American Dad: Families, Fathers and Childcare Shocks

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Abstract

Children, particularly when young, require care. Parents often outsource childcare but childcare shocks, such as illness, can return childcare responsibilities back to parents unexpectedly and may cause income loss. Teachers' strikes are an exogenous childcare shock that shift childcare to parents. A teachers' strike-day reduces family income by at least one quarter of a day's income suggesting families are unable to insure perfectly against childcare shocks. For the average and median family, fathers face higher income costs from teachers' strikes than mothers. Finally, fathers and mothers are also more likely to face income costs when sons are affected than daughters.

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1 Introduction

Children, particularly when young, require care. Parents, either by choice or by legal obligation, provide childcare through their own labor or by outsourcing childcare services. For example, many children attend school which shifts daytime monitoring of school-aged children from parents to teachers. Yet families can face childcare shocks which unexpectedly return childcare responsibilities back to parents. One example is illness. Heymann *et al.* (1996) use data from the National Medical Expenditure Survey to document the family-illness burden borne by parents. They find that roughly 1 in 4 families face more than three weeks of family illness burden in a given year and roughly 1 in 3 face a burden greater than two weeks. To the extent that childcare shocks are uninsured parents may face non-trivial income risk that is different than that faced by non-parents. Since childcare can be provided by parents individually, the within-household allocation of uninsured childcare shocks may be also unequal. How then do families respond to childcare shocks?

To identify the effect of childcare shocks, I use a natural experiment that shifts childcare to parents with school-aged children – teachers' strikes. A teachers' strike shifts childcare from the teacher and school to the family. Teacher strikes have several advantages as an identification scheme. Unlike school absences caused by illness, there is no unobserved heterogeneity in terms of the threshold at which a family determines a child is too ill to attend school. This is advantageous since there is less reason to believe that an omitted regressor, such as parental compassion, may be confounding the regression results. A second advantage is that a teachers strike occurs independently of family-level characteristics such as the number of children or the working status of the parents. Consequently the effect of a teachers' strike can be estimated without concern over selection effects across families within a treated MSA. There is also little reason to believe in reverse causality, *i.e.* that teachers' strikes cause family characteristics since teachers strikes are typically of short-duration and affect families only after 6 years from conception. Third, in principle, one can identify the effect of the teachers' strike using a single strike-year because the school age cut-off yields an untreated group of working families with children.

Using family-level wage income regressions, conditioning on year, state, MSA, the results suggest that teacher-strikes reduce family income by 0.01 log points, roughly 0.1 percent of their annual pay, per strike day for two-working parent families. This effect is significant at the one percent level. Put in perspective, a full-time wage earner who works 250 days per year suffers a loss equivalent to 2 hours of work per strike-day. This suggests that there is some ability of households to mitigate the costs of childcare demand shocks. Since one may be concerned that teachers' strikes are correlated with MSA-area socio-economic characteristics, particularly worker characteristics, a second identification scheme which I employ uses only time-variation within affected MSAs to identify the teachers' strike effect. These results are nearly identical to the estimates for the full-sample and suggest that MSA-level changes in unobserved characteristics not captured by MSA-year fixed effects may not be too severe. Two family characteristics appear important to the size of the teachers' strike effect: the working status of both parents and the age of children. I find no evidence that families with only one working parent are affected by teachers strikes which suggests that the stay-at-home parent provides insurance against such transitory shocks.[1]

To uncover the individual parent responses, I estimate wage-income equations for fathers and mothers respectively. The results in this paper imply that the effects of a teachers' strike depend non-linearly on the relative wages but, on average, fathers face higher costs than mothers. Fathers in the average family face a teachers' strike costs of -0.023 log points per strike day (0.2 per cent of annual income) while mothers in the average family are essentially unaffected (their incomes rise by 0.0007 log points) if there is child 6-12 years of age in the family. The results suggest that families with only older children are unaffected by teachers' strikes. The results also suggest that the gender of the child matters: fathers (and families) lose income when sons are affected by a teachers' strike but do not when daughters are affected.

The data used in this study come from two sources. I merge data from the Current Population Survey (CPS) and the Bureau of Labor Statistics Work Stoppage Dataset to

[1] Throughout this paper I make no distinction between stay-at-home mothers and stay-at-home fathers.

construct a dataset of wage income, family attributes, worker attributes and teacher-strike incidence for families. The dataset covers the period 1993-2002 and includes 283 metropolitan statistical areas (MSAs). I truncate the sample at 2002 because post 2002 the CPS scrambles the ages of children under 12 in the household and so I cannot accurately differentiate between school-age and non-school age children.[2] I use annual wage income as the dependent variable because the CPS collects data only on usual hours worked at the annual frequency and the interpretation of usual hours by a respondent is uncertain.[3] During this period, there were 23 separate teachers' strikes which affect approximately 1 per cent of the sample. Because the MSAs and school districts do not overlap perfectly, one may be concerned about effect sizes in the responses. Using 2000 Census data, I calculate the unconditional probability that a family in a MSA was affected by a school strike within that MSA as the fraction of the population of that MSA in the school district. The unconditional probability is roughly 25 per cent which implies that adjusting for effect sizes leads to a family effect that is equivalent to 8 hours of work per strike-day.

There is relatively little existing evidence on the allocation of transitory childcare shocks within families. Han, Ruhm and Waldfogel (2009) examine the response of parents to leave expansions for newborn children in the US and find that both fathers and mothers tend to take longer absences from work when leave provisions are expanded. Neponmyaschy and Waldfogel (2007) find that the incidence of leave-taking after birth by resident fathers was roughly 90 per cent although the typical leave was less than two weeks in duration. However, there would appear to be valid reasons to think a family's choice of parental leave taking after birth and a families choice of parental leave taking to childcare shocks from children would be different. Most obviously, mothers and fathers experience birth quite differently. There are also biological differences between parents for the care of infants as fathers are unable to breastfeed newborn children. Thus, a family's choices of maternity and paternity leave may bear little relation to a family's allocation of childcare shocks.

[2]Including the post-2002 period would, in fact, tend to strengthen the significance of the results reported in this paper.

[3]The CPS collects does data on actual hours worked during the reference week but the sample reduction implied by matching to this frequency makes such an approach infeasible.

Although the focus on this paper is on quantifying the income costs borne by parents as a result of transitory childcare shocks, it does relate to the literature on the family-gap in wages. A typical approach in the literature to estimate the effect of parenthood on wages is to estimate the average difference between the wages of otherwise comparable women who differ in their family attachments, see for instance Waldfogel (1998), Lundberg and Rose (2000), Phipps, Burton and Lethbridge (2001) or Simonsen and Skipper (2006). Simonsen and Skipper (2006) report that typical estimates of the family-gap in wages for women in the US are 10-15 percent although they themselves estimate a smaller penalty (roughly 7 per cent) using a propensity-score matching procedure for Danish women. It is important to note that this literature estimates permanent wage differences between mothers and non-mothers and so the results for transitory shocks in this paper are not contradictory.

That fathers are the primary parent affected by teachers' strikes may seem unexpected although it is not clear that such suspicion is warranted. Blau and Kahn (2000) highlight the gender differences in wage income for men and women in the US. Bertrand, Goldin and Katz (2010) examine the earnings of men and women with MBAs who work in the financial and corporate sectors and find that women are more likely to have career interruptions and lower hours of work after motherhood. Both contribute to lower wage income for women than men. Finding that fathers are more likely to face income costs from transitory childcare shocks than mothers does not undo any gender bias in wages or permanent wage income. The approach of this paper is different: to uncover the transitory income effects of parenthood using exogenous childcare shocks.

