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Oh Mother:
The Neglected Impact of School Disruptions

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Temporary school closures (TSC) represent a major challenge to policymakers across the globe due to their potential impact on instructional time and student achievement. A neglected but equally important question relates to how such closures affect the labor market behavior of parents. This paper provides novel evidence on the effect of temporary school closures on parental labor market behavior, exploiting the prevalence of primary school teacher strikes across time and provinces in Argentina. We find clear evidence that temporary school closures negatively impact the labor market participation of mothers, in particular lower-skilled mothers less attached to the labor force and mothers in dual-income households who face a lower opportunity cost of dropping out of the labor force. This effect translates into a statistically significant and economically meaningful reduction in labor earnings: the average mother whose child is exposed to ten days of TSCs suffers a decline in monthly labor earnings equivalent to 2.92% of the mean. While we do not find any effects among fathers in general, fathers with lower predicted earnings than their spouses also experience negative labor market effects. This suggests that the parental response to TSCs depend, at least in part, on the relative income of each parent. A back-of-the-envelope calculation suggest that the aggregate impact of TSCs on annual parental earnings is more than $113 million, and that the average mother would be willing to forego 1.6 months of labor earnings in order to ensure that there are no TSCs while her child is in primary school.

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1. Introduction

Temporary school closures (TSC) represent a major challenge to policymakers across the globe due to their potential impact on instructional time and student achievement, and the prevalence of TSCs – with more than 10 million K-12 students affected in the US alone per year (Wong et al. 2014) – has fueled a large interest among researchers in disentangling the effects of school disruptions on student outcomes (e.g. Mercotte and Hemelt 2008; Baker 2013; Hansen 2013; Goodman 2014; Jaume and Willén 2018).

A neglected but equally important question relates to how temporary school closures affect the labor market behavior of parents. Programs and services such as universal preschool, public schooling and subsidized after-school activities allow caregivers to substitute childcare responsibilities for employment. In the event of a TSC, parents can no longer outsource childcare responsibilities to schools. As a consequence, TSCs may lead parents to reduce work hours or drop out of the labor force, with long-lasting adverse effects on wages and disposable income.[1] Since mothers tend to be the primary caregiver in the family – even in dual-career households – such effects could disproportionally hurt mothers and exacerbate existing labor market and intra-household gender inequalities (e.g. Gauthier, Smeeding and Furstenberg 2004; Guryan, Hurst and Kearney 2008; PRC 2015).[2]

This paper provides novel evidence on the effect of temporary school closures on parental labor market behavior. The temporary school closures we consider are located in Argentina and are caused by public primary school teacher strikes. Between 2003 and 2013, Argentina experienced 576 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 (Table 1 and Figure 1). The total number of strike days during this period across all provinces was 1,974, and the average province lost 4.2% of all instructional days during this decade due to strikes. This provides us with substantial variation in the length and intensity of the TSCs that parents were exposed to over a long period of time, and makes Argentina a particularly interesting case for the study of the relationship between TSCs and parental labor market outcomes.

We begin by presenting a simple static utility maximization model that offers an intuitive framework for thinking about the decision problem that parents face when they are exposed to a temporary school closure. In this model, parents are assumed to maximize utility over childcare and consumption subject to a budget constraint and a childcare quality constraint. The parent can respond to the TSC in three ways: work and purchase private care, work and leave the child alone

[1] The struggle that parents experience in trying to deal with the fallout of school disruptions has been reported on extensively in the media with respect to the recent wave of teacher strikes in the US. See for example CNN (2012) and Reuters (2018).

[2] For recent research on childcare policies and parental labor supply, see for example Lefebvre and Merrigan (2008), Baker, Gruber and Milligan (2008), Goux and Maurin (2010), Havnes and Mogstad (2011), Gathmann and Sass (2012), Fitzpatrick (2012), Bauernschuster and Schlotter (2015).

at home, or not work and provide home care.[3] Under certain conditions, we show that the optimal decision of the parent is to resort to home care, establishing a direct link between TSCs and parental labor market outcomes. In addition to providing a framework for thinking about the decision problem faced by the average parent, we show that our model is useful for predicting how the response may differ depending on the parent's socioeconomic background, and for understanding intra-household responses to TSCs.

After having provided a framework for thinking about the decision problem that parents face when they are exposed to a temporary school closure, we exploit a rich and newly created data set on teacher strikes in Argentina to examine the reduced-form effect of school disruptions on parental labor market behavior. Our identification strategy consists of comparing the difference in outcomes between parents with and without children in primary school in provinces and years that experienced more strikes to that same difference in provinces and years that experienced fewer strikes. Our results are therefore identified off of variation in teacher strikes within and across provinces over time between parents with and without children in primary school. Our control group thus consists of parents to children that have not yet started primary school and parents to children in secondary school.[4]

The key assumption underlying our estimation strategy is that there are no province-specific secular trends, shocks, or policies, contemporaneous with teacher strikes that differentially affect the labor market outcomes of parents with and without children of primary school age. We show extensive evidence that our data are consistent with this assumption. 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, and excluding parents with particularly 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 (exploiting only variation across provinces in a given year and within provinces over time), and show that there is no effect of TSCs on the labor market behavior of

[3] Since our focus is on the parental labor market response to school disruptions, we do not explicitly differentiate between leaving the child with friends and family and leaving the child alone at home. While these two options likely have different effects on the child's development which is captured in our model by the quality of care of leaving the child alone at home, the parental labor market implications are the same.

[4] Teacher strikes in Argentina are predominantly concentrated among public primary schools teachers. Although some strikes may also affect secondary school children, parents to children in secondary school are unlikely to respond to those strikes as these children are old enough to be left alone at home in the event of an unplanned school closure. If parents to secondary school children were affected by some of the strikes, our point estimates would be attenuated. One way to examine this attenuation concern 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 8, and the fact that the point estimates in Panel A of Table 8 are not statistically significantly different from our baseline results suggest that this attenuation is small-to-negligible. This is further supported by the results in Panel B of Table 8, which shows the same dose-response difference-in-difference model estimated for parents without children in primary school. The lack of economically meaningful and statistically significant results among this subgroup of parents is consistent with the idea that this attenuation is likely negligible.

parents in our control group. These results are inconsistent with the presence of province-specific shocks (or policies) contemporaneous with teacher strikes that differentially affect the outcomes of parents with and without children of primary school age, and support a causal interpretation of our results.[5]

We find clear evidence that temporary school closures negatively impact the labor market participation of mothers, in particular lower-skilled mothers less attached to the labor force and mothers in dual-income households who face a lower opportunity cost of dropping out of the labor force. This effect translates into a statistically significant and economically meaningful reduction in labor earnings: a mother whose child is exposed to ten days of TSCs suffers a decline in total earnings equivalent to 2.92% of the mean.

While we do not find any effects among fathers in general, fathers with lower predicted earnings than their spouses also experience negative labor market effects. This result suggests that the labor supply response of parents depend, at least in part, on the relative income of each parent (Apps and Rees 2002; Blundell, Chiappori and Meghir 2005; Cherchye, De Rock and Vermeulen 2012). However, this group of households is small (less than 1/4 of all dual-parent households), and the estimated effects for this subgroup of fathers is significantly smaller than the estimated effects for mothers, such that women are disproportionally affected by TSCs.

Assuming constant effects across years, a back-of-the-envelope calculation suggests that the aggregate impact of TSCs on parental labor market earnings is more than $113 million each year, and that the average mother would be willing to forego 1.6 months of earnings in order to ensure that there are no TSCs while her child is in primary school. These numbers illustrate the importance of providing stable childcare options to mothers in order to maximize their ability to participate in the labor market and to prevent an augmentation of labor market and intra-household gender inequality.

Finally, we study an alternative but costly way in which parents can respond to teacher strikes: enrolling their children in private school. Teacher strikes in Argentina are generally restricted to public school teachers, and instead of dropping out of the labor force to provide home care, parents can transfer the child to private school. We find strong support in favor of this hypothesis, in particular among middle-income families. This finding demonstrates that the availability of alternative care options may mute some of the adverse effects of TSCs on parental labor market outcomes, though this could come at the expense of increased socioeconomic school segregation.

[5] We also note that existing research has found teacher strikes in Argentina to display an erratic behavior without any discernable trends or explanations, and that there is no apparent relationship between teacher strikes and local labor market conditions (Murillo and Ronconi 2004; Narodowki and Moschetti 2015; Jaume and Willén 2018). Even though our empirical analysis does not depend on the strict assumption that strikes are orthogonal to province-specific secular trends or shocks – only that such shocks and trends do not differentially affect parents with and without children of primary school age – it is reassuring to note that pre-existing studies find evidence in favor of a stricter version of our identifying assumption.

To the best of our knowledge, this is the first paper to provide a detailed analysis of the effect of temporary school closures on parental labor market behavior. The paper contributes to the existing literature in several important ways. While there exists a large literature examining how changes in childcare costs affect parental labor supply (e.g. Heckman 1974; Blau and Robins 1988; Ermisch 1993; Connelly 1992; Ribar 1992; Kimmel 1995; Anderson and Levine 2000), and a related literature exploring how the availability of various childcare options interact with parental labor market behavior (e.g. Berlinski, Galiani, and Gertler 2009; Cascio 2009; Gelbach 2002; Lefebvre and Merrigan 2008; Havnes and Mogstad 2011; Chiuri 2000; Fitzpatrick 2011; Goux and Maurin 2010; Nollenberger and Rodriguez-Planas 2015), none of these papers have examined the effect of unexpected and temporary changes in childcare costs/options.[6] Further, only Dunbar (2013) has examined the effects of school disruptions on parents' labor market outcomes. However, Dunbar (2013) only looks at short-term effects on earnings, and the limited variation in school disruptions makes it difficult for the author to identify precise effects.[7]

The rest of this paper is organized as follows: Section 2 presents an intuitive framework for thinking about the decision problem that parents face when they are exposed to a TSC and provides institutional background on teacher strikes in Argentina; Section 3 introduces our data and empirical estimation strategy; Section 4 presents our baseline results; Section 5 shows the results from an extensive set of robustness and falsification tests; Section 6 examines the effect of TSCs on private school enrollment: and Section 7 concludes.

2. School Disruptions and Parental Labor Market Decisions

2.1. Economic Intuition and Predictions

We begin with a simple static model of individual decision-making from which we derive certain predictions regarding the likely parental labor market response to school disruptions and temporary school closures. Our starting point is a parent who participates in the labor market and has a child in a public school that suddenly experiences a school disruption.[8] The parent has to decide what type of alternative childcare he/she wishes to provide. The parent has three possible options: work and purchase private care, work and leave the child alone at home, or drop out of

[6] 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).

[7] Specifically, Dunbar (2013) looks at how income changes for 434 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.

