

# The Effect of Teachers' Unions on Education Production: Evidence from Union Election Certifications in Three Midwestern States

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## Abstract

Using a unique data set on teachers' union election certifications I hand-collected from Public Employment Relations Boards in Iowa, Indiana, and Minnesota, I estimate the effect of teachers' unions on school district resources and on student educational attainment. Employing a difference-in-difference methodology that allows for non-parametric leads and lags of union age, I find teachers' unions have no impact on teacher pay or per-student district expenditures, but they increase teacher employment by about 5 percent. This employment increase is offset by enrollment increases in unionized districts, causing unions to have little effect on class sizes. I also estimate education production functions using high school dropout rates and find no net effect of teachers' unions on this attainment measure. These findings are in conflict with much of the past literature on teachers' union impacts and highlight the importance of correctly measuring unionization status in union impact studies.

KEYWORDS: Teachers' Unions, Public Sector Unions, Teacher Labor Markets, Education, Measurement Error.

JEL CLASSIFICATION: J51, I21, I22, H72.

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# 1 Introduction

Public school teacher collective bargaining has become a stable fixture in the American education system over the last 40 years. For example, as of 1988, all but 7 states had passed a law either allowing for the right of teachers to bargain collectively or explicitly requiring districts to bargain with teachers' unions. Furthermore, only four states had statutes prohibiting collective bargaining between public school districts and teachers (Freeman and Valletta, 1988). By 2004, 45.1% of public school teachers were members of a labor union that exists for the purpose of collective bargaining, and 50.8% were covered by a collective bargaining contract.<sup>1</sup>

Despite, or perhaps because of, the large rise in teacher organization, teachers' unions remain controversial. Opponents of teachers' unions argue these organizations take reform power away from administrators and parents as well as drain district resources (Haar, 1996 and Moe, 2001). Advocates of teacher unionization, however, believe empowering educators who are in the classroom bolsters student achievement by allowing for resources to be distributed in a more effective manner and to be used more efficiently (Retsinas, 1982). This debate is particularly relevant today as many reformers push for more competition in primary and secondary schooling. Proponents of increased school competition suggest introducing more competition into the system will reduce the importance of teachers' unions and partially undo any deleterious impacts these unions may have on districts (Chubb and Moe, 1988 and Moe, 2001). The importance of this argument is reduced if teacher unionization has no negative effect on school districts or students.

This paper analyzes the effect of teachers' unions on the allocation of school district resources as well as on student academic attainment. Historically, a major impediment to conducting this type of research has been the lack of data on which districts have teachers' unions and when they first organized. To remedy this problem, I have hand-collected teacher union election certification data for all school

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<sup>1</sup>Author's calculation from the May 2004 Current Population Survey.

districts in three Midwestern states: Iowa, Indiana and Minnesota. Because these data are available only in paper format at each state's Public Employment Relations Board office, this information has not been used before in any analysis of teacher unionization. These data allow me to construct a detailed panel of school districts that contains accurate union representation histories for every district in the sample.

Using data from the 1972–1991 Census/Survey of Government (COG/SOG), I estimate difference-in-difference models with non-parametric leads and lags for union age that allow me to analyze the time pattern of the impact of unions on school district resources. This analytic framework is unique in the teachers' union literature as it requires knowledge of each district's union status in each year covered by the sample. The election certification data I collected contain such information, which allows me to trace out the time pattern of union effects on school district resources in a manner that puts little structure on this pattern. Furthermore, by examining the pre-election trends, I can determine whether there is any evidence that changes in educational inputs affect union election timing. Previous studies were unable to undertake this type of detailed analysis because of a lack of information on union status in every year covered by the sample.

In contrast to the majority of other studies of the impact of teachers' unions, I find organization for the purpose of collective bargaining has little effect on educational inputs. Similar to studies such as Smith (1972), Balfour (1974), Zuelke and Frohreich (1977), and Kleiner and Petree (1988), my results indicate no increase in teacher pay, either in the short or long run, due to unionization.<sup>2</sup> I find full-time teacher employment increases by about 5 percent, but unionization also is associated with an increase in enrollment in union relative to non-union districts, which offsets any reductions in student-teacher ratios due to the employment increase. While the relative enrollment increases in newly unionized districts could be evidence of

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<sup>2</sup>In his comprehensive review of the literature, Freeman (1986) reports the majority of teachers' union impact studies find a positive effect of unionization on wages of between 3 and 21 percent. He also reports wage premia on the order of 5 to 10 percent for public sector protective services unions.

selection bias in my estimates, I find little evidence of such bias. My results further indicate that per-student current operating expenditures respond negligibly to teacher unionization.

Finally, I estimate education production functions using the high school dropout rate as the measure of educational output, which is calculated from the 1970–1990 U.S. Census. Use of this outcome measure is necessitated by the lack of historical student outcome data at the school district level and should be interpreted as providing suggestive evidence of the link between unionization and educational outcomes. I find teachers’ unions have no discernable net effect on high school dropout rates.

My findings are provocative in that they conflict with much of the previous literature on teachers’ unions.<sup>3</sup> Using cross-sectional data on the existence of teacher collective bargaining contracts, Eberts and Stone (1986) estimate teachers’ unions increase district costs by 15 percent, but they also increase educational productivity by 3 percent (1987). Baugh and Stone (1982) find unions increase teacher pay by between 4 and 12 percent in a study that employs teacher union membership data from the CPS. Using similar data, Moore and Raisian (1987) estimate a teacher union wage premium between 3 and 6 percent. In contrast, Kleiner and Petree (1988) find union membership and the percentage covered by contracts have a negligible effect on wages but have a positive and significant impact on SAT scores and non-wage expenditures per student at the state aggregate level.

In the most comprehensive study of teacher union impacts to date, Hoxby (1996) constructs a district-level panel from the 1972 through 1992 Census of Governments. This study is an advancement over previous cross-sectional work because it uses school district fixed effects to overcome the endogeneity of union status inherent in such estimates. The study finds the presence of a teachers’ union, as indicated by the existence of contracts combined with over 50 percent teacher union membership and the district reporting it engages in collective bargaining, increases average teacher pay

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<sup>3</sup>See Freeman (1986) for an overview of the literature.

by over five percent and current operating expenditures per student by almost three percent, while decreasing student-teacher ratios by 1.1. She also reports evidence unions increase high school dropout rates.

As a means to understand the differences between the estimates I present in this study and many of the estimates reported in previous empirical studies, I compare my unionization measure and union impact estimates with those derived from the COG Labor Relations Survey, which is the union measure used most notably in Hoxby (1996). I present suggestive evidence that the results disagree due to non-classical measurement error in the COG union measure, which points to potential measurement problems with the COG Labor Relations Survey rather than any analytical or coding errors committed by Hoxby (1996) in her careful and important study. The results of this paper underscore the importance of correctly measuring union status in union impact analyses, and I argue that the election certification data I use are a more reliable measure of union status than those used in previous work.

## 2 Theoretical Predictions

Because no comprehensive theoretical model of public sector union behavior exists, it is not clear *a priori* how unions will impact either district resources or student achievement. A central purpose of any labor union is to maximize the well-being of its members. In order to accomplish this goal, teachers' unions often advocate for higher wages, fewer hours and higher benefits for teachers. If these unions are successful in advocating for such changes, then districts might redistribute resources towards teacher pay and away from other areas of expenditure that may be more effective at increasing student achievement. As unions become more entrenched and gain more power over time, such effects could amplify as teachers extract more and more rents from districts. In addition, because unions often make it more difficult for districts to fire teachers, and because union contracts typically do not involve

performance-based compensation, any increase in teacher pay will not necessarily be correlated with an increase in teacher output. Thus, the marginal returns to teacher pay may fall due to teacher organization.<sup>4</sup>

Even a purely rent-seeking union may have a non-negative effect on student achievement. Because unions often are focused on improving working conditions as well as pay (Retsinas, 1982), teacher organization may lead to smaller class sizes and more satisfied teachers. The increase in workers' job satisfaction due to unionization is referred to as a "union voice" effect, and there is evidence in the private sector literature that giving workers a voice with which to change their working environment increases productivity (Gunderson, 2005). If teachers protect themselves from perceived or actual administrative abuses by exercising their union voice, unionization can have positive productivity effects. Additionally, any increase in wages or benefits could attract better teachers, thus increasing average teacher productivity.

In contrast to the rent-seeking model of union behavior, teachers' unions may seek explicitly to maximize student achievement. If there is misallocation of district resources absent unionization,<sup>5</sup> teachers' unions can use their collective power and their first-hand experience in the classroom to help redistribute resources in a manner that is more effective for education. Similarly, unions may have a positive impact on districts if they divert more local government funds from other sources to schools. This would result in an increase in the level of funding for schools, but not necessarily a change in the distribution.

These predictions of the impact of unionization on school districts and students are not mutually exclusive. Unions might be advocating simultaneously for increases in teacher pay, better working conditions, and for resources that will more effectively serve students. To the extent these outcomes have differential effects on achievement, simple models of union behavior do not yield unique predictions about the impact

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<sup>4</sup>This is typically called the "rent-seeking" model of union behavior, as unions seek to extract rents from the district without regard to their impact on students.

<sup>5</sup>Such a misallocation could arise due to the politicization of funding decisions at the local level or from inefficient district management. See Chubb and Moe (1988) for a discussion of these issues.

of unionization. It therefore is necessary to analyze empirically the effect teachers' unions have on students and school districts in order to evaluate the claims made by both advocates and opponents of these unions.

### 3 Data

#### 3.1 Teacher Union Election Certification Data

Studies of the impact of teachers' unions have used two forms of unionization measures, depending on the level of observation in the study. If the study is at the teacher level, the union measure typically is whether the teacher is a member of a union (Moore and Raisian, 1987 and Baugh and Stone, 1982). The largest problem with using union membership data is teachers can be employed in unionized districts without being members of the union. Furthermore, being a union member does not necessarily mean the union engages in collective bargaining; many unions in the United States function merely as professional organizations.<sup>6</sup>

Studies that take the school district as the level of observation tend to use the existence of a contract or collective bargaining agreement as the measure of teacher unionization (Eberts and Stone, 1986; Eberts and Stone, 1987; Woodbury, 1985; Kleiner and Petree, 1988; Hoxby, 1996). Absent measurement error, a collective bargaining agreement will accurately measure the presence of a union as long as all unions obtain contracts.<sup>7</sup> According to the NEA and AFT, which represent the vast majority of teachers' unions in the United States, it is rare for a unionized district to never obtain a contract, although there can be a lag between union formation and the culmination of collective bargaining in the form of a contract.

No previous union effects study has been based on data that accurately describe both the timing of unionization and the existence of a teachers' union in a given district. In order to obtain an improved measure of teacher unionization, I hand-

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<sup>6</sup>Both the NEA and the AFT began this way before the official onset of collective bargaining for teachers.

<sup>7</sup>Being unionized is necessary for engaging in collective bargaining, but a union that negotiates with a school district is not guaranteed to obtain a contract.

collected teacher union certification dates from union election certifications housed in the Public Employment Relations Board (PERB) office in Iowa, Indiana and Minnesota. When teachers in a district organize for the purpose of collective bargaining, the state PERB conducts an election. If over 50 percent of all school district teachers vote “yes,” then the board certifies the union as the sole bargaining representative of the teachers. The date of the election certification is thus the official date of unionization in each district.

To increase the accuracy of my union measure, I supplemented the certification data by searching for case law on LexisNexis as well as on the Indiana Education Employment Relations Board and the Iowa State Teachers’ Association websites that indicated when a district began collectively bargaining with teachers. If there was a negotiated contract in a district prior to the certification vote, it is likely to be picked up through these searches. Furthermore, because the unions in the three states in this analysis all are members of the National Education Association (NEA), groups of locals are aggregated into “UniServ” districts, which oversee the bargaining and governance of the union locals. I validated the election certification data by contacting the UniServ districts and requesting the date of first contract and the date of first certification for each union local in their district. Many UniServ districts did not have this information, which highlights the difficulty in collecting accurate union data. For the UniServ districts that had this information, I found the election certification data augmented with the web searches accurately represented the timing of union formation. In the few cases in which there was a discrepancy, I used the date given by the UniServ office rather than the date recorded from the PERB office.<sup>8</sup>

Iowa, Indiana and Minnesota are particularly attractive states for this analysis because all three passed “duty-to-bargain” laws in a time period covered by my

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<sup>8</sup>If two districts merge, necessitating a new union election, then the election data will assign this merger date as the date of unionization even if both districts were unionized prior to the merger. To each merged district, I assigned a unionization date equal to the earliest unionization date of the original districts. I obtained these dates from the web searches and UniServ districts as described above.

outcome data. Prior to 1972, all three states allowed collective bargaining between teachers and districts, but a school district did not have a duty to bargain with teachers if the administration did not choose to do so. As a result, there were few contracts in place prior to 1972.<sup>9</sup> These contracts were all due to “voluntary recognition” of the union by the school district. Beginning in Minnesota in 1972 and followed by Indiana in 1973 and Iowa in 1975, the states passed duty-to-bargain laws, which mandated a school district administration is legally bound to bargain in good faith with employees if the employees so desire. These laws dramatically increased unionization rates among teachers in these states (see Figure 1).

