Police Repression and Protest Behavior: Evidence from Student Protests in Chile*

Felipe González   Mounu Prem

Police repression is common in street protests but evidence about its impact is limited. We study the protest behavior of people linked to a student killed by a stray bullet coming from a policeman during a large protest. We use administrative data to follow his schoolmates and those living nearby in hundreds of protest and non-protest days. We find that repression causes a temporary deterrence effect but only on students with social links to the victim. Moreover, police repression increased adherence to a student-led boycott and had negative educational consequences, casting doubt on its effectiveness as a policy of deterrence.

Keywords: police repression, state repression, protest, students.

*December 2020. González: Pontificia Universidad Católica de Chile, Instituto de Economía. Prem: Universidad del Rosario, Department of Economics. We would like to thank Josefa Aguirre, José Ignacio Cuesta, Bruno Ferman, Marc Rockmore, Nicolás Rojas, Ignacio Silva, Danae Valenzuela, Noam Yuchtman and seminar participants at NEUDC, PUC-Chile, and Universidad del Rosario for comments and suggestions. We also thank Carolina Bernal, Katia Everke, Anita Zaldivar, and Catalina Zambrano for outstanding research assistance. Corresponding author email: fagonza4@uc.cl

1 Introduction

Police repression is a common feature of street protests around the world. Teargas, pepper spray, smoke bombs, stun grenades, beanbag rounds, pellet guns, batons, rubber bullets, and even gunshots are routinely used by officers to deter protesters and ensure public safety (Atkinson and Stiglitz, 2015). Although many theories of political violence assume that state-led repression acts as a deterrent (Acemoglu and Robinson, 2000, 2001; Besley and Persson, 2011), scholars have also argued that it might spark dissident behavior (Davenport, 2007).[1] The long-standing debate continues as empirical evaluations documenting the consequences of protest repression remain limited. The lack of evidence is unsurprising given the difficulties in measuring officer-related violence and protest behavior (Fisher et al., 2019), which has pushed researchers to the use surveys and laboratory evidence. 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 one of the first pieces of evidence of the impact of police repression on protest behavior. The context is the largest student-led movement in the history of Chile, where we observe multiple protest-related decisions of thousands of individuals before and after an extreme event of police violence. On the eve of a large protest, a sixteen-year old student was killed by a stray bullet coming from a policeman. Using administrative data on daily school attendance, we follow his schoolmates and students who lived nearby the shooting, in hundreds of protest and non-protest days to study if police repression affected their protest behavior as measured by school skipping decisions during weekday protests. We use administrative and survey data to validate school skipping in protest days as a measure of protest behavior. The empirical strategy relies on the inherent randomness of the stray bullet, both in terms of the affected students and the timing of the event, and employs coarsened-exact matching in panel data to construct a counterfactual composed by students that protested identically before this lethal act of police repression.[2]

Our main finding is that police violence had a temporary deterrence effect on subsequent protest decisions, but only among students who had social links to the student killed. We begin by showing that school skipping almost doubles in protest days and it is highly correlated with protest size. Then, we present results in three parts. In the first part we use a simple difference-in-differences

[1]A large theoretical literature argues that state repression can backfire and *increase* political dissent, perhaps because repression 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.

[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).

estimator which reveals that the death of the student caused a decrease in the protest behavior of his schoolmates in the following four days of national protest. In particular, the probability that his schoolmates skipped school in those days decreased by 7-9 percentage points from an average of 33% in the control group. The magnitude is sizable and corresponds to an economically meaningful decrease of 21-27%. Crucially, the school skipping rate of schoolmates was similar to the counterfactual during *non*-protest days. We interpret the similar attendance to regular school days as reassuring of the protest-related nature of their decisions in days of protest. In contrast, students who lived nearby the shooting remained protesting in a similar way than before. Moreover, we use data from a social organization documenting non-lethal acts of police repression, applied the same econometric strategy, and failed to find evidence of a deterrence effect in protest behavior.

The second part shows that in the long-run all students exposed to the shooting protested similarly (or more) than their comparison groups. We begin with an extended analysis of school skipping decisions in nine weekday protests in 2012 and 2013, which were also organized by students. We find that more than half of the decreased in protest behavior disappears in 2012, and the deterrence effect completely vanished in 2013. Moreover, we confirm the lack of a persistent deterrence with an empirical examination of a student-led boycott to a standardized test. A week before the test, student leaders of prominent schools and the two largest student unions called to boycott the test by not taking it, not answering the questions, or to simply skip school. Students were protesting against a high-stakes test that, according to educators and researchers, introduced perverse incentives in the system and increase segregation. Using administrative data we construct an indicator of individual adherence to the boycott by combining data on test takers and school skipping. We find that the schoolmates were *more* likely to participate in the student-led boycott.

The third part focuses on the educational consequences of repression. We study educational performance as measured by GPA and dropout rates in the following years after the student was shot. We also study the decision to take the standardized test used for college admissions. We find that, consistent with previous literature (Ang, 2020), police violence is associated with lower grades, higher dropout rates, and a lower probability of taking the college exam. The impact on GPA is similar to evidence from police killings in the U.S. In addition, we find that dropout rates more than doubled among the schoolmates and the probability of taking the exam for college admission decreases by 20 percentage points from a baseline of 70% in the comparison group. These findings call into question the effectiveness of police violence in quieting protest activities and contribute with additional evidence on the negative consequences for students. Moreover, given that the police is involved in more acts of aggressive behavior towards protesters, these results arguably constitute a lower bound of the social cost of police violence during street rallies.

Taken together, our findings suggest that police repression fails to deter protesters and has significant negative consequences. We explore the mechanisms behind these findings using insights from previous research (Aytac et al., 2018). We argue that changes in risk assessment arising from fear and anger are likely mediators. In contrast, heterogeneous information about the event is unlikely to explain the results. Several patterns in the analysis pushed us towards this conclusion. In particular, the deterrence effect is significantly larger among students who regularly shared classes with the student killed than other (equally informed) students enrolled in the same school. Similarly, the lack of a deterrence on students living nearby the shooting – likely better informed than those living farther away (Fujita et al., 2006) – suggests that information is unlikely to be relevant in this context. Finally, the negative impact on educational outcomes is arguably related to the psychological consequences of experiencing repression rather than differential information.

Our main contribution is to provide evidence of the impact of police repression on protest decisions using individual-level administrative data. Previous research has studied the impact of police and state repression on dissident behavior using laboratory evidence, online surveys, and aggregate data (García-Ponce and Pasquale, 2015; Lawrence, 2017; Aytac et al., 2018; Young, 2019b; Rozenas and Zhukov, 2019; Curtice and Behlendorf, 2020; Bautista et al., 2020b).[3] 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 arguably exogenous event of police repression over multiple years, which allows us to estimate the impact of repression over different time horizons outside of the lab.

We also contribute to the literature studying protest behavior at the individual level by estimating the impact of police repression. 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 during protests on subsequent protest behavior. In line with insights from a theoretical literature (Davenport, 2007), our results show that police violence has a transitory deterrence effect.[4]

[3] A related literature shows that police crackdowns can lead to more violence (Dell, 2015) and military bombing can backfire an increase insurgency (Dell and Querubin, 2018).

[4] A related literature studies police violence as a "trigger event" of a wave of protests (Williamson et al., 2018). Examples of these events include the shooting of Michael Brown and the following wave of protests in Ferguson in

Our results also speak to a recent literature that documents the negative consequences for students when exposed to police violence. Although research studying the cognitive impacts of violence is vast (Carrell and Hoekstra, 2010; Sharkey, 2010; Monteiro and Rocha, 2017), evidence on the effects of violence when coming from the police is more limited. A leading example comes from Los Angeles in the U.S., where high-school students who lived nearby an officer-involved killing experienced worst educational performance and psychological well-being (Ang, 2020). These negative psychological effects on students also appear after school shootings (Rossin-Slater et al., 2020). 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.

2 Background

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 across the country 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., 2020a). In 2011 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 to exert pressure on the government before the annual speech 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 the largest 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 (*Asociación Nacional de Empleados Fiscales*) and

2014, and the killing of Arthur McDuffie and the 1980 Miami riots (DiPasquale and Glaeser, 1998). In contrast, we focus on police violence *during* protests and the future protest behavior of those exposed to these violent actions.

the largest workers union in the country (*Central Única de Trabajadores*).[5] 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 Santiago took the form of a march from the main square to La Moneda Palace where the seat of the president is located, but rallies and barricades took place in several parts of the city all day long.

The sixteen years old Manuel Gutiérrez was killed by a police gunshot on the night of August 25 of 2011.[6] 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.[7] 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 his home. 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 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. 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. However, the evidence accumulated and only a couple of days after the event the policeman behind

[5] 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.

[6] 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).

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

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), "there was no intent [from the police to kill the student], we ask the family for their forgiveness" (September 2, 2011), among many other examples. Although information about the role of the police role was available, learning about it is an endogenous decision which we discuss below.

3 Data

3.1 Protest days, students, and exposure

We identified protests taking place in weekdays during the 2011, 2012, and 2013 academic years. As explained below, the focus on weekdays is solely based on our interest in school skipping decisions. We collected data on the (estimated) number of people who attended the rally from traditional media outlets such as *La Tercera* and *El Mercurio*, and complement it with data 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 protests data. 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, 3 in 2012, and 6 in 2013 for a total of 21 protest days. Seven of these protests took place before the student was killed and 14 took place after this event.[8] 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 – public or private-voucher – in the Metropolitan Region in 2011. This region is by far the most populated area in the country with almost half of the population (8 million), hosts the capital (Santiago) and corresponds to the area where the largest protest events took place in the academic years between 2011 and 2013. These students were 14-18 years old and were enrolled in grades 8-12 in 2011. Column 1 in Table 2 presents summary statistics for these students and schools.

We study the impact of police violence on two groups of students that were exposed to the shooting. The first group are the almost 750 schoolmates of the student killed by the stray bullet and we refer to them throughout the paper as simply as "schoolmates." We also look at the subgroup of 200 schoolmates 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 these students and 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.[9] 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.[10] Panel (b) in Figure 1 plots the location of these 13,000 students. We follow Ang (2020) 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

[8]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.

