STATE COLLECTIVE BARGAINING LAWS AND PUBLIC-SECTOR PAY

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Using the Public Use Microdata Sample from the 2005 to 2015 American Community Survey, the authors provide new evidence on how state collective bargaining laws affect public-sector wages. To isolate the causal effect of bargaining laws on public-sector pay, they examine wage differentials between otherwise similar public- and private-sector employees located in the same local labor market. They estimate difference-in-differences (DD) models that exploit two sources of plausibly exogenous variation: 1) policy discontinuities along state borders and 2) variation within states in collective bargaining laws in states where the majority of public workers are without collective bargaining rights. Findings show that mandatory collective bargaining laws increase public-sector wages by approximately 5 to 8 percentage points. Results therefore suggest that mandatory collective bargaining laws provide a formal mechanism through which public-sector workers are able to bargain for increased compensation.

The Great Recession and the ensuing state and local budget deficits that it prompted have reinvigorated the debate over the rights of public-sector workers and their unions. Since 2011, state legislators across the country have introduced bills designed to weaken or eliminate the collective bargaining (CB) rights of public-sector workers. Most notably, the signing of Wisconsin Act 10 in 2011, which significantly restricted the CB rights of public-sector workers, sparked a national debate over public-sector worker compensation.

Proponents of scaling back or eliminating bargaining rights have argued that public-sector employees are overpaid, and furthermore, that public-sector unions exploit their political power to elect pro-union government officials to control both sides of the bargaining table (Wellington and Winter 1972; Lewin, Keefe, and Kochan 2012). Opponents argue that

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public-sector workers are underpaid and that collective bargaining is a fundamental right.

In this article, we present new evidence on how state CB laws affect public-sector pay. Using data from the 2005 to 2015 Public Use Microdata Sample (PUMS) of the American Community Survey (ACS), we estimate standard log wage regressions that include state and local labor market fixed effects. In these models, the key explanatory variable is an interaction term between an indicator for whether an individual is a public-sector employee and an indicator for whether a state has a mandatory CB law. The coefficient on this interaction term measures how the public–private wage differential differs in states that do and do not mandate collective bargaining.

Isolating the effect of state CB laws on public-employee compensation is challenging because those laws are likely correlated with other state unobservables that influence public-sector compensation. For example, state CB laws may be correlated with unobserved worker and voter sentiment toward public-sector unions, implying that voters in states with strong CB rights might choose to provide higher compensation for public-sector workers regardless of whether those workers were covered by a CB agreement (Hirsch, Macpherson, and Winters 2012).

We attempt to address these challenges in several ways. First, rather than focusing on how state CB laws affect public-employee compensation, as most of the previous literature has done, we focus on how CB laws affect the wage *differential* between otherwise similar public- and private-sector employees. As noted by Diamond (2017), by comparing wage differentials between otherwise similar public- and private-sector workers in the same local labor market, we are able to control for unobserved differences in labor market conditions as well as differences in the skill sets and compensation schemes of public- and private-sector workers that may be correlated with both state CB laws and public-sector pay.

Second, our primary identification strategy exploits policy discontinuities at state borders to identify the effect of mandatory CB laws on public-sector wages. Specifically, we focus on workers located in commuting zones (CZs) that cross state boundaries and estimate models that include CZ-by-public-sector-employee fixed effects. In these models we are essentially comparing public- and private-sector wage differentials along state borders within CZs, where one state within a CZ mandates collective bargaining and the other state does not. Our identification strategy therefore utilizes only within-CZ variation in the strength of state CB laws and thus controls for a host of unobservables that potentially might otherwise bias estimates of the impact of state CB laws on public-sector wages.

Finally, we examine variation *within* states in CB laws. Specifically, we focus on states that generally prohibit collective bargaining and exploit that several of those states allow collective bargaining for firefighters or police (or in some cases both). We then estimate models that compare the public-and private-sector wage differential in states that authorize police and firefighters to bargain to the public- and private-sector wage differential in states that prohibit police and firefighters from bargaining.

We find that mandatory CB laws increase public-sector wages by approximately 5 to 8 percentage points. Drilling down to specific occupations, we find that mandatory CB laws increase the wages of teachers, police, and firefighters. A series of robustness checks suggests these results are highly robust. For example, our results persist across specifications that include occupation fixed effects and therefore compare wage differentials only among workers in similar occupations. They also persist across specifications that include additional state-specific local labor market controls for a variety of factors that might be correlated with mandatory CB laws, such as the propensity to vote for the Democratic presidential candidate. Finally, based on a series of falsification tests, we find no evidence that mandatory CB laws affect the wages of either federal or nonprofit employees, thus providing further evidence that our results have a causal interpretation.

Previous Research

Our work is most closely related to a relatively small strand of literature that examines the effects of public-sector CB laws on public-sector earnings. Freeman (1986) and Freeman and Valletta (1988) summarized the early literature on this topic. The majority of that literature found that a favorable legal environment toward collective bargaining increases public-sector compensation.[1] For example, Freeman and Valletta (1988) found that public-sector employees in states with laws favorable to collective bargaining have an approximate 6% wage advantage, and Ichniowski, Freeman, and Lauer (1989) found that police compensation is higher in states with stronger CB laws. Similarly, using an instrumental variable identification strategy, Hirsch et al. (2012) found that state CB laws increased teacher wages by approximately 12 percentage points. Most recently, Frandsen (2016) exploited differences in the timing of the enactment of state CB laws to isolate the effect of those laws on public-sector compensation. Using historical data from the Current Population Survey (CPS), he found that mandatory CB laws increased the wages of firefighters and police but had little effect on the wages of teachers.[2]

Our article is also closely related to two recent studies on public-sector rent extraction. Brueckner and Neumark (2014) developed a model that posited the ability of public-sector workers to extract rents would depend in

[1]Several papers focus solely on effects of unionization. Most of those papers, including Hoxby (1996) and Baugh and Stone (1982), found substantial wage effects ranging from 5 to 22%. Important exceptions include Kleiner and Petree (1988) and Lovenheim (2009) who found the impact of unionization to be close to zero for teacher pay.

[2]Zax and Ichniowski (1990) and Frandsen (2016) also provided evidence that suggests mandatory CB laws significantly increase unionization rates among public-sector workers.

part on the level of desirable local amenities. Consistent with their theoretical predictions, they found that public-sector wages were higher in states with desirable local amenities, with the effect of amenities being stronger in states that mandated collective bargaining. Building on the work of Brueckner and Neumark (2014), Diamond (2017) developed a model that predicted that a less elastic housing supply would increase the ability of public-sector workers to extract rents. Consistent with that prediction, she found that public-sector wages were higher in metropolitan areas that have a less elastic housing supply, with the effect of an inelastic housing supply being stronger in states that mandated collective bargaining.[3]

Our work builds on these studies and makes several important contributions to the literature. First, prior studies that examined the effect of state CB laws on public-sector compensation tend to be based on either cross-sectional or longitudinal data that exploits historical variation in the timing of state adoption of CB laws. For example, the analysis of Frandsen (2016) is based on changes in bargaining laws that occurred primarily during the 1960s and 1970s, and those laws have remained relatively unchanged for the past 50 years. As a result, it is unclear whether his results (or the results of other prior studies) would generalize to more recent time periods. By contrast, we exploit variation across state borders within local labor markets and use much more recent data on public-sector wages to isolate the causal effect of CB laws on public-sector compensation. Second, although Brueckner and Neumark (2014) and Diamond (2017) both found that mandatory CB laws provide public-sector workers with a formal mechanism to extract rents in the presence of desirable local amenities or an inelastic housing supply, both studies refrain from attempting to isolate the direct causal effect of mandatory CB laws on public-sector compensation. Alternatively, isolating the causal effect of CB laws is the primary focus of this article.

Data

Our primary source of data is the Public Use Microdata Sample (PUMS) from the 2005 to 2015 American Community Survey (ACS). The ACS is a nationally representative survey that contains detailed demographic and labor force participation data on approximately two million households per year. The obvious advantage of using the ACS, relative to other national representative surveys, such as the Current Population Survey (CPS), is the

[3]Our study is also indirectly related to a relatively large literature on public- and private-sector wage differentials. Krueger (1988), Borjas (2002), Allegretto and Keefe (2010), Schmitt (2010), Munnell, Aubry, Hurwitz, and Quinby (2011), and Lewin et al. (2012) all found that public-sector workers earn approximately 4–10% less than their private-sector counterparts. By contrast, Biggs and Richwine (2011), Gittleman and Pierce (2012), and Bewerunge and Rosen (2013) found that once public-sector benefits are taken into account, public-sector workers are overcompensated relative to their private-sector counterparts.

large sample size and its detailed demographic and labor force information. A second advantage is that the ACS includes information on the location of workers' primary employer (as well as residence), information that is essential to our border discontinuity analysis. Our sample consists of workers aged 18 to 64 years who worked full-time in the past 12 months.[4] We define full-time workers as individuals who reported working between 30 and 70 hours per week and who worked 50 to 52 weeks in the past 12 months. Note that in all of our analyses, we identify workers in our sample based on their place of work rather than their place of residence. Specifically, we assign information on CB laws and the CZ attributes discussed below to workers based on their place of work to ensure that we correctly match workers to the CB environment that governs their workplace environment.

