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When a strike strikes twice: Massive student mobilizations and teenage pregnancy in Chile[☆]

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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.

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 1000 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).

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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 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 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

[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).

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, 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 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.

[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.

[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.

[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.

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:

$$Strike\ Intensity_{mt}^{k} = Strike\ Period_{t} \times Strike\ Adherence_{m}^{k} \tag{1}$$

$Strike\ Period_t$ 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. $Strike\ Adherence_m^k$ captures cross-sectional variation in the average strike intensity experienced by a municipality. Formally, $Strike\ Adherence_m^k$ is computed as:

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

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). Fig. 1(b) 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 closer to the country's capital, Santiago. In Fig. 1(c), 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 600 of approximately 2330 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, Fig. 1(a) 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 Fig. 2. We plot the coefficients and confidence intervals for each characteristic of the school. All characteristics are measured before 2011, the year when protests took place.

[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 Tables A.1 and A.2.

[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.

[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.

[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.

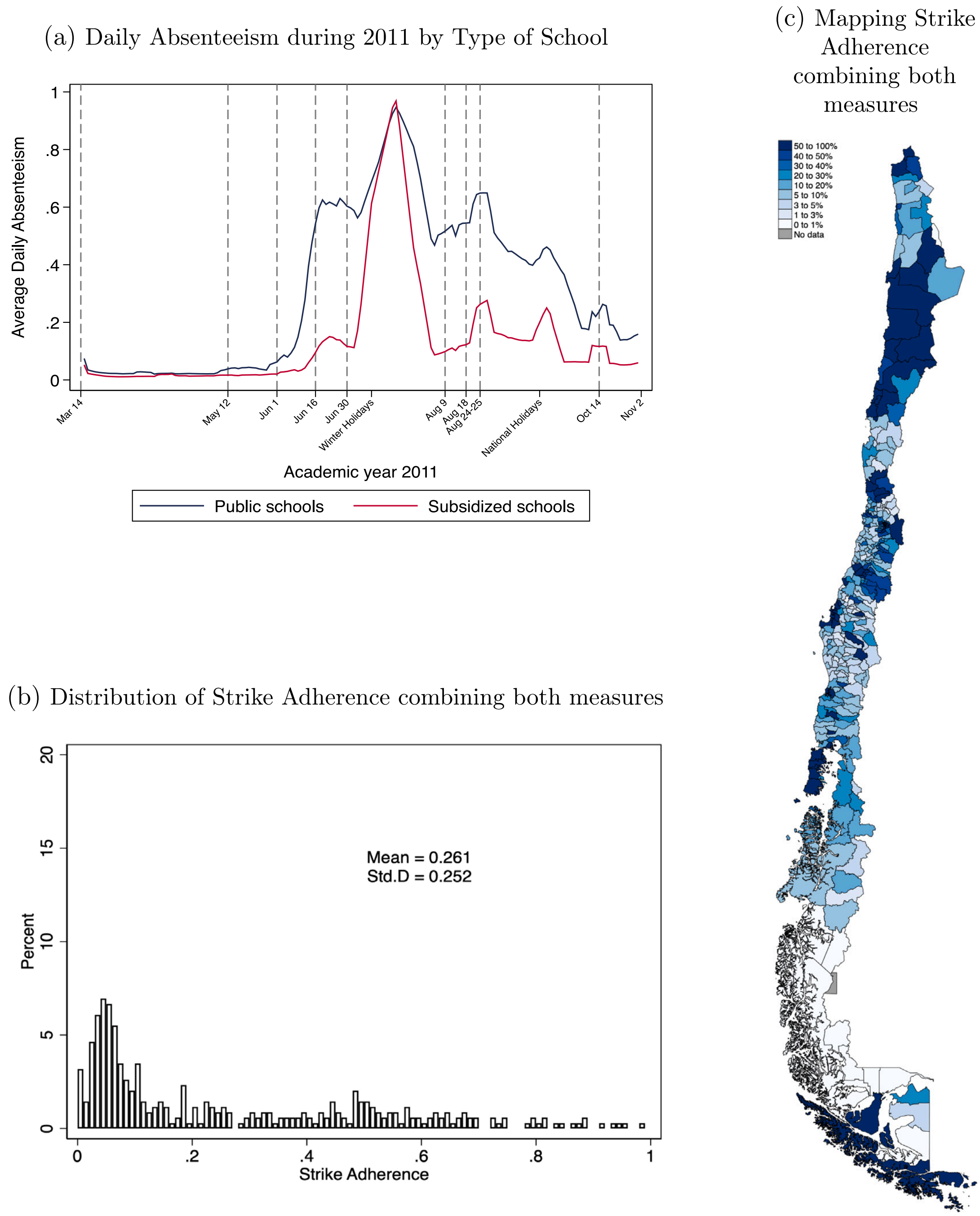

Fig. 1. Daily school absenteeism and cross-sectional variation in strike adherence using different measures.
Notes: (a) This subfigure shows the trends in daily school absenteeism in a moving average of 2 days during 2011 by type of school. The blue line represents public schools, while the red line 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. The legend in the upper-left corner of the map explains the colors used to show strike adherence at the municipality level.

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]

[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.

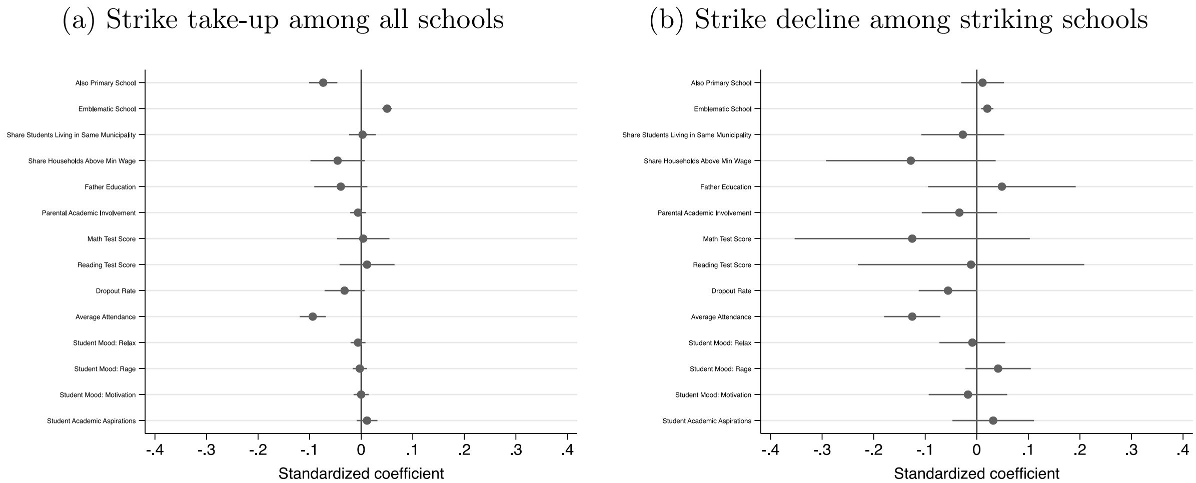

Fig. 2. Associations between school level characteristics, strike take-up, and decline.
Notes: Each panel in this figure plots the coefficients and the 95% confidence intervals for a set of school-level covariates (listed on the y-axis) for the analysis of (a) the probability of going on strike (among all schools in Chile, $N = 2505$) 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.

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 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 Fig. 1(a), 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 Fig. 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 Fig. 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 Fig. 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 "emblematic", 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]

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 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

[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.

