Ceasefire Economics: The Causal Effects of Reduced Violence on Labor Markets

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Abstract

Violence and under-development frequently coexist, yet the labor-market payoff from making communities safer is still uncertain. Exploiting El Salvador's 2012 truce—an exogenous shock that lowered homicides by an estimated 60%—I show that safer streets translated into markedly stronger local labor markets: employment rose by roughly 8% and the number of firms by about 5% within a year. Evidence from geocoded firm–crime data indicates that these gains operate through a surge in local economic activity: higher sales, rising wages, and fewer business closures, with positive spillovers to neighboring areas. When violence resurged after the truce collapsed, firm exits accelerated and job growth stalled, underscoring the fragility of these improvements.

Keywords: Organized crime, labor markets, violence reduction, economic impact, business sustainability, economic growth

JEL codes: O1, O17, O54

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1 Introduction

What is the value of reduced violence for economic activity, particularly in contexts marked by chronic insecurity? Nowhere is this question more pressing than in Latin America and the Caribbean (LAC), where homicide rates exceed the global average by more than threefold and organized criminal groups account for roughly one-half of all killings (United Nations Office on Drugs and Crime (UNODC), 2023). Their pervasive reach—54% of households report the presence of criminal organisations in their neighbourhoods (Uribe, Lessing and Schouela, 2022)—suggests deep social and economic consequences. While much research explores how economic improvements can curb criminal activity[1], far less is understood about the reverse channel—how crime itself suppresses economic performance, particularly labour-market outcomes. This gap persists largely because detailed data are scarce and plausibly exogenous shocks that enable rigorous identification are rare.

This paper leverages a uniquely sharp and unexpected security shock: the 2012 Salvadoran gang truce. Secretly negotiated between incarcerated gang leaders and government officials, the ceasefire slashed homicide rates by an estimated 60% within a matter of weeks. The speed and magnitude of this decline stand in stark contrast to other security shocks that the literature has exploited—such as the escalation of Mexico's drug war, where violence rose over several years (2007–2011), or the more gradual drop in Colombian homicides from the mid-1990s through the 2010s. This sudden, localized improvement provides an ideal natural experiment to isolate how enhanced security causally affects local labour markets and firm dynamics.

Exploiting the truce's sharp geographic and temporal variation—comparing areas heavily exposed to the ceasefire with other units that were not, before and after its onset—I document sizable gains in local labour markets. A one–standard–deviation decline in homicides raises employment by 8.4% and the number of firms by 4.6% within the first year. To better understand the mechanisms behind these effects, I perform heterogeneous analyses across economic sectors and find that non-tradable sectors—those most reliant on local economic conditions—drive the bulk of the improvements. This suggests that increased labour demand results primarily from improved firm performance, further supported by evidence of fewer business closures, higher sales, and rising wages. Positive spillovers to nearby units indicate that reductions in violence also benefit adjoining

[1]For early analyses, see Jones (1932) and Simpson (1932), as well as more comprehensive discussions by Bushway and Reuter (2011) and Mustard (2010). Specific economic shocks studied include oil shocks (Raphael and Winter-Ebmer, 2001), changes in industrial composition via Bartik instruments (Gould, Weinberg and Mustard, 2002; Fougère, Kramarz and Pouget, 2009), exchange rate movements (Lin, 2008), international mineral prices (Axbard, Benshaul-Tolonen and Poulsen, 2019), trade liberalization (Dix-Carneiro, Soares and Ulyssea, 2016), mass layoffs and plant closures (Bennett and Ouazad, 2020; Pinotti, Britto and Sampaio, 2020), and improvements in legal market conditions (Pinotti, 2017).

areas economically.

The temporary nature of the truce exposes the fragility of these gains. When violence resurged in 2014, the growth in the number of businesses was entirely reversed, though employment levels did not fully revert to pre-truce conditions. However, employment stopped growing relative to the counterfactual and began following its pre-truce trajectory, suggesting that while labor markets retained some of their gains, the underlying growth potential was not sustained. This divergence highlights that maintaining a thriving business environment requires long-term, stable reductions in crime to ensure continued economic progress beyond short-term security improvements.

To identify the effects of crime reduction on economic activity, I employ a difference-in-differences (DiD) framework that leverages substantial heterogeneity in crime declines across areas during the truce. Treatment and control units are classified using a measure of abnormal homicide reductions relative to each area's historical crime fluctuations; units experiencing sharp and unexpected drops in violence are more plausibly affected by gang negotiations. This classification strategy provides a higher-powered approach, as the sudden and pronounced reductions likely dominate potential biases. Nonetheless, I explicitly test for possible endogeneity using pre-treatment variables and placebo analyses, consistently finding robust results that help mitigate concerns regarding the validity of this method.

These estimates are likely conservative. Because the truce's duration and credibility were uncertain, firms and workers may have postponed investments and hiring that would have occurred under a permanent decline in violence. In addition, the agreement chiefly curtailed homicides; other offences such as extortion showed no clear decline, and some evidence from a later, separate gang pact points to possible rises in extortion[2].

This paper contributes to the growing literature on how organized crime and violence shape economic outcomes, particularly firm performance and labor markets. Previous studies document that criminal activities and civil conflicts negatively impact economic indicators such as stock market returns (Abadie and Gardeazabal, 2003, 2019; Guidolin and La Ferrara, 2007), the performance of export-oriented firms (Ksoll, Macchiavello and Morjaria, 2010), firm survival probabilities (Camacho and Rodriguez, 2013; Collier and Hoeffler, 2004), market prices (Rozo, 2018), and—through mafia or cartel infiltration of local governments and private firms—curtail employment growth and undermine financial stability (Fenizia and Saggio, 2024; Mirenda, Mocetti and Rizzica, 2022).

[2]Brown et al. (2024) studies a 2016 non-aggression pact among the same gangs and documents higher reported extortion, drawing on payment records from a logistics company that distributes goods across El Salvador.

Closer to the question examined here, some studies have specifically analyzed how violence affects labor market participation, emphasizing labor supply responses. For instance, Utar (2024) and Velásquez (2020) document that increased violence discourages labor market participation due to heightened fears of victimization, notably among self-employed women who shift labor hours toward household activities, and blue-collar workers in labor-intensive manufacturing firms.

My paper departs from and expands upon this literature by exploiting a unique and sharply defined natural experiment—the sudden and unprecedented gang truce in El Salvador. Unlike previous contexts characterized by gradually escalating drug conflicts or incrementally effects of policy interventions, the Salvadoran truce was rapid and substantial, sustained over a meaningful period, and unexpected due to secret negotiations. This setting enables clear identification of economic agents' responses to sudden improvements in public safety.

In addition to leveraging this distinctive shock, my analysis also comprehensively covers all sectors of the economy, contrasting with prior research that typically focuses on subsets of firms with high productivity or international exposure (e.g., stock market-listed firms, exporters, or manufacturing industries). Indeed, a novel finding of this paper is that larger firms and tradable sectors—which dominate previous studies—are not the primary beneficiaries of crime reductions. Instead, smaller firms and those in non-tradable sectors, more dependent on local economic conditions, experience the most significant positive effects. Furthermore, contrary to earlier findings (Utar, 2024; Velásquez, 2020), evidence from El Salvador suggests labor demand increases significantly following crime reduction, raising employment without suppressing wages—indeed, wages appear to rise. This demand-side response aligns with the firm-level reactions described by Rozo (2018) and Camacho and Rodriguez (2013).

Critically, this study also provides the first robust documentation of positive geographic spillovers from improved security. While previous research has primarily identified negative spillovers, with crime displacement leading to increased violence in adjacent areas Draca, Machin and Witt (2011); Dell (2015), my results show that enhanced security conditions in one locality can significantly boost economic activity and employment in neighboring zones.

These findings highlight that improvements in public safety may have broader and more positive economic consequences than previously recognized. Specifically, sectors more reliant on local economic conditions and smaller firms experience notable welfare gains. Furthermore, my results suggest that increased employment does not come at the expense of lower wages; rather, crime reduction can simultaneously enhance wages and employment, generating additional positive spillovers in

neighboring areas. This underscores the potential of targeted anti-crime interventions as a powerful tool for regional economic development and welfare improvement.

The remainder of the paper is structured as follows. Section 2 provides background on violence in El Salvador and the 2012 gang truce. Section 3 describes the unique dataset constructed for this analysis, combining geocoded administrative and crime data. Section 4 outlines the identification strategy, including the difference-in-differences framework and robustness checks. Section 5 presents the main findings on the impacts of crime reduction on employment and firm activity. Section 6 explores the mechanisms driving these effects, focusing on reduced firm closures, improved sales, and sector-specific dynamics. Section 7 examines spillover effects, and Section 8 analyzes the economic reversal following the truce's collapse. Section 9 concludes.

2 Context

2.1 Violence in El Salvador

El Salvador was characterized by high levels of violence, significantly influenced by gang activities. The country's experience with violent crime, particularly homicides and extortion, was heavily linked to the activities of gangs such as MS-13 and Barrio 18. These gangs, formed by Latin American migrants in Los Angeles, grew in strength and number upon their return to El Salvador, exacerbated by weak institutions and lax law enforcement (Arana, 2005). By 2010, crime had become the country's most pressing issue, with 61% of Salvadorans identifying it as their top concern, surpassing economic issues (35%) (LAPOP Lab, 2010).

The arrival of deported gang members from the US in the mid-1990s led to the replication of behaviors and structures familiar in the contentious environments of Los Angeles. These individuals formed local chapters of MS-13 and Barrio 18, bringing with them gang symbols, language, and tattoos, and creating a collective identity through violence. This period saw a lack of government programs to address gang violence or reintegrate deported Salvadorans, leading to an escalation of the gang crisis.

Data from the National Police indicate that at least one-third of homicides are attributable to gang-related activities. The economic ramifications of this violence are substantial, with estimates suggesting that it accounts for between 6.5% to 16% of the country's Gross Domestic Product (GDP) (Jaitman et al., 2017; Peñate et al., 2016). Crime has also been a persistent barrier to investment, with 47% of businesses identifying it as the most critical factor affecting their investment decisions

in 2010 (FUSADES, 2010–2020). Studies by Melnikov, Schmidt-Padilla and Sviatschi (2020); Kalsi (2018); Castro et al. (2025); Castro and Kotti (2022); Castro et al. (2019) reveal the profound impact of gang presence on a wide spectrum of socio-economic factors within these neighborhoods. Notably, households in these areas face significant challenges concerning income, educational opportunities, housing quality, and electoral participation, all of which are negatively influenced by the pervasive control exerted by gangs.

2.2 Truce Negotiations

In 2012, a pivotal change occurred when the Salvadoran government facilitated a truce between MS-13 and Barrio 18. This truce, mediated with the support of the Catholic Church, led to a significant drop in homicides. However, the government's role in these negotiations remained controversial, with official denials of direct involvement despite apparent concessions to gang leaders. Moreover, not all gangs operating in El Salvador were part of the truce (Insight Crime, 2015).

This truce proved remarkably effective in curbing the nation's elevated homicide rates, achieving a 60% reduction in such incidents, a decline unparalleled in the country's recent history. As part of the agreement, the government conceded to enhance prison conditions for incarcerated gang leaders.

While the truce was successful in reducing homicides, the number of reported extortions remained nearly unchanged compared to pre-truce levels (see Figure B.4). The truce lasted approximately 24 months. During this period, the Minister of Security, who was identified as the facilitator of the truce, was dismissed, gang leaders were deprived of the agreed privileges, and a new security plan was initiated. This new strategy adopted a far more confrontational approach toward gangs, intensifying direct clashes between security forces and gang members. Consequently, crime rates surged beyond pre-truce levels, culminating in a homicide rate of 103 per 100,000 inhabitants in 2015, the highest recorded since the civil war.

3 Data

This study utilized three primary data sources:

1. **Social Security Database**: This dataset provides monthly records of all registered companies in the country. It includes detailed information on the primary activities of the companies, the

number of employees, geographical locations (geocoded using the provided textual addresses), and total payroll salaries.

2. **National Civil Police Records**: This source offers daily records of homicides across the country. Similar to the social security data, these records have also been geocoded.

3. **Annual Business Surveys from FUSADES**: These surveys, conducted by a leading Salvadoran think tank, cover approximately 580 firms from 2008 to 2015. They provide self-reported measures of business performance, including sales revenue.

To integrate these datasets, I employed neighborhood administrative divisions as the unit of analysis. For each geographic polygon, I identified the number of companies, total salaries, workers, and criminal activity. The social security and crime records were combined to construct a balanced monthly panel from 2010 to 2015, encompassing 1,581 geographic polygons throughout the country. The FUSADES survey data, available at an annual frequency, provide a complementary layer of firm-level information that can be linked to these polygons based on company locations.

One potential limitation of the data is that the Social Security Institute (ISSS) firm records only provide a single registered address per company. This means that for large firms with multiple branches or operational sites, I may only observe one location—potentially their headquarters—rather than their full geographic footprint. As a result, interpreting the effects on large businesses is more challenging, as their activities may span multiple regions beyond their registered location. To mitigate this concern, the main analyses focus on firms in the bottom 95% of the employment distribution, ensuring that the estimated effects are more closely tied to local economic conditions. However, I also present results for the top 5% of firms to assess.

3.1 Identifying Treated Units

Precisely measuring gang presence poses significant challenges, as direct indicators are often unreliable or unavailable. To address this, I rely on a proxy measure that captures unusually large reductions in homicide rates during the truce. The logic is that areas most influenced by the truce will display a marked and abnormal drop in violent crime, indicative of diminished gang activity.

For the main analysis, I classify polygons as "treated" if they fall within the top 50% of the distribution of homicide reductions ($Z_{i,T}$) during the truce period (T) (i.e., those experiencing the most abnormally large homicide reductions relative to their historical variation). Robustness checks using

alternative thresholds (such as the top 40%, 30%, 20%, or 10%) suggest consistent results: polygons experiencing more pronounced drops in homicides also exhibit stronger improvements in local labor market outcomes, regardless of the specific cutoff chosen. This pattern further supports the identification strategy, as increasingly restrictive thresholds should more precisely capture areas that experienced an exogenous shock due to the truce. Further methodological details on the construction of this measure, including the historical normalization of homicide fluctuations, are provided in Appendix A.1.

3.1.1 Addressing Potential Endogeneity

A key concern in interpreting the results is whether any decline in crime could mechanically lead to improvements in employment and firm activity. If this were the case, part of the estimated effect might not stem from the exogenous shock of the truce but rather from a more endogenous relationship between crime reductions and economic growth. This would raise the possibility that my findings do not capture the causal impact driven by the truce or that at least a portion of the observed effects could be attributed to broader economic dynamics rather than the truce itself.

To address this concern, I implement multiple placebo exercises, specifically, I fully reapply the classification methodology to hypothetical "truce" events occurring several months before the actual start date. For each placebo scenario (e.g., 3, 5, 7, or 9 months before the truce), I:

1. Compute the historical distributions of homicide changes and construct the $Z_{i,t}$ scores as described in Appendix A.1.

2. Rank the polygons based on these fictitious homicide reductions and classify them into treatment and control groups according to the same criteria used for the actual truce.

3. Estimate the corresponding economic outcomes using these counterfactual treatment and control classifications.

This procedure allows me to test whether the observed economic improvements could be an artifact of the classification method or driven by underlying endogenous variation in crime and economic activity, rather than by the truce itself. If the methodology were inherently flawed or simply reflecting persistent endogenous relationships, I would observe similar labor market improvements arising from these placebo treatments. Instead, the results show no significant effects when the truce is artificially "moved" to pre-truce periods. The lack of any placebo-driven effects strongly suggests

that the measured labor market improvements are indeed driven by the actual event of the truce rather than by pre-existing conditions, methodological quirks, or endogeneity in the crime-economic activity relationship.

