The Truce Illusion: Gang Cease-Fires and the Geography of Violence in El Salvador

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Abstract

El Salvador's 2012 gang truce between MS-13 and Barrio 18 halved the national homicide rate. We test whether this reduction was concentrated in municipalities with greater pre-truce gang presence using a continuous difference-in-differences design that exploits variation in 2011 gang-member detention rates across 261 municipalities. A baseline specification with municipality and year fixed effects yields a seemingly significant result: a one-standard-deviation increase in gang intensity is associated with a 0.52 per 10,000 additional decline in homicide rates during the truce ($p = 0.005$). Yet this effect vanishes in our preferred specification with department-by-year fixed effects, and placebo tests detect similar patterns in the pre-truce period. The gang-specific "truce dividend" appears to be an artifact of broader geographic trends rather than a targeted reduction in gang violence. These findings caution against interpreting aggregate violence declines as evidence that negotiated gang peace works at the local level.

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1. Introduction

On March 9, 2012, El Salvador's two most powerful gangs—Mara Salvatrucha (MS-13) and Barrio 18—declared a cease-fire. Within weeks, the daily homicide count fell from roughly 14 to 5. The truce, brokered by a Catholic bishop and a former guerrilla commander with the tacit support of the Funes government, appeared to accomplish what years of *mano dura* policing had not: a dramatic, sustained reduction in lethal violence (Cruz and Durán-Martínez, 2016). For observers of organized crime worldwide, the Salvadoran truce became a canonical case study—cited as evidence that negotiating with gangs can save lives (Lessing, 2017; Moncada, 2016).

But did the truce actually reduce gang violence at the local level, or did it merely coincide with a broader decline? This question matters because the policy implication depends entirely on the mechanism. If the truce caused gang-heavy municipalities to experience differentially larger declines in homicides, then negotiated cease-fires represent a viable tool for reducing organized-crime violence. If the decline was geographically uniform, then the truce's apparent success may reflect something else entirely—regression to the mean, seasonal patterns, or political-economy dynamics that reduced violence independently of gang behavior.

We exploit the sharp temporal discontinuity of the 2012 truce and cross-sectional variation in pre-truce gang presence to test for gang-specific violence reductions. Our continuous difference-in-differences design interacts municipality-level gang intensity—measured by 2011 gang-member detention rates per 10,000 population—with indicators for the truce period (2012–2013) and the post-collapse period (2014–2021). The design follows the logic of dose-response studies: if the truce caused gang-affiliated individuals to reduce killings, the effect should be proportional to local gang presence (Callaway and Sant'Anna, 2021; Goodman-Bacon, 2021).

The data come from a recently published PLOS ONE dataset covering all 261 Salvadoran municipalities over 2002–2021 (Martínez-Folgar et al., 2024). This panel provides 5,220 municipality-year observations with homicide rates from police and forensic records, gang-member detention counts from the Policía Nacional Civil, and population projections from the national statistics office. The 20-year span gives us 10 pre-truce years (2002–2011) to assess parallel trends and 8 post-collapse years (2014–2021) to trace the violence trajectory after the cease-fire unraveled.

A baseline specification with municipality and year fixed effects yields a seemingly compelling result: a one-standard-deviation increase in gang intensity is associated with a 0.52 per 10,000 additional decline in homicide rates during the truce period (SE = 0.185, $p = 0.005$). The standardized effect size of -0.10 is moderate, and the coefficient is stable across leave-one-department-out exercises ($\beta \in [-0.746, -0.441]$). A binary treatment version, comparing municipalities above versus below the median gang detention rate, finds even larger effects: high-gang municipalities experienced 1.38 fewer homicides per 10,000 during the truce ($p < 0.01$). Taken at face value, these estimates suggest the truce generated a meaningful, targeted reduction in violence precisely where gangs were most active.

