Curbing or Displacing Deforestation? The Amazon Blacklist Policy

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Abstract

This paper tries to establish causality between the Amazon blacklist policy and deforestation displacement, by applying the differences-in-differences framework, on a panel of Cerrado Municipalities from 2004 through 2014. The results are statistically significant, showing evidence of displacement at intermediate distances (50-200 km). These findings are robust to different treatment cut-off definitions. Also, the parallel assumption holds when looking at the pre-trends. Counterfactual simulations show an increase in deforestation of 4,963 km^2 from 2009 through 2014, representing an offset of 29% of the direct impact of the policy.

Keywords

Displacement, Deforestation, Differences-in-Differences, Amazon Blacklist, Cerrado

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1 Introduction

The Amazon is the largest biome of Brazil, covering an area of 4,106,943 km^2, followed by the Cerrado with 2,036,448 km^2 (IBGE, 2004). However, when recent and historical deforestation rates are compared, the positions are reversed. Even though the Cerrado is considered one of the world's biodiversity hotspots (Myers et al., 2000; Silva and Bates, 2002), by the early 2000s, almost half of its total area had already been converted to pasture or cropland (Klink and Machado, 2005; MMA, 2015; Noojipady et al., 2017). Whereas, in the Amazon, this share reached only 18.2% in 2013 (Nobre, 2014). Moreover, for the 2004 through 2014 period (excluding 2005), deforestation rates were consistently higher in the smaller biome.

This picture supports the estimates of Noojipady et al. (2017), that two-thirds of the Brazilian greenhouse gas emissions were due to changes in land use and forest loss in 2005. To stop this trend, the Brazilian government launched an integrated action plan (PPCDAm[1]). The plan focused on the preservation of tropical forests typically present in the Amazon, even though the deforestation pressure was higher in the Cerrado.

The conservation policies' turning points coincide with the sharp falls in deforestation rates for the Amazon. However, as pointed out by Noojipady et al. (2017), to achieve national emission reductions, it is necessary to take into account possible cross-biome leakages. For example, between 2010 and 2013, they estimated that carbon emissions due to land use changes in Cerrado offset 5% to 7% of the avoided emission from the Amazon.

This paper explores the institutional changes in the mid-2000s that focused conservation efforts in the Amazon over the Cerrado. Specifically, the focus is the Priority Municipalities' (PMs) policy which created a blacklist, in 2008, of municipalities with high clearing rates. The question to be answered is "Did this policy displace deforestation from the Amazon to the Cerrado?"

[1] Action Plan for the Prevention and Control of Deforestation in the Legal Amazon

The rationale is that when a municipality enters the blacklist, there is an exogenous rise on the cost of deforestation due to strict law enforcement (Assunção and Rocha, 2014) or other non-enforcement mechanisms (Cisneros et al., 2015), leading to an incentive to displace. Additionally, considering the focus of the policies, the Cerrado seems to be a more attractive region compared to non-blacklisted Amazon municipalities.

Based on a panel of Cerrado municipalities from 2004 through 2014, I use a differences-in-differences framework, considering municipalities that are less than 300 km from the closest PM as treatment. The model indicates a statistically significant increase in the farming area, a proxy for deforestation, at intermediate distances from 50 to 200 km. In robustness checks, I find supporting evidence for the parallel assumption, looking at the pre-trends. Also, I verify that the results are not driven by the arbitrary treatment cut-off by testing a 250 km threshold. Finally, counterfactual simulations suggest a total displacement of 4,963 km2 from 2009 through 2014. That represents an offset of 29% of the avoided deforestation in the targeted Amazon municipalities, compared to the direct impacts estimated by Assunção and Rocha (2014).

This study is closely related to two main pieces of literature: crime literature and literature that evaluates the impact of the mid-2000s Amazon anti-deforestation policies.

From crime literature, I am interested in the debate about hotspot policing and displacement or diffusion effects. Chalfin and McCrary (2017) define hotspots policing as a reallocation of existing resources to places where crime is highly concentrated. The question that follows is if this strategy merely shifts, through the displacement effect, rather than reduces, crime. Weisburd et al. (2006) point out that for a long period of time, it was believed that displacement was inevitable; however, now many critics don't think that is the case (Barr and Pease, 1990; Gabor, 1990; Eck, 1993; Hesseling 1994; Clarke, 1995). Moreover, Clarke and Weisburd (1994) show that the phenomenon of "diffusion of benefits"—the reduction of crimes in areas outside the targets of intervention and considered the reverse of displacement—is also possible. Since there is mixed evidence of the presence and direction of spatial spillovers, documenting these effects remains an empirical challenge.

Most studies about anti-deforestation policies focused on direct impacts of the policies (Hargrave and Kis-Katos, 2013; Arima et al., 2014; Assunção and Rocha, 2014; Assunção et al., 2015; Cisneros et al., 2015; Assunção et al., 2017; Burgess et al., 2018), leaving spillover effects as a by-product (Cisneros et al., 2015; Assunção, Gandour and Rocha, 2017) when analyzed. Additionally, even when the focus was displacement, the sample used was geographically restricted to the Amazon Biome (Amin et al., 2015; Andrade, 2016). In general, this literature documents that the anti-deforestation policies were the main drivers of the observed slowdown in deforestation in the Brazilian Amazon during the mid-2000s, with small negative spillovers (Amin et al., 2015) or even positive externalities (Andrade, 2016).

Hence, my main contribution is prioritizing the identification of a spillover effect of one of the anti-deforestation policies, focusing on the much less explored Cerrado region. Additionally, I provide new evidence for the debate about hotspots policing from crime literature, applying it to the less explored setup of environmental crimes rather than urban crimes.

The rest of this paper is organized as follows: Section 2 discusses the Amazon anti-deforestation policies and the differences between the Cerrado and the Amazon; Section 3 provides a description of the data; Section 4 explains the empirical strategy used to estimate the spatial spillover effect; Section 5 discusses the results of the paper; Section 6 provides robustness checks for the model assumptions; and Section 7 concludes by summarizing the results and presenting its policy implications.

