State Collective Bargaining Laws and the Distribution of Public Sector Wages

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December, 2017

Abstract

This paper examines how state collective bargaining laws affect the distribution of public sector wages. While a large and active body of literature examines the effects of collective bargaining rights on the average wages of public sector workers, few studies have examined the impact of such rights on the distribution wages. Using data from the Public Use Microdata Sample (PUMS) of the 2005 to 2015 American Community Survey (ACS) and recently developed unconditional quantile regression techniques, I make two important contributions to the literature. First, I show that while the mean earnings estimates in this study are similar to the estimates found in earlier studies, those estimates mask substantial heterogeneity in the effects of collective bargaining rights across the wage distribution. Second, my results suggest that collective bargaining rights generally compress the wage distribution among public sector workers. The once exception is public sector teachers, for whom I find that collective bargaining rights actually increase the dispersion of wages.

JEL Codes: J31, J52, C31

Keywords: Collective Bargaining, Wage Distribution, Quantile Regressions

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1. Introduction

The passage of Wisconsin's Act 10, which effectively stripped most state and local employees of any meaningful collective bargaining (CB) rights, has brought national attention to the topic of public sector collective bargaining and reinvigorated academic interest in how state collective bargaining laws affect public sector compensation. Prior studies have tended to focus on how CB laws and public-sector unionization affect the average earnings of public sector workers and the evidence from that literature tends to be mixed. Zax and Ichniowski (1990), Lewis (1990), and Hoxby (1996) find a substantial union wage premium in the public sector. Lovenheim (2009), on the other hand, finds little evidence that teacher unions significantly effect wages, while Frandsen (2016) and Diamond (2016) find that favorable CB laws raise the earnings of police and firefighters, but have little effect on the wages of teachers. While examining how unionization and CB laws affect the average public sector wages is important, for a policy perspective, it is equally important to understand how CB laws reward members differently at various points of the wage distribution. In this paper, I examine the change in the wage distribution among public sector workers elicited by the enactment of CB legislation that enhances union power.

In the private sector, one popular argument by unionization proponents is that unions reduce overall income inequality. Unions support the standardization of compensation, as they generally seek to negotiate higher earnings for low- and middle-wage workers, and for workers with lower levels of education (Walters & Mishel, 2003). Indeed, a large body of empirical research supports the view that unionization in the private sector compresses the wage distribution among unionized and nonunionized workers. Freeman (1980) carried out a separate analysis across and within establishments and concluded that wage policies adopted by unions significantly reduce wage dispersion. Using survey data from the U.S., U.K., and Canada, DiNardo et al. (1996) implemented a re-weighting method to construct counterfactual wage densities and showed that the decline in the union membership between the late 1970s and early 1990s played an important role increasing the wage inequality. Furthermore, Card (2001) also provided similar evidence that the discrepant trends in union membership for private sector workers explains more than half of the faster growth in wage inequality compared to public sector workers.[1] More recently, Schmitt (2008), using quantile regression analysis, reported that a unionized worker at the bottom of the income distribution earns approximately 21% more than his otherwise similar counterparts, while unionized workers at the top of the distribution earn only 6% more.

[1] Between 1977 and 1992, the union coverage rate in the private sector declined from 21.7% to 11.3%, while the union coverage rate in the public sector rose from 33.4% to 36.6% over the same period. Please refer to Hirsch and MacPherson (1993).

Although there is well-documented literature on how unionization impacts wage inequality among private sector workers, there is a relatively little evidence regarding the role of unions and their bargaining power on the distribution of public sector wages. What evidence is available comes primarily from studies that examine the effect of public sector bargaining laws on the distribution of teacher wages. Using cross-sectional survey data from National Center for Education Statistics, Han (2013) found that a legal bargaining environment between teachers' unions and school districts is associated with much greater variance of earnings, which is potentially driven by the likelihood of credential-based pay structure, incorporated in CB agreements. More recently, Litten (2016) examined plausibly exogenous variation induced by contract renewal dates in Wisconsin and demonstrated that teachers' unions have a compensation dispersal rather than compression effect. Although the existing research indicates that unions and their collective bargaining agreements adversely affect teachers at the lower end of the distribution by dispersing the wages, no existing research addresses this issue for public sector workers in other professions.

To identify the distributional effect of state CB laws on public sector workers' earnings, this study implements the unconditional quantile regression (UQR) method (Firpo, Fortin, & Lemieux, 2009) coupled with an identification strategy that exploits policy discontinuities along the state borders, a methodology popularized by Holmes (1998) and Black (1999). Specifically, using data from the Public Use Microdata Sample (PUMS) of the 2005 to 2015 American Community Survey (ACS), I focus on workers in public use microdata areas (PUMAs) with centroids located within 30 miles from nearest state border. Furthermore, I add border fixed effects, which essentially utilizes within-border variation in the strength of state CB laws and thus control for a host of unobservable factors that might potentially be correlated with both public sector CB rights and the wage distribution.

Distributional analysis using quantile regression is important in its own right, as it is generally seen as having two major advantages over OLS regressions. First, as opposed to OLS regressions, quantile regressions are insensitive to outliers and produce reliable estimates even in the presence of extreme outliers (Davino, Furno, & Vistocco 2014). Second, and most importantly, quantile regressions allow one to examine how a particular policy affects the entire distribution of wages, rather than simply the mean level of wages. Understanding the distributional effect of a policy is important in its own right, as it offers insights into which parts in the wage distribution are more likely rewarded. For example, a policy that reduces inequality may be socially desirable, even if there is a zero or even negative mean impact. One of the several claims that proponents for collective bargaining rights make is that an increase in union power results in wage compression, thus reducing the wage inequality among workers. This is consistent with the idea that unions often implement wage standardization policies to tackle wage discrimination among all workers that they represent (Bryson, 2014). Conversely, unionization critics argue that unions favor credential-based pay (rather than performance-based pay), which rewards experienced workers at the higher end of the wage distribution. For example, the presence of CB agreements may have greater returns to professional development, such as getting a master's degree. As a result, workers in the lower parts of the wage distribution may end up with minimal pay raises or, in some cases, reductions.

I find first that the mean earnings estimates in this study are similar to those found in the previous literature. More importantly, however, I find that the average earnings estimates obtained from OLS regressions masks substantial heterogeneity in the effects of CB rights across the wage distribution. Second, my analysis reveals that CB rights generally compress the wage distribution among public sector workers. For teachers, however, the evidence from UQR analysis suggests that granting collective bargaining rights may be related to an increase in wage inequality among teachers. These findings are consistent with and expand earlier studies on distributional impacts of CB rights indicating a wage decompression effect among teachers (Han, 2013; Litten, 2016).

To furthermore mitigate concerns related to the potential endogeneity of CB laws, I show that my results for teachers are robust to an alternative identification strategy that exploits differences across states in the timing of the enactment of CB laws between the early 1960s and late 1970s. Specifically, I implement UQR within a difference-in-differences framework (RIF-DiD) to examine heterogeneity in the impact of CB rights on the distribution of teachers' earnings. The RIF-DiD results are qualitatively similar to my baseline estimates, suggesting that CB laws are indeed associated with a strong wage dispersal effect for teachers.

