Spillover effects of blacklisting policy in the Brazilian Amazon

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Abstract: We analyse the effects of the Priority Municipalities List, that indicates the primary targets of environmental police monitoring, on deforestation of municipalities in the neighbourhood of the listed. We argue that being a neighbour to a priority municipality causes an exogenous variation in environmental authorities' presence, and use a difference-in-differences estimator to determine the impact of such presence on deforestation. As an innovative feature, we introduce a spatial version of this estimator to correct spatial dependence. Our estimations show that the net effect of treatment is a decrease in deforestation of 15% to 36%. This result is robust to changes in the measure of deforestation as well as in the neighbourhood criteria. Estimates also indicate that effects get weaker the greater the distance to the priority municipality.

Key-words: Deforestation, spillovers, Amazon, environmental policy
JEL codes: Q23, Q28, C21

Resumo: Este trabalho avalia os efeitos da Lista de Municípios Prioritários, que indica os principais alvos de fiscalização da autoridade ambiental, sobre o desmatamento dos municípios na vizinhança dos listados. Argumenta-se que ter um vizinho listado causa uma variação exógena na presença das autoridades ambientais, e um estimador de diferença-em-diferenças é usado para determinar o impacto dessa presença sobre o desmatamento. Uma contribuição deste trabalho é acrescentar uma versão espacial do estimador para corrigir a dependência espacial na variável dependente. Nossos resultados mostram que o efeito líquido do tratamento é reduzir o desmatamento entre 15% e 36%. Esse resultado é robusto a mudanças na medida de desmatamento, assim como na definição de vizinhança.

Palavras-chaves: Desmatamento, spillovers, Amazônia, política ambiental

1 Introduction

From 2005 to 2012, deforestation in the Brazilian Amazon decreased by 75%. This slowdown followed major changes in environmental policy. A growing literature investigating the new measures adopted since 2004 suggests that stricter command and control policies are the main cause for this reduction (Assunção et al.,

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2012, 2013; Maia et al., 2011; Hargrave and Kis-Katos, 2013). Our goal is to evaluate the impact of a targeted monitoring policy, the Priority Municipalities Lists, implemented from 2008 to 2013. Unlike most of the literature, however, we are interested in spillover effects this policy might have had on non-listed subjects.

Land cover in the Amazon region has been monitored via satellite since 2004, and deforestation alerts are produced in real time, indicating to the environmental authorities the location of new clearings. Due to dispersed urban centers and lack of infrastructure in the Amazon, access to some regions and inspection of hot spots are costly. Therefore, a targeting strategy had to be developed to enhance inspection proceedings.

In 2008, a list of Priority Municipalities was released, comprising major deforesters. Although all municipalities in the Amazon are monitored via satellite, the main change introduced consists of the inspection teams being allocated primarily to those in this blacklist. These municipalities are also subject to land property regularization proceedings, georeferencing of rural properties and credit constraints.

Some effort has already been made to evaluate the effectiveness of the blacklist. Arima et al. (2014), Assunção and Rocha (2014) and Cisneros et al. (2015) find significant reductions in deforestation on listed municipalities. While Assunção and Rocha (2014) identify command and control instruments as the main cause for the slowdown, Cisneros et al. (2015) argue that other institutional and reputation pressures were the decisive features.

Our focus, however, is to investigate if blacklisting has spillover effects, that is, if the Priority Municipality List, by focusing in one municipality, affects its non-targeted neighbours. Spillovers of environmental monitoring are documented by Shimshack and Ward (2005), and spatial spillovers in deforestation are observed by Robalino (2007) and Pfaff et al. (2015).

We argue that blacklisting caused a shock in police presence on both listed municipalities and their non-listed neighbours. Once environmental police teams are located close to blacklisted municipalities, the probability of a priority municipality's neighbour being inspected is greater than that of a municipality that is far from environmental police's units, given the high transportation costs in the region. Therefore, following a well-established literature modelling criminal activity (Becker, 1968; Stigler, 1974) and environmental monitoring (Polinsky and Shavell, 1999; Russell et al., 2013; Gray and Shimshack, 2011), we expect command and control policies to affect deforestation by changing the expected value of engaging in criminal activities.

Nevertheless, if deforestation gains are sufficiently high, it may be profitable for a closely monitored producer to relocate her activities to a less monitored municipality. In this case, unmonitored neighbours would experience an increase in deforestation. Thus, even though we expect producers in listed municipalities to reduce deforestation, the effect on their neighbours is uncertain. If the overall effect of blacklisting on untreated neighbours is to reduce deforestation, the policy's effectiveness might be underestimated. However, if blacklisting dislocates deforestation to the neighbours, not taking spillover effects into account overestimates the effects and misleads the policy's accountability.

Pfaff (1999), Chomitz and Thomas (2003), Hargrave and Kis-Katos (2013) and Robalino and Pfaff (2012) document the presence of spatial dependence on Amazon deforestation. They show that not only is accumulated deforestation very concentrated in a region known as Deforestation Arch, the decision to clear new plots is also affected by neighbouring conditions. As an innovative feature, we use a spatial econometrics methods to correct possible inefficiencies or biases caused by spatial autocorrelation.

We adopt difference-in-differences estimators to measure the List's effect on non-listed neighbour's share of remaining forest cleared. Our results suggest the net effect of blacklisting on non-listed neighbours is an average decrease of 15% to 36% in the remaining forest area cleared yearly. This finding is robust to changes on the deforestation measure and on the neighbourhood definition. We also find that deforestation is a spatially dependent process, where both spatial lags of independent variables and unobserved, spatially correlated variables affect the outcome.

This paper is organized as follows. Section 2 describes the deforestation policies implemented in the Brazilian Amazon along the 2000s and discusses the mechanisms through with the Priority Municipalities List may affect deforestation. Section 3 develops a theoretical model to describe deforestation decision. Section 4 describes our empirical approach. Section 5 presents estimation results. Section 6 discusses some robustness checks. Section 7 makes the final remarks.

2 The Priority Municipalities List

The Amazon forest is the biggest tropical forest in the world, spreading for around 5.5 million squared kilometers, 60% of which are in Brazilian territory. It provides environmental services such as biodiversity sheltering, carbon storage and rainfall regulation, apart from having water reservoirs that represent 20% of the world potable water. In the last decades, the world has paid particular attention to forests and to land use change in the context of climate change. According to the Brazilian Science Ministry, in 2005, land use change was the source of 57% of greenhouse emissions in Brazil.[1] From 2012 to 2010, there was a 76% drop in Brazil's land use change carbon emissions, most of it due to deforestation slowdown in the Amazon following a series of environmental policy changes.

2.1 PPCDAm

Since the mid-2000's, the Brazilian federal government has adopted a series of actions to inhibit deforestation in the Amazon, most of which are organized within the Plan for Prevention and Control of Deforestation in the Amazon Forest (PPCDAm). PPCDAm is the result of a large effort involving different levels of government and lays down the guidelines to coordinated action towards curbing deforestation. The Plan has three basic lines: (i) land property ordination, (ii) command and control policies and (iii) promotion of sustainable activities.[2] While the third focus was largely ignored, the first two encompassed a number of initiatives, and are pointed as reasons for the decrease in deforestation observed in the the last ten years (Hargrave and Kis-Katos, 2013; Arima et al., 2014; Cisneros et al., 2015; INPE, 2013; Assunção et al., 2012; Assunção and Rocha, 2014; Maia et al., 2011).

According to Maia et al. (2011), during the first phase of PPCDAm's implementation (2004-2006), the first basic line, land property ordination, was the most effective. These actions focused mainly on the creation of conservation units and Indian reserves. From 2004 to 2008, 25 million hectares of land were declared conservation units, and 10 million became Indian reserves.[3] Land regularization was also sought after, since it was identified that most of deforestation occurred in plots occupied by producers with no land title.