2 Institutional Context

In the early 2000s, deforestation in the Brazilian Amazon rose until a peak of 2.8 million km2 in 2004. As a response, the Brazilian government created an integrated plan of action (PPCDAm) with the goal of proposing new approaches to curb deforestation in the Legal

Amazon[2]. The two main reformulations were the use of a satellite-based system to detect tropical clearings and the creation of a blacklist of the municipalities in need of special attention.

The first phase of PPCDAm started in 2004, and its main component was the strengthening of monitoring and law enforcement. Since 1989, Ibama[3] is responsible for addressing environmental violations acting as the national police authority, and, until 2004, their actions were mostly based on voluntarily anonymous accusations of illegal activities. After 2004, however, there was a massive advance in the identification process of clearings in the Amazon, due to the adoption of DETER[4], developed by INPE[5]. This system processes forest cover images in 15-day intervals, comparing the same area across time to identify signals of forest loss, and then issuing alerts with the location of the threatened areas. In practice, when the offenders are caught red-handed, they can be punished more efficiently, so timing is fundamental and DETER allowed Ibama to act more quickly (Gandour, 2018).

In 2008, the second phase of PPCDAm was initiated, marked by significant legal changes. First, the Presidential Decree 6,514 regulated the use of penalties like fines, embargoes, and seizure and destruction of equipment as punishment of environmental crimes (Brasil, 2008). Additionally, the Presidential Decree 6,321, signed in December 2007, allowed the exposure of municipalities with intense deforestation in recent years. The selection criteria to be included in the list of Priority Municipalities (PM) were: (i) total deforested area; (ii) deforested area over the past three years; and (iii) increase in deforestation rate in at least three of the last five years (Brasil, 2007). The first list was released in 2008 with thirty-six PMs, seven more were included in 2009 and 2011 and two more in 2012.

The primary mechanism of action was the adoption of a hotspot policing strategy that focused the attention of the Law Enforcement on areas with high crime rates. With a larger

[2]Legal Amazon is a geopolitical division of Brazil, includes the whole Brazilian Amazon Biome, part of the Cerrado and the Pantanal.

[3]Brazilian Institute for the Environment and Renewable Natural Resources

[4]Real-Time Detection of Deforestation System

[5]National Institute for Space Research

share of dedicated Ibama resources, alerts issued in these areas were prioritized, private land titles were revised, and licensing, georeferencing requirements, and authorizations for clearing in rural properties were made harsher (Assunção and Rocha, 2014). Furthermore, other non-command and control mechanisms became anecdotally documented, like the punishment in the polls of local politicians and the refusal of the supply chains to buy cattle from embargoed areas (Abman, 2015; Cisneros et al., 2015).

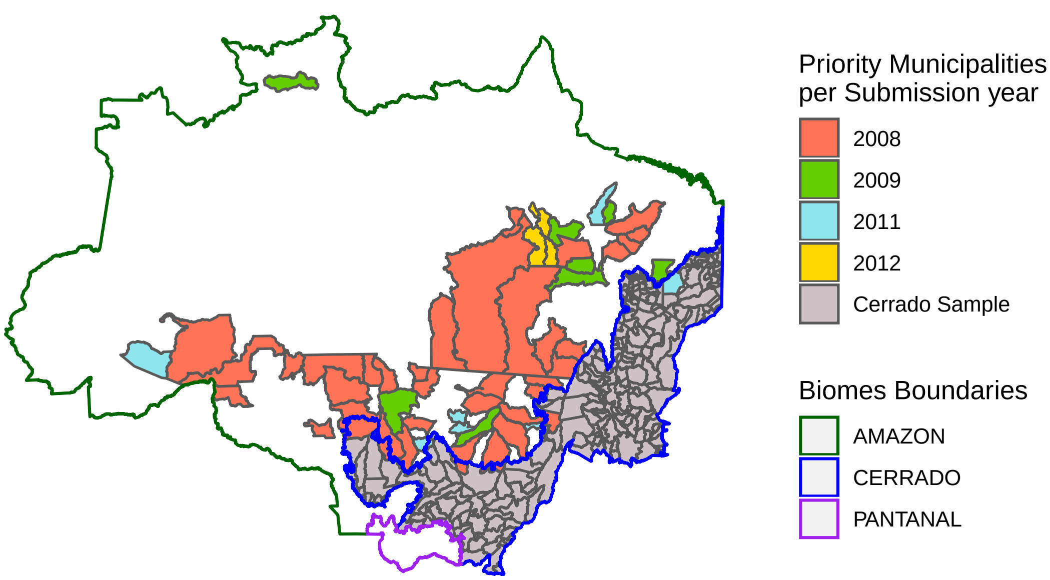
This policy has received some attention from the impact evaluation literature. Regarding direct impacts, there is evidence of a significant reduction in deforestation for these areas (Assunção and Rocha, 2014; Arima et al., 2014; Cisneros et al., 2015). For the mechanism of impact, Assunção and Rocha (2014) argue that law enforcement fully explains the reduction, while Cisneros et al. (2015) estimate that non-enforcement mechanisms account for the impact. For spatial spillover effects, Cisneros et al. (2015) find no evidence of either deterrence or displacement effects using a combination of matching and double difference frameworks. On the other hand, Andrade (2016) uses a spatial differences-in-differences model and estimates a significant and economically relevant deterrence effect, showing a reduction in forest clearing for non-blacklisted municipalities with PM as neighbors. Note that both studies look only to tropical deforestation, which excludes the majority of the Cerrado Municipalities. Therefore, this paper aims to fill this gap by focusing on spatial spillovers looking at land use changes in the Cerrado.

The Cerrado might be an interesting study case and reveal different findings because most of the policies implemented in the mid-2000s were aimed at the Amazon Biome and increased the difference between the cost of deforestation in these two areas. First, the forest code requires 80% of conservation in private properties in the Amazon Biome, while for the Cerrado it requires only 35% when inside the Legal Amazon and 20% outside it (Brasil, 2012). Secondly, the innovative monitoring system (DETER) only detects tropical clearings, thus excluding the majority area of the Cerrado that is composed by savanna-vegetation. Lastly, almost half of the Amazon Biome is considered a Protected Area, while this share is only

18.6% in the Cerrado (Nobre, 2014). As a result, Cerrado municipalities seem to be much more attractive for displacement compared to non-blacklisted Amazon municipalities.

