Economic Outcomes of Strikers in an Era of Weak Unions*

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Abstract

From 1970 to 2000, US worker participation in labor strikes decreased by 90 percent. We show using understudied measures from labor market surveys that strikers also experienced worse outcomes after 1981. Event study evidence using the PSID suggests that strikers enjoyed 5-10 percent wage gains prior to the 1980s, but flat wage changes thereafter. Additional analysis of collective bargaining agreements and person-level data from the SIPP and CPS reinforce the finding that strikes since the 1980s have not been associated with increases in wages, hours, or benefits. These findings are consistent with decreased strike effectiveness, perhaps due to employers' higher propensity to hire strike replacements, or with more negative selection into "defensive" strikes that do not allow large pay increases.

Keywords: Strikes, labor unions, collective bargaining

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1 Introduction

The threat of a strike has long been viewed as a fundamental source of workers' collective bargaining power (Kimeldorf, 2013; Card and Olson, 1995). Yet since the 1980s striking has decreased by an order of magnitude, outpacing even the concurrent decline in union membership. When strikes do occur, anecdotal evidence suggests that strike outcomes are often negative—and sometimes ruinous—for workers (Brecher, 2009; Rosenfeld, 2006). As one labor lawyer put it, "Since the eighties, it has been insane to go on strike. Every strike ends in disaster" (Rosenfeld, 2014).

Explanations for the decline of strikes suggest that a major shift in labor relations occurred in the early 1980s, when President Ronald Reagan fired 12,000 striking air traffic controllers in 1981. Researchers have argued that this action increased the use of permanent replacement workers during strikes (Schnell and Gramm, 1994; Cramton and Tracy, 1998), and, more broadly, contributed to a collapse of norms governing the post-war labor relations system (Kochan and Riordan, 2016). Anecdotal accounts from policymakers support this. Reagan's director of the United States Office of Personnel Management at the time reports that businessmen told him, "when your guy, Reagan, stood firm with those guys, I started getting tougher with my unions, too..." (Goldstein, 2019). And Paul Volcker, chair of Federal Reserve at the time, believed that this was "the most important single action of the administration in helping the anti-inflation fight" (paraphrased in Feldstein, 2007).

At the same time, technological change (Kristal, 2019) and sharpened non-union competition (Kochan et al., 1994) weakened unions and ushered in an era of concessionary bargaining. In this context, even a successful strike may leave strikers worse off than prior to the strike, albeit with fewer concessions than had the union capitulated without the strike (Rosenfeld, 2006). So two changes could have contributed to worse outcomes for strikers since the 1980s. Employers' use of replacement workers and hard line response to strikes may have diminished strike effectiveness. And concessionary bargaining may have changed selection into strikes, such that strikes were more likely to be defensive battles over the extent of workers' losses and less likely to conclude with large gains for strikers.

Yet how effective was striking before the clampdown? Despite wide consensus about the crippling of strikes, the evidence that outcomes have changed for strikers is limited. Studies of strikes come from either cross-sectional analyses, which cannot account for unobserved factors driving a correlation between wages and striking, or employ collective bargaining agreements, which may not track the same group of workers consistently. Further, no studies straddle the critical period before and after the 1980s, when strikes began declining precipitously.

In this project, we address these gaps using panel data on individual workers who report whether or not they recently participated in a strike. Our key innovation is that several large labor market surveys include understudied questions about participation in strikes, work stoppages, or labor disputes, within a battery of questions about missing work. To our knowledge, this is the first time that the panel structure of these surveys has been used to estimate the effects of strikes.

We focus on results from the Panel Study of Income Dynamics (PSID), because it allows for a comparison between the 1970s and 1980s. Also, the panel nature of the PSID lets us track strikers over time and assess whether pay changes associated with strikes might be confounded by unobservable worker characteristics or pre-trends. We also report supplementary findings on the post-1980s using the Current Population Survey (CPS), Survey of Income and Program Participation (SIPP), and a collection of strikes and contracts data.

The results confirm the idea that outcomes for strikers have changed markedly. Panel regressions and event study analysis in the PSID suggest that, in the 1970s, striking workers enjoyed a 5-10% average real pay increase following a strike. This pay gain happens sharply in the year of the strike, with no detectable pre-trend, persists for around 4 years after the strike, and is not driven by workers selectively losing their jobs. In the 1970s, striking workers ended up with higher pay than they had prior to striking. In contrast, in the 1980s and afterward, striking workers do not experience wage increases.

We confirm this latter result with panel regressions in the SIPP and CPS: if anything, strikes after 1982 are associated with small declines in pay. And while not all workers strike for higher wages, results from the SIPP show that striking workers also do not gain measurable concessions in the form of hours, job stability, or employer-sponsored health benefits, even when we restrict to those who keep their job following the strike. Finally, using a newly digitized dataset of 26,000 collective bargaining agreements, we find that the wage terms of contracts associated with strikes are no more generous, consistent with a prior analysis of older contracts and strikes with more than 1,000 workers by McConnell (1989).

Our approach addresses several limitations in prior research. First, during the same period that strikes precipitously declined, the main Bureau of Labor Statistics data series tracking strikes was discontinued.[1] As a result, empirical studies were largely limited to strikes involving 1,000 or more workers, which continued to be tracked. In a sample of manufacturing collective bargaining agreements covering more than 1,000 workers, Kochan and Riordan (2016) show that strikes are associated with small wage gains prior to 1980, but not between 1980 and 1984 (the last year of their data).

However, large strikes are not representative of strikes in general across a variety of metrics (Skeels et al., 1988). To address this issue, Rosenfeld (2006) introduces an alternative source of strike data, from the Federal Mediation and Conciliation Service, that includes strikes of all sizes. But it starts only in 1984 and

[1]This is no coincidence: President Reagan permanently replaced PATCO air traffic controllers during their 1981 strike, which historians suggest undermined a strong norm against replacing striking workers (McCartin, 2011). His administration also reduced funding for research tracking strikes (Wallace et al., 1999).

relies on self-reporting from striking labor unions. Ideally, research would use nationally representative data on strikers across all industries and bargaining unit sizes, that allows direct comparison between the pre- and post-1982 eras.

A second empirical challenge is that research on strike outcomes typically studies contract settlements achieved in the wake of a strike (McConnell, 1989; Card, 1990; Kochan and Riordan, 2016). This has two limitations, especially stark during the current era of concessionary bargaining and employers' aggressive anti-union activities. If a strike fails entirely, it can lead to the permanent elimination of a union at a facility. This became more common during the period we study, as employers would sometimes provoke a strike in order to hire replacement workers to decertify the union (Fantasia, 2009). If these cases are excluded for lack of an eventual contract to measure strike settlements, estimates of workers' strike outcomes will be positively biased. At the same time, even if a collective bargaining agreement is ultimately signed, an employer might entirely replace the bargaining unit strikers with permanent strike replacements, as in the famous 1985 P-9 Hormel strike (Moody, 1988). In that case, replacement workers benefited from higher pay in a new contract, but the majority of strikers were never rehired. So tracking wages within a job type, collective bargaining unit or an establishment could give different estimates than tracking individual workers.

Third, studies of person-level data on striking are rare and ours represents the first attempt using individual-level panel survey responses. In the closest paper to this one, Rosenfeld (2006) uses the FMCS data to study the wage changes associated with strikes for workers, operationalizing exposure to strikes at the industry-by-region level and finding null effects on workers' wages.[2] However, this approach assumes that strikes benefit not only striking workers, but also those at other firms in the same industry-region. This aggregation strategy could miss large positive effects for the small subset of workers who actually participated in a strike. We address this limitation in our analysis, by tracking wages and earnings for individual strikers over time.

Taken together, our results indicate that the role of strikes in delivering worker bargaining power changed in the 1980s. This was a critical period for wage structures in the United States. Several influential studies document that during this time inequality began rising (Bound and Johnson, 1992; Katz and Murphy, 1992) and the labor share began falling (Karabarbounis and Neiman, 2014; Elsby et al., 2013). One explanation centers on a decline in worker power, brought about in part by a policy environment that was less supportive of unions (Stansbury and Summers, 2020). Similarly, Lemieux (2008) and Piketty and Saez (2006) highlight the role of norms and wage-setting institutions in driving the rise in inequality.

Apart from a decline in union membership (Card et al., 2004), evidence on these mechanisms is indirect.

[2]Other work uses an even more aggregated approach, studying year-to-year variation in strike participation for the entire economy (Wallace et al., 1999).

Since a decrease in strike efficacy makes the threat of a strike less potent, our study provides direct evidence on a decline in worker bargaining power as suggested by Stansbury and Summers (2020). And while further research is needed to determine if the decline in strike efficacy resulted from an increased use of strike replacements inspired by the PATCO events, our findings suggest that firms' treatment of strikers changed markedly in the early 1980s. Indeed, the use of temporary help workers skyrocketed shortly after the PATCO strike, nearly doubling from 1982-1985 (Carey and Hazelbaker, 1986). This suggests that, separate from technology-based explanations, norms may have been significant determinants of worker pay trends (Lemieux, 2008).

Our study also helps arbitrate between explanations for why strike frequency has declined (e.g., see the discussion in Rosenfeld (2014)). If the strike decline is a result of excessively timid labor union leaders (Burns, 2011), then the strikes that do occur should be associated with gains for workers. Because we find that workers on average do not gain higher pay or amenities by striking, it is more likely that employer use of strike replacements or the general labor relations environment makes successful strikes difficult.

2 The decline of strikes

We first document the decline in strikes and shifting patterns of worker participation. We then turn to studying the changing wage effects of strikes, using panel data on workers and collective bargaining agreements.

To track the decline of strikes, we use Current Population Survey Annual Social and Economic Supplement (CPS ASEC) and Outgoing Rotation Group (CPS ORG) samples that include responses about work stoppages that had not been previously analyzed. Specifically, we study answers to a common question about reasons for recent missed work, which includes an option for work missed due to a work stoppage or labor dispute. Questions for missed work ("What is the main reason [this person] was absent from work last week?") and reduced work hours ("What is the main reason [this person] worked less than 35 hours last week?") both include "labor dispute" as a potential response (along with more common reasons, like illness and lay-off). This method of identifying striking workers has not been used in prior research, despite the long-run availability of these questions in standard labor market surveys.