The dependent variable in our analysis is the log of hourly wages, which is constructed using information on 1) reported total wage and salary income in the past 12 months, 2) reported number of weeks worked last year, and 3) usual hours worked per week. The hourly wage rate is then computed as total wage and salary income divided by the product of weeks worked and usual hours worked per week. We further restrict the sample to individuals with an implied hourly wage rate greater than or equal to the federal minimum wage in a given year. The full set of individual-level control variables include age, age squared, sex, years of educational attainment, four categories of marital status, and race/ethnicity indicators for individuals who self-report themselves to be Asian, black, or Hispanic.

Following Diamond (2017), among others, we restrict our analysis to workers with non-imputed earnings to guard against any bias resulting from the ACS wage imputation methods (Hirsch and Schumacher 2004; Bollinger and Hirsch 2006). Since omitting individuals with imputed earnings potentially changes the characteristics of individuals included in the sample, we follow Bollinger and Hirsch (2006) and Hirsch and Winters (2014) and reweight the respondent sample using inverse probability of response weights. Specifically, we first estimate a logit model in which the dependent variable is an indicator that takes the value of 1 for individuals with non-imputed earnings, and 0 otherwise; the regressors include the full set of control variables discussed above. We then weight our regressions by the inverse probability of response.

Nonprofit employees are included in the sample along with the private-sector for-profit employees. We exclude from our analysis self-employed workers and military personnel. We also exclude federal employees from our main analysis but utilize them subsequently in falsification tests. Our rationale for excluding federal workers relates to state and federal labor laws. Specifically, the National Labor Relations Act (NLRA) covers private-sector workers nationwide but explicitly excludes federal, state, and local

[4]We focus on workers located in the contiguous United States and thus omit Alaska and Hawaii from our analysis.

workers. State and local workers are covered separately by state-specific labor laws. Note that non-postal federal workers can be union members but collective bargaining over wages and benefits is prohibited. Non-managerial postal (USPS) workers have CB rights and can bargain over wages. As a result, although federal workers are public-sector employees, we omit them given that federal workers are not covered by state-specific CB laws.

A somewhat controversial issue is whether to control for occupational differences when examining public- and private-sector wage differentials. On the one hand, if occupation codes capture primarily unobserved differences in human capital and working conditions, then controlling for occupation would be appropriate (Gittleman and Pierce 2012). On the other hand, as Schanzenbach (2015) noted, some occupational controls may be inappropriate since little common support occurs in those occupations across the public and private sectors. For example, a significantly larger proportion of public-sector workers (25.7%) than private-sector workers (2.3%) are employed in education-related occupations. Given the controversy surrounding occupation controls, we present results without any occupation controls and results based on specifications for which we include broad, two-digit census occupational controls (25 categories).

Another controversial issue is whether to control for union coverage, which is not reported in the ACS. The consensus in the literature is to omit union coverage controls. Specifically, as noted by Hirsch, Wachter, and Gillula (2000), including a control for union coverage would be appropriate if union status was a proxy for transferrable skills, making it similar to other controls, such as educational attainment and age. Hirsch et al. (2000), however, summarized that the most credible empirical evidence on this topic does not support the belief that union status is associated with higher productivity, casting doubt on whether union status should be considered a transferrable skill.[5] More recently, Gittleman and Pierce echoed this sentiment and noted that "controlling for union coverage seems inappropriate because union wage premia probably do not reflect ability differences and those in the public work force would not likely take their public sector unionization rates with them if they were to move" (2012: 226).

The smallest identifiable geographical area in the ACS is the Public Use Microdata Area (PUMA). We used the crosswalk between 1990 CZs and 2000 PUMAs created by Autor and Dorn (2013) to allocate PUMAs to CZs for the 2005 to 2011 ACS, which includes 2000 PUMA codes.[6] For data from the 2012 to 2015 ACS, which includes 2010 PUMA codes, we used data on the division of 2010 county population across 2010 PUMAs from the Missouri Census Data Center and the crosswalk between counties and 1990

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[5]See, for example, Clark (1984) for evidence on whether union status is associated with higher productivity, and Booth (1995) for a summary of the evidence on the union productivity differential.

[6]Similar to core-based statistical areas (CBSAs), commuting zones are designed to be spatial measures of local labor markets. Unlike CBSAs, commuting zones are defined for the entire United States, not just for metropolitan areas.

CZs to allocate 2010 PUMAs to CZs. (Since CZs are aggregations of counties, the crosswalk between counties and CZs provides a perfect overlap.) In most cases, a PUMA can be matched to an exact county and therefore an exact CZ. In some instances, however, PUMAs span multiple counties that may belong to different CZs (but never different states). In that event we cannot assign an individual to a unique CZ and instead follow Autor and Dorn (2013), weighting individuals who are assigned to multiple CZs by the fraction of the area of the individual's PUMA in the given CZ.

We obtained detailed information on state CB laws from Sanes and Schmitt (2014), who documented the CB rights of teachers, firefighters, police, and all other public-sector workers in each state.[7] Classifying state CB laws can be complicated, but states fall into three general categories: CB required, CB prohibited, and CB permissible. CB-required states mandate that state and local governments "bargain in good faith" with their employees if they present themselves with a union. As categorized by Sanes and Schmitt (2014), we define CB-mandatory states as states where collective bargaining is legal and wage negotiation is also legal. By contrast, CB-prohibited states explicitly prohibit state and local employers from bargaining with worker unions. Finally, in CB-permissible states, state and local governments may choose whether to bargain if employees request to do so. As described by Frandsen (2016), lack of a statute regarding collective bargaining has typically been interpreted in the courts as an implicit prohibition. For simplicity, we therefore categorize states into two groups: those with mandatory CB laws and those that explicitly or implicitly prohibit collective bargaining.

A complication with this classification scheme relates to heterogeneity in the right to bargain across occupations within some states. To overcome this issue, in our primary analysis we drop the workers who do not match with their corresponding state's classification. For example, Texas strictly prohibits collective bargaining for all public workers except police and firefighters. Since these two occupations do not match Texas's classification, we drop police and firefighters in Texas from our sample. We also estimate models in which we focus directly on individual occupations and use the specific bargaining laws for these groups. For example, we estimate separate regressions for teachers, police, firefighters, and all other local government employees. The top panel of Table 1 shows the states that fall into the three classifications of CB laws, and the bottom panel presents the states that generally prohibit collective bargaining but allow either police or firefighters to bargain.

In the empirical analysis that follows we include controls for several state- and CZ-level variables that could potentially affect public- and private-sector

[7]To minimize inaccuracies, we compared and validated the classifications developed by Sanes and Schmitt (2014) with other sources of information on state CB laws, for example, Valletta and Freeman (1985) and Brueckner and Neumark (2014).

Table 1. Collective Bargaining Environment for Public-Sector Workers

Collective Bargaining (CB) category	*States*
CB required	California, Connecticut, Delaware, Florida, Illinois, Indiana, Iowa, Kansas, Maine, Maryland, Massachusetts, Michigan, Minnesota, Montana, Nebraska, Nevada, New Hampshire, New Jersey, New Mexico, New York, Ohio, Oregon, Pennsylvania, Rhode Island, South Dakota, Vermont, Washington, Wisconsin
CB allowed	Alabama, Arizona, Arkansas, Colorado, Idaho, Kentucky, Louisiana, Mississippi, Missouri, North Dakota, Oklahoma, Tennessee, Utah, West Virginia, Wyoming
CB prohibited	Georgia, North Carolina, South Carolina, Texas, Virginia
State	CB authorized occupations in states that generally prohibit collective bargaining
Arizona	Firefighters, Police
Georgia	Firefighters
Texas	Firefighters, Police

wage differentials. First, in all of our specifications we include an indicator for whether a state had a Right-to-Work (RTW) law in place in year *t*.[8] In states with RTW laws, employees cannot be compelled to join a union or to pay union agency fees as a condition of employment. As a result, RTW laws may potentially reduce the power of unions by reducing their membership and resources. Second, we constructed the four amenity variables used by Brueckner and Neumark (2014) in their analysis of the effects of amenities on public-sector rent extraction. The variables include *Mild* and *Dry* representing local temperature and humidity, respectively; *Proximity* measuring the average distance to the nearest coast or navigable water; and *Density* measuring population density.[9] Third, as noted by Anzia and Moe (2014), liberal states tend to grant more generous compensation packages to public-sector employees. We therefore control for the presidential Democratic vote share in the 2004 and 2008 presidential elections to proxy for voter sentiment toward public-sector employees.[10] With the exception of the RTW indicator, all of these variables were constructed by aggregating county-level data up to either the CZ level or the state-by-CZ level.

