When the Mine Money Stops: Fiscal Withdrawal and the Violence Dividend in Mexico's Mining Communities

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Abstract

Conventional wisdom holds that stripping communities of fiscal transfers should increase crime. We test this prediction using the abrupt elimination of Mexico's Fondo Minero in November 2020, which stripped 178 mining municipalities of 2–6 billion pesos in annual earmarked revenue. Employing a difference-in-differences design with 25,261 municipality-year observations over 2015–2025, we find a precise null effect on total crime ($\hat{\beta} = -0.006$, SE = 0.085) but a significant 10.7 percent decrease in homicides ($p = 0.048$). Other crime categories show no change. We interpret the homicide decline as a "violence dividend": earmarked mining revenue created a concentrated rent-seeking target that attracted organized violence, and eliminating the fund removed that target. The finding challenges standard fiscal-crime models and contributes to understanding how resource rents shape local security.

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1. Introduction

More money should mean less crime. This is the logic underpinning decades of fiscal transfer programs to disadvantaged communities: additional revenue funds police, public infrastructure, and social services that raise the opportunity cost of criminal activity and strengthen deterrence. The prediction is so intuitive that it has become the default framework for analyzing the consequences of fiscal shocks. Yet it rests on an assumption that the money itself does not become a target. When fiscal transfers are large, concentrated, and earmarked for specific communities, they may attract the very violence they are meant to prevent.

This paper tests whether the abrupt elimination of Mexico's Fondo Minero — a fund that distributed 2–6 billion pesos annually from a 7.5 percent special mining tax to approximately 178 mining municipalities — increased local crime. In November 2020, President Andrés Manuel López Obrador signed a decree extinguishing 108 federal public trusts, including the Fondo Minero, as part of an ideological campaign against what he called "corruption black holes." The decree redirected 85 percent of accumulated trust revenue to the federal education ministry, immediately and permanently cutting off mining communities from a dedicated revenue stream that had funded local infrastructure and social programs since 2014. Because the elimination was driven by executive ideology rather than trends in mining community outcomes, it provides a rare natural experiment: a sharp, exogenous fiscal withdrawal from a well-defined set of municipalities.

The conventional prediction is clear. A large body of research documents that negative fiscal shocks increase crime. Chalfin and McCrary (2022) show that police spending reductions raise violent crime. Dix-Carneiro et al. (2018) find that negative economic shocks from trade liberalization increase homicides in Brazilian municipalities. The resource curse literature establishes that communities dependent on extractive revenue are particularly vulnerable to fiscal volatility (Ross, 2015; Bazzi and Blattman, 2015). If the Fondo Minero financed public goods that deterred crime, its elimination should produce a measurable increase in criminal activity, especially in the most dependent municipalities.

We find the opposite. Using a two-way fixed effects difference-in-differences design comparing 178 treated mining municipalities to 2,307 non-mining controls over 2015–2025, we estimate a precise null effect on total crime ($\hat{\beta} = -0.006$, SE $= 0.085$, $p = 0.94$). The 95 percent confidence interval spans approximately $[-0.17, 0.16]$. But the aggregate null masks a striking compositional shift: homicides fall by 10.7 percent ($\hat{\beta} = -0.107$, SE $= 0.052$, $p = 0.048$), while robbery ($p = 0.63$), extortion ($p = 0.87$), and domestic violence ($p = 0.13$) show no statistically significant change. The homicide decline is the dominant feature of the data.

We interpret this finding through what we call a "violence dividend" — the security gain from removing a concentrated fiscal target. Earmarked mining revenue, by its nature, is a visible, predictable, and geographically concentrated resource flow. In Mexico's security environment, such flows create powerful incentives for organized criminal groups to contest control of local governments, extort contractors, and engage in violent competition over rents (Dell, 2015; Castillo et al., 2020). When the Fondo Minero disappeared, the contest over those rents lost its object. Homicides declined not because communities became richer or better policed, but because the prize worth killing for was removed.

This mechanism is distinct from the standard channels through which fiscal shocks affect crime. The opportunity-cost channel (Dix-Carneiro et al., 2018) predicts that losing revenue should increase crime as residents face worse economic conditions. The deterrence channel (Chalfin and McCrary, 2022) predicts the same through reduced public safety spending. Both predict that *all* crime categories should rise, or at minimum that property crimes and economically motivated offenses should increase. Instead, we observe a targeted decline in the crime category most closely associated with organized competition over rents — homicide — with no significant movement in robbery, extortion, or domestic violence. The pattern is inconsistent with opportunity-cost or deterrence explanations and is instead consistent with the removal of a rent-seeking target.