Bianchi (2010) reports, using data from the Wisconsin Longitudinal Study, that the gender gap in the provision of any childcare between fathers and mothers decreased by almost a factor of 4 between 1993 and 2004 and was 5.7 percentage points in 2004 . Similarly, Bianchi reports that the average weekly hours in which fathers are responsible for childcare increased from 2.6 to 6.9 hours per week from 1985 to 2003. Admittedly, mothers remain the primary care giver and increased their hours of childcare from 8.4 to 14.1 during the same period. Smith and Schaefer (2012) analyze data from the 2008 National Study of the

Changing Workforce and find that roughly half of employed fathers and mothers lack paid sick days to care for children. At the same time they find that 40 percent of fathers stay home to care for sick children (for mothers 74 percent stay home).[4] The point is not that fathers are becoming the primary childcare provider in the family but that their role is increasing and they may not have the same employment benefits as mothers. Thus, as the results in this paper suggest, they may be more likely to face income effects from childcare shocks.

This paper is organized as follows. Section 2 describes the income and demographic data used in the empirical work. Section 3 presents estimates of the effect of teachers' strikes for the family income of affected families using Mincer-type wage regressions at the family level. Section 4 uses individual-level wage regressions to estimate the allocation rule of income effects for fathers and mothers within the family. Section 5 concludes.

2 Data

The data used in this paper come from two sources: the March Annual Supplement of the Current Population Survey (hereafter CPS) and the Work Stoppage Database maintained by the Bureau of Labor Statistics.[5]

The CPS data permit the coding of family relationships in the data (as noted in Dunbar and Easton (2009)). Although the CPS data include an entry for family ID, there is some ambiguity in the CPS data regarding families and sub-families. Because I cannot accurately determine family structure when subfamilies are present, I choose to retain observations only on primary families, primary individuals and secondary individuals. This reduces the dataset by roughly two per cent. To ensure consistency across the sample, families are coded on the basis of their response to the household relationship question and any person under 18 living in the household is counted as a child. Thus, for instance, an aunt and uncle raising their niece (for whatever reason) would be coded as a family rather than relying on the response

[4]Interestingly, when fathers are asked the question of who stays home to care for a sick children, they report roughly an equal share with the mother. When mothers are the respondent, they report that fathers provide childcare only 16 percent of the time.

[5]The CPS data for the years 1993-2006 were obtained in 2006, for a fee, from the Unicon Research Corporation: www.unicon.com. Subsequently, this data became publicly available from IPUMS.

to a question of the type: "do you have any children present." This prevents the nature of family formation from affecting the results. I flag families in this way in the dataset and construct a dataset of income by families. I note that the CPS changed the reporting of children's ages as of 2002. After 2002, the ages of children under 12 were randomized to ensure respondent confidentiality. Unfortunately, this change implies that one cannot identify school-aged and pre-school-aged children in the data post 2002. Thus, I restrict the sample to the years 1993-2002.[6] I use the annual income data from the March Annual Supplement as the dependent variable of interest.[7]

The Work Stoppage data is collected by the BLS and it records the employer, the union, the dates of the strike differentiated by metropolitan statistical area (MSA). Because the CPS sample is restricted to the pre-2002 years and because the income data in the CPS is lagged one year, matching the CPS and Work Stoppage data is feasible for the years 1993-2001. The final data set includes 23 teachers' strike episodes. I merge the Work Stoppage dataset with the family-level dataset on wages and individual characteristics of the family adults matching observations by year and MSA. Of the teachers' strikes on record, the average number of calendar strike days (weighted by the number of affected families) in the sample was 7.4, with a minimum of one day and a maximum of 28. The distribution of the teachers' strikes by State is relatively diverse and it is also diverse within States by MSA. California experienced the most teachers' strikes (7) during this period and Oakland is the MSA with the most teachers' strikes (3). The distribution of strikes by States is presented in Table 1.

I note that the teachers' strike data and the geographic data do not overlap perfectly. Thus, a household in a given MSA may not have been subjected to a teachers' strike because the population in the school district is a subset of the population in the MSA. To assess the

[6]These results are, on the whole, similar to the full-sample results presented in this paper and are available upon request.

[7]Although higher frequency data such as monthly data from the Current Population Survey might be preferable, the monthly surveys do not include family characteristics and so one cannot distinguish workers by their family characteristics which makes such data unsuitable. One can link the monthly surveys to the March Annual Supplement however, as I discuss below, no linkage is possible for a window from 4-8 months after the Supplement and the resulting sample size of affected families becomes too small. Finally, it is not *a prior* clear that income effects are necessarily borne in the month of the strike. As I also discuss below, some workers may be affected because of a denied promotion, for example.

probability that a household was, in fact, treated by a teachers' strike, I use auxiliary data from the 2000 Census to compute the proportion of individuals living in the affected school district as a proportion of the total population of the MSA. These proportions are reported in the final column of Table 1. However, I caution that the proportion may either overestimate or underestimate the fraction of families affected by the teachers' strike because there is no *a priori* reason to believe that families are equally distributed across MSAs. To control for possible mis-attribution of treatment by a teachers' strike, I create a dummy variable equal to one for teachers' strike episodes in which the proportion is less than 0.2. I interact this dummy with the teachers' strike in subsequent regressions to evaluate the importance of this match effect. I report in Table 2.1 the unconditional probability that a family is affected by a teachers' strike (0.24).

2.1 Sample Restrictions

The appropriate sample of workers and the appropriate treatment group are not immediately obvious. In particular, to identify the treatment effect of a teachers' strike separately from a general MSA-level effect for household income, one needs to exploit variation within the MSA to ensure that the treatment effect is not simply a MSA region/year effect.

There is much evidence that parents (especially women) have different labor characteristics than non-parents, *e.g.* Angrist and Evans (1998), Lundberg and Rose (2002), Simonsen and Skipper (2006). Collective household models of labor supply, *e.g.* Apps and Rees (2002) and Blundell *et al.* (2005), imply that the labor supply choices of parents may differ from the labor supply choices of non-parents. To isolate the effect of teacher-strike shocks from the effect of being a parent (and thus facing other childcare shocks), I restrict the sample to include only families with two parents present and children under 18 years of age for the main results presented in this paper. This restriction implicitly assumes that the average realization of 'other' childcare shocks are equal across the treated and untreated groups. So for example, households treated by a teachers' strike do not have healthier (or less healthy) children than non-treated households. I examine this exclusion restriction by also testing teachers' strikes on families without school-aged children to ensure that I am not confounding a teachers' strike effect with other unobserved treatments.

A second restriction I employ is that no household has income that is directly affected by the teachers' strike. One goal of this paper is to identify children as a source of income shocks within households using teachers' strikes as a natural experiment that indirectly treats households with children. Thus, no household in the sample should have a worker employed as a teacher (or in the education system affected by the strike) as this would confound the direct effect of a teachers' strike with the indirect effect.

A third sample restriction I use is to restrict the sample to families in which both parents were employed for at least 48 weeks during the previous year (hereafter the 'working family'). This restriction is employed for three reasons. The first reason is that it is not clear whether families with one parent-worker are likely to be affected by a teachers' strike since a stay-at-home parent could provide childcare services.[8] Thus, finding a treatment effect for this group may be confounding an unobserved MSA/year effect with the teachers' strike (an example might be a localized flu epidemic). A second reason to exclude one working parent families is that these families have a made a different extensive margin labor supply choice. Such unobserved household heterogeneity is implicitly in the wage regression residual and would likely bias the standard errors of the estimated treatment effects for two-working-parent families (if, indeed, such effects are present). The third reason is that these families are almost certainly affected by the teachers' strike since they are working almost every week during the year in which there was a teachers' strike. Thus, there is little possibility that, for example, a parent fortuitously gains employment as a replacement care-giver during a lay-off or when between jobs. However, I also relax this restriction and consider families in which both parents work at least 40 weeks during the previous year and find no difference to the results.

