Breaking the Gang: A Preventive Approach to Reduce Recruitment in Schools

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Abstract

In this paper we study an alternative approach to reduce the expansion of criminal organizations: preventing gang recruitment at schools, where criminal organizations commonly target children. To do so, we exploit the staggered implementation of a preventive program in El Salvador that increased anti-gang recruitment campaigns by police patrolling at schools. We show that by focusing on schools, the use of police officers as a preventive measure can reduce gang recruitment at an early age, having positive long-term effects on children's human capital and potentially reducing gang's revenues. In particular, we show that children at key ages for recruitment are significantly less likely to drop out of school and join gangs. Using administrative data, we find that immediately after the start of the program the gang recruitment declined by 50% and that children are less likely to be detained in juvenile centers due to gang-related crimes. In turn, these outcomes reduce the likelihood of children becoming involved in a cycle of crime within organized crime networks. We find that affected children are less likely to be incarcerated for gang-related crimes in adulthood. As a result of this decline, we find evidence that gangs' main business, extortion, is affected in the long run. By addressing a root cause of future criminality, this policy effectively limits gang expansion and decreases revenue streams, as they have less labor to perform their activities.

1 Introduction

Criminal organizations pose a growing concern in developing countries, responsible for a significant portion of global illicit activities and associated violence (United Nations Office on Drugs and Crime 2023).[1] One common approach to reducing organized crime—massive incarceration—can inadvertently increase the strength of these groups behind bars (Lessing and Willis 2019), escalate violence toward civilians (Freedman et al. 2022), and harm communities affected by gang activity (Clear 2009; Gupta et al. 2022). Additionally, successful preventive approaches, such as redressing young individuals' behavior (Heller et al. 2016; Blattman et al. 2017), may be less effective in territories where juveniles' choice is limited due to a high control of criminal organizations. Therefore, we still lack an understanding of how to effectively address this issue, especially in the context of organized crime.

In this paper, we examine an alternative approach aimed at reducing the expansion of criminal organizations: increasing the cost of recruitment at schools, which are frequent targets for recruitment by these organizations. Research has recently suggested that impeding recruitment at young ages is the most effective way to break the criminal cycle, as it disrupts the dynamic regeneration of criminal capital (Prieto-Curiel et al. 2023; Sviatschi 2022a,b). Children are often targeted by gangs, enabling them to expand their criminal activities and compensate for losses due to incapacitation measures (Prieto-Curiel et al. 2023). This issue is particularly prevalent in developing countries and adversely affects human capital accumulation. In many countries, schools in gang territories often serve as primary recruitment grounds, increasing the likelihood of children dropping out and entering organized crime (International Labour Organization 2018; Melnikov et al. 2022).

Our study focuses on the Safe Schools program (Escuelas Seguras) in El Salvador, implemented in 2015. Between 2010 and 2014, 219 students were killed en route to or from school, many due to their resistance to gang recruitment. In response, the Ministry of Justice and Public Security (Ministerio de Justicia y Seguridad Pública, MJSP), in collaboration with the Ministry of Education (Ministerio de Educación, MINED), initiated this preventive program, assigning teams of three to five police officers to patrol high-risk schools during entry and exit hours. The intervention aimed to enhance student safety by reducing gang harassment at and around schools during peak recruitment times. A committee of experts selected 355 schools for the program based on a crime index, as well as the committee's own criteria. In the initial phase in 2015, 128 schools in 11 municipalities were successfully intervened, but

[1] According to the United Nations Office on Drugs and Crime (2023), between 2015 and 2021, close to 700,000 deaths were attributed to organized crime, a figure comparable to the casualties from armed conflicts within that time frame.

227 pre-selected schools in 24 municipalities missed the planned intervention in 2018 due to changes in government.

This setting provides three useful sources of variation for our analysis: i) school exposure to the program based on the crime index and the fact that a subset of planned schools was eventually not treated, ii) time variation in the program's implementation, and iii) variation in the age of exposure, exploiting the fact that gangs recruit in early adolescence (Cruz et al. 2017; Sviatschi 2022b). Therefore, to assess the effect of the Safe Schools program, we adopt a difference-in-differences (DID) approach (following Arkhangelsky et al. 2021).

Our novel geocoded data set combines school dropout, gang recruitment, and adult incarceration data at the individual level, derived from administrative school, police, incarceration, intelligence unit, and child protection services records. These data allow us to track student dropouts in all schools and observe changes in gang recruitment before and after the program's implementation. In particular, we take advantage of administrative data on gang recruitment from the National Intelligence Unit and Social Services, which collect data through informants in the field on the location and time an individual starts being involved with a gang. Moreover, we can observe whether children from patrolled schools are less likely to be involved in crime than adults. We also complement these data with victimization records of extortion to analyze how gang business is affected.

Our first main finding is that preventive police patrolling around schools significantly reduces school dropouts, gang recruitment, and juvenile detentions. We find that approximately 14 fewer students drop out of school, representing a 50% decrease compared to the mean of the control schools. Analysis of administrative data on recruits indicates that this reduction in school dropouts is accompanied by a reduction in recruitment, which declines by a similar margin of about 50% a year following the program's initiation, paralleling the dropout trends. Furthermore, analysis of data from Child Protection Services reveals a decrease in juvenile detentions associated with gangs for those under 18 after the program, providing further evidence of a decline in the engagement of children in gangs. Notably, the largest decline in school dropout occurs in the age range that is most commonly targeted for gang recruitment.

Our analysis indicates that the decline in recruitment and dropout at an early age has positive long-term effects on future criminal behavior, ultimately reducing the primary activities of gangs. Specifically, using incarceration records, we find that children who were at key ages of recruitment during the program's implementation in treated schools are less likely to be incarcerated for gang-related crimes (extortion and homicides) in their adult years, and are less likely to be affiliated with a gang. Consequently, we find a significant

decline in extortion activities in neighborhoods surrounding the treated schools three years after the program. These findings suggest that by constraining the recruitment labor pool for gangs, their capacity to engage in illicit activities is affected.

We find no evidence to suggest that gangs strategically increase recruitment in non-treated schools, supporting qualitative evidence that gang control and influence are highly localized (CITE).[2] On the contrary, we observe evidence of positive spillovers from nearby non-treated schools (referred to as "contaminated"). Specifically, these schools, albeit to a lesser extent, experience benefits similar to those in the Safe Schools program: reduced dropout rates, decreased recruitment, and diminished future involvement in criminal activities. These positive impacts are mainly observed in schools located within 1km of treated schools, indicating a geographic influence on the program's effectiveness.

We rule out alternative explanations for our findings. For instance, the effects are not mechanically driven by a lower incapacitation effect, as we do not observe changes in incarceration rates among individuals who were beyond school age at the time of the program. Moreover, we find no effects for individuals that were at school ages but not at key ages of recruitment. Another potential concern is that the presence of police officers at schools may lead to increased confrontations with gangs or civilians. However, we find no evidence of an increase in police-related violence in schools that participated in the program. This result is consistent with the preventive nature of the program and the selection of police officers with impeccable records of service.

Our paper makes three key contributions. First, it adds value to the existing literature on juvenile crime prevention and school dropout rates (Heller et al. 2016; Cook and Kang 2016; Eren et al. 2017; Bell et al. 2016, 2022; Sviatschi 2022a; Melnikov et al. 2022). We complement this literature by introducing an effective approach where children face constrained choices in deciding whether to join a gang, particularly in a context of forced recruitment and persistent harassment by dominant gangs. Our findings demonstrate the efficacy of police patrols around schools in disrupting interactions between students and gang members during school entry and exit. The intervention significantly reduces the likelihood of children joining gangs, which consequently lowers school dropout rates and diminishes their involvement in gang-related criminal activities during adulthood.

Second, we contribute to the vast literature studying crime reduction policies and programs (Di Tella and Schargrodsky 2004, 2013; Heller et al. 2016; Cook and Kang 2016; Bell et al. 2016; Blattman et al. 2017; Eren et al. 2017; Sanfelice 2019; Gonzalez and Komisarow

[2]One of the advantages of gang territorial control is the recruitment of children (Melnikov et al. 2022). Therefore, gang leaders tend to recruit locally and it may be difficult for them to recruit in other territories where they are not familiar and where another gang member may have control.

2020; Bell et al. 2022; Sviatschi 2022a; Melnikov et al. 2022; Tobón 2022; Prieto-Curiel et al. 2023; Fenizia and Saggio 2023). Most of the evidence has focused on developed countries and individual-motivated crimes (with the exception of Blattman et al. (2017, 2021)). We complement this literature by studying a policy to reduce the expansion of criminal organizations and associated violence. Our findings show that disrupting recruitment channels for organized criminal groups has a short and long-term impact on criminal involvement, potentially affecting their economic activities through reducing their labor force. We posit that this effect is particularly pronounced for criminal organizations whose influence is highly localized within certain territories and who are unable to easily replace their lost recruitment sources.

Finally, we contribute to the literature on how the use of police force as a crime deterrent without escalating police violence. Recent studies have shown that strategies like mass incarceration can lead to a significant increase in police violence (Freedman et al. 2022), adversely impacting disadvantaged communities (Gupta et al. 2022). Dube et al. (2021) show that psychological therapy aimed at reducing automatic responses in police officers can reduce such incidents. In our research, we present evidence that a police force can be employed as a preventive measure in strategic locations and times, effectively reducing crime without contributing to a rise in police violence.

The rest of the paper is organized as follows. Section 2 presents the setting and intervention, and Section 3 describes the data. Section 4 presents the empirical strategy. Section 5 presents the results, Section 6 discusses aggregate effects, and Section 7 discusses alternative mechanisms. Section 8 concludes.

2 Background

2.1 Gang Recruitment, Juvenile Crime, and Dropout

In the past few decades, El Salvador has grappled with increasing crime rates, mostly perpetrated by notorious gang groups such as MS-13 and Barrio 18. The criminal activities of these gangs mainly include extortion and related homicides. Throughout the 2000s, successive governments adopted a hardline approach, encapsulated in policies dubbed "Mano Dura" and "Super Mano Dura." While these strategies hinged on zero tolerance toward illicit groups and their affiliates, they failed to achieve a substantial reduction in crime levels. In a dramatic shift in 2012, the government attempted an unprecedented approach by mediating a ceasefire between rival gangs. Orchestrated through communication among imprisoned gang leaders, the initiative sought to reduce crime rates from inter-gang conflicts. Despite

initial success in curbing homicides, the truce lacked enduring efficacy, unraveling after 2013 and triggering a surge in crime rates. By 2015, El Salvador's homicide rate had soared to 103 per 100,000 inhabitants, one of the highest in the world. However, the prevalence of crime varied significantly across different age groups, with the youngest 15- to 29-year-old demographic being the most affected by violence in the country.

In El Salvador, many young individuals grow up in environments where interactions with gangs are frequent. Gangs often influence youths from an early age, pressuring them to join their ranks. Sviatschi (2022b) finds that individuals who were in primary school when gangs arrived from the United States are about 20% more likely to be incarcerated for gang-related crimes as adults. Cruz et al. (2017) highlight that nearly 60% of gang members join these groups before turning 16, and an alarming 94% fail to complete high school. This is highly concerning considering that as of 2015, 40% of educational institutions in El Salvador were located in gang-active areas, with 16% of school principals reporting student involvement in criminal networks (Ministerio de Educación 2015).

Gangs recruit children for various roles, including acting as community "antennas" to monitor residents' activities and to manage extortion and drug transactions. Children are viewed as valuable assets for the gangs as they are not likely to be prosecuted in adult courts and thus return to the streets within a short time (Cox et al. 2021). Consequently, gangs often exploit the socioeconomic disadvantages and vulnerabilities present children's home environments as a means of recruitment (Portillo 2003; Savenije 2011; Smutt and Miranda 1998; Cruz et al. 2017; Klahr 2006).

Additionally, gangs use death threats to coerce young people into joining, often leading to school dropouts and, in extreme cases, emigration. As reported by MINED in 2015, about 20% of school dropouts were linked to criminal-related activities and migration. Sviatschi (2022b) finds that minors deported at the U.S. border predominantly hail from municipalities with high crime rates and significant gang activity. This may mirror the dropout rates in El Salvador, where only 3 out of every 10 students finish their schooling, in contrast to the regional average in Latin America of 5 out of 10 (Castro and Zoido 2018).

