

# Weakening Environmental Enforcement and Homicides: Evidence from Indigenous Territories in Brazil

Pedro Hemsley<sup>a,\*</sup>, Victor Libera<sup>b</sup>, Romero Rocha<sup>a</sup>

<sup>a</sup>*Universidade Federal do Rio de Janeiro, Brazil*

<sup>b</sup>*Banco Nacional de Desenvolvimento Econômico e Social (BNDES), Brazil*

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## Abstract

Following Brazil's 2014–2016 crisis and the 2016 impeachment, fiscal adjustment and policy changes coincided with a weakening of environmental and Indigenous-protection enforcement relevant to illegal mining. We study whether this loss of state capacity was associated with higher homicide rates where illegal-mining incentives concentrate. Using a 2010–2022 municipality-year panel covering the Amazonia and Cerrado biomes, we estimate a triple-difference (DDD) model based on Indigenous-territory and gold-bearing-area exposure. Homicide rates in treated municipalities rise about 0.3 standard deviations after 2016, peaking in 2018. Results survive state-by-year fixed effects, alternative timing, population weighting, and a dual-break control for a 2013 gold-market deregulation.

*Keywords:* Illegal mining, Indigenous territories, Violence, Triple differences, Brazil

*JEL classification:* K42, Q34, Q38, C23

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## 1. Introduction

Illegal gold mining is a major source of environmental and social harm in Brazil (Pereira and Pucci, 2026). We focus on a specific cell where this problem concentrates: municipalities containing both an Indigenous territory

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\*Corresponding author.

*Email address:* `pedro.hemsley@ie.ufrj.br` (Pedro Hemsley)

and one of the gold-bearing metallogenic provinces or districts mapped by the Geological Survey of Brazil (henceforth, gold-bearing areas). Figure 1 shows these municipalities in wine color. There, high resource rents and contested land rights can intensify violent conflict; most lie in the Amazonia and Cerrado biomes. Federal enforcement of mining bans is a key constraint on illegal extraction. The impeachment of President Rousseff in 2016, amid the 2014–2016 economic crisis, was followed by fiscal adjustment and policy changes that coincided with budget cuts and staff attrition in federal enforcement agencies.

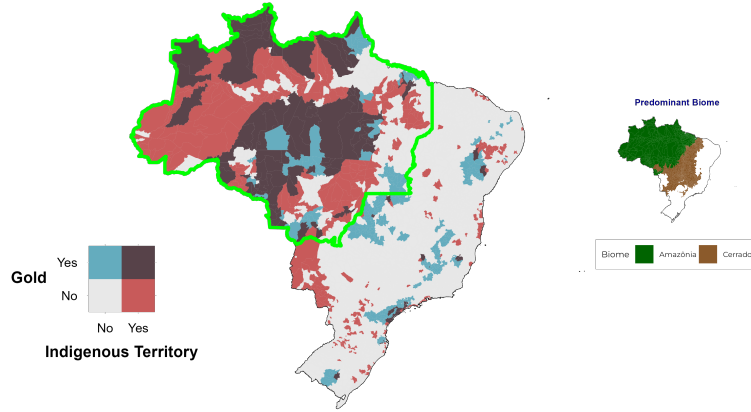


Figure 1: Wine-colored municipalities are those containing both an Indigenous territory and a gold-bearing area (the treatment cell). The inset shows the Amazonia and Cerrado biomes.