The remainder of the paper is organized as follows. Section 2 discusses the institutional background on CB, sector 3 presents the literature review, section 4 describes the data, section 5 discusses UQR and the empirical strategy, section 6 provides the results, and section 7 concludes.

2. Institutional Background

For private sector workers, unionization and collective bargaining gradually expanded following the National Labors Relations Act of 1935, and eventually peaked in the 1950s. The legislation protected the rights of private workers to collectively bargain by curtailing certain private sector labor and management practices that may potentially harm or cut down the general welfare of workers. Even with such an influential legislation, the share of the U.S. labor force belonging to a union has steadily decreased over the past 60 years, reaching 6.4% in 2016 (BLS, 2017).

During the same time period, the legal environment for public sector CB started settling in.[2] Beginning with Wisconsin in the late 1950s, a considerable number of states started granting CB rights, as shown in Table 1. The CB legislations were imposing a "duty-to-bargain" requirement on state and local governments, which obligated them to bargain in good faith if employees presented themselves with a union. Such legislations, however, do not require an agreement to be reached. The laws typically carry clauses on compulsory interest arbitration and, often, right-to-strike provisions to resolve impasses in the bargaining process. As a result of the subsequent expansion of CB rights, only 2% of the state and local workers in 1960 had the right to bargain collectively, but that share had grown to 63% by 2010 (Keefe 2015). These state-level legislations were also followed by public sector employees forming unions in greater numbers, accompanied by a dramatic rise in union membership rate from 12% in 1956 to approximately 34% in 1984 (Freeman, 1986). Alternatively, other states did not pass such legislations, or in some instances enacted an anti-union legislation, strictly prohibiting CB overall. In the modern era, 31 states allow public employees to bargaining collectively, 11 do not have legislations on CB, and eight generally bar public workers from bargaining collectively.

Adopting a duty-to-bargain rule by the state government may be followed by an alteration of the wage distribution among public sector workers. Opponents of public sector collective bargaining argue that the bargaining process is fundamentally different in the public than in the private sector. The core mission of public sector unions is to promote the interest of existing members, especially those with the most experience. As a result, unions negotiate for greater compensation and job security for workers in the top end of the wage distribution, forcing state and local governments to cut costs elsewhere. For example, Han (2016) suggested that teacher unions tend to negotiate for higher wages while raising the dismissal rate of low-quality teachers. Under such circumstances, public sector CB rights may decompress the wage distribution among workers.

On the other hand, public sector union advocates claim that collective bargaining limits income mobility and thus minimizes income inequality (Cooper & Mishel, 2015). That is, through the process of collective bargaining, unions negotiate better rates for workers at the bottom and middle of the pay scale and secure fair levels of employment terms and conditions by resisting against the managerial authority that unilaterally determines wages and subjective rules. Does guaranteeing CB rights for public sector workers reduce the wage inequality by compressing the wage distribution? Furthermore, wage contracts are typically negotiated on an occupational or departmental level. In that case, would the consequence of unions vary across different occupations? The remainder of this paper seeks to address these questions by

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[2] Before the 1950s, most states had no explicit legislation covering public sector workers, and the few laws that did exist outlawed strikes or bargaining.

empirically estimating the causal relationship between CB laws and its distributional impact on workers' wages.

3. Literature Review

Public sector unionization and CB studies can be roughly classified into two broad categories: those that estimate the effect of CB on average earnings and those that assess the impact of CB on the distribution of earnings.

3.1. CB Rights on Wage

This paper is closely related to the literature on the effects of CB laws on public sector earnings. Freeman (1986) and Freeman and Valletta (1988) summarized the early literature on this topic in detail. Most such literature provided evidence that a favorable legal environment toward collective bargaining increases the unionization rate and the bargaining power of unions. This, in turn, results in high earnings for both unionized and non-unionized workers in the public sector. Zax and Ichniowski (1990) explained that unions use political lobbying activity, in addition to collective bargaining, as a strategy to raise workers' relative employment and compensation. More recently, Frandsen (2016) exploits the timing of the enactment of state collective bargaining laws to isolate the effect of those laws on public sector compensation. His difference-in-differences estimates suggest that strong CB laws increase the compensation of firefighters and police, but have little effect on the compensation of teachers. Similarly, Brunner and Ju (2017) implemented a clever design that exploits policy discontinuities across state borderlines as a source of exogenous variation. Using private sector workers as a baseline comparison group, they found that the states with mandatory CB laws widen the public-private wage differentials by approximated 12% more than the states without such laws. Under the quantile regression framework, I utilize two sources of exogenous variations used by Brunner and Ju (2017) and Frandsen (2016): (a) policy discontinuity along state borders: (b) differences across states in the timing of duty-to-bargain legislations.

3.2. *CB Rights on Wage Distribution*

The second strand of work examines the effects of the CB rights on the wage distribution among public sector workers. Using employer-employee matched data from the Department of Education, Han (2013) explored how teacher unions affect teachers' compensation. Although not the focus of her paper, Han (2013) assessed the relationship between unionization and the variance in teacher salaries. In contrast with the results from earlier papers by Card (2001) and DiNardo et al. (1996), she provided evidence that school districts with strong union powers are associated with much more dispersed wage distribution. To support her evidence, she argued that teacher unions tend to avoid performance pay systems and lean toward credential-based pay systems. Focusing on the Wisconsin Act 10, Litten (2016) reported estimates of the distributional effects associated with the enactment of Act 10. Similar to Han (2013), his results indicated that after Act 10, total compensation fell most for teachers on the high end of the salary distribution, suggesting that teachers' unions have a compensation dispersal effect, rather than a compensation compression effect. While the existing research indicates that unions adversely affect teachers on the lower end of the distribution by dispersing the wage distribution, no existing research addresses this issue with any other public sector workers. Using the nationally representative data, I seek to fill this gap in the literature by examining the overall distributional impact of unions' power on public sector workers, as well as expanding the analysis separately for police and firefighters.

4. Data

In this section, I describe the data sources, the sample used for the analysis, and summary statistics. I employ three different data sources: state-level public sector collective bargaining rights, the American Community Survey, and PUMA-level control variables.

