The Effect of Teacher Strikes on Parents

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Teacher industrial action is one of the leading causes of temporary school closures in the world. These events leave millions of families struggling with disrupted childcare arrangements and changes in routines each year, and may have important consequences for the labor market outcomes of parents. This paper presents the first detailed analysis on the topic, exploiting recently digitalized data on teacher strikes in Argentina in a dose-response triple difference framework. Mothers respond to teacher strikes by dropping out of the labor force and this translates into a large reduction in labor earnings: 10 days of strike-induced school closures during the previous year reduces monthly labor earnings by almost 3 percent relative to the mean. A back-of-the-envelope calculation suggests that this amounts to an aggregate loss of more than $117 million dollars in Argentina each year. With respect to men, only fathers with lower predicted earnings than their spouses experience adverse labor market effects. Access to alternative care options mute some of the effects of strike-induced school disruptions on parental labor market outcomes.

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1. Introduction

Teacher industrial action is one of the leading causes of temporary school closures in the world, and over the past few years numerous countries across the globe - from Tunisia in North Africa to India in South Asia - have documented large and persistent numbers of teacher strikes.[1] These temporary school closures leave millions of families struggling with disrupted childcare arrangements and changes in routines each year, and may have important consequences for the labor market outcomes of parents. This is especially the case for more vulnerable subgroups of parents, such as low-income mums, who may find it particularly difficult to secure alternative childcare options. Unfortunately, a lack of exogenous variation in teacher strikes linked to parental labor market data has precluded a detailed analysis on this topic. As a consequence, we lack a complete understanding of how families are affected by the childcare crises that emerge from school closures, inhibiting the design of effective policy responses.

In this paper, we exploit recently digitalized data on teacher strikes in Argentina to present the first comprehensive analysis on the effect of teacher strikes on parental labor market outcomes. Between 2003 and 2014, Argentina experienced 649 teacher strikes of different lengths. These strikes translated into a loss of 94 instructional days for the average province, equivalent to more than half a year of schooling. As can be seen in Table 1, both the across- and within-province variation over time is substantial, ranging from 0 days in Ciudad de Buenos Aires in 2005 to 78 days in Chubut in 2013. This makes Argentina an ideal setting for studying our question of interest: How do teacher strikes impact the labor market outcomes of parents?

Identifying the effect of teacher strikes on parents is difficult due to the potential existence of contemporaneous shocks or policies. Specifically, strikes may be correlated with other events that occur concurrently with the strikes that also affect the labor market outcomes of parents. To overcome this challenge and isolate the effect of strikes on parents, we use a dose-response triple difference design. We exploit the fact that teacher strikes are concentrated to the primary school level, and compare the difference in outcomes between parents with and without children in primary school in provinces and years that experienced more strike days to that same difference in provinces

[1] In *Africa*, there have been month-long strikes in Tunisia and Algeria; In *South America*, high and persistent strike action has been observed in Uruguay, Chile, Venezuela and Bolivia; In *Central America*, some states in Mexico have lost more than 350 days of school in the last 14 years due to teacher strikes; In *Oceania*, last year saw the first teacher strike in South Australia in 10 years, and witnessed walkouts of more than 50,000 teachers in New Zealand; In *Asia*, large-scale and long-lasting strikes have been observed across the entire continent, including Iran, the Philippines and India; In *Europe*, 90,000 teachers in the Netherlands, as well as thousands of teachers in Bulgaria, Lithuania, Poland, France, Italy and the UK, have partaken in strike events. In the *US*, the number of labor strikes that took place in 2018 was the highest it has been in over three decades, and over one million American children were kept out of school for a combined total of more than a month due to strikes.

and years that experienced fewer strike days. This design enables us to control for potential confounders and isolate the effect of strikes on parents.

Using the triple difference framework described above, we relate strikes in the previous year to current labor market outcomes.[2] We find robust evidence that teacher strikes negatively impact the labor market participation of mothers, and that this translates into a significant reduction in earnings. Specifically, a mother whose child is exposed to ten days of teacher strikes in the previous year is 2.8 percent less likely to be employed, and suffers a decline in total earnings equivalent to 2.9 percent, relative to the respective means. A back-of-the-envelope calculation suggests that this negative effect on labor earnings amounts to an aggregate loss of more than $117 million in Argentina each year.

In contrast to its impact on mothers, teacher strikes have no effect on the labor market outcomes of fathers in general. However, they do negatively impact the labor supply of fathers with lower predicted earnings than their wives. This suggests that the labor supply response of parents depend, at least in part, on the relative income of each parent (Blundell, Chiappori and Meghir 2005; Apps and Rees 2012; Cherchye, De Rock and Vermeulen 2012). Yet, the number of households in which the mother earns more than the father is small, and the estimated effect for this subgroup of fathers is significantly smaller than the estimated effect for mothers. Consequently, teacher strikes increase labor market and intra-household gender inequality. Given the prevalence of teacher strikes across the globe, this may represent an undocumented obstacle to gender equality.

The final set of results that we present is that the availability of alternative childcare options may reduce some of the adverse effects of strikes on parental labor supply. Specifically, we show that parents who lives with other non-working adults are less likely to exit the labor force; that parents who work in the public sector (where contracts are much more likely to include provisions that allow parents to take a temporary leave of absence to provide homecare) are less affected by the strikes; and that certain parents respond to the strikes by transferring their children to private school (where they are isolated from the public teacher strikes). These results demonstrate that the existence of alternative childcare options may lessen the adverse effects of teacher strikes on parental labor market outcomes. However, we also show that this likely comes at the expense of increased socioeconomic school segregation, as teacher strikes only induce children from certain backgrounds to migrate from public to private school.

[2] Teacher strikes could have a short-term effect in which parents drop out of the labor force only for the duration of the school closure, or a long-term effect in which parents drop out of the labor force for a more extended period of time. Due to data limitations, our analysis relates strikes in the past year to current labor market outcomes. Thus, the effects we identify are reflective of the more persistent effects caused by strikes, and are net of any short-term transitory effects.

The main identifying assumptions underlying our estimation strategy are common to all dose-response triple difference specifications: there can be no province-specific shocks that occur concurrently with the strikes that differentially impact parents with and without children in primary school, and the timing of the strikes must be uncorrelated with trends in the difference in labor market outcomes between parents with and without children in primary school.

We provide extensive evidence that our data are consistent with these assumptions. In particular, our results are robust to controlling for local labor market conditions, controlling for province-specific strikes in professions outside the teaching sector, including province-specific linear time trends, including province-by-year fixed effects, and excluding parents with high exposures to teacher strikes. This means that province-specific secular trends or shocks that can bias our results must occur concurrently with the teacher strikes and differentially impact parents with children in primary school and parents without children in primary school, but be uncorrelated with factors such as non-teacher strikes and local labor market conditions.[3]

In addition, we perform two placebo tests. First, we reassign treatment from *t-1* to *t+1* and show that there are no effects of future strikes on current outcomes. Thus, parents do not systematically anticipate and respond to future strikes (something which would attenuate our results). Second, we estimate dose-response difference-in-difference models separately for our treatment and control groups (exploiting only variation across provinces in a given year and within provinces over time). This exercise demonstrates that the effects identified in our main analysis are driven exclusively by changes in outcomes among individuals in the treatment group. Taken together, the results from the robustness tests and the placebo tests are inconsistent with a violation of the identifying assumptions, and support a causal interpretation of our results.

This paper provides the first comprehensive analysis on the effect of teacher strikes on the labor market outcomes of parents.[4] It complements the existing literature in several ways. First, there is a rich literature examining the relationship between teacher strikes and student outcomes. While the results from earlier research are mixed (e.g. Zwerling 2008; Thornicroft 1994; Zirkel 1992; Caldwell and Jeffery 1983; Caldwell and Maskalski 1981), more recent studies have found consistent negative effects on student achievement (e.g. Baker 2013; Johnson 2011; Belot and Webbink 2010) and long-term labor market outcomes (Jaume and Willén 2019). Our paper

[3] For example, a province-specific recession would only bias our results if it was correlated with the number of teacher strikes in that province, but uncorrelated with factors such as non-teacher strikes and local labor market conditions, and if it differentially affected parents who had children aged 11 (in primary school) and parents who had children aged 12 (not in primary school).

[4] In a *mimeo* from 2013, Dunbar examine the effects of school disruptions on parents. However, Dunbar (2013) looks exclusively at effects on earnings, and the limited variation in school disruptions makes it difficult for the author to identify precise effects. Specifically, Dunbar (2013) looks at how income changes for around 400 families in the US that were affected by 1 of 23 strikes that occurred between 1993 and 2006. The author finds that one strike day reduces annual income by 0.1 percent.

complements this strand of research by providing the first comprehensive analysis on the impact of strikes on parents, and helps provide a more comprehensive understanding of the overall effects of teacher strikes on families. Our results show that the cost of teacher industrial action is significantly different than previously thought, as prior estimates have abstracted away from the effect on parents.

Second, there is a large literature that studies how changes in childcare costs affect parental labor supply (e.g. Heckman 1974; Blau and Robins 1988; Connelly 1992; Ribar 1992; Ermisch 1993; Kimmel 1998; Anderson and Levine 1999), and a related literature exploring how the availability of various childcare options interact with parental labor market behavior (e.g. Gelbach 2002; Chiuri 2003; Lefebvre and Merrigan 2008; Berlinski, Galiani, and Gertler 2009; Cascio 2009; Goux and Maurin 2010; Havnes and Mogstad 2011; Fitzpatrick, Grissmer, and Hastedt 2011; Fitzpatrick 2012; Nollenberger and Rodriguez-Planas 2015). None of these papers examine the effect of abrupt and temporary changes in childcare costs/options. Further, these papers have focus exclusively on the effects of expanded childcare services and reduced childcare costs (i.e. positive childcare shocks). Our paper complements these strands of literature and helps provide a more complete understanding of the relationship between childcare and parental labor supply.[5]

In terms of policy implications, our results highlight that teacher strikes have a substantial impact on the labor market outcomes of parents. Conventional solution to teacher strikes – the provision of make-up days at the end of the semester –therefore needs to be revised or supplemented as it deals only with the impact of strikes on student learning. One solution could be to hire non-teacher substitutes that staff the schools during teacher strikes, so that schools do not close during teacher walkouts. While costly, this was a solution favored by the Los Angeles school district during their 2019 district-wide teacher strike. As the frequency of teacher industrial action is increasing across the globe, this expanded understanding of the implications of teacher industrial action is imperative for designing policies that minimize fallout arising from labor conflicts in the education sector.

The rest of the paper proceeds as follows. Section 2 discusses the economic intuition underlying our analysis and introduces institutional details about the educational system and teacher strikes in Argentina; Section 3 presents the data and our empirical strategy; Section 4 discusses our

[5] In the studies that examine the relationship between childcare costs and parental labor supply, childcare costs are often perceived as a tax that lowers net wages and reduces a parent's probability to participate in the labor market. Some of the most credible studies within this field have exploited policy-induced shifts in childcare subsidies as sources of exogenous variation (see for example Brink, Nordblom, and Wahlberg 2007; Lundin, Mörk, and Öckert 2008; Blau and Tekin 2007). In general, these studies find evidence of the expected negative correlation between female labor supply and childcare costs, though the estimates of labor supply elasticities vary greatly (see Blau and Currie 2006).

main results; Section 5 presents robustness checks; Section 6 extends our main results by looking at household level outcomes and the probability of attending a private school; Section 7 concludes.

2. Background and Institutions

2.1. Economic Intuition

Programs and services such as universal preschool and primary schooling allow caregivers to substitute childcare responsibilities for employment. In the event of a teacher strike, parents can no longer outsource childcare responsibilities to schools. These events thus lead to an increase in the cost of childcare, to which parents can respond in a number of different ways: (1) take a temporary leave of absence to provide short-term home care,[6] (2) work and leave the child at home or with other family members, (3) work and purchase private care, and (4) drop out of the labor force to provide home care. Which of these options to choose depends on a number of factors, such as the relative cost and quality of the different alternatives, the parents' wages and income, the parents' care preferences, and household or family composition (e.g. number of adults in the households and location of close relatives).

In this paper, we are interested in disentangling the effect of teacher strikes on the labor market outcomes of parents, corresponding to alternative (4) above. A priori, it is not clear to what extent this represents the optimal response of parents. The resulting predictions of the effect of teacher strikes on parental labor supply are therefore ambiguous, necessitating an empirical analysis on the topic. It should be noted that (4) could reflect a transitory short-term response in which parents drop out of the labor force only for the duration of the school closure, or for a more extended period of time.[7] Both the transitory short-term effects, as well as the more persistent effects, are of economic interest. However, due to data limitations, our analysis relates strikes in the past year to current labor market outcomes. Thus, the effects we identify are reflective of the more persistent effects caused by strikes, and are net of any short-term transitory effects.

In addition to examining the direct impact of strikes on the labor market outcomes of parents – alternative (4) above – it is instructive to also explore alternatives (1) through (3). An improved

[6] Certain sector-specific collective agreements in Argentina include conditions that allow individuals to take a set number of days of leave, per year, for personal reasons. Parents under these agreements can therefore provide homecare for a few days per year without having to quit their jobs. However, these leave provisions are only included in a handful of sector-specific collective agreements and, even in these cases, the "leave for personal reasons" is restricted to very precise reasons and short periods.

[7] There are several reasons for strikes to have a long-lasting effect on parents. For example, labor market frictions or employment status switching costs may prevent parents to re-enter the labor force (Ridder and van den Berg 2003); reservation wages may be state-dependent such that it could increase with non-employment spells (Heckman and Borjas 1980); and there may be persistent adverse effects of lost labor market opportunities (Oreopoulos et al. 2012).

understanding of how strikes interact with these alternative responses help inform us about the role of access to other childcare options in mitigating the direct impact of strikes on the labor market outcomes of parents. To the best of our knowledge, we are the first in the literature to provide suggestive evidence on the parental use of each of these options. In Section 4.2 we examine alternative (1) (by looking at parents in the public sector where non-pecuniary benefits are more generous and it is easier to secure temporary leave of absence), in Section 6.1 we study alternative (2) (by examining the importance of household composition), and in Section 6.2 we provide suggestive evidence on alternative (3) (by examining the probability of purchasing and placing the child in private education). We show that the presence of these alternative options likely serves to reduce the impact of strikes on parental labor supply. This is an important finding, demonstrating that certain subgroups of parents – those with limited access to alternative childcare arrangements – are disproportionally hurt by teacher industrial action.

In addition to heterogeneous effects across different subgroups of parents, strike-induced temporary school closures (TSCs) may also impact within-household gender inequality. Specifically, assume that a rational household allocates the responsibility to provide home care such that the household's utility is maximized. The within-household decision on who should provide home care (conditional on the household choosing home care) will then be a function of the relative quality of the parents' home care, their wages, and their preference for childcare. Abstracting away from any potential gender differences in childcare quality and preferences, the existing gender wage gap suggests that mothers will be more likely to assume the childcare responsibilities in the event of TSCs. It is also possible that the parent that remains in the labor force attempts to compensate for the income loss that the household experiences by increasing work hours. This implies that mothers may be disproportionally affected by strike-induced school closures, and that these events may exacerbate the existing labor market and intra-household gender inequality (e.g. Gauthier, Smeeding and Furstenberg 2004; Guryan, Hurst and Kearney 2008; PRC 2015).

2.2 The Argentinian Education System

Argentina is divided into 24 provinces, including one autonomous city, and education falls under the joint jurisdiction of the national government and the provinces. The system is divided into four stages: kindergarten, primary education, secondary education and tertiary education. Primary education begins the calendar year in which the number of days the child is 6 years old is maximized, comprises the first six years of schooling, and is mandatory. The majority of children attend public schools, which is financed through a revenue-sharing system between the provinces and the federal government. Spending on education has increased steadily over the past 40 years. Argentina currently spends around 5.5 percent of GDP on education, equivalent to countries such as Austria, Canada, the UK and France. Public school is free at all levels, from kindergarten through tertiary education.

The fraction of students that attended private school at the primary level during our analysis period was approximately 0.23, increasing from 0.2 in 2003 to 0.26 in 2014. Private school is paid, though provinces often subsidizes the cost at the primary and secondary level. Recent research suggests that the increase in private school enrollment over this period was driven by high- and middle-income families (Gasparini et al. 2011; Jaume 2013). Although teacher strikes are usually mentioned in informal conversations and local newspaper articles as a potential explanation for this selective migration from public to private schools, current studies find no effect of primary school teacher strikes on the likelihood of being enrolled at a public primary school (Narodowski and Moschetti 2015, Narodowski, Moschetti and Alegre 2016).[8]

2.3. Teacher Strikes

The goal of this paper is to examine the parental labor market effects of temporary school closures caused by teacher strikes in Argentina. The strikes we consider consist of primary school teacher strikes between 2003 and 2014. In this section, we provide a brief overview of the history of teacher strikes in Argentina. For a more detailed account of teacher industrial action in Argentina, see Jaume and Willén (2019).

Ever since the reintroduction of democracy in 1983, industrial action and labor strikes have been persistent features of the Argentine economy. The most active social protesters are public primary school teachers, who make up more than 35 percent of all strikes in the country (Etchemendy 2013). In comparison, private primary school teachers account for less than 4 percent of the country's strikes. The occupation with the second-highest level of strikes is public administration, making up 25 percent of the strikes in the country (Etchemendy 2013).

Teacher unions are typically organized at the province level, and variation in teacher strikes across time and provinces is substantial. While there are national teacher strikes taking place at times, their occurrences are rare and short-lived. In theory, days cancelled due to adverse circumstances must be rescheduled. In practice, the prevalence of teacher strikes across time means that this rarely happens. The overwhelming majority of teacher strikes in Argentina are concentrated exclusively among public primary schools teachers, and almost all primary school teachers in a province partake in province-specific strikes.[9]

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[8] However, Jaume and Willén (2019) finds a negative effect of TSCs during primary school on the likelihood of attending a public secondary school.

[9] It should be noted that a handful of the strikes we use may affect secondary school children. However, parents to children in secondary school are unlikely to respond to those strikes as they occur very infrequently and as children in secondary school are old enough to be left alone at home (ages 12 through 17). If parents to secondary school children were affected by some of the strikes, our point estimates would be attenuated (i.e. biased toward

Table 1 shows the variation in teacher strikes across provinces and over time in Argentina between 2003 and 2014. During this period, Argentina experienced 649 teacher strikes of different lengths, with substantial variation across time and provinces, ranging from 0 days in La Pampa in 2003 to 78 days in Chubut in 2013. The total number of strike days during this period across all provinces was 2,251, which translate into a loss of 94 instructional days for the average province. This is equivalent to more than half a year of schooling (the school year in Argentina consists of 180 instructional days). This provides us with substantial variation in strike-induced school closures that parents are exposed to over a long time period.

