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Abstract

This paper evaluates the effects of El Salvador's 2022 crackdown policy on five categories of crime: homicide, assault, street robbery, motor vehicle theft, and larceny. The policy, implemented abruptly in March 2022, increased the prison population by 150% within a year. To estimate the causal effect on homicides, we adopt a multi-method approach. At the aggregate level, we apply Synthetic Control Methods (SCM), constructing a data-driven counterfactual from a donor pool of six comparable economies using quarterly homicide rates alongside macroeconomic predictors. At the departmental level, we corroborate these findings using a difference-in-differences and event-study framework that exploits within-country and cross-country variation across 32 departments in El Salvador and Honduras (control country). We then examine mass detentions as a potential mechanism underlying the observed homicide decline. For the remaining crimes, we estimate fixed-effects models using data from El Salvador's Multipurpose Household Survey. Our results show that the crackdown reduced homicide rates by approximately 80% relative to the synthetic counterfactual (four fewer homicides per 100,000 inhabitants per quarter) and by 48% in the DiD specification. Mass detentions account for approximately half of the observed homicide reduction. The policy also produced a statistically significant reduction in street robbery of 29%, while we find no detectable effects on assault, larceny, or motor vehicle theft. Taken together, the evidence points to substantial but selective crime reductions.

Keywords: El Salvador, Incarceration, Synthetic Control Methods, Drug Markets, Violence, Homicides, Crime.

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1 Introduction

Incarceration is one of the most debated tools in modern crime control. Several studies find that it reduces crime through deterrence and incapacitation (Levitt, 1996; Raphael and Johnson, 2010; Spelman, 2020). Other studies challenge these findings, arguing that effects are small or nonexistent, as new offenders often replace those who are jailed, leaving overall crime rates largely unchanged (Kovandzic and Vieraitis, 2006; Stemen, 2007). Most of this evidence comes from high-income countries, particularly the United States and Italy. We know much less about how these policies play out in developing nations.

In March 2022, El Salvador, under Nayib Bukele's administration, launched a crackdown built around three pillars: the suspension of constitutional rights including freedom of assembly and association; reforms to the criminal justice system that raised penalties for gang membership from 20 to 40 years; and a mass incarceration campaign that granted security forces the authority to detain individuals without a judicial warrant. The prison population expanded from 40,000 to 100,000, a 150% increase in under a year (Fair and Walmsley, 2024).

We analyze the effects of this crackdown across five crime categories: homicide, assault, street robbery, motor vehicle theft, and larceny. For homicides, we adopt a multi-method, multi-scale strategy. At the aggregate level, we apply Synthetic Control Methods (SCM), constructing a weighted combination of six Mesoamerican donor economies (Belize, Costa Rica, Guatemala, Honduras, Mexico, and Panama) that closely reproduces El Salvador's pre-treatment homicide trajectory and macroeconomic characteristics (Abadie and Gardeazabal, 2003; Abadie et al., 2010). At the departmental level, we complement these aggregate findings with a difference-in-differences (DiD) estimator and an event study that exploits variation across El Salvador's and Honduras's 32 departments before and after March 2022. We then examine the detention channel as a mechanism, drawing on monthly data from the El Salvador National Civil Police. For the remaining crimes, we use El Salvador's Multipurpose Household

Survey (MPHS) and a fixed-effects model that exploits variation across departments and survey months in the degree of policy exposure, with Oster's bounds to assess sensitivity to time-varying omitted variables (Oster, 2017).

The crackdown reduced homicide rates by roughly 80% relative to the synthetic counterfactual (about four fewer quarterly homicides per 100,000 inhabitants) and by 48% in the DiD specification. The surge in detention rates accounts for approximately half of this decline, pointing to mass incarceration as a central, though not exclusive, driver of the observed reduction in homicides. Beyond homicides, the policy produced a statistically significant 29% reduction in street robbery. We detect no measurable effect on larceny, motor vehicle theft, or assault.

This paper makes three contributions. First, we provide empirical evidence supporting a middle ground between competing hypotheses on mass incarceration (Kovandzic and Vieraitis, 2006; Raphael and Johnson, 2010): the policy sharply reduced some crimes while leaving others untouched, consistent with selective incapacitation rather than general deterrence. Second, almost all prior research on this topic focuses on high-income countries, the United States and Italy in particular (Buonanno and Raphael, 2013; Kovandzic and Vieraitis, 2006; Levitt, 1996; Raphael and Johnson, 2010). By examining El Salvador, we bring evidence from a developing nation where gang structures are territorially entrenched and state capacity is limited. Third, to the best of our knowledge, this is the first study to evaluate a mass incarceration campaign of this scale and abruptness: a 150% expansion in the prison population over nine months, which represents a dramatic and sudden policy shock.

Our findings should not be read as an endorsement of El Salvador's model. The policy operated under highly specific conditions, and the suspension of due-process protections raises legitimate concerns about civil liberties and wrongful detention (Blattman, 2025). At the same time, the results reveal the social costs of persistent impunity in high-crime environments. Credible enforcement and incapacitation can meaningfully reduce violent and gang-related crime, provided they operate within democratic and institutional constraints and alongside longer-run investments in education and social policy.

2 Incarceration and Crime

2.1 Theory

What is the relationship between incarceration and crime? On one hand, incarceration may reduce crime through two mechanisms: incapacitation and deterrence (Raphael and Johnson, 2010). Incapacitation works directly: incarcerated individuals cannot commit crimes against the general population. Deterrence works indirectly, raising the expected cost of criminal activity and discouraging potential offenders before a crime occurs.

Competing theories propose that incarceration has no effect on crime, with targeting and substitution as the two main alternative mechanisms (Kovandzic and Vieraitis, 2006). Under the targeting hypothesis, a small fraction of offenders commits the majority of crimes, so a mass incarceration policy may fail to reach the right individuals and leave overall rates unchanged. Under the substitution hypothesis, socioeconomic conditions like poverty and unemployment generate a continuous supply of new offenders, offsetting whatever incapacitation the policy achieves. Whether incarceration reduces crime is therefore an empirical question, not a theoretical one.

Becker (1968)'s rational model adds a further complication. Offenders in this framework weigh the benefits and costs of criminal activity; incarceration raises those costs by increasing the probability of punishment, which should reduce crime. Yet, the same policy may make potential victims feel safer, leading them to take fewer precautions and increasing their exposure to crime. The net effect is ambiguous, and the direction depends on which force dominates in a given context. We next review the empirical evidence on which crimes, if any, respond to incarceration.

2.2 Empirical Evidence

The evidence on incarceration and crime is mixed. Levitt (1996), using data from the United States, finds that a 1% increase in the prison population decreases robbery by 0.70% and burglary by 0.40%, but produces no detectable effect on larceny, motor vehicle theft, homicides, or assault. Raphael and Johnson (2010), using a state-level panel, find that the same 1% increase reduces larceny by 1.18% and burglary by 0.85%, but again produces no effect on robbery, motor vehicle theft, homicides, or assault. Kovandzic and Vieraitis (2006), using county-level panel data from Florida and fixed-effects models, find no evidence that prison population growth affects any of the crime categories they examine, including homicide, assault, robbery, burglary, and motor vehicle theft.

Other studies exploit decarceration episodes. Lofstrom and Raphael (2013) exploit a sharp reduction in incarceration in California and, using fixed-effects models, find no effects on homicides, robbery, assault, burglary, or larceny. The one exception is vehicle theft: each prison-year prevents roughly 1.2 thefts. Boylan (2025) studies court orders issued between 1970 and 1988 to address jail overcrowding in the United States, finding that while these orders reduced jail populations by 21%, they were also associated with a 15% rise in homicide rates outside jails. In a different context, Buonanno and Raphael (2013) examine the Italian government's decision to release more than one-third of its prison population in 2006. Across ten crime categories, ranging from theft and robbery to extortion and arson, they detect effects only on theft and robbery.

Put together, the literature points to three conclusions. First, competing theories generate conflicting predictions about how incarceration affects crime (Kovandzic and Vieraitis, 2006; Raphael and Johnson, 2010). Second, empirically, incarceration policies do not affect all crimes equally (Levitt, 1996; Raphael and Johnson, 2010), and in some settings produce no detectable effect on any crime (Kovandzic and Vieraitis, 2006). Third, the bulk of this evidence comes from developed nations, particularly the United

States and Italy (Buonanno and Raphael, 2013; Kovandzic and Vieraitis, 2006; Levitt, 1996; Raphael and Johnson, 2010). Little is therefore known about how such policies play out in less developed countries.

3 The Context in El Salvador: Crisis and Response

For much of the early 21st century, El Salvador ranked among the countries with the highest homicide rates in the world. The crisis was driven primarily by the territorial expansion and violent operations of two major gangs, Mara Salvatrucha (MS-13) and Barrio 18. Both groups originated from the deportation of gang-affiliated youth from the United States in the 1990s and gradually evolved into powerful criminal structures. They controlled neighborhoods, imposed illegal taxes through extortion, and resisted law enforcement with organized force.

In June 2019, President Nayib Bukele took office and launched the *Plan Control Territorial*, a multi-phase public security strategy. The plan aimed to reclaim territory from criminal organizations and reassert state authority throughout the country. It broadly included the deployment of police and military forces to high-crime urban areas and the disruption of gang communications within prisons. Full implementation was limited, however, by fiscal restrictions imposed by opposition parties (Maldonado, 2020). These constraints left gang structures and extortion practices largely intact across many urban areas.

Figure I shows that homicide rates in El Salvador fell steadily after 2015 and stood at approximately 1.5 monthly homicides per 100,000 inhabitants by 2019, a level that held stable through 2020 and 2021. That calm ended abruptly in March 2022 with a sudden surge in violence: 87 people were murdered over three days, with 62 killed on March 26 alone, the deadliest single day in El Salvador in over three decades. Authorities attributed the killings to retaliation by the MS-13 gang. In response, on March 27, 2022, the Legislative Assembly approved Decree No. 333, declaring a State of Exception.

The Constitution of El Salvador establishes the State of Exception as a legal instrument to respond to extreme circumstances, such as war or serious disturbances of public order (Américas, 2022). The measures adopted under the decree fell into three categories. First, the suspension of constitutional rights, including the rights to freedom of assembly and association, and increased surveillance of citizens' communications. Second, reforms to the criminal justice system, which raised penalties for gang membership from 20 to 40 years in prison. Third, a mass incarceration campaign that granted security forces the authority to detain individuals without a judicial warrant (Américas, 2022).

This declaration launched one of the most extensive internal security operations in recent Latin American history. Figure I shows that the monthly detention rate held stable from January 2019 through February 2022, then spiked sharply in March and April 2022, and had returned to pre-policy levels by year's end. By December 2022, the incarcerated population had reached 100,000, a 150% increase in just ten months relative to the pre-crackdown prison population of 40,000 (Urbina and Espinoza, 2023).

4 Data and Empirical Strategy

4.1 Data

4.1.1 Aggregate Quarterly Data for the Synthetic Control

For the SCM analysis, we assemble a quarterly panel covering seven countries (El Salvador and six donor economies: Belize, Costa Rica, Guatemala, Honduras, Mexico, and Panama) from 2019q3 through 2023q4. The donor pool is restricted to Mesoamerican countries that share broadly comparable history, economic structures, institutional contexts, and exposure to gang-related violence but were not themselves subject to a comparable mass-incarceration shock during the study period. The outcome variable, the quarterly homicide rate per 100,000 inhabitants, comes from monthly administrative records of the national police forces of each country (National Civil Police of El Salvador (NCP), 2024; Secretaría de Estado en el Despacho de Seguridad de Honduras (SEPOL), 2024; Belize Crime Observatory (BCO), 2024; Instituto Nacional de Estadística de Guatemala (INE), 2024; Autoridad Nacional de Transparencia y Acceso a la Información (ANTAI), Panamá, 2024; Poder Judicial de Costa Rica – Organismo de Investigación Judicial (OIJ), 2024; Observatorio de la Violencia, Ministerio de Justicia y Paz de Costa Rica, 2023; Instituto Nacional de Estadística y Geografía (INEGI), 2024).

