Does Public Safety Close the Gender Gap? Crime Reduction, Firms, and Gendered Sorting in El Salvador

Abstract:

We study whether crime reduction benefits men and women equally, using El Salvador's 2022 State of Exception, a nationwide anti-gang crackdown that sharply reduced homicides. Using administrative payroll records for the universe of formal firms, nationally representative household surveys, and a difference-in-differences design exploiting pre-crackdown violence exposure, we find that the crackdown raised formal employment substantially, with gains accruing disproportionately to men. We document a wage divergence: within formal firms, the gender pay gap narrowed by $6 per month, while household data show the aggregate gender wage gap widening by $18.

Keywords: Crime; Gender; Employment; Firm Dynamics; Public Security; El Salvador

JEL codes: O17, J23, J16, K42, L26

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I. Introduction

Over 1.5 billion people live in regions afflicted by high levels of criminal violence, yet we know little about what happens to labor markets when that violence is suddenly removed (UNODC, 2023). Theory offers ambiguous predictions about whether crime reduction generates gender-inclusive recovery. If violence acts as a gender-neutral tax on economic activity, its removal should benefit men and women proportionally. But if violence constrains labor markets through gender-specific channels, then even dramatic improvements in public safety may fail to close labor market gender gaps (Siddique, 2022; Melnikov et al., 2025). This distinction has direct policy relevance: fragile states increasingly invest in security as a foundation for economic development, yet whether the resulting gains are shared equally across genders remains an open question.

We study this question in the context of El Salvador's 2022 State of Exception (SoEx), a sudden, nationwide anti-gang crackdown that reduced homicides by over 70% within a year. The SoEx resulted in the arrest of more than 80,000 suspected gang members under an unprecedented enforcement operation. The crackdown was not accompanied, to our knowledge, by major concurrent economic reforms, fiscal stimulus, or labor market policies, providing an unusual opportunity to identify the labor market effects of improved public security.

We combine three high-quality datasets. Administrative payroll records from the national social security system (ISSS) track monthly employment and wages for the universe of registered firms (around 26,000 per month) from January 2017 to December 2023. The nationally representative household survey (EHPM, 2018–2023) provides individual-level labor market outcomes including income, hours, and job characteristics. Monthly city-level homicide rates from the National Police (PNC) measure local exposure to violent crime. We implement a difference-in-differences design comparing municipalities with high versus low pre-crackdown homicide rates before and after the SoEx, with event-study estimates confirming parallel pre-treatment trends.

The SoEx increased firm employment in high-crime areas by approximately 7%, driven primarily by male hires at a ratio exceeding 2:1. Turning to wages, we document a divergence between within-firm and aggregate gender wage responses: within formal firms, women's average salary rose by $7 per month while men's showed no significant change, narrowing the within-firm gender pay gap by $6. Yet household survey data reveal the opposite: male wage income increased by $18 per month while women's showed no improvement. One interpretation is gender-asymmetric sorting across labor market segments: men's employment gains were concentrated in formal-sector positions in expanding industries, while women's gains were concentrated in lower-quality, hours-intensive work outside the formal sector.

We organize the empirical analysis around three channels that may generate gender-differentiated labor market outcomes: mobility constraints that disproportionately benefit male-intensive activities, intra-household reallocation of non-market responsibilities toward women, and persistent labor market frictions that limit women's access to formal employment. We evaluate testable predictions from each channel, though we note that the evidence is intended to assess consistency with the framework rather than to causally isolate each mechanism.

This paper makes three contributions. First, we show that a major security intervention can generate large labor market gains that are nonetheless gender-skewed, a finding that bridges the crime-economics literature (Brodeur, 2018; Rozo, 2018; Brown et al., 2024; Slutzky and Zeume, 2024) and the gender-labor literature (Berniell et al., 2021; Siddique, 2022; Cortés et al., 2023). Second, we document that within-firm and aggregate gender wage responses can move in opposite directions following the same shock, a divergence that would be invisible in either administrative payroll records or household survey data alone. Third, we complement recent work on gang effects on mobility (Melnikov et al., 2025) and emigration responses (Abreha et al., 2025) in El Salvador by providing new evidence on domestic labor market restructuring and gendered sorting.

The remainder of the paper is organized as follows. Section II provides background on El Salvador's anti-gang crackdowns. Section III presents the conceptual framework. Section IV describes the data and empirical strategy. Section V presents firm-level results on employment,

expansion, and wages. Section VI turns to household survey evidence on individual employment, income, and the wage divergence. Section VII presents evidence on gendered sorting and labor market constraints and Section VIII concludes.

II. Background: Anti-Gang Crackdowns in El Salvador

Violent crime poses a substantial barrier to local economic development in Central America, particularly in El Salvador. Businesses incur significant losses from robbery, extortion, theft, and vandalism (Detotto and Otranto, 2010; Motta, 2017; Brown et al., 2024). Street gangs are responsible for a large share of these offenses and contribute to the country's historically high homicide rates. According to the World Bank Enterprise Survey, more than 40 percent of firms identify crime as a major obstacle to operation (Figure A.1), with private-sector losses from extortion alone estimated at around 3 percent of GDP (Yansura, 2022).

Responding to persistent gang violence, the Salvadoran government adopted two major anti-crime interventions. The first was the Territorial Control Plan (TCP), introduced in mid-2019, which expanded police and military deployments in high-crime areas and tightened prison control. Homicide rates declined during the TCP period from 35.8 per 100,000 in 2019 to 18.1 in 2021, though contemporaneous accounts suggest that part of this decline may have reflected informal negotiations between government actors and gang leaders rather than durable security restructuring (Escaño et al., 2025).

A more decisive break occurred in March 2022, when a sudden spike in violence (at least 87 killings over 25–27 March, including 62 in a single day) precipitated a nationwide State of Exception (régimen de excepción) under the Mano Dura ("Iron Fist") framework. The SoEx temporarily suspended certain constitutional rights, enabling large-scale detentions [1]. Reported detentions under the SoEx are historically large (more than 56,000) by the end of

[1] The Inter-American Commission on Human Rights reports testimonies that police asked detainees about tattoos and that perceived gang-related tattoos, as well as appearance and socioeconomic profiling, were used to justify detention under the SoEx (see, https://www.oas.org/en/iachr/reports/pdfs/2024/report_stateemergencyhumanrights_elsalvador%20%281%29.pdf).

2022, a number that covers almost the entire estimated total of Salvadoran gang members in 2019[2], underscoring the scale of the enforcement shock. The SoEx produced a sharp and sustained fall in violence: by the end of 2023, the national homicide rate had dropped to 2.4 per 100,000 people, one of the lowest in the Western Hemisphere (El Salvador in English, 2025). Figure 1 plots national homicide trends from 2017 to 2023, illustrating the timing of the TCP and SoEx and the magnitude of the subsequent decline. The decline in homicides was sharply differential across treatment and control municipalities: high-crime cities experienced dramatically larger reductions in violence after the SoEx, confirming the differential treatment intensity that underpins our identification strategy. Figure A.2 complements this by mapping municipality-level homicide rates before and after the SoEx, showing widespread improvements across the country.

Comparison between TCP and SoEx. The key distinction for our empirical design is that the SoEx represented a sharp escalation over the TCP in both pace and intensity of enforcement, relying on the same security apparatus but enabling large-scale arrests through temporarily relaxed legal constraints. The TCP's weaker labor market effects may also reflect lower policy credibility: contemporaneous accounts suggest that part of the TCP-era decline in violence reflected informal negotiations between government and gang leaders rather than durable security restructuring (Escaño et al., 2025), potentially attenuating firm and worker responses. We therefore treat the SoEx as the key shock and use TCP estimates only as a benchmark for interpreting magnitudes.

Taken together, the 2019 TCP and the 2022 SoEx constitute two distinct, large-scale security shocks. Their timing, intensity, and nationwide reach provide a useful setting for studying the economic consequences of sharp reductions in violent crime with differential local exposure.

[Insert Figure 1 here]

[2] See https://www.hrw.org/world-report/2019/country-chapters/el-salvador.

III. Conceptual Framework

An anti-gang crackdown could affect labor markets through both demand-side (firm) and supply-side (worker) channels, and these channels may operate differently for men and women. We organize our empirical analysis around three mechanisms that generate distinct, testable predictions for gendered labor market responses. This framework is intended to structure the evidence and guide empirical tests rather than to provide a fully identified structural decomposition; the mechanisms are not mutually exclusive and may reinforce one another.

Mobility constraints. Criminal organizations restrict spatial mobility through territorial control, extortion corridors, and direct threats to commuters (Melnikov et al., 2025). If these constraints bind more tightly on industries requiring cross-neighborhood logistics, such as construction, manufacturing, and transportation, then crime reduction should disproportionately expand employment in these mobility-dependent, typically male-intensive sectors. If this channel operates, employment gains should concentrate in production and infrastructure industries rather than locally consumed services, and male employment should rise more than female employment in these sectors. This prediction runs counter to a simple labor supply story: women represent 53.6 percent of the working-age population, so a proportional labor market recovery should slightly favor women in absolute terms. What distinguishes this channel from the other two is that it uniquely predicts industry-level heterogeneity in employment gains and gender-differential changes in individual mobility behavior.

Intra-household reallocation. Classic models of household time allocation (Becker, 1965; Chiappori, 1992) predict that when external labor market opportunities improve asymmetrically, favoring men through the mobility channel above, households may reoptimize by shifting non-market responsibilities toward women. This reallocation dampens women's labor supply response even if their individual constraints have eased. The key prediction is that within the same household, men's employment should rise more than women's; women's unpaid domestic work hours should increase. What sets this channel apart is that it uniquely

predicts divergent employment responses between men and women within the same household, a pattern that cannot be explained by mobility constraints or labor market frictions alone, which operate at the individual or market level rather than the within-household level.

Labor market frictions and gendered sorting. Even when women seek employment, structural barriers, including caregiving costs, employer discrimination, and higher search frictions, may channel them into informal, non-permanent positions rather than the formal-sector jobs created by firm expansion (Cortés et al., 2023). This generates a composition effect: if men disproportionately enter high-wage formal positions while women enter low-wage informal work, within-firm gender pay gaps may narrow while aggregate gender pay gaps widen. This implies that women should be less likely than men to access formal and permanent jobs, and that within-firm wages should show different gender dynamics than aggregate wages. Only this channel predicts that within-firm and aggregate gender pay gaps move in opposite directions: the wage divergence we document empirically. Neither the mobility channel nor the household reallocation channel generates this within-unit versus aggregate divergence.

These three channels are not mutually exclusive and may reinforce one another. If mobility constraints channel the initial employment gains toward men, household reallocation may further dampen women's labor supply, while labor market frictions sort the women who do seek employment into lower-quality positions. The combined effect is a labor market recovery that favors men in both the quantity and quality of jobs, producing the within-firm convergence and aggregate divergence that we document empirically.

IV. Data and Sample

Violent Crime Data. We measure violent crime using official homicide records from El Salvador's National Police (PNC). From January 2017 to March 2022, we use monthly homicide statistics published in the PNC's public security repositories. Beginning in April 2022, when reporting practices shifted, the PNC released daily homicide counts with geographic

identifiers through official channels. We aggregate these daily reports to construct consistent municipality-level monthly homicide series. The resulting series aligns closely with annual totals published in official government reports. Homicide rates are expressed per 100,000 inhabitants using municipality-level annual population estimates.

Firm-Level Data. We use administrative payroll records from the national social security system (ISSS), which cover the universe of formal private-sector firms. Establishments report employment at the branch level; we aggregate these to the firm-month level and also conduct robustness checks at the branch level. For firms operating in multiple municipalities, each municipal presence is treated as a separate firm-city unit. The panel spans January 2017 to December 2023 and includes information on total and gender-disaggregated employment, wages, number of establishments, industry classification, and geographic location. We exclude financial-sector firms, which face distinct reporting requirements[3]. The final dataset contains 26,001 firm-city units (24,928 distinct firms) across 259 municipalities and 14 provinces, yielding 1,400,468 firm-month observations. The average firm employs 35 workers.

Household-Level Data. We complement our firm-level analysis with microdata from the Encuesta de Hogares de Propósitos Múltiples (EHPM), a nationally representative household survey conducted annually since 1995 (Sviatschi, 2022). Each wave samples over 20,000 households and covers more than 85,000 individuals. We use data from the 2018 to 2023 rounds[4]. We restrict the sample to working-age individuals (15–49). Table A.7 provides detailed variable definitions and sources.

Identification challenges and interpretation. Our difference-in-differences design identifies the differential labor market response to the larger security improvement experienced by municipalities with higher pre-SoEx violence exposure. Several threats to this interpretation merit discussion. First, if high-crime municipalities experienced larger pandemic-related contractions, the post-SoEx employment gains could reflect mean reversion rather than

[3] Our main results are very similar if we do not exclude these firms.
[4] We exclude the 2020 wave because it lacks geographic identifiers, and we focus on post-2018 surveys due to a change in survey design and methodology introduced that year.

security-induced recovery. The event-study estimates in Figure 3 address this directly: pre-treatment trends are flat for at least 6 months before April 2022, with no evidence of differential recovery trajectories. Second, the SoEx involved not only violence reduction but also expanded state presence through police and military deployments. We cannot fully disentangle improved public safety from all dimensions of intensified state activity. However, the sectoral composition of employment gains, concentrated in production and infrastructure rather than public administration or consumption services—aligns more closely with the relaxation of private-sector operating constraints than with direct demand from state deployments. Table A.3 Panel D shows that using state institutional density as an alternative treatment yields qualitatively similar but smaller effects. Third, if the SoEx improved administrative compliance rather than actual employment, we would expect increases in reported workers without corresponding changes in household survey employment probabilities; the fact that both firm-level (Table 1) and individual-level (Table 5) results show employment increases is reassuring, though we cannot rule out some formalization margin. Fourth, because the SoEx was a nationwide policy, our estimates capture relative effects of differential violence exposure rather than absolute effects of security improvement. Table A.4 addresses spatial spillovers by excluding border municipalities and conducting placebo tests on adjacent areas; results are robust to these restrictions. Fifth, the mass detention of over 56,000 individuals under the SoEx—predominantly suspected gang members—raises the concern that our results may partly reflect a contraction in male labor supply rather than improved security conditions. However, the detained population was largely outside the formal labor market prior to arrest, making a binding contraction of formal labor supply unlikely. Moreover, if labor supply shortages rather than demand expansion drove the results, we would expect male wage increases across all industries; instead, male formal-sector wages rose only in production and infrastructure sectors (Table 4, Panel B), inconsistent with a general supply-shortage interpretation.

