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Peace, Bullets, and Asymmetric Dividends: The Heterogeneous Effects of the 2016 FARC Peace Agreement on Municipal Homicides in Colombia

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Abstract

Does a peace agreement reduce lethal violence evenly across a country, or only where the war was worst? This article estimates the heterogeneous effect of Colombia's 2016 peace agreement with the FARC insurgency on municipal homicide rates, using a continuous difference-in-differences design across all 1,079 Colombian municipalities over the years 2013 to 2023. The treatment is a municipal pre-violence index: each municipality's average homicide rate in the three years before the agreement, divided by the national maximum, which serves as a proxy for exposure to the conflict. The index is validated as the first stage of an instrumental-variables design, in which exposure to documented guerrilla conflict events, population, and region jointly predict it. The two-way fixed-effects estimate implies that the agreement reduced homicides sharply and in proportion to prior violence: the implied reduction rises from about 6 percent at the twenty-fifth percentile of the index to about 44 percent at the ninety-fifth. An instrumental-variables specification that isolates the component of exposure predicted by prior guerrilla activity yields a smaller but still negative and significant effect, consistent with attenuation from measurement error. Event-study estimates confirm parallel pre-agreement trends and a sharp break in the year of the agreement that persists through 2023, and a permutation test that randomly reallocates the index across municipalities rejects the estimate as a product of chance. The peace dividend in homicides was therefore real but steeply graded: concentrated in the municipalities that had suffered most, and close to negligible elsewhere. Post-conflict security policy, the findings imply, should be calibrated to the prior geography of the war rather than distributed uniformly.

1. Introduction

On November 24, 2016, after four years of negotiations in Havana, the Colombian government and the Fuerzas Armadas Revolucionarias de Colombia (FARC-EP) signed a peace agreement ending more than five decades of armed conflict. The insurgency had claimed an estimated 220,000 lives, displaced nearly eight million people, and produced an exceptionally well-documented spatial distribution of violence across Colombia's roughly 1,100 municipalities. The agreement offers a rare chance to study how the territorial structure of organized violence responds to a large, nationally uniform shock applied to a conflict with highly localized geography.

The Colombian conflict is a demanding test case precisely because its violence was never uniform. Over five decades the war concentrated in a shifting archipelago of municipalities shaped by coca cultivation, resource rents, contested land, and the reach — or absence — of the state, while much of the country experienced it only indirectly. A national peace agreement is, in this setting, a single legal and institutional event imposed on a profoundly uneven map of violence. Whether such an event compresses that map evenly or widens the gaps between its worst-affected and least-affected places is at once a theoretical question about how demobilization works and a practical one about where a government should concentrate scarce post-conflict resources. Country-level evaluations, which treat the agreement as a national on-off switch, cannot answer it; the answer lives in the cross-section.

Colombia's recent security trajectory makes the question sharp. Between 2002 and 2017 the national homicide rate fell from about 70 to 22 per 100,000, one of the steepest sustained declines in Latin America; by 2017 the country recorded its lowest annual homicide total since 1984. Yet the national average concealed extreme sub-national heterogeneity: the Pacific corridor, the Catatumbo, and the eastern foothills stayed violent even as major Andean cities improved. Which municipalities benefited most from the agreement — and by how much — is both a first-order policy question and a test of claims about how armed-group demobilization works.

I study that heterogeneity with a continuous difference-in-differences design built on a municipal pre-violence index: each municipality's average 2013–2015 homicide rate divided by the national maximum. Two points are essential. First, the index measures pre-agreement violence intensity, not FARC presence directly; the link between the two is an assumption grounded in the documented geography of the conflict, and the index also captures ELN, paramilitary-successor, and common-crime violence. Second, I make that link explicit and testable by treating the index as the first stage of an instrumental-variables design — the index is predicted by FARC conflict-event exposure (UCDP GED), log population, and region — and then estimating an IV-DiD 2SLS specification alongside the TWFE model. Figure 1 shows the index distribution; Fig. 2 reports the validation and first stage.

The panel covers 2013–2023, matching the window over which the index is built and over which the event study confirms parallel pre-trends. The 2013 and 2014 pre-period coefficients are statistically indistinguishable from zero (both $p > 0.48$), and a 500-permutation placebo test rejects the null at $p < 0.001$, so the result is not an artifact of arbitrary pre-existing spatial variation in violence.

The main findings are: (1) municipalities with higher pre-violence index values saw significantly larger homicide reductions after 2016, with a TWFE estimate of $\beta = -1.648$ (SE = 0.192); (2) the effect is steeply graded, from a 6% implied reduction at the 25th percentile of the index to 44% at the 95th; (3) an IV-DiD 2SLS specification that instruments the index with FARC-exposure, population, and region predictors interacted with the post-2016 indicator corroborates the TWFE estimate; and (4) all eight post-agreement event-study coefficients are negative and significant, with no clear attenuation through 2023 — a pattern to read cautiously given the documented territorial reorganization of Colombian organized crime.

This article makes three contributions. Empirically, it provides a systematic municipality-level estimate of how the homicide dividend of the 2016 agreement was distributed across the full set of Colombian municipalities, using total administrative homicides rather than group-attributed conflict events and a continuous rather than binary measure of exposure. Methodologically, it disciplines a pre-violence index as the first stage of an instrumental-variables design, turning a familiar but criticized proxy into an object whose relationship to documented guerrilla activity is explicit and testable, and it subjects the estimate to an event study and a permutation test rather than resting on a single difference. Substantively, it documents that the dividend was steeply graded and situates that gradient within the wider debate on whether peace agreements pacify or merely reorganize violence, connecting the econometric result to the literatures on rebel governance, criminal recomposition, and state capacity.

The paper proceeds as follows. Section 2 reviews the literature. Section 3 describes the data, the index, and its first-stage validation. Section 4 sets out the empirical strategy, including the IV-DiD. Section 5 reports results. Section 6 discusses mechanisms and limitations. Section 7 concludes.

2. Literature Review

2.1 Peace agreements and post-conflict violence

The literature on civil-war termination and peace durability is extensive (Fortna 2008; Walter 2002; Mattes & Savun 2009; Hartzell & Hoddie 2007), and the cross-national tradition linking income, state strength, and civil-war onset (Fearon & Laitin 2003; Blattman & Miguel 2010) frames the question. Both operate mostly at the country-year level and measure peace as the absence of renewed battle deaths, leaving the sub-national redistribution of violence after an agreement under-examined.

Work on the 2016 agreement has begun to close that gap, and this paper is closest to a cluster of recent quasi-experimental studies. Fajardo-Steinhäuser (2024) is the nearest comparison: a difference-in-differences contrast of historically FARC municipalities against ELN-presence ones around the 2014 ceasefire finds violence fell sizeably in FARC areas, yet with no economic dividend, which he attributes to weak pre-existing state capacity and the absence of state entry. Where he identifies the decline through a binary FARC-versus-ELN contrast, I recover a comparable territorial dividend through a continuous pre-violence index, and his economic nulls reinforce that the homicide reductions here are not, by themselves, evidence of broader recovery.