Beyond the outcome, we collect five macroeconomic predictors designed to capture the structural determinants of homicide trends at the national level. First, we use the monthly Consumer Price Index to construct quarterly inflation rates, drawn from the IMF's International Financial Statistics (International Monetary Fund, 2024). Second, we collect monthly remittance inflows from the central banks of each donor country (Banco Central de Reserva de El Salvador (BCR), 2024; Banco Central de Honduras (BCH), 2024; Banco de Guatemala, 2024; Banco de México, 2024; Banco Central de Costa Rica, 2024; Superintendencia de Bancos de Panamá, 2024), cross-validated against the pre-aggregated panel maintained by the IOM's Migration Data Unit (Organización Internacional para las Migraciones – Unidad de Información NCA, 2024). Third, we obtain monthly exports and imports from UN Comtrade (United Nations Statistics Division, 2024) and CEPALSTAT (Comisión Económica para América Latina y el Caribe (CEPAL), 2024), validated against central bank publications and expressed in per-capita terms using World Bank population estimates. Fourth, we construct M2 monetary aggregates from the IMF Monetary and Financial Statistics portal (International Monetary Fund, 2024). Fifth, we use monthly electricity consumption (kWh per capita) as a high-frequency proxy for economic activity, with data from national regulatory bodies in each country (Superintendencia General de Electricidad y Telecomunicaciones de El Salvador (SIGET), 2024; Empresa Nacional de Energía Eléctrica (ENEE), 2024; Administrador del Mercado Mayorista (AMM) de Guatemala, 2024; Instituto Costarricense de Electricidad (ICE), 2024; Centro Nacional de Control de Energía (CENACE) México, 2024; Empresa de Transmisión Eléctrica (ETESA) Panamá, 2024; Belize Electricity Limited (BEL), 2024).

4.1.2 Monthly Departmental Data for the DiD and Event Study

To examine the policy's impact on homicides at a subnational level, we use monthly homicide data from the Honduras National Police (Secretaría de Estado en el Despacho de Seguridad de Honduras (SEPOL), 2024) and the El Salvador National Civil Police (NCP) (National Civil Police of El Salvador (NCP), 2024) covering January 2021 to December 2022. We exclude 2020 from the analysis to avoid contamination from the COVID-19 pandemic. Both sources collect monthly statistics on homicides for each country's departments (14 departments for El Salvador and 18 for Honduras). El Salvador's departments serve as our treatment group, while Honduras's departments serve as our control group. From these sources, we construct monthly homicide rates per department per 100,000 inhabitants. Our final sample consists of 768 observations (32 departments $\times$ 24 months).

4.1.3 Monthly Departmental Data for the Detention Mechanism

To examine the detention channel, we use monthly data from the National Civil Police of El Salvador (NCP) (National Civil Police of El Salvador (NCP), 2024) on both homicide and detention rates for the country's 14 departments. The NCP collects monthly statistics on homicides and detentions for each department, from which we construct monthly homicide and detention rates per 100,000 inhabitants. While incarceration rates reflecting the stock of individuals in prison would be the ideal measure, our data capture monthly detention flows. Our dataset covers the period from 2019 to 2022 for all 14 departments. We exclude 2020 from our analysis due to the disruptions caused by the COVID-19 pandemic.

4.1.4 Household Survey Data for Other Crimes

To analyze the policy's effect on larceny, street robbery, motor vehicle theft, and assault, we use data from the Multipurpose Household Survey (MPHS) (Banco Central de Reserva de El Salvador, 2023). We selected these crimes to enable a direct comparison with the existing literature (Levitt, 1996; Lofstrom and Raphael, 2013; Kovandzic and Vieraitis, 2006; Raphael and Johnson, 2010). Since 2021, the MPHS has been representative across El Salvador's 14 departments and is collected annually throughout the year (Banco Central de Reserva de El Salvador, 2023). We use data from the 2022 and 2023 MPHS.[1]

The crime questions are structured to ask, "Did you or someone in your household suffer from (name of the crime) in the last 12 months?" Table 1 shows that the 2022–2023 MPHS data capture three distinct periods of exposure to the mass incarceration policy. First is the *unaffected period*, which includes survey data from January to March 2022. For example, a person interviewed in January 2022 would not have been exposed to the policy because the data refers to the January–December 2021 period, which precedes the policy's start in March 2022.

The second is the *partially affected period*, with survey data conducted between April 2022 and February 2023. For example, a person interviewed in January 2023 would be partially affected, as the data aggregates information from January to December 2022, which includes ten months (March–December 2022) of policy exposure. Last is the *fully affected period*, with data from surveys conducted from March to December 2023. For example, a person interviewed in May 2023 would have been fully exposed, as their information from May 2022 to April 2023 falls entirely within the policy's duration.

[1]We do not use the 2021 MPHS because its reported data is affected by the COVID-19 pandemic. For example, the January data for the 2021 MPHS pertains to the January–December 2020 timeframe. Similarly, we exclude the 2020 MPHS, as data was not collected for every month, particularly from April to June.

4.2 Empirical Strategy

4.2.1 Synthetic Control Methods for Homicides

Our primary strategy for estimating the causal effect of the crackdown on national-level homicide rates is the synthetic control method (SCM), introduced by Abadie and Gardeazabal (2003) and further developed by Abadie et al. (2010). SCM is well suited to our setting: we have a single treated unit (El Salvador) observed alongside a small pool of comparable but untreated donor countries, and the policy shock is abrupt and national in scope. SCM constructs a weighted combination of donor units (Synthetic El Salvador) that closely reproduces El Salvador's pre-treatment homicide trajectory. We interpret the post-treatment gap between the actual and synthetic series as the effect of the crackdown.

Formally, let Y_{1t} denote the quarterly homicide rate in El Salvador and let Y_{jt}, $j = 2, \ldots, J+1$, denote the homicide rate in each of the J donor countries. We seek a vector of non-negative weights $W = (w_2, \ldots, w_{J+1})$ summing to unity such that the synthetic control:

$$\hat{Y}_{1t}^{N} = \sum_{j=2}^{J+1} w_j Y_{jt}, \quad t \leq T_0 \tag{1}$$

closely approximates Y_{1t} during the pre-treatment period (2019q3 through 2022q1). We choose this pre-treatment period as it corresponds to Nayib Bukele's first presidential term, during which the crackdown policy was implemented. The weights are chosen to minimize:

$$||X_1 - X_0 W||_V = \sqrt{(X_1 - X_0 W)' V (X_1 - X_0 W)} \tag{2}$$

where X_1 is a vector of pre-treatment characteristics for El Salvador, X_0 is the analogous matrix for donor countries, and V is a diagonal matrix of predictor weights chosen to minimize the pre-treatment mean squared prediction error (MSPE):

$$\text{MSPE} = \sum_{t=1}^{T_0} \left(Y_{1t} - \sum_{j=2}^{J+1} w_j Y_{jt} \right)^2 \tag{3}$$

The estimated treatment effect at each post-treatment quarter $t > T_0$ is then:

$$\hat{\alpha}_{1t} = Y_{1t} - \sum_{j=2}^{J+1} w_j^* Y_{jt} \tag{4}$$

following Abadie and Gardeazabal (2003) and Abadie et al. (2010).

A key implementation choice concerns how to enter outcome lags as predictors in X_1. Including every pre-treatment outcome observation as a separate predictor is tempting but problematic: Kaul et al. (2022) show that doing so drives the optimizer to set $V_C = 0$, effectively discarding all covariates in favor of outcome fit alone and potentially biasing the post-treatment estimates. We therefore follow their recommendation and enter only two outcome lags as predictors: the homicide rate in 2019q3 (the first quarter in our sample) and in 2022q1 (the last pre-treatment quarter). These are included alongside the macroeconomic covariates averaged over the pre-treatment period, ensuring that both the outcome trajectory and the structural characteristics of El Salvador are matched.

For inference, we rely on the permutation-based approach of Abadie et al. (2010), applying the same SCM procedure iteratively to each donor country (in-space placebos). The distribution of post/pre-treatment MSPE ratios across all units allows us to gauge how unusual El Salvador's post-treatment divergence is relative to the donor pool. The small donor pool is a deliberate design choice: restricting comparators to structurally similar economies reduces the risk of overfitting and interpolation bias, preventing the algorithm from constructing a spurious pre-treatment match by combining unrelated units (Abadie et al., 2010). The cost is limited inference power, which is precisely why we complement the SCM with the DiD and event study at the departmental level.

4.2.2 Difference-in-Differences for Homicides

To estimate the causal effect of the crackdown policy on homicide rates at the departmental level, we employ a Difference-in-Differences (DiD) estimator that compares the evolution of homicide rates in El Salvador (treated group) relative to Honduras (control group) before and after the implementation of the policy in March 2022. Our baseline specification is:

$$\ln(HomRate_{dt}) = \alpha + \beta \cdot D_{dt} + \mu_d + \lambda_t + \varepsilon_{dt} \tag{5}$$

where $\ln(HomRate_{dt})$ is the log of the homicide rate per 100,000 inhabitants in department d at month-year t. D_{dt} is our treatment variable, which takes the value of one for departments in El Salvador after the implementation of the crackdown policy (March 2022), and zero otherwise. μ_d denotes department fixed effects, which control for time-invariant unobserved heterogeneity across departments. λ_t denotes month-year fixed effects, which control for aggregate shocks common to all departments in each period. ε_{dt} is the error term. Standard errors are clustered at the department level. The coefficient of interest is β; the log specification implies that β can be interpreted as the approximate percentage change in homicide rates attributable to the crackdown policy.

4.2.3 Event Study for Homicides

To assess the validity of the parallel trends assumption and to examine the dynamic effects of the crackdown policy over time, we complement our baseline DiD with an event study specification:

$$\ln(HomRate_{dt}) = \sum_{\substack{k=-14 \\ k\neq -1}}^{9} \gamma_k \cdot \mathbb{1}[t = k] \cdot Treated_d + \mu_d + \lambda_t + \varepsilon_{dt} \tag{6}$$

where $\mathbb{1}[t = k]$ is an indicator variable that equals one when the observation corresponds to period k relative to the crackdown policy date (March 2022, normalized to $k = 0$), and $Treated_d$ is an indicator that equals one for departments in El Salvador. The index k runs from -14 to 9, covering 14 months before and 9 months after the policy implementation. The coefficients γ_k capture the differential evolution of homicide rates between El Salvador and Honduras at each period k. μ_d denotes department fixed effects, λ_t denotes month-year fixed effects, and standard errors are clustered at the department level.

The event study serves two purposes. First, the pre-policy coefficients (γ_k for $k < -1$) provide a test of the parallel trends assumption: if these coefficients are jointly not statistically different from zero, we find no evidence of differential pre-trends between El Salvador and Honduras prior to the policy. Second, the post-policy coefficients (γ_k for $k \geq 0$) characterize the dynamic response of homicide rates to the crackdown. The post-policy coefficients thus reveal whether the homicide response was immediate, gradual, or sustained.

4.2.4 Fixed-Effects Model for Homicide-Detention Mechanisms

To estimate the association between detention rates and homicide rates, we estimate the following specification:

$$\ln(HomRate_{dt}) = \alpha + \beta \cdot \ln(DetRate_{dt}) + \mu_d + \gamma_m + \delta_y + \varepsilon_{dt} \tag{7}$$

where $\ln(HomRate_{dt})$ is the log of the homicide rate per 100,000 inhabitants in department d at month t, and $\ln(DetRate_{dt})$ is the log of the detention rate per 100,000 inhabitants in department d at month t. μ_d denotes department fixed effects, which control for time-invariant unobserved heterogeneity. γ_m and δ_y denote month and year fixed effects, respectively, which control for seasonal patterns and aggregate annual shocks. Standard errors are clustered at the department level. The coefficient of interest is β, which captures the elasticity of homicide rates with respect to detention rates.[2]

4.2.5 Fixed-Effects Model for Other Crimes

To examine the policy's effect on non-homicide crimes, we use a fixed-effects model that exploits variation across departments and survey months in the degree of exposure to the crackdown:

$$Y_{hdm} = \alpha + \beta_1 Treatment(1)_{hdm} + \beta_2 Treatment(2)_{hdm} + \theta X_{hdm} + \alpha_d + \gamma_m + \epsilon_{hdm} \quad (8)$$

where Y_{hdm} is a dummy variable that takes the value of 1 if a member of household h, in department d, and survey-month m reported being a victim of a particular crime. $Treatment(1)_{hdm}$ equals one for months partially impacted by the incarceration policy (MPHS April 2022–MPHS February 2023). $Treatment(2)_{hdm}$ equals one for months completely impacted by the incarceration policy (MPHS March 2023–MPHS December 2023).

