

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 that the effects are substantively significant. 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.

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 at the state level and propelled by Tea Party insurgents, sought unprecedented cutbacks to collective bargaining rights for public workers.¹

In 2011 Wisconsin became ground zero in a battle so intense that Americans throughout the country literally watched it unfold over a period of weeks on the nightly news. 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 took specific aim at the teachers unions by severely limiting collective bargaining in public education (although the Idaho reforms were overturned by another union-led ballot initiative).

Throughout these battles, Republicans argued that collective bargaining increases governmental costs, especially via outsized health and pension benefits, that restrictive work rules (such as seniority provisions) undermine effective organization—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 the kinds of pensions and health benefits that governments tend to provide—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'

¹ An online appendix containing data description and supplemental empirical analysis is available at <http://journals.cambridge.org/JOP>. Data and supporting materials for reproducing the results in the paper will be made available upon publication at (Author's website).

power base, providing members, money, and activists. When Republicans weaken collective bargaining, then, they are weakening the Democratic Party—and ultimately the values, policies, and programs 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 to an understanding of American government. And public sector unions are clearly interest groups of the first magnitude. Yet in the field of American Politics, these unions and the issues surrounding them are almost never studied.² In recent years there have been calls for making interest groups more central to the field (Bawn et al., 2012; Hacker and Pierson, 2010)—as they in fact were, in decades past (e.g., Schattschneider, 1935; Lowi, 1969; McConnell, 1970)—but if this is to happen, public sector unions need to become an integral part of that agenda. In our view, the interest group system and its effects on government and politics cannot be understood without taking them seriously into account.

New research is needed along many fronts. In this paper, 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

² For exceptions, see Johnson and Libecap (1994); Carpenter (2001); and Moe (2006, 2011).

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 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 much more work needs to be carried out if the broader effects of public sector unions on American government and politics are to be well understood—and if scholars, by venturing into this new territory, are to construct a more fully developed perspective on the American interest group system.

Public Sector Unions as Interest Groups

Over a period of many decades, going back to Schattschneider's (1935) classic study of the Smoot-Halley tariff bill, political scientists have generated a voluminous body of work on American interest groups. This work, it's fair to say, has four distinguishing characteristics. One, it sees interest groups as emerging to represent interests that are rooted in civil society, *outside* of government. Two, its focus is on the efforts of these groups to influence important types of government *policies*. Three, it sees *lobbying*—often backed by electoral clout—as the prime avenue of policy influence. And four, its main concern is with politics at the *national* level.³

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; and if they were, the effect would be to broaden every one of these distinguishing features of the way American interest groups are thought about and studied.

³ For excellent overviews of the literature that support the basic points we make here and below, see Maisel and Berry (2012) and Cigler and Loomis (2011).

Public sector unions represent interests that arise not from civil society, but from *inside* the government—for the purpose of influencing government itself. Their core interests are the job interests of public employees, which arise inevitably wherever government exists. How successfully these job interests get organized into unions and how much power is behind them can vary considerably across contexts, of course. Union density is much higher, for example, in local and state governments than in the federal government, and much higher for some public occupations—teachers, fire fighters, police officers—than others. Also, public sector unions are weaker in the Southern and border states than the Northeast, Midwest, and West Coast (e.g., Kearney, 2009). But 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 fifty states and thousands of local governments across the country. Seeing it as such opens up a vast realm of interest group politics that political scientists have yet to explore.⁴

It also encourages a new perspective on the connection between interest groups and public policy. Students of American politics have long paid attention to what might seem to be a full range of important policies—the environment, defense, agriculture, gun control, abortion, and more—in assessing interest groups and their influence, and much has been learned. The policies of greatest salience to public sector unions, however, are *personnel* policies on wages, health benefits, pensions, employment levels, and work rules. These policies fundamentally shape the costs and organization of government, and they stand to have major consequences for society, particularly in the modern era of austerity and retrenchment. 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.

⁴ Were scholars to broaden their terrain to include “inside” interests, they would be led to study more than public sector unions. Think, for example, of the associations of school administrators and school boards that are quite active in education politics, and also of such groups as 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).

Another new opening has to do with the mechanisms of interest group influence. Scholars have long put the focus on group lobbying—usually in legislatures, but with analogues in the executive (and the courts), backed by campaign contributions, grassroots activism, and other means of making lobbying more effective. Public sector unions attempt to influence policy in exactly the same ways. But they also have another avenue of influence: *collective bargaining*—a distinctive arena of governmental decision making, structured by its own rules and participants, where public 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, and indeed unrecognized, as an integral component of American government and interest group politics. Attention to public sector unions would change that.