[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.

specifications of the following econometric model for the 2007–2013 period:

$$Teenage\ Pregnancies_{mt} = \alpha + \beta Strike\ Intensity^{k}_{mt} + \gamma X_{mt} + \lambda_m + \tau_t + \varepsilon_{mt} \quad (3)$$

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, $Teenage\ Pregnancies_{mt}$, 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 × month observations with zero conceptions.[24] $Strike\ Intensity^{k}_{mt}$ is our main independent variable. The semi-log specification presented in Eq. (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 β% 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 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 Fig. 1(b).

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:

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

where s and t denote school and year. The sample consists of 2967 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 Section 2.3, $School\ on\ strike^{k}_{s}$ is a binary indicator of whether school s was on strike according to strike measure k. Y_t is an indicator variable that takes the value one when time is τ, 0 otherwise. We exclude the interaction between $School\ on\ strike^{k}_{s}$ 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 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.

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

[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.

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 Eq. (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 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 Fig. 1(b). Consequently, we conduct nineteen separate regressions, each employing one of these binary indicators.

The results in Fig. 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.

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). Fig. 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.

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

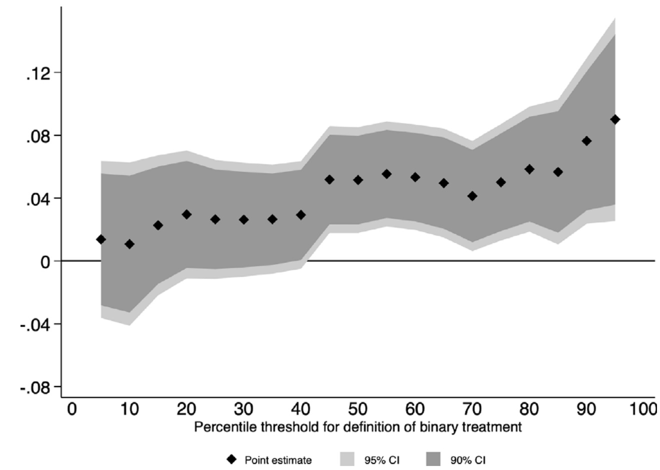

Fig. 3. Alternative thresholds for definition of binary treatment variable.
Notes: This figure plots coefficients from a specification similar to Column (3) in Table 1 using a binary indicator variable as the treatment variable. This binary indicator identifies whether each municipality falls above the $5th$, $10th, \ldots, 95th$ percentile of the strike adherence distribution depicted in Fig. 1(b). 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.

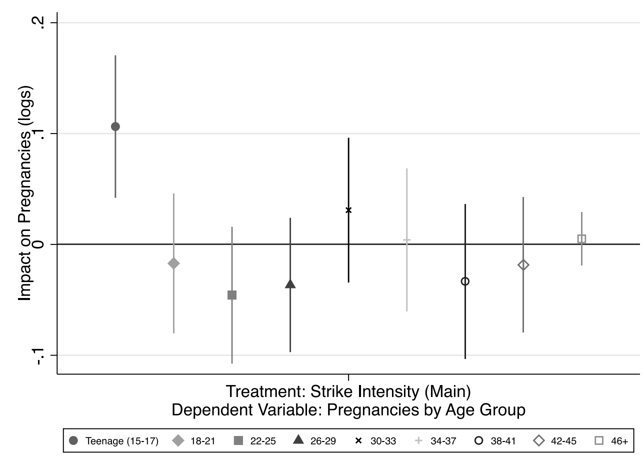

Fig. 4. Effects for different age groups.
Notes: This figure plots coefficients and their 95% confidence intervals from regressing total pregnancies (in logs) for different age groups on our primary measure of strike intensity. All specifications include municipality and month fixed effects and municipality-specific linear time trends. The observation unit is municipality-month (with 345 municipalities from January 2007 to December 2013).

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 Fig. 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 are statistically similar to the estimates obtained from other specifications.

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. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

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 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 Section 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 Eq. (2), so $Strike\ Adherence_m^k$ 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 pregnancy 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 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 Fig. 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]

[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° line.