Table 1 further demonstrates that the strike activity in Argentina is not driven by a small number of provinces that consistently experience a large number of strikes. For example, while Entre Rios had the highest number of strike-induced school closures in 2003, it only had the 11th highest number in 2014. Another example is Chaco, which had the highest number of TSC in 2014 but only the 10th highest number in 2003. One of the few exceptions to this across-year rank variation is Neuquén, which had a similar number of TSCs in 2003 and 2014 (ranked second highest in both years). However, even for Neuquén, there is substantial variation over time, from 2 TSCs in 2011 to 68 TSCs in 2013. In Section 3, we explore within-province correlation in TSCs in greater detail.

The pervasiveness of teacher strikes over time and across provinces in Argentina does not appear to be related to local labor market conditions or province-specific school conditions (e.g. Murillo and Ronconi 2004; Narodowski and Moschetti 2015; Naradowski, Mochetti and Alegre 2016; Jaume and Willén 2019). While Murillo and Ronconi (2004) find some evidence that teacher strikes are more common in provinces where union density is high and political relations with the local government is tense, Narodowski and Moschetti (2015) concludes that strikes display an erratic behavior without any discernable trends or explanations. Narodowski, Mochetti and Alegre (2016) reach a similar conclusion, and suggests that policies such as higher wages, national collective bargaining agreements, and improved work hours, have been unable to moderate teacher strikes in the country. The results from Jaume and Willén (2019) mirror those of Narodowski and Moschetti (2015), as well as those of Narodowski, Mochetti and Alegre (2016), showing that teacher strikes are not correlated with local labor market conditions (wages, unemployment rate, GDP growth) or province-specific public school conditions (e.g. teacher wages). This is illustrated in Appendix Tables A1 and A2, which show the relationship between teacher strikes and local labor

finding no effect) and akin to a lower-bound estimation. In other words, it would work against us identifying significant effects. One way to examine the presence of such attenuation is to restrict the sample to parents with children in primary school and estimate dose-response difference-in-difference models (only using the variation across provinces at a given time and within provinces over time). The results from this exercise are shown in Panel A of Table 9, and the fact that the point estimates are not statistically significantly different from our baseline results suggest that this attenuation is small-to-negligible.

market conditions (Table A1) and teacher wages (Table A2).

Rather than being driven by local labor market conditions and province-specific school conditions, the prevalence of teacher strikes is likely related to the long history of collective bargain and strong unions in the country, combined with high levels of national inflation and abrupt business cycles that increase the volatility of individual's real wages. Due to these factors, wages are arduously negotiated at the province level on a year-to-year basis, and it may take several months to settle. The contracts may also permit the re-opening of wage negotiations in a given year, especially in years when inflation is expected to be large. Despite these negotiation processes, industrial action in the teaching sector in Argentina is not associated with long periods of pre-strike action. Rather, parents usually only learn about a strike (but never about its duration) the day before it occurs.[10]

It is important to note that our analysis does not require that strikes are exogenous to province-specific trends, labor market conditions and shocks. It only requires that such factors do not differentially affect parents with children in primary school and parents without children in primary school. Thus, for our results to be biased by such confounding factors, it would have to be the case that there are secular trends or shocks (or other policies) within a given province that occur concurrently with the strikes in that province and that are correlated with the outcomes of parents to children in primary school but not correlated with the outcomes of parents to children that just completed primary school. Nevertheless, it is reassuring to note that pre-existing studies find evidence in favor of a stricter version of our identifying assumption.

In Section 5, we provide additional evidence in support of our identifying assumption. One exercise we perform in this section is to show that local school conditions (proxied by teacher wage) are unable to explain the variation in strikes that we exploit. Another test we perform is to examine the sensitivity of our results to controlling for local labor market conditions. A third exercise we perform is to control for exposure to non-teacher strike, effectively running a "horse race" by comparing the impact of teacher strikes and non-teacher strikes on the labor market behavior of parents. The results from these exercises show that non-teacher strikes and local labor market conditions cannot explain any of our results, and that controlling for such factors has no impact on the teacher strike estimates.

3. Data and Estimation Strategy

3.1. Data

[10] During the bargain period, unions sometimes announce that they may initiate a strike on a certain day in the future if some demands, usually related to wage increases, are not met by the provincial government. However, it is extremely difficult for parents to anticipate the result of these bargain processes. Further, the result of the negotiation is typically announced to the public only the day before the union's deadline.

Our data on teacher strikes come from historic reports on the Argentine economy published by Consejo Técnico de Inversiones (CTI). The CTI reports provide province-specific information on strikes by industry and month. A detailed description of how we collected and digitalized this data is available in Jaume and Willén (2019). For the empirical analysis in this paper, we use information from 2003 to 2014. Figure 1 shows the number of TSCs that took place in the previous 12 months by year and quarter for each of the provinces in Argentina.

We combine the teacher strike information with 2004-2014 Encuesta Permanente de Hogares (EPH) data, a household survey representative of the urban population of Argentina (91 percent of the population). The data contain information on year and quarter of the interview, and for every parent we construct a variable that equals the number of days of teacher strikes in the province of residence during the past year. This variable varies by the year and quarter that an individual was interviewed in, as well as by the province that an individual lives in, and represents our main treatment variable of interest.

We assume that children attend primary school between the ages of 6 and 11, and look at how the number of strikes days in the previous year, within a province, affect parents with children of primary school age compared to parents with children that are not of primary school age.[11] Figure 2 provides a visual depiction of the structure of our data and shows when the outcomes are measured relative to when the treatment is measured for two of the quarters used in our analysis. As noted in Section 2, teacher strikes could have a short-term effect in which parents drop out of the labor force only for the duration of the school closure, or a long-term effect in which parents drop out of the labor force (or suffer earnings loses) for a more extended period of time. Due to data limitations, our analysis relates strikes in the past year to current labor market outcomes. Thus, the effects we identify are reflective of the more persistent effects caused by strikes, and are net of any short-term transitory effects.[12] However, we do acknowledge that the choice of a 12-month treatment window, although consistent with the structure of our data, is arbitrary. In Section 5, we show results using alternative treatment windows ranging from 6 months to 36 months, in 6-month intervals.

The EPH data contain a rich set of labor market variables, all of which we use to examine the parental labor market response to TSCs. To study labor market participation effects, we look at the probability of being in the labor force, the probability of being employed and the probability of being unemployed. To better understand the channels through which any potential employment

[11] In Jaume and Willén (2019), primary education in Argentina is defined for children aged 6 through 12. However, starting in 2002, grade 7 became a part of secondary education. Consequently, primary school is now defined for children aged 6 through 11.
[12] Specifically, the EPH only provides information about the quarter in which the household survey was conducted. We don't know when during the quarter the interview took place. Thus, relating strikes in that quarter to the parent's labor market outcomes in the same quarter would be misleading, since we can't disentangle if the interview took place before, or after, the strike. See Section 3.2 for more detailed information.

effects operate, we study the probability of holding multiple jobs, hours worked, and the probability of holding a part-time (less than 35 hours a week) as well as a full-time (more than 35 hours a week) position. To quantify the sum total of all these effects, we look at total monthly labor earnings and hourly wages. Earnings, wages and hours worked are set to zero for those who do not report any income or work activity. Descriptive statistics of the variables we use are provided in Table 2.

We impose three sample restrictions. First, we restrict our sample to individuals that have at least one child under the age of 18. Second, parents to toddlers (children younger than 3 years old) are much less likely to be in the labor force due to parental leave and childrearing, and we therefore exclude these parents from our analysis. Third, we drop parents that experienced more than 30 days of school disruptions in the previous year (top 1 percent). These three restrictions are imposed to ensure that we have a comparable control and treatment group, that our results are representative of the parents we are interested in, and that the effects are not driven by outliers. In Section 5, we document the sensitivity of our results to relaxing these restrictions.

In addition to using the full EPH sample, we take advantage of the survey's rotating panel design and construct successive panels covering the period 2004-2014. The EPH panel design is a standard 2-2-2 scheme: each dwelling is interviewed in two consecutive quarters, then left out of the sample for two quarters, and then interviewed for another two quarters. The EPH does not follow movers. We use this data to perform a number of heterogeneity and subgroup analyses, each described in Section 4 and Section 6. It should be noted that the panels are subject to a relatively high rate of attrition, and we can only track 34.6 percent of our sample every year (without attrition the match rate would be 50 percent). Even though the tracked and untracked parents are similar in demographic characteristics (Appendix Table A3), and even though the probability of attrition does not appear to be correlated with the TSCs, we interpret results based on these data with caution.[13]

3.2. Estimation Strategy

We restrict our sample to parents with at least one child under the age of 18, and examine the difference in labor market outcomes between parents with and without children in primary school in provinces and years with more TSCs to that same difference in provinces and years with fewer TSCs. Specifically, we estimate models of the following form separately for mothers and fathers:

$$Y_{ipt(q)} = \gamma_0 + \alpha_1 Str_{pt(q)-1} + \alpha_2 Ch_{ipt(q)-1} + \beta\left(Str_{pt(q)-1} \cdot Ch_{ipt(q)-1}\right) + \gamma X_{ipt(q)-1} + \rho_p + \tau_{t(q)} + \varepsilon_{ipt(q)}, \qquad (1)$$

[13] The correlation between TSCs in the previous year and attrition is -0.02, and the point estimate obtained from running our preferred model specification (equation 1) with attrition as the dependent variable is 0.0011 with a standard error of 0.0022.

where $Y_{ipt(q)}$ is one of the labor market outcomes listed above for individual i in province p in year and quarter *t(q)*. $Str_{p\ t(q)-1}$ measures the number of TSC days that occurred in province p during the past year (in 10s of days). The variable $Ch_{ipt(q)-1}$ is a dichotomous variable taking the value of 1 if individual *i* had a child in primary school during the last year. The interaction term $Str_{p\ t(q)-1} \cdot Ch_{ipt(q)-1}$ is the variable of interest, and β measures the intent-to-treat effect of strike-induced TSCs during the past year on parental labor market outcomes.[14] Our control group consists of parents to children that have not yet started primary school and parents to children in secondary school.

Equation (1) also includes province (ρ_p) as well as year-by-quarter ($\tau_{t(q)}$) fixed effects. The province fixed effects control for variation in outcomes that are common across all individuals within a province. Thus, if some provinces are systematically more prone to teacher strikes (or tend to have longer strikes), and if parents to primary school children in these provinces have particularly poor labor market outcomes, that will be absorbed by ρ_p. The year-by-quarter fixed effects control for national shocks that impact all individuals in a given year and quarter. We also control for potential experience, potential experience squared, educational attainment and number of children in the household. These controls are in vector X. Standard errors are clustered at the province level.[15]

Conditional on the controls in the model, our effects are identified off of differences in outcomes between parents with and without children in primary school in provinces and years that experienced more strike days to that same difference in provinces and years that experienced fewer strike days. The key assumption underlying the identification of parameter β is that, conditional on the controls and fixed effects included in equation (1), there are no province-specific shocks contemporaneous with the number of teacher strike days in the previous year, that differentially affect the labor market outcomes of parents with and without children in primary school. In other words, our results would be biased if there were province-specific local shocks, uncorrelated with the year-by-quarter fixed effects and demographic controls (potential experience, potential experience squared, educational attainment and total number of children in the household), but

[14] Our estimation strategy is similar to a dose-response triple difference framework (DDD), with the first difference coming from variation across regions, the second difference coming from variation across years, and the third difference coming from comparing parents with and without primary school children. However, a strict DDD is more demanding than equation (1) as it requires the inclusion of a much larger number of fixed effects (province-by-year fixed effects, year-by-dummy of having a child in primary school fixed effects, and province-by-dummy of having a child in primary school fixed effects), such that the model must estimate more than 1,000 additional parameters. This is particularly problematic for our sub-group analyses, which are performed on relatively limited numbers of observations. Thus, our preferred specification is equation (1). However, in Table 8 Panel B we show that our results are robust to the strict DDD specification.

[15] As we only have 24 clusters, we also estimate equation (1) using wild cluster bootstrap standard errors (Cameron and Miller 2015). Our results are robust to this adjustment. See Section 4.

correlated with the number of strike days in the previous year and the current labor market outcomes of the parents, but only for those parents with children that are between 6 and 11 years old.

When discussing the results produced by equation (1), it is also useful to consider potential within-province correlation in teacher strike days over time. Specifically, if there is within-province correlation in TSCs across years, such that there is a culture of strikes in the province, then parents may have taken that into account when making their initial labor market decisions, and perhaps selected into more flexible part-time jobs. This is not a threat to our identification assumption due to our triple difference design and the inclusion of province and time fixed effects. However, understanding the extent of the within-province correlation is helpful for interpreting our results. Specifically, to the extent that TSCs are correlated across years within provinces, the effects we estimate will be a *lower bound* of the true impact of TSCs (i.e. push us towards finding no effects), since it has already been incorporated into the parents' initial labor decision. However, the within-province correlation in TSCs across years is very small, and the attenuation bias caused by such correlation is therefore likely negligible. Thus, we do not believe that the effects we identify should be considered as lower bounds in this respect.

There are five pieces of evidence suggesting that within-province correlation in TSCs across years likely do not attenuate our results. First, the within-province correlation in teacher strikes across time is very small, and never exceeds 0.13. This is illustrated in Appendix Table A4. Second, TSCs in the previous three years have no predictive power on TSCs in year *t* in regressions that control for province and time fixed effects (Appendix Table A5). Third, estimating autoregressive models of degree 1 for each of the provinces return statistically significant estimates for only two provinces, and our main results are robust to dropping these provinces (Appendix Tables A6 and A7, respectively). Fourth, we show below that future strikes do not impact current outcomes, suggesting that expectations about strike exposure is not driving the results. Finally, the effects of strikes in the previous twelve months on current outcomes are robust to controlling for strikes that took place prior to this time window, something we demonstrate in Section 4.4.

In Section 5, we perform a series of robustness and sensitivity checks to demonstrate that the data we use is consistent with a causal interpretation of our results. In particular, our results are robust to controlling for local labor market conditions, controlling for province-specific strikes in the non-teaching sector, including province-specific linear time trends, including province-by-year fixed effects, and excluding parents with high exposures to TSCs.

In addition, we perform two placebo tests. First, we reassign treatment from *t-1* to *t+1* and show that there are no effects of future strikes on current outcomes. Second, we estimate dose-response difference-in-difference models separately for our treatment and control groups. The dose-response difference-in-difference framework is easier to interpret and allows us to ensure that the effects identified in our main analysis are driven by changes in outcomes among individuals in the treatment group. However, it relies on a stricter identification assumption: that there are no province-specific secular trends, policies or shocks correlated with both teacher strikes and labor market outcomes. While a violation of this assumption (e.g. teachers striking as response to local labor market shocks) would bias the difference-in-difference estimates, it is not a threat to our triple difference specification since the control group is exposed to the same local labor market conditions. Because it is not possible to fully guarantee this stricter assumption, our main results are based on the triple difference approach. Yet, one concern with the triple difference approach is that the control group may not be perfectly comparable, and it is therefore appropriate to examine the robustness of the results to altering the model specification. Reassuringly, as we show in Section 5, our results do not change when using the difference-in-difference framework.

Taken together, the results in Section 5 are inconsistent with the presence of province-specific shocks contemporaneous with teacher strikes in the previous twelve months that differentially affect the outcomes of parents with and without children of primary school age.

4. Results

4.1 Main Results

Table 3 presents baseline estimates of the effect of strike-induced school disruptions on the labor market behavior of mothers (Panel A) and fathers (Panel B). The estimates show changes in labor market outcomes from 10 days of TSCs for the respective group. Each column in each panel comes from a separate estimation of equation (1). Section 5 discusses results obtained from our alternative specifications that include province-by-year fixed effects, province and year fixed effect interacted with having a child in primary school, and results obtained from the less demanding dose-response difference-in-difference specification.

Columns 1 and 2 of Table 3 present results for earnings and wages. Focusing on mothers, there is a statistically significant and economically meaningful negative effect of school disruptions on earnings: the estimate in column 1 indicates that a mother who has a child in primary school suffers a loss of $9.65 in total monthly earnings for each 10 days that her child's school is disrupted. This effect is -2.92 percent relative to the mean, shown directly below the estimate in the table. The associated hourly wage effect in column 2 is of comparable magnitude (-2.84 percent relative to the mean), also statistically significant at the 1 percent level.[16]

With respect to fathers (Panel B), the estimates are much smaller and not statistically significantly different from zero. The lack of significant effects among fathers provides two

[16] As we only have 24 clusters, we also estimate equation (1) using wild cluster bootstrap standard errors (Cameron and Miller 2015). Appendix Table A8 shows that our results are robust to this adjustment.

insights: (1) the average father is not adversely affected by TSCs, and (2) the average father does not appear to compensate for the mother's earnings loss by working more. The latter insight suggests that substitution in childcare and labor supply among parents is small, something we explore in more detail in Section 6.1.

One way to interpret the wage effect that we identify for mothers 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 total earnings, we calculate that the annual earnings loss induced by strikes amounts to approximately $117 million dollars.[17]

The finding that school disruptions are associated with lower earnings and wages among mothers suggest that TSCs likely have adverse effects on the labor supply of mothers as well. Table 3 shows estimates of equation (1) where the probability of being employed (column 3), being in the labor force (column 4), and being unemployed (column 5), are used as dependent variables. Among mothers, 10 days of TSCs reduces the employment probability by 0.016 percentage points and the likelihood of being in the labor force by 0.015. These effects are -2.84 percent and -2.39 percent relative to the respective means. We find no effect on unemployment; the reduction in employment is driven exclusively by mothers leaving the labor force (and not by, for example, employers discriminating against mothers likely to drop out or skip work due to TSCs). We find no labor force participation effects among fathers.