V. Firm-Level Results

A. *Descriptive Dynamics: Violent Crime and Firm Employment*

We begin by examining how short-run fluctuations in local violent crime correlate with firm-level employment. Using monthly administrative data, we estimate dynamic responses to city-level homicide rates based on within-firm variation over time. Specifically, we estimate:

$$(1) \qquad Worker_{ict} = \alpha_1 + \sum_{j=0}^{12} \beta_1^j Homicide_{c,t-j} + \Gamma_i + \Gamma_c + \Gamma_{pst} + \varepsilon_{ict}$$

Where $Worker_{ict}$ is the number of workers (all workers or men/women only) in firm i, city c, time t. The key independent variable, $Homicide_{c,t-j}$ captures homicide rate per 100,000 people and lagged by j months (i.e., lags from 0 to 12 months) from the current observation month. The specification includes firm fixed effects and city fixed effects to absorb time-invariant differences across firms and locations, as well as province-industry-year-month fixed effects to capture time-varying shocks at broader regional and sectoral levels. Standard errors are clustered at the city-year-month level, matching the spatial level at which homicide rates vary. This model estimates descriptive patterns of how firms respond to short-term fluctuations in local violence, without claiming causal inference, which we address in the next section.

Figure 2 plots the estimated dynamic coefficients separately for all workers, men, and women. Among all workers (Panel A), employment declines sharply following an increase in local homicides, with the largest reductions occurring around a three-month lag before gradually dissipating. Panel B shows even stronger and more persistent declines for male employment. In contrast, Panel C indicates that female employment responds weakly and remains statistically indistinguishable from zero at all horizons. The results imply that firms adjust male staffing more flexibly in response to short-term security shocks, whereas female employment appears more rigid in the immediate aftermath of changes in local crime.

[Insert Figure 2 here].

B. *Causal Effects: Anti-Gang Crackdowns and Firm Employment*

We next estimate the causal impact of improved public security on firm employment by exploiting two major interventions: the 2019 Territorial Control Plan (TCP) and the 2022 State of Exception (SoEx). We classify municipalities based on pre-reform homicide rates: those with annual homicide rates above the national mean are defined as "high-crime" prior to the crackdown. Consistent with this definition, Figure A.3 shows that treated cities exhibited much higher monthly homicide rates before SoEx, followed by a pronounced convergence toward the control group. We benchmark the SoEx effects against the TCP, which, as discussed in Section II, represented a less intensive intervention.

We implement a difference-in-differences (DID) framework comparing employment changes in high- and low-crime municipalities before and after each reform. The baseline specification is:

$$(2) \qquad Worker_{\text{ict}} = \alpha_2 + \delta_2 Crime_{22,c} * \text{SoEx} + \gamma_2 Crime_{19,c} * \text{TCP} + \Gamma_i + \Gamma_c + \Gamma_{pst} + \varepsilon_{ist}$$

Where $Crime_{22,c}$ is an indicator for municipalities classified as high-crime in 2022 and *SoEx* is a binary indicator equal to one if the observation falls after April 2022. $Crime_{19,c}$ is an indicator for municipalities classified as high-crime in 2019, and *TCP* is a binary indicator equal to one if the observation falls between July 2019 and February 2022, and zero otherwise.

We first estimate a standard DID specification that focuses on the 2022 State of Exception (SoEx) only. Table 1, Panel A shows that, relative to low-crime municipalities, firms in high-crime municipalities increased employment by about 2.02 workers after the SoEx. The employment gain is driven primarily by men (1.44 workers), while female employment rises by 0.58 workers.

Panel B expands the specification to jointly estimate the effects of the 2022 SoEx and the earlier 2019 Territorial Control Plan (TCP). The SoEx increased firm employment in high-crime municipalities by 2.27 workers per firm (0.73 women and 1.54 men), representing approximately 7 percent of the average pre-policy firm size, while the TCP generated smaller gains of 0.91 workers per firm (0.53 women and 0.38 men). Formal difference tests reject equality of the SoEx and TCP effects for total employment and for male employment ($p<0.001$),

but not for female employment (p=0.244), indicating that the larger SoEx-induced employment response is concentrated among men.

[Insert Table 1 here]

The SoEx produced a far larger decline in homicides than the TCP (Figure 1), and businesses appear to respond proportionally to the credibility and scale of security improvements. The stronger response among men is in line with the descriptive patterns in Section V.A, where short-run violence shocks reduced male but not female employment.

To claim causality, Figure 3 examines dynamic treatment effects to assess the validity of the parallel trend assumption. Each panel plots the estimated monthly difference in firm employment between treated and control municipalities over a 24-month window surrounding the onset of the 2022 State of Exception (SoEx). The results show that prior to the policy implementation, pre-treatment trends are flat for at least 6 months before April 2022, which means that the trends in employment are virtually parallel across high-crime and low-crime municipalities, with no statistically significant pre-treatment divergence.

The most natural alternative explanation for our results is that high-crime municipalities would have experienced faster employment growth regardless of the SoEx, due to differential recovery from the pandemic, mean reversion in economic activity, or broader state penetration. We address this concern through several complementary analyses. First, event-study estimates show no differential employment trend between treated and control municipalities in the years preceding the SoEx, including during the 2020–2021 pandemic period—ruling out pre-existing divergent trajectories (Figure 3). Second, results using a continuous measure of pre-policy homicide intensity confirm that the treatment effect scales linearly with baseline violence (Table A.3, Panel E), consistent with a dose-response relationship and inconsistent with differential mean reversion. Third, the SoEx employment effects are concentrated in production and infrastructure industries (Table 2) rather than in service sectors most affected by pandemic restrictions, further inconsistent with a pandemic-recovery interpretation. Fourth, the SoEx generated employment effects that the TCP did not, including geographic expansion through new establishments (Table 3)—suggesting that the results reflect the SoEx's distinctive

enforcement intensity rather than a continuation of pre-existing trends. Tables A.3–A.5 provide additional robustness checks, including alternative treatment thresholds (75th and 90th percentile), exclusion of municipalities exposed to both reforms, removal of border municipalities to address spatial spillovers, placebo tests on adjacent cities, and controls for emigration dynamics. Across all specifications, the estimated effects remain large and statistically significant.

The evidence suggests that the SoEx increased firm employment in areas with higher pre-crackdown exposure to violence, and that the magnitude of the response depends on the intensity of the security improvement. Both interventions expanded formal employment, but the SoEx generated substantially larger gains, particularly for men, reflecting its more pronounced reduction in violent crime.

C. *Heterogeneity Tests*

Industry Heterogeneity. A natural mechanism is that improved public safety increases consumption, which in turn stimulates local demand and employment. This channel would be expected to be particularly female-friendly, because consumption-oriented sectors (e.g., retail, accommodation, and services) tend to employ a relatively larger share of women. Thus, if the consumption channel were the primary driver, we should observe stronger employment gains for women, concentrated in consumption industries.

Alternatively, a second pattern points to mobility channels (Melnikov et al., 2025). If the anti-gang crackdown effectively disrupted the capacity of criminal organizations to extort businesses or disrupt movement across space, we should expect its economic effects to be most pronounced in sectors that are structurally exposed to such interference. This should be especially true in production (e.g., agriculture, mining, manufacturing, and construction) and infrastructure-related activities (e.g., utilities, environmental services, and transportation), where lower disruption risk and smoother logistics can relax binding constraints on output and expansion.

To assess this implication, we allow the SoEx treatment effect to vary by industry group, including consumption, production, infrastructure, and other industries. Table 2, panel A provides little support for a demand-driven consumption mechanism: the estimated post-SoEx effect in consumption industries is close to zero (and slightly negative), including for female employment, whereas the effects are large and positive in production (4.873 workers, of whom 2.73 male and 2.15 female) and infrastructure-related industries (5.779 workers, of whom 3.77 male and 2.01 female). The sectoral composition of gains is consistent with security improvements relaxing constraints that bind more tightly on mobility-dependent activities such as construction, manufacturing, and transportation, which are both male-dominated and operationally dependent on spatial mobility across locations (Melnikov et al., 2025).

[Insert Table 2 here]

Firm Age. We also examine heterogeneity by firm age (Table 2, Panel B). Younger firms exhibit significantly larger employment gains following the SoEx, supporting the view that crime disproportionately constrains less-established businesses with smaller client bases and weaker financial cushions. The interaction of firm age with the SoEx treatment is negative and significant, indicating that each additional year of firm age reduces the employment response. These results suggest that improved public security may enable younger firms to expand in ways that insecurity had previously constrained.

D. *Expansion Through Branch Growth: Spatial vs. Staffing Adjustment*

We also examine whether the SoEx affected the spatial footprint of firm activity. As shown in Table 3, both crackdowns raised employment within existing workplaces by comparable magnitudes, but only the SoEx led firms to open new establishments. The TCP period saw a net decline in workplace counts, suggesting that firms consolidated when security improvements were less sustained.

[Insert Table 3 here].

E. *Firm Wage Effects*

We next examine whether the employment expansion documented above was accompanied by changes in wages within formal firms. Using the same ISSS administrative records, we compute average monthly salary per worker at the firm level, separately by gender, and estimate Equation (2) with these wage measures as dependent variables.

Table 4 reports the results. Column (1) shows that the SoEx increased average firm-level wages by \$3.90 per month in high-crime areas. Decomposing by gender reveals a striking asymmetry: women's average wages rose by \$7.11 (column 2), while men's wages showed no significant change (column 3, \$1.58, $p > 0.10$). Consequently, the within-firm gender pay gap narrowed by \$6.24 per month (column 4). The TCP exhibits the same qualitative pattern: women's wages increased by \$3.93 while men's were unchanged, reducing the pay gap by \$3.58.

Panel B shows that these wage effects vary substantially across industry groups. In the omitted industry category (such as professional and public-service activities), the SoEx had little effect on average wages overall and on women's wages, but men's wages declined by about \$4.46 per month. In consumption industries, women's wages increased sharply relative to the omitted group, by an additional \$10.28, while men's wages showed no comparable gain. By contrast, in production and infrastructure industries, wages rose strongly for both women and men, with additional increases of \$12.22 and \$17.59, respectively. The overall wage response therefore masks considerable sectoral heterogeneity: the strongest relative gains for women were concentrated in consumption industries, whereas production and infrastructure sectors experienced broader wage growth affecting both genders.

The narrowing of the within-firm gender pay gap is therefore consistent with several mechanisms, including industry composition effects from the influx of male blue-collar workers into production and infrastructure roles (Table 2), as well as heterogeneous wage responses across sectors within formal firms. An important caveat is that the ISSS data contain firm-level average wages rather than worker-level records. The observed changes could reflect

genuine wage growth for incumbents, compositional effects from the entry of new workers at different wage levels, or a combination of both. For instance, if male new hires entered at below-average wages in production and infrastructure roles, this would mechanically compress the firm-level male average; conversely, if women entering formal firms were drawn from the upper end of the female skill distribution, this would raise the within-firm female average. We cannot decompose these channels with the available data. We return to this finding below, where we document that it contrasts sharply with aggregate employment income patterns from household survey data.

[Insert Table 4 here]

VI. Individual-Level Results

A. Individual Employment

We next turn to household survey data to examine whether the firm-level patterns documented above are reflected in individual-level outcomes. Firm administrative records capture formal-sector responses but cannot identify how individuals in informal markets adjust their labor supply, nor can they reveal within-household allocation dynamics that shape women's participation decisions (Dustmann and Fasani, 2016; Velásquez, 2020; Brown et al., 2024). The EHPM household survey allows us to assess these dimensions directly.

We use nationally representative data from El Salvador's household survey (EHPM) to examine whether individuals in municipalities with high pre-crackdown homicide rates became more likely to work following the two major anti-gang crackdowns. We estimate the following Logit difference-in-differences (DID) model to assess changes in employment probabilities:

$$(3) \quad P(Employ_{ict}) = logit^{-1}(\alpha_3 + \delta_3 Crime_{22,c} * \text{SoEx} + \gamma_3 Crime_{19,c} * \text{TCP} + \Gamma_c + \Gamma_m + \Gamma_{py} + X_i^{'} \delta + X_h^{'} \varsigma + \varepsilon_{ict})$$

Where $Employ_{ict}$ is a binary indicator equal to 1 if individual *i* is employed, and 0 otherwise. We include city, month, and province-by-year fixed effects, along with individual-level

controls (age, gender, marital status, urban status, and education), and household controls (household head age, head gender, number of household members, and emigration). Standard errors are clustered at the city-month-year level.

Table 5 shows that individuals in high-crime municipalities experienced significantly higher employment following the 2022 SoEx. Without controls (column 1), the SoEx coefficient is 0.108 ($p < 0.01$); with individual controls (column 2), the estimate slightly increases to 0.123. Column 3 adds household controls and emigration controls; the estimate increases to 0.125. By contrast, the 2019 TCP has no detectable effect on employment probabilities. These results mirror the firm-level findings and indicate a substantial labor supply response to the SoEx.

[Insert Table 5 here]

The triple interaction with gender provides evidence of heterogeneous supply responses. Women's employment gains are significantly smaller than men's, in line with persistent household responsibilities and mobility constraints documented in prior literature. To express the magnitude more intuitively, predictive margins from column (1) show that for men, the probability of employment in high-crime municipalities increased by about 1.8 percentage points. For women, the corresponding increase is only 0.1 percentage points. Thus, the employment response for women is economically negligible and about 1.7 percentage points smaller than that for men. Together with the firm-level results, these findings confirm that the employment response to improved security is substantially smaller for women.

B. Individual Income and Wage Effects

The firm-level results show that within formal firms, women's wages rose while men's did not. We now turn to household survey data, which capture the entire labor market including informal employment, to examine whether this pattern holds at the aggregate level.

Table 6, Panel A reports income effects for the full sample of individuals. The SoEx increased male wage employment income by $18.22 per month in high-crime areas. However,

the female interaction term is large, negative, and significant (–$25.99), implying that the net effect for women is approximately –$7.68; women's wage income actually declined in relative terms. The same extends to total income: men gained $13.82 while women's net effect was approximately –$7.11. Women's non-labor income increased by $4.02, suggesting increased reliance on transfers or remittances. Panel B restricts the sample to employed individuals with positive total income. Even conditional on working, men's wage employment income rose by $10.85 while the female interaction is –$12.98, indicating that employed women saw no income gains.

Panel C further shows that the SoEx did not significantly increase hourly pay overall, but the interaction term with Female is negative and statistically significant across specifications. This indicates that women's hourly pay declined relative to men's after the crackdown. However, employed women worked nearly one additional hour per week (0.83 hours, $p < 0.01$). This combination (more hours without corresponding income gains) is consistent with women entering hours-intensive but low-paying positions. Taken together, the household evidence suggests that men benefited more from post-crackdown labor market recovery, while women's gains were weaker and concentrated in lower-paying segments of the labor market.

[Insert Table 6 here]

C. The Wage Divergence: Within-Firm Convergence, Aggregate Divergence

The wage results reveal a striking divergence. Within formal firms (Table 4), the gender pay gap narrowed by $6 per month, as average female wages rose while average male wages did not change significantly. Yet household survey data (Table 6) show the opposite at the aggregate level: male wage income increased substantially while female wages were flat or declining. These two findings do not contradict each other; rather, they reflect the fact that the two data sources capture different tiers of the labor market. The ISSS payroll records capture wages within registered formal firms, whereas the household survey captures the economy-wide distribution of labor income, including informal and unregistered work. The divergence

therefore reflects not a disagreement between datasets, but a difference between what happened within the formal sector and what happened across the broader labor market.