X_{hdm} represents a series of control variables that may affect crime rates: dummy indicators for remittances, divorce status, poverty status, access to parks, and young individuals between the ages of 15 and 24 (Kovandzic and Vieraitis, 2006).[3] α_d and γ_m are department and survey-month fixed effects. We cluster standard errors at the department level. The parameters β_1 and β_2 are the estimators of interest.

[2] A natural alternative would be to include month-year fixed effects, which would absorb all aggregate shocks specific to each calendar month. However, in our setting this approach is problematic. Since the crackdown policy was implemented nationwide in March 2022 and affected all departments simultaneously, month-year fixed effects would absorb most of the variation in detention rates that is relevant for identification, as the policy shock is itself a month-year level event. We therefore follow the approach of separating month and year fixed effects, which controls for seasonality and annual trends while preserving the cross-departmental variation in detention rates that allows us to identify β.

[3] Remittances is a dichotomous variable that takes the value of 1 if the household received remittances in the last twelve months. Divorce is a dichotomous variable that takes the value of 1 if the head of the household reports being divorced. Poverty is a dichotomous variable that takes the value of 1 if the household income is less than the cost of the basic food basket (Banco Central de Reserva de El Salvador, 2023). Access to parks is a dichotomous variable that takes the value of 1 if the community has a park or green area. Finally, the variable age 15 to 24 takes the value of 1 if at least one person in that age group lives in the household.

5 Results

5.1 Descriptive Statistics

5.1.1 Pre-Treatment Balance: Synthetic Control

Table 2 reports the pre-treatment balance between El Salvador, Synthetic El Salvador, and the simple unweighted donor-pool average. Two things stand out. First, the synthetic control tracks El Salvador's average pre-treatment homicide rate nearly exactly: 5.302 versus 5.313 per 100,000 per quarter, well below the unweighted donor average of 5.694. Second, Synthetic El Salvador reproduces El Salvador's homicide rate at the two anchor quarters entered as predictors, returning 6.418 against the actual 6.959 in 2019q3, and 5.408 against 5.191 in 2022q1. The macroeconomic predictors are reasonably balanced, though M2 money supply per capita shows a wider gap. We attribute this to El Salvador's status as a dollarized economy, which carries unusually low monetary aggregates relative to neighbors that maintain independent currencies, with the exception of Panama.

5.1.2 Homicide Trends: El Salvador and Honduras

Table 3 and Figure II document the pre-crackdown trends for both countries. Prior to March 2022, Honduras fluctuated between 3 and 4 homicides per 100,000 inhabitants, while El Salvador fluctuated between 1 and 2. After the crackdown, El Salvador's rate spiked briefly and then dropped sharply, and the decline proved sustained. Honduras, by contrast, continued to fluctuate around its pre-policy average of 3.24 throughout the post-policy period (3.14), with a before–after difference of -0.10 that is small and statistically insignificant. El Salvador fell from 1.51 to 0.57, a statistically significant difference of -0.94 per 100,000 inhabitants, equivalent to a reduction of approximately 62%.

5.1.3 Victimization Rates: Household Survey

Table 4 presents descriptive statistics for the survey-based crimes. For street robbery, victimization fell from 2.75% in the unaffected period to 2.17% in the fully affected period, a 0.58-point drop corresponding to a 21% decline. Larceny, motor vehicle theft, and assault show no statistically significant change.

5.2 Findings for Homicides

5.2.1 Synthetic Control: Aggregate Findings

Table 5 reports the SCM weights. Synthetic El Salvador is built primarily from Guatemala (73.9%), Honduras (15.9%), and Mexico (10.2%), with Belize, Costa Rica, and Panama receiving zero weight. This composition is intuitive: Guatemala and Honduras share comparable levels of gang presence, urban poverty, and informal economic structures with El Salvador, while Mexico contributes through its broadly similar overall homicide rates. Among the predictors, exports per capita receives the highest weight (67.9%), followed by electricity consumption (12.0%) and the two homicide rate anchors (14.1% combined), with remittances, imports, and inflation accounting for the remainder.

Figure III plots the quarterly homicide rate for El Salvador (solid blue line) against Synthetic El Salvador (solid red line) from 2019q3 through 2023q4. In the pre-treatment period, the two trajectories track closely, both fluctuating between roughly 4.5 and 7 homicides per 100,000 inhabitants per quarter. This visual alignment is consistent with the balance statistics in Table 2. Starting in 2022q2, the first full quarter after the crackdown, the two series diverge abruptly and completely. El Salvador's homicide rate collapses from 5.2 in 2022q1 to just 1.0 in 2022q2, and continues declining to a low of 0.5 in 2023q3. Synthetic El Salvador, by contrast, remains stable at approximately 5 homicides per 100,000 throughout the post-treatment period, tracking the experience of the donor pool.

Table 6 reports the full post-treatment gaps. The difference between the actual and synthetic series ranges from roughly 4 homicides per 100,000 in 2022q2 (actual 1.003, synthetic 5.026) to nearly 4.6 in 2023q2 (actual 0.555, synthetic 5.088). Relative to the synthetic counterfactual, the crackdown is associated with a reduction of approximately 80% in the quarterly homicide rate. The effect shows no signs of decay over the seven post-treatment quarters we observe: the reduction is not a transient disruption but a sustained structural change in El Salvador's homicide landscape.

5.2.2 Synthetic Control: Inference Checks

We assess the statistical significance of the SCM estimate using the in-space placebo procedure of Abadie et al. (2010). For each donor country, we apply the same SCM algorithm as if that country had implemented the crackdown in 2022q2, building its synthetic control from the remaining units plus El Salvador. This procedure yields a distribution of placebo effects against which El Salvador's actual estimate can be compared.

Figure IV shows that El Salvador's MSPE ratio is 39.01, by far the largest in the distribution. The next highest is Guatemala at 11.02, followed by Panama at 8.13 and Costa Rica at 6.56. Three donors, Belize, Honduras, and Mexico, display ratios below 1, meaning their post-treatment fit is actually better than their pre-treatment fit. This is the expected outcome for an untreated unit. The pattern is unambiguous: El Salvador's post-treatment prediction error is 39 times larger than its pre-treatment error, while no donor country comes close.

Table 7 reports the MSPE ratios numerically. El Salvador is unambiguously the most extreme unit in the placebo distribution, with a ratio 3.5 times larger than the next closest donor country. The limited inference power in this setting is an inherent feature of our design: we restricted the donor pool to a small set of Mesoamerican economies that share El Salvador's historical, cultural, and institutional context, including comparable exposure to gang-related violence, similar levels of urban poverty, and broadly parallel economic structures. This ensures that Synthetic El Salvador is constructed from countries genuinely structurally similar to the treated unit, rather than from unrelated economies that happen to produce a good mathematical fit. Our small donor pool produces the strongest achievable evidence given the data, and El Salvador stands alone as the most extreme unit in the placebo distribution.

Figure V plots the full time path of the gap (Actual−Synthetic) for El Salvador and all six donor placebos, and reinforces this reading. During the pre-treatment period, El Salvador's gap oscillates close to zero, as expected for a well-fitted synthetic control, while the placebo gaps display more dispersion. After 2022q2, El Salvador's gap drops sharply and persistently to approximately -4 to -5 homicides per 100,000 per quarter, a trajectory that lies well outside the range of any donor placebo. No donor country exhibits a post-treatment gap of comparable magnitude or persistence. Put together, this pattern substantially strengthens the case that El Salvador's homicide decline reflects a genuine policy effect rather than a chance fluctuation.

The limited inference power that follows from our purposefully small donor pool motivates our complementary use of difference-in-differences and event study methods at the departmental level. These methods draw on a considerably larger sample (32 departments across El Salvador and Honduras), offer greater statistical power and formal pre-trend tests, and rest on distinct identifying assumptions. Together, the SCM and the DiD/Event Study offer a triangulated view of the same policy shock from two different empirical vantage points.

5.2.3 Difference-in-Differences and Event Study

The SCM estimates provide a compelling aggregate-level picture. We next turn to the departmental-level DiD and event study, which serve three complementary purposes: they exploit within-country spatial variation to validate the timing and uniformity of the effect across El Salvador's 14 departments, they draw on a larger sample with more statistical power than the SCM, and they allow us to test pre-treatment parallel trends formally. Together, the SCM and DiD provide a triangulated view of the same policy shock, each relying on distinct identifying assumptions.

Table 8 reports the main DiD results. The baseline estimate in Column (1) is -0.477, statistically significant at the 1% level, implying an approximate 48% reduction in homicide rates in El Salvador relative to Honduras. This estimate is broadly consistent with the SCM finding: both approaches identify a large and sudden decline in homicides following the crackdown, with the DiD capturing the within-department average effect and the SCM capturing the national aggregate gap against the counterfactual trajectory.

Figure VI presents the event study coefficients. The pre-policy coefficients (γ_k for $k < -1$) are not statistically different from zero, and their 95% confidence intervals consistently overlap with the zero line. This supports the parallel trends assumption and confirms that El Salvador and Honduras followed similar homicide trajectories before the crackdown. At $k = 0$ (March 2022), the coefficient is positive, consistent with the spike in violence that triggered the state of exception at the end of that month. From $k = 1$ onward, the estimated coefficients are large, negative, and statistically significant, and they remain stable through month 9.

5.2.4 Difference-in-Differences and Event Study: Robustness Checks

Table 8 and Figures VII and VIII report six robustness checks for the DiD estimate.

First, Column (2) addresses concerns about inference with a small number of clusters (32 departments) by reporting wild cluster bootstrap confidence intervals. The 95% confidence interval for the crackdown policy coefficient is $[-0.591, -0.367]$, confirming that our main result is not driven by the number of clusters.

Second, Column (3) examines sensitivity to functional form by using the homicide rate in levels rather than logs as the dependent variable. The estimated coefficient is -0.834, statistically significant at the 1% level. Given that the pre-policy mean homicide rate in El Salvador was 1.51 per 100,000 inhabitants, this implies a reduction of approximately 55% relative to the pre-policy mean, broadly consistent with our baseline estimate.

Third, Column (4) checks whether our results are driven by San Salvador, the capital department, which may behave differently from the rest of the country due to its size and urban characteristics. Excluding San Salvador, the estimated coefficient is -0.461, statistically significant at the 1% level and virtually identical to our baseline estimate, confirming that our results are not driven by this particular department.

Fourth, Column (5) addresses potential contamination of the control group. Departments in Honduras that border El Salvador (Ocotepeque, Lempira, Intibuca, La Paz, Valle, and Choluteca) may have been indirectly affected by the crackdown through spillover effects. Excluding these six border departments, the estimated coefficient is -0.482, statistically significant at the 1% level and very close to our baseline estimate of -0.477, confirming that spillover effects to neighboring Honduran departments do not meaningfully contaminate our control group.

Fifth, Figure VII presents a placebo test in which we reassign the crackdown policy date to March 2021, one year before the actual implementation. Under this false treatment date, we should find no significant effect on homicide rates if our baseline result is driven by the crackdown and not by pre-existing differential trends. Consistent with this expectation, the estimated coefficients are small in magnitude, oscillate around zero, and their 95% confidence intervals consistently overlap with the zero line both before and after the placebo date.

Sixth, Figure VIII extends the post-policy window through December 2023, covering 21 months after the crackdown. The estimated coefficients remain large, negative, and statistically significant throughout, with no signs of mean reversion. They stabilize at approximately -0.5 from month 1 onward and hold at similar levels through month 21.

Taken together, these six robustness checks consistently yield estimates that are statistically significant and close in magnitude to our baseline result of -0.477. The estimated effect of the crackdown policy on homicide rates is robust to alternative inference methods, functional forms, the exclusion of the capital department, potential spillover effects, and different time windows.

5.2.5 Mechanisms: The Role of Detentions

The crackdown policy encompassed several measures, the most prominent of which was the mass detention of individuals suspected of gang membership. We next examine the association between detention rates and the observed reduction in homicides.

