Southeast portion of the Amazon. This coincides with the area where deforestation is higher as seen in Figure 3. This is a region known as the 'Arc of Deforestation', and indigenous lands located across the arc are considered to be under higher threat of invasion and subsequent deforestation. In the last panel of Figure 13a I plot the effect for lands in this region[17] and outside. The ATT is indeed stronger in the area under more threat of deforestation.

7 Discussion

Takeaways. Titling is effective in curbing deforestation by deterring outsiders from encroaching indigenous lands and exploiting them for economic gain. The effect is stronger in areas under higher threat. Since these areas also have more enforcement (Assunção et al., 2015), this is suggestive evidence that titling is more effective when enforcement is present. This result yields the recommendation that policymakers in Brazil keep titling lands, but that titling should be coupled with enforcement in order to deter deforestation. Despite the emphasis on the importance of enforcement, this does not imply titling should happen only in areas where enforcement is viable or easier, since there could be other benefits to titling. For instance, titling can empower indigenous peoples and make them feel more represented. This can increase their political participation or draw the public's attention, leading to more favorable policies in the future.

The result is unlikely to be limited to the region studied here. Similar policies have been implemented in places like Peru (Blackman et al., 2017), Colombia (Bonilla-Mejía and Higuera-Mendieta, 2019) and Nicaragua (Liscow, 2013). These countries also have native communities. The result in this paper may help inform how and where to title lands in these and other countries.

Land deforested has economic value. Most of that land is used for cattle ranching, which leads to meat sold in markets nationally or internationally. Producers might move from indigenous lands to non-indigenous after titling, leading to a second-best production output. Quantifying this reduction in value is beyond the scope of this paper. However, we can have an estimate of the damage done to the environment. To put things in perspective, we can take the lands titled in 1991 (the largest year) as an example: the average value of instances of deforestation post-1991 for these lands is .87. This means that in a given year, a land has

[16] Due to lack of power, heterogeneity by elevation and slope cannot be computed.

[17] To be specific, in the states of Para, Tocantins, Mato Grosso and Maranhao.

87% chance of having any deforestation. Without titling, it would have had 94%. Average deforested area (conditional on any deforestation) post-1991 for these lands is 107 hectares. This means titling reduced deforested area by 107*.07=7.49 hectares, around 13 football fields, per year.

Validity. There might still be concerns of PT being violated despite the inclusion of covariates. In particular, Figure 10 shows that there is a slight upward trend (but non-significant) in the difference in outcomes for the pre-period. This is not a big cause of concern for a few reasons. First, the confidence intervals are large so I cannot reject the null hypothesis that the lead coefficients are all equal to zero. The deforestation data is noisy and for any given year the number of treated units is quite low (often below 20). Despite this, I still find a significant effect for the overall ATT. Second, if treated units are in a higher trend than the controls, the estimated ATT would be upward biased. In this case, my estimated coefficient is a lower bound on the true ATT. Third, an unobservable confounder would have to affect not just the level of deforestation but the trend. The latter is less likely to hold: if squatters choose a place based on convenience, that place will have more deforestation, but it is less likely they will deforest more and more each year. The variation year-to-year is mostly due to shocks like a surge in agricultural prices or how the current government prioritizes forest preservation (Burgess et al., 2019). Fourth, the literature on deforestation is extensive and to the best of my knowledge I included the most important predictors. Fifth, the unobservable's effect only biases the estimate insofar its effect is not correlated with other included covariates. This is especially relevant given the previous point.

8 Conclusion

Deforestation is a growing issue in the world, and often affects indigenous communities living in threatened regions. In this paper I leverage the staggered titling of indigenous lands in the Brazilian Amazon to estimate its impact on curbing deforestation in the region and I find titling to be effective. In addition, I show the effect to be limited to areas under high threat. My results have implications for policymakers in Brazil and elsewhere.

Avenues for future research include investigating what happens to squatters who are deterred from deforesting in indigenous lands. They could lose economic value from the output and change occupation. Another route for inquiry is going beyond indigenous lands and the Amazon. Given the unique cultural values of indigenous people in the region, titling lands to farmers is likely to have different effects.

References

Abman, Ryan and Clark Lundberg, "Does Free Trade Increase Deforestation? The Effects of Regional Trade Agreements," *Journal of the Association of Environmental and Resource Economists*, January 2020, *7* (1), 35–72.

Alesina, Alberto, Caterina Gennaioli, and Stefania Lovo, "Public goods and ethnic diversity: Evidence from deforestation in Indonesia," *Economica*, January 2019, *86* (341), 32–66.

Andam, Kwaw S, Paul J Ferraro, Alexander Pfaff, G Arturo Sanchez-Azofeifa, and Juan A Robalino, "Measuring the effectiveness of protected area networks in reducing deforestation," *Proc. Natl. Acad. Sci. U. S. A.*, October 2008, *105* (42), 16089–16094.

Araujo, Claudio, Catherine Araujo Bonjean, Jean-Louis Combes, Pascale Combes Motel, and Eustaquio J Reis, "Property rights and deforestation in the Brazilian Amazon," *Ecol. Econ.*, June 2009, *68* (8), 2461–2468.

Araújo, R, J Assunção, and Arthur Bragança, "The effects of transportation infrastructure on deforestation in the Amazon: A general equilibrium approach," April 2023.

Assunção, Juliano, Clarissa Gandour, and Romero Rocha, "DETER-ing Deforestation in the Amazon: Environmental Monitoring and Law Enforcement," *Am. Econ. J. Appl. Econ.*, April 2023, *15* (2), 125–156.