Because there was little voluntary recognition of teachers’ unions by school districts prior to the passage of the duty-to-bargain laws in these states,<sup>10</sup> the election certifications measure the time of first organization for the purpose of collective bargaining.<sup>11</sup> Figure 1 presents the distribution of teachers’ union certification years by state. The spikes in the distributions correspond to years in which a state passed a duty-to-bargain law. The small number of districts that were unionized prior to the passage of the state law did so through voluntary recognition by the district administration. As is evident in Figure 1, passage of a law establishing teacher collective bargaining was a major determinant of winning a unionization election.<sup>12</sup> This trend is consistent with those reported in Saltzman (1985), who argues unionization laws were largely a cause and not an outcome of teacher collective bargaining. The data show teachers’ unions established a significant presence in the public education system over the time period of this analysis in Iowa, Indiana, and Minnesota; all three states had school district teacher unionization rates of over 75 percent by 1987.

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<sup>9</sup>The supplemental web searches and the validation of the election data suggest I am accurately measuring the existence of contracts in the small number of districts that had teachers’ union contracts prior to the passage of their state’s duty-to-bargain law

<sup>10</sup>When I exclude voluntarily recognized unions from the analysis, the results are unchanged.

<sup>11</sup>In Minnesota, the duty-to-bargain law automatically declared an existing “Teachers’ Council” to have won a certification election if the majority of the council’s members belong to one teachers’ organization. While it is not entirely clear in the data which of these councils were already engaged in collective bargaining prior to 1972, these districts are marked as being “grandfathered.” All results and conclusions are fully robust to dropping these districts from the analysis. Results excluding grandfathered districts are available upon request.

<sup>12</sup>Unlike in the private sector, these elections are rarely unsuccessful. In fact, in my sample, there are no districts in which an election was lost.

The union certification data have several advantages over the measures used in earlier analyses. The first is instead of measuring whether teachers have a contract, which is the outcome of collective bargaining, I measure whether they have an agent certified by the state to engage in collective bargaining. However, the validation study showed, in the vast majority of cases, unions negotiate a contract within one school year of certification, and I found no districts in which the union did not achieve a contract. This result suggests that while the existence of a union and the existence of a negotiated contract are conceptually distinct, in practice they are similar. Analyzing the effect of winning a unionization election as opposed to negotiating a contract should yield comparable results. Secondly, because the certification dates are obtained from official state documents, there will be less measurement error than in data based on survey responses. Finally, the certification measure will not confound the existence of a union whose purpose is collective bargaining with a teachers' organization, because purely professional organizations will not engage in a unionization election.

### **3.2 Other Data Sources**

I combine my teachers' union election certification data with the Census and Survey of Governments (COG/SOG) Employment and Finance Surveys to construct measures of real monthly full-time teacher pay, full-time teacher employment, student-teacher ratios and real current operating expenditures (COE) per student for each district in the sample. All expenditures are inflated to real 2004 dollars using the CPI-U. I use student-teacher ratios as my measure of class sizes in this analysis, but it is important to note that class size and student-teacher ratios may differ in important ways. In particular, if unions bargain for more preparatory time and more support staff, the student-teacher ratio will be affected but not necessarily the number of students in each classroom. Nevertheless, this is the best measure available in the data and measures the prevailing human resources per student in each district.

I have district-level observations for the years 1972-1991, excluding 1986 due to data availability. Appendix A contains further details about the COG/SOG data.

In addition, I merge the certification data with the 1970, 1980 and 1990 U.S. Census school district summary files<sup>13</sup> to measure high school dropout rates using the following formula:

$$\text{H.S. Dropout Rate} = \left(1 - \frac{\text{total high school enrollment}}{\text{total population 14-18 years}}\right) * 100. \quad (1)$$

I also calculate total population, percent urban, average real income, median real gross rent, percent of families in poverty, percent unemployed, percent black, percent Hispanic, percent with a high school diploma or some college, percent with at least a BA, percent enrolled in private school, and total public school enrollment from these data for each district in my sample.

## 4 The Effect of Teachers' Unions on Education Production

### 4.1 Trends in School District Resources

Before undertaking an empirical examination of the effect of teachers' unions on school district resources, it is instructive to examine trends in school resources by state and union status in order to inform the empirical methodology. Trends in log real average teacher pay and log real COE per student are presented in Figures 2 and 3, respectively, by state and by when districts organized with respect to passage of their state's duty-to-bargain law.<sup>14</sup> Looking at Figure 2, across all types of districts there is a general downward trend in teacher pay in the three states. This downward trend is unlikely to be caused by unionization, as it begins prior to passage of the

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<sup>13</sup>All 1990 Census estimates are from the *School District Data Book*. The 1980 census data are taken from the 1980 *Summary Tape File 3-F* (U.S. Department of Commerce, 1980), and the 1970 data are taken from the *1970 Census Fourth Count (Population)* (U.S. Department of Commerce, 1970) and the *Census of Population and Housing, 1970: Fifth Count Tallies: Sample Data for School Districts* (U.S. Department of Education, 1970).

<sup>14</sup>Trends for log number of teachers, log student-teacher ratios and log enrollment are shown in Online Appendix Figures C-1 through C-3, respectively. Online Appendix C is available at the *Journal of Labor Economics* Website and at the author's homepage. The conclusions drawn from these figures are similar to those from log teacher pay and log COE per student.

duty-to-bargain laws in Indiana and Iowa and continues throughout the sample.<sup>15</sup> Figure 2 suggests any empirical model that seeks to identify union effects on teacher pay needs to account for this secular trend. Also, while real teacher pay exhibits some year-to-year noise, the yearly means move very similarly across union and non-union districts as well as across districts that unionized at different times relative to passage of the duty-to-bargain laws. The means presented in Figure 2 thus foreshadow one of the central results of the paper, that teachers' unions have no effect on average teacher pay. Figure 3 yields a similar conclusion to that of Figure 2; across all school district types within each state, the year-to-year variation in expenditures is virtually identical, and there is little evidence of a break from trend when the duty-to-bargain laws are enacted.

One interpretation of the trends presented in Figures 2 and 3 is that threat effects driven by passage of duty-to-bargain laws cause spillovers that affect districts that unionize and districts that do not unionize equally. Because duty-to-bargain laws led to a significant increase in the likelihood of unionization, the potential for union threat effects are particularly relevant in this setting (Farber, 2003).<sup>16</sup> However, teacher pay and current operating expenditures per student do not exhibit breaks from trend surrounding passage of duty-to-bargain laws, and districts that unionized prior to the passage of their state's duty-to-bargain law exhibit year-to-year variation identical to those who never unionize and to those who unionize later. This correlation is unlikely if the driving force behind these trends is threat effects brought about by stronger collective bargaining laws because the "early unionized" districts already have collectively bargained contracts in place and are unlikely to re-negotiate prior

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<sup>15</sup>While there is an upward spike in teacher pay in 1976 in Iowa, which is the year after passage of the duty-to-bargain law, the same upward spike is exhibited in Indiana and Minnesota and across all school districts, suggesting that it is spurious noise in the data rather than a treatment effect of unionization.

<sup>16</sup>There is considerable debate in the literature over the existence and size of union threat effects. Most of the evidence focuses on private sector unions, where some studies have found unionization raises non-union wages (Kahn, 1980 and Neumark and Wachter, 1995), reduces non-union wage dispersion (Kahn and Curme, 1987), and increases non-union benefits (Freeman, 1981). However, Farber (2003) finds less concrete evidence of union threat effects on non-union wages. While there is no evidence in the literature on threat effects of teachers' unions, Ichniowski, Freeman and Lauer (1989) find that police compensation increases equally among those that unionize and those that do not unionize due to stronger bargaining laws. This is the only evidence on public sector union threat effects in the literature.

to their contracts expiring.

More direct evidence on the relevance of threat effects can be obtained by comparing the trends in Figures 2 and 3 to those from the 18 states without duty-to-bargain laws throughout the sample period.<sup>17</sup> Trends for school districts in those states are presented in Figures 2 and 3 (as well as in Figures C-1 through C-3) and closely track those in Iowa, Indiana and Minnesota, which suggests that union threat effects are minimal for these resource measures in the three states covered by this analysis.

Another way to examine the presence of union threat effects is to conduct a cross-state difference-in-difference analysis of school district resources, comparing mean changes in Iowa, Indiana and Minnesota from before and after passage of the duty-to-bargain laws to changes in states that did not have duty-to-bargain laws and did not pass such a law during this time period. Table 1 presents estimates from such an analysis using the 1972, 1977 and 1982 Census of Governments. The first column in each panel contains state-level means for Iowa, Indiana and Minnesota, and the second column contains means for the 18 control states. The third column presents difference-in-difference estimates between each year and 1972, and the fourth column shows the standard error of this estimate. The fifth and sixth columns present the results from pooled difference-in-difference regressions that use all three years.

The cross-state estimates in Table 1 show a *negative* effect of teachers' unions on wages of between 4.0 and 5.6 percent, although the estimate is not statistically different from zero when state-specific trends are included in the model. While some of the estimates in Panel B suggest unions significantly decreased teacher employment by over 20 percent, when I control for state-specific trends I find no discernable effect. Panel C suggests unions had a small effect on student-teacher ratios that is not statistically distinguishable from zero in most columns. Finally, in Panel D, I find evidence of a negative effect of teachers' unions on per-student expenditures. Together, the estimates in Table 1 argue against large union threat effects in Iowa,

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<sup>17</sup>These states are Alabama, Arkansas, Arizona, Colorado, Georgia, Illinois, Kentucky, Louisiana, Missouri, Mississippi, Nebraska, New Mexico, Ohio, South Carolina, Texas, Utah, West Virginia, and Wyoming.

Indiana and Minnesota because, in most cases, state-average resources are moving in the opposite direction than one would predict if unions had a positive effect on teacher pay, teacher employment and per-student expenditures in all school districts in the treated states.

While comparing trends in states that did not enact duty-to-bargain laws to trends in Iowa, Indiana and Minnesota yields insight into the existence of threat effects and union spillovers, it is difficult to interpret the estimates in Table 1 as causal because the 18 control states may experience different secular variation in school resources that will confound identification of the treatment effects of interest. In the absence of union spillovers, a more credible strategy is to use non-union districts in each state to control for counterfactual trends. The remainder of this paper uses such variation to identify union effects on school district resources and on student academic attainment.

## 4.2 The Effect of Teachers' Unions on School District Resource Allocation: Empirical Methodology

To analyze the effect of teachers' unions on school district resources, I estimate the following equation on the Census/Survey of Governments data described in Section 3.2 and in Appendix A:

$$Y_{ist} = \beta_0 + \sum_{j=-5}^{10} \gamma_j I(t - \text{year}_c = j) + \tau_i + \phi_{st} + \epsilon_{ist}, \quad (2)$$

where  $Y_{ist}$  is the log of an outcome variable of interest,  $\phi_{st}$  are state-by-year fixed effects,  $\tau_i$  are district fixed effects, and  $\epsilon_{ist}$  is an error term. The term  $\text{year}_c$  refers to the calendar year in which district  $i$  became certified, and the expression  $I(t - \text{year}_c = j)$  is an indicator variable that equals 1 if district  $i$  is  $j$  years from a unionization election in year  $t$  and zero otherwise. For districts that never complete a union election and for observations for which the relative time to unionization is outside the event window, these indicator variables are set to zero. I choose an event window

from 5 years prior to 10 year post union election because sample sizes drop outside of this range. All district-year observations for which the time since certification is greater than 10 years are dropped from the analysis.

Due to data limitations, previous studies have modeled union effects by including a dummy variable for union status in their regressions. Equation (2) is more general than using a single union dummy because it semi-parametrically<sup>18</sup> estimates both short-term and long-term effects of unionization; the inclusion of dummy variables for each year relative to unionization imposes no structure on the pattern of time trends either pre- or post-treatment. This flexibility is important because unions may have non-linear impacts on districts over time that will be masked by imposing the parametric assumption that the effects are equal.<sup>19</sup> Thus, the full time pattern of union impacts over the event window allowed by the data will be estimated by equation (2), whereas standard models of union impacts are much more restrictive.

