Public Policy Under Limited State Capacity: Evidence from Deforestation Control in the Brazilian Amazon*

Tara Slough
Columbia University

Johannes Urpelainen†
Johns Hopkins SAIS

January 24, 2019

Abstract

Under limited state capacity, governments are often forced to target public policy to priority areas. We argue that such targeting offers an opportunity to learn about the spatial dimension of state capacity. We conduct an analysis of spatial spillovers from Brazil's program to control illegal deforestation in designated priority municipalities. Drawing on insights from a model of illegal deforestation under imperfect monitoring, we demonstrate that when a municipality is targeted for more enforcement, deforestation rates within about 50 kilometers of its boundaries decrease (positive spillovers from improved monitoring) but rates farther away increase (negative spillovers from displacement of illicit activity). Our theory and empirics not only illuminate the logic of public policy under limited state capacity, but also develops novel evidence about the spatial and temporal dimensions of state capacity.

Keywords: state capacity; natural resources; policy enforcement; spatial models; Latin America; Brazil

Word count: 9,926

*Slough's work on this project is supported in part by an NSF Graduate Research Fellowship, DGE-11-44155. We thank Alice Xu for her contribution to the project and Hillel Soifer for excellent comments. We also benefited from comments by seminar audiences at the National University of Singapore and the College of William & Mary.

†**Corresponding author**. Email: JohannesU@jhu.edu.

1 Introduction

Limited capacity of the state to implement and enforce policies represents a central obstacle to development (e.g., Besley and Persson, 2009; Herbst, 2014; Acemoglu, García-Jimeno, and Robinson, 2015). If a government cannot raise tax revenue, enforce the rule of law, or effectively implement public policy, then long-term economic development is difficult. Recent literature emphasizes sub-national unevenness in state capacity (Enriquez and Centeno, 2012; Soifer, 2015; Foa and Nemirovskaya, 2016) that has long been theorized to be spatially clustered (Boone, 2003; O'Donnell, 1993; Mann, 1984). In particular, descriptions of core and periphery (frontier) regions suggest that this unevenness is spatial.

Extant explanations for higher capacity in core regions include population density and related costs of administration (Herbst, 2014), colonial legacies (Acemoglu, García-Jimeno, and Robinson, 2015), and institutional choice (Boone, 2003). These explanations, however, provide limited predictions for the success of efforts to expand capacity or enforcement into the periphery. Classic accounts of the periphery provide clearer predictions about responses to state encroachment (Scott, 1998). However, we lack broader evidence about the spatial "projection dynamics" of state expansion into the periphery. We seek to better understand short-term change in the spatial dimension of state capability.

We argue that some spatially targeted policies provide fertile grounds for generating empirical evidence about the spatial dynamics of state capacity. In the context of uneven capacity for policy implementation or limited 'infrastructural power" (e.g., Mann, 1984; Soifer, 2008), governments are often forced to target their efforts to a limited number of spatially-defined priority areas. For example, governments often target industrial policies in special export zones; focus policing efforts on neighborhoods with high crime rates; and designate hot spots to conserve biodiversity.

Our empirical application is a major anti-deforestation policy in Brazil, where the most cited outcome of state weakness is environmental degradation (Soifer, 2012; Nepstad et al., 2002). The scale and visibility of deforestation in the Brazilian Amazon represents a direct consequence of the state's inability or unwillingness to enforce laws against deforestation. Although Brazil is not a weak or fragile state, the massive size of the Amazon rainforest presents a major challenge to policy implementation and law enforcement. We study the Priority Municipalities (PM) program widely credited with curbing deforestation rates by more than 80%, one of the most critical environmental measures taken in the history of Brazil. Introduced in 2007, the PM initiative is a federal program that publicly blacklists Brazilian municipalities in the Amazon with substantial recent deforestation. Listed municipalities became the targets for more rigorous monitoring and enforcement on the part of federal law enforcement agencies for environment management (Assunção, Gandour, and Rocha, 2015).

We emphasize the need to study both temporal and spatial variation in order to understand the aforementioned projection dynamics. We model the spatial *spillovers* of the spatially-targeted PM program to enforce deforestation laws in the Brazilian Amazon. We study the program's effects *outside* the directly affected area – beyond the targeted municipalities themselves. In so doing, we build upon a literature in which spatial spillovers have been used to study governance issues (e.g., Lipscomb and Mobarak, 2017; Kahn, Li, and Zhao, 2015; Cai, Chen, and Gong, 2016).

Our research design contains three elements generally absent from extant empirical work on capacity. First, we exploit temporal variation in Brazil's efforts to reduce deforestation. Our temporal measurements include 107 monthly observations, including two years of pre-policy data. Second, we observe outcomes at a highly disaggregate spatial unit, with over 30,000 grid cells. These grid cells cover a large, ecologically significant territory in the Amazon that is approximately the area of the country of Mexico. This approach allows us to model spatial projection dynamics. Finally, we study these dynamics exclusively in the periphery, so that we can model the consequences of state projection of enforcement capability in an ideal environment where the state is thought to be quite weak.

In assessing spatial spillovers, we restrict our attention to a 200-kilometer bandwidth *outside* the PMs. We first demonstrate that before the first round of PM designations, grid cells proximate to "treated" (designated) PMs exhibit parallel trends in deforestation to those proximate to "control" (non-designated) PMs. Further, we show that these trends do not vary by distance from the PM. In other words, before designation of PM status, pre-treatment trends in deforestation by distance do not differ in areas proximate to PM and non-PM municipalities.

We examine whether these trends diverge after PM designation, enabling estimation of the spillover effects of the PM program. The results are striking: deforestation rates decrease close to the municipalities designated as PMs, as the extra monitoring generates positive spillovers and deters illegal deforestation. Further from the PM, however, deforestation increases as illegal deforestation activities shift away from the monitored areas. Specifically, we find that the PM program generates positive spillovers – less deforestation – at distances less than about fifty kilometers, and negative spillovers – more deforestation – beyond that distance. We show that the magnitudes of positive and negative spillovers are substantively sizable relative to pre-treatment deforestation rates. The result is robust to variation in sample and model specification.

A simple theory of forest clearing with and without monitoring clarifies the core logic of enforcement capacity spillovers in the periphery. PM designation increases monitoring within the designated municipality and close to it, increasing the cost of illegal forest clearing. This increased cost, however, raises the price of forest products in the market, and thus extractors are willing to incur the cost of moving deeper and deeper into the jungle, displacing deforestation. On balance, monitoring deters deforestation by increasing the production cost of illegal forest products, while the price effect partially cancels out the benefit of monitoring, as a function of distance from the PM. This implies that the spatial dynamics of expanding state capacity depend on the responses of local actors.

Our results posit implications for both the study of state capacity and the consequences of targeted policy implementation. We document that the spillover effects of a targeted enforcement program are consistent with positive spillovers radiating out from the locus of implementation. However, the negative spillovers to distant regions suggest a response by local actors – here loggers or encroaching settlers – consistent with accounts of Scott (1998) and others.

Our theoretical and empirical approach offers a simple, principled, and robust approach to estimating spillovers. Given the prevalence of geographic or spatial descriptions of state capacity, a research design that recovers credible estimates of spatial spillovers removes obstacles to better test theories of state infrastructural power.

2 Projection Dynamics of State Capacity

2.1 Spatial Patterns

Existing analyses of unevenness in state capacity within countries frequently distinguish between core and periphery regions. One frequent operationalization or correlate of capacity, population density, summarizes the observed patterns (Herbst, 2014; Soifer, 2015; Foa and Nemirovskaya, 2016). In this telling, capacity is concentrated in cities and punctuated by large peripheral areas of lower capacity in between.

The temporal dimension of capacity is less explored. The core theoretical logics underpinning the these spatial patterns invoke long time horizons (Acemoglu, García-Jimeno, and Robinson, 2015). For example, population density increases incrementally and associated efficiency gains in administration occur over a long time horizon. While accretion of state presence is a long term process, public policies aim to augment state presence in much shorter periods, often in targeted areas. Hot-spot policing (i.e. Braga, 2005; Weisburd and Eck, 2004; Magaloni, Franco, and Melo, 2015) and targeted environmental protection policies (Oestreicher et al., 2009; Assunção, Gandour, and Rocha, 2015) implicitly assume that incremental increases in state presence are possible.

We argue that evidence of how increased state presence disperses spatially informs our understanding of state capacity. We term these patterns the "projection dynamics" of capacity. In order to isolate such patterns, we focus exclusively on the spatial patterns of the diffusion of state presence in the periphery. In so doing, we examine how infusions of enforcement and monitoring diffuse beyond the confines of the targeted area. This emphasis distinguishes our work from existing work on the outcomes of state capability or presence.

2.2 Spatial Spillovers: Manifestations of Projection Dynamics

When states invests in expanding enforcemtn capacity in a targeted area, these shocks to capacity may trigger changes in other areas in a general equilibrium framework of policies and outcomes. Spillovers typically stem from adaptive behavior by the subjects of the policy. For example, in a policing context, Short et al. (2010: 3961) notes, "[r]eaction-diffusion models, in which activators and inhibitors move, mix, and interact, provide a useful framework in which to investigate the formation of crime patterns and the impact of alternative policing strategies on crime hotspot stability."

If the policy is intended to discourage some behavior in a geographic area, for example, the successful implementation of such a policy may cause some individuals to shift this behavior into a neighboring area. Thus, even if such a policy is successful in one area, it may have negative effects in surrounding areas. Existing literature invokes a set of theoretical logics for negative spillovers including displacement (e.g. of crime) (Getmansky, Grossman, and Wright, 2017) and diversion of government effort away from enforcement in non-treated areas (Cai, Chen, and Gong, 2016; Kahn, Li, and Zhao, 2015; Lipscomb and Mobarak, 2017).

On the other hand, policies may also generate positive spillovers. In this case, the successful discouragement of a behavior in some area may deter such behavior in neighboring areas (for example, see Guiteras, Levinsohn, and Mobarak, 2015). The presence of positive spillovers of state presence are particularly important to our argument that state presence projects over space, even within the periphery. A simple observation of displacement (negative spillovers) outside of targeted areas provides little evidence that state presence/enforcement has changed within the peripheral area of interest. Importantly, positive spillovers provide evidence of some diffusion of capacity beyond targeted areas. We detail these observations within a general equilibrium framework with application to a deforestation program.

2.3 General Equilibrium Framework

To derive empirical implications of projection dynamics of capacity, we draw on a general equilibrium framework. We do so in the context of our empirical application, anti-deforestation policy in the Brazilian Amazon. We focus on the spatial implications of a geographically targeted policy.

In the case of anti-deforestation enforcement, we identify two countervailing forces. On one hand, enhanced monitoring in one area may have positive spillovers in neighboring areas, as the risk of being caught increases. For example, if federal inspectors in the targeted municipality improve their ability to identify and punish violators, their presence may also increase the likelihood of being caught in neighboring areas within a certain radius. On the other hand, any efforts to reduce deforestation may result in displacement. As long as the demand for products such as timber, beef, and soybeans remains high, reducing deforestation in one area creates profitable opportunities for land clearing in other areas. As ranchers and cultivators exploit these opportunities, some of the gains from targeted enforcement may be lost.