Finally, interest group research has long been heavily focused on national politics: on lobbying in Congress, national policy issues, and federal elections.⁵ But this perspective gives scant attention to much of what goes on in American government and politics—which occurs at the state and local levels, where most public money is spent, most public employees work, and countless policy decisions are regularly made on matters of great importance, 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 paper, 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

⁵ The major exception here is the work of Gray and Lowery. See their recent overview and critique of the literature in Lowery and Gray (2011).

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.⁶

They can exercise power in two basic ways. One is through collective bargaining: formal negotiations with management, backed by the unions' implicit threat to engage in strikes, slowdowns, sickouts, and other coordinated work actions if necessary. The other—available to all public sector unions, whether or not they have collective bargaining—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 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.⁷

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. This difficulty is compounded by the fact that wage settlements are very visible, easily understood by the public, attract media

⁶ As Freeman and Medoff (1984) argue, 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).

⁷ The economists who contributed to the early research on public sector unions were well aware of the politics of what they were studying, and the elements we highlight below were discussed in that literature. See, e.g., Wellington and Winter (1971); Courant, Gramlich, and Rubinfeld (1979); Bellante and Long (1981).

scrutiny—and threaten to become intensely controversial. Even when large wage gains are fiscally possible, they can set 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 in recent years as health insurance costs have soared, but another advantage remains—a big one. Health and pension benefits are extremely complicated and technical, difficult for the public to understand, difficult for the media to convey—and thus nearly invisible politically. Politicians can agree to major 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) for benefits that will be paid *in the future* by *other* politicians and taxpayers, with little impact on current budgets.⁸ From the standpoint of politicians, then, benefits are precisely the kind of policy that falls into what Bawn et al. (2012) have referred 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.

A second expectation has to do with levels of employment, which the unions also care about. Economic theory would suggest that they face a tradeoff between compensation and employment: as wages and benefits go up, labor becomes more costly and less of it will be purchased, leading to declines in employment. Governments, however, are not markets. Decisions are made on political grounds, and 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,

⁸ On the political attractiveness of promising future benefits, see Kiewiet (2010), Ichniowski (1980).

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 Kearney, 2009).⁹ 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, 1989; Zax and Ichniowski, 1988). However, the study of union impacts on compensation is almost always limited to employee pay—with no attention to fringe benefits. Only a few studies take benefits into account, because benefits are much more difficult to measure and good data sources are elusive; but such studies suggest that public sector 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 the impact of unions on total compensation. On other basic counts, the literature also fails to arrive at clear conclusions. On employment, in particular, the results are

⁹ 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, e.g., Bahrami, Bitzan, and Leitch (2009).

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 (Trejo, 1991; Valletta, 1993).

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.

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

We begin by focusing on a time period in which public sector employees were first securing collective bargaining rights: the 1970s and early 1980s.¹⁰ This is arguably the best context for estimating the causal effect of unionism on governments' finances since we can examine the conditions of 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 public sector employees over time.

In turning to data collected in the 1970s and 1980s, we are in some ways revisiting territory explored by scholars thirty to forty years ago – scholars who grappled with two of the biggest challenges

¹⁰ Prior to the 1960s, few government employees belonged to unions, and collective bargaining in the public sector was almost nonexistent. Many of the organizations that did exist were either professional associations or mutual benefit societies. That all changed in the 1960s, 1970s, and early 1980s, when most states enacted collective bargaining laws for government workers. The result was 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, where it stabilized in an equilibrium that still prevails. See Freeman (1986), West (2008), Kearney (2009), Moe (2011).

to estimating the causal impact of public employee organization: measurement and endogeneity. The measurement challenge is simply that data on public sector unions are scarce, and the data that do exist have a number of problems (see Freeman, Ichniowski, and Zax, 1988). As we describe below, we largely adopt the earlier literature's conventions for dealing with measurement problems. However, our handling of the potential endogeneity issues 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 (e.g., Trejo, 1991), and they also tend to pay higher wages. If we were to ignore the importance of city size, our estimates of the effect of unionization on wages would be biased upward. Thus, to estimate the causal effect of unionization, we have 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 public sector unionization during the 1970s and 1980s is that we can design an empirical analysis that substantially reduces the potential for omitted variable bias by isolating governments' conditions before and after their employees formed unions. Specifically, in 1972, 1977, 1982, and 1987, the Census of Government conducted a special Labor-Management Relations Survey, which included questions about whether governments had collective bargaining and whether certain groups of employees were members of unions. By assembling these data into a panel, we can estimate the effect of unionization by leveraging within-government variation, partialling out the effects of any time-constant city characteristics that could be a source of bias. This presents a tremendous opportunity to conduct clear causal inference – one that cannot be replicated using data from later periods.

