Econometrics for environmental policy evaluation: Analysis of the "Priority Municipalities" policy in the Brazilian Amazon.

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Dissertation presented to the Programa de Pós-Graduação em Economia (PPGECO), Departamento de Economia da UnB in partial fulfilment of the requirements for the degree of Master in Economics

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Brasilia, Brasil

Setembro de 2022

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Local
Mês e Ano da Defesa

FICHA CATALOGRÁFICA

G635e

Fernandes Gonçalves, Rodrigo

Econometrics for environmental policy evaluation: Analysis of the " Priority Municipalities" policy in the Brazilian Amazon. / Rodrigo Fernandes Gonçalves; orientador Marcelo de Oliveira Torres; – Brasília, 2022.

53 p.

Dissertação (Mestrado - Mestrado em Economia) – Universidade de Brasília, 2022.

1. Environmental Public Policy. 2. Econometrics. 3. Staggered Difference-in-Difference. 4. Propensity Score Matching. I. de Oliveira Torres, Marcelo, orient. II. Título.

十年树木，百年树人.

管仲

Acknowledgements

Já agradeci todos na minha monografia então não vou me repetir muito. Na minha tese de doutorado eu falo novamente e de forma mais completa... Mas de qualquer forma, tenho que agradecer minha família. Especialmente, em ordem alfabética, Andréa, Arthur, Ed e Lk. Ainda, agradeço Adilson, Juba e Marina. Não posso esquecer dos meus parceiros de gameplay JP, Delta, Mali e Naim. Todos me ajudaram muito nessa jornada.

Gostaria de agradecer também aos professores da UnB que me auxiliaram e ensinaram através de uma tela, em um momento pandêmico. Principalmente, agradecer ao Marcelo Torres pelas ideias para dissertação e auxílio fora dela. Também gostaria de agradecer ao Rafael Terra pelos conselhos. Tomas e Marina pelos ensinamentos de macroeconomia. Ainda, esse trabalho e todo conhecimento obtido durante esses anos não seriam possíveis sem o apoio do povo brasileiro e a CAPES.

Também gostaria de agradecer aos artistas que me ajudam a sobreviver e crescer como ser humano, mesmo sem eles saberem. São centenas de bandas, diretores e escritores que me fazem perceber como angústias são comuns, o que nos fazem especiais, mas não únicos.

Finalmente, meu maior agradecimento é para todos os homens e mulheres que dedicaram, dedicam e dedicarão suas vidas em defesa de um futuro para todos, buscando defender a natureza, e o ser humano como parte integral dessa natureza, do ímpeto colossal do capital, suas bifurcações e suas vontades inexoráveis.

ABSTRACT

The present work intends to understand if the Brazilian policy of blacklisting municipalities in the Amazon region was effective when it was created in 2008, and how it evolved over the years, until 2019. To this end, we applied econometrics methods. For the first question, we chose difference-in-differences with propensity score matching, with deforestation increase divided by municipal area as our dependent variable. We found that by controlling for covariates and using fixed effects, the policy was effective to decrease deforestation. Specifically, when listed, municipalities decreased deforestation per km^2 by around 0.003. For the second question, we used difference-in-differences again, but with staggered treatment. We found again that the policy was successful in 2008 but also in 2009, 2011, and 2012, still its effect decreased over time. As of 2016, the policy was no longer efficient and for the 2018 group, being listed was associated with greater deforestation. Additionally, for robustness, we answered both questions with different dependent variables, precisely: normalized and log-transformed deforestation increase. Even then, the policy seems to be useful. Next, we tried to explore the reasons behind the policy change. Our findings suggest that political alliances and the resource allocation focused on environmental conservation impacted the policy. Finally, we state some flaws and limitations of this paper, highlighting opportunities for future studies and practical recommendations.

Keywords: Environmental Public Policy, Econometrics, Staggered Difference-in-Difference, Propensity Score Matching

RESUMO

O presente trabalho pretende entender se a política brasileira de listagem de municípios na região amazônica foi eficiente quando criada em 2008, e como ela evoluiu ao longo dos anos, até 2019. Para tal, aplicamos métodos econométricos. Para a primeira questão, utilizamos o método de diferença-em-diferenças com propensity score matching, com o aumento do desmatamento dividido pela área do município como nossa variável dependente. Descobrimos que ao controlar por covariáveis e usar efeitos fixos, a política foi eficiente para diminuir o desmatamento. Especificamente, quando listados, os municípios diminuíram o desmatamento por km^2 em cerca de 0,003. Para a questão seguinte, utilizamos novamente diferença-em-diferenças, mas com tratamento escalonado. Verificamos novamente que a política foi bem-sucedida em 2008, mas também em 2009, 2011 e 2012. Seu efeito diminuiu ao longo do tempo e, a partir de 2016, a política deixou de ser eficiente. Para o grupo de 2018, estar listado foi associado a um maior desmatamento. Em busca de maior robustez, também respondemos as duas perguntas com diferentes variáveis dependentes, precisamente aumento de desmatamento normalizado e log transformado. Os resultados, ainda assim, indicam que a política funcionou. Em seguida, tentamos explorar as razões por trás da mudança de política. Nossos achados sugerem que as alianças políticas e a diminuição de recursos voltados para a conservação ambiental impactaram a política. Por fim, apontamos algumas falhas e limitações do artigo, indicando oportunidades para estudos futuros e recomendações práticas.

Palavras-chave: Políticas Públicas Ambientais, Econometria, Diferença em Diferença Escalonada, Propensity Score

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Chapter 1

Introduction

Since the 80s, the Amazon rainforest's importance has grown in the social imaginary despite its relevance being always known to local inhabitants. For instance, the environmentalist Chico Mendes fought for it through the military dictatorship, during the 70s. However, its significance to the world and for Brazilians outside the region was built mainly after climate conferences (such as Eco-92) and with the expansion of the scientific knowledge related to the environment, even though the environmental issue started to have visibility after the UN conference in Stockholm, 1972 (Sanne, 2002).

Nowadays, it is well established why we should conserve the Amazon rainforest. Being it alone half of the planet's remaining tropical forests, and home for thousands of plant and animal species, about 60% of the Amazon rainforest is within Brazil, with more than 5 million km^2 (FAO, 2011). In Brazil, the region that comprises all the states in which the forest is present is called Legal Amazon (*Amazônia Legal*). Although most of the Legal Amazon is composed by the Amazonian forest, its limits also encompass 20% of the Cerrado biome and part of the Pantanal in Mato Grosso [1].

As studies indicate, conservation of the Amazon rainforest shouldn't be postponed in favor of exploring it. Since the early 2000s, Amazon has been losing resilience and increasing human land use also appears to be contributing to the observed Amazon resilience loss

[1]Source: https://oeco.org.br/dicionario-ambiental/28783-o-que-e-a-amazonia-legal/. Retrieved August 4, 2022

(Boulton et al., 2022). According to the authors, this could imply dieback that would affect biodiversity, carbon storage, and climate change on a global scale. In our view, it must be noticed that the diminishing resilience could also impact native people and our ability to sustainably explore natural resources.

In addition, studies suggest that due to deforestation, we are getting closer to a tipping point. Once deforestation decreases evapotranspiration and, consequently, atmospheric moisture, we could cross a tipping point, where precipitation might be reduced by up to 40% in the remaining non-deforested parts (Boers et al., 2017).

Due to that, especially since the early 2000s, many policies were created to conserve the Amazon rainforest in the Legal Amazon, and a recent one is the "Priority Municipalities." In 2008, a decree[2] created the policy, and consists of adding municipalities to a priority *blacklist*, according to some criteria. These decrees are from the Ministry of the Environment (ME). If on the list, the municipality gets priority when it comes to actions to prevent and control deforestation. The inspection is mainly performed by the Brazilian Institute of Environment and Renewable Natural Resources (Portuguese: Instituto Brasileiro do Meio Ambiente e dos Recursos Naturais Renováveis, IBAMA), but also by the National Institute for Colonization and Agrarian Reform (*Instituto Nacional de Colonização e Reforma Agrária, INCRA*).

As pointed by Assunção et al., 2019, deforestation is a persistent process, and the idea of adding a city to be constantly monitored fits this trend well. The adding process usually happens once a year and, as above mentioned, depends of multiple criteria, related to deforestation increase, defined by the Ministry of the Environment (ME). Additionally, the Federal government of Brazil prioritizes its plans and programs aimed at the Amazon region for economic and social incentives to the municipalities listed. With that, forestry, agro-extractive, and sustainable agriculture can be fostered.

In general, the policy seems to work in diminishing deforestation (Assunção et al., 2019). For instance, Assunção et al. evaluate if this policy efficiently slows down deforestation, investigating if the chosen cities are optimal and calculating spillover effects. To do so, they use an econometrics method called "Changes-in-changes", a generalization of the "diff-in-

[2] Specifically the Decree nº 6.321/2007, published by the Federal Government.

diff" and, although not so widespread, it is useful when trying to understand if the chosen treatment unities are optimal. As a result, they found a spillover effect, and the list selected by the Brazilian government, even though diminishes deforestation, generates 8 percent more carbon emission than the optimal found. That is, it could be better.

Also, Assunção and Rocha found that the policy is effective in stopping deforestation and avoided, in 3 years, 11,218 km^2 of deforestation (Assunção and Rocha, 2019). They use panel data and fixed effects in their paper, with being on the list as a dummy. The slowing of deforestation, according to them, happened without growth impact and with an increasing number of fines by IBAMA.

As above mentioned, there are multiple criteria for joining and leaving the list. For this reason, the list has changed, via decree (that is, exogenously), over the years. Many municipalities left, new ones were inserted, and we can explore this process to answer a few questions about this public policy.

Therefore, our main questions are threefold:

- at the beginning (2008), was the policy effective?
- did the treatment (being listed) effect change over the years?
- if it did change, why?

To do so, we use difference-in-differences (DiD) and variations. For the first question, we use difference-in-differences (DiD) with Propensity Score Matching (PSM) to measure the effectiveness of the policy on municipalities listed in 2008. Then, since we have the dates of when all municipalities were listed in a panel database from 2006 to 2019, and multiple municipalities left the list or were inserted, we can use this information to understand how the effect of being treated changed over time, using a staggered diff-in-diff method.

Our findings suggest that the policy was effective back when it was created in 2008, decreasing deforestation per area of listed municipalities. However, when analyzing how the policy evolved over the years, it becomes clear that it has been weakened, especially after 2012. By the end of the decade, the policy had little to no effect. Additionally, the present study finds that this negative effect happened due to resource allocation and a decrease in resources for environmental protection decreasing. As it becomes clear, political alliances have an impact on the policy efficacy, and the amount of investment in monitoring organizations is diminishing.

To our knowledge, this is the first study to use staggered difference-in-differences for environmental policy evaluation. As pointed out before, multiple papers have analyzed the effectiveness of the policy during the beginning or aggregated throughout the years. However, this is the first study to measure how the Amazon's blacklisting policy evolved through the years. Additionally, another of the paper's originality is trying to comprehend why the policy changed over the years, especially understanding the impact of political alliances on it.

Furthermore, this work contributes to the growing literature on optimal policies related to environmental issues, mainly deforestation. Lastly, it adds to the recent literature about DiD with staggered treatment, specifically for environmental policies.

Our data is from 2006 to 2019 (the last year with information about GDP and deforestation at the municipality level). The present work's database has the uniqueness of indicating each and every municipality listed over the years, not done before to our knowledge. To deal with the data and run the regressions, we use RStudio.

The rest of the paper is divided as follows. In section 2 we describe the institutional situation of the policy and our objectives. Later, in section 3, we expound the data used during the work. Next, we discuss our empirical strategy and then present our results. Lastly, we conclude.

Chapter 2

Institutional Framework

The Amazon rainforest, one of many of Brazil's biomes, is a source of wealth, natural resources, the house of native people, and a large population (more than 20 million people) (Azevedo-Ramos, 2008). Additionally, studies indicate For that, the region can't be "isolated" and it must be inserted correctly into Brazilian's economy.

Historically, Amazon's deforestation has a significant relationship with economic factors. For instance, openness to foreign direct investment is connected with deforestation (Opoku and Boachie, 2020). Also, there are indications that increasing GDP per capita decreases deforestation (Faria and Almeida, 2016). In addition, as Faria and Almeida pointed out, the expansion of the agricultural frontier is one of the main causes of deforestation, especially livestock and soybean production. The soybean price, for instance, is highly correlated with land-use changes (Morton et al., 2006). With high prices and profitability, agents tend to care less about punishment and change the rainforest land to crop and pasture.