3 Data

The empirical analysis is based on a municipality-by-year panel dataset built from multiple publicly available sources, from 2004 through 2014. The sample includes the Cerrado biome of all the municipalities inside the Legal Amazon comprising 355 municipalities. Figure 1 shows the Legal Amazon region with the sample in grey, the biomes spatial boundaries, and the blacklisted amazon municipalities colored by the year of submission. This section briefly describes the variables used in the analysis. More details about the construction process and about the data sources are given in Section 9.1 (Appendix).



Note: The figure maps the Brazilian Legal Amazon and biomes spatial boundaries.
The gray region indicates the spatial sample for the analysis, defined as the area inside the Cerrado biome.
The colored municipalities are the ones in the blacklist, color varies accordingly with the submission year.
Data sources: IBGE, MMA, MapBiomas.

Figure 1: Map of Cerrado Sample and Amazon Priority Municipalities

3.1 Farming

Deforestation in the Cerrado is not as simple to classify as deforestation in the Amazon, so to overcome the data limitation I calculated the share of municipal area destined for farming to be a proxy for deforestation. As shown by Noojipady et al. (2017), 88% of the forest loss was destined to farming in the Cerrado. Therefore, these variables are closely related.

3.2 Treatment - Distance criteria

The treatment definition is based on the distance to the closest PM of the 2008 list. If the distance is less than 250 km, the unit is part of the treatment group, and if it is above 250 km, it is part of the control group. In a robustness check, I change this cut-off from 250 to 300 km. To allow spatial heterogeneity, the treatment group is divided into five subsets based on distance intervals of 50 km.

3.3 Agricultural Commodity Prices

Following Assunção et al. (2012), I use an exogenous commodity price series with annual international prices for corn, soybean, rice, sugarcane, and cattle. Then, each crop is weighed based on the share of the municipal area used as farmland for production averaged from 2000 through 2003. Moreover, for cattle, the ratio of heads of cattle to the municipal area is used for the same period. I use the period pre-sample to avoid endogeneity issues due to changes in production as a result of the policies starting to be implemented in 2004.

3.4 Weather Control

Based on the literature that forest loss can affect a region's microclimate (Nobre et al., 1991; Aragão et al., 2008), and that meteorological conditions can also affect land use decisions, controls for annual average temperature and annual total precipitation are added.

3.5 Policy control

Finally, I also use variables that capture the presence of other policies as the share of the protected area and a dummy for being a Priority Cerrado municipality. These policies might be admittedly endogenous because they probably are affected by the treatment. Thus I only use them for robustness purposes.

3.6 Summary Statistics

Tables 3 and 4 in Section 9.2 present the means and standard deviations by year of the variables used in the empirical analysis.

4 Empirical Strategy

4.1 Model

The proposed empirical strategy aims at exploring how the implementation of the Amazon Blacklist changed the land use trends in near Cerrado municipalities, which indicates the presence of displacement effects. I draw on a differences-in-differences framework to infer causality. The benchmark equation is:

$$Farming_{i,t} = \sum_{break=0-50km}^{200-250km} (\rho_{break} * Treat\_break_i * After_t) + X'_{i,t} * \omega + \alpha_i + \theta_t + \varepsilon_{i,t} \quad (1)$$

where $Farming_{i,t}$ is the fraction of the municipality i destined for farming in year t; $break$ comprises five distance intervals (0-50km; 50-100km; 100-150km; 150-200km; and 200-250km); $Treat\_break_i$ is an indicator that equals 1 when the distance from muni i to the closest PM is contained in the $break$ interval; $After_t$ is an indicator equals 1 when year t is greater than 2008; $X'_{i,t}$ is a vector of muni-level controls for weather, agricultural prices and observed policy; α_i and θ_t are, respectively, municipality and year fixed effects; $\varepsilon_{i,t}$ is the muni-year idiosyncratic error. Estimates are robust to heteroskedasticity, and standard errors are clustered at the municipality level in all specifications, making them robust to intra-municipal serial correlation (Bertrand et al., 2004). ρ_{break} are the difference-in-differences estimators of the spillover effect for each distance *break*. It is also relevant to notice that the control group, in this case, is the omitted *break* category composed by all the municipalities more than 250 kilometers distant from the PMs.

4.2 Identifying Assumption

The fundamental identifying assumption in the differences-in-differences framework is that the control group trend is a valid counterfactual for the treatment group trend in the absence of treatment. One can never directly test it since only one potential outcome is observed each year. However, to get confidence that this assumption holds, I inspect the trends of the treatment and control groups when they both have the same treatment condition, for example before the policy, in Section 6.1.

Looking at the pre-trends can give us confidence, but certainly does not pin down the identification. One might still argue that the control group is also being affected by the policy, or that, after the policy, variables relevant to land use decisions might have changed in ways that made the treatment and control group trends diverge for reasons not associated with the policy itself. For the former, I argue that 250 km, a considerable amount of distance, still is an arbitrary cut-off, so in Section 6.2 I check if our results hold using a more conservative cut-off of 300 km. For the latter, I not only add year and municipality fixed effects, controlling for all time-invariant and unit-invariant variables but also control for some covariates that vary across time and municipalities to mitigate omitted variable bias.

5 Results

5.1 Main results

Table 1 provides the estimated coefficients of the displacement effects for each distance break. All specifications include municipality and year fixed effects.