A second strand documents the unintended consequences of partial peace. Prem, Vargas, and Mejía (2021) show that as FARC-attributed violence fell, dissident factions and the ELN moved into vacated territory — substitution of armed actors, not elimination of violence. Prem, Rivera, Romero, and Vargas (2022) sharpen the mechanism: the ceasefire triggered a surge in killings of community leaders in former FARC strongholds by groups excluded from the process, conditioned by state capacity and the local value of territory. Related work shows the transition reshaped these areas on other margins too — deforestation (Prem, Saavedra & Vargas 2020) and forced displacement (Albarrán, Robles & Sanz-de-Galdeano 2025). Camacho and Mejía (2019) find the dividend was larger where the state could fill the FARC's governance functions, and Blattman, Duncan, Lessing, and Tobón (2022) give micro evidence on when criminal groups restrain violence for commercial reasons.

Taken together, these strands leave a specific gap. The comparative literature establishes that enforcement and institutional design shape whether peace endures nationally, but it is largely silent on the internal geography of the dividend; the Colombian quasi-experimental literature establishes that violence fell in former FARC areas, but reads that fall either through a binary treatment or through a single conflict indicator. What is missing is an estimate that is at once continuous — letting the effect vary smoothly with the intensity of prior exposure — and anchored to total lethal violence as

municipalities actually experience it. That is the estimate developed here, and the gradient it recovers is the quantity the comparative and case-study literatures each imply but neither measures directly.

2.2 Subnational conflict and illegal economies in Colombia

Colombia's sub-national data infrastructure has produced a rich political economy of conflict. Acemoglu, Robinson, and Santos (2013) document persistent institutional effects of paramilitary presence; Fergusson, Vargas, and Vela (2014) show how FARC presence shaped local elections; Restrepo, Spagat, and Vargas (2006) establish that conflict deaths concentrated along coca corridors, extraction zones, and strategic roads — the geography the pre-violence index traces, with its highest values in the Pacific corridor, Bajo Cauca and northeastern Antioquia, the Catatumbo, and the Meta–Caquetá piedmont. Mejía and Restrepo (2016) find the Democratic Security Policy's effects concentrated where the FARC had been strongest.

A parallel literature ties this geography to illegal economies, which is why the index is a composite rather than a clean FARC measure. Dube and Vargas (2013) show commodity price shocks moved conflict in opposite directions by local endowment; Angrist and Kugler (2008) find coca raised violence and armed-group rents; Mejía and Restrepo (2013) use a land-suitability instrument to identify the causal effect of cocaine markets on municipal violence; Castillo, Mejía, and Restrepo (2020) show supply scarcity raises violence where markets are contested. These motivate both the first-stage validation (Section 3.3) and the limitations (Section 6).

The political economy of these rents also shapes who inherits a territory when an armed group leaves. Because coca, mining, and smuggling corridors generate durable, immobile rents, the value of controlling a given municipality does not fall when the FARC demobilizes; it simply becomes contestable. The same literature that ties the geography of violence to these rents therefore predicts that the places with the most to fight over are both where the FARC's departure removed the most violence and where competition to replace it should be fiercest. This tension — a large immediate dividend in high-rent, high-exposure municipalities, set against the strongest incentives for recomposition in those same places — is central to interpreting why the estimated effect is both large and, in some years, volatile.

2.3 Rebel governance, territorial control, and criminal recomposition

Why the dividend is steeply graded needs the rebel-governance literature, which rejects the view of war zones as anarchic. Arjona (2016) shows armed groups — the FARC included — built local orders, adjudicating disputes and regulating civilian life where control was sustained; her distinction between rebelocracy and thinner order implies the FARC's departure removed both a perpetrator and an ordering institution, so the consequences of demobilization vary with the depth of prior governance. The broader program (Arjona, Kasfir & Mampilly 2015; Mampilly 2011) generalizes this, Weinstein (2007) explains why group organization and resources produce different victimization patterns, and Kalyvas (2006) supplies the foundational link between territorial control and selective violence.

The flip side is collapse. Daly (2016) argues demobilization transforms rather than dissolves armed organizations: networks, territorial knowledge, and recruitment persist and can be redeployed into criminal economies along the prior geography. Nussio (2018) qualifies the strong version, showing rank-and-file ex-combatants are not destined for renewed violence even as some commanders reconstitute criminal groups. Lessing (2018) and Durán-Martínez (2018) add the criminal-violence counterpart: post-demobilization violence depends less on the mere presence of illegal markets than on inter-group competition, visibility, and the conditionality of state repression. Together they predict that stable, non-attenuating post-2016 coefficients (Section 5.3) are consistent with partial transformation, not a fully consolidated peace.

2.4 State capacity and local order in post-conflict settings

A recurring theme, and a central implication here, is that a dividend's durability depends on what fills the space a demobilizing group leaves. Kaplan (2017) shows organized communities limited wartime violence through their own institutions; Steele (2017) links armed-actor competition to selective violence and displacement. A complementary Colombian literature stresses the uneven reach of the state itself: García Villegas and Espinosa (2013) describe an institutional apartheid across the territory, González, Bolívar, and Vásquez (2003) trace the fragmented construction of the state, and Gutiérrez Sanín (2007) ties this to weak local institutions. Read with Fajardo-Steinhäuser (2024) and Camacho and Mejía (2019), this suggests the concentrated decline documented here is most likely to endure where state entry accompanied FARC withdrawal, and to reverse where it did not.

The Colombian state's uneven territorial reach is not incidental to this story but constitutive of it. Where the state was already present, demobilization handed governance back to an existing authority; where it was absent — which, by construction, overlaps heavily with where the FARC had governed — demobilization left a vacancy that some actor would fill. This is why the same agreement can produce a durable dividend in one municipality and a transfer of control to a successor in another, and why evaluations that average over municipalities of very different state capacity recover a muddied national effect. The continuous design used here does not measure state capacity directly, but by letting the effect vary with prior exposure it isolates the municipalities where the stakes of that variation are highest, and the discussion returns to what the pattern implies for where state-building should concentrate.

2.5 Methodological literature on continuous-treatment DiD

The design draws on a fast-moving econometrics literature. Callaway, Goodman-Bacon, and Sant'Anna (2021) establish identification for continuous-treatment DiD under a continuous parallel-trends condition; de Chaisemartin and D'Haultfœuille (2022) survey how two-way fixed-effects estimators can be biased under heterogeneous effects; Goodman-Bacon (2021) decomposes the TWFE estimator under staggered timing; and Roth, Sant'Anna, Bilinski, and Poe (2023) synthesize the recent literature into practical recommendations. Although this design applies a single aggregate shock (the 2016 agreement) rather than staggered adoption — sidestepping the worst forbidden-comparison problems —

these concerns motivate the within-transform implementation, the event study, the permutation placebo, and the four robustness checks of Sections 5.3–5.6.

3. Data and the Pre-Violence Index

3.1 Homicide and population data

The outcome comes from SIEDCO, the incident-level homicide registry of the Dirección de Investigación Criminal e INTERPOL (DIJIN) of Colombia's National Police. I aggregate incidents to the municipality-year level for 2013–2023, giving a balanced panel of 11,869 municipality-year observations across 1,079 municipalities; municipality-years present in the DANE registry but absent from SIEDCO are assigned zero. Municipal homicide rates per 100,000 use DANE population projections — the 2005-census series (2013–2017) and the 2018-census series (2018–2023). The outcome is ln(homicide rate + 1), winsorized at the 99th percentile of the pooled distribution (149.1 per 100,000).