To understand the conditions under which positive and negative spillovers are likely, Appendix A1 (page APP-3) presents a formal model of forest clearing with and without hot spot monitoring. In this model, the demand curve for (illegal) forest products is exogenous and the focus of policy is on supply. Forest clearing yields profits from sales of forest products and use of the land, but the cost of forest clearing increases as the individuals engaged in forest clearing move deeper into the jungle (i.e., distance from markets or transportation networks increases).

The anti-deforestation policy imposes a fine on illegal deforestation activity, and individuals engaged in forest clearing are caught with some non-zero probability. Reflecting limited state capacity and spatial projection dynamics, the probability of being caught, and thus fined, *decreases* as individuals clear forests deeper in the jungle. Thus, in our model the anti-deforestation policy creates cross-pressure: on the one hand, moving deeper into the jungle increases the cost of forest clearing; on the other hand, doing so reduces the probability of being caught. This cross-pressure is key to understanding the spatial dimension of anti-deforestation policy under limited state capacity.

In a world without targeted monitoring, deforestation expands away from the town until the transportation costs become so high that the activity is no longer profitable given the market price. This expansion thus continues until supply and demand meet, so that the market clears. When targeted monitoring is introduced, positive spillovers result from the fact that forest clearing close to the central location now carries a higher risk of of being caught for illegal deforestation. This risk is the highest close to the central location, i.e., the PM.

At the same time, *negative spillovers* are also possible: if demand for forest clearing in the market is relatively inelastic, then the outer boundary of the deforested area moves farther away from the central location, as the price of the products sold – such as timber, soy, and beef – expands. Although monitoring suppresses supply because illegal deforestation is now punished at higher rates, the market price increases to cancel out some of the benefits. This increase in the market price results in a supply response, so that some of the benefits from expanded monitoring are lost.

On balance, then, spatial spillovers may be positive or negative. Monitoring reduces supply, and these positive spillovers are particularly strong close to the central location under formal monitoring. However, the market-clearing price increases when supply is curtailed, and thus deforestation away from the central location becomes more profitable than without monitoring. The total effect of an anti-deforestation policy consists of the direct effect in the monitored are (PM), the positive spillovers, and the negative spillovers. Our empirical estimation below is intended to capture the spillovers. We expect a neighboring municipality's PM designation to (i) reduce deforestation nearby and (ii) increase deforestation farther away.

3 Priority Municipalities and Deforestation in the Brazilian Amazon

The administration of President Lula da Silva (2003-2011) initiated a number of policies toward the management Brazil's natural resources. Two such programs are relevant to the present study. First, investment in satellite monitoring provides for detection and measurement of deforestation. In particular, the creation of the DETER satellite system in 2004 enhanced the monitoring capabilities of the Brazilian government (Assunção, Gandour, and Rocha, 2015). The data from this monitoring system serves as our main outcome variable.

Second, a presidential decree issued 2007 enabled the Ministry of the Environment (MMA) to publicize a list of municipalities (PMs) that would receive targeted monitoring and enforcement against deforestation by the Brazilian Institute of Environment and Renewable Natural Resources (IBAMA) and other law enforcement agencies. These municipalities were designated based on the following criteria:

1. Total area of forest cleared;

2. Total area of forest cleared in the last three years;

3. Increase in rate of deforestation in at least three of the last five years.

The list is updated over time with the entry and exit of new municipalities contingent on their performance in deforestation. Between 2008 and 2015, 52 municipalities were designated as PMs, starting with the first list of 36 in January 2008.

Although the criteria for inclusion on the PM list are clear and publicized, calculations of changes in deforestation trends at municipal level are not easily obtainable. As Table A1 (page APP-6) shows, the month and year of release of updates to PM lists occur at arbitrary intervals. Determination of Priority Municipalities was based on Brazilian National Institute for Space Research (INPE) time series data which has varying release dates every year.[1] In this regard, it is highly unlikely that local industries or political leaders are in a position to predict the exact month of release for new information on entry and exit from PM lists.

The designation of PMs consisted of three related changes in enforcement practices. First, PMs

[1]Researcher communication with the former Secretary of Policy Coordination for the Amazon at MMA and Director of the National Environmental Council (CONAMA).

saw an increase in the share of IBAMA resources in terms of budget and staffing.[2] Second, the activities of IBAMA agents emphasized enforcement and deforestation prevention in several ways. The program increased property registration requirements and and instituted licensing for rural properties within targeted municipalities, with more screening for fraudulent titles. Third, with additional monitoring of deforestation including the use of satellite detection, individual properties were sanctioned (embargoed) with higher frequency (Assunção and Rocha, 2014; Souza Costa Neves and Whately, 2016; Cardoso de Andrade, 2016). Collectively, the increase in enforcement resources and higher scrutiny of titles and licenses increased the likelihood of being detected and punished for illegal deforestation with costly fines.[3]

For our purposes, the PM program's federal status is important for two reasons. The program provided similar incentives to bureaucrats tasked with implementation across all targeted PMs. Further, while individual municipalities or actors therein face the temptation to clear forest for economic gain, programs implemented at lower levels may face additional pressures to shirk on enforcement. To this extent, a federal program allows for more confidence that we are leveraging a common (uniform) shock to capacity across the municipalities we study.

Estimates of the effects of the PM program suggest a substantial shock to enforcement capacity in PMs, as measured by rates of deforestation. Assunção and Rocha (2014) find that during the initial years of the program, 2008-2011, PMs avoided clearing a total of 11,369 square kilometers of the Amazon. Total deforestation observed during this period was 35% less than the projected counterfactual scenario in the absence of the initiative. These impacts were highlighted by Marina Silva, then Minister of the Environment who stated "we succeeded in perhaps one of the most important environmental measures taken in the history of Brazil: a plan to combat deforestation. It brought deforestation down by more than 80% since then, saving more than 4 billion tonnes of CO_2 emitted" (H.J., 2013).

In contrast, we focus on territories *outside* the PMs, examining whether enforcement capacity induced by the PM program transcended municipal borders. In addition to the direct effects in

[2] Note that if resources were withdrawn from non-PMs in favor of PMs and deforestation decreases in enforcement expenditures, it would be harder to detect any positive spillovers. All of our analysis corresponds to areas *outside* blacklisted municipalities.

[3] Under Brazilian law, from the year 2008 the penalty for illegal deforestation was R$5,000 per hectare, or about 2,455 USD in 2018 prices. Timber and other commodity prices vary, but in October 2017 a hectare of suitable rainforest would yield about 0.4 cubic meters of ipê hardwood and be worth 1,000 USD in export markets (Gonzales, 2017). Thus, the fine would be 2.5 times the value of a key illegal timber product.

targeted municipalities, we posit that this municipal black-listing may alter deforestation patterns in nearby areas. When enforcement activity increases within a PM, inspectors are also likely to detect and punish violations outside the municipal boundaries for two reasons. First, in the Amazon, municipal boundaries are long and not clearly demarcated. Enforcement agents may incidentally detect violations outside PM boundaries when they operate close to the boundary, even if their energies focus only the PM itself. Second, as federal officials, these agents have a clear mandate to enforce policies against violators regardless of whether they are inside or outside the PM boundaries. In response, individuals engaged in forest clearing have an incentive to avoid operating too close to PM boundaries to limit the risk of penalty.

There is no sharp discontinuity at the formal boundary; rather, the risk of being penalized decreases continuously as logging and forest clearing activities move away from the boundary. When enforcement capacity is low or non-uniform, selective punishment of deforesters in a way that generates spillover effects on behavior in neighboring jurisdictions could serve as an efficient means for the federal state to manage the local governance of natural resources.

4 Research Design

To explore spatial spillovers from Brazil's PMs program, we examine deforestation *outside* the PMs. Because we seek to measure how capacity projects as opposed to whether targeted enforcement works within targeted areas, we do not focus on the PMs themselves.[4] We focus on a 200-km buffer area surrounding the municipalities designated a PM at any point in time.

4.1 Dataset Construction

Our data comes from a variety of sources. First, we identified the municipalities designated PMs by the Brazilian federal government. In total, there are 52 municipalities that were PMs at some time between the program's inauguration in January 2008 and the end of our panel dataset in July 2015. Importantly, no PM is completely surrounded by other PMs. These municipalities – the level at which the PMs "treatment" was assigned – serve as clusters in our analyses. We create a binary treatment vector that takes the value "1" when a municipality is designated a PM and a "0" otherwise. Note that the sample is conditioned on municipalities that, at some time, become PMs.

[4] As discussed above, existing work argues that this targeting dramatically reduced deforestation within PMs (Assunção, Gandour, and Rocha, 2015).

Our counterfactual comes from the variation in the timing of PM implementation.

Surrounding these PMs, we identify the 200-kilometer buffer region, omitting any area from the buffer that crosses national boundaries. Given the vast geographic range of the PMs, this implies that we exclude portions of this buffer region located in Bolivia, Venezuela, and Guyana. Figure 1 provides a visualization of the buffer region that we study in our empirical analysis. We identify 30,162 hexagonal grid cells of edge length 5 km, the cross-sectional unit in our analysis.[5] We build a panel dataset with monthly coverage from March 2006 (nearly two years prior to the announcement of the first PMs) until July 2015, including a total of 107 months. As such, our dataset includes 3,227,334 hexagon-month observations. We measure the distance from the centroid of each hexagon to the nearest PMs (at any point in time).[6] This provides us with two pieces of information. First, a hexagon is "treated" during the months when its nearest PM is designated a PM. Second, we condition on the distance from a hexagon centroid to the nearest PM.

Our dependent variable – deforestation – comes from Brazil's DETER satellite monitoring dataset, which is sensed monthly by the MODIS satellite at a resolution of 250 × 250 meters (Wheeler et al., 2014: 7). DETER represents one of three main satellite monitoring systems: FORMA, PRODES, and DETER. While PRODES is arguably the most accurate in identifying forest clearing, it is only aggregated annually, a much coarser temporal unit. Fortunately, PRODES and DETER data are highly correlated for forest clearing events at the annual level (Wheeler et al., 2014). The DETER data identifies a monthly list "events," or instances of forest cover loss. The raw data for each event consists of polygons identifying the area that was deforested. Figure 1 provides a visualization of the events recorded by DETER that occurred *outside* the PMs between 2006 and 2015. Note that the majority of these events occur within the buffer region that is the focus of our study.

To generate a hexagon-month dataset, we utilized GIS to ascertain which events occurred in hexagon i during month t. If an event straddled multiple hexagons, it is recorded in each hexagon where some deforestation was observed at that time. From this mapping, we record whether an event occurred in hexagon i in month t and the deforested area in each hexagon.

[5] These cells are larger than the largest deforestation event we observe; most events are much smaller than 65 km^2.

[6] Contemporaneous work by Cardoso de Andrade (2016) studies spillovers to a subset of municipalities bordering the PMs, presenting evidence of only positive spillovers of the PM program proximate to municipal boundaries. However, their focus is not on state capacity and they analyze deforestation at a higher level of geographic and temporal aggregation over a shorter panel with a different research design.