We study whether this loss of state capacity was associated with higher homicide rates in the municipalities where incentives for illegal mining are strongest. Our design builds on the institutional timeline documented by Nunes et al. (2024): Brazil’s command-and-control environmental enforcement weakened progressively during the late 2010s, and 2018 marks the inflection point at which federal field inspections returned to the dispersed pattern that predated the PPCDAm (the federal deforestation-control plan for the Legal Amazon). Using a 2010–2022 municipality-year panel restricted to municipalities overlapping Amazonia or Cerrado, we estimate a triple-difference (DDD) model whose treatment cell consists of the municipalities containing both features, where extraction inside the Indigenous territory is

forbidden under Brazilian federal law (see Section 2). After 2016, homicide rates in this cell rise by 7.211 per 100,000 inhabitants relative to the comparison cells ( $p = 0.009$ ), about 0.3 standard deviations of the cross-sectional homicide-rate distribution, and the increase peaks in 2018, the inflection year in that timeline. The estimate is robust to alternative treatment timing (2017), to state-by-year fixed effects, to decomposition into simple DDs, and to population-weighted estimation.

Our note connects to two recent papers. Relative to Nunes et al. (2024), who study deforestation enforcement, we show that the same institutional weakening is visible on the illegal-gold-mining margin, in the form of a measurable violence cost. Pereira and Pucci (2026) study an earlier, 2013 gold-market deregulation that increased illegal gold mining and violence in exposed Amazon municipalities; we verify in dual-break specifications that our post-2016 effect is not relabeling their channel. More broadly, the paper connects to the literature on commodity rents, contested property rights, and local violence (Dube and Vargas, 2013; Fetzer and Marden, 2017; Moreno-Louzada and Menezes-Filho, 2024; Falcone and Rosenberg, 2025).

## 2. Institutional background

### 2.1. Legal architecture and enforcement

Mining inside Indigenous territories is forbidden under Article 231, paragraph 3 of the 1988 Brazilian Federal Constitution, which permits such activity only upon specific authorization by the National Congress — an authorization that has never been granted; small-scale gold mining (*garimpo*) is further prohibited from operating within protected areas by Law 7.805/1989; and most categories of federal Conservation Units bar mining outright under the National System of Conservation Units (Law 9.985/2000). Gold deposits located inside an Indigenous territory therefore cannot be legally extracted, so municipalities containing both an Indigenous territory and a gold-bearing area are plausibly more exposed to illegal-mining incentives. Federal enforcement of these mining bans rests primarily on IBAMA (the federal environmental enforcement agency) and ICMBio (responsible for federal Conservation Units), supported by FUNAI’s mandate to protect Indigenous territories.

## 2.2. *The post-2016 enforcement weakening and the 2013 deregulation*

The de facto change of government took place in May 2016, when the impeachment process suspended President Rousseff and the incoming administration began a fiscal adjustment; the impeachment was confirmed in August, and Constitutional Amendment 95, promulgated in December 2016, consolidated the adjustment with a new federal spending rule (Brazil, 2016). The adjustment coincided with reductions in the resources available to enforcement agencies (Abessa et al., 2019; Pereira et al., 2020; Branford and Torres, 2017): in a contemporaneous statement, FUNAI reported budget cuts in 2016 and further reductions in discretionary spending in 2017 (FUNAI, 2017). Nunes et al. (2024) report that IBAMA’s field-inspection corps fell from 965 agents in 2016 to 691 by 2020, due to the return of federal field inspections to the dispersed, pre-PPCDAm pattern, and document sharp drops in infraction notices, embargoes, and asset confiscations thereafter.

Pereira and Pucci (2026) document an earlier, 2013 deregulation that exempted gold first-buyers (the regulated stores through which raw gold legally enters the financial system) from liability for the origin of the gold they purchased; we return to this channel in Section 3.

## 3. Data and empirical strategy

### 3.1. *Data*

We use a municipality-year panel from 2010 to 2022. The outcome is homicide rate per 100,000 inhabitants, constructed from SIM/DATASUS mortality records and estimated municipal population data. We combine those data with geospatial indicators:

- $IT_i = 1$ : municipality contains at least one official Indigenous territory, as listed by FUNAI.
- $Gold_i = 1$ : municipality overlaps at least one mapped gold-bearing area.

Construction details and geometric robustness checks for both indicators are in the online supplementary material. The sample is restricted to municipalities that overlap Amazonia or Cerrado (any-overlap definition from

IBGE municipality-biome correspondence) and contains 24,362 municipality-year observations (1,874 municipalities over 13 years); 24,358 observations have non-missing homicide rates.

