Fear of the Police: Evidence from Student Protests*

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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.

Keywords: police, violence, protest, students.

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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, 2000, 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 feelings of 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 (Jacobs, 1998; Klor et al., 2020; 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,

[1] A large theoretical literature argues that state violence can backfire and *increase* political dissent, perhaps because violence reveals information about the government and protesters. See Lichbach (1987); Opp and Roehl (1990); Lohmann (1994); McAdam (1995); Moore (1998); Shadmehr and Boleslavsky (2020), among many others.

we consider the setup to be 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, and employ coarsened-exact matching to construct a counterfactual composed by students who protested identically before the shooting.[2]

The main result is that the police killing decreased adherence to street rallies and increased 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 shows that the 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, and it completely vanishes two years after when most of the schoolmates graduated from high-school. 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 support the protest nature of their decisions beyond schooling. Moreover, the lower protest participation monotonically decreases with social distance to the victim within the affected school. In contrast to the impact on the schoolmates, those who lived nearby the shooting remained protesting in a similar way than before. 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 to the most important standardized test in the country. 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 such as

[2]Similar econometric strategies have been used to estimate the impact of patient death on medical referrals (Sarsons, 2019) and the impact of deaths of academic "superstars" on the productivity of colleagues (Azoulay et al., 2010).

teacher bonuses (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 potential mechanisms by combining previous results with additional evidence on 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 a solid econometric conclusion. 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 argue that changes in risk assessment arising from fear of the police 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 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) – suggests that differential information about the event is unlikely to be relevant in this context. In addition, the higher adherence to the boycott and lower adherence to rallies suggest that the presence of the police is important. Finally, the negative impact on educational outcomes is probably more related to the psychological consequences of police violence as shown by recent research (Rossin-Slater et al., 2020).

The main contribution of this paper 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 many different manifestations of state violence such as crackdowns, military interventions, and state repression on dissident and civic engagement behavior using lab-in-the-field experiments (Young, 2019b), experiments with online and offline surveys (García-Ponce and

Pasquale, 2015; 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., 2021b; Ang and Tebes, 2021). Most of this research has shown that state violence can backfire and increase dissident behavior.

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 a renewed perceptions of injustices as mediators (Reny and Newman, 2021).[3] 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 last part of our 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), 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).

[3]Examples include the shooting of Michael Brown and the following wave of protests in Ferguson in 2014, and the killing of Arthur McDuffie and the 1980 Miami riots (DiPasquale and Glaeser, 1998), among others.

This paper shows that exposure to police violence also leads to negative consequences in terms of educational outcomes. Schoolmates of the student killed by the police gunshot experienced lower high-school performance, higher dropout rates, and lower college enrollment. These effects do not appear to vanish over time and are still sizable three years after the killing.

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., 2020), 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., 2021a). Students protested against the *de facto* for-profit nature of schools and the increasing cost of higher education in what is one 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 in the country (e.g. *Central Única de Trabajadores*).[4] 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.[5] That night the high-school student was accompanied by his older brother and a neighbor as they were passing through a footbridge over a large street, just a couple of blocks from their homes located in a neighborhood known by the name of *Jaime Eyzaguirre*. Their intention was to passively watch the protest final events of that day. The two brothers had done the same thing the night before in which fewer people were protesting in the streets.[6] According to interviews with his family, Manuel did *not* actively participated in the national strike in any form. Because of the strike his school was closed and thus during that day he visited some friends nearby. Moreover, his family members have repeatedly stated that Manuel was *not* politically active. Manuel was the youngest brother of a low-income and religious family who was known in the neighborhood to be "a good young man" removed from youth-related 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

[4]Figure A.1 provides examples of pamphlets circulating before protest days. The messages shows a wide range of demands: a new Constitution, a new tax system, and better pensions. Most weekday protests began around 10.30 AM.

[5]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 – and from a documentary produced by Manuel's older brother provides details about the most important events after the shooting (see this link).

[6]The killing happened 5 miles away from the march. Protest events such as barricades and confrontations with the police took place throughout the city, but the main march was held in the city's main square as usual.

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. There were some attempts to cover the police's involvement by arguing that the student's death was the result of a confrontation between violent protesters.[7] However, 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. He also confessed that two of their fellow policeman also fired their weapons (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.

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 perhaps surprisingly explicit about the role of the police, with some writing that "the bullet that killed Manuel Gutierrez was a police gunshot according to expert reports" (August 29, 2011) and "the policeman confessed he fired the UZI submachine gun" (August 31, 2011), among many other examples. Although information about the role of the police role was available, learning about the

[7] A television program even "confirmed" that the student's death was a drug-related incident when a neighbor stopped the reporter live to say that he and other neighbors saw the policeman firing his gun. Moreover, the day after the event the General of Police declared that policeman were not involved in the killing.

event was an endogenous decision which we discuss below.

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.[8] 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 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.[9] As expected, the police reported fewer participants than the organizers, but the correlation between both is positive and statistically significant in the sample of 21 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 2 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 has 7 teachers per 100 students.

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

[8]The focus on weekdays is solely based on our interest in *school* skipping decisions. For the same reason we omit weekday protest in January, February, July, and December because of school holidays, which leaves out important protest days such as the ones in July 14th and August 4th, among others which took place during school breaks.

[9]Note that most schools in Santiago – including the school of interest – were closed during the day of the shooting (August 25). In that protest, organizers counted more than 300,000 participants and the police reported 50,000.

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 the same as "same grade schoolmates." Students in the same grade had closer social links because they shared classes regularly. Their school was located in a middle income urban area. Panel (a) in Figure 1 shows the geographic location of the school and the place of the shooting. Column 2 in Table 2 presents summary statistics for the schoolmates and the characteristics of their school.

The second group is composed by students living nearby the shooting, regardless if they were schoolmates. 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.[10] 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.[11] Panel (b) in Figure 1 plots the location of these 13,000 students. 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 2 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

[10] The contiguous municipalities are La Florida, La Granja, Macul, Ñuñoa, San Joaquin, and Peñalolen. For reference, the location of these municipalities is marked with a square in Panel (a) of Figure 1.

[11] Table A.1 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 for the interpretation of estimates.

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. Panel (a) in Figure 2 shows that in a weekday protest school skipping was approximately 18%. In contrast, the average school skipping in the same day without a protest (e.g. Thursday) on the week before or the week after was 11%. Second, a higher school skipping rate is a strong predictor of protest size. Panel (b) in Figure 2 shows the correlation between the number of protesters and school skipping in the 20 protest days we study.[12] 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 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.[13] Then we asked college students – high-school students in 2011 – to count the number of high-school students in each image.[14] We obtained approximately 4,500 responses from 450 college students. Column 6 in Table 1 presents results which suggest that half of protesters were high-school students, with variation across protest days. Panel (c) in Figure 2 presents the visual correlation between the number of student protesters and skipping, while columns 5-6 in Table A.2 present the corresponding regression estimates. To get a sense of the magnitude of this correlation, consider that a 10 percentage points increase in school skipping – almost 90,000 students – is associated with 55,500 additional protesters (panel A, column 2) or 24,000 additional student protesters. This is, we calculate that 27 of every 100

[12]We use the average of protesters reported by the police and organizers. Figure A.2 shows that the correlation is strong and positive with each measure separately. Table A.2 present the corresponding regression coefficients.

