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

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Abstract

We empirically study the impact of massive and sudden school closures on teenage pregnancy, following the 2011 nationwide student strike in Chile. Temporary high schools' shutdown increases teenage pregnancies in 1.5% on average, while places in the highest tercile of strike exposure experienced an increase of 5%. This effect vanishes three quarters after the strike's onset and is entirely driven by first-time mothers. The sudden and unexpected closure of schools allows interpreting these findings as mirroring an incapacitation effect of schools rather than human capital accumulation as a mechanism for the causal relationship between students' strikes and teenage pregnancies.

Keywords: Teenage Pregnancy, Risky Behaviour, Student Protests, Incapacitation Effect
JEL: J13, I12, I2, D17

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1 Introduction

Risky behavior during youth (e.g., criminal activity, drug abuse, unprotected sex) can have enormous costs in later life outcomes (e.g., Gruber, 2001). Unprotected sex among teenagers, in particular, can lead to a disproportionately large fraction of HIV infections and unintended pregnancies among the youth (Chandra-Mouli et al., 2014; Dupas, 2011;Widman et al., 2016). Governments spend a large amount of resources to reduce the deleterious consequences from teenage pregnancy. One example is the school system. Schools impose time constraints reducing the time available to engage in risky behaviour– an incapacitation effect;[1] and also educate the young about the costs associated to risky actions. Both mechanisms, an incapacitation effect and human capital accumulation, 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.

While quantifying the impact of the aforementioned mechanisms is relevant to inform policy makers, identifying each of them separately remains a crucial empirical challenge in the literature. In this paper, we aim to fill these gaps by providing empirical evidence for the crucial role of school incapacitation on teenage pregnancies. To do so, we exploit high-frequency panel data variation in conceptions by women at school age and school's closure intensity across Chilean municipalities in 2011 following a national student strike across the country.

We first construct time-invariant municipality-level measures of exposure to the 2011 student strike by combining administrative, survey, and web-scrapped data. Specifically, we identify schools on strike and then compute, at the municipality level, the proportion of female students who were enrolled in those schools. We then interact this time-invariant municipality-level strike exposure with time variation in the extensive margin of the student national movement to exploit panel data variation in our strike intensity treatment.

The 2011 student mobilizations and strikes constitute a meaningful source of quasi-experimental variation to identify and quantify the causal response of teenage pregnancy to schools becoming suddenly inoperative for several reasons. First, the six-month long nationwide movement provides sufficient statistical power to detect even minor effects on pregnancies. Second, there is substantial spatial and time variation in sudden school closures across Chilean municipalities. Third, the sudden student strikes were arguably unexpected

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

by parents thus not allowing them to properly respond to mitigate the potential impact of massive school closures. Fourth, while the strike adherence of a school (in a given municipality) depended of 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 enrolment decisions for female students. Importantly, these enrolment decisions were made by parents years before the student strikes and, in many cases, involving schools outside their municipality of residence. Indeed, a substantial amount of the treatment variation for a given municipality depends on strike actions taken in schools outside that municipality. In support of these arguments, we show that municipalities' adherence to the student strike is not correlated to several key covariates one year before the start of the protests.

Comparing monthly conception rates across municipalities with different strike exposure rates we find that in municipalities where 15% (country's average) of its female high school age population were enrolled in a school on strike, teenage pregnancy increased in 1.5%. This effect corresponds to approximately 230 additional births from teenage mothers during the months of the protests; similar in magnitude (opposite in sign) to the effects from school expansion policies in Chile and other countries (Berthelon and Kruger, 2011; Black et al., 2008).

Different pieces of evidence are consistent with the hypothesis that these effects are driven by a sudden decrease in time spent under adult supervision. The main results are driven by first time mothers and couples where mother and father are both between 15 to 17 years old. Furthermore, the timing variation of the effects show that periods of higher school absenteeism coincide with periods of higher conceptions in municipalities with higher strike exposure. Likewise a seasonality analysis shows that the results found are similar to the change in conceptions during December, the first month of summer holidays when high school teenagers are most likely to spend more time unsupervised by adults than any other month of the year in Chile. Our analysis also shows that the effects are lower in municipalities with a higher teenage pregnancy at baseline, with a larger proportion of students attending COED schools as opposed to single sex schools, and in municipalities that do not have a University campus within its limits. We also find a slight increase in overall demand for Emergency Contraception in municipalities with lager strike exposure. All these analyses are consistent with new conceptions being the consequence of risky choices amongst teenagers under loosen adult supervisions due to school closures rather than inter temporal substitution of fertility choices or effects of strike adherence on more intense social interaction of students.

While we provide evidence for the plausibility of a key identification assumption (i.e., "parallel-trend" assumption), we also present additional tests to support a causal interpretation for our findings. First, we show that strike intensity has no effect on pregnancies for women who are most likely out of high-school (i.e., 18 years or older). Second, we run a simple falsification test by assigning a "fake treatment" (i.e.,the cross-sectional component of our measure of strike intensity) one year before the strikes, and find no statistical relationship between the "fake treatment" and our outcome of interest. We acknowledge that our strike intensity measure may contain some degree of measurement error thus we cannot dismiss a potential downward bias in our estimates. Moreover, attenuation bias could be particularly important given that we exploit a panel data setting wherein fixed effects at both the municipality and month level are included.[2] Therefore, point estimates likely represent a lower bound of the true causal effect of the school incapacitation effect on teenage pregnancies. To address this issue we follow Black et al. (2000) and use two alternative measures for strike intensity to show that the effects are consistent across measures and also present upper bounds of the true effect using one measure as an instrumental variable for the other.

We make several contributions to the existing literature of risky behavior in youth. 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 extended the length of the school day permanently (e.g., Berthelon and Kruger, 2011; Black et al., 2008; McCrary and Royer, 2011). This posits a challenge for the simple reason that these types of policies may affect the potential scope of an incapacitation effect as well as the accumulation of human capital in a, somehow, mechanic and substantial way.[3] Another empirical challenge relates to data limitations. Specifically, previous research has exploited relatively low frequency variation (i.e., yearly data) in both teenage pregnancies and the intensive margin of the time spent at schools. This low frequency variation exploited may hinder causal identification due to cross-sectional unit-specific omitted variables that may substantially vary at a higher time frequency (e.g., within a year).

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

[2]It can be shown that under the case where measurement error is serially uncorrelated, using a fixed effect model might increase the variance of the measurement error while it might reduce the variance of the signal thus worsening the original attenuation bias.

[3]For instance, in the case of Chile, the number of hours of education supplied increased by more than 20% when the full-day school reform was gradually introduced in 1997. Further, these types of reforms, as was indeed the case of Chile, tend to be jointly implemented with other reforms or legislations. In particular, it is conceivable that these policies also improve the quality and efficiency of the education curriculum.

test whether such phenomena can mitigate the effects of expanding schooling (Berthelon and Kruger, 2011; Black et al., 2008). Finally, among the mechanisms behind the effects of expanding schooling on teenage risky behavior are direct effects of accumulating higher levels of human capital, that change expected returns to this behavior, but also the incapacitation effects that school have on adolescents as time spent in schools is a direct substitute to time spent in other activities, such as those considered as risky (Anderson, 2014). We contribute to this debate by studying sudden and momentary school closures in a time window of approximately six months which makes it unlikely that the effects found on teenage pregnancy are due to lower human capital. 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 effect of schools on teenage pregnancy, which is considered as being harmful along the life course of both teen mothers and fathers (Dahl, 2010).

The paper is organized as follows. In section 2 we detail the data we work with and the definitions of strike intensity and teenage pregnancy. In section 3 we explain the context of the school strikes in Chile, present descriptive data of school absenteeism and teenage pregnancy, and characterize municipalities along our measures of strike adherence. 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, discuss measurement issues, and explore different heterogeneous effects. In section 6 we conclude.

2 Data and Measurement

2.1 Teenage Pregnancy

The main dependent variable in our analysis is monthly number of teenage pregnancies conceived in a municipality.[4] We use administrative data of all births and official fetal deaths in Chile provided by the Ministry of Health of Chile (MINSAL). These administrative data provides information about every pregnancy of the country besides non-institutional abortions.[5] For each observation we observe the age of each woman, date of birth, and weeks pregnant at birth and infer conception date of each pregnancy substracting weeks pregnant

[4]Our unit of analysis is a *comuna* or municipality which is the smallest administrative subdivision in Chile. Each municipality is governed by a mayor and a local council, while its administration is ran by a municipality which, according to Chilean law, may administer more than one *comunas*. However, there is only one case in which a municipality manages more than one *comuna*. Therefore, we use municipality as a term to refer to *comunas*.

[5]Abortion was not legal in Chile during this period of analysis.

at birth from birth date or date of fetal death. Since our interest is in female students at high school ages we define teenage pregnancy as pregnancies conceived by women at ages of 15 - 17 years old at time of birth or fetal death. [6] The data also contain information of birth outcomes such as birth weight and Apgar score, mother's municipality of residence, father's age when the father is identified, occupation of mother and father, level of education of mother and father, and number of children, among other variables. We aggregated individual birth records to the municipality x month of conception level for a final dataset containing the count of pregnancies for different age groups conceived in a municipality during a calendar month over years 2007 to 2011.

2.2 Strike Intensity

Measuring school strike adherence at the municipality level posits an empirical challenge. While there are no official records of strike adherence, we leverage on two sources of information to classify the universe of Chilean schools as being on strike or not in 2011. The first information is based on administrative records of daily assistance for all students in public schools in Chile provided by the Ministry of Education (MINEDUC) and available for years 2011 onwards. Using these micro data in 2011 we compute monthly school-level average days lost by high school students (9th to 12th grade). We then classify schools as being on strike if the average high school student in that school lost 10 or more days in August 2011. We focus on August 2011 for two reasons. First, August is a month of full school potential attendance without holidays or vacations. Second, the student movement peaked (in terms of adherence) in August 2011. Results are robust to using 5 days of school days lost as the threshold (see Online Appendix Table A.5).

The second classification was constructed after a web search using Wayback Machine® software which allows to search for information stored in expired url addresses. Web scrapping information from blogs written by students during this period, national media, regional and local media, including newspaper and radio coverage, and social networks we classify schools as being on strike if a school is mentioned in any of these sites as taken over by students or closed. We acknowledge that this measure could be subject to "media bias" issues which we discuss further in the descriptive and methods section.

We construct our main treatment variables as:

[6] In Chile, high school is mandatory since year 2003. According to a nationally representative household survey (CASEN 2009), 92% of females at ages 15 to 17 assisted to school in year 2009

$$\textit{Strike Intensity}_{mt}^{k} = \textit{Strike Period}_t \times \textit{Strike Adherence}_m^k \tag{1}$$

That is, $\textit{Strike Intensity}_{mt}^{k}$ is computed 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. $\textit{Strike Period}_t$ 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. $\textit{Strike Adherence}_m^k$ captures cross-sectional variation in the average strike intensity that a municipality experienced. Formally, $\textit{Strike Adherence}_m^k$ is computed as:

$$\textit{Strike Adherence}_m^k = \frac{\sum_{i=1}^{N_m} 1_{i(s)} \textit{School on strike}_s^k}{N_m} \tag{2}$$

Where i, s, and m denote a female student, school, and municipality, respectively. N_m is the total number of female students residing in municipality m whereas $\textit{School on strike}_s^k$ is a binary indicator for whether school s of female student i was on strike according to measure k. Superscript $k = 1, 2$ differentiates the two measures of strike status, according to which information is used: $k = 1$ corresponds to web-scrapped data and $k = 2$ corresponds to school assistance data. 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 important difference given that a quarter of students attend a school outside their municipality. Hence, $\textit{Strike Adherence}_m^k$ is the proportion of female students in municipality m that attended a school on strike according to measure k in year 2011. For some specifications we use a binary indicator for whether municipality m was above the 75th percentile of $\textit{Strike Adherence}_m^k$ distribution.

3 Context and Descriptive Statistics

With hope of influencing policy to reform the educational system in Chile, high school and university students - mainly from non-private institutions - first protested on the streets in May 2011. Approximately 15,000 students protested in different regions of the country few days before President Sebastian Piñera gave the annual presidential speech of May 21st.[7] Two weeks later, on June 1st of 2011, leaders of the movement convened all students of Chile

[7] See this link for more information about the first protest. Accessed on 22/08/2017.

to go on a national strike of schools and universities.[8] By June 25th more than 600 schools adhered to the strike. Strikes usually consisted in students not attending classes, or in some cases taking over school infrastructure and spending day and night inside, forbidding any school activities.[9] The strike became notorious nationally and internationally as well. In that same year a leader of the student movement was selected by Times magazine as one of the most influencing people of year 2011.[10] Strikes continued during and beyond winter school 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 in 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 the month of July. To track how strikes affected school attendance, Figure 1a illustrates the rate of daily absenteeism during year 2011 by type of school (as measured by administrative records): Public and Voucher.[11] During the first months of 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, which is illustrated by a higher increase in absenteeism during this week, and by June 30th daily absenteeism rate increased to approximately 70% in public schools and 20% in voucher schools. The large peak in the first week of July until the end of July correspond to winter breaks. While August should have been a regular school month, the Figure shows that students in public schools had an average absenteeism rate of 60% approximately, which peaks again at the end of August after which it starts to decline slowly until it normalizes to the end of the year.

Figure 1b shows the cross sectional variation in strike adherence of municipalities according to the different measures of strike adherence using school assistance data (Panel A) and web-scrapped data (Panel B). There is large heterogeneity across the country in strike adherence. An average municipality experienced a 15% adherence of resident students using both measures. When we compute adherence for the 75th percentile, Figure 1b shows a

[8] Gonzalez (2018) provides a comprehensive description of the student movement.

