Populism and Environmental Enforcement: Evidence from Brazil

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Abstract

Does electing a populist leader weaken environmental protection? I exploit municipal variation in support for Jair Bolsonaro in the 2018 Brazilian presidential election to estimate the differential decline in federal environmental enforcement. Using IBAMA administrative records on nearly 200,000 enforcement actions (2015–2025), municipalities with higher Bolsonaro vote shares show a sharp decline in the log value of fines: -0.57 log points per unit vote share nationally and -1.31 in the Legal Amazon (concentrated in Mato Grosso and Pará). The decline appears immediately in 2019 and survives in a clean pre-pandemic window ending before Brazil's first COVID case. Scaling the reduced-form deforestation effect—broadly distributed across the Amazon—by a one-standard-deviation contrast implies roughly 4,874 km^2 of additional annual deforestation and, illustratively, on the order of \$3.7 billion per year in carbon damages at the EPA social cost of carbon.

JEL Codes: Q23, Q28, D72, P16

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1 Introduction

A growing body of evidence documents the economic and institutional consequences of populist governance (Guriev and Papaioannou, 2022; Funke, Schularick, and Trebesch, 2023). One consequence follows directly from how populism governs but has gone largely unstudied: the erosion of environmental protection. Populism rewards immediate, visible benefits to its base and discounts diffuse, long-horizon public goods. Environmental protection is the canonical case: its benefits accrue over decades, while its costs—forgone extraction, agriculture, and development—fall immediately on concentrated constituencies. A populist executive optimizing for short-run support thus has little incentive to defend the environment, and, where enforcement authority is centralized under that executive, ample means to let it lapse.[1] The stakes are largest where populists have taken power in resource-rich countries, from the Amazon to Southeast Asian forests. If populist governments systematically weaken environmental enforcement, the consequences may be irreversible: deforested land does not regrow on political timescales.

This paper provides causal evidence on this question by studying how the election of Jair Bolsonaro as president of Brazil in 2018 affected federal environmental enforcement. Brazil is an ideal setting for three reasons. First, the country contains approximately 60% of the Amazon rainforest, the world's largest tropical forest and a critical carbon sink. Second, environmental enforcement is centralized in a single federal agency—IBAMA (*Instituto Brasileiro do Meio Ambiente e dos Recursos Naturais Renováveis*)—whose leadership, budget, and operational priorities are directly controlled by the president, without requiring legislative approval. Third, Bolsonaro campaigned explicitly on weakening environmental regulation, famously calling IBAMA an "industry of fines" and promising to "not demarcate one more

[1] The short-horizon bias spans the political spectrum. Left-populist governments in Latin America traded long-horizon conservation for short-horizon extractive revenue to fund redistribution (Kingsbury, 2021; Riofrancos, 2020): Rafael Correa expanded oil drilling in Ecuador's Yasuní reserve after campaigning to protect it; Evo Morales advanced the TIPNIS highway and agricultural-frontier expansion across Indigenous territory; and the Chávez and Maduro governments opened Venezuela's Orinoco Mining Arc. The operative force is the political time horizon, not left-right ideology.

centimeter of indigenous land." This combination of ecological significance, institutional centralization, and stated political intent makes the Brazilian case both tractable for causal analysis and consequential for global environmental policy.

I exploit municipal-level variation in Bolsonaro's 2018 vote share as a measure of local political alignment with the new president. Using administrative records on nearly 200,000 enforcement actions from IBAMA's open data portal—150,300 fines and 47,142 embargoes over 2015–2025—and satellite-based deforestation data from INPE's PRODES program, I document three sets of findings. The paper's core contribution is to trace one causal chain directly in administrative data: political alignment with a populist executive collapses the enforcement of a centralized federal regulator, and that collapse shows up in measurable Amazon deforestation and its welfare cost. Existing work on populism and on tropical deforestation has inferred this regulatory channel; I measure each link.

First, municipalities with higher Bolsonaro vote shares experienced a sharp decline in the intensive margin of enforcement. The log value of fines issued—the preferred headline outcome—falls by 0.57 log points per unit Bolsonaro vote share in the all-Brazil panel ($p < 0.001$) and by 1.31 log points in the Legal Amazon ($p < 0.001$), concentrated in Mato Grosso and Pará—the most enforcement-intensive, agribusiness-organized frontier states, where the collapse was sharpest, exactly as the mechanism predicts (Section 5). This intensive-margin decline appears immediately in 2019 and survives restriction to a clean pre-pandemic window ending before Brazil's first COVID case—the paper's cleanest causal design, since it isolates the 2019 collapse from any pandemic confound—where the log-value decline is -0.42 (all-Brazil, $p = 0.001$) and -1.38 (Legal Amazon, $p < 0.001$) (Section 5). The extensive-margin count of fines also falls: by 0.96 per unit vote share in the Legal Amazon ($p = 0.042$) and by 0.225 in the all-Brazil panel ($p = 0.083$). The count-level decline, however, only becomes statistically visible after 2020, so its timing overlaps with the pandemic; I treat it as supporting evidence for the intensive-margin claim rather than as a cleanly identified headline. The decline concentrates in high-value fines in municipalities with high pre-period enforcement intensity, consistent with a compositional shift in what is enforced rather than a broad decline in state capacity.

Second, the enforcement decline translated into measurable forest loss. Valued at the EPA social cost of carbon (\$51 per tCO_2) and a tropical-forest carbon density of 150 tCO_2 per hectare, the reduced-form impact of Bolsonaro vote share on deforestation in the Legal Amazon—which, unlike the enforcement result, is broadly distributed and, if anything, larger when Mato Grosso is excluded—implies approximately 4,874 km^2 of additional annual deforestation and on the order of \$3.7 billion per year in carbon damages at a per-standard-deviation contrast (95% confidence interval \$0.9–\$6.6 billion, propagating the reduced-form standard error). The decline also appears concentrated in the infraction categories tied to the agribusiness coalition: point estimates are largest for flora and forest violations and smaller for fauna, fishing, and pollution. These category-level differences are not individually significant, so I read the selectivity as *suggestive* of targeted regulatory capture (Stigler, 1971; Konisky and Woods, 2018) rather than as an independently identified result.

Third, as corroborating evidence that the collapse was a coordinated political signal rather than an isolated administrative event, I show the same anti-environmental shift propagating beyond the federal agency—a pattern I call *vertical dismantlement*. In the Brazilian Chamber of Deputies, a within-deputy difference-in-differences on deputies who served both before and during the Bolsonaro presidency shows the right-rural and left-green blocs pulling apart on exactly the dimensions of the enforcement conflict: holding each deputy to their own baseline, the agricultural-rhetoric gap between blocs widens by 8.2 percentage points ($p = 0.002$) and the environmental-rhetoric gap by 2.1 points in the opposite direction ($p = 0.083$) once the populist president took power. At the municipal level, environmental councils in pro-Bolsonaro municipalities held significantly fewer meetings, suggesting that local environmental governance deteriorated in tandem with federal enforcement. Meanwhile, soy cultivation expanded significantly more in high-Bolsonaro municipalities, establishing a direct link between enforcement collapse and agricultural frontier expansion.

The paper contributes to three literatures. On the deterrence effects of environmental enforcement, the existing literature has established that monitoring reduces illegal deforestation (Assunção, Gandour, and Rocha, 2023) and that tropical deforestation responds to institutional incentives (Burgess et al., 2012; Hargrave and Kis-Katos, 2013; Abman, 2018), but the endogeneity of enforcement allocation has limited causal inference on the deterrence elasticity. Using the populist transition as a source of exogenous variation in enforcement intensity, this paper identifies the causal effect of enforcement on deforestation—a parameter the literature has sought (Shimshack and Ward, 2005; Gray and Shimshack, 2011). Concurrent work uses different strategies: Magalhães de Oliveira et al. (2026) exploit social media exposure, and Nishijima and Pal (2024) use close municipal elections, whereas I use presidential vote share, a more direct link to the federal enforcement apparatus. My contribution beyond identification is to measure the enforcement collapse directly in IBAMA's administrative records before translating it into deforestation and a welfare cost, establishing the regulatory channel rather than inferring it from the land-use outcome alone. The point estimates are also consistent with a *selective* collapse, concentrated in the flora and forest violations that bind on the agribusiness coalition, although the category-level differences are not individually significant and I treat the selectivity as suggestive.

On the consequences of populist governance, existing work documents effects on trade, fiscal policy, and democratic institutions, but evidence on environmental policy is scarce (Ferrante and Fearnside, 2019; Vale, Berenguer, Armenteras, et al., 2021). I show that a populist executive can dismantle regulatory enforcement *without* legislative reform—through political appointments, budget reallocation, and public signaling rather than new law (Iyer and Mani, 2012; Ferraz and Finan, 2011)—and that the resulting environmental damage, operating through deforestation, is irreversible on political timescales.

Finally, the paper speaks to the political control of bureaucracy. A long literature establishes that elected principals shape the output of nominally autonomous agencies (Weingast and Moran, 1983; Stigler, 1971), yet the mechanism that transmits a political signal into administrative behavior is rarely observed directly. I trace that transmission across three levels of government around a single electoral shock: the federal enforcement collapse itself; a within-deputy realignment in congressional rhetoric, in which agribusiness-bloc deputies shift toward agricultural and away from environmental speech relative to their own pre-period baselines (Section 6); and a measured contraction in the activity of municipal environmental councils (Colonnelli, Prem, and Teso, 2020; Olken and Pande, 2012). Documenting the same signal propagating through the agency, the legislature, and local institutions at once—what I term *vertical dismantlement*—shows that a populist executive can reorient the regulatory state across levels of government without any change in formal law.

The remainder of the paper proceeds as follows. Section 2 describes the institutional setting. Section 3 presents the data. Section 4 outlines the empirical strategy. Section 5 presents the main results. Section 6 discusses mechanisms, extensions, and welfare implications. Section 7 concludes.

2 Institutional Background

2.1 Environmental Enforcement in Brazil

Brazil's environmental enforcement is centralized at the federal level through IBAMA, created in 1989 as part of the National Environmental System (SISNAMA). IBAMA is responsible for monitoring and sanctioning environmental violations across the entire national territory, including illegal deforestation, unauthorized land clearing, wildlife trafficking, and pollution. The agency operates through regional superintendencies in each state, with field agents conducting inspections, issuing fines (*autos de infração*), and imposing embargoes on illegally deforested areas.

A critical feature of IBAMA is its direct subordination to the federal executive. As an agency under the Ministry of the Environment, IBAMA's president is appointed by the minister (who is appointed by the president), and its budget, operational priorities, and personnel decisions are controlled by the executive branch. This means the president can reshape environmental enforcement through executive action alone, without requiring congressional approval. State-level environmental agencies (OEMAs) also conduct enforcement, but IBAMA remains the primary enforcer for deforestation in the Amazon, particularly through operations guided by the DETER satellite monitoring system operated by INPE. Since 2004, DETER has provided near-real-time deforestation alerts that guide IBAMA field operations, creating a monitoring-enforcement nexus that has been shown to significantly reduce deforestation (Assunção, Gandour, and Rocha, 2023). Rural credit policies also interact with deforestation incentives in the Amazon (Assunção, Gandour, Rocha, and Rocha, 2020), and political cycles have been shown to affect deforestation rates (Pailler, 2018).

IBAMA's enforcement arsenal includes three main instruments. Administrative fines (*autos de infração*) range from R$50 to R$50 million depending on the severity and extent of the violation. Embargoes (*termos de embargo*) prohibit all economic activity on areas where illegal deforestation has been detected. Seizures (*termos de apreensão*) confiscate illegally obtained products such as timber and charcoal. Each enforcement action is recorded in administrative databases with the date, location, offender identity, infraction type, and monetary value, providing a rich source of microdata for empirical analysis.

2.2 The Bolsonaro Presidency and Environmental Policy

Jair Bolsonaro was elected president on October 28, 2018, and inaugurated on January 1, 2019. His campaign included explicit anti-environmental rhetoric that signaled a departure from Brazil's established environmental governance. He repeatedly referred to IBAMA fines as an obstacle to economic development, famously stating that "the IBAMA fining frenzy is over" (*"a festa de multas do IBAMA acabou"*), and promised to end demarcations of indigenous and protected lands.

Upon taking office, Bolsonaro implemented a series of changes that reshaped environmental enforcement without requiring new legislation. IBAMA's leadership was replaced with political appointees perceived as less committed to enforcement. The agency's operational budget was progressively reduced, with executed enforcement spending declining approximately 40% in real terms between 2018 and 2021 (Vale, Berenguer, Armenteras, et al., 2021; Ferrante and Fearnside, 2019; Escobar, 2020). In April 2019, Decreto 9.760 restructured the administrative process for environmental fines, creating mandatory "conciliation hearings" that effectively paralyzed fine collection and sent a signal of impunity. Environment Minister Ricardo Salles publicly stated in a ministerial meeting in April 2020 that the COVID-19 pandemic should be used as cover to "pass the cattle" (*"passar a boiada"*)—that is, to push through deregulatory changes while media attention was focused elsewhere, and budget reallocation to non-enforcement functions created operational constraints that independently suppressed field activity even absent explicit instruction to individual inspectors.

These changes created conditions for a differential enforcement decline across municipalities. Areas politically aligned with Bolsonaro may have experienced larger declines through several channels: local IBAMA agents responding to political signals from their principals, reduced central oversight in politically friendly areas, or emboldened local actors increasing illegal activity without fear of sanctions.

The mechanism this paper tests is specific, and it is worth naming. When a single executive controls a centralized national enforcement agency—no legislative check, no federated state veto, no independent inspectorate—populist capture does not require new laws, zeroed-out budgets, or visible agent resistance. It works through *selective de-prioritization*: the agency keeps operating, the paperwork keeps flowing, but the categories of violation that hurt the governing coalition stop being punished while the categories that do not are left untouched. IBAMA is the case study. The decline concentrates in flora and forest fines—the instruments that bind on agribusiness—and is weaker for the fauna and pollution categories that do not bind on the same coalition. That ordering is what a generic state-capacity or pandemic story would not predict; as I show below, the category-level differences are suggestive rather than statistically decisive, but the pattern runs throughout the paper.

3 Data

3.1 Data Sources

To measure the relationship between political alignment and environmental enforcement, I combine five publicly available datasets. The primary enforcement data comes from IBAMA's open data portal (*Dados Abertos*), which contains individual-level records of every fine and embargo issued by the agency since 2008, including the date, municipality, fine value, offender identity (CPF/CNPJ), infraction type (flora, fauna, pollution), area classification (deforestation, burning, other), and type of enforcement action (routine, operation, demand). I aggregate these records to a municipality-by-month panel spanning January 2015 to March 2025, with the treatment period defined as January 2019 to December 2022 (the Bolsonaro presidency). After dropping cancelled fines and records with missing municipality codes, the dataset contains 150,300 individual fines and 47,142 embargoes across 4,310 municipalities. Municipality-months with no enforcement activity are coded as zeros—these represent genuine absence of enforcement, not missing data.