4.1. Public Sector CB Laws

To examine whether stronger CB environments alter the wage distribution of public sector workers, it is necessary to obtain comprehensive data on state public sector CB laws. This study primarily draws from the dataset on state CB laws collected by Freeman and Valletta (1988) from 1959 to 1986, and then extended by Rueben to cover the years through 1996. After referring to recent studies, I extended the dataset through 2015.[3] The assembled data contain state-level panel data for teachers, firefighters, and police, with an indicator variable that is equal to one if the state imposes a duty-to-bargain requirement on employers. The "duty to bargain" implies public sector employers (or the government) have a legal duty to bargain with their employees in good faith, but does not require a CB agreement to be reached. The indicator variable takes a value of zero if the state explicitly prohibits CB or if the state carries no provision for public sector CB. Table 1 summarizes the timing of passage of state legislations that granted CB rights to workers in the three occupations. Most of the duty-to-bargain legislations were introduced during the 1970s and 1990s. Only 20 states adopted duty to bargain laws for all three occupations by 1970, but 36 adopted such

[3] I obtained recent data on CB rights from Sanes and Schmitt (2014) and Brunner and Ju (2017). Detailed state-level laws that govern CB rights for teachers, police, and firefighter are also provided by Sanes and Schmitt (2014). Finally, to minimize inaccuracies, I cross-checked and validated my data with Lexis-Nexis and state government websites.

rules by 1990. Lastly, it is worth noting that a compilation with this classification scheme relates to heterogeneity in the right to bargain across occupations within some states. For example, Kentucky strictly prohibits collective bargaining for all public workers except police and firefighters, while Nevada guarantees collective bargaining rights only for local workers. To overcome this issue, I drop the workers who do not match with their corresponding state classification in the main analysis. In the subsequent analysis, I carry out separate analyses for each of the three occupations.

4.2 *American Community Survey*

This study augments quantile regression techniques with data from the 2005–2015 American Community Survey (ACS) Public Use Microdata Sample (PUMS). The ACS is a nationally representative survey administered by the U.S. Census Bureau that provides an annual portrait of social, economic, and housing data for approximately three million U.S. residents each year. The ACS also provides within-state geographical information based on approximately 2,380 Public Use Microdata Areas (PUMAs).[4] PUMAs are statistical geographic areas initially adopted by the ACS that are populated with at least 100,000 people. The obvious advantage of using the ACS for the purpose of this study is the large sample size, as the sample is restricted to the PUMAs that are located near a state border. Its detailed information about labor force activities allows this study to control for a full set of observable individual characteristics. Considering the fact that most of the legislations were passed in the early 1970s, the major disadvantage of using ACS is that it limits use of cross-state variation of CB law enactment over time. For this reason, I present these results alongside results from my secondary analysis using the March Supplements of Current Population Survey.

The study sample consists of public sector workers aged 18–65 at the time of interview, part of the labor force with positive hourly wages or weekly earnings. I exclude self-employed workers and military personnel and individuals working outside of the continental U.S. I further restrict the sample to those working in state and local government, as wages for federal workers are set through very different institutional structures. The dependent variable is the log of the hourly wage reported by workers. The full set of individual-level control variables include the log of the weekly working hours, age, age squared, sex, 16 categories of education, seven categories of marital status, and controls for Asian, black, and Hispanic individuals. Table 2 shows the descriptive statistics for state and local workers in the sample. The table

[4] PUMAs are statistical geographic areas that nest within states and are built on census tracts and counties. To account for changes in PUMA boundaries after 2011, I use crosswalk between 2000 PUMAs to 2010 PUMAs available on IPUMS website at: https://usa.ipums.org/usa/volii/puma00_puma10_crosswalk_pop.shtml

provides separate summary statistics for teachers, police, and firefighters in states with and without CB rights. A brief inspection of the table reveals several interesting facts. First, average salaries are higher for CB mandatory than CB non-mandatory states, across all occupations. Second, public sector workers in CB mandatory states tend to be older than in CB non-mandatory states. Lastly, they tend to have higher educational attainment than workers in CB non-mandatory states.

4.2 *PUMA-level Control Variables*

Some of the specifications in the analysis include a number of PUMA-level control variables that could potentially correlated with both the wage distribution and collective bargaining environment. In light of discussion by Brueckner and Neumark (2014), who argued that amenities affect public sector workers' ability to extract rent, I attempt to control for two amenity variables*:* (a) *proximity*: measuring the average distance to the nearest coast, Great Lakes, or major river; (b) *density*: measuring tract-weighted population density per square mile.[5] Finally, I attempt to account for the possibility of the relationship between voter opinion (and thus state's political environment towards public sector employees in general) and government spending on wages by including the Democratic vote share from the U.S. presidential election, obtained from the Federal Election Commission.

5. Empirical Framework

This section provides the empirical framework of this paper. First, I introduce quantile regression techniques and describe the difference between conditional and unconditional quantile regression. Then, I describe the identification strategy that exploits the variation off policy discontinuity along the state border.

5.1 *UQR*

As noted by Dube (2014), and many others, most studies are interested in estimating the effect of the explanatory variables, such as changes in state policies, on the unconditional distribution rather than distribution conditional on covariates. With the conventional quantile regression estimator, also known as conditional quantile regression (CQR), marginal effects are typically compared at fixed points on the conditional distribution. Therefore, the estimated coefficient represents the marginal effect of an explanatory variable, X on the τth quantile of Y distribution, conditioned on the set of covariates. This is

[5] Population density data are based on county-level 1990 census data (Glaeser & Khan, 2004), and proximity data are obtained from Rappaport and Sachs (2003).

problematic since the 10th percentile of wage conditional on a set of covariates may actually represent the 20th percentile of the overall (unconditional) wage distribution, producing estimates that are difficult to interpret.[6]

For this limitation of CQR, I use UQR, recently developed by Firpo, Fortin, and Lemieux (2009). The UQR estimator is based on a clever transformation of the dependent variable into the recentered influence function (RIF) as follows:

$$RIF(y; q_\tau) = q_\tau + \frac{\tau - \mathbf{1}\{y \leq q_\tau\}}{f_y(q_\tau)},$$

where q_τ indicates the value of the dependent variable at quantile τ, $\mathbf{1}\{y \leq q_\tau\}$ is an indicator variable that takes the value of 1 if $y \leq q_\tau$ and 0 otherwise, and $f_y(q_\tau)$is the marginal distribution of y at quantile τ.[7] Once the RIF is calculated for each observation, the UQR estimator is then defined as the coefficient vector from an OLS regression of *RIF* on a vector of covariates. The *RIF* regression model, $E[RIF(\mathrm{Y};\tau)|\mathbf{X}] = m_\tau(x)$, can be defined as the UQR model since the expectation of RIF for any given quantile q_τ is the value of the quantile itself, by the definition of *RIF* (Firpo et al., 2009).

In addition to the fact that quantile regressions are robust to outliers, one major advantage of using UQR over CQR is that the interpretation is no longer based on within groups. Thus, an estimated coefficient generated by UQR, the unconditional quantile marginal effect (UQME), can be interpreted in the same manner as OLS estimates. In context of this paper, the estimated coefficient from UQR represents the marginal effect of CB rights on hourly earnings at the unconditional quantile, τ, holding all else constant. Furthermore, UQR is a nonlinear estimator that is robust to a misspecification of covariates, which can be important in state policy analysis that includes fixed effects and other state-level control variables.[8]

As discussed earlier, I incorporate the RIF regression in a setting in which the effect of CB rights is identified off a policy discontinuity along state borders. As a robustness check, I also apply the method in Difference-in-Differences framework (RIF-DiD).