For completeness, I estimate the model using a sample including one-working parent

[8]Suppose one parent drives their child to school on the way to work while the second parent stays home to care for the remaining children. Driving one's child to school involves a time cost and also a fixed schedule (school starts at some particular time each day). A teachers' strike which removes this constraint may, in fact, increase the working-parent's income by removing any costs associated with driving a child to school.

families with a dummy variable for two working-parent families and find nearly identical results. Thus, as a practical matter, it does not appear that unobserved heterogeneity in the extensive margin of household labor supply matters for the treatment effect estimates.[9]

I also restrict the sample to exclude families who migrated to the strike MSA during the previous calendar year as I cannot be certain whether these families were in residence during the school-year of the strike. Since the CPS does not include the MSA from which they migrated, I cannot match these households to strikes which may have affected them. I do include families who migrated in the control group since they implicitly provide information regarding the wage structure of the MSA, although I test whether the results are sensitive to this choice and find generally that they are not.

Finally, I restrict the sample to families with only salaried workers, *i.e.* no self-employed workers, who are not employed in the public sector. Salaried workers include workers paid by the hour. There is abundant research on entrepreneurial income and the tax incentive differences between dividend payments, retained earnings and wage payments by entrepreneurs. There have also been tax code changes to the treatment of dividends over the period of the sample (notably in 2001) which may affect reported wage incomes and retained earnings. Since I cannot observe dividend payments made to households or retained earnings I drop self-employed households from the sample. Similarly, some public sector workers have benefits packages that may differ from those of private-sector workers and which may mitigate the effects of childcare shocks. Since I cannot observe such differences, I omit public-sector workers from the sample.

While the mechanism linking reduced hours worked and income is clear for workers paid by the hour, one may wonder how workers with a fixed annual salary might suffer an income effect from reduced labor supply. There are at least four plausible channels. (1) A salaried worker may be required to forgo over-time hours. (2) A salaried worker may lose bonus payments. (3) A salaried worker may have a promotion delayed. (4) A salaried worker may be required to take a day-off without pay if company policy does not permit for some

[9]These results are available upon request.

type of compassionate leave to be used in the event of a strike (such as sick days limits or requirement of a medical form).

The final sample of two, full-time working-parent, families is 24,498 observations. 434 of the families were treated by a teachers' strike and the average number of strike days for these families was roughly 6.[10] Table 2.1 presents summary statistics for the working sample.

Table 2.1: Summary Statistics

	Mean	Std. Dev.
Age Head	38.3	7.6
Age Spouse	37.1	7.2
Proportion Caucasian Head	0.87	0.002
Proportion Caucasian Spouse	0.87	0.002
Weeks Worked Head	51.9	0.4
Weeks Worked Spouse	51.9	0.4
Usual Hours Worked Head	42.8	8.9
Usual Hours Worked Spouse	38.6	9.6
Log Family Income	11.0	0 .53
Log Head Income	10.3	0 .65
Log Spouse Income	10.0	0 .68
Average Number of Children	1.81	0.86
Average Number of Children, 6-12	0.74	0.83
Unconditional Probability of Strike Treatment	0.24	0.31

3 The Family Effect

The first empirical exercise conducted in this paper is to examine the effect of teachers' strikes for family income. I consider the regression:

$$y_{j,m,t} = \alpha_{t,m} + \psi \mathsf{T}_{m,t} + \zeta \mathsf{CT}_{j,m,t} + e_{j,m,t} \tag{1}$$

where $y_{j,m,t}$ is the logarithm of the annual wage and salary income for family j in MSA

[10]The underlying assumption is that all children attend public schools in the metropolitan statistical area. To the extent that some children attend private schools whose teachers are not on strike, the estimates presented in this paper will understate the effects of teacher strikes. Private school attendance data is available in the Census files and the data there suggest that roughly 10 per cent of children attend private school.

m in period t; $\alpha_{t,m}$ is a matrix of year and MSA dummies including all interactions; T is a categorical dummy variable of teacher-strike days in MSA m in period t and CT is an interaction variable between the teacher-strike days and a treatment dummy (vector) for the household C.[11] I discuss below the coding scheme for the treatment dummy. [12] In the estimation results which follow, I cluster the standard errors by MSA to control for possible regional effects in wage setting (there are 283 clusters). I do not include covariates for either parent in the wage regression above because it is theoretically unclear how these covariates map into the logarithm of total family wage income. Nor is there any reason to believe that these covariates affect the treatment of a given family.

The family-level regression is interesting for two reasons. First, if $\zeta \neq 0$ then this implies a teachers' strike effect for the treated family although it does not differentiate treatment between family members. Second, the family-level regression can be used in limited ways to test the sample restriction assumptions. However, the family-level regression provides no evidence of a household interpretation. To see this, define ζ_m as the effect of a teachers' strike for mothers and ζ_d as the effect for fathers. The estimated family effect, ζ, is therefore a weighted average of ζ_m and ζ_d and does not uncover either ζ_m or ζ_d.

It is not obvious how the age of a school-aged child and the ages of any siblings may change the costs of teachers' strikes for parents. Although there is no legal minimum-age requirement for children to be left alone in state statutes in most US states (Illinois, Georgia, Maryland and Oregon appear to be the exceptions), it is also unclear how the state welfare agencies determine child neglect. There is also variation in the legal requirements at the municipal and county-level. Unfortunately, obtaining precise information for every MSA over the entire period of the sample appears infeasible. However, even absent the legal considerations, it is probably safe to say that most parents exercise some judgement in this regard and the data should help illuminate this judgement. It is probably safe to say that

[11]For instance, if all children are under school age, then a teachers' strike should have no effect on the childcare demands faced by this family (if neither parent is employed as a teacher) and $\mathsf{C} = 0$. I also consider only a binary dummy variable for the teachers' strike and find similar results.

[12]For the sake of exposition, I omit reporting the the MSA-year dummies. I report only the estimates of the average treatment effect, ζ, but the remaining regression results are available upon request.

most parents would not leave a 7 year-old at home unattended for a day but may feel less concerned about leaving a 13 year-old at home with a phone. To continue with this train of thought, imagine a school-aged child has an older sibling aged at least 13 years. It is also not difficult to imagine that a teachers' strike affecting both siblings would simply shift the childcare demands from the teacher to the older sibling rather than the parent. Thus, I define the treatment dummy, C, by the age of the eldest child and I focus on the treatment effect for families whose eldest child is between 6 and 12 years of age. Indeed the data appear to confirm the conjecture that the age of the eldest child is an important determinant of the magnitude of the teachers' strike effect.

I note that an underlying assumption is that all children attend public schools in the metropolitan statistical area. The March Annual Supplement to the CPS does not differentiate between public and private school attendance or home schooling. The October Education Supplement to the CPS does differentiate between public and private school attendance but does not collect the rich data on the demographic composition and economic status of households from the March Annual Supplement. In theory one could merge the data using the unique household identifier but this approach is infeasible. The interview rotation scheme of the CPS is to interview for 4 months, not interview for 8 months, and then interview again for 4 months. Thus, the overlap between samples between 4 and 8 months is zero. Since March and October are 7 months apart then there is no overlap between these samples. To the extent that some children attend private schools whose teachers are not on strike or are home-schooled, the estimates presented in this paper will understate the effects of teacher strikes. Private school attendance data is available in the Census and from the National Centre of Education Statistics (NCES) and the data there suggest that roughly 10 per cent of children attend private school and that this proportion has been roughly stable over the sample period I consider.

The results are presented in Table 2. A teachers' strike day lowers log income by roughly 0.011 log points of income per strike day for families with an eldest child between 6 and 12 years of age. The effects of a teachers' strike for families with children between 12-18 years of age is not statistically different from zero. The lack of a significant effect for families with children 12-18 does suggest that the costs of teachers' strikes are felt primarily by parents with elementary-aged children.