Municipal-level election results come from Brazil's Superior Electoral Court (TSE). I use first-round presidential results from 2018, computing Bolsonaro's vote share for each of the 5,708 municipalities.[2] Annual deforestation data at the municipality level comes from INPE's PRODES program, which uses Landsat satellite imagery to measure deforestation increments in km^2 for all municipalities in the Legal Amazon.[3] Municipal agricultural production data—including soy planted area, corn area, and cattle herd size—comes from IBGE's Municipal Agricultural Production (PAM) survey. Finally, transcripts of speeches delivered in the

[2] Bolsonaro's vote share varies substantially even within states. For example, in Mato Grosso—the state with the highest average Bolsonaro support—his vote share ranges from 27% in Querência to 76% in Lucas do Rio Verde. In Pará, it ranges from 11% in Portel to 68% in Redenção.

[3] Accessed via the `datazoom.amazonia` R package (Data Zoom, Department of Economics, PUC-Rio, 2023).

Brazilian Chamber of Deputies come from the Câmara's open data API, covering Legislatures 55 (2015–2019) and 56 (2019–2023), totaling 110,073 speeches from 949 deputies. I classify each speech for environmental and agricultural content using keyword matching on a curated list of terms, described in detail in the Appendix.

3.2 Analysis Panel

The main analysis panel is constructed by merging IBAMA enforcement data with TSE election results at the municipality level, using state and normalized municipality name as the merge key. The matched sample contains 4,250 municipalities observed over 123 months (568,629 municipality-month observations). For the Legal Amazon subsample—the primary focus for deforestation-related analyses—the panel includes 692 municipalities (106,518 observations). Additional datasets (PRODES deforestation, IBGE agricultural production, Chamber of Deputies speeches) are merged for specific analyses described in the relevant sections.

Figure 1 plots the three enforcement outcomes—fines, fine value, and embargoes per municipality-month—over the full 2015–2025 window, separately for municipalities above and below the median Bolsonaro 2018 vote share (0.399). Three features stand out. First, enforcement declines across all three margins during the Bolsonaro presidency (shaded), with a partial recovery beginning around 2022 as the political cycle turns toward Lula's election. Second, municipalities above the median Bolsonaro vote share have higher enforcement levels throughout—reflecting the concentration of IBAMA activity in agricultural-frontier areas—so the identifying variation comes from differential *changes*, which the fixed-effect structure isolates. Third, the decline begins on the value margin in 2019 and spreads to counts after 2020, the timing pattern the empirical strategy exploits.

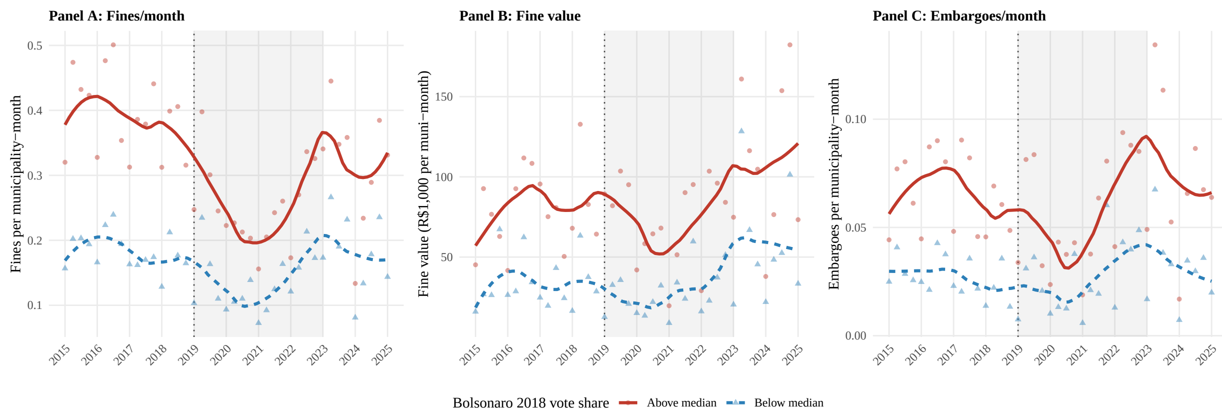

Figure 1: IBAMA Enforcement Over Time by Bolsonaro Vote Share

Notes: Points are quarterly means per municipality-month, by above/below median Bolsonaro 2018 first-round vote share (median 0.399); lines are LOESS smooths. Panel A: number of fines. Panel B: total fine value (R$1,000). Panel C: number of embargoes. Shaded area: Bolsonaro presidency (January 2019–December 2022). Sample: 4,250 municipalities, January 2015–March 2025.

4 Empirical Strategy

To estimate the effect of Bolsonaro's presidency on environmental enforcement, I employ a difference-in-differences design that exploits municipal-level variation in political alignment with the new president. The main specification is:

$$\begin{aligned} Y_{m,t} = \beta\,(\text{BolsoVote}_m \times \text{Bolsonaro}_t) + \phi\,(\text{BolsoVote}_m \times \text{Post2023}_t) \\ + \gamma\,(\log E_m \times t) + \mu_m + \delta_{s(m),t} + \alpha_{s(m)} \cdot t + \varepsilon_{m,t} \end{aligned} \tag{1}$$

where $Y_{m,t}$ is the outcome of interest—count or log value of IBAMA enforcement actions—in municipality m in month t; BolsoVote_m is Bolsonaro's first-round vote share in 2018; Bolsonaro_t is an indicator for the Bolsonaro presidency (January 2019 to December 2022); Post2023_t is an indicator for the subsequent period (January 2023 onward), included to absorb the political transition but not the focus of the analysis; $\log E_m \times t$ is the log-electorate control interacted with time; μ_m are municipality fixed effects; $\delta_{s(m),t}$ are state-by-semester fixed effects; and $\alpha_{s(m)} \cdot t$ are state-specific linear time trends.

I also report results using a binary treatment indicator (above/below median Bolsonaro vote share), which yields more precise estimates. Standard errors are clustered at the municipality level throughout. The coefficient ϕ on $\text{BolsoVote}_m \times \text{Post2023}_t$ absorbs the partial enforcement recovery under the Lula administration and is included to avoid contaminating β with post-2022 reversion; it is not a focus of the analysis.

Because BolsoVote_m varies continuously, β is a variance-weighted average of municipality-level effects, with municipalities at the extremes of the vote-share distribution receiving greater weight. In the Legal Amazon, this concentrates weight on frontier municipalities, which is substantively appropriate given the paper's focus on the agro-export frontier.

This continuous-treatment weighting raises whether the estimate is robust to the heterogeneity-robust difference-in-differences literature (Chaisemartin and D'Haultfœuille, 2020; Sun and Abraham, 2021; Callaway, Goodman-Bacon, and Sant'Anna, 2024). Two features of the design contain the concern. First, treatment timing is *common*: every municipality is exposed at the January 2019 transition, so there is no staggered adoption and none of the "forbidden" comparisons of later- to already-treated units that generate negative weights in those settings. With a single treatment date and a never-treated comparison group (below-median municipalities), the Callaway–Sant'Anna, Sun–Abraham, and de Chaisemartin–D'Haultfœuille estimators coincide with the binarized two-way fixed-effects estimate. Second, the binarized specification (Table 2), which discards the continuous dose entirely and is therefore immune to dose-weighting, is negative across all outcomes and marginally significant on the all-Brazil margins (log fine value -0.073, $p = 0.054$), and the tercile dose-response (Appendix Table A5) is monotone—the heterogeneity-robust object for a continuous treatment. The continuous coefficient is therefore best read as a variance-weighted average that is robust in sign and corroborated by the binarized and dose-response estimates, not as a quantity sensitive to the staggered-timing critique.

The headline specification deliberately omits a pre-period enforcement $\times$ time control. This is an ex ante choice, not a post hoc one. Pre-period enforcement intensity is itself an outcome of the political-economic process that generates Bolsonaro vote shares: in the Legal Amazon, agricultural frontier municipalities received both more pre-Bolsonaro IBAMA attention and voted more heavily for Bolsonaro, so pre-period enforcement is near-collinear with the treatment interaction and absorbs identifying variation rather than fixing parallel trends. I verify this directly: the pre-trend F-test is essentially identical with and without the control ($p = 0.330$ vs. $p = 0.328$), so the control does no work for parallel trends, and Appendix 7 shows that it attenuates the Legal Amazon fines-count coefficient without touching the log-value estimates that carry the headline. A pre-registered specification that dropped this control for all samples and outcomes would yield exactly Table 1; I adopt it throughout and report the alternative for transparency.

The coefficient β captures the differential change in enforcement during the Bolsonaro period for municipalities with higher Bolsonaro vote shares, relative to the pre-period. A negative β indicates that enforcement declined more in pro-Bolsonaro municipalities. I also estimate an event study specification replacing the single Post_t indicator with semester-specific interactions, using 2017H2 (the second half of 2017, the last full year before the election) as the reference period. Flat pre-trend coefficients support the parallel trends assumption, and the pre-pandemic window (Section 5) provides the sharpest test: it shows the decline begins in 2019, before any possible COVID confound.

The key identifying assumption is that, absent Bolsonaro's inauguration, enforcement trends in municipalities with different levels of Bolsonaro support would have evolved in parallel, conditional on the included fixed effects and controls. Municipality fixed effects absorb all time-invariant differences between municipalities, including geographic, economic, and political characteristics correlated with both vote shares and enforcement levels. State-by-semester fixed effects absorb all state-level policy changes, macroeconomic shocks, and commodity price cycles. State-specific linear trends control for differential pre-existing trajectories across states.

Several potential concerns deserve discussion. Municipalities that voted heavily for

Bolsonaro differ systematically from those that did not: they tend to be more rural, more dependent on agriculture, and have larger land areas (Appendix Table A1 reports pre-period characteristics by vote-share quartile). These level differences are absorbed by municipality fixed effects. The concern is whether these characteristics also predict differential enforcement *trends*. Agricultural municipalities may have different enforcement trends driven by commodity price cycles or land-use dynamics unrelated to the political transition. The state-by-semester fixed effects absorb state-level agricultural dynamics, and state-specific linear trends control for differential pre-existing trajectories. The log-electorate $\times$ time control further absorbs differential trends correlated with municipality size. I also show in the Appendix that results are robust to alternative fixed-effect structures and clustering levels (Table A2).

Another concern is that the COVID-19 pandemic (beginning March 2020) may have differentially affected enforcement in pro-Bolsonaro municipalities. Three pieces of evidence mitigate this concern. First, the decline appears concentrated by infraction type (point estimates are largest for flora and forest fines), which is harder to reconcile with a generic pandemic-driven explanation that would affect all categories equally, though I lean primarily on the clean pre-pandemic window below. Second, restricting the sample to the clean pre-pandemic window (January 2015–February 2020) leaves the intensive-margin result intact: the log value of fines falls by 0.415 log points per unit Bolsonaro vote share in the all-Brazil panel ($p = 0.001$) and by 1.384 log points in the Legal Amazon ($p < 0.001$), with the window ending before Brazil's first COVID case (Table 4 and Section 5). The extensive-margin count, by contrast, is insignificant and wrong-signed in the same window and only becomes statistically visible after 2020, so the paper leads with log value as the cleanly identified claim and treats the count decline as supporting evidence whose timing overlaps with the pandemic. Third, Environment Minister Salles explicitly stated in April 2020 that the pandemic should be used as cover to advance deregulation (the infamous "*passar a boiada*" statement), so any intensification during COVID is itself part of the mechanism rather than a confound.

A final concern involves SUTVA: if reduced enforcement in pro-Bolsonaro municipalities displaced illegal activity to neighboring municipalities, the estimated decline in enforcement would understate the true deterioration in environmental protection. Three features of the setting make displacement unlikely to be a first-order concern. First, IBAMA agents are deployed from regional superintendencies and their geographical coverage is determined by operational mandates, not by spillover-seeking offenders. Second, the deforestation reduced form is already an upper bound on the pure deterrence effect, so displacement would leave the welfare calculation conservative rather than inflated. Third, municipalities at the deforestation frontier—where the effect concentrates—tend to border other high-Bolsonaro municipalities, so any displacement would occur toward areas that also experienced enforcement declines, attenuating rather than amplifying the bias.

5 Results

I begin with the cross-sectional gradient that motivates the design, then turn to the main difference-in-differences estimates, the event study, heterogeneity by infraction type, and additional heterogeneity analyses.

5.1 The Cross-Sectional Gradient

The descriptive time series in Figure 1 showed that enforcement fell across all three margins during the Bolsonaro presidency and that the decline was steeper where Bolsonaro support was higher (in raw terms, fines in top-quartile municipalities fell 37.9% versus 25.8% in the bottom quartile). That contrast, however, is unconditional—it could reflect the fact that high-Bolsonaro municipalities start from higher enforcement levels and are concentrated in particular states. Figure 2 therefore plots the relationship the difference-in-differences actually exploits: the change in enforcement against Bolsonaro vote share, after residualizing *both* on state fixed effects and log electorate, separately for the two margins. The intensive margin (log fine value, blue) shows a clearly negative within-state gradient (slope -0.35, $p < 0.001$):

municipalities that voted more heavily for Bolsonaro, relative to others *in the same state*, saw larger declines in fine value. This is precisely the variation the state-by-semester fixed effects preserve and the cross-state, frontier-versus-non-frontier variation they discard. The extensive margin (fines count, red) is flat within state (slope -0.008, $p = 0.93$), consistent with the intensive margin being the cleanly identified outcome and the count being the weaker one, the same asymmetry that runs through the main estimates.

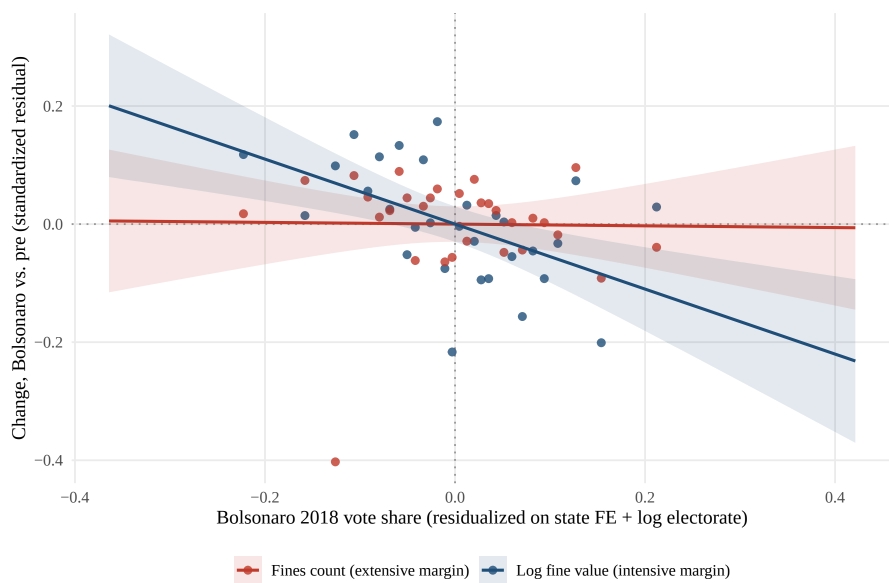

Figure 2: Within-State Gradient: Bolsonaro Vote Share and the Change in Enforcement

Notes: Each dot is the mean of 30 equal-count bins. The x-axis (Bolsonaro 2018 first-round vote share) and both outcomes are residualized on state fixed effects and log electorate. The two outcomes—change in log fine value (blue) and change in fines count (red) between the Bolsonaro period (2019–2022) and the pre-period (2015–2018)—are standardized to comparable units (z-scored residuals) so the slopes can be read on a common vertical scale. Lines: OLS fits with 95% CI.