[8] Parents with children in private school are isolated from public school disruptions. It should be noted that we cannot eliminate these parents from our empirical analysis due to endogeneity issues, and the intent-to-treat effects that we estimate should therefore be interpreted as a lower bound of the parental labor market effect of school disruptions. During our analysis period, the proportion of primary school children that attended private school was approximately 0.2.

the labor force and provide home care.[9] To make the model tractable we do not allow the parent to choose a combination of these responses (e.g. purchase some private care and provide some home care). We assume that a parent derives higher utility from better quality childcare, and that there is only one level of quality for each of the different forms of childcare. We also assume that the quality of home care and private care are strictly greater than the quality of care derived from leaving the child alone at home.[10] Given these assumptions, the parent's decision problem can be stated as follows:

$$\max \; U = X^{\beta} Q^{(1-\beta)} \tag{1}$$
$$s.t. \;\; w(1 - I_H) + N = X + I_P(P) \tag{2}$$
$$Q = I_P(Q_P) + I_H(Q_H) + I_A(Q_A) \tag{3}$$

where

$X =$ consumption of goods other than child care (price normalized to 1)
$Q =$ average quality of child care
$\beta =$ preference parameter for consumption of goods other than child care with $0 < \beta < 1$
$w =$ net wage of parent
$N =$ effective nonlabor income (including earnings of other household members)
$I_H =$ indicator equal to one if parent chooses home care
$I_P =$ indicator equal to one if parent chooses private care
$I_A =$ indicator equal to one if parent choose to leave child alone at home
$P =$ cost of private child care

In this model, a parent maximizes his/her utility over goods (*X*) and childcare quality (*Q*) subject to two constraints. The first constraint is the budget constraint, and states that total income must equal total cost of consumption on goods and childcare. The second constraint defines the quality of childcare, which can take one of three values: Q_P, Q_H or Q_A. To determine which of the three possible responses to choose, the parent is assumed to compare the utility under each of the three scenarios and pick the one that provides the highest utility. Substituting (2) and (3) into (1) and using the facts that $\sum(I_P + I_H + I_A) = 1$ and $I_P,\; I_H,\; I_A \in \{0; 1\}$, this means that the parent will compare the utility from the following three cases:

$$U(I_P = 1) = (w + N - P)^{\beta}(Q_P)^{(1-\beta)} \tag{4}$$
$$U(I_H = 1) = (N)^{\beta}(Q_H)^{(1-\beta)} \tag{5}$$
$$U(I_A = 1) = (w + N)^{\beta}(Q_A)^{(1-\beta)} \tag{6}$$

Equations (4) through (6) imply that a parent's response to a school disruption depends on the relative quality of the different care options (Q_P, Q_H, Q_A), the cost of private care (P), the net wage (w) and the effective nonlabor income (N). Though there are interesting policy implications

[9] To make the model tractable, we consider a parent's decision to leave the child with a relative or a friend synonymous with leaving the child alone at home. This does not affect our model predictions or empirical analysis.
[10] This assumption can be relaxed without affecting the predictions in this section; it is only imposed to make the illustrations in Figure 2 more tractable.

associated with each of the three alternative care options, the labor market outcomes of the parent will only be affected if he/she decides to provide home care. Based on Equations (4) through (6), a parent will choose home care if the following two conditions hold:

$$(\frac{N}{w+N})^{\beta} > (\frac{Q_A}{Q_H})^{(1-\beta)} \quad (7)$$

$$(\frac{N}{w+N-P})^{\beta} > \left(\frac{Q_P}{Q_H}\right)^{(1-\beta)} \quad (8)$$

Equations (7) and (8) state that a parent's probability to choose home care can be modeled as a positive function of N, P, and Q_H, and as a negative function of w, Q_A, and Q_P. In particular, parents are more likely to choose home care if they have a substantial non-labor income, if private care is expensive, if the quality of home care is high, if the quality of private care and the care quality from leaving the kid alone is low, and if they earn low wages.

To better illustrate the relationship between a parent's response to school disruptions and the variables in equations (7) and (8), Figure 2 displays a series of visual examples of how changes in the relative quality of the different care options, the cost of private care, the net wage, and the effective non-labor income, may affect the parental labor market response to a school disruption. The examples in subfigure 2(a) hold the relative quality of the different care options constant and show how changes in the monetary parameters w, P, and N, may affect a parent's optimal response to a school disruption. The examples in subfigure 2(b) hold the monetary parameters constant and illustrate how changes in the relative quality of the different care options may change a parent's utility maximization response to a school disruption.[11]

In addition to providing a framework for thinking about the decision problem faced by the average parent, the above discussion is also useful for predicting how the optimal response may differ across groups of parents with different socioeconomic backgrounds and characteristics. For example, single parents likely have lower N, and are more dependent on w in order to secure a subsistence level of consumption on goods, such that the probability of dropping out of the labor force to provide home care is reduced.[12] Another example concerns low-educated parents. This group will likely face lower w, such that the opportunity cost of dropping out of the labor force is smaller, and the likelihood of providing home care may therefore be larger.

The framework outlined in this section can also be used to examine and rationalize differential intra-household responses to TSCs. To do this, split w and Q_H into parent-specific components: w_{moth} , w_{father} , $Q_{H(mother)}$ and $Q_{H(father)}$. A rational household would allocate the

[11] The model presented in this section assumes that parents are working when the TSC takes place. However, TSCs may affect the labor market behavior of nonworking parents as well. Specifically, a TSC may act as an incentive to remain outside the labor force in case future strikes occur, such that nonworking parents experience a reduction in the probability to enter/reenter the labor force. In Section 4, we demonstrate that our results suggest that the labor market effects we identify operate through both a direct reduction in labor market participation among working parents and a reduction in the probability of reentering the labor force among nonworking parents.

[12] In the case of married parents, N can be interpreted as the sum of household's non-labor income and partner´s labor earnings.

responsibility to provide home care such that the household's utility is maximized. Under the framework provided above, this means that the household would delegate the task of providing home care to mothers if

$$\left(\frac{w_{fathe}\ \ +N}{w_{mothe}\ \ +N}\right)^{\beta} > \left(\frac{Q_{H(fathe\ )}}{Q_{H(mother)}}\right)^{(1-\beta)} \tag{10}$$

Equation (10) demonstrates that the intra-household decision on who should provide home care (provided that the household chooses home care) will be a function of the relative quality of the parents' home care and their net wages. For example, if $Q_{H(mot\quad)} = Q_{H(fathe\ )}$ and $w_{fathe} < w_{mothe}$, home care will be provided by the father. If $w_{father} < w_{mothe}$ but $Q_{H(mother)} > Q_{H(fathe\ )}$ such that equation (10) holds, home care will be provided by the mother. If $Q_{H(mothe\ )} > Q_{H(father)}$ and $w_{mothers} < w_{fathers}$, mothers would drop out of the labor force and provide home care.

Abstracting away from any potential gender differences in childcare quality, the documented wage gap between men and women suggests that mothers will be more likely to assume the childcare responsibilities in the event of TSCs if the household chooses home care (Blau 2016). This implies that mothers may be disproportionally affected by TSCs, and that TSCs may exacerbate labor market and intra-household gender inequality. It is important to note that our model assumes that both parents value childcare activities performed by themselves and their partner equally, which may not be the case. We recognize that this is a limitation of the model to keep it tractable. Nevertheless, the model provides an intuitive and useful tool with which one can hypothesize about, and rationalize, a couple's response to TSCs.[13]

2.2. Teacher Strikes As Temporary School Closures

The goal of this paper is to examine how TSCs interact with the labor market decisions of parents. The school disruptions we examine are located in Argentina and are caused by primary school teacher strikes between 2003 and 2013. 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 (2018).

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

[13] The model presented in this section is written in a static setting, where labor decisions occur contemporaneously with TSCs. This is a slight oversimplification of the TSC effect, because once the initial decision is made, it is likely to also affect future labor market outcomes as labor outcomes tomorrow depend labor market decisions today. In the case of parents that drop out of the labor force in response to TSCs, there might be labor market frictions preventing them from going back to work right after the TSCs. Parents may also decide to stay out of the labor force because of fear of new strike-induced school disruptions. In this case, the effort required to search for a new job may be too high if parents believe that they will be forced to drop out again soon.

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.[14] The occupation with the second-highest level of strikes is public administration, and makes up 25 percent of the strikes in the country (Etchemendy 2013).

Teacher unions are typically organized at the provincial 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.

Table 1 shows the variation in teacher strikes across provinces and over time in Argentina between 2003 and 2013. During this period, Argentina experienced 576 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 (Table 1 and Figure 1). The total number of strike days during this period across all provinces was 1,974, and the average province lost 4.2% of all instructional days during this decade due to strikes.[15] This provides us with substantial variation in the length and intensity of the school disruptions that parents are exposed to over a long period of time.[16]

The pervasiveness of teacher strikes over time and across provinces 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; Jaume and Willén 2018). For example, Narodowski and Moschetti (2015) concludes that strikes display an erratic behavior without any discernable trends or explanations. Even though our empirical analysis does not depend on the strict assumption that strikes are exogenous to province-specific secular trends or shocks – only that such shocks and trends do not differentially affect parents with and without children that attend primary school – 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 that support the assumption that strike-induced school disruptions are uncorrelated with province-specific contemporaneous secular trends, shocks and policies.

3. Data and Estimation Strategy

3.1. Data

[14] The fraction of students that attended private school at the primary level during our analysis period was 0.2. Since these students are less likely to be exposed to teacher strikes, the fact that we cannot condition on having a child in public school will attenuate our results.

[15] The average province lost 82.5 school days due to teacher strikes during this period out of 1,980 instructional days (180 days per year).

[16] It is important to note that we use variation in teacher strikes at the year-quarter level, such that Table 1 understates the variation used in this paper. A visual illustration of the variation in teacher strikes at the year-quarter level is provided in Figure 1.

The teacher strike data that we use was created by Jaume and Willén (2018) and is based on historic reports of the Argentine economy published by Consejo Técnico de Inversiones (CTI). These reports provide province-specific information on strikes by industry and month. For the empirical analysis in this paper, we use information from 2003 to 2013. Figure 1 provides a visual illustration of the variation in teacher strikes over time and across provinces that we use as identifying variation in our analysis.

The main objective of this research project is to examine how parents alter their labor market behavior in response to TSCs. To address this question, 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). This provides us with a data set of approximately 1.3 million individual-year observations. We assume that children attend primary school between the ages of 6 and 11, 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 twelve months.[17]

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 potential 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. In addition, we study the probability of holding multiple jobs, hours worked, and the probability of holding a part-time (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 working activity. Descriptive statistics of the variables we use are shown in Table 2, separately for mothers and fathers with and without children in primary school.

We impose three sample restrictions prior to conducting our baseline analysis. First, we restrict our sample to individuals that have at least one child under the age of 18 at the time of the survey. Second, parents to toddlers are much less likely to participate 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 12 months (top 1%). These three restrictions are imposed to ensure that we have a comparable control and treatment group, that our results are representative of the parents that we are interested in, and that

[17] In Jaume and Willén (2018), 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. It should be noted that the assumption that children attend primary school between the ages of 6 and 11 leads to some measurement error in treatment assignment because children start primary school the calendar year in which the number of days they are 6 years old is maximized. This assumption may thus lead to a slight attenuation of our results.

the effects are not driven by outliers. In Section 5, we show that our results are robust to relaxing these restrictions.

Preliminary evidence on the relationship between school disruptions, labor force participation and wages is displayed in Figure 3, which plots the predicted labor force participation and hourly wage for parents with and without children in primary school as a function of the number of strike-induced TSCs that took place over the past twelve months, separately for mothers and fathers. There is clear suggestive evidence of a negative association between TSCs and mothers' labor force participation as well as between TSCs and mothers' hourly wages: For each ten days of school disruptions, labor force participation declines by approximately 1.0 percentage points and hourly wages are reduced by 3.5 percent. With respect to fathers, Figure 3 suggest that they are unaffected by TSCs. Though instructive, causal inference cannot be made from these graphs.