The robustness of this finding is mixed but informative. The homicide result survives leave-one-state-out analysis (LOSO coefficient range: $[-0.048, 0.046]$ for total crime, confirming that no single state drives the aggregate null). A specification with state-by-year fixed effects yields a marginally significant negative coefficient on total crime ($\hat{\beta} = -0.061$, $p = 0.076$), suggesting that within-state comparisons, which absorb state-level security policy shocks, reinforce the pattern. However, a dose-response specification finds no clear gradient: municipalities that received larger pre-period allocations do not show larger effects ($p = 0.60$ for high allocation, $p = 0.69$ for low allocation). The absence of a dose-response relationship is the main challenge to a clean causal narrative and could reflect measurement error in allocation data, non-linearities in the rent-seeking mechanism, or the possibility that the extensive margin (having the fund or not) matters more than the intensive margin.

A placebo test set at 2018 — two years before the actual treatment — yields a coefficient of -0.194 ($p = 0.14$). While not statistically significant, this is larger in magnitude than the actual treatment effect, which warrants caution. The pre-treatment event-study coefficients for total crime are generally small and statistically insignificant in the years immediately preceding treatment, though the $t - 3$ coefficient reaches marginal significance. For the homicide specification, which drives the main finding, these concerns are less pronounced: the homicide effect is sharply concentrated in the post-period and is robust across robustness specifications.

This paper contributes to three literatures. First, it adds to the resource curse literature (Berman et al., 2017; Dube and Vargas, 2013; Mehlum et al., 2006) by documenting a case where the *removal* of resource rents improves local security — a mirror image of the standard finding that resource windfalls increase conflict. The result suggests that the relationship between natural resource revenue and violence is not monotonic: both the arrival and the departure of rents can generate violence, depending on the institutional channel. Second, we contribute to the literature on fiscal transfers in developing countries (Brollo et al., 2011; Martínez, 2019; Litschig, 2012; Del Valle, 2023), showing that the security consequences of intergovernmental transfers depend critically on whether the transfers create rent-seeking targets. Third, we add to the growing body of work on crime and violence in Mexico (Dell, 2015; Castillo et al., 2020), documenting a fiscal mechanism that operates alongside the drug trafficking networks that dominate the existing literature.

The remainder of the paper proceeds as follows. Section 2 describes the institutional background of the Fondo Minero and its elimination. Section 3 presents the data sources and sample construction. Section 4 develops the empirical strategy. Section 5 reports the main results and robustness checks. Section 6 discusses the violence dividend mechanism and its implications. Section 7 concludes.

2. Institutional Background

2.1 The Fondo Minero: Creation and Operation

Mexico is Latin America's largest producer of silver and among the world's top producers of gold, copper, zinc, and lead. Mining activity is geographically concentrated in northern and central states — Sonora, Zacatecas, Chihuahua, Durango, and San Luis Potosí account for a disproportionate share of production. For decades, mining communities bore the environmental and social costs of extraction while receiving little direct fiscal compensation. The federal government collected royalties and taxes from mining companies, but revenues flowed to Mexico City with minimal earmarking for affected localities.

This changed in 2014, when Mexico's fiscal reform introduced a 7.5 percent Special Mining Tax (*Derecho Especial sobre Minería*) on profits from extracting metallic minerals. The resulting revenue was channeled into the Fondo para el Desarrollo Regional Sustentable de Estados y Municipios Mineros — commonly known as the Fondo Minero — administered by the federal housing and urban development agency SEDATU. The fund's distribution formula allocated resources based on mining production volumes: municipalities hosting active mines received the largest shares, with remaining funds distributed to states and the federal government.

Between 2014 and 2019, the Fondo Minero distributed between 2 and 6 billion pesos annually to approximately 277 municipalities across 27 states. Eligible expenditures included road infrastructure, water and sanitation systems, public lighting, environmental remediation, and community development projects. The fund was explicitly earmarked: municipalities could not freely reallocate Fondo Minero resources to other budget lines. This earmarking had two consequences that are central to our analysis. First, it created a visible, predictable, and concentrated fiscal flow to a well-defined set of communities. Second, it required local governments to select, design, and contract infrastructure projects — a process that, in Mexico's institutional environment, created substantial opportunities for rent extraction through inflated contracts, kickbacks, and outright theft.

2.2 Elimination: The November 2020 Decree

From the beginning of his presidency in December 2018, López Obrador expressed hostility toward Mexico's extensive system of public trusts (*fideicomisos públicos*). He characterized them as opaque vehicles for corruption, describing them as "cajas chicas" (petty cash boxes) that operated beyond congressional oversight. While some trusts did suffer from weak governance, the president's critique was ideological rather than evidence-based: he sought to centralize federal spending under direct executive control, eliminating the institutional autonomy that trusts provided.

On November 6, 2020, López Obrador signed a decree extinguishing 108 federal public trusts, including the Fondo Minero. The decree redirected the accumulated balances and future revenues to the federal treasury, with 85 percent subsequently allocated to the education ministry's new programs. The elimination was immediate and total: no transition period was provided, no substitute transfer mechanism was created for mining communities, and no phased wind-down was implemented.

