The Causal Effect of Violent Crime on Economic Activity: Evidence from a Ceasefire

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Abstract

Violence and economic under-development frequently coexist, yet the labor-market returns to reductions in violence remain poorly understood. Leveraging El Salvador's 2012 gang truce, an exogenous event that reduced homicides by approximately 60%, I provide causal evidence that substantial declines in local violence translated into meaningful economic benefits. Specifically, within one year, employment in affected areas increased by approximately 8%, and the number of active firms rose by nearly 5%. Utilizing administrative data on firms and crime, I show that these improvements were driven by heightened local economic activity, characterized by increased sales, higher wages, and lower rates of business closure, accompanied by positive spillovers to adjacent areas. However, the resurgence of violence after the truce's breakdown halted employment growth and led to accelerated firm exits, highlighting the vulnerability of economic gains derived from temporary security 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

A central question in economics is how violent crime affects economic activity. Despite its relevance across both developed and developing countries, causal evidence remains scarce, largely because sudden and plausibly exogenous changes in violence are rare. This gap in the literature is striking given that criminal organizations and violent crime remain widespread. In Latin America and the Caribbean (LAC), for example, homicide rates exceed the global average by more than three-fold, and organized criminal groups account for roughly one-half of all killings (United Nations Office on Drugs and Crime (UNODC), 2023). Moreover, 54% of households report the presence of criminal organizations in their neighborhoods (Uribe, Lessing and Schouela, 2022), raising important questions about their consequences for local economic performance. While much research explores how economic improvements can curb criminal activity,[1] the reverse channel, how violent crime itself suppresses economic performance, remains poorly understood.

This paper provides causal evidence on the economic impact of violent crime by exploiting a uniquely sharp and unexpected reduction in homicides during the 2012 Salvadoran gang truce. Secretly negotiated between incarcerated gang leaders and government officials, the agreement cut homicide rates by an estimated 60% within weeks. The episode offers an unusually natural experiment to isolate how enhanced security affects labor markets and firm activity.

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 labor 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 allow for sectoral heterogeneity in the impacts and find that non-tradable sectors—those most reliant on local economic conditions—drive the bulk of the improvements. This suggests that increased labor demand results primarily from improved firm performance, further supported by evidence of fewer business closures, higher sales, and higher wages. Positive spillovers to nearby units indicate that reductions in violence also benefit adjoining areas.

The temporary nature of the truce highlights the fragility of these gains. When violence resurged

[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 through 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).

in 2014, the growth in the number of businesses was entirely reversed, whereas employment levels did not fully return to pre-truce conditions. Instead, employment ceased to grow relative to the counterfactual and resumed its earlier trajectory, suggesting that while labor markets retained some of their gains, the underlying growth potential was not sustained. This divergence underscores that preserving economic improvements requires lasting rather than temporary security interventions. Consistent with these local effects, national statistics also show a temporary rise in aggregate labor force participation during the truce years, a pattern not observed in other countries in the region, that faded once violence resurged.

To identify the effects of crime reduction on economic activity, I employ a difference-in-differences (DiD) framework. Treatment and control units are defined by abnormal homicide reductions relative to each area's historical crime fluctuations. Units experiencing sharp and unexpected drops in violence are more plausibly linked to gang negotiations. This classification strategy provides a higher-powered approach, as the sudden and pronounced reductions likely dominate potential biases. To address remaining concerns, I test for possible endogeneity using pre-treatment variables and placebo analyses, consistently finding robust results that mitigate doubts about identification. In addition, I develop a continuous measure of exposure to homicide reductions, which I use to examine how the magnitude of crime declines relates to economic outcomes. This complementary analysis allows me to test for linear and quadratic functional forms, and capture treatment intensity.

The estimates of this paper likely understate the economic benefits of crime reduction. Because the truce was temporary and its credibility uncertain, firms and workers may have postponed investments and hiring that would have occurred under a permanent decline in violence. In addition, the agreement primarily reduced homicides; other offenses such as extortion showed no clear decline, and evidence from a later non-aggression pact among the same gangs suggests that extortion may even rise in some contexts.[2] A more stable and broad-reaching reduction in crime would therefore be expected to generate larger increases in economic activity than those estimated here.

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 (Cama-

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

cho 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).

Closer to the question examined here, some studies have specifically analyzed how violence affects labor market participation. 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.

This paper departs from and expands upon this literature by leveraging a uniquely sharp and plausibly exogenous reduction in violent crime to provide direct causal evidence of its impact on economic activity. To my knowledge, it is among the first studies to show that a massive and unexpected decline in homicides can trigger substantial increases in employment and firm creation. The speed and magnitude of the response stand in contrast to prior work, which has typically examined gradual policy changes. For example, existing studies have focused on the multi-year escalation of Mexico's drug war (2007–2011) or on Colombia's more gradual homicide decline from the mid-1990s onward. In contrast, the Salvadoran truce produced a sudden and sustained drop in homicides that was both unexpected and unusually sharp, offering an ideal setting for identification by minimizing the scope for confounding dynamics, such as economic growth gradually improving security, that often complicate causal interpretation.

In addition to exploiting this distinctive shock, the analysis covers the full spectrum of economic activity rather than subsets of firms with high productivity or international exposure. Prior studies typically focus on stock market-listed firms, exporters, or specific manufacturing industries, while this paper examines broad effects across all sectors. A novel finding is that smaller firms and those in non-tradable sectors, more dependent on local demand, experience the most significant gains. This result complements recent evidence by Utar (2024), who shows that within manufacturing, plants that rely more heavily on domestic inputs and markets are especially vulnerable to violence. While that study is limited to one sector, my findings demonstrate that the economic benefits of improved security extend well beyond manufacturing and are structurally driven by the importance of local demand of goods across multiple sectors. This has broader implications for understanding how violence distorts economic geography in high-crime contexts.

Earlier work often shows that increases in crime reduce labor supply, leading to lower employment and upward pressure on wages, consistent with a leftward shift in labor supply. By contrast, in this setting both employment and wages rise, pointing to a demand-side mechanism. To interpret these patterns, I develop a simple theoretical model that treats crime as a "fear cost" for consumers in violent areas and allows labor force participation to depend on heterogeneous reservation wages. The model replicates the direction of the effects seen in the data and clarifies the conditions under which wages increase when violence falls. It highlights two opposing forces: a direct effect, where falling crime lowers fear costs, boosts local demand, and increases hiring; and a reallocation effect, where spending shifts away from safer areas, dampening labor demand there. Whether wages rise depends on which force dominates.

The features that tilt the balance toward the direct effect also characterize the Salvadoran context. Fear costs were likely high, violent areas accounted for a meaningful share of consumption, and substitution across neighborhoods was limited. These elements indicate that aggregate labor demand increased, in line with the mechanism emphasized by the model.

An additional question is whether the effects of crime reduction extend beyond directly treated areas. Spillovers matter for two reasons: they may bias the main estimates if neighboring areas are indirectly affected, and they are also outcomes of independent interest. To address this, I examine whether safer conditions in treated locations affected adjacent areas. While much of the prior literature highlights negative spillovers—crime being displaced into nearby neighborhoods (Draca, Machin and Witt, 2011; Dell, 2015)—I instead find suggestive evidence of positive spillovers, with reductions in violence fostering economic gains beyond the directly exposed units. Although the evidence is suggestive and subject to some classification limitations, the exercise indicates that the overall results remain robust: aggregate estimates of employment and firm creation stay positive even when spillovers are taken into account.

Although the institutional details of the Salvadoran truce are unique, the conditions under which its effects emerged, such as high fear costs, the importance of violent areas in overall consumption, and the reliance of households and firms on local demand, are not specific to El Salvador. Similar environments exist in other countries, suggesting that the mechanisms identified here are plausibly relevant beyond this case. At the same time, prior work has documented broader associations between crime and economic performance. For instance, Islam (2014) show that economic growth is negatively correlated with crime across firms in 27 developing countries, with the relationship especially pronounced for small and medium enterprises. By moving from such correlations to causal evidence based on a sharp and unexpected decline in violence, this paper contributes to a broader understanding of how reducing crime can foster economic growth, and under what circumstances such gains are most likely to materialize.

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 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 discusses the implications of the theoretical model and how it helps interpret the empirical results. Section 10 concludes.

2 Context

2.1 Violence in El Salvador

El Salvador has previously been characterized by high levels of violence often driven 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. Further, this period was characterized by a lack of government programs to address gang violence or reintegrate deported Salvadorans, which enabled a further 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 El Salvador 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. Following 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.

Beyond the immediate impact on violence, the truce was also accompanied by an expansion in labor force participation at the national level. As reported in Table B.1, participation increased by 1.33 percentage points (a 2.27% rise) during the truce, but this gain was fully reversed once the truce collapsed. Importantly, such fluctuations are not observed in other Latin American countries during the same period, underscoring the distinctiveness of the Salvadoran experience.

3 Data

This study relies on three primary data sources, which are integrated and aggregated at the geographic level. Specifically, the unit of analysis is a set of 1,581 polygonal geographic divisions level units—covering the entire country. All firm and crime data are aggregated to these polygons and observed over time, yielding a monthly panel from 2010 to 2015.

1. **Social Security Database (ISSS)**: This administrative dataset provides monthly records of all private-sector firms registered with the Salvadoran Social Security Institute. It includes information on firms' primary activity, number of employees, total payroll.

2. **National Civil Police Records**: This dataset offers monthly records of all reported homicides in the country.

3. **Annual Business Surveys from FUSADES**: Conducted by a leading Salvadoran think tank, these surveys cover approximately 580 firms from 2008 to 2015 and include self-reported indicators such as sales, perceived insecurity, and business expectations.

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.

One potential limitation of the data is that firm records from the Social Security Institute (ISSS) include only a single registered address per company. For large firms with multiple branches or operational sites, this could mean I observe only their headquarters or administrative address, rather than their full geographic footprint. As a result, interpreting local effects for these firms is less straightforward. To address this, the main analyses focus on firms in the bottom 95% of the employment distribution, where the registered location is more likely to coincide with actual operations. Nevertheless, I also report results that include only the top 5% of firms and the full sample, ensuring that the findings are not driven by sample selection or omitted heterogeneity in firm size.

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 using a triple-differences framework, in which I compare estimated effects across multiple hypothetical truce periods. Specifically, I fully reapply the classification methodology to placebo "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, pooling the placebo periods alongside the actual truce period within a uni-

fied triple-differences estimation.

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 provide a full description of the triple-differences estimation strategy in Section 4.1, where the model and identification assumptions are discussed in greater detail.

Additionally, 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.

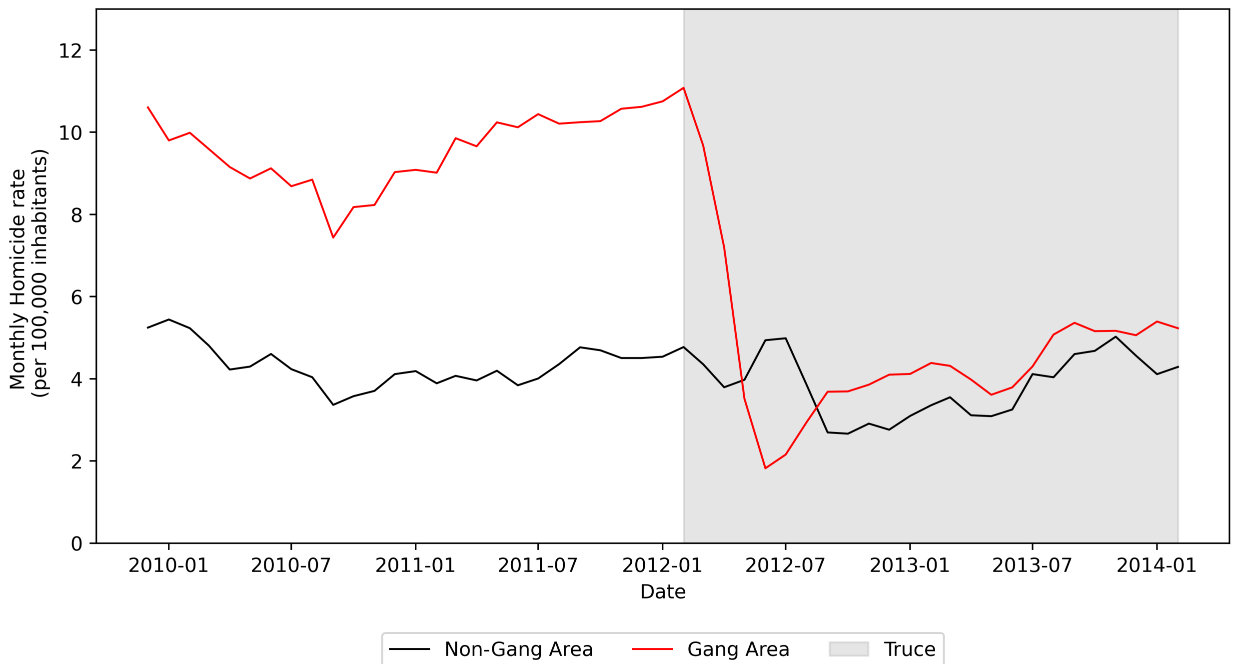
This alternative measure, however, is not without limitations. Not all gang-related crimes are perfectly recorded or correctly classified, which introduces noise. Additionally, not all gangs participated in the truce, meaning that some areas continued to experience violence despite the overall decline 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.10).

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 homicides per 100,000 inhabitants, reflecting considerably more violence in gang areas. However, following the onset of the truce, homicide rates in treated areas plummeted by approximately 60%, bringing the post-truce gap down to a mere 0.3 homicides per 100,000 inhabitants. 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.

However, the binary treatment DiD may not fully exploit variation in the intensity of the homicide reduction. To better capture this variation, I extend the analysis by modeling the intensity of the homicide reduction as a continuous dose of treatment, drawing on insights from Callaway, Goodman-Bacon and Sant'Anna (2024). Although the notion of continuous treatment is not new, their framework highlights important sources of bias in TWFE estimation when treatment varies in intensity—particularly when treatment effects are nonlinear.

To operationalize this, I redefine the treatment measure D_i as a continuous variable: let $D_i \in \mathbb{R}_+ \cup \{0\}$, where $D_i = 0$ if $Z_{i,T} \leq M_T$, and $D_i = Z_{i,T} - M_T$ if $Z_{i,T} > M_T$.

To address potential biases from TWFE in this setting, I follow a two-step residualization strategy. 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 Treatment 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 the fact that when treatment is split into discrete intensity levels, pre-trends are still absent.

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 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 alternative definition of treatment poses challenges for standard two-way fixed effects models, as treated and control areas often exhibit differential pre-trends—violating the parallel trends assumption. These pre-trends appear to stem from differences in baseline characteristics between treated and control areas—particularly in variables like the number of firms—leading to divergent trajectories prior to the truce. To address this, I follow the method proposed by Borusyak, Jaravel and Spiess (2024), which incorporates unit-specific linear time trends based on pre-treatment characteristics. Specifically, I estimate all fixed effects and trends using only untreated periods and then impute counterfactual outcomes for the treated periods. This approach helps absorb differential trends related to baseline characteristics, providing a cleaner estimate of the treatment effect. A more technical discussion of this procedure and its implications is presented in the appendix B.1.