Surprisingly, most of the economic studies of the 1980s relied on cross-sectional data rather than longitudinal data to estimate the impacts of public sector unionization and collective bargaining (e.g., Brown and Medoff, 1988). Moreover, the few studies that did use longitudinal data had a narrow temporal focus (e.g., four years) and did not have any within-unit variation in their unionization measures. In fact, the only study that puts the full 1972-1987 Census of Governments panel together to conduct a within-unit analysis of the effect of unionization is one by Hoxby (1996), whose focus is solely on the impact of teacher unionization on school district outcomes.

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.¹¹ 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, wages and staffing levels might influence whether a city's employees get organized. The direction of the bias in that case would likely be negative, however: public sector employees are probably more motivated to form unions when wages and employment are low. Nonetheless, the more general concern is that there may be city characteristics that vary over time that influence both its employees' propensity to unionize and its wages and employment—and to the extent that such factors exist, we must incorporate them into our models.

Data and Empirical Strategy

¹¹ The first organizations of police officers and firefighters were formed in the 19th century as mutual benefit societies. While some affiliated with the American Federation of Labor as early as the late 1890s, collective bargaining was rarely an option until the 1960s, when state governments began passing labor laws for public employees. Today, firefighters and police officers are among the most highly unionized government employees in the United States. For data, see www.unionstats.com.

Our data come from the U.S. Census of Government Public Employment Files from 1972, 1977, 1982, and 1987, which contain the data from the aforementioned Labor-Management Relations Surveys as well as the regular government employment and payroll information collected during each quinquennial census. In each of the years, municipal governments reported to the Census how many of their police and fire protection employees were members of employee organizations. Because there is a significant degree of 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.¹²

Dichotomizing the variable does not fully address the issue of measurement error, however. Examining the data, we find that a number of cities reported that their fire or police employees were unionized in one year but not in subsequent years. Fortunately, this too is a pattern that Freeman et al. recognized and addressed. And when they conducted telephone interviews with 258 of the governments in their data that, according to the Census, had lost (or lost and regained) 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. Most of the time, it was the former.

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 because we want to make this analysis consistent with the study we present in the next section, we limit our focus to cities that had at least 10,000 residents as of 1972. This gives us a dataset of 1,689 city

¹² When we use the percentages of police and fire employees in unions, our results are very similar. See the online appendix.

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.¹³ 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 with 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 more adults employed in manufacturing, lower poverty rates, smaller percentages of the population enrolled in elementary and high school, and lower rates of population growth. Because we suspect that these correlates of unionization might also be associated with wages, payroll, and employment, we control for the following in our models: the natural log of city population, population growth, socioeconomic status,¹⁴ percent African American, percent Hispanic, percent living in poverty, percent enrolled in elementary or high school, and the percentage of employed adults who work in manufacturing.

¹³ Average wage and payroll figures are adjusted to 1987 dollars.

¹⁴ This is an average of logged per capita income and percent of city residents with a college degree.

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 police and fire protection 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.¹⁵ 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.¹⁶

¹⁵ Average wage for police and fire protection employees was not available for 1967, but in the years 1972-1987, 40% of the typical city’s full-time payroll expenditures went to police and fire functions. Therefore, the average wage for all city employees is a reasonable proxy for police and fire average wages.

¹⁶ In this paper, we are only interested in these variables as controls, in order to reduce any bias in our estimates of union impact. We should note that there is substantial debate in the economics literature over whether union threat effects exist and, if so, how large they are. See, e.g., Lovenheim (2009) and Ichniowski, Freeman, and Lauer (1989).

Empirical Results

The results from our analysis of the Census of Government data are set out in Table 1. 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.

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 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 they are organized than when they are not—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 affiliated with a variety of organizations such as 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 that is conducive to political organizing (Stern, 1984). Moreover, firefighters are more popular than police officers,¹⁷ and their popularity may translate into greater political clout. Thus, while we expect unionized police to have greater influence on policy than nonunionized police, the effect of unionization might be smaller for police officers than for firefighters.

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, however, than those for fire departments. In columns 5 and 6, we find that police officers who formed unions saw their wages increase by about 2.3% as a result, an effect that is significant at the 1% level. Police per capita staffing levels also increased within cities where police organized, as we show 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 municipal government employers for better wages, higher employment, and an overall increase in the amount cities spent on police compensation—even if they were somewhat less successful than firefighter unions.