As noted above, the geographic mapping from the school districts which experienced a strike to the MSAs is not perfect and it is possible that some households are being treated as if they experienced a teachers' strike when, in fact, they may not have. Thus, I re-estimate considering households as treated by the teacher's strike only if the proportion of the MSA population within the striking school district was 0.2 or higher. Of the 434 households possibly affected by a teachers' strike, 173 satisfy this restriction. The estimated effect of the teachers' strike is $-0.012(0.007)$ with an associated p-value of 0.076.

The estimate for the teachers' strike days in all specifications is insignificantly different from zero which suggests that the estimates of the teachers' strike effect for households with school-aged children are not simply unobserved labour demand effects operating at the MSA-level. However, this interpretation relies on the assumption that households with one working parent are unaffected by teachers' strikes. This assumption may not be warranted because stay-at-home parents may have other time demands that are not captured by market hours worked, such as volunteer commitments. Fortunately, couples without children in which both partners work provide a natural control group as they have made the same extensive labor market decisions as two-working parents with children. I re-estimate the regression specification, Equation (3), including couples in addition to parents.[13] I interact the teachers' strike day dummy variable with a dummy variable for couples. Thus, as controls, I have one dummy variable for one-working parent families and families with no school-age children and a second dummy variable for couples without children, both interacted with the teachers' strike. The estimated effects of the teachers' strike for both controls were insignificantly different from zero with point estimates very close to zero (see column 6 of Table 2). This suggests that teachers' strikes are uncorrelated with labor demand effects operating at the MSA level and that the assumption of exogeneity of teachers' strikes with respect to

[13]Obviously for couples, $\mathsf{CT}_{j,m,t} = 0$ for all j, m and t.

two-working parent families is reasonable. Moreover, the inclusion of MSA-year dummies effectively controls for changes in MSA-level economic conditions.

The estimated effect of a teachers' strike day for families with school-aged children in which both parents work are roughly 0.011 log points across all specifications. This implies that teachers' strike days cost families approximately 0.1% of their annual income. The average log income for families is 11.0 log points and so an average teachers' strike costs families roughly 0.1 per cent of their income. In terms of average daily income, a teachers' strike day costs families roughly 1/4 of an average day's income (assuming an average of 250 working days per year). Although a teachers' strike is an exogenously determined event for parents (if they are not teachers themselves), there is some likelihood that parents form prior expectations over the occurence of a strike. This suggests that any estimated effects of teachers' strikes for parental wages may be understated if parents are able to insure against the childcare shock. Also, as noted previously, the unconditional probability that a family is affected by a teachers' strike is 0.24. If the treatment probability is correct then a strike-day costs families approximately 1 day's income.

4 Intra-Household Effect: Fathers and Mothers

Finding that families face costs from childcare shocks suggests a second question: which parent pays the cost? In households with two parents, the answer is not obvious. Apps and Rees (2002), Blundell, Chiappori and Meghir (2005) (hereafter **BCM**) and Cherchye, De Rock and Vermeulen (2012), consider household models that include the labor supply decisions of parents and, at the risk of overly simplifying their results, show that the labor supply decisions by parents depend on the relative wage income of each parent. This suggests that the teachers' strike effect can be uncovered via individual wage-income regression which include relative wage terms.

I adopt a Mincer-type regression for individual wage income to ensure that any estimated teachers' strike effects at the individual level are not simply confounding wage payments for individual attributes. For the household, the wage-income regressions are two equations:

$$
\begin{aligned}
y^h_{j,m,t} &= \alpha^h_t + \beta^h_m + X^h_{j,m,t}\gamma^h + \psi^h \mathsf{T}_{m,t} + \zeta^h \mathsf{CT}_{j,m,t} + \omega^h \mathsf{CH}_{j,m,t} + e^h_{j,m,t} \\
y^s_{j,m,t} &= \alpha^s_t + \beta^s_m + X^s_{j,m,t}\gamma^s + \psi^s \mathsf{T}_{m,t} + \zeta^s \mathsf{CT}_{j,m,t} + \omega^s \mathsf{CH}_{j,m,t} + e^s_{j,m,t}
\end{aligned}
\tag{2}
$$

where $y^i_{j,m,t}$ is the logarithm of the annual wage and salary income for individual i in family j in MSA m in period t (superscript h refers to head, superscript s refers to spouse); α_t is a vector of year dummies; β_m is a vector of MSA dummies; $X^h_{j,t}$ and $X^s_{j,t}$ are covariates for the head and spouse respectively; T is a categorical dummy variable for the presence of teacher-strike days in MSA m in period t and CT is an interaction variable between the number of teachers' strike days and a treatment dummy (vector) for the household C.[14] I discuss below the coding scheme for the treatment dummy. As covariates for *each* parent, I include age, the square of age, the presence of children under 6, 6-12 and under 18, a dummy variable for education coded into 17 categories collected by the CPS (the variable 'grdatn'), the number of household workers, the number of weeks and the square of the number of weeks worked in year t, the average number of hours worked per week and its square in year t, a dummy variable for the size of the employer by number of employees, a dummy variable for the 2 digit industry code, the gender and the race of the individual. These covariates number, at a minimum, 350 in the regressions which follow and for the sake of exposition, I omit reporting them all. I report only the estimates of the average treatment effects, ζ and ω.[15] In the estimation results which follow, I cluster the standard errors by MSA to control for possible regional effects in wage setting (there are 283 clusters). In all regression results reported below, unless otherwise specified, the sample includes families with children with two parents, headed by a male household head, in which both parents worked at least 48 weeks in the private sector (and were not teachers). As well, I control for migration when determining which families were potentially affected by a strike.

The coefficients ω^h and ω^s measure the labor allocation effects of a teachers' strike for each parent given the relative wage differences. The errors in this specification are likely

[14]For instance, if all children are under school age, then a teachers' strike should have no effect on the childcare demands faced by this family (if neither parent is employed as a teacher) and $\mathsf{C} = 0$.

[15]The full regression results are available upon request.

to be a complex combination of unobserved individual and household effects, market-wage structure in a particular MSA and possibly non-linear year effects. Certainly one would expect significant correlation between the residuals $e^h_{j,m,t}$ and $e^s_{j,m,t}$ and, indeed, this appears to be the case in practice with most correlation estimates in the neighbourhood of 0.2. I estimate a seemingly-unrelated regression.[16] One additional change from Equation (3) is to include an additional covariate, $\mathsf{CH}_{j,m,t}$, which is an interaction between the treatment of a teachers' strike and the income difference of the head and spouse. This additional covariate measures the difference in the price of a work absence for the household head and the household spouse. I compute the ratio of the average hourly earnings for each parent using the income data, weeks worked and average hours worked per week.[17] Because the relative wage is likely related to the log wage, I also include the relative wage as a covariate in X. Similarly, the relative wage may also capture MSA-level effects, perhaps because of varying degrees of gender discrimination or because of gender-biased industries. Thus ω^h and ω^s in Equation (2) identify the effect of the relative wage for the treatment of a teachers' strike for families with a school-aged child unconfounded by local wage conditions.

Since the regression specification includes the relative wage as a covariate for each parent, it is highly likely that the wage income, and hence the relative wage, suffer from measurement error which induces endogeneity in Equation (2). However, if the teachers' strike is exogenous to the household, the interaction of the teachers' strike and the relative wage to identify the effect of relative wage differences on the allocation of the childcare shock is plausibly orthogonal to the errors and hence an unbiased estimator. Because the relative wage times the treatment of a teachers' strike is one object of interest in the regressions, I also interact the relative wage with the number of children included as covariates and include these interactions as covariates in all regressions.

The estimates for the treatment effects, presented in Table 3 and Table 4 for fathers

[16]See Davidson and Mackinnon (2004).