2.2 The Role of Schools in Gang Recruitment

Schools in disadvantaged neighborhoods are prime targets for gangs seeking new recruits. These institutions are filled with young individuals who fit the ideal profile for potential gang members in terms of age and socioeconomic background. In El Salvador, public schools have been widely recognized as key sites for gangs to exert control over territories and expand their activities, with recruitment being a central strategy (López Ramírez and Ramírez

2015).[3] On the one hand, when gangs control a school, they can prevent access to students from rival gangs' territories, potentially weakening their rivals' influence. On the other hand, students who are recruited act as representatives of the gangs in their own communities, increasing the gang's potential to expand. In both scenarios, recruitment plays a crucial role for gangs as it increases their control and the likelihood of expansion, both in the short and long term (López Ramírez and Ramírez 2015; Prieto-Curiel et al. 2023).

In this context, qualitative evidence suggests that gangs often harass and actively recruit students outside school premises during their commute to and from school. This time of day is notably risky for students in gang neighborhoods, where parents often restrict outdoor time to minimize gang exposure (López Ramírez and Ramírez 2015). In this setting, recruitment can be coercive, involving threats and physical violence to pressure students into joining.[4] For instance, police records indicate that from 2010 to 2014, 219 students were killed, all while commuting to or from school (Ministerio de Educación 2015). Furthermore, various press articles have connected some of these homicides to gangs punishing students for refusing to join (Ayala y 15; López e 27). Owing to this pervasive issue, school dropout rates related to gangs in El Salvador are alarmingly high, with 30,367 students leaving school due to gang harassment and crime between 2009 and 2013 (Ministerio de Educación 2015).

2.3 The Safe School Program

To address the escalating security concerns, in 2015 the Salvadoran government collaborated with international and local stakeholders—including the United Nations Program and civil society organizations—to develop a multi-faceted security plan. One of the main pillars of this plan was the the Safe School program, jointly implemented by the National Civil Police (Policía National Civil, PNC) and MINED. The program's primary objective was to prevent gang recruitment in and around schools by incorporating two main components:

1. **Perimeter Security:** PNC officers were deployed to patrol schools (teams of three to five police officers), notably during arrival or departure times, to prevent potential gang recruitment. Importantly, to ensure student safety, the guidelines for police officers focused on prevention.

[3] An important aspect of the relationship between gangs and schools in El Salvador is that students do not typically create gangs to combat external groups from other schools or neighborhoods. Instead, gangs in El Salvador are sophisticated gangs that view schools as strategic locations for their economic activities.

[4] An important distinction here is that the coercive nature of recruitment has surged through the increase of territorial control by gangs. For example, Sviatschi (2022b) presents evidence of how gangs initially used persuasive strategies (e.g., social status, protection, etc.) in their beginnings when the gang leaders were initially deported to El Salvador.

2. **Preventive Activities:** While patrolling, targeted information campaigns were carried out to educate students about the risks of gang involvement and the role of the preventive police. These campaigns also took place directly in classrooms twice per year, where police officers gave presentations to promote awareness about the police patrolling program and negative impacts of joining gangs. The primary aim of these campaigns was to deter students from engaging in gang activities and to inform them about the program.

The program aimed to prevent gang-student interactions during school hours and commutes. Analysis of geolocated patrolling data from 2018 and 2019 (Figure A1 and Table A2) show that in treated schools, patrolling occurred mostly around school entrance and exit times. This observation, in line with our qualitative evidence, indicates that the program's objective was to clear the area of gangs when students were most likely to be present. The initiative mostly entailed adjusting the patrolling schedule and locations of the existing police force rather than increasing the number of officers. This is evidenced by the stable police presence at stations near treated and control schools post-implementation, as displayed in Panel A of Figure 7.

Building on the program's objective to curb gang-student interactions during school hours and commutes, we further delineate its preventive aspects. First, as part of a broader government strategy, the initiative targeted gang reduction in El Salvador using preventative measures (CNSC 2015). Second, the presence of police in schools was specifically intended to protect students from being harassed by gangs. This approach included not only protective measures but also educational and community-building activities, such as informing students about the risks of joining gangs and strengthening community-police ties (see pictures in Figure A2). Consistently, as shown in the lower panels of Figure 7, we find no evidence of a differential increase in police violence in selected schools after the program started. This suggests that the heightened patrolling did not lead to more confrontations between the police, gang members, or civilians in patrolling (we further discuss these results in the next sections).

The selection of schools for the program was a collaborative effort between the PNC and MINED. It was primarily based on a prioritization index that contains information about crime around schools, including gang presence, robberies, drug trafficking, rapes, weapon use, and school size (enrollment). The program's rollout was structured in phases, initially incorporating 117 schools in 2015, followed by 121 in the second phase in 2017, and an intended 260 in the third phase. Table A8 outlines the selection process, showing that the treated schools are larger and have a higher crime index than never-treated schools. While

these differences do not affect our research design, which relies on parallel trends, for robustness, we control for baseline characteristics interacted with time fixed effects.

The implementation of the program was closely coordinated with local governments. However, the local elections in 2018 and the national election in 2019 in El Salvador led to shifts in political power, which impacted the planned rollout of the program (Valencia 2019). In particular, the third phase was never implemented, while the implementation of the second phase was disrupted during the governmental transition. Thus, in our analysis, we exclude schools from the second phase and instead use schools from the third phase as our control group.

3 Data

3.1 Educational Records

The data for our study were obtained from MINED, which provided yearly administrative records of all students who were in any school in El Salvador from 2010 to 2019. Each student's record includes a unique identifier, their grade and school, grade completion status, and basic demographic details. From 2014 to 2017, MINED conducted tests in a sample of schools to assess students' social-emotional skills, specifically focusing on self-reliance and anger management.

Our main outcome of interest is the "dropout" metric, which tracks the progress of students through the educational system across years. Specifically, a student is categorized as a dropout in year t if they are reported to be in the education system of El Salvador in year $t-1$ but is not in year t. Schools are responsible for reporting if any student is currently in the school in that specific academic year, using their unique identifier. We take the individual-level data and collapse them at the school grade level. Since we have geocoordinates for all the schools, this allows us to merge them with fine-grained public security and crime data.

Our main measure of dropout has the advantage over more traditional measures by observing not only if a student drops out of a certain school but also by allowing us to see if they drop out from the entire education system. As a robustness check, we also use a different measure derived from the school census conducted by MINED every year at the beginning and end of each academic year. This alternative measure calculates dropout in year t as the difference between enrollment at the end of the academic year and the beginning of it, providing a measure of dropout within the academic year.

3.2 Program Data

The MJSP supplied data on the first and second phases of the program's implementation. For each year, they provided a list of municipalities, police sectors (groups of neighborhoods), and schools where interventions occurred. Our research primarily focuses on these intervened schools, where the PNC, in collaboration with the MJSP, increased patrols during school hours to deter gang activity and recruitment.

To quantify the patrolling efforts, we also acquired geocoded and time-stamped data on patrol events by PNC officers from 2018 to 2019. We consider a patrol event as school targeted if it occurred within 300 meters of the school's geocoordinates.

3.3 Crime and Incarceration Records

From the PNC, we obtained detailed data on major gang-related crimes from 2012 to 2019. These data include the date and time of each extortion and homicide event, the precise geocoordinates, victim demographics, and whether the incident was gang related. Using these data, we create a monthly count of all crime events in the vicinity of each school if the incident occurred within 300 meters of the school's location.

Additionally, we used incarceration data provided by the General Directorate of Prisons (Dirección General de Centros Penales, DGCP). The DGCP supplied us with the date of incarceration, comprehensive demographic information, and addresses, which we geocoded. Incarcerations were associated with a school if the location of the incident was within 300 meters of the school. Furthermore, we collected municipal-level data on youth detentions from the Salvadoran Institute for Children and Youth (Instituto Salvadoreño de la Niñez y Juventud, ISNA). These data contain general demographic information, as well as crime and date of arrest, for all youth incarcerated in the country from 2010 to 2020. Finally, we also collected count gang recruitment and profiling data–akin to "know and suspected" gang member records—from the PNC, which compiles this information mainly through field informants.

Table A1 presents summary statistics of the main variables used in our analysis.

4 Empirical Strategy

4.1 Synthetic DID

To assess the effect of the Safe Schools program on dropout rates, recruitment, and future involvement in criminal activities, we adopt a synthetic DID (SDID) approach, as suggested

by Arkhangelsky et al. (2021). Thus, in our analysis we compare, before and after 2015 (the program's inaugural year), schools selected in the program's first phase with those designated for the third phase that was not implemented. The use of the SDID estimator is at least as robust as the DID estimator, outperforming it in situations where parallel trends do not hold (e.g., in scenarios resembling those highlighted in concerns raised by Roth 2022).

In this approach, for each school i in the control group, the method calculates a weight $\hat{\omega}_i$ such that the pre-trends in the outcome variables are approximately parallel with those of the treated schools. As a result, the average outcome for treated schools is parallel to the weighted average of control schools before the program started. Furthermore, for control schools, the method calculates weights $\hat{\lambda}_t$ for years t that balance pre-exposure years with post-exposure years. Consequently, the average post-treatment outcome for each control school exhibits a constant difference from the weighted average of the pre-treatment outcome. Thus, the SDID estimator $\hat{\beta}^{sdid}$ is calculated as

$$\hat{\beta}^{sdid} = \hat{\delta}_{SS} - \sum_{i=1}^{N_{Con}} \hat{\omega}_i \hat{\delta}_i, \tag{1}$$

where

$$\hat{\delta}_{SS} = \frac{1}{N_{SS}} \sum_{i=N_{Con}+1}^{N} \hat{\delta}_i.$$

$\hat{\delta}_{SS}$ is the outcome average of treated schools, which has constant weights $\frac{1}{N_{SS}}$, and N_{con} is the number of control schools. Furthermore, each control unit incorporates the time weights in the form

$$\hat{\delta}_i = \frac{1}{T_{\text{post}}} \sum_{t=T_{\text{pre}}+1}^{T} Y_{it} - \sum_{t=1}^{T_{\text{pre}}} \hat{\lambda}_t Y_{it},$$

where Y_{it} is our outcome for school i in year t. Having these weights in mind, the SDID $\hat{\beta}^{sdid}$ estimator is estimated using a traditional (but non-parametric) two-way fixed effects model, with a bootstrap variance estimation, where the procedure is clustered at the school level. Consequently, our SDID estimator $\hat{\beta}^{sdid}$ is comparable to the traditional DID estimator $\hat{\beta}^{did}$, usually estimated with the equation

$$Y_{mt} = \alpha_i + \delta_t + \hat{\beta}^{did}(Program_t \times SafeSchools_i) + \epsilon_{it}. \tag{2}$$

The difference is that in this case, the control weights are constant (i.e., $\hat{\omega}_i^{\text{did}} = N_{con}^{-1}$) as well as the time weights ($\hat{\lambda}_t = T_{pre}^{-1}$). In this equation, Y_{it} is an outcome for school i and year t. $Program_t$ is a dummy that indicates if year t is after 2014, and $SafeSchools_i$ is a dummy that

indicates if school i participated in the program. α_i and δ_t are school and year fixed effects, respectively. Therefore, $\hat{\beta}^{did}$ captures the differential effect of the Safe Schools program on outcome Y_{it}.

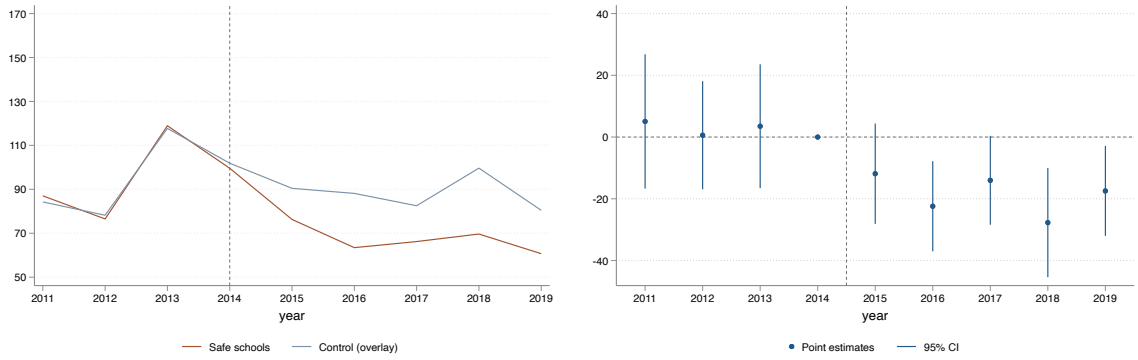
We also present the traditional DID estimators and their unweighted dynamic effect in the robustness section. As a further robustness check, we include baseline characteristics interacted with year and department-year fixed effects to account for relevant covariates in the context of the policy's implementation. For instance, the prioritization index was used to inform the committee that selected the treated schools about the level of crime around schools and other school variables (e.g., school size). Additionally, given the absence of variation within municipalities in our treatment, we use the next administrative unit in size—departments—to flexibly control for common shocks in these units. Finally, we control for whether the schools were urban, considering the higher criminal intensity of gangs in cities and the potential common shocks in these areas.