[9]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.

[10]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.

"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.

3.2 Daily school attendance and protests

We measure individual protest behavior in the population of students $i \in \mathcal{I}$ 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 school 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 December, with a winter break in July. Previous research has shown that school skipping rates increased sharply in protest days during the 2011 academic year (González, 2020). To ensure that a decision to skip school was made by students, we drop the set of schools that were reported as closed during the day of the protest. We detect school closures using the administrative data and complement it with a stringent definition in which we consider the school as closed in a given day if no student was reported present.

We offer three empirical exercises to support the use of skipping decisions as a measure of protest behavior. First, school skipping rates increased sharply and significantly on protest days. Panel (a) in Figure 2 shows that in a weekday protest the average 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%. Thus school skipping increases by 7 percentage points during protest days, an increase of 64% over the mean during non-protest days. 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 21 protest days we study.[11] To better estimate this correlation, Table A.2 present the corresponding regression coefficients. 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 within protests in a given year. Skipping and year effects explain more than 40-50 percent of the variation in protest size (columns 2 and 4), a large number 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. This method

[11]For this exercise we use the average of the number of protesters reported by the police and organizers. Figure A.2 shows that the correlation is also strong and positive with each one of these measures separately.

further supports the use of school skipping as a measure of protest behavior. We proceeded in three steps. First, we downloaded videos of all the protests in our data from YouTube.[12] Second, we selected 10 random images from the largest shots of each video to maximize coverage of attendees. Lastly, we used a survey to ask college students – at the time high-school students themselves – to count the number of high-school students in each of these images.[13] We obtained approximately 4,500 responses from 450 college students. Column 6 in Table 1 presents results which suggest that approximately 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 school 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 students who skipped school decided to attend the rally (24,000 over 90,000).

4 Econometric strategy

This section presents our econometric strategy to estimate the impact of police repression on protest behavior. We describe the work-horse econometric models which we build upon in next sections. In short, we use a difference-in-differences strategy combined with an exact matching to select the comparison group that acts as the counterfactual. The estimation relies on the inherent randomness of the stray bullet, both in terms of the affected students and the timing of the event. Spillovers on educational performance are estimated using the same matching procedure in the cross-section of students and we discuss below its strengths and limitations.

As mentioned, we study two groups of students who were particularly exposed to the police shooting. The first are the 750 schoolmates of the student killed. The second are the 250 students who lived nearby the event. To estimate the impact of police violence on these students we need a comparison group to estimate the counterfactual outcome of students in the absence of the shooting. Given the presence of hundreds of thousands of students living in the same city, we use coarsened exact matching to select two groups of students that we argue constitute a valid counter-

[12]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.

[13]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.

factual. After explaining the selection of the comparison group, we present the estimating equation and the assumptions needed to interpret estimates as the causal effect of police violence.

4.1 Selection of the comparison groups

The selection of the comparison group is based on a matching procedure that uses information before the shooting and proceeds in two steps. The pool of potential students in the comparison group comes from the 300,000 students in the Metropolitan region. In the first step, we find 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. In the case of the schoolmates, this step decreased the number of schools from 2,000 to 122 and the number of students to 44,331. In the second step, we find students who were observationally equivalent in terms of 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 through August). 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 students in 416 cells. Column 3 in Table 2 shows characteristics of the comparison group.

We use a similar procedure to build a comparison group for students who lived nearby the event. The potential controls are the 4,000 students who lived within 3 miles of the shooting, were enrolled in grades 8-10 in 2011, and reported a valid address. We applied the coarsened exact 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.[14] 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.[15]

[14]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 repression on those living nearby the home and the school of the student killed.

[15]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.

4.2 *Estimating equations*

The core of our econometric strategy exploits the high frequency of the data and within student variation in school skipping decisions across 12 weekday protests in 2011. More precisely, we begin the analysis by estimating the following regression 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 student ϕ_i and cell-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 students that were observationally identical before the student was killed. The indicator $S_{j(i)}$ takes the value of one for classmates of the student killed ("schoolmates"). In the case of geographic exposure $S_{j(i)}$ takes the value of one for students who lived within 0.5 miles of the shooting ("neighbors"). The indicators D_t^k take the value of one for each of the protest days after the event.[16] 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 or neighbor students when compared to their respective comparison groups.

We also use an augmented version with more structure in which we exploit skipping decisions in non-protest days. The idea is closely related to one of a placebo test. In particular, if results are related to *protest* behavior, then we should *not* observe an increase in skipping rates during days without protests, otherwise it raises concerns about a change in non-protest behavior. We then stack non-protests days to the protest days data and estimate the following equation:

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

where all variables and estimation methods are defined as before and we include two additional indicators. The first indicator is "Protest Day_t" and takes the value of one for days with a protest and zero for non-protest days. The second indicator is "After_t" and takes the value of one for the period after the student was killed. The coefficient of interest is γ_1 and measures the differential skipping decisions after the event during protest days, using non-protest days after the event as

[16] 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}.

comparison. In contrast, γ_2 represents the "placebo" exercise and measures the differential skipping decisions after the event in non-protest days. The difference $\gamma_1 - \gamma_2$ is our measure of protest behavior. Note that it is indeed plausible that police shootings increase school absenteeism more generally (Ang, 2020), in which case we expect that $\gamma_2 > 0$ and the difference $\gamma_1 - \gamma_2$ to reveal the *additional* impact of violence on protest behavior.

4.3 Inference

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 simple procedure based on randomization inference (Fisher, 1935; Young, 2019a). More precisely, we proceed in three steps. First, we assign the treatment to a *control* school, implement our econometric strategy – both the matching and the estimation – 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. In the case of neighbors, we use the Conley (1999) spatial HAC standard errors with the exact home location of students and allow decisions to be correlated within 3 miles. 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.[17]

[17] 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.

5 Protest behavior

We begin with a descriptive analysis of protest behavior 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. We focus on protest days in 2011, seven of which took place before the stray bullet and five afterwards. Panels (a) and (b) in Figure 3 strongly 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 decrease in this protest behavior appears to be somewhat transitory given that in the fifth protest after the event both groups skipped school similarly. In contrast, panel (c) reveals smaller differences between students who lived nearby the event and the comparison group. We now proceed to a discussion of results using the previously described econometric strategy.

5.1 Short-run: Protest days in 2011

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 in protesting behavior among the schoolmates. The largest impact appears in the second to fourth protest days, which took place approximately one month after the student's death. In particular, the schoolmates had a skipping rate that was 12 percentage points lower that the comparison group during those three protest days. 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 somewhat larger in magnitude when we focus on the subsamples of schoolmates who were in the same grade, suggesting that social proximity is an important explanation for these findings. Panel (e) uses the sample of students who lived nearby and their comparison group and results are weaker, with perhaps a smaller deterrence effect that is not statistically different from zero. Table A.3 presents the corresponding regression coefficients for these figures.

Table 3 presents a more parametric version of equation (1) in which we estimate the average impact in the five protest days after the event and results confirm a deterrence effect. Panels A and B present estimates related to social and geographic proximity respectively. We present results using day fixed effects (odd columns) and a more flexible specification that includes cell-by-day fixed effects (even columns). Panel A shows that the schoolmates (columns 1-2) and those in the same grade (columns 3-4) had 7 and 9 pp. lower skipping rates after the student was killed. These coefficients are statistically different from zero when using standard errors clustered by school (p-value<0.01) and marginally insignificant when using randomization inference (p-values of 0.10 and 0.16). Panel B shows that the geographic exposure is associated to a smaller impact on protest behavior which is not statistically different from zero when using clustered standard errors. Conley (1999) spatial errors always deliver more statistically significant estimates (i.e. lower standard errors) and thus we conservatively report clustered standard errors in the remaining of the paper. The point estimates are always similar if we use day or cell-by-day fixed effects. However, the precision of coefficients increases with the latter set of unobservable heterogeneity.

Table 4 examines the potential contribution of non-social spillovers to the previous findings. In particular, we worry that some of the impact that we interpret as related to social proximity might arise for non-social reasons. Two leading concerns motivate this analysis, one related to the impact of the shooting on schools nearby the event and another one related to across-school spillovers. Fortunately, the analysis suggests these are unlikely to confound our interpretation. In columns 1-3 we drop from the analysis of schoolmates all students in the control group that were enrolled in schools located within 3, 4, and 5 miles from the shooting. Reassuringly, results are remarkably similar and again larger among schoolmates in the same grade. In column 4 we follow a similar strategy but drop all students enrolled in "connected schools." We define school s as connected to the school of the schoolmates $\tilde{s}$ if some student migrated from school s to $\tilde{s}$ in the previous year (2010). Our concern is that *past* social links confound some of the contemporaneous social effects. The exercise reveal again similar estimates with statistical significance when using clustered errors and p-values between 0.10 and 0.20 when using randomization inference.

Table 5 presents our most complete analysis of school skipping decisions. These results confirm the impact on protest behavior. Column 1 replicates the previous estimation strategy but using non-protest (instead of protest) days. We observe a precisely estimated impact of zero, i.e. police violence during the protest had zero impact on school absenteeism in days without a protest. The remaining columns present estimates of equation (2) where we study school skipping in protest and non-protest days jointly. Column 2 stacks all non-protest days in 2011 to the data with protest days. Columns 3 and 4 stack one non-protest day for each protest. To illustrate the idea, consider that most protests took place on Thursdays, so in that case we use skipping decisions from the Thursday of the week before (or after) without a protest. We implement the latter specifications (i) to improve comparability across protest and non-protest days, and (ii) because non-protest days of the same week could be contaminated by the protests if students require some level of organization.[18] Differences across estimates are revealing about the issues previously mentioned. Panel A presents estimates for all schoolmates and panel B for those in the same grade. 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 (randomization) p-values between 0.02 and 0.08. The magnitude of the coefficient is 2 pp. larger when focusing on same grade schoolmates, again suggesting social proximity matters, with (randomization) p-values between 0.03 and 0.10.

