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Public Sector Unions and the Costs of Government

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Abstract: Public sector unions are major interest groups in American politics, but they are rarely studied. New research would not only shed much-needed light on how these unions shape government and politics, but also broaden the way scholars think about interest groups generally: by highlighting interests that arise *inside* governments, drawing attention to long-ignored types of policies and decision arenas, and underlining the importance of groups in subnational politics. Here we explore the effects of public sector unions on the costs of government. We present two separate studies, using different datasets from different historical periods, and we examine several outcomes: salaries, health benefits, and employment. We find that unions and collective bargaining increase the costs of government and the effects are especially large for benefits. We view this analysis as an opening wedge that we hope will encourage a more extensive line of new research—and new thinking about American interest groups.

Keywords: union, interest group, costs, collective bargaining, government

The 2010 elections unleashed a perfect storm for America’s public sector unions. State and local governments were in financial crisis. Public pensions were dangerously underfunded. And Republicans, empowered by big electoral gains, sought unprecedented cutbacks to collective bargaining rights.¹

In 2011, Wisconsin became ground zero. Led by Governor Scott Walker, the state legislature weathered demonstrations by some 100,000 people to enact sweeping reforms that weakened public sector bargaining. Ohio Republicans followed suit (with a bill later overturned via a union-led ballot measure). New Jersey, under Republican Governor Chris Christie, prohibited public sector bargaining over health benefits. Michigan Republicans enacted a right-to-work law. Republican-controlled governments in Indiana, Idaho, Tennessee, and Michigan severely limited collective bargaining in public education (although the Idaho reforms were overturned by another union-led ballot initiative).

Throughout, Republicans argued that collective bargaining raises governmental costs, especially via outsized health and pension benefits, that restrictive work rules (such as seniority provisions) undermine performance, and thus that bargaining should be restricted. Democrats countered that collective bargaining is a fundamental right, that public workers are underpaid, that all workers should have decent pensions and health benefits, and thus that bargaining should be valued and protected.

Looming above these arguments was a political reality that heightened the stakes considerably. Public sector unions are a bulwark of the Democratic Party, and collective bargaining is the unions’ power base, providing members, money, and activists. When Republicans weaken collective bargaining, they are weakening the Democratic Party—and ultimately the values and policies the party stands for.

For political scientists, public sector unions raise issues of far-reaching importance. What are the effects of unions and collective bargaining on the costs of government? How do they affect government organization and public service provision? What are the connections between union power, the electoral and political strength of the Democratic Party, and the substance of American public policy?

These questions couldn’t be more basic, and public sector unions are clearly interest groups of the first magnitude in American politics—yet they are rarely studied (for exceptions, see Carpenter 2001; DiSalvo 2014; Moe 2011, 2006) In recent years, there have been calls for making interest groups more central to the field (Bawn et al. 2012; Hacker and Pierson 2010). But for this to happen, public sector unions need to be on the agenda. The interest group system and its effects on American government and politics cannot be understood without taking them seriously into account.

New research is needed along many fronts. In this article, we focus on one key issue: the impact of public sector unions on the costs of government. This is a reasonable opening wedge, as unions are clearly dedicated to increasing wages and benefits, and labor costs are fundamental to the operation of government. But it is also an opening wedge of salience to our times: for governments throughout the United States have entered an era of fiscal austerity in which cost issues are at the forefront of politics.

We present two separate studies based on different data sets from different historical periods. The first uses data from 1972 through 1987—when many cities were getting collective bargaining for the first time—to explore the impact of unionization on city payrolls for police and fire departments. The second uses more refined data from 1992 through 2010 to explore whether cities with collective bargaining and politically active unions had higher wages, health benefits, and employment levels for police officers and firefighters than cities without collective bargaining and active unions.

Our findings show that strong unions do tend to increase the costs of government. As we discuss, this is what should be expected on theoretical grounds. But more work needs to be carried out if the

¹ An online appendix containing data description and supplemental empirical analysis is available at XX. Data and supporting materials for reproducing the results in the paper will be made available upon publication at <http://gspp.berkeley.edu/directories/faculty/sarah-anzia>.

broader effects of public sector unions on American government and politics are to be understood—and if scholars are to construct a more fully developed perspective on the American interest group system.

Public Sector Unions as Interest Groups

For many decades, going back to Schattschneider's (1935) study of the Smoot-Hawley tariff bill, scholars have generated a voluminous body of work on American interest groups. This work has four distinguishing characteristics. One, it sees groups as arising from civil society, *outside* of government. Two, it focuses on group efforts to influence important types of government *policies*. Three, it sees *lobbying* as the prime avenue of policy influence. And four, its main concern is with *national* politics.²

Any close observer of American politics would agree that public sector unions are interest groups of major importance. But they have yet to be studied as such. If they were, the effect would be to broaden every one of these distinguishing features of the way scholars think about interest groups.

Public sector unions represent interests that arise not from civil society, but from *inside* the government—to influence government itself. Their core interests are the job interests of public employees, which arise inevitably wherever government exists. How successfully and powerfully these job interests get organized can vary considerably across contexts, of course. Union density is much higher in local and state governments than in the federal government and much higher for some public occupations—teachers, firefighters, police officers—than others. Also, public sector unions are weaker in Southern and border states than elsewhere (DiSalvo 2014). Still, the job interests of public workers are ubiquitous, and so are public sector unions. The result is a sphere of intense interest group activity that regularly occurs in all 50 states and thousands of local governments across the United States. Seeing it as such opens up a vast realm of interest group politics that political scientists have yet to explore.³

It also encourages a new perspective on interest groups and public policy. Students of American politics have long paid attention to a broad range of policies—on the environment, defense, agriculture, gun control, abortion, and more—in assessing group influence. The policies of greatest salience to public sector unions, however, are *personnel* policies on wages, health benefits, pensions, employment, and work rules. These policies fundamentally shape the costs and organization of government and stand to have major consequences for society. Yet they have never been part of how “policy” is thought about in American politics or how group influences on “policy” are studied. They need to be.

Another new opening has to do with the mechanisms of interest group influence. Scholars have long focused on group lobbying, backed by campaign contributions, grassroots activism, and other means of making lobbying more effective. Public sector unions seek to influence policy in the same ways. But they also have another avenue of influence: *collective bargaining*—a distinctive arena of government, with its own rules and participants, where policies of great importance are made. This is a key realm in which interest groups attempt to shape public policy to their advantage—but it has gone almost entirely unstudied. Attention to public sector unions would change that.

