Eyes beneath the canopy: co-enforcing environmental crackdowns in the Brazilian Amazon.

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Abstract How do state-society relations affect law enforcement crackdowns? I propose that sharp increases in state resources will be most effective wherever we find synergistic dynamics between officials and local groups. I argue that this pattern is the result of the two actors engaging in the co-production of law enforcement, where state agents provide technical expertise while communities offer their deep knowledge of the terrain. I test this theory by examining anti-deforestation policies in the Brazilian Amazon. Exploiting the blacklisting of municipalities between 2008 and 2019 I show that while the crackdown led to significant forest cover retention within indigenous lands, its effects outside of them were mixed. This pattern is the result of uneven shifts in the costs of crime: increases in enforcement are shaped by indigenous presence, which in turn affect future criminal behavior. Consequently, good environmental outcomes require a combination of formal policies *and* local support.

Keywords *Brazil*; *environmental enforcement*; *co-production*; *deforestation*; *indigenous.*

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> *"To protect the Amazon region, it is good to combine ancestral wisdom with contemporary technical knowledge, always working for a sustainable management of the land while also preserving the lifestyle and value systems of those who live there."*
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> Pope Francis (2020, p. 39)

1 State, society, and deforestation in the tropics.

On June 30th, 2016, ninety-five federal police officers, thirty-two environmental agents, sixteen tax officials, and two aircraft descended on a patch of land in the Eastern Brazilian Amazon. Twenty-four individuals were arrested on site, and another nineteen arrest warrants were issued across the country (IBAMA, 2016d). These trespassers were accused of cutting down more than 290 km^2 of the rainforest, generating damages amounting to more than USD$350 million, and relying on workers in conditions akin to modern slavery. This move was the result of Operation *Rios Voadores*, a joint task force encompassing five Brazilian federal agencies. While the story broke national news (Folha de São Paulo, 2016), comparatively less attention was placed on a key ally in said operation: the Kayapó people of the Menkragnoti indigenous community. As the environmental agency later reported, allied indigenous groups identified the location of several camps that were used by illegal loggers, as well as guiding officers to different hotspots (IBAMA, 2016d). The partnership culminated with a striking image: indigenous leaders standing side by side with environmental enforcement agents, watching more than a dozen chainsaws burn in a fire.

Can state-society relations shape the effectiveness of law enforcement crackdowns, environmental or otherwise? Recent work on community policing has identified the potential benefits of incorporating citizen input in law enforcement (Nanes, Ravanilla, & Haim, 2023), yet we still lack a general theory of why state agents decide to rely on civil society for enforcement, as well as measuring its effectiveness outside of traditional urban contexts.

Furthermore, while "co-production" has been proposed to be a useful tool for improving social conditions (World Bank, 2003), few studies have sought to measure its effects in a systematic way (Voorberg, Bekkers, & Tummers, 2015). Following key insights from the governance literature (Baldwin, 2016; Evans, 1997; Levi, 2008; Ostrom, 1996; Tsai, 2007), this paper seeks to fill both gaps by studying how the effectiveness of crackdowns varies depending on support from local communities in the Brazilian Amazon. Tackling this issue will help to dispel the idea that the state is the only relevant player in law enforcement, particularly in underdeveloped settings. Furthermore, in the context of an ever-increasing threat of climate change the question of how best to protect areas like the Amazon is crucial, as rainforests represent some of the biggest carbon sinks in the planet and their erosion is considered to be among the largest drivers of biodiversity loss (Lapola et al., 2023).

In this paper I argue that contexts of administrative constraints—such as curbing deforestation in the tropics—will incentivize state officials to rely on local allies for information gathering and monitoring, in line with the "fire-alarm" model of oversight (McCubbins & Schwartz, 1984). In turn, relying on civil society input means that crackdowns—understood as sharp increases in monitoring and enforcement resources—will have heterogeneous effects: while areas where state officials are met with willing local partners will observe an improvement, the outcome will be unclear wherever agents are forced to work on their own. I will conceptualize this relationship as the "co-production" of law enforcement (Ostrom, 1996), where local allies contribute valuable on-the-ground information that complements officials' technical expertise, thereby "shoring up" efforts to curb illegal activity. Finally, I argue that the heterogeneous effects of crackdowns are the result of uneven shifts in the risks of illegal activity, as potential law-breakers will pivot away from areas they perceive as being closely monitored.

I test these claims by studying deforestation in the Brazilian Amazon, a map of which is included in Figure 1. I start by training a text classification model and show that, in addition to satellite-based monitoring, environmental agencies in Brazil systemat-

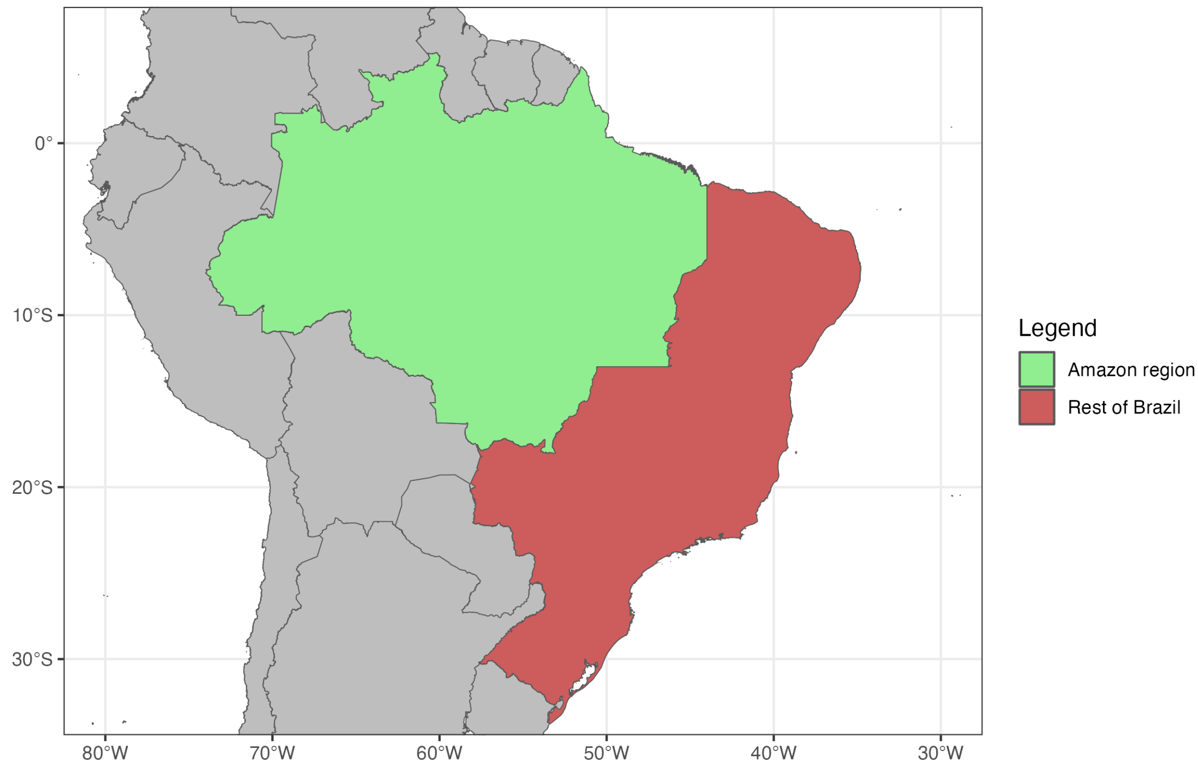

Figure 1: *the Brazilian Amazon and the rest of the country within South America.*

ically collaborate with indigenous communities. Then, exploiting a mid-2000s policy that blacklisted worst-performing municipalities—the *Lista de Municípios Prioritários*—I implement a triple-difference design intended to identify divergences between indigenous and non-indigenous areas as a result of the crackdown. Furthermore, I implement additional panel models to identify whether this divergence is the result of an improvement in indigenous lands or worsening conditions in non-indigenous lands. I find improvements in forest cover among treated indigenous territories, while non-indigenous areas experienced a decrease in cover. As I will explain, these results are consistent with recent findings of spillover deforestation from targeted areas to their surroundings (Slough & Urpelainen, 2019). In order to support the main mechanism I focus on individual deforestation events since 2016 and show that environmental fines are issued at higher rates in indigenous territories under increased monitoring schemes. Furthermore, I also provide evidence that the rate of enforcement is associated with decreases in future deforestation.

Aside from recent work on the electoral politics of deforestation (Fernández Milmanda & Garay, 2020; Harding, Prem, Ruiz, & Vargas, 2023; Pailler, 2016; Sanford, 2021), previous studies on forest loss have focused mostly on the role of state enforcement. While there has been disagreement over the effect of state territorial reach (Revelo-Rebolledo, 2019), most scholars agree that enforcement capacity has a negative effect on deforestation (Assunção & Rocha, 2019; Prem, Saavedra, & Vargas, 2020; Tacconi, Rodrigues, & Maryudi, 2019). However, across these studies the effectiveness of crackdowns has been conceptualized as depending exclusively on the state, neglecting the role of civil society. Of the studies that have incorporated societal actors, findings appear to be contradictory. For example, while some have linked ethnic diversity to increased deforestation due to challenges of collective action (Alesina, Gennaioli, & Lovo, 2019), others have posited that group recognition by the state can decrease forest loss when paired with collective property rights (Baragwanath & Bayi, 2020; Gulzar, Lal, & Pasquale, 2023). Community involvement has also showed mixed results, as experimental research has tied it to more inclusive governance arrangements, but without a clear effect on forest cover (Christensen, Hartman, & Samii, 2021; Slough, Kopas, & Urpelainen, 2021). Finally, while recent work on the co-enforcement of crackdowns has stressed how communities are key at providing resources to officials (Amengual, 2016), the of role intelligence gathering and monitoring has been under examined, particularly in remote settings. This paper seeks to contribute by highlighting the role of non-state actors, speaking to the contradictory effects of societal involvement, and continuing to elucidate the dynamics behind the co-enforcement of crackdowns.

2 Fire alarms and the success of crackdowns.

In this section I will lay out how state-society relations influence the enforcement of crackdowns, which I denote as the "intel-to-outcome" stage. Furthermore, I will also describe the broader context where such cooperation takes place—i.e., the "grievance-to-intel" stage.

2.1 Intel-to-outcome: collaboration shores up enforcement.

Consider a case where state officials choose to enforce the law in order to curb criminal activity. What factors influence their success? I use the term "crackdown" to denote this attempt, which should be understood as a sharp increase in the monitoring and enforcement resources available to street-level officials, as well as a general mandate to enforce the law by political authority. Crackdowns are usually targeted as a consequence of limited resources (Slough & Urpelainen, 2019), which leads to an explicit focus on "troublesome" areas. As the previous definition suggests, the two main factors influencing the success of a crackdown should be the amount of resources that are available to state agents, as well as the strength of the political mandate under which they are operating. Thus, this framework suggests that variations in the effectiveness of crackdowns will follow variations in the state's coercive capacity (Alcañiz & Gutierrez, 2020; Slough & Urpelainen, 2019), as well as variations in political pressure for or against cracking down (Harding et al., 2023; Holland, 2015).

The framework presented in the previous paragraph conforms to the "police-patrol" model of oversight, as laid out by McCubbins and Schwartz (1984): a centralized bureaucracy enforces regulations by monitoring behavior for potential violations and responding accordingly. This model places an onerous burden on the overseeing body, which can sometimes be unfeasible—for example, if the mandate is broad, if the jurisdiction is massive, and if the agency lacks sufficient resources. As McCubbins and Schwartz (1984) explain, the previous obstacles lead to some agencies adopting a "fire-alarm" model of oversight: instead of reviewing each action to assess whether it complies with regulations, an agency can incentivize third parties to denounce actions they perceive as harmful before deciding to act. Thus, while the police-patrol model suggests that variations in the effectiveness of crackdowns will follow variations in resources available for enforcement, the fire-alarm model suggests that success will follow variations in the availability of local third parties that are willing to collaborate with the agency in order to monitor for compliance.

While the model proposed by McCubbins and Schwartz (1984) was conceived to explain the relationship between the US Congress and the Executive, it has served as a framework to understand law enforcement in general (Nanes et al., 2023). Thus, instead of organized interests contesting perceived Executive overreach, fire-alarm enforcement describes instances of cooperation between citizens and state actors with the aim of curbing illegal activity. As the governance literature has pointed out, in such cooperative relations officials tend to offer technical expertise and resources, while communities tend to offer their deep knowledge of the territory and local challenges (Evans, 1997). Most law enforcement takes place in contexts of limited resources, which can be further compounded by administrative constraints—a point that will be expanded upon later. In circumstances such as those, bureaucratic "porousness" can be a tool for providing information to street-level bureaucrats that lack the proper context for decision-making (Amengual, 2016). Thus, civil society actors can help "shore up" the state's attempts to control illegal activity. The concept of "co-production," proposed by Elinor Ostrom (1996), encapsulates this idea of a synergistic relationship between state and society, one that is more efficient and effective than either party working on their own.

The use of "intel" to describe what is being shared is apt, as the main good being exchanged will be information: where trespassing has taken place, who the culprits are, when they might reappear, and who is being affected by criminal activity. Local communities can leverage their experience inhabiting an area and provide practical knowledge (Scott, 2008), which state officials lack. However, it is unlikely that said groups will be spread evenly across the territory where illegal activity is taking place, but rather clustered in certain areas. Consequently, the distribution of allied groups will impact the effectiveness of the increase of resources that a fire-alarm crackdown has brought: areas with presence of allied groups should experience a stronger effect than areas without them. Figure 2 summarizes the expected effects of a crackdown within the framework described thus far, alongside the expected outcome for the case of anti-deforestation efforts.