Two features of SIEDCO bear on interpretation. First, it records homicides as reported and investigated by the police, so its spatial coverage tracks the presence of the police themselves; in the most remote and contested municipalities — the very places with the highest pre-violence index — undercounting is most likely, and if police coverage improved after 2016 this would bias the estimated decline away from zero. I return to this in the limitations. Second, SIEDCO aggregates all intentional homicides and does not identify the perpetrating actor, which is deliberate: the object of interest is the total lethal violence a community experiences, not only the fraction attributable to a named armed group. That is the margin on which residents, local officials, and the state actually operate, and the margin on which a peace dividend, if real, must ultimately register.

The population denominators come from two DANE projection series built on successive censuses, spliced at 2017–2018. Because the series rest on different base populations, the splice introduces small discontinuities in a handful of municipalities; these are common to all municipalities in a given year and are absorbed by the year fixed effects, and the within-municipality identification does not rely on cross-municipal level comparisons of the rate. The panel runs from 2013 to 2023: it begins in 2013 because the pre-violence index is built over 2013–2015, and it ends in 2023 so that the post-agreement window is long enough to separate a durable effect from a transient one while remaining within a period of stable municipal boundaries and reporting practices.

3.2 The pre-violence index

The municipal pre-violence index is

$$\text{Index}_i = \text{mean}(\tau_i, 2013–2015) \,/\, \max[\text{mean}(\tau, 2013–2015)],$$

where τ_i is the homicide rate per 100,000. The index is bounded in [0, 1] and equals 1 for the municipality with the highest average pre-agreement rate, Policarpa (Nariño, 217.3 per 100,000), in the Patía river

corridor. It measures pre-agreement violence intensity and serves as a proxy for conflict exposure; it is not a census of FARC presence. Figure 1 plots its distribution (histogram and empirical CDF): the index is strongly right-skewed, so most municipalities sit near zero and a small tail carries the mass of the pre-agreement violence.

Three choices in the index deserve comment. Normalizing by the national maximum rather than standardizing to mean zero keeps the treatment on an interpretable [0, 1] scale, so that β recovers the effect at the empirical extreme of exposure and the implied effects at lower percentiles follow by simple scaling. Averaging over three pre-agreement years rather than a single year smooths the idiosyncratic annual spikes to which small-population municipalities are prone, reducing attenuation from classical measurement error in the treatment. And using the homicide rate rather than a count makes the index comparable across municipalities of very different size, so that a small, intensely violent municipality and a large city with the same rate receive the same exposure score. The cost of these choices is that the index inherits whatever non-FARC violence was present in 2013–2015; the first-stage validation in Section 3.3 is the device that bounds how much of the index that non-FARC violence can explain.

3.3 Validation and first stage

Because the index is a proxy, I validate it against an independent measure of FARC activity — FARC-attributed conflict events from the UCDP Georeferenced Event Dataset (GED v24.1) — and use that validation as the first stage of the IV-DiD design. The first stage regresses the index on log FARC-event exposure, mean log population, and region:

$$\text{Index}_i = \alpha + \delta_1 \cdot \ln(\text{FARC_events}_i + 1) + \delta_2 \cdot \ln(\text{pop}_i) + \delta_3 \cdot \text{Region}_i + v_i.$$

The first stage has an R^2 of 0.170, a joint F of 37, and a partial F on ln(FARC events + 1) of 32; its coefficient on ln(FARC events + 1) is + 0.017 (SE 0.003, $p < 0.001$), so municipalities with more documented FARC activity have systematically higher pre-violence indices. Three descriptive patterns corroborate the proxy. First, the share of municipalities with any FARC event rises monotonically across index terciles — 41.9%, 59.3%, and 71.7% from low to high — and mean event counts rise with it (2.3, 5.3, 9.1; Kruskal–Wallis H = 101.9, $p < 0.001$). Second, the correlation between the index and FARC event counts is $r = 0.220$ ($p < 0.001$). Third, 622 of the 1,079 municipalities (57.6%) record at least one FARC-attributed event through 2015. UCDP coverage is imperfect — of 5,501 FARC-attributed events, 749 of the 751 that lack a municipal code are recovered by nearest-centroid reverse-geocoding — so the first-stage R^2 is best read as a lower bound on the index–FARC relationship. Figure 2 plots the validation; the IV-DiD estimate built on this first stage appears in Section 5.1.

The first stage also clarifies what the instrument can and cannot do. Its explanatory power is real but bounded: documented guerrilla activity, population, and region jointly account for a meaningful share of the variation in the index, yet most of that variation remains idiosyncratic, reflecting both genuine non-FARC violence and the incompleteness of event-based conflict data for remote Colombian municipalities. The reverse-geocoding step, which recovers events lacking an administrative code by

assigning them to the nearest municipal centroid, materially improves coverage but introduces its own measurement error near municipal boundaries. These features argue for reading the first stage as evidence that the index is not merely generic violence — it tracks documented conflict — rather than as a claim that the index is a clean measure of FARC presence, and they are why the instrumented and reduced-form estimates are reported side by side rather than one being privileged.

3.4 Descriptive statistics by index decile

Table 1 summarizes municipal characteristics by decile of the index. Three patterns stand out: the index is strongly right-skewed (the top decile averages about 79.5 homicides per 100,000 versus near zero in the bottom decile); the share of municipalities with FARC presence rises monotonically from bottom to top decile; and the regional composition shifts from predominantly Andina in the lower deciles to an increasing Pacífico share at the top, consistent with the FARC's concentration in Pacific-corridor departments.

Table 1
Municipal characteristics by decile of the pre-violence index (2013–2015).

Decile	N mun.	Index (median)	Rate (median)	FARC pres. (%)	FARC events (mean)	Dominant regions
D1 (0–10th)	108	0.00	0.0	32.4%	1.5	Andina (81%), Caribe (9%)
D2	108	0.02	3.8	40.7%	2.2	Andina (47%), Caribe (41%)
D3	108	0.04	7.3	47.2%	2.9	Andina (59%), Caribe (29%)
D4	108	0.06	11.4	51.9%	3.3	Andina (51%), Caribe (26%)
D5	108	0.07	15.2	52.8%	5.3	Andina (56%), Caribe (21%)
D6	107	0.10	19.7	64.5%	5.7	Andina (57%), Caribe (22%)
D7	108	0.12	26.0	72.2%	7.4	Andina (56%), Caribe (18%)
D8	108	0.16	33.3	70.4%	7.2	Andina (55%), Pacífico (17%)
D9	108	0.23	49.3	71.3%	9.9	Andina (51%), Pacífico (30%)
D10 (90–100th)	108	0.35	79.5	73.1%	10.2	Andina (47%), Pacífico (33%)

4. Empirical Strategy

4.1 Continuous DiD

The estimating equation is

$\ln(Hom_{i,t} + 1) = \alpha_i + \lambda_t + \beta \cdot (Index_i \times Post_t) + \gamma \cdot \ln(pop_{i,t}) + \varepsilon_{i,t}$,

where α_i are municipality fixed effects, λ_t year fixed effects, and $Post_t = 1[year \geq 2016]$. Municipality fixed effects absorb all time-invariant local characteristics; year fixed effects absorb common shocks. Because the index is normalized to [0, 1], β is the maximum differential effect — between a municipality at Index = 1 and a hypothetical one at Index = 0 — comparing the post-agreement period (2016–2023) to the pre-period (2013–2015). I implement TWFE via the within-transform and cluster standard errors at the municipality level. Identification rests on a continuous parallel-trends condition: absent the agreement, municipalities of different index intensity would have followed parallel homicide paths (Callaway, Goodman-Bacon & Sant'Anna 2021).