[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.

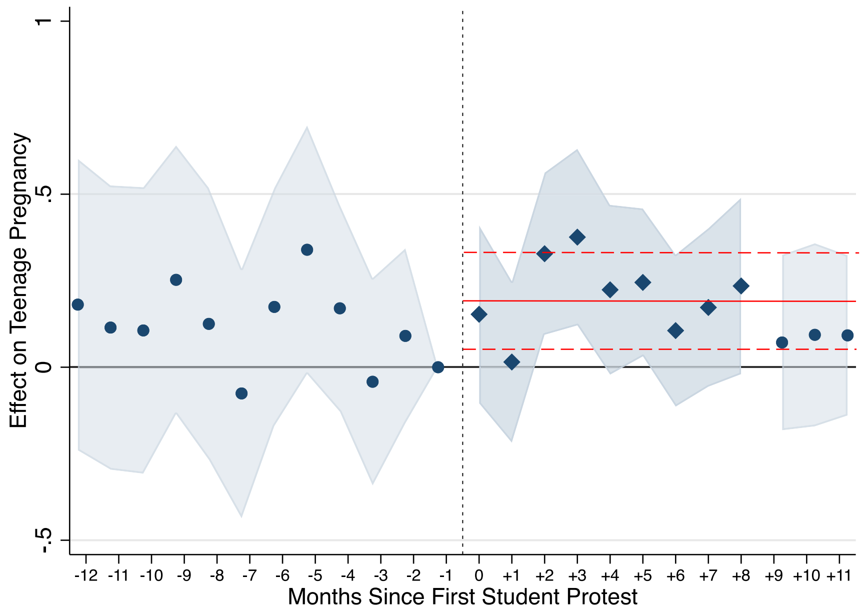

Fig. 5. Event study.
Notes: This figure plots the coefficients and the 95% confidence intervals for twenty-three-month dummies interacted with strike adherence for 12 months-window before and after the start of the student protest (i.e., on May 2011). The coefficient for April 2011 is normalized to zero. 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).

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.

Fig. 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 Fig. 1(a).

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 Fig. 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] Fig. 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 significant trend before the strike period. Noteworthy, subfigures (l) and (m) of Fig. 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.

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

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 Eq. (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. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

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 Fig. 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* 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 Fig. A.3. Each panel in Fig. 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.

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

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

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. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

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 Fig. 1(a). 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 Fig. 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 Fig. 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 Fig. 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 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 Eq. (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 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 Fig. 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

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.

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 dependent 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. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

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 Eq. (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 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 2500 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 Fig. 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, 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.

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

[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.