To obtain a better understanding of the labor market effects discussed above, Table 3 also provides results from estimation of equation (1) using a set of more detailed labor market characteristics: the probability of engaging in part-time work, the probability of engaging in full-time work, hours worked, and the probability of holding a second job. With respect to part- and full-time work, the results in Table 3 show a reduction in the probability of working part-time among mothers, and no effect on the probability of working full-time. With respect to hours worked and multiple jobs, the results suggest that there is no statistically significant effect on hours worked among mothers, but that there is a reduction in the probability of having a second job. Consistent with our previous findings for fathers, we do not find any statistically significant effects among fathers.[18]

To summarize, we find that TSCs in the last year have adverse effects on the current labor force participation of mothers. These effects are associated with substantial reductions in monthly

[17] This number is obtained by scaling the point estimate (9.65) with the average number of strikes in the past year (0.75), multiplied by the total number of mothers to primary school children in Argentina (1,300,000), multiplied by the number of months in a given year (12).

[18] Our results are robust to the Bonferroni correction for multiple hypothesis testing, as well as to the less conservative Sankoh, Huque, and Dubey (1997) adjusted Bonferroni correction, which takes into account the correlation among outcomes. These results are provided in Appendix Table A9.

labor earnings and hourly wages, demonstrating that disruptions of childcare services negatively impact the socioeconomic position of mothers. That we do not find any economically or statistically significant effects among fathers is consistent with the idea that disruptions to essential childcare services cause a widening of within-household gender gap.

4.2 Subgroup Analysis

Our baseline results show that TSCs in the previous year reduces mothers' labor force participation and labor earnings. This section probes the data further to better understand which groups of parents that are particularly affected by TSCs. The results from these auxiliary analyses help provide suggestive evidence on the mechanisms underlying our main estimates. Results for mothers are shown in Table 4, and results for fathers are shown in Appendix Table 10. It should be noted that there are differences in sample sizes across some of the groups examine in this subsection, such that our ability to detect effects is greater for some groups than for others. We first discuss results for mothers, and then for fathers.

In Panel A of Table 4, we explore if TSCs differentially affect single and married mothers. The rationale underlying this hypothesis is that a single mother (sole income earner) likely faces larger constraints to labor market exit than a married mother. The results displayed in Panel A of Table 4 support this prediction: while we see large and statistically significant adverse effects on the labor supply among married mothers, the effects for single mothers are very small and sometimes even positive. Further, only one estimate for single mothers is statistically significant at conventional levels.[19]

Panel B examines whether a TSCs impact mothers with a certain level of education more than others. The idea behind this stratification is that low-educated mothers (high school or less) likely have lower wages, and therefore face a smaller opportunity cost of dropping out of the labor force. With respect to high-educated mothers (at least some college), they are more likely to have their children in private schools that are isolated from public sector teacher strikes, and are more likely to afford purchasing temporary alternative childcare services in the event of a TSC, such that their labor market behavior likely is less affected. The results in Panel B support this prediction, showing that the effects we identify are driven predominantly by low-educated mothers. This result suggests that less-skilled mothers with weaker labor market attachments are more affected by school disruptions.

Another potential implication discussed in Section 2 is that the within-family decision on

[19] The positive earnings effect among single mothers could potentially be due to an effort to increase work to finance private education where strikes do not take place (see Section 6.2), since these individuals are more dependent on their wages and cannot necessarily leave the workforce. However, we are careful to infer anything from this result, as none of the other outcomes are statistically significant at conventional levels.

whether to provide home care – and which parent should provide it –depends on the relative earnings of the husband and wife, and on the relative quality of childcare that they can provide. Data limitations prevent us from looking at within-household effect heterogeneity by relative childcare quality (unobserved), but we can stratify the sample based on whether the predicted earnings of the wife are larger or smaller than those of the husband (obtained through estimation of Mincer earnings functions in which earnings are predicted based on education, potential experience, year and province). The results from this analysis are shown in Panel C of Table 4.

Looking across Panel C of Table 4, there are economically meaningful and statistically significant adverse labor market effects among wives with lower potential earnings than their husbands. Even though the point estimates oftentimes are not statistically significantly different for mothers with higher predicted earnings than their husbands, the effects as percentages of the means are much smaller, and none of them are statistically significantly different from zero. This result is in line with previous research (e.g. Apps and Rees 2002; Blundell, Chiappori and Meghir 2005; Cherchye, De Rock and Vermeulen 2012) that have found the labor supply decisions of parents to depend, at least in part, on the relative income of each parent.[20]

To further explore how the relative earnings of the parents affect the intra-household response to TSCs, and to ensure that the results in Panel C of Table 4 are not simply due to unbalanced sample sizes across the two groups, we stratify our sample by the quartile of the predicted relative earnings distribution and reestimate equation (1) for each of these quartiles. The results from this exercise are shown in Figure 3. In the first three quartiles, the mother earns less than the father. In the last quartile, the mother earns more than the father. The results show that the effect of TSCs on parental labor market outcomes depend on the relative wage of the parents: mothers are more affected the bigger the predicted earnings gap between the mother and the father is, and the father is only affected when he is predicted to earn less than the mother. Still, the group of households in which the mother is predicted to earn more than the father is small (less than 1/4 of all dual-parent households), and the estimated effect for fathers in this subgroup is statistically significantly smaller than the effect for mothers. Consequently, mothers are disproportionally affected by TSCs, and TSCs can therefore be seen as exacerbating the existing labor market and intra-household gender inequality.

In Panel D of Table 4, we examine if the parental effect of TSCs differ depending on

[20] The samples underlying the results in Panels B and C are substantially different across the groups. To ensure that the difference in sample size is not driving our results, we have performed a permutation exercise in which we estimate equation (1) 200 times for low-educated mothers (Panel B) and mother's with lower earnings than their husbands (Panel C) for random subsamples of these groups that are of equal size to those groups they are being compared to. The average point estimates and p-values obtained from this exercise are shown in Appendix Table A11, and are not statistically or economically significantly different from those in Table 4. This suggests that the differences in effects across the groups are not simply due to sample size differences.

whether the child is enrolled in early primary school grades or late primary school grades. The intuition behind this hypothesis is that parents may be more willing to leave the labor market and take care of a young child, since younger children may be less able to take care of themselves. However, parents to young children are less likely to be in the labor force (Appendix Figure A1), so the average labor market response among these mothers will likely be smaller. Further, the children we look at are between 6 and 11 years old, and it is likely that mothers to even the oldest primary school children consider them too young to be left alone at home.[21] Looking across Panel D of Table 4, there is strong evidence of adverse parental labor market affects associated with school disruptions irrespective of which grade the child is attending: all outcomes of interest are statistically significant and economically meaningful for mothers of children in both the lower and higher grades of primary school. None of these estimates are statistically significantly different from each other.

Another potential source of effect heterogeneity discussed in Section 2 is that parents who work in sectors and occupations with less generous non-pecuniary benefits may be more affected by TSCs due to their inability to use sick days and flexible work arrangements to take care of their children. To examine effect heterogeneity across this dimension, we use our constructed panels and estimate the effect of TSCs separately for parents who worked in the public sector and the private sector twelve months ago (at the onset of the TSC measure that we use), using the public sector as a proxy for the availability of more generous non-pecuniary benefits. The results from this analysis are displayed in Panel A (public sector workers) and Panel B (private sector workers) of Appendix Table A12. The results in Appendix Table A12 reveal that most of the labor market effects are concentrated among mothers in the private sector, supporting the idea that occupations with more work flexibility may serve as an important tool for reducing the impact of TSCs, potentially allowing parents to take temporary leaves of absence to provide short-term home care.

Appendix Table A10 displays the results from our subgroup analysis on fathers. The baseline results in Table 3 suggest that fathers do not change their labor market behavior in response to disruptions of their children's school services. Generally, this conclusion carries over to the subgroups that we look at in Appendix Table A10, with the exception of the results on labor market characteristics (probability of holding a second job and probability of working part-time), for which

[21] While there are not always explicit laws on how old a child must be before the parents are allowed to leave them alone at home, social service guidelines across the globe tend to suggest that children of primary school age should not be left alone for more than a few hours. In New Zealand, it is explicitly forbidden to leave a child under the age of 14 alone at home; the National Society for the prevention of Cruelty to Children in the UK recommend not to leave a child under the age of 12 alone at home; only a few states in the US have imposed a legal age below which a child cannot be left alone (e.g. Illinois require that the child is at least 14 years old), but most Departments of Health and Human Services across US states recommend that children up to 12 years old are not left alone for more than three hours; national guidelines in Argentina state that neglect and cruelty to children charges can be pressed on parents that leave children under the age of 13 alone for more than a few hours.

the result suggest that some subgroups benefit and others are hurt. However, none of these effects impact the earnings or employment of the fathers.

As mentioned above, the one exception concerns fathers married to females that have higher predicted earnings than they do (Panel C of Appendix Table A10 and Figure 3). These fathers experience both adverse labor market participation and earnings effects due to strikes: A father whose child is exposed to ten days of TSCs suffers a decline in hourly wage equivalent to 2.09 percent of the mean. This supports the idea that parental relative income matters for how households respond to TSCs.

4.3 Effect Heterogeneity

i. Quantile Treatment Effects

Equation (1) estimates mean impacts, and abstracts away from any potential distributional effects of TSCs. However, given some of the findings in Section 4.2 – that low-educated mothers are disproportionally hurt - it is likely that mothers at the lower end of the wage and employment distribution are affected differently from mothers at the higher end of the wage and employment distribution. Thus, the effects displayed in Table 3 may conceal important effect heterogeneity across the wage and work hours distributions. To study this question in detail, we estimate the impact of TSCs on the quantiles of the unconditional (marginal) distribution of wages and hours using RIF regressions. This allows us to estimate the effect of TSCs on the full marginal distribution of wages and hours. [22]

The results from this exercise are shown in Table 5. Looking across the table, the results for both wages (Panel A) and hours (Panel B) show that the adverse effects identified in Table 3 are driven by mothers at the margin of employment located at percentiles 50th-70th (percentiles 10th-40th are equal to zero) and that high-wage mothers are unaffected by TSCs. This result is consistent with the idea that high-wage mothers are more likely to have their children in private school (not subject to strikes) and more likely to afford purchasing temporary childcare services in the event of a TSC. With respect to fathers, we find no effects in any quantiles of their wage and hours distributions.

ii. Non-linear Treatment Effects

Equation (1) assumes that the effects of teacher strikes are linear in TSC days. This is not necessarily the case: parents may be able to provide home care or organize informal childcare for a few days per year without having to quit their jobs, such that short strikes are associated with very limited effects while long strikes are associated with much larger effects.

[22] RIF regressions allows us to estimate the effect of TSCs on the entire marginal distribution of wages and hours. See Firpo, Fortin, and Lemieux (2009) for more details on the use of RIF regressions.

While it is difficult to disentangle such effects with the data we have, we explore this question in Table 6**,** using a more flexible version of equation (1) that allows the labor market effects to differ for TSCs that last for less than one week, between one and two weeks, and more than two weeks. The results from this exercise suggest that individuals experiencing less than one week of TSCs are unaffected, that individuals exposed to between 1 and 2 weeks of TSCs experience statistically significant and economically meaningful adverse effects, and that individuals exposed to more than 2 weeks of TSCs experience even larger adverse effects.

The results from this exercise are consistent with the idea that very short childcare disruptions do not impact parental labor supply. However, the results also show that the effects are not exclusively driven by individuals in the right tail of the TSC distribution. These findings highlight that the baseline results obtained from estimation of equation (1) represent the average effects of teacher strike days, and that the effects of shorter strikes are smaller while the effects of longer strikes are larger. However, to facilitate interpretation of our results, we focus on average effects in the remained of the paper.

4.4 Suggestive Evidence on Effect Persistence

The above results provide clear evidence that TSCs have adverse labor market effects on mothers. However, these results do not provide insight into the temporary/permanent nature of the effects. Rather, they show the labor market effects of TSCs that took place in the previous year on current outcomes, which may be temporary, permanent, or both.

To obtain suggestive evidence on the persistence of the effects, we estimate an augmented version of equation (1) in which we add – as separate variables – the number of TSC days that the mother was exposed to in previous years. Specifically, we estimate the following modified version of equation (1):

$$\begin{aligned} Y_{ipt(q)} = \gamma_0 &+ \alpha_1 Str_{pt(q)-1} + \alpha_2 Ch_{ipt(q)-1} + \beta_1\left(Str_{pt(q)-1} \cdot Ch_{ipt(q)-1}\right) + \alpha_3 Str_{pt(q)-2} \\ &+ \alpha_4 Ch_{ipt(q)-2} + \beta_2\left(Str_{pt(q)-2} \cdot Ch_{ipt(q)-2}\right) + \alpha_5 Str_{pt(q)-3} + \alpha_6 Ch_{ipt(q)-3} \\ &+ \beta_3\left(Str_{pt(q)-3} \cdot Ch_{ipt(q)-3}\right) + \gamma X_{ipt(q)} + \rho_p + \tau_{t(q)} + \varepsilon_{ipt(q)} \end{aligned} \tag{2}$$

where $Str_{p\,t(q)-1}$ measures the number of TSCs in province p during the past 12 months (in 10s of days), $Str_{p\,t(q)-2}$ measures the number of TSCs during the past 13-24 months, and $Str_{p\,t(q)-3}$ measures the number of TSCs during the past 25-36 months. All other variables are defined as in equation (1). Using this equation, β_1 measures the intent-to-treat effect of TSCs that took place in the past twelve months (0-1 year ago), β_2 measures the intent-to-treat effect of TSCs that took place in the past 13-24 months (1-2 years ago), and β_3 measures the intent-to-treat effect of TSCs that took place in the past 25-36 months (2-3 years ago).

The results from this exercise are shown in Table 7. Looking across the table, the results reaffirm that strikes in the past year have a substantial impact on parental labor market behavior, even after controlling for strikes in previous years. This is consistent with the lack of a correlation of provincial strikes over time discussed in Section 3. The results also suggest that the impact of TSCs on parental labor market behavior fades the further back in time they occurred, and that the parental labor market outcomes have been restored within one year after the strikes have taken place. The one exception to this concerns wages, which shows suggestive evidence of more persistent effects. While this effect is very small, it is consistent with the literature on the potential scarring effects of periods of non-employment (e.g Arulampalam 2001; Eliason and Storrie 2006; Eriksson and Rooth 2014).

5. Robustness and Sensitivity Checks

In this section, we study the sensitivity of our results to changes in sample composition and model specification. Due to the lack of statistically significant and economically meaningful effects among fathers in our main specifications, we only discuss the results for mothers in this section, all of which are shown in Table 8.[23] Results for fathers are shown in Appendix Table A14.[24]

One of the main concerns is that there may be other province-specific secular trends, shocks or policies concurrent with the number of teacher strike days in the past twelve months that differentially impact the labor market behavior of mothers with and without children in primary school. In Panels A through D of Table 8, we examine this concern by estimating modified versions of equation (1). In Panel A we include province-specific linear time trends; in Panel B we estimate the more demanding dose-response triple difference specification that incorporates province-by-year fixed effects, year-by-dummy of having a child in primary school fixed effects, and province-by-dummy of having a child in primary school fixed effects; in Panel C we include local labor market controls; and in Panel D we control for the number of public administration strikes in the previous year and its interaction with having a child in primary school, effectively running a "horse

[23] In Appendix Table A13 we examine the sensitivity of our estimates to relaxing the main sample restrictions discussed in the data section: excluding parents with toddlers (children aged 0-2) and including individuals who experienced more than 30 strikes in any given year. We also show results from using only parents to secondary school children as controls (excluding parents with children aged 3 through 5). As shown in the table, our main results are robust to each of these adjustments, with the sole exception of labor market participation outcomes when including mothers with toddlers. That the effects become noisier when parents with toddlers are included is expected, as the labor force participation of these mothers is very low (Appendix Figure 1).

[24] With respect to the results for fathers in Appendix Table A14, some of the modified specifications lead to statistically significant results with respect to the probability of holding a second job and working part-time. However, none of those effects are robust across our specifications, and none of them appear to have an impact on the labor income and employment status of the fathers. We interpret this as suggesting that our main results for fathers in Table 3 are robust to these alternative specifications as well.

race" by comparing the impact of teacher strikes and non-teacher strikes on the labor market behavior of parents.[25]

All of the point estimates produced by the alternative specifications in Panels A through D of Table 8 are consistent with our baseline results. The results from these exercises thus show that factors such as secular trends, local labor market conditions, and non-teacher strikes, cannot explain any of our main results. This is expected given our triple difference design. Specifically, our effects are identified off of differences in outcomes between parents with and without children in primary school in provinces and years that experienced more strike days to that same difference in provinces and years that experienced fewer strike days. Thus, our results would only be biased if factors such as the ones above (e.g. local labor market conditions) are correlated with the number of strike days in the previous year and the current labor market outcomes of the parents, but only for those parents with children that are between 6 and 11 years old (and uncorrelated with the year-by-quarter fixed effects and demographic controls). The results from these alternative specifications support a causal interpretation of our main findings.

In Panel E, we eliminate all individuals working in the primary education sector from our analysis sample (7 percent of the female sample). The rationale underlying this auxiliary analysis is that teachers, as well as other non-teaching and non-permanent support staff, may find it easier to take care of their own children during strike-induced TSCs since they are not at work, such that the inclusion of these individuals in our main specification causes an attenuation of the estimated effects. The results in Panel E are consistent with this belief, showing slightly larger point estimates, and smaller standard errors, compared to the baseline results in Table 3.

Panel F of Table 8 reports the results from a placebo test in which we reassign treatment from *t-1* to *t+1*. Strikes that occurred in *t+1* cannot affect labor market outcomes in *t*, and if this exercise returns statistically significant effects, that is indicative of our results being driven by province-specific secular trends that affect parents with and without children in primary school differently over time. None of the point estimates are statistically significant. The results are also consistent with parents not behaving strategically in anticipation of future strikes. This is not surprising given the results in Appendix Tables A4-A6 discussed in Section 3, where we show that

[25] The specification that incorporates province and time fixed effects interacted with having a child in primary school is very demanding, increasing the number of parameters that needs to be estimated with 1400. This imposes substantial restrictions on the model, which is why we do not include these two-dimensional fixed effects in our baseline estimate. As expected, the results produced by this alternative model leads to an increase in the standard errors and reduces the size of the point estimates slightly (Panel B of Table 8). However, the results obtained from this specification are consistent with our baseline results, and we fail to reject the null hypothesis that these results are statistically significantly different from our main estimates. For the specification that incorporates local labor market controls, these controls consist of average wage, unemployment rate and per capita family income in the province during the past twelve months. We use the EPH surveys from previous quarters to estimate these controls, and we therefore lose observations from the first year of the sample (2004) for which we cannot compute them.

most strikes behave erratically within each providence over time, making it difficult for parents to form informative expectations of future strike behavior.[26]

The results produced so far have relied on the dose-response triple difference discussed in the empirical methods section. An alternative is to restrict the sample to parents with children in primary school and estimate a dose-response difference-in-difference. While this methods relies on a stricter identification assumption (that there are no province-specific secular trends, policies or shocks correlated with both teacher strikes and the outcomes), it is easier to interpret the results from this model, and it is instructive to see if the results obtained from our preferred specification are systematically different from those obtained from this alternative approach. To this end, we estimate the following model (variables are defined as before):

$$Y_{ipt(q)} = \gamma_0 + \alpha_1 Str_{pt(q)-1} + \gamma X_{ipt(q)-1} + \rho_p + \tau_{t(q)} + \varepsilon_{ipt(q)}. \qquad (3)$$

The results from this exercise are shown in Panel A of Table 9. The results obtained from this alternative specification are consistent with the results in Table 3. The one exception to this relates to the effect on part-time work, which seizes to be statistically significant at conventional levels. We interpret the results from this exercise as evidence against potential bias from province-specific secular trends, policies or shocks correlated with both teacher strikes and the outcomes.