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

[14]Figure A.3 provides more details about the images and the method. It is important to mention that high-school students are potentially recognizable in these images because they wear school uniforms and are younger than the rest.

students who skipped school decided to attend the rally (24,000 over 90,000).

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 constitute 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 age 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 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 2 shows some characteristics of the comparison group.

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.[15] Panel (b) in Figure 1 plots the location of the neighbor students and the potential controls. Column 6 in Table 2 presents summary statistics for this comparison group of students.[16]

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 $S_{j(i)}$ 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.[17] For estimation of the linear probability model in equation (1), we follow Iacus et al. (2012) and use weights to account for the different number of 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.

[15] As robustness check we use as controls *all* students who lived within 0.5-3 miles from the shooting which leaves us with 199 and 558 treated and control students enrolled in 227 schools and classified in 100 cells. We also explore the impact of the shooting on those living nearby the home and the school of the student killed.

[16] The intersection between the group of schoolmates and the group of neighbors is unfortunately too small in statistical terms to study the impact on students who were socially close *and* lived nearby the shooting.

[17] 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}.

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:

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

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 $\gamma_2 > 0$ and the difference $\gamma_1 - \gamma_2$ to reveal the *additional* impact of police violence on protest behavior.

C. Randomization inference and Fisher's exact p-values

Student decisions are likely to be correlated within schools for multiple reasons, e.g. they are governed by the same institutions and affected by similar shocks. To account for this correlation we begin by clustering standard errors at the school level. 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). Indeed, a recent method to assess the appropriateness of our inference method reveals that our analysis is likely to fall in this category (Ferman, 2019). Similarly, our analysis of the decisions made by students living nearby the event has to account for the possibility of 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, 2019a). 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 use the Conley (1999) heteroskedasticity and autocorrelation consistent (HAC) standard errors with the home address of students and allow decisions to be correlated within 3 miles.[18]

V. Protest behavior

We begin with a descriptive analysis of protest behavior in the period 2011-2013, before and after the student was killed. Panels (a), (b), and (c) in Figure 3 presents average school skipping rates in protest days among students exposed to the shooting and the corresponding comparison group. Panels (a) and (b) 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. A synthetic control analysis delivers very similar patterns (Figure A.4).

[18]We chose 3 miles as the cutoff because our strategy assumes that students were treated if they lived closer than 1.5 miles from the shooting, which implies that the maximum distance between neighbor students is 3 miles. In any case, as a robustness check we also allowed decisions to be correlated within 6 or 10 miles and we obtain tighter confidence intervals. Thus, we view our decision to use a cutoff of 3 miles as statistically conservative.

A. Protest days in 2011-2013

Panels (d), (e), and (f) in Figure 3 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 schoolmates in the same grade, suggesting that social proximity is important. Panel (f) looks at students who lived nearby and results are weaker, with perhaps a smaller skipping rate that is not statistically different from zero. Table A.3 presents the corresponding regression coefficients for these figures and the corresponding exact p-values in columns 1 and 2 for the case of the schoolmates.

The previous estimates reveal some differences in 2011 when compared to later years, which motivates a more parametric econometric specification which splits these periods. In 2012 there were fewer and less intense protests but 2013 saw the return of massive rallies.[19] To estimate the impact of police violence in the 2012 and 2013 years, we look at school skipping in the three weekday protests in 2012 and the five in 2013 (Table 1) using a panel of students. We observe a total of 20 weekday protests in 2011-2013 and estimate the following regression equation:

$$Y_{ijst} = \beta_1 \left(S_{j(i)} \times D_{1t}\right) + \beta_2 \left(S_{j(i)} \times D_{2t}\right) + \phi_i + \phi_{st} + \varepsilon_{ijst} \tag{3}$$

where Y_{ijst} is the skipping indicator for student i, enrolled in school j in 2011, classified in cell s, and observed in protest day t. The indicator D_{1t} takes the value of one for the whole period after the shooting and D_{2t} takes the value of one only in years 2012 and 2013. All remaining variables and estimation techniques are the same as before. Under this specification the parameter β_1 measures the short-run impact of police violence and $\beta_1 + \beta_2$ measure the long-run impact.

We present results from four specifications. The first uses data from all protest days in 2011 and 2012. The second uses data from all protest days in 2011-2013. The third and fourth specifica-

[19]Presidential and Congress elections were held in November of 2013 and rallies slowly vanished. Protests enjoyed significant support from citizens and prominent leaders of the 2011 protests were elected as members of the Congress. The left-wing candidate was elected president with a proposal of free tertiary education.

tions 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) with three differences. First, 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.[20] Second, there is non-random attrition related to high-school dropouts, which in the following section we show is related to the stray bullet, making the long-run estimates arguably a lower bound. And third, there is some school switching: 70 of 489 remaining schoolmates were enrolled in a different school in 2012, and 27 of the remaining 242 switched schools in 2013. We always consider that the schoolmates who switched school are part of the original group of students exposed to police violence.

Table 3 presents estimates of equation (3). Panel A supports the hypothesis that part of the effect of police violence on protest behavior was somewhat transitory. The short-run impact is always negative and reveals a decrease in school skipping similar to the ones presented in Figure 3, i.e. 7 pp. (exact p-value of 0.10). In contrast, the long-run impact captured by $\widehat{\beta}_2$ is always positive, offsetting the initial drop in protest behavior (exact p-value of 0.30). The same patterns appear when we use the daily and collapsed data. More than half of the decrease in protest behavior is offset in 2012 (0.04/0.07 = 0.57) and the negative effect disappears in 2013 when most schoolmates already graduated from high-school. Combined with the dynamic coefficients in panels (c) and (d) of Figure 3, these results suggest that the effect of police violence slowly vanishes 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 (panel B).

To further improve our understanding of the event under analysis, we explored the impact of less severe police violence during protests held in August of 2012 using data from a social organization (Codepu, 2012).[21] 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

[20]The same attrition occurs in 2013 with students in 11th grade in 2011. The exception are students repeating the grade, who we observe and include in the estimation. Retention among high-school students had an average of 6% in 2010 and increased by 4 pp. in 2011. Retention is higher in 9th grade and decreases in higher grades.

[21]The organization assists victims of repression, raises awareness issues, and documents human rights violations. In the context of the student protests in 2011-2013, the organization watched and documented acts of police repression. An article published 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 are not there to join the protests or interfere, only to monitor and record what happens when the police crack down on the protests” (Bonnefoy, 2012).

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 Table A.6 show similar protest behavior after these less severe events. We also estimated 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. 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. Table A.7 shows a precisely estimated zero impact of these deaths on the protest behavior of students. These additional results suggest that the impact of the killing on protest behavior 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.

B. Protest and non-protest days in 2011

Table 4 presents estimates of equation (2), i.e. the average impact of the shooting in the five protest days after the event in 2011 using protest and non-protest days. We focus on this year because the sample is fixed as in later years some cohorts graduate from high-school. Column 1 begins by omitting protest days from the estimation and focusing on non-protest days. The estimate in this column 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. We do this to improve comparability across protest and non-protest days and because non-protest days of the same week could be contaminated by the protests if students require some level of organization.[22] 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 same grade schoolmates (8-10 pp.), again suggesting social proximity matters, with exact p-values between 0.03 and 0.10.