_ , _ , and Rudi Rocha, "Deforestation slowdown in the Brazilian Amazon: prices or policies?," *Environ. Dev. Econ.*, December 2015, *20* (6), 697–722.

Balboni, Clare, Aaron Berman, Robin Burgess, and Benjamin A Olken, "The Economics of Tropical Deforestation," *Annu. Rev. Econom.*, September 2023, *15* (1), 723–754.

_ , Robin Burgess, Anton Heil, Jonathan Old, and Benjamin A Olken, "Cycles of Fire? Politics and Forest Burning in Indonesia," *AEA Papers and Proceedings*, May 2021, *111*, 415–419.

Baragwanath, Kathryn and Ella Bayi, "Collective property rights reduce deforestation in the Brazilian Amazon," *Proc. Natl. Acad. Sci. U. S. A.*, August 2020, *117* (34), 20495–20502.

BenYishay, Ariel, Silke Heuser, Daniel Runfola, and Rachel Trichler, "Indigenous land rights and deforestation: Evidence from the Brazilian Amazon," *J. Environ. Econ. Manage.*, November 2017, *86*, 29–47.

Benzeev, Rayna, Sam Zhang, Marcelo Artur Rauber, Eric A Vance, and Peter Newton, "Formalizing tenure of Indigenous lands improved forest outcomes in the Atlantic Forest of Brazil," *PNAS Nexus*, January 2023, *2* (1), gac287.

Berman, Ron and Ayelet Israeli, "The Value of Descriptive Analytics: Evidence from Online Retailers," *Marketing Science*, November 2022, *41* (6), 1074–1096.

Blackman, Allen, Leonardo Corral, Eirivelthon Santos Lima, and Gregory P Asner, "Titling indigenous communities protects forests in the Peruvian Amazon," *Proc. Natl. Acad. Sci. U. S. A.*, April 2017, *114* (16), 4123–4128.

Bonilla-Mejía, Leonardo and Iván Higuera-Mendieta, "Protected Areas under Weak Institutions: Evidence from Colombia," *World Dev.*, October 2019, *122*, 585–596.

Borusyak, Kirill, Xavier Jaravel, and Jann Spiess, "Revisiting Event-Study Designs: Robust and Efficient Estimation," *Rev. Econ. Stud.*, 2024.

Braghieri, Luca, Ro'ee Levy, and Alexey Makarin, "Social Media and Mental Health," *Am. Econ. Rev.*, November 2022, *112* (11), 3660–3693.

Burgess, Robin, Francisco J M Costa, and Ben Olken, "The Brazilian Amazon's Double Reversal of Fortune," August 2019.

_ , Matthew Hansen, Benjamin A Olken, Peter Potapov, and Stefanie Sieber, "The Political Economy of Deforestation in the Tropics*," *Q. J. Econ.*, November 2012, *127* (4), 1707–1754.

Busch, Jonah and Kalifi Ferretti-Gallon, "What Drives Deforestation and What Stops It? A Meta-Analysis," *Review of Environmental Economics and Policy*, January 2017, *11* (1), 3–23.

Callaway, Brantly and Pedro H C Sant'Anna, "Difference-in-Differences with multiple time periods," *J. Econom.*, December 2021, *225* (2), 200–230.

Carreira, Igor, Francisco J M Costa, and Joao P Pessoa, "The Deforestation Effects of Trade and Agricultural Productivity in Brazil," June 2022.

Chen, Jiafeng and Jonathan Roth, "Logs with zeros? Some problems and solutions," December 2022.

Chomitz, Kenneth M and Timothy S Thomas, "Determinants of land use in Amazônia: A fine-scale spatial analysis," *Am. J. Agric. Econ.*, November 2003, *85* (4), 1016–1028.

Cisneros, Elías, Krisztina Kis-Katos, and Nunung Nuryartono, "Palm oil and the politics of deforestation in Indonesia," *J. Environ. Econ. Manage.*, July 2021, *108*, 102453.

de Chaisemartin, Clément and Xavier D'Haultfœuille, "Two-way fixed effects and differences-in-differences with heterogeneous treatment effects: a survey," *Econom. J.*, June 2022, *26* (3), C1–C30.

Gatti, Luciana V, Luana S Basso, John B Miller, Manuel Gloor, Lucas Gatti Domingues, Henrique L G Cassol, Graciela Tejada, Luiz E O C Aragão, Carlos Nobre, Wouter Peters, Luciano Marani, Egidio Arai, Alber H Sanches, Sergio M Corrêa, Liana Anderson, Celso Von Randow, Caio S C Correia, Stephane P Crispim, and Raiane A L Neves, "Amazonia as a carbon source linked to deforestation and climate change," *Nature*, July 2021, *595* (7867), 388–393.

Goodman-Bacon, Andrew, "Difference-in-differences with variation in treatment timing," *J. Econom.*, December 2021, *225* (2), 254–277.

Hargrave, Jorge and Krisztina Kis-Katos, "Economic Causes of Deforestation in the Brazilian Amazon: A Panel Data Analysis for the 2000s," *Environ. Resour. Econ.*, April 2013, *54* (4), 471–494.

Kline, Patrick and Enrico Moretti, "100 YEARS OF EVIDENCE FROM THE TENNESSEE VALLEY AUTHORITY," *Q. J. Econ.*, 2014, *129* (1), 275–332.