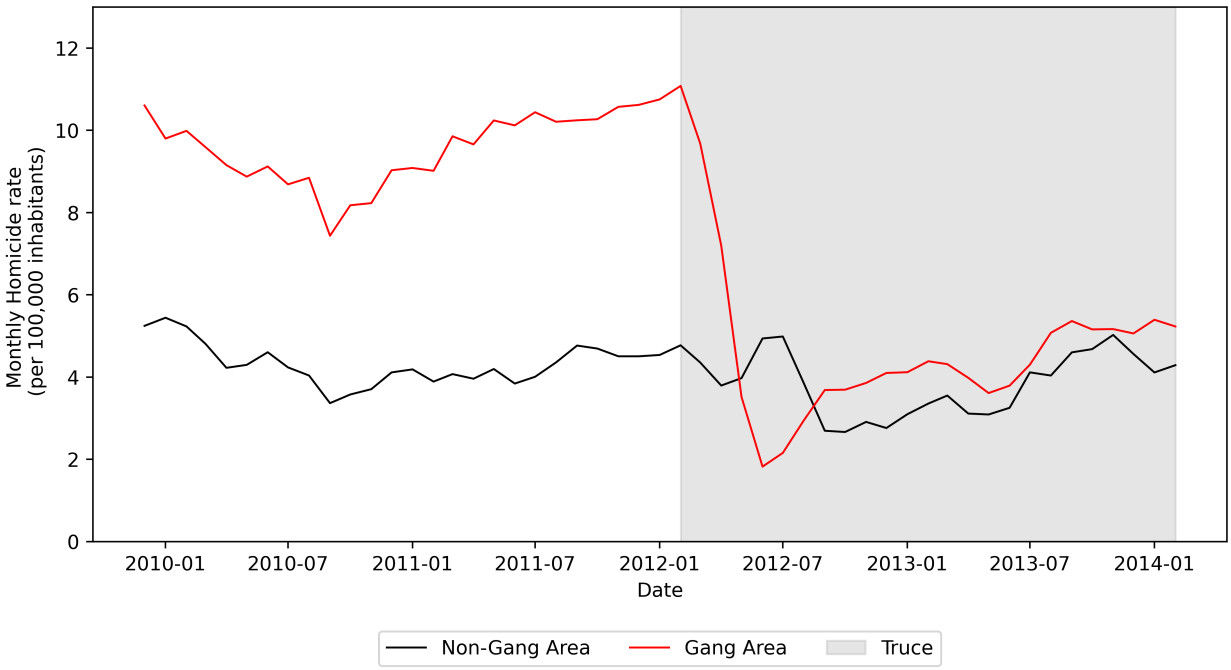
I implement an alternative approach to classify treatment areas based on pre-truce gang activity. Instead of defining treatment purely by observed crime reductions during the truce, I use historical data on gang-related homicides to identify areas where gangs were active in the years leading up to the truce. The rationale behind this classification is that these areas were more likely to have been directly affected by the gang negotiations and, consequently, to experience a reduction in violence. By using a measure independent of the actual crime decline during the truce, this approach strengthens the causal interpretation of the results.

This alternative measure, however, is not without limitations. Not all gang-related crimes are perfectly recorded or correctly classified, which introduces noise. Additionally, not all gangs participated in the truce, meaning that some areas continued to experience violence despite the overall decline in homicides. As a result, while this gang-based classification tends to identify polygons that indeed experienced substantial crime declines during the truce, it may fail to capture the full set of impacted areas. This imperfection likely leads to a more conservative estimate of the true effect, thus providing a lower bound on the truce's impact (see Figure B.9).

3.1.2 Characterizing Treated and Control Areas: Homicide Rates and Economic Conditions

On average, one year after the truce, the homicide rate in treated areas declined markedly. Prior to the truce, these polygons had substantially higher homicide rates than their control counterparts. Figure 1 presents the evolution of monthly homicide rates per 100,000 inhabitants from January 2010 to January 2014, contrasting treated areas with control areas. Initially, the gap in homicide rates stood at approximately 5.41 points, reflecting considerably more violence in gang areas. However, following the onset of the truce, homicide rates in treated areas plummeted by approximately 65%, bringing the post-truce gap down to a mere 0.3 points. In essence, the truce substantially narrowed the historically persistent violence differential between treated and control areas. Notably, this sharp decline in violence coincides with an increase in economic activity, as reflected in firm entry and employment growth (Figure B.1), suggesting a strong correlation between crime reduction and economic activity.

Figure 1: Homicide Rates in Treated and Control Areas



Note: This figure displays the monthly homicide rate per 100,000 inhabitants, computed using a moving average, for areas classified as gang-controlled (treated) and non-gang-controlled (control). The shaded gray region indicates the truce period.

Table 1 provides summary statistics that further distinguish treated and control areas. Consistent with the notion that gangs operate primarily in more densely populated urban and suburban environments, treated areas feature considerably higher levels of economic activity. For instance, the average number of firms per block in treated areas (22.22) exceeds that of control areas (8.54). Similarly, treated areas host a larger workforce (253 workers per block, compared to 105 in controls) and slightly higher average wages ($736 versus $556). These regions also exhibit greater firm turnover, with more frequent openings and closings per quarter.

Table 1: Summary Statistics

	Area without gangs		Area with gangs	
Variable	Mean	SD	Mean	SD
Panel A				
Number of firms per block	8.54	24.67	22.22	82.25
Number of firms per block (excl. top 5%)	8.11	22.87	20.91	76.13
Number of workers per block	104.71	310.31	253.33	750.98
Number of workers per block (excl. top 5%)	55.37	185.2	151.42	627.29
Average wages	556.48	557.16	736.21	551.79
Average wages (excl. top 5%)	500.32	477.5	658.1	447.36
Number of opened firms per quarter	0.26	0.86	0.62	2.66
Number of closed firms per quarter	0.17	0.6	0.43	1.67
Yearly average growth in number of workers (%)	0.51	17.91	1.84	14.65
Yearly average growth in number of workers (%) (excl. top 5%)	2.87	34.97	4.95	37.7
Yearly average growth in number of firms (%)	1.67	28.82	2.26	24.12
Yearly average growth in number of firms (%) (excl. top 5%)	3.08	34.93	5.13	36.21
Yearly average growth of wages (%)	0.27	17.98	1.85	16.67
Yearly average growth of wages (%) (excl. top 5%)	1.66	28.58	2.14	23.42
Panel B				
Homicides Rate (per 100k hab.) before truce	4.22	14.98	9.63	21.51
Homicides Rate (per 100k hab.) after truce	3.93	16.42	4.27	14.89
Number of Blocks (Primary Geographical Unit)	807.0	807.0	774.0	774.0

Notes: Panel A reports the descriptive statistics on business activity, while Panel B provides descriptive statistics on crime, drawing on homicide rates observed 24 months before and after the truce.

4 Identification Strategy

The primary identification strategy employs a standard two-way fixed effects (TWFE) difference-in-differences (DiD) framework. Under this approach, treatment status is defined as a binary indicator $D_i \in \{0, 1\}$, determined by the abnormal reduction in homicides during the truce, where $D_i = 1$ if $Z_{i,T} > M_T$ and 0 otherwise, where $Z_{i,T}$ denotes the measure of homicide reduction for unit i during the truce period (T), and M_T represents the median of $Z_{i,T}$.

To assess the plausibility of the parallel trends assumption required for causal interpretation, I

estimate an event-study specification:

$$y_{i,t} = \alpha_i + \gamma_t + \sum_{k=-K}^{K} \delta_k D_{i,t+k} + \varepsilon_{i,t},$$

where $y_{i,t}$ is the outcome for unit i at time t, α_i and γ_t are unit and time fixed effects, and $D_{i,t+k}$ are event-time indicators. By examining the coefficients $\hat{\delta}_k$ for periods prior to the treatment, I verify the absence of differential pre-treatment trends. The parameters are estimated by ordinary least squares (OLS) using the full panel dataset.

However, the binary treatment DiD may not fully exploit variation in the intensity of the homicide reduction. To exploit the continuous nature of the treatment, I extend the analysis by modeling the intensity of the homicide reduction as a continuous dose of treatment, following insights from Callaway, Goodman-Bacon and Sant'Anna (2024). Since homicide reductions vary continuously, I redefine the treatment measure D_i as a continuous variable. Specifically, let $D_i \in \mathbb{R}_+ \cup \{0\}$, defined as: $D_i = 0$ if $Z_{i,T} \leq M_T$ and $D_i = Z_{i,T} - M_T$ if $Z_{i,T} > M_T$.

In continuous-treatment DiD, certain biases can arise from TWFE estimation. To address these concerns, I proceed in two steps. First, using only untreated units ($D_i = 0$), I estimate $y_{i,t} = \alpha_i + \gamma_t + \varepsilon_{i,t}$ to obtain $\hat{\alpha}_i$ and $\hat{\gamma}_t$. I then define adjusted outcomes for treated units ($D_i > 0$), as $\Delta\tilde{y}_{i,t} = y_{i,t} - \hat{\alpha}_i - \hat{\gamma}_t$.

Next, I model $\mathbb{E}[\Delta\tilde{y}_{i,t} \mid D_i = d] = \Delta y_t(d)$ using a parametric dose-response function. For instance, a linear form $\Delta y_t(d) = \beta_1 d$ or a quadratic form $\Delta y_t(d) = \beta_1 d + \beta_2 d^2$ is estimated by OLS. The Average Causal Response (ACR) then follows directly from the estimated coefficients. In the linear case, the marginal effect is $\hat{\beta}_1$, while for the quadratic specification it is $\hat{\beta}_1 + 2\hat{\beta}_2 d$. Averaging these marginal effects over treated units yields:

$$\widehat{ACR} = \frac{1}{N_{D>0}} \sum_{i:D_i>0} (\hat{\beta}_1 + 2\hat{\beta}_2 D_i),$$

the empirical counterpart to

$$ACR = \mathbb{E}\left[\left. \frac{\partial \mathbb{E}[\Delta Y \mid D = d]}{\partial d} \right|_{d=D}, D > 0 \right].$$

Under the strong parallel trends assumption, the ACR can be interpreted as a causal parameter analogous to a dose-response function. In sum, this continuous-treatment DiD framework allows me to move beyond a binary notion of treatment. While it does not yield a conventional Average Treat-

ment Effect (ATE) or Average Treatment on the Treated (ATT), it provides policy-relevant insights into how incremental reductions in violence translate into improvements in labor market outcomes. This richer characterization hinges on the strong parallel trends assumption, which, although more challenging to verify in a continuous setting, is supported by evidence that splitting the treatment into discrete intensity levels maintains the absence of pre-trends.

4.1 Additional Specifications and Robustness Checks

I implement two alternative identification strategies to assess the robustness of the results (detailed derivations and discussions are presented in the Appendix B.1).

First, I consider a gang-related treatment specification, where areas are classified as treated if they experienced gang-related homicides before the truce. This specification incorporates unit-specific linear time trends based on initial economic conditions. Following Borusyak, Jaravel and Spiess (2024), I estimate all fixed effects and trends using only untreated periods and then impute counterfactual outcomes for treated periods. The resulting residuals yield estimates of the treatment effect that are free from conflation with unit-level trends.

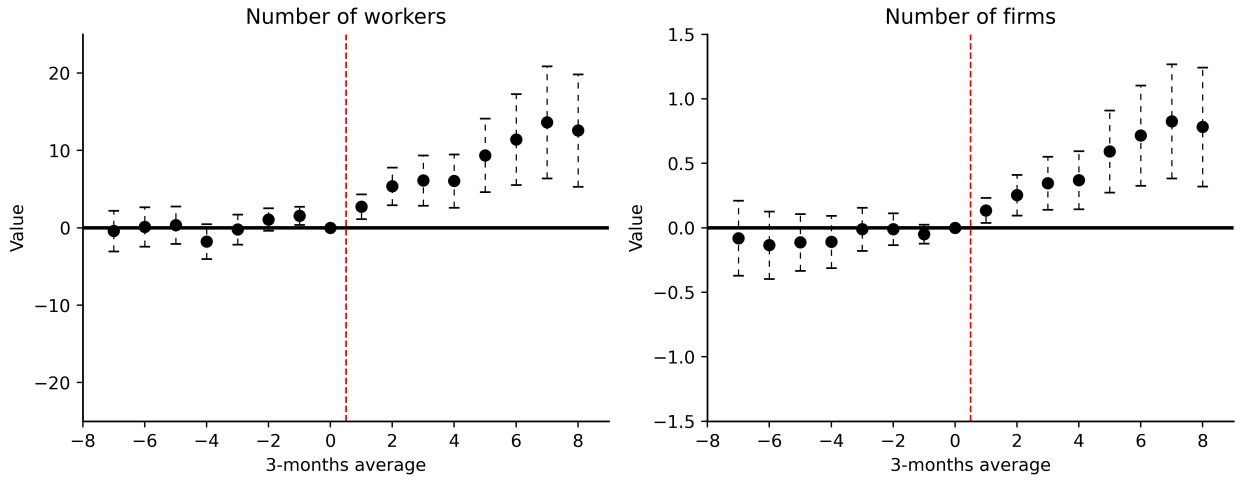
Second, I conduct placebo analyses by reassigning the treatment period to hypothetical truce events occurring 3, 5, 7, and 9 months prior to the actual truce. This approach tests whether the observed effects on economic activity could arise from the method used to classify treated and control areas rather than the truce itself. The analysis combines data from these placebo periods and the actual truce, creating a categorical variable s to indicate the timing of treatment, where $s = 1$ corresponds to the actual truce. Using a triple difference-in-differences model with unit and time fixed effects fully interacted with s, I estimate the outcomes. The model distinguishes between the average effect of being classified as treated and the additional impact when treatment and control classifications coincide with the truce period. Event studies further validate these findings by examining trends before and after the placebo and actual truce periods. The results show no significant effects during placebo periods, confirming that the observed economic improvements are attributable to the truce.

5 Empirical Findings

5.1 Baseline Treatment Effects

I initially examine the impact of the truce on companies within the bottom 95%. Figure 2 depicts the truce's effects on both the number of workers and the number of companies in areas that experienced a decrease in crime. The x-axis aggregates data into three-month intervals, covering a span of 24 months both before and after the truce. In the event study, it is not possible to discern pre-existing trends in these outputs and a notable increase is observed following the truce between gangs.

Figure 2: Effect of the truce on the number of workers and companies, excluding top 5% companies



Notes: This Event Study focuses exclusively on the treatment group selected during the truce period. The X-axis consolidates data into three-month intervals, depicting trends for 24 months both before and after the truce to illustrate longitudinal effects.

The corresponding DiD estimates are presented in Table A.1. For smaller and medium-sized businesses (bottom 95% of firms by size), the truce leads to a statistically significant rise of approximately 8.313 workers per block, representing a 5.49% increase relative to the mean, and an increase of about 0.57 firms per block, translating into a 2.54% uptick. By contrast, the top 5% of firms exhibit no statistically significant response; the estimates are close to zero, indicating that these large enterprises do not substantially alter their employment or establishment numbers in response to improved security conditions. This lack of impact for top firms is also evident in the corresponding

event-study analysis (Figure A.1), which shows no discernible pre-trends or post-truce changes in this subset. When aggregating all firms together, the overall effect remains positive and significant, with an estimated increase of around 10.9 workers (4.3%) and 0.56 firms (2.4%), suggesting that the bulk of the economic response to reduced crime stems from small and medium-sized enterprises.

These results are robust to alternative sample splits. In the Online Appendix, I report estimates using different cutoffs for defining treated and control units (e.g., top 40% vs. bottom 40%, top 30% vs. bottom 30%, and so forth). As the sample becomes more selective—focusing on polygons with increasingly extreme crime reductions—the effects grow stronger. For instance, comparing only the top 20% vs. bottom 20% leads to an even larger increase in employment of about 6.4% and a 3.1% rise in the number of firms (see Appendix Table B.1). The corresponding event-study figures confirm no apparent pre-trends, with the divergence emerging only after the truce is implemented (see Appendix Figure B.6).

These results focus exclusively on formal sector firms, as the dataset does not include informal business activity. While this paper cannot directly address whether the informal sector experienced similar or divergent effects, supplementary evidence suggests that overall labor market participation did increase. Using World Development Indicators (WDI) data from the World Bank, which estimates employment rates based on nationally representative household surveys (without distinguishing between formal and informal work), labor force participation rose by approximately 1.5 percentage points (a 2.5% increase) at the national level. This increase coincides with the implementation of the truce and dissipates following its termination (Figure B.5).

5.2 Robustness Check: Placebo Assignments in Pre-Truce Periods

I redefine the treatment and control groups as if the truce had occurred 3, 5, 7, or 9 months prior to its actual start date, and then reapply the entire selection procedure and estimation strategy to these counterfactual periods. As detailed in the Online Appendix B.1, I implement a triple-differences design that simultaneously considers the actual truce period and these placebo windows. This approach allows me to isolate the portion of the estimated effect attributable to the actual timing of the truce from any pattern that would emerge simply by applying the selection methodology at an arbitrary point in time.

The results, reported in Table B.4, show no significant effects for any of the placebo assignments. The "Treatment (Placebo)" coefficients remain statistically indistinguishable from zero, indicating that no meaningful changes in employment or the number of firms occur when the truce is artificially

moved back in time. In contrast, the "Treatment x Truce" interaction consistently yields positive and significant estimates, confirming that the improvements are indeed tied to the actual truce period. The corresponding event-study analysis, illustrated in Figure B.8, similarly reveals no pre-trends or spurious effects under the placebo timings, reinforcing that the observed outcomes emerge only when the actual truce is in effect.

These placebo results support the conclusion that the main findings are not driven by endogenous selection or methodological artifacts. Instead, the positive labor market responses are associated with the truce's onset and its subsequent reduction in homicides.

5.3 Robustness Check: Gang-Related Homicide Treatment Classification

To provide further evidence supporting the effects of the truce, I consider an alternative classification: polygons are deemed treated if they recorded gang-related homicides before the truce began. This approach aims to capture a exogenous source of treatment intensity, as high pre-truce gang activity experiences larger subsequent declines in violence once the truce takes effect.