This divergence is consistent with gender-asymmetric sorting across labor market segments. Formal-sector firms captured by ISSS administrative data represent the higher-quality segment of the labor market. Within these firms, the influx of lower-wage male workers into production and infrastructure roles may have compressed the male average while average wages for women within these firms increased, narrowing the within-firm gap. Household data, by contrast, capture the entire labor market including informal work. As we show in Table 9, women are systematically less likely than men to access formal or permanent positions; their employment gains appear more concentrated outside the formal sector. This compositional difference provides a plausible explanation for the opposing movements in within-firm and aggregate gender gaps. Entry into formal firms may also have become more selective for women than for men during the recovery. If women needed to clear a higher threshold to access expanding formal jobs, those observed in formal payrolls may have been disproportionately drawn from the upper end of the female skill distribution, raising the within-firm female average even as the broader set of female workers entered lower-paying segments. Because the ISSS data contain firm-level average wages rather than worker-level records, we cannot directly decompose the observed wage changes into incumbent effects and new-entrant compositional effects; the sorting interpretation rests on the convergence of evidence across multiple outcomes.

This compositional sorting interpretation is supported by three pieces of evidence. First, employed women's working hours increased by nearly one hour per week, consistent with hours-intensive informal work. Second, women's share of household earnings increased, indicating they are working more despite low wages. Third, informal employment did not decline after the crackdown (Table 9), meaning formal job creation did not crowd out informal work. Rather, women's entry was additive to the informal sector. Collectively, these three facts support a compositional sorting interpretation of the divergence between within-firm and aggregate gender outcomes.

An important limitation is that our household data are repeated cross-sections rather than a panel, so we cannot directly observe individual worker transitions between formal and informal employment. The sorting interpretation rests on converging evidence across multiple datasets and outcomes (within-firm wage dynamics, aggregate income patterns, hours worked, earnings shares, formal/informal employment probabilities, and sectoral composition) rather than on a single definitive test.

VII. Evidence on Gendered Sorting and Labor Market Constraints

The preceding sections document that the SoEx generated gender-asymmetric outcomes that operated differently across tiers of the labor market: within registered formal firms, gender pay gaps narrowed; across the economy-wide distribution, gender inequality widened. In this section, we evaluate empirically testable implications of the three channels identified in our conceptual framework (Section III). Of these, the labor market frictions channel, which predicts gendered sorting across job quality tiers, most directly accounts for the within-firm versus aggregate divergence documented above. We also present evidence on mobility constraints and intra-household reallocation as contributing factors. For each channel, we present the theoretical prediction alongside the corresponding empirical evidence. These exercises are intended to assess whether the data patterns align with the channels in our framework, rather than to causally isolate each mechanism.

Mobility constraints. Security improvements should be reflected in individuals' ability to move and participate in market work that requires spatial mobility, consistent with Melnikov et al. (2025), who show that people can be restricted from moving across gang- and non-gang-controlled territories that may influence the labor market. We test this implication using a behavioral proxy for individual mobility, defined as whether the respondent reports spending on any (i) transportation, (ii) fuel, or (iii) vehicle repair. As shown in Table 7, the proxy increases after the crackdowns in high-crime cities, but the increase is markedly weaker for women.

The sectoral pattern in the household data aligns with the firm-side heterogeneity: employment gains are concentrated in production- and infrastructure-oriented activities that are more mobility-dependent and more male-intensive. An important limitation of the mobility proxy is that transportation and fuel expenditures are likely jointly determined with employment, as individuals who find jobs, particularly in production and infrastructure, will mechanically increase commuting-related spending. The proxy therefore captures a behavioral outcome consistent with relaxed mobility constraints, but we cannot rule out reverse causality from employment to expenditure. The primary evidence for the mobility channel rests on the firm-side sectoral composition of employment gains (Table 2) rather than the household-level expenditure proxy alone. Consistent with this channel, the within-firm wage gains for women (Table 4) suggest that women who do access formal-sector positions benefit from the improved security environment, but the aggregate gender pay gap (Table 6) reflects that fewer women reach this higher-quality segment of the labor market.

[Insert Table 7 here]

Intra-household reallocation of non-market labor. Classic models of household behavior (Becker, 1965; Chiappori, 1992) emphasize that family members allocate their time between market and non-market activities to maximize joint utility, with decisions influenced by relative earnings potential, bargaining power, and social norms. In many settings, these forces lead to a persistent gendered division of labor, whereby women disproportionately take on unpaid caregiving and domestic work.

When public security improves and male employment rises, such as after a major crime crackdown, households may reoptimize by shifting non-market tasks toward women. This adjustment can occur either because men face greater labor market pull, or because traditional norms expect women to fill domestic gaps. Villanueva-Moya and Expósito (2025) show that women, more than men, often report higher utility from prioritizing household responsibilities over formal employment, especially when household income rises. While crime reduction

boosts aggregate employment, it may also entrench gendered roles and inhibit women's labor market participation.

We first test this channel by identifying within-household labor allocation, i.e., whether the post-crackdown employment response differs between men and women within the same household. In Table 5 Panel B, we restrict the sample to households in which both the household head and the spouse are of working age, and keep only these two individuals. This subsample allows a cleaner comparison of gender-specific employment responses holding constant household-level characteristics and constraints.

Column (1) estimates the logit DID model on this restricted sample with the same set of controls and fixed effects as in Panel A. The SoEx effect for men is substantially larger in this sample (0.413, $p<0.01$), while the triple interaction with female is negative and significant (−0.422, $p<0.01$), implying that women's employment gains are markedly smaller than men's even among comparable head–spouse households.

Columns (2) and (3) sharpen the within-household comparison by introducing household fixed effects using conditional logit (clogit), which absorbs all time-invariant household characteristics and focuses on within-household differences between the head and spouse. Because the EHPM is a repeated cross-section rather than a panel, the household fixed effect absorbs all characteristics of a given household within a single survey wave; the identifying variation comes from comparing the employment probability of the male head versus the female spouse within the same household, across high-crime and low-crime municipalities, before and after the SoEx. With household fixed effects, any regressor that does not vary within the household (e.g., the city-level treatment indicator, city, month, region-year fixed effects) is mechanically absorbed. The gender-differential effect remains: the coefficient on the triple interaction term remains negative and statistically significant (−0.364 in column 2; −0.361 in column 3 for households with children). This pattern is consistent with a reallocation of market work within the household rather than a uniform increase in employment for both spouses.

Additionally, we test the intra-household reallocation channel using non-market labor time-use data from the EHPM, focusing on hours spent on unpaid domestic activities, including

childcare, household chores, and resource collection. The results, presented in Table 8, reveal that following the SoEx, unemployed women in high-crime municipalities significantly increased their non-market labor. This effect is especially pronounced in households with children, in line with intra-household substitution dynamics.

[Insert Table 8 here]

Labor Market Frictions. Even when women seek to remain attached to the labor market, they may face persistent trade-offs and structural barriers that limit their participation in formal or permanent employment. Classic household models emphasize that caregiving responsibilities raise the opportunity cost of time for women, who often select jobs offering flexibility over stability to reconcile market work with non-market demands (Killingsworth and Heckman, 1986; Berniell et al., 2021). These constraints are exacerbated in contexts with limited childcare infrastructure or rigid job structures. Prior research documents that women tend to exhibit greater risk aversion in job selection, opting for safer or informal arrangements even when more secure opportunities arise (Cortés et al., 2023). Moreover, beyond preferences and constraints within the household, women also face broader frictions in the labor market, including higher search costs, weaker professional networks, and spatial or mobility constraints (Donovan et al., 2023). As a result, improvements in public safety that spur aggregate job growth may not be enough to equalize access to high-quality employment across genders.

We examine these dynamics using a multinomial logit model that categorizes individuals as unemployed, employed in permanent jobs, or employed in non-permanent jobs. Results in Table 9, Panel A show that the SoEx significantly increased the probability of permanent employment in high-crime areas. However, the triple interaction with gender is negative and significant, indicating that women were less likely than men to transition into stable roles.

[Insert Table 9 here]

In the second specification in Table 9 Panel B, we assess changes in employment in formal-sector firms, defined as firms registered with both the tax and social security authorities. These positions typically offer better job security and benefits but also demand more rigid schedules

and higher formal qualifications. We find that both formal-sector and informal-sector employment rose significantly in response to the 2022 SoEx, but women appear to benefit less in the formal sector. The gender-specific interaction term is negative and significant, implying that women remain systematically underrepresented in the formal sector even as the total number of such opportunities increases.

These findings point to two features of post-crackdown adjustment: employment shifts broadly toward more formal industry, yet women are systematically less likely than men to move into formal jobs. Informal employment does not decline, suggesting that the increase in formal jobs is not primarily driven by a reallocation from informal to formal work. Crime reduction thus appears to support broad-based labor market recovery, but persistent gender-specific frictions continue to shape who accesses higher-quality employment. These patterns provide a plausible compositional explanation for the divergence between within-firm and aggregate wage dynamics documented above: men appear to sort disproportionately into high-wage formal positions that narrow the within-firm pay gap, while women's employment gains are more concentrated outside the formal sector, widening the aggregate gender gap.

To further examine the sectoral composition of employment gains, we classify industries into formal-intensive and informal-intensive sectors following Bandiera et al. (2023), who categorize agriculture, construction, wholesale and retail, and restaurants as high-informality sectors. Table A.6 shows that employment gains within registered firms are overwhelmingly concentrated in formal-intensive industries, at a ratio of approximately 7:1. Women's employment gains in informal-intensive industries are indistinguishable from zero; the coefficient is negligible and statistically insignificant under both classification schemes (0.063 and 0.049, respectively). By contrast, men gain significantly in both sector types, though the formal-sector effect is roughly four times larger. Combined with the household survey evidence that employed women work longer hours without corresponding wage gains (Table 6), these results suggest that women's labor market entry appears to occur disproportionately outside the formal firm registries captured by administrative data—consistent with entry into unregistered informal employment that is not observed in the ISSS payroll records. The finding that women

gain zero employment in informal-intensive *registered* firms while aggregate household data show female employment rising suggests that much of the new female employment may be occurring in unregistered businesses and self-employment outside the administrative data.

VIII. Conclusion

This paper documents three facts about the labor market consequences of El Salvador's 2022 State of Exception. First, firm-level employment in high-crime municipalities rose by approximately 7%, with male gains exceeding female gains by more than 2:1. Second, within formal firms, the gender pay gap narrowed by $6 per month. Third, in household survey data, aggregate male wage income rose by $18 while female wages showed no improvement. The divergence between within-firm and aggregate gender wage responses is consistent with gender-asymmetric sorting across labor market segments: men disproportionately entered higher-quality formal positions in expanding industries, while women's employment gains appear to have been more concentrated in lower-quality positions outside the formal sector.

The evidence across multiple datasets points to persistent barriers in women's access to higher-quality jobs, reinforced by household reallocation dynamics and mobility-related constraints. While we do not claim to causally identify each channel, the convergence of firm-level sectoral evidence, household-level employment-type evidence, within-household comparisons, and time-use data supports the interpretation that structural barriers shape who benefits from security-induced labor market recovery.

The findings suggest that security improvements alone may not deliver gender-inclusive labor market recovery when access to stable, formal employment remains uneven across genders. Complementary policies addressing the structural barriers documented here, particularly women's access to formal-sector jobs, may be needed to ensure that the economic dividends of improved security are broadly shared. Because the SoEx was a nationwide policy, our difference-in-differences estimates capture relative effects of differential violence exposure

rather than absolute effects of security improvement; this scope condition should be borne in mind when interpreting the magnitudes.

Several limitations merit acknowledgment. Our analysis focuses on short-run labor market effects; whether the observed sorting patterns persist, intensify, or reverse as labor markets adjust over longer horizons remains an open question. We document mechanisms through correlational evidence across multiple datasets rather than isolating each channel causally. Future research could examine long-run wage trajectories, firm survival dynamics, and whether targeted interventions can improve women's access to formal employment following security shocks. Our analysis focuses on observed labor market outcomes and does not evaluate the broader welfare implications of the SoEx, including its documented human rights costs.

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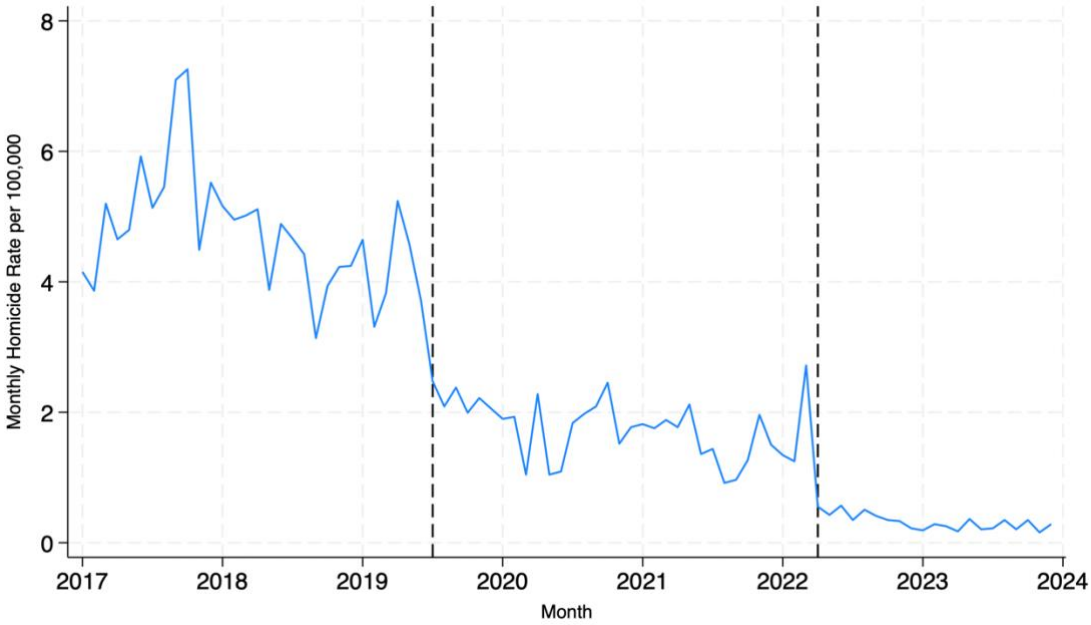


Figure 1. Monthly Homicide Rate in El Salvador

Notes: This figure shows the monthly homicide rate per 100,000 people in El Salvador from January 2017 to December 2023. The data are sourced from official government statistics and government official Twitter account (@PNCSV). The first vertical dashed line indicates July 2019, when the Territorial Control Plan (TCP) was implemented. The second vertical dashed line marks April 2022, the start of the State of Exception (SoEx). The graph illustrates a sharp and sustained decline in homicides over time, particularly following the 2019 and 2022 crackdowns.