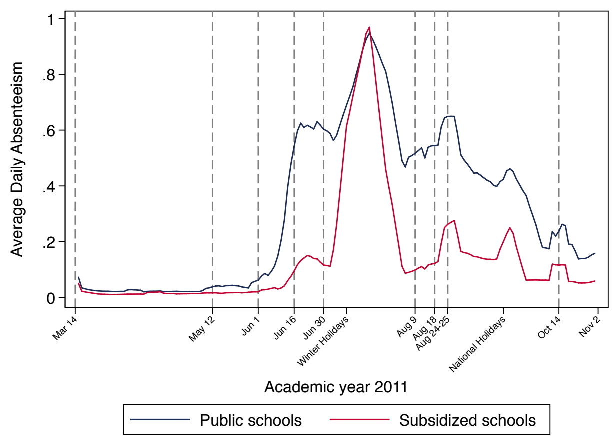
[9] In some cases, municipal authorities with help of the Ministry of Interior used police to force students out of schools. See link, for instance. Accessed on 22/08/2017.

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

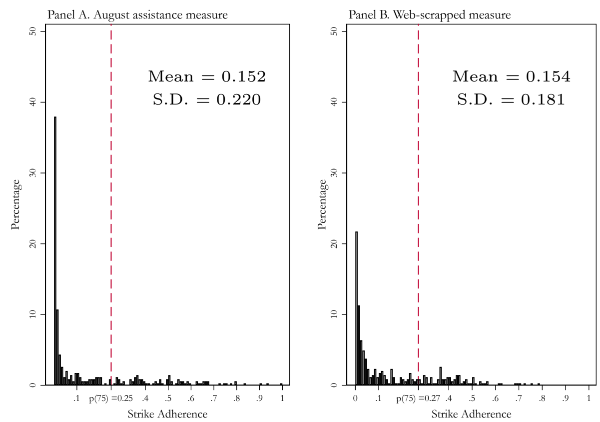
[11] In Chile, schools are roughly divided into Public (45%), Voucher (45%), and Private (10%). We focus on the first two in this paper since Private school adherence to strikes was minor.

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

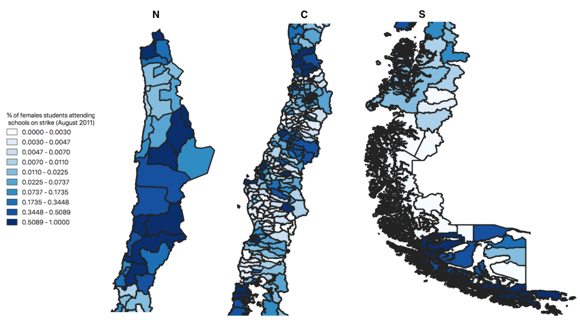
(a) Daily Absenteeism during 2011 by Type of School



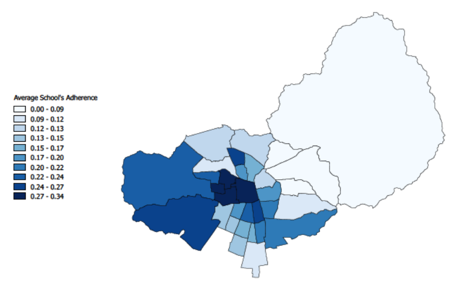
(b) Strike Adherence Distribution



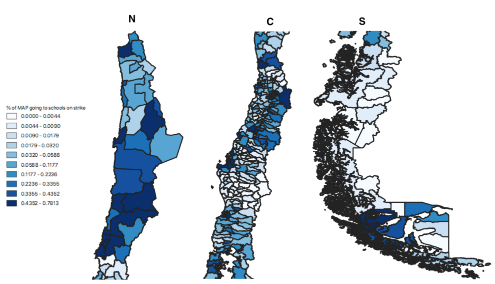
(c) Geographic Dispersion of Strike Adherence using Daily Assistance Data



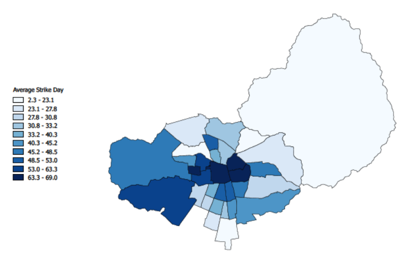
(d) Geographic Dispersion of Strike Adherence using Daily Assistance Data: Metropolitan Region



(e) Geographic Dispersion of Strike Adherence using Web-Scrapped Data



(f) Geographic Dispersion of Strike Adherence using Web-Scrapped Data: Metropolitan Region



Notes: (a) This Figure shows the trends in daily school absenteeism in moving average of 2 days during 2011 by type of school. The blue line represents public schools while the red line are voucher schools. (b) This Figure shows the distribution of each municipality according to the variable of Strike Adherence obtained from measures using assistance data for August (Panel A) and using data from press releases (Panel B) defined in equation 1. The red vertical lines are drawn at the median and at the 75th percentile of the distribution. (c) This Figure shows the geographic distribution of strike adherence using daily assistance data where we define strike adherence as the proportion of female students in a municipality that attend a school that lost 10 days or more of school days in August. (d) This Figure shows the geographic distribution of strike adherence using daily assistance data for the Metropolitan Region in Chile. (e) This Figure shows the geographic distribution of strike adherence using web scrapping data where we define strike adherence as the proportion of female students in a municipality that attend a school mentioned as being on strike. (f) This Figure shows the geographic distribution of strike adherence using web scrapping data for the Metropolitan Region in Chile.

strike adherence of 25% or more when using data from daily assistance while this number is 27% using our measure obtained from web scrapping. Even though distributions look alike between the two measures the composition of schools may differ importantly if classification of schools' strike status is sensible on the measure that we use. In particular using daily assistance data may induce false positive classification error, i.e. schools that did not go on strike but are classified as being on strike due to the arbitrary rule of 10 school days lost by the average student during the month of August. On the other hand, classification based on web-scrapped data is necessarily subject to false negative error, i.e. schools are classified as being on strike if they are mentioned in any of the many sources that we used. In the econometric analyses both of them are used to address measurement error.

Another concern is that the strike was concentrated in particular municipalities, for instance those closer to the capital of the country, Santiago. In Figures 1c to 1f we depict municipalities in the country according to both strike adherence measures. We divide the country into North, Central, South zone, and the Metropolitan Region. Municipalities painted darker experienced a higher strike adherence. The Figures show that the strike was distributed similarly across the territory with municipalities experiencing low and high intensity in different locations of the country, which mitigates out concerns of geographical selection. Also, differences between the two panels reflects that the measures of strike adherence might be composed by different schools.

Finally, we characterize Chilean municipalities along several key variables that may potentially correlate with our measures of strike adherence and teenage pregnancy. To do so, we conduct balance tests in which we look at a set of 37 municipality-level characteristics during pre-treatment period (i.e., year 2010, or the most recent available one in pre-2011 period) including demographics, educational outcomes, municipality resources, fertility outcomes, and prevalence of contraceptive methods.[12] Specifically, we run OLS regressions of each characteristic on one of our four definitions for strike adherence (i.e, based on daily assistance data or web-scrapped press releases data) and 15 region dummies to ensure that the comparison in the covariates is made between municipalities within the same region. We focus on the continuous measures of strike adherence as well as on their discrete versions based on median values.

The results are reported in Tables A.1, A.2, A.3, and A.4. Since we run regressions on multiple outcomes, we also report False Discovery Rate (FDR) adjusted p-values following Anderson (2008). Regardless of the strike adherence measure we use, municipality charac-

[12]See section A.2 for description of variables.

teristics in the pre-strike period are largely balanced along demographic characteristics such as poverty, per capita household income, and school-age population or local government expenditures and investment in education (both in levels and changes from previous year). An exception is population density: municipalities with higher levels of strike adherence also display higher population density. Unsurprisingly, these municipalities also present higher levels of (log) pregnancies. Nonetheless, these differences vanish if we control for population counts in our tests. In fact, we do not find any statistical difference in birth rates (neither in levels nor in changes). Further, there are no statistical differences in the growth of pregnancies during the pre-treatment period which we interpret as supportive evidence for the parallel-trends assumption in our key econometric exercise.

Municipalities with higher strike adherence do not display differences on key school indicators such as attendance, grade promotion or dropout rates. The same results hold if we look at the aforementioned rates exclusively for female students or whether we look at levels in 2010 or their differences from previous year. Importantly, our strike adherence measures do not statistically predict differences in the prevalence of contraceptive methods among adolescents, both in levels or in changes over time, or access to the emergency contraceptive ("morning after") pill. The only exception is female condom usage but the differences are rather small: the prevalence of this contraceptive method is 0.1 percentage points lower in municipalities with levels of strike adherence above its median. In any case, less than 1 percent of young women in Chile declares female condom usage.

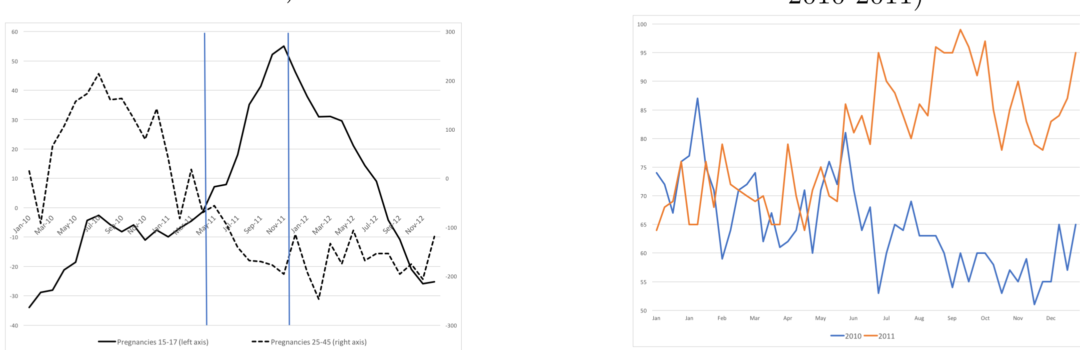
4 Empirical Strategy

Figure 2a plots the monthly evolution of pregnancies conceived each month for 15 - 17 year old women (solid line with levels in the left vertical axis) and 25 - 45 year old women (dashed line with levels in the right vertical axis). Pregnancies for both age groups show downward trends over our period of analysis so we de-trended the data before plotting lines and constructed 12-month moving averages. Blue vertical lines delimit the period of students' protests, i.e. April-December 2011. Trends unveil a remarkable increase in teenage pregnancy during that period, with a peak in conceptions during November 2011. Roughly, the cumulative increase in teenage pregnancies between the two vertical lines amount to approximately 200 births which represents a 1.3% increase with respect to total teenage pregnancies in year 2010. On the other hand, pregnancies for women in the range 25-45 years old, potentially not affected by the strike do not show the same pattern.[13, 14] The Figure shows that while pregnancies for other age groups did not change around the time of the process, they increased significantly for the group that is most likely affected by school protests. In addition, we explore google trends in the search for the word “embarazada” (i.e., Spanish for *pregnant*) to explore any spikes around the period of the school strike in Chile. We plot the number of searches that include the word pregnant for years 2010 (blue) and 2011 (orange) in Figure 2b. The weekly number of google searches that included the word “pregnant” started to increase noticeably between May and June of year 2011, compared to the same months in 2010, and that the level of search remained at a high level for the rest of the year.

Figure 2: Trends in Number of Conceptions and Google Search of Term ”Pregnant” around the Strike Period

(a) Pregnancies by Age Group (detrended 2010-2013)

(b) Google Trends for Term ”Pregnant” (Chile, 2010-2011)



Notes: (a) This figure shows evolution of pregnancies conceived each month by age groups 15 to 17 (left axis) and 25 - 45 (right axis) for Jan 2010 - Dec 2012. Series were detrended and represent the residual from a regression of pregnancy counts (12 month moving averages) on a linear trend. The period within blue vertical lines corresponds to months of student mobilization (i.e., April-December 2011). (b) This figure shows the weekly evolution of internet searches for the term ”embarazada” (i.e., spanish for pregnant). A value of 100 is the peak popularity for the term in Chile for the period Jan 2010- Dec 2011. A value of 50 means that the term is half as popular.

The spike in conceptions of teenage girls and trends in google search may purely reflect a temporary shock common to all teenage girls in the country and have nothing to do with school protests. To disentangle the relation between exposure to school strikes and teenage pregnancies, we study how trends in the count of teenage pregnancies conceived in a municipality x month change during the school strike period and whether this change can be

[13]We look at this age group in this Figure since a large proportion of women in ages 18-24 are attending college during the strike.

[14]The peak in pregnancies for this age group coincides with the aftermath of the strong earthquake occurred on February 27, 2010. There is evidence of increasing fertility rates resulting from catastrophic events (see, Nobles et al., 2015).

interpreted as a causal effect of school closures. In particular we estimate different specifications of the following econometric model for the 2007-2013 period:

$$ln(Teenage\ Pregnancies_{mt} + 1) = \alpha + \beta Strike\ Intensity^{k}_{mt} + \gamma X_{mt} + \lambda_m + \tau_t + \varepsilon_{mt} \qquad (3)$$

where m and t denote municipality and time (month) of conception, respectively. Our sample consists of municipality-month observations for 345 municipalities over 84 months. Our main dependent variable, $Teenage\ Pregnancies_{mt}$, is the number of children conceived during month t and who were born from teenage mothers residing in municipality m at the moment of birth. We add one to consider municipality x month observations with zero conceptions. Our results are robust to using the inverse hyperbolic sine transformation (see Table A.6). $Strike\ Intensity^{k}_{mt}$ is our main independent variable. The semi-log specification presented in equation (3) facilitates the interpretation of the point estimate for β as a standard semi-elasticity, i.e. a variation in a unit of strike intensity has an effect of $\beta\%$ on teenage pregnancies.

