POLICE VIOLENCE, STUDENT PROTESTS, AND EDUCATIONAL PERFORMANCE*

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Abstract

We study the protest behavior of teenagers linked to a student killed by a stray bullet coming from a policeman in Chile. We use administrative data to follow the schoolmates of the victim and those living nearby the shooting in hundreds of protest and non-protest days. We find that police violence causes lower protest participation in street rallies but more adherence to test boycotts. These effects appear among schoolmates of the victim and *not* among students living nearby the killing. Negative educational consequences suffered by the schoolmates combined with previous results suggest that psychological mechanisms are a plausible explanation.

JEL codes: D7, D9, I2.

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I. Introduction

State violence is routinely used to ensure public safety (Atkinson and Stiglitz, 2015). Some scholars argue that it prevents unlawful actions (Acemoglu and Robinson, 2001; Besley and Persson, 2011), while others emphasize that it can spark dissident behavior (Davenport, 2007; Passarelli and Tabellini, 2017).[1] The consequences are likely to depend on the relative magnitude of emotions related to fear and anger around the victims (Aytac et al., 2018). Yet empirical analyses of dissident behavior in the social network of victims are remarkably limited. The lack of evidence is unsurprising given the difficulties in measuring state violence and dissident behavior outside of the lab (Fisher et al., 2019). The fact that violence is usually targeted and occurs in disadvantaged areas further complicates an empirical evaluation (Klor et al., 2021; Fryer, 2020).

This paper offers novel evidence of the impact of police violence on protest behavior and educational performance in a middle-income country. The context is the 2011 student-led protests in Chile, where we observe multiple protest-related decisions of hundreds of thousands of teenagers before and after an extreme event of police violence. In the middle of a protest wave, a sixteen-year old student was killed by a stray bullet coming from a policeman. The event was confirmed by ballistic expert reports, judiciary records, and the officer himself. Using administrative data on daily school attendance, we follow the schoolmates of the teenager killed and students living nearby the shooting in hundreds of protest and non-protest days to study if the shooting affected their protest behavior as measured by school skipping decisions during weekday protests.

We begin the analysis with a validation of our protest measure using surveys and police reports. In the survey, we show representative images of protest videos to hundreds of people and ask them to identify high-school students, which allows us to quantify their presence at dozens of weekday rallies. Similarly, police reports confirm a strong empirical relationship between school skipping rates and the number of people at these rallies. To estimate the impact of the shooting on protest behavior, we use a matching difference-in-differences estimator and randomization inference. Given the availability of detailed administrative data for hundreds of thousands of high-school students, the setup is particularly well-suited for this strategy. As exogenous variation related to police violence, we rely on the accidental nature of the stray bullet, both in terms of the affected students and the timing of the event. In addition, we employ coarsened-exact matching to construct a counterfactual composed by students who attended similar schools, were similar in terms of socioeconomic and educational characteristics, and protested identically before the shooting.

The main result is that the police killing decreased adherence to street rallies and increased

[1] See also Lichbach (1987); Opp and Roehl (1990); Lohmann (1994); McAdam (1995); Moore (1998); Shadmehr and Boleslavsky (2022), among many others.

participation in test boycotts but only among students who were socially close to the victim. The lower school skipping rate in weekday protests slowly fades away over time and it is larger among teenagers who regularly shared classes with the victim. In terms of the educational performance of affected students, we provide suggestive evidence of deteriorating outcomes including a lower probability of enrolling in higher-education. The results are presented in three parts.

The first part of the results section shows that the police killing decreased the probability that the schoolmates of the victim skipped school in protest days by 7 percentage points from an average of 33% in the control group. Half of this decrease fades away one year after the killing. Crucially, the skipping rate of schoolmates was similar to the group of students acting as the counterfactual during *non*-protest days with a precisely estimated null coefficient. The lack of an impact on non-protest days is important as it further supports the protest nature of their decisions. In contrast to the impact on the schoolmates, those who lived nearby the shooting remained protesting in a similar way than their comparison group. These findings are not present in less severe acts of police violence nor in killings of teenagers without police involvement.

The second part studies individual-level adherence to boycott an important standardized test. A week before test day, student organizations called to boycott the test by not taking it, not answering the questions, or to simply skip school. According to educators and researchers, the test introduces perverse incentives and increases segregation (Hsieh and Urquiola, 2006). Although test scores are never disclosed to students, school-level scores have been regularly used to inform parents about school quality and to guide the design of policies (Cuesta et al., 2020). Using administrative data we construct an indicator of individual boycott adherence by combining data on test takers and school skipping. We find that the schoolmates were 13 percentage points *more* likely to participate in the student-led boycott from a baseline of 12% test absenteeism in the control group.

The last part of the results section explores the educational consequences of police violence. We find that exposure to the shooting is consistently although not significantly associated with lower grades and higher dropout rates but again only among students socially close to the victim. The magnitude of estimates is remarkably close to comparable numbers from the United States (Ang, 2021), although exact p-values prevent us from rejecting a null impact. In addition, we provide novel evidence of the shooting strongly decreasing the probability of taking the exam to access higher education by 29 percentage points from a baseline of 86% in the comparison group.

What is the mechanism explaining our findings? We provide a collection of evidence which suggests that changes in risk assessment arising from emotional cues are the most likely explanation. Several patterns pushed us towards this interpretation. The impact on protest behavior is significantly larger among students who regularly shared classes with the victim when compared to other students (younger and older) enrolled in the same school. Similarly, the lack of an impact on students living nearby the shooting – likely equal or better informed than those living farther away (Fujita et al., 2006; Enke et al., 2021) – suggests that differential information or memory of the event is unlikely to explain the findings. In addition, the higher adherence to the boycott and lower adherence to rallies suggest that the risk associated with the presence of the police could be important. Finally, the suggestive negative educational impacts combined with a limited role for parental involvement also point towards psychological consequences of police violence being important, as shown by recent research in the United States (Rossin-Slater et al., 2020).

Our main contribution is to provide evidence of the impact of police violence on protest-related decisions using individual-level administrative data. Officer shootings are perhaps the most ubiquitous representation of state violence and the study of individual decisions without the intervention of a researcher is rare (Davenport, 2007). Previous research has studied the consequences of crackdowns, military interventions, and state repression on dissident and civic engagement behavior using lab-in-the-field experiments (Young, 2019a), experiments with online and offline surveys (Lawrence, 2017; Aytac et al., 2018; Curtice and Behlendorf, 2021), and quasi-experimental methods with aggregate data (Dell, 2015; Dell and Querubin, 2018; Rozenas and Zhukov, 2019; Insler et al., 2019; Bautista et al., 2021a; Ang and Tebes, 2021).

There are two novelties in our analysis. First, we use administrative data for the entire population of students in a large Latin American city. The large number of observations help us to develop an econometric strategy that exploits the availability of hundreds of thousands of potential controls. The focus on Latin America expands our current body of knowledge to a middle-income country with an established democracy and well-functioning institutions. Second, we are able to follow individuals exposed to an exogenous event of police violence over multiple years, which allows us to estimate the impact of violence over different time horizons outside of the lab.

The study of a stray bullet coming from a policeman makes this paper also related to a literature studying the causes and consequences of the actions of the police. Previous research has shown that police violence can act like a "trigger event" for a wave of protests (Williamson et al., 2018) with decreased favorability toward the police and renewed perceptions of injustices as mediators (Reny and Newman, 2021). Related research in the U.S. has also emphasized the racial discrimination practiced by police officers (Fryer, 2020; Goncalves and Mello, 2021). In contrast to those articles, we depart from discriminatory practices in the U.S. to show that unintentional or non-targeted police violence can also have important consequences among those indirectly exposed.

The educational analysis relates to a recent literature that documents the negative consequences of those exposed to police violence. Although research studying the cognitive impacts of violence is vast (Carrell and Hoekstra, 2010; Sharkey, 2010; Monteiro and Rocha, 2017; Cabral et al., 2020; Prem et al., 2021), evidence on the effects of violence when coming from the police is more limited. The exceptions also come mostly from the U.S., where people indirectly exposed to officer-related killings experienced a deterioration of their mental health and worst educational performance (Bor et al., 2018; Legewie and Fagan, 2019; Ang, 2021). These negative psychological effects also appear on students after school shootings (Rossin-Slater et al., 2020; Levine and McKnight, 2021). We provides suggestive evidence of negative educational consequences: schoolmates of the student killed by the police gunshot experienced significantly lower college enrollment.

Finally, we also contribute to the literature studying protest behavior at the individual level by estimating the impact of police violence. Previous research has emphasized the importance of social networks (Cantoni et al., 2019; González, 2020), habit formation (Bursztyn et al., 2021), and the role of information communication technologies in facilitating coordination (Manacorda and Tesei, 2020; Enikolopov et al., 2020). We contribute with novel evidence on the impact of police violence on subsequent protest behavior around the social network of the victim. In line with insights from part of the theoretical literature, our results show that police violence can have a transitory deterrence effect at least in the case when violence is non-targeted.

II. Student protests and the stray bullet

A. The 2011 student movement

The student movement of 2011 triggered one of the largest protest waves in the history of Chile. As part of the revolt, hundreds of thousands of students skipped school on weekdays with the goal of replacing institutions that were installed in 1981 as part of a reform package during the seventeen-year dictatorship led by General Augusto Pinochet (Bautista et al., 2021b). Students protested against the *de facto* for-profit nature of schools and the increasing cost of higher education in what is one of the most market-oriented systems in the world (Figlio and Loeb, 2011). The first large protest was held in May 12 and it was triggered by unexpected delays in the assignment of students' scholarships and bus passes. After a handful of relatively small protests, the movement exploded in early June, gathering support from citizens and large worker organizations (González, 2020). The main protest days have been extensively documented in newspapers, research articles, and chronicles of the events (Simonsen, 2012; Figueroa, 2012; Jackson, 2013).

The largest and most violent protests took place in August, particularly during the two-day national strike of the 24th and 25th. The first day was a strike in which people stayed mostly at home to protest. The second day experienced one of the largest rallies in the country's history with almost half a million participants in the capital's main square. The two-day strike was organized by the National Association of Public Employees and the largest workers union. As a consequence of the national strike, and because teachers in the public sector were part of the association of public employees, most high-schools were closed during these two days. The main activity in the city capital took the form of a march from the main square to La Moneda Palace where the seat of the president is located, but barricades took place in several parts of the city all day long.

B. The stray bullet incident

The sixteen years old Manuel Gutiérrez was killed by a police gunshot on the night of August 25 of 2011.[2] The high-school student was accompanied by his brother and a neighbor as they were passing through a footbridge over a large street, just a couple of blocks from their homes. Their intention was to passively watch the protest final events of that day. According to interviews with his family, Manuel did *not* actively participated in the national strike. Because of the strike his school was closed and thus during that day he visited some friends nearby. Manuel was the youngest brother of a religious family who was known in the neighborhood to be "a good young man" removed from conflicts, and an active participant of religious activities in the local church.

According to official judiciary records, the night of August 25 the policeman Miguel Millacura fired his UZI submachine gun with the goal of dispersing protesters. An investigation determined that the stray bullet hit the footbridge and then hit Manuel in the chest. A neighbor drove the student to a public hospital where he died that night. Witnesses of the event, including his brother, saw the policeman firing the gun and were quick to officially declare it when asked about the events of the night. The evidence accumulated and only a couple of days after the event the policeman behind the gunshot confessed that he took the UZI submachine gun, fired it with the goal of dispersing protesters, and "suspected" that he was the one causing the student's death (La Segunda, 2011).

In August 28, just three days after the shooting, the ballistic expert report determined that the bullet that killed the student came from an UZI submachine gun. The following day the report reached the press and it became the focus of the news. In August 30 of 2011, the General of the Police stated that "unfortunately, one of our people, in breach of all regulations, used his weapon when it did not correspond. He also tried to hide information, breaking another principle that is fundamental for the police, the truth" (own translation from Villarubia 2011). As a consequence, Miguel Millacura was detained the night of August 30, removed from the police, and put in custody. Eight other policeman were also removed from their jobs for hiding information.

[2]The events described in this section come from Tamayo (2015) who provides details about the student's life based on interviews with family members, friends, and neighbors.

III. Data

A. *Weekday protests and exposure to police violence*

We identified protests taking place in weekdays within the 2011, 2012, and 2013 academic years.[3] Data on the estimated number of people who attended each of these rallies comes from traditional media outlets such as *La Tercera* and *El Mercurio*, and from academic articles (CLACSO, 2012). These estimates were constructed using police reports, organizer reports, or using standard crowd-counting techniques based on aerial images (Fisher et al., 2019). Table A.1 provides a summary of the weekday protests to be analyzed. We restrict attention to protest days with more than 10,000 people, calculated as the average reported by police and organizers. This restriction leaves us with 12 protest days in 2011, three in 2012, and five in 2013 for a total of 20 protest days. Seven of these protests took place before the student was killed and 13 took place after this event.[4] As expected, the police reported fewer participants than the organizers, but the correlation between both is positive and statistically significant in the sample of 20 protest days (p-value<0.01).

Our population of interest are the 300,000 students enrolled in more than 2,000 schools in the city capital in 2011. This city is by far the most populated area in the country with almost half of the population (8 million) and hosted the largest protest events. In 2011, the students of interest were 14-18 years old and were enrolled in grades 8-12. Column 1 in Table 1 presents summary statistics for these students and their schools. The average student was born in 1995, attended school more than 91% of the time, and half are women. The average school served a total of 449 students, with 18% being from low-income families, and had 7 teachers per 100 students.

[Table 1 here]

We study the impact of police violence on two groups of students who were exposed to the shooting. The first group are the almost 750 schoolmates of the student killed by the stray bullet and we refer to them throughout the paper simply as "schoolmates." We also look at the subgroup of 200 schoolmates who were enrolled in the same grade as the student killed and we refer to them as "classmates": students with closer social links because they shared classes with the victim. Their school was located in a middle income urban area (Figure A.1). Column 2 in Table 1 presents summary statistics for the schoolmates and the characteristics of their school.