Table 1: Distance Breaks Regression Results

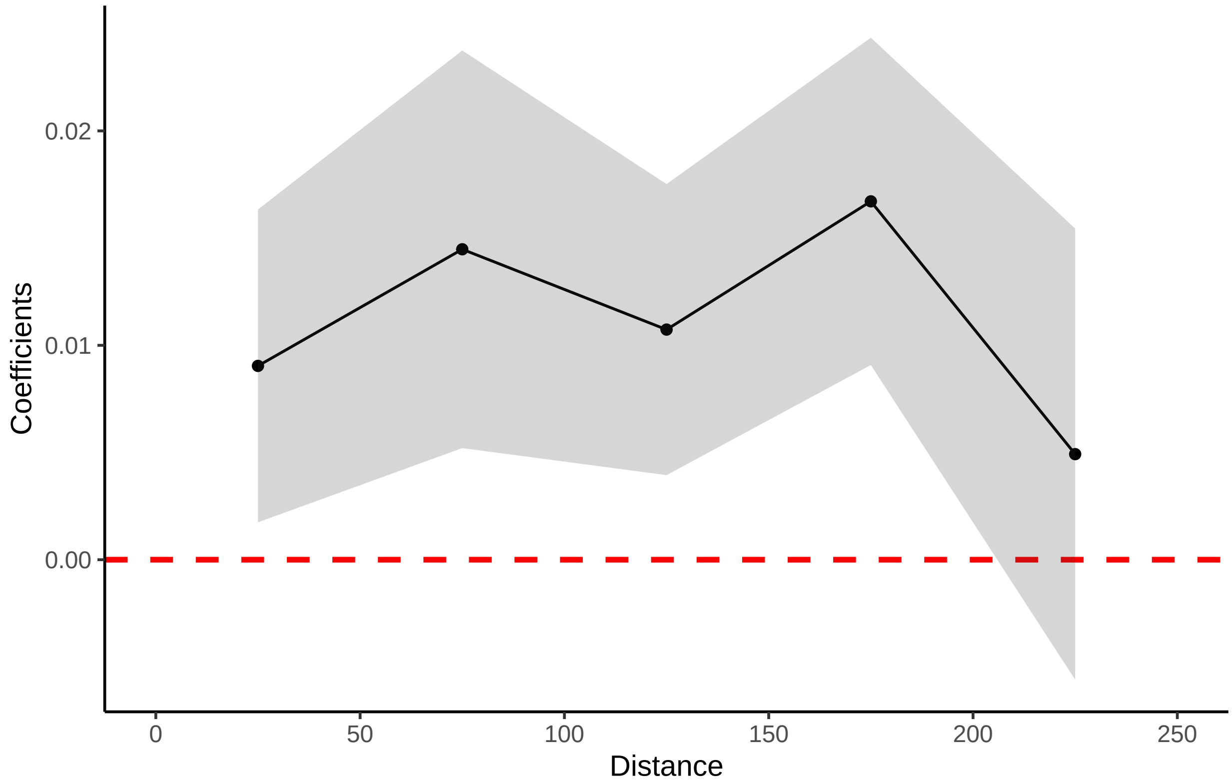
VARIABLES	(1) Farming	(2) Farming	(3) Farming	(4) Farming
After x Treat (0-50km)	0.01034***	0.00980***	0.00904**	0.00905**
	(0.00364)	(0.00363)	(0.00354)	(0.00377)
After x Treat (50-100km)	0.01622***	0.01540***	0.01448***	0.01468***
	(0.00466)	(0.00457)	(0.00449)	(0.00461)
After x Treat (100-150km)	0.01224***	0.01190***	0.01073***	0.01078***
	(0.00330)	(0.00332)	(0.00329)	(0.00333)
After x Treat (150-200km)	0.01834***	0.01825***	0.01671***	0.01671***
	(0.00391)	(0.00388)	(0.00370)	(0.00368)
After x Treat (200-250km)	0.00632	0.00628	0.00492	0.00490
	(0.00518)	(0.00518)	(0.00510)	(0.00510)
Observations	3,905	3,905	3,905	3,905
Number of Municipalities	355	355	355	355
R-squared	0.21011	0.21244	0.22627	0.22655
ctrl FE	yes	yes	yes	yes
ctrl weather	no	yes	yes	yes
ctrl prices	no	no	yes	yes
ctrl policy	no	no	no	yes

Notes: The table reports fixed effects coefficients for Equation 1 (Section 5.1). The dependent variable is the share of the municipal area destined for Farming. Reported independent variables are the diff-in-diff estimators. After is a policy indicator $= 1\{year > 2008\}$. Treat ($break$ km) are treatment indicators $= 1\{Distance\, to\, the\, closest\, PM \subset break\}$. The control group is the omitted category $1\{Distance > 250km\}$. Controls are added gradually to the specification. The no/yes markers in bottom rows indicate the inclusion of the following sets of muni-level controls: (i) muni and year fixed effects; (ii) climate: precipitation and temperature; (iii) weighted agricultural prices: cattle, corn, soybean, rice, and sugarcane; and (iv) observed policy: share protected area and cerrado priority municipality status. The muni-by-year panel includes 355 municipalities located in the Cerrado biome within the Legal Amazon and covers 2004 through 2014 period. Standard errors are robust and clustered at the municipality level. Significance levels: *** p<0.01, ** p<0.05, * p<0.10.

Column 1 controls only for fixed effects. Column 2 adds temperature and precipitation controls. Column 3 adds weighted agricultural prices including, cattle, soybean, rice, sugarcane, and corn. Column 4 adds observed policies including the share of protected area and a dummy for Cerrado priority municipality.

The preferred specification is Column 3 because it uses all the sets of controls, except for the one that might be endogenous. These results corroborate the hypothesis of displacement effects. For municipalities between 50 and 200 km, the policy had a positive and significant impact at the 1% level, generating an increase in the range of 1.07 and 1.67 percentage points in the fraction destined for farming, representing a deforested area of similar magnitude. For municipalities less than 50 km away there is still a significant impact, but at the 5% level and smaller in magnitude. That can be explained by the fact that when offenders are very close to areas with hotspot policing they might perceive an increase in the cost of illegal deforestation, thus reducing their activities and attenuating the displacement effect. Also, it can be inferred that the displacement reach is 200 km, considering that the coefficient for the 200-250 km break is not statistically different from zero at any usual significance level. By looking across the columns it is clear that the coefficients are stable, serving as evidence that they represent a causal impact.

Figure 2 represents the coefficients from Column 3 of Table 1 graphically, showing how the impact of the policy varies spatially with a 95% confidence interval.



Notes: The graph plots the fixed effects coefficients from our preferred specification (Table 1, column 3) and the shaded area is the 95% confidence interval.