The second basic line, consisting on command and control policies, is indicated by the same authors as the most effective during the second phase of the Plan (2007-2010). Until 2004, the Brazilian Institute for the Environment and Renewable Natural Resources (IBAMA), which acts as environmental police, based its inspection efforts on anonymous complaints. In that year, a system of satellite monitoring, DETER (Real-Time Deforestation Detection), was developed by the National Institute for Space Research (INPE). The INPE has been measuring yearly deforestation in Brazil via satellite since 1988, as part of the PRODES (Satellite Monitoring of Deforestation in the Legal Amazon) project. DETER, on the other hand, produces images of the Amazon in a more timely manner, issuing deforestation alerts every two weeks. Through the coordinated action of IBAMA and INPE, it is now possible to quickly identify new deforestation spots and monitor the region more efficiently, as reflected by the number of fines issued by IBAMA.[4]

Apart from the new monitoring system, IBAMA also hired new personnel and invested on training and on spreading new units of the agency across the Amazon. Despite these actions, it is still a challenge to visit all of

[1] Ministério da Ciência Tecnologia e Informação (2013).
[2] Casa Civil (2004)
[3] Assunção et al. (2012), p. 9.
[4] prices, p. 8.

the sites indicated by DETER as deforestation hot spots.[5] For this reason, the development of the second phase of the Plan aimed at better focusing IBAMA's activities, which, according to Maia et al. (2011), accounts for the success in reducing deforestation in the period. This strategy for targeting command and control policies, the Priority Municipalities List, is the focus of this project.

2.2 Priority municipalities list

In January 2008, through the Decree nº 6.321/07, the Ministry of Environment (MMA) became responsible for creating a list of priority municipalities, which are subject to more strict environmental regulation. These municipalities, that must be a part of the Amazon biome, should be selected according to three criteria: (1) total cleared area in the municipality; (2) area cleared in the last three years; (3) increase in deforestation rate in at least three of the last five years.

Once in this list, municipalities should be monitored and supported by the federal government in the implementation of measures to reduce deforestation rates and promote sustainable activities. Rural properties in the priority municipalities are subject to registration in the National Institute for Colonization and Land Reform (INCRA), which might require georeferencing and land titles verification. The issuance of authorizations for land clearing in properties of medium to large size in these municipalities is also subject to georeferencing through the Rural-Agricultural Register (CAR), and agricultural credit granting is subject to the observance of environmental regulations in the properties. What is reportedly the most important feature of the policy, however, is that IBAMA's teams are primarily focused on listed municipalities, which means that DETER alerts located within these municipalities receive more attention.[6] Therefore, once a municipality is blacklisted, it is subject to more rigorous environmental inspection.

Every year, MMA must publish the list of Priority Municipalities, and announce the criteria to be unlisted, including, (i) having eighty percent of the municipality's private rural land monitored and in accordance with INCRA's technical criteria and (ii) maintaining deforestation under a limit established by IBAMA and announced yearly. Once a municipality is no longer in the list, it is classified as a municipality with deforestation under control and monitored.

In spite of reinforced monitoring efforts, environmental fines are seldom enforced. According to Souza-Rodrigues (2011), although the value of fines are extremely high when compared to average gross revenue,[7] between 2005 and 2009, only 0.6% of the total value of issued fines was actually paid. Assunção et al. (2013) argue, however, that these are not the only sanctions applied. Apart from being fined, producers caught illegally clearing forest are also subject to having their production seized and destroyed, their land embargoed and their machinery apprehended and auctioned.

Even though no official position as to the List's status has been taken since then, the last time a Decree listing criteria and municipalities was in 2013. That year, only the criteria to exit the list were published, and five municipalities were unlisted. Nonetheless, neither new additions nor new listing criteria were published, and interviews with local actors revealed that this is not the only instance of diminished federal attention to deforestation.

We analyse the criteria to enter the list using a logistic regression to estimate, for the years when the list was active, the probability of being listed as a function of that year's published criteria. In 2010, no municipalities were added to the list, so we did not include this year in our estimations. Results are indicate that the binding criteria vary from one year to the other. For 2009, none of the criteria were significant. An analysis of the criteria distribution still leaves doubts on how some of the cutoff values were selected. As reported by Cisneros et al. (2015) and Arima et al. (2014), we were unable to recreate the List using the announced criteria. Some

[5] Sousa (2016)

[6] Assunção and Rocha (2014).

[7] Fines range from US$ 2,300 to US$ 23,000 per hectare, compared to an average revenue of US$ 120/ha in 2006.

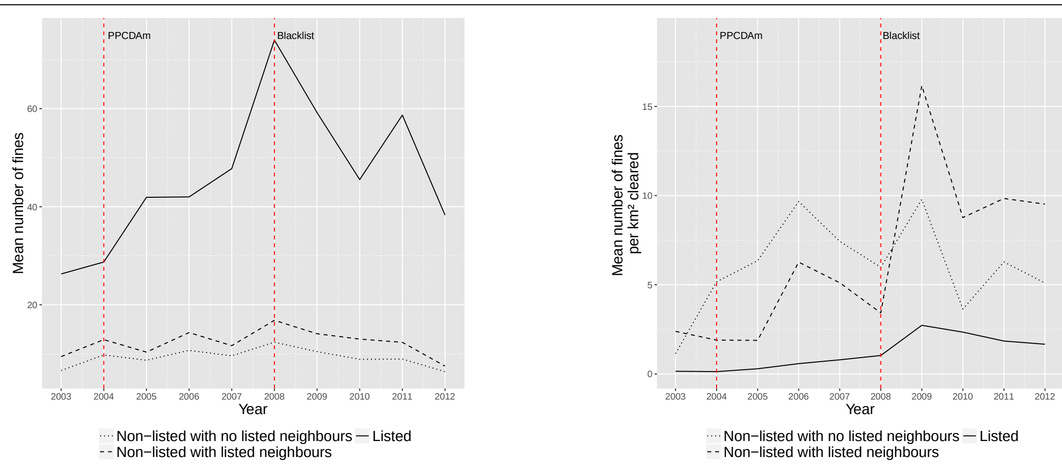
municipalities that satisfy the criteria to be listed are consistently left out of the list (the most prominent case is Itaituba), and municipalities with very low values for the variables supposed to constitute the criteria are listed (take, for example, Tapurah and Grajaú in 2011). This indicates that other criteria are also used, possibly political, which means blacklisting could be endogenous.

2.3 Mechanism discussion

Works evaluating the effect of increased inspection efforts on deforestation usually take the number of fines as a proxy for environmental police presence (Assunção et al., 2013; Hargrave and Kis-Katos, 2013). We believe, however, that this may not be a good measure. As previously discussed, environmental law enforcement in the Brazilian Amazon works as follows: all municipalities are monitored via satellite, alerts are sent to environmental authorities every two weeks, police inspects some of the alerts, and inspections may or may not result in fines. Though we have data on deforestation alerts and number of fines issued, inspections are endogenous and depend on environmental police efforts, non-observable in the data. Consequently, the number of fines is also endogenous. We propose to use entrance in the Priority Municipality List as an exogenous source of variation in police presence on non-listed neighbours of listed municipalities.

We argue that the blacklisting policy caused a shock in environmental law enforcement on both listed municipalities and their non-listed neighbours. Every municipality in the Legal Amazon is subject to satellite monitoring, but inspection efforts by environmental police varies across the region. Transportation costs in the Amazon are high, due to the lack of infrastructure in the region and to high rainfall levels. Some locations can only be accessed by boat, and travel may take more than a week. Since the Priority Municipalities List created a new system for the allocation of police inspections, targeting primarily the listed municipalities, it also affected the cost of monitoring municipalities close to them. Once IBAMA's teams come to a listed municipality, the cost of inspecting its non-listed neighbours is lower than that of monitoring more distant municipalities. This causes a decrease in the expected value of deforestation, since the probability of being fined becomes higher.

