

The Long-run Effects of Teacher Strikes: *Evidence from Argentina*^a

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Abstract

This paper presents novel evidence on the effect of school disruptions on student long-run outcomes, exploiting variation in the prevalence of teacher strikes within and across provinces in Argentina over time. On average, a student during our analysis period lost 88 days of primary school due to teacher strikes, making this a particularly interesting case for the study of the consequences associated with school disruptions. We find robust evidence in support of adverse labor market effects when the students are between 30 and 40 years old: being exposed to the average incidence of teacher strikes during primary school reduces annual labor market earnings of males and females by 3.2 and 1.9 percent, respectively. A back-of-the-envelope calculation suggests that this amounts to an aggregate annual earnings loss of \$2.34 billion, equivalent to the cost of raising the employment income of all Argentinian primary school teachers by 62.4 percent. We also find evidence of an increase in unemployment, an increase in the probability of not working or studying, and a decline in the skill levels of the occupations into which students sort. Our analysis suggests that these labor market effects are driven, at least in part, by a reduction in educational attainment. Using data on students who have just finished primary school, we demonstrate that many of the adverse education effects are visible immediately after primary school. We also find some suggestive evidence that exposure to teacher strikes in early grades have larger effects than strikes in later grades. Finally, our analysis identifies significant intergenerational treatment effects, such that children of individuals who were exposed to teacher strikes also experience adverse education effects.

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1. Introduction

Teacher industrial action is a prevalent feature of public education systems across the globe; during the past few years teacher strikes have been observed in Argentina, Canada, Chile, China, France, Germany, India, Israel, Lebanon, Mexico, Russia and the US (e.g. Charleston, Seattle, East St. Louis, Pasco, Prospect Heights and Chicago). A shared belief among policymakers across several of these countries is that teacher strikes disrupt learning and negatively impact student educational attainment. In some countries this sentiment has led to the enactment of legislation that severely restricts teachers' right to strike.¹ However, despite the prevalence of teacher strikes across the globe - and the debates surrounding them - there is a lack of empirical work that credibly examines how they affect student long-run outcomes.

In this paper, we construct a new data set on teacher strikes in Argentina and use this to present the first evidence in the literature on the effect of school disruptions caused by teacher strikes on student long-run outcomes. Between 1983 and 2014 Argentina experienced 1,500 teacher strikes, with substantial variation across time and provinces, making this a particularly interesting case for the study of teacher strikes. We analyze the relationship between exposure to strikes in primary school and relevant education, labor market and sociodemographic outcomes when the exposed students are between 30 and 40 years old.² We also examine if the effects we identify carry over to the individuals' children.

To identify the effect of strike-induced school disruptions, we rely on a difference-in-difference method that examines how education and labor market outcomes changed among adults who were exposed to more days of teacher strikes during primary school compared to adults who were exposed to fewer days of strikes. The sources of variation we exploit come from within-province differences in strike exposure across birth cohorts and within-cohort differences in strike exposure across provinces. On average, provinces lost 372 instructional days due to strikes between 1983 and 2014, ranging from 188 days in La Pampa to 531 days in Rio Negro. The average number of primary school days lost due to teacher strikes was 88 among the individuals in our analysis sample – equivalent to half a year of schooling.

The main assumptions underlying our estimation strategy are that there are no shocks (or other policies) contemporaneous with teacher strikes that differentially affect the various cohorts and that the timing of teacher strikes is uncorrelated with prior trends in outcomes across birth cohorts within each province. We show extensive evidence that our data are

¹ For example, even though 33 states in the US have passed duty-to-bargain laws that require districts to negotiate with a union, only 13 states allow teachers to go on strike in the event of a bargaining impasse (Colasanti 2008).

² We focus on this age range because existing literature suggests that labor market outcomes at this age are informative about lifetime outcomes (e.g. Haider and Solon 2006; Böhlmark and Lindquist 2006).

consistent with these assumptions. In particular, our results are robust to controlling for local labor market conditions, including province-specific linear time trends, accounting for cross-province mobility, excluding regions with persistently high frequencies of teacher strikes, and controlling for province-specific non-teacher strikes. We also show that the effects we identify disappear when reassigning treatment to cohorts that have just graduated from – or have not yet started - primary school, indicating that the timing of teacher strikes is uncorrelated with trends in outcomes across birth cohorts within each province.

We find robust evidence in support of adverse labor market effects when the students are between 30 and 40 years old: being exposed to the average incidence of teacher strikes during primary school reduces wages for males and females by 3.2 and 1.9 percent, respectively. We find some suggestive evidence that exposure to strikes in early grades have larger effects than exposure in later grades, though these differences are often not statistically significantly different from zero. The prevalence of teacher strikes in Argentina means that the effect on the economy as a whole is substantial: A back-of-the-envelope calculation suggests an aggregate annual earnings loss of \$2.34 billion. This is equivalent to the cost of raising the average employment income of all primary school teachers in Argentina by 62.4%.

In addition to adverse wage and earnings effects, our results reveal negative effects on several other labor market outcomes. With respect to males, we find evidence of both an increase in the likelihood of being unemployed and of occupational downgrading. The effects are very similar for females. However, rather than an occupational downgrading effect, we find an increase in home production (neither working nor studying). Our analysis suggests that these adverse labor market effects are driven, at least in part, by declines in educational attainment: being exposed to the average incidence of strikes leads to a reduction in years of schooling by 2.02 and 1.58 percent for males and females, respectively. By looking at 12-17 year olds, we show that negative education effects are visible immediately after children have finished primary school, and that they are larger among children from more vulnerable households. Our analysis reveals that strikes affect individuals on other sociodemographic dimensions as well. Specifically, individuals exposed to teacher strikes have less educated partners and lower per capita family income. Finally, we find significant intergenerational effects: children of individuals exposed to strikes during primary school suffer negative education effects as well.

Our paper contributes to the existing literature in several important ways. First, no other paper has examined the effects of strike-induced school disruptions on student long-run outcomes. Given the large literature demonstrating that short-run program effects on student

outcomes can be very different from long-run effects (e.g., Chetty et al. 2011; Deming et al. 2013; Lovenheim and Willén 2016), this is of great value to policymakers. Second, the prevalence of teacher strikes that we exploit is much greater than that which has been used in earlier studies. This allows us to obtain more precise estimates, and examine a richer set of outcomes. Third, this paper makes use of a novel data set which we have created based on information from historic business reports on the Argentine economy. This data is a great tool for other researchers interested in questions centering on teacher strikes and industrial action.

It is important to highlight that the pervasive level of teacher strikes during our analysis period is not a deviation from the norm in Argentina, and current students are exposed to similar levels of strikes. This cements the relevance of our paper and highlights the urgency of implementing reforms that can reduce the prevalence of teacher strikes in the country. One policy could be to introduce labor contracts that extend over several years, and only allow teachers to strike if a bargaining impasse is reached when renewing these multi-year contracts. This would eliminate sporadic teacher strikes while still allowing teachers to use industrial action as a tool to ensure fair contracts.

This paper proceeds as follows: Section 2 provides an overview of the education system in Argentina and offers theoretical predictions of how teacher strikes may affect student outcomes; Section 3 discusses pre-existing research; Section 4 introduces the data; Section 5 presents our empirical strategy; Section 6 discusses our results; and Section 7 concludes.

2. Background & Theoretical Predictions of Teacher Strikes

2.1 The Argentinian Education System

Education in Argentina is the responsibility of the provinces and is divided into four stages: kindergarten, primary education, secondary education and tertiary education.³ Primary education begins the calendar year in which the number of days the child is 6 years old is maximized, and comprises the first seven years of schooling. During our analysis period, only primary education was mandatory in Argentina (Alzúa et al. 2015).⁴ Since then, compulsory schooling has grown to include secondary education as well, increasing the length of mandatory education from 7 to 12 years. Public education is financed through a revenue-sharing system between the provinces and the federal government, and is free at all levels.

³ Primary education was decentralized in 1978 and secondary education was decentralized in 1992. However, the national government remains highly involved in terms of setting curriculum, regulations and financing.

⁴ The youngest cohort in our main analysis sample finished primary school in the year prior to the implementation of the *Federal Education Law* (1998; approved in 1993) which extended mandatory education to encompass secondary schooling as well.

The fraction of students that attended private school at the primary level during our analysis period was approximately 0.18, and this fraction remained relatively constant across the years that we examine. Since 2003, however, private enrollment at the primary level has increased substantially. Existing research suggests that this increase is driven by high- and middle-income families, leading to an increase in socioeconomic school segregation between private and public schools (Gasparini et al. 2011; Jaume 2013).⁵

2.2 Teacher Strikes in Argentina

The presence of unions, collective bargaining and labor strikes in Argentina can be traced back to the early years of the 20th century, except for the years during which the country was subject to military dictatorships (CEA 2009).⁶ Following the most recent reinstatement of democracy (1983), industrial action has quickly regained its status as a pervasive feature of the Argentinian labor market. Since then, public sector teachers have been the most active protesters in the country, making up 35 percent of all strikes (Etchemendy 2013). In comparison, private school teachers account for less than 4 percent of the country's strikes. The occupation with the second most strikes is public administration, accounting for 25 percent of the country's strikes (Chiappe 2011; Etchemendy 2013).

Teacher unions are typically organized at the provincial level, and variation in teacher strikes across time and provinces is substantial. On average, provinces have lost 372 instructional days due to teacher strikes between 1983 and 2014 (6.7 percent of total instructional days), ranging from 188 days in La Pampa to 531 days in Rio Negro, with a standard deviation of 109 days.⁷ The pervasive level of strikes during our analysis period is not a deviation from the norm in Argentina, and current students are exposed to similar levels of strikes. Panel A of Figure 1 shows the variation in the number of days of teacher strikes by province from 1977 to 2014, and Panel B of Figure 1 displays the number of strikes by province during the same period (a strike can last for a single day or several weeks).

No study has examined the effect of teacher strikes on student outcomes in Argentina, but two studies have attempted to disentangle the factors underlying the prevalence of teacher strikes in the country. The results are mixed: Murillo and Ronconi (2004) finds that teacher strikes are more common in provinces where union density is high and political relations with

⁵ A commonly held belief is that individuals perceive private education as superior due to the fact that teacher strikes are less pronounced at these institutions, but existing literature finds no effect of teacher strikes on the likelihood of being enrolled at a public institution (Narodowski and Moschetti 2015). We examine this in detail in Section 6.4.

⁶ During the dictatorships, labor strikes were prohibited and collective bargaining limited.

⁷ In theory, days cancelled due to adverse circumstances must be rescheduled. However, the prevalence of teacher strikes across time means that this rarely happens.

the local government is tense, while Narodowski and Moschetti (2015) concludes that strikes display an erratic behavior without any discernable trends or explanations. What these studies have in common is that they both emphasize the lack of a relationship between local labor market conditions and teacher strikes. This is important since our main identification assumption is that there are no shocks contemporaneous with teacher strikes that differentially affect the different cohorts – something we explore at length in Section 6.3.

2.3 Theoretical Predictions

This paper exploits variation in teacher strikes within and across provinces over time to identify the reduced form effect of teacher strikes on student outcomes. As such, this is not a full analysis of the benefits and costs associated with teacher strikes, but rather a partial equilibrium analysis that uses strikes to measure the effect of school disruptions on students' outcomes. Specifically, we are not measuring the benefits or costs in a general equilibrium / dynamic sense. For example, the ability of teachers to strike may give them leverage in negotiations, leading to changes in working conditions that attract a different quality of teachers or affect investments in schooling, and this may impact future student cohorts.⁸ In this section, we provide a discussion on a large subset of the potential implications of teacher strikes. This should not be viewed as an exhaustive list, but rather as an overview of some of the more common ways in which teacher industrial action can impact students. In addition to serving as an overview of what teacher strikes may do, this section is imperative for understanding which subset of these factors our empirical strategy is able to pick up.

The main way in which strikes can affect student outcomes is by reducing the time students spend in school. Theoretical as well as empirical research provide clear predictions that reduced instructional time lowers academic achievement (Cahan and Cohen 1989; Neal and Johnson 1996; Lee and Barro 2001; Gormley and Gayer 2005; Cascio and Lewis 2006; Luyten 2006; Pischke 2007; Marcotte 2007; Sims 2008; Marcotte and Helmet 2008; Hansen 2008; Leuven et al. 2010; Fitzpatrick et al. 2011; Rivkin and Schiman 2013; Goodman 2014). However, these papers only examine the short-run educational effects of school disruptions.

In addition to reducing effective instructional time, teacher strikes may, among other things, affect teacher effort, alter resource levels and allocation, affect academic expectations and graduation requirements, alter the value of a diploma, change the value differential between a public and a private degree, and change the composition of teachers. The direction and magnitude of the effects flowing through these channels will depend on the nature and

⁸ However, in Section 6.3 we provide suggestive evidence that this type of general equilibrium effect does not take place.

outcome of the strike. For example, if the unions go on strike to bargain for higher wages and are successful, the strike may improve teacher effort and productivity. However, if the teachers are unsuccessful, and the strike is in effect for several months, academic expectations and graduation requirements may be adjusted downwards with the potential implications of a reduction in the value of a diploma. Further, even if the teachers are successful, an increase in teacher pay may be financed through a reallocation of resources from other inputs that enter the education production function, and this can lead to a reduction in education quality.

The above discussion makes clear that the effect of teacher strikes on education production can be both positive and negative, and the resulting predictions of the effects of teacher strikes on student outcomes are therefore ambiguous. With respect to the current study, it is important to note that we use teacher strikes to measure the effect of school disruptions on student long-term outcomes through a partial equilibrium analysis. To the extent that the above factors impact current students, they will contribute to the effects that we estimate. However, some of the factors discussed above may only impact future student cohorts, and our estimation strategy does not permit us to fully identify those effects.^{9, 10}

In addition to having direct effects on student education outcomes, teacher strikes may impact several non-educational outcomes as well. For example, unless parents can make alternative child care arrangements (which will depend on whether it was an expected or unexpected strike, and on the resources that the parents possess), strikes will increase leisure time and the risk that students engage in bad behavior and criminal activity (e.g. Anderson 2014; Henry et al. 1999). This can directly impact the future education and labor market outcomes of children. Though we cannot look directly at the relationship between strikes and engagement in criminal activity, to the extent that this occurs and affects the long-run outcomes of students, it will be a part of the effects captured by our point estimates.