Another major advantage of equation (2) is that it includes district and time fixed effects. This feature contrasts with most of the previous work on union impacts, which has been cross-sectional (Freeman, 1986). Such a design often is necessitated by the lack of time series data on teacher unionization, but if unionization depends on unobservable factors that are correlated with both the decision to unionize and district outcomes (such as a bad administration), cross-sectional estimates will be biased. In contrast, the fixed effects model compares the same district at different times relative to the unionization year and controls for any unobservable (and unchanging) effects.

The central identifying assumption of the model is

$$E(\epsilon_{ist} | I(t - \text{year}_c = j) \quad \forall j \in [-5, 10], \tau_i, \phi_{st}) = 0. \quad (3)$$

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<sup>18</sup>The specification is semi-parametric because I impose the parametric assumption that the relative time effects and the state-specific year effects are additively separable. This is a standard assumption built into linear regression models.

<sup>19</sup>One might expect the time pattern of union effects to differ over time for several reasons. If unions focus first on gaining a foothold in the district rather than on affecting change, the short-run and long-run union impacts will differ. Unions also may need time to learn how to successfully bargain with administrators. Lastly, unions can change the administration in the long-run by supporting pro-union candidates for school board and local office. Note also that unions likely affect long-run equilibrium district outcomes past 10 years in a manner that I am unable to capture with my data.

Satisfying (3) necessitates that, conditional on the fixed effects, the timing of unionization is uncorrelated with potential outcomes. If there is selection into unionization based on pre-union wages or expenditures, estimates of the  $\gamma_j$  parameters from equation (2) will be biased. In addition, if school boards anticipate unionization and enact policy to attempt to defeat the organization movement in the district, it will become apparent in the pre-election relative time to unionization estimates. I therefore estimate  $\gamma_s$  prior to the union election ( $j < 0$ ) in order to test for any selection on the outcome variable that may be a causal factor in the decision to hold an election. Rather than controlling for differential pre-treatment trends across districts that do and do not unionize, my difference-in-difference setup allows me to test directly for the existence of such trends. Note that because the Census of Governments panel begins in 1972 and the collective bargaining laws were passed in 1972, 1973, and 1975 in Minnesota, Indiana, and Iowa, respectively, the relative time dummies with  $j < 0$  will be identified predominantly off of districts that unionize relatively later in the sample.<sup>20</sup>

The Census/Survey of Governments contains no school district demographic information. Given this limitation, it is important to think about why school districts unionized when they did. I investigate this question by comparing means of observable district demographic characteristics by district unionization status and timing using the 1980 U.S. Census data described in Section 3.2. Columns (i) and (ii) of Table 2 compare districts that never unionize to districts that do unionize as of 2004. The table indicates districts that never unionize have more high school graduates, fewer high school dropouts, are less urban, have a lower private school enrollment rate, are smaller, and have a higher poverty rate but lower median rent

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<sup>20</sup>Because the school district panel is unbalanced with respect to relative time to unionization, each  $\gamma_j$  is identified off of a potentially different set of school districts. See Appendix Table B-1 for the number of observations and the distribution of districts across states that identify each relative year effect. The unbalanced nature of the panel will cause the estimates to be biased if there are unobserved (or unmodeled) heterogeneous treatment effects. To test for this source of bias, I estimate equation (2) separately for those districts that unionize within one year of their state's passage of the duty-to-bargain law. Results are qualitatively and quantitatively similar to those presented below, which is not surprising given over 84 percent of treated observations fall into this group. I also estimate equation (2) using a balanced panel of school districts that I observe in every survey year. These results are presented in the Web Appendix Table C-1 and are similar to results from using the unbalanced panel, although the standard errors become larger due to sample size reductions.

than districts that unionize. Columns (iv) and (v) in Table 2 compare districts that unionized within a year of the passage of their state’s duty-to-bargain law and those that unionized later. The comparison of means suggests districts that unionized immediately following passage of their state’s duty-to-bargain law had a larger percentage of adults with a bachelor’s degree, were larger, more urban, had higher median rent, unemployment rate and district enrollment, but had a lower poverty rate than those that unionized later. Overall, this exercise suggests districts in larger cities and suburbs organized earlier, while the more rural districts unionized later or not at all.<sup>21</sup>

What effect can one expect these differences to have on the estimates from equation (2)<sup>22</sup> given the parameter of interest in this study is the average treatment effect on the treated (ATT)? Note selection into unionization based on perceived or actual gains from organizing will not bias identification of the ATT; such selection only will bias identification of the average treatment effect. Because the district fixed effects control for any time-invariant differences in outcome levels between the school districts, what is needed to identify the ATT is for the state-specific year effects to accurately reflect the counterfactual trends in the dependent variables for the treated observations. Correctly identifying  $\phi_{st}$  is therefore the main difficulty in estimating the treatment effect on the treated using equation (2).

The state-specific year effects are identified off state-specific yearly variation in the dependent variable from the control group (i.e., non-treated observations). In the main results presented in Section 4.3.1, I estimate equation (2) using all districts

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<sup>21</sup>There are many explanations for this trend in the literature on the history of teachers’ unions. The first is administrative abuses were most severe in the larger and more urban districts, therefore inducing a union vote. Secondly, the urban districts tended to be more industrialized and have a higher fraction of the populace with union membership. These populations may have been more favorable to teachers’ unions, thereby increasing the returns to unionizing. Finally, there are historical reasons the NEA and AFT were focused on the cities: the NEA started project URBAN in 1968 specifically to target city school districts as a response to AFT successes there. See Murphy (1990) for a detailed history of teacher organization.

<sup>22</sup>Most of the differences between the districts that never unionize, the districts that unionize early, and the districts that unionize later are due to the urban/rural distinction. When I drop all districts that have census blocks in urban areas, the panel becomes much more balanced with respect to the observables in Table 1. Results from estimation of equation (2) with this sample are presented in Web Appendix Table C-2, which shows the union impact estimates do not change appreciably, nor do the substantive conclusion from those estimates change, when this restriction is imposed.

that never unionize combined with all district-year observations for which the relative time to union election is less than or equal to 10. The control group in this sample is comprised of never-unionized districts and those district-year observations for which the relative time to unionization is less than -5. This sample is attractive because it uses all observations that arguably are unaffected by the treatment, which allows for the most power in identifying all parameters of equation (2). In Section 4.3.2, I show a series of robustness checks that illustrate my estimates are not particularly sensitive to the control group used.

### 4.3 The Effect of Teachers' Unions on School District Resource Allocation: Results

#### 4.3.1 Baseline Results

Figures 4 through 7 depict the estimates of  $\gamma_j$  from equation (2) for log real monthly full-time teacher pay, log full-time teacher employment, log student-teacher ratios and log real current operating expenditures per student, respectively. In each figure, the solid line indicates the point estimates of the  $\gamma$  coefficients from each relative-year-to-union-election dummy variable, and the dotted lines represent the bounds of the 95 percent confidence interval calculated from the standard errors that are clustered at the school district level.<sup>23</sup> Full regression estimates for the results in Figures 4 through 7 are reported in Table B-1.

As predicted by the trends in Figures 2 and 3, the results consistently indicate unions have little impact on school district resource levels. Focusing on Figure 4, there is no evidence teachers' unions increase teacher pay in either specification;<sup>24</sup> none of the point estimates is statistically distinguishable from zero at the 5 percent level, and most are less than 2% in magnitude. There also are no evident pre-election

<sup>23</sup>For ease of interpretation, I drop the relative time indicator variable for  $j=-1$  (the year prior to unionization) throughout this analysis. The  $\gamma_j$  coefficients therefore identify treatment effects relative to the effect for the year prior to unionization,  $\gamma_{-1}$ . Note in Figures 4 through 8 I include a zero for the point estimates in relative year  $j=-1$ , but the gaps in the standard error bounds reflect that this zero is imposed rather than estimated.

<sup>24</sup>It is important to note these are average wages. Unions may change the wage structure within districts without shifting the mean.

trends or anticipation effects that suggest there is selection in union election timing based on teacher pay trends.

These results contradict the vast majority of teachers' union impact studies that find a positive union wage premium (See Freeman (1986) for an overview). Hoxby's (1996) estimate of 5.1% is also outside the 95% confidence interval estimated here for all but the last two years of the event window. Secondly, although there is evidence in the literature that the union wage premium increased substantially over the 1970's (Freeman, 1986 and Baugh and Stone, 1982), no such increase appears in Figure 4.<sup>25</sup> Over time, as the union position became more solidified in these school districts, there is no statistically significant evidence they achieved wage gains for their members.

Results for full-time teacher employment are shown in Figure 5 and suggest employment increases immediately following unionization by close to 5 percent and remains at this level over time. The majority of the post-election estimates are statistically distinguishable from zero at the 5 percent level. These results are consistent with a model of union behavior in which teachers bargain over class size, preparatory time, and non-instructional supervision responsibilities, causing more teachers to be hired.<sup>26</sup>

Despite the increase in teacher employment, Figure 6 illustrates winning a unionization election has little effect on student-teacher ratios; while all point estimates for  $j > 0$  are negative, none is statistically significant. The explanation for the seemingly contradictory results in Figures 5 and 6 is student enrollment increases after unionization. I estimated equation (2) using log student enrollment as the dependent variable; these results are presented in Figure 8. I find enrollment is unaffected in the first two years following unionization but then increases to about 5 percent over the next three years and remains at this level for the remainder of the event window.

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<sup>25</sup>The explanation commonly given for this increase is in the earlier years of the teacher unionization movement, unions were focused on gaining a foothold in the district rather than on wage gains. As unions became more accepted over the course of the 1970s, they turned their attention to obtaining wage increases for their constituents.

<sup>26</sup>Figure 5 also could be evidence of a principal-agent model in which the union representatives seek to maximize union dues by forcing the district to hire more teachers.

Thus, teacher employment increases immediately upon unionization, but within four years after certification, enrollment expansion in treated relative to control districts undoes the decline in class sizes that would occur from increased teacher employment.<sup>27</sup> Importantly, there is little evidence of pre-unionization selection in Figure 8, which suggests this result is not being driven by selection into unionization based on recent enrollment patterns.

Unlike private sector unions, public sector unions can try to influence the total amount of resources available as well as their share of resources (Freeman, 1986 and Courant, Gramlich and Rubinfeld, 1979); through political lobbying and public relations, teachers' unions can increase the provision of public education.<sup>28</sup> Figure 7 examines this possibility by analyzing the effect of teachers' unions on log real COE per student. There is considerable variation in the estimates: the first year post-unionization shows a positive and significant spike in COE per student of about 4 percent, after which the estimates become negative and remain close to zero for the remainder of the event window. Furthermore, Figure 7 shows some variation in relative pre-treatment trends, but examination of Figure 3 suggests these differences are more likely due to noise in the data than indicate selection on relative trends in per-student current operating expenditures. Note that this figure represents changes in *per-student* expenditures. As enrollment is increasing by about 5 percent over this period in unionized districts relative to control districts, total expenditures do increase, though not enough to keep up with the enrollment increases. One interpretation of Figure 7 is teachers' unions successfully guard against per-student expenditure losses in the face of rising relative enrollment. However, an equally plausible interpretation is unions have little effect, especially in the long-run, on this outcome.

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<sup>27</sup>Note that most districts in the three states are losing population over this time period. The enrollment change is due to slower net out-migration rather than faster net in-migration in unionized districts relative to non-union districts (see Figure C-3).

<sup>28</sup>Interestingly, this is one area where the administration and teachers' union might agree. One explanation for the acquiescence of school boards to teacher unionization might be that the administration hopes to increase provision of public education through the union's political actions.

### 4.3.2 Robustness Checks

As discussed in Section 4.2, the critical assumption underlying identification of the  $\gamma$  coefficients in equation (2) is the use of an appropriate control group to account for secular variation in school district resources. Recall that Figures 1 and 2 as well as Figures C-1 through C-3 suggest my estimates should not be particularly sensitive to the within-state non-union districts I use as a control group, because yearly trends are very similar across districts that unionized at different times and across districts that never unionized and those that did. Nonetheless, I assess the fragility of my results to the choice of estimation sample by estimating equation (2) using additional samples that each imply a different control group. First, I restrict the estimation sample to include only never-unionized districts and the district-year observations for which the relative time to certification falls within the event window. The control group implied by this estimation sample is comprised of only the never-unionized districts and is attractive relative to the control group used to generate the main estimates because the proportion of districts that do unionize and never unionize is not changing over time. Furthermore, this control group will be unaffected by union effects on the dependent variable more than 5 years prior to unionization. I also obtain estimates using only those district-year observations for which the relative time to unionization is less than or equal to 10. This sample is the same as the one used to estimate the parameters shown in Figures 4 through 8, but it excludes never unionized districts. The implied control group is thus the district-year observations for which the relative time to certification is less than -5.