I posit that the main effect of a crackdown will be a shift in the perceived costs of engaging in illegal activity for law-breakers. In general, more enforcement resources should

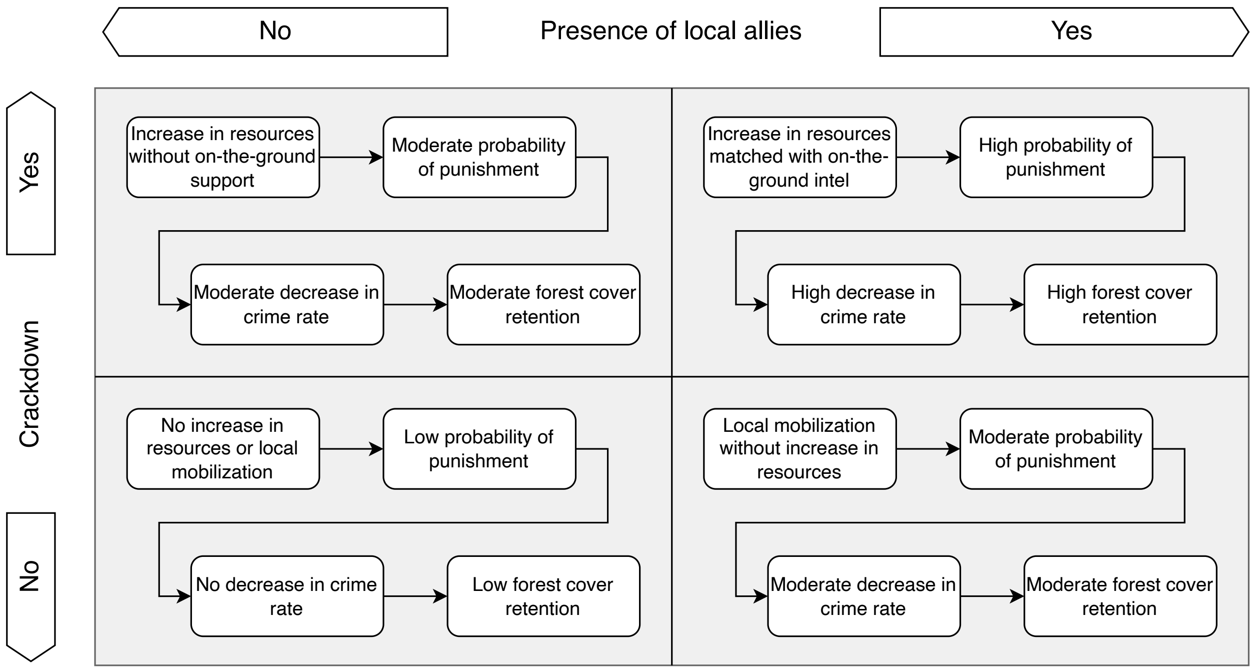

Figure 2: *fire-alarm enforcement means the success of crackdowns depend on local support.*

lead to an increased probability of punishment: all else being equal, an increase in the frequency of surveillance should lead to a higher probability of punishing illegal actions. If a higher probability of punishment tends to dissuade potential law-breakers (Becker, 1968; Olken, 2007), then the expectation is that there should be an improvement across the board in areas that are subject to the crackdown, while areas not subject to it should not see an improvement. Furthermore, when a crackdown is co-enforced by state officials and unevenly-spread allies the costs compound. Law-breakers should be especially dissuaded from engaging in illegal activity in areas under crackdowns where state officials have local allies, given the even higher probability of punishment. In summary, my hypotheses are:

- *H1: areas under a crackdown will see improvements in contrast to non-targeted areas.*
- *H2: the effect of the crackdown will be greater in areas where local allies are present than in areas where they are absent.*

2.2 Grievance-to-intel: why collaborate?

Why would state agents and local communities collaborate to begin with? Answering this question can provide insight about the theory's scope conditions, which are illustrated by Figure 3. My point of departure concerns the existence of illegal activity, which tends to create costs. Said costs can be spread among the population or concentrated on certain groups. As scholars of collective action have pointed out, the latter tends to spur mobilization (Olson, 2003). Furthermore, a group's capacity to react will also be a function of its cohesion, which can be shaped by a common life experience or ascriptive characteristics such as race or religion (Varshney, 2003). Thus, if illegal activity results in concentrated costs for highly cohesive groups, we should expect local actors to mobilize against it. On the side of the state, while criminal activity represents a constant challenge to its authority (Weber, 1958), that threat will not necessarily lead to state action unless there is pressure to act, be it domestic or international. As the literature on business and politics explains, the degree to which the public will demand state action will depend on the visibility of the topic being discussed (Culpepper, 2010): in cases where the costs of illegality are mostly hidden, regulatory capture will be likely; in contrast, in cases where the costs are visible, the state will be more likely to get involved as a result of public pressure.

Under these conditions, then, illegal activity will lead both state actors and certain civil society groups to mobilize. One alternative they face is to act alone: state actors could directly seek to enforce the law—following the police-patrol model previously discussed—and civil society groups could resort to vigilantism. Why they might decide to collaborate will depend on the specific administrative context. First, on the side of the state, officials might be interested in cooperating in contexts where enforcement is complicated and costly, either due to the task itself or the political environment. In these circumstances, officials might be interested in outsourcing part of their tasks to avoid incurring in high costs (Rich, 2022). Outsourcing allows for a leaner bureaucracy, which lowers the stake behind opposition control. Furthermore, the organization does not need to be as present as it otherwise would,

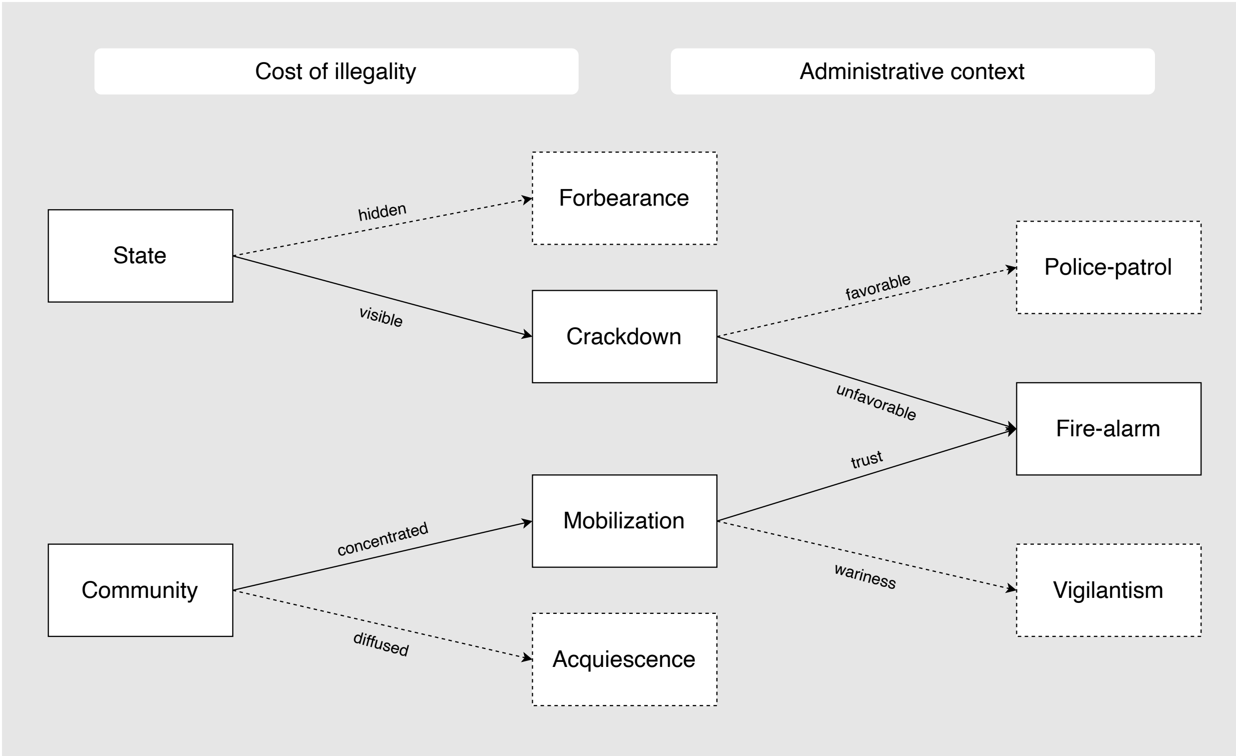

Figure 3: *a stylized account of the pre-conditions for co-enforcing a crackdown.*

which minimizes backlash to unpopular enforcement. And second, for civil society groups to cooperate with government officials would require a previous relationship of trust. Trust can arise from citizens towards authorities if the former believe that the latter are providing useful public goods (Levi, 2003; Nanes et al., 2023; Wilke, 2020). Moreover, bureaucrats can court civil society groups directly in order to create independent sources of support (Carpenter, 2002).[1] The result of these factors will be agents and aligned groups willing to collaborate in the process of enforcing the law.

3 The Brazilian Amazon: an ideal testing ground?

The Brazilian section of the Amazon rainforest represents an attractive setting to test this theory. As Tacconi et al. (2019) explain, the country went from being one of the worst

[1]Consequently, for the argument to hold trust from these groups need not be directed at the state at large, but at the agencies in charge of enforcement.

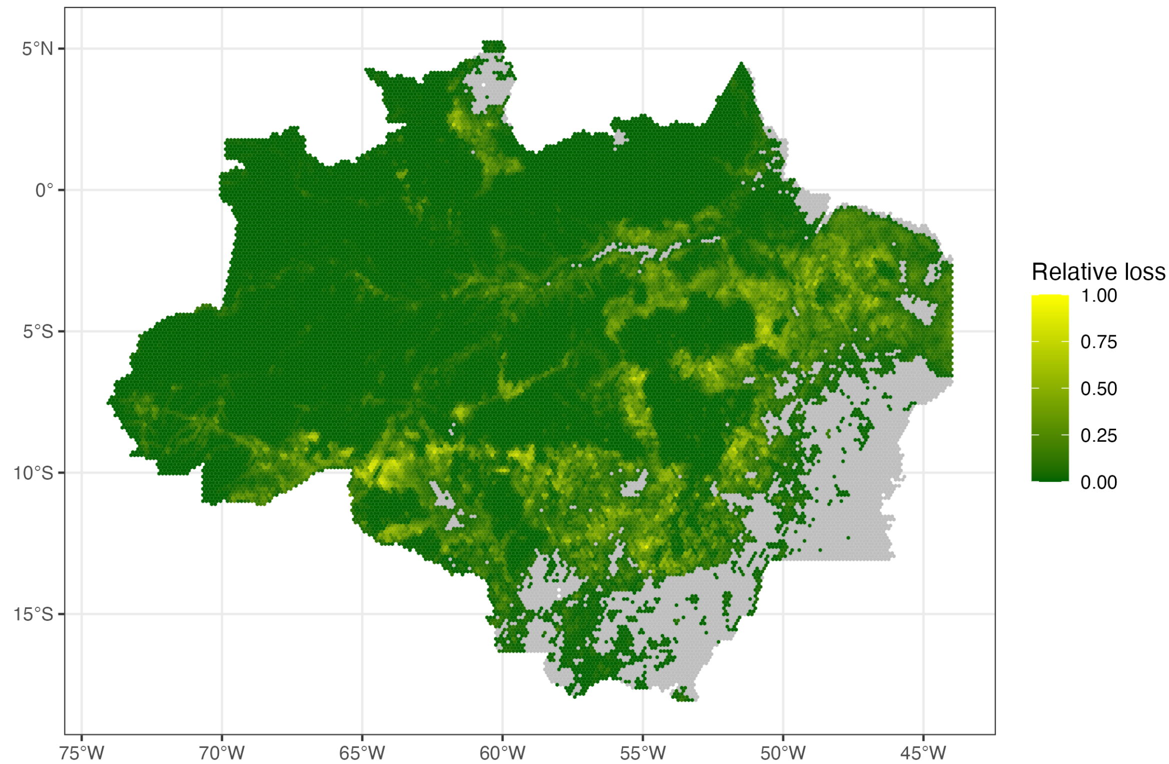

Figure 4: *cumulative change in forest cover since 2000, previously-cleared areas removed.*

offenders in terms of deforestation in the late nineties, to implementing a series of relatively successful measures. Subsequent administrations have installed harsh penalties for environmental crimes; furthermore, the creation of Conservation Units during the last three decades has also contributed to slowing down the rate of deforestation (Rylands & Brandon, 2005). While its section of the Amazon is still the one with the highest degree of absolute deforestation (Ungar, 2018), most of it took place prior to the mid 2000s crackdown. Still, deforestation continues to this day, around 90% of which is illegal (Reuters, 2016). As Figure 4 illustrates, forest cover has decreased unevenly throughout the region, a puzzle that has motivated recent research on Brazil (Assunção & Rocha, 2019; Pailler, 2018; Ruggiero, Pfaff, Nichols, Rosa, & Metzger, 2021). Moreover, environmental enforcement can provide something that most other policies cannot: an objective, state-independent measure of success. Using NASA's Landsat data between 2000-2019, I can measure whether enforcement actions in the past have been successful without having to depend on the state's own reports.