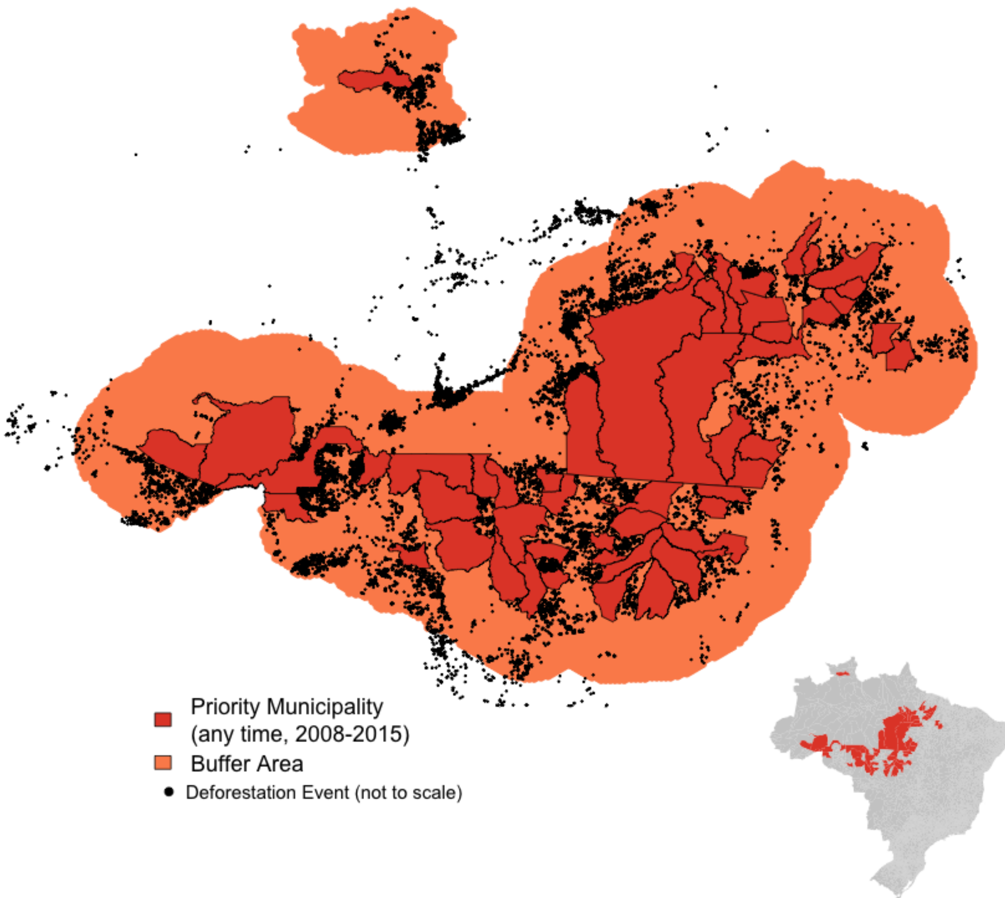

Figure 1: Deforestation events from DETER satellite data overlaid on the 200-kilometer buffer around PMs in the Brazilian Amazon, 2006-2015. Note that the deforestation events are not drawn to scale.

Our sample is described in Table A1, where we aggregate by PM. Note that this is the level at which assignment occurs as well as the level of clustering of the standard errors. The cluster size varies substantially, a point to which we return in robustness tests. We account for potential concerns induced by this variation in our robustness checks. Our panel is balanced: we have 107 observations per hexagon.[7] By construction, all clusters eventually receive treatment, though there is substantial variation in the entrance and, in some cases, exit dates from PM status.

4.2 Estimation Equation

Our main specifications take the form of a generalized difference-in-difference estimator. We focus on the conditional impact of having a neighboring municipality designated as a PM, per our theoretical propositions. Consider a hexagon i located in municipality k (not a PM) for which the closest PM is j in time t. Time t denotes calendar month (i.e. January) m. As such, our main estimation equation is as follows:

$$Y_{ijktm} = \beta_1 Z_{jt} + \beta_2 Z_{jt}\text{Distance}_{ij} + \boldsymbol{\phi}_t + \boldsymbol{\gamma}_i + \epsilon_{ijktm}, \tag{1}$$

where Z_{jt} is a time-varying binary indicator for whether the neighboring PM is designated as such in time t and Distance_{ij} is the hexagon's distance from that municipality. The estimator of the conditional ATT of a neighbor's PM status, by distance, is given by $\beta_1 + \beta_2 \times \text{Distance}_{ij}$.

Deforestation exhibits clear seasonality, peaking annually between June and October (see Appendix A4, page APP-9). Our ultimate specification proceeds beyond the traditional difference-in-difference estimator to include a hexagon-month vector of fixed effects, where month, m, corresponds to a calendar month (i.e. January). This specification absorbs seasonal variation in deforestation at the hexagon grid-cell level:

$$Y_{ijtm} = \beta_1 Z_{jt} + \beta_2 Z_{jt}\text{Distance}_{ij} + \boldsymbol{\phi}_t + \boldsymbol{\gamma}_i + \boldsymbol{\nu}_{im} + \epsilon_{ijktm} \tag{2}$$

Because Distance_{ij} is time invariant, the base distance term is subsumed by the hexagon-level fixed effects (γ_i). While Equation 2 serves our main estimator given clear patterns of seasonality, we also report a set of simpler models without fixed effects and with coarser cross-sectional fixed

[7] DETER does not publish data from six months during our panel: November and December 2006; January, February, and March 2007; and December 2009. As such, we have no observations from these months in our dataset.

effects that demonstrate that the patterns we document are also immediately evident in simpler (if unidentified) models.

We utilize estimate linear probability models with OLS for all specifications. In order to appropriately quantify uncertainty, all standard errors are clustered at the level of the neighboring (closest) PM, indexed by j above, as this is the unit of treatment assignment (Bertrand, Duflo, and Mullainathan, 2004).

4.3 Dependent Variable

The DETER data provides fine-grained data on monthly deforestation. In the original shape files, the unit of observation is the deforestation event, for which we observe deforested area as a spatial polygon. This leaves two evident operationalizations of our dependent variable. First we construct a binary indicator for whether or not an event occurs in hexagon i in month t. Second we calculate the total deforested area in hexagon i in month t. Because this variable is highly skewed, we use the natural log of area deforested + 1 as our outcome variable.

Although the DETER data, like any other satellite source, suffers from some measurement error related to cloud cover and other environmental conditions, this error is unlikely to produce systematic bias. For there to be systematic bias, the measurement error from the satellite and environmental conditions would have to be correlated with distance from PM in a way that differs before and after the PM designation enters into force. We believe this is unlikely. The unsystematic measurement error related to environmental conditions may reduce efficiency.

4.4 Explanatory Variables

Our treatment variable is a binary indicator of the PM status of the closest PM to hexagon i in time t. As shown in Table A1, all hexagons have some months in both "treatment" (designated a PM) and "control" (not designated a PM) during the duration of our panel.

Our theory asserts that targeted investments in state capacity like the PMs program should diffuse on a geographic basis. We estimate this diffusion on the basis of the interaction of a distance covariate and PM treatment status. None of the municipal boundaries of the PMs changed during the period of our study. We estimate the geographic diffusion in two ways. First, we use a continuous measure of the shortest distance from the centroid of each hexagon, i, to a PM, j, boundary. This ranges from 0 to 200 kilometers, given the buffer region that we identify. Second, to assess the possibility for non-linearity in the conditional (spillover) effect of the PM designation, we bin the distance measure into four categories: 0-50 kilometers, 50-100 kilometers, 100-150 kilometers, and 150-200 kilometers. Additional robustness checks further assess the functional form of the conditional ATT as a function of distance.

4.5 Identifying Assumptions

The key identifying assumption upon which our difference-in-difference based empirical analysis rests is that of parallel or common trends. iven the nature of our time series data in which municipalities may be assigned as a PM or subsequently removed from the list, it is clearest to examine the plausibility of this assumption by comparing the pre-treatment trends of grid cells linked to the municipalities assigned to treatment in January 2008 ($n = 36$) versus those that were not assigned to treatment until 2009 or later ($n = 18$). In sum, we have 17 months of pretreatment DETER data.[8] In Figure 2, we present the mean deforestation rates within the two groups. The raw data as well as the superimposed local polynomial regression lines suggest few grounds for questioning the plausibility of the parallel trends assumption at least within this subset of time periods. Figure A6 computes the *difference* in the slopes of the local polynomial regression lines for the treatment and control groups. A non-zero difference indicates a violation of the parallel trends assumption. In this case, zero is consistently within the bootstrapped (non-parametric) 95% confidence intervals, providing no evidence that the assumption has been violated.

We further conduct a parametric test of the parallel trends assumption, presented in Table 1. In this test, we estimate an OLS model in which we regress the pretreatment hexagon-level deforestation events (top panel) and logged area deforested (bottom panel) on polynomial time trends (up to a quartic term). In this model, we also interact the time trends with treatment assignment indicator that captures treatment assignment in January 2008. We cluster standard errors at the level of treatment assignment, the PM. The finding of a statistically significant interaction between treatment and the time trend (any specification thereof) provides grounds for a rejection of the null hypothesis of parallel trends. This test assesses whether the slopes of the time trend in pre-treatment deforestation patterns are equivalent (parallel) in the "treatment" and "control" grid cells. Indeed, none of the estimated interactions merits a rejection of the null hypothesis of equivalent

[8] These 17 months span 22 calendar months; there is a period of five months of missing data in the middle of this period.