The results related to social proximity are robust to the use of two alternative matchings to select the comparison group. The first also matches the predetermined test scores of *students* (on top of schools), decreasing the sample to 498 exposed students. After applying the econometric strategy, we are left with 426 and 3,764 students in treated and control groups. The second also includes terciles of reported family income, further decreasing the sample to 362 and 2,052 students in each group. These additional exercises reveal the trade-off that we face; when we add more variables to the matching procedure, the number of treated students decreases significantly. Figure A.4 presents estimates of equation (1) using these alternative matchings and we reach the same conclusion of a deterrence effect. The estimates are also robust to other robustness exercises. Table A.5 shows that results are similar if we focus on the sample of students who remained in school (non-dropouts) and Figure A.5 shows that results are robust to the exclusion of single protest days from the estimation and thus specific protests are unlikely to be the explanation for our findings. The last robustness check uses the same econometric strategy and shows that the shooting had little impacts on students who lived nearby the home or the school of the student killed (Table A.6).

In an effort to explore the impact of non-lethal acts of police repression on protest behavior we use data from a social organization (Codepu, 2012).[19] We study acts of police repression taking place during protests held in August of 2012. 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). Empirically, we use the same econometric strategy with the date of the event. We do not observe their grade and thus estimate the impact on the 3,500 schoolmates, which in this case includes the victims themselves. Table A.7 presents results. Column 1 shows estimates from a specification with day fixed effects and column 2 with cell-by-day fixed effects.

[18] 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.

[19] 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).

The results show little evidence of a deterrence effect. If anything, the point estimates suggest that students exposed to non-lethal acts of police repression were more prone to protesting in the following protest days, although this behavior is not statistically different from zero.

5.2 Long-run: Protest days in 2012 and 2013

Student-led rallies continued in the following two years after 2011. After the December-February summer break organizations of students returned to the academic year with new leaders and a renewed interest in exerting pressure on the government to change educational institutions. The year 2012 was less intense in terms of protests, which can be seen in Table 1 in the fewer number of massive rallies. However, the year 2013 saw the return of a large a number of protests that spread throughout the entire academic year from March to October. 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.

To identify the long-run impact of police violence, we begin by looking at school skipping decisions in days of protests in 2012 and 2013. Table 1 shows that there were three weekday protests in 2012 and six in 2013. With these additional nine days, we construct a panel of students observed in 21 weekday protests and estimate the following augmented 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 again a school skipping indicator for student i, enrolled in school j in 2011, classified in cell s, and observed in protest day t. All remaining variables and estimation techniques are the same as in previous sections. The parameters of interest are β_1 and β_2 and measure the short- and long-run impact of police violence the protest behavior of schoolmates. We present results from four specifications. The first specification uses data from all protest days in 2011 and 2012 and the second uses data from all protest days in 2011-2013. The third and fourth specifications mimic the previous ones but collapse the data by period to avoid an artificial inflation of statistical power when decisions could be serially correlated within students (Bertrand et al., 2004). We consider a short-run period (2011) and a long-run period (2012-2013). For the latter the dependent variable changes from an indicator to having a mathematical support in the $[0, 1]$ interval.

The estimating samples are again composed by the schoolmates of the student killed (panel A), students who lived nearby the shooting (panel B), and their corresponding comparison group.

However, there are three differences that need to be mentioned. First, when we include protest days in other years there is mechanical attrition due to the graduation of high school students. For example, when we study decisions in 2012 we do not observe the cohort of students in their senior year (12th grade) 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. This type of attrition makes the long-run estimates arguably a lower bound. In any case, we also present estimates using the sample of non-dropouts. And third, there is some school switching. For example, we observe that 70 out of the 489 remaining schoolmates were enrolled in a different school in 2012, and 27 out of the remaining 242 switched schools in 2013. We always consider students who switched school as part of the original group of schoolmates exposed to police violence.

Table 6 presents estimates of the linear probability model in equation (3). All columns in panel A support the hypothesis that the deterrence effect of police violence was transitory. The estimated parameter $\widehat{\beta}_1$ – which measures the short-run impact – is always negative and reveals a decrease in school skipping similar to the ones presented in Tables 3 and 5, i.e. 7 pp. (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 (p-values between 0.17 and 0.30). Exactly the same patterns appear when we use the daily data and the data collapsed by period. More than half of the decrease in protest behavior is offset in 2012 ($0.04/0.07 = 0.57$) and the deterrence disappears in 2013. Combined with the dynamic coefficients in panels (c) and (d) of Figure 3, these results suggest that most of the deterrence effect disappears shortly after the act of police violence. We observe a similar pattern for the case of geographic proximity to the shooting in panel B. However, estimates are again smaller than in the case of social proximity and statistically indistinguishable from zero.

5.3 The 2013 student-led boycott

In 2013 organizations of students led a large boycott against one of the most important standardized tests in the country, the SIMCE. This test had been used for almost two decades as a crucial metric in what is arguably one of the most developed accountability systems in the world (Cuesta et al., 2020). School-level test scores are used to inform parents about the quality of schools and the state uses them to implement educational policies. Newspapers routinely disseminate rankings of schools based on test scores, and schools use their scores as an advertisement device to increase

[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.

the enrollment of students. The test had and continues to have many critics who argued that it incentivizes teaching to the test, it does not reflect school quality but rather the socioeconomic background of students, and as such it increases the segregation in the system.[21]

The mathematics and language tests had to be taken by all high-school students in twelve grade on November 20 of 2013. One week before test day, 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). Students were building the boycott using as inspiration the policy recommendations of educators and researchers, some of them members of an organization known as "Alto al Simce" (Stop the Simce). These critics wrote an influential "open letter for a new system of evaluation in education" (own translation) in September of 2013 which emphasized the problems with the test. Although calls to boycott the standardized test have been relatively common after 2013, the only previous attempt happened in 2006 in the middle of another wave of high-school protests.[22]

We test for individual adherence to the boycott using daily school attendance data together with administrative data with information about test takers. The former dataset allows us to measure the decision to skip school the day of the test. The latter dataset reveals the decision of students to not take the test even if they were in the school that day. According to the public entity in charge of the test in all but two schools the test was implemented as planned. This is important because it means that students had the opportunity to decide whether to take or not the test. We focus on a narrow window of weekdays around the day of the test and construct a panel data of high-school students in the 12th grade who we observed daily. These students were enrolled in 8th grade in 2011 and they were also deterred from protesting after the gunshot (Table A.4, column 1). Then, we estimate the following regression equation using the daily panel of students:

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

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. The first corresponds to an indicator variable that takes the value of one if the student decided to skip school. The second dependent variable is an indicator that takes the value of one for students who decided

[21] 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).

[22] Similar boycotts have also been observed in the U.S. An example is the well-known teacher boycott in Seattle that sparked a national conversation about the use of standardized tests in public education (Hagopian et al., 2014).

to skip the test. We define skipping the test as either skipping school or going to school but not taking the test. Finally, 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 of interest is τ and measures the differential adherence to the boycott of students exposed to police violence when compared to the matched sample of students.

Table 7 presents estimates of equation (4). Panel A studies the impact of social proximity by looking at the schoolmates. Columns 1-2 study the decision to skip school and columns 3-4 the decision to skip the test. Panel A presents evidence consistent with a higher adherence to the boycott among the schoolmates. For reference, note that the school skipping rate in the comparison group reveals that 13% of students were absent on test day, which is slightly higher than a regular year without boycott. The estimates show that the skipping rate increased by 8 pp. among the schoolmates. When we employ our preferred measure of adherence we find that participation in the boycott was twice as large among these students, with a randomization p-value of 0.08. In contrast, panel B again reveals a similar adherence among students living nearby the event and their comparison group. Figure 4 presents the non-parametric version of these estimates. The figures show that the differential decisions among schoolmates were related to the standardized test, as they exhibit a similar school attendance than the comparison group in days before and after November 20. 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.

5.4 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, it is possible that geographic or social proximity to the event 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 state repression 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 deterrence effect of repression 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 A.4 revisits this result but using grade-to-grade variation to test for the existence of this pattern more flexibly. The deterrence effect on 11th graders is almost twice the size of the impact in other grades. 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.

6 Educational performance

This section investigates whether police violence had educational consequences for students that were socially and geographically exposed. Previous research has found negative psychological and educational consequences associated to acts of police repression in the U.S. (Ang, 2020; 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, 2019) and thus one of the most consequential decisions young people make in their life (Altonji et al., 2012).

6.1 Empirical strategy

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

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

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. Recall that schoolmates were socially close to the student killed and neighbor students were geographically exposed to the event. The parameter of interest is δ and measures the differential educational performance among students socially or geographically exposed to the shooting with respect to their estimated counterfactual. 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).

The selection of the comparison group is again the outcome of the coarsened exact matching from section 4.1. However, this empirical strategy exhibits two differences with respect to the previous estimation that are worth mentioning. First, we use cross-sectional variation instead of panel data. This decision is based on the nature of the variation in the dependent variable. 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, we use the probability of school closure as estimated using a LASSO procedure combined with cross-validation. 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 matching procedure that exploits the (partial) availability of scores in a national standardized test, which guarantees that students in treated and control groups had similar educational performance before the shooting.

6.2 Results

Table 8 presents estimates of equation (5). The analysis of schoolmates can be found in panel A and that of the neighbors in panel B. In all columns and panels we use as a non-parametric control of predetermined performance a set of fixed effects for the ventiles of GPA. This is, we always compare students who had a similar GPA in previous years. Columns 1-3 show that police violence is strongly associated with a lower performance in the short- and long-run. In particular, we observe a persistent decrease in GPA of affected students of around 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, 2020). Interestingly, the negative impact appears both in the analysis of the schoolmates and those who lived nearby, although estimates are more noisy in the latter group. Columns 4-6 show that the probability that the schoolmates dropped out of high-school increased by 3-4 pp., which is more than twice the size of the dropout probability in the control group. These estimates suggest a large negative impact of police violence that is persistent over time. Moreover, column 7 shows that students affected by police violence were also 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. The latter two estimates are statistically significant when using randomization inference (p-value of 0.03) but estimates in columns 1-6 have p-values that range between 0.10 and 0.36. In contrast, we observe a precisely estimated null effect on neighbor students in terms of dropout rates and the college exam. In sum, we find sizable negative effects of police violence among those who were in close social proximity to the student killed but little impacts on those who lived nearby.