Figure I shows that the monthly detention rate followed an inverted U-shape during the crackdown: it rose sharply in March and April 2022 and then fell in subsequent months. This pattern presents an empirical challenge. Including the full post-policy period would mechanically attenuate the estimated coefficient toward zero, as the initial surge in detentions is followed by a decline while homicide rates remain persistently low. We therefore restrict the sample to the period ending in April 2022, the month in which the detention rate peaked, in order to isolate the detention effect during the initial phase of the policy. This is not an ad hoc restriction: Figure VI shows that homicide rates stabilized precisely after April 2022 and held at that level throughout the remainder of the study period. This yields a final sample of 392 observations, corresponding to 14 departments observed over 28 months: 12 months in 2019, 12 months in 2021, and 4 months in 2022 (January through April).

The baseline estimate in Column (1) of Table 9 yields an elasticity of -0.150, statistically significant at the 5% level. A 1% increase in the detention rate is associated with a 0.15% reduction in homicide rates across departments. Given that the detention rate increased by approximately 150% during the crackdown, this elasticity implies a predicted reduction of approximately 23% (0.15×150) through the detention channel. Mass detentions thus account for approximately 50% of the 48% reduction in homicides associated with the crackdown policy.

The remaining columns assess the robustness of this finding. The wild cluster bootstrap confidence interval in Column (2) is $[-0.287, -0.389]$, excluding zero. Column (3) uses homicide rate levels instead of logs; the coefficient is -0.377, statistically significant at the 5% level. Column (4) excludes San Salvador department and returns -0.140, virtually identical to the baseline. Column (5) adds department-specific linear time trends to account for the pre-existing downward trend in homicide rates observed in 2019; the coefficient holds at -0.138, statistically significant at the 5% level, and our results are therefore not driven by pre-existing trends. Column (6) reports Oster's bounds (Oster, 2017); the interval $[-0.192, -0.131]$ lies entirely below zero, so the estimated association remains negative even under the most conservative assumptions about selection on unobservables.

The nature of this analysis prevents us from establishing a strictly causal relationship. That said, the fixed-effects estimates hold across a range of alternative specifications and, alongside Oster's bounds, are not sensitive to omitted variable bias. Mass detentions account for approximately half of the observed reduction in homicides associated with the crackdown policy.

5.3 Findings for Other Crimes

Going beyond homicides matters. Homicides are the most visible and measurable manifestation of gang violence, but they are not the only form of criminality that affects daily life and economic welfare in El Salvador. Crimes like street robbery, larceny, and assault directly reduce household welfare, constrain economic mobility, and impose both direct and indirect costs on firms and families. Street robbery, in particular, affects workers, small business owners, and commuters in ways that have measurable economic consequences: it shrinks labor market participation, raises transaction costs, and discourages investment in high-crime areas. Whether the crackdown generated spillovers into these non-homicide crimes is therefore essential for a complete welfare assessment of the policy.

5.3.1 Fixed Effects: Survey Findings

Table 10 reports the fixed-effects results from Equation (8). Columns 1 through 4 cover larceny, street robbery, motor vehicle theft, and assault, respectively.

For street robbery, the policy produced a statistically significant reduction. The Treatment 2 coefficient, which captures the fully affected period, is -0.008 (significant at the 1% level), implying that the probability of street robbery victimization fell by 0.80 percentage points for individuals in the fully affected group, a 29% reduction relative to the pre-policy mean of 2.75%. The Treatment 1 coefficient for the partially affected period is -0.004 and not statistically significant, which is consistent with the survey's measurement structure: partially exposed households have not yet accumulated a full year of post-policy experience, and the estimated effect is attenuated accordingly.

For larceny, motor vehicle theft, and assault, we find no statistically significant policy effect in either the partially or fully affected period. The Treatment 2 coefficients are 0.001, -0.001, and -0.002, respectively, all small and indistinguishable from zero. These null results are themselves informative. They suggest that the crackdown's effects on crime were concentrated in categories closely tied to gang activity (homicide and robbery), while more opportunistic property crimes (larceny, vehicle theft) and interpersonal violence (assault) went largely unaffected. This pattern fits an incapacitation logic: removing gang members from the streets curbs gang-related crimes but leaves untouched the socioeconomic conditions that drive non-gang offending.

5.3.2 Robustness Checks

We conduct four robustness checks to assess the sensitivity of the survey-based results. Table 11 reports the full results.

Sensitivity to Omitted Variable Bias (Panel A). We apply the bounding methodology of Oster (2017) to assess whether our street robbery result could be explained by unobserved time-varying confounders. The Oster bounds for Treatment 2 with respect to street robbery are $[-0.039, -0.006]$, an interval that lies entirely below zero, confirming that the estimated effect is not sensitive to omitted variable bias under the assumptions of the Oster framework. For the remaining crimes, the bounds are centered near zero, consistent with the null results in our main specification.

Wild Cluster Bootstrap Standard Errors (Panel B). Panel B reports p-values corrected using the wild cluster bootstrap procedure of Cameron et al. (2008). For street robbery under Treatment 2, the corrected p-value is 0.008, confirming that the result is statistically significant at the 1% level even after accounting for the small cluster count. The corrected p-values for larceny (0.849), motor vehicle theft (0.426), and assault (0.399) remain far from conventional significance thresholds, reinforcing the null results for these crimes.

Excluding the Department of San Salvador (Panel C). Panel C tests whether the results are driven by the capital department, which concentrates economic activity, policing resources, and gang presence in ways that may differ from the rest of the country. The Treatment 2 coefficient for street robbery remains -0.009, statistically significant at the 1% level, confirming that our findings are not exclusively driven by this single department.

Excluding Control Variables (Panel D). Our main results include household-level control variables (remittances, divorce, poverty, access to parks, and youth) that may affect crime rates. One might argue that the unexpected, nationwide nature of the crackdown renders these controls unnecessary or even potentially endogenous. Panel D shows the results without control variables. The Treatment 2 coefficient for street robbery remains -0.008, statistically significant at the 1% level, confirming that our main finding does not depend on their inclusion. The null results for larceny, motor vehicle theft, and assault are likewise unchanged.

Taken together, these four robustness checks consistently support our main findings: the crackdown policy reduced street robbery by approximately 29%, and this result holds up to concerns about omitted variable bias, inference with a small number of clusters, capital-city effects, and the inclusion of control variables.

6 Discussion

This paper evaluates the effects of El Salvador's 2022 crackdown policy on five categories of crime using a triangulated empirical approach. At the aggregate level, the SCM estimates indicate a reduction of approximately 80% in the quarterly homicide rate relative to the synthetic counterfactual. At the departmental level, the DiD estimate implies a 48% reduction. The mechanism analysis shows that the increase in detentions accounts for approximately half of the observed reduction in homicides. Household survey data and fixed-effects models further reveal a statistically significant 29% reduction in street robbery. We detect no measurable effects on assault, larceny, or motor vehicle theft. Taken together, these results point to a pattern of selective effectiveness on crime.

Our findings both confirm and extend prior evidence on the relationship between incarceration and crime. Our results align with evidence from higher-income settings: Levitt (1996) and Raphael and Johnson (2010) similarly document that incarceration policies reduce some crimes but not others, and Buonanno and Raphael (2013) find selective effects on theft and robbery following a mass pardon in Italy. El Salvador's case is distinctive in two respects. First, the scale of the intervention (a 150% increase in the prison population over ten months) is unprecedented in the literature, yet the pattern of selective reductions is broadly similar to what has been found in more moderate interventions. Second, unlike most prior studies, which rely on decarceration episodes or prison overcrowding litigation in high-income countries, our setting involves an abrupt and large-scale incarceration shock in a developing nation where gang structures are more territorially concentrated and visible. This makes our setting well suited to SCM estimation in the Mesoamerican context, just as with previous policies in the region such as the Mexican Drug War in 2006 (Balmori de la Miyar, 2016).

Two limitations deserve mention. First, our analysis covers the short to medium term; whether these reductions persist over longer horizons, as gang structures reorganize outside prison, remains to be seen. Second, the available data do not allow us to distinguish empirically between incapacitation and deterrence. The absence of effects on crimes less associated with gang activity, such as larceny, points to incapacitation as the primary driver, but this interpretation remains suggestive.

7 Conclusion

El Salvador's 2022 crackdown policy constitutes one of the most dramatic and abrupt incarceration shocks in recent history. We combine Synthetic Control Methods, Difference-in-Differences, an Event Study, and Fixed Effects to document large and sustained reductions in homicide rates following the crackdown, alongside a meaningful decline in street robbery. These reductions appear immediately after the policy's implementation, persist through the end of our study window in December 2023, and survive a comprehensive battery of robustness checks.

Several implications for policymakers follow from this evidence. First, large-scale incarceration can be effective in reducing violent and gang-related crimes, but should not be expected to reduce all forms of criminal behavior. While the policy substantially reduced homicides and street robbery, we find no detectable effects on assault, larceny, or motor vehicle theft; different types of crime appear to respond to different underlying mechanisms and may therefore require distinct policy approaches. Second, the benefits of the policy must be weighed against its costs and risks. The crackdown improved public safety along several dimensions, but the suspension of due process created a significant risk of wrongful detention, as the subsequent release of over 8,000 individuals makes plain (Suárez, 2025). Third, the replicability of El Salvador's model is likely to be limited. Its effectiveness depended in part on the ability to rapidly identify gang members through visible markers such as tattoos, a feature specific to MS-13 and Barrio 18. In countries where criminal organizations operate more covertly, this approach would be considerably harder to implement.

Finally, the structure of El Salvador's criminal economy may have made the crackdown particularly effective. Much of the revenue of MS-13 and Barrio 18 depended on territorial extortion directed at local residents and businesses. Once those organizations were disrupted through mass incarceration, the underlying criminal rents could disappear with them, since communities had little incentive to support the emergence of replacement groups. This dynamic may differ in settings where criminal organizations rely more heavily on retail drug markets or other illicit activities sustained by persistent consumer demand.

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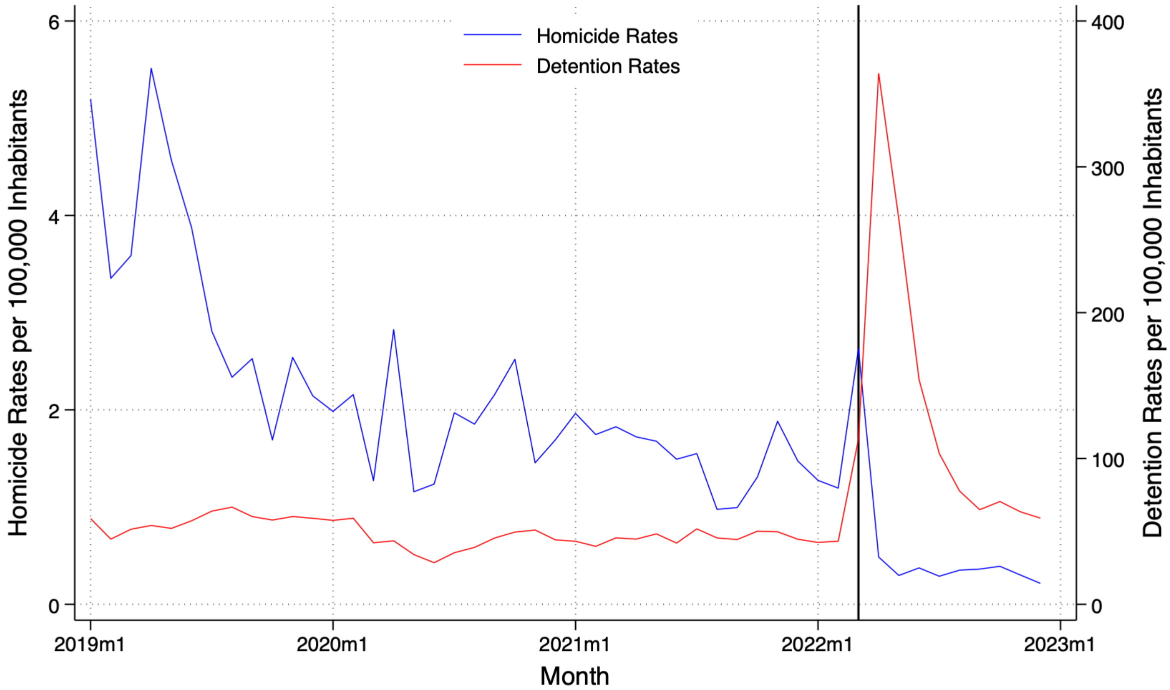
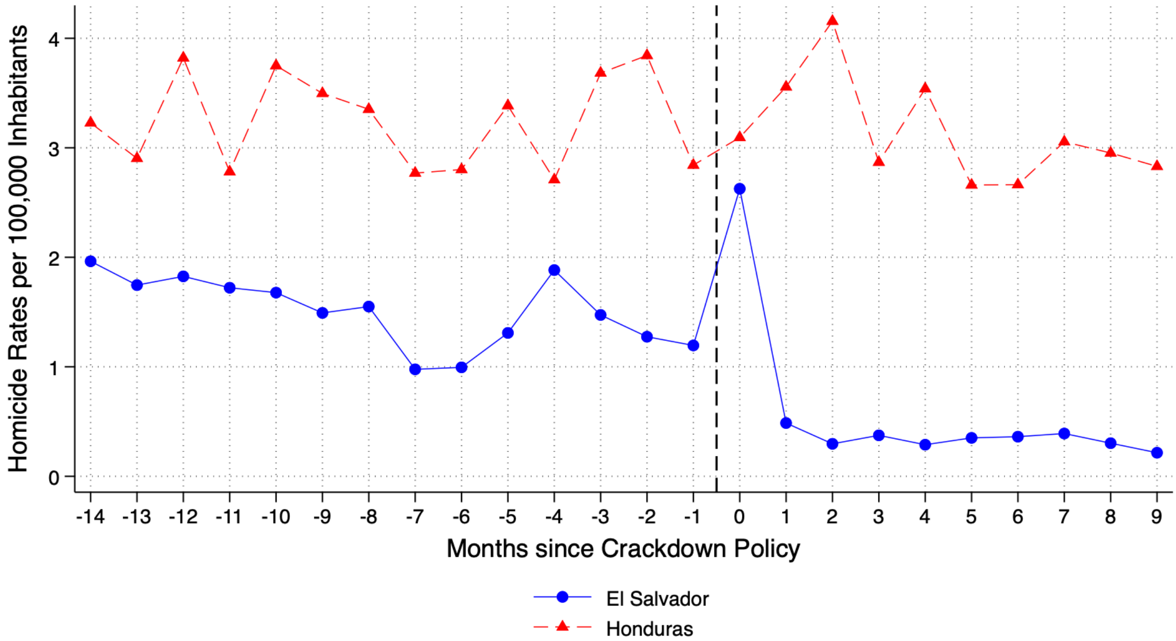
8 Figures