5.2 *Identification Strategy*

[6] This is not a concern with OLS since the OLS estimator satisfies the law of iterated expectations. In other words, the estimated coefficient, β_{OLS}, represents the marginal effect of some explanatory variable on both the conditional and unconditional means of the dependent variable.
[7] The density is estimated using a Gaussian kernel with optimal bandwidth.
[8] For example, Maclean, Webber, and Marti (2014) presented an example that demonstrates how CQR can be misapplied under state fixed effects.

To examine the effect of CB laws on the wage distribution of public sector workers, I estimate the model of the following form:

$$\ln(wage_{ipst}) = \beta_0 + \beta_1 CB_s + \alpha X_{ipst} + \delta_b + \gamma Z_p + \lambda_t + \varepsilon_{ipst}, \quad (1)$$

where $wage_{ipst}$ is the hourly wage of worker i, in puma *p*, in state s, in year *t*, CB_s is a dummy variable indicating whether state s has a duty-to-bargain law, X_{ipst} is a vector of observable individual characteristics, Z_p is the vector of control variables at the PUMA-level, δ_b and λ_t are border and year fixed effects, respectively, and ε_{ipst} is a random disturbance term.

The main parameter of interest is β_1, which represents the difference in wages for otherwise similar public sector workers in states with and without a CB law. The inclusion of border fixed effects implies that the impact of CB laws on workers' wages based solely on those PUMAs that its centroids is located substantially close to state boundaries and where each side of the border contain states with different CB environments. Restricting attention to variation near state borderlines (PUMAs with centroids located within 50 and 30 miles from its closest border) better accounts for any unobservable variables that could potentially correlate with both the CB law and worker wages. An important identifying assumption for CB_s to have a causal interpretation is that the population shares that remain below the given wage level associated with a given quantile must be the same as for the counterfactual group if the treatment group has not been treated. In other words, the identification assumption would be violated if there were a discontinuity in the unobservable time-varying characteristic that independently affected the population share below a given wage level. In addition to including controls for potential variables, such as PUMA-level amenities and vote shares for Democratic presidential candidates, the results for a series of balancing tests suggest PUMAs on either side of state borders appear remarkably similar to each other.

Considering that the length of state borders varies from less than 10 miles to over 700 miles, one might be concerned with the systematic differences that are not observable among PUMAs adjacent to the same border. To address such a concern, I also conduct an analysis restricting the sample to individuals located in commuting zones that straddle over two states with different CB status and use within commuting zone, across state variations of CB. Hence, the estimation equation takes the following from:

$$\ln(wage_{icst}) = \beta_0 + \beta_1 CB_s + \alpha X_{icst} + \theta_c + \gamma M_{cs} + \lambda_t + \varepsilon_{icst}, \quad (2)$$

where θ_c and M_{cs} are commuting zone fixed effects and commuting zone-by-state control variables that might bias the results. Although the sample size is significantly reduced by limiting the sample to workers located in commuting zones, the advantage of commuting zone analysis as opposed to border analysis is that now I can account for the labor market-specific unobservable characteristics by adding the commuting zone fixed effects. For instance, the composition of industry and occupations may differ significantly across labor markets, affecting the estimate of interest.

I estimate equations (1) and (2) with both OLS and UQR to assess the heterogeneous impact of CB rights on quantile specific wages.

6. Results

The analysis using quantile regressions focuses on whether explicit duty-to-bargain legislation led to a relatively compressed or decompressed wage distribution for public sector workers. In this section, I present balancing test results and the regression estimates from the OLS and UQR estimators of CB on wages.

6.1. Balancing Tests

The key threat to identification in our study is that PUMAs on one side of the state line may differ systematically in other ways from those on the other side. Before I present the baseline results, I attempt to address this concern by conducting a series of balancing tests estimating models of the form:

$$Y_{ps} = \rho_0 + \rho_1 CB_s + \gamma_b + \upsilon_{ps}, \qquad (3)$$

where Y_{ps} includes PUMA-level amenity variables, the vote shares of Democratic presidential candidates, and demographic characteristics taken from the 2005 to 2015 ACS PUMS. Table 3 shows differences in means for observable PUMA-specific characteristics between PUMAs located on the treated (CB legal) side of the state border over PUMAs on the non-treated (CB non-legal) side. Columns 1–2 show estimated coefficients and p-values for all PUMAs, without any sample restriction. As the results indicate, PUMAs located in CB mandatory states are more likely to support a Democratic presidential candidate in the 2000, 2004, 2008, and 2012 presidential elections, have significantly higher mean household income, higher population density, less likely to be located near a major body of water, and higher educational attainment. These results suggest that there are key differences between PUMAs in states with strong union bargaining power and those in states with weak bargaining power.

Columns 3–4 present the balancing test results where the sample is now restricted to PUMAs whose centroid is within 50 miles of a state border. Once border fixed effects are included (and thus comparing PUMAs along the same border), most of the estimated coefficients are statistically insignificant at the 10%

level, suggesting that the samples appears much more balanced. The pattern is similar when I restrict the sample to PUMAs within 30 miles of a state border (Columns 5–6), and I find no evidence of imbalance among observable characteristics, thus raising the confidence in the identifying assumption that PUMAs located on either side of a state border are observably and unobservably similar.

6.2 *Main Results*

I first assess the impact of CB on state and local workers' wage exploiting discontinuity along state borders. Table 4 presents the coefficient estimates for β_1, both at the mean and at quantiles, specifically the 10^{th}, 25^{th}, 50^{th}, 75^{th} and 90^{th} quantiles using the UQR estimator. Across all columns, the results in the first row show that the estimated OLS coefficient on the CB indicator is positive and statistically significant, ranging from 7% to 8.4% mean wage gain for all state and local workers in mandatory CB states. These results are consistent with Brunner and Ju (2017) suggesting that public sector workers in CB legal states earn approximately 5–8% more than otherwise similar workers in non-collective bargaining states.

The results from estimating UQR are below the OLS results in Table 4. In all specifications in this section, I include border and year fixed effects. Beginning with the full sample that includes all state and local workers (Columns 1-2), the results with PUMAs <50 miles from a state border indicate that mandatory CB laws play a different role in the reported quantiles, namely, the estimated coefficients suggest that the CB legal state and local workers at the lower end of the wage distribution (10^{th}–50^{th} quantile) earn between 10–11% wage premium compared to those in non-CB legal states. Interestingly, such a premium does not remain for workers at the upper end of the wage distribution. Although the estimates remain positive and statistically significant at the 1% level, the wage premium drops to 7–8% and 5–6% for the workers at the 75^{th} and 90 quantiles, respectively. Once I restrict the sample to PUMAs located 30 miles from a nearest state border, the magnitude of estimated coefficients, β_1, decreases, but the overall results remain qualitatively identical and suggest that CB rights grant a considerably greater earnings advantage for workers at the lower end of the distribution. Finally, focusing on local workers (columns 5-8) reveals similar results.