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.[23] The closure was announced after the academic year ended in 2013 claiming a decrease in enrollment rates, and the school was (and remains) closed, despite the fact that the law mandates that closures have to be announced three months in advance. 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 mentioned, the main matching procedure we employ in the previous section selected a comparison group of students with a similar protest behavior before the shooting. 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

[23] 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).

important exercise because it guarantees that we are comparing students with similar educational performance before the shooting. Table A.8 presents results from this exercise and estimates are of similar statistical significance and of essentially the same economic magnitude.

Finally, we present two additional exercises that explore the impact on educational performance in more depth. The first exercise studies a potential heterogeneous effect on schoolmates in the same grade. Econometrically, 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, with panel A using the main matching procedure and panel B the extended matching that exploits test scores from the standardized test, as in Table A.8. 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. The second exercise estimates the impact of police violence by enrollment grades in 2011. If the impact of police violence vanishes over time we expect the effect to be larger for 12th grade students and smaller among students in 8th grade. However, Table A.10 presents somewhat persistent effects. We conclude that police repression is associated with a persistent negative impact on educational performance that is widespread across the schoolmates of the student killed.

7 Conclusion

Most states employ police officers to repress protesters and ensure public safety, but the consequences are largely unknown. This paper studied the impact of police repression during a large student movement in Chile and showed that police violence appears to be an ineffective way to deter protest behavior and has significant negative consequences. In particular, we documented that high school students who were in close social proximity to a student that was killed by a police gunshot experienced a small temporary decrease in their protest behavior and students living nearby the event remained protesting as they were before the shooting. Besides the limited impact of repression on protest behavior, we also document a collection of negative educational consequences for students who were exposed to police violence.

Taken together our findings cast doubt on the effectiveness of police repression towards protesters. The lack of a persistent effect on protest behavior is particularly notable when taking into account that we have studied arguably the most violent manifestation of state repression. In this sense, we conjecture that any other form of police violence is likely to have a smaller and more transitory impact on protest behavior, further limiting the effectiveness of repression as a policy of deterrence.

Similarly, we also expect other forms of police repression 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 during street rallies.

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 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 acts of police repression. As such, we hypothesize that the impact of police repression on the protest behavior of adults could be even smaller. Similarly, our focus on one salient act of police repression has the benefit of being precisely defined and allows an easier tracking of the subpopulation more exposed to it. But acts of police repression can be heterogeneous and have different impacts on protesters. The study of an extreme act of violence such as the death of a student at a protest allows us to perhaps interpret our findings as a bound on the impact of police repression.

Finally, we believe 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 which policy is the most effective to ensure public safety in the context of protests. Our findings suggest that said policy needs to maximize public safety while minimizing its negative spillovers. Although many rallies are authorized and exhibit little violence, confrontations between the police and protesters – or even between groups of protesters – have become more common particularly in countries experiencing more polarization, making this question of particular importance. Protest-related violence is also much more common in developing countries where the capacity of the state is more limited, making policies perhaps highly specific to the context. 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.

References

Acemoglu, D. and Robinson, J. (2000). *Economic origins of dictatorship and democracy*. Cambridge University Press.

Acemoglu, D. and Robinson, J. (2001). A theory of political transitions. *American Economic Review*, 91(4):938–63.

Aguirre, J. and Matta, J. J. (2019). Walking in your footsteps: Sibling spillovers in higher education choices. *Working Paper*.

Altonji, J. G., Bloom, E., and Meghir, C. (2012). Heterogeneity in human capital investments: High schools curriculum, college major, and careers. *Annual Review of Economics*, 4(1):185–223.

Ang, D. (2020). The effects of police violence on inner-city students. *Quarterly Journal of Economics*.

Atkinson, A. B. and Stiglitz, J. E. (2015). *Lectures on Public Economics*. Princeton University Press.

Aytac, S. E., Schiumerini, L., and Stokes, S. (2018). Why do people join backlash protests? Lessons from Turkey. *Journal of Conflict Resolution*, 62(6):1205–1228.

Azoulay, P., Graff Zivin, J. S., and Wang, J. (2010). Superstar extinction. *Quarterly Journal of Economics*, 125(2):549–589.

Bautista, M. A., González, F., Martínez, L., Muñoz, P., and Prem, M. (2020a). Dictatorship, higher education, and social mobility. *Working Paper*.

Bautista, M. A., González, F., Martínez, L., Muñoz, P., and Prem, M. (2020b). The geography of repression and opposition to autocracy. *American Journal of Political Science*.

Becker, G. (1968). Crime and punishment: An economic approach. *Journal of Political Economy*, 76(2):169–217.

Bertrand, M., Duflo, E., and Mullainathan, S. (2004). How much should we trust differences-in-differences estimates? *Quarterly Journal of Economics*, 119(1):249–275.

Besley, T. and Persson, T. (2011). The logic of political violence. *Quarterly Journal of Economics*, 126(3):1411–45.

Bonnefoy, P. (2012). Volunteers keep watch on protests in Chile. *New York Times*.

Bursztyn, L., Cantoni, D., Yang, D. Y., Yuchtman, N., and Zhang, Y. J. (2020). Persistent political engagement: Social interactions and the dynamics of protest movements. *American Economic Review: Insights*.

Cantoni, D., Yang, D. Y., Yuchtman, N., and Zhang, Y. J. (2019). Protests as strategic games: Experimental evidence from Hong Kong's democracy movement. *Quarterly Journal of Economics*, 134(2):1021–1077.

Carrell, S. E. and Hoekstra, M. L. (2010). Externalities in the classroom: How children exposed to domestic violence affect everyone's kids. *American Economic Journal: Applied Economics*, 2(1):211–228.

CLACSO (2012). *Cronología del conflicto social. Chile 2012*. Consejo Latinoamericano de Ciencias Sociales.

Codepu (2012). Informe sobre violencia policial contra estudiantes chilenos, realizado por la Corporación Codepu. *Corporación de Promoción y Defense de los Derechos del Pueblo*.

Conley, T. (1999). GMM estimation with cross sectional dependence. *Journal of Econometrics*, 91(1):1–45.

Cooperativa (2013). Estudiantes secundarios llamaron a no rendir la prueba Simce. *Cooperativa*.

Cuesta, J. I., González, F., and Larroulet, C. (2020). Distorted quality signals in school markets. *Journal of Development Economics*, 147:102532.

Curtice, T. B. and Behlendorf, B. (2020). Street-level repression: Protest, policing, and dissent in Uganda. *Journal of Conflict Resolution*.

Davenport, C. (2007). State repression and political order. *Annual Review of Political Science*, 10:1–23.

Dell, M. (2015). Trafficking networks and the Mexican drug war. *American Economic Review*, 105(6):1738–1779.

Dell, M. and Querubin, P. (2018). Nation building through foreign intervention: Evidence from discontinuities in military strategies. *Quarterly Journal of Economics*, 133(2):701–764.

DiPasquale, D. and Glaeser, E. L. (1998). The Los Angeles riot and the economics of urban unrest. *Journal of Urban Economics*, 43(1):52–78.

Enikolopov, R., Makarin, A., and Petrova, M. (2020). Social media and protest participation: Evidence from Russia. *Econometrica*.

Ferman, B. (2019). A simple way to assess inference methods. *Working Paper*.

Ferman, B. and Pinto, C. (2019). Inference in differences-in-differences with few treated groups and heteroskedasticity. *Review of Economic and Statistics*, 101(3):452–467.

Figlio, D. and Loeb, S. (2011). School accountability. In Hanushek, E. A., Machin, S., and Woessmann, L., editors, *Handbook of the Economics of Education*, volume 3, pages 383–421.

Figlio, D. N. and Getzler, L. S. (2002). Accountability , Ability and Disability: Gaming the System. Working Paper 9307, National Bureau of Economic Research.

Figueroa, F. (2012). *Llegamos para quedarnos: crónicas de la revuelta estudiantil*. LOM ediciones.

Fisher, D. R., Andrews, K. T., Caren, N., Chenoweth, E., Heaney, M. T., Leung, T., Perkins, N., and Pressman, J. (2019). The science of contemporary street protest: New efforts in the United States. *Science Advances*, 5(10):EAAW5461.

Fisher, R. A. (1935). *The Design of Experiments*. Edinburgh: Oliver and Boyd Ltd.

Fryer, R. G. (2020). An empirical analysis of racial differences in police use of force. *Journal of Political Economy*.

Fujita, K., Henderson, M. D., Eng, J., Trope, Y., and Liberman, N. (2006). Spatial distance and mental construal of social events. *Psychological Science*, 17(4):278–282.

García-Ponce, O. and Pasquale, B. (2015). How political repression shapes attitudes toward the state: Evidence from Zimbabwe. *Working Paper*.

González, F. (2020). Collective action in networks: Evidence from the Chilean student movement. *Journal of Public Economics*, 188.

Grau, N., Hojman, D., and Mizala, A. (2018). School closure and educational attainment: Evidence from a market-based system. *Economics of Education Review*, 65(1-17).

Hagopian, J., Kohn, A., and Ravitch, D. (2014). *More Than a Score: The New Uprising Against High-Stakes Testing*. Haymarket Books.

Iacus, S. M., King, G., and Porro, G. (2012). Causal inference without balance checking: Coarsened exact matching. *Political Analysis*, 20(1):1–24.

Jackson, G. (2013). *El país que soñamos*. Debate.

Jacobs, D. (1998). The determinants of deadly force: A structural analysis of police violence. *American Journal of Sociology*, 103(4):837–862.

