

Teacher Salaries, State Collective Bargaining Laws, and Union Coverage

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Initial draft, June 2010
Current draft, December 2012

Abstract

What are the causal effects of collective bargaining (CB) on teacher salaries? This seemingly simple question is difficult to answer because (a) national data measuring school district salaries and collective bargaining is limited in scope, while individual teacher data from the CPS mismeasure CB coverage; (b) union wage effects depend not only on coverage but also on an array of CB law provisions difficult to quantify; (c) union coverage and state CB laws are endogenous since each has been influenced by union sentiment among workers and voters; and (d) OLS estimates of the wage effect of CB coverage and laws may be biased by measurement error and endogeneity. We attempt to address these issues using measures of historical labor sentiment, by creating indices of CB law strength, and by using alternative national data sets containing information on teacher salaries and coverage. As in prior studies, we find modest union salary effects for teachers using standard methods, albeit smaller than found for the private sector. A union benefits premium estimated from SASS substantially exceeds the salary premium. Estimates of CB law and coverage effects using IV (with historical data measuring labor sentiment as instruments) are unrealistically large. We then examine historical data on teacher salaries from 1949 through 2009. We find that in 1959 (the 1960 Census), prior to adoption of state CB laws and subsequent coverage, teacher salaries were higher in states that would eventually adopt CB laws and bargaining. By 1979 all but a few of the states that would pass CB laws had done so. The union salary advantage for teachers increased between 1959 and 1979. Our tentative assessment is that roughly half the 10 percent OLS salary advantage seen for union teachers is causally due to CB laws and coverage, while the other half is not caused by collective bargaining, resulting instead from pre-existing sentiment or other factors correlated with coverage.

JEL Codes: I21, J31, J45, J5

Prepared for the “Public Sector Collective Bargaining” session at the American Economic Association Meetings, San Diego, January 6, 2013. We appreciate helpful comments on our earlier draft from William Boal and Gregory Gilpin.

1. Introduction

As widely recognized, unionization in the U.S. private sector has declined substantially over roughly a 50 year period. Public sector union membership, however, grew rapidly through the early 1990s and then again in the 2000's. By 2009, more than half of all U.S. union members were employed in the public sector. Public school teachers constitute the largest single group of public sector employees and account for about a third of all unionized public sector workers. Estimates from the Current Population Survey (CPS) show that among primary and secondary public school teachers, 60% are union members and 65% are covered by collective bargaining agreements.¹ Using administrative data from the Schools and Staffing Survey (SASS), it is estimated that teachers in 62% of school districts were covered by CB agreements during the 1999-2000 school year.

Given the size, importance, and cost of primary and secondary education, coupled with the fact that personnel costs are the primary costs of education, knowledge regarding how unions and collective bargaining affect teacher salaries (and other outcomes) is essential. Yet knowledge of union effects is limited owing to data and methodological limitations. Nationwide union coverage data at the school district level is no longer readily available from the Census of Governments (as used by Hoxby, 1996), while membership and coverage data at the individual teacher level are measured with substantial error. More fundamentally, union membership, coverage, bargaining power, and salary outcomes are influenced by state-level collective bargaining laws, making it methodologically difficult to identify and disentangle causal effects of state laws and union coverage on teacher salaries.

Evidence in the research literature is very much mixed. As we will show, standard OLS wage equation analysis from the CPS and from SASS clearly shows that unionized teachers and school districts receive pay that exceeds that for nonunion teachers and districts. That said, the union-nonunion wage differential for school teachers (and public sector workers generally) is substantially smaller (roughly half to two-thirds) than that seen in the private sector. In contrast to standard estimates that identify a significant union salary premium for teachers, recent studies employing econometric strategies designed to better capture causal effects conclude that the adoption of teacher collective bargaining laws and subsequent unionization did little to raise teacher salaries (Lovenheim 2009; Frandsen 2012). Each set of studies has specific strengths and weaknesses, so it is not altogether clear whether either set of studies provides results that are reliable or can be generalized.

Our analysis does not fully resolve discrepancies between studies finding a sizable union impact on teacher salaries versus studies concluding there is little effect from collective bargaining. These studies can be reconciled, however, if union school districts (and states with teacher collective bargaining), while currently paying higher salaries than in nonunion school districts (states) paid higher salaries to teachers *prior* to unionization. If this were the case, it could explain estimates of premiums using OLS cross-section analysis and explain why one finds little change in salaries associated with the passage of state bargaining laws and the adoption of collective bargaining.

We examine historical and contemporary evidence. Specifically, we first develop measures of recent state collective bargaining laws and school district unionization. These are shown to be associated with higher teacher salaries (about 10%) and substantially higher teacher benefits (about 30%). We then use these same measures of unionization and collective bargaining laws to determine whether these *current* measures are associated with higher salaries in states *prior* to the adoption of state collective bargaining.

¹ All calculations are from the authors, some of which are included at <http://www.unionstats.com>.

Teacher salaries from earlier periods are measured using the 1950 and 1960 decennial Censuses of Population, prior to widespread collective bargaining, as well as subsequent decennial Censuses following the evolution of teacher collective bargaining. Although results are preliminary and incomplete, the principal conclusion is that school teachers in states eventually adopting CB laws were paid more than were teachers in states never adopting CB laws. Such evidence helps reconcile research showing moderate or substantial union-nonunion differences in teacher salaries and recent papers concluding that the adoption of collective bargaining had very limited effects on teacher salaries.

In what follows, we first provide background on state collective bargaining laws and unionization among public school teachers. A brief (non-comprehensive) summary of prior literature on teacher salaries and unionization is provided, with emphasis on methods used. We then lay out our approach to the estimation of union wage effects, with emphasis on data sources and the measurement of state collective bargaining laws and union coverage.

2. Background and prior literature

Prior to the 1960s few states allowed or provided provisions that facilitated collective bargaining among teachers, and state or local employees more broadly. Starting around 1960 and continuing through the early 1970s and (to a far lesser extent) beyond, states passed laws that allowed collective bargaining for public employees, a large subset of these states passing laws specifying a duty to bargain.² The rights and provisions incorporated in these public sector laws vary to some degree across different groups of workers, say, teachers, police, and firefighters. The most fundamental legal feature differentiating states is their choice to either adopt provisions regarding rights to bargain or duty to bargain, or remain silent. States with CB laws include a set of provisions covering dispute resolution procedures, typically designating some combination of fact finding, mediation, and arbitration, with specification of the procedure type (say, final offer or standard interest arbitration), whether such procedures are voluntary or required, and whether there can be legislative override of such procedures. Also designated are rights to strike and penalties (or not) associated with a non-allowed strike. And there are often provisions on union security; that is, whether membership and dues are required for covered workers.

Valletta and Freeman (1988) assembled, discuss, and analyze what has become a widely used data set identifying various provisions on state public sector bargaining laws. For example, Farber (2006) uses the Valletta-Freeman data set, providing a compact summary of these provisions and their adoption among various groups of workers. In this paper we follow the classification of bargaining laws used in a similar but more recent study by Lund and Maranto (1996), which in turn is updated in reasonably straightforward fashion based on information in (occasional) issues of *State Notes* from the Education Commission of the States (2008). More broadly, Schneider (1996) has provided legal analysis and an in-depth discussion of the evolution of state collective bargaining laws.

An earlier literature examining the effects of collective bargaining on wages in the public sector is nicely summarized by Ehrenberg and Schwarz (1986) and Lewis (1988). Freeman (1986) provides an insightful comparison of private and public sector unionization. Generally, union wage effects (using standard OLS analysis) for various worker groups in the public sector are found to be roughly half as large as in the private sector. Estimated union wage effects for teachers from these early studies are particularly low (Ehrenberg and Schwarz, 1986, Table 22.1, p. 1225). One interpretation of the public sector evidence is that bargaining power is lower in the public than private sector, in part because the

² Wisconsin enacted collective bargaining laws for public school teachers in 1959, the first state to do so. Massachusetts and Michigan followed in 1965, with many others following in the late 1960s and early 1970s.

right to strike is often attenuated. Caution in accepting such a conclusion, however, is warranted. First, as Freeman (1986) points out, many public sector workers are in professional occupations, where private sector union wage effects are modest. Second, unions impact benefits as well as wages and public sector workers typically have more generous benefits and job security. Third, unions affect public sector outcomes in part through the political process, so they may increase employment as well as compensation and, more to the point, increase pay for non-covered as well as covered public workers. To the extent that unions increase pay for non-covered public workers, union-nonunion wage gaps are reduced.

Recent estimates continue to point to lower public sector than private sector union wage differentials. Hirsch and Macpherson (2012) provide OLS regression estimates of union wage effects by year for 1973-2011 for the private and public sectors using time-consistent methods and controls (see Figure 1). For the private sector for 2000-2011, union wage effects range between .16 and .20 log points. Estimates for all public sector workers for 2000-2011 are about roughly half as large, ranging between .08 and .13 (the lower-end estimate for the private sector is 2011 and for the public sector 2009). Not surprisingly, estimates for the entire public sector are in line with our subsequent OLS estimates for public school teachers.

