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Less Crime, More Schooling: Evidence from El Salvador's Gang Crackdown

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Less Crime, More Schooling: Evidence from El Salvador's Gang Crackdown

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Abstract

We study how El Salvador's March 2022 state of exception, a mass-incarceration crackdown that abruptly dismantled the country's system of gang governance, affected school attendance. Our design compares teenage boys, the group most exposed to gang recruitment and victimization, with pre-teen boys observed in the same quarters, using household-survey microdata that span 2017 to 2024. School attendance among teenage boys rose by about 4 percentage points, roughly 6 percent of their pre-crackdown rate. The effect appears immediately at the onset of the crackdown, grows over the following two years, and survives placebo tests, alternative comparison groups, and sensitivity analyses. The gains were largest among adolescents from poor households, a pattern that runs against a pure income channel, since detaining household earners should have pushed schooling in the opposite direction. A large literature documents how rising crime and armed conflict reduce schooling. We document the reverse: dismantling gang governance raised educational investment among the adolescents most exposed to recruitment by criminal organizations.

Keywords: El Salvador, criminal governance, gangs, incarceration, schooling.
JEL: I25, I26, K14, K42.

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1 Introduction

In many developing countries, criminal organizations exert territorial control that shapes the daily lives of households. Gangs restrict mobility, extort businesses, and recruit young men, and in doing so they distort the incentives families face when they invest in their children's human capital. Does dismantling this system of criminal governance increase schooling? The answer depends on which of two opposing forces dominates. Yet, removing criminal control may relax mobility constraints, revive local economic activity, and raise the returns to education, all of which strengthen the incentive to attend school. A crackdown built on mass incarceration may instead generate negative income shocks when detained household members are also earners, which tightens family budgets and pushes children out of school. Which force dominates is an empirical question.

We study El Salvador's 2022 state of exception, one of the largest internal security operations in recent Latin American history. After the deadliest weekend in the country's recent history, the government launched a nationwide crackdown on the street gangs that had governed much of its territory. Beginning in March 2022, it suspended constitutional protections, raised prison sentences for gang membership from 20 to 40 years, and detained tens of thousands of suspected gang members. Within a year the prison population had more than doubled and homicides had fallen by about 42 percent (Silverio-Murillo et al., 2025). The decline in gang presence and violence was abrupt and it has persisted.

To identify the effect of this shock on schooling, we compare teenage boys, the group most exposed to the gang recruitment and victimization the crackdown removed, with pre-teen boys observed in the same quarters, using difference-in-differences and event-study designs. Salvadoran gangs recruit overwhelmingly in the early-to-mid teens (Sviatschi, 2022b; Castro et al., 2025), which places pre-teens largely below the recruitment margin and makes them a contemporaneous benchmark for what teenage attendance would have been absent the policy. School attendance in our setting is far from universal. Unlike in high-income countries, where compulsory schooling comes with near-universal attendance, many Salvadoran children do not attend school despite the legal requirement. Schooling therefore reflects an active household choice that weighs expected returns against costs, including the opportunity cost of a child's time and the risks that crime imposes, which leaves substantial scope for the local security environment to move attendance.

This age-based strategy has a long tradition in economics, from the cohort comparisons in Duflo (2001) to evaluations of age-targeted policies that use unaffected age groups as contemporaneous controls or placebos (Bryan et al., 2020). We implement it with monthly microdata from El Salvador's Multipurpose Household Survey (EHPM) that cover 2017 to 2024. Our preferred estimates exclude the pandemic year 2020.

The crackdown raised teen boys' school attendance by about 4 percentage points, roughly 6 percent of their pre-crackdown rate of 0.73. The increase appears at the March 2022 state of exception and builds over the following two years. The estimate is stable across fixed-effects specifications and geographic units, and it survives a battery of placebo and robustness checks. We focus on boys because the recruitment and victimization channels the crackdown disrupted fell disproportionately on males. The corresponding estimate for girls is, if anything, larger, but a pre-existing upward trend in girls' attendance confounds it, making the crackdown's contribution difficult to disentangle from the preexisting trend.[1]

Our central contribution is to examine the reverse direction of a well-established literature. A large body of work shows that increases in crime and armed conflict reduce schooling: gangs recruit adolescents and raise dropout (Kalsi, 2018; Melnikov et al., 2020; Sviatschi, 2022b), drug-war and cartel violence lower attainment and test scores (Brown and Velasquez, 2017; Monteiro and Rocha, 2015), and armed conflict leaves lasting scars on exposed cohorts (Duque, 2024; Acosta et al., 2023). We provide the mirror image: dismantling criminal governance at a national scale raises school attendance. Two further features distinguish the paper. First, we study a contemporaneous behavioral response, whether adolescents stay in school while the policy is in force, rather than the long-run scarring the conflict literature emphasizes; the relevant margin is therefore recruitment-age adolescents rather than young children. Second, our national, age-based design complements the neighborhood-level evidence in Melnikov et al. (2020) and the school-level evidence in Castro et al. (2025) on gangs and schooling in El Salvador.

Our objective is positive rather than normative. The state of exception involved substantial trade-offs: the suspension of constitutional protections, due-process concerns, and the risk of wrongful detention, all in exchange for a safer environment (Aleman, 2022; U.S. Department of State, 2023). We take no position on whether the benefits justified those costs. We examine one consequence of a large policy shock, in the same spirit as a broad literature on the human-capital effects of conflicts, security interventions, and other contested policies whose consequences are often unintended.

Our findings do speak to the costs of criminal governance itself. Salvadoran gangs not only recruited adolescents, they also shaped everyday decisions by restricting mobility, enforcing territorial boundaries, and exposing young people to violence and coercion. These constraints reached families' schooling decisions. We find that dismantling gang governance relaxed them and raised school attendance among recruitment-age boys, with the largest gains in poor households. The implication is not that mass incarceration is the appropriate policy response. It is that restoring the state's ability to guarantee security and freedom of movement may be an important condition for human-capital accumulation in high-crime environments.

The remainder of the paper is organized as follows. Section 2 reviews the literature on criminal governance and education. Section 3 describes the rise of gang rule in El Salvador and the 2022 state of exception. Section 4 introduces the data and the difference-in-differences design. Section 5 presents the main estimates and event studies, and Section 6 reports the robustness checks. Section 7 examines heterogeneity and mechanisms. Section 8 concludes.

[1] Appendix A presents the full empirical analysis for girls.

2 Criminal Governance and Education

2.1 Theory

Our starting point is the human-capital model of educational choice: households invest in a child's schooling when the expected returns exceed the costs, including the opportunity cost of the child's time (Becker, 1964). In our setting, households make that decision under criminal governance, the exercise of authority and social control by criminal organizations that partially substitute for or complement the state (Lessing, 2020). Gangs such as those in El Salvador operate through more than violence. They establish systems of rule built on territorial control, the enforcement of order, the provision of selective goods, and the regulation of local economic activity. They therefore shape the environment in which families decide whether to keep their children in school.

Territorial control by organized crime can depress schooling on each of these margins (Melnikov et al., 2020; Sviatschi, 2022b). First, gangs raise the opportunity cost of schooling by recruiting adolescents as lookouts, couriers, or extortion collectors, and by making the trip to school unsafe. Second, they tighten household budgets, since extortion and violence depress local incomes and push children to substitute work for school (Brown and Velasquez, 2017). Third, by restricting mobility and the jobs residents can hold, they lower the returns to education and the incentive to invest in it (Melnikov et al., 2020). Each channel maps onto a term in the schooling-investment decision, and each predicts lower enrollment where criminal governance is heaviest.

When the state confronts such governance, the economics of crime predicts that raising the certainty and severity of punishment deters offending and that incapacitating offenders mechanically reduces it (Becker, 1968). El Salvador's 2022 crackdown pressed on both margins at once, through mass arrests and sharply longer sentences. Central American governments had tried "mano dura" (iron-fist) policing before with little durable effect on gang power (Wolf, 2012). What distinguishes the 2022 state of exception is a scale and persistence that produced an abrupt and lasting collapse in violence, which we document in Section 3.

What happens to schooling when criminal governance is dismantled is, in theory, ambiguous. The three channels should reverse: ending recruitment lowers the opportunity cost of schooling, reduced extortion and revived local activity relax household budget constraints, and a freer labor market raises the returns to education. Still, the effect may be muted if the underlying structural conditions, such as poverty, school quality, and limited labor-market opportunity, remain unchanged.

The instrument also matters, and its effect on the households it touches is not obvious. Detaining a working-age earner is a negative income shock that can pull children out of school. In Sweden, Dobbie et al. (2018) find that parental incarceration lowers children's high-school completion, with the harm concentrated in the most disadvantaged families. Incarceration can also remove a harmful household member, and elsewhere the net effect on children is neutral or even positive: close to zero in the United States (Norris et al., 2021) and, among low-income families in Colombia, an increase in children's educational attainment (Arteaga, 2023). Because El Salvador detained a large share of its young men in a short window, this household channel is not merely hypothetical, though its sign is genuinely ambiguous. Whether teen attendance rises or falls is therefore an empirical question, and the answer may differ across groups depending on which channel dominates.

2.2 Empirical Evidence

A large body of empirical work documents that crime and violence depress schooling. Far less is known about what happens when the state rolls them back. We organize the evidence around four themes: crime and schooling, armed conflict and schooling, the age-based exposure designs we build on, and the recruitment of adolescents by gangs. We then draw the El Salvador evidence together.

The causal literature on crime and schooling is consistent in sign. In Mexico's war on drugs, Brown and Velasquez (2017) find that rising homicide reduced years of completed schooling, most of all for boys, while Michaelsen and Salardi (2020) show that homicides near schools shortly before national exams depress test scores through acute stress. In Rio de Janeiro's favelas, Monteiro and Rocha (2015) find that armed battles between drug gangs during the school year lower math achievement. In Peru, Sviatschi (2022a) shows that higher coca prices pulled children aged 11 to 14 out of school and into the drug trade, and in Colombia Duque (2024) finds that cartel-era violence raised dropout and lowered exam scores.

The evidence is particularly clear in El Salvador. Kalsi (2018) shows that the arrival of U.S.-deported gang members reduced educational attainment, with the largest effects among adolescent boys, the group most exposed to recruitment. At a finer geographic scale, Melnikov et al. (2020) find that residents living just inside gang-controlled blocks in San Salvador are about 15 percentage points less likely to complete high school, a pattern they attribute to recruitment, restricted mobility, and poverty. Martínez Dahbura (2018) exploits the 2012 gang truce, the only previous nationwide shock to gang activity in the country, and documents heterogeneous effects on schooling by age and gender. Rather than the emergence or intensification of criminal governance, we study its abrupt dismantling.