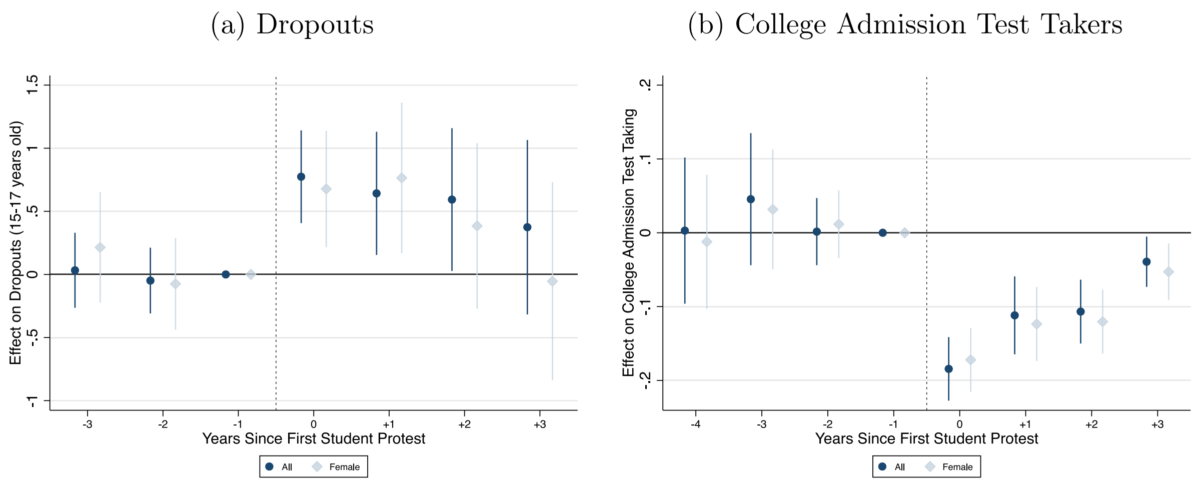

Fig. 6. Dropouts and college admission test taking at the school level
Notes: These figures are the graphical representation of the results in Columns (1) and (2) of Tables A.4 and A.5, respectively. Each subfigure presents the coefficients and 95% confidence intervals for years represented as dummy 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.

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.

Disclosure statement

During the preparation of this work the authors used ChatGPT in order to improve readability. After using this tool/service, the authors reviewed and edited the content as needed and take full responsibility for the content of the publication.

CRediT authorship contribution statement

Pablo A. Celhay: Conceptualization, Data curation, Formal analysis, Investigation, Methodology, Writing – original draft, Writing – review & editing. **Emilio Depetris-Chauvin:** Conceptualization, Data curation, Formal analysis, Investigation, Project administration, Supervision, Validation, Writing – original draft, Writing – review & editing. **Cristina Riquelme:** Conceptualization, Data curation, Formal analysis, Investigation, Validation, Writing – original draft, Writing – review & editing.

Declaration of competing interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

Data availability

Data will be made available on request.

Appendix A

A.1. Additional tables

See Tables A.1–A.5.

A.2. Additional figures

See Figs. A.1–A.4.

