Spatial difference-in-differences and event study: identification and application to the case of Priority List of Municipalities in the Brazilian Amazon

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Abstract

Difference-in-differences (DID) has long been a staple in estimating treatment effects in applied econometrics, with recent advancements relaxing traditional assumptions to explore heterogeneous and spillover effects. While heterogeneous effects analysis examines causal impacts across diverse groups and periods, spillover effects analysis delves into the influence of treatments on neighboring units. Incorporating spatial dependence within the DID framework, Spatial Difference-in-Differences (SDID) models have emerged as a powerful tool for analyzing such effects, particularly in settings where observations represent fixed geographical units. This study contributes to the literature by explicitly formalizing underlying assumptions and employing an SDID model to analyze the impact of Brazil's Priority Municipalities List on deforestation in the Amazon region. Utilizing both

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traditional DID and SDID methodologies, we uncover significant reductions in deforestation odds ratios within listed municipalities and neighboring unlisted municipalities. Furthermore, we introduce an event study approach linked with SDID to explore the policy's anticipatory effects. Our findings underscore the effectiveness of the Priority Municipalities List in curbing deforestation and highlight the importance of spatially explicit methodologies in environmental policy evaluation. This article advances methodological discussions surrounding SDID estimation and provides empirical insights into the efficacy of targeted environmental policies in combating deforestation in sensitive ecosystems like the Amazon.

Keywords: Spatial diff-in-diff, spillover effects, spatial event study, causal inference, deforestation, Brazilian Amazon

JEL Code: C21, C23, K32, Q5, R11

1 Introduction

Difference-in-differences (DID) stands as a cornerstone quasi-experimental method within applied econometrics, widely employed to estimate treatment effects (Ashenfelter and Card, 1985; Card, 1990; Card and Krueger, 1994; Meyer et al., 1995; Angrist and Krueger, 2000; Bertrand et al., 2004; Angrist and Pischke, 2008; Athey and Imbens, 2006; Lechner et al., 2011). However, recent advancements in econometric techniques have brought about a critical shift, relaxing traditional assumptions underlying DID analyses. Key among these is the recognition that real-world scenarios often exhibit heterogeneous and spillover effects stemming from treatments.

In acknowledging heterogeneous effects, scholars such as Callaway and Sant'Anna (2021); De Chaisemartin and d'Haultfoeuille (2023); Wing et al. (2024) have introduced staggered treatment effect analysis, enabling the examination of causal effects across distinct groups and time periods. Similarly, with regards to spillover effects, researchers like Dubé et al. (2014); Delgado and Florax (2015); Chagas et al. (2016) have integrated spatial dependence within the DID framework, focusing on the transmission of treatment effects onto neighboring units. This becomes particularly pertinent when dealing with spatially fixed observations, where spatial correlation in treatments and responses can manifest.

It is noteworthy that while DID assumes the Stable Unit Treatment Value Assumption (SUTVA), scenarios exist where this assumption needs to be relaxed, especially in the presence of spatial spillovers or network effects. For instance, Dubé et al. (2014) and Delgado and Florax (2015) consider cases where SUTVA may hold for outcome variables but not for treatment variables, leading to biased treatment effect estimates (Kolak and Anselin, 2020). These studies underscore the necessity of accounting for spatial interdependence, as overlooking it can result in significant biases, as demonstrated through simulations by Delgado and Florax (2015).

Further complicating matters are scenarios where spillovers occur within a network, such as in social networks or supply chains. In such cases, the assumption of no interference between units may not hold, necessitating a flexible approach to treatment effect estimation. By embracing spatial diff-in-diff methodologies, researchers can better capture these complex interactions, thereby advancing our understanding of how treatments diffuse through spatial and networked systems.

In this study, we build upon these advancements by explicitly formalizing the underlying assumptions and employing a spatial diff-in-diff model. We conducted a Monte Carlo Simulation to further assess the performance of the proposed estimator in identifying spillover effects on treated and untreated regions. The simulation results reveal that our estimator outperforms alternative methods in accurately capturing and quantifying spillover effects in treated and untreated areas. By systematically varying parameters and scenarios, we could robustly evaluate the estimator's performance across various conditions, providing confidence in its effectiveness for capturing the complexities of spatial diffusion processes. We also apply this model to analyze the impact of the Brazilian Amazon's Priority Municipalities List on deforestation, incorporating an event study approach to explore anticipatory effects. Through our empirical analysis, we contribute to the growing body of literature on spatial diff-in-diff, shedding light on the spatial dynamics of treatment effects and their implications for policy and decision-making.

The Monte Calo simulation and the application validate our findings, reinforcing the reliability and applicability of our spatial diff-in-diff approach to uncovering the nuanced dynamics of treatment effects in spatially interconnected environments.

This article is structured as follows: Section 2 describes the spatial difference-in-differences estimator and the established conditions for effect identification. Section 3 presents an application of the method, including an event study and spatial event study analysis. The final section concludes.

2 Treatment and Spillover

Consider a model with two time periods, $t = 1, 2$. Units indexed by i are drawn from one of two populations. Units of the treated population ($D_i = 1$) receive a treatment of interest between period $t = 1$ and $t = 2$, whereas units of the untreated population (comparison or control) ($D_i = 1$) remain untreated in both periods.

The treatment effect spills over to regions close to the treated regions. For simplicity, consider the situation in that the region has only one neighbor, j. Let $Y_{i,t}(D_i = 0, D_j = 0)$ denote unit i's potential outcome in t if i and their neighbor j remain untreated in both periods. Similarly, $Y_{i,t}(D_i = 1, D_j = 0)$ denotes unit i's potential outcome in t if i is exposed to treatment by the second period and their neighbor j is untreated in both periods. Let $Y_{i,t}(D_i = 0, D_j = 1)$ denote unit i's potential outcome in t if i remains untreated in both periods and their neighbor j is untreated in the first period but exposed to treatment by the second period. Finally, $Y_{i,t}(D_i = 1, D_j = 1)$ denotes unit i's potential outcome in period t if i and their neighbor j is untreated in the first period but exposed to treatment by the second period.

For notation simplicity, we denote these situations, respectively, by $Y_{i,t}(00), Y_{i,t}(10), Y_{i,t}(01)$, and $Y_{i,t}(11)$.

Let D_i a dummy variable identifying if i region is treated and D_j a dummy variable identifying if j neighbor region j is treated. The observed outcome is given by

$$Y_{i,t} = D_i D_j Y_{i,t}(11) + D_i(1 - D_j)Y_{i,t}(10) + (1 - D_i)D_j Y_{i,t}(01) + (1 - D_i)(1 - D_j)Y_{i,t}(00) \tag{1}$$

We are interested in the average treatment effect on the treated (ATET) in period $t = 2$. There is a direct treatment effect on i, given that the neighbor j is untreated compared to the situation where both are untreated

$$ATET : \tau_{10} = E[Y_{i,2}(10) - Y_{i,2}(00) D_i = 1, D_j = 0] \tag{2}$$

There is also an average spillover effect on the untreated (ASENT), i.e., the spillover effect on i when j is treated compared to the situation where both are untreated

$$ASENT : \tau_{01} = E[Y_{i,2}(01) - Y_{i,2}(00) D_i = 0, D_j = 1] \tag{3}$$

And there is a composed effect, direct and indirect, when both regions are treated compared to the situation where both are untreated

$$\tau_{11} = E[Y_{i,2}(11) - Y_{i,2}(00) D_i = 1, D_j = 1] \tag{4}$$

We define the Average Spillover Effect on Treated (ASET) as the difference between the composed effect (τ_{11}) and the ATET effect (τ_{10}), then

$$ASET : \tilde{\tau}_{11} = \tau_{11} - \tau_{10} \tag{5}$$

We must change the usual hypothesis of the classical diff-in-diff approach to identify the three effects. The identification of the treatment effect, in general, assumes three assumptions: (i) homogeneity and no interference, (ii) parallel trend, and (iii) no anticipatory effects. Assumption (i) is usually called Stable Unit Treatment Value Assumption (SUTVA). We need to relax the SUTVA to identify spillover effects, specifically the no interference assumption.

The homogeneity assumption refers to the potential homogeneity of the effect over different regions. In other words, the potential effect of the treatment is the same, regardless of the region where the treatment is applied. This assumption is important to identify the average treatment effect from observed sample means.

Assumption 1 (Homogeneity)**.** *For each unit, there are no different forms or versions of each treatment level, which leads to different potential outcomes.*

$$E[Y_{i,t}(D)|D_i = D] = E[Y_{j,t}(D)|D_j = D],\ \textit{for all}\ (i,j) \tag{6}$$

That is, the potential Y value for a region in the treated group is the same for all regions, and the same occurs for the untreated group.