5 Results

5.1 Impact of the Safe Schools program on dropout

Our analysis reveals a significant differential decrease in dropout rates among treated schools after the program started compared to control schools. Figure 1 displays the dropout estimations for both the treated and control schools for each year of the analysis (following the methodology detailed in Arkhangelsky et al. 2021), with Panel A showing these estimators. Employing the unit weights derived from the SDID estimation, we conduct a weighted event study (WES), represented in Panel B. The results illustrate a noticeable reduction in dropout rates that begins in 2015 and becomes more pronounced in 2016, remaining relatively stable until the conclusion of our analysis in 2019.

Figure 1
Effect of the Safe Schools program on student dropout

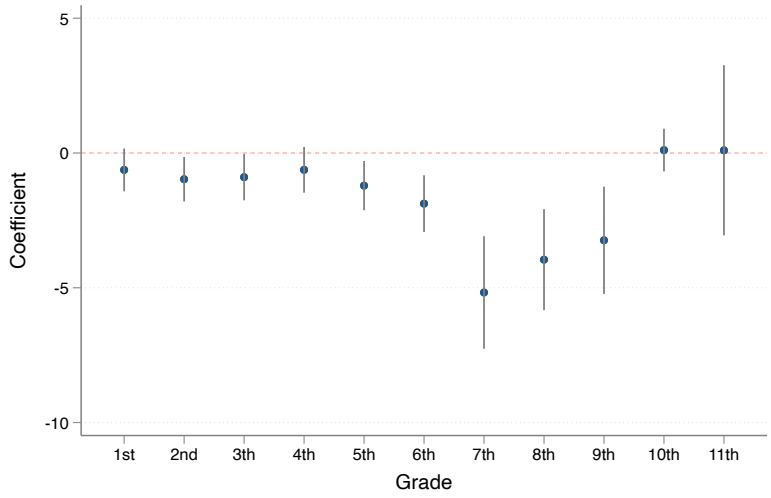


A. SDID **B.** WES

Notes: Panel A presents the SDID estimator graph for dropout rates as proposed by Arkhangelsky et al. (2021). The dotted line marks the year before the start of the Safe Schools program. The red line represents the trend of the dependent variable for treated schools, while the blue line depicts this trend for control schools. To facilitate interpretation, the average difference between the treated and control schools in the pre-program period is added to the control averages. Panel B plots a traditional event study where the control schools are weighted using the unit weights derived from Arkhangelsky et al. (2021).

In Figure 2, we plot the $\hat{\beta}^{sdid}$ for each grade. Interestingly, the figure reveals a pattern consistent with the ages typically identified as more vulnerable to starting a life of crime, as found by Sviatschi (2022a,b). Specifically, the decrease in dropout rates for treated schools appears to be more pronounced in grades 6–9, which, within the Salvador education system, correspond to ages 12–16 years.

Figure 2
Dropout coefficients across cohorts



Notes: This figure presents the SDID estimator graph for dropout rates in each grade and their 95% confidence intervals, as proposed by Arkhangelsky et al. (2021). Therefore, the figure illustrates the SDID estimators and confidence intervals of 12 regressions, using Equation 1. Each point estimate represents the differential difference in dropout rates between the treated and control schools after the program started. For grades 10–11, we exclude schools that do not have those levels.

To better understand the magnitude of the effects, in Table 1 we report our estimators. Approximately 20 fewer students drop out of school, representing a 35% decrease from the mean of the control group in Table 1. Notably, our results suggest that around 14 of these students originate from grades 6–9 (column 3), indicating a percentage decrease of more than 50% relative to the mean of the control schools. The reduction in dropout rates is much lower for both the grades 1–5 and 10–11 cohorts, either in terms of the coefficient (4 and no students, respectively) or in terms of the percentage decrease (a 21% and 2% decrease, respectively). In Figure A6 we report the dynamic effects of these estimations.

We also implement a triple DID (TDID) design to assess the statistical differences in dropout rates between cohorts. Specifically, we compare dropout rates among students in the grades 1–5, 6–9, and 10–11 cohorts across treated and control schools before and after the program started. Table A3 presents the TDID estimators of a fully saturated model, derived from our analysis. In column 1, when compared against the grade 1–5 cohort (the omitted cohort), we find a significant and negative differential effect for the grades 6–9 cohort and a positive and statistically insignificant difference for the grades 10–11 cohort. In column 2, we report similar findings when focusing exclusively on schools that include high school grades (i.e., the grades 10–11 cohort).

As previously mentioned, our dropout results suggest a cohort pattern consistent with

findings in the crime literature. Specifically, the policy's effects appear most pronounced in cohorts more vulnerable to engaging in criminal activity in gangs. We interpret this pattern as suggestive evidence that the policy might be effective in preventing juveniles from beginning a life of crime. This interpretation aligns with the policy's objective, which was to use police patrols around schools to minimize any interaction between gangs and students during their commute. In Figure A1 and Table A2, we observe heightened patrol activity in treated schools around school opening (7:30 am) and closing times (1:00 pm). This pattern suggests that the police patrolled to prevent the presence of gang members around schools at critical times, when students were most vulnerable.

Table 1
Dropout across grades

	(1)	(2)	(3)	(4)
	All	Grade		
		1–5	6–9	10–11
Safe schools	-20.404***	-4.247**	-14.130***	0.407
	(4.992)	(1.758)	(3.020)	(1.724)
Mean dep. var.	57.66	20.12	26.83	17.24
Age range	7-20	7-12	12-16	16-20
Observations	3,195	3,195	3,195	2,178
Schools	355	355	355	242
Safe schools	128	128	128	101

Notes: This table presents the SDID estimators of Equation 1 as proposed by Arkhangelsky et al. (2021). In column 1, the dependent variable is the total number of students who drop out from school i in year t. In columns 2–4, the number of students who drop out from school i in year t is disaggregated based on their enrollment in grades 1–5, 6–9, and 10–11, respectively. Column 4 excludes schools that do not teach grades 10–11. The variable $Safeschools$ is a dummy that indicates whether the school was part of the program. Significance levels are denoted as follows: *** for p<0.01, ** for p<0.05, and * for p<0.1.

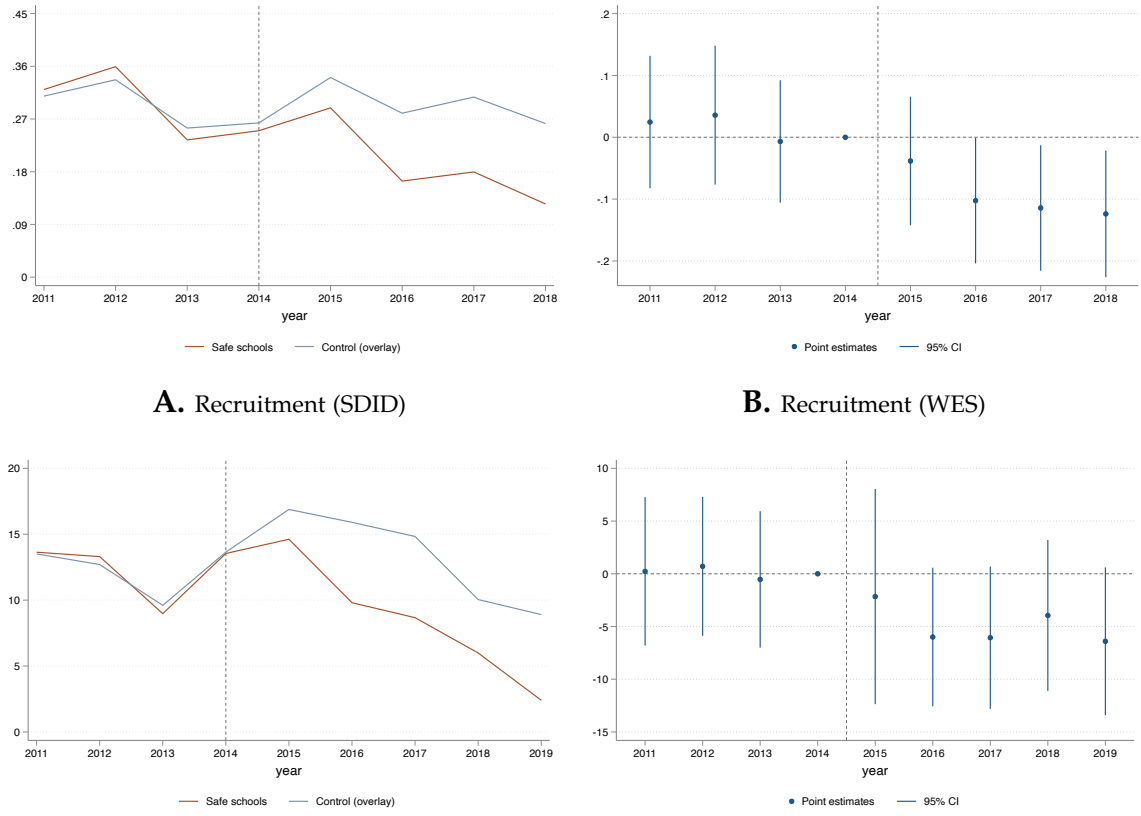
5.2 Recruitment and Teen Crime Involvement

To understand whether the reduction in dropout rates is due to lower gang recruitment, in this subsection we present our results on the Safe Schools program concerning teen recruitment and future criminal activity, using administrative confidential records. As previously explained, our measure of recruitment is obtained from an independent intelligence depart-

ment of the Ministry of Security that registers the entrance of new members into gangs. This department uses geolocation to track the residences of these new members, enabling us to determine those who have recently joined the gangs and live near schools. Additionally, we measure the number of individuals who entered a juvenile criminal center at the municipality level as explained in the data section.

Figure 3 presents the dynamic SDID estimates and the WES in the same manner used for the dropout analysis. Panels A and C show the SDID averages according to Arkhangelsky et al. (2021), while Panels B and D illustrate the WES plots. In Panels A and B, we observe a clear decline in the probability of recruitment in the treated schools starting from 2016, one year after the program's initiation, compared to the control group. Similarly, Panels C and D show a decrease in teen admissions to juvenile centers for gang-related crimes, starting one year following the program's initiation. Consistent with the nature of the policy, admissions to juvenile centers due to non-gang activities did not decrease (see Figure A4).

Figure 3
Effect of the Safe Schools program on recruitment and teenagers criminal activity



A. Recruitment (SDID)

B. Recruitment (WES)

C. Teen admissions, gang related (SDID)

D. Teen admissions, gang related (WES)

Notes: Panels A and C present SDID estimator graphs for the probability of recruitment within a 300-meter radius surrounding schools and the number of teen admissions to juvenile centers at the municipality level for gang-related crimes, as proposed by Arkhangelsky et al. (2021). The dotted line marks the year before the start of the Safe Schools program. The red line represents the trend of the dependent variable for treated schools, while the blue line depicts this trend for control schools. To facilitate interpretation, the average difference between the treated and control schools in the pre-program period is added to the control averages. Panels B and C plot a traditional event study where the control schools are weighted using the unit weights derived from Arkhangelsky et al. (2021).

We present our SDID estimators in Table 2. Column 1 shows that the probability of new recruits decreases by 10 percentage points, representing a 50% percentage change from the control group's mean. Additionally, we find a reduction of 5 teenagers per municipality entering juvenile centers for gang-related offenses after the program's implementation—a 70% percentage decrease. Furthermore, we find an estimate close to zero for teen admissions to juvenile centers for non-gang-related activities in treated schools post-program.

Our findings support the qualitative evidence highlighting the pivotal role of schools in connection with criminal groups. Aligning with the qualitative evidence from El Salvador, which suggests that gangs use schools to bolster their influence, our findings show that im-

plementing policies that curtail gang-student interactions during crucial spaces and times can thwart recruitment efforts. In addition, in environments marked by persistent exposure to criminal groups, or where these groups wield substantial control in the areas near schools, interventions focusing solely on modifying student behavior may have limited efficacy (Muraven and Baumeister 2000; Job et al. 2010; Baumeister et al. 2018). This is due to the constrained agency that children possess within such contexts (López Ramírez and Ramírez 2015). Thus, the Safe Schools program offers an alternative approach to addressing these issues.