The identifying assumption is a continuous analogue of parallel trends: conditional on the fixed effects, municipalities of different pre-violence intensity would, absent the agreement, have followed homicide paths that move in parallel. This is weaker than requiring treated and untreated units to be alike in levels — the fixed effects absorb all stable differences — but stronger than it may appear, because it rules out that high- and low-index municipalities were already diverging for reasons unrelated to the agreement. The event study in Section 4.3 tests exactly this, and the permutation test in Section 5.5 provides a complementary check by asking whether the estimated gradient could arise from the spatial distribution of pre-existing violence alone. Inference clusters standard errors at the municipality level, allowing arbitrary serial correlation within a municipality over the eleven years of the panel; with a large number of clusters (1,079), cluster-robust inference is well behaved here, and the permutation test offers a design-based alternative that does not rely on an asymptotic approximation.

4.2 IV-DiD 2SLS

The pre-violence index is an endogenous proxy: it is measured with error and may be correlated with determinants of the post-2016 homicide path other than FARC exposure. I therefore also estimate an IV-DiD 2SLS model that instruments the treatment $Index_i \times Post_t$ with the first-stage predictors of Section 3.3 interacted with $Post_t$ — that is, $\{\ln(FARC_events_i + 1), \ln(pop_i), Region_i\} \times Post_t$ — within the same two-way fixed-effects structure and with municipality-clustered standard errors. Intuitively, the IV isolates the component of pre-violence exposure that is predicted by documented FARC activity, population, and region, and asks whether that component drives the post-2016 decline. A 2SLS estimate of the same sign and significance indicates that measurement error and index-specific confounding are not driving the result; classical measurement error in the index attenuates the 2SLS estimate toward zero relative to TWFE.

The design rests on an exclusion restriction: conditional on the fixed effects and controls, the predicted component of exposure — the part explained by documented FARC events, population, and region interacted with the post-agreement indicator — affects post-2016 homicides only through the pre-violence index, and not through some other channel that switched on in 2016. This restriction is not guaranteed, and the 2SLS estimate is best read as a robustness device rather than a cleaner identification than the reduced form: if FARC-event exposure also proxies for coca suitability or distance to the state, the instrument inherits those correlations. Its value is narrower and worth stating plainly. The estimate isolates the part of the pre-violence index that documented guerrilla activity can account for, and asks whether that part carries the effect. That it does — negative, significant, and attenuated in the direction classical measurement error predicts — makes it less likely that the reduced-form result is driven by idiosyncratic, non-conflict violence captured by the raw index.

4.3 Event study

To test parallel trends and trace dynamics I estimate

$$\ln(\text{Hom}_{i□} + 1) = \alpha_i + \lambda_□ + \Sigma(t \neq 2015)\ \beta_□ \cdot (\text{Index}_i \times 1[\text{year} = t]) + \gamma \cdot \ln(\text{pop}_{i□}) + \varepsilon_{i□},$$

with 2015 as the reference year. Under no differential pre-trend, $\beta_□$ for $t \in \{2013, 2014\}$ should be zero. Interaction terms are within-transformed by municipality, so coefficients capture within-municipality changes in the index–violence relationship. The post-2016 path also shows whether the effect attenuates over time.

4.4 Threats to identification

The principal threat is a violation of parallel trends; the event study addresses it directly. A second is mean reversion — extreme-violence municipalities drifting toward the mean regardless of the agreement — which would appear as negative, significant pre-period coefficients; their absence (both $p > 0.48$) is evidence against it. A third is spatial spillovers, if demobilization displaced violence to neighboring municipalities, which this design does not model. A fourth is reporting: if the agreement expanded police coverage in high-index municipalities, part of the measured decline would reflect better recording; cross-validation against INMLCF forensic data would address this in future work.

Of these threats, differential reporting is the hardest to dismiss and the most consequential for the sign of the bias. If the agreement brought police and civil administration into previously ungoverned municipalities — exactly the high-exposure places that drive the result — then some of the measured post-2016 decline could reflect a transition from undercounting toward fuller recording, which would understate the true fall and bias the estimate toward zero rather than away from it; in that case the reported effects are conservative. The opposite concern, that improved recording inflates the apparent decline, would require reporting to have worsened in high-exposure municipalities after the agreement, which runs against the direction of state entry. Adjudicating this requires a second homicide series with different coverage properties — the forensic records of the Instituto Nacional de Medicina Legal — and a reporting-robustness exercise that the replication package is structured to accommodate.

5. Results

5.1 Main estimates

Across every specification the effect is negative, large, and significant at the 1% level (Table 2).

Table 2
Main results: continuous difference-in-differences, municipal panel 2013–2023.

Specification	β (Index×Post)	SE	95% CI	N	Mun. FE	Year FE
(1) OLS pooled	−1.642***	(0.192)	[− 2.019, − 1.266]	11,869	No	No
(2) TWFE	−1.648***	(0.192)	[− 2.025, − 1.271]	11,869	Yes	Yes
(3) TWFE + Region×Year	−1.736***	(0.211)	[− 2.149, − 1.323]	11,869	Yes	Yes
(4) IV-DiD 2SLS	−1.067**	(0.417)	[− 1.885, − 0.250]	11,869	Yes	Yes

The OLS and TWFE estimates are virtually identical (− 1.642 vs. −1.648), so the result is not driven by cross-sectional level differences; region-by-year effects raise it slightly to − 1.736. The implied maximum effect (Index = 1, Policarpa) is a reduction of about 81% = 1 − exp(− 1.648) relative to a municipality at Index = 0. Specification (4) instruments Index×Post with {ln(FARC events + 1), ln(pop), region} × Post (first-stage F = 37); the 2SLS estimate is − 1.067 (SE 0.417, p = 0.011) — the same sign and significant at the 5% level, attenuated relative to TWFE as classical measurement error in the index would predict, which corroborates the main result.

It is worth translating these coefficients out of logs. A municipality near the ninety-fifth percentile of the index entered the post-agreement period with a homicide rate on the order of the upper-decile average of roughly 80 per 100,000; an implied reduction of about 44% corresponds to a fall of some 35 homicides per 100,000 — a change of the same order as the entire national decline over the preceding fifteen years, concentrated in the municipalities that had suffered most and realized within a few years of the agreement. At the median of the index the implied reduction is about 13%, and below the twenty-fifth percentile it is in single digits and statistically hard to distinguish from the ordinary movement of low-violence municipalities. The gradient is therefore not a subtle artifact visible only in logs; it is a first-order difference in the lived security of Colombian communities, almost entirely confined to the places the conflict had marked most heavily. This is the sense in which a national average — mixing the steep declines of a few hundred municipalities with the near-flat paths of the rest — substantially understates what the agreement did where it did the most.

5.2 Effects across the distribution

Because the index is bounded in [0, 1] and enters linearly, the implied effect at percentile p is β̂ × Index_p, with standard errors by the delta method (Table 3).

Table 3
Implied effects across the distribution of the pre-violence index.