3.1 The Brazilian environmental enforcement framework.

At the heart of Brazil's environmental enforcement framework are two agencies under the Ministry of the Environment: the *Instituto Brasileiro do Meio Ambiente e dos Recursos Naturais Renováveis*—IBAMA, in charge of general environmental enforcement—and the *Instituto Chico Mendes de Conservação da Biodiversidade*—ICMBio, in charge of managing national parks. In this paper I focus on IBAMA, as it has a wider scope and jurisdiction over both indigenous and non-indigenous land (Reuters, 2021). In the process of monitoring for deforestation, IBAMA receives help from Brazil's space research agency, the *Instituto Nacional de Pesquisas Espaciais*. INPE sends IBAMA daily reports of potential hotspots, which are based on near real-time analysis of satellite imagery (CIFOR, 2013). Since the two agencies began collaborating, these alerts have fallen under the umbrella of two subsequent programs: DETER-1 and DETER-2, both of which have relied on a combination of US and Brazilian satellites (Assunção, Gandour, & Rocha, 2023). Once IBAMA receives the information from INPE, the agency chooses specific priority locations to send strike teams that are transported in a combination of land, river, and airborne operations (Jackson, 2016). Said operations are regularly undertaken jointly with either local law enforcement, the *Fundação Nacional dos Povos Indígenas*—FUNAI, in charge of indigenous affairs—and/or the armed forces. Once the site is located, IBAMA officers usually torch equipment, issue fines to anyone found responsible, and embargo the farmland of the culprits (Reuters, 2016). In the aftermath of said operations, municipalities with a high number of hotspots can be blacklisted, and individuals can see their access to credit blocked. Taken together, these measures represent one of the toughest efforts to punish offenders in the region (Tacconi et al., 2019).

Even with this sophisticated framework, anti-deforestation efforts in the Amazon face severe administrative constraints, in line with the scope conditions laid out in Section 2.2. First, due to the nature of the activity itself. Considering the size of the Brazilian Amazon—equivalent to the area of India—enforcement of any kind will inevitably be challenging. As of December 2023 the agency employs 4,785 civil servants (Portal da Transparencia, 2023), which, considering the size of the region, would amount to around 1,048 km^2 per employee. A former enforcement director wondered how he was supposed to protect every biome in Brazil with "Brancaleone's army" (El País, 2016), a reference to a badly-equipped force faced with great challenges. Furthermore, given that it is a civilian agency, IBAMA cannot start criminal investigations on its own, but rather needs to coordinate with the Federal Police to do so (IBAMA, 2017e). The size of the region combined with the logistics of coordinating with other agencies—ICMBio if the action lies within national parks, FUNAI if it lies within indigenous lands—can lead to delayed responses, which gives time for illegal loggers to withdraw from recently cleared areas. And second, anti-deforestation policies also face administrative constraints due to resistance from part of the population. As I will explain in Section 3.3, environmental rules in the Brazilian Amazon are contentious (Thaler, 2017). Enforcement has sparked multiple acts of violence, which have ranged from widespread protests to the destruction of IBAMA and ICMBio offices (IBAMA, 2017d). Furthermore, recent work has linked attempts to limit logging to increases in overall violence (Chimeli & Soares, 2017).

3.2 Cracking down: the *Lista de Municípios Prioritários.*

The main instrument through which municipalities have been sanctioned in Brazil has been the *Lista de Municípios Prioritários*,[2] or LMP. A program originated from a presidential decree in 2007, its main objective has been to drive down deforestation rates across the *Amazônia Legal*, or the socio-environmental region that includes the Brazilian Amazon.[3] The main official criteria for assigning a municipality to the LMP has been threefold: historical

[2] Priority Municipalities List.

[3] An attractive alternative to studying the Amazônia Legal is to subset the area and focus on the Brazilian section of the Amazon Biome, given that Amazônia Legal also includes two other biomes. While including said areas could distort the results, because the Brazilian state considers the Amazon region to coincide with the Amazônia Legal, policies aimed at curtailing deforestation apply to the entire region. Following other similar studies (Assunção & Rocha, 2019; dos Santos Massoca & Brondízio, 2022; Slough & Urpelainen, 2019) I've decided to focus on the Amazônia Legal.

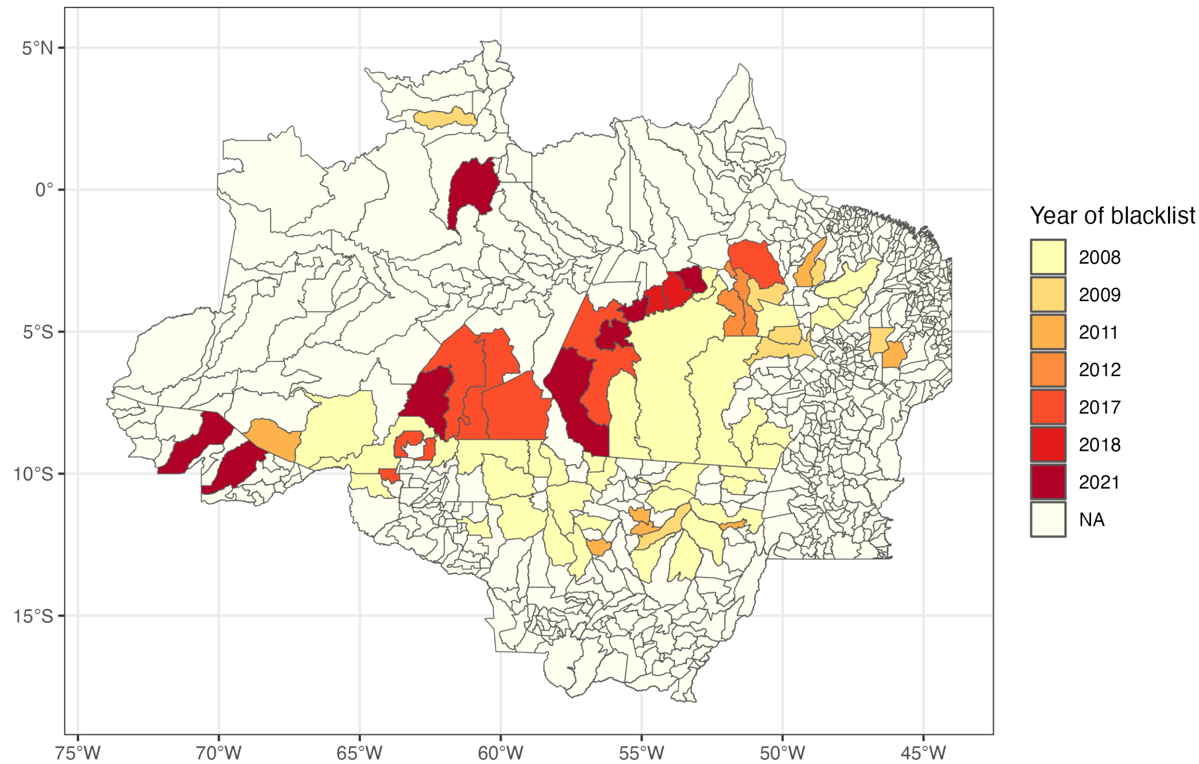

Figure 5: *timing of the LMP treatment for municipalities in the Amazônia Legal.*

forest loss; forest loss in the last three years; and the rate of increase in recent forest loss (Assunção & Rocha, 2019; dos Santos Massoca & Brondízio, 2022). The data used for this determination, however, is not easily available.[4] Since the first cohort of 36 municipalities blacklisted in 2008, over the last 15 years the program has expanded at irregular intervals to include a total of 70 municipalities,[5] which are shown in Figure 5. Table 10 in Appendix 2 includes a detailed list of the municipalities that have received the treatment.

The inclusion of a municipality into the LMP by the federal government entailed three distinct measures (Slough & Urpelainen, 2019). First, blacklisted municipalities saw an increase in the amount of resources devoted by IBAMA to enforcing forestry regulations. This meant both increases in the budget allocated to said tasks, as well as the number

[4]Officially, PRODES data produced by the INPE is used by the Brazilian government as a basis for policy; however, this data is only published aggregated at the regional level, not the municipality level.

[5]Even though it is technically possible for municipalities to be removed from the blacklist, I conceptualize the treatment as irreversible. Only five out of the seventy municipalities were ever removed from the list, and only one was removed before 2019—the year that marks the end of this study.

of agents dedicated to routine checks and non-routine operations. Second, teams at INPE devoted more time and efforts to monitoring tasks, meaning that deforestation events in blacklisted municipalities tended to be more likely to be spotted. And third, the program also included provisions meant to incentivize land registration, as illegal land titles were rampant and were believed to be a significant driver of forest loss. As Slough and Urpelainen (2019) explain, "...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" (p. 9). The LMP, as a result, presents an opportunity to measure how the effects of crackdowns vary depending on the context. The treatment itself—being blacklisted—is assigned by the federal government,[6] and cannot formally be opted-out. However, changes in forest cover—the key criteria for assigning the treatment—are not randomly spread across the territory, as Figure 4 shows. Nevertheless, the existence of clear criteria for inclusion allows for credible "as-if" randomness, given that I can use them as control variables.

3.3 Co-producing environmental enforcement in Brazil.

Preferences around environmental enforcement vary greatly depending on the actor. Individuals whose livelihood depends on agriculture, for instance, will tend to conceive environmental regulations as imposed from above. In contrast, groups that rely on the existence of the rainforest for their everyday life will tend to view enforcement as a public good that the state should provide. In line with the literature, I situate most indigenous communities in the Brazilian Amazon within the latter camp (Baragwanath & Bayi, 2020). These groups tend to have a vested interest in forest conservation, as this activity represents most of their income thanks to tourism and federal transfers (Cunha, Neto, & Morsello, 2022; Lima & Weiler, 2015; Posey, 1985; Ros-Tonen & Werneck, 2009). In contrast, non-indigenous communities in the Amazon tend to live off agriculture (Pereira, de Santana Ribeiro, da Silva Freitas, &

[6] According to official records, 18 municipalities that were originally included in the LMP appear to have been later placed on a slightly less exacting regime. What this entails is not clear in the documents, soI do not distinguish between the two.

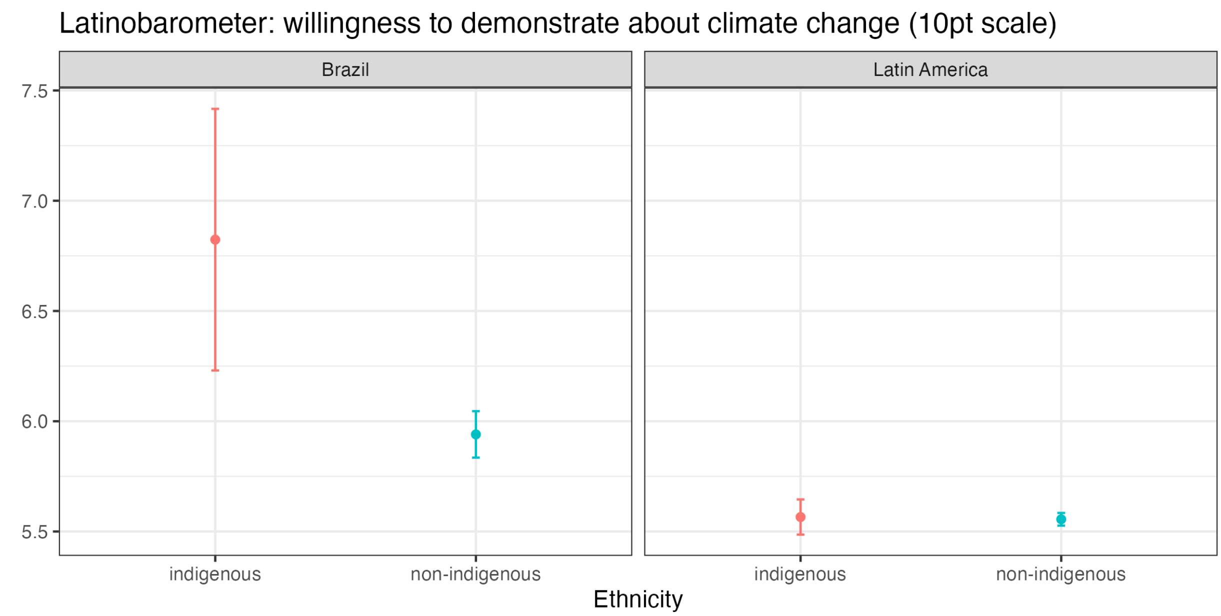

Figure 6: *ethnicity and probability of participation in protests about climate change.*

de Barros Pereira, 2020; Toni, 2003). In line with what I proposed in the theoretical section, this would suggest that indigenous communities should be particularly susceptible to the costs of illegal logging, which is backed by journalistic reports (The New York Times, 2023). I will now attempt at substantiating these claims.

There are no systematic studies of public opinion on environmental regulations that measure differences in preferences between indigenous and non-indigenous individuals in the region. A coarse approximation is provided by a 2020 Latinobarómetro poll, which asked individuals how likely they were to participate in protests related to climate change (Latinobarómetro, 2020). As Figure 6 shows, in contrast to the rest of the region, respondents in Brazil that identify as indigenous are significantly more likely to participate in such protests than other citizens. Furthermore, recent scholarship has shown that areas where indigenous people have been granted collective ownership rights have displayed higher forest cover retention than the rest of the Brazilian Amazon (Baragwanath & Bayi, 2020). These findings are consistent with the idea that preferences towards environmental enforcement vary. I do not claim that all indigenous individuals abstain from deforestation, or that there are no pro-conservation, non-indigenous groups. Rather, the focus is placed on indigenous communities because said groups experience concentrated costs and group cohesion in a way that alternative pro-conservation groups do not.

For indigenous groups to collaborate with law enforcement—what we could call an extreme case of "quasi-voluntary compliance" (Levi, 2008)—not only do the former have to be convinced that the public good being provided is useful, but a prior history of goodwill needs to exist. In line with this, over the years IBAMA officials have reached out to indigenous communities in a systematic fashion, either to help train on forest fire prevention (IBAMA, 2018d, 2019c, 2020a), by coordinating elaborate schemes to return wildlife to indigenous areas (IBAMA, 2019b), or by helping map out indigenous lands (Instituto Socioambiental, 2005). Another trust-building strategy has been the donation of seized timber by IBAMA to indigenous communities to fund improvement programs (IBAMA, 2015a, 2017b, 2017c, 2018b), as well as confiscated goods such as wild fish (IBAMA, 2018b, 2019a) and other supplies taken from trespassers (The New York Times, 2023). Luciano Evaristo, a former enforcement director at IBAMA, was emblematic of this approach, as he received indigenous leaders in his office in Brasilia multiple times and traveled regularly to remote communities (El País, 2016; IBAMA, 2015b; OEco, 2020). In this, IBAMA seems to resemble other examples of bureaucracies actively seeking autonomy by forming coalitions with civil society groups (Carpenter, 2002).