Among more recent studies of teachers and unions, the best known and perhaps most ambitious is Hoxby's (1996) analysis of union effects on multiple educational outcomes. Using Census of Governments (COG) data for 1972-1992, Hoxby constructs a district level panel data in which school districts are categorized as unionized if they engage in collective bargaining and contracts cover a majority of teachers. She estimates that unions raise teacher salaries by a modest 5%, based on a model that includes school district fixed effects and using collective bargaining law changes to instrument changes in unionization. Unfortunately, district-level coverage data for more recent years can no longer be obtained from the COG.

A study by Lovenheim (2009) casts further doubt on unions' ability to substantially increase teacher salaries. Lovenheim obtains hand-collected data for three states – Iowa, Indiana, and Minnesota – on the dates on which district-level union contracts for teachers were first obtained. Following careful analysis, he concludes that there is little evidence of any positive salary effect for teachers due to their initial unionization. It is difficult to know whether one can generalize these results to other states or to recent time periods (initial union certification in these states occurred primarily in the mid-1970s following passage of public sector labor laws). But Lovenheim's results appear to be internally valid and point both to the importance of accounting for union endogeneity and the difficulty in obtaining reliable estimates of union wage effects using currently available data on teacher salaries and union coverage.

More recently, Frandsen (2011) provides a comprehensive analysis of the effects of public sector collective bargaining laws on the earnings, benefits, and employment of teachers, police, and firefighters. Causal effects are identified based on changes in collective bargaining rights for these three groups at different times in different states. Using standard cross-sectional analysis, he finds modest effects on teacher pay and benefits. But using differences-in-differences models, he finds little effect from collective bargaining on teacher pay and benefits, with or without state fixed effects and state-specific time trends.

In a recent paper, Brunner and Squires (2011) examine how union salary effects for teachers vary with district size. They find that union impacts are effectively zero in small districts but that they increase with respect to district size. In our analysis in this draft we account for district size (using SASS) or city size (using the CPS), but do not allow union effects to vary with size.

In short, standard cross-sectional analyses of teacher salaries and collective bargaining find modest

union effects on pay, whereas those studies examining changes in salaries based on changes in collective bargaining or collective bargaining laws tend to find little if any union effect.³

3. Estimation

Our goal is to measure the impact of “unionization” on teacher salaries, where unionization reflects some combination of teacher collective bargaining coverage (COV) and collective bargaining law strength (LAW). A general form of the wage (salary) equation is as follows.

$$(1) \quad S = X\beta + \theta\text{COV} + \mu$$

$$(2) \quad S = X\beta + \gamma\text{LAW} + \mu$$

where S is the natural log of teachers’ salaries or hourly earnings (including work hours outside the classroom); COV measures individual or district-level union coverage; and LAW (whose measurement we discuss subsequently) represents state-level collective bargaining law strength. X is a vector of other salary determinants and β is the corresponding coefficient vector, and μ is the error term with mean zero and assumed i.i.d. across states (s.e. are clustered by state). The specific measures of earnings, union coverage, and other variables are discussed subsequently. Ideally, we would like to identify both the separate and interactive effects of LAW and COV ; for example, to see if the impact of union coverage is dependent on the strength of a state’s collective bargaining law provisions. We provide limited analysis along these lines.

To the extent that COV and LAW are largely overlapping, it is difficult to distinguish their separate effects. Moreover, current COV and LAW may in part reflect underlying and possibly long-standing factors that influence teacher salaries. We will examine this by including recent COV and LAW measures in salary equations from years prior to teacher collective bargaining laws or coverage.

4. Data

In this section we describe the several data sources used in this study. We estimate teacher salary equations for recent years using individual teacher data from the Current Population Survey (CPS) and school district data from the Schools and Staffing Survey (SASS) for 1999-2000 (analysis of the 2007 SASS data will be included in the next version). We then describe measures of state collective bargaining law provisions and historical labor sentiment. Finally, we examine the relationship between current CB laws and unionization on teacher salaries using the Censuses of Population and the American Community Surveys (ACS) for 1950 through 2010.

Below we describe each set of data and key variables. This is followed by descriptive data showing means in Tables 1 and 2 for the CPS and in Tables 3 and 4 for SASS.

Teacher data from the CPS. Data for teachers from the CPS provide information on individual teachers for earnings, union status, location, and personal attributes, among other things. We use the CPS Outgoing Rotation Group (ORG) monthly earnings files for January 2000 through (in this version) September 2009. Our public school teacher sample is constructed by selecting employed wage and salary workers who are primary, secondary, or special education teachers (based on occupation codes),

³ An additional finding in the cross-sectional literature is that union effects for beginning teachers are close to zero, but substantial for senior teachers. Using data from the Schools and Staffing Survey (SASS), Winters (2011) finds little union effect for starting salaries, but a substantial effect for those with an MA and 20 years of experience. A principal focus of the Winters study is the role of spillover effects on salaries from unionization and pay (and other characteristics) in nearby school districts. These effects are included in his estimates.

employed at elementary or secondary schools (based on industry codes), and who are public sector workers (based on a class variable). We restrict the sample to full-time teachers, based on usual hours worked per week being at least 30. Following the additional exclusion of those with imputed earnings (see below), our estimation sample includes 30,008 teachers.

The CPS union coverage measure. An ideal measure of teacher union membership and coverage would provide at the individual level information on whether a teacher is a union member and whether she is covered by a collective bargaining agreement in her teaching job. Measures from the CPS reflect something close to this, but with error due both to misreporting and the survey frame of union questions. All employed wage and salary worker are asked: “on this job are you a member of a labor union or of an employee association similar to a union?” Those who are *not* members are asked the follow-up question: “on this job are you covered by a union or employee association contract?” Because those who are union members are not asked whether they are covered by a collective bargaining agreement, the BLS and (typically) researchers assume that covered workers include all members, plus non-members who state they are covered. There is some unknown but potentially large degree of misclassification in this coverage measure. Teachers not covered by a collective bargaining agreement but who are members of a union or association similar to a union (e.g., the National Educational Association) will be incorrectly classified as covered even if there is no misreporting in the CPS variables. Even in states that provide no legal mechanism for collective bargaining of teachers (or forbid collective bargaining), a substantial number of teachers in the CPS report that they are union members.

Additional misclassification results if teachers who are not members or who state they are not members incorrectly answer the coverage question. Such misreporting is understandable since some teachers will not be knowledgeable about the process through which their salaries are determined. An additional source of measurement error exists because in the CPS a single primary household member provides responses for all household members. Thus, roughly half of all individual records in the CPS have answers provided by (or at least through) a “proxy” respondent, most typically a spouse. It would not be surprising if proxy responses on CB coverage are measured with greater error than self-responses, even if correct on average.

Imputed earnings. An important data restriction is that we include only teachers who have their earnings reported and exclude nonrespondents whose earnings are imputed by the Census Bureau. Excluding imputed earnings avoids what otherwise can be serious “match bias” (Hirsch and Schumacher 2004; Bollinger and Hirsch 2006). Nonrespondents have earnings assigned based on the earnings of a “donor” matched based on a set of characteristics (sex, plus broad occupation, age, schooling, hours worked, and race categories), but donor match criteria do not include union status, state or metropolitan area of residence, detailed occupation, foreign born status, ethnicity, marital status, or other correlates of earnings. Failure to match on a right-hand-side wage equation variable such as union coverage, causes severe attenuation in estimated wage gap estimates (e.g., union, foreign born, industry, and city size effects), on the order of about 25% (Bollinger and Hirsch 2006). Nonresponse rates in the CPS ORGs are about 30%. Inclusion of teachers who have earnings imputed would cause substantial mismatch on union status and state of residence (hence state CB laws), two key measures in our analysis.⁴

⁴ Omitting imputed earners removes match bias, but the (far less serious) issue of nonignorable response bias remains if nonrespondents have systematically different wages than do respondents, conditional on controls. Inclusion of nonrespondents, however, does nothing to account for response bias since their imputed earnings values are based on the reported earnings of respondents. Bollinger and Hirsch (forthcoming) utilize selection model corrections to account for response bias. They conclude that such bias is modest, particularly among women.

The CPS provides individual measures of union membership and collective bargaining coverage. Earnings are measured by usual weekly earnings on the primary job divided by usual hours worked per week on that job. Weekly earnings reported during summer months by those who state their primary occupation is a public school teacher are for their academic year teaching job and match up well with earnings reported in non-summer months. The hours worked reported by teachers can include teacher work hours beyond those scheduled at school, thus likely to provide a more accurate measure of hourly earnings than does earnings divided by scheduled school hours (on this issue, see Allegretto et al. 2004). Weekly earnings are top-coded at \$2,885, leaving public school teachers virtually unaffected. In our measurement of a state wage index for all workers, top-coded earners are assigned estimated mean earnings based on year- and gender-specific estimates of mean earnings above the cap assuming a Pareto distribution in the right tail of earnings beyond the median.