### 3.2. Baseline DDD

Our baseline specification is

$$y_{it} = \beta_1(Gold_i \times Post_t) + \beta_2(IT_i \times Post_t) + \beta_3(Gold_i \times IT_i \times Post_t) + \mu_i + \lambda_t + \varepsilon_{it} \quad (1)$$

where  $y_{it}$  is homicide rate,  $\mu_i$  are municipality fixed effects, and  $\lambda_t$  are year fixed effects. We use  $Post_t = \mathbf{1}\{t \geq 2016\}$  as our baseline post indicator to mark the federal political transition, which began in May 2016. As a robustness check, we use  $Post_t = \mathbf{1}\{t \geq 2017\}$ , the first full year after that transition and the adoption of the new federal spending rule. We focus on municipalities in Amazonia or Cerrado, the biomes most directly connected to environmental conflict.<sup>1</sup> Standard errors are clustered by municipality. The coefficient of interest is  $\beta_3$ . Identification requires that, conditional on the fixed effects, homicide rates in the four  $Gold \times IT$  cells would have evolved in parallel absent the enforcement change; the pre-period evidence in Section 4 supports this assumption.

### 3.3. Dual-break DDD

To separate the post-2016 effect from the 2013 channel emphasized by Pereira and Pucci (2026),<sup>2</sup> we estimate a piecewise specification that allows the DDD interaction to shift at two distinct breaks, 2013 and 2016 (or,

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<sup>1</sup>Main results hold in alternative samples. Restricting to the Legal Amazon yields DDD estimates of 8.444 (s.e. 3.024) with treatment starting in 2016 and 9.143 (s.e. 3.204) starting in 2017; including all Brazilian municipalities yields 6.639 (s.e. 2.217) and 7.255 (s.e. 2.357). We prefer the Amazonia + Cerrado sample because it targets the environmentally relevant municipalities.

<sup>2</sup>Our exercise differs from theirs in three respects: shock (post-2016 enforcement weakening vs. the 2013 deregulation), design (DDD with  $Gold \times IT$  as the treatment cell vs. DiD with gold-deposits-inside-protected-areas as the treated group), and sample (Amazonia+Cerrado in 2010–2022 vs. Amazon-only in 2006–2019).

alternatively, 2017):

$$\begin{aligned}
y_{it} = & \sum_{b \in \{2013, 2016\}} \\
& [\beta_{1b}(Gold_i \times Post_t^b) + \beta_{2b}(IT_i \times Post_t^b) + \beta_{3b}(Gold_i \times IT_i \times Post_t^b)] \\
& + \mu_i + \lambda_t + \varepsilon_{it}.
\end{aligned} \tag{2}$$

In this specification,  $Post_t^{2013}$  and  $Post_t^{2016}$  are nested step indicators, so  $\beta_{3,2013}$  captures the differential change in homicide rates of treated municipalities that turns on in 2013 and persists thereafter, while  $\beta_{3,2016}$  captures the additional incremental change from 2016 onward, on top of any 2013 effect. The total post-2016 DDD effect is therefore  $\beta_{3,2013} + \beta_{3,2016}$ . If our 2016 estimate is just relabeling the 2013 channel,  $\beta_{3,2016}$  should shrink toward zero once  $\beta_{3,2013}$  is allowed to enter. We report analogous estimates replacing 2016 with 2017.

## 4. Results

### 4.1. Main estimates

Table 1 reports core estimates. In the main model (post-2016), the triple interaction is 7.211 (s.e. 2.759,  $p = 0.009$ ), about 0.3 standard deviations of the cross-sectional homicide-rate distribution. With treatment starting in 2017, it is 7.767 (s.e. 2.949,  $p = 0.009$ ).