The impact of the shooting on schoolmates in 2011 is a robust finding and several exercises

[22]School skipping decisions vary markedly across days of the week. 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.

ease concerns about the effect of the 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: with more variables the number of treated students decreases substantially. Therefore, we implemented several other matchings using different subsets of variables in the main estimation. The goal is to be confident that none of these subsets has a particular influence in our estimates. Reassuringly, Figure 5 shows that 13 different combinations of the baseline variables deliver similar results. In these additional specifications we omit skipping indicators in even protest days before the shooting (alternative 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.4) or exclude single protest days from the estimation (Figure A.6). The last set of robustness checks use the same strategy and shows 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.5), 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.5).

C. 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).[23] 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.[24] In 2013 the mathematics and language tests had to be taken by all twelve graders on November 20. One week before,

[23] School-level test scores are used to inform parents about school quality and the state uses them to implement educational policies such as teacher performance bonuses. Newspapers routinely disseminate rankings of schools based on test scores, and schools use their scores as an advertisement device to increase the enrollment of students.

[24] Standardized tests have been the focus of controversies in many countries. Critics argue that the importance of these tests can introduce perverse incentives for schools to change the metric by mechanisms different than improving the educational performance of students (Figlio and Getzler, 2002; Kane and Staiger, 2002; Neal, 2013).

student leaders of prominent schools and the two largest student unions 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).[25]

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:

$$Y_{ijst} = \tau\left(S_{j(i)} \times \text{Test day}_t\right) + \phi_i + \phi_{st} + \varepsilon_{ijst} \tag{4}$$

where Test day_t is an indicator variable that takes the value of one in November 20. All remaining variables and parameters are defined as before. We use two dependent variables: an indicator variable that takes the value of one if the student decided to skip school, and an indicator that takes the value of one 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 present estimates from two specifications, one that uses two school days before and after the test, and another that uses four days before and after the test. The parameter τ 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.

Table 5 presents estimates of the linear probability model in equation (4). Panel A presents evidence consistent with a higher adherence to the boycott among the schoolmates. The estimates show that the skipping rate increased by 8 pp. among the schoolmates from a base of 13%, although estimates are not statistically significant at conventional levels (exact p-value of 0.12). When we employ our preferred measure of adherence (columns 3-4) 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, panel B again reveals a similar adherence to the boycott among students living nearby the shooting

[25]Test boycotts have become common after 2013 but before that year the only previous attempt happened in 2006 during another wave of student protests. Similar boycotts have been observed in the U.S. such as the teacher boycott in Seattle that sparked a national conversation about the use of standardized tests (Hagopian et al., 2014).

and their comparison group. Figure 4 shows that the differential decisions among schoolmates were related to the test, as they exhibit a similar school attendance than the comparison group in days before and after test day. Again, the figure supports the hypothesis that the event appears to have had little impact on neighbor students. 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.

D. Discussion of mechanisms

Two mechanisms can explain why students socially close to the victim of police violence participated less in protests after the event. The first relates to information. Although media outlets covered the event, geographic or social proximity to the event could have increased knowledge about what happened (Fujita et al., 2006). Under this explanation, the schoolmates rationally changed their decisions because they updated their beliefs about the cost of protesting (Becker, 1968; Young, 2019b), the probability of success, or their beliefs about the government (Lohmann, 1994; Pierskalla, 2010). A second explanation is related to social psychology and emphasizes the differential impact of the traumatic experience of an officer killing a student. A social link to a victim of police violence can trigger both fear and anger, with the latter sometimes out weighting the former and leading to "backlash protest" (Aytac et al., 2018).

Three pieces of evidence suggest that the social psychology mechanism is more likely to be important than the information mechanism. First, if emotions are the mediating factor we expect the lower participation in protests to be larger among students who were closer to the victim. The results indeed suggest that this is the case as students in the same grade decreased their protesting behavior by more than other schoolmates. Table 6 revisits this result but using grade-to-grade variation to test for the existence of this pattern more flexibly. The impact of police violence on 11th graders is larger than the impact in other grades. More generally, the impact seems to be monotonically decreasing with respect to the grade of the student killed (11th grade). As all students in the school of the victim were likely to be equally informed about the shooting, this pattern suggests that social proximity and emotions matter. In a similar fashion, we expect students who lived nearby the shooting to be better informed than students living farther away (Fujita et al., 2006). Because protest behavior does *not* change with distance to the shooting, this constitutes additional evidence against heterogeneous information being an explanation for our findings. The third piece of evidence is presented in the next section, in which we argue that a change in emotional states is more likely to have an impact on educational outcomes than a change in information.

VI. Educational performance

This section investigates whether police violence had educational consequences. Previous research has found negative effects associated to acts of police repression in the U.S. (Ang, 2021; Rossin-Slater et al., 2020) but evidence from other parts of the world 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).

A. *Empirical strategy*

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

$$Y_{ijs} = \delta S_{j(i)} + f(X_{ij}) + \phi_s + \varepsilon_{ijs} \quad (5)$$

where Y_{ijs} represents an educational outcome of student i, who was enrolled in school j in 2011, and was classified in cell s by the coarsened exact matching algorithm. The indicator $S_{j(i)}$ takes the value of one for schoolmates or neighbor students and zero for the selected comparison group of students. 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 control students in each cell (Iacus et al., 2012). We also calculate Fisher's exact p-values and report these as well for ever estimate.

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. This decision is based on the nature of the variation in the dependent variable which varies from year to year instead of day by day. Dropouts introduce mechanical non-random attrition and the college exam can be taken any year *after* graduating from high school and as many times as the student wants. Second, we include a non-parametric vector of control variables $f(X_{ij})$ which are constructed to account for the differential performance of schools and students 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, besides size and average test scores we also use the probability of school closure as estimated using a LASSO procedure combined with cross-validation. This probability captures school-level performance in a market-based system in which closures ares related to enrollment trends and performance in standardized testing. For students, we use a non-parametric bin model for GPA in previous years. We also check whether results are similar when using an augmented coarsened matching procedure 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.

B. The impact of police violence on educational performance

Table 7 presents estimates of equation (5). 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, although results are not statistically significant at conventional levels when using exact p-values. In terms of point estimates, we observe a persistent decrease in GPA of 0.04-0.15 points, 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 again noisy in the latter group.

Columns 4-6 in Table 7 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, although again results are at most marginally significant when using randomization inference. Finally, column 7 shows that students affected by police violence were less likely to take the college entry exam in the period 2011-2018. In particular, their probability of taking the exam decreases by 29 pp., a large decrease from an average of 86% in the control group. In contrast to the previous columns in this table, the latter two estimates are statistically significant when using randomization inference (exact p-value of 0.03). We again observe a precisely estimated null effect on neighbor students in terms of dropout rates and the college exam.