Overall, these findings from the quantile regression analysis suggest substantial heterogeneous effect hidden in the OLS analysis and furthermore highlight that the lowest-paid public sector workers benefit the most from an increase in CB power, leading to a wage decompression effect.

6.3. *Teachers, Police, Firefighters*

Having established the effect of CB rights on different points of the wage distribution for public sector workers, I conduct separate analyses for each of the three occupations (teachers, police, and firefighters) in the public sector to obtain further insight into how duty-to-bargain clauses affect different occupations, as wage contracts are typically negotiated on an occupational or departmental level and vary from area to area. For example, teachers in neighboring states or even counties districts are likely to have different pay scales based on CB agreements settled by both parties.

Table 4 presents occupational-specific estimated coefficients of interest from the preferred specification that includes border fixed effects and PUMA controls. All standard errors for OLS regressions are clustered at the border level, while all standard errors for UQR regressions are cluster-bootstrapped at the border level using 500 repetitions. Columns 1 and 2 of Table 5 replicate the results reported in columns 2 and 4 of Table 4 for comparison purposes, while columns 3, 5, and 7 represent the results for teachers, police, and firefighters, respectively. The analysis with PUMAs <50 miles from a nearest state border shows that the OLS coefficient for teachers is positive but marginally significant at the 10% level. Columns 5–8 replicate the mean analysis for police and firefighters. The results based on PUMAS <50 miles suggest that, on average, police and firefighters in states without collective bargaining laws earn approximately 7–10% and 4–5% less, respectively. However, the estimated coefficients for these analyses are statistically indistinguishable from 0, except for the <30 miles analysis for police (Column 6). This is qualitatively consistent with the estimates provided by recent studies[9].

Turning to the UQR estimates, the estimated quantile regression coefficients for teachers in column 3 contrasts with the results for all state and local workers. Specifically, teachers at the 10^{th} through 50^{th} quantiles gain a statistically insignificant 3% wage premium, while teachers at the 75^{th} and 90^{th} quantiles enjoy a statistically significant 5–7% wage advantage compared to otherwise similar teachers in non-bargaining states. Restricting the analysis to PUMAs located <30 miles from the nearest state border (Column 4) weakens the magnitude of such a trend and levels the distribution, although the dramatic increase in the wage premium is still present between the 50^{th} quantile and the 75^{th} quantile. These results strongly suggest that teachers with the highest income (and thus the most experience) garner the most benefit from teacher unions' CB rights, contributing to a substantial increase is wage inequality among public school teachers.

Columns 5–8 of Table 5 show the results from repeating this analysis for police and firefighters. As expected, the estimated coefficients for police are all positive (column 5), although some are statistically

[9] Litten (2016) found that Act 10 led to decrease in total teacher compensation by 8%, roughly two-thirds of which is driven through reduced fringe benefits. For police and firefighters, Frandsen (2016) provided evidence that police in CB legal states earn approximately 7% more, while firefighters enjoy a 13% wage premium. Brunner and Ju (2017), on the other hand, showed that police and firefighters earn 12% and 8% more, respectively.

insignificant. Going through the quantile specific estimates, the point estimates range from 6–14% between the 10–50th quantiles, while the estimates range from 1–3% for workers in the 75th and 90th quantiles. However, neither coefficient is statistically significant. Moving onto the analysis with PUMAs <30 miles (column 6), I find that the estimates between the 10–50 quantiles dramatically increase by 2–3 log percentage points. On the other hand, the two estimates for the 75 and 90th quantiles decrease minimally. Overall, I conclude that for police officers, most of the wage benefits associated with CB rights are concentrated in the lower and middle portions of the wage distribution. Columns 7 and 8 present results for firefighters only. As shown in column 3, the estimated coefficient on CB is much larger for workers in the 10th and 25th percentiles (16.5% and 11.6%, respectively) compared to workers in the middle and upper quantiles. These results portray a similar story to police, yet the largest gains from CB rights are clustered around the lower rather than the middle percentile. Despite a modest fluctuation of UQR estimates going from the <50 miles sample to the <30 miles sample, the results remain similar for firefighters.

In summary, disaggregating the sample by occupation indicates that depending on the occupation, being able to bargain with government employers has different impacts on various levels in the wage distribution. To be specific, Table 5 provide strong evidence that, among teachers, the most experienced and highest paid benefit most from mandatory CB laws, leading to a wage dispersal effect, rather than the wage compression effect that is known to be associated with greater union power. For police and firefighters, I find the opposite distribution, suggesting that relatively inexperienced and lowest paid police and firefighters benefit, leading to a reduction in wage inequality.

6.4 *Commuting Zone Results*

Table 5 presents the results from the commuting zone analysis. A full set of individual characteristics, commuting zone fixed effects, and year fixed effects are included in all specifications. All standard errors for OLS regressions are clustered at the commuting zone level, and all standard errors for UQR regressions are cluster-bootstrapped at the commuting zone level using 500 repetitions. Columns 1, 3, 5, and 7 present the estimated OLS coefficient and UQR coefficient without controls, where I add commuting zone-by-state controls in columns 2, 4, 6, and 8. Focusing separately by groups, the pattern of results is similar to those found in the main analysis. Notably, the estimated OLS indicates that there is a statistically significant 7–9% wage premium associated with the right to collectively bargain for earnings. Moreover, the relative magnitude of coefficients decreases for public sector workers at the upper end of the wage distribution, suggesting wage compression effect. The magnitude of estimated UQR coefficients, for the most part, remains consistent compared to the coefficients in Table 4.

Narrowing the focus to specific occupations, OLS and UQR estimates show a qualitatively similar pattern to the border results. OLS estimates for teachers remain unchanged, while they are somewhat altered in magnitude for police (0.095 to 0.101), and firefighters (0.044 to 0.009). The results for UR estimates generally convey the same story as the border results for all three occupations. Teachers located at the upper end of the wage distribution enjoy a relatively stable wage premium at approximately 4–6%, depending on the sample restriction. Moving down to columns 6 and 8, the estimated coefficient is greatest for police and firefighters between the 10th and 50th percentiles, suggesting a possible relationship between CB rights and a reduction in wage inequality among police and firefighters.

Overall, the commuting zone results provide strong evidence that the borderline results were not driven by unobserved differences in labor market conditions across PUMAs located on the opposite side of a border.

7. Robustness Check

RIF-DiD: Teachers

In my subsequent analysis, I implement UQR, within RIF-DiD as an alternative identification strategy. Using the 1968–2006 March Annual Social and Economic Supplement of the CPS (March CPS),[10] this alternative identification strategy essentially compares public school teachers in states with duty-to-bargain legislation versus those in states without such legislation under a simple difference-in-differences framework. Similar to the ACS, the March CPS is also a nationally representative survey that contains detailed information on topics such as income, employment status, and demographic characteristics. For sample consistency purposes, I place similar restrictions on the March CPS as in the ACS. The obvious advantage of the March CPS is that it provides nationally representative survey data every year since early 1960s, making use of the variations in the CB status across states over time. One of the two disadvantages of using the March CPS is that it does not permit me to identify federal workers prior to 1984, which makes aggregate analysis impractical. Furthermore, another limitation is that it does not provide a large sample, which may place major constraints on the distributional analysis. For these reasons, I focus my analysis on teachers only.