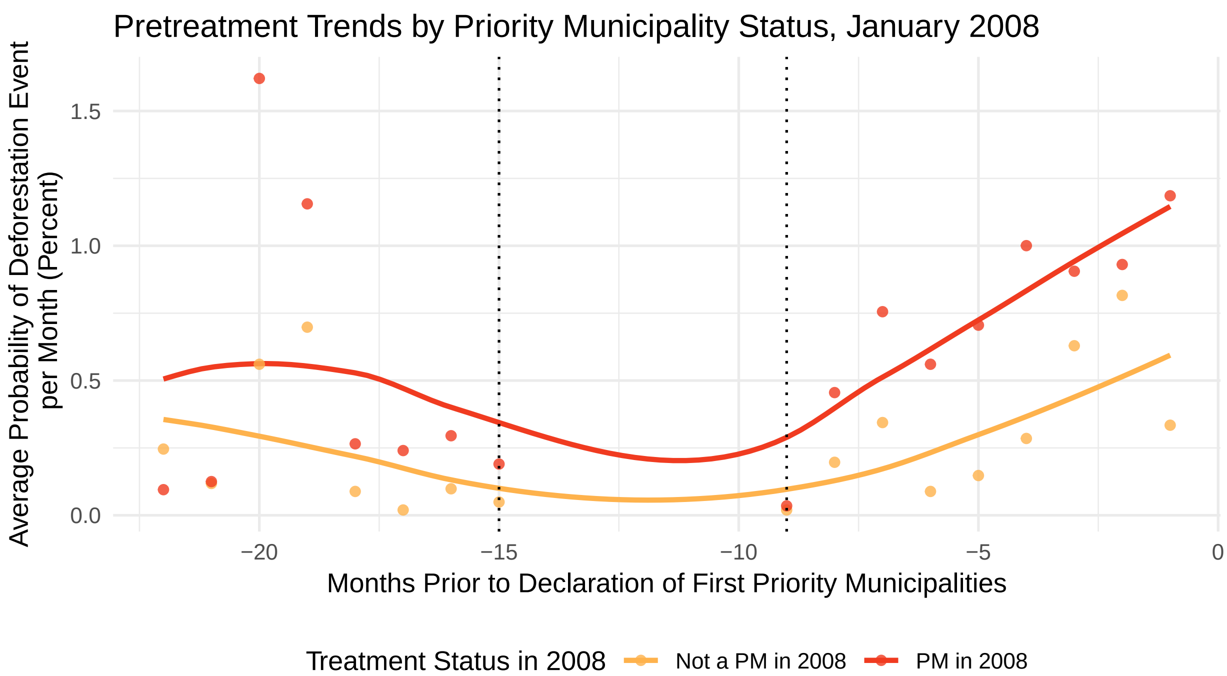

Figure 2: Average pre-treatment trends among the hexagons assigned to PM status in January 2008 and those not yet designated as PMs in 2008. The dotted vertical lines show the period during which data is missing. Visually, these trends lend credence to the parallel trends assumption.

slopes at the $\alpha = 0.1$ level. In a second set of tests (Columns 2, 4, and 6) we assess whether the slope of these trends differed systematically with distance from the border to the nearest PM. Here again, we do not recover evidence of a violation of the assumption of parallel trends.

We conduct these tests on a subset of the time series and find no evidence contrary to the parallel trends assumption. However, the treatment status of PMs changes throughout panel as municipalities are declared PMs or graduate from PM status (see Figure A3 for a visualization). This test of the parallel-trends assumption corresponds to the first assignment of PMs (January 2008-February 2009). As such, we report the effects of a neighbor's PM designation within the first assignment as well as across the full panel ("All Assignments") in the analyses that follow. Both samples include all pretreatment periods.

The Stable Unit Treatment Value Assumption (SUTVA) is an assumption underlying the difference-in-difference estimator. In this paper, we conceptualize units, grid cells outside PMs, as cluster-assigned to having a PM "neighbor." We argue that the spillover mechanism is geographic: PM protections radiate to nearby geographic locales. We model our empirical specifications as

PANEL A: BINARY DEFORESTATION EVENT INDICATOR (RESCALED BY A FACTOR OF 100)						
Neighbor is PM in 2008	0.232	0.686	0.046	0.302	−0.468	−0.410
	(0.212)	(0.524)	(0.126)	(0.344)	(0.329)	(0.456)
Neighbor is PM in 2008 × Month	−0.016	−0.057	0.072	0.125	0.479	0.689
	(0.044)	(0.102)	(0.050)	(0.095)	(0.296)	(0.481)
Neighbor is PM in 2008 × Month^2	0.001	0.003	−0.008	−0.016	−0.090	−0.130
	(0.002)	(0.005)	(0.007)	(0.014)	(0.058)	(0.100)
Neighbor is PM in 2008 × Month^3			0.0003	0.001	0.006	0.008
			(0.0002)	(0.001)	(0.004)	(0.007)
Neighbor is PM in 2008 × Month^4					−0.0001	−0.0002
					(0.0001)	(0.0002)
Neighbor is PM in 2008 × Distance		−0.005		−0.003		0.0004
		(0.004)		(0.003)		(0.004)
Neighbor is PM in 2008 × Distance × Month		0.001		−0.001		−0.003
		(0.001)		(0.001)		(0.003)
Neighbor is PM in 2008 × Distance × Month^2		−0.00002		0.0001		0.001
		(0.00004)		(0.0001)		(0.001)
Neighbor is PM in 2008 × Distance × Month^3				−0.00000		−0.00004
				(0.00000)		(0.00004)
Neighbor is PM in 2008 × Distance × Month^4						0.00000
						(0.00000)
DV Scale	$\{0, 100\}$	$\{0, 100\}$	$\{0, 100\}$	$\{0, 100\}$	$\{0, 100\}$	$\{0, 100\}$
PANEL B: LOG(AREA DEFORESTED + 1) (RESCALED BY A FACTOR OF 100)						
Neighbor is PM in 2008	0.250	0.535	0.109	0.155	−0.318	−0.539
	(0.225)	(0.563)	(0.143)	(0.421)	(0.234)	(0.438)
Neighbor is PM in 2008 × Month	−0.004	−0.010	0.063	0.170	0.401	0.720
	(0.036)	(0.089)	(0.045)	(0.092)	(0.252)	(0.479)
Neighbor is PM in 2008 × Month^2	0.0001	−0.0001	−0.007	−0.019	−0.075	−0.130
	(0.002)	(0.004)	(0.006)	(0.012)	(0.049)	(0.098)
Neighbor is PM in 2008 × Month^3			0.0002	0.001	0.005	0.008
			(0.0002)	(0.0004)	(0.003)	(0.007)
Neighbor is PM in 2008 × Month^4					−0.0001	−0.0002
					(0.0001)	(0.0001)
Neighbor is PM in 2008 × Distance		−0.004		−0.001		0.003
		(0.004)		(0.003)		(0.003)
Neighbor is PM in 2008 × Distance × Month		0.0002		−0.001		−0.004
		(0.001)		(0.001)		(0.003)
Neighbor is PM in 2008 × Distance × Month^2		−0.00000		0.0001		0.001
		(0.00003)		(0.0001)		(0.001)
Neighbor is PM in 2008 × Distance × Month^3				−0.00000		−0.00005
				(0.00000)		(0.00004)
Neighbor is PM in 2008 × Distance × Month^4						0.00000
						(0.00000)
DV Scale	$[0, 422.59]$	$[0, 422.59]$	$[0, 422.59]$	$[0, 422.59]$	$[0, 422.59]$	$[0, 422.59]$
Distance from PM	✓	✓	✓	✓	✓	✓
Observations	512,754	512,754	512,754	512,754	512,754	512,754

Table 1: Parallel trends test. All models include all constituent terms. We report only the interactions between the January 2008 treatment indicator and the polynomial time trends as our tests of the parallel trends assumption. All models estimated with OLS with standard errors clustered at the level of the nearest PM.

such: if this model is incorrect, our inferences may be biased. Qualitatively, we have no reason to dispute this model. While we group grid cells into clusters–a necessity for estimating the empirical specifications presented here–potential outcomes could be affected by the treatment assignment of other nearby PMs.

In the following section we also present a placebo test to strengthen the credibility of our empirical findings by providing one test of our final identifying assumption: excludability of treatment assignment. In particular, we aim to rule out the possibility that another phenomenon–another government policy, economic condition, or climate condition–that drives changes in deforestation patterns is correlated with the timing of treatment of PM status. Here, we find no consistent evidence of a violation of this assumption.

5 Findings

5.1 Main Results

We begin our presentation of the results with a descriptive analysis of the raw data, as shown in Figure 3. The upper panel shows the probability of deforestation events in grid cells at different distances from the nearest PM, before and after the PM status designation. As the graph shows, the raw data themselves hint at a clear result. Deforestation rates decrease close to the PM upon designation, but the difference disappears as distance increases to 50 kilometers. In turn, the relationship is actually reversed after 150 kilometers, as PM designation begins to increase deforestation – consistent with the idea of spatial displacement of deforestation away from PMs. The lower panel shows the differences in probability of a deforestation event, before versus after PM designation. Strikingly, the magnitude of these differences relative to baseline rates in the top panel are substantively quite large for cells quite close to and quite distant from the boundary with the nearest PM.

Table 2 estimates the effect of a neighbor's PM designation on deforestation patterns. The marginal effect of interest is that of having a neighboring municipality designated as a PM at different distances. It is captured by the formula $\widehat{\beta}_1 + \widehat{\beta}_2 \text{Distance}_{ij}$, where $\widehat{\beta}_1$ is the estimated coefficient for the neighboring PM designation and $\widehat{\beta}_2$ is the coefficient for the interaction term. All models thus include an indicator for PM designation and the interaction term with distance. Those without hexagon cell fixed effects (models 1-3) also include the constituent term for distance;

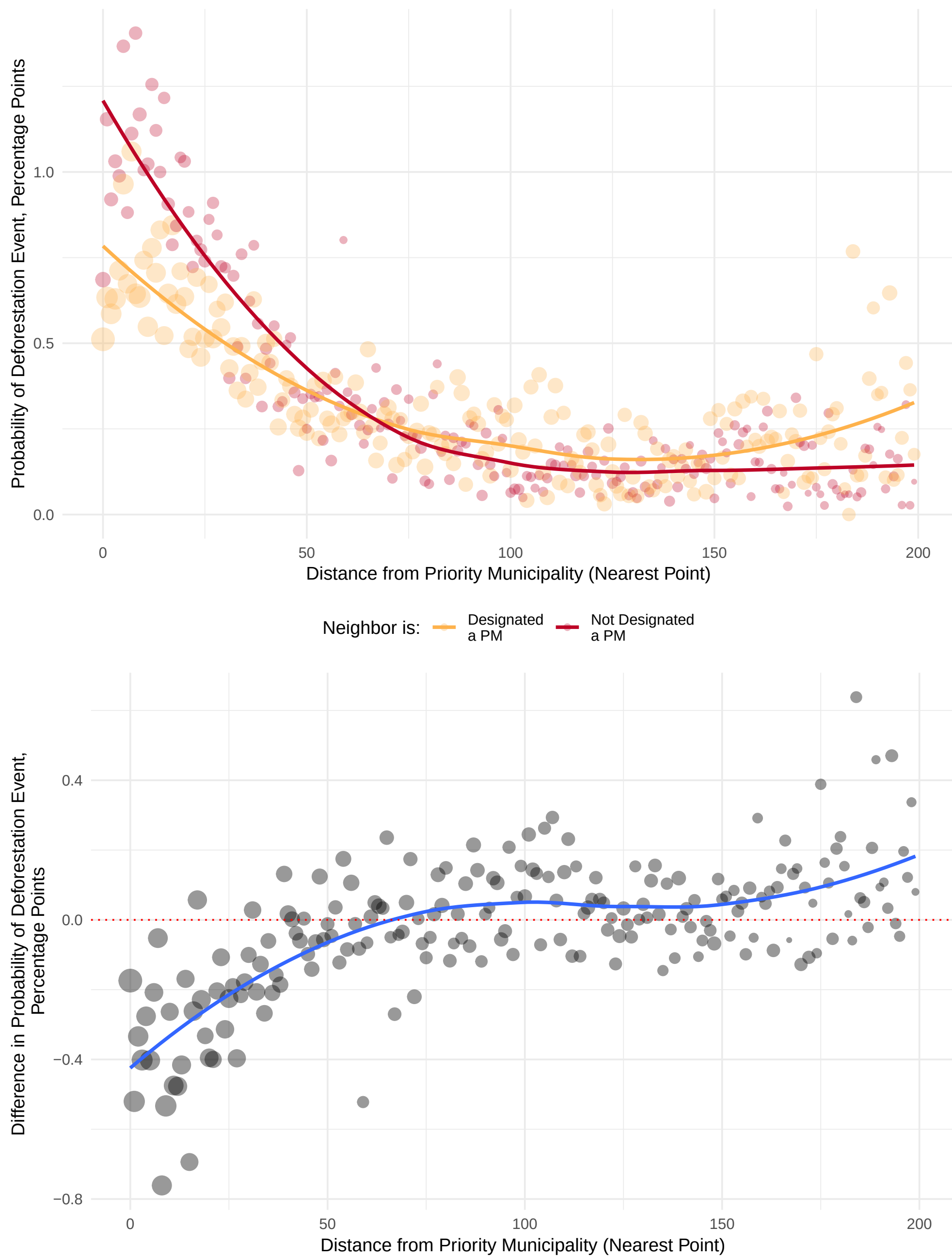

Figure 3: Descriptive analysis of our grid-cell level dataset. The points indicate bins of 1 kilometer; their size corresponds to the number of observations within a bin.