We also absorb fully flexible state-year shocks via state-by-year fixed effects. The post-2016 triple interaction remains positive and statistically significant: 8.691 (s.e. 2.726,  $p = 0.001$ ).

Table 1: Main DDD Estimates in Amazonia + Cerrado Municipalities (2010–2022)

|                                                           | Dependent Variable: Homicide Rate per 100,000 |                     |                                     |
|-----------------------------------------------------------|-----------------------------------------------|---------------------|-------------------------------------|
|                                                           | Post $\geq$ 2016                              | Post $\geq$ 2017    | Post $\geq$ 2016 +<br>State-year FE |
|                                                           | (1)                                           | (2)                 | (3)                                 |
| Gold $\times$ Post                                        | 2.663**<br>(1.066)                            | 2.762**<br>(1.139)  | 0.884<br>(1.125)                    |
| IT $\times$ Post                                          | -0.344<br>(1.006)                             | 0.214<br>(0.990)    | -2.673**<br>(1.202)                 |
| Gold $\times$ IT $\times$ Post                            | 7.211***<br>(2.759)                           | 7.767***<br>(2.949) | 8.691***<br>(2.726)                 |
| Municipality FE                                           | Yes                                           | Yes                 | Yes                                 |
| Year FE                                                   | Yes                                           | Yes                 | No                                  |
| State-year FE                                             | No                                            | No                  | Yes                                 |
| Observations                                              | 24,358                                        | 24,358              | 24,345                              |
| Standard errors clustered by municipality in parentheses. |                                               |                     |                                     |
| *** $p < 0.01$ , ** $p < 0.05$ , * $p < 0.1$ .            |                                               |                     |                                     |

To improve interpretation, we decompose the DDD into simple DDs. Within  $IT = 1$  municipalities, the DD effect of  $Gold \times Post2016$  is 9.874 ( $p < 0.001$ ), while within  $IT = 0$  it is 2.663 ( $p = 0.013$ ). Symmetrically, within  $Gold = 1$ , the DD effect of  $IT \times Post2016$  is 6.867 ( $p = 0.008$ ), versus -0.344 ( $p = 0.733$ ) within  $Gold = 0$ . This pattern is consistent with the DDD estimate.

Additional robustness checks are also supportive. Population-weighted DDD estimates are 9.505 ( $p = 0.015$ ) with municipality and year FE, and 7.221 ( $p = 0.042$ ) with municipality and state-by-year FE. Within the pre-2016 period, DDD break estimates using only data through 2015 and candidate break dates of 2013 and 2014 are not statistically significant ( $p = 0.113$  and  $p = 0.411$ ). These tests provide no evidence of discrete differential shifts before 2016; the event-study leads below provide a complementary assessment of pre-treatment dynamics.

Table 2 reports dual-break estimates. The incremental post-2016 effect ranges from 5.618 to 6.906 and remains statistically significant; with a 2017 second break, it ranges from 6.348 to 7.379. The 2013 component is smaller and imprecisely estimated in most specifications. The post-2016 increment is therefore not solely explained by the earlier 2013 deregulation shock.

Table 2: Dual-break DDD Estimates

| Specification                                                                                                                                                                             | 2013 Effect       | Increment After<br>Second Break | Total Later Effect   |
|-------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------|-------------------|---------------------------------|----------------------|
| 2013 + 2016 breaks                                                                                                                                                                        | 3.187<br>(2.219)  | 5.618**<br>(2.765)              | 8.805***<br>(3.168)  |
| 2013 + 2016 breaks + State-year FE                                                                                                                                                        | 3.571<br>(2.266)  | 6.906**<br>(2.834)              | 10.477***<br>(3.067) |
| 2013 + 2017 breaks                                                                                                                                                                        | 3.311<br>(2.204)  | 6.348**<br>(2.924)              | 9.659***<br>(3.417)  |
| 2013 + 2017 breaks + State-year FE                                                                                                                                                        | 3.978*<br>(2.246) | 7.379**<br>(3.062)              | 11.357***<br>(3.334) |
| The first column is the DDD effect after 2013 and before the second break.<br>Clustered standard errors by municipality in parentheses.<br>*** $p < 0.01$ , ** $p < 0.05$ , * $p < 0.1$ . |                   |                                 |                      |