Percentile	Index	Effect (log-pts)	Implied reduction
p25	0.038	−0.062	6.0%
p50	0.083	−0.136	12.7%
p75	0.159	−0.262	23.1%
p90	0.279	−0.460	36.9%
p95	0.352	−0.580	44.0%
p100 (max)	1.000	−1.648	80.8%

Read down the table, the implied effects trace the same steep gradient the design was built to detect. The effect is negligible through the lower half of the distribution — the municipalities where the conflict was faint — and accelerates sharply in the upper tail, where a one-step move up the percentile ladder buys a far larger reduction than the same move lower down. This convexity is a direct consequence of the right-skewed index: because exposure is concentrated in a thin upper tail, so is the dividend. It also means that the policy-relevant quantity is not the average effect across municipalities, which is dominated by the many low-exposure places where little happened, but the effect in the tail, which is where the agreement did its work.

5.3 Event study

Event study · SIEDCO / DANE · panel 2013–2023, reference year 2015 · 95% CI from municipality-clustered SE

The 2013 and 2014 coefficients are statistically zero (β_{2013} = 0.187, p = 0.52; β_{2014} = 0.113, p = 0.70), supporting parallel pre-trends. From 2016 every coefficient is negative and significant (range − 1.16 to − 1.91, mean − 1.50), largest in 2016 itself — consistent with an immediate decline at demobilization rather than a gradual process. The post-2016 path shows no systematic attenuation through 2023; read this cautiously, since stable net coefficients can also reflect a balance between the peace dividend and partial territorial recomposition.

The shape of the post-2016 path is as informative as its level. An effect that grew gradually would point to slow institutional channels — courts, policing, service delivery — taking hold; instead the effect appears at full strength in 2016 and fluctuates around that level thereafter, the signature of a discrete removal of an armed actor rather than a cumulative process of state-building. At the same time the path does not decay back toward zero, as it would if successor organizations were simply refilling the vacated territory at the same intensity. The coefficients wobble from year to year — the 2019 and 2022 dips are

visible in Fig. 3 — in a way consistent with the episodic violence of a reconfiguring criminal landscape, but the central tendency holds well below zero through 2023. The honest reading is that the aggregate differential dividend proved durable over the seven post-agreement years observed here, while leaving open whether its composition — who kills whom, and why — shifted beneath a stable total.

5.4 Parallel trends

Figure 4 plots median homicide rates by index tercile. In 2013–2014 the three terciles move in parallel; from 2016 the top tercile diverges sharply downward while the lower terciles barely move — the structure the event study shows.

5.5 Placebo test

Randomly reassigning the 1,079 index values across municipalities (keeping the marginal distribution, destroying its spatial structure) and re-estimating 500 times yields a null distribution centered at 0.002 (SD 0.212). The real coefficient of − 1.648 lies 7.8 standard deviations below that mean, and no placebo reaches it (permutation $p < 0.001$, 0 of 500).

5.6 Robustness

Table 4
Robustness checks (two-way fixed effects).

Specification	β	SE	N	Note
(5) No winsorization	−1.637***	(0.189)	11,869	ln(rate + 1), raw
(6) Population > 5,000	−1.335***	(0.190)	10,152	p99 winsor.
(7) Log count + pop. offset	−0.829***	(0.098)	11,869	ln(count + 1)
(8) Drop top-5 index mun.	−1.905***	(0.186)	11,814	p99 winsor.

Across all four checks β stays negative and significant at the 1% level (Table 4). Among the three rate-based specifications it ranges from − 1.33 (dropping municipalities under 5,000 people) to − 1.91 (dropping the five highest-index municipalities), so the result is not an artifact of winsorization or of a handful of extreme municipalities; the log-count specification (− 0.829) is smaller in magnitude only because its outcome is counts rather than rates, and points the same way.

The robustness checks also discipline how much weight the single headline coefficient should bear. The estimate is most sensitive to the choice of outcome scale — moving from a rate to a count lowers it, as expected, since counts do not adjust for the large populations of the least violent municipalities — and least sensitive to winsorization and to dropping the highest-index municipalities, the two checks that most directly probe whether a handful of extreme places drive the result. That the coefficient actually grows when the five most-exposed municipalities are removed is reassuring rather than troubling: it

shows the gradient is a property of the whole upper tail, not of a few outliers whose idiosyncratic histories might otherwise be doing the work.

6. Discussion

6.1 Interpretation and mechanisms

The 2016 agreement generated a spatially heterogeneous peace dividend, concentrated in municipalities with higher pre-agreement violence and, by the first-stage validation, deeper engagement with FARC-related conflict. The TWFE estimate of $\beta = -1.648$, the clean pre-trends, the permutation-placebo rejection, the four robustness checks, and the IV-DiD corroboration together support a causal reading — with the caveat that the index is a proxy for FARC exposure, not a direct measure. The large 2016 coefficient ($\beta_{2016} = -1.914$) implies the decline materialized quickly, faster than state-building or institutional channels, which take years (Kalyvas 2006; Blattman & Miguel 2010). Arjona's (2016) framework adds a complementary mechanism: where FARC rebelocracy was established, the group had been regulating local violence, so its departure removed both a perpetrator and an ordering institution — a net decline even as new actors began to enter.

It helps to separate the mechanisms that could produce this pattern, because they carry different implications. The most direct is the simple cessation of FARC-perpetrated killings and of the clashes, reprisals, and collateral deaths that accompanied an active insurgency; where the FARC was most present this channel mechanically removes the most violence, and its speed matches the immediate 2016 break. A second channel is the removal of the FARC as a local order. Following Arjona (2016), in zones of sustained control the group had regulated disputes, policed everyday conduct, and suppressed freelance violence as a by-product of governing; its departure withdrew that order, which on its own would raise violence, yet in the first post-agreement years the net effect of losing both the perpetrator and the order was a sharp decline — consistent with the perpetrator channel dominating initially. A third channel works against the first two: the entry of dissident factions, the ELN, and criminal successors competing over vacated rents and routes, which the event study's non-attenuation and the evidence of Prem, Rivera, Romero, and Vargas (2022) suggest was already under way. The data here cannot apportion the decline among these channels, but their relative timing — immediate drop, durable level, year-to-year volatility — is consistent with a large and fast perpetrator effect partly offset, over time, by recomposition.

6.2 The territorial-vacuum hypothesis

The absence of systematic post-2016 attenuation is suggestive but not strong evidence against the vacuum hypothesis. Prem, Rivera, Romero, and Vargas (2022) document a surge in killings of community leaders by non-participating groups, and Daly (2016) shows demobilization redeploys rather than dissolves armed networks — mechanisms that can generate new violence whose differential intensity across high- and low-index municipalities stays stable even as absolute levels shift. Testing the hypothesis properly requires tracking armed-actor identities over time with disaggregated event data,

beyond this paper's scope. The most this design can say is that the aggregate differential effect does not erode systematically through 2023.

Distinguishing a durable dividend from a hidden recomposition ultimately requires data this design does not use. Total homicides can hold steady or fall even as the identity of perpetrators turns over, because a successor group that consolidates control may, for a time, kill less than the contest it won; conversely, a stable total can mask a shift from conflict deaths to the targeted killing of community leaders documented by Prem, Rivera, Romero, and Vargas (2022). The event-study path is consistent with either a genuinely consolidated peace or a quieter but still-contested equilibrium, and the two are not observationally distinguishable in aggregate homicide counts. Resolving them is the natural next step, and it requires actor-attributed conflict data layered onto the municipal panel used here.