In addition to using the full EPH sample, we also take advantage of the survey's rotating panel design and construct successive panels covering the period 2004-2014.[18] We use this data to study yearly changes in earnings, wages, and work hours to confirm the results in our baseline specification. In addition, we use this data to analyze movements in and out of the labor force during the year that the strike-induced TSCs took place. This allows us to disentangle if the labor force participation effects are driven by employed parents dropping out of the labor force, by nonworking parents being disincentivized to reenter the labor force, or both. 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 main 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 A1), and even though the probability of attrition does not appear to be correlated with the TSCs, we interpret the results from the panel estimation with caution and avoid splitting the sample into smaller subgroups.[19]

3.2. Estimation Strategy

We exploit variation in teacher strikes within and across provinces over time between parents with and without children in primary school. Specifically, we estimate models of the following form separately for mothers and fathers:

$$Y_{ipt(q)} = \gamma_0 + \alpha_1 Str_{pt(q)-1} + \alpha_2 Ch_{ipt(q)-1} + \beta\left(Str_{pt(q)-1} \cdot Ch_{ipt(q)-1}\right) + \gamma X_{ipt(q)-1} + \rho_p + \tau_{t(q)} + \varepsilon_{ipt(q)} \tag{11}$$

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[18] The EPH employs the 2-2-2 rotation scheme: each selected dwelling is interviewed in two consecutive quarters, then left out of the sample for two quarters, and then interviewed for another two quarters. EPH does not follow movers, such that the attrition rate is relatively high.

[19] The correlation between TSCs in the previous 12 months and attrition is -0.02, and the point estimate obtained from running our preferred model specification (equation 11) with attrition as the dependent variable is 0.0011 with a standard error of 0.0022.

$Y_{ipt(q)}$ 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 teacher strikes that occurred in province p during the twelve months leading up to the interview. $Ch_{ipt(q)-1}$ is a dichotomous variable taking the value of 1 if individual i had a child of primary school age during the last twelve months. The main variable of interest is $\left(Str_{p\ t(q)-1} \cdot Ch_{ipt(q)-1}\right)$, and β measures the intent-to-treat effect of strike-induced school disruptions during the past twelve months on current parental labor market outcomes.[20]

Equation (11) also includes province (ρ_p) and year-by-quarter ($\tau_{t(q)}$) fixed effects. The province fixed effects control for variation in outcomes that are common across all respondents within a province and the year-by-quarter fixed effects control for national shocks that impact all individuals at a given time. We also control for potential experience, potential experience squared, educational attainment and total number of children in the household. These controls are in the vector *X*. Standard errors are clustered at the province level.[21]

Conditional on the controls in the model, the identifying variation comes from differences in exposure to strikes within districts over time between parents that live with and without a child of primary school age. The key assumption underlying the identification of parameter β is that there are no province-specific secular trends, shocks, or other policies, contemporaneous with teacher strikes that differentially affect the labor market outcomes of parents with and without children of primary school age.

It should be noted that (1) between 15 and 20 percent of primary school children in Argentina attended private school during our analysis period, and (2) not all primary schools teachers in a province may strike when there is a province-specific primary school teacher strike as coded in our data. Both these factors will attenuate our results. The estimates produced by equation (11) are therefore best interpreted as intent-to-treat estimates of the effect on TSCs on parental labor market behavior, and should be seen as a lower bound of the true TSC effect.

In Section 5, we perform a series of robustness and sensitivity checks to demonstrate that the data 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 particularly high exposures to TSCs. In addition, we perform

[20] Our estimation strategy can be related 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 (11) 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 (11). However, in Table 7 we show that our results are robust to the strict DDD specification.

[21] As we only have 24 clusters, we also estimate our preferred specification using wild cluster bootstrap standard errors as discussed in Cameron and Miller (2015). Online Appendix Table A2 shows that our results are robust to this adjustment.

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 (exploiting only variation across provinces in a given year and within provinces over time), and show that there is no effect of TSCs on the labor market behavior of parents in our control group. The results in Section 5 are inconsistent with the presence of province-specific shocks (or policies) contemporaneous with teacher strikes that differentially affect the outcomes of parents with and without children of primary school age, and support a causal interpretation of our results.

4. Results

4.1 Baseline Results

Table 3 presents baseline estimates of the effect of strike-induced TSCs 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 (11), and the point estimates should be interpreted as the intent-to-treat effect of TSCs on the labor market behavior of parents. 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 model (requires stronger identification assumptions).[22]

Columns 1 and 2 of Table 3 present results for earnings and wages. Focusing on mothers, there is clear evidence of a large negative effect of school disruptions on earnings: the point estimate in column (1) indicates that a mother who has a child of primary school age suffers a loss of \$9.65 in total monthly earnings from each 10 days that her child's school is disrupted. This effect is -2.92% relative to the mean, shown directly below the estimate in the table. The associated hourly wage effect in column (2) is of a comparable magnitude (-2.84% relative to the mean), and is also statistically significant at the 1% level. With respect to fathers (Panel B), the estimates are much smaller and not statistically significantly different from zero.

Assuming constant effects across years, the point estimate on labor earnings in Table 3 suggests that the average mother in our sample will experience a total earnings reduction of

[22] The specification that incorporates province and time fixed effects interacted with having a child in primary school is incredibly demanding, increasing the number of parameters that needs to be estimated by more than 1,000. 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 attenuates the results slightly (Panel E of Table 7). However, the results obtained from this strict DDD specification are consistent with our baseline results, and we fail to reject the null hypothesis that these results are statistically significantly different from our baseline estimates. This greatly improves the causal interpretation of our baseline results.

$521.1 during the course of her child's time in primary school due to TSCs.[23] This is equivalent to 13% of the average annual income of the mothers in our sample. Thus, the average mother in our sample would be willing to forego at least 1.6 months of labor earnings in order to prevent teachers from striking during her child's primary school time. Another way in which one can think about this effect 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 $113 million.[24]

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, consistent with the framework outlined in Section 2.1. To examine this question in detail, Table 3 also shows estimates of equation (11) with respect to both labor market participation outcomes and job characteristics.

With respect to labor market participation, Table 3 shows estimates of equation (11) 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 strike-induced TSCs lower the employment probability by approximately 0.016 percentage points and reduces the likelihood of being in the labor force by 0.015. These effects are -2.84% and -2.39% relative to the respective means. We find no effect with respect to unemployment; the reduction in employment is driven exclusively by mothers leaving the labor force (and not by, for example, employers discriminating against individuals likely to drop out or skip work due to TSCs). Consistent with the lack of statistically and economically significant wage and earnings estimates for fathers, we find no evidence of labor force participation effects among fathers.

To better understand the labor market participation effects, Table 3 also shows estimates of the probability to engage in part-time work (column 7), and the probability to engage in full-time work (column 8).[25] The results show a clear reduction in the probability of working part-time among mothers, and no effect on the probability of working full-time. This suggests that mothers less attached to the labor force drive the results.

With respect to intensive labor supply effects, Table 3 shows estimates of equation (11) where hours worked (column 5) and the probability of holding a second job (column 6) are used as dependent variables. Among mothers, the results suggest that there is no statistically significant

[23] This number is obtained by multiplying the point estimate on earnings (9.65) with the average number of strikes in the past 12 months (0.75), the number of months in a year (12) and the number of years of primary school (6). The average monthly earnings for a mother in our sample is $327.

[24] This number is obtained by scaling the point estimate (9.65) with the average number of strikes in the past 12 months (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).

[25] To avoid selection bias due to the effect of strikes on employment decisions, non-employed mothers are also included in these estimations.

effect on hours worked, 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 on the intensive margin of labor supply among men.

The effects displayed in Table 3 may conceal important heterogeneous effects across the wage and work hours distributions. This is of particular interest given the results in columns (7) and (8), which suggest the effects may be driven by mothers at the margin of employment. We explore this possibility in Table 4 by estimating the impact of TSCs on the quantiles of the unconditional (marginal) distribution of wages (Panel A) and work hours (Panel B) using RIF regressions.[26] The results in both Panel A and Panel B demonstrate that the adverse effects identified in Table 3 are driven by mothers at the margin of employment located at percentiles 50^{th}-70^{th} (wages and hours of percentiles 10^{th}-40^{th} are equal to zero) and that high-wage mothers are unaffected by TSCs. This could be because 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 again find no economically meaningful or statistically significant results in any quantiles of their wage and hours distributions.

To summarize, the baseline results in Table 3 show that TSCs have adverse effects on the labor force participation of mothers. These effects are associated with substantial reductions in monthly labor earnings and hourly wages, demonstrating that disruptions of childcare services negatively impact the socioeconomic position of mothers. The results in Table 4 reveal that these effects are driven by mothers at the lower end of the wage and work hours distributions. 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 intra-household gender gap.

4.2 Panel Results

To corroborate our baseline results and shed additional light on the parental labor market implications of TSCs, we take advantage of the rotating panel design of EPH and construct successive panels covering the period 2004-2014. We use this data to study yearly changes in earnings, wages, and work hours. By looking at changes in these outcomes over time, we are adding an additional layer of protection against violations of our estimation assumptions by more conservatively controlling for time invariant unobserved factors. In addition to verifying our baseline results, we take advantage of the panel data structure to analyze labor flow responses to TSCs by conditioning on employment status prior to the TSCs – something that is not possible to do with cross-sectional data. This allows us to disentangle if the labor force participation effects

[26] 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.

that we identify are driven by employed mothers dropping out of the labor force, by nonworking mothers being disincentivized to reenter the labor force, or both.

Table 5 presents the panel data estimates of the effect of strike-induced TSCs on the labor market decisions of parents. Panel A shows the results for all mothers and fathers, Panel B shows the results for mothers and fathers conditional on being employed last year, and Panel C shows the results for mothers and fathers conditional on not being employed last year. Each cell in each panel comes from a separate estimation of equation (11).

The results in Panel A of Table 5 confirm our baseline results in Table 3, showing economically meaningful and statistically significant adverse effects on earnings, employment and labor force participation. While the magnitude of the earnings effect is similar to our baseline result, the effects on employment and labor force participation are larger. Consistent with the baseline results in Table 3, we do not find any effects among fathers for any of the outcomes. The robustness of our results across Tables 3 and 5 are particularly encouraging given the fact that the samples underlying the estimation of the results in Table 5 are almost 70 percent smaller than the samples underlying the estimation of the results in Table 3, such that one would expect larger standard errors and noisier point estimates.

The estimates in Panels B and C of Table 5 show that the negative earnings effect among mothers is driven entirely by those employed at the time of the TSC. The point estimate is much larger than that in Table 3, suggesting that the inclusion of nonworking mothers in our baseline sample may contribute to an attenuation of our results. Another interesting finding in Panel B is that the mothers who were employed at the time of the strike also experience an adverse effect on work hours, something the pooled estimates underlying Table 3 could not identify. Finally, we find that there are negative labor flow effects not only for mothers that are employed at the time of the TSC, but also among mothers that were not employed at the time of the TSC (Panel C). This suggest that the adverse labor supply effects identified in our baseline estimations are driven both by employed parents dropping out of the labor force, and by nonworking parents being disincentivized to reenter the labor force.

4.3 Effect Heterogeneity

In Table 6, we examine if the baseline results discussed above vary systematically across different subgroups, as predicted by the framework outlined in Section 2. Since we do not find any statistically significant or economically meaningful effects among fathers in our baseline specification, we only discuss effect heterogeneity among mothers. Appendix Table A3 show the results for fathers.