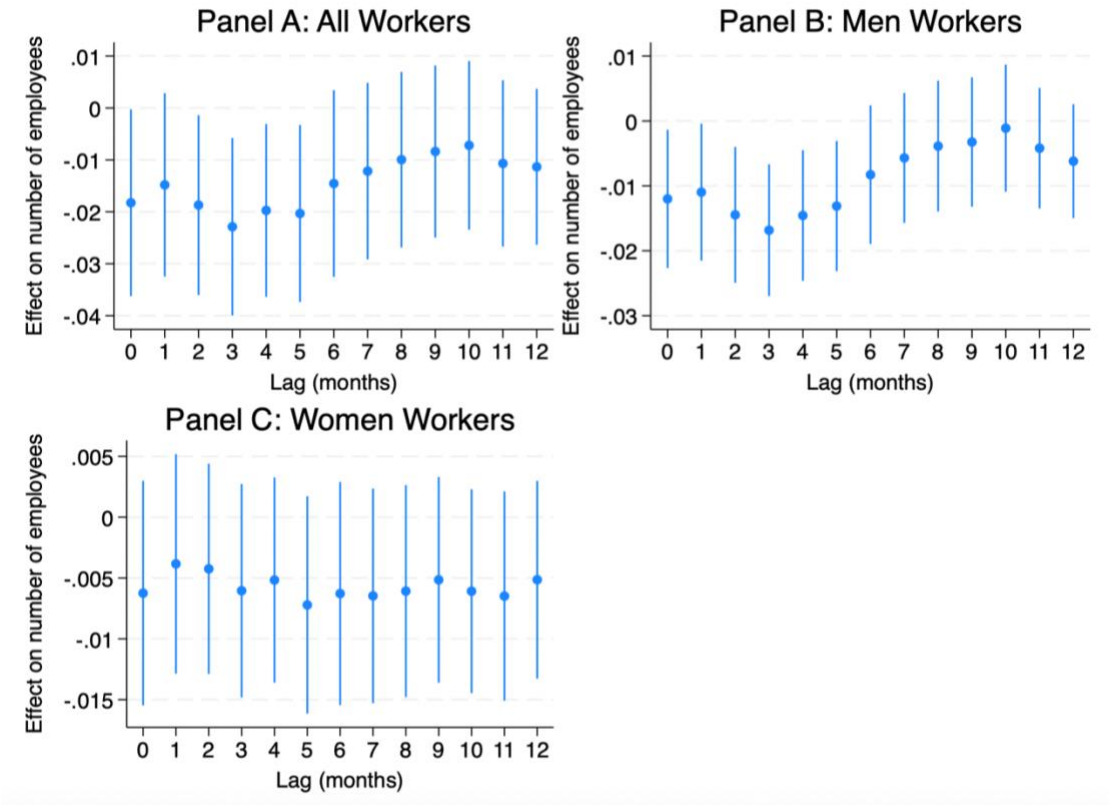


Figure 2. Lagged Effects of Homicide Rate on Employment by Gender

Notes: Each panel plots estimates of β_1^t from equation (1), capturing the dynamic effects of local homicide rates on firm-level employment. The dependent variable is the total number of employees at the firm, separately reported for all workers, women, and men. The key explanatory variables are the contemporaneous homicide rate by 100,000 people and its lags up to 12 months, measured at the city level. All specifications include firm fixed effects, city fixed effects, and region-by-industry-by-month-by-year fixed effects. Standard errors clustered at the city-by-month-by-year level.

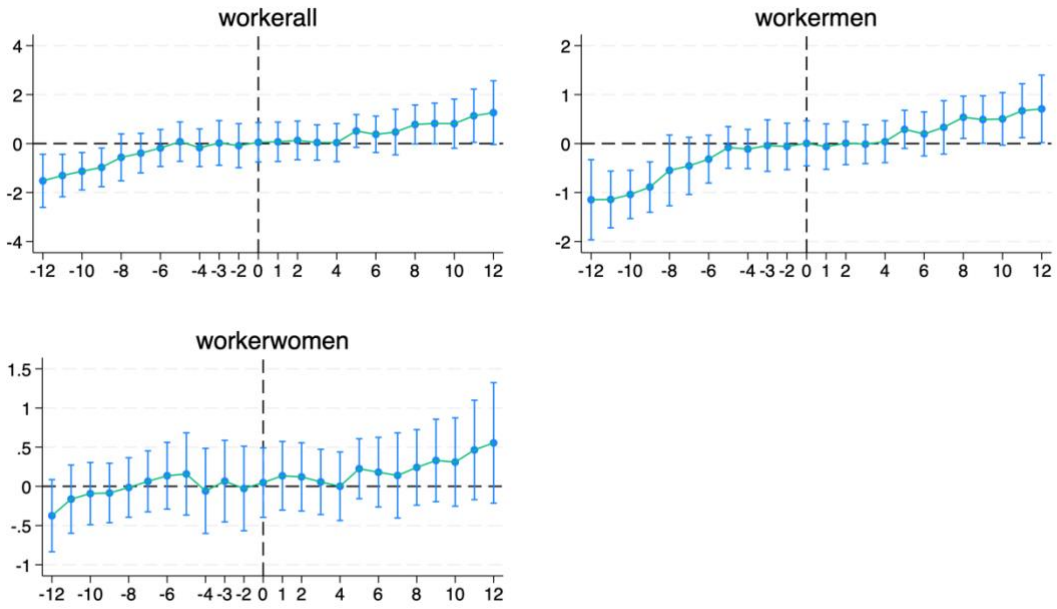


Figure 3. The dynamic impact of the 2022 SoEx on Firm Employment (Above Mean)

Notes: Each panel shows event-study estimates of the differential evolution of firm-level employment in high-crime versus low-crime cities around the onset of the 2022 State of Exception (SoEx). The outcome variable is the number of employees at the firm, separately for all workers, women, and men. The event window spans from 12 months before to 12 months after the onset of the SoEx (i.e., event time -12 to +12). Event time -1, corresponding to March 2022, is the omitted reference period. Each point represents the estimated monthly difference between treated and control cities (Treated groups denote cities with above-mean pre-SoEx homicide rates). All regressions include firm fixed effects, city fixed effects, and region-by-industry-by-month-by-year fixed effects. Standard errors are clustered at the city-by-month-by-year level. Error bars indicate 95% confidence intervals.

Table 1—Anti-Gang Crackdown and Firm Employment

	All	Women	Men
	(1)	(2)	(3)
Panel A: Number of employees at firm (SoEx)			
Above_Mean_22 * Post_SoEx	2.018***	0.582***	1.436***
	(0.260)	(0.132)	(0.148)
Adjusted R-squared	0.9529	0.9508	0.9503
Panel B: Number of employees at firm (SoEx and TCP)			
Above_Mean_22 * Post_SoEx	2.270***	0.728***	1.542***
	(0.256)	(0.134)	(0.143)
Above_Mean_19 * Post_TCP	0.911***	0.529***	0.382***
	(0.237)	(0.135)	(0.133)
T-stat: SoEx vs TCP	4.01	1.16	5.85
P-value: SoEx vs TCP	0.000	0.244	0.000
Adjusted R-squared	0.9529	0.9508	0.9503
Observations	1,398,244	1,398,244	1,398,244
Unique Firm	25,669	25,669	25,669
Firm FEs	√	√	√
City FEs	√	√	√
Region-Industry-Year-Month FEs	√	√	√

Notes: This table reports difference-in-differences (DID) estimates of the impact of anti-gang crackdown on firm-level employment. The dependent variable is the number of employees at the firm, separately for all workers, women, and men. In panel A, the independent variable *Above_Mean_22* denotes cities with above-mean pre-SoEx crackdown homicide rates, and *Post_SoEx* denotes the period after April 2022. Panel B adds *Above_Mean_19* denotes cities with above-mean pre-TCP crackdown homicide rates. *Post_TCP* denotes the period between July 2019 to Feb 2022. Regressions include firm fixed effects, city fixed effects, and region-by-industry-by-month-by-year fixed effects. Standard errors clustered at the city-by-month-by-year level are reported in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively. √ denotes included in the regression.

Table 2—Anti-Gang Crackdown And Firm Employment: Heterogeneity Tests

	Dependent variable: Number of employees		
	All	Women	Men
	(1)	(2)	(3)
Panel A: Industry heterogeneity			
Above_Mean_22 * Post_SoEx	0.921***	-0.275**	1.197***
	(0.282)	(0.118)	(0.290)
Above_Mean_22 * Post_SoEx * Consumption Industry	-1.073***	0.197	-1.270***
	(0.339)	(0.125)	(0.350)
Above_Mean_22 * Post_SoEx * Production Industry	3.952***	2.420***	1.533***
	(0.870)	(0.612)	(0.484)
Above_Mean_22 * Post_SoEx * Infrastructure Industry	4.858***	2.281***	2.577***
	(0.657)	(0.234)	(0.515)
Adjusted R-squared	0.9529	0.9508	0.9503
Observations	1,398,244	1,398,244	1,398,244
Unique Firm	25,669	25,669	25,669
Panel B: Firm age heterogeneity			
Above_Mean_22 * Post_SoEx	3.711***	1.165***	2.546***
	(0.307)	(0.145)	(0.185)
Above_Mean_22 * Post_SoEx * Firm Age	-0.080***	-0.024***	-0.056***
	(0.012)	(0.006)	(0.007)
Above_Mean_19 * Post_TCP	2.212***	1.047***	1.165***
	(0.295)	(0.138)	(0.188)
Above_Mean_19 * Post_TCP * Firm Age	-0.081***	-0.032***	-0.049***
	(0.011)	(0.004)	(0.008)
Firm Age	-0.964***	0.058***	-1.022***
	(0.212)	(0.014)	(0.208)
Adjusted R-squared	0.9529	0.9508	0.9503
Observations	1,398,155	1,398,155	1,398,155
Unique Firm	25,668	25,668	25,668
Firm FEs	√	√	√
City FEs	√	√	√
Region-Industry-Year-Month FEs	√	√	√

Notes: This table reports difference-in-differences (DID) estimates of the impact of anti-gang crackdown on firm-level employment, with a focus on heterogeneity by firm industry and firm age. The dependent variable is the number of employees at the firm, separately for all workers, women, and men. The independent variable *Above_Mean_22* denotes cities with above-mean pre-SoEx homicide rates, and *Post_SoEx* denotes the period after April 2022. *Above_Mean_19* denotes cities with above-mean pre-TCP crackdown homicide rates. *Post_TCP* denotes the period between July 2019 to Feb 2022. In panel A, the industries are categorized by

Consumption Industry, Production Industry, Infrastructure Industry, and Other Industry. In Panel B, *Firm age* is measured as the number of years between the firm's founding year and the year of observation. All regressions include firm fixed effects, city fixed effects, and region-by-industry-by-month-by-year fixed effects. Standard errors are clustered at the city-by-month-by-year level. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively. √ denotes included in the regression.

Table 3—Anti-Gang Crackdown and Firm Expansion

	All	Women	Men
	(1)	(2)	(3)
Panel A: Number of employees at workplace			
Above_Mean_22 * Post_SoEx	1.361***	0.451***	0.910***
	(0.432)	(0.160)	(0.289)
Above_Mean_19 * Post_TCP	1.328***	0.718***	0.610***
	(0.232)	(0.130)	(0.132)
T-stat: 2022 vs 2019	0.06	-1.33	0.84
P-value: 2022 vs 2019	0.951	0.185	0.402
Adjusted R-squared	0.9301	0.9358	0.9242
Observations	1,536,167	1,536,167	1,536,167
Unique Workplace	28,118	28,118	28,118
Workplace FEs	√	√	√
City FEs	√	√	√
Region-Industry-Year-Month FEs	√	√	√

	Firm level	City level	
	# of workplaces	# of workplaces	# of firms
	(1)	(2)	(3)
Panel B: Number of workplaces and firms			
Above_Mean_22 * Post_SoEx	0.003*	1.833*	2.773***
	(0.002)	(1.067)	(0.936)
Above_Mean_19 * Post_TCP	-0.001	-1.590***	-1.216***
	(0.001)	(0.487)	(0.467)
T-stat: 2022 vs 2019	2.17	2.34	3.14
P-value: 2022 vs 2019	0.030	0.019	0.002
Adjusted R-squared	0.9098	0.9987	0.9986
Observations	1,398,244	21,070	21,070
Unique Firm/City	25,669	259	259
Firm FEs	√		
City FEs	√	√	√
Region-Industry-Year-Month FEs	√		
Region-Year-Month FEs		√	√

Notes: This table reports difference-in-differences (DID) estimates of the impact of anti-gang crackdown on firm-level expansion. In Panel A, the dependent variable is the number of employees at the workplace. In Panel B, column 1, the dependent variable is the number of workplaces at the firm. In Panel B, columns 2–3, the dependent variable is the number of workplaces and firms at the city level, respectively. The independent variable *Above_Mean_22* denotes cities with above-mean pre-SoEx homicide rates, and *Post_SoEx* denotes the period

after April 2022. *Above_Mean_19* denotes cities with above-mean pre-TCP crackdown homicide rates. *Post_TCP* denotes the period between July 2019 to Feb 2022. In Panel A, regressions include workplace fixed effects, city fixed effects, and region-by-industry-by-month-by-year fixed effects. In Panel B, column 1, regressions include firm fixed effects, city fixed effects, and region-by-industry-by-month-by-year fixed effects. In Panel B, columns 2–3, regressions include city fixed effects and region-by-month-by-year fixed effects. For all regressions, standard errors clustered at the city-by-month-by-year level are reported in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively. √ denotes included in the regression.

Table 4—Anti-Gang Crackdown and Firm Wages

	Average Wages			Pay Gap
	All	Women	Men	Men - Women
	(1)	(2)	(3)	(4)
Panel A: Baseline results				
Above_Mean_22 * Post_SoEx	3.897***	7.114***	1.582	-6.239***
	(0.752)	(1.150)	(0.977)	(1.641)
Above_Mean_19 * Post_TCP	1.449**	3.929***	0.332	-3.575***
	(0.722)	(1.019)	(0.880)	(1.342)
T-stat: 2022 vs 2019	2.67	2.46	1.14	-1.62
P-value: 2022 vs 2019	0.008	0.014	0.253	0.105
Adjusted R-squared	0.8394	0.7789	0.8411	0.6516
Observations	1,398,168	1,160,160	1,257,007	1,019,082
Panel B: Industry Heterogeneity				
Above_Mean_22 * Post_SoEx	-0.045	-1.103	-4.460***	-1.639
	(1.290)	(1.505)	(1.614)	(2.288)
Above_Mean_22 * Post_SoEx *	0.389	10.278***	-1.202	-17.839***
Consumption Industry	(1.705)	(2.389)	(2.366)	(3.439)
Above_Mean_22 * Post_SoEx *	10.197***	12.216***	17.594***	6.565**
Production and Infrastructure Industry	(2.368)	(2.455)	(2.773)	(3.165)
Adjusted R-squared	0.8394	0.7789	0.8411	0.6516
Observations	1,398,168	1,160,160	1,257,007	1,019,082
Unique Firm	25,669	22,405	23,581	20,149
Firm FEs	√	√	√	√
City FEs	√	√	√	√
Region-Industry-Year-Month FEs	√	√	√	√

Notes: This table reports difference-in-differences (DID) estimates of the impact of anti-gang crackdown on firm-level average wage. In columns 1–3, the dependent variable is the average wage per employee at the firm, separately for all workers, women, and men. In column 4, the dependent variable is the gender pay gap, measured as the difference between the firm-level average wage of men and that of women (Men − Women). In panel A, the independent variable *Above_Mean_22* denotes cities with above-mean pre-SoEx crackdown homicide rates, and *Post_SoEx* denotes the period after April 2022. *Above_Mean_19* denotes cities with above-mean pre-TCP crackdown homicide rates. *Post_TCP* denotes the period between July 2019 to Feb 2022. Panel B reports heterogeneous effects of the 2022 SoEx across broad industry groups. The industries are categorized by *Consumption Industry, Production and Infrastructure Industry, and Other Industry*. Regressions include firm fixed effects, city fixed effects, and region-by-industry-by-month-by-year fixed effects. Standard errors clustered at the city-by-month-by-year level are reported in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively. √ denotes included in the regression.