[3] The focus on weekdays is solely based on our interest in *school* skipping decisions. We omit weekday protest in January, February, July, and December because of school holidays.

[4] Note that most schools in Santiago – including the school of interest – were closed during the day of the shooting (August 25) when organizers counted more than 300,000 participants.

The second group is composed by students living nearby the shooting. To explore these "spatial effects," we geocoded administrative data with self-reported home addresses. We restricted attention to the 34,000 students who lived in the six municipalities that are contiguous to the location of the shooting. Unfortunately, the home address data is only available for students in grades 8-10, approximately 24,000 of the 34,000 students. Moreover, the home address was only reported by 13,000 students.[5] We follow Ang (2021) and say that the subset of students living closer than 0.5 miles from the shooting were exposed and we call them "neighbor students" or simply "neighbors." Column 4 in Table 1 shows the characteristics of students within 3 miles of the shooting and column 5 shows the characteristics of the 191 neighbor students in the analysis for whom we found a comparison student. The comparison group is discussed extensively below.

B. Daily school attendance and protests

We measure the protest behavior of student $i \in \mathcal{I}$ with an indicator that takes the value of one if student i skipped school in a weekday protest $t \in \mathcal{T}$. Administrative data on daily attendance is collected by the Ministry of Education for the purpose of allocating resources across schools (Cuesta et al., 2020). Since 2011 the daily data is available for the entire academic year, which in Chile goes from March through November, with a winter break in July. Previous research has shown that school skipping rates increased sharply in protest days (González, 2020). To ensure a skipping decision was made the school needs to be opened and hence we drop from the analysis the less than 5 percent of schools that were closed during the protest days we study.

We offer three empirical exercises to support the use of skipping decisions as protest behavior. First, skipping rates increased sharply on protest days: weekday protest school skipping was approximately 18% and in the same day without a protest on the week before or the week after was 11% (Figure A.4, panel A). Second, a higher school skipping rate is a strong predictor of protest size (Figure A.4, panel B).[6] The positive correlation is robust to the use of levels or logarithms and increases in magnitude when we include year fixed effects, indicating that the predictive power of school skipping holds across protests within a given year. School skipping and year effects explain more than 40-50 percent of the variation in protest size (columns 2 and 4), a strong predictive power considering that the number of protesters is probably measured with error.

For the third exercise, we estimated the number of high-school students in each protest using a crowd-counting method that exploits visual information in videos of the rallies. We downloaded

[5] Table A.2 shows that students reporting an address had higher school attendance, higher GPA, and were more likely to be females. Below we discuss the consequences of this selection.

[6] We use the average of protesters reported by the police and organizers. Figure A.2 and Table A.3 show that the correlation is strong and positive with each measure separately.

videos of the protests in our data from YouTube and selected 10 random images from the longest shots of each video to maximize coverage of attendees.[7] Then we asked college students – high-school students in 2011 – to count the number of high-school students in each image.[8] We obtained approximately 4,500 responses from 450 college students. Half of protesters appeared to have been high-school students (Table A.1). There is a strong correlation between the number of student protesters and skipping (Figure A.4, panel C; Table A.3, columns 5-6). To get a sense of the magnitude of this correlation, consider that a 10 percentage points increase in school skipping (30,000 students) is associated with 55,500 additional protesters (Table A.3, panel A, column 2) or 24,000 additional student protesters. This is, we calculate that 80 of every 100 students who skipped school decided to attend the rally (24,000 over 30,000). Given that skipping is also correlated with other forms of protest such as public manifestations outside of government buildings, we interpret the school skipping indicator as a broad measure of protest behavior.

IV. Econometric strategy

To estimate the impact of police violence on protest behavior, we use a difference-in-differences approach combined with a matching procedure to select the comparison group. The estimation relies on the inherent randomness of the stray bullet, both in terms of the affected students and the timing of the event. Given the presence of thousands of other students living in the same city, we use coarsened exact matching to select a group that we argue constitutes a valid counterfactual.

A. Selection of the comparison group

Schoolmates. The selection of the comparison group is based on a matching procedure that uses information before the shooting. The potential teenagers in this group are the 300,000 students aged 14-18 who lived in the city capital. The first step finds matches for the *school* using quintiles of enrollment and scores in a well-known standardized test. The former variable captures school size and the latter the socioeconomic background of students and school quality. When studying the schoolmates, this step decreased the number of schools from 2,000 to 122 and the number of students to 44,331. The second step finds *students* who were observationally equivalent in the following variables: seven school skipping indicators in the seven protest days before the event, exact grade (8-12), gender indicator, and quartiles of school attendance in the whole period before

[7] We collected 1.9 videos per protest. A video is composed by takes, and a take is characterized by its length. The average video has 39 takes, and the average take lasts 49 seconds. To construct the sample of images, we took random screenshots from takes which lasted more than 5 seconds.

[8] Figure A.3 provides details about the method. Note that high-school students are potentially recognizable because they wear school uniforms and are younger than the rest.

the event (March-August). Below we show that different combinations of these and additional variables, and the use of synthetic controls as alternative strategy, all deliver similar results. Operationally each student is assigned to a cell of observationally identical students. We obtain an estimating sample that reveals the school skipping decisions of 739 schoolmates and 21,810 other students in 416 cells. Column 3 in Table 1 shows some characteristics of the comparison group.

We highlight that the schoolmates of the victim constitute a particular set of individuals who differ from the average student population. Table 1 reveals that they attended a school with mostly male students and came from a relatively more privileged background. The comparison group was selected to construct a valid counterfactual for the schoolmates and as such also differs from the average student population. In the presence of heterogeneous treatment effects, these observable differences might hinder the generalizability of results to other subpopulations of students.

Neighbors. The potential controls for students who lived nearby the event are the 4,000 students who lived within 3 miles of the shooting and reported a valid home address in the survey where this information is available (i.e. street, number, and county) . We applied the matching procedure to the subset of 3,600 who lived between 0.5 and 3 miles from the shooting, which returns a total of 2,000 students enrolled in 228 schools. To avoid treatment externalities à la Miguel and Kremer (2003), we select as controls the subset of students who were enrolled in schools without neighbor students and drop those living within 0.5-1.5 miles from the shooting. The latter restriction leaves us with 191 neighbor students and 453 control students, classified in 93 cells, and who attended 199 schools.[9] Panel (b) in Figure A.1 plots the location of the neighbor students and the potential controls. Column 6 in Table 1 presents summary statistics for this comparison group of students.

B. Estimating equations

We begin by exploiting within student variation in school skipping decisions across the 20 weekday protests within the school calendar in 2011-2013. In particular, we estimate the following equation:

$$Y_{ijst} = \sum_{k=1}^{T} \beta_k \left(S_{j(i)} \times D_t^k \right) + \phi_i + \phi_{st} + \varepsilon_{ijst} \tag{1}$$

where Y_{ijst} is the skipping school indicator for student i, who is enrolled in school j, was assigned to cell s, and made her decision in day t. The equation includes a full set of student ϕ_i and cell-by-day ϕ_{st} fixed effects. The latter is a flexible source of unobserved heterogeneity which allows to use day-to-day variation within narrow groups of observationally identical students. The indicator $S_{j(i)}$

[9] As robustness check we use as controls *all* students within 0.5-3 miles from the shooting. We also explore the impact on those living nearby the home and the school of the student killed.

takes the value of one for schoolmates of the student killed and zero otherwise. In the geographical analysis, the indicator $S_{j(i)}$ takes the value of one for students who lived within 0.5 miles of the shooting. The indicators D_t^k take the value of one for each of the protest days after the event.[10] For estimation of this linear probability model, we follow Iacus et al. (2012) and use weights to account for the different number of treated and control students in each cell. The coefficients of interest are β_k and measure the differential skipping rates among the schoolmates/neighbors when compared to their respective comparison groups after the killing of the student.

We also use an augmented version with more structure in which we also exploit skipping decisions in non-protest days within the 2011 school calendar. We focus on 2011 to keep the sample of students fixed because some graduate or dropout of school after the end of that year. Beyond sample concerns, the motivation to use non-protest days is closely related to a placebo exercise. If there is a change in *protest* behavior, then we should *not* observe changes in skipping during days without protests, otherwise it raises concerns about a change in non-protest behavior, e.g. school skipping due to grief or school activities related to the killing and unrelated to protests. For this estimation we stack non-protests days to the protest days in the data and estimate:

$$Y_{ijst} = \gamma_1 \left(S_{j(i)} \times \text{Protest Day}_t \times \text{After}_t \right) + \gamma_2 \left(S_{j(i)} \times \text{After}_t \right) + \phi_i + \phi_{st} + \varepsilon_{ijst} \tag{2}$$

where all variables and estimation methods are defined as before and we include two additional indicators: "Protest Day$_t$" which takes the value of one for days with a protest and zero for non-protest days, and "After$_t$" which takes the value of one for the period after the student was killed. The coefficient γ_1 measures the differential skipping after the event during protest days, using non-protest days after the event as an additional dimension of comparison. In contrast, γ_2 measures the differential skipping after the event in non-protest days. Note that police shootings could have increased school absenteeism more generally (Ang, 2021), in which case we expect that $\gamma_2 > 0$.

C. Randomization inference

Student decisions are likely to be correlated within schools for multiple reasons. To account for this correlation, we begin by clustering standard errors by school. However, when we study the decisions of the schoolmates there is only one school in the treatment group. In the presence of few treated clusters the inference method derived from school-level heteroskedasticity can be invalidated by variation in school sizes (Ferman and Pinto, 2019). A recent method reveals that our analysis is likely to fall in this category (Ferman, 2021). Similarly, our geographic analysis has

[10] Note that similar indicators D_t^k for the period *before* the event cannot be included because the coarsened exact matching absorbs these and thus are implicitly included in the fixed effects ϕ_{st}.

to account for spatially correlated decisions. We now explain how we tackle these issues.

We use two inference methods to assess the statistical significance of social and spatial effects. In the former, we implement a three-step procedure based on randomization inference (Fisher, 1935; Young, 2019b). First, we assign the treatment to a *control* school, implement our econometric strategy and save the estimator. Second, we repeat the first step for each one of the 2,000 high schools in the data, leaving us with 2,000 estimators. And third, we compare the estimator of the school which actually experienced the shooting with the distribution of estimators from other schools to determine its statistical significance. We say the estimator is statistically significant at the 10% (5%) if it lies above the 90th (95th) percentile of the distribution of estimators, i.e. we compute Fisher's exact p-values (Imbens and Rubin, 2015). In the case of neighbors, we adopt a conservative approach and use standard errors clustered by school as Conley (1999) heteroskedastic and autocorrelation consistent errors are always smaller in magnitude.

V. The impact of police violence

A. Protest behavior

We begin with a descriptive analysis of protest behavior. Panels (a) and (b) in Figure 1 suggest that school skipping rates decreased among the schoolmates following the stray bullet event. For reference, note that a "business-as-usual" skipping rate has historically been between 8-10%. Therefore, skipping rates above 10% can be plausibly attributed to the protest. The lower school skipping rate is larger in the protests immediately after the shooting. In contrast, panel (c) reveals smaller differences between students who lived nearby the event and the comparison group.

[Figure 1 here]

Panels (d), (e), and (f) in Figure 1 present estimates of equation (1). The former two panels suggest that the stray bullet caused a temporary deterrence effect among the schoolmates. The largest impact of 12 percentage points lower skipping appears in the second to fourth protest days, i.e. one month after the student's death. Moreover, given that students in the comparison exhibited a skipping rate of 25-30%, the estimated change in school skipping corresponds to an economically significant decrease of 40-48%. This number is larger among the classmates, suggesting that social proximity is important. Panel (f) looks at students who lived nearby and results are weaker and not statistically different from zero. Table A.4 presents the corresponding regression coefficients.

The previous estimates reveal some differences in 2011 when compared to later years, which motivates a specification splitting these periods. We estimate a parametric version of equation (1) using the 20 weekday protests in 2011-2013. Table A.5 supports the hypothesis that part of the effect of police violence on protest behavior was somewhat transitory. More than half of the decrease in protest behavior is offset in 2012 (0.04/0.07 = 0.57). Combined with the dynamic coefficients in panels (c) and (d) of Figure 1, these results suggest that the effect of police violence slowly vanished after the shooting. We observe a similar pattern for the case of geographic proximity to the shooting but estimates are again smaller and statistically indistinguishable from zero.

The same empirical strategy applied to (i) less severe events of police violence and (ii) accidents or homicides of 14-18 years old, reveals a null impact on protest behavior (Tables A.8, A.9). These additional results suggest that our findings can be attributed to the combination of a killing coming from the police and not to any type of violence coming from the police or non-police killings.

[Table 2 here]

Table 2 presents estimates of equation (2), i.e. using a fixed sample of students. Column 1 focuses on non-protest days and reveals that the killing had zero impact on skipping in days without a protest. Column 2 stacks all protest and non-protest days in 2011 and columns 3-4 stack only one non-protest day for each protest. For the latter if a protest took place on a Thursday, we use skipping decisions from the Thursday of the week before (or after) without a protest.[11] Overall, the estimates in both panels reveal that schoolmates were between 6-8 pp. less likely to skip school in days of protest following the stray bullet, with exact p-values between 0.02 and 0.08, and the magnitude of the coefficient is 2 pp. larger when focusing on classmates (8-10 pp.), again suggesting social proximity matters, with exact p-values between 0.03 and 0.10.