In Panel A of Table 6, we explore if single mothers are differentially affected by school disruptions than married mothers. The rationale underlying this hypothesis is that a single mother likely faces larger constraints to labor market exit than married mothers since she is the sole income earner in the family and must be able to secure a subsistence level of income. With respect to the theoretical framework outlined in Section 2, single parents have lower *N (*effective nonlabor income)*,* and are more dependent on *w* (own net wage) to secure a minimum level of consumption on goods, such that the probability of dropping out of the labor force to provide home care is reduced. This theoretical prediction is supported by the results in Panel A: while we see large adverse effects on both the extensive and the intensive margin of labor supply among married mothers, there are no statistically significant results among single mothers.[27]

Panel B examines whether a mother's labor market response to school disruptions vary systematically with her level of education. The theoretical predictions from our model suggest that low-educated mothers (high school or less) face a smaller opportunity cost of dropping out of the labor force (*w* is lower), and the effect among these mothers may therefore be larger. With respect to high-educated mothers (at least some university education), 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 by TSCs (Jaume 2011). The results from this subanalysis suggest that the effects we identify are driven by low-educated mothers. This result is also consistent with our results regarding part-time and full-time employment in Table 3, as well as with the quantile results in Table 4, which suggest that less-skilled mothers with weaker labor market attachment are more affected by school disruptions.

An interesting prediction from Section 2.1 is that the within-family decision on whether to provide home care – and which parent that should provide it – will depend 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 of the parents (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 level, potential experience, year and province). The results from this auxiliary analysis are displayed in Panel C of Table 6.

Looking across Panel C of Table 6, there are economically meaningful and statistically significant adverse labor market effects among wives with lower potential earnings than their

[27] An alternative to analyzing the effects of TSCs separately for mothers and fathers is to aggregate the data to the household level and examine the total effect of TSCs on the family. In Appendix Table A4 we estimates the effects of TSCs on household level outcomes, stratifying the sample by whether it is a two-parent or single-parent household. Consistent with our theoretical prediction and baseline empirical results, we find that the effects are driven by two-parent households. The absolute magnitude of the point estimates of households´ earnings and labor market participation are similar to those of married mothers, indicating that it is married mothers who bear the cost of the TSCs.

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. Conditional on mothers and fathers being able to provide a similar quality of childcare to their child, these results are consistent with the framework outlined in Section 2.1. This result is also 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.

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 6 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 (11) for each of these quartiles. The results from this exercise are shown in Figure 4. Each bar shows the effect as a percentage of the mean for that quartile, and lines extending from the bars show the 95% confidence intervals clustered at the province level. 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 demonstrate that the effect of TSCs on parental labor market outcomes depend directly 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 estimated effect for mothers, such that women are disproportionally affected by TSCs.

In Panel D of Table 6, we examine if the effect of school disruptions on parental labor market decisions differ depending on 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 the child the younger the child is, since younger children may be less able to take care of themselves and stay home alone. However, 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.[28] Looking across the columns, there is strong evidence of adverse parental labor market affects associated with school disruptions

[28] 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.

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 point estimates are statistically significantly different from each other.[29]

Appendix Table A3 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 A3 – while a few of the estimates become significant for a couple of the subgroups, none of the groups appear to be systematically affected by school disruptions. As mentioned above, the one exception concerns fathers who are married to females that have higher predicted earnings than they do (Panel D). These fathers experience both adverse labor market participation as well as earnings effects due to strikes: A father whose child is exposed to ten days of TSCs suffers a decline in hourly wages equivalent to 2.09% of the mean. This supports the idea that parental relative income matters for how households respond to TSCs.

4.4 Effect Persistency

The above results provide clear evidence that TSCs have adverse labor market effects on mothers. To better understand the dynamics of these effects – whether they are transitory or permanent – we estimate a series of regressions based on equation (11) where we allow our treatment window to vary from the past 6 months to the past 36 months in 6-month intervals. Results are shown in Figure 5 for mothers and in Figure 6 for fathers.

The results in Figure 5 provide several interesting insights about the relationship between TSCs and parental labor market behavior. First, the results in Panel A (labor force participation) and Panel B (employment) demonstrate that the labor force participation effects identified in Table 3 are strongest in the immediate aftermath of the strikes, after which they gradually diminish. With a 36 month reference period, there are no longer any effects on these variables, suggesting that the long-term effect on labor force participation is negligible.

Second, in contrast to our baseline results in Table 3, the results in Panel C (unemployment) suggest that there is a positive effect of TSCs on unemployment, but only when using a time window of 18 to 30 months. This result suggests that a fraction of the mothers that decide to voluntarily drop out and provide home care in the aftermath of USDs

[29] It is intuitive to think that the effects of teacher strikes would be attenuated if parents can rely on family or friends to take care of their children in the event of a TSC. Unfortunately, our data do not allow us to explore this hypothesis in detail. Specifically, we can only identify households with and without other family members living under the same roof (for which we find similar effects), but not if these relatives are able to take care of the child (e.g. they might have health or age-related problems) or if parents live close to other relatives and friends, which is relatively common in Argentina.

may experience frictions to reenter the labor market once they decide to return to work. However, the barrier to reentry is not too substantial, and is no longer present when using a time window of 36 months.

Third, Panel D (hourly wages) and Panel E (monthly earnings) of Figure 5 demonstrate that the wage and earnings effects are also strongest in the immediate aftermath of the strikes, after which they gradually diminish. However, in contrast to the labor force participation and employment effects in Panels A and B, the effects on earnings and wages do not disappear when using a time window of 36 months. That the negative wage and earnings effects appear to be more permanent are consistent with the scarring effects of periods of non-employment identified in previous studies (e.g Arulampalam 2001; Eliason and Storrie 2006; Eriksson and Rooth 2014).

5. Robustness and Sensitivity Checks

In this section, we explore evidence on the sensitivity of our baseline 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 robustness and sensitivity of our results for mothers in this section, all of which are shown in Table 7. Results for fathers are shown in Appendix Table A5.

Panel A demonstrates that the results in Table 3 are robust to the exclusion of the individual-level control variables used in our preferred specification (potential experience, number of children and education). This is expected, because even though individual-level covariates can help reduce the error variance of the regressions, only group-level covariates should matter for identification in our specification.

One of the main concerns associated with using teacher strikes as an instrument for school disruptions is that there may be other province-specific secular trends, shocks or policies contemporaneously with teacher strikes that differentially impact the labor market behavior of mothers with and without children in primary school. In Panels B through E of Table 7, we examine this concern in detail by estimating modified versions of equation (11). In Panel B we show the sensitivity of the results to the inclusion of province-specific linear time trends; in Panel C we show results from estimating 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 D we report how the results change when including local labor market controls; and in Panel E we show the sensitivity of the results to the inclusion of the number of public administration strikes in the previous 12 months and its interaction with having a child in primary school.[30] All of the point estimates produced by these alternative specifications are consistent with our baseline results.

Panels F through K show how our estimates change when we alter our analysis sample and relax the sample restrictions described in the data section. In Panel F we include mothers with toddlers (children less than 3 years old). These mothers are excluded from the main analysis since they are more likely to be detached from the labor force, attenuating the results. The results in Panel F of Table 7 confirm the attenuation concern, showing slightly smaller point estimates and somewhat larger standard errors. However, both the labor income as well as the job characteristics outcomes remain statistically significant at conventional levels.

In Panel G we include only mothers with children in primary or secondary school (dropping mothers with children aged 3-5), such that our control group only consists of mothers to children in secondary school. The results in Panel G of Table 7 show that our results are unaffected by the removal of these mothers from the control group – none of the estimates are statistically significantly different from the baseline results.

In Panel H we include individuals subject to more than 30 strikes in any given year. As explained in the data section, these individuals are dropped from our preferred specification to ensure that the results are not driven by outliers. The results in Panel H show that the point estimates become larger, but not statistically significantly larger, when these individuals are included.

In Panel I, we examine if our results change when we exclude parents that had their first child prior to reaching the legal age of 16, acknowledging that this group may be systematically different on unobserved dimensions and not representative of the population at large. This reduces the sample size with approximately 3,000 mothers (2%). This exercise returns point estimates that are slightly larger (and standard errors that are smaller) than our baseline results, consistent with the idea that young teen mothers may be different on unobserved dimensions (their attachment to the labor force, for example, is likely much lower).

In Panel J we eliminate all teachers from our analysis sample (7% of the female sample). The rationale underlying this auxiliary analysis is that teachers 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

[30] The local labor market controls consist of average wage, unemployment rate and family per capita family income in the province during the twelve months leading up to the interview. 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. The specification that incorporates province and time fixed effects interacted with having a child in primary school is incredibly 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 attenuates the results slightly (Panel E of Table 7). 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 baseline estimates. This greatly improves the causal interpretation of our baseline results.

these individuals in our main specification causes an attenuation of the estimated effect. The results in Panel J are consistent with this belief, showing slightly larger point estimates, and smaller standard errors, compared to the baseline results in Table 3.

In Panel K we look at the effect on adult non-mothers (aged 18 to 50) that live in households with children of primary school age. This subanalysis is carried out in order to investigate whether there is any evidence of parents sharing the increased childrearing responsibility caused by TSCs with non-parent adults in the household. The results in Panel K suggest that this is not the case, showing no statistically significant effects on the labor market outcomes of non-mother adult females in the households.

The last Panel of Table 7 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 in Panel L are statistically significant; the results are consistent with the assumption that the strikes are uncorrelated with province-specific outcome trends.

The results produced so far have relied on the dose-response triple difference model discussed in the empirical methods section. An alternative estimation strategy is to restrict the sample to parents with children in primary school and estimate a dose-response difference-in-difference model. 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 difference-in-difference model. To this end, we restrict the sample to parents with children in primary school and estimate the following model (variables are defined as before)

$$Y_{ipt(q)} = \gamma_0 + \alpha_1 Str_{pt(q)-1}\, \gamma X_{ipt(q)-1} + \rho_p + \tau_{t(q)} + \varepsilon_{ipt(q)}. \quad (12)$$

The results from this exercise are shown in Panel A of Table 8. Looking across the columns, the results obtained from this alternative specification are consistent with the results in Table 3. This suggests that the potential bias from province-specific secular trends, policies or shocks correlated with both teacher strikes and the outcomes is small.

In addition to estimating equation (12) for our treatment group (parents with children in primary school), we also estimate equation (12) for our control group. Since individuals in our control group were not affected by the TSCs, this provides us with a nice 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 8. Looking across the panel, none of the effects are statistically significantly different from zero, providing additional support for a causal interpretation of our findings.

6. Private School Enrollment

As discussed in Section 2, parents can respond to TSCs in three different ways: (1) work and purchase private care, (2) work and leave the child at home, or (3) drop out of the labor force and provide home care. While the focus of this paper is on (3), the EPH survey contains information on whether the parent's child attend public or private primary school, and this allows us to also provide suggestive evidence on (1).

Table 9 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 (11) using children as unit of observation, and we add controls sequentially across columns. In column (i) we use the same controls as in our baseline 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 12 months).

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 twelve months is associated with a 1.8 percentage point reduction in the probability of a child attending a public school. This is an important finding, suggesting that the availability of alternative childcare options may serve to mute the impact of TSCs on parental labor market behavior. This implies that the effects identified in Table 3 may have been larger absent the existence of private school.

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 particularly interesting given the lack of labor market effects among this subgroup of parents in Table 6. This is consistent with the discussion in Section 2 that single-parents are less likely to drop out of the labor force due to dependence on w to secure a subsistence level of income, but may still respond by moving the child to private school.

To better understand which households move their children to private school in response to TSCs, Figure 7 presents results stratified by quartile of the family earnings distribution in *t-1* (using the panel data sample). Three interesting 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-income families already have enrolled their children in private school. 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 income distribution as well. While it is likely that these children are sent to relatively worse private schools compared to children in households that are in the middle of the family earnings distributions, this is unfortunately nothing we can explore. Third, Argentina has been experiencing a surge in socio-economic school segregation between public and private schools since the early 1990s (Gasparini, Jaume, Serio y Vazquez. 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.