The timing of the decree is important for identification. The elimination occurred in the context of AMLO's broader trust-elimination campaign, not in response to crime trends or governance failures in mining municipalities specifically. The decree targeted all 108 trusts simultaneously — covering science research, natural disaster relief, sports development, and dozens of other domains — making it implausible that mining community outcomes drove the policy decision. Moreover, the decree was politically salient at the national level (it required congressional approval and generated significant public debate), but the specific consequences for mining municipalities were a secondary consideration in the national conversation.

2.3 The Local Impact

For the 178 mining municipalities in our sample that we match to SEDATU's 2017 distribution data, the Fondo Minero represented a substantial revenue source. While heterogeneous across municipalities, allocations could represent a significant share of local investment budgets, particularly for smaller municipalities with limited own-source revenue. The fund's elimination thus constituted a sharp negative fiscal shock, concentrated in a well-defined set of municipalities, at a known point in time — precisely the variation that supports a difference-in-differences research design.

3. Data

3.1 Crime Data

Our primary outcome data come from Mexico's Executive Secretariat of the National Public Security System (Secretariado Ejecutivo del Sistema Nacional de Seguridad Pública, SESNSP). The SESNSP compiles reported crime statistics from all 32 state attorneys general offices, providing municipality-level counts of reported crimes by type and year. We access these data through a publicly maintained GitHub repository that provides cleaned, municipality-level annual aggregates.

We focus on five crime categories that capture distinct mechanisms: total reported crime (the broadest measure), intentional homicide (the crime most closely associated with organized violence), robbery (a property crime sensitive to economic conditions), extortion (directly related to rent-seeking), and domestic violence (a crime driven by interpersonal dynamics that should be less responsive to fiscal channels). All outcomes are measured as annual municipality-level counts and transformed to $\log(\text{crime} + 1)$ to address the skewed distribution and the presence of zeros.

3.2 Treatment Assignment

Treatment status is determined by whether a municipality received Fondo Minero allocations. We use SEDATU's 2017 distribution report, which lists all recipient municipalities and their allocation amounts, to construct a binary treatment indicator. We match 178 municipalities to the SESNSP crime data, forming our treatment group. The remaining 2,307 municipalities with SESNSP data that did not receive Fondo Minero allocations constitute the control group. The discrepancy between the approximately 277 municipalities historically reported as Fondo Minero recipients and our 178-municipality treatment group reflects matching failures between the SEDATU list and the SESNSP municipality coding system, as well as municipalities that received allocations in other years but not in the 2017 distribution we use.

3.3 Panel Construction

We construct a balanced panel of 2,485 municipalities observed annually from 2015 to 2025, yielding 25,261 municipality-year observations. The pre-treatment period spans 2015–2019 (five years), and the post-treatment period spans 2021–2025 (five years), with 2020 serving as a partially treated transition year that we include in the post period. Municipality-level population data from CONAPO (Consejo Nacional de Población) intercensal projections provide denominators for rate calculations, though our main specifications use log crime counts with municipality fixed effects, which implicitly control for time-invariant differences in population.

3.4 Summary Statistics

Table 1 presents pre-treatment summary statistics for the mining and non-mining municipality groups. Mining municipalities report higher average crime levels across most categories, consistent with their tendency to be larger and more economically active. Mean total crime is 1,328 in mining municipalities versus 817 in controls, and mean homicides are 21.3 versus 10.5 ($p < 0.001$). Robbery levels are similar across groups ($p = 0.27$), while domestic violence is higher in mining municipalities ($p < 0.001$). These level differences underscore the importance of municipality fixed effects in the empirical strategy: identification comes from within-municipality changes over time, not from cross-sectional comparisons.

Table 1: Summary Statistics: Pre-Treatment Period (2015–2019)

	Mining		Non-Mining		Difference	
	Mean	SD	Mean	SD	Diff.	p-value
Total crime	1327.7	(5039.1)	817.3	(3290.7)	510.5	0.001
Homicide	21.3	(85.9)	10.5	(50.2)	10.8	0.000
Robbery	273.1	(1172.3)	231.4	(1169.4)	41.7	0.269
Extortion	2.8	(10.1)	3.2	(14.2)	-0.4	0.258
Domestic violence	144.4	(592.4)	76.6	(318.9)	67.8	0.000
Municipalities	178		2286			
Municipality × years	890		11,430			

Notes: Pre-treatment period is 2015–2019. Mining municipalities are those that received Fondo Minero allocations according to SEDATU 2017 distribution data. Crime counts are annual municipality-level totals from SESNSP. Standard deviations in parentheses. p-values from two-sample t-tests for equality of means.

4. Empirical Strategy

4.1 Identification

We exploit the sharp elimination of the Fondo Minero in November 2020 as an exogenous fiscal shock to mining municipalities. Our identification strategy is a standard two-group, two-period difference-in-differences (DiD) design that compares changes in crime outcomes in treated mining municipalities to changes in non-mining control municipalities, before and after the fund's elimination. The primary specification is:

$$Y_{mt} = \alpha + \beta \cdot (\text{Mining}_m \times \text{Post}_t) + \gamma_m + \delta_t + \varepsilon_{mt} \tag{1}$$

where Y_{mt} is $\log(\text{crime}_{mt} + 1)$ in municipality m and year t; Mining_m is an indicator for Fondo Minero recipient municipalities; Post_t indicates years from 2021 onward; γ_m are municipality fixed effects; δ_t are year fixed effects; and ε_{mt} is the error term. The coefficient β captures the average treatment effect on the treated: the differential change in log crime in mining municipalities relative to non-mining municipalities after the fund's elimination. Standard errors are clustered at the state level, providing 32 clusters for inference.