Second, I implement a triple difference-in-differences (DDD) strategy to test whether the observed effects are specific to the actual truce period or could also emerge spuriously under alternative treatment timings. To do so, I construct placebo datasets in which the truce is hypothetically assigned to begin 3, 5, 7, or 9 months before the real truce's actual start date. For each placebo timing, I replicate the treatment classification procedure and generate a corresponding dataset. These datasets are then concatenated—along with the actual truce dataset—into a single stacked dataset, with an indicator variable s identifying the scenario: $s = 1$ corresponds to the actual truce, and $s \neq 1$ denotes a placebo.

I then estimate a triple differences model with unit and time fixed effects fully interacted with s. This model isolates the average effect of being classified as treated across all scenarios and estimates the additional effect when treatment status coincides with the real truce period ($s = 1$). Put differently, the identifying variation comes from comparing the effect of treatment in the actual truce period relative to placebo periods. Event study analyses complement this approach by verifying the absence of anticipatory or spurious effects in the placebo periods. The results confirm that significant economic improvements are only observed during the real truce period, strengthening the interpretation that the truce—rather than the classification method—drives the main results.

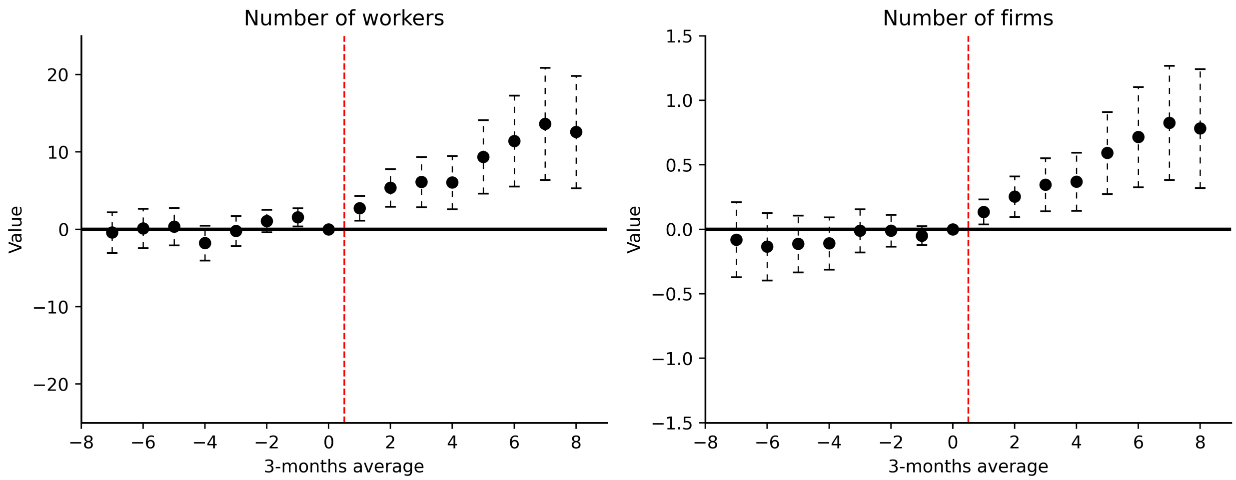
5 Empirical Findings

5.1 Baseline Treatment Effects

I begin the empirical analysis by examining how local economic outcomes evolved around the time of the truce. The goal is to assess whether the sharp and exogenous drop in violence coincided with observable changes in employment and firm creation. To this end, I estimate an event study specification that traces the dynamics of key economic indicators before and after the truce. 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. Within the event study, pre-existing trends in these outputs are indistinguishable, and a significant increase is evident subsequent to the truce between gangs.

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. Expressed as elasticities relative to the observed decline in homicides among treated blocks, these estimates imply crime-employment and crime-firm elasticities of approximately 0.11 and 0.05, respectively. 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 an event study analysis that only considers these large companies (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.

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

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.2). The corresponding event-study figures confirm no apparent pre-trends, with the divergence emerging only after the truce is implemented (see Appendix Figure B.7).

To further assess the robustness of these findings, I implement sensitivity analyses following the methodology of Rambachan and Roth (2023), which relaxes the parallel trends assumption. Under relative magnitude restrictions, the estimated effects on both workers and firms remain statistically significant even when allowing for post-truce violations of parallel trends up to four times larger than the maximum pre-truce deviation. This implies that the results are robust to substantial deviations from parallel trends, particularly for the second year after the truce (see Appendix Figures B.5 and B.6).

Under the alternative smoothness restrictions, the estimated effects also remain robust. The results for workers only become insignificant if we allow for deviations from a linear extrapolation of the pre-trend that exceed the equivalent of an entire year of average employment growth in the post-truce period. For firms, the breakdown point corresponds to approximately one-third of a typical year's firm growth. These thresholds suggest that the observed positive effects are not driven by minor trend extrapolation errors and hold even under meaningful departures from the strict parallel trends assumption (see Appendix Figures B.5 and B.6).

These estimates come from formal sector firms, as the data do not capture informal business activity. To provide a broader view, I complement the analysis with national labor market indicators. Household survey data from the World Bank's WDI show that labor force participation rose by about 1.33 percentage points (roughly 2.27%) during the truce, before returning to pre-truce levels once it ended (Table B.1). This short-lived increase is not mirrored in the regional average for Latin America and the Caribbean, suggesting that the shift in El Salvador reflects a country-specific response likely tied to the temporary decline in violence.

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.5, 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.9, 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.

One might expect, based on standard economic theory, that any sharp decline in crime—regardless of timing—would improve local labor market conditions. However, no such pattern emerges in the placebo windows. This apparent discrepancy may reflect the fact that, outside of the truce, sudden and sizable crime drops are rare, and when they do occur, they tend to be noisy or short-lived. As a result, the placebo classifications may fail to capture meaningful shocks large enough to produce detectable economic responses. Supporting this interpretation, Table B.6 presents evidence from a modified approach that restricts the placebo analysis to polygons experiencing large reductions in crime. In these more extreme cases, we do observe a positive association with subsequent employment and firm growth, consistent with the notion that substantial crime is correlated with economic activity.

5.3 Robustness Check: Gang-Related Homicide Treatment Classification

To provide further evidence supporting the effects of the truce, I consider an alternative classification: areas 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 areas of 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 3.56%, and firm counts increase by 3.03%, as reported in Table B.4. Given the observed reduction in crime within treated polygons, these effects correspond to crime-employment and crime-firm elasticities of approximately 0.34 and 0.29, respectively. While the employment elasticity is not statistically different from previous estimates, the elasticity for firm counts is significantly higher, aligning with expectations that gang-intensive areas would be particularly responsive to reductions in gang-related violence. This robustness underscores the validity of the main findings and confirms the economic significance of reduced violence.

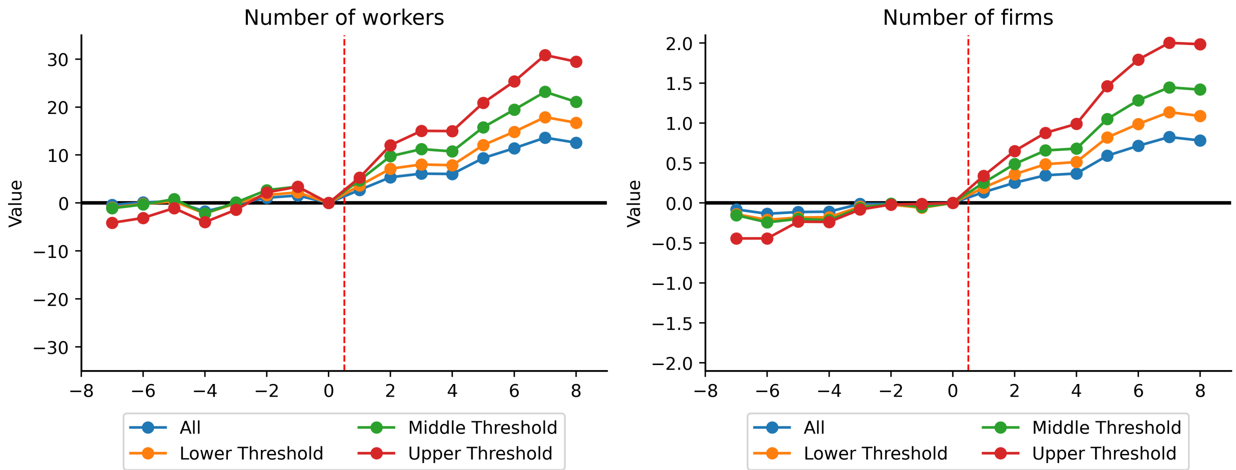
5.4 Continuous Treatment Intensity and Average Causal Response (ACR)

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 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—in this case referring to the standard deviation of historical changes in each unit's homicide rate—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 one 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 suggest that reductions in violent crime are associated with improvements in employment and business. To better understand the forces driving these changes, I outline several potential mechanisms informed by prior literature. These are not exhaustive, but illustrate some of the plausible channels through which crime reduction may spur economic activity. One possibility is that improved security reduces uncertainty for firms, encouraging investment, expansion, and the opening of new businesses—particularly in previously high-risk areas. A second possibility is that safer conditions expand the effective labor supply by enabling workers to commute to or relocate near areas that were previously unsafe, potentially increasing workforce participation and placing downward pressure on wages. A third possibility is that reductions in violence enhance consumer confidence, encouraging households to spend more freely and engage more actively in the local economy.

These channels are not mutually exclusive, and distinguishing between them is important for informing policy. Understanding whether crime reduction primarily stimulates local consumption, encourages business investment, or facilitates labor mobility can guide more targeted interventions aimed at sustaining economic recovery in high-crime areas. The evidence presented in this study points most clearly toward a context-specific mechanism in which improved security boosts local demand for goods and services, prompting firms to expand operations and increase labor demand. Specifically, I observe (i) better business conditions, including fewer firm closures and higher reported sales; (ii) stronger consumer confidence—measured directly using survey data; (iii) higher wages, consistent with growing demand for labor rather than an inflow of workers; and (iv) stronger effects concentrated in non-tradable sectors, which are especially sensitive to local consumer activity. Together, these patterns support the view that crime reduction relaxes demand-side constraints for goods and services in gang-affected communities, allowing local businesses to expand and labor markets to grow.

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.

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 labor 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% of the firm size distribution, my estimates indicate a statistically significant wage increase of 19.80 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 general 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 employment in **H** (*Hotels and Restaurants*) and **I** (*Transport and Storage*) grow by 9.85% and 7.73%, respectively (see Figure B.11). 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 initially detect spillovers, I modify the primary difference-in-differences (DiD) specification by adding a dummy variable indicating adjacency to treated polygons. Specifically, I estimate a model of the following form: $Y_{i,t} = \alpha_i + \gamma_t + \tau D_{i,t} + \rho S_{i,t} + \epsilon_{i,t}$, where $Y_{i,t}$ denotes the economic outcome of interest (employment or number of firms) in polygon i at time t, α_i and γ_t are polygon and time fixed effects respectively, $D_{i,t}$ is the treatment indicator, and $S_{i,t}$ captures spillover exposure. I implement two complementary versions of this specification. In the first, $S_{i,t}$ is defined as a dummy equal to 1 if the polygon is directly adjacent to one treated polygon. In the second, $S_{i,t}$ is defined as a continuous measure equal to the distance (in kilometers) to the nearest treated polygon. Together, these alternative definitions provide complementary evidence on how proximity to treated areas shapes indirect economic impacts.

This specification allows simultaneous estimation of both direct and indirect effects of the truce: τ captures the direct effect of crime reduction in treated polygons, while ρ identifies spillover effects, which may include both treated and untreated units.

One important caveat is the treatment definition used to identify spillovers. While the main results presented earlier rely on a treatment classification based on observed homicide reductions during the truce, this definition might inadvertently include units already affected by spillovers, potentially misclassifying some adjacent units as directly treated. To mitigate this issue, the spillover analysis in this section employs the alternative treatment definition based solely on the presence of pre-truce gang-related homicides. This provides a more exogenous proxy for treatment. However, even this approach has limitations: some areas treated during the truce may not have previously recorded gang-related homicides, implying that some polygons classified as control may indeed have been treated[3]. Given this residual uncertainty about whether control units are truly untreated, the estimated spillover effects should be interpreted as an upper bound of their likely magnitude.

Results from this spillover specification (see Table A.4) show that the estimated direct effect of the truce on treated areas remains positive and statistically significant even after accounting for spillover effects. Although the magnitude of the direct effect declines slightly relative to the baseline specification, this difference is not statistically significant. Importantly, the coefficients on the spillover variable are both positive and statistically significant, indicating that polygons adjacent to treated areas also benefited economically from the truce. The estimated spillover corresponds to an increase of approximately 11% in employment and about 6% in the number of firms relative to direct effect of the truce. While these indirect effects are smaller in magnitude than the direct impacts, they nonetheless provide evidence of meaningful economic diffusion beyond the originally treated areas.

8 Aftermath of the Truce: Rising Crime and Economic Reversal

Shortly after the truce dissolved, homicide rates began to rise steadily in both treated and control polygons, eventually surpassing their pre-truce levels in several areas. As shown in Figure B.12, 2015 marked the most violent year in El Salvador's recent history, with the national homicide rate exceeding 100 per 100,000 inhabitants—implying that nearly 0.1% of the population was killed that year. This surge in violence coincided with a sharp escalation in the government's anti-gang efforts, often referred to as *mano dura* ("iron-fist") policies, which emphasized aggressive policing. These confrontational tactics intensified conflict between security forces and gang networks, likely fueling further instability and reversing many of the temporary security gains achieved during the truce.

In tandem with rising crime, the economic indicators that had improved during the truce began to reverse. Figure A.6 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

[3] Thus, while results from the main definition are also estimated and presented in the Appendix B.7, the analysis here focuses on the more exogenous gang-related homicide classification to interpret spillovers.

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 collapsed, 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 Discussion

To organize the empirical results, I develop a simple theoretical model that rationalizes the main patterns observed during the truce (see Appendix B.3 for the formal derivation). The model treats crime as an additional "fear cost" that consumers face when purchasing in violent areas, and it allows labor force participation to depend on heterogeneous reservation wages. Although deliberately parsimonious, the framework is rich enough to reproduce the salient outcomes in the data: wages increased, employment expanded, incumbents hired more workers, and new firms entered. More importantly, it provides a lens to examine two central puzzles that the empirical analysis alone cannot resolve: why wages rose rather than fell, and under what conditions employment growth reflects greater participation rather than mere reallocation across zones.

The first puzzle concerns wages. Existing studies such as Utar (2024) and Velásquez (2020) find that wages move in the same direction as crime: when violence increases, wages rise, largely because higher risk constrains the effective supply of labor. The Salvadoran case stands in sharp contrast. After the truce, wages increased even as crime fell, and they did so alongside higher employment. The mechanism here works through demand. When violence is high, consumers attach a fear cost to purchasing in violent zones: the effective price is higher than the producer price. When crime falls, this fear cost drops, making goods in those areas cheaper in real terms. Households redirect spending toward the previously violent zones, and firms expand production to meet this demand. Aggregate labor demand therefore shifts outward, pushing up both employment and wages.

The model makes clear why two opposing forces emerge. The *direct effect* operates through the price channel: when crime is high, the fear cost inflates the effective price of goods in violent areas, which reduces demand and lowers labor needs there. When crime falls, that cost diminishes, local demand expands, and firms hire more workers. The magnitude of this effect depends on how sensitive consumers are to crime in their purchasing decisions, how important violent areas are in overall spending, and how steeply demand reacts to price changes. By contrast, the *reallocation effect* works in the opposite direction: as violent areas become relatively cheaper, households shift spending away from safer areas. This reduces labor demand in those safer zones, partially offsetting the aggregate gain. Whether wages rise or not depends on which effect dominates. When the fear cost is large, when violent areas already represent a meaningful share of the economy, and when substitution across neighborhoods is not too strong, the direct effect overpowers reallocation, aggregate labor demand rises, and wages increase.