Finally, it is important to highlight specific instances of collaboration between IBAMA officials and indigenous tribes. Among the reports that deal with this topic, an important element is how indigenous communities possess on-the-ground knowledge that the agency lacks, as well as being present in many parts of the territory. As Evaristo put it in an interview, "(...) the indigenous people are the eyes and ears of IBAMA" (Reuters, 2016), meaning that they are key at identifying deforestation hotspots and helping target enforcement operations. The importance of indigenous groups in identifying deforestation hotspots has been featured in many accounts (El País, 2016; IBAMA, 2016a, 2016b; ICMBio, 2018;

Jornal da Record, 2016; Repórter Brasil, 2020). Moreover, the agency has reported how in some cases loggers selectively cut around large trees in order to avoid detection from satellites, which stresses the importance of local informants even more (IBAMA, 2016d). Beyond sounding the alarm, indigenous groups have also collaborated in other stages of the enforcement process. According to some reports indigenous groups have helped locate and search illegal camps (IBAMA, 2016d; The New York Times, 2023), pointed to the location of machinery and gold extracting boats that need to be torched (IBAMA, 2018c, 2020b), provided information regarding who is being affected by the illegal activity (El País, 2016; IBAMA, 2016c, 2017a; ICMBio, 2018; Repórter Brasil, 2020), and helped monitor the aftermath of IBAMA's operations (El País, 2016; IBAMA, 2015a; Reuters, 2017). In that way, it would appear that it is not only practical knowledge that communities offer, but also some degree of technical expertise as well.

Indigenous communities seem to have direct communication lines with some enforcement officials. For example, there has been mentions of radios being distributed by IBAMA agents to local tribes to expedite reports of hotspots (IBAMA, 2016d; Reuters, 2016), as well as accounts of WhatsApp groups between indigenous leaders and high-ranking IBAMA officials (El País, 2016). At times, however, the line between communication and open pressure appears to be unclear. At several stages indigenous activists have occupied IBAMA offices when they thought they were not doing enough (Globo, 2015), have traveled to the capital to exert direct pressure on the agency (IBAMA, 2016c), and directly called on the president to act (Globo, 2023). Furthermore, collaboration has not precluded independent action on the part of indigenous communities, who in some cases have formed self-defense groups (CIR, 2019; Instituto Kabu, 2023). However, said organizations have also been shown to cooperate with IBAMA (El País, 2016; IBAMA, 2020b), which underlines that mobilizing and collaborating are not incompatible.

The question remains: can collaboration between state officials and indigenous groups be measured in a systematic way? Considering the lack of official data, press releases pub-

Table 1: *prevalence of collaboration depends on indigenous status.*

	Total press releases	Instances of collaboration	Instances of no collaboration
Non-indigenous	2187	416	1771
Indigenous	1186	469	717

lished by Brazilian agencies involved in enforcing environmental regulations offer a tentative opportunity to test for this precondition. I focus on reports that discuss deforestation in the region. After manually coding a subset of the data I train a supervised text classification model to identify instances of collaboration in order to make inferences using the entire sample. I then compare the prevalence of collaboration when discussing indigenous lands with the likelihood of collaboration elsewhere. If, for instance, the same language was used in both instances, it would undermine my claim that IBAMA officials cooperate with local communities in indigenous lands at a higher rate than the rest of the region. Details regarding this procedure are discussed in Appendix 1. Table 1 presents the main results, which seem to suggest that instances of collaboration are significantly more likely in indigenous areas. An odds ratio test reveals a positive and statistically significant relation: reports are almost three times more likely to report collaboration inside indigenous lands than outside of them. Moreover, as explained in Appendix 1 this result holds even when comparing between indigenous and non-indigenous organizations, which highlights how the identity of the potential partner matters. Thus, there is some preliminary evidence that state officials are more likely to cooperate with local groups when enforcing anti-deforestation regulations in indigenous lands than outside of them.

4 Empirical analysis: varying effects of crackdowns.

In summary, the main argument of this paper is that under administrative constraints crackdowns will have heterogeneous effects, with the increase in state resources being most effective in areas where agents have local allies to collaborate with. This section will provide evidence that suggests that in Brazil, the LMP crackdown has lead a to divergence in forest cover between indigenous and non-indigenous lands within treated municipalities. Furthermore, I illustrate that the results are not only driven by worsening conditions in treated non-indigenous areas, but also by improvements in indigenous lands.

4.1 Main variables and descriptive statistics.

Consistent with previous sections, a municipality will be considered "treated" if it has been assigned to the blacklist, and the treatment is coded for the year that the LMP was assigned.[7] In order to measure indigenous presence I use indigenous lands as a proxy, based on data published by INPE (2022). Fine-grained information on the distribution of communities within the Amazon does not exist, and other potential proxies such as census data should be regarded as suspect due to the limitations of state-collected information in such a remote area. Thus, the spatial distribution of indigenous lands is used as a best approximation to the concept. Said areas are formally recognized by the Brazilian state, and, as Baragwanath and Bayi (2020) explain, "...once homologated, a territory becomes the permanent possession of its indigenous peoples, no third party can contest its existence, and extractive activities carried out by external actors can only occur after consulting the communities and the National Congress" (p. 20496).

The outcome variable will be forest cover in a given year between 2000 and 2019 (Hansen et al., 2013). According to Wulder, Masek, Cohen, Loveland, and Woodcock (2012), Landsat's data "...is fine enough to detect and monitor anthropogenic changes in land cover, while at the same time having an imaging footprint that is sufficiently large to enable wide-area applications" (p. 3). Forest cover for a given year will be calculated by taking the closed-canopy area of a unit and dividing it by the total area. Due to the difficulty of combining measures of forest gain and forest loss—current growth data is only available at the 10-year

[7] For example, the first cohort is blacklisted on January 25th, 2008; therefore, these municipalities are coded as "treated" for the year 2008. The exception are the cases where units are treated at the tail end of the year, such as the fourth cohort, which was treated on September 28th, 2017. In such cases, the treatment is assigned to start on the next year.

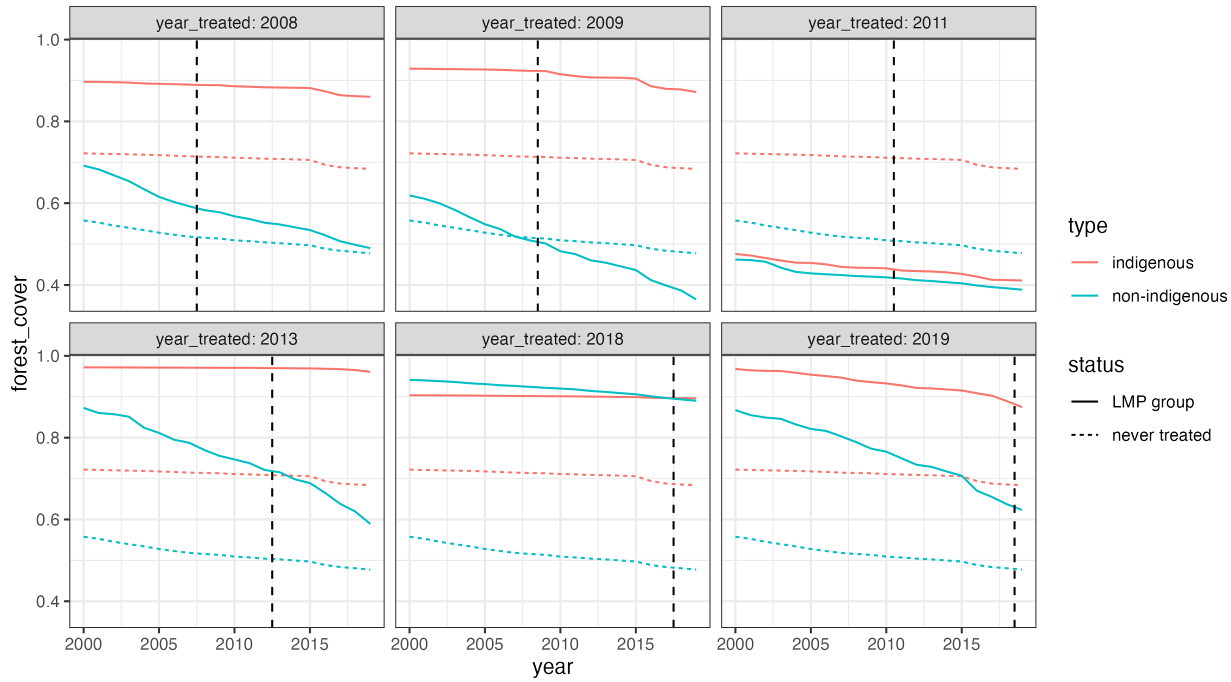

Figure 7: *comparison between treated and control units across time.*

level—forest cover is thus a monotonically-decreasing variable. Another alternative outcome variable is forest cover *loss*, meaning the forest cover at a given time minus the forest cover at the previous year. While this variable is used by part of the literature (Assunção, McMillan, Murphy, & Souza-Rodrigues, 2022; Slough & Urpelainen, 2019), it suffers from several issues. First, it is extremely volatile, considering that deforestation rates can vary widely between years, which could hide a general trend towards smaller tree cover. The variable's mercurial nature makes it susceptible to regression toward the mean, thus exaggerating the short-term effects of targeted enforcement. Second, it is at the mercy of the effects of depletion: high deforestation can be followed by low deforestation not due to a specific policy, but simply because there is no forest cover left to clear, thus overestimating the effect of policies aimed at worst-performing areas. Thus, relative forest cover represents an improvement over forest loss, particularly when controlling for starting cover.

Figure 7 presents a first look at changes in relative forest cover across time, comparing four groups across six cohorts: blacklisted indigenous areas, blacklisted non-indigenous areas, untreated indigenous areas, and untreated non-indigenous areas. A cursory examination of this figure points towards potential issues when estimating the effect of the treatment, which is marked by a vertical dashed line for each cohort: that the pre-treatment trends do not appear to be parallel, which is usually an indication that the treatment itself cannot be said to be the direct cause of the change. The main explanation for this lies within the design of the LMP: because units were targeted due to previously-decreasing forest cover, it is extremely unlikely that in the absence of the treatment they would have followed a similar trend as the untreated units. Thus, in order to arrive at credible causal estimates I need to adopt strategies that account for the determinants of the treatment.

4.2 Estimating the effects of the LMP using triple-differences.

As was explained in the Section 3, an advantage that the LMP offers is that it was targeted to only a subset of municipalities within the Brazilian Amazon, which allows me to isolate the effect of the crackdown from the passage of time. Given the staggered nature of the treatment and the fact that it only affects a subset of the data, an attractive first approach at the problem is to conduct a difference-in-difference analysis incorporating an interaction term depending on indigenous status. However, because these areas might be systematically different in time-invariant ways, it would be possible for the resulting heterogeneous effects to be caused by other factors. An alternative strategy such as a triple-difference design alleviates some of these concerns.

As Olden and Møen (2022) explain, if the traditional difference-in-difference estimator measures changes in the differences between treated and untreated units before and after the treatment, a triple-difference design goes one step further: it measures changes in the difference between the difference of two sub units between treated and untreated units before and after the treatment. In practice, a triple-difference design is simply a difference-in-difference with the difference between the outcomes of the two sub units as the dependent variable (Olden & Møen, 2022). In my case, the unit of analysis are 180 municipalities in the

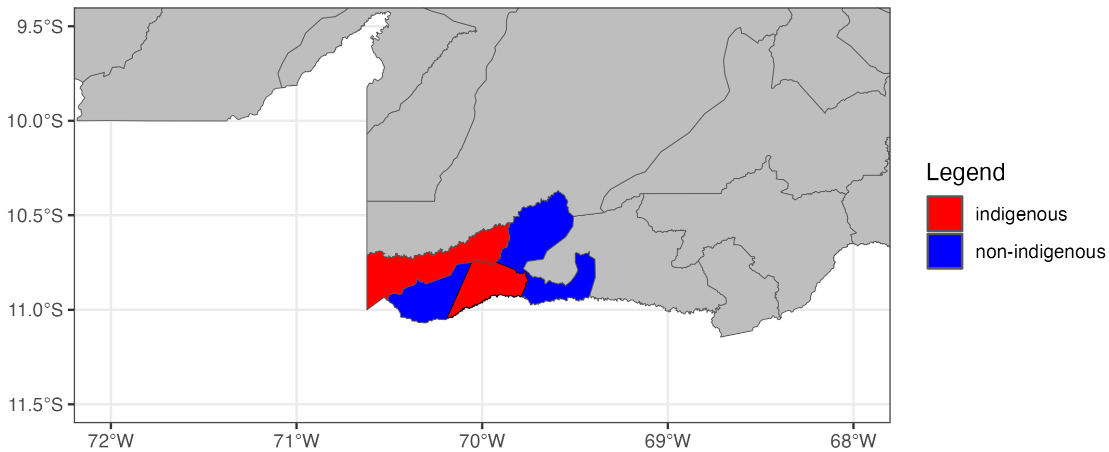

Figure 8: *illustrating the two subunits of analysis using Assis Brasil.*

Brazilian Amazon[8] observed yearly between 2000 and 2019. Even though variation in size between units is a disadvantage at this level of aggregation, the choice is necessary in order to be able to distinguish between the two sub units: indigenous and non-indigenous areas within each municipality. Figure 8 illustrates this through the example of Assis Brasil, in the state of Acre. Said municipality encompasses part of two indigenous territories: Cabeceira do Rio Acre and Mamoadate, which are the homes of the Yaminawa and Machineri people. I am interested in the difference in forest cover between the combined indigenous territory and the rest of the municipality. Thus, the outcome variable for a given year is

$$\tilde{Y} = \frac{\text{forest area}_{indigenous}}{\text{total area}_{indigenous}} - \frac{\text{forest area}_{non-indigenous}}{\text{total area}_{non-indigenous}}$$

or the difference in relative cover between indigenous and non-indigenous territories. For this particular case, the triple-difference design alleviates the threat of time-invariant confounding—i.e. systematic differences between indigenous and non-indigenous areas having diverging effects due to other reasons correlated with indigenous status. By using the difference between the sub units as the outcome we are conditioning on systematic disparities between indigenous and non-indigenous areas that shared across the population of munic-

[8]In order to obtain reasonable results, only municipalities with between 5% and 95% of their territory comprising indigenous lands are included.

ipalities. Indigenous and non-indigenous areas in blacklisted municipalities would need to be systematically different from each other in ways dissimilar to their counterparts in non-blacklisted municipalities for this type of confounding to be a problem. Furthermore, in order to truly threaten the parallel trends assumption these systematic distinctions would need to explain the time-varying divergence in ways unrelated to their indigenous status.