[8]Data on the timing of enactment of state RTW laws come from National Conference of State Legislatures.

[9]Climate data are from the Area Resource File (ARF) maintained by the Health Resources & Services Administration. Mild temperature is the negative of the sum of the absolute values of the differences between monthly average temperature and 20 degrees Celsius, summed over January, April, July, and October. Dry weather is the negative of the average monthly precipitation for those four months. Coastal proximity data come from Rappaport and Sachs (2003), and proximity is the negative of the average distance from each county's centroid to the nearest coast, Great Lake, or major river. Population density data come from 2005–2009 ACS data.

[10]County-level presidential vote tallies for the 2004 and 2008 presidential elections were obtained from the Federal Election Commission. We use vote tallies from the 2004 presidential election for years 2005–2008 and vote tallies from the 2008 presidential election for years 2009–2015.

Table 2. Summary Statistics

	CB mandatory				CB non-mandatory			
	Public		Private		Public		Private	
Variables	*Mean*	*SD*	*Mean*	*SD*	*Mean*	*SD*	*Mean*	*SD*
Salary	55,484	31,213	58,672	57,825	44,900	27,622	54,008	50,182
Individual controls								
Age	45.634	10.839	42.979	11.588	44.947	11.088	42.542	11.612
Female	0.540	0.498	0.453	0.498	0.587	0.492	0.440	0.496
Less than high school	0.018	0.135	0.064	0.245	0.025	0.157	0.075	0.264
High school degree	0.458	0.498	0.580	0.494	0.440	0.496	0.603	0.489
Bachelor's degree	0.255	0.436	0.238	0.426	0.276	0.447	0.223	0.416
Advanced degree	0.268	0.443	0.118	0.323	0.259	0.438	0.099	0.299
Black	0.087	0.282	0.054	0.226	0.140	0.347	0.100	0.300
Hispanic	0.094	0.292	0.118	0.322	0.085	0.279	0.111	0.314
Asian	0.043	0.202	0.062	0.241	0.017	0.129	0.028	0.166
Married	0.676	0.468	0.633	0.482	0.689	0.463	0.655	0.476
State and CZ controls								
Right-to-Work	0.177	0.381	0.187	0.390	0.842	0.365	0.832	0.374
Mild	–37.666	12.096	–38.362	12.090	–29.443	8.863	–29.644	8.784
Dry	–7.606	2.501	–7.582	2.415	–8.252	2.800	–8.147	2.756
Density	0.792	0.798	0.696	0.699	0.205	0.089	0.207	0.088
Proximity	–0.078	0.119	–0.076	0.096	–0.222	0.209	–0.222	0.209
Democratic vote share 2004	0.523	0.053	0.518	0.054	0.410	0.043	0.410	0.043
Democratic vote share 2008	0.574	0.053	0.570	0.052	0.449	0.050	0.449	0.050
Observations	631,399		3,674,311		368,432		1,959,983	

Notes: Summary statistics for wages and individual-level control variables are from 2005–2015 American Community Survey (ACS) Public Use Microdata Sample (PUMS) data. Climate data are from the Area Resource File (ARF) maintained by Quality Resource Systems under Health Resources and Services Administration. Data on population density are from the 2005–2009 ACS summary file, aggregated to the CZ level. County-level presidential vote tallies for the 2004 and 2008 presidential elections come from the Federal Election Commission and are aggregated to the CZ level. CB, collective bargaining; CZ, commuting zone; SD, standard deviation.

Furthermore, we standardize all of these variables to have a mean of 0 and a standard deviation of 1.

Table 2 provides the mean and standard deviation of the variables used in our analysis. We present separate summary statistics for public- and private-sector employees in states with and without mandatory CB laws. As Table 2 reveals, both public- and private-sector wages tend to be higher in CB-mandatory states. Furthermore, in both sets of states, public-sector workers tend to be older and have higher educational attainment than do private-sector workers. This outcome is consistent with previous findings in the literature. Finally, voters in CB-mandatory states appear to be much more likely to vote for the Democratic presidential candidate.

Empirical Framework

To examine how state CB laws affect public-sector wages, we begin by estimating models of the following form:

$$\ln(wage_{imst}) = \beta_0 + \beta_1 Pub_{imst} + \beta_2(Pub_{imst} * CB_s) + X_{imst}\alpha + \delta_m + \theta_s + \lambda_t + \varepsilon_{imst}, \tag{1}$$

where $wage_{imst}$ is the hourly wage of worker i in CZ m, state s, in year t; Pub_{imst} is an indicator variable that takes the value of unity if individual i works in the public sector; CB_s is an indicator for whether state s has a mandatory CB law for public-sector workers; X_{imst} is a vector of individual characteristics; δ_m, θ_s, and λ_t are CZ, state, and year fixed effects, respectively; and ε_{imst} is a random disturbance term. Because Equation (1) includes state fixed effects, the level effect of CB_s is omitted. In all of our specifications, we also include an indicator for whether state s had a RTW law in place in year t, and that variable interacted with the public-sector employee indicator and the CB-mandatory indicator.

The coefficient of primary interest in Equation (1) is β_2, the coefficient on the interaction between the public-sector employee indicator and the indicator for states with mandatory CB laws. Specifically, β_2 measures how the wage differential between public- and private-sector workers changes when public-sector workers are covered by a mandatory CB law. The inclusion of both state and CZ fixed effects implies that we are controlling for any state- or CZ-level, sector-invariant unobservables (i.e., unobservables that affect public- and private-sector wages in the same way) that are potentially correlated with our key variable of interest and thus might otherwise bias our estimates.[11]

The identifying assumption underlying Equation (1) is that the magnitude of the public–private wage gap in states without mandatory CB laws is identical to what it would be in states with mandatory CB laws, had those states not passed CB laws. This assumption will be violated if unobserved factors differentially affect the wages of otherwise similar public- and private-sector workers and are correlated with CB laws. For example, Brueckner and Neumark (2014) found that public- and private-sector wage differentials are larger in states with more desirable local amenities. Thus, if mandatory CB laws are correlated with amenity levels, estimates of β_2 will be biased unless one fully controls for differences in amenities across CB-mandatory and CB-non-mandatory states. Similarly, if public- and private-sector wage differentials are larger in more liberal states because those states grant more generous compensation packages to public-sector employees, estimates of β_2 will again be biased if CB laws are correlated with a state's political leanings.

To address these potential sources of bias, our preferred specification builds on Equation (1) by exploiting policy discontinuities at state borders to identify the effect of mandatory CB laws on public-sector wages.

[11]Note that because we focus on how state CB laws affect public- and private-sector wage differentials, we are essentially using a difference-in-differences (DD) identification strategy, in which the first difference is the average difference between otherwise similar public- and private-sector workers and the second difference is how that difference changes if a worker is located in a mandatory CB state.

Specifically, we focus on workers located in CZs that cross state boundaries and estimate models that replace the CZ fixed effects in Equation (1) with CZ-by-public-sector-worker fixed effects (δ_{m*Pub}):

$$(2)\qquad \ln(wage_{imst}) = \beta_0 + \beta_2(Pub_{imst}*CB_s) + X_{imst}\alpha + \delta_{m*Pub} + \theta_s + \lambda_t + \mu_{imst}.$$

The inclusion of state and CZ-by-public-sector-worker fixed effects in Equation (2) implies that we are now identifying the effect of mandatory CB laws on public-sector wages using only within local labor market (CZ) variation in the strength of state CB laws. That is, our model is now identified from CZs that cross state boundaries where one state in the CZ has a mandatory CB law whereas the other state in the same CZ does not. Furthermore, note that the inclusion of CZ-by-public-sector-worker fixed effects provides nonparametric controls for any unmeasured CZ-level factors that might differentially affect the earnings of otherwise similar private- and public-sector workers. Thus, the inclusion of these fixed effects controls for the differential effect any sector-invariant CZ-level observable or unobservable factor may have on public- and private-sector wage differentials. As a result, they fully control for all of the factors identified by Brueckner and Neumark (2014) and Diamond (2017) that differentially shift public- and private-sector wage differentials, namely desirable amenities and housing supply elasticities.

