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Paz Total? How Ceasefires Backfire*

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June 23, 2026

Abstract

Ceasefires are often designed to reduce violence while facilitating peace negotiations or humanitarian access. But poorly designed truces can backfire. This paper examines the 2023 ceasefires decreed by Colombia's government with several organized criminal groups simultaneously under the *Paz Total* (Total Peace) policy. Using difference-in-differences on a municipality-month panel, we find that while more visible and salient forms of violence such as homicides, terrorist attacks and massacres were unaffected, less visible forms of violence against civilians, such as extortion, forced recruitment of minors, and threats, increased substantially in municipalities with ceasefire group presence. These patterns are consistent with strategic substitution: armed groups shifted from more visible to less visible forms of violence when political constraints changed. We formalize this mechanism in a structural model of strategic violence allocation, calibrate its parameters to the reduced-form estimates, and simulate policy counterfactuals. The calibration implies that roughly a 50% increase in the detectability of less visible forms of violence, through independent verification missions or community reporting systems, would be needed to fully offset the ceasefire's perverse effects, restoring total violence to approximately its pre-ceasefire level. Our findings highlight the unintended consequences of inadequately designed ceasefire agreements and underscore the need for credible monitoring and enforcement mechanisms.

Keywords: Ceasefires, Violence, Criminal Governance.

JEL Classification: D74, K42.

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¿Paz Total? Cómo los ceses al fuego pueden resultar contraproducentes[*]

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June 23, 2026

Abstract

Los ceses al fuego suelen diseñarse para reducir la violencia y facilitar negociaciones de paz. Sin embargo, mal diseñados pueden resultar contraproducentes. Este artículo examina los ceses al fuego decretados en 2023 por el gobierno colombiano en el marco de la política de Paz Total. Utilizando un diseño de diferencias en diferencias encontramos que, mientras las formas de violencia más visibles y notorias (homicidios, ataques terroristas y masacres) no se vieron afectadas, las formas menos visibles de violencia contra la población civil (extorsión, reclutamiento forzado de menores, amenazas) aumentaron en municipios con presencia de grupos cobijados por los ceses. Estos patrones son consistentes con un mecanismo de sustitución estratégica de la violencia por parte de los grupos criminales: cuando cambian las restricciones políticas, estos grupos sustituyen formas de violencia más visibles por formas menos visibles. Formalizamos este mecanismo en un modelo estructural de asignación estratégica de la violencia, calibramos sus parámetros y simulamos contrafactuales de política. La calibración implica que sería necesario aumentar en un 50% la probabilidad de detección de las formas menos visibles de violencia —como misiones de verificación independientes— para contrarrestar plenamente los efectos perversos de los ceses al fuego. Nuestros resultados resaltan las consecuencias no previstas de ceses al fuego mal diseñados y subrayan la necesidad de contar con mecanismos creíbles de monitoreo y verificación.

Palabras clave: Ceses al fuego, violencia, gobernanza criminal.

Clasificación JEL: D74, K42.

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1 Introduction

Ceasefires are widely recognized as pivotal mechanisms in conflict resolution, serving as temporary or permanent pauses in hostilities to facilitate humanitarian action, foster dialogue, and establish the groundwork for lasting peace agreements. Yet the empirical literature on whether ceasefires actually reduce violence remains remarkably thin, and the theoretical literature warns that poorly designed truces can backfire (Clayton et al., 2021; Sticher, 2022). This paper provides the first *subnational* causal evaluation of a large-scale ceasefire initiative, decreed by the Colombian government from January to June 2023. The policy consisted of multiple ceasefires implemented simultaneously with distinct organized criminal groups under the *Paz Total* (Total Peace) initiative. It represented a major departure from the predominantly militarized responses of past Colombian governments, which reserved peace dialogues for guerrilla organizations with explicit political aims. By extending negotiations to groups with varied motives—from active insurgencies to criminal organizations—the government aimed to reduce violence, mitigate humanitarian crises, and build trust for subsequent peace negotiations. But despite the stated intentions, the *Paz Total* policy failed to deliver its intended reduction in violence. Instead, armed groups intensified criminal governance and civilian victimization.

Using detailed monthly municipal-level data from Colombia's Special Jurisdiction for Peace (JEP), we construct a panel covering the period between June 2022 and June 2023, and estimate the causal effect of the ceasefire on multiple dimensions of violence. Our difference-in-differences design exploits variation in armed group presence across municipalities combined with the timing of the ceasefire implementation in January 2023. The ceasefire covered 39% of Colombian municipalities, 65% of the country's territory, and approximately 29% of its population—a scale that makes the policy's outcomes consequential for the entire country.

We uncover a striking pattern of strategic substitution. While more visible forms of violence—homicides, massacres, and terrorist attacks—were largely unaffected by the ceasefire, less visible forms of violence surged: extortion increased by 337%, threats by 70% and forced recruitment of minors by 168%. Criminal governance, a composite of illegal checkpoints, lockdowns, extortion, threats, forced recruitment of minors and forced displacement, rose by 32%. This pattern suggests that, in the absence of clear protocols and credible verification mechanisms, armed groups exploited the ceasefire to consolidate territorial control through less visible coercion, substituting more visible forms of violence that could attract scrutiny and jeopardize negotiations with less detectable forms of predation. Importantly, our findings show that the ceasefire reduced direct combat engagement and state lethality against armed groups, but did not reduce operations against illegal economies: clashes between military forces and criminal groups decreased by 73% and non-state combatants killed decreased by 46%, while state operations against illegal economies were unaffected by the ceasefires.

These findings are robust to a battery of specification checks: imputation-based DID (Borusyak et al., 2024), doubly robust difference-in-differences (Sant'Anna and Zhao, 2020) with covariates selected via double-selection LASSO (Belloni et al., 2014), alternative control group definitions, and alternative data sources. Across all specifications, the core result of increased less visible violence and criminal governance with no change in more visible forms violence holds.

To move beyond reduced-form effects and provide quantitative policy guidance, we develop a structural model of strategic violence allocation. In the model, an armed group chooses a portfolio of more visible and less visible violence facing a government enforcement technology in which more visible violence is more detectable. The ceasefire enters as a dual shock: a reputational cost on more visible violence (the group is "at the table") and an enforcement dampening that weakens the government's capacity to punish both types. The model delivers five equilibrium predictions that map one-to-one onto our five core empirical findings, including the null effect on more visible violence, the surge in less visible violence, and the expansion of criminal governance. We estimate the model's six structural parameters via GMM, matching them to the reduced-form DID coefficients and pre-ceasefire violence means.

The calibrated model enables three policy counterfactuals that speak directly to the design of future ceasefire agreements. First, and most robustly, a *ceasefire with enhanced monitoring*: raising the detectability of less visible activities by approximately 50%—through independent monitoring and verification missions or community reporting systems—would fully neutralize the adverse effects on less visible violence and bring total violence approximately back to its pre-ceasefire level. This result depends an enforcement dampening parameter which is identified by the differential reduction in government enforcements during the ceasefire period. Second, a *ceasefire with enforcement*: eliminating the enforcement dampening while preserving the reputational channel would reduce more visible violence without increasing less visible violence—the best of both worlds, though politically difficult given the tensions between defense authorities and peace negotiators that shaped the *Paz Total* process. Third, an *optimal ceasefire design*: minimizing total violence subject to the armed group's participation constraint yields a combination of minimal enforcement dampening and high reputational costs that cuts total violence by approximately 19% relative to pre-ceasefire levels. These counterfactuals provide concrete, quantitative guidance for ceasefire design and directly question the architecture of the *Paz Total* initiative.

This paper contributes to several strands of the literature. First, we contribute to the emerging empirical literature on ceasefires and violence. While theoretical and descriptive work on ceasefires has emerged in recent years (Clayton et al., 2021, 2023b; Sticher, 2022; Bara and Clayton, 2023; Fortna, 2004), quantitative causal evidence remains scarce. Clayton et al. (2023a) emphasize this gap explicitly. One exception is Armand et al. (2023), which uses a regression discontinuity design on the universe of ceasefires since 1993 and find marginal effects on violence but positive effects on economic recovery; importantly, their effects are driven by state-based conflicts while civilian-targeted violence remains unaffected—a pattern consistent with our strategic substitution findings. Lundgren et al. (2023) study local ceasefires in Syria, while Ryland et al. (2018) provide descriptive evidence from previous Colombian peace processes. We advance this literature in three ways: by providing the first subnational causal estimates, by looking beyond fatalities to the full portfolio of violence that armed groups deploy, and by showing that evaluating ceasefires on their stated objective—reducing lethal violence—can miss the most consequential effects entirely.

Second, we show that the *composition* of violence matters as much as its *level*. Most empirical work evaluates ceasefires by counting battle deaths or conflict events and declaring success or failure (Fortna, 2004; Armand et al., 2023). Our disaggregated analysis reveals that this approach is misleading: a ceasefire can leave lethal violence unchanged while dramatically expanding the coercive apparatus through which armed groups govern civilian populations. The 32% increase in criminal governance we document has direct welfare consequences for millions of people, but it would be invisible in a fatality-based evaluation. This finding connects to the broader literature on criminal governance (Lessing, 2017; Arias, 2017; Blattman et al., 2025; Sanchez de la Sierra, 2020) and on how constraints on one form of violence can amplify others (Condra and Shapiro, 2012; Dell, 2015; Castillo et al., 2020).

Third, we bridge reduced-form causal inference with structural policy design to map the DiD estimates to six interpretable parameters. The resulting calibrated model generates quantitative answers to questions such as how much monitoring investment is needed, what would happen if enforcement were maintained, and what the optimal ceasefire looks like. This approach delivers concrete, actionable policy guidance rather than the type of generic recommendations that typically accompany empirical studies of peace processes.

2 Context

2.1 How can ceasefires backfire

Ceasefires agreements are not neutral pauses in hostilities, they are strategic instruments embedded in the broader political and military calculations of conflict actors (Sticher, 2022; Bara and Clayton, 2023). Clayton et al. (2021) distinguish between a ceasefire's immediate objective—cessation of hostilities—and its underlying purpose, such as advancing negotiations or protecting civilians. When the two are misaligned, or when the design lacks credible verification mechanisms, ceasefires may fail. Consistent with this, Armand et al. (2023) find that ceasefires worldwide reduce state-based conflict but leave other forms of violence—particularly civilian-targeted hostilities—largely unaffected.

Several mechanisms can generate perverse effects. First, a ceasefire may alter the relative costs of different types of violence without reducing armed groups' underlying incentives to control territory. In the light of the bargaining framework of Fearon (1995) and Walter (2002), a ceasefire is credible only when parties face costs for defection that exceed the benefits of continued predation. When monitoring is weak—as in the Colombian case, see below—the effective cost of less visible violence falls, while the cost of more visible violence rises due to the political attention generated by the ceasefire. This asymmetry creates incentives for substitution across different types of violence.

Second, ceasefires may generate a commitment problem. Again, Fearon (1995) and Walter (2002) argue that civil war settlements fail when parties cannot credibly commit to disarming (or when guarantors are absent). In Colombia, the absence of established verification protocols and the premature declaration of ceasefires before formal negotiations had begun created precisely this environment. Armed groups faced minimal costs for violations that were difficult to observe, while the state's hands were tied by the ceasefire decrees themselves.

Third, ceasefires that constrain state military operations without corresponding constraints on non-state actors can shift the balance of territorial control. Fearon

(2004) emphasizes that civil wars persist when the state cannot credibly project power into contested territories. If a ceasefire disproportionately constrains the state, it may create a vacuum that armed groups fill with intensified governance and predation—what the literature on criminal governance terms "criminal state-building" (Lessing, 2017; Sanchez de la Sierra, 2020).

A growing literature documents how armed groups govern civilian populations through a combination of coercion and service provision (Arjona, 2016; Lessing, 2017; Arias, 2017; Blattman et al., 2025), regulating daily life, extracting rents, and resolving disputes. Ceasefires may create a particularly favorable environment for the expansion of criminal governance for two reasons. First, by reducing the probability of military operations, ceasefires lower the operating costs of criminal governance activities that require a stable presence (i.e., extortion networks, illegal checkpoints, and forced recruitment). Second, the political legitimacy conferred by participating in a peace process may embolden groups to expand their governance functions, signaling to local populations that their rule is becoming permanent.