Aside from addressing some concerns around time-invariant confounding, triple-difference designs also rely on a more plausible version of the "parallel trends" assumption, which deals with time-varying confounding. Instead of assuming that the treated and untreated units would have behaved similarly in the absence of the treatment, the only assumption is that in the absence of the treatment the sub units in each of the two groups would have behaved similarly relative to each other. However, after an initial examination of the trends displayed in Figure 7 there appears to be cause for concern about the plausibility of an unconditional parallel trends assumption. The inclusion of control variables can lessen these concerns, considering that they could help explain the timing of the treatment as well as the outcome. As mentioned in Section 3.2, the main criteria for assigning municipalities to the LMP appears to have been substantial increases in the previous rate of deforestation (Slough & Urpelainen, 2019). It is reasonable to expect that past performance also affects current levels of forest cover. Thus, my claim will be that, *conditional on deforestation in the preceding years*, assignment to the LMP was "as-if" random. I measure prior deforestation for a municipality in year t in two ways: as the relative municipal forest loss for $t-1$, and the increase in forest loss from $t-2$ to $t-1$. Due to high correlation between them I estimate separate models. As I mention in Section 6, other idiosyncratic and political determinants of blacklisting should be examined in the future.

While the traditional work-horse approach in the past was to use generalized difference-in-differences with $\tilde{Y}$ as the outcome, in contexts of multi-stage staggered treatments this estimator has been shown to introduce distortions (Goodman-Bacon, 2021). While many estimators that alleviate this concern have been proposed, I rely on the alternative proposed

Table 2: *results of the triple-difference models.*

Dependent Variable:	Difference in forest cover		
Model:	(1)	(2)	(3)
treatment	0.031***	0.0272***	0.0306***
	(0.0067)	(0.0073)	(0.0065)
Covariates			
deforestation increase		Yes	
deforestation lag			Yes
Fit statistics			
Observations	3,600	3,060	3,240

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

by Callaway and Sant'Anna (2021). The CS estimator relies on what the authors call "group-time average treatment effects," meaning that each cohort-year has a specific ATT that can then be aggregated, while also allowing for conditioning on covariates. Table 2 presents the results of three alternative specifications using the CS estimator and bootstrapped standard errors. The estimate itself is an average of the effects across group-times weighted by group size. The main models rely on the "never-treated" as the control group as is convention (Marcus & Sant'Anna, 2021), but similar results are achieved when using the "not-yet-treated," as Table 11 in Appendix 2 shows.

The models included in Table 2 suggest that the treatment led to a divergence between indigenous and non-indigenous areas of the treated municipalities. Before introducing any controls, we can observe that within treated municipalities indigenous lands managed to retain an average of 3.1% more forest cover than non-indigenous lands when compared to untreated municipalities. After conditioning on the main determinants of the treatment said estimate decreases to 2.7% and 3.0%. All estimates are statistically significant at the traditional thresholds. Moreover, as Figure 9 illustrates, the effect of the treatment builds over time, with the municipalities that were treated early displaying a difference in forest cover between indigenous and non-indigenous lands of 7% when compared to untreated municipalities. The aforementioned figure corresponds to the model with the "increase in deforestation" control, but Figure 11 in Appendix 2 shows that similar results appear when

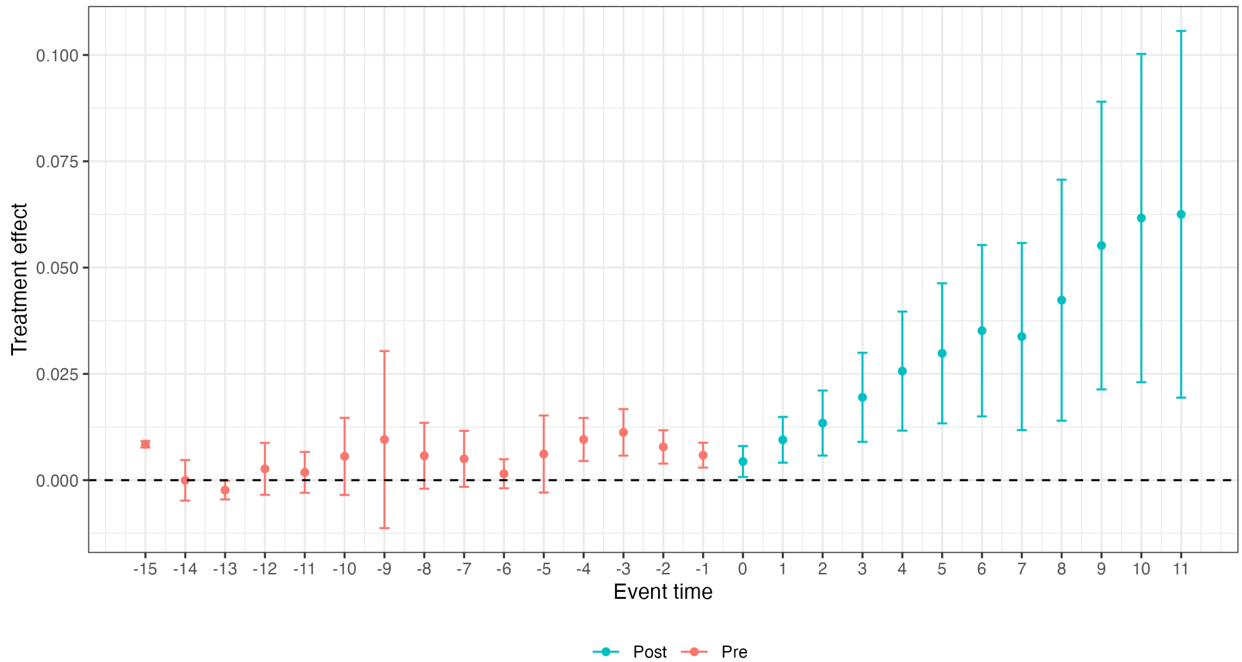

Figure 9: *conditional divergence between subunits over time.*

I use the "lagged deforestation" control.

Figure 9 allows me to discuss the plausibility of the parallel trends assumption. While some divergence in the few years prior to the treatment is present—which should be working against my estimates—it pales in comparison to the divergence that takes place after the treatment. In practice, this means that the current pre-treatment divergence present in Figure 9 is not enough to cast doubt on the causal quantity identified. Moreover, to illustrate that the design is robust to the specific estimation procedure, I rely on Imai, Kim, and Wang (2021)'s panel matching approach, the results of which are included in Figure 12 in Appendix 2. Both the estimates and their significance remain relatively unchanged in comparison to the ones relying on CS estimator. Finally, considering that recent approaches do not account for spatial autocorrelation, I rely on a traditional generalized difference-in-difference estimator with Conley standard errors to test its impact on my estimates. As Table 12 in Appendix 2 shows, both estimates and standard errors remain stable.

In summary, there appears to be evidence in favor of an effect of the LMP treatment on forest cover. In particular, the treatment seems to have widened the gap between indigenous and non-indigenous lands among the treated municipalities in comparison to the untreated ones. Conditioning on the main determinants of the treatment appears to have yielded confidence that the main assumptions of difference-in-difference designs hold—or at least that the effect is strong enough to alleviate some concerns. However, the question remains: is the increase in the gap in forest cover a result of an improvement in indigenous lands? Or, rather, is this the result of worsening conditions in treated non-indigenous lands? Because of its reliance on differences among sub units, the previous design cannot answer this question, and an alternative must be considered.

4.3 Disentangling heterogenous treatment effects.

In order to examine what is driving the divergence identified in the previous section it is necessary to study the heterogeneous effects of the treatment when comparing to the universe of untreated units, indigenous and non-indigenous alike. While previous problems with this approach still hold—that by interacting a time-varying treatment with time-invariant characteristics I open the door for time-invariant confounding—the results from the previous approach assuage some of these concerns. Problems identified with the generalized difference-in-difference estimator notwithstanding, interacting the treatment indicator with a time-invariant moderator provides a useful approximation, as the Callaway and Sant'Anna (2021) approach does not yet accomodate this setup.

For this approach it is necessary to have a small enough unit of analysis that can be wholly assigned into either indigenous *or* non-indigenous lands. Thus, I divide the territory of the Amazônia Legal using a hexagonal grid into around 30,000 units of approximately 166 km^2 in size.[9] Considering the disparity between the sizes of municipalities in the Brazilian Amazon—where some coincide with city limits while others are country-sized—using an

[9]There are three regular polygons that can be used to tesselate an area: triangles, squares, and hexagons. Of the three, hexagons have the lowest perimeter-to-area ratio, which minimizes distortions introduced by these artificial borders. Furthermore, honeycomb grids are better able to account for curved boundaries, making them particularly well suited to account for both topographical features and irregular borders.

Table 3: *two-way fixed effects with a time-invariant moderator.*

Dependent Variable:	Hexagon forest cover		
Model:	(1)	(2)	(3)
Variables			
treatment	-0.0592***	-0.0561***	-0.0147***
	(0.0082)	(0.0078)	(0.0033)
treatment × indigenous_land	0.0669***	0.0638***	0.0481***
	(0.0069)	(0.0065)	(0.0084)
Covariates			
Forest loss (lagged)		Yes	Yes
Municipal loss (lagged)			Yes
Municipal cover (lagged)			Yes
Fixed-effects			
hex_id	Yes	Yes	Yes
year	Yes	Yes	Yes
Fit statistics			
Observations	501,049	474,678	474,678

Clustered (hex_id & id_municipio) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

artificial grid means that I am comparing equivalent units. Still, I cluster the standard errors at the hexagon and municipality levels. An additional advantage of this design is that it takes into account the entire region, rather than focusing on municipalities that have both indigenous and non-indigenous lands. The outcome variable is a hexagon's forest cover, measured as forest area over total area. Finally, considering the main criteria for assignment into the LMP is previous performance I include a series of related metrics at the municipal and hexagon level.

Table 3 presents the results of the TFWE model with heterogeneous treatment effects resulting from indigenous status. Considering the universe of non-treated hexagons as a baseline, assignment into the LMP appears to have had different effects depending on whether a hexagon lies within indigenous lands or not according to the fully-specified model. While in the former the treatment is associated with 3% retention of forest cover, in the latter group assignment into the LMP appears to be associated with a 1% *decrease* in forest cover. Returning to the question asked at the end of the previous subsection, the increase in the gap between indigenous and non-indigenous areas appears to be the result of both an improvement in indigenous areas and worsening conditions in non-indigenous areas. As I will explain in Section 6, these findings are consistent with recent discussions of displacement of deforestation to areas that are less monitored (Eisenbarth, Graham, & Rigterink, 2021; Slough & Urpelainen, 2019). It is important to note that while spillover from treated indigenous areas to treated non-indigenous areas would not threaten SUTVA, *spillover from the former into non-treated areas would*. This potential threat should be further analyzed in future research.

5 Mechanism: why heterogenous treatment effects?

Why do we observe a divergence as the result of the treatment? In this section I will provide evidence in favor of the mechanism laid out in Section 2.1. The effect of crackdowns on the probability of enforcement should be greater in areas with allied groups, which in turn should shape future criminal behavior. To this end I leverage records of environmental fines issued on-site by IBAMA (IBAMA, 2022), which represent instances of state enforcement against deforestation and have been used as such by recent studies (Assunção et al., 2023).[10] First, I will show how the number of fines issued varies as a function of the LMP and indigenous presence. Then, I will present evidence that suggests that the record of fines influences future deforestation.

5.1 Indigenous presence moderates the effect on enforcement.

As Figure 2 illustrates, the effect of a crackdown on the probability of punishment should be moderated by the presence of local allies. While the ideal procedure to test this would be to repeat the triple-difference design implemented in Section 4.1 with the number of fines and the amount fined as the new outcomes, data limitations make this impossible, as most

[10] I only include fines issued by IBAMA that are related to deforestation, and omit other sanctions such as attempts against wildlife or incorrect transportation of dangerous chemicals.

environmental fines lack coordinates before the mid 2010s, several years after the LMP was created. In practice, this means that I am unable to measure whether a fine was issued inside indigenous lands before that date. However, municipality information is available, which allows me to test whether the LMP lead to an increase in fines *at all*. As Table 13 in Appendix 2 shows, there is a positive and significant effect of the LMP on the number of fines and the amount fined at a municipal level. This is a useful start.

In order to deal with the previous limitation, I pivot from aggregate analysis to individual instances of deforestation, which has the added benefit of being free from potential aggregation bias. My main units of analysis, then, are the more than 280,000 deforestation events between mid-2016 and late 2022 reported by the DETER-2 program (INPE, 2022), which not only records the precise location and size of a deforestation event, but also its date. Furthermore, as Table 14 in Appendix 2 illustrates, since the mid-2010s almost all of the fines have detailed location data that can be used to mark the specific instance of enforcement and the amount fined. This fine-grained data is needed to correctly identify whether a deforestation event was followed by enforcement. In order to measure this I draw a 10km buffer around the centroid of each of the events, and record whether there were any fines issued by IBAMA within that area over the next three months. The size of the buffer is intended to account for occasional imprecise records of the location, and the length of the enforcement window is intended to account for delayed responses. Thus, the outcome of interest is the *rate of enforcement*, measured as the number of fines issued within the buffer in that time window. Figure 10 shows the spatial distribution of deforestation events in 2017 alongside fines issued by IBAMA that same year.