Furthermore, to provide additional evidence that our results are not being driven by unobservable factors that vary across states within the same CZ, we also estimate specifications for which we interact the public-sector employee indicator and the CB-mandatory indicator with several *state-specific* CZ-level factors, namely population density; proximity to the nearest coast, Great Lake, or major river; and the Democratic vote share in the 2004 and 2008 presidential elections.[12] We add these controls to further account for the possibility that the returns to these attributes may differ across the public and private sector or across CB-mandatory and non-mandatory states. Finally, to control for the possibility that the returns to individual characteristics (e.g., educational attainment and age) may also vary across the public and private sector or across states with and without mandatory CB laws, we estimate specifications in which we interact the individual characteristics in Equation (1) and Equation (2) with both the public-sector worker indicator and the indicator for states with a mandatory CB law.

We present estimates from Equations (1) and (2) based on two separate samples of workers. The first sample includes all state and local government workers and all private-sector workers; the second sample is limited to local government workers and all private-sector workers. Our rationale for providing separate results that focus solely on local government employees relates to our identification strategy. Recall that our primary identification strategy

[12]We do not include the other two amenity controls used by Brueckner and Neumark (2014), namely Mild and Dry, since these variables have almost no variation within CZs.

exploits policy discontinuities at state borders and compares public- and private-sector wage differentials in CZs that cross state borders where one state has a mandatory CB law and the other state does not. We do this to ensure we are comparing workers in the same labor market and workers who are exposed to very similar amenities, general political leanings, and other CZ attributes; characteristics that we show are balanced in Table 3. Since most CB agreements for local government employees are negotiated at the local level, the fact that characteristics of CZs that cross state boundaries are quite balanced provides increased confidence that our coefficient of primary interest, β_2, will not be biased by unobserved factors that are correlated with mandatory CB laws. By contrast, state employees have contracts that are negotiated at the state level, and as we show in Table 3, overall characteristics of states with and without mandatory CB laws differ greatly. We therefore have less confidence that we can identify the causal impact of CB laws on public- and private-sector wage differentials for state workers since those differentials may still be influenced by unobserved state-specific characteristics.

We also note that with freely mobile labor and the absence of differences in non-pecuniary working conditions or worker productivity, one might expect that any pay differential between public-sector workers in states with and without mandatory CB laws would be competed away, particularly when such policy discontinuities occur along state borders. However, there are several reasons why wages may not equalize. First, labor mobility between the unionized (CB-mandatory) and non-unionized (CB-non-mandatory) sectors is likely to be restricted because of the non-competitive features of union agreements that govern employment conditions and pay scales. Second, occupational licensing requirements that differ across states, non-portable pension benefits, and residency requirements for public-sector workers are likely to further impede labor mobility.[13]

In addition to exploiting policy discontinuities at state borders in state CB laws, we also examine variation *within* states in bargaining laws and consider states where the majority of public workers are without bargaining rights. Specifically, we exploit the fact that several of the CB-prohibited states extend CB rights to firefighters or police, or in some cases both. For example, Texas, which prohibits collective bargaining for most public-sector workers, grants CB rights to police and firefighters. Similarly, Georgia extends CB rights only to firefighters. Using this sample of states, we estimate models of the following form:

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[13]See, for example, Kim, Koedel, Ni, and Podgursky (2016) and Goldhaber, Grout, Holden, and Brown (2015) for evidence of significant barriers to cross-state teacher mobility, even along state borders. Also see Black, Kolesnikova, and Taylor (2014) for evidence that the labor force participation of women (a group overrepresented in the public sector) is highly sensitive to commute times, and Boyd, Lankford, Loeb, and Wyckoff (2005) for evidence that teachers have strong preferences to teach nearby to where they grew up.

Table 3. Balancing Tests

Variable	All CZ comparison		Within straddling CZ comparison	
	CB coefficient (1)	p value (2)	CB coefficient (3)	p value (4)
Voting, climate and 2005–2009 ACS Summary File variables				
Democratic vote share 2004	0.041***	0.000	0.022	0.188
Democratic vote share 2008	0.074***	0.000	0.015	0.515
Population density	24.47***	0.001	2.778	0.815
Mild climate	–6.76***	0.000	–0.005	0.981
Dry climate	1.529	0.150	1.194	0.136
Proximity to water	0.116	0.969	9.164	0.135
Mean household income	4,149***	0.000	–139.7	0.938
Fraction college educated	0.028***	0.000	–0.005	0.789
Log population	0.155	0.172	0.118	0.664
Observations	870		44	
2005–2015 PUMS ACS variables				
Age	0.657***	0.000	0.340	0.130
Fraction female	0.002	0.668	0.001	0.841
Fraction less than high school	–0.010	0.313	0.003	0.498
Fraction high school degree	–0.019	0.234	0.034	0.498
Fraction college degree	0.012	0.174	–0.022	0.527
Fraction advanced degree	0.017**	0.017	–0.015	0.438
Fraction married	–0.017**	0.017	–0.004	0.757
Fraction black	–0.060***	0.001	0.062	0.209
Fraction Asian	0.034***	0.002	–0.015	0.341
Fraction Hispanic	0.009	0.804	–0.007	0.627
Management and financial operations occupations	0.005	0.377	–0.020	0.405
Professional and related occupations	0.005	0.398	–0.009	0.540
Service occupations	0.017***	0.000	0.010	0.275
Sales and related occupations	–0.004**	0.021	0.005	0.411
Office and administrative support occupations	–0.002	0.487	–0.002	0.868
Farming, fishing, and forestry occupations	0.001	0.101	0.002	0.261
Construction and extraction occupations	–0.011***	0.000	0.007	0.240
Installation, maintenance, and repair occupations	–0.005***	0.000	0.003	0.590
Production occupations	–0.004	0.461	0.002	0.806
Transportation and material moving occupations	–0.002	0.249	0.003	0.756
Observations	6,634,125		388,394	

Notes: Presents differences in means tests for various commuting zone attributes. Estimates are from a regression of CZ attribute on indicator for a mandatory collective bargaining state. Columns (1) and (2) provide comparisons based on all CZs. Columns (3) and (4) include CZ fixed effects and restrict the sample to CZs that cross state boundaries and where the collective bargaining environment differs on either side of the state border. The 10 occupation categories are defined by the Bureau of Labor Statistics (http://www.bls.gov/cps/cenocc2010.pdf). Standard errors clustered at the commuting zone level. ACS, American Community Survey; CB, collective bargaining; CZ, commuting zone; PUMS, Public Use Microdata Sample.
***$p < 0.01$; **$p < 0.05$; *$p < 0.1$.

$$(3) \quad \ln(wage_{ist}) = \gamma_0 + \gamma_1 Pub\_Occ_{ist} + \gamma_2(Pub\_Occ_{ist} * CB_s) + X_{ist}\alpha + \theta_s + \lambda_t + \omega_{ist},$$

where Pub_Occ_{ist} is a set of indicator variables for police and firefighters, respectively, and $Pub\_Occ_{ist} * CB_s$ is a set of interaction terms between each of those indicators and an indicator for whether a state has a mandatory CB law for the given occupation.

Results

Balancing Tests

To provide initial evidence that estimates from our preferred specification given by Equation (2) have a causal interpretation, Table 3 presents difference-in-means tests for observable state-specific CZ characteristics among states that do and do not mandate collective bargaining. The top panel of Table 3 reports balancing tests for Democratic presidential vote shares, the four local amenities from Brueckner and Neumark (2014), and several characteristics generated using the 2005 to 2009 ACS summary files. The bottom panel reports balancing tests for observable characteristics taken from the 2005 to 2015 ACS PUMS. Columns (1) and (2) present estimated coefficients and p values from models in which we regress state-specific CZ-level characteristics on an indicator for whether public-sector employees in a CZ are covered by a mandatory CB law.[14] Note that the estimates reported in columns (1) and (2) utilize all of the variation across CZs in the listed characteristics. The results reveal significant differences in the characteristics of CZs located in CB-mandatory and non-mandatory states. For example, CZs located in CB-mandatory states have significantly higher mean household incomes, higher population density, and contain voters who are significantly more likely to support the Democratic presidential candidate.

