

After four decades of largely 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 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, fire fighters, and police, comprising over a quarter of state and local public employment.

Many studies have documented a substantial difference in pay and benefits between public sector employees in environments favoring collective bargaining and those in environments that do not. Freeman and Valletta (1988) found public sector workers in states with laws favorable to collective bargaining had around 6 percent higher wages than workers in states with no provisions for collective bargaining. Zax and Ichniowski (1990) documented a substantial increase in local government unionization in states with duty-to-bargain laws over the five-year period 1977 to 1982.

One mechanism by which collective bargaining laws may affect outcomes is by encouraging the formation of unions, who in turn use the collective bargaining framework to achieve gains for their members (Hoxby 1996). Focusing on public school teachers, Baugh and Stone (1982) found a union/nonunion wage gap of about 12 to 22 percent in the late 1970s. Kearney and Morgan (1980) also found significant wage gaps for state employees in a variety of occupations. Studies of fire fighters have found significantly higher compensation when a union is present, due primarily to a shorter work week 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, concludes that the public sector union gap is about 8 to 12 percent, which includes a substantial gap in fringe benefits.

An important question raised by the studies documenting public sector compensation gaps is whether the differences can be attributed to the causal effect of collective bargaining laws, or

whether they reflect unobserved differences between workers who enjoy collective bargaining rights and those who do not. Several previous studies have used longitudinal, within-city, or sample selection designs to control for unobserved factors and isolate the causal effects of collective bargaining rights, or the leading channels of union representation or union contracts. Valletta (1993) estimated the effect of union contracts on wages and employment for several municipal government departments over the period 1977 to 1980, and, for fire fighters, 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) and found a positive effect of collective bargaining agreements on department expenditures, although this was possibly due to offsets elsewhere, as little evidence was found for total municipal expenditures. More recently, two studies of a quasi-experimental flavor have found similarly mixed evidence on the effects of teacher unions on wage, employment, and expenditures. Lovenheim (2009), using a differences-in-differences design based on teacher union certifications found very little union 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 also finds that collective bargaining rights have little effect on per-pupil spending. While these studies focus on relatively narrow periods, the mixed evidence on wage and employment effects calls into question the reliability of the earlier literature and suggests the need for a broader study. The current study builds on this evidence by using variation in collective bargaining rights over a much longer time horizon, yielding more reliable inference and the ability to control more thoroughly for prior trends.

This paper estimates the causal effect of public sector collective bargaining using data from all U.S. states and over the period from the 1960s to 2010. It controls for unobserved confounding factors by exploiting differences across states in the timing of laws governing the collective bargaining rights of public employees, combining data on public sector collective bargaining laws, the Current Population Survey, and the U.S. Census Bureau's historical database on individual government finances in a differences-in-differences framework. The paper's main contribution is in compiling and combining these disparate sources of data and applying a generalized differences-in-differences strategy to control for unobserved confounding factors across states and over time. In its attempt to control for these unobserved factors, the paper is similar to Zax and Ichniowski (1990), who employed a clever design which 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. This paper complements that analysis by controlling for classes of confounding factors that may not be captured by past propensity to unionize, by expanding the analysis beyond the 1977 to 1982 time frame covered by that paper, and by estimating effects on wages and hours.

The main findings are that collective bargaining rights increase the presence of unions among public employees but have a mixed effect on compensation. The estimates suggest that collective bargaining rights have little effect on teachers' pay or benefits. Fire fighters' pay and benefits, however, appear to be modestly increased, as does police pay. Further evidence from school districts implies that collective bargaining has little effect on teacher employment or fringe benefits. The findings imply that formal collective bargaining rights have at most a modest impact on public sector labor costs for these occupations.

In exploiting the differences in timing across states of changes in collective bargaining rights for public employees this paper is similar to Hoxby's (1996) study of the impact of teachers' unions on education outputs, which uses changes in collective bargaining laws as an instrumental variable for changes in teacher unionism. That study finds a significant effect of teacher unionism on school inputs, which would seem to be at odds with this paper and with Lovenheim (2009). However, the numerical magnitude of the effect of collective bargaining laws implied by the results in Hoxby (1996) is very close to the findings reported here.

Background

The organized labor movement in the public sector got its start later than 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 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 Kennedy's 1962 Executive Order 10988, which recognized unions in the federal sector (Marlow and Orzechowski 1996). By 2010, 36.2 percent of public sector employees were members of a union, while only 6.9 percent of private sector employees were (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 the early 1960s (Valletta and Freeman 1988).¹

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

¹ Laws requiring collective bargaining typically impose a "duty to bargain" on the part of the government, which requires the government to bargain in good faith if a union presents itself, but does not require that an agreement actually be reached.

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

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 are questions that 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 dataset on public sector collective bargaining laws was originally constructed by Richard Freeman and Robert Valletta in 1985 (see Valletta and Freeman 1988), and codes the relevant laws for every state and every year from 1955 to 1985 and for five different occupational groups. This dataset was later extended by Kim Rueben to cover the years through 1996. This paper uses the extended Rueben dataset as a starting place, and augments it using data on public school teacher collective bargaining laws in Lindy (2011) and 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, the analysis in this paper distills state laws on collective bargaining into three categories: prohibited, permitted, and required. The prohibited category includes statutes which explicitly prohibit state employers from bargaining with worker representatives, but also situations where state law makes no provision for collective bargaining, as courts have typically interpreted this as prohibiting collective bargaining (Freeman and Valletta 1988). The permitted category includes statutes which authorize the employer to

² As Freeman (1986) notes, 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.