Table 2
Recruitment and teen crime involvement

	(1)	(2)	(3)
	New recruits	Teen incarceration	
		Gang related	Non-gang related
Safe schools	-0.107***	-5.000**	-0.005
	(0.026)	(2.208)	(0.460)
Mean dep. var.	0.19	7.30	1.02
Observations	2840	2142	2142
Clusters	355	238	238
Treated clusters	128	11	11

Notes: This table presents the SDID estimators of Equation 1 as proposed by Arkhangelsky et al. (2021). In column 1, the dependent variable is a dummy indicating whether a student living within 300 meters around school i was recruited by gangs in year t. Columns 2 and 3 represent the number of teenagers entering a juvenile center due to gang-related and non-gang-related crimes in municipality i and year t, respectively. In column 1, $Safeschools$ is a dummy indicating whether the school participated in the program, while for columns 2 and 3, it indicates if municipality i had a school that participated in the program. Significance levels are denoted as follows: *** for p<0.01, ** for p<0.05, and * for p<0.1.

5.3 Future Criminal Involvement

Was the policy sufficiently effective in deterring affected cohorts from engaging in criminal activities as adults? To investigate this, we use incarceration data. However, this approach presents certain challenges. First, due to data constraints, we cannot link students impacted by the policy to their criminal records. Second, incarcerated individuals must be 18 years or older, a cohort that in principle is not the primary target group age of the Safe Schools program. Therefore, to address both issues, we focus on potential cohorts affected by the policy in terms of time (i.e., their age in 2015, when the policy started) and space (i.e., their proximity to treated schools).

In terms of time, Table 1 and Figure 2 show that the ages most affected by the policy were those in grades 6–9 (13–16 years old), particularly those in grade 7 (14 years old). Therefore, we can use the incarceration data by identifying individuals who were that age in 2015. In terms of space, in line with our other findings, we consider individuals who resided near treated schools, specifically within 300 meters of the treated schools. Following this logic, in Figure 4, we present our SDID estimators at the school level, using as the outcome the probability that a gang-affiliated individual living within 300 meters of the school and within the [X, X+1] age range was incarcerated. We perform this analysis for each year after the policy, enabling us to observe results for different cohorts and years. Panels A–E depict plots for each year. Ages range from 18 to 27 years across all years; however, the x-axis displays their age in 2015 (the year the program began), which varies across years.

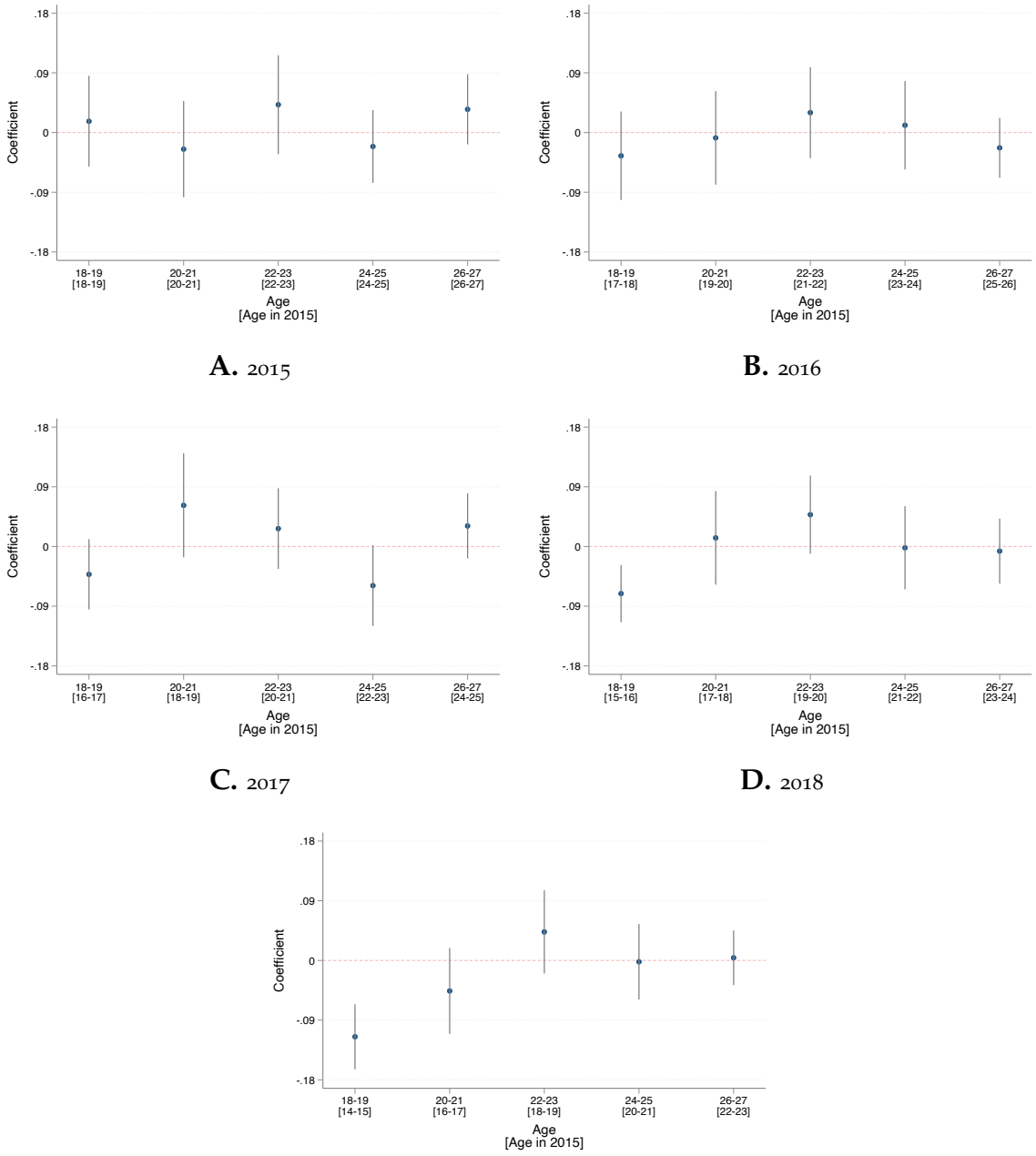
Our results indicate that the treated schools experience a reduction in the probability of having a gang-affiliated individual incarcerated who was in the age range of 18–19 years in 2018 and 2019 (Panels D and E). These individuals correspond to those who were 15–16 years old and 14–15 years old in 2015, respectively. Consistently, we do not find significant results for older cohorts in any year after the program started (Panels A–C).

We confirm the statistical significance of these differences by conducting a fully saturated triple difference-in-differences analysis, which we present in Table 3. In the first column, we only consider the post-years 2015–2017, with the treated cohort defined as individuals aged 18–19 years in each year, thus falling within the age range of 16–18 years in 2015. Our findings indicate that this cohort in treated schools did not exhibit any significant differences compared to older cohorts following the policy's implementation. However, in the second column, when we consider 2018 and 2019 as the post-policy years, we observe a negative differential effect of 10 percentage points among the 18- to 19-year-old cohort for treated schools, which represents a percentage decrease of 40% relative to the mean. That is, cohorts that fall within the 14–16 age range in 2015 and live nearby treated schools have a negative

differential effect compared to older cohorts in the same schools after the policy started.

In Table A4, we present our SDID estimators for gang- and non-gang-affiliated individuals. In column 1, we observe our main SDID estimator for the probability that a gang-affiliated individual aged 18 or 19 years is incarcerated, considering only 2018 and 2019 as our post-policy period. Columns 2–4 present our estimator discriminating by the type of crime. In column 4, we present our SDID estimator, considering only individuals with no gang affiliation aged 18–19 years, taking only 2018 and 2019 as our post-policy period. In general, the effect we observe for incarceration is mainly driven by extortion and homicides, which are the two main crimes committed by gangs. Consistently, we do not observe a statistically significant decrease for other types of crimes related to gangs nor a significant decrease in the probability of incarceration of non-gang-affiliated individuals.

Figure 4
Probability of incarceration across cohorts



A. 2015 **B.** 2016

C. 2017 **D.** 2018

E. 2019

Notes: This figure presents SDID estimator graphs and their 95% confidence intervals, using Equation 1 as proposed by Arkhangelsky et al. (2021). The dependent variable is the probability that an individual residing within a 300-meter radius around school *i*, aged within the [X, X+1] range, is incarcerated. Panels A–E depict plots for each year. Ages range from 18 to 27 across all years; however, the x-axis displays their age in 2015 (the year the program began), which varies across years.

Table 3
Incarceration of gang members

	(1)	(2)
	2015-2017	2018-2019
After × Safe Schools × Youngest	0.004	-0.101***
	(0.033)	(0.036)
After × Safe Schools	-0.029	0.003
	(0.032)	(0.033)
Mean dep. var.	0.25	0.25
Observations	4,970	4,260
Clusters	355	355
Treated clusters	128	128
Cohort-Year FE	Yes	Yes

Notes: This table presents triple difference-in-differences estimators for the probability of incarceration among individuals living within a 300-meter radius around school i and belonging to cohort j in year t. In column 1, the post-policy years considered are 2015–2017, and in column 2, we only consider the years 2018 and 2019. *Youngest* is a dummy indicating the youngest cohort, that is, those who were 18 or 19 years old in year t. In column 1, the youngest cohort was 16–19 years old in 2015, while in column 2, it was 14–16 years old in 2015. $After$ is a dummy that indicates if the year was after 2014, the year before the policy started. $Safeschools$ is a dummy indicating if school i participated in the program. Significance levels are denoted as *** for $p < 0.01$, ** for $p < 0.05$, and * for $p < 0.1$.

5.4 Robustness Exercises

We implement a series of robustness exercises on our primary outcomes to assess the strength of our results across various specifications and scenarios. In Table A5, we present the findings from some of these exercises based on our dropout outcomes. In the first column, we use an alternative database for measuring dropout rates. This database is constructed using a school-grade-level census conducted at the beginning and end of the academic year. Consequently, the dropout rate is calculated as the difference between these two points, providing a measure of dropout occurrences within a single academic year.[5] In general, our results' patterns are equivalent to the ones found with using our main database; however they are smaller since this database only accounts for dropout within a year.

In columns 2 and 3, we employ alternative transformations for our outcomes, using the logarithm of dropout rates and the dropout rate (dropout over enrollment). Columns 3, 4, and 5 introduce additional controls—department-year, urban-year, and 2015 index-year, respectively—to account for relevant covariates in the context of how the policy was imple-

[5]In contrast, our current measure also encompasses students who drop out across multiple years, thereby offering a more comprehensive perspective.

mented. Including department-year controls provides a flexible means to control for unforeseen variations within the same administrative units. By including urban-year controls, we facilitate the isolation and comparison of schools in urban and rural settings, acknowledging potential differences in crime dynamics. Including the 2015 index-year controls allows us to consider the primary data source guiding the policy's implementation. Our estimators remain very similar to the ones originally found.

Columns 7 and 8 employ alternative estimation methods. Column 7 presents the traditional DID estimator, which is crucial for understanding the behavior of our SDID estimators without unit weights, as suggested by Arkhangelsky et al. (2021). This comparison provides insights into the local nature of our findings. Notably, we find no significant discrepancies compared to our original findings, suggesting that our results do not depend on specific unit weights. Similarly, column 8 uses a Poisson model, considering the count data nature of our variables. Once again, the pattern of the results aligns closely with our initial findings.

In columns 9 and 10, we adopt alternative definitions for our sample groups. Column 9 includes all non-treated schools as controls (i.e., 4,971 control schools). This approach proves beneficial as the SDID method benefits from a larger donor pool in the control group, enhancing the accuracy of pre-treatment trend replication. Moreover, given the potential lack of information on unobservable variables influencing policy decisions, this method adds greater transparency in the control group selection. In general, the estimators from this approach align closely with our primary estimates. Additionally, Figure A5 presents the dynamic effects of this exercise, finding that the pre-trends closely parallel each other.

Finally, since not all schools have the most advanced grades, in column 10, we exclusively consider only schools with grades 10–11 to investigate whether the observed patterns in our results across grades are influenced by changes in our sample composition. We do not observe such differences in the estimates.