To identify the causal spillover effects, we assume an adapted version of the parallel trend assumption because now we assume that treatment on one region can impact the neighbor, as follows:

Assumption 2 (Parallel trend with spillover)**.**

$$E[Y_{i,2}(00) - Y_{i,1}(00)|D_i = 1, D_j = 1] = E[Y_{i,2}(00) - Y_{i,1}(00)|D_i = 0, D_j = 0] \tag{7a}$$

$$E[Y_{i,2}(00) - Y_{i,1}(00)|D_i = 1, D_j = 0] = E[Y_{i,2}(00) - Y_{i,1}(00)|D_i = 0, D_j = 0] \tag{7b}$$

$$E[Y_{i,2}(00) - Y_{i,1}(00)|D_i = 0, D_j = 1] = E[Y_{i,2}(00) - Y_{i,1}(00)|D_i = 0, D_j = 0] \tag{7c}$$

The parallel trend with spillover assumption preserves the same idea of the classical approach. It requires that the difference between the treated and untread groups is constant over time; in the absence of treatment, it does not matter if the neighbor is or is not treated.

Assumption 3 (No anticipatory effects with spillover)**.**

$$Y_{i,1}(00) = Y_{i,1}(01) = Y_{i,1}(10) = Y_{i,1}(11),\ \textit{for all}\ i\ \textit{and}\ j. \tag{8}$$

The no-anticipation assumption states that the treatment has no causal effect before its implementation. This is important for the identification of τ_{10}, τ_{01} or τ_{11}, since otherwise, the changes in the outcome for the treated group between period 1 and 2 could reflect not just the causal effect in period $t = 2$ but also the anticipatory effect in period $t = 1$ (Abbring and van den Berg, 2003; Malani and Reif, 2015).

Proposition 1. *Under the assumption 1-3, the ATET (τ_{10}), ASENT (τ_{01}) and the spillover effect on treated, ASET ($\tilde{\tau}_{11}$) in period 2 are idenfied.*

Proof. The proof is in the appendix A. □

Things are more complicated when we consider the (more realistic) situation in which regions have more than one neighbor. We still consider that the treatment effect spills over to regions close to the treated regions and extends to regions with two or more neighbors.

Consider a case with two neighbors, j and k, to present the notation. Let $Y_{i,t}(D_i = 0, D_j = 0, D_k = 0)$ denote unit i's potential outcome in period t if i and their neighbors j and k remains untreated in both periods. For short, $Y_{i,t}(D_i = 0, D_j = 0, D_k = 0)$. We can generalize the notation for more than two neighbors considering $Y_{i,t}(0, D_{ij})$ denote unit i's potential outcome in period t if i is untreated, and D_{ij} now representing the indicator if neighbors of i are treated or untreated in the second period. Then, $D_{ij} = 0$ when the j-th neighbor is untreated, and $D_{ij} = 1$ when it is treated. In the same way, $Y_{i,t}(1, D_{ij}$ denotes unit i's potential outcome in period t if i is untreated in the first period but exposed to treatment and D_{ij} the indicator if their neighbors are treated or untreated in the second period.

We must introduce additional assumptions to identify the causal effect in this more general context. The first assumption to introduce now considers if it makes a difference in the neighbor's position in the network. It is not a huge problem if the network is knowledge. The research eventually wants to test the causal effect considering a given neighborhood structure, like a hierarchical system of cities or a given spatial position. It is the assumption under the spatial difference-in-difference models (Delgado and Florax, 2015; Chagas et al., 2016; Yan et al., 2022).

In a situation with two neighbors, we consider the case where the spillover effect is the same if one or another neighbor is treated. It is the homogeneity assumption in the case of spillover in a network system. We called this *isopotropic* assumption.

Assumption 4 (Isotropic Assumption)**.** *The spillover effect is independent of the direction:*

$$Y_{i,t}(110) = Y_{i,t}(101) \tag{9}$$

$$Y_{i,t}(010) = Y_{i,t}(001) \tag{10}$$

This assumption can be extended to the situation with more than two neighbors.

$$Y_{i,t}(1, D_{ij}) = Y_{i,t}(1, D_{ik}) \tag{11}$$

$$Y_{i,t}(0, D_{ij}) = Y_{i,t}(0, D_{ik}) \tag{12}$$

where D_{ij} and D_{ik} represent different arrangement of i's region neighbors.

The isotropic assumption allows estimating the indirect effect for an untreated region, for instance, with two neighboring regions, one treated and the other not, compared to the situation in which none of the regions are treated: $\tau_{010} = \tau_{001}$ or $\tau_{110} = \tau_{101}$. However, this effect is not necessarily the same as if a different number of neighboring regions are treated. In some situations, exposure to the treatment is enough to guarantee some effect, but in others, the intensity of exposure matters. This may also be true for spillovers. Regions with more treated neighbors may have more impact than regions with only one treated neighbor. We consider that spillover treatment effect has an additive component effect, that is,

Assumption 5 (Aditivity)**.** *The spillover effect is additive. Then, a region with more neighbors treated has at least the same indirect impact as a region with fewer neighbors:*

$$\tau_{011} \geq \tau_{001} \tag{13}$$

$$\tau_{111} \geq \tau_{101} \tag{14}$$

As a consequence of assumption 5, we can decompose the spillover effect into each treated neighbor's effect. When a region has more than one treated neighbor, the result of the spillover may be decomposed in the effect of each neighbor treated, plus a combined effect, which amplifies or reduces the intensity of the spillover effect. Consider the situation in which only two neighbors exist for each region, and both are treated for a given region. Then

$$\tau_{011} = \tau_{000} + \tau_{010} + \tau_{001} + \tau_{000}\tau_{010} + \tau_{000}\tau_{001} + \tau_{010}\tau_{001} \tag{15}$$

In other words, the spillover effect is given by the sum of the effects in which none of the neighbors are treated (τ_{000}), the effects in which only one of the neighbors is treated (τ_{010} and τ_{001}), plus all possible interactions between these effects. Where the region itself is not treated, τ_{000} represents the counterfactual situation of absence of any treatment and spillover, and, therefore, this effect is null, as are their interactions. Consequently,

$$\tau_{011} = \tau_{010} + \tau_{001} + \tau_{010}\tau_{001} \tag{16}$$

In the situation where the region itself is also treated, the decomposition becomes

$$\tau_{111} = \tau_{100} + \tau_{010} + \tau_{001} + \tau_{100}\tau_{010} + \tau_{100}\tau_{001} + \tau_{010}\tau_{001} \tag{17}$$

The interaction effect can be of interest. It can be the case where the spillover effect is non-linear behavior and may depend on the intensity of the treated neighbors. Specific work can be done in the future, considering this situation. In the present case, we will consider the situation where the interaction effect is negligible. In that case, regions with some neighbors not treated would receive a smaller spillover effect. How this spillover effect decays with the number of untreated neighbors can vary from case to case and can be an empirical question. Consider, for instance, the situation with two neighbors. If both are treated, the spillover effect can be higher than if just one is treated; that is,

$$\tau_{010} = \lambda(\eta)\tau_{011} \tag{18}$$

Where $0 \leq \lambda(\eta) \leq 1$ is a function of η, the proportion of treated neighbors. For each region, $\eta_i = \sum_j \frac{1[D_{ij}=1]}{N_i}$,with N_i the total number of neighbors in that region i.