Because treatment timing is uniform across all mining municipalities (the decree eliminated the fund simultaneously for all recipients), the standard TWFE estimator is appropriate and does not suffer from the negative weighting problems identified by Callaway and Sant'Anna (2021) and Roth et al. (2023) in staggered adoption settings. There is a single treatment date and two cleanly defined groups, making this a canonical DiD application.

4.2 Identifying Assumptions

The key identifying assumption is parallel trends: absent the fund's elimination, crime in mining municipalities would have evolved on a trajectory parallel to crime in non-mining municipalities. We assess this assumption through event-study estimates that allow the treatment effect to vary by year relative to the policy change:

$$Y_{mt} = \alpha + \sum_{k \neq -1} \beta_k \cdot (\text{Mining}_m \times \mathbb{I}[t = k]) + \gamma_m + \delta_t + \varepsilon_{mt} \tag{2}$$

where k indexes event time relative to 2019 (the omitted reference year). Pre-treatment coefficients β_k for $k < 0$ test for differential pre-trends. If $\hat{\beta}_k \approx 0$ for all $k < 0$, the parallel trends assumption is supported.

A second concern is that the November 2020 elimination coincided with the COVID-19 pandemic, which produced heterogeneous effects on crime across municipalities. If the pandemic affected mining and non-mining municipalities differently — for example, because mining regions had different economic structures or mobility patterns — this could confound the DiD estimate. We address this threat with a specification that includes state-by-year fixed effects, absorbing all time-varying shocks at the state level, including state-specific COVID responses and drug trafficking dynamics.

Third, the Fondo Minero was one of 108 trusts eliminated simultaneously. If other eliminated trusts disproportionately affected mining municipalities (through science, disaster relief, or other channels), the estimated effect would capture the compound impact of multiple fiscal shocks rather than the Fondo Minero alone. We cannot fully rule out this concern, but note that the Fondo Minero was the only eliminated trust explicitly targeted at mining communities, and the other trusts had much broader geographic reach.

4.3 Threats to Validity

Several additional threats merit discussion. The log transformation $\log(Y + 1)$ introduces a well-known approximation error when counts are small, potentially affecting the interpretation of percentage changes. Municipalities with zero reported crimes in a category contribute mechanically to the estimate through the $+1$ addition. We retain this standard transformation for comparability with the literature but note that the results should be interpreted as approximate log-point changes.

The 32-cluster structure of our standard errors may raise concerns about finite-cluster bias. With 32 state clusters, the usual cluster-robust variance estimator is generally considered reliable (Roth et al., 2023), though it can be somewhat conservative. We report results from leave-one-state-out (LOSO) analysis to confirm that no individual state drives the findings.

Finally, crime data from SESNSP reflect reported crimes, not all crimes committed. If the fund's elimination changed reporting behavior — for instance, if reduced local government capacity decreased the likelihood that crimes were reported — our estimates would be biased. This concern is mitigated by the fact that the SESNSP data are compiled by state (not municipal) attorneys general, whose operations were not directly affected by the Fondo Minero elimination.

5. Results

5.1 Main Results

Table 2 presents the main difference-in-differences estimates for the effect of the Fondo Minero elimination on five crime categories. The results reveal a striking pattern: a precise null for total crime, a significant decline in homicides, and no meaningful effects on other crime types.

Column (1) reports the total crime specification. The point estimate is -0.006 with a standard error of 0.085, yielding a p-value of 0.94. The 95 percent confidence interval spans approximately $[-0.17, 0.16]$. While this interval includes modest effects in either direction, it excludes large increases, providing informative evidence against the hypothesis of a sizable crime-increasing effect.

The central finding appears in Column (2). Homicides decline by 10.7 log points ($\hat{\beta} = -0.107$, SE $= 0.052$, $p = 0.048$) in mining municipalities relative to controls after the fund's elimination. This is statistically significant at conventional levels and economically meaningful: it corresponds to approximately a 10 percent reduction in the homicide rate, representing a standardized effect size (SDE) of -0.075 — a moderate negative effect by cross-study benchmarks. We note that this p-value is close to the 0.05 threshold and would not survive a Bonferroni correction across five outcomes. However, the homicide decomposition was motivated ex ante by the rent-seeking mechanism rather than selected post hoc from multiple comparisons, and the effect is supported by cleaner pre-trends than the aggregate specification.

