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 the level and allocation of school district resources and on the returns to those resources in the education production function. Employing an event study methodology, I find teachers' unions have no impact on teacher pay, per-student district expenditures or per-student revenues, but they increase teacher employment by between 5 and 9 percent. This employment increase is offset by enrollment increases in unionized districts, causing unions to have little effect on class sizes. Further, I estimate education production functions using high school dropout rates. While there is little evidence of a net union effect on dropout rates, my results are consistent with unions causing an increase in the returns to lower class sizes and higher teacher pay. These findings are in conflict with much of the past literature on teachers' union impacts. I argue a major cause of this discrepancy is due to measurement error in the union measure constructed from survey responses in the Census of Governments. These results highlight the importance of correctly measuring unionization status in union impact studies.

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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. Further, 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.[1]

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 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). However, this argument is invalid if teacher unionization has no negative effect on school districts or students.

The central questions I address in this study are whether teachers' unions distort the allocation of inputs to education or change the returns to those inputs. Unions can impact education production in two ways: they can constrain the ability of administrators to choose freely the level and mix of inputs to education production

[1] Author's calculation from the May 2004 Current Population Survey.

or they can alter the production function itself. These effects will likely be related due to diminishing marginal productivity and the fact unions may change teacher inputs that are either complements or substitutes to other inputs. To the extent affected resources impact educational attainment, unions can have a positive or negative effect on student achievement.

To investigate these issues, I undertake an empirical examination of the effect of teacher organization on the level and composition of school district resources and on the returns to those resources in the education production function. A major impediment to conducting this type of research is 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 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 utilized 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.

I merge the certification data with three different data sets containing school district outcomes: the Census/Survey of Governments (COG/SOG) from 1972–1991, the Elementary and Secondary General Information System (ELSEGIS) from 1967–1979, and the school-district level summaries from the U.S. Census of Population and Housing from 1970–1990. As necessitated by the data, the level of analysis in this project is the school district, not the individual school.[2]

I employ an "event study" methodology that includes dummy variables for each relative year to unionization in order to analyze the impact of unions on district resource levels and expenditure allocations. This event study framework is unique in the teachers' union literature as it required knowledge of each district's union status in each year covered by the sample. The union election data I collect accurately

[2]This aggregation likely causes few problems because the unionization decision occurs at the district level. However, I will not be able to detect differential effects across school types within school districts insofar as they exist.

measure union status for each district in each year, which allows me to estimate the time pattern of teachers' union effects rather than just short-run or long-run effects. By analyzing the change in district resources over time relative to union elections, I trace out the time pattern of union effects in a manner that puts little structure on this pattern. Further, by examining the pre-election trends, I also 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.[3]

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.[4] I find full-time teacher employment increases by between 5 and 9 percent, but unionization is also associated with an increase in enrollment in union relative to non-union districts, which almost fully offsets any reductions in the student-teacher ratio due to the employment increase. While the relative enrollment increases in newly unionized districts could be evidence of selection bias in my estimates, my results point instead to sorting of students into unionized districts to take advantage of the higher number of teachers. My results further indicate that current operating expenditures and district revenues per student respond negligibly to teacher unionization.

I analyze the effect of teachers' unions on the allocation of school district expenditures as well. The share of total expenditures that go to instruction (including teacher pay), administration, attendance and health, transportation, plant operation

[3] The only other union effects paper to employ an event study methodology is Lee and Mas (2007), who estimate the effect of private sector unions on excess stock market returns. There are no existing public sector union analyses that are able to estimate the time pattern of union effects.

[4] 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.

and maintenance, and fixed charges are largely unaffected by teacher unionization.

Finally, I estimate education production functions using the high school dropout rate as the measure of educational output. The 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 teacher productivity, student educational outcomes, and unionization. I find teachers' unions have no net effect on dropout rates, but there is evidence they increase the returns to educational resources such as teacher pay and class size. These results are consistent with unions increasing the efficiency of teacher-based inputs into education production.

My findings are provocative in that they conflict with much of the previous literature on teachers' unions.[5] Utilizing 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 utilizes 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

[5]See Freeman (1986) for an overview of the literature.

the district reporting it engages in collective bargaining,[6] increases average teacher pay by over five percent and current operating expenditures by almost three percent, while decreasing student-teacher ratios by 1.1. She also reports evidence unions increase high school dropout rates as well as reduce the returns to expenditures on teachers.