Figure 2: Distance Breaks Regression Coefficients

5.2 Counterfactual Simulation and Economic Impact

To assess the economic impact, I propose a counterfactual exercise setting all the treatment variables to zero. Consequently, I simulate a scenario with no implementation of the blacklist policy, so I calculate the predicted value of the farming area by multiplying the farming share by the municipality area (Estimated Farming Area), and finally doing the same for the baseline model with the observed data (Observed Farming Area).

Table 2: Counterfactual Exercise - No Blacklist Policy

Year	Observed Farming Area	Estimated Farming Area	Difference observed-estimated
2009	282,180	281,225	955
2010	284,571	283,666	905
2011	287,324	286,369	955
2012	291,189	290,081	1,108
2013	293,710	292,643	1,067
2014	295,251	294,278	973
Total 2009-2014	1,733,225	1,728,262	4,963

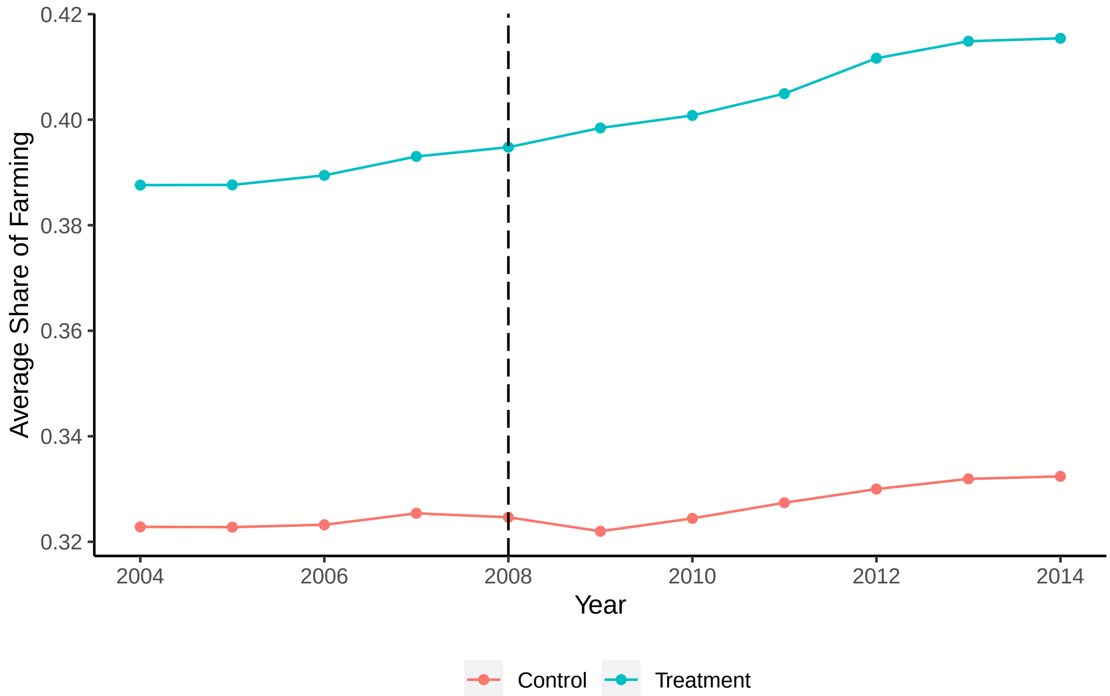
Notes: All areas are in square kilometers. The counterfactual simulation is conducted using estimated coefficients from our preferred specification (Table 1, column 3). The hypothetical scenario sets the treatment interaction terms from 2009 through 2014 as zero to capture the complete absence of the blacklist policy. Observed Farming Area shows total recorded sample area destined for Farming; Estimated Farming Area shows total estimated sample Farming Area in the hypothetical scenario; Difference reports the difference between observed and estimated totals.

Comparing these two annual results (Difference observed-estimated), I calculate an increase of 4,963 km2 due to displacement effects, for 2009 through 2014, in the sample. Then I compare it to the direct impact, estimated by Assunção and Rocha (2014), of 11,396 km2 of avoided clearings due to the same policy. To make the comparison more similar, I use the average displacement per year: 827.17 km2 and the direct impact per year: 2,849 km2. Using these numbers, I calculate that the cross-biome leakage generated an offset of 29% in the policy impact.

6 Robustness Checks

6.1 Parallel Assumption Tests

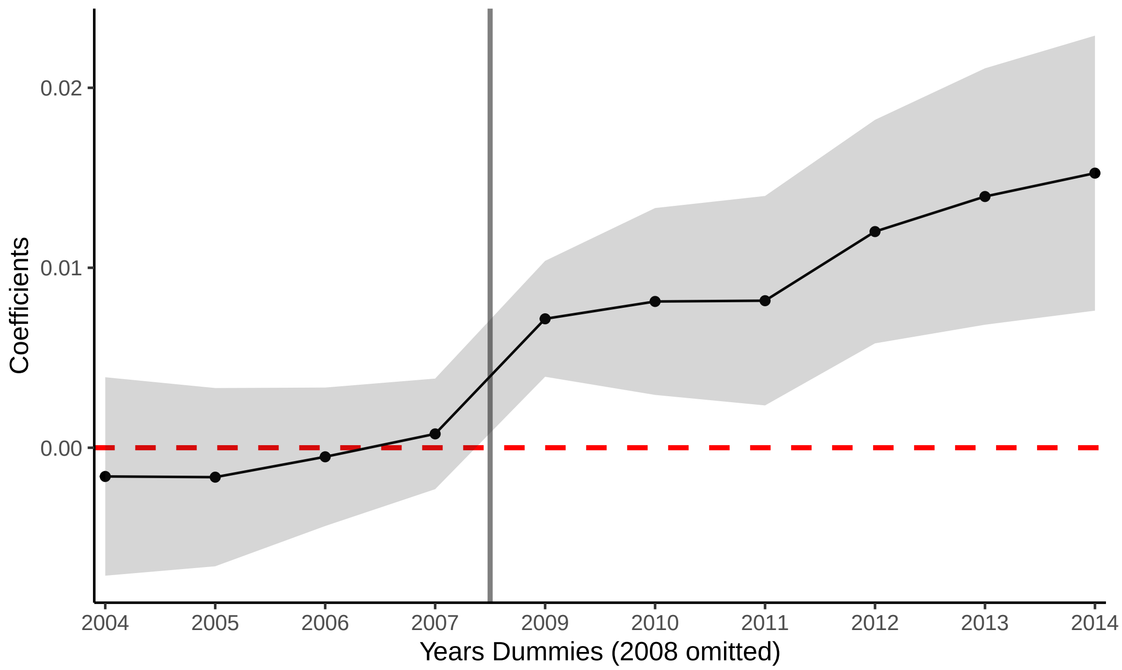
To get a visual notion of the trends, I calculate the average farming share for treatment and control groups for each year and plotted it as shown in Figure 3.