In order to summarize these estimates, I estimate regressions on each sample that include a linear term for relative union age (including negative ages), which is set to zero for never unionized districts, a union status dummy variable equal to 1 if the district is unionized in a given year, and an interaction between union status and union age. This model is a form of equation (2) in which I constrain the relative

pre- and post-unionization trends to be linear.<sup>29</sup> As expected, estimates from these robustness checks are very similar in both magnitude and quality to those presented in Section 4.3.1 and are shown in Table 3.

Another way to identify union effects is to control directly for pre-treatment trends of districts that unionize.<sup>30</sup> Results from such a model are presented in Table 4, in which the analysis sample is all district-year observations with relative years to union election less than 11. I control for both year and district fixed effects as well as a linear measure of relative union age. Due to the exclusion of districts that do not unionize and of district-year observations with union ages greater than 10 years, the linear union age coefficient controls for pre-union trends in outcomes among districts that will unionize in the future. These estimates should be unaffected by union threat effects, but they will not be consistent for the ATT if there are secular trends in the dependent variable that are spuriously correlated with union timing.

The results are consistent in both magnitude and statistical significance to those presented in Figures 4 through 8. In particular, I find little evidence of a union effect on teachers' wages and student-teacher ratios in the short or long-run. While the estimated effects on teacher employment are somewhat smaller than the estimates in Figure 5, they are positive and qualitatively similar. The estimates for current operating expenditures per student are consistent with those in Figure 7 directly after unionization, but they suggest a potential *negative* longer-run effect. Finally, Table 4 shows a similar enrollment effect to the estimates in Figure 8. The similarity of these results to those reported in Section 4.3.1 suggests union threat effects are not biasing my identification of the effect of teachers' unions on school district resources and that unions have little effect on school district resource allocation. However, if unions influence teacher productivity, they still can affect student achievement. I next turn to an empirical analysis of the effect of unions on high school dropout rates

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<sup>29</sup>Semi-parametric estimates of the  $\gamma$  coefficients, estimated using equation (2) on these samples, are available from the author upon request.

<sup>30</sup>Note that controlling for pre-treatment trends of districts that unionized is akin to a first-difference model, rather than the difference-in-difference model given by equation (2).

to test for such effects.

#### 4.4 The Effect of Teachers' Unions on High School Dropout Rates

In order to test whether teachers' unions affect educational attainment among students, I estimate linear education production functions using high school dropout rates as my outcome measure. This analysis therefore will be focused on those at the lower end of the educational attainment distribution. Because the high school dropout rate is calculated from 1970, 1980 and 1990 school district-level U.S. Census data (see Section 3.2), I cannot employ the difference-in-difference methodology given by equation (2) due to the fact there are relative years to union election with few observations. Instead, I impose a linear structure on the time pattern of teacher union effects, though results are unchanged if I allow for quadratic time patterns. I estimate linear education production functions of the form:

$$\begin{aligned} \text{Dropout Rate}_{ist} = & \beta_0 + \beta_1 \text{Union}_{ist} + \beta_2 \text{Union} * (\text{Union Age})_{ist} \\ & + \beta_3 \text{Union Age} + \delta X_{ist} + \tau_i + \phi_{st} + \epsilon_{ist}, \end{aligned} \quad (4)$$

where all union variables are defined the same as in Table 3 (see Section 4.3.2),<sup>31</sup>  $X$  is a vector of demographic characteristics that are listed in Section 3.2, and all other variables are as previously defined.<sup>32</sup> The coefficient on  $(\text{Union Age}) * \text{Union}$  identifies the existence and magnitude of time-varying union effects on high school dropout rates, while the coefficient on  $\text{Union Age}$  will detect selection into unionization based on high school dropout rate trends.

Results from estimation of equation (4) are presented in Table 5. When the union age terms are excluded, there is no apparent effect of teachers' unions on high school

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<sup>31</sup>In equation (4),  $\text{Union Age}$  includes non-zero values for all negative union ages, whereas in Table 3,  $\text{Union Age}$  is set to zero for all values less than -5.

<sup>32</sup>The education production function given by (4) is admittedly crude in the sense that I am unable to control for student-level factors such as previous test scores and a vector of historical educational inputs. This limitation is necessitated by the data, but under the assumption that unionization is conditionally exogenous, which has been the identifying assumption throughout the analysis, the exercise still yields insight into the relationship between teachers' unions and high school dropout rates.

dropout rates: the coefficient on the union dummy is 0.100 and is not statistically significant at even the 10 percent level. However, when union age is added to the model, column (ii) shows unionization is associated with a 1.8 percentage point increase in high school dropout rates in the short-run, but in the long-run is associated with a decrease in dropout rates. The estimates imply that after 7.7 years, the union effect on dropout rates becomes negative. As column (iii) shows, this result is basically unchanged by controlling for union age prior to unionization, which suggests selection effects are negligible in this context.

The results presented in Table 5 are suggestive that unions do not impact high school dropout rates; at least on average. As discussed in Section 2, unions likely change many of the aspects of the teacher-administrator relationship, each of which has a different implication for teacher productivity. For example, by making it more difficult to fire teachers and by linking pay to experience and education level instead of to output, unions can reduce teacher productivity. However, unions can be productivity-enhancing by protecting teachers from bad administrative practices and giving them a voice with which to influence their workplace. Table 5 is suggestive either that unions have no effect on productivity or that the positive productivity effects of unionization are canceled out by the negative effects. How teachers' unions influence the effectiveness of educational inputs is an important topic for future research.

## 5 Discussion

Taken together, the results presented above indicate teachers' unions have little net effect on resource allocation and student educational attainment.<sup>33</sup> What theories

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<sup>33</sup>These findings are consistent with some of the private sector union literature. Freeman and Kleiner (1990) find very modest wage increases among newly unionized firms in the 1980s that they sampled, relative to similar non-union firms. Dinardo and Lee (2004) estimate the effect of unionization on business survival, employment, output, productivity, and wages using a regression discontinuity framework that compares firms in which the union barely won the certification election to firms in which the union barely lost. Local to the discontinuity, they find private sector unions have little effect on these outcome measures. However, in recent work, Lee and Mas (2008) use an event study analysis to show private sector unions have a sizeable negative effect on excess stock market returns. They also present evidence that this negative effect goes away around the election discontinuity of 50%. The interpretation

of school district and union behavior might be consistent with my findings? One model that fits into the context of the above results is Tiebout sorting (Tiebout, 1956). Tiebout sorting could occur due to the increases in teacher employment and current operating expenditures per student directly after unionization. To the extent parents value these increased resource levels, enrollment in unionized districts should increase relative to non-unionized districts, which is what the data show. These results are thus consistent with the larger literature on parental valuation of school resources (Black, 1999 and Brasington, 1999). Such studies typically present evidence that more school resources lead to increased demand among parents as measured by changes in housing prices. It is natural to expect increases in demand to lead to enrollment increases as well.

That the relative enrollment increases found in my analysis are the same magnitude as the teacher employment effects and that they occur gradually after the unionization decision is highly suggestive they are in response to the shift in resources post-unionization. Conversely, unions may be reacting to expected relative enrollment increases in their district to force the administration to keep class sizes and expenditures per student roughly constant. Both models of union behavior will produce the data patterns reported in Section 4. To test the latter explanation, I use the age distribution in each school district from the 1980 U.S. Census to explore whether the 0-5 age population in any given year has power in predicting the timing of the unionization election. I find no evidence of correlation between union vote timing and forecastable population in the school district. Furthermore, because the cities are more likely to unionize and because these are the areas most likely to be experiencing relative growth, I estimate equation (2) using only those districts that have no Census block points in an urban area at any point in the sample period. The results of this exercise are reported in Table C-2 and are both qualitatively and quan-

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of this result is that “strong” unions have larger effects than “weak” unions. Interestingly, teachers’ unions tend to be quite strong, and vote shares in favor of unionization among teachers typically are close to 100%. Given the large institutional differences between public and private sector unions, however, it is difficult to draw too many conclusions from comparisons of results across sectors

titatively similar to those presented above. The results therefore point to a Tiebout sorting explanation for the relative enrollment increases in unionized districts rather than evidence of selection into unionization based on beliefs about future enrollment changes or growth in city versus rural populations. That the relative enrollment increases in unionized districts negates any class size and per-student expenditure gains from rising employment and expenditure levels following unionization, however, is unambiguous in the data.

Another explanation for my results is teachers' unions simply may be ineffective at influencing resource allocation. This could occur if unions face restrictive district budget constraints; if there are few rents to extract, the unions will not be able to affect school district budgets, regardless of their underlying goals. Further, union aggressiveness in extracting rents may be limited by a fear of taxpayer backlash at the local level. It remains an open question in the literature whether teacher unionization causes tax revolts, but unions may react to this possibility by reducing the degree to which they attempt to influence educational inputs. Teachers' unions also may achieve non-salary benefits for teachers, such as health care and pensions,<sup>34</sup> as well as give teachers a voice in setting work rules and practices (Moe, 2001; Murphy, 1990; Retsinas, 1982; and Johnson, 2004). While the data on such outcomes are difficult to obtain, examining union impacts on these factors is an important area for future work.

My union impact estimates differ both quantitatively and qualitatively from much of the established literature on teachers' union effects. One plausible explanation for these differences is that unions have different effects in Iowa, Indiana, and Minnesota than in the rest of the United States. Another explanation, however, is that the union election data more accurately capture the timing and extent of teacher unionization than previous measures have. In order to gain insight into the differences between

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<sup>34</sup>Freeman (1986) cites evidence that public sector unions raise non-wage benefits by more than they raise wages, though the evidence is scant for teachers' unions. Freeman (1981) finds the same effect for private sector unions.

my estimates and previous estimates in the literature, Online Appendix Table D-1<sup>35</sup> compares my union measure with the union measure constructed from the Census of Governments Labor Relations Surveys, used most notably in Hoxby (1996). There are substantial differences across the two measures that strongly suggest measurement error exists in the COG union measure. Interpreting all differences between the union certification measure and the COG union measure as measurement error in the latter, I find misclassification rates of up to 47 percent in the Census of Governments.

Appendix D also presents replications of the analysis from Hoxby (1996) using my sample of 3 states and both union measures in order to determine the relevance of the differences in union measures on union impact estimates. Though the results of this replication are somewhat inconclusive due to large standard errors, my estimates using the COG union measure, particularly for teacher pay, are consistent with those presented in Hoxby (1996), but I find that using my union measure produces substantively different estimates. I then undertake an analysis of the properties of the measurement error in the COG union measure, treating the election certification data as the true measure of union status for each school district. I find the measurement error in the Census of Governments is correlated with the outcome variables used in this analysis, which implies the bias is not guaranteed to attenuate the coefficient estimates because it is correlated with teacher salary, per-student expenditure, class size and high school dropout rate changes at the school district level (Bound, Brown and Mathiowetz, 2001). I also perform Bound, Brown, Duncan and Rodgers (1994) decompositions that decompose the measurement error into the part that is due to misclassification of union status and the part that is due to the correlation of this misclassification with the regression error. My results indicate both forms of bias are present and reinforce each other for teacher pay, COE per student, and student-teacher ratios but work in opposite directions for high school dropout rates. The central implication of the results presented in Appendix D is that obtaining

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<sup>35</sup>Online Appendix D is available at the *Journal of Labor Economics* Website and at the author's homepage.

accurate measures of union status and unionization timing is critical to obtaining accurate estimates of union impacts.

## 6 Conclusion

Using new hand-collected data on the timing of teachers' union election certifications in Iowa, Indiana and Minnesota combined with school district-level data from the Census/Survey of Governments, I investigate the impact of teachers' unions on school district educational resources. Contrary to many past studies on teachers' unions (Hoxby, 1996; Freeman, 1986; Moore and Raisian, 1987; and Baugh and Stone, 1982), I find unions have no effect on teacher pay. I also present evidence teacher unionization causes an increase in full-time teacher employment of about 5 percent, a negligible decrease in student-teacher ratios, and only has a short-run positive effect on current operating expenditures per student.

I estimate the impact of unions on high school dropout rates using 1970-1990 U.S. Census school district summary data and find little evidence that unions affect this outcome measure. However, I am unable to determine with my data whether similar results would be found for other achievement measures that include more students from higher portions of the ability distribution.

The results and conclusions of this analysis raise a puzzle: why do teachers bother to organize, especially at the high rates observed in the data, given the lack of wage and class size effects? One possible answer to this puzzle is teachers perceive organization increases their pay. Indeed, when talking to union members during this study, wage increases were the most commonly mentioned benefit of unionization, in contrast to what this analysis shows. Another important reason for unionizing is to give teachers a voice with which to improve their working conditions as well as to establish well-defined rules governing hiring and firing, pay structure and promotion. There is anecdotal evidence teachers' unions provide these benefits (Woodbury,

1985), although I lack the data to test for such effects. Finally, unionization may increase non-wage benefits such as pensions or health care that are valued by teachers. Unionization thus can influence how satisfied teachers are with their job, and consequently, may affect parent and student satisfaction with their school district.