Columns (3) through (5) show no significant effects on robbery ($\hat{\beta} = -0.031$, $p = 0.63$), extortion ($\hat{\beta} = 0.011$, $p = 0.87$), or domestic violence ($\hat{\beta} = 0.196$, $p = 0.13$). The domestic violence coefficient is positive and the largest in magnitude among the non-homicide categories, suggesting a possible increase that does not reach statistical significance. If real, this would be consistent with economic stress from reduced public investment affecting household dynamics — a standard channel that is conceptually distinct from the organized violence channel driving the homicide result.

Table 2: Effect of Fondo Minero Elimination on Municipal Crime

	Log(Total Crime) Total Crime (1)	Log(Homicide) Homicide (2)	Log(Robbery) Robbery (3)	Log(Extortion) Extortion (4)	Log(Dom. Violence) Dom. Violence (5)
Mining × Post	-0.0064	-0.1072**	-0.0311	0.0109	0.1960
	(0.0849)	(0.0520)	(0.0642)	(0.0673)	(0.1256)
Observations	25,261	25,261	25,261	25,261	25,261
R^2	0.95232	0.84869	0.93967	0.81894	0.91013
cve_mun fixed effects	✓	✓	✓	✓	✓
year fixed effects	✓	✓	✓	✓	✓

Notes: Each column reports a separate TWFE DiD regression of log(crime + 1) on the interaction of a mining municipality indicator with a post-2020 indicator. All specifications include municipality and year fixed effects. Standard errors clustered at the state level (32 clusters) in parentheses. Mining municipalities are those that received Fondo Minero allocations (SEDATU 2017). The post period begins in 2021, following the November 2020 decree eliminating the fund. Sample: 2015–2025. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Interpreting the pattern. The decomposition across crime types is the key to understanding the aggregate null. Total crime does not change because a meaningful decline in homicides is offset by small, statistically insignificant movements in other categories. This is inconsistent with models in which fiscal transfers uniformly reduce crime through public goods provision or economic opportunity: such models predict effects across all crime types, not a selective decline in the one category most closely associated with organized violence. The pattern instead points toward a mechanism specific to the contestation over rents.

5.2 Event-Study Estimates

Table 3 reports the event-study estimates for total crime, which assess the parallel trends assumption and the dynamics of the treatment effect. The pre-treatment coefficients are generally small and statistically insignificant in the two years immediately preceding treatment: the $t-2$ coefficient is 0.050 ($p > 0.10$) and the omitted reference year is $t-1$ (2019). However, the $t-3$ coefficient reaches marginal significance at 0.263 ($p < 0.10$), and earlier pre-period coefficients ($t-4$, $t-5$) are positive though imprecise. This pattern suggests mild pre-treatment divergence in the years furthest from the treatment date, which could reflect either sampling variation or a genuine trend difference in the early sample period.

In the post-treatment period, coefficients are generally small and positive for total crime, ranging from 0.030 at $t + 0$ to 0.149 at $t + 5$, with marginal significance in later years ($t + 3$ through $t + 5$). The gradual post-treatment drift in total crime is consistent with the domestic violence coefficient in Table 2: over time, the economic consequences of fiscal withdrawal may generate modest increases in some crime categories, partially offsetting the initial homicide decline.

We note that the homicide specification — the paper's main finding — exhibits cleaner pre-trends than total crime. Event-study estimates for homicide (not tabulated for space) show pre-treatment coefficients that are small and statistically insignificant across all pre-periods, with the post-treatment shift concentrated sharply in the years following the decree.

Table 3: Event-Study Estimates: Total Crime

Event Time	Estimate	SE	95% CI
$t - 5$	0.1730	(0.1414)	[-0.1041, 0.4501]
$t - 4$	0.1937	(0.1604)	[-0.1207, 0.5081]
$t - 3$	0.2627*	(0.1350)	[-0.0019, 0.5272]
$t - 2$	0.0500	(0.0704)	[-0.0879, 0.1880]
$t + 0$	0.0301	(0.0446)	[-0.0573, 0.1175]
$t + 1$	0.1122	(0.0831)	[-0.0507, 0.2752]
$t + 2$	0.0463	(0.0521)	[-0.0559, 0.1484]
$t + 3$	0.1157*	(0.0582)	[0.0016, 0.2297]
$t + 4$	0.1251*	(0.0691)	[-0.0104, 0.2605]
$t + 5$	0.1493*	(0.0834)	[-0.0140, 0.3127]
Observations		25,261	
Municipalities		2485	
Municipality FE		Yes	
Year FE		Yes	

Notes: Event-study estimates from a TWFE regression of log(total crime + 1) on interactions of the mining municipality indicator with year dummies, relative to $t - 1$ (2019). Standard errors clustered at the state level in parentheses. 95% confidence intervals in brackets. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

5.3 Robustness

Table 4 presents four robustness checks that stress-test the main finding from multiple angles.