5.2 Main Estimates

Table 1 presents the main DiD results. Columns 1–3 report all-Brazil estimates for fines count, log fine value, and embargoes. Columns 4–6 report the same outcomes for the Legal Amazon. The headline specification includes municipality fixed effects, state $\times$ semester fixed effects, state linear trends, and a log-electorate $\times$ time control. I discuss the decision to omit a pre-period enforcement × time control at the end of this subsection and in Appendix Table A4.

The preferred headline outcome is the log value of fines issued, the intensive margin of enforcement. Romano-Wolf multiple testing corrections for all six Table 1 outcomes are reported in Appendix 7 (Table A12). In the all-Brazil sample, a one-unit higher Bolsonaro vote share is associated with a 0.569 log-point decline in monthly fine value ($p < 0.001$, column 2), and in the Legal Amazon with a 1.313 log-point decline ($p < 0.001$, column 5). The count of fines is also a meaningful decline: the all-Brazil coefficient is -0.225 ($p = 0.083$, column 1) and the Legal Amazon coefficient is -0.963 ($p = 0.042$, column 4). The Legal Amazon fines-count effect corresponds to roughly one fewer fine per municipality-month at the extreme of the vote-share distribution, a magnitude consistent with the sharp aggregate enforcement decline documented in Figure 1. Embargoes—a separate enforcement instrument (columns 3 and 6)—show no differential decline, which I discuss below.

Table 1: Effect of Bolsonaro Vote Share on IBAMA Enforcement

	All Brazil			Legal Amazon		
	Fines (1)	Log fine value (2)	Embargoes (3)	Fines (4)	Log fine value (5)	Embargoes (6)
Bolsonaro Vote × Post 2019	-0.225*	-0.569***	0.026	-0.963**	-1.313***	0.018
	(0.130)	(0.112)	(0.058)	(0.472)	(0.378)	(0.258)
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes
State × Semester FE	Yes	Yes	Yes	Yes	Yes	Yes
State Linear Trends	Yes	Yes	Yes	Yes	Yes	Yes
Log Electorate × Time	Yes	Yes	Yes	Yes	Yes	Yes
Observations	568,629	568,629	568,629	106,518	106,518	106,518
R^2	0.172	0.174	0.131	0.152	0.175	0.134

Notes: This table presents the headline difference-in-differences estimates of the effect of 2018 Bolsonaro vote share on IB enforcement. Columns (1)–(3) use the all-Brazil panel and columns (4)–(6) the Legal Amazon. The dependent variables ar monthly count of fines, the log value of fines, and the count of embargoes. The reported coefficient is the interaction of Bols vote share with the post-2019 (Bolsonaro presidency) indicator; a post-2023 (Lula) interaction is included but not shown columns include municipality fixed effects, state × semester fixed effects, state-specific linear time trends, and a log-elector time control. The pre-period enforcement × time control is deliberately omitted as a bad control (Appendix Table A4). Star errors clustered at the municipality level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

To address recent concerns about the interpretation of continuous-treatment two-way fixed effects estimators, Table 2 re-estimates the main specification with a binarized treatment (above vs. below the median of 2018 Bolsonaro vote share, 0.399). The binarized coefficients are directionally consistent with Table 1—negative across all outcomes in both samples—and marginally significant in the two all-Brazil columns (fines: -0.056, $p = 0.068$; log value: -0.073, $p = 0.054$). Magnitudes are smaller because the binary split discards within-group variation. The Legal Amazon binarized coefficients are directionally negative but imprecise. I report the continuous specification as the headline and the binarized specification as a co-primary estimand that shares the sample and fixed-effect structure of the headline; it is reported here to address the concern that continuous-treatment two-way fixed effects can weight observations in ways the binary split avoids.

Table 2: Binarized Main Specification (Above vs. Below Median Bolsonaro Vote)

	All Brazil		Legal Amazon	
	Fines (1)	Log fine value (2)	Fines (3)	Log fine value (4)
Above-Median Bolsonaro × Post 2019	-0.056*	-0.073*	-0.108	-0.073
	(0.031)	(0.038)	(0.123)	(0.120)
Municipality FE	Yes	Yes	Yes	Yes
State × Semester FE	Yes	Yes	Yes	Yes
State Linear Trends	Yes	Yes	Yes	Yes
Observations	568,629	568,629	106,518	106,518
R^2	0.189	0.174	0.160	0.175

Notes: This table presents the main specification with a binarized treatment, equal to one for municipalities above the median 2018 Bolsonaro vote share (0.399). It is a co-primary estimand reported because, unlike the continuous treatment, it discards within-group variation and is immune to continuous-dose weighting. The reported coefficient is above-median × post-2019; a post-2023 interaction and a pre-period enforcement × time control are included but not shown. Columns (1)–(2): all Brazil; (3)–(4): Legal Amazon. Municipality, state × semester fixed effects, and state linear trends throughout. Standard errors clustered at the municipality level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

The embargoes columns are statistical nulls and deserve explicit discussion. Embargoes, a separate instrument that physically suspends economic activity on a property, show no differential decline in pro-Bolsonaro municipalities: +0.026 (SE 0.058) in the all-Brazil panel and +0.018 (SE 0.258) in the Legal Amazon, both far from conventional significance. Two explanations are consistent with the data. Embargoes are issued through specialized operations with distinct institutional procedures (requiring field presence, geo-referenced documentation, and multi-agency coordination) that are less responsive to short-run political pressure than routine fine issuance. The operational complexity of embargoes may insulate them from informal executive direction in ways that fine assessments are not. Second, embargoes are rare events (mean well under one per municipality-month), so the null may reflect limited power rather than zero effect. Taken together, the embargo result is informative: it suggests that the enforcement collapse operated mainly through monetary penalties, the most politically manipulable instrument, rather than through the more operationally complex instruments that require field presence and interagency coordination. This distinction matters for policy design: restoring enforcement requires specifically rebuilding the deterrence function of fines, not just the apparatus of physical embargoes.

A natural concern with any municipality-level specification is whether to include a pre-period enforcement $\times$ time control to absorb differential trends in enforcement intensity. I omit this control in the headline specification because pre-period enforcement is itself an outcome of the same political-economic process that generates Bolsonaro vote shares (pro-Bolsonaro agricultural frontier municipalities also received more pre-Bolsonaro IBAMA attention), and in the Legal Amazon it is near-collinear with the treatment interaction and absorbs identifying variation. Appendix Table A4 reports the comparison. Adding the control leaves the all-Brazil log-value and embargo estimates essentially unchanged, modestly tightens the all-Brazil fines-count standard error, and attenuates the Legal Amazon fines-count coefficient from -0.963 ($p = 0.042$) to -0.452 ($p = 0.234$). The pre-trend F-test is essentially identical across the two specifications ($p = 0.328$ without, $p = 0.330$ with), so the control is not improving parallel trends—it is absorbing treatment variation. The headline numbers in Table 1 therefore reflect the cleaner, non-bad-control specification; Appendix 7 reports the more conservative alternative for readers who prefer it.

5.3 Event Study

Figure 3 presents event studies for all four headline outcomes—fines and log fine value, in the all-Brazil and Legal Amazon samples—using the continuous treatment and the headline specification, normalized to the second half of 2017, within the last full year before the 2018 election cycle. The event study is illustrative of the *dynamics* and of the parallel-trends assumption; the magnitude is carried by the pooled estimates in Table 1.

Three patterns stand out. First, pre-trends are flat: joint F-tests on the pre-election coefficients (2015H1–2018H1) do not reject zero in any of the four panels (p between 0.23 and 0.35). Second, the effect emerges around the political transition. The window from the October 2018 election to the January 2019 inauguration (shaded orange) shows the onset, consistent with anticipation once Bolsonaro's victory was known, and the decline then concentrates in 2019H2–2021H1. Third, the dynamics differ across samples in a substantively important way. In the all-Brazil panels, enforcement begins to recover by 2022 as the political cycle turns and Lula's election approaches; the log-value coefficient returns toward zero by the end of the Bolsonaro term. In the Legal Amazon, the deforestation frontier, there is no such recovery: the decline persists through 2022. The place where the enforcement collapse matters most for forest loss is precisely where it is most persistent.

The event-study coefficients are individually noisier than the pooled estimates, reflecting the non-uniform timing of the effect—concentrated early, recovering nationally by 2022. The event study is therefore illustrative of the dynamics; the headline identification rests on the pooled difference-in-differences, the pre-COVID window, the Oster bounds, and the placebo and within-state evidence above, not on the event-study average.

5.4 Enforcement by Infraction Type

A natural prediction is that the decline should concentrate in infraction types affecting the governing coalition's interests. Figure 4 presents separate event studies by category, and the

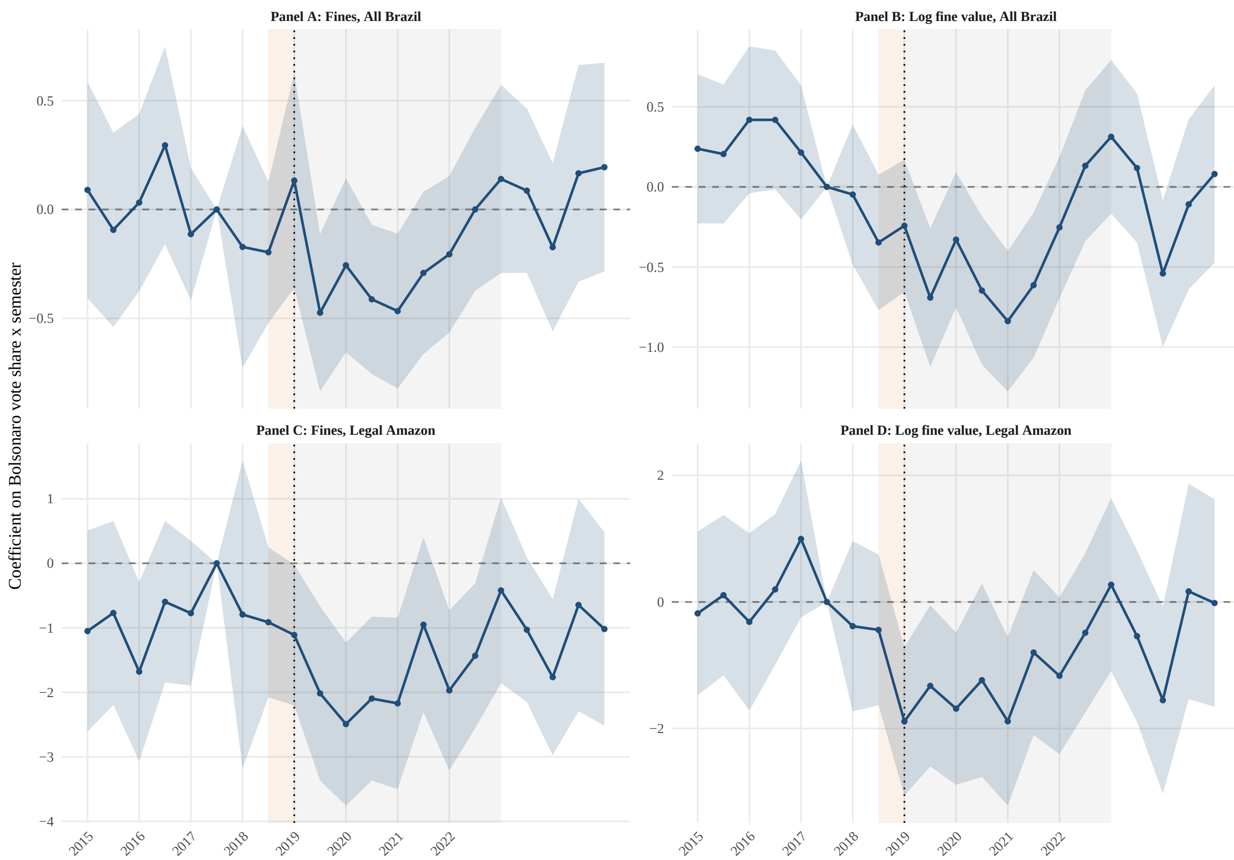

Figure 3: Event Study: IBAMA Enforcement × Bolsonaro Vote Share, by Outcome and Sample

Notes: Continuous treatment (Bolsonaro 2018 vote share). Reference: 2017H2 (second half of 2017, the last full year before the election). Municipality FE, state × semester FE, state linear trends, log electorate × time. Clustered SE, 95% CI. Orange band: October 2018 election to January 2019 inauguration (anticipation window). Grey: Bolsonaro presidency (2019–2022).

point estimates line up with this prediction: the decline is largest for flora and forest fines (Panel A) and progressively smaller for fauna and fishing (Panel B) and pollution (Panel C). The ordering is more pronounced in the Legal Amazon, where the overall effect concentrates (flora and forest -0.92 versus pollution -0.13). I am cautious about over-reading this pattern, however: the disaggregated monthly category counts are noisy, and a cross-equation test cannot reject equality of the category treatment effects (joint $\chi^2(2) = 2.00$, $p = 0.37$ for all Brazil; $\chi^2(2) = 1.56$, $p = 0.46$ for the Legal Amazon; Appendix Table A3). The category decomposition is therefore best read as *suggestive* of targeted de-prioritization rather than as an independently identified result. The more robust evidence that the decline is compositional comes from the intensive margin: total fine value falls while the average fine per infraction does not, implying a shift away from the high-value flora and forest category (Appendix 7). Appendix Table A3 reports the corresponding pooled decomposition by infraction and action type.

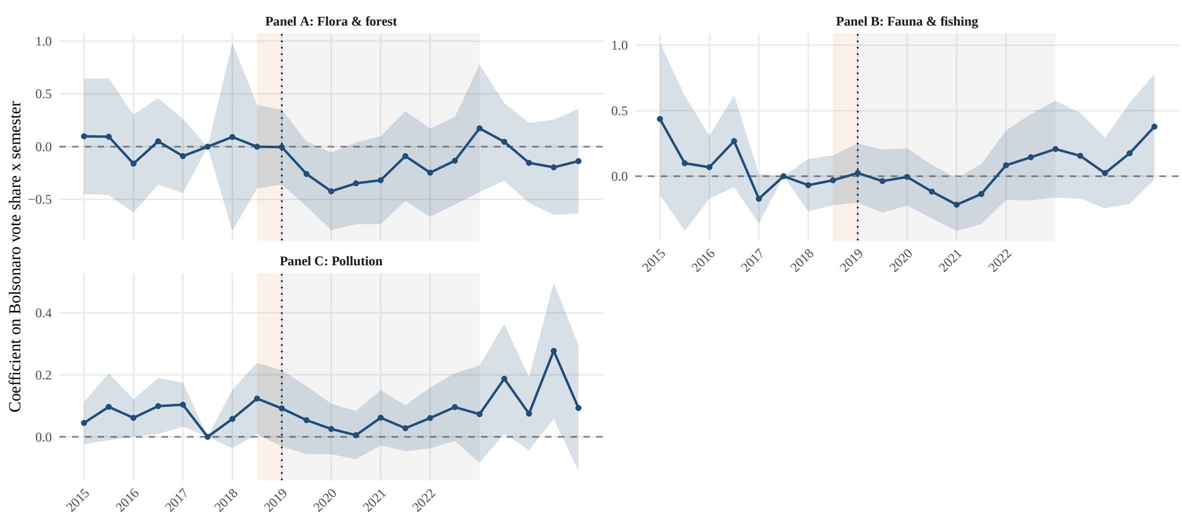

Figure 4: Event Study by Infraction Type

Notes: Separate event studies by infraction category, continuous treatment (Bolsonaro 2018 vote share interacted with semester). Reference: 2017H2. Municipality FE, state × semester FE, state linear trends, and log electorate × time. SE clustered at municipality level. 95% CI. The orange band marks the October 2018 election to January 2019 inauguration; the grey band marks the Bolsonaro presidency.