A final factor that makes it difficult to anticipate the likely effects of strikes on student outcomes concerns treatment heterogeneity. The most likely source of heterogeneity relates to

⁹ To obtain suggestive evidence on the effect of strikes on future student outcomes, we have reestimated our baseline equation using exposure to strikes prior to school start as our treatment variable. If teacher strikes affect future student outcomes we would expect this analysis to return significant results. The results from this exercise are shown in Column H of Table 7. All point estimates are small and not statistically significant, suggesting that our data is inconsistent with this idea.

¹⁰ Given the large number of students affected by teacher strikes in Argentina, there could also be general equilibrium effects at the labor market level (for a discussion on how large educational shocks may generate GE effects on the labor market, see Moretti (2004) and Jaume (2017)). For example, old cohorts might benefit from younger cohorts being exposed to strikes since that lowers the competition that they face on the labor market. However, we provide suggestive evidence that these GE effects are not driving our results through a detailed placebo test. Specifically, we reassign the treatment variable for birth cohort c to birth cohort $c-7$, such that the measure of exposure to teacher strikes is the number of days (in tens of days) of primary school strikes that took place while the individuals were 13 – 19 years old (Panel E of Table 7). If teacher strikes affect past student cohorts (through, for example, reductions in labor market competition), we would expect this analysis to return significant and positive results. This exercise returns small and not statistically significant results, suggesting that this type of GE effect is not driving our results.

the socioeconomic characteristics of the students' families: wealthy parents will be able to move their children to private school if they believe strikes to hurt their children. Depending on the prevalence of this behavior, it may lead to a segregated school system with additional adverse effects on the students left behind. Another source of heterogeneity relates to when during primary school children are exposed to strikes. Research suggests that young children are more susceptible to policy interventions in general, and children who lose instructional time in first grade may suffer more than children who lose instructional time in the final grade of primary school (Shonkoff and Meisels 2000; Cunha and Heckman 2007; Doyle et al. 2009; Chetty et al. 2015). Finally, the effect of several short disruptions may be very different from the effect of one long disruption. We explore all these types of heterogeneity in Section 6.3.

3. Prior Literature on Teacher Strikes

The majority of the existing research on teacher strikes is cross sectional with identification strategies that are vulnerable to omitted variable bias (Caldwell and Maskalski 1981; Caldwell and Jefferys 1983; Zirkel 1992 Thornicroft 1994; Zwerling 2008; Johnson 2009). Specifically, students, teachers and schools subject to strikes may be different from those that are not on dimensions that we cannot observe. If these differences have independent effects on the outcomes, this will bias the results. Further, these studies have focused on contemporaneous effects (test scores) of teacher strikes that are of very short duration. These two factors significantly limit our understanding of the consequences associated with school disruptions caused by teacher strikes. This is particularly the case given the large literature suggesting that short-run program effects on student outcomes can be very different from any long-run effects (e.g., Chetty et al. 2011; Deming et al. 2013; Lovenheim and Willén 2016).

Abstracting away from potential identification issues, the results from the above studies are mixed. While some studies find no association between strikes and student outcomes (e.g. Zwerling 2008; Thornicroft 1994; Zirkel 1992), others find marginally statistically significant and negative effects (e.g. Johnson 2009; Caldwell and Maskalski 1981; Caldwell and Jefferys 1983). Taken together, these studies suggest that school disruptions caused by teacher strikes have a minimal impact on student outcomes.¹¹

To the best of our knowledge, only two studies on teacher strikes and student outcomes have relied on research designs that are not cross sectional: Belot and Webbink (2010) and Baker (2013). Belot and Webbink (2010) exploit a reform in Belgium in 1990 that led to

¹¹ It should be noted that these studies – just as the current paper – are unable to look at any general equilibrium effects associated with teacher strikes, and therefore do not provide a complete analysis of the benefits and costs associated with teacher strikes.

substantial and frequent strikes in the French-speaking community but not in the Flemish-speaking community of the country. By comparing the difference in education outcomes between individuals in school to those not in school in the French-speaking community to that same difference in the Flemish-speaking community, the authors find that strikes causes a reduction in education attainment and an increase in grade repetition. Though interesting, this paper is not able to examine if the education effects carry over to the labor market, if there are non-educational effects of teacher strikes and if there are intergenerational effects. Further, the point estimates in Belot and Webbink (2010) provide the intent-to-treat effect of exposure to all strikes in 1990 among students in all grade school years. This makes it difficult to extrapolate the marginal effect of teacher strikes on students in specific grade years.

Baker (2013) evaluates the effect of teacher strikes on student achievement in Ontario by comparing the change in test score between grade 3 and 6 for cohorts exposed to a strike to the corresponding change for cohorts that were not subject to a strike. The results suggest that strikes that lasted for more than 10 days and took place in grade 5 or 6 have statistically and economically significant negative effects on test score growth, while strikes that occurred in grades 2 or 3 do not. However, data limitations prevent the author from examining long-run education and labor market effects – one of the main contributions of the current analysis.

To summarize, the majority of the existing research on teacher strikes is cross sectional with identification strategies that are vulnerable to omitted variable bias. More current papers rely on identification strategies less susceptible to such issues, but limited variation in teacher strikes coupled with poor outcome data has led these studies to only examine the educational effects of strikes in the short- and medium-term. There is no paper that has explored the long-run educational attainment and labor market effects of teacher strikes. Further, no study has been able to examine if there are intergenerational effects associated with teacher strike exposure. This cements the importance of our empirical investigation on the topic.

4. Data

4.1 Teacher Strikes

Data on teacher strikes are obtained from historic reports on the Argentine economy published by Consejo Técnico de Inversiones (CTI). These reports provide province-specific information on strikes per month, and we use information from 1977 to 1998 to construct our data set. We assume that children begin school the calendar year they turn 6, and graduate

from primary school at the age of 12. This means that we have information on exposure to teacher strikes while in primary school for children born between 1971 and 1985.¹²

For our main analysis, we restrict attention to teacher strikes in primary school. Results from specifications that use strike exposure during primary school, and potential strike exposure during secondary school, are shown in the Online Appendix.¹³ Our decision to focus on strikes in primary school is based on data limitations and the fact that there are multiple levels of selection that complicate the analysis of strike effects in secondary school.

First, our data on teacher strikes is not representative of teacher strikes in secondary school. Specifically, the data that we have collected from the historic records of Consejo Técnico de Inversiones convey information about primary school teacher strikes, and the educational system in Argentina was structured in such a way that secondary school teachers were highly unlikely to join primary school teachers on their strikes.¹⁴ It is also important to note that secondary education was decentralized at the province level only after 1992, and that they therefore most likely did not participate in any of the province-specific strikes that took place prior to this year. This means that our treatment measure is incredibly noisy, and oftentimes wrong, with respect to exposure in secondary school.

Second, during our analysis period only primary education was mandatory (less than 60% attended secondary school). This is problematic not only because it will force us to assign incorrect treatment dosages in secondary school to more than 40 percent of the population, but also because it introduces a substantial selection problem: if school disruptions in primary school affects educational attainment, it may impact who enters secondary education, causing selection bias.¹⁵

Third, private school enrollment was very high at the secondary level (30 percent) during our analysis period, and strikes are much less prevalent in the private sector: while public teachers make up about 35 percent of all strikes in Argentina, private teachers account

¹² The assumption that children attend primary school between the ages of 6 and 12 leads to some measurement error in treatment assignment because children start primary school the calendar year in which the number of days they are 6 years old is maximized. This assumption will thus slightly attenuate our results. Using household survey data on the educational attainment of 6 year olds between 2003-2015, we estimate that more than 70 percent of individuals in our sample are assigned to the right cohort.

¹³ The effect of exposure to strikes in primary school is robust to the inclusion of potential exposure to strikes in secondary school – all point estimates are within the 95 percent confidence interval of the baseline results. The effect of potential exposure to teacher strikes in secondary school has no effect on student long-run outcomes. As noted in the text, we do not believe that this represents effect heterogeneity, but rather treatment heterogeneity caused by measurement error and selection bias (i.e. that children of secondary school-age were usually not subject to strikes). Because of this, we believe that the correct level of analysis is exposure in primary school.

¹⁴ To obtain suggestive support for this, we have randomly selected some of the strikes reported by Consejo Técnico de Inversiones and examined what was written about these strikes in the national newspapers at the time of those strikes. In all cases, the national newspapers report that primary school teachers participated in the strikes, and that secondary school teacher only participated occasionally.

¹⁵ This selection concern is something that we find suggestive evidence of with respect to the likelihood of finishing secondary school (Table 2) and enrollment in secondary school (Table 8). We also find evidence of effect heterogeneity with respect to these outcomes on the family income dimension (Online Appendix Table A8).

for less than 4 percent. This is much less of an issue at the primary level since enrollment in private schools was half that of enrollment in private schools at the secondary level.

Table 1 depicts our identifying variation. Looking across the table, there is substantial variation both within provinces over time and across provinces in any given year. Table 1 also shows that the average number of days of teacher strikes that these cohorts were exposed to during primary school is 40 (3.2 percent of primary school).¹⁶ If one takes national teacher strikes into account this number increases to 88 (6.98 percent).¹⁷ As discussed in Section 2, strikes were prohibited during the military junta of 1977-1983. This explains why the oldest cohorts in our sample are exposed to fewer days of teacher strikes.

4.2 Long-run Outcomes

Our main outcome data come from the 2003-2015 waves of the Encuesta Permanente de Hogares (EPH), a household survey representative of the urban population of Argentina (91 percent of the population). We restrict our analysis to individuals between the ages of 30 and 40 because these individuals are typically on a part of their earnings profile where current earnings are reflective of lifetime earnings (e.g. Haider and Solon 2006; Böhlmark and Lindquist 2006). Figure 2 shows the data structure for a sample of birth cohorts.¹⁸

Critical to our identification strategy is our ability to link respondents to their province of birth, because teacher strikes may lead to selective sorting across provinces, especially if exposure to strikes affects school quality. Teacher strikes could also impact post-primary school mobility patterns if strike-induced education effects affect one's access to national labor markets. Relying on birth province rather than current province of residence eliminates these endogenous migration issues. It is still the case that a fraction of respondents will be assigned the wrong treatment dose as families can move across provinces such that birth province is different from the province in which the child attended primary education. However, Online Appendix Table A1 shows that the province of residence is the same as the birth province for 93 percent of 13 year olds in Argentina, and any bias resulting from this mobility is therefore likely to be very small.¹⁹

To construct our analysis sample, we collapse the data on the birth province – birth year – EPH year level. Aggregation to this level is sensible because treatment varies on the birth

¹⁶ Primary school in Argentina is comprised of 1260 instructional days, 180 days per year.

¹⁷ We ignore national teacher strikes when constructing our treatment measure as they are subsumed by the cohort fixed effects that we use.

¹⁸ The birth cohorts range from 1971 to 1985. These are the only cohorts that are between 30 and 40 years old when the outcomes are measured (2003-2015) for which we can perfectly calculate exposure to teacher strikes during primary school. This means that we do not have a balanced panel of age observations across the EPH waves. In Section 6.3 we show that limiting our analysis to EPH waves 2011-2015 for which we have a balanced panel has no impact on our results.

¹⁹ In Section 6.3 we further show that our results are robust to excluding the five provinces with the highest migration rates.

province – birth year level. Online Appendix Table A2 provides summary statistics of the outcome variables we use in our analysis. For educational attainment, we generate dummy variables for completion of secondary education and for having obtained at least a Bachelor’s degree. These indicators are constructed from a years of education variable that we also use to examine the educational attainment effect of strike exposure. With respect to labor market outcomes, we look at the proportion of people that are unemployed, out of the labor force and dedicated to home production (neither studying nor working). To construct a measure of occupational skill we follow Lovenheim and Willén (2016) and calculate the fraction of workers in each 3-digit occupation code that has more than a high school degree. We use this to rank occupations by skill level to examine if strike exposure leads individuals to sort into lower-skilled occupations. We also use the EPH measures of hours worked and earnings. Regarding earnings, we consider both the log of hourly wage and log of total labor earnings. Since teacher strikes may affect labor force participation and unemployment, we also study the effect on the level of labor earnings, which includes individuals with zero earnings.²⁰

Preliminary evidence on the relationship between teacher strikes and student long-run outcomes is displayed in Figure 3, which plots the predicted years of schooling (Panel A) and labor income (Panel B) as a function of the number of days of teacher strikes during primary school.²¹ There is clear suggestive evidence of a strong linear negative correlation between teacher strikes and later-in-life outcomes: For each 180 days of teacher strikes (one year of primary school) labor income is reduced by 6.7 percent, and years of education decline by 3.1 percent, relative to the sample means.²² Though instructive, it is important to note that causal inference cannot be made from these graphs.

We also examine the effect of strikes on several sociodemographic outcomes: the likelihood of being the household head; the likelihood of being married; the number of children in the household; the age of the oldest child; the education level of the partner; and the per capita income of the household. In addition, we analyze intergenerational effects by examining the effect of teacher strikes on two education outcomes of children to individuals who were exposed to strikes in primary school. We first construct a dummy variable that equals 1 if the child is not delayed at school (age of the child minus years of education plus 6 is greater than zero). We then construct a variable of the educational gap of the child, defined by years of schooling plus 6 minus age. We collapse these variables at the household level.

²⁰ To account for potential selection bias influencing our wage and earnings effect, we follow Lee (2009) and estimate worst case scenario treatment bounds.

²¹ The figures are obtained through a model that includes birth year, birth province and EPH fixed effects. See figure notes for information.