in models that include hexagon cell fixed effects, the base coefficient for distance is subsumed in the hexagon fixed effects. The key result is straightforward: close to the PM, the neighbor's PM designation reduces deforestation, as shown by the consistently negative and stastically significant point estimates. However, as the positive and precisely estimated interaction term shows, this negative effect attenuates toward zero as distance grows, and ultimately turns positive. This effect is clear whether examining events (Panels A and B) or the area deforested (Panels C and D). Moreover, these findings – a reduction in deforestation near the PM boundary and an increase in deforestation in distant areas – are consistent in both the original treatment assignment (Panels A and C) and the full panel (Panels B and D).

In Table 3, we test for nonlinearities in the effects of PM designation, by distance. The structure of the specifications parallels the previous one, except the linear distance variable is now replaced with binary indicators for different distance intervals, $\text{Distance}_{ij} \in \{\text{0-50 km}, \text{50-100 km}, \text{100-150 km}, \text{150-200 km}\}$. The omitted category is 0-50 km. Thus the effect of a neighboring PM designation is the sum of the PM coefficient and the interaction coefficient for the distance bin of interest. As the PM coefficient is negative but the interaction coefficients are positive and increasing in the distance category, the results are clear: as distance increases, the negative effect of PM designation in a neighboring municipality on deforestation attenuates and ultimately suggests an increase in deforestation rates in the 150-200 km bin. There is no evidence of nonlinearity in these effects.

Figure 4 illustrates the results on the full sample (all post-treatment periods) graphically; we report a parallel figure for the first-assignment subsample appears in Figure A7. The left panel shows the marginal effect of neighbor's PM designation as a function of distance, showing that the effect on deforestation flips from to become positive at around 100 kilometers. The right panel shows analogous estimates from the nonlinear model.

How large are these effects? We examine a highly disaggregate unit of analysis: hexagon grid cell months. As such, point estimates should, by construction, be quite small. To assess the substantive significance of these effects, we compare these estimates – measures of the difference in deforestation rates – to "baseline" rates of deforestation, by distance, prior to the implementation of the PM program. In Appendix A8 (page APP-18), we show that these differences represent sizable shifts relative to the baseline. For example, at the border (distance of 0), the reduction in deforestation among treated hexagons that is attributable to a neighbor's designation as a PM

	(1)	(2)	(3)	(4)	(5)
PANEL A: BINARY DEFORESTATION EVENT INDICATOR (RESCALED BY A FACTOR OF 100), FIRST ASSIGNMENT ONLY					
Neighboring Municipality is PM	−0.308	−0.404	−0.549	−0.590	−0.452
	(0.119)	(0.117)	(0.141)	(0.157)	(0.195)
Distance from PM	−0.006	−0.003	−0.003		
	(0.001)	(0.001)	(0.001)		
Neighboring Muni. is PM : Distance	0.003	0.004	0.004	0.004	0.003
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Observations	935,022	935,022	935,022	935,022	935,022
PANEL B: BINARY DEFORESTATION EVENT INDICATOR (RESCALED BY A FACTOR OF 100), ALL ASSIGNMENTS					
Neighboring Municipality is PM	−0.240	−0.314	−0.258	−0.272	−0.216
	(0.115)	(0.086)	(0.074)	(0.074)	(0.081)
Distance from PM	−0.005	−0.003	−0.003		
	(0.001)	(0.001)	(0.001)		
Neighboring Muni. is PM : Distance	0.002	0.003	0.003	0.003	0.002
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Observations	3,227,334	3,227,334	3,227,334	3,227,334	3,227,334
DV Scale	{0, 100}	{0, 100}	{0, 100}	{0, 100}	{0, 100}
PANEL C: LOG(AREA DEFORESTED + 1) (RESCALED BY A FACTOR OF 100), FIRST ASSIGNMENT ONLY					
Neighboring Municipality is PM	−0.348	−0.426	−0.486	−0.534	−0.420
	(0.109)	(0.104)	(0.134)	(0.150)	(0.174)
Distance from PM	−0.005	−0.003	−0.003		
	(0.001)	(0.001)	(0.001)		
Neighboring Muni. is PM : Distance	0.003	0.003	0.003	0.003	0.003
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Observations	935,022	935,022	935,022	935,022	935,022
PANEL D: LOG(AREA DEFORESTED + 1) (RESCALED BY A FACTOR OF 100), ALL ASSIGNMENTS					
Neighboring Municipality is PM	−0.335	−0.392	−0.267	−0.289	−0.257
	(0.090)	(0.070)	(0.058)	(0.062)	(0.063)
Distance from PM	−0.004	−0.003	−0.003		
	(0.001)	(0.001)	(0.001)		
Neighboring Muni. is PM : Distance	0.002	0.003	0.003	0.003	0.003
	(0.001)	(0.0005)	(0.0005)	(0.0005)	(0.001)
Observations	3,227,334	3,227,334	3,227,334	3,227,334	3,227,334
DV Scale	[0, 422.59]	[0, 422.59]	[0, 422.59]	[0, 422.59]	[0, 422.59]
Municipality FE		✓	✓		
Hexagon FE				✓	✓
Month FE		✓			
Month, Year FE			✓	✓	✓
Hexagon-Month FE					✓

Table 2: Estimates of the conditional ATT of neighbor's status as a PM on both deforestation outcomes. The ATT is the sum of the coefficient for neighbor's PM status and the coefficient for the interaction term multiplied by the distance. Standard errors are clustered at the level of the PM ($n = 52$).

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Binary deforestation event indicator (rescaled by a factor of 100)						
Neighboring Municipality is PM	−0.314	−0.574	−0.445	−0.234	−0.255	−0.197
	(0.104)	(0.145)	(0.181)	(0.100)	(0.071)	(0.075)
50-100 km from PM	−0.671			−0.556		
	(0.090)			(0.077)		
100-150 km from PM	−0.834			−0.690		
	(0.098)			(0.084)		
150-200 km from PM	−0.818			−0.662		
	(0.109)			(0.092)		
Neighboring Muni. is PM: 50-100 km	0.340	0.403	0.358	0.242	0.264	0.220
	(0.080)	(0.081)	(0.103)	(0.072)	(0.055)	(0.062)
Neighboring Muni. is PM: 100-150 km	0.399	0.447	0.351	0.276	0.311	0.252
	(0.099)	(0.110)	(0.159)	(0.090)	(0.074)	(0.086)
Neighboring Muni. is PM: 150-200 km	0.426	0.539	0.454	0.334	0.366	0.319
	(0.136)	(0.130)	(0.187)	(0.117)	(0.075)	(0.093)
DV Scale	{0, 100}	{0, 100}	{0, 100}	{0, 100}	{0, 100}	{0, 100}
Sample		First Assignment			All Assignments	
Observations	935,022	935,022	935,022	3,227,334	3,227,334	3,227,334
Panel B: Log(Area deforested + 1) (rescaled by a factor of 100)						
Neighboring Municipality is PM	−0.346	−0.509	−0.403	−0.329	−0.273	−0.241
	(0.101)	(0.144)	(0.169)	(0.084)	(0.060)	(0.062)
50-100 km from PM	−0.593			−0.480		
	(0.085)			(0.072)		
100-150 km from PM	−0.744			−0.593		
	(0.092)			(0.081)		
150-200 km from PM	−0.743			−0.584		
	(0.102)			(0.088)		
Neighboring Muni. is PM: 50-100 km	0.306	0.345	0.307	0.268	0.286	0.262
	(0.084)	(0.089)	(0.113)	(0.059)	(0.055)	(0.058)
Neighboring Muni. is PM: 100-150 km	0.381	0.433	0.346	0.331	0.358	0.326
	(0.095)	(0.104)	(0.139)	(0.075)	(0.068)	(0.073)
Neighboring Muni. is PM: 150-200 km	0.405	0.490	0.420	0.365	0.388	0.364
	(0.113)	(0.119)	(0.154)	(0.080)	(0.066)	(0.069)
DV Scale	[0, 422.59]	[0, 422.59]	[0, 422.59]	[0, 422.59]	[0, 422.59]	[0, 422.59]
Sample		First Assignment			All Assignments	
Observations	935,022	935,022	935,022	3,227,334	3,227,334	3,227,334
Hexagon FE		✓	✓		✓	✓
Month FE		✓	✓		✓	✓
Hexagon-Month FE			✓			✓

Table 3: Estimates of the conditional ATT of neighbor's status as a PM on deforestation outcomes. The ATT is the sum of the coefficient for neighbor's PM status and the interaction coefficient for the distance bin of interest. The base (omitted) category is 0-50km. Standard errors are clustered at the level of the PM.