Assumption 6 (Decomposition)**.** *The spillover effect can be decomposed into the effects of each treated neighbor. The interaction effect is negligible.*

Assumption 7 (Proportionality)**.** *The spillover effects are proportional to the number of neighbors treated, i.e.,*

$$\tau_{0,D_j} = \sum_j \lambda D_j \tau_{(0,\mathbf{1})} \tag{19}$$

$$\tau_{1,D_j} = \sum_j \lambda D_j \tau_{(1,\mathbf{1})} \tag{20}$$

where $\mathbf{1}$ *represents the situation where* $D_j = 1$ *for all* j*.*

Proposition 2. *Under assumptions 1-7, a two-way fixed effect with spatial controls, as in*

$$Y_{i,t} = \beta^d D_{i,t} \times t + \beta^t \sum_j \lambda_i D_{i,t} D_{j,t} \times t + \beta^u \sum_j \lambda_i (1 - D_{it}) D_{j,t} \times t + \mu_i + \delta_t + u_{i,t} \tag{21}$$

can identify the treatment effect as

- *a) ATET (Average Treatment Effect Direct on Treated):* $\tau_{1,\mathbf{0}} = \beta^d$
- *b) ASET (Average Spillover Effect on Treated):* $\tilde{\tau_{1,\mathbf{1}}} = \beta^t$
- *c) ASENT (Average Spillover Effect on Untreated):* $\tau_{0,\mathbf{1}} = \beta^u$

Proof. The proof is in the appendix A. □

Let w_{ij}^{NS}, $\forall i, j \in N$ be the neighborhood relationship between region i and region j, and let W^{NS} be the matrix that relates each region under study to all the rest. We will follow the usual assumptions related to spatial models. For each $j \neq i$, we can define if it is or not a neighbor of i for some criteria. Different criteria will result in different neighbor relationships. The most common neighborhood criteria include contiguity, k nearest neighbors, inverse distance, etc. Neighborhood relationships must be linked with the expected spillover effects for the causal inference proposal. Usual classification in a non-spatial approach includes any kind of clusters, like classmates and workplaces. We can use an indicator to assign a determined region j to their neighbors, i. This assignment could not ensure asymptotic convergence once neighbors increased like the sample. Then, the neighborhood matrix used to be standardized. The most usual standardization attending assumption 8 b) is the row standardization, $w_{ij} = \frac{(w_{ij}^{NS})}{\sum_j w_{ij}^{NS}} = \frac{w_{ij}^{NS}}{N_i}$

Assumption 8 (Neighborhood matrix)**.** *The neighborhood matrix is*

i A region is not a neighbor of itself, i.e., $w_{ii}^{NS} = 0$.

ii The row and column sums of the standardized matrices W *are bounded uniformly in absolute value. More specifically, this restriction means that the sum of all neighborhood weights for a given region equals 1, that is,* $\sum_j w_{ij} = 1$.

In a standard application, $\lambda_{i,t} D_{j,t}$ can be defined as

$$\lambda_{i,t} D_{j,t} = \sum_j w_{ij} D_{j,t} \tag{22}$$

And then,

Proposition 3. *Under assumptions 1-8, the spillover effect can be estimated using a spatial two-way fixed effect model*

$$Y_{i,t} = \beta^d D_{i,t} + \beta^t D_{i,t} \sum_j w_{ij} D_{j,t} + \beta^u (1 - D_{i,t}) \sum_j w_{ij} D_{j,t} + \mu_i + \delta_t + \varepsilon_{i,t} \tag{23}$$

Proof. The proof is a consequence of proposition 2 and (22). □

Equation (23) is similar to the spatial diff-in-diff model by Chagas et al. (2016), referred for them as the unrestricted model. They also proposed a restricted version that coincides with the estimator by Delgado and Florax (2015), as follows:

$$Y_{i,t} = \beta^d D_{i,t} + \beta^r \sum_j w_{ij} D_{j,t} + \mu_i + \delta_t + \varepsilon_{i,t} \tag{24}$$

In this case, the spillover effects on treated and untreated neighbors are jointly estimated (hence the term restricted effect).

Event study in the context of difference-in-differences is widely used to analyze the impact of specific events on a particular variable of interest in economics, finance, and other fields. It is especially useful for assessing the dynamic effects of the treatment, exploring eventual anticipatory behavior, or reinforcing results over a long time, The relevance of event study with diff-in-diff lies in its ability to provide empirically solid evidence on the effects of specific events. There is no harm to identification in (23) and (24) with the introduction of lagged and anticipated effects, as is common in event study specifications. However, the spatial dimension also allows us to analyze the spatial lag and lead, considering the eventual anticipatory effect (lag) or reinforcement effect (lead). Thus,

$$\begin{aligned} Y_{i,t} = & \sum_{k=-p}^{q} \beta^{d_k} D_{i,t-k} + \sum_{k=-p}^{q} \beta^{t_k} D_{i,t-k} \sum_j w_{ij} D_{j,t-k} + \\ & \sum_{k=-p}^{q} \beta^{u_k} (1 - D_{i,t-k}) \sum_j w_{ij} D_{j,t-k} + \mu_i + \delta_t + \varepsilon_{i,t} \end{aligned} \tag{25}$$

$$Y_{i,t} = \sum_{k=-p}^{q} \beta^{d_k} D_{i,t-k} + \sum_{k=-p}^{q} \beta^{r_k} \sum_j w_{ij} D_{j,t-k} \mu_i + \delta_t + \varepsilon_{i,t} \tag{26}$$

In these equations, $\sum_{k=-p}^{q} \beta^{d_k} D_{i,t-k}$ captures the dynamic effects of lag and leads of the treatment. β^{d_k}, when $j = 0$, represents the treatment effect β^d in equations (23) and (24). When $j > 0$, β^j is interpreted as lagged trend measures, whereas for $j < 0$, β^{d_k} represents future effects on deforestation. In the same way, β^{t_k}, β^{u_k} and β^{r_k} represent the spillover effects under the unrestricted or restricted SDID specification.

Another literature that has grown in recent years concerns the identification of heterogeneous effects related to different treated groups at different points in time and/or for different periods (Callaway and Sant'Anna, 2021; De Chaisemartin and d'Haultfoeuille, 2023; Wing et al., 2024). This is a very active field, and its interactions with space can be explored in the future.

2.1 Monte Carlo Simulation

We run Monte Carlo experiments to study the finite sample performance of the proposed SDID estimator. The true data-generating process is given by

$$y_{it} = \alpha_0 + \alpha_1 x_{it} + \alpha_2 D_{it} + \alpha_3 D_{it} \sum_j w_{ij} D_{jt} + \alpha_4 (1 - D_{ij}) \sum_j w_{ij} D_{jt} + \mu_i + \tau_t + \varepsilon_{it} \tag{27}$$

where x_{1t} was drawn from a standard normal distribution and, to accommodate a common trend factor, like in Delgado and Florax (2015), $x_{i,t} = 1.02 x_{i-1,t}$ for $i \geq 2$. For $t < d_t$, $D_{i,t} = 0$ for all region i. For $t \geq d_t$, the proportion $p = \{.1, .2, .5, .8, .9\}$ of the regions are randomly selected for the treatment following a discrete uniform distribution. We ensure that at least two neighbors also receive the treatment. d_t is fixed as the half of the period, with $t = \{5, 10\}$, and $n = \{30, 50, 100\}$. The regions are selected once and are treated since $t \geq d_t$ until the end. We construct the spatial weight matrix following a circular world (Baltagi and Liu, 2011) in this way: for $i = 1, ..., n/10$ and $i = 2n/10, ..., n$ the i-th row has non-zero elements in positions $i-1$ and $i-1$; for $i = n/10, ..., 2n/10$, the i-th row has non-zero elements in positions $\{i-3, ...i-1, i+1, ..., i+3\}$ if $n \leq 50$ and $\{i-5, ...i-1, i+1, ..., i+5\}$ if $n > 50$. After, the matrix was row normalized. The fixed effects μ_i and τ_t come from a standard normal, in the same way ε_{it}, so that, $\mu_i \sim N(0,1)$, $\tau_t \sim N(0,1)$ and $\varepsilon_{it} \sim N(0,1)$. For the parameters, $\alpha_0 = \alpha_1 = \alpha_2 = 1$, $\alpha_3 = 1$ and $\alpha_4 = \{-1, 1\}$. For each experiment, we perform 10,000 replications. For each replication, we estimate the treatment and the spillover effect, according to the case, using the classical DID, the restricted DID of Delgado and Florax (2015), and the unrestricted SDID. Following Kelejian and Prucha (1999) and Baltagi and Liu (2011), we define *bias* as the difference between the median and the true parameter value and

RMSE is defined as $[bias^2 + (IQ/1.35)^2]^{1/2}$ where IQ is the interquantile range, that is, $IQ = c_1 - c_3$, with c_1 and c_3 the 0.75 and 0.25 quantile, respectively. Kelejian and Prucha (1999) argue that these statistics are closely related to the standard measures of bias and root mean squared error (RMSE) but, unlike these measures, are guaranteed to exist.

The three estimators have about a good performance in estimating the ATET effect when the treatment and the spillover effect are equal, that is, $\alpha_3 = \alpha_3 = \alpha = 4$. However, the classic diff-in-diff and the Spatial DID restricted underperform when the spillover effect on the neighbor untreated is different ($\alpha_3 = 1$ and $\alpha_4 = -1$). In that situation, SDID unrestricted better estimates the ATET effect. The result is the same if the sample is small, $n = 30$ and $t = 5$ or higher, $n = 100$ and $t = 10$.