³ Coverage by collective bargaining is captured by the CPS question on whether the respondent is covered by a union contract. This question is unfortunately only asked 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.

bargain and which give employee organizations the right to present proposals or meet and confer with the employer. The required category includes statutes which either imply or make explicit the duty of the employer to bargain. 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 showed, most of the relevant laws took effect in either the 1960s or 1970s, although there were a number of changes after 1980.

{{Place Table 1 about here}}

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The second source of data comes from the 1962 through 2010 Current Population Surveys (CPS). Extracts containing age, race, sex, state, education, earnings, hours, benefits, and union status variables for public sector teachers, fire fighters, 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, Ruggles, Alexander, Flood, Genadek, Schroeder, Trampe, and Vick 2010), and were available every year. 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 to 2010.⁴ Records were selected where the individual was employed, had strictly positive earnings, were state or local government employees, and were police, fire fighters, or elementary or secondary school teachers between 18 and 65 years old. Records with allocated earnings and hours were dropped. The appendix contains further details. Summary statistics of these data are reported in Table 2. The table shows that for teachers, fire fighters, 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.

{{Place Table 2 about here}}

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. This paper uses 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

⁴ The May supplements and MORG files were obtained from the National Bureau of Economic Research website at http://www.nber.org/data/cps_index.html.

secondary education were selected and collapsed to the state-year level. The appendix contains further details.

{{Place Figure 2 about here}}

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—chosen for their differences in timing of collective bargaining laws, which are also shown in the figure. As the figure shows, New Hampshire enacted a law requiring school districts to bargain collectively with teachers at the end of the 1970s, shortly following which per-pupil education spending and per-pupil salary started to rise sharply. Was this rise due to collective bargaining rights? The case of 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 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-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

In order to control for possible confounding factors in the relationship between collective bargaining laws and outcomes, this paper uses a differences-in-differences design taking 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 factors that trend over time. The third specification includes both region-by-year effects and state-specific trends, 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 is the generalization of checking for parallel trends in the canonical two-group differences-in-differences design. This check of the identification strategy yields encouraging results for teachers and firefighters, but may indicate some misspecification for police. Figure 3 plots coefficients and 95-percent confidence 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 union membership which includes region-by-year effects and state-specific trends separately for teachers, fire fighters, and police. 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. 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 fire fighters 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, 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 due 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 this is the case then caution may be warranted in interpreting the results on police in the next section.

{ {Place Figure 3 about here} }

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

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 to code a variable equal to zero if collective bargaining is prohibited, one if it is allowed but not required, and two if it is required. The appendix reports results for a more flexible specification with dummy variables for collective bargaining allowed and required. The linear specification is not rejected in the majority of these specifications and is much more precise and so we take it as the main specification. Even if the true effect is nonlinear, the coefficient δ represents a weighted average of the effect of moving from a regime where collective bargaining is prohibited to a regime where it is allowed and the effect of moving from a regime where collective bargaining is allowed to a regime where it is required. This captures the typical case; of the 180 collective bargaining law changes over the period considered, 122, or just over two-thirds, involved such a “one-step” change.

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 and fire fighters, and may have had a modest effect on union presence for police. Table 3 reports estimates and standard errors of the effect of collective bargaining laws on indicators for union membership and union coverage separately for teachers, fire fighters, and police. The large and highly significant estimates in the range of .11-.20 in column (1) 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 effects are controlled for (column 2) the estimate for teachers in Panel A drops to a more modest .05-.06, although still highly significant. Columns (3) and (4) control in different ways for time-varying effects (see previous section), all of which give estimates in the .05-.06 range for teacher union membership and .08-.09 for teacher union contract coverage. For fire fighters (panel B), when state trends (column 3) or state trends and region-by-year effects (column 4) are included the estimated effect is about .05-.06. The evidence for fire fighters supports small to moderate, but positive, effects on union presence. For police (panel C) the DD estimates of the effect on union membership and coverage drop to the marginally significant .02-.04 range when state effects and other controls are added. As a whole, these results support the conclusion that enacting collective bargaining rights increased union presence among public school teachers and fire fighters, with a more modest effect on union presence among police.

{{Place Table 3 about here}}

Effects on compensation and hours

The next set of analyses answers the question of whether collective bargaining laws also affected compensation and hours, perhaps through the increased union presence estimated above. The results suggest that collective bargaining laws had a minimal effect on teachers' compensation but perhaps a small negative effect on hours. For fire fighters the most reliable estimates point to a modest effect on earnings but no effect on hours. The evidence for police suggests an increase in the hourly wage but a decrease in hours, leaving annual earnings unchanged. Table 4 reports estimates and standard errors of the effect of CB laws on earnings and hours variables. The cross-section results in column (1) show that for all three occupations CB laws are associated with about a 6-10 log point increase in hourly wages, a 4-6 point increase in annual earnings, and a decrease in weekly hours worked, especially for fire fighters. These estimates are consistent with previous literature which has found higher earnings among public employees who bargain collectively and, especially for fire fighters (Ashenfelter 1971), lower hours. The estimates after controlling for unobserved confounding factors, however, suggest that much of this may reflect something other than the causal effect of collective bargaining rights. For teachers (panel A), the estimates for the effect on hourly wages 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 1 or 2 percent effect on hourly wage. Estimates for teachers' annual earnings are actually negative, but quite small and not significantly different from zero. Estimates for annual and weekly hours are also small, in the -.01 to -.02 range, and are occasionally statistically significant. The evidence on the whole is suggestive, though, that collective bargaining rights very slightly reduced teachers' hours, but with a corresponding slight reduction in annual earnings, so the effective hourly wage remained unchanged.