5.5 Heterogeneity

Table 3 explores heterogeneity across subsamples. The enforcement decline is strongest in Legal Amazon municipalities with high pre-period enforcement intensity (column 3), in Mato Grosso, the epicenter of soy expansion (column 4), and in smaller municipalities with less media visibility (column 5). The Appendix reports additional heterogeneity analyses including the triple-difference specification with pre-period enforcement (-0.639, $p = 0.006$). A leave-one-state-out analysis (Appendix Figure A4) shows that dropping Mato Grosso halves the Legal Amazon intensive-margin coefficient ($-1.31 \rightarrow -0.66$, $p = 0.115$); the Legal Amazon point estimates should be read as driven primarily by Mato Grosso and Pará, which are precisely the agro-export frontier states the paper's mechanism targets.

Table 3: Heterogeneity in the Enforcement Decline

	All Brazil (1)	Legal Amazon (2)	High Pre-Enf (3)	Mato Grosso (4)	Small Munis (5)
Bolsonaro Vote × Post 2019	-0.187**	-0.452	-0.417	-0.748***	-0.055*
	(0.094)	(0.379)	(0.492)	(0.285)	(0.033)
Municipality FE	Yes	Yes	Yes	Yes	Yes
State × Semester FE	Yes	Yes	Yes	Yes	Yes
State Linear Trends	Yes	Yes	Yes	Yes	Yes
Controls × Time	Yes	Yes	Yes	Yes	Yes
Observations	568,629	106,518	77,490	18,204	189,543
R^2	0.189	0.160	0.154	0.095	0.090

Notes: This table presents heterogeneity in the enforcement decline across subsamples. Dependent variable: log monthly fine value. Post-2023 dummy included. Standard errors clustered at municipality level. Col. (3): Legal Amazon municipalities in top tercile of pre-period enforcement. Col. (4): Mato Grosso only. Col. (5): municipalities below median electorate size. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

5.6 Pre-COVID Window

To directly address the concern that the enforcement decline is a COVID artifact, Table 4 restricts the sample to January 2015 through February 2020, the pre-pandemic Bolsonaro window. Two findings emerge. First, the intensive margin (log value of fines) shows a strong and immediate decline in this window: -0.415 log points in the all-Brazil panel ($p = 0.001$)

and −1.384 in the Legal Amazon panel ($p < 0.001$). This cannot be a pandemic effect—the window ends before Brazil's first COVID case. Second, the extensive margin (fines count) is insignificant and wrong-signed in the pre-COVID window (+0.022, $p = 0.908$ for all Brazil; +0.384, $p = 0.491$ for the Legal Amazon). In the pre-pandemic window, the extensive margin goes to approximately zero, confirming that the count decline documented in Table 1 is a post-2020 phenomenon rather than a 2019 intensive-margin signal. The count-level enforcement decline emerges in 2020 or later, and its timing overlaps with the pandemic. Taken together, the Bolsonaro enforcement collapse began on the intensive margin in 2019 and spread to the extensive margin only in 2020: COVID can plausibly explain the post-2020 count decline but not the 2019 value decline.

To clarify the mechanism behind the pre-COVID intensive-margin decline, Appendix Table A10 decomposes the total fine value result by estimating the specification on log average fine value (total_valor / n_fines) in the pre-COVID window, restricting to municipality-months with at least one fine issued. The average fine amount is not differentially smaller in high-Bolsonaro municipalities; the all-Brazil coefficient is +0.52 ($p = 0.169$) and the Legal Amazon coefficient is +1.21 ($p = 0.076$), both non-negative. The intensive-margin total value decline therefore does not reflect smaller individual fines being issued, but rather a compositional shift toward lower-value infraction categories, consistent with Figure 4, which shows that high-value flora and forest fines collapsed while lower-value fauna and pollution fines held steady. See Appendix Figure A3 for the event study restricted to the pre-COVID window; it is consistent with the tabulated result but imprecise.

5.7 Effect on Deforestation

Did the enforcement decline translate into more deforestation? Table 5 presents reduced-form estimates for the Legal Amazon using annual PRODES deforestation data. Column 2 shows a large and statistically significant increase in forest loss: a one-unit higher Bolsonaro vote share is associated with 40.3 km^2 of additional deforestation per year ($p = 0.010$, clustered

Table 4: Pre-COVID Window (2015m1–2020m2)

	All Brazil		Legal Amazon	
	Fines (1)	Log fine value (2)	Fines (3)	Log fine value (4)
Bolsonaro Vote × Post 2019	0.022	-0.415***	0.384	-1.384***
	(0.190)	(0.130)	(0.557)	(0.387)
Municipality FE	Yes	Yes	Yes	Yes
State × Semester FE	Yes	Yes	Yes	Yes
State Linear Trends	Yes	Yes	Yes	Yes
Observations	286,626	286,626	53,692	53,692
R^2	0.211	0.185	0.175	0.179

Notes: This table presents the main specification restricted to the pre-pandemic window (January 2015–February 2020), which ends before Brazil's first COVID-19 case and therefore isolates the 2019 enforcement decline from any pandemic confound. The dependent variables are the monthly count of fines and the log value of fines; columns (1)–(2) use the all-Brazil panel and (3)–(4) the Legal Amazon. The reported coefficient is Bolsonaro vote share × post-2019. Municipality and state × semester fixed effects, state linear trends, and log-electorate × time throughout. Standard errors clustered at the municipality level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

SE 15.70). Appendix Table A9 shows that this result survives and is actually larger when Mato Grosso is excluded from the Legal Amazon sample: the ex-Mato-Grosso deforestation coefficient is +48.6 km^2/yr ($p = 0.024$, SE 21.48), compared to +40.3 ($p = 0.010$) for the full Legal Amazon. This is a substantive finding: deforestation from the enforcement collapse was not concentrated in Mato Grosso alone—the remaining Amazon states show an even stronger reduced-form relationship between Bolsonaro support and forest loss. The enforcement results, by contrast, are weaker without Mato Grosso (see Appendix Table A9), suggesting that Mato Grosso's agribusiness-frontier municipalities drove the IBAMA fine value decline, while the ultimate deforestation outcomes were distributed more broadly across the Amazon.

Scaled to the cross-sectional standard deviation of vote share and aggregated across the 692 Legal Amazon municipalities, the headline coefficient implies 4,874 km^2 of additional forest lost each year, an area larger than the state of Rhode Island and roughly twenty times the land area of the city of São Paulo. These are hectares that do not grow back on political timescales.

This reduced form is the paper's welfare anchor, and the logic is worth making explicit. The identifying variation, Bolsonaro vote share interacted with the post-2019 period, is the same variation that drives the monthly first stage on fines in Table 1. Column 1 of Table 5 reports the annual analog of that first stage: the point estimate is negative and consistent in sign with the monthly Legal Amazon coefficient (-7.59 fines per unit vote share, $p = 0.335$), though the annual frequency sacrifices precision relative to the monthly panel. Because the deforestation reduced form is measured directly on the outcome of ultimate interest rather than through an instrumented first stage, it absorbs both the enforcement channel and any other channel through which political alignment raises clearing activity (rural credit, land-grab expectations, local agent behavior); the coefficient should therefore be read as an upper bound on the pure deterrence elasticity rather than as a structural parameter. The welfare calculation in Section 6 is anchored on this reduced form, not on the fines coefficient, which is why it does not rescale when the fines specification changes.

Table 5: Enforcement and Deforestation: Reduced Form (Legal Amazon, 2015–2022)

	Annual Fines (1)	Deforestation (km^2) (2)
Bolsonaro Vote × Post 2019	-7.592 (7.870)	40.289** (15.699)
Municipality FE	Yes	Yes
State × Year FE	Yes	Yes
State Linear Trends	Yes	Yes
Log Electorate × Time	Yes	Yes
Observations	6,440	6,440
R^2	0.721	0.834

Notes: This table presents the reduced-form effect of Bolsonaro vote share on deforestation in the Legal Amazon, the welfare anchor of the paper. Legal Amazon, annual panel 2015–2022. Standard errors clustered at municipality level in parentheses. Col. (1): annual fines count. Col. (2): annual PRODES deforestation (km^2). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

6 Mechanisms, Extensions, and Welfare

I now turn to mechanisms behind the enforcement decline, its consequences for deforestation and agricultural expansion, and a welfare calculation.

6.1 Corroborating Evidence: Vertical Dismantlement

The enforcement decline was not confined to the federal agency. I document that the anti-environmental signal propagated into the legislature, a pattern I call "vertical dismantlement." The congressional-rhetoric evidence below is causally identified through a within-deputy difference-in-differences; the municipal-alignment and municipal-institution patterns that follow are descriptive triangulation, presented to show that the federal enforcement collapse coincided with observable changes across levels of government.

6.1.1 Congressional Rhetoric: A Within-Deputy Difference-in-Differences

I analyze speeches from the Brazilian Chamber of Deputies, classifying deputies into ideological blocs—*right-rural* (PL, PP, DEM, PSD, and allied parties) and *left-green* (PT, PSOL, PCdoB, PDT, PSB, PV)—by party affiliation. Figure 5 presents the descriptive time series of environmental and agricultural rhetoric by bloc. The two blocs diverge sharply after Bolsonaro's inauguration: the left-green bloc raises environmental rhetoric and cuts agricultural rhetoric, while the right-rural bloc holds environmental rhetoric flat and intensifies agricultural rhetoric.

To identify whether this divergence reflects a behavioral shift rather than compositional turnover, I restrict to the 287 deputies who served in *both* the pre-Bolsonaro (Legislature 55, 2015–2019) and Bolsonaro-era (Legislature 56, 2019–2023) congresses—of whom 189 belong to the right-rural or left-green blocs (the remainder sit in centrist parties with no clear environmental valence and are dropped)—and estimate a within-deputy difference-in-

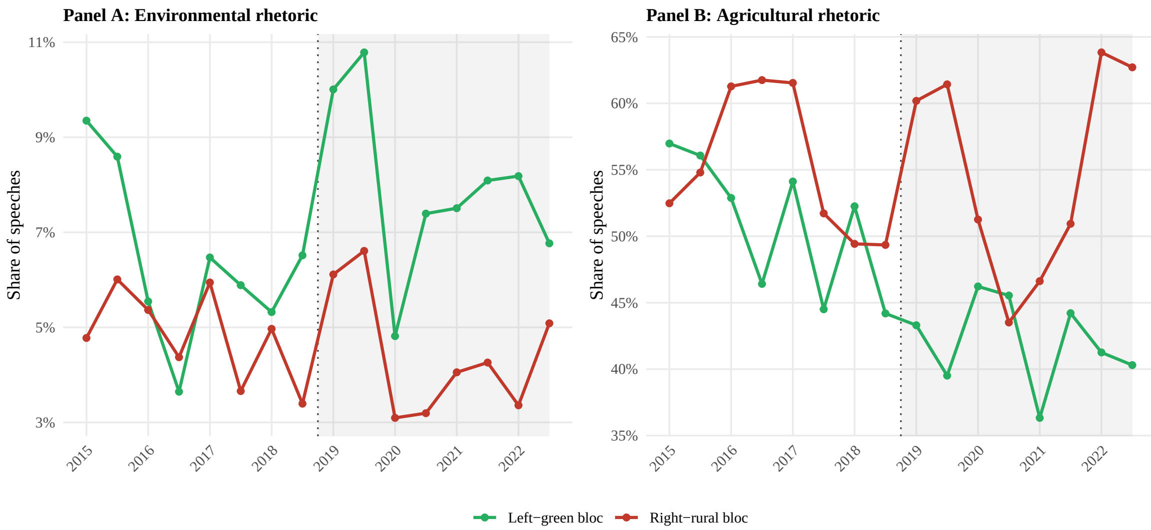

Figure 5: Congressional Rhetoric by Ideological Bloc Over Time

Notes: Deputy-weighted share of speeches mentioning environmental (Panel A) or agricultural (Panel B) keywords, by ideological bloc, by semester. Shaded area and dotted line: Bolsonaro presidency (from January 2019). Keyword lists in Appendix 7.

differences:

$$\text{RhetoricShare}_{d,t} = \alpha_d + \lambda_t + \delta\,(\text{RightRural}_d \times \text{Post}_t) + \varepsilon_{d,t}, \tag{2}$$

where d indexes deputies, t semesters, α_d are deputy fixed effects (each deputy is compared to their own pre-period baseline), and λ_t are semester fixed effects that absorb common time trends. The coefficient δ is the differential within-deputy change in rhetoric for right-rural relative to left-green deputies after the populist transition. Standard errors are clustered at the deputy level. Crucially, the specification uses semester fixed effects rather than party-by-semester fixed effects: the bloc-level realignment is the object of interest, not a nuisance to be absorbed.

The estimand is the *relative* shift between blocs, holding each deputy to their own baseline. The within-deputy gap in agricultural rhetoric widens by 8.2 percentage points after the transition ($\delta = 0.082$, $p = 0.002$), and the gap in environmental rhetoric widens by 2.1 points in the opposite direction ($\delta = -0.021$, $p = 0.083$). What the design identifies is the divergence, not the absolute movement of either bloc, and this is by construction: the semester fixed effects absorb common shocks that move both blocs together, including the COVID-19 pandemic, which compressed topic-specific floor speech across the board after early 2020. The components behind the agricultural gap are symmetric (right-rural deputies raise agricultural rhetoric while left-green deputies cut it); the environmental gap is carried by the left-green bloc raising environmental rhetoric while the right-rural bloc holds flat. In both cases the politically relevant object is the same: the two camps pull apart on exactly the dimensions—agriculture and the environment—that define the enforcement conflict.

Figure 6 plots the event-study coefficients relative to a clean pre-election baseline (2017H2). Pre-trends are flat—joint F-tests on the pre-2018 coefficients do not reject zero ($p = 0.130$ for agriculture, $p = 0.098$ for the environment)—and the divergence emerges around the 2018 election and persists through the presidency. Because the bloc realignment may begin at the election (October 2018) rather than the inauguration, I also estimate a specification dating treatment to the election: the agricultural gap is, if anything, slightly larger ($\delta = 0.093$, $p < 0.001$), consistent with the bloc realigning as soon as its preferred candidate prevailed.