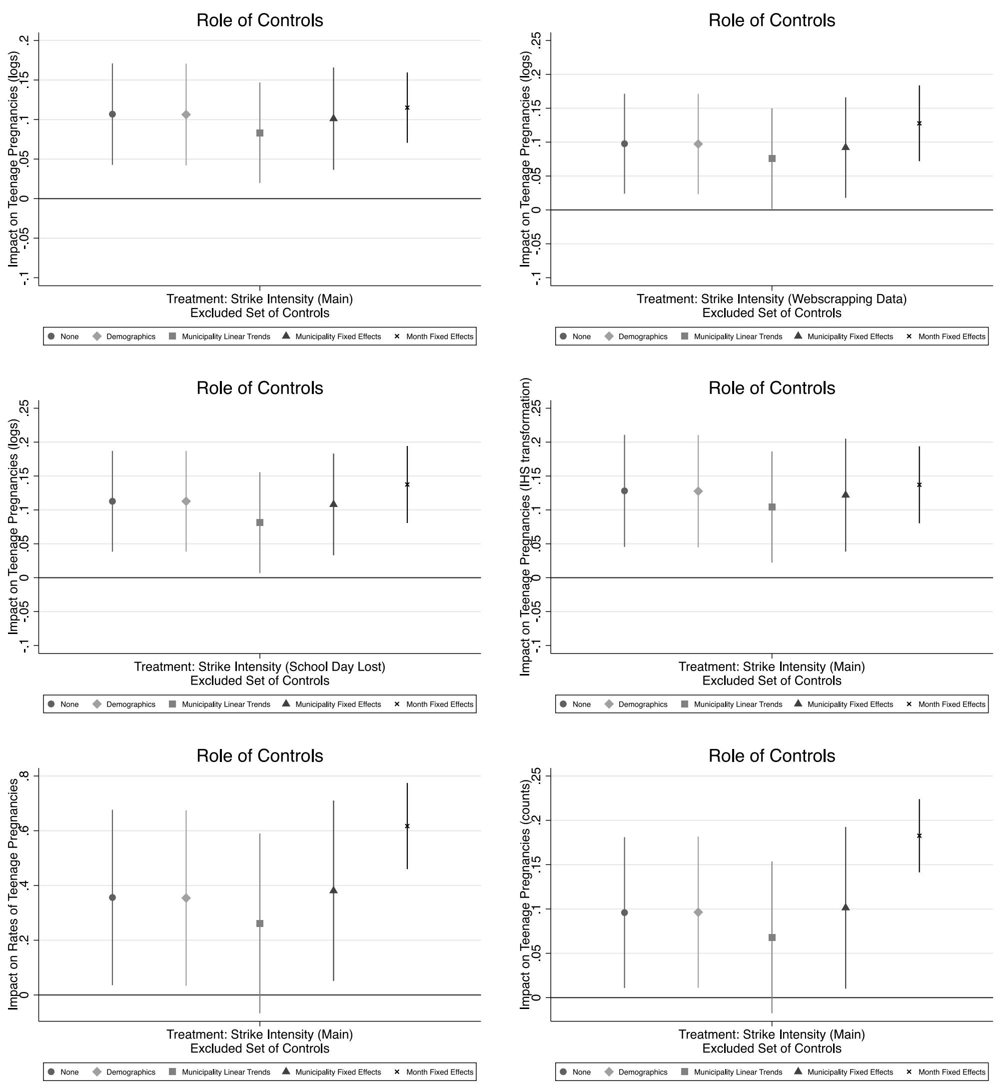

Fig. A.1. Exclusion of controls.

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).