Table 5—Anti-Gang Crackdown and Individual Labor Allocation

	Y: Employed		
	(1)	(2)	(3)
Panel A: Full working-age sample			
Above_Mean_22 * Post_SoEx	0.108***	0.123**	0.125**
	(0.037)	(0.053)	(0.053)
Above_Mean_22 * Post_SoEx * Female	-0.106**	-0.117**	-0.120**
	(0.043)	(0.058)	(0.058)
Above_Mean_19 * Post_TCP	-0.017	-0.081	-0.078
	(0.035)	(0.049)	(0.050)
Above_Mean_19 * Post_TCP * Female	0.003	0.065	0.067
	(0.041)	(0.053)	(0.053)
Female	-1.399***	-2.163***	-2.187***
	(0.017)	(0.023)	(0.023)
Pseudo R-squared	0.0865	0.1877	0.1955
Observations	173,660	139,148	139,148
City FEs	√	√	√
Month FEs	√	√	√
Region-Year FEs	√	√	√
Individual controls		√	√
Household controls			√
	(1)	(2)	(3)
Panel B: Subsample—Working-age head–spouse pairs			
Above_Mean_22 * Post_SoEx	0.413***	Absorbed	Absorbed
	(0.105)		
Female	-3.315***	-3.066***	-3.179***
	(0.042)	(0.055)	(0.064)
Above_Mean_22 * Post_SoEx * Female	-0.422***	-0.364**	-0.361**
	(0.110)	(0.143)	(0.159)
Above_Mean_19 * Post_TCP	-0.051	Absorbed	Absorbed
	(0.091)		
Above_Mean_19 * Post_TCP * Female	0.023	0.096	0.099
	(0.094)	(0.113)	(0.130)
Pseudo R-squared	0.3393	0.7568	0.7728
Observations	58,943	30,446	25,502
City FEs	√		
Month FEs	√		
Region-Year FEs	√		

Household FEs		√	√
Individual controls	√	√	√
Household controls	√		

Notes: This table reports estimates of the impact of anti-gang crackdown on individual labor allocation using household survey data. Dependent variable *Employed* is a binary indicator equal to one if the respondent is currently employed, 0 for unemployed. The independent variable *Above_Mean_22* denotes cities with above-mean pre-SoEx homicide rates, and *Post_SoEx* denotes the period after April 2022. *Above_Mean_19* denotes cities with above-mean pre-TCP crackdown homicide rates. *Post_TCP* denotes the period between July 2019 to Feb 2022. In panel A, we use a logit model. All regressions include city, month, and region-by-year fixed effects. Columns 2 and 3 add individual controls, including age, age squared, gender, marital status, urban status, and education. Column 3 adds household controls, including household head age, head gender, household members, and emigration. In Panel B, the sample is restricted to working-age head–spouse pairs. In column 1, we use a logit model with the same specification as in Panel A column 3. In columns 2 and 3, we use a conditional logit model and introduce household fixed effects, which absorb household controls, city-level treatment indicator, city, month, and region-by-year fixed effects. In column 3, we restrict the sample to households with children. Standard errors are clustered at the city-by-year-month level. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively. √ denotes included in the regression.

Table 6—Anti-Gang Crackdown and Individual Income

	Dependent Variables			
	Employment Income	Self-Employment Income	Income from Non-Employment Activities	Total Income
	(1)	(2)	(3)	(4)
Panel A: Full Sample				
Above_Mean_22 * Post_SoEx	18.217***	-3.999	-2.688***	13.824***
	(3.769)	(4.543)	(0.473)	(5.360)
Above_Mean_22 * Post_SoEx * Female	-25.990***	2.996	4.015***	-20.934***
	(3.956)	(4.453)	(0.556)	(5.542)
Above_Mean_19 * Post_TCP	-2.590	-3.782	-0.096	-5.817
	(3.164)	(3.411)	(0.581)	(4.358)
Above_Mean_19 * Post_TCP * Female	-0.979	6.112*	1.362**	6.187
	(3.457)	(3.347)	(0.630)	(4.506)
Female	-123.151***	-23.740***	0.923***	-152.874***
	(1.411)	(1.752)	(0.296)	(2.154)
Adjusted R-squared	0.2425	0.0294	0.0341	0.1983
Observations	139,148	139,148	139,148	139,148
Panel B: Subsample: Employed only				
Above_Mean_22 * Post_SoEx	10.853**	-5.011	-2.422***	6.098
	(4.231)	(5.868)	(0.520)	(6.191)
Above_Mean_22 * Post_SoEx * Female	-12.978***	3.297	3.780***	-8.083
	(4.640)	(6.229)	(0.641)	(6.584)
Above_Mean_19 * Post_TCP	-5.140	-3.798	0.124	-7.837
	(3.579)	(4.580)	(0.694)	(5.166)
Above_Mean_19 * Post_TCP * Female	2.996	8.332*	1.466*	12.076**
	(4.426)	(4.949)	(0.792)	(5.492)
Female	-85.753***	7.112***	0.401	-74.014***
	(1.852)	(2.546)	(0.311)	(2.759)
Adjusted R-squared	0.2220	0.0325	0.0283	0.1501
Observations	88,888	88,888	88,888	88,888
Individual controls	√	√	√	√
Household controls	√	√	√	√
	Working Hours Per Week		Hourly Pay	
	(1)	(2)	(3)	(4)
Panel C: Working Hours and Hourly Pay				
Above_Mean_22 * Post_SoEx	0.068	0.125	0.166	0.111
	(0.287)	(0.284)	(0.104)	(0.095)

Above_Mean_22 * Post_SoEx * Female	0.833***	0.791***	-0.311***	-0.276***
	(0.263)	(0.267)	(0.103)	(0.100)
Above_Mean_19 * Post_TCP	-0.331	-0.284	0.004	-0.054
	(0.280)	(0.279)	(0.089)	(0.082)
Above_Mean_19 * Post_TCP * Female	0.200	0.052	0.009	0.065
	(0.271)	(0.271)	(0.104)	(0.100)
Female	0.551***	0.941***	-1.317***	-1.669***
	(0.117)	(0.121)	(0.040)	(0.041)
Adjusted R-squared	0.0257	0.0395	0.0822	0.2062
Observations	96,932	88,888	96,932	88,888
Individual controls		√		√
Household controls		√		√
City FEs	√	√	√	√
Month FEs	√	√	√	√
Region-Year FEs	√	√	√	√

Notes: This table reports difference-in-differences (DID) estimates of the impact of anti-gang crackdowns on individual income using household survey data. In panel A and B, the dependent variables are *employment income, self-employment income, income from non-employment activities, and total income*, respectively. In panel C, the dependent variable is *working hours per week*, which is defined as the usual weekly hours worked; *hourly pay,* which is defined as monthly employment income divided by estimated monthly hours worked, where monthly hours are calculated as hours worked in the previous week plus 3.33 times usual weekly hours worked. In Panel B and C, the sample is restricted to employed individuals with total income greater than zero. The independent variable *Above_Mean_22* denotes cities with above-mean pre-SoEx homicide rates, and *Post_SoEx* denotes the period after April 2022. *Above_Mean_19* denotes cities with above-mean pre-TCP crackdown homicide rates. *Post_TCP* denotes the period between July 2019 to Feb 2022. All regressions include city, month, and region-by-year fixed effects. In panel A and B, all regressions include individual controls, including age, age squared, gender, marital status, urban and education, and household controls, including household head age, head gender, household members and emigration. In panel C, column 2 adds individual controls, and column 3 further adds household controls. Standard errors clustered at the city-by-year-month level are reported in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively. √ denotes included in the regression.

Table 7—Anti-Gang Crackdown and Individual Mobility

	Y: Individual Mobility Proxy		
	(1)	(2)	(3)
Above_Mean_22 * Post_SoEx	0.131***	0.114***	0.116***
	(0.036)	(0.040)	(0.040)
Above_Mean_22 * Post_SoEx * Female	-0.144***	-0.122***	-0.128***
	(0.037)	(0.040)	(0.040)
Above_Mean_19 * Post_TCP	0.057*	0.033	0.036
	(0.033)	(0.037)	(0.037)
Above_Mean_19 * Post_TCP * Female	-0.099***	-0.088**	-0.090**
	(0.034)	(0.038)	(0.038)
Female	-0.637***	-0.838***	-0.862***
	(0.015)	(0.017)	(0.017)
Pseudo R-squared	0.0770	0.1312	0.1352
Observations	173,606	139,104	139,104
Firm FEs	√	√	√
City FEs	√	√	√
Region-Industry-Year-Month FEs	√	√	√
Individual controls		√	√
Household controls			√

Notes: This table reports estimates of the impact of anti-gang crackdown on individual mobility. In all regressions, we use a logit model. Dependent variable *Individual Mobility Proxy* is a binary indicator equal to one if the respondent reports any spending on (i) transportation, (ii) fuel, or (iii) vehicle repair, and zero otherwise. The independent variable *Above_Mean_22* denotes cities with above-mean pre-SoEx homicide rates, and *Post_SoEx* denotes the period after April 2022. *Above_Mean_19* denotes cities with above-mean pre-TCP crackdown homicide rates. *Post_TCP* denotes the period between July 2019 to Feb 2022. All regressions include city, month, and region-by-year fixed effects. Column 2 and 3 add individual controls, including age, age squared, gender, marital status, urban and education. Column 3 adds household controls, including household head age, head gender, household members and emigration. Standard errors are clustered at the city-by-year-month level. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively. √ denotes included in the regression.

Table 8—Anti-Gang Crackdown and Household Responsibility

	Y: Housework hours per week			
	Full samples		With children family	
	(1)	(2)	(3)	(4)
Female * Unemployment	5.623***	5.612***	5.818***	5.751***
	(0.128)	(0.177)	(0.166)	(0.228)
Female * Unemployment * Post_SoEx	0.770***	0.715***	1.237***	1.285***
	(0.215)	(0.247)	(0.284)	(0.322)
Female * Unemployment * Post_TCP		-0.009		0.235
		(0.247)		(0.316)
Adjusted R-squared	0.1100	0.1117	0.0930	0.0977
Observations	139,148	139,148	90,824	90,824
Firm FEs	√	√	√	√
City FEs	√	√	√	√
Region-Industry-Year-Month FEs	√	√	√	√
Individual controls	√	√	√	√
Household controls	√	√	√	√

Notes: This table reports estimates of the impact of anti-gang crackdown on household responsibility allocation. we use an OLS model, the dependent variable is the number of hours per week spent on unpaid domestic labor. The independent variable *Above_Mean_22* denotes cities with above-mean pre-SoEx homicide rates, and *Post_SoEx* denotes the period after April 2022. *Above_Mean_19* denotes cities with above-mean pre-TCP crackdown homicide rates. *Post_TCP* denotes the period between July 2019 to Feb 2022. *Unemployment* is a binary indicator equal to one if the respondent is currently unemployed, 0 for employed. All regressions include individual controls include age, age squared, gender, marital status, urban and education; household controls, including household head age, head gender, household members and emigration as well as city, month, and region-by-year fixed effects. Standard errors are clustered at the city-by-year-month level. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively. √ denotes included in the regression.

Table 9—Anti-Gang Crackdown and Individual Employment Type

	Y: Multinomial Employment Types (Permanent job (P), Others (O), Unemployed (U))			
	P vs U	O vs U	P vs U	O vs U
	(1)	(2)	(3)	(4)
Panel A: Employed as Permanent job				
Above_Mean_22 * Post_SoEx	0.151***	0.069	0.173***	0.083
	(0.041)	(0.043)	(0.058)	(0.057)
Above_Mean_22 * Post_SoEx * Female	-0.144***	-0.068	-0.170***	-0.081
	(0.049)	(0.049)	(0.063)	(0.062)
Above_Mean_19 * Post_TCP	-0.003	-0.038	-0.076	-0.094*
	(0.039)	(0.040)	(0.054)	(0.053)
Above_Mean_19 * Post_TCP * Female	-0.040	0.050	0.035	0.104*
	(0.044)	(0.049)	(0.058)	(0.059)
Pseudo R-squared	0.0775		0.1648	
Observations	173,660		139,148	
	(Formal Firm (F), Others (O), Unemployed (U))			
	F vs U	O vs U	F vs U	O vs U
	(1)	(2)	(3)	(4)
Panel B: Employed in a formal firm				
Above_Mean_22 * Post_SoEx	0.156***	0.083**	0.168***	0.102*
	(0.046)	(0.040)	(0.062)	(0.055)
Above_Mean_22 * Post_SoEx * Female	-0.196***	-0.061	-0.230***	-0.080
	(0.054)	(0.046)	(0.070)	(0.060)
Above_Mean_19 * Post_TCP	-0.009	-0.026	-0.099*	-0.078
	(0.042)	(0.038)	(0.057)	(0.052)
Above_Mean_19 * Post_TCP * Female	-0.066	0.038	0.027	0.084
	(0.047)	(0.046)	(0.061)	(0.057)
Pseudo R-squared	0.0853		0.1977	
Observations	173,660		139,148	
City FEs	√		√	
Month FEs	√		√	
Region-Year FEs	√		√	
Individual controls			√	
Household controls			√	

Notes: This table reports estimates of the impact of anti-gang crackdown on individual employment type. In all regressions, we use a multinomial logit model. Specifically, in Panel A, the dependent variable equals 1 if the respondent is currently employed as a permanent employee (P), 2 for non-permanent employee (Other), versus 0 for unemployed. In Panel B, the

dependent variable equals 1 if the respondent is currently employed in a formal firm (F), defined as a firm registered with both the tax and social security authorities, 2 for non-formal firm (Other), versus 0 for unemployed. The independent variable *Above_Mean_22* denotes cities with above-mean pre-SoEx homicide rates, and *Post_SoEx* denotes the period after April 2022. *Above_Mean_19* denotes cities with above-mean pre-TCP crackdown homicide rates. *Post_TCP* denotes the period between July 2019 to Feb 2022. Individual controls include age, age squared, gender, marital status, urban and education. Household controls, including household head age, head gender, household members and emigration. All regressions include city, month, and region-by-year fixed effects. Standard errors are clustered at the city-by-year-month level. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively. √ denotes included in the regression.