I rely on Poisson regressions to accommodate the count nature of the outcome. The main predictor of rate of enforcement will be the interaction between the LMP crackdown and the presence of indigenous communities. The former is coded as positive depending on whether the deforestation event takes place within a municipality that has been assigned to the LMP. As with previous models, I code "indigenous presence" as positive based on

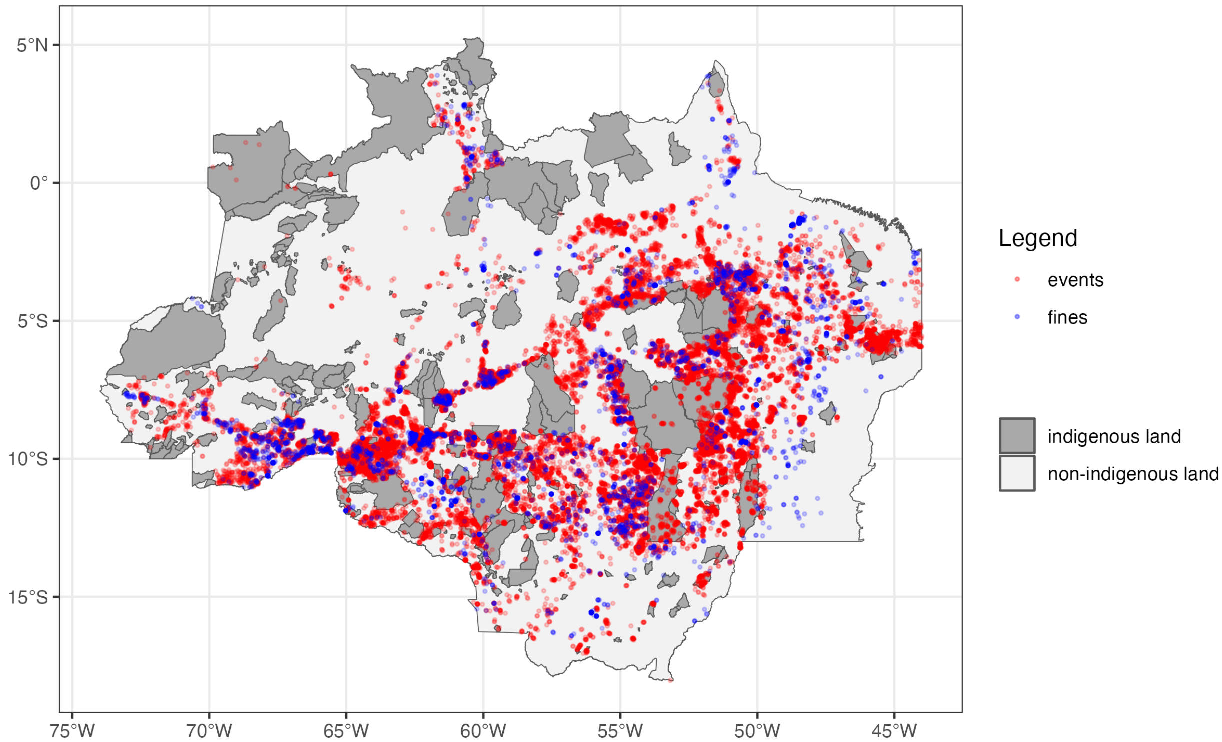

Figure 10: *spatial distribution of indigenous lands alongside 2017 events and fines.*

whether the deforestation event takes place within indigenous lands (INPE, 2022).

Table 4 presents the results of said models. I cluster the standard errors around municipalities and years. Among the controls included in some of the models are the coordinates of the event, a linear time trend, the deforested area, the number of events that took place in a 20km radius during the previous 4 months, and the as-the-crow-flies distance to the nearest IBAMA office. The results suggests that the effect of the LMP crackdown is moderated by the presence of local indigenous communities, and this is the case across specifications. Events that take place in LMP municipalities are issued two times more fines than ones outside of them. Additionally, this rate more than doubles in indigenous lands, with events that take place inside both blacklisted areas and indigenous lands being issued almost five times more fines than those outside of both.

In order to test whether the results are driven by the choice of the model, I replicate the three models using logistic regressions with a dichotomized version of the outcome—whether

Table 4: *determinants of the rate of enforcement.*

Dependent Variable:	Number of fines		
Model:	(1)	(2)	(3)
Variables			
treatment	0.7314***	1.019***	0.9093***
	(0.1712)	(0.1489)	(0.1640)
treatment × indigenous_land	0.8438**	0.7428**	0.5642*
	(0.3799)	(0.3602)	(0.2884)
Covariates			
IBAMA distance		Yes	Yes
year		Yes	Yes
coordinates		Yes	Yes
previous events			Yes
area			Yes
Fit statistics			
Observations	292,429	292,429	267,599

Clustered (id_municipio & year) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

a fine is issued or not. Additionally, I also replicate the models using linear regressions and the amount fined as the outcome. I present the results of these new models in Table 15 in Appendix 2. The original finding holds for the case of the binary outcome models. However, the opposite is true for the models with the amount fined as the outcome, which suggests that while the presence of indigenous communities moderates the effect on the rate of enforcement, the same is not true for its intensity. Finally, Table 16 in Appendix 2 shows that the choice of the buffer and window does not affect the sign of the estimates. Additionally, said table also includes two versions of the simple model with Conley standard errors to account for potential spatial autocorrelation. Both display similar results as the original models.

5.2 Enforcement affects future deforestation.

In order to complete the causal chain illustrated in Figure 2 I need to show that the rate of enforcement—which is influenced by the interaction between the LMP and indigenous presence—has an effect on criminal behavior. Considering that reports show that environ-

Table 5: *effectiveness of enforcement rate.*

Dependent Variable:	Deforested area (m^2)					
Model:	(1)	(2)	(3)	(4)	(5)	(6)
lag number of fines	-13,430.5		-10,597.9		-8,848.3	
	(9,349.8)		(7,999.3)		(7,464.8)	
lag total fined		-52.34***		-52.23**		-51.66**
		(19.39)		(21.97)		(21.73)
Covariates						
lag deforested area			Yes	Yes	Yes	Yes
year			Yes	Yes		
Fixed-effects						
hex_id	Yes	Yes	Yes	Yes	Yes	Yes
year					Yes	Yes
Fit statistics						
Observations	184,597	184,597	158,226	158,226	158,226	158,226

Clustered (id_municipio & hex_id) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

mental fines in the Brazilian Amazon tend to have weak repercussions (El País, 2016), it is possible that the findings of the previous section are not translated into lower crime rates. In that case, the diverging effects of the crackdown on forest cover identified in Section 4 would not necessarily be due to differences in enforcement rates, which would undermine my conclusions. Thus, I need to asses the effectiveness of environmental enforcement. To do so I return to the hexagonal grid as the unit of analysis, and implement a series of regular two-way fixed effects models with the main treatment being either the number of fines or the total amount fined in the previous year. The outcome in this case is the sum of the area from deforestation events that took place within a hexagon in a given year. I include the previous crime rate as a control due to its status as a time-varying confounder.

Table 5 presents the results, which mostly conform to expectations. In all four models the lagged measure of the enforcement rate is negatively correlated with deforested area, which supports my theory. Similar results are obtained by using Conley standard errors, as Table 17 in Appendix 2 shows. While only some estimates are statistically significant, these findings are strengthened by recent quasi-experimental evidence of environmental fines being effective at curbing deforestation in Brazil (Assunção et al., 2023).

6 Discussion and contributions.

In summary, my central claim in this paper is that, in contexts of administrative constraints, crackdowns will be most effective wherever state agents are met with willing local partners. I argue that this is the result of changes in the behavior of would-be law-breakers, who will perceive that the probability of punishment in areas where law enforcement is co-produced with local partners to be the highest. By focusing on subnational variation of anti-deforestation enforcement in the Brazilian Amazon I show how state-society relations can shape the natural environment. I first show the results of a text classification model which suggests that state agents and indigenous communities systematically collaborate in enforcing environmental regulations. Then, through a combination of triple-difference and two-way fixed effects models I provide evidence that indigenous and non-indigenous lands diverged in terms of forest cover as a result of an environmental crackdown in the mid-2000. This divergence was the combination of improvements within indigenous lands and worsening conditions outside of them. Finally, I support the proposed mechanism by showing that environmental crimes are punished at higher rates in indigenous lands under strict monitoring regimes, and that in turn, this pattern of enforcement shapes future deforestation patterns.

The findings provided in this paper seem to support the recent literature on the importance of community governance in limiting deforestation (Baragwanath & Bayi, 2020; Gulzar et al., 2023), while also providing a counterpoint to recent work that questions the effect of citizen input (Blair et al., 2021; Christensen et al., 2021; Slough et al., 2021). Furthermore, this paper also touches on state building. The expansion of state authority over environmental enforcement in the Brazilian Amazon was seen by many as officials encroaching on the rights of individuals to earn a living (Thaler, 2017). What explains successful enforcement in this context is directly related to classical discussions about why and when states take on new responsibilities and carry them out successfully. Within this framework, the literature on state building has not paid enough attention to relationships of co-production between state officials and citizens. Said relationships lessen the principal-agent dilemma not by leading to "principled agents"—as Rueschemeyer (2005) proposes—but instead by creating "agentialled principals": rather than by internalizing new rules, provision is ensured by a willing counterpart that can cooperate with state officials to implement policies. Furthermore, co-production can be said to extend the infrastructural power of the state by deputizing citizens and turning them into quasi-state agents (for a similar discussion around indigenous autonomy, see McMurry, 2021).

It is important to recognize a seemingly-counterintuitive finding: how is it that ethnic differences between state agents and communities do not seem to harm coordination in anti-deforestation enforcement in Brazil, as (Alesina et al., 2019) have shown for other contexts? This fact is particularly perplexing considering the contentious relationships between many indigenous groups and the state in the Americas (Yashar, 2005). The specifics around indigenous identity in Brazil might answer this dilemma. In contrast to other civil society groups, indigenous communities are formally recognized by the Constitution to have collective rights. Thus, the Brazilian state deals with indigenous communities as concrete actors, and subsequent governments are bound by the Constitution to protect their way of life (Baragwanath & Bayi, 2020). FUNAI as an autonomous federal institution plays a significant role here, considering that its sole purpose is to ensure that the rights of indigenous people are being respected, even in the face of considerable societal resistance (The New York Times, 2022). By keeping constant lines of communication with local tribes, the agency serves as an intermediary with IBAMA and has reported deforestation within indigenous lands in several instances (IBAMA, 2018a). Some states also have their own indigenous affairs agencies, which in some cases include direct participation by tribes themselves (CPI-Acre, 2022). Thus, as in the case of India (Gulzar et al., 2023), the recognition of diversity seems to aid in curbing forest cover loss in Brazil.

Another surprising finding is that, by and large, an increase in state presence without a willing partner in civil society does not seem to lead to better outcomes, but rather *worse* ones. Alongside other authors (Eisenbarth et al., 2021; Slough & Urpelainen, 2019), I identify potential spillover effects from areas that are truly treated—i.e., that undergo a crackdown co-enforced by local allies—to areas where individuals do not fear punishment, even when they are supposedly being monitored. As Slough and Urpelainen (2019) explain, the complex nature of the issue at hand also implies that punishment-only measures will never truly succeed at stamping out deforestation. Consequently, the results discussed here suggest that caution is advised when considering the LMP crackdown as an unequivocal success, and stress the importance of outreach on the part of bureaucrats in order to cultivate trust-based relationships with communities. As a result, this paper presents a cautionary tale: enforcement is not just about brute force or more state resources; how officials interact with civil society matters.

I identify three potential strands of future research. First, my analysis rests on the assumption that indigenous communities favor curbing deforestation at higher rates than the rest of the population in the Amazon. While I believe this assumption is reasonable, it should nonetheless be supported by more empirical evidence. Studying variations in support would help identify who to rely on in the quest to conserve rainforests, while also illustrating how indigenous communities are no monoliths. A potential alternative to surveys would be to examine voting patterns at the polling station level, as support for candidates such as Jair Bolsonaro could proxy for preferences toward the environment. Second, the role of politics within this process has not yet been exhausted. Future work should focus on the political determinants of the selection of areas subject to crackdowns, as well as exploring what role local authorities play in this process. And third, a comparative approach is needed with urgency. Recent literature on other states in the Amazon basin point to a lack of cooperation between agents and local communities (Harding et al., 2023; Revelo-Rebolledo, 2019), which could explain why countries such as Colombia or Peru have high relative deforestation rates. By studying variations in co-production across contexts we could help uncover important determinants of deforestation in the Amazon, as well as for other tropical rainforests that display similar issues (The Economist, 2023).

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Supplemental material

Appendix 1: text analysis procedure

As explained in Section 3.3, I analyze official press releases in order to provide systematic evidence that state officials cooperate with local groups at different rates depending on whether an operation takes place within indigenous lands or not. While interviews and other forms of qualitative evidence would be ideally suited for this task, due to resource constraints I resort to analyzing reports published by agencies that engage in environmental enforcement describing routine operations. If my main theory is correct, we should expect the language used in press releases related to activities in indigenous lands to show signs of cooperation with locals, in line with the "fire-alarm" oversight model (McCubbins & Schwartz, 1984). In contrast, in operations unrelated to indigenous areas we should expect language that does not imply cooperation with locals, but rather autonomous action by officers—in line with the "police-patrol" oversight model. For this objective I scraped around 25,000 press releases published by five Brazilian agencies that take part in enforcement against illegal loggers: IBAMA (2016-2023), FUNAI (2009-2023), ICMBio (2011-2022), the Federal Police (2019-2023), and the Ministry of Defense (2006-2023).