The second puzzle is whether employment gains in truce zones came from workers moving from safer areas or from new entrants joining the labor force. The available data cannot fully disentangle these channels: firm-level social security records cover only the formal sector, and thus miss potential shifts between the formal and informal economy. What we can observe, however, is that overall labor force participation in El Salvador increased during the truce years. Descriptively, this rise does not appear to be part of a broader regional trend, as average participation across Latin America remained relatively stable in the same period (see Table B.1). While this evidence is not causal, it suggests that the truce coincided with a meaningful expansion of participation at the national level, which provides context for interpreting the formal-sector patterns.

The model clarifies how both reallocation and participation operate, and under what conditions one margin may dominate the other. At fixed wages, the mechanism is straightforward: a reduction in crime lowers the fear cost of consuming in violent areas, making them relatively cheaper. Consumers redirect spending toward these areas, firms there expand production, and local employment rises. This is a pure reallocation effect, since the increase in violent zones is mirrored by contractions elsewhere. But once we account for general equilibrium, wages also adjust. Higher wages raise the income of all households and draw in new workers whose reservation wages were just above the previous equilibrium. Participation therefore expands, total demand increases, and employment rises more broadly. In equilibrium, both channels operate simultaneously: reallocation concentrates gains in formerly violent areas, while participation explains why the overall economy absorbs more workers.

The model also highlights when participation is likely to be the stronger force. This happens when households are very responsive to wage changes—for instance, when outside income is limited or safety nets are weak, so that even small wage increases bring new workers into the labor force. It also depends on how strongly wages respond to falling crime: if the outward shift in labor demand is large, the rise in wages amplifies the participation margin. Conversely, reallocation tends to dominate when substitution possibilities across neighborhoods are strong, when violent areas initially account for only a small share of consumption, or when wage responses to falling crime are weak.

In the Salvadoran case, the evidence suggests that participation played a particularly important role. Wages rose substantially following the truce, safety nets were limited, and employment growth was concentrated in non-tradables with few substitutes across space. These features align closely with the conditions under which the model predicts participation to dominate. It is therefore plausible that both reallocation and participation were at work, but that the surge in labor force participation was central to the observed employment gains.

10 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? 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. Although 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):

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

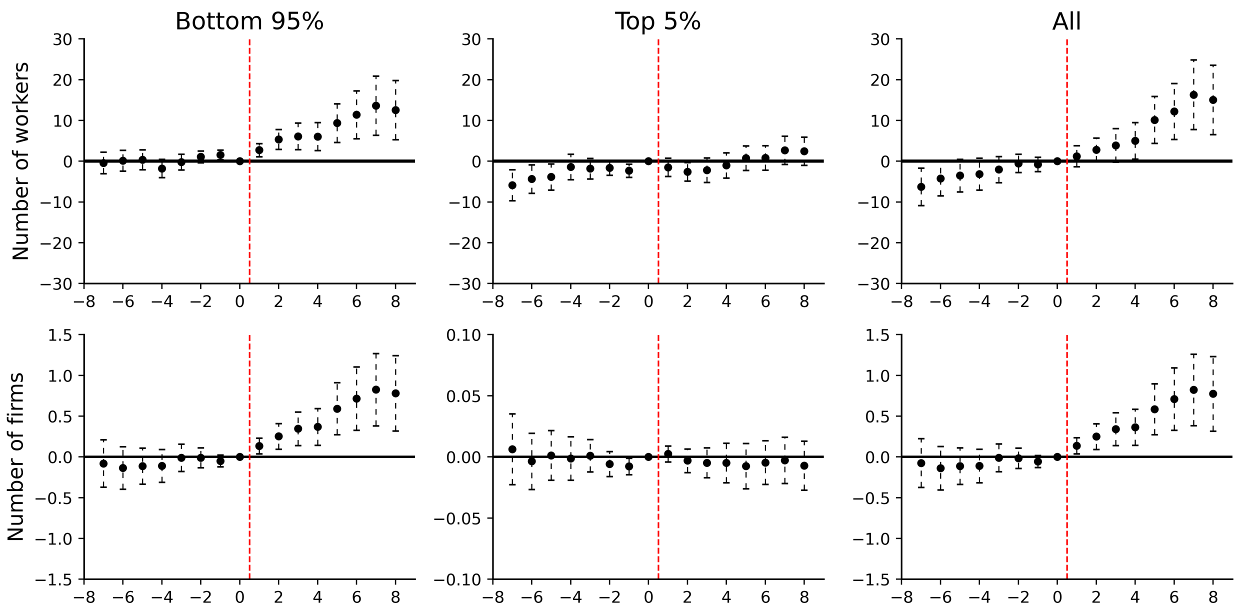
Using these statistics, I construct a standardized measure:

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

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.

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.

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

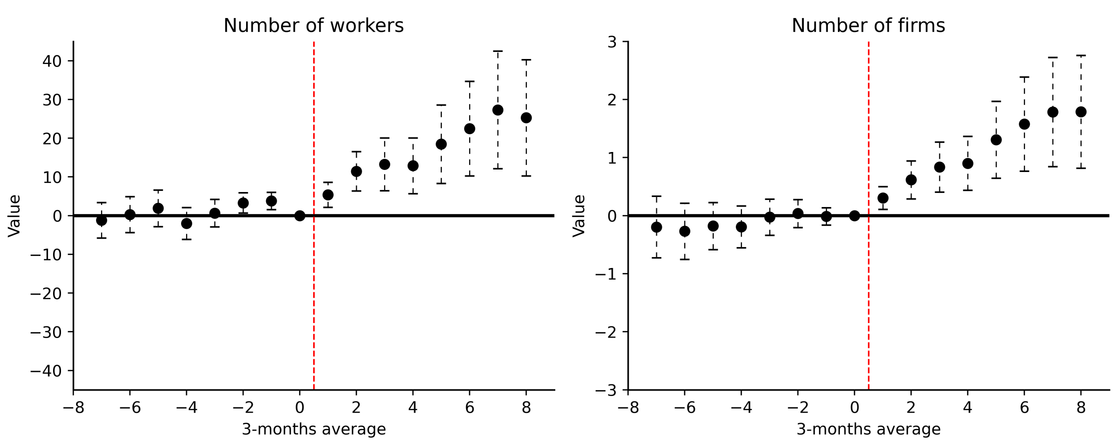
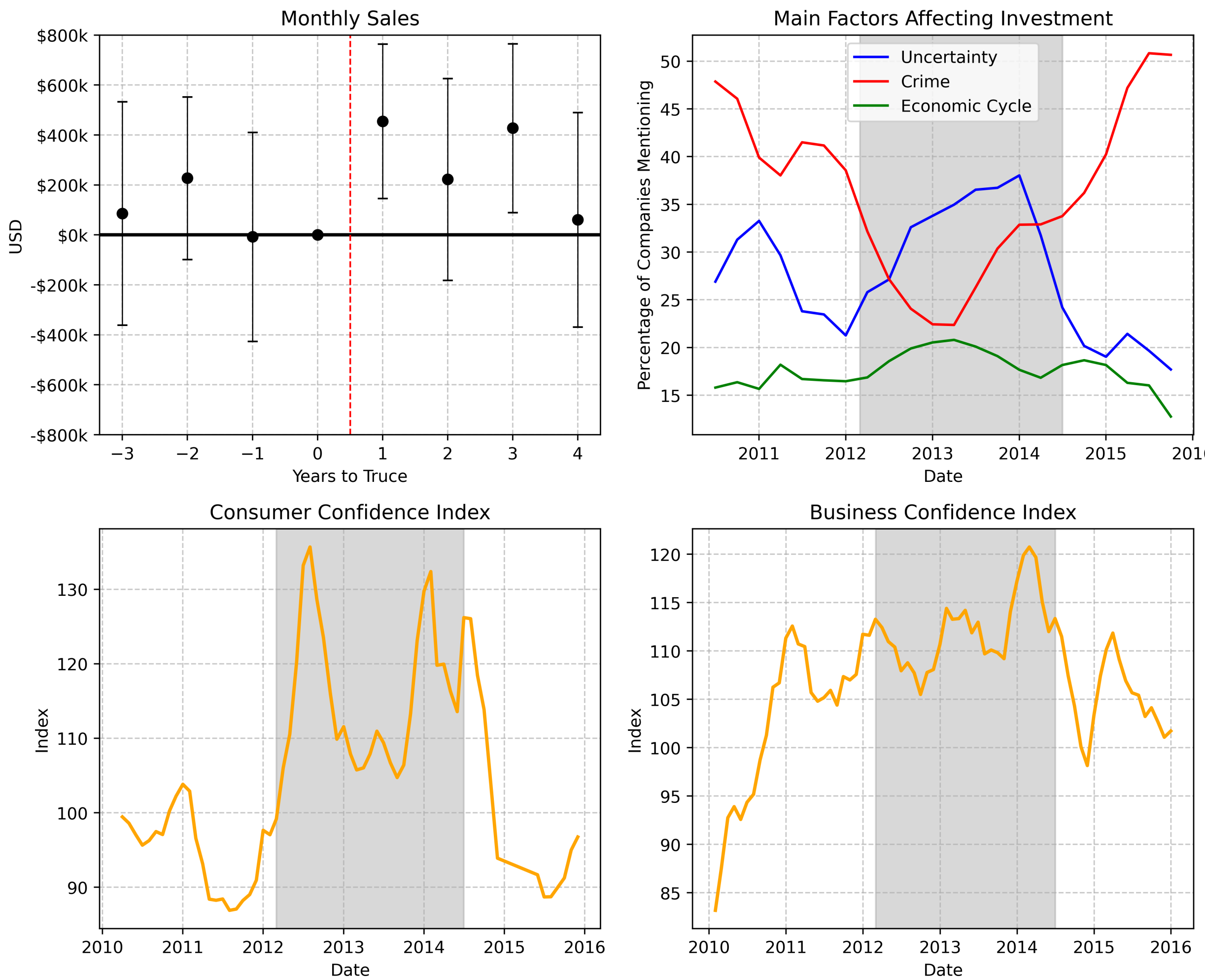


Table A.2: Impact of Crime Reduction on Business Closures and Openings by Treatment Intensity

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	

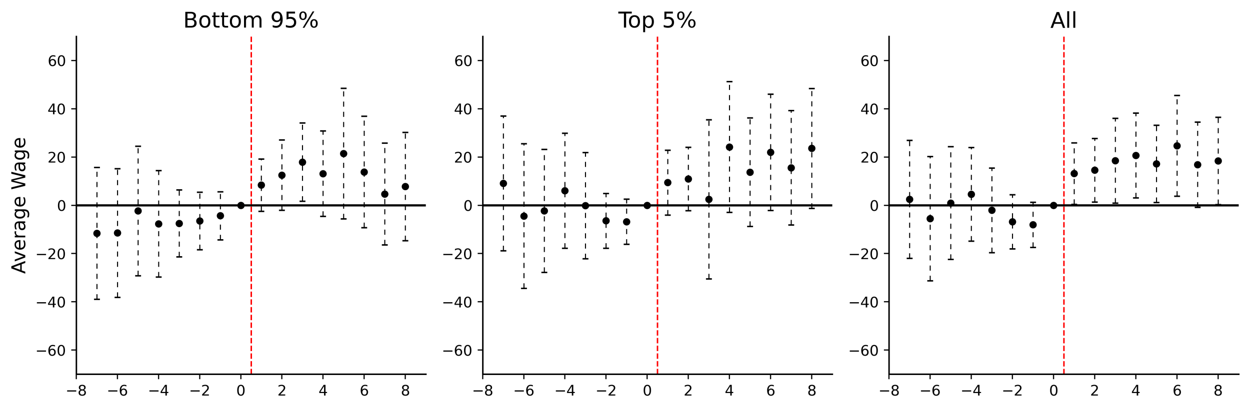
Notes: This table reports the effects of crime reduction on the business closures and openings, broken down by treatment intensity thresholds. The "Treatment" row presents the baseline results, while the lower, middle, and upper thresholds correspond to increasingly higher crime reduction intensity categories. 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



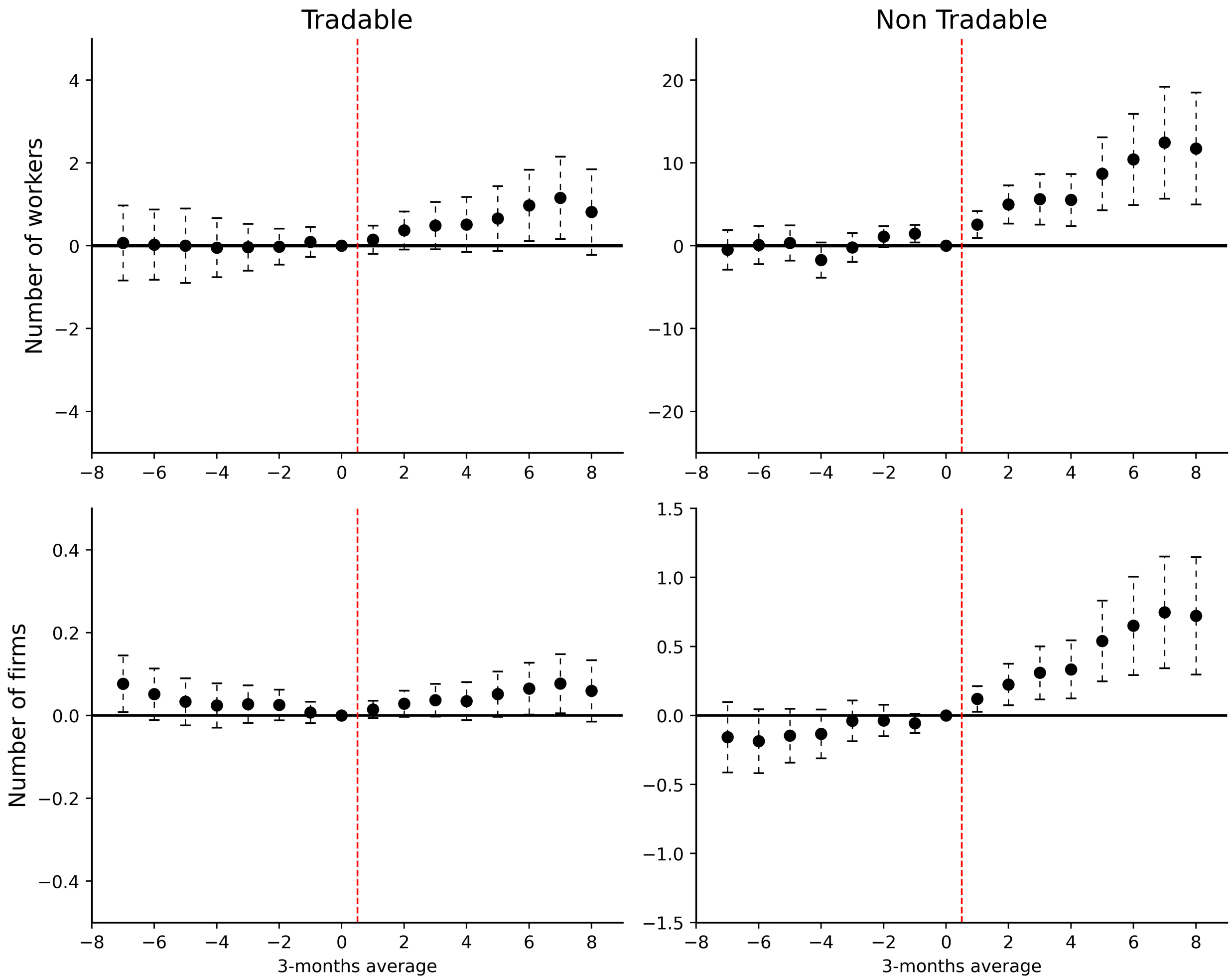
Notes: This Event Study focuses exclusively on the treatment group selected during the truce period. The X-axis consolidates data into three-month intervals, depicting trends for 24 months both before and after the truce to illustrate longitudinal effects. The sample is restricted to polygons that have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

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.