Figure 1 shows that the trend in incidence of strike participation identified with this survey-based method is very similar to that identified with Bureau of Labor Statistics (BLS) reports or Federal Mediation and Conciliation Service (FMCS) administrative records: after a strike wave in the late 1960s and 1970s, strikes plummeted in the 1980s and have declined further since then. Specifically, both the BLS and CPS ASEC sources point to around a 90 percent decline in worker participation in strikes, from 1970 to 2000. Note that this decline is not simply due to a decline in the size of strikes: Figure B.1 shows that the rate of strikes across all size categories has fallen consistently.

Unlike the FMCS and BLS administrative data, which are strictly establishment-level, these household labor market surveys provide data on characteristics of the individual workers involved in strikes. This approach thereby allows the first analysis of the demographic composition of strikers and its change over time. To do this, we focus in on data from the CPS ASEC. The CPS ASEC asks respondents a series of questions about income and work, including the critical question about missed work due to labor disputes. These data provide the longest running continuous survey data that identifies participation in a work stoppage.

In order to separate shifts among strikers from changes in the composition of American workers, we estimate the following regression within different periods for survey respondents who are in the labor force (i.e., employed or unemployed):

$$Covariate_{it} = \alpha + \beta * Strike_{it} + \delta_t + e_i \tag{1}$$

where $Covariate_{it}$ is person i's trait (e.g., age, indicator for college degree) in year t, $Strike_{it}$ is an indicator for reporting a strike, and δ_t indicate year fixed effects to capture any broad time trends.

In Figure 2, we report point estimates and confidence intervals of β for several covariates grouping the 1960s, 1970s, 1980s, 1990s, and 2000s or later. We divide β by the mean of the outcome among non-striking workers in that time period and multiply by 100 so that the coefficient is interpretable as the percent difference in that trait between strikers and the rest of the workforce.

Panel (A), showing results for age, suggests that strikers were almost 10 percent older than the rest of the workforce in the 1980s. Since 2000, in contrast, striking workers have been 10 percent younger. Next, in Panel (B), we show that strikers are consistently 40-80 percent less likely than other workers to have a college degree. Panel (C) suggests that strikers were around 50 percent more likely to be Black prior to the 1990s. After that, point estimates are positive but the confidence intervals include broad ranges due to a small number of striking black workers in the survey. Plots (F), (G), and (H) show that strikers in recent years are relatively more likely to be lower in the income distribution, work in the public sector, and to be female. These patterns are consistent with unionization shifting to public sector, and typically female, occupations, along with the growing concentration of union activism in relatively low-paid service jobs.

3 Wage outcomes of strikes

3.1 Person-level data

Next, we discuss strike outcomes for workers in the Panel Survey of Income Dynamics (PSID) and Survey of Income and Program Participation (SIPP). Data from the Current Population Survey (CPS) provides another opportunity to study outcomes for strikers in a panel setting, but these results mainly serve to confirm findings in the first two datasets; we discuss them in Appendix A.

3.1.1 PSID

The Panel Survey of Income Dynamics (PSID) is the longest running nationally representative longitudinal household survey, administered annually from 1968 to 1997 and biennially thereafter (ISR, 2019). Household members are asked about key economic outcomes over the past year, including employment status and earnings. Beginning in surveys covering 1976, the PSID asked the following question about strike activity: "Did you miss any work in [year] because you were on strike?"[3] We use this variable to measure strike participation. Table C.1 shows descriptive statistics for strikers and non-strikers across the two periods we study. Notably, about 20 percent of strikers in the PSID belong to the public sector. Figure B.2 shows that the PSID strike measure echoes the sharp decline in strike incidence documented in Figure 1

First we ask whether, conditional on controls and person fixed effects, wages tend to be higher after a respondent reports going on strike. We measure hourly wages directly for workers paid by the hour. For workers paid an annual salary, we calculate their hourly wages by adjusting for weeks worked in the last year and hours worked in a usual week.

Importantly, we allow the effect of strikes to vary according to whether the strike occurred before or after 1982, the year immediately following the salient PATCO air controllers strike and around when strike activity sharply declined. We estimate the following model using ordinary least squares, predicting hourly wages, y_{it}:

$$log(y_{it}) = \alpha_i + \beta_0 * poststrike_{it} + \beta_1 * poststrike_{it} * pre1982_i + \boldsymbol{w'_{it}}\boldsymbol{\gamma} + \epsilon_{it}, \tag{2}$$

where α_i denotes person fixed effects, $poststrike_{it}$ is an indicator that switches from 0 to 1 the year that a respondent goes on strike, $pre1982_{it}$ is an indicator equal to 1 if the respondent's (earliest) strike happened before 1982, and $\boldsymbol{w'_{it}}$ gives a vector of time-varying controls including age, age squared, tenure, education level, and union membership, along with fixed effects for industry and state. The standard errors are clustered

[3] In 1976, this is question D46 (variable 4502). Prior to this, the PSID asked if the respondent was *either* unemployed or on strike in the previous year, so the question is not usable. Another question asks whether someone lost their job due to a strike, but this of course conditions on job loss.

at the person level. Finally, we study the short-term and long-term outcomes of strikes, including observations from strikers who are either within 1 year or 5 years of their strike.

Table 1 shows the results of this estimation. The first two columns show the 1-year window for strikers with and without more stringent controls; the third and fourth columns show these same specifications but using a 5-year strike window. At the bottom of the table, we include a count of the number of strikers in the specification to track how many are omitted due to missing controls. Across columns, the post-strike indicator is small and insignificant, suggesting that strikes after 1982 have not been associated with wage increases. In the most stringent specifications, the 95 percent confidence intervals reject wage increases above 2-3 percent.

The interaction term, however, shows a consistently positive wage gain for pre-1982 strikes, suggesting a wage increase of 7 percent in the stringent 1-year specification and 5 percent in the stringent 5-year specification. This suggests that workers striking before 1982 enjoyed concessions in the form of higher wages.

We choose the 1982 cutoff based on the historical literature emphasizing a dramatic break in employers' reaction to strikes following Reagan's hiring of permanent replacement workers in the PATCO strike. However, this incident occurred amidst a longer decline in labor union membership, so the differences we see before and after 1982 could also reflect a gradual worsening in strike outcomes throughout the period. To test whether the change in strike outcomes occurred specifically following PATCO, we replace the period dummy in Equation 2 with indicators for each calendar year. Figure 3 shows that strike outcomes prior to 1982 were consistently positive and show no systematic decline. The pattern for strikes after 1982 is far noisier, consistent with the decline in strike incidence documented above. However, the coefficients, especially in the remainder of the 1980s, tend to be close to zero. This suggests that the early 1980s was indeed an important moment for changing norms around strikes.

The PSID is a longitudinal sample, with most respondents surveyed for multiple years. We can thus estimate an event study to test whether pre-existing trends in wages may be affecting the estimated effects in Table 1. We estimate the dynamic effects from Equation 2 by substituting the standard event time indicators for the $poststrike_{it}$ indicator:

$$log(y_{it}) = \alpha_i + \sum_{k \in S} \delta_k^0 \mathbf{1}(t = k) + \sum_{k \in S} \delta_k^1 \mathbf{1}(t = k) * pre1982_i + \boldsymbol{w_{it}'\gamma} + \epsilon_{it}, \tag{3}$$

The terms from Equation 2 are defined similarly. The set S counts years since the respondent's first strike, binned at 5 years before and after the strike and omitting -1, the year before the strike. The series δ_k^0 give the standard event study coefficients for strikes occurring after 1982, where δ_k gives the implied change in wage k years after a strike. The series δ_k^1 gives the differential effects for pre-1982 workers.

We show the results in Figure 4. The point estimates for event times less than zero suggest that neither group of strikers shows evidence of pre-trending in the years leading up to their first strike. At year 0, the year of the strike, pre-1982 strikers experience a large 10 percent spike in wages, while post-1982 striker wages remain flat. The positive effect for earlier strikers seems to fade out by four years after the strike. At that point, post-1982 strikers show evidence of wage losses.

These results establish a sharp increase in wages for workers who go on strike—but only for those who do so prior to 1982. We next test alternative explanations for these findings. First, some workers switch jobs following a strike. In the Appendix, we test whether these respondents are driving the results by studying a subsample of firm stayers. The results in Table C.2 use the same setup as Equation 2 and shows that the difference between the pre-1982 and more recent strike outcomes is not driven by workers differentially switching employers following strikes across the two periods. Even focusing on stayers, striking workers experienced wage gains in the pre-1982 period, but not afterward.

We also investigate the role of non-employment. Our main specifications study log wages, so workers with no wages are dropped. This could complicate the interpretation if striking workers are likely to be fired (and disproportionately so before 1982). Table C.3 shows that strikers are no more likely to end up non-employed than other workers, but that the 1970s strikers may have had slightly negative employment outcomes. In Table C.5, we test how this changes the overall effect on wages by assigning zero wages to non-employed workers and using the level of wages as the outcome. The results suggest that the period difference in strike outcomes is not offset by differences in employment rates following the strike. The non-logged models are noisier but the interacted coefficient is consistently positive and, in the 5-year window, significant at the 0.05 level.

Another simple explanation for the change in striker outcomes around 1981 is shifting industry composition of strikes. For example, if strikes consistently bring wage gains in manufacturing, but not in other industries, then shifting employment of workers in manufacturing could explain changing strike outcomes. In Table C.4, we control for the interaction between striking and (a) whether the strike took place at a public sector employer and (b) whether the strike took place at a manufacturing employer. The results show that even controlling for these prominent industry and sectoral changes in the composition of union members, a substantial change in strike outcomes after 1981 persists. Both public sector and manufacturing strikes are, if anything, less likely to bring wage increases than strikes in other industries.

Together, these results provide some of the strongest evidence that workers—at some point—benefited from strikes. The pre-1982 strikers in the PSID report a clear and sharp increase in wages.