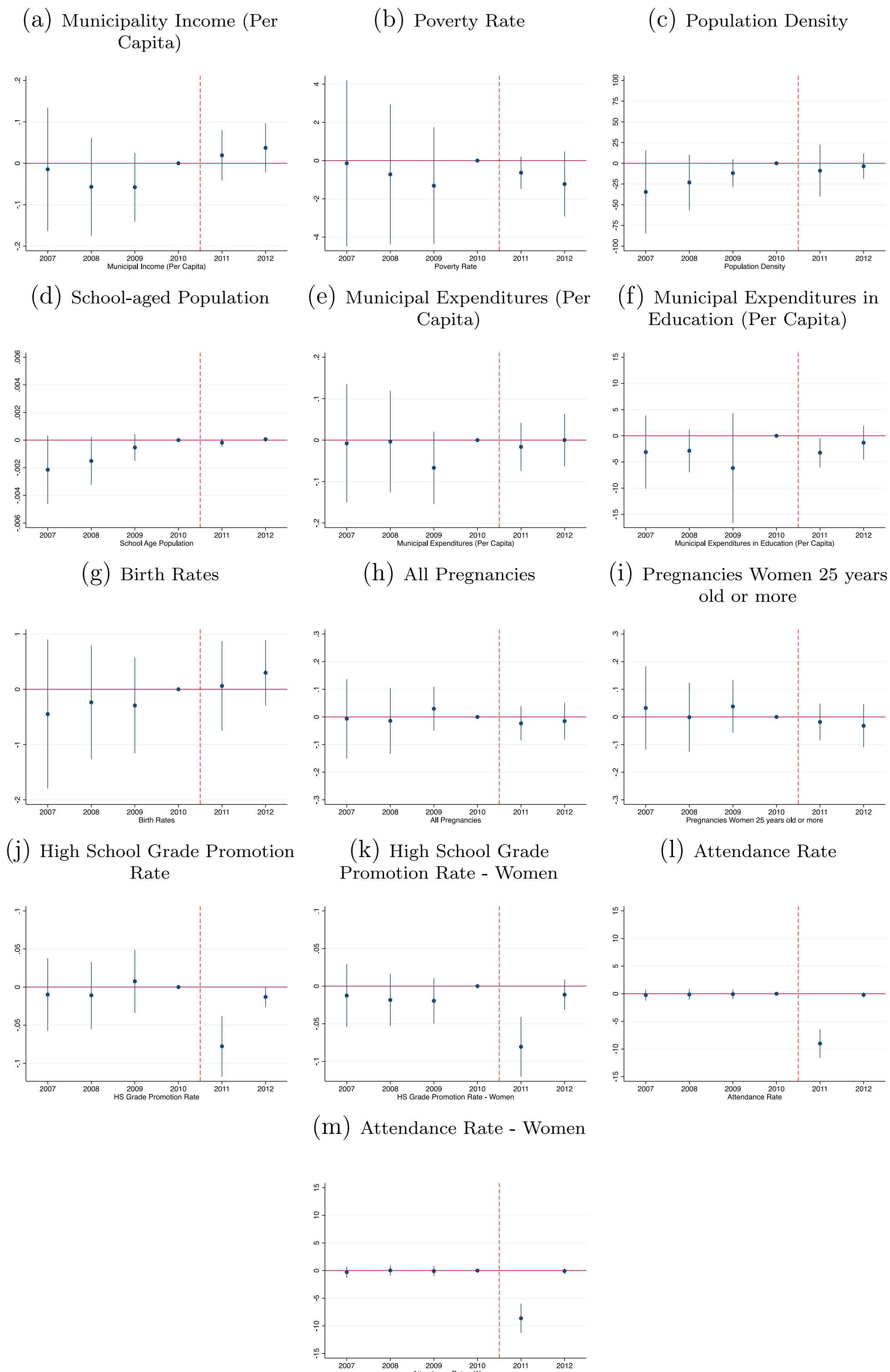

Fig. A.2. Pre-trends analysis.
Notes: Each figure presents the coefficients and 95% confidence intervals for years represented as dummy variables, interacted with a municipality-level strike adherence dummy, covering six years before and after the year of the student protest (i.e., 2011). The coefficient 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.