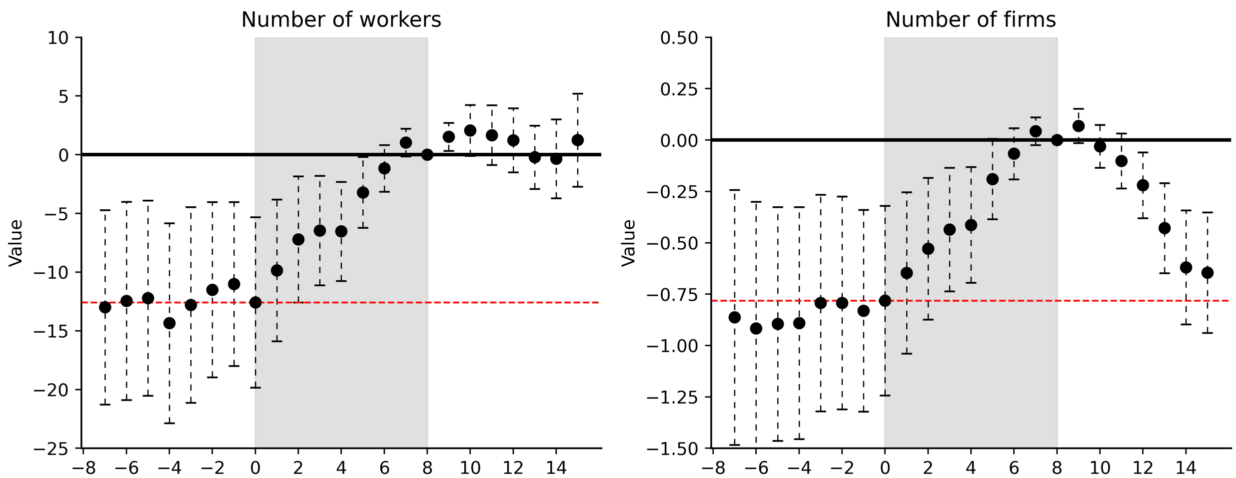
Table A.4: Spillover Effects Using Gang-Related Homicide Definition

Variables	**Workers**	**Workers**	**Workers**	**Number of firms**	**Number of firms**	**Number of firms**
	Coef/SE	Coef/SE	Coef/SE	Coef/SE	Coef/SE	Coef/SE
Treatment	6.906*	6.394*	6.908*	0.862***	0.828***	0.862***
	(3.739)	(3.676)	(3.741)	(0.243)	(0.239)	(0.243)
Spillover - neighbor		0.730**			0.048*	
		(0.299)			(0.029)	
Spillover - distance (km)			0.176***			0.014***
			(0.054)			(0.005)
Obs.	25296	25296	25296	25296	25296	25296

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

Notes: This table presents spillover effect estimates using the alternative treatment definition based on pre-truce gang-related homicides. Estimates are derived from a two-way fixed effects (TWFE) model with differentiated linear time trends for units, following the methodology in Borusyak et al. (2024). Errors are determined using a block bootstrap method. Spillover – neighbor is a binary variable equal to one if a given unit shares a border with one treated unit (i.e. an adjacent "neighbor"). Spillover – distance (km) measures the straight-line distance, in kilometers, from each unit to its nearest treated unit.

Figure A.6: 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). The graph features a shaded gray area to delineate the period of the truce between gangs.

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:

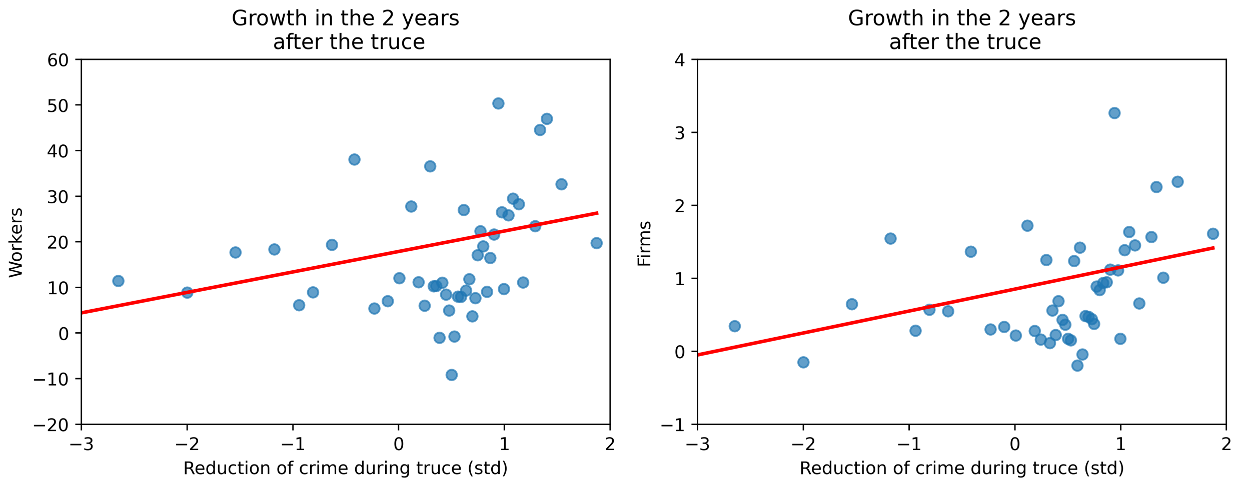
$$Y_{ist} = \alpha + \tau_0 D_{ist} + \tau_1 D_{ist} \mathbb{1}\{s \in Truce\} + \gamma_{is} + \lambda_{ts} + \epsilon_{ist} \tag{3}$$

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.

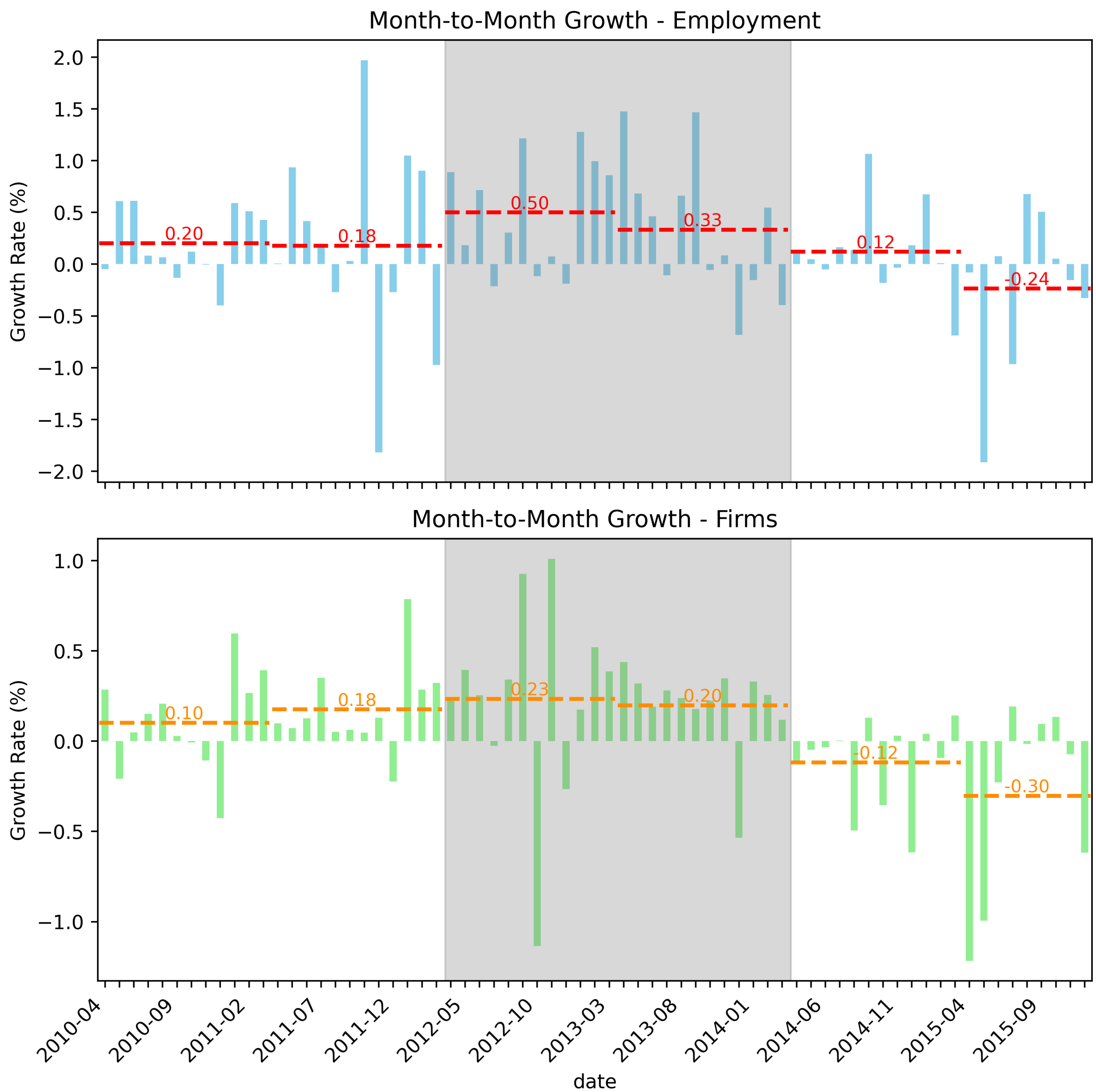
B.2 Figures and Tables

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



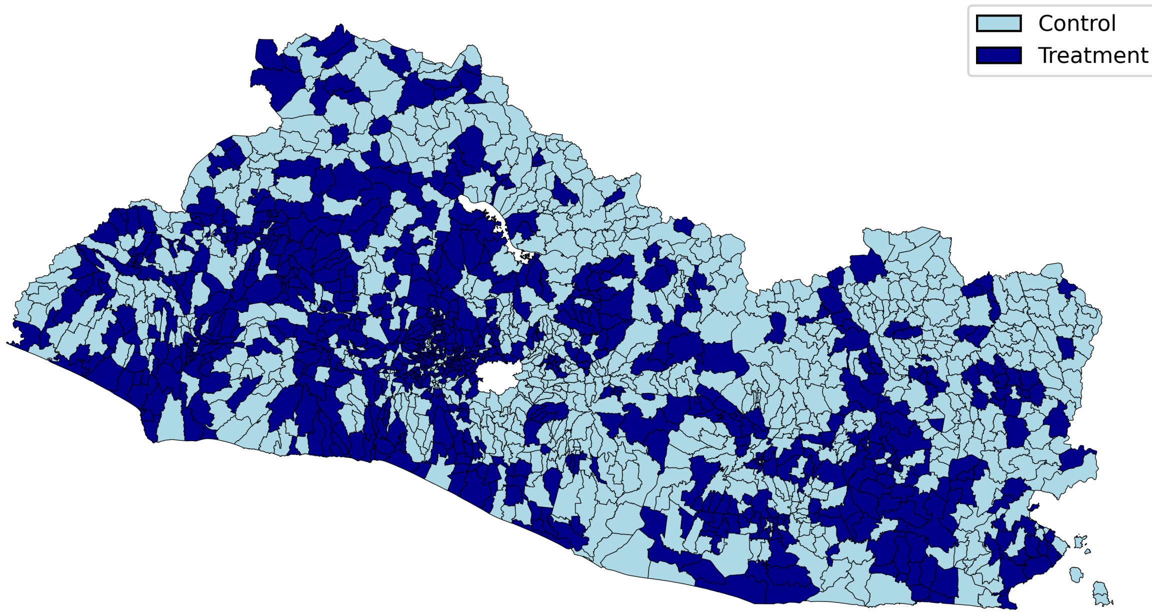
Notes: This figure presents the relationship between the reduction in crime during the truce and two key economic indicators: employment (left panel) and firm presence (right panel). Each point represents a bin of 50 observations, with a fitted linear regression line shown in red. The left panel illustrates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



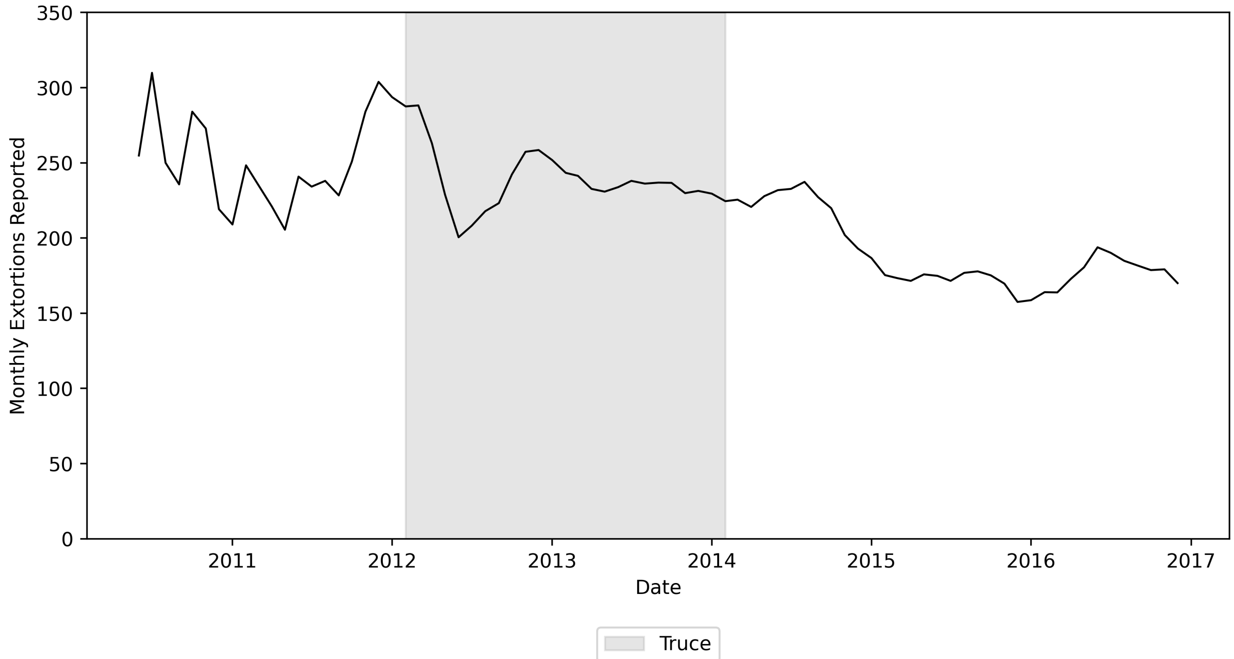
Notes: This figure presents the monthly percentage growth of employment (top panel) and firms (bottom panel) over the study period. Each bar represents the month-to-month growth rate, while the dashed red and orange lines indicate the average growth rates for specific periods. The shaded gray area highlights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