Laurance, William F, Ana K M Albernaz, Götz Schroth, Philip M Fearnside, Scott Bergen, Eduardo M Venticinque, and Carlos Da Costa, "Predictors of deforestation in the Brazilian Amazon," *J. Biogeogr.*, May 2002, *29* (5-6), 737–748.

Lawrence, Deborah and Karen Vandecar, "Effects of tropical deforestation on climate and agriculture," *Nat. Clim. Chang.*, December 2014, *5* (1), 27–36.

Liscow, Zachary D, "Do property rights promote investment but cause deforestation? Quasi-experimental evidence from Nicaragua," *J. Environ. Econ. Manage.*, March 2013, *65* (2), 241–261.

Mapbiomas, "As florestas do Brasil Colecao 8.0," 2023.

_ , "MAPEAMENTO ANUAL DE COBERTURA E USO DA TERRA NO BRASIL ENTRE 1985 A 2022-Colecao 8.0," 2023.

Marchand, Sébastien, "The colonial origins of deforestation: an institutional analysis," *Environ. Dev. Econ.*, June 2016, *21* (3), 318–349.

Pailler, Sharon, "Re-election incentives and deforestation cycles in the Brazilian Amazon," *J. Environ. Econ. Manage.*, March 2018, *88*, 345–365.

Pfaff, Alexander, Juan Robalino, Diego Herrera, and Catalina Sandoval, "Protected Areas' Impacts on Brazilian Amazon Deforestation: Examining Conservation-Development Interactions to Inform Planning," *PLoS One*, July 2015, *10* (7), e0129460.

Roth, Jonathan, "Pretest with Caution: Event-Study Estimates after Testing for Parallel Trends," *American Economic Review: Insights*, September 2022, *4* (3), 305–322.

Sant'Anna, Pedro H C and Jun Zhao, "Doubly robust difference-in-differences estimators," *J. Econom.*, November 2020, *219* (1), 101–122.

Simonet, Gabriela, Julie Subervie, Driss Ezzine de Blas, Marina Cromberg, and Amy E Duchelle, "Effectiveness of a REDD+ project in reducing deforestation in the Brazilian Amazon," *Am. J. Agric. Econ.*, January 2019, *101* (1), 211–229.

Soares-Filho, Britaldo, Paulo Moutinho, Daniel Nepstad, Anthony Anderson, Hermann Rodrigues, Ricardo Garcia, Laura Dietzsch, Frank Merry, Maria Bowman, Letícia Hissa, Rafaella Silvestrini, and Cláudio Maretti, "Role of Brazilian Amazon protected areas in climate change mitigation," *Proceedings of the National Academy of Sciences*, 2010, *107* (24), 10821–10826.

Sun, Liyang and Sarah Abraham, "Estimating dynamic treatment effects in event studies with heterogeneous treatment effects," *J. Econom.*, December 2021, *225* (2), 175–199.

Wooldridge, Jeffrey M, "Simple approaches to nonlinear difference-in-differences with panel data," *SSRN Electron. J.*, 2022.

9 Appendix

9.1 Anticipation

There might be a concern of anticipation from the step previous to the Presidential decree. For instance, for lands that had their boundaries recognized by the Minister of Justice squatters could anticipate they will be titled soon and therefore increase deforestation.

One thing to note is the main specification is looking at the extensive margin, while this effect will likely go through the intensive. Also, despite the average number of years between the last two steps being 4 years, there is a lot of variation.

Figure 14 plots the effect of the delimitation year on deforestation and finds no effect.

Figures and Tables

Table 1: Deforestation Predictors

Variable	**Coefficient**	**P-value**
Elevation (m)	-0.5079	< 0.001
Slope (%)	0.8414	< 0.001
Distance to Road (km)	-0.4128	< 0.001
Distance to River (km)	0.4178	< 0.001
Distance to Port (km)	-0.7291	< 0.001
Distance to Rail (km)	-1.2115	< 0.001
Distance to Urban Areas (km)	2.0044	< 0.001
Number of Observations: 11340		

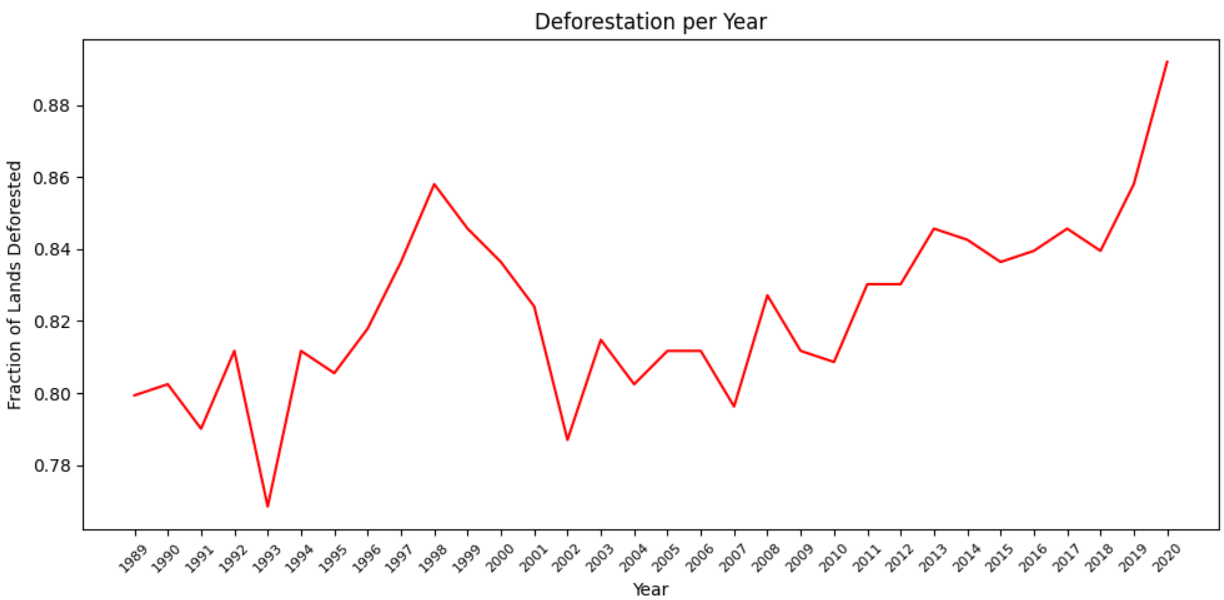
Notes: This table presents the results of a logistic regression of deforestation instance (dummy if there is any deforestation in a given land-year) on variables commonly found to be predictors. Distances are calculated as the minimum distance from any point in a given land to any point in the variable in question. Roads and ports are as of 1990 and urban centers is measured in 2000.