A brief explanation of the RIF-DiD method is as follows. Let τ_t be the population share that remains below a specific level of wage y', for state s at time t. The population share below y' is observed in pre-treatment period t = 0 and in a post-treatment period $t = 1$. Furthermore, $D = 1$ if state s has adopted

[10] CPS-March Supplements was accessed via the Integrated Public Use Microdata Series (IPUMS) database. The sample starts from 1968 since the CPS did not provide detailed occupation codes prior to 1968, which does not affect this study since the majority of the CB legislations were passed in the 1970s.

CB in period t, $D = 0$ otherwise. Then, the estimated impact of at a specific level of wage, y', measured as the change in population shares, is given by:

$$-[(\tau_1|D = 1) - (\tau_0|D = 1)] - [(\tau_1|D = 0) - (\tau_0|D = 0)]$$

The quantile treatment effect can then be calculated by dividing the population share by a kernel estimate of the joint density of the wage at the wage level y'. The identifying assumption of the RIF-DiD method is that the pre- and post-changes in the population that remain below the wage level, y', must be same in the treatment state as in the comparison state, in cases in which there is no treatment (Havnes & Mogstad, 2015).

Implementing the RIF-DiD method in the context of this study, the main regression equation takes the following form:

$$\ln(wage_{ist}) = \alpha_s + \delta_t + \pi CB_{st} + X'_{ist}\beta + \varepsilon_{ist}, \tag{4}$$

where $\ln(wage_{ist})$ denotes the log of hourly wages for teachers in state *i*, in state *s*, in year *t*, *CB* is an indicator variable taking the value of one if the state has a duty-to-bargain rule and zero otherwise, X'_{ist} is a vector of covariates including age, age squared, sex, race, education, and marital status, α_s and δ_t are state and year fixed effects, respectively, and lastly, ε_{ist} is the error term. The main parameter of interest is π, which represents the difference in wages for otherwise similar public sector workers in states with and without a CB law.

Table 7 displays the OLS and RIF-DiD estimates on CB. I find that the OLS estimates reported in the first row suggest a zero mean impact, which is consistent with the findings by Frandsen (2016). Turning to quantile-specific estimates, the findings using RIF-DiD remain comparable to those using ACS data. However, there is a considerable difference for workers at the lower end of the wage distribution, that is, teachers at the 10^{th} percentile acquire an approximately 7% and statistically significant wage premium over observably similar teachers in non-CB states. Adding a region-specific time trend (column 2) increases the magnitude of this gap for teachers at the upper end of the distribution, indicating that the ratification of a duty-to-bargain rule may, in fact, contribute to the income inequality by dispersing earnings among teachers.

8. Conclusion and Discussion

A thorough investigation of how the benefits are distributed among individuals provides an important context to evaluate the efficacy of public policy. Given that wage inequality is the defining challenge in modern society, a policy that reduces inequality may be socially desirable, even if there is a zero or even negative mean impact. In this paper, I examined the distributional effect of CB laws on public sector workers' wages and showed that OLS estimates mask substantial heterogeneity across the entire wage distribution.

As described previously, the key threat to identification in this study is that the distribution of public sector workers in states with CB laws may differ systematically from the distribution of otherwise similar public sector workers in states without such laws. In this paper, I attempt to address this challenge by carrying out a border analysis by restricting the sample to workers working in PUMAs where the PUMA centroid is less than 50 and 30 miles from the nearest border. The findings in this paper present new evidence that granting duty-to-bargain laws may lead to overall wage compression for public sector workers. In particular, the results show that the OLS estimates are positive and insignificant in many instances, but the UQR estimates suggest that a wage differential between CB legal vs. non-CB legal workers is large at the lower end of the wage distribution, but much smaller at the higher end. Focusing on specific occupations, CB rights appear to decompress the wage distribution (and thus raise inequality) among teachers and compress the wage distribution among police and firefighters. These results are robust to commuting zone analysis, where I restrict the sample to commuting zones crossing states with different CB settings. Finally, the results for teachers are robust to an alternative identification strategy that utilizes differences across states in the time of laws governing CB rights for public sector workers, suggesting the causal interpretation of the findings in this paper.

The fact that we do not see evidence of wage compression among teachers is somewhat puzzling, considering that unions are known to help reduce inequality. This result, however, potentially indicates that the mechanism through which CB affects teachers' salary structure differently than how they affect salary structure of other public-sector workers. For example, it is widely known that teachers have a unique tenure system that significantly affects their turnover rate. In general, school districts have a strong motivation to layoff low-quality teacher during the probationary period, especially if unions demand higher salaries (Han, 2016). To reward their members, unions may use their CB power to ask for higher raises once a teacher receives tenure. In other words, there would be a much greater increase in salary associated with seniority for teachers in CB legal states, which would possibly explain the sudden jump in wages at the 75th quantile. Although, such explanation may be true, I cannot rule out alternative explanations for the findings in this paper.

Nonetheless, the overall results presented in this paper have important implications for the much-debated literature on public sector CB rights. The findings point out that the effects of CB vary systematically across the wage distribution, and that workers at the lower end of the distribution seem to be the primary beneficiaries. However, these benefits are more clustered at the upper end of the wage distribution for teachers. Taken together, it is important for policymakers to be aware of and consider the distribution sensitive effects of laws that govern CB rights for public sector workers, as they could unintentionally benefit one sub-group, while having no impact on another.

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Table 1. Collective Bargaining Environment for Public Sector Workers

Duty-to-Bargain (CB) Law	1960s	WI (1960), CT (1966), DE (1966), MI (1966), NY (1968), RI (1968), VT (1968), WA (1968), NJ (1969)
	1970s	CA (1970), IN (1970), ME (1970), NE (1970), NV (1970), OR (1970), HI (1971), PA (1971), SD (1971), KS (1972), OK (1972), AK (1973), MN (1974), MT (1974), FL (1976), NH (1976)
	1980s	IL (1985), OH (1985)
Without Duty-to-Bargain (CB) Law		AL, AR, CO, GA, ID, KY, LA, MD, MS, MO, NM, NC, ND, TN, TX, UT, VA, WV, WY

Sources: Data are from Valletta and Freeman (1988), Kim Rueben's update (1997), Sanes and Schmitt (2012), Brunner and Ju (2017)