Kane, T. J. and Staiger, D. O. (2002). The Promise and Pitfalls of Using Imprecise School Accountability Measures. *The Journal of Economic Perspectives*, 16(4):pp. 91–114.

Klor, E. F., Saiegh, S., and Satyanath, S. (2020). Cronyism in state violence: Evidence from labor repression during Argentina's last dictatorship. *Journal of the European Economic Association*.

La Segunda (2011). Cabo Millacura en peritaje de baleo a menor: "no hubo intención, pido disculpas a la familia". *La Segunda*.

Lawrence, A. K. (2017). Repression and activism among the Arab Spring's first movers: Evidence from Morocco's february 20th movement. *British Journal of Political Science*, 47(3):699–718.

Lichbach, M. I. (1987). Deterrence or escalation? The puzzle of aggregate studies of repression and dissent. *Journal of Conflict Resolution*, 31(2):266–297.

Lohmann, S. (1994). The dynamics of informational cascades: The monday demonstrations in Leipzig, East Germany, 1989-91. *World Politics*, 47(1):42–101.

Manacorda, M. and Tesei, A. (2020). Liberation technology: Mobile phones and political mobilization in Africa. *Econometrica*, 88(2):533–567.

McAdam, D. (1995). "Initiator" and "spin-off" movements: Diffusion processes in protest cycles. In Traugott, M., editor, *Repertoires and Cycles of Collective Action*, pages 217–40. Durham, NC: Duke University Press.

Miguel, E. and Kremer, M. (2003). Worms: Identifying impacts on education and health in the presence of treatment externalities. *Econometrica*, 72(1):159–217.

Monteiro, J. and Rocha, R. (2017). Drug battles and school achievement: Evidence from Rio de Janeiro's favelas. *Review of Economics and Statistics*, 99(2):213–228.

Moore, W. H. (1998). repression and dissent: substitution, context, and timing. *American Journal of Political Science*, 42(3):851–873.

Neal, D. (2013). The Consequences of Using one Assessment System to Pursue two Objectives. *Journal of Economic Education*, 44(4):pp. 339–352.

Opp, K.-D. and Roehl, W. (1990). Repression, micromobilization, and political protest. *Social Forces*, 69(2):521–547.

Pierskalla, J. (2010). Protest, deterrence, and escalation: The strategic calculus of government repression. *Journal of Conflict Resolution*, 54(1):117–45.

Rossin-Slater, M., Schnell, M., Schwandt, H., Trejo, S., and Uniat, L. (2020). Local exposure to school shootings and youth antidepressant use. *Proceedings of the National Academy of Sciences*.

Rozenas, A. and Zhukov, Y. M. (2019). Mass repression and political loyalty: Evidence from Stalin's 'terror by hunger'. *American Political Science Review*, 113(2):569–583.

Sarsons, H. (2019). Interpreting signals in the labor market. *Working Paper*.

Shadmehr, M. and Boleslavsky, R. (2020). International pressure, state repression and the spread of protest. *Journal of Politics*.

Sharkey, P. (2010). The acute effect of local homicides on children's cognitive performance. *Proceedings of the National Academy of Sciences*, 107(26):11733–11738.

Simonsen, E. (2012). *Mala educación: historia de la revolución escolar*. Debate.

Tamayo, T. (2015). *Todos Somos Manuel Gutiérrez: Vida y Muerte de un Mártir de la Democracia*. Santiago, Chile: Ediciones B.

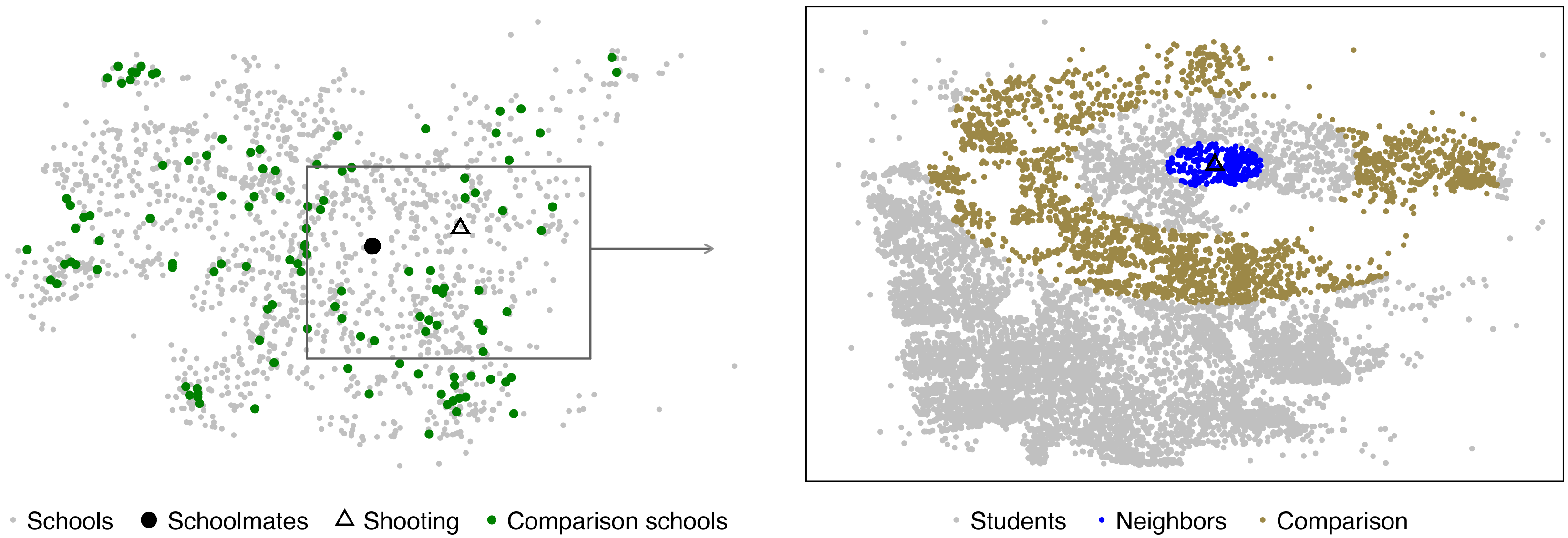
Villarubia, G. (2011). General Gordon hizo cambiar parte policial para proteger a su hijo que chocó y se dio a la fuga. *CIPER*.

Williamson, V., Trump, K.-S., and Levine Einstein, K. (2018). Black lives matter: Evidence that police-caused deaths predict protest activity. *Perspectives on Politics*, 16(2):400–415.

Young, A. (2019a). Channelling Fisher: Randomization tests and the statistical insignificance of seemingly significant experimental results. *Quarterly Journal of Economics*, 134(2):557–598.

Young, L. E. (2019b). The psychology of state repression: Fear and dissent decisions in Zimbabwe. *American Political Science Review*, 113(1):140–155.

Figure 1: Schools and students in the analysis

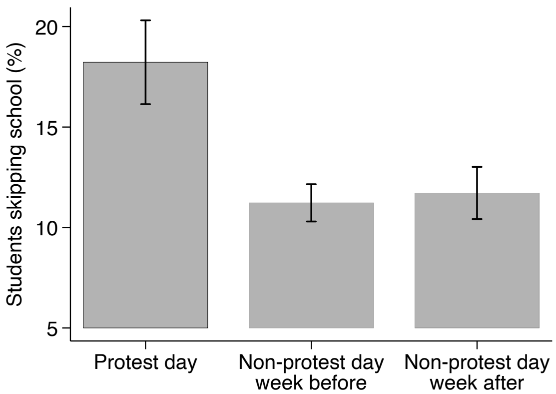


(a) Schools

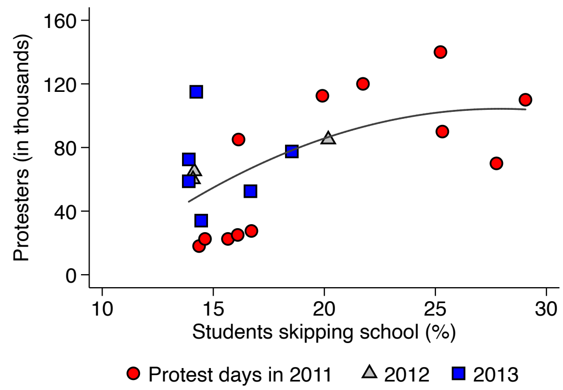
(b) Neighbor students and comparison group of students

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 mark the selected area (as a black hollow square) to study spatial spillovers. Panel (b) shows the location of students in the sample, highlighting the ones who following Ang (2020) were geographically exposed to the shooting (in blue) and the comparison group of students (in brown). More details in section 3.1.