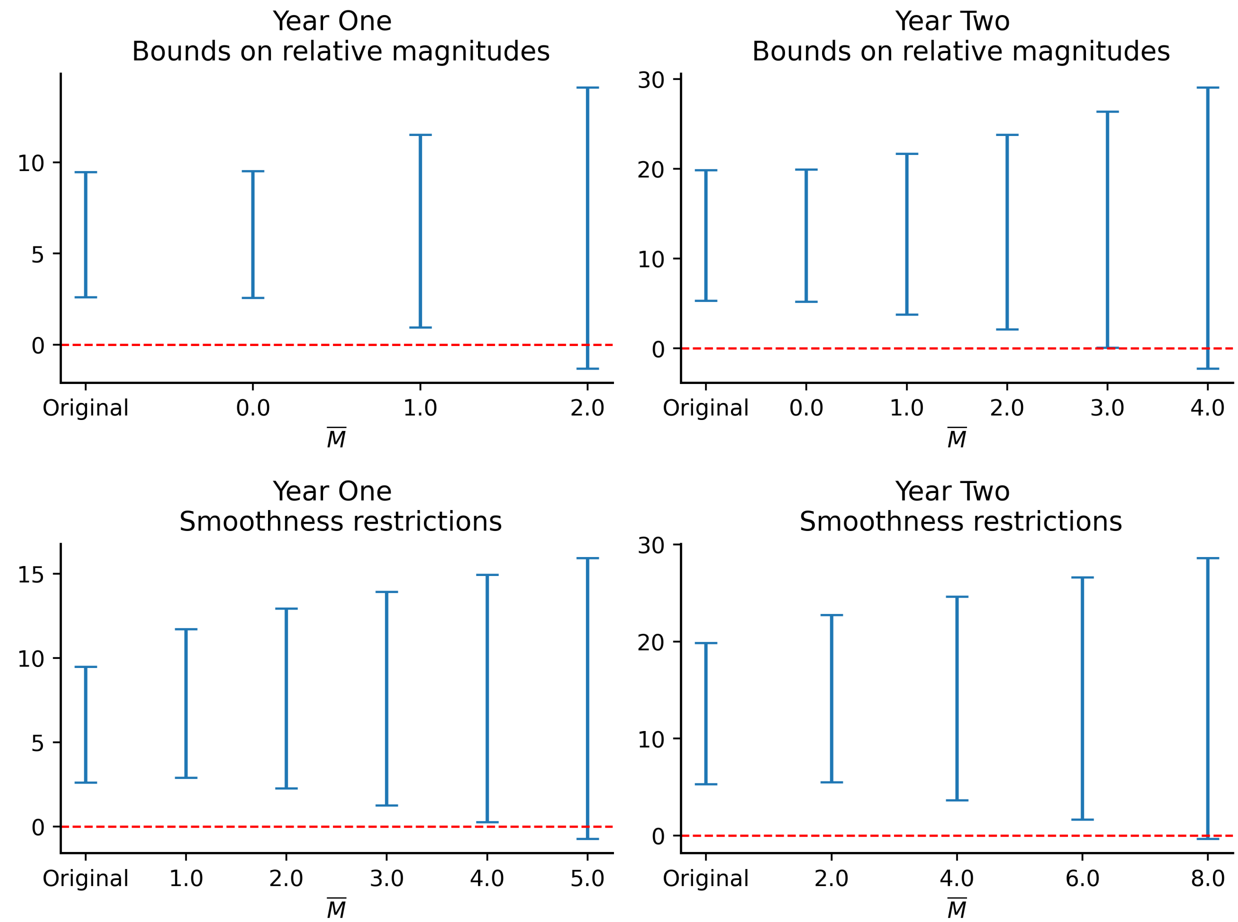
Notes: This graph illustrates the trend in monthly reported extortions from 2011 to 2017, with a specific focus on the truce period highlighted in grey. Despite the implementation of the truce, aimed at reducing gang violence, the data indicates that extortion rates remained relatively stable throughout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

Table B.1: Employment to Population Ratio Over Time using WDI data

Region	Year-by-Year								Summary		
	2008	2009	2010	2011	2012	2013	2014	2015	Pre-Truce	Truce	Difference
El Salvador	59.02	58.16	58.10	58.55	59.40	59.88	58.40	57.77	58.46	59.22	-0.77
LATAM and Caribbean (excl. high income)	60.90	60.16	60.16	60.00	60.24	60.09	59.63	59.67	60.31	59.99	0.32
Difference	-1.88	-2.00	-2.06	-1.45	-0.84	-0.21	-1.23	-1.90	-1.85	-0.76	-1.09

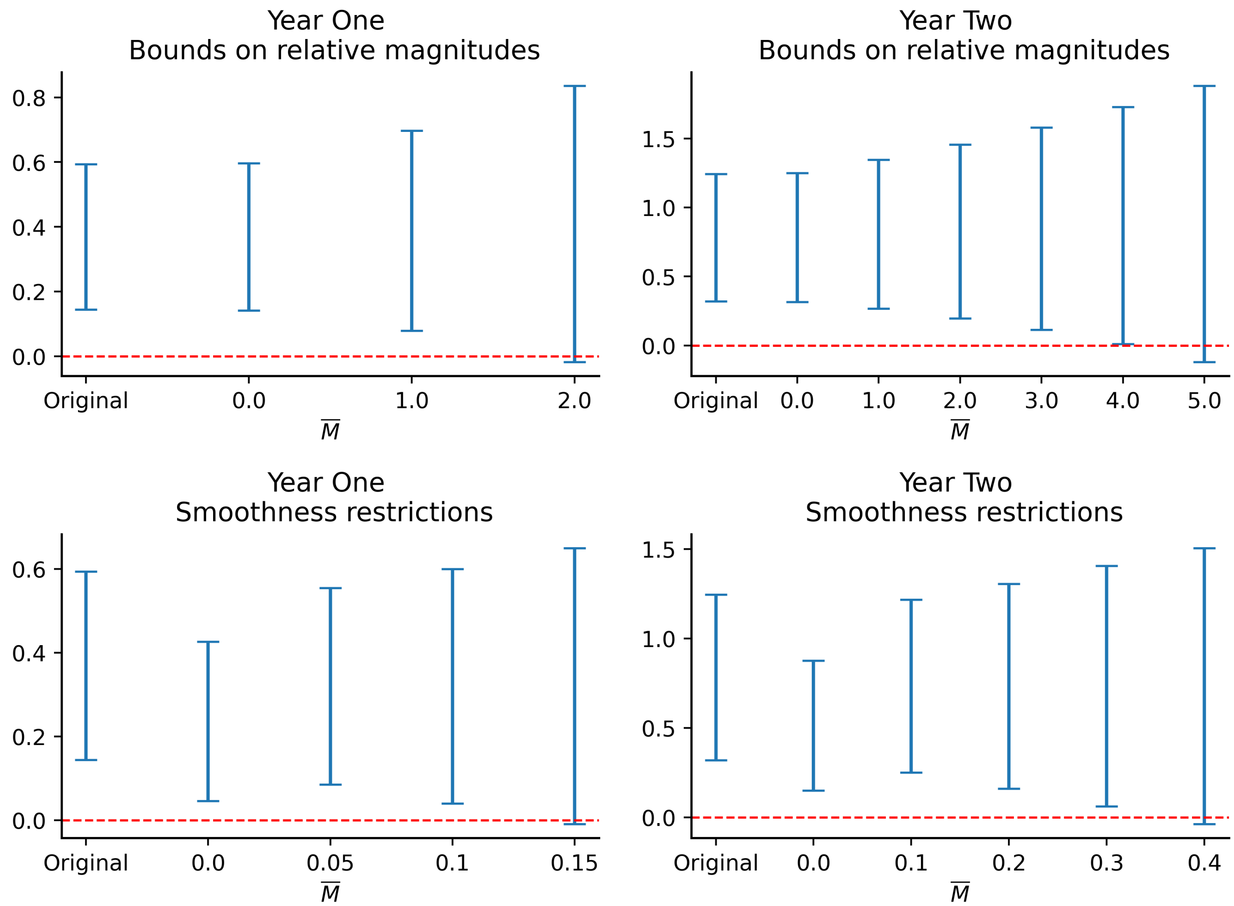
Notes: This table presents employment rate for the period 2009–2015, using data from the World Development Indicators (WDI), which are based on household surveys. The employment-to-population ratio (15+, total %), is the proportion of the working-age population engaged in employment each year. The truce period spans 2012–2014, and the pre-truce period corresponds to the earlier years shown in the table (2008–2011).

Figure B.5: Robust Confidence Intervals for the Effect on the Number of Workers: Sensitivity to Violations of Parallel Trends



*Notes: This figure reports robust 95% confidence intervals for the estimated treatment effects at year 1 and year 2 post-truce using the methodology developed by Rambachan and Roth (2023). The top panels present results under **relative magnitude restrictions**, where $\overline{M}$ denotes the maximum size of post-treatment deviations from parallel trends relative to the worst violation observed pre-treatment. For example, $\overline{M} = 2$ allows post-treatment violations up to twice as large as the maximum observed pre-treatment deviation. The bottom panels present results under **smoothness restrictions**, where $\overline{M}$ represents the maximum allowed change in the slope of the trend across consecutive post-treatment periods. In all panels, the label "Original" corresponds to the confidence interval under the exact parallel trends assumption.*

Figure B.6: Robust Confidence Intervals for the Effect on the Number of Firms: Sensitivity to Violations of Parallel Trends

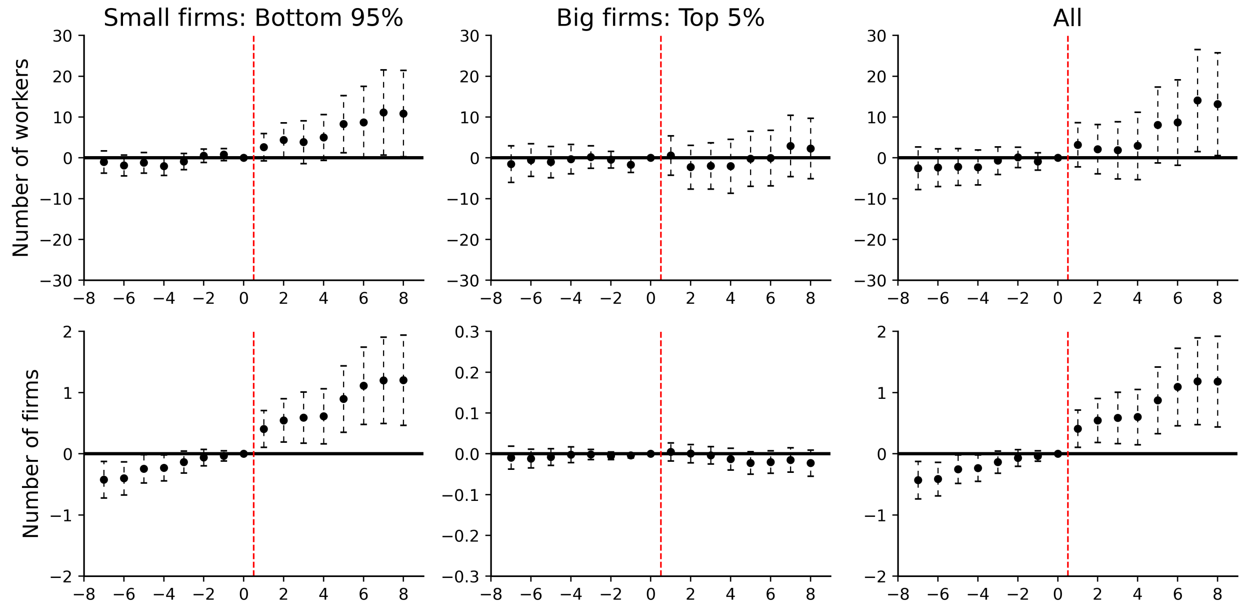


*Notes: This figure reports robust 95% confidence intervals for the estimated treatment effects at year 1 and year 2 post-truce using the methodology developed by Rambachan and Roth (2023). The top panels present results under **relative magnitude restrictions**, where $\overline{M}$ denotes the maximum size of post-treatment deviations from parallel trends relative to the worst violation observed pre-treatment. For example, $\overline{M} = 2$ allows post-treatment violations up to twice as large as the maximum observed pre-treatment deviation. The bottom panels present results under **smoothness restrictions**, where $\overline{M}$ represents the maximum allowed change in the slope of the trend across consecutive post-treatment periods. In all panels, the label "Original" corresponds to the confidence interval under the exact parallel trends assumption.*

Figure B.7: Event-Study Results for Different Treatment-Intensity Thresholds

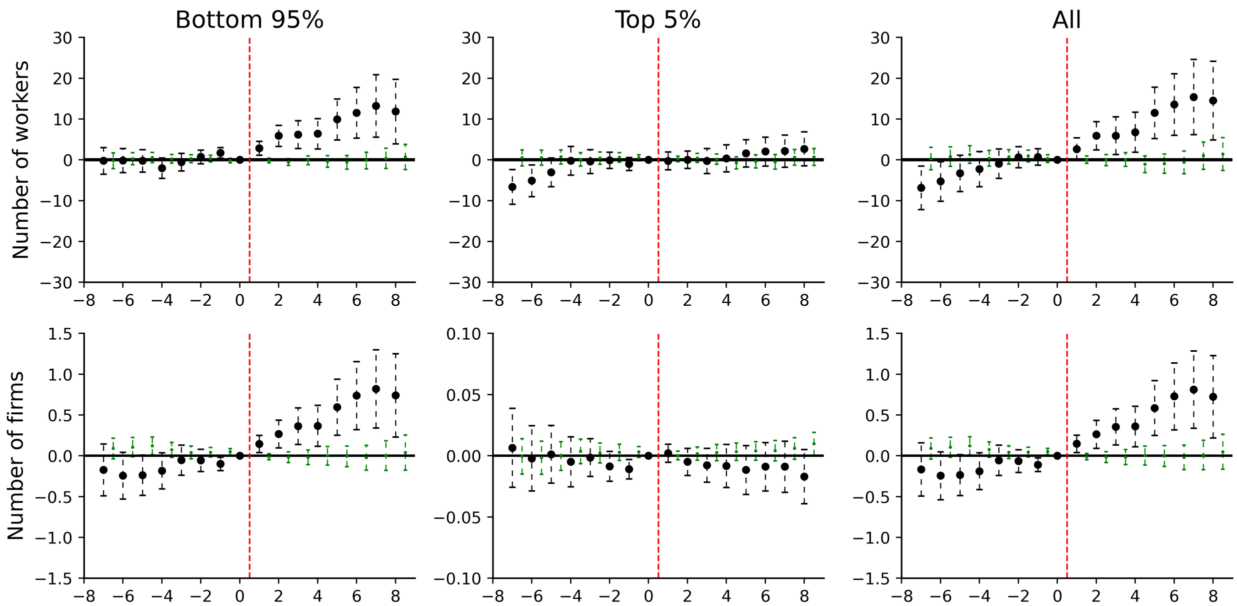
Notes: This Event Study focuses exclusively on the treatment group selected during the truce period. The X-axis consolidates data into three-month intervals, depicting trends for 24 months both before and after the truce to illustrate longitudinal effects.