Columns (3) and (4) of Table 3 present results from balancing tests that utilize only the identifying variation used in our preferred specification given by Equation (2), namely within local labor market (CZ) variation in the strength of state CB laws that originates from policy discontinuities at state borders. Specifically, we restrict the sample to CZs that cross state borders and where the bargaining environment on either side of the border differs. We then regress state-specific CZ-level characteristics on an indicator for whether public-sector employees in a CZ are covered by a mandatory CB law and also include CZ fixed effects. As a result, the estimates reported in columns (3) and (4) are now identified from CZs that cross state boundaries where one state in the CZ has a mandatory CB law whereas the other state in the same CZ does not. In columns (3) and (4), none of the estimated coefficients are significantly different from zero, and they tend to be smaller in magnitude than those reported in column (1). Thus, once we include CZ fixed effects, we find that the observable state-specific characteristics of CZs are quite balanced across states that do and do not mandate collective bargaining: a finding that increases our confidence in the identifying assumption underlying Equation (2).

[14]The reported p values in Table 3 are based on standard errors that are clustered at the CZ level.

Table 4. Estimated Effects of Mandatory CB Laws on Public-Sector Pay

Variable	*(1)*	*(2)*	*(3)*	*(4)*	*(5)*	*(6)*
	State and local government workers vs. Private-sector workers					
Public	–0.117***	–0.113***	–0.0974***			
	(0.00784)	(0.00899)	(0.0107)			
Public × CB	0.110***	0.103***	0.107***	0.0610***	0.0589***	0.0599***
	(0.0111)	(0.0141)	(0.0137)	(0.0135)	(0.0132)	(0.0129)
Observations	6,634,125	6,634,125	6,634,125	388,394	388,394	388,394
	Local government workers vs. Private-sector workers					
Public	–0.120***	–0.109***	–0.0910***			
	(0.00875)	(0.00964)	(0.0118)			
Public × CB	0.117***	0.0975***	0.103***	0.0616***	0.0628***	0.0605***
	(0.0123)	(0.0125)	(0.0134)	(0.0117)	(0.0135)	(0.0141)
Observations	6,235,000	6,235,000	6,235,000	370,079	370,079	370,079
CZ FE	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes
State-specific CZ controls	No	Yes	Yes	No	Yes	Yes
Individual interactions	No	No	Yes	No	No	Yes
CZ-by-Public FE	No	No	No	Yes	Yes	Yes

Notes: Data from the American Community Survey (ACS) 2005–2015. All specifications include the full set of individual-level controls, right-to-work (RTW) status, and year fixed effects. Top panel reports results for the combined sample of state and local government workers and all private-sector workers. Bottom panel limits the sample to local government workers and all private-sector workers. Columns (1)–(3) utilize all CZs, and columns (4)–(6) restrict the sample to CZs that cross state boundaries and where the collective bargaining environment differs on either side of the state border. Robust standard errors clustered at the state level in parentheses. CB, collective bargaining; CZ, commuting zone; FE, fixed effects.
***$p < 0.01$; **$p < 0.05$; *$p < 0.1$.

Main Results

Our benchmark regression results based on the estimation of Equations (1) and (2) are reported in Table 4.[15] All of the models are weighted using the person weight provided by the ACS multiplied by the fraction of the area of the individual's PUMA in the given CZ and the inverse probability of response. Furthermore, all standard errors are clustered at the state level to allow for within-state autocorrelation of the disturbance term. Note that although Table 4 reports only the estimated coefficient on the public-sector employee indicator, and the interaction between that variable and the mandatory CB indicator, all the specifications reported in Table 4 and subsequent tables include the full set of individual-level control variables, the

[15]Blackburn (2007) demonstrated that if the distribution of the error term is not independent of the regressors, standard OLS estimates from semi-log wage equations can lead to estimates of percentage wage gaps that are biased. To address that possibility, we also estimated models based on a generalized linear model (GLM) with a log-link. Results from this alternative estimation procedure were similar to those reported in Table 4 and are available upon request.

RTW indicator, and the RTW indicator interacted with the public-sector employee indicator and the CB-mandatory indicator.[16]

The top panel of Table 4 reports results for the full sample of state and local government workers and all private-sector workers whereas the bottom panel limits the sample to local government workers and all private-sector workers. Column (1) reports results based on a specification that includes state and CZ fixed effects. In both the top and bottom panel, the estimated coefficient on the Public $\times$ CB interaction is positive and statistically significant. In terms of magnitude, our results suggest mandatory CB laws increase public-sector wages by approximately 0.11 log points.[17]

We next present results based on specifications for which we interact the public employee indicator and the mandatory CB indicator with a number of state-specific CZ-level control variables, namely the four amenity variables used by Brueckner and Neumark (2014) and the fraction of voters within a CZ who supported the Democratic candidate for president.[18] As shown in column (2), the inclusion of these additional controls slightly attenuates the estimated coefficient on the Public $\times$ CB interaction, but it remains statistically significant at the 1% level. Column (3) includes additional controls that take the form of interactions between the individual-level controls (e.g., educational attainment and age) and the public-sector employee and CB-mandatory indicators. Again, the inclusion of these controls has only a modest impact on the results.

Results based on our preferred specification given by Equation (2) are presented in columns (4)–(6) of Table 4. There we report results based on specifications identical to those reported in columns (1)–(3) except we replace the CZ fixed effects with CZ-by-public-employee fixed effects and restrict the sample to workers located in CZs that cross state boundaries and where the bargaining environment on either side of the border differs. Restricting the sample to CZs that cross state boundaries and controlling for CZ-by-public-employee fixed effects reduces the magnitude of the estimated coefficients on the Public $\times$ CB interaction by approximately 40 to 50%, but they remain statistically significant in both the top and bottom panels. In our preferred specification that includes the full set of amenity and individual interactions (column (6)), our results now suggest that mandatory CB laws increase public-sector wages by approximately 0.06 log points.

Table 5 replicates the results reported in Table 4 but adds occupation fixed effects, based on the Standard Occupational Classification system

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[16]As noted previously, the full set of individual-level control variables include age, age squared, sex, years of educational attainment, four categories of marital status, and controls for Asian, black, and Hispanic.

[17]Because of concerns over the comparability of public- and private-sector workers, we refrain from interpreting the estimated coefficient on the public-sector worker indicator as showing whether public-sector workers are "overpaid" or "underpaid" relative to their private-sector counterparts.

[18]As noted previously, the four amenity variables include local temperature and weather controls (Mild and Dry), the average distance to the nearest coast (Proximity), and population density (Density).

Table 5. Estimated Effects of Mandatory CB Laws on Public-Sector Pay with Occupational Controls

Variable	*(1)*	*(2)*	*(3)*	*(4)*	*(5)*	*(6)*
	State and local government workers vs. Private-sector workers					
Public	–0.0428***	–0.0364***	–0.0386***			
	(0.00694)	(0.00845)	(0.00871)			
Public × CB	0.100***	0.0897***	0.0966***	0.0543***	0.0486***	0.0517***
	(0.0108)	(0.0137)	(0.0131)	(0.0116)	(0.0117)	(0.0115)
Observations	6,634,125	6,634,125	6,634,125	388,394	388,394	388,394
	Local government workers vs. Private-sector workers					
Public	–0.0225***	–0.0113	–0.00924			
	(0.00655)	(0.00730)	(0.00862)			
Public × CB	0.107***	0.0858***	0.0921***	0.0569***	0.0562***	0.0576***
	(0.00978)	(0.0108)	(0.0115)	(0.00842)	(0.00980)	(0.0110)
Observations	6,235,000	6,235,000	6,235,000	370,079	370,079	370,079
CZ FE	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes
State-specific CZ controls	No	Yes	Yes	No	Yes	Yes
Individual interactions	No	No	Yes	No	No	Yes
CZ-by-Public FE	No	No	No	Yes	Yes	Yes

Notes: Data from the American Community Survey (ACS) 2005–2015. All specifications include the full set of individual-level controls, 23 occupational controls defined by Current Population Survey (CPS), right-to-work (RTW) status, and year fixed effects. Top panel reports results for the combined sample of state and local government workers versus all private-sector workers. Bottom panel limits the sample to local government workers versus all private-sector workers. Columns (1)–(3) utilize all CZs, and columns (4)–(6) restrict the sample to CZs that cross state boundaries and where the collective bargaining environment differs on either side of the state border. Robust standard errors clustered at the state level in parentheses. CB, collective bargaining; CZ, commuting zone; FE, fixed effects.
$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$.

two-digit occupation codes, to all specifications.[19] In our preferred specifications that restrict the sample to workers located in CZs that cross state boundaries and include CB-by-public employee fixed effects (columns (4)–(6)), the estimated coefficients on the Public × CB interaction are slightly smaller than the corresponding estimates reported in Table 4, but all estimates remain statistically significant.