B. The student-led boycott

Students boycotted one of the most important standardized tests in 2013, the SIMCE. This test had been used for almost two decades as a crucial metric in the educational system as it serves as an input to design educational policies, to inform parents about schools, and to track the performance of students (Cuesta et al., 2020). Although scores are never disclosed to students and the test does not have consequences for them, the metric had and continues to have many critics who argue that it incentivizes teaching to the test, it does not reflect school quality but rather the socioeconomic background of students, and it increases segregation in the system.[12] The tests had to be taken by

[11]The non-protest days we include from the week *before* are: May 5 and 25, June 9, August 2 and 11, September 7 and 15, October 11, November 11. The days we add from the week *after* are: May 19, June 8, August 16, September 21, October 6 and 25, November 25.

[12]Tests can introduce perverse incentives to change the metric by mechanisms different than improving educational performance (Figlio and Getzler, 2002; Kane and Staiger, 2002; Neal, 2013).

all twelve graders on November 20, 2013. One week before, student leaders of prominent schools called for a boycott which consisted in not taking the test, not answering the questions in the test, or to skip school and join a rally in the city's main square (Cooperativa, 2013).

We test for adherence to the boycott using administrative data on daily school attendance and test takers. The former allows us to measure the decision to skip school the day of the test and the latter reveals the decision of students to not take the test even if they were in the school that day. We focus on a narrow window of weekdays around the day of the test and construct a panel data of twelve graders observed daily. Then, we estimate the following equation:

$$Y_{ijst} = \sum_{k=1}^{T} \tau_k \left(S_{j(i)} \times D_t^k \right) + \phi_i + \phi_{st} + \varepsilon_{ijst} \tag{3}$$

where D_t^k is an indicator variable that takes the value of one for each day around November 20. All remaining variables and parameters are defined as before. We use two dependent variables: an indicator for students who decided to skip school, and an indicator for students who decided to skip the test. We define skipping the test as either skipping school or going to school but not taking the test. We use four days before and after the test. The parameter τ_k measures the differential adherence to the boycott of students exposed to police violence when compared to the matched sample of students. We again repeat the estimation for the schoolmates and the neighbors.

[Figure 2 here]

Figure 2 presents estimates of the linear probability model in equation (3). Panel (a) shows evidence consistent with a higher adherence to the boycott: the skipping rate of schoolmates increased by 8 pp. from a base of 13% (exact p-value of 0.12). Panel (b) employs our preferred measure of adherence and we find that participation in the boycott was twice as large among these students (26 versus 13%) with an exact p-value of 0.08. In contrast, panels (c) and (d) reveal a similar adherence to the boycott among students living nearby the shooting. Table A.11 presents the equivalent parametric estimates. In sum, we observe that in the long-run police violence increased the protest-related behavior of students who were socially close to the student killed.

C. Educational performance

Previous research has found negative effects associated with acts of police violence in the U.S. (Ang, 2021; Rossin-Slater et al., 2020) but evidence from other countries is scarce. We study educational performance as measured by GPA, dropout decisions, and the decision to take the college entry examination in the following years after the shooting. The college exam is by far the most important determinant of access to higher education in Chile (Aguirre and Matta, 2021) and thus one of the most consequential decisions young people make in their life (Altonji et al., 2012).

We begin the analysis by focusing on affected students and their corresponding comparison groups. In particular, we estimate the following cross-sectional regression equation:

$$Y_{ijs} \quad = \quad \delta S_{j(i)} + f(X_{ij}) + \phi_s + \varepsilon_{ijs} \tag{4}$$

where Y_{ijs} is an educational outcome of student i, enrolled in school j in 2011, and classified in cell s by the coarsened exact matching algorithm. The indicator $S_{j(i)}$ takes the value of one for schoolmates or neighbors and zero for the selected comparison group. The parameter δ measures the differential educational performance among students socially or geographically exposed to the shooting. Similar to the previous strategy we again include a full set of cell fixed effects ϕ_s, cluster standard errors at the school level, and use weights to account for the different number of treated and control students in each cell (Iacus et al., 2012). We also calculate Fisher's exact p-values and family-wise error rate (FWER) corrected p-values as suggested by Romano and Wolf (2005).

The selection of the comparison group exhibits two differences with respect to the previous estimation. First, we use cross-sectional variation instead of panel data due to the nature of the variation in the dependent variable which varies from year to year. Second, we include a non-parametric control $f(X_{ij})$ to account for differential before the shooting. This is an important aspect to consider given that our matching procedure guarantees a similar protest behavior between treated and control groups before the event, but it does *not* guarantee that the two groups were similar in terms of performance. For schools, we use test scores. For students, we use a non-parametric bin model for GPA in previous years. We also use an augmented coarsened matching that exploits the (partial) availability of individual-level test scores in a standardized test, which guarantees that students in treated and control groups had similar educational performance before the shooting.

Table 3 presents estimates of equation (4). We always use as a non-parametric control of predetermined performance a set of fixed effects for the ventiles of GPA, i.e. we always compare students who had a similar GPA in previous years. Columns 1-3 in panel A show that police violence is consistently associated with a lower performance among the schoolmates: we observe a persistent decrease in GPA of approximately 0.07-0.15 standard deviations (σ) and thus similar to the impact of 0.08σ found in the U.S. (Ang, 2021). Interestingly, the negative coefficient appears both in the analysis of the schoolmates and those who lived nearby, although estimates are noisier in the latter group. Columns 4-6 look at dropout decisions, which take the value of one when a student is *not* enrolled in a school in a given year and zero otherwise, and show that the probability that the schoolmates dropped out of high-school increased by 3-4 pp. from a base of 2% in the control group. These results are *not* statistically significant at conventional levels when using exact p-values but they are significant when using multiple hypothesis testing (p-value MHT) and therefore we interpret them as suggestive evidence. In contrast, column 7 shows that students affected by police violence were significantly less likely to take the college entry exam in the period 2011-2018, regardless of the inference method. In particular, their probability of taking the exam decreases by 29 pp., a large decrease from an average of 86% in the control group.[13]

[Table 3 here]

As robustness check, we re-estimated the impact on educational performance using the augmented matching that exploits standardized tests for a subsample of students. Table A.10 shows that point estimates and statistical significance are similar. In addition, panel (d) in Figure 3 shows the same patterns using combinations of the baseline variables to perform the matching. To explore heterogeneous effects based on social proximity, we use equation (4) and add an interaction term between $S_{j(i)}$ and an indicator for classmates. Table A.12 presents estimation results. The evidence is mostly inconclusive. Finally, estimates of the impact of police violence by enrollment grades in 2011 suggests that the negative consequences are far from vanishing over time (Table A.13). We conclude that police violence is weakly associated with negative educational performance and significantly associated with lower college enrollment.

D. Robustness to alternative specifications

The impact of the shooting on schoolmates is a robust finding and several exercises ease concerns about the effect of specification decisions we made. For example, similar results arise if we also match on *student* test scores or family income (Figure A.5). However, including more variables entails a trade-off because the number of treated students decreases substantially. Therefore, we implemented two other sets of alternative matching designs, one using different subsets of variables in the main estimation, and another using additional variables to address specific concerns.

Reassuringly, Figure 3 shows that 13 different combinations of the baseline variables deliver similar results. In these alternative specifications – specifications 2-14 in the x-axis, specification 1 is the baseline – we omit skipping indicators in even protest days before the shooting (specification 2), in odd protest days (3), each covariate separately (4-12), skipping in all protest days (13), and a last specification in which we only use grade as covariate (14). The estimates are also robust if we focus on the sample of non-dropouts (Table A.6) or exclude single protest days from the

[13] The school of the student killed was surprisingly closed in 2014. Column 8 shows that results are stronger when we flexibly control for the probability of school closure, suggesting that the event is unlikely to be confounding our estimates.

estimation (Figure A.6). Similarly, in a related set of robustness checks regarding the neighbors of the event we use the same strategy and show that the shooting had little impacts on students who lived nearby the home or the school of the student killed (columns 1-4 in Table A.7), and that the main result is unaffected if we include distance to La Moneda – seat of the incumbent President – as an additional covariate in the matching procedure (columns 5-6 in Table A.7).

[Figure 3 here]

We also complement the main analysis with five additional matching specifications which suggest that concerns related to omitted variables are unlikely to be driving our results. Figure 3 present these results and Table A.14 the corresponding coefficients and observations in the control group. Including student-level test scores (specification 15) or household income (specification 16) as additional matching covariates deliver very similar results, suggesting that differences in cognitive abilities or economic conditions are unlikely to explain our findings. Perhaps differences in the level of educational involvement of parents in both groups could drive our results. Specification 17 augments the set of variables in the matching with a self-reported measure of parental involvement, constructed directly from parents' questionnaires which we discuss in detail below. Reassuringly, results are again similar. Yet another concerns relates to observed differences in the share of low-income students across treated and control schools (Table 1, columns 2 and 3). Specification 18 shows that results are again the same when including this school-level variable in the matching algorithm. Finally, differences in the propensity to engage in protest behavior could affect our estimation. To address this concern, we create a school-level proxy of protest behavior during the 2006 high-school rallies using differences in school absenteeism rates between 2006 and 2005. In those rallies, high-school students protested with the goal of reforming the market-oriented nature of the educational system, the first and last significant protest wave in the 1990-2010 period. The results are again robust to this alternative matching specification.

Finally, two synthetic controls methods also support our findings. First, we implement the original method proposed by Abadie and Gardeazabal (2003) and Abadie et al. (2010). Second, we implement the recent synthetic difference-in-differences estimator suggested by Arkhangelsky et al. (2021) which allows for unit-level fixed effects and also puts more weights in similar periods before and after treatment. Reassuringly, we find similar results for both methods (Figure A.7).

VI. Mechanisms

Several mechanisms can explain why students close to the victim changed their protest behavior. First, different information about the shooting and its impact on beliefs. Second, different emotions of fear and anger after experiencing an officer-related killing. Third, the role of parents, who could have exerted effort to protect or persuade students. The results in this section suggest that social-psychology mechanisms related to emotions are the most likely explanation for our findings.

A. Information and memory

The first mechanism which could explain our findings is related to differential information and memory of the shooting, both of which are likely to affect individual beliefs about the perceived cost of protesting (Becker, 1968; Young, 2019a), the probability of success, or perceptions about the government type (Lohmann, 1994; Pierskalla, 2010). Although media outlets covered the event, geographic proximity could have facilitated learning or memory of the shooting due to contextual reasons (Fujita et al., 2006; Enke et al., 2021). More precisely, we posit that the neighborhood acts as the context which affects associative memory and creates imperfect recollections of the shooting among partially naïve students (Mullainathan, 2002; Bordalo et al., 2021).

Two pieces of evidence suggest asymmetric information or memory are unlikely to explain our findings. First, the police involvement in the gunshot appeared all over media outlets. An internet search of news articles with the query "Manuel Gutierrez" between August 25 and the next weekday protest (September 14) returns articles from the leading newspapers (*El Mercurio*, *La Tercera*), leading online media (e.g. *La Segunda*, *El Mostrador*, *Biobio*), and leading radios stations (e.g. *Cooperativa*, *ADN*), media sources with remarkably different political leanings. The articles are explicit about the role of the police: "the bullet that killed Manuel Gutierrez was a police gunshot according to expert reports" (August 29, 2011); "the policeman confessed he fired the UZI submachine gun" (August 31, 2011), among many other examples.

The availability of news dampens but does not prevent the existence of differential information. Media consumption and retention of local news are endogenous processes which we conjecture are related to the neighborhood. A student who read an article online and lived one block from the shooting was likely to update his beliefs about police violence more than a student who lived farther away. Therefore, the second piece of evidence against information as a mechanism relates to the lack of a differential impact on student neighbors: Tables A.5 and A.11 show that protest behavior does *not* change with distance to the shooting. Moreover, given that all schoolmates were likely to be equally informed, the systematically larger impact of the shooting on classmates suggests that other mechanisms are relatively more important.

B. Social-psychology

Additional results suggest that a social psychology mechanism is likely to be important to understand the collection of findings. If emotions such as fear or anger are the mediating factors, we expect the change in protest behavior to be more pronounced among students who were emotionally closer to the victim of police violence. A social tie to a victim can trigger both fear and anger, with the latter sometimes out weighting the former and leading to "backlash protest" (Aytac et al., 2018). The link between emotions and risky behavior is traditionally explained by changes in risk assessment derived from emotions (Lerner and Keltner, 2001; Druckman and McDermott, 2008; Card and Dahl, 2011; Campos-Vazquez and Cuilty, 2014).

In the context of our study, we use the fact that social interactions – and the associated positive or negative emotions – are stronger among students *within* the same grade rather than *across* grades (González, 2020). Moreover, the structure of within school interactions implies that the farther away from the grade of the victim (11th grade) the larger the social distance: 11th graders were emotionally closer to the victim than 10th graders, 10th graders were closer than 9th graders, and 9th graders were closer than 8th graders. A strong implication of this conjecture is that the impact should follow a kink pattern with the largest impact on 11th graders.

To test for this mechanism, we perform a sub-sample analysis which flexibly estimates heterogeneous responses across students within the school of the victim. We implement this exercise for the short-run protest behavior because we observe students from all 8-12 grades in the same year. The results indeed support the existence of a kink and suggest that students in the same grade decreased their protesting behavior by more than other schoolmates. Table 4 presents the results. Strikingly, the impact of police violence on 11th graders is larger than the impact on *both* 12th graders and 10th graders. In fact, the impact on 11th graders is two-and-a-half times larger than the impact on 8th graders and one-third larger than the impact on 12th graders. We find a similar but statistically noisier pattern when studying the probability of taking the college exam across graduation years (Table A.13). The monotonically decreasing effect with respect to the grade of the student killed suggests that a social-psychology mechanism is important to explain the findings.

[Table 4 here]

C. Parental involvement

Can the role of parents explain our collection of findings? Additional results suggest they had little role to play. If parents differentially reacted to the shooting, then the interpretation of results would be more closely related to parental behavior in the fear of danger rather that protest behavior. More precisely, we worry about potential unobservable parental traits driving differences in protest behavior. Given the suggestive evidence of similar information, another important trait is their propensity to get involved in schooling decisions. These unobservables could explain school skipping in protest days. At the same time, parents might not care about participation in the test boycott because danger could have been perceived as lower within the school.