Despite being a process that happens since the XVI century, with Portugal deforesting and exploring *Paubrasilia*, tropical deforestation has only become an international concern in the 80s (Barbier and Burgess, 2001). This is mainly due to natural scientists who informed the world about the potential consequences of tropical forest destruction. Barbier et al. also noticed that, and since then, economists have developed more and more tools to analyze tropical deforestation and policies aiming to slow down this movement.

In the last decades, the federal government created multiple policies trying to deal with deforestation expansion. One major policy is the "Priority List".

As pointed out in Section 1, this policy was created in 2007, intended to start in 2008, by the Ministry of the Environment (ME) with Decree no. 6.321/2007. The idea of the policy is to introduce municipalities in a list, according to some criteria. The fundamental philosophy behind the policy was to share the responsibility between government and private actors for deforestation in the region (Lima, 2008).

If on the list, multiple penalties are imposed on all actors operating in the municipality. In general, the government acting within priority municipalities is more rigorous, with environmental monitoring and harsher law enforcement (Assunção and Rocha, 2019). As the authors point out, licensing and georeferencing requirements for rural establishments, requested by INCRA, were more severe in listed municipalities, as an effort to identify fraudulent documents (also known as *grilagem*) and illegal occupations. Additionally, rural landowners located in listed municipalities have greater restrictions to obtain rural credit.

There are three criteria for a municipality to be added to the list:

- Total deforested area, that is, accumulated deforestation;
- Total deforested area in the last three years;
- If the deforestation rate has increased in at least three years, from the last five ones.

Furthermore, only municipalities in the Amazon region are added to this specific list. For that, it's worth mentioning that municipalities with Cerrado (a Brazilian biome), for instance[1]. Nonetheless, the same methods applied throughout the present paper can be used to analyze the policy in municipalities that contain Cerrado.

On the other hand, for the municipalities to leave the list, they must accomplish three goals[2]:

[1] Sources of deforestation and the reasons behind it are different from environmental damage in the Amazon Rainforest.

[2] Source: http://combateaodesmatamento.mma.gov.br/images/conteudo/PPCDAM_2aFase.compressed.pdf. Retrieved July 7, 2022

- own 80% of its territory, excepting conservation units and indigenous lands, rural land areas properly monitored through the Rural Environmental Registry (*Cadastro Ambiental Rural, CAR*);
- deforestation in the previous year was equal to or less than 40km^2;
- the average deforestation of the last two years was equal to or less than 60% in relation to the average of the three years preceding those couple years.

When created, the list had 36 municipalities and that accounted for 45% of the Amazon deforestation in 2007, an astonishing number since 547 cities intersect the biome (Assunção and Rocha, 2019). Currently, there are 52 municipalities on the list. In Figure 2.1 presented below, we plotted two graphs. Graph A shows how many municipalities were on the list over the years. Graph B shows the percentage of deforestation in listed municipalities in relation to the total Legal Amazon deforestation.

With both graphs, we can see how the percentage of deforestation blacklisted has been growing, despite the stable numbers of municipalities blacklisted. In 2019, for instance, this number was 63,8%. That shows the relevance of these municipalities regarding environmental damage, the importance of looking directly at them, and the present work's importance.

In Figure 2.2 we created a timeline of all listed municipalities, from 2008 to 2022 where the green line represents if the city was listed again. As we can see, multiple municipalities have left and then returned to the list later. Additionally, we plotted this timeline grouped by Federative units in Figure A.1. Mato Grosso and Pará are the units with the most listed municipalities.

When it comes to an overview, since 2005, there has been a movement in annual deforestation decrease, as seen in Figure 2.3. We reached the lowest point in 2012 and kept stable for some years until a small increase beginning in 2015. In 2019, however, when Jair Messias Bolsonaro, the current Brazilian president, was sworn in, 10,129 km^2 were deforested in the Legal Amazon region, a 34,4% increase compared to the previous year. For the next couple of years, the number continued to grow. Finally, in 2021, 13,235 km^2 were deforested, the

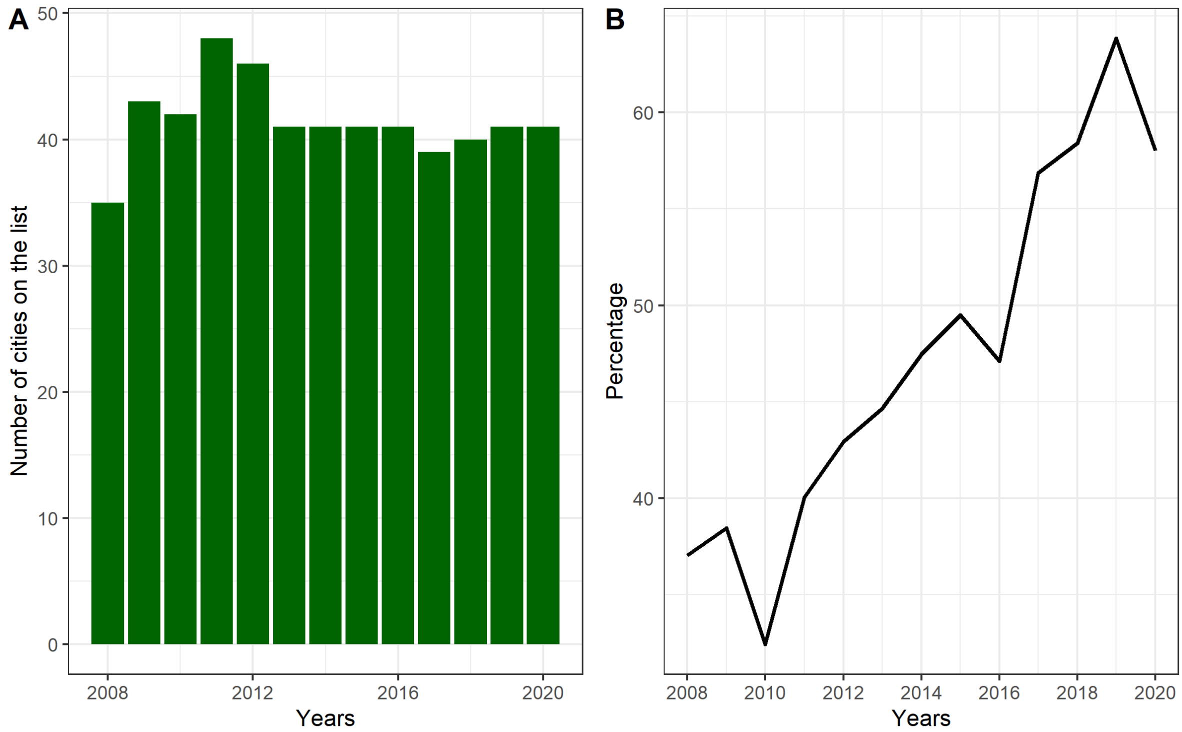

Figure 2.1: The Number of municipalities on the list and the percentage of deforestation in listed municipalities in relation to the total Legal Amazon deforestation, from 2008 to 2020

Data source: INPE

highest number since 2008, and in 2022 we are already breaking records [3]

Many reasons are cited for this downfall trend from 2005 to 2015. New policies were created, and enforcement actions have taken place such as indigenous lands expansion, supervision, satellite images with much more precision, and so on. For instance, The Amazon Basin registered the highest proportion of forests managed for social services. In Brazil, these areas are primarily allocated to indigenous peoples, helping to conserve local culture and avoid deforestation (FAO, 2011).

This creation of indigenous land was part of a plan called Action Plan for the Prevention and Control of Deforestation in the Legal Amazon (*Plano de Ação para Prevenção e Controle do Desmatamento na Amazônia Legal, PPCDAm*). Launched in 2004, it was a unified plan

[3]Source:https://www1.folha.uol.com.br/ambiente/2022/02/desmatamento-na-amazonia-brasileira-bate-recorde-em-janeiro.shtml. Retrieved August 4, 2022.

where 17 Ministries would work together, trying to deal with Amazon's deforestation. The main idea behind is that: like any other criminal activity, there is an economic factor behind deforestation and repression alone would not work. That being, according to the Ministry of the Environment, the plan had 3 main axes (Mello and Artaxo, 2017):

- Land and territory regularization;
- Environmental monitoring and control;
- Promotion of sustainable economic activities.

With the new policy, during ex-president Lula's first term, from 2002 to 2006, and despite food price rises, the main vectors of deforestation (soybeans and livestock) continued to grow but with less impact on the Amazon rainforest (Boucher et al., 2013).

As above mentioned, the downfall trend continued until 2014, with a slight increase during 2015-2018. This happened conjointly with Dilma Rousseff's second term, from 2014 to 2016, and, later, during Michel Temer's term, from 2016 to 2018. During both these governments, surveillance expenditure decreased, as pointed out throughout Subsection 5.3.2. Nonetheless, the deforestation increase was slow until a structural break, represented by the blue vertical in Figure 2.3. Then, it seems clear that despite efforts during the beginning of the century, the deforestation rate started to increase from 2014. The present paper intends to help to understand why this happened.

Lastly, it is important to notice that the absolute majority of Brazilian deforestation is illegal. For instance, in 2020, more than 99% of deforestation alerts (95,2% in area) did not have a vegetation clearance authorization registered with IBAMA, mandatory for the activity to be legal (AZEVEDO et al., 2020).

For the reasons presented above, policies to impend deforestation are still relevant, especially the low-cost and effective ones. In the following chapter, we present the data used during the rest of the work.