The DD estimates for fire fighters' (panel B) hourly wage and annual earnings are around .05 to .06 in the most reliable specification in column (4), but are not very precise. The estimates of the effect on hours are close to zero, with the most reliable estimate for weekly hours being -.003, and is precise enough to rule out the large negative effects from the previous literature.

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 significant .049 (s.e.=.024). The corresponding estimate for annual earnings is smaller and not significantly different from zero, consistent with the marginally significant negative effect on weekly hours.

As a whole, the estimates in Table 4 support the notion that collective bargaining rights have very little effect on teachers' money compensation, although there is suggestive evidence that collective bargaining rights lead to some reduction in hours with some salary offset. The results for fire fighters are consistent with a modest increase in cash compensation, but not with a

significant reduction in hours. The results for police imply a modest increase in hourly pay and a decrease in hours.

{ {Place Table 4 about here} }

The modest wage increases found for fire fighters 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 the earlier literature. 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). The next section will explore these issues for school teachers.

Effects on per-pupil salary and education spending

It is possible that while collective bargaining laws had little effect on teachers' pay, they may have increased compensation in other ways, such as 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.⁶ 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 where the dependent variables are the log of per-pupil salary and the log of per-pupil educational expenditure. The results in column (1) show that in the cross section states with collective bargaining laws have much higher per-pupil salary and educational expenditure than states without. However, the DD results in columns (2) through (4) show effects very close to zero for all specifications, with an estimate of $-.014$ (s.e.=.015) for log per-pupil salary and $-.012$ (s.e.=.015) for log per-pupil expenditure in the most reliable specification in column (4). This is 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 salaries 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.⁷ The result on per-pupil expenditure further implies that the effect of collective bargaining laws on educational expenditure other than salary—the lion's share of which is employee benefits—is also minimal.

⁶ Instructor salary accounts for over two-thirds of education expenditure, and post-1992, when separate data are available, benefits account for an additional 17 percent. 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.

⁷ Lovenheim (2009) actually finds an effect on enrollment, although it's not clear what mechanism could be driving this.

{{Place Table 5 about here}}

The null effects found here for teachers are at odds with early studies of teacher union impacts such as Baugh and Stone (1982), which found large union effects on teacher pay. However they are consistent 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, 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), close in magnitude to the findings here.⁸

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.⁹ 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). There may also be spillover 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.

Conclusion

Using an estimation strategy based on differences in timing across states of changes in collective bargaining rights for public employees, this paper found that while collective bargaining rights did increase union presence among public school teachers, they had little effect on average on public school teachers' compensation, hours, benefits, or employment. For fire fighters and police, on the other hand, the evidence suggests that collective bargaining rights did modestly increase the hourly wage by around 5 log points.

⁸ The large magnitude of Hoxby's (1996) estimated effect of unionization stems from dividing the small reduced form estimate by a similarly small "first-stage" estimate of the effect of collective bargaining laws on unionization status. The required assumption that collective bargaining laws affect outcomes only through their marginal impact on unionization rates is implausible in my setting. For example, if collective bargaining laws impact existing unions' ability to bargain, the exclusion restriction would be violated, biasing the estimated effect of union status upwards.

⁹ That 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/>).

Previous studies of public sector collective bargaining have typically found higher pay, a shorter workweek (at least for fire fighters), 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 paper, however, suggest that the causal effects of collective bargaining rights may be more limited, at least for certain groups of workers.

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 paper 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.

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 fire fighters' compensation, while having little effect for teachers? This is especially puzzling considering that where fire fighters 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 gender—teachers are 70 percent female in the sample, while fire fighters are 99 percent male—or differences in geographic mobility, but also to the possibility that public school teachers may be able to effectively bargain outside of the formal apparatus 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.

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Appendix

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 1983-2010 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: on the basis of the employment status variable they were employed (at work or with a job) and had strictly positive earnings; on the basis of class of worker they were government employees, or for files from years after 1987, state or local government employees; on the basis of industry and occupation variables they were either police, fire fighters, or elementary or secondary school teachers; they were between 18 and 65 years old; 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 where 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 dataset 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 where: they corresponded to school districts (type code = 5); enrollment was positive; they reported positive elementary and secondary spending; total education expenditure was equal to total elementary and secondary education expenditure; total salary expenditure was positive; elementary and secondary education direct expenditure was positive. Appendix Table 4 shows how many records remained after applying each criterion. Of the school district records with positive enrollment, around three percent 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 and years were reported by the Census Bureau (via email correspondence), so these observations were flagged and dropped.

Table 1. Timing of state laws governing public sector collective bargaining rights

	Before 1970	Between 1970 and 1980	After 1980
		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. Fire fighters	
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

Notes: Timing of passage of state laws either permitting or requiring employers to bargain collectively with public employees. Data are from Valletta and Freeman (1988), Kim Rueben's update thereof, Lindy (2011), and the National Council on Teacher Quality.