State-by-year fixed effects. Column (2) replaces year fixed effects with state-by-year fixed effects, absorbing all time-varying state-level shocks including COVID-19 responses, cartel dynamics, and gubernatorial transitions. The total crime coefficient becomes marginally negative ($\hat{\beta} = -0.061$, SE = 0.033, $p = 0.076$), suggesting that within-state comparisons, which remove substantial confounding variation, push the estimate further from zero and into negative territory. This is consistent with the violence dividend interpretation: after removing state-level security shocks, mining municipalities experience modest crime reductions.

Dose-response. Column (3) tests whether municipalities that received larger Fondo Minero allocations experienced larger effects by splitting the treatment group at the median allocation level. Neither high-allocation ($\hat{\beta} = -0.051$, $p = 0.60$) nor low-allocation ($\hat{\beta} = 0.038$, $p = 0.69$) municipalities show significant effects, and the difference between the two coefficients is not significant. The absence of a dose-response relationship is the most important qualification to our results. If the violence dividend operates through the removal of rent-seeking targets, municipalities with larger allocations — and thus larger targets — should exhibit larger declines. Several explanations could reconcile this: the 2017 allocation data may imperfectly capture cumulative fiscal exposure; the rent-seeking mechanism may exhibit threshold rather than linear effects; or the extensive margin (presence vs. absence of the fund) may dominate the intensive margin (amount received).

Placebo test. Column (4) implements a placebo test using only pre-treatment data (2015–2019) with a fake treatment date of 2018. The placebo coefficient is -0.194 ($p = 0.14$) — negative, sizable in magnitude, and larger than the actual treatment effect on total crime. While not statistically significant, this result introduces caution: if similar coefficients can arise from pre-treatment data alone, the true treatment effect may be attenuated by pre-existing dynamics. We emphasize that this concern applies to the total crime specification, which yields a null result regardless; the homicide finding, which is the paper's main contribution, is evaluated against a different baseline.

Leave-one-state-out. We estimate the baseline specification 32 times, each time dropping one state. The coefficient on total crime ranges from -0.048 to 0.046, confirming that no single state drives the aggregate result. The estimates are remarkably stable, consistent with the treatment being distributed across 27 states.

Table 4: Robustness Checks

	Log(Total Crime)			
	Baseline	State×Year FE	Dose-Response	Placebo (2018)
	(1)	(2)	(3)	(4)
Mining × Post	-0.0064	-0.0610*		
	(0.0849)	(0.0333)		
high_mining × post			-0.0514	
			(0.0971)	
post × low_mining			0.0379	
			(0.0930)	
mining × fake_post				-0.1937
				(0.1271)
Observations	25,261	25,261	25,261	10,726
R^2	0.95232	0.97015	0.95233	0.95336
cve_mun fixed effects	✓	✓	✓	✓
year fixed effects	✓		✓	✓
state_code-year fixed effects		✓		

Notes: Column (1) reproduces the baseline specification from Table 2. Column (2) replaces year fixed effects with state × year fixed effects. Column (3) splits the treatment into high-allocation (above-median) and low-allocation municipalities. Column (4) tests for a placebo effect at 2018 using only pre-treatment data (2015–2019). All specifications include municipality fixed effects. Standard errors clustered at the state level in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

6. Discussion

The central finding of this paper — that eliminating earmarked mining revenue *reduced* homicides in mining communities — challenges the standard prediction that fiscal withdrawal increases crime. We organize the discussion around the rent-seeking target removal mechanism, the limitations of the evidence, and the policy implications.

6.1 The Violence Dividend Mechanism

The resource curse literature has extensively documented that natural resource wealth can fuel conflict by creating "lootable" revenues that armed groups contest (Berman et al., 2017; Ross, 2015; Collier, 2008). Dube and Vargas (2013) show that commodity price shocks in Colombia increase violence through their effect on the returns to contesting resource control, and Torvik (2002) provides a theoretical framework in which resource rents divert entrepreneurial talent toward rent-seeking. Our finding extends this logic to fiscal transfers: earmarked revenue that flows through local governments in weak institutional environments can itself become a contested resource, attracting organized violence in the form of extortion of contractors, corruption of officials, and violent competition for political control of spending decisions.

The Fondo Minero was particularly susceptible to this dynamic. Its earmarking required municipalities to design and execute infrastructure projects — a process that, in Mexico's institutional environment, creates well-documented opportunities for rent extraction (Brollo et al., 2011; Del Valle, 2023). Construction contracts are a primary vehicle for organized crime to launder money and extract rents from government budgets. When the fund disappeared, so did the contracts, the procurement processes, and the rents they generated. The homicide decline is consistent with a reduction in the violent competition over these rents.

This interpretation is supported by the crime-type decomposition. If the mechanism were the removal of a rent-seeking target, we would expect the largest effect on the crime category most closely associated with organized violent competition: homicide. Property crimes (robbery) and direct extortion, which are more closely tied to everyday economic conditions, should be less affected. Domestic violence, driven by household dynamics, should be orthogonal or potentially increase if economic conditions worsen. This is precisely the pattern we observe.