One must be careful in drawing too general a conclusion from the results presented above, as this study includes only three states concentrated in the Midwest. Rather than interpreting my results as representative of union impacts for the United States as a whole, one can view this study as provocative in suggesting the commonly accepted effects of teachers' unions – raising wages and reducing teacher productivity – may not be robust to the use of more accurate union data. The main implication of this study is more research using such data is necessary to understand more fully the nature and impact of collective bargaining in public education and to inform meaningful labor relations policy.

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## Appendix A: Census and Survey of Governments

The Census of Governments is conducted every five years beginning in 1957, however data only are available electronically starting in 1972. The survey contains expenditure and employment data for every independent government in the United States, including independent school districts. Independent school districts are those deemed separate enough from other local governments that they are considered their own autonomous government. In Iowa and Indiana, all school districts are independent. In Minnesota, however, about 7 percent of students are enrolled in dependent school districts. Thus, the universe of school districts in the COG/SOG is close to the full universe of school districts in the three states included in this analysis.

The Survey of Governments is conducted in each non-COG year beginning in 1973 and contains a random sample of local governments included in the previous census. In 1979, the Census Bureau began sampling every school district in certain states (including Iowa and Minnesota) for their *Annual Survey of Local Government Finances - School Systems (F-33)* survey. The employment survey, which is conducted separately, remained a random sample for all states.

Because the Census Bureau does not code school districts in a systematic manner, the only way to combine information across years is to merge files based on district name. However, in the 1975 finance and 1986 finance and employment files, these names are missing. Thus, I am forced to exclude data from these survey years from the analysis. I do use the 1975 employment data, however.

I construct measures of real monthly full-time teacher pay, full-time teacher employment, student-teacher ratios and current operating expenditures (COE) for each district in the sample. All financial variables are inflated to real 2004 dollars using the CPI-U. The definitions of most of these variables are straightforward and come directly from the COG/SOG, with the exception of teacher pay and the student-teacher ratio.

I construct real monthly full-time teacher pay by dividing the gross monthly payroll for full-time instructional staff by the number of full-time instructional staff. Full-time staff are defined by the number of hours they are paid to work; both full-time and part-time teacher employment include the same categories of staff members. Note also that in the COG/SOG data, “teachers” include educational support staff and school-level administrators, such as principals and guidance councilors. However, other administrators, such as the superintendent, are excluded from this category. Unfortunately, there are no district-level data from this period on teacher pay that will allow me to further separate this group. To the extent unions affect the mix of full-time teachers in the school district through changes in seniority rules and hiring practices, the impact on teacher pay only will be detected if these changes shift the mean salary of teachers.

The student-teacher ratio is my measure of class size (Woodbury (1985) and Hoxby (1996) also use this measure). I calculate the student-teacher ratio by dividing total enrollment by the number of full-time equivalent teachers in each school district. While this ratio does not measure the exact number of students included in each class, it is a reasonable and standard approximation of the human resources per student in each district.

**Table 1: Cross-State Difference-in-Difference Estimates of the Effect of Teachers' Unions on School District Resources**

Panel A: Ln(Real Monthly Full-Time Teacher Pay)						
Year	Treated States	Untreated States	Difference in Differences	Standard Error	State Fixed Effects	State Trends
1972	8.189	8.050				
1977	8.074	7.983	-0.048**	(0.017)	-0.040**	-0.056
1982	8.014	7.916	-0.041	(0.026)	(0.015)	(0.044)
Panel B: Ln(Full-Time Teacher Employment)						
Year	Treated States	Untreated States	Difference in Differences	Standard Error	State Fixed Effects	State Trends
1972	4.164	3.461				
1977	4.255	3.760	-0.208**	(0.043)	-0.220**	0.012
1982	4.171	3.871	-0.403**	(0.091)	(0.059)	(0.052)
Panel C: Ln(Student-Teacher Ratio)						
Year	Treated States	Untreated States	Difference in Differences	Standard Error	State Fixed Effects	State Trends
1972	2.845	2.805				
1977	2.717	2.662	0.015	(0.022)	0.042**	-0.009
1982	2.655	2.571	0.045	(0.029)	(0.020)	(0.022)
Panel D: Ln(Real COE Per Student)						
Year	Treated States	Untreated States	Difference in Differences	Standard Error	State Fixed Effects	State Trends
1972	8.343	8.141				
1977	8.412	8.273	-0.063**	(0.030)	-0.081**	-0.046
1982	8.459	8.345	-0.088**	(0.034)	(0.028)	(0.052)

<sup>1</sup> Source: Author's calculation from the 1972, 1977 and 1982 Census of Governments as described in the text.

<sup>2</sup> The first column contains yearly means for Indiana, Iowa and Minnesota. The second column contains means for the 18 states that did not have a duty-to-bargain law as of 1987: Alabama, Arkansas, Arizona, Colorado, Georgia, Illinois, Kentucky, Louisiana, Missouri, Mississippi, Nebraska, New Mexico, Ohio, South Carolina, Texas, Utah, West Virginia, and Wyoming.

<sup>3</sup> The third column contains the difference-in-difference estimate from the first two columns, which is the difference between "treated" and "untreated" in the given year minus the difference between "treated" and "untreated" in 1972. The fourth column presents the standard error of this difference, clustered at the state level: \*\* indicates significance at the 5 percent level and \* indicates significance at the 10 percent level.

<sup>4</sup> The final two columns present difference-in-difference estimates using all three years that control for state and year fixed effects in the fifth column and control for state and year fixed effects as well as state-specific linear year trends in the sixth column. Standard errors clustered at the state level are in parentheses: \*\* indicates significance at the 5 percent level and \* indicates significance at the 10 percent level.

**Table 2: Means of Demographic Characteristics from the 1980 School District Census Data for Never Unionized vs. Ever Unionized Districts and for Districts that Unionize Within a Year of their State’s Passage of a Duty-to-Bargain Law vs. Districts that Unionize Later**

Demographic Variable	(i) Never Unionized	(ii) Ever Unionized	(iii) Difference (i)-(ii)	(iv) Unionized At Law	(v) Unionized After Law	(vi) Difference (iv)-(v)
Percent Black	0.15 (1.00)	0.55 (3.12)	-0.40 (0.27)	0.62 (3.42)	0.25 (1.11)	0.37 (0.25)
Percent Hispanic	0.10 (0.32)	0.16 (0.78)	-0.06 (0.07)	0.17 (0.85)	0.11 (0.34)	0.07 (0.06)
Percent Some High School Graduate	14.81 (3.57)	16.16 (4.98)	-1.35** (0.44)	16.26 (5.04)	15.70 (4.72)	0.56 (0.40)
Percent Some College	56.33 (6.09)	54.45 (6.24)	1.88** (0.57)	54.26 (6.51)	55.29 (4.86)	-1.03** (0.50)
Percent BA	16.97 (3.88)	16.51 (4.16)	0.46 (0.38)	16.44 (4.21)	16.85 (3.90)	-0.42 (0.33)
Percent Urban	11.88 (4.90)	12.88 (5.65)	-0.99* (0.51)	13.05 (5.92)	12.16 (4.25)	0.89* (0.45)
Percent Private Enrollment	3.74 (17.30)	9.79 (27.67)	-6.06** (2.43)	11.02 (29.20)	4.51 (18.94)	6.51** (2.22)
Log Average Income	5.80 (6.72)	7.63 (7.83)	-1.84** (0.70)	7.70 (7.39)	7.36 (9.51)	0.34 (0.63)
Log Median Rent	9.59 (0.17)	9.59 (0.18)	0.00 (0.02)	9.59 (0.18)	9.57 (0.17)	0.02 (0.01)
Percent Unemployed	6.08 (0.19)	6.12 (0.19)	-0.04** (0.02)	6.13 (0.19)	6.07 (0.18)	0.06** (0.02)
Percent Below Poverty	2.62 (1.36)	2.86 (1.39)	-0.24* (0.13)	2.91 (1.38)	2.62 (1.38)	0.30** (0.11)
Public School Enrollment/100	5.31 (3.25)	4.80 (2.86)	0.51* (0.27)	4.69 (2.81)	5.28 (3.05)	-0.59** (0.23)
	10.48 (32.62)	22.34 (42.93)	-11.85** (3.82)	24.54 (46.08)	12.84 (23.02)	11.71** (3.44)
N	136	1007	.	817	190	.

Columns (i) and (ii) present means for all districts by whether a district unionized and columns (iv) and (v) present means for districts that unionize by whether a district unionized within the same year as passage of a state duty-to-bargain law or after, respectively. All demographic characteristics are calculated from the 1980 Census as described in the text. Standard deviations are in parentheses in columns (i), (ii), (iv), and (v). The difference between the two preceding columns are presented in columns (iii) and (vi) and the standard error of this difference is in parentheses in these columns: \*\* indicates significance at the 5 percent level and \* indicates significance at the 10 percent level.

**Table 3: Regression Results from Fixed Effects Estimates of Teachers' Union Impacts on Resource Levels from the Census/Survey of Governments Using Different Control Groups**

Panel A: Results Using Never Unionized Districts as Control Group					
Dependent Variable	Dependent Variable: Log of				
	Real Monthly Full Time Teacher Pay	Full Time Teacher Employment	Student-Teacher Ratio	Real COE Per Student	Total Enrollment
Union Age	-0.003** (0.005)	0.001** (0.009)	-0.001 (0.010)	0.005 (0.006)	0.000 (0.004)
Union	0.013** (0.014)	0.051** (0.025)	-0.012 (0.002)	-0.002 (0.014)	0.032** (0.016)
Union*(Union Age)	0.005** (0.005)	-0.001 (0.009)	-0.022** (0.010)	-0.007 (0.007)	0.005 (0.005)

Panel B: Results Using Late Unionized Districts as Control Group					
Dependent Variable	Dependent Variable: Log of				
	Real Monthly Full-Time Teacher Pay	Full Time Teacher Employment	Student-Teacher Ratio	Real COE Per Student	Total Enrollment
Union Age	-0.005** (0.002)	-0.002 (0.004)	-0.026** (0.002)	0.030** (0.008)	-0.017** (0.003)
Union	0.005 (0.008)	0.044** (0.018)	-0.011 (0.017)	0.005 (0.012)	0.022 (0.018)
Union*(Union Age)	0.001 (0.001)	-0.001 (0.002)	0.002 (0.002)	-0.001 (0.002)	0.004* (0.002)

<sup>1</sup> Source: Author's calculation as described in the text. The analysis sample in Panel A includes only district-year observations with time to unionization less than or equal to 10 years and greater than -6 years as well as never unionized districts. The analysis sample in Panel B includes only district-year observations with time to unionization less than or equal to 10 years, excluding never unionized districts. The implicit control group in the Panel B regressions are district-year observations that unionize more than 5 years in the future in any calendar year.

<sup>2</sup> Regressions include school district and state-specific year fixed effects. The variable *Union Age* is the number of years relative to election certification, which includes negative values for the 5 years prior to certification. For districts that do not unionize and for district-year observations that unionize more than 5 years in the future, *Union Age* is set to zero. The variable *Union* is a dummy variable equal to 1 if a district has successfully unionized as of that calendar year.

<sup>3</sup> All standard errors are clustered at the school district level: \* indicates significance at the 10 percent level and \*\* indicates significance at the 5 percent level.