Table 2. Sample Means by Occupation for Selected Variables, 1973-1996

	All	CB law		required
		Prohibited	Allowed (excl required)	
A. Teachers				
N	71,7			48,17
Age	57	12,926	10,658	3
Female	39.4	38.5	38.2	39.9
Union member	0.68			
Union covered	5	0.763	0.690	0.658
hourly wage	17.0			
annual earnings	5	14.42	16.02	18.14
annual hours	330			
hours/week	43	28942	30849	34877
weeks/year	198			
	5	2047	1981	1965
	41.2	42.4	41.2	40.9
	47.3	47.5	47.1	47.2
B. Fire fighters				
N	5,49			
Age	0	637	992	3,861
Female	37.6	36.0	37.5	37.9
Union member	0.01			
Union covered	2	0.031	0.010	0.009
hourly wage	16.4			
annual earnings	8	13.86	13.64	17.81
annual hours	426			
hours/week	41	37570	37219	45200
weeks/year	274			
	0	2852	2900	2673
	53.7	55.6	57.1	52.3
	51.0	51.2	50.7	51.0

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		C. Police		
N	14,161	2,497	2,151	9,513
Age	36.6	36.8	36.5	36.5
Female	0.111	0.118	0.089	0.113
Union member	0.554	0.268	0.399	0.672
Union covered	0.598	0.309	0.436	0.718
hourly wage	17.93	15.34	16.02	19.09
annual earnings	38639	33163	34875	41019
annual hours	2174	2189	2192	2165
hours/week	42.9	43.4	43.3	42.7
weeks/year	50.6	50.5	50.5	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.

Table 3. Effects of Collective Bargaining Laws on Union Presence, 1973-1996

Dependent variable	X-section		Differences-in-differences			
	(1)		(2)	(3)	(4)	
A. Teachers						
Union membership	.196 *** (.016)		.058 *** (.016)	.050 *** (.013)	.055 *** (.016)	
Union covered	.195 *** (.013)		.072 *** (.011)	.088 *** (.015)	.085 *** (.016)	
B. Fire Fighters						
Union membership	.124 *** (.037)		.027 (.020)	.053 ** (.026)	.060 ** (.025)	
Union covered	.113 *** (.037)		.024 (.019)	.054 ** (.025)	.059 ** (.027)	
C. Police						
Union membership	.177 *** (.037)		.036 ** (.017)	.016 (.021)	.035 ** (.015)	
Union covered	.175 *** (.038)		.029 * (.016)	.012 (.021)	.029 (.019)	
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. All regressions control for year effects, sex, race, age, education, and marital status, in addition to the factors indicated in the bottom four rows. Data are from the following CPS files: 1973-1981 May Supplements, the 1983-1996 Merged Outgoing Rotation Group, with samples as described in the text. *Statistically significant at the .10 level; **at the .05 level; ***at the .01 level.

Table 4. Effects of Collective Bargaining Laws on Pay and Hours, 1962-1996

Dependent variable	X-section		Differences-in-differences		
	(1)		(2)	(3)	(4)
A. Teachers					
ln(hourly wage)	.075 *** (.013)		-.001 (.007)	-.002 (.008)	.008 (.008)
ln(annual earnings)	.040 *** (.008)		-.010 (.010)	-.008 (.012)	-.013 (.012)
ln(annual hours)	-.035 *** (.008)		-.009 (.007)	-.005 (.009)	-.021 ** (.008)
ln(hours/week)	-.030 *** (.005)		-.011 ** (.005)	-.007 (.006)	-.018 *** (.006)
ln(weeks/year)	-.006 * (.003)		.002 (.004)	.002 (.004)	-.002 (.004)
B. Fire fighters					
ln(hourly wage)	.106 *** (.033)		.034 * (.019)	.010 (.025)	.052 (.033)
ln(annual earnings)	.059 *** (.021)		.018 (.015)	.031 (.022)	.059 * (.035)
ln(annual hours)	-.047 *** (.016)		-.017 (.017)	.021 (.015)	.007 (.024)
ln(hours/week)	-.047 *** (.016)		-.014 (.016)	.018 (.014)	.003 (.022)
ln(weeks/year)	.000 (.003)		-.002 (.003)	.003 (.004)	.004 (.008)

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C. Police

ln(hourly wage)	.062 ** (.025)	.004 (.010)		.050 *** (.014)	.049 ** (.024)
ln(annual earnings)	.050 * (.026)	-.020 (.014)		.034 (.024)	.019 (.030)
ln(annual hours)	-.012 *** (.005)	-.024 *** (.008)		-.016 (.012)	-.030 * (.015)
ln(hours/week)	-.011 *** (.003)	-.017 ** (.007)		-.013 (.009)	-.018 * (.011)
ln(weeks/year)	-.002 (.004)	-.007 * (.004)		-.004 (.006)	-.012 (.010)
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. All regressions control for year effects, sex, race, age, education, and marital status, population, and annual percentage population change, in addition to the factors indicated in the bottom four rows. Data are from the CPS from years 1962 through 1996, with samples as described in the text. *Statistically significant at the .10 level; **at the .05 level; ***at the .01 level.

