When a Strike Strikes Twice:
Massive Student Mobilizations and Teenage Pregnancy in Chile

Pablo A. Celhay*
Emilio Depetris-Chauvin‡
Cristina Riquelme §

February 22, 2024

Abstract

This paper empirically studies the impact of massive and sudden school closures following the 2011 nationwide student strike in Chile on teenage pregnancy. We observe an average increase of 2.7% in teenage pregnancies in response to temporary high school shutdowns, equal to 1.9 additional pregnancies per lost school day. The effect diminishes after three quarters since the strike's onset. The effects are predominantly driven by first-time mothers aligned with high-school absenteeism periods and are unrelated to the typical seasonality of teenage fertility or pregnancies in other age groups. Additionally, we document that the strike had a larger disruptive role by affecting students' educational trajectories, evidenced by a persistent increase in dropout rates and a reduction in college admission test take-up for both female and male students.

Keywords: Teenage Pregnancy, Risky Behavior, Student Protests, Human Capital
JEL: J13, I12, I2, D17

The authors thank Alejandro Corvalán, Marcel Fafchamps, Francisco Gallego, Felipe González, Seema Jayachandran, Melissa Kearney, Jeanne Lafortune, as well as participants in seminars at Cornell University VERB Seminar, the 3^{rd} IZA Workshop on Gender and Family Economics, the Development Workshop and Applied Microeconomics Workshop at Pontificia Universidad Católica de Chile, Economics Workshop at the Department of Industries of the University of Chile and Department of Economics at Universidad Diego Portales, for helpful comments. We thank Damian Clarke and Nicolás Grau for sharing their data on emergency contraception pill disbursements and school occupation status, respectively. Matías Muñoz and Ela Díaz provided excellent research assistance. This article used the SIMCE databases of the Chilean Ministry of Education as a source of information. The authors thank the Ministry of Education for access to the information. All the study results are the author's responsibility and do not compromise said institution in any way. We also thank the Department of Evaluation, Measurement, and Educational Registration (DEMRE) of the University of Chile for providing the Higher Education Admission System databases to develop this article. Usual disclaimers apply.

*Escuela de Gobierno and Instituto de Economía, Pontificia Universidad Católica de Chile. Email: pacelhay@uc.cl

‡Instituto de Economía, Pontificia Universidad Católica de Chile. Email: edepetris@uc.cl

§Department of Economics, University of Maryland, College Park. Email: riquelme@umd.edu

1 Introduction

Teen pregnancy sets young mothers and their children on life trajectories that are usually associated with worse education, health and labor market outcomes (e.g., Chevalier and Viitanen, 2003, Diaz and Fiel, 2016, Fletcher and Wolfe, 2009, Fletcher, 2012, Francesconi, 2008, Levine and Painter, 2003, Marcotte, 2013, Bailey, 2013 Kearney and Levine, 2015). Across countries, teen pregnancy rates are much more concentrated among low-income teenagers, deepening inequality and stagnating social mobility (Azevedo et al., 2012; Kearney and Levine, 2012; Kearney and Levine, 2014). The question of which strategies effectively reduce motherhood in adolescents remains open, but schools are often recognized to play a significant role. Therefore, it is crucial to understand the mechanisms by which schools influence teenage pregnancy rates (Black et al., 2008). In this paper, we empirically study the change in teen pregnancy in the context of a sudden and unexpected closure of schools following the 2011 nationwide student strike in Chile.

The lessons derived from our study's context hold significance for middle-income and developing economies. Within the OECD countries, Chile is fourth in terms of the highest teen fertility rate, after Costa Rica, Mexico, and Colombia (OECD, 2022). Notably, while overall fertility rates have consistently decreased over the past decades, the decline in teenage fertility rates has been more stagnant. From 1960 to 2014, the number of births per 1,000 women aged 20-24 has decreased by 62%. In contrast, teenage births experienced a 39% decrease during the same period. Similar patterns can be observed in Mexico and Colombia. Conversely, most developed countries have witnessed more significant declines in fertility rates in all age groups (OECD, 2022).

Schools impose time constraints on students, reducing the free time available for students to engage in risky behavior;[1] and also educate young people about the costs associated

[1]Teens spending much time unsupervised by adults, particularly in school holidays where others have documented a rise in teenage conceptions (e.g., Buckles and Hungerman, 2013).

with risky actions. Students are also influenced by classmates or other types of peers (e.g., Evans et al. (1992); Gaviria and Raphael (2001)) within schools, neighborhoods, or other locations. These mechanisms, and others, may explain the empirical results on the effects of schools in reducing crime activity (e.g., Jacob and Lefgren, 2003 and Anderson, 2014), drug abuse (Griffin et al., 2004), pregnancy (Black et al., 2008), and sexually transmitted diseases (Alsan and Cutler, 2013) among teenagers. Although quantifying the impact of the previously mentioned mechanisms is relevant to informing policymakers, it is not easy to disentangle one from another. In our setting, we fill this gap by being able to rule out human capital accumulation as a mechanism.

Our empirical analysis exploits the variation of high-frequency panel data in conceptions by women of school age and the intensity of school closures in Chilean municipalities during the student strike. To do so, we first constructed time-invariant municipality-level exposure measures to the 2011 student strike by combining administrative, survey, and web-scraped data. Specifically, we identify schools on strike and then compute, at the municipality (of residence) level, the proportion of female students aged 15-17 enrolled in those schools. We then interact the time-invariant municipality-level strike exposure variable with time variation in the student national movement's extensive margin to exploit panel data variation in our strike intensity treatment.

When comparing monthly conception rates across municipalities with different strike exposure rates, we find that in municipalities where 26% (country average) of its female high school population were enrolled in a school on strike, teenage pregnancy increased by 2.7%. This corresponds in absolute numbers to 309 additional pregnancies once we consider total teen conceptions in the pre-strike year in the same months as a reference. Taking into account the average number of days lost during this nationwide student strike, teen conceptions at the national level increased by 1.9 per lost school day. This magnitude is similar to that of Berthelon and Kruger (2011), which finds that teenage conceptions in Chile decrease by 1.1

per day of additional school, followed by a reform that changes the school schedule from half to full days.

While we cannot pinpoint a specific mechanism, our evidence suggests that increased teen pregnancies may correlate with reduced adult supervision and teenagers engaging in riskier behavior during the student strike. The main results are driven by first-time mothers and couples in which the mother and the father are between 15 and 17 years old. We also estimate a placebo effect of strike adherence on the number of conceptions of women 18-19 years of age who are likely out of school, finding a null effect. If strikes were associated with specific sexual behavior among young individuals, irrespective of their school attendance, and if these strikes were confounded with our variables, we might observe a positive effect on conceptions among women who have recently left school due to their age. We also find no correlation with other age groups, suggesting that our main estimate does not simply capture a general trend in pregnancies.

Furthermore, our analysis demonstrates that the timing of the effect aligns with higher school absenteeism periods that coincide with increased conceptions in municipalities with greater exposure to strikes. Furthermore, when examining seasonal patterns, we find that the magnitude of the results is comparable to the changes in conceptions observed during December, which marks the beginning of the summer holidays in Chile when high school teenagers are more likely to spend additional unsupervised time. In addition, we do not find any effect on birth outcomes; that is, additional pregnancies of adolescents during this period do not present differences in birth outcomes compared to common pregnancies for this particular group.

We also explore the effects of strikes on different outcomes related to students' educational paths. We find that schools that eventually experienced strikes are similar to non-striking schools regarding dropout rates and college admission test take-up. However, when strikes occurred, we observed a significant increase in dropout rates and a decrease in the take-up of college admission tests. Moreover, dropout rates and college admission test take-up took approximately two to three years to return to their pre-strike levels. This gradual recovery suggests that the disruption caused by the strikes had a lasting effect on the student's educational trajectories. The impact on dropout rates is similar among female and male students, indicating that dropouts are not solely attributable to teenage pregnancy. This also suggests that the strike had a more prominent disruptive role, affecting students' decisions in other dimensions beyond sexual behavior, perhaps due to a blurring of norms and changes in expectations regarding the benefits of completing secondary education.

There are several reasons to consider that the 2011 student mobilizations and strikes constitute a significant source of quasi-experimental variation to identify and quantify the causal effect of schools becoming suddenly inoperative on teenage pregnancy. First, the six-month-long nationwide movement provides sufficient statistical power to detect even minor impacts on the number of pregnancies. Second, there is substantial spatial and time variation in sudden school closures in Chilean municipalities. Third, the sudden student strikes were arguably unexpected by parents, thus not allowing them to respond appropriately to mitigate the potential impact of massive school closures. Fourth, while the strike adherence of a school (in a given municipality) depended on students' decision (albeit strongly affected by a nationwide movement), the degree of the strike intensity (i.e., the cross-sectional component of our treatment) in a given municipality depended on pre-treatment enrollment decisions for female students. Importantly, these enrollment decisions were made by parents years before the student strikes and, in many cases, involved schools outside their municipality of residence. Indeed, a substantial variation in treatment for a given municipality depends on strike actions taken in schools outside that municipality.

Although we provide evidence to support the plausibility of a critical identification assumption (i.e., the "parallel-trend" assumption), we also present additional tests to support a causal interpretation of our findings. We run multiple event-study analyses that show no differential pre-trends in teenage pregnancies across municipalities with different strike adherence nor significant pre-trends on different covariates that are likely to be related to teenage pregnancies.

In addition, recent studies show that treatment effect heterogeneity can complicate the interpretation of the parameters estimated when using a difference-in-differences approach with continuous treatment (e.g., Callaway et al., 2021). To mitigate concerns, we also model below our treatment as nineteen different binary indicators where each equals one if a municipality is above the $5th$, $10th, \ldots$, $95th$ percentile of the distribution of strike adherence. All results remain similar.

Our results contribute to the literature and policy dialogue on how schools (and the government, in general) can prevent unwanted teenage pregnancies, which can have long-term implications for GDP growth, education, and labor supply. First, we contribute to the broad literature on teenage pregnancy (Kearney and Levine, 2012; Kearney and Levine, 2015) and fertility at an early age by looking at the role that schools play (Ní Bhrolcháin and Beaujouan, 2012). Previous research has focused on the implementation of either compulsory schooling laws or reforms that permanently extended the length of the school day (e.g., Berthelon and Kruger, 2011; Black et al., 2008; McCrary and Royer, 2011). These interventions pose a challenge for the simple reason that these policies may substantially affect human capital mechanically.[2] Another usual empirical challenge relates to data limitations. Specifically, previous research has exploited relatively low-frequency variation (i.e., yearly data) in teenage pregnancies and the intensive margin of time spent at schools. This low-frequency variation may hinder causal identification due to unit-specific cross-sectional omitted variables that may substantially vary at a higher frequency (e.g., within a year).

Furthermore, we contribute to this literature by looking at school closures and, therefore,

[2]For instance, in the case of Chile, the number of hours of formal education increased by more than 20% as the full-day school reform was gradually implemented, starting in 1997. In addition, as was the case for Chile, these reforms tend to be jointly implemented with other reforms or legislation. In particular, it is conceivable that these policies also improve the quality and efficiency of the educational curriculum.

test whether such phenomena can mitigate the effects of expanding schooling (Berthelon and Kruger, 2011; Black et al., 2008; Tan, 2017). Finally, one of the mechanisms behind the effects of expanding schooling on teenagers' risky behavior is the direct effect of accumulating higher levels of human capital that changes the expected returns to this behavior. By estimating the impact of school closures on teenage pregnancy every month, we can rule out a decrease in human capital as a mechanism to explain the increase in teenage pregnancy from other factors (such as reduced adult supervision and changes in risky behavior). However, we find evidence of deterioration of human capital in the longer run when we look at school dropouts during the year of the strike movement. As such, our paper is also related to the literature on the non-labor market effects of schools (Duflo et al., 2015; Oreopoulos and Salvanes, 2011) by studying the impact of schools on teenage pregnancy, which is considered harmful throughout the life course of both teen mothers and teen fathers (Dahl, 2010).

The paper is organized as follows. In section 2, we detail the data with which we work and the definitions of strike intensity and teenage pregnancy. We also present descriptive data on school absenteeism and teenage pregnancy and characterize municipalities along with our measures of strike adherence. In section 3, we provide the context surrounding the school strikes in Chile. We also describe the strike take-up and decline process, presenting quantitative and qualitative insights. In section 4, we discuss our empirical strategy to estimate the effect of school strikes on teenage pregnancies. In section 5, we present the main results, indirect tests for identification assumptions, different heterogeneity analyses, mechanisms, and robustness checks to our main conclusions. In section 6, we conclude.

2 Data and Measurement

2.1 Teenage Pregnancy

The primary dependent variable in our analysis is the monthly number of teenage pregnancies conceived in a municipality.[3] We use administrative data of all births and official fetal deaths in Chile provided by the Ministry of Health of Chile (MINSAL). This administrative dataset includes information about every birth in the country besides non-institutional abortions, reporting babies' characteristics at birth, like gender, gestational age in weeks, height, and weight, for both live and stillbirths. It also provides information about the mother and father of the babies (when identified) at the time of birth, such as age, education, municipality of residence, and the number of children they have.[4] We compute the approximate date of conception for each birth by subtracting gestational age at delivery from the birth date. Since our interest is in high school-aged female students, for the remainder of this document, we define teen pregnancy as the birth of a woman 15 to 17 years of age (inclusive) at the time of delivery.[5] We aggregate individual birth records at the municipality times month of conception level for a final dataset containing the number of conceptions for different maternal age groups in a municipality during each calendar month from 2007 to 2013.

2.2 School-level Academic Outcomes

We also investigated the potential impact of strikes on high-stakes academic outcomes, explicitly examining whether they influenced students' likelihood of dropping out of school or pursuing higher education. We developed two key metrics to assess these outcomes at the

[3] Our unit of study is a *comuna* or municipality, the smallest administrative subdivision in Chile. According to Chilean law, a mayor and a local council govern each municipality, which may administer more than one *comunas*. However, in practice, only one municipality manages more than one *comuna*. Therefore, we also use the term municipalities to refer to *comunas*.

[4] Abortion was not legal in Chile during the analysis period.

[5] Completing high school has been mandatory in Chile since 2003. According to the nationally representative household survey CASEN (2009), 92% of women aged 15 to 17 attended school in 2009.

school level. Firstly, we utilized the Ministry of Education's official administrative performance records at the individual student level to calculate dropout rates from 2007 to 2011. A student was categorized as a dropout if they failed to return to school the following year, despite being expected to do so based on their enrolled grade and given Chile's absence of a minimum dropout age requirement. To compute the dropout rate for each year, we divided the number of high school students identified as dropouts by the total number of high school students anticipated to return to school in the subsequent year.

Secondly, to analyze college admission test participation, we utilized restricted access data from the DEMRE (Departamento de Evaluación, Medición y Registro Educacional) on the nationwide college admission test (Prueba de Selección Universitaria, PSU) results for all students who undertook the test between 2007 and 2014. We classified each student as a PSU test taker if they completed either the Spanish or Math test. We then aggregated the number of test takers by school and applied a logarithmic function (plus one) to this figure for analysis.

2.3 Strike Intensity

Measuring school strike adherence at the municipality level posits an empirical challenge. Although there are no official records of strike adherence, we leverage two sources of information to classify each school as being on strike or not in 2011. The first classification is constructed after a web search using Wayback Machine® software, which allows the search for information stored in expired URL addresses. By web scraping information from blogs written by students during this period, national media, regional and local media, including newspaper, radio coverage, and social networks, we classify each school as being on strike if it is mentioned in any of these sites as taken over by students or closed during 2011.

The second classification is based on official administrative records by the Ministry of Education (MINEDUC) of all students' daily attendance in all schools in Chile, which are available only for 2011 and after. Using this microdata for 2011, we compute monthly school-level average days lost by high school students (9th to 12th grade). We then classify a school as on strike if the average high school student in that school did not attend ten or more days during August 2011.[6] We focus on August 2011 for two reasons. First, August is a month of full potential for school attendance, with no holidays or vacations. Second, the student movement peaked (in terms of adherence) in August 2011.

Although both measures of strike status are susceptible to measurement error, such as media bias in the case of the web-scraping measure or misreporting in the attendance measure, they yield similar estimates regarding the impact of strikes on teenage pregnancy. To err on the side of caution, we create a third classification measure by combining the two original treatment variables.[7] In this combined measure, a school is considered to be on strike if either of the two measures indicates so. Subsequently, we construct our primary treatment variable based on this combined measure. This measure takes the value of one or zero depending on whether school s was on strike or not.[8]

Our goal is to construct a measurement of strike intensity at the municipality level during the strike period in the following way:

$$Strike\ Intensity^{k}_{mt} = Strike\ Period_t \times Strike\ Adherence^{k}_{m} \tag{1}$$

$Strike\ Period_t$ captures the duration of nationwide protests by taking the value of 1 from April 2011 to December 2011 and 0 for all other months in the sample. $Strike\ Adherence^{k}_{m}$ captures cross-sectional variation in the average strike intensity experienced by a municipality. Formally, $Strike\ Adherence^{k}_{m}$ is computed as:

[6] As discussed below, results are robust to using five days of school days lost as the threshold (see Online Appendix Table B.1).

[7] We extensively discuss potential consequences of measurement error of strike status in our estimations in subsection B.4.

[8] We conduct robustness checks in our analysis, using either of the two variables in all specifications. The results of these robustness checks are provided in Appendix Table A.1 and Table A.2.

$$Strike\ Adherence_m^k = \frac{\sum_{i=1}^{N_m} 1_{i(s)} School\ on\ strike_s^k}{N_m} \tag{2}$$

Where i, s, and m denote a female student, school, and municipality, respectively. N_m is the total number of female students living in municipality m aged 15 to 17,[9] while $School\ on\ strike_s^k$ is a binary indicator of whether school s where female student i attends was on strike according to our different k measures. The superscript $k \in \{1, 2, 3\}$ differentiates the three measures of strike classification, according to which information is used: $k = 1$ corresponds to web-scraped data, $k = 2$ corresponds to school attendance data and $k = 3$ to the combination of both classifications. The microdata of the MINEDUC includes the municipality of residence of each student, so we can aggregate variables at the municipality of residence, which makes an essential difference given that a quarter of students attend a school outside their municipality. Hence, $Strike\ Adherence_m^k$ is the proportion of female students in municipality m who attended a school on strike according to the measure k in 2011. We also specify $Strike\ Adherence_m^k$ as quantiles (20 dummy variables) to explore nonlinearities in the effect of strikes on teenage pregnancy.

As such, measure $Strike\ Intensity_{mt}^k$ is calculated as the interaction of two components: a time-varying binary indicator of the students' strike period and a strike adherence measure constructed as the proportion of female students residing in municipality m who attended a school on strike (for each of the $k \in \{1, 2, 3\}$ classification measures). Figure 1b shows municipalities' cross-sectional variation in strike adherence according to the measure that combines web scraping and the daily attendance data, with the average municipality experiencing a 26% adherence of resident students.[10]

One concern is that the strike was concentrated in particular municipalities, such as those

[9]We do this to match the ages for which we build our dependent variable.

[10]Figure B.1 in the online appendix shows daily school absenteeism and the cross-sectional variation for each definition of strike adherence separately.

closer to the country's capital, Santiago. In Figure 1c, we depict municipalities in the country according to their strike adherence.[11] The figure shows that the strike was distributed similarly across the territory, with municipalities experiencing low and high intensity in different locations in the country, mitigating concerns about geographical selection. In Figure B.3, we zoom in on the Metropolitan Region of Chile to show that, even within a region, there is substantial heterogeneity in strike adherence. We address selection in other dimensions in the next section.

3 Background on the 2011 Chilean Student Strikes

In May 2011, high school and university students in Chile launched a protest movement intending to influence policy and reform the country's educational system. This was Chile's second major student strike during the 2000s, following the notable "Penguin Revolution" of 2006. Led primarily by high school students, both protests sought to address similar issues regarding educational reform. However, the 2011 protest became one of Chile's most significant movements, lasting more than seven months. In contrast, the 2006 protests lasted less than two months, while disruption of school activities was more intermittent and particularly concentrated in large urban areas (Bellei and Cabalin, 2013).[12]

Approximately 15,000 students protested in different regions of the country a few days before President Sebastian Piñera gave the annual presidential speech on May 21st.[13] Two weeks later, on June 1st of 2011, leaders of the student movement convened all Chilean students to go on a national strike of schools and universities.[14] By June 25th, more than

[11] Figure B.2 depicts the geographical distribution of our treatment under the three alternative definitions.

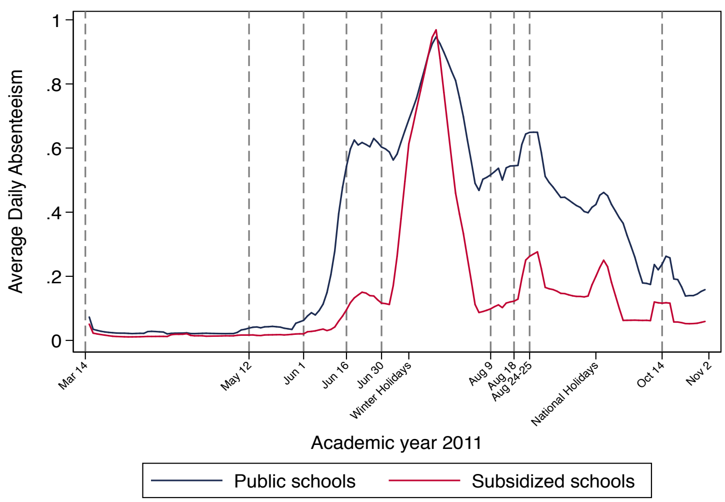
[12] The 2006 protests concluded on June 7th, after 22 days, when the government of Michelle Bachelet responded to the demands of the student movement by establishing a national council for educational reform. This decision was made in recognition of the concerns raised by the students during the protests and was designed to address the need for significant changes in the education system. See the following press article link. Accessed on 07/05/2023.

[13] See this link for more information about the first protest. They were accessed on 22/08/2017.