Online Appendix

I. Robustness Tests for Causal Effects

In order to verify that our results are not sensitive to the construction of the treatment variable, Table A.3 provides robustness checks using alternative constructions of high-crime exposure and institutional conditions. Panel A classifies high-crime cities as those with pre-crackdown homicide rates at or above the 75th percentile of the national distribution, while Panel B uses a stricter threshold at the 90th percentile. In both cases, the estimated effects of the 2022 SoEx remain large and statistically significant across all worker groups, and the difference between SoEx and TCP remains statistically significant at the 1 percent level. Panel C uses the mean homicide rate between March 2021 and February 2022, deliberately excluding the spike in March 2022, to avoid distortion from short-term anomalies in treatment classification. The results continue to support the main findings.

We further assess the robustness of our results by considering an alternative construction of treatment based on the strength of state institutional presence. The rationale is that municipalities with a higher density of state infrastructure—such as police stations, military facilities, and government administrative buildings—may have greater capacity to enforce the SoEx measures more effectively. We define high state presence as cities with above-mean density of such institutions per 100,000 residents, drawing on administrative spatial data from Open Street Maps. Panel D in Table A.3 show that re-estimating our baseline specification using this alternative treatment definition yields positive and statistically significant employment effects across all worker groups, reinforcing the conclusion that the results are not sensitive to how treated cities are defined. In Panel E, we use a continuous measure of treatment intensity by interacting the pre-policy annual homicide rate. The resulting estimates remain positive and statistically significant, indicating stronger employment responses in municipalities with higher baseline violence.

To further assess the robustness of our findings, we first address the overlap between the TCP and SoEx, as the earlier crackdown may have influenced the dynamics of violence across municipalities prior to the implementation of the 2022 SoEx. In Panel A of Table A.4, we exclude all cities that were treated under both the 2019 TCP and the 2022 SoEx, restricting the sample to municipalities newly exposed to the 2022 crackdown. The estimated effects remain strong and statistically significant across all outcome groups, indicating that our findings are not driven by residual effects from the earlier policy.

We also examine the possibility of spatial spillover effects between treated and control cities. If gang activity or state enforcement actions spilled over into neighboring municipalities, treatment effects may be biased. To better identify and isolate such spillovers, Panels B and C in Table A.4 define treated cities based on the 90^{th} percentile of the national homicide rate distribution. This stricter threshold is necessary because under the above-mean definition, nearly all neighboring municipalities would also be classified as treated, making it difficult to identify adjacent controls for comparison. Parallel pre-treatment trends for the decile-based classification are verified in Figure A.4.

In Panel B of Table A.4, we exclude all cities that share a border with SoEx-treated municipalities. The results remain positive and statistically significant, suggesting that any potential spillovers into neighboring cities are not driving our main findings. In Panel C, we conduct a placebo test by reclassifying as treated those municipalities that neighbor SoEx-treated cities, while excluding the originally treated cities. The estimated effects in this placebo group are small and statistically indistinguishable from zero, reinforcing the conclusion that observed employment gains are concentrated in directly treated areas and are not artifacts of spatial spillovers.

To further ensure that our findings are not confounded by concurrent changes in emigration dynamics, we incorporate country–month level data on apprehensions of illegal or undocumented migrants by the U.S. Border Patrol, following Abreha et al. (2025). Table A.5 presents the results from this specification, which adds triple interaction terms with monthly apprehension levels. The estimated employment effects remain positive and statistically

significant, but the interaction terms are insignificant, indicating that fluctuations in emigration intensity do not explain the observed employment gains.

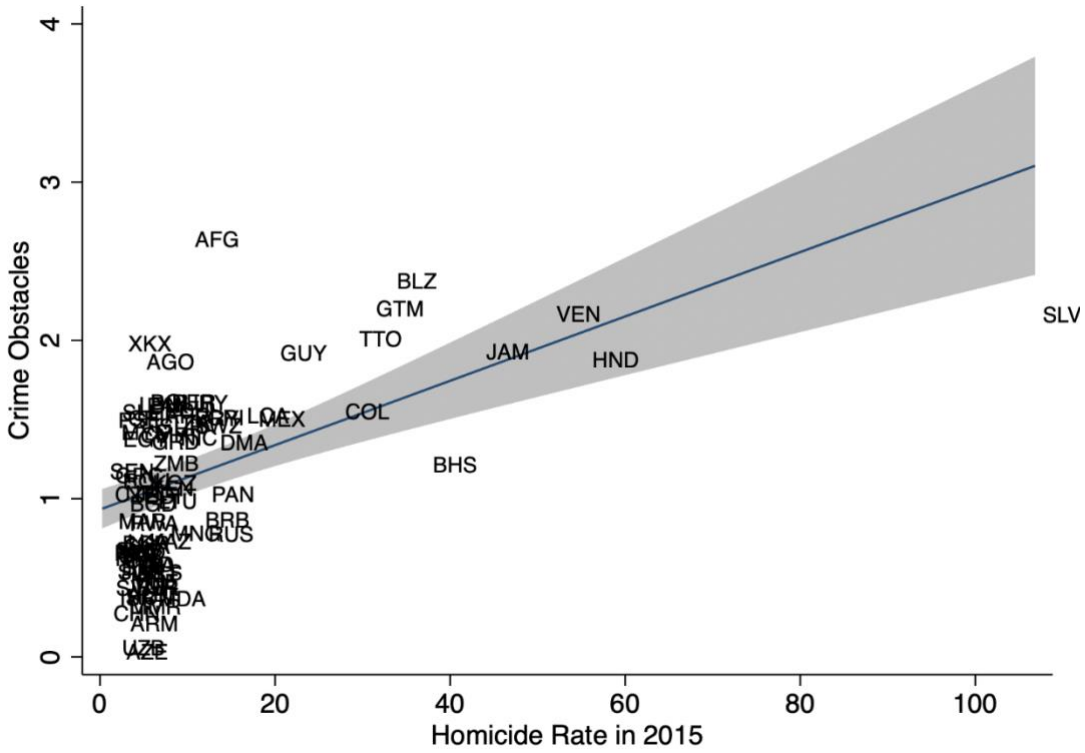
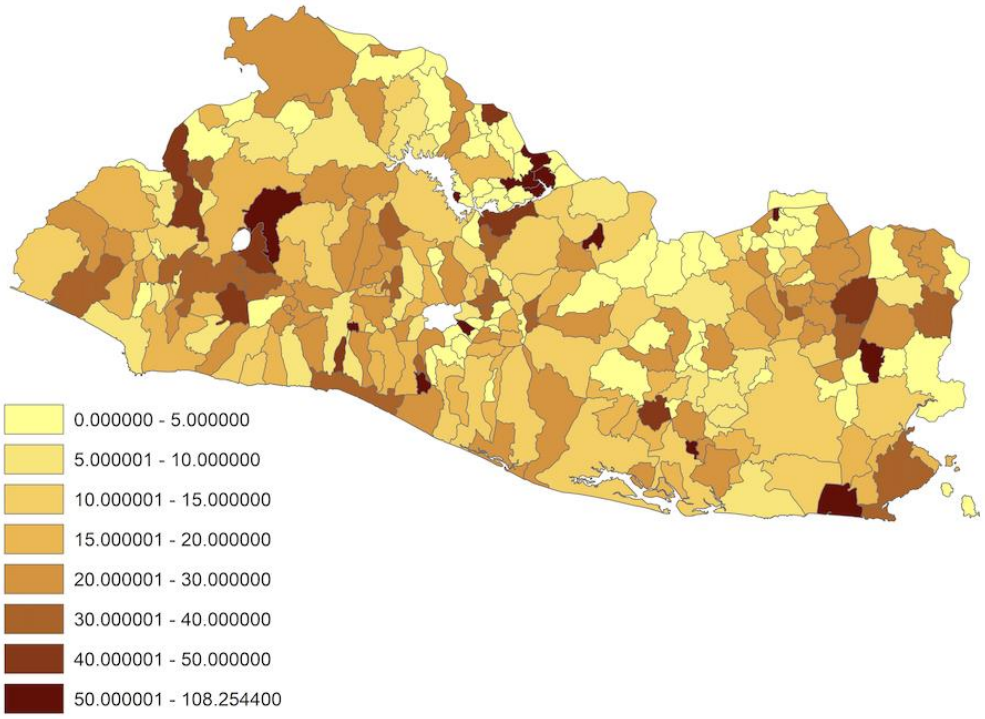


Figure A.1. Homicide Rate And Self-Reported Crime As Obstacle

Notes: This figure plots the relationship between a country's homicide rate and the extent to which crime is reported as an obstacle to doing business. The x-axis represents the homicide rate per 100,000 people in 2015, based on data from the World Bank. The y-axis indicates the average crime obstacle score as reported by firms in the World Bank Enterprise Surveys conducted between 2010 and 2018, where higher values correspond to a greater share of firms identifying crime as a severe obstacle. The fitted line is accompanied by a 95% confidence interval. Country codes represent individual observations. "SLV" is El Salvador.

Panel A: Apr 2021 – Mar 2022



Panel B: Apr 2022 – Mar 2023

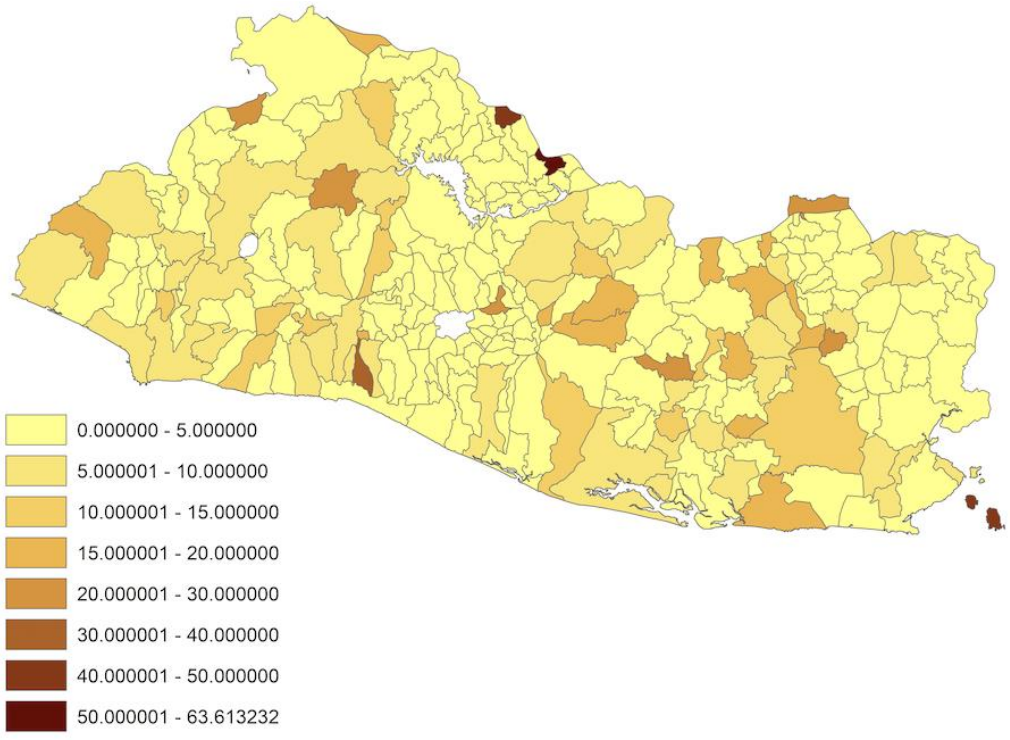


Figure A.2. Annual Homicide Rate Before And After The State of Exception

Notes: The two maps show municipality-level average annual homicide rates before (Panel A) and after (Panel B) the implementation of the State of Exception in March 2022. Darker shades represent higher homicide rates.

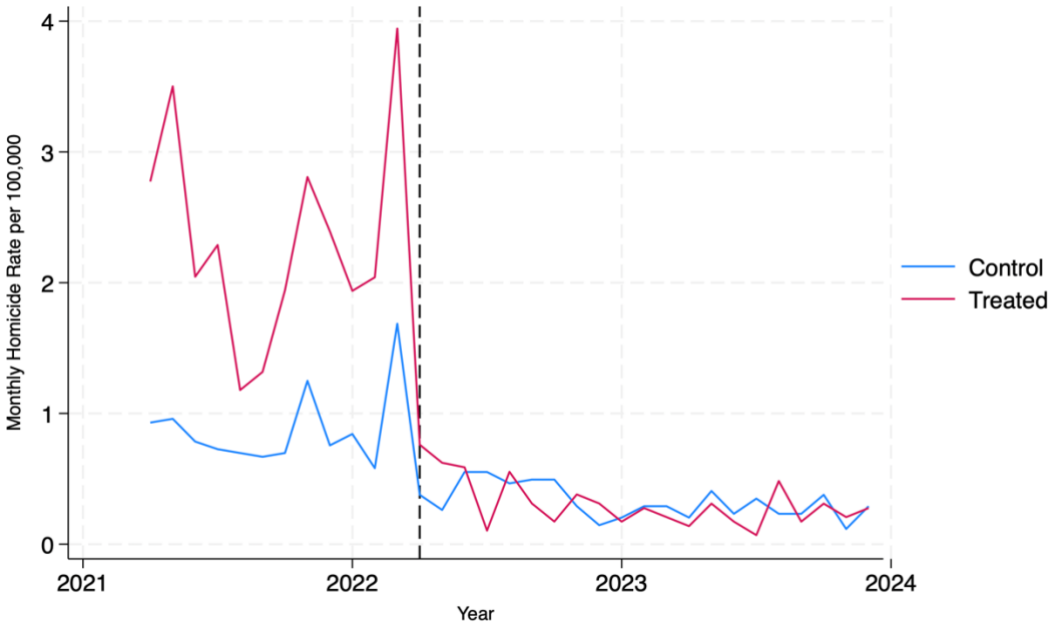


Figure A.3. Monthly Homicide Rates in Treated and Control Cities

Notes: This figure plots the average monthly homicide rate per 100,000 people for treated and control cities in El Salvador over the sample period (Apr 2021 to Dec 2023). Treated cities are those with above-mean pre-SoEx homicide rates (Apr 2021 to Mar 2022), while control cities are those with below-mean pre-SoEx homicide rates. The vertical dashed line marks April 2022, the start of the State of Exception (SoEx). The figure shows that treated cities had substantially higher homicide rates before the crackdown, but homicide rates declined sharply in both groups after SoEx, with the gap between treated and control cities narrowing considerably thereafter.