Control variables included in vector X include a graduate degree beyond BA, potential experience (age–years schooling–6) and its square, gender, metro size dummies, race, ethnicity, marital status, foreign born, and, importantly, a state-specific opportunity cost wage index.

School-level data from SASS. The alternative data used to measure teacher earnings and union coverage is the school district survey of the 1999-2000 Schools and Staffing Survey (SASS) conducted by the National Center for Education Statistics (NCES) and completed by school district administrators. The SASS sample originally includes more than 4,600 school districts, about a quarter of all districts in the U.S. Our final sample containing complete data includes 4,565 districts.⁵ An important advantage of the SASS data is that it provides information on benefits. Hence we are able to provide estimates of union-nonunion differences in benefits as well as earnings.

SASS reports each school district’s salary schedule for teachers at different experience levels, with and without graduate degrees, and for some salary supplements (e.g., coaching). We use the log salary of a teacher with “average characteristics.” The SASS teacher file reports that half of all teachers have an advanced degree beyond a B.A. and mean experience of about 15 years. The SASS district file reports the salary schedule for education-experience combinations such as BA0, BA10, MA0, and MA20. To construct the salary for an “average” teacher, we compute the average salary for BA10 and MA20, with our dependent variable being $\ln((BA10+MA20)/2)$. The salary measure does not include extra pay for special credentials (apart from the MA) or extra duties. As reported in Winters (2011), teacher salaries reported in SASS exhibit greater variation among experienced than among beginning teachers. Union coverage in the SASS data is measured by an indicator variable on whether the district engages in collective bargaining, which includes about 60% of the districts in the sample (about 8% of districts have meet and confer agreements but not collective bargaining). In addition to measuring salaries and CB coverage, the wage regression includes a number of control variables, listed in table notes and discussed more fully in Winters (2011).

Opportunity cost state wage index. We are interested in how state CB laws and coverage affect teacher salaries. Wages vary across states (even conditional on worker and job attributes) owing to cost-of-living differences and area specific amenities (Roback 1982). Even were there ideal cost-of-living information, one does not expect equilibrium wages to rise fully with respect to prices unless one could

⁵ Additional data used with the SASS data are obtained from the NCES Common Core of Data (CCD), the NCES School District Demographics System (SDDS), the Bureau of Labor Statistics (BLS) Local Area Unemployment Statistics (LAUS), and the NCES Comparable Wage Index (CWI) developed by Taylor and Fowler (2006). The CWI measures the wages in the local labor market of occupations comparable to teaching based on microdata from the 2000 decennial Census and is available for nearly every district in the SASS.

fully control for area amenities. For example, amenities (e.g., good weather or near water) will raise land prices and cost of living, but may increase or decrease nominal wages since workers value living in this location. In short, equilibrium wage differences by locality reflect the tradeoff that workers make with respect to area amenities and cost of living (i.e., nominal wage differences should equate utility across markets for marginal workers so that we have an equivalent “real” wage). Absent appropriate measures for the cost of living and a full accounting for area amenities, one can best account for their joint impact by constructing a wage index that reflects the market determined nominal wage differences across localities for statistically equivalent workers (DuMond et al. 1999). An alternative way to think about such an area wage index is that it reflects the opportunity cost for individuals (e.g., teachers) in a given location to be in their current rather than some alternative occupation.

For the CPS analysis, we construct our wage index by estimating a nationwide wage equation for all wage and salary workers, ages 21-65, with a BA degree (but no graduate degree), controlling for typical wage covariates. We include in the wage equation dummy variables for all states, whose coefficients measuring relative log wage differentials. These coefficients are then used to construct the wage index. Inclusion of the state wage index is important. States that tend to have relatively weak collective bargaining law strength and low coverage levels for teachers (e.g., southern states) tend to have lower market-wide nominal wages. Failure to control for the alternative wage would cause us to overstate the positive impact of collective bargaining law and union coverage on teacher salaries. For the SASS analysis, we use a NCES Comparable Wage Index (CWI) developed by Taylor and Fowler (2006). CWI measures the wages in the local labor market of occupations comparable to teaching based on microdata from the 2000 decennial Census and is available for nearly every district in the SASS.

State public sector collective bargaining law index for teachers. State collective bargaining laws contain numerous provisions, are difficult to collect and compare across states, and cannot be readily summarized with respect to bargaining power strength or weakness. An early effort at collecting such information and then incorporating state law measures in empirical analyses was an important study by Valleta and Freeman (1988), included in a larger volume of studies on public sector unionization. Lund and Maranto (1996) provide a more recent effort using a somewhat different classification of state collective bargaining laws. The Education Commission of the States (2008) collects similar information on CB laws for teachers and publishes this information in occasional issues of *State Notes*, the most recent on in 2008.

We collected information on the provisions of state collective bargaining laws for teachers by state based on the classifications in Lund and Maranto, and revised and updated their data based on information provided in *State Notes*. State CB law indicator variables measure the duty to bargain, terminal resolution procedures, strike provisions, and right-to-work laws. What we refer to as duty to bargain is a single binary variable classifying states as either silent on (or explicitly ruling out) collective bargaining (coded zero) or specifying CB bargaining rights, often designating that a district has a duty to bargain with unionized teachers (coded one). A state’s use of terminal resolution procedures in the event of an impasse are classified using the following binary variables: silent/none; legislative resolution to override, mediation, fact finding (classified voluntary or mandatory), and arbitration. Arbitration is classified by type (standard interest arbitration or with final offer solution) and as being either voluntary or mandatory. Strike provisions are classified along the following dimensions: silent, strikes prohibited with penalties, strikes prohibited with no penalty, or strikes allowed. And finally, each state is classified as either having or not having a right-to-work law that applies to teachers.

Because state public sector CB laws are diverse and have multiple provisions, we construct a factor index of the laws based on all the unique categories of provisions in the laws. We then estimate the principal factors of these law provisions, the first three of which have eigenvalues greater than 1.0 (for a discussion of factor analysis, see Gorsuch 1983). The first factor accounts for 35.5% of the total variance in the laws, while the second and third factors account for 31.0% and 20.0%, for a cumulative total of 86.5% (by construction, each factor is orthogonal to each other, with mean zero and a s.d. of 1.0). We then combine these three factors into a single weighted CB law index, with the weights based on the relative proportions of total variance explained by each of the factors, with a mean and s.d. across the teacher weighted sample of -.01 and .53. Collapsing the state laws into a single index is for convenience in presentation. The mean of the law index is substantially lower for the non-covered than covered samples of teachers in the CPS (-.32 versus .10). The difference is much larger when comparing covered and non-covered school districts in SASS, where coverage is measured with little error as compared to the CPS. Here the mean of LAW for non-covered districts is -.49 and for covered districts .27.

Instruments for state bargaining laws and union coverage. An intended contribution of the paper had been to explicitly account for the likely endogeneity of state collective bargaining laws and the current union coverage of teachers. We would do so by selecting instruments that reflect historical measures of state-specific labor and union sentiment. The logic of the approach was that while historical union sentiment has no direct effect on current teacher salaries, voter and worker sentiment from the past affected the adoption of subsequent state bargaining laws that in turn influence current teacher coverage and salaries. We subsequently determine that the IV estimates are not plausible, but that the analysis is informative.

In our IV analysis, we use three sets of instruments. The instruments are measures of sentiment for labor based on state employment law activities in 1919 (Fishback et al. 2009), AFL-CIO “COPE” ratings of the voting records of each state’s Senate and Congressional delegations from 1965 through 1975, and state level union density in 1964. Each of these measures is described below.

A recent study by Fishback et al. (2009) provides a labor sentiment index by state for 1899, 1909, and 1919. We use their 1919 index. The index is weighted by shares of workers affected and built from information on 17 categories of regulation, from pages of laws, and from appropriations per worker on labor issues. The regulations are indexed by nonparametric “coordinate analysis” (similar to factor analysis), previously used by statisticians and political scientists to describe voting patterns. Regulation categories include workplace activities regulations (e.g., mine safety); hours regulation, women and children’s activity regulation; employer regulation and workers’ compensation; unionization legislation; payday regulations (wages in cash, frequency); holidays, and minimum wages. The labor sentiment index ranges from lows of .064 in Florida and .105 in Alabama to highs of .586 in New York and .638 in Massachusetts, with an unweighted mean across states of .35 and weighted mean across our sample of teachers of .37 (.30 for the non-covered and .40 for the covered samples).

The AFL-CIO’s Committee on Political Education (COPE) provides ratings for each state’s Congressional delegation, measuring the percentage of votes by each Senator and Representative in a given year agree with organized labor’s position on selected bills (votes are classified as R or W for right or wrong). These ratings are published in the CQ Almanac (various years). We construct an index measuring the percentage of “right” votes for each state’s delegation averaged over the years 1965 and 1967-1975. Each state’s average is based on the equal-weighted average of its Senate and House delegation scores. The mean COPE score across our CPS sample-weighted average is 53.9%, indicating 54% of Congressional votes were in accordance with the position taken by the AFL-CIO. As expected the

mean COPE rating across the sample of non-covered teachers is lower than for the covered teachers, 42% versus 58%. Across the sets of states classified as having weak, moderate, and strong CB laws, the COPE means are 38%, 57%, and 63%, respectively.