This explanation depends on the assumption that the Priority Municipalities List causes a shift in environmental police presence. We consider this a plausible assumption because it is one of the announced measures. Nevertheless, it cannot be directly tested, since there is no available record of IBAMA inspections or of the value of total penalties imposed on violators. The number of fines, however flawed a measure of police presence, can give us some insight on that.



Source: IBAMA

(a) Number of fines

Source: IBAMA

(b) Number of fines per km^2 cleared

Figure 1: Mean number of environmental fines by group

Figure 1a shows the evolution of the absolute number of fines in the region. In 2008, the first year the List was active, all groups faced an increase in the number of fines as compared to 2007, but this effect is stronger in listed municipalities and their neighbours than in the other municipalities in the biome. The downward trend in the number of fines after 2008 is probably related to deforestation slowdown, and absolute numbers are hard to compare due to the size of municipalities, as previously pointed. Therefore, we consider the number of fines to deforestation increment ratio as a proxy for monitoring stringency. This indicator is shown in figure 1b, which tells us a similar, but more interesting story: both listed municipalities and their non-listed neighbours present higher values for our stringency proxy after the List was created. Even though non-neighbours also show an initial raise in 2009, the average value for the whole period in these municipalities is actually lower than before 2008. Again, though we cannot affirm that we have causal evidence, these observations also seems to support our monitoring mechanism assumption. In fact, non-listed neighbours present a very similar stringency trend to non-neighbours, but they face a permanent shock after 2008, one that is even higher than that faced by listed municipalities.

As an alternative mechanism, municipalities could respond to the possibility of entering the list and facing economic slowdown as consequence. If there is a response to the listing, such response would have to come from the municipality, an not individual farmers, through some sort of institutional change.[8] That may be easier to observe. For such institutional effort to affect deforestation, a very well designed policy is necessary to induce individual farmers to cooperate and prevent free riding.

This economic threat mechanism assumes that the economy of listed municipalities suffers a slowdown as a consequence of their inclusion to the list. An economic slowdown could be a consequence of the greater scrutiny producers in listed municipalities are subject to regarding agricultural credit. Given the similar trends among listed and non-listed, we conducted a simple difference-in-differences test to check for impacts of blacklisting on credit availability. Results indicate that, controlling for GDP and state and year fixed effects, agricultural credit did decrease on blacklisted municipalities, but credit for cattle raising experienced a raise and the effect on the sum of both, though negative, is not statistically significant.

Assunção and Rocha (2014) highlight that the blacklist didn't affect listed municipalities' economies. Still, we propose another way to test the validity of the proposed mechanism: we use matching techniques to create control and treatment groups with equal chances of being listed. Once we match municipalities on the probability of being listed, the policy's observed effect should be attributed to other causes, more specifically, following our model, to increased monitoring.

3 Theoretical framework

The decision to clear a forested area can be modeled as part of the land use decision, which is typically characterized by a model of porftfolio composition Young (1997). We assume there are two alternative and concurrent land use possibilities: agricultural production and forest conservation. The model described below is based on pfaff1999.

Let i be a producer that disposes of a capital Q to invest in a plot of land, which will be spent in sunk costs such as buying/renting the land and machinery. In any given period t, producer i in municipality j chooses land

[8] That is the case, for instance, of Programa Municípios Verdes, which was first implemented in 2013. This program is an initiative of Pará's state government and consists in creating a pact with local actors to reduce deforestation so that municipalities can either get out of the List faster or avoid being listed at all.

use l, either preserving the standing forest or clearing it, by solving the following problem:

$$\begin{aligned} \underset{l\in\{cleared,forest\}}{\text{Max}} \pi^l_{ijt} \quad &= P^l_{ijt} Y^l_t(x_{ijt}) - CT^l_t(z_{ijt}) \\ \text{s.t.} \qquad & \begin{cases} x_{ijt} = x_{ijt}(l) \\ z_{ijt} = z_{ijt}(l) \end{cases} \quad , \end{aligned} \tag{1}$$

where P_{jt} is the price of agricultural production; Y^l_t is the output as function of a input vector x_{ijt}; and CT^l_t is the total cost of production as a function of a vector z_{ijt} that includes the price of inputs and transportation, taxes and licensing costs.

Land use decision can be described by

$$\underset{l\in\{cleared,forest\}}{\text{Max}} V^l_{ijt} = \underset{l\in\{cleared,forest\}}{\text{Max}} \pi^l_{ijt}$$

Therefore, the producer will decide to clear the plot if the profit with agricultural activity is greater than the profit from exploring the standing forest:

$$l_{ijt} = \text{cleared if and only if, } V^c_{ijt} > V^f_{ijt} \tag{2}$$

Law enforcement literature, on the other hand, models the decision to break the law as a function of the following variables (Polinsky and Shavell, 1999): g, the gain from illegal activity; π^e, the expected probability of being caught; and L, the loss caused by the punishment. A risk-neutral agent decides to break the law if and only if his gain is greater than the expected loss from punishment, i.e., if

$$g \geq \pi^e L$$

Assume, for simplicity, that forested land is not used, that is, no revenue is gained from selling forest products and the land owner does not incur in any sort of maintenance cost, so that

$$V^f_{ijt} = 0, \forall i, j, t.$$

Consider, in addition, that different plot owners have different perceptions and/or face different probabilities of being caught. Finally, suppose all deforestation is prohibited. When a firm is caught clearing land, her production will be seized and the land, embargoed, which means there will be no revenue. Apart from that, the instruments and machinery will be seized and fines may be applied, so the firm will suffer an additional loss of L_{ijt}. In this case, the gain with deforestation, that is, with buying a plot of forest land, clearing it, and then using it for production purposes, V^d_{ijt}, is different from the gain from a previously cleared plot V^c_{ijt}, and is given by

$$V^d_{ijt} = (1 - \pi^e_{ijt}) V^c_{ijt} - \pi^e_{ijt} L_{ijt}.$$

With these changes, we can model producer i's decision to deforest a land plot in municipality j in time t as

$$d_{ijt} = 1[(1 - \pi^e_{ijt}) V^c_{ijt} - \pi^e_{ijt} L_{ijt} > 0]. \tag{3}$$

Total deforested area in municipality j is then given by

$$D_{jt} = \sum_{\in i} 1[(1 - \pi_{it}) V^c_{ijt} - \pi_{it} L_{ijt} > 0] area_{ij}.$$

We will now extend this framework to allow for the possibility that the agent may move from municipality j to a municipality k, located at a distance $d_{k,j}$ from j. The producer is faced now with the decision to choose (i) if she will stay in municipality j or move to k; (ii) if she will clear the plot or not. We will assume that moving the production means incurring in dislocation costs $\phi(d_{k,j}) > 0$, since sunk costs are not entirely mobile (rents are non-refundable, transporting equipment is costly, etc.). Once forest land does not generate any revenue and dislocation costs are positive, the agent will never choose to move if not to clear the land. On the other hand, moving will only be profitable if

$$(1-\pi^e_{ikt})V^c_{ikt} - \pi^e_{ikt}L_{kjt} - \phi(d_{k,j}) > (1-\pi^e_{ijt})V^c_{ijt} - \pi^e_{ijt}L_{ijt} \tag{4}$$

Adding a few assumptions will make the analysis easier, though they are not strictly necessary. First, suppose that the value of fines is irrelevant as compared to Q, so that the loss with sanctions is due to instruments and machinery apprehension[9]. Additionally, assume that, given the investment Q, the maximum profit from land clearing is the same in any municipality. This might happen if prices and land productivity are equal in both municipalities. Although both assumptions can be easily relaxed, they make the interpretation of our results more straight forward.