In Table A6, we present analogous analyses, this time focusing on the recruitment and incarceration of affected cohorts in 2018 and 2019. Our findings show consistency with our main results, indicating robustness. Given the inherent variability in recruitment and incarceration occurrences around schools, as defined in our study, we avoid using alternative outcome transformations for robustness checks. Moreover, our outcomes stem from a singular data source, lack count data characteristics, and are not limited to schools focusing solely on secondary education.

In Figure A7, we present a randomization inference exercise for our main outcomes. We randomly assign treatment to approximately 36% of the 355 schools in our sample (approximately the same proportion as the treated schools), estimate the SDID estimators, and repeat

the process 1,000 times. We then plot the distribution derived from the exercise and present a vertical dotted line with the original effect for each outcome. In general, we observe p-values close to zero, indicating a very low likelihood that our results are attributable to chance.

6 Aggregate Effects

6.1 Spillover Effects

Our results thus far indicate a localized impact of the Safe Schools program. Specifically, schools directly targeted by the policy observe a decrease in dropout rates, recruitment, and future involvement in criminal activities among the affected cohorts. However, the preventive nature of the policy, which involves an increased police presence around schools during critical hours, could lead to potential spillover effects.

On the one hand, neighboring schools might experience positive spillovers if the presence of police deters gangs from harassing students in schools near treated schools. On the other hand, since the policy disrupts a recruitment source for gangs—vulnerable students from public schools—they might strategically increase harassment and recruitment in nearby schools not affected by the increased patrolling.

To capture these spillover effects, we identify contaminated schools—non-treated schools in proximity to treated schools—as those located within the area between two circles with radii of X and $X + 1$ kilometers[6]. Figure 5 presents the SDID estimations using these contaminated schools. In Panels A, C, and E, the SDID estimations for treated schools are shown, with the contaminated schools serving as the donor pool and the x-axis showing various donut sizes. Conversely, Panels B, D, and F depict the SDID estimations where the contaminated schools are treated as the units of analysis and control schools form the donor pool.

Our findings reveal that the policy generates positive spillover effects on schools neighboring the treated schools. Treated schools see a decrease of 15 students in dropout rates following the policy's implementation compared to contaminated schools located within the donut-shaped area defined by radii of $[(X), (X + 1)]$. This effect is smaller than our primary result shown in column 1 of Table 1. However, as the donut areas extend farther from the treated schools, the observed effects gradually align more closely with our main result. Panel B confirms these effects, with contaminated schools showing a significant differential decrease in dropout (approximately 5 students) compared to control schools, but only in the

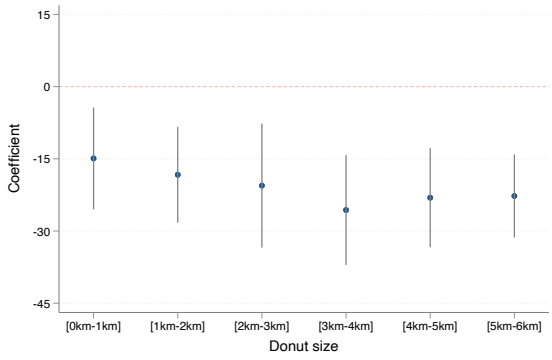
[6]Notice that for recruitment and incarceration, we exclude contaminated schools that are 600 meters or less from any treated school so their buffers will not intersect.

most proximate schools.

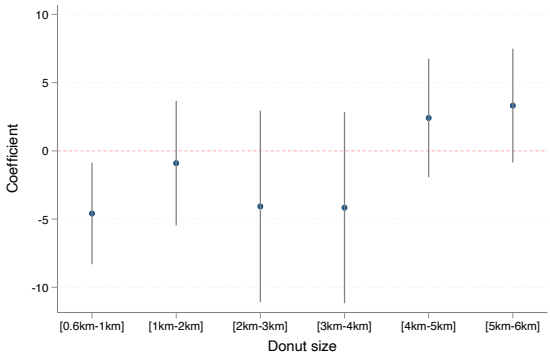
Our results concerning the probability of new recruitment around schools show a pattern akin to that of dropout. Panel C indicates that schools located within donut sizes of [0km, 1km] and [1km, 2km] benefit from the policy, as no discernible differential change is observed for treated schools after the program's implementation. However, as the distance from schools increases, the effect begins to align with our primary result (column 1 in Table 2). Panel D portrays a similar trend, indicating a differential decrease in the probability of recruitment for contaminated schools located within the first two donut ranges compared to the control schools, resembling the magnitude of our main estimate.

Finally, Panels E and F present estimates for the probability of future incarceration among affected cohorts. Once again, we observe some positive spillover effects for the most proximate contaminated schools, followed by a convergence of the effect toward our main finding (i.e., the estimate for [15–16] and [14–15] cohorts in 2018 and 2019 displayed in Figure 4).

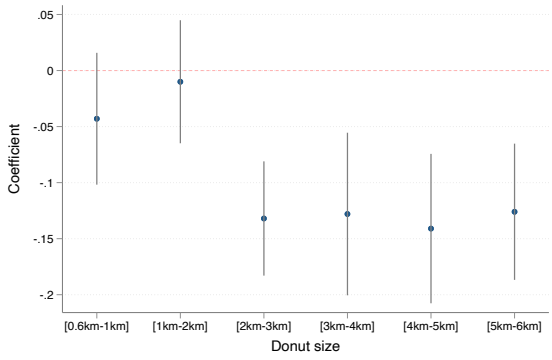
Figure 5
Spillover effects



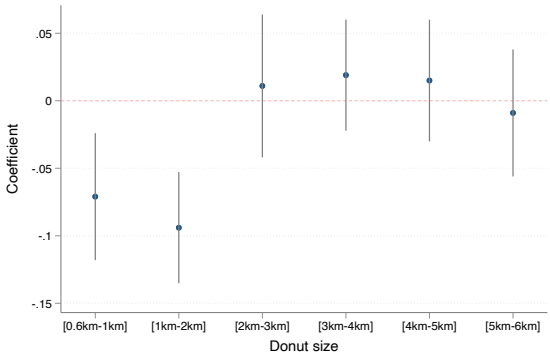
A. Treated vs contaminated (dropout)



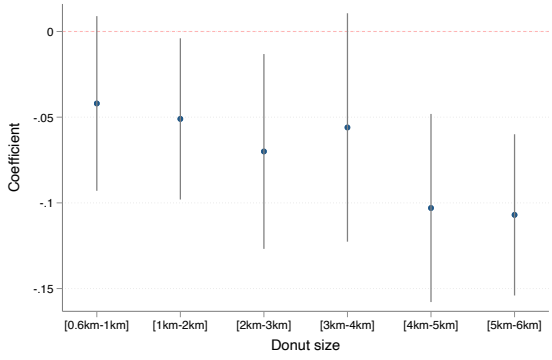
B. Contaminated vs controls (dropout)



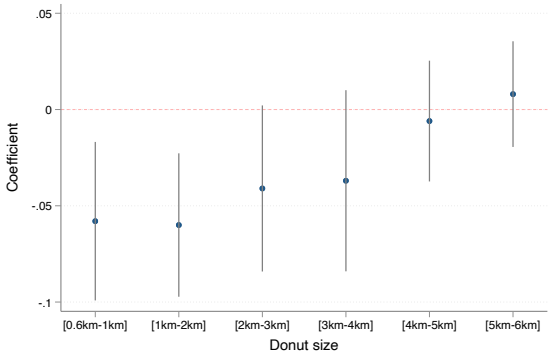
C. Treated vs contaminated (recruitment)



D. Contaminated vs controls (recruitment)



E. Treated vs contaminated (incarceration)



F. Contaminated vs controls (incarceration)

Notes: Panels A, C, and E present the SDID estimations of dropout rates, recruitment, and future involvement in criminal activities among affected cohorts in treated schools, using contaminated schools located within different donut areas as the donor pool. Panels B, D, and F display our SDID estimations for contaminated schools located within different donut areas, using our standard control schools as the donor pool. We define contaminated schools as those situated within donut-shaped areas with radii of $[(X),(X+1)]$ kilometers, located between two circles with respective radii of X and $X+1$ kilometers.

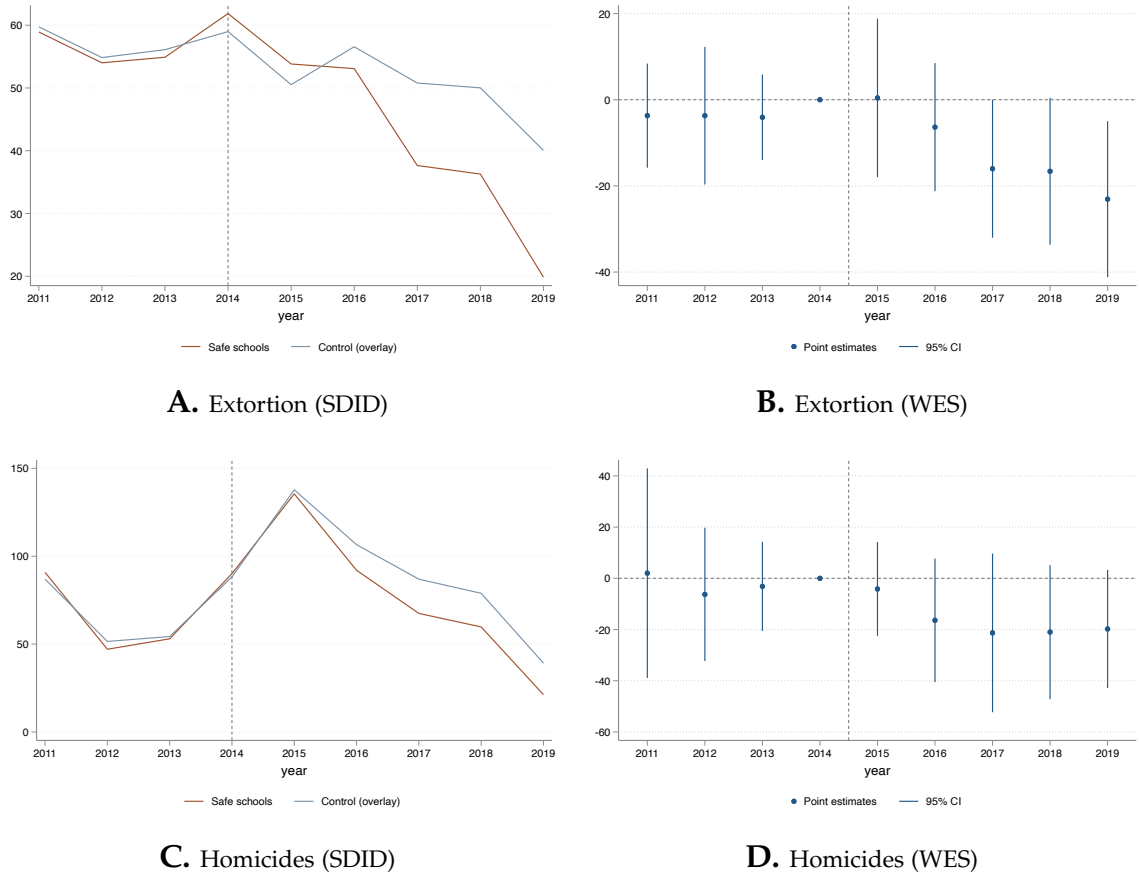
6.2 Impact of Lower Gang Recruitment on Gang Activities

As previously mentioned, since the policy disrupts a significant labor supply for gangs, we anticipate a strategic reaction from them. We therefore examine the impact on gang-related activities at the municipal level to understand the policy's aggregate effect. Due to the absence of more detailed criminal activity data on gang operations, we use municipality data based on victim reports. These data allow us to capture a broader, more general response to the policy.

Figure 6 presents the SDID estimations and WES, as done in our previous analyses. There is a gradual yet slight reduction in the two primary criminal activities associated with gangs—homicides and extortion—following the policy's implementation in municipalities with treated schools. If criminal groups were compensating for the loss of potential members caused by the policy, we would anticipate either no reduction or even an increase in criminal activities. However, our observation of the opposite trend leads us to interpret this as suggestive evidence indicating the gangs' inability to recover from the setback caused by the policy's impact[7]. Notably, the gradual decline in extortion and homicides aligns with recent literature suggesting that diminishing recruitment effectively reduces future criminal activity, though these effects may take several years to become evident (Prieto-Curiel et al. 2023).

[7]Consistent with this result, in Figure A6, we find a slight differential increase in the probability that a small firm opens 300m around our treated schools, in comparison to control schools, after the start of the program.