Figure I: Monthly Homicide and Incarceration Rates: 2019–2022



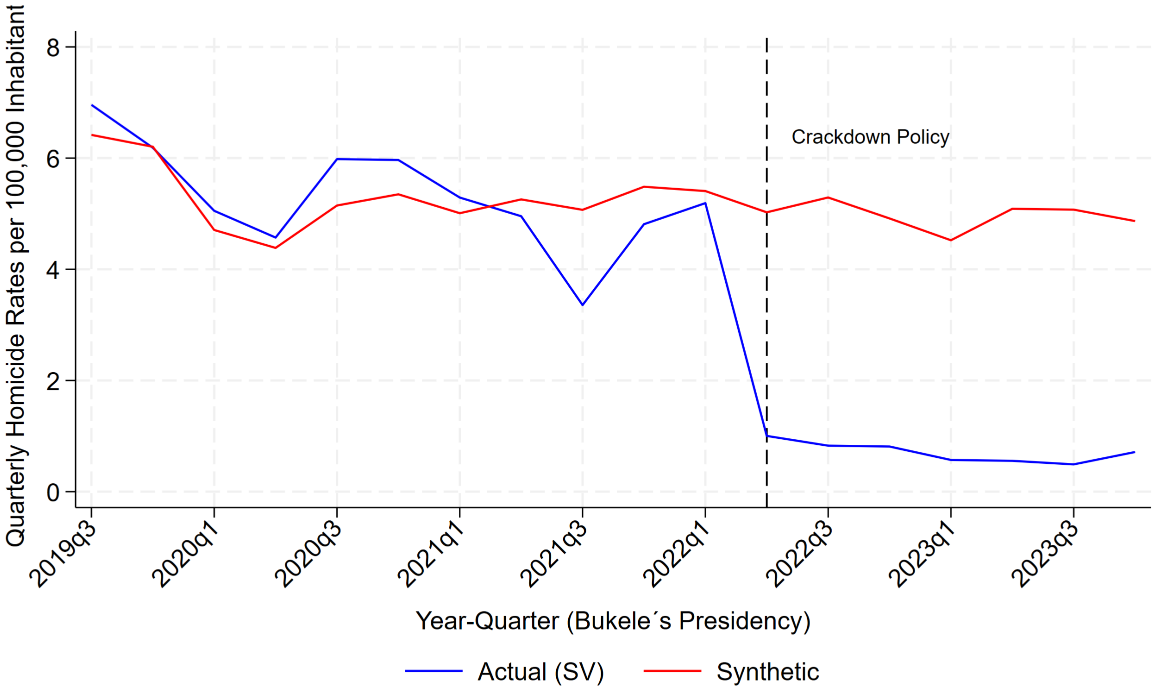
SOURCE: National Civil Police of El Salvador (NCP) (National Civil Police of El Salvador (NCP), 2024).
NOTES: The blue line depicts the monthly homicide rate per 100,000 inhabitants in El Salvador. The red line depicts the monthly detention rate per 100,000 inhabitants. The vertical black line marks March 2022, the month in which the crackdown policy was implemented.

Figure II: Homicide Rates: El Salvador vs Honduras



SOURCE: Honduras National Police (Secretaría de Estado en el Despacho de Seguridad de Honduras (SEPOL), 2024) and National Civil Police of El Salvador (NCP) (National Civil Police of El Salvador (NCP), 2024).
NOTES: The blue line with circles depicts the monthly homicide rate per 100,000 inhabitants in El Salvador (treated group). The red dashed line with triangles depicts the monthly homicide rate per 100,000 inhabitants in Honduras (control group). The vertical dashed line marks March 2022, the month of the crackdown policy implementation.

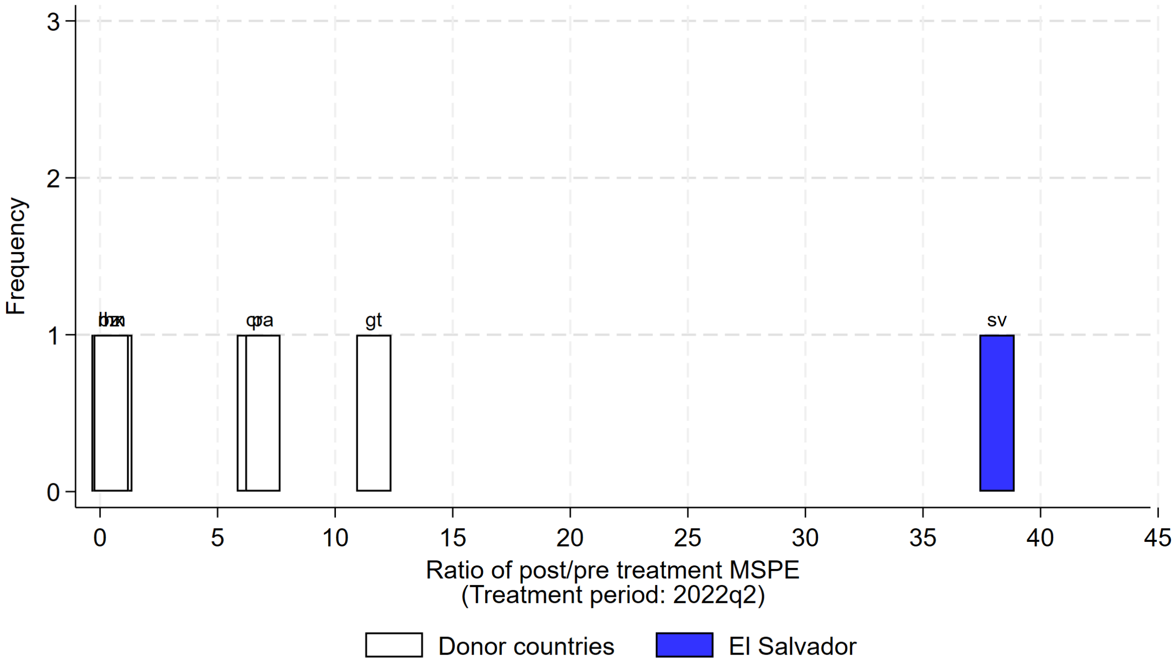
Figure III: Actual vs. Synthetic El Salvador: Quarterly Homicide Rates



SOURCE: National Civil Police of El Salvador (NCP) (National Civil Police of El Salvador (NCP), 2024); Honduras National Police (Secretaría de Estado en el Despacho de Seguridad de Honduras (SEPOL), 2024); Belize Crime Observatory (Belize Crime Observatory (BCO), 2024); Instituto Nacional de Estadística de Guatemala (Instituto Nacional de Estadística de Guatemala (INE), 2024); Datos Abiertos Panamá (Autoridad Nacional de Transparencia y Acceso a la Información (ANTAI), Panamá, 2024); Poder Judicial de Costa Rica (Poder Judicial de Costa Rica – Organismo de Investigación Judicial (OIJ), 2024; Observatorio de la Violencia, Ministerio de Justicia y Paz de Costa Rica, 2023); INEGI México (Instituto Nacional de Estadística y Geografía (INEGI), 2024); IMF Monetary and Financial Statistics (International Monetary Fund, 2024); national electricity regulators (Superintendencia General de Electricidad y Telecomunicaciones de El Salvador (SIGET), 2024; Empresa Nacional de Energía Eléctrica (ENEE), 2024; Administrador del Mercado Mayorista (AMM) de Guatemala, 2024; Instituto Costarricense de Electricidad (ICE), 2024; Centro Nacional de Control de Energía (CENACE) México, 2024; Empresa de Transmisión Eléctrica (ETESA) Panamá, 2024; Belize Electricity Limited (BEL), 2024).

NOTES: The vertical dashed line marks 2022q2, the first quarter after the crackdown policy. The blue solid line is the actual quarterly homicide rate in El Salvador. The red solid line is Synthetic El Salvador, constructed as a weighted combination of Guatemala (73.9%), Honduras (15.9%), and Mexico (10.2%). Donor weights are reported in Table 5.

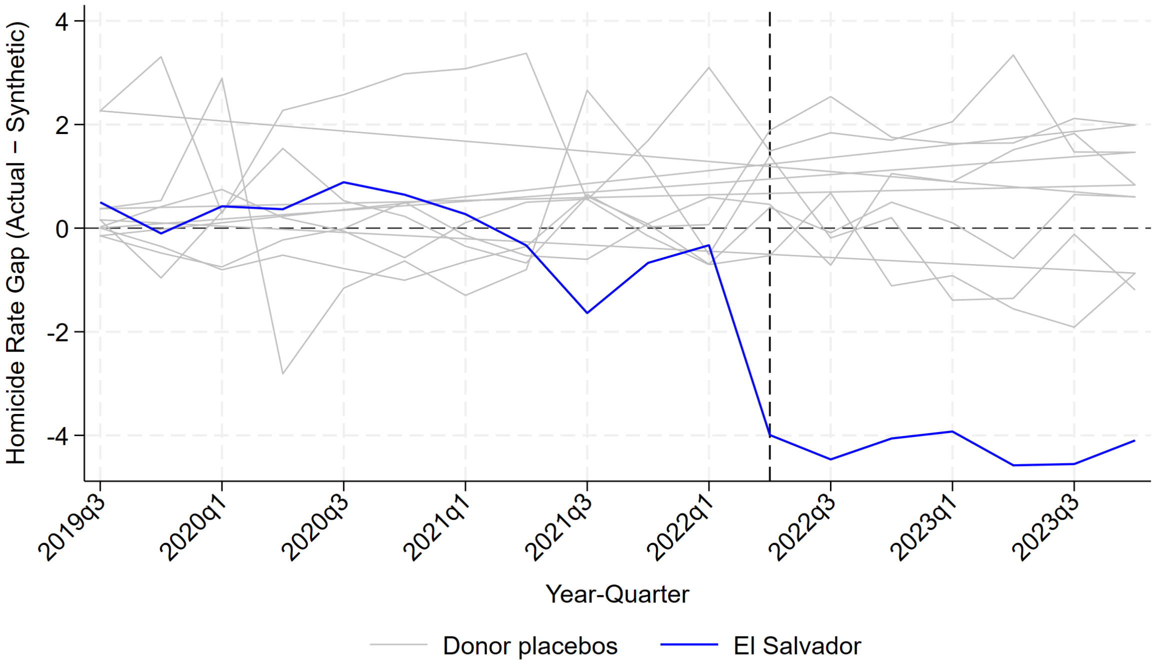
Figure IV: Distribution of Post/Pre-Treatment MSPE Ratios