The classical DID does not estimate the spillover effect, so the values in panels B and C for this estimator are not reported. The SDID restricted does not differentiate the spillover effect on neighbors treated or untreated; for this, the estimated values are the same in panels B and C for this estimator. Only SDID unrestricted considers different effects on neighbors treated and neighbors untreated. As before, the two estimators have similar performance when the spillover effects are equal, but SDID restricted has a bigger bias and RMSE when $\alpha_3 \neq \alpha_4$. Once again, SDID unrestricted better estimates the ASET and ASETNT effects in the small ($n = 30$ and $t = 5$) or big sample ($n = 100$ and $t = 10$).

3 Empirical Application

Between 2005 and 2012, deforestation in the Brazilian Amazon decreased by 75% due to significant changes in environmental policy. A growing body of literature examining the new measures implemented since 2004 suggests that the primary drivers of this reduction were stricter enforcement and command-and-control policies (Assunção et al., 2013a,b; Maia et al., 2011; Hargrave and Kis-Katos, 2013; Burgess et al., 2016; Souza-Rodrigues, 2019).

Land cover in the Amazon region has been monitored via satellite since 2004, with

Table 1: Bias and RMSE of DID, SDID restricted and SDID unrestricted

			DID		SDID restricted		SDID unrestricted	
n	t	True value	Bias	RMSE	Bias	RMSE	Bias	RMSE
Panel A: ATET estimator								
30	5	$\alpha_2 = \alpha_3$	0.0543	0.3189	−0.0002	0.2738	0.0104	0.5361
30	5	$\alpha_2 \neq \alpha_3$	1.0420	1.3348	1.0617	1.3669	0.0386	0.5130
30	10	$\alpha_2 = \alpha_3$	0.0157	0.2628	−0.0010	0.2355	0.0114	0.4468
30	10	$\alpha_2 \neq \alpha_3$	1.0432	1.3389	1.0580	1.3671	0.0304	0.4539
50	5	$\alpha_2 = \alpha_3$	0.0706	0.3241	0.0001	0.2790	0.0078	0.5545
50	5	$\alpha_2 \neq \alpha_3$	1.0387	1.3020	1.0723	1.3353	0.0477	0.5497
50	10	$\alpha_2 = \alpha_3$	0.0603	0.3097	−0.0002	0.2669	0.0088	0.5433
50	10	$\alpha_2 \neq \alpha_3$	1.0456	1.3130	1.0909	1.3616	0.0562	0.5349
100	5	$\alpha_2 = \alpha_3$	−0.0004	0.2111	−0.0010	0.2017	−0.0167	0.3608
100	5	$\alpha_2 \neq \alpha_3$	1.0145	1.3647	1.0127	1.3881	0.0000	0.4033
100	10	$\alpha_2 = \alpha_3$	−0.0422	0.1765	−0.0011	0.1694	−0.0272	0.2864
100	10	$\alpha_2 \neq \alpha_3$	1.0192	1.3797	1.0247	1.4074	−0.0017	0.3528
Panel B: ASET estimator								
30	5	$\alpha_2 = \alpha_3$			0.0000	0.4106	−0.0016	0.6280
30	5	$\alpha_2 \neq \alpha_3$			−0.9203	1.2626	−0.0020	0.5913
30	10	$\alpha_2 = \alpha_3$			0.0013	0.3491	−0.0011	0.5315
30	10	$\alpha_2 \neq \alpha_3$			−0.9059	1.2215	−0.0002	0.5024
50	5	$\alpha_2 = \alpha_3$			−0.0003	0.4184	−0.0020	0.6365
50	5	$\alpha_2 \neq \alpha_3$			−0.9421	1.2482	−0.0020	0.6058
50	10	$\alpha_2 = \alpha_3$			−0.0002	0.4000	−0.0017	0.6030
50	10	$\alpha_2 \neq \alpha_3$			−0.9046	1.2186	−0.0017	0.5650
100	5	$\alpha_2 = \alpha_3$			0.0007	0.2963	−0.0010	0.4510
100	5	$\alpha_2 \neq \alpha_3$			−1.0121	1.3800	−0.0011	0.4599
100	10	$\alpha_2 = \alpha_3$			0.0014	0.2458	0.0005	0.3825
100	10	$\alpha_2 \neq \alpha_3$			−1.0218	1.4032	−0.0002	0.3812
Panel C: ASENT estimator								
30	5	$\alpha_2 = \alpha_3$			0.0000	0.4106	0.0153	0.5880
30	5	$\alpha_2 \neq \alpha_3$			−0.9203	1.2626	0.0315	0.6081
30	10	$\alpha_2 = \alpha_3$			0.0013	0.3491	0.0186	0.4942
30	10	$\alpha_2 \neq \alpha_3$			−0.9059	1.2215	0.0299	0.5459
50	5	$\alpha_2 = \alpha_3$			−0.0003	0.4184	0.0094	0.6150
50	5	$\alpha_2 \neq \alpha_3$			−0.9421	1.2482	0.0469	0.6393
50	10	$\alpha_2 = \alpha_3$			−0.0002	0.4000	0.0112	0.5986
50	10	$\alpha_2 \neq \alpha_3$			−0.9046	1.2186	0.0554	0.6228
100	5	$\alpha_2 = \alpha_3$			0.0007	0.2963	−0.0318	0.4297
100	5	$\alpha_2 \neq \alpha_3$			−1.0121	1.3800	0.0008	0.4855
100	10	$\alpha_2 = \alpha_3$			0.0014	0.2458	−0.0550	0.3489
100	10	$\alpha_2 \neq \alpha_3$			−1.0218	1.4032	−0.0013	0.4268

DID refers to the classical two-way diff-in-diff estimator.
SDID restricted refers to the Delgado and Florax (2015) spatial diff-in-diff estimation.
SDID unrestricted compute different spillover effects on neighbors treated and neighbors untreated.
Classical DID does not compute the spillover effect; for this, values in panels B and C for this estimator are not reported.
SDID restricted does not differentiate the spillover effect on neighbors treated or untreated. Then, the estimated values are the same in panels B and C.
bias is the difference between the median and the true parameter value, $RMSE = \left[bias^2 + (IQ/1.35)^2\right]^{1/2}$, where IQ is the interquartile range. (Kelejian and Prucha, 1999)

real-time deforestation alerts indicating the location of new clearings to environmental authorities. However, due to dispersed urban centers and the need for more infrastructure in the Amazon, accessing some regions and inspecting identified hot spots is only sometimes feasible. Consequently, policy targeting strategies were developed to enhance the effectiveness of inspection procedures.

Several studies have assessed the effectiveness of the prioritized municipalities list. Arima et al. (2014); Cisneros et al. (2013); Harding et al. (2018); Koch et al. (2019); Cisneros et al. (2015), and Assunção and Rocha (2019) observed significant reductions in deforestation in the listed municipalities. While Assunção and Rocha (2019) identified command-and-control instruments as the main driver of the reduction, Cisneros et al. (2015) argue that other institutional and reputational pressures were decisive.

Based on established literature modeling criminal activity (Becker, 1968; Stigler, 1970) and environmental monitoring (Polinsky and Shavell, 2007; Russell et al., 1986; Gray and Shimshack, 2011), the study hypothesizes that command-and-control policies affect deforestation by altering the expected value of engaging in criminal activities. However, suppose the gains from deforestation are relatively high. In that case, it may be profitable for closely monitored producers to relocate their activities to less-watched municipalities, resulting in increased deforestation in unmonitored neighbors. Hence, while reductions in deforestation are expected in listed municipalities, the effect on their neighbors is uncertain. Failure to consider spillover effects in studies may underestimate the policy's effectiveness. Conversely, not considering these effects may overestimate the policy's impact and reduce its responsibility to address deforestation in neighboring regions.

3.1 The Prevention and Control Plan for Deforestation in the Legal Amazon (PPCDAm)

Between 1998 and 2004, deforestation rates soared, prompting international pressure and domestic activism. In response, Brazil launched the PPCDAm, a comprehensive initiative to combat deforestation and foster sustainable development in the Amazon region. The plan, built on four pillars—land and territorial planning, environmental monitoring and control, promoting sustainable, productive activities, and economic and normative instruments—marked a turning point in Brazil's environmental policy landscape.

During the 2000s, a notable shift in deforestation patterns emerged, characterized by small deforested polygons replacing large clearings. This shift coincided with the implementation of the PPCDAm, indicating its impact on altering deforestation dynamics. Studies by Rosa et al. (2012) and Michalski et al. (2010) underscore the role of property size as a key determinant in regional deforestation patterns, highlighting the differential impact of the plan across various property sizes.