Figure B.8: 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.9: 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.2: 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	

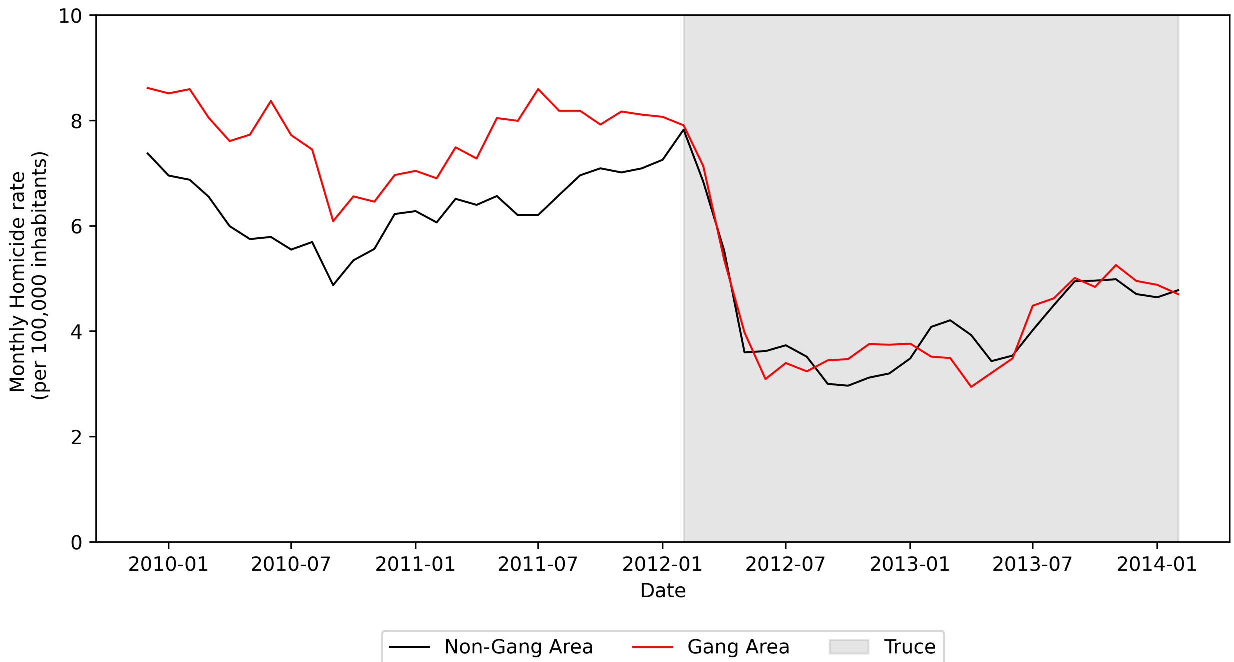
Notes: This table presents regression results for the impact of the truce on workers, and number of firms, across small and medium firms (bottom 95%). Coefficients and their respective standard errors, shown in parentheses, indicate the estimated effects. The errors were clustered at the level of geographic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

Table B.3: 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	6.38	20.77	7.61	14.87
Homicides Rate (per 100k hab.) after truce	4.1	17.98	4.09	11.35
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.10: 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.4: 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	6.90**	(3.56%)	0.86***	(3.03%)
	(3.75)		(0.24)	
Obs.	25296		25296	
		Big firms: Top 5%		
Treatment	-0.06	(-0.04%)	-0.01	(-0.58%)
	(2.64)		(0.01)	
Obs.	25296		25296	
		All firms		
Treatment	6.85*	(2.10%)	0.85***	(2.83%)
	(4.62)		(0.25)	
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.

Table B.5: 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.

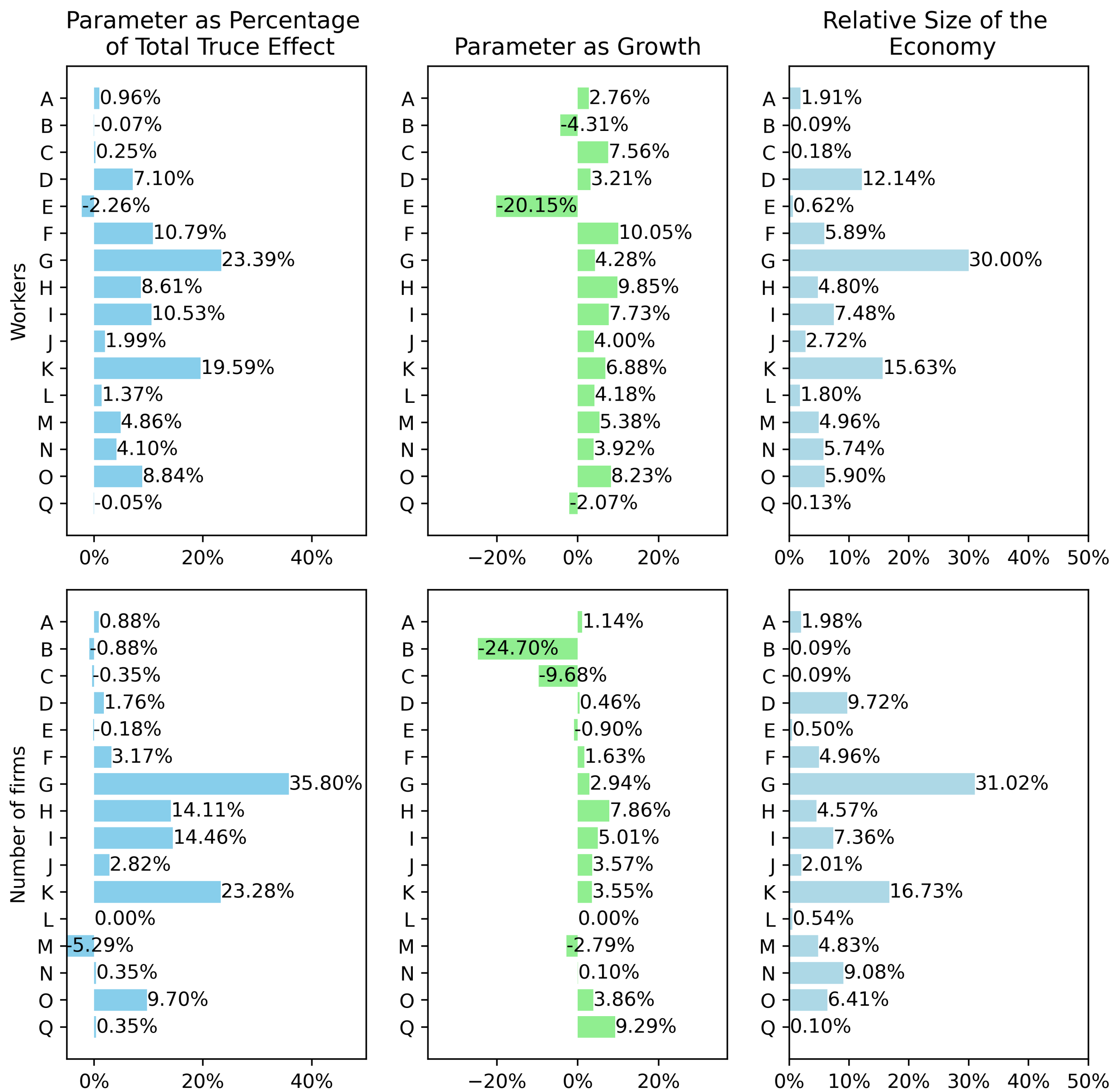
Table B.6: Effect of the truce: Placebo Triple-Differences Estimates by Treatment Intensity

Variables	**Treatment**	**Lower Threshold**	**Middle Threshold**	**Upper Threshold**
		Workers		
Treatment (Placebo)	-0.275	0.363	1.830	3.638***
	(0.872)	(0.952)	(1.142)	(1.389)
Treatment x Truce	8.588***	10.573***	12.239**	16.613*
	(2.682)	(3.386)	(4.787)	(8.862)
R-squared	0.9968	0.9968	0.997	0.9968
Obs.	126480	111024	95616	80256
		Number of firms		
Treatment (Placebo)	-0.070	-0.011	0.104	0.164
	(0.079)	(0.085)	(0.101)	(0.124)
Treatment x Truce	0.636***	0.814***	0.921***	1.283**
	(0.200)	(0.244)	(0.333)	(0.559)
R-squared	0.9988	0.9989	0.9989	0.9989
Obs.	126480	111024	95616	80256

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

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 "Treatment" column presents the baseline results, while the lower, middle, and upper thresholds correspond to increasingly higher crime reduction intensity categories. 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.11: Sectoral Growth and Economic Proportions During the Truce



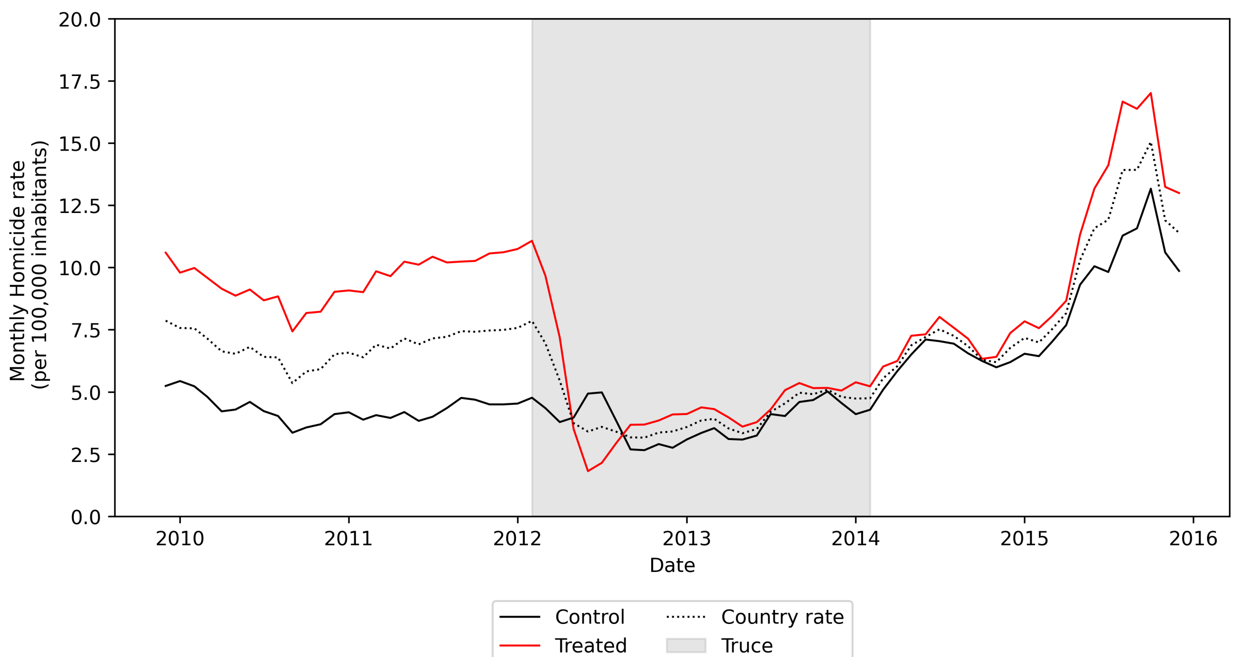
Notes: This graph presents a three-part analysis based on different sectors as classified by the CIIU Rev. 3 standard. The first column represents baseline regression estimates, calculated using sector-specific outcome variables to assess the impact of the truce on each sector. The second column indicates 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.

Table B.7: Spillover Effects defining Treatment Based on Declines in Homicide Rates During the Truce Period

Variables	**Workers**	**Workers**	**Workers**	**Number of firms**	**Number of firms**	**Number of firms**
	Coef/SE	Coef/SE	Coef/SE	Coef/SE	Coef/SE	Coef/SE
Treatment	8.313***	7.609***	4.526**	0.566***	0.493***	0.249
	(2.537)	(2.530)	(2.218)	(0.184)	(0.187)	(0.182)
Spillover - neighbor		4.505***			0.466***	
		(1.128)			(0.108)	
Spillover - distance (km)			2.620***			0.219***
			(0.509)			(0.041)
Obs.	25296	25296	25296	25296	25296	25296

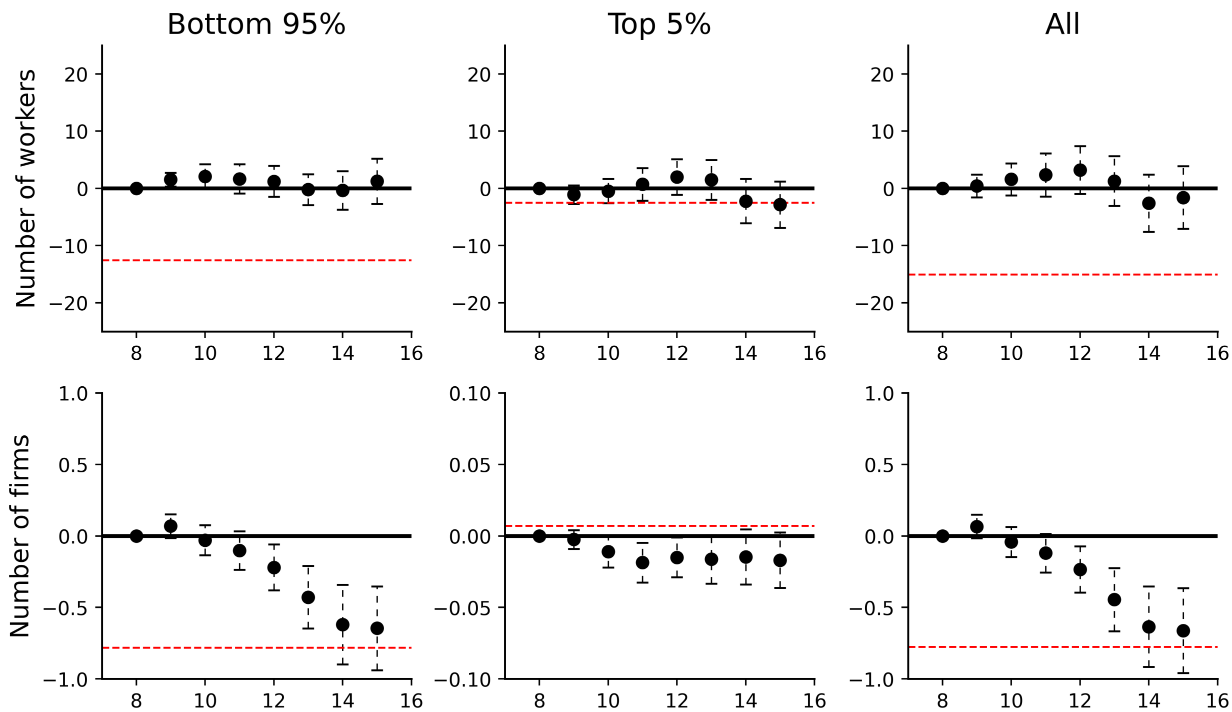
$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

Figure B.12: Homicides rates in Treated Areas and Control Areas after truce collapsed



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.

Figure B.13: 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.3 Theoretical Framework

This appendix develops a simple model to organize the main mechanisms behind the empirical results. The model considers a static economy with a representative household (unit mass), competitive firms, and two geographic zones $z \in \{H, L\}$ that differ in local crime. For tractability, I abstract from capital and from trade in tradables, and focus on the production and consumption of a single non–tradable good. This choice reflects the empirical evidence: changes in employment concentrate in the non–tradable sector and wages adjust locally.