Table 2. Summary Statistics

	All State and Local Workers				Teachers				Police				Firefighters			
	CB Required		CB Not Required		CB Required		CB Not Required		CB Required		CB Not Required		CB Required		CB Not Required	
	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
Salary	54083	31570	44180	27688	54495	21483	44637	15391	67240	29190	46941	19755	71332	29400	51802	20665
Log(wage)	3.116	0.486	2.894	0.461	3.186	0.413	2.959	0.329	3.326	0.421	2.978	0.380	3.261	0.414	2.916	0.384
Work Hours	41.558	6.144	42.401	6.143	42.642	6.891	44.325	6.956	43.084	5.997	43.217	5.896	49.181	7.450	50.779	7.803
Age	45.497	10.951	44.781	11.183	43.590	11.241	42.734	11.127	39.477	9.529	39.306	10.220	40.109	9.449	38.265	9.803
Female	0.564	0.496	0.596	0.491	0.745	0.436	0.785	0.411	0.138	0.345	0.132	0.339	0.042	0.202	0.038	0.190
Less than HS	0.022	0.146	0.030	0.171	0.002	0.042	0.002	0.049	0.005	0.070	0.007	0.083	0.006	0.079	0.007	0.086
HS Degree	0.451	0.498	0.447	0.497	0.039	0.194	0.043	0.204	0.638	0.481	0.721	0.449	0.786	0.410	0.818	0.386
College Degree	0.255	0.436	0.272	0.445	0.386	0.487	0.511	0.500	0.305	0.460	0.238	0.426	0.186	0.389	0.157	0.363
Adv. Degree	0.273	0.445	0.250	0.433	0.573	0.495	0.443	0.497	0.052	0.222	0.034	0.181	0.022	0.148	0.018	0.133
Black	0.096	0.295	0.151	0.358	0.056	0.229	0.105	0.307	0.084	0.278	0.119	0.324	0.051	0.219	0.078	0.269
Hispanic	0.096	0.295	0.087	0.282	0.071	0.256	0.077	0.266	0.111	0.314	0.088	0.284	0.076	0.265	0.071	0.256
Asian	0.041	0.199	0.016	0.127	0.021	0.144	0.010	0.098	0.022	0.146	0.006	0.080	0.009	0.094	0.005	0.073
Married	0.665	0.472	0.678	0.467	0.702	0.457	0.715	0.451	0.695	0.461	0.711	0.453	0.726	0.446	0.713	0.452
N	826,802		483,357		185,301		103,728		35,069		17,903		12,396		6,433	

Notes: Table presents summary statistics for salary and individual controls for public sector workers. All variables are based on 2005-2015 American Community Survey PUMS Data.

Table 3. Balancing Tests

	All PUMAs		< 50 Miles		< 30 Miles	
Variable	CB coef. (1)	p-value (2)	CB coef. (3)	p-value (4)	CB coef. (5)	p-value (6)
Voting and Climate Variables						
Dem Vote Share 00	15.681***	0.000	4.338	0.434	5.329	0.407
Dem Vote Share 04	14.750***	0.000	6.079*	0.062	6.952*	0.071
Dem Vote Share 08	14.678***	0.000	4.393	0.312	4.569	0.368
Dem Vote Share 12	14.773***	0.000	4.261	0.350	4.443	0.409
Mean HH Income	8,000**	0.022	632.17	0.847	-1,513.0	0.735
Total Population	499,934	0.000	64,742	0.493	56,064	0.551
Population Density	2,206***	0.000	-178.453	0.553	-153.3	0.627
Mild	-2.370***	0.004	-1.182	0.383	-0.563	0.694
Dry	-3.488***	0.000	0.748	0.548	0.567	0.678
Proximity to Water	-15.287***	0.000	1.057	0.833	-1.355	0.748
2005-2015 PUMS ACS Variables						
Age	0.5000***	0.003	0.047	0.763	0.070	0.622
Fraction Female	0.008*	0.079	0.014	0.379	0.006	0.285
Fraction Less than HS	-0.013	0.148	-0.010**	0.014	-0.011***	0.003
Fraction High School Degree	-0.028**	0.047	0.005	0.847	0.004	0.898
Fraction College Degree	0.019***	0.009	0.008	0.635	0.009	0.628
Fraction Advanced Degree	0.022***	0.001	-0.003	0.799	-0.002	0.897
Fraction Married	-0.022**	0.022	-0.011	0.299	-0.013	0.290
Fraction Black	-0.055**	0.010	-0.005	0.756	0.004	0.837
Fraction Asian	0.034***	0.005	-0.001	0.862	-0.005	0.445
Fraction Hispanic	0.008	0.852	-0.007	0.523	-0.012	0.235
Management, business, and financial operations occupations	0.008*	0.074	0.004	0.764	0.004	0.740
Professional and related occupations	0.011**	0.020	0.001	0.929	0.001	0.896
Service occupations	0.014***	0.004	0.006	0.254	0.004	0.459
Sales and related occupations	-0.002	0.189	0.000	0.996	0.001	0.719
Office and administrative support occupations	-0.002	0.455	0.000	0.979	0.002	0.554
Farming, fishing, and forestry occupations	0.001	0.483	-0.001	0.431	0.000	0.856
Construction and extraction occupations	-0.013***	0.000	-0.004	0.249	-0.006	0.204
Installation, maintenance, and repair occupations	-0.007***	0.000	-0.002	0.348	-0.003	0.184
Production occupations	-0.006	0.330	-0.001	0.932	-0.002	0.760
Transportation and material moving occupations	-0.004**	0.050	-0.001	0.843	-0.002	0.775

Notes: Table presents differences in means tests for PUMA level attributes. Each point estimate is from a separate regression of the listed PUMA level characteristics on indicator of for a mandatory collective bargaining state. Columns 1-2 are the full sample of public sector workers. Columns 3-4 restrict to individuals in PUMAs whose centroid is less than 50 miles from a state border. Columns 5-6 restrict to individuals in PUMAs whose centroid is less than 30 miles from a state border. Columns 5-6 include border fixed effects. Standard errors are clustered by state in columns 1-2, and by state-by-border in columns 3-6. *** p<0.01, ** p<0.05, * p<0.1

Table 4. Main Results

	All State and Local Workers				Local Workers Only			
	<50 Miles (1)	<50 Miles (2)	<30 Miles (3)	<30 Miles (4)	<50 Miles (5)	<50 Miles (6)	<30 Miles (7)	<30 Miles (8)
OLS	0.084***	0.075***	0.079***	0.070***	0.072***	0.061***	0.064***	0.051***
	(0.0168)	(0.0129)	(0.0203)	(0.0146)	(0.0204)	(0.0146)	(0.0200)	(0.0143)
10	0.104***	0.098***	0.098***	0.092***	0.104***	0.096***	0.096**	0.086***
	(0.0184)	(0.0190)	(0.0336)	(0.0260)	(0.0310)	(0.0346)	(0.0433)	(0.0215)
25	0.114***	0.106***	0.112***	0.103***	0.094***	0.084***	0.089***	0.076***
	(0.0232)	(0.0188)	(0.0337)	(0.0326)	(0.0206)	(0.0197)	(0.0271)	(0.0191)
50	0.103***	0.091***	0.093***	0.079**	0.077***	0.062***	0.065**	0.047***
	(0.0220)	(0.0276)	(0.0238)	(0.0334)	(0.0251)	(0.0189)	(0.0254)	(0.0174)
75	0.082***	0.070***	0.076***	0.062***	0.068***	0.053***	0.063***	0.044**
	(0.0238)	(0.0159)	(0.0170)	(0.0160)	(0.0174)	(0.0175)	(0.0179)	(0.0173)
90	0.058***	0.046***	0.044***	0.031*	0.051***	0.035**	0.040**	0.024*
	(0.0177)	(0.0120)	(0.0138)	(0.0162)	(0.0180)	(0.0148)	(0.0183)	(0.0142)
PUMA Controls		x		x		x		x
N	621,644	621,644	425,536	425,536	383,062	383,062	273,676	273,676