Finally, interest group research has long focused on national politics: on lobbying in Congress, national policy issues, and federal elections.⁴ But much of American government and politics occurs at the state and local levels, where most public money is spent, most public employees work, and policy

² For excellent overviews of the literature, see Maisel and Berry (2012) and Cigler and Loomis (2011).

³ “Inside” interests would also include, for example, associations of school administrators and school boards, the National Governors Association, the National Conference of State Legislatures, and the National League of Cities. All are currently off the scholarly radar screen. See Cigler (2011).

⁴ The major exception is the work of Gray and Lowery (see, for example, Lowery and Gray 2010).

decisions are regularly made on matters ranging from education to public safety to social welfare. The study of public sector unions would encourage group research at these lower levels of government and encourage a new, more bottom-heavy perspective on the American interest group system.

Expectations

In this article, we examine the impact of public sector unions on the costs of government. Our expectations are straightforward. Public sector unions seek higher wages, better benefits, and job protections for their members, which are costly for governments to provide. And because unions can mobilize money and manpower in ways the unorganized cannot, there is good reason to believe that government workers will exercise greater power when they are unionized, resulting in higher costs.⁵

They can exercise power in two basic ways. One is through collective bargaining: formal negotiations with management, backed by implicit threats of strikes, slowdowns, sickouts, and other coordinated work actions. The other is through politics, including elections, where the unions can make endorsements, provide campaign contributions, deploy activists, generate publicity, engage in lobbying activities, and in other ways seek to influence official decisions on matters related to jobs.

In our own project, it has been quite a challenge to get good data on union organization and collective bargaining across cities and time, and as a practical matter, it has simply not been possible to delve into the distinctive political processes and activities at work in the hundreds of cities we examine. More detailed research along these lines remains for the future. But even though our tests—with one exception—cannot directly explore the political mechanisms that public sector unions employ, our expectations about union influence on wages, benefits, and employment are conditioned by the political context. Two expectations are most relevant to the analysis we carry out here.⁶

First, there is a political asymmetry between wages and benefits. Wages come out of current operating budgets that are often highly constrained at the state and local levels, and citizens are averse to raising taxes—making it difficult for unions to win large wage increases. Also, wage settlements are very visible, easily understood by the public, attract media scrutiny—and threaten to be intensely controversial, setting off political shock waves that even friendly politicians may be eager to avoid.

Fringe benefits are much more attractive politically. In past decades, they were relatively inexpensive and thus easier for politicians to afford and unions to win. This advantage has faded as health insurance costs have soared, but another advantage remains. Health and pension benefits are extremely complicated, difficult for the public to understand, difficult for the media to convey—and thus nearly invisible politically. Politicians can boost health and pension benefits without citizens or journalists having any sense of the true costs—all the more so because many of these costs involve legally binding promises (to retirees) that will be paid *in the future* by *other* politicians and taxpayers, with little impact on current budgets.⁷ This is precisely the kind of policy, then, that falls into what Bawn et al. (2012) refer to as the “electoral blind spot” of ordinary voters—allowing politicians and parties to respond to interest groups without voters understanding what is going on. Voters understand wages, they have trouble understanding benefits, and this stacks the political deck in favor of benefits.

⁵ It is possible that unions also increase productivity, but the literature on union productivity effects in the public sector is sparse and inconclusive (see Hirsch 2004).

⁶ Early work on public sector unions by economists was sensitive to these and other aspects of the political context (see, for example, Wellington and Winter 1971).

⁷ On the political attractiveness of promising future benefits, see Kiewiet (2010) and Ichniowski (1980).

A second expectation has to do with employment. Economic theory points to a trade-off between compensation and employment: as wages and benefits go up, less labor will be purchased and employment will decline. Governments, however, are not markets. The unions can attempt to use their power with politicians to increase compensation *and* employment. The question is: would the unions want to do that? The answer might seem obvious, but it actually isn't.

For any given budget, existing union members can be better paid if there are fewer workers to be compensated. That being so, the unions have incentives to keep their numbers down. On the other hand, higher employment means more members and money for the unions—and more political power—which they can use later on to push for higher compensation (and other goals). So in this respect, they have incentives to try to increase their numbers. How unions should balance these conflicting incentives is a matter of strategy and internal (member) pressures, and different approaches may be rational.

In the empirical analysis to follow, we expect to find that when public sector workers get organized into unions, they can use their influence—through collective bargaining, through politics—to increase both wages and benefits, but they are likely to have much greater impact on benefits. As for their impact on employment: the logic cuts both ways, and we will simply see what the data reveal.

Research

Research on public sector unions was in vogue during the 1970s and 1980s but then tailed off during the 1990s. Almost all of this early work sought to explore union impacts on governments' wage and salary expenditures, budgets, and employment (see DiSalvo 2014).⁸ This literature shows that government expenditures on wages and salaries do tend to be higher due to unions (e.g., Ashenfelter 1971; Ichniowski 1980; Zax and Ichniowski 1988). Studies of compensation, however, are almost always limited to employee pay. Few take benefits into account, because they are far more difficult to measure and good data are elusive; these few efforts suggest, though, that public unions have much bigger impacts on benefits than wages (Hunter and Rankin 1988; Zax 1988). Thus, it appears that by focusing on earnings alone, the literature underestimates union impacts on total compensation. On other basic counts, the literature also fails to arrive at clear conclusions. On employment, in particular, the results are ambiguous. Some studies show that unions bring about higher levels of government employment, while others show that they have no impact or even a negative impact (e.g., Trejo 1991).

These were pioneering studies, and they were on the right track in exploring how unions affect government. But the literature lost its momentum and largely petered out before even the most basic questions were answered with confidence. It is up to today's scholars to revisit these issues, build on what the early studies were able to achieve, and breathe new life into a moribund research enterprise.

Our purpose here is to contribute toward that effort by presenting two studies of union effects on the cost of government. These studies go beyond the existing literature in important respects: they are based on better measures of key variables, they handle endogeneity issues differently, and they introduce new and more modern data that help to bring the literature up to date.