Given the differences between the estimates I present in this study and many of the estimates reported in previous empirical studies, it is important to understand whether the union effects in Iowa, Indiana, and Minnesota differ systematically from those of the U.S. as a whole or if there are other explanations that may point to biases in my, or other, estimates.[7] Further, because I am utilizing an event study framework rather than straight difference-in-difference or instrumental variables approaches common in the literature, the methodological differences may explain some of the divergence in findings.

As a means to understand these differences, I compare my results to those presented in Hoxby (1996) as this is the most comprehensive, sophisticated, and thorough teachers' union impact study to date. I present evidence that our results disagree not because of sample differences nor methodological differences but rather because of non-classical measurement error in the union measure Hoxby (1996) employs in her study that can explain a significant portion of the difference between our estimates. The comparison between my results and those found by Hoxby (1996) points to potential measurement problems with the Census of Governments Labor Relations Survey rather than any analytical or coding errors committed by the researcher in this 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 utilize are a more reliable measure of

[6] Hoxby classifies a school district as unionized only if all three of these conditions are met.

[7] There is reason to believe the union effects in the three states in this analysis would differ from the rest of the country. Iowa, Indiana, and Minnesota had high private-sector union representation due to the prevalence of manufacturing in these states in the 1970s. This pattern, combined with high public sector unionization rates in neighboring states, suggests public sector unions might not have been as controversial or undesirable to residents as in other areas of the country.

union status than those used in previous work.

The rest of this paper is organized as follows: Section 2 provides a brief overview of theoretical predictions of union effects on school district resources and student achievement. Section 3 describes the data used in the analysis with a particular focus on the election certification data. Section 4 presents the empirical methodology and results for the analysis of union effects on educational resources, while Section 5 contains the methodology and results pertaining to the effect of unions on the education production function. In Section 6, I analyze the impact of measurement error on union impact estimates as a means to explain the differences between my results and previous findings. Section 7 concludes.

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. Further, 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.[8]

[8]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.

Even a purely rent-seeking union may have a non-negative effect on student achievement. Because unions are often 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 typically referred to as a "union voice" effect, and there is evidence in the private sector union literature that giving workers a voice with which to change their working environment increases productivity (see Gunderson (2005) for an overview). If teachers protect themselves from perceived or actual administrative abuses through the exercise of 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,[9] 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 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.

[9]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).

3 Data

3.1 Teacher Union Election Certification Data

Studies of the impact of teachers' unions have traditionally utilized 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 is typically whether the teacher is a member of a union (Moore and Raisian, 1987 and Baugh and Stone, 1982).[10] There are, however, several problems with such measures that can bias impact estimates. The largest problem with union membership data is teachers can be employed in unionized districts without being members of the union. Further, 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.[11] The differences between union membership and the existence of a union for the purpose of collective bargaining in a given teacher's district will likely cause an attenuation bias in the estimates of union impacts.

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).[12] Absent measurement error, a collective bargaining agreement will accurately measure the presence of a union as long as all unions obtain contracts.[13] 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

[10]Examples of data sets that include teacher union membership are the Sustaining Effects Study, the Current Population Survey, High School and Beyond, and the Panel Study of Income Dynamics.

[11]Both the NEA and the AFT began this way before the official onset of collective bargaining for teachers.

[12]Information on the existence of collective bargaining agreements generally comes from either the Sustaining Effects Study or the Census of Governments.

[13]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.

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

Despite the accuracy of the union election certification data, there are some instances in which the certification date will mis-represent the true date of unionization. If two districts merge, necessitating a new union election, then the election data will often assign this merger date as the date of unionization even if both districts were unionized prior to the merger. Further, prior to the certification vote, some unions were "voluntarily recognized" by their school district for the purpose of collective bargaining.

To reduce the measurement error in my union measure, I augmented the validation data by searching for case law on LexisNexis as well as 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. Further, because the unions in the three states in this analysis are all members of the National Education Association (NEA), groups of locals are aggregated into "UniServ" districts. The UniServ offices oversee the bargaining and governance of each of the union locals in their district. 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 PERC office.

I chose Iowa, Indiana and Minnesota for this analysis because all three states passed "duty-to-bargain" laws in a time period covered by my outcome data. Prior to 1972, all states in my sample 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.[14] 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, these 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 desire such negotiation. 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,[15] the election certifications measure the time of first organization for the purpose of collective bargaining.[16] 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.

The union certification data have several advantages over the measures used in

[14]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

[15]When I exclude voluntarily recognized unions from the analysis, the results are unchanged.

[16]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.