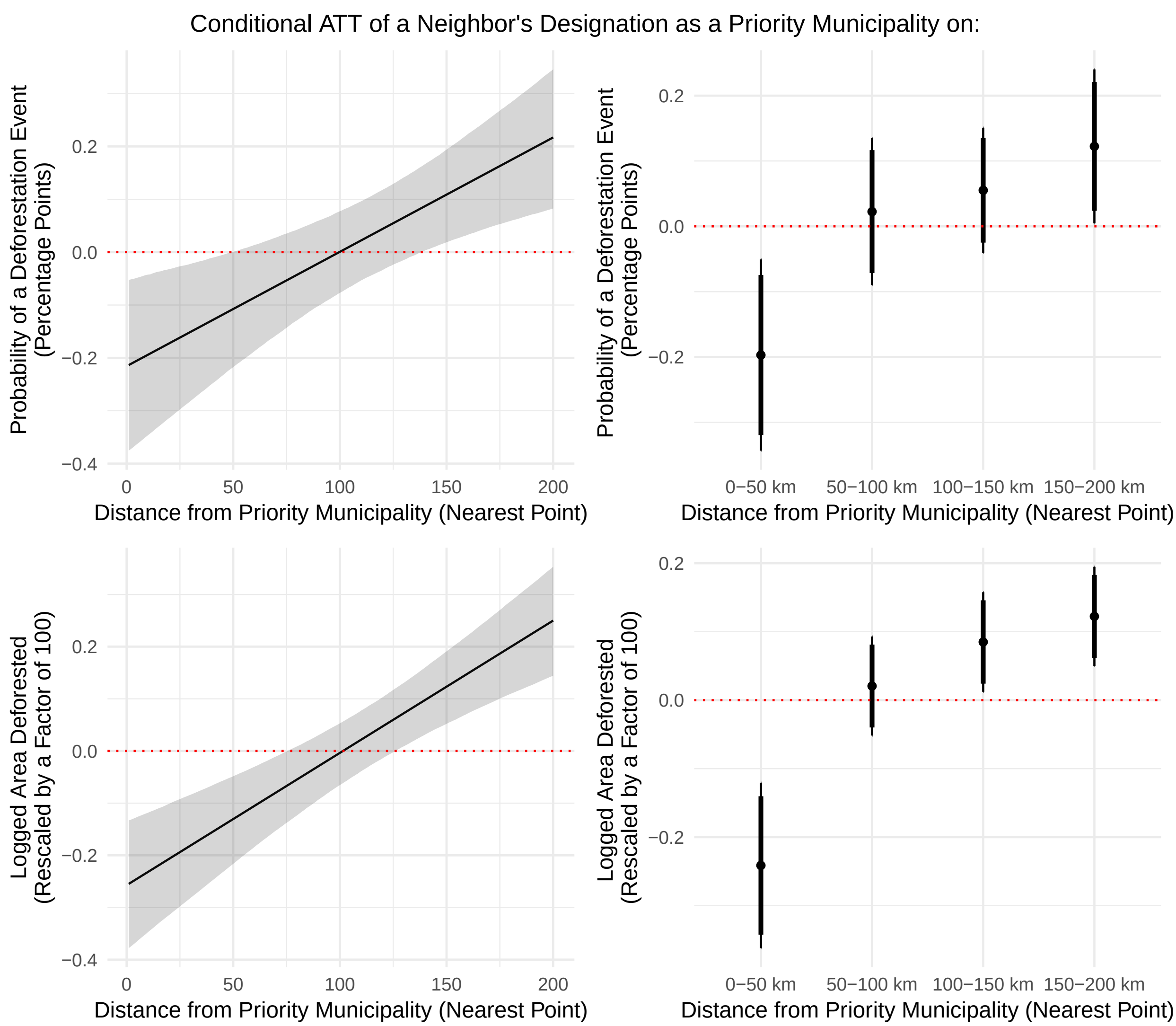

Figure 4: Conditional ATT estimates of a neighbor's status as a PM on deforestation outcomes. Graphed estimates depict specifications from Table 2 Column [5] in Panels B (upper left) and D (lower left) and Table 3 Panel A, Column [6] (upper right) and Panel B, Column [6] (lower right). 95% confidence intervals around point estimate are estimated using by standard errors clustered at the PM level.

is approximately 16 percent of the pretreatment deforestation rate. At 200 kilometers from the border, the estimated increase in deforestation represents a 48 percent increase in deforestation rates.[9] While we lack sufficiently detailed estimates of geographic variation in forest biomass or environmental conditions needed to quantify these effects in terms of environmental impact, the area of the Amazon that we study contains the largest stocks of forest biomass, by area, in the world, suggesting deforestation in this region even at small scales has exaggerated environmental significance (Saatchi and Morel, 2011).

5.2 Robustness Tests

We probe sensitivity of our findings to a variety of alternate specifications. First, show that our results are not driven by our designation of a temporal unit of aggregation: the month. As we aggregate from months to quarters, and then to years in Table A3 (page APP-15), point estimates increase in magnitude due to the aggregation and our inferences remain the same. In cells proximate to PMs, the probability of a deforestation event reduces when a PM is designated in the neighboring municipality. However, deforestation activity is displaced to areas further away from the PM.

We also test the robustness of estimates to dropping clusters, or the cells grouped with specific municipalities. Given the heterogeneous size of clusters, it is a natural concern that results may be driven by one influential PM. We drop one cluster – the grid cells associated with one PM – at a time and re-estimate the main specification (Table 4, Column [5]). In Appendix A9 (page APP-13), we graph both coefficients against the number of gridcells per cluster. All point estimates are similar in magnitude and sign. All estimates remain statistically significant at conventional thresholds.

Finally, recent considerations of difference-in-difference designs with multiple periods of post-treatment data have identified concerns with the substantive interpretation of the ATT estimand. In the presence of non-constant treatment effects, these estimates represent a weighted average of multiple difference-in-difference comparisons (Hull, 2018; Goodman-Bacon, 2018). In the first assignment period that we report in all tables and figures, these quantities are simpler conceptually. The consistence of our findings across specifications is reassuring. To further alleviate concerns about our substantive inferences, we consider how the addition of each period of post-treatment

[9] Further, the aggregate impact of these differences compounds over time. Appendix A8 (page APP-18) provides a back-of-the-envelope calculation that suggests that over three years, the differences that we estimate changes from -5 to +15 percentage points in the probability that an deforestation event occurs in a hexagon during this timeframe.

data affects our inferences. Figure A10 (page APP-16) suggests that the main finding of this paper of positive spillovers proximate to PMs with negative spillovers further afield are evident in shorter, conceptually simpler subsets of the post-treatment time series.

5.3 Placebo Test

In Figure 4, we conduct a placebo test. In each panel, we add a placebo variable to our main model: the 12-month period before actual PM designation. We utilize a 12-month placebo because deforestation patterns are seasonal. The placebo indicator should not have any impact on the relationship between distance from a future neighboring PM and deforestation, as the PM designation has not yet been made at this stage. In each case, the placebo variable is interacted with distance.

	(1)	(2)	(3)	(4)
PANEL A: BINARY DEFORESTATION EVENT INDICATOR (RESCALED BY A FACTOR OF 100)				
Neighboring Municipality is PM	−0.665	−0.500	−0.428	−0.364
	(0.163)	(0.213)	(0.095)	(0.104)
Placebo PM Indicator	0.231	0.162	0.270	0.255
	(0.119)	(0.159)	(0.088)	(0.092)
Neighboring Muni. is PM × Distance	0.004	0.004	0.004	0.003
	(0.001)	(0.001)	(0.001)	(0.001)
Placebo PM Indicator × Distance	−0.002	−0.001	−0.002	−0.002
	(0.001)	(0.002)	(0.001)	(0.001)
DV Scale	{0, 100}	{0, 100}	{0, 100}	{0, 100}
PANEL B: LOG(AREA DEFORESTED + 1) (RESCALED BY A FACTOR OF 100), FIRST ASSIGNMENT ONLY				
Neighboring Municipality is PM	−0.477	−0.356	−0.320	−0.283
	(0.156)	(0.190)	(0.072)	(0.074)
Placebo PM Indicator	−0.135	−0.172	0.056	0.046
	(0.135)	(0.188)	(0.073)	(0.076)
Neighboring Muni. is PM × Distance	0.003	0.002	0.003	0.002
	(0.001)	(0.001)	(0.001)	(0.001)
Placebo PM Indicator × Distance	0.001	0.002	0.00002	0.0001
	(0.001)	(0.002)	(0.001)	(0.001)
DV Scale	[0, 422.59]	[0, 422.59]	[0, 422.59]	[0, 422.59]
Observations	935,022	935,022	3,227,334	3,227,334
Sample	First Assignment		All Assignments	
Hexagon FE	✓	✓	✓	✓
Month FE	✓	✓	✓	✓
Hexagon-Month FE		✓		✓

Table 4: Placebo test. In this table, we test whether the 12-month period prior to PM designation – a placebo that should not have any substantive effects – conditions the relationship between distance from a future neighboring PM and deforestation.

The table reveals no consistent evidence of placebo effects. With the exception of Panel A, Columns [3] and [4], the placebo variable itself is always small and is never statistically significant and the interaction with distance is consistently near-zero. With the full sample of assignments and the binary dependent variable (Panel A, Columns [3] and [4]), the significance estimates on the placebo and interaction between the placebo and distance cut *against* our finding. While this result provides suggests the need for some caution in interpreting the results on the full sample of assignments, this finding is not robust to the operationalization of the dependent variable (Panel B) or in the original treatment sample (Columns [1] and [2]).

In Figures 5, we provide graphical illustrations of these placebo tests from Columns [2] and [4] from Table 4. For comparison, the red lines show the estimated conditional ATT from the true treatment and the yellow lines shows estimates from the placebo treatment. As in the table, the conditional ATT of the placebo is never distinguishable from zero except in the upper right panel, where it cuts in the opposite direction of the true treatment, albeit at a lower magnitude.

These results provide no consistent evidence against our identifying assumptions. They indicate that in the same season, one year before PM designation, there was no change in the relationship between distance and deforestation. The result is consistent with the idea that the PM designation, instead of factors such as a progressing deforestation frontier, drove deforestation away from the PM boundary upon designation.[10]

5.4 Evaluating the Mechanisms

Our theory asserts that positive spillovers occur as enhanced monitoring increases perceptions of the probability of enforcement in territory near PMs. As such, we would expect higher perceptions of the likelihood of enforcement in areas proximate to PM boundaries. Qualitatively, deforesters report a keen awareness of increased monitoring by IBAMA. Phillips (2011) interviews a large-scale cattle rancher – and admitted deforester – from the state of Amazonas who complains that "everyone knows that IBAMA is photographing us and what we are doing from two metres above." While such anecdotes suggest a keen awareness of the increased monitoring and sanctioning associated with the PM program, we lack systematic measures of such perceptions across space and time.

We bolster such qualitative accounts by examining state enforcement behavior. We analyze data

[10]Notably, in the one specification in which we find significant ATTs of the placebo, the point estimates cut in the opposite direction of the observed ATTs of the PM program. This finding cuts exactly against a hypothesis of a gradually expanding deforestation frontier.