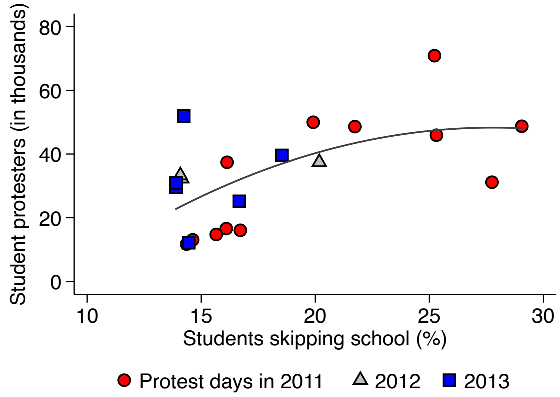
Figure 2: School skipping and protesters



(a) School skipping and protest days



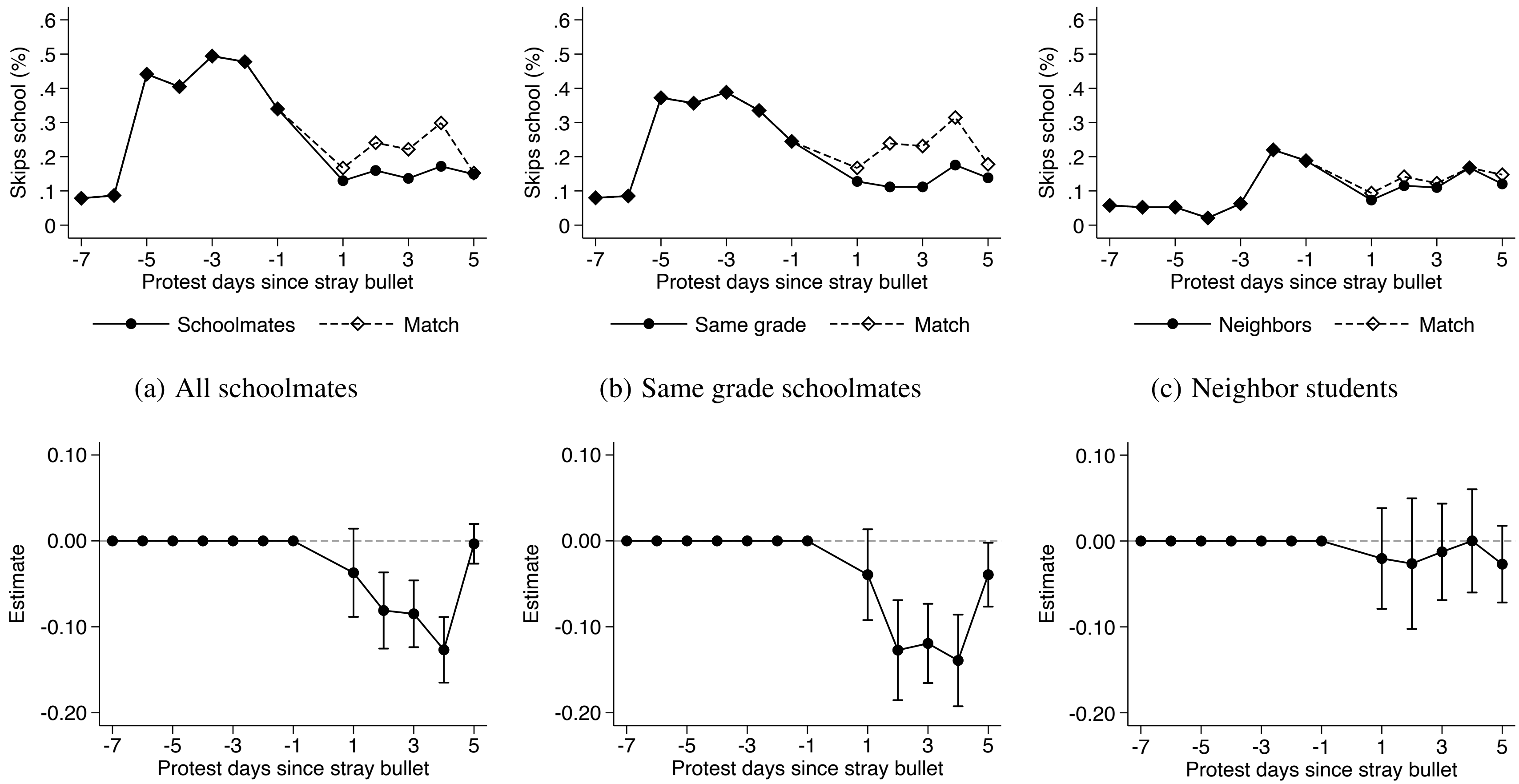
(b) School skipping and protesters



(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. More details in section 3.2.

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

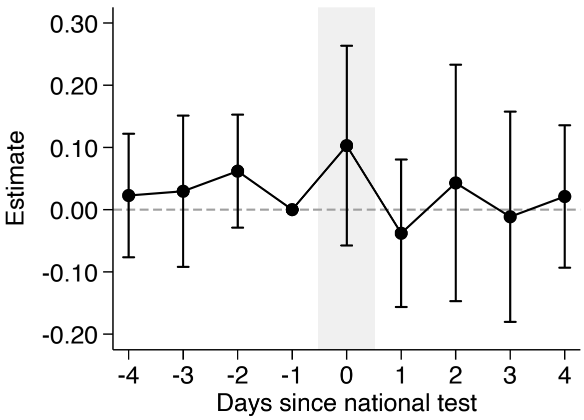


(a) All schoolmates (b) Same grade schoolmates (c) Neighbor students

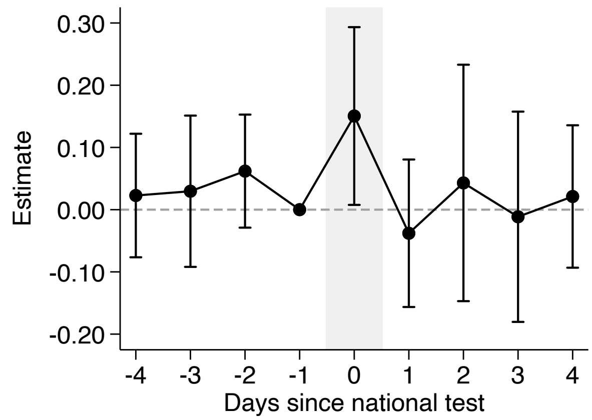
(d) All schoolmates (e) Same grade schoolmates (f) Neighbor students

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. 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. 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. More details in section 5.1.

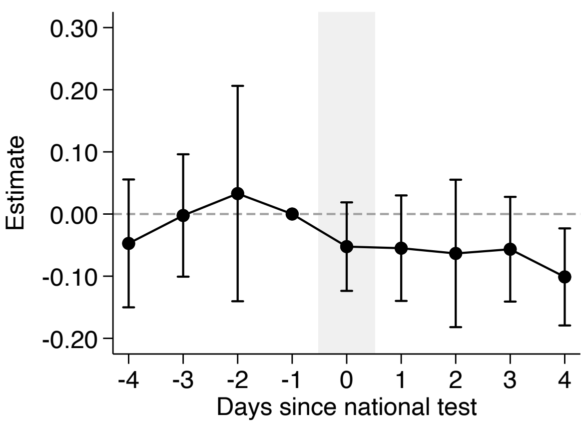
Figure 4: Student-led boycott



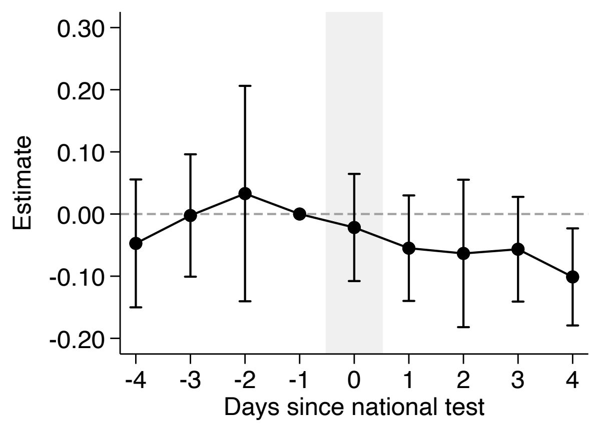
(a) Skips schools, schoolmates



(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 exposed to police violence when compared to a 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 omitted category is the day before test day. More details in section 5.3.

Table 1: Protest days in weekdays, 2011-2013

			Estimated number of protesters in the rally			
Year	Month	Day	By police	By organizers	High-school students	Day of week
(1)	(2)	(3)	(4)	(5)	(6)	(7)
2011	May	12	15,000	30,000	65%	Thursday
	June	1	20,000	35,000	58%	Wednesday
		16	80,000	100,000	51%	Thursday
		23	25,000	25,000	66%	Thursday
		30	80,000	200,000	51%	Thursday
	August	9	70,000	150,000	44%	Tuesday
		18	40,000	100,000	44%	Thursday
	September	14	6,000	30,000	65%	Wednesday
		22	60,000	180,000	41%	Thursday
		29	20,000	150,000	44%	Thursday
	October	19	25,000	200,000	44%	Wednesday
	November	18	5,000	40,000	58%	Friday
2012	April	25	50,000	80,000	50%	Wednesday
	May	16	20,000	100,000	55%	Wednesday
		28	40,000	150,000	44%	Thursday
2013	April	11	80,000	150,000	45%	Thursday
	May	8	37,500	80,000	50%	Wednesday
	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. 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. More details in section 3.

Table 2: Summary statistics

	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 in parentheses. The variables in italics are used as inputs for the coarsened exact matching algorithm. 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 for the comparison group. More details in section 3.1.

Table 3: The impact of police violence on protest behavior

The dependent variable is an indicator for school skipping in a weekday protest				
	All schoolmates		Same grade	
Panel A	(1)	(2)	(3)	(4)
Schoolmate × After student killed by police	-0.07	-0.07	-0.09	-0.09
	(0.02)	(0.01)	(0.03)	(0.02)
	[0.10]	[0.10]	[0.16]	[0.16]
Observations	270,588	270,588	60,300	60,300
Students in sample	22,549	22,549	5,025	5,025
Average dependent variable	0.33	0.33	0.27	0.27
Student fixed effects	Yes	Yes	Yes	Yes
Day fixed effects	Yes	No	Yes	No
Cell-day fixed effects	No	Yes	No	Yes
	Neighbor students (< 0.5 miles) compared to students who live…			
Panel B	[0.5-3] miles away		[1.5-3] miles away	
Neighbor × After student killed by police	-0.01	-0.01	-0.02	-0.02
	(0.03)	(0.02)	(0.03)	(0.02)
	[0.04]	[0.04]	[0.05]	[0.05]
Observations	9,084	9,084	7,728	7,728
Students in sample	2,212	2,212	1,509	1,509
Average dependent variable	0.10	0.10	0.09	0.09
Student fixed effects	Yes	Yes	Yes	Yes
Day fixed effects	Yes	No	Yes	No
Cell-day fixed effects	No	Yes	No	Yes

Notes: Each observation corresponds to a skipping school decision of a high-school student in a protest that took place on a weekday. We observe twelve protest days in 2011. Panel A presents estimates of the differential protest behavior of schoolmates of the student killed relative to a selected comparison group of students, i.e. social effects. Panel B repeats the same strategy but studies students who lived nearby the shooting, i.e. spatial effects. In both panels we select the control group of students using a coarsened exact matching procedure. Estimates of linear probability models with day fixed effects in columns 1-3 and cell-day fixed effects in columns 2-4. Standard errors are clustered at the school level in parentheses (panels A and B), *p*-values from randomization inference in square brackets in panel A and spatially correlated errors following Conley (1999) in square brackets panel B. More details in section 5.1.