X_{mt} is a vector of controls including municipality-specific linear trends, total pregnancies (in logs) -to account for changes in global fertility rates-, poverty rate, per capita government expenditure (in logs), student population in public schools (in logs), population (in logs), and female population (in logs). β is consistently estimated by OLS if there are no changes in unobserved or uncontrolled variables that are correlated with the variation in the intensity of the strike. These could be, for instance, variables reflecting variations in the supply of prevention programs for teenage pregnancy. The high frequency data exploited allows to control for seasonality in a very granular fashion. Further, we include municipality fixed effects (λ_m) that account for unobserved characteristics that are common within each municipality over time. Month-specific conditions common to all municipalities are controlled for using month of conception fixed effects (τ_t). Likewise, focusing on a small window of time allows controlling for any type of endogeneity problems due to internal migration patterns such as moving to areas with lower strike intensity. We allow the error term ε to be correlated within a municipality.

Another key identifying assumption in estimating (3) is that 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, we explore whether observed trends in teenage pregnancies in periods before the strike are correlated to measures of strike intensity. In particular, we run regression (3) by lagging *Strike Intensity*$^k_{mt}$ by one year. Results in column (5) of Table 1 show that there are no differential trends in the log number of teenage pregnancies across municipalities with different strike adherence, using daily assistance measure *Strike Intensity*$^1_{mt}$. Overall, pre-trend analyses support the assumption that trends in potential teenage pregnancy rate across municipalities are unrelated to variations of strike exposure. In addition, Tables A.1, A.2, A.3, and A.4 show that the strike adherence measures we use are balanced along several municipality demographic characteristics both in levels and changes from previous year.

5 Results

In this section we present estimations of the effect of school strike exposure in a municipality and on teenage pregnancy by exploring average effects, falsification tests, measurement issues from the construction of our measures of strike intensity, and a heterogeneity analysis to shed light on potential mechanisms. In all, our results show that there is a sizable increase in teenage pregnancy during the strike period. Municipalities with an average strike adherence (15% of their student population) experienced an increase of 1.5 - 2.8% in their teenage pregnancy rate. This magnitude is similar to the effect caused by a school policy that rolled out Full Day School schedules across Chile studied by Berthelon and Kruger (2011) and similar in magnitude to the effects found by Black et al. (2008) for compulsory school laws in USA and Norway. Different falsification tests confirm these results and heterogeneity analyses, among other complementary regressions, shed light that loosen adult supervision during the strike period is most likely to be the mechanism behind main effects.

5.1 Main Results

We first present results using *Strike Intensity*$^1_{mt}$, daily assistance measure, as the independent variable in equation (3). Column (1) in Table 1 shows that a municipality with exposure of 100%, i.e. all resident high school students attend a school on strike, experienced a monthly increase of 9.4% in conceptions during the strike period after controlling for month of conception and municipality fixed effects. To ease interpretation: a municipality with an average proportion of students attending a school on strike (15%) experienced an increase of 1.4% in teenage pregnancies.

Column (2) shows that the results remain similar after controlling for municipality poverty rates, the log of per capita government expenditure in education, log of student population in public schools, the log of total population, and the log of total female population. The association between *Strike Intensity*$^1_{mt}$ and teenage pregnancy rates remains virtually unaltered which provides evidence that municipality fixed effects work well to account for differences across municipalities with different strike adherence. Column (3) adds municipality-specific trends by interacting municipality fixed effects with a linear trend in months. The coefficient increases slightly and is more precisely estimated: a municipality that experienced an average rate of exposure to strikes experienced an increase of 2% in teenage pregnancies. Next, we ran the same regression using a binary independent variable, equal to 1 if a municipality is above the $75th$ percentile of strike exposure distribution shown in Panel A of Figure 1b. Results show that municipalities above this threshold experienced and increase of 5% in teenage pregnancies. Column (5) shows the results from a regression that lags treated period for one year as a falsification test that explores whether trends during the pre strike period are related to whether a municipality experiences different strike intensity. The results show that there are no differential pre trends in the log number of teenage pregnancies across municipalities with different strike adherence.

Table 1: Effect of Strike Exposure on Teenage Pregnancy

	Dep. Var.: $ln(1 + Teenage\ Pregnancies_{mt})$				
	(1)	(2)	(3)	(4)	(5)
Strike Intensity^1	0.094**	0.099**	0.133***		
	(0.044)	(0.044)	(0.043)		
Strike Intensity$^1 \geq$75th pct.				0.050***	
				(0.018)	
Strike Intensity^1_$t-1$					-0.002
					(0.049)
Observations	28980	28344	28344	28344	28344
Adjusted R^2	0.076	0.078	0.087	0.087	0.087
Month FE	Yes	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes	Yes
Full Controls	No	Yes	Yes	Yes	Yes
Municipality Linear Trend	No	No	Yes	Yes	Yes

This table reports fixed effects estimates of the effect of strike exposure measures on teenage pregnancies. All specifications include a constant and total pregnancies (in logs) as a control. The full set of controls include poverty rate, per capita government expenditure in education (in logs, per student in public school), student population in public schools (in logs), population (in logs), and female population (in logs). Municipality linear trends are included by interacting municipality fixed effects with a linear trend in months. The unit of observation is a municipality-month (345 municipalities over the period Jan 2007 - Dec 2013). Strike Intensity$^1 \geq$ 75th pct. is a binary variable, equal to 1 if a municipality is above the $75th$ percentile of strike exposure distribution shown in Panel A of Figure 1b. Robust standard errors clustered at the municipality level in parentheses. ***p<0.01, **p<0.05, * p<0.10.

Taken together, results in Table 1 suggest that the relation between strike intensity and teenage pregnancy is robust to different specifications and is likely interpreted as the effect of school strikes on the pregnancy probability of a teenage female student. A simple extrapolation to national levels of a 1.4% increase in teenage pregnancies conceived in an average month and municipality correspond to 230 additional teenage pregnancies with respect to the same period in the previous year.

Next we look at whether the effects shown before are driven by newly pregnant high school age mothers, rather than high school age females that already have one child. If new conceptions are from first time mothers then 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 (1) and (2) in Table 2 show that the effects of strike adherence on teenage pregnancy rate, in the fully controlled regression, are driven entirely by new mothers.

In addition, one concern with our estimates is that they may capture differential changes in pregnancy trends due to other factors. We run the same regression using conception of women of age 18 to 19, they are most likely to be out of school and their fertility choices not affected by the school strike. Results in column (3) shows that there is no association between strike adherence and pregnancy rates for women age 18 to 19 years old. Likewise, teenage pregnancies have been related to adverse birth outcomes (e.g., Conde-Agudelo et al., 2005; Donoso et al., 2014); Smith and Pell, 2001. We run equation (3) changing the dependent variable to the natural logarithm of number of fetal deaths obtained from national registries. The results in column (4) show no significant effects on the number of fetal deaths.[15]

Moreover, the data includes the age of the father if there is a man that recognises the newborn. With these data we form teenage couples if the age of the mother and father is between 15 to 17 years old. If unexpected changes in adult supervision create the opportunity to engage in riskier behavior then this should impact all teenage students not only girls. As such, one would expect that changes in conceptions are being driven by teenage couples rather than couples formed by teenage girls and older males. The results in column (5) shows that an increase from 0 to 100% in strike adherence is associated to an increase in 9% on teenage couples, i.e. a municipality with average adherence experienced an increase in teenage couples of 1.5%, similar in magnitude to the increase in teenage pregnancies.

[15]The sign is negative which indicates that fetal deaths may have even decrease. However, the effect is small and 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 2: Effect of Strike Exposure on Teenage Pregnancy, Number of Teen Couples, and Demand for Emergency Contraception

Dep. Var.:	$ln(1+Teenage\ Pregnancies_{mt})$				$ln(1+\#\ Teen\ Couples_{mt})$	$ln(ECP)_{mt}$
	Preg.=1^{st}	Preg.$\geq 2^{nd}$	Age = 18-19	Fetal Deaths		
	(1)	(2)	(3)	(4)	(5)	(6)
Strike Intensity[1]	0.132***	-0.018	-0.034	-0.006	0.092**	0.154*
	(0.045)	(0.033)	(0.044)	(0.012)	(0.037)	(0.087)
Observations	20472	20472	20472	20700	20472	20472
Adjusted R^2	0.067	0.030	0.088	-0.000	0.014	0.597
Month FE	Y	Y	Y	Y	Y	Y
Municipality FE	Y	Y	Y	Y	Y	Y
Full Controls	Y	Y	Y	Y	Y	Y
Municipality Linear Trend	Y	Y	Y	Y	Y	Y

This table reports fixed effects estimates of the effect of strike exposure measures on teenage pregnancies. All specifications include a constant and total pregnancies (in logs) as a control. The full set of controls include poverty rate, per capita government expenditure in education (in logs, per student in public school), student population in public schools (in logs), population (in logs), and female population (in logs). Municipality linear trends are included by interacting municipality fixed effects with a linear trend in months. The unit of observation is a municipality-month (345 municipalities over the period Jan 2007 - Dec 2013). Strike Intensity[1]$\geq$ 75th pct. is a binary variable, equal to 1 if a municipality is above the $75th$ percentile of strike exposure distribution shown in Panel A of Figure 1b. Robust standard errors clustered at the municipality level in parentheses. *** p<0.01, ** p<0.05, * p<0.10.

Finally, we use data from the *Resúmenes Estadísticos Mensuales* kept by the Ministry of Health that includes the number of emergency contraception pills (ECP) disbursed by public health facilities for each month since year 2009. We construct the log of the number of ECP disbursed in each municipality by counting the total number of pills disbursed by health facilities in the same municipality and estimating equation 3 using this new variable as the outcome variable in the right hand side. The results in column (6) of Table 2 show a positive association between strike adherence and disbursement of pills, however the results are sensible to model specification (see Appendix Table A.12), due in part to measurement error in the dependent variable where we include disbursements of pills to all age groups and cannot identify disbursements made separately to teenagers, young adult or adult women.

5.2 Measurement of school strike: two proxies for school's strike status

One estimation problem in previous results is measurement error in Strike Intensity, a concept for which there are no official records such as which students and schools adhere to the strike or the duration of the strike. So far results shown are computed using the measure of strike adherence constructed from web scrapped data. There could be different reasons why these data is measured with error but most importantly we classify schools as being on strike if the school shows up as being on strike or taken over in our web search. As such, there is neccesarily a misclassification problem where schools that were on strike are not being coded in this variable if they do not show up in our historical search; hence by construction there are false negative coded in this binary indicator. Classification error - wrong assignment of strike status to schools - is necessarily non-classical. To illustrate this, consider the following equation that links strike status in equation (2) at the school level to its true status:

$$School\ on\ strike_s^k = School\ on\ strike_s^* + \mu_s^k \quad \text{for } k = 1, 2 \tag{4}$$

In this equation $School\ on\ strike_s^*$ is a binary indicator for true strike status. As Bound et al. (2001) illustrates, measurement error in a binary variable is necessarily non-classical as the covariance between the error and the true measure is negative. In our case if $School\ on\ strike_s^k = 0$ and $School\ on\ strike_s^* = 1$ then $\mu_s^k = -1$; $School\ on\ strike_s^k = 1$ and $School\ on\ strike_s^* = 0$ then $\mu_s^k = 1$. Similar for other combination of values between observed and true value of strike status. This implies that $cov(School\ on\ strike_s^*, \mu_s) < 0$ and so measurement error presents additional challenges to estimation than the classical error-in-variables model where $\text{cov}(\textit{School on strike}_s^*, \mu_s^k) = 0$. Aggregating equation (4) to the municipality level to construct $\textit{Strike Adherence}_m^k$ in equation (2) aggregates measurement error μ_s^k to the municipality level as well. Hence, the new relation between municipality true strike adherence and measured adherence is given by:

$$\textit{Strike Adherence}_m^k = \textit{Strike Adherence}_m^* + \psi_m^k \quad \text{for } k = 1, 2 \tag{5}$$

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

In this section we follow and adapt methods in Black et al. (2000) to bound effects of Strike Intensity on teenage pregnancy. They show that under this structure of measurement error and plausible assumptions, using one proxy measure as an instrumental variable for another proxy, estimates an upper bound of the coefficient of interest.[16] To do this we use $\textit{Strike Intensity}_m^2$ as an alternative classification of a school strike status, obtained from daily assistance data from the month of August where each school is coded as being on strike if the average student lost 10 days of school in that month. All results remain robust to using a threshold of five days (see A.1). One important consideration when using this method is that classification error in both measures of a school's strike status rises from our own coding errors in the construction of the concept of strike. It is plausible then to assume that misclassification error of strike status, μ_s^k, is independent of the probability that a student becomes pregnant, our main outcome of interest. This is an important assumption that is unlikely to hold in other settings, such as response error in survey data (Bollinger and David, 1997), and allows, among other assumptions, to construct bounds proposed by Black et al. (2000).

We revise the main results using $\textit{Strike Intensity}_m^2$, constructed from daily assistance data, as an instrument for $\textit{Strike Intensity}_m^1$. All previous results using $\textit{Strike Intensity}_m^2$ are included in the online Appendix and remain robust. The first column in Table 3 shows that there is a high correlation between the two measures: a 1% increase in $\textit{Strike Intensity}_m^2$ increases $\textit{Strike Intensity}_m^1$ in 0.52%. The F-statistic for $\textit{Strike Intensity}_m^2$ in this regression is 116.5. Columns (2) to (5) show the same specifications as in Table 1 where we show respectively a simple specification including municipality and time fixed effects, then add

[16]see Appendix A.3 for an explanation of this method, discussion of assumptions, and its application to this case.

demographic controls, add municipality linear trends, and re specify the independent variable as a binary indicator that equals to one if the municipality is in the 75th percentile of the distribution of strike adherence.