Nonetheless, some control areas also experience substantial homicide reductions due to the possibility that crimes were not officially classified as gang-related or the fact that not all gangs participated in the truce. Despite this issue, the results remain robust: the gang-related classification yields significant improvements in both employment and firm counts. Specifically, employment gains are 3.71%, and firm counts increase by 3.02%, as reported in Table B.3. This robustness underscores the validity of the main findings and confirms the economic significance of reduced violence.

5.4 Continuous Treatment Intensity and Average Causal Response (ACR)

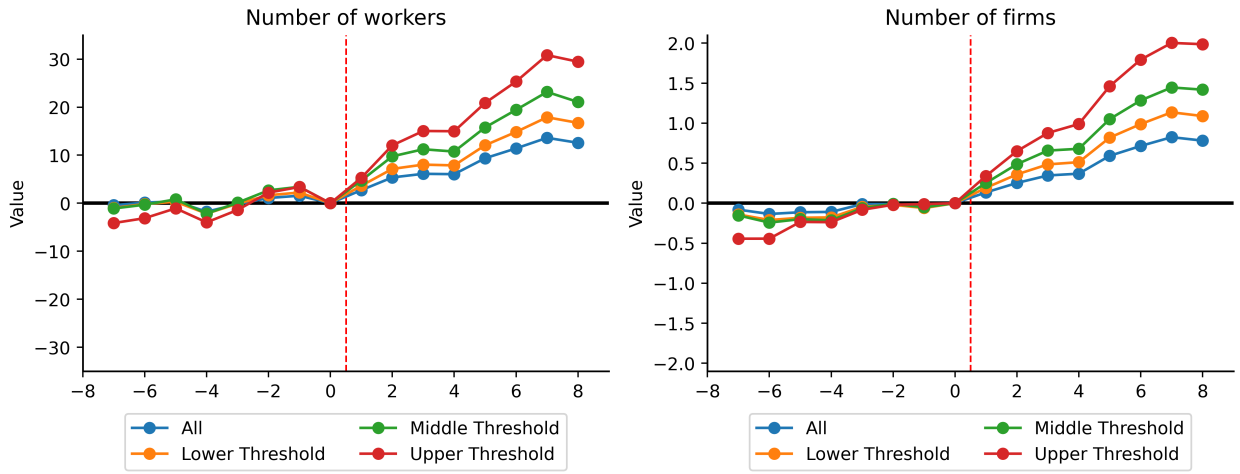
While the binary treatment and control classification provides insights into overall effects, it may obscure the nuanced relationship between the magnitude of homicide reduction and subsequent economic outcomes. To capture this relationship, I treat the reduction in crime as a continuous measure of treatment intensity and estimate the Average Causal Response (ACR). By examining how increments in the "dose" of crime reduction translate into changes in employment and firm counts, this approach can yield a more policy-relevant parameter, especially under the strong parallel trends assumption.

Verifying strong parallel trends is more challenging in a continuous-treatment setting. To address this, I sort polygons by their homicide reduction intensity and classify them into three groups.

These groups are defined according to their position in the distribution of homicide reductions: The **High Threshold** corresponds to polygons with homicide reductions at or above the 75th percentile, representing the top 25% of areas with the largest crime reductions. The **Intermediate Threshold** includes polygons at or above the 50th percentile, capturing areas in the top half of the distribution. Finally, the **Low Threshold** covers polygons at or above the 25th percentile, which encompasses approximately the top 75% of crime reductions.

By estimating event-study specifications separately for each of these threshold-defined groups, I can test whether the parallel trends assumption holds across varying intensities of treatment. The corresponding event-study graphs (see Figure 3) reveal no evidence of differential pre-trends for any group. Furthermore, when modeling the continuous treatment linearly and testing for parallel trends directly (see Appendix Figure A.2), the results confirm that the linear specification satisfies the required assumptions. These findings strengthen the validity of interpreting the ACR estimates as capturing a causal dose-response relationship, driven by the varying intensity of crime reductions.

Figure 3: Effect of the truce by Degrees of Intensity in Declines, excluding top 5% companies



Notes: The X-axis consolidates data into three-month intervals, depicting trends for 24 months both before and after the truce to illustrate longitudinal effects.

Table 2 reports the estimated ACR for employment and the number of firms, measured one and two years after the truce. Both linear and quadratic specifications produce broadly similar results. A one-standard-deviation reduction in homicide rates increases the number of workers by about 12.7 to 12.9 (8.4%–8.5%) after one year and roughly 26.1 to 26.5 (17.2%–17.5%) after two years.

For the number of firms, the corresponding increases range from approximately 1.0 to 1.03 (4.6%) after one year and about 1.9 to 1.95 (8.7%–8.8%) after two years. These consistent and substantial effects suggest that intensifying the reduction in violent crime delivers meaningful economic gains, with the impact growing more pronounced over time; however, due to the limited duration of the truce, identifying long-term effects in this context remains challenging.

In short, the ACR framework offers a richer perspective on the economic consequences of improved security. Under strong parallel trends, these estimates indicate that even incremental reductions in crime—on the order of a standard deviation—can significantly boost local employment and business activity within one to two years of the truce.

Table 2: Average Causal Response

Dependent	**Model**	**One Year**		**Two Years**	
		Coef/SE	Per. Change	Coef/SE	Per. Change
Workers	Lineal	12.72***	(8.40%)	26.11***	(17.24%)
		(3.71)		(7.16)	
	Quadratic	12.90**	(8.52%)	26.46**	(17.48%)
		(6.49)		(12.50)	
Firms	Lineal	1.01***	(4.55%)	1.93***	(8.68%)
		(0.26)		(0.47)	
	Quadratic	1.03**	(4.61%)	1.95**	(8.78%)
		(0.46)		(0.81)	

Notes: This table reports the Average Causal Response (ACR) estimates, which capture the effect of a one-standard-deviation reduction in crime on employment (workers) and the number of firms. Results are presented for one year and two years after the truce. The models include both linear and quadratic specifications, with coefficients representing the marginal effect of crime reduction. Percentage changes (in parentheses) are calculated relative to the baseline mean. Standard errors are reported in parentheses.

6 Mechanisms: Linking Crime Reduction to Labor Demand

The preceding results indicate that reduced crime rates coincide with more robust labor market outcomes and business activity. To gain insight into the underlying processes driving these changes, I examine mechanisms that suggest crime reduction primarily stimulates economic growth by boosting consumer confidence and increasing labor demand. When local communities become safer, households feel more secure making purchases and engaging in economic transactions, lead-

ing to greater demand for goods and services—particularly in gang-affected areas. As businesses benefit from this increased demand, they expand operations and hire more workers, fueling labor market improvements. If this mechanism holds, I expect to observe several patterns: (i) better business conditions, reflected in fewer firm closures and higher sales; (ii) increased consumer confidence, leading to greater local spending; (iii) higher wages as firms compete for labor; and (iv) stronger effects in non-tradable sectors, such as retail and hospitality, which rely more heavily on local demand.

6.1 Reduced Firm Closures as Key Evidence of Improved Conditions

A more stable business environment should manifest in firms' long-term decisions, especially those related to market exit. One of the clearest signals of improved local conditions is a decline in business closures, as fewer enterprises opt to shut down when uncertainty diminishes and profitability prospects brighten. Table A.2 presents results for both firm closures and openings across varying intensity thresholds of crime reduction.

As the treatment intensity increases (from "Treatment" to "Upper Threshold"), the reduction in firm closures becomes more pronounced, reaching over a 13% decrease in the highest-intensity category. These declines in closures represent a substantial portion of the net increase in the number of firms—a key finding, given that new entries alone cannot fully explain the observed expansions in the business base. The data also suggest a modest increase in firm openings, but the dominant driver of growth stems from firms staying in the market longer. A potential concern is that the observed firm openings was driven by gang-affiliated individuals formalizing their businesses rather than genuine economic expansion. To assess this, I match registered firms with a national database of individuals with criminal records, including those arrested as of 2019. The results indicate that fewer than 20 businesses (approximately 0.07% of total firms) were linked to individuals with known gang affiliations. This negligible share suggests that the observed economic effects cannot be explained by gangs transitioning into the formal economy.

Such evidence is relatively rare in the literature, as information on firm closures and openings at this level of granularity is often difficult to obtain. Documenting these dynamics in response to improved security thus provides a unique and direct indicator that local economic conditions are stabilizing or improving.

6.2 Firm Performance: Sales Growth and Consumer Confidence Index

The evidence from the Dynamic Business Survey conducted by FUSADES suggests that sales growth in the designated polygons increased during the truce period, aligning with the reductions in crime observed in my main analysis. Employing my standard event-study specification, I find that the uptick in sales is concentrated only in the truce years. Once the truce ends, sales revert to their pre-truce levels, which is consistent with the notion that benefits may dissipate once violence resumed. The estimated magnitude of the sales increase during the truce corresponds to approximately an 18% improvement in total sales relative to the pre-truce baseline. As shown in the top-left panel of Figure A.3, the event-study estimates highlight this temporal pattern in sales.

Additional supportive evidence, albeit descriptive and more aggregate, emerges from the Economic Outlook Reports of FUSADES (2010–2020). During the truce, the consumer confidence index, produced by the think tank FUSADES, improved at the national level. This index is a composite measure capturing households' major purchase decisions, and future economic expectations, and the broader economic outlook over both short- and long-term horizons. These patterns are illustrated in the bottom-left panel of Figure A.3. Moreover, when surveyed about the main barriers to investment, firms consistently identified crime as a dominant concern prior to the truce—cited by approximately 40–50% of respondents. During the truce period, this concern fell to around 25%, with "uncertainty" supplanting crime as the primary worry, as depicted in the top-right panel of Figure A.3. After the truce ended, the crime concern indicator rebounded to its pre-truce levels, underscoring the transient nature of the improvement.

Interestingly, not all confidence measures moved in tandem. The business confidence index, which captures firms' current economic perceptions and their expectations, remained largely unchanged during the truce (bottom-right panel of Figure A.3). This discrepancy may reflect the inherent complexity of business sentiment. While firms acknowledged the reduction in crime-related disruptions, other structural uncertainties—about the truce's permanence or broader macroeconomic trajectories—may have tempered any substantial shifts in business confidence.

6.3 Labor Market Tightening: Wage Increases in Active Areas

Building on the evidence that firms not only survive longer but also potentially increase their sales under lower-crime conditions, I next examine wage dynamics. In particular, I focus on polygons with at least 10 employees to mitigate noise stemming from very small establishments. This sample

refinement allows for a clearer identification of wage increases, which appear most pronounced in areas experiencing substantial economic activity. Such a pattern is consistent with a demand-side mechanism: as crime abates and markets become more secure, firms bid up wages to attract and retain labor in tighter local labor markets.

Table A.3 presents the coefficient estimates and percentage changes in wage per capita across different subsets of firms. In the bottom 95%, my estimates indicate a statistically significant wage increase of 19.8 dollars, corresponding to a 2.69% increase. Although the coefficient for the top 5% sample is not statistically significant, combining all firms yields a significant overall wage gain of 18.87 dollars, or 2.87%.

These findings align with the notion that the truce boosted local labor demand sufficiently to outweigh any concurrent labor-supply shifts, thereby pushing wages upwards. The event-study patterns (see Figure A.4) corroborate this interpretation: average wages begin to rise after the onset of the truce, and the effect is particularly visible among the bottom 95% of the wage distribution.

In summary, the overall evidence supports the idea that a more stable business environment—characterized by falling crime—can tighten local labor markets. As a result, firms appear to raise wages to secure the workforce required to capitalize on improved security conditions and growing demand.

6.4 Sectoral Heterogeneity and Growth Patterns

Beyond the overall increases in employment and firm counts, disaggregating the results by tradable and non-tradable sectors sheds light on the mechanisms driving these expansions. Following the methodology of Knight and Johnson (1997), I classified four-digit CIIU industries as tradable if a substantial portion of their output is sold internationally or if domestic consumption is partly met by imports, I find that the truce did not generate statistically significant effects on tradable sectors in terms of new jobs or new firms (see Figure A.5). In contrast, non-tradable activities—which depend more heavily on local market conditions—show pronounced growth, suggesting that the surge in security primarily boosted local demand. This pattern is especially relevant given that many smaller enterprises operate in non-tradable sectors, underscoring their vulnerability to high-crime environments and their responsiveness to sudden improvements in security.

While **G** (*Commerce*) and **K** (*Real Estate, Renting, and Business Activities*) contribute the largest absolute increases in employment and firm counts, several traditionally smaller, locally oriented sectors exhibit notably high growth rates. For instance, **F** (*Construction*) experiences a 10.05% rise in employment relative to its pre-truce baseline, while **H** (*Hotels and Restaurants*) and **I** (*Transport*

and Storage) grow by 9.85% and 7.73%, respectively (see Figure B.10). These non-tradable sectors are typically indicators of local market vitality, reflecting enhanced consumer confidence, improved conditions for investments, and greater commercial exchange within newly secured areas. Taken together, the evidence reinforces the view that declining violence—in this case, triggered by the temporary truce—can spur immediate, localized gains in economic activity, particularly among smaller firms and service-oriented industries that rely on stable, secure environments.

7 Spillover Effects

A key concern when interpreting the previous results is the potential for *spillovers* between treated and untreated polygons. In principle, spillovers can take both *positive* and *negative* forms. On one hand, improvements in a treated neighborhood—such as reduced crime and heightened economic activity—can *boost* nearby localities via shared consumer demand, commuting flows, or enhanced commercial linkages. On the other hand, economic activity could *shift* away from untreated areas if investors and businesses abandon higher-crime neighborhoods in favor of safer ones, thereby creating negative spillovers.

To detect spillovers, I employ a continuous difference-in-differences framework in which *driving distance (measured in minutes)* to a treated polygon serves as the continuous treatment variable. Concretely, for polygons that are not treated, I measure their driving distance to the closest treated polygon. I then augment the event-study setup by interacting time fixed effects with the distance measure, effectively comparing how outcomes evolve in polygons closer to treated areas versus those farther away, both *before* and *after* the truce. In my setting, Figure A.6 reveals predominantly *positive* spillovers in the immediate vicinity of treated areas. Notably, the gains are largest when the adjacent treated polygon experiences a high drop in homicides, suggesting that stronger security improvements in one polygon have a more robust economic impact on its neighbors. As might be expected, better conditions in neighboring areas could improve economic conditions locally. To test whether other, more random changes might also lead to economic improvements, I conduct several placebo tests (3, 5, 7, and 9 months before the truce). As shown in Figure B.12, these placebos do not yield similar results—only during the truce does the security change translate into economic improvements. This suggests that the effect is likely driven by a more prolonged and exogenous security shift, while other potential changes are merely noise.

7.1 Constructing a New Counterfactual

To measure the truce's *pure* effect net of spillovers, I create a new control group matched on variables that potentially drive spillovers. Specifically, I focus on two factors: (i) the shortest distance to a treated neighborhood, (ii) the homicide rate reduction in that neighborhood. Since these variables are endogenously linked to homicide rates—for instance, a decline in homicides in one polygon may contribute to declines in nearby areas—my initial treatment definition (which relies solely on observed crime reductions during the truce) may unintentionally select polygons that are also geographically closer to other treated zones and were affected by the truce due to their connection with neighboring areas rather than directly by the truce itself. Indeed, this relationship becomes possible when comparing homicide rates between control groups and treatment areas, as shown in Figure B.11, which presents a scatter plot of homicide reduction against proximity to a treated group.

To address this, I rely on the alternative treatment measure based on pre-truce gang-related homicides as an exogenous proxy for predicted crime drops and then match the control polygons to the treated polygons on the two key spillover-related dimensions. I use nearest-neighbor matching with bias adjustment in a Euclidean space of these covariates. By design, this procedure yields a new control group exhibiting similar exposure to—and capacity for—potential positive or negative spillovers, as indicated by the improved balance in the "Matched" columns of Table B.5.

Table B.5 also shows that the treatment polygons are systematically closer to other treated polygons and tend to share higher homicide reductions in adjacent areas than do untreated polygons.

7.2 Revised Estimates and Magnitude of Spillovers.

With the matched control in place, I re-estimate the truce's effect via an event-study approach. Before the truce, treated and control polygons follow parallel paths. After the truce begins, the estimates remain positive but are *attenuated* relative to our baseline in Table B.3. Additionally, the estimates exhibit higher variance. Table 3 compares the original estimates (*With Spillovers*) to those based on the matched control (*Correcting Spillover*):

Even after accounting for spillovers, the truce maintains a positive and meaningful effect on local employment and firm creation. New estimates indicate that the spillover-adjusted effect amounts to $\sim 75.8\%$ of the original estimate for *Workers* and $\sim 55.6\%$ for *Number of Firms*. In other words, at least 25% of the employment effect and 45% of the firm-creation effect in my baseline analysis can

Table 3: Correcting for Spillovers: Treatment Effects on Workers and Firms

		Workers		Number of firms	
Category	**Description**	Coef/Se	Per. Change	Coef/Se	Per. Change
With Spillover	Treatment Effect	7.19	3.71	0.86	3.02
		(3.24)		(0.24)	
Correcting Spillover	Treatment Effect	5.45	2.59	0.48	1.62
		(4.30)		(0.36)	
Correction as per.	Correction/Spillover	75.81		55.61	

Notes: "With Spillovers" refers to estimates using the original, unmatched control group. "Correcting Spillover" uses the matched control. Standard errors are in parentheses. "Correction as %" shows the ratio of the revised estimate to the original, implying that the difference (15% and 39%, respectively) was attributable to spillovers.

be attributed to spillovers between treated polygons.