A third instrument is the level of state unionization prior to the large growth of teachers' unions and, more generally, the widespread adoption of public sector labor laws and rapid growth in coverage. We utilize a series of union density by state in 1964 among all nonagricultural workers, available in a *Monthly Labor Review* study by Hirsch, Macpherson, and Vroman (2001), based on union reported data from the *Directory of National and International Unions*, BLS 1965, made time consistent with later household-based CPS figures. Mean union density across the U.S. was 29.3%, with lows of 7.0% in South Carolina and 8.4% in North Carolina and highs of 44.5% in Washington and 44.8% in Michigan.

In first stage results (not shown), each of the three instruments, the 1919 labor sentiment index, the COPE voting records, and the 1964 state union density, is a highly significant determinant of the state law index, LAW, and individual level union coverage, COV, conditional on other covariates. When all three are included, they are jointly significant, with state union density being the most highly significant and the 1919 state labor sentiment index being most weakly related (and not significant). Tests for the exogeneity of LAW and COV are clearly rejected and the Hansen J test of the overidentification restriction is not significant, indicating that we cannot reject the hypothesis that the instruments are valid.

Historical data from Censuses of Population and American Community Surveys.

While historic sentiment with respect to labor is prior to and helps predict eventual collective bargaining laws and unionization, past sentiment may also reflect current worker/voter sentiment that has a direct effect on current salaries, independent of collective bargaining per se. This very real possibility makes it likely that our IV estimates overstate union salary effects. This interpretation is consistent with a literature suggesting that unionization is an "experience good" (Gomez and Gunderson 2004) and that areas of the country with historical high levels of labor support and unionization (say unionized steel mills) are more likely to have relatively higher current unionization of hospitals, grocery stores, and the like (Holmes 2006).

In the end we disregard our IV results and move in a different direction. Rather than use historical measures of sentiment to produce independent variation in current collective bargaining, we instead use current measures of collective bargaining and examine how these are related to teacher salary differences prior to collective bargaining. If teacher salaries were higher (lower) in markets that eventually adopted collective bargaining, then standard OLS measures of current union salary effects for teachers are overstated (understated).

To provide such an analysis, we use household data allowing us to measure how teacher salaries varied across states prior to the passage of state collective bargaining laws and teacher unionization. We use microdata from the 1960-2000 decennial censuses and the selected years after 2000 from the American Community Surveys (ACS), all available from the Integrated Public Use Microdata Series (IPUMS) produced and distributed by Ruggles et al. (2010). Each sample used is a nationally representative one percent sample of the U.S. population. Our sample includes full-time public school teachers. Data for non-teachers are used to construct area wage measures. Earnings are measured by annual wage and salary earnings during the previous calendar year. For the decennial censuses, the reference period for earnings is the previous calendar year, e.g. earnings in the 2000 Census are for the 1999 calendar year. The ACS is conducted throughout the year and the reference period for earnings is the previous 12 months prior to the survey.

5. Descriptive evidence

In what follows, we first focus on results from the CPS, which include nationwide data on individual teachers. Similar descriptive data are reported for the school district SASS data. Teacher salaries differ substantially by union status, although little can be said about causal effects absent additional analysis. As seen in Table 1, teacher wages during the decade 2000-2009 averaged \$23.97 in 2009 dollars. There was a substantial difference in mean hourly earnings for covered and non-covered teachers, \$25.48 versus \$19.76 (a 28.9% unadjusted arithmetic union advantage and 28.4% based on the mean log differential). To see how salaries vary based on the strength of state collective bargaining laws, in Table 2 we divide the CPS teacher sample into three groups of states, 15 “weak strength” states with very low factor analysis index scores, 19 with “moderate” strength scores, and 14 with “strong” scores on bargaining strength. In the 15 low strength states, mean hourly earnings are substantially lower than the national average, \$20.63 versus \$23.97, as is union coverage, which is “only” 48.5% (many if not most of these teachers are members but not in fact covered by a collective bargaining agreement). In the middle and high strength states, average earnings are similar, \$25.15 and \$25.36, while union coverage rates are 79.8% and 87.1%, respectively. The key dividing line appears to be between states that are silent on collective bargaining rights for teachers (the “weak strength” group) and states that delineate CB rights.

Apart from the descriptive data on teacher salaries, Tables 1 and 2 provide CPS means on several of the conditioning variables and the three historical union sentiment instruments. Union as compared to nonunion teachers are more likely to have graduate degrees (52.3% vs. 37.6%), tend to be older (19.4 vs. 18.0 years of potential experience), are more highly concentrated in large metropolitan areas (19.5% of union vs. 6.1% of nonunion teachers reside in metro areas with populations greater than 5 million), and are slightly less likely to be nonwhite and Hispanic. Also noteworthy (Table 2) is the .07 higher opportunity cost log wage variable in strong versus weak CB LAW states, showing the importance of controlling for alternative wages. Focusing on the historical measures of sentiment, covered teachers are more likely to reside in states with strong labor sentiment in 1919, higher AFL-CIO COPE Congressional vote ratings during 1965-1975, and higher levels of state union density in 1964.

Descriptive data from SASS are shown in Tables 3 and 4. The difference in mean log salaries between covered and non-covered districts is .159 log points or 17.2% (or in dollars, \$6,146 or 18.6%). The smaller difference in contractual SASS salaries and CPS hourly earnings is likely because SASS salary measures reflects the same seniority across all districts and covered districts have more senior teachers than non-covered districts. In the CPS, particularly in states with little collective bargaining, more senior and higher paid teachers may be substantially more likely to report being a member of a union or employee association than are relatively new teachers. For variables that are largely comparable in the CPS and SASS, we see similar patterns. Although we do not have individual teacher attributes in our SASS analysis, we do measure district level teacher and student attributes, as shown in Tables 3-4.

6. Union coverage effects on teacher salaries

We now turn to the results of our statistical analysis, showing results using CPS individual teacher data, the SASS district level data, and historical Census/ACS data. Results shown in Table 5 begin with simple models examining union salary differentials associated with union coverage (COV) and, separately, state CB law strength (LAW). Included as control variables in the CPS analysis are the log of the opportunity cost alternative wage in the state, graduate degree, potential experience and its square, race, ethnicity, marital status, foreign-born, and six metropolitan area size dummies (with very small metros and non-metro areas being the omitted base group). Included as controls in the SASS district

analysis are the CWI opportunity cost index, six metro size dummies, the number of days in the school year, the log of district enrollment, the student-teacher ratio, the percentage of teachers who teach secondary grades, the percentage of teachers who are Black, Hispanic, Asian, and American Indian, the percentage of students who are Black, Hispanic, Asian, and American Indian, and the percentage of students who are eligible for free or reduced lunch (the base results are highly similar including just the CWI index and metro size dummies).

Turning first to the CPS, using OLS we obtain a union log salary differential of .124 (13.2%). Examining instead the salary effect of the CB law index (LAW), we obtain a small (.019) and statistically insignificant coefficient. Since the LAW index has a standard deviation of .53, the .019 coefficient reflects only a 2 percent salary difference (all else constant) moving from states that are silent on or ban collective bargaining to those who have among the most favorable bargaining laws. Using SASS district level data, the OLS coverage variable is .104, similar to the CPS result, while the LAW effect is .096, highly similar to the SASS coverage result but far higher than the CPS LAW coefficient estimate.

We expect both COV and LAW to be endogenous (exogeneity is statistically rejected). The IV results use GMM; 2SLS results are highly similar. IV estimates point to higher union effects from teacher coverage and from state bargaining law strength. The COV coefficient almost doubles moving from OLS to IV, from .124 to .233 (26.2%). Were we looking solely at the CPS results, we would assume that much of the increase in moving to IV results from correction to CPS measurement error in individual level COV (causing attenuation in the OLS estimate). Using SASS district level data, however, where coverage is measured with far less error, we likewise find an approximate doubling of the COV coefficient. If anything, we would have expected endogeneity to bias upward rather than downward a precisely measured coverage coefficient. Hence, we are reluctant to interpret the IV results as causal, or at least not an unbiased measure of the average treatment effect. We return to this point below.

Moving from OLS to IV likewise results in a substantial increase in the estimated effect of state CB laws, with the IV coefficient on LAW using the CPS data now being significant and a nontrivial .127 log wage difference, all else the same, between states with strong versus weak CB laws. Using the SASS data, the LAW coefficient doubles from .096 to .210. Using the SASS, COV status and the LAW index appear to be capturing largely the same teacher wage effects.

Our interpretation of the much larger estimates of union salary using IV than OLS is that differences in COV or LAW generated by our labor sentiment instruments reflect differences in bargaining power and opportunities across states, those states with strong labor sentiment providing political and economic settings relatively most conducive to awarding unionized teachers relatively high salaries. That is, the same historical sentiment for unions captured by our instruments lingers today and produces variability in predicted coverage and bargaining laws associated with higher teacher salaries.