Table 2: Summary Statistics by Treatment Group

Variable	($T < 2000$) Mean	($T >= 2000$) Mean	(Never Treated) Mean	**p-value** 1-2	**p-value** 1-3
Elevation (m)	197.88	143.02	190.07	0.00	0.77
Slope (%)	0.30	0.25	0.40	0.28	0.37
Distance to River (km)	61.27	45.40	44.15	0.06	0.13
Distance to Road (km)	190.81	231.77	220.97	0.08	0.36
Distance to Port (km)	517.52	462.73	475.65	0.11	0.44
Distance to Rail (km)	730.95	741.23	489.98	0.86	0.00
Distance to Urban (km)	572.24	549.47	426.17	0.59	0.01
Baseline Deforestation	0.83	0.77	0.79	0.23	0.49
Number of Observations	178	104	42	—	

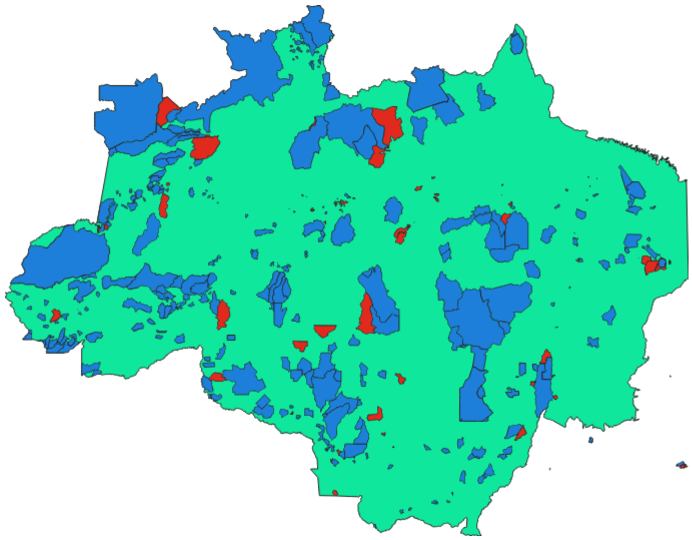
Notes: This table presents land-level summary statistics by groups created from titling year. In particular, lands titled before 2000, after 2000 and never titled. Distances are calculated as the minimum distance from any point in a given land to any point in the variable in question. Roads and ports are as of 1990 and urban centers is measured in 2000. Baseline deforestation is the average number of lands that had **any** deforestation across years 1987 and 1988. The p-value is from a t-test of difference in means between groups.

Figure 1: Deforestation Trend



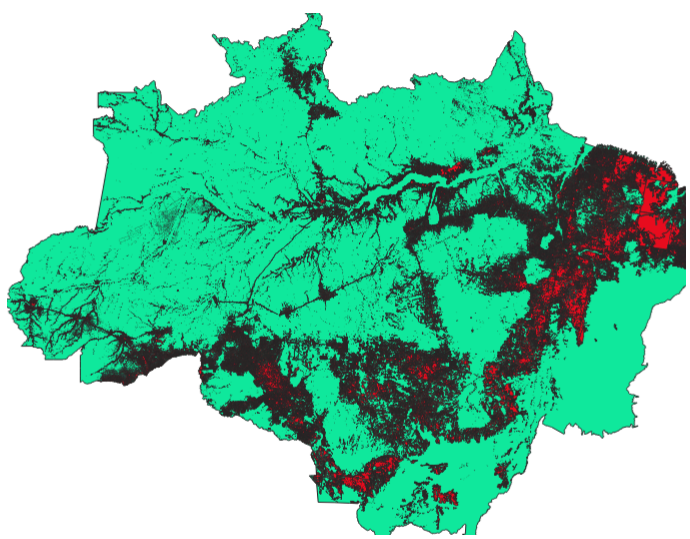
Notes: Fraction of indigenous lands that had any deforestation per year. A land is considered to have had an instance of deforestation if we observe any pixel deforested within its land boundaries.