Notes: The graph plots the trends of the average area destined for Farming for treatment and control groups for the period 2004−2014. Data Sources: MapBiomas

Figure 3: Visual Inspection of Parallel Trends

As shown above, the pre-trends are very similar. However, to formally test the parallel assumption for the pre-trends, I use the leads and lags regression. This model requires a treatment time dummy for each year before and after policy implementation (2008), and the same set of covariates is used as a control as in the preferred specification from column 3 of Table 1. Figure 4 represents the coefficients for the time dummies graphically.



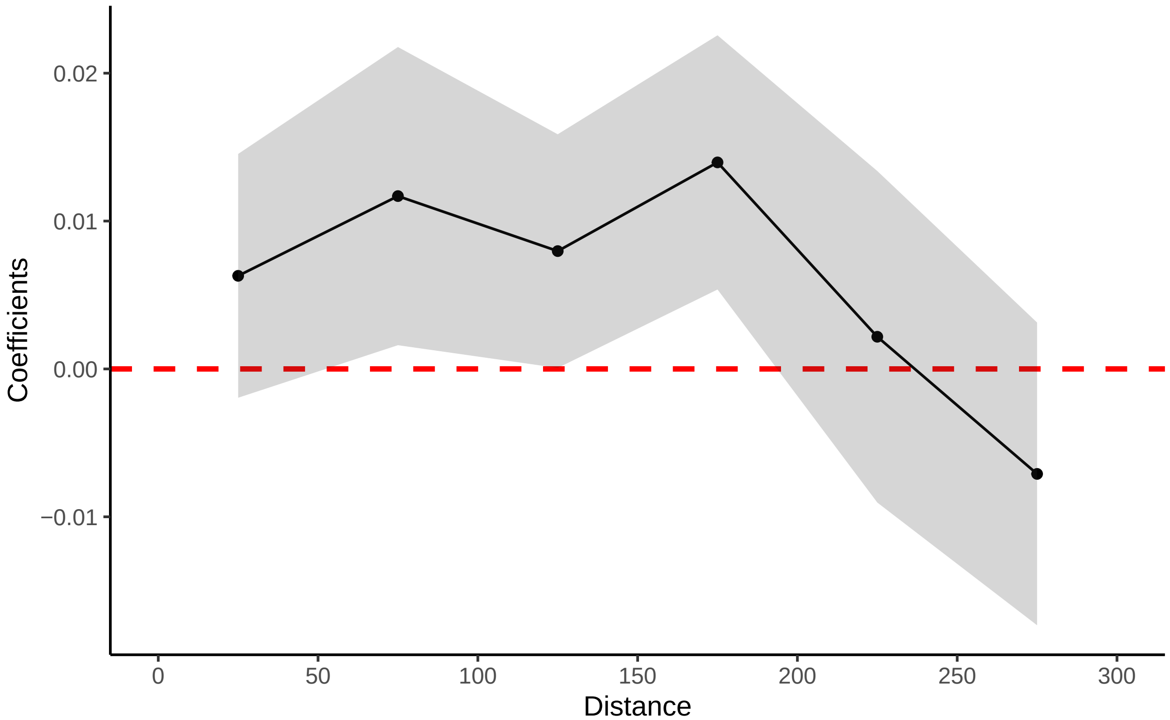
Notes: The graph plots the fixed effects coefficients of the Leads and Lags dummies using the set of controls from our preferred specification (Table 1, Column 3).
The shaded area is a 95% confidence interval.
Data Sources: MapBiomas, MMA and IBGE

Figure 4: Parallel Assumption Test - Leads and Lags

Based on this graph, I can assume that the parallel pre-trends hypothesis cannot be rejected in any year before the policy and that there is a persistent and increasing impact of the policy in the following years. This result is consistent with the coefficients of Table 1 and also with the fact that more municipalities were added to the list in 2009, 2011, and 2012. In summary, the evidence supports the claim that both groups have similar trends in the absence of the treatment.

6.2 Treatment Cut-off Robustness Check

As discussed in Section 5.2, it is necessary to check if the results are being driven by an arbitrary choice of treatment cut-off and also to be sure that the control group is not being affected by the policy. To address both concerns, I use the same model (1), though add a sixth $Treat\_break$ dummy for the 250-300 km distance interval instead of classifying the control group, like the ones more than 250 km far away I consider using a 300 km threshold.



Notes: The graph plots the fixed effects coefficients from the robustness specification similar to the one used in (Table 1, column 3) but adding an extra regressor: After x Treat (250−300km). In this case, the control group are the municipalities more than 300km far away from the closest PM (instead of 250km as in the baseline specificatio

Figure 5: Distance Breaks Robustness Coefficients - 300km

Figure 5 shows results similar to the ones in Figure 2. The coefficients for intermediate distances (50-200 km) are significant at the 5% level and have similar magnitudes. The reach of displacement is 200 km because neither coefficient for (200-250 km and 250-300 km) is statistically significant. The (0-50 km) coefficient became non-significant. However, as explained before, the closest interval might have some deterrence effect mixing the results. It is also important to notice that this specification might suffer from a lack of statistical power, because when I changed the cut-off I made the proportion of units in the treatment group to be more distant from the optimal ratio of equal distribution, thus there is less variance on the regressors and less precision for statistical inference. Briefly, it can be concluded that the overall results did not change much. Therefore, they are robust to the cut-off definition.