⁸ A more recent literature on employee earnings uses national surveys of individuals to estimate the wage premiums associated with union membership in both the public and private sectors. This work does not tie individuals or unions to specific units of government and so does not explore the governmental issues we address here. That said, it does show that comparable public employees have higher wages when they are members of unions (see, for example, Bahrami, Bitzan, and Leitch 2009).

Unionization and City Wages, Employment, and Payroll in the 1970s and 1980s

We begin by focusing on the 1970s and early 1980s, when public workers were first securing collective bargaining rights.⁹ This is arguably the best context for estimating causal effects, since we can examine the same governments *before* and *after* their employees unionized. Thus, unlike a study using more current data, which would rely on cross-sectional variation to estimate union effects, our analysis lets us leverage within-government variation in the union status of employees over time.

In turning to data from the 1970s and 1980s, we are revisiting the measurement and endogeneity challenges of the early research. The measurement challenge is simply that data on public sector unions are scarce, and the data that do exist have various problems (see Freeman, Ichniowski, and Zax 1988). As we describe below, we largely adopt the early literature's conventions for dealing with these problems. However, our handling of endogeneity improves upon existing work, producing better estimates of public sector unionization's effects on cities' wages, employment, and payroll expenditures.

The main endogeneity concern is that cities whose employees form unions and secure collective bargaining are different from cities whose employees remain unorganized, and those differences could be correlated with compensation and staffing levels. For example, large cities are more likely to have unionized employees than small cities, and they also tend to pay higher wages (e.g., Trejo 1991). If we were to ignore the importance of city size, our estimates of the effect of unionization on wages would be biased upward. Thus, we need to partial out the effects of any city characteristics that influence both its employees' proclivity to organize and its compensation and employment practices.

The advantage of studying unionization during the 1970s and 1980s is that we can design an analysis that substantially reduces the potential for omitted variable bias by isolating governments' conditions before and after their workers formed unions. Specifically, in 1972, 1977, 1982, and 1987, the Census of Governments conducted a special Labor-Management Relations Survey, which included items about whether cities had collective bargaining and unions. By assembling these data into a panel, we can estimate union effects by leveraging within-government variation, partialing out the effects of any time-constant city characteristics that could be sources of bias. This is a tremendous opportunity to conduct clear causal inference—one that cannot be replicated using data from later periods.¹⁰

Thus, our goal in this initial empirical study is to use variation in unionization within cities over time to estimate the impact of unions on cities' average wages, staffing levels, and payroll expenditures. We focus on two groups of employees that make up a large percentage of overall city employment: firefighters and police officers. Because our data allow us to include city-level fixed effects, we eliminate any sources of omitted variable bias that are constant within cities over time.

There are other potential sources of endogeneity that must also be acknowledged and addressed. For example, wage levels might influence whether a city's employees get organized. The direction of the bias in that case would likely be negative, however: public workers are probably more motivated to form unions when wages are low. Nonetheless, the more general concern is that there may be city

⁹ Before 1960, few public employees were unionized or had collective bargaining. Over the next two decades, most states enacted public sector labor laws, leading to an explosion of unionization and collective bargaining in state and local governments (outside the South). By the early 1980s, union membership had soared to 37% of the public workforce. See Freeman (1986) and Moe (2011).

¹⁰ Most of the early economic studies relied on cross-sectional data to estimate union effects (e.g., Brown and Medoff 1988). The only study using the full Census of Governments panel to conduct a within-unit analysis of union effects is Hoxby (1996), whose focus is solely on teachers unions.

characteristics that vary over time that influence both its employees' propensity to unionize and its wages (and employment)—and if such factors exist, we must incorporate them into our models.

Data and Empirical Strategy

Our data come from the U.S. Census of Governments Public Employment Files from 1972, 1977, 1982, and 1987, which contain data from the Labor-Management Relations Surveys as well as employment and payroll information collected during each quinquennial census. In each of the years, governments reported how many of their police and fire employees were members of employee organizations. Because there is measurement error in these figures, we follow Freeman, Ichniowski, and Zax (1988) in creating dichotomous measures of unionization. We code a city as having an organized fire (police) department if at least some of its fire (police) protection employees are in unions.¹¹

Dichotomizing the variable does not fully address the measurement problem, however. The data show that a number of cities reported their fire or police employees were unionized in one year but not in subsequent years. Fortunately, this too is a pattern Freeman, Ichniowski, and Zax (1988) recognized and addressed. And when they conducted phone interviews with 258 governments that had supposedly lost collective organization, they found that *not a single one* had actually lost it. In every case, city employees had either organized and stayed organized or they had never organized at all.

To minimize the effects of these reporting errors, we made minor adjustments to our coding of the fire and police organization variables by examining within-city patterns over time. Cases that were sufficiently ambiguous were dropped. A full description of our coding decisions is in the online appendix, along with a sensitivity analysis.

Because we want to focus on cities large enough to house their own police and fire departments, and to make this analysis consistent with the study we present in the next section, we focus on cities that had at least 10,000 residents as of 1972. This gives us a dataset of 1,689 city police departments and 1,400 city fire departments tracked from 1972 to 1987 at five-year intervals. In total, 368 of the police departments and 241 of the fire departments first became unionized over the course of this time period.

For both fire and police employees, we analyze three outcomes: log average wage, log employment per capita, and log payroll expenditures per capita.¹² Our model of each is as follows:

$$\ln(\text{payroll}_{it}) = \alpha_i + \beta(\text{union}_{it}) + X_{it}\varphi + \delta_t + \varepsilon_{it}$$

Subscript i denotes the city, and t denotes the year. The α_i are city fixed effects, the δ_t are year fixed effects, β and φ are regression coefficients, and ε_{it} is an error term. The variable union_{it} is a binary indicator variable equal to one if the employee group is organized; the regression coefficient β is the average effect of the treatment (unionization) on the treated. We use ordinary least squares to estimate the models, and we cluster the standard errors by city to correct for autocorrelation within cities over time.

X_{it} is a matrix of time-variant control variables constructed using data from the 1970, 1980, and 1990 U.S. Censuses of Population. As we show in the online appendix, the cities where police officers and firefighters formed unions were larger in population, higher in socioeconomic status, and had lower percentages of African Americans and Hispanics than cities that never unionized. They also had higher manufacturing employment, lower poverty rates, lower school enrollment rates, and lower population growth. Because these correlates of unionization might also be associated with wages, payroll, and

¹¹ When we use the percentages of police and fire employees in unions, our results are very similar. See the online appendix.