The core theoretical prediction that emerges from this discussion is that armed groups substitute between different types of violence in response to changes in the political environment and constraints. This prediction draws on the logic of strategic violence developed by Kalyvas (2006), who argues that armed actors calibrate violence to their informational and political constraints. When certain types of violence become politically costly, groups shift to less detectable forms of coercion. More visible forms of violence (homicides, massacres, armed confrontations) generate media attention, public outrage, and pressure on the government to suspend negotiations. In contrast, less visible forms of violence (extortion, threats, forced recruitment, criminal governance) allow groups to consolidate territorial control without attracting the scrutiny that could trigger a ceasefire collapse. This strategic substitution hypothesis finds support in related empirical work: Dell (2015) shows that drug trafficking organizations in Mexico responded to military crackdowns by reallocating violence across territories; Condra and Shapiro (2012) demonstrate that armed groups in Iraq adjusted their violence tactics in response to civilian reactions (e.g. punishment, information flows, and local preferences); and Castillo et al. (2020) find evidence of strategic reallocation of violence in the Mexican drug war following supply disruptions.

These conceptual arguments motivate a formal model that clarifies the channels through which ceasefires affect violence choices and generates sharp testable predictions. We develop that model after mapping this theoretical discussion to the specific features of the Colombian 2023 ceasefires.

2.2 *Paz Total* and the 2023 ceasefires in Colombia

Since President Gustavo Petro took office in August 2022, Colombia's security landscape has been shaped by the *Paz Total* policy. It represented a major departure from the predominantly militarized responses of past governments, which generally reserved peace dialogues for guerrilla organizations with explicit political aims. By contrast, the new approach extended negotiations to groups with varied motives, from the long-established National Liberation Army (ELN from the Spanish acronym) to criminal organizations like the Gaitanista Self-Defense Forces of Colombia (AGC, also called *Clan del Golfo*) and the newly configured dissidents of the Revolutionary Armed Forces of Colombia (FARC), which demobilized in 2016 (Bonilla and Daza, 2025; Saffon and Garcia, 2023). The ceasefires were enacted via a set of government decrees, each one referring to one of five criminal organizations or insurgencies: the FARC dissidencies (Estado Mayor Central—EMC—and Segunda Marquetalia); ELN; and two neo-paramiliray organizations or criminal bands (AGC and Autodefensas Conquistadoras de la Sierra Nevada—ACSN).

Early assessments revealed the emergence of unintended consequences, highlighting the lack of adequate sequencing and planning. For instance, Llorente et al. (2023) argue that the decrees lacked explicit rules or protocols, which opened the door to contradictory interpretations and limited compliance. In many regions, state security forces struggled to understand which offensive operations to pause and which to continue, and local populations received insufficient information about their rights and protections. Eventually, groups such as the AGC unilaterally suspended cooperation and allegedly engaged in operations to expand territorial control (Bonilla and Daza, 2025). These setbacks exposed a fundamental tension: while the administration aimed to reduce violence and garner humanitarian relief, organized criminal groups continued to pursue strategic gains, either by quietly entrenching themselves or by capitalizing on the absence of military pressure (Saffon and Garcia, 2023). Bonilla and Daza (2025) characterize *Paz Total* as a tapestry of presidential announcements and exploratory talks with a range of criminal and insurgent factions, with no strategic backbone or coherent implementation roadmap.

The policy also appears to have ignored the underlying complexity of local realities. Llorente et al. (2023) detail how certain zones experienced partial or overlapping ceasefires, creating a patchwork of security regimes. In some territories violence subsided due to the consolidation of single dominant actors (e.g. the Bajo Cauca region, dominated by the AGC), whereas in the departments of Putumayo and Cauca, ongoing territorial disputes among multiple armed groups led to sustained or even heightened confrontations (Saffon and Garcia, 2023). Limited state presence in rural and remote regions further constrained the government's capacity to enforce ceasefire provisions and protect civilians. As a result, civilian populations frequently found themselves caught in the crossfire of violent confrontations, unprotected by a state apparatus unsure of its mandate to act.

A further source of uncertainty was the absence of a legal framework to negotiate with criminally-motivated organizations. For instance, negotiations with groups primarily pursuing illicit profits (such as the AGC) are constrained by constitutional prohibitions on amnesties for crimes like drug trafficking (Bonilla and Daza, 2025; Saffon and Garcia, 2023). This legislative vacuum has hampered the administration's capacity to offer credible incentives for demobilization, and disagreements between defense authorities and peace negotiators have further stalled progress (Saffon and Garcia, 2023). In contrast, Colombia has in place legal and internationally recognized mechanisms to negotiate with insurgencies such as the ELN.

In sum, the Colombian case illustrates the complexity of negotiating ceasefires with multiple armed actors pursuing diverse political, economic, and criminal agendas and lacking strategic planning, robust legal frameworks, and verifiable milestones. The absence of those elements exacerbates the risk that such agreements may backfire, increasing localized violence rather than fostering sustainable peace.

3 A Model of Ceasefires and Strategic Violence Substitution

Section 2 highlights that ceasefires are not neutral pauses in conflict but rather strategic instruments whose effects depend critically on design and enforcement. This section develops a theoretical framework that bridges conceptual insights from the ceasefire and criminal governance literature, with a formal model of strategic violence allocation. The framework clarifies why poorly designed ceasefires can increase, rather than decrease, criminal violence, and it generates five testable predictions that we confront with data in Section 6.

3.1 Setup

Consider a municipality in which an armed group (A) operates and the government (G) chooses enforcement effort. We analyze a single period and compare equilibria before and after a ceasefire announcement. The criminal group chooses, in each municipality, the levels of more visible and less visible violence it exerts.

- **More visible violence** $v \geq 0$: homicides, massacres, terrorist attacks, armed confrontations. Actions that are salient and easily detected.
- **Less visible violence** $c \geq 0$: extortion, threats, forced recruitment, forced displacement, criminal governance. Less salient and harder to verify.

Both instruments generate rents but through different channels that we abstract from. Rather, for simplicity we assume that armed group's gross rents are

$$R(v, c) = \alpha v^a + \beta c^b, \tag{1}$$

where $\alpha, \beta > 0$ are productivity parameters and $a, b \in (0, 1)$ impose diminishing returns. We assume $b > a$, namely less visible violence is more efficient at generating rents per unit of effort, reflecting that extortion and criminal governance extract resources directly while more visible violence operates indirectly through territorial control.

Regarding the detection technology, the government detects and punishes violence with an intensity that depends on enforcement effort $e \geq 0$ and the visibility of violence. The expected punishment cost is

$$P(v, c, e) = e[\delta_v v + \delta_c c],$$

where $\delta_v > \delta_c > 0$ are detection probabilities, implying that more visible violence is more detectable than less visible violence. Put it differently, a massacre is far more likely to trigger a military response than an increase in extortion demands.

The ceasefire. The ceasefire is modeled as triggering two simultaneous parameter shifts. First, a *reputational cost* $\phi > 0$: the armed group incurs an additional cost ϕv for more visible forms of violence, since being at the "negotiating table" makes salient violence politically costly. This cost reflects the political logic emphasized in the ceasefire literature: more visible violence during negotiations undermines the group's credibility as a negotiating partner and can trigger a breakdown of the process. Instead, less visible violence is harder to attribute during negotiations so it carries no such penalty.

Second, an *enforcement dampening* $\lambda \in (0,1)$: the ceasefire scales government enforcement effectiveness by $(1-\lambda)$, reflecting the political constraints on military operations against a negotiating partner. These constraints arise from disagreements between defense authorities and peace negotiators (as seen in the Colombian case discussed in section 2.2) and from international pressure to give negotiations a chance. The government also faces an additional political cost $\mu > 0$ of enforcement during the ceasefire.

3.2 Equilibrium

Given enforcement e and ceasefire parameters (ϕ, λ), the armed group solves:

$$\max_{v,c\geq 0} \; [\alpha v^a + \beta c^b] - (1-\lambda)e[\delta_v v + \delta_c c] - \phi v - \frac{\eta}{2}(v^2 + c^2), \tag{2}$$

where $\eta > 0$ is a convex effort-cost parameter capturing the armed group's resource constraints (fighters, logistics, organizational capacity).

That is, the armed group chooses the levels of more visible and less visible violence to maximize its rents (first term in squared brackets) net of: the enforcement cost, the reputational cost, and the cost of effort (second to fourth terms respectively). Interior first-order conditions are:

$$v: \qquad \alpha a v^{a-1} = (1-\lambda)e\delta_v + \phi + \eta v, \tag{3}$$

$$c: \qquad \beta b c^{b-1} = (1-\lambda)e\delta_c + \eta c. \tag{4}$$

The left-hand side of each condition is the marginal rent from each type of violence (decreasing, since $a, b < 1$). The right-hand side is the marginal cost: enforcement penalties (scaled by detectability), reputational cost (only for v), and effort cost.

Meanwhile, the government chooses enforcement $e \geq 0$ to minimize a weighted sum of violence against civilians and enforcement costs:

$$\min_{e\geq 0} \gamma_v v(e) + \gamma_c c(e) + \frac{\kappa}{2}e^2 + \mu e, \tag{5}$$

where $\gamma_v > \gamma_c > 0$ are the government's welfare weights on more visible and less visible violence ($\gamma_v > \gamma_c$ because more visible violence is more salient so it generates more political pressure), $\kappa > 0$ is the marginal cost of enforcement effort, and $\mu \geq 0$ is an additional political cost of enforcement introduced by the ceasefire ($\mu = 0$ pre-ceasefire, $\mu > 0$ post-ceasefire).

The government anticipates the armed group's best-response functions $v(e)$ and $c(e)$ implicitly defined by (3)–(4). Substituting into (5), the first-order condition for e is:

$$\gamma_v \, v'(e) + \gamma_c \, c'(e) + \kappa \, e + \mu \;=\; 0. \tag{6}$$

Since higher enforcement reduces both v and c (i.e., $v'(e) < 0$ and $c'(e) < 0$), the first two terms are negative, providing the marginal benefit of enforcement. The last two terms are the marginal cost.

Assume that pre-ceasefire: $\phi = \lambda = \mu = 0$. Instead, post-ceasefire all three parameters are positive. Denote the pre- and post-ceasefire equilibria as (v_0, c_0, e_0) and (v_1, c_1, e_1) respectively.

Proposition 1 (Violence substitution)**.** *Holding enforcement fixed, a ceasefire ($\phi > 0, \lambda > 0$) causes: (i) more visible violence to weakly decrease: $v_1 \leq v_0$ and; (ii) less visible violence to strictly increase: $c_1 > c_0$.*

Intuition. From (3), the ceasefire raises the marginal cost of more visible violence by $\phi - \lambda e \delta_v$ (that is, the reputational penalty net of enforcement relief). From (4), the marginal cost of less visible violence falls by $\lambda e \delta_c$ with no offsetting reputational cost.[1]

Corollary 1. *More visible violence is unchanged ($v_1 = v_0$) when $\phi = \lambda e_0 \delta_v$: the reputational cost exactly offsets the enforcement relief for salient activities.*

The interpretation of this result is straightforward: Kalyvas (2006) shows that armed actors calibrate violence to political constraints. In our setting, the constraint shifts when groups enter negotiations. The reputational cost makes more visible violence more costly, but the enforcement dampening makes it cheaper to execute. When these two effects exactly cancel (the knife-edge condition), more visible violence is unchanged. Our empirical finding that more visible and salient violence is statistically unchanged (see section 6) is consistent with this condition holding approximately.

Proposition 2 (Government enforcement under ceasefire)**.** *Under the ceasefire, government enforcement is reduced ($e_1 < e_0$) when the additional political cost μ of enforcing against a negotiating partner is greater than the increased marginal benefit of enforcement from rising less visible violence, discounted by the low weight γ_c.*

[1] See formal proof in Appendix D.

Intuition. This result reflects the political logic of a ceasefire. The government trades enforcement intensity for political capital. Because less visible violence is low-salience, the political cost of tolerating it is small relative to the political benefit of maintaining the ceasefire. This mechanism reflects the institutional reality in Colombia where the peace negotiation team wielded veto power over defense operations.