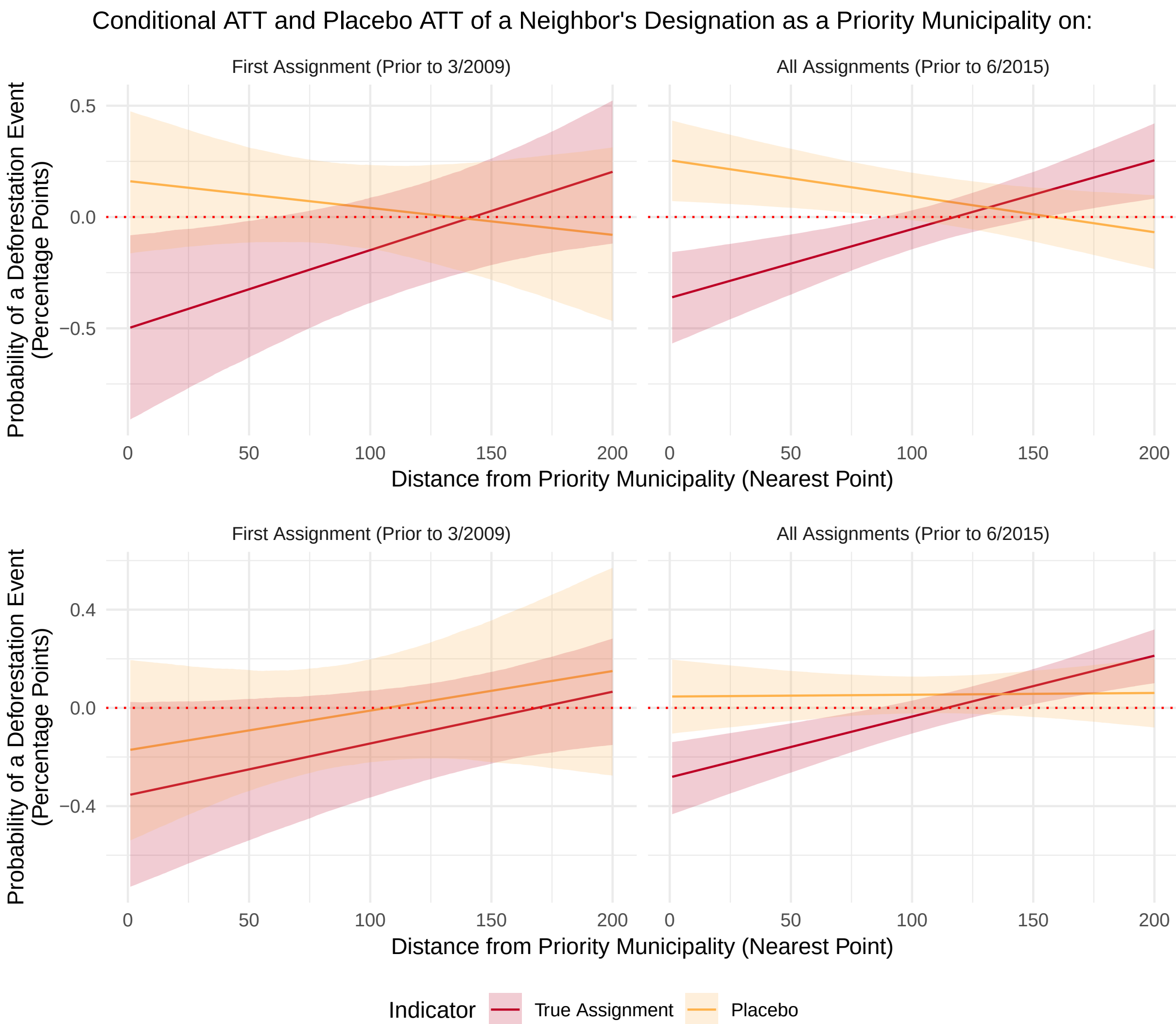

Figure 5: Conditional ATT (red) and placebo (gold) estimates corresponding to Table 4, Models in Columns [2] and [4] (both panels). The conditional ATT is the sum of the coefficient for neighbor's PM status and the interaction coefficient for the distance bin of interest. 95% confidence intervals around point estimate are estimated using by standard errors clustered at the PM level.

on the imposition of embargoes against further deforestation on individual rural properties found to participate in illegal deforestation. This program began in 2005 and, unlike the PM program, covers all municipalities. We observe embargoes at the *municipal* level, with much less precision than the deforestation events. We use a very similar empirical strategy to test whether the imposition of the PM program differentially impacts the imposition of these embargoes as a function of proximity to PMs. First, we identify the 604 municipalities that comprise PMs or are represented in our grid dataset. We construct a municipality-month level dataset from 2005-2015 that records the number of embargoes levied at the municipality level during each month in this period. In lieu of the distance measures in the main analysis given the larger cross-sectional unit, we use an indicator for municipalities that are immediate, second-, and third-degree, and fourth-degree neighbors of PMs (see Figure A19, page APP-26).

Using embargoes to measure the level of monitoring and the risk to deforesters presents some inferential challenges. Embargoes are enforced in response to deforestation events. Thus, a decrease in embargoes could indicate lower levels of enforcement or the occurrence fewer acts to punish. As such, we examine the embargoes descriptively and in light of the empirical findings on the occurrence of deforestation.

Figure 6 analyzes the rate of embargoes issued per month (top panel) in the year previous to and the first year of PM designation, 2007 and 2008. We graph these years to eliminate concerns about seasonality and comparability in the descriptive analysis, though the substantive results hold in the full first-assignment sample as well as the entire panel (see Appendix A4, page APP-27). Two patterns are evident: the rate of enforcement both pre- and post-treatment is highest in PMs and decreases monotonically in distance from the PM (the degree of the neighbor). In 2008, embargoes were issued at higher rates than in 2007, increasing an increase in enforcement efforts.

The difference-in-difference estimates in the second panel are estimated using a generalized difference-in-difference design paralleling the empirical strategy in the former analyses. We see that the PM program substantially increased enforcement in treated PMs in 2008, lending credence to our assertion of a shock to enforcement capacity (or willingness to enforce). The magnitude of the differential increase is particularly striking in light of the large decreases in deforestation events documented by Assunção and Rocha (2014). While the point estimate on "first degree" neighbor is positive, its confidence interval bounds zero. We do not observe differential increases in enforcement

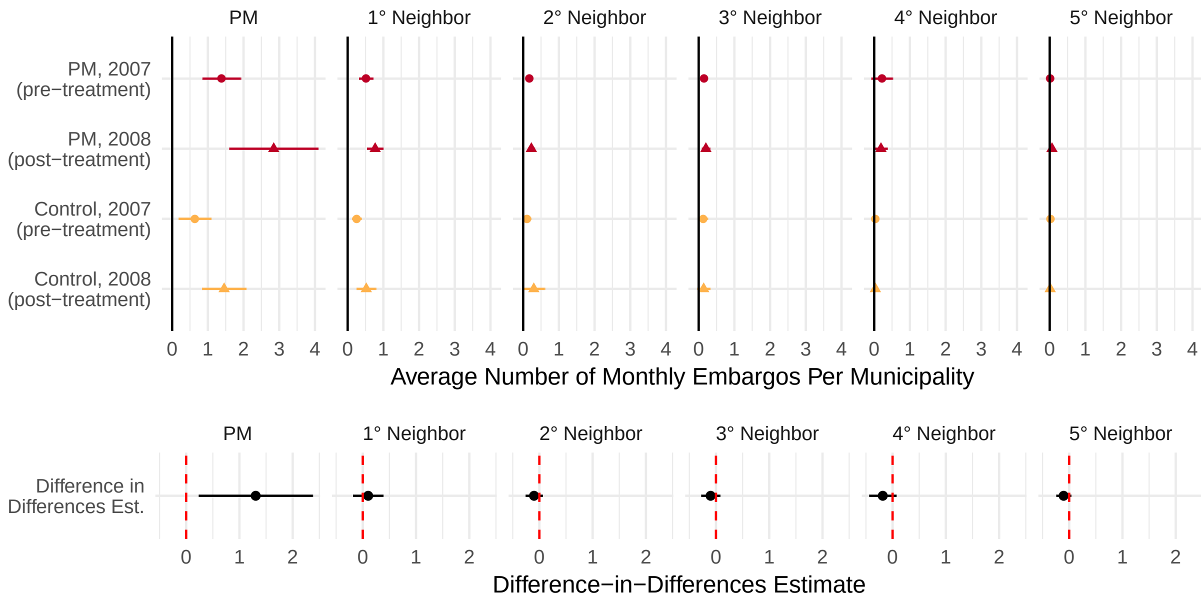

Figure 6: Analysis of embargoes levied in 2007 (pre-treatment) and 2008 (first post-treatment year) by municipal proximity to the nearest PM. The estimates in the second panel come from a generalized difference-in-difference model described in Appendix A12 (page APP-26) and capture the ATT of a PM's/neighbor's PM designation. 95% confidence intervals are calculated from standard errors clustered by nearest PM. $n = 14,496$ with 52 clusters.

outside PMs.

However, we must combine these findings with the evidence from the analysis of deforestation suggests that rates of deforestation outside the PMs do not change differentially between the "treatment" and "control" groups while the underlying deforestation patterns do. Close to the PMs, we see differential decreases in deforestation, but no differential decrease in enforcement. Taken together, this analysis suggests that the *risk* of enforcement, conditional on a deforestation event occurring, is higher in areas proximate to PMs. Thus, in tandem, the qualitative and quantitative evidence provide qualified support for the posited mechanism that increased monitoring differentially increases the risk of being caught in areas close to PMs.

5.5 Treatment Effect Heterogeneity

Our theory asserts that the cost of deforestation increases in distance from a central location. However, this cost should also vary in access to transportation networks that are vital in moving products to market. Thus, distance to waterways – a central mode of transportation in the region – provides a second source of variation in the costs of deforestation that is independent of spillovers in the likelihood of enforcement. To this end, we anticipate an attenuation in the estimated spillover effect by distance in areas with close proximity to the waterways, since the cost of extraction is these areas is lower independent of distance from the PMs. We estimate heterogeneous treatment effects on the basis of distance to waterways.

The measurement of waterways is quite straightforward. We code a binary indicator for close access to roads or water, respectively, at thresholds ranging from 2.5 to 10 kilometers. We estimate the conditional effects of a neighboring municipality being assigned to the PM program as a function of distance and access to water with a triple interaction, as detailed in the Appendix A9 (page A9). Our classifications of grid cells and proximity to water and roads are depicted in Figure A14.

Figure A15 depicts the estimated conditional effects of PM proximity as a function of access to waterways. Consistent with expectations, the magnitude of the conditional effect of a neighbor's PM designation and distance from the PM are slightly attenuated relative to the main estimate. The attenuation of the conditional ATTs proximate to waterways is consistent with our theoretical model, however, these differences are not statistically significant at conventional thresholds.

5.6 After Priority Municipality Designation

One remaining question persists: do spillover effects change once PM status is withdrawn? While only 11 municipalities had exited the program by 2015 (see Figure A3, page APP-7), we can examine whether spillovers in neighboring territory changed with this designation. There are, however, few reasons to expect large changes. One one hand, exit was conditioned on successful reductions in deforestation in the targeted PM. Once a municipality exited, it was moved to a "monitored" list, with continued (albeit fewer) resources and support from the federal government to reduce deforestation (Souza Costa Neves and Whately, 2016). On the other, once illegal activities are displaced, infrastructure has been created in distant areas to support related industries. There are then fewer incentives for deforestation activity to contract back toward the boundary with a PM.

Indeed, Appendix A11 (page APP-24) shows that we detect no discernible difference in the spillover effects in nearby territory when a PM moves to "monitored" (graduated) status. The conditional ATTs of a neighbor's designation as "monitored" status resemble those of a neighbor's designation as a PM. The finding has two plausible interpretations. First, lower intensities of monitoring than full PM status are sufficient to generate the positive and negative spillover patterns we cite. Second, the infusion of capacity coming from the PM designation altered capacity for enforcement – and known risks to deforesters – and infrastructure for extraction such that similar spillover effects persisted after its removal. Because of the sequencing of PM and "monitored" status, we cannot distinguish between these interpretations with the research design at hand.