6.3 Mean reversion

The continuous DiD design limits mean-reversion concerns two ways. The TWFE estimator identifies β from within-municipality changes relative to the national trend, not cross-sectional level comparisons; and under mean reversion the pre-period event-study coefficients should be negative and significant as high-index municipalities drift toward the mean, whereas they are statistically zero with a sharp break only at 2016.

Mean reversion deserves emphasis because it is the most plausible non-causal story for a result like this one, and because the design was chosen largely to defeat it. A naive before-after comparison of the most violent municipalities would confound the agreement with the ordinary tendency of extreme values to fall. The continuous difference-in-differences design blocks that confound in two ways: it identifies the effect from the interaction of exposure with the post-agreement indicator, net of each municipality's own mean and the national trend, and it predicts a specific counterfactual signature under pure reversion — steadily declining pre-period coefficients — that the event study does not show. The pre-period coefficients are flat and statistically zero, and the decline appears as a discrete break at 2016 rather than a smooth drift, which is the pattern an intervention produces and reversion does not.

6.4 Limitations

Four limitations condition the reading. First, the index is a proxy: its variation also reflects ELN, paramilitary-successor (BACRIM/AGC), and common-crime violence (Durán-Martínez 2018; Lessing 2018); the first-stage R^2 of 0.170 confirms a meaningful but partial link to FARC-attributed events, and CNMH front-level presence data or fully geocoded UCDP GED would sharpen identification. Second, SIEDCO undercounts violence where police coverage is thin, so post-agreement coverage gains in high-index municipalities would bias the decline downward. Third, the design does not model spatial spillovers across municipal boundaries. Fourth, the balanced panel assigns zeros to municipality-years absent from SIEDCO, which adds measurement error if absence reflects data gaps rather than true zeros.

6.5 Policy implications and external validity

The central policy implication follows directly from the gradient. If the homicide dividend of demobilization is concentrated where prior violence was highest, then post-conflict security and investment allocated uniformly across municipalities will systematically underserve the places that both suffered most and have the most to lose if the dividend reverses. Calibrating effort to the prior geography of the war — the geography the pre-violence index captures — is the efficient response, and it aligns with the finding of Fajardo-Steinhäuser (2024) and Camacho and Mejía (2019) that the dividend is larger, and likelier to endure, where the state can occupy the space a demobilizing group leaves. Sequencing matters as much as targeting: because the violence reduction arrives immediately while state-building is slow, the early post-agreement window is when successor actors establish themselves, and therefore when preventive investment yields the highest return.

How far these findings travel beyond Colombia depends on which features of the case are essential. The design requires a conflict with a well-measured sub-national footprint, a reasonably comprehensive homicide registry, and a demobilization sharp enough in time to support an event study; these conditions hold in a growing number of post-conflict settings with administrative micro-data, though not in all. The substantive lesson — that a national agreement can deliver a large but spatially concentrated dividend, and that the durability of that dividend turns on what replaces the demobilized group — is more general, and it reframes the common complaint that peace agreements fail to pacify a whole country. The agreement studied here did not pacify Colombia uniformly, but it did not fail where it mattered most, and that uneven result is precisely what a theory of locally varying rebel governance and state capacity would predict.

7. Conclusion

Colombia's 2016 peace agreement with the FARC produced a steeply graded territorial peace dividend, concentrated in municipalities with higher pre-agreement violence. Using a continuous difference-in-differences design with a municipal pre-violence index — validated against independent FARC conflict-event data, used as the first stage of an IV-DiD, and subjected to a permutation placebo — I estimate $\beta = -1.648$ (SE = 0.192), robust across four specification checks, supported by statistically zero pre-period event-study coefficients, corroborated by IV-DiD 2SLS, and unambiguously rejected by the placebo distribution. The implied dividend runs from a 6% reduction at the 25th percentile of the index to 44% at the 95th, so aggregate national estimates substantially understate the effect for the most conflict-affected municipalities.

Three implications follow. First, post-conflict security resources should be allocated by the geographic intensity of the prior conflict, not uniformly: a uniform per-municipality allocation would underserve exactly the places where the conflict — and now the dividend — is most concentrated. Second, the rapid 2016 effect implies comprehensive demobilization cuts violence immediately through the cessation of direct killings, before state-building can plausibly act — reinforcing Fajardo-Steinhäuser (2024) and Camacho and Mejía (2019) that agreements need complementary state-capacity investments to convert violence reductions into broader dividends, and that early security investment should pre-empt

successor armed actors in vacated territory. Third, the absence of systematic attenuation through 2023 is encouraging but must be monitored: the territorial recomposition documented by Prem, Vargas, and Mejía (2021) and Prem, Rivera, Romero, and Vargas (2022) may act differentially over longer horizons.

These findings also sharpen how the success of a peace agreement should be judged. Measured against the aspiration of a uniformly pacified country, the 2016 agreement looks partial: violence persisted, successor actors advanced, and the national homicide rate later edged upward. Measured against what demobilization can plausibly deliver — a large, fast, and durable reduction in lethal violence precisely in the territories an insurgency had dominated — the agreement delivered, and did so on the margin that matters most for the communities that bore the war. Holding these two readings together, rather than collapsing into either triumph or disappointment, is what a sub-national evidence base makes possible, and it is the posture this article recommends for evaluating the agreements that will follow.

The priority extensions are: an instrumental-variables design using geographic or historical FARC-presence predictors to instrument the index more directly; a spatial DiD that models cross-municipal spillovers; and a SIEDCO-versus-INMLCF comparison to gauge the role of differential reporting coverage.

Declarations

Conflicts of interest

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Author contributions

The manuscript has a single author, responsible for conceptualization, data curation, formal analysis, software, writing of the original draft, and review and editing.

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Data availability

The data, codebook, and replication code for the empirical analysis in this article can be found at https://drive.google.com/file/d/1WyYWhX8g1vaCVlIII7RyuK1fyvLqI9jR/view?usp=sharing

The analysis was conducted in Python 3.11 (pandas, statsmodels, linearmodels, scipy). All underlying sources are public: incident-level homicide records from SIEDCO (Policía Nacional de Colombia, via datos.gov.co), municipal population projections from DANE, and FARC-attributed conflict events from the UCDP Georeferenced Event Dataset, version 24.1. No access restrictions apply, and the replication package reproduces every table and figure from these inputs.

Declaration of generative AI use

Generative AI (Anthropic's Claude) was used to assist in drafting and editing the manuscript text, organizing the literature review, and reviewing and documenting the replication code. The author verified all analytical results, numerical values, citations, and interpretations, and takes full responsibility for the content. Generative AI was not used to generate data or empirical results.