**Table 4: Regression Results from Fixed Effects Estimates of Teachers' Union Impacts on Resource Levels from the Census/Survey of Governments Controlling for Pre-Treatment Trends**

Dependent Variable	Dependent Variable: Log of				
	Real Monthly Full Time Teacher Pay	Full Time Teacher Employment	Student-Teacher Ratio	Real COE Per Student	Total Enrollment
Union Age	-0.004** (0.001)	0.008** (0.003)	-0.022** (0.002)	0.022** (0.001)	-0.015** (0.002)
Union Age:					
0 Years	0.005 (0.017)	0.016 (0.012)	-0.013 (0.010)	0.037** (0.013)	-0.003 (0.011)
1 Year	0.002 (0.008)	0.017 (0.014)	-0.014 (0.011)	0.018 (0.015)	-0.003 (0.012)
2 Years	0.007 (0.010)	0.012 (0.017)	-0.018 (0.014)	-0.014 (0.032)	0.015 (0.017)
3 Years	0.023** (0.011)	0.017 (0.019)	0.001 (0.014)	-0.004 (0.013)	0.015 (0.019)
4 Years	0.000 (0.012)	0.023 (0.021)	0.000 (0.014)	-0.002 (0.013)	0.024 (0.020)
5 Years	-0.012 (0.012)	0.030 (0.022)	-0.015 (0.016)	-0.003 (0.014)	0.030 (0.021)
6 Years	0.000 (0.013)	0.020 (0.024)	-0.004 (0.017)	-0.033** (0.015)	0.042* (0.022)
7 Years	-0.018 (0.015)	0.022 (0.029)	-0.014 (0.021)	-0.057** (0.017)	0.046** (0.023)
8 Years	-0.004 (0.016)	0.032 (0.029)	0.001 (0.019)	-0.034** (0.017)	0.050** (0.024)
9 Years	-0.042* (0.016)	0.072** (0.031)	-0.021 (0.021)	0.002 (0.017)	0.048* (0.026)
10 Years	0.012 (0.017)	0.004 (0.032)	-0.002 (0.021)	-0.004 (0.018)	0.043 (0.027)
Constant	8.195** (0.007)	4.611** (0.014)	2.793** (0.037)	8.438** (0.009)	7.136** (0.010)
F-Test of No Union Effect	10.21 [0.00]	6.92 [0.00]	0.89 [0.55]	8.70 [0.00]	1.78 [0.05]
N	7518	7518	7078	10250	10703
# Clusters	1027	1027	1027	1027	1027
R <sup>2</sup>	0.711	0.975	0.615	0.566	0.983

<sup>1</sup> Source: Author's calculations as described in the text. The analysis sample includes only district-year observations with time to unionization less than or equal to 10 years.

<sup>2</sup> Regressions include school district and year fixed effects. The variable *Union Age* is the number of years relative to election certification, which includes negative values for years prior to certification. All standard errors are clustered at the school district level: \* indicates significance at the 10 percent level and \*\* indicates significance at the 5 percent level.

<sup>3</sup> The "F-Test of No Union Effect" is a test for joint significance of the union age dummy coefficients. P-values of the test for joint significance are presented in brackets beneath the F-statistic.

**Table 5: The Effect of Teachers' Unions on High School Dropout Rates, 1970-1990**

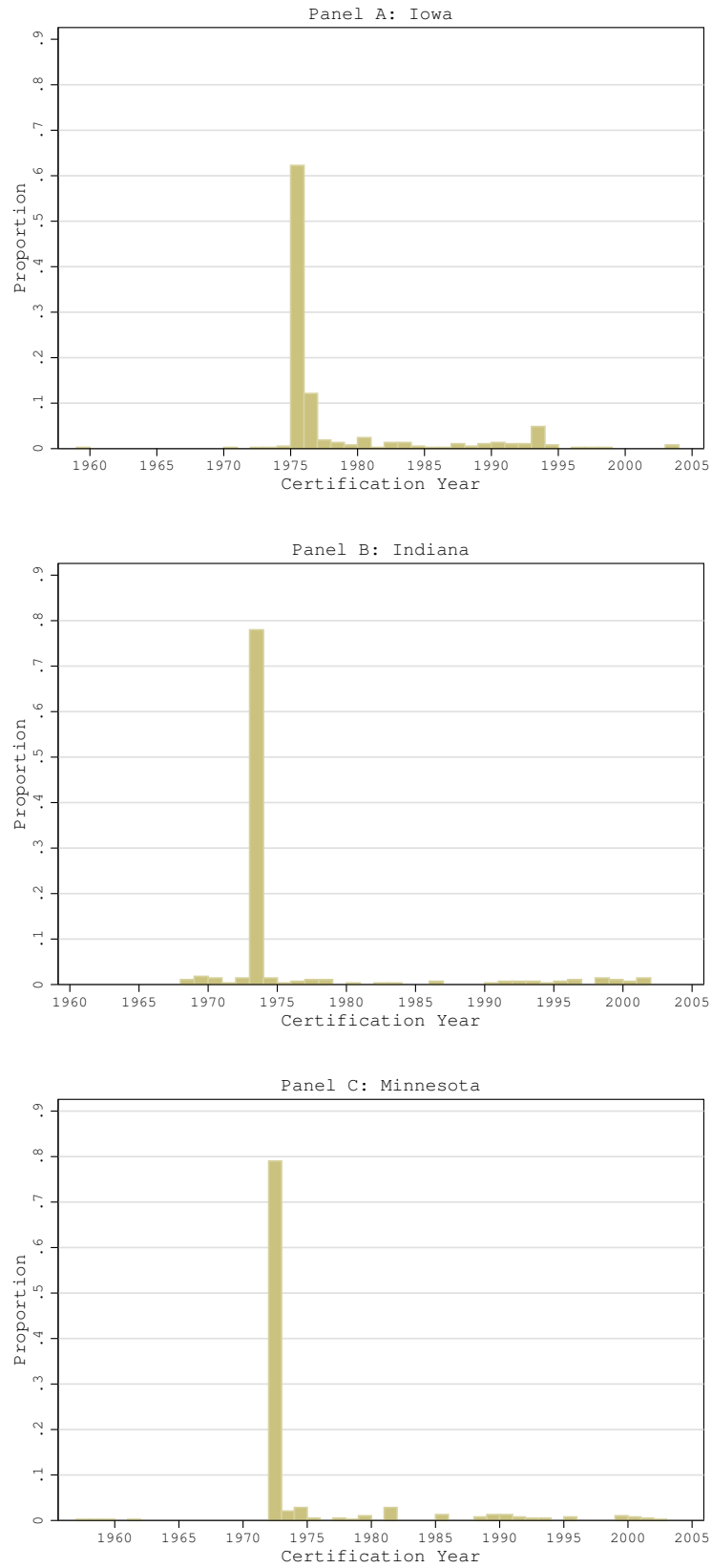
Independent Variable	Dependent Variable: High School Dropout Rate in Percent		
	(i)	(ii)	(iii)
Union	0.100 (0.889)	1.809* (0.933)	2.103** (1.058)
Union*(Union Age)	.	-0.235** (0.093)	-0.190** (0.124)
Union Age	.	.	-0.095 (0.124)
Log Population	23.482** (3.624)	23.538** (3.620)	23.557** (3.610)
Percent Urban	1.633** (0.700)	1.545** (0.700)	1.517** (0.703)
Log Average Income	-13.861** (3.216)	-13.376** (3.192)	-13.326** (3.190)
Log Median Rent	2.338 (1.788)	2.207 (1.779)	2.125 (1.768)
Percent Below Poverty	-0.208** (0.100)	-0.194** (0.099)	-0.194** (0.099)
Percent Unemployed	-0.050 (0.099)	-0.026 (0.099)	-0.025 (0.099)
Percent Black	0.057 (0.371)	0.138 (0.376)	0.139 (0.377)
Percent Hispanic	0.266 (0.251)	0.317 (0.252)	0.328 (0.250)
Percent 12–15 Years School	0.009 (0.069)	0.022 (0.070)	0.023 (0.070)
Percent 16+ Years School	0.263** (0.076)	0.256** (0.076)	0.258** (0.076)
Percent Private Enrollment	-0.201** (0.088)	-0.208** (0.087)	-0.207** (0.088)
Log Public School Enrollment	-22.446** (3.209)	-22.483** (3.194)	-22.505** (3.180)
Constant	-18.693 (14.967)	-19.441 (15.131)	-19.237 (15.154)

<sup>1</sup> Source: Author's calculation as described in the text from the 1970, 1980, and 1990 U.S. Census School District Files.

<sup>2</sup> The variable *Union Age* is the number of years relative to election certification, which includes negative values for years prior to certification. For districts that do not unionize, *Union Age* is set to zero. The variable *Union* is a dummy variable equal to 1 if a district has successfully unionized as of that calendar year.

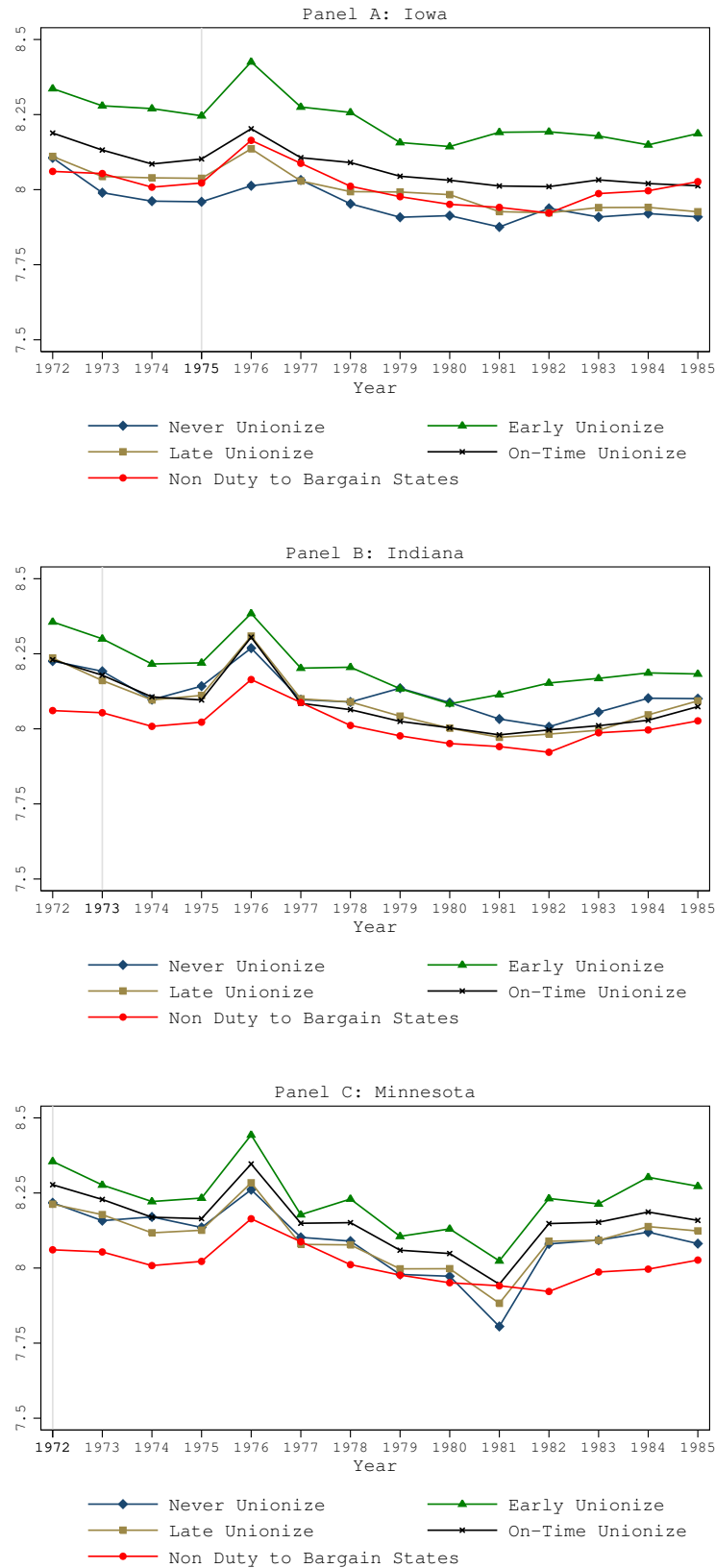
<sup>3</sup> All models include state-by-year and school district fixed effects. Standard errors are clustered at the school district level and are in parentheses: \*\* indicates significance at the 5 percent level and \* indicates significance at the 10 percent level.