Figure 2: Indigenous Lands in the Brazilian Amazon



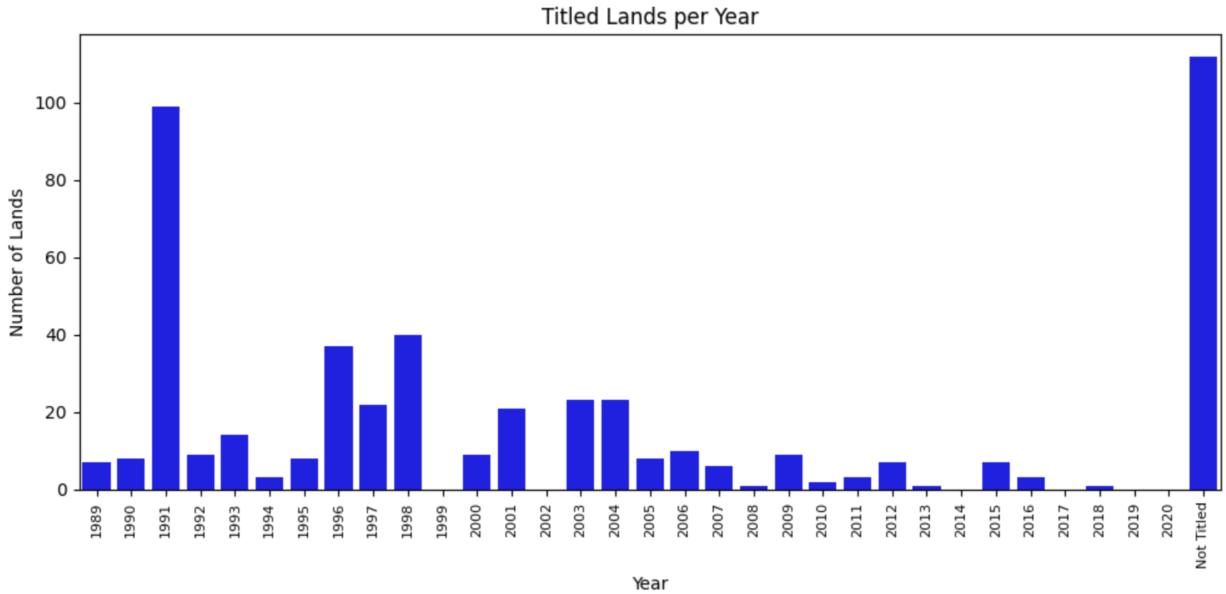
Notes: This is a map of all (that have been studied and catalogued so far) indigenous lands in the Brazilian Amazon region. In blue are lands that have already been titled, in red are the ones that have not as of 2022.

Figure 3: Deforestation in the Brazilian Amazon



Notes: This is a map of accumulated deforested area in the Brazilian Amazon by 2007 compiled by PRODES, the Basin Restoration Program managed by Brazil's national water agency.

Figure 4: Titling Policy



Notes: This displays number of titled indigenous lands per year.

Figure 5: Rivers and Ports

Notes: This displays rivers and ports circa 1990.

Figure 6: Roads

Notes: This displays paved roads circa 1990.

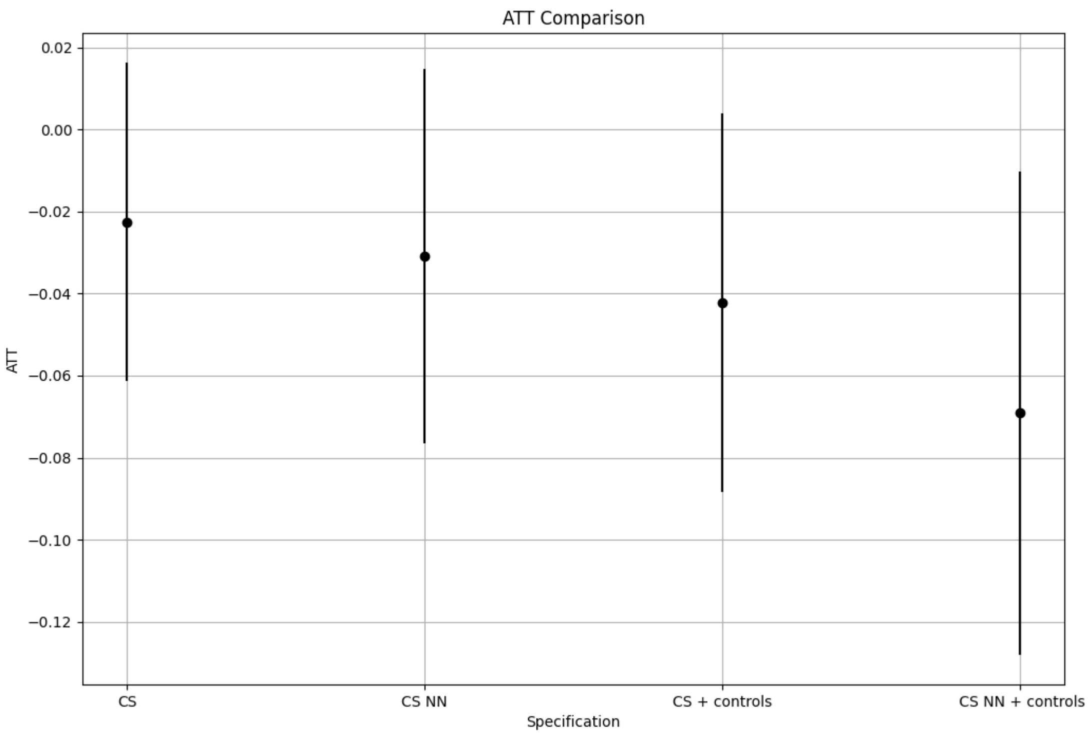
Figure 7: Railroads

Notes: This displays railroads circa 1990.