One potential concern with the results based on specifications that include CZ-by-public-sector-employee fixed effects (our preferred specification), is that we are identifying the effect CB laws have on wages based on the relatively small number of CZs that cross state boundaries, where one state in the CZ has a mandatory CB law and the other does not. Specifically, as shown in Table A.1 of the Appendix, our results are based on 21 CZs that

[19]We also estimated models that, in addition to the occupational controls, included a separate indicator for teachers to account for the fact teachers tend to earn substantially less than other college graduates. Adding a dummy variable for teachers yielded results that were similar to those reported in Table 5. Results are available upon request.

Table 6. Estimated Effects of Mandatory CB Laws on Public-Sector Pay: PUMAs Located within 20 Miles of State Borders

Variable	*(1)*	*(2)*	*(3)*	*(4)*	*(5)*	*(6)*
	State and local government workers vs. Private-sector workers					
Public	–0.0221*	–0.0197*	–0.0210**			
	(0.0112)	(0.0101)	(0.00980)			
Public × CB	0.0909***	0.0728***	0.0807***	0.0611***	0.0560***	0.0644***
	(0.0212)	(0.0192)	(0.0182)	(0.0150)	(0.0149)	(0.0157)
Observations	1,908,487	1,908,487	1,908,487	360,728	360,728	360,728
R-squared	0.478	0.478	0.479	0.468	0.469	0.470
	Local government workers vs. Private-sector workers					
Public	–0.00344	–0.00172	0.00166			
	(0.0101)	(0.00830)	(0.00763)			
Public × CB	0.0886***	0.0665***	0.0727***	0.0603***	0.0521***	0.0602***
	(0.0191)	(0.0170)	(0.0154)	(0.00726)	(0.0127)	(0.0141)
Observations	1,808,689	1,808,689	1,808,689	341,596	341,596	341,596
R-squared	0.480	0.480	0.481	0.471	0.472	0.472
PUMA FE	Yes	Yes	Yes	Yes	Yes	Yes
PUMA amenities	No	Yes	Yes	No	Yes	Yes
Individual interactions	No	No	Yes	No	No	Yes
Border-by-Public FE	No	No	No	Yes	Yes	Yes

Notes: Data from the American Community Survey (ACS) 2005–2015. All specifications include the full set of individual-level controls, 23 occupational controls defined by Current Population Survey (CPS), right-to-work (RTW) status, and year fixed effects. Top panel reports results for the combined sample of state and local government workers and all private-sector workers. Bottom panel limits the sample to local government workers and all private-sector workers. Columns (1)–(3) utilize all PUMAs located within 20 miles of a state border, and columns (4)–(6) restrict the sample to PUMAs located within 20 miles of a state border and where the collective bargaining environment differs on either side of the state border. Robust standard errors clustered at the state level in parentheses. CB, collective bargaining; FE, fixed effects; PUMA, Public Use Microdata Area.
***$p < 0.01$; **$p < 0.05$; *$p < 0.1$.

cross state boundaries and have bargaining environments that vary. Although this apparent limitation has no impact on the validity of our analysis, it may affect generalizability.

We therefore present further evidence on the generalizability of our results by conducting similar analyses to those reported in Table 5 (models with occupation fixed effects) except we now utilize all the PUMAs that are located within 20 miles of a state border and where the states along the border have CB environments that differ. Results are reported in Table 6.[20] Column (1) reports results based on specifications that include PUMA fixed effects, column (2) adds the same amenity interactions used in column (2) of Table 4 except the amenities are now measured at the PUMA level, and column (3) adds the full set of interactions between the individual

[20]We also conducted balancing tests, like those reported in Table 3, for the PUMA sample. The results are quite similar to those reported in Table 3 and are available upon request.

characteristics and the public-sector worker indicator and the CB-mandatory indicator. Finally, columns (4)–(6) replicate the specifications in columns (1)–(3) but restrict the sample to state borders where the bargaining environment differs on either side of the border and add border-by-public fixed effects so that our estimates are identified based on PUMAs in which the bargaining environment differs on either side of a state border. The standard errors reported in Table 6 are once again clustered at the state level.

The results reported in Table 6 are generally quite similar to those reported in Table 5, which speaks to the generalizability of our results. Specifically, in our preferred specifications reported in columns (4)–(6), all of the point estimates on the Public × CB interaction are statistically significant and similar in magnitude to those reported in Table 5.

Teachers, Police, and Firefighters

In this section, we examine whether the results shown in Tables 4 and 5 hold for workers in specific occupations, namely teachers, police, and firefighters. Specifically, in Table 7 we estimate specifications identical to those in Table 4 except we use detailed occupation-specific bargaining laws and include only government workers employed in the given occupation, comparing their wages to the overall sample of private-sector workers.[21]

Results based on our preferred specification that includes CZ-by-public-sector-employee fixed effects and the full set of individual and CZ interaction terms are reported in columns (2), (5), (8), and (11) for teachers, police, firefighters, and all other local public-sector employees, respectively. In every case we find that the estimated coefficient on the Public × CB interaction is positive and statistically significant, with the exception of firefighters. We note, however, that even for firefighters the point estimate on the Public × CB interaction is relatively large in magnitude and similar to the estimates for other public-sector occupations but is noisily estimated. That the estimate for firefighters is noisy is not surprising given the significantly smaller sample sizes for firefighters. In terms of magnitude, our results suggest that mandatory CB laws increase the wages of teachers by approximately 0.10 log points and police, firefighters, and all other local workers by 0.12, 0.08, and 0.04 log points, respectively. Using a DD identification strategy that exploits the plausibly exogenous timing of when states adopted mandatory CB laws, Frandsen (2016) found that mandatory CB laws increase the wages of police and firefighters by 0.075 and 0.129 log points, respectively. Our estimates for police and firefighters are generally comparable to his estimates.[22] Frandsen (2016) also found, however, that

[21] In Table 7 and all other tables in which we focus on specific occupations, we do not include occupation fixed effects.

[22] Based on a *t*-test, we cannot reject the null hypothesis that the estimates obtained by Frandsen (2016) are the same as the estimates reported in Table 7 for police and firefighters.

Table 7. K-12 Teachers, Police, Firefighters, Other Local Public Employees

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Variable	*K-12 teachers*			*Police*			*Firefighters*			*Other local public employees*	
Public	–0.199***			0.0374			–0.0353			–0.101***	
	(0.0215)			(0.0275)			(0.0272)			(0.0109)	
Public × CB	0.0771***	0.0900***	0.110***	0.164***	0.121***	0.117***	0.137***	0.0664	0.0793	0.102***	0.0388***
	(0.0258)	(0.0290)	(0.0203)	(0.0351)	(0.0198)	(0.0328)	(0.0444)	(0.0393)	(0.0478)	(0.0159)	(0.0109)
Observations	5,827,910	348,019	348,019	5,678,687	339,113	339,113	5,650,218	337,715	337,715	6,050,505	359,114
CZ FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual interactions and CZ controls	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes	No	Yes
CZ-by-Public FE	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes	No	Yes
Quality of work environment	No	No	Yes	No	No	Yes	No	No	Yes	No	No

Notes: Data from the American Community Survey (ACS) 2005–2015. All specifications include the full set of individual-level controls, right-to-work (RTW) status, and year fixed effects. Columns (1)–(3) restrict the sample of government workers to K-12 teachers, and columns (4)–(6), (7)–(9), and (10)–(11) restrict the sample of government workers to police, firefighters, and all other local government workers, respectively. Robust standard errors clustered at the state level in parentheses. CB, collective bargaining; CZ, commuting zone; FE, fixed effects.

***$p < 0.01$; **$p < 0.05$; *$p < 0.1$.

mandatory CB laws have little effect on the wages of teachers, a result that stands in contrast to our finding that such laws increase the wages of teachers by approximately 0.10 log points.[23]

The results reported in Tables 4 to 7 consistently suggest that mandatory CB laws increase public-sector wages. Several possible explanations for this finding exist. First, observed wage differentials could arise because of unobserved productivity or skill differences between public- and private-sector workers in CB-mandatory and non-mandatory states. Specifically, one possible explanation for our results is that compared to CB-non-mandatory states, CB-mandatory states employ public-sector workers who are relatively more skilled than their private-sector counterparts. Second, our results may reflect compensating differentials in pay for nonpecuniary job attributes that vary across CB environments. For example, pupil–teacher ratios (class size) may vary across CB environments and teachers in states with higher pupil–teacher ratios may earn higher wages to compensate them for having larger classes.[24] The final explanation is rent extraction: public-sector employees in CB-mandatory states may be better situated to extract rents than are public-sector employees in CB-non-mandatory states.