In addition to estimating equation (3) for our treatment group, we also estimate equation (3) for our control group. Since individuals in our control group were not affected by the TSCs, this provides us with a placebo test which we can use to further ensure that there are no province-specific secular trends, policies or shocks correlated with both teacher strikes and the outcomes. The results from this exercise are shown in Panel B of Table 9. Looking across the panel, none of the effects are statistically significantly different from zero, providing additional support for a causal interpretation of our findings. Again, the one exception to this relates to the effect on part-time work, in which we find a significant effect. We therefore encourage caution when interpreting the results for part-time work.

Finally, we acknowledge that the choice of a 12-month treatment window, although consistent with the structure of our data (Figure 2), is arbitrary. In Figure 4, we document the sensitivity of our results to the choice of treatment window, allowing it to vary from the past 6 months to the past 36 months in 6-month intervals. Though our results are generally robust to the

[26] Note that only the variables for which we found statistically significant effects in our main table are included in the robustness table. In appendix Table A15 we estimate the same regressions for the rest of our outcomes as well, and those estimates remain not statistically significant. The exception to this relates to the effect of unemployment, which appears marginally statistically significant in Panel A (including province-specific linear time trends). Although this result supports the idea that TSCs have adverse effect on the labor outcomes of mothers, we interpret it with caution because it is not robust to other specifications and it is only significant at 10 percent level.

choice of the length of the window, Figure 4 demonstrates that the effects identified in Table 3 are strongest when we us a treatment window of between 6-12, after which they gradually diminish. Using a treatment window of 36 months, the point estimates are smaller and some of are no longer statistically significant at conventional levels. In Figure 5, we show that the absence of results for fathers is robust to the length of the treatment window.

6. Extensions

6.1 Household level analysis

In this section, we perform two auxiliary household-level analyses to better understand the total impact of TSCs. First, we study if the individual-level effects identified in Section 4 translate into overall effects on the parents. The idea behind this analysis is to understand if the negative employment and earnings effects from our individual-level analysis translate into negative earnings effect for the parents, or if there is a substitution effect between spouses such that the net effect on total parental earnings is zero. This question speaks directly to the substitution in childcare and labor supply between parents. Second, we ask whether the baseline results identified in Table 3 depend on the household composition of the parent. The idea underlying this analysis is that parents may only respond to TSCs by leaving the labor force if there are no other individuals available at the house that can take care of the child during the school disruptions.

With respect to the first question, Table 10 shows results from estimating equation (1) at the household level (restricting the sample to two-parent households). Looking across the table, the point estimates closely mirror the main estimates for mothers in Table 3. However, the percent effects with respect to earnings and wages are much smaller since parental wages and earnings exceed individual wages and earnings. These results suggest that there is very limited substitution in childcare and labor supply across parents. While we are unable to disentangle the mechanisms underlying the lack of a substitution effect across household members, this result demonstrates that the negative effects experienced by the mother is not offset by the father working more, such that the TSCs reduce total household income. [27]

Concerning effect heterogeneity with respect to household composition, Table 11 shows results from estimating equation (1) for mothers stratified by household composition. First, we stratify our treatment group based on whether there are older siblings living in the house or not (Panel A). The idea underlying this exercise is that older siblings may be able to share the childcare

[27] An alternative way to evaluate household-level responses is to study the labor markets effects of TSCs for other adult members of the household. Appendix Table A16 shows very limited effects among non-parent household members, providing further suggestive evidence on a lack of significant substitution effects across parents and household members.

responsibility with the parents in the event of TSCs. Results from this exercise suggest that mothers to primary school children who have older siblings do not respond differently from mothers to primary school children who do not have older siblings. This suggests that the existence of older siblings in the household does not reduce the impact of TSCs on the mothers.

Acknowledging that the lack of effect heterogeneity with respect to the existence of older siblings may be because older siblings are still in school and thus not able to help the parents with providing childcare (secondary schools do not generally participate in primary school strikes), we proceed with stratifying our sample based on whether there are other adults living in the house or not (Table 11 Panel B). Results from this exercise suggest that mothers who have other (non-parent) adults in the house do not respond differently from mothers who do not have other (non-parent) adults in the house. We speculate that this could be due the existence of two competing effects: In larger households there are more individuals that can share childcare, but these household also have a larger income such that it is easier for mothers to drop out (since her share of household earnings is much smaller in a large household with many adult members).

Finally, we stratify the sample based on whether the mother lives in a household with other (non-parent) adults who are not in the labor force (Table 11 Panel C). This is an interesting group to study, because not only is there no household-level earnings effect that could work in the opposite direction, but it is also clear that mothers do not need to exit the labor force in the event of a TSC if these members can provide childcare. Results from this stratification provide evidence that this household composition is important for anticipating the individual-level response to TSC: there is no labor market response among mothers living with other (non-parent) non-employed adults, while there are large adverse labor market effects among mothers that do not live with other (non-parent) non-employed adults. This result supports the idea that the effect of TSCs depends, at least in part, on household composition and the availability of alternative childcare options. It is worth mentioning that labor market participation of other household members does not appear to depend on TSCs (Appendix Table A16), which allows us to interpret the results in Panel C as suggestively causal given that we are not conditioning on an endogenous variable (labor market participation of non-parent adults).

6.2 Private School Enrollment

As discussed in Section 2, parents can respond to TSCs in four different ways: (1) take a temporary leave of absence to provide short-term home care, (2) work and leave the child at home or with other family members, (3) work and purchase private care, and (4) drop out of the labor force to provide home care. While the focus of this paper is on (4), the sector-level analysis in Section 4.2 has provided suggestive evidence of (1), and the household level analysis in Section 6.1 has provided suggestive evidence of (2). In this section, we exploit the fact that the EPH survey contains information on whether the parent's child attend public or private primary school to also obtain suggestive evidence on (3).

Table 12 presents estimates of the effect of strike-induced TSCs on the probability that a given child attends a public primary school. The estimates show changes in the probability of public school enrollment from 10 days of TSCs. Each column in each panel comes from a separate estimation of equation (1) using children as the unit of observation, and we add controls sequentially across columns. In column (i) we use the same controls as in our main specification (Table 3), in column (ii) we add province-specific linear time trends, and in column (iii) we include local labor market controls (the province-specific average unemployment rate, wage and per capita family income in the previous year). When interpreting these results, it should be noted that parents may make use of the private school option not only due to childcare concerns, but also due to concerns about the loss of human capital that results from their child missing a nonnegligible percent of instructional days per year in public schools. Our data does not allow us to say anything about the relative importance of these potential explanations.

Panel A presents results for all children, as well as for females and males separately. There is clear evidence of a negative effect of TSCs on public school enrollment across all three samples: 10 days of TSCs in the past year is associated with an approximate 1.8 percentage point reduction in the probability of a child attending public school. The availability of alternative childcare options may thus serve to mute the impact of TSCs on parental labor market behavior.

To contextualize the importance of this effect, our coefficient estimate in Table 12 suggests that 10 days of strike-induced TSCs are associated with an increase in the probability of attending private primary school by 0.019 percentage points. Using the fact that approximately 4.5 million children attend primary school in Argentina in any given year, and that the average province experienced 7.8 teacher strikes per year, we perform a back-of-the-envelope calculation which suggests that approximately 67 thousand children are moved from public to private school each year due to TSCs.[28]

Panel B show results stratified by household type: children in single-parent households and children in two-parent households. The results in Panel B reveal that the effect of TSCs on public school enrollment is economically and statistically significant across both single-and dual-parent households. The effect on single-parent households is interesting given the lack of effects among this subgroup of parents in Table 4, and is consistent with the idea that single-parents are less likely to drop out of the labor force due to reliance on labor earnings to secure a subsistence level of income, but may still respond by moving the child to private school.

[28] *4,500,000 * 0.019 * 0.78.*

To better understand which households move their children to private school in response to TSCs, Figure 6 presents results stratified by quartile of the family earnings distribution in *t-1* (at the onset of our strike measure, using the panel data). Three observations are worth mentioning. First, there is no effect among children from the top quartile of the household earnings distribution. This is expected given that the majority of high-earnings families already have enrolled their children in private education. Second, while the effect is significantly larger among the two middle quartiles of the family earnings distribution, the figure reveals significant effects among households in the bottom quartile of the family earnings distribution as well. While it is likely that these children are sent to lower-quality private schools compared to children in households that are in the middle of the family earnings distributions, data limitations prevent us from exploring this.[29] Third, Argentina has been experiencing a surge in socio-economic school segregation between public and private schools since the early 1990s (Gasparini et al. 2011). Our findings suggest that teacher strikes may have played an important role on the selective migration of children from middle-income households from public to private schools, exacerbating school segregation of children from low-income households. Further research is needed to properly account for the effect of teacher strikes on school segregation in Argentina.

7. Discussion and Conclusion

Teacher industrial action is one of the leading causes of temporary school closures in the world, and over the past few years countries across all continents of the world have documented unprecedented numbers of teacher strikes. These events disturb conventional childcare arrangements and may have detrimental effects on parental labor market outcomes. Yet, despite the prevalence of teacher strikes, and the debates surrounding them, we know very little about their impact on parents. As a consequence, we lack a complete understanding of how families are affected by the childcare crises that emerge from school closures, inhibiting the design of effective policy responses. To the best of our knowledge, this paper presents the first comprehensive analysis on the topic.

We show that teacher strikes negatively affect the labor force participation of mothers. These adverse labor supply effects translate into economically meaningful reductions in earnings and wages: a mother whose child is exposed to ten days of teacher strikes in the last year experiences a decline in earnings equivalent to 2.92 percent of the mean. These effects are predominantly driven by low-skilled mothers at the margin of employment, such that strikes disproportionally hurt an already vulnerable subgroup of mothers, who may find it particularly difficult to secure alternative

[31] There is substantial heterogeneity in the cost of private schools in Argentina, ranging from "free private schools" that are financed through grants from the province government and usually crowded, to more elite private schools that cost up to $550 per month (McEwan 2002; Almeida et al. 2017; Valente; 1997).

childcare options. A back-of-the-envelope calculation suggests that the aggregate impact of teacher strikes on parental labor market earnings is more than $117 million dollars each year. This is equivalent to raising the annual wage of all primary school teachers in the entire country with approximately 6 percent.

While we do not find any effects among fathers in general, fathers who are married to women with higher predicted relative earnings also experience negative labor market effects: A father who earns less than his wife and whose child is exposed to ten days of teacher strikes suffers a decline in his hourly wage equivalent to 2.09 percent of the mean. This result suggests that the labor supply response of parents depend, at least in part, on the relative income of each parent. However, this group of households is small, such that women are disproportionally affected by teacher strikes. Finally, we show that the availability of alternative childcare options mute some of the adverse effects of strikes on parental labor market outcomes.

That strike-induced school disruptions have significant adverse effects on the labor outcomes of mothers, while the effects on fathers are much smaller and only present in households where the father is expected to earn less than the mother, represents an important finding. It demonstrates that mothers are particularly vulnerable to unexpected and temporary negative childcare shocks, and that such shocks may serve to augment the observed labor market and within-household gender inequality. Given the prevalence of teacher strikes across the globe, this may represent an additional and undocumented obstacle to gender equality and wage parity.

In terms of policy implications, our results show that the conventional solution to teacher strikes – the provision of make-up days at the end of the semester – needs to be revised or supplemented as it deals only with the impact of strikes on students, and neglects the impact on parents. One solution could be to hire non-teacher substitutes that staff the schools during teacher strikes, so that schools do not close during teacher walkouts. While costly, this was a solution favored by the Los Angeles school district during their 2019 district-wide teacher strike.

As the frequency of teacher industrial action is increasing in several countries across the globe, this expanded understanding of its implications is imperative for designing policies that minimizes fallout arising from labor conflicts in the education sector. An important area for future research is to investigate the extent to which the results we find generalize to other regions where strike-induced temporary school closures have been particularly prevalent in recent years.

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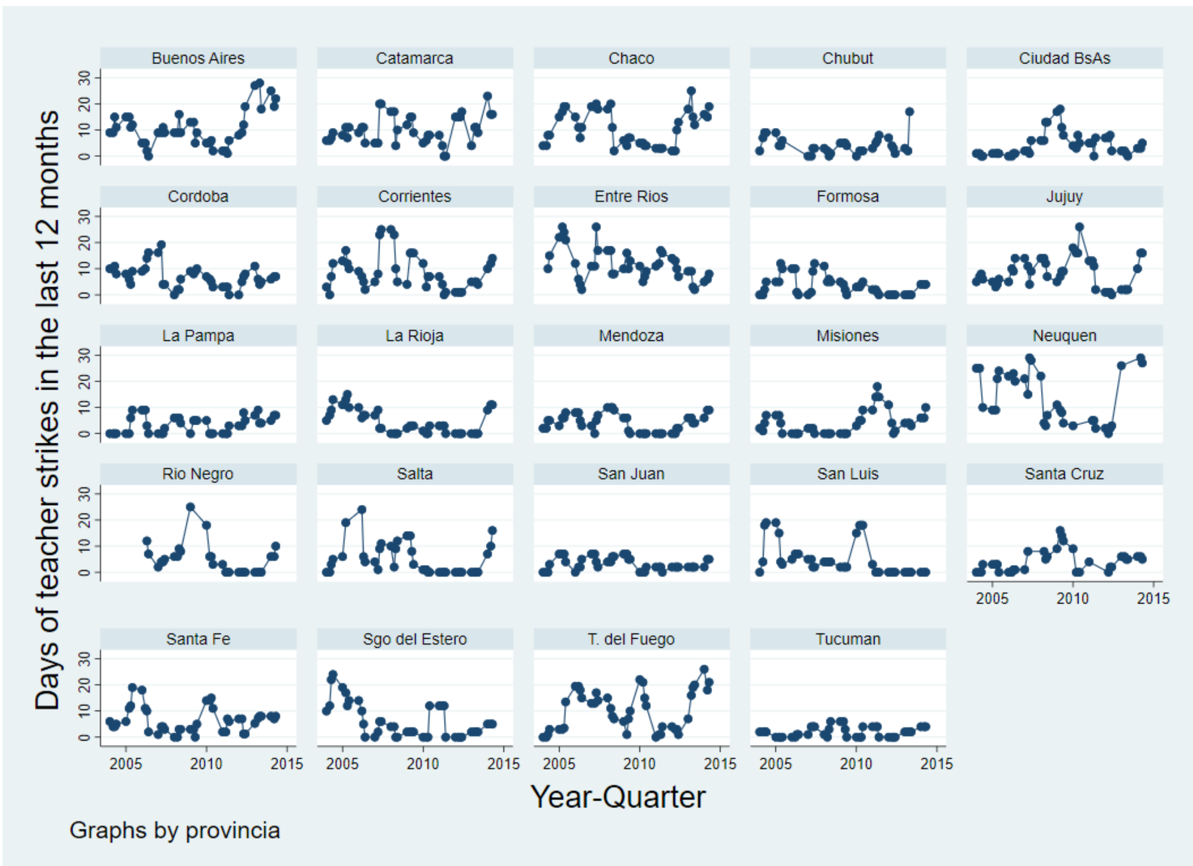
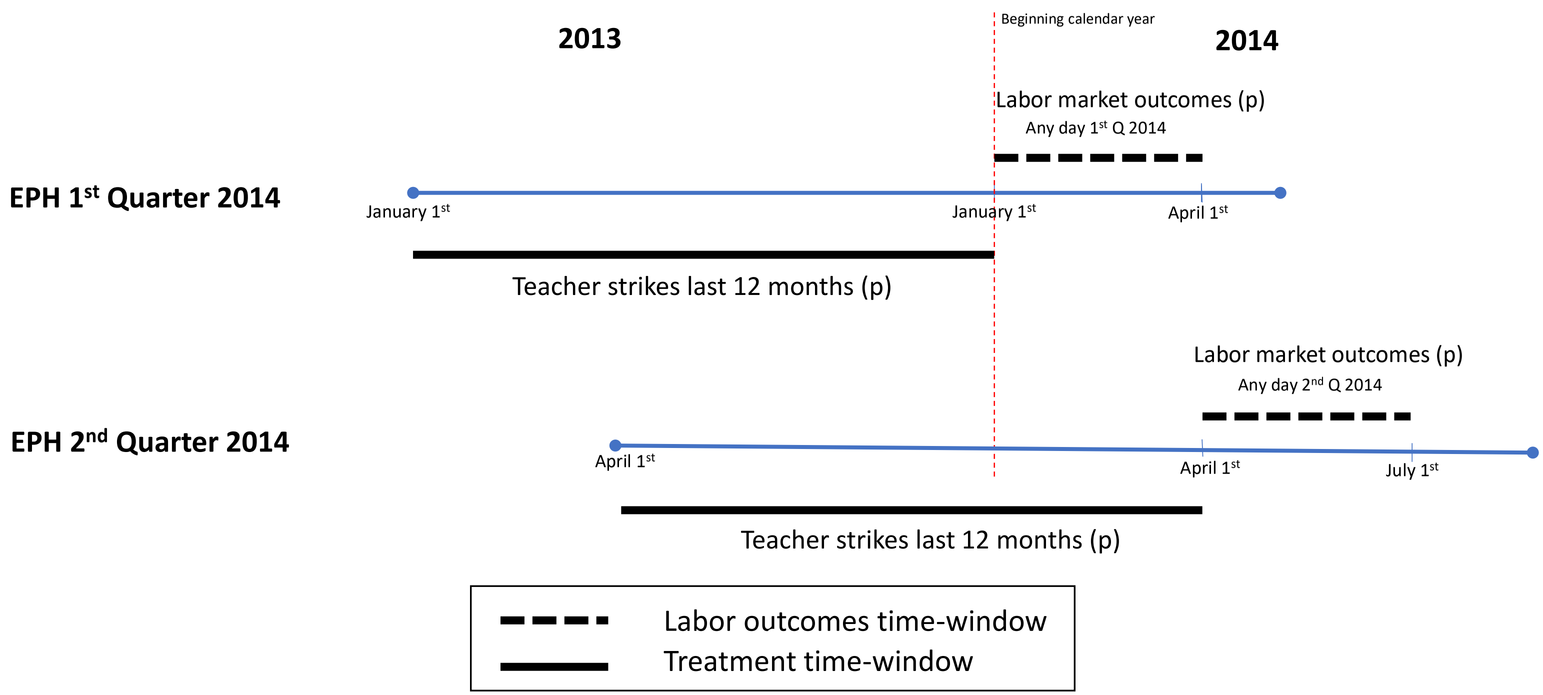
Figure 1: Variation in teacher strikes across provinces and time



Notes: The figure shows the total number of days of strike-induced school disruptions across province and time. Authors' calculations from historic reports on the Argentine economy published by Consejo Técnico de Inversiones (2003-2013) and collected by Jaume and Willén (2019).