Figure 8: Urban Centers

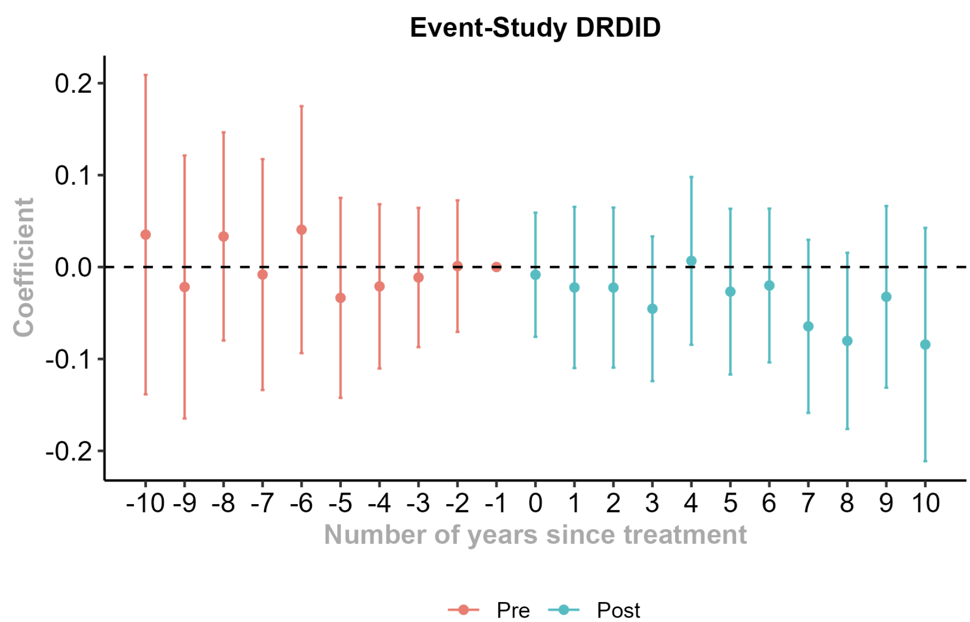
Notes: This displays urban centers circa 2000.

Figure 9: Effect of Titling on Deforestation - Overall Effect



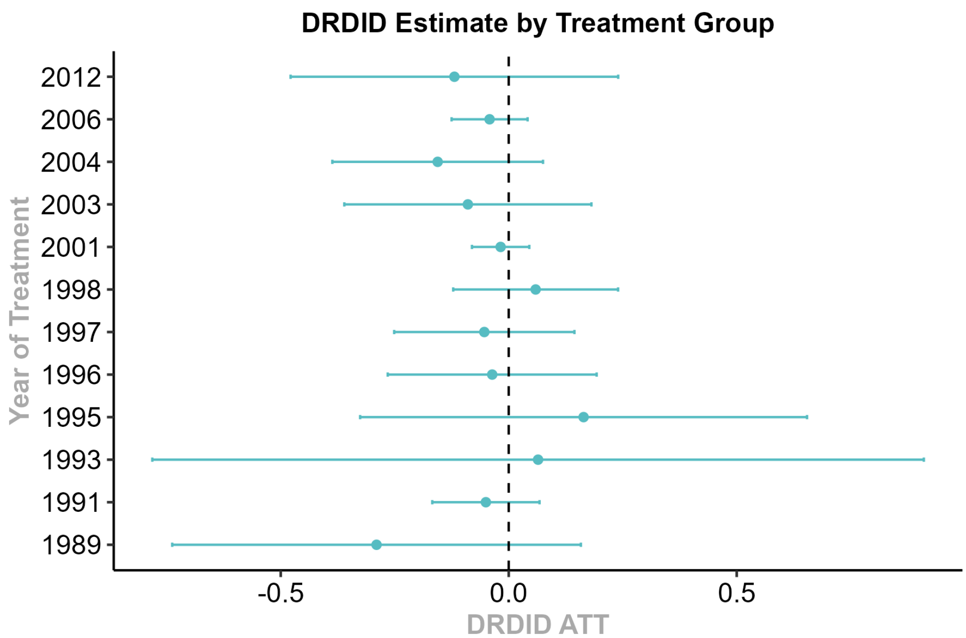
Notes: This figure plots the coefficient for the overall ATT of titling on deforestation instance using the Callaway and Sant'Anna (2021) model. The estimate is in percentage points. The first and third are the CS model with no covariates, the second and fourth include covariates and are estimated using Doubly-Robust methods as described in Section 4.3. Estimates 3 and 4 are computed by removing neighbors from the sample to mitigate spillovers. 95% confidence intervals are displayed. Standard errors are computed by bootstrap with 1000 replications.