6.3 Caveats of the model

Although this model seems to identify a causal spatial spillover impact of the policy, it can still suffer from omitted variable bias and possible biases caused by spatial autocorrelation in the dependent variable. For example, if unobserved policies were implemented after 2008, in the control group and not in treatment it might make us overestimate the impact, but since I am restricting the sample to the same administrative region Legal Amazon, it minimizes the probability of these biases.

7 Final Considerations

This research provides evidence of cross-biome leakage across the Amazon and the Cerrado borders. Leakage can make it more difficult to achieve national reductions in emissions. It fills the gap in the anti-deforestation evaluation literature by focusing on a less explored region, Cerrado, and less explored impact, spillovers. This paper also contributes to the Crime Literature by estimating spillover effects generated by a hotspot policing strategy.

The results suggest that the blacklist policy generated a displacement effect, mostly at intermediate distances (50-200 km). I use a differences-in-differences strategy to establish a causal relationship between the policy of interest and the side-effect generated by it. Robustness tests provide supporting evidence for the parallel trends assumption, and the coefficients are stable when controls are gradually included and when the treatment definition changes. Moreover, I assess the economic relevance using a counterfactual simulation, estimating a scenario with no blacklist policy. From this exercise, I estimate a leakage of 4,963 km2, representing an offset of 29% of direct impacts.

In the light of these results, I argue that is necessary to extend existing policies, like DETER, to be able to detect clearings in other vegetation and allow the government to issue more alerts in the Cerrado. Also, it is necessary to make more specific policies that take into account biome differences and attempt to reconcile agricultural production and conservation efforts. Lastly, this paper and previous works (Gonzalez-Navarro, 2013; Davis, 2008; Andrade, 2016; Pfaff and Robalino, 2017; Gandour, 2018) have shown spillover effects to be relevant, so these effects always need to be considered in Policy Impact Evaluations.

For future research, as pointed out by Andrade (2016), land use and forest loss variables may be spatially dependent. Therefore, a spatial econometric model should be used to correct for this spatial autocorrelation. Furthermore, spatial heterogeneities might be better captured when using data in a more finer-scale than the municipality level (Donaldson and Storeygard, 2016). Finally, it would be useful to include in a single analysis of the spillover effects in both biomes to calculate the net impact on neighbors.

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9 Appendix

9.1 Data - Variable constructions and sources

To define the biomes boundaries I used the one made available by MapBiomas, based on Biomes Limits Map from IBGE and refined using the Territories Limits and the phytophysiognomies Map. For the municipalities boundaries, I used the 2015 IBGE definition.

9.1.1 Farming

MapBiomas is a multi-institutional initiative that since 2015 aims to generate a time-series with annual data of the land cover and land use for all Brazilian biomes. The initiative automatizes the satellite images with advanced techniques like Random Forest. So, to obtain a good proxy of deforestation, I aggregated the categories relative to farming use (pasture and agriculture) to the municipality level, using the collection 2.3, available in the raster format with 30 meters resolution. After aggregating the area of interest, I divided it by the municipality area that is inside the Legal Amazon and the Cerrado biome, generating the fraction of the municipal area destined to farming.

9.1.2 Treatment - Distance criteria

To define the treatment region, I first calculated the distance to the closest Priority Municipality of the 2008 list, then generated indicator variables based on 50 km distance breaks. The breaks used were: 0-50 km, 50-100km, 100-150 km, 150-200 km, 200-250 km, and 250-300 km. In the baseline, I include all of these five dummies, so the control group is the omitted category (i.e., all municipalities above 300 km). To calculate the distances, I spatialized the PM list provided by the MMA based on the 2015 IBGE municipalities division.

9.1.3 Agricultural Commodity Prices

The source for the annual price index (USD, 2010 base year) was the World Bank Pink Sheet. I gathered data for soybean, rice, sugar (as a proxy for sugarcane), and cattle. To add variance across the municipalities, I weigh the prices by the commodity relevance in each municipality. For that I used data from IBGE on agricultural production and the following formula:

$$PPA_{itc} = PP_{tc} * A_{ic,2000-2003}$$

where PPA_{itc} is the weighted real price of commodity c in municipality i and year t; PP_{tc} is the Pink Sheet real price of commodity c in year t' and $A_{ic,2000-2003}$ is the municipality specific weight. For crops, the weight is given by the share of the municipal area used as farmland for crop c in municipality i averaged over 2000 and 2003. To avoid endogeneity, I only consider the period before the sample for the analysis and the policies implementation. For beef cattle, given that annual pasture specific for beef is unobservable, the weight is given by the ratio of heads of cattle to the municipal area in municipality i averaged over 2000 and 2003.

9.1.4 Weather Control

I compiled weather data from the Matsuura and Willmott (2015) dataset that created a regular grid worldwide of estimated precipitation and temperature over land. They use extrapolations techniques based on data collected at weather stations. It is a monthly dataset, so for precipitation, I calculated a total value by year and for temperature and took the annual average.

9.1.5 Policy control

For the Cerrado Priority Municipalities we did the same spatializing process as the Amazon Priority Municipalities and then created an indicator variable equals to 1 if the municipality i in year t was in the list. For the protected areas, I gathered data from multiple sources (FUNAI, ISA and MMA) and calculated the fraction of the municipal area that was legally protected for each sample year.