Figure 6
Criminal activity



A. Extortion (SDID)

B. Extortion (WES)

C. Homicides (SDID)

D. Homicides (WES)

Notes: Panels A and C present SDID estimator graphs for the extortion and homicides reports per 100,000 inhabitants as proposed by Arkhangelsky et al. (2021). The dotted line marks the year before the start of the Safe Schools program. The red line represents the trend of the dependent variable for municipalities where the treated schools are located, while the blue line depicts this trend for the rest of the municipalities, which are used as controls. To facilitate interpretation, the average difference between the treated and control schools in the pre-program period is added to the control averages. Panels B and D plot a traditional event study where the control municipalities are weighted using the unit weights derived from Arkhangelsky et al. (2021).

7 Alternative Mechanisms

7.1 Police Violence

Until now, our focus has been on the preventive aspect of police patrolling, a key component of the program. The preventive nature of this strategy offers an alternative to the more aggressive policing methods that focus on incapacitating criminal members, typically through increased arrests or fatalities. This approach is significant as it could reduce the costs associated with more aggressive tactics while also minimizing the risk of undermining the

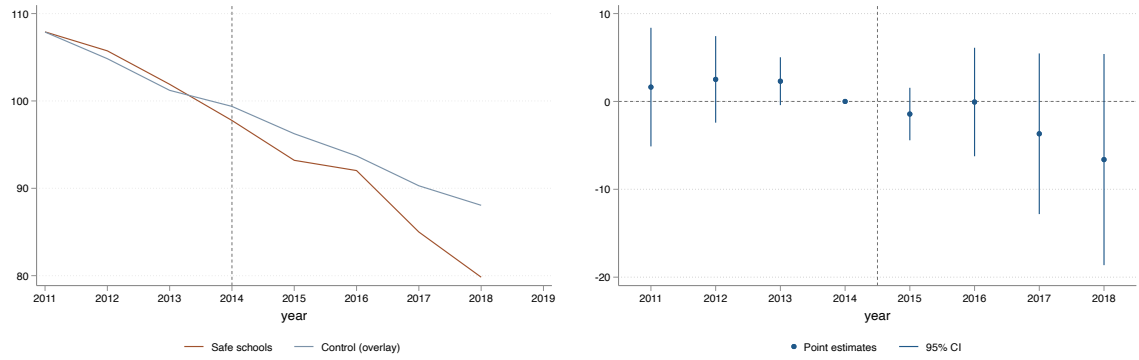
legitimacy of justice institutions, including potential police abuse and the occurrence of false positives.

Nevertheless, while the policy aims to employ police presence as a preventive measure—by increasing patrols in critical areas and times to deter gang activity—this heightened police presence might inadvertently lead to confrontations, potentially escalating instances of police violence. Such outcomes could unintentionally act as an incapacitation mechanism, leading to a reduction in recruitment and dropout rates. This is contrary to our original assertion that emphasized deterrence as the primary mechanism.

Overall, we find no evidence supporting such a mechanism. Figure 7 examines if there was a notable change in the number of police officers at stations near treated schools following the policy's implementation. Our findings show no significant decrease in the years after the program started, although we observe a slight downward trend in 2017 and 2018, potentially associated with a decrease in crime, as discussed in Section 6.2. More importantly, in Panel B, we investigate whether the likelihood of a homicide committed by a police officer in the vicinity of treated schools increased. Contrary to escalating confrontations, we note a slight decrease, suggesting that the policy did not exacerbate the use of force by police officers or increase encounters with gangs.

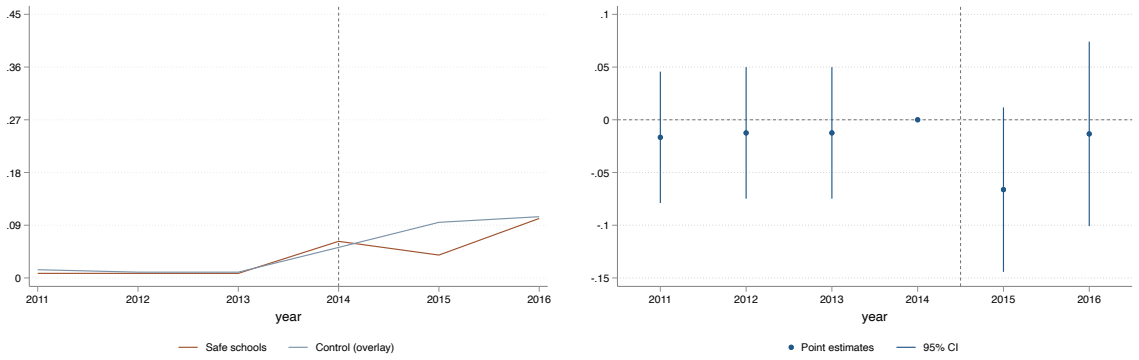
Finally, if the policy were to lead to increased incapacitation, we would expect to see a higher likelihood of incarceration for all cohorts since the policy started. However, as shown in Figure 4, no changes in incarceration rates for gang-related crimes are evident until 2018–2019. At that point, we observe an increase but only for cohorts affected by the policy's prevention of gang contact around schools. Furthermore, Figure 6 shows a gradual reduction in gang criminal activity, consistent with a gradual decrease in their workforce due the disruption in their recruitment processes.

Figure 7
Law enforcement and police violence



A. Number of officers (SDID)

B. Number of officers (WES)



C. Police-involved deaths (SDID)

D. Police-involved deaths (WES)

Notes: Panels A and C present SDID estimator graphs as proposed by Arkhangelsky et al. (2021). The dependent variables in Panels A and B correspond to the number of officers at the police station level, while those in Panels C and D correspond to the probability of homicides committed by a police officer within a 300-meter radius surrounding schools. The dotted line marks the last year before the Safe Schools program started. The red line represents the trend of the dependent variable for treated units, while the blue line represents this trend for control units. Concerning the police stations, treated stations are those located less than 2km away from treated schools, while the controls are the remaining police stations. To aid in interpretation, the average difference between the treated and control schools in the pre-program period is added to the control averages. Panels B and D display a traditional event study where the control municipalities are weighted using the unit weights derived from Arkhangelsky et al. (2021).

7.2 Juvenile Behavior

Recent literature has highlighted the importance of cognitive behavioral therapy in addressing juvenile crime and dropout (Heller et al. 2016). We argue that while these policies can have significant effects in contexts where young people have a certain level of discretion in their decisions, they may be ineffective in contexts where gangs exert significant territorial control, constantly harassing kids to join them.

First, as previously discussed, the primary objective of the police was not to modify the behavior of students but to prevent their contact with gangs while on their way to schools. Additionally, in Table A7, we assessed a subset of schools to determine if there was a change in how students manage conflict and emotions, using a survey conducted between 2014 and 2017. Our estimates indicate that the percentage of students in these schools who exhibited an improved ability to avoid conflicts, demonstrate increased patience, better anger control, set clearer future goals, and display resilience did not show a differential change following the policy's implementation; these changes were close to zero. Therefore, our analysis suggests a lack of clear evidence indicating a significant change in student behavior attributable to the policy.

8 Conclusion

This paper provides evidence that focusing on gang recruitment at schools can significantly reduce school dropout rates and prevent criminal careers. Moreover, by restricting the flow of young recruits into gangs, our findings reveal a significant impact on their labor-intensive extortion operations. The results suggest that over the long run, policies targeting recruitment may lead to a reduction of criminal groups and weaken their financial strength.

These results generalize to other contexts where criminal organizations recruit locally, such as in the rest of Central America, Mexico, Peru, Colombia, and recently Ecuador. For example, recent reports in Ecuador highlighted how drug cartels often recruit children at schools (Vistazo 2022). As an adolescent who joined the groups at the age of 12 said *"The children know that if they refuse, they will be killed, they have no choice."*

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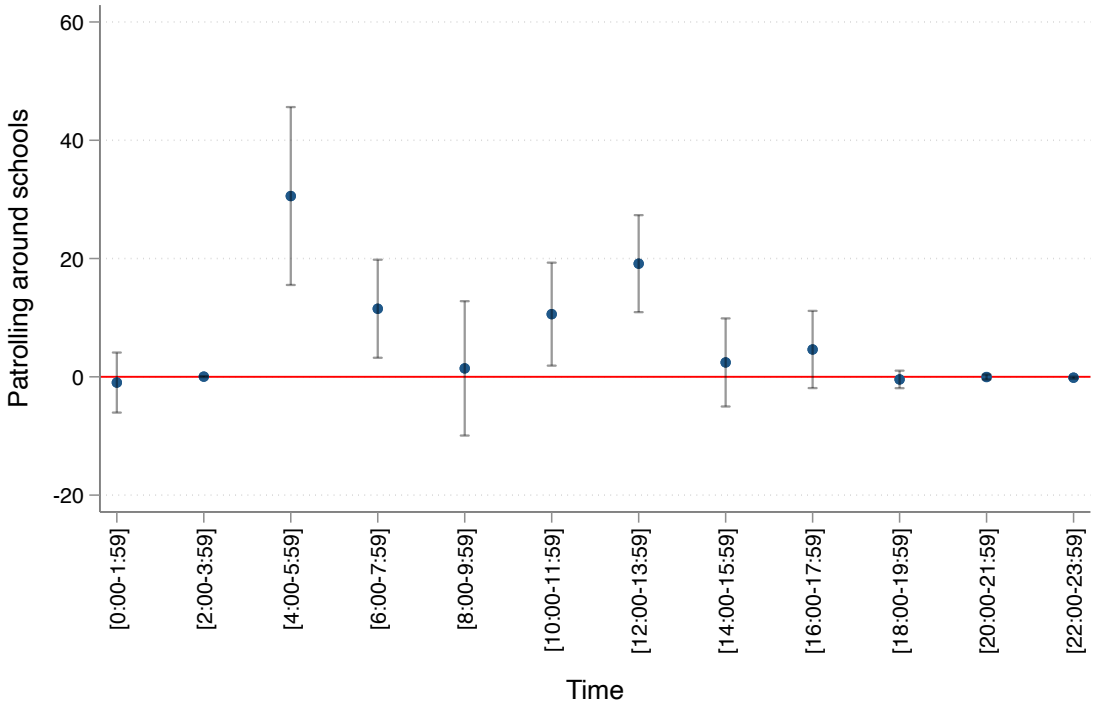
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APPENDIX (For Online Publication)

Breaking the Gang: A Preventive Approach to Reduce Recruitment in Schools in El Salvador

Figure A1
Patrolling around schools



Notes: This figure illustrates the estimates and 95% confidence intervals for the average difference in the number of patrolling within a 300-meter radius from schools, between the treated and controls for 2018 and 2019. We exclude January and December, which are vacation days.

Figure A2
The Safe Schools program



A. Officer outside a school

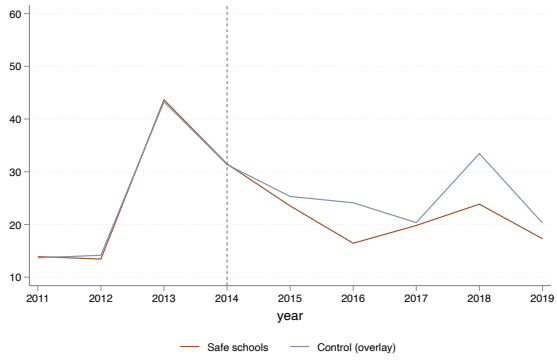


B. Officers patrolling

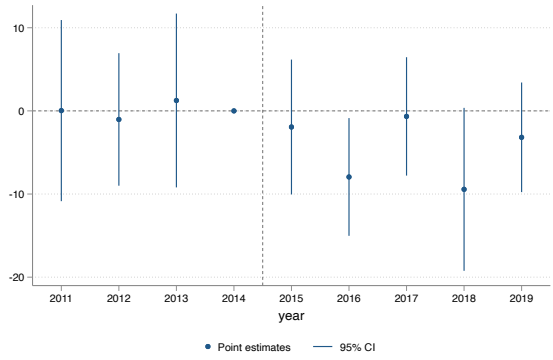


C. Officers explaining the policy

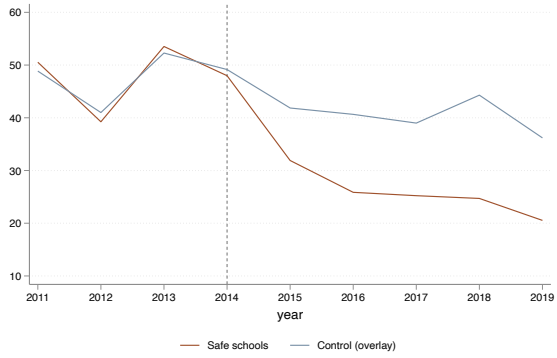
Figure A3
Effect of the Safe Schools program on dropout across grades