In work not shown, we attempt to distinguish between the effects of CB laws and CB coverage (LAW and COV). It is difficult to separate these in a precise way, but our analysis clearly points to a stronger effect for COV than for LAW and little effect for LAW after accounting for COV. Our interpretation is that COV is a key determinant of teacher salaries, but that a collective bargaining legal regime (LAW) is a necessary and important determinant of union coverage. In addition, the legal regime may reflect sentiment toward labor and education, thus being correlated with salaries independent of whether there exists CB coverage. Even in states without collective bargaining rights, many teachers belong to the NEA, some of these states have formalized meet and confer arrangements, and teachers may have strong lobbying abilities and political influence outside of collective bargaining.

A final extension of our basic result concerns differential effects union coverage may have on the salaries of beginning and experienced teachers. Using the SASS data, Winters (2011) concludes that unions have little if any effect on starting teacher salaries, but do have effects throughout the rest of the seniority distribution. He interprets this finding in terms of a median voter model in which experienced teachers have the most influence on union bargaining goals, as well as on the need for non-covered school systems to pay starting salaries reasonably similar to those in covered districts in order to compete for teachers at the margin during the period when they are most mobile.⁶ Table 6 uses the SASS data to roughly replicate Winters' finding, showing that the OLS coefficient on COV for beginning teachers (i.e., those with a B.A. and zero experience) is effectively zero (0.01 and a t-ratio of 0.56). By contrast, senior teachers (those with an M.A. and 20 years experience) have an OLS coefficient on COV of 0.12 (with t ratio 6.08). When we extend the analysis using our IV approach, the beginning teacher salary estimate is roughly 5% (.056) and significant (t of 1.96). As expected based on our prior results, for senior teachers the IV estimate is roughly double the OLS estimate, rising from .12 to .24. Estimates using LAW rather than COV are similar, with the IV estimates being .05 and .21 for beginning and experienced teachers, respectively. In short, collective bargaining coverage may modestly increase starting teacher salaries, but by far less than for experienced teachers.

7. Teacher Collective Bargaining and Nonwage Benefits

The CPS monthly earnings files do not include measures of nonwage benefits. The March CPS provides information on the offer and take-up of employer provided health insurance and pensions, but not their dollar value.⁷ One can examine expenditures by employers in the National Compensation Survey's Employer Costs for Employee Compensation (ECEC) program (US BLS, 2011). The ECEC provides breakdowns for some large occupations, including public sector teachers, but they do not differentiate between teachers covered and not covered by union contracts or public sector workers by region.

The 2000 SASS, however, does provide information on the "benefit rate for teachers as a percentage of payroll" that includes "district contributions on behalf of employees for Social Security and other payroll taxes; retirement; medical, dental, disability, unemployment, and life insurance; and all other fringe benefits" (NCES 2000, p. 33).⁸ This variable measures total district expenditures on teacher benefits as a percentage of total teacher salaries in the district. West and Mykerezi (2011) estimate that the SASS benefits ratio is greater in unionized districts by 4.2 percentage points, but do not consider the effects of unions on total compensation. Cowen (2009) uses the NCES Common Core of Data (CCD) to look at the effects of collective bargaining coverage on total district expenditures on teacher salaries and teacher compensation for a subset of states. He finds that coverage increases expenditures on teacher salaries and total compensation by roughly equal percentages in those states.

⁶ Such results are also consistent with school district monopsony power for experienced but not beginning teachers. If this is the case, our results could be explained by monopsony power being exercised against experienced teachers in nonunion districts, but offset by unions in districts with collective bargaining.

⁷ The March CPS provides information for all rotation groups for the principal job during the previous calendar year. The March survey has the disadvantage of less precise earnings measures than the outgoing rotation groups (ORG) and sample sizes that would be roughly one-third as large as the ORGs (one month of all rotation groups versus twelve months of a quarter sample).

⁸ A few districts report a zero value. The SASS benefits data display considerably higher variance than does the CCD measure, but they have nearly identical mean ratio values. Using the CCD, we obtain highly similar results to those shown in Table 8.

We use the SASS benefit rate variable to estimate the value of benefits, which in turn are used to estimate union-nonunion differentials in benefits and total compensation in a regression framework analogous to those seen for wages. We measure average teacher benefits by the average salary times the benefit rate, and total compensation as the average salary times one plus the benefit rate. As seen previously, the average salary is measured as the arithmetic average of the base salary for teachers with a bachelor's degree and 10 years of teaching experience and of teachers with a master's degree and 20 years of experience, i.e., $(BA10+MA20)/2$. We then take the natural log of both the average benefits and total compensation. The log benefits and log compensation regressions include the same variables as in the Table 5 SASS regressions. One caveat is that in the SASS analysis there is not control for differences in teacher experience. Union teachers are likely to be older and more experienced on average than are nonunion teachers, which may be associated with a higher benefit ratio even given an identical schedule of benefits.⁹

The first two columns of Table 7 reproduce the SASS log salary results previously shown in Table 5. The next two provide estimates from log benefit regressions and the final two from log compensation regressions. The OLS estimate of union-nonunion gap in benefits is 0.306, roughly triple the effect on salaries. Combining salaries and benefits, we obtain an estimate of the OLS union compensation gap of 0.146, about .04 log points higher than the union wage gap of 0.104. OLS estimates using LAW rather than COV are qualitatively similar to those using COV (recall that the s.d. of COV is close to 1). When we move to IV, union gap estimates substantially exceed those using OLS. The union compensation gap of 0.271 using IV is about .05 log points higher than the 0.220 IV salary gap estimate. In short, there is clear evidence that the union compensation gap exceeds that for salary owing to substantially higher benefits for teachers in union than in nonunion districts.

8. Was there a “Union” Premium in Teacher Salaries Prior to Collective Bargaining?

In order to help understand whether estimated union effects are causal, we examine whether there existed a “union” salary premium in states with teacher collective bargaining *prior to* such bargaining. The first state collective bargaining law was adopted in Wisconsin in 1959. Data from the 1950 and 1960 Censuses of Population, which measure annual earnings in the previous calendar year (1949 and 1959, respectively), should not reflect any direct effect on teacher salaries from collective bargaining. If we find our measures of unionization (the current state collective bargaining law index, LAW, and the extent of CB coverage by state for the 1999-2000 school year) associated with teacher salary premiums in 1960 similar to those seen in years following collective bargaining, it would imply that observed union salary differentials are not causal, consistent with findings in Lovenheim (2009) and Frandsen (2011). If the pre-CB effects were, say, zero (or half as large), it would suggest that all (or half) of the estimated CB effects using recent data are causal. Although our estimates are not precise, the results presented below suggest that a good share (perhaps half) of the standard OLS union premium for teachers reflects pre-existing higher salaries in states that eventually adopted collective bargaining, while the remaining share (perhaps

⁹ As seen in Table 1 for the CPS sample, covered teachers have 1½ more years of potential experience than do nonunion teachers. Older teachers may have higher health insurance costs, although they also have higher salaries, leaving the effect on the benefit ratio indeterminate. Defined benefit pension costs should increase with tenure. Turnover, particularly among non-vested employees, will lower costs. In preliminary work not shown, we find no systematic relationship between the 2000 level of benefits by state and current levels of teacher pension underfunding by state. There is correlation between our measures of unionization by state and the level of underfunding, but these are weak in magnitude and statistical significance.

half) reflects the effect of collective bargaining. In short, unions and collective bargaining do appear to have substantive causal effects on teacher salaries, but the magnitude is small.¹⁰

Table 8 provides state-level regression estimates of teacher salary differences from the Censuses of Population for 1950, 1960, 1970, 1980, 1990, and 2000, plus similar estimates from the ACS for 2007 and 2010 (before and toward the end of the Great Recession). We include the 48 contiguous states. The Census long-form surveys measure the calendar year prior to the survey, while the ACS measure the 12 months prior to each survey. There should be no effect of CB laws and collective bargaining in the 1950 and 1960 Censuses. Seven states had passed CB laws by 1968 (two of these in 1968) and an additional eight adopted in 1969. So one should observe a rather limited direct effect in 1969 (the 1970 Census). By 1979 (the 1980 Census) one should observe close to a full impact of collective bargaining, with a total of 28 states adopting laws by 1978 (all but one of these by 1975; Alaska, Hawaii, and D.C., who also adopted laws in the early 1970s, but are not in our estimation sample). Only three additional states adopted laws after 1978, so it's reasonable to treat the 1980 Census as the first year of data in which we should observe a "full" collective bargaining effect.¹¹

Table 8 provides regression results for teacher log salary regressions using the 48 contiguous states. We regress the state mean of log salaries for full-time public school teachers on the two union measures without controls and a second that controls for a state's log salary level for full-time, full-year, private sector, wage and salary workers. Given the large differences in wage levels and cost of living across the U.S. and relative changes over this sixty year period, we attach little weight to estimates not controlling for state salary levels. We provide coefficients for two different measures, the collective bargaining LAW index previously used and a measure we compiled from SASS measuring the (weighted) share of each state's school districts that have a collective bargaining agreement in 1999-2000.¹²

The (left-side) estimates in Table 8 absent control for state wage levels are not informative regarding the effects of collective bargaining. The coefficients instead reflect what were enormous historical wage and income differences between states that would not adopt public sector collective bargaining (many of these in the South) and those states that eventually would adopt. Changes in the coefficients over time appear to primarily reflect the large narrowing in wages and income during the twentieth century between states, in particular the well-known narrowing of north/south differences.