Table 3: Effect of Strike Exposure on Teenage Pregnancy

Dep. Var.:	*Strike Intensity*^{1} OLS	$ln(1 + \textit{Teenage Pregnancies}_{mt})$ IV Estimation				
	(1)	(2)	(3)	(4)	(5)	(6)
Strike Intensity^2	0.518***					
	(0.048)					
Strike Intensity^1		0.144**	0.148**	0.183**		
		(0.072)	(0.072)	(0.072)		
Strike Intensity^1$\geq$75th pct.					0.061*	
					(0.032)	
Strike Intensity$^2_{t-1}$						-0.035
						(0.075)
Observations	28,344	28,980	28,344	28,344	28,296	28,344
Adjusted R^2	0.659	0.064	0.067	0.076	0.076	0.075
Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes
Full Controls	Yes	No	No	Yes	Yes	Yes
Municipality Linear Trend	Yes	No	Yes	Yes	Yes	Yes

This table reports fixed effects estimates of the effect of strike exposure measures on teenage pregnancies. *Strike Intensity*^{1} refers to the measure of Strike Intensity using webscrapping data. *Strike Intensity*$^1 \geq$ 75th pct. is a binary variable, equal to 1 if a municipality is above the $75th$ percentile in the cross sectional distribution of *Strike Intensity*^{1}. Column (1) shows the correlation coefficient of *Strike Intensity*^{1} and *Strike Intensity*^{2}. Column (2) shows the effect of *Strike Intensity*^{1} on teenage pregnancies using *Strike Intensity*^{2} as an instrument to correct for measurement error. Columns (3) to (5) run the same regressions suing different specifications. Column (6) is a placebo test where we instrument *Strike Intensity*$^1_{t-1}$ with *Strike Intensity*$^2_{t-1}$. All specifications include a constant and total pregnancies (in logs) as a control. The full set of controls include poverty rate, per capita government expenditure in education (in logs, per student in public school), student population in public schools (in logs), population (in logs), and female population (in logs). Municipality linear trends are included by interacting municipality fixed effects with a linear trend in months. The unit of observation is a municipality-month (345 municipalities over the period Jan 2007 - Dec 2013). Robust standard errors clustered at the municipality level in parentheses. ***p<0.01, **p<0.05, *p<0.10.

Under assumptions in Black et al. (2000), estimates of equation (3) through OLS (see Table 1) should be biased toward zero and IV estimates shown in Table 3 should represent an upper bound of the effect of school strikes on teenage pregnancy rates. In fact the IV estimates are larger in every case. Column (5), shows that a municipality with an average strike exposure experienced a 2.8% increase in the teenage pregnancy rate during the strike period, bounding the effect between 1.4% - 2.8% for average exposure municipalities taking coefficients from the fully controlled version. Column (5) in the Table 3 shows that municipalities in the 75th percentile of the strike distribution experienced an increase of 6.1% in the teenage pregnancy rate, obtaining a range of effects between 5 - 6.1% for this group of municipalities. The last column shows IV estimates of the falsification test where we lag the strike period by one year. The null effects hold after instrumenting.

These estimates are obtained after several assumptions including independence of ψ_m^k (or μ_s^k) and ε_{mt}. However, the fact that the estimates are consistent with a lower and upper bound for *Strike Intensity*$_m^*$ provides empirical support for the assumptions. In particular independence of ε_{mt} from ψ_m^k is highly plausible as we argue that measurement error in this case is mainly driven by researchers' construction of the concept of a strike which is likely to be independent of whether a particular student in the data becomes pregnant.[17]

5.3 Heterogeneity of the effects of Strikes on Teenage Pregnancy

Having discussed measurement issues of strike intensity and its consequences, in this section we explore heterogenous effects of the strike on teenage pregnancy to discuss plausible mechanisms behind school closures and teenage conceptions. We present OLS and IV results in each case using *Strike Intensity*$_m^1$ as our measure of interest and interpret these as a lower and upper bound of the true effect of school closures during the strike period.

Timing of events.- We explore whether the effect of strike adherence on teenage conceptions follow a similar pattern as school assistance shown in Figure 1a. This would support the interpretation that the effects found are due to sudden school closures represented by the large rate of absenteeism. To do this, we allow for *Strike Period*$_t$ to vary bi-monthly between April and November of year 2011. Table 4 shows there are no changes in conceptions during the first months which coincides with a period of high or normal assistance as shown in Figure 1a. The next row shows the coefficients for the period of June-July where the effects increase to 0.21 - 0.51 which corresponds to a 3% - 8% in teenage pregnancies for a municipality exposed to an average adherence of strike intensity. The large effect decrease slightly but continue to be high in the period of August - September of 2011, period in which there is still a large rate of absenteeism. Finally, as the strike fades away in the last months of the year changes in birth conceptions are unrelated with municipality's strike adherence. The same pattern is reflected in columns (3) and (4) when we use the binary indicator of strike adherence.

[17] This is probably one of the most sensible assumptions since a small correlation between ε_{mt} and ψ_m^k can make previous estimations fail.

Table 4: Effect of Strike Exposure on Teenage Pregnancy: Timing differences

	Dep. Var.: *ln(1 + Teenage Pregnancies$_{mt}$)*			
	OLS	IV	OLS	IV
	(1)	(2)	(3)	(4)
Strike Intensity[1] x Apr-May	-0.018	-0.014		
	(0.067)	(0.112)		
Strike Intensity[1] x Jun-Jul	0.207***	0.505***		
	(0.074)	(0.137)		
Strike Intensity[1] x Aug-Sep	0.193**	0.243**		
	(0.077)	(0.118)		
Strike Intensity[1] x Oct-Nov	-0.043	-0.035		
	(0.077)	(0.126)		
Strike Intensity[1] ≥75th pct. x Apr-May			-0.008	-0.007
			(0.036)	(0.060)
Strike Intensity[1] ≥75th pct. x Jun-Jul			0.094**	0.211***
			(0.039)	(0.071)
Strike Intensity[1] ≥75th pct. x Aug-Sep			0.075*	0.104
			(0.040)	(0.065)
Strike Intensity[1] ≥75th pct. x Oct-Nov			-0.038	-0.005
			(0.040)	(0.063)
Observations	28,296	28,296	28,296	28,296
Adjusted R^2	0.087	0.075	0.087	0.075
Month FE	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes
Full Controls	Yes	Yes	Yes	Yes
Municipality Linear Trend	Yes	Yes	Yes	Yes

This table reports fixed effects estimates of the effect of strike exposure measures on teenage pregnancies. All specifications include a constant and total pregnancies (in logs) as a control. The full set of controls include poverty rate, per capita government expenditure in education (in logs, per student in public school), student population in public schools (in logs), population (in logs), and female population (in logs). Municipality linear trends are included by interacting municipality fixed effects with a linear trend in months. The unit of observation is a municipality-month (345 municipalities over the period Jan 2007 - Dec 2013). Strike Intensity[1]≥ 75th pct. is a binary variable, equal to 1 if a municipality is above the $75th$ percentile of strike exposure distribution shown in Panel A of Figure 1b. Robust standard errors clustered at the municipality level in parentheses. ***p<0.01, **p<0.05, *p<0.10

One concern with these results is that teenage pregnancy is seasonal (e.g., Buckles and Hungerman, 2013). How do these estimates compare to seasonality of teenage pregnancy in Chile? July is a period of holidays so that this effect might be capturing effects of school closures due to the holiday season rather than school closures due to strikes. However, the identification of the effect is in deviations of every July for months in previous years 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 is not confounding a seasonality effect. To explore seasonality we use teenage conceptions in years 2007 to 2010 and run a regression of the logarithm of teenage pregnancies on a dummy for each month of the year with January as a base group pooling all years and including year and municipality fixed effects. We plot coefficients for each month in the first panel of Figure A.1. The results show that teenage conceptions peak in December, month in which adult supervision is typically more lax as schools end typically at the end of November and parents are more likely to be at work. This pattern is very different than conceptions of women in other age groups (see other panels of Figure A.1).

Social Norms.- Previous studies have found that teenagers' risky actions are sensible 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 are larger in municipalities with higher teenage pregnancy rates at baseline years. In our analyses we separate the sample of municipalities in two groups: municipalities above the national median and below the national median of teenage pregnancies in year 2010. Results are shown in Table 5. While both groups show a positive direction of the effects of strike exposure on teenage conceptions, the lower and upper bound of the effects are more robust in the group of municipalities with lower teenage pregnancy at baseline years. We consider these results as suggestive and they indicate that social norms proxy by teenage pregnancy rates before strikes are not associated to post strike effects on teenage pregnancy rates.

Partner search costs.- During strikes, students are spending more time at home rather than in schools although some students are in schools unsupervised by adults. To look at how the effects vary by the probability of finding peers or sexual partners we first study if the effects vary with population size of the municipality and explore differential effects for municipalities above and below the median of population size. The results show that there is not a clear pattern of effects in municipalities of different population size. Likewise, students that attend co-educational schools as opposed to single sex schools may confront lower search costs of a sexual partner. If these costs reduce the probability of having (unprotected) sex at any period of time then the results should be higher for students who attend co-education schools compared to those who attend single sex schools. To test for this we construct the percentage of students in a municipality that attend a co-educational school and test for differential effects for municipalities above and below the median of the proportion of students in co-educational schools. The results show that estimates are more robust in the group of municipalities with a lower proportion of students assisting co-education schools which is consistent with students finding sex partners outside schools.

Next we look at differential effects in municipalities that have a college campus within their territory and municipalities that do not have a college campus. As the strike movement convened college students as well, partners of teenage girls may be college students of nearby college campuses, increasing teenage conceptions in municipalities with a college campus. College students are also less risky and use more contraception methods, so that the results could go either way. We find that positive effects of strike adherence on teenage conceptions are explained by municipalities that do not have a college campus in their territory, consistent with the effects on teenage conceptions being more likely to be the result of unprotected sex by teenagers in high school age.

6 Conclusion

Different studies show that school expansion policies reduce risky behavior among teenagers, which may be explained by time constraints that schools impose on teenagers but also by human capital accumulation, sexual education, or by changing expectations of risky choices. We contribute to this literature by studying how teenage pregnancy changes when schools become suddenly inoperative. We do so by exploiting quasi-experimental variation from a massive student strike movements in Chile that lasted six months, plausibly ruling out pathways related to education and allowing to interpret the effects as being related to less time spent under adult supervision.

We find that school absenteeism has a significant effect on teenage pregnancy. The results are robust to several specifications and falsification tests, and are very similar in magnitude (opposite in sign) to related studies that look at school policy expansions. They are also similar to the seasonal effects of the month of December in a typical year; month in which teenagers are out of school and are more likely to spend time unsupervised by parents. A number of heterogeneity analysis and auxiliary data on emergency contraception disbursement shed light that loosen adult supervision during the strike period is most likely to be the mechanism behind main effects. The findings highlight the potential benefits of policy interventions like sexual education and advising in school or promoting contraception access amongst teenagers. Such interventions appear to be particularly relevant in the eve of school closing.

Table 5: Effect of Strike Exposure on Teenage Pregnancy

	Baseline Teen Pregnancy Ratio				Baseline Population Size			
	≤50th pctile.		>50th pctile.		≤50th pctile.		>50th pctile.	
	OLS	IV	OLS	IV	OLS	IV	OLS	IV
Strike Intensity[1]	0.137**	0.252**	0.139**	0.162	0.087	0.296*	0.143**	0.105
	(0.067)	(0.102)	(0.060)	(0.112)	(0.079)	(0.167)	(0.056)	(0.081)
Observations	13500	13500	13500	13500	13896	13896	14448	14448
Adjusted R^2	0.090	0.078	0.099	0.088	0.076	0.063	0.116	0.105
	Percentage of COED Students				University Campus			
	≤50th pctile.		>50th pctile.		outside Muni.		within Muni.	
	OLS	IV	OLS	IV	OLS	IV	OLS	IV
Strike Intensity[1]	0.153***	0.152*	0.106	0.215	0.119**	0.189*	0.087	0.082
	(0.058)	(0.081)	(0.069)	(0.133)	(0.054)	(0.100)	(0.075)	(0.090)
Observations	14208	14208	14136	14136	22332	22332	6012	6012
Adjusted R^2	0.098	0.087	0.079	0.067	0.078	0.067	0.141	0.130
Month FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Full Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipality Linear Trend	Yes	Yes	Yes	Yes	Yes	Yes	Yes	yes

This table reports fixed effects estimates of the effect of strike exposure measures on teenage pregnancies. All specifications include a constant and total pregnancies (in logs) as a control. The full set of controls include poverty rate, per capita government expenditure in education (in logs, per student in public school), student population in public schools (in logs), population (in logs), and female population (in logs). Municipality linear trends are included by interacting municipality fixed effects with a linear trend in months. The unit of observation is a municipality-month (345 municipalities over the period Jan 2007 - Dec 2013). Strike Intensity[1]≥ 75th pct. is a binary variable, equal to 1 if a municipality is above the *75th* percentile of strike exposure distribution shown in Panel A of Figure 1b. Robust standard errors clustered at the municipality level in parentheses. ***p<0.01, **p<0.05, *p<0.10

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A Appendix

A.1 Balance in Covariates

Table A.1: Balance in Covariates: Strike Adherence Measure 1 (continuous)