Table 5. Effects of Collective Bargaining Laws on Per-Pupil Education Expenditure, 1962-1996

Dependent variable	X-section		Differences-in-differences		
	(1)		(2)	(3)	(4)
ln(per pupil salary)	.108 *** (.026)		-.028 * (.015)	-.012 (.017)	-.014 (.015)
ln(per pupil expenditure)	.132 *** (.028)		-.018 (.015)	-.011 (.016)	-.012 (.015)
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. All regressions control for year effects, sex, race, age, education, and marital status, population, and annual percentage population change, in addition to the factors indicated in the bottom four rows. Data are from the Census Bureau's Historical Database on Individual Government Finances. *Statistically significant at the .10 level; **at the .05 level; ***at the .01 level.

Appendix Table 1a. Effects of Collective Bargaining Laws on Union Membership, 1973-1996

	X-section		Differences-in-differences			
	(1)		(2)	(3)	(4)	
Teachers						
CB Permitted	.161 ***		.027	.044	.052	
	(.044)		(.038)	(.033)	(.035)	
Duty to Bargain	.387 ***		.097 **	.097 ***	.110 ***	
	(.032)		(.041)	(.032)	(.032)	
p-value for linearity	.434		.289	.801	.934	
Fire Fighters						
CB Permitted	-.057		-.121 ***	-.031	-.028	
	(.094)		(.032)	(.049)	(.053)	
Duty to Bargain	.193 **		.060 **	.100 **	.117 **	
	(.079)		(.030)	(.045)	(.046)	
p-value for linearity	.030		.000	.021	.097	
Police						
CB Permitted	.099		-.094 *	-.016	.019	
	(.094)		(.050)	(.039)	(.043)	
Duty to Bargain	.344 ***		.063	.027	.067 **	
	(.074)		(.040)	(.044)	(.034)	
p-value for linearity	.405		.002	.209	.651	
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 indicators. The reported p-value for linearity is from a t-test of the null hypothesis that the population coefficient on Duty to Bargain is twice the coefficient on CB Permitted (the restriction imposed by the CB index specification used in the main tables). All regressions control for year effects, sex, race, age, education, and marital status, in addition to the factors indicated in the bottom four rows. Data are from the following CPS files: 1973-1981 May Supplements, the 1983-1996 Merged Outgoing Rotation Group, with samples as described in the text.

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

Appendix Table 1b. Effects of Collective Bargaining Laws on Union Coverage, 1973-1996

	X-section		Differences-in-differences			
	(1)		(2)	(3)	(4)	
Teachers						
CB Permitted	.183 ***		.046	.065	.077 **	
	(.041)		(.031)	(.041)	(.038)	
Duty to Bargain	.389 ***		.129 ***	.164 ***	.169 ***	
	(.027)		(.030)	(.038)	(.034)	
p-value for linearity	.749		.327	.474	.805	
Fire Fighters						
CB Permitted	-.046		-.134 ***	-.023	-.034	
	(.096)		(.031)	(.066)	(.058)	
Duty to Bargain	.177 **		.056 *	.103 **	.114 **	
	(.080)		(.029)	(.047)	(.051)	
p-value for linearity	.062		.000	.120	.086	
Police						
CB Permitted	.093		-.071	.011	.042	
	(.096)		(.052)	(.040)	(.043)	
Duty to Bargain	.339 ***		.052	.024	.060	
	(.076)		(.038)	(.045)	(.040)	
p-value for linearity	.386		.021	.953	.688	
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 indicators. The reported p-value for linearity is from a t-test of the null hypothesis that the population coefficient on Duty to Bargain is twice the coefficient on CB Permitted (the restriction imposed by the CB index specification used in the main tables). All regressions control for year effects, sex, race, age, education, and marital status, in addition to the factors indicated in the bottom four rows. Data are from the following CPS files: 1973-1981 May Supplements, the 1983-1996 Merged Outgoing Rotation Group, with samples as described in the text.

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

Appendix Table 2. Effects of Collective Bargaining Laws on Pay and Hours, 1973-1996

Dependent variable	X-section		Differences-in-differences		
	(1)		(2)	(3)	(4)
A. Teachers					
ln(hourly wage)	.072 *** (.013)		-.016 * (.009)	.010 (.012)	.006 (.012)
ln(annual earnings)	.035 *** (.009)		-.002 (.017)	.006 (.019)	-.011 (.022)
ln(annual hours)	-.036 *** (.009)		.014 (.015)	-.004 (.017)	-.018 (.019)
ln(hours/week)	-.028 *** (.006)		.003 (.009)	-.002 (.011)	-.010 (.011)
ln(weeks/year)	-.009 ** (.004)		.010 (.007)	-.002 (.009)	-.008 (.010)
B. Fire fighters					
ln(hourly wage)	.092 ** (.036)		.046 ** (.023)	.026 (.053)	.044 (.053)
ln(annual earnings)	.055 ** (.025)		.016 (.020)	.034 (.048)	.069 (.053)
ln(annual hours)	-.037 ** (.016)		-.030 * (.015)	.008 (.019)	.025 (.030)
ln(hours/week)	-.037 ** (.015)		-.024 * (.014)	-.003 (.018)	.003 (.028)
ln(weeks/year)	.000 (.004)		-.006 (.004)	.012 *** (.004)	.023 ** (.011)