²² 180 days is also the difference between the 10th and the 90th percentile of strike exposure among the individuals included in our sample.

4.3 Local Labor Market Controls

One of the main threats to our research design is the possibility that teacher strikes are driven by local labor market conditions, such that the effects we identify do not represent the effect of exposure to teacher strikes during primary school holding all else constant, but rather the effect of teacher strikes *and* local labor market conditions during primary school.

To minimize this identification threat we include two variables in our estimating equation that control for variation in local labor market conditions across provinces and time. First, we collect data on public administration strikes by province and year from CTI (the occupation with the largest number of strikes during our analysis period after teachers) and compute days of exposure to public administration strikes for each birth year - birth province cell during primary school. By controlling for public administration strikes, we exploit variation in teacher strikes net of any general province-specific events and conditions that fuel labor conflict. Second, we collect data on province-specific GDP.²³ We average the province-specific GDP during the seven years of primary school for each birth year -birth province cell.

The local labor market controls reduces the risk that our results are driven by local labor market conditions; such factors have to be uncorrelated with province-specific GDP and public administration strikes and not absorbed by our fixed effects, but correlated with teacher strikes and independently affect the outcomes. One way in which this could happen is if teacher strikes are triggered by province-specific public school conditions. If, for example, poor material conditions or low salaries trigger strikes, and if these factors are not subsumed by our fixed effects and labor market controls, these factors could bias our results (as they could cause lower outcomes even if strikes had not occurred). To ensure that such factors are not driving our results, we have obtained data on teacher wages from the Ministry of Education. By showing that teacher wages are not related to strikes, we argue that our data is inconsistent with the idea that province-specific public school conditions drive our results.

4.4 Short-run Outcomes

To examine if the long-run effects that we identify are present immediately after the children have been subject to teacher strikes, or if the effects develop over time, we complement our main analysis with an analysis on the effect of teacher strikes on outcomes of students who have just finished primary school.²⁴ The data that we use for this analysis come from the 2003-2015 EPH waves for children between 12 and 17 years old. We concentrate on

²³ This data comes from Mirabella (2002), who estimates province GDP using residential electricity consumption.

²⁴ Grade 7 became a part of secondary education in 2002. In this section the treatment variable is still defined as the days of strike while students were in primary school, which is now when the children were between 6 and 13 years old.

educational outcomes since most of these individuals have not yet entered the labor market. These outcomes are: the likelihood of having attended primary school, the probability of attending public school, years of education, the likelihood that the main activity is home production, and the likelihood of being enrolled in secondary school. Unfortunately, we do not have access to any test score data that can provide further evidence on the direct effect of teacher strikes on human capital accumulation. Though this analysis is useful for understanding the channels through which the long-run effects operate, it is important to note that this sample is different from our main analysis sample, and that these individuals were exposed to teacher strikes in a time period different from our main analysis sample. Some caution is therefore encouraged when comparing the results from the two analyses.

5. Empirical Methodology

We exploit cross-cohort variation in exposure to teacher strikes during primary school within and across provinces over time in a dose-response difference-in-difference framework. Specifically, we estimate models of the following form separately for men and women:

$$Y_{pct} = \beta_0 + \beta_1 TS_Exposure_{pc} + \gamma X_{pc} + \phi_t + \vartheta_c + \varphi_p + \delta T_c + \theta T_p + \varepsilon_{pct}, \quad (1)$$

where Y_{pct} is an outcome for respondents born in province p , in birth cohort c and observed in EPH year t . Regressions are weighted by the number of observations in each birth province - birth year - calendar year cell. The variable of interest is $TS_Exposure$ and measures the number of days (in tens of days) that the cohort was exposed to strikes during primary school. Standard errors are clustered on the birth province level.²⁵

Equation (1) includes province (φ_p), birth cohort (ϑ_c) and calendar year (ϕ_t) fixed effects as well as a province-specific linear time trend (θT_p) and a cohort-specific linear time trend (δT_c). θT_p absorbs any trend in Y over time within a province, and δT_c absorbs any trend in Y over time within a birth cohort. Equation (1) further contains a vector of province-specific covariates (X_{pc}) that control for average socioeconomic and demographic characteristics of the province while the cohort was in primary school.

In addition to using equation (1) as defined above, we estimate models that substitute the time trends for birth province-by-calendar year and birth year-by-calendar year fixed effects. The province-by-calendar year fixed effects control for variation in Y that is common

²⁵ As we only have 25 clusters, we also estimate our results using cluster bootstrap with asymptotic refinement (wild cluster bootstrap) as discussed in Cameron and Miller (2015). Online Appendix Table A4 shows that our results are robust to this adjustment.

across birth cohorts within a province in a given year (e.g. province-specific macroeconomic shocks) and the birth year-by-calendar year fixed effects control for any systematic difference across birth years that may be correlated with exposure to teacher strikes and the outcomes of interest. Though more flexible than equation (1), this is a much more demanding specification, in particular bearing in mind our relatively low number of observations. However, our results are robust to which of these specifications we use; results obtained from the more demanding specification are shown in Online Appendix Table A3.²⁶

The unit of observation is a birth province – birth year – calendar year, and the identifying variation stems from cross-cohort variation in exposure to teacher strikes during primary school within and across provinces. There are two main assumptions underlying our estimation strategy. First, that there are no shocks (or other policies) contemporaneous with teacher strikes that differentially affect the different cohorts. The most serious threat to this assumption is that strikes may be caused by political events or economic conditions that vary at the birth province – birth year level and independently affect the outcomes of interest. To limit this identification threat, we control for public administration strikes and provincial GDP during primary school. These controls significantly reduce the risk that our results are driven by local conditions or secular shocks; such shocks would have to be uncorrelated with provincial GDP and public administration strikes but correlated with teacher strikes and have an independent effect on our outcomes (and survive the fixed effects and linear time trends). We also use data on teacher wages to show that the data is inconsistent with the idea that our results are driven by province-specific public school conditions.

The second assumption underlying our analysis is that the timing of teacher strikes must be uncorrelated with prior trends in outcomes across birth cohorts within each province. The conventional method for examining the validity of this assumption is to estimate event-study models that non-parametrically trace out pre-treatment relative trends as well as time varying treatment effects. Our research design does not lend itself well to this approach, and we rely on two alternative methods for illustrating that the timing of teacher strikes is uncorrelated with prior trends in outcomes across birth cohorts within each province.

²⁶ We also perform our analysis using an instrumental variable approach in which we instrument teacher strikes with public administration (pa) strikes. This strategy relies on assumptions that are distinct from those underlying our preferred method: that exposure to pa strikes must be a good predictor of exposure to teacher strikes and that, conditional on the covariates and fixed effects included in the model, exposure to pa strikes cannot have an independent effect on the outcomes of interest. The most serious threat to the exclusion restriction is that pa strikes may have an effect on student outcomes that does not operate through exposure to teacher strikes (which is why we have included exposure to pa strikes as a control variable in equation (1)). However, given the rich set of fixed effects as well as the control for province-specific GDP that we include in our model, this is unlikely. Our main results are robust to this alternative approach. The main take-away from this exercise is that – even if we cannot ascertain the validity of the assumptions underlying either one of our two methods – the fact that our results are robust to which of these methods we use limit the sources of bias that can invalidate our results. The reason is that the two methods rely on completely different sets of assumptions. Results from the instrumental variable approach are available upon request.

First, we incorporate province-specific linear time trends to show that our results are not driven by trends in outcomes across birth cohorts within each province. Second, we reassign the treatment variable for birth cohort c to birth cohort $c-7$, such that the measure of exposure to teacher strikes is the number of days (in tens of days) of primary school strikes that took place while the individuals were 13 – 19 years old. As these individuals have already completed primary school they should be unaffected by these strikes, and the coefficient on $TS_Exposure$ should not be statistically or economically significant.²⁷

It should be noted that a fraction of individuals in each birth province – birth year – calendar year cell attended private primary school (18 percent), and it is unusual for private school teachers to participate in teacher strikes; while public teachers make up approximately 35 percent of all strikes in Argentina, private teachers account for less than 4 percent of all strikes (Chiappe 2011; Etchemendy 2013). As we assign treatment status based on public school teacher strikes, our point estimates will suffer a slight attenuation bias.²⁸

6. Results

6.1 Long-term Effects of Teacher Strikes

i. Educational attainment

Panel A of Table 2 presents gender-specific estimates of the effect of teacher strikes on educational attainment. Each cell in the table comes from a separate estimation of equation (1). Panel A provides strong evidence of adverse education effects associated with teacher strikes. Specifically, ten days of strikes (0.79 percent of primary school) increases the number of both males and females that do not graduate from high school by 30 out of every 1000, and reduces the number of years of education by approximately 0.025.²⁹ These effects represent declines of 0.5 and 0.2 percent relative to the respective means, which are shown directly below the estimates in the table. With respect to tertiary education, we find that ten days of strikes lead to an increase in the number of males that do not complete college with 24 out of every 1000, but that it does not have an impact on females.

The average individual in our sample experienced 88 days of strikes during primary school. Scaling the point estimates to account for this level of exposure suggests that the average male (female) cohort suffered adverse effects with respect to the proportion obtaining

²⁷ 13-19 year olds may have been exposed to strikes as well (though this is unlikely given our discussion in Section 4.1). If strikes are correlated across years within provinces, this model may therefore produce economically and statistically significant results. This makes any null results even more powerful in supporting our identifying assumptions.

²⁸ We could divide our estimates by the fraction affected by the strikes to obtain an approximation of the treatment-on-the-treated effect. However, since some private school teachers participate in strikes, we report the more conservative estimates without adjusting for take-up.

²⁹ Early childhood investments are often argued to yield higher returns than investments that target older children, such that exposure to strikes in early grades may have larger effects. We explore this question in detail in Section 6.2.

a high school diploma, a college degree and years of schooling equivalent to 4.75 (3.69), 12.76 (2.82) and 2.02 (2.02) percent, relative to the means.³⁰ These results demonstrate that teacher strikes not only have adverse short-term education effects (reduction in the proportion that obtain a high school diploma), but that they also impact the education decisions of individuals as they move up the education ladder (proportion that obtain a college degree and years of education).³¹ This is an important finding that has not been documented before.

ii. Employment, labor force participation & home production

Pre-existing research has documented a strong positive relationship between educational attainment and labor market opportunities (e.g. Ashenfelter et al. 1999; Card 1999; Harmon et al. 2003; Heckman et al. 2006).³² This suggests that teacher strikes may also affect students' labor market outcomes. Panel B of Table 2 examines this question in detail, showing estimates for the proportion of individuals who are unemployed, not in the labor force and whose main activity is home production. Looking across the panel, there is clear evidence that strikes lead to an increase in the proportion that is unemployed: 10 days of strikes lead to an increase in the proportion of unemployed individuals by one percentage point among both males and females (1.4 percent relative to the respective means). This demonstrates that the negative education effects of strike-induced school disruptions carry over to the labor market.

With respect to labor force participation, our results suggest that there is no statistically significant effect of exposure to teacher strikes on the probability of being a labor force participant. This suggests that school disruptions caused by teacher strikes have an impact on the intensive margin of employment, but that it does not necessarily change the composition of workers that make up the labor force.³³

Finally, the results in Panel B of Table 2 also show that strikes increase the proportion of people whose main activity is home production, though this effect is only statistically significant among females.³⁴ In terms of effect size, the results suggest that 10 days of strikes

³⁰ This rescaling assumes linear treatment effects. Given the suggestive evidence in Figure 3 this is not an unreasonable assumption.

³¹ In section 6.4 we study the effect of teacher strikes on contemporaneous educational outcomes for children aged 12-17, something that we cannot do for our main analysis sample due to data limitations. This auxiliary analysis reveals negative educational effects consistent with the results for older cohorts discussed in this section.

³² However, it is not necessarily the case that adverse educational effects carry over to the labor market (e.g. Böhlmark and Willén 2017).

³³ It is worth noting that if we control for province-specific linear birth year trends (as in Panel F of Table 7), we do find significant negative labor force participation effects among women (exposure to 10 days of strikes reduces female labor force participation by 0.14 percent relative to the mean). Our inability to detect this effect in our baseline table may therefore be due to secular shifts in labor market opportunities that occurred for women over the cohorts we consider (Blau and Kahn 2013; Bick and Bruggeman 2014; Gasparini and Marchioni 2015).

³⁴ This effect is driven by a slight fall in labor force participation (not statistically significant) combined with a decline in university enrollment. It is not unusual in Argentina to be enrolled at a university after the age of 30.

moves 30 out of every 1000 females into home production. The male estimate is smaller but not statistically significantly different.

iii. Earnings & wages

The adverse employment and education effects identified in Table 2 suggest that teacher strikes may negatively impact earnings and wages as well. This is examined in Panel C of Table 2 with respect to log earnings, log wages and the level of earnings. The results show that both males and females experience reductions in log wages. The table also shows that strikes reduce total earnings of females but not males. However, in terms of effects sizes, the gender-specific estimates are not statistically significantly different from each other. In terms of magnitudes, the results indicate that 10 days of strikes lead to a reduction in male earnings by 0.2 percent (log-specification), wages by 0.3 percent, and total earnings by \$1.8 (level-specification). For females, the numbers are 0.2, 0.2 and \$1.9. Scaling the point estimates to account for the average level of exposure to strikes suggests that men (women) in our sample suffered adverse effects of 1.85 (1.94), 2.82 (1.67) and 2.02 (4.49) percent, respectively.³⁵

One way to interpret the wage effect that we identify is to aggregate it up to the country level and consider the total effect on the economy. While such back-of-the-envelope calculations must be cautiously interpreted, it is informative for understanding the potential magnitude of the effect. Using the point estimates on log wage, we calculate that the annual earnings loss induced by strikes amounts to \$2.34 billion.³⁶ This is equivalent to the cost of raising all primary school teacher wages in Argentina by 62.4 percent, suggesting that it may be worth raising teacher wages if it will prevent them from going on strike.³⁷

The point estimates in Panel C of Table 2 suggest that the return to education in Argentina is about 6 percent.³⁸ This estimate is slightly lower than the pre-existing estimates in Argentina of 7-12.5 percent (Kugler and Psacharopoulos 1989; Pessino 1993; Pessino

³⁵ It is worth pointing out that the wage/earnings effects that we identify may be driven by changes both on the intensive and the extensive margin due to the employment effects identified in Table 2. To overcome this problem, we have used the trimming procedure for bounding treatment effects in the presence of sample selection (in this case, bounding the wage and earnings effects due to the potential selection problem caused by the fact that strikes also impact employment) developed by Lee (2009). To implement this bounding procedure, we first identify the excess number of individuals selected out of the earnings/wage sample because of treatment (identified through our negative employment effect), and then trim the upper and lower tails of the outcome (log wage and log earnings) distributions of each birth year-birth province-survey year cell according to this number multiplied by teacher strikes. This provides us with a worst-case scenario bound. Since the employment effect is relatively modest for males, this exercise does not have a large effect on our estimates and the bounds are very tightly estimated. We find larger bounds for females, consistent with larger employment effect, but the upper bound is always negative, indicating that not all of the effect on earnings is driven by the extensive margin. Specifically, for log earnings we obtain a lower bound of -0.0039 and an upper bound of -0.0026 for males (-0.0045 and -0.0012 for females), and for log wages we obtain a lower bound of -0.0031 and an upper bound of -0.0017 for males (-0.0045 and -0.0006 for females).