In early December of 2013 it was announced that the school of the student killed was going to be closed. According to anecdotal evidence from interviews, the announcement was surprising.[26] The closure was announced after the academic year ended in 2013 claiming a decrease in enrollment rates, and the school was (and remains) closed. Given that this closure is likely to have had an impact on dropout rates (Grau et al., 2018), it can introduce bias in the educational impacts on 2014 and afterwards. Given that the closure was unexpected from the point of view of students, it is unlikely to have had an impact in years 2011-2013. As robustness check, we constructed a measure of school performance to compare students in treated and control groups that were enrolled in schools with a similar probability of being closed. Operationally, we estimate a cross-sectional probit regression using data from 2010 and before in which we empirically predict an indicator that takes the value of one for schools that were closed on a LASSO-selected vector of changes in enrollment and other characteristics of schools. We then 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. Reassuringly, column 8 shows that results are if anything stronger, suggesting that the closure of the school is unlikely to be confounding our estimates.

As robustness check, we re-estimated the impact on educational performance using the augmented matching that exploits the availability of standardized tests for a subsample of students. We consider this to be an important exercise because it guarantees that we are comparing students with similar educational performance before the shooting. Table A.8 presents results from this ex-

[26]An interview with a member of the student assembly can be found in this link. The interview took place on December 10th of 2013 and the student said that "[. . .] last week the owner of the school told us we didn't have a school anymore, which at this time of the year we do not consider to be an appropriate decision" (own translation).

ercise and point estimates are essentially of the same economic magnitude. In addition, panel F in Figure 5 shows that the results are similar using combinations of the baseline variables to perform the matching. To explore heterogeneous effects based on social proximity, we use equation (5) and add an interaction term between $S_{j(i)}$ and an indicator that takes the value of one for the subset of schoolmates that were in the same grade of the student killed. Table A.9 presents estimation results. The evidence is mostly inconclusive with sometimes pointing towards a larger negative impact on same grade schoolmates and sometimes suggesting the absence of heterogeneous impacts. 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.10). We conclude that police violence is associated with a persistent negative impact on educational performance that is widespread across the schoolmates of the student killed.

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 deteriorating educational performance. 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 the key 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.

The focus of our empirical analysis has benefits and limitations that are worth mentioning. Among the benefits is that the actions of students are well documented and as a consequence they are somewhat easier to track over time. The measurement of protest behavior for thousands of individuals in multiple days using administrative data is unusual. However, a limitation is that high school students are still in their formative years and thus they might be particularly sensitive to this type of violence. As such, we hypothesize that the impact of police violence on the protest behavior and cognitive outcomes of adults could be smaller. Similarly, our focus on one salient act of police violence has the benefit of being precisely defined and allows an easier tracking of the subpopulation more exposed to it. But acts of violence 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 to the impact of police violence.

Finally, we believe that the results in this paper 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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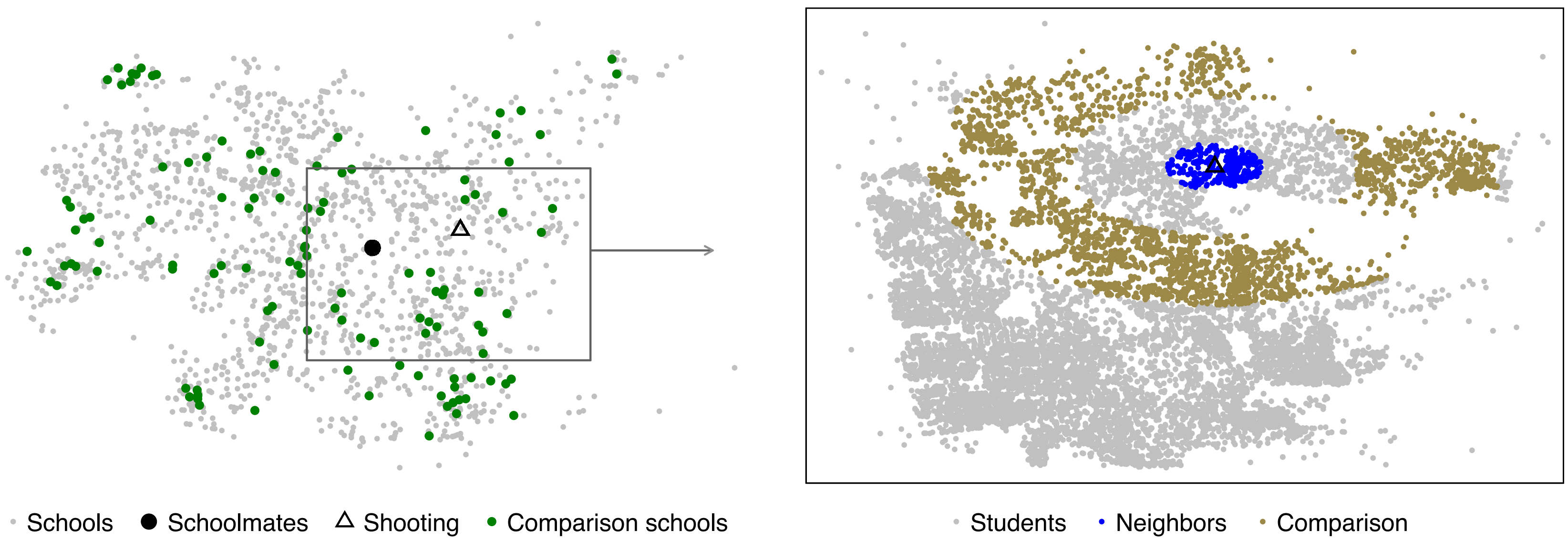
Figure 1: Schools and students in the analysis

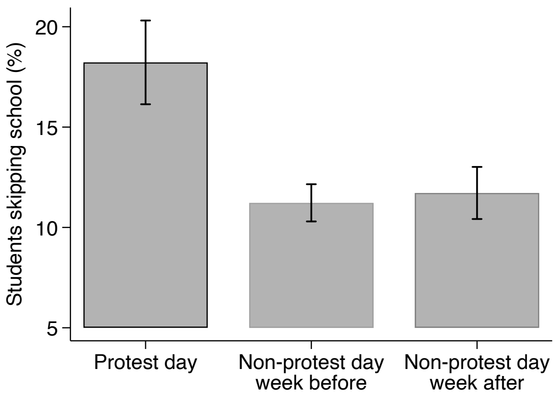


(a) Schools

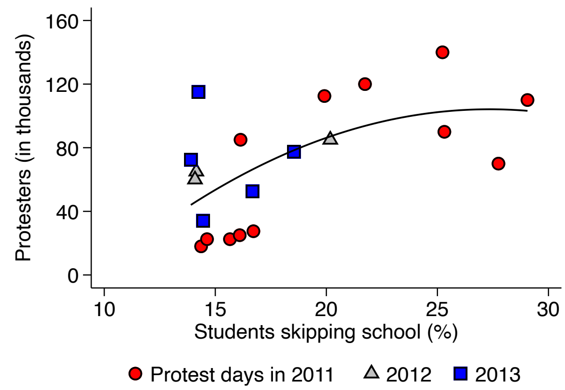
(b) Neighbor students and comparison group of students

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).