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C. Police

ln(hourly wage)	.057 ** (.026)	.003 (.012)		.062 *** (.015)	.029 (.021)
ln(annual earnings)	.048 * (.027)	-.019 (.017)		.030 (.029)	.000 (.034)
ln(annual hours)	-.008 * (.005)	-.022 * (.012)		-.032 * (.018)	-.029 (.023)
ln(hours/week)	-.007 ** (.003)	-.011 (.010)		-.031 *** (.010)	-.024 ** (.012)
ln(weeks/year)	-.001 (.004)	-.012 ** (.005)		-.001 (.009)	-.005 (.014)
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. All regressions control for year effects, sex, race, age, education, and marital status, population, and annual percentage population change, in addition to the factors indicated in the bottom four rows. Data are from the CPS from years 1973 through 1996, with samples as described in the text. *Statistically significant at the .10 level; **at the .05 level; ***at the .01 level.

Appendix Table 3a. Effects of Collective Bargaining Laws on Per-Pupil Education Expenditure, 1973-1996

Dependent variable	X-section		Differences-in-differences			
	(1)		(2)	(3)	(4)	
ln(per pupil salary)	.103	***	-.041	*	-.040	-.040
	(.027)		(.022)		(.038)	(.034)
ln(per pupil expenditure)	.131	***	-.032		-.030	-.025
	(.030)		(.024)		(.040)	(.036)
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. All regressions control for year effects, sex, race, age, education, and marital status, population, and annual percentage population change, in addition to the factors indicated in the bottom four rows. Data are from the Census Bureau's Historical Database on Individual Government Finances. *Statistically significant at the .10 level; **at the .05 level; ***at the .01 level.

Appendix Table 3b. Effects of Collective Bargaining Laws on Per-Pupil Education Expenditure, 1962-2010

Dependent variable	X-section		Differences-in-differences			
	(1)		(2)	(3)	(4)	
ln(per pupil salary)	.100 (.026)	***	-.032 (.017)	* (.017)	-.012 (.017)	-.013 (.017)
ln(per pupil expenditure)	.123 (.027)	***	-.023 (.016)		-.009 (.018)	-.010 (.019)
State effects?	N		Y		Y	Y
State trends?	N		N		Y	Y
Region x year effects?	N		N		N	Y

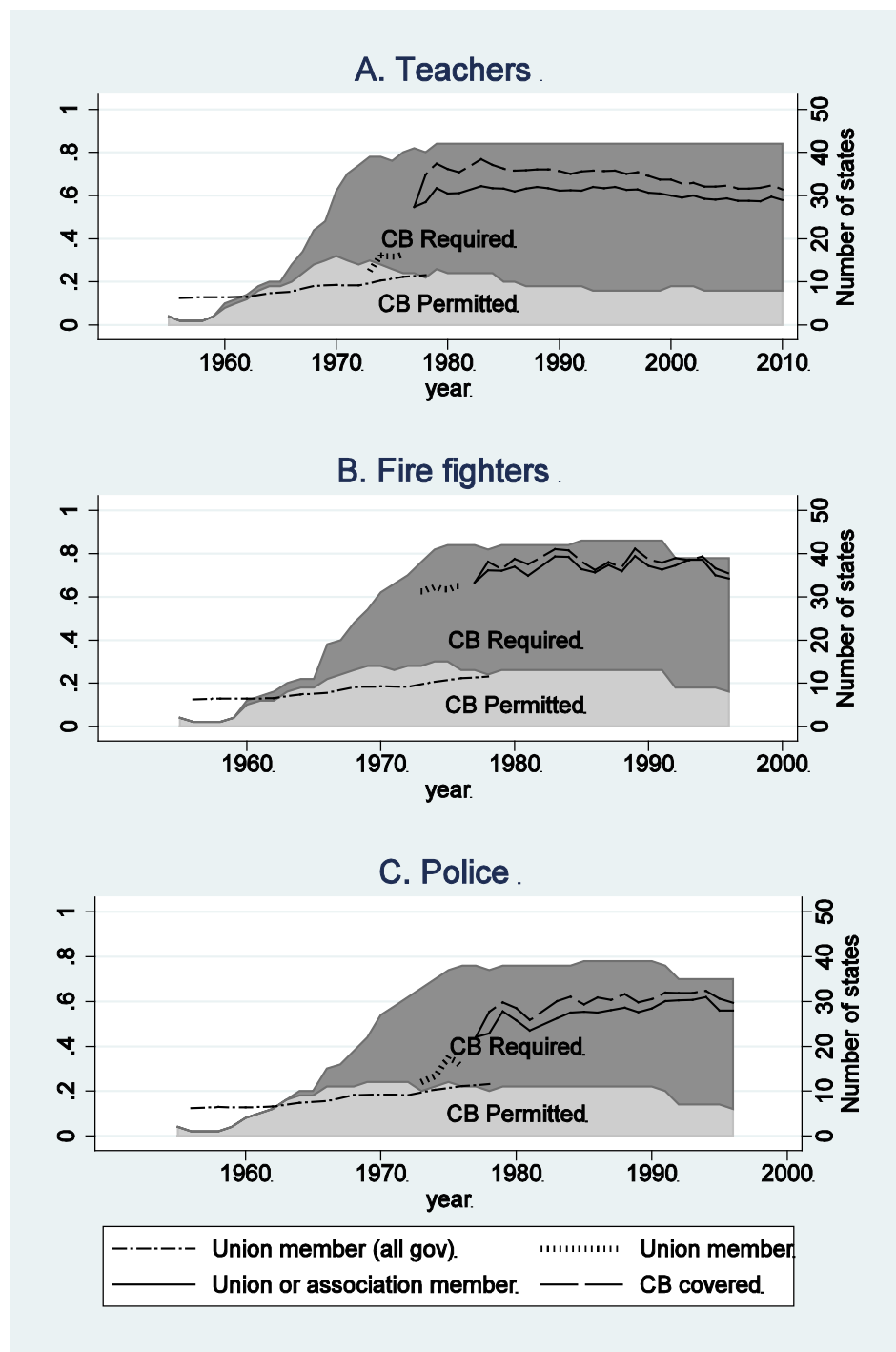
Notes: Regression coefficients and clustered standard errors (by state) on the collective bargaining (CB) law variable. All regressions control for year effects, sex, race, age, education, and marital status, population, and annual percentage population change, in addition to the factors indicated in the bottom four rows. Data are from the Census Bureau's Historical Database on Individual Government Finances. *Statistically significant at the .10 level; **at the .05 level; ***at the .01 level.