¹² Average wage and payroll figures are adjusted to 1987 dollars.

employment, our models control for the following: the natural log of city population, population growth, socioeconomic status,¹³ percent African American, percent Hispanic, percent living in poverty, percent enrolled in elementary or high school, and percent employed in manufacturing.

It is also possible that city officials consider the pay rates of surrounding cities in deciding how much to pay their workers. If, for example, a city were to discover that its wages were lower than the wages of similar cities in the area, perhaps it would increase its wages in the next year to avoid losing its employees to nearby cities. Even if there is no such “competition effect,” there may have been a “threat effect” in the 1970s and 1980s. Specifically, if officials in nonunionized cities with unionized neighbors increased their wages to avoid the dissatisfaction—and potential unionization—of their employees, the result would be a tendency toward the equalization of wages across union and nonunion cities.

In our models of average wages, we adopt the following strategy to allow for these effects: For each state and each year of 1967, 1972, 1977, and 1982, we regress average wage for all city employees on logged city population and logged city per capita income.¹⁴ The residuals become our measure of the extent to which a city’s wages five years prior deviated from the wages of cities similar in size and cost of living within the same state. We include this variable to test whether city officials compensate for having below-market wages in the previous period by increasing wages. If its coefficient is negative, that would be evidence of a competition effect. In a second specification, we also interact this variable with unionization. If we find that it is predominantly the unorganized cities that increase their wages in response to having low wages in the previous period, that would be evidence of a threat effect.¹⁵

Empirical Results

The results from our analysis of the Census of Governments data are set out in Table 1.¹⁶ First, in columns 1 and 2, we ask whether the average wage of municipal fire-protection employees increased after they formed unions. Looking at the coefficients on the *Union* indicator, it is clear that the answer is yes. On average, the effect of unionization was a statistically significant 3.9% increase in the average wage of fire-protection employees. Given that one of the main reasons unions form is to pressure for higher wages, this effect is precisely what we should expect: it indicates that firefighter unions were successful in increasing their members’ pay in the years shortly following their organization.

<<Table 1 about here>>

The results in column 3 demonstrate that the wage premium that accrued to unionized fire-protection employees did not come at the expense of fire-department staffing levels, at least in the short run. To the contrary, we find that per capita fire-protection employment *increased* by 7.6% in cities where firefighters formed unions, an effect that is statistically significant at the 1% level. And unsurprisingly, since unionization led to both increased wages and increased per capita employment in fire departments, total per capita fire-protection payroll expenditures increased when firefighters formed unions. As we show in column 4, a city whose fire-protection employees organized for the first time could expect to spend nearly 11% more on fire-protection salaries and wages as a result. This effect

¹³ This is an average of logged per capita income and percent of city residents with a college degree.

¹⁴ Average wage for police and fire-protection employees was not available for 1967, but in the years 1972–87, 40% of the typical city’s full-time payroll expenditures went to police and fire functions. The average wage for all city employees is thus a reasonable proxy for police and fire average wages.

¹⁵ In this article, we are only interested in these variables as controls, in order to reduce any bias in our estimates of union effect. However, there is substantial debate in the economics literature over whether union threat effects exist (e.g., Lovenheim 2009; Ichniowski, Freeman, and Lauer 1989).

¹⁶ All of our discussion of statistical significance reflects the results of one-tailed tests on the directional hypotheses and two-tailed tests on the remaining hypotheses.

holds above and beyond the effects of national trends in firefighter wages, time-constant city characteristics, and time-varying city characteristics like city size and socioeconomic status.

For police officers, our expectations are the same as for firefighters—they should have more influence when unionized—but there is also reason to believe that police unions may have a smaller impact. First, firefighters have long had stronger organizations than police officers: nearly all local firefighter unions are affiliated with the International Association of Fire Fighters, whereas local police unions are variously affiliated with AFSCME, SEIU, and police-specific organizations like the Fraternal Order of Police and the International Union of Police Associations (see Kearney 2009). Firefighters also work long shifts together and have more downtime between calls: a working environment conducive to political organizing. Moreover, firefighters are more popular than police officers,¹⁷ and their popularity may translate into greater political clout.

Columns 5 to 8 of Table 1 present our results for police-officer personnel policies. As expected, the effects of unionization on police departments' wages, employment levels, and payroll expenditures were strong, positive, and statistically significant. They are also smaller in magnitude than those for fire departments. Columns 5 and 6 show that police officers who formed unions saw their wages increase by about 2.3%, an effect significant at the 1% level. Police staffing levels also increased, as shown in column 7: relative to national trends in police-department size, municipal police departments employed 2.3% more employees per capita after those employees organized. Together, these increases in wages and employment resulted in an average 3.7% increase in per capita payroll expenditures for police. Thus, in the years immediately following unionization, police unions were successful in pressuring their governments for better wages, higher employment, and overall increases in spending on compensation—even if they were somewhat less successful than firefighter unions.

Most control variables behave as expected, so to save space we do not discuss their effects here. Worth noting is the evidence that cities did react to whether their wages were low or high relative to others. Columns 1 and 5 show that when a city's average wage in the prior period was relatively low, cities responded by increasing police- and fire-protection wages. However, it is not clear from these results whether all cities adjusted in this way—a general competition effect—or nonunion cities were adjusting to the threat of unionization. In columns 2 and 6, we interact the lagged deviation variable with the indicators for union status. Column 2 shows that there was no significant difference between the two types of cities' adjustments to fire-protection wages, and column 6 shows that if anything it was primarily the unionized cities that adjusted police wages upward in response to being lower than average five years prior. These results suggest that it was a general competition effect at work, not a threat effect.

Most important, though, on the question of how unionization affected wages, employment, and payroll expenditures, our results are clear. We find that police and fire unions were successful at increasing wages, staffing levels, and expenditures on employee compensation in the 1970s and 1980s. These results are robust to a variety of alterations in the city sample and model specification,¹⁸ and they are precisely as we should expect. And by using within-city variation in unionization over time, we rule

¹⁷ For example, in a 2009 Harris Poll, 62% of respondents said that firefighters have “very great prestige,” while only 44% said the same of police officers. See Corso (2009).