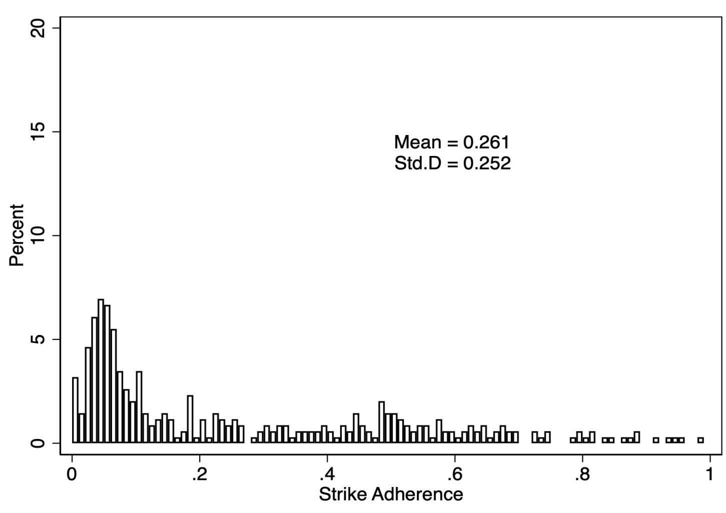
[14] González (2020) provides a comprehensive description of the student movement.

Figure 1: Daily School Absenteeism and Cross-Sectional Variation in Strike Adherence using Different Measures

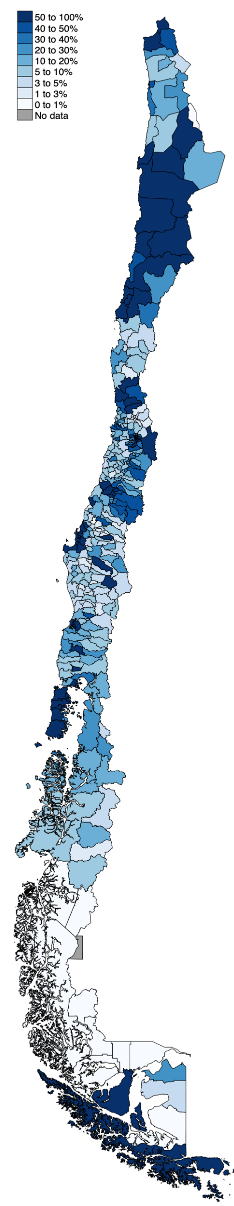
(a) Daily Absenteeism during 2011 by Type of School



(b) Distribution of Strike Adherence combining both measures



(c) Mapping Strike Adherence combining both measures



Notes: (a): This subfigure shows the trends in daily school absenteeism in a moving average of 2 days during 2011 by type of school. The blue line represents public schools, while the red line represents voucher schools. (b): This subfigure shows the strike adherence distribution at the municipality level according to the strike adherence measure obtained by combining both measures. (c): This subfigure shows the geographic distribution of strike adherence after combining both measures.

600 of approximately 2,330 high schools adhered to the strike. Strikes usually consisted of students not attending classes or taking over school infrastructure and spending day and night inside, forbidding any school activities.[15] The strike became notorious nationally and internationally as well. In that same year, Times magazine selected a leader of the student movement as one of the most influential people of the year 2011.[16] Strikes continued during and beyond school winter break, with protests reaching a peak of adherence in late August of 2011, after which the movement started to fade out.

One of the main mechanisms through which school strikes affect teenage pregnancy rates is by (unexpectedly) relaxing time spent by students under adult supervision (e.g., teachers, principals) while adult caregivers are at work. A typical school year in Chile consists of 40 weeks of classes and typically runs from the first week of March to mid-December, with two to three weeks of winter break in July. To track how strikes affected school attendance, Figure 1a illustrates the rate of daily absenteeism in 2011 by type of school (measured by administrative records): Public and voucher.[17]

During the first months of the school year, absenteeism was negligible. The first noticeable increase occurs on May 12th in public schools, the day of the first protest. Few schools started to strike during the first week of June, illustrated by a prominent increase in absenteeism during this week. By June 30th, the daily absenteeism rate increased to approximately 70%

Using school-level data, we used a linear probability model to examine factors influencing the likelihood of a specific school going on strike. To understand how we determine the strike status, please refer to section 2.[18] The results of the linear probability model are presented in Figure 2. We plot the coefficients and confidence intervals for each characteristic of the

[15]In some cases, municipal authorities, with the help of the Ministry of Interior, used police to force students out of schools. See link, for instance. They were accessed on 22/08/2017.

[16]See this link for the coverage of Times magazine. See this link for full coverage in the New York Times in the year 2012.

[17]In Chile, schools are roughly divided into public (45%), voucher (45%), and private (10%). We focus on the first two in this paper since the adherence of private schools to strikes was minor.

[18]Table B.10 in the online appendix presents summary statistics for all variables employed in this analysis.

school. All characteristics are measured before 2011, the year when protests took place.

The main factors at the school level that are associated with strike participation include the composition of schools in terms of grade levels and whether they are considered iconic public schools or *emblematic*[19]. Additionally, the probability model also considers other characteristics, such as the proportion of students residing in the same municipality to proxy networks within schools, the percentage of households where the head earns more than the minimum wage, the average level of education of fathers, the level of parental involvement in school, the average 10th-grade test scores on standardized national exams, the dropout rate, average attendance, measures of student mood and student aspirations derived from student survey data.[20]

Our findings suggest that schools including grades first to sixth (primary school) are less likely to participate in strikes than schools covering only middle and high school grades. However, schools classified as *emblematic* have a higher propensity to strike. One plausible explanation for these results is that schools offering primary education face more pressure from parents' associations, discouraging student participation in strikes due to potential disruptions to young children's daily attendance. On the other hand, *emblematic* schools have a long-standing tradition of involvement in political movements, with many former students becoming presidents. These schools have historically played an influential role in student political associations.

In addition, our analysis shows no statistically significant association between strike status and socioeconomic characteristics of students proxied by school-level average wage of parents and parental education. We also do not observe statistically significant associations between strike status and academic proxies, such as average test scores in standardized national exams and dropout rate. We do find a negative correlation between strike status and school

[19]"Emblemático" is a term used in Chile to describe a public school that combines academic excellence, tradition, and prestige. These institutions are recognized for their comprehensive and influential educational initiatives, often ranking among the top public high schools in the country.

[20]For a detailed description of the variables we use, please refer to Online Appendix subsection B.3.

attendance in previous years to the strike. There is also no association between school-level strike adherence and measures of student aspirations (i.e., going to college), students' mood status, or the degree of parental involvement in school activities.[21]

The end of the 2011 movement.– During the subsequent months, protest activities experienced a gradual decline due to multiple factors. These factors encompassed concerns among students and parents about academic progression, the initiation of formal negotiations, and a loss of societal support primarily attributable to the focus of the media on violent protesters.[22]

To understand the depicted decline shown in Figure 1a, we created a variable to gauge the probability that a school would achieve a 90% attendance rate in 2011 after August of that same year. Using this variable as a dependent factor and the same covariates shown in Figure 2 as independent variables, we performed a linear probability model. Our analysis was limited to the sample of striking schools exclusively.

The findings in panel (b) of Figure 2 reveal that the *emblematic* status, pre-strike attendance, and dropout rates account for the continuation of absenteeism even after the strike period. This suggests a similar pattern between the occurrence of strikes and the persistence of absenteeism. Furthermore, we do not find statistically significant relationships between strike persistence and socioeconomic characteristics of students, academic indicators and measures of student aspirations (e.g., going to college), students' mood status, or parental involvement in school activities.

In summary, the findings of both panels of Figure 2 indicate that certain school characteristics serve as significant covariates for both the probability of strike occurrence and its duration. In our subsequent econometric analysis, we provide evidence that incorporating school differences based on whether the school has primary levels, is classified as "emblem-

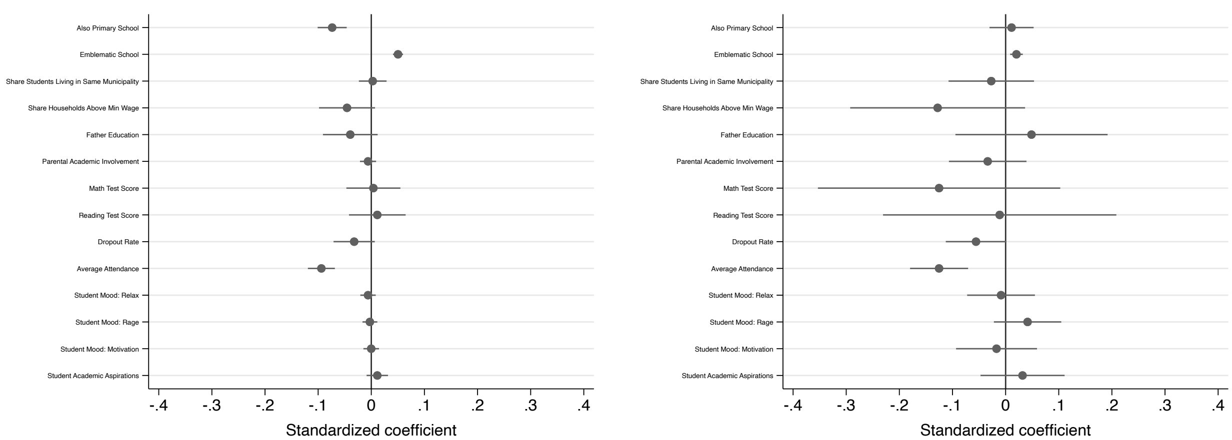
[21]This data is collected through surveys administered to all students, parents, and teachers across different grade levels (4th, 8th, 10th) in schools. These surveys serve as a valuable complement to the national test score data known as SIMCE. See Online Appendix subsection B.3 for a detailed description of how we constructed these indicators.

[22]The government started a program called "Salvemos el año" (save the school year) for students who wanted to continue their studies while strikes were ongoing.

atic," or had low attendance rates before the strike does not impact our main results. The distinction of *emblematic* is crucial to address potential selection bias stemming from the 2006 protests. The possibility that the 2006 strike led certain parents to select schools away from those with a higher propensity to strike may indeed influence the results. Distinguishing the analysis between these schools can help mitigate this issue. These characteristics, though relevant to the strikes, do not change the overall conclusions of our study.[23]

Figure 2: Associations between School Level Characteristics, Strike Take-up, and Decline

(a) Strike take-up among all schools (b) Strike decline among striking schools



Notes: Each panel in this figure plots the coefficients and the 95% confidence intervals for a set of school-level covariates (listed on the y-axis) for the analysis of (a) the probability of going on strike (among all schools in Chile, N = 2,505) and (b) The probability of recovering pre-strike assistance levels during 2011 among schools that were on strike (N = 455). All covariates are standardized. Both regressions are based on OLS, include municipality fixed effects, and cluster the standard errors at the municipality level. Figure B.4 in the online appendix shows the results without municipality fixed effects.

4 Empirical Strategy

4.1 School strikes and Teenage pregnancy

To unravel the relationship between exposure to school strikes and teenage pregnancies, we study how trends in the number of teenage pregnancies conceived in a municipality in a

[23] Figure B.5 in the online appendix shows a similar analysis for the case of the probability that the school will be occupied during the strike period.

month change during the school strike period and whether this change can be interpreted as a causal effect of school closures. In particular, we estimate different specifications of the following econometric model for the 2007-2013 period:

$$\textit{Teenage Pregnancies}_{mt} = \alpha + \beta \textit{Strike Intensity}^k_{mt} + \gamma X_{mt} + \lambda_m + \tau_t + \varepsilon_{mt} \tag{3}$$

Where m and t denote the municipality and conception time (month), respectively. The sample consists of municipality-month observations for 345 municipalities over 84 months. The main dependent variable, $\textit{Teenage Pregnancies}_{mt}$, is the (log) number of children conceived during the month t and who were born to teenage mothers who lived in the municipality m at the time of birth. Since we log-transformed the dependent variable in our preferred specification, we add one to include municipality $\times$ month observations with zero conceptions.[24] $\textit{Strike Intensity}^k_{mt}$ is our main independent variable. The semi-log specification presented in equation (3) facilitates the interpretation of the point estimate for β as a standard semi-elasticity, that is, a variation in a unit of strike intensity has an effect of $\beta\%$ on teenage pregnancies. However, throughout our analysis, we demonstrate that our main results are not contingent on the transformations applied to our dependent variable.

X_{mt} is a vector of controls that includes municipal-specific linear trends, total pregnancies (in logs) for women 25 to 45 years old - to account for changes in global fertility rates -, poverty rate, per capita government expenditure (in logs), the teenage student population in public schools (in logs), population (in logs), and female population (in logs). Incorporating this set of controls aims to improve the precision of our estimations. However, later on, we demonstrate that their inclusion does not affect our main results. OLS consistently estimates β if no changes in unobserved or uncontrolled variables correlate with the variation in the strike's intensity. For example, these could be variables reflecting variations in the supply of prevention programs for teenage pregnancy. The high-frequency data exploited allow the

[24] Roughly 30% of the municipality $\times$ month observations have zero conceptions for teenage girls.

seasonality to be controlled very granularly. Furthermore, we include municipality fixed effects (λ_m) that account for unobserved common characteristics within each municipality over time. The month-specific conditions common to all municipalities are controlled using month-of-conception fixed effects (τ_t). Likewise, focusing on a short time window allows for controlling any endogeneity problem due to internal migration patterns, such as moving to lower strike intensity areas. We allow the error term ε to be correlated within a municipality.

Another key identifying assumption in estimating (3) is that the trends in potential outcomes for municipalities with high intensity would have been the same as in municipalities with low intensity had they experienced equally low levels of strike intensity. And vice versa. Although this assumption is not testable, commonly known as parallel trends in potential outcomes, in section 5, we explore whether observed trends in teenage pregnancies in periods before the strike are correlated with strike intensity measures.

In addition, recent studies show that the heterogeneity of the treatment effect can complicate the interpretation of the parameters estimated when using a difference-in-differences approach with continuous treatment (e.g., Callaway et al., 2021). To mitigate concerns, we also model below our treatment as nineteen different binary indicators where each is equal to one if a municipality is above the $5th$, $10th, \ldots, 95th$ percentile of the distribution of strike adherence shown in Figure 1b.

4.2 School strikes and Academic outcomes

We also delve into analyzing the effects of strikes on school dropout rates and college application behavior. Strikes within the education sector can potentially disrupt the learning environment and significantly affect students' educational journeys. Through examining these specific outcomes, we aim to shed light on how strikes can shape students' academic trajectories and future prospects.[25]

Prolonged disruptions within the educational system can lead to increased student disengagement and a higher likelihood of premature school leaving. Furthermore, we investigate the influence of strikes on college application behavior. College admission represents a significant milestone for students aspiring to pursue higher education and broaden their career opportunities. Strikes-induced disruptions may potentially sway students' decisions and actions regarding college applications. To explore this phenomenon, we utilize data from Chile's Ministry of Education, which offers extensive information on school dropout rates across various educational levels.

We compile dropout rates and the uptake of college admission tests at the school level from 2008 to 2014. We employ an event study analysis methodology to analyze the impact of strikes on these outcomes. This approach enables us to compare outcomes between schools that experienced strikes and those that did not, both before and after the onset of strikes.[26]

We run the following regression at the school level:

$$y_{st} = \sum_{\substack{\tau=2007 \\ \tau\neq 2010}}^{2013} \gamma_\tau \left(\textit{School on strike}_s^k \times 1[\Upsilon_t = \tau] \right) + \beta Z_{st} + \lambda_s + \theta_t + \varepsilon_{st} \tag{4}$$

Where s and t denote school and year. The sample consists of 2,967 schools and eight years of data. The two dependent variables of interest, y_{st}, are school s dropout and college admissions test take-up rates in year t. As previously defined in subsection 2.3, $\textit{School on strike}_s^k$ is a binary indicator of whether school s was on strike according to strike measure k. Υ_t is an indicator variable that takes the value one when time is τ, 0 otherwise. We exclude the interaction between $\textit{School on strike}_s^k$ and τ_{2010} to use that year as the baseline for comparisons. Z_{st} is a vector including school-specific time trends. Furthermore, we include school

[25]For a comprehensive analysis of the impact of strikes on educational outcomes of students, refer to Gaete (2018).

[26]For a detailed description of the construction of the dropout indicator, please see Online Appendix B.3.

fixed effects (λ_s) that account for unobserved common characteristics of students within each school over time, while aggregate shocks common to all schools are controlled for by including year fixed effects (θ_t). Lastly, we allow the error term ε_{st} to be correlated within a school.

5 Results

This section presents estimations of the effect of school strike exposure in a municipality on teenage pregnancy by exploring average effects, falsification tests, measurement issues from the construction of our strike intensity measures, and a heterogeneity analysis to shed light on potential mechanisms.

All considered, our results suggest that in absolute numbers, a 2.7% increase in teen conceptions corresponds to 309 additional pregnancies, or 39 pregnancies per treated month, once we consider total teen conceptions in the pre-strike year in the same months as a reference. As the average number of days lost during a strike month reached approximately 14 school days, a simple back-of-the-envelope calculation suggests that teen conceptions increased by approximately 2.7 per school day lost. In contrast, Berthelon and Kruger (2011) find that teenage conceptions in Chile decrease by 1.1 per each day of additional school followed by a reform that changes the school schedule from half to full days – similar in absolute magnitude to our effects. In addition, Rau et al. (2021) find that being exposed to a 45% price reduction in anti-conception pills during a year decreases teen conceptions by 584 per month – a much more considerable effect than school closures. Our results are also similar in magnitude to the effects found by Black et al. (2008) for compulsory school laws in the USA and Norway.

Different falsification tests confirm these results, and heterogeneity analyses, among other complementary regressions, shed light on the fact that laxer adult supervision during the strike period is most likely to be the mechanism behind the main effects. We also find substantial adverse effects on school dropouts and college admission test taking, suggesting that the impact of strikes goes beyond teenage pregnancy rates.

5.1 Main Results

We first present results using the three measures of strike intensity, based on strike adherence constructed from web-scraped data, attendance data, and strike adherence that combines both measures. The analysis is conducted at the municipality-month level, encompassing 345 municipalities from January 2007 to December 2013.[27] Table 1 shows the main results from estimating different versions of Equation 3.

The results in columns (1) to (3) of Table 1 show that the effect of strikes on teenage pregnancy is similar across the three measures we use. A municipality with an additional exposure of 10 percentage points, signifying a ten percentage point increase in the number of resident high school female students attending schools on strike, witnessed a monthly rise in conceptions during the strike period ranging from 10% to 11%. For a more straightforward interpretation, consider that a municipality with an average proportion of students on strike (26% according to the combined measure) experienced a 2.7% increase in teenage pregnancies during the strike period.

Across various specifications, the relationship between strike adherence and teenage pregnancy and its absolute magnitude remains consistent. In particular, Column (4) employs an inverse hyperbolic sine transformation of the number of births to women aged 15-17 as the dependent variable. In Column (5), the rate of teenage pregnancies is utilized, which is defined as the number of births to women aged 15-17 divided by the number of public school female students aged 15-17 while also accounting for weights based on the number of public students aged 15-17 in the municipality. Furthermore, Column (6) utilizes a Poisson model where the number of births to women aged 15-17 is the dependent variable.

Next, we explore potential non-linearities in the association between strike intensity and

[27] Table B.11 in the online appendix presents summary statistics for all variables employed in the analysis.

Table 1: Effect of Strike Exposure on Teenage Pregnancy

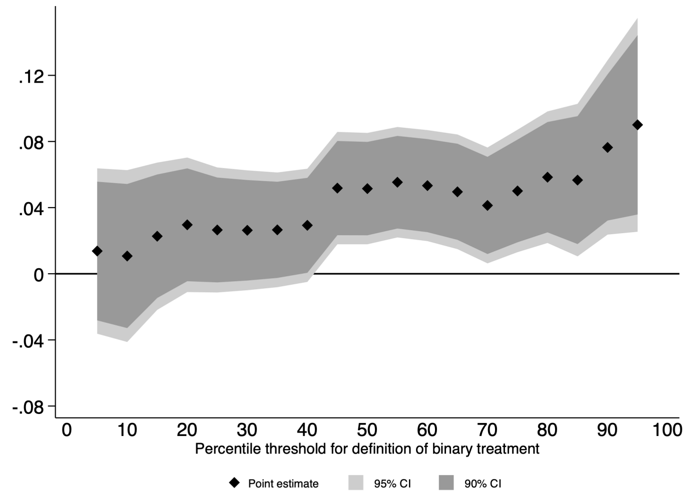
Dependent Variable: Teenage Pregnancies (births to women aged 15-17)	in logs			IHS transf.	Rates	Counts
	(1)	(2)	(3)	(4)	(5)	(6)
Strike Intensity (Web-Scrapped)	0.098***					
	(0.037)					
Strike Intensity (Attendance)		0.113***				
		(0.038)				
Strike Intensity (Main)			0.107***	0.128***	0.353**	0.096**
			(0.033)	(0.042)	(0.163)	(0.043)
Mean of Dependent Variable	1.03	1.03	1.03	1.31	3.72	3.73
Observations	28,980	28,980	28,980	28,980	28,980	28,560
Adjusted R^2 /Pseudo R^2	0.794	0.794	0.794	0.780	0.191	0.641

Notes: This table reports estimates of the effect of strike exposure using different ways to measure the outcome of interest, teenage pregnancies, and three alternative measures of Strike Intensity. Columns (1) to (5) present the results for an estimation of an OLS fixed-effects model, varying the definition of the dependent variable, while Column (4) shows the results of a Poisson regression model. Columns (1) to (3) use the logarithm of the number of births to women aged 15-17 plus one as the dependent variable. Column (4) uses an inverse hyperbolic sine transformation of the number of births to women aged 15-17. Column (5) uses the rate of teenage pregnancies, defined as the number of births to women aged 15-17 over the number of public school female students aged 15-17, including weights for the number of public students aged 15-17 in the municipality. Column (6), the Poisson model, directly uses the number of births to women aged 15-17. All specifications have the same controls: a constant, the logarithm of pregnancies of women 25 to 45 years old as a control, the logarithm of the population aged 15-17 enrolled in public schools, municipality poverty rate, per capita government expenditure in education (in logs, per student in public school), total population (in logs), and total female population (in logs). Municipality linear time trends are included by interacting municipality fixed effects with a linear trend in months. The observation unit is municipality-month (with 345 municipalities from January 2007 to December 2013). Column (6) has fewer observations as five municipalities have zero births to women aged 15-17 each month and are therefore excluded in the Poisson model computation. Robust standard errors are clustered at the municipality level in parentheses. *** p<0.01, ** p<0.05, * p<0.10.

teenage pregnancy rates. Specifically, we investigate whether the effect of strikes varies across different levels of adherence. We employ the exact specification as Column (3) in Table 1 using a binary indicator variable as the treatment variable. This binary indicator identifies whether each municipality falls above the $5th$, $10th, \ldots, 95th$ percentile of the strike adherence distribution depicted in Figure 1b. Consequently, we conduct nineteen separate regressions, each employing one of these binary indicators.