Figure 10: Effect of Titling on Deforestation - Event Study



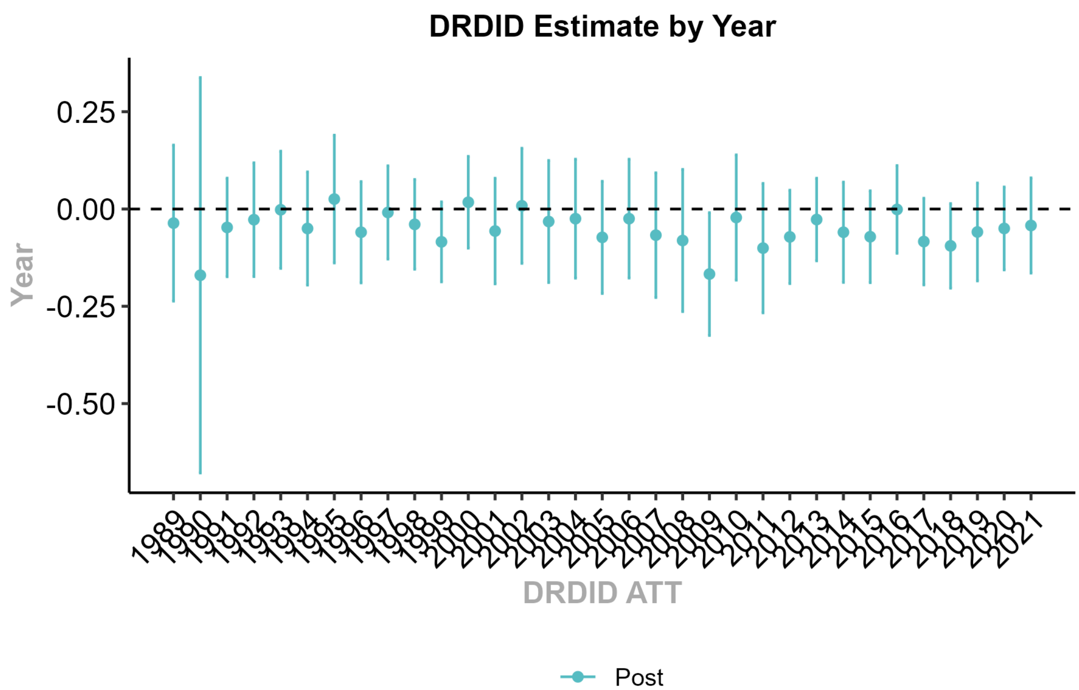
Notes: This is an event-study plot of the effect of titling on deforestation instance using the Doubly-Robust implementation of the Callaway and Sant'Anna (2021) model with covariates and removing neighbors from the sample to avoid spillover concerns. The estimate is in percentage points. 95% confidence intervals are displayed. Standard errors are computed by bootstrap with 1000 replications.

Figure 11: Effect of Titling on Deforestation - by Year of Treatment



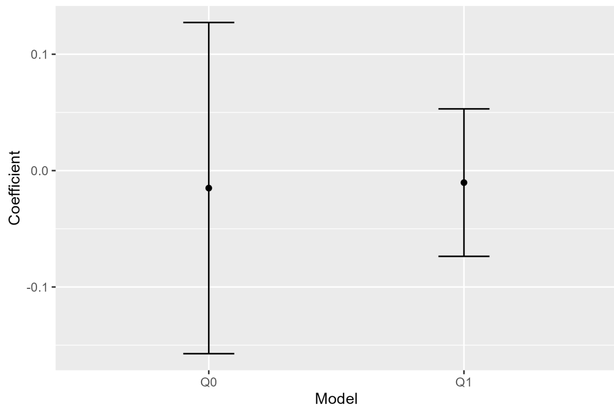
Notes: This displays the ATT separately by year of treatment. ATT is estimated using the Doubly-Robust implementation of the Callaway and Sant'Anna (2021) model with covariates and removing neighbors from the sample to avoid spillover concerns. The estimate is in percentage points. 95% confidence intervals are displayed. Standard errors are computed by bootstrap with 1000 replications.

Figure 12: Effect of Titling on Deforestation - by Calendar Year

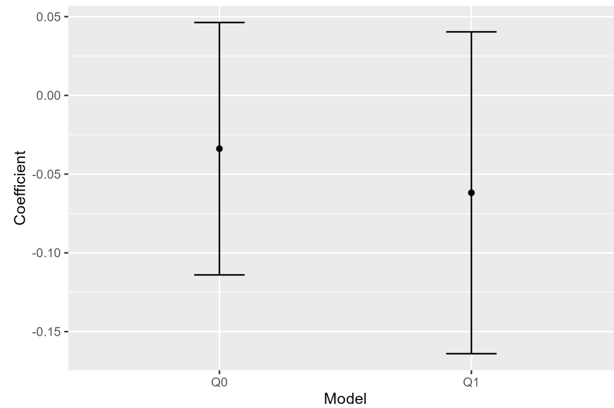


Notes: This displays the ATT separately by year. ATT is estimated using the Doubly-Robust implementation of the Callaway and Sant'Anna (2021) model with covariates and removing neighbors from the sample to avoid spillover concerns. The estimate is in percentage points. 95% confidence intervals are displayed. Standard errors are computed by bootstrap with 1000 replications.

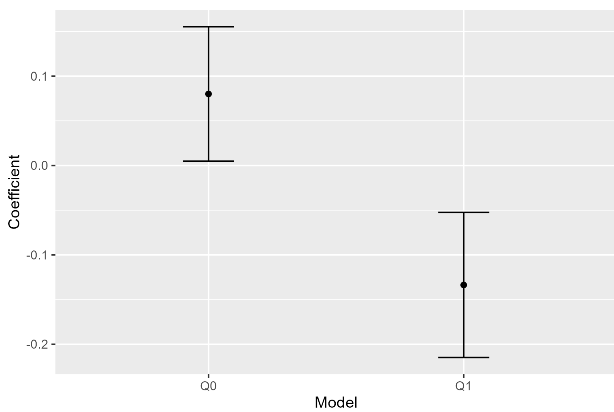
Figure 13: Heterogeneous Treatment Effects for Market Access Variables