[17]Strictly speaking the computed wage may confound the effect of a teachers' strike if the teachers' strike has asymmetrical effects on the log wage of the parents. One method to control for this effect is to estimate iteratively since the treatment is strictly exogenous to the household. I do this correction and find the effect is small and does not change the estimates to four decimal places.

and mothers, respectively, are remarkably consistent and somewhat surprising. Specification (1)-(3) of each table report the estimates from the individual wage regressions for different polynomial specifications of how the relative wage affects the allocation of the teachers' strike. The linear specification of the relative wage effect, specification (1), suggests that fathers of children 6-18 face a strike cost of -0.024 log points per strike day but that this effect diminishes with the relative wage at a slope of 0.015 log points. Mothers, in contrast, have an intercept of 0.026 and a slope of -0.014. Taken together, these results suggest that fathers tend to bear the burden of the teachers' strike costs unless their wage is much larger than their spouses. The threshold at which fathers no longer face costs from teachers' strikes $-\zeta^h/\omega^h = 1$ which implies a relative wage threshold of 1.6. A similar calculation for mothers implies a threshold of 1.86 at which mothers appear to face a cost from a teachers' strike. The median relative wage for families affected by a teachers' strike is 1.33. Thus, in the majority of families, fathers pay a higher cost as a result of a teachers' strike than mothers.

The estimates from the linear specification would seem interesting but also possibly concerning given the family-level results reported in Table 2. At issue is the age of the child: in the family specification the teachers' strike affected families with young children, 6-12 years of age, but the individual level regressions suggest that age of the child is not important (beyond being of school age). Fortunately, the estimates from specification (2) help to reconcile these apparent differences. Specification (2) includes a quadratic polynomial for the relative wage. Including the non-linear affects shifts the estimated treatment effect to children 6-12 years of age for fathers: the estimated intercept is -0.036 and the slope is 0.023. The quadratic terms are statistically insignificantly different from zero. For mothers the estimated effects for children 6-12 years of age are not statistically significant but the estimated effects for children 6-18 remain so with an estimated intercept of 0.032 and a slope of -0.031. The higher order effects appear important for mothers with offsetting effects depending on the age of the child(ten). Thus, non-linear effects appear important for understanding within household bargaining and help reconcile the family-level results with the individual results. Specification (3), which includes a cubic polynomial in the relative wage, does not offer any reason to change these conclusions. The estimated results for specification (3) suggest that the cubic terms are not relevant for the allocation of teachers' strike effects.

Nevertheless, there are a number of reasons to think that the estimates reported thus far may be overstated. For example, in a number of the MSAs, the fraction of parents possibly affected by a teachers' strike is very small. Considering these parents as being affected by a teachers' strike when in fact they are not could potentially bias the results.[18] A second concern is that teachers' strikes are of different durations and families might adjust their behaviour as a strike continues. If these adjustments are different than the initial response then the distribution of the length of the strike might matter quantitatively and qualitatively to the results. It is also possible that comparing families treated by a teachers' strike against families in MSAs which never experienced a teachers' strike might be confounded by unobserved intra-MSA differences.[19] There is also no evidence that families are necessarily adjusting optimally. Finally, the SUR specification in Equation (2) does not include MSA-year interaction dummies and so labour demand effects or differences in perceived economic conditions might be linked to the strike effect.[20]

Specifications (4)-(9) investigate these concerns. Specification (4) only counts households as potentially treated by a teachers' strike if the proportion of families living within the striking school district comprise at least 20 per cent of the MSA's population (I omit all households in which the treatment probability is less that 20 per cent). For fathers, the estimated intercept and slope effects are -0.031 and 0.025 respectively. For mothers the estimated intercept and slope effects are 0.029 and -0.019 respectively. These estimates are similar to those of Specification (1) which is otherwise the same specification.[21] Specification (5) includes a dummy variable indicating whether that parent is the highest wage earner in

[18] I remind the reader that the MSA is the lowest geographic unit I can use for the purposes of this study.

[19] For instance, are two-working-parent families in Utah comparable to two-working parent families in Chicago?

[20] The full set of interactions is not included for computational reasons.

[21] Using the quadratic specification results is similar point estimates as well but they are generally not significant. The quadratic

the family or not and also interacts this dummy variable with the teachers' strike treatment. The high earner dummy variable interaction is significantly different from zero for both mothers and fathers. For fathers the estimated interaction affect is -0.037 and for mothers the estimated effect is -0.031. These estimates are both negative which suggests that the household is re-optimizing since it is not simply the low income earner who is bearing the strike costs. Specification (6) includes a dummy variable indicating if the strike is longer than one week. The inclusion of the 'long strike' dummy is particularly important for the fathers' results but has no significant effect for mothers. For fathers the fixed cost of a teacher-strike day increase in magnitude to -0.087 log points and the slope increases to 0.045 if the strike is less than one week in duration. If the strike is longer than one week the costs fall by 0.068 log points per strike day (-0.034 for the slope). Thus, as a strike's duration increases families appear to find ways to adjust and reduce the impact of the strike on fathers. Specification (7) restricts the sample to only MSAs that experienced a teachers' strike. The results are largely identical to those reported for specification (2) which is otherwise the same specification. Specification (8) trims the sample by restricting it to the 2.5-97.5 percentiles by MSA-year of the relative wage. In this specification, the non-linear estimates lose precision for men yet the overall finding, that fathers' incomes are negatively affected by a teachers' strike, remains. Finally, Specification (9) estimates the individual wage income regressions using a full set of MSA-year dummies, including interactions, by OLS. This controls for labour demand effects or differences in perceived economic conditions at the MSA level.

The conclusion from these robustness checks is fourfold. First, different sample specifications do not appear to change, qualitatively, the results. Second, families appear to re-optimize their labor allocations when they are affected by a teachers' strike. Third, the non-linearities of the relative wage effects do not appear to be easily explained by non-linearities in the length of the strike, the choice of control group, or on which parent has the highest wage income. Fourth, the inclusion of MSA-year interaction dummies does not change the estimated effects of teachers' strikes for fathers or for mothers.

The results across all specifications are qualitatively consistent and suggest that differences in income are a significant determinant of who provides childcare when faced with a teachers' strike. However, as noted above, the results suggest that it is not simply the lower income earner who provides childcare. Thus, there is a relative bias towards childcare provision by one parent – in this case by fathers. To see this, note that if both parents have the same hourly wage then the teachers' strike lowers the fathers income by 0.015 log points if he has children 6-12 years of age (using the estimates from specification (2)) while the mothers income increases by 0.007 log points using the same specification if she has children 6-18 years of age. One interesting observation is that parents appear to treat children differently depending on their age.

4.1 Boys, Girls, Fathers and Mothers

The individual level results for fathers and mothers are suggestive of gender differences in the response of parents to teachers' strikes. Fathers appear most response when children 6-12 are affected by a teachers' strike. Yet it is premature to conclude that the results imply that fathers are necessarily more caring towards young children. One issue is the gender of children in the family because it is not clear that the gender of children affected by a teachers' strike is randomly assigned within the sample. It may be that the parental response to a teachers' strike depends on the gender of the children affected. For instance, are fathers more likely to be affected by a teachers' strike if it is their son's school?

To investigate this question, I code children as either boys or girls and re-estimate the analog of specifications (1) and (2) from Tables 3 and 4. The estimates are reported in Table 5. Both fathers and mothers appear affected by a teachers' strike if there is a boy 6-18 years of age in the family. Considering the linear specification of the relative wage effect suggests that fathers face a cost with an intercept of -0.019 to -0.029 and a slope with respect to the relative wage of 0.012 to 0.018. Mothers, in contrast, face a intercept of 0.023 and a slope of -0.009 to -0.013. There is some evidence that mothers are also affected if there is a girl in the family but this finding is not robust across specifications. Qualitatively the results suggest that fathers are the primary parent affected when there is a boy in the family. The non-linear specification for the relative wage effect shifts the costs for fathers of boys 6-12 years of age as it did in the gender-neutral results reported above.