The results in Figure 3 indicate that the effects become statistically significant once the level of adherence exceeds a threshold above the median. While the point estimates for municipalities situated at the highest percentiles exhibit increased magnitudes (monotonically), the confidence intervals are too wide, and the point estimates are not significantly different from those of municipalities at the median adherence level.

Figure 3: Alternative thresholds for definition of binary treatment variable



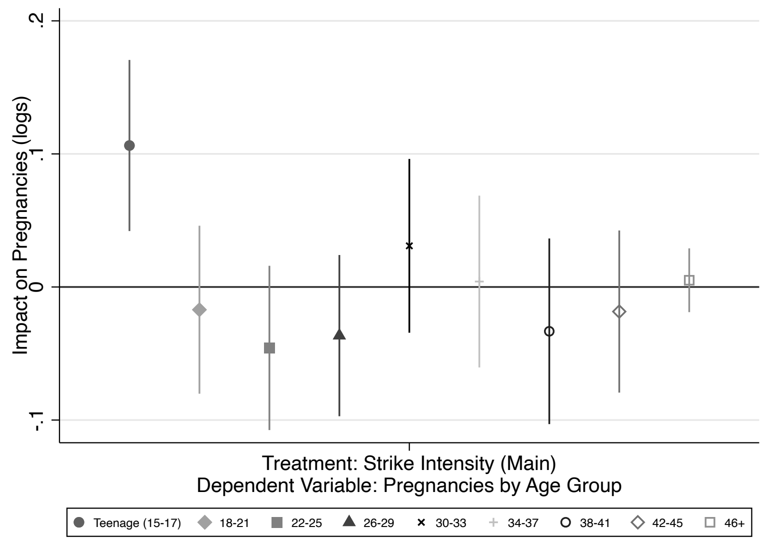
Notes: This figure plots coefficients from a specification similar to Column (3) in Table 1 using a binary indicator variable as the treatment variable. This binary indicator identifies whether each municipality falls above the $5th$, $10th, \ldots, 95th$ percentile of the strike adherence distribution depicted in Figure 1b. Consequently, there are nineteen separate regressions, each employing one of these binary indicators. This figure plots the estimates for each regression, with the red point indicating the estimate for a threshold of 75 percent.

Robustness checks.– A primary concern is that the strike exposure variable may be capturing an association with overall fertility trends in municipalities rather than specifically focusing on teenage pregnancies. To ensure that the main effect of the strikes on teenage pregnancies presented in Table 1 is not simply capturing a general trend in pregnancies, we conduct a simple check by estimating our main model for pregnancies at different age intervals (i.e., we focus on three years-intervals). Figure 4 suggests that our measure of strike intensity only predicts an increase in pregnancies for women aged 15-17 since no other age group displays a statistically significant coefficient.

In addition, we present alternative specifications to the ones in Table 1, allowing for different sets of controls. The inclusion or exclusion of these controls does not significantly impact the magnitude of our primary coefficient of interest. In Figure A.1, we show the sensitivity of coefficients to the inclusion and exclusion of different sets of controls.[28]

[28] In most cases, our point estimates are statistically significant at standard confidence levels, except when using teenage pregnancy rates as the dependent variable in Poisson model specifications. In these specific instances, the point estimates are marginally insignificant. However, it is worth noting that these estimates

Figure 4: Effects for Different Age Groups



Notes: This figure plots coefficients and their 95% confidence intervals from regressing total pregnancies (in logs) for different age groups on our primary measure of strike intensity. All specifications include municipality and month fixed effects and municipality-specific linear time trends. The observation unit is municipality-month (with 345 municipalities from January 2007 to December 2013).

Our school-level analysis in section 3 unveiled that three school characteristics, namely whether the school also had primary level instruction, its *emblematic* status, and attendance rates before the strike, seem to be significant predictors of both the extensive and intensive margin of strike adherence. This begs the question of whether our results are confounded with these characteristics. We check whether these characteristics influence our findings. To do so, we construct a measure of school attendance at the municipality level, utilizing attendance data from all years before the strike. This is similar to the method employed for constructing strike status at the municipality level. In this case, we constructed a binary indicator at the school level to identify schools with an average attendance below 90%. Using this binary indicator, we created a municipality-level variable, representing the percentage of students in that municipality attending schools with consistently low attendance rates (below 90%) in the years leading up to the strike. We added this variable interacted with the strike period dummy

are statistically similar to the estimates obtained from other specifications.

as a control variable. Our main results, shown in Table B.2, remain unaltered. Similarly, controlling in the same way for the proportion of female students aged 15-17 attending secondary schools with primary education does not affect the results Table B.3. Finally, in subsection 5.3, we explore the role of *emblematic* schools and show that this type of school is not driving our results.

We next explore whether our main results change if the intensity of the strikes is defined differently, using boys' exposure to the strikes instead of that of girls. We do this by modifying the methodology in Equation 2, so $\textit{Strike Adherence}^k_m$ captures cross-sectional variation in the average strike intensity experienced by a municipality as experienced by *male* students, defined as the proportion of male students in municipality m who attended a school on strike according to the measure k in 2011.

Table A.3 replicates our main results, as presented in Table 1. Comparing both tables, estimates using boys' exposure are slightly smaller but show little to no difference in the results depending on whether boys' or girls' strike adherence was used. This is not surprising if we consider the high correlation between the two variables, as most schools in Chile are coeducational, with no significant differences expected across municipalities for these variables. [29] However, if we include both variables together in a horse race, only our original variable using female exposure remains statistically significant (results not shown), whereas the point estimate for the alternative measure reduces substantially in size. Nevertheless, larger standard errors are observed due to strong multicollinearity, as anticipated.

5.2 The dynamic of teenage pregancy rates before and after strikes

In this section, we examine the dynamics of teenage pregnancy rates and covariates before and after the strike period, thereby indirectly addressing the assumption of parallel trends in

[29]Figure B.7 displays the correlation between female and male main strike exposure measures at the municipality level, showing little dispersion away from the 45^o line.

potential outcomes underlying our previous analysis. To achieve this, we employ several event study analyses, wherein different variables are used as the dependent variable and regressed against time dummies – in years or months, depending on the frequency of the available data, and interactions of time dummies with our measure of strike adherence. These specifications include municipality and time fixed effects and municipality-specific linear trends.

The initial analysis we present is the event study analysis focusing on teenage pregnancy rates as the dependent variable. The results are displayed in Figure 5. The figure exhibits 95% confidence intervals for twenty-three month dummies interacted with strike adherence, covering a 12-month window before and after the onset of the student protest in May 2011. To establish a reference point, the coefficient for April 2011 is normalized to zero.[30]

The figure provides evidence that there are no statistically significant differences in the association between strike adherence and teenage pregnancy rates when comparing the period preceding the onset of the student movement to the period of April 2011. This finding supports the notion of parallel trends in potential outcomes, which aligns with our interpretation of the estimates presented in Table 1 as causal effects of strikes on teenage pregnancy rates. The absence of significant differences suggests that the observed effects are not driven by pre-existing trends or factors unrelated to strikes, further supporting the validity of our findings regarding the impact of strikes on teenage pregnancy rates.

Figure 5 shows that the point estimates for the first two months of the strike are not statistically different from zero, while the effects increase significantly in months 2 and 3, which corresponds to July and August, and continue to be relatively high in September and October (months 4 and 5 in the graph). This coincides with a period in which there is still a significant absenteeism rate, as shown in Figure 1a.

[30]The coefficients are estimated from a unique regression of teenage pregnancies (in logs), which includes municipality and month fixed effects as well as municipality-specific linear trends, the logarithm of pregnancies of women 25 to 45 years old, the logarithm of teenage population enrolled in public schools, poverty rate, per capita government expenditure in education (in logs, per student in public school), total population (in logs), and total female population (in logs). This is column (3) of Table 1 specification.

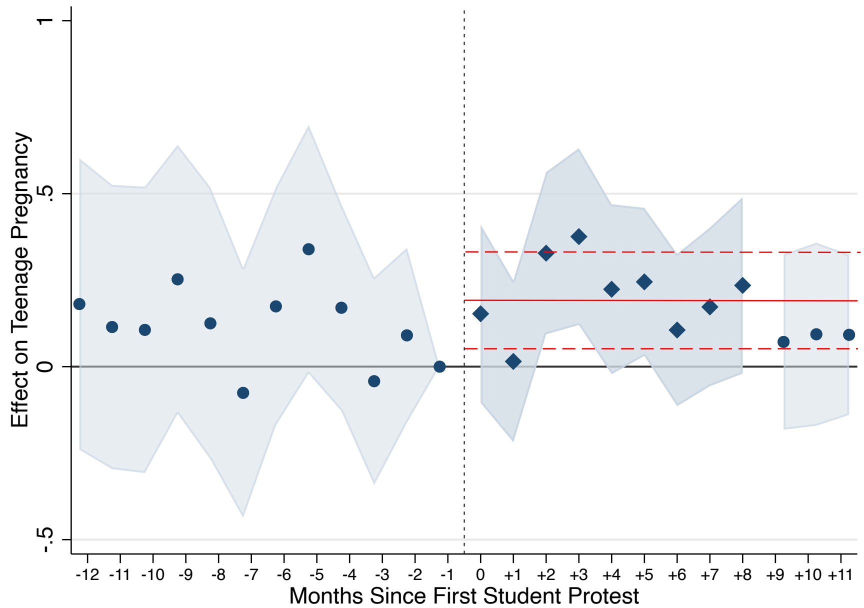
Finally, as the strike fades away in the last months of the year, changes in birth conceptions are unrelated to the municipality's strike adherence. The figure also displays in the horizontal red lines the point estimate and 95% confidence intervals from a difference in difference estimation (from column (1) of Table B.4), showing that period-specific point estimates during the strike fluctuate within this confidence interval.

By increasing the number of lags for the pre-treatment period and using alternative measures of strike adherence based on web-scrapped data and school days lost, Figure B.6 provides further evidence regarding the robustness of the results discussed for Figure 5. Two results are worth highlighting. Firstly, regardless of the measure used to calculate strike adherence, we observe a spike in pregnancies during periods of stronger strike p without witnessing any systematic pre-trend. Secondly, the results using web-scrapped data (Panel b of Figure B.6) show lower average coefficients for the pre-treatment period.

We repeat this analysis for multiple covariates of teenage pregnancy to analyze parallel trends across municipalities with different strike adherence in different dimensions. To do this, we characterize Chilean municipalities using various potential covariates of teenage pregnancy, including demographic characteristics, educational outcomes, municipality resources, fertility outcomes, and the prevalence of contraceptive methods among teenagers.[31] Figure A.2 shows our analysis of the temporal evolution of per capita municipal income, poverty rates, per capita municipal expenditures, per capita municipal investment in education, school-age population, population density, birth rates, pregnancy rates for different age groups, high school promotion and attendance rates (for both all students and female students), and disbursements for contraceptive methods among youth aged 14-19. For most of these measures, we have access to annual data from 2007 to 2013, so our analysis relies on data at the year level. The evidence shown in the figure is consistent with the parallel trends assumption as we observe no significant differences across municipalities and no statistically

[31] See subsection B.3 for a detailed description of these variables.

Figure 5: Event Study



Notes: This figure plots the coefficients and the 95% confidence intervals for twenty-three-month dummies interacted with strike adherence for 12 months-window before and after the start of the student protest (i.e., on May 2011). The coefficient for April 2011 is normalized to zero. Confidence intervals are based on heteroskedasticity-robust standard errors clustered by the municipality. Circles correspond to periods when the strike was not active, while diamonds indicate periods in which the strike was active. The coefficients are estimated from a unique regression of teenage pregnancies (in logs), which includes municipality and month fixed effects as well as municipality-specific linear trends, the logarithm of pregnancies of women 25 to 45 years old, the logarithm of the teenage population enrolled in public schools, poverty rate, per capita government expenditure in education (in logs, per student in public school), total population (in logs), and total female population (in logs). Red horizontal lines represent point estimate (solid line) and 95% confidence intervals (dashed lines) from a difference-in-difference estimation using the same sample estimated (see column 1 of Table B.4).

significant trend before the strike period. Noteworthy, subfigures (l) and (m) of Figure A.2 demonstrate that average attendance rates at the municipality level do not exhibit differential trends in the years before the strike.

5.3 Analysis by type of school

In addition to using different specifications of the dependent variable (see Table 1), various measurements of strike intensity, and a different set of control variables, we conduct additional robustness checks to the construction of the strike intensity variable based on school's characteristics. The results are shown in Table 2. Column (1) shows the results from the main results shown in column (3) of Table 1.

First, we investigate the effects of strikes by including private schools when calculating strike intensity, which were initially excluded from the main analysis. The results in column (2) show that the point estimates remain similar, indicating that including private schools does not significantly alter the observed effects.

Next, we refine our measure of strike intensity by adopting a more detailed approach. More precisely, we classify schools as being on strike only if students occupy them. We use web scraping data that explicitly identifies schools where students spent extended periods, including nights and days, within the premises during the strike period.[32] The results in column (3) indicate that the point estimates do not undergo substantial changes when considering only occupied schools.

Furthermore, when we further decompose strike intensity by distinguishing between occupied and unoccupied schools, we observe no statistically significant difference between these two groups. This finding suggests that the effects of strikes on teenage pregnancy rates are comparable for schools that were occupied by students and those that were not.

We conduct a similar decomposition by focusing solely on schools classified as *emblematic* or iconic schools. These schools are widely recognized for their academic excellence, tradition, and prestigious status, often ranking among the top public high schools in the country. As shown in Figure 2, *emblematic* schools are also more prone to strike participation. The distinction of *emblematic* is vital to tackle potential selection bias from the 2006 protests. The chance that the 2006 strike prompted some parents to choose schools less likely to strike could impact results. Analyzing these schools separately can address this concern.

When constructing a strike intensity measure using only *emblematic* schools, we observe a substantial increase in the effects, roughly four times higher than the overall analysis. However, it is essential to note that the mean of the independent variable within the *emblematic*

[32] Occupation status data taken from Donoso et al. (2016)

group remains relatively small compared to the average strike intensity measure encompassing all schools. Specifically, the independent variable represents only 1% of the average strike intensity of 26%. It is worth highlighting that *emblematic* schools constitute only a tiny fraction of Chile's total number of schools. Further, the standardized results (not shown in Table 2) suggest that the impact on teenage pregnancy is 60% bigger for municipalities with strike intensity coming from not *emblematic* schools.

These findings emphasize that *emblematic* schools, despite their higher likelihood of participating in strikes, represent a small proportion of the overall school population in Chile. While the effects are amplified when considering only *emblematic* schools, it is crucial to recognize the limited representativeness of this subgroup. Thus, caution should be exercised when generalizing the findings to the broader context of school strikes and their nationwide impact on teenage pregnancy rates. Furthermore, upon excluding *emblematic* schools from our analysis, we found that the point estimates of strike intensity in our main specification did not experience significant changes. This suggests that the inclusion or exclusion of *emblematic* schools does not significantly impact our study's overall findings and principal conclusions.

To address concerns regarding the potential geographical concentration of the student movement, we replicate the exact specification as column (3) in Table 1 systematically excluding from the sample one geographic region of Chile at a time. The results of this validation exercise are presented in Figure A.3. Each panel in Figure A.3 represents a different specification of the dependent variable. The point estimates Within each panel correspond to those obtained after excluding a specific region. In total, we obtained sixteen point estimates, each excluding a different region of Chile. Our analysis shows that the effects of strikes on the outcomes of interest remain unchanged across the various specifications, indicating that the main results of our study are not driven by any particular geographical zones within the country.

Table 2: Effect of Strike Exposure on Teenage Pregnancy: Decomposition

Dependent Variable: Teenage Pregnancies (births to women aged 15-17), in logs	(1)	(2)	(3)	(4)	(5)	(6)
Strike Intensity	0.107***					
	(0.033)					
Strike Intensity (Including Private Schools)		0.099***				
		(0.032)				
Strike Intensity (Occupied Schools)			0.105**	0.106**		
			(0.043)	(0.042)		
Strike Intensity (Not Occupied Schools)				0.107**		
				(0.052)		
Strike Intensity (Emblematic Schools)					0.420***	0.362***
					(0.152)	(0.131)
Strike Intensity (Not Emblematic Schools)						0.096***
						(0.034)
Mean of Dependent Variable	1.03	1.03	1.03	1.03	1.03	1.03
Observations	28,980	28,980	28,980	28,980	28,980	28,980
Adjusted R^2	0.794	0.794	0.794	0.794	0.794	0.794

Notes: This table reports fixed-effects estimates of the effect of strike exposure measures on teenage pregnancies. Strike Intensity is computed following equation 2, with five additional alternative measures constructed by including (excluding) observations depending on their school dependency, occupied, and *emblematic* status in 2011. All specifications have the same controls, including a constant, the logarithm of pregnancies of women 25 to 45 years old as a control, the logarithm of the population aged 15-17 enrolled in public schools, municipality poverty rate, per capita government expenditure in education (in logs, per student in public school), total population (in logs), and total female population (in logs). Municipality linear time trends are included by interacting municipality fixed effects with a linear trend in months. The observation unit is municipality-month (with 345 municipalities from January 2007 to December 2013). Robust standard errors are clustered at the municipality level in parentheses. *** p<0.01, ** p<0.05, * p<0.10.

5.4 Heterogeneity of the effects of Strikes on Teenage Pregnancy

In this section, we explore the heterogeneous effects of the strike on teenage pregnancy to discuss plausible mechanisms behind school closures and teenage conceptions. We present OLS results in each case using the exact specification as in column (3) Table 1 as our preferred model.

Timing of events.- We explore whether the effect of strike adherence on teenage conceptions follows a similar pattern as school attendance shown in Figure 1a. This would support the interpretation that the effects are due to sudden school closures represented by the enormous absenteeism rate. To do this, we go back to figure Figure 5 and observe no changes in conceptions during the first months of the strike, which coincides with a period of high or regular attendance. However, the coefficients increase to 0.21 - 0.51 in June and July, corresponding to a 3% - 8% in teenage pregnancies for a municipality exposed to an average adherence of strike intensity. These are the months when schools experience the lowest attendance rates. The large effect decreased slightly but remained high from August through September of 2011, when there was still a large absenteeism rate. Finally, as the strike fades away in the last months of the year, changes in birth conceptions are unrelated to the municipality's strike adherence.

One concern with this result is that teenage pregnancy is seasonal (e.g., Buckles and Hungerman, 2013). July is a period of holidays so that this effect might be capturing the impact of school closures due to the holiday season rather than school closures due to strikes. However, identifying the effect comes from deviations of conceptions every July in previous and subsequent years since we control for month-fixed effects. Unless July of 2011 was an unusual holiday season - other than coinciding with the strike period - the effect does not confound a seasonality effect.

To explore seasonality in teenage and adult conceptions, we run a regression model of the logarithm of per-day pregnancies in a month on a dummy for each calendar month using January as a base group. We consider all conceptions from 2007 to 2010 and include fixed effects for year and municipality in the estimation. We plot the coefficients associated with each month for teenage conceptions in the left panel of Figure A.4. The results suggest that teenage conceptions are substantially less frequent during the school year, from March to early December. This pattern is very different from the one associated with conceptions of older women, where the school year is not expected to play a relevant role (see right panel of Figure A.4).

Risky behavior proxies.– Next, we look at whether the effects are driven by first-pregnancy high-school-age females rather than pregnancies of high-school-age females with already more than one child. If new conceptions are from first-time mothers, then it is more likely that

Table 3: Effect of Strike Exposure on Other Outcomes

	Teenage Pregnancies			Teenage Couples
	(1) Order: 1	(2) Order: 2+	(3) Age: 18-19	(4)
Strike Intensity	0.111***	-0.012	0.001	0.045*
	(0.034)	(0.021)	(0.033)	(0.025)
Observations	28,980	28,980	28,980	28,980
Adjusted R^2	0.787	0.367	0.827	0.618

Notes: This table reports fixed-effects estimates of the effect of strike exposure measures on different outcomes. Each dependent variable corresponds to the natural logarithm applied to the variable plus one. All specifications have the same controls, including a constant, the logarithm of pregnancies of women 25 to 45 years old as a control, the logarithm of the population aged 15-17 enrolled in public schools, municipality poverty rate, per capita government expenditure in education (in logs, per student in public school), total population (in logs), and total female population (in logs). Municipality linear time trends are included by interacting municipality fixed effects with a linear trend in months. The observation unit is municipality-month (with 345 municipalities from January 2007 to December 2013). Robust standard errors are clustered at the municipality level in parentheses. *** p<0.01, ** p<0.05, * p<0.10.

these are a consequence of risky behavior as teenage mothers who have a second pregnancy are more likely to have planned it (e.g., Raneri and Wiemann, 2007, Meade and Ickovics, 2005). Column (2) in Table 3 shows that the effects of strike adherence on teenage pregnancy rate, in the fully controlled regression, are driven entirely by new mothers.

In addition, we investigated whether municipalities with higher strike exposure also experienced increased condom demand. We use the number of people aged 14 to 19 who enrolled in counseling on condom use and condom disbursement through the *Fertility Regulation Program* as a proxy to measure the extent of the demand for condom access.[33] Table B.5 presents the results restricting the sample to the most populated ones to limit the noise associated with data collection, using the same specification as Equation 3 and the median of the municipality population size as the threshold for inclusion in the final sample.[34] Column (1) uses the main teenage pregnancy variable, replicating remarkably similar results to Column (4) in Table

[33]Refer to Appendix B.3 for precise definitions and data constructions.

[34]An important caveat with this data is that the monthly report seems unreliable due to abrupt non-periodical variations. This problem seems particularly relevant in small municipalities, although some urban municipalities also seem to report erratically. When this data is aggregated at the yearly level, it presents less erratic behavior in the more populated municipalities.