A parallel literature studies armed conflict, which typically identifies effects from cohorts differentially exposed by their age and location during the violence. In Latin America, Chamarbagwala and Morán (2011) show that rural Maya cohorts who were school-aged during Guatemala's civil war in high-violence departments completed up to a year less schooling, and León (2012) finds lasting education losses from Peru's political violence. Similar cohort-exposure results hold for the Tajik civil war (Shemyakina, 2011), the Bosnian war (Swee, 2015), and the Rwandan genocide (Akresh and de Walque, 2008). Two features of this work bear on our design. First, for El Salvador specifically, Acosta et al. (2023) find that cohorts exposed in early childhood to the civil war completed 0.85 fewer years of schooling and attended at an 8 percentage-point lower rate decades later, the same country and a similar empirical approach applied to an earlier shock. Second, the age at which exposure matters depends on the outcome. Several studies find that early-childhood exposure has the largest effects on long-run attainment (Duque, 2024; León, 2012), while others identify adolescence as the critical period: Akresh et al. (2012) show that adolescent exposure has lasting effects on adult height, and Swee (2015) finds that conscription of draft-age males reduced secondary enrollment but left primary schooling largely unaffected. Because our outcome is contemporaneous attendance rather than long-run attainment, the relevant margin in our setting is recruitment-age adolescents.

Methodologically, we build on an empirical literature that identifies the effect of a national shock by comparing groups differentially exposed to it by age. The approach originates with Duflo (2001), who identified the returns to Indonesia's school-construction program by comparing birth cohorts young enough to benefit with those already too old, and Osili and Long (2008) extended it to other national policies. The same logic underlies age-targeted policy evaluations that use unexposed age bands as placebos. Studying U.S. Tobacco-21 laws, Bryan et al. (2020) validate their design on 18-to-20-year-olds against untreated 13-to-15-year-olds, and Kaestner et al. (2003) show that welfare reform moved schooling for the targeted group in a targeted-versus-untargeted comparison. Our teen-versus-pre-teen design belongs to this family: teenagers are exposed to the gang risks the crackdown removed, pre-teens largely are not, and the pre-teens serve as the contemporaneous control.

That teenagers rather than younger children are the exposed group rests on a well-documented age profile of gang recruitment. In the United States, gang joining is rare before age 12, peaks around ages 13 to 14, and declines thereafter (Pyrooz and Sweeten, 2015; Gilman et al., 2014; Lahey et al., 1999), and gang membership itself lowers educational attainment (Pyrooz, 2014). For El Salvador, Sviatschi (2022b) documents that recruitment concentrates in the early-to-mid teens and that cohorts exposed to gang arrival at those ages, but not at older ones, suffer lasting schooling and incarceration effects (Sviatschi, 2019). Castro et al. (2025) show that a school-based program deterring recruitment cut dropout, with effects concentrated at ages 12 to 16. This age-selectivity is not unique to gangs, since armed groups in conflict settings claim adolescents while sparing young children (Blattman and Annan, 2010; Swee, 2015). Put together, this work places pre-teens below the recruitment margin and teens squarely within it. If Salvadoran gangs recruit somewhat younger than U.S. gangs, pre-teens are partially exposed, and to the extent that the crackdown reaches their schooling through the same channels, this contamination of the comparison group biases our estimate downward.

The El Salvador evidence points in one direction: gangs suppressed schooling at precisely the ages they recruited (Kalsi, 2018; Melnikov et al., 2020; Sviatschi, 2022b, 2019). Teen attendance is the margin that responds to household and security shocks, while pre-teen attendance is near-universal (Rubio, 2020), and a school-level security program raised attendance at recruitment ages (Castro et al., 2025). Companion work on the 2022 crackdown confirms that the policy sharply reduced crime, and homicides fell by about 42 percent (Silverio-Murillo et al., 2025). The treatment therefore had real bite. What remains unknown, and what we provide evidence on, is whether rolling back criminal governance at national scale raised schooling. Our contribution is threefold: we study the removal rather than the arrival of crime, we measure a contemporaneous behavioral response distinct from the long-run scarring the conflict literature emphasizes, and we identify the effect from the age gradient of exposure. Our national-scale variation complements the school-level (Castro et al., 2025) and neighborhood-level (Melnikov et al., 2020) evidence.

3 The Context in El Salvador: Crisis and Response

For decades following its civil war, El Salvador faced persistently high levels of violence tied to the consolidation of transnational street gangs, chiefly Mara Salvatrucha (MS-13) and Barrio 18 (Wolf, 2012; Ruiz, 2025). A large body of research traces the origins of these organizations to U.S. immigration policies in the 1990s, which deported gang-affiliated individuals from the United States to El Salvador (Kalsi, 2018; Wolf, 2012). Once repatriated, these actors rapidly rebuilt their organizational structures and reasserted territorial control (Melnikov et al., 2020).

By the late 2010s, gang territorial control had produced a paradoxical equilibrium. Criminal organizations maintained coercive dominance over specific neighborhoods, enforced informal borders, and restricted residents' movement. Gang governance became embedded in everyday life across many communities and shaped local labor markets, mobility, and economic activity. At the same time, homicide rates declined markedly from their mid-2010s peak. That decline masked the persistence of extortion, forced recruitment, and localized violence, particularly in urban areas (Melnikov et al., 2020).

The equilibrium collapsed in March 2022, when El Salvador experienced a sudden surge in lethal violence that culminated in the deadliest day in decades, with 62 homicides recorded on March 26 alone. Authorities attributed the killings to MS-13 retaliation. On March 27, the Legislative Assembly approved Decree No. 333, declaring a State of Exception and granting security forces broad authority to conduct warrantless arrests in gang-controlled territories (Aleman, 2022).

The Constitution of El Salvador allows a State of Exception during war or severe threats to public order (Américas, 2022). The 2022 decree introduced three main measures. First, it temporarily suspended several constitutional protections, including freedom of assembly and association, and expanded government surveillance powers over private communications. Second, reforms to the criminal code raised prison sentences for gang membership from 20 to 40 years. Third, the government launched a mass incarceration campaign that authorized security forces to detain individuals without prior judicial warrants (Américas, 2022).

The declaration launched one of the most extensive internal security operations in recent Latin American history. Before the State of Exception, quarterly arrests averaged about 10,000 individuals. Between April and June 2022 alone, authorities conducted over 46,000 arrests, more than four times the pre-policy baseline (Figure 1). The surge in detentions drove the prison population from roughly 40,000 to over 100,000 inmates, a 150% increase within a single year (Fair and Wlamsley, 2024).

The burden of gang activity fell unevenly across ages, which is why our analysis centers on teenage boys. Salvadoran gangs recruit overwhelmingly from adolescent males, with recruitment concentrated in the early-to-mid teens (Wolf, 2012; Castro et al., 2025).[2] These same years mark the onset of victimization and forced participation: lookout duty, extortion collection, and pressure to affiliate, all of which make schooling costly for teenagers in gang-controlled areas.[3]

Consistent with this margin, Melnikov et al. (2020) find that living inside gang territory in San Salvador lowers the probability of completing high school by about 15 percentage points, through recruitment, restricted mobility, and poverty, and Kalsi (2018) shows that the arrival of the gangs reduced schooling most for the boys they targeted. Pre-teen boys sit below this recruitment margin, and their attendance is near-universal and largely insulated from these pressures. This age gradient is what our design exploits, since the crackdown lifted risks that bound on teenage boys (ages 13–18) but not on younger children, which makes pre-teens (ages 7–12) a natural benchmark for what teen attendance would have done absent the policy. Because recruitment and its attendant pressures fall overwhelmingly on males, we focus on boys throughout, and Appendix A shows that the same design does not identify a clean effect for girls.

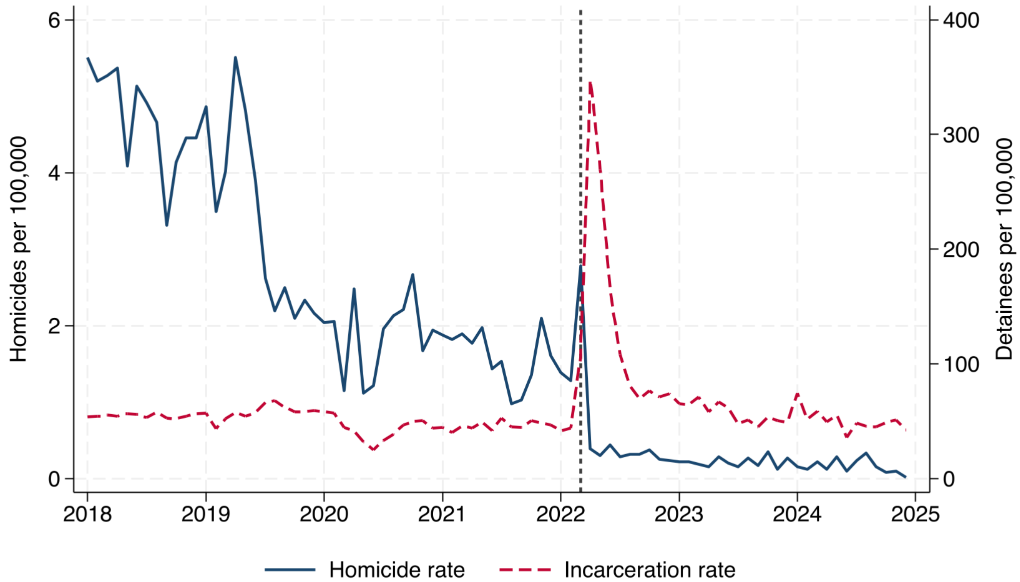
3.1 Descriptive evidence

The government declared the state of exception on 27 March 2022, after the deadliest weekend in the country's recent history. The homicide series spikes sharply in March 2022 (Figure 1). Within a month, the monthly incarceration rate rose from about 50 to more than 350 detainees per 100,000 inhabitants. The monthly homicide rate, already trending down since 2018, fell from roughly 1.5–2 to about 0.3 per 100,000, where it has remained since. The crackdown therefore reduced gang presence and violence abruptly and persistently. This is the policy shock whose consequences for schooling we study.

[2] The same age profile appears in U.S. cohort data, where gang joining is rare before age 12, peaks around ages 13–14, and declines thereafter (Pyrooz and Sweeten, 2015; Gilman et al., 2014). For El Salvador, Castro et al. (2025) show that police patrols deterring recruitment in schools cut dropout and reduced gang recruitment by about 20 percent, with effects concentrated among children at recruitment ages.

[3] The age-selectivity of armed recruitment, and its schooling consequences, are not unique to gangs. In conflict settings armed organizations draw disproportionately on adolescents rather than young children. The Lord's Resistance Army in Uganda, for instance, abducted teenage boys while largely sparing those below about age eleven, and the resulting human-capital losses concentrate on the adolescent conscription margin (Blattman and Annan, 2010).

Figure 1: Homicides and incarceration in El Salvador



Notes: The figure plots monthly homicide rates (left axis) and detention rates (right axis), measured per 100,000 inhabitants, for El Salvador as a whole over the period 2018m1–2024m12. The dashed vertical line marks the onset of the state of exception (27 March 2022). *Source:* Administrative records from El Salvador's National Civil Police (PNC).

How did school attendance respond to this large-scale crackdown on gangs and organized crime? Among boys, attendance among pre-teens holds steady at about 0.95 throughout the sample period, while attendance among teenagers is substantially lower and the teen–pre-teen gap is stable before the March 2022 state of exception (Panel A of Figure 2). After the crackdown, teenage attendance rises visibly relative to its pre-treatment mean while pre-teen attendance barely moves. This divergence is the raw pattern our difference-in-differences estimates formalize.

Figure 2: Mean school attendance (levels), teen vs. pre-teen 2017–2024.