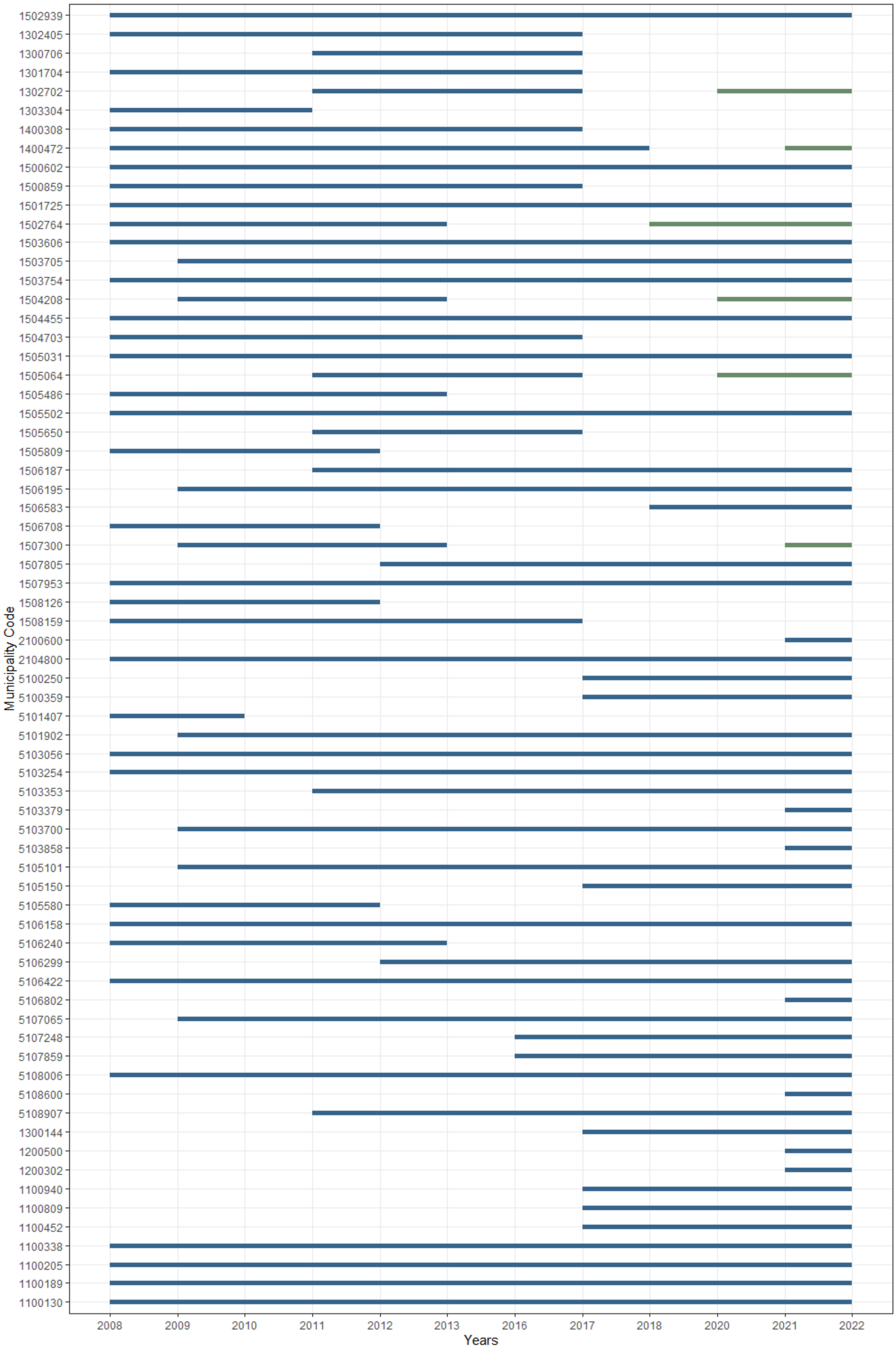

Figure 2.2: Timeline of listed municipalities

Data source: Brazilian Federal Government

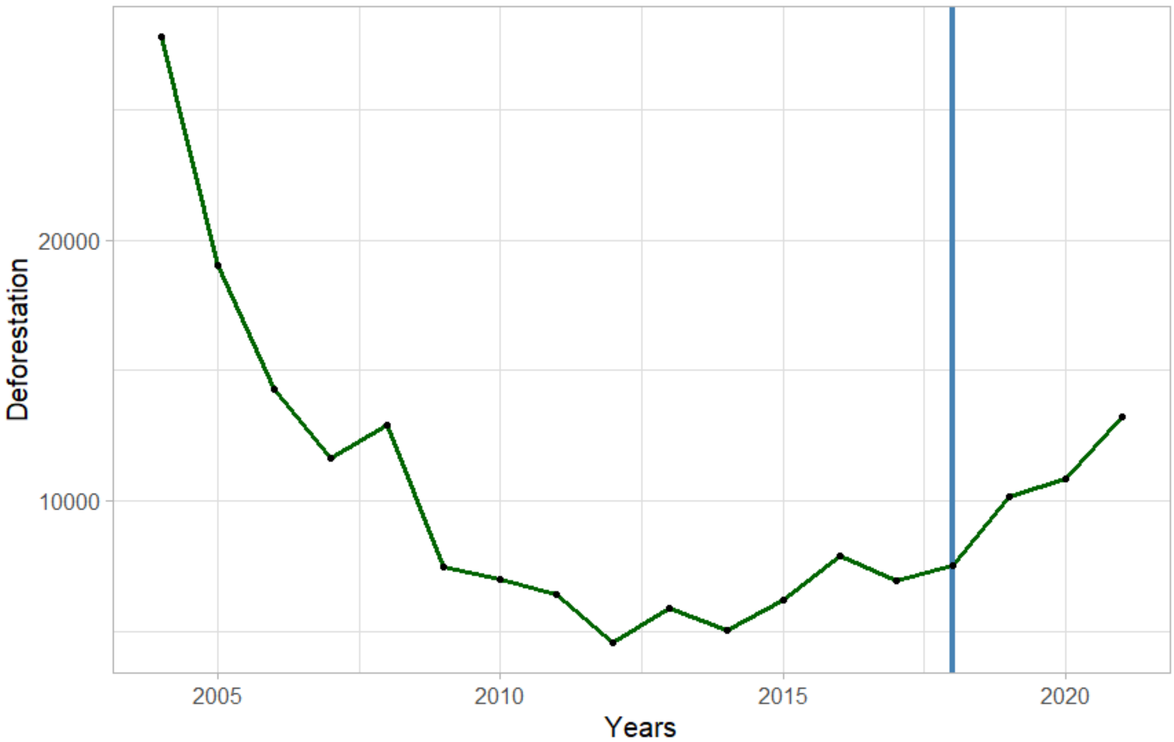

Figure 2.3: Annual deforestation in the Legal Amazon, 2004 to 2021 (km^2)

Data source: INPE

Chapter 3

Data and Descriptive analysis

In this chapter, we introduce all variables used in the following chapters. Our data contain variables from 2006 to 2019, for all municipalities that belong to the Legal Amazon.

The focus of the policy and our dependent variable is deforestation. We got all our data from the Project for Monitoring Deforestation in the Legal Amazon by Satellite (*Projeto de Monitoramento do Desmatamento na Amazônia Legal por Satélite, PRODES*), a georeferenced project of the National Institute for Space Research (*Instituto Nacional de Pesquisas Espaciais, INPE*). The PRODES estimates five variables within each municipality: deforestation, forest, non-forest, hydrography, and cloud. The information is given in squared kilometers.

Following, we created three variables. The first one, and our main, is given by:

$$Deforest\_per\_km_{it} = \frac{ADI_{it}}{(Mun\_Area_i)} \tag{3.1}$$

where $Deforest\_per\_km_{it}$ is the deforestation increase divided by territorial area, and our variable of interest, for municipality i and year t; ADI_{it} is the annual deforestation increment, and Mun_Area_i is the area of municipality i.

We believe that although not so easily interpretable, this dependent variable can capture better the perception of agents once total deforestation increases do not take into account relative deforestation and area. Either way, for robustness, we created log-transformed and normalized deforestation increase. The latter was done according to Assunção and Rocha, 2019, and stated by the following expression:

$$Norm\_Deforest_{it} = \frac{ADI_{it} - \overline{ADI}_{it}}{sd\left(ADI_{it}\right)} \tag{3.2}$$

where $Norm\_Deforest$ is the normalized annual deforestation increment; ADI_{it} is the annual deforestation increment; and $\overline{ADI}_{it}$ and $sd(ADI_{it})$ are, respectively, the mean and the standard deviation of the annual deforestation increment, in the period from 2006 to 2019. Finally, the subscripts indicate municipalities (i) and year (t). The results for these variables are contained in the Appendix B.

As for agricultural prices, we were also heavily inspired by Assunção and Rocha, 2019. As the authors point out and showed in Assunção et al., 2015, agricultural output prices are endogenous to local agricultural production. Also, agricultural commodity prices from the southern Brazilian state of Paraná are highly correlated with average local crop prices calculated for the Legal Amazon municipalities. Thus, we have an exogenous indicator of the local market situation. With this information, we collected prices from the Secretary of Agriculture and Supply of the State of Paraná, from 2006 to 2019. Next, we gathered data for municipal agricultural production (temporary and permanent crops), on the the Brazilian Institute of Geography and Statistics (*Instituto Brasileiro de Geografia e Estatística, IBGE*) website.

With both variables, we were able to create a weighted real price indicator for each of the most important crops[1] of the region. This weighted price was created according to the expression:

$$PPA_{itc} = PP_{tc} * A_{ic,2006-2007} \tag{3.3}$$

where PPA_{itc} indicates the weighted real price of crop c in municipality i and year t, PP_{tc} represents the Paraná-based real price of crop c in year t expressed as an index with the base year 2006; and $A_{ic,2006-2007}$ is the share of the municipal area used as farmland to produce crop c in municipality i, averaged over 2006 through 2007 period. As pointed out by the authors, this last term captures the relative importance of crop c within municipality i' s agricultural production in the years right before the sample periods.

[1]Soybean, cassava, rice, corn, and sugar-cane.

Following, with the PPA_{itc} for all crops, we were able to create a single index, weighted according to principal component analysis, based on Assunção et al., 2015. The weights are shown in Table 3.1 below.

Table 3.1: Crop Weights

Crop	**Weight**
Corn	0.6362
Soybean	0.5940
Rice	0.4879
Sugar-Cane	0.063
Cassava	0.0171

Then, we did a similar process to cattle and created a PPB_{it} index for all municipalities i and year t.

Finally, to control for socioeconomic factors we included GDP per capita and population density (Cisneros et al., 2015), given by the following equation:

$$Dens_{it} = \frac{POP_{it}}{(Mun\_Area_i)} \tag{3.4}$$

where $Dens_{it}$ represents our population density variable, for municipality i and year t; POP_{it} considers the general population in municipality i and year t and Mun_Area_i represents municipality area for i, which is constant over time. All the data used for these variables were obtained from IBGE.

We obtained the list of municipalities manually from the decrees of the Ministry of the Environment. This process took a while as some municipalities left and then came back, so, we had to find out when they were inserted again. Nevertheless, in the end, it was possible to obtain a solid database, with information on the entry and exit of 69 municipalities from 2008 to 2019.

Finally, all monetary variables used during the work were deflated (when fixed effects were not available) using RStudio, with the package *deflateBR*, in which January 2020 was the reference month (Meireles, 2018). In Table 3.2, we present the statistical description of the variables used, from 2006 to 2019.

Table 3.2: Summary Statistics for variables used

Variable	Min	Mean	Sd	Pctile[25]	Pctile[75]	Max
Deforestation_per_km	0	0.002	0.004	0	0.002	0.058
Population_Density	0	26.018	140.836	2.028	17.202	2784.108
Population	931	32871.404	105361.98	6953.751	28469.513	2182763.116
GDP_per_capita	1346.1	13095.76	15287.703	5551.737	14852.43	291965.35
PPA	0	0.794	2.093	0.053	0.654	34.194
PPB	0.1	3620.564	4142.54	676.024	4977.117	29189.849

Next, in Figure 3.1, we plot the accumulated deforestation through the years. The white lines represent listed municipalities. It is visible how the Amazon Agricultural Frontier (in the south of the biome) is the main focus of deforestation and, consequently, of the listing policy. The two municipalities with the largest deforested area are Altamira and São Félix do Xingu, both located in the state of Pará.

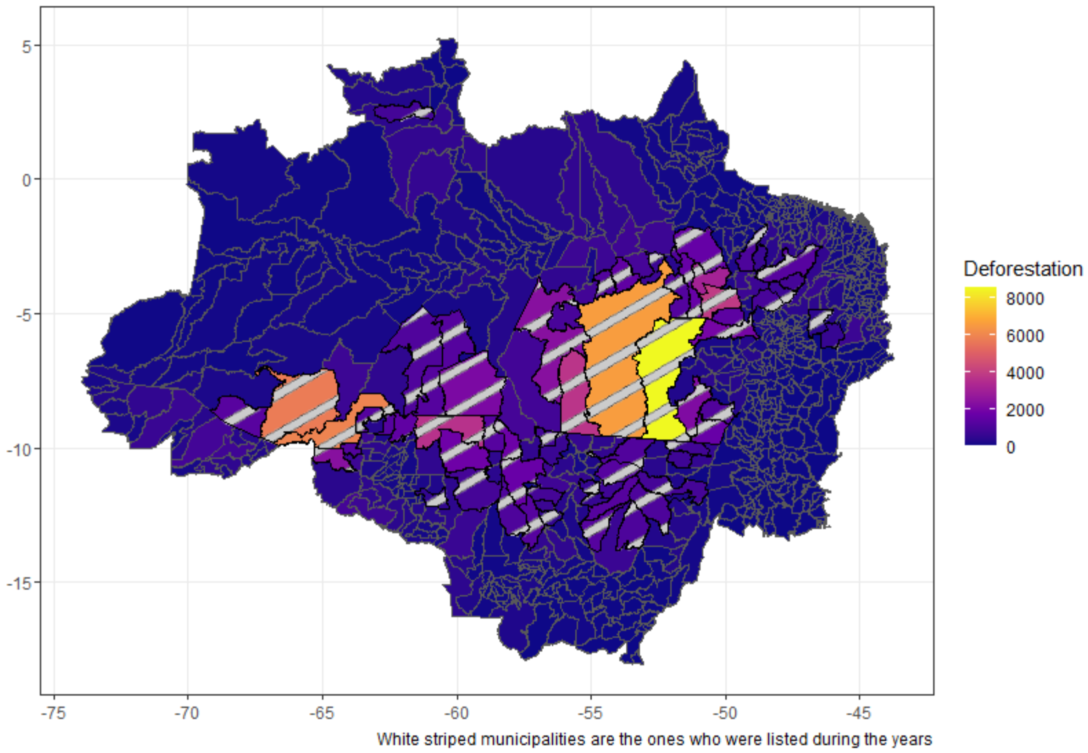

Figure 3.1: Accumulated deforestation and listed municipalities during the years

Data source: INPE

Chapter 4

Empirical Strategy

4.1 Was blacklisting effective in 2008?

For the first question of the present work, we use DiD with Propensity Score Matching (DiD-PSM) so we can compare better municipalities and increase the robustness of the analysis. This is also important when considering the parallel trend assumption. This assumption claims that without the treatment, both the control and treatment groups would have the same changes in the dependent variable (Gardner, 2021). When matching, we can compare municipalities that had historically similar trends, our analysis gets more robust, and the common trend assumption is more plausible (Gebel and Voßemer, 2014).