The divergence tracks ideological-bloc membership rather than constituency-level Bolsonaro support. Replacing the binary bloc with a continuous, municipality-vote-weighted measure of each deputy's constituency Bolsonaro exposure yields point estimates of the same sign but no longer statistically distinguishable from zero ($p = 0.21$ for agriculture, $p = 0.80$ for the environment). I read this as consistent with a bloc-level realignment, the agribusiness-aligned camp moving as a coalition once its candidate won the presidency, rather than a bottom-up response scaled to district-level Bolsonaro support, though the noisier continuous measure also has less power, so I do not lean heavily on the contrast.

6.1.2 Municipal Political Alignment

If the enforcement decline reflects a top-down political signal, it should be amplified in municipalities where local political leaders are also aligned with the agribusiness coalition. I

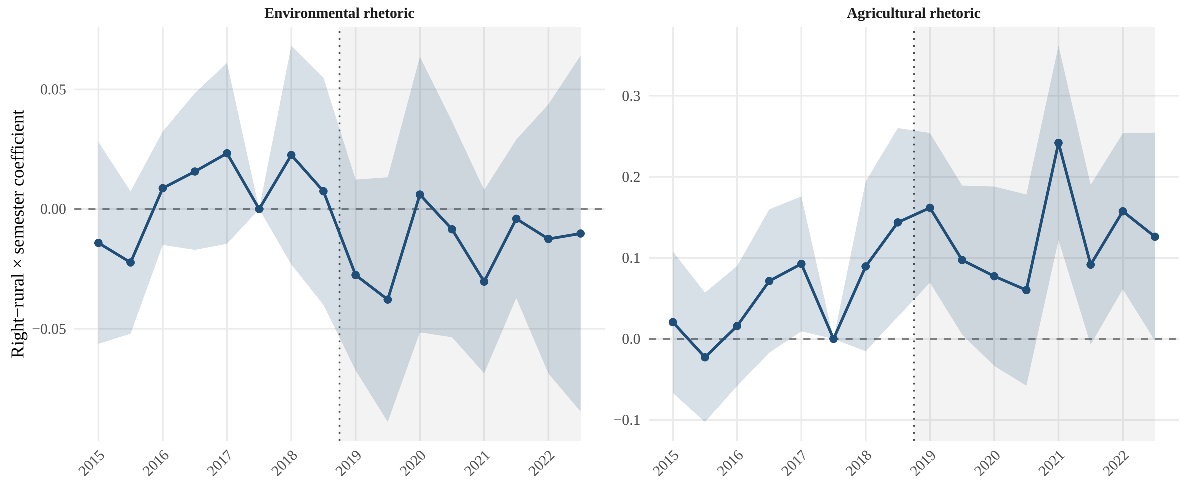

Figure 6: Within-Deputy Rhetoric Event Study (Right-Rural × Semester)

Notes: Event-study coefficients from Equation 2, replacing Post_t with semester interactions (reference: 2017H2, the second half of 2017, the last full year before the election). Sample: deputies serving in both legislatures (189 in the right-rural/left-green estimating sample). Deputy and semester fixed effects. 95% CI from deputy-clustered standard errors. Shaded: Bolsonaro presidency.

test this by interacting the main DiD with the party of the mayor elected in 2016. Using TSE municipal election data, I classify mayors into right-rural (PL, PP, DEM, PSD, and allied parties) and left-green (PT, PSOL, PCdoB, PDT, PSB, PV) blocs.

In the Legal Amazon, the enforcement decline is largest in municipalities with right-aligned mayors: the coefficient on BolsoVote × Post is -1.082 ($p < 0.05$) for municipalities with right-rural mayors, compared to -0.929 (insignificant) for those with left-green mayors and -0.677 for the full sample. This suggests that political alignment at the municipal level amplifies the federal enforcement decline—when both the president and the mayor are aligned with agribusiness, the dismantlement of environmental governance is strongest. This result should be interpreted as suggestive rather than causal: the mayor party classification is endogenous to the same political-economy forces that drive the main result, and the MUNIC DiD (two survey waves, 2017 vs. 2020) does not permit pre-trend testing. These patterns corroborate the main enforcement finding but do not establish an independent causal chain.

6.1.3 Municipal Environmental Institutions

Using IBGE's MUNIC survey (2017 vs. 2020), I examine whether municipal environmental institutions also deteriorated. Table 6 reports DiD estimates for seven measures of municipal environmental capacity. The formal existence of councils, funds, and licensing capacity did not change differentially. However, the *functioning* of these institutions did: municipalities above the median Bolsonaro vote share held 1.38 fewer environmental council meetings per year ($p < 0.05$), a decline of approximately one-third from the pre-period mean. The institutions formally survived but stopped being active—a pattern consistent with, though not conclusively established as caused by, a political signal that de-prioritized environmental governance at all levels.

Table 6: Municipal Environmental Governance (MUNIC 2017 vs. 2020, Legal Amazon)

	Env Agency (1)	Exclusive Secretary (2)	Env Council (3)	Env Fund (4)	Does Licensing (5)	Federal Training (6)	Council Meetings (7)
High Bolsonaro	0.002	-0.037	0.138***	0.121***	0.212***	0.125***	1.196***
	(0.018)	(0.045)	(0.038)	(0.044)	(0.041)	(0.047)	(0.426)
Post 2019	-0.090***	0.050	-0.017	0.052*	0.052*	-0.026	-0.238
	(0.018)	(0.033)	(0.031)	(0.031)	(0.031)	(0.033)	(0.293)
High Bolsonaro × Post	0.023	-0.023	-0.028	0.038	-0.044	-0.038	-1.382***
	(0.025)	(0.050)	(0.046)	(0.052)	(0.049)	(0.053)	(0.524)
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,384	1,384	1,384	1,384	1,384	1,384	962
R^2	0.056	0.141	0.120	0.124	0.192	0.027	0.068

Notes: This table presents difference-in-differences estimates for measures of municipal environmental governance capacity. DiD comparing municipalities above vs. below median Bolsonaro vote share, 2017 vs. 2020 (IBGE MUNIC survey). Legal Amazon only. Heteroskedasticity-robust standard errors. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

6.2 The Economic Tradeoff: Agricultural Expansion

Reduced enforcement created space for agricultural frontier expansion. Using IBGE's Municipal Agricultural Production data for Legal Amazon municipalities, I test whether soy, corn, and cattle expanded differentially in pro-Bolsonaro areas. Table 7 shows that both soy planted area (+1.33 log points, $p < 0.05$) and soy production value (+1.62 log points, $p < 0.02$) increased significantly more in high-Bolsonaro municipalities during the Bolsonaro period. The effects on corn and cattle are small and insignificant, suggesting the relevant agricultural margin was soy, consistent with the soy-driven deforestation frontier documented by Rajão et al. (2020). The state-by-year fixed effects in this specification absorb state-level commodity price cycles—including the differential soy price shocks from the 2018–2019 US-China trade war, which affected Brazilian soy exports broadly—so the differential expansion in high-Bolsonaro municipalities identifies municipality-level effects rather than state-level commodity dynamics. The fact that only soy, the crop most associated with new land clearing at the Amazon frontier, responded to the enforcement decline, while corn (grown on existing farmland) and cattle (a more established activity) did not, provides additional evidence that reduced enforcement specifically enabled agricultural expansion on newly deforested land.

Table 7: Agricultural Expansion (Legal Amazon)

	Log Soy Area (1)	Log Soy Value (2)	Log Corn Area (3)	Log Cattle Herd (4)
Bolsonaro Vote × Post 2019	1.331**	1.618**	-0.112	-0.118
	(0.587)	(0.687)	(0.101)	(0.097)
Municipality FE	Yes	Yes	Yes	Yes
State × Year FE	Yes	Yes	Yes	Yes
Controls × Time	Yes	Yes	Yes	Yes
Observations	62,280	62,280	62,280	62,280
R^2	0.942	0.931	0.879	0.092

Notes: This table presents the effect of Bolsonaro vote share on agricultural expansion (soy, corn, and cattle) in the Legal Amazon. Legal Amazon municipalities, annual panel 2015–2023. Standard errors clustered at municipality level. Post-2023 dummy included. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

6.3 Welfare Implications

The reduced-form estimate on deforestation implies a large welfare cost. The Legal Amazon reduced form (column 2 of Table 5) yields a deforestation coefficient of 40.29 km^2 per year per unit of 2018 Bolsonaro vote share (clustered SE 15.70, $p = 0.010$). Scaling by the cross-sectional standard deviation of vote share in the Legal Amazon (0.1748) yields approximately 7.04 km^2 of additional annual deforestation per municipality at a one-standard-deviation contrast, or roughly 4,874 km^2 per year aggregated across the 692 Legal Amazon municipalities. Translating via the EPA social cost of carbon (\$51 per tCO_2) and a tropical-forest carbon density of 150 tCO_2 per hectare (\$765,000 per km^2) yields an annual carbon cost on the order of **\$3.7 billion per year** (a 95% interval of roughly \$0.9–\$6.6 billion that propagates only the reduced-form standard error). This figure is best read as an illustrative magnitude rather than a precise estimate: it conditions on point values for the social cost of carbon and the carbon density and on linear aggregation across municipalities, none of whose uncertainty enters the reported interval. Table 8 reports the full calculation, and Appendix Table A11 shows how it moves with the social cost of carbon.

Appendix Table A11 reports sensitivity of the welfare figure to the social cost of carbon: the range spans \$1.46 billion per year at \$20/tCO_2 to \$13.89 billion per year at the Biden-era \$190/tCO_2. This figure is anchored on the deforestation reduced form and is mechanically independent of the fines-count first stage; it therefore does not rescale when the fines specification changes. Panel B of Table 8 reports a per-unit (zero-to-one) linear-extrapolation upper bound for transparency, but this should be read as an upper bound under linear extrapolation and not as a point prediction. The calculation abstracts from biodiversity, watershed, and health margins, so \$3.7 billion is itself conservative as a total welfare cost.

6.4 Policy Implications

Three observations follow from the welfare calculation and the identification. First, the Lula administration's enforcement recovery can be read directly from the data. The coefficient ϕ on $\text{BolsoVote}_m \times \text{Post2023}_t$ in Equation 1 captures the differential enforcement trajectory under Lula relative to the pre-period. The estimated ϕ for log fine value is -0.171 (SE 0.138, $p = 0.216$) in the all-Brazil panel and -0.378 (SE 0.470, $p = 0.421$) in the Legal Amazon, both statistically insignificant. This means the Lula recovery was not differentially concentrated

Table 8: Welfare Calculation

Quantity	Value
SD of Bolsonaro vote share (Legal Amazon)	0.173
Deforestation coefficient (km^2 per unit vote share)	40.3
Deforestation coefficient (km^2 per 1-SD vote share)	7.0
Panel A: Per-1-SD contrast (preferred)	
Aggregate additional deforestation (km^2/yr)	4,825
Annual carbon cost (USD billion)	\$3.69
Panel B: Per-unit contrast (upper bound, linear extrapolation)	
Aggregate additional deforestation (km^2/yr)	10,355
Annual carbon cost (USD billion)	\$7.92

Notes: This table presents the welfare calculation. It translates the Legal Amazon deforestation reduced form (Table 5) into additional annual deforestation and carbon damages, valued at the EPA social cost of carbon (\$51/tCO_2) and a tropical-forest carbon density of 150 tCO_2/ha. Panel A reports the per-standard-deviation contrast; Panel B a per-unit linear-extrapolation upper bound. The 95% confidence interval propagates the reduced-form standard error.

in high-Bolsonaro municipalities: aggregate enforcement increased substantially after 2023 (consistent with the 147% increase in IBAMA fines reported relative to 2019–2022 averages), but this recovery was uniformly distributed across the political spectrum rather than targeted toward previously suppressed municipalities. The asymmetry is substantively interesting: the enforcement collapse under Bolsonaro was politically targeted (high-Bolsonaro areas lost disproportionately more), but the restoration under Lula was institutional (recovered uniformly, consistent with budget restoration and leadership replacement rather than politically targeted remediation). Political reversal is feasible, but it appears to work through institutions, not through the same municipal-level political alignment that drove the decline.

Second, the welfare calculation implies a cost-effectiveness ratio for enforcement investment. IBAMA's executed enforcement budget in 2019 was approximately R\$600 million (\$120 million at the period exchange rate). At roughly \$3.7 billion in annual carbon damages from the enforcement collapse, the implied ratio of welfare loss to enforcement cost is approximately 31:1—each dollar of enforcement budget cut generated roughly \$31 in carbon damages. This ratio is sensitive to the SCC used (under the Biden-era \$190/tCO_2, the ratio rises to approximately 115:1) and to the Mato Grosso concentration caveat discussed above, but even under conservative assumptions it suggests enforcement is substantially underfinanced relative to its social value.

Third, the suggestive selectivity pattern points to a policy implication: increasing total enforcement spending may be insufficient to restore environmental protection if the apparent de-prioritization of flora and forest categories is not also reversed. The fauna and pollution categories declined less, consistent with their being less salient to the coalition. Restoration may require not just budget but re-prioritization of the categories that bind on the agribusiness coalition.

7 Conclusion

Exploiting municipal variation in support for Jair Bolsonaro in the 2018 Brazilian presidential election, this paper documents a sharp, differential, and cleanly identified collapse in federal environmental enforcement during the Bolsonaro presidency. The log value of fines issued by IBAMA falls by 0.57 log points per unit of Bolsonaro vote share in the all-Brazil panel and by 1.31 log points in the Legal Amazon, with both effects appearing immediately in 2019 and surviving restriction to a clean pre-pandemic window. The Legal Amazon fines count falls by 0.96 per unit vote share ($p = 0.042$). The Legal Amazon enforcement estimates are concentrated in Mato Grosso and Pará; the deforestation reduced form on which the welfare calculation rests, by contrast, is broadly distributed across the Amazon and is larger when Mato Grosso is excluded. Scaling that reduced-form impact on deforestation by a one-standard-deviation contrast and aggregating across the 692 Legal Amazon municipalities implies roughly 4,874 km^2 of additional annual Amazon deforestation and, illustratively, on the order of \$3.7 billion per year in carbon damages at the EPA social cost of carbon.[4]

[4] This calculation uses the Obama-era central estimate of \$51/tCO_2 (Interagency Working Group on Social Cost of Greenhouse Gases, 2016); the Biden-era recalculation of \$190/tCO_2 (U.S. Environmental Protection Agency, 2023) would imply roughly \$13.8 billion per year. The paper uses the lower figure throughout to facilitate comparison with the existing environmental economics literature. As discussed in Section 6, the

The evidence is most consistent with a specific mechanism rather than a diffuse decline in state capacity. The decline is not uniform across categories: it concentrates in flora and forest violations—the instruments that bind on the agribusiness coalition—and is weaker for fauna and pollution, although the category-level differences are suggestive rather than individually significant. Embargoes, a separate instrument used by more specialized operations, also do not respond. The pattern is most consistent with targeted de-prioritization of a centralized enforcement agency along the infraction categories whose enforcement hurts the populist coalition, with the cleanly identified result being the intensive-margin enforcement collapse and its translation into deforestation. The policy implication is precise: restoring Amazon protection requires not simply increasing IBAMA's budget, but specifically reversing the political de-prioritization of flora and forest enforcement—the categories that bind on the agribusiness coalition and that drove the welfare loss documented here. The institutions survive; the paperwork survives; what stops is the punishment of the violations the governing coalition wants unpunished.