Two diagnostic tests unravel this interpretation and motivate our preferred specification. First, when we replace year fixed effects with department-by-year fixed effects—absorbing all time-varying shocks at the department level (El Salvador's 14 departments are roughly analogous to U.S. states)—the truce coefficient falls to -0.206 (SE $= 0.179$, $p = 0.250$) and loses statistical significance entirely. We treat this as the preferred specification because it isolates within-department, across-municipality variation in gang intensity. This means that the apparent gang-specific truce effect is explained by differential trends across departments, not by within-department variation in gang presence. Second, placebo tests that assign false truce dates at 2005 and 2008 detect statistically significant "treatment effects" of similar magnitude to the true truce estimate. If the same pattern appears before the truce existed, the pattern cannot be caused by the truce.

Our findings contribute to a growing literature on criminal governance and the political economy of violence. Sviatschi (2022) demonstrates that gang presence spreads through the deportation of gang members from the United States to Central America, generating persistent increases in local violence. We study the mirror image: whether negotiated peace among gangs can undo this violence at the local level. Our null result—that it cannot, at least through this channel—complements her finding by showing that gang violence has geographic determinants that operate independently of gang-level agreements. Dell (2015) shows that Mexico's militarized enforcement strategy displaced violence across municipalities; our paper asks whether the negotiation alternative fares better, and finds that it does not produce gang-specific local effects either.

The paper also speaks to the broader literature on conflict reduction and cease-fires. Berman et al. (2011) show that government spending can reduce insurgent violence, while Dube and Vargas (2013) demonstrate that commodity price shocks drive civil conflict. Both papers establish that violence responds to local economic conditions. Our contribution is to show that a national-level cease-fire, even one that achieves dramatic aggregate reductions, does not necessarily alter the geographic distribution of violence. The mechanism appears to operate at a scale above the municipality—consistent with department-level political dynamics rather than neighborhood-level gang decisions.

Methodologically, our paper illustrates the importance of absorbing higher-level geographic trends in difference-in-differences designs. The two-way fixed effects specification (municipality

+ year) is the standard in the crime literature (Chalfin and McCrary, 2018; Draca et al., 2011), but it attributes any differential trend between high-gang and low-gang areas to the treatment. Our results show this attribution can be misleading when the treatment variable is spatially correlated with pre-existing geographic trends—a concern emphasized by Imai and Kim (2021) in the context of unit fixed effects and by Goodman-Bacon (2021) in the TWFE decomposition literature. The department-by-year specification provides a more demanding test by asking whether, within a given department and year, municipalities with more gang activity experienced differentially larger violence reductions. The answer is no.

The remainder of the paper proceeds as follows. Section 2 provides background on the Salvadoran gang truce. Section 3 describes the data. Section 4 presents the empirical strategy. Section 5 reports results. Section 6 discusses implications, and Section 7 concludes.

2. Institutional Background

The Salvadoran gang landscape. El Salvador's gang problem has roots in the mass deportation of Salvadoran nationals from the United States during the 1990s (Sviatschi, 2022). MS-13 and Barrio 18, both formed in Los Angeles, were transplanted to a post-civil-war society with weak institutions and abundant weapons. By the 2000s, the two gangs controlled territory across the country, extorting businesses, recruiting youth, and engaging in persistent turf wars that made El Salvador one of the most violent countries in the world outside of active war zones (Soares and Naritomi, 2010).

The 2012 truce. In early 2012, with the national homicide rate exceeding 70 per 100,000, the government of President Mauricio Funes facilitated secret negotiations between imprisoned leaders of MS-13 and Barrio 18. On March 9, both gangs announced a cease-fire. The government's role was initially denied but later acknowledged; it included transferring gang leaders from maximum-security to lower-security facilities (Cruz and Durán-Martínez, 2016). The immediate effect was dramatic: homicides fell by roughly 40 percent within weeks, and the reduced rate persisted through 2013.

The political economy of the truce. The truce was deeply controversial. Critics argued that it legitimized gang leadership, gave gangs political leverage, and may have concealed rather than prevented killings—Cruz and Durán-Martínez (2016) document evidence that gangs hid bodies to maintain the appearance of peace. Supporters pointed to the saved lives as sufficient justification. The truce became a central issue in the 2014 presidential election, which was won by Salvador Sánchez Cerén of the FMLN, who ran on a platform of ending negotiations with gangs.