¹⁸ The results do not change substantively when we include state-year fixed effects, which can account for the independent effect of state-year-specific shocks, such as the passage of a state collective bargaining law. In addition, when we estimate these models using a smaller subset of cities for which we did not make corrections to the unionization variable, our results are the same. When we eliminate the city fixed effects and control only for Census region, our estimates generally increase in magnitude and remain statistically significant. See the online appendix for details.

out the possibility that time-constant characteristics of cities are driving the effects, which is a significant improvement over the existing studies that generate estimates from cross-sectional data.

Yet studying this time period has some disadvantages. Most important, we do not know how much cities spent on nonsalary compensation. We would expect union effects to be even more pronounced for benefits like health insurance and pensions, which means that Table 1's estimated effects are probably lower bounds. Moreover, our results give union effects as of 25 to 40 years ago. Clearly, we also want to know how unions influence the cost of government today. In the next section, therefore, we carry out a study using more current data that include information on employee benefits.

Collective Bargaining and Cities' Expenditures on Salaries and Benefits, 1992-2010

By the mid-1980s, the wave of public sector unionization had subsided, and a new equilibrium had set in. Cities that were going to unionize had done so, and cities without unions were to remain without them. In this section, we examine the contours of this new equilibrium, investigating the consequences of collective bargaining for government in the 1990s and 2000s. To do this, we have assembled a new dataset on the bargaining status, employment levels, and compensation practices of city police and fire departments. Not only is this dataset more current, but it also contains data on city expenditures on employees' health, hospital, disability, and life insurance benefits. In addition, it contains basic information on the political activities of public sector unions, which we can use to conduct a preliminary test of how those political activities shape the costs of government.

Any study relying on recent data to examine the impact of public sector unions must deal with a key challenge: very few governments adopted collective bargaining for the first time after the 1980s. In our current dataset, as a result, the independent variable of interest does not change within cities over time. This makes it all the more important that we (and anyone studying public sector unions) find ways to make appropriate comparisons between cities with and without collective bargaining.

Data and Empirical Strategy

Our data on city staffing and compensation come from the annual Police and Fire Personnel, Salaries, and Expenditures surveys conducted by the International City/County Management Association (ICMA). Since 1992, ICMA has sent questionnaires annually to all municipal governments with more than 10,000 in population to ask about their police and fire departments, their personnel policies, and their spending on various budget items. We have assembled all available years of data into a panel. Because a different set of municipalities responds to the survey each year, the panel has significant gaps, with most municipalities appearing in the dataset in some years but not others.

For both police and fire employees, we focus our analysis on five dependent variables. They are: department spending per employee on salaries and wages (including supplemental forms of pay like longevity pay, hazard pay, holiday pay, and overtime pay); department spending per employee on health, hospital, disability, and life insurance benefits; department employment per city resident; department spending on salaries per capita; and department spending on health benefits per capita. We take the natural log of all variables and adjust the dollar values for inflation. A complete description of how we assembled and cleaned these variables is available in the online appendix. We have different numbers of observations for each dependent variable, but the maximum is 16,809 for police departments (in 2,243 unique municipalities) and 8,809 for fire departments (in 1,177 unique municipalities).

Our key independent variables are binary indicators of whether municipal police- and fire-protection employees have collective bargaining,¹⁹ which we construct using three groups of data. The first is the Law Enforcement Management and Administrative Statistics (LEMAS) surveys conducted by the Bureau of Justice Statistics in 1987, 1990, 1993, 1997, 2000, and 2003, which asked U.S. law enforcement agencies whether their sworn police officers have collective bargaining. Second, we use the ICMA's Labor Management Relations surveys from 1988 and 1999, which asked about the bargaining status of various municipal employees. Lastly, we use a 1977 Census of Governments survey that documented whether certain groups of city employees were part of a bargaining unit. We describe how we combined these datasets and coded the collective bargaining indicators in the online appendix.

Our estimation strategy is similar to that of the previous section. The unit of analysis is again the municipality-year, and we estimate the impact of collective bargaining using OLS with standard errors clustered by municipality.²⁰ We include all control variables from Table 1 as well as logged population density (since denser cities are more likely to have organized employees and higher demand for public safety services) and logged median rent (to account for cost of living differences). All demographic data come from the 1990, 2000, and 2010 Censuses and the 2005–2009 American Community Survey. In our models of per-employee salary and health benefits expenditures, we also include a control for the competition effect we observed in the 1970s and 1980s, using a similar strategy.

The main difference in empirical strategy compared to the previous section is that, because there are so few cities where police or firefighters got collective bargaining for the first time after 1992, we do not include city fixed effects. We therefore include three additional sets of controls in all of our models that we consider to be important for explaining between-city differences in collective bargaining norms and employment and compensation policies. Because more liberal, Democratic cities are more likely to have unionized public sector workers and more generous compensation, we control for the percentage of the two-party vote that went to Al Gore in the 2000 presidential election in the city's parent county. And because some states and regions have more worker-friendly cultures than others, we control for the rate of private sector union membership in each state and year using data compiled by Hirsch and MacPherson (2011), and we also include dummy variables for three of the four geographic regions in the United States. As in our earlier models, we include year fixed effects.

Empirical Analysis

We start with an analysis of city fire departments, with results set out in Table 2. In column 1, we estimate the effect of collective bargaining on salary spending per employee, and the effect proves to be larger here than the union effect (due to the simple presence of a union) we estimated for the 1970s and 1980s: on average, fire departments with collective bargaining spend about 9% more per employee on salaries and wages. Notably, however, the impact is far greater on per-employee *benefit* spending on health, dental, disability, and life insurance. As we show in column 2, fire departments with collective bargaining spend an average of 25% more on those benefits for the typical employee.

<<Table 2 about here>>

In column 3, we test whether collective bargaining also has an effect on employment levels. We find that it does not: there is no significant difference between per capita fire-protection employment in

¹⁹ Note that the key independent variables here are different from the *Union* indicators in the first study. Here we estimate the effect of collective bargaining, whereas for the earlier period (where we did not have data on collective bargaining), we estimated the effect of the presence of unions in the department.