3.1.2 SIPP

One possible explanation for the results in the PSID is that strikes after 1982 were still just as effective as before, but the demands of workers shifted to job amenities such as health insurance, hours, or employment stability instead of wages (Rosenfeld, 2014). In this section we turn to the Survey of Income and Program Participation (SIPP) to identify whether strikes are associated with any positive changes in these outcomes.

The SIPP is a collection of 3-4 year panels meant to better track employment dynamics and program participation. Respondents are surveyed every four months, and questions include a weekly accounting of employment status along with hourly wage and (for later panels) job identifiers. We use 15 panels spanning 1984-2010.

In every panel, the SIPP includes a question on the reason for not being at work with an option to indicate a labor dispute. Respondents are only asked this question if they report being absent for a week or more, which means the SIPP will not record workers who went on strikes lasting fewer than five days. SIPP surveys are performed every four months (each four month period is called a wave), and "seam effects" tend to cause a bunching of changes in reported status right at the first month of each wave (Fujita and Moscarini, 2017). In order to avoid these issues, we collapse the data down to the person-wave level, so that each observation for a given respondent represents an average taken across the constituent months of the wave. This yields an average of 7 observations per respondent. Descriptives for this data are in Table C.6, split by striking status. Only 35 percent of striking workers in this sample report being in a union, although this is six times larger than the share for non-striking workers.

We use a similar fixed effect specification as in Equation 2, except with an added indicator for being on strike to isolate the lost earnings from missing work:

$$y_{it} = \beta_0 * poststrike_{it} + \beta_1 * onstrike_{it} + \boldsymbol{w'_{it}}\boldsymbol{\gamma} + \alpha_i + \epsilon_{it}. \tag{4}$$

In this setup, y_{it} is either log earnings, weekly hours, an indicator for not being employed, or an indicator for the employer paying partially for insurance. We report estimates of β_0 and β_1 in Table 2. Column (1) includes all strikers. To separate out any effects on employment, column (2) retains only strikers who are still at their previous job. And to address the potential for misreporting, column (3) retains only strikers that reported being in a union. Each specification has fixed effects for person and period (i.e., SIPP wave), and controls for age and education. Columns (2) and (3) restrict to surveys done in 1990 or after because the measures of same job and union status are not reliable before then.

Beginning with the top row of findings for log earnings, the results suggest a consistent 20 percent dip in earnings for respondents on strike across samples. However, the indicator on $poststrike_{it}$ finds no significant wage benefits as in the post-1982 PSID sample, with the upper end of the 95 percent confidence interval ruling out increases above 2-3 percent, depending on the sample.

The results for job amenities similarly suggest no benefits to striking. Hours show no consistent change, and workers are 6-8 percent more likely to be unemployed after striking. Finally, workers are less likely to have employer-sponsored health insurance after a strike, even restricting to the workers who stayed at their previous job.

3.2 Collective bargaining agreements

Data from collective bargaining agreements provides another window into the average wage outcomes of strikes. We noted the disadvantages to this approach in the introduction. The advantages of using these data are that they explicitly spell out the raise schedule achieved in the wake of a strike and they are not vulnerable to the attrition that results if a worker in a panel survey switches jobs during a strike.

To build an updated dataset of strikes and collective bargaining agreement outcomes, we combine two establishment-level sources of data. First, we obtained data on all strikes reported to the Federal Mediation and Conciliation Service (FMCS) from 1984 to 2018. These data should cover strikes authorized by unions covered by the National Labor Relations Act (Rosenfeld, 2006). Thus, in contrast to the data used by McConnell (1989), these data will cover strikes of less than 1,000 workers. However, because the FMCS relies on self-reporting from striking labor unions, the data are likely incomplete. They are also missing unauthorized or "wildcat" strikes, strikes in the railway and airline industries and most public sector strikes. The FMCS data indicate the start and end date of each strike, along with employer name, city, industry and number of striking workers.

Next, we use Bloomberg BNA data on collective bargaining wage settlements. These settlement data are gathered from press reports, direct BNA reporting, and official union publications. These reports cover a broad range of collective bargaining agreements, but likely overrepresent larger and more prominent bargaining settlements. The raw BNA data include text entries specifying the annual wage increases given in each contract. We use a combination of text processing and hand coding to derive percent and dollar amount changes from these text entries.

We merge these contract settlement data into the FMCS work stoppages data, using a fuzzy string matching algorithm on the listed name of the business within states. We successfully match 751 of the 14,000 strikes to the 27,000 BNA collective bargaining settlements. Note that this low merge rate is mainly due to the small true intersection between the data sources: the BNA data only include a fraction of collective bargaining agreements and the vast majority of agreements are settled without a strike. Highly probable contract-strike matches are included in the analysis if the strike began two years before or one year after the effective date of the contract.[4] As evidence of the success of the match, Figure B.3 shows that the majority of strike matches identified this way—and without any additional conditioning on date besides the multi-year window—are within 60 days of the effective date of the contract.

Table C.7 shows descriptive statistics for all the collective bargaining agreements in the BNA data, split by whether we matched the contract to a strike within one year of the contract effective date. The means suggest that struck firms are more likely to be in manufacturing and healthcare, and are slightly smaller in terms of the number of workers covered by the agreement.

Most of the BNA data reports raises in terms of either the percentage or dollar raise each year, and the contracts vary in length. The raises reported in dollar terms are more difficult to analyze because base wages are not recorded (c.f. McConnell, 1989). We present analyses using percent or dollar raises separately.[5] As in McConnell (1989) and Card (1990), we incorporate information on expected inflation in our main outcome, noting that the results are stable across several other techniques for distilling the raise schedules into one number.

We calculate the expected real growth rate of wages for percent-coded contracts as follows. Each year, the real growth of wages is the percent increase in wages minus the expected percent increase in the CPI as of the year of the contract. We use these growth rates to calculate the total percent change in real wages over the length of the contract. Then, the average real annual growth is given by the compounded growth rate necessary to achieve this increase over the contract length. This calculation ensures that contracts of differing length are comparable. Analyses of contracts of dollar raises are performed separately, where each year's raises are deflated according to expected inflation at the time of the contract signing and then averaged. We also report model estimates which use as the outcome the nominal percent raise in the first year of the contract.

Figure 5 shows the raw average of nominal percent wage changes for BNA-reported agreements with and without an associated strike. While the data exhibit some cyclicality, with low wage settlements in the early 1990s and the Great Recession, the two series track each other quite closely. Judging from these simple averages, wage agreements settled following strikes do not appear different than those settled through negotiation alone.

However, this leaves open the possibility that strikes drive up the wages on agreements that, based on observable and unobservable characteristics, are predicted to be lower. Without exogenous variation in

[4] In Figure B.4, we show that the results are not sensitive to the window used to define the close to strike variable.

[5] McConnell (1989) takes a different approach by directly including information on base wages from contracts, or, when that is not available, industry averages, and combining base wages, the raise schedules, and price level data to infer the course of real wages for each bargaining pair.

strikes, the association between wage schedules and an indicator for strikes could be biased. For instance, a low initial offer by the firm could increase the probability of a strike and the probability of a low wage settlement, even if the strike substantially increased the wage agreement from the initial offer. This might cause us to spuriously record a negative effect of strikes on wages.

We follow McConnell (1989) in testing the relationship across a series of increasingly stringent specifications. In particular, firm fixed effects should capture time-invariable unobserved factors that may be associated with increased strike probability and decreased wages, and state-by-year fixed effects control for economic conditions. We employ the specification below, estimated using OLS:

$$y_{it} = \beta x_{it} + \boldsymbol{W'_{it}\gamma} + \delta_i + \epsilon_{it}. \tag{5}$$

In this setup, y_{it} is the wage outcome for firm i in year t, x_{it} is an indicator for whether firm i had a strike in year t, $\boldsymbol{W'_{it}}$ is a matrix of controls, δ_i denotes firm fixed effects, and ϵ_{it} is the residual. Standard errors are clustered at the firm level.

The results in the top row of Table C.8 show results using the expected real growth of wages as the outcome and displaying the coefficient β on the indicator for whether a strike occurred at the establishment. All regressions include as controls the log of workers covered by the agreement and indicators for: lump sum payments or bonuses, other provisions, a retroactive contract, an "overterm" contract, the source of the information (BNA or union publication), and the contract length. In addition to these contract controls, Column (1) includes year and state fixed effects; Column (2) adds state-by-year and state-by-industry fixed effects; Columns (3)-(6) report increasingly stringent specifications all with firm fixed effects. Across columns we lose observations from firms and covariate cells that only appear once, where the biggest drop from column (2) to (3) is from the addition of firm fixed effects.

Together, these specifications reinforce the picture from the raw averages shown in Figure 5, suggesting that strikes are not associated with increased wages. Across specifications, the point estimates are consistently small, suggesting a difference in percent wage changes between -0.07 and 0.05 percent. For the inflation-adjusted measure in the top row, the upper end of each 95% confidence interval rules out increases in the real expected wage of .25 percent. Results in the next two rows, which use the percentage change in the first year or the deflated average dollar raise, are similar.

4 Conclusion

Our analysis finds that, since 1982, striking has not been associated with increased wage settlements. Even comparing individual striking workers before and after their strike activity indicates no sizable increase in wages or benefits following a strike. Relative to similar workers in the same industries and regions, strikers end up with no higher pay after striking. Workers who strike lose labor earnings during the strike, but have no higher wages upon returning to work. Contrary to the theory that union leaders are under-striking, strikes do not seem to lift the constrained wage growth typical of unionized sectors during this period.

The key limitation of our study is that we cannot fully address union selection into strikes. First, previous research on the determinants of strikes finds that market concentration and labor market tightness influence the probability of a strike (Abowd and Tracy, 1989; Tracy et al., 1986). We attempt to account for this in both the individual and contracts analysis with the state-by-industry fixed effects, although this could still leave local omitted factors affecting both the probability of a strike and the wage settlement.

At a more granular level, we cannot check for the possibility that unions are striking in response to time-varying omitted variables, for example an aggressive initial employer bargaining offer. In this case, seemingly indistinguishable wage settlements following strikes relative to non-strike settlements may in fact be the result of increased wages relative to that starting offer. Put differently, our estimates are consistent with positive strike effects on wages, coupled with negative selection into strikes—with selection into striking changing in the early 1980s. And this is likely to be a persistent problem in studies of strikes: even if the initial offer was observed, it could still be argued that some *intended*—but still unobserved—offer was averted by the threat of a strike.