This matters beyond Brazil because the institutional configuration that made the capture possible is not unique. Environmental enforcement concentrated in a single federal agency, whose leadership and operational priorities are set by the executive without legislative check, is the default arrangement in much of Latin America, much of Africa, and a growing share of Southeast Asia. The implication is direct: when a single elected executive can command a national enforcement agency, populist capture has a ready-made channel—one that does not require new laws, does not require defunding, and does not require visible bureaucratic resistance to work. Environmental federalism that concentrates enforcement authority in the executive is fragile precisely in the ways this paper measures.

The generalizability of these findings turns on which institutional features are necessary for the mechanism to operate. The mechanism requires a centralized enforcement agency whose leadership and operational priorities are set by the executive without a meaningful

dollar figure is an illustrative magnitude: it conditions on point values for the carbon price and density and ignores their uncertainty.

legislative check. Three countries provide analogous designs where a similar study could, in principle, be run. Indonesia's KLHK (Directorate General for Law Enforcement, Ministry of Environment and Forestry) is a presidentially appointed body with unitary command over forest enforcement, facing ongoing deforestation pressure and contested resource-nationalist politics. Peru's OEFA (Organismo de Evaluación y Fiscalización Ambiental) is a single federal enforcement body under the executive, with no independent environmental judiciary of binding authority. The DRC's ICCN (Institut Congolais pour la Conservation de la Nature) operates under ministerial appointment with no independent legislative oversight, in a context of active deforestation pressure and politically driven conservation politics. All three satisfy the structural requirement: executive-appointed, centralized, unitary enforcement with no formal countervailing institution.

Conversely, the mechanism is attenuated where blocking conditions hold. Federal systems with independent state-level enforcement agencies—India's State Pollution Control Boards, for example—fragment the executive's command over the enforcement apparatus and create veto points that populist capture cannot easily overcome. Countries with environmental courts that have standing to block executive direction (Costa Rica's Sala IV, Colombia's Constitutional Court) introduce a legal constraint that changes the political cost of de-prioritization. Settings where enforcement is substantially co-governed with NGOs or privatized introduce outside monitors with independent incentives to expose selective enforcement. The $3.7 billion annual welfare number is not directly portable to these contexts: it is anchored on PRODES deforestation satellite data and calibrated to the EPA social cost of carbon for a US welfare numeraire. The *methodology*, however, is portable. Any researcher with equivalent enforcement microdata, administrative fine records by infraction category, and satellite-based land-use change measures could apply the same reduced-form design and welfare translation to another country with the relevant institutional features.

Concretely, 4,874 km^2 per year is an area larger than the state of Rhode Island, or roughly the combined footprint of greater Los Angeles, each year, cleared from the world's largest tropical forest. That is the physical quantity a single election bought. The trees do not grow back on the timescale of a presidential term.

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Appendix

A. Pre-Period Balance

Table A1 presents pre-period (2015–2018) municipality characteristics by quartile of Bolsonaro's 2018 vote share. Municipalities in higher quartiles have larger electorates and substantially higher enforcement levels, reflecting the concentration of IBAMA activity in agricultural frontier areas.

Table A1: Pre-Period Municipality Characteristics by Bolsonaro Vote Quartile

Quartile	N	Vote Share	Electorate	% Amazon	Fines/Mo	Embargoes/Mo	Value (R$1K)
1	1063	0.137	9996	13.5	0.110	0.004	9.4
2	1063	0.295	16275	25.0	0.198	0.024	42.4
3	1062	0.477	29424	12.8	0.236	0.032	46.6
4	1062	0.623	38796	13.7	0.368	0.039	73.3

Notes: This table presents pre-period balance: municipality characteristics by quartile of Bolsonaro vote share. Pre-period (2015–2018) municipality characteristics by quartile of Bolsonaro 2018 first-round vote share. N: municipalities per quartile. Electorate: total registered voters (TSE 2018). % Amazon: share in Legal Amazon. Fines/Mo and Embargoes/Mo: monthly mean per municipality. Value: mean monthly fine value in R$1,000.

B. Robustness

Table A2 presents the main result under alternative specifications. Column 1 reproduces the baseline. Column 2 clusters standard errors at the state level (27 clusters) rather than the municipality level. Column 3 drops state-specific linear trends. Column 4 uses state-by-year fixed effects instead of state-by-semester. The coefficient is stable across specifications, ranging from −0.187 to −0.208.

C. Enforcement Decomposition

Table A3 decomposes the enforcement decline by infraction type (flora and forest, fauna and fishing, pollution) and action type (routine, operations). The largest decline is in flora and forest fines (−0.237), consistent with the selectivity mechanism, while fauna and fishing (−0.105), pollution (−0.021), routine (−0.042), and operations (+0.020) are smaller in magnitude. The disaggregated monthly category counts are noisier than the aggregate fine-value outcome, so no single category

Table A2: Robustness: Alternative Specifications

	Baseline (1)	State-Level Clustering (2)	No State Trends (3)	Year FE (not Semester) (4)
Bolsonaro Vote × Post 2019	-0.187**	-0.187*	-0.208**	-0.187**
	(0.094)	(0.096)	(0.093)	(0.094)
Municipality FE	Yes	Yes	Yes	Yes
State × Semester FE	Yes	Yes	Yes	
State × Year FE				Yes
State Linear Trends	Yes	Yes		Yes
Controls × Time	Yes	Yes	Yes	Yes
Clustering	Municipality	State	Municipality	Municipality
Observations	568,629	568,629	568,629	568,629
R^2	0.189	0.189	0.189	0.187

Notes: This table presents the main result under alternative specifications (clustering, fixed-effect structure, trends). Dependent variable: log monthly fine value. All-Brazil panel. Post-2023 dummy included but not shown. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

is individually significant at conventional levels. To test selectivity directly, I estimate the three category effects jointly in a fully category-saturated stacked specification (which recovers the cross-equation covariance) and test equality of the flora/forest, fauna, and pollution treatment effects. Equality is *not* rejected: joint $\chi^2(2) = 2.00$, $p = 0.37$ for all Brazil, and $\chi^2(2) = 1.56$, $p = 0.46$ for the Legal Amazon. The pairwise flora-minus-pollution difference is -0.22 ($p = 0.19$) in the full sample and -0.79 ($p = 0.30$) in the Legal Amazon. The point estimates are ordered as the selectivity hypothesis predicts—largest for flora and forest, smallest for pollution—and the gap widens in the Legal Amazon where the overall effect concentrates, but the category-level differences are not statistically significant. The decomposition is therefore informative about the *pattern* rather than delivering category-by-category inference, and I treat selectivity as suggestive throughout.

D. Additional Figures

E. Pre-COVID Event Study

Figure A3 shows the event study for the fines count restricted to January 2015 through February 2020. The pre-COVID window is consistent with the tabulated result in Table 4: the count-level

Table A3: Enforcement Decomposition by Infraction and Action Type

	By infraction type			By action type	
	Flora & forest	Fauna & fishing	Pollution	Routine	Operations
	(1)	(2)	(3)	(4)	(5)
Bolsonaro Vote × Post 2019	-0.237	-0.105	-0.021	-0.042	0.020
	(0.161)	(0.075)	(0.031)	(0.099)	(0.148)
Observations	568,629	568,629	568,629	568,629	568,629
R^2	0.295	0.225	0.085	0.217	0.185

Notes: This table presents the enforcement decline decomposed by infraction category and by action type. Dependent variable: monthly count of fines in each category. Categories from IBAMA's infraction-type and action-type classifications; "Flora & forest" bundles vegetation and forest-clearing infractions (the raw data does not support a clean split of deforestation from flora). All columns include municipality FE, state × semester FE, state linear trends, and log electorate × time. Post-2023 (Lula) interaction included. SE clustered at municipality level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

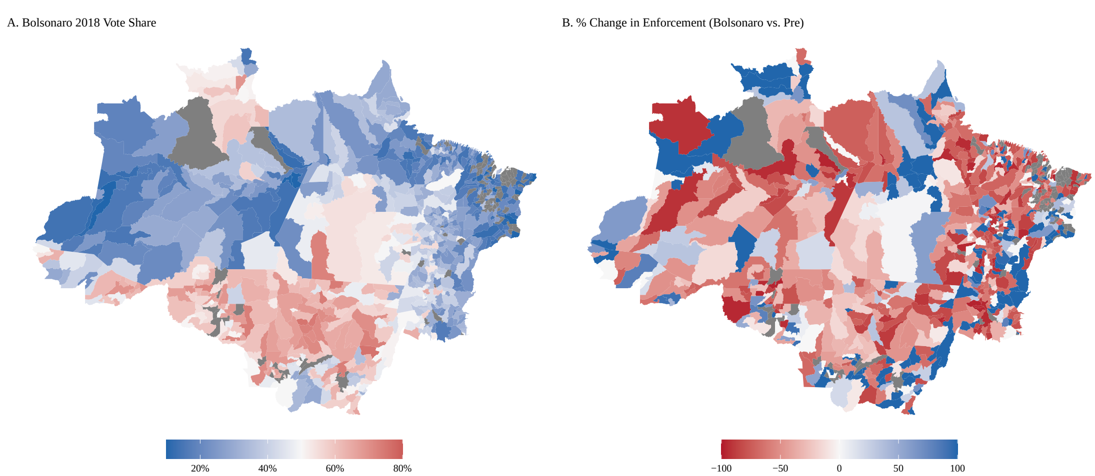

Figure A1: Maps: Bolsonaro Vote Share and Enforcement Change in the Legal Amazon

Notes: Left: Bolsonaro 2018 first-round vote share by Legal Amazon municipality (quartile shading). Right: change in mean monthly IBAMA fines between the Bolsonaro period (2019–2022) and the pre-period (2015–2018). Source: TSE municipal election results; IBAMA Dados Abertos.

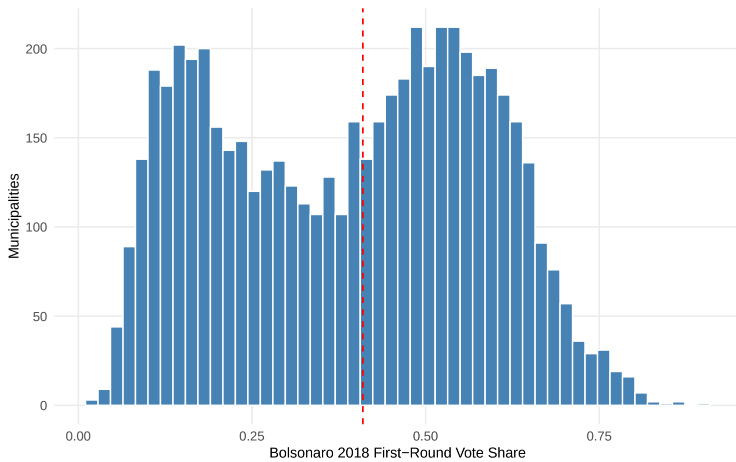

Figure A2: Distribution of Bolsonaro 2018 First-Round Vote Share

Notes: Distribution of Bolsonaro 2018 first-round vote share across 5,708 Brazilian municipalities. Dashed vertical line: national median (0.399). Source: TSE.

coefficients are noisy and statistically indistinguishable from zero, consistent with the interpretation in the main text that the extensive-margin decline only becomes visible after 2020.

F. Bad-Control Sensitivity: Pre-Period Enforcement × Time

The headline specification in Table 1 omits a municipality-level pre-period enforcement mean interacted with time. I treat this interaction as a *bad control*: pre-period enforcement is itself an outcome of the same data-generating process that produces Bolsonaro vote shares, and in the Legal Amazon sample it is near-collinear with the treatment interaction and absorbs identifying variation.

Table A4 reports the Table 1 specification with and without this control in adjacent columns. In the all-Brazil panel the log-value coefficients are nearly identical (-0.569 without vs. -0.562 with); the fines-count coefficient attenuates mildly (-0.225 vs. -0.187) and its standard error tightens, shifting significance from 10% to 5%. In the Legal Amazon panel, adding the control more than halves the fines-count coefficient and strips its significance (-0.963, $p = 0.042$, without vs. -0.452, $p = 0.234$, with), while log value is essentially unchanged (-1.313 vs. -1.244). The pre-trend F-test on the semester-by-treatment coefficients is essentially identical across the two specifications ($p = 0.328$ without, $p = 0.330$ with), so the control is not doing work on parallel trends—it is

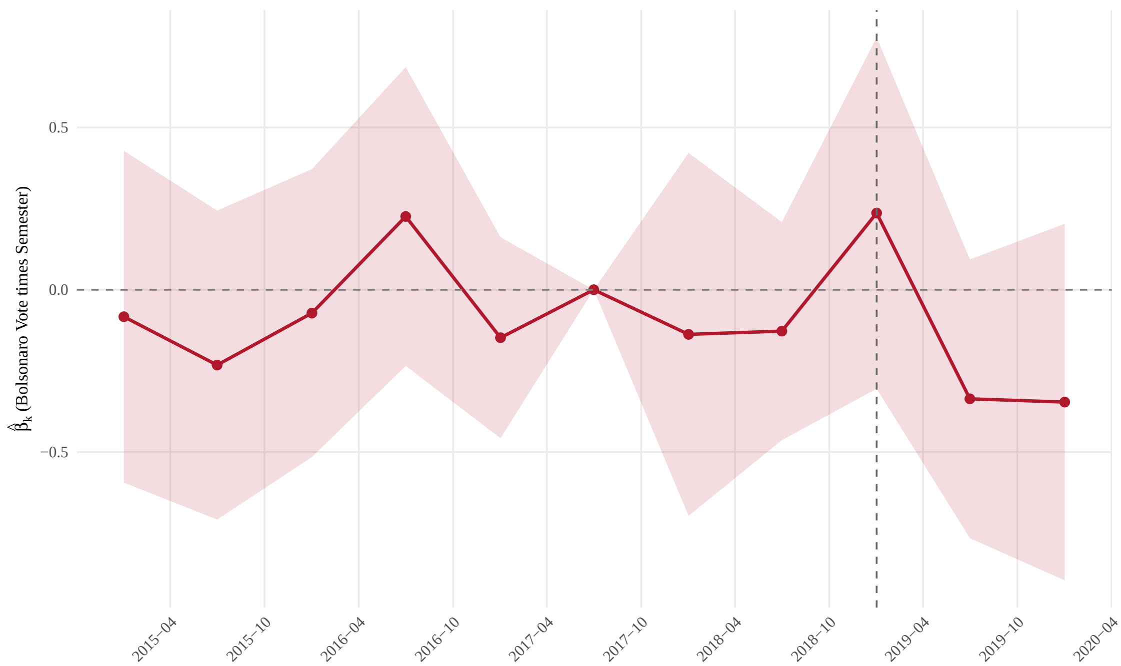

Figure A3: Pre-COVID Event Study (Fines Count, 2015m1–2020m2)

Notes: Event study restricted to January 2015–February 2020. Continuous treatment (Bolsonaro 2018 vote share). Reference: 2017H2. Same controls and fixed effects as Figure 3. 95% CI.

absorbing legitimate treatment variation in the Legal Amazon sample, where pre-period enforcement and Bolsonaro vote share are most tightly correlated. The conservative specification is reported here for transparency, but the headline in Table 1 uses the cleaner spec.