4. Columns (2) and (3) change the dependent variable to the number of enrollments into the program for accessing condoms, separately for teenage and adult populations, showing a positive association between strike intensity and adherence to the program with condoms as a contraceptive method, with the magnitude of the effect being much more prominent for the teenage population. We do not find any statistical association between our treatment and the demand for other methods of contraception by the teenage population. Lastly, Figure B.8 shows the dynamics of the association between strike intensity and enrollment into the program seeking access to condoms as a contraceptive method for teenagers. Albeit estimates are noisy, as one should expect when the dependent variable is measured, it displays an apparent increase in enrollment once the strike is onset that prevails in time.

Placebo effects using other age groups.– To address concerns related to strike variation capturing overall fertility behavior, we reexamine our regression analysis using conceptions among women aged 18 to 19 who are likely out of school and, therefore, not directly affected by the cross-sectional variation in strike adherence among schools we study. The results in column (3) show no significant association between strike adherence and pregnancy rates for this age group. The fact that we observe precise null effects on females at ages 18-19 suggests that the effects are driven by a high school-specific phenomenon identified by the cross-sectional variation in our measure of school strike adherence. Results in Figure 4 show this is also the case for any other age group of women.

To delve deeper into this analysis, we also examine whether a differential change in pregnancies is observed during the strike period for women aged 18 to 24 years old (which represents the most prevalent age group for university students in Chile) in municipalities with different proportions of female students attending Higher Education institutions (which we interpret as a proxy for strike exposure for this age group). We employ the same specification as shown in Equation 3. The results presented in Table B.6 indicate that the association between attending higher education and pregnancies among women aged 18 to 24 is minimal and not statistically significant. A critical consideration of this analysis is that Higher Education, unlike high school, is not compulsory. Furthermore, the dependent variable encompasses all females in this age group, and we cannot condition the dependent variable solely on the characteristics of women attending a higher education institution.

Same age partner.– Moreover, the data includes the father's age if a man recognizes the newborn as his child. We form teenage couples with this information if the mother and father are 15 to 17 years old. If unexpected changes in adult supervision create the opportunity to engage in riskier behavior, this should impact all teenage students. Given the setting, one would expect changes in conceptions to be driven by teenage couples rather than couples formed by teenage girls and older males (e.g., out-of-school boys). The results in column (4) in Table 3 show that the pattern of teenage couples is similar to that found in teenage pregnancies.

Social norms.- Previous studies have found that teenagers' risky behavior is sensitive to peer effects and social norms (e.g., Bandiera et al., 2020; Coyle et al., 2004 and Dupas et al., 2018). To test for social norms, we explore whether the effect of school closures on teenage conceptions is larger in municipalities with higher teenage pregnancy rates at baseline years. We divide the municipalities into two groups: those above the national median and those below the national median of teenage pregnancies in 2010. Results are shown in Table 4 in Columns (1) and (2). The estimates indicate no differences between these municipalities, suggesting that social norms proxy by teenage pregnancy rates before strikes are not associated with post-strike effects on teenage pregnancy rates.

Partner search costs.- During strikes, students tend to spend more time at home rather than attending school, and some of them may be unsupervised by adults. To explore how the effects of strikes may vary based on the likelihood of students finding peers or sexual partners, we examine the relationship between strike effects and municipality population size. We also investigate potential differential effects for students attending co-educational

Table 4: Effect of Strike Exposure on Teenage Pregnancy: Heterogeneity Analysis

	Dependent Variable: Teenage Pregnancies (births to women aged 15-17), in logs							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Strike Intensity	0.101*	0.111**	0.098*	0.086*	0.092*	0.110**	0.094**	0.083
	(0.053)	(0.046)	(0.051)	(0.046)	(0.047)	(0.048)	(0.039)	(0.059)
Population	Baseline Teenage Pregnancies		Baseline Population Size		Share of COED Students		University Campus	
	Below Median	Above Median	Below Median	Above Median	Below Median	Above Median	Outside	Within
Mean of Depedent Variable	1.05	1.14	0.41	1.65	1.30	0.76	0.77	2.04
Observations	13,608	13,608	14,448	14,532	14,448	14,532	22,932	6,048
Adjusted R^2	0.793	0.769	0.263	0.750	0.837	0.658	0.655	0.834

Notes: This table reports fixed-effects estimates of the effect of strike exposure measures on different outcomes. Each dependent variable corresponds to the natural logarithm applied to the variable plus one. All specifications have the same controls, including a constant, the logarithm of pregnancies of women 25 to 45 years old as a control, the logarithm of the population aged 15-17 enrolled in public schools, municipality poverty rate, per capita government expenditure in education (in logs, per student in public school), total population (in logs), and total female population (in logs). Municipality linear time trends are included by interacting municipality fixed effects with a linear trend in months. The observation unit is municipality-month (with 345 municipalities from January 2007 to December 2013). Robust standard errors are clustered at the municipality level in parentheses. *** p<0.01, ** p<0.05, * p<0.10.

versus single-sex schools.

Regarding population size, we analyze whether the effects of strikes differ in municipalities above and below the median population size. Columns (3) and (4) of our results indicate no clear pattern of effects observed across municipalities of different population sizes. This suggests that the impact of strikes does not show a consistent relationship with the municipality's population size.

Additionally, we consider the possibility that attending co-educational schools may lower the search costs for finding a sexual partner, potentially affecting the likelihood of engaging in (unprotected) sex. To examine this hypothesis, we create a variable indicating the percentage of students in a municipality attending co-educational schools. We then test for differential effects of strikes by comparing municipalities above and below the median proportion of students in co-educational schools. The results in Columns (5) and (6) reveal that the estimated effects are similar across municipalities, with different proportions of students attending co-educational schools.

We also examine the differential effects of the strike movement in municipalities with a college campus within their territory compared to those without a college campus. Since the strike movement also involved college students, it is plausible that teenage girls' partners were students from nearby college campuses. This potential association raises the possibility of increased teenage conceptions in municipalities with a college campus. However, it is crucial to consider that college students generally exhibit less risky behaviors and have a higher likelihood of using contraception methods. This aspect introduces uncertainty regarding the direction of the estimated effects.

Our findings in columns (7) and (8) indicate that the point estimates of the effects are similar across municipalities with and without a college campus. However, these estimates are not statistically significant for municipalities with a college campus due to a significant drop in the sample size, exceeding 80%. The reduced sample size in municipalities with a college campus limits the statistical power to detect significant effects. Despite this constraint, the comparable point estimates suggest that the presence or absence of a college campus does not significantly alter the overall findings of our analysis. [35]

5.5 Effects on birth outcomes

Teenage pregnancies have been related to adverse birth outcomes (e.g., Conde-Agudelo et al., 2005; Donoso et al., 2014; Smith and Pell, 2001). In this section, we investigate the effects of strikes on teenage birth outcomes, considering that teenage pregnancies generally carry higher risks and are associated with poorer health outcomes at birth, such as lower birth weight and shorter gestation periods. Our analysis uses data on birth outcomes from birth records, allowing us to examine five specific birth outcomes for teenage births: gestation at birth, the rate of premature births, fetal deaths at birth, birth weight, and the rate of infants born with low birth weight (below 2,500 grams).[36]

[35] Tables B.7, B.8, and B.9 in the online appendix show results from Table 4 for different specifications of the dependent variable.

[36] For fetal deaths, we interpret the sign with caution since abortion was not legal at that time in Chile, which drives a high sample selection problem when studying death at birth, particularly in the teenage population.

Table 5: Effect of Strike Exposure on Teenage Pregnancy: Birth Characteristics

	Gestation at Birth (1)	Premature Births (2)	Fetal Death (3)	Average Weight at Birth (4)	Low Birth Weight (5)
Strike Intensity	-0.006*	0.037	-0.007	-0.016	0.040
	(0.003)	(0.022)	(0.009)	(0.012)	(0.024)
Mean of Dependent Variable	3.68	0.16	0.02	8.08	0.22
Observations	19,905	28,980	28,980	19,905	28,980
Adjusted R^2	0.028	0.398	0.068	0.028	0.464

Notes: This table reports fixed-effects estimates of the effect of strike exposure measures on different outcomes. Each dependent variable corresponds to the natural logarithm applied to the variable plus one. All specifications have the same controls, including a constant, the logarithm of pregnancies of women 25 to 45 years old as a control, the logarithm of the population aged 15-17 enrolled in public schools, municipality poverty rate, per capita government expenditure in education (in logs, per student in public school), total population (in logs), and total female population (in logs). Municipality linear time trends are included by interacting municipality fixed effects with a linear trend in months. The observation unit is municipality-month (with 345 municipalities from January 2007 to December 2013). Robust standard errors are clustered at the municipality level in parentheses. *** p<0.01, ** p<0.05, * p<0.10.

To assess the impact of strikes on these birth outcomes, we employ the same analytical specification as in column (3) of Table 1. This approach enables us to evaluate whether the additional births occurring during strike periods may be less desired or associated with increased birth risks compared to average teenage pregnancies. If, at the margin, these additional births are less wanted or related to more risk at birth than average teenage pregnancies, we would observe that the association of strike intensity and birth outcomes indicates that in municipalities with higher strike adherence, teenage pregnancy outcomes arose on average.

The findings presented in Table 5 indicate that the observed effects of strikes on teenage birth outcomes suggest marginal births are associated with increased risk, as evidenced by lower gestational age and birth weight. However, it is essential to note that these effects are small in magnitude and do not reach statistical significance. This suggests that the additional teenage pregnancies occurring during strikes have similar average birth outcomes compared to teenage pregnancies overall. In other words, strikes do not appear to significantly impact the health outcomes of teenage births beyond what is typically observed in teenage pregnancies.

5.6 Effects on dropout and college test take-up

In this section, we focus on analyzing the effects of strikes on school dropout rates and college application behavior. The results of the event study analysis for different strike measures are presented in Tables A.4 and A.5, while Figure 6 presents the results graphically for the effects associated to the primary strike adherence measure over time, consistent with the results of columns (1) and (2).[37] Before the strike, schools that eventually experienced strikes were similar to non-striking schools regarding dropout rates and college admission test take-up. However, a significant increase in dropout rates and a decrease in college admission test take-up is observed in the year when the strike occurred. The effects are similar if we disaggregate outcomes by gender. In particular, the schools that took up strikes experienced an increase of 0.7 percentage points in their dropout rate. This represents a 20% increase in dropout rates in comparison to the average level of dropouts in the year 2010 (3.4%).

The subsequent analysis uses the logarithm of the number of individuals who took the college admission test in a given year in school. The results show a drop of approximately 20% in the number of students taking the test to be admitted to college during the strike year.[38] Furthermore, our study reveals that it takes approximately two to three years for dropout rates and college admission test take-up to return to pre-strike levels. This indicates a gradual recovery process after the disruption caused by the strike, as the educational system and student engagement stabilize over time.

Taken together, our results show that the impact of school closures on teenage pregnancy is not driven by human capital, as we use variation in the timing every month. Nevertheless,

[37] All in all, the results are robust to how we measure strike exposure, be it through the webs-scrapping measure, the days of attendance measure or the main measure combining both. Refer to columns (3) to (6) in Tables A.4 and A.5 for estimate comparisons depending on the strike measure.

[38] The reason for employing the count of students who take the test, rather than using the rate of students, is due to the ambiguity of the choice of denominator. It is unclear whether we should consider the total student population or only those who graduate, with the latter being endogenous to the treatment. Consequently, opting for the count of test-taking students offers a more straightforward and unbiased approach to our analysis.

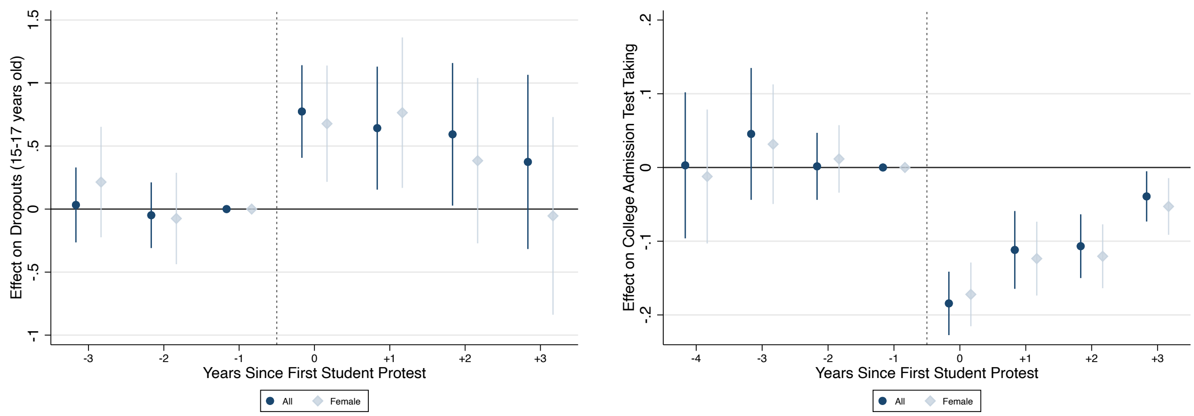
we do observe a decline in human capital in the longer term when analyzing school dropouts during the strike year and after, consistent with them having a more prominent disruptive role.

These findings jointly provide valuable short-term and medium-term insights into the consequences of strikes on students' educational pathways beyond their effect on teenage pregnancy rates. However, the relationship between these effects is beyond the scope of this paper due to data limitations. In particular, we would need microdata of the educational outcomes of students linked to birth records at the individual level, which is not available to researchers. In addition, it is essential to point out that these longer-run results are based only on annual data, which removes most of the within-strike year variance across municipalities, imposing a stronger requirement on the parallel trend assumption.

Furthermore, the fact that there are no differences in the effects on school outcomes by gender suggests that teenage pregnancy is not driving school-level outcomes. If so, we would have expected the impact on school-level outcomes to be larger (in absolute terms) for girls. It may be the case that teenage pregnancy remains a rare event and thus explains only a small portion of dropout rates or college admission uptake. In fact, teenage pregnancy is infrequent in Chile, with approximately 0.48 per 1000 inhabitants in 2019.

Figure 6: Dropouts and College Admission Test Taking at the School Level

(a) Dropouts (b) College Admission Test Takers



Notes: These figures are the graphical representation of the results in Columns (1) and (2) of Tables A.4 and A.5, respectively. Each subfigure presents the coefficients and 95% confidence intervals for years represented as dummy variables, interacted with a school-level strike adherence dummy, covering seven years before and after the year of the student protest (i.e., 2011). The coefficient for the year before the strike (i.e., 2010) is the reference point normalized to zero. The confidence intervals are calculated based on heteroskedasticity-robust standard errors clustered at the school level. Panel (a) displays the coefficients estimated from a regression analysis of the dropout rate at the school level against the measure of strike intensity. The regression includes school and year fixed effects and school-specific linear trends. Panel (b) presents the same analysis but uses the logarithm of the number of students taking the college admission test as the dependent variable.

6 Conclusion

Different studies have demonstrated that school expansion policies had a positive impact in reducing risky behaviors among teenagers. This effect can be attributed to various factors such as time constraints, increased human capital accumulation, improved sexual education, and changes in expectations regarding risky choices. In this paper, we contribute to this literature by examining how teenage pregnancy rates are affected when schools become suddenly inoperative. We utilize a quasi-experimental variation from a large-scale student strike movement in Chile that lasted for six months. Focusing on the absence of schooling, we can interpret the observed effects as primarily related to reduced time spent under adult supervision.

Our analysis reveals a significant association between school absenteeism during the strike and teenage pregnancy rates. These findings remain robust across various specifications and falsification tests and exhibit a similar magnitude (but opposite sign) to related studies investigating the effects of school policy expansions. Furthermore, the effects align with the seasonal patterns typically observed in December, when teenagers are out of school and more likely to spend unsupervised time. Heterogeneity analyses further support the notion that relaxed adult supervision during the strike period serves as the primary mechanism driving the observed effects. We also show that strikes disrupt the educational trajectory of students, leading to increased dropout rates during strike periods and potentially limiting access to higher education opportunities.

These findings underscore the potential benefits of policy interventions such as sexual education and counseling within schools, as well as initiatives that promote access to contraception among teenagers. Implementing such interventions becomes particularly crucial when schools are facing closures or disruptions. By addressing the issue of reduced adult supervision during strike periods, these interventions can help mitigate the risks associated with teenage pregnancies and promote the well-being of adolescents.

References

Alsan, M. M. and Cutler, D. M. (2013). Girls' education and hiv risk: evidence from uganda. *Journal of health economics*, 32(5):863–872.

Anderson, D. M. (2014). In school and out of trouble? the minimum dropout age and juvenile crime. *Review of Economics and Statistics*, 96(2):318–331.

Azevedo, J. P., Favara, M., Haddock, S. E., López-Calva, L. F., Muller, M., and Perova, E. (2012). Teenage pregnancy and opportunities in latin america and the caribbean: on teenage fertility decisions, poverty and economic achievement.

Bailey, M. J. (2013). Fifty years of family planning: new evidence on the long-run effects of increasing access to contraception. *Brookings Papers on Economic Activity*, pages 341–409.

Bandiera, O., Buehren, N., Burgess, R., Goldstein, M., Gulesci, S., Rasul, I., and Sulaiman, M. (2020). Women's empowerment in action: Evidence from a randomized control trial in africa. *American Economic Journal: Applied Economics*, 12(1):210–59.

Bellei, C. and Cabalin, C. (2013). Chilean student movements: Sustained struggle to transform a market-oriented educational system. *Current Issues in Comparative Education*, 15(2):108–123.

Berthelon, M. E. and Kruger, D. I. (2011). Risky behavior among youth: Incapacitation effects of school on adolescent motherhood and crime in chile. *Journal of public economics*, 95(1-2):41–53.

Black, D. A., Berger, M. C., and Scott, F. A. (2000). Bounding parameter estimates with non-classical measurement error. *Journal of the American Statistical Association*, 95(451):739–748.

Black, S. E., Devereux, P. J., and Salvanes, K. G. (2008). Staying in the classroom and out of the maternity ward? the effect of compulsory schooling laws on teenage births. *The Economic Journal*, 118(530):1025–1054.

Bollinger, C. R. and David, M. H. (1997). Modeling discrete choice with response error: Food stamp participation. *Journal of the American Statistical Association*, 92(439):827–835.

Bound, J., Brown, C., and Mathiowetz, N. (2001). Measurement error in survey data. In *Handbook of econometrics*, volume 5, pages 3705–3843. Elsevier.

Buckles, K. S. and Hungerman, D. M. (2013). Season of birth and later outcomes: Old questions, new answers. *Review of Economics and Statistics*, 95(3):711–724.

Callaway, B., Goodman-Bacon, A., and Sant'Anna, P. H. C. (2021). Difference-in-differences with a continuous treatment.

Chevalier, A. and Viitanen, T. K. (2003). The long-run labour market consequences of teenage motherhood in britain. *Journal of population economics*, 16(2):323–343.

Conde-Agudelo, A., Belizán, J. M., and Lammers, C. (2005). Maternal-perinatal morbidity and mortality associated with adolescent pregnancy in latin america: Cross-sectional study. *American journal of obstetrics and gynecology*, 192(2):342–349.

Coyle, K. K., Kirby, D. B., Marín, B. V., Gómez, C. A., and Gregorich, S. E. (2004). Draw the line/respect the line: A randomized trial of a middle school intervention to reduce sexual risk behaviors. *American Journal of Public Health*, 94(5):843–851.

Dahl, G. B. (2010). Early teen marriage and future poverty. *Demography*, 47(3):689–718.

de Estudios MINEDUC, C. (2020). Liceos emblemáticos y bicentenario 2019. *Apuntes-I*.

Diaz, C. J. and Fiel, J. E. (2016). The effect (s) of teen pregnancy: Reconciling theory, methods, and findings. *Demography*, 53(1):85–116.

Donoso, E., Carvajal, J. A., Vera, C., and Poblete, J. A. (2014). La edad de la mujer como factor de riesgo de mortalidad materna, fetal, neonatal e infantil. *Revista médica de Chile*, 142(2):168–174.

Donoso, S., Grau, N., and Hojman, D. (2016). Unpacking the diffusion of protest tactics: the onset, duration, and ending of school sit-ins. *Working Paper*.

Duflo, E., Dupas, P., and Kremer, M. (2015). Education, hiv, and early fertility: Experimental evidence from kenya. *American Economic Review*, 105(9):2757–97.

Dupas, P., Huillery, E., and Seban, J. (2018). Risk information, risk salience, and adolescent sexual behavior: Experimental evidence from cameroon. *Journal of Economic Behavior & Organization*, 145:151–175.

Evans, W. N., Oates, W. E., and Schwab, R. M. (1992). Measuring peer group effects: A study of teenage behavior. *Journal of Political Economy*, 100(5):966–991.

Fletcher, J. M. (2012). The effects of teenage childbearing on the short-and long-term health behaviors of mothers. *Journal of Population Economics*, 25(1):201–218.

Fletcher, J. M. and Wolfe, B. L. (2009). Education and labor market consequences of teenage childbearing evidence using the timing of pregnancy outcomes and community fixed effects. *Journal of Human Resources*, 44(2):303–325.

Francesconi, M. (2008). Adult outcomes for children of teenage mothers. *Scandinavian Journal of Economics*, 110(1):93–117.

Gaete, G. (2018). Follow the leader: Student strikes, school absenteeism and persistent consequences on educational outcomes. *School Absenteeism and Persistent Consequences on Educational Outcomes (April 2018)*.

Gaviria, A. and Raphael, S. (2001). School-based peer effects and juvenile behavior. *Review of Economics and Statistics*, 83(2):257–268.

González, F. (2020). Collective action in networks: Evidence from the chilean student movement. *Journal of Public Economics*, 188:104220.

Griffin, K. W., Botvin, G. J., and Nichols, T. R. (2004). Long-term follow-up effects of a school-based drug abuse prevention program on adolescent risky driving. *Prevention Science*, 5(3):207–212.

Jacob, B. A. and Lefgren, L. (2003). Are idle hands the devil's workshop? incapacitation, concentration, and juvenile crime. *American Economic Review*, 93(5):1560–1577.

Kearney, M. S. and Levine, P. B. (2012). Why is the teen birth rate in the united states so high and why does it matter? *Journal of Economic Perspectives*, 26(2):141–63.