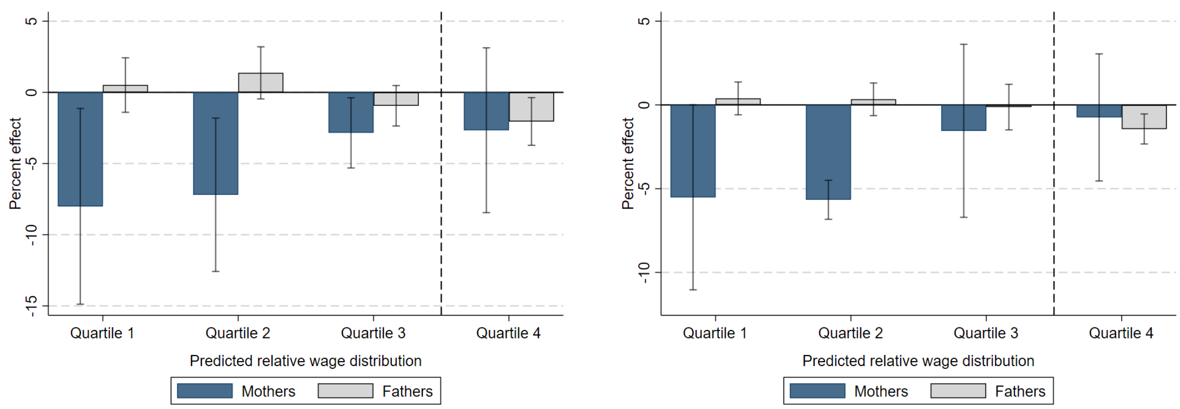
Figure 2: Example of the data structure for two quarters of the EPH household survey



Notes: The figure shows the structure of the data and when the outcomes are measured (solid line) relative to when the treatment is measured (dashed line) for two of the EPH quarters in the sample. With respect to the labor market outcomes, the EPH data only provide information about the quarter in which the household survey was conducted, not about the specific day or month.

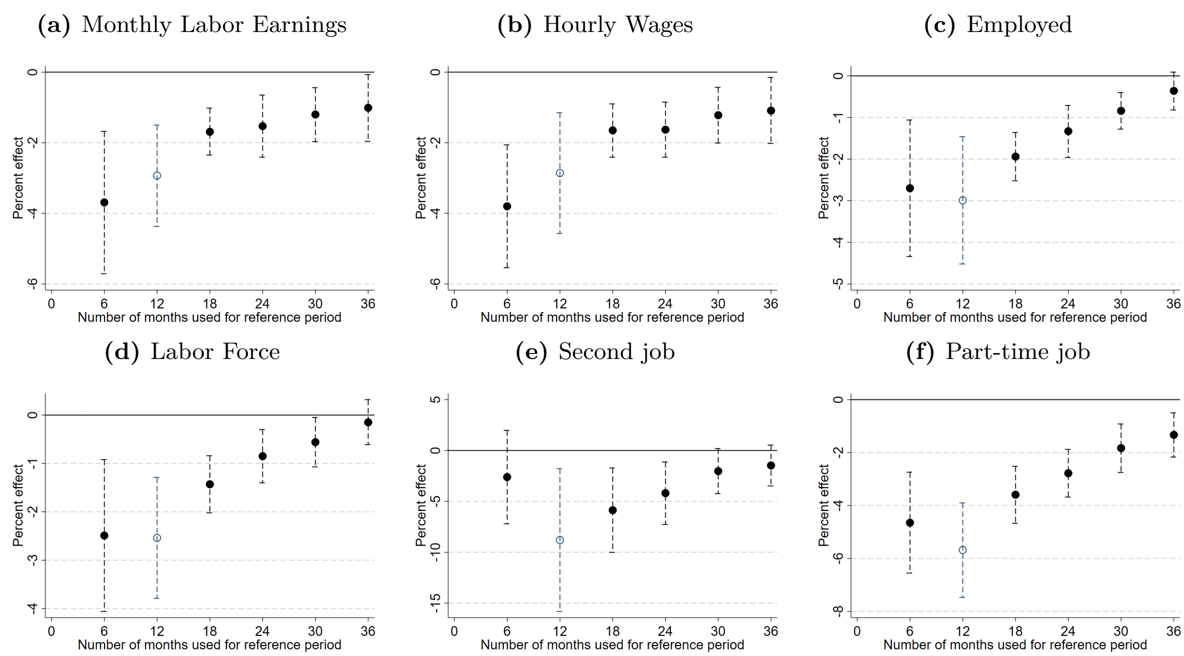
Figure 3: Within-household effects by quantile of the predicted relative income distribution

(a) Hourly Wage **(b)** Employed



Notes: Authors' estimation of equation (1) using the rotating 2004-2014 EPH panel on 18-50 year old parents, estimated separately for mothers and fathers by quartile of the predicted relative earnings of mothers with respect to fathers (potential earnings are estimated using a standard gender-specific Mincer equation controlling for potential experience, education, calendar year and province). In the bottom three quartiles, the mother's predicted earnings is less than the father's predicted earnings. In the top quartile, the mother's predicted earnings is higher than the father's predicted earnings. Regressions include province and year-quarter fixed effects as well as controls for potential experience, potential experience squared, education (indicator variables of incomplete primary, complete primary, incomplete secondary, complete secondary, incomplete tertiary, and complete tertiary), and number of children under the age of 18. Regressions further include an indicator variable for having a child of primary school age and a variable that measures the number of strike-induced school disruptions that took place during the past year (in tens of days). The figures shows point estimates (as a percentage of the mean) of the interaction between having a child of primary school age and the number of strike-induced school disruptions that took place during the past year. The coefficient measures the intent-to-treat effect of strike-induced school disruptions during the past year on current parental labor market outcomes. The lines extending from the bars show the 95% confidence intervals with standard errors clustered at the province level.

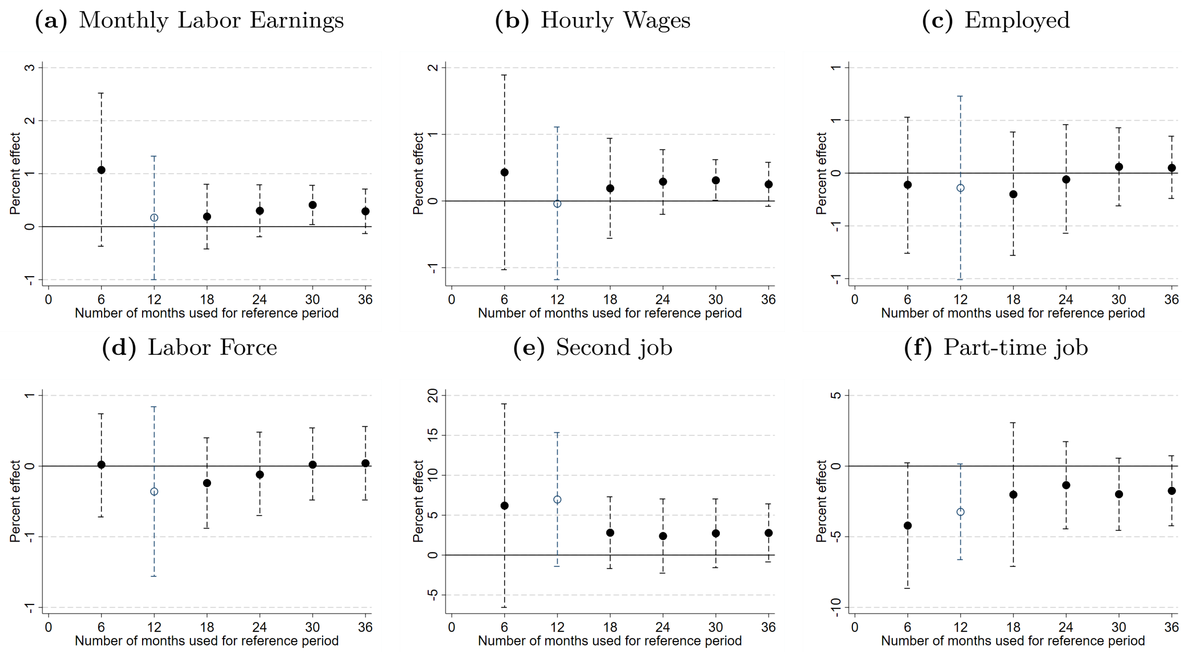
Figure 4: Sensitivity of results to alternative treatment windows, mothers



(a) Monthly Labor Earnings **(b)** Hourly Wages **(c)** Employed **(d)** Labor Force **(e)** Second job **(f)** Part-time job

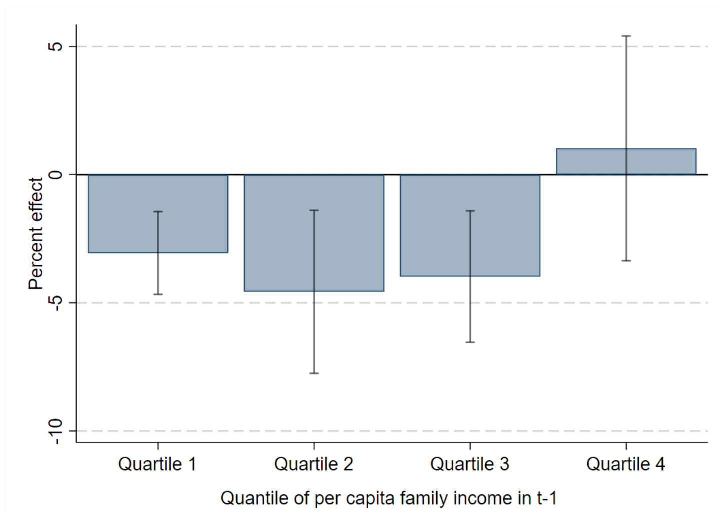
Notes: Authors' estimation of equation (1) using 2004-2014 EPH data on 18-50 year old mothers. Regressions include province and year-quarter fixed effects as well as controls for potential experience, potential experience squared, education (indicator variables of incomplete primary, complete primary, incomplete secondary, complete secondary, incomplete tertiary, and complete tertiary), and number of children under the age of 18. Regressions further include an indicator variable for having a child of primary school age and a variable that measured the number of strike-induced school disruptions that took place during the past 6 to 36 months (in 6-month intervals, measured in tens of days). The figures show point estimates (as a percentage of the mean) of the interaction between having a child of primary school age and the number of strike-induced school disruptions that took place during the past 6 to 36 months. The coefficient measures the intent-to-treat effect of past strike-induced school disruptions on current parental labor market outcomes. The lines extending from the point estimates show the 95% confidence intervals with standard errors clustered at the province level.

Figure 5: Sensitivity of results to alternative treatment windows, fathers



Notes: Authors' estimation of equation (1) using 2004-2014 EPH data on 18-50 year old fathers. Regressions include province and year-quarter fixed effects as well as controls for potential experience, potential experience squared, education (indicator variables of incomplete primary, complete primary, incomplete secondary, complete secondary, incomplete tertiary, and complete tertiary), and number of children under the age of 18. Regressions further include an indicator variable for having a child of primary school age and a variable that measured the number of strike-induced school disruptions that took place during the past 6 to 36 months (in 6-month intervals, measured in tens of days). The figures show point estimates (as a percentage of the mean) of the interaction between having a child of primary school age and the number of strike-induced school disruptions that took place during the past 6 to 36 months. The coefficient measures the intent-to-treat effect of past strike-induced school disruptions on current parental labor market outcomes. The lines extending from the point estimates show the 95% confidence intervals with standard errors clustered at the province level.

Figure 6: Effect of teacher strikes on public school enrollment by quintile of per capita family income in previous year



Notes: Authors' estimation of equation (1) using the rotating 2004-2014 EPH panel on school-age children (6 to 17 year old), estimated separately by quartile of the per capita family income in t-1. The dependent variable is a dummy variable equal to one if the child attends a public school. Regressions include province and year-quarter fixed effects as well as controls for age and number of siblings under the age of 18. Regressions further include an indicator variable of primary school age in t-1 and a variable that measures the number of strike-induced school disruptions that took place during the past year (measured in tens of days). The figure shows point estimates (as a percentage of the mean) of the interaction between attending primary school in t-1 and the number of strike-induced school disruptions that took place during the past year. The coefficient measures the intent-to-treat effect of strike-induced school disruptions during the past year on current likelihood of attending a public school. The lines extending from the bars show the 95% confidence intervals with standard errors clustered at the province level.

Table 1: Days of disrupted schooling due to teacher strikes, by year and province

	2003	2004	2005	2006	2007	2008	2009	2010	2011	2012	2013	2014	**Average**	**Total**
Buenos Aires	9	15	5	9	9	9	9	6	6	19	18	22	11.3	136
Catamarca	6	8	9	5	20	10	9	8	0	17	9	18	9.9	119
Chaco	4	15	15	19	18	2	7	6	3	13	12	23	11.4	137
Chubut	2	9	37	0	3	1	4	4	8	1	78	16	13.6	163
Ciudad BsAs	1	1	0	2	6	13	8	7	7	2	0	6	4.4	53
Cordoba	10	8	9	16.2	4	6	10	1	0	8	5	5	6.9	82
Corrientes	3	13	9	5	25	5	16	7	1	1	4	15	8.7	104
Entre Rios	64	22	12	11	17	8	13	11	16	7	2	11	16.2	194
Formosa	0	5	10	0	12	5	0	4	0	0	0	4	3.3	40
Jujuy	5	5	5	14	9	7	9	16	2	0	2	21	7.9	95
La Pampa	0	0	9	0	2	4	5	0	3	5	4	7	3.3	39
La Rioja	5	11	10	7	2	0	3	0	0	0	0	11	4.1	49
Mendoza	2	3	8	3	7	9	0	0	0	2	4	9	3.9	47
Misiones	2	7	0	2	0	0	0	5	14	1	3	10	3.7	44
Neuquen	25	9	22	21	28	7	4	48	2	3	68	23	21.7	260
Rio Negro	2	5	16	2	5	8	38	3	0	0	0	10	7.4	89
Salta	0	6	33	4	11	12	3	0	0	0	0	16	7.1	85
San Juan	0	7	0	7	2	6	5	1	0	2	2	5	3.1	37
San Luis	0	19	5	5	2	4	2	18	0	0	0	0	4.6	55
Santa Cruz	0	3	0	1	45	6	12	1	63	2	5	5	11.9	143
Santa Fe	6	6	18	1	3	3	5	11	6	1.25	8	8	6.4	76
Sgo del Estero	10	19	14	0	6	0	2	0	0	0	2	5	4.8	58
T. del Fuego	0	3	19.5	13	14	7	10	12	4	1	20	21	10.4	125
Tucuman	2	0	0	1	4	6	0	0	0	0	2	6	1.8	21
Average	6.6	8.3	11.1	6.2	10.6	5.8	7.3	7.0	5.6	3.6	10.3	11.5	**7.8**	**93.8**
Total	158	199	266	148	254	138	174	169	135	85	248	277	**-**	**2,251**

Notes: Authors' calculations based on historic reports on the Argentine economy published by Consejo Técnico de Inversiones (CTI) and collected by Jaume and Willén (2019). It should be noted that the analysis exploits variation at the year-quarter level, representing a finer lever of variation than that shown in the table (see Figure 1).

Table 2: Descriptive statistics

	Females			Males		
	Mothers		No kids	Fathers		No kids
	Kids in primary	Kids not in primary		Kids in primary	Kids not in primary	
Observations	107,938	60,462	219,694	79,509	41,057	243,820
i. Demographics						
Age	37.88	39.94	30.35	39.49	40.58	29.61
Potential experience	21.96	23.67	12.82	23.92	24.81	12.93
Years of education	10.92	11.27	12.53	10.56	10.77	11.68
No of kids < 19 in the hh	2.38	1.44	0.60	2.40	1.46	0.55
ii. Earnings						
Total labor earnings	313	355	339	746	760	480
Hourly wage	2.52	2.76	2.47	4.29	4.41	3.07
iii. Employment						
Hours worked	19.3	21.4	21.9	46.4	46.2	31.8
Employed	0.58	0.63	0.60	0.95	0.95	0.74
In labor force	0.64	0.68	0.69	0.98	0.98	0.83
Unemployment	0.05	0.05	0.09	0.03	0.03	0.09
Second job	0.08	0.09	0.06	0.07	0.07	0.04
Work Part-time	0.31	0.32	0.25	0.14	0.14	0.18
Work Full-time	0.25	0.28	0.33	0.78	0.78	0.54

Notes: Authors' tabulations using 2004-2014 EPH data on 18-50 years old respondents. Potential experience is defined as age less years of education less five. Total labor earnings and wages are expressed in 2011 purchasing power parity (PPP) dollars, and are set to zero for those who do not report any income or working activity. Second job is defined for all individuals and is equal to 1 when total hours worked is larger than hours worked in main activity and zero otherwise. Part-time job is defined for all individuals and is equal to one when total hours worked is lower than 35 and zero otherwise.