The PPCDAm garnered significant success in forest conservation, with Pereira (2015) reporting the preservation of 8.36 thousand square kilometers annually across 760 municipalities in the Legal Amazon from 2005 to 2015. Moreover, the plan facilitated a reduction in forest conversion for agriculture and livestock purposes, signaling a decoupling of agricultural expansion from deforestation (Gollnow et al., 2018; Amaral et al., 2021).

A pivotal innovation under the PPCDAm was implementing the Real-Time Deforestation Detection (DETER) system in 2004, enabling real-time monitoring of deforested areas using satellite imagery. Assunção et al. (2013b) demonstrated the effectiveness of DETER-based monitoring in reducing deforestation rates by 60% compared to a scenario without policy changes from 2007 to 2011.

In 2008, Decree 6.5514/2008 introduced a new federal administrative procedure for investigating environmental offenses and imposing administrative sanctions, enhancing enforcement capabilities. As a result of these concerted efforts, the PPCDAm witnessed a 53% reduction in deforestation during its first phase (2004-2008) and a further 65% reduction during its second phase (2008-2012), underscoring the plan's effectiveness in curbing deforestation.

3.2 The List of Priority Municipalities

Command-and-control policies have been considered the most effective in combating deforestation. Until 2004, the Brazilian Institute of the Environment and Renewable Natural Resources (IBAMA), acting as environmental police, based its inspection efforts on anonymous reports. That year, the National Institute for Space Research (INPE) developed the satellite monitoring system DETER. INPE has been measuring deforestation in Brazil by satellite since 1988 as part of the PRODES project (Deforestation Calculation Program for the Amazon). DETER, in turn, produces more frequent images of the Amazon and issues deforestation alerts every two weeks. Through the coordinated action of IBAMA and INPE, it is possible to quickly identify new deforestation areas and monitor the region more efficiently, as evidenced by the fines imposed by IBAMA.

In late 2007, through Decree No. 6,321 of December 21, 2007, the Federal Government created a list of priority municipalities and established the Ministry of the Environment (MMA) to list the municipalities. The list is named because municipalities are prioritized for reinforcing PPCDAm policies such as integrating and improving monitoring and control actions by federal agencies, land, and territorial planning, and encouraging environmentally sustainable economic activities. These municipalities are subject to stricter environmental regulations. According to the decree, the listed municipalities are part of the Amazon biome chosen according to three criteria: (i) the total deforested area in the municipality, (ii) the deforested area in the three previous years, and (iii) an increase in the deforestation rate in at least three of the five previous years.

Once on the list, municipalities are monitored and supported by the Federal Government in implementing measures to reduce deforestation rates and promote sustainable activities. Rural properties in priority municipalities must register with INCRA, which may require georeferencing and verification of land titles. The issuance of authorizations for soil clearing on medium and large properties in these municipalities would also be subject to georeferencing through the Rural Environmental Registry, and agricultural credit granting would be subject to compliance with environmental standards on properties. However, the most important aspect of this policy is that IBAMA teams now pay special attention to listed municipalities, meaning that DETER alerts in these municipalities receive more attention (Assunção et al., 2013b). Thus, a municipality on the priority list is subject to more rigorous environmental inspections.

Each year, MMA published the List of Priority Municipalities and announced the criteria for leaving the list, including (i) having eighty percent of the municipality's private rural lands monitored and under INCRA's technical criteria and (ii) keeping the deforestation rate at least 30% below the maximum deforestation rate observed in the municipality over the past five years, under the same parameter conditions. These conditions must be met simultaneously for the municipality to leave the list.

3.3 Evaluating the Effectiveness of the List of Priority Municipalities

Several studies have investigated the impact of the list of priority municipalities on deforestation rates in the Amazon region. Arima et al. (2014) found that deforestation rates in priority municipalities decreased by 35% compared to non-priority municipalities. Similarly, Assunção et al. (2013b) observed a 25% reduction in deforestation rates in priority municipalities compared to non-priority ones. Furthermore, Assunção and Rocha (2019) attributed a significant portion of the overall reduction in deforestation rates to including municipalities on the priority list. These findings suggest that the list of priority municipalities has effectively curbed deforestation in the Amazon region.

However, some studies have also highlighted potential unintended consequences of the policy. For example, Harding et al. (2018) found evidence of deforestation spillovers from priority municipalities to neighboring areas that were not on the list. Similarly, Koch et al. (2019) observed an increase in deforestation rates in neighboring municipalities following the implementation of the policy. These findings suggest that while the list of priority municipalities may have successfully reduced deforestation in designated areas, it may have displaced deforestation to neighboring regions.

In addition to its direct impact on deforestation rates, the list of priority municipalities may have influenced land use and land cover change in the Amazon region. For example, Cisneros et al. (2013) found that the policy led to changes in land use patterns, shifting towards less intensive land uses such as pasture and agroforestry. Similarly, Cisneros et al. (2015) observed changes in land cover, with a decrease in the proportion of land devoted to agriculture following the implementation of the policy.

Overall, the evidence suggests that the list of priority municipalities has effectively reduced deforestation rates in the Amazon region. However, the policy may have also had unintended consequences, such as deforestation spillovers to neighboring areas and changes in land use patterns. These findings highlight the importance of carefully evaluating the effectiveness of environmental policies and considering their potential impacts on broader socio-ecological systems.

3.4 Data

We consider treated municipalities listed in some period. Moreover, we consider potentially treated municipalities that can receive treatment spillover, not listed but neighbors of listed. The control group comprises municipalities that are not listed and without listed neighbors (Figure 1).

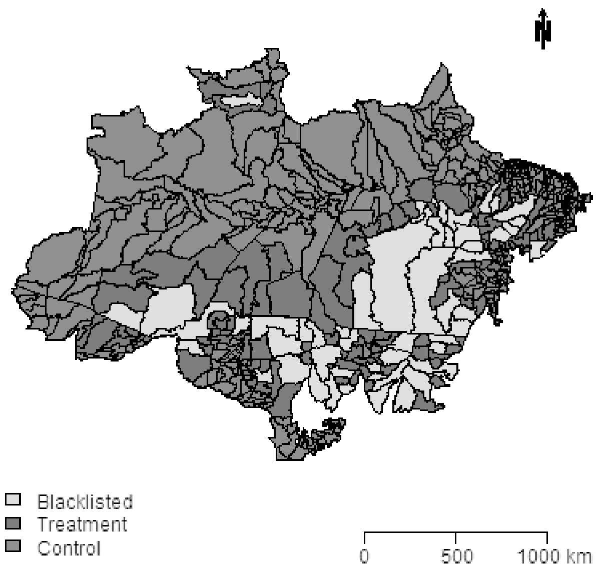


Figure 1: Treatment, neighbors, and control groups

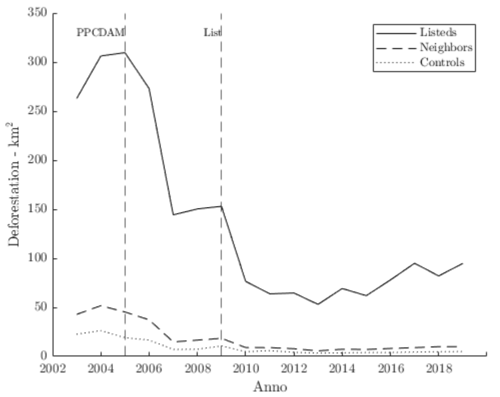
The dependent variable is constructed considering the ratio of deforested areas in a given year to the remaining forest area in the previous year, calculated from INPE data. The remaining forest area is the difference between the total area of the municipality and the sum of accumulated deforestation, non-forest area, and water area. This variable, thus constructed, is preferable to using forest area obtained directly from data from the National Institute for Space Research (INPE) due to the large number of missing observations.

A relative deforestation measure is necessary to ensure comparability between groups. As the criteria for entering the list depend on absolute deforestation values, listed municipalities are among those that naturally deforest the most. Considerable variation between municipality areas must also be considered when using municipal-level data.