Appendix Table 4. Sample Restrictions in Education Expenditures Dataset

Restriction	Observations Remaining
Starting number of school district-year records	419,892
Enrollment positive	409,048
Elementary/secondary education expenditures strictly positive	397,342
Total expenditures equal to elementary/secondary education expenditures	395,758
Total salary positive	395,312
Elementary/secondary education direct expenditure positive	395,304

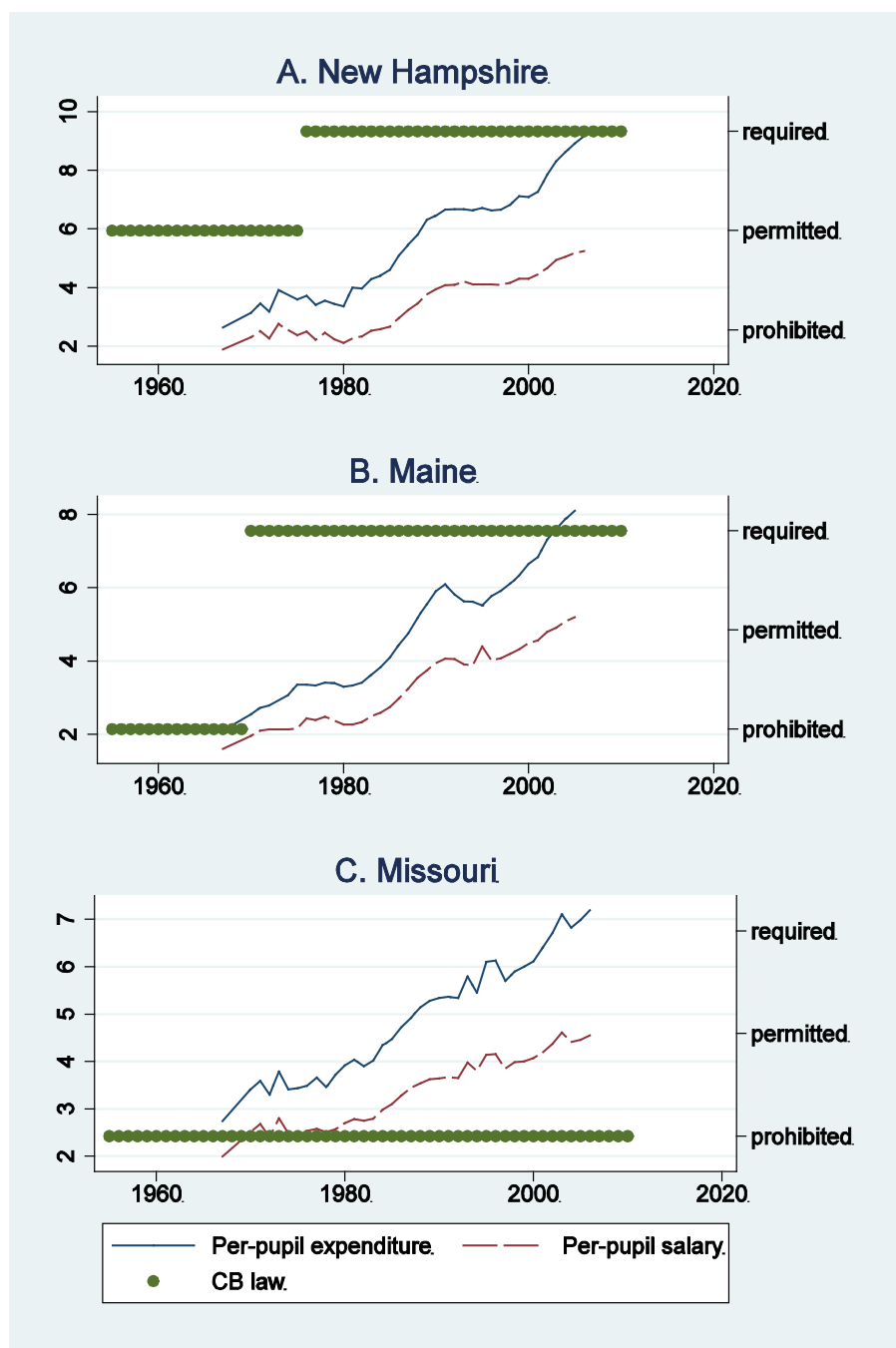
Notes: Sample restrictions and the number of remaining observations from the Census Bureau's Historical Database on Individual Government Finances. The starting number of records is the total number of school district observations (typecode = 5) pooled across the years 1967 and 1970-2006, the years for which spending data were available.