³⁶ To obtain this number, we first multiply the gender-specific wage effects with the total gender-specific labor income for the country (Using 2014 EPH data). We then add these two numbers together and scale the sum by the average treatment exposure (88 days). The result is interpreted as the total earning loss for the Argentinean economy if all employed workers were exposed to the average treatment, assuming that the gender-specific effects on earnings are constant across age groups.

³⁷ Teacher wages are approximately \$13,000 a year, and there were 289,812 primary school teachers in 2014.

³⁸ This number is obtained by multiplying the estimated wage effects by 18, as the school year consists of 180 instructional days.

1996; Gasparini et al. 2001; Galiani and Sanguinetti 2003; Patrinos et al. 2005). However, it is important to note that school missed due to sporadic school closures is fundamentally different than less schooling because one leaves school at an earlier age. In one case, curriculum and learning is repeatedly interrupted and in the other it is not. As such, human capital accumulation might be very different and hence the estimated "return" to years of schooling could be very different.³⁹ It is not clear how much one would want to extrapolate from our estimates about returns to schooling that is more general than the impact of disrupted education. Nevertheless, this type of comparison helps anchor our estimates and put the effects in relation to more known education interventions.

The wage and earnings results in Table 2 may conceal important heterogeneous effects across the earnings and wage distributions. We explore this possibility in Table 3 with respect to total earnings (Panel A) and log wages (Panel B). The results in Panel B demonstrate that strikes affect all but the tails of both the male and the female wage distributions. The magnitude of the effect is relatively constant across the different deciles. These results indicate that people in the left tail of the wage distribution would have done equally poorly without strikes, and that people in the right tail of the wage distribution would have done equally well without strikes, while the rest of the individuals would have done better. With respect to total earnings, our results are again very similar across men and women.

To better understand the pattern of these wage and earnings results, Panel C shows results from a similar heterogeneity analysis with respect to educational attainment. These results largely mirror the wage and earnings results in the sense that there are statistically significant and adverse effects across almost all deciles of the distribution, and that the magnitude of the effect is relatively constant across the different deciles.

iv. Occupational quality, informal employment & hours worked

In addition to the extensive margin employment effects that we identify above, the adverse effect of strikes on earnings could be driven by a reduction in work hours or by worse employment conditions. This is examined in Panel D of Table 2, where we look at occupational sorting, hours worked and the proportion that work in the informal sector.

The results show that being exposed to 10 days of strikes in primary school has no effect on hours worked, but it does have a large negative effect on occupational sorting among

³⁹ For example, conventional education economics suggests that the return to education consists of two components – a human capital component and a signaling component (Lange and Topel 2006). While a reduction in formal schooling and strikes may both negatively affect human capital accumulation, only a reduction in formal schooling – and not strikes – will likely affect the signaling value of education.

men.⁴⁰ This effect is not present among women, and we can reject the null hypothesis that there is no difference in effect size across genders. With respect to the average male who was exposed to 88 days of teacher strikes during primary school, the occupational sorting effect represents an effect of 1.32 percent relative to the sample mean.

With respect to the likelihood of working in the informal sector, we find a precise null effect among males but a sizable effect among females. Although the coefficient falls just outside of being statistically significant at conventional levels, the estimate is much larger than its standard error, and in alternative specifications where we include province-specific linear cohort trends (Panel F of Table 6) or where we replace the time trends with two-dimensional fixed effects (Online Appendix Table A3), we find statistically significant effects. We cautiously interpret this as indicative of an effect on the likelihood of working in the informal sector among females. For the average female in our sample who was exposed to the 88 days of teacher strikes during primary school, the increase in the likelihood of working in the informal sector represents an effect of 4.2 percent relative to the mean.

vi. Socioeconomic & intergenerational effects of teacher strikes

There is a large literature documenting a strong positive relationship between an individual's education- and labor market outcomes and his/her socioeconomic position (e.g. Finer and Zolna 2014). Teacher strikes may therefore also impact outcomes such as the likelihood of being married, the probability of being the head of the household, fertility, the educational attainment of the partner, and per capita household income.⁴¹ Table 4 shows results from estimation of equation (1) for each of these outcomes.

Table 4 shows that strikes affect the characteristics of the partners of the individuals that are exposed to teacher strikes. Specifically, the results show that the partners of females that were exposed to strikes are less educated, such that females experience a marriage downgrade with respect to partner skill: being exposed to the average level of strikes during primary school leads to a decline in the years of education of females' partners by 4.7 percent relative to the sample mean. We do not find a significant effect among males. In addition to marriage downgrading, the point estimates in Table 4 show that strikes affect per capita family income: the average individual in our sample is exposed to 88 days of teacher strikes, and this is associated with a decline in per capita household income by around 4.4 percent relative to the

⁴⁰ The results are robust to alternative measures of occupational quality, such as average wage or years of education in one's occupation.

⁴¹ Given the structure of the EPH, we can only identify children of the head, or the spouse of the head, of the household.

sample mean. The effect is not statistically significantly different across genders.⁴² There are no statistically significant effects on the likelihood of being married, the probability of being the head of the household, or on the outcomes regarding fertility decisions.

Given that strikes have adverse effects on student education and labor market outcomes, and also influences other sociodemographic outcomes, there may be intergenerational effects associated with strikes. This question is explored in Table 5, using the intergenerational outcome variables discussed in Section 4 as dependent variables. Across the table, there is evidence of adverse intergenerational education effects among females but not males. In terms of magnitude, being exposed to ten days of strikes in primary school leads to a 0.43 percent increase in the probability that the child is delayed at school, and to an increase in the education gap of 1.45 percent, relative to the respective means. These results have not been documented before, and additional research that examines these questions should be encouraged.

6.2 Heterogeneous Treatment Effects

A large literature has documented that human capital accumulates over time, such that human capital obtained at one point in time facilitates further skill attainment later in life (e.g. Heckman et al. 2006). Therefore, early childhood investments are often argued to yield higher returns than education investments that target older children.⁴³ With respect to the current analysis, this suggests that exposure to strikes in early grades may have larger adverse effects on long-run educational and labor market outcomes.

Table 6 shows the differential effect of strikes in grade years 1 through 4 and 5 through 7 on the long-term education and labor market outcomes. The table provides some evidence that the effects are stronger if strikes occur in early grade years: even though the point estimates on strikes in later grade years oftentimes are not statistically significantly different from the point estimates on strikes in earlier grade years, the standard errors are generally larger such that many of the effects are only statistically significant in early grades. With respect to the magnitude of the effects, only for two outcomes do we find that the effect of teacher strikes in early school grades is statistically significantly different from the effect of teacher strikes in later school grades: years of education and total earnings for females.

Another source of heterogeneity concerns the fact that the effect of several short disruptions may be very different from the effect of one long disruption. Unfortunately, the

⁴² The point estimate on per capita family income is identified off of changes in the labor earnings of the individuals exposed to strikes as well as off of changes in the labor earnings of their partners and household's composition.

⁴³ This argument is also based on research that finds young children to be more receptive to learning (e.g. Shonkoff and Phillips (2000)).

strike data that we have collected only provides us with the number of days of strikes, and the number of strikes, per month and province (not the length of each individual strike). However, even if we cannot identify the duration of each strike, the data allow us to identify the average duration of the strikes that each cohort was exposed to. To obtain suggestive evidence of effect heterogeneity with respect to disruption type, we have therefore estimated our main specification with the continuous treatment measure of strike exposure, but also included a variable that accounts for the average duration of the strikes that the cohort was exposed to (calculated as the total number of days of strikes divided by the total number of strikes). The results from this exercise are shown in Online Appendix Table A10. Looking across the table, we find no evidence that the average duration of the strikes that the cohort was exposed to has an impact on the long-run outcomes we look at independently off the effect of the number of days of strikes. However, the inclusion of this variable strengthens our main results, both with respect to magnitude and statistical significance.

6.3 Robustness & Sensitivity Analysis

The results obtained from our preferred specification support the idea that school disruptions caused by teacher strikes have adverse effects on long-term educational attainment and labor market outcomes. In this section, we explore evidence on whether these results are driven by other policies, trends or events that are not accounted for by the controls in equation (1).

In Panel A and Panel B of Table 7 we exclude the city of Buenos Aires and the province and city of Buenos Aires, respectively. These geographic areas differ slightly from the rest of Argentina, and the purpose of this exercise is to ensure that our results are not exclusively driven by these areas. The results are robust to the exclusion of these regions.

In Panel C we estimate equation (1) without the five provinces that have the highest cross-province mobility rates.⁴⁴ The point estimates produced for this subsample of provinces are not statistically significantly different from our baseline results. This demonstrates that our results are robust to accounting for cross-province mobility.

Panel D eliminates pre-2010 EPH survey years to ensure that our results are robust to a balanced panel of age observations. Despite a dramatic loss of observations (recall that our baseline analysis relies on the 2003-2015 EPH waves), the point estimates are not statistically significantly different from our baseline results when imposing this restriction.

Panel E displays results from estimation of equation (1) when we have reassigned treatment for birth cohort c to birth cohort $c-7$. These cohorts are very close in age and are

⁴⁴ Chaco, Corrientes, Misiones, Rio Negro and Santa Cruz . See Online Appendix Table A1.

likely exposed to similar province-specific macroeconomic environments. However, the *c-7* cohorts have already completed primary school when the documented teacher strikes took place, and if our baseline estimates successfully isolate the effect of teacher strikes on student outcomes, we should not find any statistically effects among these cohorts. None of the point estimates are statistically significant; the results are consistent with the assumption that the strikes are uncorrelated with trends in outcomes across birth cohorts within each province.

Panel F shows results for our preferred specification when province-specific linear birth year trends have been included. These results help us examine if the estimates simply are driven by trends in outcomes across birth cohorts within each province. The results from this exercise are not statistically significantly different from our baseline estimates.

One concern with our analysis is that the results may not be driven by adverse effects of teacher strikes on the outcomes of exposed students, but rather by positive effects of teacher strikes on future student cohorts (both of these stories would produce a negative DID estimator). This would be true if, for example, strikes have no impact on exposed students, but the ability of teachers to strike gives them leverage in negotiations, leading to changes in working conditions that attract a different quality of teachers or affect investments in schooling in a way that benefits future students. To examine this threat to identification, Panel H of Table 7 shows results obtained from reestimating our baseline equation using exposure to strikes prior to school start as our treatment variable (when the students are between 0 to 4 years old). If teacher strikes affect future student outcomes, we would expect this analysis to return significant results. The reason is that strikes that took place before the individuals started school cannot affect their time in school. However, if, for example, teacher strikes positively affect working conditions, strikes can have an impact on the education quality that these individuals experience when they start school. All point estimates are small and not statistically significant, suggesting that our data is inconsistent with this idea.

One of the main threats to valid inference in our paper is that our results are simply picking up differences in outcomes caused by province-specific variation in macroeconomic performance across time. To explore this question, we use post-2003 EPH data (data on local labor markets do not exist before 2003) to examine the relationship between teacher strikes and local labor market conditions. The results from this exercise are shown in Online Appendix Table A6. In Column (1) we show the correlation between teacher strikes and the unemployment rate, the average hourly wages and the average per capita family income. In Column (2) we add days of public administration strikes, calendar year and province fixed

effects as well as province-specific time trends.⁴⁵ Once we add these controls, there is no relationship between the local labor market climate and teacher strikes.

Even if our results are not driven by province-specific variation in macroeconomic performance over time, it could still be the case that province-specific public school conditions are driving the results (e.g. poor material conditions or low wages). If such school conditions are correlated with teacher strikes but not absorbed by our fixed effects, time trends or local labor market controls, they may contaminate our effects as these conditions could have led to lower outcomes of exposed cohorts even if strikes did not happen. To explore this possibility, we look at the relationship between teacher strikes and teacher wages.

The results from this exercise are shown in Online Appendix Table A7. In Column (1) we show the correlation between teacher wages and strikes. In Column (2) we add controls for public administration strikes as well as calendar year and province fixed effects. There is no significant relationship between teacher wage and teacher strikes. These results suggest that our results are not driven by province-specific public school conditions across time.⁴⁶

6.4 Short-run effects

In this section we analyze the effect of teacher strikes on students who have just finished primary school.⁴⁷ The purpose of this exercise is to examine if the strike effects occur immediately after the children have experienced school disruptions, or if they develop over time. We focus on children between 12 and 17 years old when performing this analysis.⁴⁸ We concentrate on educational outcomes since most of these individuals have not yet entered the labor market. These outcomes are: the likelihood of having attended primary school, the probability of attending a public institution, years of education, the likelihood that the main activity is home production, and the likelihood of being enrolled in school. We perform this analysis on the individual level to control for household characteristics.⁴⁹

Table 8 displays the results for each of the above outcomes using two different specifications. Column (1) incorporates the same controls as in our preferred specification.⁵⁰

⁴⁵ The results are robust to the inclusion of the 30th and the 70th percentiles of the per capita family income (intended to capture any effect of a change in the distribution of per capita family income). Results available upon request.