Table 3: Main Results

	Labor income		Labor market participation			Job characteristics			
	Earnings	Wages	Employed	Labor force	Unemployed	Hours	Second job	Part-time job	Full-time job
	(1)	(2)	(3)	(4)	(5)	(5)	(6)	(7)	(8)
Panel A: Mothers									
Strike days (10s of days)	-9.654***	-0.072***	-0.016***	-0.015***	0.001	-0.302	-0.006**	-0.017***	0.001
(N= 168,362)	(2.312)	(0.021)	(0.005)	(0.004)	(0.001)	(0.218)	(0.003)	(0.003)	(0.003)
% Effect	-2.92	-2.84	-2.84	-2.39	3.01	-1.55	-8.37	-5.58	0.30
Panel B: Fathers									
Strike days (10s of days)	0.746	-0.002	-0.002	-0.002	-0.000	0.085	0.005	-0.004*	0.002
(N=120,524)	(4.710)	(0.024)	(0.004)	(0.003)	(0.001)	(0.172)	(0.003)	(0.002)	(0.006)
% Effect	0.10	-0.06	-0.19	-0.21	-0.68	-0.68	0.18	6.95	-2.93

Notes: Authors' estimation of equation (1) using 2004-2014 EPH data on 18-50 year old parents. Regressions include province and year-quarter fixed effects as well as controls for potential experience, potential experience squared, education (indicator variables of incomplete primary, complete primary, incomplete secondary, complete secondary, incomplete tertiary, and complete tertiary), and number of children in the household. Regressions further include an indicator variable for having a child of primary school age and a variable that measures the number of strike-induced school disruptions that took place during the past year (in tens of days). The table shows point estimates of the interaction between having a child of primary school age and the number of strike-induced school disruptions that took place during the past year. The coefficient measures the intent-to-treat effect of strike-induced school disruptions during the past year on current parental labor market outcomes. 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 4: Subgroup analysis, mothers

	Labor income		Labor market participation		Labor market characteristics	
	Earnings	Wages	Employed	Labor Force	Second Job	Part-time job
	(1)	(2)	(3)	(4)	(6)	(7)
Panel A: By marital status						
i. Married	-11.172***	-0.103***	-0.015***	-0.014***	-0.005	-0.020***
(N=135,315)	(3.522)	(0.033)	(0.004)	(0.004)	(0.004)	(0.005)
% Effect	-3.69	-4.36	-2.98	-2.54	-8.77	-7.18
ii. Single	3.440	0.098**	-0.002	0.002	-0.006	0.010
(N=33,047)	(9.519)	(0.038)	(0.006)	(0.005)	(0.005)	(0.010)
% Effect	0.77	3.05	-0.28	0.22	-4.44	2.61
Panel B: By Educational level						
i. High school or less	-10.018***	-0.082***	-0.020**	-0.017**	-0.008**	-0.023***
(N=120,437)	(3.137)	(0.019)	(0.007)	(0.006)	(0.004)	(0.005)
% Effect	-4.68	-4.98	-4.17	-3.27	-14.58	-9.16
ii. Some university or more	-11.715	-0.067	-0.003	-0.005	-0.001	0.003
(N=47,925)	(8.631)	(0.093)	(0.005)	(0.005)	(0.003)	(0.006)
% Effect	-1.88	-1.39	-0.34	-0.67	-1.09	0.74
Panel C: Wife with lower vs. higher potential earnings than husband						
i. Lower earnings	-11.220***	-0.098***	-0.018**	-0.016**	-0.011***	-0.022***
(N=98,965)	(2.243)	(0.016)	(0.008)	(0.007)	(0.003)	(0.008)
% Effect	-4.58	-5.15	-3.89	-3.27	22.02	-9.20
ii. Higher earnings	-11.385	-0.113	-0.008	-0.008	0.010	-0.013
(N=36,350)	(12.301)	(0.112)	(0.013)	(0.012)	(0.007)	(0.011)
% Effect	-2.47	-3.12	-1.31	-1.24	10.21	-3.53
Panel D: Child in lower vs. higher grades						
i. In grades 1-3	-8.467**	-0.045**	-0.013**	-0.012**	-0.004	-0.013***
(N=131,798)	(3.310)	(0.017)	(0.006)	(0.005)	(0.003)	(0.004)
% Effect	-2.58	-1.80	-2.39	-1.96	-4.72	-4.47
ii. In grade 4-6	-12.242***	-0.132***	-0.018***	-0.017***	-0.011***	-0.020***
(N=97,034)	(4.139)	(0.041)	(0.004)	(0.004)	(0.003)	(0.003)
% Effect	-3.45	-4.90	-3.06	-2.67	-13.69	-6.52

Notes: Authors' estimation of equation (1) using 2004-2014 EPH data on 18-50 year old mothers. Regressions include province and year-quarter fixed effects as well as controls for potential experience, potential experience squared, education (indicator variables of incomplete primary, complete primary, incomplete secondary, complete secondary, incomplete tertiary, and complete tertiary), and number of children. Regressions further include an indicator variable for having a child of primary school age and a variable that measures the number of strike-induced school disruptions that took place during the past year (measured in tens of days). The table shows point estimates of the interaction between having a child of primary school age and the number of strike-induced school disruptions that took place during the past year. The coefficient measures the intent-to-treat effect of strike-induced school disruptions during the past year on current parental labor market outcomes. Panel A stratifies the sample based on marital status. Panel B stratifies the sample based on educational attainment (more or less than 12 years of schooling). Panel C looks separately at married mothers with lower and higher potential earnings than their partners (potential earnings are estimated using standard gender-specific Mincer equations controlling for potential experience, education, year, and region). Panel D looks separately at mothers with the youngest child in grades 1-3 during the previous year and mothers with youngest child in grades 4-6 during the previous year. 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: Distributional effects of school disruptions on parents' labor market outcomes

Panel A: Hourly wages

	10th	20th	30th	40th	50th	60th	70th	80th	90th
Mothers	-	-	-	-	-0.243***	-0.150**	-0.128**	-0.035	-0.032
(N= 168,362)	-	-	-	-	(0.086)	(0.070)	(0.059)	(0.087)	(0.121)
% Effect	-	-	-	-	-19.0	-6.0	-3.7	-1.0	-0.8
Fathers	-0.037	-0.007	0.002	-0.003	-0.011	-0.041	-0.027	0.086	0.107
(N=120,524)	(0.051)	(0.037)	(0.034)	(0.033)	(0.045)	(0.052)	(0.053)	(0.076)	(0.130)
% Effect	-3.4	-0.4	0.2	0.0	-0.3	-0.7	-0.3	1.7	1.3

Panel B: Hours worked

	10th	20th	30th	40th	50th	60th	70th	80th	90th
Mothers	-	-	-	-	-2.128**	-0.942*	-0.176	-0.048	0.290
(N= 168,362)	-	-	-	-	(0.928)	(0.562)	(0.575)	(0.251)	(0.315)
% Effect	-	-	-	-	-16.0	-3.6	-0.4	0.0	0.8
Fathers	-0.186	0.107	0.081	-0.333	0.055	0.055	0.112	0.185	0.425
(N=120,524)	(0.569)	(0.514)	(0.108)	(0.271)	(0.157)	(0.151)	(0.363)	(0.201)	(0.452)
% Effect	-1.0	0.2	0.2	-0.8	0.0	0.0	0.1	0.3	0.6

Notes: Authors' estimation of equation (1) using RIF regressions on 2004-2014 EPH data on 18-50 year old parents. Regressions include province and year-quarter fixed effects as well as controls for potential experience, potential experience squared, education (indicator variables of incomplete primary, complete primary, incomplete secondary, complete secondary, incomplete tertiary, and complete tertiary), and number of children. Regressions further include an indicator variable for having a child of primary school age and a variable that measures the number of strike-induced school disruptions that took place during the past year (measured in tens of days). The table shows point estimates of the interaction between having a child of primary school age and the number of strike-induced school disruptions that took place during the past year. The coefficient measures the intent-to-treat effect of strike-induced school disruptions during the past year on current parental labor market outcomes. 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 6: Non-linear effects of strike-induced school disruptions, mothers

	Labor income		Labor market participation		Labor market characteristics	
	Earnings	Wages	Employed	Labor Force	Second Job	Part-time job
	(1)	(2)	(3)	(4)	(6)	(7)
Less than a week	8.956	-0.099	0.002	0.004	-0.013*	-0.032***
	(7.745)	(0.086)	(0.009)	(0.008)	(0.007)	(0.008)
Between one and two weeks	-11.334*	0.061	-0.020**	-0.019***	0.001	0.008
	(6.010)	(0.081)	(0.008)	(0.006)	(0.004)	(0.006)
More than two weeks	-22.971***	-0.197***	-0.026**	-0.024***	-0.010	-0.032***
	(5.931)	(0.048)	(0.009)	(0.008)	(0.006)	(0.004)
Observations	168,362	168,362	168,362	168,362	168,362	168,362

Notes: Authors' estimation of equation (1) using 2004-2014 EPH data on 18-50 year old mothers. Regressions include province and year-quarter fixed effects as well as controls for potential experience, potential experience squared, education (indicator variables of incomplete primary, complete primary, incomplete secondary, complete secondary, incomplete tertiary, and complete tertiary), and number of children. Regressions further include an indicator variable for having a child of primary school age and a variable that measures the number of strike-induced school disruptions that took place during the past year (measured in tens of days). The coefficient for less than a week refers to the marginal effect of teacher strikes lasting for 1 to 7 days. The coefficient for between one and two weeks refers to the marginal effect of strikes lasting for 8 to 14 days. The coefficient for more than two weeks refers to the marginal effect of strikes lasting for more than 14 days. 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 7: Persistence of effects, mothers

	Labor income		Labor market participation		Job characteristics	
	Earnings	Wages	Employed	Labor force	Second job	Part-time job
	(1)	(2)	(3)	(4)	(6)	(7)
Strike days last 12 months	-10.695***	-0.061***	-0.011**	-0.011**	-0.004*	-0.009***
	(2.964)	(0.017)	(0.005)	(0.004)	(0.002)	(0.002)
% Effect	-3.20	-2.37	-1.89	-1.89	-4.87	-2.99
Strike days 13 to 24 months	-4.363	-0.032*	-0.006	-0.005	-0.003	-0.004
	(2.681)	(0.017)	(0.004)	(0.004)	(0.002)	(0.005)
% Effect	-1.31	-1.25	-1.09	-0.79	-3.94	-1.28
Strike days 25 to 36 months	-6.213	-0.040	-0.006	-0.003	-0.003	0.000
	(3.777)	(0.030)	(0.004)	(0.004)	(0.002)	(0.003)
% Effect	-1.86	-1.56	-1.08	-0.47	-4.09	0.01

Notes: Authors' estimation of equation (2) using 2004-2014 EPH data on 18-50 year old mothers. Regressions include province and year-quarter fixed effects as well as controls for potential experience, potential experience squared, education (indicator variables of incomplete primary, complete primary, incomplete secondary, complete secondary, incomplete tertiary, and complete tertiary), and number of children. Regressions further include an indicator variable for having a child of primary school age and a variable that measures the number of strike-induced school disruptions that took place during the past 12 months (measured in tens of days). The table shows point estimates of the interaction between having a child of primary school age during the referred period (last 12 months, last 13 to 24 months, and last 25 to 36 months) and variables containing the number of strike-induced school disruptions that took place during that period. The coefficients measure the intent-to-treat effect of strike-induced school disruptions in past periods on current parental labor market outcomes. 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 8: Robustness and sensitivity analysis, mothers

	Labor income		Labor market participation		Labor market characteristics	
	Earnings	Wages	Employed	Labor Force	Second Job	Part-time job
	(1)	(2)	(3)	(4)	(6)	(7)
Panel A: Including province-specific linear time trends						
Strike days (10s of days)	-9.332***	-0.070***	-0.016***	-0.014***	-0.007**	-0.017***
(N= 168,362)	(2.186)	(0.019)	(0.005)	(0.004)	(0.003)	(0.003)
% Effect	-2.82	-2.75	-2.81	-2.34	-8.62	-5.59
Panel B: Including all fixed effects for DDD						
Strike days (10s of days)	-5.785**	-0.042*	-0.014**	-0.013**	0.002	-0.017***
(N= 168,362)	(2.714)	(0.025)	(0.005)	(0.005)	(0.003)	(0.002)
% Effect	-1.75	-1.67	-2.45	-2.16	2.56	-5.54
Panel C: Controlling for local labor market conditions during the past year						
Strike days (10s of days)	-9.577***	-0.081***	-0.016***	-0.016***	-0.006*	-0.017***
(N= 146,910)	(1.689)	(0.022)	(0.005)	(0.004)	(0.003)	(0.003)
% Effect	-2.89	-3.17	-2.94	-2.60	-7.37	-5.79
Panel D: Controlling for public administration strikes during the past year						
Strike days (10s of days)	-10.971***	-0.075***	-0.015**	-0.013**	-0.006**	-0.019***
(N= 168,362)	(3.326)	(0.021)	(0.006)	(0.005)	(0.003)	(0.002)
% Effect	-3.32	-2.94	-2.64	-2.19	-8.03	-6.27
Panel E: Drop parents who work in the primary education sector						
Strike days (10s of days)	-12.137***	-0.124***	-0.017***	-0.016***	-0.006**	-0.020***
(N= 154,990)	(2.538)	(0.027)	(0.006)	(0.005)	(0.003)	(0.004)
% Effect	-4.16	-5.83	-3.33	-2.77	-10.06	-7.93
Panel F: Reassigning treatment from t-1 to t+1						
Strike days (10s of days)	-3.176	0.002	-0.023	0.002	0.000	-0.001
(N= 151,483)	(3.279)	(0.004)	(0.019)	(0.004)	(0.004)	(0.002)
% Effect	-0.96	0.31	-0.91	0.31	0.04	-1.92

Notes: Authors' estimation of equation (1) using 2004-2014 EPH data on 18-50 year old mothers. See footnote in Table 3. The coefficient measures the intent-to-treat effect of strike-induced school disruptions during the past year on current parental labor market outcomes. Panel A includes province-specific linear time trends. Panel B includes all fixed effects for a DDD approach (year-by-province, year-by-dummy of having a child of primary school age, and province-by-dummy of having a child of primary school age). Panel C controls for average unemployment, average wages, and average per capita family income at the province level during the past year. Panel D controls for the number public administration strike days that took place in the previous year and its interaction with having a child in primary school. Panel E drops parents that work in the primary education sector. Panel F shows results from a placebo test in which treatment has been reassigned from t-1 (strikes in the past year) to t+1 (strikes in the next 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 9: Dose-response difference-in-differences results

	Labor income		Labor market participation		Labor market characteristics	
	Earnings	Wages	Employed	Labor Force	Second Job	Part-time job
	(1)	(2)	(3)	(4)	(6)	(7)
Panel A: Parents with children in primary school (treatment group)						
i. Mothers	-12.764***	-0.053**	-0.012**	-0.012*	0.001	-0.003
(N=107,892)	(3.615)	(0.022)	(0.005)	(0.007)	(0.002)	(0.003)
% Effect	-4.08	-2.16	-2.15	-2.02	1.00	-0.88
ii. Fathers	-2.247	0.02	-0.003**	-0.001	0.001	0.004
(N=79,479)	(4.729)	(0.027)	(0.001)	(0.001)	(0.002)	(0.004)
% Effect	-0.30	0.49	-0.34	-0.12	2.19	2.93
Panel B: Parents without children in primary school (control group)						
i. Mothers	-6.57	-0.008	0.002	0.002	-0.001	0.014***
(N=60,470)	(4.079)	(0.029)	(0.002)	(0.003)	(0.002)	(0.004)
% Effect	-1.81	-0.29	0.37	0.36	-1.43	4.60
ii. Fathers	-3.855	0.002	-0.005	-0.001	-0.004	0.006
(N=33,047)	(5.400)	(0.028)	(0.003)	(0.002)	(0.003)	(0.004)
% Effect	-0.51	0.05	-0.49	-0.08	-6.71	3.92

Notes: Authors' estimation of equation (2) using 2004-2014 EPH data on 18-50 year old parents with children in primary school (panel A) and without children in primary school (panel B). Regressions include province and year-quarter fixed effects as well as controls for potential experience, potential experience squared, education (indicator variables of incomplete primary, complete primary, incomplete secondary, complete secondary, incomplete tertiary, and complete tertiary), and number of children. The table shows point estimates of the number of strike-induced school disruptions that took place during the past year. The coefficient measures the intent-to-treat effect of strike-induced school disruptions during the past year on current parental labor market outcomes. 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 10: Effects of strike-induced school disruptions, household-level analysis

	Parents' labor income		Parent's labor market participation			
	Total earnings (3)	Total wage (4)	All parents employed (5)	All parents in LF (6)	One-parent unemployed (7)	Total hours (8)
Strike days (10s of days)	-10.730**	-0.093***	-0.018***	-0.015**	-0.000	-0.012
(N=142,530)	(4.899)	(0.029)	(0.005)	(0.006)	(0.002)	(0.309)
% Effect	-1.04	-1.38	-3.23	-2.92	-0.17	-0.02

Notes: Authors' estimation of equation (1) using 2004-2014 EPH data on households with 18-50 year old parents. Sample restricted to two-parent households. Regressions include province and year-quarter fixed effects as well as household controls (averages for adult individuals in the sample) for potential experience, potential experience squared, education (indicator variables of incomplete primary, complete primary, incomplete secondary, complete secondary, incomplete tertiary, and complete tertiary), and number of children. Regressions further include an indicator variable for having a child of primary school age and a variable that measures the number of strike-induced school disruptions that took place during the past year (measured in tens of days). The table shows point estimates of the interaction between having a child of primary school age and the number of strike-induced school disruptions that took place during the past year. The coefficient measures the intent-to-treat effect of strike-induced school disruptions during the past year on current household labor market outcomes. Panel A shows results for the entire sample. Panel B and C stratifies the sample based on marital status by looking separately at two-parent households and single-parent households. 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 11: Effects by household composition, mothers

	Labor income		Labor market participation		Labor market characteristics	
	Earnings	Wages	Employed	Labor Force	Second Job	Part-time job
	(1)	(2)	(3)	(4)	(6)	(7)
Panel A. If there are older siblings in the house						
i. With siblings aged 12-30	-5.895**	-0.047**	-0.018***	-0.018***	-0.018***	-0.018***
(N=119,639)	(2.498)	(0.017)	(0.006)	(0.006)	(0.006)	(0.006)
% Effect	-1.81	-1.87	-3.17	-2.96	-11.18	-6.78
ii. Without siblings aged 12-30	-13.397**	-0.097**	-0.014***	-0.011**	-0.004	-0.013***
(N=109,193)	(4.777)	(0.044)	(0.005)	(0.004)	(0.004)	(0.002)
% Effect	-3.77	-3.61	-2.39	-1.71	-5.5	-4.29
Panel B. If there are other adults (18-70) besides parents						
i. With other adults	-7.190**	-0.081***	-0.023**	-0.024***	-0.014***	-0.022***
(N=58,028)	(3.222)	(0.024)	(0.008)	(0.008)	(0.004)	(0.005)
% Effect	-2.37	-3.5	-4.16	-4.09	-17.43	-7.35
ii. Without other adults	-8.313***	-0.053**	-0.012*	-0.010*	-0.002	-0.014***
(N=110,334)	(2.829)	(0.019)	(0.006)	(0.005)	(0.002)	(0.004)
% Effect	-2.41	-2.01	-2.11	-1.63	-2.87	-4.63
Observations						
Panel C. If there are other non-employed adults (18-70) besides parents						
i. With other non-employed	8.366	0.060	-0.001	-0.001	-0.009**	-0.006
(N=42,535)	(5.696)	(0.046)	(0.007)	(0.005)	(0.004)	(0.004)
% Effect	2.7	2.52	-.12	-.22	-11.37	-1.89
ii. Without other non-employed	-14.719***	-0.113***	-0.021**	-0.020**	-0.005*	-0.021***
(N=125,827)	(4.225)	(0.020)	(0.009)	(0.008)	(0.003)	(0.005)
% Effect	-4.36	-4.35	-3.71	-3.23	-6.92	-6.83

Notes: Authors' estimation of equation (1) using 2004-2014 EPH data on 18-50 year old mothers. Regressions include province and year-quarter fixed effects as well as controls for potential experience, potential experience squared, education (indicator variables of incomplete primary, complete primary, incomplete secondary, complete secondary, incomplete tertiary, and complete tertiary), and number of children. Regressions further include an indicator variable for having a child of primary school age and a variable that measures the number of strike-induced school disruptions that took place during the past year (measured in tens of days). The table shows point estimates of the interaction between having a child of primary school age and the number of strike-induced school disruptions that took place during the past year. The coefficient measures the intent-to-treat effect of strike-induced school disruptions during the past year on current parental labor market outcomes. Panel A stratifies the treatment group based on whether there are older siblings living in the house or not. Panel B stratifies the sample based on whether there are other adults living in the house or not. Panel C stratifies the sample based on whether the parent lives in a household with other adults who are not employed. 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.