To empirically assess the role of parents, we use survey data from questionnaires implemented during days of standardized tests. Operationally, we create a predetermined covariate related to parental involvement in schooling decisions using their responses to four survey questions. Crucially, the survey was designed to be able to link these responses to students. Three questions asked whether parents knew about the existence of document X in the school, where X stands for "rules of procedure," "school-level annual goals," and "school's educational project." A fourth question asked parents if individual educational achievement was ever disclosed. Parents could choose among "YES," "NO" and "DON'T KNOW" answers. We created an indicator that takes the value of one if the parent responded differently from "DON'T KNOW" and zero otherwise and then took the average of the four answers for each student (avg. 0.88, st. dev 0.20).

We use our measure of parental involvement to reestimate our main econometric models adding this covariate in the matching procedure. The enhanced model guarantees that we are comparing students with similar types of parents. The results are presented in Figure 3 (specification 17) and Table A.14 (column 17). As can be seen from these coefficients, all results remain similar.

VII. Conclusion

We have shown that high school students who were in close social proximity to a student that was killed by a police gunshot experienced a transitory decrease in their protest behavior and lower college enrollment. In contrast, students living nearby the event appeared to be unaffected in these dimensions, suggesting that social proximity to the student killed and the associated psychological mechanisms are likely to be important mediating factors. The lack of a persistent effect on protest behavior is particularly notable given that we have studied an extreme event of police violence. In this sense, we conjecture that any other form of police-related violence is likely to have smaller impacts on protest behavior. Similarly, we also expect other forms of police violence to have smaller educational impacts. However, given that police officers are involved in many different types of aggressive behavior towards protesters, the negative educational consequences we have documented arguably constitute a lower bound of the social cost of police violence.

Our analysis has benefits and limitations. Among the benefits is that the actions of students are well documented and easy to track over time. The measurement of protest behavior for thousands of individuals across multiple days using administrative data is unusual. However, one limitation is that high school students are still in their formative years and thus they might be particularly sensitive to police violence. As such, we hypothesize that the impact on adults could be smaller. Similarly, our results are specific to the high-school students affected by the shooting who come from a relatively more privileged background. We highlight that the generalizability of results likely depends on individual characteristics which might differ across subpopulations (e.g. risk aversion). Relatedly, our focus on one salient act of police violence has the benefit of being precisely defined, but violent events can be heterogeneous and have different impacts. The study of an extreme event such as the death of a student allows us to perhaps interpret our findings as a bound.

Finally, we believe that our results illuminate many possible avenues for future research. From a policy perspective, one of the most important questions is related to the overall effectiveness of police violence. Our findings emphasize that any action coming from the police needs to be implemented in a way that minimizes its negative spillovers. Confrontations between the police and protesters have become more common particularly in countries experiencing more polarization, making this question of particular importance. Possible policies include the use of cameras to held policeman accountable or bans to the use of projectiles such as pellet guns. A rigorous evaluation of these alternative policies is an important area of future research.

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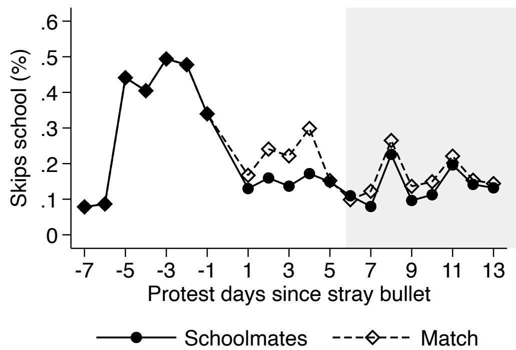
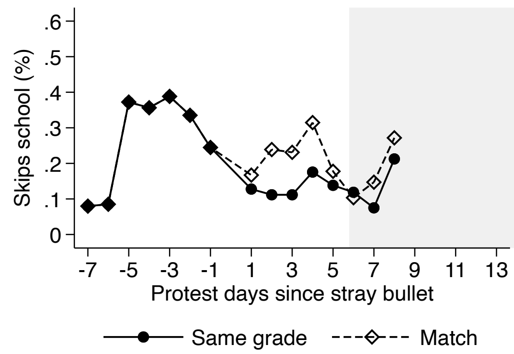
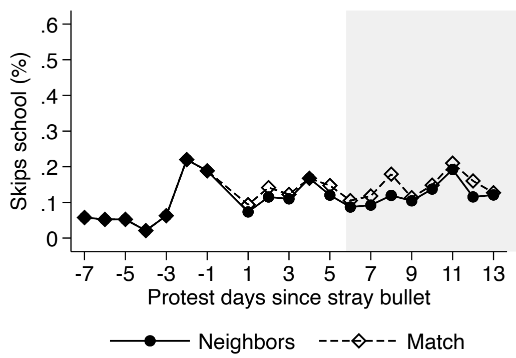
Figure 1: School skipping in weekday protests before and after the shooting



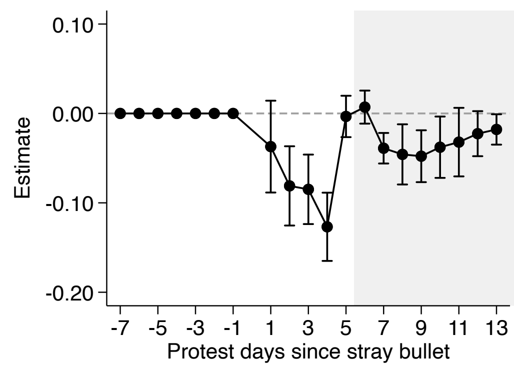
(a) All schoolmates



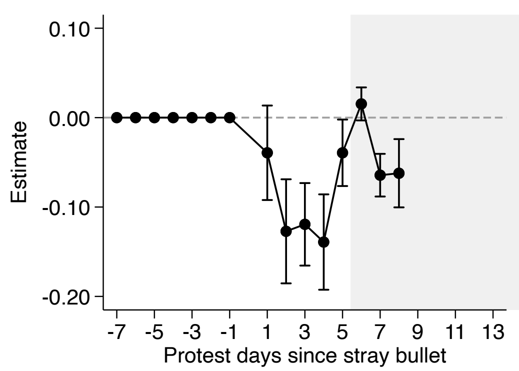
(b) Classmates



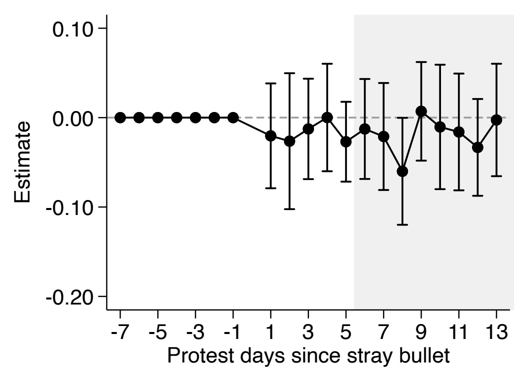
(c) Neighbor students



(d) All schoolmates



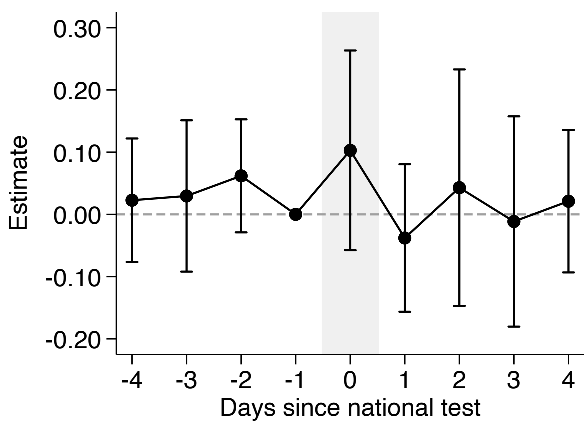
(e) Classmates



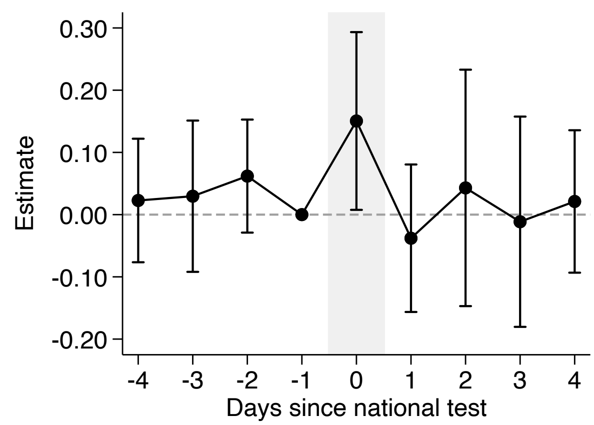
(f) Neighbor students

Notes: Panels (a) and (b) present the average school skipping rate among the schoolmates of the student killed ("Schoolmates" and "Same grade") and a selected comparison group ("Match") during weekday protests in 2011 (white area) and 2012-2013 (gray area). Panel (c) repeats the exercise but looking at students who lived within 0.5 miles of the exact place where the student was killed ("Neighbors") and a selected comparison group during weekday protests in 2011-2013. Panels (d), (e), and (f) present event study estimates that reveal the differential protest behavior across groups with the corresponding 95 percent confidence interval for each estimate. Note that the vast majority of "Classmates" graduated in 2012 and thus we do not observe them in 2013.

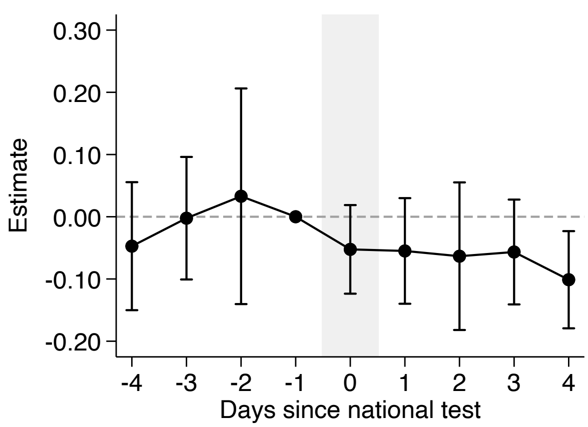
Figure 2: Student-led boycott



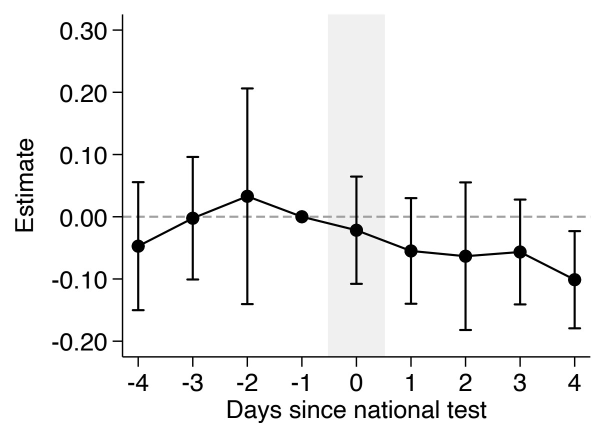
(a) Skips schools, schoolmates



(b) Skips test, schoolmates



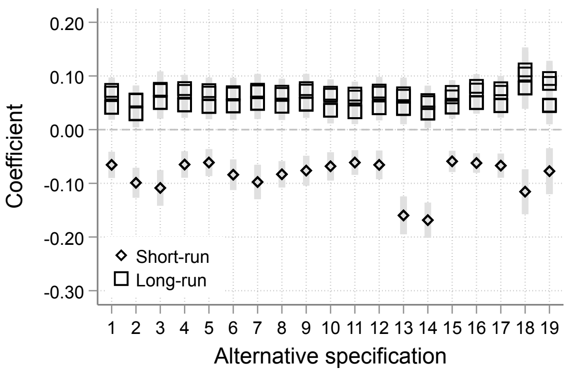
(c) Skips schools, neighbor students



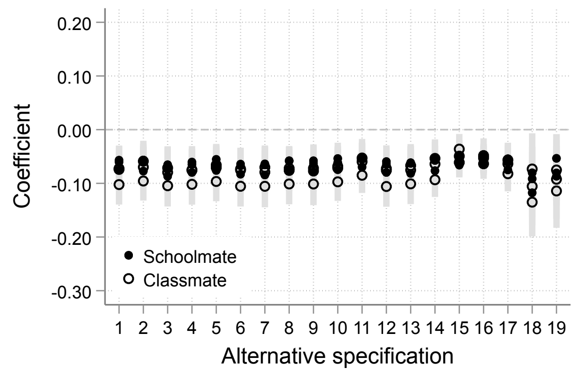
(d) Skips test, neighbor students

Notes: Event study estimates of the differential adherence to the student-led boycott among schoolmates/neighbors exposed to police violence when compared to their matched set of students. The boycott consisted in not taking a well-known standardized test that is used to implement public policies and measure the educational performance of students and schools. Black dots represent point estimates and vertical lines the 95% confidence interval. The *y*-axis measures the differential attendance of schoolmates/neighbors in percentage points and the *x*-axis weekdays around test day. The omitted category is the day before test day.