Proposition 3 (Criminal governance)**.** *Assume criminal governance (the armed group's provision of order, dispute resolution, taxation, and regulation in the territory it controls) is a function of less visible violence capacity and government enforcement:* $g = g(c, e)$ *with* $g'(c) > 0$*,* $g'(e) < 0$*. Then,* $c_1 > c_0$ *and* $e_1 < e_0 \Rightarrow g_1 > g_0$*, so the ceasefire expands criminal governance.*

Intuition. When the state's enforcement capacity is dampened by ceasefire constraints, profit-seeking armed groups use the lower risk from military operations to use less visible violence with the aim of restructuring their territorial control toward institutions that govern civilian populations through extortion networks, forced recruitment pipelines, and social coercion (Lessing, 2017; Arias, 2017; Blattman et al., 2025; Sanchez de la Sierra, 2020).

The mechanisms identified in Propositions 1 to 3 combine to generate a full characterization of ceasefire equilibrium:

Proposition 4 (Ceasefire equilibrium)**.** *Under the ceasefire* $(\phi > 0, \lambda > 0, \mu > 0)$*, with the knife-edge condition* $(\phi = \lambda e_0 \delta_v)$ *approximately satisfied:*

1. $c_1 > c_0$*: less visible violence increases;*
2. $v_1 \approx v_0$*: more visible violence unchanged;*
3. $e_1 < e_0$*: government enforcement decreases.*
4. $g_1 > g_0$*: criminal governance increases;*

These four predictions map directly onto the four core empirical findings in Section 6.[2]

4 Data

Our empirical analysis focuses on criminal violence committed by FARC dissidents (Segunda Marquetalia and Estado Mayor Central) and the AGC, as these groups

[2]The model also generates additional testable predictions on heterogeneity by armed group capacity, state presence, detection technology, ceasefire duration, and multi-group competition. We present these in Appendix D. One caveat of our stylized model is that it is static and therefore does not formally capture dynamic commitment problems or reputation-building across ceasefire rounds.

represent the most significant non-state armed actors involved in the first *Paz Total* ceasefire. Although the ACSN was also included, it is not a prominent actor in the broader Colombian conflict, and its presence was confined to only two municipalities. By focusing on FARC dissidents and AGC, we ensure that our estimates capture the dynamics of the most consequential ceasefire participants.

Our primary data source is a comprehensive set of conflict events compiled by the Special Jurisdiction for Peace (JEP), which includes homicides, terrorist attacks, massacres, kidnappings, forced displacement, forced recruitment, extortion, threats, lockdowns, illegal checkpoints, armed clashes, military operations, and the outcomes of such operations. These event-based data are recorded with detailed information on the location (municipality), date, type of event, a brief description, and the alleged perpetrator. Using these data, we construct a monthly panel covering June 2022 to June 2023, in which the number of violent incidents is standardized to facilitate comparability across municipalities and over time. We also use these records to construct armed group presence indicators prior to the ceasefires: a municipality is classified as having the presence of a given group if any conflict event involving that group was observed during 2019–2021.

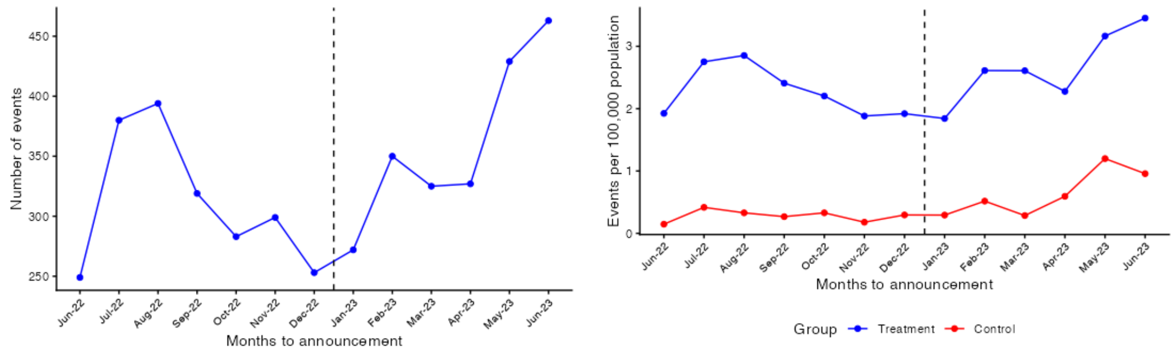
We supplement the primary data with additional conflict information from the Electoral Observatory Mission (MOE). The MOE data are particularly useful for robustness checks on criminal governance outcomes (illegal checkpoints, lockdowns, extortion, and threats).

Finally, we incorporate municipal characteristics from multiple sources: coca crop presence from the Illicit Crops Integrated Monitoring System (SIMCI) project of the Colombia office of the United Nations Office on Drugs and Crime (UNODC), and socioeconomic indicators from the CEDE panel at Universidad de los Andes, including population, rurality index, area, altitude, distance to departmental capital and to Bogotá, the poverty rate, and municipal revenues and expenditures.

4.1 Descriptive patterns

Panel A of Figure 1 depicts the raw evolution of total violent events across Colombia from June 2022 to June 2023. Panel B disaggregates this trajectory by comparing municipalities exposed to the *Paz Total* "treatment" (those with presence of groups for which a ceasefire was decreed) with municipalities not exposed to the *Paz Total* "treatment" (those without such presence). Both panels highlight January 2023, when all the ceasefires officially began. There is a marked increase in violent events

Figure 1. Raw data: Violence from June 2022 to June 2023



(a) Events

(b) Events by treatment

This figure presents the evolution of violent events over time. Panel A displays the overall trend for our sample, including events of terrorism, homicides, forced displacement, massacres, kidnappings, forced recruitment of minors, illegal checkpoints, lockdowns, extortion, threats, and armed clashes. Panel B disaggregates the data, comparing municipalities affected by the Paz Total policy with those that were not. The dotted vertical line marks the beginning of the first Paz Total ceasefire announcement in January 2023.

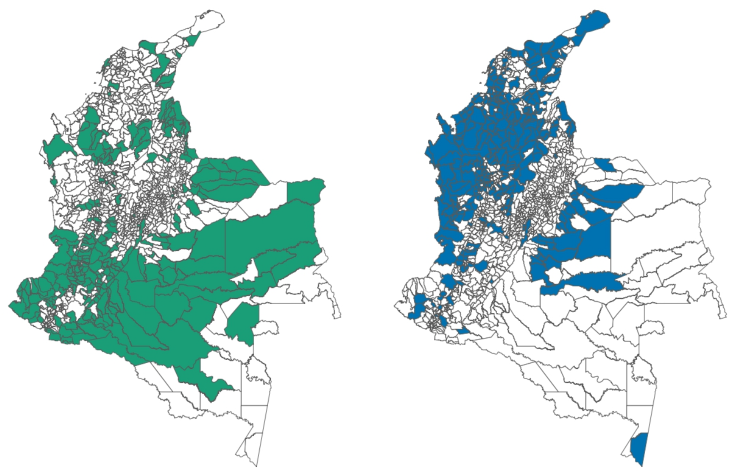
from that month onward in all the territory, but differentially larger in treated municipalities.[3]

Figure 2 maps the pre-ceasefire spatial distribution of armed group presence. Panel A shows municipalities with pre-ceasefire presence of FARC dissidences and Panel B shows the pre-ceasefire presence of the AGC. The maps confirm concentrated presence in southwestern Colombia (specifically in the departments of Cauca, Nariño), the south (Putumayo, Meta), the Pacific coast (Chocó), northern Antioquia, the Darién region in the North West, and areas bordering Venezuela. The union of the territory covered by the groups whose pre-ceasefire presence is reported in the appendix in Figure B2 constitutes by definition the set of municipalities included in the ceasefire announcement. The ceasefire encompasses 39% of Colombian municipalities, 65% of the country's territory, and approximately 29% of its population.

Appendix Table A1 reports the descriptive statistics of all our outcomes.

[3]Appendix Figure 2 presents a covariate balance assessment using standardized mean differences for pre-sample (2018) municipal characteristics. Treated municipalities tend to have greater coca crop presence, higher crime rates, greater distance from Bogotá, higher homicide rates, more land conflicts, larger populations and area, higher government revenues and spending, more terrorist attacks, and higher poverty levels. These differences imply that the presence of insurgents and criminal groups targeted for the ceasefires is not fortuitous, which motivates our identification strategy.

Figure 2. Non-state armed groups presence in Colombian municipalities



(a) Ex-FARC (b) AGC

This figure shows the spatial distribution of armed groups in Colombia, highlighting the areas where different non-state armed actors maintained a presence between 2019-2021.

5 Empirical Strategy

The spike in violence following the January 2023 ceasefire announcement (Figure 1) could be driven by factors other than the causal effect of the *Paz Total* ceasefire. The potential confounding role of omitted factors is illustrated by the differences in observable pre-ceasefire characteristics reported in Appendix Figure B1. To account for such a possibility, our difference-in-differences (DiD) empirical strategy leverages variation in both the timing of the start of the ceasefires and in the geographic coverage of municipalities affected by the policy. Let i index municipalities and t index months, the *conceptual* difference-in-differences estimand is:

$$y_{it} = \alpha_i + \gamma_t + \beta(D_i \times Post_t) + \varepsilon_{it} \tag{7}$$

where y_{it} is a (population-weighted) standardized measure of criminal violence in municipality i during month t; D_i equals one if the municipality is covered by the first *Paz Total* ceasefire; $Post_t$ equals one for the post-treatment period (after January 2023 onward); α_i captures municipality fixed effects; and γ_t captures month fixed effects.

In practice, however, we do not estimate a standard two way fixed effects specification such as the one described in 7. As mentioned, treated municipalities differ from controls along several baseline dimensions. We therefore use the imputation estimator of Borusyak et al. (2024) (hereafter, BJS) as our preferred specification. It first estimates the untreated potential-outcome model using only untreated observations and then imputes counterfactual outcomes for treated municipalities in the post-ceasefire period. In our setting, this approach provides a transparent way to construct the untreated counterfactual while preventing treated post-ceasefire outcomes from influencing the estimated counterfactual path. The identifying assumption is that, conditional on municipality and month fixed effects, as well as selected pre-treatment covariates, treated municipalities would have followed the same untreated-outcome path as comparable control municipalities absent the ceasefire. We restrict the control group to municipalities with no presence of any non-state armed group included in the ceasefire and eliminate from the sample all municipalities with more than 100,000 inhabitants (effectively dropping large urban areas with little to no presence or illegal armed groups). Standard errors are clustered at the municipality level. Under that assumption, β, measures the differential change in criminal violence (or government enforcement activity) following the ceasefire in municipalities where ceasefire groups were active, relative to municipalities without such groups.

While the identifying assumption is inherently untestable, we assess its plausibility through an event-study design of the form:

$$y_{it} = \alpha_i + \gamma_t + \sum_{j \neq \text{Dec2022}} \beta_j (D_i \times \gamma_j) + \varepsilon_{it} \tag{8}$$

where each β_j captures the differential violence between treated and control municipalities in sample month j relative to the December 2022, which we omit to avoid collinearity.

We plot the β_j coefficients together with their confidence intervals to examine pre-treatment dynamics. This also allows us to study post-treatment dynamics. If violence in treated municipalities was already diverging from control municipalities before the beginning of the ceasefire, this would undermine our confidence in the assumption that counterfactual pre-trends would have stayed parallel absent the treatment, preventing a causal interpretation of the estimated ceasefire effects.

6 Results

6.1 Main Results

The main results focus on violence committed by the groups included in the ceasefire (Segunda Marquetalia, Estado Mayor Central, and the AGC), as well as actions by government forces.