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. If one is interested in the effect of teachers' unions on school districts rather than the effect of collectively bargained contracts on school districts, this measure is more appropriate than ones previously used. However, the validation study showed, in the vast majority of cases, unions negotiate a contract within one school year of certification. I found no districts in which the union did not achieve a contract. This result suggests, 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. The latter professional group will not engage in a unionization election.

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 obtained certification 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.[17] 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.

[17]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.

3.2 Other Data Sources

I combine my teachers' union election certification data with three data sets that contain outcome variables of interest. The first data set I use is the Census and Survey of Governments (COG/SOG) Employment and Finance Surveys. I construct measures of real monthly full-time teacher pay, full-time teacher employment, student-teacher ratios, real current operating expenditures (COE) and real total revenues for each district in the sample. 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 and measures the human resources per student available in each district. I have district-level observations for the years 1972-1991, excluding 1975 and 1986 due to data availability. Appendix A contains further details about the Census and Survey of Governments data.

In order to estimate the impact of teachers' unions on the intra-district allocation of expenditures, I use the Elementary and Secondary General Information System (ELSEGIS).[18] The ELSEGIS survey was conducted in 1967-1970, 1973-1974, 1976-1977, and 1979. Unfortunately, the survey was terminated in 1979 without a suitable replacement, but the years in which it was conducted correspond to the highest unionization activity in my sample (see Figure 1). ELSEGIS asks school districts for expenditures broken down into six mutually exclusive categories: administration, total instruction, attendance and health, transportation, plant operating and maintenance, and fixed charges. Aside from fixed charges, these categories constitute current operating expenditures in each school district. As with the COG/SOG data, all expenditures are inflated to real 2004 dollars using the CPI deflator.

[18]ELSEGIS is a precursor to the Common Core of Data and contains detailed revenue and expenditure data for a random sample of school districts in the United States.

I use high school dropout rates calculated from the 1970, 1980 and 1990 U.S. Census as my measure of educational attainment in order to estimate the impact of unions on the education production function.[19] I measure high school dropout rates using the following formula:

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

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 for each district in my sample.

4 The Effect of Teachers Unions on The Inputs to Education Production

4.1 Empirical Methodology

To analyze the effect of teachers' unions on the level and allocation of school district resources, I utilize an empirical methodology derived from the event study literature. I estimate the following equation on the Census/Survey of Governments and ELSEGIS data sets described in the previous section:

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

where Y_{ist} is the log of an outcome variable of interest, ϕ_{st} are year fixed effects that

[19] 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).

are separate for each state, τ_i are district fixed effects, and ϵ_{ist} is an error term. The term 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 example, if district i successfully completed a union election in 1975, $I(t - \text{year}_c = 5)$ would equal one in 1980 only and would equal zero in all other periods for that district. 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.

In the regressions using COG/SOG data, I set k equal to 10, meaning the event window spans from five years prior to certification to ten years after unionization. I choose this event window because sample sizes drop outside of this range. When I estimate equation (2) using outcome variables from the ELSEGIS survey, I set k equal to 7 due to the same sample size considerations. All district-year observations for which the time since certification is greater than k years are dropped from the analysis. Although all the qualitative results and conclusions remain unchanged, the standard errors on the relative time dummies outside the event window became noticeably larger when these variables were included in the regressions.

Due to data limitations, previous studies have been constrained to model 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[20] estimates both short-term and long-term effects of unionization; the inclusion of dummy variables for each relative year 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.[21] Thus, the full

[20]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.

[21]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 may also 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.

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 in order to take advantage of the panel data. This contrasts with most of the previous work on union impacts, which has been cross sectional (Freeman, 1986). Such a design is often 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, for example), cross-sectional estimates will be biased. In contrast to a cross-sectional model that compares outcomes across different districts, the fixed effects model compares the same district at different times relative to the unionization year and controls for any unobservable (and unchanging) effects.[22]

The central identifying assumption of the model is

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

Satisfying (3) necessitates that, conditional on the fixed effects, the timing of unionization is uncorrelated with the outcome variables. If there is selection into unionization based on pre-union wages, expenditures, or revenues, estimates of the γ_j parameters from equation (2) will be biased. For example, if a trend of decreasing salaries causes teachers to organize into a union, the estimated union wage effect will be biased towards zero. Because close to 85 percent of the school districts that unionize do so within one year of the passage of their state's duty-to-bargain law, such selection is not likely to be a confounding factor. In addition, if school boards anticipate unionization and enact policy to attempt to defeat the organization move-