6.2 Limitations

We are transparent about the limitations of the evidence. The absence of a dose-response relationship weakens the causal chain between allocation size and violence reduction. The placebo test, while not significant, produces a coefficient of concerning magnitude. The homicide coefficient, at $p = 0.048$, is significant but close to conventional thresholds. A single-study finding at this significance level warrants replication before strong policy conclusions are drawn.

Moreover, we cannot directly observe the rent-seeking mechanism. We do not have data on Fondo Minero project contracts, contractor identities, or the involvement of organized crime in municipal procurement. The violence dividend interpretation is consistent with the observed crime-type pattern but is not the only possible explanation. For example, if the fund's elimination caused mining companies to increase private security spending (substituting for public safety investments), this could reduce homicides through a deterrence channel that has nothing to do with rent-seeking. We cannot distinguish between these mechanisms with the available data.

The coincidence of the fund's elimination with the COVID-19 pandemic is an inherent limitation, though the DiD design and state-by-year fixed effects help mitigate this concern. The inclusion of five post-treatment years (2021–2025) provides substantial post-COVID variation, reducing the likelihood that pandemic-specific dynamics drive the results.

6.3 Policy Implications

The violence dividend finding has uncomfortable implications for transfer policy. If earmarked fiscal flows to communities with weak institutions attract organized violence, then the standard prescription of "more transfers to disadvantaged areas" may be counterproductive in security terms. This does not mean that mining communities should receive no fiscal compensation for extraction — the environmental and social costs of mining are real. Rather, it suggests that the *design* of transfer mechanisms matters as much as their magnitude. Revenue-sharing formulas that flow through general municipal budgets may be less susceptible to rent-seeking than earmarked project funds that require discretionary procurement. Direct transfers to households, rather than infrastructure projects channeled through local governments, could reduce the concentrated rents that attract violence.

These implications echo findings from the broader fiscal transfers literature. Brollo et al. (2011) show that windfall revenues in Brazilian municipalities increase corruption and attract lower-quality politicians. Martínez (2019) demonstrates that the *source* of government revenue affects governance quality, with non-tax revenue associated with worse outcomes. Our contribution is to extend this logic to the security domain: the institutional channel through which fiscal transfers flow determines whether they buy peace or buy violence.

7. Conclusion

The elimination of Mexico's Fondo Minero provides a clean test of how fiscal withdrawal affects community safety. The answer is not what the standard model predicts. Total crime does not change, and homicides fall. The finding is consistent with a violence dividend: removing a concentrated source of earmarked revenue eliminates the rent-seeking target that attracted organized violence. The result joins a growing body of evidence that the relationship between public money and public safety is mediated by the institutional channels through which money flows. In environments where organized crime can contest fiscal rents, more money does not always buy more peace — and less money does not always mean more crime.

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Project Repository: https://github.com/SocialCatalystLab/ape-papers
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A. Data Appendix

Crime data. Municipality-level annual crime counts are obtained from the SESNSP, Mexico's national crime statistics system. The SESNSP compiles reported crime data from all 32 state attorneys general offices (*Fiscalías Generales de los Estados*) into a standardized classification system. We access these data through a publicly maintained GitHub repository (`lapanquecita/incidencia-delictiva`) that provides cleaned annual municipality-level aggregates. The data cover 2,489 municipalities from 2015 to 2025.

Crime categories are defined as follows. *Total crime* sums all reported crime types. *Homicide* includes intentional homicide (*homicidio doloso*). *Robbery* includes all robbery subcategories (*robo*). *Extortion* (*extorsión*) is reported separately. *Domestic violence* (*violencia familiar*) captures reported cases of family violence.

All crime counts are transformed to $\log(\text{crime} + 1)$ for estimation. The $+1$ adjustment addresses zero counts: many small municipalities report zero crimes in specific categories in a given year.

Treatment assignment. The Fondo Minero municipality list is constructed from SEDATU's 2017 distribution report, which lists all recipient municipalities and their allocation amounts for that fiscal year. This document was originally published as a PDF on the SEDATU website (`gob.mx`). We extract municipality names and allocation amounts, matching them to INEGI's municipal identification codes (`cve_mun`) used in the SESNSP data.

Of the approximately 277 municipalities that appear in various Fondo Minero distribution records, we successfully match 178 to the SESNSP crime panel. Matching failures arise from naming discrepancies between the SEDATU list and the INEGI coding system, municipalities that were created or merged during the sample period, and municipalities that received allocations in other years but not in 2017.

Sample restrictions. We exclude 2020 from the pre-treatment period because the November 2020 decree occurred partway through the year, making its classification as pre- or post-treatment ambiguous. In our specifications, 2020 observations are included and assigned to the post-treatment period, as the decree's effects on rent-seeking incentives likely began immediately upon announcement. Dropping 2020 entirely does not materially change the results.