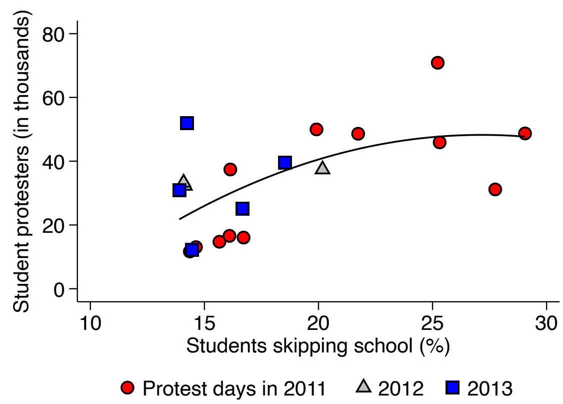
Figure 2: School skipping and protesters



(a) School skipping and protest days



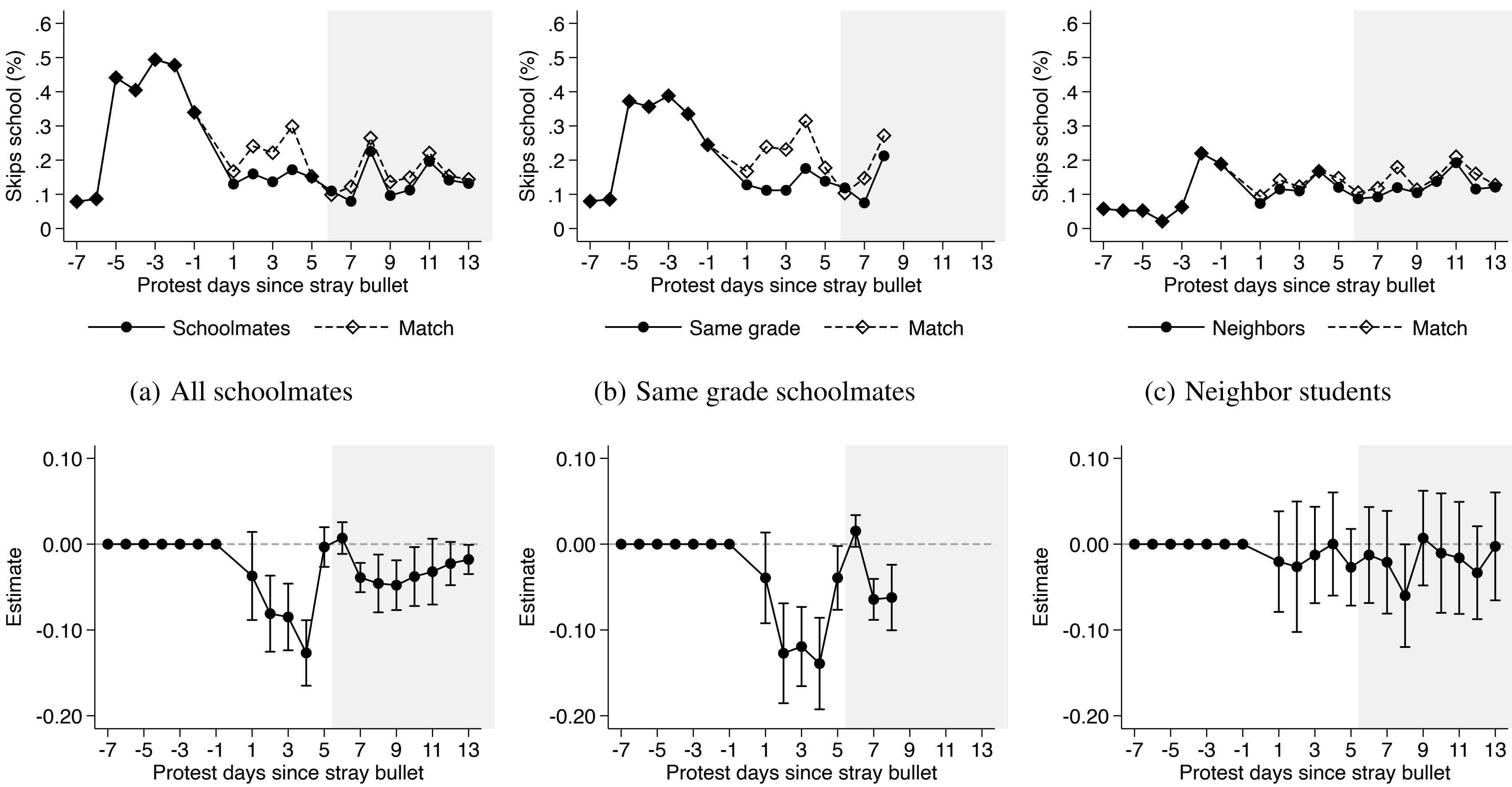
(b) School skipping and protesters



(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 3: School skipping in weekday protests before and after the student was killed

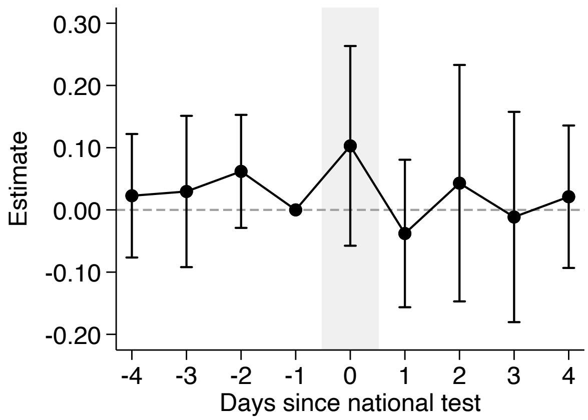


(a) All schoolmates (b) Same grade schoolmates (c) Neighbor students

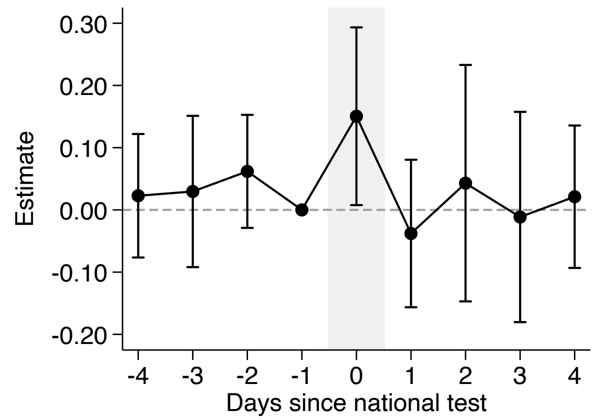
(d) All schoolmates (e) Same grade schoolmates (f) Neighbor students

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 "Same grade schoolmates" graduated in 2012 and thus we do not observe them in 2013.