	N	Mean	Estimate	Std. Errors	p-value	Anderson's q-value
Population Density per square km	335	819	3,554	757	0.000	0.000
Family income (per capita, 2009)	334	162,743	19,245	41,471	0.643	0.854
% School-age Population	345	0.219	0.002	0.010	0.883	0.962
Poverty Rate in 2009	334	0.123	-0.006	0.016	0.711	0.872
Expenditures (per capita)	345	290.162	-91.795	112.974	0.417	0.697
Δ Expenditures (per capita)	340	12.513	14.194	27.940	0.612	0.854
Education Investment (per capita)	344	2.127	-0.241	1.134	0.832	0.962
Δ Education Investment (per capita)	339	-1.324	3.361	7.571	0.657	0.854
HS Grade Promotion Rate	341	0.916	-0.000	0.014	0.989	0.995
Δ HS Grade Promotion Rate	340	-0.002	0.001	0.011	0.937	0.989
HS Female HS Grade Promotion Rate	341	0.933	-0.009	0.015	0.532	0.809
Δ HS Female HS Grade Promotion Rate	340	0.001	0.014	0.015	0.341	0.697
HS Attendance Rate	341	0.840	-0.000	0.013	0.995	0.995
Δ HS Attendance Rate	340	0.013	0.012	0.015	0.406	0.697
HS Female Attendance Rate	341	0.829	-0.002	0.017	0.886	0.962
Δ HS Female Attendance Rate	340	0.017	-0.018	0.020	0.371	0.697
HS Dropout Rate	343	0.082	-0.003	0.018	0.869	0.962
Δ HS Dropout Rate	341	0.007	0.009	0.010	0.378	0.697
HS Female Dropout Rate	341	0.065	0.018	0.011	0.101	0.275
Δ HS Female Dropout rate	338	0.004	0.008	0.009	0.347	0.697
Birth Rate	345	13.565	2.258	1.091	0.039	0.149
Δ Birth Rate	345	0.165	0.243	0.515	0.637	0.854
Ln Pregnancies by Women 15-17	344	3.024	2.466	0.473	0.000	0.000
Δ Ln Pregnancies by Women 15-17	344	-0.056	-0.092	0.145	0.526	0.809
Ln Total Pregnancies	344	5.574	2.952	0.537	0.000	0.000
Δ Ln Total Pregnancies	344	-0.032	0.092	0.048	0.056	0.193
% Male Condom Usage	345	0.001	0.000	0.000	0.674	0.854
Δ % Male Condom Usage	345	-0.000	-0.000	0.000	0.013	0.084
% Female Condom Usage	345	0.004	-0.001	0.000	0.000	0.001
Δ % Female Condom Usage	345	-0.000	-0.000	0.000	0.422	0.697
% IUD Usage	345	0.039	-0.007	0.003	0.012	0.084
Δ % IUD Usage	345	-0.004	0.001	0.001	0.101	0.275
% Combination Pill Usage	345	0.043	-0.007	0.003	0.022	0.120
Δ % Combination Pill Usage	345	0.001	-0.001	0.000	0.063	0.200
% Progestin-only Pill Usage	345	0.008	-0.002	0.001	0.039	0.149
Δ % Progestin-only Pill Usage	345	0.001	-0.000	0.000	0.036	0.149
Pill Available (Indicator)	344	0.547	-0.293	0.186	0.116	0.295
Distance to Nearest Available Pill	344	10,213	-27,429	30,113	0.363	0.697

This table reports point estimates, robust standard errors, p-values, and False Discovery Rate (FDR) adjusted p-values (Anderson, 2008) for 37 regressions of a covariate (listed at the left) on our continuous measure of strike adherence based on daily assistance data of August 2011. All covariates are measured in most recent pre-treatment year. Δ refers to the change between 2010 and 2009. All estimates are based on OLS regressions using 15 region dummies to ensure that the comparison in the covariates is made between municipalities within the same region.

Table A.2: Balance in Covariates: Strike Adherence Measure 1 (Indicator 1 if above median)

	N	Mean	Estimate	Std. Errors	p-value	Anderson's q-value
Population Density per square km	335	819	1,599	305	0.000	0.000
Family income (per capita, 2009)	334	162,743	-5,149	17,604	0.770	0.944
% School-Age Population	345	0.219	-0.000	0.004	0.973	0.982
Poverty Rate (2009)	334	0.123	-0.001	0.006	0.837	0.973
Expenditures (per capita)	345	290.162	-43.678	47.317	0.357	0.658
Δ Expenditures (per capita)	340	12.513	-6.664	12.267	0.587	0.797
Education Investment (per capita)	344	2.127	0.072	0.547	0.896	0.973
Δ Education Investment (per capita)	339	-1.324	-0.062	2.717	0.982	0.982
HS Grade Promotion Rate	341	0.916	-0.001	0.005	0.857	0.973
Δ HS Grade Promotion Rate	340	-0.002	0.001	0.004	0.759	0.944
HS Female Grade Promotion Rate	341	0.933	-0.006	0.005	0.200	0.443
Δ HS Female Grade Promotion Rate	340	0.001	-0.000	0.006	0.932	0.982
HS Attendance Rate	341	0.840	-0.004	0.005	0.364	0.658
Δ HS Attendance Rate	340	0.013	0.003	0.005	0.521	0.733
HS Female Attendance Rate	341	0.829	-0.004	0.007	0.510	0.733
Δ HS Female Attendance Rate	340	0.017	-0.003	0.007	0.625	0.819
HS Dropout Rate	343	0.082	0.005	0.006	0.413	0.683
Δ HS Dropout Rate	341	0.007	0.006	0.004	0.146	0.404
HS Female Dropout Rate	341	0.065	0.009	0.003	0.012	0.079
Δ HS Female Dropout Rate	338	0.004	0.003	0.004	0.431	0.683
Birth Rate	345	13.565	0.875	0.433	0.044	0.240
Δ Birth Rate	345	0.165	0.227	0.181	0.210	0.443
Ln Pregnancies by Women 15-17	344	3.024	0.892	0.171	0.000	0.000
Δ Ln Pregnancies by Women 15-17	344	-0.056	-0.054	0.051	0.291	0.583
Ln Total Pregnancies	344	5.574	1.030	0.195	0.000	0.000
Δ Ln Total Pregnancies	344	-0.032	0.024	0.017	0.160	0.404
% Male Condom Usage	345	0.001	0.000	0.000	0.893	0.973
Δ % Male Condom Usage	345	-0.000	-0.000	0.000	0.077	0.365
% Female Condom Usage	345	0.004	-0.000	0.000	0.002	0.018
Δ % Female Condom Usage	345	-0.000	-0.000	0.000	0.154	0.404
% IUD Usage	345	0.039	-0.001	0.001	0.425	0.683
Δ % IUD Usage	345	-0.004	0.001	0.000	0.011	0.079
% Combination Pill Usage	345	0.043	-0.002	0.001	0.159	0.404
Δ % Combination Pill Usage	345	0.001	-0.000	0.000	0.178	0.424
% Progestin-only Pill Usage	345	0.008	-0.001	0.000	0.147	0.404
Δ % Progestin-only Pill Usage	345	0.001	-0.000	0.000	0.143	0.404
Pill Available (indicator)	344	0.547	-0.110	0.073	0.131	0.404
Distance to Nearest Available Pill	344	10,213	-6,740	9,292	0.469	0.712

This table reports point estimates, robust standard errors, p-values, and False Discovery Rate (FDR) adjusted p-values (Anderson, 2008) for 37 regressions of a covariate (listed at the left) on an indicator taking value 1 if our continuous measure of strike adherence based on daily assistance data of August 2011 is above its median value, 0 otherwise. All covariates are measured in most recent pre-treatment year. Δ refers to the change between 2010 and 2009. All estimates are based on OLS regressions using 15 region dummies to ensure that the comparison in the covariates is made between municipalities within the same region.

Table A.3: Balance in Covariates: Strike Adherence Measure 2 (continuous)

	N	Mean	Estimate	Std. Errors	p-value	Anderson's q-value
Population Density per square km	334	819	1,508	448	0.001	0.008
Family Income (per capita, 2009)	334	162,743	40,581	27,379	0.139	0.502
% School-age Population	344	0.219	-0.013	0.010	0.200	0.502
Poverty rate (2009)	334	0.123	-0.013	0.012	0.284	0.518
Expenditures (per capita)	344	290.162	77.954	117.150	0.506	0.713
Δ Expenditures (per capita)	339	12.513	42.799	32.953	0.195	0.502
Education Investment (per capita)	343	2.127	1.670	1.190	0.161	0.502
Δ Education Investment (per capita)	338	-1.324	10.342	9.376	0.271	0.518
HS Grade Promotion Rate	341	0.916	0.015	0.012	0.233	0.518
Δ HS Grade Promotion Rate	340	-0.002	0.013	0.012	0.300	0.518
HS Female Grade Promotion Rate	341	0.933	0.019	0.013	0.149	0.502
Δ HS Female Grade Promotion Rate	340	0.001	0.037	0.017	0.035	0.221
HS Attendance Rate	341	0.840	0.003	0.011	0.775	0.818
Δ HS Attendance Rate	340	0.013	0.006	0.012	0.596	0.755
HS Female Attendance Rate	341	0.829	0.005	0.015	0.744	0.808
Δ HS Female Attendance Rate	340	0.017	0.001	0.018	0.961	0.961
HS Dropout Rate	342	0.082	-0.005	0.009	0.558	0.744
Δ HS Dropout Rate	340	0.007	-0.002	0.009	0.814	0.836
HS Female Dropout Rate	341	0.065	-0.010	0.008	0.203	0.502
Δ HS Female Dropout Rate	338	0.004	-0.011	0.010	0.268	0.518
Birth Rate	344	13.565	0.300	0.860	0.727	0.808
Δ Birth Rate	344	0.165	0.301	0.415	0.469	0.686
Ln Pregnancies by Women 15-17	343	3.024	0.993	0.326	0.003	0.019
Δ Ln Pregnancies by Women 15-17	343	-0.056	-0.123	0.098	0.212	0.502
Ln Total Pregnancies	343	5.574	1.366	0.374	0.000	0.006
Δ Ln Total Pregnancies	343	-0.032	0.047	0.038	0.211	0.502
% Male Condom Usage	344	0.001	-0.000	0.000	0.445	0.686
Δ % Male Condom Usage	344	-0.000	-0.000	0.000	0.147	0.502
% Female Condom Usage	344	0.004	-0.001	0.000	0.000	0.003
Δ % Female Condom Usage	344	-0.000	-0.000	0.000	0.655	0.772
% IUD Usage	344	0.039	-0.007	0.002	0.001	0.008
Δ % IUD Usage	344	-0.004	0.001	0.001	0.041	0.221
% Combination Pill Usage	344	0.043	0.001	0.002	0.671	0.772
Δ % Combination Pill Usage	344	0.001	0.000	0.000	0.567	0.744
% Progestin-only Pill Usage	344	0.008	0.001	0.001	0.453	0.686
Δ % Progestin-only Pill Usage	344	0.001	0.000	0.000	0.657	0.772
Pill Available (indicator)	343	0.547	0.143	0.134	0.288	0.518
Distance to Nearest Available Pill	343	10,213	-22,053	24,639	0.371	0.614

This table reports point estimates, robust standard errors, p-values, and False Discovery Rate (FDR) adjusted p-values (Anderson, 2008) for 37 regressions of a covariate (listed at the left) on our continuous measure of strike adherence based on web-scrapped press releases data. All covariates are measured in most recent pre-treatment year. Δ refers to the change between 2010 and 2009. All estimates are based on OLS regressions using 15 region dummies to ensure that the comparison in the covariates is made between municipalities within the same region.

Table A.4: Balance in Covariates: Strike Adherence Measure 2 (Indicator 1 if above median)

	N	Mean	Estimate	Std. Errors	p-value	Anderson's q-value
Population Density per square km	334	819	728	310	0.019	0.148
Family Income (per capita, 2009)	334	162,743	23,616	17,136	0.169	0.576
% School-age Population	344	0.219	-0.006	0.004	0.144	0.576
Poverty Rate (2009)	334	0.123	-0.010	0.006	0.119	0.563
Expenditures (per capita)	344	290.162	-1.828	42.959	0.966	0.972
Δ Expenditures (per capita)	339	12.513	10.826	12.442	0.385	0.609
Education Investment (per capita)	343	2.127	1.171	0.846	0.167	0.576
Δ Education Investment (per capita)	338	-1.324	4.325	3.654	0.237	0.591
HS Grade Promotion Rate	341	0.916	0.005	0.006	0.378	0.609
Δ HS Grade Promotion Rate	340	-0.002	0.006	0.006	0.272	0.591
HS Female Grade Promotion Rate	341	0.933	0.006	0.006	0.318	0.609
Δ HS Female Grade Promotion Rate	340	0.001	0.015	0.007	0.045	0.242
HS Attendance Rate	341	0.840	0.002	0.005	0.697	0.883
Δ HS Attendance Rate	340	0.013	0.006	0.006	0.373	0.609
HS Female Attendance Rate	341	0.829	0.004	0.007	0.559	0.759
Δ HS Female Attendance Rate	340	0.017	0.004	0.008	0.644	0.843
HS Dropout Rate	342	0.082	-0.000	0.004	0.972	0.972
Δ HS Dropout Rate	340	0.007	-0.002	0.004	0.550	0.759
HS Female Dropout Rate	341	0.065	-0.001	0.004	0.849	0.947
Δ HS Female Dropout Rate	338	0.004	-0.005	0.004	0.280	0.591
Birth Rate	344	13.565	0.067	0.374	0.858	0.947
Δ Birth Rate	344	0.165	0.209	0.179	0.243	0.591
Ln Pregnancies by Women 15-17	343	3.024	0.575	0.150	0.000	0.002
Δ Ln Pregnancies by Women 15-17	343	-0.056	-0.036	0.044	0.409	0.621
Ln Total Pregnancies	343	5.574	0.726	0.167	0.000	0.001
Δ Ln Total Pregnancies	343	-0.032	0.011	0.016	0.512	0.749
% Male Condom Usage	344	0.001	-0.000	0.000	0.263	0.591
Δ % Male Condom Usage	344	-0.000	-0.000	0.000	0.182	0.576
% Female Condom Usage	344	0.004	-0.000	0.000	0.000	0.001
Δ % Female Condom Usage	344	-0.000	0.000	0.000	0.897	0.947
% IUD Usage	344	0.039	-0.003	0.001	0.006	0.056
Δ % IUD Usage	344	-0.004	0.001	0.000	0.035	0.223
% Combination pill Usage	344	0.043	-0.000	0.001	0.869	0.947
Δ % Combination Pill Usage	344	0.001	0.000	0.000	0.367	0.609
% Progestin-only Pill Usage	344	0.008	-0.000	0.000	0.892	0.947
Δ % Progestin-only Pill Usage	344	0.001	0.000	0.000	0.858	0.947
Pill Available (indicator)	343	0.547	0.077	0.067	0.249	0.591
Distance to Nearest Available Pill	343	10,213	-10,965	12,121	0.366	0.609

This table reports point estimates, robust standard errors, p-values, and False Discovery Rate (FDR) adjusted p-values (Anderson, 2008) for 37 regressions of a covariate (listed at the left) on an indicator taking value 1 if continuous measure of strike adherence based on web-scrapped press releases data is above its median value, 0 otherwise. All covariates are measured in most recent pre-treatment year. Δ refers to the change between 2010 and 2009. All estimates are based on OLS regressions using 15 region dummies to ensure that the comparison in the covariates is made between municipalities within the same region.