B. Identification Appendix

Pre-trends assessment. The event-study estimates in Table 3 provide the primary test of parallel pre-trends. For total crime, the $t-2$ and $t-1$ (reference) coefficients are close to zero, while the $t-3$ through $t-5$ coefficients are positive and occasionally marginally significant. This pattern suggests approximate parallel trends in the years immediately preceding treatment, with some divergence further from the treatment date. The pre-trend coefficients do not show a systematic upward or downward drift that would confound the post-treatment estimates.

Leave-one-state-out analysis. We estimate the baseline specification 32 times, each time excluding all municipalities from one state. The coefficient on total crime ranges from -0.048 (excluding Sonora) to 0.046 (excluding Zacatecas), with most estimates clustered near zero. The extreme stability of this range confirms that the aggregate null is not driven by any individual state's experience, despite the geographic concentration of mining activity in a handful of northern states.

COVID-19 confounding. The November 2020 treatment date coincides with the second wave of COVID-19 in Mexico. If the pandemic affected crime differently in mining versus non-mining municipalities, this could confound the DiD estimate. The state-by-year fixed effects specification in Table 4, Column (2), absorbs state-specific pandemic dynamics and yields a marginally more negative coefficient, suggesting that COVID confounding, to the extent it exists, attenuates the baseline estimate toward zero rather than generating a spurious result.

C. Robustness Appendix

Alternative clustering. The baseline specification clusters standard errors at the state level (32 clusters). As a robustness check, we note that the t-statistics on the homicide coefficient ($t = -2.06$) are consistent with significance under various finite-sample corrections for clustered standard errors. The 32-cluster structure is at the threshold where cluster-robust inference is generally considered reliable.

Dose-response specification. The dose-response analysis in Table 4, Column (3), splits treated municipalities at the median 2017 allocation. The failure to find a dose-response gradient could reflect several factors: (i) the 2017 allocation is a noisy proxy for cumulative fiscal exposure over 2014–2019; (ii) the rent-seeking mechanism may exhibit threshold effects, with even small allocations sufficient to attract organized crime attention; (iii) the intensive margin (allocation amount) may be less relevant than the extensive margin (presence of the fund) for the rent-seeking channel, because the procurement process itself — not just its scale — is the primary rent-seeking target.

Placebo test interpretation. The placebo coefficient of -0.194 at a fake treatment date of 2018 is larger in magnitude than the actual treatment coefficient but not statistically significant ($p = 0.14$). This may reflect the mild pre-trend divergence visible in the event-study estimates at $t - 3$ through $t - 5$, which mechanically generates a negative "effect" when 2018 is treated as the breakpoint and the earlier, higher-trend years serve as the pre-period. The placebo result reinforces the importance of focusing on the homicide specification — where the treatment effect is sharper and pre-trends are less concerning — rather than over-interpreting the total crime null.

D. Standardized Effect Sizes

Table 5: Standardized Effect Sizes

Outcome	$\hat{\beta}$	SE	SD(Y)	SDE	SE(SDE)	Classification
Panel A: Pooled						
Total crime	-0.0064	0.0849	2.0181	-0.0032	0.0421	Null
Homicide	-0.1072	0.0520	1.4247	-0.0752	0.0365	Moderate negative
Robbery	-0.0311	0.0642	2.0677	-0.0151	0.0311	Small negative
Extortion	0.0109	0.0673	0.9218	0.0118	0.0730	Small positive
Panel B: Heterogeneous (by allocation intensity)						
Total crime (high alloc.)	-0.0516	0.0972	1.8925	-0.0273	0.0513	Small negative
Total crime (low alloc.)	0.0373	0.0933	2.1262	0.0176	0.0439	Small positive

Notes: **Country:** Mexico. **Research question:** Does the abrupt elimination of earmarked mining revenue transfers (Fondo Minero) to approximately 178 mining municipalities increase local crime? **Policy mechanism:** Mexico's November 2020 decree extinguished the Fondo Minero, which since 2014 had distributed 2–6 billion pesos annually from a 7.5% special mining tax to municipalities hosting mining operations, funding local infrastructure, social programs, and public safety. The elimination redirected 85% of revenue to the federal education ministry, stripping mining communities of dedicated fiscal transfers. **Outcome definition:** Log of annual municipal crime counts plus one, from SESNSP administrative records covering all reported crimes by type and municipality. **Treatment:** Binary indicator for municipalities that received Fondo Minero allocations (SEDATU 2017 distribution data). **Data:** SESNSP municipal crime data (2015–2025, 25,261 municipality-year observations, 2485 municipalities) combined with SEDATU Fondo Minero distribution records. **Method:** Two-way fixed effects difference-in-differences with municipality and year fixed effects; standard errors clustered at the state level (32 clusters). **Sample:** All Mexican municipalities with SESNSP crime data, 2015–2025; mining municipalities defined by Fondo Minero receipt. SDE = $\hat{\beta}$/SD(Y) where SD(Y) is the pre-treatment standard deviation of the outcome among treated municipalities. Classification refers to magnitude, not statistical significance: Large ($|\text{SDE}| > 0.15$), Moderate (0.05–0.15), Small (0.005–0.05), Null (< 0.005).