⁴⁶ To explore this further, Online Appendix Figure A1 shows the change in real teacher wages and teacher strikes in each province for the entire period 1996-2009. We find no association between changes in wages and the number of teacher strikes during this period.

⁴⁷ Due to educational reforms during the past two decades, grade 7 became a part of secondary education in 2002, and mandatory education was extended from 7 to 12 years in 1998. In this section the treatment variable is still defined as the days of strike while students were in primary school, which is now when the children were between 6 and 11 years old.

⁴⁸ We exclude birth cohorts 1986-1990 because the role-out of the *Federal Education Law* (1998; approved in 1993) was taking place at a different rates in each province (Alzúa et al. 2015).

⁴⁹ These results are robust to estimation at the aggregate level used in our main analysis. These results are available upon request.

⁵⁰ Except for GDP at the province level for which there is no reliable data available in recent years.

Column (2) incorporates additional local labor market controls (the unemployment rate and the average wage in each province-year) and family characteristics.⁵¹

With respect to females, the results in Table 7 show that there is a decline in public education enrollment of 0.93 percent relative to the mean. This represents a 5.3 percent decline after scaling the coefficient to account for the average level of strikes among these individuals (57 days). We also find an increase in the likelihood of home production by 3.45 percent, and a decrease the probability of being enrolled by 4.02 percent. For males, exposure to strikes reduces the years of education by 0.29 percent relative to the mean. These results suggest that the negative education effects are visible immediately after the students finish primary school. That the short-run effects are smaller than the long-run effects (Table 2) is consistent with the total effect of teacher strikes during primary school becoming more noticeable when all education decisions have been made.

In Section 2.3 we note that there may be heterogeneous treatment effects of teacher strikes with respect to the socioeconomic characteristics of the student's parents: wealthy parents can afford to move their children to private institutions if they believe the strikes hurt their children, and more educated parents are more likely to be capable to replace lost instructional days with home schooling. Even though we do not have information on parental wealth and educational attainment for the individuals included in our main analysis, we can examine this for children that are between 12-17 years old. In Online Appendix Table A8, we estimate the effect of strikes by per capita family income and maximum years of education of the head of the household. Consistent with our predictions, we find evidence that the most affected students are those from the most socioeconomically disadvantaged households.

7. Discussion and Conclusion

Teacher industrial action is a prevalent feature of public education systems across the globe. Despite a large theoretical literature on labor strikes and a reignited debate over the role of teachers' unions in education, there is a lack of empirical work that evaluates the effect of teacher strikes on student outcomes. This paper contributes to the literature by providing a detailed analysis of the effect of teacher strikes on long-run education and labor market outcomes. This is not a full analysis of the benefits and costs associated with teacher strikes, but rather a partial equilibrium analysis that uses teacher strikes to measure the effect of school disruptions on student long-term outcomes.

⁵¹ 4 dummies for province-specific quartiles of per capita family income and 5 dummies for the maximum educational level of the head of the household: primary education or less, incomplete secondary, complete secondary, incomplete tertiary, and complete tertiary.

Our results identify adverse long-run educational and labor market effects for both males and females. For males, we find that school disruptions fueled by teacher strikes lead to a reduction in educational attainment, an increase in the likelihood of being unemployed, occupational downgrading, and has adverse effects on both labor market earnings and hourly wages. The effects are very similar for females, with the exception that there is no effect on occupational sorting. Rather, there is an increase in the probability of engaging in home production. By looking at 12-17 years old, we demonstrate that the negative educational effects are visible immediately after children have finished primary school, and that these effects are concentrated among children from the most vulnerable households. Our analysis reveals that strikes affect individuals on other socioeconomic dimensions as well. Specifically, individuals exposed to teacher strikes have less educated partners and lower per capita family income. We also find adverse intergenerational effects on their children.

The prevalence of teacher strikes in Argentina means that the effect on the economy as a whole is substantial: A back-of-the-envelope calculation amounts to an aggregate annual earnings loss of \$2.34 billion. This is equivalent to the cost of raising the average employment income of all primary school teachers in Argentina by 62.4 percent. This suggests that it may be worth raising teacher wages if this will prevent them from going on strike.

Taken together, our results stress the importance of stable labor relations between government and industry and emphasize the necessity of a good bargaining environment that reduces the number of strikes that students are exposed to. Given that the negative effects that we identify last for years and even generations, both unions and government should make substantial attempts to limit the prevalence of strikes. One policy could be to introduce labor contracts that extend over several years and only allow teachers to strike if a bargaining impasse is reached when renewing these multi-year contracts. This would eliminate sporadic strikes while still allowing teachers to use industrial action as a tool to ensure fair contracts.

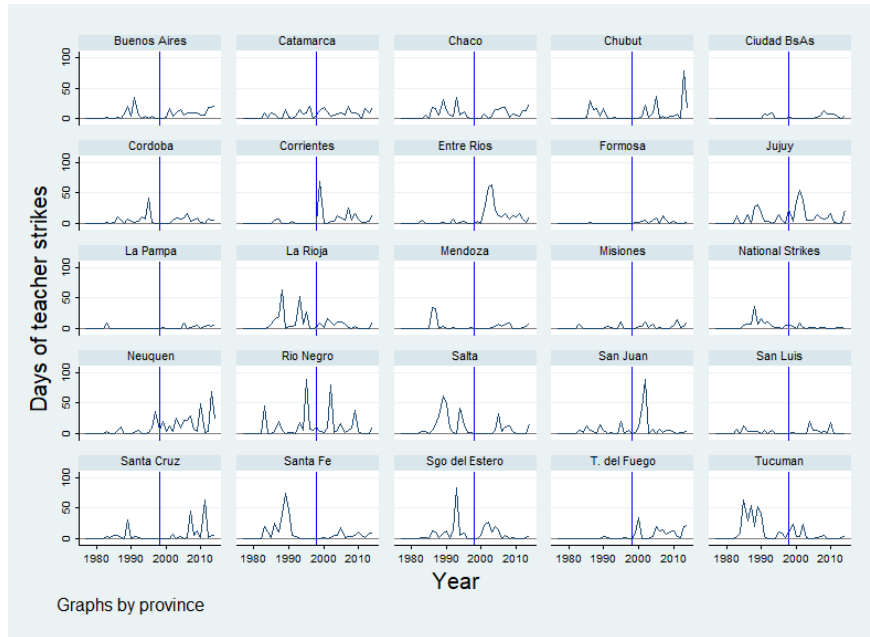
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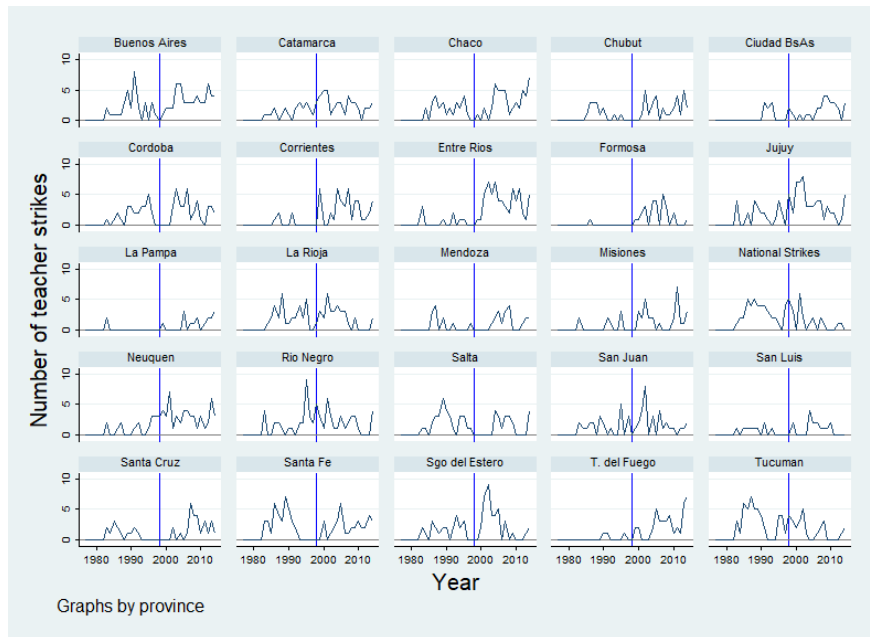
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Figure 1: Variation in Teacher Strikes 1977-2014



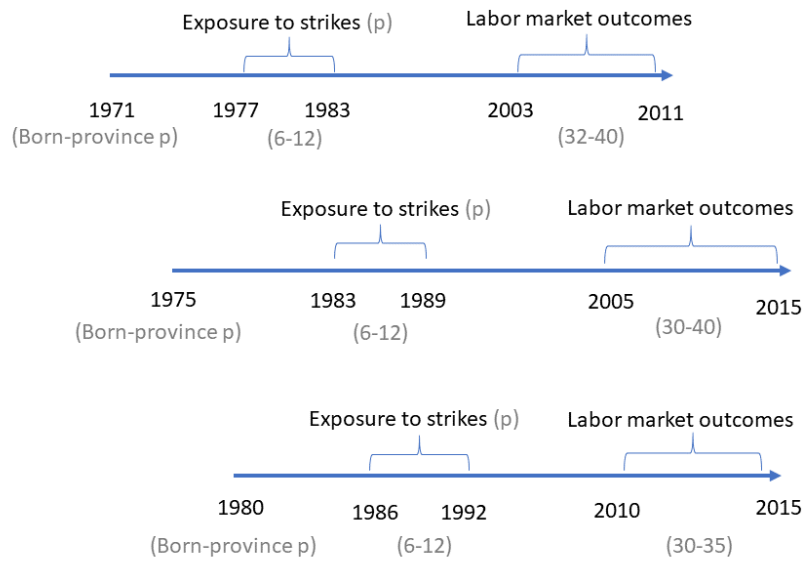
Panel A: Days of Teacher Strikes



Panel B: Number of Teacher Strikes

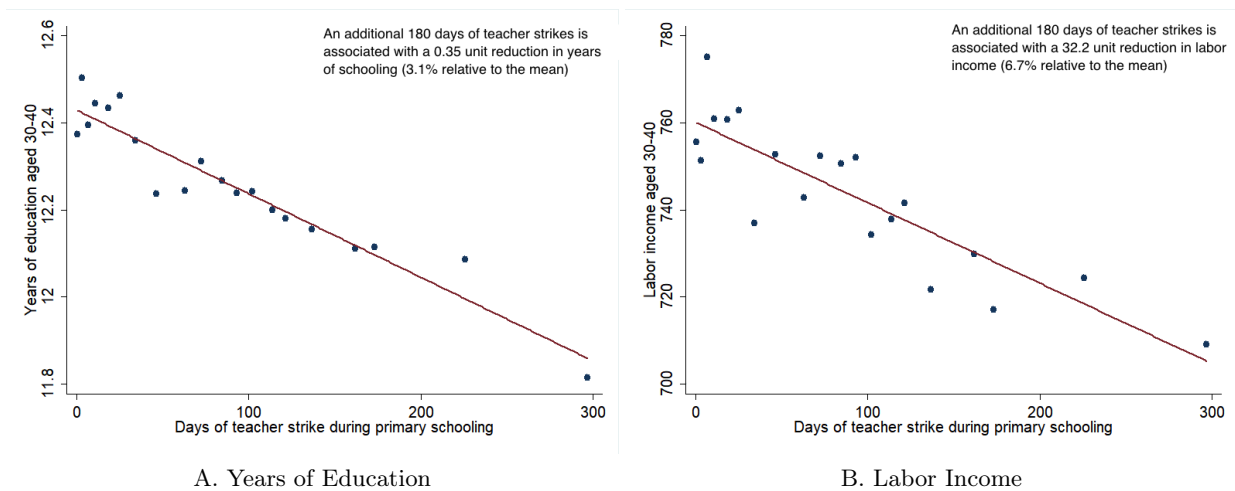
Notes: Authors' tabulations based on historic reports on the Argentine economy published by Consejo Técnico de Inversiones (1977-2014). Panel A shows the number of days of teacher strikes for each province (including national teacher strikes). Panel B displays the number of teacher strikes for each province. The variation to the left of the vertical lines is used in our main analysis when we examine long-run outcomes. The variation to the right of their vertical line is used in our supplemental analysis in Section 6.4 when we look at short-run outcomes.

Figure 2: Data Structure for a Subsample of Birth Cohorts



Notes: Visual illustration of data structure for three cohorts that are part of our main analysis.

Figure 3: Correlation Between Teacher Strikes and Student Outcomes



Notes: The figures are binned scatterplots showing the correlation between (a) teacher strikes and years of education and (b) teacher strikes and labor income. The horizontal axis shows the number of days of teacher strikes during primary school, which varies at birth year- birth province level. The vertical axis shows the average years of education (a) and the average labor income (b) for each birth year- birth province-survey year cell, controlling for province, birth cohort and survey year fixed effects. The data is divided into 20 equally sized groups based on days of strike exposure. Each point correspond to the group average of the variable on the vertical axes. 180 days of teacher strikes is equivalent to a full year of primary school (and the difference between the 10th and the 90th percentile of teacher strike exposure among the individuals included in our sample).