The first step was to do a sample selection for the control group by matching with propensity score, without replacement. To create the matching, we used criteria defined by the Ministry of the Environment itself. As our data pointed out, many municipalities accomplished the last criteria (3 positive growing deforestation rates over the last five years), had a high level of deforested area accumulated and through the last three years, and weren't listed. This opened a window to match. For robustness, we tested other matching criteria such as deforestation per municipality area. Nevertheless, the results were similar. To do the matching, we calculated the probability of a municipality to be listed in 2008 (the propensity score) and used this probability to match this municipality with a non-listed one with a similar score, without replacement. With this process, we can devise a non-experimental procedure for evaluation of the policy (Heckman et al., 1997). We have done this with RStudio's package MatchIt (Ho et al., 2011).

After the matching, we had 2 groups, with 34 municipalities each. The first one is the group of treated/listed municipalities. The second group was the control group (municipalities that could have been on the list but were not).

We present below the deforestation increase per km^2 through the years (2006 to 2019). The blue vertical line represents the beginning of the policy. The red line represents municipalities in the treatment group and the light blue line represents cities from the control group. As we can see, both groups historically have had similar trends, with a higher increase for control near the treatment start. We suppose that the trend will be parallel once we control for covariates.

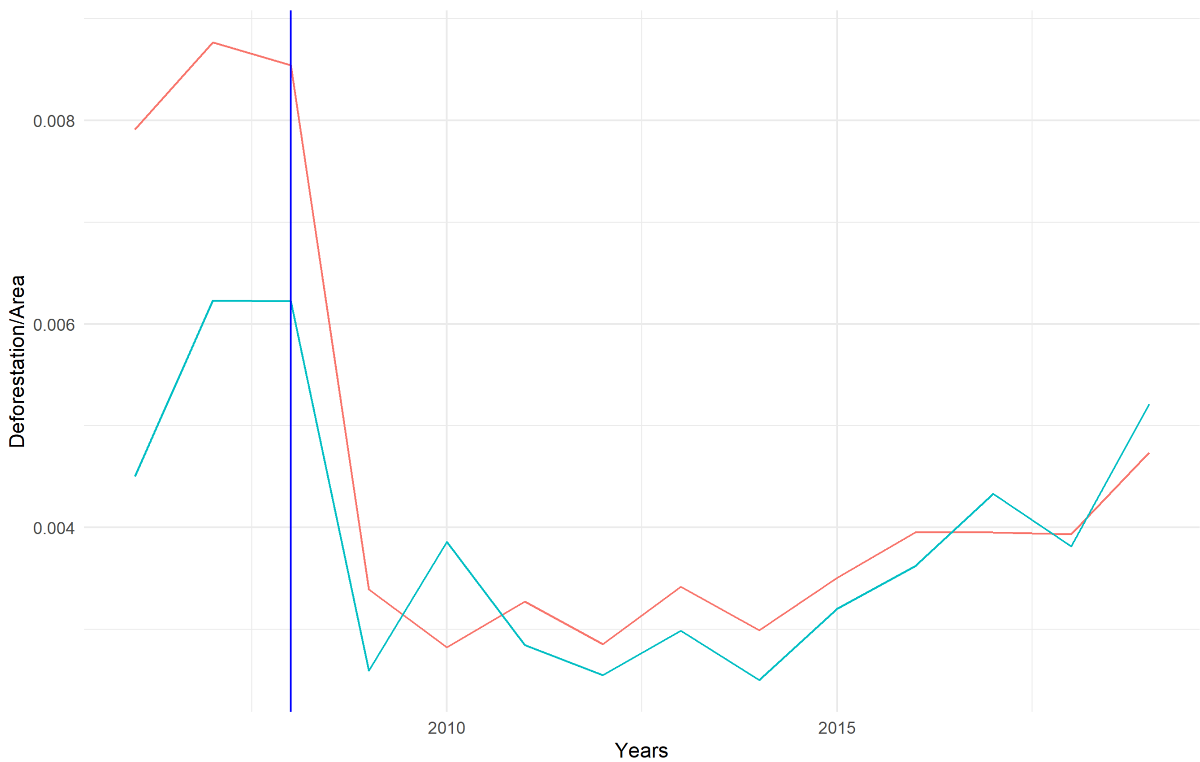

Figure 4.1: Time series of deforestation increase per km^2

Data source: Brazilian Federal Government

The next step was to DiD. The idea behind the classic DiD is an attempt to identify the causal effect of a specific treatment when the parallel trend assumption is valid (Gardner, 2021). We were able to compare control and treated groups.

Mathematically, as summarized by Gebel and Voßemer, suppose that there is a group of municipalities that were treated ($D = 1$) and municipalities that were not ($D = 0$). For these groups, two potential deforestation outcomes at each time are defined (Y^0, Y^1) but we can only observe one (the realized) and the other deforestation level remains as an unobserved counterfactual. What the DiD does is comparing the change in deforestation $E(Y^1_{t+1} - Y^0_t|D = 1)$ of the listed group and the alteration of the counterfactual change trend in deforestation $E(Y^0_{t+1} - Y^0_t|D = 1)$ they would have experienced without the treatment. This counterfactual trend is approximated by the actual change in deforestation $E(Y^0_{t+1} - Y^0_t|D = 0)$ of the not listed group.

Besides, it is worth mentioning that the method presented above is only valid when the parallel trend assumption works, that is:

$$E(Y^0_{t+1} - Y^0_t|D = 1) = E(Y^0_{t+1} - Y^0_t|D = 0) \tag{4.1}$$

The regression equation is as follows:

$$Y_{\text{it}} = \beta_0 + \beta_1 D_i + \beta_2 Post_t + \beta_3 D_i * Post_t + \epsilon_{\text{it}} \tag{4.2}$$

where D_i is a dummy, as above mentioned, indicating whether the municipality i was treated or not; $Post_t$ is also a dummy but for time, that is equal to one if post-treatment.

Finally, when estimating, our coefficient of interest is $\hat{\beta}_3$, given by the interaction between the two dummies, which measures the effect of the policy on the treated group.

4.1.1 Covariates

Below, we present the statistics for both groups and a mean-difference test, before the treatment (that is, for 2007). As can be seen, the difference between their means is still significant for most variables. For this reason, we have to control for these variables, and this is done during Chapter 5. Controlling happens inserting a covariate matrix X_{it} in the Equation 4.2.

Table 4.1: Summary Statistics according to groups

plot	Treated				Control				
Variable	Sd	Mean	Max	Min	Sd	Mean	Max	Min	Test
Deforestation_per_km	0	0.009	0.023	0.001	0	0.006	0.024	0	F= 2.997^{*}
Population_Density	2.8	2.756	10.834	0.344	7.7	5.74	33.539	0.322	F= 4.795^{**}
Population	81968.9	47289.367	369345.091	5554	26170.1	26284.406	118193.905	3174	F= 2.035
GDP_per_capita	4771.2	8793.209	26893.97	3616.22	2221.1	6091.544	10700.42	2422.87	F= 9.045^{***}
PPA	0.7	0.349	3.614	0.002	0.4	0.22	1.442	0.001	F= 0.937
PPB	1214	1573.24	4843.791	122.291	2259.9	2367.36	8863.842	5.09	F= 3.407^{*}

Statistical significance markers: * p<0.1; ** p<0.05; *** p<0.01

4.2 Has the effect of the treatment changed over the years? Two models.

For the next part of the work, we explore how effective the policy has been over the years. As it has been done historically, we could use a two-way fixed effect (TWFE) linear regression and estimate the average effect of being listed through the years, avoiding constant omitted variables. Nevertheless, the current literature presents pitfalls of this method and, for that, it should be avoided (Callaway and Sant'Anna, 2021).

In addition, Gardner pointed out that in the classic two-period and two-groups case, presented in Subsection 4.1, differences-in-differences regression gets the difference in outcomes between treated and untreated units after removing group and period effects. With valid parallel trends' assumption, this translates to the average effect of the treatment on the treated group. The line of thought presented above, however, fails when there are multiple groups and periods. When the adoption of a treatment is staggered, and the average effects of the treatment depends on group and period, the classic difference-in-differences does not identify an easily interpretable measure of the typical effect of the treatment (Gardner, 2021).

There are ways, nevertheless, to bypass both these issues. And although being contemporary literature, we already have some insights and applicability. For the present paper, we use the method proposed by Callaway and Sant' Anna. The idea is that if treatment stays on, we can compare group A (for instance, a group of municipalities that were inserted on the list in year t), group B (inserted in year $t+1$), and so on, and group N (never on the list) (Callaway and Sant'Anna, 2021). The method developed by Callaway and Sant' Anna estimates the average treatment effect for the treated subpopulation (ATT) by comparing the average change in outcomes experienced by the treated group to the average change in outcomes experienced by the comparison group.

On the other hand, this model demands multiple assumptions. We present them below, intuitively and mathematically:

1. Irreversibility of treatment - if treated, the treatment can not be "turned off":

$$D_{t-1} = 1 \text{ implies that } D_t = 1 \tag{4.3}$$

   where, as noted before, $D_t = 1$ means being treated in year t;

2. Random Sampling - implies access to panel data;

3. Limited Treatment Anticipation - restricts treatment anticipation for all "eventually treated" groups, that is, there is a known $\delta \geq 0$ such that:

$$\mathbb{E}\left[Y_t(g) \mid X, G_g = 2008\right] = \mathbb{E}\left[Y_t(0) \mid X, G_g = 2008\right] \text{ a.s. for all groups g,}$$
$$t \in \{2008, \ldots, 2019\} \text{ such that } t < g - \delta \tag{4.4}$$

   where G represents the time period when a unit first becomes treated and Y is deforestation increase, G_g is a binary variable that is equal to one if a unit is first treated in period g and X is the covariates matrix;

4. Conditional Parallel Trends Based on a "Never-Treated" Group - for each group and year after 2008, such that $t \geq g - \delta$:

$$\mathbb{E}\left[Y_t(0) - Y_{t-1}(0) \mid X, G_g = 1\right] = \mathbb{E}\left[Y_t(0) - Y_{t-1}(0) \mid X, C = 1\right] \tag{4.5}$$

   where C is a binary variable that is equal to one for municipalities that do not participate in the treatment in any time period;

5. Conditional Parallel Trends Based on "Not-Yet-Treated"Groups - letting δ be as defined in Assumption 3, for each group and each $(s,t) \in \{2009,\ldots,2019\} \times \{2009,\ldots,2019\}$ such that $t \geq g - \delta$ and $t + \delta \leq s < \bar{g}$,

$$\mathbb{E}\left[Y_t(0) - Y_{t-1}(0) \mid X, G_g = 1\right] = \mathbb{E}\left[Y_t(0) - Y_{t-1}(0) \mid X, D_s = 0, G_g = 0\right] \text{ a.s.}$$

where s represents "not-yet-treated" group by time s;

6. Overlap - for each $t \in \{2008,\ldots,\mathcal{T}\}$ and groups g, there exist some $\varepsilon > 0$ such that $P\left(G_g = 1\right) > \varepsilon$ and $p_{g,t}(X) < 1 - \varepsilon$.

For Assumption 2, we could assume that the effects of being listed during a period are never forgotten by the municipality. However, as there are cities that left the list and then came back, is valid to suppose that the effect of being treated can be "lost" over years. Due to that, we decided to use only municipalities that have never left the list. That being said, we chose the sample according to the irreversibility assumption. That is: only municipalities listed and that never left the list are considered treated.

Next, no anticipation is assumed to be valid (that is, $\delta = 0$). That is because the deforestation agents can't know before it happens if the municipality that they are acting is going to be on the list. Also, since the list is not a well-known policy and works more as a resource targeting, for institutional reasons, it is plausible that deforestation agents don't know much about it.