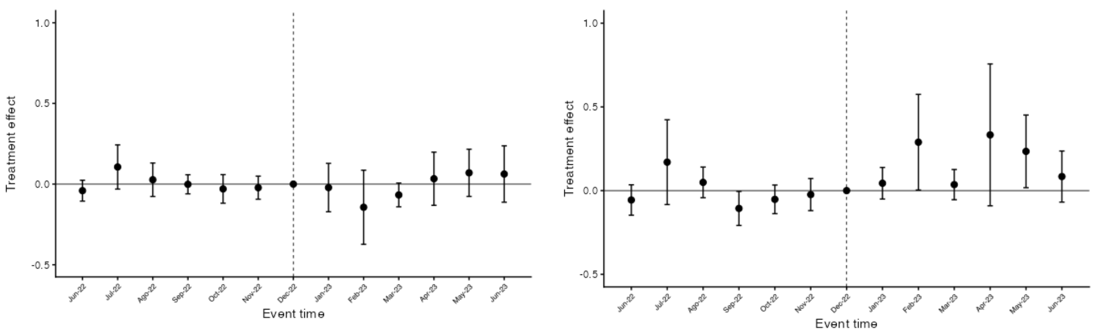
Table 1. Impact of the Paz Total ceasefire on more visible and less visible violence in Colombia

Dep. Var: Standardized index of:	More Visible violence	Less Visible violence
	(1)	(2)
Post × Treated	-0.011	0.140**
	(0.041)	(0.057)
Observations	14,157	14,157
Municipalities	1,089	1,089
Municipality FE	Yes	Yes
Time FE	Yes	Yes

Estimates are from an imputation-based difference-in-differences model (Equation 7) following Borusyak et al. (2024). Standard errors, clustered at the municipality level, are reported in parentheses. The sample is a municipality-month panel covering 1,089 municipalities over 13 months. *Treated* equals 1 for municipalities with a presence of a non-state armed group involved in the first *Paz Total* ceasefire (Segunda Marquetalia, Estado Mayor Central, or Autodefensas Gaitanistas de Colombia). *Post* equals 1 for the post-treatment period. Outcomes are two aggregate measures of more visible and less visible violence. More Visible violence includes: terrorism, homicides, massacres, clashes, and checkpoints. Less visible violence includes: extortion, threats, forced recruitment of minors, kidnappings, lockdowns, and forced displacement. Each column reports treatment effect estimates for a different standardized outcome, as specified in the column header. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 1 reports the estimated difference-in-differences coefficient of the ceasefire on violence that we code as either *more visible* (Column 1) or *less visible* (Column 2). The former includes terrorism, homicides, massacres, clashes (where fire is exchanged with government forces), and checkpoints. In turn, less visible violence includes extortion, threats, forced recruitment of minors, kidnappings, lockdowns, and forced displacement. Appendix table B1 provides a detailed justification of which violence outcomes are coded as more or less visible. All outcomes are standardized. The implementation of the ceasefire under the *Paz Total* initiative had no effect on more visible violence (there is a small and non-significant reduction of 1 percent of a standard deviation, equivalent to 1.6 percent of the mean of the estimation sample), but it substantially increases less visible violence in affected municipalities relative to the rest of the country. On average, between January and June 2023, less visible violence differentially increased by 14 percent of a standard deviation, equivalent to 38 percent of the outcome mean. This pattern is consistent with Proposition 1: a ceasefire causes more visible violence to weakly decrease and less visible violence to strictly increase.

Figure 3. Effect of the first Paz Total ceasefire announcement on more visible and less visible violence in Colombia (Event-study)



(a) More visible violence

(b) Less visible violence

The figure plots coefficients from Table 1 estimated using the event-study specification in Equation 8, following Borusyak et al. (2024). 95% percent confidence intervals are shown. Estimates are based on municipality-month data for two outcomes, as indicated in each subfigure title. Standard errors are clustered at the municipality level.

Figure 3 reports the event-study companion to Table 1, plotting the BJS imputation coefficients from Equation 8 and their 95 % confidence intervals month-by-month. Panel (a) shows more visible violence; panel (b) shows less visible violence. The pre-treatment coefficients are close to zero and statistically indistinguishable from zero in both panels, with no visible drift in the seven months leading up to the announcement. The post-treatment path differs sharply across the two outcomes. More visible violence stays flat or decreases slightly after the ceasefire, consistent with the small, negative and imprecisely estimated point estimate in column 1 of Table 1. Instead, less visible violence increases relative to the control group during most of the post-ceasefire period. The flat pre-period is suggestive evidence that treated and control municipalities were on parallel trends before the ceasefire announcement.

Table 2. Impact of the first Paz Total ceasefire announcement on violence in Colombia: Type of violence

Panel A: More visible violence

Dep. Var: Standardized count of:	Terrorism	Homicides	Massacres	Clashes	Checkpoints
	(1)	(2)	(3)	(4)	(5)
Post × Treated	-0.077	-0.059	0.014	-0.173**	0.069
	(0.119)	(0.051)	(0.055)	(0.076)	(0.056)
Observations	14,157	14,157	14,157	14,157	14,157
Municipalities	1,089	1,089	1,089	1,089	1,089
Municipality FE	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes

Panel B: Less visible violence

Dep. Var: Standardized count of:	Extortion	Threats	Forced recruitment	Kidnapping	Lockdowns	Forced displacement
	(1)	(2)	(3)	(4)	(5)	(6)
Post × Treated	0.336*	0.238***	0.145**	0.193	-0.057	-0.118
	(0.203)	(0.078)	(0.070)	(0.129)	(0.036)	(0.124)
Observations	14,157	14,157	14,157	14,157	14,157	14,157
Municipalities	1,089	1,089	1,089	1,089	1,089	1,089
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes

Estimates are from an imputation-based difference-in-differences model (Equation 7) following Borusyak et al. (2024). Standard errors, clustered at the municipality level, are reported in parentheses. The sample is a municipality-month panel covering 1,089 municipalities over 13 months. *Treated* equals 1 for municipalities with a presence of a non-state armed group involved in the first *Paz Total* ceasefire (Segunda Marquetalia, Estado Mayor Central, or Autodefensas Gaitanistas de Colombia). *Post* equals 1 for the post-treatment period. *Panel A* reports more visible violence, including terrorism, homicides, massacres, clashes, and checkpoints. *Panel B* reports less visible violence, including extortion, threats, forced recruitment, kidnapping, lockdowns, and forced displacement. Each column reports treatment effect estimates for a different standardized outcome, as specified in the column header. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 2 examines each individual component of the more visible and less visible violence indicators. As for more visible violence, Panel A suggests clashes is the only outcome for which the effect of the ceasefire is statistically significant. For the other outcomes the effect size in standard deviation of the outcome is smaller and imprecisely estimated. The policy differentially reduced bilateral clashes with participation of the targeted groups by 53 percent relative to the sample mean. In turn, Panel B suggests that the effect of the ceasefire is positive, significant and large on three of the six indicators of less visible violence (extortion, threats and forced recruitment). The estimated effect on kidnappings is also positive and large but less precisely estimated. There is a negative but non-statistically significant effect on lockdowns and forced displacement. Appendix Figure B3 presents the event-study equivalents for each non-visible component.

Having established, in line Proposition 1, the strategic substitution that participating illegal organizations followed over the duration of the ceasefire, we now turn to examine the behavior of government forces during the same period. The model discusses the trade-off that government forces face under a ceasefire scenario: the additional political cost of engaging in enforcement against participating groups may offset the marginal benefit from confronting the increase in less visible violence (see Proposition 2). The net effect of the ceasefire on military operations is *a priori* ambiguous, but if the additional political cost of enforcing is greater than the increased marginal benefit of enforcement it will be negative.

Table 3. Impact of the first Paz Total ceasefire announcement on state military operations

Dependent variable: Number of events (standardized)	State clashes	Nonstate combatants killed	Military Ops: Illegal economies
	(1)	(2)	(3)
Post × Treated	-0.208**	-0.082*	0.014
	(0.083)	(0.047)	(0.052)
Observations	14,157	14,157	14,157
Municipalities	1,089	1,089	1,089
Municipality FE	Yes	Yes	Yes
Time FE	Yes	Yes	Yes

Estimates are from an imputation-based difference-in-differences model (Equation 7) following Borusyak et al. (2024). Standard errors, clustered at the municipality level, are reported in parentheses. The sample is a municipality-month panel covering 1,089 municipalities over 13 months. *Treated* equals 1 for municipalities with a presence of a non-state armed group involved in the first *Paz Total* ceasefire (Segunda Marquetalia, Estado Mayor Central, or Autodefensas Gaitanistas de Colombia). *Post* equals 1 for the post-treatment period. Outcomes capture different dimensions of state military operations. Each column reports treatment effect estimates for a different standardized outcome, as specified in the column header. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 3 shows that while direct military confrontations with armed groups declined sharply—as reflected in large reductions in state clashes (Column 1) and non-state combatants killed (Column 2)—operations against illegal economies remained largely unchanged. This pattern is consistent with a relaxation of military pressure on armed groups without a corresponding reduction in efforts aimed at disrupting illicit economic activities. This heterogeneity is consistent with the model, as the disruption of illegal economies faces less political pressure in the face of a ceasefire. In terms of magnitude, state clashes against participating illegal actors differentially dropped by almost 21 percent of a standard deviation (73 percent of the mean), and the killings of enemy combatants by state forced dropped by 8 percent of a standard deviation (46 percent of the sample mean). The increase in enforcement against illegal economies is less than 5 percent of the mean and not statistically significant. Appendix Figure B4 presents the corresponding event-study estimates.

Appendix Table A2 further unpacks the aggregate measure of military operations against illegal economies reported in column (3) of Table 3. Consistent with the null effect observed for the aggregate measure of military operations against illegal economies, we find no statistically significant impact of the ceasefire on any of its individual components. In particular, the estimates indicate no discernible changes in cocaine laboratory destruction, illegal mining operations, seizures of illegal drugs, seizures of chemical precursors, destruction of machinery used in illegal mining, or other enforcement activities targeting illicit economic infrastructure. Taken together, these results suggest that the ceasefire did not weaken the state's efforts to disrupt illegal economies. Rather, the evidence points to a more selective effect, concentrated on reducing direct military engagement with armed groups while leaving enforcement against illicit markets broadly unchanged.

Finally, we study the impact of the ceasefire on criminal governance, namely the capacity of non-state actors to provide order, resolve disputes, tax, and regulate the territory they control. The theoretical model finds that, because the ability to engage in these activities is a function of the amount of less visible violence and reduced military effort, the ceasefire intensifies criminal governance (see Proposition 3). Consistently, Table 4 finds that the ceasefire differentially increased an index of criminal governance (the average of the standardized measures of checkpoints, lockdowns, extortion, threats, forced recruitment of minors, and forced displacement) in treated municipalities by 32 percent relative to the estimation sample mean.

Armed groups leveraged the *Paz Total* ceasefire to strengthen their economic extraction mechanisms as well as to exert greater social control through intimidation tactics. This expansion of criminal governance highlights the unintended consequences of the ceasefire: it provided armed groups with an opportunity to consolidate territorial influence and reinforce coercive strategies while the state apparatus hesitated to intervene. Appendix Figure B5 plots the month-by-month evolution of the criminal governance effect.

Table 4. Impact of the first Paz Total ceasefire announcement on criminal governance

Dep Var.: standardized index of:	Criminal governance
	(1)
Post × Treated	0.114**
	(0.049)
Observations	14,157
Municipalities	1,089
Municipality FE	Yes
Time FE	Yes

Estimates are from an imputation-based difference-in-differences model (Equation 7) following Borusyak et al. (2024). Standard errors, clustered at the municipality level, are reported in parentheses. The sample is a municipality-month panel covering 1,089 municipalities over 13 months. *Treated* equals 1 for municipalities with a presence of a non-state armed group involved in the first *Paz Total* ceasefire (Segunda Marquetalia, Estado Mayor Central, or Autodefensas Gaitanistas de Colombia). *Post* equals 1 for the post-treatment period. The measure of criminal governance is the average of indicators including checkpoints, lockdowns, extortion, threats, forced recruitment, and forced displacement. Each column reports treatment effect estimates for a different standardized outcome, as specified in the column header. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

6.2 Robustness Checks

Inclusion of covariates. We start by assessing the robustness of the baseline specification to the inclusion of covariates in two complementary ways. First, we re-estimate the baseline specification adding municipal covariates interacted with the year fixed effects. These were selected via the double-LASSO, based on a large set of predetermined or time invariant covariates candidates (Belloni et al., 2014). The results are reported on Appendix Table A3. Second, we estimate the doubly robust difference-in-differences estimator of Sant'Anna and Zhao (2020) on the same covariates set, which remains consistent if either the outcome regression model or the propensity score model are correctly specified (see Appendix Table A4). Point estimates barely move under either approach, and the pattern of findings holds across types of violence, state military operations and criminal governance.