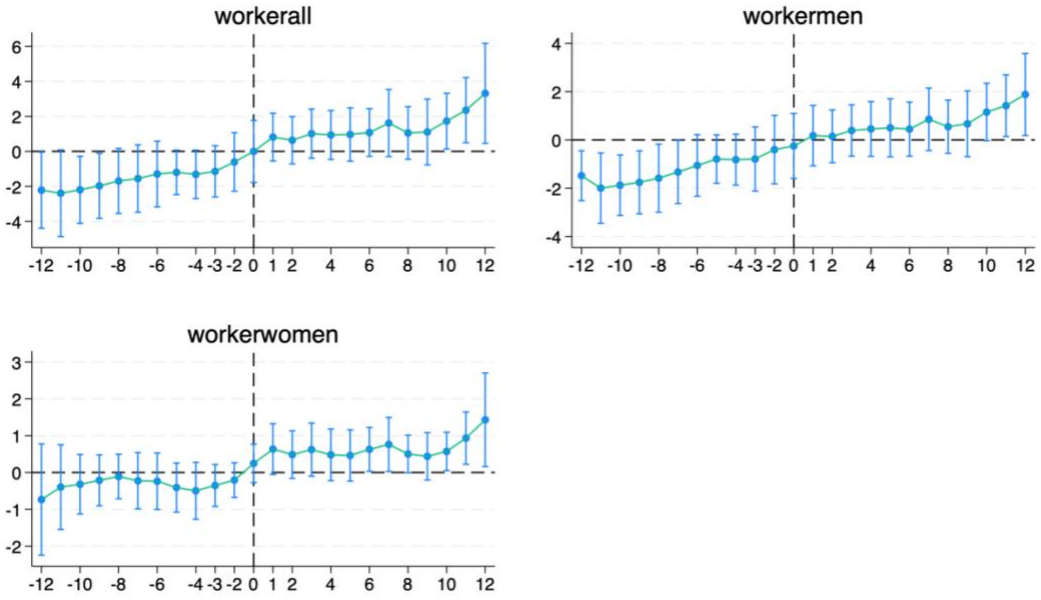


Figure A.4. The dynamic impact of the 2022 SoEx on Firm Employment (Above Top Decile)

Notes: Each panel shows event-study estimates of the differential evolution of firm-level employment in high-crime versus low-crime cities around the onset of the 2022 State of Exception (SoEx). The outcome variable is the number of employees at the firm, separately for all workers, women, and men. The event window spans from 12 months before to 12 months after the onset of the SoEx (i.e., event time -12 to +12). Event time -1, corresponding to March 2022, is the omitted reference period. Each point represents the estimated monthly difference between treated and control cities (Treated groups denote cities with pre-SoEx homicide rates at or above the 90th percentile of the national distribution). All regressions include firm fixed effects, city fixed effects, and region-by-industry-by-month-by-year fixed effects. Standard errors are clustered at the city-by-month-by-year level. Error bars indicate 95% confidence intervals.

Table A.1—Summary Statistics

	Obs	Mean	SD	Min	Max
City-level variables					
Homicide rate	21,070	2.387	7.225	0.000	371.641
Number of workplaces	21,070	73.014	582.198	1	9,752
Number of firms	21,070	66.467	527.884	1	8,897
Above_mean_22	259	0.429	0.496	0	1
Above_mean_19	259	0.421	0.495	0	1
Above_top_quartile_22	259	0.251	0.434	0	1
Above_top_quartile_19	259	0.251	0.434	0	1
Above_top_decile_22	259	0.097	0.296	0	1
Above_top_decile_19	259	0.100	0.301	0	1
Above_mean_22_(no_spike)	259	0.425	0.495	0	1
High state presence	259	0.324	0.469	0	1
Placebo_above_top_decile_22	236	0.424	0.495	0	1
PreAnnualHomicideRate_2022	259	17.258	17.163	0.000	108.106
PreAnnualHomicideRate_2019	259	49.989	43.947	0.000	237.493
Country-level variables					
Apprehension	81	3.232	2.809	0.032	15.981
Firm-level variables					
Workers-all	1,400,468	34.724	162.706	0	8,043
Workers-women	1,400,468	13.368	77.689	0	3,565
Workers-men	1,400,468	21.357	99.566	0	4,722
Consumption industry	1,400,468	0.366	0.482	0	1
Production industry	1,400,468	0.189	0.391	0	1
Infrastructure industry	1,400,468	0.064	0.245	0	1
Other industry	1,400,468	0.381	0.486	0	1
Number of workplaces	1,400,468	1.098	1.103	1	68
Firm age	1,400,379	15.458	13.581	0	114
Wage per all	1,400,391	495.363	431.092	0.000	45,152.085
Wage per women	1,162,537	497.035	428.777	0.000	90,000.000
Wage per men	1,259,340	511.322	485.347	0.000	27,000.960
Wage pay gap (men-women)	1,021,486	19.101	398.997	-89,695.830	16,977.203
Workplace level variables					
Workers-All	1,538,401	31.611	138.358	0	8,043
Workers-Women	1,538,401	12.169	68.316	0	3,521
Workers-Men	1,538,401	19.442	83.809	0	4,722
Household level variables					
Employment status	173,660	0.614	0.487	0	1
Female	173,660	0.535	0.499	0	1
Urban	173,660	0.530	0.499	0	1
Age	173,660	30.157	9.898	15	49
Age square	173,660	1,007.401	627.606	225	2,401

Married	173,660	3.757	2.205	1	6
Education	139,148	1.897	0.976	0	6
Emigration	173,660	0.137	0.344	0	1
Household head age	173,660	46.230	14.494	15	98
Household head female	173,660	0.352	0.478	0	1
Household members	173,660	4.307	1.812	1	23
Employment income	331,019	90.305	195.436	0	7,583
Self-employment income	331,019	38.575	206.141	0	30,020
Income from non-employment activities	331,019	13.801	84.716	0	26,000
Total income	331,019	154.360	297.367	0	30,020
Working hours per week	106,577	8.095	14.224	0	72
Hourly pay	145,178	3.947	5.351	0.000	151.667
Individual mobility proxy	173,660	0.238	0.426	0	1
Permanent	173,660	0.956	0.852	0	2
Formal	173,660	1.039	0.900	0	2
Housework hour	173,660	18.528	17.607	0	112

Notes: This table presents summary statistics of our variables at the city, firm, workplace, and household levels. See Table A.7 in the online appendix for variable definitions.

Table A.2—Sample Distribution By Industry

Industry	Firm-year Obs		Average of firm employees		
	N	%	All	Women	Men
A: Agriculture, Forestry and Fishing	38,967	2.67	27	6	20
B: Mining and Quarrying	1,593	0.11	27	3	24
C: Manufacturing	131,129	8.98	109	48	61
D: Electricity, Gas, Steam and Air Conditioning Supply	7,276	0.50	55	10	45
E: Water Supply, Sewerage, Waste Management and Remediation Activities	16,658	1.14	14	2	12
F: Construction	92,945	6.36	21	3	18
G: Wholesale and Retail Trade; Repair of Motor Vehicles and Motorcycles	359,485	24.61	26	10	16
H: Transportation and Storage	65,974	4.52	25	5	20
I: Accommodation and Food Service Activities	55,503	3.80	41	19	22
J: Information and Communication	40,158	2.75	37	12	25
K: Financial and Insurance Activities (drop)	60,265	4.13	43	22	21
L: Real Estate Activities	46,033	3.15	11	3	8
M: Professional, Scientific and Technical Activities	148,476	10.16	16	6	10
N: Administrative and Support Service Activities	91,048	6.23	90	29	61
O: Public Administration and Defense	2,025	0.14	50	25	25
P: Education	139,259	9.53	14	8	6
Q: Human Health and Social Work Activities	66,218	4.53	20	12	8
R: Arts, Entertainment and Recreation	15,342	1.05	17	7	10
S: Other Service Activities	82,379	5.64	15	7	8

Notes: This table reports the distribution of firm-year observations by 2-digit industry classification, based on the section "economic activity" of administrative employment data from El Salvador's social security records. Column 1 shows the number of firm-year-month observations, and column 2 reports their share in the overall sample (in percent). Columns 3–5 present the average number of total employees, female employees, and male employees per firm within each industry.

Table A.3—Anti-Gang Crackdown and Firm Employment (Alternative Constructions of Treatment Variables)

	All	Women	Men
	(1)	(2)	(3)
Panel A: Treated = Cities with Above 75th Percentile in Homicide Rate			
Above_Top_Quartile_22 * Post_SoEx	2.871***	1.080***	1.791***
	(0.295)	(0.169)	(0.159)
Above_Top_Quartile_19 * Post_TCP	0.010	-0.315***	0.325***
	(0.176)	(0.087)	(0.115)
T-stat: SoEx vs TCP	7.84	6.82	7.16
P-value: SoEx vs TCP	0.000	0.000	0.000
Adjusted R-squared	0.9529	0.9508	0.9503
Panel B: Treated = Cities with Above 90th Percentile in Homicide Rate			
Above_Top_Decile_22 * Post_SoEx	3.798***	1.523***	2.275***
	(0.358)	(0.203)	(0.198)
Above_Top_Decile_19 * Post_TCP	-0.950***	-0.527***	-0.423**
	(0.274)	(0.140)	(0.166)
T-stat: SoEx vs TCP	10.00	7.86	9.96
P-value: SoEx vs TCP	0.000	0.000	0.000
Adjusted R-squared	0.9529	0.9508	0.9503
Panel C: Treated = Cities with Above Mean in Homicide Rate (exclude homicide spike, 2021.3-2022.2)			
Above_Mean_22_(No_Spike) * Post_ SoEx	2.499***	0.785***	1.714***
	(0.298)	(0.156)	(0.170)
Above_Mean_19 * Post_TCP	0.994***	0.551***	0.443***
	(0.238)	(0.136)	(0.133)
T-stat: SoEx vs TCP	4.19	1.29	6.03
P-value: SoEx vs TCP	0.000	0.197	0.000
Adjusted R-squared	0.9529	0.9508	0.9503
Panel D: Treated = Cities with Above Mean Density of Police, Military, and Government Facilities			
High_State_Presence * Post_SoEx	1.324***	0.819***	0.505***
	(0.298)	(0.161)	(0.164)
Adjusted R-squared	0.9529	0.9508	0.9503
Panel E: Continuous Measures of Treated: Pre-Annual Homicide Rate			
PreAnnualHomicideRate_2022 * Post_SoEx	0.056***	0.013***	0.043***
	(0.010)	(0.005)	(0.006)

PreAnnualHomicideRate_2019 * Post_TCP	0.025***	0.007***	0.018***
	(0.003)	(0.002)	(0.002)
T-stat: SoEx vs TCP	3.00	1.15	4.11
P-value: SoEx vs TCP	0.003	0.252	0.000
Adjusted R-squared	0.9529	0.9508	0.9503
Observations	1,398,244	1,398,244	1,398,244
Unique Firm	25,669	25,669	25,669
Firm FEs	√	√	√
City FEs	√	√	√
Region-Industry-Year-Month FEs	√	√	√

Notes: This table reports difference-in-differences (DID) estimates of the impact of anti-gang crackdowns on firm-level employment. The dependent variable is the number of employees at the firm, separately reported for all workers, women, and men. The independent variable, Panel A defines treated cities as those with pre-crackdown homicide rates at or above the 75th percentile of the national distribution. Panel B defines treated cities as those with pre-crackdown homicide rates at or above the 90th percentile of the national distribution. Panel C defines treated cities as those with average homicide rates at or above the national mean between March 2021 and February 2022, excluding the March 2022 spike in the construction of the *Above_Mean_22* variable. The definition of *Above_Mean_19* remains the same as in Table 1. Panel D defines *High_State_Presence* as cities with above-mean density of police, military, and government facilities per 100,000 people. In panel E, *PreAnnualHomicideRate_2022* denotes continuous annual homicide rate at city level in April 2021 to March 2022. *PreAnnualHomicideRate_2019* denotes continuous annual homicide rate at city level in June 2018 to May 2019. Post_*SoEx* denotes the period after April 2022. Post_*TCP* denotes the period between July 2019 to Feb 2022. All regressions include firm fixed effects, city fixed effects, and region-by-industry-by-month-by-year fixed effects. Standard errors clustered at the city-by-month-by-year level are reported in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively. √ denotes included in the regression.

Table A.4—Anti-Gang Crackdown and Firm Employment (Spillover effects)

	All	Women	Men
	(1)	(2)	(3)
Panel A: Exclude cities both treated in TCP and SoEx (Above Mean)			
Above_Mean_22 * Post_SoEx	1.852***	0.419*	1.433***
	(0.373)	(0.232)	(0.181)
Above_Mean_19 * Post_TCP	0.356	0.060	0.296*
	(0.246)	(0.143)	(0.152)
Adjusted R-squared	0.9460	0.9417	0.9463
Observations	600,601	600,601	600,601
Panel B: Exclude all cities with adjacent treated neighbors in SoEx (Above top Decile)			
Above_Top_Decile_22 * Post_SoEx	2.140***	0.800***	1.341***
	(0.401)	(0.215)	(0.215)
Above_Top_Decile_19 * Post_TCP	-1.357***	-0.483***	-0.874***
	(0.334)	(0.167)	(0.202)
Adjusted R-squared	0.9595	0.9603	0.9539
Observations	988,871	988,871	988,871
Panel C: Placebo Treated = Neighbors of Treated Cities in SoEx (Above top Decile)(Excl. Original Treated)			
Placebo_Above_Top_Decile_22 * Post_SoEx	0.461	0.385	0.076
	(0.396)	(0.246)	(0.189)
Above_Top_Decile_19 * Post_TCP	-0.974***	-0.621***	-0.352*
	(0.346)	(0.177)	(0.205)
Adjusted R-squared	0.9529	0.9508	0.9503
Observations	1,370,780	1,370,780	1,370,780
Firm FEs	√	√	√
City FEs	√	√	√
Region-Industry-Year-Month FEs	√	√	√

Notes: This table presents robustness checks addressing potential spatial spillover effects between treated and control cities. The dependent variable is the number of employees at the firm, reported separately for all workers, women, and men. In Panel A, the treated group denotes cities with above-mean national pre-crackdown homicide rates. We exclude all cities that were treated under both the 2019 TCP and the 2022 SoEx. In Panels B–C, the treated group denotes cities with pre-crackdown homicide rates at or above the 90th percentile of the national distribution. Panel B excludes all cities that share a border with treated municipalities. In Panel C, municipalities neighboring treated cities are reclassified as treated ("placebo" treated), while original treated cities are excluded. All regressions include firm fixed effects, city fixed effects, and region-by-industry-by-month-by-year fixed effects. Standard errors are clustered at the

city-by-month-by-year level and reported in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively. √ denotes included in the regression.