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Figures

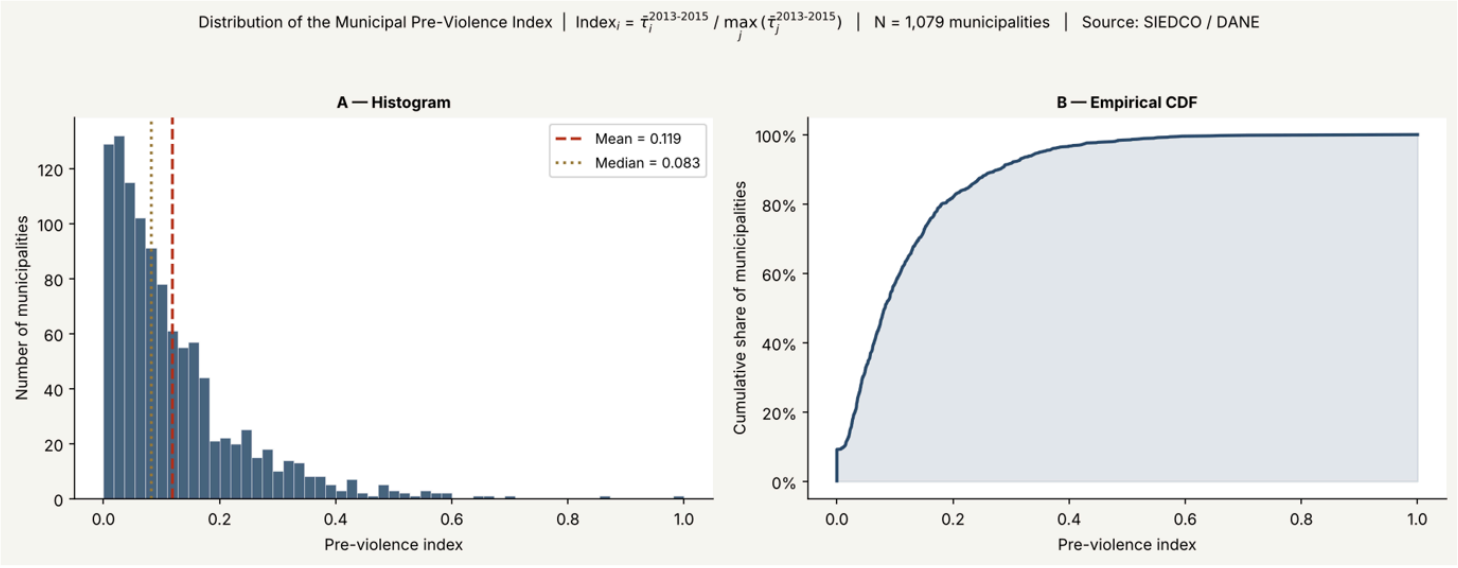


Figure 1

Distribution of the municipal pre-violence index: histogram and empirical cumulative distribution. Source: SIEDCO and DANE.

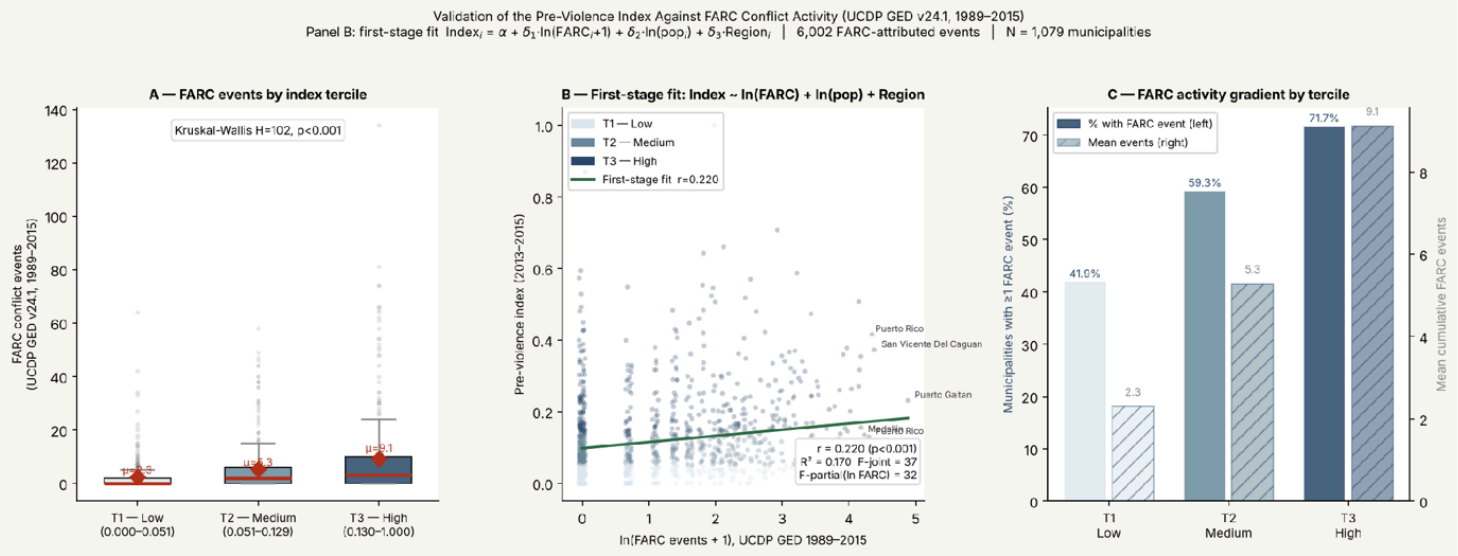


Figure 2

Validation of the pre-violence index against FARC conflict activity (UCDP GED v24.1) and the first-stage fit. Panel A, FARC events by index tercile; Panel B, first-stage fit of the index on log FARC events, log population, and region; Panel C, activity gradient by tercile. Source: SIEDCO, DANE, and UCDP GED.

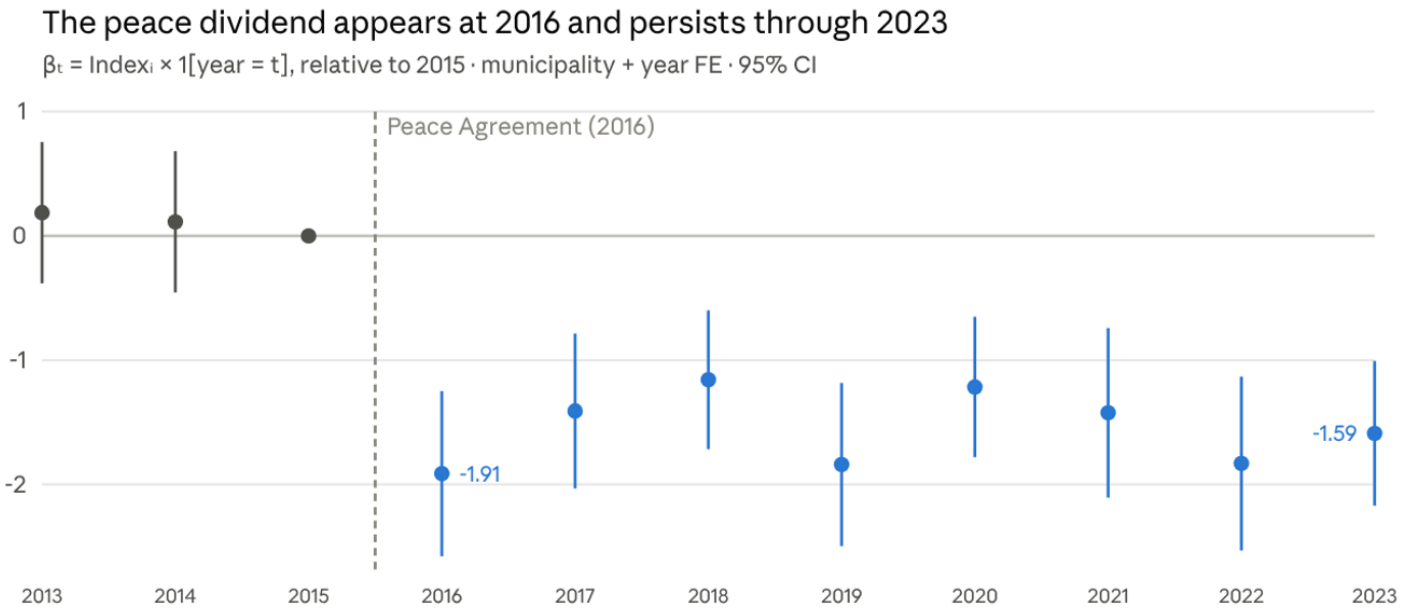


Figure 3

Event study of the heterogeneous effect by year, relative to 2015. Points are coefficients on the pre-violence index interacted with year indicators; whiskers are 95% confidence intervals from municipality-clustered standard errors. Source: SIEDCO and DANE.