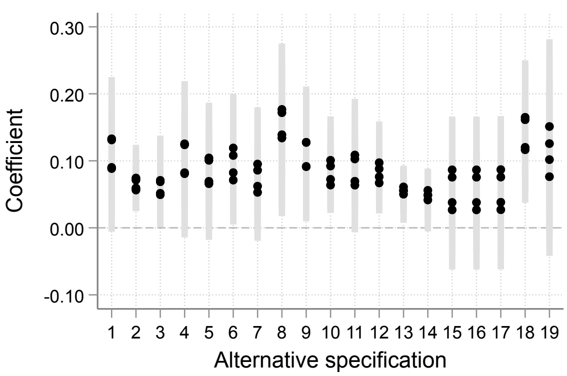
Figure 3: Robustness to alternative specifications of the matching



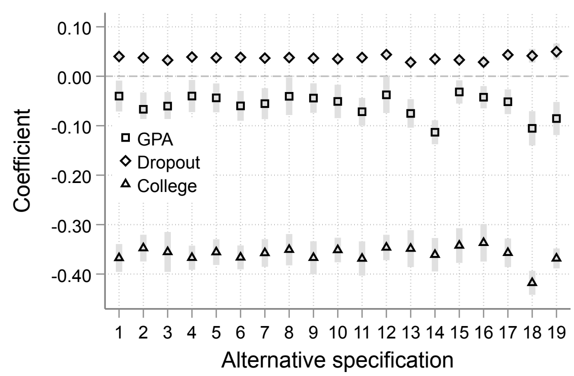
(a) Protest results in Table A.5



(b) Short-run protest results in Table 2



(c) Boycott results in Table A.11



(d) Educational results in Table 3

Notes: The estimated impacts of the police shooting (y-axis) are robust to 18 alternative specifications (x-axis, specification 1 is the baseline result). These alternative specifications omit skipping indicators in even protest days before the shooting (specification 2), in odd protest days (3), each covariate separately (4-12), skipping in all protest days (13), and use grade as the only matching covariate (14). Specifications 15-19 add the following predetermined variables in the matching algorithm: student-level test scores (15), household income (16), parents' educational involvement (17), percentage of low-income students in the school (18), and a measure of school-level protest behavior in the 2006 student rallies (19). Each robustness exercise is performed on *all of the results* in the corresponding table: 8 coefficients in Table A.5 (4 short- and 4 long-run), 6 in Table 2 (3 schoolmates and 3 classmates), 4 in Table A.11, and 3 in Table 3 (GPA, dropout, and college). The vertical gray lines represent 95 percent confidence intervals.

Table 1: Summary statistics for students and school in the analysis

	Social proximity			Geographic proximity		
	All	Schoolmates	Matched sample	Within 3 miles	Neighbors	Matched sample
Panel A: Students	(1)	(2)	(3)	(4)	(5)	(6)
School attendance < Aug' 2011	0.88	0.85	0.86	0.91	0.91	0.92
	(0.14)	(0.16)	(0.14)	(0.11)	(0.12)	(0.12)
Share female	0.51	0.11	0.11	0.48	0.46	0.46
	(0.50)	(0.31)	(0.31)	(0.50)	(0.50)	(0.50)
School attendance in 2010	0.91	0.90	0.91	0.93	0.94	0.93
	(0.12)	(0.12)	(0.09)	(0.08)	(0.05)	(0.09)
Year of birth	1995	1995	1995	1996	1996	1996
	(2)	(1)	(1)	(1)	(1)	(1)
GPA in 2010	5.3	5.1	5.3	5.4	5.5	5.4
	(0.8)	(0.8)	(0.6)	(0.7)	(0.6)	(0.7)
Total number of students	303,797	739	21,810	3,950	191	453
Panel B: Schools						
Students enrolled	449	1,074	1,315	880	958	912
	(504)		(557)	(647)	(686)	(633)
Average test score	257	280	294	269	271	270
	(25)		(10)	(23)	(19)	(25)
Share low-income students	0.18	0.07	0.14	0.13	0.12	0.15
	(0.19)		(0.10)	(0.13)	(0.12)	(0.13)
Teachers per student	0.07	0.05	0.04	0.05	0.05	0.05
	(0.07)		(0.01)	(0.02)	(0.02)	(0.02)
Total number of schools	2,179	1	122	317	44	155

Notes: This table presents averages and standard deviation of pre-determined covariates at the student and school level. The variables in italics are used as inputs for the coarsened exact matching algorithm, but we check for the robustness of results to a wide range of specifications. School attendance < Aug' 2011 in panel A captures school attendance before the shooting (August 25, 2011). The group of "Schoolmates" and "Neighbors" are the students exposed to police violence in the analysis of social and geographic proximity respectively. The matched sample are the students chosen by the matching algorithm as the comparison group.

Table 2: School skipping decisions in protest and non-protest days

	Dependent variable: Indicator for school skipping			
	All non-protest days		One non-protest day	
Panel A	Without protest days	With protest days	Week *before*	Week *after*
	(1)	(2)	(3)	(4)
Schoolmate × After × Protest day		-0.08	-0.06	-0.06
		(0.01)	(0.01)	(0.01)
		[0.02]	[0.08]	[0.08]
Schoolmate × After	0.001	0.001	-0.003	-0.001
	(0.004)	(0.004)	(0.006)	(0.007)
	[0.42]	[0.41]	[0.54]	[0.55]
Observations	3,057,570	3,328,163	454,301	388,953
Students	22,544	22,549	22,549	22,549
Average dependent variable	0.11	0.13	0.25	0.27
Student fixed effects	Yes	Yes	Yes	Yes
Cell-day fixed effects	Yes	Yes	Yes	Yes
Panel B				
Classmate × After × Protest day		-0.10	-0.08	-0.08
		(0.02)	(0.02)	(0.02)
		[0.03]	[0.10]	[0.10]
Classmate × After	-0.001	-0.002	-0.007	-0.014
	(0.005)	(0.005)	(0.008)	(0.008)
	[0.57]	[0.56]	[0.53]	[0.49]
Observations	678,995	739,298	100,675	86,810
Students	5,022	5,025	5,025	5,025
Average dependent variable	0.11	0.12	0.21	0.22
Student fixed effects	Yes	Yes	Yes	Yes
Cell-day fixed effects	Yes	Yes	Yes	Yes

Notes: Each observation corresponds to a skipping school decision of a high-school student in one of the twelve protest days and additional non-protest days, all within the 2011 school year. Estimation using different specifications of linear probability models. Panel A uses *all* non-protest days in the 2011 school year and panel B only includes a single non-protest day from the week before each of the twelve protest days. Standard errors are clustered at the school level in parentheses and p-values from randomization inference in square brackets.

Table 3: The impact of police violence on educational performance

	GPA			Dropout			Ever takes college exam (2011-2018)	
	2011	2012	2013	2011	2012	2013		
Panel A	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Schoolmate	-0.04	-0.08	-0.14	0.04	0.03	0.04	-0.29	-0.37
	(0.02)	(0.02)	(0.01)	(0.00)	(0.00)	(0.00)	(0.02)	(0.01)
	[0.36]	[0.32]	[0.21]	[0.10]	[0.25]	[0.17]	[0.03]	[0.03]
Students	22,108	18,033	13,221	22,108	18,033	13,221	22,442	22,442
Average dependent variable	5.28	5.36	5.41	0.03	0.04	0.03	0.86	0.86
p-value MHT	0.07	0.03	0.02	0.02	0.05	0.03	0.01	0.01
Panel B								
Neighbor student	-0.05	-0.10	-0.08	-0.01	-0.03	-0.01	0.04	0.03
	(0.05)	(0.05)	(0.07)	(0.02)	(0.02)	(0.01)	(0.04)	(0.05)
Students	637	632	623	637	632	623	634	624
Average dependent variable	5.35	5.32	5.39	0.04	0.06	0.04	0.78	0.79
p-value MHT	0.70	0.37	0.67	0.72	0.70	0.72	0.72	0.72
Cell fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Ventiles of past GPA fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Ventiles of Pr(closure) fixed effects	No	No	No	No	No	No	No	Yes

Notes: Each observation corresponds to the educational outcome of a student. Cross-sectional estimates compare the educational performance of students exposed to police violence with a selected comparison group. We estimate the probability of school closure in column 8 "Pr(closure)" using cross-sectional probit regression with data from 2010 and before: we empirically predict an indicator for schools that were closed on a LASSO-selected vector of changes in enrollment and other school characteristics and use the estimated model to assign the predicted probability of closure to each school in our sample and include a non-parametric control for ventiles of this probability. Standard errors are clustered at the school level in parentheses and *p*-values from randomization inference in square brackets. The bottom of each panel also presents the family-wise error rate (FWER) corrected *p*-values (*p*-value MHT) as suggested by Romano and Wolf (2005).

Table 4: Police violence and protest behavior by social distance to the victim

	Dependent variable: Indicator for school skipping				
Grade in 2011:	8th	9th	10th	11th (victim's grade)	12th
	(1)	(2)	(3)	(4)	(5)
Schoolmate × After student killed × Protest day	-0.041	-0.054	-0.068	-0.104	-0.076
	(0.011)	(0.013)	(0.018)	(0.019)	(0.015)
	[0.18]	[0.21]	[0.17]	[0.03]	[0.21]
Schoolmate × After student killed	-0.030	0.001	0.006	-0.002	0.003
	(0.008)	(0.006)	(0.009)	(0.005)	(0.007)
	[0.10]	[0.39]	[0.34]	[0.56]	[0.40]
Observations	651,167	692,689	674,608	739,298	570,328
Students	4,289	4,700	4,580	5,025	3,955
Student fixed effects	Yes	Yes	Yes	Yes	Yes
Cell-day fixed effects	Yes	Yes	Yes	Yes	Yes
Average of dependent variable	0.103	0.105	0.151	0.122	0.135

Notes: Each observation corresponds to a skipping school decision of a high-school student in one of the twelve protest days and non-protest days, all within the 2011 school year. Estimates of linear probability models. Standard errors are clustered at the school level and we present Fisher's exact p-values in square brackets.

ONLINE APPENDIX

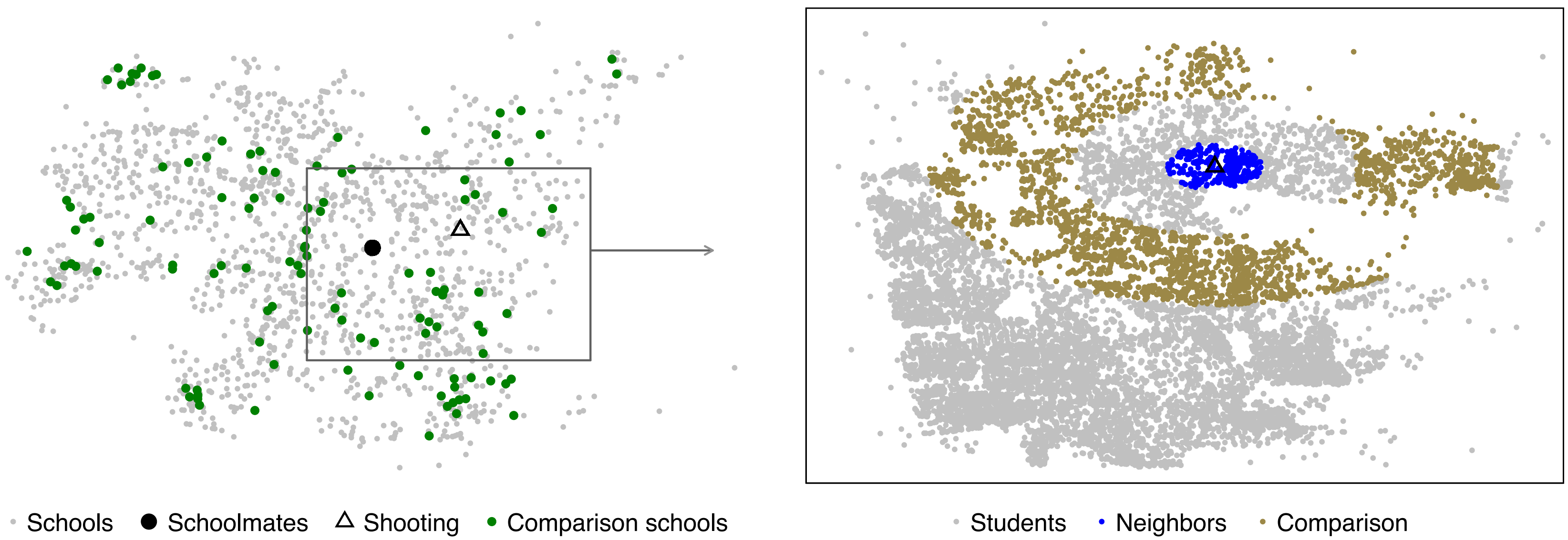
Police Violence, Student Protests, and Educational Performance

Felipe González *Mounu Prem*

List of Figures

List of Tables

Figure A.1: Schools and students in the analysis

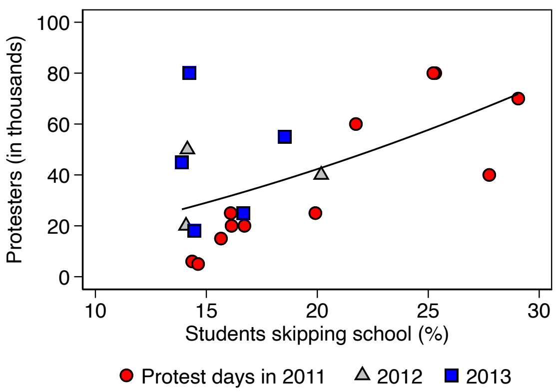


(a) Schools

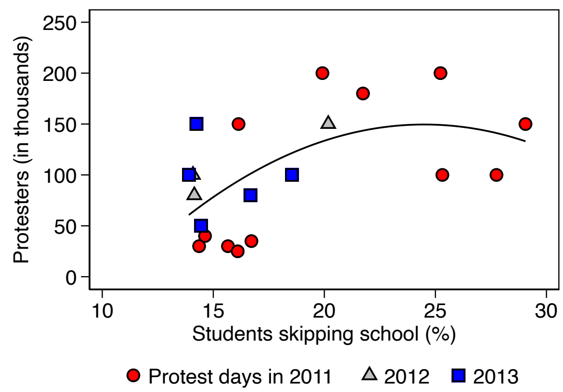
(b) Neighbor students and comparison group of students

Notes: Panel (a) shows the location of all schools in the city we study with the schools in the estimating sample highlighted in green. The school of the student killed is shown as a black circle and the location of the shooting in a black triangle. We also marked the selected area (black hollow square) to study spatial spillovers. Panel (b) shows the location of students in the sample, highlighting the ones who were geographically exposed to the shooting (in blue) and the comparison group of students (in brown).