Strikingly, the results suggest that there is no evidence of an effect of a teachers' strike for families with girls. I caution that one should not conclude necessarily that results imply a gender-bias in the treatment of children by parents. The numbers of affected children become small when broken out by age and gender and it is difficult to test alternative hypotheses with any precision. For instance, one might wonder whether having a older daughter in the family causes parents to shift unexpected childcare demands to that child. Unreported results suggest this possibility but the standard errors of the estimates are too large to merit any confidence. And if indeed this is in fact the case, it may be that parents pay that child for the childcare services. As well, it might be the case with older children that parents have formed networks with other parents, including some parents who do not provide market labor or other parents with older daughters. Why this might differentially (positively) affect the costs of teachers' strikes for families with daughters, if indeed it does, is an open question. However, the estimates might equally be interpreted as suggesting that boys are more costly to raise and that fathers are more suited to the task for school-aged children. Thus, rather than gender-bias in the treatment *by* parents, the results may indicate gender-bias in treatment *for* parents.

4.2 Strike Cost Estimates

One consistent observation from the estimates reported for the individual regressions is that the income costs within the family are not uniformly negative.[22] Teachers' strikes affect fathers and mothers differently and teachers' strike costs appear to depend on the age and gender of the children concerned.

To compare the estimated costs of a teachers' strike with the family estimates reported in Section 3, I use the estimates from specification (2) in Tables 3 and 4 to calculate the

[22]Indeed, it is not entirely clear that one should expect a teachers' strike to impose a labor income cost on families although the estimates reported in this paper suggests it does.

individual costs of a teachers' strike. I then add these estimated costs together to calculate the estimated cost for the family. The total number of families affected by a teachers' strike is 358, thus this is the sample size for the estimated costs statistics. The median strike cost is -0.008 log points and the mean cost is -0.022 per strike day. I note that the estimated strike costs are similar in magnitude to those reported in Section 3 although the estimates are not directly comparable because of the logarithmic transformation of income. There is a fairly large degree of variation in the per-day strike costs – the standard deviation of the mean is 0.14, due largely to approximately 10 outliers. Comparing fathers' strike costs and mother's strike costs, the median for both is 0 while the mean for fathers is -0.023 and the mean for mothers is 0.0007. To get some sense of how the costs trade-off in the family, I regress the fathers' estimated cost on the mothers' estimated cost and obtain an estimated intercept of -0.022 and a slope of -0.46. For the average household, a one log point decrease in the mothers' cost increases the fathers by 0.46 log points. This result is somewhat surprising because it appears to suggest that fathers face lower relative costs for teachers' strike than mothers, although I caution that is only necessarily true for the average affected household.

I plot in Figure 4.1 a scatterplot of the estimated strike costs for both parents for families, which experienced a teacher's strike, using the estimates from specification (2) in Tables 3 and 4 (and removing 11 outliers to reduce the scale of the graph). As is clear, there is a negative slope which indicates intra-household trade-offs in strike costs. The distribution of the strike costs exhibits some variation but much of that variation is due to differences in the lengths of the strikes. In Figure 4.2 I plot a scatterplot of the estimated strike costs for both parents for families, which experienced a teacher's strike of one week or less in duration. As is evident in the graph, the variation in the strike cost estimates is smaller in the restricted sample. In both graphs, roughly half of the households fathers appear to pay a childcare cost from a teachers' strike while in the remaining half fathers appear to increase income from a teachers' strike. This suggests that in many households it is Pareto-efficient for one parent to work more in response to a teachers' strike while the other parent 'pays' the childcare cost. Indeed, in only 28 of the 358 families do both parents face a cost from a teachers' strike. I caution that the results here are sensitive to the sample restrictions as noted above.

One might wish to ask whether the results can be cast as a revealed preference for a particular parent to provide childcare. When one parent provides less market labor then that parent (and the family) gain the utility flow from less labor supply and the family also gains the utility flow from more childcare production by that parent (assuming the additional leisure is spent on home production). The family also loses the utility from the lower income earned by that parent. Thus there are three 'parts' to the analysis. If one parent is the higher wage earner and only that parent faces a cost from a teachers' strike then the family may be revealing a preference: the utility flow from less labor supply **and** more childcare production is higher for that parent. Without further assumptions one cannot go further and claim that children are preferring that parent. It could equally be that this parent has the more unpleasant job. Nevertheless, one can ask how many families have a preference for less labor supply **and** more childcare production from a particular parent. There is no apparent preference for 282 of the 358 families. In the remaining 76 families there is a revealed preference for fathers to have less labor supply and more childcare production.

Figure 4.1: Estimated Strike Costs by Parent

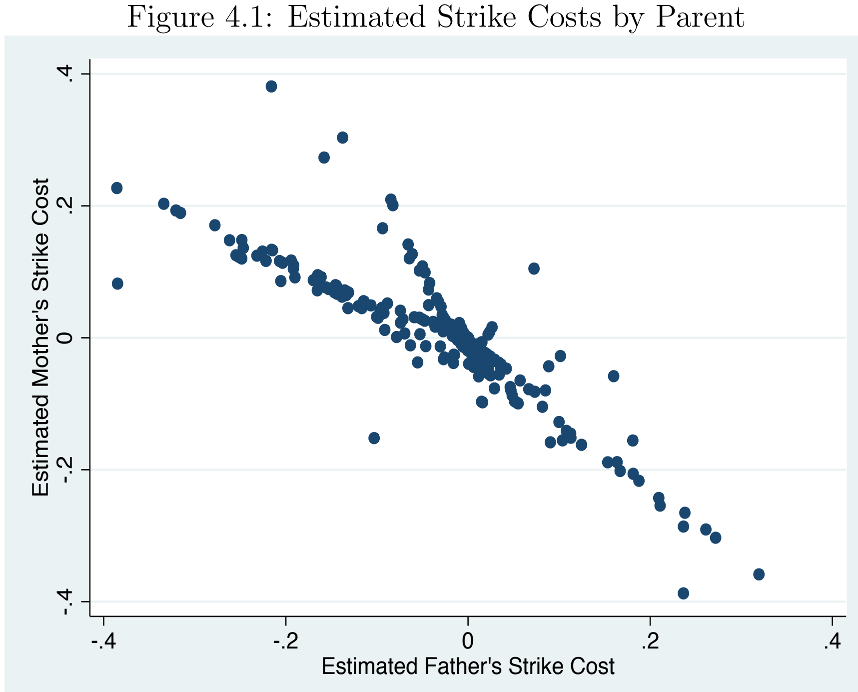
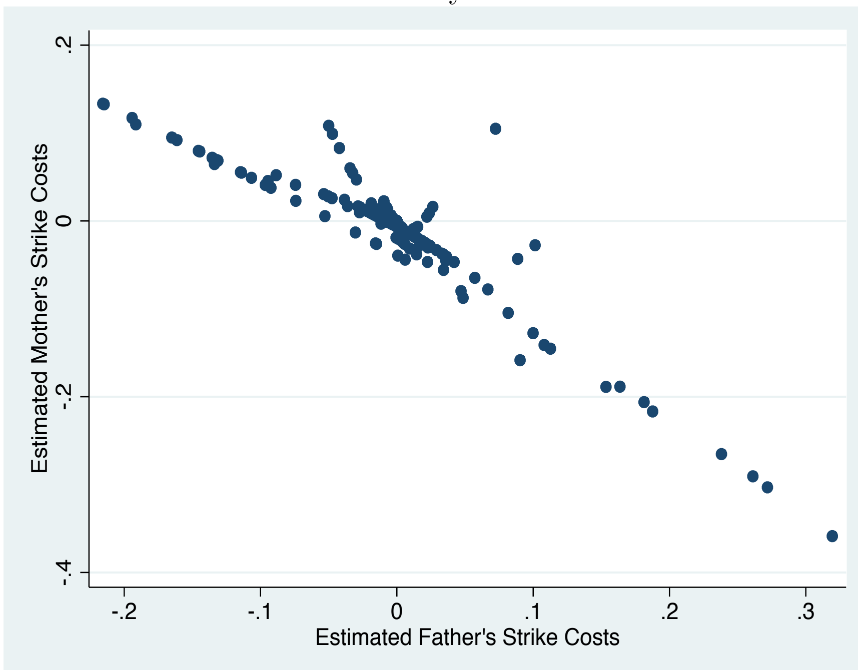