²⁰ We also run each of our models using robust estimation to ensure that our estimates are not sensitive to potential outliers and leverage points. See the online appendix.

cities with and without collective bargaining.²¹ As we know, then, that collective bargaining has no effect on employment but does increase wages and benefits, it follows that it also leads to an increase in overall spending on compensation: cities with collective bargaining spend 9% more per city resident on fire-protection salaries (column 4) and 25% more on benefits (column 5).

These results clearly indicate that collective bargaining has a sizeable effect on the amount municipal fire departments spend on employee compensation. Moreover, they show that it is imperative to take fringe benefits into account when estimating the effect of collective bargaining on public employee compensation, for this is where unions have secured the greatest gains.²²

In Table 3, we find that the effects of collective bargaining in city police departments are similar to those of fire departments. On average, cities where police have collective bargaining spend over 10% more on salaries per police employee than cities where police lack bargaining rights. As with firefighters, the spending gap is much wider for health benefits. The typical city police department with collective bargaining spends about 21% more on health benefits per employee than the typical city without collective bargaining.

<<Table 3 about here>>

In contrast to our findings for fire departments, however, we find that collective bargaining does have an impact on police employment levels: on average, per capita police employment is 5.8% lower in cities where police have bargaining rights. While this difference could be due to different strategies across the two sectors, it dovetails with our earlier findings that police unions tend not to be as influential as firefighter unions. Even with the negative impact on police employment, though, we still find that the consequences of collective bargaining for cities' *overall* per capita expenditures on police compensation are positive: cities where police have collective bargaining spend 4.3% more on salaries per capita (column 4) and about 16.5% more on health benefits per capita (column 5).

In sum, the cities where public sector employees secured collective bargaining have progressed along a markedly different path than the cities whose employees never pursued or won bargaining rights. Municipal police and fire departments with collective bargaining spend significantly more on their employees' salaries than similar departments without collective bargaining. In police departments, that salary premium has come with slightly lower per capita employment levels. But most important, we find that the biggest gap between bargaining and nonbargaining cities is in the area of health benefits expenditures. When it comes to health benefits for police- and fire-protection employees, cities with collective bargaining are spending 15 to 25% more than cities without collective bargaining.

The Effect of Public Sector Unions' Political Activities

²¹ Our study of the earlier, formative period showed that the emergence of unions tended to increase firefighter employment. The results here suggest either that the employment premium for unionized fire departments no longer exists or that any such premium is not a direct effect of collective bargaining.

²² As for the control variables: we should note that, unlike in our analysis of the 1970s and 1980s, we find no evidence of a competition effect in the modern era. (The estimated coefficient indicates that cities paying lower than average wages in the past paid lower—not higher—wages in the current period.) We also find (see the online appendix) no evidence of a threat effect. These findings might be explained in various ways—for example, that the early period was one of disruption and jockeying for position among cities, while the latter period was one of equilibrium. But we won't pursue these possibilities here because they are not central to the analysis. We include competition and threat effects in our models simply as controls, in order to limit the possibilities of bias in our estimates of union effects.

Until now, we have relied on indicators of unionization and collective bargaining to estimate the effect of public sector unions on the costs of government. As we argued earlier, however, public-sector unions have two main routes of influence: collective bargaining and politics. Because there is good reason to believe that the most politically active government employees are those who are in unions and have collective bargaining rights, the effects we have estimated so far likely capture some of the impact of unions' political activity. Even so, we would like to test for an effect of politics directly. To do this, we need to add a measure of unions' political activity to the models we presented in Tables 2 and 3.

Unfortunately, we know of no good measures of unions' political activities in city politics. The only measures that do exist are those collected by the ICMA as part of its Labor Management Relations surveys—and there are several problems with them. First, they only exist for 1988 and 1999, and only for cities that responded to those surveys. Second, the surveys asked about police and fire unions' involvement in only a few types of political activities, neglecting certain areas of activity that could be especially important to union influence. Third, the ICMA datasets do not distinguish between cities where the unions are politically *inactive* and cities where the respondents did not answer the political activities questions. Even so, these data are currently the best available. Thus, in spite of the low quality of the measures, we use them to conduct a preliminary test of the political activity part of our argument.

To keep the analysis simple, we focus on one form of activity common to local politics: whether unions endorse candidates in elections. We create a variable equal to one for all years if a city reported in either 1988 or 1999 that its firefighter union endorses candidates in elections, and the same for police. By adding these variables to the models from Tables 2 and 3, we test whether union political activity has an impact on compensation and employment above and beyond the effect of collective bargaining.

The results are summarized in Table 4. (To conserve space, we do not present the full set of estimates.) For fire employees, we find that political activity has a strong, positive, and statistically significant effect on all our dependent variables, even as collective bargaining continues to have effects similar to those estimated in Table 2. In columns 1 and 2, we find that both collective bargaining and political activity have positive, statistically significant effects on per employee salary and health benefits expenditures. For collective bargaining, the effects are 8% and 24%, respectively, and for political activity, they are 2.3% and 3.1%. Moreover, while collective bargaining has no discernible impact on per capita fire-protection employment—just as we found in Table 2—column 3 shows that political activity is associated with an employment *increase* of 6.4%, significant at the 1% level. In columns 4 and 5, we find that collective bargaining and political activity both increase the amount spent on salaries and health benefits per capita: collective bargaining increases per capita salary expenditures by 4.4% and per capita health expenditures by 19%, and political activity increases salary expenditures by 10% and health benefits expenditures by 12%. All these effects are statistically significant, and they support the argument that public sector unions have influence both through collective bargaining and politics. <<Table 4 about here>>

For police protection employees, the results are more nuanced but still generally support our argument. In columns 1 and 2, we find that collective bargaining continues to have a large, positive, and significant effect on per employee salaries and health benefits, but the estimated effect of police endorsement activity is statistically insignificant. However, when we turn to police employment levels in column 3, we estimate a significant positive coefficient for police political activity. This suggests that perhaps some of the negative effect of collective bargaining on police employment (also found in Table 3) can be offset by the engagement of police unions in politics. In column 4, because of the large negative effect of collective bargaining on police employment, we estimate an insignificant effect of collective bargaining on cities' per capita salary expenditures, but the effect of endorsements is 6.3% and significant at the 1% level. Finally, in column 5, we find that both bargaining and political activity

have positive and significant effects on cities' per capita health expenditures: the effect is 13.5% for collective bargaining and 4.6% for police endorsement activity.²³

The results of Table 4 are preliminary, but the general direction of the estimates is clear: both collective bargaining and unions' political activity increase the amount that police and fire departments spend on employee compensation. This is what we should expect: public sector unions have two potential routes of influence, and both of them should contribute to their success.