Notes: Dashed horizontal lines denote each group's average over the pre-treatment period (2017q1–2022q1) and the post-treatment period (2022q2–2024q4). Teenagers are defined as individuals aged 13–18, and pre-teens as those aged 7–12. The dashed vertical line marks the onset of the crime crackdown (2022q2). The shaded gray band denotes the COVID-19 pandemic year (2020), which is excluded from our preferred specifications. *Source:* Authors' calculations using microdata from El Salvador's Multipurpose Household Survey (EHPM), Oficina Nacional de Estadística y Censos (ONEC), formerly DIGESTYC, 2017–2024.

Girls are different. Panel B of Figure 2 reveals an upward trend in attendance among teenage girls that begins well before the crackdown, and such a trend violates the parallel-trends assumption our design requires. What could account for a trend among girls but not boys? One plausible explanation is the secular decline in adolescent pregnancy, a leading cause of dropout that disproportionately affects girls. El Salvador has long had one of the highest adolescent birth rates in Latin America, but the rate fell steadily from about 77 births per 1,000 women aged 15–19 in 2010 to about 67 in 2020 and 54 in 2023 (World Bank, 2024). This decline coincided with sustained policy efforts, including the launch in November 2017, within our pre-treatment period, of the "National Intersectoral Strategy for the Prevention of Pregnancy in Girls and Adolescents" (ENIPENA, 2017–2027), which coordinated health, education, and child-protection services to reduce early pregnancy (Gobierno de El Salvador, 2017). Because adolescent pregnancy disproportionately leads girls to leave school, its secular decline is consistent with the gradual rise in girls' attendance we observe before the crackdown. A broader regional trend toward greater educational investment in girls may have reinforced the pattern.

Whatever its source, a pre-existing upward trend makes the identifying assumptions less credible for girls, and we therefore cannot attribute the estimated change in girls' attendance to the crackdown. Our main analysis focuses on boys, for whom we find no evidence of differential pre-treatment trends. We analyze girls separately and discuss their results in Appendix A.

4 Data and Empirical Strategy

4.1 Data

We use microdata from El Salvador's Multipurpose Household Survey (EHPM), a nationally representative household survey fielded continuously throughout the year. For each household member the survey records whether the individual attended school during the previous week, our outcome of interest, along with age, sex, and region of residence. It also collects household characteristics such as poverty status, household headship, and urban or rural location, which we use in the heterogeneity analysis. We work with the 2017–2024 survey waves, aggregated to the person-quarter level.

Our preferred specifications exclude calendar year 2020 for two reasons. First, the COVID-19 pandemic simultaneously disrupted schooling and survey collection. The EHPM was not fielded during April–June 2020, and the 2020q3 wave contains roughly half the observations collected in neighboring quarters, about 1,550 person-quarter observations of children aged 7–18 against roughly 3,000 in 2020q1 and 2020q4. Teen attendance also jumps sharply in the two quarters that follow the missing wave, which we attribute to the temporary shift to remote instruction and to changes in how attendance was recorded during lockdowns rather than to changes in the underlying teen–pre-teen differential. Second, the 2020 survey does not report respondents' department of residence, which prevents us from estimating specifications that rely on department-level fixed effects. Throughout the paper we report estimates on the full sample alongside our preferred specifications that exclude 2020.

Before the crime crackdown, school attendance among teenage boys was 0.725, well below the near-ceiling rate of 0.960 for pre-teens (Table 1). After the crackdown, teen attendance rose to 0.779 while pre-teen attendance barely moved, from 0.960 to 0.973. This widening teen–pre-teen gap is the raw pattern our empirical analysis explains.

Table 1: Summary statistics: school attendance, boys

	Pre-teens (7–12)		Teens (13–18)	
	Pre	Post	Pre	Post
School attendance	0.960	0.973	0.725	0.779
	(0.196)	(0.162)	(0.447)	(0.415)
Age	9.5	9.5	15.6	15.5
	(1.7)	(1.7)	(1.7)	(1.7)
Observations	18,079	8,187	19,353	7,777

Notes: Descriptive statistics for boys using person-quarter observations from the EHPM over the full sample period (2017–2024, including 2020). The pre-treatment period spans 2017q1–2022q1, and the post-treatment period spans 2022q2–2024q4. Entries report sample means, with standard deviations in parentheses. School attendance is a binary indicator. *Source:* Authors' calculations using microdata from El Salvador's Multipurpose Household Survey (EHPM), Oficina Nacional de Estadística y Censos (ONEC), formerly DIGESTYC, 2017–2024.

4.2 Empirical Strategy: Difference-in-Differences

Our identification strategy compares teen boys (ages 13–18), the group most exposed to gang recruitment and victimization, with pre-teen boys (ages 7–12), who lie largely below the recruitment margin, before and after the March 2022 state of exception. The identifying assumption is that, absent the crackdown, school attendance would have evolved similarly for the two age groups. Under this assumption, any differential change in attendance after the crackdown identifies the effect of dismantling gang governance on recruitment-age adolescents. We estimate the following linear probability model:

$$\text{Attends school}_{i,g,t} = \beta_1 \left(\text{Teen}_i \times \text{Post}_t\right) + \delta\, \text{Teen}_i + \gamma_t + \alpha_g + \varepsilon_{i,g,t}, \tag{1}$$

where i indexes individuals, g denotes geography (region or department), and t indexes calendar quarters. The indicator Teen_i equals one for teenagers, who constitute the treatment group, and zero for pre-teens, who serve as the comparison group. The indicator Post_t equals one from 2022q2 onward, the first full quarter after the declaration of the state of exception on 27 March 2022. The terms γ_t and α_g are full sets of year-quarter and geography fixed effects, and δ captures the average attendance gap between the two age groups. Because the quarter fixed effects absorb the main effect of Post_t, the timing of the crackdown enters only through the interaction. The coefficient of interest, β_1, measures the differential change in school attendance among teenagers relative to pre-teens after the crackdown.

Equation (1) maps directly onto Table 2. Column 1 drops the geography fixed effects, so identification comes from the nationwide change in the teen–pre-teen differential around the crackdown. Columns 2 and 3 add α_g at the region and department level, which lets the average level of attendance differ across geographies.[4] Our preferred specifications relax two assumptions the baseline imposes, a common national time path and a common national teen–pre-teen gap, by letting both vary across geography:

$$\text{Attends school}_{i,g,t} = \beta_1 \left(\text{Teen}_i \times \text{Post}_t\right) + \delta_g\, \text{Teen}_i + \gamma_{g,t} + \varepsilon_{i,g,t}, \tag{2}$$

where $\gamma_{g,t}$ denotes quarter $\times$ geography fixed effects, which let each region or department follow its own attendance trajectory, and δ_g denotes teen $\times$ geography fixed effects, which let each carry its own baseline teen–pre-teen gap. Identification then comes from within-geography changes in the teen–pre-teen differential around the onset of the crackdown, with geography defined either at the level of the four survey regions or the fourteen departments. Equation (2) is the specification reported in Table 3. We also estimate an event-study version of both specifications that replaces the Teen $\times$ Post interaction with a full set of Teen $\times$ quarter interactions, which traces the dynamic

[4] The EHPM identifies households' region and department of residence, except in calendar year 2020, when the department identifier is unavailable. Regions are not official governing bodies but help describe the country's geography, climate, and general culture. Departments are official administrative units, each headed by a governor appointed by the central government.

evolution of the effect and lets us assess parallel trends from the pre-treatment coefficients.

In our preferred estimates, we cluster standard errors by age $\times$ geography, using either regions or departments (60 and 168 clusters, respectively). Two features of our design motivate this choice. First, treatment status is a deterministic function of age, which makes age the relevant level of treatment assignment (Abadie et al., 2023). Further, identification across all specifications relies on changes in the teen–pre-teen differential over time, so clustering by age $\times$ geography allows arbitrary correlation among the observations that contribute to the same source of identifying variation. Second, because we pool repeated cross-sections over many quarters, outcomes within a given age-by-geography cell are likely serially correlated, and ignoring that correlation is a well-known source of downward-biased standard errors in difference-in-differences designs (Bertrand et al., 2004). Clustering by age $\times$ geography accommodates the correlation while leaving enough clusters for conventional cluster-robust inference to perform well (Cameron and Miller, 2015). Clustering only by geography would leave too few clusters, and clustering only by age would ignore persistent geographic dependence. Region identifiers appear in every survey wave, so region-based specifications use the full 2017–2024 sample, while department identifiers are unavailable in 2020 and department-based specifications necessarily exclude that year.

5 Results

5.1 Difference-in-differences estimates

The crackdown increased school attendance among teenage boys by approximately 4 percentage points. The estimated coefficient on Teen $\times$ Post ranges from 0.041 to 0.044 across specifications and is highly statistically significant (Table 2). Relative to the pre-treatment attendance rate of 0.725, this represents an increase of roughly 6 percent.

In particular, Columns 1–3 exclude 2020 and progressively introduce geographic fixed effects. Column 1 includes calendar year-quarter and teen fixed effects, so identification comes from the change in the difference in attendance between teenagers and pre-teens before and after the crackdown. Column 2 adds region fixed effects, allowing average attendance to differ across regions, while Column 3 instead adds department fixed effects. The estimated effect remains essentially unchanged, which suggests that the result is not driven by persistent differences in school attendance across geographic areas. Columns 4 and 5 repeat the first two specifications using the full 2017–2024 sample, including 2020, and yield nearly identical estimates. We subject these estimates to more demanding geographic controls in Table 3.

Table 2: Boys, teens vs. pre-teens

	Dependent variable: School attendance				
	Excluding 2020			Full sample	
	(1)	(2)	(3)	(4)	(5)
Teen × Post	0.0443***	0.0443***	0.0431***	0.0416***	0.0417***
	(0.0051)	(0.0059)	(0.0062)	(0.0051)	(0.0062)
Time (year-quarter) FE	Yes	Yes	Yes	Yes	Yes
Teen FE	Yes	Yes	Yes	Yes	Yes
Region FE	-	Yes	-	-	Yes
Department FE	-	-	Yes	-	-
Observations	49,576	49,576	49,576	53,396	53,396

Notes: Difference-in-differences estimates for boys, comparing teenagers (ages 13–18) with pre-teens (ages 7–12). The dependent variable is an indicator for school attendance and the reported coefficient is Teen × Post. Columns 1–3 exclude calendar year 2020, while Columns 4–5 use the full 2017–2024 sample. The specification adds geography progressively: Columns 1 and 4 include quarter and teen fixed effects only; Columns 2 and 5 add region fixed effects; Column 3 adds department fixed effects. Standard errors, reported in parentheses, are clustered by age in Columns 1 and 4, by age × region in Columns 2 and 5, and by age × department in Column 3. The pre-treatment mean among teenage boys is 0.725 in the full sample and 0.724 when excluding 2020. $^{*}\,p < 0.10$, $^{**}\,p < 0.05$, $^{***}\,p < 0.01$. *Source:* Authors' calculations using microdata from El Salvador's Multipurpose Household Survey (EHPM), Oficina Nacional de Estadística y Censos (ONEC), formerly DIGESTYC, 2017–2024.