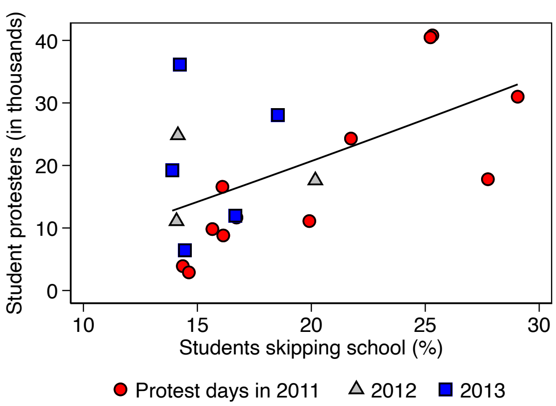
Figure A.2: School skipping is robustly related to the number of protesters



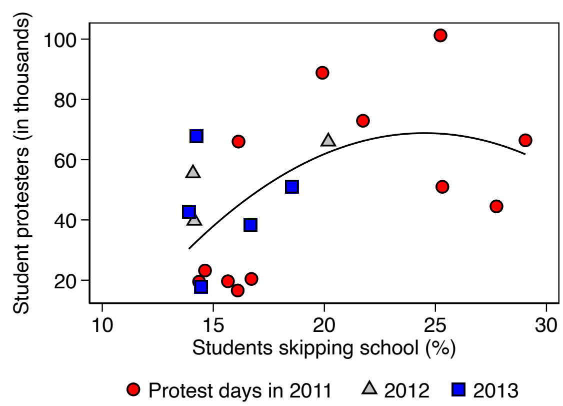
(a) Police reports: Protesters



(b) Organizer: Protesters



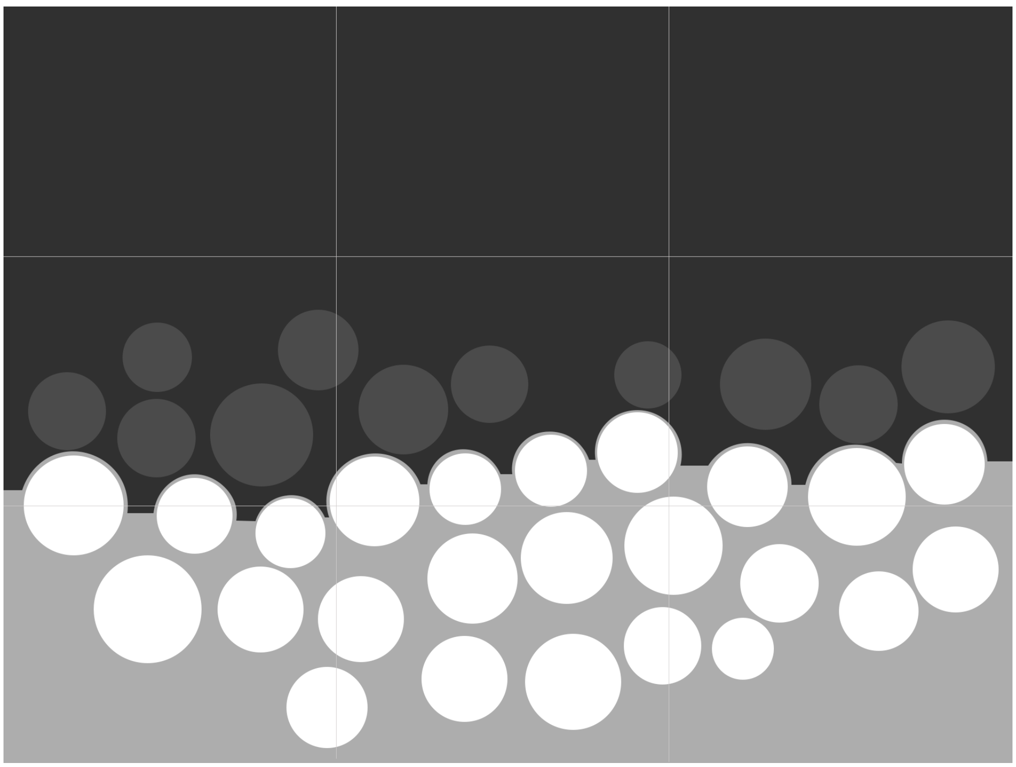
(c) Police reports: Student protesters



(d) Organizer: Student protesters

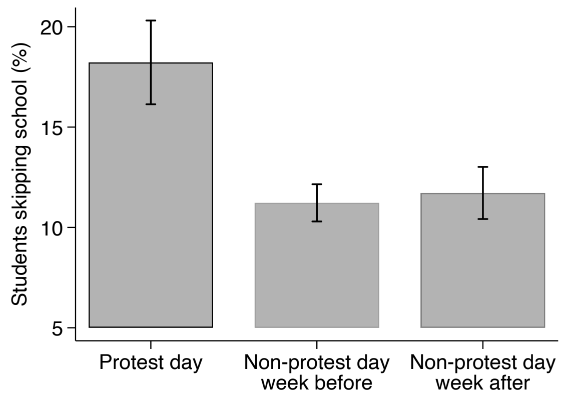
Notes: Own construction using data from police and organizer reports. These figures present the partial correlation between the percentage of high-school students skipping school and the total number of protesters (Panels A and B), and the partial correlation with student protesters (Panels C and D). The number of student protesters was calculated using online surveys and videos of rallies.

Figure A.3: Details about crowd count of high-school students

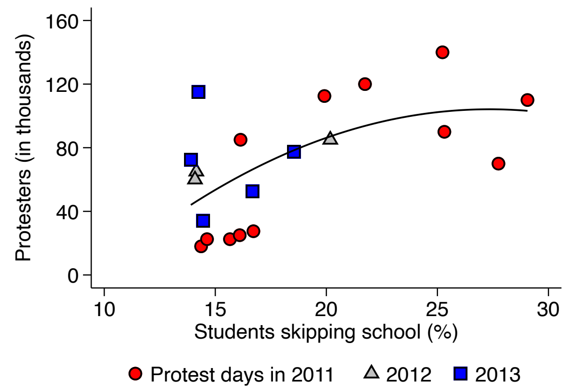


Notes: This figure presents the sketch of an image, where a crowd is identifiable in the front, and a non-identifiable crowd is located in the back. The classification of the image into identifiable and non-identifiable areas was done by a research assistant who was unaware of the goal of this exercise. We asked 450 college students to count the number of high-school students in the front of the image and with those responses we take the average across images within a protest and calculate the share of high-school students among protesters.

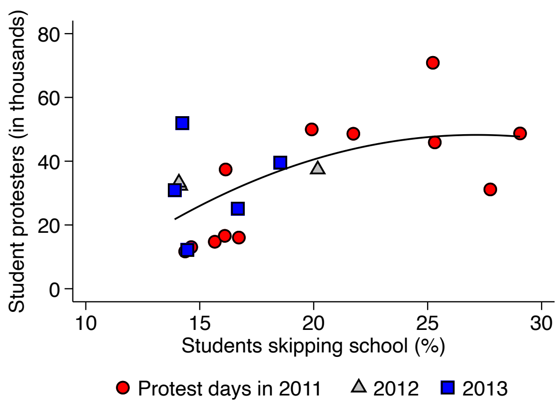
Figure A.4: School skipping and protesters



(a) School skipping and protest days



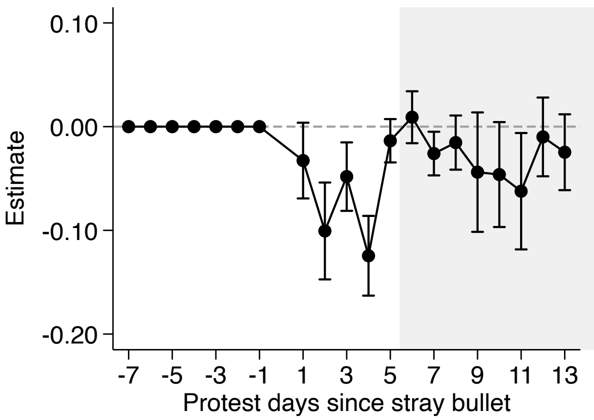
(b) School skipping and protesters



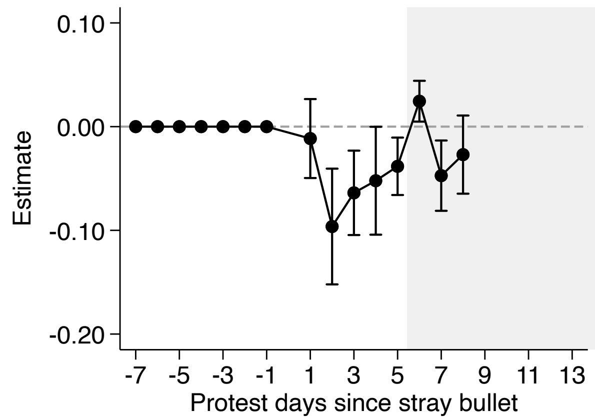
(c) School skipping and student protesters

Notes: Panel (a) shows that the average school skipping rate in protest days is 18.22 with a 95% confidence interval [16.14, 20.31] and the average in non-protest days are 11.23 and 11.72 the week before and the week after. The difference in means between protest and non-protest days is statistically significant with a p-value< 0.01. Panels (b) and (c) present the partial correlation between the percentage of high-school students skipping school and the total number of protesters, and student protesters respectively. The number of student protesters was calculated using online surveys and videos of rallies.

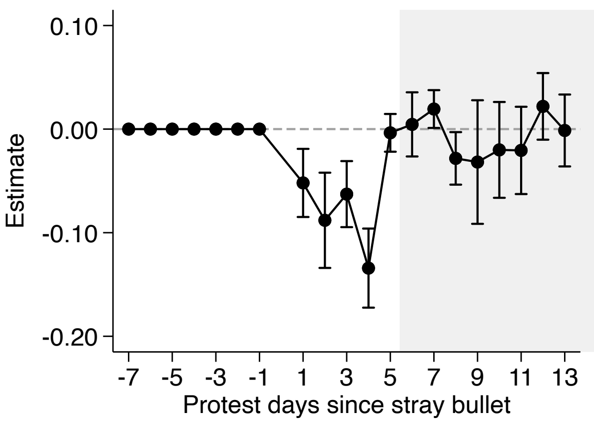
Figure A.5: Alternative matching strategies with additional covariates



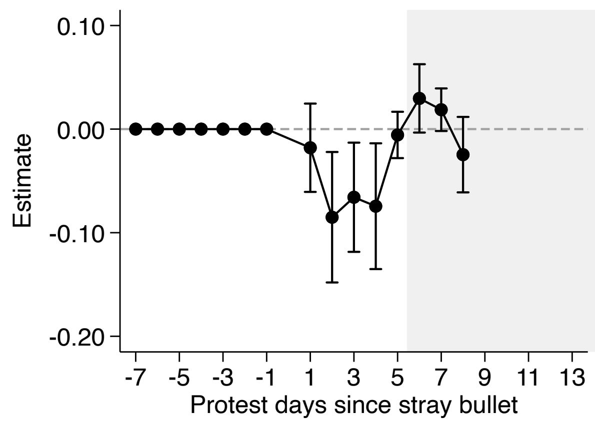
(a) All schoolmates, matching #2



(b) Classmates, matching #2



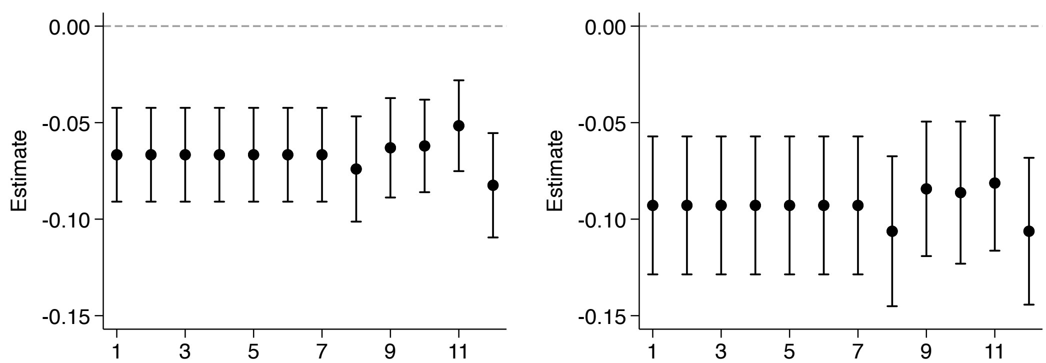
(c) All schoolmates, matching #3



(d) Classmates, matching #3

Notes: Estimates of equation (1) using daily school attendance data from the 2011-2013 academic years. The *y*-axis measures the differential change in school skipping rates among schoolmates of the student killed when compared to a sample of students that were observationally identical before the event. Note that the vast majority of "Classmates" graduated in 2012 and thus we do not observe them in 2013. Matching #2 uses the baseline predetermined variables plus standardized test scores for students. Matching #3 uses baseline predetermined variables, plus standardized tests for students and terciles of reported family income. These alternative matching strategies deliver similar results at the cost of decreasing the number of students who were socially close to the student killed. Vertical lines denote 95 percent confidence intervals calculated using standard errors clustered at the school level.