Table A.5—Anti-Gang Crackdown And Firm Employment With Trend of Emigration

	Dependent variable: Number of employees		
	All	Women	Men
	(1)	(2)	(3)
Above_Mean_22 * Post_SoEx	2.906***	0.948***	1.958***
	(0.576)	(0.318)	(0.308)
Above_Mean_19 * Post_TCP	0.805**	0.651***	0.154
	(0.326)	(0.192)	(0.170)
Above_Mean_22 * Post_SoEx * Apprehension	-0.198	-0.052	-0.146
	(0.225)	(0.128)	(0.117)
Above_Mean_19 * Post_TCP * Apprehension	0.074	-0.030	0.104**
	(0.067)	(0.033)	(0.043)
Adjusted R-squared	0.9560	0.9540	0.9532
Observations	1,346,167	1,346,167	1,346,167
Unique Firm	25,262	25,262	25,262
Firm FEs	√	√	√
City FEs	√	√	√
Region-Industry-Year-Month FEs	√	√	√

Notes: This table reports difference-in-differences (DID) estimates of the impact of anti-gang crackdown on firm-level employment, with a focus on the trend of emigration. The dependent variable is the number of employees at the firm, separately for all workers, women, and men. The independent variable *Above_Mean_22* denotes cities with above-mean pre-SoEx homicide rates, and *Post_SoEx* denotes the period after April 2022. *Above_Mean_19* denotes cities with above-mean pre-TCP crackdown homicide rates. *Post_TCP* denotes the period between July 2019 to Feb 2022. *Apprehension* is the number of encounters of illegal/undocumented migrants by the U.S. Border Patrol, measured in thousands, at the country–month level from Jan 2017 to Sep 2023. All regressions include firm fixed effects, city fixed effects, and region-by-industry-by-month-by-year fixed effects. Standard errors are clustered at the city-by-month-by-year level. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively. √ denotes included in the regression.

Table A.6—Anti-Gang Crackdown And Effects on Formal and Informal Sectors

	All		Women		Men	
	Formal	Informal	Formal	Informal	Formal	Informal
	(1)	(2)	(3)	(4)	(5)	(6)
Above_Mean_22 * Post_SoEx	6.076***	0.903***	2.583***	0.063	3.493***	0.840***
	(0.797)	(0.197)	(0.485)	(0.075)	(0.346)	(0.150)
Above_Mean_19 * Post_TCP	1.471**	0.729***	0.681	0.491***	0.790***	0.238**
	(0.667)	(0.146)	(0.429)	(0.054)	(0.291)	(0.121)
Adjusted R-squared	0.9604	0.9466	0.9560	0.9437	0.9639	0.9408
Observations	307,610	1,090,628	307,610	1,090,628	307,610	1,090,628
Unique Firm	5,477	20,237	5,077	20,636	5,477	20,237
Firm FEs	√	√	√	√	√	√
City FEs	√	√	√	√	√	√
Region-Industry-Year-Month FEs	√	√	√	√	√	√

Notes: This table reports difference-in-differences (DID) estimates of the impact of anti-gang crackdown on firm-level employment in the formal and informal sectors. The dependent variable is the number of employees at the firm, separately for all workers, women, and men. The independent variable *Above_Mean_22* denotes cities with above-mean pre-SoEx homicide rates, and *Post_SoEx* denotes the period after April 2022. *Above_Mean_19* denotes cities with above-mean pre-TCP crackdown homicide rates. *Post_TCP* denotes the period between July 2019 to Feb 2022. Firms are classified as belonging to the formal or informal sector based on the sectoral classification from the 2011 World Bank's STEP Skills Measurement Program survey, following Bandiera et al. (2023). For more details on the classification, see Table A.7. All regressions include firm fixed effects, city fixed effects, and region-by-industry-by-month-by-year fixed effects. Standard errors are clustered at the city-by-month-by-year level. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively. √ denotes included in the regression.

Table A.7—Data Sources and Definitions

Variable Name	Definition and Source
Panel A: City-level variables	
Homicide rate	Monthly homicide rate by 100,000 people. Source: Government monthly report from National Police (Policía Nacional Civil, PNC) and daily report from official Twitter account (@PNCSV).
Number of workplaces	Number of workplaces in the private sectors. Source: National social security agency (the Instituto Salvadoreño del Seguro Social, ISSS).
Number of firms	Number of firms in the private sectors. Source: National social security agency (the Instituto Salvadoreño del Seguro Social, ISSS).
Post_SoEx	A binary indicator equal to one for the period from April 2022 to December 2023, and 0 otherwise.
Post_TCP	A binary indicator equal to one for the period from Jul 2019 to Feb 2022, and 0 otherwise.
Above_mean_22	A binary variable equal to one if a city's average annual homicide rate from April 2021 to March 2022 exceeds the national mean for the same period, and 0 otherwise. Source: authors own calculation and PNC
Above_mean_19	A binary variable equal to one if a city's average annual homicide rate from June 2018 to May 2019 exceeds the national mean for the same period, and 0 otherwise. Source: authors own calculation and PNC
Above_top_quartile_22	A binary variable equal to one if a city's average annual homicide rate from April 2021 to March 2022 exceeds the 75% of national homicide rate for the same period, and 0 otherwise. Source: authors own calculation and PNC
Above_top_quartile_19	A binary variable equal to one if a city's average annual homicide rate from June 2018 to May 2019 exceeds the 75% of national homicide rate for the same period, and 0 otherwise. Source: authors own calculation and PNC
Above_top_decile_22	A binary variable equal to one if a city's average annual homicide rate from April 2021 to March 2022 exceeds the 90% of national homicide rate for the same period, and 0 otherwise. Source: authors own calculation and PNC
Above_top_decile_19	A binary variable equal to one if a city's average annual homicide rate from June 2018 to May 2019 exceeds the 90% of national homicide rate for the same period, and 0 otherwise. Source: authors own calculation and PNC
Above_mean_22_(no_spike)	A binary variable equal to one if a city's average annual homicide rate from March 2021 to February 2022 exceeds the national mean for the same period, and 0 otherwise. Source: authors own calculation and PNC
Placebo_above_top_decile_22	A binary variable equal to one if a municipality is adjacent to a treated city (i.e., cities whose average annual homicide rate from April 2021 to March 2022 exceeds the 90th percentile of the national distribution), and 0 otherwise. Treated cities themselves are excluded from the sample. Source: authors' own calculation and PNC.
High state presence	A binary variable equal to one if a city above-average density of police, military, and government facilities per 100,000 people, including military barracks, military checkpoint, military range, military danger area, military training area, military base, military airfield, military naval base, military landuse, military service for army, military service for navy, police stations, police building, police checkpoint, government administrative. Source: Open street map.
Preannualhomiciderate_2022	Continuous annual homicide rate at city level in April 2021 to March 2022. Source: authors own calculation and PNC
Preannualhomiciderate_2019	Continuous annual homicide rate at city level in June 2018 to May 2019. Source: authors own calculation and PNC
Panel B: Country-level variables	
Apprehension	The variable measures the number of encounters of illegal or undocumented migrants with the U.S. Border Patrol, measured in thousands, whose recorded citizenship is "El Salvador," at the country–month level. Because data for fiscal year 2024 (from

	October 2023 onward) are not yet available, the sample covers the period from January 2017 to September 2023. Source: U.S Border Patrol through a Freedom of Information Act (FOIA) request.
Panel C: Firm-level variables	
Workers-all	Number of employees for each firm in each city. Source: National social security agency (the Instituto Salvadoreño del Seguro Social, ISSS).
Workers-women	Number of female employees for each firm in each city. Source: National social security agency (the Instituto Salvadoreño del Seguro Social, ISSS).
Workers-men	Number of male employees for each firm in each city. Source: National social security agency (the Instituto Salvadoreño del Seguro Social, ISSS).
Consumption Industry	A binary indicator if the industry of firm belongs to G: Wholesale and Retail Trade; Repair of Motor Vehicles and Motorcycles; I: Accommodation and Food Service Activities; R: Arts, Entertainment and Recreation; S: Other Service Activities. Source: National social security agency (the Instituto Salvadoreño del Seguro Social, ISSS).
Production Industry	A binary indicator if the industry of firm belongs to A: Agriculture, Forestry and Fishing; B: Mining and Quarrying; C: Manufacturing; F: Construction. Source: National social security agency (the Instituto Salvadoreño del Seguro Social, ISSS).
Infrastructure Industry	A binary indicator if the industry of firm belongs to D: Electricity, Gas, Steam and Air Conditioning Supply; E: Water Supply, Sewerage, Waste Management and Remediation Activities; H: Transportation and Storage. Source: National social security agency (the Instituto Salvadoreño del Seguro Social, ISSS).
Others Industry	A binary indicator if the industry of firm belongs to J: Information and Communication; L: Real Estate Activities; M: Professional, Scientific and Technical Activities; N: Administrative and Support Service Activities; O: Public Administration and Defense; P: Education; Q: Human Health and Social Work Activities. Source: National social security agency (the Instituto Salvadoreño del Seguro Social, ISSS).
Formal Sector	Formal sector includes B: Mining and Quarrying. C: Manufacturing. D: Electricity, Gas, Steam and Air Conditioning Supply. E: Water Supply, Sewerage, Waste Management and Remediation Activities. H: Transportation and Storage. J: Information and Communication. L: Real Estate Activities. Source: National social security agency (the Instituto Salvadoreño del Seguro Social, ISSS).
Informal Sector	Informal sector includes A: Agriculture, Forestry and Fishing. F: Construction. G: Wholesale and Retail Trade; Repair of Motor Vehicles and Motorcycles. I: Accommodation and Food Service Activities. M: Professional, Scientific and Technical Activities. N: Administrative and Support Service Activities. O: Public Administration and Defense. P: Education. Q: Human Health and Social Work Activities. R: Arts, Entertainment and Recreation. S: Other Service Activities. Source: National social security agency (the Instituto Salvadoreño del Seguro Social, ISSS).
Firm age	Number of years from the firm's establishment year to the observation year. Source: National social security agency (the Instituto Salvadoreño del Seguro Social, ISSS).
Number of workplaces	Number of workplaces for each firm in each city. Source: National social security agency (the Instituto Salvadoreño del Seguro Social, ISSS).
Wage per all	Average monthly wage per employee for each firm in each city, calculated across all workers. Source: National social security agency (the Instituto Salvadoreño del Seguro Social, ISSS).
Wage per women	Average monthly wage per female employee for each firm in each city. Source: National social security agency (the Instituto Salvadoreño del Seguro Social, ISSS).
Wage per men	Average monthly wage per male employee for each firm in each city. Source: National social security agency (the Instituto Salvadoreño del Seguro Social, ISSS).
Wage pay gap (men-women)	Difference between the average monthly wage per male employee and the average monthly wage per female employee for each firm in each city. Source: National social security agency (the Instituto Salvadoreño del Seguro Social, ISSS).

Panel D: Workplace level variables	
Workers-all	Number of employees for each workplace. Source: National social security agency (the Instituto Salvadoreño del Seguro Social, ISSS).
Workers-women	Number of female employees for each workplace. Source: National social security agency (the Instituto Salvadoreño del Seguro Social, ISSS).
Workers-men	Number of male employees for each workplace. Source: National social security agency (the Instituto Salvadoreño del Seguro Social, ISSS).
Panel E: Household level variables	
Employment status	A binary variable equal to one if the respondent is currently employed, 0 otherwise. Source: Encuesta de Hogares de Propósitos Múltiples (EHPM)
Female	A binary variable equal to one if the respondent is female, 0 otherwise. Source: Encuesta de Hogares de Propósitos Múltiples (EHPM)
Urban	A binary variable equal to one if the respondent lives in urban, 0 otherwise. Source: Encuesta de Hogares de Propósitos Múltiples (EHPM)
Age	Age of respondent. Source: Encuesta de Hogares de Propósitos Múltiples (EHPM)
Age square	Square of age of respondent. Source: Encuesta de Hogares de Propósitos Múltiples (EHPM)
Married	Six categories of marital status: 1 is "In a relationship"; 2 is "Married"; 3 is "Widowed"; 4 is "Divorced"; 5 is "Separated"; 6 is "Single". Source: Encuesta de Hogares de Propósitos Múltiples (EHPM)
Education	Seven categories of education: 0 is "None or Early childhood education"; 1 is "Nursery school (1st to 3rd grade)"; 2 is "Primary school (1st to 9th grade)"; 3 is "Secondary (10th to 13th grade)"; 4 is "University (1st to 15th year)"; 5 is "Non-university higher education (1st to 3rd)"; 6 is "Special education (cycles I, II, III, IV)". Source: Encuesta de Hogares de Propósitos Múltiples (EHPM)
Household head age	Age of household head. Source: Encuesta de Hogares de Propósitos Múltiples (EHPM)
Household head female	A binary variable equal to one if the household head is female, 0 otherwise. Source: Encuesta de Hogares de Propósitos Múltiples (EHPM)
Household members	Number of household members excluding domestic servants. Source: Encuesta de Hogares de Propósitos Múltiples (EHPM)
Emigration	A binary variable equal to one if the household has at least one member abroad, 0 otherwise. Source: Encuesta de Hogares de Propósitos Múltiples (EHPM)
Permanent	A variable equal to one if the respondent is currently employed as a permanent employee, 2 if the respondent is currently employed as a non-permanent employee (Other), versus 0 for unemployed. Source: Encuesta de Hogares de Propósitos Múltiples (EHPM)
Formal	A variable equal to one if the respondent is currently employed in a formal firm, defined as a firm registered with both the tax and social security authorities, 2 if the respondent is currently employed in a non-formal firm (Other), versus 0 for unemployed. Source: Encuesta de Hogares de Propósitos Múltiples (EHPM)
Housework hour	Total number of hours per week spent on unpaid domestic labor, including caring for and looking after children, repairing or maintaining the home, furniture, appliances, doing household chores, carrying firewood and water. Source: Encuesta de Hogares de Propósitos Múltiples (EHPM)
Have children family	A binary variable equal to one if the household has at least one child younger than 14 years old, and 0 otherwise. Source: Encuesta de Hogares de Propósitos Múltiples (EHPM)
Individual Mobility Proxy	A binary indicator equal to one if the respondent reports any spending on (i) transportation, (ii) fuel, or (iii) vehicle repair, and 0 otherwise. Source: Encuesta de Hogares de Propósitos Múltiples (EHPM)
Employment income	Monthly income from wage employment. Source: Encuesta de Hogares de Propósitos Múltiples (EHPM).
Self-employment income	Monthly income from self-employment activities. Source: Encuesta de Hogares de Propósitos Múltiples (EHPM).

Income from non-employment activities	Monthly income from non-employment activities. Source: Encuesta de Hogares de Propósitos Múltiples (EHPM).
Total income	Total monthly individual income from all reported sources. Source: Encuesta de Hogares de Propósitos Múltiples (EHPM).
Working hours per week	Usual weekly hours worked. Source: Encuesta de Hogares de Propósitos Múltiples (EHPM).
Hourly pay	Monthly employment income divided by estimated monthly hours worked, where monthly hours are calculated as hours worked in the previous week plus 3.33 times usual weekly hours worked. Source: Encuesta de Hogares de Propósitos Múltiples (EHPM).