These assumptions give us

$$L_{ijt} = L_{ikt} = L_t(Q_i) = L_{it} \tag{5}$$

$$V^c_{ijt} = V^c_{ikt} = V^c_t(Q_i, Z) = V^c_{it} \tag{6}$$

Then equation (4) can be written as

$$(\pi^e_{ijt} - \pi^e_{ikt})(V^c_{it} + L_{it}) > \phi(d_{k,j}) \tag{7}$$

When the expected probability of being punished increases, the expected gains decrease and the expected losses increase. Therefore, the higher the difference in the expected probability of being punished between the two municipalities, the more an agent will be willing to pay to move from one municipality to another.

Thus, combining conditions (2) and (7), we have that the decision to clear the plot in municipality j is described as

$$d_{ijt} = 1[(1-\pi^e_{ijt})V^c_{it} > \pi^e_{ijt}L_{ijt}] \times 1[(\pi^e_{ijt} - \pi^e_{ikt})(V^c_{it} + L_{it}) \geq \phi(d_{k,j})], \tag{8}$$

while the decision to move and clear a plot in municipality k is described as

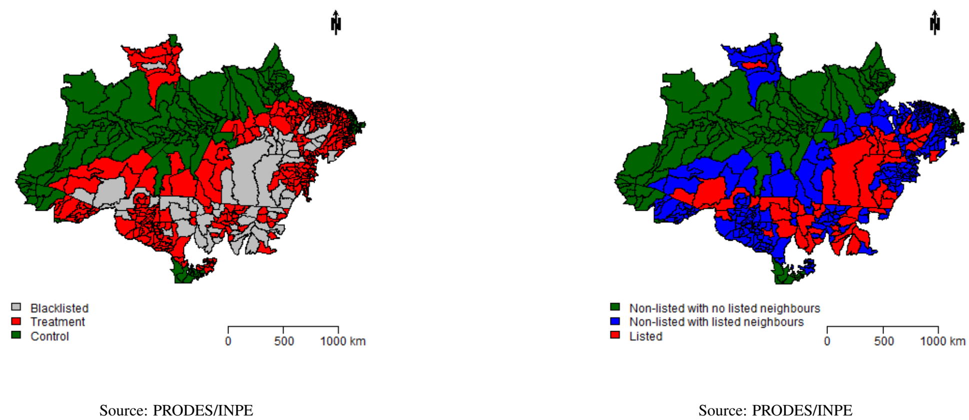
$$d_{ikt} = 1[(1-\pi^e_{ikt})V^c_{it} > \pi^e_{ikt}L_{ijt}] \times \{1 - 1[(\pi^e_{ijt} - \pi^e_{ikt})(V^c_{it} + L_{it}) \geq \phi(d_{k,j})]\}. \tag{9}$$

Thus, the difference between the expected probabilities of being caught in municipalities j and k, $p\pi^e_{ijt} - \pi^e_{ikt}$, affects the location of deforestation. Now, lets say municipality j was treated in period 1, so that the expected probability of being punished in this period is greater than that of the previous period, that is, $\pi^e_{ij1} > \pi^e_{ij0}$, and municipality k was not. The spillover effect on deforestation in k caused by treating j depends on how π^e_{ikt} is affected. If π^e_{ikt} decreases, because agents expect monitoring to focus exclusively on treated municipalities, deforestation will likely increase in municipality k, both because production will became profitable for agents on k who did not produce previously and because moving the production to k may became more profitable for producers previously located in municipality j. If π^e_{ikt} is left unchanged (or increases very little), then it may still be profitable for some producers in j, namely those with high V_{it}, to move to k, which induces an increase in deforestation in the untreated municipality k. Finally, if π^e_{ikt} increases enough, because agents expect monitoring efforts to cross municipalities' borders, then not only deforestation will not migrate from municipality j to municipality k, it will also decrease in municipality k, since it will no longer be profitable for agents previously located in k to clear the land.

[9] This assumption is not as unrealistic as it might seem. Indeed, evidence shows that fines are rarely paid and other sanctions are responsible for the costs of being caught (Souza-Rodrigues, 2011; Assunção et al., 2013).

4 Empirical approach

As discussed in section 2, we are not able to affirm that blacklisting is exogenous. However, a municipality does not choose its own location, and the agents (federal government, majors, farmers) cannot choose the neighbours of listed municipalities. Therefore, though there is potentially endogeneity in the Priority Municipality list, being a neighbour to a priority municipality is random. With this idea in mind, we build our control and treatment groups: we consider non-listed municipalities with listed neighbours as being treated, and non-listed municipalities with no listed neighbours as control group. Because the literature suggests that deforestation is a spatially dependent process (Pfaff, 1999; Chomitz and Thomas, 2003; Hargrave and Kis-Katos, 2013; Robalino and Pfaff, 2012), which is intuitive considering the spatial distribution of treated and control municipalities shown in figure 2, we test for spatial correlation and use a spatial version of our main estimator to confirm our results.



(a) Non-spatial specification

(b) Spatial specification

Figure 2: Control and treatment groups

4.1 Difference-in-differences estimator

We begin by evaluating the effect of blacklisting in non-listed neighbours through a simple difference-in-differences estimator. One advantage of using this estimator is that it controls for unobserved time invariant variables. These is particularly useful, since local characteristics that influence agricultural potential may not be easily measured.

Let y_{it} be the deforestation in municipality i and year t; x_{it} a vector of observable characteristics; $trat_{it}$ a a treatment dummy ($trat_{it} = 1$ if municipality i was treated in year t and $trat_{it} = 0$ otherwise); and $trat_i$ a dummy indicating if municipality i is in the treatment group, i.e., if it was ever treated. The equation to be estimated through ordinary least squares is

$$y_{it} = \alpha + x_{it}'\beta + \gamma trat_i + \delta trat_{it} + u_{it} \tag{10}$$

Before the treatment,

$$y^b_{it,1} = \alpha + \gamma + x_{it}'\beta + u_{it} \tag{11}$$

$$y^b_{it,0} = \alpha + x_{it}'\beta + u_{it}, \tag{12}$$

where $y^b_{it,1}$ is the deforestation in treated municipalities before the treatment, and $y^b_{it,0}$, the deforestation in non-treated municipalities before treatment. Note that the values for both groups do not need to be equal, because of parameter γ.

After the treatment, we have

$$y^a_{it,1} = \alpha + \gamma + \delta + x'_{it}\beta + u_{it} \tag{13}$$

$$y^a_{it,0} = \alpha + x'_{it}\beta + u_{it}. \tag{14}$$

The control group represents the counter factual trajectory, the one treated municipalities would follow were they not subject to the intervention. Since the trajectories were not identical to begin with, we need to correct for this difference, and then we have our treatment effect:

$$\text{ATT} = E[y^a_{it,1}] - E[y^b_{it,1}] - E[y^a_{it,0}] - E[y^b_{it,0}] = \delta \tag{15}$$

The estimated equation is

$$y_{it} = \alpha + \gamma trat_i + \delta trat_{it} + \phi_1 t + \phi_2 uf_i x'_{it}\beta + u_{it}, \tag{16}$$

where y_{it} is a deforestation measure, $trat_i$ is a dummy variable indicating if municipality i ever had a listed neighbour, $trat_{it}$ is a dummy variable indicating if municipality i had listed neighbours in year t, t is a year dummy capturing time fixed effects, uf_i is a state dummy, and x'_{it} is a set of controls including agricultural characteristics, other policies implemented and economic structure of municipalities. We also include state and year fixed effects.