To better understand the nature of the private school effects, we estimate a series of regressions based on equation (11) where we allow our treatment window to vary from the past 6 months to the past 36 months in 6-month intervals. Results from this exercise are shown in Panels A (full sample) and B (panel sample) of Figure 8. The figure demonstrates that the TSC effect on private school enrollment is strongest in the immediate aftermath of the strikes, after which it gradually diminishes. However, even with a 36-month reference period we observe statistically significant effects, suggesting that this effect is more permanent than the labor force participation effects explored in Figure 6. This effect is perhaps unsurprising given that parents may want to avoid moving their children back and forth between different schools over time.

To further explore the nature of the TSC effects on private school enrollment, Panels C though F of Figure 8 show the same results stratified by quantile of the family income distribution. The figure reveals that the effects are more persistent among children from middle-income families, reinforcing the idea that strike-induced TSCs may have contributed to increase school segregation.

7. Discussion and Conclusion

Temporary school closures are common features of education systems across the globe, and a relatively large literature has investigated how TSCs impact the short- and long-run education and labor market behavior of students. A neglected but equally important question relates to how TSCs affect the labor market behavior of parents. This is the first paper to present a detailed analysis on this topic. First, we provide a framework for thinking about the decision problem faced by parents in the event of a disruption to their children's school services. Second, we exploit a novel identification strategy coupled with a rich and newly created data set to test the predictions of the model and examine the reduced-form effect of school disruptions on parental labor market decisions. To obtain plausibly exogenous variation in TSCs, we use variation in teacher strikes within and across provinces over time between parents with and without children in primary school.

Results indicate that school disruptions 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 TSCs experiences a decline in earnings equivalent to 2.92% of the mean. Through auxiliary analysis we find that these effects are predominantly driven by low-skilled mothers at the margin of employment, such that TSCs disproportionally hurt an already vulnerable subgroup of mothers. A back-of-the-envelope calculation suggests that the average mother would be willing to forego more than 1.6 months of earnings in order to ensure that there are no TSCs while her child is in primary school. 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 TSCs suffers a decline in his hourly wage equivalent to 2.09% 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 TSCs. These results thus imply that interruptions to core childcare services may exacerbate existing labor market and intra-household gender inequality by disproportionately affecting mothers.

Our findings illustrate the importance of providing stable childcare options to mothers in order to maximize their ability to participate in the labor market and to prevent an augmentation of labor market and intra-household gender inequality. While the effect of TSCs on student outcomes can be reduced by offering make-up days at the end of the semester, this type of policy intervention would be unsuccessful in reducing the impact of TSCs on parental labor market behavior. An increased awareness of how TSCs affect parental labor market outcomes is therefore imperative for guiding the development of future childcare policies and establishing policy responses to TSCs.

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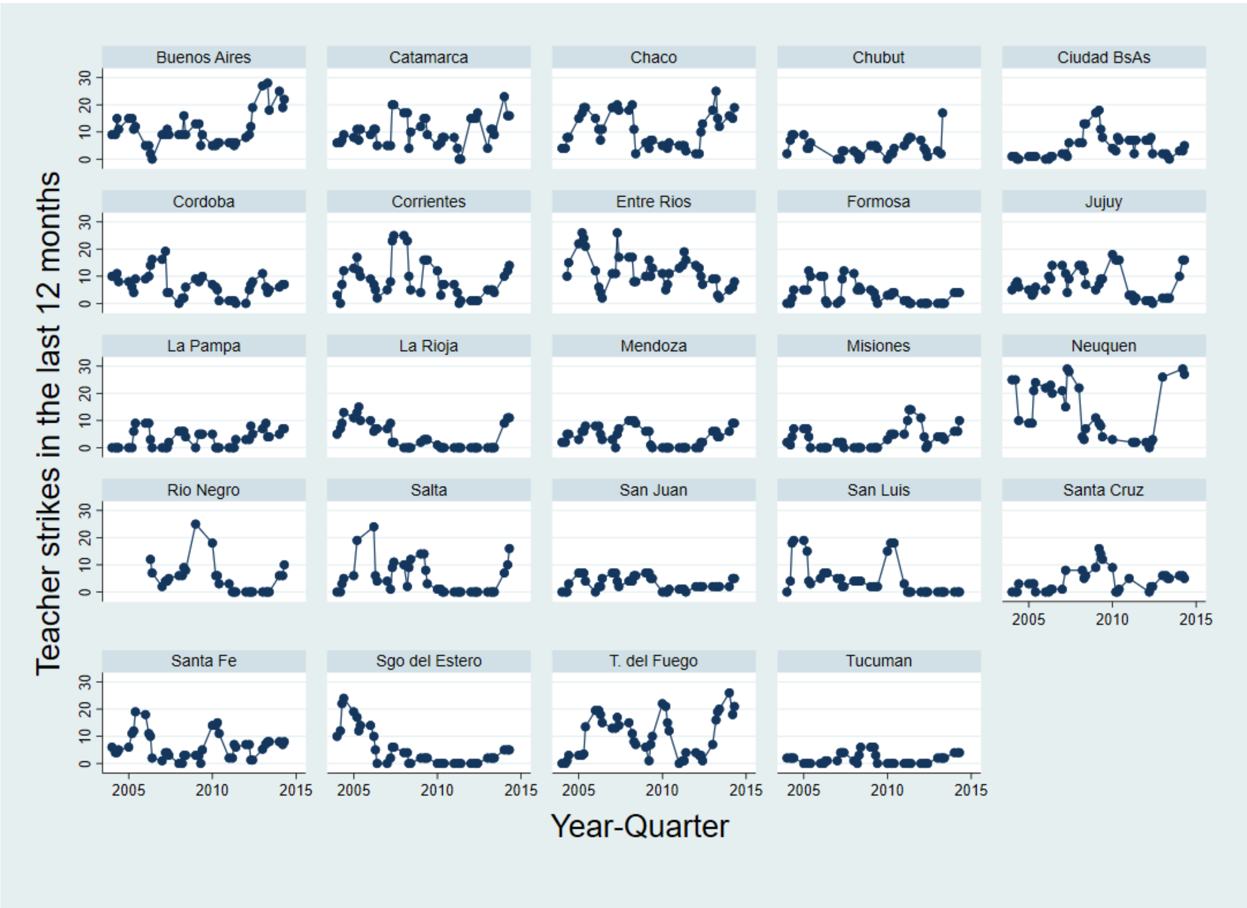
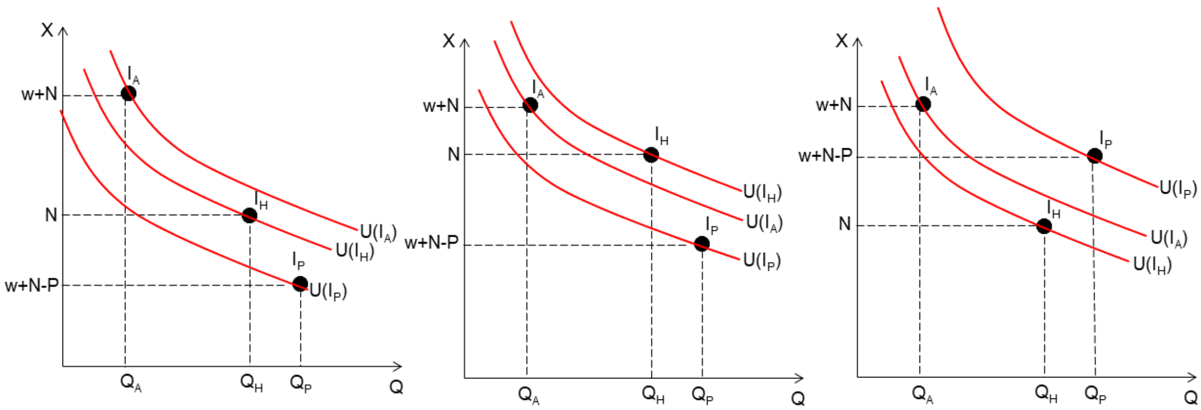
Figure 1: Variation in teacher strikes across provinces and over time



Notes: The figure shows the total number of days of strike-induced school disruptions across province and over time. Authors' calculations from historic reports on the Argentine economy published by Consejo Técnico de Inversiones (2003-2013) and collected by Jaume and Willén (2018).

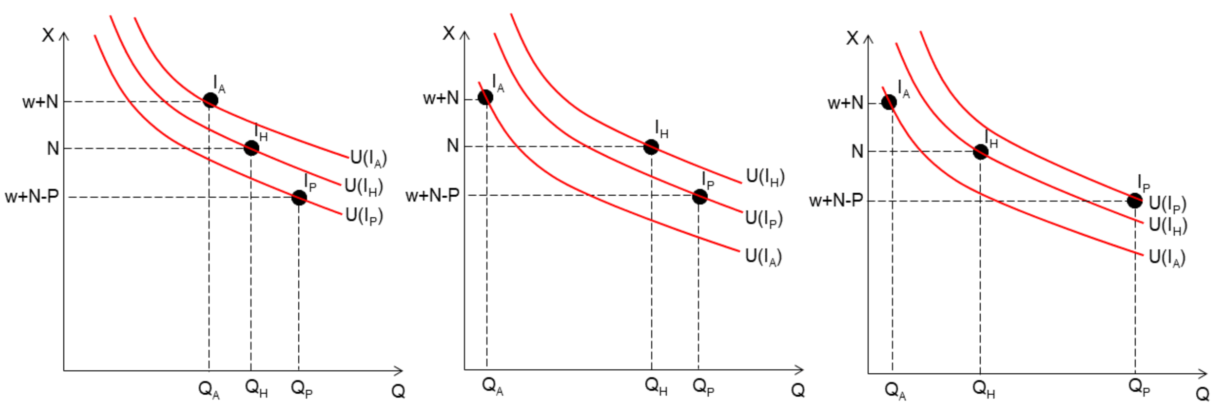
Figure 2: Illustration of possible parental responses to school distruptions

(a) Varying the monetary parameters



(a) High wage, high private cost (b) High nonlabor income, high private cost (c) High wage, low private cost

(b) Varying the relative quality of the different care options

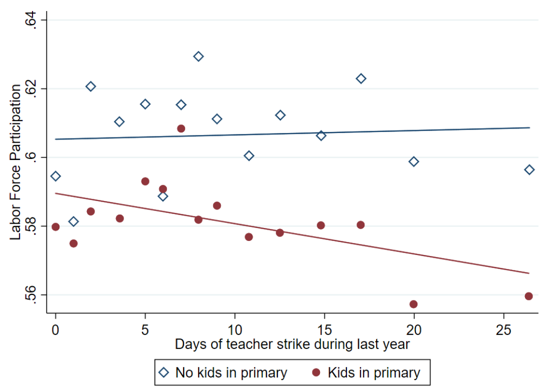


(a) High relative quality of home alone (b) High relative quality of home care (c) High relative quality of private care

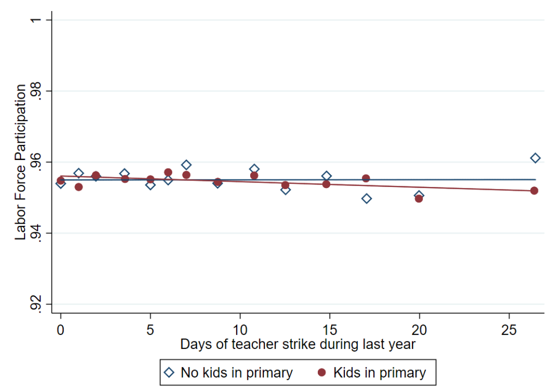
Notes: Panel (a) shows different possible parental responses to school disruptions when the relative quality of the different care options (leaving the kid alone at home (I_A), providing home care (I_H), and purchasing alternative private care (I_P)) are held constant, with different configurations of the individual's wage (w), non-labor in income (N), and cost of private care (P). Panel (b) displays different possible parental responses to school disruptions when the monetary parameters are held constant but the relative quality of the different care options are allowed to vary.