Figure 4.2: Estimated Strike Costs by Parent: One Week Strike or Less



5 Conclusion

This paper has used teacher's strikes as a source of exogenous variation in childcare demands to estimate the effects of unanticipated childcare demands for working parents. The results imply that working parents share childcare responsibilities although the teachers' strike effect for fathers is larger than that for mothers in the 'average' family. By logical extension, the results suggest that any individual-level wage income regressions for parents in families should control for labor supply decisions that result from family-level decision making.

One may wonder what conclusions may be drawn from the results beyond the implications for individual incomes and family-decision making. For instance, why do fathers face an income effect from a teachers' strike? There are at least two plausible interpretations and the data do not allow one to distinguish between either. It may be that fathers are less likely to select into occupations with more family-friendly leave-taking policies. Although this might seem implausible given the evidence presented in this paper, there are doubtlessly many possibly explanations which would support this as an equilibrium. Mothers, in contrast, may select into more family-friendly occupations and may, by their employment contracts, be insured against temporary income fluctuations from family shocks (possibly at a permanent income cost). The results of Simonsen and Skipper (2006) hint at this possibility. However, another explanation may be that fathers and mothers bargain over the allocations of family shocks and mothers provide insurance to the family for permanent family shocks (such as having children) while fathers agree to provide insurance against transitory family shocks. The results of the family gap literature may hint in this direction. Unfortunately for this paper, the data do not appear to allow identification of these channels.

It is also worth noting that teacher-strikes are unlikely to be the most common reason that children are unexpectedly absent from school and remain at home – illness and injury are far more common. The exact extent to which illness- and injury-related absences occur is, however, not entirely clear. In 2007, the NCES conducted a standardized test, the National Assessment of Educational Progress (NAEP) and explicitly asked about students absences in the previous month. Using these data to estimate the average days per month a student is absent from school yields a estimate of approximately 11 absence days per school year for grade 4 students (grade 8 students are similar). By asking a retrospective question in 2007, the response is less likely to capture absences related to student (or parent) opinions regarding standardized testing (for instance, parents and students may choose to not write a test. Both NCES data are nationally representative data and in 2007 reported surveying over 350,000 students at the grade 4 and grade 8 level. These data suggest that an upper bound on the average family illness burden is roughly 20 days per year assuming an average family fertility rate of roughly 2 and independence between child absences. Using the estimates from the teacher-strike regressions of -0.008 log points per day for the median family, this would imply a *family* income effect of roughly 1.6 per cent of the median family log wage. However, in terms of average relative wages for fathers and mothers, this would imply an income effect of -4.2 per cent for fathers and a (statistically insignificant) gain of 0.2 per cent for mothers per year.

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6 Appendix – Tables

Table 1: The Geography of Teacher Strikes, 1993-2006

Organization	State(s)	City	Union	Begin Date	End Date	Proportion
Boston Public Schools	MA	Boston	American Federation of Teachers	27-Oct-93	27-Oct-93	0.13
Providence Public Schools	RI	Providence	American Federation of Teachers	05-Sep-95	14-Sep-95	0.52
East St. Louis School District	IL	East St. Louis	American Federation of Teachers	03-Sep-96	15-Sep-96	0.01
Detroit Board of Education	MI	Detroit	Detroit Federation of Teachers	30-Aug-99	07-Sep-99	0.23
Contra Costa County Public Schools	CA	Contra Costa County	5 unions	26-Jun-96	26-Jun-96	0.39
Jersey City Public Schools	NJ	Jersey City	Jersey City Education Assn.	19-Nov-98	23-Nov-98	1
Dayton Board of Education	OH	Dayton	National Education Assn. (Ind.)	25-Mar-93	09-Apr-93	0.18
Vallejo Board of Education	CA	Vallejo	National Education Assn. (Ind.)	16-Apr-93	17-Apr-93	0.23
Youngstown Public Schools	OH	Youngstown	Youngstown Education Assn./NEA (Ind.)	08-Sep-93	05-Oct-93	0.15
Ann Arbor Board of Education	MI	Ann Arbor	National Education Assn. (Ind.)	29-Aug-94	11-Sep-94	0.21
Livonia Board of Education	MI	Livonia	National Education Assn. (Ind.)	01-Sep-94	06-Sep-94	0.02
Federal Way Board of Education	WA	Federal Way	National Education Assn. (Ind.)	06-Sep-94	13-Sep-94	0.03
Denver Public Schools	CO	Denver	Denver Classroom Teachers Assn./NEA (Ind.)	10-Oct-94	15-Oct-94	0.27
Anchorage School District	AK	Anchorage	National Education Assn. (Ind.)	12-Oct-94	14-Oct-94	1
Oakland Unified School District	CA	Oakland	Oakland Education Assn./NEA (Ind.)	28-Nov-95	29-Nov-95	0.18
Oakland Unified School District	CA	Oakland	Oakland Education Assn./NEA (Ind.)	30-Jan-96	30-Jan-96	0.18
San Diego Public Schools	CA	San Diego	San Diego Teachers Assn./NEA (Ind.)	01-Feb-96	08-Feb-96	1
Oakland Unified School District	CA	Oakland	Oakland Education Assn./NEA (Ind.)	15-Feb-96	20-Mar-96	0.18
Compton Unified School District	CA	Compton	Compton Teachers Assn./NEA (Ind.)	10-Jun-96	10-Jun-96	0.01
Birmingham Board of Education	AL	Birmingham	Alabama Education Assn./NEA (Ind.)	15-Nov-99	16-Nov-99	0.29
Buffalo Board of Education	NY	Buffalo	Buffalo Teachers Fedn./NEA (Ind.)	07-Sep-00	07-Sep-00	0.25
Buffalo Board of Education	NY	Buffalo	Buffalo Teachers Fedn./NEA (Ind.)	14-Sep-00	14-Sep-00	0.24
Anchorage School District	AK	Anchorage	Totum Assn of Educ. Support Personnel (Ind.)	15-Jan-99	21-Jan-99	1

Table 2: Teacher Strikes and Family Pay

		Regression Specification				
Treatment		(1)	(2)	(3)	(4)	(5)
ψ		-0.002	-0.002	0.004	-0.003	0.0003
		(0.004)	(0.003)	(0.004)	(0.004)	(0.003)
ζ	C					
	Child 6-12	-0.012*	-0.011**	-0.015*	-0.012	-0.013**
		(0.003)	(0.005)	(0.006)	(0.007)	(0.005)
	Child 6-18	0.005	0.005	0.009	0.006	0.005
		(0.005)	(0.004)	(0.006)	(0.004)	(0.004)
	Couples, no children					-0.001
						(0.003)
Nobs		24498	21356	2768	24498	38692
R^2		0.18	0.19	0.18	0.18	0.17

(1) Baseline sample as described in the text. (2) Sample further restricted to households in which both workers usually work more than 25 hours per week. (3) Sample restricted to include only MSAs which experienced a teachers' strike. (4) Treatment effect restricted to MSAs in which 20 % or more of households affected. (5) Sample includes couples with no children. Robust standard errors clustered at the MSA level reported in parentheses and stars refer to the signficance of the results: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Nobs refers to the number of observations included. Full estimation results are available upon request.