Alternative data sources. We also check whether the established findings pertaining the effects of the *Paz Total* ceasefires on various forms of violence are an artifact of the JEP data source. If we use data from the Electoral Observation Mission (MOE) instead, which measures comparable forms of violence but from outside the Colombian institutional establishment (unlike JEP), we find very similar patterns (see Appendix Table A5): State clashes against ceasefire illegal group participants fall by 0.131 SD (133 percent of the mean; Panel C); less visible violence increases differentially by 46 percent; and the criminal-governance index does so by 79 percent (Panel D); Extortion, forced recruitment, and kidnapping all rise as well. In addition, the estimated effect magnitudes using MOE data are similar to those obtained using JEP.

ELN classification. The government initially included the ELN in the *Paz Total* ceasefire but rescinded its participation four days later. Our main specification therefore assigns ELN municipalities to the control group. We test the sensitivity of this choice in two ways: (i) reclassifying ELN municipalities as treated (as originally intended, see Appendix Table A6), and (ii) dropping them from the sample altogether (see Appendix Table A7). Point estimates and statistical significance are essentially unchanged under both alternatives, consistent with the four-day ELN-specific ceasefire reversal having been too brief to alter the behavior of a group its size and importance.

Alternative control group. The main specification compares municipalities with FARC-dissidence groups or AGC presence with all other conflict-affected municipalities. One concern is that such a control group could be contaminated if other active illegal organizations changed their behavior following the multiple and simultaneous ceasefires. We therefore re-estimate the main specification on a "clean" sample that restricts the control group to municipalities with no documented armed-group presence (see Appendix Table A8). Point estimates and statistical significance are essentially unchanged across more visible violence, less visible violence, and state operations, so the inclusion of non-ceasefire-groups territories in the control pool is not driving our results.

6.3 A hidden electoral promise?

Lastly, we explore a heterogeneous effect that can shed light to one potential mechanism explaining our findings about the effect of the *Paz Total* ceasefires on the level and composition of conflict violence in Colombia. In particular, we explore whether the political support of Gustavo Petro in the presidential elections that resulted in his election in mid 2022 is correlated with our findings. President Petro championed *Paz Total* during his campaign, and the ceasefire was the policy that kicked off this policy agenda. If the ceasefire's effects vary with political support to the elected government that promoted *Paz Total* (for instance, because armed groups in pro-Petro municipalities anticipated more lenient enforcement or because local governments in these areas were less likely to challenge ceasefire violations) then we would expect an informative heterogeneity along this dimension.

We interact the treatment indicator with the municipal-level vote share for Gustavo Petro in the 2022 elections. We find suggestive evidence that increases in criminal governance are larger in municipalities with higher Petro vote shares, though the interaction effects are imprecisely estimated (see Appendix Table A9). This pattern is consistent with armed groups interpreting political alignment as a signal of an eventual reduced enforcement risk, but we interpret these results cautiously given the limited statistical power.

7 Structural Calibration and Policy Counterfactuals

The reduced-form estimates in Section 5 establish that ceasefires increased less visible violence and criminal governance while leaving more visible forms of violence unchanged and weakening government enforcement actions against criminal groups. The theoretical framework in Section 3 suggests that this occurs because the ceasefire-triggered reputational cost of enforcement, ϕ, and the dampening of enforcement, λ, induce a strategic substitution across different forms of violence by illegal armed groups participating in the policy. In this section, we use the model's structure to recover the magnitudes of these forces from the data and ask three policy-relevant questions that reduced-form analysis alone cannot answer.

7.1 Estimation Strategy

We recover the model's structural parameters by matching model-implied moments to reduced-form estimates via a Generalized Method of Moments (GMM) procedure. The model's equilibrium conditions (equations 3–4) map parameters to observable outcomes, and we search for the parameter vector that makes the model's predictions as close as possible to the data.

Because the model has more parameters than can be separately identified, we impose four normalizations that pin down units without loss of generality: $\alpha = 1$ (rents from more visible violence measured in units of α), $a = 0.5$ (standard concavity for more visible violence), $\delta_v = 1$ (detectability of more visible violence as the unit of measurement), and $e_0 = 1$ (pre-ceasefire enforcement normalized to one). The governance technology parameters are set to $\xi = 1$ and $\sigma = 0.5$. These normalizations leave six free parameters:

$$\theta = (\beta,\ b,\ \eta,\ \delta_v/\delta_c,\ \phi,\ \lambda).$$

In turn, the reduced-form analysis provides us with six empirical moments that we can use in the calibration. These are:

1. Estimated coefficient on more visible violence (from Table 1, col. 1): $\hat{m}_1 = -0.011$;
2. Estimated coefficient on less visible violence (from Table 1, col. 2): $\hat{m}_2 = 0.140$;
3. Estimated coefficient on criminal governance (from Table 4): $\hat{m}_3 = 0.114$;
4. Estimated coefficient on non-state combatants killed by government forces (from Table 3, col. 2): $\hat{m}_4 = -0.082$;
5. Pre-ceasefire ratio of more visible to less visible violence: $\hat{m}_5 = 1.5$;

Each estimated coefficient captures the differential change in the standardized outcome in treated municipalities relative to controls following the ceasefire announcement. To map model quantities to these units, we convert proportional changes in the model's violence levels to standardized effects using the pre-ceasefire mean in standard-deviation units.

GMM objective. The model-implied moments (see vector $m(\theta)$) are obtained by solving the armed group's first-order conditions (3)–(4) in both the pre-ceasefire ($\phi = \lambda = 0$) and post-ceasefire ($\phi > 0$, $\lambda > 0$) regimes, then computing the proportional change in each violence type. The structural parameters are estimated by minimizing:

$$\hat{\theta} = \arg\min_{\theta} \, [\hat{m} - m(\theta)]' \, W \, [\hat{m} - m(\theta)], \tag{9}$$

where W is a diagonal weighting matrix that gives more weight to more precisely estimated moments.[4] Standard errors are obtained via parametric bootstrap with 500 replications, perturbing each moment by its estimated standard error.

7.2 Parameter Estimates

Table 5 reports the structural parameter estimates. The enforcement dampening parameter $\hat{\lambda} = 0.33$ indicates that the ceasefire reduced the effective bite of government enforcement by approximately 33%. This estimate is identified primarily through moment $\hat{m}_4$, the differential 33% reduction in non-state combatants killed during the ceasefire period, and has a (bootstrap) standard error of 0.18.[5]

The reputational cost is $\hat{\phi} = 0.35$. Therefore, the ratio $\hat{\phi}/(\hat{\lambda} \cdot e_0 \cdot \delta_v) = 0.35/0.33 = 1.05$, which is essentially unity. This implies that the reputational cost and enforcement relief for more visible violence cancel almost precisely, giving emoirical support to the knife-edge condition (Corollary 1), by which the reputational cost satisfies $\phi \approx \lambda e_0 \delta_v$. In turn, this is consistent with the near-zero point estimate on more visible violence (1.1 percent of a standard deviation according to Table 1).

The detectability ratio $\hat{\delta}_v/\hat{\delta}_c = 3.47$ implies that more visible violence is substantially more detectable than less visible violence. One caveat is that this parameter is not well-identified because the bootstrap variance is effectively unbounded. Qualitatively, however, $\hat{\delta}_v/\hat{\delta}_c > 1$ in *every* bootstrap draw, so the directional conclusion, namely more visible violence is more detectable, is robust even if the exact magnitude is imprecise.

The less visible rent productivity parameter $\hat{\beta} = 0.43$ and concavity $\hat{b} = 0.84$ indicate diminishing but substantial returns to less visible violence. Finally, the effort

[4] We use multi-start optimization (50 random starting points) to guard against local minima, with Nelder-Mead followed by gradient-based refinement. All 50 runs converge to the same global minimum ($Q = 1.797$), providing confidence that the reported estimates are not artifacts of initialization.

[5] Using the reduction in state-initiated clashes with armed groups instead of the reduction in non-state combatants killed as the fourth moment yields $\hat{\lambda} \approx 0.62$ — roughly double the baseline estimate. We prefer non-state combatants killed because it directly measures state lethality against armed groups, whereas bilateral clashes reflect choices by both parties and fall mechanically under a ceasefire regardless of enforcement effort. Our baseline calibration therefore provides a lower bound on enforcement dampening.

cost parameter $\hat{\eta}$ is the least precisely identified. This is expected as it governs the *level* of violence rather than the *change*, and the DID design identifies changes more sharply than levels.

Table 5. Structural Parameter Estimates

Parameter	Estimate	Bootstrap SE	Interpretation
λ (enforcement dampening)	0.333	0.184	Ceasefire reduced enforcement by 33%
ϕ (reputational cost)	0.347	0.190	Political cost of more visible violence
δ_v/δ_c (detectability ratio)	3.47	—	More visible violence more detectable (weakly id.)
β (less visible rent productivity)	0.426	0.257	Returns to less visible violence
b (less visible concavity)	0.839	0.119	Diminishing returns curvature
η (effort cost)	2.639	13.672	Resource constraints (weakly identified)

GMM objective $Q = 1.797$. Normalizations: $\alpha = 1$, $a = 0.5$, $\delta_v = 1$, $e_0 = 1$.
Bootstrap: 500 replications, parametric. Multi-start: 50 random starting points.
95% CI for λ: [0.00, 0.69]. "—" denotes parameter not identified by current moments.

Model fit. The model matches three of the six moments exactly at the optimum: the DID on more visible violence (-0.011 data vs. -0.011 model), the DID on non-state combatants killed (-0.083 vs. -0.083), and the pre-ceasefire more visible/less visible ratio (1.50 vs. 1.50). The remaining two moments show relatively small discrepancies, especially for less visible violence, where the DID estimate is 0.140 but the model yields 0.165 (an 18% discrepancy). The gap is somewhat larger for criminal governance: the DID estimate is 0.114 but the model yields 0.052 (a 54% discrepancy). This likely reflects model's parsimonious structure (one armed group and two violence types). If anything, this is a conservative feature: the model understates how much the ceasefire amplified criminal governance. Importantly, however the directional predictions are correct for all five moments.

7.3 Policy Counterfactuals

We employ the calibrated model to illustrate three counterfactual exercises that ask how violence outcomes would have differed under alternative ceasefire designs featuring enhanced monitoring or maintained enforcement. We also explore what would an optimal ceasefire look like in our context.

7.3.1 Ceasefire with Enhanced Monitoring

Suppose the ceasefire had been accompanied by a monitoring mechanism—community reporting systems, independent verification missions (as in the 2016 FARC peace agreement), or technological surveillance—that increased the detectability of less visible violence from δ_c to δ_c^*. The critical monitoring threshold that fully offsets the ceasefire's effect on less visible violence is given by:

$$\frac{\delta_c^*}{\delta_c} = \frac{1}{1-\lambda}.$$

which follows directly from the first-order condition (4). Absent any enforcement dampening pre-ceasefire ($\lambda = 0$), the marginal cost of less visible violence is $e\delta_c$. The ceasefire reduces this to $(1-\lambda)e\delta_c$, causing less visible violence to rise. However, raising detectability to δ_c^* restores the original marginal cost whenever $(1-\lambda)\delta_c^* = \delta_c$. The larger the enforcement dampening λ, the greater the monitoring investment required.

The calibrated value of the enforcement dampening parameter $\hat{\lambda} = 1/3$ (see Table 5), implies that $\delta_c^*/\delta_c = 1.5$, so monitoring must be 50% more intense than before the ceasefire to fully neutralize the less visible violence increase while leaving the reduction in more visible violence intact. This could be achieved, for instance, by third-party verification missions with explicit mandates to monitor less visible forms of coercion at the local level.

7.3.2 Ceasefire with Maintained Enforcement

Suppose the government had committed to maintaining full enforcement ($\lambda = 0$), mechanically eliminating the channel through which less visible violence increases while keeping the reputational cost for allowing more visible violence $\phi > 0$.

Numerically solving the armed group's first-order conditions (3)–(4) at $\lambda = 0$ and the calibrated value of $\phi = \hat{\phi} = 0.35$ (and holding all other structural parameters at their calibrated values, see Table 5), predicts a 31% reduction in more visible violence relative to pre-ceasefire levels, roughly twenty times the 1.6% decline observed under the actual ceasefire. Less visible violence remains unchanged.