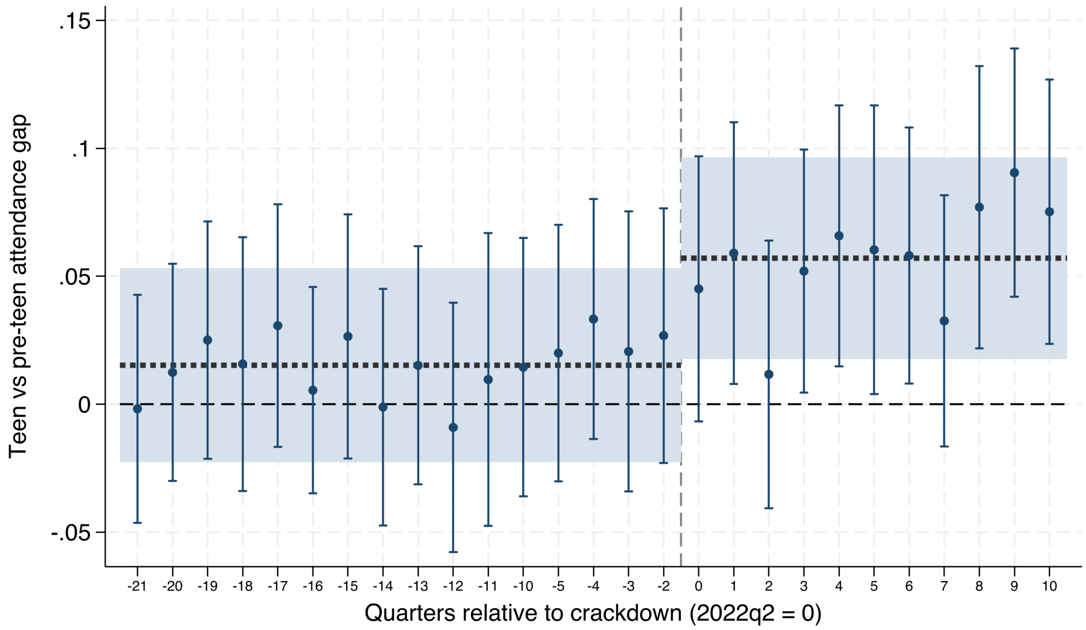
5.2 Event studies

The event study traces the dynamic evolution of the treatment effect over time and provides a visual assessment of parallel trends, by testing whether teenagers and pre-teens exhibited differential attendance trends before the crackdown. Each event study plots the coefficients on the Teen × quarter interactions, omitting the quarter immediately preceding the crackdown (2022q1, event time $t - 1$) as the reference period. The dashed vertical line marks the onset of the crackdown, and whiskers denote 95% confidence intervals. The absence of systematic differences in the pre-treatment coefficients supports parallel trends.

The teen–pre-teen attendance gap is stable before the state of exception and widens only afterward (Figure 3). This event study corresponds to Column 3 of Table 2, which includes time, teen, and department fixed effects, clusters standard errors by age × department, and excludes calendar year 2020.[5] After the onset of the crackdown, the estimated effects emerge immediately and rise steadily throughout 2023 and 2024, consistent with the identifying assumptions behind the design.

[5] The pre-treatment coefficients are flat, fluctuating around 0.015 with no discernible trend, and a joint test of the pre-treatment coefficients cannot reject their equality to zero ($p = 0.87$).

Figure 3: Event study: teen vs. pre-teen boys, excluding 2020



Notes: Event-study estimates using the same specification as Column 3 of Table 2, including time, teen, and department fixed effects, with standard errors clustered by age $\times$ department. Calendar year 2020 is excluded. The omitted reference quarter is 2022q1 (event time $t-1$). Whiskers denote 95% confidence intervals. The dashed horizontal lines indicate the average of the pre-crackdown event-study coefficients (0.015) and the average of the post-crackdown coefficients (0.057); the shaded bands show the corresponding 95% confidence intervals ([−0.023, 0.053] and [0.018, 0.096]). Their difference (0.042) corresponds to the estimate reported in Table 2, Column 3 (0.043). *Source:* Authors' calculations using microdata from El Salvador's Multipurpose Household Survey (EHPM), Oficina Nacional de Estadística y Censos (ONEC), formerly DIGESTYC, 2017–2024.

6 Robustness

6.1 Flexible geographic controls

The estimated effect of the crackdown remains remarkably stable under more demanding controls for geographic heterogeneity (Table 3). In particular, we allow attendance to follow geography-specific time paths and allow the teen–pre-teen attendance gap to differ across geographies. Across all three specifications, the coefficient on Teen $\times$ Post is 0.042 in Column 1, 0.040 in Column 2, and 0.040 in Column 3, all significant at the 1% level and very close to the baseline estimates reported in Table 2.

Specifically, Columns 1 and 2 use the sample excluding 2020, allowing us to implement the robustness checks at both the regional and department levels. Column 1 uses regions, with region $\times$ time fixed effects allowing each region to follow its own attendance trajectory and teen $\times$ region fixed effects allowing each region to have its own baseline teen–pre-teen attendance gap. Column

2 implements the same specification at the department level. We exclude 2020 in both specifications because the 2020 survey wave does not contain department identifiers, which prevents us from constructing the department-level fixed effects. Column 3 therefore returns to the regional specification, allowing us to recover the full 2017–2024 sample, including 2020.

These interacted fixed effects therefore absorb both geography-specific changes in attendance over time and persistent differences in the teen–pre-teen attendance gap across geographies. Identification comes from within-geography changes in the attendance differential between teenagers and pre-teens around the onset of the crackdown. The stability of the estimates across geographic levels and sample definitions indicates that the main result is not driven by differential geographic trends, persistent geographic differences in the teen–pre-teen attendance gap, or the exclusion of the 2020 survey wave.

Table 3: Boys, teens (13–18) vs. pre-teens (7–12): interacted geographic fixed effects

	Dependent variable: School attendance		
	Excluding 2020		Full sample
	(1)	(2)	(3)
Teen × Post	0.0424***	0.0399***	0.0397***
	(0.0054)	(0.0058)	(0.0055)
Time (year-quarter) FE	Yes	Yes	Yes
Teen FE	Yes	Yes	Yes
Region FE	Yes	-	Yes
Department FE	-	Yes	-
Time × region FE	Yes	-	Yes
Teen × region FE	Yes	-	Yes
Time × dept. FE	-	Yes	-
Teen × dept. FE	-	Yes	-
Observations	49,576	49,576	53,396

Notes: Difference-in-differences estimates for boys (teens 13–18 vs. pre-teens 7–12) under interacted geographic fixed effects. The dependent variable is an indicator for school attendance and the reported coefficient is Teen × Post. Each column includes quarter × geography and teen × geography fixed effects, absorbing the geography, time, and teen main effects together with their interactions. Columns 1 and 3 use region; Column 2 uses department. Columns 1–2 exclude calendar year 2020, while Column 3 uses the full 2017–2024 sample; because the 2020 wave carries no department code, a department specification necessarily excludes 2020. Standard errors, reported in parentheses, are clustered by age × region in Columns 1 and 3, and by age × department in Column 2. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$. *Source:* Authors' calculations using microdata from El Salvador's Multipurpose Household Survey (EHPM), Oficina Nacional de Estadística y Censos (ONEC), formerly DIGESTYC, 2017–2024.

6.2 Alternative geographic units and clustering

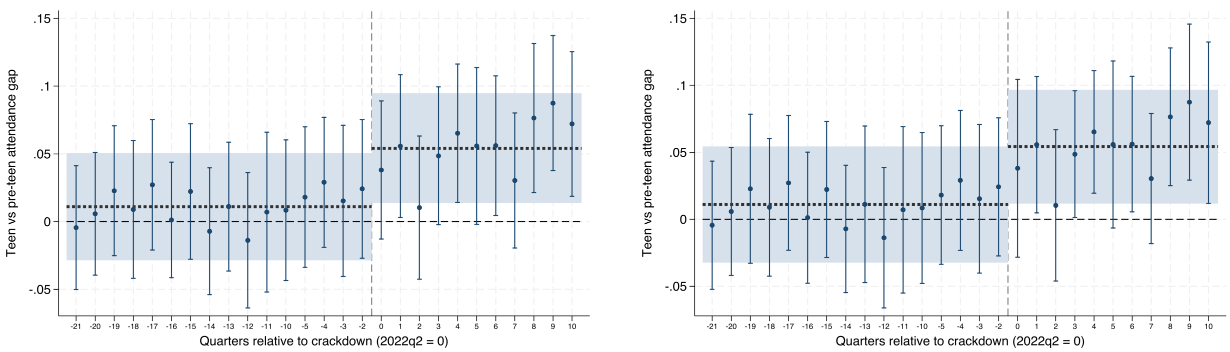
We next examine whether our results depend on the geographic unit used both to absorb geographic heterogeneity and to cluster standard errors. Region is the only geographic identifier available throughout the full sample, because the 2020 survey does not report department of residence.

Once 2020 is excluded, we can estimate specifications using either the country's four regions (60 age × region clusters) or its fourteen departments (168 age × department clusters). To isolate the role of the clustering dimension, we estimate the additive event-study specification, which includes only quarter and teen fixed effects (Figure 4). Because the underlying regression is identical across panels, the estimated coefficients are the same and only the clustering differs, age × department in Panel A and age × region in Panel B.

Figure 4: Event study, additive specification: alternative clustering

Panel A: clustering on age×department

Panel B: clustering on age×region



Notes: Event-study estimates using the 2017-2024 sample, excluding calendar year 2020, and the additive specification reported in Columns 1 and 2 of Table 3 (quarter and teen fixed effects only). The omitted reference quarter is 2022q1 (event time −1). Whiskers denote 95% confidence intervals. Standard errors are clustered by age × department in Panel A and by age × region in Panel B. The dashed horizontal lines indicate the average of the pre-crackdown coefficients (0.011 in both panels) and the average of the post-crackdown coefficients (0.054); the shaded bands show the corresponding 95% confidence intervals (pre: [−0.029, 0.051] in Panel A and [−0.032, 0.054] in Panel B; post: [0.014, 0.095] in Panel A and [0.012, 0.097] in Panel B). *Source:* Authors' calculations using microdata from El Salvador's Multipurpose Household Survey (EHPM), Oficina Nacional de Estadística y Censos (ONEC), formerly DIGESTYC, 2017–2024.

The event-study estimates show flat pre-treatment coefficients under both clustering schemes, and the post-crackdown coefficients rise steadily after the onset of the crackdown, averaging about 0.054. Inference is therefore insensitive to whether we cluster by age × department or age × region, and the absence of differential pre-trends survives even in this parsimonious specification without geographic controls. Replacing region with department fixed effects likewise leaves the estimated treatment effect essentially unchanged, since the department-interacted specification yields 0.040 (Table 3, Column 2), very close to its region-based counterpart. Overall, our findings are robust to both the choice of geographic unit and the level at which we cluster.

6.3 Placebo tests

Placebo tests probe the design from other directions, and a significant placebo would reveal that the estimator manufactures differences the crackdown story cannot explain. We run two. The first uses a younger group that the change in crime policy should not have affected. The second imposes different timings for the start of the crackdown.