From a policy perspective, recognizing these spillovers underscores the potential regional gains that can arise from crime reduction in a subset of neighborhoods. Indeed, improvements in security and economic activity in one area may transmit benefits to adjoining locales through shared labor pools, stronger commercial linkages, or greater consumer mobility.

8 Aftermath of the Truce: Rising Crime and Economic Reversal

Shortly after the truce dissolved, homicide rates in both the treatment and control polygons began to climb, eventually surpassing their pre-truce levels in some treated areas. Figure B.13 indicates that 2015 became one of the most violent years in El Salvador's recent history, with more than 100 homicides per 100,000 inhabitants—implying that nearly 0.1% of the population was killed that year. This escalation partly reflected the government's *mano dura* (iron-fist) policy, which led to intensified clashes between law enforcement and gang members. Such confrontations likely contributed to an overshoot of violence beyond the pre-truce baseline, as stakeholders lost confidence in the sustainability of the security gains.

In tandem with rising crime, the economic indicators that had improved during the truce began to reverse. Figure A.8 shows that while the growth in employment levels decelerated, it remained above the pre-truce baseline for most other periods. Similar to the pre-truce period, employment began to follow the same trajectory as its counterfactual, suggesting that once violence resurged, labor market dynamics reverted to their previous patterns. By contrast, the total number of firms steadily declined, ultimately returning to its pre-truce levels by the end of 2015. Interestingly, both treatment and control polygons experienced comparable increases in crime after the ceasefire col-

lapsed, yet the treatment polygons exhibited a stronger negative correlation between rising violence and firm exits. One possible explanation is that these areas had previously benefited more from the truce—experiencing a substantial "peace dividend"—and were therefore more vulnerable to the shock of resurgent insecurity. This suggests that firms in treated areas may have adjusted their operations under the assumption of sustained improvements in security, making them more susceptible to instability when violence returned.

A clear causal link between the end of the truce and the subsequent dip in economic activity cannot be firmly established, due in part to potential reverse-causality concerns. Nevertheless, two interrelated observations emerge. First, the treatment polygons that experienced considerable improvement during the truce also show a more pronounced deterioration in the number of firms once crime re-escalated, consistent with the hypothesis that smaller or more vulnerable businesses—those which thrived under safer conditions—could not withstand the renewed threats. Second, employment appeared more resilient since it did not sink below the pre-truce levels overall.

9 Conclusion

This paper exploits the unique natural experiment provided by the 2012 Salvadoran gang truce to address a critical question: how does reduced violence influence economic growth, particularly for small and medium-sized enterprises? The truce, by significantly decreasing homicide rates, provides a rare opportunity to disentangle the causal effects of improved security on local labor markets and business conditions.

The results underscore the transformative potential of crime reduction. A one-standard-deviation drop in homicides leads to an 8.4% increase in employment and a 4.6% rise in the number of firms within the first year. These effects grow over time, with employment and firm counts increasing by 17.2% and 8.7%, respectively, two years after the truce. The mechanisms driving these outcomes appear closely tied to heightened labor demand. Improved consumer confidence, evidenced by increased sales and reduced firm closures, suggests a more favorable business environment during the truce. While firm openings also rise, the primary driver of net growth is the significant decline in closures. Sectoral analyses further reveal that gains are concentrated in locally dependent industries such as commerce, real estate, and hospitality, highlighting how reductions in crime stimulate economic activities sensitive to consumer confidence and perceptions of safety.

Interestingly, the analysis also uncovers positive spillover effects. Neighboring areas close to

treated polygons exhibit improved employment outcomes, underscoring the broader regional benefits of crime reduction. These findings have important implications for policymakers, as they suggest that targeted interventions in high-crime areas can generate economic benefits that extend beyond their immediate boundaries.

The study also reveals the fragility of these gains. When the truce collapsed, violence resurged, and many of the economic improvements reversed, disproportionately impacting small enterprises. This underscores the critical importance of sustainability in violence-reduction efforts. Without lasting security, the businesses most sensitive to improvements in safety are also the most vulnerable to renewed instability.

In conclusion, this study demonstrates the significant but conditional economic benefits of reducing violence. By providing robust evidence on how improved security fosters local labor demand and business growth, it highlights the critical role of sustainable policies in ensuring long-term economic resilience in high-crime contexts.

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A Appendix

A.1 Construction of the Homicide Reduction Index

To systematically classify treatment and control areas, I construct a standardized measure of abnormal homicide declines. Let $H_{p,t}$ denote the homicide rate in polygon p at month t. I first compute a short-run change in the homicide rate as follows:

$$\Delta H_{p,t} = \left(\frac{1}{36} \sum_{\tau=t-36}^{t-1} H_{p,\tau} \right) - \left(\frac{1}{3} \sum_{\tau=t}^{t+2} H_{p,\tau} \right).$$

This measure $\Delta H_{p,t}$ compares the mean homicide rate over the 36 months prior to t with the mean rate over the subsequent 3 months. During the truce, I expect some polygons to exhibit larger values of $\Delta H_{p,t}$, indicating a sharp reduction in homicide rates.

Next, I standardize each observed drop against the historical distribution of homicide changes within the same polygon. Define the polygon-specific historical mean and standard deviation of these changes (computed over a pre-truce period):

$$\mu_p = \mathbb{E}[\Delta H_{p,t'}], \quad \sigma_p = \sqrt{\mathbb{E}[(\Delta H_{p,t'} - \mu_p)^2]},$$

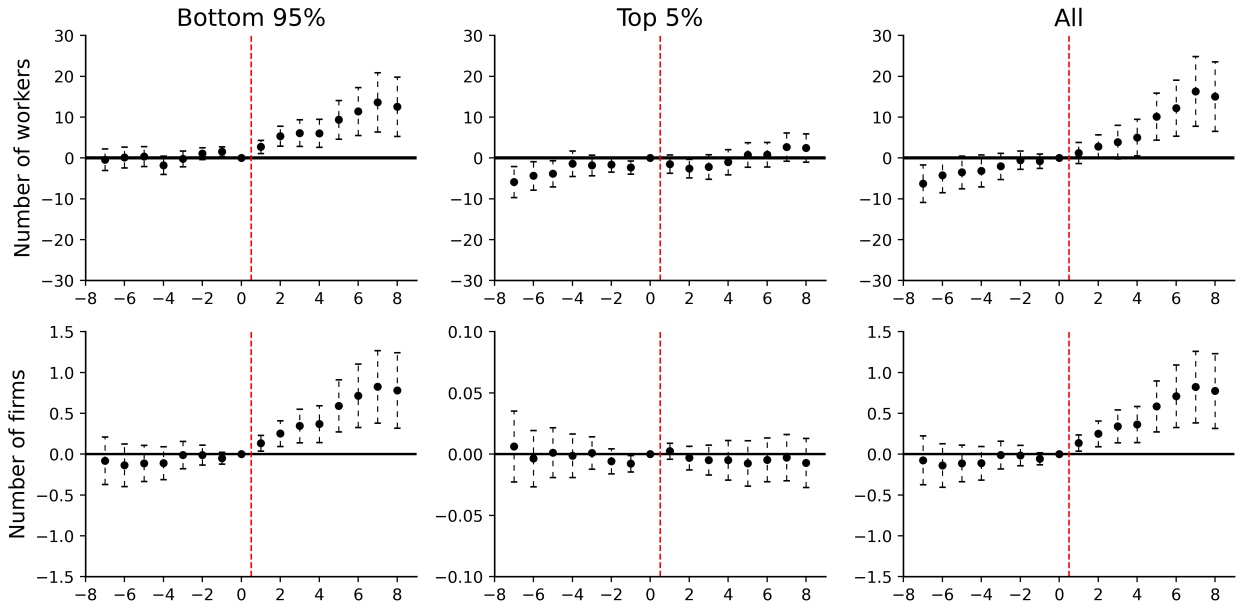
Using these statistics, I construct a standardized measure:

$$Z_{p,t} = \frac{\Delta H_{p,t} - \mu_p}{\sigma_p}.$$

This $Z_{p,t}$ score indicates how unusual the homicide reduction is for polygon p at time t, relative to its own historical variability. A more positive $Z_{p,t}$ means that the current drop in homicides is large relative to what would typically be expected in that polygon based on past fluctuations.

A.2 Figures and Tables

Figure A.1: Event-Study defining Treatment Based on Declines in Homicide Rates During the Truce Period



Notes: The X-axis consolidates data into three-month intervals, depicting trends for 24 months both before and after the truce to illustrate longitudinal effects.

Figure A.2: Event-Study Results for Average Causal Response

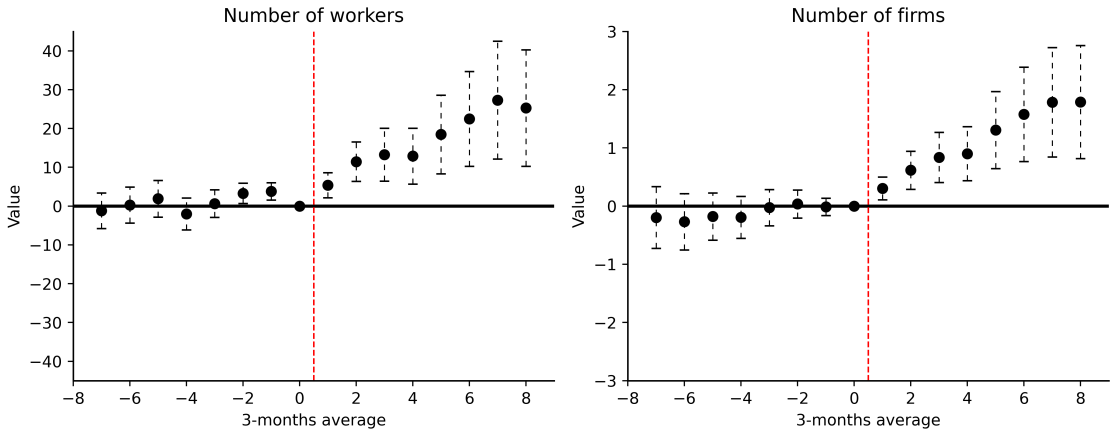


Table A.1: Effect of the truce: Defining Treatment Based on Declines in Homicide Rates During the Truce Period

Variables	**Workers**		**Number of firms**	
	Coef/SE	Per. Change	Coef/SE	Per. Change
		Small and medium firms: Bottom 95%		
Treatment	8.313***	(5.490%)	0.566***	(2.544%)
	(2.537)		(0.184)	
R-squared	0.9958		0.9986	
Obs.	25296		25296	
		Big firms: Top 5%		
Treatment	2.584	(2.536%)	-0.003	(-0.225%)
	(1.639)		(0.008)	
R-squared	0.9829		0.9993	
Obs.	25296		25296	
		All firms		
Treatment	10.896***	(4.301%)	0.563***	(2.387%)
	(3.140)		(0.185)	
R-squared	0.995		0.9988	
Obs.	25296		25296	

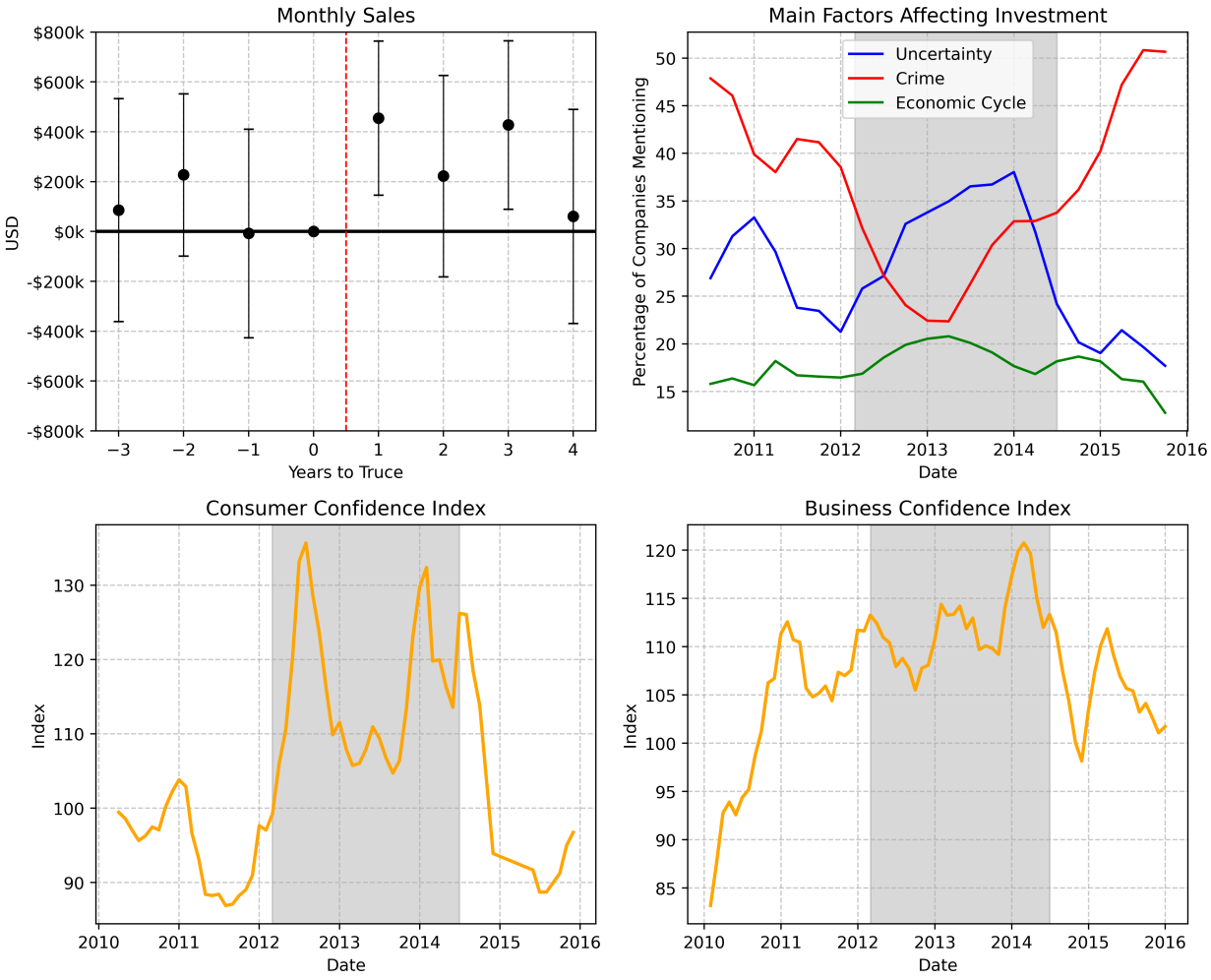
*Notes: This table presents regression results for the impact of the truce on workers, and number of firms, across small and medium firms (bottom 95%) and big firms (top 5%). Coefficients and their respective standard errors, shown in parentheses, indicate the estimated effects. The errors were clustered at the level of geographic units interacted with the period it's treatment condition was selected. The significance of coefficients is denoted by asterisks: *p<0.05, **p<0.01, ***p<0.001.*

Table A.2: Impact of Crime Reduction on Business Closures and Openings by Treatment Intensity

Variables	**Closing business**		**Opening business**	
	Coef/SE	Per. Change	Coef/SE	Per. Change
		Treatment		
Treatment	-0.041**	(-9.705%)	0.035*	(5.739%)
	(0.018)		(0.019)	
R-squared	0.7825		0.832	
Obs.	25296		25296	
		Lower Threshold		
Treatment	-0.059***	(-11.369%)	0.040*	(5.421%)
	(0.022)		(0.023)	
R-squared	0.7905		0.8408	
Obs.	22192		22192	
		Middle Threshold		
Treatment	-0.076**	(-12.306%)	0.056*	(6.393%)
	(0.030)		(0.030)	
R-squared	0.8056		0.8549	
Obs.	19104		19104	
		Upper Threshold		
Treatment	-0.108**	(-13.517%)	0.065	(6.672%)
	(0.051)		(0.048)	
R-squared	0.8324		0.8777	
Obs.	16016		16016	

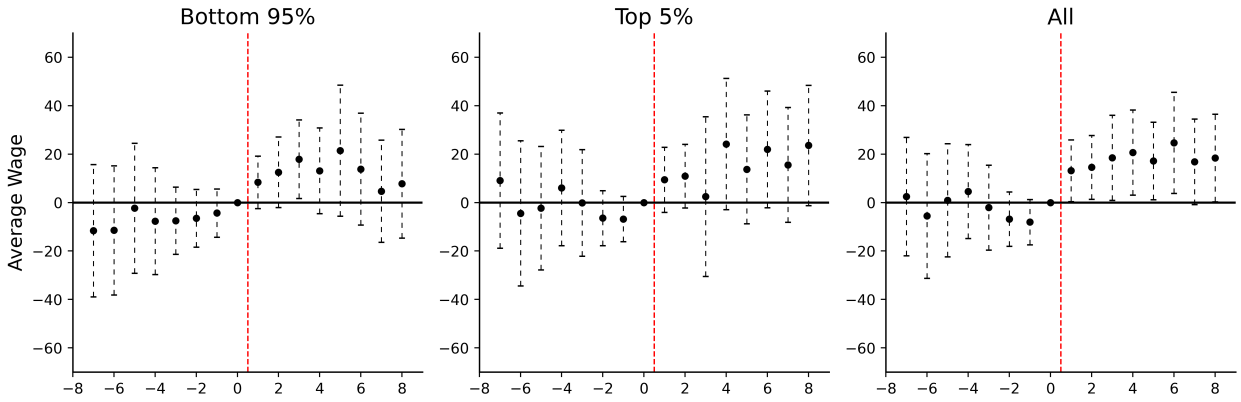
Notes: This table reports the effects of crime reduction on the business closures and openings, broken down by treatment intensity thresholds. The "Treatment" row presents the baseline results, while the lower, middle, and upper thresholds correspond to increasingly higher crime reduction intensity categories. The coefficients represent the estimated treatment effects, with standard errors in parentheses. A 1 percentage point decrease in closures (e.g., 13.5% at the upper threshold).