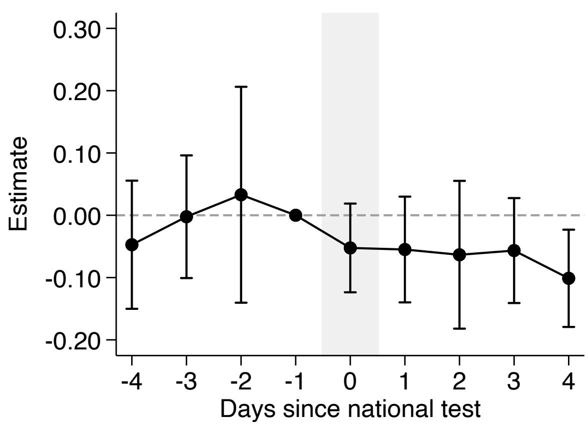
Figure 4: Student-led boycott



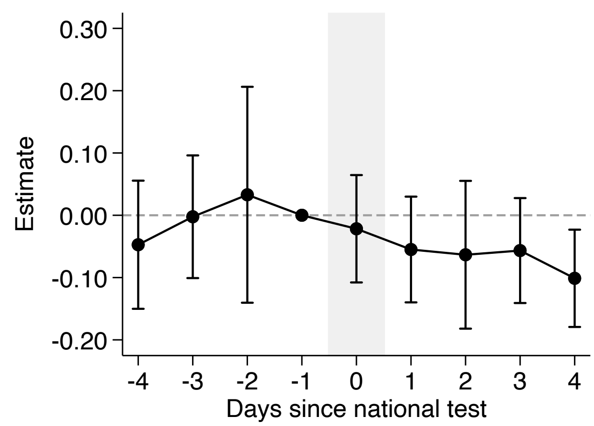
(a) Skips schools, schoolmates



(b) Skips test, schoolmates



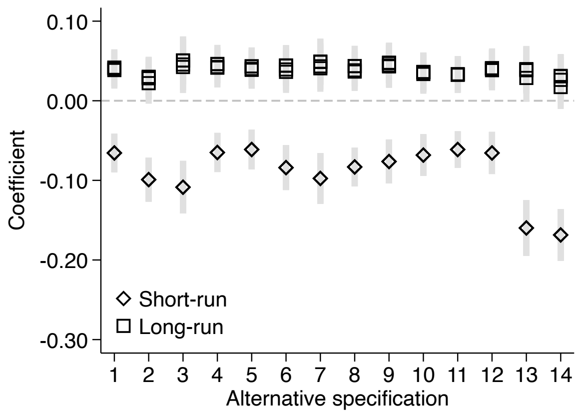
(c) Skips schools, neighbor students



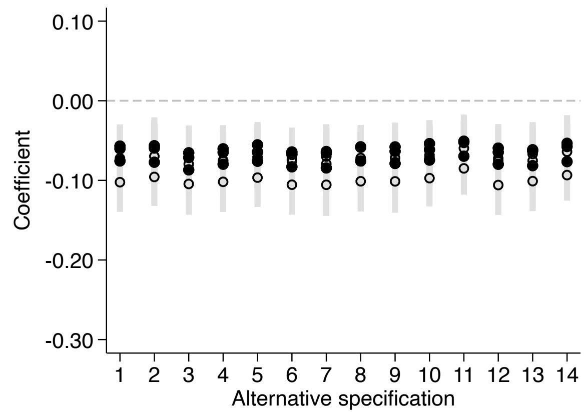
(d) Skips test, neighbor students

Note: 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.

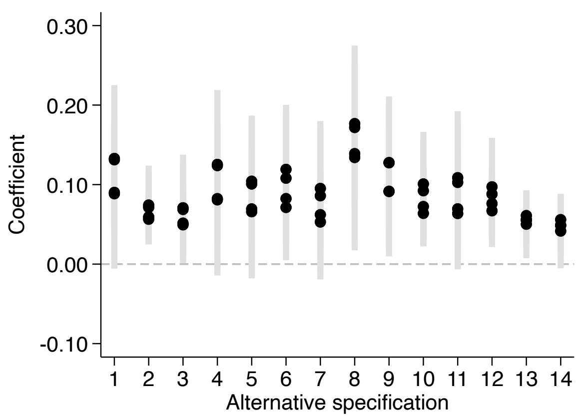
Figure 5: Robustness to alternative specifications of the matching



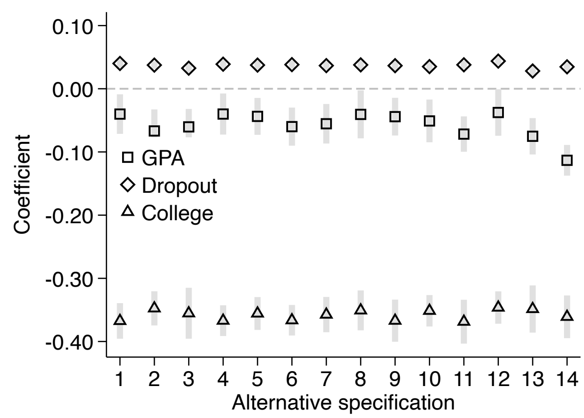
(a) Protest results in Table 3



(b) Less protest behavior in Table 4



(c) Boycott results in Table 5



(d) Educational results in Table 7

Notes: This table shows that the estimated impacts of the police shooting (*y*-axis) are robust to 13 alternative specifications (*x*-axis, specification 1 is the baseline result in all panels). In these additional specifications we omit skipping indicators in even protest days before the shooting (alternative 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 "Short-run" and "Long-run" results 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. The "Educational" results correspond to the (negative) impact of the shooting on the schoolmates of the student killed. The vertical gray lines represent 95 percent confidence intervals.

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

Year	Month	Day	Estimated number of protesters in the rally: By police	Estimated number of protesters in the rally: 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 2: Summary statistics for students and school in the analysis

	Social proximity			Geographic proximity		
	All	Schoolmates	Matched sample	All 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 3: 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	0.07	0.04	0.07
	(0.01)	(0.01)	(0.01)	(0.01)
	[0.30]	[0.30]	[0.30]	[0.30]
Observations	323,085	400,539	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: 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. We observe twelve protest days in 2011, three in 2012, and five in 2013. Estimation using different specifications of linear probability models. Standard errors are clustered at the school level in parentheses and p-values from randomization inference in square brackets.

Table 4: 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				
Same grade × After × Protest day		-0.10	-0.08	-0.08
		(0.02)	(0.02)	(0.02)
		[0.03]	[0.10]	[0.10]
Same grade × 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 5: 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	0.08	0.08	0.13	0.13
	(0.05)	(0.04)	(0.05)	(0.04)
	[0.12]	[0.12]	[0.08]	[0.08]
Observations	17,188	30,933	17,188	30,933
Students	3,441	3,441	3,441	3,441
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 6: 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]	[0.11]	[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
Avg. 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.

Table 7: 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	-0.36
	(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
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	617
Average dependent variable	5.35	5.32	5.39	0.04	0.06	0.04	0.78	0.79
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. Standard errors are clustered at the school level in parentheses and p-values from randomization inference in square brackets.