Table 1: Days of Teacher Strikes During Primary School by Birth Cohort and Birth Province

	1971	1972	1973	1974	1975	1976	1977	1978	1979	1980	1981	1982	1983	1984	1985	Mean
Buenos Aires	2	2	3	5	6	14	35	36	71	76	74	77	69	52	50	38
Catamarca	9	11	21	29	29	30	45	38	36	29	35	42	51	56	55	34
Chaco	0	5	5	23	40	45	76	88	88	91	108	97	103	74	62	60
Chubut	0	0	2	31	45	62	65	82	82	80	53	39	23	20	3	39
Ciudad Bs.As.	0	0	0	0	0	0	0	0	7	13	22	22	22	22	22	9
Cordoba	1	1	2	13	19	19	27	30	32	34	34	35	76	70	66	31
Corrientes	0	0	0	5	12	12	12	12	16	16	11	4	4	4	4	7
Entre Rios	6	6	6	6	6	6	8	2	2	10	10	11	15	13	13	8
Formosa	0	0	0	2	2	2	2	2	2	2	0	0	0	0	0	1
Jujuy	12	12	12	27	27	54	85	91	95	98	83	87	75	49	31	56
La Pampa	9	9	9	9	9	9	9	0	0	0	0	0	0	0	0	4
La Rioja	0	1	9	24	44	107	107	110	112	110	147	134	99	98	95	80
Mendoza	0	0	0	35	68	68	72	72	72	74	39	6	6	2	3	34
Misiones	7	7	7	7	7	7	7	0	3	5	5	5	15	15	15	7
Neuquen	4	4	4	9	19	19	19	15	17	22	17	7	9	17	53	16
Rio Negro	45	45	45	49	68	73	73	30	31	31	45	31	114	121	125	62
Salta	4	8	8	13	27	56	118	163	168	170	165	193	178	117	69	97
San Juan	5	7	19	23	27	27	41	41	40	30	25	21	40	26	27	27
San Luis	7	7	19	22	25	28	31	24	29	17	19	16	13	10	10	18
Santa Cruz	4	6	12	17	19	19	49	46	47	42	37	35	35	5	4	25
Santa Fe	19	29	31	56	67	106	180	207	203	205	180	169	130	56	10	110
Sgo del Estero	2	3	3	16	27	29	38	48	47	62	132	126	132	123	111	60
T. Del Fuego	0	0	0	0	0	0	0	4	6	6	6	6	6	8	4	3
Tucuman	4	13	76	105	159	179	232	269	264	201	172	118	109	65	26	133
Mean	6	7	12	22	31	40	55	59	61	59	59	53	55	43	36	40

Notes: Authors' tabulations based on historic reports of the Argentine economy published by Consejo Técnico de Inversiones (1977-1998). The table shows the total number of days of exposure to teacher strikes during primary school for each birth year- birth province cell. Individuals that are born 1971-1985 and are between 30 and 40 years old in 2003-2015 (when the outcomes are measured) are used in our main analysis.

Table 2: Effect of Strike Exposure on Individual Outcomes

Panel A. Educational Attainment			
	High School Diploma	College Degree	Years of Schooling
	Males	Males	Males
	Females	Females	Females
Strike Exposure	-0.0030** (0.0012)	-0.0024*** (0.0006)	-0.0262*** (0.0064)
% Effect	-0.54%	-1.45%	-0.23%
			-0.0217*** (0.0062) -0.18%
Panel B. Employment			
	Unemployed	Not in Labor Force	Home Production
	Males	Males	Males
	Females	Females	Females
Strike Exposure	0.0008** (0.0003)	-0.0004 (0.0005)	0.0009 (0.0005)
% Effect	1.91%	-0.97%	1.30%
			0.0027*** (0.0007) 0.82%
Panel C. Wages and Earnings			
	Log Earnings	Log Wages	Total Earnings
	Males	Males	Males
	Females	Females	Females
Strike Exposure	-0.0021* (0.0012)	-0.0032** (0.0012)	-1.7039 (1.3505)
% Effect	-	-	-0.23%
			-1.9064*** (0.6236) -0.51%
Panel D. Occupational Quality and Work Hours			
	Occupational Sorting	Total Hours	Informal
	Males	Males	Males
	Females	Females	Females
Strike Exposure	-0.0015*** (0.0004)	-0.0111 (0.0417)	-0.0002 (0.0011)
% Effect	-0.85%	-0.03%	-0.06%
			0.0017 (0.0010) 0.48%

Notes: Authors' estimation of equation (1) using 2003-2015 EPH data on 30-to 40 year old respondents that were born 1971-1985. The unit of observation is a birth province - birth year - EPH year, and the sample consists of 2460 observations. Regressions include birth province, birth year and EPH survey year fixed effects as well as controls for local GDP and exposure to public administration strikes during primary school. Regressions further include a cohort-specific and a province-specific linear time trend. Regressions are weighted by the number of individual observations used to calculate the averages for each birth year-birth province-EPH year cell. The coefficient measures the effect of being exposed to ten additional days of teacher strikes in primary school on the respective outcomes. Standard errors are clustered at the birth province level. *** indicates significance at the 1% level, ** indicates significance at the 5% level and * indicates significance at the 10% level.

Table 3: Heterogeneous Effects of Strike Exposure on Wages, Earnings and Educational Attainment

Panel A. Total Earnings										
	10th	20th	30th	40th	50th	60th	70th	80th	90th	
Panel A: Males										
Strike Exposure	0.4344 (1.8023)	-0.2033 (1.0603)	-1.3683 (0.9074)	-1.1062 (0.9234)	-1.3523 (0.9581)	-2.1281* (1.0421)	-3.1197** (1.4134)	-2.3928 (1.8882)	-5.1729 (3.8195)	
% Effect	0.25%	-0.06%	-0.31%	-0.20%	-0.21%	-0.28%	-0.35%	-0.22%	-0.38%	
Panel B: Females										
Strike Exposure	0.0938 (0.0702)	0.2931 (0.3156)	-0.0724 (0.9536)	-2.0409 (1.6880)	-2.4561 (1.7629)	-2.3283 (1.7143)	-3.7698** (1.3419)	-3.1255** (1.4796)	-2.3525 (2.2849)	
% Effect	0.83%	0.98%	-0.10%	-1.39%	-0.98%	-0.63%	-0.75%	-0.47%	-0.26%	
Panel B. Log Wages										
	10th	20th	30th	40th	50th	60th	70th	80th	90th	
Panel A: Males										
Strike Exposure	0.0019 (0.0020)	-0.0025* (0.0012)	-0.0040*** (0.0011)	-0.0041*** (0.0011)	-0.0043*** (0.0013)	-0.0048*** (0.0012)	-0.0045** (0.0016)	-0.0037 (0.0022)	-0.0039 (0.0023)	
Panel B: Females										
Strike Exposure	0.0010 (0.0031)	0.0006 (0.0022)	-0.0004 (0.0015)	-0.0021 (0.0013)	-0.0025* (0.0013)	-0.0035** (0.0012)	-0.0026* (0.0013)	-0.0034** (0.0013)	-0.0022 (0.0021)	
Panel C. Years of Education										
	10th	20th	30th	40th	50th	60th	70th	80th	90th	
Panel A: Males										
Strike Exposure	-0.0122 (0.0079)	-0.0252** (0.0097)	-0.0409* (0.0204)	-0.0296* (0.0157)	-0.0251* (0.0130)	-0.0292** (0.0104)	-0.0358*** (0.0089)	-0.0426*** (0.0133)	-0.0230* (0.0117)	
% Effect	-0.17%	-0.31%	-0.44%	-0.29%	-0.22%	-0.24%	-0.28%	-0.30%	-0.15%	
Panel B: Females										
Strike Exposure	-0.0212*** (0.0071)	-0.0169 (0.0182)	-0.0116 (0.0263)	-0.0414** (0.0168)	-0.0383*** (0.0083)	-0.0540*** (0.0094)	-0.0342** (0.0152)	-0.0292** (0.0106)	0.0065 (0.0074)	
% Effect	-0.29%	-0.20%	-0.12%	-0.38%	-0.32%	-0.42%	-0.25%	-0.20%	0.04%	

Authors' estimation of equation (1) using 2003-2015 EPH data on 30-to 40 year old respondents that were born 1971-1985. The unit of observation is a birth province - birth year - EPH year, and the sample consists of 2460 observations. Regressions include birth province, birth year and EPH survey year fixed effects as well as controls for local GDP and exposure to public administration strikes during primary school. Regressions further include a cohort-specific and a province-specific linear time trend. All outcomes are expressed in 2005 PPP dollars. The % effect is dropped for log wage and log earnings as the point estimates are already interpreted as a percentage change. Regressions are weighted by the number of individual observations used to calculate the averages for each birth year-birth province-EPH year cell. The coefficient measures the effect of being exposed to ten additional days of teacher strikes in primary school on the respective outcomes. Standard errors are clustered at the birth province level. *** indicates significance at the 1% level, ** indicates significance at the 5% level and * indicates significance at the 10% level.

Table 4: Effect of Strike Exposure on Socioeconomic Outcomes

	Head of Household		Married		Number of Kids	
	Males	Females	Males	Females	Males	Females
Strike Exposure	-0.0015 (0.0009)	-0.0010 (0.0007)	-0.0005 (0.0007)	-0.0004 (0.0006)	0.0030 (0.0030)	0.0041 (0.0028)
% Effect	-0.20%	-0.12%	-0.07%	-0.06%	-0.23%	-0.51%

	Age of older kid		Per Capita Family Income		Years of Schooling of Partner	
	Males	Females	Males	Females	Males	Females
Strike Exposure	0.0083 (0.0101)	0.0195 (0.0173)	-0.0046** (0.0019)	-0.0045** (0.0017)	-0.0068 (0.0093)	-0.0370*** (0.0104)
% Effect	0.07%	0.16%	-	-	-0.10%	-0.53%

Notes: Authors' estimation of equation (1) using 2003-2015 EPH data on 30-to 40 year old respondents that were born 1971-1985. The unit of observation is a birth province - birth year - EPH year, and the sample consists of 2460 observations. Regressions include birth province, birth year and EPH survey year fixed effects as well as controls for local GDP and exposure to public administration strikes during primary school. Regressions further include a cohort-specific and a province-specific linear time trend. Educational attainment of the partner is defined for heads of households or spouses to heads of households. Regressions are weighted by the number of individual observations used to calculate the averages for each birth year-birth province- EPH year cell. The coefficient measures the effect of being exposed to ten additional days of teacher strikes in primary school on the respective outcomes. Standard errors are clustered at the birth province level. *** indicates significance at the 1% level, ** indicates significance at the 5% level and * indicates significance at the 10% level.

Table 5: Intergenerational Treatment Effects

	Not Delayed at School		Gap in Years of Education	
	Males	Females	Males	Females
Strike Exposure	0.0012 (0.0015)	-0.0031*** (0.0009)	0.0022 (0.0034)	-0.0073*** (0.0025)
% Effect	0.16%	-0.43%	-0.48%	1.45%

Notes: Authors' estimation of equation (1) using 2003-2015 EPH data on 30-to 40 year old respondents that were born 1971-1985. The unit of observation is a birth province - birth year - EPH year, and the sample consists of 2460 observations. Regressions include birth province, birth year and EPH survey year fixed effects as well as controls for local GDP and exposure to public administration strikes during primary school. Regressions further include a cohort-specific and a province-specific linear time trend. Not being delayed at school is a dummy variable that takes the value of one if the age of the child minus years of education plus 6 is greater than zero. The educational gap is defined as years of schooling plus 6 minus age. Regressions are weighted by the number of individual observations used to calculate the averages for each birth year-birth province- EPH year cell. The coefficient measures the effect of being exposed to ten additional days of teacher strikes in primary school on the respective outcomes. Standard errors are clustered at the birth province level. *** indicates significance at the 1% level, ** indicates significance at the 5% level and * indicates significance at the 10% level.

Table 6: Heterogeneous Treatment Effects of Strike Exposure by School Grade

	Years of Education	Occupational Sorting	Log Wage	Total Earnings	Unemploy.	Home Production
Panel A: Males						
<i>Strike Exposure grade years 1-4</i>	-0.0295*** (0.0099)	-0.0015** (0.0006)	-0.0039** (0.0015)	-1.5495 (1.4588)	0.0011** (0.0004)	0.0010* (0.0006)
<i>Strike Exposure grade years 5-7</i>	-0.0240*** (0.0073)	-0.0014*** (0.0004)	-0.0027 (0.0021)	-1.8088 (1.5825)	0.0006 (0.0004)	0.0008 (0.0006)
Panel B: Females						
<i>Strike Exposure grade years 1-4</i>	-0.0355*** (0.0074)	-0.0011* (0.0006)	-0.0035** (0.0013)	-3.2691*** (0.9841)	0.0013* (0.0007)	0.0027** (0.0013)
<i>Strike Exposure grade years 5-7</i>	-0.0126* (0.0065)	0.0003 (0.0006)	-0.0008 (0.0012)	-0.9976 (0.8975)	0.0006 (0.0005)	0.0026*** (0.0008)

Notes: Authors' estimation of equation (1) using 2003-2015 EPH data on 30-to 40 year old respondents that were born 1971-1985. The unit of observation is a birth province - birth year - EPH year, and the sample consists of 2460 observations. Regressions include birth province, birth year and EPH survey year fixed effects as well as controls for local GDP and exposure to public administration strikes during primary school. Regressions further include a cohort-specific and a province-specific linear time trend. Regressions are weighted by the number of individual observations used to calculate the averages for each birth year-birth province- EPH year cell. The coefficients measure the effect of being exposed to ten additional days of teacher strikes in grade years 1-4 and grade years 5-7 on the respective outcomes. Standard errors are clustered at the birth province level. *** indicates significance at the 1% level, ** indicates significance at the 5% level and * indicates significance at the 10% level.