Collapse and escalation. The truce effectively collapsed in mid-2014 as the new government withdrew support and reimposed confrontational policing. Homicides surged: the rate reached 9.67 per 10,000 in 2015, making El Salvador the "murder capital of the world." This escalation continued through 2016 before gradually declining. In 2019, Nayib Bukele was elected president, and in 2022, following a particularly violent weekend, he declared a state of exception (*régimen de excepción*) that suspended civil liberties and led to mass incarceration. Homicides fell precipitously, but through a mechanism—authoritarian repression—fundamentally different from the negotiated truce (Lessing, 2017).

Why the truce matters for policy. The Salvadoran truce is one of the few cases worldwide in which a government negotiated a cease-fire with criminal organizations rather than insurgent groups. It provides a rare opportunity to study whether such negotiations can reduce violence at the local level, or whether their effects operate only at the aggregate level through mechanisms—such as political signaling or media dynamics—that do not require gangs to actually change their behavior in specific communities.

3. Data

Our data come from the supplementary materials of Martínez-Folgar et al. (2024), a study of spatio-temporal variation in Salvadoran homicide rates. The dataset comprises three components covering all 261 municipalities.

Homicide rates. Annual homicide counts per 10,000 population for 2002–2021 (dataset S1). For 2002–2007 and 2014–2021, the source is the Policía Nacional Civil; for 2008–2013, the source is the Instituto de Medicina Legal. The resulting panel contains $N = 5{,}220$ municipality-year observations. The pre-truce mean homicide rate (2002–2011) is 3.73 per 10,000. During the truce (2012–2013), the mean falls to 3.27—a 12 percent decline. After the collapse (2014–2021), the mean rises to 5.14, reflecting the 2015 peak of 9.67 per 10,000.

Gang intensity. Annual gang-member detention counts by municipality for 2011–2018 (dataset S2). We use the 2011 measure—the last full year before the truce—to construct our treatment variable: gang detentions per 10,000 population. This measure captures the *revealed* presence of gang activity as detected by police operations. Of 261 municipalities, 140 (54 percent) have nonzero 2011 gang detentions. The mean detention rate is 3.43 per 10,000 with a standard deviation of 7.49, reflecting substantial right skew driven by heavily gang-affected urban municipalities.

Population. Annual population projections by municipality for 2002–2022 (dataset S3) from the Oficina Nacional de Estadística y Censos (ONEC). These are used to construct per-capita rates for both homicides and gang detentions.

Summary statistics. Section 3 presents descriptive statistics for the key variables across the three periods.

Table 1: Summary Statistics

	Mean	SD	Min	Max
Panel A: Homicide rate (per 10,000)				
Full sample (2002–2021)	4.24	5.17	0.00	81.66
Pre-truce (2002–2011)	3.73	4.27		
Truce (2012–2013)	3.27	2.73		
Post-collapse (2014–2021)	5.14	6.38		
Panel B: Treatment intensity				
Gang detentions per 10,000 (2011)	3.43	7.49	0.00	81.99
Any 2011 detentions (=1)	0.53	0.50		
High-gang municipality (=1)	0.50	0.50		
Panel C: Municipality characteristics				
Population (2011)	23,777	40,496	655	308,036

Notes: Panel of 261 municipalities observed annually from 2002 to 2021 (N = 5,220 municipality-years). Homicide rates are per 10,000 population from Policía Nacional Civil and Instituto de Medicina Legal records. Gang intensity is PNC gang-member detentions per 10,000 population in 2011, the last full pre-truce year. Population from ONEC projections.

4. Empirical Strategy

Main specification. We estimate a continuous difference-in-differences model that interacts time-invariant municipality-level gang intensity with period indicators:

$$\text{Hom}_{mt} = \alpha_m + \gamma_t + \beta_1 \left(\text{Gang}_m \times \text{Truce}_t\right) + \beta_2 \left(\text{Gang}_m \times \text{PostCollapse}_t\right) + \varepsilon_{mt} \tag{1}$$

where Hom_{mt} is the homicide rate per 10,000 in municipality m and year t; α_m are municipality fixed effects; γ_t are year fixed effects; Gang_m is the 2011 gang detention rate per 10,000 (continuous); $\text{Truce}_t = \mathbb{I}\{2012 \leq t \leq 2013\}$; and $\text{PostCollapse}_t = \mathbb{I}\{t \geq 2014\}$. Standard errors are clustered at the municipality level.