Figure A.3: Sales Growth, Obstacles to Investment, and Confidence Measures During the Truce



Notes: The top-left panel shows event-study estimates for sales growth in polygons affected by the truce. The top-right panel illustrates the share of firms citing crime as the main obstacle to investment (red line) versus uncertainty (blue line) and Economic Cycle (green line) over time. The bottom-left panel shows the consumer confidence index, interpolated linearly for the missing period from September 2014 to May 2015, and the bottom-right panel presents the business confidence index. All panels are based on data and descriptive statistics from Economic Outlook Reports of FUSADES (2010–2020).

Figure A.4: Event-Study Results for Wages



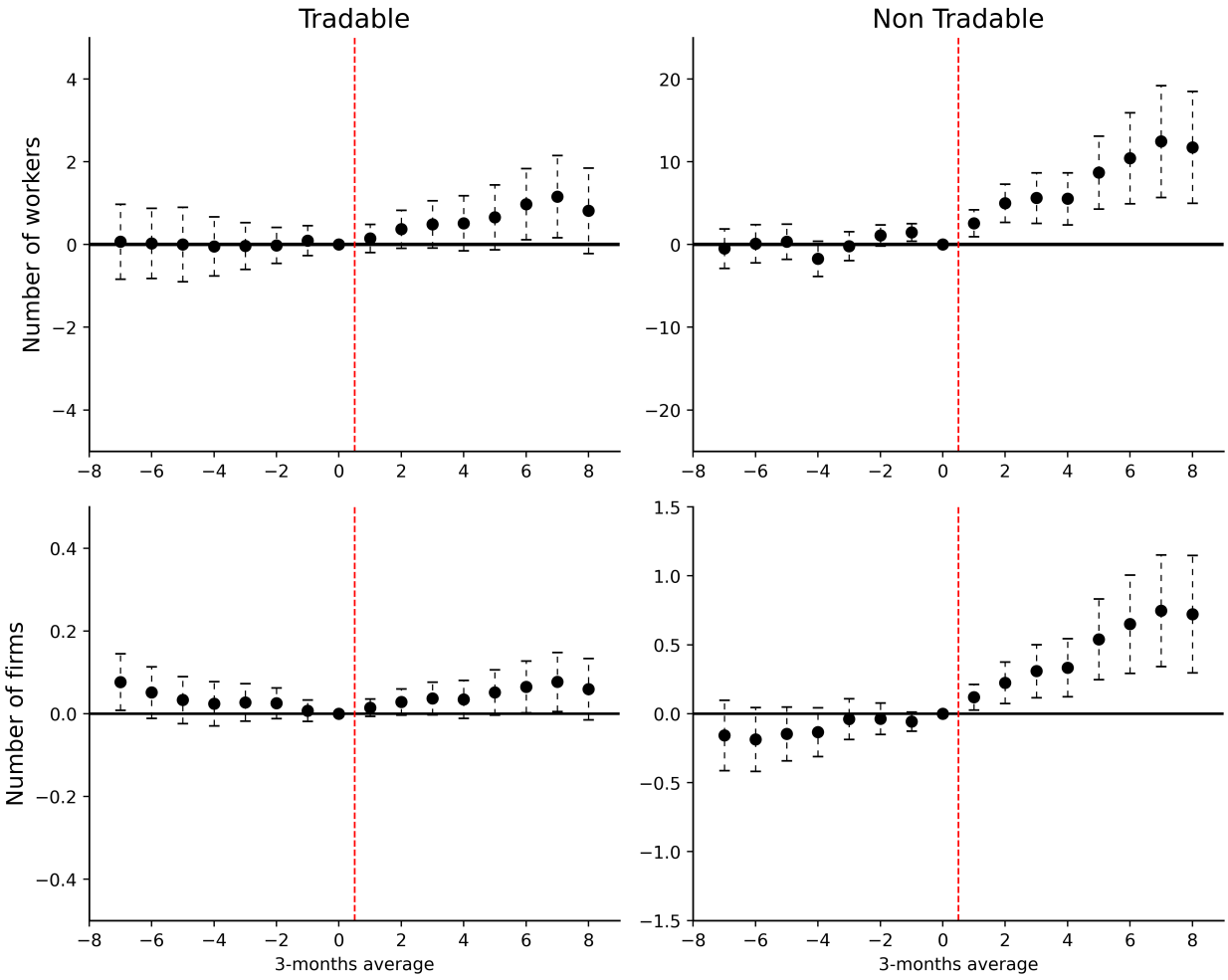
Notes: This Event Study focuses exclusively on the treatment group selected during the truce period. The X-axis consolidates data into three-month intervals, depicting trends for 24 months both before and after the truce to illustrate longitudinal effects. The sample is restricted to polygons that have at least 10 employees.

Table A.3: Effect of the truce on Wages: Defining Treatment Based on Declines in Homicide Rates During the Truce Period

Variables	**Bottom 95%**		**Top 5%**		**All firms**	
	Coef/SE	Per. Change	Coef/SE	Per. Change	Coef/SE	Per. Change
			Wage per capita			
Treatment	19.813**	(2.693%)	15.841	(4.243%)	18.876**	(2.871%)
	(8.843)		(12.249)		(9.393)	
R-squared	0.9547		0.9765		0.8779	
Obs.	11808		11808		11808	

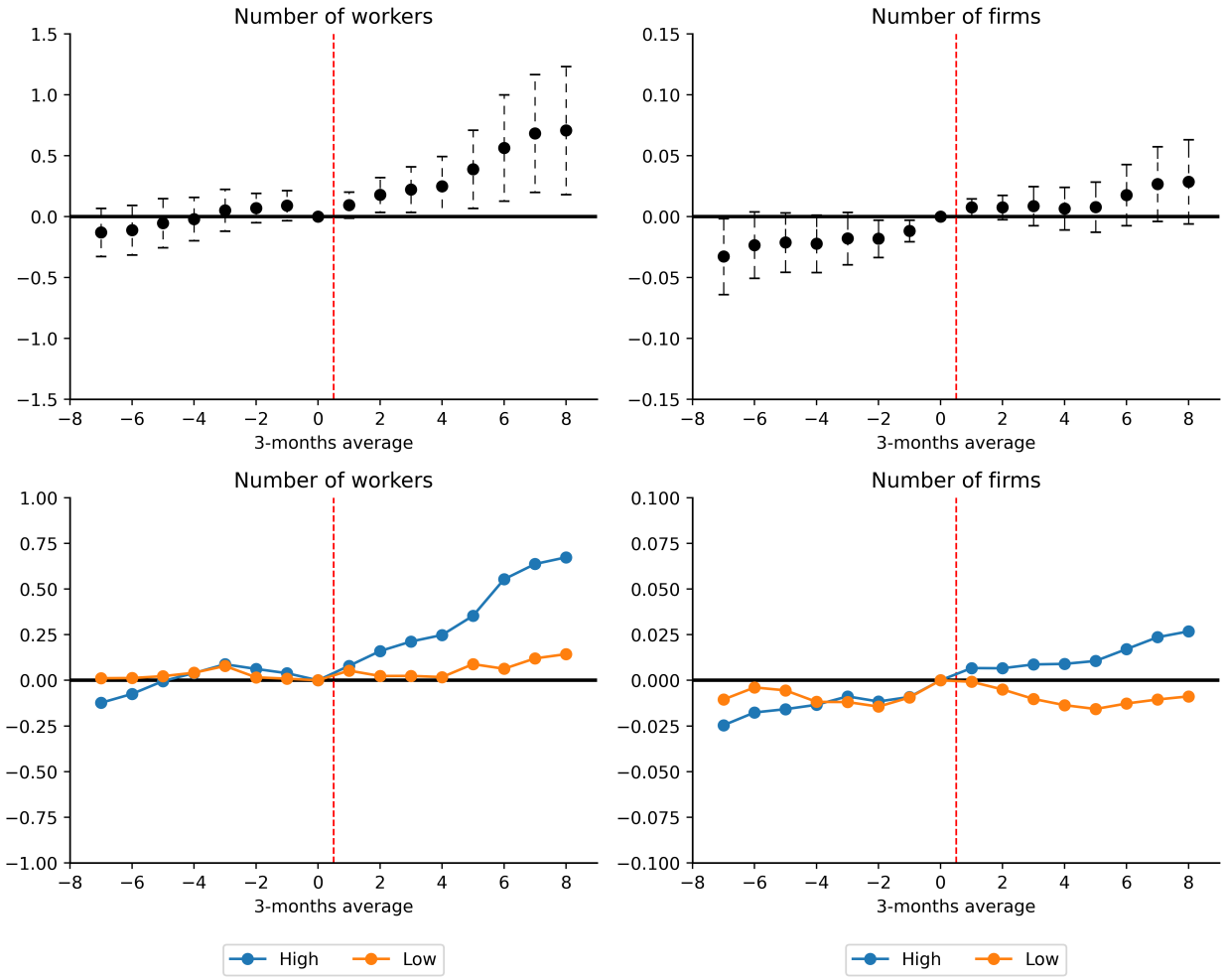
*Notes: The table reports estimated coefficients for the effect of the truce (Treatment) on per-capita wages. Percentage changes (Per. Change) in parentheses are based on the estimated coefficients relative to pre-truce baseline values. The sample is restricted to polygons that have at least 10 employees. The errors were clustered at the level of geographic units. The significance of coefficients is denoted by asterisks: *p<0.05, **p<0.01, ***p<0.001.*

Figure A.5: Effect of the truce: Tradable vs Non tradable sectors



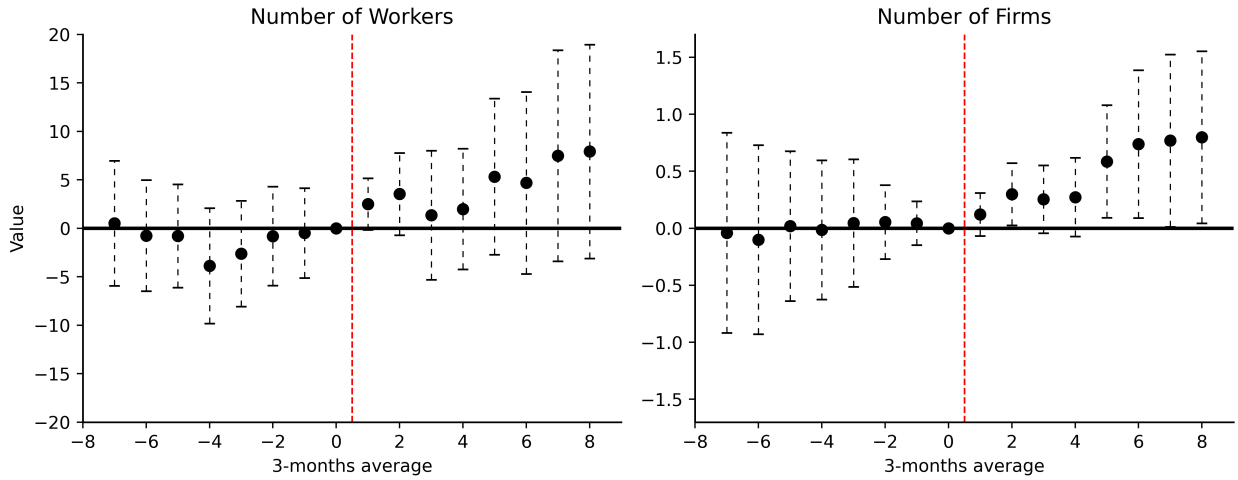
Notes: This event study estimates the impact of the gang truce on firms, distinguishing between tradable and non-tradable sectors based on Knight and Johnson (1997). Tradable sectors are those more exposed to international trade, while non-tradable sectors primarily serve local markets. The X-axis consolidates data into three-month intervals, depicting trends for 24 months both before and after the truce to illustrate longitudinal effects.

Figure A.6: Spillover Effects of Proximity to Treated Areas



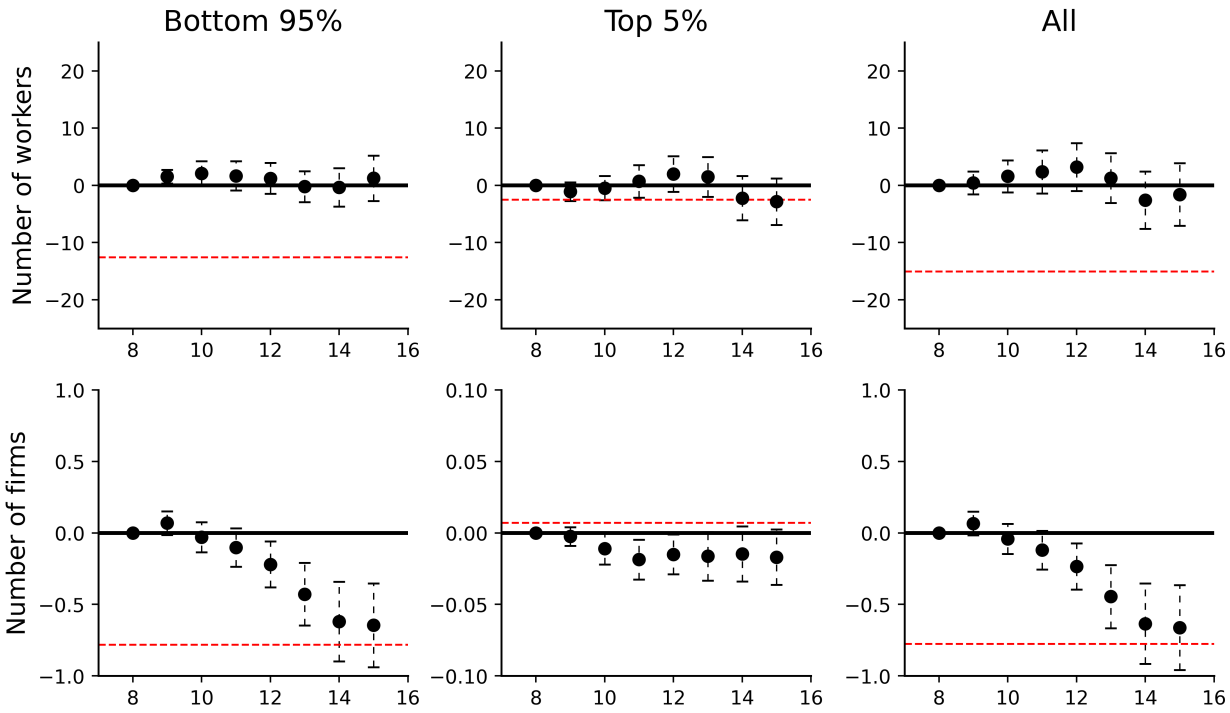
Notes: The first row shows the estimated effects of a one-minute reduction in driving distance to a treated polygon on the number of workers (left) and the number of firms (right) in untreated polygons. The second row decomposes these effects based on the magnitude of the homicide drop in the nearby treated polygon: High (above the mean) and Low (below the mean). The analysis employs a continuous difference-in-differences framework, treating driving distance (in minutes) as a continuous treatment variable. The red vertical line represents the timing of the treatment. Error bars reflect 95% confidence intervals. Results highlight that proximity to areas with significant reductions in homicides (High) generates stronger economic benefits compared to areas with smaller reductions (Low). Estimates are derived using models with robust controls to isolate the effects of proximity.