Most of our control variables behave as expected, and so to save space we do not discuss their effects here. Worth noting is the evidence that city officials did react to whether their wages were low or high relative to similar cities. In columns 1 and 5, we find that when a city's average wage in the

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 Regina A. Corso, “Firefighters, Scientists, and Doctors Seen as Most Prestigious Occupations,” Harris Interactive, August 4, 2009.

previous period was relatively low, city officials responded by increasing police and fire protection wages. However, it is not clear from these results whether all cities adjusted in this way – which would suggest a general competition effect – or whether adjustments were mostly made by nonunion cities in response 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 importantly, though, on the question of how the unionization of municipal police and fire departments affected their wages, employment, and payroll expenditures, our results are very clear. Across the board, we find that police and fire unions were successful in 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 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, while examining this critical period in history is the only way to observe cities before and after their employees unionized, it has some disadvantages. Most importantly, we have no way of knowing how much cities spent on non-salary forms of compensation at the time. If anything, we expect the effects of unionization were even more pronounced for fringe benefits like health insurance and

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.

retirement packages, which means that our estimates in Table 1 are probably lower bounds on the effects of unionization for overall employee compensation. Moreover, the results in Table 1 illustrate the effect of unionization as of 25 to 40 years ago. Clearly, we also want to know how unionization influences the cost of government today. In the next section, therefore, we carry out a study using more current data that include information on public sector employees' benefits.

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

By the mid-1980s, the rapid wave of public sector unionization had subsided and a new equilibrium had set in. For the most part, the groups of public employees that were going to unionize had already 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 public sector collective bargaining for government in the 1990s and 2000s. To do this, we have assembled a rich new dataset on the collective bargaining status, employment levels, and compensation practices of police and fire departments in American municipal governments. Not only is this dataset more current than the Census of Government dataset, but it also contains data on cities' 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.

As we noted above, any study that relies on recent data to examine the impact of public sector unionism must deal with a considerable challenge: very few governments adopted collective bargaining for the first time after the 1980s. In our current dataset, then, 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. Fortunately, our analysis of the 1970s and 1980s gives us some sense of what we

should expect. But that was 25 to 40 years ago. Clearly, it is also critical that we understand the difference public sector unions make for governments today.

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 protection employees, we focus our analysis on five dependent variables. They are: the amount the department spends per employee on salaries and wages, including base salaries as well as supplemental forms of pay like longevity pay, hazard pay, holiday pay, and overtime pay; the amount the department spends per employee on health, hospital, disability, and life insurance benefits; the total number of employees in each department per city resident; the total amount spent on employees' salaries per capita; and total amount spent 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, 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 rely on the Labor Management Relations surveys conducted by ICMA in 1988 and 1999, which asked a series of questions about the collective bargaining status of various groups of municipal employees. Lastly, we use a special 1977 survey conducted by the Census of Governments that documented whether certain groups of employees in each municipal government were part of a bargaining unit. We describe how we combined these datasets and coded the collective bargaining indicators in the online appendix.

Our strategy for estimating the impact of collective bargaining is similar to the one we used in 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.¹⁹ We include all of the 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 in the city (to account for cost of living differences within and across cities). All of the demographic data come from the 1990, 2000, and 2010 Censuses and the 2005-2009 estimates from the Census' 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 between our empirical strategy here and that of 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 policies, we

¹⁹ 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.

control for the percentage of the two-party vote that went to Al Gore in the 2000 presidential election in the municipality's parent county. And because some states and regions have more worker-friendly cultures than others,²⁰ we control for the rate of private sector union membership in each state and year using data compiled by Hirsch and MacPherson (2003, 2011), and we also include dummy variables for three of the four geographic regions in the United States. As in all of our earlier models, we include year fixed effects.

Empirical Analysis

We start with an analysis of municipal fire departments, the results of which are set out in Table 2. In column 1, we estimate the effects of collective bargaining on the amount cities spend per employee on salaries. The effect we estimate here is larger than the effect we estimated using data from the 1970s and 1980s: on average, municipal fire departments with collective bargaining spend about 9% more per employee on salaries and wages. Notably, however, the differences between fire departments with and without collective bargaining are even greater when it comes to per-employee expenditures 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. Relative to the average across cities, this amounts to an extra \$1,507 per employee per year.

In column 3, we test whether these wage and benefit premiums come at the cost of fire protection employment levels. We find that they do not: there is no significant difference between per capita fire protection employment in cities with and without collective bargaining.²¹ Thus, it comes as no surprise

²⁰ Most notably, the southern and border states have historically been less union-friendly than other regions. See, for example, the state and regional unionization rates at www.unionstats.com.