9.2 Summary Statistics

Table 3: Summary Statistics Table 2004-2008

	2004	2005	2006	2007	2008
Farming	0.373	0.373	0.374	0.378	0.379
	(0.195)	(0.193)	(0.192)	(0.192)	(0.193)
Distance	168.5	168.5	168.5	168.5	168.5
	(123.2)	(123.2)	(123.2)	(123.2)	(123.2)
After	0	0	0	0	0
Treatment (0-50 km)	0.180	0.180	0.180	0.180	0.180
Treatment (50-100 km)	0.130	0.130	0.130	0.130	0.130
Treatment (100-150 km)	0.169	0.169	0.169	0.169	0.169
Treatment (150-200 km)	0.175	0.175	0.175	0.175	0.175
Treatment (200-250 km)	0.118	0.118	0.118	0.118	0.118
Treatment (250-300 km)	0.0873	0.0873	0.0873	0.0873	0.0873
Control (>250km)	0.228	0.228	0.228	0.228	0.228
Control (>300km)	0.141	0.141	0.141	0.141	0.141
Price, Corn	1.419	1.214	1.462	1.851	2.341
	(2.700)	(2.310)	(2.782)	(3.522)	(4.455)
Price, Sugarcane	0.000333	0.000445	0.000649	0.000417	0.000491
	(0.00188)	(0.00252)	(0.00367)	(0.00236)	(0.00278)
Price, Soybean	8.646	7.512	7.165	9.653	12.19
	(24.89)	(21.62)	(20.63)	(27.79)	(35.10)
Price, Rice	3.051	3.563	3.701	3.734	6.902
	(4.074)	(4.758)	(4.942)	(4.986)	(9.216)
Price, Cattle	91.20	92.09	87.42	84.18	94.18
	(77.55)	(78.31)	(74.34)	(71.58)	(80.08)
Rain	331.2	278.6	327.3	278.9	323.0
	(211.9)	(192.0)	(211.7)	(189.3)	(206.8)
Temperature	25.65	25.89	25.65	25.99	25.45
	(1.345)	(1.488)	(1.275)	(1.303)	(1.368)
Cerrado Priority Muni	0	0	0	0	0.0310
Protected Area	0.110	0.110	0.112	0.112	0.112
	(0.239)	(0.239)	(0.239)	(0.239)	(0.239)
Observations	355	355	355	355	355

Note: The table reports annual averages and standard deviations (in parenthesis) at the municipal level for the variables used in the analysis. The sample includes all Legal Amazon Municipalities of the Cerrado Biome. Sources and units: Farming (share of the municipal area destined for Farming, MapBiomas); Distance (distance in kilometers to the closest Pirority Muni (2008), IBGE and MMA); After $1\{year > 2008\}$; Treatment (*break* km) $1\{Distance \subset break\}$; Control (>$x$ km) $1\{Distance > x\,km\}$; Prices (year 2010 USD, World Bank, PAM/IBGE and PPM/IBGE); Rain (annual average millimeters, Matsuura e Willmott (2015)); Temperature (annual average celsius degrees, Matsuura e Willmott (2015)); Cerrado Priority Muni (MMA), Protected Area (share of the municipal area that is protected, INCRA and FUNAI). The table was divided into two parts 2004-2008 and 2009-2014. Standard deviations were omitted for dummy variables.

Table 4: Summary Statistics Table 2009-2014

	2009	2010	2011	2012	2013	2014
Farming	0.381	0.383	0.387	0.393	0.396	0.396
	(0.193)	(0.195)	(0.197)	(0.196)	(0.195)	(0.195)
Distance	168.5	168.5	168.5	168.5	168.5	168.5
	(123.2)	(123.2)	(123.2)	(123.2)	(123.2)	(123.2)
After	1	1	1	1	1	1
Treatment (0-50 km)	0.180	0.180	0.180	0.180	0.180	0.180
Treatment (50-100 km)	0.130	0.130	0.130	0.130	0.130	0.130
Treatment (100-150 km)	0.169	0.169	0.169	0.169	0.169	0.169
Treatment (150-200 km)	0.175	0.175	0.175	0.175	0.175	0.175
Treatment (200-250 km)	0.118	0.118	0.118	0.118	0.118	0.118
Treatment (250-300 km)	0.0873	0.0873	0.0873	0.0873	0.0873	0.0873
Control (>250km)	0.228	0.228	0.228	0.228	0.228	0.228
Control (>300km)	0.141	0.141	0.141	0.141	0.141	0.141
Price, Corn	1.852	2.006	2.836	2.923	2.551	1.927
	(3.523)	(3.818)	(5.397)	(5.561)	(4.854)	(3.666)
Price, Sugarcane	0.000743	0.000840	0.000925	0.000772	0.000637	0.000622
	(0.00420)	(0.00475)	(0.00523)	(0.00437)	(0.00360)	(0.00352)
Price, Soybean	10.86	10.79	11.69	12.87	11.77	10.92
	(31.27)	(31.06)	(33.64)	(37.06)	(33.88)	(31.43)
Price, Rice	6.280	5.337	5.341	5.577	5.033	4.273
	(8.386)	(7.126)	(7.132)	(7.447)	(6.721)	(5.705)
Price, Cattle	84.35	103.4	112.4	116.0	114.6	141.4
	(71.73)	(87.95)	(95.58)	(98.65)	(97.43)	(120.2)
Rain	353.0	293.5	352.6	283.3	320.1	327.6
	(220.5)	(202.8)	(231.2)	(201.1)	(216.4)	(220.7)
Temperature	25.41	26.28	25.50	25.58	25.60	25.53
	(1.262)	(1.515)	(1.320)	(1.478)	(1.584)	(1.325)
Cerrado Priority Muni	0.0338	0.0338	0.0423	0.113	0.113	0.113
Protected Area	0.114	0.114	0.114	0.114	0.114	0.114
	(0.242)	(0.242)	(0.242)	(0.242)	(0.242)	(0.242)
Observations	355	355	355	355	355	355

Note: The table reports annual averages and standard deviations (in parenthesis) at the municipal level for the variables used in the analysis. The sample includes all Legal Amazon Municipalities of the Cerrado Biome. Sources and units: Farming (share of the municipal area destined for Farming, MapBiomas); Distance (distance in kilometers to the closest Pirority Muni (2008), IBGE and MMA); After $1\{year > 2008\}$; Treatment (*break* km) $1\{Distance \subset break\}$; Control (>$x$ km) $1\{Distance > x\,km\}$; Prices (year 2010 USD, World Bank, PAM/IBGE and PPM/IBGE); Rain (annual average millimeters, Matsuura e Willmott (2015)); Temperature (annual average celsius degrees, Matsuura e Willmott (2015)); Cerrado Priority Muni (MMA), Protected Area (share of the municipal area that is protected, INCRA and FUNAI). The table was divided into two parts 2004-2008 and 2009-2014. Standard deviations were omitted for dummy variables.