The (right-side) estimates that include a state wage level index are informative. Treating 1960 as our base year, we see that prior to public sector collective bargaining there were non-trivial union earnings differentials prior to collective bargaining (given the sample size of 48, standard errors are large). The point estimates indicate a 5 percent (log point) increase in teacher salaries associated with a roughly 2 standard deviation difference in the (eventual) CB law index and an 8 percent increase in teacher salaries associated with a change from zero to (an eventual) 100 percent CB coverage. By 1969, seven states had

¹⁰ An early use of this empirical strategy can be seen in Lumsden and Petersen (1975), who estimate the effect of right-to-work laws (RTW) on state union density. They regress union density in 1939, 1953, and 1968 on controls and a RTW dummy equal to one for states that would adopt RTW by 1968. They find large negative coefficients, similar in magnitude for all three years (but most negative in 1939), even though no state had a RTW law in 1939 (Taft-Hartley made RTW an option in 1947). They conclude that the union density/RTW relationship is not causal.

¹¹ In future analysis, we intend to examine the effect of years since CB law passage.

¹² The weighting accounts for each district's number of FTE teachers and for SASS sample weights that make the school district samples representative at the state level. Our preference is to use a micro-level rather than aggregated state regression for this analysis. We have estimated individual teacher level wage equations with age and education controls, but have not yet developed within-state indices for non-teacher salaries. Our preliminary analysis leads to conclusions similar to those stated in this draft.

passed CB laws (two of these in 1968), so one might expect to observe partial but not full effects from eventual unionization. Both the CB law index and CB coverage coefficients increase from their 1959 values of .05 and .08 to values of .08 and .10 in 1969. By 1979 (the 1980 Census) all but three of the eventually adopting CB law states had already done so. Thus we treat the 1960 to 1980 change as our best estimate of the (arguably) causal effects of collective bargaining. The coefficients on the CB law index had increased from .054 to .125 between the 1960 and 1980 Census. And the coefficients on state teacher coverage in the 1999-2000 academic year increased from .082 to .168. Thus, our tentative conclusion based on these initial results is that roughly half the standard cross-sectional union-nonunion differential in teacher salaries is likely to be causal, while roughly half reflects state sentiment and other factors that led to higher teacher salaries independent of (but correlated with) unionization.

Moving forward to later years provides suggestive results that require further examination. The union-nonunion wage advantages seen in the 1980 Census are substantially lower in the 1990 and 2000 Censuses and in the 2007 ACS, just prior to the Great Recession. One possible explanation is that teacher unions' initial bargaining power could not be sustained. A more convincing explanation is that over time teachers in non-unionized school districts (states) throughout the country had sufficient political power and public support to narrow salary differentials with their counterparts in states with collective bargaining.¹³ Finally, the increase in union-nonunion teacher salary differences between 2007 and 2010 is likely to reflect the Great Recession, with unionized teachers having CB contracts and possible political sway that made it relatively difficult (but far from impossible) to postpone or eliminate contractual salary increases. In contrast, state legislatures and local authorities can more easily freeze salaries (or mandate furloughs) where they do not face contractual constraints. An alternative explanation might be that there existed differential impacts of the recession in states with and without teacher collective bargaining, something that we can easily examine (in a future version).

9. Conclusion

Despite the importance of collective bargaining among public school teachers, it has been difficult to estimate the causal impact of collective bargaining laws and union coverage on teacher salaries. The difficulty stems from several factors, including limited nationwide collective bargaining and salary data for school districts, measurement error in individual-reported coverage data from the CPS, and the endogeneity and joint determination of state public sector collective bargaining laws and teacher coverage, both being influenced by historical and current union sentiment. Whereas standard OLS estimates of covered-noncovered teacher salary differences systematically find substantive effects, albeit lower than are union gaps in the private sector, recent studies examining salary changes before and after teacher collective bargaining tend to find zero or small union salary and benefit effects.

In this paper, we attempt to address these issues in several ways. We first create indices of CB law strength using factor analysis and provide alternative measures of CB coverage using the CPS and SASS. In addition to providing "standard" OLS estimates of the salary effects of CB laws and coverage, which we find to be about 10%, we also estimate the effects on nonwage benefits. These are found to be larger than for salaries, leading to a union compensation advantage about 4 percentage points higher than the salary advantage.

¹³ That said, we do not find such a systematic decline in union coefficients over time when we use the CPS to estimate annual teacher wage equations (these results not shown).

How much of the estimated union effect is causal? We initially “fail” at our attempt to account for the endogeneity of state laws and coverage using three instruments for unionization that measure historical state-level labor sentiment. Rather than decreasing estimated union effects, the IV estimates are roughly double the OLS estimates. We regard these as implausible and speculate that the variation in coverage produced by these instrument reflect variation in union coverage resulting from historic labor sentiment. Such sentiment not only led to passage of CB laws, but might also have produced higher teacher salaries even in the absence of collective bargaining.

To address the possibility that states adopting collective bargaining would have had higher teacher salaries in its absence, we turn to historical teacher salary data using the 1950 through 2000 Censuses of Population long-form and the ACS for 2007 and 2010. We examine whether union salary effects existed in states adopting strong public collective bargaining laws *prior to* passage of these laws and teacher collective bargaining. We focus in particular on the 1960 and 1980 Censuses, since 1959 earnings measured in the former should not be directly influenced by subsequent CB laws or coverage, and by 1979 all but a few states that would eventually adopt CB laws had already done so. Hence, any changes in CB law and coverage coefficients between 1959 and 1979 arguably reflect causal union effects.

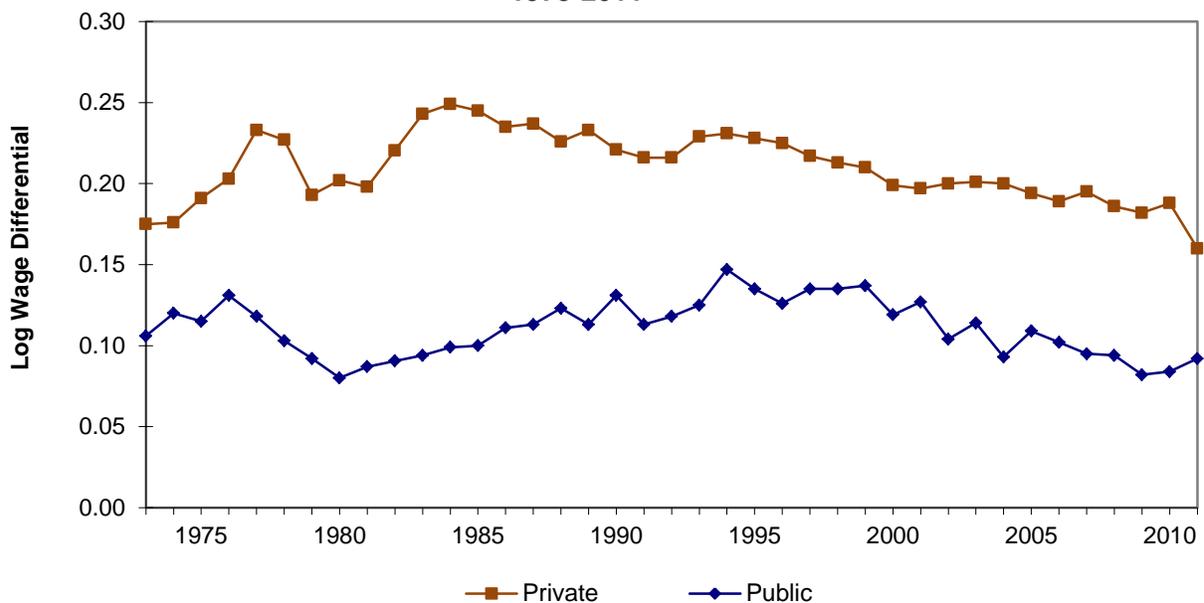
Although our analysis is preliminary, our initial evidence suggests that states with CB laws and coverage for teachers displayed higher teacher salaries prior to these laws and coverage being adopted. This indicates that some proportion (which we tentatively estimate to be roughly half) of the union salary advantage seen for teachers is not causal, reflecting instead sentiment for labor/education or some other unmeasured factors associated with higher salaries. That said, the remaining half of the salary advantage for unionized teachers appears to be causal, stemming from collective bargaining laws and coverage. The magnitude is not large, perhaps in the neighborhood of a 5 percent effect. We do not have pre-1960 information on teacher benefits, so we cannot provide a similar analysis with respect to benefits. Given the magnitude of the union benefit advantage, it would be surprising were not a meaningful share of these benefits a causal result of collective bargaining.