ONLINE APPENDIX

Fear of the Police: Evidence from Student Protests

Felipe González and Mounu Prem

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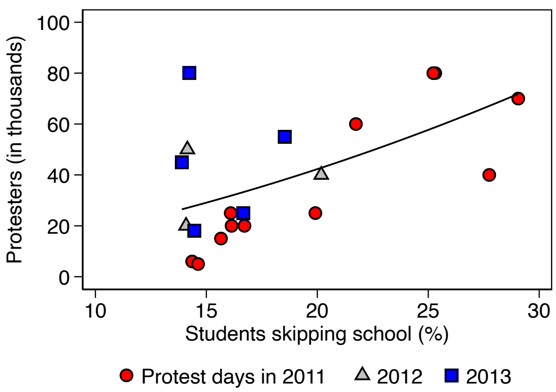
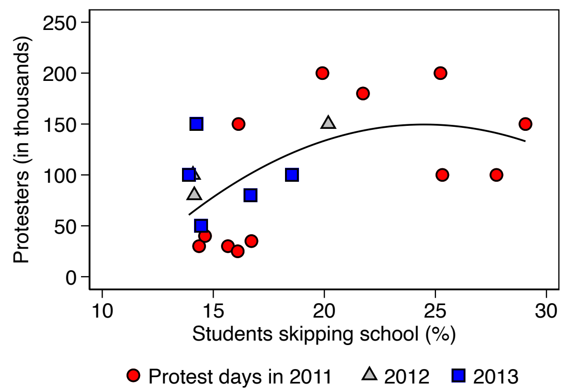
Figure A.1: Information about protest days

Notes: Flyers circulating before protest days in 2011.

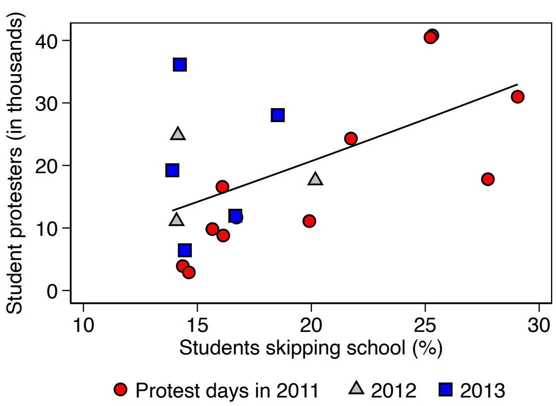
Figure A.2: School skipping is robustly related to the number of protesters



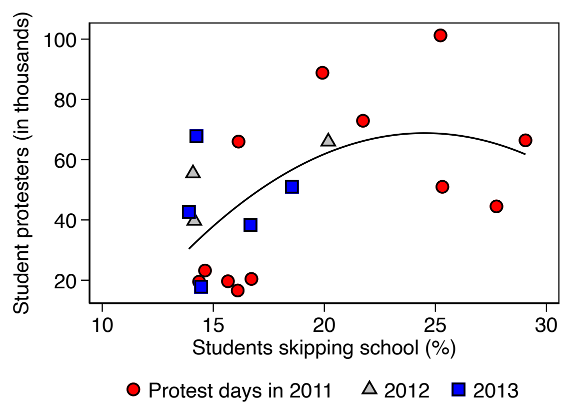
(a) Police reports: Protesters



(b) Organizer: Protesters



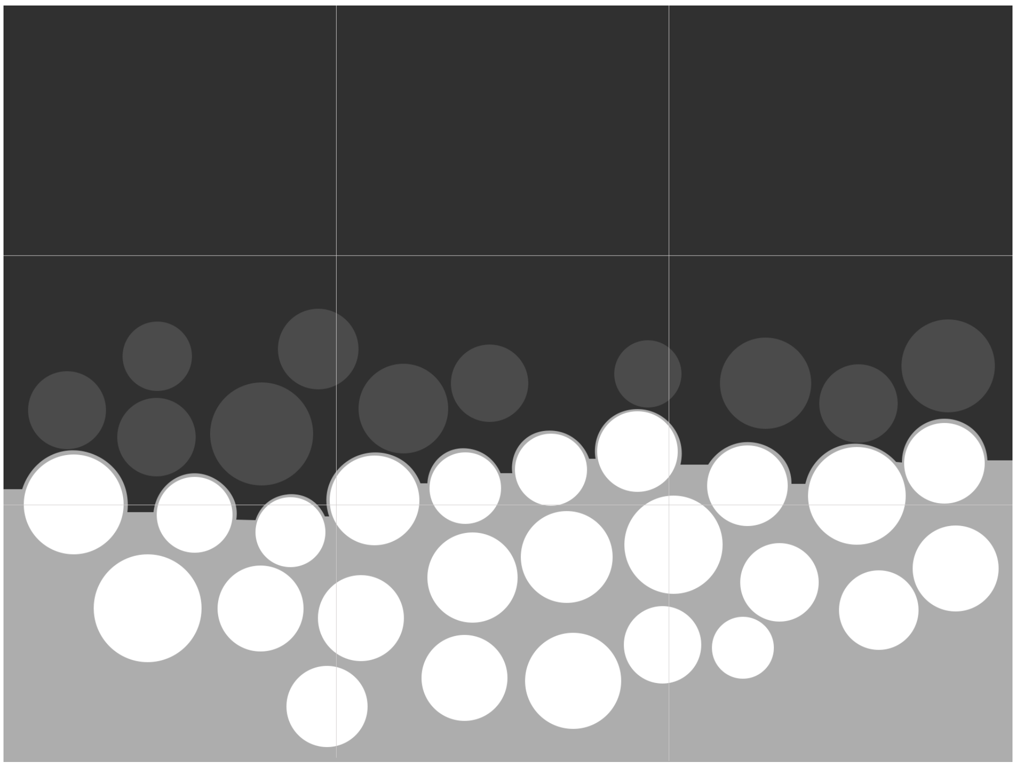
(c) Police reports: Student protesters



(d) Organizer: Student protesters

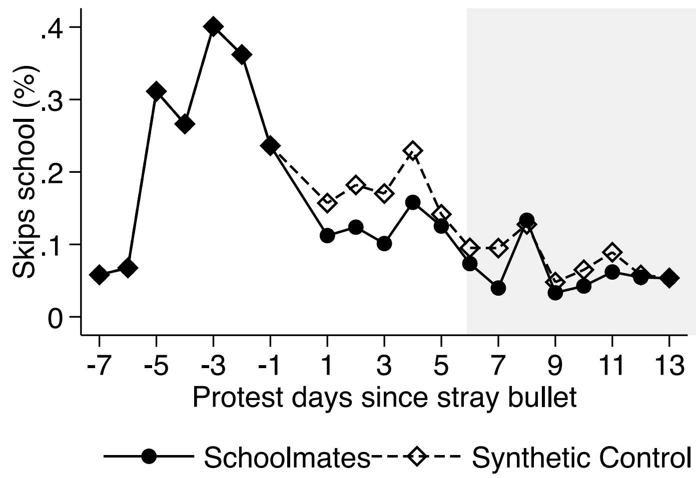
Notes: Own construction using data from police and organizer reports. These figures present the partial correlation between the percentage of high-school students skipping school and the total number of protesters (Panels A and B), and the partial correlation with student protesters (Panels C and D).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