In terms of the first explanation (productivity or skill differences), we note that in our preferred specifications we include interactions between individual-level observable measures of skill, such as educational attainment and age (a proxy for experience), and both the public-sector worker indicator and the CB-mandatory indicator. Thus, our fully interacted specifications control for observable measures of skill that vary across the public and private sector and across CB-mandatory and non-mandatory states. Although we cannot completely rule out the presence of other unobservable skill differences between public-sector workers in CB-mandatory and CB-non-mandatory states, the fact that our results are highly robust to the inclusion of these additional interactions casts doubt on whether skill differentials fully explain our findings.

As an initial test of the second explanation (compensating differentials), in columns (3), (6), and (9) of Table 7 we add additional covariates designed to capture the quality of the public-sector work environment. Specifically, in column (3), we add the state-specific pupil–teacher ratio in a CZ and that variable interacted with both the indicator for public-sector teachers and the indicator for CB-mandatory states to the specification

[23]Our results are also at odds with those obtained by Lovenheim (2009). Using historical data on the timing of teachers' union election certification for school districts in three Midwestern states, he found that unionization had little effect on teacher salaries. By contrast, our results are consistent with the results of Winters (2011), who found that collective bargaining significantly increases the salaries of experienced teachers.

[24]This variance could arise, for example, if there is a trade-off between wages and employment and teachers in CB-mandatory states bargain for higher wages in exchange for lower employment leading to higher pupil–teacher ratios.

Table 8. Estimated Effects of Mandatory CB Laws on Public-Sector Pay: States without Public-Sector CB Rights

	(1)	(2)	(3)	(4)
Variable	*Comparison vs. Public-sector workers*		*Comparison vs. Private-sector workers*	
Firefighter	0.0155	0.0165	–0.1407***	–0.139***
Robust SE	(0.0279)	(0.0280)	(0.0262)	(0.0256)
Confidence interval	[–0.277, 0.063]	[–0.026, 0.062]	[–0.181, –0.100]	[–0.177, –0.101]
Firefighter × CB	0.139***	0.137***	0.1117***	0.110***
Robust SE	(0.0284)	(0.0291)	(0.0263)	(0.0263)
Confidence interval	[0.094, 0.184]	[0.091, 0.183]	[0.071, 0.152]	[0.070, 0.150]
Police	0.0925**	0.0927***	–0.0802***	–0.0798***
Robust SE	(0.0212)	(0.0212)	(0.0178)	(0.0177)
Confidence interval	[0.057, 0.133]	[0.0593, 0.129]	[–0.112, –0.050]	[–0.112, –0.048]
Police × CB	0.157***	0.156***	0.145***	0.144***
Robust SE	(0.0315)	(0.0315)	(0.0369)	(0.0371)
Confidence interval	[0.093, 0.214]	[0.095, 0.214]	[0.077, 0.207]	[0.083, 0.212]
Observations	285,464	285,464	1,628,585	1,628,585
State year FE	Yes	Yes	Yes	Yes
State-by-Year FE	No	Yes	No	Yes

Notes: Sample restricted to states without collective bargaining rights for most government workers. All specifications include the full set of individual-level controls and year fixed effects. Columns (1) and (2) compare firefighters and police to all other local government workers. Columns (3) and (4) compare firefighters and police to all private-sector workers. Robust standard errors clustered at the state level in parentheses. Wild-clustered bootstrap confidence intervals in brackets. CB, collective bargaining; FE, fixed effects; SE, standard error.
***$p < 0.01$; **$p < 0.05$; *$p < 0.1$.

reported in column (2).[25] The inclusion of these additional covariates has little impact on our results: The estimates reported in column (3) are quite similar to those reported in column (2). In columns (6) and (9), respectively, we add state-specific measures of the number of police officers per capita and the number of firefighters per capita in a CZ and those variables interacted with both the indicators for police officers and firefighters and the indicator for CB-mandatory states. Once again, we find that our results are largely unaffected by the inclusion of these additional covariates. We interpret these results as suggesting that compensating differentials are likely not the primary explanation of our core findings. Given we find little evidence that our results are being driven by skill differences or compensating differentials, we conclude that the most plausible explanation for our results is that public-sector employees with strong bargaining rights receive rents.

[25]Brueckner and Neumark (2014) conducted a similar test in their analysis of the relationship between local amenities and public- and private-sector wage differentials. To construct CZ-level estimates of the pupil–teacher ratio we used data from the National Center for Education Statistics (NCES) from 2005–2010 on the number of teachers and number of pupils in each school district within a CZ. We then aggregated those to the CZ (or CZ-by-state) level to construct the CZ measure of the pupil–teacher ratio.

Estimates Based on States that Restrict CB Rights

In Table 8 we turn to results based on Equation (3), which exploits variation within states in CB laws. Specifically, we focus on states that generally prohibit collective bargaining and exploit the fact that several of those states allow collective bargaining for firefighters or police, or in some cases both.[26] We then estimate two sets of models. In the first set, we restrict the sample to public-sector workers and estimate models that compare the wages of police and firefighters to those of all other public-sector workers in states that do and do not authorize police and firefighters to bargain. In the second set, we restrict the sample to police, firefighters, and all private-sector workers and estimate models that now compare the wages of police and firefighters to those of private-sector workers in states that do and do not authorize police and firefighters to bargain.

Columns (1) and (2) of Table 8 report results for a comparison group comprising all government workers other than police and firefighters, and columns (3) and (4) report results for a comparison group comprising private-sector workers. We report only the estimated coefficients on the police and firefighter indicators and those indicators interacted with the CB-authorized indicator but note that all specifications include the full set of individual-level control variables. Because the number of states in our sample is small (eight states), conventional clustering methods are likely to produce standard errors that are too small (Cameron and Miller 2015). As demonstrated by Cameron, Gelbach, and Miller (2008), however, the wild clustered bootstrap performs quite well even with as few as six clusters. We therefore report both traditional standard errors clustered at the state level and wild clustered bootstrap confidence intervals.

Turning first to the results reported in column (1), we find that the ability to collectively bargain increases the wages of firefighters by approximately 0.14 log points, an estimate that is statistically significant at the 1% level. As shown in the bottom rows of Table 8, we find similar results for police. In column (2) we replace state and year fixed effects with state-by-year fixed effects to control for intertemporal differences across states in wage gaps. The inclusion of these state-by-year fixed effects has little effect on our results. As shown in columns (3) and (4), we find qualitatively similar results when we switch the comparison group to private-sector workers rather than all other public-sector workers. Specifically, the ability to collectively bargain increases the wages of firefighters by approximately 0.11 log points and the wages of police by approximately 0.14 log points.

[26]The states are Alabama, Arizona, Georgia, Mississippi, North Carolina, South Carolina, Texas, and Virginia. In addition to being states that typically prohibit collective bargaining, all of these states are also right-to-work states. Among these states, Georgia extends CB rights to firefighters but no other public-sector workers; Arizona and Texas extend CB rights to both firefighters and police but no other public-sector workers.

Table 9. Falsification Tests

Variable	*(1)*	*(2)*	*(3)*	*(4)*	*(5)*	*(6)*
	Federal government workers vs. Private-sector workers					
Public	0.191***	0.188***	0.190***			
	(0.0106)	(0.0106)	(0.00963)			
Federal worker × CB	–0.0121	–0.00641	–0.00515	0.00510	–0.00584	0.00478
	(0.0168)	(0.0154)	(0.0136)	(0.0151)	(0.0169)	(0.0182)
Observations	5,845,456	5,845,456	5,845,456	363,515	363,515	363,515
	Nonprofit workers vs. All other private-sector workers					
Public	–0.0601***	–0.0595***	–0.0699***			
	(0.00474)	(0.00555)	(0.00668)			
Nonprofit worker × CB	0.0183***	0.0184***	0.0145*	–0.00338	0.000492	0.00127
	(0.00629)	(0.00667)	(0.00736)	(0.00407)	(0.00375)	(0.00743)
Observations	5,634,294	5,634,294	5,634,294	336,810	336,810	336,810
CZ FE	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes
State-specific CZ controls	No	Yes	Yes	No	Yes	Yes
Individual interactions	No	No	Yes	No	No	Yes
CZ-by-Public FE	No	No	No	Yes	Yes	Yes

Notes: Top panel presents falsification tests that compare federal workers to all private-sector workers; bottom panel compares nonprofit workers to all other private-sector workers. All specifications include the full set of individual-level controls, 23 occupational controls defined by Current Population Survey (CPS), right-to-work (RTW) status, and year fixed effects. Columns (1)–(3) utilize all CZs, and columns (4)–(6) restrict the sample to CZs that cross state boundaries and where the collective bargaining environment differs on either side of the state border. Robust standard errors clustered at the state level in parentheses. CB, collective bargaining; CZ, commuting zone; FE, fixed effects.
***$p < 0.01$; **$p < 0.05$; *$p < 0.1$.