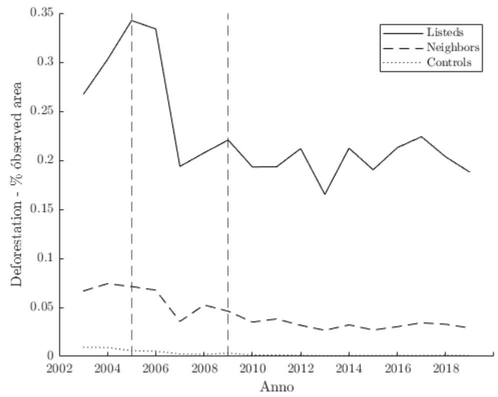
The relative variable is transformed using a logistic transformation, as is common in the literature (Pffaf, 1999; Assunção et al., 2021), as follows:

$$y_{it} = \log\left(\frac{Y_{it}}{1 - Y_{it}}\right) \tag{28}$$

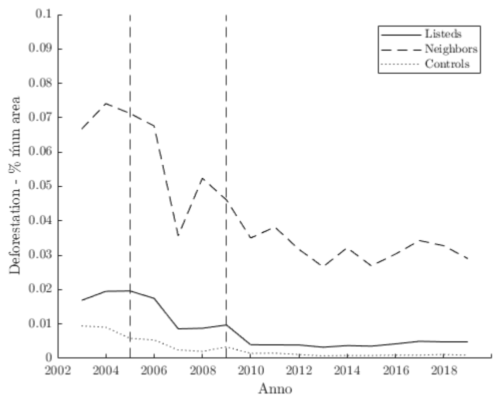
where Y_{it} is one of the three mentioned relative measures.



(a) deforested area - km^2



(b) %imputed area



(c) % mun area

Figure 2: Average deforested by group - listed, neighbor and control municipalities

Figure 2 shows the trajectories of different deforestation measures for the three analysis groups. It makes it clear that priority municipalities are among the highest deforesters in absolute terms, with an average exceeding 300 km^2 in 2004 - the year of the implementation of PPCDAm. Non-treated neighboring municipalities deforested an average of about 50 km^2 in the same year, and other municipalities less than 25 km^2. In relative terms, considering the forest area, priority municipalities deforested nearly half of all existing forests around 2004. Municipalities in the other groups never reached such a mark. However, non-treated neighboring municipalities stand out, for which the proportion of deforested areas was about 5%, on average, throughout the period. However, when considering the area of the municipalities, it is noticed that these municipalities (non-treated neighbors) are the ones that deforest the most as a proportion of their territory, confirming that the list prioritizes municipalities that deforest the most, as they are larger.

In any case, for all groups, it is noted that deforestation did not increase and certainly decreased for the main groups (treated) - which suggests that the policy's spillover effect also aims to curb deforestation in nearby areas.

The database consists of a panel covering municipalities in the Amazon biome from 2001 to 2018. The PPCDAm was launched in March 2004, so 2005 was the first year it was active for the entire period. The Brazilian Legal Amazon covered 771 municipalities in 9 states during the analyzed period. Spatial references come from IBGE maps on municipal borders in 2007. The final sample comprises 502 municipalities that have more than 40% of their area within the Amazon biome.

The annual deforestation data is from PRODES, calculated and disclosed by INPE. As deforestation occurs during the dry season, most satellite images from which the data is derived were taken between July and September. Annual deforestation rates are calculated with August 1 as the reference date[1]. Land use is identified from the satellite image as having the best visibility (minimum cloud cover). According to the soil image fraction, shadow, and vegetation, it is classified as forest, non-forest, deforested, water, and cloud. PRODES can identify deforested areas larger than 6.25 ha. PRODES also

[1]As PRODES defines the year from August of $t-1$ to July of t, this period was used to define the year for control variables, whenever possible.

calculates the total accumulated deforestation, which could be an important control since municipalities with a small remaining forest area should have lower deforestation rates. Other variables from this same database are the unseen area and the area covered by clouds in the final satellite images. As discussed by Butler and Moser (2007), they are relevant as controls for measurement errors.

The value of rural credit granted to each municipality in a given year is available in the ESTBAN database (Banking Statistics) from the Central Bank of Brazil (BACEN) for the period 2000-2012. Pffaf (1999) argues that credit supply is endogenous, as deforestation attracts new bank branches and increases demand for credit. As a solution, the value of credit from the previous year is used in the estimates, which is also consistent with deforestation dynamics. Since BACEN only publishes annualized data, credit information could not be transformed into the PRODES annual period.

Data on environmental fines were made available by IBAMA upon request. The database contains information on all fines imposed by IBAMA from 2000 to 2020, including the offender's name, type of infraction, process status, and fine amount. Georeferenced data containing the type of conservation unit, responsible authority, and year of creation are provided by MMA. Georeferenced data on indigenous lands, including date of creation, area, and ethnicity, are available on the National Indian Foundation (FUNAI) website. Information on Bolsa Verde beneficiaries was compiled by do Nascimento and Chagas (2021). Population and GDP data come from IBGE's regional accounts system. The cultivated area was obtained from IBGE's survey of municipal agricultural production (PAM) and includes both temporary and permanent crops. Monetary data were deflated by the IPCA (National Consumer Price Index).

3.5 Results

The results of the regression model 23 are shown in Panel A of Table 2. They indicate that, on average, being a municipality on the priority list reduces the odds ratio of deforestation by almost 57% when considering the classical diff-in-dif approach without any controls (model 1). This percentage remains statistically similar when introducing controls for

Table 2: Results of the classic and spatial diff-in-diff models

	Dependent Variable: Deforestation Ratio by Remaining Forest Area (Imputed)						
Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A - classic diff-in-diff model							
Priority	-0.5698^{***}	-0.5573^{***}	-0.5827^{***}	-0.5753^{***}	-0.5737^{***}	-0.5651^{***}	-0.5706^{***}
	(0.0612)	(0.0592)	(0.0591)	(0.0592)	(0.0592)	(0.059)	(0.0592)
Adj-R2	0.7424	0.7509	0.7523	0.7527	0.7529	0.7553	0.7555
AIC	4.6318	4.6907	4.7007	4.7025	4.7032	4.7200	4.7205
Panel B - restricted spatial diff-in-diff model							
Priority	-0.5507^{***}	-0.5345^{***}	-0.5623^{***}	-0.5556^{***}	-0.5535^{***}	-0.5439^{***}	-0.5652^{***}
	(0.0606)	(0.0589)	(0.059)	(0.0591)	(0.059)	(0.0587)	(0.0584)
Neighbors	-0.2099^{**}	-0.2566^{***}	-0.2240^{***}	-0.2160^{***}	-0.2217^{***}	-0.2554^{***}	-0.8606^{***}
	(0.0836)	(0.081)	(0.0815)	(0.0813)	(0.0815)	(0.083)	(0.1756)
Adj-R2	0.7425	0.7512	0.7525	0.7528	0.7530	0.7556	0.7565
AIC	4.6327	4.6923	4.7019	4.7036	4.7044	4.7215	4.7278
Panel C - unrestricted spatial diff-in-diff model							
Priority	-0.4291^{***}	-0.4402^{***}	-0.4790^{***}	-0.4742^{***}	-0.4698^{***}	-0.4647^{***}	-0.5614^{***}
	(0.0769)	(0.0765)	(0.0765)	(0.0766)	(0.0765)	(0.0761)	(0.0585)
Treated Neighbors	-0.8389^{***}	-0.7451^{***}	-0.6532^{**}	-0.6360^{**}	-0.6531^{**}	-0.6597^{**}	-1.2209^{***}
	(0.2975)	(0.2846)	(0.2851)	(0.2858)	(0.285)	(0.2827)	(0.319)
Untreated Neighbors	-0.0857	-0.1600^{**}	-0.1397^{*}	-0.1337^{*}	-0.1368^{*}	-0.1737^{**}	-0.7766^{***}
	(0.0779)	(0.0761)	(0.0764)	(0.0761)	(0.0761)	(0.0772)	(0.166)
Adj-R2	0.7428	0.7514	0.7527	0.7530	0.7532	0.7557	0.7566
AIC	4.6348	4.6936	4.7029	4.7046	4.7054	4.7224	4.7285
Observations				9,036			
Controls							
Geographical area		✓	✓	✓	✓	✓	✓
Procted area			✓	✓	✓	✓	✓
Economic Activity				✓	✓	✓	✓
IBAMA action					✓	✓	✓
Spatial Lag on X						✓	✓
Neighbors × IBAMA							✓

The dependent variable is the odds ratio of the deforestation rate increment in t relative to the remaining forest area of $t-1$, given by $y_{it} = \log(Y_{it}/(1-Y_{it}))$, where Y_{it} is the value of the mentioned ratio.
The remaining forest area is the difference between the total area of the municipality and the sum of accumulated deforestation, non-forest area, and water area.
Geographical area controls include data on the stock fo deforestation and the agricultural area. Data on deforestation increment, forest area, and accumulated deforestation are obtained from INPE.
Protected area controls include legal and indigenous reserves. The Ministery of Enviromental and FUNAI provide data.
Economic activity controls include information on income transfer (Bolsa Verde), credit, GDP per capita, share of agricultural on GDP and populations density. Bolsa Verde data was compiled by do Nascimento and Chagas (2021). Credit information came from the Central Bank.
IBAMA action controls include information on quantity and values of fine applied by IBAMA.
Robust standard errors in parentheses.
Significance levels: *** 0.1%, **1%, *5%.

accumulated deforestation areas (model 2), protected area (model 3), economic activity (model 4), IBAMA action (model 5), neighborhood controls (model 6) or the interaction between IBAMA action and neighborhood of the treated.