#### 4.2. Dynamic evidence and pre-trends

Figure 2 shows dynamic DDD coefficients for the triple interaction, with 2015 as reference year. Coefficients for 2010–2014 are jointly insignificant in both baseline and state-year FE specifications ( $p = 0.699$  and  $p = 0.511$ ). The 2016 and 2017 coefficients are small and statistically indistinguishable from zero; the differential effect emerges sharply in 2018 (11.969,  $p = 0.006$ ) and remains positive in subsequent years, though less precisely estimated. This timing lines up directly with the institutional inflection identified by Nunes et al. (2024).

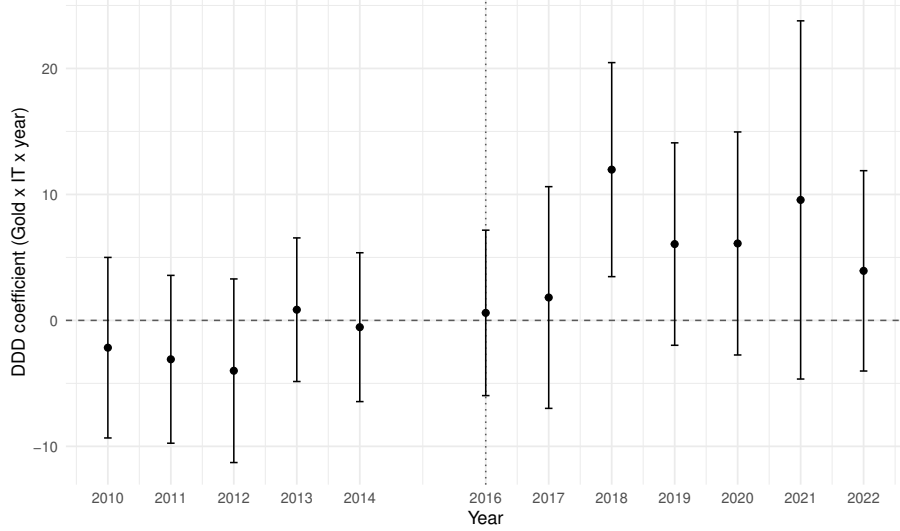


Figure 2: Dynamic DDD event-study coefficients (reference year: 2015). The dotted vertical line marks the start of the baseline post period in 2016.

## 5. Conclusion

In municipalities overlapping Amazonia or Cerrado, homicide rates rose after the 2016 political transition and ensuing fiscal adjustment specifically where Indigenous territories and gold-bearing areas coexist, with the differential increase peaking in 2018 — the year that Nunes et al. (2024) identify as the inflection point of Brazil’s federal environmental-enforcement model. The result survives alternative treatment timing, fully flexible state-year shocks, decomposition into simple DDs, pre-period break tests, and population weighting.

In dual-break specifications, the incremental post-2016/post-2017 effect remains large and significant after allowing for the 2013 deregulation shock documented by Pereira and Pucci (2026), suggesting it is not solely explained by the earlier channel.

This evidence is consistent with the view that weaker effective enforcement in environmentally sensitive and resource-rich territories can generate substantial local violence costs.

## **Data availability**

All data used in the paper are drawn from publicly available sources: mortality records (SIM/DATASUS), municipal population estimates and biome correspondence (Brazilian Institute of Geography and Statistics, IBGE), Indigenous territory polygons (National Foundation of Indigenous Peoples, FUNAI), and gold provinces and districts (Geological Survey of Brazil, SGB). The assembled municipality-year panel and replication code are available on Zenodo: [doi:10.5281/zenodo.22213133](https://doi.org/10.5281/zenodo.22213133).

## **Declaration of competing interest**

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

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