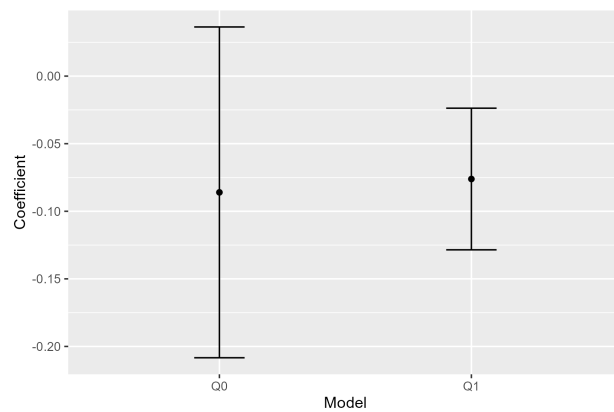
(a) Roads



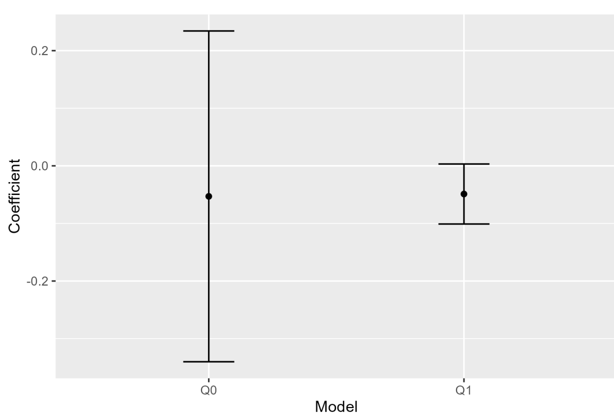
(b) Rivers



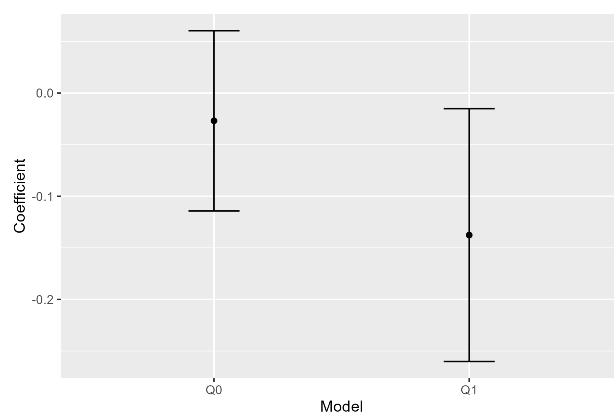
(c) Ports



(d) Railroads



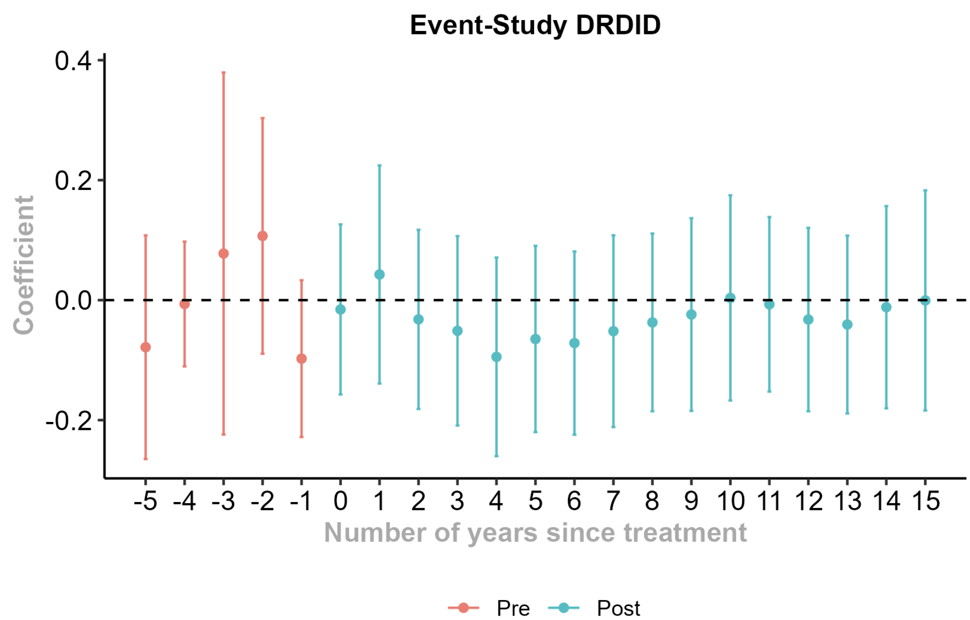
(e) Urban Center



(f) Threat

Notes: This displays the ATT separately for units with values below (Q0) or above (Q1) the median for different variables. ATT is estimated using the Doubly-Robust implementation of the Callaway and Sant'Anna (2021) model with covariates, removing neighbors from the sample to avoid spillover concerns. The estimate is in percentage points. 95% confidence intervals are displayed. Standard errors are computed by bootstrap with 1000 replications.

Figure 14: Event-Study of Delimitation on Deforestation



Notes: This is an event-study plot of the effect of delimitation on deforestation instance using the Doubly-Robust implementation of the Callaway and Sant'Anna (2021) model with covariates and removing neighbors from the sample to avoid spillover concerns. The estimate is in percentage points. 95% confidence intervals are displayed. Standard errors are computed by bootstrap with 1000 replications.