The teen-versus-pre-teen design attributes the rise in attendance to the crackdown reducing the recruitment and victimization risks faced by boys of recruitment age but not by younger children. A natural concern is that the estimate instead reflects a smooth age profile in attendance, with older children following a different trajectory over time for reasons unrelated to the crackdown. We therefore apply the same difference-in-differences design entirely within the pre-teen population, splitting pre-teens, who lie below the recruitment margin, into a placebo treatment group (ages 10–12) and a placebo comparison group (ages 7–9). Because both groups are less exposed to recruitment, a meaningful treatment effect should be absent, and a significant estimate would instead indicate that the design mechanically generates differences between adjacent age groups. The placebo estimates are small (0.005–0.006), statistically insignificant, and stable across samples and fixed-effects specifications (Table 4). The design does not produce spurious effects among adjacent age groups that are similarly unexposed to recruitment, which supports our reading that the main estimates capture the consequences of dismantling gang governance rather than age-specific attendance trends.

Table 4: Placebo age cutoff within the pre-teens: boys 10–12 vs. 7–9

	Dependent variable: School attendance			
	Excl. 2020		Full sample	
	(1)	(2)	(3)	(4)
Placebo 10–12 × Post	0.0056	0.0053	0.0064	0.0058
	(0.0053)	(0.0044)	(0.0053)	(0.0045)
Time (year-quarter) FE	Yes		Yes	
Group FE	Yes		Yes	
Time × region FE		Yes		Yes
Group × region FE		Yes		Yes
Observations	24,361	24,361	26,266	26,266

Notes: Placebo difference-in-differences estimates for boys aged 7-12, where the placebo treatment group consists of boys aged 10–12 and the comparison group of boys aged 7–9. The post-treatment period begins in 2022q2. Columns 1 and 2 exclude calendar year 2020, while Columns 3 and 4 use the full sample (2017–2024). Columns 1 and 3 include additive quarter and group fixed effects. Columns 2 and 4 include quarter × region and group × region fixed effects. The dependent variable is an indicator for school attendance. Standard errors, reported in parentheses, are clustered by age × region. $^{*}\, p < 0.10$, $^{**}\, p < 0.05$, $^{***}\, p < 0.01$. *Source:* Authors' calculations using microdata from El Salvador's Multipurpose Household Survey (EHPM), Oficina Nacional de Estadística y Censos (ONEC), formerly DIGESTYC, 2017–2024.

The second placebo preserves the true age comparison but assigns the crackdown to fictitious dates in periods when the actual policy was not yet in place. Because treatment status is constant throughout these windows, any estimated effect would reflect differential trends between teenagers and pre-teens rather than the crackdown itself. If the teen–pre-teen gap had already been widening before 2022, imposing an arbitrary policy date would generate a spurious effect. A valid design should instead produce estimates close to zero whenever no policy change occurred.

We re-estimate the teen-versus-pre-teen difference-in-differences using a placebo intervention in 2018q3 on the pre-COVID sample (2017q1–2019q4) and a second placebo in 2021q2 on the pre-crackdown sample (through 2022q1, excluding 2020). Both placebo estimates are small and statistically insignificant, ranging from -0.005 to 0.009, in sharp contrast to the large and significant effect at the true intervention date of 2022q2 (Table 5). The increase in teen attendance therefore emerges only when the crackdown actually occurs, which is further evidence against a pre-existing divergence in attendance trends between teenagers and pre-teens.

Table 5: Placebo-in-time: fake crackdown dates, boys

	Dependent variable: School attendance			
	Fake date 2018q3		Fake date 2021q2	
	(1)	(2)	(3)	(4)
Teen × Post (placebo)	-0.0057	-0.0052	0.0094	0.0075
	(0.0095)	(0.0094)	(0.0109)	(0.0104)
Time (year-quarter) FE	Yes		Yes	
Teen FE	Yes		Yes	
Time × region FE		Yes		Yes
Teen × region FE		Yes		Yes
Observations	25,288	25,288	33,612	33,612

Notes: Placebo difference-in-differences estimates for boys comparing teenagers (ages 13–18) with pre-teens (ages 7–12). Columns 1–2 assign a placebo crackdown date of 2018q3 and are estimated using the pre-COVID sample (2017q1–2019q4). Columns 3–4 assign a placebo crackdown date of 2021q2 and are estimated using the pre-crackdown sample (through 2022q1), excluding calendar year 2020. Columns 1 and 3 include additive quarter and teen fixed effects. Columns 2 and 4 include quarter × region and teen × region fixed effects. The dependent variable is an indicator for school attendance. Standard errors, reported in parentheses, are clustered by age × region.
$^{*}\, p < 0.10$, $^{**}\, p < 0.05$, $^{***}\, p < 0.01$. *Source:* Authors' calculations using microdata from El Salvador's Multipurpose Household Survey (EHPM), Oficina Nacional de Estadística y Censos (ONEC), formerly DIGESTYC, 2017–2024.

Together, these falsification tests address the two most natural alternatives to the security interpretation: that the estimate reflects a mechanical age gradient, or a teen–pre-teen gap that was already diverging before the crackdown. Neither alternative survives.

6.4 Prior-period difference-in-differences

As a final robustness check, we estimate the effect of the crackdown without using pre-teens as a comparison group. We instead compare teenage boys observed during the crackdown with teenage boys observed over the same calendar quarters in an earlier period. The design follows the year-over-year approach of Leslie and Wilson (2020), who study the effects of COVID-19 social distancing, a nationwide shock affecting all cities simultaneously, by comparing outcomes in 2020 with the corresponding calendar weeks of 2019. That design uses the immediately preceding period as the counterfactual and differences out seasonality.

We stack two 12-quarter windows: a treatment window (2021–2023) spanning the March 2022 crackdown, and a reference window (2017–2019). We re-index quarters as $q = 1, \ldots, 12$ within each window so that each quarter in the treatment window is matched to the corresponding calendar quarter in the reference window. We estimate the following specification, following Leslie and Wilson (2020):

$$\text{Attends school}_{i,g,q,p} = \beta_0 + \beta_1 \,\text{Policy}_{q,p} + a_g + \gamma_q + \nu_p + \varepsilon_{i,g,q,p}, \tag{3}$$

where $\text{Policy}_{q,p}$ equals one for the post-crackdown quarters of the treatment window (2022q2–2023q4), a_g denotes geographic fixed effects (region, with department used as a robustness check), γ_q denotes re-indexed quarter fixed effects, and ν_p is a window fixed effect. A separate post-period indicator is perfectly collinear with the quarter fixed effects and is therefore absorbed, as in Leslie and Wilson (2020). The coefficient of interest, β_1, compares school attendance during the post-crackdown period with attendance in the corresponding calendar quarters of the reference window, net of seasonality and any level differences between the two windows. The estimation sample consists of boys aged 13–18 observed in the 2017–2019 and 2021–2023 survey waves. As in the main analysis, calendar year 2020 is excluded because of the COVID-19 disruption, while 2024 is omitted because it has no counterpart in the reference window. Standard errors are clustered by age $\times$ region.

Identification in specification (3) requires that, absent the crackdown, school attendance among teenage boys would have evolved similarly across the 2021–2023 and 2017–2019 windows. This is a stronger assumption than the within-period parallel-trends assumption behind our main design, so we view the exercise as corroborative. The exercise is valuable because it eliminates the need for a pre-teen comparison group: if our baseline estimates were driven by unusual changes among pre-teens rather than improvements in teen attendance, this alternative design would not reproduce them.

Teen school attendance rises by 0.048–0.050 following the crackdown, about 6.8 percent of the pre-crackdown attendance rate of 0.725 (Table 6). The estimated effect is stable across alternative geographic units (region versus department), across additive and interacted fixed-effects specifications, and across functional forms, since the logit specification in Column 5 yields a similar positive and significant effect. The magnitude is also consistent with our baseline. A total increase in teen attendance of roughly 5 percentage points, combined with the small increase among pre-teens (Table 1), implies a teen–pre-teen difference close to the 4-percentage-point estimate in Table 2.

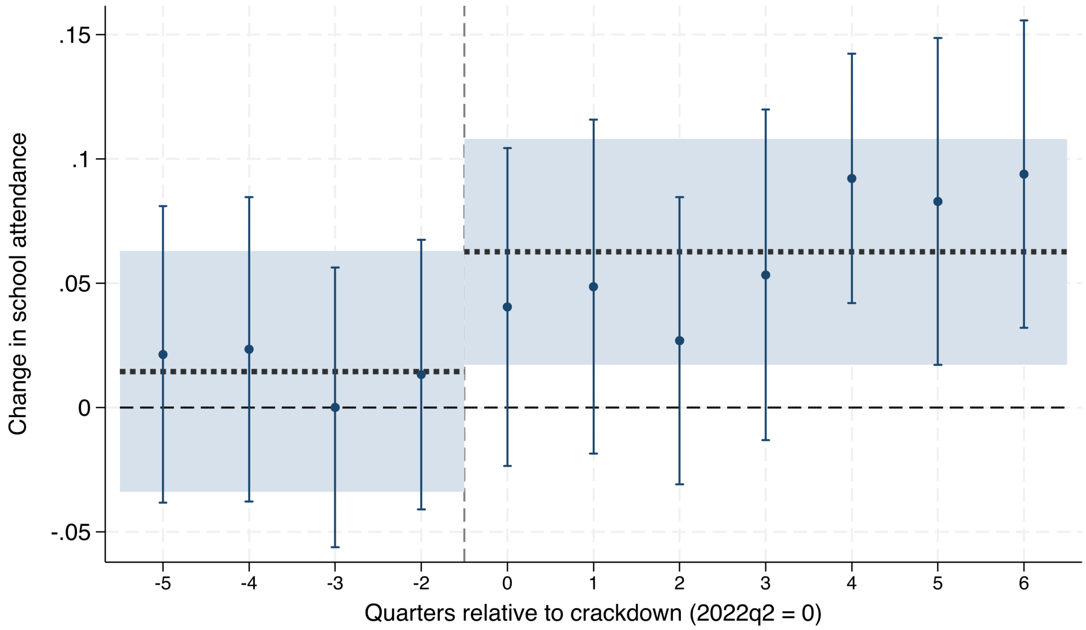
Table 6: Prior-period DiD: teen boys (13–18)

	Dependent variable: School attendance				
	(1)	(2)	(3)	(4)	(5)
	LPM	LPM	LPM	LPM	Logit
Crackdown policy	0.0495***	0.0504***	0.0480***	0.0495***	0.2626***
	(0.0121)	(0.0120)	(0.0123)	(0.0124)	(0.0632)
Region FE	Yes				Yes
Department FE			Yes		
Quarter FE	Yes		Yes		Yes
Window FE	Yes		Yes		Yes
Region × quarter FE		Yes			
Region × window FE		Yes			
Dept. × quarter FE				Yes	
Dept. × window FE				Yes	
Observations	22,409	22,409	22,409	22,409	22,409

Notes: Difference-in-differences estimates using stacked 12-quarter windows for boys aged 13–18. The reference window covers 2017–2019, and the treatment window covers 2021–2023; calendar years 2020 and 2024 are excluded. Quarters are re-indexed from 1 to 12 within each window. The indicator Crackdown policy equals one from 2022q2 through 2023q4 in the treatment window. Columns 1 and 4 report linear probability models, while Column 5 reports the corresponding logit coefficient (log-odds). The dependent variable is an indicator for school attendance. The pre-treatment mean among teenage boys in the treatment window (2021q1–2022q1) is 0.725. Standard errors, reported in parentheses, are clustered by age × region (30 clusters). $^{*}\,p < 0.10$, $^{**}\,p < 0.05$, $^{***}\,p < 0.01$. *Source:* Authors' calculations using microdata from El Salvador's Multipurpose Household Survey (EHPM), Oficina Nacional de Estadística y Censos (ONEC), formerly DIGESTYC, 2017–2024.