SOURCE: National Civil Police of El Salvador (NCP) (National Civil Police of El Salvador (NCP), 2024); Honduras National Police (Secretaría de Estado en el Despacho de Seguridad de Honduras (SEPOL), 2024); Belize Crime Observatory (Belize Crime Observatory (BCO), 2024); Instituto Nacional de Estadística de Guatemala (Instituto Nacional de Estadística de Guatemala (INE), 2024); Datos Abiertos Panamá (Autoridad Nacional de Transparencia y Acceso a la Información (ANTAI), Panamá, 2024); Poder Judicial de Costa Rica (Poder Judicial de Costa Rica – Organismo de Investigación Judicial (OIJ), 2024; Observatorio de la Violencia, Ministerio de Justicia y Paz de Costa Rica, 2023); INEGI México (Instituto Nacional de Estadística y Geografía (INEGI), 2024); IMF Monetary and Financial Statistics (International Monetary Fund, 2024); national electricity regulators (Superintendencia General de Electricidad y Telecomunicaciones de El Salvador (SIGET), 2024; Empresa Nacional de Energía Eléctrica (ENEE), 2024; Administrador del Mercado Mayorista (AMM) de Guatemala, 2024; Instituto Costarricense de Electricidad (ICE), 2024; Centro Nacional de Control de Energía (CENACE) México, 2024; Empresa de Transmisión Eléctrica (ETESA) Panamá, 2024; Belize Electricity Limited (BEL), 2024).

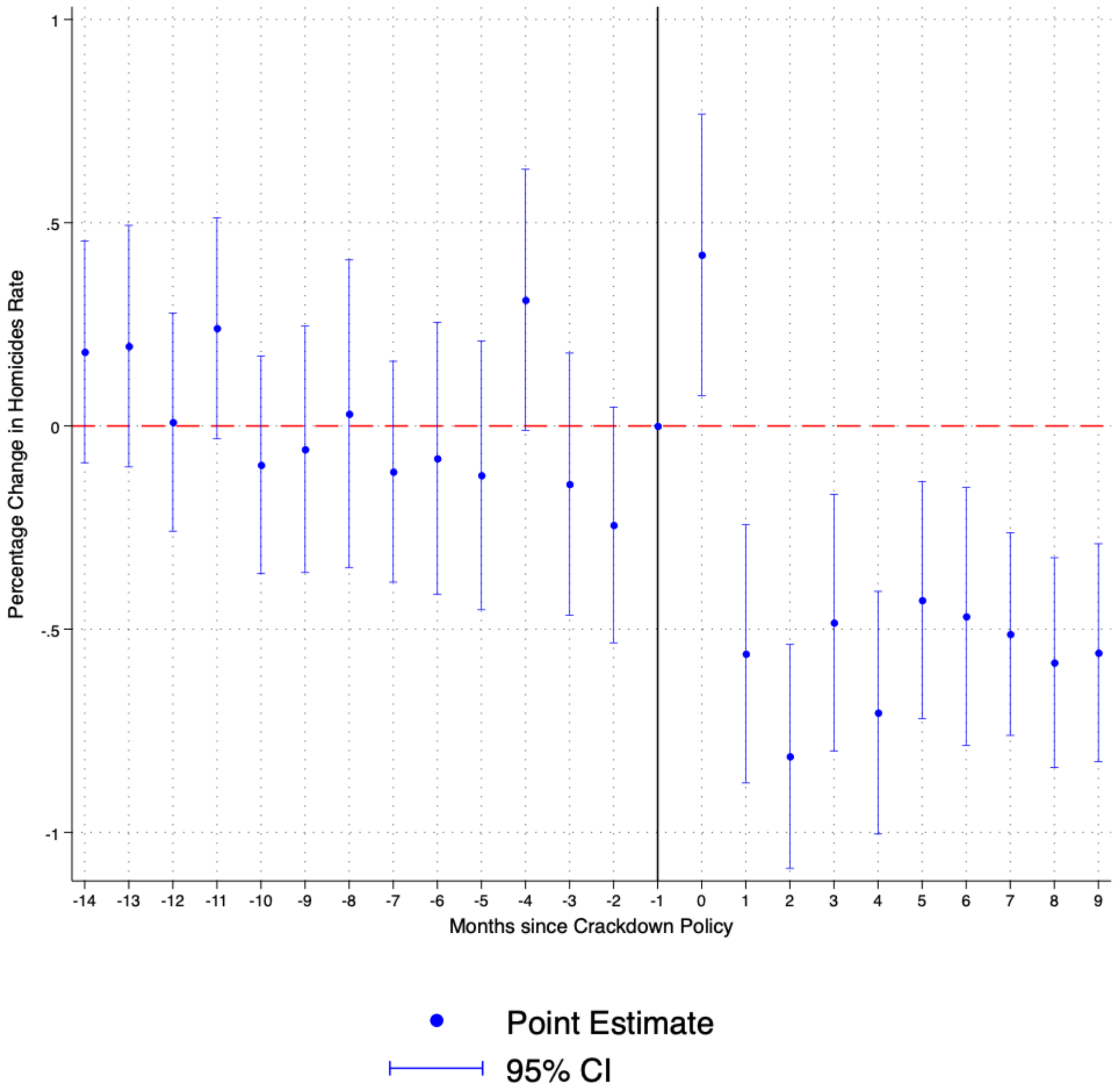
NOTES: Each bar represents one unit's ratio of post-treatment to pre-treatment MSPE. White bars are donor countries; the blue bar is El Salvador (sv). Country codes: bz = Belize, cr = Costa Rica, gt = Guatemala, hn = Honduras, mx = Mexico, pa = Panama, sv = El Salvador. El Salvador's ratio (39.01) far exceeds those of all donor placebos, making it the most extreme unit in the distribution by a margin 3.5 times larger than the next closest donor.

Figure V: In-Space Placebos: SCM Gap Paths



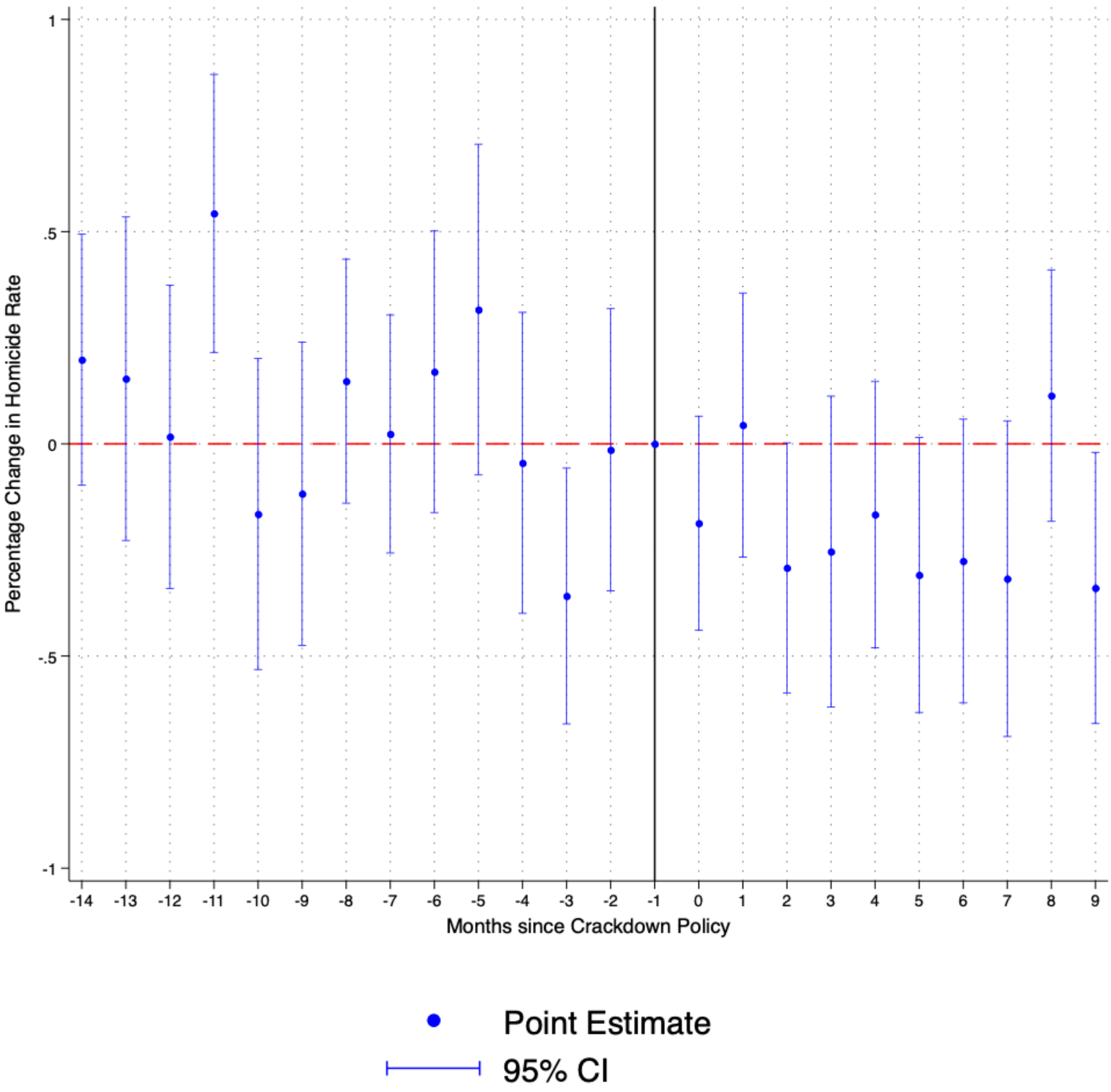
SOURCE: National Civil Police of El Salvador (NCP) (National Civil Police of El Salvador (NCP), 2024); Honduras National Police (Secretaría de Estado en el Despacho de Seguridad de Honduras (SEPOL), 2024); Belize Crime Observatory (Belize Crime Observatory (BCO), 2024); Instituto Nacional de Estadística de Guatemala (Instituto Nacional de Estadística de Guatemala (INE), 2024); Datos Abiertos Panamá (Autoridad Nacional de Transparencia y Acceso a la Información (ANTAI), Panamá, 2024); Poder Judicial de Costa Rica (Poder Judicial de Costa Rica – Organismo de Investigación Judicial (OIJ), 2024; Observatorio de la Violencia, Ministerio de Justicia y Paz de Costa Rica, 2023); INEGI México (Instituto Nacional de Estadística y Geografía (INEGI), 2024); IMF Monetary and Financial Statistics (International Monetary Fund, 2024); national electricity regulators (Superintendencia General de Electricidad y Telecomunicaciones de El Salvador (SIGET), 2024; Empresa Nacional de Energía Eléctrica (ENEE), 2024; Administrador del Mercado Mayorista (AMM) de Guatemala, 2024; Instituto Costarricense de Electricidad (ICE), 2024; Centro Nacional de Control de Energía (CENACE) México, 2024; Empresa de Transmisión Eléctrica (ETESA) Panamá, 2024; Belize Electricity Limited (BEL), 2024).
NOTES: The blue line is El Salvador (treated unit). Gray lines represent in-space placebo gaps for each of the six donor countries, each treated as if the crackdown occurred in 2022q2. El Salvador's post-treatment gap lies well outside the distribution traced by any donor placebo.