Falsification Test

In this section, we report results based on a series of falsification tests in which we examine whether mandatory state CB laws also affect the wages of federal or nonprofit workers. Non-postal federal employees are on a federal government defined pay schedule called General Schedule (GS).[27] The schedule is based solely on the level of experience, education, and the position applied for, which do not reflect state or local laws that govern the bargaining environment. Similarly, nonprofit workers are in many ways quite similar to public-sector workers, with the exception that they are not covered by mandatory state CB laws.[28] Therefore, if our previous results have a causal interpretation we should find that state CB laws have little impact on the wages of either federal or nonprofit workers.[29]

[27] Non-managerial postal (USPS) workers utilize a different pay scale and can bargain over wages. We therefore drop federal postal workers from the sample in the falsification tests.

[28] See Hirsch, Macpherson, and Preston (2017) for evidence on the similarities between nonprofit and public-sector workers.

[29] Brueckner and Neumark (2014) and Diamond (2017) also used federal workers as a falsification test in their studies.

Table 9 reports results from our falsification tests. The specifications reported in Table 9 are identical to those reported in Table 5 except that in the top panel of Table 9 the sample is now restricted to federal employees and all private-sector workers, and in the bottom panel the sample is restricted to nonprofit employees and all other private-sector workers. In all six columns the point estimate on the Federal worker × CB interaction is small in magnitude and statistically insignificant. In the bottom panel, the point estimate on the Nonprofit worker × CB interaction is positive and statistically significant in the first three columns but small in magnitude relative to the results reported in Table 5. More important, in our preferred specifications that include CZ-by-nonprofit worker fixed effects (columns (4)–(6)), the estimated coefficient on the Nonprofit worker × CB interaction is quite small in magnitude and statistically insignificant. Thus, our falsification tests reveal little evidence that mandatory CB laws increase the wages of either federal or nonprofit workers, providing further evidence that our core results have a causal interpretation.

Conclusion

In recent years, a number of states have attempted to pass legislation (sometimes successfully) that would substantially curtail or eliminate the CB rights of public-sector workers. In the midst of the worst economic crisis since the Great Depression, proponents of limiting public-sector employee bargaining rights have argued that public-sector workers are overpaid and that mandatory CB laws grant public-sector unions additional negotiation power through political influence that could potentially distort the labor market and have detrimental impacts on government finances. In this article, we provide new evidence on whether and how mandatory CB laws affect public-sector wages.

Using a variety of identification strategies, we find that mandatory CB laws raise public-sector wages by approximately 0.05 to 0.08 log points. Drilling down to specific occupations, we find that mandatory CB laws increase the wages of teachers, firefighters, police, and all other local public workers. Furthermore, we exploit variation *within* states in CB laws and consider states where the majority of public-sector workers are without bargaining rights. Within these states, we estimate DD models that compare the public- and private-sector wage differential in states that authorize police and firefighters to bargain to the public- and private-sector wage differential in states that prohibit police and firefighters from bargaining. Once again we find that, even in states that generally prohibit collective bargaining, the extension of bargaining rights to certain occupations, namely police and firefighters, tends to increase the wages of workers in those occupations.

Our finding that mandatory CB laws increase public employee wages is consistent with the majority of the existing literature. Indeed, our estimates of the effect of collective bargaining on the wages of firefighters and police are quite comparable to Frandsen's (2016) DD estimates that exploit the historical timing of when states adopted mandatory CB laws. We note, however, that our finding that mandatory CB laws increase the wages of teachers stands at odds with the results of Frandsen (2016) and Lovenheim (2009), who found that collective bargaining and teacher unionization have little effect on teacher salaries. Among the many potential explanations for why our results differ from these prior studies, one could be related to the time period being studied. Specifically, both Frandsen (2016) and Lovenheim (2009) focused on the historical timing of when states adopted mandatory CB laws or when teachers within districts first became unionized to identify the effects of CB laws and unions on teacher salaries. Thus, their analyses are based primarily on changes in CB laws and unionization that occurred during the 1960s and 1970s. By contrast, we exploit contemporaneous variation across state borders within local labor markets to isolate the causal effect of CB laws on teacher salaries and utilize very recent (2005–2015) data on teacher salaries. Nevertheless, given the mixed evidence in the literature on the effect of mandatory CB laws on teacher compensation, more research on this topic is clearly needed.

As noted previously, recent articles by Brueckner and Neumark (2014) and Diamond (2017) provided evidence that mandatory CB laws provide a formal mechanism through which public-sector workers can extract rents in areas with low housing supply elasticities or high levels of desirable amenities. In this article, we show that even after controlling for the effects of housing supply elasticities and amenities, mandatory CB laws appear to have a direct effect on public-sector wages.

An important caveat to our work is that we focus solely on the impact of mandatory CB laws on wages and not total compensation. To the extent that such laws also lead to higher total compensation in the form of increased retirement and health benefits, our results may be a lower bound on the overall effect of mandatory collective bargaining on public-sector compensation. In fact, recent evidence suggests this may indeed be the case. Diamond (2017) found that a decrease in the housing supply elasticity increased the probability that local public-sector workers receive some employer contribution toward health insurance premiums, but only in states that mandate collective bargaining. Similarly, using an identification strategy that exploited the timing of state adoption of CB laws, Frandsen and Webb (forthcoming) found that mandatory CB laws significantly increase government contributions to pensions while simultaneously reducing employee contributions. Finally, using data from the 1999–2000 Schools and Staffing Survey (SASS), Hirsch et al. (2012) found that state CB laws increase average benefits by approximately 0.2 log points, an effect that is roughly twice as large as the effect of mandatory CB laws on average salaries.

Appendix

Table A.1. Commuting Zones Crossing State Boundaries

1990 CZ ID	CZ name	Public- and private-sector workers			Public-sector workers		
		CB law	No CB law	Total	CB law	No CB law	Total
		(1)	(2)	(3)	(4)	(5)	(6)
7600	Jacksonville, FL	28,424	1,400	29,824	3,491	285	3,776
11304	Arlington, VA	39,692	51,440	91,132	5,966	6,294	12,260
12701	Cincinnati, OH	40,161	9,286	49,447	4,073	1,104	5,177
13101	Louisville, KY	4,903	23,985	28,888	513	2,660	3,173
15300	Parkersburg, WV	2,434	3,186	5,620	416	541	957
15600	Wheeling City, WV	3,018	3,497	6,515	499	527	1,026
15800	Athens City, OH	1,932	1,327	3,259	494	207	701
17501	Cumberland, MD	3,370	1,951	5,321	762	382	1,144
23600	Burlington, IA	3,557	1,330	4,887	548	256	804
24701	St. Louis, MO	5,283	48,249	53,532	754	4,431	5,185
26404	Lemmon, SD	911	1,546	2,457	133	269	402
26704	Grand Forks, ND	2,643	1,005	3,648	461	100	561
26801	Fargo, ND	1,934	3,453	5,387	311	373	684
29502	Kansas City, MO	22,083	27,627	49,710	2,783	3,243	6,026
29901	Joplin, MO	3,121	4,277	7,398	618	496	1,114
30601	El Paso, TX	2,548	12,080	14,628	731	2,620	3,351
35300	Farmington, NM	1,878	919	2,797	358	206	564
35802	Ontario, OR	1,425	1,637	3,062	349	344	693
38100	Yuma, AZ	2,135	2,627	4,762	654	577	1,231
38402	Pullman, WA	3,079	2,631	5,710	1,080	682	1,762
38601	Spokane, WA	10,258	878	11,136	1,572	147	1,719
Total		184,789	204,331	389,120	26,566	25,744	52,310

Notes: Table lists commuting zones (CZs) that cross state boundaries and one state in the CZ mandates collective bargaining (CB) and the other state does not. Columns (1)–(3) show sample sizes by CB law for public- and private-sector workers. Columns (4)–(6) provide the same information for only public-sector workers.

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