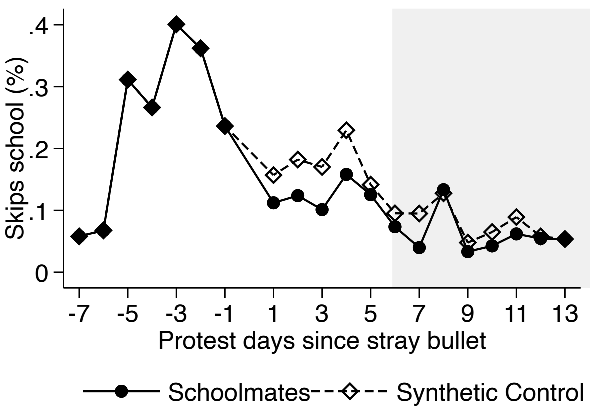
Figure A.6: Robustness of deterrence results when omitting single protest days



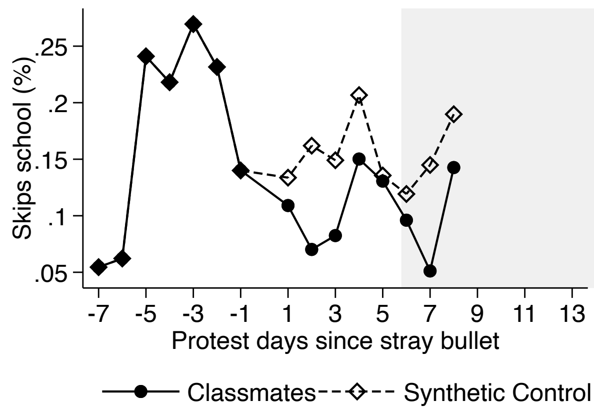
(a) All schoolmates (b) Classmates

Notes: Estimates of the parametric version of equation (1) with the corresponding 95% confidence interval. Each estimate comes from an estimation in which we drop one of the 12 protest days in 2011.

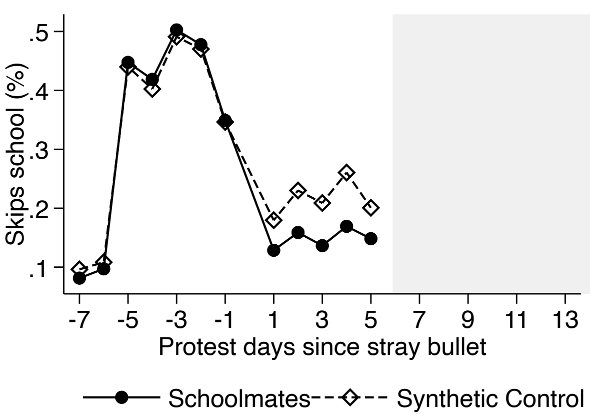
Figure A.7: Synthetic control estimates



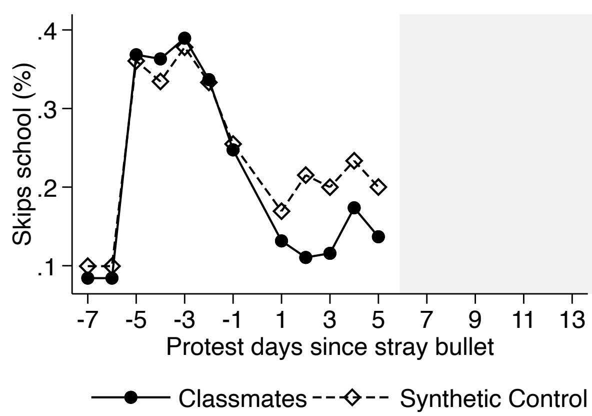
(a) Schoolmates (Abadie and Gardeazabal, 2003)



(b) Classmates (Abadie and Gardeazabal, 2003)



(c) Schoolmates (Arkhangelsky et al., 2021)



(d) Classmates (Arkhangelsky et al., 2021)

Notes: Synthetic control estimates for the impact of the stray bullet on protest behavior. The unit of observation is a high-school student in the 2011-2013 period. Panel (a) and (c) construct the counterfactual for all schoolmates of the student killed and panels (b) and (d) for the subset of schoolmates who were enrolled in the same grade ("classmates"). In both of these cases we use high school students in the same city and school skipping on weekday protests within the school calendar before the event to construct the counterfactual. In panels (a) and (b), we implement the original method proposed by Abadie and Gardeazabal (2003) and Abadie et al. (2010). In panels (c) and (d), we implement the recent synthetic difference-in-differences estimator suggested by Arkhangelsky et al. (2021) which allows for unit level fixed effects and also puts more weights in similar periods before and after treatment. Note that the vast majority of "classmates" graduated in 2012 and thus we do not observe them in 2013. The gray area denotes the years 2012 and 2013.

Table A.1: Weekday protests within the school calendar, 2011-2013

Year	Month	Day	Estimated number of protesters in the rally: By police	By organizers	High-school students	Day of week
(1)	(2)	(3)	(4)	(5)	(6)	(7)
2011	May	12	15,000	30,000	65%	Thursday
	June	1	20,000	35,000	58%	Wednesday
		16	80,000	100,000	51%	Thursday
		23	25,000	25,000	66%	Thursday
		30	80,000	200,000	51%	Thursday
	August	9	70,000	150,000	44%	Tuesday
		18	40,000	100,000	44%	Thursday
	September	14	6,000	30,000	65%	Wednesday
		22	60,000	180,000	41%	Thursday
		29	20,000	150,000	44%	Thursday
	October	19	25,000	200,000	44%	Wednesday
	November	18	5,000	40,000	58%	Friday
2012	April	25	50,000	80,000	50%	Wednesday
	May	16	20,000	100,000	55%	Wednesday
		28	40,000	150,000	44%	Thursday
2013	April	11	80,000	150,000	45%	Thursday
	June	13	45,000	100,000	43%	Thursday
		26	55,000	100,000	51%	Wednesday
	September	5	25,000	80,000	48%	Thursday
	October	17	18,000	50,000	36%	Thursday

Notes: Own construction using police records, organizer reports, and data from newspapers. Please note that our use of school attendance data prevents us from considering weekday protests in January, February, July, and December because of the summer and winter breaks. In column 6 we calculate the percentage of high-school students in each of these protests using a crowd-counting method in which college students responded online surveys to count the number of high school students in randomly selected images of protest videos.

Table A.2: Differences across students with and without a valid home address

	With valid home address	Without (or invalid) home address	Difference (1) - (2)
	(1)	(2)	(3)
Avg. school attendance until August 2011	0.91	0.88	0.03
	(0.10)	(0.15)	(0.002)
Avg. school attendance in 2010	0.93	0.91	0.02
	(0.08)	(0.14)	(0.002)
Indicator female	0.51	0.48	0.03
	(0.50)	(0.50)	(0.006)
Year of birth	1996.1	1996.1	0.07
	(1.0)	(1.2)	(0.015)
GPA in 2010	5.43	5.21	0.22
	(0.63)	(0.90)	(0.010)
Students	13,376	10,712	

Notes: Columns 1 and 2 present the mean and standard deviation in parenthesis. Column 3 presents the difference and the standard error in parenthesis.

Table A.3: School skipping and number of protesters

	Dependent variable is:					
	Protesters (in thousands)		Log protesters		Log student protesters	
Panel A	(1)	(2)	(3)	(4)	(5)	(6)
Percentage of students skipping school	4.38	5.54	0.07	0.10	0.06	0.08
	(1.45)	(1.51)	(0.02)	(0.02)	(0.02)	(0.02)
R-squared	0.33	0.42	0.29	0.50	0.31	0.45
Average dependent variable	70.23	70.23	4.08	4.08	3.38	3.38
Panel B - Police reports						
Percentage of students skipping school	2.93	3.99	0.09	0.13	0.08	0.11
	(1.01)	(0.90)	(0.03)	(0.03)	(0.03)	(0.03)
R-squared	0.33	0.50	0.30	0.58	0.29	0.49
Average dependent variable	38.95	38.95	3.41	3.41	2.71	2.71
Panel C - Organizer reports						
Percentage of students skipping school	5.92	7.32	0.07	0.10	0.06	0.08
	(2.17)	(2.44)	(0.02)	(0.03)	(0.02)	(0.02)
Observations	20	20	20	20	20	20
R-squared	0.25	0.31	0.24	0.42	0.25	0.38
Year fixed effects	No	Yes	No	Yes	No	Yes
Average dependent variable	102.5	102.5	4.44	4.44	3.74	3.74

Notes: This table presents estimates of the empirical relationship between the number of protesters (dependent variable, Y) and the percentage of students 14-18 years old skipping school that day ($X \in [0, 100]$. The number of protesters comes from Table A.1. Robust standard errors in parentheses. All coefficients are statistically significant at the 5%.

Table A.4: Main estimates using a dynamic specification

Student exposed:	Schoolmates		Neighbor students (< 0.5 miles) compared to students who live...	
	All	Classmate	[0.5-3] miles	[1.5-3] miles
	(1)	(2)	(3)	(4)
Schoolmate × protest day 1 after the killing	-0.04	-0.04	-0.00	-0.02
	(0.03)	(0.03)	(0.03)	(0.03)
	[0.31]	[0.41]		
Schoolmate × protest day 2 after the killing	-0.08	-0.13	-0.02	-0.03
	(0.02)	(0.03)	(0.04)	(0.04)
	[0.28]	[0.28]		
Schoolmate × protest day 3 after the killing	-0.08	-0.12	-0.00	-0.01
	(0.02)	(0.02)	(0.03)	(0.03)
	[0.15]	[0.15]		
Schoolmate × protest day 4 after the killing	-0.13	-0.14	-0.00	0.00
	(0.02)	(0.03)	(0.03)	(0.03)
	[0.09]	[0.19]		
Schoolmate × protest day 5 after the killing	-0.00	-0.04	-0.04	-0.03
	(0.01)	(0.02)	(0.02)	(0.02)
	[0.59]	[0.41]		
Schoolmate × protest day 6 after the killing	0.01	0.02	-0.01	-0.01
	(0.01)	(0.01)	(0.03)	(0.03)
	[0.41]	[0.67]		
Schoolmate × protest day 7 after the killing	-0.04	-0.06	-0.03	-0.02
	(0.01)	(0.01)	(0.03)	(0.03)
	[0.24]	[0.23]		
Schoolmate × protest day 8 after the killing	-0.05	-0.06	-0.05	-0.06
	(0.02)	(0.02)	(0.03)	(0.03)
	[0.34]	[0.37]		
Schoolmate × protest day 9 after the killing	-0.05		0.01	0.01
	(0.01)		(0.03)	(0.03)
	[0.26]			
Schoolmate × protest day 10 after the killing	-0.04		-0.03	-0.01
	(0.02)		(0.03)	(0.04)
	[0.28]			
Schoolmate × protest day 11 after the killing	-0.03		-0.01	-0.02
	(0.02)		(0.03)	(0.03)
	[0.47]			
Schoolmate ×protest day 12 after the killing	-0.02		-0.04	-0.03
	(0.01)		(0.03)	(0.03)
	[0.45]			
Schoolmate × protest day 13 after the killing	-0.02		-0.00	-0.00
	(0.01)		(0.03)	(0.03)
	[0.39]			
Observations	387,630	74,265	14,838	12,634
Student fixed effects	Yes	Yes	Yes	Yes
Cell-day fixed effects	Yes	Yes	Yes	Yes
Students	22,549	5,025	757	644
Avg. dependent variable	0.33	0.27	0.10	0.09

Notes: Each observation corresponds to a skipping school decision of a high-school student in a protest that took place on a weekday within the school calendar in the 2011-2013 period. Estimates of linear probability models. Standard errors are clustered at the school level and Fisher's exact p-values from randomization inference in square brackets.

Table A.5: Protest decisions in the short- and long-run

	Daily data		Collapsed by period	
	2011-2012	2011-2013	2011-2012	2011-2013
Panel A	(1)	(2)	(3)	(4)
Schoolmate × After ∈ 2011 [α]	-0.07	-0.07	-0.07	-0.07
	(0.01)	(0.01)	(0.01)	(0.01)
	[0.10]	[0.10]	[0.10]	[0.10]
Schoolmate × After ∈ 2012-13 [β]	0.04	0.04	0.04	0.04
	(0.01)	(0.01)	(0.01)	(0.01)
	[0.30]	[0.30]	[0.30]	[0.30]
Observations	323,085	387,630	62,597	62,598
Students	22,549	22,549	22,549	22,549
Student fixed effects	Yes	Yes	Yes	Yes
Cell-day fixed effects	Yes	Yes	Yes	Yes
Average dependent variable	0.33	0.33	0.33	0.33
Exact p-value: $(\alpha+\beta) = 0$	0.35	0.29	0.35	0.30
Panel B				
Neighbor × After ∈ 2011 [α]	-0.02	-0.02	-0.02	-0.02
	(0.02)	(0.02)	(0.02)	(0.02)
Neighbor × After ∈ 2012-13 [β]	-0.01	0.00	-0.01	0.00
	(0.02)	(0.02)	(0.02)	(0.02)
Observations	9,579	13,245	1,905	1,905
Students	644	644	644	644
Student fixed effects	Yes	Yes	Yes	Yes
Cell-day fixed effects	Yes	Yes	Yes	Yes
Average dependent variable	0.11	0.11	0.11	0.11
p-value: $(\alpha+\beta) = 0$	0.09	0.27	0.09	0.26

Notes: This table presents short- and long-run estimates of police violence on protest behavior. We present four specifications. Column 1 uses data from all protest days in 2011 and 2012. Column 2 uses data from all protest days in 2011-2013. Columns 3-4 mimic the previous ones but collapse the data by period (Bertrand et al., 2004). We consider a short-run (2011) and a long-run period (2012-2013). Note that there is mechanical attrition due to the graduation of the older students, e.g. in 2012 we do not observe the cohort of students in their senior year in 2011. In addition, there is non-random attrition related to high-school dropouts, making the long-run estimates arguably a lower bound. Lastly, there is 12-14% school switching but we always consider switchers to be part of the original group of students exposed to police violence. Each observation corresponds to a skipping school decision of a high-school student in a protest that took place on a weekday within the school calendar. Estimates of linear probability models. Standard errors are clustered at the school level and Fisher's exact p-values from randomization inference in square brackets.