The event-study version replaces the policy indicator with a full set of event-time indicators (quarters -5 to 6 of the treatment window), omitting quarter $t-1$ as the reference period (Figure 5). The pre-crackdown coefficients show no systematic trend, which indicates that teen attendance in 2021 and early 2022 closely tracked its counterpart in 2017–2019. After the crackdown the estimated effects rise steadily, reaching approximately 0.09 six quarters later and averaging 0.063 over the post-crackdown period (95% confidence interval $[0.017, 0.108]$).

Figure 5: Prior-period event study: teen boys (13–18)



Notes: Event-study estimates for boys aged 13–18 using the same stacked-window specification as Table 6, including region, re-indexed quarter, and window fixed effects, with standard errors clustered by age × region. The omitted reference quarter is 2022q1 (event time $t-1$). Whiskers denote 95% confidence intervals. The dashed horizontal lines indicate the average of the pre-crackdown coefficients (0.015) and the average of the post-crackdown coefficients (0.063); the shaded bands show the corresponding 95% confidence intervals ([−0.034, 0.063] and [0.017, 0.108], respectively). *Source:* Authors' calculations using microdata from El Salvador's Multipurpose Household Survey (EHPM), Oficina Nacional de Estadística y Censos (ONEC), formerly DIGESTYC, 2017–2024.

7 Heterogeneity and mechanisms

Which teenagers and which households in El Salvador saw the largest increases in school attendance? The preceding analysis estimates an average treatment effect: the crackdown raised attendance among teenage boys by about four percentage points. We next examine how that effect varies across households, using triple-difference specifications that let the teen–pre-teen effect vary with household characteristics.[6] Because some of these characteristics may themselves respond to the crackdown, we read the results as suggestive rather than causal. Interacting a treatment with subgroup characteristics identifies where the effect concentrates but recovers the underlying mechanism only under additional assumptions (Kwon and Roth, 2026; Baker et al., 2025).

We organize the discussion around two dimensions, household structure and economic disadvantage (Table 7); Appendix C reports how common each characteristic is in the two age groups. We do

[6]We regress school attendance on Teen×Post, Post×X, Teen×X, and the triple interaction Teen×Post×X, including quarter and teen fixed effects and clustering standard errors by age×department.

not observe whether a household experienced the incarceration of an adult member, but households lacking a resident parent may have been more vulnerable to such shocks, or to the loss of adult support more generally. If the crackdown disproportionately removed adult household members through incarceration, these households would face tighter budgets and weaker parental support for schooling, which should reduce adolescents' educational investment at the margin. Columns 1 and 2 provide no evidence of such heterogeneity. The estimated treatment effect does not differ according to whether the household head is the boy's father or mother, or whether the boy's father resides in the household.

Table 7: Mechanisms: heterogeneity by household characteristic

	Dependent variable: School attendance			
	Head not parent (1)	Father present (2)	Poor (household) (3)	Rural (4)
Teen × Post	0.0432***	0.0457***	0.0296***	0.0367***
	(0.0068)	(0.0100)	(0.0070)	(0.0075)
Teen × Post × X (DDD)	0.0016	0.0022	0.0419***	0.0068
	(0.0151)	(0.0123)	(0.0141)	(0.0131)
Observations	49,333	45,207	49,576	49,576

Notes: Triple-difference estimates. Each column interacts Teen×Post with the household characteristic indicated in the column. The dependent variable is an indicator for school attendance. All specifications include quarter and teen fixed effects. The characteristics are: the household head is neither the boy's father nor mother (column 1); the boy's father is a co-resident household member (column 2); the household is classified as poor, in extreme or moderate poverty (column 3); and the household is located in a rural area (column 4). Standard errors, clustered by age×department (168 clusters), are reported in parentheses. $^{*}\,p < 0.10$, $^{**}\,p < 0.05$, $^{***}\,p < 0.01$. *Source:* Authors' calculations using microdata from El Salvador's Multipurpose Household Survey (EHPM), Oficina Nacional de Estadística y Censos (ONEC), formerly DIGESTYC, 2017–2024.

We next turn to economic disadvantage, which requires particular caution because contemporaneous household poverty may itself respond to the crackdown if incarcerated members had previously contributed to household income. Standard models of household investment in education predict that negative income shocks reduce educational investment. We find the opposite. Teenagers in poor households gained substantially more than those in non-poor households, a differential of about four percentage points that is significant at the one-percent level (Column 3). One reading is that poorer households faced the tightest gang-related constraints before the crackdown and therefore benefited most when the costs and risks of sending adolescents to school fell. The reduction in gang governance appears to have more than offset any adverse income effects from the incarceration of household members.

We also ask whether the schooling response differs between urban and rural households. Because gangs were historically concentrated in urban areas, the benefits of the crackdown might be larger there. Lower expected returns to formal education in rural labor markets could work the other way and generate a smaller response, so the prediction is ambiguous. Column 4 provides no evidence that rural residence modifies the treatment effect. Overall, the evidence points to economic disadvantage, rather than household structure or rural residence, as the principal source of heterogeneity in the schooling response.

Put together, these patterns point to a consistent mechanism. The response was largest precisely where the constraints imposed by gang governance were likely strongest before the crackdown, in the poorest households. This is consistent with Becker's framework, in which families choose educational investments by weighing expected benefits against expected costs, including the risks that crime imposes. By reducing those risks, the crackdown appears to have raised the expected return to schooling most where gang governance had previously bound hardest. The larger gains among poorer households are difficult to reconcile with a pure household-income mechanism, under which negative income shocks from incarceration would have reduced schooling. The evidence fits better with an interpretation in which the reduction in gang-related constraints dominated any adverse income effects and relaxed the day-to-day frictions that had kept adolescents out of school.

8 Conclusions

El Salvador's 2022 state of exception dismantled, within a matter of weeks, the system of gang governance that had shaped daily life across much of the country for decades. We examined how this transformation affected schooling by exploiting the sharp age gradient in exposure to gang recruitment, comparing teenage boys, the group most exposed to the recruitment and victimization the crackdown disrupted, with pre-teen boys, who lay largely below that margin. School attendance among teenage boys rose by about 4 percentage points relative to pre-teens. The effect appeared immediately at the onset of the crackdown and grew over the following two years, and it proved robust across placebo tests, alternative comparison groups, and a range of sensitivity analyses.

These findings suggest that criminal governance imposes substantial educational costs that extend beyond the direct effects of violence. Our results complement a large literature on crime, conflict, and human capital. Where previous work shows schooling declining as gangs and violence expand, we find that attendance rises when criminal governance is abruptly dismantled. The evidence is consistent with gang governance imposing substantial frictions on schooling decisions that fall disproportionately on adolescents at recruitment age. Removing those frictions appears to have relaxed an important constraint on educational investment. The effect emerges precisely among recruitment-age boys and immediately after the crackdown begins, which supports that reading. That the same design does not yield a credible estimate for girls, whose attendance was already trending upward before the policy, reinforces this interpretation.

The gains were also far from uniform, and their distribution points to the same interpretation. Teenagers in poorer households responded most, even though the incarceration of household members should, through an income channel, have pushed the other way. We attribute this pattern to the everyday grip of gang governance over adolescents' schooling, a constraint the crackdown relaxed, rather than to the threat of lethal violence alone.

Several caveats qualify these conclusions. First, we measure a contemporaneous behavioral response, school attendance while the policy is in force, rather than long-run educational attainment. Whether these gains translate into additional years of completed schooling and improved learning outcomes remains an open question. Second, our estimate identifies the effect of the crackdown on recruitment-age adolescents relative to pre-teens, and to the extent that some pre-teens were also exposed to gang recruitment, our estimates are likely attenuated toward zero. Finally, the state of exception entailed substantial costs in civil liberties and due process that lie outside the scope of our analysis, so our findings should not be read as an endorsement of any particular security policy. They show that criminal governance imposed important educational costs on Salvadoran adolescents and that dismantling it relaxed an important barrier to school attendance. More broadly, the results suggest that reducing criminal control over communities may be an important precondition for human-capital accumulation in high-crime environments.

Declaration of Generative AI and AI-assisted Technologies in the Manuscript Preparation Process

During the preparation of this work the authors used Claude in order to improve the language and readability of the manuscript. After using this tool, the authors reviewed and edited the content as needed and take full responsibility for the content of the published article.

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Appendix

A Crackdown and girls' school attendance

Although boys were the primary targets of gang recruitment, gangs also reached adolescent girls through other channels. Girls faced sexual harassment and violence, coercive relationships with gang members, restrictions on mobility, and the broader insecurity of living under gang control. These forms of criminal governance could alter the expected costs and returns to schooling and, in turn, household educational investment, so a positive schooling response to the crackdown is plausible. This appendix applies the paper's age-based difference-in-differences design to girls.

The estimated effect is large and statistically significant. The identifying assumptions, however, are not supported for girls, because school attendance among teenage girls was already rising before the crackdown, which prevents a causal reading of the estimate. We report the estimates together with the evidence that shows why they should not be interpreted as causal, and we discuss plausible sources of the pre-existing trend.

Table A1: Summary statistics: school attendance, girls

	Pre-teens (7–12)		Teens (13–18)	
	Pre	Post	Pre	Post
School attendance	0.964	0.981	0.733	0.800
	(0.186)	(0.136)	(0.443)	(0.400)
Age	9.5	9.5	15.6	15.5
	(1.7)	(1.7)	(1.7)	(1.7)
Observations	16,952	7,639	19,103	7,583

Notes: Descriptive statistics for girls using person-quarter observations from the EHPM over the full sample period (2017–2024, including 2020). The pre-treatment period spans 2017q1–2022q1, and the post-treatment period spans 2022q2–2024q4. Entries report sample means, with standard deviations in parentheses. School attendance is a binary indicator variable. *Source:* Authors' calculations using microdata from El Salvador's Multipurpose Household Survey (EHPM), Oficina Nacional de Estadística y Censos (ONEC), formerly DIGESTYC, 2017–2024.

Attendance among pre-teen girls is already very high and rises only modestly, from 0.964 before the crackdown to 0.981 afterward, while attendance among teenage girls is substantially lower but rises more sharply, from 0.733 to 0.800 (Table A1). The raw before–after increase is therefore 0.067 for teens against 0.017 for pre-teens, a difference-in-differences of about 5 percentage points, larger than the corresponding 4-percentage-point estimate for boys. Taken at face value, these patterns would suggest an even stronger effect of the crackdown on girls' schooling. The rest of this appendix explains why that reading is not warranted.