Kearney, M. S. and Levine, P. B. (2014). Income inequality, social mobility, and the decision to drop out of high school. Technical report, National Bureau of Economic Research.

Kearney, M. S. and Levine, P. B. (2015). Media influences on social outcomes: The impact of mtv's 16 and pregnant on teen childbearing. *American Economic Review*, 105(12):3597–3632.

Levine, D. I. and Painter, G. (2003). The schooling costs of teenage out-of-wedlock childbearing: Analysis with a within-school propensity-score-matching estimator. *Review of Economics and Statistics*, 85(4):884–900.

Marcotte, D. E. (2013). High school dropout and teen childbearing. *Economics of Education Review*, 34:258–268.

McCrary, J. and Royer, H. (2011). The effect of female education on fertility and infant health: Evidence from school entry policies using exact date of birth. *American Economic Review*, 101(1):158–95.

Meade, C. S. and Ickovics, J. R. (2005). Systematic review of sexual risk among pregnant and mothering teens in the usa: pregnancy as an opportunity for integrated prevention of std and repeat pregnancy. *Social Science & Medicine*, 60(4):661–678.

Ní Bhrolcháin, M. and Beaujouan, É. (2012). Fertility postponement is largely due to rising educational enrolment. *Population studies*, 66(3):311–327.

OECD (2022). Family data base: Age of mothers at childbirth and age-specific fertility. https://www.oecd.org/els/soc/SF_2_3_Age_mothers_childbirth.pdf. Accessed: 2022-06-30.

Oreopoulos, P. and Salvanes, K. G. (2011). Priceless: The nonpecuniary benefits of schooling. *Journal of Economic perspectives*, 25(1):159–84.

Raneri, L. G. and Wiemann, C. M. (2007). Social ecological predictors of repeat adolescent pregnancy. *Perspectives on sexual and reproductive health*, 39(1):39–47.

Rau, T., Sarzosa, M., and Urzúa, S. (2021). The children of the missed pill. *Journal of Health Economics*, 79:102496.

Smith, G. C. and Pell, J. P. (2001). Teenage pregnancy and risk of adverse perinatal outcomes associated with first and second births: population based retrospective cohort study. *Bmj*, 323(7311):476.

Tan, P. L. (2017). The impact of school entry laws on female education and teenage fertility. *Journal of Population Economics*, 30(2):503–536.

A Appendix

A.1 Additional tables

Table A.1: Effect of Strike Exposure on Teenage Pregnancy: Strike defined by Web Scrapping

Dependent Variable: Teenage Pregnancies (births to women aged 15-17)				
	in logs	IHS transf.	Rates	Counts
	(1)	(2)	(3)	(4)
Strike Intensity (Web)	0.098***	0.113**	0.337*	0.090**
	(0.037)	(0.048)	(0.171)	(0.045)
Observations	28,980	28,980	28,980	28,560
Adjusted R^2 /Pseudo R^2	0.794	0.780	0.191	0.641

Notes: This table reports estimates of the effect of strike exposure using different ways to measure the outcome of interest, teenage pregnancies. Columns (1) to (3) present the results for an estimation of an OLS fixed-effects model, varying the definition of the dependent variable, while Column (4) shows the results of a Poisson regression model. Column (1) uses the logarithm of the number of births to women aged 15-17 plus one as the dependent variable. Column (2) uses an inverse hyperbolic sine transformation of the number of births to women aged 15-17. Column (3) uses the rate of teenage pregnancies, defined as the number of births to women aged 15-17 over the number of public school female students aged 15-17, including weights for the number of public students aged 14-17 in the municipality. Column (4), the Poisson model, directly uses the number of births to women aged 15-17. All specifications have the same controls: a constant, the logarithm of pregnancies of women 25 to 45 years old as a control, the logarithm of the population aged 14-17 enrolled in public schools, municipality poverty rate, per capita government expenditure in education (in logs, per student in public school), total population (in logs), and total female population (in logs). Municipality linear time trends are included by interacting municipality fixed effects with a linear trend in months. The observation unit is municipality-month (with 345 municipalities from January 2007 to December 2013). Column (4) has fewer observations as five municipalities have zero births to women aged 15-17 each month and are therefore excluded in the Poisson model computation. Strike Intensity is computed following Equation 2, where *School on strike* is defined according to only the web scrapping strike measure. Robust standard errors are clustered at the municipality level in parentheses. *** p<0.01, ** p<0.05, * p<0.10.

Table A.2: Effect of Strike Exposure on Teenage Pregnancy: Strike defined by Attendance (10 lost days)

Dependent Variable: Teenage Pregnancies (births to women aged 15-17)				
	in logs	IHS transf.	Rates	Counts
	(1)	(2)	(3)	(4)
Strike Intensity (Att)	0.113***	0.139***	0.136	0.040
	(0.038)	(0.049)	(0.171)	(0.045)
Observations	28,980	28,980	28,980	28,560
Adjusted R^2 /Pseudo R^2	0.794	0.780	0.191	0.641

Notes: This table reports estimates of the effect of strike exposure using different ways to measure the outcome of interest, teenage pregnancies. Columns (1) to (3) present the results for an estimation of an OLS fixed-effects model, varying the definition of the dependent variable, while Column (4) shows the results of a Poisson regression model. Column (1) uses the logarithm of the number of births to women aged 15-17 plus one as the dependent variable. Column (2) uses an inverse hyperbolic sine transformation of the number of births to women aged 15-17. Column (3) uses the rate of teenage pregnancies, defined as the number of births to women aged 15-17 over the number of public school female students aged 15-17, including weights for the number of public students aged 14-17 in the municipality. Column (4), the Poisson model, directly uses the number of births to women aged 15-17. All specifications have the same controls: a constant, the logarithm of pregnancies of women 25 to 45 years old as a control, the logarithm of the population aged 14-17 enrolled in public schools, municipality poverty rate, per capita government expenditure in education (in logs, per student in public school), total population (in logs), and total female population (in logs). Municipality linear time trends are included by interacting municipality fixed effects with a linear trend in months. The observation unit is municipality-month (with 345 municipalities from January 2007 to December 2013). Column (4) has fewer observations as five municipalities have zero births to women aged 15-17 each month and are therefore excluded in the Poisson model computation. Strike Intensity is computed following Equation 2, where *School on strike* is one if students in that school lost more than ten days on average during August. Robust standard errors are clustered at the municipality level in parentheses. *** p<0.01, ** p<0.05, * p<0.10.

Table A.3: Effect of Male Strike Exposure on Teenage Pregnancy

Dependent Variable: Teenage Pregnancies (births to women aged 15-17)	in logs			IHS transf.	Rates	Counts
	(1)	(2)	(3)	(4)	(5)	(6)
Strike Intensity Male (Web-Scrapped)	0.091**					
	(0.036)					
Strike Intensity Male (Attendance)		0.093**				
		(0.038)				
Strike Intensity Male (Main)			0.099***	0.118***	0.338**	0.094**
			(0.034)	(0.044)	(0.166)	(0.044)
Mean of Dependent Variable	1.03	1.03	1.03	1.31	3.72	3.73
Observations	28,980	28,980	28,980	28,980	28,980	28,560
Adjusted R^2 / Pseudo R^2	0.794	0.794	0.794	0.780	0.191	0.641

Notes: This table reports estimates of the effect of strike exposure using different ways to measure the outcome of interest, teenage pregnancies, and three alternative measures of Strike Intensity Males as defined in Equation (2'). Columns (1) to (5) present the results for an estimation of an OLS fixed-effects model, varying the definition of the dependent variable, while Column (4) shows the results of a Poisson regression model. Columns (1) to (3) use the logarithm of the number of births to women aged 15-17 plus one as the dependent variable. Column (4) uses an inverse hyperbolic sine transformation of the number of births to women aged 15-17. Column (5) uses the rate of teenage pregnancies, defined as the number of births to women aged 15-17 over the number of public school female students aged 15-17, including weights for the number of public students aged 15-17 in the municipality. Column (6), the Poisson model, directly uses the number of births to women aged 15-17. All specifications have the same controls: a constant, the logarithm of pregnancies of women 25 to 45 years old as a control, the logarithm of the population aged 15-17 enrolled in public schools, municipality poverty rate, per capita government expenditure in education (in logs, per student in public school), total population (in logs), and total female population (in logs). Municipality linear time trends are included by interacting municipality fixed effects with a linear trend in months. The observation unit is municipality-month (with 345 municipalities from January 2007 to December 2013). Column (6) has fewer observations as five municipalities have zero births to women aged 15-17 each month and are therefore excluded in the Poisson model computation. Robust standard errors are clustered at the municipality level in parentheses. *** p<0.01, ** p<0.05, * p<0.10.

Table A.4: Effect of Strike Exposure on Academic Outcomes: Dropout Rates at the School-level

	Strike Intensity (Main)		Strike Intensity (Web-Scrapped)		Strike Intensity (Attendance)	
	(1) Pooled	(2) Female	(3) Pooled	(4) Female	(5) Pooled	(6) Female
Strike x 2008	0.033	0.214	-0.012	0.200	0.156	0.430
	(0.152)	(0.224)	(0.158)	(0.236)	(0.200)	(0.307)
Strike x 2009	-0.049	-0.075	-0.120	-0.137	-0.040	-0.192
	(0.133)	(0.185)	(0.132)	(0.187)	(0.138)	(0.184)
Strike x 2010	-	-	-	-	-	-
	-	-	-	-	-	-
Strike x 2011	0.774***	0.677***	0.797***	0.732***	0.633***	0.500*
	(0.187)	(0.235)	(0.188)	(0.245)	(0.213)	(0.280)
Strike x 2012	0.642***	0.765**	0.707***	0.791**	0.507*	0.742**
	(0.249)	(0.304)	(0.254)	(0.321)	(0.266)	(0.350)
Strike x 2013	0.593**	0.384	0.536*	0.367	0.407	0.120
	(0.288)	(0.334)	(0.291)	(0.347)	(0.308)	(0.367)
Strike x 2014	0.374	-0.054	0.378	-0.020	0.253	-0.130
	(0.352)	(0.400)	(0.356)	(0.412)	(0.379)	(0.457)
Mean of Dependent Variable	3.57	3.57	3.45	3.46	3.72	3.70
Observations	22,004	21,414	21,637	21,057	17,374	17,346
Adjusted R^2	0.739	0.676	0.680	0.608	0.711	0.620

Notes: This table reports the event study estimates proposed in Equation 4, with school-level dropout rates as the dependent variable of interest. Columns (1) and (2) present the results using the union of both strike measures, while columns (3) and (4) use the web-scrapped measure of strike adherence, and columns (5) and (6) use the attendance-based measure. Odd columns use dropout rates pooling students together, while even columns only include female students (and therefore lose some observations by excluding only male schools). School linear time trends are included by interacting school fixed effects with a linear trend in years. The observation unit is school-year. Robust standard errors are clustered at the school level in parentheses. *** p<0.01, ** p<0.05, * p<0.10.

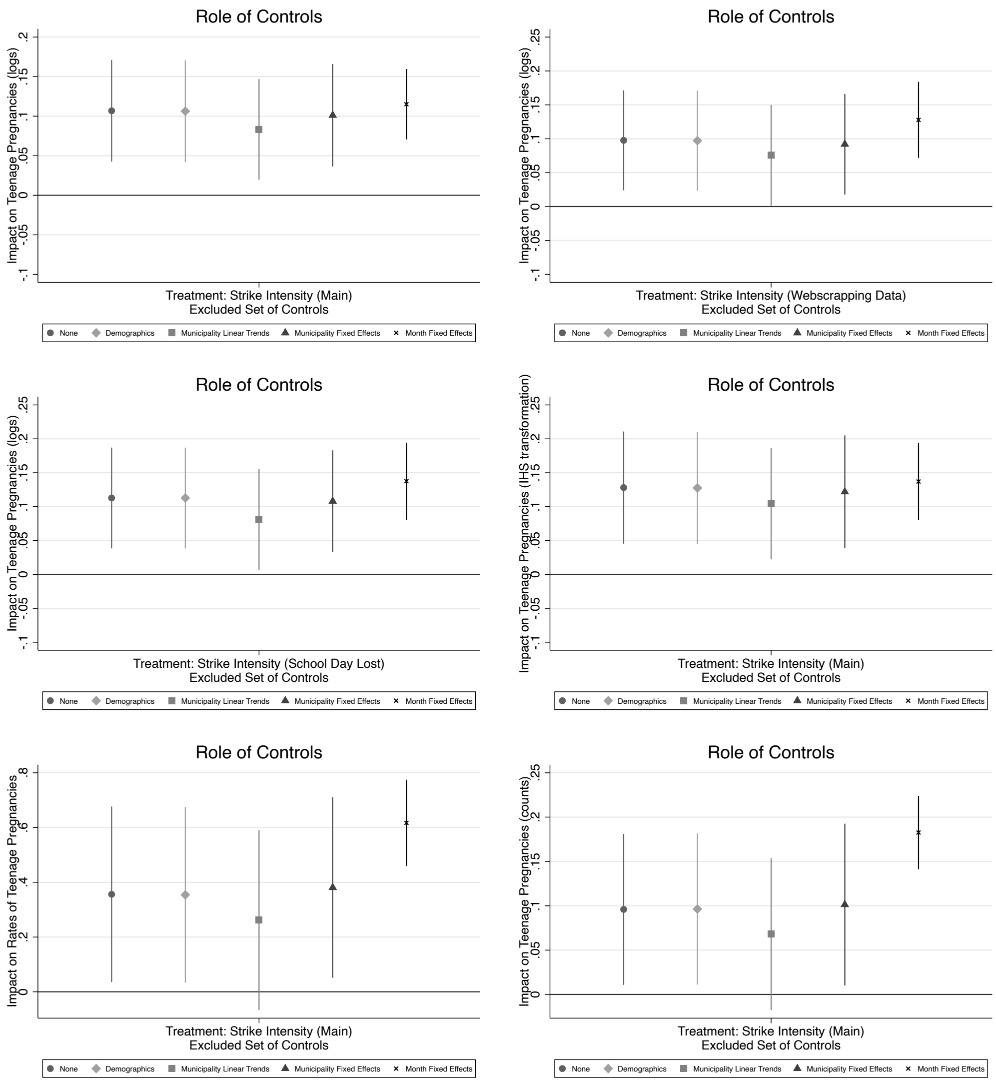
Table A.5: Effect of Strike Exposure on Academic Outcomes: College Admission Take-up Rates at the School-level

	Strike Intensity (Main)		Strike Intensity (Web-Scrapped)		Strike Intensity (Attendance)	
	(1) Pooled	(2) Female	(3) Pooled	(4) Female	(5) Pooled	(6) Female
Strike x 2007	0.003	-0.012	0.054	0.037	0.033	-0.025
	(0.051)	(0.046)	(0.049)	(0.045)	(0.067)	(0.060)
Strike x 2008	0.046	0.032	0.089**	0.070*	0.059	0.040
	(0.046)	(0.041)	(0.043)	(0.040)	(0.061)	(0.054)
Strike x 2009	0.002	0.012	0.002	0.012	0.029	0.023
	(0.023)	(0.023)	(0.023)	(0.023)	(0.027)	(0.028)
Strike x 2010	-	-	-	-	-	-
	-	-	-	-	-	-
Strike x 2011	-0.184***	-0.172***	-0.202***	-0.189***	-0.195***	-0.185***
	(0.022)	(0.022)	(0.022)	(0.022)	(0.024)	(0.026)
Strike x 2012	-0.112***	-0.124***	-0.114***	-0.124***	-0.106***	-0.126***
	(0.027)	(0.026)	(0.028)	(0.026)	(0.031)	(0.031)
Strike x 2013	-0.107***	-0.120***	-0.110***	-0.124***	-0.129***	-0.155***
	(0.022)	(0.022)	(0.023)	(0.023)	(0.024)	(0.027)
Strike x 2014	-0.039**	-0.053***	-0.047***	-0.059***	-0.073***	-0.090***
	(0.017)	(0.020)	(0.017)	(0.020)	(0.018)	(0.023)
Mean of Dependent Variable	3.72	3.03	3.76	3.06	3.82	3.21
Observations	24,981	24,981	24,494	24,494	19,556	19,556
Adjusted R^2	0.891	0.906	0.889	0.905	0.875	0.887

Notes: This table reports the estimates of the event study estimates proposed in Equation 4, with school-level college admission test-taking rates as the dependent variable of interest. Columns (1) and (2) present the results using the union of both strike measures, while columns (3) and (4) use the web-scrapped measure of strike adherence, and columns (5) and (6) use the attendance-based measure. Odd columns use dropout rates pooling students together, while even columns only include female students (and therefore lose some observations by excluding only male schools). School linear time trends are included by interacting school fixed effects with a linear trend in years. The observation unit is school-year. Robust standard errors are clustered at the school level in parentheses. *** p<0.01, ** p<0.05, * p<0.10.

A.2 Additional figures

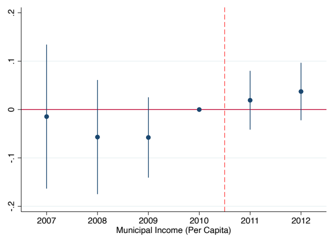
Figure A.1: Exclusion of Controls



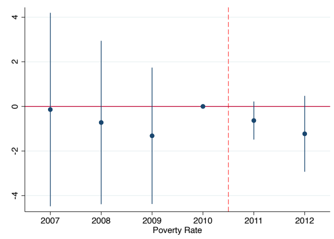
Notes: These figures plot the main point estimate for strike intensity when omitting one set of controls at a time. The figures also vary the type of transformation of the dependent variable and the definition of the main treatment (i.e., either using the definition of strike adherence based on web scrapped data, absenteeism data, or both). All estimations except the one at the bottom right are based on OLS regressions (the exception is based on Poisson regressions).

Figure A.2: Pre-Trends Analysis

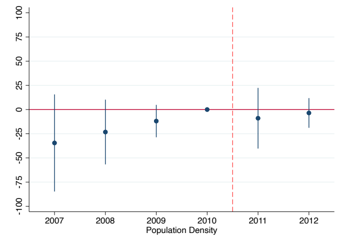
(a) Municipality Income (Per Capita)



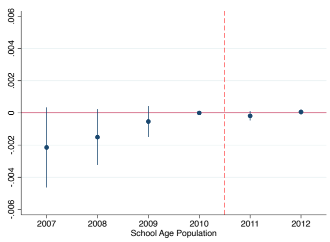
(b) Poverty Rate



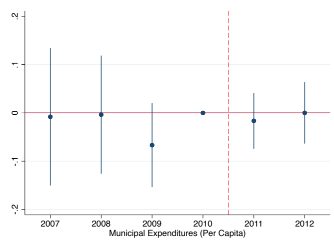
(c) Population Density



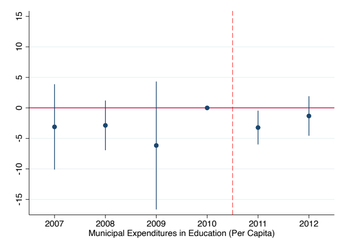
(d) School-aged Population



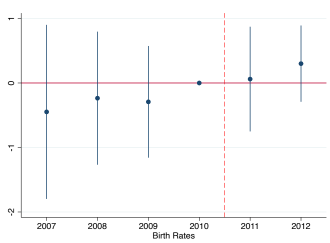
(e) Municipal Expenditures (Per Capita)



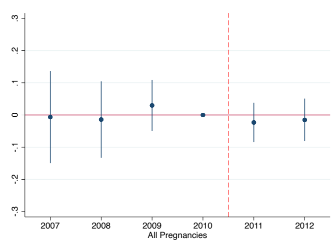
(f) Municipal Expenditures in Education (Per Capita)



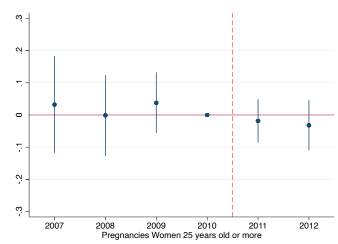
(g) Birth Rates



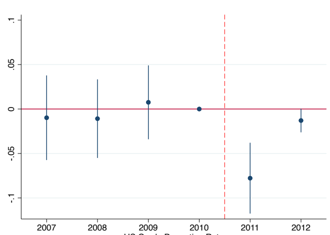
(h) All Pregnancies



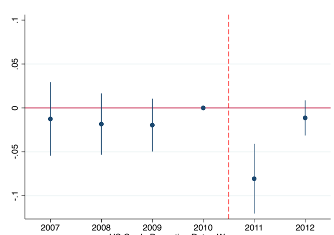
(i) Pregnancies Women 25 years old or more



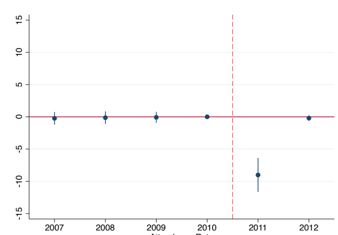
(j) High School Grade Promotion Rate



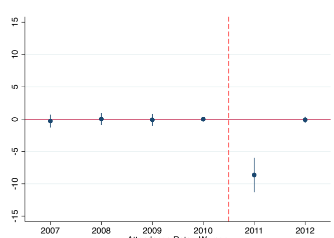
(k) High School Grade Promotion Rate - Women