Table 7: Robustness and Sensitivity Checks

	Years of Schooling	Occupational Sorting	Log Wage	Total Earnings	Unemployed	Home Production
Panel A: Excluding city of Bs.As.						
Male	-0.0262*** (0.0064)	-0.0015*** (0.0004)	-0.0032** (0.0012)	-1.7039 (1.3505)	0.0008** (0.0003)	0.0009 (0.0005)
Female	-0.0217*** (0.0062)	-0.0003 (0.0004)	-0.0019* (0.0010)	-1.9064*** (0.6236)	0.0009* (0.0004)	0.0027*** (0.0007)
Panel B: Excluding province and city of Bs.As.						
Male	-0.0235*** (0.0069)	-0.0012*** (0.0004)	-0.0034** (0.0016)	-0.6997 (1.1583)	0.0005* (0.0003)	0.0007 (0.0006)
Female	-0.0227*** (0.0070)	-0.0004 (0.0004)	-0.0021* (0.0011)	-2.1205*** (0.6666)	0.0008 (0.0005)	0.0027*** (0.0008)
Panel C: Excluding provinces with high migration						
Male	-0.0234*** (0.0055)	-0.0013*** (0.0004)	-0.0030** (0.0014)	-1.5504 (1.4377)	0.0009*** (0.0003)	0.0011* (0.0006)
Female	-0.0228*** (0.0060)	-0.0003 (0.0004)	-0.0026** (0.0011)	-2.0903*** (0.6843)	0.0007* (0.0004)	0.0028*** (0.0008)
Panel D: Balanced panel (survey year greater than 2010)						
Male	-0.0216*** (0.0074)	-0.0015*** (0.0004)	-0.0023** (0.0010)	-1.8006 (1.1935)	0.0008** (0.0004)	0.0012** (0.0004)
Female	-0.0203*** (0.0068)	-0.0001 (0.0006)	-0.0016 (0.0014)	-1.3777 (0.8659)	0.0009 (0.0007)	0.0033*** (0.0010)
Panel E: Reassigning treatment from cohort c to cohort c+7						
Male	-0.0061 (0.0129)	0.0006 (0.0004)	0.0022 (0.0013)	-1.7665 (1.2947)	-0.0003 (0.0002)	0.0002 (0.0005)
Female	-0.0132 (0.0112)	-0.0011 (0.0007)	0.0007 (0.0020)	0.0649 (1.3080)	-0.0002 (0.0005)	0.0002 (0.0010)
Panel F: Including province-specific linear cohort trends						
Male	-0.0192* (0.0094)	-0.0017*** (0.0005)	-0.0045** (0.0020)	-3.9414* (1.9962)	0.0007* (0.0004)	0.0008 (0.0006)
Female	-0.0119 (0.0090)	0.0002 (0.0006)	-0.0020* (0.0011)	-2.9745*** (0.7630)	0.0014** (0.0005)	0.0037*** (0.0008)
Panel G: Eliminating cohorts expose to >200 days of strikes (top 1%)						
Male	-0.0262*** (0.0064)	-0.0015*** (0.0004)	-0.0032** (0.0012)	-1.7039 (1.3505)	0.0008** (0.0003)	0.0009 (0.0005)
Female	-0.0217*** (0.0062)	-0.0003 (0.0004)	-0.0019* (0.0010)	-1.9064*** (0.6236)	0.0009* (0.0004)	0.0027*** (0.0007)
Panel H: Exposure to teacher strikes prior to school start (0-4 years old)						
Male	0.0554 (0.0463)	0.0027 (0.0028)	-0.0022 (0.0071)	1.2707 (5.9759)	0.0012 (0.0020)	-0.0024 (0.0018)
Female	-0.0033 (0.0366)	-0.0020 (0.0038)	-0.0031 (0.0109)	-4.5268 (5.2562)	0.0011 (0.0029)	0.0008 (0.0069)

Notes: Authors' estimation of equation (1) using 2003-2015 EPH data on 30-to 40 year old respondents that were born 1971-1985. The unit of observation is a birth province - birth year - EPH year, and the sample consists of 2460 observations. Regressions include birth province, birth year and EPH survey year fixed effects as well as controls for local GDP and exposure to public administration strikes during primary school. Regressions further include a cohort-specific and a province-specific linear time trend. Panel A exclude the City of Buenos Aires (CABA). Panel B excludes both CABA and the province of Buenos Aires. Panel C excludes the five provinces with the highest cross-province mobility rates (Chaco, Corrientes, Misiones, Rio Negro and Santa Cruz). Panel D eliminates pre-2010 EPH survey years to obtain a balance panel. Panel E shows results from the falsification test where we have reassigned the treatment variable for cohort c to cohort c+7. Panel F incorporates province-specific linear birth year trends to the estimation of equation (1). Panel G drops the top 1 percent of the teacher strike exposure distribution. Regressions are weighted by the number of individual observations used to calculate the averages for each birth year-birth province-year. Panel H looks at the effect of teacher strikes when the individual was between 0 and 4 years using 30-37 year old respondents that were born 1980-1985 (since we do not have strike information for older cohorts during their first years). The coefficient in Panels A through G should be interpreted as the effect of being exposed to teacher strikes for ten extra days during primary school. The coefficient in Panel H should be interpreted as the effect of being exposed to teacher strikes for ten extra days when between 0 to 4 years old. Standard errors are clustered at the birth-province level. *** indicates significance at the 1% level, ** indicates significance at the 5% level, and * indicates significance at the 10% level.

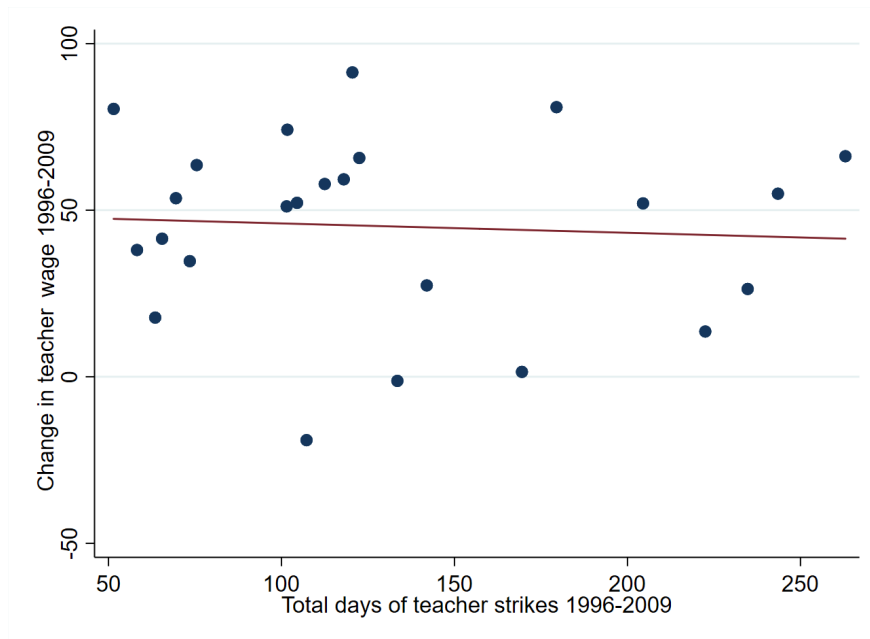
Table 8: Short-Term Effects of Strike Exposure (12-17 Year Olds)

	Public Education		Years of Education		Home Production		Not Enrolled	
	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)
Panel A: Males								
Strike Exposure	-0.0025 (0.0030)	-0.0014 (0.0028)	-0.0235* (0.0130)	-0.0243* (0.0119)	0.0018 (0.0016)	0.0018 (0.0015)	0.0031 (0.0023)	0.0031 (0.0023)
% Effect	-0.32%	-0.18%	-0.29%	-0.31%	2.96%	2.96%	3.89%	3.89%
Panel B: Females								
Strike Exposure	-0.0074* (0.0041)	*-0.0060 (0.0035)	-0.0051 (0.0102)	-0.0063 (0.0106)	0.0021* (0.0012)	0.0022* (0.0012)	0.0032** (0.0014)	0.0032** (0.0015)
% Effect	-0.93%	-0.75%	-0.05%	-0.07%	3.45%	3.61%	4.02%	4.02%
Controlling for wage and unemployment		X		X		X		X
Controlling for household characteristics		X		X		X		X

Notes: Authors' estimation of equation (1) using 2003-2015 EPH data on 12 to 17 year old respondents. Column (1) show results using individual-level data and the same controls as in our baseline specification. The specification used to produce the results in Column (2) incorporates local labor market variables that may influence the wealth of the family: the unemployment rate and the average wage in each province. It further includes 4 dummies of province-specific quartiles of per capita family income and 5 dummies for the maximum educational level of the head or spouse of the household (primary education or less, incomplete secondary, complete secondary, incomplete tertiary, and complete tertiary). Public education is a dummy variable equal to one if attending a public school. Home production is a dummy that equals 1 if the respondent is neither working nor studying. Standard errors are clustered at the birth province level. The coefficients are interpreted as the effect of being exposed to teacher strikes for ten extra days during primary school. *** indicates significance at the 1% level, ** indicates significance at the 5% level and * indicates significance at the 10% level.

Online Appendix: Not for publication.

Figure A1: Correlation Between Teacher Strikes and Teacher Wages



Notes: The figure is a binned scatterplot showing the relationship between teacher strikes and teacher wages. The horizontal axis shows the days of teacher strikes between 1996 and 2009, which varies at province level. The vertical axis depicts the change in real teacher wage over the same period for each of the provinces of Argentina. Each point corresponds to one specific province in Argentina 180 days of teacher strikes is equivalent to a full year of primary school.

Table A1: Cross-Province Mobility of
13 Year Olds

Province	Fraction Non-movers
Buenos Aires	0.979
Catamarca	0.963
Chaco	0.855
Chubut	0.930
Ciudad Bs.As.	0.999
Cordoba	0.947
Corrientes	0.850
Entre Rios	0.905
Formosa	0.942
Jujuy	0.932
La Pampa	0.952
La Rioja	0.968
Mendoza	0.947
Misiones	0.836
Neuquen	0.979
Rio Negro	0.715
Salta	0.943
San Juan	0.949
San Luis	0.945
Santa Cruz	0.835
Santa Fe	0.975
Sgo del Estero	0.942
T. del Fuego	0.943
Tucuman	0.952

Notes: Authors' tabulations using 2003-2015 EPH data on 13 year old respondents. The table shows the fraction of 13 year olds that live in the same province that they were born. Bold numbers represents the five provinces with the smallest fractions of non-movers.

Table A2: Dependant Variable Means

	Male	Female
<i>Panel A: Educational Attainment</i>		
Secondary Education Completed	0.559	0.620
Years of Education	11.178	11.731
Tertiary Education Completed	0.166	0.248
<i>Panel B: Employment</i>		
Unemployment	0.042	0.066
Not in Labor Force	0.041	0.312
Home Production	0.069	0.329
Informal Sector	0.309	0.354
Hours Worked	42.265	21.239
Occupational Sorting	0.177	0.284
<i>Panel C: Wage and Earnings</i>		
Log Total Earnings	6.489	6.123
Total Earnings	731.8	372.3
Log Wage	1.255	1.257
<i>Panel D: Other Socioeconomic Outcomes</i>		
Head of Household or Spouse	0.743	0.801
Married	0.716	0.688
Number of Children	1.353	1.671
Log Per Capita Family Income	6.791	6.650
Years of Schooling of Partner	11.732	10.357
Age of older kid	11.331	12.315
<i>Panel D: Intergenerational Outcomes</i>		
Not Delayed at School	0.728	0.714
Gap in Years of Education	-0.462	-0.503

Notes: Authors' tabulations using 2003-2015 EPH data on 30-40 years old respondents born between 1971 and 1985. Home production is defined as neither working nor studying. Informality is defined as the share of employed workers that are salaried employee in a small firm (less than 5 employees), are self-employed without a university degree, or are family workers with zero earnings. Occupational sorting is evaluated by constructing an index of occupation quality based on the proportion of workers in each occupation with more than a high school degree. Not being delayed at school is a dummy variable that takes the value of one if the age of the child minus years of education plus 6 is greater than zero. The educational gap defined as years of schooling plus 6 minus age.

Table A3: Effect of Strike Exposure on Individual Outcomes; Two-dimensional Fixed Effects

Panel A. Educational Attainment									
	High School Diploma		College Degree		Years of Schooling				
	Males	Females	Males	Females	Males	Females			
Strike Exposure	-0.003** (0.001)	-0.003** (0.001)	-0.003*** (0.001)	-0.001 (0.006)	-0.026*** (0.007)	-0.024*** (0.006)			
% Effect	-0.52%	-0.47%	-1.39%	-0.40%	-0.23%	-0.20%			
Panel B. Employment									
	Unemployed		Not in Labor Force		Home Production				
	Males	Females	Males	Females	Males	Females			
Strike Exposure	0.001** (0.000)	0.001* (0.000)	-0.000 (0.001)	0.001 (0.001)	0.001 (0.001)	0.003*** (0.001)			
% Effect	1.67%	1.22%	-0.73%	0.29%	1.30%	0.85%			
Panel C. Wages and Earnings									
	Log Earnings		Log Wages		Total Earnings				
	Males	Females	Males	Females	Males	Females			
Strike Exposure	-0.002* (0.001)	-0.002 (0.001)	-0.003*** (0.001)	-0.002* (0.001)	-1.653 (1.264)	-1.772** (0.663)			
% Effect	-	-	-	-	-0.23%	-0.48%			
Panel D. Occupational Quality and Work Hours									
	Occupational Sorting		Total Hours		Informal				
	Males	Females	Males	Females	Males	Females			
Strike Exposure	-0.001*** (0.000)	-0.000 (0.000)	-0.010 (0.043)	-0.019 (0.058)	-0.000 (0.001)	0.002** (0.001)			
% Effect	-0.79%	-0.14%	-0.02%	-0.09%	-0.03%	0.54%			

Notes: Authors' estimation of equation (1) using 2003-2015 EPH data on 30-to 40 year old respondents born between 1971 and 1985. The unit of observation is a birth province - birth year - EPH year, and the sample consists of 2460 observations. Regressions include birth province, birth year and EPH survey year fixed effects as well as controls for local GDP and exposure to public administration strikes. Regressions further include birth province by EPH survey year and birth year by EPH survey year fixed effects. Regressions are weighted by the number of individual observations used to calculate the averages for each birth year-birth province- EPH year cell. The coefficient measures the effect of being exposed to ten additional days of teacher strikes in primary school on the respective outcomes. Standard errors are clustered at the birth province level. *** indicates significance at the 1% level, ** indicates significance at the 5% level and * indicates significance at the 10% level.