The coefficient β_1 measures the differential change in homicide rates during the truce for municipalities with higher pre-truce gang presence, relative to the pre-truce period (2002–2011). If the truce reduced gang violence specifically, $\beta_1 < 0$: places with more gangs should experience larger declines. The coefficient β_2 captures the post-collapse differential: if violence rebounds symmetrically, $\beta_2 \approx 0$; if it overshoots, $\beta_2 > 0$.

Event study. To trace the dynamics of the effect and visually assess pre-trends, we estimate an event-study specification:

$$\text{Hom}_{mt} = \alpha_m + \gamma_t + \sum_{k \neq 2011} \beta_k \left(\text{Gang}_m \times \mathbb{I}\{t = k\}\right) + \varepsilon_{mt} \tag{2}$$

with 2011 as the omitted reference year. The β_k coefficients trace out the year-by-year differential association between gang intensity and homicide rates.

Identifying assumption. The key assumption is that, absent the truce, municipalities with different levels of gang intensity would have followed parallel trends in homicide rates. This is a strong assumption, and we test it in two ways. First, the event-study coefficients for 2002–2010 directly assess whether pre-existing differential trends exist. Second, we run placebo regressions assigning false truce dates at 2005 and 2008.

Department-by-year fixed effects. The most demanding test replaces year fixed effects γ_t with department-by-year fixed effects δ_{dt}, where d indexes El Salvador's 14 departments. This specification absorbs all time-varying shocks at the department level—including department-specific security policies, migration patterns, and economic conditions—and identifies the truce effect solely from within-department variation in gang intensity. If the municipality-and-year specification yields a significant effect but the department-by-year specification does not, the apparent effect is driven by cross-department trends rather than within-department gang-specific dynamics.

5. Results

Main results. Section 5 reports the main estimates. Column (1) presents the preferred specification with municipality and year fixed effects. A one-unit increase in the 2011 gang detention rate is associated with a $\beta_1 = -0.519$ decline in the homicide rate during the truce (SE = 0.185, $p = 0.005$). The post-collapse coefficient is $\beta_2 = -0.626$ (SE = 0.267, $p = 0.020$), indicating that higher-gang municipalities continued to have relatively lower homicide rates after the truce ended—a puzzling pattern if the truce were the mechanism, since its collapse should have reversed the differential.

Column (2) reports the binary treatment specification, comparing municipalities above the median gang detention rate to those below it. High-gang municipalities experienced 1.376 fewer homicides per 10,000 during the truce (SE = 0.317, $p < 0.01$) and 1.921 fewer during the post-collapse period (SE = 0.423, $p < 0.01$). The larger post-collapse coefficient deepens the puzzle: the effect grows after the truce ends, inconsistent with a mechanism tied to the cease-fire itself.

Table 2: Effect of Gang Intensity on Homicide Rates During and After the Truce

	(1) OLS	(2) Muni FE	(3) Muni+Year FE	(4) Log	(5) Binary
Panel A: Truce period (2012–2013)					
Gang intensity × Truce	-0.455***	-0.519***	-0.519***	-0.046	
	(0.161)	(0.170)	(0.185)	(0.040)	
High-gang × Truce					-1.376***
					(0.317)
Panel B: Post-collapse (2014–2021)					
Gang intensity × Post	1.411***	-0.624*	-0.626**	-0.143**	
	(0.216)	(0.324)	(0.267)	(0.058)	
High-gang × Post					-1.921***
					(0.423)
Municipality FE	No	Yes	Yes	Yes	Yes
Year FE	No	No	Yes	Yes	Yes
N	5,220	5,220	5,220	5,220	5,220

Notes: Standard errors clustered at the municipality level in parentheses. * p<0.10, ** p<0.05, *** p<0.01. The dependent variable is the homicide rate per 10,000 population in columns (1)–(3) and (5), and log(homicide rate + 0.1) in column (4). Gang intensity is standardized 2011 gang-member detentions per 10,000 population. "Truce" equals one for 2012–2013. "Post" equals one for 2014–2021. Column (5) uses a binary treatment indicator (above-median gang intensity).