Figure A.7: Event Study with Matched Controls to Correct Spillover Effects



Notes: This event study analyzes the effects of truce without spillovers on the number of employees and firms in treated areas. A new control group was created using a matching approach that pairs untreated areas with treated ones based on their similarity in characteristics that could explain spillover effects. The covariates used for matching include the the minimum driving duration to treated areas, the intensity of reductions in violence in neighboring areas, and homicides rates reduction during the truce. Matching was conducted using a nearest-neighbor algorithm to identify the most similar untreated area for each treated one, with adjustments for potential biases in the covariates and robust standard errors to account for variability in the estimates.

Figure A.8: Trends in the Number of Workers and Firms Post-Truce



Notes: This figure illustrates the changes in the number of workers and firms within the treatment group, benchmarked against levels observed just before the truce (indicated by the dotted red line) and 24 months after the gang truce pact (shown on the horizontal axis).

B Online Appendix

B.1 Additional Specifications

B.1.1 Treatment Based on Gang-Related Homicides

To delineate the effects using an alternative treatment definition, I again employed the Two-Way Fixed Effects (TWFE) model, refining the specification by incorporating differentiated time trends for units with higher initial firm counts. Specifically, treatment status D_i is now defined by whether a polygon experienced gang-related homicides prior to the truce: $D_i = 1$ if polygon i had gang-related homicides before the truce, and $D_i = 0$ otherwise. The model specification is presented as follows:

$$Y_{it} = \alpha + \tau D_{it} + \gamma_i + \lambda_t + t\omega_i + \epsilon_{it} \tag{1}$$

In this equation, Y_{it} denotes the outcome of interest for each unit i at time t, D_{it} is the treatment indicator activated post-truce, γ_i and λ_t represent the unit and time fixed effects respectively, and $t\omega_i$ indicates a unique time trend associated with the number of firms at the beginning of the panel. The coefficient τ is crucial as it measures the average treatment effect on the treated units.

The model was estimated without using the treated units in the post-truce period. This methodology is inspired by Borusyak, Jaravel and Spiess (2024). To estimate this model, I first fit the fixed effects—$\hat{\alpha}$, $\hat{\gamma}_i$, $\hat{\lambda}_t$, and $\hat{\omega}_i$—using only observations from untreated periods. These fitted effects are then utilized to impute the potential outcomes for the treated units in the no-treatment scenario, thus allowing us to compute the estimated treatment effect for each treated observation using the residual from the real value with the potential estimated as follows:

$$\hat{\tau}_{it} = Y_{it} - \hat{\alpha} - \hat{\gamma}_i - \hat{\lambda}_t - t\hat{\omega}_i \tag{2}$$

Errors are determined using a block bootstrap method. By employing this method, I ensure that the estimation process discerns distinct trends without inadvertently confounding the treatment effect with these characteristics.

B.1.2 Placebo tests

The primary modification to the conventional method involves merging multiples data collections, where treatment units are identified based on experiencing an abnormal drop in homicide activity during the truce period, and placebo periods 3,5,7 and 9 months prior to the truce. These datasets are then combined, incorporating a categorical variable s, which indicates whether the assignment to treatment or control occurred during the truce. This variable interacts fully with the unit-fixed effects, time-fixed effects, and the treatment indicator.

The modified TWFE model, incorporating the creation of three datasets and the interaction with a dummy variable for the truce period, is specified as follows:

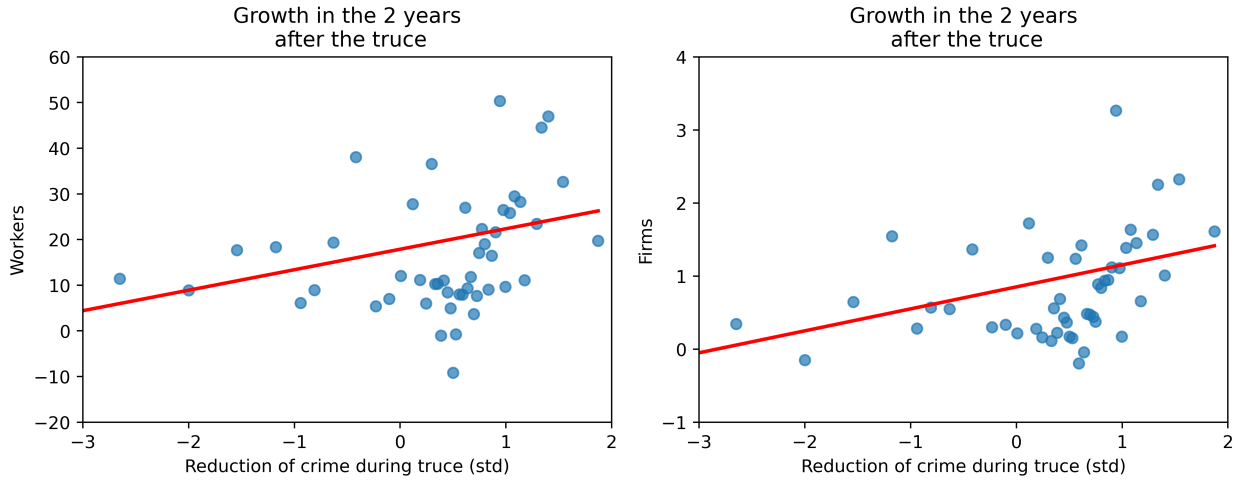
$$Y_{ist} = \alpha + \tau_0 D_{ist} + \tau_1 D_{ist} \mathbb{1}\{s \in Truce\} + \gamma_{is} + \lambda_{ts} + \epsilon_{ist} \tag{3}$$

where Y_{ist} represents the outcome of interest for unit i selected as control or treatment in period s at time t before the event s. D_{ist} is the treatment indicator, activated post-event s for units identified as treated, γ_{is} are the unit fixed effects (polygons) interacted s, λ_{ts} are the time fixed effects interacted s, and ϵ_{ist} is the idiosyncratic error term. The errors were clustered at the level of geographic units interacted with s. The coefficient τ_0 captures the average treatment effect regardless the period s when the treatment and control were identified. The coefficient τ_1 however, measures the additional effect when the treatment and control selections coincide with the truce period.

To further dissect the dynamics of the treatment effect, I implement an Event Study analysis. This approach examines the evolution of the impact pre- and post-treatment. The model is augmented with interaction terms between the treatment and time dummies.

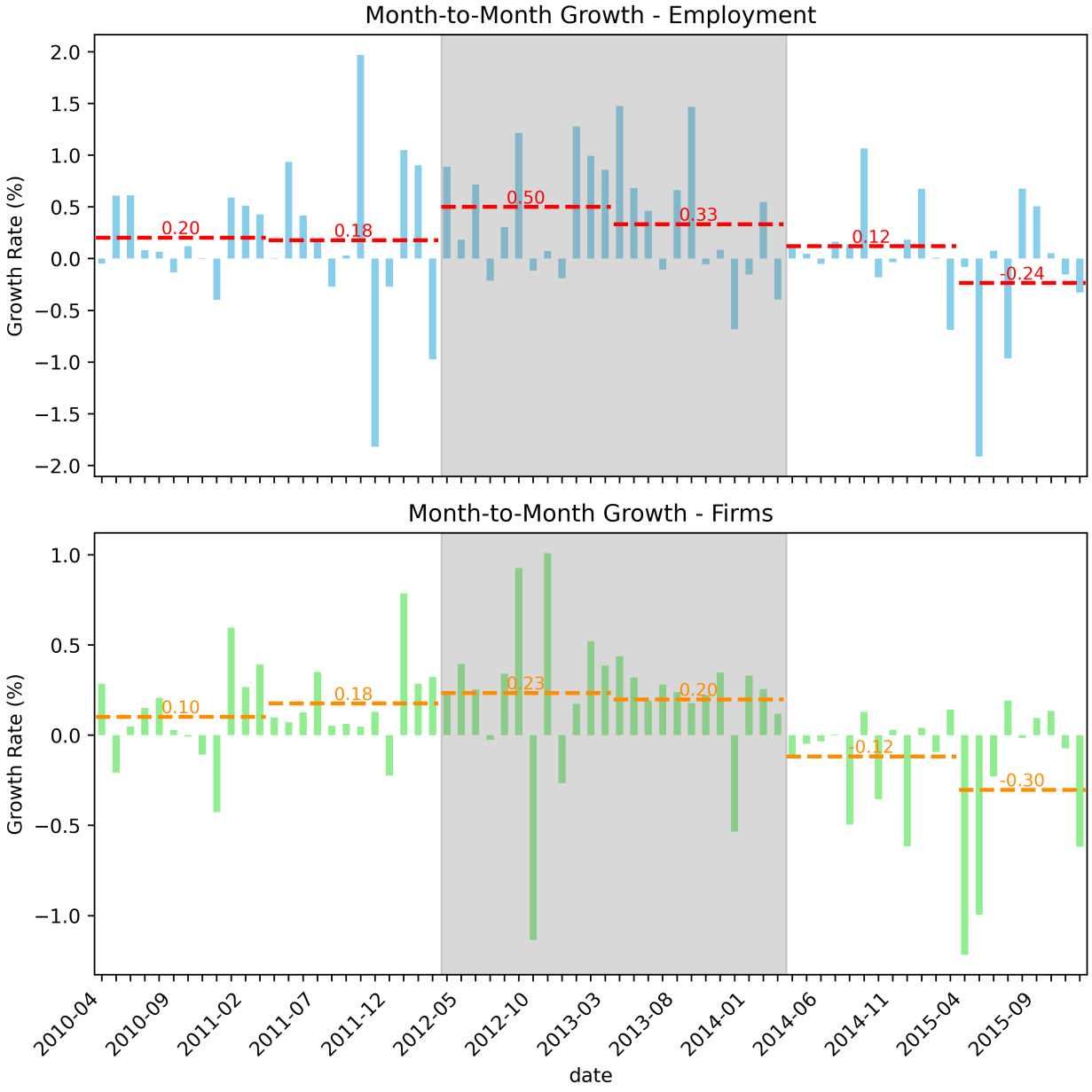
B.2 Figures and Tables

Figure B.1: Relationship Between Crime Reduction during the truce and Economic activity



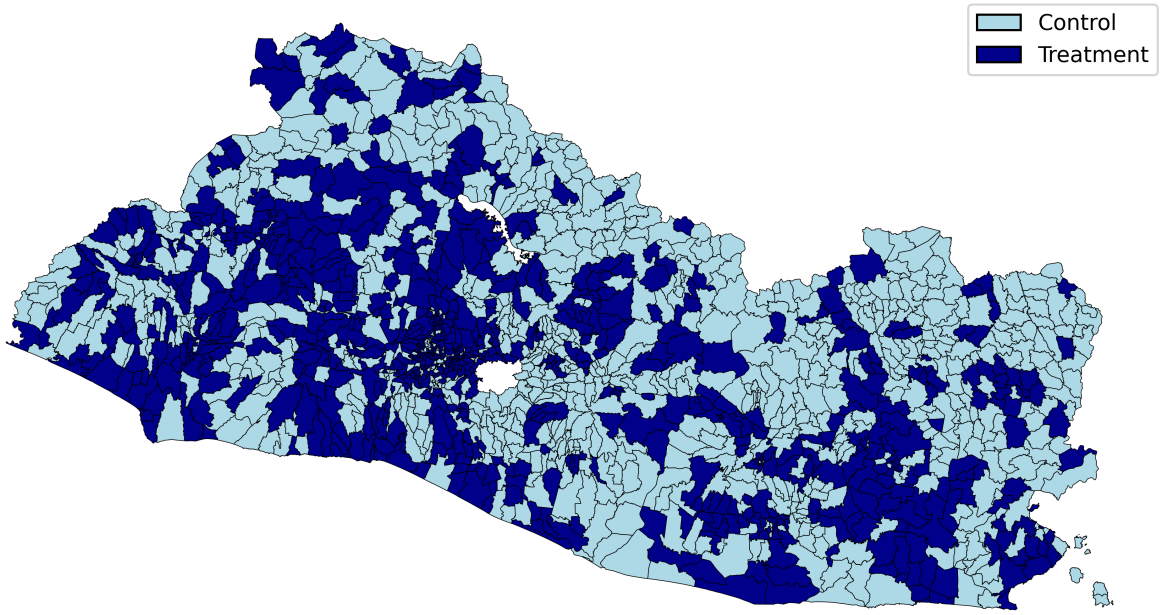
Notes: This figure presents the relationship between the reduction in crime during the truce and two key economic indicators: employment (left panel) and firm presence (right panel). Each point represents a bin of 50 observations, with a fitted linear regression line shown in red. The left panel illustrates the variation in the number of workers, while the right panel depicts the variation in the number of firms, both in response to crime reduction.

Figure B.2: Monthly Growth in Employment and Firms



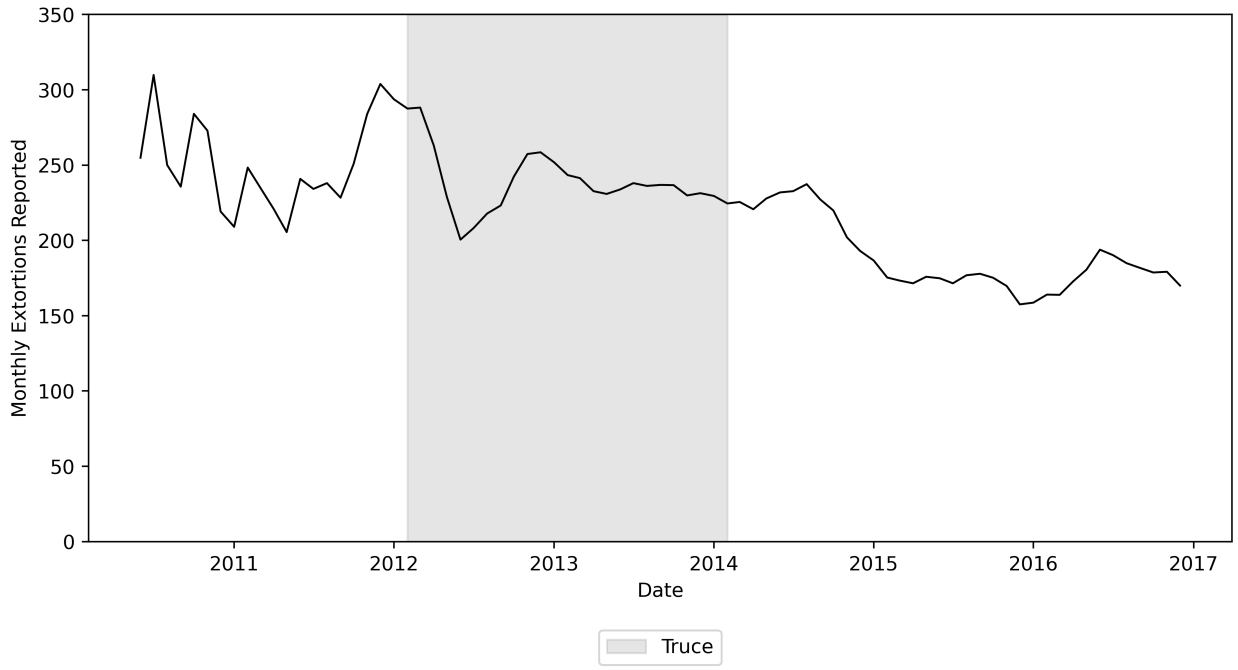
Notes: This figure presents the monthly percentage growth of employment (top panel) and firms (bottom panel) over the study period. Each bar represents the month-to-month growth rate, while the dashed red and orange lines indicate the average growth rates for specific periods. The shaded gray area highlights the truce period.

Figure B.3: Geographic Distribution of Control and Treatment Blocks Based on Crime Reduction During the Truce



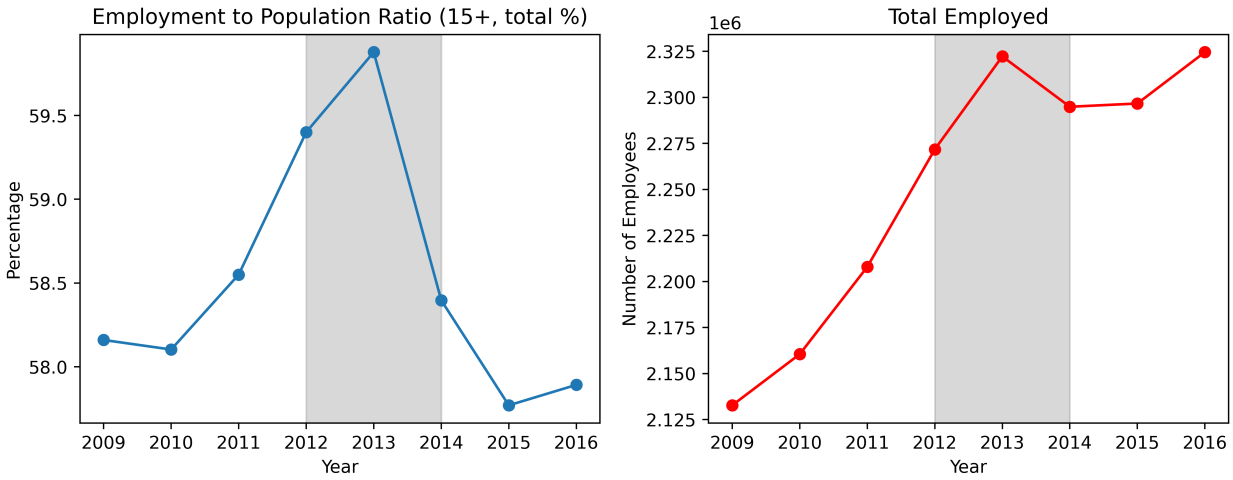
Notes: This map illustrates the spatial distribution of treatment and control blocks across El Salvador, categorized based on the observed reduction in crime rates during the truce period. Areas shaded in blue represent treatment blocks where significant declines in crime were recorded, suggesting these areas were directly influenced by the truce. Conversely, areas in light gray are designated as control blocks, where the truce's impact on crime rates was minimal or non-existent.