Conclusion

In this article, we carry out two studies of the impact of American public sector unions on the costs of government. In the first, we leverage within-city variation in public sector unionism from 1972 to 1987 and find that the unionization of police and fire employees increased average wages, employment, and total payroll expenditures in municipal police and fire departments across the country. In the second, using more current data from 1992 through 2010, we find that municipal police and fire departments with collective bargaining spend more on salaries, but more still on their employees' health, hospital, disability, and life insurance benefits—on the order of 15 to 25% more; and when the unions are politically active in elections, our data suggest (tentatively) that their impacts are still greater.

We think it makes sense to use costs as an opening wedge in studying public sector unions and their larger significance. Costs are fundamental to an understanding of government. They inevitably shape how government operates, what it can afford to do, and how well it can serve its citizens—and, particularly in this era of austerity, they are integrally bound up with politics, policy, and power.²⁴

But again, this is just an opening wedge. Going forward, research on these unions must cast a wider net—and the payoffs from doing so are likely to be substantial. The most obvious reason is that public sector unions are interest groups of genuine importance. They have more than eight million members, are among the top contributors to political campaigns, are well organized and active in the political process at all levels of government, and are core members of the Democratic coalition—yet political scientists have never seriously studied them. Were this to change, it would promote a better, more complete understanding of the American interest group system and how it shapes government.

The study of public sector unions would also broaden the contours of the way scholars have long approached the study of interest groups. It would prompt them to look inside the government for politically relevant interests, not just at those that arise from the outside in civil society. It would prompt them to expand the range of politically relevant policies to include important personnel issues—wages, health insurance, pensions, employment—that have thus far been ignored. It would prompt them to look beyond the usual political processes to include collective bargaining—for the first time—as an important arena of public decision making. And it would prompt them to recognize that a big portion of interest group politics—with far-reaching policy consequences—occurs at the state and local levels and that a focus on national politics is very limiting.

²³ We have also run models that include an interaction between bargaining and endorsements. We do not have a clear expectation, however, about what the coefficient on the interaction should be. A negative coefficient would mean that political activity has a greater effect for unions that do not have collective bargaining. A positive coefficient would mean that political activity is more effective when unions have bargaining rights. Both are plausible—as is the hypothesis that the coefficient on the interaction should be zero. As it happens, when we run the models with interaction terms, no clear pattern emerges.

²⁴ See Pierson (2001) and Kiewiet and McCubbins (2014).

Finally, we want to emphasize the broader importance of one of our basic findings: that public sector unions have much greater influence over benefits than they do over wages. This finding provides empirical support for the theoretical argument that, because benefits are far more complex, technical, and difficult to understand than wages, they fall into an “electoral blind spot” (Bawn et al. 2012) that makes these policies especially attractive to politicians and parties as a means of satisfying interest group demands—while ordinary voters are kept in the dark. The concept of the “electoral blind spot,” we think, has real promise. It helps explain why some policies are favored over others. But it also offers what may prove to be a very valuable theoretical bridge—one that contributes to a larger understanding of interest groups, parties, politicians, and voters.

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Table 1 Effect of Public Employee Organization on Wages, Employment, and Payroll, 1972–87

	<i>Fire-Protection Employees</i>				<i>Police-Protection Employees</i>			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Average Wage		Employment	Payroll	Average Wage		Employment	Payroll
Union	0.038	0.038	0.073	0.101	0.023	0.023	0.023	0.036
	(0.011)	(0.011)	(0.020)	(0.021)	(0.010)	(0.010)	(0.010)	(0.012)
Ln(Population)	0.167	0.167	-0.096	0.081	0.138	0.137	-0.221	-0.091
	(0.029)	(0.029)	(0.059)	(0.065)	(0.024)	(0.024)	(0.026)	(0.034)
SES	0.058	0.058	0.037	0.089	0.062	0.063	0.075	0.131
	(0.021)	(0.021)	(0.033)	(0.039)	(0.016)	(0.016)	(0.019)	(0.023)
% Black	-0.094	-0.096	0.13	0.039	-0.138	-0.139	0.262	0.146
	(0.082)	(0.082)	(0.198)	(0.221)	(0.083)	(0.082)	(0.119)	(0.149)
% Hispanic	0.281	0.281	-0.964	-0.68	0.274	0.275	-0.01	0.254
	(0.124)	(0.124)	(0.298)	(0.339)	(0.101)	(0.101)	(0.123)	(0.154)
% Manufacturing	0.145	0.145	0.371	0.484	0.054	0.053	0.442	0.57
	(0.102)	(0.102)	(0.213)	(0.214)	(0.091)	(0.091)	(0.112)	(0.135)
% in School	0.149	0.154	-0.465	-0.45	-0.015	-0.004	-0.738	-0.691
	(0.217)	(0.217)	(0.514)	(0.564)	(0.186)	(0.186)	(0.206)	(0.256)
% in Poverty	-0.413	-0.414	-0.372	-0.769	-0.353	-0.353	-0.176	-0.499
	(0.073)	(0.073)	(0.152)	(0.167)	(0.065)	(0.065)	(0.080)	(0.100)
Population Growth	0.022	0.022	0.251	0.279	0.059	0.058	0.183	0.24
	(0.035)	(0.035)	(0.058)	(0.066)	(0.030)	(0.030)	(0.030)	(0.038)
Lagged Deviation from State Avg.	-0.078	-0.054			-0.071	-0.038		
	(0.020)	(0.037)			(0.017)	(0.026)		
Union X Lagged Deviation from State		-0.038				-0.053		
		(0.044)				(0.032)		
Observations	5149	5149	5600	5600	5959	5959	6756	6756
Unique municipalities	1316	1316	1400	1400	1527	1527	1689	1689
R ²	0.86	0.86	0.92	0.91	0.86	0.86	0.87	0.87

Note: Robust standard errors clustered by municipality in parentheses. All models include municipality fixed effects and year fixed effects.