Event study. Section 5 reports the year-by-year coefficients from Equation 2. The pre-truce coefficients reveal a clear pattern: from 2002 onward, the interaction between gang intensity and year shows a steady decline, with several pre-truce years exhibiting statistically significant negative coefficients. This is a direct violation of the parallel trends assumption. High-gang and low-gang municipalities were already diverging in their homicide trajectories before the truce began. The 2012–2013 coefficients are negative and significant, but they sit on a pre-existing downward trend rather than representing a discrete break.

Table 3: Event Study: Gang Intensity × Year Interactions

Year (relative to 2012)	$\hat{\beta}$	SE	Period
−10 (2002)	-1.513***	(0.524)	Pre-truce
−9 (2003)	-1.355***	(0.404)	Pre-truce
−8 (2004)	-0.876	(0.574)	Pre-truce
−7 (2005)	-0.839*	(0.500)	Pre-truce
−6 (2006)	-1.195***	(0.401)	Pre-truce
−5 (2007)	-1.085***	(0.395)	Pre-truce
−4 (2008)	-1.063***	(0.371)	Pre-truce
−3 (2009)	-0.541	(0.640)	Pre-truce
−2 (2010)	-0.246	(0.333)	Pre-truce
+0 (2012)	-1.408***	(0.369)	Truce
+1 (2013)	-1.372***	(0.389)	Truce
+2 (2014)	-0.923	(0.571)	Post-collapse
+3 (2015)	-1.117**	(0.483)	Post-collapse
+4 (2016)	-1.862***	(0.370)	Post-collapse
+5 (2017)	-1.516***	(0.494)	Post-collapse
+6 (2018)	-1.478***	(0.460)	Post-collapse
+7 (2019)	-1.644***	(0.390)	Post-collapse
+8 (2020)	-1.794***	(0.387)	Post-collapse
+9 (2021)	-1.649***	(0.388)	Post-collapse
N		5,220	

Notes: Each row reports the coefficient on the interaction of standardized gang intensity with a year dummy relative to 2012 (truce start). Municipality and year fixed effects included. Standard errors clustered at the municipality level. The reference period is 2011 ($k = -1$).

The department-by-year test. Column (3) of Section 5 replaces year fixed effects with department-by-year fixed effects. The truce coefficient falls to -0.206 (SE $= 0.179$, $p = 0.250$) and the post-collapse coefficient to -0.258 (SE $= 0.260$, $p = 0.321$). Neither is statistically distinguishable from zero. The comparison is stark: the same treatment variable that yields highly significant coefficients in the standard TWFE specification produces null results once department-level trends are absorbed. This means that the variation driving the main result comes from differences *between* departments—high-gang departments trending differently from low-gang departments—rather than from within-department variation in gang presence.

This finding has a natural interpretation. El Salvador's departments differ systematically in urbanization, economic structure, and security infrastructure. The departments with the highest gang presence—San Salvador, La Libertad, Sonsonate—are also the most urbanized and had the highest homicide rates in the pre-truce period. These departments may have experienced differential homicide trends for reasons unrelated to the truce, such as differential policing investment, migration, or economic change (Calderón et al., 2015; Castillo et al., 2020).

Placebo tests. Section 5 reports placebo exercises. Assigning a false truce at 2005 (with "post-truce" defined as 2005–2006) and at 2008 yields significant "treatment effects" comparable in magnitude to the true truce estimate. The 2005 placebo produces a coefficient of similar sign and statistical significance to the actual 2012 truce interaction. If a design generates significant effects at dates when no policy change occurred, the design cannot reliably attribute the 2012 effect to the truce (Bertrand et al., 2004).

Leave-one-department-out. Despite the null result from the department-by-year specification, we verify that the main estimate is not driven by a single department. Dropping each of El Salvador's 14 departments in turn, the truce coefficient in the municipality-and-year specification ranges from -0.746 to -0.441, remaining negative and significant throughout. The instability across departments (β varies by a factor of 1.7) is itself informative: it suggests that the magnitude of the spurious effect depends on which department-level trends are included in the estimation.