Panel B on Table 2 considers the same previous models but now adds the variable of untreated neighbors of treated municipalities. The definition of neighborhood considered a criterion of k-nearest neighbors, where the number of neighbors (k) was obtained using an information criterion[2].

The result suggests a spillover effect that increases the beneficial impact of the policy in 20% to 25% when all the neighbors are treated. The direct effect on the treated remains similar to the one verified in the previous Panel. The noteworthy point now is an additional effect of deforestation reduction due to the spillover to neighboring municipal-

[2]The Akaike criterion was used, as suggested in Stakhovych and Bijmolt (2009).

ities. The magnitude of the impact is significant, representing almost half of the direct effect on the treated.

Finally, it is possible to investigate more carefully how this spillover effect behaves between treated and untreated municipalities (neighbors of other treated units). Panel C on Table 2 reports this exercise.

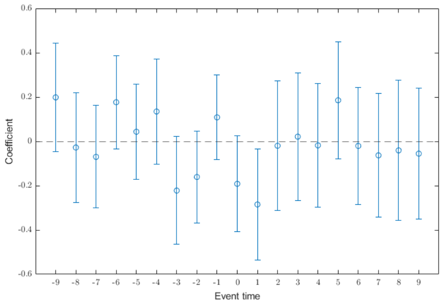
Also, in this case, the direct effect of treatment on treated municipalities is in the same order of magnitude. The significant difference lies in the spillover effect. In this case, the differentiation of groups allows us to verify that the effect on the group of treated neighbors of the treated is significantly greater than in the situation where the average effect is considered (restricted models). Compared to the previous model, the impact is about two times larger (more negative) for treated municipalities with treated neighbors.

The untreated municipalities' neighbors who are treated also benefit from the treatment if more neighbors are treated. The effect is about 1/4 to 1/3 of the impact on treated municipalities. For both set of spatial models, restricted and unrestricted, the last model (model 7) presents a significant difference compared to the others specification. It occurs when we include an additional interaction between the IBAMA's action and the neighbor on the treated variable. The negative effect on the spillover variable stems from the perception of a higher enforcement risk in neighboring municipalities. In both situations, the spillover effect increased, suggesting that the action of the environmental policy is an important channel to the success of the spillover.

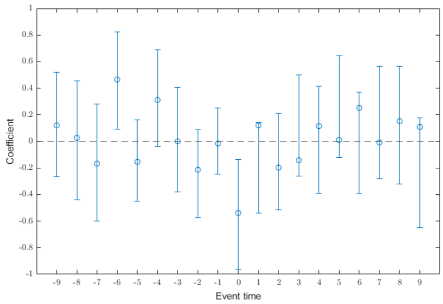
3.5.1 Event Study

In this section, we present a spatial event study analysis focusing on the effect of the Priority List on the deforestation of treated and neighbors of the treated municipalities. As usual in this literature, we report the graphs with the coefficient and the confidence interval, including lags and leads.

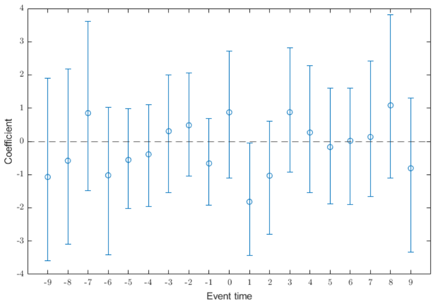
Confirming the previous result, we can reject the hypothesis of a treatment or spillover dynamic effect for more than two periods, suggesting that the policy has effect immediately, or at a maximum, one year after. Figure 3a shows a negative and significant effect



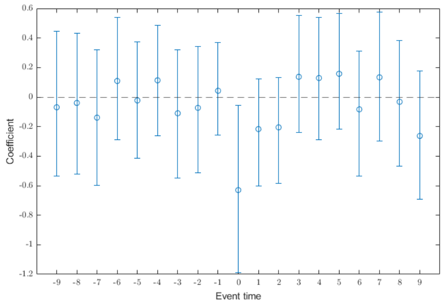
(a) Treatment on treated



(b) Treatment on neighbors



(c) Treatment on neighbors treated



(d) Treatment on neighbors untreated

Figure 3: Spatial Event Study Analysis

one period ahead. All the other periods are insignificant, with the same lag periods - which is interpreted as confirming the nonanticipatory assumption.

Figure 3a also confirms the negative effect on neighbors (treated or untreated), but the effect in this case is contemporaneous. There is a significant and positive anticipatory effect six periods before, which seems to have little relationship with the policy itself and does not persist when detailing the spillover effect on treated and untreated neighbors (Figures 3c and 3d). Furthermore, this positive signal is smaller in magnitude than the negative and significant spillover effect in the first period after treatment.

As mentioned, figure 3c and 3d detailed the event study considering the spillover effect on neighbors treated and untreated. Here, it is interesting to note that both groups' temporal impact seems different. The effect on neighbors treated is similar to the treated group, with the impact occurring one period ahead, while for neighboring municipalities not treated, the effect is contemporaneous. In both cases, there is no evidence of anticipatory or staggered effects after one period ahead, similar to the case of direct effects on the treated (Figure 3a).

The findings of these studies have important implications for environmental policy in the Amazon region. First, they suggest that command-and-control policies, such as the list of priority municipalities, can effectively reduce deforestation rates. However, policymakers must be mindful of unintended consequences, such as deforestation spillovers to neighboring areas. Future research should continue to explore these unintended consequences and identify strategies for mitigating them.

Second, the studies highlight the importance of monitoring and enforcement mechanisms in environmental policy. The success of the list of priority municipalities is at least partially attributable to increased monitoring and enforcement efforts in designated areas. As such, policymakers should prioritize investments in monitoring and enforcement infrastructure to support the implementation of environmental policies.

Finally, the findings underscore the need for a holistic approach to environmental policy in the Amazon region. While command-and-control policies like the list of priority municipalities can effectively reduce deforestation rates, they must be complemented by efforts to promote sustainable land use practices and address the underlying drivers of deforestation. Future research should continue to explore integrated approaches to environmental policy that address both the proximate and underlying causes of deforestation in the Amazon region.

In conclusion, the list of priority municipalities has been an important tool in Brazil's efforts to combat deforestation in the Amazon region. While the policy has effectively reduced deforestation rates in designated areas, it has also had unintended consequences, such as deforestation spillovers to neighboring regions. Moving forward, policymakers should continue to refine and adapt environmental policies to address these challenges and promote sustainable development in the Amazon region.

4 Final Considerations

Introducing spatial diff-in-diff techniques has enriched empirical analyses by allowing researchers to account for spatial heterogeneity and spillover effects when assessing the causal impact of interventions or events. In this paper, we explicitly explain the assumptions underlying this estimator in a way that was not done in previous works. The underlying assumptions make clear the regular conditions of the spillover effect in the space, which need to be considered by the research applied in this work.

We also have explored the intersection of spatial difference-in-differences methodology and event study analysis, shedding light on the nuanced dynamics of spatially localized effects of significant events. By extending this approach to event study analysis, we have discerned the direct effects of events on treated units and the spatially dispersed impacts on neighboring areas. In the spatial context, event studies serve as a valuable tool for understanding the localized effects of events, such as policy changes, natural disasters, infrastructure projects, or social interventions, on geographic regions or spatially defined entities. By employing spatial analysis techniques alongside event study methodologies, researchers can uncover nuanced patterns of spatial heterogeneity in the response to events, providing insights into spatially differentiated impacts and the underlying mechanisms driving them. Our investigation has underscored the importance of integrating spatial considerations into traditional event study frameworks, offering a more comprehensive understanding of the spatial dimensions of economic phenomena and policy impacts.

The empirical application presented in this paper has demonstrated the utility of spatial event study analysis in uncovering the spatial dynamics of policy interventions or other significant events. By examining spillover effects on treated and untreated neighbors, we have elucidated spatial diffusion's temporal patterns and magnitude, providing valuable insights for policymakers and stakeholders.