Table A4: Bad-Control Sensitivity: With and Without Pre-Period Enforcement × Time

	All Brazil				Legal Amazon			
	Fines		Log value		Fines		Log value	
	With (1)	Without (2)	With (3)	Without (4)	With (5)	Without (6)	With (7)	Without (8)
Bolsonaro Vote × Post 2019	-0.187**	-0.225*	-0.562***	-0.569***	-0.452	-0.963**	-1.244***	-1.313***
	(0.094)	(0.130)	(0.111)	(0.112)	(0.379)	(0.472)	(0.377)	(0.378)
Pre-Period Enforcement × Time	Yes	No	Yes	No	Yes	No	Yes	No
Municipality & State × Semester FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State Linear Trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	568,629	568,629	568,629	568,629	106,518	106,518	106,518	106,518

Notes: This table presents the headline specification with and without the pre-period enforcement × time control, which control (it is an outcome of the same process that generates Bolsonaro vote share). For each outcome and sample, the "Wit and the "Without" column is the headline. Adding the control leaves the all-Brazil and log-value estimates essentially unchang Amazon fines-count coefficient, confirming it absorbs treatment variation rather than improving parallel trends. The repor vote share × post-2019; a post-2023 interaction and log-electorate × time are included throughout. Standard errors clustered parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

G. Additional Robustness Suite

This appendix collects four robustness checks flagged as minor in the second-pass identification review: a tercile dose-response, Oster (2019) bounds on selection-on-unobservables, two placebo tests (wrong-timing and wrong-group), and a leave-one-state-out sensitivity for the four headline outcomes.

G.1 Dose-response (terciles). Table A5 replaces the continuous Bolsonaro vote share with terciles (bottom tercile omitted). The point estimates are monotonic in every column: the middle tercile is about one-third of the top tercile in magnitude, and the top tercile carries essentially all of the effect. In the Legal Amazon panel the top-tercile fines coefficient is -0.415 ($p = 0.020$) versus a statistically indistinguishable -0.114 for the middle tercile, and the log-value coefficients are -0.469 (top, $p = 0.002$) versus -0.114 (middle, $p = 0.253$). The pattern rules out a purely linear dose-response and is consistent with a regime effect concentrated in the highest-vote-share third of municipalities.

Table A5: Dose-Response: Bolsonaro Vote Share Terciles

	Fines (1)	Log Value (2)	Fines (LA) (3)	Log Value (LA) (4)
Tercile 2 (middle) × Bolsonaro	-0.0724**	-0.1091***	-0.1137	-0.1144
	(0.0364)	(0.0414)	(0.0858)	(0.0999)
Tercile 3 (top) × Bolsonaro	-0.1371***	-0.2164***	-0.4146**	-0.4685***
	(0.0441)	(0.0477)	(0.1783)	(0.1506)
Tercile 2 (middle) × Lula	-0.0164	-0.0399	0.0295	0.1117
	(0.0467)	(0.0478)	(0.1136)	(0.1234)
Tercile 3 (top) × Lula	-0.0435	-0.1011*	-0.2787	-0.2993
	(0.0577)	(0.0579)	(0.2277)	(0.1927)
Log Electorate × Time	-0.0016***	-0.0003*	-0.0028*	-0.0001
	(0.0005)	(0.0002)	(0.0014)	(0.0005)
Municipality FE	Yes	Yes	Yes	Yes
State × Semester FE	Yes	Yes	Yes	Yes
State Linear Trends	Yes	Yes	Yes	Yes
Observations	568,629	568,629	106,518	106,518
R^2	0.172	0.174	0.152	0.175

Notes: This table presents the dose-response of enforcement to Bolsonaro 2018 vote share entered as terciles (bottom tercile omitted). Cols. (1)–(2): all Brazil; cols. (3)–(4): Legal Amazon. Same FE and controls as Table 1. Standard errors clustered at municipality level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

G.2 Oster (2019) bounds. Table A6 reports Oster-style selection-on-unobservables bounds for the four headline coefficients. I compute the restricted model (treatment + fixed effects only) and the full headline model (adding log electorate × time), and solve for the δ that would drive the treatment effect to zero under $R^2_{\max} = \min(1.3 \times R^2_{\text{full}}, 1)$, the Oster convention. All four outcomes clear the $\delta > 1$ bar: $\delta = 2.20$ for all-Brazil fines, 4.76 for all-Brazil log value, 5.19 for Legal Amazon fines, and 1.20 for Legal Amazon log value. The Legal Amazon log value is the closest call — selection on unobservables would need to be 1.2× selection on observables to null it — but the bound clears. Given the richness of the included controls (municipality fixed effects, state×semester fixed effects, and state linear trends), a δ of 1.2 is non-trivial but not implausible; it nonetheless satisfies the conventional threshold for a robust result. The identified set at $\delta = 1$ keeps all four coefficients negative.

Table A6: Oster (2019) Bounds on Selection on Unobservables

	β^{restr}	β^{full}	R^2_{restr}	R^2_{full}	δ (null)	$\beta(\delta{=}1)$
Fines (all Brazil)	-0.519	-0.225	0.000	0.001	2.20	-0.122
Log Value (all Brazil)	-0.623	-0.569	0.000	0.000	4.76	-0.449
Fines (Legal Amazon)	-1.383	-0.963	0.000	0.001	5.19	-0.777
Log Value (Legal Amazon)	-1.334	-1.313	0.000	0.000	1.20	-0.217

Notes: This table presents Oster (2019) selection-on-unobservables bounds for the four headline coefficients. β^{restr}: treatment coefficient with fixed effects only. β^{full}: headline specification (adds log electorate × time). $R^2_{\max} = \min(1.3 \times R^2_{\text{full}}, 1)$ on the within-transformation. δ is the value of selection on unobservables (relative to observables) required to set the treatment effect to zero; $\delta > 1$ is the Oster convention for a robust result. $\beta(\delta{=}1)$ is the identified-set point under equal selection.

G.3 Placebo tests. Table A7 reports two wrong-timing placebos. Column 1 assigns treatment at January 2016 (the original placebo); column 2 assigns treatment at January 2017 (post-Temer inauguration). In column 1, all four coefficients are small and statistically indistinguishable from zero ($p = 0.485$ to 0.921), confirming the specification does not mechanically produce effects. In column 2, the fines-count and Legal Amazon coefficients are again insignificant; the all-Brazil log fine value coefficient is -0.264 ($p = 0.038$). We interpret this as a Temer-period effect rather than a specification failure: Michel Temer took office in August 2016 and also replaced IBAMA leadership and reduced enforcement budgets (Escobar, 2020). That municipalities with higher Bolsonaro vote shares (which correlate with agro-frontier areas that also supported Temer) show an earlier enforcement decline is consistent with the political economy of Brazilian agribusiness, not with a spurious correlation. To test this formally rather than by eyeballing, I estimate the Temer-period and Bolsonaro-period effects jointly in a single pre-COVID regression (2015m1–2020m2) with both vote-share interactions and the headline controls. The Bolsonaro-period log-value decline (-0.542, $p = 0.001$) is significantly larger in magnitude than the Temer-period decline (-0.231, $p = 0.068$): the difference is -0.311 (SE 0.130, $p = 0.017$ two-sided). The Bolsonaro-period effect is therefore not merely a continuation of the Temer-period trend, consistent with Bolsonaro's more aggressive dismantlement. Table A8 reports the wrong-group placebo (Haddad vote share): all four coefficients flip positive and are significant at 1%, the correct falsification pattern.[5]

Table A7: Placebo: Wrong Treatment Timing (January 2016 and January 2017)

	Jan. 2016 placebo (1)	Jan. 2017 placebo (2)
Fines (all-Brazil)	0.114	-0.078
	(0.163)	(0.196)
Log value (all-Brazil)	0.014	-0.264**
	(0.139)	(0.127)
Fines (Legal Amazon)	0.268	0.586
	(0.551)	(0.687)
Log value (Legal Amazon)	0.248	0.301
	(0.403)	(0.373)
Municipality FE	Yes	Yes
State $\times$ Semester FE	Yes	Yes
State Linear Trends	Yes	Yes

Notes: This table presents two wrong-timing placebo tests, assigning a placebo treatment at January 2016 (column 1) and January 2017 (column 2), before the Bolsonaro transition. Sample: 2015m1–2018m12. Standard errors clustered at municipality level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Placebo treatment assigned at January 2016 (col. 1) and January 2017 (col. 2). January 2016 coincides with Lava Jato peak and pre-impeachment period; January 2017 is the post-Temer stabilization period. Standard errors clustered at municipality level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

[5] Bolsonaro and Haddad 2018 first-round vote shares are mechanically negatively correlated across municipalities (correlation ≈ -0.95 in the sample), reflecting the two-candidate dynamics of Brazil's first round. The Haddad placebo test is therefore not fully independent variation — it largely recovers $-1\times$ the Bolsonaro estimate under the null of a genuine mechanism. The test's value lies in confirming that the Bolsonaro result is not picking up a generic correlate of political competition intensity, since a non-mechanism confound would not necessarily predict the sign flip.

Table A8: Placebo: Wrong Treatment Group (Haddad 2018 Vote Share)

	Fines (1)	Log Value (2)	Fines (LA) (3)	Log Value (LA) (4)
Log Electorate × Time	-0.0016***	-0.0003*	-0.0028*	-0.0002
	(0.0005)	(0.0002)	(0.0015)	(0.0005)
Haddad (PT) Vote × Bolsonaro Period	0.3165***	0.4451***	1.280***	1.005***
	(0.1146)	(0.0979)	(0.4936)	(0.3787)
Haddad (PT) Vote × Lula Period	-0.0342	0.1288	0.1213	0.2073
	(0.1392)	(0.1158)	(0.5295)	(0.4446)
Municipality FE	Yes	Yes	Yes	Yes
State × Semester FE	Yes	Yes	Yes	Yes
State Linear Trends	Yes	Yes	Yes	Yes
Observations	568,629	568,629	106,518	106,518
R^2	0.172	0.174	0.152	0.175

Notes: This table presents a wrong-group placebo test. Haddad (PT) 2018 first-round vote share replaces Bolsonaro vote share, with real 2019 treatment timing. A mechanism-driven effect should flip sign. Cols. (1)–(2): all Brazil; cols. (3)–(4): Legal Amazon. Standard errors clustered at municipality level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

G.4 Leave-one-state-out. Figure A4 re-estimates the four headline coefficients dropping one Brazilian state at a time. Each panel shows the 27 coefficient–confidence-interval pairs; the dotted red line is the baseline full-sample estimate. No drop flips the sign in any of the four outcomes. The all-Brazil log-value result is the most stable: every drop keeps significance at 1% and the coefficient ranges from -0.42 (drop Mato Grosso) to -0.76 (drop Minas Gerais). The all-Brazil fines count is the least stable among the headline outcomes: dropping Mato Grosso, Pará, Amazonas, Tocantins, or any of several smaller states pushes it below 10% significance, although the point estimate remains negative. In the Legal Amazon panel the extensive margin loses significance only when Mato Grosso or Pará is dropped; the intensive margin (log value) is stable across all drops except Mato Grosso, where the coefficient halves ($-1.31 \rightarrow -0.66$, $p = 0.115$) — Mato Grosso is doing about half of the Legal Amazon intensive-margin work. The paper flags this in the main text: the Legal Amazon point estimates should be read as "mostly Mato Grosso and Pará," which is substantively interesting (these are exactly the agro-export frontier states the theory targets) but which a skeptical referee will note.

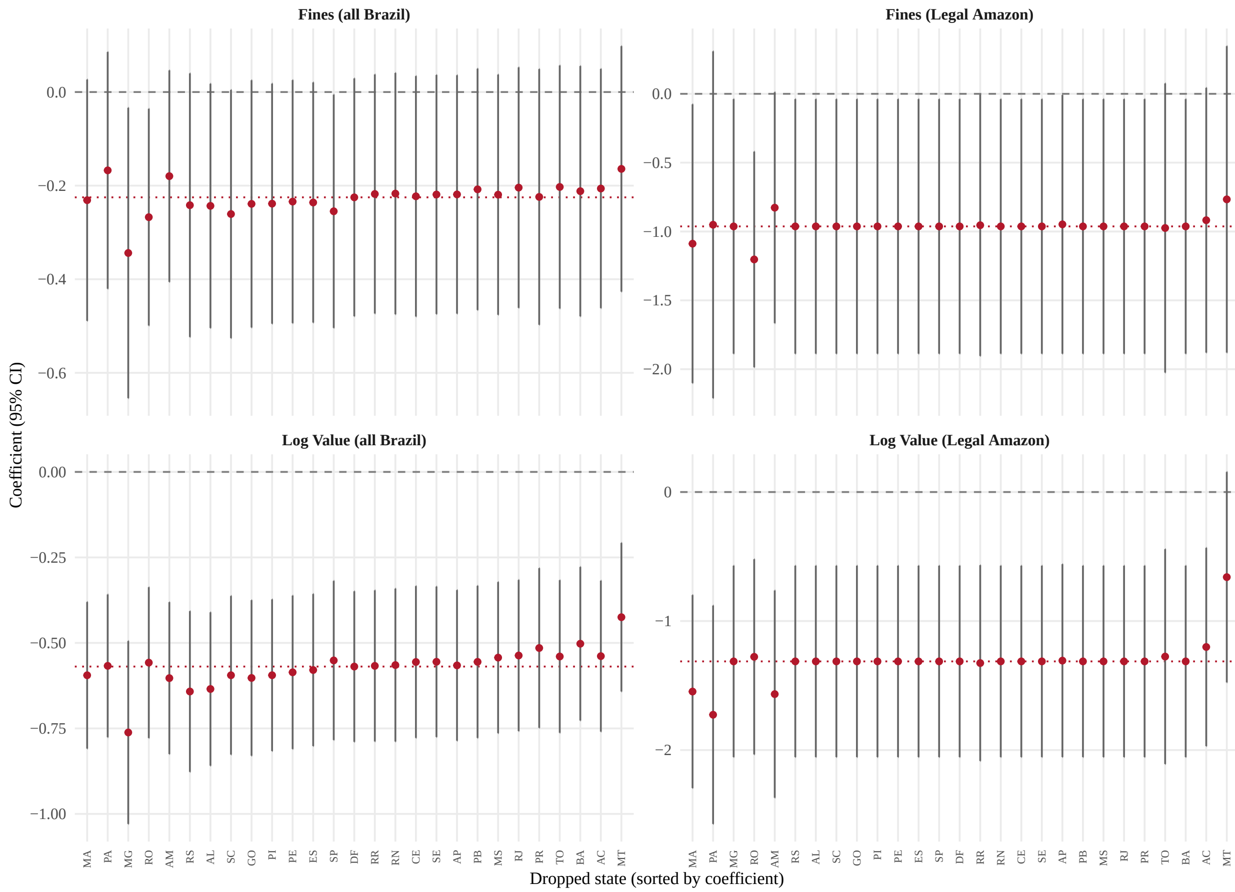

Figure A4: Leave-One-State-Out Sensitivity for the Four Headline Coefficients

Notes: Each point is the Bolsonaro Vote × Post-2019 coefficient from the headline specification (Table 1) re-estimated dropping one state at a time. Error bars are 95% confidence intervals from municipality-clustered standard errors. States are sorted by estimate magnitude within each panel. The dotted red line marks the full-sample baseline. Many non-Amazon drops produce identical estimates in the Legal Amazon panels because those states contribute no observations to the Legal Amazon sample.