Table A4: P-values from Wild Cluster Bootstrap Standard Errors Method

	Years of Education	Occupational Sorting	Log Wage	Total Earnings	Unemployment	Home Production
Panel A: Males						
Strike Exposure	-0.026**	-0.002**	-0.003**	-1.704	0.001**	0.001
P-Value from Wild Cluster Bootstrap Standard Error Method	0.029	0.016	0.032	0.275	0.045	0.134
Panel A: Females						
Strike Exposure	-0.022**	-0.0000	-0.002	-1.906*	0.001	0.003**
P-Value from Wild Cluster Bootstrap Standard Error Method	0.044	0.682	0.119	0.057	0.143	0.022

Notes: Authors' estimation of equation (1) using 2003-2015 EPH data on 30 to 40 year old respondents born between 1971 and 1985. The unit of observation is a birth province - birth year - EPH year, and the sample consists of 2460 observations. Regressions include birth province, birth year and EPH survey year fixed effects as well as controls for local GDP and exposure to public administration strikes. Regressions further include a cohort-specific and a province-specific linear time trend. Regressions are weighted by the number of individual observations used to calculate the averages for each birth year-birth province- EPH year cell. The p-values show the probability of observing the given coefficient value under the null hypothesis of no effect, estimated using the wild cluster method with Rademacher 2 point distribution following Cameron and Miller (2015). The bootstrap uses 999 replications. To facilitate interpretation of the results, stars (*) have been used after the coefficient estimates to indicate which level the coefficient estimates were significant at when the standard errors were clustered at the birth province level. *** indicates significance at the 1% level, ** indicates significance at the 5% level and * indicates significance at the 10% level.

Table A5: Effect of Controlling for Non-teacher Strikes and GDP

	Years of Education	Occupational Sorting	Log Wage	Total Earnings	Unemploy.	Home Production
Panel A: Without controls for PA strikes and GDP						
<i>i. Male</i>						
Strike Exposure	-0.0233*** (0.0064)	-0.0015*** (0.0004)	-0.0034*** (0.0010)	-2.1796* (1.1480)	0.0008*** (0.0003)	0.0006 (0.0004)
<i>ii. Female</i>						
Strike Exposure	-0.0176*** (0.0053)	-0.0003 (0.0003)	-0.0020*** (0.0007)	-2.5964*** (0.6296)	0.0010** (0.0004)	0.0029*** (0.0007)
Panel B: With controls for PA strikes and GDP						
<i>i. Male</i>						
Strike Exposure	-0.0262*** (0.0064)	-0.0015*** (0.0004)	-0.0032** (0.0012)	-1.7039 (1.3505)	0.0008** (0.0003)	0.0009 (0.0005)
PA Strike Exposure (0.0123)	0.0004 (0.0009)	-0.0005 (0.0035)	-0.0014 (2.3253)	-2.0821 (0.0004)	-0.0001 (0.0008)	-0.0010
GDP	-1.4222*** (0.3645)	-0.0355 (0.0271)	-0.0345 (0.0871)	-6.9421 (67.6629)	-0.0020 (0.0184)	0.0132 (0.0323)
<i>ii. Female</i>						
Strike Exposure	-0.0217*** (0.0062)	-0.0003 (0.0004)	-0.0019* (0.0010)	-1.9064*** (0.6236)	0.0009* (0.0004)	0.0027*** (0.0007)
PA Strike Exposure	0.0121 (0.0110)	-0.0005 (0.0009)	-0.0012 (0.0020)	-3.3382** (1.4787)	0.0002 (0.0014)	0.0009 (0.0014)
GDP	-0.7139 (0.4904)	-0.0662* (0.0365)	-0.0531 (0.0468)	-74.3703 (62.6525)	-0.0049 (0.0328)	0.0406 (0.0546)

Notes: Authors' estimation of equation (1) using 2003-2015 EPH data on 30-to 40 year old respondents born between 1971 and 1985. The unit of observation is a birth province - birth year - EPH year, and the sample consists of 2460 observations. All regressions include birth province, birth year and EPH survey year fixed effects. Regressions further include a cohort-specific and a province-specific linear time trend. Panel A excludes controls for public administration strikes and province-specific GDP while Panel B includes these controls. Regressions are weighted by the number of individual observations used to calculate the averages for each birth year-birth province-year. The coefficient is interpreted as the effect of being exposed to teacher strikes for ten extra days in primary school. Standard errors are clustered at the birth province level. *** indicates significance at the 1% level, ** indicates significance at the 5% level and * indicates significance at the 10% level.

Table A6: Effect of Local Labor Market Conditions on Teacher Strikes

	Teacher Strikes	
	(1)	(2)
Unemployment rate	0.6355** (0.2591)	1.1255 (0.9366)
Average wage	0.3605 (0.6432)	-1.8366 (5.0689)
Average per capita income	0.0016* (0.0009)	-0.0072 (0.0061)
Public administration strike exposure		X
Province FE		X
Year FE		X
Province-specific time trends		X
R-squared	0.047	0.407

Notes: Authors' estimation of equation (1) using 2003-2015 EPH data and strike data from CTI. The unemployment rate, average wages and average per capita family income describe the labor market conditions for each province and year. The results in Column (1) are based on a specification that regresses the number of days of teacher strikes during the period 2003-2015 on labor market conditions not controlling for any other factor. Column (2) adds days of strikes in public administration, calendar year and province fixed effects, and province-specific time trends. Regressions are weighted by the number of individual observations used to calculate the averages for each province-year. Standard errors are clustered at the province level. *** indicates significance at the 1% level, ** indicates significance at the 5% level and * indicates significance at the 10% level.

Table A7: Effect of Teacher Wages on Teacher Strikes

	Teacher Strikes	
	(1)	(2)
Teacher wage year t	0.0102 (0.0126)	-0.0119 (0.0130)
Teacher wage year t-1	0.0103 (0.0133)	0.0229 (0.0212)
Teacher wage year t+1	-0.0177 (0.0114)	0.0272 (0.0174)
Public administration strike exposure		X
Province FE		X
Year FE		X
Province-specific time trends		X
R-squared	0.018	0.569

Notes: Authors' estimation of equation (1) using 1996-2009 data on teacher wages from the Ministry of Education in Argentina and strike data from CTI. The wages correspond to the wages of primary school teachers with 10 years of experience in each province and year. Both columns include province and calendar year fixed effects. Standard errors are clustered at the province level. The coefficient is interpreted as the effect of 10 days of teacher strikes on teacher wages. *** indicates significance at the 1% level, ** indicates significance at the 5% level and * indicates significance at the 10% level.

Table A8: Heterogeneous Treatment Effects of Strike Exposure on Short-Term Outcomes (12-17 Year Olds)

	<i>Male</i>				<i>Female</i>			
	Public School	Years of School	Home Prod.	Not Enrolled	Public School	Years of School	Home Prod.	Not Enrolled
Panel A: Stratification by Parental Education								
At most primary education	-0.0073** (0.0034)	-0.0513*** (0.0151)	0.0030 (0.0019)	0.0043 (0.0032)	-0.0087** (0.0038)	-0.0256 (0.0202)	0.0011 (0.0025)	0.0023 (0.0027)
Some secondary education	-0.0039 (0.0030)	-0.0307* (0.0160)	0.0019 (0.0018)	0.0038 (0.0030)	-0.0089** (0.0034)	-0.0077 (0.0122)	0.0029* (0.0016)	0.0041** (0.0019)
Secondary education	-0.0011 (0.0029)	-0.0181 (0.0114)	0.0020 (0.0014)	0.0032 (0.0020)	-0.0037 (0.0034)	-0.0101 (0.0136)	0.0017 (0.0011)	0.0028* (0.0014)
Some tertiary education	0.0019 (0.0034)	-0.0176* (0.0097)	0.0013 (0.0019)	0.0019 (0.0024)	-0.0027 (0.0034)	0.0032 (0.0101)	0.0024* (0.0013)	0.0031** (0.0014)
Tertiary education	0.0009 (0.0042)	-0.0087 (0.0130)	0.0012 (0.0013)	0.0017 (0.0016)	-0.0051 (0.0060)	0.0067 (0.0083)	0.0012 (0.0009)	0.0022** (0.0010)
Panel B: Stratification by Family Income								
First quartile	-0.0059 (0.0044)	-0.0339** (0.0144)	0.0021 (0.0017)	0.0041 (0.0031)	-0.0110** (0.0052)	-0.0220 (0.0165)	0.0033* (0.0017)	0.0050** (0.0020)
Second quartile	-0.0022 (0.0022)	-0.0353** (0.0136)	0.0029 (0.0018)	0.0047* (0.0024)	-0.0077* (0.0039)	-0.0069 (0.0097)	0.0025 (0.0015)	0.0033* (0.0017)
Third quartile	0.0024 (0.0019)	-0.0179 (0.0162)	0.0011 (0.0017)	0.0021 (0.0025)	-0.0020 (0.0021)	-0.0003 (0.0101)	0.0017 (0.0011)	0.0024* (0.0012)
Fourth quartile	-0.0010 (0.0049)	-0.0080 (0.0089)	0.0009 (0.0012)	0.0012 (0.0016)	-0.0052 (0.0059)	0.0027 (0.0090)	0.0012 (0.0008)	0.0023** (0.0010)

Notes: Authors' estimation of equation (1) using 2003-2015 EPH data on 12 to 17 year old respondents. The results are based on individual-level regressions and use the same controls as that in column (2) of Table 7. Panel A interacts the treatment variable with 5 dummies for the maximum educational level of the head or spouse of the household (primary education or less, incomplete secondary, complete secondary, incomplete tertiary, and complete tertiary). Panel B interacts the treatment variables with 4 dummies of province-specific quartiles of per capita family income. Standard errors are clustered at the birth province level. The coefficients are interpret as the effect of being exposed to teacher strikes for ten extra days in primary school. *** indicates significance at the 1% level, ** indicates significance at the 5% level and * indicates significance at the 10% level.

Table A9: Effect of Strikes During Secondary School

	Years of Education	Occupational Sorting	Log Wage	Total Earnings	Unemploy.	Home Production
Panel A: Male						
Primary Exposure	-0.0157 (0.0119)	-0.0012* (0.0006)	-0.0023 (0.0025)	-2.0373 (1.4239)	0.0013*** (0.0004)	0.0019*** (0.0006)
Secondary Exposure	0.0131 (0.0130)	0.0003 (0.0007)	0.0021 (0.0029)	0.2899 (2.9300)	0.0007 (0.0005)	0.0013* (0.0008)
Panel B: Female						
Primary Exposure	-0.0188** (0.0076)	0.0003 (0.0006)	0.0001 (0.0018)	-1.9758* (1.1483)	0.0015*** (0.0005)	0.0024** (0.0010)
Secondary Exposure	0.0020 (0.0093)	0.0006 (0.0007)	0.0033 (0.0019)	-0.2375 (1.0948)	0.0007 (0.0005)	-0.0004 (0.0010)

Notes: Authors' estimation of equation (1) using 2003-2015 EPH data on 30-to 40 year old respondents born between 1971 and 1985. Panel A defines strike exposure as all the teacher strikes recorded in the historic CTI documents that took place during the years in which cohorts were supposed to attend primary and secondary school (age 6 to 17). Panel B divides the independent variable of interest into two: exposure to teacher strikes in primary school (age 6 to 12) and in secondary school (13 to 17). The unit of observation is a birth province - birth year - EPH year, and the sample consists of 2460 observations. All regressions include birth province, birth year and EPH survey year fixed effects. Regressions further include a cohort-specific and a province-specific linear time trend. Regressions are weighted by the number of individual observations used to calculate the averages for each birth year-birth province- EPH year cell. The coefficient measures the effect of being exposed to ten additional days of teacher strikes. Standard errors are clustered at the birth province level. *** indicates significance at the 1% level, ** indicates significance at the 5% level and * indicates significance at the 10% level.

Table A10: Effect of Teacher Strikes on Individual Outcomes, Accounting For Average Duration of Strikes

	Years of Schooling	Occupational Sorting	Log Wage	Total Earnings	Unemployed	Home Production
Panel A: Male						
Treatment	-0.0262** (0.0094)	-0.0016*** (0.0005)	-0.0038** (0.0016)	-3.0675* (1.5472)	0.0010** (0.0004)	0.0016*** (0.0005)
Average strike duration	0.0209 (0.0321)	0.0002 (0.0014)	-0.0014 (0.0062)	0.7146 (4.1989)	-0.0011 (0.0013)	-0.0018 (0.0018)
Panel B: Female						
Treatment	-0.0173** (0.0070)	-0.0008* (0.0004)	-0.0028* (0.0014)	-3.7888*** (0.9512)	0.0008 (0.0006)	0.0040*** (0.0010)
Average strike duration	0.0116 (0.0395)	0.0027 (0.0019)	0.0035 (0.0025)	6.0249 (4.1639)	-0.0002 (0.0017)	-0.0060 (0.0038)

Notes: Authors' estimation of equation (1) using 2003-2015 EPH data on 30-to 40 year old respondents that were born 1971-1985. The unit of observation is a birth province - birth year - EPH year, and the sample consists of 2460 observations. Regressions include birth province, birth year and EPH survey year fixed effects as well as controls for local GDP and exposure to public administration strikes during primary school. Regressions further include a cohort-specific and a province-specific linear time trend. Standard errors are clustered at the birth-province level. *** indicates significance at the 1% level, ** indicates significance at the 5% level, and * indicates significance at the 10% level.