Figure 1: Distribution of Teachers' Union Election Certifications by State



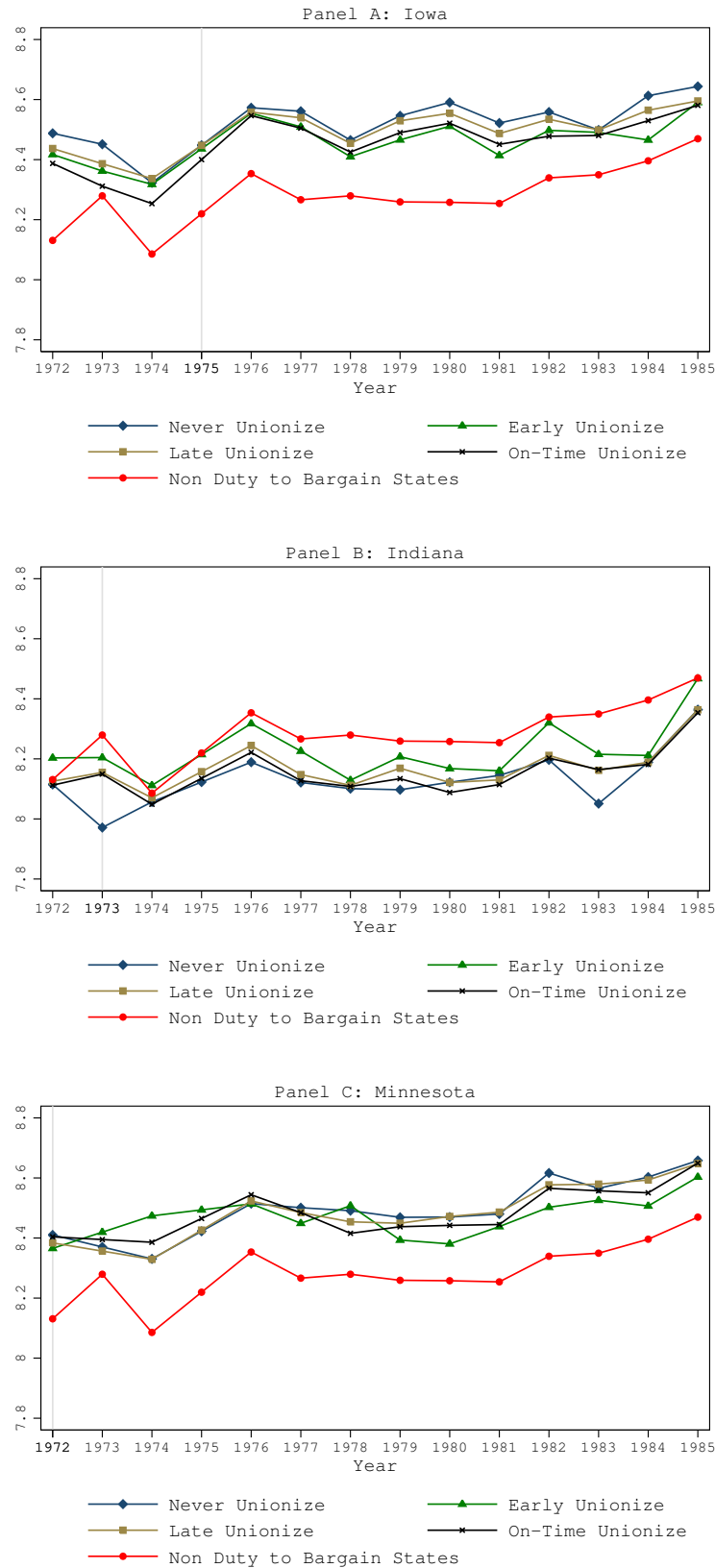
Source: Teachers' union election certification data described in the text.

Figure 2: Trends in Log Real Average Teacher Pay, by State



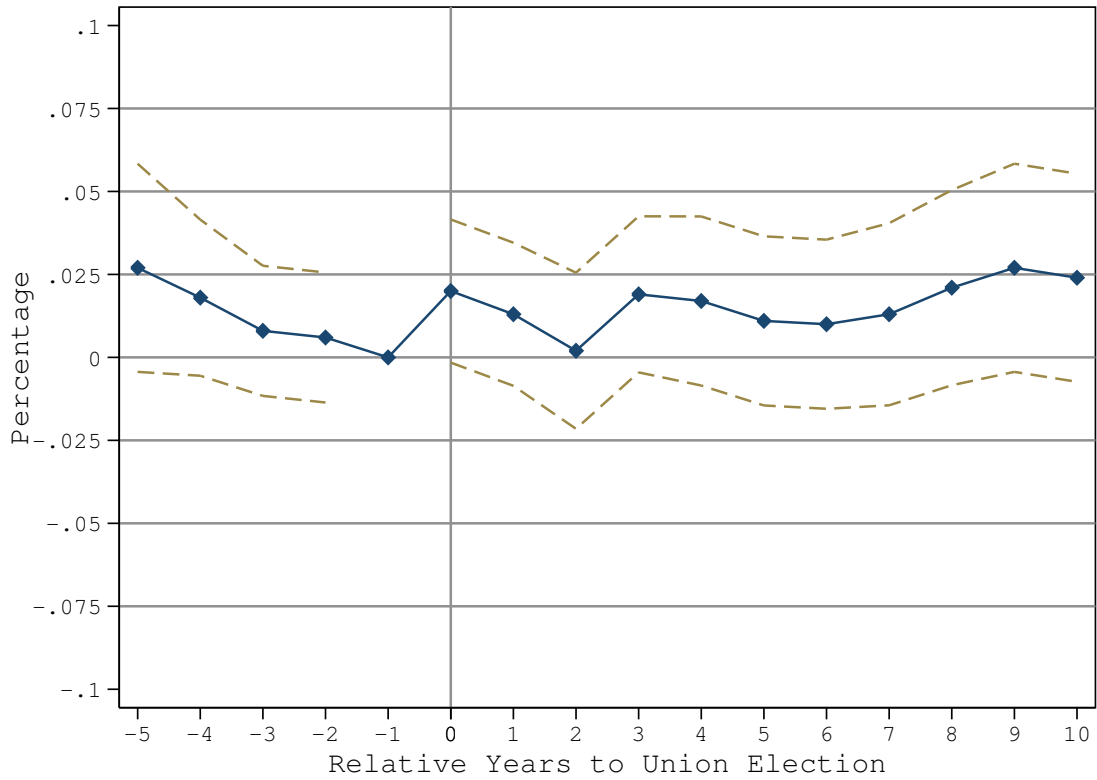
Source: Teachers' union election certification data and Census/Survey of Governments as described in the text. In each panel, the vertical line represents the year in which a duty-to-bargain law was passed in the state. "Never Unionize" districts are those that do not have a successful union election vote by 2004, "Early Unionize" districts are those that unionize prior to passage of their state's duty-to-bargain law, "Late Unionize" districts are those that unionize more than one year after passage of their state's duty-to-bargain law, and "On-Time Unionize" districts are those that unionize within 1 year of passage of their state's duty-to-bargain law. "Non Duty to Bargain States" are the 18 states that did not have a duty-to-bargain law as of 1982: Alabama, Arkansas, Arizona, Colorado, Georgia, Illinois, Kentucky, Louisiana, Missouri, Mississippi, Nebraska, New Mexico, Ohio, South Carolina, Texas, Utah, West Virginia, and Wyoming. All means are inflated to 2004 dollars using the CPI-U.

Figure 3: Trends in Log Real Current Operating Expenditures per Student, by State



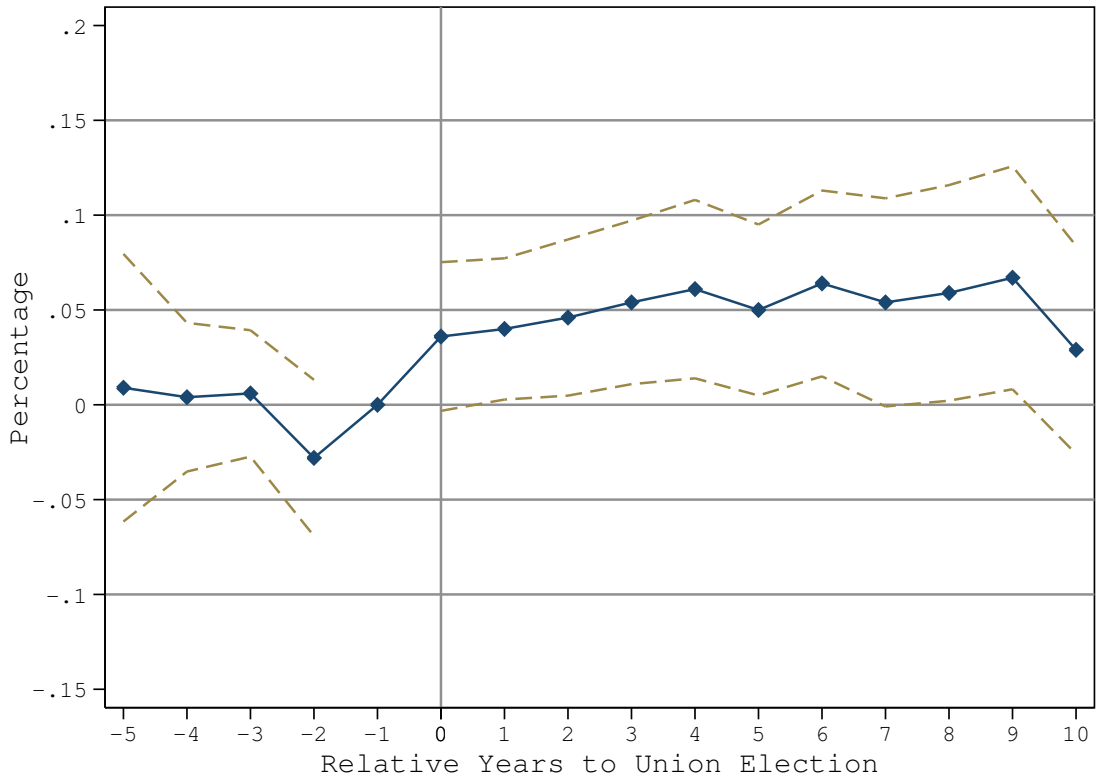
Source: Teachers' union election certification data and Census/Survey of Governments as described in the text. In each panel, the vertical line represents the year in which a duty-to-bargain law was passed in the state. "Never Unionize" districts are those that do not have a successful union election vote by 2004, "Early Unionize" districts are those that unionize prior to passage of their state's duty-to-bargain law, "Late Unionize" districts are those that unionize more than one year after passage of their state's duty-to-bargain law, and "On-Time Unionize" districts are those that unionize within 1 year of passage of their state's duty-to-bargain law. "Non Duty to Bargain States" are the 18 states that did not have a duty-to-bargain law as of 1982: Alabama, Arkansas, Arizona, Colorado, Georgia, Illinois, Kentucky, Louisiana, Missouri, Mississippi, Nebraska, New Mexico, Ohio, South Carolina, Texas, Utah, West Virginia, and Wyoming. All means are inflated to 2004 dollars using the CPI-U.

**Figure 4: The Effect of Teachers' Unions on Log Real Monthly Full-Time Teacher Pay**



<sup>1</sup> Source: Author's calculations from the 1972–1991 Census/Survey of Governments as described in the text.  
<sup>2</sup> The solid line represents coefficient estimates from estimation of equation (2) in the text. Dotted lines are the bounds of the 95 percent confidence interval calculated from standard errors that are clustered at the district level.  
<sup>3</sup> Relative year -1 is omitted in order to make all estimates relative to the year prior to unionization. I include a zero for the point estimates in relative year  $j=-1$ , but the gaps in the standard error bounds reflect that this zero is imposed rather than estimated.

**Figure 5: The Effect of Teachers' Unions on Log Full-Time Teacher Employment**

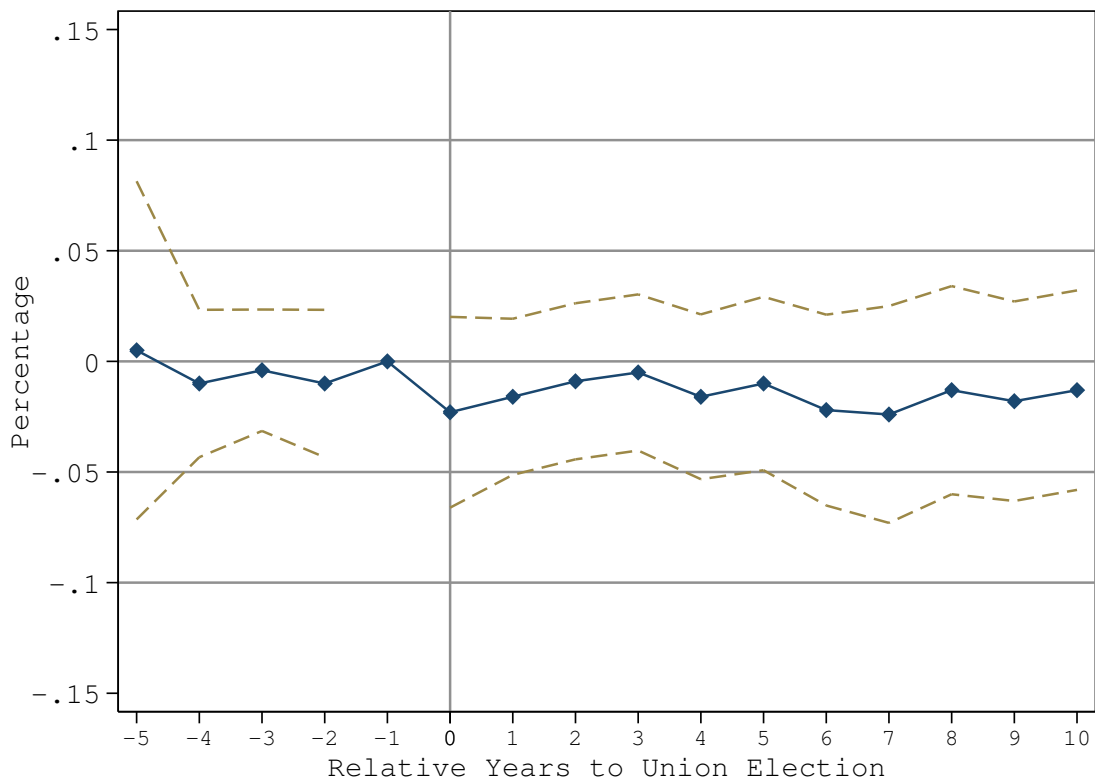


<sup>1</sup> Source: Author's calculations from the 1972–1991 Census/Survey of Governments as described in the text.