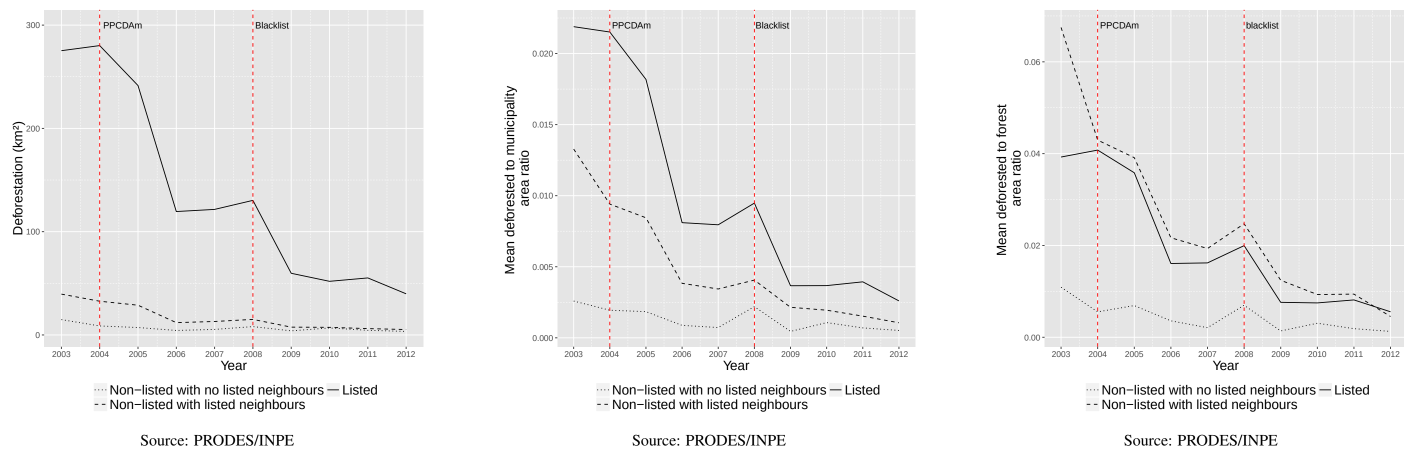
We use the ratio between the cleared area in a given year and the remaining forest area in the previous year as dependent variable. The remaining forest area was calculated from INPE data as the difference between total municipality area and the sum of accumulated deforestation, non-forest and water covered area. We chose not to use INPE's forest area due to the large number of missing observations.

The use of a relative measure of deforestation is necessary to ensure comparability between groups. Since the criteria to enter the list depend exclusively on absolute values of deforestation, listed municipalities are among the largest in the region. When using data on municipality level, we must take into account the considerable variance presented by municipalities' areas. As robustness checks, we also considered the cleared to observed forest area ratio, the cleared to total municipality area ratio and the absolute deforestation increment as dependent variables. Compared to these options, the share of remaining forest cleared has the advantage of compensating for previous deforestation, therefore taking into account the fact that as forest area gets smaller, clearing it becomes harder.

Figure 3 shows the trajectories for all three measures of deforestation. It indicates that both absolute and relative deforestation present similar trends for all groups, with the same turning points for the whole analysed period (2005-2012), in spite of different slopes. Therefore, this condition for identification of the difference-in-differences estimator is satisfied.

The neighbourhood criterion used to separate treatment and control groups is inverse distance. Because of large variance in the size of municipalities in the Amazon region, adjacency and nearest neighbours criteria are not adequate. The maximum distance cutoff is chosen according to the Akaike Information Criterion (AIC).

Following Bertrand et al. (2002), we use cluster-robust standard errors to control for shocks that affect municipalities in the same micro-region. Even though we control for regional shocks, there's evidence in the literature that deforestation is a spatially correlated process. For this reason, we also test an alternative specification, using spatial econometrics methods.



(a) Mean deforested area by group (b) % of municipality area cleared (c) % of remaining forest cleared

Figure 3: Deforestation and area by group

4.2 Spatial econometrics

Our usual ordinary least squares estimates assume that variables are randomly distributed with regard to location. However, there are situations where the values observed in one location depend on the values of neighbouring observations. In the presence of spatial dependence, non-spatial estimators may be inefficient, when only the error terms are spatially correlated, or even biased, if the dependent variable is spatially autocorrelated.

Spatial econometric methods deal with this problem by modelling spatial dependence and taking it into account during estimations. There are three main spatial models. The first, and simplest, describes exogenous interaction effects, when the dependent variable in one unit depends on the independent variables of neighbouring units. The Spatial Lag of X Model (SLX) is described in equation 17. This is the case, for example, when the treatment of one municipality affects its neighbours as well.

$$Y = X\beta + WX\theta + \varepsilon \tag{17}$$

The second model, called Spatially Autoregressive Model (SAR), accounts for endogenous interaction effects, when the dependent variable depends on the neighbouring dependent variables. This situation typically arises when decisions are made simultaneously, and only the spatially dependent equilibrium is observed. The SAR model can be described as

$$Y = \rho WY + X\beta + \varepsilon \tag{18}$$

Finally, correlated effects, when the error terms are spatially correlated, are described by the Spatial Error Model (SEM) in equation 19, and are consistent with situations where unobserved variables follow a spatial pattern.

$$Y = X\beta + u,\, u = \lambda Wu + \varepsilon, \tag{19}$$

where Y is a vector of dependent variables, X is a matrix of covariates, u is the error term and W is a matrix describing the neighbourhood relations among units.

These three models can be combined to build new models, up to a general spatial model that encompasses all three types of dependence. We, however, are only interested in two of them, the Spatial Durbin Model (SDM), that combines SAR and SLX models, and the Spatial Durbin Error Model (SDEM), that combines SLX and SEM models:

$$\text{SDM:} \quad Y = \rho WY + X\beta + WX\theta + \varepsilon \tag{20}$$

$$\text{SDEM:} \quad Y = X\beta + WX\theta + u,\, u = \lambda Wu + \varepsilon \tag{21}$$

The choice among spatial models is made in two steps [10]: first, we conduct a Langrange Multiplier test on the non-spatial specification. This test estimates the SEM and the SAR version of the original non-spatial regression model and test for statistical significance of the spatial coefficients. The LM test allows us to determine if there is spatial correlation and to choose between an autoregressive and a spatial error model. Then, we estimate the chosen model with and without lags of the exogenous variables and use AIC to chose the best specification.

4.3 Spatial difference-in-differences estimator[11]

Let us now combine the difference-in-differences estimator described in section 4.1 and the spatial models described in section 4.2. For what follows, let w_i be a $N \times 1$ vector assigning weights to other municipalities, according to a neighbourhood criterion and d_t an $N \times 1$ vector of treatment dummies ($d_{t_j} = 1$ if municipality j was treated in year t and $d_{t_j} = 0$ otherwise).

If we suppose that deforestation follows a spatially dependent process, $\mu(x_{it})$, that can take any of the forms in section 4.2, our before-treatment equations in 11 become

$$y^b_{it,1} = \alpha + \gamma + \mu(x_{it}) + u_{it} \tag{22}$$

$$y^b_{it,0} = \alpha + \mu(x_{it}) + u_{it} \tag{23}$$

Once the treatment is implemented, assuming that it may affect the neighbours of the treated, we have

$$y^a_{it,1} = \alpha + \gamma + \delta_0 + w_i' d_t \delta_1 + \mu(x_{it}) + u_{it} \tag{24}$$

$$y^a_{it,0} = w_i' d_{it} \delta_1 + \mu(x_{it}) + u_{it} \tag{25}$$

Notice that α captures the direct effect of blacklisting on the listed, while β captures the indirect effects on both listed and non-listed neighbours of the listed. Therefore, three different effects of the policy can be identified:

$$\text{ATE} = E[y^a_{it,1} - y^b_{it,1}] - E[y^a_{it,0} - y^b_{it,0}] = \delta_0 \tag{26}$$

$$\text{ATT} = E[y^a_{it,1} - y^b_{it,1}] = \delta_0 + w_i' d_{it} \delta_1 \tag{27}$$

$$\text{ATNT} = E[y^a_{it,0} - y^b_{it,0}] = w_i' d_{it} \delta_1 \tag{28}$$

In matrix notation, we have

$$Y = \alpha\iota + \mu(X) + [\delta_1 + (I_T \otimes W)\delta_2]D + U \tag{29}$$

where Y is a $NT \times 1$ vector, ι is a $NT \times 1$ vector of ones, X is a $NT \times K$ matrix of covariates, D is a $NT \times 1$ vector of dummy variables indicating the presence of treatment, I_T is a square identity matrix of dimension $T \times T$, $\otimes$ is the Kroenecker product operator, W is a $N \times N$ neighbourhood weight matrix and U is a $NT \times 1$ vector of errors. μ,α and β are the parameters to be estimated.