Table A.5: Robustness of Main Results in Table 1 Across Definition of Strike Intensity and Model Specification

	Dep. Var.: $ln(1 + Teenage\ Pregnancies_{mt})$								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Strike Intensity^1	0.094**	0.099**	0.133***						
	(0.044)	(0.044)	(0.043)						
Strike Intensity$^1 \geq$75th pct.				0.035**	0.037**	0.050***			
				(0.018)	(0.018)	(0.018)			
Strike Intensity$^1_{t-1}$							0.028	0.019	-0.002
							(0.047)	(0.049)	(0.049)
Strike Intensity^2	0.074**	0.077**	0.095***						
	(0.036)	(0.036)	(0.036)						
Strike Intensity$^2 \geq$75th pct.				0.024	0.024	0.035*			
				(0.018)	(0.018)	(0.018)			
Strike Intensity$^2_{t-1}$							-0.003	-0.006	-0.018
							(0.038)	(0.040)	(0.039)
Strike Intensity$^2_5$	0.080***	0.082***	0.104***						
	(0.030)	(0.030)	(0.030)						
Strike Intensity$^2_5 \geq$75th pct.				0.039**	0.039**	0.052***			
				(0.018)	(0.018)	(0.018)			
Strike Intensity$^2_{5,t-1}$							0.018	0.015	0.002
							(0.030)	(0.032)	(0.032)
Observations	28980	28344	28344	28980	28344	28344	28980	28344	28344
Adjusted R^2	0.076	0.078	0.087	0.076	0.078	0.087	0.076	0.078	0.087
Month FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Municipality FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Full Controls	N	Y	Y	N	Y	Y	N	Y	Y
Municipality Linear Trend	N	N	Y	N	N	Y	N	N	Y

This table reports fixed effects estimates of the effect of strike exposure measures on teenage pregnancies. All specifications include a constant and total pregnancies (in logs) as a control. The full set of controls include poverty rate, per capita government expenditure in education (in logs, per student in public school), student population in public schools (in logs), population (in logs), and female population (in logs). Municipality linear trends are included by interacting municipality fixed effects with a linear trend in months. The unit of observation is a municipality-month (345 municipalities over the period Jan 2007 - Dec 2013). *Strike Intensity*^{1} refers to the measure of Strike Intensity using webscrapping data, *Strike Intensity*^{1} $\geq$ 75th pct. is a binary variable, equal to 1 if a municipality is above the $75th$ percentile in the cross sectional distribution of *Strike Intensity*^{1} . *Strike Intensity*^{2} refers to the measure of Strike Intensity using school days lost in August and the threshold of 10 days lost. *Strike Intensity*^{2} $\geq$ 75th pct. is a binary variable, equal to 1 if a municipality is above the $75th$ percentile in the cross sectional distribution of *Strike Intensity*^{2}. *Strike Intensity*$^2_5$ refers to the measure of Strike Intensity using school days lost in August and the threshold of 5 days lost. *Strike Intensity*$^2_5$ $\geq$ 75th pct. is a binary variable, equal to 1 if a municipality is above the $75th$ percentile in the cross sectional distribution of *Strike Intensity*$^2_5$. Robust standard errors clustered at the municipality level in parentheses. Subscript $t-1$ lags the period of strike in one year. ***p<0.01, **p<0.05, *p<0.10

Table A.6: Robustness of Main Results in Table 1 using the Inverse Hyperbolic Sine Transformation of the Number of Teenage Pregnancies as the Dependent Variable Across Definition of Strike Intensity and Model Specification

	Dep. Var.: Inverse Hyperbolic Sine Transformation of $TeenagePregnancies_{mt}$)								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Strike Intensity^1	0.144***	0.141**	0.151***						
	(0.055)	(0.055)	(0.056)						
Strike Intensity^1≥75th pct.				0.055**	0.053**	0.058**			
				(0.022)	(0.023)	(0.023)			
Strike Intensity^1_$t-1$							0.050	0.033	-0.092
							(0.065)	(0.069)	(0.074)
Strike Intensity^2	0.116**	0.114**	0.117**						
	(0.048)	(0.048)	(0.049)						
Strike Intensity^2≥75th pct.				0.043*	0.041*	0.043*			
				(0.024)	(0.024)	(0.024)			
Strike Intensity^2_$t-1$							0.012	0.003	-0.054
							(0.053)	(0.055)	(0.062)
Strike Intensity_5^2	0.122***	0.121***	0.125***						
	(0.039)	(0.039)	(0.039)						
Strike Intensity_5^2≥75th pct.				0.061***	0.059***	0.062***			
				(0.023)	(0.023)	(0.023)			
Strike Intensity_5^2_$t-1$							0.042	0.035	-0.035
							(0.042)	(0.044)	(0.051)
Observations	20700	20472	20472	20700	20472	20472	20700	20472	20472
Adjusted R^2	0.064	0.065	0.070	0.064	0.065	0.070	0.064	0.065	0.070
Month FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Municipality FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Full Controls	N	Y	Y	N	Y	Y	N	Y	Y
Municipality Linear Trend	N	N	Y	N	N	Y	N	N	Y

This table reports fixed effects estimates of the effect of strike exposure measures on teenage pregnancies using the Inverse Hyperbolic Sine Transformation on the number of teenage pregnancies as the dependent variable. All specifications include a constant and total pregnancies (in logs) as a control. The full set of controls include poverty rate, per capita government expenditure in education (in logs, per student in public school), student population in public schools (in logs), population (in logs), and female population (in logs). Municipality linear trends are included by interacting municipality fixed effects with a linear trend in months. The unit of observation is a municipality-month (345 municipalities over the period Jan 2007 - Dec 2013). *Strike Intensity*^{1} refers to the measure of Strike Intensity using webscrapping data, *Strike Intensity*^{1} $\geq$ 75th pct. is a binary variable, equal to 1 if a municipality is above the $75th$ percentile in the cross sectional distribution of *Strike Intensity*^{1} . *Strike Intensity*^{2} refers to the measure of Strike Intensity using school days lost in August and the threshold of 10 days lost. *Strike Intensity*^{2} $\geq$ 75th pct. is a binary variable, equal to 1 if a municipality is above the $75th$ percentile in the cross sectional distribution of *Strike Intensity*^{2}. *Strike Intensity*$^2_5$ refers to the measure of Strike Intensity using school days lost in August and the threshold of 5 days lost. *Strike Intensity*$^2_5$ $\geq$ 75th pct. is a binary variable, equal to 1 if a municipality is above the $75th$ percentile in the cross sectional distribution of *Strike Intensity*$^2_5$. Robust standard errors clustered at the municipality level in parentheses. Subscript $t-1$ lags the period of strike in one year. ***p<0.01, **p<0.05, *p<0.10

Table A.7: Robustness of Results on First Pregnancies in Table 2 Across Definition of Strike Intensity and Model Specification

	Dep. Var.: *ln(1 + Teenage First Pregnancies$_{mt}$)*								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Strike Intensity^1	0.100**	0.103**	0.137***						
	(0.045)	(0.045)	(0.045)						
Strike Intensity^1≥75th pct.				0.042**	0.043**	0.057***			
				(0.017)	(0.017)	(0.018)			
Strike Intensity$^1_{t-1}$							0.046	0.036	0.015
							(0.046)	(0.048)	(0.048)
Strike Intensity^2	0.069*	0.071*	0.093**						
	(0.037)	(0.037)	(0.038)						
Strike Intensity^2≥75th pct.				0.023	0.023	0.036*			
				(0.018)	(0.018)	(0.019)			
Strike Intensity$^2_{t-1}$							0.019	0.016	0.001
							(0.037)	(0.040)	(0.039)
Strike Intensity$^2_5$	0.079***	0.080***	0.104***						
	(0.030)	(0.030)	(0.031)						
Strike Intensity$^2_5$≥75th pct.				0.040**	0.040**	0.053***			
				(0.017)	(0.017)	(0.018)			
Strike Intensity$^2_{5,t-1}$							0.032	0.028	0.014
							(0.030)	(0.031)	(0.031)
Observations	28980	28344	28344	28980	28344	28344	28980	28344	28344
Adjusted R^2	0.070	0.072	0.080	0.070	0.072	0.080	0.070	0.071	0.080
Month FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Municipality FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Full Controls	N	Y	Y	N	Y	Y	N	Y	Y
Municipality Linear Trend	N	N	Y	N	N	Y	N	N	Y

This table reports fixed effects estimates of the effect of strike exposure measures on teenage pregnancies for first time pregnancies. All specifications include a constant and total pregnancies (in logs) as a control. The full set of controls include poverty rate, per capita government expenditure in education (in logs, per student in public school), student population in public schools (in logs), population (in logs), and female population (in logs). Municipality linear trends are included by interacting municipality fixed effects with a linear trend in months. The unit of observation is a municipality-month (345 municipalities over the period Jan 2007 - Dec 2013). *Strike Intensity*^{1} refers to the measure of Strike Intensity using webscrapping data, *Strike Intensity*^{1} $\geq$ 75th pct. is a binary variable, equal to 1 if a municipality is above the $75th$ percentile in the cross sectional distribution of *Strike Intensity*^{1} . *Strike Intensity*^{2} refers to the measure of Strike Intensity using school days lost in August and the threshold of 10 days lost. *Strike Intensity*^{2} $\geq$ 75th pct. is a binary variable, equal to 1 if a municipality is above the $75th$ percentile in the cross sectional distribution of *Strike Intensity*^{2}. *Strike Intensity*$^2_5$ refers to the measure of Strike Intensity using school days lost in August and the threshold of 5 days lost. *Strike Intensity*$^2_5$ $\geq$ 75th pct. is a binary variable, equal to 1 if a municipality is above the $75th$ percentile in the cross sectional distribution of *Strike Intensity*$^2_5$. Robust standard errors clustered at the municipality level in parentheses. Subscript $t-1$ lags the period of strike in one year. ***p<0.01, **p<0.05, *p<0.10

Table A.8: Robustness of Results on Second or more Pregnancies in Table 2 Across Definition of Strike Intensity and Model Specification

	Dep. Var.: *ln(1 + Teenage Pregnancies (second or more)$_{mt}$)*								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Strike Intensity^1	-0.059*	-0.058*	-0.023						
	(0.035)	(0.035)	(0.033)						
Strike Intensity$^1 \geq$75th pct.				-0.041***	-0.041***	-0.026*			
				(0.015)	(0.015)	(0.014)			
Strike Intensity$^1_{t-1}$							0.021	0.016	-0.011
							(0.034)	(0.035)	(0.034)
Strike Intensity^2	-0.003	-0.002	0.010						
	(0.026)	(0.026)	(0.026)						
Strike Intensity$^2 \geq$75th pct.				-0.013	-0.014	-0.006			
				(0.014)	(0.014)	(0.013)			
Strike Intensity$^2_{t-1}$							-0.010	-0.013	-0.021
							(0.023)	(0.024)	(0.024)
Strike Intensity$^2_5$	-0.012	-0.011	0.002						
	(0.021)	(0.021)	(0.020)						
Strike Intensity$^2_5 \geq$75th pct.				-0.015	-0.015	-0.007			
				(0.014)	(0.014)	(0.014)			
Strike Intensity$^2_{5,t-1}$							-0.006	-0.009	-0.018
							(0.020)	(0.021)	(0.021)
Observations	28980	28344	28344	28980	28344	28344	28980	28344	28344
Adjusted R^2	0.029	0.031	0.045	0.029	0.031	0.045	0.029	0.031	0.045
Month FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Municipality FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Full Controls	N	Y	Y	N	Y	Y	N	Y	Y
Municipality Linear Trend	N	N	Y	N	N	Y	N	N	Y