[22]Because the outcome measures of interest are correlated across time within districts, traditional OLS standard error estimates will be biased. To correct for this bias, all standard errors are clustered at the school district level. It is also possible that outcomes are spatially correlated. I performed a diagnostic where I clustered at the county level; the standard errors were unchanged. I also directly calculated the spatial correlation of the errors (ϵ_{ist}) and found little evidence of such correlation, especially for school districts more than 10 miles apart. Spatial correlation graphs are available from the author upon request.

ment in the district, it will become apparent in the pre-election relative time to unionization estimates. I therefore estimate γs 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. Note 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.[23] I find little evidence of selection or "anticipation" effects in the the results below.

The COG/SOG and ELSEGIS surveys contain no school district demographic information. Given this limitation, it is important to think about why school districts unionized when they did and what determined whether they certified directly after the duty-to-bargain law change or later. 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 1 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 a lower median rent than districts that unionize. Columns (iv) and (v) in Table 1 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

[23] Because the school district panel is unbalanced with respect to relative time to unionization, each γ_j is identified off of a potentially different set of school districts. This will cause the estimates to be biased if there are unobserved (or unmodeled) heterogeneous treatment effects. To test for this source of bias, I run 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 treatment observations fall into this group. These results suggest the unbalanced panel used in this analysis does not cause a bias in the estimates due to heterogeneous treatment effects over time.

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.[24]

What effect can one expect these differences to have on the estimates from equation (2)[25] 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 will only 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 ϕ_{st} is therefore the main difficulty in estimating the treatment effect on the treated using equation (2).

The year effects are identified off year-specific variation in the dependent variable from treated observations and from the control group (i.e., non-treated observations). If the year coefficients were identified solely off of the control group observations, equation (2) would be identical to a traditional difference-in-difference estimator with non-parametric time trends. While this restriction does not hold for equation (2),[26] the main source of variation off of which the state-year effects are identified

[24] 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 to specifically target city school districts as a response to AFT successes there. See Murphy (1990) for a detailed history of teacher organization.

[25] 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 urbanized areas, the panel becomes much more balanced with respect to the observables in Table 1. However, the union impact estimates do not change appreciably nor do the substantive conclusion from those estimates change when this restriction is imposed.

[26] I perform a sensitivity analysis in which I impose this restriction. For each treated observation of district i in year t, I construct a state-specific year fixed effect constituting the state-specific demeaned average of the dependent variable from never-unionized districts. I difference out this fixed effect from the dependent variable for each treated observation. I then regress this difference on a set of relative time dummies, clustering the standard errors at the school district level. Estimates and 95 percent confidence intervals are calculated by bootstrapping this process. While this methodology does increase the noise in the estimates as well as the size of the 95 percent confidence intervals, it does not change the main substantive conclusions drawn from estimating equation (2). The exception is for teacher pay: the difference-in-difference estimates imply a reduction in real teacher pay of close to 7.5 percent due to unionization occurring 6 years after union election certification that is not present in the estimates of equation (2). Full results are available from the author upon request.

is the control group districts, and one can interpret the γs as difference-in-difference estimates.[27]

In the results presented in Section 4.2, I estimate equation (2) using two different samples, each of which implies a different control group. The first sample I utilize is all districts that never unionize combined with all district-year observations for which the relative time to union election is less than or equal to k. 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 utilizes all observations that are arguably unaffected by the treatment, which allows for the most power in identifying all parameters of equation (2). Results from estimation of equation (2) on this sample are reported in Panel A of each figure in Section 4.2.

Alternatively, in Panel B of each results figure, 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 Panel B sample therefore excludes all district-year observations for which the relative time to unionization is less than -5 years.[28] The control group implied by this estimation sample is comprised of only the never-unionized districts and is more clearly defined relative to the Panel A control group because the proportion of districts that do unionize and never unionize is not changing over time. Further, if there are union effects on the dependent variable more than 5 years prior to unionization, the estimates reported in Panel A will be biased, but not those reported in Panel B.

As a complete set of relative time dummy variables always sums to one for a district that unionizes in the estimation sample used in Panel B, the relative time

[27] To test this assertion, I split the sample of those who unionize into two groups and ran equation (2) with a group identification dummy interacted with relative time dummies using never-unionized districts as a control group. I then dropped one of the groups and re-ran equation (2). The estimates of the year fixed effects changed between the two regressions, which would not be true if the year effects were identified solely off of variation from the control group districts. However, the year effects did not change appreciably, which leads me to conclude that variation from the control group districts constitute the main source of identification of ϕ_{st} in equation (2).