The term $(I_T \otimes W)D\beta$ in equation 29 is the average indirect effect of treatment (on both treated and non-treated municipalities). However, this effect may vary among this two categories of municipalities, so that the average effect expressed by β might be misleading. The different effects on treated and untreated neighbours could be captured through a decomposition of the W matrix:

$$I_T \otimes W = W_{t,t} + W_{t,nt} + W_{nt,t} + W_{nt,nt}, \tag{30}$$

[10] See Florax et al. (2003) and Anselin and Florax (1995)

[11] This section follows Chagas et al. (2016).

where

$$W_{t,t} = diag(D) \times (I_T \otimes W) \times diag(D) \tag{31}$$
$$W_{t,nt} = diag(D) \times (I_T \otimes W) \times diag(\iota - D) \tag{32}$$
$$W_{nt,t} = diag(\iota - D) \times (I_T \otimes W) \times diag(D) \tag{33}$$
$$W_{nt,nt} = diag(\iota - D) \times (I_T \otimes W) \times diag(\iota - D) \tag{34}$$

and $diag(D)$ is a $NT \times NT$ matrix with D in the main diagonal and zeros elsewhere, ι is a vector of ones and $W_{i,j}$ represents the neighbourhood relations of municipality j on municipality i (i, j=t, for treated, or nt, for untreated).

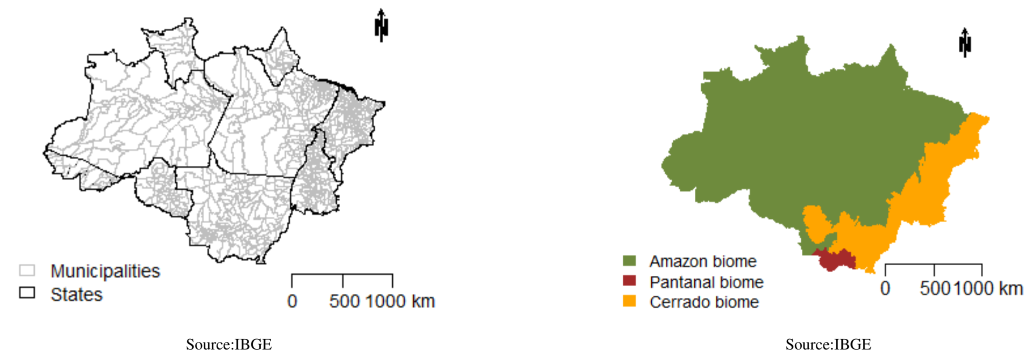
Then, substituting (30) in (29) gives us

$$Y = \alpha\iota + \mu(X) + [\delta_0 + W_{t,t}\delta_1 + W_{nt,t}\delta_2]D + U^{12} \tag{35}$$

Equation (35) is a Spatial Difference-in-Differences model that incorporates spill-over effects of the treatment on non-treated municipalities. The $\mu(X)$ term's form depends on the spatial model chosen. Note that we cannot remove listed municipalities from the sample, because that would compromise the estimation of the correct spatial dependence process. However, we know that coefficients δ_0 and δ_1 area probably biased, so we're only interested in δ_2, the ATNT estimator, that gives us the effects of blacklisting on non-listed neighbours of blacklisted municipalities.

4.4 Data description

Our database consists in a panel covering the municipalities within the Amazon biome from 2005 to 2012. The PPCDAm was launch in March 2004, so 2005 is the first year for which it was active for the whole INPE period. Since the criteria to enter the List were not published for 2013, we suspect other measures might not have been implemented, so we end our time series in 2012. During the analysed period, the Brazilian Legal Amazon covered 771 municipalities in 9 different states. Our spatial references are IBGE's maps on municipality frontiers of 2007. Figure 4 shows the political division and the different biomes present in the Amazon region. Our final sample comprises the 503 municipalities that have more than 40% of their area within the Amazon biome.



(a) Political limits (b) Biome limits

Figure 4: Amazon region political and biome limits

[12] Note that $W_{nt,nt}D$ and $W_{t,nt}D$ are both null vectors, which is why they do not appear in the equation.

Data on yearly deforestation comes from INPE's PRODES. Since deforestation occurs during the dry season, most images are taken between July and September, and yearly deforestation rates are calculated having August 1st as reference-date.[13] The land-use is identified from the satellite image with better visibility (minimum cloud coverage) and classified as forest, non-forest, cleared, water and cloud according to the image-fraction of soil, shadow and vegetation, and PRODES can identify cleared areas larger then 6,25 ha. PRODES also calculates total accumulated deforestation, which could be an important control, since municipalities with little remaining forest areas should present smaller deforestation rates. Other variables from this same database are the area unobserved and the area covered by cloud in final satellite images. As discussed by Butler and Moser (2007), these are relevant as measurement errors controls.

The value of rural credit provided to each municipality in a given year is available in the Statistical Yearbook of Rural Credit of the Brazilian Central Bank (BACEN) for the 2000-2012 period. Pfaff (1999) argues that credit supply is endogenous, once deforestation attracts new bank agencies and increases the demand for credit. As a solution, we use the value of credit in the previous year in estimations, which is also consistent with the dynamics of deforestation. Since BACEN only makes public annualized data, information on credit could not be transformed into the PRODES yearly period. We used the IPCA index to express values in 1999 reais.

Data on environmental fines were made available by IBAMA upon request. The database has information on all fines issued by IBAMA from 2000 to 2014, including name of the violator, type of infraction, process status and fine value. Georeferenced data containing type of conservation unit, responsible authority and year of creation are provided by MMA. Georeferenced data on indigenous land, including date of creation, area and ethnicity, are available on the National Indian Foundation (FUNAI) website. Information on Bolsa Verde recipients is available on MMA's website. Population and PIB data come from IBGE's regional account system. Cultivated area comes from IBGE's survey on municipalities's agricultural production (PAM), and include both temporary and permanent cultures.

5 Results

Results from regression model 16 are shown in table 1. They indicate that, on average, having a listed neighbour reduces the share of remaining forest cleared in 0.8 percentage points every year. Pre-treatment deforestation mean was 2.2%, so this represents a 36% reduction in deforestation. This negative effect is present in all specifications, although there's a slight reduction in the coefficient as more controls are added.

This result is consistent with literature evidence concerning spillover effects of environmental monitoring. It contradicts, however, to results found in Cisneros et al. (2015), that indicate there are no spillovers. Since non-listed municipalities are not the focus of this study, their identification suffers from some shortcomings we try to overcome. First of all, they use absolute deforestation as dependent variable, which (i) causes confusion with the criteria to enter the list and (ii) confuses identification, since this variables has different trajectories among groups. Second, their analysis include municipalities from other biomes, where measuring the area cleared is a challenge.

As previously discussed, our results may be biased because of spatial dependence. To test for spatial autocorrelation, we perform a Moran's I test on the residuals of regression 16. This test indicates if the spatial pattern expressed by residuals is clustered, dispersed or random. The results are presented in table 2, and show that residuals are positively correlated through space. The null hypothesis of no spatial clustering is rejected for all tests. These results indicate that the spatial distribution of deforestation is far from random, and that higher deforestation rates in its neighbourhood are positively correlated to a municipalities' own deforestation.

[13] Since PRODES defines year t as going from August $t-1$ to July t, we use this period to define the year for control variables whenever it is possible.