The results suggest that the Priority Municipality List affects the listed municipalities, reducing the odds ratio of the annual deforestation of the remaining forest by around 50%. This result may be even higher in the case of treated municipalities that have also treated neighbors. The effect of the list on non-listed municipalities could be positive or negative. The negative coefficients observed in the presented estimates indicate that the incentive for deforestation reduction, caused by the higher probability of punishment, is greater than the incentive for agricultural activity expansion in neighboring municipalities. The results indicate that the list caused a more than 70% reduction in deforestation in non-listed neighbors of listed municipalities. This results from an untreated municipality if all its neighbors are treated. The result is lower with fewer treated neighbors.

Explicitly, the assumptions underlying the spatial diff-in-diff method and integrating spatial diff-in-diff and event study methodologies hold great promise for advancing our understanding of spatially differentiated impacts and guiding evidence-based decision-making in various fields, from urban planning and regional development to environmental policy and beyond. By embracing a spatially informed approach to event analysis, researchers can better capture the complex interplay between events, geography, and socio-economic outcomes, paving the way for more effective policy interventions and targeted interventions in spatially diverse contexts.

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A Proof to propositions

Proposition 1: Consider the average treatment effect on the treated (ATET),

$$\tau_{10} = E[Y_{i,2}(10) - Y_{i,2}(00) D_i = 1, D_j = 0].$$

Of course, $E[Y_{i,2}(00) D_i = 1, D_j = 0]$ is non-observable. Using (7b):

$$E[Y_{i,2}(00) D_i = 1, D_j = 0] = E[Y_{i,1}(00) D_i = 1, D_j = 0] + E[Y_{i,2}(00) - Y_{i,1}(00) D_i = 0, D_j = 0]$$

From assumption 3 (equation (8)),

$$E[Y_{i,1}(00) D_i = 1, D_j = 0] = E[Y_{i,1}(10) D_i = 1, D_j = 0] \tag{29}$$

Then

$$\begin{aligned}
\tau_{10} &= E[Y_{i,2}(10) - Y_{i,2}(00) D_i = 1, D_j = 0] \\
&= E[Y_{i,2}(10) - Y_{i,1}(00) | D_i = 1, D_j = 0] - E[Y_{i,2}(00) - Y_{i,1}(00) | D_i = 0, D_j = 0] \\
&= \underbrace{E[Y_{i,2}(10) - Y_{i,1}(10) | D_i = 1, D_j = 0]}_{\text{Change for } D_i = 1, D_j = 0} - \underbrace{E[Y_{i,2}(00) - Y_{i,1}(00) | D_i = 0, D_j = 0]}_{\text{Change for } D_i = 0, D_j = 0}
\end{aligned}$$

Consider the average spillover effect on the untreated (ASENT),

$$\tau_{01} = E[Y_{i,2}(01) - Y_{i,2}(00) D_i = 0, D_j = 1].$$

As $E[Y_{i,2}(00) D_i = 0, D_j = 1]$ is non-observable, we use (7c):

$$E[Y_{i,2}(00) D_i = 0, D_j = 1] = E[Y_{i,1}(00) D_i = 0, D_j = 1] + E[Y_{i,2}(00) - Y_(i,1)(00) D_i = 0, D_j = 0]$$

Again, from assumption 3 (equation (8)),

$$E[Y_{i,1}(00) D_i = 0, D_j = 1] = E[Y_{i,1}(01) D_i = 0, D_j = 1]$$

Then

$$\begin{aligned}
\tau_{01} &= E[Y_{i,2}(01) - Y_{i,2}(00) D_i = 0, D_j = 1] \\
&= E[Y_{i,2}(01) - Y_{i,1}(00) | D_i = 0, D_j = 1] - E[Y_{i,2}(00) - Y_{i,1}(00) | D_i = 0, D_j = 0] \\
&= \underbrace{E[Y_{i,2}(01) - Y_{i,1}(01) | D_i = 0, D_j = 1]}_{\text{Change for } D_i = 0, D_j = 1} - \underbrace{E[Y_{i,2}(00) - Y_{i,1}(00) | D_i = 0, D_j = 0]}_{\text{Change for } D_i = 0, D_j = 0}
\end{aligned}$$

Finally, we can consider the composed spillover effect on the region treated and neighbor treated. In this situation, there is a composed spillover effect, direct and indirect (ASET):

$$\tau_1 1 = E[Y_{i,2}(11) - Y_{i,2}(00) D_i = 1, D_j = 1].$$

Again, the non-observable term $E[Y_{i,2}(00) D_i = 1, D_j = 1]$ is substituted using (7a):

$$E[Y_{i,2}(00) D_i = 1, D_j = 1] = E[Y_{i,1}(00) D_i = 1, D_j = 1] + E[Y_{i,2}(00) - Y_(i, 1)(00) D_i = 0, D_j = 0]$$

From non-anticipatory assumption (equation (8)),

$$E[Y_{i,1}(00) D_i = 1, D_j = 1] = E[Y_{i,1}(11) D_i = 1, D_j = 1]$$

Then

$$\begin{aligned}
\tau_{11} &= E[Y_{i,2}(11) - Y_{i,2}(00) D_i = 1, D_j = 1] \\
&= E[Y_{i,2}(11) - Y_{i,1}(00) | D_i = 1, D_j = 1] - E[Y_{i,2}(00) - Y_{i,1}(00) | D_i = 0, D_j = 0] \\
&= \underbrace{E[Y_{i,2}(11) - Y_{i,1}(11) | D_i = 1, D_j = 1]}_{\text{Change for } D_i = 1, D_j = 1} - \underbrace{E[Y_{i,2}(00) - Y_{i,1}(00) | D_i = 0, D_j = 0]}_{\text{Change for } D_i = 0, D_j = 0}
\end{aligned}$$

□

Proposition 2: Considering (21), $E[Y_{i,2}(1,\mathbf{0})] = \beta^d + \mu + \delta_2$, $E[Y_{i,1}(1,\mathbf{0})] = \mu + \delta_1$, $E[Y_{i,2}(0,\mathbf{0})] = \mu + \delta_2$ and $E[Y_{i,1}(0,\mathbf{0})] = \mu + \delta_1$, then

$$\begin{aligned}
\tau_{1,\mathbf{0}} &= E[Y_{i,2}(1,\mathbf{0}) - Y_{i,1}(1,\mathbf{0}) | D_i = 1, \mathbf{D} = \mathbf{0}] - E[Y_{i,2}(0,\mathbf{0}) - Y_{i,1}(0,\mathbf{0}) | D_i = 1, \mathbf{D} = \mathbf{0}] \\
&= (\beta^d + \mu + \delta_2) - (\mu + \delta_1) - (\mu + \delta_2) + (\mu + \delta_1) = \beta^d
\end{aligned}$$

In the same way, $E[Y_{i,2}(1,\mathbf{1})] = \beta^d + \beta^t + \mu + \delta_2$, $E[Y_{i,1}(1,\mathbf{1})] = \mu + \delta_1$, $E[Y_{i,2}(0,\mathbf{0})] = \mu + \delta_2$ and $E[Y_{i,1}(0,\mathbf{0})] = \mu + \delta_1$. Consequently,

$$\begin{aligned}
\tau_{1,\mathbf{1}} &= E[Y_{i,2}(1,\mathbf{1}) - Y_{i,1}(1,\mathbf{1}) | D_i = 1, \mathbf{D} = \mathbf{1}] - E[Y_{i,2}(0,\mathbf{0}) - Y_{i,1}(0,\mathbf{0}) | D_i = 1, \mathbf{D} = \mathbf{0}] \\
&= \beta^d + \beta^t.
\end{aligned}$$

And then, $\tilde{\tau}_{1,\mathbf{1}} = \tau_{1,\mathbf{1}} - \tau_{1,\mathbf{0}} = \beta^t$.

Finally, still considering (21), $E[Y_{i,2}(0,\mathbf{0})] = \beta^u + \mu + \delta_2$, $E[Y_{i,1}(0,\mathbf{0})] = \mu + \delta_1$, $E[Y_{i,2}(0,\mathbf{0})] = \mu + \delta_2$ and $E[Y_{i,1}(0,\mathbf{0})] = \mu + \delta_1$. Then,

$$\begin{aligned}
\tau_{0,\mathbf{1}} &= E[Y_{i,2}(0,\mathbf{0}) - Y_{i,1}(0,\mathbf{0}) | D_i = 1, \mathbf{D} = \mathbf{0}] - E[Y_{i,2}(0,\mathbf{0}) - Y_{i,1}(0,\mathbf{0}) | D_i = 1, \mathbf{D} = \mathbf{0}] \\
&= (\beta^u + \mu + \delta_2) - (\mu + \delta_1) - (\mu + \delta_2) + (\mu + \delta_1) = \beta^u
\end{aligned}$$

$\square$