This table reports fixed effects estimates of the effect of strike exposure measures on teenage pregnanciese for second time pregnancies or more. All specifications include a constant and total pregnancies (in logs) as a control. The full set of controls include poverty rate, per capita government expenditure in education (in logs, per student in public school), student population in public schools (in logs), population (in logs), and female population (in logs). Municipality linear trends are included by interacting municipality fixed effects with a linear trend in months. The unit of observation is a municipality-month (345 municipalities over the period Jan 2007 - Dec 2013). *Strike Intensity*^{1} refers to the measure of Strike Intensity using webscrapping data, *Strike Intensity*$^1 \geq$ 75th pct. is a binary variable, equal to 1 if a municipality is above the $75th$ percentile in the cross sectional distribution of *Strike Intensity*^{1} . *Strike Intensity*^{2} refers to the measure of Strike Intensity using school days lost in August and the threshold of 10 days lost. *Strike Intensity*$^2 \geq$ 75th pct. is a binary variable, equal to 1 if a municipality is above the $75th$ percentile in the cross sectional distribution of *Strike Intensity*^{2}. *Strike Intensity*$^2_5$ refers to the measure of Strike Intensity using school days lost in August and the threshold of 5 days lost. *Strike Intensity*$^2_5 \geq$ 75th pct. is a binary variable, equal to 1 if a municipality is above the $75th$ percentile in the cross sectional distribution of *Strike Intensity*$^2_5$. Robust standard errors clustered at the municipality level in parentheses. Subscript $t-1$ lags the period of strike in one year. ***p<0.01, **p<0.05, *p<0.10

Table A.9: Robustness of Results on Pregnancies for female students Age 18 to 19 in Table 2 Across Definition of Strike Intensity and Model Specification

	Dep. Var.: $ln(1 + Pregnancies\ 18\text{-}19\ Years\ Old\ Women_{mt})$								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Strike Intensity^1	-0.056	-0.052	-0.033						
	(0.043)	(0.043)	(0.044)						
Strike Intensity^1≥75th pct.				-0.022	-0.022	-0.013			
				(0.018)	(0.018)	(0.019)			
Strike Intensity$^1_{t-1}$							0.086**	0.072*	0.057
							(0.042)	(0.043)	(0.042)
Strike Intensity^2	-0.003	-0.002	0.005						
	(0.038)	(0.039)	(0.040)						
Strike Intensity^2≥75th pct.				0.001	0.002	0.007			
				(0.018)	(0.018)	(0.019)			
Strike Intensity$^2_{t-1}$							0.029	0.026	0.018
							(0.036)	(0.038)	(0.038)
Strike Intensity$^2_5$	-0.002	-0.001	0.012						
	(0.031)	(0.031)	(0.032)						
Strike Intensity$^2_5$≥75th pct.				-0.007	-0.006	0.002			
				(0.019)	(0.019)	(0.020)			
Strike Intensity$^2_{5,t-1t-1}$							0.023	0.015	0.006
							(0.033)	(0.034)	(0.034)
Observations	28980	28344	28344	28980	28344	28344	28980	28344	28344
Adjusted R^2	0.096	0.097	0.101	0.096	0.097	0.101	0.096	0.097	0.101
Month FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Municipality FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Full Controls	N	Y	Y	N	Y	Y	N	Y	Y
Municipality Linear Trend	N	N	Y	N	N	Y	N	N	Y

This table reports fixed effects estimates of the effect of strike exposure measures on teenage pregnancies for female at ages 18 to 19. All specifications include a constant and total pregnancies (in logs) as a control. The full set of controls include poverty rate, per capita government expenditure in education (in logs, per student in public school), student population in public schools (in logs), population (in logs), and female population (in logs). Municipality linear trends are included by interacting municipality fixed effects with a linear trend in months. The unit of observation is a municipality-month (345 municipalities over the period Jan 2007 - Dec 2013). *Strike Intensity*^{1} refers to the measure of Strike Intensity using webscrapping data, *Strike Intensity*^{1} $\geq$ 75th pct. is a binary variable, equal to 1 if a municipality is above the $75th$ percentile in the cross sectional distribution of *Strike Intensity*^{1} . *Strike Intensity*^{2} refers to the measure of Strike Intensity using school days lost in August and the threshold of 10 days lost. *Strike Intensity*^{2} $\geq$ 75th pct. is a binary variable, equal to 1 if a municipality is above the $75th$ percentile in the cross sectional distribution of *Strike Intensity*^{2}. *Strike Intensity*$^2_5$ refers to the measure of Strike Intensity using school days lost in August and the threshold of 5 days lost. *Strike Intensity*$^2_5$ $\geq$ 75th pct. is a binary variable, equal to 1 if a municipality is above the $75th$ percentile in the cross sectional distribution of *Strike Intensity*$^2_5$. Robust standard errors clustered at the municipality level in parentheses. Subscript $t-1$ lags the period of strike in one year. ***p<0.01, **p<0.05, *p<0.10

Table A.10: Robustness of Results on Fetal Deaths in Table 2 Across Definition of Strike Intensity and Model Specification

	Dep. Var.: *ln(1 + Fetal Deaths$_{mt}$)*								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Strike Intensity^1	-0.007	-0.005	-0.005						
	(0.013)	(0.014)	(0.014)						
Strike Intensity^1≥75th pct.				-0.003	-0.002	-0.003			
				(0.006)	(0.006)	(0.007)			
Strike Intensity$^1_{t-1}$							0.007	0.003	0.003
							(0.015)	(0.015)	(0.016)
Strike Intensity^2	-0.008	-0.008	-0.010						
	(0.011)	(0.011)	(0.012)						
Strike Intensity^2≥75th pct.				-0.004	-0.004	-0.006			
				(0.006)	(0.006)	(0.006)			
Strike Intensity$^2_{t-1}$							-0.005	-0.003	-0.003
							(0.011)	(0.012)	(0.013)
Strike Intensity$^2_5$	-0.001	-0.000	-0.003						
	(0.008)	(0.008)	(0.008)						
Strike Intensity$^2_5$≥75th pct.				-0.003	-0.004	-0.004			
				(0.006)	(0.006)	(0.006)			
Strike Intensity$^2_{5,t-1}$							-0.008	-0.008	-0.007
							(0.008)	(0.009)	(0.009)
Observations	28980	28344	28344	28980	28344	28344	28980	28344	28344
Adjusted R^2	0.001	0.001	0.000	0.001	0.001	0.000	0.001	0.001	0.000
Month FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Municipality FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Full Controls	N	Y	Y	N	Y	Y	N	Y	Y
Municipality Linear Trend	N	N	Y	N	N	Y	N	N	Y

This table reports fixed effects estimates of the effect of strike exposure measures on fetal deaths. All specifications include a constant and total pregnancies (in logs) as a control. The full set of controls include poverty rate, per capita government expenditure in education (in logs, per student in public school), student population in public schools (in logs), population (in logs), and female population (in logs). Municipality linear trends are included by interacting municipality fixed effects with a linear trend in months. The unit of observation is a municipality-month (345 municipalities over the period Jan 2007 - Dec 2013). *Strike Intensity*^{1} refers to the measure of Strike Intensity using webscrapping data, *Strike Intensity*^{1} $\geq$ 75th pct. is a binary variable, equal to 1 if a municipality is above the $75th$ percentile in the cross sectional distribution of *Strike Intensity*^{1} . *Strike Intensity*^{2} refers to the measure of Strike Intensity using school days lost in August and the threshold of 10 days lost. *Strike Intensity*^{2} $\geq$ 75th pct. is a binary variable, equal to 1 if a municipality is above the $75th$ percentile in the cross sectional distribution of *Strike Intensity*^{2}. *Strike Intensity*$^2_5$ refers to the measure of Strike Intensity using school days lost in August and the threshold of 5 days lost. *Strike Intensity*$^2_5$ $\geq$ 75th pct. is a binary variable, equal to 1 if a municipality is above the $75th$ percentile in the cross sectional distribution of *Strike Intensity*$^2_5$. Robust standard errors clustered at the municipality level in parentheses. Subscript $t-1$ lags the period of strike in one year. ***p<0.01, **p<0.05, *p<0.10

Table A.11: Robustness of Results on Teenage Couples in Table 2 Across Definition of Strike Intensity and Model Specification

	Dep. Var.: $ln(1 + \ Teenage\ Couples_{mt})$								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Strike Intensity^1	0.067**	0.066*	0.090**						
	(0.034)	(0.034)	(0.037)						
Strike Intensity$^1 \geq$75th pct.				0.020	0.021	0.030*			
				(0.016)	(0.016)	(0.017)			
Strike Intensity$^1_{t-1}$							-0.004	0.002	-0.012
							(0.035)	(0.036)	(0.038)
Strike Intensity^2	0.015	0.016	0.021						
	(0.028)	(0.028)	(0.030)						
Strike Intensity$^2 \geq$75th pct.				0.012	0.012	0.016			
				(0.017)	(0.016)	(0.017)			
Strike Intensity$^2_{t-1}$							-0.017	-0.020	-0.023
							(0.027)	(0.029)	(0.030)
Strike Intensity$^2_5$	0.026	0.026	0.029						
	(0.023)	(0.023)	(0.024)						
Strike Intensity$^2_5 \geq$75th pct.				0.011	0.010	0.012			
				(0.016)	(0.016)	(0.017)			
Strike Intensity$^2_{5,t-1}$							-0.001	-0.001	-0.002
							(0.023)	(0.024)	(0.025)
Observations	28980	28344	28344	28980	28344	28344	28980	28344	28344
Adjusted R^2	0.008	0.009	0.013	0.008	0.009	0.013	0.008	0.009	0.013
Month FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Municipality FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Full Controls	N	Y	Y	N	Y	Y	N	Y	Y
Municipality Linear Trend	N	N	Y	N	N	Y	N	N	Y

This table reports fixed effects estimates of the effect of strike exposure measures on the log of teenage couples. All specifications include a constant and total pregnancies (in logs) as a control. The full set of controls include poverty rate, per capita government expenditure in education (in logs, per student in public school), student population in public schools (in logs), population (in logs), and female population (in logs). Municipality linear trends are included by interacting municipality fixed effects with a linear trend in months. The unit of observation is a municipality-month (345 municipalities over the period Jan 2007 - Dec 2013). *Strike Intensity*^{1} refers to the measure of Strike Intensity using webscrapping data, *Strike Intensity*^{1} $\geq$ 75th pct. is a binary variable, equal to 1 if a municipality is above the $75th$ percentile in the cross sectional distribution of *Strike Intensity*^{1} . *Strike Intensity*$^2_5$ refers to the measure of Strike Intensity using school days lost in August and the threshold of 5 days lost. *Strike Intensity*$^2_5$ $\geq$ 75th pct. is a binary variable, equal to 1 if a municipality is above the $75th$ percentile in the cross sectional distribution of *Strike Intensity*$^2_5$. Robust standard errors clustered at the municipality level in parentheses. Subscript $t-1$ lags the period of strike in one year. ***p<0.01, **p<0.05, *p<0.10

Table A.12: Robustness of Results on Logarithm of Emergency Contraceptions in Table 2 Across Definition of Strike Intensity and Model Specification

	Dep. Var.: $ln($ *Emergency Contraception Pills*$_{mt})$								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Strike Intensity^1	0.168*	0.167*	0.154*						
	(0.087)	(0.088)	(0.087)						
Strike Intensity$^1 \geq$75th pct.				0.055	0.055	0.049			
				(0.036)	(0.036)	(0.037)			
Strike Intensity^1_$t-1$							0.020	0.032	0.091
							(0.089)	(0.091)	(0.085)
Strike Intensity^2	0.022	0.017	0.006						
	(0.074)	(0.075)	(0.074)						
Strike Intensity$^2 \geq$75th pct.				0.008	0.007	-0.001			
				(0.036)	(0.036)	(0.036)			
Strike Intensity^2_$t-1$							-0.051	-0.025	-0.015
							(0.073)	(0.076)	(0.070)
Strike Intensity_5^2	0.034	0.032	0.024						
	(0.060)	(0.060)	(0.060)						
Strike Intensity_5$^2 \geq$75th pct.				0.015	0.014	0.008			
				(0.036)	(0.037)	(0.037)			
Strike Intensity_5, t-1^2							-0.035	-0.020	-0.032
							(0.061)	(0.064)	(0.061)
Observations	20700	20472	20472	20700	20472	20472	20700	20472	20472
Adjusted R^2	0.443	0.445	0.596	0.443	0.445	0.596	0.443	0.445	0.596
Month FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Municipality FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Full Controls	N	Y	Y	N	Y	Y	N	Y	Y
Municipality Linear Trend	N	N	Y	N	N	Y	N	N	Y

This table reports fixed effects estimates of the effect of strike exposure measures on the log of emergency contraception pills disbursed. All specifications include a constant and total pregnancies (in logs) as a control. The full set of controls include poverty rate, per capita government expenditure in education (in logs, per student in public school), student population in public schools (in logs), population (in logs), and female population (in logs). Municipality linear trends are included by interacting municipality fixed effects with a linear trend in months. The unit of observation is a municipality-month (345 municipalities over the period Jan 2007 - Dec 2013). *Strike Intensity*^{1} refers to the measure of Strike Intensity using webscrapping data, *Strike Intensity*^{1} $\geq$ 75th pct. is a binary variable, equal to 1 if a municipality is above the $75th$ percentile in the cross sectional distribution of *Strike Intensity*^{1} . *Strike Intensity*$^2_5$ refers to the measure of Strike Intensity using school days lost in August and the threshold of 5 days lost. *Strike Intensity*$^2_5$ $\geq$ 75th pct. is a binary variable, equal to 1 if a municipality is above the $75th$ percentile in the cross sectional distribution of *Strike Intensity*$^2_5$. Robust standard errors clustered at the municipality level in parentheses. Subscript $t-1$ lags the period of strike in one year. ***p<0.01, **p<0.05, *p<0.10

Table A.13: Robustness of Results in Table 3 on the Effect of Strike Exposure on Teenage Pregnancy using the Alternative Measure of Five Days of School Days Lost