<sup>2</sup> The solid line represents coefficient estimates from estimation of equation (2) in the text. Dotted lines are the bounds of the 95 percent confidence interval calculated from standard errors that are clustered at the district level.

<sup>3</sup> Relative year -1 is omitted in order to make all estimates relative to the year prior to unionization. I include a zero for the point estimates in relative year  $j=-1$ , but the gaps in the standard error bounds reflect that this zero is imposed rather than estimated.

**Figure 6: The Effect of Teachers' Unions on Log Student-Teacher Ratios**

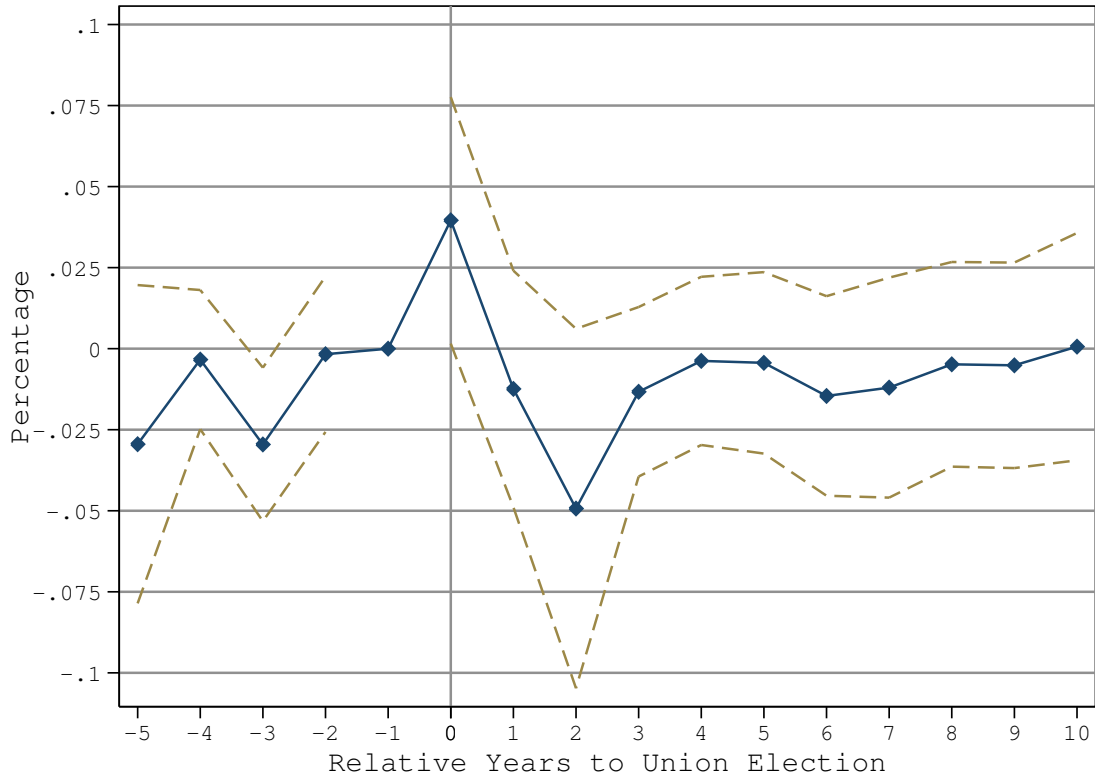


<sup>1</sup> Source: Author's calculations from the 1972–1991 Census/Survey of Governments as described in the text.

<sup>2</sup> The solid line represents coefficient estimates from estimation of equation (2) in the text. Dotted lines are the bounds of the 95 percent confidence interval calculated from standard errors that are clustered at the district level.

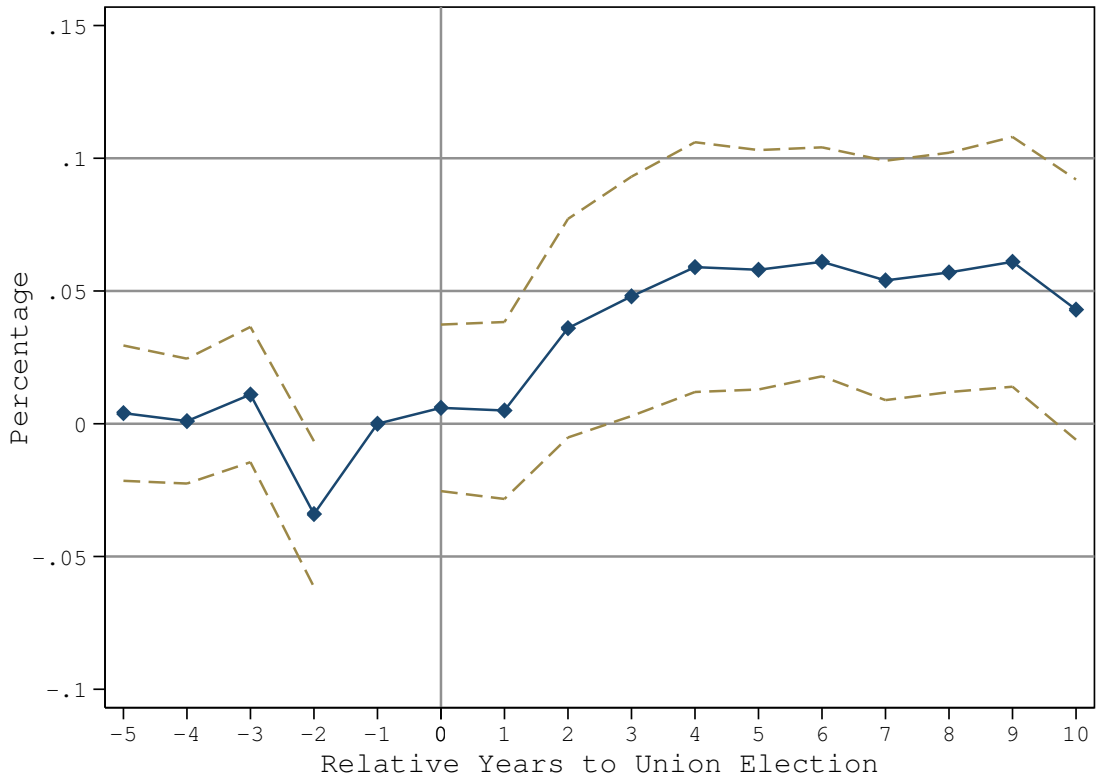
<sup>3</sup> Relative year -1 is omitted in order to make all estimates relative to the year prior to unionization. I include a zero for the point estimates in relative year  $j=-1$ , but the gaps in the standard error bounds reflect that this zero is imposed rather than estimated.

**Figure 7: The Effect of Teachers' Unions on Log Real Current Operating Expenditures Per Student**



<sup>1</sup> Source: Author's calculations from the 1972–1991 Census/Survey of Governments as described in the text.  
<sup>2</sup> The solid line represents coefficient estimates from estimation of equation (2) in the text. Dotted lines are the bounds of the 95 percent confidence interval calculated from standard errors that are clustered at the district level.  
<sup>3</sup> Relative year -1 is omitted in order to make all estimates relative to the year prior to unionization. I include a zero for the point estimates in relative year  $j=-1$ , but the gaps in the standard error bounds reflect that this zero is imposed rather than estimated.

**Figure 8: The Effect of Teachers' Unions on Log Student Enrollment**



<sup>1</sup> Source: Author's calculations from the 1972–1991 Census/Survey of Governments as described in the text.

<sup>2</sup> The solid line represents coefficient estimates from estimation of equation (2) in the text. Dotted lines are the bounds of the 95 percent confidence interval calculated from standard errors that are clustered at the district level.

<sup>3</sup> Relative year -1 is omitted in order to make all estimates relative to the year prior to unionization. I include a zero for the point estimates in relative year  $j=-1$ , but the gaps in the standard error bounds reflect that this zero is imposed rather than estimated.

**Table B-1: Regression Results from Fixed Effects Estimates of Teachers' Union Impacts on Resource Levels from the Census/Survey of Governments Using the Sample Comprised of Never-Unionized Districts and Observations With Relative Years to Union Election Less Than 11**

Relative Time	Dependent Variable: Log of									
	Real Monthly Full-Time Teacher Pay	Full Time Teacher Employment	Student-Teacher Ratio	Real COE Per Student	Total Enrollment	Number of Observations	Percent From Iowa	Percent From Indiana	Percent From Minnesota	Average Calendar Year
-5 Years	0.027* (0.016)	0.009 (0.036)	0.005 (0.039)	-0.029 (0.025)	0.004 (0.013)	120	58.3	17.5	24.2	1978.0
-4 Years	0.018 (0.012)	0.004 (0.020)	-0.010 (0.017)	-0.003 (0.011)	0.001 (0.012)	301	74.4	9.4	16.3	1977.7
-3 Years	0.008 (0.010)	0.006 (0.017)	-0.004 (0.014)	-0.030** (0.012)	0.011 (0.013)	242	85.1	6.3	8.6	1976.3
-2 Years	0.006 (0.010)	-0.028 (0.021)	-0.010 (0.017)	-0.002 (0.012)	-0.034** (0.014)	432	67.8	9.1	23.1	1978.1
0 Years	0.020* (0.011)	0.036* (0.020)	-0.023 (0.022)	0.040** (0.020)	0.006 (0.016)	663	24.4	23.8	51.7	1974.7
1 Year	0.013 (0.011)	0.040** (0.019)	-0.016 (0.018)	-0.012 (0.019)	0.005 (0.017)	588	42.5	31.6	25.9	1976.1
2 Years	0.002 (0.012)	0.046** (0.021)	-0.009 (0.018)	-0.049* (0.028)	0.036* (0.021)	566	42.8	33.8	23.5	1977.0
3 Years	0.019 (0.012)	0.054** (0.022)	-0.005 (0.018)	-0.013 (0.013)	0.048** (0.023)	555	40.4	35.0	24.7	1977.7
4 Years	0.017 (0.013)	0.061** (0.024)	-0.016 (0.019)	-0.004 (0.013)	0.059** (0.024)	725	44.1	32.4	23.5	1978.5
5 Years	0.011 (0.013)	0.050** (0.023)	-0.010 (0.020)	-0.004 (0.014)	0.058** (0.023)	846	37.6	23.5	38.9	1979.0
6 Years	0.010 (0.013)	0.064** (0.025)	-0.022 (0.022)	-0.015 (0.016)	0.061** (0.019)	695	44.6	34.7	20.7	1980.5
7 Years	0.013 (0.014)	0.054* (0.028)	-0.024 (0.025)	-0.012 (0.017)	0.054** (0.023)	890	35.1	27.0	38.0	1980.9
8 Years	0.021 (0.015)	0.059** (0.029)	-0.013 (0.025)	-0.005 (0.014)	0.057** (0.023)	831	37.4	21.5	41.0	1981.9
9 Years	0.027* (0.016)	0.067** (0.030)	-0.018 (0.023)	-0.005 (0.016)	0.061** (0.024)	864	33.7	27.0	39.4	1982.7
10 Years	0.024 (0.016)	0.029 (0.028)	-0.013 (0.023)	0.001 (0.018)	0.043* (0.025)	710	49.6	14.5	35.9	1983.4
Constant	8.095** (0.022)	4.617** (0.041)	2.463** (0.032)	8.758** (0.017)	6.780** (0.019)					
N	8434	8434	7969	12140	12623					
# Clusters	1165	1165	1165	1165	1165					
R <sup>2</sup>	0.739	0.977	0.626	0.604	0.984					

<sup>1</sup> Source: Parameter estimates from estimation of equation (2) in the text.

<sup>2</sup> Regressions include school district and state-specific year fixed effects. All standard errors are clustered at the school district level. \* indicates significance at the 10 percent level and \*\* indicates significance at the 5 percent level.

<sup>3</sup> Relative year -1 is omitted to make all estimates relative to the year prior to certification.