²¹ The explanation could be that the firefighters have lost power over the years. On the other hand, as we argue in the Expectations section, it can be rational for unions to pursue either higher or lower employment levels; and it could be that, with the change in their environment—from one of disruption and growth to one of settled equilibrium—their strategies changed to put less emphasis on employment levels. We do not have data on this, though, and cannot test for it.

that cities where firefighters have collective bargaining spend more overall on employee compensation, which we show in columns 4 and 5. In column 4, we find that cities with collective bargaining spend 9% more per city resident on fire protection salaries. And as column 5 shows, the differences are bigger for spending on health benefits, an area in which cities with collective bargaining spend over 25% more.

These results clearly indicate that collective bargaining for fire protection employees 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: that is the area in which unions have secured the greatest gains over the years.²²

In Table 3, we find that the effects of collective bargaining in municipal 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 protection employee than cities where police do not have formal bargaining rights. As with firefighters, the gap between the two types of cities is wider when it comes to expenditures on health benefits. The typical municipal police department with collective bargaining spends about 21% more on health benefits per employee – or about \$1200 – than the typical city without collective bargaining. Both of these positive effects are statistically significant at the 1% level.

In contrast to our findings for fire departments, however, we find that police departments with collective bargaining operate at lower per capita staffing levels than non-bargaining departments.

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 this more current data set. (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 may well 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. As we noted earlier, 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.

Specifically, in column 3, we find that 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. And because the salary and health benefits premiums observed in columns 1 and 2 are partially offset by these lower employment levels, the consequences of collective bargaining for cities' total per capita expenditures on police compensation are slightly more muted than for fire protection. In columns 4 and 5, we find that cities where police have collective bargaining spend 4.3% more on salaries per capita and about 16.5% more on health benefits per capita.

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 non-bargaining 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

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 police and firefighter unions' political activity in each city 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.

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

²³ We have also run models that include an interaction between collective bargaining and endorsements activity to test whether the impact of political activity differs for cities with and without bargaining rights. We do not have a clear theoretical expectation, however, about what the coefficient on the interaction term 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 unions' political activity is more effective when those unions have bargaining rights. Both are plausible

We view the results of Table 4 as preliminary, but the general direction of the estimates is clear: both collective bargaining and public sector unions' political activity increase the amount that municipal police and fire departments spend on employee compensation. As we have argued from the outset, 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 paper, 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 much wider net—and in our view, 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

hypotheses – as is the hypothesis that the coefficient on the interaction term should be zero. As it happens, when we run the models from Table 4 with interaction terms, no clear pattern emerges.

²⁴ On the modern era generally, see Pierson (2001). For an account of cost-politics in a much earlier era, see, e.g., McDonald's (1986) history of San Francisco politics in the late 1800s.

than eight million members nationwide, are among the top contributors to political campaigns, are well organized and active in the political process at all levels of government, are core members of the Democratic coalition, and more—yet political scientists have never seriously studied them. Were this to change, it would surely promote a better, more complete understanding of the American interest group system and how it shapes American politics and 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.

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 “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 a very valuable theoretical bridge—one that helps connect our 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, 1972-1987

	<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-squared	0.86	0.86	0.92	0.91	0.86	0.86	0.87	0.87

Notes: Robust standard errors clustered by municipality in parentheses. All models include municipality fixed effects and year fixed effects. Hypothesis tests on *Union* are one-tailed in columns 1-2, 4-6, and 8; all other tests are two-tailed. * significant at 10%; ** significant at 5%; *** significant at 1%

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-squared	0.51	0.55	0.28	0.31	0.34

Notes: Robust standard errors clustered by municipality in parentheses. All models include region and year fixed effects. Hypothesis tests on *Collective Bargaining* are one-tailed in all but column 3; all other tests are two-tailed. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 3: Collective Bargaining and Police Protection Compensation and Employment

	<i>Salary expenditures / employee (1)</i>	<i>Health expenditures / employee (2)</i>	<i>Employment (3)</i>	<i>Salary expenditures per capita (4)</i>	<i>Health expenditures per capita (5)</i>
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-squared	0.55	0.59	0.25	0.37	0.37

Notes: Robust standard errors clustered by municipality in parentheses. All models include region and year fixed effects. Hypothesis tests on *Collective Bargaining* are one-tailed in all but column 3; all other tests are two-tailed. * significant at 10%; ** significant at 5%; *** significant at 1%.

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-squared	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-squared	0.55	0.59	0.26	0.39	0.37
N	10,196	9,002	12,705	12,003	10,685

Notes: Robust standard errors clustered by municipality in parentheses. Models include all variables from Tables 2 and 3. All hypothesis tests are one-tailed except for column 3. * p<0.1; ** p<0.05; *** p<0.01