Dep. Var.:	*Strike Intensity*^{1} OLS	*ln(1 + Teenage Pregnancies$_{mt}$)* IV Estimation				
	(1)	(2)	(3)	(4)	(5)	(6)
Strike Intensity$^2_5$	0.386***					
	(0.041)					
Strike Intensity^1		0.208***	0.213***	0.271***		
		(0.079)	(0.080)	(0.081)		
Strike Intensity$^1 \geq$75th pct.					0.105***	
					(0.037)	
Strike Intensity$^1_{t-1}$						0.006
						(0.083)
Observations	28344	28980	28344	28344	28344	28344
Adjusted R^2	0.614	0.064	0.066	0.075	0.075	0.075
Month FE	Y	Y	Y	Y	Y	Y
Municipality FE	Y	Y	Y	Y	Y	Y
Full Controls	Y	N	N	Y	Y	Y
Municipality Linear Trend	Y	N	Y	Y	Y	Y

This table reports fixed effects estimates of the effect of strike exposure measures on teenage pregnancies. *Strike Intensity*^{1} refers to the measure of Strike Intensity using webscrapping data, *Strike Intensity*^{1} $\geq$ 75th pct. is a binary variable, equal to 1 if a municipality is above the $75th$ percentile in the cross sectional distribution of *Strike Intensity*^{1}. *Strike Intensity*$^2_5$ refers to the measure of Strike Intensity using school days lost in August and the threshold of 5 days lost. *Strike Intensity*$^2_5$ $\geq$ 75th pct. is a binary variable, equal to 1 if a municipality is above the $75th$ percentile in the cross sectional distribution of *Strike Intensity*$^2_5$. Subscript $t-1$ lags the period of strike in one year. Column (1) shows the correlation coefficient of *Strike Intensity*^{1} and *Strike Intensity*$^2_5$. Column (2) shows the effect of *Strike Intensity*^{1} on teenage pregnancies using *Strike Intensity*$^2_5$ as an instrument to correct for measurement error. Columns (3) to (5) run the same regressions suing different specifications. Column (6) is a placebo test where we instrument *Strike Intensity*$^1_{t-1}$ with *Strike Intensity*$^2_{5,t-1}$. All specifications include a constant and total pregnancies (in logs) as a control. The full set of controls include poverty rate, per capita government expenditure in education (in logs, per student in public school), student population in public schools (in logs), population (in logs), and female population (in logs). Municipality linear trends are included by interacting municipality fixed effects with a linear trend in months. The unit of observation is a municipality-month (345 municipalities over the period Jan 2007 - Dec 2013). Robust standard errors clustered at the municipality level in parentheses. Robust standard errors clustered at the municipality level in parentheses. ***p<0.01, **p<0.05, *p<0.10

Table A.14: Robustness of Results in Table 4 on timing of the Effect of Strike Exposure on Teenage Pregnancy using the Alternative Measure of Five Days of School Days Lost

	Dep. Var.: $ln(1 + Teenage\ Pregnancies_{mt})$			
	(1) OLS	(2) IV	(3) OLS	(4) IV
Strike Intensity[1] x Apr-May	-0.011	0.058		
	(0.085)	(0.156)		
Strike Intensity[1] x Jun-Jul	0.378***	0.475***		
	(0.090)	(0.174)		
Strike Intensity[1] x Aug-Sep	0.182**	0.447***		
	(0.092)	(0.169)		
Strike Intensity[1] x Oct-Nov	-0.026	0.016		
	(0.095)	(0.164)		
Strike Intensity[1] x Apr-May			-0.004	-0.023
			(0.035)	(0.081)
Strike Intensity[1] x Jun-Jul			0.121***	0.174**
			(0.039)	(0.084)
Strike Intensity[1] xAug-Sep			0.060	0.214**
			(0.038)	(0.084)
Strike Intensity[1] x Oct-Nov			-0.003	-0.015
			(0.037)	(0.080)
Observations	28344	28296	28344	28296
Adjusted R^2	0.087	0.076	0.087	0.075
Month FE	Y	Y	Y	Y
Municipality FE	Y	Y	Y	Y
Full Controls	Y	Y	Y	Y
Municipality Linear Trend	Y	Y	Y	Y

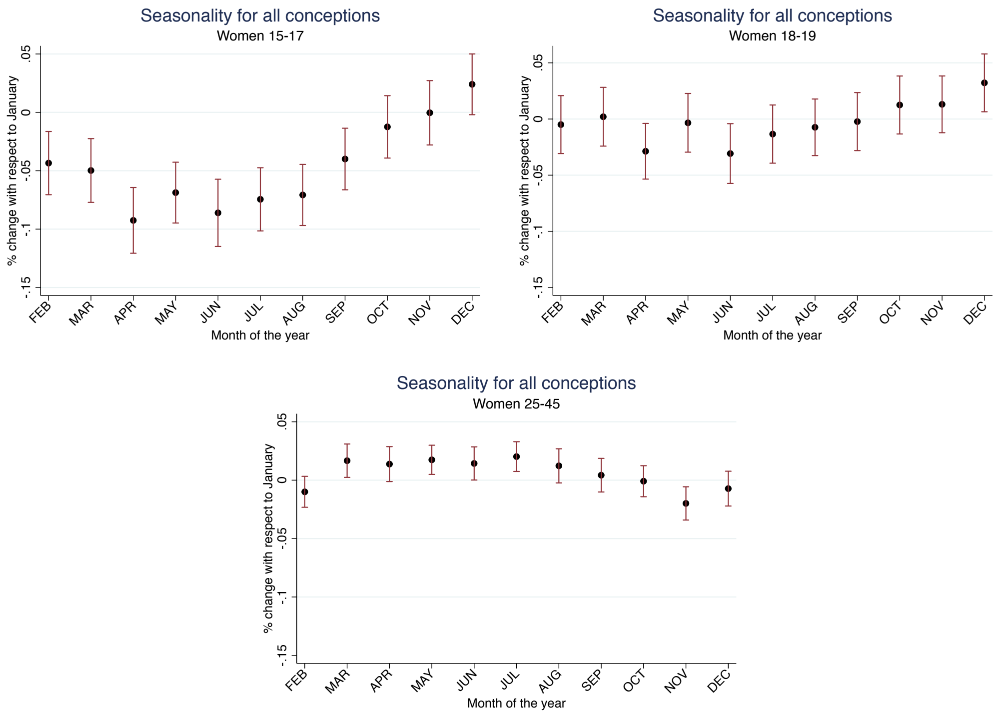
This table reports fixed effects estimates of the effect of strike exposure measures on teenage pregnancies. All specifications include a constant and total pregnancies (in logs) as a control. The full set of controls include poverty rate, per capita government expenditure in education (in logs, per student in public school), student population in public schools (in logs), population (in logs), and female population (in logs). Municipality linear trends are included by interacting municipality fixed effects with a linear trend in months. The unit of observation is a municipality-month (345 municipalities over the period Jan 2007 - Dec 2013). Strike Intensity[1]$\geq$ 75th pct. is a binary variable, equal to 1 if a municipality is above the $75th$ percentile of strike exposure distribution shown in Panel A of Figure 1b. Robust standard errors clustered at the municipality level in parentheses. ***p<0.01, **p<0.05, *p<0.10

Table A.15: Robustness of Results in Table 5 on the Heterogeneity of the Effect of Strike Exposure on Teenage Pregnancy using the Alternative Measure of Five Days of School Days Lost

	Baseline Teen Pregnancy Ratio				Baseline Population Size			
	≤50th pctile.		>50th pctile.		≤50th pctile.		>50th pctile.	
	OLS	IV	OLS	IV	OLS	IV	OLS	IV
Strike Intensity[1]	0.137**	0.337***	0.139**	0.232*	0.087	0.421**	0.143**	0.160*
	(0.067)	(0.122)	(0.060)	(0.122)	(0.079)	(0.179)	(0.056)	(0.095)
Observations	13500	13500	13500	13500	13896	13896	14448	14448
Adjusted R^2	0.090	0.078	0.099	0.088	0.076	0.062	0.116	0.105
	Percentage of COED Students				University Campus			
	≤50th pctile.		>50th pctile.		outside Muni.		within Muni.	
	OLS	IV	OLS	IV	OLS	IV	OLS	IV
Strike Intensity[1]	0.153***	0.225***	0.106	0.332**	0.119**	0.292***	0.087	0.161
	(0.058)	(0.087)	(0.069)	(0.156)	(0.054)	(0.111)	(0.075)	(0.104)
Observations	14208	14208	14136	14136	22332	22332	6012	6012
Adjusted R^2	0.098	0.087	0.079	0.067	0.078	0.066	0.141	0.130
Month FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Full Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipality Linear Trend	Yes	Yes	Yes	Yes	Yes	Yes	Yes	yes

This table reports fixed effects estimates of the effect of strike exposure measures on teenage pregnancies. All specifications include a constant and total pregnancies (in logs) as a control. The full set of controls include poverty rate, per capita government expenditure in education (in logs, per student in public school), student population in public schools (in logs), population (in logs), and female population (in logs). Municipality linear trends are included by interacting municipality fixed effects with a linear trend in months. The unit of observation is a municipality-month (345 municipalities over the period Jan 2007 - Dec 2013). Strike Intensity[1] ≥ 75th pct. is a binary variable, equal to 1 if a municipality is above the $75th$ percentile of strike exposure distribution shown in Panel A of Figure 1b. Robust standard errors clustered at the municipality level in parentheses. ***p<0.01, **p<0.05, *p<0.10

Figure A.1: Seasonality of conceptions for different age groups (years 2007 - 2010)



Notes:

A.2 Description of Variables

This section describes the set of municipality-level variables (and their sources) used for the balance check exercises displayed in Tables A.1, A.2, A.3, and A.4.

As previously exposed in this investigation, we extracted information about birth statistics by different demographic groups from the vital statistics data of the Department of Statistics and Health Information (DEIS) of the Ministry of Health of the Government of Chile. This database records the universe of births in Chile, covering at least 99 percent of all births in published aggregate figures. We then compute the municipality-level logarithms of the count of all births, and births by mothers aged 15 to 17.

We use municipality level information reported by the National System of Municipal Information (SINIM) for comparing resources available for each municipality in each year. We also collect data on population density per square kilometer, per capita real investment in education, per capita total expenditures, and birth rates. We compute the percentage of school-aged people in each municipality (i.e., people aged 6 to 19 years old).

We use the 2009 National Socioeconomic Characterization Survey (CASEN) by the Ministry of Planning of the Government of Chile. The municipality-level averages are weighted using the total population-projected adjusted municipality expansion factor, which is provided by the survey. We then compute the average per capita family income and the average percentage of people living in poverty.

Education performance indicators are computed using information gathered by the National Performance records kept by the Ministry of Education of the Government of Chile. This record covers the universe of students in the educational system of Chile. We compute average yearly attendance, promotion, and dropout rates, separately for female and also pooling male and female students together.

Finally, data on contraceptive methods is taken from the online appendix dataset by Bentancor and Clarke (2017), where the authors exhaustively compiled the results of several waves of Youth Survey data in Chile. We include in the analysis the usage of male and female condoms, progestin-only and combination contraceptive pills, and intra-uterine dispositive (IUD). We include a dummy variable indicating accessibility to the emergency contraceptive pill in the municipality; and the distance in kilometers to the closest municipality with access to the pill.

A.3 General Issues on Measurement Error

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

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

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

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

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

One thing to consider is that the error in this case comes from our own construction of the concept of a strike. Then it is plausible to assume that this misclassification error is independent of the probability that the student becomes pregnant, our main outcome of interest. For instance, this is less plausible in misclassification error in the response of survey questions by the same person under study, i.e. the student responding whether her school was on strike or not, which could be correlated with unobservables that determine the probability of becoming pregnant. This assumption is important, because, if it holds, then we can more easily adjust the estimated coefficients by the term C in equation A.3 if we knew the probabilities in it. If we do not know them, we can construct bounds for β_{OLS}

using that expression. If the assumption does not hold then we do not know the form of the bias since each component of C, for instance π_{10} would be defined for each individual separately. However, there is little reason to think that our error by construction is related to the probability that a teen in a particular school becomes pregnant.

A.3.1 Aggregation of a binary variable and structure of Measurement Error

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

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

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

A.3.2 Application to the measurement of a School's Strike Status

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

$$z_{im}^{a} = x_{im}^{*} + \mu_{im}^{a}$$

$$z_{im}^{b} = x_{im}^{*} + \mu_{im}^{b}$$

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

$$y_m = \beta_0 + \beta_1 x_m^{*} + \varepsilon_m \tag{A.4}$$

However we do not observe the true proportion of students who attended a school on strike but rather can estimate:

$$y_m = \beta_0 + \beta_1 z_m^{k} + \varepsilon_m \tag{A.5}$$

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

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

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

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

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

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

In particular, (A1) requires that the errors of misclassification of strike status are not related to each other, other than their relation to x^*, the true strike status. We believe that this assumption holds as the process of misclassification of each term are independent as they rise from two unrelated data sources: (1) web scrapping data, and (2) micro-data of official records on daily assistance. (A2) states that the errors of misclassification, μ^a and μ^b, are independent of y, the probability that a teenage girl becomes pregnant. Importantly, the process of classification error in both measures of a school's strike status rises from researchers' coding errors in the construction of proxies of strike. It is plausible then to assume that (A2), holds in this setting as our own mistakes in coding a school on strike is independent on whether a teenage girl in that school became pregnant during our period of analysis. This is an important assumption that is unlikely to hold in other settings, such as response error in survey data (Bollinger and David, 1997). (A3) is a standard assumption and relates also to the fact that misclassification error is independent of the data generating process of y. (A4) assumes that the "error is not too severe" (Black et al., 2000, pp. 740) so that the covariance of each independent measure with the true fraction of schools on strike surpasses the covariance between both proxies. (A5) holds by construction as strike status is a binary indicator at the school level (see section A.3.1).

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

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

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

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

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

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