Table 4: Robustness of results to other spillover structures

	Excludes students enrolled in			
	schools nearby the shooting			
	[0-3] miles	[0-4] miles	[0-5] miles	connected schools
Panel A: All schoolmates	(1)	(2)	(3)	(4)
Schoolmate × After student killed	-0.07	-0.06	-0.06	-0.07
	(0.01)	(0.01)	(0.01)	(0.01)
	[0.10]	[0.14]	[0.14]	[0.10]
Observations	256,104	243,828	214,692	235,368
Students	21,342	20,319	17,891	19,614
Student fixed effects	Yes	Yes	Yes	Yes
Cell-day fixed effects	Yes	Yes	Yes	Yes
Panel B: Same grade				
Schoolmate × After student killed	-0.10	-0.08	-0.08	-0.10
	(0.02)	(0.01)	(0.02)	(0.02)
	[0.12]	[0.19]	[0.20]	[0.13]
Observations	57,108	54,060	48,156	52,344
Students	4,759	4,505	4,013	4,362
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 a protest that took place on a weekday. We observe twelve protest days in 2011. Estimates of the differential protest behavior of schoolmates of the student killed relative to a selected comparison group of students selected using a coarsened exact matching procedure. Columns 1-3 drop all students in the control group who were enrolled in a school located nearby the shooting. Column 4 drops all students in the control group who were enrolled in schools attended by schoolmates before 2011. Standard errors are clustered at the school level in parentheses and p-values from randomization inference in square brackets. More details in section 5.1.

Table 5: School skipping decisions in protest and non-protest days

	Dependent variable: Indicator for school skipping			
			One non-protest day	
	All non-protest days		Week *before*	Week *after*
Panel A	(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 × Non-protest day	0.00	0.00	0.00	0.00
	(0.00)	(0.00)	(0.01)	(0.01)
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 × Non-protest day	0.00	0.00	-0.01	-0.01
	(0.00)	(0.00)	(0.01)	(0.01)
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 of 2011 and additional non-protest days. Estimation using different specifications of linear probability models. Panel A uses *all* non-protest days in the 2011 academic 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. More details in section 5.1.

Table 6: 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 student killed	-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 2011	0.04	0.07	0.04	0.07
	(0.01)	(0.01)	(0.01)	(0.01)
	[0.30]	[0.17]	[0.30]	[0.17]
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
Panel B				
Neighbor × After student killed	-0.02	-0.02	-0.02	-0.02
	(0.02)	(0.02)	(0.02)	(0.02)
Neighbor × After 2011	-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

Notes: Each observation corresponds to a skipping school decision of a high-school student in a protest that took place on a weekday. We observe twelve protest days in 2011, three in 2012, and six 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. More details in section 5.2.

Table 7: 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. More details in section 5.2.

Table 8: 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. More details in section 6.2.

ONLINE APPENDIX

Police repression and protest behavior: Evidence from student protests in Chile

Felipe González and Mounu Prem

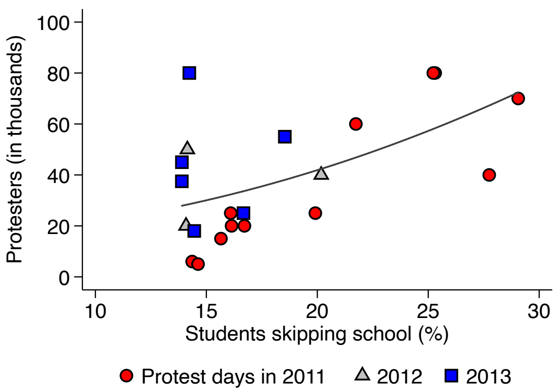
List of Figures

List of Tables

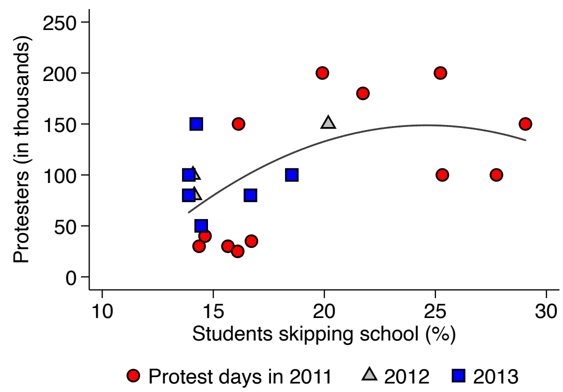
Figure A.1: Context, protest days

Notes: Section 2 provides more details.

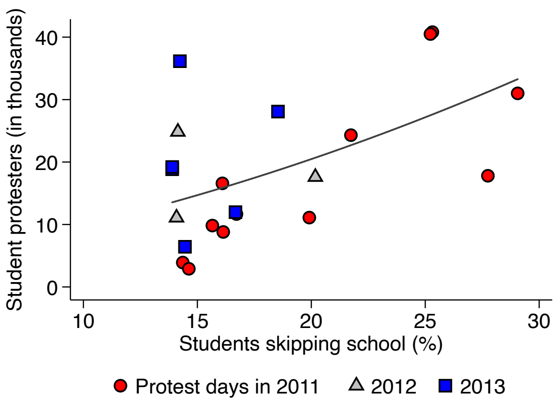
Figure A.2: Robustness, school skipping and protesters



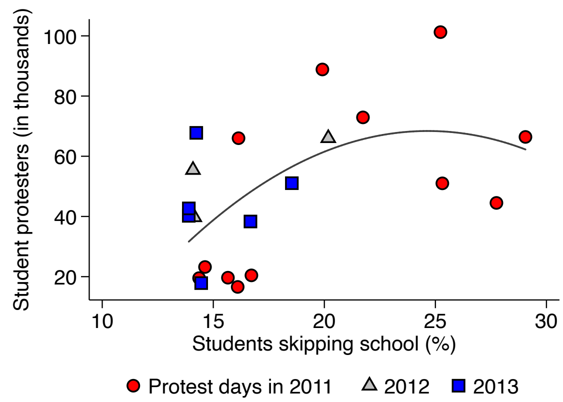
(a) Police reports: Protesters



(b) Organizer: Protesters



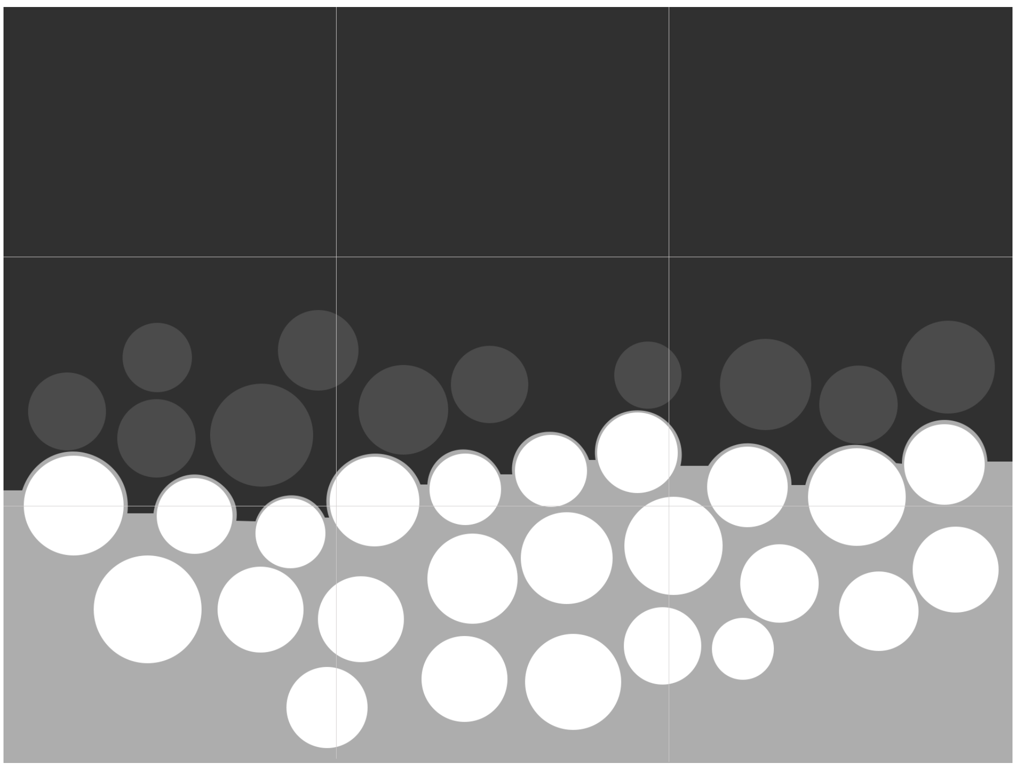
(c) Police reports: Student protesters



(d) Organizer: Student protesters

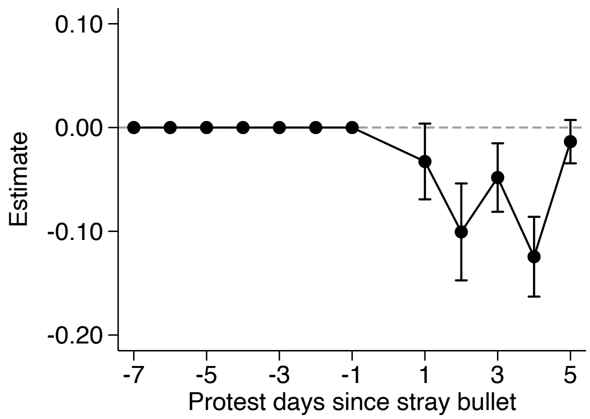
Notes: Own construction using data from police and organizer reports. These figures present the partial correlation between the percentage of high-school students skipping school and the total number of protesters (Panels A and B), and the partial correlation with student protesters (Panels C and D). The number of student protesters was calculated using online surveys and videos of rallies. More details in section 3.2.