Notwithstanding this limitation, we make two main contributions. First, we update an older labor relations and labor economics literature on the business and wage effects of strikes. In labor economics, several studies have modeled wage effects of strikes. But, these studies rely on wage data from collective bargaining agreements (McConnell, 1989; Card, 1990). While we replicate this approach in our study, we also consider the effect of strikes on individual workers who participated in a work stoppage. This individual-level panel approach allows us to distinguish effects on strikers' wages from worker turnover and workforce composition changes that may happen during the course of a strike. By observing real, respondent-reported wages, in addition to formal collective bargaining agreements, we also avoid possible measurement error arising from contract implementation. After 1982, strikes are associated with neither stronger wage settlements nor with higher real wages or earnings among workers affected by a work stoppage.

Second, we contribute to the broader labor economics literature on rent-sharing, inequality and declining worker bargaining power. Aggregate data show median wage stagnation and declining unionization. We focus on the main mechanism through which unions can force employers to share rents or otherwise increase pay: strikes. By showing that higher wage settlements no longer followed strikes in the more recent period, we provide evidence in support of the broader argument that worker bargaining power has declined.

Strikes may still be costly for employers, which would preserve the strike threat as a potential means through which unions can win higher wages in negotiations. However, if workers do not see tangible improvements in pay and working conditions following strikes, they are less likely to take strike action. This may ultimately undermine the credibility of unions' strike threats and contribute to declining worker bargaining power.

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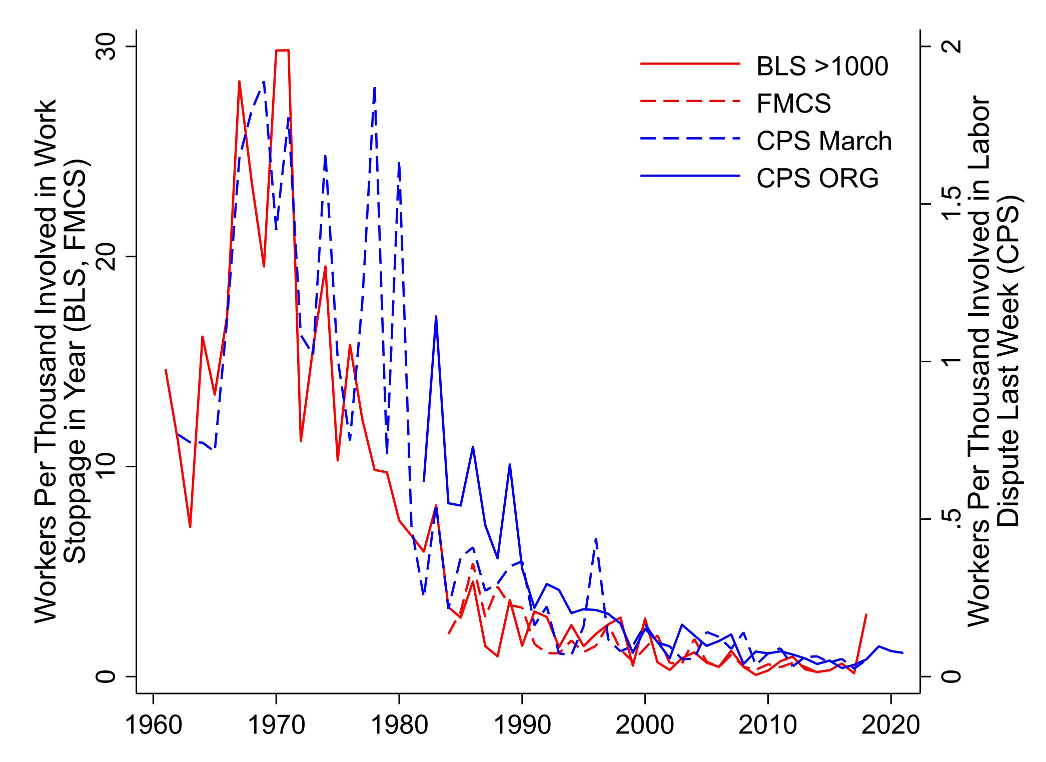
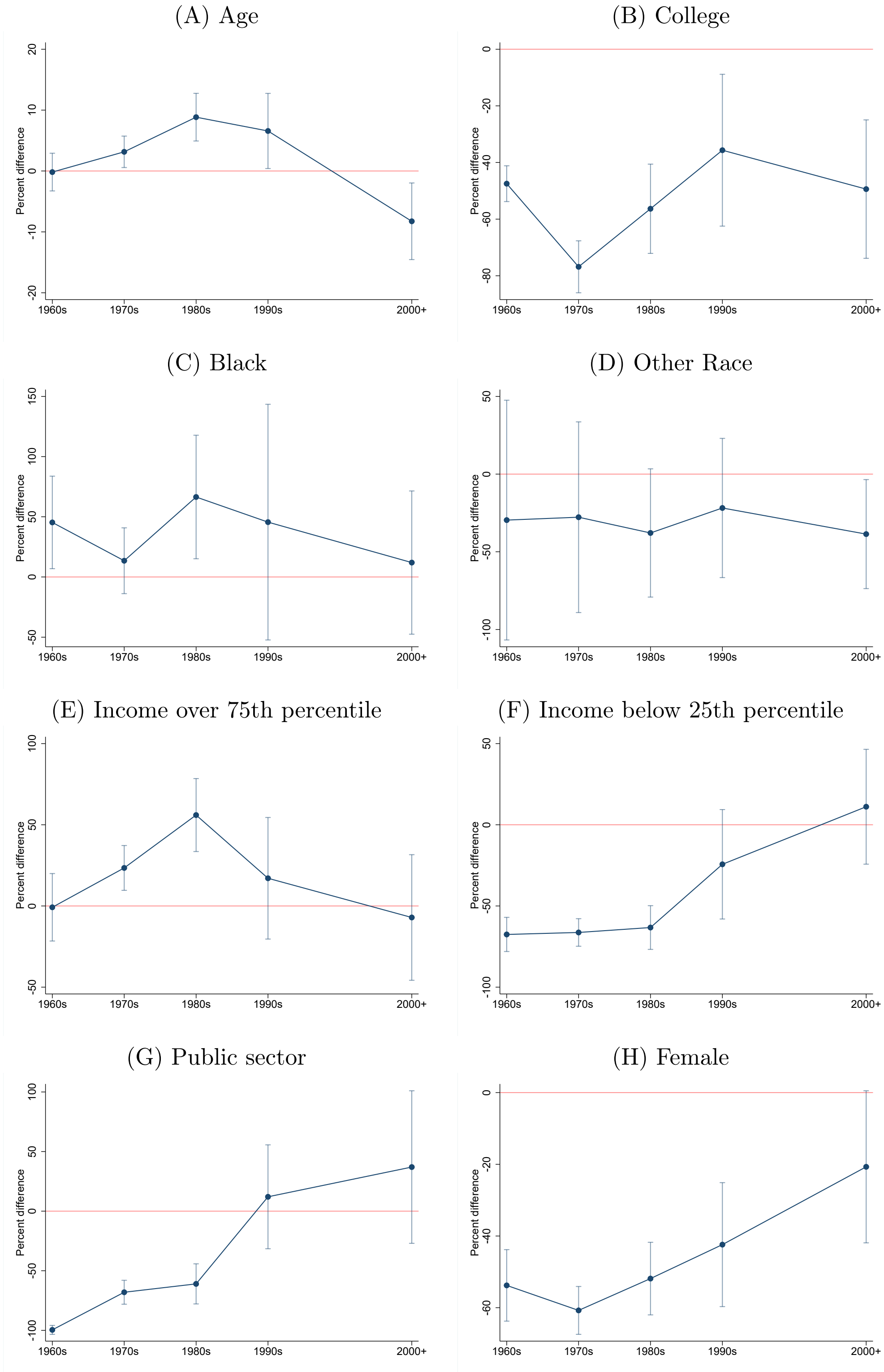
5 Figures

Figure 1: The Decline of Strikes



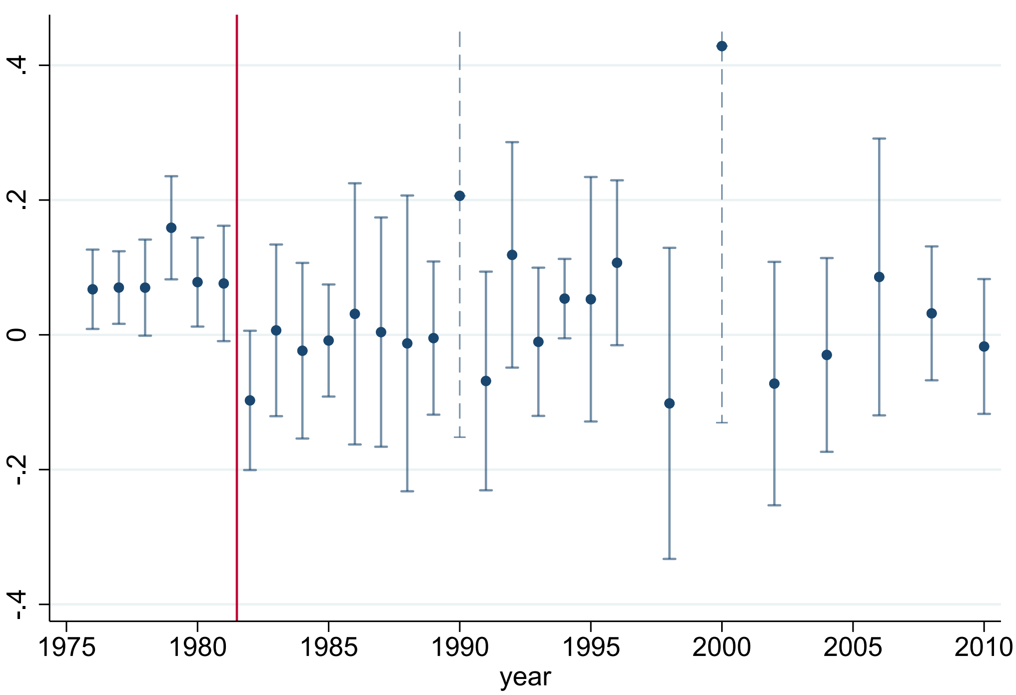
Source: CPS and BLS Work Stoppages Data. Note that BLS figures only cover workers in stoppages involving at least 1000 workers. CPS figures refer to the week preceding the survey; BLS and FMCS figures refer to a calendar year.

Figure 2: Selection into striking, CPS ASEC



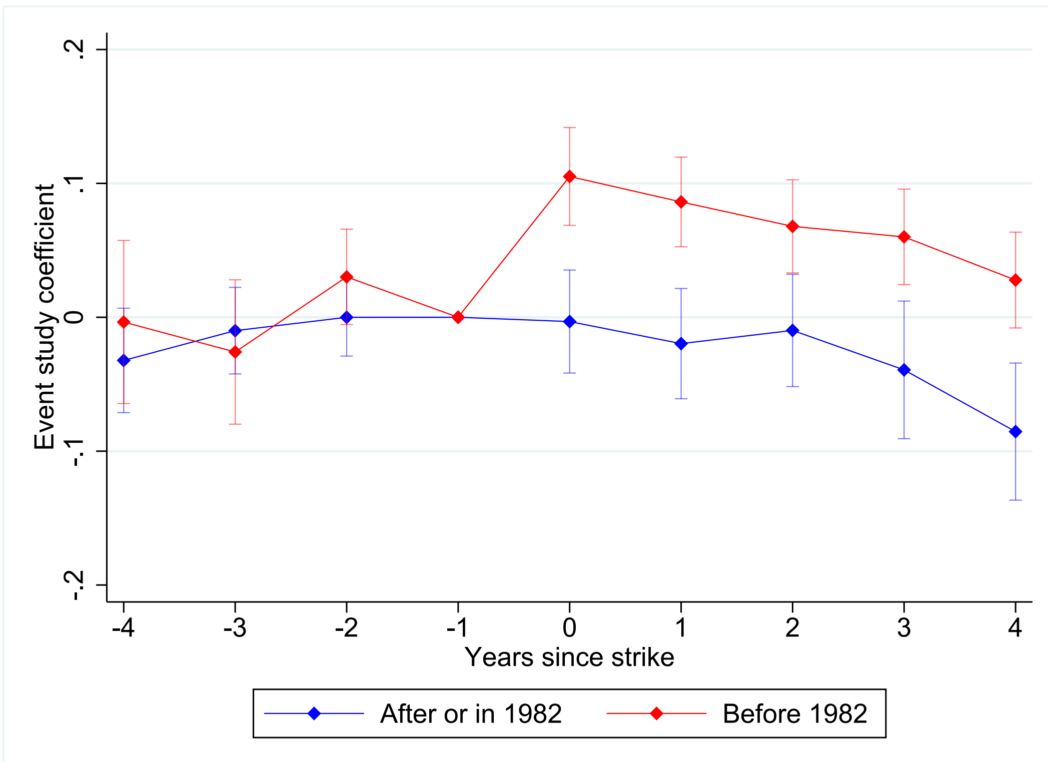
Note: This shows regression estimates with the listed covariates as outcomes (Age, indicator for college degree, etc.) and an indicator for strike as the explanatory variable, controlling for year fixed effects. We use workers in the CPS ASEC. We divide the coefficient by the mean of the outcome among non-striking workers in that time period so that the coefficient is interpretable as the percent difference in that trait between strikers and the rest of the workforce.

Figure 3: Wage Outcomes of Strikes by Year



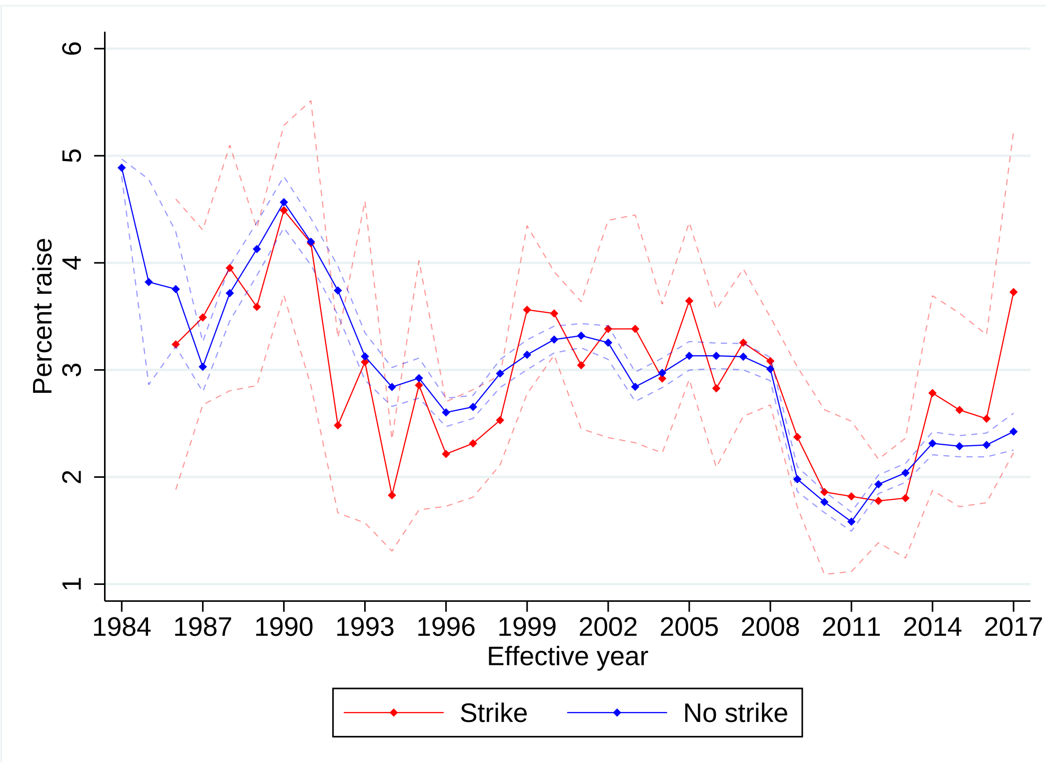
Note: This figure shows difference-in-difference estimates of strike outcomes on hourly wages for each calendar year available in PSID. Two very large confidence intervals are capped for legibility (dashed lines). Standard errors clustered at the person level. Source: PSID.

Figure 4: PSID event study estimates for strikers, split by year of strike



Note: This figure shows estimates of event study coefficients from the model specified in Equation 3. The blue line and whiskers show the point estimates and 95% confidence interval for the dynamic effects δ_k^0, describing the trajectory for workers who went on strike after or in 1982. The red line and whiskers show the point estimate and 95% confidence interval for $\delta_k^0 + \delta_k^1$, tracing the wage trajectory for strikers prior to 1982. Standard errors clustered at the person level. Source: PSID.

Figure 5: Average percent raises for strike and non-strike collective bargaining agreements



Note: This figure shows the average percent raise across all years of the contract for collective bargaining agreements (CBAs) from the BNA database, split by whether or not the CBA was matched to a strike. Dashed lines give the 95 percent confidence interval for the estimated mean. The BNA data is sparse before 1986; there were no matching strikes for those years.

6 Tables

Table 1: Wage Outcomes of Strikes, Before and After 1982

	+/- 2 year window		+/- 5 years window	
	(1)	(2)	(3)	(4)
Post-strike	0.01	-0.01	0.02	-0.03
	(0.02)	(0.02)	(0.02)	(0.02)
Post-strike * Pre-1982 strike	0.08***	0.07***	0.08***	0.07***
	(0.02)	(0.02)	(0.02)	(0.02)
Age		0.06***		0.06***
		(0.00)		(0.00)
Age2		-0.00***		-0.00***
		(0.00)		(0.00)
Tenure		0.01***		0.01***
		(0.00)		(0.00)
Years of education		0.06***		0.06***
		(0.01)		(0.01)
Union membership		0.12***		0.12***
		(0.01)		(0.01)
Fixed effects:				
Year	Yes	Yes	Yes	Yes
Worker	Yes	Yes	Yes	Yes
Region X Industry.		Yes		Yes
R^2	0.69	0.78	0.69	0.78
N strikers	914	662	939	675
Observations	182588	108506	185563	110912

Note: Outcome is logged hourly wages. Sample includes all observations of non-striking workers. Strikes are defined as missing work due to participation in a strike anytime during the previous year. Post-strike is a period including the year of the strike and the period after the strike. Specifically, columns (1) and (2) retain observations from striking workers within 1 year of their strike; in columns (3) and (4), the window is extended for 5 years around the strike. Models compare strike outcomes before and after 1982. Sample size and number of strikers varies across models due to the exclusion of singleton observations from the fixed effects estimation. Standard errors are clustered at the respondent level.
Source: PSID.
* $p < .05$; ** $p < .01$; *** $p < .001$ (two-tailed tests)

Table 2: Labor market before and after strikes in the SIPP

	(1)	(2)	(3)
	Ln(earnings)		
On strike	-0.181***	-0.170***	-0.210***
	(0.023)	(0.025)	(0.035)
Post strike	-0.023	-0.024	-0.031
	(0.022)	(0.024)	(0.029)
Outcome mean	7.96	8.06	7.99
R-squared	0.817	0.828	0.814
	Hours		
On strike	-0.666*	0.225	0.109
	(0.395)	(0.550)	(0.667)
Post strike	-0.796*	0.520	0.605
	(0.434)	(0.459)	(0.731)
Outcome mean	39.11	39.55	39.09
R-squared	0.723	0.725	0.717
	Not employed		
On strike	-0.047***		-0.039***
	(0.009)		(0.012)
Post strike	0.076***		0.056***
	(0.013)		(0.016)
Outcome mean	0.39		0.39
R-squared	0.811		0.807
	Employer pays for insurance		
On strike	-0.019	-0.078***	-0.017
	(0.013)	(0.016)	(0.022)
Post strike	-0.056***	-0.046***	-0.060***
	(0.014)	(0.014)	(0.022)
Outcome mean	0.38	0.56	0.38
R-squared	0.735	0.726	0.729
N	5,089,397	2,450,810	4,231,825
N strikers	970	363	330

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: This table shows estimates of strike effects for respondents to the SIPP using Equation 4. Column (1) includes all strikers; column (2) retains only strikers who are still at their previous job; column (3) retains only strikers that reported being in a union. Each specification has fixed effects for person and period (i.e., SIPP wave).

A CPS results

A.1 Data and methodology

The Current Population Survey (CPS) is a monthly panel in which a household is surveyed for each of 4 initial months, is out of sample for 8 months and is then surveyed again for 4 additional months. In each month, employed respondents are asked whether they missed work in the last week. Those that did are asked why. The vast majority of respondents missed work due to vacation, sickness or parental leave. But, the survey also allows respondents to note that they missed work due to a "Labor dispute". This option has been available on CPS surveys since 1962, but as far as we know it has never been used as a measure of strike participation. Using this survey question (along with an analogous question asking about partial absences) allows us to provide the first individual-level analyses of wage effects of striking.

We focus on wage changes around strikes by using the CPS Outgoing Rotation Group (ORG) panel data (1982-2021). During the 4th and 16th months of CPS survey participation, respondents enter the Outgoing Rotation Group and are asked questions about wages and earnings. For workers who are paid hourly, we calculate weekly earnings by multiplying by workers' last week's hours; for workers paid weekly, we calculate hourly wages by dividing by usual weekly hours. Very low earnings (below $1 in hourly terms are dropped). Top-coded observations are replaced with a Pareto imputation (Western and Rosenfeld, 2011). We drop extreme values created by this approach above $200 per hour. We predict wage changes by comparing wages reported in the 4th month to those reported one year later when a worker reports a strike during any of the intervening survey months.

Specifically, we use the following wage equation to estimate the effect of strikes on wages in the CPS, estimated using OLS and analogus to Equation 2 except without a pre-1982 interaction:

$$log(y_{it}) = \beta * poststrike_{it} + \boldsymbol{w'_{it}}\boldsymbol{\gamma} + \alpha_i + \epsilon_{it}. \tag{6}$$

In this setup hourly or weekly earnings y_{it} for worker i in year t are predicted by absence from work due to a labor dispute during the preceding year, x_{it}. The one year panel in the CPS outgoing rotation groups allows us to add worker fixed effects α_i and compare earnings before and after the labor dispute-related absence. In $\boldsymbol{w'_{it}}$, we include a series of controls for observable characteristics: union membership, education, age, age^2 and a combined year-region-industry-occupation fixed effect. These controls allow us to compare the change in earnings following a labor dispute, relative to changes in earnings for very similar workers who do not experience a strike.

A.1.1 Results

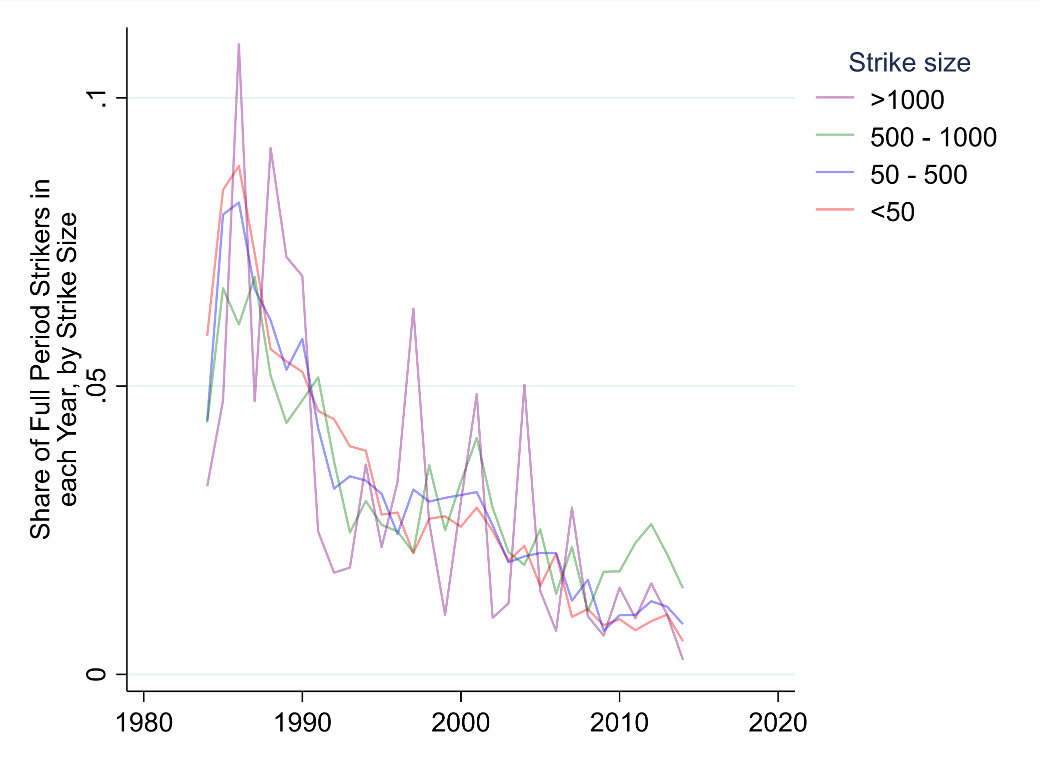
Table C.9 shows the main findings from the CPS. Model 1 compares hourly wages for workers before and after a labor dispute. The point estimate is slightly negative. Results are similar in column (2), which adds controls for union membership, education and age. Column (3) adds a set of fixed effects for year, region, industry and occupation. This allows us to compare wage changes for strikers to non-striking workers in the same type of job and the same geographical area. Column (3) similarly shows no significant positive wage effects. Overall, the consistent null effects from these hourly wage models indicate that strikes are not associated with above average wage changes. Across the models, we can rule out abnormal wage increases above 1-2%.

The second set of models predict usual weekly earnings. Shifting to weekly earnings allows us to add non-hourly workers. Here, the results are even more striking. Across models with controls and the job fixed effect defined above, strikes are associated with declines in usual earnings, relative to similar workers. Far from strikes leading to excessive wage settlements, these individual-level panel data suggest that if anything pay raises determined by strikes are actually slightly lower than average.

Together, these null results across establishment- and worker-level panel rule out substantial average wage increases associated with striking. Relative to similar collective bargaining agreements, agreements forged following a strike include no more generous wage settlements. Relative to similar non-striking workers, strikers do not receive higher wages following a strike.

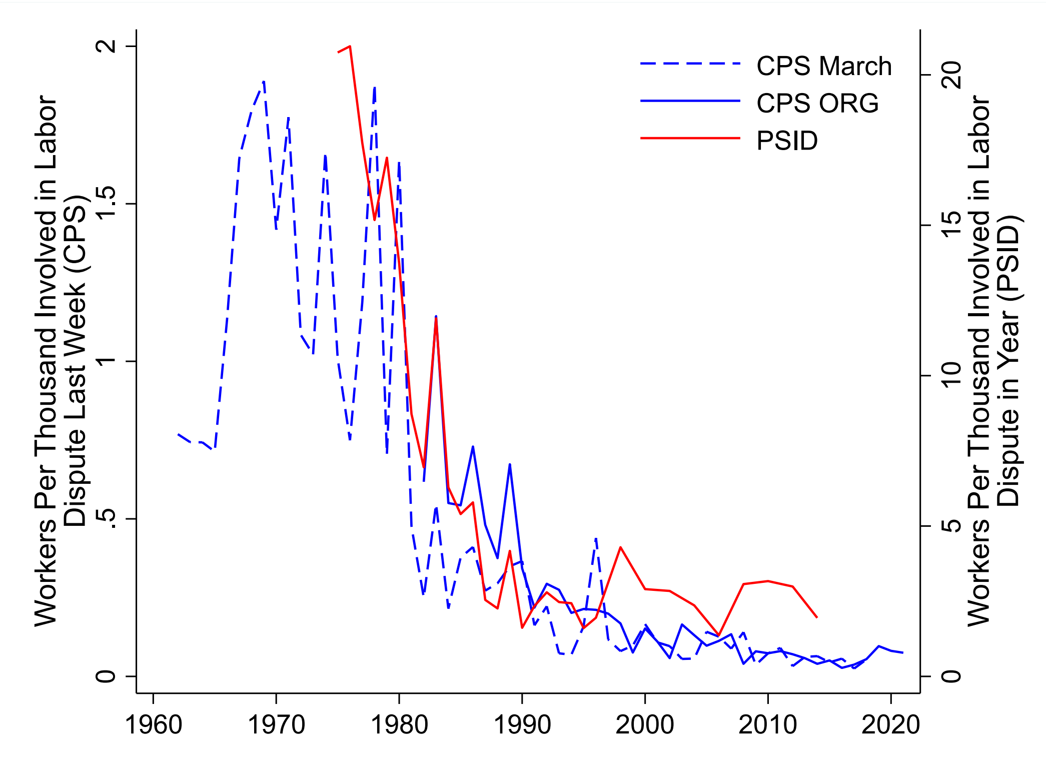
B Appendix figures

Figure B.1: Strike Size and the Decline of Strikes



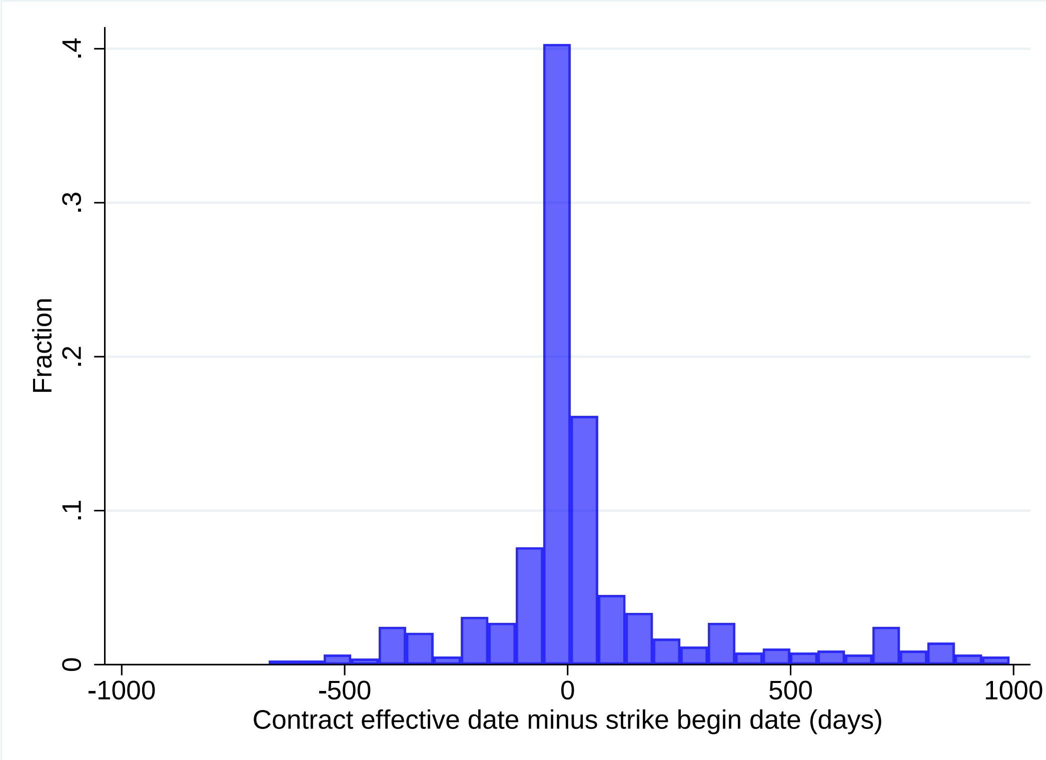
Source: FMCS.

Figure B.2: The Decline of Strikes (Comparing PSID and CPS-ORG)



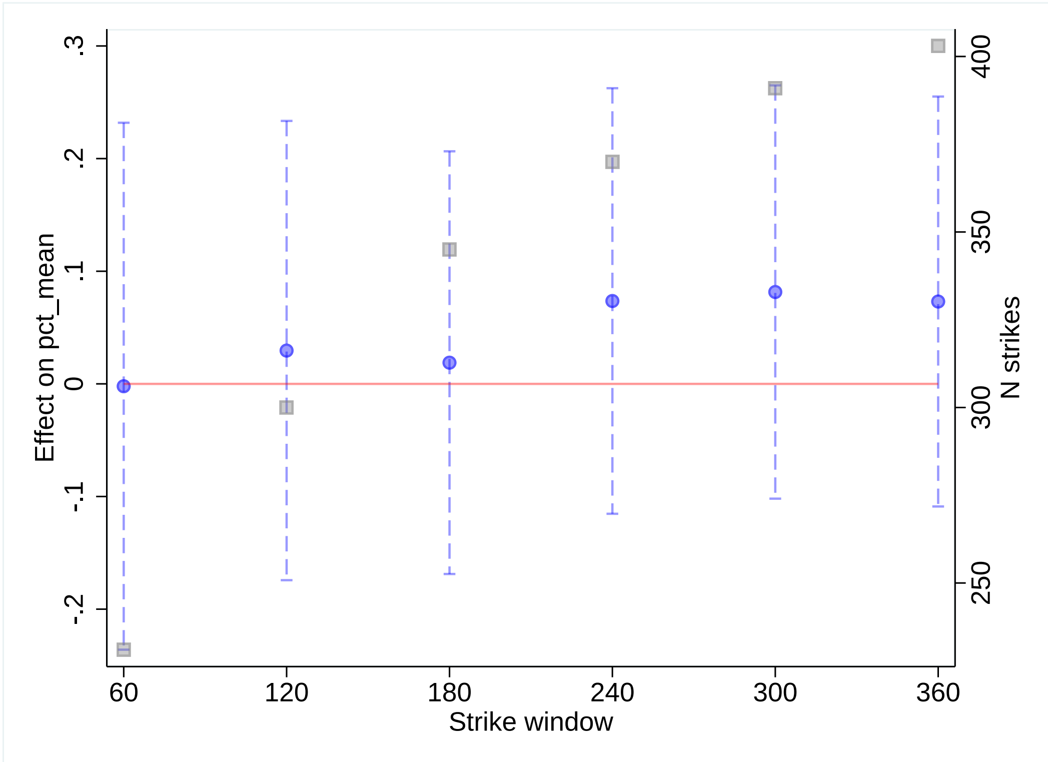
Source: CPS and PSID. CPS-ORG figures refer to the week preceding the survey; PSID figures refer to the prior calendar year.

Figure B.3: Days between effective date of contract and strike begin date



Note: This histogram shows that the majority of strikes matched to contracts occur close to the effective date of the contract.

Figure B.4: Contracts regression estimates with varying strike window



The blue dots and vertical lines show the point estimates and from the column (1) regression in Table C.8 varying the window (in days) used to define the indicator for close to strike. The grey squares give the number of strike-contract matches for the given window (right y-axis).

C Appendix tables

Table C.1: Descriptive Statistics (PSID)

	1976-1981		1982-2015	
	Non-strikers mean	Ever Strike mean	Non-strikers mean	Ever Strike mean
log(Hourly Wage)	2.39	2.60	2.35	2.62
Hourly Wage	12.18	14.49	12.75	15.17
Union Member	0.21	0.69	0.16	0.65
Public Sector	0.24	0.20	0.20	0.19
Manufacturing	0.19	0.29	0.19	0.35
LTHS	0.26	0.29	0.20	0.23
HS	0.41	0.45	0.35	0.42
Some College	0.17	0.16	0.25	0.21
BA	0.11	0.07	0.14	0.10
Above BA	0.04	0.04	0.05	0.04
Age	32.66	32.88	31.43	35.14
Female	0.45	0.26	0.48	0.32
N	6170	701	16818	792

Source: PSID.

Table C.2: Wage Outcomes of Strikes, Before and After 1982 (Same Employer Stayers Only)

	+/- 2 year		+/- 5 years	
	(1)	(2)	(3)	(4)
Post-strike	0.01	-0.01	0.02	-0.03
	(0.02)	(0.02)	(0.02)	(0.02)
Post-strike * Pre-1982 strike	0.08***	0.07***	0.08***	0.07***
	(0.02)	(0.02)	(0.02)	(0.02)
Age		0.06***		0.06***
		(0.00)		(0.00)
Age2		-0.00***		-0.00***
		(0.00)		(0.00)
Tenure		0.01***		0.01***
		(0.00)		(0.00)
Years of education		0.06***		0.06***
		(0.01)		(0.01)
Union membership		0.12***		0.12***
		(0.01)		(0.01)
Fixed effects:				
Year	Yes	Yes	Yes	Yes
Worker	Yes	Yes	Yes	Yes
Region X Industry.		Yes		Yes
R^2	0.69	0.78	0.69	0.78
N strikers	914	662	939	675
Observations	182588	108506	185563	110912

Note: Outcome is logged hourly wages and includes only employer-stayer workers. Sample includes all observations of non-striking workers. Strikes are defined as missing work due to participation in a strike anytime during the previous year. Post-strike is a period including the year of the strike and the period after the strike. Specifically, columns (1) and (2) retain observations from striking workers within 1 year of their strike; in columns (3) and (4), the window is extended for 5 years around the strike. Models compare strike outcomes before and after 1982. Sample size and number of strikers varies across models due to the exclusion of singleton observations from the fixed effects estimation. Standard errors are clustered at the respondent level.

Source: PSID.

* $p < .05$; ** $p < .01$; *** $p < .001$ (two-tailed tests)

Table C.3: Employment Outcomes of Strikes, Before and After 1982

	+/- 2 year		+/- 5 years	
	(1)	(2)	(3)	(4)
Post-strike	0.00	-0.01	0.01	-0.00
	(0.01)	(0.01)	(0.01)	(0.01)
Post-strike * Pre-1982 strike	-0.01	0.00	-0.02	-0.02
	(0.02)	(0.01)	(0.01)	(0.01)
Age		0.00***		0.00***
		(0.00)		(0.00)
Age2		-0.00***		-0.00***
		(0.00)		(0.00)
Years of education		0.00		0.00
		(0.00)		(0.00)
Union membership		0.04***		0.04***
		(0.00)		(0.00)
Fixed effects:				
Year	Yes	Yes	Yes	Yes
Worker	Yes	Yes	Yes	Yes
Region X Industry.		Yes		Yes
R^2	0.35	0.28	0.35	0.27
N strikers	1085	1044	1105	1068
Observations	253692	226933	257106	230145

Note: Outcome is employment. Sample includes all observations of non-striking workers. Strikes are defined as missing work due to participation in a strike anytime during the previous year. Post-strike is a period including the year of the strike and the period after the strike. Specifically, columns (1) and (2) retain observations from striking workers within 1 year of their strike; in columns (3) and (4), the window is extended for 5 years around the strike. Models compare strike outcomes before and after 1982. Sample size and number of strikers varies across models due to the exclusion of singleton observations from the fixed effects estimation. Standard errors are clustered at the respondent level.
Source: PSID.
* $p < .05$; ** $p < .01$; *** $p < .001$ (two-tailed tests)

Table C.4: Wage Outcomes of Strikes, Before and After 1982 (Controlling for Manufacturing and Public Sector Interactions)

	+/- 2 year window		+/- 5 years window	
	(1)	(2)	(3)	(4)
Post-strike	0.03	0.01	0.03	-0.01
	(0.02)	(0.01)	(0.02)	(0.02)
Post-strike * Pre-1982 strike	0.08***	0.07***	0.08***	0.08***
	(0.02)	(0.02)	(0.02)	(0.02)
Post-strike * Public sector strike	-0.05	-0.07	-0.04	-0.06
	(0.03)	(0.04)	(0.03)	(0.04)
Post-strike * Manufacturing strike	-0.04	-0.03	-0.03	-0.02
	(0.02)	(0.02)	(0.03)	(0.02)
Age		0.06***		0.06***
		(0.00)		(0.00)
Age2		-0.00***		-0.00***
		(0.00)		(0.00)
Tenure		0.01***		0.01***
		(0.00)		(0.00)
Years of education		0.07***		0.06***
		(0.01)		(0.01)
Union membership		0.12***		0.12***
		(0.01)		(0.01)
Fixed effects:				
Year	Yes	Yes	Yes	Yes
Worker	Yes	Yes	Yes	Yes
Region X Industry.		Yes		Yes
R^2	0.69	0.78	0.69	0.78
N strikers	914	662	939	675
Observations	182588	108506	185563	110912

Note: Outcome is logged hourly wages. Sample includes all observations of non-striking workers. Strikes are defined as missing work due to participation in a strike anytime during the previous year. Post-strike is a period including the year of the strike and the period after the strike. Specifically, columns (1) and (2) retain observations from striking workers within 2 years of their strike; in columns (3) and (4), the window is extended for 5 years around the strike. Models compare strike outcomes before and after 1982. Sample size and number of strikers varies across models due to the exclusion of singleton observations from the fixed effects estimation. Standard errors are clustered at the respondent level.
Source: PSID.
* p < .05; ** p < .01; *** p < .001 (two-tailed tests)

Table C.5: Wage (non-logged) Outcomes of Strikes, Before and After 1982

	+/- 2 year		+/- 5 years	
	(1)	(2)	(3)	(4)
Post-strike	0.42	0.35	0.40	0.04
	(0.26)	(0.27)	(0.24)	(0.25)
Post-strike * Pre-1982 strike	0.60	0.57	0.83*	0.89*
	(0.37)	(0.36)	(0.38)	(0.36)
Age		0.85***		0.85***
		(0.06)		(0.06)
Age2		-0.01***		-0.01***
		(0.00)		(0.00)
Years of education		1.31***		1.30***
		(0.29)		(0.29)
Union membership		1.84***		1.88***
		(0.22)		(0.22)
Fixed effects:				
Year	Yes	Yes	Yes	Yes
Worker	Yes	Yes	Yes	Yes
Region X Industry.		Yes		Yes
R^2	0.25	0.24	0.25	0.25
N strikers	930	893	952	920
Observations	201631	177765	204760	180704

Note: Outcome is hourly wages (non-logged and including zero values). Sample includes all observations of non-striking workers. Strikes are defined as missing work due to participation in a strike anytime during the previous year. Post-strike is a period including the year of the strike and the period after the strike. Models compare strike outcomes before and after 1982. Sample size and number of strikers varies across models due to the exclusion of singleton observations from the fixed effects estimation. Standard errors are clustered at the respondent level.
Source: PSID.
* $p < .05$; ** $p < .01$; *** $p < .001$ (two-tailed tests)

Table C.6: SIPP descriptives

Variable	(1) No strike	(2) Has strike
Female	0.52	0.33
	(0.50)	(0.47)
Age	34.51	36.71
	(21.56)	(11.88)
White	0.82	0.84
	(0.38)	(0.37)
Hourly wage	23.49	31.29
	(14.32)	(18.09)
Earnings	2793.77	6437.61
	(5609.78)	(6275.79)
Hours	39.54	40.64
	(14.52)	(10.36)
Insured	0.70	0.76
	(0.46)	(0.42)
Union	0.06	0.35
	(0.23)	(0.48)
Public sector	0.17	0.11
	(0.38)	(0.31)
High school	0.79	0.84
	(0.41)	(0.37)
College	0.20	0.09
	(0.40)	(0.29)
Ag., Mining	0.02	0.02
	(0.15)	(0.13)
Construction	0.06	0.07
	(0.23)	(0.25)
Manufacturing	0.17	0.31
	(0.37)	(0.46)
Transport	0.04	0.10
	(0.20)	(0.30)
Communication	0.02	0.10
	(0.15)	(0.30)
Retail, Wholesale, Servs	0.26	0.21
	(0.44)	(0.41)
FIRE, Bus. Servs	0.17	0.08
	(0.38)	(0.27)
Health, Educ, Public Admin.	0.25	0.11
	(0.43)	(0.32)
Year	1995.67	1993.47
	(8.08)	(8.08)
Observations	932,454	1,011

Standard deviations in parentheses.

Table C.7: Descriptive statistics for collective bargaining agreements

Variable	(1) No strike	(2) Has strike
Number of workers	2480.55	2001.24
	(12368.27)	(6046.26)
Manufacturing	0.19	0.35
	(0.40)	(0.48)
Transportation	0.08	0.06
	(0.27)	(0.24)
Education	0.15	0.07
	(0.35)	(0.26)
Healthcare	0.09	0.17
	(0.28)	(0.38)
Effective year	2001.40	1999.10
	(8.89)	(8.16)
Contract duration (yrs)	3.14	3.35
	(1.07)	(1.01)
Strike duration (days)	-	34.21
	-	(53.40)
Retroactive	0.07	0.05
	(0.26)	(0.22)
Overterm	0.13	0.12
	(0.33)	(0.32)
In dollars	0.34	0.40
	(0.47)	(0.49)
Average expected real raise	0.30	0.24
	(1.40)	(1.58)
Observations	26,120	751

Table C.8: Strike effects in collective bargaining agreements

	(1)	(2)	(3)	(4)	(5)
	Inflation adjusted average percent raise				
Strike	0.000209	-0.0149	-0.0719	-0.0219	0.00462
	(0.0811)	(0.0835)	(0.115)	(0.116)	(0.121)
R-squared	0.252	0.423	0.668	0.744	0.757
N strikes	437	382	281	264	230
N	16,634	15,077	8,856	8,408	7,628
	Average nominal percent raise, first year				
Strike	0.0569	-0.0203	-0.0900	-0.00146	0.0412
	(0.136)	(0.146)	(0.179)	(0.191)	(0.210)
R-squared	0.196	0.385	0.611	0.688	0.712
N strikes	437	382	281	264	230
N	16,634	15,077	8,856	8,408	7,628
	Deflated average dollar raise				
Strike	-0.00847	-0.0464	0.0271	-0.0370	-0.0619
	(0.0291)	(0.0385)	(0.0558)	(0.0871)	(0.109)
R-squared	0.127	0.453	0.624	0.723	0.748
N strikes	246	215	129	113	101
N	7,151	6,030	3,590	3,255	2,785
Contract controls	X	X	X	X	X
State	X	X	X		
Year	X				
State x Year		X		X	X
State x Industry		X			X
Firm		X	X	X	X

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: This table reports estimates of the coefficient on a strike indicator from Equation 5 for three different collective bargaining agreement outcomes (inflation adjusted percent raise, average nominal percent raise, and dollar raise) across five different specifications estimated using ordinary least squares. Each column represents a different set of controls, with the control variables indicated with "X" in the bottom rows of the table.

Table C.9: Wage and Earnings Effects of Strikes

	log(Hourly Wage)			log(Weekly Earnings)		
	(1)	(2)	(3)	(4)	(5)	(6)
After Labor Dispute	-0.01	-0.02	-0.01	-0.03*	-0.04*	-0.03*
	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)	(0.02)
Union Member		0.04***	0.04***		0.06***	0.06***
		(0.00)	(0.00)		(0.00)	(0.00)
HS		0.08***	0.07***		0.17***	0.16***
		(0.00)	(0.00)		(0.00)	(0.00)
Some College		0.10***	0.08***		0.19***	0.17***
		(0.00)	(0.00)		(0.00)	(0.00)
BA		0.26***	0.23***		0.39***	0.36***
		(0.00)	(0.00)		(0.01)	(0.01)
Above BA		0.30***	0.26***		0.44***	0.40***
		(0.01)	(0.01)		(0.01)	(0.01)
Age		0.04***	0.03***		0.06***	0.05***
		(0.00)	(0.00)		(0.00)	(0.00)
Age sq		-0.00***	-0.00***		-0.00***	-0.00***
		(0.00)	(0.00)		(0.00)	(0.00)
Fixed effects:						
Yr.	Yes	Yes		Yes	Yes	
Yr. X Region X Ind. X Occ.			Yes			Yes
Worker	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.85	0.85	0.86	0.88	0.88	0.88
Within-R^2	0.00	0.01	0.01	0.00	0.01	0.01
Number of Strikers	2973	2620	2601	2973	2620	2601
Observations	3924748	3773478	3751566	3924586	3773358	3751446

Outcomes are (1) logged hourly earnings for hourly workers and (2) logged usual earnings for non-hourly workers and hourly wage*usual hours for hourly workers. Sample size varies across models due to the exclusion of singleton observations from fixed-effects models. Standard errors are clustered at the worker level and in parentheses.

Source: CPS ORG.

* $p < .05$; ** $p < .01$; *** $p < .001$ (two-tailed tests)

Table C.10: CPS descriptives

Variable	(1) No strike	(2) Has strike
Female	0.52	0.30
	(0.50)	(0.46)
Age	42.24	39.36
	(18.67)	(12.32)
White	0.75	0.74
	(0.43)	(0.44)
hourwagear	19.79	36.20
	(14.37)	(21.48)
earnweekr	820.25	940.39
	(693.40)	(514.07)
hours	36.43	38.85
	(10.74)	(7.44)
Union Member	0.08	0.54
	(0.27)	(0.50)
Public Sector	0.09	0.07
	(0.28)	(0.26)
HS	0.32	0.47
	(0.47)	(0.50)
BA	0.14	0.07
	(0.34)	(0.26)
year	1997.35	1988.18
	(11.80)	(9.83)
Ag., Mining	0.04	0.06
	(0.20)	(0.24)
Construction	0.07	0.10
	(0.25)	(0.30)
Manufacturing	0.15	0.32
	(0.35)	(0.47)
Transport	0.04	0.10
	(0.20)	(0.30)
Communication	0.02	0.09
	(0.15)	(0.29)
Retail, Wholesale, Servs	0.27	0.12
	(0.45)	(0.33)
FIRE, Bus. Servs	0.18	0.06
	(0.39)	(0.24)
Health, Educ, Public Admin.	0.22	0.14
	(0.42)	(0.34)
Observations	8,608,136	7,404

Standard deviations in parentheses.