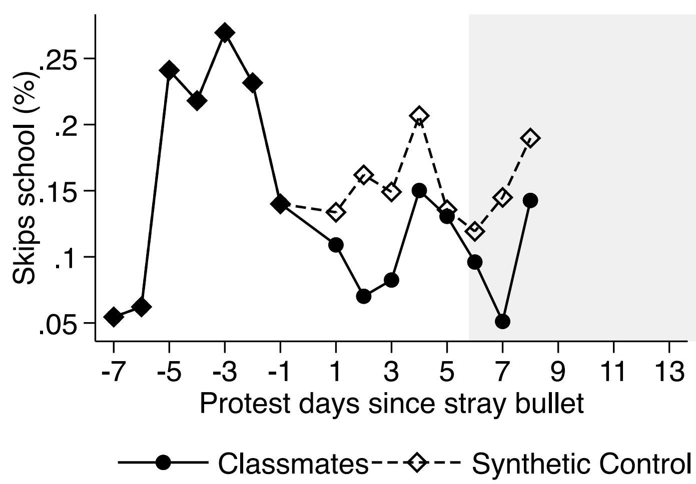


Notes: This figure presents the sketch of an image, where a crowd is identifiable in the front, and a non-identifiable crowd is located in the back. The classification of the image into identifiable and non-identifiable areas was done by a research assistant who was unaware of the goal of this exercise. 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: Synthetic control estimates



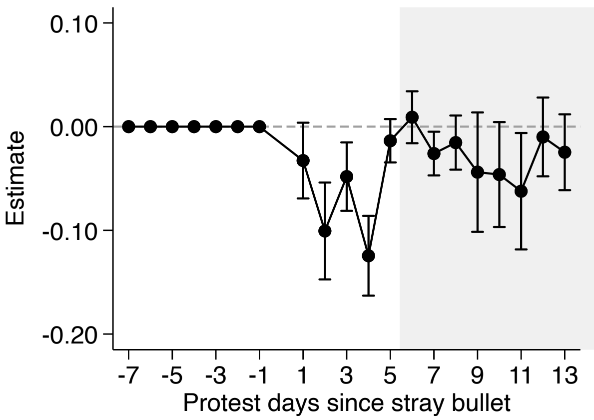
(a) Schoolmates



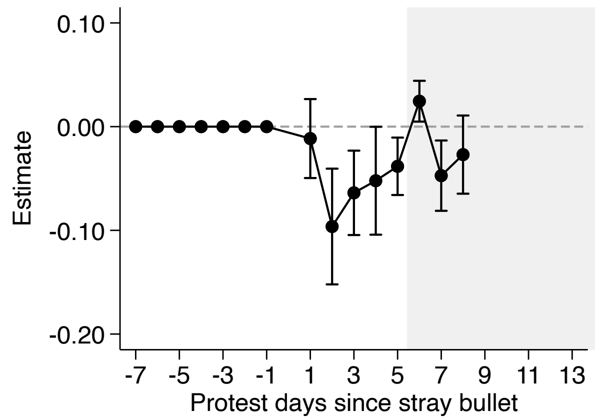
(b) Classmates

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) constructs the counterfactual for all schoolmates of the student killed and panel (b) 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. The average differences between the treated group and their corresponding synthetic control in 2011 are -5.1 and -4.9 percentage points in panels (a) and (b) respectively. The same differences after 2011 are -1.7 and -5.5 percentage points respectively. 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.

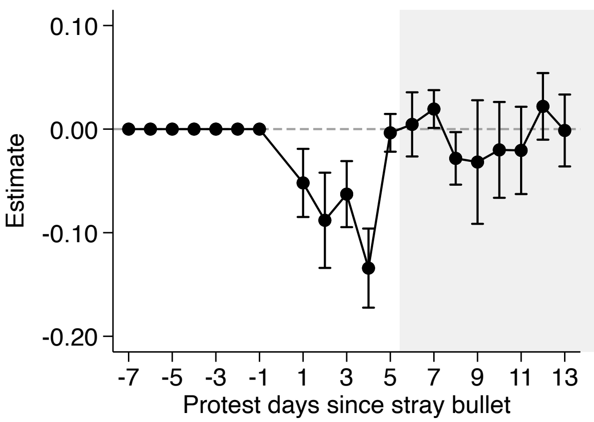
Figure A.5: Alternative matching strategies with additional covariates



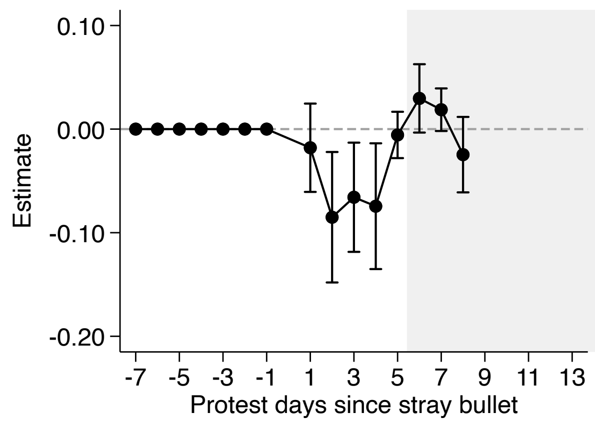
(a) All schoolmates, matching #2



(b) Same grade, matching #2



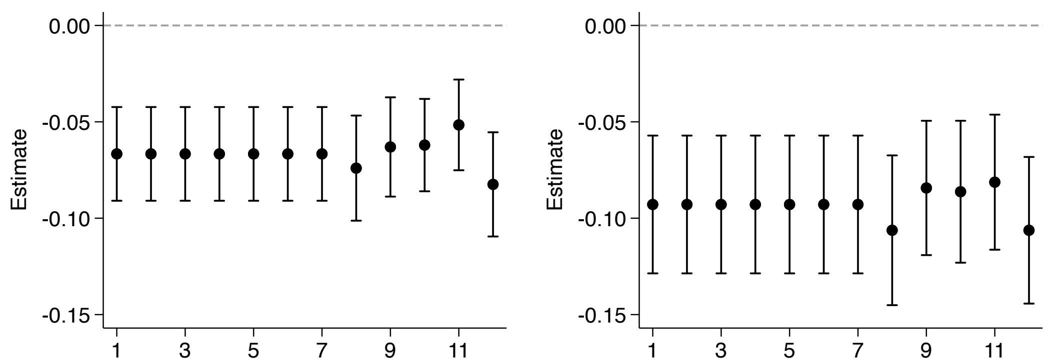
(c) All schoolmates, matching #3



(d) Same grade, 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 "Same grade schoolmates" 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) Same grade

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.

Table A.1: 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)	(0.12)	(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.2: 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) and the number of students 14-18 years old skipping school that day. The number of protesters comes from Table 1. Robust standard errors in parentheses. All coefficients are statistically significant at the 5%.

Table A.3: Main estimates using a dynamic specification

Student exposed:	Schoolmates		Neighbor students (< 0.5 miles) compared to students who live...	
	All	Same grade	[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.4: 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		Same grade	
	(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.5: 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.6: 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: 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. We explore the impact of non-lethal police repression during protests held in August of 2012 using data from a social organization. The victims were 14-18 years old students, their school is clearly identified, and there is photographic evidence of the consequences of repression (e.g. bruises, broken teeth). We use the same strategy but estimate the impact of only on the 3,500 schoolmates (grades are unknown) and the matching delivers a control group of 24,000 students. Standard errors are clustered at the school level.

Table A.7: 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) $\times$ 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: 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. We estimate the change in protest behavior among 14-18 years old *after* the death of a 14-18 yrs old person in August of 2011 in the same county. We identified deaths using administrative data from the National Health Statistics Bureau (DEIS) and the causes of death using the International Classification of Deaths (ICD). Standard errors are clustered at the county level.

Table A.8: 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
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.

Table A.9: 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.10: 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.