Lastly, according to the authors, Assumptions 4 and 5 are two different *conditional* parallel trends assumptions that generalize the two-period parallel trends assumption to the case where there are multiple time periods and multiple treatment groups. As they are interchangeable, we take Assumption 4 as valid. Specifically, it states that conditional on covariates, the average outcomes for the group first treated in all periods g and for the "never-treated" group would have followed parallel paths in the absence of treatment. We test it for group 0 and assume true for the others g's.

Finally, the last assumption is very strong and states that a positive fraction of the population starts treatment in any period g. Unfortunately, for some years there was no list modification. Nonetheless, we assume some flexibility taking into account the large amount assumptions for the estimator. Also, as the authors point out, provided that one is comfortable with parametric extrapolation and is sufficiently confident that the outcome regression working models are correctly specified, we can assume that the coefficients are at least close to the real one. Additionally, the last assumption also assumes that for all g and t, the generalized propensity score is uniformly bounded away from one, and this seems valid for our case[1].

In the next Chapter, we present our empirical findings for the strategies presented above and compare them.

[1]The derivation of the method is beyond the scope of this study. It is rigorously shown in the cited work. Since the method is dense, it would take a lot of time and as this work has chosen to be more empirical than theoretical, we omit it here. The same is valid for the coefficient estimation.

Chapter 5

Results

5.1 The Policy in 2008

First, we present results for the evaluation of the policy in 2008.

In Table 5.1, the coefficients and their respective standard deviations (in parentheses) are informed. In it, we have three columns. The first column shows how the diff-in-diff behaves without controls and without fixed effects. According to it, our coefficient of interest has the expected sign and the coefficient of interest was statistically significant at 10%. Being listed implies less deforestation.

Next, we add controls but still with no fixed effects. For this case, the coefficient is not significant and the signal is the opposite of what was expected. Lastly, we add time and locality fixed effects, creating a robust specification, that being our main one. When we do it, the sign flips and the coefficient is significant, and being listed implies less deforestation increase per km^2. For all specifications, we didn't cluster the standard errors since the recent literature recommends caution when clustering (Abadie et al., 2017).

5.2 Staggered treatment

During this section, we present how the policy evolved from 2008 to 2019, for all groups of municipalities that were inserted on the list and never left. As above-mentioned, we evaluated

Table 5.1: Diff-in-diff with Propensity Score, for 2008

Dependent Variable:		Deforestation_per_km	
Model:	(1)	(2)	(3)
Variables			
(Intercept)	0.0045***	0.0050***	
	(0.0002)	(0.0003)	
time × listed	-0.0005*	8.03×10^{-5}	-0.0030***
	(0.0003)	(0.0003)	(0.0008)
GDP_per_capita		-3.3×10^{-8}**	3.2×10^{-8}*
		(1.39×10^{-8})	(1.72×10^{-8})
PPA		-0.0001	-0.0008*
		(0.0002)	(0.0004)
PPB		-1.38×10^{-7}***	-1.61×10^{-7}
		(4.78×10^{-8})	(1.45×10^{-7})
Population_Density		8.72×10^{-5}***	-0.0006*
		(2.82×10^{-5})	(0.0003)
Fixed-effects			
Municipality			Yes
Year			Yes
Fit statistics			
Observations	966	966	966
R^2	0.00283	0.03421	0.65714
Within R^2			0.05944

*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1* the staggered treatment using the method developed by Callaway and Sant'Anna, where the significance level is 5%. It is important to notice that the construction of the confidence band presented below is not usual. According to the authors (Callaway and Sant'Anna, 2021):

> Unlike commonly used pointwise confidence bands, our simultaneous confidence bands asymptotically cover the entire path of the group-time average treatment effects with fixed probability and take into account the dependency across different group-time average treatment effect estimators. Thus, our proposed confidence bands are arguably more suitable for visualizing the overall estimation uncertainty than more traditional pointwise confidence intervals.

The derivation of the formula used can be found in the authors' paper. With that, the confidence band described above is calculated and shown below.

In Table 5.2, we have the aggregated versions within groups. It is noteworthy that for groups 2008, 2009, 2011, and 2012, the policy seems effective. According to this, being listed decreased deforestation for all the first four years of the policy, but the effect was stronger for the 2009's group. For this group, being listed was associated with decreasing deforestation per municipality km^2 by around 0.0114. Furthermore, our findings suggest that from 2016 forward, the policy has stopped being significant. Nonetheless, for the 2018 group, the coefficient was significant again, but it has the opposite sign. Moreover, year-by-year values for all groups are available in Table A.1.

Graphically, we can observe how the ATT aggregated behave for all groups in Figure A.2. Additionally, it is possible to see how the coefficients behaved through the years, for each group, in Figure A.3.

Lastly, as the authors point out, it is possible to immediately measure group-time average treatment effects into average treatment effects at different lengths of exposure to the treatment. Thus, we can obtain the value for the overall Average Treatment Effect, based on group/cohort aggregation. According to it, the policy was efficacious in general. That is, taking into account all the periods since its conception, the creation of the listing policy was able to reduce deforestation increase per km^2. Therefore, even with heterogeneous effects between groups, the policy was effective.

Table 5.2: Average Treatment Effect for all groups

Group	Estimate	Std.Error	[95% Simult. Conf. Band]
2008	-0.0039	0.0009	[-0.0057, -0.0020]*
2009	-0.0114	0.0029	[-0.0174, -0.0053]*
2011	-0.0035	0.0003	[-0.0042, -0.0027]*
2012	-0.0036	0.0001	[-0.0039, -0.0034]*
2016	0.0014	0.0009	[-0.0005, 0.0032]
2017	-0.0008	0.0022	[-0.0054, 0.0039]
2018	0.0027	0.0001	[0.0026, 0.0028]*

Signif. codes: '' confidence band does not cover 0*

Table 5.3: Aggregated Average Treatment Effect

ATT	Std. Error	[95% Conf. Int.]
-0.004	0.0013	[-0.0066, -0.0014]*

Signif. codes: '' confidence band does not cover 0*

5.3 Why?

During this section, we explore the reasons behind the policy's weakening. In order to do this, we test two main hypotheses. First, could it be a resource targeting from the Federal Government to allied municipalities? Or, perhaps, the Federal Government provides flexible monitoring for allied cities, making the policy weaker. Second, could it be that the resources, in general, are diminishing and monitoring institutions are not able to work properly?

First, we present evidence of how politicians can affect institutional public policies and their funding.

Studies showed that local Brazilian politicians can affect deforestation. For instance, Pailler, using panel data for the period from 2002 to 2012, showed that the deforestation rate increases when a mayor is trying to be re-elected and this increase is primarily linked to corruption and electoral financing (Pailler, 2018).

Contemporarily, we can highlight the effect of a president on environmental public policy. For instance, once Bolsonaro was sworn in, in 2019, one of the first measures adopted by the new minister of the environment was extinguishing the secretary responsible for the PPCDAm[1], although revoked by decree the following year[2] after popular and media pressure, the product of a massive fire on the Amazon. Despite the flaws, it is a consensus that PPCDAm was one of the main reasons for this steady decline (CEPAL et al., 2011) and, yet, Bolsonaro' s crew has diminished it.

In addition, as pointed out before, an axis of the plan was monitoring and controlling, which is carried out by NGOs and federal institutions such as IBAMA and the Chico Mendes Institute for Biodiversity Conservation [*Instituto Chico Mendes de Conservação da Biodiversidade (ICMBio)*], without plain political control. About NGOs, Bolsonaro claimed many times that the they were firing Amazon to get more money and attention, even accusing Leonardo DiCaprio of helping them[3] and has called them "cancer"[4].

When it comes to IBAMA, Bolsonaro appointed the military to commissioned posts, not specialists[5]. Even though the army has always supported monitoring institutions, in 2019, via decree, they started to control and organize the monitoring. All of these decisions contributed to less surveillance and poor perfomance. As a result, IBAMA and ICMbio's infraction notices declined by 30% and IBAMA gave 80% fewer fines, with the president celebrating it, at the same time that deforestation increased raised rapidly[6].

[1]Source: http://www.planalto.gov.br/ccivil_03/_ato2019-2022/2019/decreto/D9672.htm. Retrieved August 4, 2022

[2]Source: http://www.planalto.gov.br/ccivil_03/_ato2019-2022/2020/Decreto/D10455.htm#art7. Retrieved August 4, 2022.

[3]Source: https://www.correiobraziliense.com.br/app/noticia/politica/2019/11/28/interna_politica,810018/bolsonaro-leonardo-dicaprio-esta-colaborando-com-queimada-na-amazon.shtml. Retrieved August 4, 2022.

[4]Source: https://oglobo.globo.com/brasil/bolsonaro-chama-ongs-de-cancer-entidades-contra-atacam-24624003. Retrieved August 4, 2022.

[5]Source: https://www.opendemocracy.net/pt/servidores-ibama-expoem-absurdos-doutrina-militar-combate-ao-crime-ambiental-amazonia/. Retrieved August 4, 2022.

[6]Source: https://www.cnnbrasil.com.br/nacional/autos-de-infracao-do-ibama-e-icmbio-caem-30-no-

For these reasons, we believe it is important to test for both hypotheses presented, which we do throughout the following subsections.

5.3.1 Resource Targeting

If the policy and institutions are not strong enough, a political change, like a federal election can affect it. To understand, then, how parties can affect the policy and how it can get weaker over the years, we check if the policy was weaker or stronger for allied municipalities in 2008. Our hypothesis is that if the policy was not firm enough in 2008 and flexible to political coalitions, it could be susceptible to weakening in the years that followed 2008 if the Federal Government so wishes.

To do it, we employ triple diff-in-diff (DDD). The triple DiD can be understood as the difference between two difference-in-differences estimators (Olden and Møen, 2020). As the authors point and prove, we only need parallel assumption between allies and non-allies to infer causality. However, leftist politicians (from PT, the labor party in power during 2008) tend to be more environmentally friendly than right-wing politicians. The, we compare PT with PDT, another left-center party, so the parallel assumption would be more realistic.

Still based on Olden and Møen and similar to Equation 4.2, the following equation summarizes our specification:

$$\begin{aligned} Y_{\text{it}} &= \beta_0 + \beta_1 D_i + \beta_2 A_{it} + \beta_3 Post_t + \beta_4 D_i * A_{it} + \\ &\quad \beta_5 D_i * Post_t + \beta_6 A_{it} * Post_t + \beta_7 D_i * A_{it} * Post_t + \epsilon_{\text{it}} \end{aligned} \tag{5.1}$$

where, just as Equation 4.2, D is a dummy indicating if the municipality was treated or not; $Post$ is a dummy for time, which equal to one if post-treatment. However, now we have a dummy A_{it} equals to one if the municipality i in year t is an ally of the president, that is, if they belong to the same party.

governo-bolsonaro/, https://oeco.org.br/salada-verde/em-evento-bolsonaro-comemora-reducao-de-80-das-multas-do-ibama/, and https://deolhonosruralistas.com.br/2020/02/02/mapa-mostra-por-municipio-os-maiores-multados-por-desmatamento-nos-ultimos-25-anos/. Retrieved August 4, 2022

Our coefficient of interest is the triple interaction and it is the difference between treatment for allied municipalities and treatment of non-allied ones. Our results for 2008 are in Table 5.4, where our dependent variable is log-linearized deforestation. We chose to do it so the parallel is valid, as we can see in Figure 5.1.