I then subset the data to focus exclusively on activity related to deforestation, which I do by selecting press releases that mentioned one of the following terms: *desmatamento* (deforestation), *garimpo* (illegal mining site), *fogo* (fires), *incêndio* (forest fire), *madeira* (timber), and *grilagem* (fake land titles). I also consider variations of these words, like *desmatado* and *garimpeiros*. While terms related to forest fires could seem to be unrelated to illegal logging, in reality a significant part of forest fires in the region are linked to deforestation activity. After subsetting press releases depending on whether they discuss efforts to curb deforestation I classify them into two camps: those related to indigenous areas, which are the ones that mention the words *indígena* or *índio*, and press releases unrelated to indigenous areas. After both procedures I arrive at a total of 3,373 press releases, of which 1,186 deal with indigenous lands.

In order to measure differing rates of cooperation between indigenous and non-indigenous

Table 6: *relation between indigenous status and language in press release.*

Dependent Variables: Model:	Count (1)	Mention (2)	Count (3)	Mention (4)
Indigenous status	0.5755***	0.2474***	0.2899**	0.1710***
	(0.0313)	(0.0174)	(0.0750)	(0.0188)
Fixed-effects				
agency			Yes	Yes
Fit statistics				
Observations	3,373	3,373	3,373	3,373

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

press releases I adopt two approaches. As a first approximation I compiled a small dictionary of words that are usually associated with collaboration, which I gathered after reading part of the press releases: *denúncia* (delation), *cooperação* (cooperation), *apoio* (support), and *coordenação* (coordination). This initial approach is simplistic, but allows me to analyze differences in language patterns between the two types of press releases. Table 6 presents the results of four lineal models intended to measure this correlation. The first two models do not control for agency, while the latter include agency fixed effects. The logic behind agency fixed effects is that some environmental agencies might have more to do with indigenous affairs—like FUNAI—and also display more frequent collaboration-related language. Both in the models with a count outcome variable and with a binary outcome—one mention or more—press releases related to indigenous lands tend to use vocabulary linked to collaboration more than those unrelated to indigenous affairs. In both cases the effect decreases when measuring within-agency variation. Overall, indigenous status is associated with around a 25% increase in the probability of mentioning one of these words. This finding provides initial evidence in favor of systematic collaboration.

The second approach involves training a supervised text classification machine learning model to identify instances of collaboration, based on the manual coding of a subset of press releases. While the dictionary approach can provide initial evidence of differences between press releases, the words listed do not fully capture the nuances of identifying cooperation in real life. Thus, I randomly sample 240 press releases, ensuring a similar number per agency:

IBAMA (48), FUNAI (40), ICMBio (48), the Federal Police (59), and the Ministry of Defense (45). Due to concerns that press releases displaying collaboration might be uncommon I adopt an additional block sampling strategy, where half of the press releases that I code include one of the words in the dictionary listed in the previous paragraph, and half do not. Finally, out of the 240 press releases 96 mention indigenous lands, while 144 do not. As to the coding itself, I adapt McCubbins and Schwartz (1984)'s idea of "fire-alarm" enforcement to collaboration in anti-deforestation efforts. In order for a press release to belong to the former category I focus on mentions of cooperation with local groups, processes of consultation, support from communities, instances of citizen reports, etc. Because I aim at an exhaustive classification scheme every press release that does not include those patterns is assign to the negative category. However, for completeness' sake I follow McCubbins and Schwartz (1984) and consider the archetypal "police-patrol" press release to stress centralized monitoring, instances of targeted surveillance, autonomous operations, a reliance on remote sensing, etc. Table 7 includes examples of phrasing that is suggestive of either types of enforcement.

After manually coding a random sample of 240 press releases, I train two types of classification models: random forests and extreme gradient boosting. In both cases I implement standard pre-processing techniques—such as removing punctuation, symbols, numbers, Portuguese stop words, as well as stemming—in order to simplify and clean the text. I transform the corpus into a document-term matrix, and remove the terms that only appear in a small number of documents. I do not rely on TF-IDF weighting, as it does not improve the accuracy of the models. I perform a ten-fold cross validation procedure in order to minimize overfitting, relying on the caret package for R (Kuhn, 2012). After tuning the models I select the best-performing combination of parameters for both models, which achieve an accuracy of 67.14% (random forest) and 67.5% (extreme gradient boosting). Table 1 in Section 3.3 represents the predictions for the latter model, considering that it has a greater accuracy.

I replicate the dictionary models presented previously, but now with the predicted collaboration using the different supervised models as the outcome. Table 8 presents the results

Table 7: *examples of different types of enforcement.*

Agency	Enforcement	Text
FUNAI	fire-alarm	"...In Xingu National Park, 14 firefighters from Previfogo managed to control the fire. In Kapoto Jarina, 15 indigenous brigade members are fighting the fire in the region. The support of the indigenous brigade members is fundamental for controlling the fires. In addition to knowing the region, they have technical knowledge to fight the flames, says the Interim Territorial Monitoring Coordinator, Thomas Simões..."
FUNAI	fire-alarm	"...These advanced units of the FUNAI have consolidated themselves as important spaces for dialogue and articulation with indigenous peoples, non-indigenous communities in the vicinity, and anthropologists, both on the occasion of training workshops and, on a more daily basis, in meetings to exchange diverse experiences. The training courses and workshops regularly bring together representatives of the Jamamadi, Jarawara, Banawa, Apurinã and Paumari indigenous peoples for ongoing training and reflection on the territory and surveillance work..."
IBAMA	fire-alarm	"...The trees with the highest crowns were preserved so that other species could be cut down without the crime being identified by satellites that detect deforestation. In order to avoid future inspections by Ibama, Antônio José Junqueira Vilela Filho monitored, via satellite, the burnings carried out by the criminal group. The complaint about the gang's actions was made by Kayapó Indians, from the Menkragnoti Indigenous Territory (TI), in Altamira. Using amateur radio communication, the Indians verified that the camps organized by the deforesters were strategically distributed throughout the territory..."
IBAMA	fire-alarm	"...IBAMA is part of a task force to rescue wild animals from the fires in the Pantanal (MT) and to bring food and water to the places affected by the fire. In addition to IBAMA, the following actions are part of the actions: the Chico Mendes Institute for Biodiversity Conservation (ICMBio), the Federal District Institute for the Environment and Water Resources (Ibram-DF)... Civil society organizations also participate in the task force. IBAMA employees are helping with the activities... The actions include rescuing, treating and disposing of the impacted fauna..."
IBAMA	police-patrol	"...The vessel was also carrying 22 cubic meters of coal without documentation and two tracajás that would be consumed by the crew. The ferry's owners were arrested for receiving illegal wood. Fines of BRL 5,000 per animal were also imposed, as the species is endangered. The monitoring of the Amazon River is part of the Arquimedes Operation, which has investigated irregularities in 444 containers loaded with wood in the ports of Manaus.
IBAMA	police-patrol	IBAMA agents inspected 14 enterprises with suspicious commercial activities and identified 165,900 cubic meters of charcoal without legal origin sold to steelmakers in Maranhão in the last three years. This volume is equivalent to the load of at least 1,700 trucks adapted to transport the product. The action was carried out after analysis of data generated by the Forestry Origin Document System (DOF)..."
ICMBio	police-patrol	"...The captured images will be available to ICMBio and other environmental protection agencies. According to the president of ICMBio, Homero Cerqueira, it is necessary to improve land inspection and regularization in the region. "The arc of deforestation is getting closer and closer to conservation areas. We are looking for partnerships, because alone it is very difficult to protect such a large area", said the president of ICMBio..."
PF	police-patrol	"...The work aims to prevent and repress crimes against the environment in the municipalities that make up the district of the city of Altamira in Pará. During the operation, from the 9th to the 11th of May, inspections were carried out in several municipalities, considered critical in deforestation numbers. As a result of the actions, several assets used in crimes were seized: two trucks, a tractor, 900 liters of diesel oil, a satellite radio, which is prohibited, and a firearm. 9.9 cubic meters of wood were also retained..."

of said models, some of which include agency fixed effects. In comparison to the dictionary approach, we see that the estimates have decreased. Now, indigenous status is associated with between a 7% and a 24% increase in the probability of displaying collaboration. Thus it would appear that the dictionary approach was either overestimating the prevalence of collaboration in indigenous lands, or underestimating cooperation with local groups outside of them. As a result, *if we are to believe what is reported in these press releases*, it appears that collaboration between environmental enforcement officials and indigenous communities happens in a systematic fashion.

Table 8: *relation between indigenous status and predicted collaboration.*

Dependent Variable:	Collaboration			
Learning algorithm:	Random forest		Extreme gradient boosting	
Model:	(1)	(2)	(3)	(4)
Indigenous status	0.2486***	0.0855***	0.2052***	0.0700***
	(0.0148)	(0.0192)	(0.0155)	(0.0204)
Fixed-effects				
agency		Yes		Yes
Fit statistics				
Observations	3,373	3,373	3,373	3,373

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

Table 9: *indigenous status vs other civil society organizations.*

Dependent Variable:	Collaboration			
Learning algorithm:	Random forest		Extreme gradient boosting	
Model:	(1)	(2)	(3)	(4)
Indigenous status	0.2696***	0.0759***	0.1958***	0.0493*
	(0.0219)	(0.0269)	(0.0227)	(0.0283)
Fixed-effects				
agency		Yes		Yes
Fit statistics				
Observations	1,812	1,812	1,812	1,812

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

However, it is possible that there is a divergence between what is reported in press releases and reality. A way of partially addressing this is to compare collaboration with indigenous groups with other organized actors. Environmental agencies might be over or under-reporting cooperation with civil society organizations; however, there is little *a priori* motivation for this to vary depending on the type of organization. If, for example, reports related to indigenous communities displayed higher rates of collaboration than reports related to "extractivist" communities—non-indigenous state-sanctioned traditional producers—or than *quilombos*—communities of former slaves—then my findings would be strengthened. To that end, I code a new status variable as positive if there is mention of *indígena* or *índio*, and negative if there is no mention of those words but there is mention of *comunidad* (community), *extrativista*, *quilombo*, *organização* (organization), or *liderança* (leadership).

This entails using the same collaboration score predicted with the classification models, as well as dropping reports that do not deal with any organized actor. Table 9 shows that collaboration remains more prevalent in indigenous settings. This speaks to my background theory as well, as not only are crackdowns more effective where the state meets *potential* allies, but the particular identity of the latter matters.

Appendix 2: additional tables and figures

Table 10: *list and timing of treated municipalities.*

Nº	State	Municipality	Date	Presidency
01	AM	Lábrea	2008/01/25	da Silva
02	MT	Alta Floresta	2008/01/25	da Silva
03	MT	Aripuanã	2008/01/25	da Silva
04	MT	Brasnorte	2008/01/25	da Silva
05	MT	Colniza	2008/01/25	da Silva
06	MT	Confresa	2008/01/25	da Silva
07	MT	Cotriguaçu	2008/01/25	da Silva
08	MT	Gaúcha Do Norte	2008/01/25	da Silva
09	MT	Juara	2008/01/25	da Silva
10	MT	Juína	2008/01/25	da Silva
11	MT	Marcelândia	2008/01/25	da Silva
12	MT	Nova Bandeirantes	2008/01/25	da Silva
13	MT	Nova Maringá	2008/01/25	da Silva
14	MT	Nova Ubiratã	2008/01/25	da Silva
15	MT	Paranaíta	2008/01/25	da Silva
16	MT	Peixoto De Azevedo	2008/01/25	da Silva
17	MT	Porto Dos Gaúchos	2008/01/25	da Silva
18	MT	Querência	2008/01/25	da Silva
19	MT	SF Do Araguaia	2008/01/25	da Silva
20	MT	Vila Rica	2008/01/25	da Silva
21	PA	Altamira	2008/01/25	da Silva
22	PA	Brasil Novo	2008/01/25	da Silva
23	PA	Cumaru Do Norte	2008/01/25	da Silva
24	PA	Dom Eliseu	2008/01/25	da Silva
25	PA	Novo Progresso	2008/01/25	da Silva
26	PA	Novo Repartimento	2008/01/25	da Silva
27	PA	Paragominas	2008/01/25	da Silva
28	PA	Rondon Do Pará	2008/01/25	da Silva
29	PA	SM Das Barreiras	2008/01/25	da Silva
30	PA	Santana Do Araguaia	2008/01/25	da Silva
31	PA	São Félix Do Xingu	2008/01/25	da Silva
32	PA	Ulianópolis	2008/01/25	da Silva
33	RO	Nova Mamoré	2008/01/25	da Silva
34	RO	Porto Velho	2008/01/25	da Silva
35	RO	Machadinho D'oeste	2008/01/25	da Silva