Figure 3: Correlation between labor force participation, wages and school disruptions

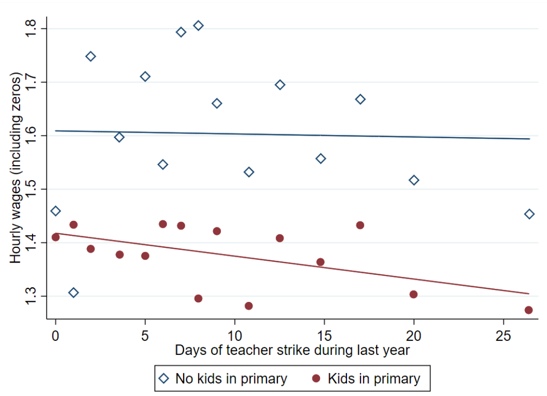
(a) Labor force participation (mothers)



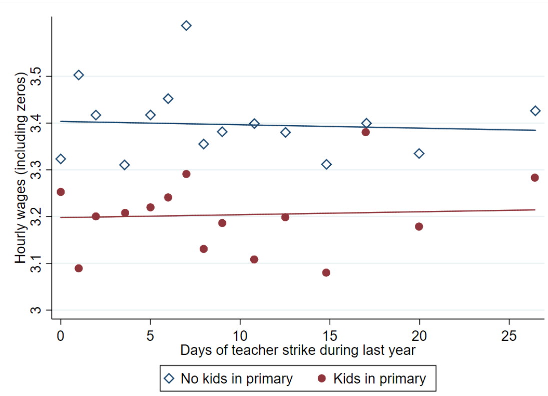
(b) Labor force participation (fathers)



(c) Hourly wages (mothers)



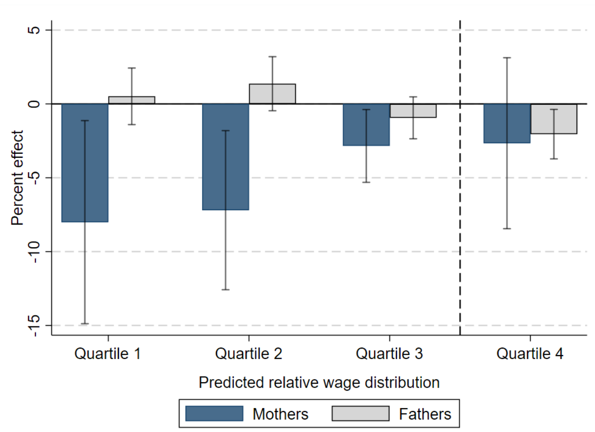
(d) Hourly wages (fathers)



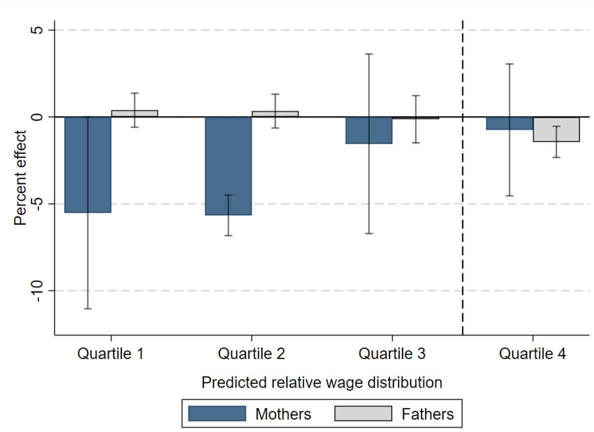
Notes: The figure shows binned scatterplots of the correlation between school disruptions in the past 12 months and labor force participation (panels A and B) and hourly wages (panels C and D). The horizontal axis shows the number of school disruption due to teacher strikes during the past 12 months, which varies at the year-quarter-province level. The vertical axis shows the average labor force participation (panels A and B) and the average hourly wage (panels C and D) for each year-quarter-province, controlling for province, quarter, and year fixed effects. The data is divided into 20 equally sized bins based on the number of days of school disruptions during the past 12 months.

Figure 4: Intra-household effects by quantile of the predicted relative income disribution

(a) Hourly Wage

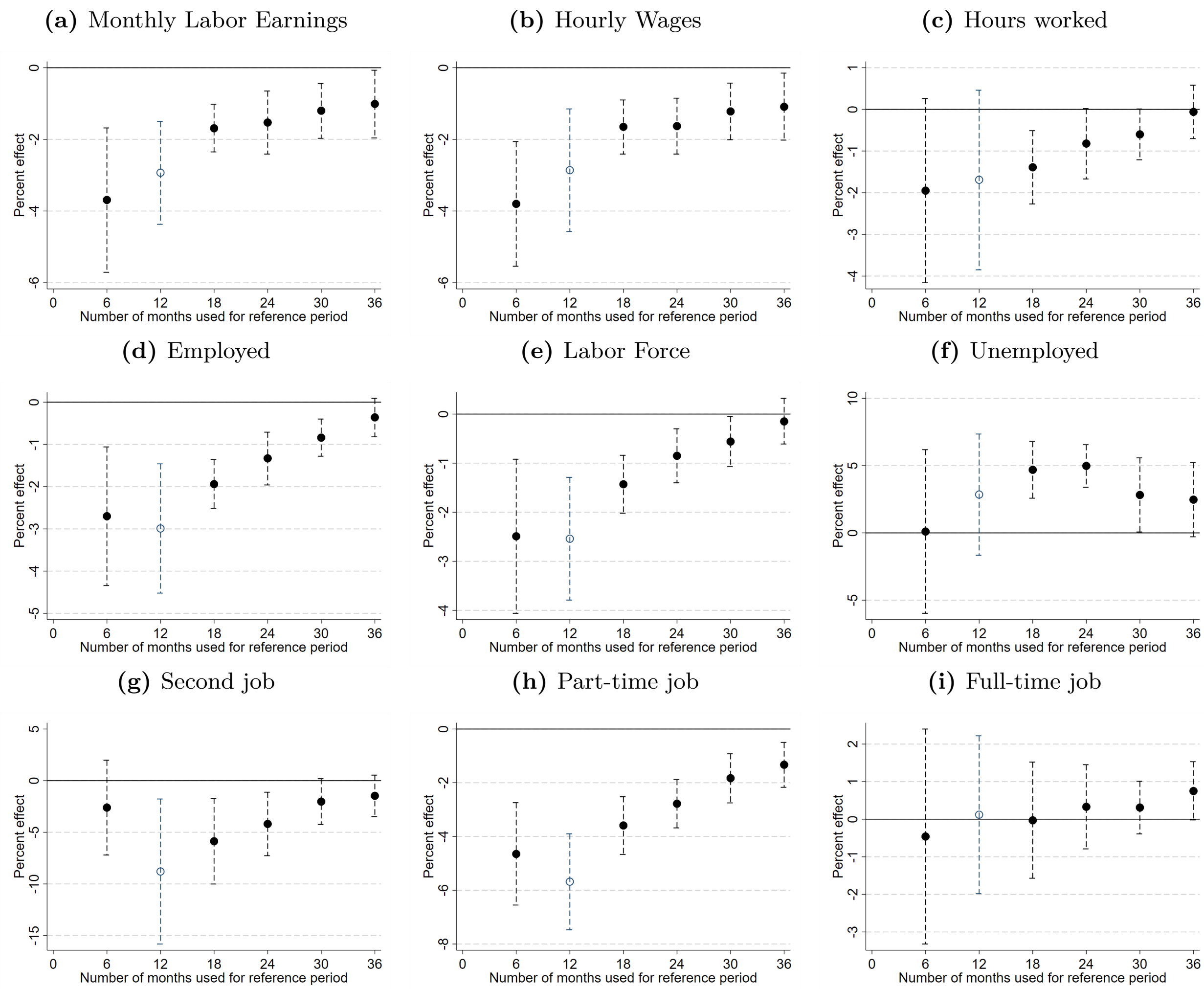


(b) Employed



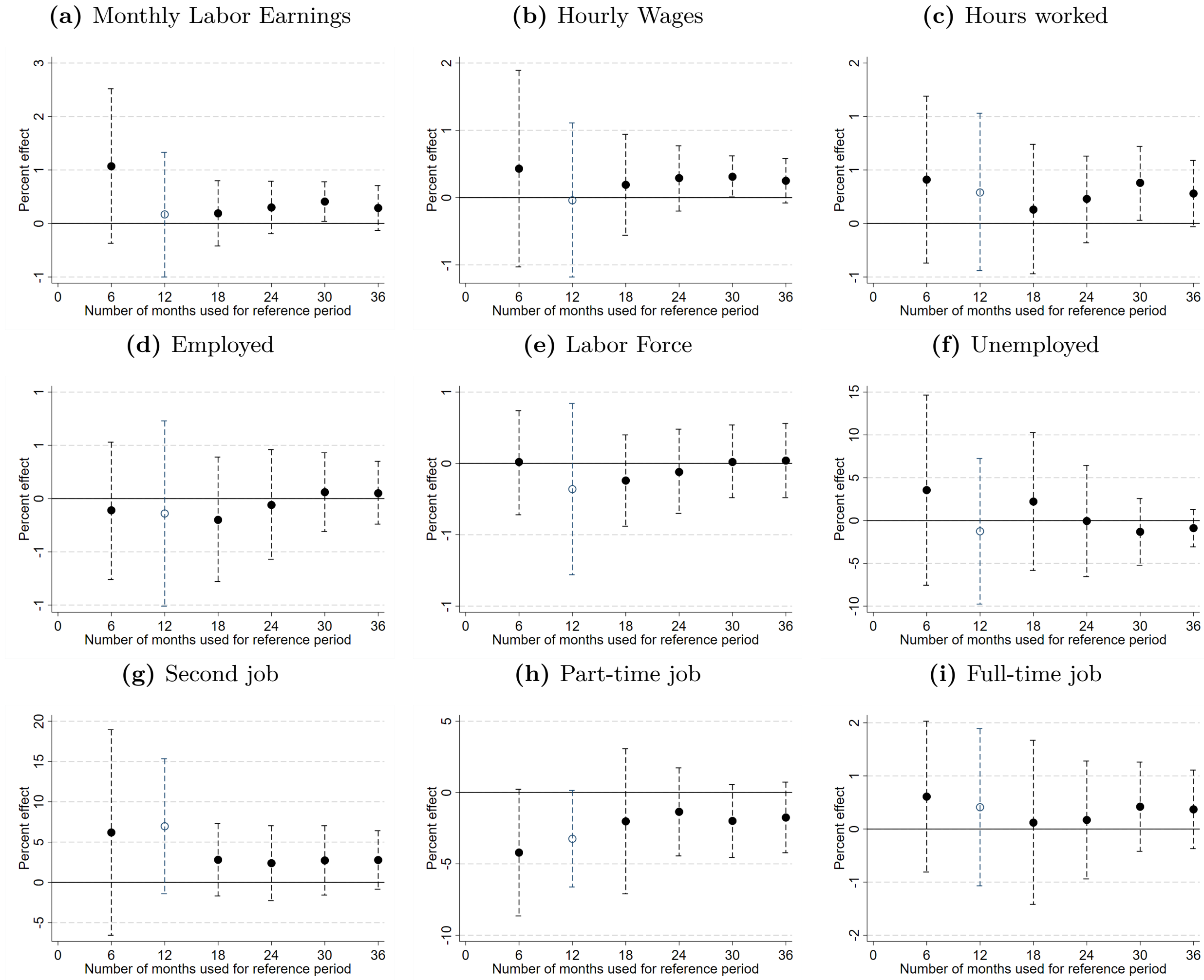
Notes: Authors' estimation of equation (11) using the rotating 2004-2014 EPH panel on 18-50 year old parents, estimated seperately for mothers and fathers by quartile of the predicted relative earnings of mothers with respect to fathers (potential earnings are estimated using a standard gender-specific Mincer equation controlling for potential experience, education, calendar year and region). 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 measured the number of strike-induced school disruptions that took place during the past 12 months (measured 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 12 months. The coefficient measures the intent-to-treat effect of strike-induced school disruptions during the past twelve months 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 5: Effects of school disruptions on mothers for different reference periods