B.3.1 Environment and Agents

There is a representative household of unit measure. Individuals have heterogeneous reservation wages κ drawn from a continuous distribution $F(\cdot)$ with support $[0, \bar{\kappa}]$. An individual participates in the labor force iff the market wage w is at least as large as their reservation wage, so aggregate labor supply is $L_S(w) = F(w)$.

Each participant earns wage income w; all individuals also receive a minimal outside income $b \geq 0$ (transfers, remittances, or intra–household pooling). Hence per–capita income is

$$\bar{I}(w) \;=\; b \;+\; w\,F(w).$$

B.3.2 Goods and Crime

The economy produces a single non–tradable good N in two zones $z \in \{H, L\}$, which differ in local crime $c_z \in \mathbb{R}_+$, with $c_H > c_L \geq 0$. Crime does not affect firms' technologies but enters the consumer side as an additive "fear cost" (or disutility) per unit purchased in that zone. Formally, the effective consumer price in zone z is

$$\tilde{p}_z \;=\; p + \chi(c_z),$$

where p is the producer price and $\chi : \mathbb{R}_+ \to \mathbb{R}_+$ is continuous and strictly increasing, $\chi'(c) > 0$.

B.3.3 Firms and Technology

Production is competitive in each zone. Firms hire labor ℓ_z to produce output y_z with linear technology,

$$y_z \;=\; A\,\ell_z, \qquad A > 0,$$

with A common across zones and unaffected by crime. Perfect competition and zero profits imply the common producer price equals unit cost:

$$p \;=\; \frac{w}{A}.$$

B.3.4 Preferences, Demand, and Price Index

Preferences. The representative household allocates expenditure across zones to maximize

$$U \;=\; \Big(\sum_{z \in \{H,L\}} \alpha_z\, q_z^{\frac{\sigma-1}{\sigma}} \Big)^{\frac{\sigma}{\sigma-1}}, \qquad \alpha_z > 0,\ \sum_z \alpha_z = 1,\ \ \sigma > 1,$$

where σ is the elasticity of substitution across zones and α_z are preference weights.

Effective prices and budget. Let P_z denote the effective consumer price in zone z:

$$P_z \;=\; p + \chi(c_z) \;=\; \frac{w}{A} + \chi(c_z),$$

and let I denote total expenditure. The budget constraint is $\sum_z P_z q_z = I$.

Optimal demand. The first–order conditions for $\{q_z\}$ yield the standard CES demand system. The CES (across–zone) price index is

$$\mathcal{P} \;=\; \Big(\sum_z \alpha_z\, P_z^{1-\sigma}\Big)^{\frac{1}{1-\sigma}},$$

and optimal expenditure shares are

$$s_z \;=\; \frac{P_z q_z}{I} \;=\; \frac{\alpha_z\, P_z^{1-\sigma}}{\sum_k \alpha_k P_k^{1-\sigma}}, \qquad \sum_z s_z = 1.$$

Marshallian demands are therefore

$$q_z \;=\; \frac{s_z\, I}{P_z} \;=\; \frac{\alpha_z\, P_z^{-\sigma}}{\sum_k \alpha_k P_k^{1-\sigma}}\; I.$$

B.3.5 Income, Technology, and Prices

Given $F(w)$, per–capita income is $\bar{I}(w) = b + wF(w)$. With linear technology $y_z = A\ell_z$ and perfect competition, $p = w/A$, so the effective consumer price in zone z is

$$P_z(w, c_z) \;=\; \frac{w}{A} + \chi(c_z).$$

B.3.6 Market Clearing and Labor Demand

Because the good is non–tradable, local goods markets clear by zone: $y_z = q_z$, hence $\ell_z = q_z/A$. Aggregating, total labor demand generated by consumption is

$$L_D(w; c_H, c_L) \;=\; \sum_z \ell_z \;=\; \frac{1}{A}\sum_z q_z \;=\; \frac{\bar{I}(w)}{A}\,\frac{\sum_z \alpha_z\, P_z(w, c_z)^{-\sigma}}{\sum_k \alpha_k\, P_k(w, c_k)^{\,1-\sigma}}.$$

Labor market clearing requires $L_D(w; c_H, c_L) = L_S(w) = F(w)$, i.e.

$$\boxed{\frac{b + wF(w)}{A} \; \frac{\displaystyle\sum_{z\in\{H,L\}} \alpha_z \left(\frac{w}{A} + \chi(c_z)\right)^{-\sigma}}{\displaystyle\sum_{k\in\{H,L\}} \alpha_k \left(\frac{w}{A} + \chi(c_k)\right)^{1-\sigma}} \;=\; F(w).} \tag{E}$$

Standing assumptions. Unless otherwise stated, I assume: (i) F is continuous, non–decreasing on $[0, \bar{\kappa}]$ with $F(0) = 0$, $F(\bar{\kappa}) = 1$; (ii) χ is continuous and strictly increasing with $\chi'(c) > 0$; (iii) $\sigma > 1$ and $\alpha_z > 0$; (iv) $A > 0$ and $b \geq 0$. These regularities guarantee continuity and monotonicity properties used below to establish existence (and sufficient conditions for uniqueness) of $w^\star$ solving (E).

B.3.7 Competitive Equilibrium

Given crime levels (c_H, c_L), a competitive equilibrium is a tuple

$$\left\{w,\, p,\, (P_z, q_z, y_z, \ell_z)_{z\in\{H,L\}}\right\}$$

such that: (i) households maximize utility subject to $\bar{I}(w) = b + wF(w)$ and prices (P_H, P_L), yielding demands q_z; (ii) firms maximize profits with technology $y_z = A\ell_z$, implying $p = w/A$; (iii) goods markets clear, $y_z = q_z$; and (iv) the labor market clears, $\sum_z \ell_z = F(w)$.

B.3.8 Existence and Uniqueness

Equilibrium requires a wage $w^\star$ solving the labor market clearing condition

$$L_D(w; c_H, c_L) = F(w).$$

Lemma B.3.1 (Existence) *Suppose $F : [0, \bar{\kappa}] \to [0, 1]$ is continuous, non–decreasing with $F(0) = 0$ and $F(\bar{\kappa}) = 1$, and $\chi(\cdot)$ is continuous and strictly increasing. Then there exists at least one $w^\star \in [0, \bar{\kappa}]$ such that $L_D(w^\star; c_H, c_L) = F(w^\star)$.*

Lemma B.3.2 (Uniqueness) *Suppose F is strictly increasing and Lipschitz continuous, and that*

$$\sup_{w\in[0,\bar{\kappa}]} \left| \frac{dL_D(w; c_H, c_L)}{dw} \right| < \inf_{w\in[0,\bar{\kappa}]} F'(w).$$

Then the equilibrium wage $w^\star$ is unique.

Proof. Define $g(w) = L_D(w; c_H, c_L) - F(w)$. By continuity, g is continuous on $[0, \bar{\kappa}]$. The slope condition ensures $g'(w) < 0$, so g is strictly decreasing. Thus g can cross zero at most once. Existence guarantees at least one crossing, hence the solution is unique. ■

B.3.9 Equilibrium Objects

Given $w^\star$ solving (E), equilibrium allocations and prices are

$$p^\star = \frac{w^\star}{A}, \qquad P_z^\star = \frac{w^\star}{A} + \chi(c_z), \qquad q_z^\star = \frac{\alpha_z \,(P_z^\star)^{-\sigma}}{\sum_k \alpha_k \,(P_k^\star)^{\,1-\sigma}}\; \bar{I}(w^\star),$$

$$\ell_z^\star = \frac{q_z^\star}{A}, \qquad y_z^\star = q_z^\star, \qquad n^\star = F(w^\star).$$

B.3.10 Comparative Statics: Detailed Sign Derivations

Assume $\chi(c) = \beta c$ with $\beta > 0$, $F(w) = w/\bar{\kappa}$ on $[0, \bar{\kappa}]$, $\sigma > 1$, and $b \geq 0$. Then

$$P_z(w, c_z) = \frac{w}{A} + \beta c_z, \qquad \bar{I}(w) = b + \frac{w^2}{\bar{\kappa}}.$$

Let

$$S_{1-\sigma} \equiv \sum_{k \in \{H,L\}} \alpha_k P_k^{1-\sigma}, \qquad s_z = \frac{\alpha_z P_z^{1-\sigma}}{S_{1-\sigma}},$$

so total labor demand can be written as

$$L_D(w; c_H, c_L) = \frac{\bar{I}(w)}{A} \cdot \frac{\sum_z \alpha_z P_z^{-\sigma}}{\sum_k \alpha_k P_k^{1-\sigma}} = \frac{\bar{I}(w)}{A} \sum_z s_z \frac{1}{P_z}. \qquad \text{(LD-avg)}$$

Step 1. $\partial s_H / \partial c_H < 0$ (holding w fixed). Since $s_z \propto P_z^{1-\sigma}$ with $\sigma > 1$,

$$\frac{\partial s_H}{\partial \ln P_H} = (1-\sigma)\, s_H (1 - s_H) < 0, \qquad \frac{\partial \ln P_H}{\partial c_H} = \frac{\beta}{P_H} > 0,$$

hence $\boxed{\partial s_H / \partial c_H < 0}$ at fixed w. *Interpretation:* When crime rises in H, its effective price increases, and consumers reallocate expenditure away from H.

Step 2. $\partial L_D/\partial c_H < 0$ **(holding w fixed).** Differentiating (LD-avg) at fixed w,

$$\frac{\partial L_D}{\partial c_H} = \frac{\bar{I}(w)}{A}\left[\sum_z \frac{\partial s_z}{\partial c_H}\frac{1}{P_z} + s_H\,\frac{\partial(1/P_H)}{\partial c_H}\right].$$

Using $s_L = 1 - s_H$,

$$\sum_z \frac{\partial s_z}{\partial c_H}\frac{1}{P_z} = \frac{\partial s_H}{\partial c_H}\left(\frac{1}{P_H} - \frac{1}{P_L}\right), \qquad \frac{\partial(1/P_H)}{\partial c_H} = -\frac{\beta}{P_H^2}.$$

With $P_H \geq P_L$, the reallocation term is weakly positive, while the direct term is strictly negative. A sufficient condition for the direct effect to dominate is

$$\boxed{\frac{\beta\, s_H}{P_H^2} > \left|\frac{\partial s_H}{\partial c_H}\right| \cdot \left|\frac{1}{P_H} - \frac{1}{P_L}\right|},$$

which implies $\boxed{\partial L_D/\partial c_H < 0}$ at fixed w.

Interpretation: Crime in H reduces aggregate labor demand through two forces: (i) a direct price effect that lowers demand in H, and (ii) a reallocation effect that increases demand in L. The sufficient condition requires the direct negative effect to dominate reallocation. This is easily satisfied when β is large, s_H is non-negligible, and the substitution margin (via σ or a large price gap between H and L) is not too strong. In economic terms: if crime substantially raises effective prices in a zone where consumers already spend a meaningful share, then more crime unambiguously reduces aggregate labor demand, even accounting for substitution toward safer zones.

Step 3. $dw^\star/dc_H < 0$. Let $G(w, c_H) \equiv L_D(w; c_H, c_L) - F(w)$. At $w^\star$, $G(w^\star, c_H) = 0$. By the implicit function theorem,

$$\frac{dw^\star}{dc_H} = -\frac{\partial G/\partial c_H}{\partial G/\partial w} = -\frac{\dfrac{\partial L_D}{\partial c_H}}{\dfrac{\partial L_D}{\partial w} - F'(w)}.$$

Under uniqueness, the denominator is negative; by Step 2, the numerator is negative. Hence $\boxed{dw^\star/dc_H < 0}$.

Interpretation: When crime falls in H, labor demand shifts outward. To clear the market, the equilibrium wage must rise.

Step 4. $ds_H/dc_H < 0$**.** From $s_H = \alpha_H P_H^{1-\sigma}/S_{1-\sigma}$ and $d\ln S_{1-\sigma} = \sum_k (1-\sigma)s_k \, d\ln P_k$,

$$\frac{ds_H}{dc_H} = s_H(1-\sigma)\left[\frac{\beta}{P_H}(1-s_H) + \frac{1}{A}\Big(\frac{1}{P_H} - \sum_k s_k \frac{1}{P_k}\Big)\frac{dw}{dc_H}\right].$$

Since $\sigma > 1$, the outside factor $(1-\sigma) < 0$. The bracket is positive because: (i) the direct term $\frac{\beta}{P_H}(1-s_H) > 0$, and (ii) $\frac{dw}{dc_H} < 0$ while $\frac{1}{P_H} - \sum_k s_k(1/P_k) \leq 0$, so their product is non-negative. Thus $\boxed{ds_H/dc_H < 0}$.

Interpretation: Higher crime in H reduces its expenditure share both directly (by raising its price) and indirectly (via wage effects). Hence, under a truce, s_H increases: households shift more of their budget toward the safer H zone.

Step 5. $dq_H^\star/dc_H < 0$ **and** $dq_L^\star/dc_H \geq 0$**.** Since $q_z = s_z \bar{I}(w)/P_z$,

$$d\ln q_z = d\ln s_z + d\ln \bar{I} - d\ln P_z.$$

(A) Zone H. The three terms are: (i) $d\ln s_H/dc_H < 0$ (Step 4), (ii) $d\ln \bar{I}/dc_H < 0$ because wages fall with crime, and (iii) $d\ln P_H/dc_H = (\beta + (1/A)\, dw/dc_H)/P_H$, typically positive if the crime premium β dominates the wage effect. Subtracting this positive term reinforces the decline. Hence $\boxed{dq_H^\star/dc_H < 0}$.

Interpretation: More crime in H reduces local quantity demanded through three channels: lower expenditure share, lower incomes, and higher effective prices. A truce ($dc_H < 0$) reverses these forces, increasing q_H.

(B) Zone L. Here, $d\ln s_L/dc_H = -d\ln s_H/dc_H > 0$, $d\ln \bar{I}/dc_H < 0$, and $-d\ln P_L/dc_H > 0$ because $dw/dc_H < 0$ lowers P_L. Thus two forces (reallocation and cheaper local price) are positive, while the income channel is negative. The net effect is weakly positive, giving $\boxed{dq_L^\star/dc_H \geq 0}$.

Interpretation: As crime rises in H, consumers reallocate spending toward L and also face lower prices in L (via lower wages). Both increase q_L, though the negative income effect partly offsets them. Under a truce, q_L weakly decreases.

B.3.11 Participation vs. Reallocation of Employment

I decompose the effect of a crime reduction in H into two channels: (i) a *reallocation effect* (crime affects relative prices and shares at fixed wages), and (ii) a *participation effect* (through the equilibrium wage response dw/dc_H).