G.5 Event Study Without State Linear Trends

Figure A5 replicates the event study of Figure 3 (fines count, continuous treatment) dropping the state-specific linear trends from the fixed-effects specification. The pre-trend joint F-test is $F = 1.179$ ($p = 0.314$) without trends, compared to $F = 1.213$ ($p = 0.296$) in the headline specification with state trends. Pre-trends are jointly flat under both specifications. The post-treatment negative shift is visible in both, though somewhat attenuated without trends, consistent with the state-level fixed effects absorbing some common enforcement dynamics.

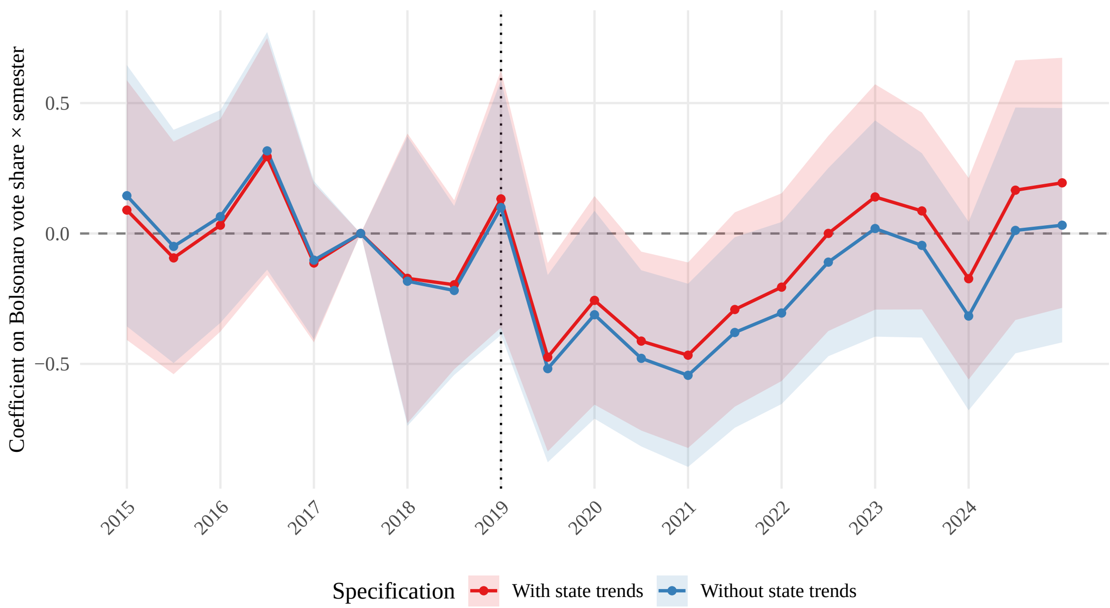

Figure A5: Event Study: With and Without State Linear Trends

Notes: Continuous treatment (Bolsonaro 2018 vote share). Reference: 2017H2. Blue: headline specification (with state trends). Red: specification without state trends. Municipality FE and state×semester FE included in both. Clustered SE. 95% CI. Shaded: Bolsonaro presidency.

G.6 Infraction-Type Event Study: Pre-COVID Window

Figure A6 replicates Figure 4 (event study by infraction category, continuous treatment, reference 2017H2) restricted to the pre-COVID window (2015H1–2020H1). The flora and forest category—which carries the selectivity result—has a clean pre-trend (joint $F = 0.40$, $p = 0.88$) and turns negative in 2019, confirming that the selectivity pattern in Figure 4 is not a COVID-period artifact. The fauna and fishing ($F = 1.92$, $p = 0.07$) and pollution ($F = 2.65$, $p = 0.01$) categories show some pre-period movement in this shorter window, reflecting the noisiness of these rarer infraction counts once the panel is restricted to five years; their post-2019 coefficients remain near zero, so the category nulls in the full sample reflect genuine absence of effect rather than offsetting pre-trends.

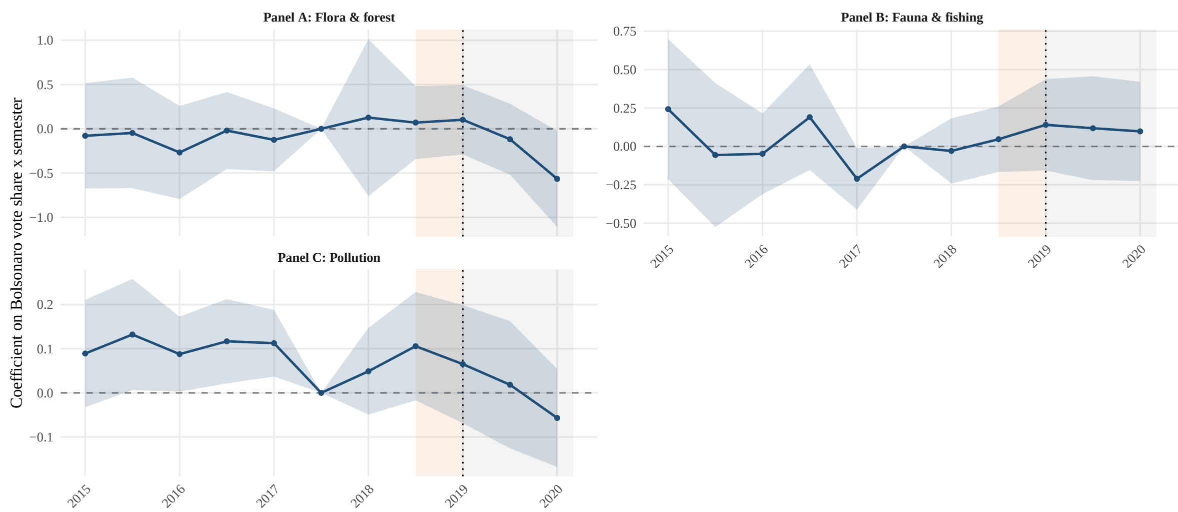

Figure A6: Event Study by Infraction Type: Pre-COVID Window (2015H1–2020H1)

Notes: Same specification as Figure 4 (continuous treatment, Bolsonaro 2018 vote share interacted with semester; reference 2017H2) restricted to January 2015–February 2020. Municipality FE, state $\times$ semester FE, state linear trends, and log electorate $\times$ time. SE clustered at municipality level. 95% CI.

G.7 Legal Amazon Ex-Mato Grosso

Table A9 reports the Legal Amazon headline enforcement results with and without Mato Grosso. Both fines count and log fine value lose conventional significance without Mato Grosso (log value: -0.660, $p = 0.113$; fines count: -0.766, $p = 0.177$), confirming that Mato Grosso drives the Legal Amazon enforcement result. However, the deforestation reduced form is larger and more significant without Mato Grosso (+48.6 km^2/yr, $p = 0.024$), as discussed in Section 5. This divergence—enforcement concentrated in Mato Grosso, deforestation broadly distributed—is consistent with the paper's mechanism: Mato Grosso had the highest pre-period enforcement intensity and the most organized agribusiness lobby, so it experienced the sharpest enforcement collapse; but deforestation cleared by smaller actors in Pará, Rondônia, and other states also accelerated once the political signal was transmitted.

A caveat on inference in this subsample: the Legal Amazon spans only nine states, of which Mato Grosso and Pará dominate the treatment variation, so the effective number of state-level clusters at the frontier is small. Inference throughout clusters at the municipality level (692 clusters in the Legal Amazon); the all-Brazil state-clustered robustness (Appendix Table A2) and the leave-one-state-out analysis (Figure A4) characterize sensitivity to the state structure directly. A wild-cluster bootstrap, the standard small-cluster correction, was unavailable for the R version used; the leave-one-out evidence is the more transparent substitute here and already shows the Legal Amazon estimates should be read as "mostly Mato Grosso and Pará."

Table A9: Legal Amazon: Full vs. Ex-Mato Grosso

	Fines	Log Value	Fines	Log Value
	Full Legal Amazon		Ex-Mato Grosso	
	(1)	(2)	(3)	(4)
Bolsonaro Vote × Post 2019	-0.963^{**}	-1.313^{***}	-0.767	-0.660
	(0.472)	(0.379)	(0.568)	(0.416)
Observations	106,518	106,518	88,314	88,314
R^2	0.152	0.175	0.162	0.173
Municipality FE	Yes	Yes	Yes	Yes
State × Semester FE	Yes	Yes	Yes	Yes
State Linear Trends	Yes	Yes	Yes	Yes

Notes: This table presents the Legal Amazon headline results with and without Mato Grosso. Headline specification: municipality FE, state×semester FE, state linear trends, log electorate×time. Columns (1)–(2): full Legal Amazon sample (N = 106,518 obs, 692 municipalities). Columns (3)–(4): Legal Amazon excluding Mato Grosso (N = 88,314 obs, 563 municipalities). Standard errors clustered at municipality level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

G.8 Fine Value Decomposition (Pre-COVID Window)

Table A10 decomposes the pre-COVID log fine value result by separately estimating the specification on log average fine value per active municipality-month (conditional on at least one fine being issued). Columns (1) and (3) replicate the log total fine value result from Table 4. Columns (2) and (4) estimate the same specification on log(total_valor / n_fines). The average fine amount is not differentially smaller in high-Bolsonaro municipalities: the coefficients are +0.52 ($p = 0.169$) and +1.21 ($p = 0.076$), both non-negative. The intensive-margin total value decline thus does not reflect smaller individual fines but rather a compositional shift toward lower-value infraction categories, consistent with Figure 4.

Table A10: Fine Value Decomposition: Total vs. Average (Pre-COVID Window)

	Log Value	Log Avg Value	Log Value	Log Avg Value
	All Brazil		Legal Amazon	
	(1)	(2)	(3)	(4)
Bolsonaro Vote × Post 2019	-0.421^{***}	0.523	-1.458^{***}	1.208^{*}
	(0.131)	(0.380)	(0.392)	(0.680)
Observations	286,626	19,736	53,692	6,353
R^2	0.185	0.457	0.179	0.379
Municipality FE	Yes	Yes	Yes	Yes
State × Semester FE	Yes	Yes	Yes	Yes
State Linear Trends	Yes	Yes	Yes	Yes

Notes: This table decomposes the pre-COVID fine-value result into total and average fine value per municipality-month. Pre-COVID window: January 2015–February 2020. Cols. (1),(3): log total fine value. Cols. (2),(4): log average fine value per municipality-month, restricted to municipality-months with at least one fine issued. Same FE and controls as Table 4. Standard errors clustered at municipality level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

G.9 Welfare: SCC Sensitivity

Table A11 reports the annual carbon damage welfare figure across four social cost of carbon values. The baseline ($51/t$CO_2$) follows the Obama-era EPA central estimate used throughout the paper. The calculation in each row applies the same deforestation coefficient (+40.29 km^2/yr per unit vote share, Table 5) and aggregation (1-SD contrast × 692 Legal Amazon municipalities); only the carbon valuation changes.

Table A11: Welfare Sensitivity to Social Cost of Carbon

SCC ($/t$CO_2$)	Source	Point estimate	95% CI lower	95% CI upper
$20/t$CO_2$	IPCC lower bound	$1.46B	$0.35B	$2.58B
$51/t$CO_2$	Obama EPA (baseline)	$3.73B	$0.88B	$6.58B
$100/t$CO_2$	IPCC upper scenario	$7.31B	$1.73B	$12.89B
$190/t$CO_2$	Biden EPA	$13.89B	$3.28B	$24.50B
Note: Baseline ($51/t$CO_2$) follows the Obama-era EPA central estimate used throughout.				

Notes: This table presents the sensitivity of the annual welfare cost to the assumed social cost of carbon. All calculations use the headline deforestation coefficient from Table 5 and aggregate over 692 Legal Amazon municipalities at a one-SD (0.1748) vote-share contrast. Carbon density: 150 tCO_2/ha. 95% CIs propagate the deforestation reduced-form standard error.

G.10 Romano-Wolf Multiple Testing Correction

Table A12 reports Romano-Wolf step-down adjusted p-values for the six headline outcomes in Table 1 (Romano and Wolf, 2005; Clarke, Romano, and Wolf, 2020). The Romano-Wolf procedure controls the familywise error rate (FWER) across simultaneous hypotheses, accounting for the covariance structure of the test statistics via a parametric (multivariate-normal) step-down resampling of the cluster-robust estimates (B = 9,999 draws); the wild-cluster-bootstrap implementation was unavailable for the R version used, and the parametric approach is the standard alternative. Unadjusted p-values are reproduced for comparison. The all-Brazil log fine value result ($p < 0.001$ unadjusted) is robust to Romano-Wolf correction. The Legal Amazon log fine value is similarly robust. The all-Brazil fines count, which is the weakest headline result ($p = 0.083$), remains in the same significance tier after adjustment; the Legal Amazon count result ($p = 0.042$) is more sensitive to the FWER correction.

H. Keyword Lists for Speech Classification

Environmental keywords: meio ambiente, desmatamento, floresta, amazonia/amazônia, ibama, icmbio, mudança climática, emissão, carbono, sustentável, biodiversidade, queimada, código florestal, licenciamento ambiental, reserva legal, terra indígena.

Agricultural keywords: agronegócio, agropecuária, produtor rural, soja, pecuária, gado,

Table A12: Romano-Wolf Multiple Testing Correction

Sample	Outcome	Coef.	SE	p (unadj.)	p (Romano–Wolf)
All Brazil	Fines (count)	-0.225*	(0.130)	0.083	0.159
All Brazil	Log fine value	-0.569***	(0.112)	0.000	0.000***
All Brazil	Embargoes	0.026	(0.058)	0.658	0.660
Legal Amazon	Fines (count)	-0.963**	(0.472)	0.041	0.080*
Legal Amazon	Log fine value	-1.313***	(0.378)	0.001	0.001***
Legal Amazon	Embargoes	0.018	(0.258)	0.946	0.945

Notes: This table presents Romano-Wolf multiple-testing-adjusted p-values for the six headline outcomes. Romano-Wolf step-down adjusted p-values (Romano and Wolf, 2005). Parametric (multivariate-normal) step-down resampling from the estimated covariance matrix of the cluster-robust test statistics, $B = 9{,}999$ draws. All-Brazil family (top panel) and Legal Amazon family (bottom panel) tested separately. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

rebanho, safra, plantio, lavoura, agrotóxico, marco temporal, crédito rural, plano safra.