Figure VI: Event Study: Main Result



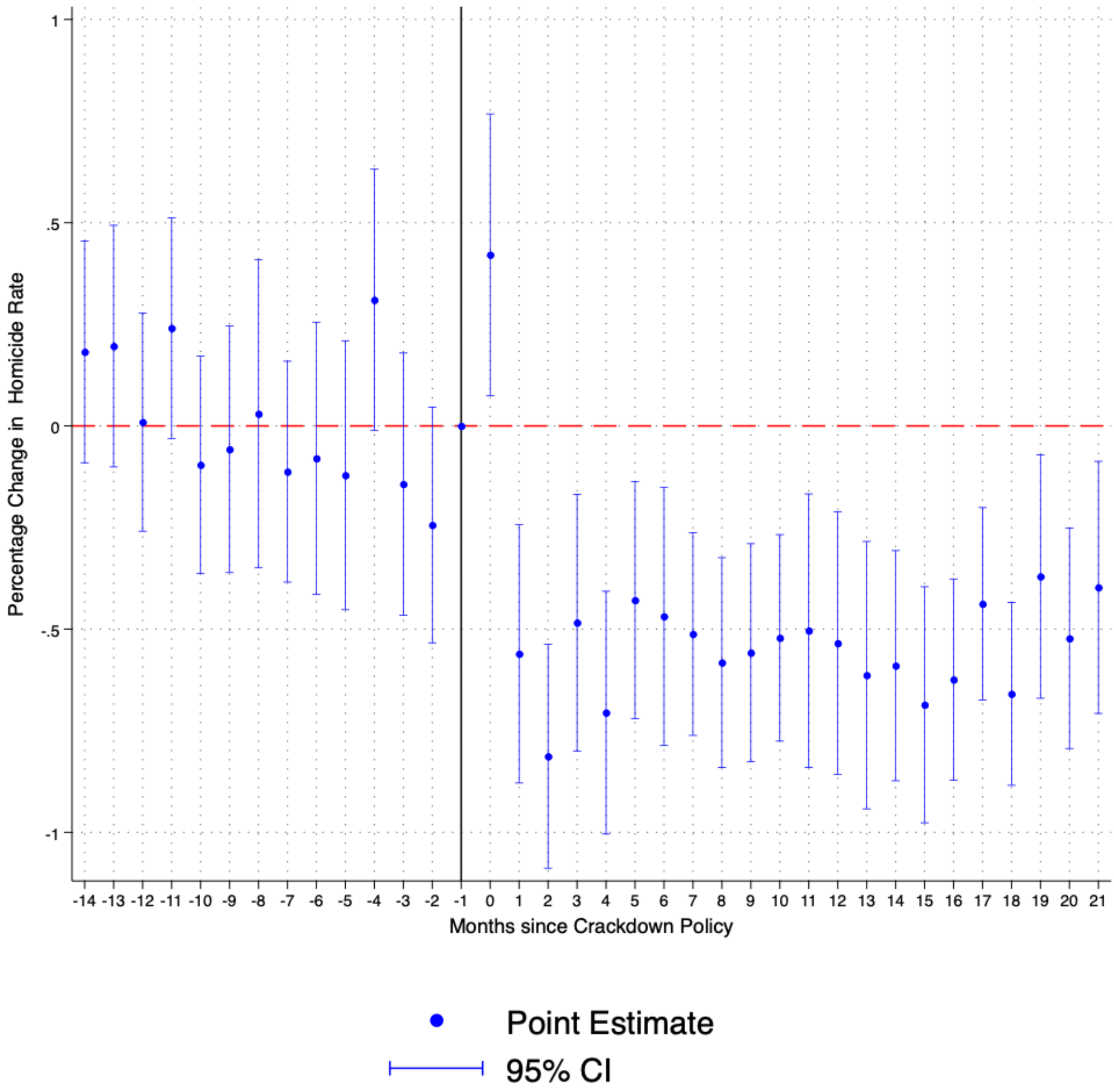
SOURCE: Honduras National Police (Secretaría de Estado en el Despacho de Seguridad de Honduras (SEPOL), 2024) and National Civil Police of El Salvador (NCP) (National Civil Police of El Salvador (NCP), 2024).
NOTES: The dependent variable is the log of the homicide rate per 100,000 inhabitants. Each blue dot represents the point estimate of the event-study coefficient γ_k, capturing the differential change in log homicide rates between El Salvador and Honduras at each period k relative to the placebo treatment date (March 2021, normalized to $k = 0$). The horizontal lines through each dot display the 95% confidence intervals. The placebo treatment date is reassigned one year prior to the actual policy to verify that the baseline result is not driven by pre-existing differential trends.

Figure VII: Event Study: Placebo



SOURCE: Honduras National Police (Secretaría de Estado en el Despacho de Seguridad de Honduras (SEPOL), 2024) and National Civil Police of El Salvador (NCP) (National Civil Police of El Salvador (NCP), 2024).
NOTES: The dependent variable is the log of the homicide rate per 100,000 inhabitants. Each blue dot represents the point estimate of the event-study coefficient γ_k, capturing the differential change in log homicide rates between El Salvador and Honduras at each period k relative to the placebo treatment date (March 2021, normalized to $k = 0$). The horizontal lines through each dot display the 95% confidence intervals. The placebo treatment date is reassigned one year prior to the actual policy to verify that the baseline result is not driven by pre-existing differential trends.

Figure VIII: Event Study: Extended Time Window



SOURCE: Honduras National Police (Secretaría de Estado en el Despacho de Seguridad de Honduras (SEPOL), 2024) and National Civil Police of El Salvador (NCP) (National Civil Police of El Salvador (NCP), 2024).
NOTES: The dependent variable is the log of the homicide rate per 100,000 inhabitants. Each blue dot represents the point estimate of the event-study coefficient γ_k, capturing the differential change in log homicide rates between El Salvador and Honduras at each period k relative to the crackdown policy date (March 2022, normalized to $k = 0$). The horizontal lines through each dot display the 95% confidence intervals. The post-policy window is extended through December 2023 (21 months after implementation) to assess the persistence of the policy effect.

9 Tables

Table 1: Survey Data Collection and Incarceration Policy Impact Analysis

Data Source		Collect Data From:					Months Imp. by Policy	Affected Status	Treatment Group
MPHS	Survey	Start Month	Start Year		End Month	End Year	Count	Status	Code
MPHS 2022	January	January	2021	–	December	2021	0	Not affected	0
	February	February	2021	–	January	2022	0	Not affected	0
	March	March	2021	–	February	2022	0	Not affected	0
	April	April	2021	–	March	2022	1	Partially affected	1
	May	May	2021	–	April	2022	2	Partially affected	1
	June	June	2021	–	May	2022	3	Partially affected	1
	July	July	2021	–	June	2022	4	Partially affected	1
	August	August	2021	–	July	2022	5	Partially affected	1
	September	September	2021	–	August	2022	6	Partially affected	1
	October	October	2021	–	September	2022	7	Partially affected	1
	November	November	2021	–	October	2022	8	Partially affected	1
	December	December	2021	–	November	2022	9	Partially affected	1
MPHS 2023	January	January	2022	–	December	2022	10	Partially affected	1
	February	February	2022	–	January	2023	11	Partially affected	1
	March	March	2022	–	February	2023	12	Completely affected	2
	April	April	2022	–	March	2023	12	Completely affected	2
	May	May	2022	–	April	2023	12	Completely affected	2
	June	June	2022	–	May	2023	12	Completely affected	2
	July	July	2022	–	June	2023	12	Completely affected	2
	August	August	2022	–	July	2023	12	Completely affected	2
	September	September	2022	–	August	2023	12	Completely affected	2
	October	October	2022	–	September	2023	12	Completely affected	2
	November	November	2022	–	October	2023	12	Completely affected	2
	December	December	2022	–	November	2023	12	Completely affected	2

SOURCE: Multipurpose Household Survey (MPHS), Banco Central de Reserva de El Salvador (Banco Central de Reserva de El Salvador, 2023).
NOTES: Months Imp. by Policy refers to the number of months the survey reference period overlaps with the crackdown policy. Treatment Group: 0 = Not affected, 1 = Partially affected, 2 = Completely affected. Period of analysis: January 2022–December 2023 (MPHS 2022 and 2023).

Table 2: Matching Period Characteristics: El Salvador, Synthetic El Salvador, and Donor Pool (Pre-treatment period: 2019q3–2022q1)

	El Salvador	Synthetic El Salvador	Donor Pool*
A. Outcome variable			
Homicide rate[a] (avg., per 100,000 qrt.)	5.302	5.313	5.694
Homicide rate, 2019q3[b]	6.959	6.418	6.157
Homicide rate, 2022q1[c]	5.191	5.408	5.259
B. Macroeconomic predictors (pre-treatment period average)^d			
Quarterly Inflation (%)	0.231	0.333	0.279
Remittances per capita	265.652	173.754	115.254
Exports per capita	306.032	291.938	772.723
Imports per capita	532.433	415.273	801.799
M2 Money Supply per capita	2640.365	2205.921	5167.807
Electricity Consumption (kWh pc)	1042.187	881.132	1499.966

SOURCE: National Civil Police of El Salvador (NCP) (National Civil Police of El Salvador (NCP), 2024); Honduras National Police (Secretaría de Estado en el Despacho de Seguridad de Honduras (SEPOL), 2024); Belize Crime Observatory (Belize Crime Observatory (BCO), 2024); Instituto Nacional de Estadística de Guatemala (Instituto Nacional de Estadística de Guatemala (INE), 2024); Datos Abiertos Panamá (Autoridad Nacional de Transparencia y Acceso a la Información (ANTAI), Panamá, 2024); Poder Judicial de Costa Rica (Poder Judicial de Costa Rica – Organismo de Investigación Judicial (OIJ), 2024; Observatorio de la Violencia, Ministerio de Justicia y Paz de Costa Rica, 2023); INEGI México (Instituto Nacional de Estadística y Geografía (INEGI), 2024); IMF Monetary and Financial Statistics (International Monetary Fund, 2024); national electricity regulators (Superintendencia General de Electricidad y Telecomunicaciones de El Salvador (SIGET), 2024; Empresa Nacional de Energía Eléctrica (ENEE), 2024; Administrador del Mercado Mayorista (AMM) de Guatemala, 2024; Instituto Costarricense de Electricidad (ICE), 2024; Centro Nacional de Control de Energía (CENACE) México, 2024; Empresa de Transmisión Eléctrica (ETESA) Panamá, 2024; Belize Electricity Limited (BEL), 2024).

NOTES: Period of analysis: 2019q3–2022q1 (pre-treatment matching period).

* Simple unweighted average across the six donor countries: Belize, Costa Rica, Guatemala, Honduras, Mexico, and Panama.

[a] Average over all 11 pre-treatment quarters (2019q3–2022q1). We choose this pre-treatment period as it corresponds to Nayib Bukele's first presidential term, during which the crackdown policy was implemented.

[b] Homicide rate in 2019q3 (first period in sample; enters as matching predictor).

[c] Homicide rate in 2022q1 (last pre-treatment quarter; enters as matching predictor).

[d] Macroeconomic predictors, with the exceptions of inflation (%) and electricity consumption (kWh pc), are in constant USD per capita.

Table 3: Descriptive Statistics

	Honduras			El Salvador		
	Before	After	Difference	Before	After	Difference
Homicide Rate	3.24	3.14	-0.10	1.51	0.57	-0.94***
N	252	180		196	140	

SOURCE: Honduras National Police (Secretaría de Estado en el Despacho de Seguridad de Honduras (SEPOL), 2024) and National Civil Police of El Salvador (NCP) (National Civil Police of El Salvador (NCP), 2024).
NOTES: Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Period of analysis: January 2021–December 2022.

Table 4: Descriptive Statistics of Survey Records

	Not Affected	Partially Affected	Completely Affected	Difference	Difference
	(1)	(2)	(3)	(1 vs 2)	(1 vs 3)
Larceny (%)	1.59	1.49	1.71	-0.10*	0.12
Street robbery (%)	2.75	2.59	2.17	-0.16	-0.58**
Motor vehicle theft (%)	0.28	0.23	0.27	-0.05	0.01
Assault (%)	1.30	1.16	1.39	-0.14	0.09
Observations	4,204	14,733	13.034	18,937	17,238

SOURCE: Multipurpose Household Survey (MPHS), Banco Central de Reserva de El Salvador (Banco Central de Reserva de El Salvador, 2023).
NOTES: Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Period of analysis: January 2022–December 2023 (MPHS 2022 and 2023).