Notes: Data from the American Community Survey (ACS) 2005-2015. Each point estimate is from a separate Unconditional Quantile Regression (URQ). All specifications include the full set of individual-level controls, border, and year fixed effects. Standard errors are clustered at the border level in OLS specifications and cluster-bootstrapped with 500 repetitions in UQR specifications, and are reported in parentheses. ACS earnings sample weights are applied in all regressions. *** p<0.01, ** p<0.05, * p<0.1

Table 5. Teachers, Police, and Firefighters

	All State and Local Workers		Teachers		Police		Firefighters	
	<50 Miles	<30 Miles	<50 Miles	<30 Miles	<50 Miles	<30 Miles	<50 Miles	<30 Miles
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
OLS	0.075***	0.070***	0.039*	0.040**	0.071	0.095***	0.054	0.044
	(0.0129)	(0.0146)	(0.0232)	(0.0183)	(0.0520)	(0.0356)	(0.0330)	(0.0391)
10	0.098***	0.092***	0.030	0.038*	0.059	0.091	0.164	0.239
	(0.0190)	(0.0260)	(0.0255)	(0.0196)	(0.0871)	(0.0613)	(0.1185)	(0.072)
25	0.106***	0.103***	0.026	0.038	0.140**	0.167***	0.116*	0.096
	(0.0188)	(0.0326)	(0.0415)	(0.0270)	(0.0614)	(0.0628)	(0.0659)	(0.0655)
50	0.091***	0.079**	0.031	0.039**	0.112*	0.155**	0.043	0.077**
	(0.0276)	(0.0334)	(0.0297)	(0.0169)	(0.0579)	(0.0630)	(0.0339)	(0.0315)
75	0.070***	0.062***	0.065***	0.052**	0.034	0.025	0.006	0.021
	(0.0159)	(0.0160)	(0.0251)	(0.0228)	(0.0317)	(0.0437)	(0.0274)	(0.0266)
90	0.046***	0.031*	0.048*	0.040*	0.013	0.008	0.012	-0.0005
	(0.0120)	(0.0162)	(0.0283)	(0.0231)	(0.0253)	(0.0327)	(0.0295)	(0.0287)
N	621,644	425,536	146,620	103,441	26,294	18,782	10,372	7,509

Notes: Data from the American Community Survey (ACS) 2005-2015. Each point estimate is from a separate Unconditional Quantile Regression (URQ). All specifications include the full set of individual-level controls, PUMA-level controls, border, and year fixed effects. Columns 3-4 restrict the sample to K-12 Teachers, while columns 5-6 and 7-8 restrict sample to police and firefighters, respectively. Standard errors are clustered at the border level in OLS specifications and cluster-bootstrapped with 500 repetitions in UQR specifications, and are reported in parentheses. ACS earnings sample weights are applied in all regressions. *** p<0.01, ** p<0.05, * p<0.1

Table 6. Commuting Zone Analysis

	All State and Local Workers		Teachers		Police		Firefighters	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
OLS	0.090***	0.073***	0.045	0.042*	0.113***	0.101***	0.049	0.009
	(0.0271)	(0.0147)	(0.0274)	(0.0223)	(0.0293)	(0.0217)	(0.0336)	(0.0272)
10	0.104*	0.086***	0.0505	0.042	0.148	0.122	0.104**	0.062
	(0.0600)	(0.0262)	(0.0326)	(0.0289)	(0.1797)	(0.1197)	(0.0441)	(0.0385)
25	0.118**	0.095**	0.039	0.027	0.238**	0.216**	0.0555	0.030
	(0.0536)	(0.0407)	(0.0531)	(0.0288)	(0.0965)	(0.0864)	(0.0529)	(0.0765)
50	0.110***	0.091***	0.035	0.044	0.159***	0.146***	0.054	0.011
	(0.0391)	(0.0218)	(0.0391)	(0.0274)	(0.0389)	(0.0397)	(0.0555)	(0.0638)
75	0.077***	0.063***	0.054*	0.054*	0.046*	0.044	0.055	0.0041
	(0.0225)	(0.0205)	(0.0295)	(0.0325)	(0.0262)	(0.0321)	(0.0690)	(0.0408)
90	0.045	0.034	0.047	0.043	0.027	0.030	0.036	-0.049
	(0.0388)	(0.0438)	(0.0408)	(0.0339)	(0.0237)	(0.0347)	(0.1024)	(0.0740)
CZ-by-State Controls		x		x		x		x
N	110,971	110,971	24,878	24,878	4,981	4,981	1,557	1,557

Notes: Data from the American Community Survey (ACS) 2005-2015. Each point estimate is from a separate Unconditional Quantile Regression (URQ). Sample restricted to commuting zones that straddle boundaries of states with a different collective bargaining environment. All specifications include the full set of individual-level controls, commuting zone, and year fixed effects. Standard errors are clustered at the commuting zone level in OLS specifications and cluster-bootstrapped with 500 repetitions in UQR specifications, and are reported in parentheses. ACS earnings sample weights are applied in all regressions. *** p<0.01, ** p<0.05, * p<0.1

Table 7. Teachers using 1968-2006 ASEC

	(1)	(2)
	log_wage	log_wage
OLS	0.0219	0.026
	(0.0252)	(0.0262)
0.1	0.065***	0.068***
	(0.0198)	(0.0219)
0.25	0.015	0.017
	(0.0104)	(0.0117)
0.5	0.007	0.015
	(0.0084)	(0.0090)
0.75	0.030***	0.046***
	(0.0097)	(0.0097)
0.9	0.036***	0.068***
	(0.0134)	(0.0139)
Region x Time Trend		x
N	69,394	69,394

Notes: Data from the Current Population Survey (1968-2006). All specifications include the full set of individual-level controls, state fixed effects and year fixed effects. Standard errors are clustered at the state level in OLS specifications and cluster-bootstrapped with 500 repetitions in UQR specifications, and are reported in parentheses. CPS-March supplement weights are applied in all regressions. *** p<0.01, ** p<0.05, * p<0.1