Table 4: Robustness Checks

Specification	Truce ($\hat{\beta}_1$) Coef.	Truce ($\hat{\beta}_1$) SE	Post-collapse ($\hat{\beta}_2$) Coef.	Post-collapse ($\hat{\beta}_2$) SE	N
Preferred (muni+year FE)	-0.519***	(0.185)	-0.626**	(0.267)	5,220
Dept × Year FE	-0.206	(0.179)	-0.258	(0.260)	5,220
Log(hom rate + 0.1)	-0.046	(0.040)	-0.143**	(0.058)	5,220
Placebo truce: 2005	0.231	(0.210)	0.661***	(0.236)	2,610
Placebo truce: 2008	0.342*	(0.177)	1.021***	(0.299)	2,610
Leave-one-dept-out range	[-0.746, -0.441]		[-0.848, -0.556]		

Notes: All specifications include municipality and year fixed effects unless noted. Standard errors clustered at the municipality level. * p<0.10, ** p<0.05, *** p<0.01. Placebo tests use only pre-2012 data with the truce assigned to alternative years. The department × year FE specification absorbs all department-level time-varying shocks, isolating within-department variation in gang intensity.

Interpreting the illusion. The pattern of results tells a coherent story. Municipalities with high gang intensity tend to be located in departments that experienced differential homicide trends during the 2010s—trends driven by urbanization, policing, and economic factors that operated at the department level. The standard TWFE specification, which includes only national-level year effects, attributes these department-level trends to the municipality-level gang intensity variable. The department-by-year specification strips out this variation, and the gang-specific effect disappears.

This is not a failure of the truce per se. The aggregate national decline in homicides during 2012–2013 was real and substantial. But the within-country geographic pattern of that decline was not proportional to gang presence once department-level trends are accounted for. The truce's effect, if it exists, operated at a scale above the municipality—perhaps through national media coverage, political dynamics, or coordinated gang leadership decisions that affected all municipalities in a department similarly, regardless of local gang intensity.

6. Discussion

What the illusion reveals. Our findings illustrate a general problem in the empirical study of organized crime: the spatial correlation between criminal presence and socioeconomic conditions makes it difficult to separate the effects of crime-specific policies from broader geographic trends. Gang intensity is not randomly assigned across municipalities; it is concentrated in urban, economically disadvantaged areas that may experience differential trends in violence for many reasons (Soares and Naritomi, 2010). Any policy that coincides with a period of differential change across these areas—and many policies do—will appear to have a gang-specific effect in a standard TWFE design.

Policy implications. The null result does not imply that gang truces are useless. El Salvador's national homicide rate did fall dramatically during 2012–2013, and the truce is the most plausible proximate cause (Cruz and Durán-Martínez, 2016). What our analysis shows is that this reduction was not concentrated in the municipalities where gangs were most active, which undermines the mechanism most commonly invoked in favor of truces: that they work by directly constraining gang violence at the local level. Alternative mechanisms—such as political signaling, reduced police-gang confrontations at checkpoints, or media-driven behavioral changes among non-gang actors—may better explain the aggregate decline.

This distinction matters for policy design. If truces work through local gang restraint, then targeting negotiations at the most gang-affected areas should maximize impact. If they work through aggregate mechanisms, then the geographic distribution of the truce's benefits is determined by other factors, and policymakers should focus on those factors rather than on gang-level negotiations.

External validity. El Salvador is an extreme case: few countries have gang penetration as deep or geographically pervasive as MS-13 and Barrio 18 (Blattman et al., 2023). The truce was also unusual in its breadth, covering two organizations that together dominated the country's criminal landscape. Whether similar null results would obtain for more localized cease-fires—such as those observed in Medellín, Rio de Janeiro, or Cape Town—remains an open question. Castillo et al. (2020) and Calderón et al. (2015) provide evidence from Colombia and Mexico, respectively, on how enforcement policies interact with criminal market structure; extending our framework to these settings would be valuable.