(l) Attendance Rate

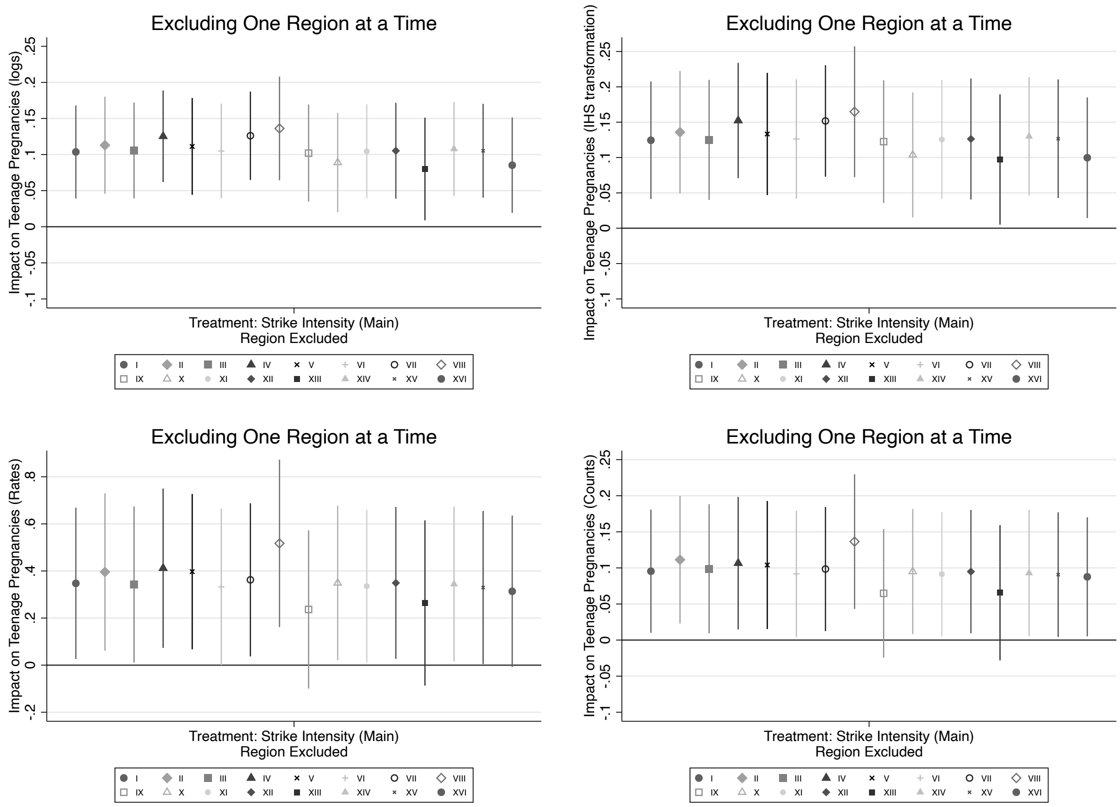


(m) Attendance Rate - Women



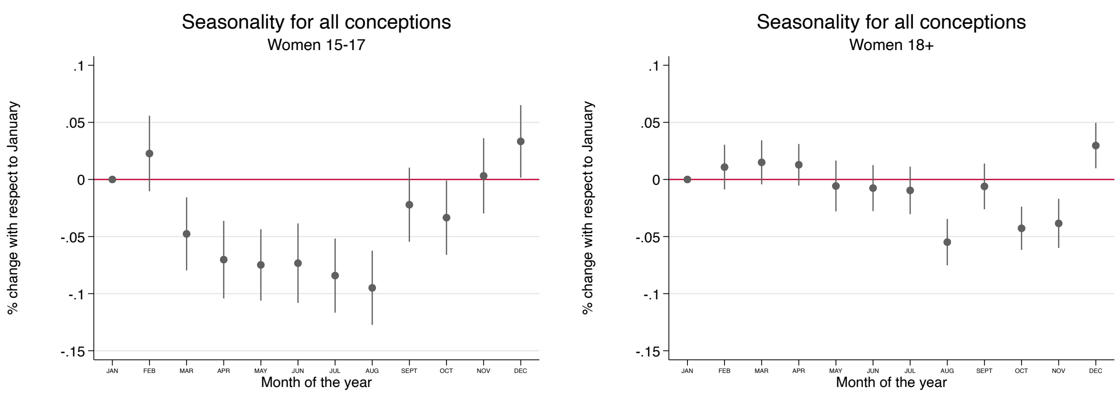
Notes: Each figure presents the coefficients and 95% confidence intervals for years represented as dummy variables, interacted with a municipality-level strike adherence dummy, covering six years before and after the year of the student protest (i.e., 2011). The coefficient for the year before the strike (i.e., 2010) serves as the reference point and is normalized to zero. The confidence intervals are calculated based on heteroskedasticity-robust standard errors clustered at the municipality level. All panels display the coefficients estimated from a regression analysis at the municipality level, with municipality and year fixed effects, along with municipality-specific linear trends.

Figure A.3: Exclusion of Regions



Notes: These figures plot the main point estimate for strike intensity when omitting one Region at a time. Each subfigure plots the main specification for a different dependent variable transformation. All estimations except the one at the bottom right are based on OLS regressions (the exception is based on Poisson regressions).

Figure A.4: Seasonality of conceptions for different age groups (years 2007 - 2010)



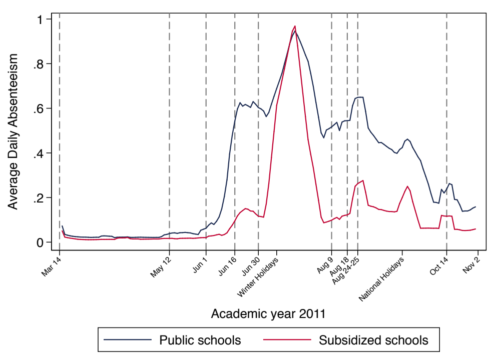
Notes: These figures explore the seasonality in conceptions by separately running a regression of the logarithm of per-day pregnancies for each age group on a set of dummies for each month of the year, with January as a base group pooling all years (we consider monthly conceptions from 2007 to 2010), including fixed effects for year and municipality. Coefficients for each month dummy are plotted in each figure.

B Online Appendix - Not Intended for Publication

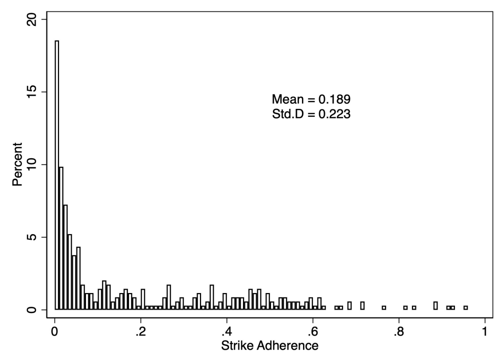
B.1 Figures

Figure B.1: Daily School Absenteeism and Cross-Sectional Variation in Strike Adherence using Different Measures

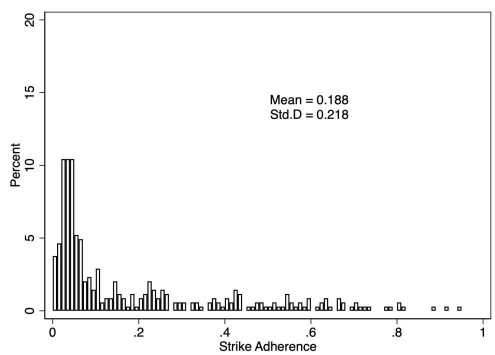
(a) Daily Absenteeism during 2011 by Type of School



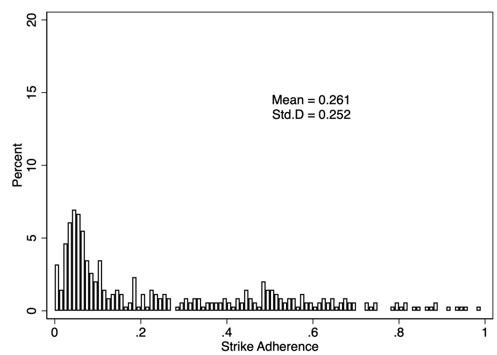
(b) Distribution of Strike Adherence using Web scrapping Data



(c) Distribution of Strike Adherence using Attendance Data



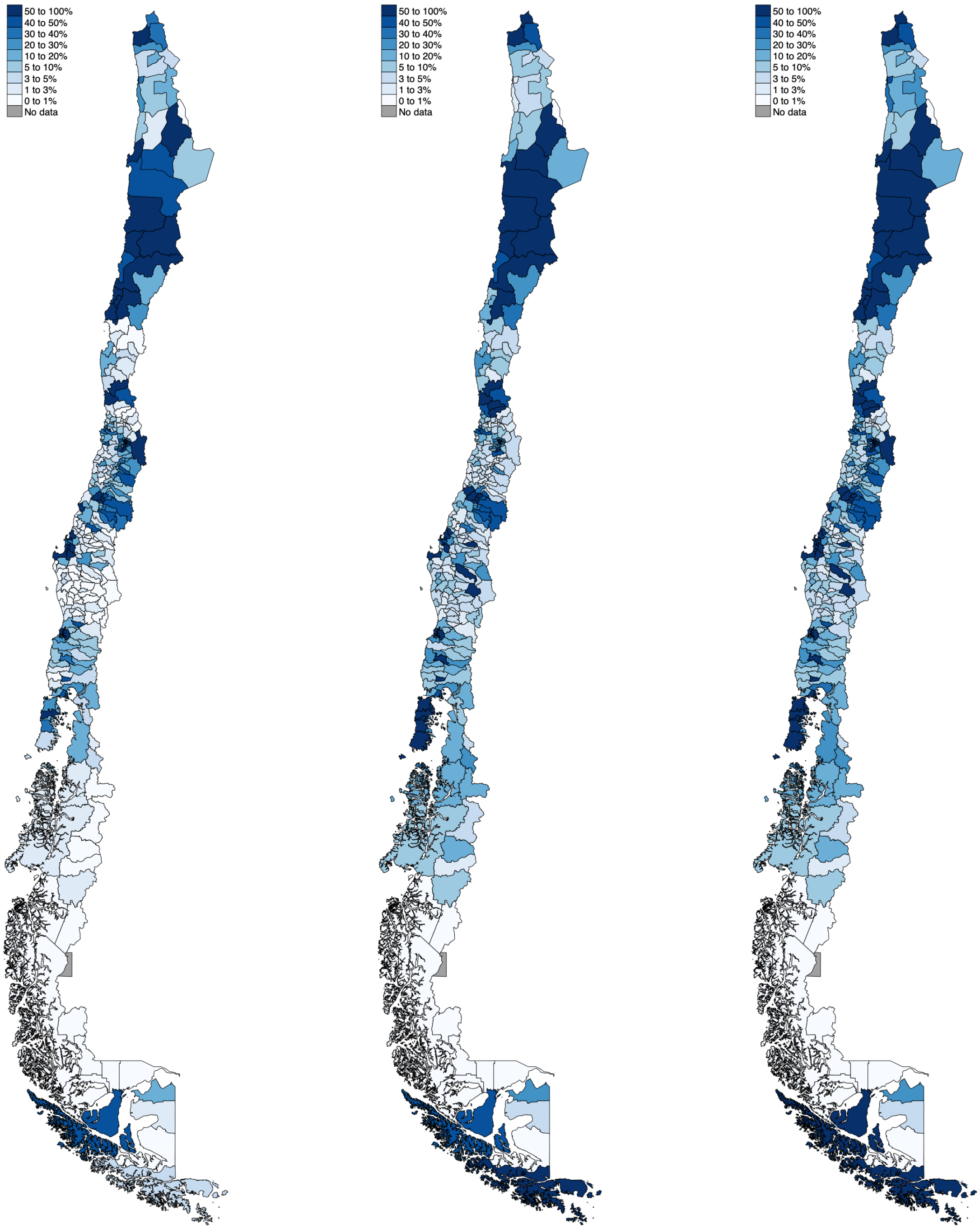
(d) Distribution of Strike Adherence combining both measures



Notes: (a): This figure shows the trends in daily school absenteeism in a moving average of 2 days during 2011 by type of school. The blue line represents public schools, while the red line represents voucher schools. (b), (c) and (d): These figures show the distribution of each municipality according to the variable of strike adherence obtained from measures using web scraping data, attendance data for August, and combining both measures. The figures also show each variable's mean and standard deviation within each graph.

Figure B.2: Mapping Strike Intensity by Measure

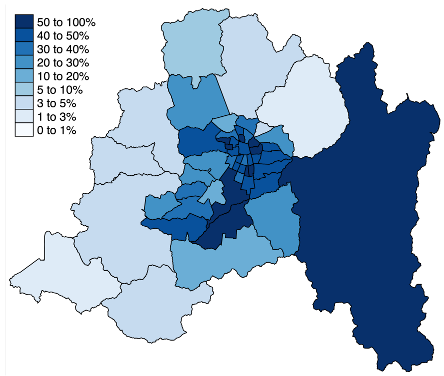
(a) Strike Adherence using Web scrapping Data   (b) Strike Adherence using Attendance Data   (c) Strike Adherence combining Both Measures



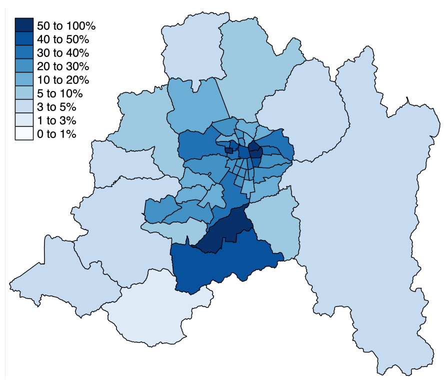
Notes: This figure shows the geographic distribution of strike adherence across Chilean municipalities. Subfigure (a) presents the strike attendance levels according to the variable of strike adherence obtained from measures using web scraping data. In contrast, (b) and (c) use the attendance data for August and both measures combined, respectively.

Figure B.3: Mapping Strike Intensity by Measure - Metropolitan Region

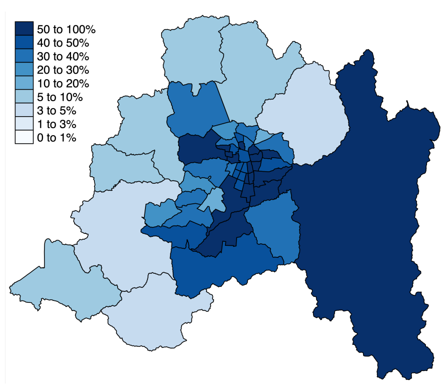
(a) Strike Adherence using Web scrapping Data



(b) Strike Adherence using Attendance Data



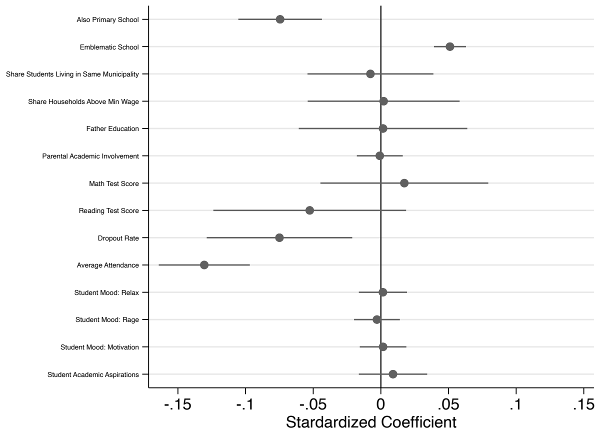
(c) Strike Adherence combining both Measures (Main)



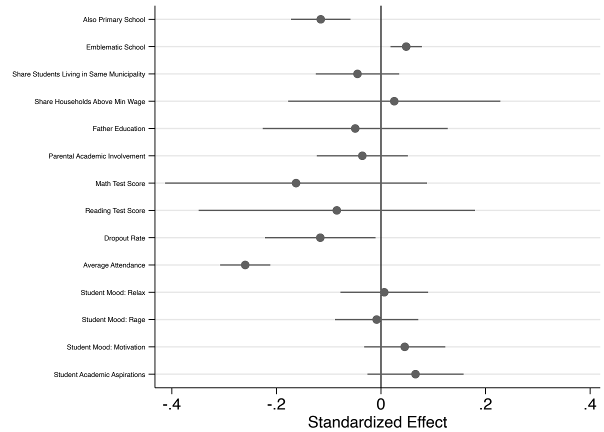
Notes: This figure shows the geographic distribution of strike adherence across municipalities in the Metropolitan Region. Subfigure (a) presents the strike attendance levels according to the variable of strike adherence obtained from measures using web scraping data. In contrast, (b) and (c) use the attendance data for August and both measures combined, respectively.

Figure B.4: Associations between School Level Characteristics and Strike Take-up without Municipality Fixed Effects

(a) Strike take-up among all schools



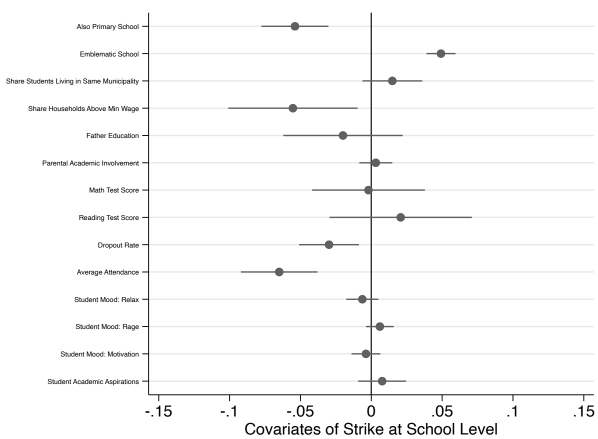
(b) Strike decline among striking schools



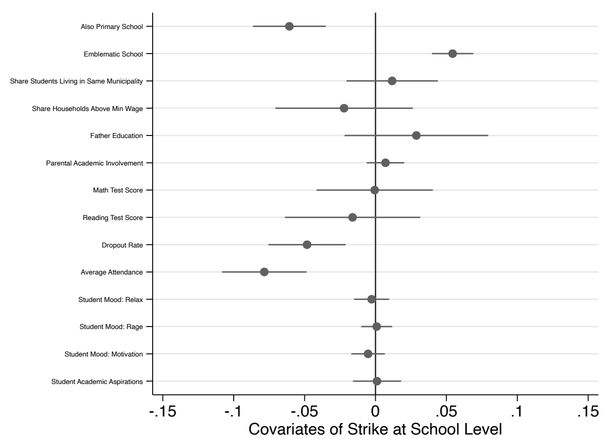
Notes: Each panel in this figure plots the coefficients and the 95% confidence intervals for a set of school-level covariates (listed on the y-axis) for the analysis of (a) the probability of going on strike (among all schools in Chile, N = 2,505) and (b) The probability of recovering pre-strike assistance levels during 2011 among schools that were on strike (N = 455). All covariates are standardized. Both regressions are based on OLS and cluster the standard errors at the municipality level. Unlike Figure 2, the regression does not include municipality fixed effects.

Figure B.5: Associations between School Level Characteristics and School Occupation

(a) With Municipality Fixed Effects



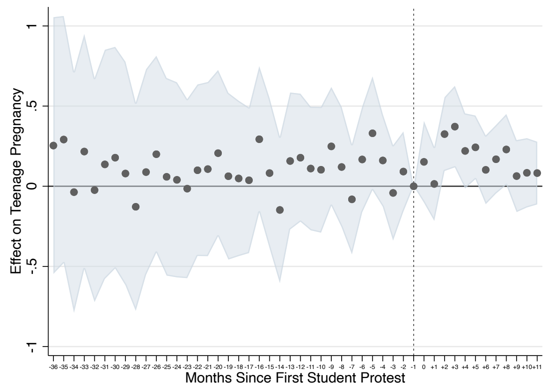
(b) Without Municipality Fixed Effects



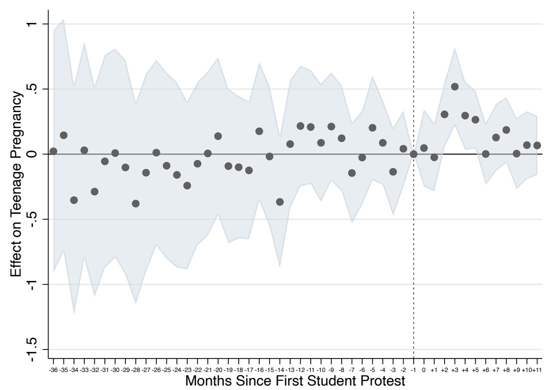
Notes: Each panel in this figure plots the coefficients and the 95% confidence intervals for a set of school-level covariates (listed on the y-axis) for the analysis of the probability of the school being occupied during the strike (among all schools in Chile, N = 2,505). All covariates are standardized. Both regressions are based on OLS and cluster the standard errors at the municipality level. Panel A (B) includes (excludes) municipality fixed effects.

Figure B.6: Event Study (Extended)

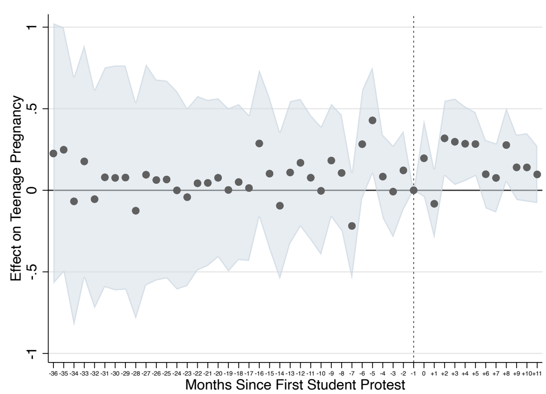
(a) Main



(b) Webscrapping Data



(c) School Days Lost



Notes: Each figure plots the coefficients and the 95% confidence intervals for twenty-three month dummies interacted with strike adherence for 36 months before and 12 months after the start of the student protest (i.e., on May 2011). Panel (a) uses the main strike intensity measure, whereas Panels (b) and (c) use strike intensity based on web-scrapped data and school days lost, respectively. The coefficient for April 2011 is normalized to zero. Confidence intervals are based on heteroskedasticity-robust standard errors clustered at the municipality level. The coefficients are estimated from a unique regression of teenage pregnancies (in logs), which includes municipality and month fixed effects as well as municipality-specific linear trends, the logarithm of pregnancies of women 25 to 45 years old, the logarithm of the teenage population enrolled in public schools, poverty rate, per capita government expenditure in education (in logs, per student in public school), total population (in logs), and total female population (in logs).