Table 5: Synthetic Control Weights

Panel A: Donor Country Weights (^*)	
Country	Weight
Belize	0.0000
Costa Rica	0.0000
Guatemala	0.7390
Honduras	0.1590
Mexico	0.1020
Panama	0.0000
Panel B: Predictor Variable Weights (diagonal of ^)*	
Variable	Weight
Quarterly Inflation (%)	0.0001
Remittances per capita	0.0555
Exports per capita	0.6789
Imports per capita	0.0040
M2 Money Supply per capita	0.0003
Electricity Consumption (kWh pc)	0.1200
Homicide Rate (2019q3)	0.0947
Homicide Rate (2022q1)	0.0465

SOURCE: National Civil Police of El Salvador (NCP) (National Civil Police of El Salvador (NCP), 2024); Honduras National Police (Secretaría de Estado en el Despacho de Seguridad de Honduras (SEPOL), 2024); Belize Crime Observatory (Belize Crime Observatory (BCO), 2024); Instituto Nacional de Estadística de Guatemala (Instituto Nacional de Estadística de Guatemala (INE), 2024); Datos Abiertos Panamá (Autoridad Nacional de Transparencia y Acceso a la Información (ANTAI), Panamá, 2024); Poder Judicial de Costa Rica (Poder Judicial de Costa Rica – Organismo de Investigación Judicial (OIJ), 2024; Observatorio de la Violencia, Ministerio de Justicia y Paz de Costa Rica, 2023); INEGI México (Instituto Nacional de Estadística y Geografía (INEGI), 2024); IMF Monetary and Financial Statistics (International Monetary Fund, 2024); national electricity regulators (Superintendencia General de Electricidad y Telecomunicaciones de El Salvador (SIGET), 2024; Empresa Nacional de Energía Eléctrica (ENEE), 2024; Administrador del Mercado Mayorista (AMM) de Guatemala, 2024; Instituto Costarricense de Electricidad (ICE), 2024; Centro Nacional de Control de Energía (CENACE) México, 2024; Empresa de Transmisión Eléctrica (ETESA) Panamá, 2024; Belize Electricity Limited (BEL), 2024).
NOTES: Period of analysis: 2019q3–2023q4.

Table 6: Post-treatment predictions (quarterly)

Quarter	Actual (SV)	Synthetic
2022q2	1.003	5.026
2022q3	0.828	5.292
2022q4	0.812	4.915
2023q1	0.571	4.522
2023q2	0.555	5.088
2023q3	0.491	5.073
2023q4	0.713	4.868

SOURCE: National Civil Police of El Salvador (NCP) (National Civil Police of El Salvador (NCP), 2024); Honduras National Police (Secretaría de Estado en el Despacho de Seguridad de Honduras (SEPOL), 2024); Belize Crime Observatory (Belize Crime Observatory (BCO), 2024); Instituto Nacional de Estadística de Guatemala (Instituto Nacional de Estadística de Guatemala (INE), 2024); Datos Abiertos Panamá (Autoridad Nacional de Transparencia y Acceso a la Información (ANTAI), Panamá, 2024); Poder Judicial de Costa Rica (Poder Judicial de Costa Rica – Organismo de Investigación Judicial (OIJ), 2024; Observatorio de la Violencia, Ministerio de Justicia y Paz de Costa Rica, 2023); INEGI México (Instituto Nacional de Estadística y Geografía (INEGI), 2024); IMF Monetary and Financial Statistics (International Monetary Fund, 2024); national electricity regulators (Superintendencia General de Electricidad y Telecomunicaciones de El Salvador (SIGET), 2024; Empresa Nacional de Energía Eléctrica (ENEE), 2024; Administrador del Mercado Mayorista (AMM) de Guatemala, 2024; Instituto Costarricense de Electricidad (ICE), 2024; Centro Nacional de Control de Energía (CENACE) México, 2024; Empresa de Transmisión Eléctrica (ETESA) Panamá, 2024; Belize Electricity Limited (BEL), 2024).
NOTES: Period of analysis: 2022q2–2023q4 (post-treatment period).

Table 7: In-space placebo: MSPE ratios

Country	MSPE pre	MSPE post	Ratio post/pre
Belize	2.6940	1.0327	0.3833
Costa Rica	0.1948	1.2786	6.5629
Guatemala	0.3301	3.8436	11.6436
Honduras	6.4065	4.0257	0.6284
Mexico	0.4713	0.2230	0.4732
Panama	0.1993	1.3803	6.9258
El Salvador¬	0.4727	18.0337	38.1480

¬: El Salvador (treated unit)

SOURCE: National Civil Police of El Salvador (NCP) (National Civil Police of El Salvador (NCP), 2024); Honduras National Police (Secretaría de Estado en el Despacho de Seguridad de Honduras (SEPOL), 2024); Belize Crime Observatory (Belize Crime Observatory (BCO), 2024); Instituto Nacional de Estadística de Guatemala (Instituto Nacional de Estadística de Guatemala (INE), 2024); Datos Abiertos Panamá (Autoridad Nacional de Transparencia y Acceso a la Información (ANTAI), Panamá, 2024); Poder Judicial de Costa Rica (Poder Judicial de Costa Rica – Organismo de Investigación Judicial (OIJ), 2024; Observatorio de la Violencia, Ministerio de Justicia y Paz de Costa Rica, 2023); INEGI México (Instituto Nacional de Estadística y Geografía (INEGI), 2024); IMF Monetary and Financial Statistics (International Monetary Fund, 2024); national electricity regulators (Superintendencia General de Electricidad y Telecomunicaciones de El Salvador (SIGET), 2024; Empresa Nacional de Energía Eléctrica (ENEE), 2024; Administrador del Mercado Mayorista (AMM) de Guatemala, 2024; Instituto Costarricense de Electricidad (ICE), 2024; Centro Nacional de Control de Energía (CENACE) México, 2024; Empresa de Transmisión Eléctrica (ETESA) Panamá, 2024; Belize Electricity Limited (BEL), 2024).
NOTES: Period of analysis: 2019q3–2023q4.

Table 8: Homicides: Main Result and Robustness Checks

	(1) Main	(2) Wild Cluster	(3) Homicide Rate	(4) No San Salvador	(5) No Border Depts.
Crackdown Policy (DID)	-0.477***	-0.477***	-0.834***	-0.461***	-0.482***
	(0.056)	[-0.591, -0.367]	(0.170)	(0.056)	(0.067)
N	768	768	768	744	624
Department FE	Yes	Yes	Yes	Yes	Yes
Month-Year FE	Yes	Yes	Yes	Yes	Yes

SOURCE: Honduras National Police (Secretaría de Estado en el Despacho de Seguridad de Honduras (SEPOL), 2024) and National Civil Police of El Salvador (NCP) (National Civil Police of El Salvador (NCP), 2024).
NOTES: Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Period of analysis: January 2021–December 2022.All specifications include department and month-year fixed effects. Standard errors clustered at the department level in parentheses. Column (2) reports wild cluster bootstrap confidence intervals in brackets. Column (3) uses homicide rate levels instead of log. Column (4) excludes San Salvador department. Column (5) excludes Honduras border departments to address contamination concerns.

Table 9: Detention Mechanism: Main Result and Robustness Checks

	(1) Main	(2) Wild Cluster	(3) Homicide Rate	(4) No San Salvador	(5) Time Trends	(6) Oster's bounds
Log (Detention Rate)	-0.150**	-0.150**	-0.377**	-0.140**	-0.138**	-0.150**
	(0.055)	[-0.287, -0.389]	(0.171)	(0.057)	(0.057)	[-0.192, -0.131]
N	392	392	392	364	392	392
Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Department FE	Yes	Yes	Yes	Yes	Yes	Yes

SOURCE: National Civil Police of El Salvador (NCP) (National Civil Police of El Salvador (NCP), 2024).
NOTES: Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Period of analysis: 2019–2022 (excluding 2020).Dependent variable is log homicide rate except in Column (3) where levels are used. Column (2) reports wild cluster bootstrap confidence intervals in brackets. Column (3) uses homicide rate levels instead of log. Column (4) excludes San Salvador department. Column (5) includes department-specific linear trends. Column (6) reports Oster bounds in brackets.

Table 10: Fixed-Effects: Main Results of Survey Records

	Larceny	Street Robbery	Motor Vehicle Theft	Assault
	(1)	(2)	(3)	(4)
Treatment (1)	-0.003	-0.004	-0.002	-0.005
	(0.003)	(0.003)	(0.001)	(0.003)
Treatment (2)	0.001	-0.008***	-0.001	-0.002
	(0.003)	(0.002)	(0.002)	(0.003)
Constant	0.017***	0.025***	0.005**	0.013***
	(0.003)	(0.003)	(0.002)	(0.003)
Control Variables	Yes	Yes	Yes	Yes
Fixed Effects	Yes	Yes	Yes	Yes
Observations	31971	31971	31971	31971
Pre-policy Mean	0.0159	0.0275	0.0028	.0130
Treatment (1) % change	-18.87%	-14.55%	71.43%	-38.46%
Treatment (2) % change	6.29%	-29.09%	35.71%	15.38%

SOURCE: Multipurpose Household Survey (MPHS), Banco Central de Reserva de El Salvador (Banco Central de Reserva de El Salvador, 2023).
NOTES: Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Period of analysis: January 2022–December 2023 (MPHS 2022 and 2023). Control variables are remittances, divorce, poverty, access to parks, and young people between the ages of 15 and 24. Baseline fixed effects are included at the department and survey-month. Robust standard errors are clustered at the department level.

Table 11: Robustness Checks: Survey Records

Panel A: Oster's Bounds

	Larceny	Street Robbery	Motor Vehicle Theft	Assault
	(1)	(2)	(3)	(4)
Treatment (1)	[-0.002, ,-0.008]	[-0.028, ,0.001]	[-0.004, -0.001]	[-0.013, -0.003]
Treatment (2)	[-0.009, ,0.002]	[-0.039, -0.006]	[-0.004, -0.003]	[-0.011, 0.005]
Control Variables	X	X	X	X
Fixed Effects	X	X	X	X
Observations	31971	31971	31971	31971

Panel B: Wild Cluster Standard Errors

	Larceny	Street Robbery	Motor Vehicle Theft	Assault
	(1)	(2)	(3)	(4)
Treatment (1)	-0.003	-0.004	-0.002	-0.005
	(0.401)	(0.254)	(0.242)	(0.102)
	[0.409]	[0.256]	[0.223]	[0.101]
Treatment (2)	0.001	-0.008***	-0.001	-0.002
	(0.858)	(0.005)	(0.408)	(0.438)
	[0.849]	[0.008]	[0.426]	[0.399]
Control Variables	X	X	X	X
Fixed Effects	X	X	X	X
Observations	31971	31971	31971	31971

Panel C: Excluding San Salvador Department

	Larceny	Street Robbery	Motor Vehicle Theft	Assault
	(1)	(2)	(3)	(4)
Treatment (1)	-0.003	-0.005	-0.002*	-0.006
	(0.004)	(0.003)	(0.001)	(0.004)
Treatment (2)	-0.001	-0.009***	-0.003**	-0.004
	(0.003)	(0.003)	(0.001)	(0.003)
Control Variables	X	X	X	X
Fixed Effects	X	X	X	X
Observations	26363	26363	26363	26363

Panel D: Excluding Control Variables

	Larceny	Street Robbery	Motor Vehicle Theft	Assault
	(1)	(2)	(3)	(4)
Treatment (1)	-0.003	-0.004	-0.002	-0.005
	(0.003)	(0.003)	(0.001)	(0.003)
Treatment (2)	0.001	-0.008***	-0.001	-0.002
	(0.003)	(0.002)	(0.002)	(0.003)
Fixed Effects	X	X	X	X
Observations	31971	31971	31971	31971

SOURCE: Multipurpose Household Survey (MPHS), Banco Central de Reserva de El Salvador (Banco Central de Reserva de El Salvador, 2023).
NOTES: Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Period of analysis: January 2022–December 2023 (MPHS 2022 and 2023).Panel A presents the bounds in brackets estimated using Oster's bounds methodology. Panel B estimates the results using wild cluster standard errors. The p-values are presented in parenthesis, and the p-values using wild cluster standard errors are presented in brackets. Panel C presents the model without control variables. Panel D presents results without San Salvador Department Control variables are remittances, divorce, poverty, access to parks, and young people between the ages of 15 and 24. Baseline fixed effects are included at the department and survey-month. Robust standard errors are clustered at the department level.