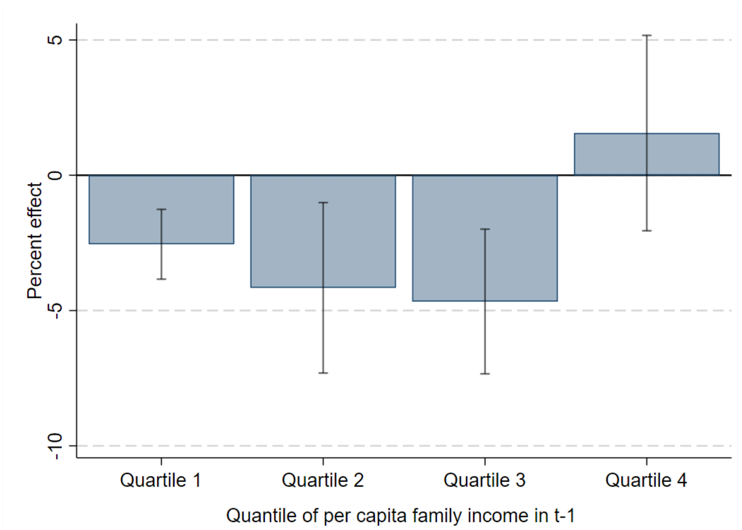
Notes: Authors' estimation of equation (11) using 2004-2014 EPH data on 18-50 year old mothers. Regressions include province and year-quarter fixed effects as well as controls for potential experience, potential experience squared, education (indicator variables of incomplete primary, complete primary, 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 6: Effects of school disruptions on fathers for different reference periods



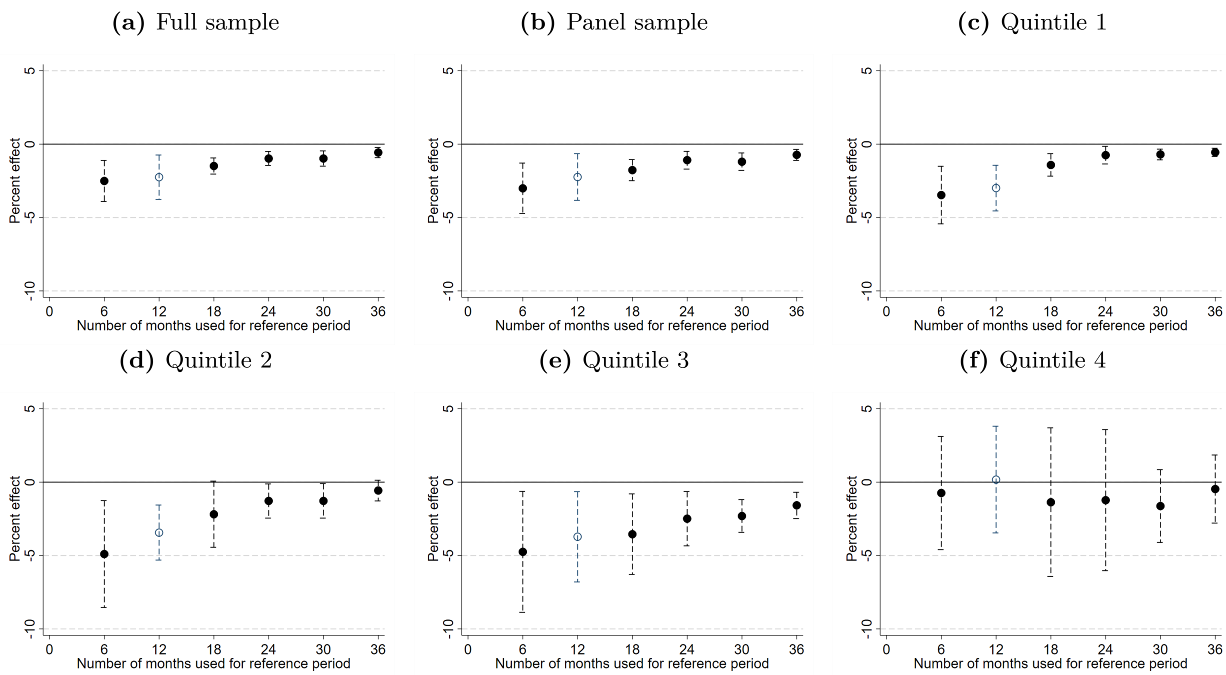
Notes: Authors' estimation of equation (11) using 2004-2014 EPH data on 18-50 year old fathers. Regressions include province and year-quarter fixed effects as well as controls for potential experience, potential experience squared, education (indicator variables of incomplete primary, complete primary, 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 7: Effect of school disruption on public school enrollment by quintile of per capita family income in t-1



Notes: Authors' estimation of equation (11) using the rotating 2004-2014 EPH panel on 18-50 year old parents, estimated separately by quartile of the per capita family income in t-1. 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 12 months (measured in tens of days). The figures 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 12 months. The coefficient measures the intent-to-treat effect of strike-induced school disruptions during the past twelve months 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.

Figure 8: Effects of school disruptions on public school enrollment for different reference periods



Notes: Authors' estimation of equation (11) using 2004-2014 EPH full data and EPH panel data on 6-17 year old children, estimated separately for the full EPH data, the EPH panel data, and by quartile of the per capita family income in t-1. 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 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 attending primary school 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 likelihood of attending a public school. The lines extending from the point estimates 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	**Mean**	**Total**
Buenos Aires	9	15	5	9	9	9	9	6	6	19	18	10.4	114
Catamarca	6	8	9	5	20	10	9	8	0	17	9	9.2	101
Chaco	4	15	15	19	18	2	7	6	3	13	12	10.4	114
Chubut	2	9	37	0	3	1	4	4	8	1	78	13.4	147
Ciudad BsAs	1	1	0	2	6	13	8	7	7	2	0	4.3	47
Cordoba	10	8	9	16.2	4	6	10	1	0	8	5	7.0	77
Corrientes	3	13	9	5	25	5	16	7	1	1	4	8.1	89
Entre Rios	64	22	12	11	17	8	13	11	16	7	2	16.6	183
Formosa	0	5	10	0	12	5	0	4	0	0	0	3.3	36
Jujuy	5	5	5	14	9	7	9	16	2	0	2	6.7	74
La Pampa	0	0	9	0	2	4	5	0	3	5	4	2.9	32
La Rioja	5	11	10	7	2	0	3	0	0	0	0	3.5	38
Mendoza	2	3	8	3	7	9	0	0	0	2	4	3.5	38
Misiones	2	7	0	2	0	0	0	5	14	1	3	3.1	34
Neuquen	25	9	22	21	28	7	4	48	2	3	68	21.5	237
Rio Negro	2	5	16	2	5	8	38	3	0	0	0	7.2	79
Salta	0	6	33	4	11	12	3	0	0	0	0	6.3	69
San Juan	0	7	0	7	2	6	5	1	0	2	2	2.9	32
San Luis	0	19	5	5	2	4	2	18	0	0	0	5.0	55
Santa Cruz	0	3	0	1	45	6	12	1	63	2	5	12.5	138
Santa Fe	6	6	18	1	3	3	5	11	6	1.25	8	6.2	68
Sgo del Estero	10	19	14	0	6	0	2	0	0	0	2	4.8	53
T. del Fuego	0	3	19.5	13	14	7	10	12	4	1	20	9.4	104
Tucuman	2	0	0	1	4	6	0	0	0	0	2	1.4	15
Mean	6.6	8.3	11.1	6.2	10.6	5.8	7.3	7.0	5.6	3.6	10.3	**7.5**	**82.5**
Total	158	199	266	148	254	138	174	169	135	85	248	-	**1,974**

Notes: Authors' calculations based on historic reports on the Argentine economy published by Consejo Técnico de Inversiones (CTI) and collected by Jaume and Willén (2018).

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	
Panel A: Cross-section data						
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. Labor market outcomes						
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
Panel B: One-year panel data						
Observations	37,391	20,265	67,973	26,065	12,964	74,336
i. Employment dynamics						
Change in income	15.1	15.2	31.0	11.3	14.7	35.5
Change in wage	0.11	0.09	0.22	0.13	0.14	0.27
Change in hours worked	0.29	-0.17	1.13	-0.39	-1.24	1.52
ii. Labor flows						
Employed to not employed	0.09	0.08	0.12	0.03	0.03	0.12
Not employed to employed	0.09	0.08	0.09	0.03	0.03	0.07
In LF to not in LF	0.10	0.09	0.09	0.01	0.01	0.05
Not in LF to in LF	0.09	0.09	0.11	0.01	0.01	0.08

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. Change in income and wages are defined in 2011 PPP dollars and correspond to the absolute difference in the 1-year panel data. Change in work hours contains the absolute difference in hours worked in the one-year panel data and includes zeros at initial and final interview. Labor flows are expressed as shares of the total sample.

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									
Disrupted Schooling	-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									
Disrupted Schooling	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 (11) 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 12 months (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 12 months. The coefficient measures the intent-to-treat effect of strike-induced school disruptions during the past twelve months 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: Distributional effects of school disruptions on parents' labor 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 (11) 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 12 months (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 12 months. The coefficient measures the intent-to-treat effect of strike-induced school disruptions during the past twelve months 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 5: Panel regressions

	Absolute change in			Labor flows (status in year t)	
	Earnings	Wages	Hours	Employed	In Labor Force
	(1)	(2)	(3)	(4)	(5)
Panel A: All parents					
Mothers	-10.433*	0.027	-0.256	-0.033***	-0.025***
(N=55,682)	(5.659)	(0.043)	(0.228)	(0.009)	(0.008)
% Effect	-3.33	1.05	-1.32	-5.71	-4.14
Fathers	-15.180	-0.104	-0.491	0.001	-0.005
(N=38,840)	(10.233)	(0.074)	(0.640)	(0.007)	(0.005)
% Effect	-2.02	-2.36	-1.06	0.12	-0.49
Panel B: Parents employed in t-1					
Mothers	-26.928***	-0.007	-1.044***	-0.020***	-0.013***
(N=31,050)	(8.277)	(0.056)	(0.370)	(0.005)	(0.004)
% Effect	-5.19	-0.17	-3.46	-2.38	-1.50
Fathers	-16.649	-0.117	-0.438	0.004	-0.000
(N=37,115)	(12.222)	(0.087)	(0.545)	(0.004)	(0.003)
% Effect	-2.15	-2.58	-0.93	0.40	-0.05
Panel C: Parents not-employed in t-1					
Mothers	0.463	-0.041	0.111	-0.016***	-0.010**
(N=24,627)	(3.883)	(0.028)	(0.260)	(0.004)	(0.004)
% Effect	0.80	-6.88	1.91	-7.65	-3.75
Fathers	9.636	0.080	-1.484	-0.051	-0.096**
(N=1,957)	(25.805)	(0.188)	(2.671)	(0.065)	(0.035)
% Effect	3.08	3.73	-5.47	-7.67	-12.27