Table 3: Teacher Strikes and Parental Pay: Fathers

	Regression Specification								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
ψ	0.002	0.002	0.002	0.001	0.004	0.006	0.005	0.002	0.005
	(0.007)	(0.007)	(0.007)	(0.008)	(0.007)	(0.008)	(0.007)	(0.007)	(0.006)
Teachers' Strike Days interacted with									
Child 6-12	-0.002	-0.036**	-0.040*	0.016	-0.002	0.004	-0.035*	-0.038*	0.0001
	(0.011)	(0.013)	(0.020)	(0.013)	(0.011)	(0.036)	(0.014)	(0.018)	(0.012)
Child 6-12 x R. Wage	-0.003	0.023*	0.022	-0.017	-0.015	-0.007	0.025*	(0.026)	-0.004
	(0.007)	(0.011)	(0.026)	(0.010)	(0.009)	(0.018)	(0.011)	(0.022)	(0.009)
Child 6-18	-0.024*	-0.012	-0.017	-0.031*	-0.012	-0.087**	-0.011	-0.011	-0.025*
	(0.011)	(0.012)	(0.015)	(0.014)	(0.010)	(0.031)	(0.013)	(0.014)	(0.012)
Child 6-18 x R. Wage	0.015**	0.008	0.023	0.025**	0.024**	0.045**	0.002	0.012	0.016*
	(0.006)	(0.008)	(0.020)	(0.009)	(0.008)	(0.016)	(0.009)	(0.017)	(0.007)
Child 6-12 x R. Wage^2		-0.002	0.0004				-0.003	-0.003	
		(0.002)	(0.008)				(0.002)	(0.006)	
Child 6-18 x R. Wage^2		0.0001	-0.006				0.001	-0.002	
		(0.001)	(0.007)				(0.002)	(0.005)	
Child 6-12 x R. Wage^3			-0.0001						
			(0.004)						
Child 6-18 x R. Wage^3			0.0003						
			(0.0003)						
Child 6-12 x Father High Earner					0.026				
					(0.015)				
Child 6-18 x Father High Earner					-0.037**				
					(0.012)				
Child 6-12 x Long Strike						-0.005			
						(0.038)			
Child 6-18 x Long Strike						0.068*			
						(0.031)			
Child 6-12 x R. Wage x Long Strike						0.004			
						(0.020)			
Child 6-18 x R. Wage x Long Strike						-0.034*			
						(0.017)			
Nobs	18932	18932	18932	18715	18932	18932	2423	18589	18932

Standard errors in parentheses and stars refer to the signficance of the results: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. R. Wage refers to the relative hourly wage for parents. Nobs refers to the number of observations included. Full estimation results are available upon request.

Table 4: Teacher Strikes and Parental Pay: Mothers

	Regression Specification								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
ψ	0.004	0.004	0.005	0.006	0.003	-0.005	-0.003	0.003	-0.007
	(0.007)	(0.007)	(0.006)	(0.009)	(0.007)	(0.008)	(0.006)	(0.006)	(0.005)
Teachers' Strike Days interacted with									
Child 6-12	0.003	- 0.0004	-0.006	-0.003	0.022	0.025	-0.017	-0.012	0.003
	(0.011)	(0.013)	(0.019)	(0.013)	(0.016)	(0.036)	(0.011)	(0.017)	(0.008)
Child 6-12 x R. Wage	-0.005	0.009	0.017	-0.0003	-0.014	-0.021	0.018	0.015	-0.003
	(0.007)	(0.010)	(0.025)	(0.010)	(0.009)	(0.018)	(0.009)	(0.021)	(0.006)
Child 6-18	0.026*	0.032**	0.031*	0.029*	0.013	0.056	0.037***	0.035**	0.032***
	(0.011)	(0.009)	(0.014)	(0.014)	(0.016)	(0.031)	(0.010)	(0.013)	(0.009)
Child 6-18 x R. Wage	-0.014*	-0.031***	-0.036	-0.019*	-0.005	-0.018	-0.032***	-0.034*	-0.017**
	(0.006)	(0.008)	(0.019)	(0.009)	(0.008)	(0.015)	(0.007)	(0.016)	(0.005)
Child 6-12 x R. Wage^2		-0.006***	-0.009	0.007			-0.006***	-0.006	
		(0.002)	(0.008)	0.014			(0.001)	(0.005)	
Child 6-18 x R. Wage^2		0.006***	0.008	0.007			0.006***	0.006	
		(0.001)	(0.007)	0.014			(0.001)	(0.005)	
Child 6-12 x R. Wage^3			0.0001						
			(0.0004)						
Child 6-18 x R. Wage^3			-0.0001						
			(0.0003)						
Child 6-12 x Mother High Earner					-0.031*				
					(0.015)				
Child 6-18 x Mother High Earner					0.001				
					0.012				
Child 6-12 x Long Strike						-0.028			
						(0.038)			
Child 6-18 x Long Strike						-0.029			
						(0.031)			
Child 6-12 x R. Wage x Long Strike						0.020			
						(0.019)			
Child 6-18 x R. Wage x Long Strike						0.002			
						(0.017)			
Nobs	18932	18932	18932	18715	18932	18932	2423	18589	18932

Standard errors in parentheses and stars refer to the signficance of the results: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. R. Wage refers to the relative hourly wage for parents. Nobs refers to the number of observations included. Full estimation results are available upon request.

Table 5: Teacher Strikes and Parental Pay: Child Gender

		Fathers			Mothers	
ψ	-0.003	-0.002	-0.001	0.001	0.003	0.001
	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.005)
Boys 6-18	-0.019*	-0.029**	-0.014	0.023**	0.023*	0.027*
	(0.008)	(0.011)	(0.011)	(0.008)	(0.011)	(0.011)
Girls 6-18	-0.001	0.009	0.001	0.019*	0.016	0.045
	(0.008)	(0.014)	(0.026)	(0.008)	(0.014)	(0.024)
Boys 6-18 x R. Wage	0.012**	0.018**	0.002	-0.009*	-0.013*	-0.024**
	(0.004)	(0.006)	(0.009)	(0.004)	(0.006)	(0.008)
Girls 6-18 x R. Wage	0.002	-0.001	0.019	-0.013**	-0.010	-0.048
	(0.004)	(0.010)	(0.030)	(0.004)	(0.009)	(0.028)
Boys 6-12		0.015	-0.044*		0.002	0.021
		(0.014)	(0.022)		(0.014)	(0.020)
Girls 6-12		-0.019	-0.028		-0.002	-0.038
		(0.016)	(0.029)		(0.015)	(0.027)
Boys 6-12 x R. Wage		-0.009	0.043*		0.004	-0.011
		(0.008)	(0.019)		(0.008)	(0.018)
Girls 6-12 x R. Wage		0.005	-0.002		-0.003	0.047
		(0.010)	(0.032)		(0.010)	(0.030)
Boys 6-18 x R. Wage^2			0.003			0.004**
			(0.001)			(0.001)
Girls 6-18 x R. Wage^2			-0.009			0.011
			(0.007)			(0.007)
Boys 6-12 x R. Wage^2			-0.006			0.000
			(0.003)			(0.003)
Girls 6-12 x R. Wage^2			0.008			-0.014*
			(0.007)			(0.007)
N		18932			18932	

Standard errors in parentheses and stars refer to the signficance of the results: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. R. Wage refers to the relative hourly wage for parents.Nobs refers to the number of observations included. Trimmed refers to a sample restricted to the 1-99 percentiles of the relative wage. Full estimation results (for the roughly 350 coefficients) are available upon request.