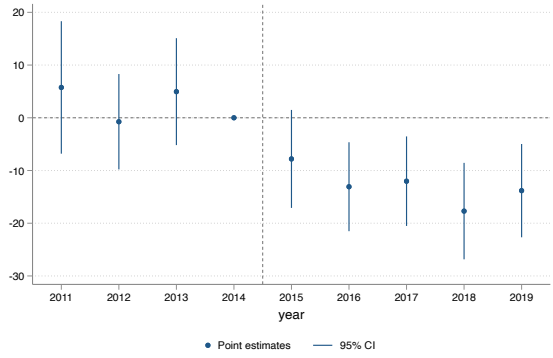
A. Grades 1st-5th



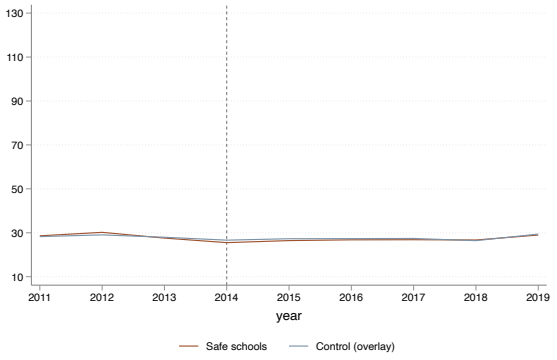
B. Grades 1st-5th (WES)



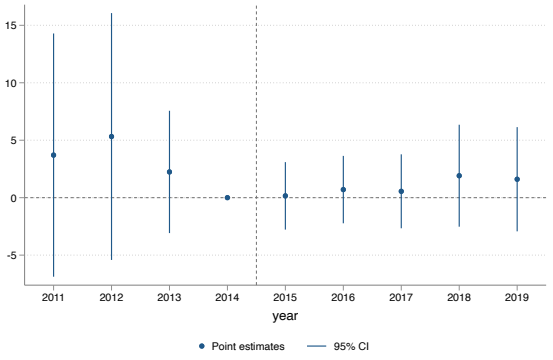
C. Grades 6th-9th



D. Grades 6th-9th (WES)



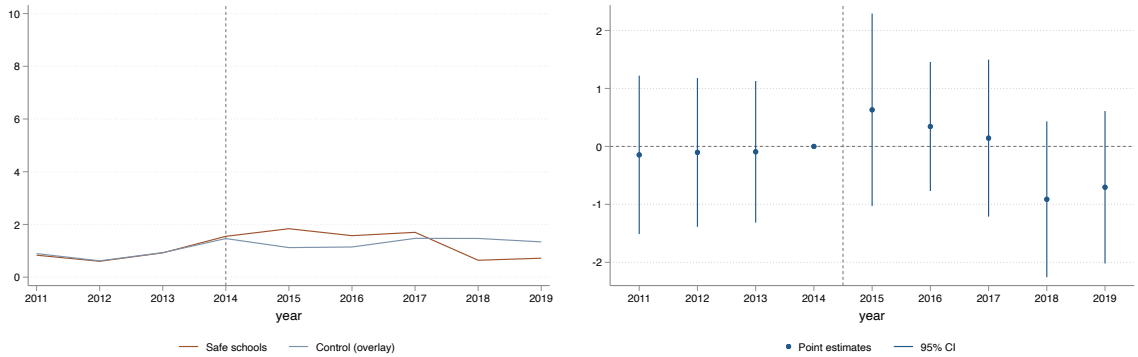
E. Grades 10th-11th



F. Grades 10th-11th (WES)

Notes: Panel A presents the SDID estimator graph for dropout across grades, as proposed by Arkhangelsky et al. (2021). The dotted line marks the year before the start of the Safe Schools program. The red line represents the trend of the dependent variable for treated schools, while the blue line depicts this trend for control schools. To facilitate interpretation, the average difference between the treated and control schools in the pre-program period is added to the control averages. Panel B plots a traditional event study where the control schools are weighted using the unit weights derived from Arkhangelsky et al. (2021).

Figure A4
Effect of the Safe Schools program on teenagers' non-gang criminal activity

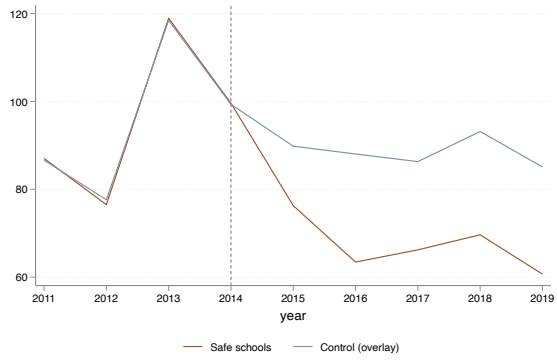


A. Teen admissions, non-gang related (SDID)

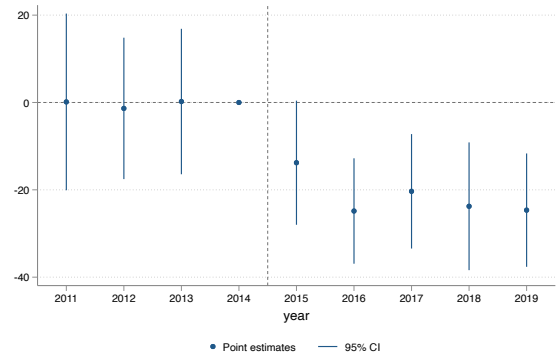
B. Teen admissions, non-gang related (WES)

Notes: Panel A presents the SDID estimator graph for the number of teen admissions to juvenile centers at the municipality level for non-gang-related crimes, as proposed by Arkhangelsky et al. (2021). The dotted line marks the year before the start of the Safe Schools program. The red line represents the trend of the dependent variable for treated schools, while the blue line depicts this trend for control schools. To facilitate interpretation, the average difference between the treated and control schools in the pre-program period is added to the control averages. Panel B plots a traditional event study where the control schools are weighted using the unit weights derived from Arkhangelsky et al. (2021).

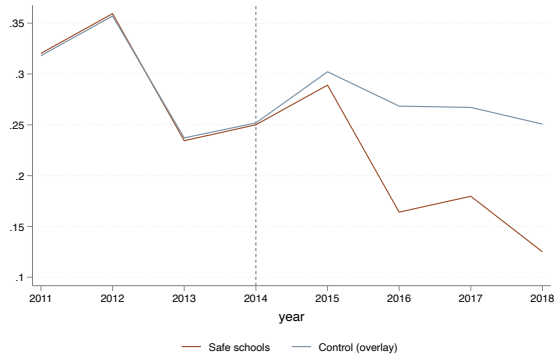
Figure A5
Dynamic effects of the Safe Schools program using all schools as the control group



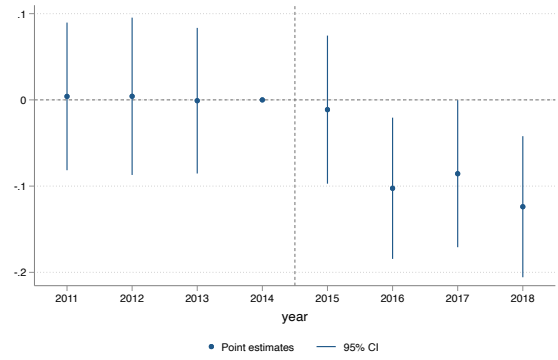
A. All dropout (SDID)



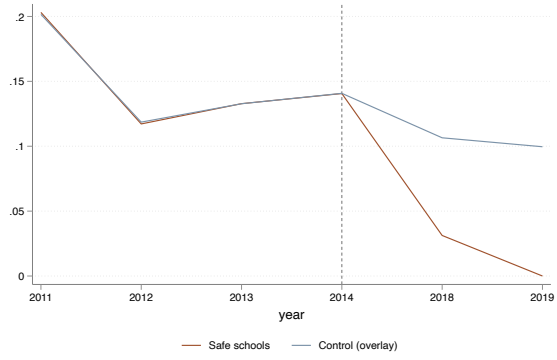
B. All dropout (WES)



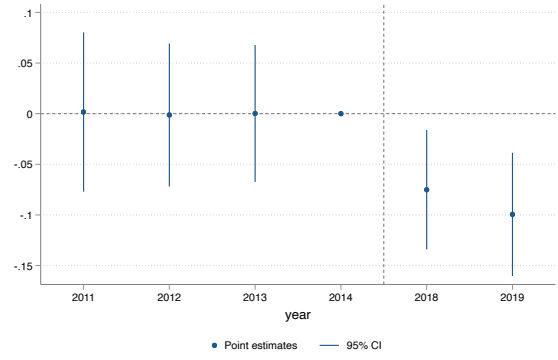
C. Recruitment (SDID)



D. Recruitment (WES)



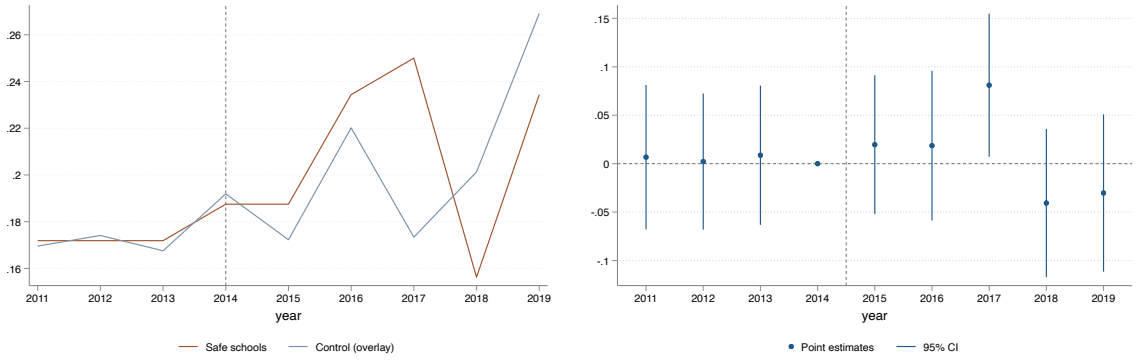
E. Incarceration (SDID)



F. Incarceration (WES)

Notes: Panels A, C, and E present SDID estimator graphs as proposed by Arkhangelsky et al. (2021), taking as the donor pool all schools in El Salvador except the treated schools. The dotted line marks the year before the start of the Safe Schools program. The red line represents the trend of the dependent variable for treated schools, while the blue line depicts this trend for control schools. To facilitate interpretation, the average difference between the treated and control schools in the pre-program period is added to the control averages. Panels B, D, and E plot a traditional event study where the control schools are weighted using the unit weights derived from Arkhangelsky et al. (2021).

Figure A6
Effect of the Safe Schools program on the probability of a firm entry

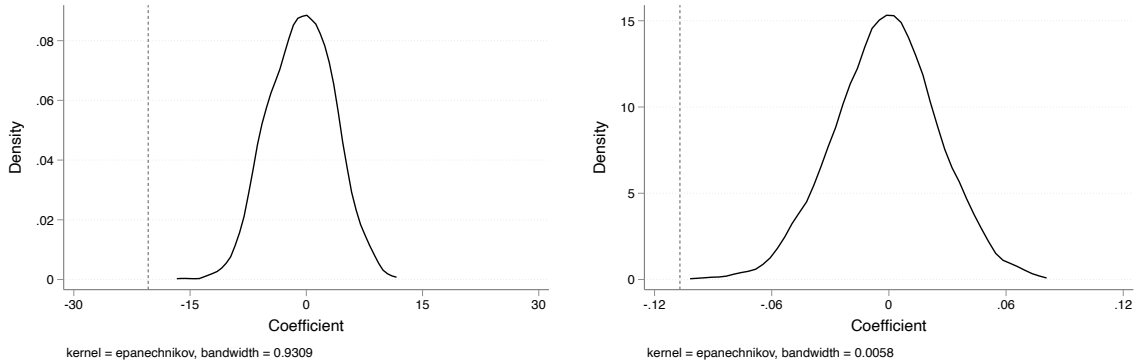


A. Small firms

B. Small firms (WES)

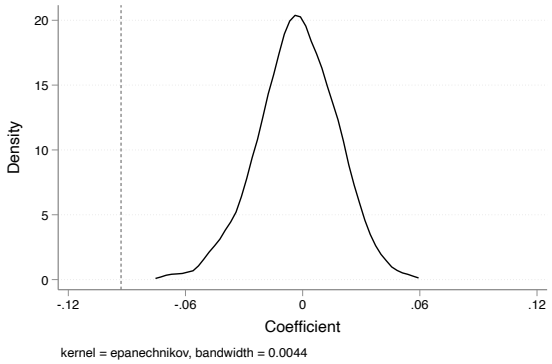
Notes: Panel A (C) presents the SDID estimator graph for the probability that a big (small) firms opens 300 meters around the school, as proposed by Arkhangelsky et al. (2021). The dotted line marks the year before the start of the Safe Schools program. The red line represents the trend of the dependent variable for treated schools, while the blue line depicts this trend for control schools. To facilitate interpretation, the average difference between the treated and control schools in the pre-program period is added to the control averages. Panel B (D) plots a traditional event study where the control schools are weighted using the unit weights derived from Arkhangelsky et al. (2021).