Table A.6: Robustness of long-run results to dropouts

The dependent variable is an indicator for school skipping in a weekday protest				
Panel A: Year 2011	All schoolmates		Classmates	
	(1)	(2)	(3)	(4)
Schoolmate × After student killed	-0.08	-0.07	-0.09	-0.09
	(0.03)	(0.01)	(0.03)	(0.02)
Observations	239,172	239,172	54,924	54,924
Students	19,931	19,931	4,577	4,577
Student fixed effect	Yes	Yes	Yes	Yes
Day fixed effects	Yes	No	Yes	No
Cell-day fixed effects	No	Yes	No	Yes
Average dependent variable	0.33	0.33	0.26	0.26
Panel B: Years 2011-2013	Daily data		Collapsed by period	
	2011-2012	2011-2013	2011-2012	2011-2013
Schoolmate × After student killed	-0.08	-0.08	-0.08	-0.08
	(0.01)	(0.01)	(0.01)	(0.01)
Schoolmate × After 2011	0.04	0.04	0.04	0.04
	(0.01)	(0.01)	(0.01)	(0.01)
Observations	227,226	266,241	43,840	43,840
Students	15,951	15,951	15,951	15,951
Student fixed effects	Yes	Yes	Yes	Yes
Cell-day fixed effects	Yes	Yes	Yes	Yes
Average dependent variable	0.32	0.32	0.32	0.32

Notes: Each observation corresponds to a skipping school decision of a high-school student in a protest that took place on a weekday. Estimates of linear probability models. The estimation uses the sample of students who never dropout of school during the years we empirically examine. Standard errors are clustered at the school level.

Table A.7: Distance to home/school of victim and distance to La Moneda

Dependent variable: Indicator school skipping in weekday protest						
	Students who lived nearby home/school of student killed				Robustness of result to distance to La Moneda	
	home		school		schoolmates	classmates
	(1)	(2)	(3)	(4)	(5)	(6)
Schoolmate × After student killed	-0.03	-0.03	0.05	0.05	-0.05	-0.10
	(0.03)	(0.02)	(0.04)	(0.03)	(0.02)	(0.03)
Observations	8,052	8,052	7,500	7,500	22,764	5,556
Students	671	671	625	625	1,897	463
Student fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Day fixed effects	Yes	No	Yes	No	No	No
Cell-day fixed effects	No	Yes	No	Yes	Yes	Yes
Average dependent variable	0.10	0.10	0.15	0.15	0.29	0.18

Notes: Each observation corresponds to a skipping school decision of a high-school student in a protest that took place on a weekday within the 2011 school calendar. Estimates of linear probability models. Columns 1-4 check for the impact of distance to the home and school of the victim and report a coefficient which is not statistically different from zero. Columns 5-6 show that the results are robust to including the distance to La Moneda palace as an additional covariate in the matching algorithm. Note that again the impact on the classmates is twice the size of the impact on schoolmates Standard errors are clustered at the school level.

Table A.8: The impact of non-lethal police repression

Dependent variable: Indicator school skipping in weekday protest		
	(1)	(2)
Schoolmate × After non-lethal police repression	0.05	0.05
	(0.03)	(0.05)
Observations	210,874	210,754
Students	27,619	27,619
Student fixed effects	Yes	Yes
Day fixed effects	Yes	No
Cell-day fixed effects	No	Yes
Average dependent variable	0.47	0.47

Notes: To further improve our understanding of the shooting, we explored the impact of less severe police violence during protests held in August of 2012 using data from a social organization. An article in the New York Times describes their work as "small troops of observers in blue or white helmets, armed with notebooks, cameras, voice recorders and gas masks. They [. . .] monitor and record what happens when the police crack down on the protests." The victims were 14-18 years old students, their school is clearly identified, and there is photographic evidence of police violence (e.g. bruises, broken teeth). We use the same empirical strategy on the 3,500 schoolmates (grades are unknown) and the matching delivers a control group of 24,000 students. The results in this table show similar protest behavior after these less severe events. Each observation corresponds to a skipping school decision of a high-school student in a protest that took place on a weekday. Estimates of linear probability models. Standard errors are clustered at the school level.

Table A.9: The impact of deaths of 14-18 yrs old on protest behavior

The dependent variable is the county average school skipping in a weekday protest

	External cause	Accident	Homicide
	(1)	(2)	(3)
1(death 14-18 yrs old) × After	-0.003	0.002	-0.001
	(0.016)	(0.008)	(0.014)
Observations	564	564	564
County fixed effects	Yes	Yes	Yes
Day fixed effects	Yes	Yes	Yes
Counties	47	47	47
Avg. dependent variable	0.178	0.178	0.178
Counties with deaths	10	1	5

Notes: In this table we estimate the impact of deaths of 14-18 years old in August 2011 due to accidents or homicides unrelated to the police using data from the National Health Statistics (DEIS) and the causes of death using the International Classification of Deaths (ICD). Unfortunately, we cannot match these to a school, so we use county-level data. We focus on the the 47 counties in the three largest cities. The results show a precisely estimated zero impact of these deaths on the protest behavior of students. Each column presents estimates using a panel of counties located in the three largest cities – where half of the population lives – observed during 12 weekday protests in 2011. Standard errors are clustered at the county level.

Table A.10: Robustness of educational results using more covariates in the matching

	GPA			Dropout			Ever takes college exam (2011-2018)	
	2011	2012	2013	2011	2012	2013		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Schoolmate	-0.07	0.01	-0.08	0.03	0.04	0.03	-0.28	-0.36
	(0.02)	(0.02)	(0.02)	(0.01)	(0.00)	(0.01)	(0.02)	(0.03)
Students	4,106	2,691	1,428	4,106	2,691	1,428	4,126	4,126
Average dependent variable	5.17	5.21	5.35	0.04	0.03	0.03	0.83	0.83
p-value MHT	0.36	0.16	0.36	0.03	0.03	0.19	0.01	0.01
Cell fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Ventiles of past GPA fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Ventiles of Pr(closure) fixed effects	No	No	No	No	No	No	No	Yes

Notes: Each observation corresponds to the educational outcome of a student. Cross-sectional estimates that compare the educational performance of students exposed to police violence with a selected comparison group. This table uses an augmented matching that exploits the availability of standardized tests for a subsample of students. This exercise guarantees that we are comparing students with similar educational performance before the shooting. Standard errors are clustered at the school level. We also present *p*-values that control the family-wise error rate following Romano and Wolf (2005).

Table A.11: Student-led boycott to the 2013 standardized test

	Indicator skipping school		Indicator skipping test	
Days around test day:	[-2,2]	[-4,4]	[-2,2]	[-4,4]
	(1)	(2)	(3)	(4)
Panel A				
Schoolmate × National test day	0.09	0.09	0.13	0.13
	(0.05)	(0.04)	(0.05)	(0.04)
	[0.12]	[0.12]	[0.08]	[0.08]
Observations	17,730	31,915	17,730	31,915
Students	3,551	3,551	3,551	3,551
Student fixed effects	Yes	Yes	Yes	Yes
Cell-day fixed effects	Yes	Yes	Yes	Yes
Average of dependent variable	0.13	0.13	0.14	0.13
Panel B				
Neighbor × National test day	-0.03	-0.02	0.00	0.02
	(0.03)	(0.03)	(0.04)	(0.04)
Observations	1,868	3,360	1,868	3,360
Students	374	374	374	374
Student fixed effects	Yes	Yes	Yes	Yes
Cell-day fixed effects	Yes	Yes	Yes	Yes
Average of dependent variable	0.12	0.13	0.12	0.13

Notes: Each observation corresponds to a skipping school (skipping test in columns 3-4) decision of a high-school student in a weekday around the day of a standardized test. Standard errors are clustered at the school level in parentheses and *p*-values from randomization inference in square brackets.

Table A.12: The impact on the educational performance of classmates

	GPA			Dropout			Ever takes college exam (2011-2018)	
	2011	2012	2013	2011	2012	2013		
Panel A	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Schoolmate	-0.03	-0.12	-0.13	0.04	0.04	0.04	-0.28	-0.35
	(0.02)	(0.02)	(0.02)	(0.00)	(0.00)	(0.00)	(0.02)	(0.01)
Schoolmate × Same grade	-0.04	0.10	-0.39	-0.00	-0.02	-0.08	-0.05	-0.06
	(0.02)	(0.02)	(0.04)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Students	22,108	18,033	13,221	22,108	18,033	13,221	22,442	22,442
Average dependent variable	5.28	5.36	5.41	0.03	0.04	0.03	0.86	0.86
Panel B								
Schoolmate	-0.05	-0.06	-0.06	0.04	0.05	0.04	-0.29	-0.37
	(0.03)	(0.03)	(0.02)	(0.01)	(0.01)	(0.01)	(0.02)	(0.03)
Schoolmate × Same grade	-0.04	0.13	-0.75	-0.03	-0.02	-0.11	0.02	0.01
	(0.03)	(0.02)	(0.19)	(0.01)	(0.01)	(0.07)	(0.02)	(0.02)
Students	4,106	2,691	1,428	4,106	2,691	1,428	4,126	4,126
Average dependent variable	5.17	5.21	5.35	0.04	0.03	0.03	0.83	0.83
Cell fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Ventiles of Pr(closure) fixed effects	No	No	No	No	No	No	No	Yes

Notes: Each observation corresponds to the educational outcome of a student. Cross-sectional estimates that compare the educational performance of students exposed to police violence with a selected comparison group. Standard errors are clustered at the school level.

Table A.13: College exam results by grade of the schoolmates

	Dependent variable: Indicator for taking the college exam				
Grade in 2011:	12th grade	11th grade	10th grade	9th grade	8th grade
	(1)	(2)	(3)	(4)	(5)
Schoolmate	-0.20	-0.34	-0.34	-0.31	-0.16
	(0.03)	(0.03)	(0.03)	(0.02)	(0.02)
Students	3,947	5,007	4,555	4,660	4,273
Cell fixed effects	Yes	Yes	Yes	Yes	Yes
Ventiles past GPA fixed effects	Yes	Yes	Yes	Yes	Yes
Average dependent variable	0.89	0.88	0.83	0.83	0.84

Notes: Each observation corresponds to the educational outcome of a student. Cross-sectional estimates that compare the educational performance of students exposed to police violence with a selected comparison group. We identified if students took the college exam in any year before 2018. Standard errors are clustered at the school level.

Table A.14: Robustness of results to alternative matching specifications

	Combinations within baseline covariates														additional covariates				
	baseline	drops odd protest days	drops even protest days	drops covariate 1	drops covariate 2	drops covariate 3	drops covariate 4	drops covariate 5	drops covariate 6	drops covariate 7	drops covariate 8	drops covariate 9	all but protest days	only school grade	student test scores	household income	parents' involvement	low-income students	protests in 2006
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)	(19)
Table A.5																			
Short-run	-0.07	-0.10	-0.11	-0.06	-0.06	-0.08	-0.10	-0.08	-0.08	-0.07	-0.06	-0.07	-0.16	-0.17	-0.06	-0.06	-0.07	-0.12	-0.07
Long-run	0.04	0.03	0.05	0.05	0.04	0.04	0.05	0.04	0.05	0.04	0.03	0.04	0.04	0.03	0.04	0.05	0.04	0.08	0.05
Table 2																			
Schoolmates	-0.08	-0.08	-0.09	-0.08	-0.08	-0.08	-0.08	-0.08	-0.08	-0.07	-0.07	-0.08	-0.08	-0.08	-0.07	-0.07	-0.08	-0.12	-0.09
Classmates	-0.10	-0.10	-0.10	-0.10	-0.10	-0.11	-0.11	-0.10	-0.10	-0.10	-0.08	-0.11	-0.10	-0.09	-0.06	-0.06	-0.08	-0.14	-0.11
Table A.11																			
Boycott	0.09	0.06	0.05	0.08	0.07	0.07	0.05	0.13	0.09	0.07	0.07	0.07	0.05	0.04	0.03	0.03	0.03	0.12	0.08
Table 3																			
GPA	-0.04	-0.07	-0.06	-0.04	-0.04	-0.06	-0.06	-0.04	-0.04	-0.05	-0.07	-0.04	-0.08	-0.11	-0.03	-0.04	-0.05	-0.11	-0.09
Dropout	0.04	0.04	0.03	0.04	0.04	0.04	0.04	0.04	0.04	0.04	0.04	0.04	0.03	0.03	0.03	0.03	0.04	0.04	0.05
College	-0.37	-0.35	-0.36	-0.37	-0.36	-0.37	-0.36	-0.35	-0.37	-0.35	-0.37	-0.35	-0.35	-0.36	-0.34	-0.34	-0.36	-0.42	-0.37
N in control group	21,906	39,562	34,075	23,877	24,133	28,196	25,611	26,020	27,695	26,021	37,806	38,312	54,386	55,989	13,267	11,339	16,767	7,992	5,750

Notes: The estimated impacts of the police shooting (y-axis) are robust to 18 alternative specifications (specification 1 is the baseline result). These alternative specifications omit skipping indicators in even protest days before the shooting (specification 2), in odd protest days (3), each covariate separately (4-12), skipping in all protest days (13), and use grade as the only matching covariate (14). Specifications 15-19 add the following predetermined variables in the matching algorithm: student-level test scores (15), household income (16), parents' educational involvement (17), percentage of low-income students in the school (18), and a measure of school-level protest behavior in the 2006 student rallies (19). The "Short-run" and "Long-run" results in panel (a) correspond to the deterrence and reversal of deterrence after the shooting in 2011 and afterwards (2012-13). The "Boycott" results correspond to skipping a high-stakes standardized test as a way of protesting against the educational system.