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Fig. 5: Private and Public Sector Union Wage Premiums, 1973-2011



1983 estimates:	private .243	public .094
1993 estimates:	private .229	public .125
2003 estimates:	private .201	public .114
2011 estimates:	private .160	public .092

Source: B.T. Hirsch & D.A. Macpherson, Union Membership and Earnings Data Book, BNA, 2011.

Table 1
CPS Means by CB Coverage Status

	All		Non-covered		Covered	
	Mean	S.D.	Mean	S.D.	Mean	S.D.
Dependent variable:						
Log Real Wage	3.08	0.45	2.90	0.44	3.15	0.43
Wage (2009\$)	\$23.97	\$12.03	\$19.76	\$9.41	\$25.48	\$12.49
Instruments:						
1919 State Labor Sentiment Index	0.37	0.14	0.30	0.12	0.40	0.14
COPE Score, 1965-1975 State Average	53.94	20.37	42.41	18.33	58.07	19.45
1964 State Union Membership rate	26.28	9.97	19.78	8.93	28.61	9.27
CB laws and coverage:						
State Collective Bargaining Law Index (LAW)	-0.01	0.53	-0.32	0.48	0.10	0.51
Covered by CB contract (COV)	0.736	0.441	0.000	0.000	1.000	0.000
Independent variables:						
Opportunity cost log wage index	-0.14	0.07	-0.17	0.06	-0.13	0.07
Graduate Degree	48.41%	49.98%	37.62%	48.45%	52.28%	49.95%
Potential Experience	19.02	10.48	17.98	10.63	19.39	10.40
Female	73.58%	44.09%	73.73%	44.01%	73.53%	44.12%
CBSA Size:						
100,000 to 249,999	7.41%	26.19%	7.45%	26.25%	7.39%	26.17%
250,000 to 499,999	9.12%	28.79%	9.76%	29.68%	8.89%	28.46%
500,000 to 999,999	9.49%	29.31%	10.16%	30.22%	9.25%	28.98%
1,000,000 to 2,499,999	15.60%	36.29%	15.03%	35.74%	15.80%	36.48%
2,500,000 to 4,999,999	25.80%	43.76%	16.46%	37.08%	29.15%	45.45%
5,000,000 and up	15.95%	36.61%	6.07%	23.88%	19.49%	39.61%
Nonwhite	7.82%	26.85%	9.68%	29.58%	7.15%	25.77%
Hispanic	4.32%	20.34%	4.94%	21.68%	4.10%	19.83%
Marital Status:						
Married Spouse Present	71.10%	45.33%	72.02%	44.89%	70.77%	45.48%
Divorced, Widowed, or Separated	12.65%	33.24%	11.68%	32.12%	12.99%	33.62%
Immigrant Status:						
Foreign Born, Citizen	2.03%	14.10%	1.61%	12.57%	2.18%	14.61%
Foreign Born, Non-Citizen	0.92%	9.56%	1.14%	10.61%	0.85%	9.16%
N	30,008		7,910		22,098	

Notes: The main data source is the Current Population Survey Outgoing Rotation Group from January 2000 to September 2009. The sample is restricted to employed wage and salary workers who are primary, secondary, or special education teachers, employed at elementary or secondary schools, and who are public sector workers. We restrict the sample to full-time teachers, based on usual hours worked per week being at least 30. Workers with imputed earnings are deleted from the sample.

Table 2:
CPS Means by State Collective Bargaining Law Strength Index

	Low Index		Medium Index		High Index	
	Mean	S.D.	Mean	S.D.	Mean	S.D.
Dependent variable:						
Log Real Wage	2.96	0.37	3.12	0.48	3.14	0.45
Real Wage (December 2009\$)	\$20.63	\$8.43	\$25.15	\$13.56	\$25.36	\$12.15
Instruments:						
1919 State Labor Sentiment Index	0.27	0.10	0.39	0.17	0.44	0.07
COPE Score, 1965-1975 State Average	38.34	17.03	56.86	19.65	63.31	15.92
1964 State Union Membership rate	17.30	6.93	27.12	9.37	32.62	6.92
CB laws and coverage:						
State CB Law Index (LAW)	-0.66	0.08	-0.08	0.22	0.58	0.27
Covered by CB contract (COV)	0.485	0.500	0.798	0.402	0.871	0.335
Independent variables:						
Opportunity cost log wage Index	-0.18	0.04	-0.14	0.08	-0.11	0.07
Graduate Degree	43.32%	49.55%	52.87%	49.92%	47.54%	49.94%
Potential Work Experience	18.46	10.26	19.07	10.56	19.42	10.54
Female	76.77%	42.23%	73.20%	44.29%	71.42%	45.18%
CBSA Size:						
100,000 to 249,999	7.53%	26.38%	7.05%	25.61%	7.71%	26.68%
250,000 to 499,999	10.59%	30.77%	8.80%	28.33%	8.28%	27.55%
500,000 to 999,999	9.36%	29.13%	11.79%	32.25%	7.02%	25.56%
1,000,000 to 2,499,999	15.91%	36.58%	17.27%	37.80%	13.46%	34.13%
2,500,000 to 4,999,999	17.12%	37.67%	25.08%	43.35%	33.66%	47.26%
5,000,000 and up	3.33%	17.94%	16.13%	36.78%	25.99%	43.86%
Nonwhite	11.22%	31.56%	7.27%	25.97%	5.68%	23.16%
Hispanic	5.46%	22.73%	3.51%	18.39%	4.31%	20.32%
Marital Status:						
Married Spouse Present	72.15%	44.83%	71.31%	45.23%	70.01%	45.82%
Divorced, Widowed, or Separated	13.39%	34.06%	11.92%	32.41%	12.86%	33.47%
Immigrant Status:						
Foreign Born, Citizen	1.18%	10.81%	2.36%	15.17%	2.35%	15.14%
Foreign Born, Non-Citizen	0.93%	9.59%	0.88%	9.33%	0.97%	9.79%
N	8,290		11,497		10,221	

See text for description of the state collective bargaining law strength index, LAW.

Low strength CB LAW index states are: Alabama, Arizona, Arkansas, Colorado, Georgia, Kentucky, Louisiana, Mississippi, Missouri, North Carolina, South Carolina, Texas, Virginia, West Virginia, and Wyoming.

Medium strength CB LAW index states are: Connecticut, Delaware, Florida, Indiana, Iowa, Maryland, Massachusetts, Michigan, Minnesota, Nebraska, Nevada, New Mexico, New York, North Dakota, Oklahoma, Rhode Island, South Dakota, Tennessee, and Utah.

High strength CB LAW index states are: California, Idaho, Illinois, Kansas, Maine, Montana, New Hampshire, New Jersey, Ohio, Oregon, Pennsylvania, Vermont, Washington, and Wisconsin.

**Table 3:
SASS Means by CB Coverage Status**

	All		Non-covered		Covered	
	Mean	S.D.	Mean	S.D.	Mean	S.D.
Dependent variable						
Log Average Salary	10.543	0.199	10.444	0.139	10.603	0.205
Average Salary	38677	7980	34672	4834	41118	8516
Instruments						
1919 State Labor Sentiment Index	0.374	0.137	0.277	0.108	0.434	0.118
COPE Score, 1965-1975 State Average	53.406	20.310	40.371	17.281	61.353	17.755
1964 State Union Membership rate	26.435	10.277	18.250	7.013	31.425	8.619
CB laws and coverage						
State Collective Bargaining Law Index (LAW)	-0.019	0.531	-0.494	0.344	0.270	0.401
Covered by CB Contract (COV)	0.621	0.485	0.000	0.000	1.000	0.000
Independent variables						
Log Comparable Wage Index (CWI)	-0.119	0.124	-0.152	0.101	-0.099	0.132
Days in School Year	178.698	5.944	177.910	5.601	179.178	6.095
Log Enrollment	7.691	1.477	7.639	1.534	7.722	1.441
Student-Teacher Ratio	15.127	5.000	14.314	3.245	15.622	5.760
% Teaching Secondary Grades	39.46%	17.26%	39.43%	13.67%	39.48%	19.12%
% of Teachers Hispanic	4.04%	14.38%	4.95%	16.20%	3.48%	13.12%
% of Teachers Black	4.24%	11.25%	7.77%	15.51%	2.09%	6.69%
% of Teachers American Indian	1.14%	5.92%	1.58%	6.98%	0.87%	5.16%
% of Teachers Asian	0.54%	2.83%	0.41%	3.76%	0.62%	2.07%
% of Students Hispanic	8.82%	17.64%	9.95%	20.08%	8.13%	15.94%
% of Students Black	9.80%	19.06%	16.41%	24.78%	5.76%	12.95%
% of Students American Indian	3.66%	13.39%	4.76%	16.00%	2.99%	11.47%
% of Students Asian	1.70%	4.23%	0.81%	2.51%	2.24%	4.91%
% of Students with Free or Reduced Lunch	39.82%	25.07%	49.47%	24.05%	33.93%	23.83%
MSA Size:						
100,000 to 249,999	8.00%	27.13%	8.91%	28.49%	7.44%	26.25%
250,000 to 499,999	6.00%	23.76%	6.07%	23.89%	5.96%	23.68%
500,000 to 999,999	5.52%	22.84%	5.78%	23.35%	5.36%	22.53%
1,000,000 to 2,499,999	8.85%	28.41%	5.96%	23.68%	10.61%	30.81%
2,500,000 to 4,999,999	5.63%	23.05%	6.42%	24.52%	5.15%	22.10%
5,000,000 and up	14.22%	34.93%	3.93%	19.44%	20.49%	40.37%
N	4,565		1,729		2,836	

Notes: Data are primarily from the 1999-2000 Schools and Staffing Survey (SASS) district level file. Dependent variable is the log salary for a teacher with average characteristics.