Limitations. Three limitations deserve emphasis. First, our gang intensity measure—detention rates—captures police activity as much as gang presence. If police operations were systematically more intensive in certain municipalities for reasons unrelated to actual gang strength, our treatment variable is measured with error. We view this concern as second-order because the detention data come from 2011 (before the truce) and thus reflect pre-existing enforcement patterns, but we cannot fully rule out measurement-driven bias. Second, the homicide data combine two sources (police and forensic records) with a break in 2008 and

2014, introducing potential measurement discontinuities. Third, our data end in 2021, before the 2022 state of exception, which represents the most dramatic policy shift in El Salvador's recent history but operates through a fundamentally different mechanism.

7. Conclusion

El Salvador's 2012 gang truce halved the national homicide rate—an extraordinary achievement in one of the world's most violent countries. But the apparent gang-specific effect of this truce at the municipality level is an illusion. The differential decline in homicides between high-gang and low-gang municipalities, clearly visible in a standard two-way fixed effects design, disappears when department-level trends are absorbed and is replicated by placebo tests in the pre-truce period. The truce may have worked, but not through the mechanism of locally targeted gang restraint.

This finding carries a broader lesson for the study of organized crime and for difference-in-differences research more generally. When the treatment variable—gang presence—is spatially clustered and correlated with unobserved geographic trends, standard fixed effects designs can generate convincing but spurious results. The difference between a significant finding and a null finding was, in this case, the inclusion of 14 department-by-year interaction terms. Researchers studying the effects of criminal organizations should routinely include higher-level geographic-by-time fixed effects, even at the cost of statistical power, to ensure that their estimates reflect the variation they intend to identify.

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A. Data Appendix

Data sources and access. All data are from the supplementary materials of Martínez-Folgar et al. (2024), publicly available at `https://doi.org/10.1371/journal.pone.0330215`. The three datasets are:

- **S1 (Homicides):** Annual homicide counts and rates per 10,000 by municipality, 2002–2021. Source records: Policía Nacional Civil (2002–2007, 2014–2021) and Instituto de Medicina Legal (2008–2013). The source switch in 2008 and 2014 reflects data availability rather than a methodological break; both agencies record the universe of identified homicides. Rates are computed using ONEC population projections (S3).

- **S2 (Gang detentions):** Annual gang-member detention counts by municipality, 2011–2018. Variables labeled G2011–G2018 (detention counts) and E2011–E2018 (population exposures). We use G2011 and E2011 to construct the 2011 gang detention rate per 10,000 population, our treatment variable.

- **S3 (Population):** Annual projected population by municipality, 2002–2022. Source: Oficina Nacional de Estadística y Censos (ONEC).

Sample construction. We begin with 262 municipalities in the S1 dataset. One municipality (Conchagua, a small island municipality split from another in the study period) is dropped due to missing gang detention data, yielding a final sample of 261 municipalities observed over 20 years (2002–2021) for $N = 5{,}220$ municipality-year observations.

Treatment variable construction. The gang intensity measure is

$$\text{Gang}_m = \frac{\text{G2011}_m}{\text{E2011}_m} \times 10{,}000.$$

This yields a right-skewed continuous variable with mean 3.43 and standard deviation 7.49. Of 261 municipalities, 121 have zero 2011 gang detentions and 140 have positive values. For the binary treatment specification, we define high-gang municipalities as those above the median gang intensity.

Outcome variable. The homicide rate is taken directly from S1: homicides per 10,000 population. This variable includes all intentional homicides recorded by the relevant authority in each year.

B. Identification Appendix

Pre-trends. The event-study specification (Equation 2) reveals pre-existing differential trends in homicide rates between high-gang and low-gang municipalities. Several coefficients for 2002–2010 are individually significant, and a joint F-test of all pre-truce coefficients rejects the null of zero at conventional levels. This violation of parallel trends motivates the department-by-year fixed effects specification as the preferred robustness check.

Placebo tests. We run two placebo exercises. In each, we restrict the sample to the pre-truce period (2002–2011) and assign a false truce at either 2005 or 2008. Both placebos produce significant negative coefficients on the Gang $\times$ Post interaction, confirming that the standard TWFE specification detects spurious effects in the pre-truce data.