As a caveat, the practical challenge of such an approach is credibility. If the government maintains enforcement intensity against a negotiating partner, the armed group may interpret this as bad faith, potentially collapsing the talks. The model captures this tension through the participation constraint: a group will only accept a ceasefire if its post-ceasefire payoff exceeds a threshold. The maintained-enforcement scenario reduces the armed group's payoff significantly, which may violate the participation constraint for groups with high outside options.

7.3.3 Optimal Ceasefire Design

The optimal ceasefire minimizes total violence $T = v + c$ subject to a participation constraint requiring that the armed group retains, say, at least 80% of its pre-ceasefire utility. Using the calibrated model parameters, we can search over the policy space (ϕ, λ) to find the design that achieves this.

The exercise yields an optimal design with moderate reputational pressure ($\phi^* \approx 0.38$) and near-zero enforcement dampening ($\lambda^* \approx 0.01$). Evaluated at these parameters, the model's FOCs imply a 19% reduction in total violence relative to pre-ceasefire levels—compared to the 15% increase of the actual ceasefire design.

8 Conclusion

We provide the first causal evaluation of a simultaneous multi-group national ceasefire on disaggregated criminal violence. Colombia's 2023 *Paz Total* ceasefires involved five armed groups but did not establish clear monitoring mechanisms, generating a stark pattern of strategic violence substitution: more visible violence (homicides, massacres, clashes) was unaffected, while less visible violence (extortion, threats, forced recruitment of minors, kidnappings, lockdowns and forced displacement) and criminal governance surged. A structural model calibrated to the reduced-form estimates attributes the substitution to enforcement dampening ($\hat{\lambda} = 0.33$): armed groups exploited the relaxation of military pressure to expand the less visible instruments of territorial control.

Ceasefire announcements without credible verification alter the strategic decisions of armed groups by raising the political cost of more visible violence through reputational pressure, while simultaneously reducing the effective deterrence of less visible violence through enforcement dampening. These two channels are separable: a monitoring mechanism that raises the detectability of less visible activities by 50%—through independent verification missions or community reporting systems—would fully offset the adverse effects on less visible violence and bring total violence approximately back to its pre-ceasefire level (equation 10). Increasing monitoring is an actionable policy because it does not require the politically difficult commitment of maintaining military pressure against a negotiating partner. Future ceasefire agreements should therefore establish monitoring protocols *before* the ceasefire is declared, with mandates that cover less visible and salient forms of violence such as extortion, threats, forced recruitment, and criminal governance.

The *Paz Total* experience also reveals a structural limitation of extending peace-negotiation logic to criminally motivated organizations. Criminal groups whose primary objective is rent extraction respond to ceasefires differently than insurgencies with political grievances: reduced military pressure lowers the cost of predation without altering the underlying incentives for territorial control. Indeed, the *Paz Total* ceasefires enabled a 32% expansion of criminal governance—extortion, threats, forced recruitment of minors, forced displacement, illegal checkpoints and lockdowns—in treated municipalities. This expansion has concrete consequences for the daily lives of millions of Colombians in territories controlled by armed groups, and risks entrenching governance structures that will be difficult to dismantle even after negotiations conclude. The architecture of the ceasefire—simultaneous, undifferentiated, without monitoring—was poorly suited to organizations whose primary activity is territorial predation. Correcting it requires institutional investment: monitoring missions with mandates broad enough to detect less visible coercion, not political will alone.

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A Appendix Tables

Appendix Table A1. Summary Statistics: Outcomes in the Estimation Sample

	Full sample		Pre-period mean	
	Mean	SD	Treated	Control
Panel A: Visible conflict				
Visible conflict index	0.201	0.752	0.456	0.026
Terrorism	0.018	0.156	0.042	0.001
Homicides	0.149	0.616	0.334	0.019
Massacres	0.005	0.070	0.008	0.002
Clashes	0.021	0.179	0.058	0.002
Checkpoints	0.008	0.101	0.015	0.001
Panel B: Non-visible conflict				
Non-visible conflict index	0.106	0.489	0.178	0.015
Extortion	0.012	0.118	0.012	0.001
Threats	0.059	0.292	0.099	0.010
Forced recruitment	0.006	0.085	0.007	0.000
Kidnapping	0.008	0.098	0.014	0.002
Lockdowns	0.012	0.149	0.026	0.001
Forced displacement	0.009	0.110	0.020	0.001
Panel C: Criminal governance				
Criminal governance index	0.106	0.496	0.179	0.015
Panel D: State enforcement				
State clashes	0.012	0.124	0.035	0.002
Non-state combatants killed	0.005	0.071	0.013	0.001
Military ops: Illegal economies	0.051	0.350	0.104	0.009
Panel E: Military operations against illicit economies				
Drug lab destructions	0.009	0.111	0.019	0.001
Illegal mine destructions	0.006	0.084	0.008	0.001
Forced eradication	0.001	0.044	0.005	0.000
Chemical seizures	0.011	0.122	0.023	0.001
Mining input seizures	0.001	0.027	0.002	0.000
Drug seizures	0.023	0.173	0.048	0.006
Municipalities	1089		448	641
Observations	14,157			

Notes: The estimation sample is a balanced municipality-month panel covering 1089 municipalities over 13 months (June 2022–June 2023), totalling 14,157 observations. Full sample mean and SD are computed over all observations in the estimation sample; these are the moments used to standardize outcomes in the regression tables (each $y_{std} = (y - \bar{y})/\hat{\sigma}$). Pre-period mean columns report the average monthly events per municipality during the seven pre-ceasefire months (June–December 2022), separately for Treated municipalities—those with presence of Estado Mayor Central, Segunda Marquetalia, or Autodefensas Gaitanistas de Colombia ($N = 448$)—and Control municipalities ($N = 641$). The percentage effects reported in the regression tables recover $\hat{\beta}_{std} \times \hat{\sigma}/\bar{y}_{pre,treated} \times 100$, so the Treated pre-period mean is the relevant denominator for interpreting effect sizes in raw-count units. All outcomes are raw event counts from the JEP Monitoring Mechanism, except the composite indices (visible conflict, non-visible conflict, criminal governance, military operations against illegal economies), which are simple averages of their component variables standardized to zero mean and unit variance.

Appendix Table A2. Impact of the first Paz Total ceasefire announcement on state military operations: Type of military operation

Dep. var: standardized number of:	Destruction of cocaine labs	Destruction of illegal mines	Eradication operations	Seizure of chemicals	Seizure of mining equipment	Seizure of illicit drugs
	(1)	(2)	(3)	(4)	(5)	(6)
Post × Treated	-0.013	0.086	0.002	-0.007	0.005	0.037
	(0.049)	(0.063)	(0.020)	(0.047)	(0.006)	(0.158)
Observations	14,157	14,157	14,157	14,157	14,157	14,157
Municipalities	1,089	1,089	1,089	1,089	1,089	1,089
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes

Estimates are from an imputation-based difference-in-differences model (Equation 7) following Borusyak et al. (2024). Standard errors, clustered at the municipality level, are reported in parentheses. The sample is a municipality-month panel covering 1,089 municipalities over 13 months. *Treated* equals 1 for municipalities with a presence of a non-state armed group involved in the first *Paz Total* ceasefire (Segunda Marquetalia, Estado Mayor Central, or Autodefensas Gaitanistas de Colombia). *Post* equals 1 for the post-treatment period. Outcomes capture state military operations against illegal economies. Each column reports treatment effect estimates for a different standardized outcome, as specified in the column header. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Appendix Table A3. Impact of the Paz Total ceasefire on violence, state military operations, and criminal governance (selected controls × years fixed effects)

Dep. Var.: standardized number of:	Post × Treated
Panel A: More visible and less visible violence	
More visible violence	0.010 (0.031)
Less visible violence	0.100** (0.047)
Panel B: Less visible violence components	
Extortion	0.242 (0.171)
Threats	0.210*** (0.060)
Forced recruitment	0.086 (0.061)
Kidnapping	0.064 (0.075)
Lockdowns	-0.034 (0.030)
Forced displacement	0.003 (0.013)
Panel C: State military operations	
State clashes	-0.131** (0.051)
Nonstate combatants killed	-0.027 (0.024)
Military Ops: Illegal economies	0.030 (0.029)
Panel D: Criminal governance	
Criminal governance	0.086** (0.040)
Observations	14,157
Municipalities	1,089
Municipality FE	Yes
Time FE	Yes

Estimates are from a two-way fixed effects model with municipality and time fixed effects. Standard errors, clustered at the municipality level, are reported in parentheses. The sample is a municipality-month panel covering 1,089 municipalities over 13 months. *Treated* equals 1 for municipalities with a presence of a non-state armed group involved in the first *Paz Total* ceasefire (Segunda Marquetalia, Estado Mayor Central, or Autodefensas Gaitanistas de Colombia). *Post* equals 1 for the post-treatment period. Outcomes are standardized event counts, grouped into five thematic panels. Each row reports the treatment effect estimate for a different standardized outcome, with the clustered standard error in parentheses next to the coefficient. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Appendix Table A4. Impact of the Paz Total ceasefire on violence, state military operations, and criminal governance (Doubly robust DiD)

Dep. Var.: standardized number of:	Post × Treated
Panel A: More visible and less visible violence	
More Visible violence	0.030 (0.056)
Less visible violence	0.122* (0.072)
Panel B: Less visible violence components	
Extortion	0.508 (0.351)
Threats	0.120 (0.099)
Forced recruitment	0.210*** (0.082)
Kidnapping	0.011 (0.177)
Lockdowns	-0.024 (0.063)
Forced displacement	-0.053 (0.073)
Panel C: State military operations	
State clashes	-0.181 (0.148)
Nonstate combatants killed	-0.028 (0.026)
Military Ops: Illegal economies	0.067 (0.059)
Panel D: Criminal governance	
Criminal governance	0.133* (0.072)
Observations	14,157
Municipalities	1,089

Estimates are from a doubly robust difference-in-differences model following Sant'Anna and Zhao (2020). Standard errors, clustered at the municipality level, are reported in parentheses. The sample is a municipality-month panel covering 1,089 municipalities over 13 months. *Treated* equals 1 for municipalities with a presence of a non-state armed group involved in the first *Paz Total* ceasefire (Segunda Marquetalia, Estado Mayor Central, or Autodefensas Gaitanistas de Colombia). *Post* equals 1 for the post-treatment period. Outcomes are standardized event counts, grouped into five thematic panels. Each row reports the treatment effect estimate for a different standardized outcome, with the clustered standard error in parentheses next to the coefficient. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Appendix Table A5. Impact of the Paz Total ceasefire (robustness to using data from the Misioón de Observación Electoral–MOE)

Dep. Var.: standardized number of:	Post × Treated
Panel A: More visible and less visible violence	
More visible violence	-0.013 (0.022)
Less visible violence	0.068*** (0.018)
Panel B: Less visible violence components	
Extortion	0.191** (0.080)
Threats	-0.045 (0.031)
Forced recruitment	0.068*** (0.026)
Kidnapping	0.131** (0.056)
Lockdowns	0.038 (0.030)
Forced displacement	-0.128 (0.186)
Panel C: State military operations	
State clashes	-0.131** (0.056)
Panel D: Criminal governance	
Criminal governance	0.075** (0.035)
Observations	14,157
Municipalities	1,089

Estimates are from an imputation-based difference-in-differences model (Equation 7) following Borusyak et al. (2024), using non-attributed conflict outcomes from the Mision de Observacion Electoral (MOE) source. Standard errors, clustered at the municipality level, are reported in parentheses. The sample is a municipality-month panel covering 1,089 municipalities over 13 months. *Treated* equals 1 for municipalities with a presence of a non-state armed group involved in the first *Paz Total* ceasefire (Segunda Marquetalia, Estado Mayor Central, or Autodefensas Gaitanistas de Colombia). *Post* equals 1 for the post-treatment period. Outcomes are standardized event counts, grouped into five thematic panels. Each row reports the treatment effect estimate for a different standardized outcome, with the clustered standard error in parentheses next to the coefficient. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Appendix Table A6. Impact of the Paz Total ceasefire on violence, state military operations, and criminal governance (including ELN-presence municipalities in the treatment group)