Table 1: Results from main specification

	Dependent variable:				
	Deforestation to remaining forest ratio				
	(1)	(2)	(3)	(4)	(5)
Non-listed with listed neighbours	-0.012^{***}	-0.008^{***}	-0.008^{***}	-0.008^{***}	-0.007^{***}
	(0.002)	(0.002)	(0.003)	(0.003)	(0.003)
Accumulated deforestation$_{t-1}$			0.031^{***}	0.028^{**}	0.028^{**}
			(0.011)	(0.013)	(0.014)
Squared accumulated deforestation$_{t-1}$			-0.008	-0.007	-0.006
			(0.013)	(0.014)	(0.014)
Cultivated area			0.00003	0.00003	-0.00000
			(0.0001)	(0.0001)	(0.0001)
Protected areas				-0.004	-0.004
				(0.005)	(0.005)
Indigenous land				-0.001	-0.0003
				(0.003)	(0.004)
Bolsa verde (log)				0.0005^{**}	0.001^{**}
				(0.0002)	(0.0002)
Agricultural credit$_{t-1}$ (log)					0.0004
					(0.0004)
PIB per capita$_{t-1}$ (log)					-0.001
					(0.002)
Agricultural share of PIB$_{t-1}$					0.003
					(0.004)
Population density$_{t-1}$					0.00000
					(0.00001)
State and year fixed effects	No	Yes	Yes	Yes	Yes
Observations	3,488	3,488	3,299	3,299	3,035
Adjusted R^2	0.100	0.171	0.210	0.211	0.205

Notes: All specifications include measurement error controls and group intercepts. Monetary values are in 1999 BRL. Standard errors are clustered by micro region. *p<0.1; **p<0.05; ***p<0.01.

Table 2: Moran's I test for main specification

	Observed	Expected	Std. Error	P-value
2005	0.654	-0.003	0.018	0.000
2006	0.659	-0.003	0.019	0.000
2007	0.663	-0.003	0.018	0.000
2008	0.663	-0.003	0.018	0.000
2009	0.674	-0.003	0.018	0.000
2010	0.678	-0.003	0.018	0.000
2011	0.686	-0.003	0.018	0.000
2012	0.430	-0.003	0.018	0.000

In order to correct possible biases or inefficiencies in our estimates due to spatial dependence, we use a spatial difference-in-differences estimator, described in section 4.3. LM test results for our non-spatial model, shown in table 3, indicate that a Spatial Error Model is prefered to an autoregressive one. Results for the Spatial Lag of X version of that model, shown in table 4, point in the same direction. To choose between the SEM and the SDEM Models, we estimate them both and than compare their AIC values.

To estimate the spatial regression, we need a fully balanced panel, so economic controls had to be excluded due to missing observations. Results are presented in table 5. The first column of this table shows the coefficients from our main specification, excluding listed municipalities from the sample, the same as column 4 in table 1. In column 2, we present the coefficients from a non-spatial model including listed municipalities. The SEM model

Table 3: LM test for spatial specification

Model	Statistic	DF	P-value
SEM	1706.8	1	0.0000
SAR	428.95	1	0.0000
SEM (robust)	1276.9	1	0.0000
SAR (robust)	1.0930	1	0.2958
SARAR	1707.9	2	0.0000

Table 4: LM test for spatial specification in SLX model

Model	Statistic	DF	P-value
SDEM	1616.7	1	0.0000
SDM	1000.0	1	0.0000
SDEM (robust)	644.04	1	0.0000
SDM (robust)	27.374	1	1.7e-07
SARAR	1644.1 8	2	0.0000

then includes lagged errors; the SLX, lagged values for some of the independent variables; and the SDEM, lags of errors and independents variables.

Using AIC values to chose the model that best describes spatial dependence leaves us with an SDEM specification. This means (i) that independent variables from other municipalities affect the land clearing decision and (ii) that we have spatially correlated unobserved variables affecting deforestation.[14]

We observe negative and significant coefficients for having listed neighbours in all spatial models. The inclusion of listed municipalities in the sample reduces the coefficient for having a listed neighbours, as does that of spatially dependent errors. Our benchmark model indicates a reduction of 0.32 percentage points in the share of remaining forest cleared. This represents a 15% reduction in deforestation.

Coefficient estimates for other independent variables are consistent with our non-spatial specification, except for the significance of protected areas and non-significance of bolsa verde. This is explained by the different standard errors used in the non-spatial and the spatial models. For our non-spatial specification, we clustered standard errors by micro-region, while the spatial model considers a spatially dependent error structure as described in equation 21.[15]

6 Robustness checks

To check our results, we conducted some robustness exercises. First, we test other measures of deforestation as dependent variables. Cleared to observed remaining forest area, absolute deforestation and deforestation as share of municipality area are used in non-spatial difference-in-differences estimations. For the spatial regression, we only used the latter.[16] Estimates show that the negative and significant effects are robust to changes in the definition of deforestation.

To test our proposed enforcement mechanism as opposed to possible economic threats, we use matching techniques to create control and treatment groups with equal chances of being listed. Once we match municipalities on the probability of being listed, the policy's observed effect should be attributed to other causes, more

[14] Similarly, to determine the most appropriate neighbourhood matrix, we estimate the SDEM model with different neighbourhood criteria and choose the one with the highest AIC value (Stakhovych and Bijmolt, 2009). We tested a queen contiguity matrix as well as inverse distance matrices with cutoffs ranging from 2 to 5. Results indicate the best choice as the inverse distance matrix with a 2.6 cutoff. This was the matrix used for estimations presented in both tables 1 and 5.

[15] The spatial modelling of the error should be preferred to cluster-robust standard errors, since the latter is only efficiency for block-diagonal spatial processes (Anselin and Arribas-Bel, 2013).

[16] INPE's observed forest has missing observations in some years, so it could not be used, and maximum likelihood processes did not converge when using absolute deforestation.

Table 5: Spatial model specification test

	Dependent variable:				
	Deforestation to remaining forest ratio				
	(Main)	(Non-spatial)	(SEM)	(SLX)	(SDEM)
Non-listed with listed neighbours	-0.008^{***}	-0.007^{***}	-0.003^{*}	-0.007^{***}	-0.003^{*}
	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)
Listed		-0.005^{*}	-0.002	-0.005^{*}	-0.002
		(0.003)	(0.003)	(0.003)	(0.003)
Accumulated deforestation$_{t-1}$	0.028^{***}	0.034^{***}	0.030^{***}	0.033^{***}	0.026^{***}
	(0.013)	(0.013)	(0.007)	(0.007)	(0.007)
Squared accumulated deforestation$_{t-1}$	-0.007	-0.011	-0.008	-0.011	-0.006
	(0.014)	(0.014)	(0.006)	(0.007)	(0.006)
Cultivated area	0.000	-0.000	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Protected area	-0.004	-0.004	-0.003^{*}	-0.003^{*}	-0.004^{**}
	(0.005)	(0.006)	(0.002)	(0.002)	(0.002)
Indigenous reserves	-0.001	-0.002	-0.004	-0.003	-0.004
	(0.003)	(0.003)	(0.004)	(0.004)	(0.004)
Bolsa verde (log)	0.0005^{**}	0.0004^{**}	0.000	0.000	0.000
	(0.0002)	(0.0002)	(0.000)	(0.000)	(0.000)
Neighbours' accumulated deforestation$_{t-1}$				0.0002^{***}	0.0003^{***}
				(0.0001)	(0.0001)
Neighbours' cultivated area				-0.00003^{***}	-0.00004^{***}
				(0.00001)	(0.00001)
Neighbours' protected area				-0.000	0.000
				(0.000)	(0.000)
Neighbours' indigenous reserves				-0.000	-0.001
				(0.001)	(0.001)
R^2	0.217	0.223		0.228	
Adj. R^2	0.211	0.218		0.221	
Num. obs.	3299	3592	3592	3592	3592
AIC			-16291.331		-16305.452
Log Likelihood			8338.570		8347.145
Lambda: statistic			0.011		0.011
Lambda: p-value			0.000		0.000

Notes: All specifications include measurement error controls, group intercepts and state and year fixed effects. Monetary values are in 1999 BRL. Standard errors in non-spatial specifications are clustered by micro region. *p<0.1; **p<0.05; ***p<0.01.

specifically, following our model, to increased monitoring. In order to guarantee common support in the probability of being treated between non-listed municipalities with listed neighbours and non-listed municipalities with no listed neighbours, we had to trim observations in the first group with a propensity score equal to or bigger than 43%. This trimming has the additional advantage of removing from the sample municipalities that might have been intentionally kept out of the List, despite presenting high values for the criteria, which further reduces the possibility of analysing endogenously created groups.