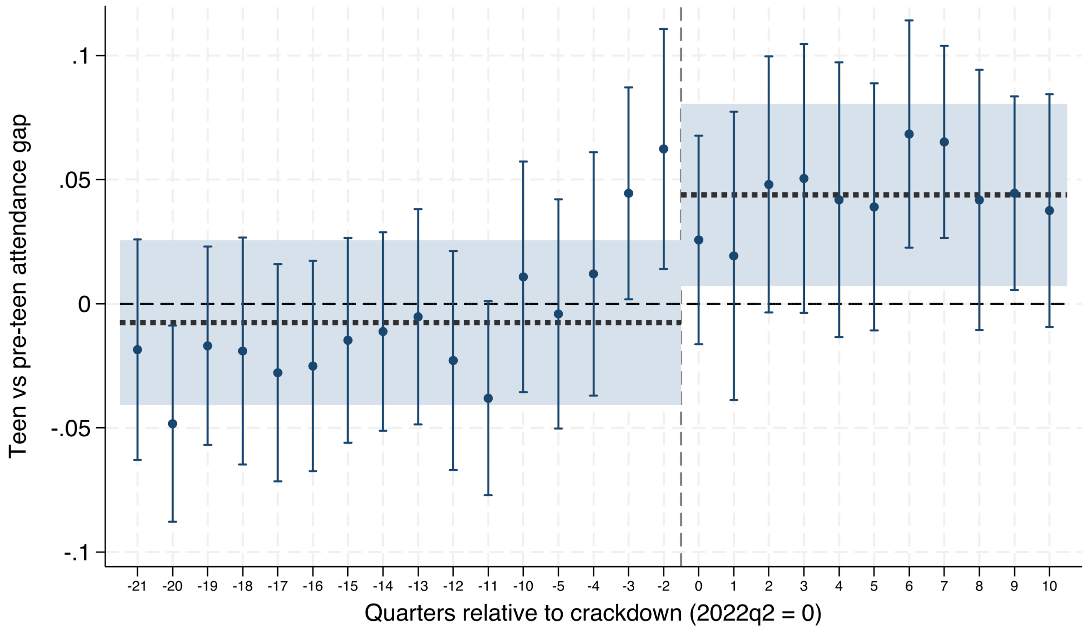
The estimated DiD coefficients (Tables A2 and A3) are, if anything, larger than those for boys, but the identifying assumptions are not supported. Three pieces of evidence point to this conclusion. First, attendance among teenage girls trends clearly upward throughout the pre-treatment period, unlike the essentially flat pre-treatment series for teenage boys in Figure 2. Second, the event-study estimates show steadily rising lead coefficients even after excluding 2020, rather than the flat pre-treatment profile we observe for boys (Figure A1). Third, the placebo-in-time exercise (Table A5) estimates a spurious "crackdown effect" of 0.049–0.051 when the policy is artificially assigned to 2021q2, virtually identical to the estimate at the true intervention date, whereas the corresponding placebo for boys is economically and statistically indistinguishable from zero (Table 5). Even the 2018q3 placebo yields a positive and marginally significant estimate under the interacted specification, so the trend predates the crackdown by several years. Put together, this evidence indicates that the widening attendance gap among girls reflects a pre-existing trend rather than the 2022 crackdown.

Table A2: Girls, teens (13–18) vs. pre-teens (7–12): Regions

	Dependent variable: School attendance			
	Full sample		Excl. 2020	
	(1)	(2)	(3)	(4)
Teen × Post	0.0504***	0.0478***	0.0548***	0.0526***
	(0.0067)	(0.0061)	(0.0071)	(0.0063)
Time (year-quarter) FE	Yes		Yes	
Teen FE	Yes		Yes	
Time × region FE		Yes		Yes
Teen × region FE		Yes		Yes
Observations	51,277	51,277	47,458	47,458

Notes: Difference-in-differences estimates for girls using the region-level specification. The specifications correspond to those reported in Table 2. Columns 1–2 use the full sample (2017–2024), while Columns 3–4 exclude calendar year 2020. Columns 1 and 3 include additive quarter and teen fixed effects. Columns 2 and 4 include quarter × region and teen × region fixed effects. The dependent variable is an indicator for school attendance. The pre-treatment mean among teenage girls is 0.733 in the full sample and 0.730 when excluding 2020. Standard errors, reported in parentheses, are clustered by age × region (60 clusters).
$^{*}\,p < 0.10$, $^{**}\,p < 0.05$, $^{***}\,p < 0.01$. *Source:* Authors' calculations using microdata from El Salvador's Multipurpose Household Survey (EHPM), Oficina Nacional de Estadística y Censos (ONEC), formerly DIGESTYC, 2017–2024.

Figure A1: Event study: teen vs. pre-teen girls, excluding 2020



Notes: Event-study estimates for girls using the same specification as Figure 3, including quarter × region and teen × region fixed effects, with standard errors clustered by age × region. Calendar year 2020 is excluded. The omitted reference quarter is 2022q1 (event time −1). Whiskers denote 95% confidence intervals. The dashed horizontal lines indicate the average of the pre-crackdown coefficients (−0.008) and the average of the post-crackdown coefficients (0.044); the shaded bands show the corresponding 95% confidence intervals ([−0.041, 0.026] and [0.007, 0.081], respectively). *Source:* Authors' calculations using microdata from El Salvador's Multipurpose Household Survey (EHPM), Oficina Nacional de Estadística y Censos (ONEC), formerly DIGESTYC, 2017–2024.

Table A3: Girls, teens (13–18) vs. pre-teens (7–12): Departments and regions

	Dependent variable: School attendance			
	(1)	(2)	(3)	(4)
	Dept.	Region	Dept.	Region
Teen × Post	0.0548***	0.0548***	0.0517***	0.0526***
	(0.0062)	(0.0071)	(0.0058)	(0.0063)
Time (year-quarter) FE	Yes	Yes		
Teen FE	Yes	Yes		
Time × dept. FE			Yes	
Teen × dept. FE			Yes	
Time × region FE				Yes
Teen × region FE				Yes
Observations	47,458	47,458	47,458	47,458

Notes: Difference-in-differences estimates for girls comparing department- and region-level specifications using the 2017–2024 sample, excluding calendar year 2020. The dependent variable is an indicator for school attendance. The specifications correspond to those reported in Table 3. Columns 1–2 report the additive specification, including quarter and teen fixed effects only. Columns 3–4 report the interacted specification, including quarter × geography and teen × geography fixed effects, where geography is defined at the department or region level, respectively. Standard errors, reported in parentheses, are clustered by age × department in columns 1 and 3 (168 clusters) and by age × region in columns 2 and 4 (60 clusters). The pre-treatment mean among teenage girls is 0.730. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$. *Source:* Authors' calculations using microdata from El Salvador's Multipurpose Household Survey (EHPM), Oficina Nacional de Estadística y Censos (ONEC), formerly DIGESTYC, 2017–2024.

Table A4: Placebo age cutoff within the pre-teens: girls 10–12 vs. 7–9

	Dependent variable: School attendance			
	Excl. 2020		Full sample	
	(1)	(2)	(3)	(4)
Placebo 10–12 × Post	0.0042	0.0042	0.0026	0.0022
	(0.0049)	(0.0041)	(0.0050)	(0.0043)
Time (year-quarter) FE	Yes		Yes	
Group FE	Yes		Yes	
Time × region FE		Yes		Yes
Group × region FE		Yes		Yes
Observations	22,770	22,770	24,591	24,591

Notes: Placebo difference-in-differences estimates for girls aged 7–12, where the placebo treatment group consists of girls aged 10–12 and the comparison group of girls aged 7–9. The dependent variable is an indicator for school attendance. The specifications correspond to those reported in Table 4. The post-treatment period begins in 2022q2. Columns 1–2 exclude calendar year 2020, while Columns 3–4 use the full sample (2017–2024). Columns 1 and 3 include additive quarter and group fixed effects. Columns 2 and 4 include quarter × region and group × region fixed effects. Standard errors, reported in parentheses, are clustered by age × region. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$. *Source:* Authors' calculations using microdata from El Salvador's Multipurpose Household Survey (EHPM), Oficina Nacional de Estadística y Censos (ONEC), formerly DIGESTYC, 2017–2024.

Table A5: Placebo-in-time: fake crackdown dates, girls

	Dependent variable: School attendance			
	Fake date 2018q3		Fake date 2021q2	
	(1)	(2)	(3)	(4)
Teen × Post (placebo)	0.0115	0.0125*	0.0510***	0.0493***
	(0.0082)	(0.0070)	(0.0102)	(0.0091)
Time (year-quarter) FE	Yes		Yes	
Teen FE	Yes		Yes	
Time × region FE		Yes		Yes
Teen × region FE		Yes		Yes
Observations	24,361	24,361	32,236	32,236

Notes: Placebo difference-in-differences estimates for girls comparing teenagers (ages 13–18) with pre-teens (ages 7–12). The specifications correspond to those reported in Table 5. Columns 1–2 assign a placebo crackdown date of 2018q3 and are estimated using the pre-COVID sample (2017q1–2019q4). Columns 3–4 assign a placebo crackdown date of 2021q2 and are estimated using the pre-crackdown sample (through 2022q1), excluding calendar year 2020. Columns 1 and 3 include additive quarter and teen fixed effects. Columns 2 and 4 include quarter × region and teen × region fixed effects. The dependent variable is an indicator for school attendance. Standard errors, reported in parentheses, are clustered by age × region. Unlike the corresponding placebo estimates for boys (Table 5), the placebo intervention in 2021q2 produces a large and statistically significant estimated "effect," consistent with the upward pre-treatment trend in teenage girls' school attendance. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. *Source:* Authors' calculations using microdata from El Salvador's Multipurpose Household Survey (EHPM), Oficina Nacional de Estadística y Censos (ONEC), formerly DIGESTYC, 2017–2024.

B Honest DiD sensitivity analysis

The event studies and placebo tests in the main text support the parallel-trends assumption our design requires. The pre-crackdown coefficients are flat and jointly indistinguishable from zero (Figure 3), and assigning the crackdown to fictitious dates yields estimates near zero (Table 5). This appendix asks how large a violation of that assumption the estimated effect could tolerate. We apply the relative-magnitudes version of the Rambachan–Roth sensitivity analysis to two event studies estimated on the region-interacted specification, clustered by age×region: the full 2017–2024 period, and the full period dropping calendar year 2020 (Table A6). The exercise expresses the permitted post-period violation as a multiple $\bar{M}$ of the largest pre-period violation, and asks how large that multiple can be before the average post-crackdown effect is no longer distinguishable from zero. The target parameter is the average of the post-period event-study coefficients (11 quarters, 2022q2–2024q4), and we scan $\bar{M}$ over a grid from 0 to 1 in steps of 0.05. The breakdown $\bar{M}$ is the first grid point at which the robust confidence interval includes zero. Figures A2 and A3 plot the robust confidence sets for the two variants.