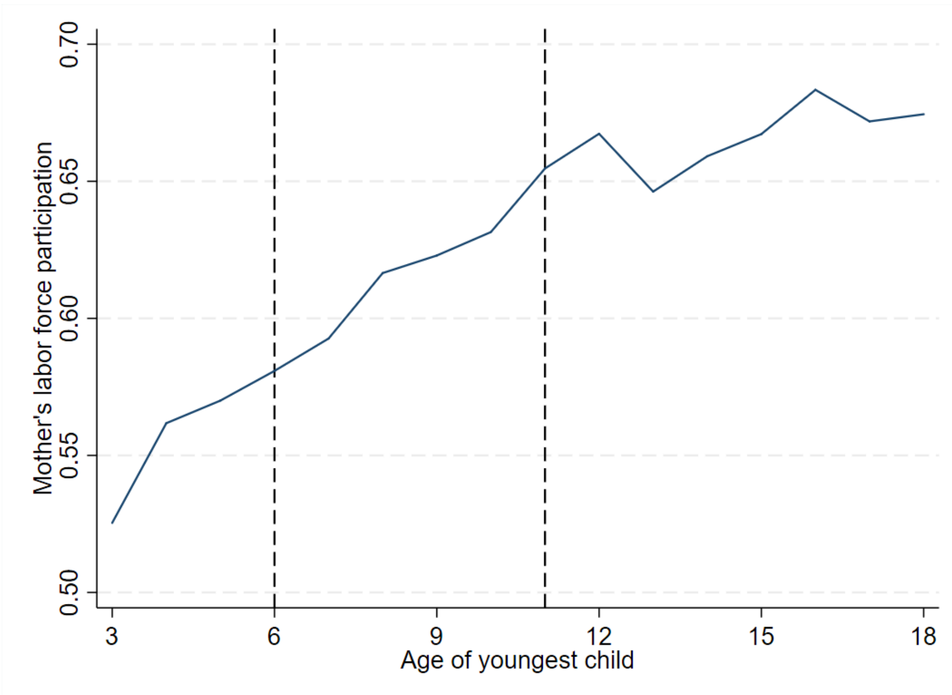
Table 12: Effect on public school enrollment

	(1)	(2)	(3)
Panel A: all observations			
i. all	-0.019***	-0.019***	-0.020***
(N=471,369)	(0.006)	(0.006)	(0.007)
% Effect	-2.47	-2.47	-2.52
ii. Males	-0.022***	-0.022***	-0.022***
(N=239,419)	(0.006)	(0.006)	(0.007)
% Effect	-2.75	-2.76	-2.76
iii. Females	-0.016**	-0.016**	-0.017**
(N=231,950)	(0.006)	(0.006)	(0.006)
% Effect	-2.10	-2.10	-2.20
Panel B: By number of parents			
i. Two-parent household	-0.020***	-0.020***	-0.019***
(N=327,844)	(0.006)	(0.006)	(0.006)
% Effect	-2.58	-2.59	-2.53
ii. Single-parent household	-0.021***	-0.021***	-0.021***
(N=92,047)	(0.005)	(0.005)	(0.006)
% Effect	-2.49	-2.46	-2.56
Province-specific linear time trends		X	X
Local labor market controls in t-1			X

Notes: Authors' estimation of equation (1) using 2004-2014 EPH data on school-age children (6-17 year old). The dependent variable is a dummy variable equal to one if the child attends a public school. Regressions include province and year-quarter fixed effects as well as controls for age and number of siblings under the age of 18. Regressions further include an indicator variable of primary school age in t-1 and a variable that measures the number of strike-induced school disruptions that took place during the past year (measured in tens of days). The table shows point estimates of the interaction between the indicator variable of primary school age in t-1 and the number of strike-induced school disruptions that took place during the past year. The coefficient measures the intent-to-treat effect of strike-induced school disruptions during the past year on the probability that the child attends a public school. 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.

Online Appendix

Figure A1: Labor force participation of mothers by age of youngest child



Notes: The figure shows average labor force participation of mothers based on the age of her youngest child, using 2004-2014 EPH panel data on 18-50 years old respondents. Children of ages inside the interval between the vertical dashed lines are in primary school.

Table A1: Effect of local labor market conditions on teacher strikes

	Teacher Strikes	
	(1)	(2)
Unemployment rate	0.9794***	0.5534
	(0.3300)	(0.5608)
Average wage	-0.6503*	-0.4839
	(0.3334)	(0.5305)
Average per capita income	0.0037**	0.0033
	(0.0017)	(0.0032)
Province FE		X
Year FE		X
R-squared	0.060	0.297

Notes: The table is based on Table A6 from Jaume and Willén (2019). It shows OLS regressions of teacher strikes on local labor market conditions, using 2003-2015 EPH data and strike data to construct yearly province aggregates. 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 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 A2: Effect of teacher wages on teacher strikes

	Teacher Strikes	
	(1)	(2)
Teacher wage year t	0.0102	-0.0027
	(0.0126)	(0.0139)
Teacher wage year t-1	0.0103	0.0094
	(0.0133)	(0.0163)
Teacher wage year t+1	-0.0177	0.0205
	(0.0114)	(0.0217)
Province FE		X
Year FE		X
R-squared	0.018	0.295

Notes: The table is based on Table A7 from Jaume and Willén (2019). It shows OLS regressions of teaches' strikes in year t in teacher wages on years t+1, t, and t-1, using 1996-2009 data on teacher wages from the Ministry of Education in Argentina and strike data to construct yearly aggregates at the province level. 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 A3: Descriptive characteristics, matched and unmatched parents in panel data

	Mothers				Fathers			
	Kids in primary		Kids not in primary		Kids in primary		Kids not in primary	
	Matched	Not Matched	Matched	Not Matched	Matched	Not Matched	Matched	Not Matched
Observations	36,070	71,879	26,351	53,166	19,588	40,914	13,228	27,832
i. Demographics								
Age	37.82	37.50	39.43	39.10	40.28	39.60	40.98	40.14
Potential experience	21.89	21.56	23.82	23.51	23.97	23.30	25.18	24.36
Years of education	10.93	10.94	10.61	10.59	11.31	11.29	10.80	10.78
No of kids $<$ 19 in the hh	2.48	2.44	2.49	2.46	1.48	1.45	1.50	1.47
ii. Earnings								
Total labor earnings	315	313	743	730	367	365	763	743
Hourly wage	2.52	2.51	4.34	4.21	2.85	2.80	4.49	4.32
iii. Labor Force Participation								
Hours worked	19.1	19.8	46.5	46.5	21.5	22.3	45.8	46.0
Employed	0.58	0.59	0.96	0.95	0.63	0.64	0.95	0.95
In labor force	0.62	0.64	0.98	0.98	0.67	0.68	0.98	0.97
Unemployment	0.04	0.04	0.02	0.03	0.03	0.04	0.02	0.03
Second job	0.08	0.08	0.07	0.07	0.08	0.09	0.07	0.07
Work Part-time	0.32	0.32	0.15	0.15	0.33	0.32	0.15	0.15
Work Full-time	0.24	0.25	0.78	0.77	0.28	0.29	0.77	0.77

Notes: Authors' calculations using 2004-2014 EPH panel data on 18-50 years old respondents for the last year observed in the 1-year panel data. Potential experience is defined as age less years of education less five. Total labor earnings and wages are set to zero for those who do not report any income or working activity and are expressed in 2011 purchasing power parity (PPP) dollars. Second job is defined for all parents and is equal to 1 when total hours worked is larger than hours worked in main activity and zero otherwise. Part-time job is defined for all parents and is equal to one when total hours worked is lower than 35 and zero otherwise.

Table A4: Correlation between days of strikes in year t and previous years

	Days of strike in year t
Days of strike in year t	1
Days of strike in year t-1	0.1315
Days of strike in year t-2	0.0802
Days of strike in year t-3	0.1251

Notes: Authors' calculations using 2003-2014 data on days of teacher strikes for yearly aggregates at the province level.

Table A5: Relationship between days of strikes in year t and previous years

	(1)	(2)	(3)
Days of teacher strike t-1	0.000	0.003	0.002
	(0.050)	(0.051)	(0.051)
Days of teacher strike t-2		-0.034	-0.031
		(0.054)	(0.055)
Days of teacher strike t-3			-0.018
			(0.053)
R2	0.168	0.166	0.163

Notes: Authors' calculations using 2003-2014 data on days of teacher strikes for yearly aggregates at the province level. Estimations include province and year fixed effects. 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 A6: AR(1) province-specific estimations of strike-induced school disruptions

Buenos Aires		*Corrientes*		*Mendoza*		*San Luis*	
days of strike t-1	0.524	days of strike t-1	-0.161	days of strike t-1	0.244	days of strike t-1	-0.166
sd	(0.319)	sd	(0.320)	sd	(0.326)	sd	(0.318)
R2	0.134	R2	-0.073	R2	-0.041	R2	-0.071
Catamarca		*Entre Rios*		*Misiones*		*Santa Cruz*	
days of strike t-1	-0.234	days of strike t-1	0.589***	days of strike t-1	-0.100	days of strike t-1	-0.260
sd	(0.325)	sd	(0.159)	sd	(0.290)	sd	(0.307)
R2	-0.046	R2	0.538	R2	-0.087	R2	-0.026
Chaco		*Formosa*		*Neuquen*		*Santa Fe*	
days of strike t-1	0.413	days of strike t-1	-0.170	days of strike t-1	-0.304	days of strike t-1	-0.235
sd	(0.309)	sd	(0.305)	sd	(0.292)	sd	(0.302)
R2	0.067	R2	-0.067	R2	0.008	R2	-0.037
Chubut		*Jujuy*		*Rio Negro*		*Sgo del Estero*	
days of strike t-1	-0.101	days of strike t-1	-0.098	days of strike t-1	-0.079	days of strike t-1	0.348
sd	(0.312)	sd	(0.177)	sd	(0.139)	sd	(0.197)
R2	-0.089	R2	-0.067	R2	-0.065	R2	0.162
Ciudad BsAs		*La Pampa*		*Salta*		*T. del Fuego*	
days of strike t-1	0.542*	days of strike t-1	-0.241	days of strike t-1	0.041	days of strike t-1	0.305
sd	(0.261)	sd	(0.320)	sd	(0.324)	sd	(0.316)
R2	0.232	R2	-0.041	R2	-0.098	R2	-0.006
Cordoba		*La Rioja*		*San Juan*		*Tucuman*	
days of strike t-1	0.246	days of strike t-1	0.458	days of strike t-1	-0.049	days of strike t-1	0.016
sd	(0.312)	sd	(0.267)	sd	(0.031)	sd	(0.113)
R2	-0.036	R2	0.151	R2	0.120	R2	-0.098

Notes: Authors' calculations using 2003-2014 data on days of teacher strikes for yearly aggregates at the province level. An AR(1) model was estimated for each province separately. The R squared reported is adjusted by degrees of freedom. *** indicates significance at the 1% level, ** indicates significance at the 5% level and * indicates significance at the 10% level.

Table A7: Dropping provinces where strikes are correlated across years (Ciudad BsAs and Entre Rios)

	Labor income		Labor market participation		Labor market characteristics	
	Earnings	Wages	Employed	Labor Force	Second Job	Part-time job
	(1)	(2)	(3)	(4)	(6)	(7)
Panel A: Mothers						
Disrupted Schooling	-7.452***	-0.051***	-0.016***	-0.014**	-0.007**	-0.018***
(N= 153,233)	(2.435)	(0.011)	(0.005)	(0.005)	(0.003)	(0.004)
% Effect	-2.29	-2.03	-2.80	-2.26	-9.62	-5.93
Panel B: Fathers						
Disrupted Schooling	-2.160	-0.018	-0.004*	-0.004*	0.003	-0.003
(N= 109,531)	(5.039)	(0.030)	(0.002)	(0.002)	(0.003)	(0.002)
% Effect	-.29	-.45	-.44	-.39	4.07	-2.41

Notes: Authors' estimation of equation (1) using 2004-2014 EPH data on 18-50 year old parents. Sample is restricted to provinces where strikes in t-1 are not significantly correlated with strikes in t-2 (dropping Ciudad de Buenos Aires and Entre Rios according to Appendix Table A6). Regressions include province and year-quarter fixed effects as well as controls for potential experience, potential experience squared, education (indicator variables of incomplete primary, complete primary, incomplete secondary, complete secondary, incomplete tertiary, and complete tertiary), and number of children in the household. Regressions further include an indicator variable for having a child of primary school age and a variable that measures the number of strike-induced school disruptions that took place during the past year (measured in tens of days). The table shows point estimates of the interaction between having a child of primary school age and the number of strike-induced school disruptions that took place during the past year. The coefficient measures the intent-to-treat effect of strike-induced school disruptions during the past year on current parental labor market outcomes. Standard errors 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 A8: P-values from wild cluster bootstrap

	Labor income		Labor market participation			Labor market characteristics			
	Earnings	Wages	Employed	Labor force	Unemployed	Hours	Second job	Part-time job	Full-time job
	(1)	(2)	(3)	(4)	(5)	(5)	(6)	(7)	(8)
Panel A: Mothers									
Strike days (10s of days) (N= 168,362)	-9.654***	-0.072***	-0.016***	-0.015***	0.001	-0.302	-0.006**	-0.017***	0.001
P-Value from Wild Cluster Bootstrap	0.002	0.002	0.022	0.044	0.356	0.440	0.176	0.002	0.789
Panel B: Fathers									
Strike days (10s of days) (N=120,524)	0.746	-0.002	-0.002	-0.002	-0.000	0.085	0.005	-0.004*	0.002
P-Value from Wild Cluster Bootstrap	0.889	0.903	0.641	0.563	0.833	0.637	0.178	0.114	0.651

Notes: Authors' estimation of equation (1) using 2004-2014 EPH data on 18-50 year old parents. Regressions include province and year-quarter fixed effects as well as controls for potential experience, potential experience squared, education (indicator variables of incomplete primary, complete primary, incomplete secondary, complete secondary, incomplete tertiary, and complete tertiary), and number of children in the household. Regressions further include an indicator variable for having a child of primary school age and a variable that measures the number of strike-induced school disruptions that took place during the past year (in tens of days). The table shows point estimates of the interaction between having a child of primary school age and the number of strike-induced school disruptions that took place during the past year. The coefficient measures the intent-to-treat effect of strike-induced school disruptions during the past year on current parental labor market outcomes. The p-values show the probability of observing the given coefficient value under the null hypothesis of no effect, and it is based on 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 province level (Table 3). *** indicates significance at the 1% level, ** indicates significance at the 5% level and * indicates significance at the 10% level.

Table A9: Multiple hypothesis correction, mothers

	Labor income		Labor market participation			Labor market characteristics			
	Earnings	Wages	Employed	Labor force	Unemployed	Hours	Second job	Part-time job	Full-time job
	(1)	(2)	(3)	(4)	(5)	(5)	(6)	(7)	(8)
Strike days (10s of days) (N= 168,362)	-9.654	-0.072	-0.016	-0.015	0.001	-0.302	-0.006	-0.017	0.001
P-value Table 3	0.0004	0.0025	0.0021	0.0020	0.1930	0.1788	0.0369	0.0000	0.7799
P-value adjustment (Bonferroni)	0.0029***	0.0197**	0.0165**	0.016**	0.8200	0.7932	0.2599	0.0001***	0.9999
P-value adjustment (Sankoh et. al)	0.0013***	0.0108**	0.0061***	0.009***	0.5872	0.4730	0.2061	0.0000***	0.9915

Notes: Authors' estimation of equation (1) using 2004-2014 EPH data on 18-50 year old parents. Regressions include province and year-quarter fixed effects as well as controls for potential experience, potential experience squared, education (indicator variables of incomplete primary, complete primary, incomplete secondary, complete secondary, incomplete tertiary, and complete tertiary), and number of children in the household. Regressions further include an indicator variable for having a child of primary school age and a variable that measures the number of strike-induced school disruptions that took place during the past year (measured in tens of days). The table shows point estimates of the interaction between having a child of primary school age and the number of strike-induced school disruptions that took place during the past year. The coefficient measures the intent-to-treat effect of strike-induced school disruptions during the past year on current parental labor market outcomes. The p-values show the probability of observing the given coefficient value under the null hypothesis of no effect, and it is based on Bonferroni correction (the significant cutt-off is divided by the number of variables being tested) and the Sankoh, Huque, and Dubey (1997) correction (the Bonferroni correction is adjusted by the mean correlation among the outcomes other than the tested outcome). 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 province level (Table 3). *** indicates significance at the 1% level, ** indicates significance at the 5% level and * indicates significance at the 10% level.