Figure B.4: Trends in Monthly Reported Extortions During the Truce Period



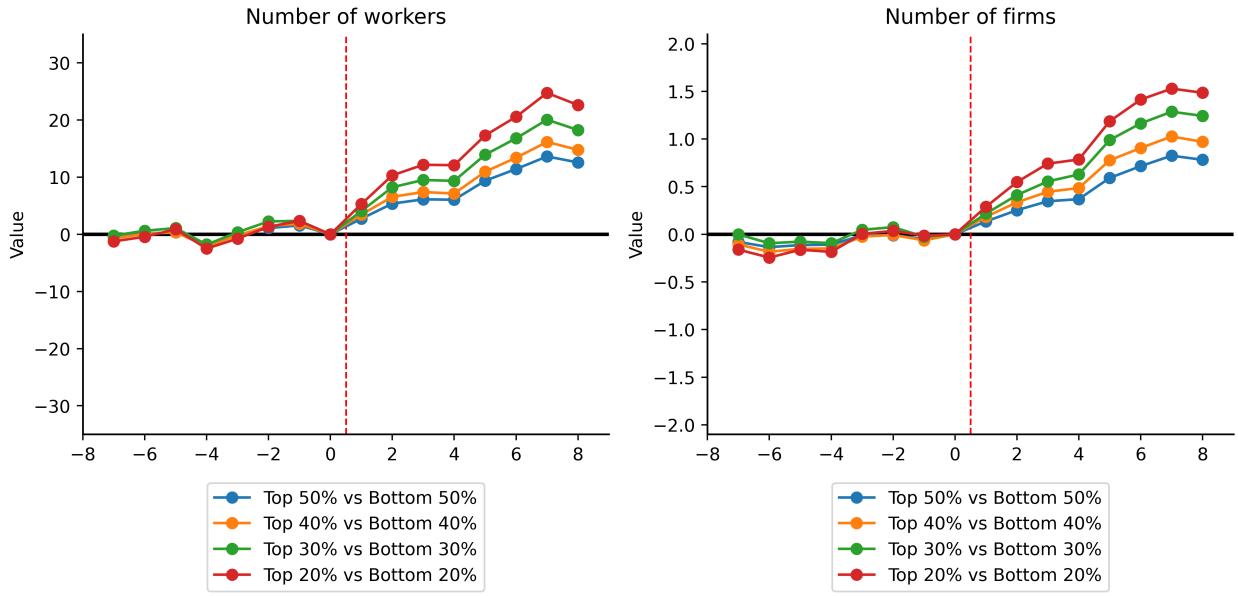
Notes: This graph illustrates the trend in monthly reported extortions from 2011 to 2017, with a specific focus on the truce period highlighted in grey. Despite the implementation of the truce, aimed at reducing gang violence, the data indicates that extortion rates remained relatively stable throughout the truce period.

Figure B.5: Employment to Population Ratio and Total Employed Over Time using WDI data



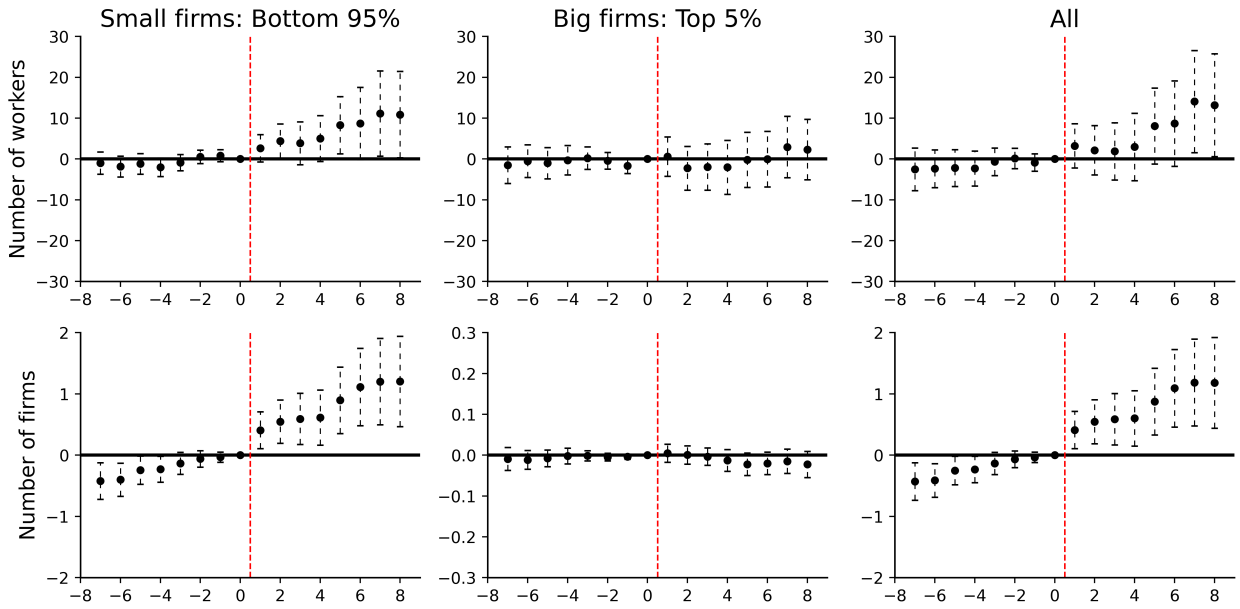
Notes: This figure presents two key labor market indicators for the period 2009–2016, using data from the World Development Indicators (WDI), which are based on household surveys. The left panel displays the employment-to-population ratio (15+, total %), illustrating the proportion of the working-age population engaged in employment each year. The right panel depicts the total number of employed individuals in the economy. The gray-shaded region (2012–2014) marks the truce period.

Figure B.6: Event-Study Results for Different Treatment-Intensity Thresholds



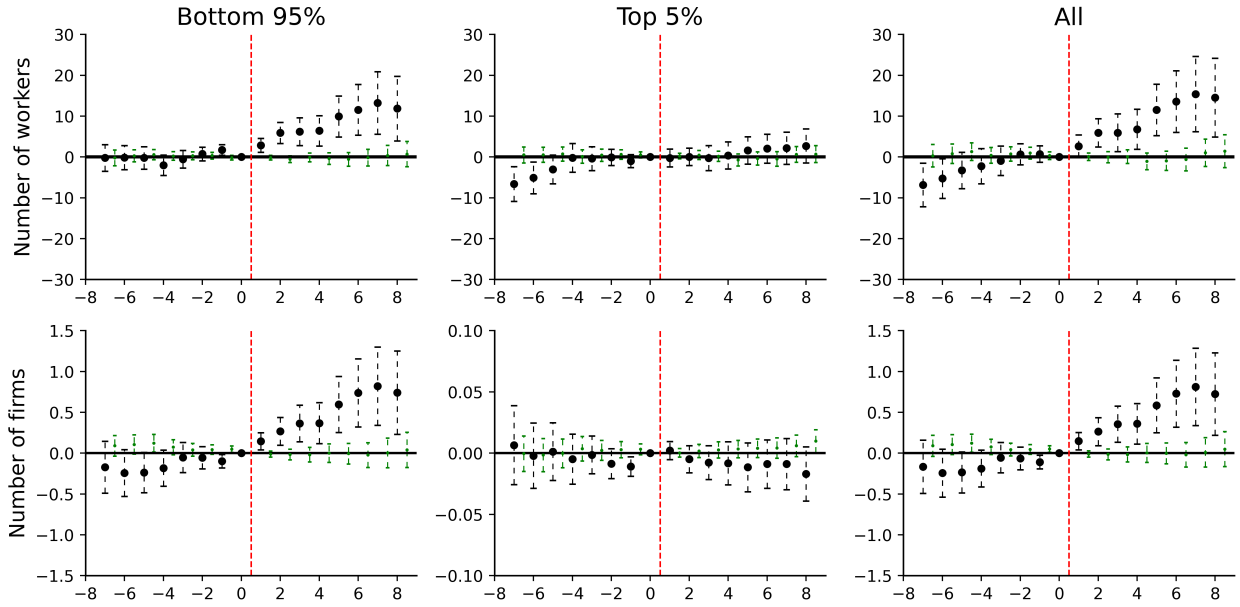
Notes: This Event Study focuses exclusively on the treatment group selected during the truce period. The X-axis consolidates data into three-month intervals, depicting trends for 24 months both before and after the truce to illustrate longitudinal effects.

Figure B.7: Event-Study defining Treatment Based on Gang-Related Homicides Before the Truce



Notes: The X-axis consolidates data into three-month intervals, depicting trends for 24 months both before and after the truce to illustrate longitudinal effects. For error estimation, a block bootstrap method is employed for the post-truce period, while the pre-truce period uses Ordinary Least Squares (OLS) with errors clustered at the unit level.

Figure B.8: Event-Study Results for Placebo Truce Assignments



Notes: This Event Study focuses exclusively on the treatment group selected during the truce period. The X-axis consolidates data into three-month intervals, depicting trends for 24 months both before and after the truce to illustrate longitudinal effects. The green intervals indicate the average Event Study of the Placebos.

Table B.1: Sensitivity of Treatment Effects to Alternative Sample Cutoffs

Variables	**Workers**		**Number of firms**	
	Coef/SE	Per. Change	Coef/SE	Per. Change
		Treatment		
Treatment	8.313***	(5.490%)	0.566***	(2.544%)
	(2.537)		(0.184)	
R-squared	0.9958		0.9986	
Obs.	25296		25296	
		Top 40% vs Bottom 40%		
Treatment	9.884***	(5.666%)	0.727***	(2.854%)
	(3.130)		(0.225)	
R-squared	0.9959		0.9987	
Obs.	19808		19808	
		Top 30% vs Bottom 30%		
Treatment	11.939***	(5.778%)	0.833***	(2.785%)
	(4.053)		(0.289)	
R-squared	0.9961		0.9987	
Obs.	14848		14848	
		Top 20% vs Bottom 20%		
Treatment	15.681***	(6.380%)	1.088***	(3.083%)
	(5.814)		(0.395)	
R-squared	0.9962		0.9989	
Obs.	9920		9920	
		Top 10% vs Bottom 10%		
Treatment	9.997**	(5.898%)	0.677	(2.564%)
	(4.297)		(0.415)	
R-squared	0.993		0.9965	
Obs.	5104		5104	

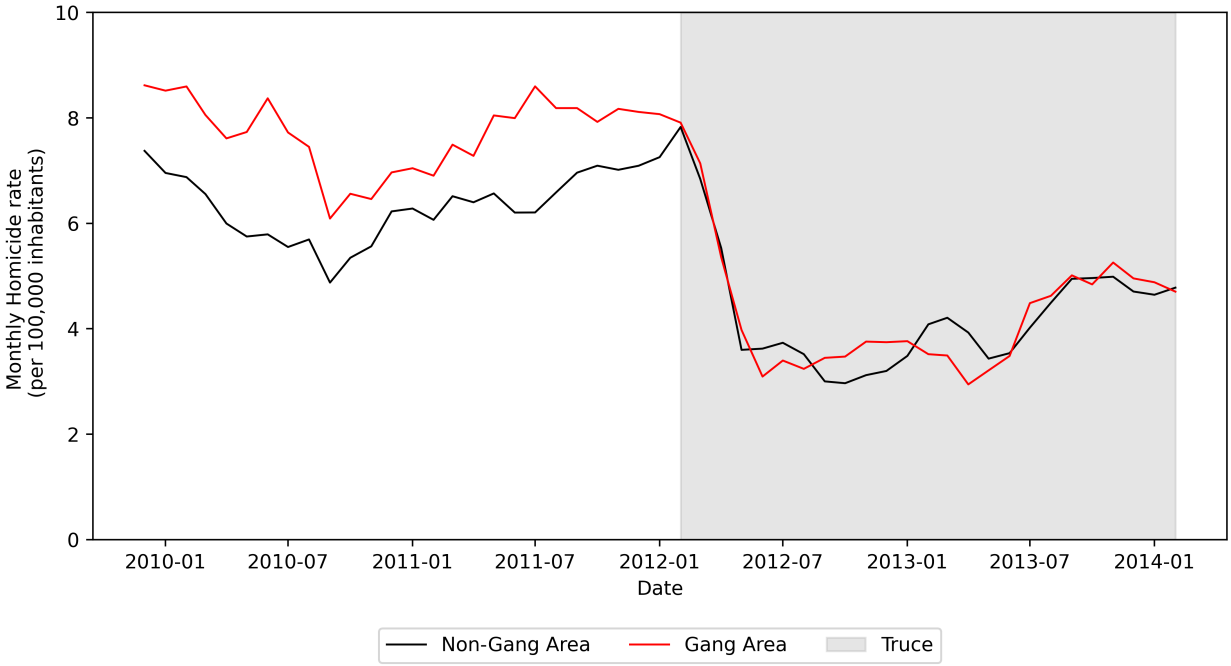
*Notes: This table presents regression results for the impact of the truce on workers, and number of firms, across small and medium firms (bottom 95%). Coefficients and their respective standard errors, shown in parentheses, indicate the estimated effects. The errors were clustered at the level of geographic units interacted with the period it's treatment condition was selected. The significance of coefficients is denoted by asterisks: *p<0.05, **p<0.01, ***p<0.001.*

Table B.2: Summary Statistics: Defining Treatment Based on Gang-Related Homicides Before the Truce

	Area without gangs		Area with gangs	
Variable	Mean	SD	Mean	SD
	Panel A			
Number of firms per block	6.55	23.12	28.42	90.23
Number of firms per block (excl. top 5%)	6.21	21.1	26.77	83.61
Number of workers per block	79.28	272.91	326.47	826.45
Number of workers per block (excl. top 5%)	42.18	172.02	193.76	690.09
Average wages	514.7	541.89	841.38	533.38
Average wages (excl. top 5%)	471.82	481.49	738.04	400.92
Number of opened firms per quarter	0.19	0.83	0.81	2.92
Number of closed firms per quarter	0.11	0.53	0.57	1.85
	Panel B			
Homicides Rate (per 100k hab.) before truce	6.81	22.08	7.84	15.57
Homicides Rate (per 100k hab.) after truce	3.51	15.41	3.45	10.31
Number of Blocks (Primary Geographical Unit)	953.0	953.0	628.0	628.0

Notes: Panel A reports the descriptive statistics of the business base and panel B reports the descriptive statistics of the crime base using homicide rates 15 months before and after the truce into consideration.

Figure B.9: Homicides rates in Treated Areas and Control Areas: Defining Treatment Based on Gang-Related Homicides Before the Truce



Note: This graph illustrates the monthly homicide rate per 100,000 inhabitants, using a moving average. Additionally, the graph features a shaded gray area to delineate the period of the truce between gangs.