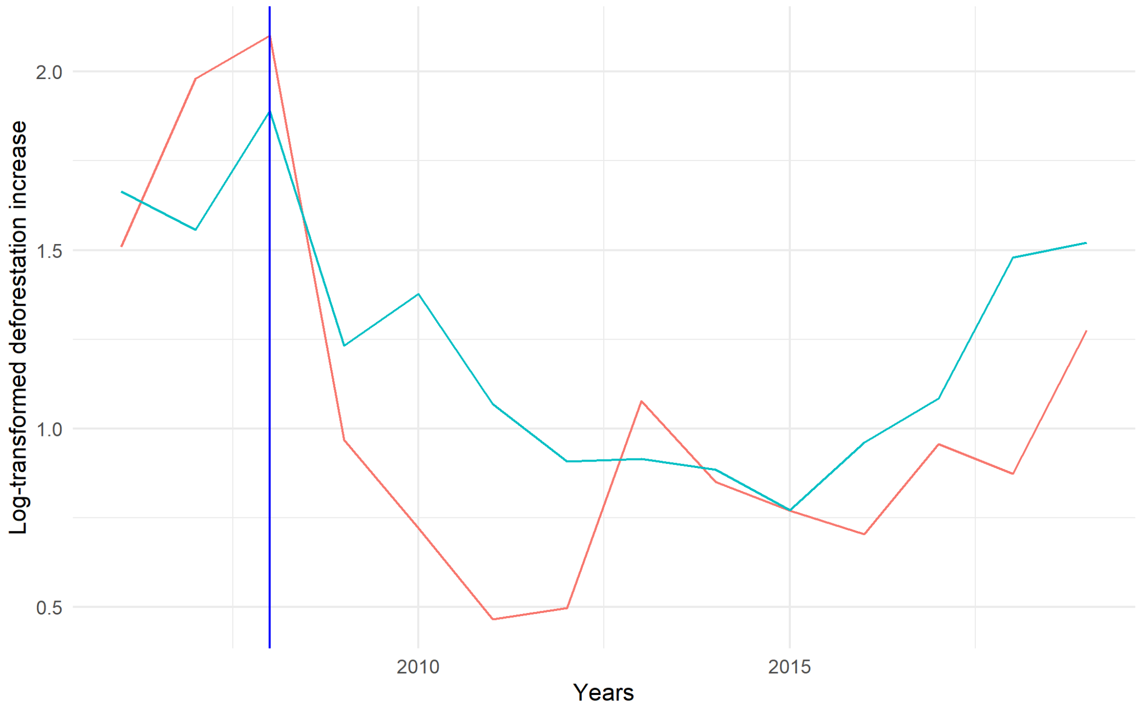

Figure 5.1: Time series of normalized deforestation increase

The red line represents non-ally cities and the blue represents allies. Data source: Brazilian Federal Government

In the first column, we present a simple triple diff-in-diff, without control and fixed effects. In this case, we see that our coefficient of interest is negative but non-significant. That is, in this specification, in a municipality where the mayor is an ally or belongs to the same coalition as the president, the policy is more effective. This result maintains in the second column. However, when we add fixed effects, our coefficient is still significant but the sign flips. That is: in 2008, politicians' collusions affected the policy, where allied municipalities were able to relax the policy. Is important to notice how the β_7 coefficient, that is, municipalities listed that were allied, disappears when there are fixed effects. This happens because this variable is captured by space and time fixed effects.

Ultimately, it seems that at least in 2008, the year of the policy creation, there was space for an institutional maneuver. We could expect a negative coefficient due to the federal administration focusing resources on allies' localities. Nonetheless, that does not appear to be valid. Moreover, it is important to remember that this structure may have changed since 2008. Finally, we are not seeing tacit alliances, elections coalitions, or ideological affinities, that could change the results presented above.

Either way, we extrapolate the results presented above and now, we see how many mayors are Bolsonaro's allies. Thus, we can analyze if part of the policy's weakening is due to alliances. To do it, we present in Table 5.5 the number of mayors, by their party, for all cities located within Legal Amazon. According to it, it is obvious how few municipalities were governed by PSL, Bolsonaro's party. This can be explained, somewhat, due to the mayor's election taking place in 2016, before Bolsonaro and PLS' ascension. When we investigate municipalities where PSL was part of the winning mayor coalition, the number jumps to 156, approximately 20% of all mayors. Additionally, this analysis does not consider *Centrão* parties' characteristics, that is, parties without political ideology which act according to the ruling presidential party.

All that being said, we can suppose that political alliances and resource targeting were, at least partially, responsible for the increase in deforestation and the decrease in efficacy of the policy from 2016 forth.

5.3.2 Resource Amount

For the second subsection, we test the next hypothesis, that is, that the resource amount focused on monitoring deforestation is decreasing. In order to do so, we analyze qualitatively how the funding for conservation, IBAMA, ICMBIO, and INCRA evolved over the years. We also present the number of infraction notices from IBAMA, in the Amazon Region, from 2006 to 2019.

First, Table 5.3.2 indicates all our variables and their descriptive statistics. We obtained all data from the Federal Government website and deflated it to 2020, so it is better comparable.

Table 5.4: Triple diff-in-diff, for 2008

Dependent Variable:	ln_incremento		
Model:	(1)	(2)	(3)
Variables			
(Intercept)	0.9459***	1.082***	
	(0.0743)	(0.0965)	
time × lista_inicial	2.647***	2.586***	-1.910***
	(0.3975)	(0.3964)	(0.5620)
time × ally	0.0854	0.0177	-0.0805
	(0.1018)	(0.1046)	(0.1735)
lista_inicial × ally	3.996***	3.832***	
	(0.5434)	(0.5412)	
time × lista_inicial × ally	-3.787***	-3.689***	1.651***
	(0.7715)	(0.7695)	(0.6276)
pib_per_deflac		$5.37 \times 10^{-6*}$	-1.32×10^{-6}
		(3.11×10^{-6})	(3.3×10^{-6})
ppa		-0.0493**	-0.0319
		(0.0238)	(0.0960)
ppaboi		$-3.49 \times 10^{-5***}$	3.78×10^{-5}
		(1.28×10^{-5})	(3.56×10^{-5})
dens		-0.0010***	-0.0036*
		(0.0003)	(0.0020)
Fixed-effects			
code_muni			Yes
ano			Yes
Fit statistics			
Observations	1,176	1,169	1,169
R^2	0.11917	0.13411	0.85423
Within R^2			0.02111

*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table 5.5: Number of mayors, by party, in the Legal Amazon, from 2017 to 2020

Party	Number of mayors
PMDB	147
PSDB	118
PSD	69
PR	54
PDT	51
PSB	44
PP	42
PC do B	38
DEM	32
PT	24
PV	23
PRB	22
PROS	19
PSC	16
PTB	14
SD	14
PSL	3
Others	32

Next, we present the total amount spent by the Federal Government on the conservation sub-function, from 2006 to 2019. In Figure 5.2, we can observe how the value fluctuates over the years. However, it peaked in 2018, but with a large decrease in the years that followed.

Table 5.6: Summary Statistics for conservation expenses

Variable	Min	Mean	Sd	Pctile[25]	Pctile[75]	Max
Infraction_IBAMA	2629	5105.5	1565.918	3989.75	5947.75	8845
Spent_IBAMA	1500928043.2	1728846952.563	185629210.82	1591575781.042	1795529053.903	2120468072.888
Spent_INCRA	2388203718.9	3905076213.652	1573857196.258	2770327010.994	4590026464.621	7509962820.427
ICMBIO	506746552.8	716311273.053	140088916.025	668247157.358	745278669.71	1044449092.142
Conservation	130832106.6	302631639.264	143410251.488	183920902.636	406128067.637	609991499.919

Following, we plot IBAMA's (one of the main agents with regard to monitoring defor-

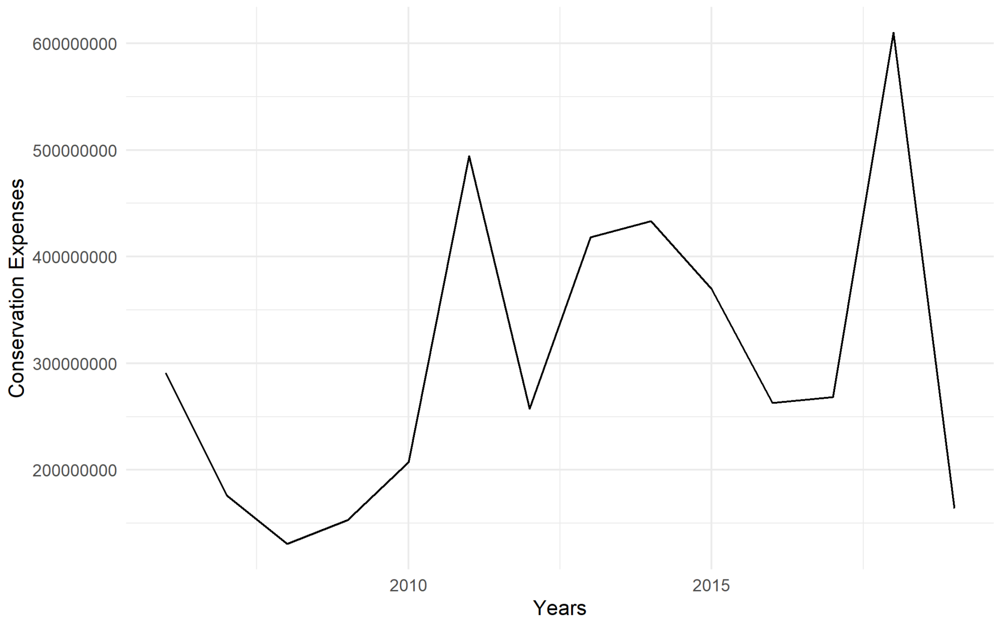

Figure 5.2: Time series of Conservation Expenses, from the Federal Government

Data source: Brazilian Federal Government & Observatório do Clima

estation, as pointed out before) budget and its infraction notices in the Amazon region, also from 2006 to 2019. As is visible, IBAMA's budget decreased steadily since 2006, achieving its lowest in 2019. Without surprise, there is a high correlation between budget and the number of infractions, once the institution becomes weaker with less money. Therefore, in 2019, the number of infraction notices was also the lowest. On top of that, the number continues to fall[7].

Finally, we present the budget from the two other institutions. The first, in panel A of Figure 5.4, is ICMBio. The second institution, in panel B, is INCRA. Both these institutions have a positive correlation with biodiversity conservation. As we can see, while the ICMBio's budget increased over time, INCRA's quickly decreased.

It seems that, despite some institutions being immune to budget changes, like ICMBio, INCRA and IBAMA were partially undermined. As a result, the number of areas in which these organizations acted, the number of agents, and so on, were probably impacted. Hence, we

[7] Source: https://g1.globo.com/jornal-nacional/noticia/2020/11/19/cgu-mostra-que-em-oito-meses-julgamentos-dos-autos-de-infracao-do-ibama-cairam-quase-90percent.ghtml. Retrieved August 4, 2022.

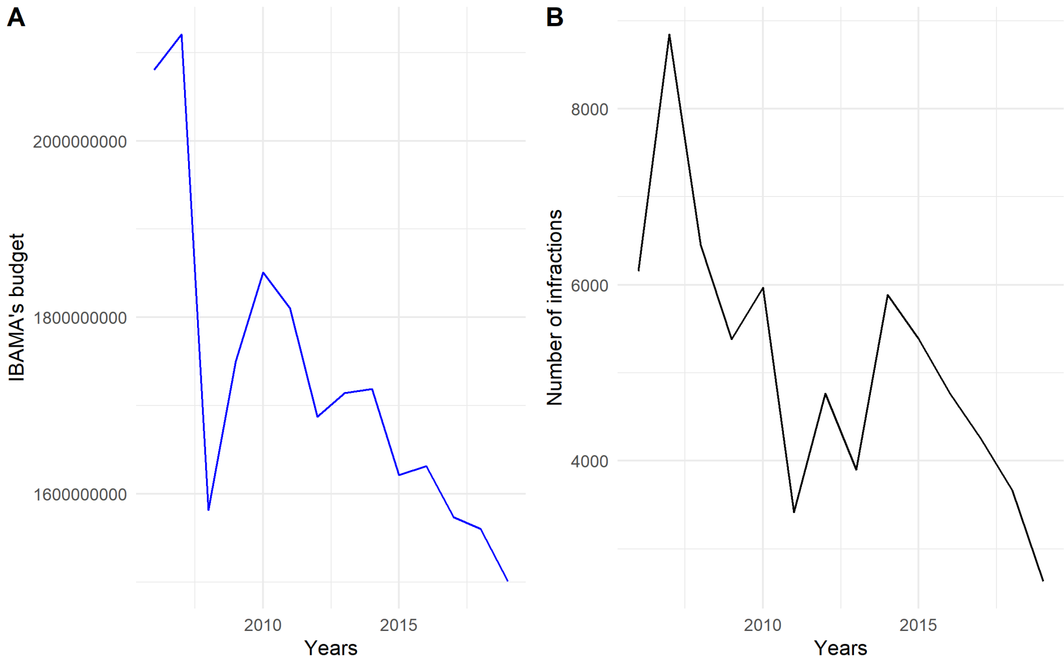

Figure 5.3: Time series of IBAMA's Budget and its infraction notices

Data source: Brazilian Federal Government

can assume that the diminishing of resource amount focused on environmental conservation, and the weakening of environmental institutions did impact the effect of the policy.