We matched groups on propensity score by year. Results with matched sample are consistent with those from our main specification. Their estimates present no change in the sign or significance of control variables' coefficient. Estimates for the effect of the blacklist in non-listed neighbours of listed municipalities have a higher absolute value in this specification, indicating an average reduction of 35% in yearly deforestation. Therefore, even if we do not take into account the possible economic threat represented by the List, we still have a negative and significant spillover on non-listed neighbours.

Table 6: Robustness checks

	Listed	Non-listed with listed neighbours
Share of INPE's remaining forest cleared		-0.011^{**}
		(0.004)
Absolute deforestation (km^2)		-7.845^{***}
		(2.058)
Share of municipality area cleared		-0.002^{***}
		(0.0005)
Share of municipality area cleared (spatial regression)	-0.005^{***}	-0.001^{***}
	(0.001)	(0.000)
Share of remaining forest cleared (matched sample)		-0.009^{***}
		(0.003)

Notes: All specifications include measurement error controls, group intercepts and state and year fixed effects. Monetary values are in 1999 BRL. Standard errors are clustered by micro region. Other control variables are: share of cultivated area, share of protected areas, share of indigenous lands, bolsa verde, agricultural credit in the previous year (log), GDP per capita in the previous year (log), share of agricultural GDP in the previous year, population density in the previous year. *p<0.1; **p<0.05; ***p<0.01.

In table 7, we change the definition of treatment. In model 16, any non-listed municipality with at least one listed neighbour is considered treated. For our specification in table 7, on the other hand, we take into account the number of listed neighbours. These results cast more light over possible mechanisms. Municipalities with only one listed neighbour show no significant response to the listing of their neighbours. As the number of neighbours grows, however, they seem to show a stronger response: coefficients become significant for municipalities with two listed neighbours and then monotonically increase in absolute value as the number of neighbours rises. The exceptions are municipalities with four and seven neighbours, that show non-significant coefficients.

This is consistent with the model presented in section 3: for municipalities with only one listed neighbours, the increase in the expected probability of being punished is not strong enough to offset the potential gains from agricultural activity. However, as the number of listed neighbours grows, so does the police presence in the region, decreasing the expected value of land clearing as the probability of being caught grows larger.

7 Final remarks

We investigated the spillover effects of a blacklisting policy, the Priority Municipalities List. Unlike other evaluations of this policy, we are mainly interested in the effects it had on non-listed neighbours of listed municipalities. We also contribute to the literature by considering a spatial estimator, which to our knowledge had never been adopted to evaluate deforestation policy impacts. Our results suggest that the Priority Municipalities List does affect municipalities that are listed, reducing the share of remaining forest cleared yearly by 15% to 36%, on average.

As modeled in section 3, the List's effect on non-listed municipalities could, *a priori*, be either positive or negative. The negative coefficients observed in our estimates indicate that the incentive to reduce deforestation caused by increased probability of being punished is greater than the incentives to expand agricultural activities into neighbouring municipalities. Our initial results indicate that the list caused a 33% reduction in deforestation on non-listed neighbours of listed municipalities. Once we correct for spatial dependence, however, the estimated effect falls to a 15% reduction. The negative and significant coefficient is robust to changes in the definition of

Table 7: Results with number of neighbours as treatment variable

	Dependent variable:				
	Deforestation to remaining forest ratio				
	(1)	(2)	(3)	(4)	(5)
1 neighbour	-0.015^{***}	-0.001	0.00004	0.0003	0.00000
	(0.002)	(0.003)	(0.002)	(0.002)	(0.002)
2 neighbours	-0.018^{***}	-0.005^{*}	-0.004^{*}	-0.005^{**}	-0.006^{***}
	(0.002)	(0.003)	(0.002)	(0.002)	(0.002)
3 neighbours	-0.016^{***}	-0.005^{**}	-0.006^{**}	-0.007^{***}	-0.008^{***}
	(0.003)	(0.002)	(0.003)	(0.003)	(0.003)
4 neighbours	-0.001	-0.002	-0.003	-0.002	-0.002
	(0.007)	(0.005)	(0.004)	(0.004)	(0.003)
5 neighbours	0.001	-0.005	-0.008^{*}	-0.008^{*}	-0.008^{**}
	(0.005)	(0.005)	(0.005)	(0.004)	(0.004)
6 neighbours	-0.008^{***}	-0.012^{***}	-0.011^{***}	-0.010^{***}	-0.010^{***}
	(0.002)	(0.003)	(0.002)	(0.003)	(0.002)
7 neighbours	-0.008	-0.005	-0.008	-0.009	-0.009
	(0.005)	(0.005)	(0.006)	(0.006)	(0.006)
8 neighbours	-0.010^{***}	-0.007^{***}	-0.010^{***}	-0.010^{***}	-0.010^{***}
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
9 neighbours	-0.008^{***}	-0.008^{***}	-0.012^{***}	-0.013^{***}	-0.013^{***}
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
10 or more neighbours	-0.010^{***}	-0.009^{***}	-0.013^{***}	-0.014^{***}	-0.014^{***}
	(0.002)	(0.003)	(0.002)	(0.002)	(0.002)
State and year fixed effects	No	Yes	Yes	Yes	Yes
Agricultural activities controls	No	No	Yes	Yes	Yes
Concurring policies controls	No	No	No	Yes	Yes
Economic controls	No	No	No	No	Yes
Observations	3,488	3,488	3,299	3,299	3,035
Adjusted R^2	0.101	0.171	0.210	0.211	0.204

Notes: All specifications include measurement error controls and group intercepts. Monetary values are in 1999 BRL. Standard errors are clustered by micro region. Agricultural activities control: share of cultivated area. Concurring policies controls: share of protected areas, share of indigenous lands, bolsa verde. Economic controls: agricultural credit in the previous year (log), GDP per capita in the previous year (log), share of agricultural GDP in the previous year, population density in the previous year. *p<0.1; **p<0.05; ***p<0.01.

both deforestation and neighbourhood. Since the effect on non-listed municipalities is a reduction deforestation, not taking such an effect into account when evaluating the impact of the blacklist means to underestimate the policy's ability to reduce deforestation in the Amazon biome.

Additional tests suggest that the effect decreases the further a municipality is from the one that is blacklisted and that the higher the number of listed the neighbours, the higher the reduction in deforestation. Though we do not perform a definitive causal test indicating that the observed impact is due to stricter police presence, additional analysis allow us to dismiss the possibility of economic threats as an alternative mechanism. Since unlike blacklisted municipalities, non-listed neighbours are not subject to further restrictions, the stricter enforcement remains the most plausible mechanism at work.

These results are in line with those presented by Assunção et al. (2012), Assunção et al. (2013) and Assunção and Rocha (2014), highlighting the importance of command and control instruments to reduce deforestation. It also indicates that these works probably underestimated the policy's effect in the Amazon biome as whole, and suggests that the spatial distribution of environmental law enforcement can be used to improve efficiency of deforestation policies.

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