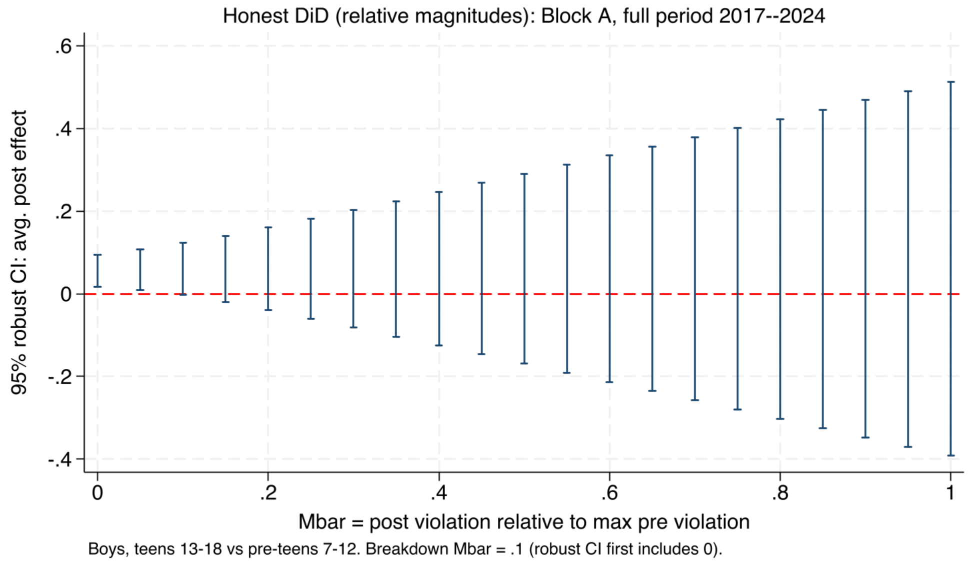
Table A6: Honest DiD (relative magnitudes)

	Original 95% CI	Breakdown $\bar{M}$	Robust CI, $\bar{M} = 0.5$
Full period 2017–2024	[0.016, 0.094]	0.10	[-0.169, 0.291]
No 2020	[0.016, 0.094]	0.15	[-0.127, 0.276]

Notes: Boys only, teens 13–18 vs. pre-teens 7–12. Target parameter: average of the 11 post-period event-study coefficients (2022q2–2024q4). "Original" is the unrestricted 95% CI; "Breakdown $\bar{M}$" is the smallest $\bar{M}$ on the 0.05 grid at which the robust CI includes zero. Time×region and teen×region FE, standard errors clustered on age×region. *Source:* Authors' calculations using microdata from El Salvador's Multipurpose Household Survey (EHPM), Oficina Nacional de Estadística y Censos (ONEC), formerly DIGESTYC, 2017–2024.

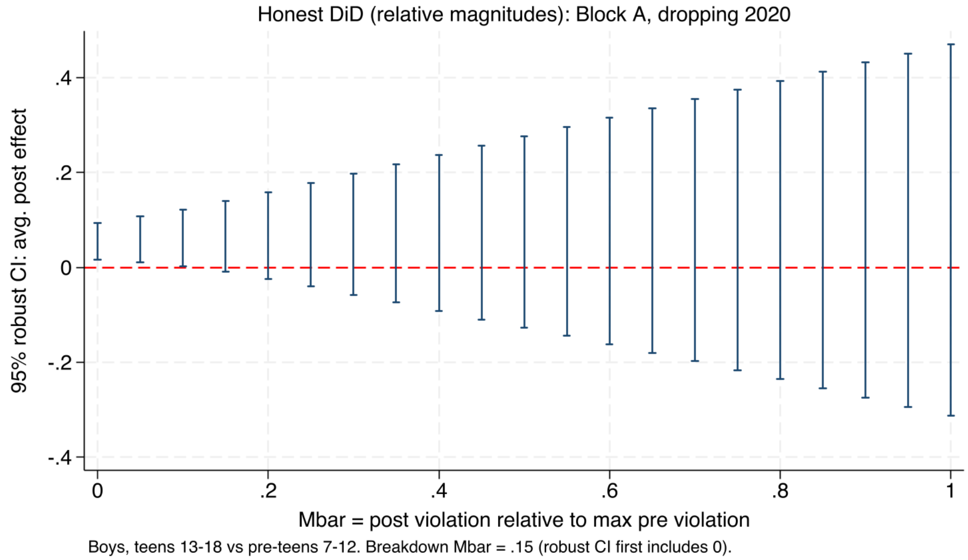
The unrestricted average post effect is about 0.055 (95% CI [0.016, 0.094]) in both variants. The breakdown $\bar{M}$ is 0.10 in the full-period variant (Figure A2), unchanged when only the -6 quarter is dropped, and 0.15 once 2020 is excluded (Figure A3), so the positive effect survives post-period trend violations of up to 10–15% of the largest pre-period violation. The small breakdown values reflect the relative-magnitudes benchmark itself. In the full-period variant the largest pre-period violation is the pandemic collapse, so even a small $\bar{M}$ permits a large absolute violation; excluding 2020 shrinks the benchmark to the largest ordinary pre-period jump and the breakdown rises. The mild upward drift of the 2017–2019 leads documented in the event studies keeps the no-2020 benchmark from being smaller still. Because $\bar{M}$ is expressed relative to the largest pre-period violation, a small breakdown value does not by itself indicate a fragile estimate. It follows from pre-period violations that are themselves small, which is what the flat leads in Figure 3 and the placebo-in-time estimates in Table 5 document. We therefore read this exercise as consistent with parallel trends rather than as evidence against them.

Figure A2: Honest DiD sensitivity: full period 2017–2024



Notes: Robust 95% confidence sets for the average post-period effect as a function of $\bar{M}$ (relative-magnitudes restriction); "Original" is the unrestricted CI. The breakdown value is the first $\bar{M}$ whose confidence set includes zero. *Source:* Authors' calculations using microdata from El Salvador's Multipurpose Household Survey (EHPM), Oficina Nacional de Estadística y Censos (ONEC), formerly DIGESTYC, 2017–2024.

Figure A3: Honest DiD sensitivity: dropping year 2020



Notes: Same construction as Figure A2, dropping calendar year 2020 from the underlying event study.
Source: Authors' calculations using microdata from El Salvador's Multipurpose Household Survey (EHPM), Oficina Nacional de Estadística y Censos (ONEC), formerly DIGESTYC, 2017–2024.

Repeating the exercise for two alternative fixed-effects choices leaves the results essentially unchanged (Table A7). The first replaces the region interactions with department interactions (time×department and teen×department fixed effects, clustered by age×department), which necessarily excludes 2020 because that wave lacks the department code. The second removes the geographic interactions altogether, leaving the additive specification with time and teen fixed effects only, estimated on the full period and excluding 2020. The unrestricted average post effect is about 0.05 in all three variants and the breakdown $\bar{M}$ is 0.10 throughout, the same value as the full-period region-interacted benchmark and slightly below the 0.15 obtained there once 2020 is excluded. The sensitivity conclusion therefore does not hinge on how, or whether, geography enters the specification.

Table A7: Honest DiD (relative magnitudes): alternative specifications

	Original 95% CI	Breakdown $\bar{M}$	Robust CI, $\bar{M} = 0.5$
Additive FE, full period 2017–2024	[0.013, 0.096]	0.10	[-0.199, 0.312]
Additive FE, no 2020	[0.013, 0.096]	0.10	[-0.139, 0.283]
Dept.-interacted FE, no 2020	[0.013, 0.087]	0.10	[-0.147, 0.258]

Notes: Boys only, teens 13–18 vs. pre-teens 7–12. Same exercise as Table A6: target parameter = average of the 11 post-period event-study coefficients (2022q2–2024q4); "Original" is the unrestricted 95% CI; "Breakdown $\bar{M}$" is the smallest $\bar{M}$ on the 0.05 grid at which the robust CI includes zero. Additive rows: time and teen fixed effects only (no geographic control), standard errors clustered on age×region. Dept.-interacted row: time×department and teen×department fixed effects, clustered on age×department.
Source: Authors' calculations using microdata from El Salvador's Multipurpose Household Survey (EHPM), Oficina Nacional de Estadística y Censos (ONEC), formerly DIGESTYC, 2017–2024.

C Crime and household characteristics

This appendix reports two sets of descriptive statistics referenced in the main text: the department-level counterparts to the national crime series in Figure 1, and the household characteristics used as moderators in Table 7.

The homicide rate falls in every department across the four periods, from a national monthly average of 4.0 per 100,000 in 2018–2019 to 0.1 from 2024 onward, while detentions per 100,000 rise sharply during the crackdown, from a national monthly average of 46 in the year before to 93, and then ease (Table A8). The decline in homicides is broad-based rather than concentrated in a few departments.

Table A8: Homicides and detentions per 100,000, by department and period

	2018–2019		2021–2022m2		2022m3–2023		2024 onward	
Department	Homicides	Detentions	Homicides	Detentions	Homicides	Detentions	Homicides	Detentions
AHUACHAPAN	3.9	75.8	1.4	50.3	0.5	86.4	0.1	42.7
CABAÑAS	3.8	50.2	1.6	35.6	0.5	100.5	0.1	35.4
CHALATENANGO	2.7	48.5	0.9	39.1	0.4	88.4	0.2	39.8
CUSCATLAN	3.7	40.3	1.5	39.9	0.2	81.2	0.2	32.8
LA LIBERTAD	3.6	39.6	1.5	30.6	0.5	73.8	0.1	41.6
LA PAZ	4.9	45.6	1.6	42.0	0.3	84.7	0.1	50.6
LA UNION	3.3	54.9	1.4	43.1	0.4	67.8	0.2	43.6
MORAZAN	4.3	58.8	2.0	41.0	0.3	77.4	0.1	29.8
SAN MIGUEL	5.3	45.2	1.1	46.1	0.4	78.0	0.2	45.8
SAN SALVADOR	4.3	58.1	1.8	44.9	0.3	101.4	0.1	54.0
SAN VICENTE	3.3	81.1	1.2	53.8	0.6	101.2	0.1	76.9
SANTA ANA	3.4	76.9	1.5	64.8	0.5	109.1	0.1	65.5
SONSONATE	3.9	56.0	1.9	53.0	0.4	126.3	0.1	67.6
USULUTAN	4.7	64.7	1.6	51.2	0.3	93.1	0.2	32.1
National	4.0	56.4	1.6	45.6	0.4	93.3	0.1	49.6

Notes: Average monthly homicides and detentions per 100,000 inhabitants, by department, over four periods: 2018–2019; January 2021–February 2022 (before the 27 March 2022 state of exception); March 2022–December 2023 (the crackdown); and 2024 onward (through 2025m8). Calendar year 2020 is omitted. The national row aggregates counts across departments before forming rates, matching Figure 1. *Source:* Administrative records from El Salvador's National Civil Police (PNC), at the department $\times$ month level.

All four household characteristics used as moderators in Table 7 are common in both age groups and slightly more prevalent among pre-teens (Table A9). Teens are somewhat less likely to live in a poor household (0.36 versus 0.42) or with a co-resident father (0.56 versus 0.61).

Table A9: Household characteristics, by age group

	Pre-teens (7–12)	Teens (13–18)
Household head not a parent	0.234	0.207
Father co-resident	0.607	0.559
Poor household	0.415	0.356
Rural	0.516	0.495
Observations	24,361	25,215

Notes: Sample shares (means of binary indicators) among boys aged 7–18, 2017–2024 excluding 2020, separately for pre-teens (7–12) and teens (13–18). Each share is computed over the variable's own non-missing observations; the co-resident-father and household-head measures require the household roster and cover fewer observations. These are the moderators used in Table 7. *Source:* Authors' calculations using microdata from El Salvador's Multipurpose Household Survey (EHPM), Oficina Nacional de Estadística y Censos (ONEC), formerly DIGESTYC, 2017–2024.