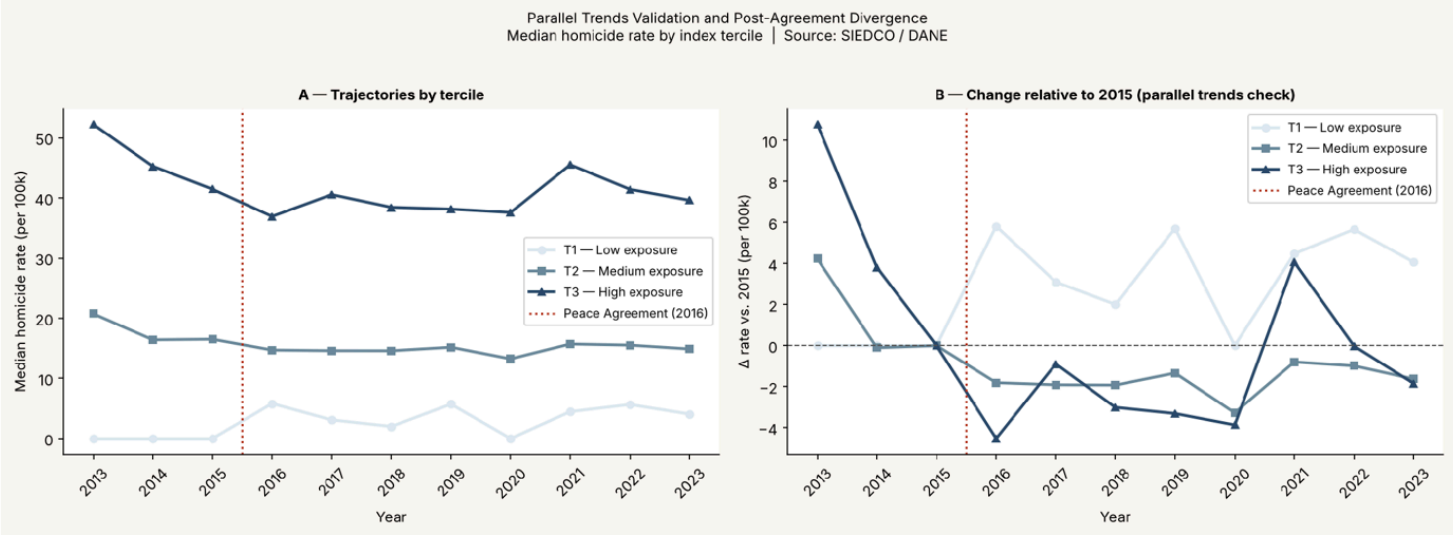


Figure 4

Median municipal homicide rate by index tercile: levels (Panel A) and change relative to 2015 (Panel B). Source: SIEDCO and DANE.

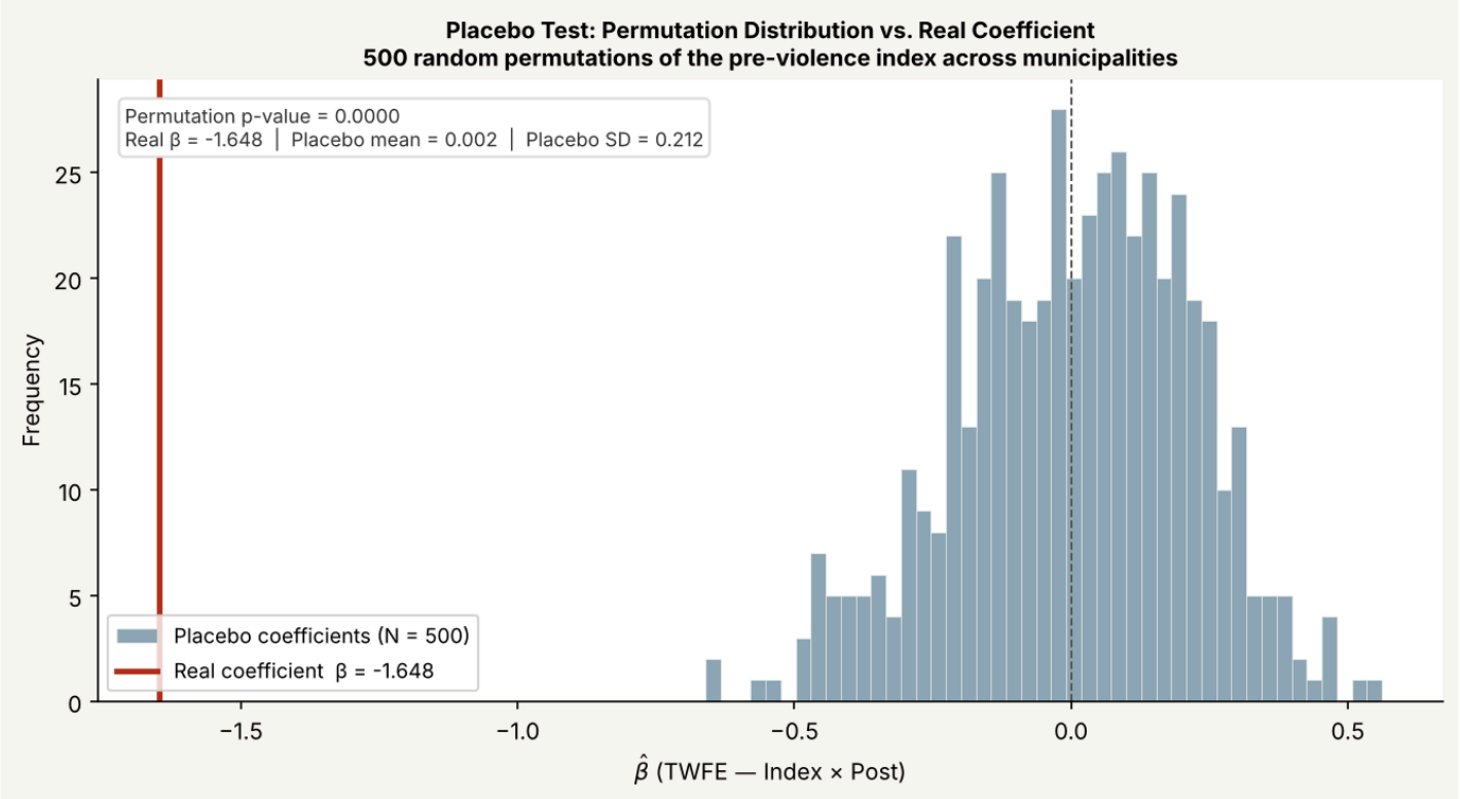


Figure 5

Permutation placebo distribution (500 random reallocations of the index across municipalities) versus the estimated coefficient. Source: SIEDCO and DANE.