Table B.3: Effect of the truce: Defining Treatment Based on Gang-Related Homicides Before the Truce

Variables	**Workers**		**Number of firms**	
	Coef/SE	Per. Change	Coef/SE	Per. Change
		Small and medium firms: Bottom 95%		
Treatment	7.19**	(3.71%)	0.86***	(3.02%)
	(3.24)		(0.24)	
Obs.	25296		25296	
		Big firms: Top 5%		
Treatment	-0.23	(-0.17%)	-0.01	(-0.50%)
	(3.17)		(0.01)	
Obs.	25296		25296	
		All firms		
Treatment	6.96*	(2.13%)	0.85***	(2.82%)
	(4.40)		(0.24)	
Obs.	25296		25296	

*Notes: This table presents regression results for the impact of the truce on workers, number of firms, and wage per capita across small and medium firms (bottom 95%) and big firms (top 5%). Coefficients and their respective standard errors, shown in parentheses, indicate the estimated effects. For error estimation, a block bootstrap method is employed. The significance of coefficients is denoted by asterisks: *p<0.05, **p<0.01, ***p<0.001.*

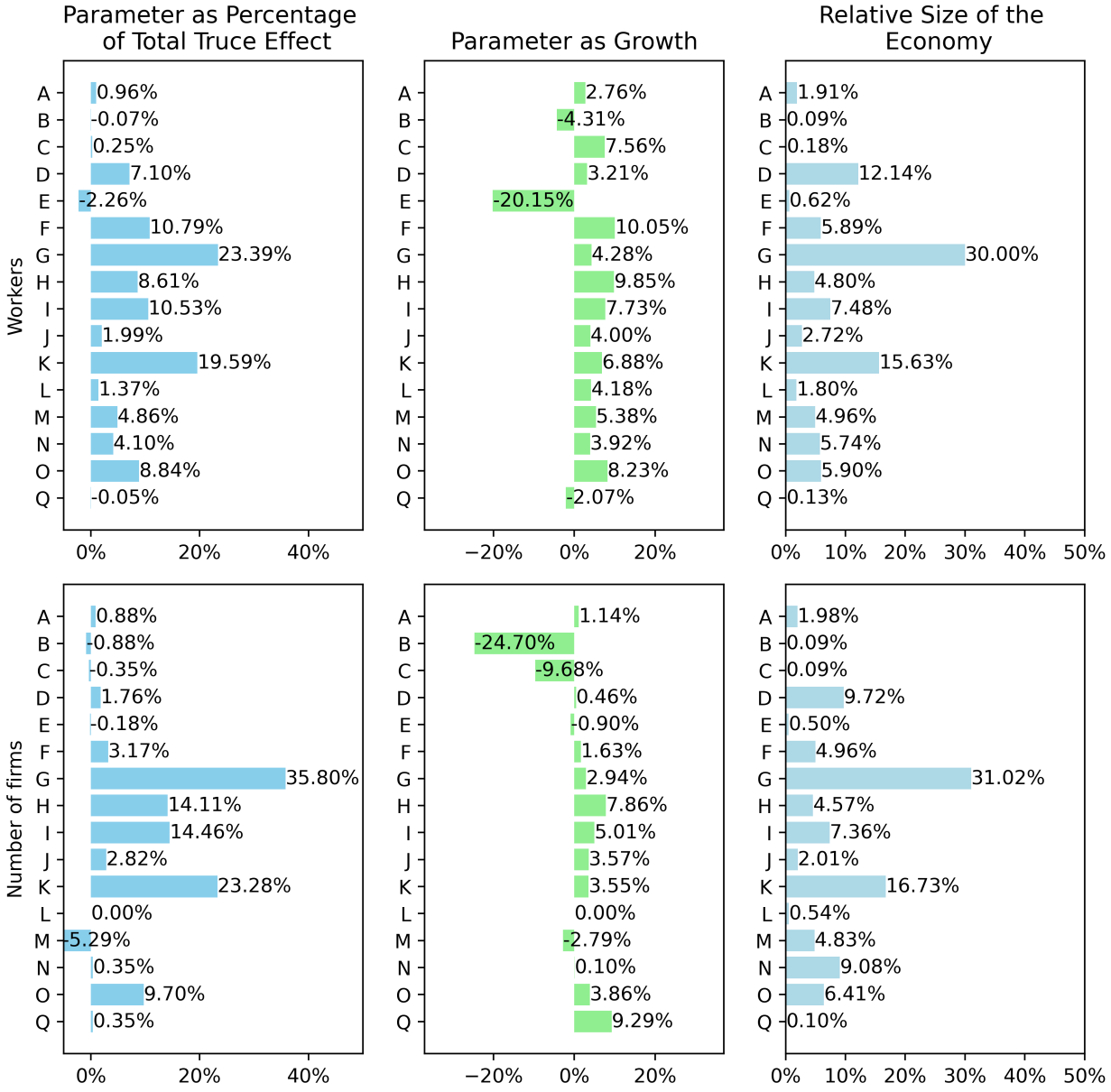
Table B.4: Effect of the truce: Placebo Triple-Differences Estimates

Variables	**3 months before**	**5 months before**	**7 months before**	**9 months before**	**All placebos**
			Workers		
Treatment (Placebo)	-0.387	-0.112	0.038	-0.641	-0.275
	(2.060)	(1.841)	(1.610)	(1.394)	(0.872)
Treatment x Truce	8.700***	8.424***	8.274***	8.953***	8.588***
	(3.267)	(3.134)	(3.004)	(2.894)	(2.682)
R-squared	0.9961	0.9964	0.9965	0.9966	0.9968
Obs.	50592	50592	50592	50592	126480
			Number of firms		
Treatment (Placebo)	0.102	-0.146	-0.129	-0.108	-0.070
	(0.170)	(0.160)	(0.155)	(0.148)	(0.079)
Treatment x Truce	0.463*	0.711***	0.694***	0.674***	0.636***
	(0.250)	(0.244)	(0.241)	(0.236)	(0.200)
R-squared	0.9987	0.9987	0.9988	0.9988	0.9988
Obs.	50592	50592	50592	50592	126480

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

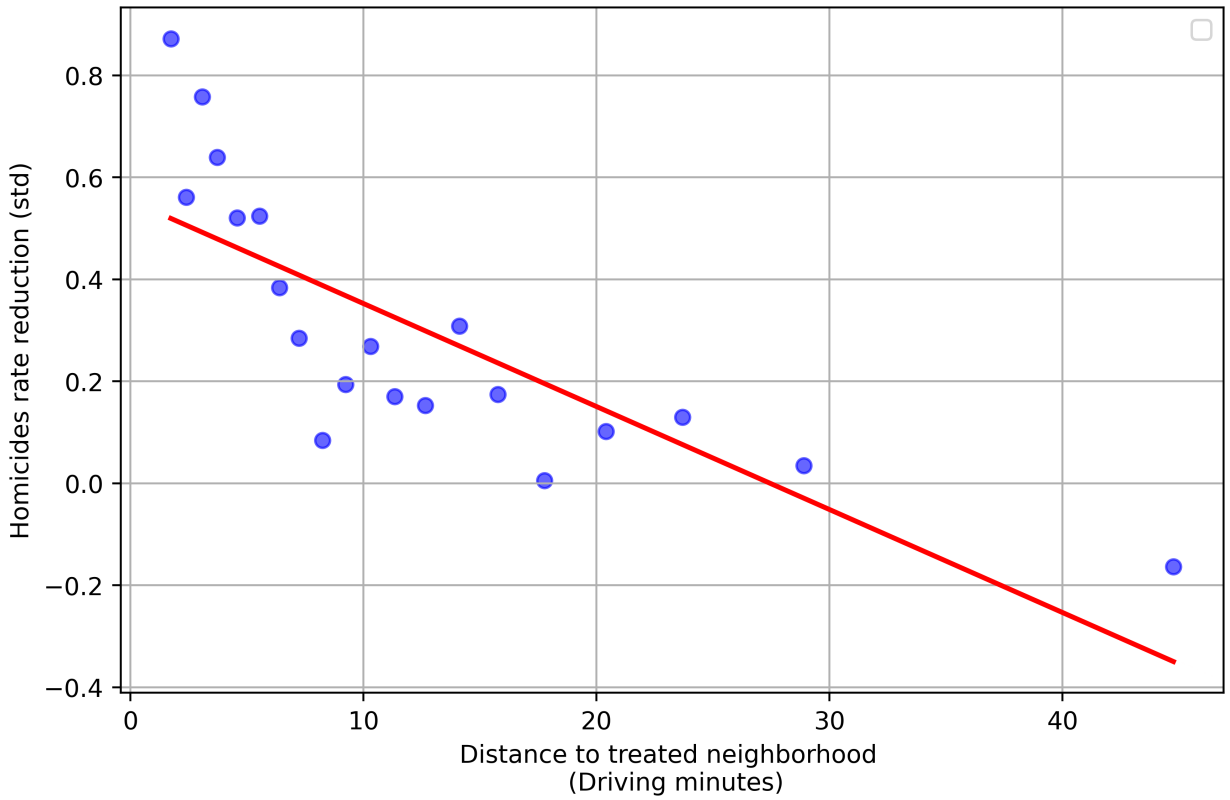
Notes: This table reports the triple-differences estimates examining the effect of the truce and several placebo periods set 3, 5, 7, and 9 months prior to the actual onset of the truce. Each column presents results from a separate specification. The rows labeled "Treatment (Placebo)" capture the hypothetical impact of assigning treatment status using the same procedure but well before the truce actually occurred. The row "Treatment x Truce" represents the additional effect observed exclusively during the actual truce period. Standard errors (in parentheses) are clustered at the polygon level interacted with placebo period or truce.

Figure B.10: Sectoral Growth and Economic Proportions During the Truce



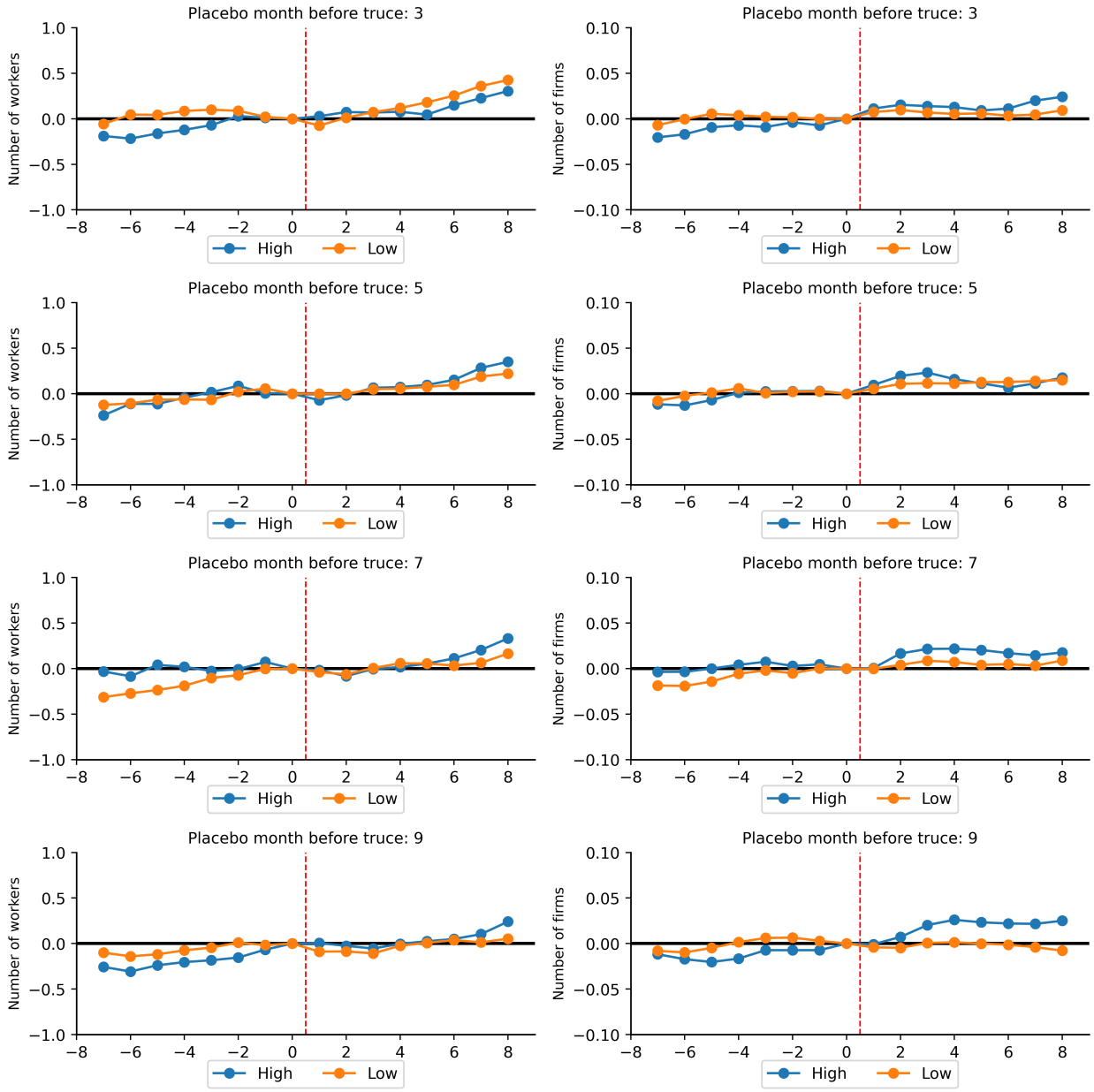
Notes: This graph presents a three-part analysis based on different sectors as classified by the CIIU Rev. 3 standard. The first column represents baseline regression estimates, calculated using sector-specific outcome variables to assess the impact of the truce on each sector. The second column indicates the relative growth in each sector due to the truce, calculated as the treatment effect estimator divided by the pre-truce levels of the corresponding indicator. The third column provides context by showing the percentage that each sector contributes to the overall economy just prior to the truce. The sample exclude top 5% firms.

Figure B.11: Effect of Proximity to Treated Neighborhoods on Homicide Reduction



Notes: This figure illustrates the relationship between driving time to treated neighborhoods and the standardized reduction in homicide rates. The red line represents the fitted linear regression. Treatment is defined based on whether the neighborhood had gang-related homicides prior to the truce.

Figure B.12: Spillover Effects of Proximity to Treated Areas: Placebos



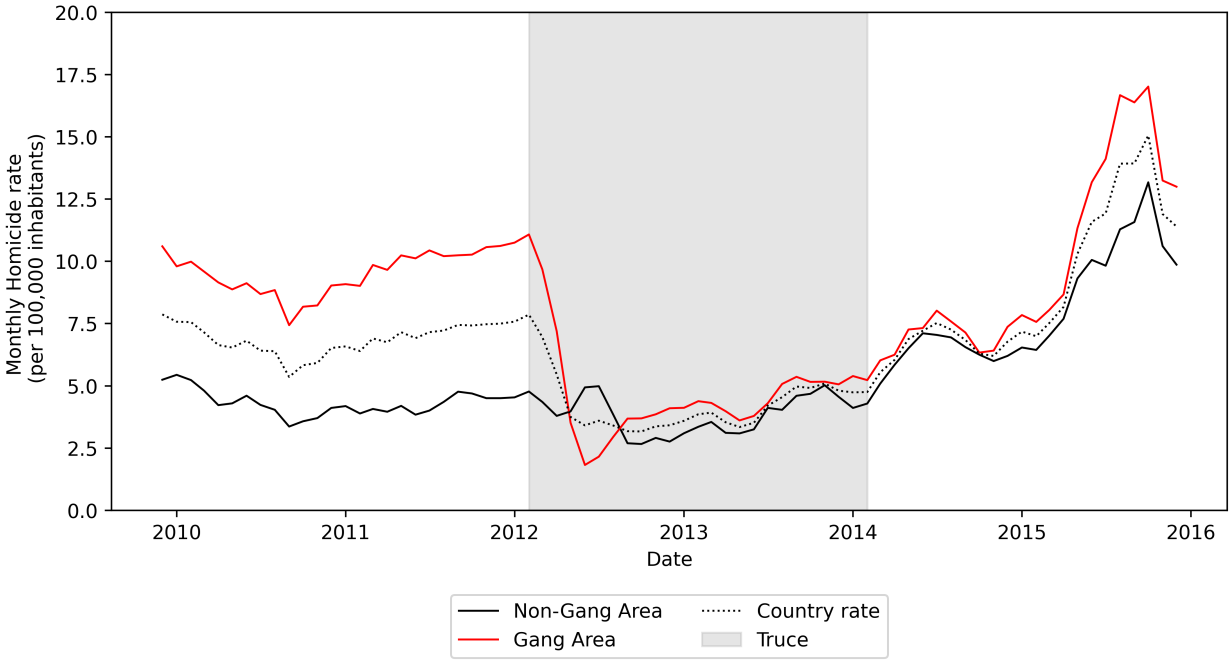
Notes: The graphs decompose these effects based on the magnitude of the homicide drop in the nearby treated polygon: High (above the mean) and Low (below the mean) for different periods before the truce (placebos). The analysis employs a continuous difference-in-differences framework, treating driving distance (in minutes) as a continuous treatment variable.

Table B.5: Covariate Balance: Correcting Spillover using Matching

	Standardized differences		Variance ratio	
	Raw	Matched	Raw	Matched
Distance to treated neighborhood	-1.25	-0.13	0.21	1.17
Homicides rate reduction of neighborhood	0.40	0.02	0.94	1.08

Notes: This table reports the balance of covariates before (raw) and after (matched) matching. Variance ratios compare the variances of the treated and control groups. Values closer to 0 (for standardized differences) and 1 (for variance ratios) indicate improved covariate balance after matching.

Figure B.13: Trends in the Number of Workers and Firms Post-Truce, excluding top 5% companies



Notes: This figure illustrates the changes in the number of workers and firms within the treatment group, benchmarked against levels observed just before the truce (indicated by the dotted red line) and 24 months after the gang truce pact (shown on the horizontal axis).