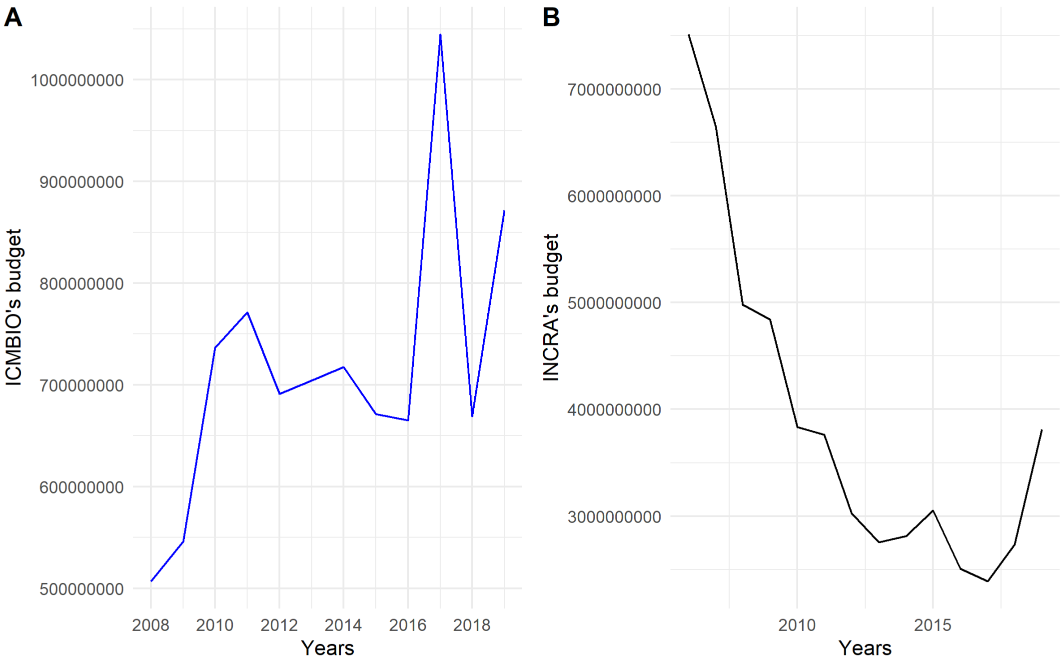

Figure 5.4: Time series of INCRA and ICMBio's Budget

Data source: Brazilian Federal Government

Chapter 6

Conclusion

As stated before, many policies aiming at deforestation have been created in the last couple of decades. In 2008, the Brazilian federal government made "Priority Municipalities" in which municipalities that deforested the most, according to multiple criteria, were added to a list to receive harsher monitoring. Throughout this work, we were interested in understanding if the policy was effective when created, how the effect of this policy changed over the past years, and obtain a vague insight regarding the reason why this has happened.

To do this, we used DiD, with variations. For the first question, we used DiD with Propensity Score Matching (DiD-PSM), where the probability of being listed in 2008 was created for all municipalities, and, then, we did the matching without replacement. With that, we were able to create pairs of listed municipalities and unlisted ones. After the matching, we made use of the classic DiD, taking parallel trends for granted.

We found that with our sample of 68 municipalities and main specification, the policy was effective when created. According to it, being listed in 2008 implied a decrease in deforestation per municipal area. Our findings suggest that, in 2008, listed municipalities decreased deforestation per km^2 by around 0.003.

Next, we aimed to understand how the policy evolved over the years. To answer this question, we used staggered DiD. We found that the policy's effect wasn't homogeneous. It seems that it was more efficacious for earlier adopters, that is, for municipalities listed until 2012.

Following this, we tried to understand the reasons behind the weakening of the policy. We find evidence to support both of our hypotheses, that is, the policy is open to political urges, and institutions responsible for monitoring and enforcing the blacklisting policy got less budget over the years.

As for policy recommendations and practicalities, according to our findings, political will and a wrong focus on the budget are two reasons why the policy has been weakened over the years. Thus, we need to strengthen institutions and make them less fragile to political desires. The same goes for the federal budget and the monitoring organizations (IBAMA etc), which must be more rigid. We cannot bear the risk of weakening the inspection of the Amazon simply because the Federation wanted to spend less resources on environmental inspection and monitoring. Additionally, although not analyzed here, studies have shown how the list may not be optimal (Assunção et al., 2019). For that, it is important to understand the criteria used to insert municipalities on the list, and examine if they suit well.

Finally, there are some open gaps and issues that the present work did not explore or dealt with. Firstly, there is probably spatial spillover, impending a better estimation of the policy (Assunção et al., 2019). It is possible, however, to deal with DiD with local spatial spillover. Since the displacement effect is the main source of spillover, we could model it and incorporate it into our regression (Butts, 2021).

Besides that, we could not further explore the reasons behind the policy efficiency changes, and future works could deepen this knowledge. Lastly, our sample is not large due to data limitations and, due to how the policy was designed, we couldn't accomplish totally all assumptions of the method. Nonetheless, we believe we could capture, at least partially, the effects of being blacklisted, the reasons behind the policy decaying, and add insights to the literature.