Table 2 Collective Bargaining and Fire-Protection Compensation and Employment

	<i>Salary Expenditures / Employee</i>	<i>Health Expenditures / Employee</i>	<i>Employment</i>	<i>Salary Expenditures per Capita</i>	<i>Health Expenditures per Capita</i>
	(1)	(2)	(3)	(4)	(5)
Collective Bargaining	0.086 (0.016)	0.224 (0.023)	-0.007 (0.031)	0.086 (0.031)	0.227 (0.042)
Ln(Population)	0.054 (0.006)	0.027 (0.010)	-0.039 (0.012)	0.016 (0.013)	-0.014 (0.017)
SES	0.024 (0.010)	-0.021 (0.019)	0.112 (0.025)	0.14 (0.025)	0.086 (0.035)
Ln(Median Rent)	0.386 (0.041)	0.356 (0.067)	-0.244 (0.075)	0.103 (0.070)	-0.003 (0.106)
% Democrat	0.186 (0.054)	0.088 (0.097)	-0.005 (0.115)	0.234 (0.110)	0.264 (0.161)
Ln(Population Density)	0.034 (0.010)	0.023 (0.017)	-0.032 (0.020)	0.01 (0.020)	-0.013 (0.030)
% in Poverty	-0.258 (0.131)	0.082 (0.255)	0.269 (0.257)	-0.052 (0.247)	0.006 (0.356)
% Black	-0.071 (0.050)	-0.266 (0.101)	0.498 (0.094)	0.442 (0.097)	0.315 (0.138)
% Hispanic	0.144 (0.053)	-0.007 (0.089)	-0.192 (0.123)	-0.043 (0.130)	-0.206 (0.168)
% in School	0.089 (0.204)	0.62 (0.354)	-1.259 (0.407)	-1.13 (0.408)	-0.563 (0.539)
% Manufacturing	0.213 (0.082)	0.861 (0.141)	0.536 (0.167)	0.78 (0.166)	1.415 (0.240)
% Private Sector Union	1.658 (0.178)	2.646 (0.343)	0.126 (0.406)	1.702 (0.410)	2.74 (0.551)
Population Growth	-0.025 (0.013)	-0.017 (0.019)	-0.094 (0.020)	-0.114 (0.022)	-0.089 (0.030)
Lagged Dev. from State Avg.	0.13 (0.025)	0.402 (0.038)			
Observations	6897	6009	8809	8471	7530
R ²	0.51	0.55	0.28	0.31	0.34

Note: Robust standard errors clustered by municipality in parentheses. All models include region and year fixed effects.

Table 3 Collective Bargaining and Police Protection Compensation and Employment

	<i>Salary Expenditures / Employee</i>	<i>Health Expenditures / Employee</i>	<i>Employment</i>	<i>Salary Expenditures per Capita</i>	<i>Health Expenditures per Capita</i>
	(1)	(2)	(3)	(4)	(5)
Collective Bargaining	0.098 (0.010)	0.19 (0.016)	-0.056 (0.019)	0.042 (0.021)	0.153 (0.029)
Ln(Population)	0.044 (0.004)	0.006 (0.008)	-0.03 (0.008)	0.01 (0.008)	-0.025 (0.013)
SES	0.024 (0.007)	-0.012 (0.013)	0.026 (0.015)	0.054 (0.016)	0.017 (0.023)
Ln(Median Rent)	0.311 (0.027)	0.249 (0.041)	0.039 (0.043)	0.338 (0.048)	0.265 (0.064)
% Democrat	0.089 (0.034)	0.02 (0.065)	0.191 (0.069)	0.288 (0.072)	0.261 (0.105)
Ln(Population Density)	0.036 (0.006)	0.045 (0.010)	0.002 (0.012)	0.036 (0.012)	0.051 (0.018)
% in Poverty	-0.354 (0.069)	-0.031 (0.134)	0.344 (0.174)	-0.054 (0.175)	0.192 (0.240)
% Black	0.017 (0.030)	-0.22 (0.064)	0.727 (0.067)	0.767 (0.071)	0.502 (0.100)
% Hispanic	0.13 (0.033)	-0.046 (0.060)	0.17 (0.065)	0.317 (0.069)	0.109 (0.095)
% in School	-0.159 (0.117)	0.242 (0.220)	-1.43 (0.248)	-1.677 (0.264)	-1.166 (0.376)
% Manufacturing	0.118 (0.055)	0.532 (0.093)	0.037 (0.102)	0.162 (0.109)	0.698 (0.159)
% Private Sector Union	1.604 (0.119)	2.872 (0.230)	-0.36 (0.260)	1.227 (0.270)	2.423 (0.354)
Population Growth	-0.018 (0.006)	-0.015 (0.014)	-0.05 (0.010)	-0.07 (0.013)	-0.069 (0.014)
Lagged Dev. from State Avg.	0.188 (0.026)	0.487 (0.048)			
Observations	13,227	11,531	16,809	15,865	13,976
R ²	0.55	0.59	0.25	0.37	0.37

Note: Robust standard errors clustered by municipality in parentheses. All models include region and year fixed effects.

Table 4 Effects of Collective Bargaining and Political Activity

	<i>Salary Expenditures / Employee</i>	<i>Health Expenditures / Employee</i>	<i>Employment</i>	<i>Salary Expenditures per Capita</i>	<i>Health Expenditures per Capita</i>
	(1)	(2)	(3)	(4)	(5)
<u>Fire-Protection Employees:</u>					
Collective Bargaining	0.079 (0.017)	0.218 (0.025)	-0.039 (0.033)	0.043 (0.033)	0.174 (0.045)
Political Activity	0.023 (0.012)	0.031 (0.024)	0.062 (0.026)	0.091 (0.027)	0.115 (0.040)
R ²	0.51	0.56	0.29	0.32	0.34
N	6,232	5,469	7,917	7,604	6,818
<u>Police-Protection Employees:</u>					
Collective Bargaining	0.093 (0.011)	0.199 (0.019)	-0.083 (0.023)	0.003 (0.025)	0.127 (0.034)
Political Activity	0.003 (0.009)	-0.02 (0.018)	0.052 (0.019)	0.061 (0.020)	0.045 (0.031)
R ²	0.55	0.59	0.26	0.39	0.37
N	10,196	9,002	12,705	12,003	10,685

Note: Robust standard errors clustered by municipality in parentheses. Models include all variables from Tables 2 and 3.