Table B.1: Effect of Strike Exposure on Teenage Pregnancy: Strike defined by Attendance (5 lost days)

Dependent Variable: Teenage Pregnancies (births to women aged 15-17)	in logs	IHS transf.	Rates	Counts
	(1)	(2)	(3)	(4)
Strike Intensity (Att5)	0.112***	0.139***	0.290*	0.084**
	(0.030)	(0.038)	(0.156)	(0.042)
Observations	28,980	28,980	28,980	28,560
Adjusted R^2 /Pseudo R^2	0.794	0.780	0.191	0.641

This table reports estimates of the effect of strike exposure using different ways to measure the outcome of interest, teenage pregnancies. Columns (1) to (3) present the results for an estimation of an OLS fixed-effects model, varying the definition of the dependent variable, while Column (4) shows the results of a Poisson regression model. Column (1) uses the logarithm of the number of births to women aged 15-17 plus one as the dependent variable. Column (2) uses an inverse hyperbolic sine transformation of the number of births to women aged 15-17. Column (3) uses the rate of teenage pregnancies, defined as the number of births to women aged 15-17 over the number of public school female students aged 15-17, including weights for the number of public students aged 14-17 in the municipality. Column (4), the Poisson model, directly uses the number of births to women aged 15-17. All specifications have the same controls: a constant, the logarithm of pregnancies of women 25 to 45 years old as a control, the logarithm of the population aged 14-17 enrolled in public schools, municipality poverty rate, per capita government expenditure in education (in logs, per student in public school), total population (in logs), and total female population (in logs). Municipality linear time trends are included by interacting municipality fixed effects with a linear trend in months. The observation unit is municipality-month (with 345 municipalities from January 2007 to December 2013). Column (4) has fewer observations as five municipalities have zero births to women aged 15-17 each month and are therefore excluded in the Poisson model computation. Strike Intensity is computed following Equation 2, where *School on strike* is one if students in that school lost more than five days on average during August. Robust standard errors are clustered at the municipality level in parentheses. *** p<0.01, ** p<0.05, * p<0.10.

B.2 Tables

Table B.2: Effect of Strike Exposure - Robustness To Pre-Treatment Attendance Rates

Dependent Variable: Teenage Pregnancies (births to women aged 15-17)	in logs	IHS transf.	Rates	Counts
	(1)	(2)	(3)	(4)
Strike Intensity	0.101**	0.125**	0.506**	0.130**
	(0.047)	(0.061)	(0.227)	(0.058)
Strike Period x Pre-Treatment Low Attendance	0.011	0.005	-0.239	-0.053
	(0.066)	(0.085)	(0.277)	(0.068)
Observations	28,980	28,980	28,980	28,560
Adjusted R^2 /Pseudo R^2	0.794	0.780	0.191	0.641

This table reports estimates of the effect of strike exposure using different ways to measure the outcome of interest while controlling by the interaction of a time-varying binary indicator of the students' strike period and municipality-level measure of pre-treatment low attendance indicator. This indicator is computed by applying formula 2 to schools with low pre-treatment attendance rates (i.e., mean attendance rates below 90% for 2007-2010). Columns (1) to (3) present the results for an estimation of an OLS fixed-effects model, varying the definition of the dependent variable, while Column (4) shows the results of a Poisson regression model. Column (1) uses the logarithm of the number of births to women aged 15-17 plus one as the dependent variable. Column (2) uses an inverse hyperbolic sine transformation of the number of births to women aged 18-24. Column (3) uses the rate of teenage pregnancies, defined as the number of births to women aged 18-24 over the number of females aged 18-24. Column (4), the Poisson model, directly uses the number of births to women aged 18-24. All specifications have the same controls: Municipality linear time trends are included by interacting municipality fixed effects with a linear trend in months. The observation unit is municipality-month (with 345 municipalities from January 2007 to December 2013). Column (4) has fewer observations as five municipalities have zero births to women aged 18-24 each month and are therefore excluded in the Poisson model computation. Robust standard errors are clustered at the municipality level in parentheses. *** p<0.01, ** p<0.05, * p<0.10.

Table B.3: Effect of Strike Exposure - Robustness To Overlap with Primary Schools

Dependent Variable: Teenage Pregnancies (births to women aged 15-17)				
	in logs	IHS transf.	Rates	Counts
	(1)	(2)	(3)	(4)
Strike Intensity	0.105***	0.126***	0.356**	0.095**
	(0.033)	(0.042)	(0.163)	(0.044)
Strike Period x Also Primary Education	-0.091	-0.108	0.346	-0.166
	(0.066)	(0.084)	(2.039)	(0.424)
Observations	28,980	28,980	28,980	28,559
Adjusted R^2 /Pseudo R^2	0.794	0.780	0.191	0.641

This table reports estimates of the effect of strike exposure using different ways to measure the outcome of interest while controlling by the interaction of a time-varying binary indicator of the students' strike period and municipality-level measure of the proportion of female students aged 14-17 who attended a school that also has primary education. This indicator is computed by applying the formula 2 to schools with both primary and secondary education. Columns (1) to (3) present the results for an estimation of an OLS fixed-effects model, varying the definition of the dependent variable, while Column (4) shows the results of a Poisson regression model. Column (1) uses the logarithm of the number of births to women aged 15-17 plus one as the dependent variable. Column (2) uses an inverse hyperbolic sine transformation of the number of births to women aged 18-24. Column (3) uses the rate of teenage pregnancies, defined as the number of births to women aged 18-24 over the number of females aged 18-24. Column (4), the Poisson model, directly uses the number of births to women aged 18-24. All specifications have the same controls: Municipality linear time trends are included by interacting municipality fixed effects with a linear trend in months. The observation unit is municipality-month (with 345 municipalities from January 2007 to December 2013). Column (4) has fewer observations as five municipalities have zero births to women aged 18-24 each month and are therefore excluded in the Poisson model computation. Robust standard errors are clustered at the municipality level in parentheses. *** p<0.01, ** p<0.05, * p<0.10.

Table B.4: Effect of Strike Exposure on Teenage Pregnancy: Pre-Post Comparison

Dependent Variable: Teenage Pregnancies (births to women aged 15-17)	in logs	IHS transf.	Rates	Counts
	(1)	(2)	(3)	(4)
Strike Adherence x Post	0.190***	0.234***	1.062***	0.287***
	(0.070)	(0.089)	(0.384)	(0.101)
Observations	8,625	8,625	8,625	8,234
Adjusted R^2 /Pseudo R^2	0.794	0.780	0.171	0.643

This table reports estimates of the effect of strike exposure using different ways to measure the outcome of interest, teenage pregnancies, but restricting the period of interest to 12 months before and after the onset of the strikes (April 2010 - April 2012). Columns (1) to (3) present the results for an estimation of an OLS fixed-effects model, varying the definition of the dependent variable, while Column (4) shows the results of a Poisson regression model. Column (1) uses the logarithm of the number of births to women aged 15-17 plus one as the dependent variable. Column (2) uses an inverse hyperbolic sine transformation of the number of births to women aged 15-17. Column (3) uses the rate of teenage pregnancies, defined as the number of births to women aged 15-17 over the number of public school female students aged 15-17, including weights for the number of public students aged 14-17 in the municipality. Column (4), the Poisson model, directly uses the number of births to women aged 15-17. All specifications have the same controls: a constant, the logarithm of pregnancies of women 25 to 45 years old as a control, the logarithm of the population aged 14-17 enrolled in public schools, municipality poverty rate, per capita government expenditure in education (in logs, per student in public school), total population (in logs), and total female population (in logs). Municipality linear time trends are included by interacting municipality fixed effects with a linear trend in months. The observation unit is municipality-month (with 345 municipalities from March 2010 to April 2012). Column (4) has fewer observations as five municipalities have zero births to women aged 15-17 each month and are therefore excluded in the Poisson model computation. Strike Intensity is multiplied by Post, a binary variable taking value one starting April 2011 and 0 otherwise. Robust standard errors are clustered at the municipality level in parentheses. *** p<0.01, ** p<0.05, * p<0.10.

Table B.5: Effect of Strike Exposure on Adherence to Contraception

	Teenage Pregnancies	Condoms		Other Methods
	(1)	(2)	(3)	(4)
	in logs	Teenagers	Adults	Teenagers
Strike Intensity	0.088*	0.202**	-0.063	-0.010
	(0.046)	(0.085)	(0.091)	(0.045)
Observations	10,380	10,380	10,380	10,380
Adjusted R^2	0.742	0.691	0.744	0.883

This table reports fixed-effects estimates of the effect of strike exposure measures on different outcomes. Each dependent variable corresponds to a natural logarithm plus one of the total count. All specifications include a constant, the logarithm of the population aged 15-17 enrolled in public schools, municipality poverty rate, per capita government expenditure in education (in logs, per student in public school), total population (in logs), and total female population (in logs). Column (1) includes the logarithm of pregnancies of women 25 to 45 years old as a control. Column (2) includes the logarithm of adult enrollment in the Fertility Regulation Program associated with access to condoms. Column (4) includes the logarithm of adult enrollment in the Fertility Regulation Program associated with access to other contraceptive methods. Municipality linear time trends are included by interacting municipality fixed effects with a linear trend in months. The observation unit is municipality-month (with 173 municipalities from January 2009 to December 2013). Robust standard errors are clustered at the municipality level in parentheses. *** p<0.01, ** p<0.05, * p<0.10.

Table B.6: Effect of Strike Exposure in Higher Education on Pregnancies (women aged 18-24)

Dependent Variable: Pregnancies in Higher Education (births to women aged 18-24)				
	in logs	IHS transf.	Rates	Counts
	(1)	(2)	(3)	(4)
Strike Intensity (Higher Education)	0.019	0.021	-0.454	-0.001
	(0.058)	(0.071)	(3.334)	(0.038)
Observations	27,216	27,216	27,216	27,216
Adjusted R^2 /Pseudo R^2	0.902	0.884	0.600	0.868

This table reports estimates of the potential effect of strike exposure for women in tertiary education age (i.e., 18-24 years old). Our measure of exposure is computed as the interaction between a time-varying binary indicator of the students' strike period and a higher education strike adherence measure constructed as the proportion of 18-24 women residing in the municipality who attended tertiary education in 2011. Columns (1) to (3) present the results for an estimation of an OLS fixed-effects model, varying the definition of the dependent variable, while Column (4) shows the results of a Poisson regression model. Column (1) uses the logarithm of the number of births to women aged 15-17 plus one as the dependent variable. Column (2) uses an inverse hyperbolic sine transformation of the number of births to women aged 18-24. Column (3) uses the rate of teenage pregnancies, defined as the number of births to women aged 18-24 over the number of females aged 18-24. Column (4), the Poisson model, directly uses the number of births to women aged 18-24. All specifications have the same controls: Municipality linear time trends are included by interacting municipality fixed effects with a linear trend in months. The observation unit is municipality-month (with 345 municipalities from January 2007 to December 2013). Column (4) has fewer observations as five municipalities have zero births to women aged 18-24 each month and are therefore excluded in the Poisson model computation. Robust standard errors are clustered at the municipality level in parentheses. *** p<0.01, ** p<0.05, * p<0.10.

Table B.7: Effect of Strike Exposure on Teenage Pregnancy: Heterogeneity Analysis - Inverse Hyperbolic Sine Transformation

	Baseline Teenage Pregnancies		Baseline Population Size		Share of COED Students		University Campus	
	(1) Below Median	(2) Above Median	(3) Below Median	(4) Above Median	(5) Below Median	(6) Above Median	(7) Outside	(8) Within
Strike Intensity	0.123*	0.132**	0.126*	0.096	0.105*	0.138**	0.115**	0.098
	(0.068)	(0.059)	(0.066)	(0.059)	(0.060)	(0.063)	(0.051)	(0.071)
Observations	13,608	13,608	14,448	14,532	14,448	14,532	22,932	6,048
Adjusted R^2	0.778	0.752	0.263	0.725	0.825	0.647	0.644	0.821

This table reports fixed-effects estimates of the effect of strike exposure measures on different outcomes. Each dependent variable corresponds to the inverse hyperbolic sine transformation of the variable. All specifications have the same controls, including a constant, the logarithm of pregnancies of women 25 to 45 years old as a control, the logarithm of the population aged 15-17 enrolled in public schools, municipality poverty rate, per capita government expenditure in education (in logs, per student in public school), total population (in logs), and total female population (in logs). Municipality linear time trends are included by interacting municipality fixed effects with a linear trend in months. The observation unit is municipality-month (with 345 municipalities from January 2007 to December 2013). Robust standard errors are clustered at the municipality level in parentheses. *** p<0.01, ** p<0.05, * p<0.10.

Table B.8: Effect of Strike Exposure on Teenage Pregnancy: Heterogeneity Analysis - Rates

	Baseline Teenage Pregnancies		Baseline Population Size		Share of COED Students		University Campus	
	(1) Below Median	(2) Above Median	(3) Below Median	(4) Above Median	(5) Below Median	(6) Above Median	(7) Outside	(8) Within
Strike Intensity	0.269	0.437**	0.843*	0.228	0.236	0.463*	0.549**	0.107
	(0.260)	(0.207)	(0.459)	(0.183)	(0.232)	(0.245)	(0.216)	(0.262)
Observations	13,608	13,608	14,448	14,532	14,448	14,532	22,932	6,048
Adjusted R^2	0.181	0.171	0.079	0.290	0.258	0.114	0.110	0.425

This table reports fixed-effects estimates of the effect of strike exposure measures on different outcomes. Each dependent variable corresponds to rates, with the count as the numerator and the number of female public school students enrolled in 2011 as the denominator. All specifications have the same controls, including a constant, the logarithm of pregnancies of women 25 to 45 years old as a control, the logarithm of the population aged 15-17 enrolled in public schools, municipality poverty rate, per capita government expenditure in education (in logs, per student in public school), total population (in logs), and total female population (in logs). Municipality linear time trends are included by interacting municipality fixed effects with a linear trend in months. The observation unit is municipality-month (with 345 municipalities from January 2007 to December 2013). Robust standard errors are clustered at the municipality level in parentheses. *** p<0.01, ** p<0.05, * p<0.10.

Table B.9: Effect of Strike Exposure on Teenage Pregnancy: Heterogeneity Analysis - Poisson Regression

	Baseline Teenage Pregnancies		Baseline Population Size		Share of COED Students		University Campus	
	(1) Below Median	(2) Above Median	(3) Below Median	(4) Above Median	(5) Below Median	(6) Above Median	(7) Outside	(8) Within
Strike Intensity	0.089	0.109**	0.198*	0.067	0.076	0.106*	0.142**	0.032
	(0.078)	(0.049)	(0.116)	(0.048)	(0.062)	(0.062)	(0.056)	(0.071)
Observations	13524.000	13608.000	14028.000	14532.000	14364.000	14196.000	22512.000	6048.000
Pseudo R^2	0.653	0.604	0.155	0.561	0.665	0.485	0.466	0.594

This table reports fixed-effects estimates of the effect of strike exposure measures on different outcomes using a Poisson regression model. Each dependent variable corresponds to counts. All specifications have the same controls, including a constant, the logarithm of pregnancies of women 25 to 45 years old as a control, the logarithm of the population aged 15-17 enrolled in public schools, municipality poverty rate, per capita government expenditure in education (in logs, per student in public school), total population (in logs), and total female population (in logs). Municipality linear time trends are included by interacting municipality fixed effects with a linear trend in months. The observation unit is municipality-month (with 345 municipalities from January 2007 to December 2013). Robust standard errors are clustered at the municipality level in parentheses. *** p<0.01, ** p<0.05, * p<0.10.

Table B.10: School-level Descriptive Statistics

	Mean	Std. Dev.	Min	Max	N
Panel A. Data using Pooled Pre-Period Years					
Strike Adherance: Both	0.19	0.40	0.00	1.00	2,505
Strike Adherance: Attendance	0.18	0.38	0.00	1.00	2,491
Strike Adherance: Web Scrapping	0.13	0.33	0.00	1.00	2,028
Strike Adherance: Occupied	0.10	0.30	0.00	1.00	2,505
Strike Fadeout: Back to Normal in 2011	0.60	0.49	0.00	1.00	485
Also Primary School	0.73	0.44	0.00	1.00	2,505
Emblematic School	0.01	0.09	0.00	1.00	2,505
Share Students Living in Same Municipality	0.77	0.25	0.03	1.00	2,505
Share Households Above Min Wage	0.69	0.27	0.00	1.00	2,505
Father Education: HS Degree or more	0.64	0.28	0.00	1.00	2,505
Parental Academic Involvement Index	0.84	0.04	0.60	0.97	2,505
Student Academic Aspirations	0.95	0.06	0.54	1.00	2,505
Math Test Score	259.60	45.14	174.00	377.00	2,505
Reading Test Score	262.46	32.93	168.00	342.00	2,505
Dropout Rate	0.04	0.04	0.00	0.60	2,505
Average Attendance	91.89	3.37	71.23	100.00	2,505
Student Mood: Relax	0.56	0.09	0.12	1.00	2,505
Student Mood: Rage	0.66	0.09	0.14	1.00	2,505
Student Mood: Motivation	0.67	0.09	0.19	1.00	2,505
Panel B. Data using Pre-Period Years					
Dropouts	0.04	0.06	0.00	1.00	22,004
Dropouts - Females	0.04	0.06	0.00	1.00	21,424
College Admission Test Takers	3.72	1.38	0.00	6.96	24,981
College Admission Test Takers - Females	3.03	1.39	0.00	6.96	24,981

This table presents summary statistics for the main variables used at the school level in this paper. We pooled information from 2007 to 2010 in Panel A, indicating average values before the strike. In Panel B, information is at a school-year level for years 2007-2013 for dropout variables and years 2007-2014 for college admission test takers. For information on the construction of the variables, please refer to subsection B.3.

Table B.11: Municipality-level Descriptive Statistics

	Mean	Overall S.D.	Between S.D.	Within S.D.	Min	Max	N
Pregnancies of Mothers aged 15-17 (logs)	1.03	0.94	0.84	0.43	0.00	4.32	28,980
Pregnancies of Mothers aged 15-17 (IHS)	1.31	1.17	1.03	0.55	0.00	5.00	28,980
Pregnancies of Mothers aged 15-17 (rate)	3.90	9.15	1.78	8.97	0.00	1,000.00	28,980
Pregnancies of Mothers aged 15-17 (counts)	3.67	6.06	5.68	2.13	0.00	74.00	28,980
Pregnancies of Mothers aged 15-17 (logs)	1.03	0.94	0.84	0.43	0.00	4.32	28,980
Pregnancies of Mothers aged 18-21 (logs)	1.75	1.17	1.11	0.40	0.00	5.14	28,980
Pregnancies of Mothers aged 22-25 (logs)	1.74	1.20	1.14	0.40	0.00	5.22	28,980
Pregnancies of Mothers aged 26-29 (logs)	1.72	1.23	1.17	0.40	0.00	5.19	28,980
Pregnancies of Mothers aged 30-33 (logs)	1.61	1.23	1.16	0.41	0.00	5.09	28,980
Pregnancies of Mothers aged 34-37 (logs)	1.39	1.16	1.08	0.42	0.00	4.73	28,980
Pregnancies of Mothers aged 38-41 (logs)	0.99	0.98	0.89	0.42	0.00	4.13	28,980
Pregnancies of Mothers aged 42-45 (logs)	0.38	0.57	0.45	0.36	0.00	3.00	28,980
Pregnancies of Mothers aged 46+ (logs)	0.03	0.15	0.05	0.14	0.00	1.95	28,980
First Pregnancies of Mothers aged 15-17 (logs)	0.99	0.92	0.82	0.43	0.00	4.22	28,980
Later Pregnancies of Mothers aged 15-17 (logs)	0.14	0.35	0.20	0.28	0.00	2.56	28,980
Pregnancies of Mothers aged 18-19 (logs)	1.23	1.02	0.92	0.43	0.00	4.51	28,980
Pregnancies of Mothers aged 25-45 (logs)	2.67	1.40	1.37	0.32	0.00	6.22	28,980
Pregnancies with Both Parents aged 15-17 (logs)	0.31	0.54	0.42	0.33	0.00	3.00	28,980
Average Gestational at Birth, Mothers aged 15-17 (logs)	2.49	1.72	1.13	1.30	0.00	3.78	20,700
Fraction Premature Births, Mothers aged 15-17 (logs)	0.16	0.37	0.23	0.28	0.00	2.40	28,980
Fetal Deaths, Mothers aged 15-17 (logs)	0.02	0.13	0.04	0.12	0.00	1.61	28,980
Average Weight at Birth, Mothers aged 15-17 (logs)	5.55	3.75	2.44	2.85	0.00	8.56	28,980
Fraction Low Birth Weight, Mothers aged 15-17 (logs)	0.22	0.43	0.29	0.31	0.00	2.48	28,980
Strike Adherance: Both	0.28	0.26	0.26	0.00	0.00	0.98	28,980
Strike Adherance: Attendance	0.19	0.22	0.22	0.00	0.00	0.96	28,980
Strike Adherance: Web Scrapping	0.19	0.22	0.22	0.00	0.00	0.94	28,980
Strike Adherance: Both, only Occupied Schools	0.13	0.19	0.19	0.00	0.00	0.92	28,980
Strike Adherance: Both, excluding Occupied Schools	0.13	0.17	0.17	0.00	0.00	0.89	28,980
Strike Adherance: Both, only Emblematic Schools	0.01	0.04	0.04	0.00	0.00	0.47	28,980
Strike Adherance: Both, excluding Emblematic Schools	0.25	0.24	0.25	0.00	0.00	0.98	28,980

This table presents summary statistics for the main variables used in this paper. It corresponds to information on 345 municipalities monthly for seven years, from 2007 to 2013.

B.3 Description of Variables

This section describes the set of variables used for the analysis, separately describing the set of municipality- and school-level variables (and their sources).

B.3.1 School-level variables

Strike adherance indicators.– We collected data to create the treatment variable of strike intensity from two main sources. The first one is restricted-access official daily attendance records by the Ministry of Education, allowing us to estimate what fraction of high school students were not attending each school during August 2011. The second one comes from a web scrapping process to identify which schools are known to be on strike. We used Wayback Machine® software to search for information stored in expired URL addresses, web scraping information from blogs written by students during this period, national media, regional and local media, including newspapers, radio coverage, and social networks. We classify each school as being on strike if mentioned in any of these sites as taken over by students or closed during 2011. Lastly, we create an "occupied" status for each school by exploiting data kindly shared by Nicolás Grau (Donoso et al., 2016), in which they further classified whether each school experienced a student sit-in.

Official administrative records "Enrollment" and "Performance".– These two datasets come from the Ministry of Education's official administrative records and are publicly available at the student level. Using the "Enrollment" dataset for the years 2007 to 2010 (before the strike), we identify many potential factors that might influence a school to go on strike, as it has information on the universe of students enrolled in any school at the beginning of the academic year. We can characterize each school as private, voucher, or public; whether it has both primary and secondary levels; whether it is a co-educational school; and the average fraction of students that live in the same municipality as the municipality of the school they attend. We then use the "Performance" dataset for the years 2007 to 2011 to compute the past average attendance at the school level (pooled and by gender) and average school dropout rates, identifying as a dropout a high school student that is not yet a senior but is not back to school the following year, on any school or grade. Lastly, we identify schools as *emblematic* according to Appendix Table 1 in MINEDUC 2020.