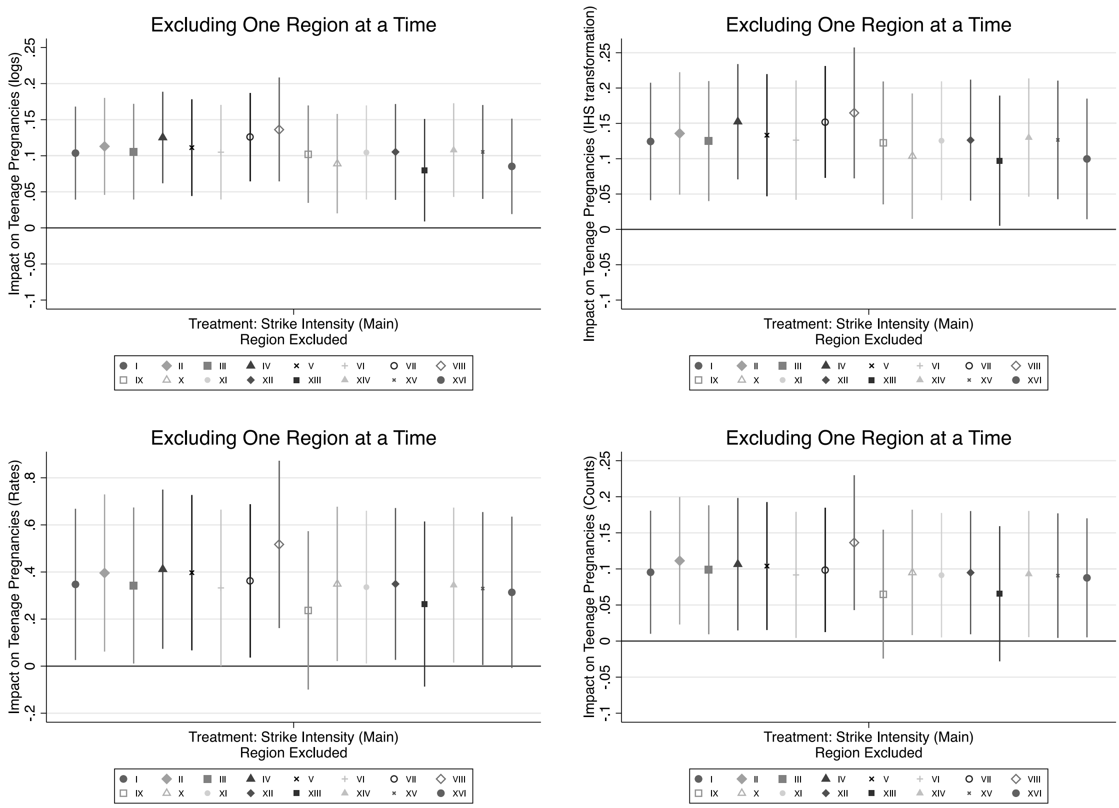

Fig. A.3. Exclusion of regions.
Notes: These figures plot the main point estimate for strike intensity when omitting one Region at a time. Each subfigure plots the main specification for a different dependent variable transformation. All estimations except the one at the bottom right are based on OLS regressions (the exception is based on Poisson regressions).

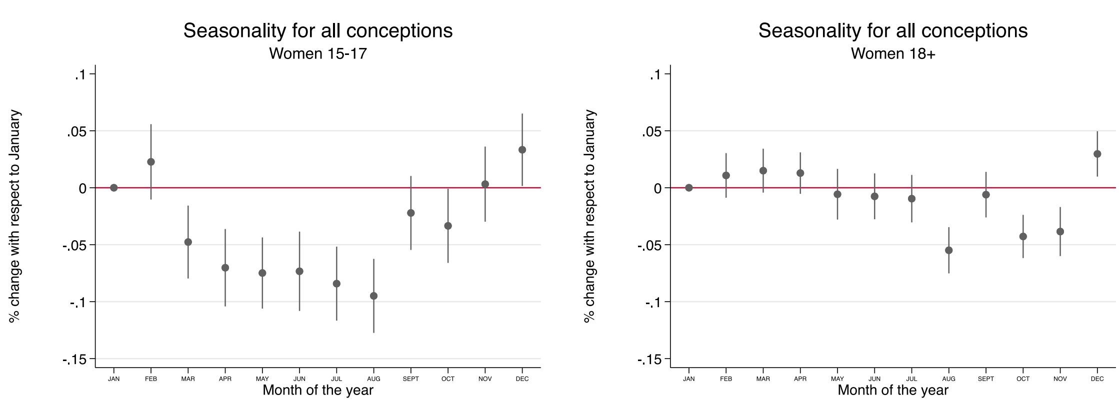

Fig. A.4. Seasonality of conceptions for different age groups (years 2007–2010).
Notes: These figures explore the seasonality in conceptions by separately running a regression of the logarithm of per-day pregnancies for each age group on a set of dummies for each month of the year, with January as a 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.

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 Eq. (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 Eq. (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)	(2)	(3)	(4)	(5)	(6)
	Pooled	Female	Pooled	Female	Pooled	Female
Strike × 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 × 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 × 2010	–	–	–	–	–	–
	–	–	–	–	–	–
Strike × 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 × 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 × 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 × 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 Eq. (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)	(2)	(3)	(4)	(5)	(6)
	Pooled	Female	Pooled	Female	Pooled	Female
Strike × 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 × 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 × 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 × 2010	–	–	–	–	–	–
	–	–	–	–	–	–
Strike × 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 × 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 × 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 × 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 Eq. (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. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Appendix B. Supplementary material

Supplementary material related to this article can be found online at https://doi.org/10.1016/j.jdeveco.2024.103274. The supplementary material corresponds to Appendix B, which includes additional supporting figures, tables, description of variables, and a complementary discussion on general issues on measurement error.

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