Nº	State	Municipality	Date	Presidency
36	RO	Pimenta Bueno	2008/01/25	da Silva
37	PA	Pacajá	2009/03/24	da Silva
38	PA	Marabá	2009/03/24	da Silva
39	PA	Itupiranga	2009/03/24	da Silva
40	RR	Mucajaí	2009/03/24	da Silva
41	MT	Feliz Natal	2009/03/24	da Silva
42	PA	Tailândia	2009/03/24	da Silva
43	MA	Amarante DM	2009/03/24	da Silva
44	PA	Moju	2011/05/24	Rousseff
45	MA	Grajaú	2011/05/24	Rousseff
46	AM	Boca Do Acre	2011/05/24	Rousseff
47	MT	Alto Boa Vista	2011/05/24	Rousseff
48	MT	Tapurah	2011/05/24	Rousseff
49	MT	Cláudia	2011/05/24	Rousseff
50	MT	Santa Carmem	2011/05/24	Rousseff
51	PA	Anapu	2012/09/28	Rousseff
52	PA	S José Porfírio	2012/09/28	Rousseff
53	AM	Apuí	2017/09/08	Temer
54	AM	Manicoré	2017/09/08	Temer
55	AM	Novo Aripuanã	2017/09/08	Temer
56	PA	Portel	2017/09/08	Temer
57	PA	Itaituba	2017/09/08	Temer
58	RO	Buritis	2017/09/08	Temer
59	RO	Candeias DJ	2017/09/08	Temer
60	RO	Cujubim	2017/09/08	Temer
61	PA	Placas	2018/11/19	Temer
62	PA	Uruará	2018/11/19	Temer
63	AC	Feijó	2021/01/11	Bolsonaro
64	AC	Sena Madureira	2021/01/11	Bolsonaro
65	AM	Humaitá	2021/01/11	Bolsonaro
66	PA	Jacareacanga	2021/01/11	Bolsonaro
67	PA	Medicilândia	2021/01/11	Bolsonaro
68	PA	Rurópolis	2021/01/11	Bolsonaro
69	PA	Trairão	2021/01/11	Bolsonaro
70	RR	Rorainópolis	2021/01/11	Bolsonaro

Table 11: *using not-yet-treated as control group.*

Dependent Variable:	Difference in forest cover		
Model:	(1)	(2)	(3)
treatment	0.0303***	0.0297***	0.0264***
	(0.0066)	(0.0065)	(0.0069)
Covariates			
deforestation increase		Yes	
deforestation lag			Yes
Fit statistics			
Observations	3,600	3,060	3,240

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

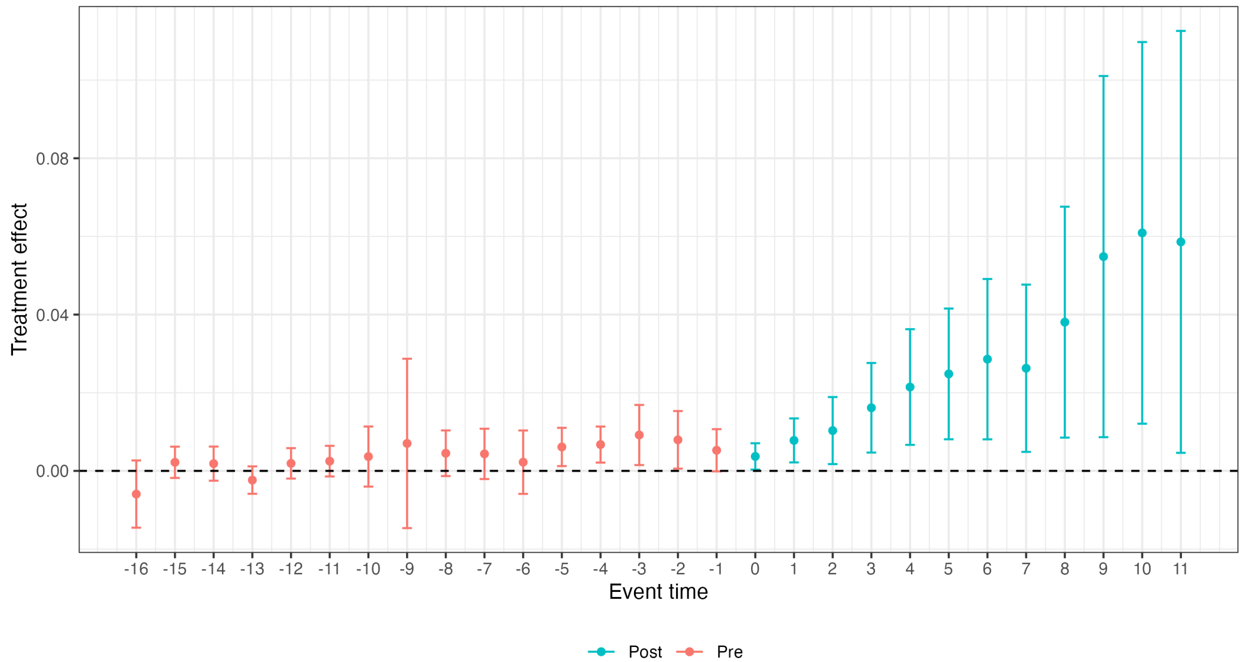

Figure 11: *conditional divergence between subunits over time for alternative covariate.*

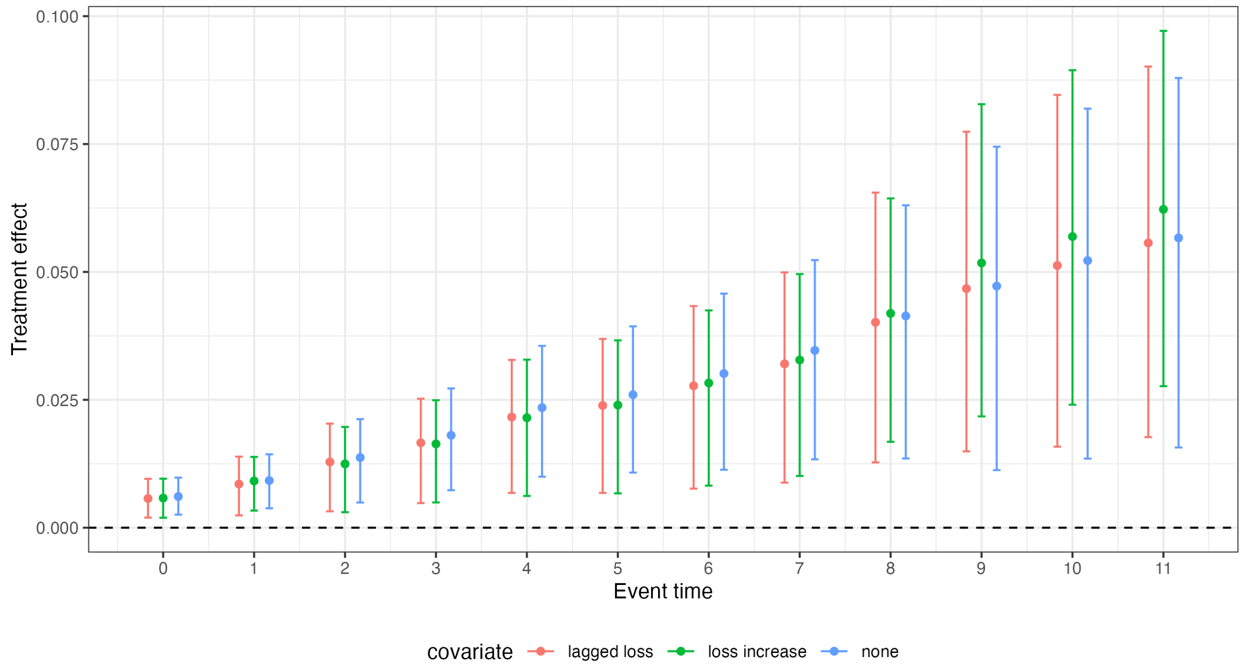

Figure 12: *estimates using Imai et al. (2021)'s panel matching approach.*

Table 12: *generalized triple-difference estimator with Conley standard errors.*

Dependent Variable:	difference_cover					
Model:	(1)	(2)	(3)	(4)	(5)	(6)
Variables						
treat	0.0582***	0.0500***	0.0461***	0.0582***	0.0500***	0.0461***
	(0.0113)	(0.0102)	(0.0097)	(0.0128)	(0.0115)	(0.0111)
lag_m_loss_1		-0.4947**			-0.4947*	
		(0.2519)			(0.2527)	
loss_increase			-0.0001			-0.0001
			(0.0002)			(0.0002)
Fixed-effects						
id_municipio	Yes	Yes	Yes	Yes	Yes	Yes
year	Yes	Yes	Yes	Yes	Yes	Yes
Fit statistics						
S.E. type	Conley (50km)	Conley (50km)	Conley (50km)	Conley (100km)	Conley (100km)	Conley (100km)
Observations	3,600	3,240	3,060	3,600	3,240	3,060
R^2	0.98338	0.98680	0.98786	0.98338	0.98680	0.98786
Within R^2	0.11631	0.11408	0.09265	0.11631	0.11408	0.09265

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

Table 13: *CS diff-in-diff estimator with fines as outcome and one year anticipation.*

Dependent Variables:	Number of fines	Amount fined (1,000 R$)
Model:	(1)	(2)
treatment	15.9384**	18330.86***
	(7.604)	(6977.713)
Covariates		
Fit statistics		
Observations	3,600	3,600

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

Table 14: *proportion of environmental fines with missing coordinates.*

Year	Fines with location	Total number of fines	Coverage
2000	26	13200	0.0019697
2001	41	14356	0.0028559
2002	86	22194	0.0038749
2003	201	13591	0.0147892
2004	368	13976	0.0263309
2005	630	17147	0.0367411
2006	2479	12532	0.1978136
2007	4796	11897	0.4031268
2008	5512	12077	0.4564047
2009	4400	9123	0.4822975
2010	4273	8084	0.5285750
2011	4387	7201	0.6092209
2012	4182	5549	0.7536493
2013	4234	5107	0.8290582
2014	6413	6490	0.9881356
2015	7155	7175	0.9972125
2016	7000	7021	0.9970090
2017	6222	6235	0.9979150
2018	6192	6202	0.9983876
2019	4785	4831	0.9904782
2020	2971	2996	0.9916555
2021	4053	4065	0.9970480
2022	4140	4144	0.9990347

Table 15: *alternative specifications for the event models.*

Dependent Variables:		enforcement			amount_1000	
Model:	(1)	(2)	(3)	(4)	(5)	(6)
	Logit	Logit	Logit	OLS	OLS	OLS
Variables						
Constant	-2.101***	-2.339***	-2.309***	176.1***	163.3*	217.3**
	(0.1443)	(0.1458)	(0.1555)	(39.82)	(72.34)	(67.06)
treat	0.8296***	1.108***	0.9918***	696.5***	710.8**	614.8**
	(0.1539)	(0.1602)	(0.1556)	(187.6)	(207.4)	(182.0)
indigenous_land	-1.296***	-0.9351***	-0.6979***	-83.82	27.14	98.53
	(0.2222)	(0.2252)	(0.2483)	(51.54)	(82.43)	(103.9)
treat × indigenous_land	0.7586**	0.6403*	0.5431*	-349.4	-411.0*	-421.8
	(0.3772)	(0.3494)	(0.3019)	(195.9)	(210.7)	(215.5)
log_distance		0.0343	0.0274		99.14	87.51
		(0.1278)	(0.1326)		(68.72)	(69.81)
year		-0.0849**	-0.1461***		88.91***	56.35**
		(0.0334)	(0.0301)		(12.60)	(21.21)
lat		0.2471***	0.2573***		56.92	50.44
		(0.0730)	(0.0711)		(40.42)	(40.74)
long		-0.4642***	-0.4311***		-144.9	-116.1
		(0.0898)	(0.0907)		(120.3)	(121.4)
previous_nearby			0.3151***			253.5**
			(0.0926)			(78.77)
area			-0.1166*			9.722
			(0.0696)			(21.32)
Fit statistics						
Observations	292,429	292,429	267,599	292,429	292,429	267,599
Squared Correlation	0.02345	0.04466	0.05669	0.01475	0.01878	0.02540
Pseudo R^2	0.02786	0.05111	0.06403	0.00079	0.00101	0.00137
BIC	257,464.3	251,357.6	227,113.3	5,472,422.3	5,471,274.8	5,020,540.7

Clustered (id_municipio & year) standard-errors in parentheses

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

Table 16: *varying the size of the buffer and enforcement window.*

Dependent Variables:	fines_5km	fines_15km	fines_2m	fines_6m	fines	
Model:	(1)	(2)	(3)	(4)	(5)	(6)
Variables						
Constant	-2.134***	-0.4839**	-1.727***	-0.8205***	-1.122***	-1.122***
	(0.1752)	(0.1914)	(0.1891)	(0.2061)	(0.1756)	(0.2125)
treat	0.7228***	0.7429***	0.7342***	0.6880***	0.7314***	0.7314***
	(0.1705)	(0.1698)	(0.1919)	(0.1698)	(0.2174)	(0.0845)
indigenous_land	-1.183***	-1.504***	-1.265***	-1.533***	-1.456***	-1.456***
	(0.2496)	(0.2891)	(0.2622)	(0.3109)	(0.3471)	(0.3721)
treat × indigenous_land	0.5643	0.9032**	0.5769	0.9510**	0.8438*	0.8438
	(0.4291)	(0.3858)	(0.3861)	(0.4184)	(0.4728)	(0.5311)
Fit statistics						
Observations	292,429	292,429	292,429	292,429	292,429	292,429
Squared Correlation	0.00606	0.01476	0.00682	0.01214	0.01052	0.01052
Pseudo R^2	0.02058	0.03293	0.02294	0.02740	0.02726	0.02726
BIC	350,868.9	1,229,156.0	495,901.2	915,027.7	762,819.8	762,819.8

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

Table 17: *two-way fixed effects models with Conley standard errors.*

Dependent Variable:	area_events			
Model:	(1)	(2)	(3)	(4)
Variables				
lag_number_fines	-8,848.3		-8,848.3	
	(6,520.6)		(6,918.1)	
lag_area_events	-0.0741***	-0.0735***	-0.0741***	-0.0735***
	(0.0190)	(0.0191)	(0.0258)	(0.0260)
lag_amount_fines		-51.66**		-51.66*
		(25.07)		(30.42)
Fixed-effects				
hex_id	Yes	Yes	Yes	Yes
year	Yes	Yes	Yes	Yes
Fit statistics				
S.E. type	Conley (10km)	Conley (10km)	Conley (50km)	Conley (50km)
Observations	158,226	158,226	158,226	158,226
R^2	0.34591	0.34598	0.34591	0.34598
Within R^2	0.00573	0.00583	0.00573	0.00583

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