SIMCE test: Math and Reading.– We use restricted access to 10th grade SIMCE performance in 2010 at the student level to create school averages for each school.

SIMCE questionnaires: Parents and students.– We used restricted access SIMCE questionnaires to 10th graders in 2010 (a year before the strikes) to create some factors that might help identify a school's leniency to join the strikes. The questionnaires are applied to both parents and students separately. We create indicator variables at the parent and student levels and then collapse the answers at the school level to get the average answers in each school.

From the parent's questionnaire, we estimate using the 14 possible brackets for family income (i) whether the family income is above or below the average in the country ($729,700 CLP of 2010), (ii) whether the family income is above the minimum wage ($138.460 CLP of 2010). We also created two indicators on parental education for each parent: (i) has less than high school education and (ii) has post-secondary education. We then average them across students within the school to get shares for each school.

From the students' questionnaire, we created five variables. The first one is "Parental Academic Involvement, which proxies students' perception of their parent's involvement with their education. We use a set of questions starting with: "Are any of your parents or guardians involved in the following activities to help you?" (translated from the original Spanish version, "¿Alguno de tus padres o personas que se hacen cargo de ti hace las siguientes actividades para ayudarte?"), for each of the eight categories: "Explains to me the topics I do not understand," "Helps me study," "Helps me do my homework (but does not completely do it without me)," "Knows or finds out about my grades," "Gets happy when I get good grades," "Reprimends me when I get bad grades," "Demands I get good grades," "Is willing to help me when I have issues with a topic or need help to complete a homework." Three options are allowed as answers: "Never or rarely," "Sometimes," and "Always or almost always." We create a parental involvement index for each student as the proportion of "Sometimes" or "Always or almost always" answers across the eight categories and then average them within the school. The second variable we create is "Student Academic Aspirations," which we code from the question: "What is the highest educational level that you would like to complete"? ("¿Cuál es el nivel de educación más alto que te gustaría completar?"). Three options are allowed as answers: "12th grade," "Technical degree," and "University degree." We assign each student a value one if they answered "Technical degree" or "University degree" and compute the share at the school level. The last three variables come from a self-assessment of a student's mood, which we label "Mood: Relax," "Mood: Rage," and "Mood: Motivation." We code it from the questions: "During the last month, how frequently have you been in this situation?" ("En el último mes, ¿con qué frecuencia te han sucedido las siguientes situaciones?"), with "Mood: Relax" being the proportion of students in the school answering "Sometimes" or "Always or almost always" to the statement "You have trouble relaxing," and similarly define "Mood: Anger" and "Mood: Motivation" from statements "You feel irritated or angry very easily" and "You feel not motivated, you have trouble getting interested in something," as the proportion of students in the school answering "Sometimes" or "Always or almost always" to those statements.

College admission test takers.– We used restricted access college admission selection process (PSU) data by the DEMRE (Departamento de Evaluación, Medición y Registro Educacional) on the nationwide college admission test results for all students taking the test between 2007 and 2014, where we tag each student as having taken the PSU if they take either Spanish or Math, and then add up by school the number of students taking the test, and apply logarithm (plus one) to it.

B.3.2 Municipality-level variables

Birth and fetal death variables.– We accessed information about the universe of birth statistics in Chile using the vital statistics data of the Department of Statistics and Health Information (DEIS) of the Ministry of Health of the Government of Chile from 2007 to 2014. This database was publicly available online in 2016, at the time of data retrieval. It records the universe of births in Chile, covering at least 99 percent of all births in published aggregate figures and indicating information associated with each birth, such as maternal age, father's age, the number of siblings, weight at birth, gestation weeks at birth and importantly, municipality of residence of the mother. We then compute the municipality-level logarithms of the count of all births and fetal deaths by mothers of each age subgroup on each month by subtracting the gestational age from birthday to establish the conception month for each baby

Strike intensity variables.– We merge these two school-level strike adherence measures to the 2011 administrative "Enrollment" records by the Ministry of Education, allowing us to know where each student lives and their age and gender. We compute the main strike intensity measures as the fraction of women living in the municipality aged 15 to 17 attending public school (in any municipality) that are attending a school identified as on strike by any of the two strike indicators. In addition, we also generated an indicator for whether students took over each school through the web scraping process.

Municipality characteristics.– We use publicly available municipality-level information reported by the National System of Municipal Information (SINIM) to compare each municipality's resources yearly. We collect data on population density per square kilometer, per capita income, per capita real investment in education, per capita expenditures, poverty rates, and birth rates. We compute the percentage of school-aged people in each municipality (i.e., people aged 6 to 19).

Education variables.– We use publicly available official administrative records by the Ministry of Education on "Performance." This record covers the universe of students in Chile's educational system. We compute average municipality-specific yearly attendance, promotion, and dropout rates (for grades 9-11) separately for females and pooling male and female students together. We compute the fraction of high school students in each comuna grade promoted at the end of the academic year. We also compute for each municipality the number of primary student residents attending schools with secondary instruction in any comuna.

Health variables.– Finally, data on contraceptive methods are taken from the publicly available Ministry of Health's Monthly Summary Statistics (REM, for its acronym in Spanish) for 2009-2013, as 2009 is the first available year. We compute for each month the number of patients aged 19 and younger enrolling in the *Programa de Regulación de Fertilidad* (Fertility Regulation Program) at each health center, which includes the municipality in which they are located. We use this information to isolate the entry of patients to the program as a way to access condoms.

B.4 General Issues on Measurement Error

One of the main estimation issues we face is binary misclassification for whether a student attended a school on strike. Let x_{ij} be a binary indicator for whether or not a student i who resides at municipality j attends a school on strike. We can relate this binary indicator to its true value, x^*_{ij}, by:

$$x_{ij} = x^*_{ij} + \mu_{ij} \tag{B.1}$$

Following Bound et al. (2001), let $prob(x_{ij} = 1|x^*_{ij} = 0) = \pi_{10}$ and $prob(x_{ij} = 0|x^*_{ij} = 1) = \pi_{01}$ be the probability of false negative and false positive responses, and $\pi = prob(x^*)$, i.e., the actual rate of schools on strike. One thing to notice with binary indicators is that the measurement error μ_{ij} is non-classical since $cov(x^*_{ij}, \mu_{ij}) < 0$. Furthermore, under the assumption that μ_{ij} is independent of y_{ij} (the outcome of a linear regression), the estimation of an OLS regression of y_{ij} on x_{ij} yields:

$$\beta_{OLS} = \beta \left[1 - prob(x^*_{ij} = 1|x_{ij} = 0) - prob(x^*_{ij} = 0|x_{ij} = 1)\right] \tag{B.2}$$

$$\beta_{OLS} = \beta \underbrace{\left[1 - \frac{\pi_{01}\pi}{\pi_{01}\pi + (1-\pi_{10})(1-\pi)} - \frac{\pi_{10}\pi}{\pi_{10}(1-\pi) + (1-\pi_{01})\pi}\right]}_{C} \tag{B.3}$$

It is essential to notice that the measurement error in our setting comes from how we construct our strike variable. It is plausible to assume that this misclassification error is independent of a student's probability of becoming pregnant, our primary outcome of interest. This assumption would be less plausible to hold if, for instance, the misclassification error in response to survey questions by the same person under study, i.e., the student responding whether her school was on strike or not, could be correlated with unobservables that determine the probability of becoming pregnant. This assumption is important because, if it holds, we can more easily adjust the estimated coefficients by the term C in equation B.3 if we know its probabilities. If we ignore the probabilities, we can still construct bounds for β_{OLS} using that expression. If the assumption does not hold, then we do not know the form of the bias since each component of C, for instance, π_{10} would be defined for each individual separately. However, there is little reason to think that our error by construction is related to the probability that a teen in a particular school becomes pregnant.

B.4.1 Aggregation of a binary variable and structure of Measurement Error

Using the micro-data of students and schools, we calculate the fraction of female students who live in the municipality who attend a school on strike. In this aggregation, we drag the misclassification error of each school's strike status attended by girls who reside in municipality m. Let x_{im} be a binary indicator for whether student i who resides in municipality m attends a school on strike. This indicator relates to the actual strike status of the school misclassification of strike status is of the school x^*_{im} as: $x_{im} = x^*_{im} + \mu_{im}$. Aggregating at the municipality level, we get that:

$$
\begin{aligned}
x_{im} =& x^*_{im} + \mu_{im} \\
\frac{1}{n_m}\sum_{i=1}^{n_m} x_{im} =& \frac{1}{n_m}\sum_{i=1}^{n_m}(x^*_{im} + \mu_{im}) \\
\bar{x}_{\cdot m} =& \bar{x}^*_{\cdot m} + \bar{\mu}_{\cdot m} \\
cov(\bar{x}^*_{\cdot m}, \bar{\mu}_{\cdot m}) =& cov\left(\frac{1}{n_m}\sum_{i=1}^{n_m} x^*_{im}, \frac{1}{n_m}\sum_{i=1}^{n_m} \mu_{im}\right) \\
=& \left(\frac{1}{n_m}\right)^2 cov\left(\sum_{i=1}^{n_m} x^*_{im}, \sum_{i=1}^{n_m} \mu_{im}\right) \\
=& \left(\frac{1}{n_m}\right)^2 \sum_{i=1}^{n_m}\sum_{k=1}^{n_m} cov(x^*_{im}, \mu_{km}) \\
=& \underbrace{\left(\frac{1}{n_m}\right)^2 \sum_{i=1}^{n_m} cov(x^*_{im}, \mu_{im})}_{<0}
\end{aligned}
$$

So, assuming independence across observations, the measurement error of the proportion of female students who attended a school on strike is also non-classical, as the covariance of the error and the actual rate is negative.

B.4.2 Application to the measurement of a School's Strike Status

This section applies notation from Black et al. (2000) to our case study. Suppose we observe a variable z^a_{im} for whether student i's in municipality m was on strike. We also observe an alternative measure z^b_{im}. x^*_{im} is the actual strike status of the school.

$$z^a_{im} = x^*_{im} + \mu^a_{im}$$

$$z^b_{im} = x^*_{im} + \mu^b_{im}$$

To simplify our case, suppose we run the following regression at the municipality level that associates teenage pregnancy rate to the proportion of female students who attended a school on strike.

$$y_m = \beta_0 + \beta_1 x^*_m + \varepsilon_m \tag{B.4}$$

However, we do not observe the real proportion of students who attended a school on strike but instead can estimate:

$$y_m = \beta_0 + \beta_1 z^k_m + \varepsilon_m \tag{B.5}$$

for $k \in \{a, b\}$. The assumptions in Black et al. (2000) are:

A1 μ^a and μ^b are independent conditional on x^* .

A2 $E(y|x^*) = E(y|x^*, z^k)$ for $k \in \{a, b\}$.

A3 ε_j independent of μ^k for $k \in \{a, b\}$.

A4 $cov(x^*, z^k) > cov(z^a, z^b) > 0$ for $k \in \{a, b\}$.

A5 $cov(x^*, \mu^k) < 0$ for $k \in \{a, b\}$.

In particular, (A1) requires that the errors of misclassification of strike status are not related to each other, other than their relation to x^*, the actual strike status. We believe that this assumption holds as the process of misclassification of each term is independent as they arise from two unrelated data sources: (1) web-scraping data and (2) micro-data of official records on daily attendance. (A2) states that the errors of misclassification, μ^a and μ^b, are independent of y, the probability that a teenage girl becomes pregnant. Notably, the process of classification error in both measures of a school's strike status arises from researchers' coding errors in constructing the strike proxies. It is plausible, then, to assume that (A2) holds in this setting as our own mistakes in coding a school on strike are independent of whether a teenage girl in that school became pregnant during our analysis period. This critical assumption is unlikely to hold in other settings, such as response errors in survey data (Bollinger and David, 1997). (A3) is a standard assumption and also relates to misclassification error being independent of the data-generating process of y. (A4) assumes that the "error is not too severe" (Black et al., 2000, pp. 740) so that each independent measure's covariance with the correct fraction of schools on strike surpasses the covariance between both proxies. (A5) holds by construction as strike status is a binary indicator at the school level (see section B.4.1).

These assumptions together allow constructing bounds proposed by Black et al. (2000) in the following form. Suppose we estimate the following regression by OLS: $y_m = \beta_0 + \beta_1 z^a_m + \varepsilon_m$. The *plim* of β_1 is:

$$\begin{aligned} plim\hat{\beta}_1 =& \frac{Cov(y_m, x^*_m + \mu^a_m)}{Var(x^*_m + \mu^a_m)} \\ =& \beta_1 \frac{Var(x_m) + Cov(x^*_m, \mu^a_m)}{Var(x^*_m) + 2Cov(x^*_m, \mu^a_m) + Var(\mu^a_m)} \\ <& \beta_1 \end{aligned}$$

If $Var(\mu^a_m) + Cov(x^*_m, \mu^a_m) > 0$, then OLS estimates a lower bound for β_1.

Having access to an additional measure z^b_m we can get an upper bound for β_1 using z^b_m as an instrument for z^a_m. Following Black et al. (2000), we have that:

$$
\begin{aligned}
plim\hat{\beta}_1^{IV} =& \frac{Cov(y_m, z_m^a)}{Cov(z_m^a, z_m^b)} \\
=& \beta_1 \frac{Var(x_m^*) + Cov(x_m^*, \mu_m^a)}{Var(x_m^*) + Cov(x_m^*, \mu_m^a) + Cov(x_m^*, \mu_m^b) + Cov(\mu_m^a, \mu_m^b)} \\
>& \beta_1
\end{aligned}
$$

Imposing (A4), we have an upper bound for β_1.

B.4.3 Measurement of school strike: two proxies for school's strike status

As there may be measurement error concerns in our strike intensity measures, we address this in this section. There could be different reasons why these data are measured with error, but most importantly, we classify schools as being on strike if the school shows up as being on strike or taken over in our web search. There will be a misclassification problem if schools on strike are not coded in this variable because they do not or are classified as such according to attendance data. This could generate false negatives or false positives by construction. Classification error - the wrong assignment of strike status to schools - is necessarily non-classical. To illustrate this, consider the following equation that links strike status in equation (2) at the school level to its actual status:

$$
School\ on\ strike_s^k = School\ on\ strike_s^* + \mu_s^k \quad \text{for } k = 1, 2 \tag{B.6}
$$

In this equation, $School\ on\ strike_s^*$ is a binary indicator for true strike status. As Bound et al. (2001) illustrates, measurement error in a binary variable is necessarily non-classical as the covariance between the error and the true measure is negative. In our case if $School\ on\ strike_s^k = 0$ and $School\ on\ strike_s^* = 1$ then $\mu_s^k = -1$; $School\ on\ strike_s^k = 1$ and $School\ on\ strike_s^* = 0$ then $\mu_s^k = 1$. Similar to other combinations of values between observed and true value of strike status. This implies that $cov(School\ on\ strike_s^*, \mu_s) < 0$ and so measurement error presents additional challenges to estimation than the classical error-in-variables model where $cov(School\ on\ strike_s^*, \mu_s^k) = 0$. Aggregating equation (B.6) to the municipality level to construct $Strike\ Adherence_m^k$ in equation (2) aggregates measurement error μ_s^k to the municipality level as well. Hence, the new relation between municipality true strike adherence and measured adherence is given by:

$$
Strike\ Adherence_m^k = Strike\ Adherence_m^* + \psi_m^k \quad \text{for } k = 1, 2 \tag{B.7}
$$

Where $\psi_m = \dfrac{\sum_{i=1}^{N_m} 1_{i(s)} \mu_s}{N_m}$. After aggregating the data ψ_m^k holds similar properties as μ_s^k. In particular, $cov(Strike\ Adherence_m^*, \psi_m) < 0$, biasing coefficients downwards whenever the covariance between the true measure and the error is lower than the variance of the error itself (Black et al., 2000).

In this section, we follow and adapt methods in Black et al. (2000) to estimate the effects of strike intensity on teenage pregnancy. They show that under this structure of measurement error and plausible assumptions, using one proxy measure as an instrumental variable for another proxy estimates an upper bound of the coefficient of interest. To do this, we use as an instrument, $\textit{Strike Intensity}^2_m$, obtained from daily attendance data from August, where each school is coded as being on strike if the average student lost ten days of school in that month.

One important consideration when using this method is that classification errors in both measures of a school's strike status arise from our coding errors in constructing the concept of a strike. Then, it is plausible to assume that the misclassification error of strike status, μ^k_s, is independent of the probability that a student becomes pregnant, our main outcome of interest. This is an important assumption that is unlikely to hold in other settings, such as response error in survey data (Bollinger and David, 1997), and allows, among other assumptions, to construct bounds proposed by Black et al. (2000).

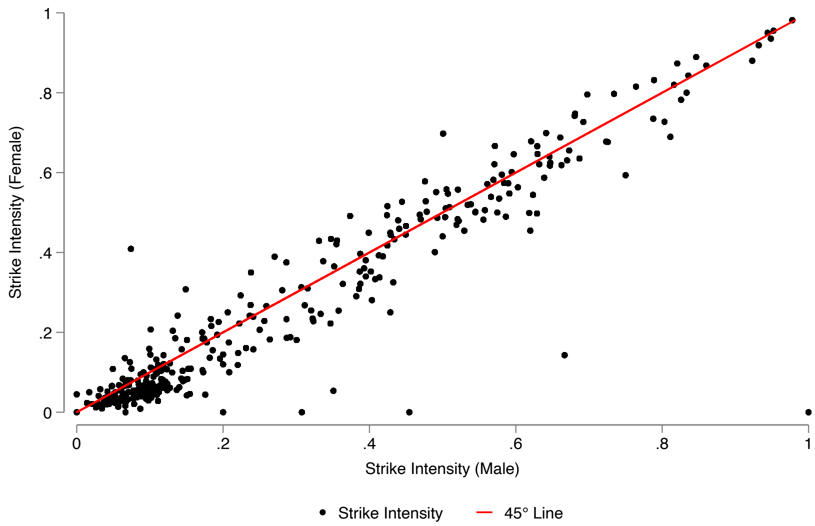
We revise the main results using $\textit{Strike Intensity}^2_m$, constructed from daily attendance data, as an instrument for $\textit{Strike Intensity}^1_m$. The results shown in Table B.12 show that the effects are very similar to the ones in our main specification. Under assumptions in Black et al. (2000), estimates of equation (3) using one of the two measures should be biased toward zero, and IV estimates shown in Table B.12 should represent an upper bound of the effect of school strikes on teenage pregnancy rates.

Table B.12: Effect of Strike Exposure on Teenage Pregnancy: Instrumental Variables Estimation

	Teenage Pregnancies		Pregnancies	Teenage Couples
	(1) All	(2) Order: 1	(3) Age: 18-19	(4)
Strike Intensity (Web-Scrapped)	0.181***	0.172**	0.021	0.051
	(0.065)	(0.068)	(0.067)	(0.051)
Observations	28,980	28,980	28,980	28,980
Adjusted R^2	-0.015	-0.015	-0.015	-0.015

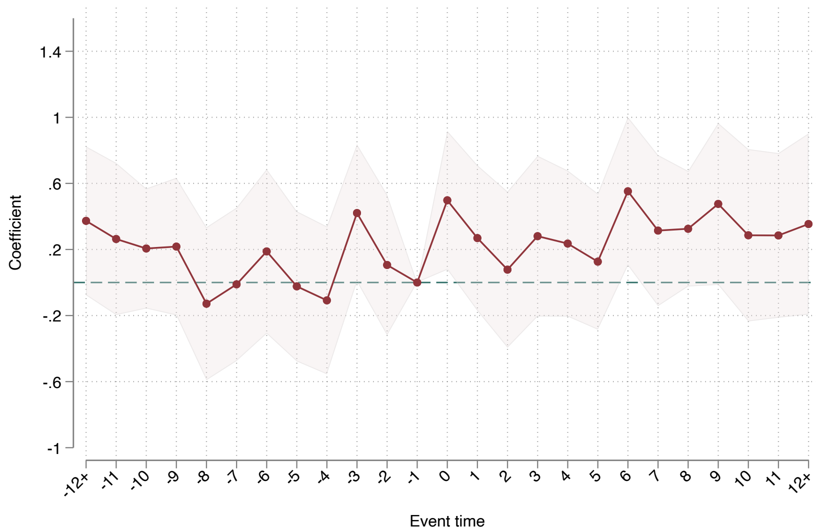
This table reports instrumental variables estimates of the effect of strike exposure on different outcomes. We instrumented strike intensity as measured by web scraping with strike intensity as measured by schools having more than ten attendance days lost. All specifications have the same controls: a constant, the logarithm of pregnancies of women 25 to 45 years old as a control, the logarithm of the population aged 14-17 enrolled in public schools, municipality poverty rate, per capita government expenditure in education (in logs, per student in public school), total population (in logs), and total female population (in logs). Municipality linear time trends are included by interacting municipality fixed effects with a linear trend in months. The observation unit is municipality-month (with 345 municipalities from January 2007 to December 2013). Robust standard errors are clustered at the municipality level in parentheses. *** p<0.01, ** p<0.05, * p<0.10.

Figure B.7: Correlation between Female and Male Strike Intensity Measures



Notes: This figure shows the scatter for two strike intensity measures at the municipality level computed using strike exposure for female (Y axis) and male students (X axis).

Figure B.8: Associations between Strike and Adherence to Fertility Program through use of condoms



Notes: This figure plots the coefficients and the 95% confidence intervals for twenty-four month dummies interacted with the main strike intensity measure for 12 months before and 12 months after the start of the student protest (i.e., on May 2011). The coefficient for April 2011 is normalized to zero. Confidence intervals are based on heteroskedasticity-robust standard errors clustered at the municipality level. The coefficients are estimated from a unique regression of teenage enrollment in the Fertility Regulation Program associated with access to condoms (in logs), which includes municipality and month-fixed effects as well as municipality-specific linear trends, the logarithm of adult enrollment in the Fertility Regulation Program associated with access to condoms, the logarithm of the teenage population enrolled in public schools, poverty rate, per capita government expenditure in education (in logs, per student in public school), total population (in logs), and total female population (in logs).