A. Setup. Employment in H is

$$\ell_H = \frac{q_H}{A}, \qquad q_H = \frac{s_H\,\bar{I}(w)}{P_H}, \qquad P_z = \frac{w}{A} + \chi(c_z), \qquad \bar{I}(w) = b + wF(w).$$

CES shares are

$$s_z = \frac{\alpha_z P_z^{1-\sigma}}{\sum_k \alpha_k P_k^{1-\sigma}}.$$

B. Total differential. Differentiating q_H w.r.t. c_H,

$$\frac{d\ln q_H}{dc_H} = \underbrace{\left(\tfrac{\partial \ln s_H}{\partial c_H} - \tfrac{\partial \ln P_H}{\partial c_H}\right)_w}_{\mathcal{R}_H} + \underbrace{\left(\tfrac{\partial \ln s_H}{\partial w} + \tfrac{\partial \ln \bar{I}}{\partial w} - \tfrac{\partial \ln P_H}{\partial w}\right)}_{\mathcal{P}_H} \cdot \frac{dw}{dc_H}.$$

Thus

$$\frac{d\ell_H}{dc_H} = \frac{\ell_H}{A}\Big[\mathcal{R}_H + \mathcal{P}_H \cdot \tfrac{dw}{dc_H}\Big],$$

where $\mathcal{R}_H$ is the reallocation effect and $\mathcal{P}_H$ the participation elasticity.

C. Reallocation effect at fixed w. At fixed w,

$$\mathcal{R}_H = \frac{\partial \ln q_H}{\partial c_H}\Big|_w = (1-\sigma)(1-s_H)\frac{\chi'(c_H)}{P_H} - \frac{\chi'(c_H)}{P_H} = -\sigma(1-s_H)\frac{\chi'(c_H)}{P_H} < 0.$$

Result: crime always reduces demand in H through relative prices; a truce ($dc_H < 0$) raises ℓ_H via reallocation. With $\chi(c) = \beta c$, $\mathcal{R}_H = -\sigma(1-s_H)\beta/P_H$.

D. Participation effect. For the wage channel,

$$\mathcal{P}_H = \frac{\partial \ln q_H}{\partial w} = -\frac{\sigma}{AP_H} - \frac{1-\sigma}{A}\sum_k s_k \frac{1}{P_k} + \frac{F(w) + wF'(w)}{b + wF(w)}.$$

Under $F(w) = w/\bar{\kappa}$, the last term simplifies to $\frac{2w/\bar{\kappa}}{b + w^2/\bar{\kappa}}$. *Result:* $\mathcal{P}_H$ is positive when the income sensitivity term dominates the negative price effects, which occurs if participation is elastic and price dispersion is limited.

E. Dominance condition. The total effect is

$$\frac{d\ell_H}{dc_H} = \frac{\ell_H}{A}\Big[\mathcal{R}_H + \mathcal{P}_H \cdot \tfrac{dw}{dc_H}\Big].$$

Since $dw/dc_H < 0$, participation dominates reallocation if

$$\Big|\mathcal{P}_H \tfrac{dw}{dc_H}\Big| \;>\; \sigma(1-s_H)\tfrac{\chi'(c_H)}{P_H}.$$

This condition is more likely when (i) F is steep (large entry margin), (ii) b is small (outside income limited), (iii) substitution across zones is weak (low σ), or (iv) s_H is non-trivial.

F. Closed-form threshold under linear crime cost and uniform F. With $\chi(c) = \beta c$ and $F(w) = w/\bar{\kappa}$,

$$\mathcal{R}_H \;=\; -\sigma(1-s_H)\frac{\beta}{P_H}, \quad \mathcal{P}_H \;=\; -\frac{\sigma}{AP_H} - \frac{1-\sigma}{A}\sum_k s_k \frac{1}{P_k} + \frac{2w/\bar{\kappa}}{b + w^2/\bar{\kappa}}.$$

Hence, *participation dominates reallocation* iff

$$\boxed{\left| -\frac{\sigma}{AP_H} - \frac{1-\sigma}{A}\sum_k s_k \frac{1}{P_k} + \frac{2w/\bar{\kappa}}{b + w^2/\bar{\kappa}} \right| \cdot \left|\frac{dw}{dc_H}\right| \;>\; \sigma(1-s_H)\frac{\beta}{P_H}.}$$

Sufficient (transparent) cases:

- **High participation elasticity.** If $\dfrac{2w/\bar{\kappa}}{b + w^2/\bar{\kappa}}$ is large (low b and/or moderate w), then $\mathcal{P}_H$ is large and the participation channel is strong.
- **High wage responsiveness.** When $|dw/dc_H|$ is large, the amplification from wages ensures that the participation margin dominates reallocation.
- **Small relative-price gap.** If $\frac{1}{P_H} \approx \sum_k s_k \frac{1}{P_k}$ (prices not too dispersed), the negative price terms in $\mathcal{P}_H$ partly offset, raising $\mathcal{P}_H$.
- **Mild substitution across zones.** Lower σ shrinks $|\mathcal{R}_H|$, making participation more likely to dominate.
- **Moderate initial reallocation mass.** If s_H is not tiny (so $1 - s_H$ is not large), $|\mathcal{R}_H|$ is smaller.

Interpretation. The threshold inequality highlights that the participation effect hinges on two elements: the sensitivity of labor supply to wages and the magnitude of the wage response dw/dc_H.

When wages react strongly to crime reductions, the extensive margin expands markedly and participation dominates. Conversely, when wages respond weakly, reallocation across zones remains the main driver. This distinction provides the formal underpinning for the empirical result that the employment surge in truce zones is primarily explained by higher labor force participation.

G. Effects in L. By symmetry,

$$\mathcal{R}_L = (\sigma-1)(1-s_H)\frac{\chi'(c_H)}{P_H} > 0, \qquad \mathcal{P}_L = \frac{\partial \ln q_L}{\partial w} = -\frac{1-\sigma}{A}\Big(\sum_k s_k \frac{1}{P_k} - \tfrac{1}{P_L}\Big) + \frac{F + wF'}{b + wF} - \tfrac{1}{AP_L}.$$

Thus

$$\frac{d\ell_L}{dc_H} = \frac{\ell_L}{A}\Big[\mathcal{R}_L + \mathcal{P}_L \cdot \tfrac{dw}{dc_H}\Big],$$

with the sign depending on whether the negative income effect dominates the positive reallocation and price terms.

H. Empirical mapping.

- **Reallocation in H (at fixed w)** is always beneficial under a truce: $\mathcal{R}_H = -\sigma(1-s_H)\frac{\chi'(c_H)}{P_H} < 0 \Rightarrow dc_H < 0 \Rightarrow d\ell_H > 0$.
- **Participation in H** dominates when $\Big|\mathcal{P}_H\, dw/dc_H\Big| > \sigma(1-s_H)\frac{\chi'(c_H)}{P_H}$, i.e. when the wage response (driven by F and the GE of L_D) and income sensitivity are strong relative to substitution across zones.
- **In** L, the reallocation sign flips: $\mathcal{R}_L > 0$, and the wage term works through cheaper P_L vs. lower income; the net is *weakly* the same sign as dc_H.

B.3.12 Extension: Entry and Incumbent Expansion with Heterogeneous Productivity

I enrich the within–zone market structure so that a reduction in crime can generate both (i) expansion of incumbents and (ii) entry of (typically small) new firms. Crime only affects demand via an additive fear cost $\chi(c_z)$.

E.1. Within–zone demand. Within zone $z \in \{H, L\}$, the non–tradable is a CES aggregate of varieties $i \in \Omega_z$:

$$C_z = \Big(\int_{i\in\Omega_z} q_{zi}^{\frac{\varepsilon-1}{\varepsilon}}\, di\Big)^{\frac{\varepsilon}{\varepsilon-1}}, \qquad \varepsilon > 1.$$

Total zone expenditure is $S_z = s_z(w, \mathcal{P}_H, \mathcal{P}_L)\, \bar{I}(w)$, with s_z determined across zones as in the baseline (elasticity $\sigma > 1$). The consumer price for variety i is

$$\tilde{p}_{zi} = p_{zi} + \chi(c_z).$$

E.2. Technology, pricing, and heterogeneity. At entry, firm i draws productivity $\varphi_i \sim G$ with support $[\underline{\varphi}, \infty)$. Production is linear with fixed operating labor $f_o > 0$ and sunk entry cost $f_e > 0$ (in labor units):

$$\ell_{zi} = f_o + \frac{y_{zi}}{A\varphi_i}, \qquad y_{zi} = q_{zi}.$$

Monopolistic competition with CES demand implies a constant markup

$$p_{zi} = \mu \frac{w}{A\varphi_i}, \qquad \mu \equiv \frac{\varepsilon}{\varepsilon - 1} > 1,$$

hence $\tilde{p}_{zi} = \mu \frac{w}{A\varphi_i} + \chi(c_z)$.

E.3. Variety demand, revenues, and cutoff. Given S_z and the within–zone CES,

$$q_{zi} = S_z\, \tilde{p}_{zi}^{-\varepsilon}\, \mathcal{P}_z^{\varepsilon-1}, \qquad \mathcal{P}_z^{1-\varepsilon} = \int_{j \in \Omega_z} \tilde{p}_{zj}^{1-\varepsilon} dj.$$

Operating profits are

$$\pi_{zi}^{\text{op}} = (p_{zi} - mc_{zi}) q_{zi} - w f_o = \frac{1}{\varepsilon}\, r_{zi} - w f_o, \qquad r_{zi} \equiv \tilde{p}_{zi} q_{zi}.$$

The zero–profit cutoff φ_z^* in zone z solves

$$\pi_{zi}^{\text{op}}(\varphi_z^*; w, S_z, c_z) = 0 \quad \Longleftrightarrow \quad r_z(\varphi_z^*; w, S_z, c_z) = \varepsilon w f_o. \tag{ZP}$$

Let n_z be the mass of active firms and $\mathcal{P}_z = \left(n_z\, \mathbb{E}[\tilde{p}_{zi}^{1-\varepsilon} \mid \varphi \geq \varphi_z^*]\right)^{\frac{1}{1-\varepsilon}}$.

E.4. Labor market. Total labor demand (fixed + variable) equals labor supply $F(w)$:

$$F(w) = \sum_{z \in \{H, L\}} \left(n_z f_o + \int_{\varphi \geq \varphi_z^*} \frac{q_z(\varphi)}{A\varphi}\, dH_z(\varphi) \right),$$

where H_z is the productivity distribution among active firms in z. Together with $S_z = s_z(w, \mathcal{P}_H, \mathcal{P}_L)\,\bar{I}(w)$, these conditions pin down $(w, \varphi_H^*, \varphi_L^*, n_H, n_L)$.

B.3.13 Comparative Statics: Crime Reduction in H

Consider $dc_H < 0$ and $dc_L = 0$. Maintain $\varepsilon > 1$, $\sigma > 1$, $\chi'(c) > 0$, and $F'(w) > 0$.

R.1. Incumbent expansion in H (at fixed w). For any incumbent i in H,

$$q_{Hi} = S_H\,\tilde{p}_{Hi}^{-\varepsilon}\,\mathcal{P}_H^{\varepsilon-1}.$$

Log-differentiating and holding w fixed,

$$\frac{\partial \ln q_{Hi}}{\partial c_H}\Big|_w = \frac{\partial \ln S_H}{\partial c_H}\Big|_w - \varepsilon\frac{\chi'(c_H)}{\tilde{p}_{Hi}} + (\varepsilon-1)\chi'(c_H)\,\mathbb{E}_H\left[\frac{1}{\tilde{p}}\right].$$

Since $\partial \ln S_H/\partial c_H|_w = \partial \ln s_H/\partial c_H < 0$ (baseline) and the CES weights over-represent low prices, $\mathbb{E}_H[1/\tilde{p}] \le 1/\tilde{p}_{Hi}$ for productive firms. Hence the price-index term cannot dominate the own-price term and

$$\boxed{\frac{\partial \ln q_{Hi}}{\partial c_H}\Big|_w < 0}, \qquad \boxed{dq_{Hi}/dc_H < 0 \text{ at fixed } w}.$$

Intuition. Crime raises each firm's effective price and the price index; both lower q_{Hi}, and the share shift away from H reinforces this effect.

R.2. Cutoff movement $d\varphi_H^*/dc_H$. Differentiate (ZP) w.r.t. c_H:

$$\frac{\partial r_H}{\partial \varphi}\frac{d\varphi_H^*}{dc_H} + \frac{\partial r_H}{\partial c_H} + \frac{\partial r_H}{\partial w}\frac{dw}{dc_H} = \varepsilon f_o \frac{dw}{dc_H}.$$

Therefore,

$$\frac{d\varphi_H^*}{dc_H} = -\frac{\dfrac{\partial r_H}{\partial c_H} + \left(\dfrac{\partial r_H}{\partial w} - \varepsilon f_o\right)\dfrac{dw}{dc_H}}{\dfrac{\partial r_H}{\partial \varphi}}.$$

With $\partial r_H/\partial\varphi > 0$, $\partial r_H/\partial c_H < 0$, and $dw/dc_H < 0$ (baseline GE), a sufficient condition is that the direct crime term dominates the wage term, yielding

$$\boxed{\frac{d\varphi_H^*}{dc_H} > 0}, \quad \text{i.e.} \quad dc_H < 0 \Rightarrow d\varphi_H^* < 0.$$

Intuition. Lower crime raises revenue at each φ, relaxing selection and allowing lower-productivity firms to operate.

R.3. Entry dn_H/dc_H. With a fixed pool of potential entrants M_H, $n_H = M_H[1 - G(\varphi_H^*)]$, hence

$$\boxed{\frac{dn_H}{dc_H} = -M_H g(\varphi_H^*)\frac{d\varphi_H^*}{dc_H} < 0},$$

so $dc_H < 0 \Rightarrow dn_H > 0$. *Intuition.* As the cutoff falls, more draws clear the threshold; entrants are typically near the cutoff and therefore small.

R.4. General-equilibrium wage and participation. Aggregating fixed and variable labor across zones and equating to $F(w)$, the outward shift in S_H and n_H raises total labor demand. With $F'(w) > 0$,

$$\boxed{dw^\star/dc_H < 0}, \qquad \boxed{dn^\star/dc_H < 0}.$$

Intuition. The wage increases to clear the labor market, bringing additional participants into employment and further expanding S_H.

R.5. Zone L. With c_L fixed, $w^\star \uparrow$ raises $\tilde{p}_{Li}$ and expenditure shifts toward H. Thus,

$$\boxed{dq_{Li}^\star/dc_H \geq 0, \quad d\ell_{Li}^\star/dc_H \geq 0, \quad dn_L^\star/dc_H \geq 0}.$$

Intuition. When crime falls in H, L weakly contracts due to substitution and higher local prices induced by the higher wage.

R.7. Summary of sufficient signs. For $dc_H < 0$,

Incumbents in H:	$dq_{Hi} > 0$,	$d\ell_{Hi} > 0$;	
Cutoff in H:	$d\varphi_H^* < 0$;	Entry:	$dn_H > 0$;
Wage/participation:	$dw^\star > 0$,	$dn^\star > 0$;	
Zone L:	$dq_{Li} \leq 0$,	$d\ell_{Li} \leq 0$,	$dn_L \leq 0$ (weak).

Remarks

- *Incumbent expansion is strict for all firms with prices below the within–H weighted average*, since then $-\varepsilon\,\chi'/\tilde{p}_{Hi} + (\varepsilon - 1)\chi'\mathbb{E}_H[1/\tilde{p}] \leq -\chi'/\tilde{p}_{Hi} < 0$. High–$\varphi$ incumbents therefore expand the most.

- *Cutoff easing* holds if the direct effect $|\partial r_H/\partial c_H|$ dominates the wage term in R.2, which is natural when $|dw/dc_H|$ is moderate (Step 3 condition $|dw/dc_H| < A\beta$ is a convenient sufficient proxy).

- *Entry in H* then follows mechanically from free entry and $d\varphi^*_H/dc_H > 0$ when M_H is fixed (or from an increase in M_H if the pool of potential entrepreneurs responds to profits).