Appendix A

Appendix: Images & Tables

Figure A.1: Timeline of listed municipalities grouped by Federative units

Data source: Brazilian Federal Government

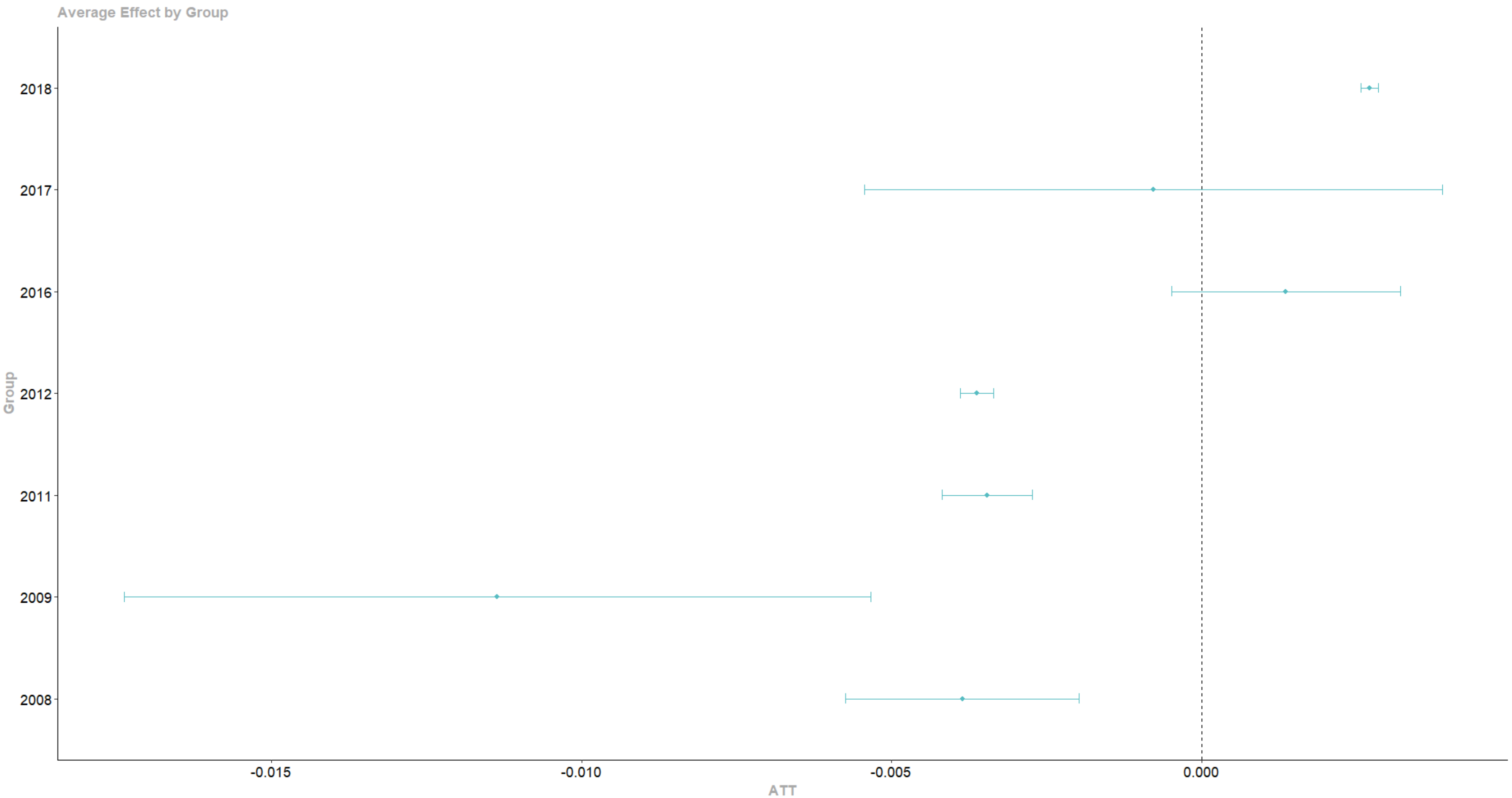

Figure A.2: ATT by Group

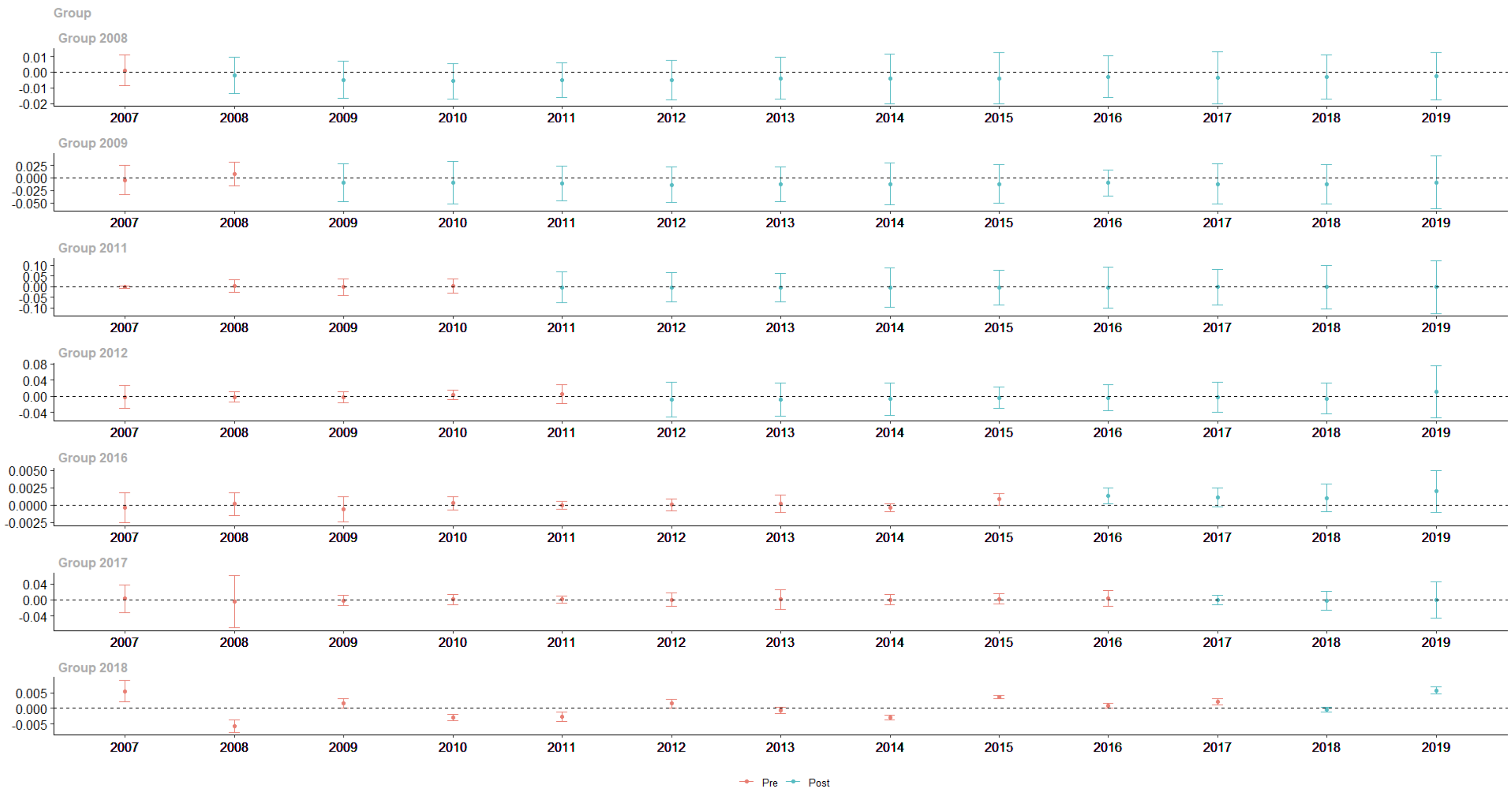

Figure A.3: Timeline of the coefficients

Table A.1: Average Treatment Effect per group, from 2007 to 2019

Group	Time	ATT(g,t)	Std.Error	[95% Simult. Conf. Band]
2008	2007	0.0012	0.0008	[-0.0105, 0.0130]
2008	2008	-0.0020	0.0010	[-0.0158, 0.0118]
2008	2009	-0.0049	0.0010	[-0.0196, 0.0098]
2008	2010	-0.0056	0.0009	[-0.0187, 0.0074]
2008	2011	-0.0050	0.0009	[-0.0181, 0.0081]
2008	2012	-0.0051	0.0010	[-0.0199, 0.0096]
2008	2013	-0.0039	0.0011	[-0.0201, 0.0123]
2008	2014	-0.0042	0.0012	[-0.0217, 0.0132]
2008	2015	-0.0038	0.0013	[-0.0230, 0.0153]
2008	2016	-0.0028	0.0011	[-0.0179, 0.0123]
2008	2017	-0.0034	0.0013	[-0.0223, 0.0156]
2008	2018	-0.0030	0.0012	[-0.0201, 0.0141]
2008	2019	-0.0025	0.0012	[-0.0203, 0.0153]
2009	2007	-0.0041	0.0024	[-0.0389, 0.0308]
2009	2008	0.0080	0.0019	[-0.0199, 0.0359]
2009	2009	-0.0101	0.0030	[-0.0535, 0.0332]
2009	2010	-0.0094	0.0035	[-0.0591, 0.0402]
2009	2011	-0.0106	0.0029	[-0.0522, 0.0310]
2009	2012	-0.0136	0.0029	[-0.0552, 0.0280]
2009	2013	-0.0127	0.0028	[-0.0530, 0.0277]
2009	2014	-0.0125	0.0034	[-0.0607, 0.0358]
2009	2015	-0.0120	0.0032	[-0.0575, 0.0336]
2009	2016	-0.0103	0.0021	[-0.0408, 0.0202]
2009	2017	-0.0125	0.0032	[-0.0591, 0.0342]
2009	2018	-0.0125	0.0030	[-0.0559, 0.0308]
2009	2019	-0.0088	0.0043	[-0.0703, 0.0526]
2011	2007	-0.0026	0.0005	[-0.0094, 0.0041]
2011	2008	0.0020	0.0013	[-0.0174, 0.0214]
2011	2009	-0.0027	0.0017	[-0.0274, 0.0220]
2011	2010	0.0030	0.0015	[-0.0181, 0.0241]
2011	2011	-0.0035	0.0030	[-0.0467, 0.0398]
2011	2012	-0.0034	0.0029	[-0.0450, 0.0383]

Group	Time	ATT(g,t)	Std.Error	[95% Simult. Conf. Band]
2011	2013	-0.0043	0.0029	[-0.0462, 0.0375]
2011	2014	-0.0042	0.0038	[-0.0596, 0.0512]
2011	2015	-0.0046	0.0035	[-0.0550, 0.0459]
2011	2016	-0.0033	0.0040	[-0.0614, 0.0547]
2011	2017	-0.0026	0.0035	[-0.0523, 0.0470]
2011	2018	-0.0027	0.0042	[-0.0638, 0.0585]
2011	2019	-0.0026	0.0051	[-0.0764, 0.0712]
2012	2007	-0.0018	0.0022	[-0.0337, 0.0302]
2012	2008	-0.0020	0.0010	[-0.0164, 0.0125]
2012	2009	-0.0020	0.0011	[-0.0173, 0.0133]
2012	2010	0.0030	0.0010	[-0.0111, 0.0171]
2012	2011	0.0056	0.0019	[-0.0215, 0.0327]
2012	2012	-0.0084	0.0035	[-0.0587, 0.0420]
2012	2013	-0.0084	0.0033	[-0.0565, 0.0396]
2012	2014	-0.0074	0.0033	[-0.0546, 0.0397]
2012	2015	-0.0037	0.0021	[-0.0339, 0.0265]
2012	2016	-0.0038	0.0026	[-0.0419, 0.0343]
2012	2017	-0.0023	0.0030	[-0.0459, 0.0413]
2012	2018	-0.0055	0.0030	[-0.0487, 0.0377]
2012	2019	0.0105	0.0052	[-0.0646, 0.0856]
2016	2007	-0.0004	0.0001	[-0.0025, 0.0017]
2016	2008	0.0002	0.0001	[-0.0017, 0.0021]
2016	2009	-0.0005	0.0001	[-0.0025, 0.0014]
2016	2010	0.0003	0.0001	[-0.0007, 0.0012]
2016	2011	-0.0001	0.0000	[-0.0008, 0.0006]
2016	2012	0.0001	0.0001	[-0.0009, 0.0010]
2016	2013	0.0001	0.0001	[-0.0011, 0.0014]
2016	2014	-0.0004	0.0000	[-0.0010, 0.0002]
2016	2015	0.0009	0.0001	[0.0001, 0.0016]*
2016	2016	0.0013	0.0001	[0.0002, 0.0024]*
2016	2017	0.0011	0.0001	[-0.0001, 0.0023]
2016	2018	0.0010	0.0001	[-0.0010, 0.0030]
2016	2019	0.0020	0.0002	[-0.0010, 0.0049]

Group	Time	ATT(g,t)	Std.Error	[95% Simult. Conf. Band]
2017	2007	0.0026	0.0027	[-0.0361, 0.0413]
2017	2008	-0.0045	0.0053	[-0.0806, 0.0716]
2017	2009	-0.0021	0.0010	[-0.0164, 0.0122]
2017	2010	0.0012	0.0010	[-0.0135, 0.0159]
2017	2011	0.0009	0.0006	[-0.0083, 0.0102]
2017	2012	0.0002	0.0013	[-0.0185, 0.0188]
2017	2013	0.0008	0.0018	[-0.0249, 0.0266]
2017	2014	0.0003	0.0009	[-0.0131, 0.0136]
2017	2015	0.0019	0.0010	[-0.0125, 0.0164]
2017	2016	0.0031	0.0015	[-0.0183, 0.0245]
2017	2017	-0.0004	0.0009	[-0.0133, 0.0125]
2017	2018	-0.0021	0.0018	[-0.0287, 0.0245]
2017	2019	0.0001	0.0037	[-0.0531, 0.0533]
2018	2007	0.0055	0.0003	[0.0016, 0.0095]*
2018	2008	-0.0056	0.0002	[-0.0079, -0.0034]*
2018	2009	0.0015	0.0001	[-0.0002, 0.0033]
2018	2010	-0.0029	0.0001	[-0.0039, -0.0018]*
2018	2011	-0.0027	0.0001	[-0.0045, -0.0008]*
2018	2012	0.0016	0.0001	[-0.0001, 0.0033]
2018	2013	-0.0007	0.0001	[-0.0019, 0.0005]
2018	2014	-0.0029	0.0001	[-0.0038, -0.0020]*
2018	2015	0.0035	0.0000	[0.0030, 0.0041]*
2018	2016	0.0008	0.0001	[0.0000, 0.0017]
2018	2017	0.0021	0.0001	[0.0010, 0.0033]*
2018	2018	-0.0004	0.0001	[-0.0012, 0.0005]
2018	2019	0.0058	0.0001	[0.0042, 0.0073]*

—

Signif. codes: '' confidence band does not cover 0*

P-value for pre-test of parallel trends assumption: 0

Control Group: Never Treated, Anticipation Periods: 0

Estimation Method: Doubly Robust

Appendix B

Appendix: Normalized and log-transformed deforestation increase

For robustness, we present in this Appendix the regression results when we change the dependent variable from deforestation divided by municipal area. Here we use normalized deforestation increase first. Then, we use log-transformed deforestation increase. For each case, the sample selection (from the matching part) is redone, so the propensity score is more accurate.

B.1 The Policy in 2008

The first Table is similar to the structure presented during 5 but we change the dependent variable to normalized deforestation increase. Our main specification corroborates our main findings.

Next, in Table B.2 we change the dependent variable to log linearized deforestation. The results, for the last column, are still similar. According to it, being listed in 2008, reduced the deforestation increase by approximately 45%.

Dependent Variable:	Normalized_Deforestation		
Model:	(1)	(2)	(3)
Variables			
(Intercept)	-0.3217***	-0.2297***	
	(0.0211)	(0.0334)	
time × listed	-0.1684***	-0.1629***	-0.4496***
	(0.0320)	(0.0349)	(0.1063)
GDP_per_deflated		-1.81×10^{-6}	-5.22×10^{-7}
		(1.44×10^{-6})	(2.83×10^{-6})
PPA		0.0003	-0.0882
		(0.0243)	(0.0580)
PPB		$-1.3 \times 10^{-5***}$	$-5.43 \times 10^{-5**}$
		(4.88×10^{-6})	(2.62×10^{-5})
Population_Density		-0.0017	-0.0517
		(0.0029)	(0.0414)
Fixed-effects			
Municipality			Yes
Year			Yes
Fit statistics			
Observations	966	966	966
R^2	0.02797	0.04379	0.27406
Within R^2			0.05424

*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table B.1: Diff-in-diff with Propensity Score, for 2008, where the dependent variable is normalized deforestation increase

Dependent Variable:	Ln_Deforestation		
Model:	(1)	(2)	(3)
Variables			
(Intercept)	2.557***	3.348***	
	(0.0635)	(0.0892)	
time × listed	1.074***	1.007***	-0.6466***
	(0.0961)	(0.0930)	(0.1535)
GDP_per_deflt		1×10^{-6}	2.02×10^{-6}
		(3.86×10^{-6})	(3.86×10^{-6})
PPA		-0.3140***	-0.2078**
		(0.0649)	(0.0801)
PPB		-0.0002***	$-9.42 \times 10^{-5**}$
		(1.33×10^{-5})	(4.02×10^{-5})
Population_Density		-0.0032	-0.0914*
		(0.0078)	(0.0544)
Fixed-effects			
Municipality			Yes
Year			Yes
Fit statistics			
Observations	963	963	963
R^2	0.11492	0.31060	0.84680
Within R^2			0.06527

*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table B.2: Diff-in-diff with Propensity Score, for 2008, where the dependent variable is log linearized deforestation increase

B.2 Staggered treatment

Now, we present the staggering treatment effect, but changing the dependent variable. As we can see, the main result, that the policy was effective when created but its effectiveness was diminished as years passed, still verifies in both regressions. Nonetheless, this effect is weaker. With Table B.4 we can see that in 2009, specifically, being listed was responsible for decreasing deforestation by around 70%.

Table B.3: Average Treatment Effect for all groups, using normalized deforestation as dependent variable

Group	Estimate	Std.Error	[95% Simult. Conf. Band]
2008	-0.4653	0.1173	[-0.72, -0.2107]*
2009	-1.5989	0.4376	[-2.5484, -0.6495]*
2011	-0.1442	1.6761	[-3.7813, 3.493]
2012	-0.6469	0.6668	[-2.0939, 0.8001]
2016	0.7052	0.4228	[-0.2122, 1.6227]
2017	-0.0012	0.1754	[-0.3819, 0.3794]
2018	0.0666	0.0684	[-0.0819, 0.2151]

Table B.4: Average Treatment Effect for all groups, using log linearized deforestation as dependent variable

Group	Estimate	Std.Error	[95% Simult. Conf. Band]
2008	-0.2774	0.13	[-0.638, 0.0831]
2009	-0.6933	0.1705	[-1.1662, -0.2205]*
2011	0.2559	0.6017	[-1.4125, 1.9242]
2012	-0.4982	0.3381	[-1.4358, 0.4393]
2016	-0.1925	0.3201	[-1.08, 0.6951]
2017	-0.2244	0.094	[-0.4849, 0.0361]
2018	0.0925	0.0683	[-0.0969, 0.2819]

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