Notes: Authors' estimation of equation (11) using the rotating 2004-2014 EPH panel 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 12 months (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 12 months. The coefficient measures the intent-to-treat effect of strike-induced school disruptions during the past twelve months 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: 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=135315)	(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	-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 (11) 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 and the number of strike-induced school disruptions that took place during the past 12 months. The coefficient measures the intent-to-treat effect of strike-induced school disruptions during the past twelve months 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 7: 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: Excluding individual controls						
Disrupted Schooling	-8.015***	-0.061***	-0.016***	-0.014***	-0.006**	-0.017***
(N= 168,362)	(2.091)	(0.020)	(0.004)	(0.004)	(0.003)	(0.003)
% Effect	-2.42	-2.41	-2.81	-2.33	-8.38	-5.75
Panel B: Including province-specific linear time trends						
Disrupted Schooling	-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 C: Including all fixed effects for DDD						
Disrupted Schooling	-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 D: Controlling for local labor markets at t-1						
Disrupted Schooling	-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

Notes: Authors' estimation of equation (11) 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 and the number of strike-induced school disruptions that took place during the past 12 months. The coefficient measures the intent-to-treat effect of strike-induced school disruptions during the past twelve months on current parental labor market outcomes. Panel A excludes individual controls (age, education, experience, experience squared and number of children). Panel B includes province-specific linear trends. Panel C 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 D controls for unemployment, average wages, and average per capita family income at the province level during the past 12 months. Panel E controls for the number public administration strike days that took place in the previous 12 months and its interaction with having a child in primary school. Panel F includes families with toddlers (less than 2 years old). Panel G drops parents from the control group that only had non-school aged children. Panel H includes the top 1% of observations in terms of school disruptions during the past 12 months (more than 30 dyas). Panel I drops parents who had the first child when they were younger than the legal age (16 years old). Panel J drops parents that work as teachers. Panel K estimates the effect only for non-parent females in the household. Panel L shows results from a placebo test in which treatment has been reassigned from t-1 (strikes in the past 12 months) to t+1 (strikes in the next 12 months). 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: Robustness and sensitivity analysis, mothers (continue)

	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 E: Controlling for public administration strikes						
Disrupted Schooling	-10.971***	-0.075***	-0.015**	-0.013**	-0.006**	-0.019***
(N= 146,910)	(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 F: 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 G: Keeping parents with kids in primary or seconday 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
Panel H: 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 I: Drop parents that get first kid under legal age of 16						
Disrupted Schooling	-10.172***	-0.076***	-0.017***	-0.016***	-0.007**	-0.018***
(N= 165,257)	(2.439)	(0.020)	(0.005)	(0.005)	(0.003)	(0.003)
% Effect	-3.05	-2.98	-2.99	-2.56	-8.39	-5.88
Panel J: Drop parents who are teachers						
Disrupted Schooling	-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 K: Non-parents members of the households aged 13-50						
Females	5.888	0.033	0.001	0.000	-0.001	-0.001
(N= 294,412)	(9.045)	(0.049)	(0.010)	(0.010)	(0.001)	(0.002)
% Effect	2.17	1.66	0.14	-0.02	-1.70	-0.56
Panel L: Reassigning treatment from t-1 to t+1						
Disrupted Schooling	0.002	-3.176	-0.023	0.002	0.000	-0.001
(N= 151,483)	(0.004)	(3.279)	(0.019)	(0.004)	(0.004)	(0.002)
% Effect	0.31	-0.96	-0.91	0.31	0.04	-1.92

Table 8: Dose-response difference-in differences estimations

	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 kids 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 kids 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 (12) 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 12 months. The coefficient measures the intent-to-treat effect of strike-induced school disruptions during the past twelve months 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 9: School disruptions and public school enrollment

	(1)	(2)	(3)
Panel A: all observations			
i. all	-0.018***	-0.018***	-0.018**
(N=436,864)	(0.006)	(0.006)	(0.007)
% Effect	-2.25	-2.26	-2.32
ii. Males	-0.021***	-0.021***	-0.021***
(N=222,460)	(0.007)	(0.007)	(0.008)
% Effect	-2.65	-2.66	-2.68
iii. Females	-0.013**	-0.013**	-0.014**
(N=214,404)	(0.006)	(0.006)	(0.007)
% Effect	-1.71	-1.70	-1.79
Panel B: By number of parents			
i. Two-parent household	-0.018***	-0.018***	-0.018**
(N=304,494)	(0.006)	(0.006)	(0.007)
% Effect	-2.40	-2.42	-2.37
ii. Single-parent household	-0.017**	-0.016**	-0.017***
(N=84,008)	(0.006)	(0.006)	(0.006)
% Effect	-1.96	-1.93	-2.06
Province-specific linear time trends		X	X
Local labor market controls in t-1			X

Notes: Authors' estimation of equation (11) using 2004-2014 EPH data on 18-50 year old parents. The dependent variable is a dummy variable equal to one if the school that the child attends is public. 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 12 months (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 12 months. The coefficient measures the intent-to-treat effect of strike-induced school disruptions during the past twelve months 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: Not for publication.

Table A1: Descriptive characteristics, matched and unmatched parents

	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 2005-2014 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. Change in income and wages are defined in 2011 PPP dollars and correspond to the absolute difference in the 1-year panel data. Change in work hours contains the absolute difference in hours worked in the one-year panel data and includes zeros at initial and final interview. Labor flows are expressed as shares of the total sample.

Table A2: P-values from wild cluster bootstrap

	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									
Disrupted Schooling (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									
Disrupted Schooling (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 (11) 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 12 months (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 12 months. The coefficient measures the intent-to-treat effect of strike-induced school disruptions during the past twelve months 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 A3: 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 (11) 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 12 months (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 12 months. The coefficient measures the intent-to-treat effect of strike-induced school disruptions during the past twelve months 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 standar 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 A4: Effects of school disruptions, household-level analysis

	Parents' labor income		Parent's labor market participation			
	Total earnings (1)	Total wage (2)	All parents employed (3)	All parents in LF (4)	One-parent unemployed (5)	Total hours (6)
Panel A: All households						
Disrupted Schooling	-3.578	-0.033	-0.019***	-0.017***	0.001	-0.033
(N= 178,302)	(7.240)	(0.029)	(0.005)	(0.006)	(0.002)	(0.029)
% Effect	-0.39	-0.55	-3.11	-3.09	1.37	-0.55
Panel B: Two-parent households						
Disrupted Schooling	-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
Panel C: Single-parent households						
Disrupted Schooling	3.273	0.084**	-0.003	-0.009	0.006	-0.761
(N=35,772)	(7.177)	(0.034)	(0.004)	(0.007)	(0.005)	(0.517)
% Effect	0.70	2.56	-0.38	-1.17	10.31	-2.54

Notes: Authors' estimation of equation (11) 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 12 months (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 12 months. The coefficient measures the intent-to-treat effect of strike-induced school disruptions during the past twelve months 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 fathers with wives that have lower or higher potential earnings than they have (potential earnings are estimated using standar gender-specific Mincer equations controlling for potential experience, education, year, and region). Panel D looks seperately at fathers with the youngest child in grades 0-3 during the previous year and fathers 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 A5: 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: Excluding individual controls						
Disrupted Schooling	5.421	0.025	-0.002	-0.002	0.004	-0.005*
(N= 168,362)	(5.463)	(0.028)	(0.004)	(0.003)	(0.003)	(0.003)
% Effect	0.72	0.61	-0.19	-0.20	5.97	-3.65
Panel B: Including province-specific linear time trends						
Disrupted Schooling	0.382	-0.004	-0.002	-0.002	0.004	-0.004*
(N= 168,362)	(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 C: Including all fixed effects for DDD						
Disrupted Schooling	1.049	0.017	0.001	0.000	0.006	-0.001
(N= 168,362)	(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 D: Controlling for local labor markets at t-1						
Disrupted Schooling	2.886	0.006	-0.003	-0.003	0.005**	-0.004
(N= 146,910)	(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

Notes: Authors' estimation of equation (11) 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 12 months (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 12 months. The coefficient measures the intent-to-treat effect of strike-induced school disruptions during the past twelve months on current parental labor market outcomes. Panel A excludes individual controls (age, education, experience, experience squared and number of children). Panel B includes province-specific linear trends. Panel C 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 D controls for unemployment, average wages, and average per capita family income at the province level during the past 12 months. Panel E controls for the number public administration strike days that took place in the previous 12 months and its interaction with having a child in primary school. Panel F includes families with toddlers (less than 2 years old). Panel G drops parents from the control group that only had non-school aged children. Panel H includes the top 1% of observations in terms of school disruptions during the past 12 months (more than 30 dyas). Panel I drops parents who had the first child when they were younger than the legal age (16 years old). Panel J drops parents that work as teachers. Panel K estimates the effect only for non-parent males in the household. Panel L shows results from a placebo test in which treatment has been reassigned from t-1 (strikes in the past 12 months) to t+1 (strikes in the next 12 months). 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 A5: Robustness and sensitivity analysis, fathers (continue)

	Labor income		Labor market participation		Labor market characteristics	
	Darnings	Wages	Dmployed	Labor Force	Second Job	Part-time job
	(1)	(2)	(3)	(4)	(6)	(7)
Panel E: Controlling for public administration strikes						
Disrupted Schooling	3.633	0.002	-0.001	-0.002	0.006**	-0.005**
(N= 146,910)	(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 F: Including families with toddlers (age younger kid <3)						
Disrupted Schooling	-0.458	0.011	0.000	0.000	-0.001	-0.003
(N= 296,290)	(3.388)	(0.034)	(0.003)	(0.001)	(0.001)	(0.002)
% Effect	-0.06	0.25	0.02	0.03	-1.10	-1.80
Panel G: Keeping parents with kids in primary or seconday school						
Disrupted Schooling	2.352	0.01	-0.001	-0.003	0.003	-0.003
(N= 152,441)	(5.003)	(0.027)	(0.004)	(0.003)	(0.002)	(0.005)
% Effect	0.31	0.25	-0.12	-0.27	3.75	-2.33
Panel H: Include outliers (top 1%)						
Disrupted Schooling	1.758	0.011	-0.002	-0.001	0.003	-0.003
(N= 173,455)	(2.940)	(0.014)	(0.003)	(0.002)	(0.003)	(0.002)
% Effect	0.23	0.27	-0.22	-0.13	4.95	-1.96
Panel I: Drop parents that get first kid under legal age of 16						
Disrupted Schooling	0.096	-0.001	-0.001	-0.002	0.005	-0.004
(N= 165,257)	(4.789)	(0.026)	(0.004)	(0.003)	(0.003)	(0.002)
% Effect	0.01	-0.01	-0.15	-0.21	6.90	-2.69
Panel J: Drop parents who are teachers						
Disrupted Schooling	-0.325	0.008	-0.002	-0.002	0.005	-0.004
(N= 154,990)	(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 K: Non-parents members of the households aged 13-50						
Males	-2.898	-0.052*	-0.012***	-0.011**	0.002*	-0.006**
(N= 294,412)	(5.593)	(0.030)	(0.003)	(0.005)	(0.001)	(0.003)
% Effect	-0.71	-2.10	-1.84	-1.52	6.97	-4.03
Panel L: Reassigning treatment from t-1 to t+1						
Disrupted Schooling	0.000	3.056	0.018	0.000	-0.001	-0.001
(N= 151,483)	(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