Dep. Var.: standardized number of:	Post × Treated
Panel A: More visible and less visible violence	
More visible violence	0.011 (0.035)
Less visible violence	0.105** (0.049)
Panel B: Less visible violence components	
Extortion	0.259 (0.175)
Threats	0.155** (0.063)
Forced recruitment	0.125** (0.059)
Kidnapping	0.164 (0.111)
Lockdowns	-0.052 (0.032)
Forced displacement	-0.006 (0.037)
Panel C: State military operations	
State clashes	-0.146*** (0.051)
Nonstate combatants killed	-0.089** (0.041)
Military Ops: Illegal economies	0.003 (0.047)
Panel D: Criminal governance	
Criminal governance	0.084** (0.041)
Observations	14,157
Municipalities	1,089

Estimates are from an imputation-based difference-in-differences model (Equation 7) following Borusyak et al. (2024), using outcomes from the JEP source. Standard errors, clustered at the municipality level, are reported in parentheses. The sample is a municipality-month panel covering 1,089 municipalities over 13 months. *Treated* equals 1 for municipalities with a presence of any armed group initially included in the first *Paz Total* ceasefire announcement, including the ELN, whose participation was rescinded four days later. *Post* equals 1 for the post-treatment period. Outcomes are standardized event counts, grouped into five thematic panels. Each row reports the treatment effect estimate for a different standardized outcome, with the clustered standard error in parentheses next to the coefficient. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Appendix Table A7. Impact of the Paz Total ceasefire on violence, state military operations, and criminal governance (excluding municipalities with only ELN presence from the estimation sample)

Dep. Var.: standardized number of:	Post × Treated
Panel A: More visible and less visible violence	
More visible violence	-0.021 (0.045)
Less visible violence	0.132** (0.058)
Panel B: Less visible violence components	
Extortion	0.350 (0.213)
Threats	0.226*** (0.080)
Forced recruitment	0.131* (0.074)
Kidnapping	0.206 (0.136)
Lockdowns	-0.096* (0.051)
Forced displacement	-0.084 (0.096)
Panel C: State military operations	
State clashes	-0.223*** (0.084)
Nonstate combatants killed	-0.101** (0.051)
Military Ops: Illegal economies	0.028 (0.049)
Panel D: Criminal governance	
Criminal governance	0.104** (0.049)
Observations	13,884
Municipalities	1,068

Estimates are from an imputation-based difference-in-differences model (Equation 7) following Borusyak et al. (2024), using outcomes from the JEP source. Standard errors, clustered at the municipality level, are reported in parentheses. Relative to the main specification, the sample excludes municipalities whose only armed-group presence is the ELN, whose participation in the first *Paz Total* ceasefire announcement was rescinded four days later. The resulting sample is a municipality-month panel covering 1,068 municipalities over 13 months. *Treated* equals 1 for municipalities with a presence of Segunda Marquetalia, Estado Mayor Central, or Autodefensas Gaitanistas de Colombia. *Post* equals 1 for the post-treatment period. Outcomes are standardized event counts, grouped into five thematic panels. Each row reports the treatment effect estimate for a different standardized outcome, with the clustered standard error in parentheses next to the coefficient. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Appendix Table A8. Impact of the Paz Total ceasefire on violence, state military operations, and criminal governance (restricting the control group to municipalities with no armed-group presence)

Dep. Var.: standardized number of:	Post × Treated
Panel A: More visible and less visible violence	
More visible violence	-0.022 (0.024)
Less visible violence	0.056** (0.027)
Panel B: Less visible violence components	
Extortion	0.081 (0.075)
Threats	0.130** (0.061)
Forced recruitment	0.035 (0.034)
Kidnapping	0.139 (0.161)
Lockdowns	-0.035 (0.031)
Forced displacement	-0.014 (0.037)
Panel C: State military operations	
State clashes	-0.130** (0.059)
Nonstate combatants killed	-0.025 (0.049)
Military Ops: Illegal economies	0.002 (0.036)
Panel D: Criminal governance	
Criminal governance	0.037 (0.023)
Observations	13,637
Municipalities	1,049

Estimates are from an imputation-based difference-in-differences model (Equation 7) following Borusyak et al. (2024), using outcomes from the JEP source. Standard errors, clustered at the municipality level, are reported in parentheses. The control group is restricted to municipalities with no presence of the ELN or any other armed group, isolating the treatment effect from potential contamination via neighbouring armed-group activity. The resulting sample is a municipality-month panel covering 1,049 municipalities over 13 months. *Treated* equals 1 for municipalities with a presence of Segunda Marquetalia, Estado Mayor Central, or Autodefensas Gaitanistas de Colombia. *Post* equals 1 for the post-treatment period. Outcomes are standardized event counts, grouped into five thematic panels. Each row reports the treatment effect estimate for a different standardized outcome, with the clustered standard error in parentheses next to the coefficient. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

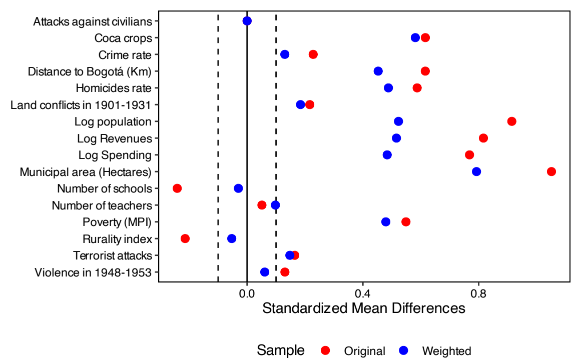
Appendix Table A9. Impact of the Paz Total ceasefire on violence, state military operations, and criminal governance – Heterogeneity by electoral alignment of the municipality 2022 presidential election

Dep. Var.: standardized number of:	*Petro won*	*Petro lost*
	Post × Treated	Post × Treated
Panel A: More visible and less visible violence		
More visible violence	-0.023 (0.084)	-0.023 (0.069)
Less visible violence	0.182** (0.088)	0.102 (0.066)
Panel C: State military operations		
State clashes	-0.408*** (0.117)	-0.090 (0.099)
Nonstate combatants killed	-0.180* (0.097)	-0.033 (0.096)
Military Ops: Illegal economies	0.042 (0.051)	-0.009 (0.100)
Panel D: Criminal governance		
Criminal governance	0.170** (0.086)	0.068 (0.044)
Observations	4,940	9,217
Municipalities	380	709

Estimates are from an imputation-based difference-in-differences model (Equation 7) following Borusyak et al. (2024), using outcomes from the JEP source. Standard errors, clustered at the municipality level, are reported in parentheses. The two sets of columns split the main estimation sample by the result of the 2022 presidential election: *Petro won* restricts to municipalities where Gustavo Petro received a plurality of the vote (380 municipalities, 4,940 observations); *Petro lost* restricts to municipalities where he did not (709 municipalities, 9,217 observations). *Treated* equals 1 for municipalities with a presence of Segunda Marquetalia, Estado Mayor Central, or Autodefensas Gaitanistas de Colombia. *Post* equals 1 for the post-treatment period. Outcomes are standardized event counts, grouped into five thematic panels. Each row reports the treatment effect estimate for a different standardized outcome, with the clustered standard error in parentheses next to the coefficient. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

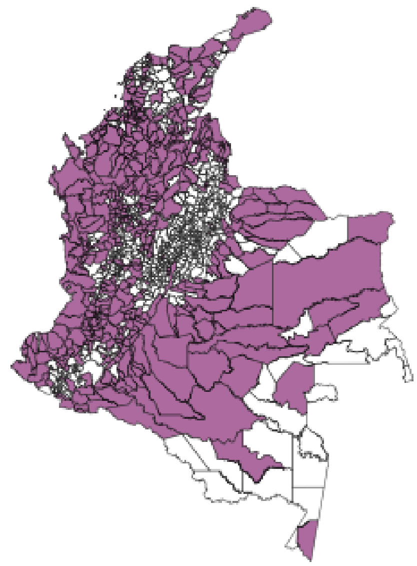
B Appendix Figures

Appendix Figure B1. Covariate balance



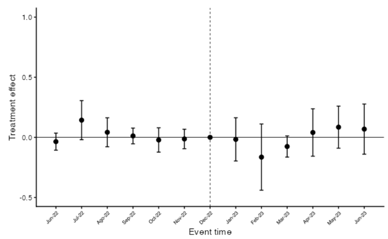
This figure presents a covariate balance assessment for a set of municipal characteristics measured in 2018. It compares standardized mean differences between treatment and control municipalities.

Appendix Figure B2. Colombian municipalities affected by the first Paz Total ceasefire

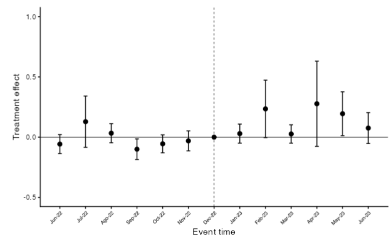


This figure illustrates the spatial distribution of the groups participating in the Paz Total ceasefire, displaying the union of municipalities where Segunda Marquetalia, Estado Mayor Central, and Autodefensas Gaitanistas de Colombia maintained a presence between 2019-2021.

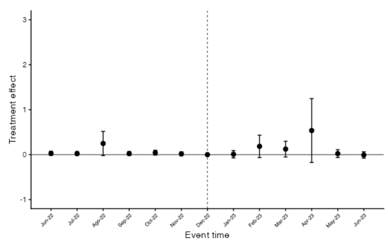
Appendix Figure B3. Effect of the first Paz Total ceasefire announcement on less-visible violence components (Event-study)



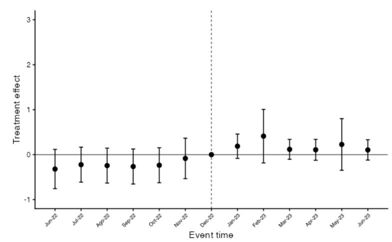
(a) Extortion



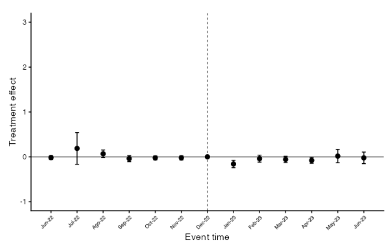
(b) Threats



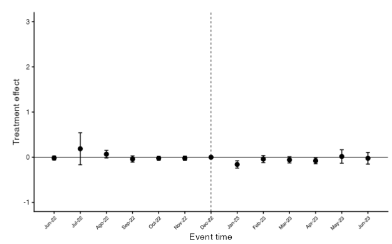
(c) Forced recruitment



(d) Kidnappings



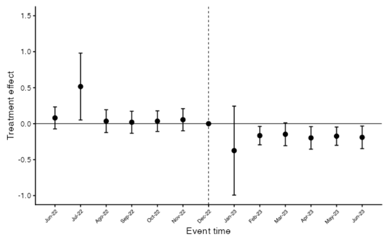
(e) Lockdowns



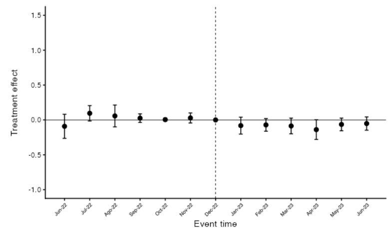
(f) Forced displacement

The figure plots coefficients from Table 2, Panel B, estimated using the event-study specification in Equation 7, following Borusyak et al. (2024). 95% percent confidence intervals are shown. Estimates are based on municipality-month data for the five non-visible conflict outcomes (extortion, threats, forced recruitment, kidnapping, and lockdowns), as indicated in each subfigure title. Standard errors are clustered at the municipality level.

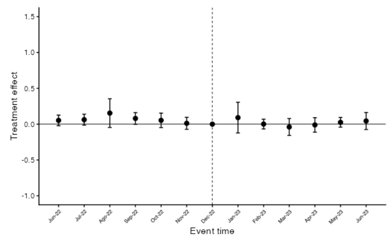
Appendix Figure B4. Effect of the first Paz Total ceasefire announcement on state military operations (Event-study)



(a) State clashes



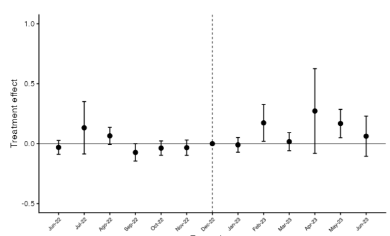
(b) Non-state combatants killed



(c) Military Ops: Illicit economies

The figure plots coefficients from Table 3, estimated using the event-study specification in Equation 7, following Borusyak et al. (2024). 95% percent confidence intervals are shown. Estimates are based on municipality-month data for three state military operation outcomes (state clashes, non-state combatants killed, and military operations against illicit economies), as indicated in each subfigure title. Standard errors are clustered at the municipality level.