Table A10: Subgroup analysis, fathers

	Labor income		Labor market participation		Labor market characteristics	
	Earnings	Wages	Employed	Labor Force	Second Job	Part-time job
	(1)	(2)	(3)	(4)	(6)	(7)
Panel A: By marital status						
i. Married	-1.625	-0.017	-0.002	-0.003	0.004	-0.003
(N=116,258)	(4.471)	(0.022)	(0.004)	(0.003)	(0.003)	(0.003)
% Effect	-0.22	-0.41	-0.23	-0.27	5.33	-2.36
ii. Single	41.762	0.212	0.006	0.014*	0.042***	-0.017*
(N=4,266)	(29.441)	(0.168)	(0.010)	(0.008)	(0.008)	(0.009)
% Effect	6.24	5.51	0.64	1.51	45.62	-7.93
Panel B: By Educational level						
i. High school or less	-2.569	0.000	-0.003	-0.003	0.005**	0.000
(N=94,685)	(3.212)	(0.017)	(0.003)	(0.003)	(0.002)	(0.002)
% Effect	-0.38	-0.01	-0.32	-0.27	11.49	-0.02
ii. Some university	12.414	-0.018	0.000	-0.001	0.001	-0.022***
(N=25,839)	(19.132)	(0.088)	(0.007)	(0.004)	(0.006)	(0.007)
% Effect	1.21	-0.32	0.04	-0.07	0.84	-12.33
Panel C: Wife with lower vs. higher potential earnings than husband						
i. Lower earnings	-1.799	0.009	0.002	-0.001	0.002	-0.003
(N=85,513)	(3.274)	(0.018)	(0.004)	(0.003)	(0.004)	(0.003)
% Effect	-0.24	0.21	0.19	-0.11	3.68	-1.96
ii. Higher earnings	-1.707	-0.088**	-0.014***	-0.007**	0.006	-0.005
(N=36,350)	(8.676)	(0.036)	(0.004)	(0.003)	(0.004)	(0.004)
% Effect	-0.22	-2.09	-1.43	-0.76	9.74	-3.88
Panel D: Younger kid in lower vs. higher grades						
i. In grades 1-3	-1.825	-0.016	-0.002	-0.003	0.005	-0.005**
(N=94,980)	(5.101)	(0.026)	(0.004)	(0.003)	(0.003)	(0.002)
% Effect	-0.24	-0.39	-0.21	-0.31	7.37	-3.63
ii. In grade 4-6	6.590	0.029	-0.001	-0.000	0.004*	-0.002
(N=66,589)	(5.570)	(0.035)	(0.003)	(0.002)	(0.002)	(0.003)
% Effect	0.87	0.69	-0.13	-0.01	6.08	-1.49

Notes: Authors' estimation of equation (1) using 2004-2014 EPH data on 18-50 year old fathers. Regressions include province and year-quarter fixed effects as well as controls for potential experience, potential experience squared, education (indicator variables of incomplete primary, complete primary, incomplete secondary, complete secondary, incomplete tertiary, and complete tertiary), and number of children. Regressions further include an indicator variable for having a child of primary school age and a variable that measures the number of strike-induced school disruptions that took place during the past year (measured in tens of days). The table shows point estimates of the interaction between having a child of primary school age and the number of strike-induced school disruptions that took place during the past year. The coefficient measures the intent-to-treat effect of strike-induced school disruptions during the past year on current parental labor market outcomes. Panel A stratifies the sample based on marital status. Panel B stratifies the sample based on educational attainment (more or less than 12 years of schooling). Panel C looks separately at married mothers with lower and higher potential earnings than their partners (potential earnings are estimated using standard gender-specific Mincer equations controlling for potential experience, education, and region). Panel D looks separately at mothers with the youngest child in grades 1-3 and mothers with youngest child in grades 4-6. 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 A11: Random subsample analysis

	Labor income		Labor market participation		Labor market characteristics	
	Earnings	Wages	Employed	Labor force	Second job	Part-time job
	(1)	(2)	(3)	(4)	(6)	(7)
i. High school or less (Panel B)	-7.698	-0.072	-0.019	-0.017	-0.008	-0.023
P-value	0.07	0.04	0.01	0.01	0.05	0.00
(N=47,925)						
ii. Lower earnings (Panel C)	-10.441	-0.096	-0.017	-0.017	-0.010	-0.023
P-value	0.07	0.04	0.03	0.075	0.04	0.01
(N=36,350)						

Notes: Authors' estimation of equation (1) using selected subsamples of the 2004-2014 EPH data on 18-50 year old mothers. Regressions include province and year-quarter fixed effects as well as controls for potential experience, potential experience squared, education (indicator variables of incomplete primary, complete primary, incomplete secondary, complete secondary, incomplete tertiary, and complete tertiary), and number of children. Regressions further include an indicator variable for having a child of primary school age and a variable that measures the number of strike-induced school disruptions that took place during the past year (measured in tens of days). The table shows point estimates of the interaction between having a child of primary school age and the number of strike-induced school disruptions that took place during the past year. The coefficient measures the intent-to-treat effect of strike-induced school disruptions during the past year on current parental labor market outcomes. The table shows average point estimates for groups of low-educated mothers (Panel B in Table 4) and mothers with lower earnings than their husbands (Panel C in Table 4). We consider 200 random subsamples of these groups that are of equal size to those groups they are being compared to. The p-values show the probability of observing the given coefficient value under the null hypothesis of no effect.

Table A12: Effect by public/private employment in t-1

	Labor income		Labor market participation		Labor market characteristics	
	Earnings	Wages	Employed	Labor force	Second job	Part-time job
	(1)	(2)	(3)	(4)	(6)	(7)
Panel A: Employed in Public Sector in t-1						
Mothers	-22.967*	-0.053	0.005	-0.001	-0.012	0.003
(N=9,386)	(13.327)	(0.119)	(0.006)	(0.007)	(0.011)	(0.013)
% Effect	-3.04	-0.83	0.02	-0.07	-7.79	0.49
Fathers	13.491	-0.325**	-0.001	0.000	0.000	-0.027
(N=6,913)	(31.530)	(0.124)	(0.002)	(0.003)	(0.020)	(0.020)
% Effect	1.44	-5.37	0.00	0.02	0.15	-8.46
Panel B: Employed in Private Sector in t-1						
Mothers	-30.028***	-0.196***	-0.029***	-0.018***	-0.016*	-0.028**
(N=21,669)	(8.327)	(0.058)	(0.007)	(0.006)	(0.008)	(0.011)
% Effect	-7.19	-6.06	-0.10	-2.10	-14.39	-7.06
Fathers	-0.124	0.013	0.005	-0.000	0.008	0.007***
(N=29,970)	(3.696)	(0.029)	(0.004)	(0.004)	(0.006)	(0.002)
% Effect	-0.02	0.31	0.01	-0.03	14.21	6.86

Notes: Authors' estimation of equation (1) using the rotating 2004-2014 EPH panel on 18-50 year old parents employed the year before, by sector of employment. Regressions include province and year-quarter fixed effects as well as controls for potential experience, potential experience squared, education (indicator variables of incomplete primary, complete primary, incomplete secondary, complete secondary, incomplete tertiary, and complete tertiary), and number of children. Regressions further include an indicator variable for having a child of primary school age and a variable that measures the number of strike-induced school disruptions that took place during the past year (measured in tens of days). The table shows point estimates of the interaction between having a child of primary school age and the number of strike-induced school disruptions that took place during the past year. The coefficient measures the intent-to-treat effect of strike-induced school disruptions during the past year on current parental labor market outcomes. 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 A13: Sensitivity to main sample restrictions, mothers

	Labor income		Labor market participation		Labor market characteristics	
	Earnings	Wages	Employed	Labor Force	Second Job	Part-time job
	(1)	(2)	(3)	(4)	(6)	(7)
Panel A: Including families with toddlers (age younger kid <3)						
Disrupted Schooling	-7.208**	-0.037*	-0.009	-0.009	-0.003**	-0.006
(N= 296,290)	(2.987)	(0.020)	(0.006)	(0.006)	(0.001)	(0.004)
% Effect	-2.35	-1.55	-1.59	-1.53	-4.30	-1.90
Panel B: Include outliers (top 1%)						
Disrupted Schooling	-11.347***	-0.065***	-0.012**	-0.012**	-0.004*	-0.010***
(N= 173,455)	(2.963)	(0.019)	(0.006)	(0.005)	(0.002)	(0.003)
% Effect	-3.40	-2.56	-2.10	-2.06	-5.63	-3.27
Panel C: Keeping only parents with children in primary or secondary school						
Disrupted Schooling	-11.755**	-0.087*	-0.021***	-0.019***	-0.008***	-0.020***
(N= 152,441)	(4.329)	(0.045)	(0.005)	(0.005)	(0.002)	(0.004)
% Effect	-3.57	-3.43	-3.78	-3.16	-10.23	-6.76

Notes: Authors' estimation of equation (1) using 2004-2014 EPH data on 18-50 year old mothers. Regressions include province and year-quarter fixed effects as well as controls for potential experience, potential experience squared, education (indicator variables of incomplete primary, complete primary, incomplete secondary, complete secondary, incomplete tertiary, and complete tertiary), and number of children. Regressions further include an indicator variable for having a child of primary school age and a variable that measures the number of strike-induced school disruptions that took place during the past year (measured in tens of days). The table shows point estimates of the interaction between having a child of primary school age and the number of strike-induced school disruptions that took place during the past year. The coefficient measures the intent-to-treat effect of strike-induced school disruptions during the past year on current parental labor market outcomes. Panel A includes families with toddlers (less than 2 years old). Panel B includes the top 1% of observations in terms of school disruptions during the past year (more than 30 days). Panel C drops parents from the control group that only had non-school aged children. 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 A14: Robustness and sensitivity analysis, fathers

	Labor income		Labor market participation		Labor market characteristics	
	Earnings	Wages	Employed	Labor Force	Second Job	Part-time job
	(1)	(2)	(3)	(4)	(6)	(7)
Panel A: Including province-specific linear time trends						
Strike days (10s of days)	0.382	-0.004	-0.002	-0.002	0.004	-0.004*
(N= 120,524)	(4.635)	(0.024)	(0.004)	(0.003)	(0.003)	(0.002)
% Effect	0.05	-0.10	-0.20	-0.20	6.69	-3.05
Panel B: Including all fixed effects for DDD						
Strike days (10s of days)	1.049	0.017	0.001	0.000	0.006	-0.001
(N= 120,524)	(4.271)	(0.032)	(0.003)	(0.002)	(0.005)	(0.005)
% Effect	0.14	0.41	0.11	-0.04	8.80	-0.65
Panel C: Controlling for local labor market conditions during the past year						
Strike days (10s of days)	2.886	0.006	-0.003	-0.003	0.005**	-0.004
(N= 106,468)	(4.594)	(0.022)	(0.004)	(0.002)	(0.002)	(0.002)
% Effect	0.38	0.15	-0.30	-0.27	7.53	-2.78
Panel D: Controlling for public administration strikes during the past year						
Strike days (10s of days)	3.633	0.002	-0.001	-0.002	0.006**	-0.005**
(N= 120,524)	(5.374)	(0.026)	(0.004)	(0.002)	(0.002)	(0.002)
% Effect	0.48	0.05	-0.13	-0.22	8.51	-3.30
Panel E: Drop parents who work in the primary education sector						
Strike days (10s of days)	-0.325	0.008	-0.002	-0.002	0.005	-0.004
(N= 118,024)	(4.332)	(0.026)	(0.004)	(0.003)	(0.003)	(0.002)
% Effect	-0.04	0.20	-0.19	-0.21	8.06	-2.81
Panel F: Reassigning treatment from t-1 to t+1						
Strike days (10s of days)	0.000	3.056	0.018	0.000	-0.001	-0.001
(N= 108,606)	(0.003)	(8.519)	(0.043)	(0.003)	(0.002)	(0.002)
% Effect	0.00	0.41	0.44	0.00	-0.10	-1.97

Notes: Authors' estimation of equation (1) using 2004-2014 EPH data on 18-50 year old fathers. See footnote in Table 3. Panel A includes province-specific linear time trends. Panel B includes all fixed effects for a DDD approach (year-by-province, year-by-dummy of having a child of primary school age, and province-by-dummy of having a child of primary school age). Panel C controls for average unemployment, average wages, and average per capita family income at the province level during the past year. Panel D controls for the number public administration strike days that took place in the previous year and its interaction with having a child in primary school. Panel E drops parents that work in the primary education sector. Panel F shows results from a placebo test in which treatment has been reassigned from t-1 (strikes in the past year) to t+1 (strikes in the next 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 A15: Robustness and sensitivity analysis for additional labor market outcomes

	Mothers			Fathers		
	Unemployed	Hours	Full-time job	Unemployed	Hours	Full-time job
	(5)	(5)	(8)	(5)	(5)	(8)
Panel A: Including province-specific linear time trends						
Strike days (10s of days)	0.002*	-0.316	0.001	-0.002	0.000	0.002
	(0.001)	(0.216)	(0.003)	(0.003)	(0.001)	(0.006)
% Effect	3.58	-1.62	0.26	-0.21	1.06	0.25
Panel B: Including all fixed effects for DDD						
Strike days (10s of days)	0.001	-0.361	0.003	-0.001	-0.184	0.002
	(0.001)	(0.321)	(0.006)	(0.002)	(0.250)	(0.007)
% Effect	1.3	-1.85	1.1	-4.49	-0.4	0.24
Panel C: Controlling for local labor markets at t-1						
Strike days (10s of days)	0.001	-0.278	0.001	0.000	0.034	0.001
	(0.002)	(0.246)	(0.003)	(0.001)	(0.146)	(0.006)
% Effect	1.48	-1.43	0.34	0.51	0.07	0.14
Panel D: Controlling for public administration strikes						
Strike days (10s of days)	0.002	-0.276	0.004	-0.001	0.135	0.003
	(0.002)	(0.316)	(0.005)	(0.002)	(0.150)	(0.005)
% Effect	3.32	-1.42	1.53	-2.84	0.29	0.43
Panel E: Drop parents who are teachers						
Strike days (10s of days)	0.001	-0.248	0.003	-0.000	0.035	0.002
	(0.001)	(0.227)	-0.002	(0.001)	(0.159)	(0.005)
% Effect	2.94	-1.33	0.97	-0.82	0.07	0.24
Panel F: Reassigning treatment from t-1 to t+1						
Strike days (10s of days)	-0.001	0.142	-0.001	-0.001	-0.014	0.000
	(0.001)	(0.219)	(0.006)	(0.001)	(0.145)	(0.005)
% Effect	-3.11	0.73	-0.25	-3.33	-0.03	0.04

Notes: Authors' estimation of equation (1) using 2004-2014 EPH data on 18-50 year old mothers and father (same sample size Table 8 and Table A15). See footnote in Table 3. The coefficient measures the intent-to-treat effect of strike-induced school disruptions during the past year on current parental labor market outcomes. Panel A includes province-specific linear time trends. Panel B includes all fixed effects for a DDD approach (year-by-province, year-by-dummy of having a child of primary school age, and province-by-dummy of having a child of primary school age). Panel C controls for average unemployment, average wages, and average per capita family income at the province level during the past year. Panel D controls for the number public administration strike days that took place in the previous year and its interaction with having a child in primary school. Panel E drops parents that work in the primary education sector. Panel F shows results from a placebo test in which treatment has been reassigned from t-1 (strikes in the past year) to t+1 (strikes in the next 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 A16: Effect of strike-induced school disruptions on labor market outcomes of non-parent adults

	Labor income		Labor market participation		Labor market characteristics	
	Earnings	Wages	Employed	Labor Force	Second Job	Part-time job
	(1)	(2)	(3)	(4)	(6)	(7)
Panel A. Non-parent adults over 17 years old						
Strike days (10s of days)	-0.493	-0.009	-0.006	-0.004	0.001	-0.001
(N=910,938)	(3.850)	(0.022)	(0.005)	(0.007)	(0.001)	(0.001)
% Effect	-0.18	-0.49	-1.22	-0.77	1.73	-0.76
Panel B. Non-parent adults age 18-34						
Strike days (10s of days)	0.998	-0.015	-0.006	-0.006	0.001	-0.005***
(N=375,851)	(7.377)	(0.044)	(0.007)	(0.008)	(0.001)	(0.002)
% Effect	0.35	-0.82	-1.09	-0.96	4.14	-2.83
Panel C. Non-parent adults age 35-59						
Strike days (10s of days)	4.146	0.031	-0.007	-0.007	-0.002	-0.001
(N=258,336)	(5.269)	(0.027)	(0.005)	(0.006)	(0.004)	(0.003)
% Effect	0.87	1.01	-0.99	-0.92	-2.39	-0.55
Panel D. Non-parent adults over 59 years old						
Strike days (10s of days)	2.084	-0.001	0.007	0.010	0.006	0.003
(N=292,926)	(5.846)	(0.024)	(0.008)	(0.008)	(0.005)	(0.002)
% Effect	1.85	-0.12	3.91	4.99	64.01	4.09

Notes: Authors' estimation of equation (1) using 2004-2014 EPH data on all non-parent adults (Panel A) and by age group (Panels B-D). Regressions include province and year-quarter fixed effects as well as controls for potential experience, potential experience squared, education (indicator variables of incomplete primary, complete primary, incomplete secondary, complete secondary, incomplete tertiary, and complete tertiary), and number of children. Regressions further include an indicator variable for living in a household with a child of primary school age and a variable that measures the number of strike-induced school disruptions that took place during the past year (measured in tens of days). The table shows point estimates of the interaction between living in a households that has a child of primary school age and the number of strike-induced school disruptions that took place during the past year. The coefficient measures the intent-to-treat effect of strike-induced school disruptions during the past year on current labor market outcomes of non-parents in the households. 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.