Figure 1. Public sector unionization rates and collective bargaining laws



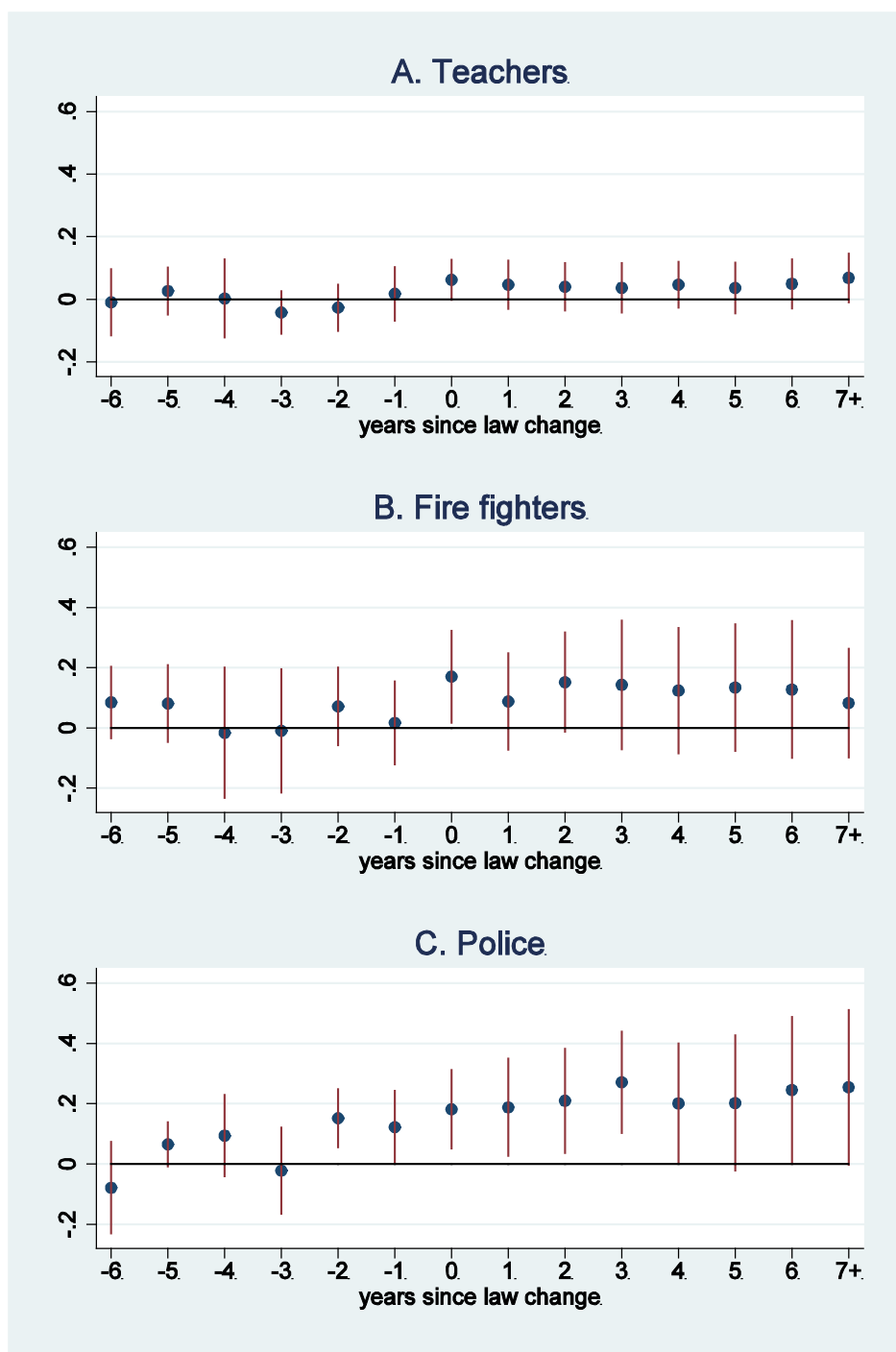
Notes: Unionization rate scale is on the left axis. Number of states with collective bargaining rights as indicated in the shaded regions given by the right-hand scale. Data are from Freeman (1986), Freeman and Valletta (1988), and the 1973-2010 Current Population Surveys.

Figure 2. Per-pupil salary, per-pupil education expenditure, and public school teacher collective bargaining rights over time for selected states



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

Figure 3. Coefficient estimates and 95-percent confidence intervals on the effects of collective bargaining law changes prior to (leads) and after (lags) the date of the actual law change



Notes: Coefficients are from regressions that also control for year effects, state effects, state trends, region-by-year effects, sex, marital status, race, education, and age. Data are from Freeman (1986), Freeman and Valletta (1988), and the 1973-1996 Current Population surveys.