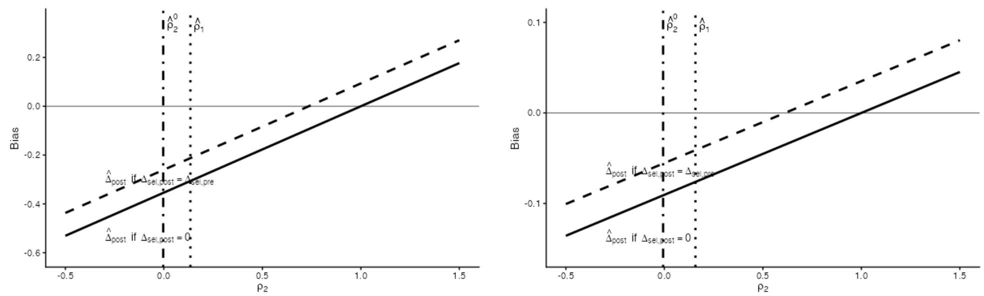
Appendix Figure B5. Effect of the first Paz Total ceasefire announcement on criminal governance (Event-study)



(a) Criminal governance

The figure plots coefficients from Table 4, estimated using the event-study specification in Equation 7, following Borusyak et al. (2024). 95% percent confidence intervals are shown. Estimates are based on municipality-month data for an outcome capturing dimensions of criminal governance. The main criminal governance index is the average of standardized indicators for checkpoints, lockdowns, extortion, threats, forced recruitment, and forced displacement; the spillover measure is the population-weighted average of this index in neighboring municipalities. Standard errors are clustered at the municipality level.

Appendix Figure B6. Selection-based bias decomposition: state clashes and criminal governance



(a) State clashes (b) Criminal governance

Each panel plots the bias of the difference-in-differences estimator under imperfect foresight, decomposed following Ghanem et al. (2026) into the sum of $\Delta_{\text{sel,post}}$ (selection on post-treatment unobservables) and $(\rho_2 - 1) \cdot \text{pre\_diff}$ (deviation from the martingale property), where ρ_2 is the unobserved AR(1) persistence of the demeaned untreated potential outcome from $t = 1$ to $t = 2$. The solid line shows the bias under $\Delta_{\text{sel,post}} = 0$ (no selection on post-treatment unobservables); the dashed line shows the bias under $\Delta_{\text{sel,post}} = \hat{\Delta}_{\text{sel,pre}}$ (post-treatment selection equal in sign and magnitude to its pre-treatment counterpart). The dotted vertical line marks $\hat{\rho}_1$, the pre-treatment AR(1) persistence under linear-martingale relaxation with time homogeneity; the dot-dash vertical line marks $\hat{\rho}_2^{;0}$, the control-group AR(1) persistence under unconfoundedness. Panel (a) uses the standardized state-clash index. Panel (b) uses the standardized criminal-governance index.

C Classification of Violent Events

Appendix Table B1. Classification of conflict-related variables: visibility and criminal governance content

Variable	**Category**	**Justification**
Homicides	More visible violence	Lethal, recorded by police and forensic systems; canonical "more visible" violence indicator.
Massacres	More visible violence	Public, lethal, and politically salient.
Terrorism	More visible violence	Defined administratively by tactic (bombings, attacks on infrastructure); high media salience by design.
Clashes	More visible violence	Combat events with security forces or between groups; more visible particularly when government forces are involved.
Illegal checkpoints	More visible (governance act)	Overtly public displays of territorial authority.
Kidnappings	Less visible violence	Heavily underreported: families are threatened with the victim's death if they contact authorities or press, the perpetrator is typically unknown to the household, and media coverage concentrates on a small number of high-profile cases unrepresentative of the true distribution.
Lockdowns	Less visible (governance act)	Captive populations held in place by threats, landmines, and combat in remote areas.
Extortion	Less visible (governance act)	Heavily underreported due to repeated victim–perpetrator interaction and fear; textbook governance/taxation mechanism.
Threats	Less visible (governance act)	Coercive enforcement mechanism that produces no body; underrecorded administratively.
Recruitment of minors	Less visible (governance act)	Occurs inside households and communities under group control; suppressed reporting due to fear and complicity.
Forced displacement	Less visible at act level (governance act); visible in administrative aggregates	Coercive act often hidden. Best understood as a consequence of territorial-consolidation strategies.

Notes: "More visible" refers to events likely to be recorded in mainstream administrative or media sources; "less visible" refers to events systematically underreported due to victim fear, perpetrator-victim interdependence, or remoteness from state information infrastructure. "Governance act" indicates that the variable maps onto one of the core governance dimensions identified by Arjona and Feldmann (2026): rule-making, dispute resolution, taxation/extraction, and service provision. The recommended core coercive criminal governance index combines extortion, threats, illegal checkpoints, and confinamientos.

D Model Proofs and Extensions

This appendix contains the formal proofs for the propositions in Section 3 and presents additional testable predictions from the model.

D.1 Proof of Proposition 1 (Violence Substitution)

Proof. (i) From the first-order condition (3), the marginal cost of v increases by $\phi - \lambda e \delta_v$. For ϕ sufficiently large relative to $\lambda e \delta_v$, the right-hand side shifts up. Since the left-hand side $\alpha a v^{a-1}$ is decreasing in v, equilibrium v falls. When $\phi \approx \lambda e \delta_v$, the two effects approximately cancel and $v_1 \approx v_0$.

(ii) From (4), the marginal cost of c decreases by $\lambda e \delta_c > 0$ with no offsetting reputational cost. Since $\beta b c^{b-1}$ is decreasing in c, a lower marginal cost implies higher equilibrium c.

(iii) Define total violence $T = v + c$. The change is $\Delta T = (c_1 - c_0) - (v_0 - v_1)$. Implicit differentiation of (4) gives:

$$\frac{dc}{d(\lambda e \delta_c)} = \frac{1}{\beta b(1-b)c^{b-2} + \eta} > 0.$$

Because less visible violence has higher rent productivity ($b > a$) and lower detection ($\delta_c < \delta_v$), the quantity response in c dominates the response in v for the parameter range consistent with the data. □

D.2 Proof of Proposition 2 (Government Enforcement)

Proof. The government's first-order condition for enforcement is $\gamma_v v'(e) + \gamma_c c'(e) + \kappa e + \mu = 0$. Under the ceasefire, two effects operate: (i) the additional political cost $\mu > 0$ raises the marginal cost of enforcement; (ii) the increase in less visible violence raises the marginal benefit of enforcement. When μ is large enough to offset the increased marginal benefit from deterring less visible violence—which is discounted by the low weight γ_c—the government's optimal enforcement is approximately unchanged. □

D.3 Additional Testable Predictions

Proposition 5 (Population heterogeneity)**.** *Let $\delta_c(N)$ be increasing in population N and $\mu(N)$ be increasing in N. Then: (i) the ceasefire-induced increase in criminal governance is smaller in more populous municipalities; (ii) the ceasefire-induced decline in government enforcement is larger in more populous municipalities.*

Proof. (i) The increase in less visible violence (and hence governance) from the ceasefire is governed by:

$$\Delta c \propto \frac{\lambda\, e\, \delta_c(N)}{\beta b(1-b)c_0^{b-2} + \eta}.$$

In high-N municipalities, baseline $\delta_c(N)$ is high, so the pre-ceasefire marginal cost of less visible violence is high and baseline c_0 is low. The key is that the *effective* post-ceasefire detection is $(1-\lambda)\delta_c(N)$. For N sufficiently large, $(1-\lambda)\delta_c(N)$ may exceed $\delta_c(\bar{N})$ for low-population municipalities $\bar{N}$, meaning that the marginal cost of less visible violence *after* the ceasefire in a populous municipality remains higher than the marginal cost *before* the ceasefire in a small municipality. In such cases, the armed group finds little room to expand governance.

More formally, the governance response depends on the *proportional* change in the marginal cost of c, which is $\lambda e \delta_c(N)/[e\delta_c(N) + \eta c_0]$. While this proportional change does not depend on δ_c when $\eta = 0$ (pure enforcement model), with convex effort costs ($\eta > 0$), municipalities with high δ_c have low baseline c_0 and therefore a larger share of marginal cost coming from the effort term ηc_0, which is *unaffected* by the ceasefire. This dilutes the proportional enforcement relief, reducing the equilibrium governance response.

(ii) The government's enforcement FOC is $\gamma_v v'(e) + \gamma_c c'(e) + \kappa e + \mu(N) = 0$. Higher $\mu(N)$ shifts the marginal cost of enforcement up, reducing optimal e. In high-N municipalities where enforcement decisions are politically salient—attracting media coverage, opposition scrutiny, and international attention—the government optimally reduces enforcement more aggressively to preserve the political viability of negotiations. □

Empirical implication: Population-weighted regressions (which give more weight to populous municipalities) should show a smaller governance effect and a larger enforcement decline relative to unweighted regressions. This is exactly what we observe in Section 6.3.

Proposition 6 (Capacity heterogeneity)**.** *The increase in less visible violence is larger in municipalities where the armed group has greater organizational capacity (higher β, lower η).*

Proof. Implicit differentiation of (4) with respect to λ:

$$\frac{dc}{d\lambda} = \frac{e\,\delta_c}{\beta b(1-b)c^{b-2} + \eta}.$$

A higher β implies a higher baseline c_0, which makes the denominator smaller through the c^{b-2} term (since $b < 1$, this is decreasing in c), amplifying the response. A lower η directly reduces the denominator. □

Empirical implication: Municipalities with longer histories of armed-group presence, established extortion networks, or coca cultivation should exhibit larger increases in less visible violence after the ceasefire.

Proposition 7 (State presence heterogeneity)**.** *The increase in less visible violence is larger in municipalities with lower baseline state presence (lower e_0).*

Proof. In municipalities with low e_0, the baseline marginal cost of less visible violence is low, so the armed group operates at higher c_0. When the ceasefire reduces effective enforcement further to $(1-\lambda)e_0$, the armed group has more room to expand less visible operations before hitting the steep part of the effort-cost curve. □

Empirical implication: The treatment effect on less visible violence should be stronger in municipalities farther from departmental capitals, with fewer police stations per capita, or with lower fiscal capacity.

Proposition 8 (Monitoring and detection)**.** *The ratio of less visible to more visible violence c/v is increasing in the detectability gap $\delta_v - \delta_c$. Municipalities with greater media presence or human-rights monitoring (higher δ_v) exhibit more substitution toward less visible violence.*

Proof. From the ratio of first-order conditions (3) and (4):

$$\frac{\alpha a v^{a-1}}{\beta b c^{b-1}} = \frac{(1-\lambda)e\delta_v + \phi + \eta v}{(1-\lambda)e\delta_c + \eta c}.$$

An increase in δ_v (holding δ_c fixed) raises the right-hand side, requiring a higher left-hand side in equilibrium, which occurs when v falls relative to c. □

Proposition 9 (Escalation over time)**.** *If the armed group learns about its environment (i.e., the true detection probability δ_c is revealed over time), less visible violence increases over the duration of the ceasefire as the group discovers enforcement is indeed relaxed.*

Empirical implication: Event-study coefficients for less visible violence should exhibit a pattern of gradual increase rather than an immediate level shift.

Proposition 10 (Inter-group dynamics)**.** *In municipalities where multiple armed groups operate, the increase in less visible violence is amplified if all groups receive ceasefires simultaneously, because the relaxation of enforcement benefits all groups and competition for rents from the civilian population intensifies.*

Proof sketch. Extend the model to two groups competing for rents from a civilian population of fixed size. Each group's less visible violence imposes negative externalities on the other (rent contestation). With symmetric ceasefire shocks, both groups expand c, leading to a Nash equilibrium with higher total less visible violence than the single-group case. $\square$

Empirical implication: The treatment effect should be stronger in municipalities with overlapping armed-group presence.