Table 4:
SASS Means by State Collective Bargaining Law Strength Index

Variable	Low Index		Medium Index		High Index	
	Mean	S.D.	Mean	S.D.	Mean	S.D.
Dependent variable						
Log Average Salary	10.468	0.124	10.541	0.241	10.608	0.181
Average Salary	35433	4359	38961	9717	41144	7540
Instruments						
1919 State Labor Sentiment Index	0.260	0.104	0.410	0.152	0.437	0.072
COPE Score, 1965-1975 State Average	38.257	17.568	55.815	18.178	63.889	16.437
1964 State Union Membership rate	17.513	6.416	27.143	10.143	33.292	6.927
CB laws and coverage						
State Collective Bargaining Law Index (LAW)	-0.655	0.082	-0.060	0.203	0.557	0.266
Covered by CB Contract (COV)	0.043	0.203	0.806	0.395	0.932	0.252
Independent variables						
Log Comparable Wage Index (CWI)	-0.138	0.099	-0.132	0.128	-0.091	0.134
Days in School Year	177.964	5.877	178.873	6.156	179.149	5.735
Log Enrollment	8.055	1.406	7.502	1.484	7.564	1.476
Student-Teacher Ratio	14.580	3.016	14.553	3.661	16.141	6.930
% Teaching Secondary Grades	39.62%	12.02%	40.97%	13.83%	37.87%	22.92%
% of Teachers Hispanic	4.70%	14.95%	3.21%	13.46%	4.27%	14.71%
% of Teachers Black	9.78%	16.88%	2.02%	6.33%	1.69%	6.23%
% of Teachers American Indian	0.57%	4.58%	2.17%	8.21%	0.62%	3.80%
% of Teachers Asian	0.45%	4.01%	0.33%	1.94%	0.83%	2.28%
% of Students Hispanic	10.84%	20.75%	5.86%	13.48%	9.95%	17.96%
% of Students Black	20.42%	26.46%	5.52%	12.55%	4.94%	11.86%
% of Students American Indian	1.69%	9.69%	6.65%	17.51%	2.45%	10.78%
% of Students Asian	0.89%	2.68%	1.27%	2.77%	2.80%	5.89%
% of Students with Free or Reduced Lunch	49.15%	23.50%	36.72%	24.18%	34.92%	25.08%
MSA Size:						
100,000 to 249,999	9.53%	29.38%	6.77%	25.14%	7.87%	26.94%
250,000 to 499,999	6.99%	25.50%	5.50%	22.80%	5.66%	23.11%
500,000 to 999,999	5.90%	23.56%	5.50%	22.80%	5.23%	22.27%
1,000,000 to 2,499,999	6.84%	25.25%	11.18%	31.52%	8.30%	27.60%
2,500,000 to 4,999,999	9.02%	28.66%	2.43%	15.40%	5.84%	23.46%
5,000,000 and up	4.08%	19.78%	14.76%	35.48%	22.26%	41.61%
N	1,374		1,565		1,626	

See text for description of the state collective bargaining law strength index, LAW, and Table 2 for a list of states in each law strength group.

**Table 5:
OLS and IV Wage Equation Results: CPS and SASS**

	OLS	IV
CPS results:		
COV	0.124 (6.85)	0.227 (3.82)
LAW	0.019 (0.71)	0.123 (3.13)
SASS results:		
COV	0.104 (5.665)	0.220 (5.12)
LAW	0.096 (4.65)	0.210 (6.29)

Shown are OLS and IV (GMM) estimates from wage equation estimates, with t- ratios and z scores are in parentheses. Standard errors clustered on state. Instruments for COV and LAW are the 1919 Labor Sentiment Index, State COPE Scores, and 1964 union membership density.

CPS: N=30,008 public school teachers. Dependent variable is average hourly earnings over scheduled and non-scheduled hours worked. Control variables in vector X include a state-specific opportunity cost wage index, graduate degree beyond BA, potential experience (age–years schooling–6) and its square, gender, metro size dummies, race, ethnicity, marital status, and foreign born.

SASS: N=4,565 school districts. Dependent variable is the average of scheduled salary for teachers with B.A. and 10 years experience and teachers with M.A. and 20 years experience. Control variables included in vector X include the CWI opportunity cost index, six metro size dummies, the number of days in the school year, the log of district enrollment, the student-teacher ratio, the percentage of teachers who teach secondary grades, the percentage of teachers and (separately) percentage of students Black, Hispanic, Asian, and American Indian, and the percentage of students eligible for free or reduced lunch

Table 6: SASS Results, OLS and IV for Experienced and Beginning Teachers

	OLS	IV
Experienced Teachers (M.A., 20 years)		
COV	0.124 (6.08)	0.239 (5.80)
LAW	0.113 (5.00)	0.211 (5.79)
Beginning Teachers (B.A., 0 years)		
COV	0.009 (0.56)	0.056 (1.96)
LAW	0.027 (1.72)	0.051 (1.95)

Notes: N=4,565. The t-ratios and z-scores from GMM IV in parentheses are computed using standard errors clustered by state. Dependent variable is the log salary for a teacher with average characteristics, calculated for teachers with 20 years seniority and an M.A. and for beginning teachers with a B.A. Additional controls are the same as stated in the note to Table 3.

**Table 7:
SASS 1999-2000 Salary, Benefits, Total Compensation Results, OLS and IV**

	Log Average Salaries		Log Average Benefit Rate		Log Total Compensation	
A. OLS Results						
COV	0.104 (5.66)		0.306 (2.18)		0.146 (4.78)	
LAW	0.096 (4.65)		0.216 (1.51)		0.118 (3.51)	
B. IV Results						
COV	0.220 (5.12)		0.426 (2.35)		0.271 (4.97)	
LAW	0.210 (6.29)		0.290 (1.86)		0.249 (5.60)	
Kleibergen-Paap LM Statistic	10.765	12.998	10.765	12.998	10.765	12.998
P-value	0.013	0.005	0.013	0.005	0.013	0.005

Notes: N=4565. t-ratios and z-scores in parentheses are computed using standard errors clustered by state. IV uses GMM. Additional controls include the log of the Comparable Wage Index, six metro size dummies, the number of days in the school year, the log of district enrollment, the student-teacher ratio, the percentage of teachers who teach secondary grades, the percentage of teachers who are Black, Hispanic, Asian, and American Indian, the percentage of students who are Black, Hispanic, Asian, and American Indian, and the percentage of students who are eligible for free or reduced lunch.

**Table 8: The Effect of Current Collective Bargaining
Laws and Coverage on Historical State Teacher Salaries**

Year	No controls		Control for state earnings	
	CB Law Index	%Cov Districts	CB Law Index	%Cov Districts
1950	0.258 (5.20)	0.288 (4.48)	0.060 (1.04)	0.015 (0.21)
1960	0.235 (6.63)	0.310 (6.94)	0.054 (1.44)	0.082 (1.69)
1970	0.199 (7.86)	0.266 (8.29)	0.078 (3.44)	0.104 (3.35)
1980	0.188 (7.15)	0.248 (7.53)	0.125 (5.12)	0.168 (5.43)
1990	0.181 (5.85)	0.249 (6.41)	0.037 (1.65)	0.105 (4.73)
2000	0.149 (5.48)	0.195 (5.39)	0.065 (3.01)	0.103 (4.09)
2007	0.165 (6.12)	0.196 (5.22)	0.089 (3.60)	0.114 (3.92)
2010	0.173 (5.88)	0.212 (5.32)	0.092 (3.57)	0.135 (4.86)

State level regressions based on author calculations from microdata. In each year, 48 states are included (Alaska, Hawaii, and DC not included). CB Law Index is LAW (current) and % Cov Districts (1999-2000) is COV, as previously defined. No public sector bargaining laws were in effect prior to 1959; all but three were in effect by 1978. School teachers include primary, secondary, and special education teachers in the public sector. The right-side results include a state earnings index (logged) for private sector wage and salary workers. t-statistics in parentheses.

Data source: IPUMS, with 1950 through 2000 being the Census of Population long form measuring previous calendar year and 2007 and 2010 being the American Community Survey measuring the 12 months prior to the survey.