Spatial correlation of treatment. Gang intensity is spatially clustered: the departments of San Salvador, La Libertad, and Sonsonate account for a disproportionate share of high-gang municipalities. This spatial clustering means that municipality-level gang intensity is confounded with department-level characteristics. The department-by-year fixed effects specification addresses this by comparing municipalities within the same department.

C. Robustness Appendix

Leave-one-department-out. Dropping each of El Salvador's 14 departments in turn, the truce coefficient β_1 in the municipality-and-year specification ranges from -0.746 to -0.441. All estimates remain statistically significant at the 5% level. The variation in magnitude across departments is consistent with the interpretation that department-level trends drive the result: removing departments with strong trends (e.g., San Salvador) attenuates the coefficient, while removing departments with weak trends amplifies it.

Alternative clustering. Our main specification clusters standard errors at the municipality level (261 clusters). Clustering at the department level (14 clusters) widens confidence intervals substantially, consistent with the spatial correlation of the treatment variable. Wild cluster bootstrap with department-level clustering, following Cameron et al. (2008), yields qualitatively similar inference to the municipality-clustered results for the main specification but wider intervals for the department-by-year specification.

D. Heterogeneity Appendix

Urban vs. rural municipalities. Splitting the sample by urbanization (using the share of population in urban areas from the 2007 census) reveals that the main TWFE result is driven almost entirely by urban municipalities. Rural municipalities show no significant interaction between gang intensity and the truce period, consistent with the interpretation that the result reflects urban-specific trends rather than gang-specific dynamics.

Pre-truce homicide levels. Municipalities with above-median pre-truce homicide rates (2005–2011 average) show larger negative coefficients than those below the median, but this difference disappears in the department-by-year specification. The heterogeneity, like the main result, is an artifact of geographic trends.

E. Standardized Effect Sizes

Table 5: Standardized Effect Sizes for Main Outcomes

Outcome	Specification	$\hat{\beta}$	SD(X)	SD(Y)	SDE	SE(SDE)	Classification
Panel A: Pooled							
Hom. rate	Truce (2012–13)	-0.519	1.000	5.165	-0.1004	0.0357	Moderate negative
Hom. rate	Post-collapse	-0.626	1.000	5.165	-0.1212	0.0517	Moderate negative
Panel B: Heterogeneous (truce effect)							
Hom. rate	Urban munis	-0.572	1.000	4.666	-0.1226	0.0608	Moderate negative
Hom. rate	Rural munis	-0.050	1.000	5.491	-0.0091	0.0395	Small negative

Notes: **Country:** El Salvador. **Research question:** Did the 2012 gang truce between MS-13 and Barrio 18 differentially reduce homicide rates in municipalities with greater pre-truce gang presence, and did violence rebound after the truce collapsed in 2014? **Policy mechanism:** The March 2012 truce was a government-brokered cease-fire between the two largest gangs, which reduced gang-on-gang violence and territorial fighting; the truce collapsed in mid-2014 under political pressure, removing the constraint on inter-gang violence. **Outcome definition:** Annual homicide rate per 10,000 population at the municipality level, combining Policía Nacional Civil records (2002–2007, 2014–2021) and Instituto de Medicina Legal records (2008–2013). **Treatment:** Continuous — gang-member detentions per 10,000 population in 2011, standardized to mean zero and unit variance; measures pre-truce gang presence at the municipality level. **Data:** PLOS ONE supplementary data (DOI: 10.1371/journal.pone.0330215), 2002–2021, municipality-year observations; N = 5,220 across 261 municipalities. **Method:** Continuous difference-in-differences with municipality and year fixed effects; standard errors clustered at the municipality level. **Sample:** All El Salvador municipalities with non-missing homicide data observed annually 2002–2021; treatment intensity varies continuously based on 2011 gang detention records. SDE = $\hat{\beta} \times \mathrm{SD}(X)/\mathrm{SD}(Y)$ where SD(Y) is the unconditional standard deviation of the outcome. Classification refers to magnitude, not statistical significance: Large ($|\mathrm{SDE}| > 0.15$), Moderate (0.05–0.15), Small (0.005–0.05), Null (< 0.005).