Figure A7
Randomization inference



A. All dropout

B. Recruitment



C. Incarceration

Notes: This graph presents the distribution of the SDID estimators when treatment is randomly assigned. We randomly assign treatment to approximately 36% of the 355 schools in our sample, estimate the SDID estimators following Arkhangelsky et al. (2021), and repeat the process 1,000 times. The vertical dotted line represents the original effect for each outcome.

Table A1
Summary statistics

	(1) Mean	(2) Standard deviation	(3) Min.	(4) Max.
School level				
Enrollment	530.26	429.98	14.00	1,913.00
Dropout (levels)	70.08	83.02	0.00	407.00
Dropout (rate)	0.14	0.13	0.00	0.71
Any gang incarceration	0.25	0.43	0.00	1.00
Any gang incarceration 18-19 y/o	0.09	0.28	0.00	1.00
Any non-gang incarceration	0.35	0.48	0.00	1.00
Any new recruit	0.23	0.42	0.00	1.00
Prioritization index (2015)	0.26	0.26	0.00	0.94
Min. Dist. to Police Station	2.50	2.89	0.05	12.93
Total officers per student	0.10	0.19	0.00	1.23
Municipality level				
Juvenile admissions (gang related)	7.36	10.66	0.00	48.69
Extortion reports	30.96	33.26	0.00	167.79
Homicide reports	47.53	40.23	0.00	187.01
Population (thousands)	23.48	36.73	1.71	241.40
Prioritization index (2015)	0.33	0.19	0.06	0.94

Notes: This table presents the descriptive statistics of the main outcomes used in the paper. Incarceration and recruitment variables are dummies that indicate if incarceration occurred within 300 meters of the school. Total officers is the number of police officers at the closest police station. All municipality variables are per 100,000 inhabitants.

Table A2
Patrolling around schools

	(1) All	(2) Entrance	(3) Exit	(4) Other
Safe schools	80.250***	42.044***	30.610***	7.596
	(22.951)	(9.912)	(7.913)	(9.833)
Mean dep. var.	49.26	16.46	10.42	22.38
Observations	681	681	681	681
Schools	355	355	355	355
Safe schools	128	128	128	128

Notes: This table illustrates the estimates and standard errors for the average difference in the number of patrolling activities within a 300-meter radius from schools, between the treated and control schools for 2018 and 2019. We exclude January and December, which are vacation months. Standard errors are clustered at the municipality level. Significance levels are denoted as *** for $p < 0.01$, ** for $p < 0.05$, and * for $p < 0.1$.

Table A3
Dropout: TDID dropout estimators

	(1)	(2)
	All	Schools with secondary
After × Safe Schools	-4.697***	-3.825
	(1.795)	(2.320)
After × Safe Schools × 6–9	-10.836***	-10.977***
	(2.897)	(3.550)
After × Safe Schools × 10–11	3.419	1.994
	(3.262)	(4.197)
Mean dep. var.	20.12	25.05
Observations	9,578	6,534
Schools	355	242
Safe Schools	128	101

Notes: This table presents TDID and year t. In column 1, we consider all schools, while in column 2 we exclude those that teach grades 10–11. *6–9* is a dummy indicating if the cohort corresponds to grades 6–9. *10–11* is a dummy indicating if the cohort corresponds to grades 10–11. $After$ is a dummy that indicates if the year was after 2014, the year before the policy started. $Safeschools$ is a dummy indicating if school i participated in the program. Significance levels are denoted as *** for $p < 0.01$, ** for $p < 0.05$, and * for $p < 0.1$.

Table A4
Incarceration of affected cohort across gang affiliation

	(1)	(2)	(3)	(4)	(5)
	Gang affiliation				No affiliation
	All	Homicides	Extortion	Others	
Safe schools	-0.093***	-0.053***	-0.033***	-0.014	-0.017
	(0.022)	(0.017)	(0.012)	(0.011)	(0.019)
Mean dep. var.	0.050	0.020	0.018	0.023	0.033
Observations	2,130	2,130	2,130	2,130	2,130
Schools	355	355	355	355	355
Safe schools	128	128	128	128	128

Notes: This table presents the SDID estimators and standard errors as proposed by Arkhangelsky et al. (2021). The outcome is the probability that an 18- to 19-year-old individual residing within a 300-meter radius around school i is incarcerated in 2018 or 2019 (i.e., individuals who were 14–16 years old in 2015, the first year of the Safe Schools program). In columns 1–4, we only consider individuals who were affiliated with MS-13 or Barrio 18. Column 1 considers all types of crimes of gang-affiliated individuals. Column 2 considers only individuals incarcerated due to homicides, and column 4 considers those incarcerated due to extortion. Finally, column 5 considers only individuals with no gang affiliation. Significance levels are denoted as *** for $p < 0.01$, ** for $p < 0.05$, and * for $p < 0.1$.

Table A5
Robustness exercises for dropout outcomes

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Dropout within the same year	Outcomes definitions		Additional covariates			Alternative estimations		Alternative samples	
		Logarithm	Rate	Department-Year	Urban-Year	2015 index-Year	DID	Poisson	All schools	Schools with secondary
Panel A: Grades 1–5										
Safe schools	-1.968	-0.155**	-0.017	-2.206	-3.621**	-4.519***	-4.697***	-0.201**	-2.501	-5.259**
	(1.598)	(0.068)	(0.011)	(1.639)	(1.759)	(1.685)	(1.796)	(0.081)	(1.655)	(2.440)
Mean dep. var.	19.39	2.20	0.11	20.12	20.12	20.12	20.12	20.12	8.58	25.05
Observations	2,840	3,195	3,195	3,195	3,195	3,195	3,195	3,195	45,891	2,178
Schools	355	355	355	355	355	355	355	355	5,099	242
Safe schools	128	128	128	128	128	128	128	128	128	101
Panel B: Grades 6–9										
Safe schools	-7.205***	-0.251***	-0.051***	-9.358***	-11.860***	-13.055***	-15.376***	-0.331***	-15.749***	-14.673***
	(1.948)	(0.088)	(0.011)	(2.633)	(2.923)	(2.680)	(3.236)	(0.074)	(2.767)	(3.540)
Mean dep. var.	17.67	2.41	0.15	26.83	26.83	26.83	26.83	26.83	12.96	37.00
Observations	2,840	3,195	3,195	3,195	3,195	3,195	3,195	3,195	45,891	2,178
Schools	355	355	355	355	355	355	355	355	5,099	242
Safe schools	128	128	128	128	128	128	128	128	128	101
Panel C: Grades 10–11										
Safe schools	-3.874**	0.018	-0.004	-0.152	0.336	0.350	-1.276	-0.087	-0.728	0.407
	(1.872)	(0.044)	(0.013)	(1.316)	(1.225)	(1.342)	(2.656)	(0.132)	(2.448)	(1.724)
Mean dep. var.	3.66	0.56	0.05	10.71	10.71	10.71	10.71	10.71	6.75	17.24
Observations	2,840	3,195	3,195	3,195	3,195	3,186	3,195	3,195	45,891	2178
Schools	355	355	355	355	355	355	355	355	5,099	242
Safe schools	128	128	128	128	128	128	128	128	128	101

Notes: This table presents robustness exercises for our dropout outcomes. Panel A illustrates exercises related to the dropout of students in grades 1–5, Panel B for grades 6–9, and Panel C for grades 10–11. Column 1 uses dropout within the same year, employing census data provided by MINED as the outcome. Columns 2 and 3 explore various outcome transformations, including logarithmic and dropout rates (number of students who drop out over the number of students enrolled). Columns 3, 4, and 5 introduce the following fixed effects: department-year, urban-year, and 2015 index-year, respectively. Columns 7 and 8 use different estimation methods: the traditional DID estimator and Poisson model, respectively. Column 9 considers all schools except for those that were treated and were the second phase as controls. Column 10 excludes schools that do not teach students in grades 10–11. Significance levels are denoted as follows: *** for $p<0.01$, ** for $p<0.05$, and * for $p<0.1$.

Table A6
Robustness exercises for recruitment and incarceration

	(1)	(2)	(3)	(4)	(5))
	Additional covariates			DID	All schools
	Department-Year	Urban-Year	2015 index		
Panel A: Recruitment					
Safe schools	-0.091***	-0.100***	-0.103***	-0.110***	-0.082***
	(0.026)	(0.026)	(0.029)	(0.028)	(0.027)
Mean dep. var.	-0.01	-0.02	-0.03	0.19	0.11
Observations	2,840	2,840	2,840	2840	40,792
Schools	355	355	355	355	5,099
Safe schools	128	128	128	128	128
Panel B: Incarceration					
Safe schools	-0.075***	-0.081***	-0.087***	-0.099***	-0.087***
	(0.020)	(0.021)	(0.024)	(0.022)	(0.022)
Mean dep. var.	-0.03	-0.03	-0.03	0.05	0.04
Observations	2,130	2,130	2,130	2,130	30,594
Schools	355	355	355	355	5,099
Safe schools	128	128	128	128	128

Notes: This table presents robustness exercises for our dropout outcomes. Panel A illustrates exercises related to the probability of recruitment around schools. Panel B displays exercises concerning the probability of incarceration among affected cohorts, specifically those who were 14–16 years old in 2015. In columns 1–3, we introduce the following fixed effects: department-year, urban-year, and 2015 index-year, respectively. Column 7 presents our results using the traditional DID estimator. Column 9 considers all schools except for those that were treated and in the second phase as controls. Significance levels are denoted as follows: *** for $p<0.01$, ** for $p<0.05$, and * for $p<0.1$.

Table A7
Social-emotional skills

	(1) Avoid discussions	(2) Patience	(3) Anger control	(4) Goals	(5) Resilience
Safe schools	0.029	0.003	-0.041	-0.006	-0.039
	(0.050)	(0.029)	(0.034)	(0.011)	(0.029)
Mean dep. var.	0.49	0.71	0.84	0.94	0.36
Observations	99	99	99	99	99
Schools	33	33	33	33	33
Safe schools	21	21	21	21	21

Notes: This table presents the SDID estimators of Equation 1 as proposed by Arkhangelsky et al. (2021). In columns 1–5, the dependent variable is the share of students surveyed in *school* i and year t who agreed with the statements of avoiding discussions, being patient, controlling anger, setting goals for their futures, and being resilient, respectively. $Safeschools$ is a dummy indicating whether the school participated in the program. Significance levels are denoted as follows: *** for p<0.01, ** for p<0.05, and * for p<0.1.

Table A8
Prioritization criteria of safe schools

	(1) Safe schools	(2) Control schools	(3) Estimated difference	(4) P-value
Enrollment	530.34	396.05	134.289	0.000
	(432.71)	(365.42)	(21.823)	
Dropout	0.19	0.21	-0.013	0.816
	(0.60)	(1.12)	(0.054)	
Rural	0.45	0.62	-0.172	0.000
	(0.50)	(0.48)	(0.027)	
Prioritization index	7.02	6.61	0.413	0.090
	(4.25)	(4.41)	(0.243)	

Notes: This table presents the variables that were considered to prioritize the phases of the Safe Schools program. Column 1 (column 2) presents the average of the treated (control) schools and their standard errors. Column 3 presents the estimated difference, while column 4 presents its p-value.

Table A9
Recruitment and teenagers crime involvement

	(1)	(2)	(3)
	Dropout	Recruitment	Adult incarceration
Safe schools × After × High rank	-23.395***	0.030	-0.170***
	(7.042)	(0.059)	(0.049)
After × High rank	3.256	0.023	0.030
	(2.074)	(0.027)	(0.022)
Safe schools × After	-7.104*	-0.112***	-0.037
	(3.770)	(0.036)	(0.026)

Notes: