

THE EFFECTS OF COLLECTIVE BARGAINING RIGHTS ON PUBLIC EMPLOYEE COMPENSATION: EVIDENCE FROM TEACHERS, FIREFIGHTERS, AND POLICE

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Widespread public-sector unionism emerged only in the 1960s, as individual states opened the door to collective bargaining for state and municipal workers. In this study, the author exploits differences in timing of legislative reforms across states to construct estimates of the causal effects of public-sector collective bargaining rights on pay, benefits, and employment for teachers, firefighters, and police. Perhaps surprisingly, estimates that allow for state fixed effects and state-specific trends show little effect on teachers' pay, benefits, or employment, despite significantly increasing union presence among teachers. For firefighters, the results show a substantial positive effect on wages. For police, the wage effect was more modest but the workweek was significantly shortened.

After four decades of expanding collective bargaining rights for public employees, many U.S. states—beginning with Wisconsin and Ohio—are enacting or considering measures to curb public-sector collective bargaining (Greenhouse 2011). The debate over collective bargaining rights for public employees brings to the forefront a question of longstanding interest to labor economists: Do collective bargaining rights allow employees to negotiate more generous pay and benefits packages or higher employment than they would obtain in the absence of such rights? If they do, then for opponents the implication is that public-sector collective bargaining rights strain state budgets, and one strategy for resolving budget crises is therefore to revoke collective bargaining rights for public-sector employees. For proponents, the implication is that revoking collective bargaining rights

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could result in poorer pay and benefits, and a reduction in quality and quantity of important public services like police and fire protection, and public education. Critical to either argument is the empirical question of what effects collective bargaining rights have on public-sector employees' compensation and employment levels. I address this question by estimating the causal effect of collective bargaining rights on union presence, compensation, and employment for three groups of public employees: school teachers, firefighters, and police, which make up more than a quarter of state and local public employment.

Identifying the causal impact of collective bargaining rights on public employee outcomes is challenging because of potentially confounding unobserved factors across states and over time. Average outcomes in states with strong collective bargaining rights may differ from outcomes in states without them for many reasons besides the causal impact of collective bargaining rights. Likewise, comparisons of outcomes within a state before and after a change in collective bargaining rights could reflect longer term trends in the state, which themselves led to changes in collective bargaining laws, rather than the effect of the laws themselves.

In this article, I seek to overcome these challenges by using state-level panel data on collective bargaining rights and public-employee outcomes over several decades, exploiting differences across states in the timing of laws governing the collective bargaining rights of public employees to control for unobserved confounding factors. The empirical strategy controls for time-invariant state effects, state-specific time trends, and year effects in a differences-in-differences framework. The analysis combines data on public-sector collective bargaining laws, the Current Population Survey, and the U.S. Census Bureau's historical database on individual government finances from all U.S. states and over the period from the 1960s through 1996 for firefighters and police and through 2010 for teachers.

The main findings are that public-sector collective bargaining rights significantly increase union presence in terms of membership among teachers, firefighters, and police, and in terms of union coverage for teachers and firefighters. The impacts on pay and hours, however, are mixed. Among public school teachers, collective bargaining laws have a minimal effect on hourly wages, and further evidence from school district expenditures suggests minimal effects on benefits and employment as well. Bargaining laws significantly, though modestly, reduce school teachers' hours, however. For firefighters, in comparison, the evidence suggests collective bargaining laws substantially increase hourly wages, with a smaller and statistically insignificant reduction in hours. For police, the evidence suggests a modest increase in wages but a significant reduction in hours.

These findings are consistent with and expand on results from earlier studies of collective bargaining rights for public-sector workers, which also found substantial effects on union presence and modest wage effects (Freeman and Valletta 1988; Zax and Ichniowski 1990). This article also complements findings from the literature on collective bargaining per se and

unionization in the public sector, surveyed by Lewis (1990), by identifying an important driver of collective bargaining and unionization.

Relation to the Literature on Public-Sector Collective Bargaining and Unionism

This article relates closely to three strands of the literature on collective bargaining in the public sector. The first, and most closely related, includes studies that have focused specifically on the impacts of laws governing collective bargaining rights for public-sector employees, which typically impose a “duty to bargain” on the part of the government. A duty to bargain requires the government to bargain in good faith if a union presents itself, but does not require that an agreement actually be reached. The second includes studies of the impact of collective bargaining per se in the public sector, and the third is the large literature on unionization in the public sector.

This article contributes most directly to the small literature on the effects of collective bargaining laws for public-sector workers. The most consistent finding of these studies is that stronger collective bargaining laws substantially increase the presence of unions in terms of membership and union coverage. Freeman and Valletta (1988), using cross-sectional analysis of state and local employees, found that a change in collective bargaining laws corresponding roughly to a move from no provision to a legally mandated duty to bargain was associated with approximately a 20% increase in collective bargaining coverage, very close to the cross-sectional results reported in the empirical results section below. Similarly, Zax and Ichniowski (1990), who employed a clever design that used cross-sectional analysis but selected on a sample of never-unionized departments in a stable legal environment to control for past propensity to unionize, found a substantial effect of duty-to-bargain laws on unionization rates among city employees from 1977 to 1982. This literature has also found modest impacts of collective bargaining laws on public employee wages. Freeman and Valletta found that moving from no provision to a duty to bargain increased wages by 6 to 8% in cross-sectional analysis, although the estimate was smaller in longitudinal analysis. They found an effect even when controlling for collective bargaining status, suggesting that collective bargaining laws have direct effects beyond their marginal impact on unionization or bargaining coverage. In summary, the findings in this article are consistent with this literature and build on it by analyzing a broader set of outcomes over a wider time frame and controlling more fully for unobserved confounding factors at the state level. The findings here are qualitatively similar to the earlier literature, but quantitatively smaller, plausibly because the empirical strategy here is less susceptible to bias than the earlier, cross-sectional estimates are.

A second related strand of the literature has focused not on collective bargaining *rights*, but on the impacts of collective bargaining itself. Collective bargaining is undoubtedly an important channel through which collective bargaining rights affect outcomes, but as Freeman and Valletta’s findings

suggested, it may not be the only one. The literature on the impacts of collective bargaining on public-sector employee outcomes has produced mixed results. Smith's (1972) study of collective bargaining among school teachers found little evidence for an increase in teacher salaries. Valletta (1993) estimated the effect of union contracts on wages and employment for several municipal government departments over the period 1977 to 1980, as well as for firefighters, and found some evidence of a positive employment effect of collective bargaining. Evidence on wage effects was inconclusive. In an earlier paper, Valletta (1989) exploited differences in bargaining status among departments within a city but for a single year (1980). He found a positive effect of collective bargaining agreements on department expenditures, although this was possibly attributable to offsets elsewhere as little evidence was found for total municipal expenditures. This article complements this literature by identifying an important driver of collective bargaining among public employees and corroborating some of the findings, namely, minimal wage effects for school teachers. The significant wage effects for other occupations found in this article may also help resolve some of the ambiguous findings in the literature.

Finally, this article is related to the larger literature on the effects of public-sector unionization, although the connection may be somewhat oblique: unions may have important effects even outside of a collective bargaining framework, and collective bargaining rights have impacts on outcomes through channels other than their marginal impact on unionization rates. Focusing on public school teachers, Baugh and Stone (1982) found a union/nonunion wage gap of about 12 to 22% in the late 1970s. Kearney and Morgan (1980) also found significant wage gaps for state employees in a variety of occupations. Studies of firefighters have found significantly higher compensation when a union is present, attributable primarily to a shorter workweek and higher benefit levels (Ashenfelter 1971; Ichniowski 1980). Police unions are also associated with higher earnings, although evidence on the union association with police employment is mixed (Freeman and Valletta 1988; Trejo 1991). The association between unions and employment for the public sector as a whole, however, appears to be positive (Marlow and Orzechowski 1996). Lewis's (1990) survey of 75 studies, including many of those mentioned above, concluded that the public-sector union wage gap is about 8 to 12%, which includes a substantial gap in fringe benefits. The findings in this article are qualitatively consistent with these earlier findings of the public-sector unions literature, except for the minimal effect of collective bargaining rights on teachers' wages found from this study. One potential explanation is the possibility that teacher unions may realize much of their impact outside of a collective bargaining framework.

More recent studies, however, suggest the explanation may be that unobserved differences between teachers and school districts with and without unions are confounding the cross-sectional comparisons on which most of the early literature is based. Findings from several studies of a quasi-experimental flavor that arguably control more fully for unobserved confounders are

consistent with the small effects of collective bargaining rights for teachers found in this study. Lovenheim (2009), using a differences-in-differences design based on teacher union certifications, found very little effect on teacher pay or district expenditures, but a modest effect on employment. Lindy's (2011) analysis of the lapse and subsequent renewal of New Mexico's collective bargaining laws for teachers found that collective bargaining rights have little effect on per-pupil educational spending, consistent with the findings here. Finally, Hoxby's (1996) study of education expenditures and outcomes supports the findings that collective bargaining rights have little effect on per-pupil spending, the lion's share of which comprises teacher salaries. Using variation in the timing of states' changes in collective bargaining laws similar to this study, Hoxby's implied reduced form estimate of the effect of collective bargaining rights on log per-pupil spending is .02, within the confidence interval of the estimates reported here.¹

The comparison between the findings of the large literature on public-sector unions and the findings of this and other papers on the effects of collective bargaining rights raises an important question: What, if anything, do the effects of collective bargaining laws imply about the impacts of public-sector unions? An appealing strategy would be to use changes in collective bargaining laws as an instrumental variable for union status to estimate the causal effects of public-sector unions on outcomes, as Hoxby (1996) did for education expenditures. The exclusion restriction imposed by that approach, however, attributes the entire impact of collective bargaining rights to the marginal impact on unionization, an assumption that does not seem plausible in light of Freeman and Valletta's (1988) findings described above. For example, if adopting collective bargaining laws strengthens the bargaining position of existing unions, the exclusion restriction would be violated. The findings in this article for police support this observation: the lack of evidence for an effect of collective bargaining rights on union coverage for police together with stronger evidence for higher wages and a shorter workweek suggest the primary impact of collective bargaining rights for police may be to strengthen the position of existing unions. The empirical approach in this article is thus to treat union presence as an important outcome in its own right and possibly a leading mechanism for the effects of collective bargaining rights, although not the only mechanism.

Institutional Background

The organized labor movement in the public sector got its start later than it did in the private sector. As late as the 1950s, during the heyday of private-sector labor unions, few public-sector employees were unionized and state

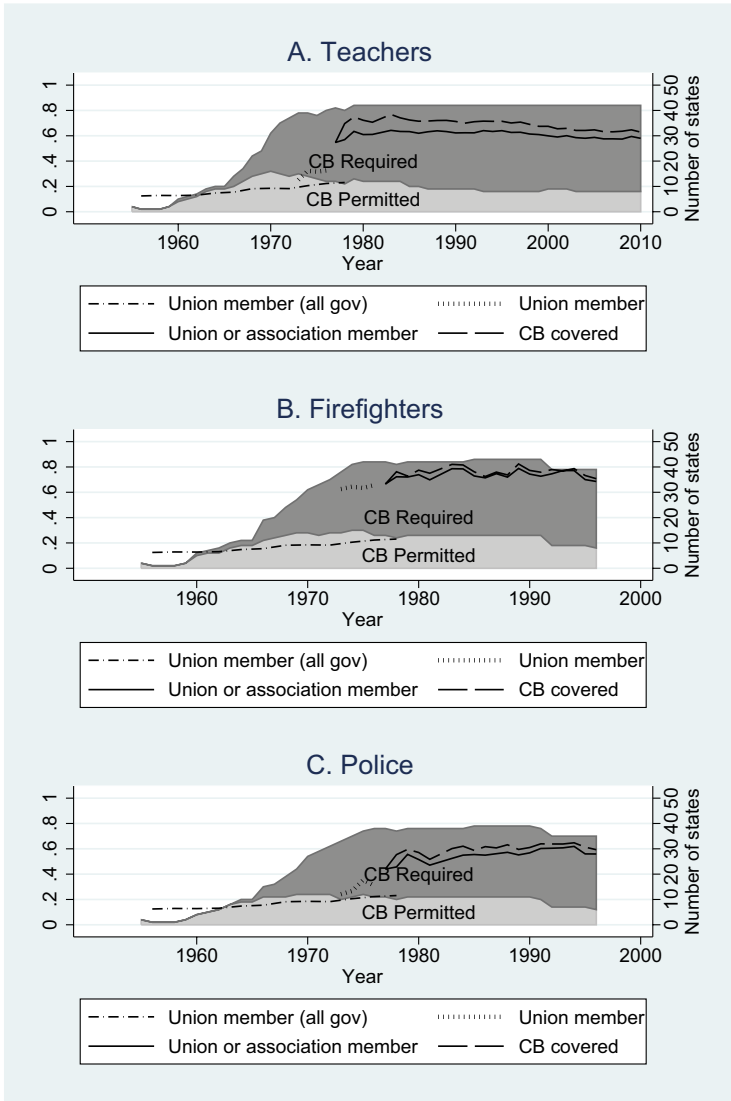
¹Hoxby's (1996) IV estimate of the effect of unionization on per-pupil spending is .1233 (Table III) while the "first stage" effect of collective bargaining rights on the unionization rate is .178 (Appendix 2). The implied reduced form effect of collective bargaining rights on per-pupil spending is therefore $.1233 \times .178 = .0219$.

laws prohibited governments from collectively bargaining with public employees (Freeman 1986). Beginning in the 1960s, however, public-sector employees began to organize in greater numbers and states started granting collective bargaining rights. For teachers' unions, a pivotal development was a 1961 organizing campaign by the American Federation of Teachers (AFT) at public schools in New York City in which the AFT won the representation election, paving the way for collective bargaining (Smith 1972). Another development spurring collective bargaining by public employees was President John F. Kennedy's 1962 Executive Order 10988, which recognized unions in the federal sector (Marlow and Orzechowski 1996). By 2010, 36.2% of public-sector employees were members of a union, while only 6.9% of private-sector employees were members (Bureau of Labor Statistics 2011).

Accompanying the increase in the public-sector unionization rate was the passage of laws by most states authorizing or requiring governments to bargain collectively with public employee unions, beginning with Wisconsin in 1960 (Valletta and Freeman 1988). Laws touching public-sector collective bargaining range from implicit or explicit prohibition to a requirement to bargain if a union presents itself, with gradations in-between. At one end of the range, the absence of a statute regarding collective bargaining in the public sector has typically been interpreted in the courts as an implicit prohibition (Freeman and Valletta 1988). Thirty-nine states were in this category regarding teachers at the beginning of the sample period in 1962, but by 2010 only three remained in this category. States have also explicitly prohibited collective bargaining, as five states do for teachers as of 2010. The next category of laws authorize employers to bargain but do not require them to do so, or give employee groups the right to present proposals to the employer or to meet and confer with the employer. Finally, state laws may require the employer to bargain, as only Wisconsin had for teachers in 1962, but which 34 states had by 2010.

Figure 1 illustrates these trends, plotting the unionization rate, the number of states with laws permitting public-sector collective bargaining, and the number of states requiring collective bargaining by year. The shaded regions of the figure show the number of states permitting (light gray) or requiring (dark gray), with the remainder prohibiting collective bargaining. By the end of the period, the vast majority of states had provisions either allowing or requiring collective bargaining with public employees. The figure also plots several statistics that illustrate the growth in the public-sector unionization rate. The earliest series shows a steady increase in the unionization rate among all government employees starting in the 1960s (unionization rates tallied separately by occupation in the public sector were not collected by the Bureau of Labor Statistics at that time). The next series shows a growth in the union membership rate for all three occupations between 1973 and 1978, although the growth is modest for firefighters, which was already at a high level by 1973. The final two series in each panel show the union or similar labor association membership rate, and the rate of coverage by collective bargaining, which for teachers tended to grow

Figure 1. Public-Sector Unionization Rates and Collective Bargaining Laws



Sources: Data are from Freeman (1986), Freeman and Valletta (1988), and the 1973–2010 Current Population Surveys.

Notes: Unionization rate scale is on the left axis. Number of states with collective bargaining rights as indicated in the shaded regions given by the right-hand scale.

through the mid-1980s but plateau and taper a little in the 1990s and early 2000s.^{2,3} For firefighters and police, the rate grew into the 1990s.

²As Freeman (1986) noted, including membership in labor associations similar to unions when describing the growth in public-sector collective bargaining is appropriate, since prior to the 1970s labor associations did not operate as unions or bargain collectively, but starting in the 1970s they did, as the changes in the National Education Association during this period illustrates.

³Coverage by collective bargaining is captured by the CPS question on whether the respondent is covered by a union contract. This question, unfortunately, is asked only of respondents who reported no union membership. The union coverage variable therefore indicates union membership or coverage by a union contract, even though not all union members are covered by a union contract.

The dramatic expansion of collective bargaining rights for public-sector workers followed by increases in union presence suggest that collective bargaining rights may have facilitated union formation and potentially affected other labor market outcomes for public-sector workers, as the empirical results below suggest. Alternatively, both trends could be driven at least in part by underlying changes in attitudes toward collective bargaining and demand for public services. Is there a causal connection between collective bargaining laws and union presence? Did the passage of collective bargaining laws affect labor market outcomes among public-sector employees? These questions will be addressed in the empirical work.

Data

This study combines three data sources: state-level public-sector collective bargaining laws, the Current Population Survey, and the U.S. Census Bureau's Historical Database on Individual Government Finances. Each is described below, with further details given in the Appendix.

The data set on public-sector collective bargaining laws was originally constructed by Richard Freeman and Robert Valletta in 1985 (see Valletta and Freeman 1988), and it codes the relevant laws for every state and every year from 1955 to 1985 for five different occupational groups. This data set was later extended by Kim Rueben to cover the years through 1996. I use the extended Rueben data set as a starting place and augment it using data on public school teacher collective bargaining laws in Lindy (2011) and data from the National Council on Teacher Quality to extend the series for teachers through 2010. While state laws vary substantially in their exact provisions for public-sector collective bargaining, states fall roughly into three categories: collective bargaining prohibited, permitted, or required. The prohibited category includes not only statutes that explicitly prohibit state employers from bargaining with worker representatives but also situations in which the state law makes no provision for collective bargaining, because courts have typically interpreted this as prohibiting collective bargaining (Freeman and Valletta 1988). The permitted category includes statutes that authorize the employer to bargain and that give employee organizations the right to present proposals or to meet and confer with the employer. The required category includes statutes that either imply or make explicit the duty of the employer to bargain should the workers demand it. Table 1 shows the timing of when states enacted laws either permitting or requiring employers to bargain collectively with public employees. As Figure 1 also shows, most of the relevant laws took effect in either the 1960s or 1970s, although there were a number of changes after 1980.

The second source of data comes from the Current Population Survey (CPS). Extracts containing age, race, sex, state, education, earnings, hours, and union status variables for public-sector teachers, firefighters, and police were created from the CPS files. Hours and earnings were taken from the March annual files, extracted using the Integrated Public Use Microdata Series (IPUMS) system (King et al. 2010), and were available every year. The

Table 1. Timing of State Laws Governing Public-Sector Collective Bargaining Rights

<i>Collective bargaining status</i>	<i>Before 1970</i>	<i>Between 1970 and 1980</i>	<i>After 1980</i>
A. Teachers			
Permitted	AK, AR, CA, GA, ID, IL, KY, MN, NE, NH, NM, OR, UT, VA, WV	AZ, CO, LA, OH, TN, WY	
Required	CT, MA, MI, NJ, NY, RI, VT, WA, WI	AK, CA, DE, FL, HI, ID, IN, IA, KS, ME, MD, MN, MT, NV, NH, ND, OK, OR, PA, SD	IL, NE, OH, TN, NM
B. Firefighters			
Permitted	AL, AK, AR, CA, ID, IL, MN, MO, NH, NM, OR, UT, VA, WV	AZ, GA, IN, KS, LA, SC	
Required	CT, DE, ME, MA, MI, NJ, NY, PA, RI, VT, WA, WI, WY	AK, CA, FL, HI, ID, IA, KY, MN, MT, NE, NV, NH, OK, OR, SD, TX	OH, IL
C. Police			
Permitted	AK, AR, CA, ID, IL, MN, NH, NM, OR, UT, VA, WV	AZ, IN, KS, LA, SC	
Required	CT, DE, MA, MI, NJ, NY, PA, RI, VT, WA	AK, CA, FL, HI, IA, KY, ME, MN, MT, NE, NV, NH, OK, OR, SD, TX, WI	OH, IL

Sources: Data are from Valletta and Freeman (1988), Kim Rueben’s update thereof (1997), Lindy (2011), and the National Council on Teacher Quality.

Notes: Timing of passage of state laws either permitting or requiring employers to bargain collectively with public employees.

CPS data cover the years 1962 through 2010 for teachers and 1968 through 1996 for firefighters and police, since firefighters and police did not have separately identifiable occupation codes prior to 1968. The extract includes only the period through 1996 for firefighters and police because public-sector collective bargaining laws are available through only 1996 for those occupations. Union status variables are available in the CPS only from 1973.

Union status variables were taken from the May Supplement files from 1973 through 1981, and from the Merged Outgoing Rotation Group (MORG) files from 1982 through 2010.⁴ From 1973 to 1976, the union status information in the May Supplement contains the response to a question of whether the worker is a member of a labor union. Starting in 1977 the CPS modified the question to include employee associations similar to a union, which increased the measured unionization rate among public-sector workers substantially, as Figure 1 shows. Also starting in 1977, workers who responded that they were not members of a union or similar employee association were then asked if they were covered by a union or employee association contract. The CPS did not ask union or association members if

⁴The May supplements and MORG files were obtained in April 2011 from the National Bureau of Economics Research website at <http://www.nber.org/data/cpsindex.html>.

they were covered by a union or association contract. The analysis will focus on two notions of union presence based on these variables: union membership, which will include association membership starting in 1977, and union coverage, which will mean union membership or (starting in 1977) coverage by a union contract. Not all workers included in union coverage are covered by a union contract, however, since not all union members are covered by a union contract.

Records were selected for individuals who were employed, had strictly positive earnings, were state or local government employees, and were police, firefighters, or elementary or secondary school teachers between 18 and 65 years old. Records with allocated earnings and hours were dropped to avoid biases stemming from the imputation procedure (Hirsch and Schumacher 2004; Lemieux 2006). The Appendix contains further details (see also <https://economics.byu.edu/frandsen/Pages/Publications.aspx> for additional tabular material).

Summary statistics of these data are reported in Table 2. The table shows that for teachers, firefighters, and police, where public employers have a duty to bargain (“required”), union presence is stronger, pay is higher, and hours are shorter than where collective bargaining is prohibited, consistent with the stylized descriptive facts found in the literature cited above. The empirical work will attempt to disentangle the causal effects of collective bargaining from the selection effects reflected in these differences.

The final source of data is the Historical Database on Individual Government finances, constructed by the U.S. Census Bureau largely from the annual Survey of Governments or the Census of Governments conducted every five years, covering the years 1967 through 2006. I analyze the detailed financial records of school districts in this database to create state-level measures of per-pupil salary spending and per-pupil total education spending. Nearly 400,000 records corresponding to school districts with positive enrollment and which were administering strictly primary or secondary education were selected and collapsed to the state-year level. The Appendix contains further details (see also <https://economics.byu.edu/frandsen/Pages/Publications.aspx>).

Both to illustrate the data used in the analysis and to highlight the importance of the research design, Figure 2 plots these per-pupil spending measures over time for three states—New Hampshire, Maine, and Missouri—which were chosen for their differences in timing of collective bargaining laws, which are also shown in the figure. New Hampshire enacted a law requiring school districts to bargain collectively with teachers at the end of the 1970s, shortly after which per-pupil education spending and per-pupil salary started to rise sharply. Was this rise due to collective bargaining rights? The case on Maine suggests not. Maine passed a law requiring collective bargaining nearly a decade earlier but actually saw per-pupil spending stagnate directly after, only to experience a rise nearly identical to New Hampshire’s in the 1980s. Perhaps the effect of New Hampshire’s law change spilled over into neighboring Maine and was responsible for both state’s increase in spending? One

Table 2. Sample Means by Occupation for Selected Variables

Variable	All	CB law		
		Prohibited	Allowed (excl required)	Required
A. Teachers, 1962–2010				
N	133,945	23,876	17,899	92,170
Age	40.3	39.8	39.1	40.8
Female	0.700	0.749	0.690	0.680
Union member*	0.596	0.297	0.448	0.729
Union member or covered*	0.666	0.382	0.537	0.792
Hourly wage	17.56	15.23	16.41	18.93
Annual earnings	34114	30312	32081	36377
Annual hours	2001	2062	2013	1970
Hours/week	41.6	42.7	42.0	41.1
Weeks/year	47.4	47.9	47.4	47.3
B. Firefighters, 1968–1996				
N	5,490	637	992	3,861
Age	37.8	36.6	37.4	38.2
Female	0.010	0.023	0.008	0.008
Union member*	0.734	0.610	0.558	0.810
Union member or covered*	0.756	0.637	0.591	0.827
Hourly wage	16.33	13.50	13.91	17.87
Annual earnings	42428	37101	38218	45149
Annual hours	2762	2913	2928	2664
Hours/week	54.6	57.9	58.4	52.4
Weeks/year	50.5	50.4	50.1	50.7

Notes: Sample sizes and means for selected variables. Samples consist of extracts of public-sector teachers, firefighters, and police from the CPS May supplements, Merged Outgoing Rotation Groups, and March supplements as described in the text. Wage and earnings variables are deflated to year 2000 dollars.

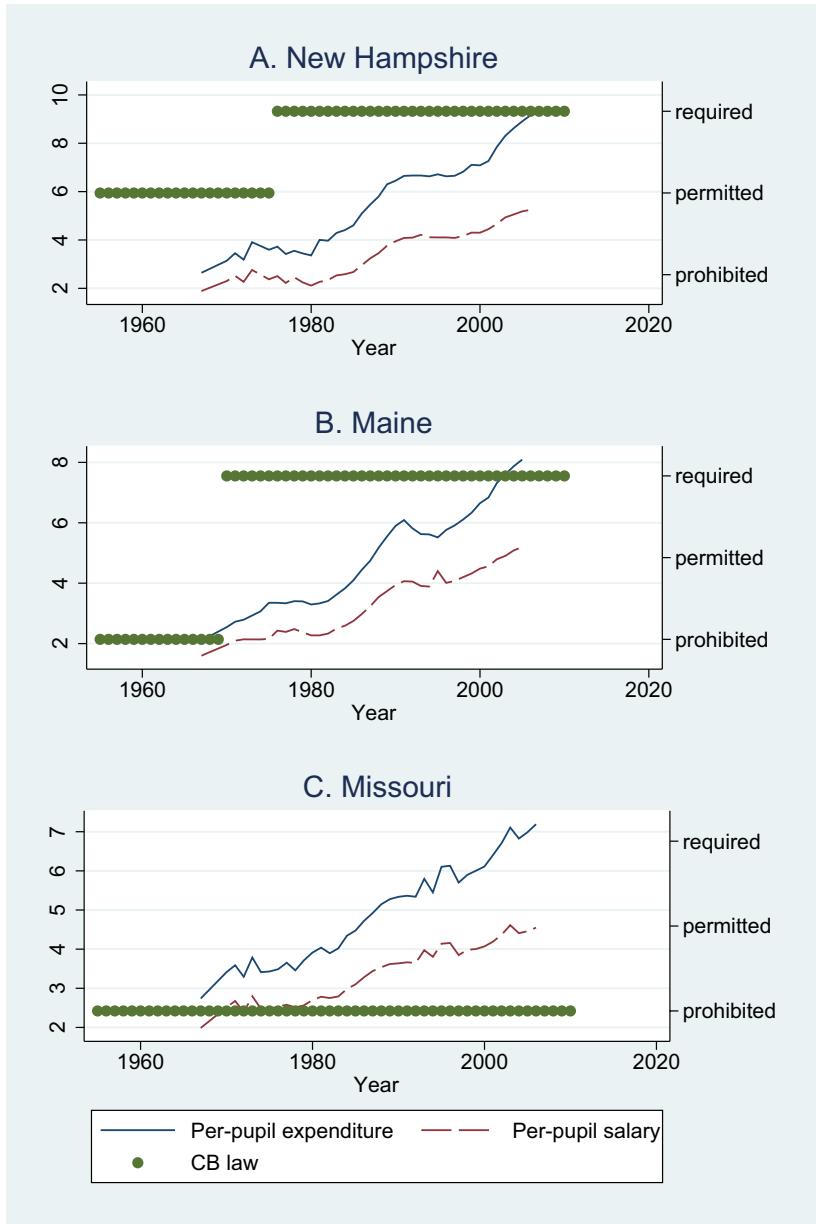
*For period starting 1973.

would not expect such a spillover to extend to Missouri, and yet Missouri experienced a nearly identical pattern—though at a lower level—of spending stagnation in the 1970s and a sharp rise in the 1980s, despite the fact that Missouri had no law change during the whole period. A before-and-after analysis of New Hampshire, and a cross-section analysis of all three states, would have pointed to a large positive effect of collective bargaining rights, and yet it’s clear from the figures that no such conclusion is warranted, at least on the basis of these three states. This example highlights the need to take into account the possibility of aggregate shocks at the year level and unobserved state factors in the estimation strategy, as the next section develops formally.

Econometric Framework

To control for possible confounding factors in the relationship between collective bargaining laws and outcomes, I use a differences-in-differences

Figure 2. Per-Pupil Salary, Per-Pupil Education Expenditure, and Public School Teacher Collective Bargaining Rights over Time for Selected States



Sources: Data on expenditures are from the U.S. Census Bureau’s Historical Database on Individual Government Finances; data on collective bargaining rights are from Freeman and Valletta (1988), Kim Rueben’s update thereof (1997), and Lindy (2011).

design that takes advantage of the different timing of collective bargaining law changes among states. This framework relates outcome Y_{ist} for individual i living in state s in year t to collective bargaining laws CB_{st} in regression equations such as the following:

$$Y_{ist} = \delta CB_{st} + X'_{ist}\beta + \alpha_{0s} + \gamma_t + f(s,t) + \varepsilon_{ist},$$

where X_{ist} is a vector of covariates including age, sex, race, and education. The α_{0s} terms control for any unobserved state-level factors that are constant over time. The γ_t terms control for factors that affect all states but may change from year to year, such as macroeconomic shocks. The $f(s,t)$ term controls for time-varying relative changes across states that could lead to bias even after controlling for state effects $\{\alpha_{0s}\}$ and year effects $\{\gamma_t\}$ if they are correlated with changes in collective bargaining laws. Such confounding changes could include the general population shift from the Northeast to the Southwest, the decline in heavy industries, shifting attitudes toward unionism, and preferences for public services, which are difficult to measure but likely to be correlated with CB_{st} . The disturbance term ε_{ist} may have arbitrary serial correlation within states but is uncorrelated with CB_{st} . Inference is therefore clustered at the state level.

While the most general specification for $f(s,t)$, namely state-by-year interactions, would be collinear with CB_{st} , the empirical work uses several slightly more restrictive specifications for $f(s,t)$. The first specification groups states by Census region (Northeast, Midwest, West, South) and includes a set of region-by-year interactions.⁵ This specification controls flexibly for confounding factors that vary at a broad geographical level but could still be susceptible to bias from factors that change at the state level. A second specification for $f(s,t)$ is a set of state-specific linear trends $\{\alpha_{1s} \times t\}$, which control for any unobserved state-level secular trends over time that may be correlated with collective bargaining law passage, and also includes the region-by-year interactions, and might be expected to be least susceptible to bias. The identifying assumption is that any underlying unobserved factors at the state level that influence both the outcome and the adoption of public-sector collective bargaining laws vary smoothly over time.

A testable implication of this assumption is that outcome shocks relative to a state-level linear trend should be uncorrelated with future law changes. This approach is a generalization of checking for parallel trends in the canonical two-group differences-in-differences design. The implication can be tested in a regression of the outcome on several leads and lags of the law change variable, controlling for state effects, state trends, and other covariates (Autor 2003). Nonzero coefficients on the lead terms (prior to law changes) would imply violations of the identifying assumption and would suggest omitted factors may be driving both law changes and the outcome. This check of the identification strategy yields encouraging results for teachers and firefighters but may indicate some misspecification for police. Appendix Figures A.1 through A.5 illustrate the results of this specification check for union membership, union coverage, hourly wages, annual earnings, and weekly hours.⁶ Each figure plots coefficients and 95% confidence

⁵Specifications grouping states more finely by the nine Census divisions and including division-by-year effects were also estimated, and they resulted in estimates very similar to the region-by-year specification, but slightly less precise.

⁶Access Appendix materials at <https://economics.byu.edu/frandsen/Pages/Publications.aspx>.

intervals for leads of law changes up to six years prior to the law change and lags of up to six years after (and an indicator for seven-plus years) from a regression of the given outcome, which includes region-by-year effects and state-specific trends separately for teachers, firefighters, and police. In Appendix Figure A.1, panel A for teachers, the coefficients corresponding to years prior to a law change are very close to zero, with some slightly negative and some slightly positive, and none significant. The terms corresponding to the year of the law change and beyond are systematically positive (and jointly significantly different from zero). For firefighters (panel B) the plot looks very similar, with coefficients having no systematic departure from zero prior to the law change but uniformly positive coefficients the year of the law change and thereafter. For police (panel C), however, the plot shows significant coefficients on terms corresponding to the two years prior to a law change. Given the number terms being estimated, this may be attributable to sampling error even without the presence of confounding factors, or it may suggest unobserved factors driving both law changes and changes in union membership for police. If the significant coefficients on pre-law change indicators reflect confounding factors, then caution may be warranted in interpreting the results on police in the next section. Specifications dropping the two years prior to law changes for police will be discussed in the section on robustness of the results. Appendix Figure A.2, showing the specification checks for the union coverage outcome, looks extremely similar to the checks for union membership, showing no significant pre-period coefficients for teachers and firefighters, but perhaps some misspecification for police. Appendix Figures A.3 through A.5 show similar plots for log hourly wages, log annual earnings, and weekly hours. These plots show little evidence of confounding changes in the outcome prior to law changes. With few exceptions, coefficients corresponding to years prior to a law change are insignificant, with the only significant and systematically positive effects coming after law changes. One exception is a significant negative effect for firefighters' wages corresponding to five years before a law change in Appendix Figure A.3, panel B. This exception is likely attributable to sampling variation, as it doesn't appear to be part of a systematic trend in the pre-period, and a corresponding positive deviation in hours is shown in Appendix Figure A.5, panel B. In summary, the specification checks suggest that including state effects and state trends plausibly controls for potentially confounding unobserved factors at the state and year level.

Under the assumption that any other state-level disturbances are uncorrelated with the passage of CB laws, the coefficient δ identifies the causal effect of CB laws on the outcomes. The main specification for the collective bargaining law variable, *CB*, will be an indicator for a duty to bargain, which means the government employer is required to bargain if a union should present itself. The appendix reports results for a more flexible specification with a dummy variable for collective bargaining permitted (but not required) in addition to the duty-to-bargain indicator, and a linear specification where collective bargaining prohibited is coded as 0, permitted is coded

as 1, and duty to bargain is coded as 2. The results for all three specifications are qualitatively similar, although the most flexible specification with dummies for each category is less precise.

The regression specification does not include an indicator for union status on the right side, which is a departure from many labor market outcome regression specifications. This omission is for two reasons. First, union status is potentially an important outcome in its own right, and a possible mediating factor for the effect of collective bargaining on other outcomes; the estimated coefficient on collective bargaining laws would therefore be missing potentially an important component of the total effect of collective bargaining were union status included as a regressor. Second, at the individual level, union status is simultaneously determined with wages (and other job characteristics) and is therefore likely correlated with unobserved determinants of wage. For example, DiNardo and Lee (2004) found the estimated effects of union status change substantially when unobserved confounders are controlled for in a regression discontinuity design. Including union status as a regressor in this setting would therefore introduce bias, absent credible quasi-experimental control over union status.

A further refinement of the specification would allow collective bargaining laws to have time-varying effects. The analysis of leads and lags of the changes in collective bargaining laws discussed above and illustrated in Appendix Figures A.1 through A.5 suggests that there may be some evidence of effects growing slowly over time. The specification with a single indicator for collective bargaining rights will therefore give the average effect over the post-law period for each state. A more detailed analysis of the time profile of effects will be left for future work.

Empirical Results

Effects on Union Presence

Differences-in-differences (DD) estimates show that enacting collective bargaining laws had a modest but positive impact on union membership and coverage rates for teachers, firefighters, and police. Table 3 reports estimates and standard errors of the effects of duty-to-bargain laws on indicators for union membership and union coverage separately for teachers, firefighters, and police. The sample period is from 1973 to 2010 for teachers and from 1973 to 1996 for firefighters and police.⁷ The large and highly significant estimates in the range of .18 to .25 in column (1) of Table 3 across all occupations are from cross-section regressions that do not control for state effects. While these coefficients match up closely with Freeman and Valletta's (1988) estimates of the effect of CB rights on collective bargaining coverage, they are likely to partially reflect unobserved differences between states with and without collective bargaining requirements. When state

⁷Estimates restricting to a uniform sample period are reported in the Appendix and are discussed below in the section titled Robustness of the Results and Alternative Specifications.

Table 3. Effects of Duty-to-Bargain Laws on Union Presence

<i>Dependent variable</i>	<i>X-section</i>	<i>Differences-in-differences</i>		
	(1)	(2)	(3)	(4)
	A. Teachers			
Union membership	.245*** (.027)	.092*** (.017)	.066*** (.018)	.072*** (.018)
Union covered	.250*** (.024)	.090*** (.015)	.097*** (.019)	.103*** (.018)
	B. Firefighters			
Union membership	.204*** (.044)	.047 (.036)	.129*** (.048)	.143*** (.044)
Union covered	.182*** (.044)	.035 (.040)	.134*** (.052)	.141*** (.046)
	C. Police			
Union membership	.246*** (.058)	.043 (.028)	.057** (.026)	.061** (.024)
Union covered	.244*** (.058)	.031 (.029)	.039 (.031)	.044 (.030)
Controls?	Y	Y	N	Y
State effects?	N	Y	Y	Y
State trends?	N	N	Y	Y
Region x year effects?	N	Y	Y	Y

Sources: Data are from the following CPS files: 1973–1981 May Supplements, the 1983–1996 Merged Outgoing Rotation Group (through 2010 for teachers), with samples as described in the text.

Notes: Regression coefficients and clustered standard errors (by state) on an indicator for a state law mandating a duty to bargain. Models indicating “Y” for controls include year effects, private-sector unionization rate, fraction of workers in manufacturing, sex, race, age, education, and marital status as additional covariates.

*Statistically significant at the .10 level; ** at the .05 level; *** at the .01 level.

effects are controlled for (column 2), the estimate for teachers in panel A drops to a more modest .09, although that is still highly significant. Columns (3) and (4) control for state-specific trends without and with individual covariates, and give highly significant estimates near .07 for teacher union membership and .10 for teacher union coverage. For firefighters (panel B), when state effects (but not trends) are included, the effects on membership and coverage are smaller and insignificant; but when state trends are controlled for (columns 3 and 4), the estimated effects on union membership and coverage are around .13 to .14 and highly significant. The difference state trends make for firefighters implies that firefighter union membership and coverage was not growing as fast at baseline in states that adopted duty-to-bargain laws, possibly because their unionization rate was already much higher, as the estimates in column (1) showed. For police (panel C), duty-to-bargain laws increased union membership by an estimated .061 (s.e. = .024) when state trends are controlled for. The estimated effect on union coverage is smaller, .044, and insignificant. The smaller estimates for police should be interpreted with caution, as they may be affected by the shocks to police membership and coverage prior to a law change

discussed above. As a whole, these results support the conclusion that enacting collective bargaining rights significantly increased union presence in terms of membership and coverage among public school teachers and firefighters, with a more modest effect on union presence among police.

Effects on Compensation and Hours

The next set of analyses answers the question of whether collective bargaining laws also affected compensation and hours, perhaps in part through the increased union presence estimated above. The results suggest that collective bargaining laws had a minimal effect on teachers' hourly wages but modestly reduced weekly hours. For firefighters the estimates point to a sizeable increase in wages and possibly a slight reduction in hours. The evidence for police suggests an increase in the hourly wage but a decrease in hours, with an insignificant effect on annual earnings. Table 4 reports estimates and standard errors of the effect of duty-to-bargain laws on earnings and hours variables for all three occupations. The cross-section results in column (1) show that for all three occupations, duty-to-bargain laws are associated with a 10 to 18 log point increase in hourly wages, a 6 to 10 point increase in annual earnings, and a decrease in weekly hours worked, especially for firefighters. These estimates are consistent with previous literature, which has found higher earnings among public employees who bargain collectively and, especially for firefighters (Ashenfelter 1971), lower hours. These differences may partially reflect unobserved differences between states with and without duty-to-bargain requirements, however. For teachers (panel A), the estimates for the effect on hourly wages that control for state effects and state trends are fairly precisely estimated to be near zero for each of the specifications in columns (2) through (4). The estimates are precise enough to rule out more than a 2 or 3% effect on hourly wage. Estimates for teachers' annual and weekly hours, however, are negative and highly significant, ranging from about -3 to -4 log points. The net effect on teachers annual earnings is therefore also about -3 to -4 log points, although less precisely estimated. The estimated effects on weeks per year are very close to zero, $-.005$ (s.e. = $.008$).

The DD estimates for firefighters (panel B) point to a significant 10 to 15 log point increase in hourly wage, with a smaller effect on annual earnings, although those estimates are noisier and not significant. Estimates for annual and weekly hours controlling for state trends range from about $-.03$ to $-.05$, but are not significant. The estimated effects on weeks per year are very close to zero.

Finally, the DD estimates of the effects for police in panel C suggest a modest effect on hourly wages, with the most reliable specification in column (4) showing a marginally significant $.074$ (s.e. = $.045$). The corresponding estimate for annual hours is a significant $-.038$ (s.e. = $.018$), which appears to be driven primarily by a reduction in hours per week (estimate = $-.025$, s.e. = $.015$). The net effect on annual earnings is thus smaller than the effect on the hourly wage and is not significant (estimate = $.036$, s.e. = $.051$).

Table 4. Effects of Duty-to-Bargain Laws on Pay and Hours

<i>Dependent variable</i>	<i>X-section</i>	<i>Differences-in-differences</i>		
	(1)	(2)	(3)	(4)
	A. Teachers			
ln(hourly wage)	.113*** (.025)	.005 (.012)	.007 (.014)	.000 (.014)
ln(annual earnings)	.061*** (.017)	-.025* (.014)	-.034* (.019)	-.038* (.020)
ln(annual hours)	-.053*** (.015)	-.030*** (.010)	-.031** (.013)	-.038** (.016)
ln(hours/week)	-.041*** (.010)	-.025*** (.008)	-.036*** (.010)	-.033*** (.011)
ln(weeks/year)	-.012** (.006)	-.005 (.005)	-.005 (.007)	-.005 (.008)
	B. Firefighters			
ln(hourly wage)	.179*** (.047)	.096* (.054)	.152*** (.056)	.129** (.064)
ln(annual earnings)	.095*** (.030)	.076 (.047)	.104 (.073)	.092 (.078)
ln(annual hours)	-.084*** (.026)	-.020 (.018)	-.048 (.041)	-.037 (.039)
ln(hours/week)	-.086*** (.027)	-.014 (.020)	-.042 (.036)	-.034 (.033)
ln(weeks/year)	.002 (.005)	-.006 (.007)	-.006 (.015)	-.003 (.015)
	C. Police			
ln(hourly wage)	.103** (.044)	.048* (.028)	.028 (.049)	.074* (.045)
ln(annual earnings)	.083* (.044)	.017 (.029)	-.032 (.057)	.036 (.051)
ln(annual hours)	-.020*** (.007)	-.031* (.017)	-.061*** (.020)	-.038** (.018)
ln(hours/week)	-.017*** (.006)	-.019 (.012)	-.034* (.017)	-.025* (.015)
ln(weeks/year)	-.003 (.006)	-.012 (.010)	-.027* (.015)	-.013 (.013)
Controls?	Y	Y	N	Y
State effects?	N	Y	Y	Y
State trends?	N	N	Y	Y
Region x year effects?	N	N	N	Y

Notes: Regression coefficients and clustered standard errors (by state) on the collective bargaining (CB) law variable. Models indicating “Y” for controls include year effects, fraction of workers in manufacturing, sex, race, age, education, and marital status, population, and annual percentage population change as additional covariates. Data are from the CPS from years 1968 through 1996 (1962 through 2010 for teachers), with samples as described in the text.

*Statistically significant at the .10 level; ** at the .05 level; *** at the .01 level.

As a whole, the estimates in Table 4 support the notion that collective bargaining rights result in a reduction of hours of about 3 to 4 log points, which is very similar across the three occupations. The effect on wages, however, differs substantially across occupations. Duty-to-bargain laws appear to

make very little difference in teachers' hourly wages, while substantially raising firefighters' wages and modestly increasing police wages.

The wage increases found for firefighters and police agree with much of the previous literature that has found public-sector collective bargaining associated with positive wage gaps, but smaller than for the private sector. The results for school teachers, however, contrast strongly with some of the earlier literature, but are consistent with more recent findings that have also found limited effects for teachers. The zero wage effects rule out significant increases in money pay but may not rule out increases in total compensation, including fringe benefits or working conditions (e.g., class size).

Effects on Per-Pupil Salary and Education Spending

While collective bargaining laws had little effect on teachers' wages, they may have increased compensation in other ways, such as through increased retirement benefits, or increased employment, which would improve working conditions by reducing class sizes. The following analyses of per-pupil salary expenditure and per-pupil education expenditure—which includes expenditure on all benefits—test whether collective bargaining laws had effects on other benefits or employment for teachers.⁸ The results suggest that collective bargaining laws had little effect on per-pupil salary expenditure and educational expenditure. Table 5 reports coefficients and standard errors from regressions in which the dependent variables are the log of per-pupil salary and the log of per-pupil educational expenditure. The sample period is from 1967 to 2006, the period for which expenditure data were available. The results in column (1) show that in the cross section states with duty-to-bargain laws have much higher per-pupil salary and educational expenditure than states without. However, the DD results in columns (2) through (4) show effects small in magnitude and insignificant for all specifications, with an estimate of -0.025 (s.e. = $.021$) for log per-pupil salary and -0.034 (s.e. = $.023$) for log per-pupil expenditure in the most reliable specification in column (4). The point estimates here are quite close to the reductions in annual earnings reported in Table 5 for teachers, despite being based on data from completely different sources. The lack of evidence for any increase is also consistent with Lovenheim's (2009) finding, using a district-level differences-in-differences design based on representation elections, that teacher unions have little effect on per-pupil spending. Assuming that collective bargaining laws had no effect on public school enrollment, and given the zero effect on individual wages from Table 4, the result on per-pupil salary implies that collective bargaining laws did not increase teacher employment or reduce class sizes on average.⁹ The result on per-pupil expenditure further implies that

⁸Instructor salary accounts for more than two-thirds of education expenditure, and post-1992, when separate data are available, benefits account for an additional 17%. Thus, the current education spending measure, while it potentially includes a wide variety of items, chiefly reflects teacher compensation, while the per-pupil normalization captures changes in class size, which is possibly an important compensating differential and measure of employment change.

⁹Lovenheim (2009) actually found an effect on enrollment, although it is not clear what mechanism could be driving this.

Table 5. Effects of Duty-to-Bargain Laws on Per-Pupil Education Expenditure, 1967–2006

<i>Dependent variable</i>	<i>X-section</i>		<i>Differences-in-differences</i>	
	(1)	(2)	(3)	(4)
ln(per pupil salary)	.152*** (.043)	-.013 (.018)	-.030 (.022)	-.025 (.021)
ln(per pupil expenditure)	.186*** (.046)	-.012 (.018)	-.037 (.023)	-.034 (.023)
Controls?	Y	Y	N	Y
State effects?	N	Y	Y	Y
State trends?	N	N	Y	Y
Region x year effects?	N	Y	Y	Y

Sources: Data are from the Census Bureau's Historical Database on Individual Government Finances.

Notes: Regression coefficients and clustered standard errors (by state) on the collective bargaining (CB) law variable. All regressions control for year effects, fraction of workers in manufacturing, sex, race, age, education, and marital status, population, and annual percentage population change, in addition to the factors indicated in the bottom four rows.

*Statistically significant at the .10 level; ** at the .05 level; *** at the .01 level.

the effect of collective bargaining laws on educational expenditure other than salary—the lion's share of which is employee benefits—is also minimal.

The null effects found here for teachers, while consistent with Smith's (1972) study of collective bargaining and teacher salaries, are at odds with early studies of teacher union impacts such as Baugh and Stone (1982), which found large union effects on teacher pay. They are consistent, however, with more recent studies such as Lovenheim (2009) and Lindy (2011), which found negligible effects of collective bargaining on teacher pay using more reliable research designs based on natural experiments that plausibly control for unobserved confounding factors. On their face, these results would also seem to be at odds with Hoxby's (1996) findings that teacher unions increase per-pupil spending. However, as described above, the implied reduced form impact of collective bargaining laws on spending in that paper's specification (but not reported there) is quite small (about .02), within the confidence intervals of the findings here.

Why would collective bargaining rights have no effect on compensation levels? One possible explanation is that teachers' unions have little bargaining power, or that compensation and employment levels are secondary to other union objectives.¹⁰ While this is a possibility, it is not the only explanation. Another possibility is that granting formal collective bargaining rights has little impact on effective bargaining between teachers and employers. For example, school districts in states that prohibit collective bargaining still often solicit input from teacher representatives when setting policies (Hess and West 2006). More detailed analysis below suggests that laws permitting (though not mandating) bargaining may have a significant effect on

¹⁰That teachers' unions do not push for higher compensation and employment flies in the face of traditional models of union objectives (Dunlop 1944), but tenure and professional development are often associated with teacher union goals (see <http://www.aft.org/issues/>).

teachers' compensation, supporting the conjecture that formally mandating bargaining for teachers has little incremental effect. There may also be spill-over effects if school districts tend to benchmark compensation levels with other districts, including those in other states. If this is the case, collective bargaining rights as a whole may have substantial general equilibrium effects, but for a particular state taking other state policies as given, granting collective bargaining rights may have minimal effects, consistent with the results found here. Still, these explanations could potentially apply equally well to firefighters and police and so do not necessarily account for the difference between the wage effects for teachers and the other two occupations. One factor that distinguishes teachers from the other occupations is number: In 1996 there were roughly 10 times as many school teachers as firefighters and three times as many school teachers as local police. A given wage increase for teachers may therefore place much more strain on local budgets than the same wage increase for police or firefighters. As a result, teachers may have relatively more success bargaining over other dimensions, such as working conditions, than over wages.

Another potential difference between teachers and the other occupations is mobility: If teachers are less geographically or occupationally mobile than firefighters and police, this could affect their bargaining position, as local employers would have a greater degree of monopsony power. Using the sample of teachers, police, and firefighters from the 1988 through 1996 CPS files, I constructed indicators for having moved to a different county within the past year, having moved to a different state in the past year, and having changed jobs in the past year. Teachers in the sample were slightly less likely to have moved from the county or state and to have changed jobs than firefighters or police. The difference was highly significant for changing jobs, but only marginally significant for moving between counties or states. These differences in mobility are therefore suggestive of a potential explanation for the small wage effects for teachers.

Robustness of the Results and Alternative Specifications

The main findings that collective bargaining laws have little impact on teachers' wages while increasing firefighters' and police wages and shortening the workweek across all occupations are robust to alternative choices of sampling period, specifications of the collective bargaining law variable, and sets of controls used in the analysis.

Restricting the sampling period to one that is uniform across all outcomes and occupations leaves the estimates qualitatively unchanged, although somewhat less precise. Appendix Tables A.2, A.3, and A.4 report estimates of the effects of duty-to-bargain laws on union presence, pay and hours, and educational expenditure as in Tables 3, 4, and 5, but restricting to the years 1973 to 1996, the period when data are available for all outcomes across all three occupations. In Appendix Table A.2, reporting effects on union presence, panels B and C for firefighters and police are identical to panels B

and C in Table 3, since that analysis was already restricted to 1973 to 1996. Even in panel A for teachers, though, where the sampling period is different, the estimates are extremely similar, showing positive effects on union membership and coverage of nearly the same magnitude. Appendix Table A.3 shows that the estimated effects on pay and hours are not substantively affected by the change in sampling period. Estimates for teachers show very small effects on the hourly wage and negative effects on hours, although they are less precisely estimated in this more limited sampling period. Likewise, estimates for firefighters show substantial positive effects on the hourly wage and negative effects on hours, although these, too, are less precisely estimated. The police estimates for the limited sampling period continue to show significant reductions in hours, but the estimates for the hourly wage, while still positive for the most reliable specification, are no longer significant. Appendix Table A.4 reports estimates of the effects of duty-to-bargain laws on per-pupil salary expenditure and per-pupil total expenditure restricted to the years 1973 to 1996. Like the full sample results in Table 5, the cross-section results in column 1 show much higher levels of spending in states with duty-to-bargain laws, but when state effects and trends are controlled for, the estimated effect is much smaller or even negative, although not significantly different from zero. The specification without covariates (column 3) is slightly more negative and marginally significant. These estimates and the full-sample results in Table 5 are consistent with the negative effects on teachers' annual earnings in Table 4.

Collective bargaining laws entail a wide variety of requirements, ranging from no statutes at all, to bargaining permitted but not required, to a state-mandated duty to bargain. Regression analysis of collective bargaining laws could therefore specify the laws in a variety of ways. The analysis so far has focused on a binary duty-to-bargain indicator for precision and transparency of interpretation, but the main qualitative findings are largely unchanged under alternative specifications. Appendix Tables A.5 through A.8 report alternative estimates of the effects on union membership, union coverage, log hourly wages, and log weekly hours. Each table reports estimates from a linear specification and a more flexible dummy specification. The linear specification constructs a collective bargaining law index equal to 0 if collective bargaining is prohibited or if there is no collective bargaining statute (courts have typically interpreted the absence of a law as prohibition); equal to 1 if collective bargaining is permitted (but not required) or if worker groups have the right to present proposals or meet and confer; and equal to 2 if laws mandate a duty to bargain. The coefficient on this index corresponds to a weighted average of a one-step change from collective bargaining prohibited to permitted, and from permitted to required. This captures the typical case; of the 180 bargaining law changes over the period considered, 122, or just more than two-thirds, involved such a one-step change. The more flexible dummy specification includes indicators for each level of the collective bargaining index, omitting the category corresponding to collective bargaining prohibited or no collective bargaining laws.

Estimates of the effects of collective bargaining laws on union presence in terms of union membership and coverage from these alternative specifications support the findings in the main analysis. Appendix Table A.5 reports estimates for union membership from the linear and dummy specifications. As in the main results in Table 3, the estimates show modest but significant coefficients in the linear specification in the range of .03 to .06 for the model that includes state trends and controls (column 7). The dummy specification in column (8) shows that most of this impact is driven by the duty-to-bargain indicator, with the “bargaining” permitted indicator insignificant for each of the three occupations, perhaps justifying the focus on the duty-to-bargain indicator in the main results. As in the main specification, the estimates for firefighters and police are much smaller (and even negative) when state trends are not included, implying that the secular trend in union growth was higher in states that did not have bargaining laws. The linear and dummy specifications for union coverage (Appendix Table A.6) also support the main analysis in Table 3, with stronger effects for teachers and insignificant effects for police.

Estimates from the alternative specifications of the effects of collective bargaining laws on wages support the main results in Table 4 and also suggest some new insights. Appendix Table A.7 reports estimates from the linear and dummy specifications of collective bargaining laws on log hourly wages for teachers, firefighters, and police. As in the main results, the cross-section results show that wages are higher across all three occupations in states with stronger collective bargaining laws, especially where there is a duty to bargain. When state effects and state trends are controlled for, the coefficients on the linear index and on the duty-to-bargain indicator in the dummy specification for firefighters and police become much more modest, though still positive and marginally significant. For firefighters, the estimated coefficient on bargaining permitted is actually negative when state trends are included, though it is extremely imprecisely estimated and is likely attributable to sampling variation in hours worked (see below). For teachers, the coefficient on the linear specification drops very close to zero when state effects and state trends are included, as in the main results. The dummy specifications may suggest some new insights on the impact of collective bargaining laws on teacher wages, however. The coefficient on bargaining permitted is positive and significant when state trends are controlled for, while the coefficient on duty to bargain is smaller and only marginally significant. This finding is consistent with the main results that duty-to-bargain laws do not have a large impact on teacher wages, but suggests that the reason may be that teachers realize most of the benefits of collective bargaining (at least in terms of wages) when collective bargaining is merely permitted or when teachers have the right to meet and confer or present proposals. This supports the conjecture made in the discussion at the end of the presentation of the main results that a likely explanation for the small impact of duty-to-bargain laws for teachers is because teachers are able to bargain effectively over wages even in the absence of a formal mandated framework.

The results from linear and dummy specifications of the effects of collective bargaining laws on weekly hours largely support the findings in the main analysis in Table 4. Appendix Table A.8 reports estimates from the linear and dummy specifications of the effects on log weekly hours. As in the main results, cross-sectional regressions show uniformly lower hours per week across all three occupations in states with stronger collective bargaining laws. For teachers and police, the negative effect on hours largely holds up when state effects and state trends are controlled for, as in the main analysis. For firefighters, the main results showed much weaker evidence for a decrease in hours, and the alternative specifications here in panel B show no evidence at all. The coefficients on the linear collective bargaining law index and the duty-to-bargain are very close to zero. The coefficients on the bargaining permitted indicator in models with state trends are actually positive, though very noisily estimated and not significant.

The estimates in the main results are also robust to alternative sets of controls and states. Estimates from models controlling for state-specific quadratic and cubic trends, models that deflate wages and earnings by a state-specific cost-of-living index, models that omit CPS state-groups with non-uniform collective bargaining laws, and models that omit observations for police during the two years prior to a change in collective bargaining laws are largely similar to the main results. Appendix Table A.9 reports estimates from the alternative control strategies for union membership and coverage, and Appendix Table A.10 reports estimates for pay and hours. These tables report only estimates from models controlling for state effects and state trends. The estimates for union membership and coverage in Appendix Table A.9 look very similar with two exceptions. Estimates from models with state-specific cubic trends for teachers appear somewhat attenuated and either marginally significant or insignificant, although they are noisy enough that one would not reject that their probability limits are equal to those of the estimates in the main results. The second difference is that estimates for police that omit observations within two years prior to a law change are much more pronounced, which is not surprising given the patterns in Appendix Figure A.1. Estimates in Appendix Table A.10 that use the alternative control strategies for the impacts on pay and hours are also largely similar to the main results. As in the main results, the effects on teachers' hourly wages are very close to zero for each of the alternative control strategies, while effects on weekly hours are significantly negative. For firefighters (panel B), the estimated effects on wages look similar to the main results, except for the model with state-specific cubic trends (lower), which is estimated much less precisely than the others. As in the main results, none of the estimates for firefighters' hours are significantly different from zero. For police (panel C), the main results that duty-to-bargain laws appear to have modestly increased police hourly wage while somewhat reducing hours are largely borne out in the models with alternative control strategies. As with other outcomes, the effects on wages and earnings are much more pronounced when the two years prior to a law change are

omitted, and attenuated when state-specific cubic trends are added to the model.

Finally, the estimates are robust to models that account for a possible interaction between collective bargaining laws and right-to-work laws (RTW). RTW laws are prohibitions against union security clauses such as a union shop, where joining the union within some specified time after hire is a condition of employment. These estimates explore the possibility that RTW statutes partially nullify the effects of collective bargaining rights (Ichniowski and Zax 1991), suggesting that the main estimates are an average of possibly very different effects in states with and without RTW statutes for public employees. Estimates controlling for RTW interactions suggest this is largely not the case: The estimates are very similar to the main estimates, implying that the effects of collective bargaining requirements do not vary significantly by RTW status. The estimates reported in Appendix Tables A.11 and A.12 are identical to Tables 3 and 4 except they include an indicator for a public employee RTW law and the interaction of that indicator with the collective bargaining requirement indicator as additional controls. The reported coefficients on the collective bargaining main effect thus have the interpretation as the effect in non-RTW states. These effects look very similar to the corresponding main effects with one possible exception: The estimates effects on firefighters' wages and earnings appear substantially larger when controlling for RTW interactions, meaning wage gains for firefighters may have been concentrated in non-RTW states. None of the interaction coefficients (not reported) were significant, however, so this is only suggestive.

Conclusion

Using an estimation strategy based on differences in timing across states of changes in collective bargaining rights for public employees, this article provided evidence on the effects of collective bargaining rights on union presence, pay, and hours of teachers, firefighters, and police. The estimates suggest that while laws mandating a duty to bargain on the part of the employer appeared to increase union presence across all three occupations, the impacts on pay were mixed. Among public school teachers, duty-to-bargain laws had a minimal effect on hourly wage, or, to the extent captured by per-pupil education expenditures, on benefits or employment. Duty-to-bargain laws significantly, though modestly, reduced school teachers' hours, however. For firefighters, the evidence suggests duty-to-bargain laws substantially increased hourly wages, with a smaller and statistically insignificant reduction in hours. The effects for police were similar to firefighters, though evidence of a wage increase was weaker and evidence of hours reductions stronger.

Previous studies of public-sector collective bargaining have typically found higher pay, a shorter workweek (at least for firefighters), more generous benefits, and greater employment. Current debates in state legislatures also imply that stakeholders believe the fiscal consequences of public employee collective bargaining rights are substantial. The results found in

this article, however, suggest that the causal effects of collective bargaining rights may be more limited, especially for teachers. Even for firefighters and police, for whom there is evidence of a positive wage effect, the fiscal consequences are partially offset by the negative effect on hours. While qualitatively similar to earlier research on collective bargaining rights in terms of the directions of the effects, the quantitatively smaller estimates found here plausibly control more fully for unobserved confounding factors at the state level that may have biased earlier cross-sectional estimates.

In summary, the evidence suggests that enacting collective bargaining laws had on the whole a relatively small effect on state spending for public employees. One caveat to this interpretation for the current debate is that the effect now of revoking collective bargaining rights may not simply be the reverse of granting these rights in the past. Another caveat is that the data used in this article would not have reflected any unfunded retiree benefits, and if collective bargaining rights increased this type of benefit, the results may understate the true impact of collective bargaining rights. Last, the estimates represent the average effect across unionized and nonunionized public employees, and may mask heterogeneity in the true effect size across these groups. Though the bulk of public employees in the occupations and time periods analyzed are unionized, the causal effects among unionized workers only may be larger than the average effects identified here.

A puzzle raised by the results is that the effects of collective bargaining rights vary substantially across occupations. Why would collective bargaining rights have a significant effect on firefighters' compensation, while having little effect for teachers? This finding is especially puzzling considering that where firefighters have bargaining rights, they are much less likely to be permitted to strike than are teachers (Valletta and Freeman 1988). Possible explanations may relate to differences in number—there are roughly 10 times more teachers than firefighters, making a given wage increase for teachers much more burdensome on local budgets than the same increase for firefighters or police—or to differences in geographical or occupational mobility. Another possibility is that teachers are more able to effectively bargain outside of the formal apparatus than other occupations even in states where statutes do not provide for collective bargaining. Determining what factors influence whether groups of workers are able to change outcomes at the bargaining table is left for future research.

Appendix A

CPS Sample Selection

Two different CPS extracts were used in the analysis: one for estimation of the effects on union membership and union coverage, and another for the effects on earnings and employment outcomes. The extract for union outcomes was drawn from the 1973 through 1981 May supplement files and the 1983 through 2010 Merged Outgoing Rotation Group (MORG) files, obtained

from the National Bureau of Economic Research (NBER). The extract for other outcomes was drawn from the 1962 through 2010 March files, extracted using the IPUMS system. In all cases, observations were selected if: 1) on the basis of the employment status variable they were employed (at work or with a job) and had strictly positive earnings; 2) on the basis of class of worker they were government employees, or for files from years after 1987, state or local government employees; 3) on the basis of industry and occupation variables they were either police, firefighters, or elementary or secondary school teachers; 4) they were between 18 and 65 years old; and 5) their earnings and hours were not allocated. For years prior to 1977 not all states were individually identifiable, so collective bargaining law indicators were averaged over state groups for each occupation for those years. Simply dropping those state group-year-occupation cells for which collective bargaining laws were not constant did not affect any of the results. All variables with money units were converted to year 2000 dollars using the CPI. In some years, weeks worked were intervalled, so for these years weeks worked were imputed using cell means from years for which actual weeks worked were available, where the cells were defined by weeks worked interval, occupation, year, sex, marital status, race, education, age, and state. Finally, the extract used for analysis of earnings and wages was trimmed using the criteria in DiNardo, Fortin, and Lemieux (1996), where only observations with an hourly wage from \$1 to \$100 (in 1979 dollars) were retained.

Historical Database on Individual Government Finances

Analysis of per-pupil education and salaries was done using a data set constructed from the U.S. Census Bureau's Historical Database on Individual Government Finances, downloaded from the Census Bureau's website. Records in this database are at the government unit and year level. For the years 1967 and 1970–2006 (the years for which per-pupil spending data were available), records were selected when: 1) they corresponded to school districts (type code = 5); 2) enrollment was positive; 3) they reported positive elementary and secondary spending; 4) total education expenditure was equal to total elementary and secondary education expenditure; 5) total salary expenditure was positive; and 6) elementary and secondary education direct expenditure was positive. Appendix Table A.1 shows how many records remained after applying each criterion. Of the school district records with positive enrollment, around 3% of records were dropped. The resulting extract was aggregated to the state and year level, and total salary was divided by total enrollment to produce a state- and year-level per-pupil salary measure. Total (current) education expenditure was defined by subtracting elementary and secondary education capital outlay from elementary and secondary education direct expenditure, and dividing by total enrollment. Errors in the enrollment data for certain states-year combinations were reported by the Census Bureau (by way of e-mail correspondence), so these observations were flagged and dropped.

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