Figure A.3: Crowd counting high-school students

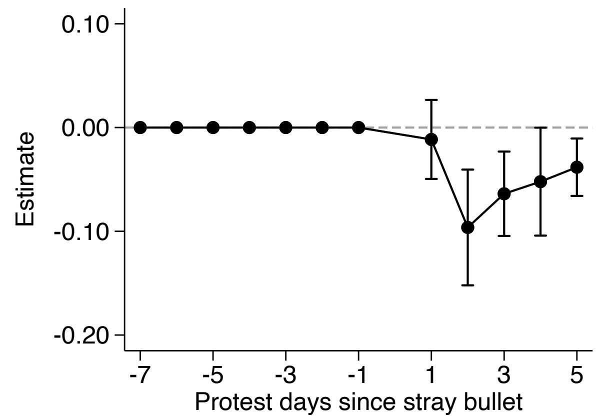


Notes: This figure presents the sketch of an image, where a crowd is identifiable in the front, and a non-identifiable crowd is located in the back. The classification of the image into identifiable and non-identifiable areas was done by a research assistant who was unaware of the goal of this exercise. We asked 450 college students to count the number of high-school students in the front of the image.

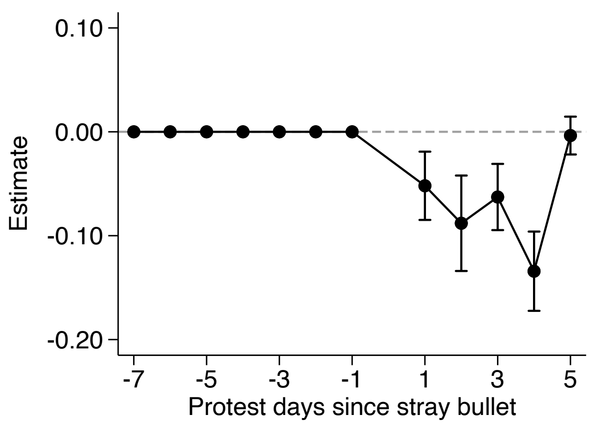
Figure A.4: Robustness, alternative matching strategies



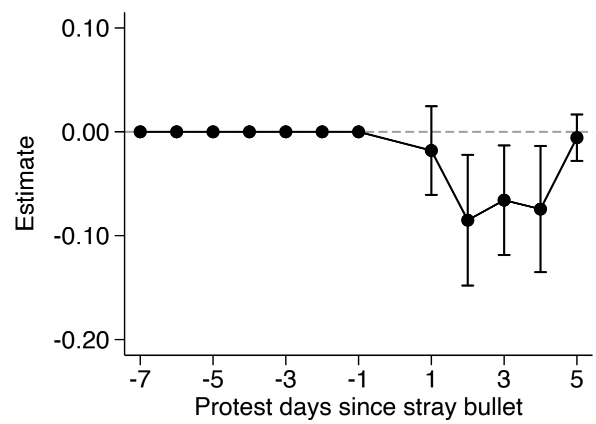
(a) All schoolmates, matching #2



(b) Same grade, matching #2



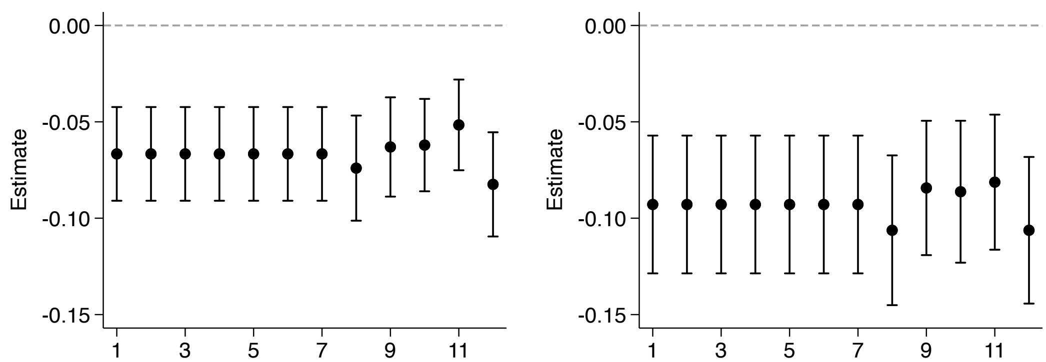
(c) All schoolmates, matching #3



(d) Same grade, matching #3

Notes: Estimates of equation (1) using daily school attendance data from the 2011 and 2012 academic years. The *y*-axis measures the differential change in school skipping rates among classmates of the student killed when compared to a sample of students that were observationally identical before the event. Standard errors are clustered at the school level. More details in section 5.1.

Figure A.5: Robustness, omits 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: Descriptive evidence, differences by availability of 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: Additional results, school skipping, and 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	4.31***	5.55***	0.07***	0.10***	0.06***	0.08***
	(1.38)	(1.48)	(0.02)	(0.02)	(0.02)	(0.02)
R-squared	0.33	0.42	0.28	0.50	0.30	0.44
Average dependent variable	69.68	69.68	4.08	4.08	3.38	3.38
Panel B - Police reports						
Percentage of students skipping school	2.82***	3.99***	0.08***	0.13***	0.08***	0.11***
	(0.96)	(0.88)	(0.03)	(0.03)	(0.03)	(0.03)
R-squared	0.32	0.50	0.28	0.58	0.26	0.48
Average dependent variable	38.88	38.88	3.42	3.42	2.72	2.72
Panel C - Organizers						
Percentage of students skipping school	5.88**	7.33***	0.07***	0.10***	0.06***	0.08***
	(2.08)	(2.40)	(0.02)	(0.03)	(0.02)	(0.02)
Observations	21	21	21	21	21	21
R-squared	0.26	0.32	0.23	0.42	0.24	0.38
Year fixed effects	No	Yes	No	Yes	No	Yes
Average dependent variable	101.4	101.4	4.44	4.44	3.73	3.73

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. More details to calculate the number of student protesters in columns 5-6 can be found in section 3.2.

Table A.3: Additional results, estimates from 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)
Student exposed × day 1	-0.04	-0.04	-0.03	-0.02
	(0.03)	(0.03)	(0.02)	(0.02)
	[0.31]	[0.41]		
Student exposed × day 2	-0.08	-0.13	-0.00	-0.01
	(0.02)	(0.03)	(0.03)	(0.03)
	[0.28]	[0.28]		
Student exposed × day 3	-0.08	-0.12	-0.02	-0.02
	(0.02)	(0.02)	(0.03)	(0.03)
	[0.15]	[0.15]		
Student exposed × day 4	-0.13	-0.14	-0.02	-0.05
	(0.02)	(0.03)	(0.03)	(0.03)
	[0.09]	[0.19]		
Student exposed × day 5	-0.00	-0.04	-0.04	-0.04
	(0.01)	(0.02)	(0.02)	(0.02)
	[0.61]	[0.41]		
Observations	270,588	60,300	26,544	18,108
Student fixed effects	Yes	Yes	Yes	Yes
Cell-day fixed effects	Yes	Yes	Yes	Yes
Students	22,549	5,025	2,212	1,509
Avg. dependent variable	0.33	0.27	0.12	0.11

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. Standard errors are clustered at the school level and p-values from randomization inference in square brackets.

Table A.4: Additional results, main estimates by grade

	Dependent variable: Indicator school skipping in weekday protest				
Grade in 2011:	8th	9th	10th	11th	12th
	(1)	(2)	(3)	(4)	(5)
Schoolmate × After student killed	-0.06	-0.05	-0.06	-0.09	-0.06
	(0.01)	(0.01)	(0.02)	(0.02)	(0.02)
Observations	51,468	56,400	54,960	60,300	47,460
Students	4,289	4,700	4,580	5,025	3,955
Student fixed effects	Yes	Yes	Yes	Yes	Yes
Cell-day fixed effects	Yes	Yes	Yes	Yes	Yes
Average dependent variable	0.192	0.338	0.385	0.266	0.359

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. Standard errors are clustered at the school level. More details in section 5.1.

Table A.5: Robustness, main results and 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	0.08	0.04	0.08
	(0.01)	(0.01)	(0.01)	(0.01)
Observations	227,226	274,044	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. Standard errors are clustered at the school level and p-values from randomization inference in square brackets. More details in section 5.1.

Table A.6: Additional results, distance to other locations

Dependent variable: Indicator school skipping in weekday protest				
	Students who lived nearby home/school of student killed			
	home		school	
	(1)	(2)	(3)	(4)
Schoolmate × After non-lethal police repression	-0.03	-0.03	0.05	0.05
	(0.03)	(0.02)	(0.04)	(0.03)
Observations	8,052	8,052	7,500	7,500
Students	671	671	625	625
Student fixed effects	Yes	Yes	Yes	Yes
Day fixed effects	Yes	No	Yes	No
Cell-day fixed effects	No	Yes	No	Yes
Average dependent variable	0.10	0.10	0.15	0.15

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. Standard errors are clustered at the school level. More details in section 5.1.

Table A.7: Additional results, non-lethal police repression

Dependent variable: Indicator school skippping in weekday protest		
	(1)	(2)
Schoolmate × After non-lethal police repression	0.06	0.05
	(0.04)	(0.05)
Observations	222,334	222,190
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. Standard errors are clustered at the school level. More details in section 5.1.

Table A.8: Robustness, educational performance

	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. Standard errors are clustered at the school level. More details in section 5.2.

Table A.9: Additional results, educational performance of same grade schoolmates

	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. More details in section 5.2.

Table A.10: Additional results, college exam

	Dependent variable: Indicator for taking the college exam before 2018				
Grade in 2011:	12th	11th	10th	9th	8th
	(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. Standard errors are clustered at the school level. More details in section 5.2.