6 Conclusion

We propose that we can understand the spread of state presence or capacity by exploiting short-term temporal variation. These patterns, which we term "projection dynamics," suggest that state presence spreads geographically, or overflows the boundaries of regions in which presence is higher. However, the relationship between the state and its subjects imposes limits to the the spread of state presence or enforcement. In order to identify these patterns empirically, we focus on the spillovers of a targeted policy to combat deforestation in the Brazilian Amazon.

Our analysis of the Brazilian PMs provide evidence for the proposed projection dyanmics. The results suggest a logic of displacement: although deforestation rates decrease close to PM borders after PM designation, they increase farther away from the PM borders after such designation. Moreover, the argument and methods provide a set of implications about the effectiveness for targeted policies that increase state presence or enforcement.

Probing the generalizability of the results is an important next step for research on the spatial projection of state capacity. In our case, a simple but principled model of the market for deforestation under imperfect monitoring correctly predicts the logic of displacement. The analytical conditions that underpin this prediction are that (i) policies that increase state presence and enforcement in targeted geographies, and (ii) by suppressing undesired activity in one area, increase the profitability of similar activities farther away. Conversely, policies that subsidize desired industrial activities may have positive spillovers around targeted areas, but they could also reduce the demand for similar industrial activities elsewhere.

The results posit practical implications for efforts to control deforestation. While we do not have rich enough data to estimate the optimal allocation of PM designations across the Amazon, our results do suggest that the contiguity of PMs is a potential problem. Increased oversight, monitoring, and enforcement at regular intervals across the Amazon could suppress the displacement of deforestation, and thus negate the displacement problem that we have identified. A potentially useful guideline for policy design would consider the direct effect of targeting (in PMs) and spillover effects including reduced deforestation nearby and increased deforestation farther away. Instead of designating municipalities based on their own deforestation status alone, environmental authorities should investigate trends in nearby areas. Designating PMs across the territory would bring benefits in that displacement of forest clearing to remote areas would be reduced. A checkered targeting scheme could prove effective in controlling the displacement of deforestation: any hexagon that is relatively close to at least one PM would benefit from enhanced monitoring.

Our results also serve as a warning against decentralized or subsidy-based measures to control deforestation. With decentralized measures or subsidies for forest conservation, local agents have incentives to focus narrowly on their own jurisdiction at the expense of neighboring areas. Given these incentives, the spatial displacement of deforestation could severely reduce the total effectiveness of the policy, as the lack of centralized monitoring would suppress positive and amplify negative spillovers. In this regard, our findings provide empirical support for the classical argument in "environmental federalism" (e.g., Oates and Portney, 2003) that cross-jurisdictional externalities call for centralized policy implementation. In contrast, programs such as "payments for ecosystem services" may lose some of their effectiveness, as they reward localized conservation without efforts to strenghten monitoring and enforcement in nearby areas. This design, our results suggest, has a flaw in that it produces negative but not positive spillovers.

These findings have broader implications for the study of state capacity. We argue that targeted policies of state intervention can be fruitfully exploited to understand the micrologic of state capacity expansion. Our results and research design thus offer a way forward for this research agenda. Beyond the environmental realm, areas of potential applications encompass policies providing many types of public goods. In particular, targeted policies to to reduce the spread of of infectious diseases, combat crime and violence, and encourage industrialization with investments in infrastructure may be particularly fruitful. Moreover, our argument for spillovers in state capability suggest that a naive approach ignoring spillovers will either underestimate or overestimate the total efficacy of the policy.

References

Acemoglu, Daron, Camilo García-Jimeno, and James A. Robinson. 2015. "State Capacity And Economic Development: A Network Approach." *American Economic Review* 105 (8): 2364–2409.

Assunção, Juliano, Clarissa Gandour, and Romero Rocha. 2015. "Deforestation Slowdown in the Brazilian Amazon: Prices or Policies?" *Environment and Development Economics* 20 (6): 697–722.

Assunção, Juliano, and Romero Rocha. 2014. "Getting Greener by Going Black: The Priority Municipalities in Brazil." Climate Policy Initiative.
URL: *https://climatepolicyinitiative.org/publication/getting-greener-going-black-priority-municipalities-brazil/*

Bertrand, Marianne, Esther Duflo, and Sendhil Mullainathan. 2004. "How Much Should we Trust Differences-in-Differences Estimates?" *Quarterly Journal of Economics* 119 (1): 249–275.

Besley, Timothy, and Torsten Persson. 2009. "The Origins of State Capacity: Property Rights, Taxation, and Politics." *American Economic Review* 99 (4): 1218–1244.

Boone, Catherine. 2003. *Political Topographies of the African State: Territorial Authority and Institutional Choice.* New York: Cambridge University Press.

Braga, Anthony A. 2005. "Hot Spots Policing and Crime Prevention: A Systematic Review of Randomized Controlled Trials." *Journal of Experimental Criminology* 1: 317–342.

Cai, Hongbin, Yuyu Chen, and Qing Gong. 2016. "Polluting Thy Neighbor: Unintended Consequences of China's Pollution Reduction Mandates." *Journal of Environmental Economics and Management* 76: 86–104.

Cardoso de Andrade, Luíza. 2016. "Spillover Effects of Blacklisting Policy in the Brazilian Amazon." Thesis, Universidad de São Paulo.

Enriquez, Elaine, and Miguel Angel Centeno. 2012. "State Capacity: Utilization, Durability, and the Role of Wealth vs. History." *RIMCIS-International and Multidisciplinary Journal of Social Sciences* 1 (2): 130–162.

Foa, Roberto Stefan, and Anna Nemirovskaya. 2016. "How State Capacity Varies within Frontier States: A Multicountry Subnational Analysis." *Governance* 29 (3): 411–432.

Getmansky, Anna, Guy Grossman, and Austin L. Wright. 2017. "Border Fortification and the Economics of Crime." Working Paper, University of Essex.
URL: *https://www.researchgate.net/publication/304251182_Border_Fortification_and_the_Economics_of_Crime*

Gonzales, Jenny. 2017. "Brazilian Police Nab Amazon Timber Thieves Who Faked Forest Credits.".
URL: *https://news.mongabay.com/2017/10/brazilian-police-nab-amazon-timber-thieves-who-fak*

Goodman-Bacon, Andrew. 2018. "Difference-in-Differences with Variation in Treatment Timing." `https://s3.amazonaws.com/vu-my/wp-content/uploads/sites/2318/2018/06/18160651/ddtiming_6_15_2018.pdf`.

Guiteras, Raymond, James Levinsohn, and Ahmed Mushfiq Mobarak. 2015. "Encouraging Sanitation Investment in the Developing World: A Cluster-Randomized Trial." *Science* 348 (6237): 903–906.

Herbst, Jeffrey. 2014. *States and Power in Africa: Comparative Lessons in Authority and Control.* Revised ed. Princeton: Princeton University Press.

H.J. 2013. "Almost Out of Time: An Interview with Marina Silva." *The Economist.*
URL: *http://www.economist.com/blogs/americasview/2013/09/interview-marina-silva-0*

Hull, Peter. 2018. "Estimating Treatment Effects in Mover Designs." `http://www.mit.edu/~hull/movers_042018.pdf`.

Kahn, Matthew E., Pei Li, and Daxuan Zhao. 2015. "Water Pollution Progress at Borders: The Role of Changes in China's Political Promotion Incentives." *American Economic Journal: Economic Policy* 7 (4): 223–242.

Lipscomb, Molly, and Ahmed Mushfiq Mobarak. 2017. "Decentralization and Water Pollution Spillovers: Evidence from the Redrawing of County Boundaries in Brazil." *Review of Economic Studies* 84 (1): 464–507.

Magaloni, Beatriz, Edgar Franco, and Venessa Melo. 2015. "Killing in the Slums: An Impact Evaluation of Police Reform in Rio de Janeiro." Stanford Center for International Development Working Paper.
URL: *http://globalpoverty.stanford.edu/sites/default/files/Edgar_Franco_Vivanco-Killing_slums.pdf*

Mann, Michael. 1984. "The Autonomous Power of the State: Its Origins, Mechanisms, and Results." *European Journal of Sociology* 25 (2): 185–213.

Nepstad, Daniel, David McGrath, Ane Alencar, A.C. Barrros, G. Carvalho, Marcio Santilli, and M. del C. Vera Diaz. 2002. "Frontier Goverance in Amazonia." *Science* 295: 629–630.

Oates, Wallace E., and Paul R. Portney. 2003. "The Political Economy of Environmental Policy." *Handbook of Environmental Economics* 1: 325–354.

O'Donnell, Guillermo. 1993. "On the State, Democratization and Some Conceptual Probems: A Latin American View with Glances at Some Postcommunist Countries." *World Development* 21 (8): 1355–1369.

Oestreicher, Jordan S., Karina Benessaiah, Ruiz-Jaen Maria C., Sean Sloan, Kate Turner, Johanne Pelletier, Bruno Guay, Katheryn E. Clark, Dominique G. Roche, Manfred Meiners, and Catherine Potvin. 2009. "Avoiding Deforestation in Panamanian Protected Areas: An Analysis of Protection Effectiveness and Implications for Reducing Emissions from Deforestation and Forest Degradation." *Global Environmental Change* 19: 279–291.

Phillips, Tom. 2011. "Brazil's crackdown on deforestation of the Amazon.".

Saatchi, Sassan S., Nancy L. Harris Sandra Brown Michael Lefsky Edward T. A. Mitchard William Salas Brian R. Zutta Wolfgang Buermann Simon L. Lewis Stephen Hagen Silvia Petrova Lee White Miles Silman, and Alexandra Morel. 2011. "Benchmark map of forest carbon stocks in tropical regions across three continents." *Proceedings of the National Academy of Sciences* 108 (24): 9899–9904.

Scott, James C. 1998. *Seeing Like a State: How Certain Schemes to Improve the Human Condition Have Failed*. New Haven and London: Yale University Press.

Short, Martin B., P. Jeffrey Brantingham, Andrea L. Bertozzi, and George E. Tita. 2010. "Dissipation and Displacement of Hotspots in Reaction-Diffusion Models of Crime." *Proceedings of the National Academy of Sciences* 107 (9): 3961–3965.

Soifer, Hillel. 2008. "State Infrastructural Power: Approaches to Conceptualization and Measurement." *Studies in Comparative International Development* 43 (3-4): 231–251.

Soifer, Hillel. 2012. "Measuring State Capacity in Contemporary Latin America." *Revista de Ciencia Politica* 32 (3): 585–598.

Soifer, Hillel David. 2015. *State Building in Latin America*. New York: Cambridge university press.

Souza Costa Neves, Estela Maria, and Marussia Whately. 2016. "Municipalities and Policies Against Deforestation in the Brazilian Amazon." *Novos estudos CEBRAP* 35 (3).

Weisburd, David, and John Eck. 2004. "What Can Police do to Reduce Disorder and Fear?" *Annals of the American Academy of Political and Social Science* 593: 42–65.

Wheeler, David, Dan Hammer, Robin Kraft, and Aaron Steele. 2014. "Satellite-Based Forest Clearing Detection in the Brazilian Amazon: FORMA, DETER, and PRODES." World Resources Institute, Washington DC.