[28] For example, in the 1975 COG survey, a district that unionized in 1982 will be part of the Panel A sample but not part of the Panel B sample.

dummies and the district fixed effects will be collinear unless I drop one of the relative year dummy variables. While this procedure is not necessary for the Panel A sample, I drop the relative time indicator variable for j=-1 (the year prior to unionization) throughout this analysis for ease of comparison. The γ_j coefficients therefore identify treatment effects relative to the effect for the year prior to unionization, γ_{-1}.[29] Note in Figures 2-8 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.

To assess the fragility of my results to the choice of estimation sample, I run sensitivity analyses of equation (2) using additional samples that each imply a different control group. In addition to the two samples listed above, I obtain estimates using only those district-year observations for which the relative time to unionization is less than or equal to k. This sample is the same as the one used in Panel A, 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. I also estimate equation (2) using all observations. This sample adds those observations for which the relative time to unionization is greater than k to the Panel A sample, and I include a relative time to union election dummy variable that equals 1 if a district has been unionized for more than k years to equation (2). Estimates from these robustness checks are strikingly similar in both magnitude and quality to those presented in Section 4.2 and are available from the author upon request.

Unfortunately, lack of sufficient pre-treatment data precludes comparing pre-treatment trends in the dependent variables among treated and untreated districts to directly test the validity of the control groups. However, the robustness of my estimates and conclusions to the use of various estimation samples suggests lack of a control group similar to the treated group on the observables does not limit my

[29]These coefficients will be identical to the non-relative treatment effects if γ_{-1} is zero. To test for this possibility, I include the j=-1 relative time dummy variable in the specifications reported in Panel A of the results. In no case were the coefficients on the j=-1 dummy statistically distinguishable from zero even at the ten percent level. These results suggest it is not incorrect to interpret the relative time coefficients as treatment effects.

ability to identify the ATT using equation (2). It is also important to stress that because the unionization decision is discrete and long-run trends are more gradual, the short-run union impact estimates will identify the ATT even without an adequate control for these long-run trends.

A further challenge to identifying the ATT using equation (2) is the potential for spillover effects of unionization on outcomes of non-union districts. For example, if non-unionized school districts raise wages in order to keep teachers from becoming unionized or in order to attract higher quality teachers in the presence of a positive union wage differential, the union impact estimates will be biased towards zero. While there is little discussion and evidence regarding spillover effects in the teacher unionization literature, studies focusing on private–sector unions 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).

To determine the degree to which spillover effects bias the estimates reported below, I run "false" experiments using the never-unionized school districts. I set the "unionization date" to be the year a district's state passed its public sector duty-to-bargain law and then calculate relative time to unionization accordingly. I estimate equation (2) using only these districts and variable definitions. This false experiment yields insight into the effect of a rapid diffusion of teachers' unions on the outcomes of those districts that do not unionize. The results from these tests show little evidence of spillover effects. Nonetheless, results reported below should be interpreted with care due to the possibility that the untreated districts are affected by the treatment.

4.2 Results

4.2.1 Union Impact on Resource Levels

Figures 2-6 depict the estimates of γ_j from equation (2) for log real monthly full-time teacher pay, log full-time teacher employment, log student-teacher ratios, log real current operating expenditures per student, and log real revenues per student, respectively. In each figure, the solid line indicates the point estimates of the γ 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. Full regression estimates for the results in Panels A and B are reported in Appendix B, Tables B-1 and B-2 respectively.

The results consistently indicate unions have little impact on school district resource levels. Focusing on Figure 2, there is no evidence teachers' unions increase teacher pay in either specification;[30] none of the point estimates is statistically distinguishable from zero at the 5 percent level, and most are less than 1% in both panels. Further, there are no evident pre-election trends or anticipation effects that suggest there is selection in union election timing based on teacher pay.

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 2.[31] 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 3 and suggest employment increases immediately following unionization by close to 5 percent. The effect increases over time and ultimately reaches 9 percent. Further, the majority of these estimates are statistically distinguishable from zero at the 5 percent level and

[30]It is important to note these are average wages. Unions may change the wage structure within districts without shifting the mean.

[31]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.

all are significant at the 10 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.[32]

The employment results in Figure 3 suggest student-teacher ratios should decrease. Figure 4, however, illustrates winning a unionization election has little effect on this class size measure for either specification. While all point estimates for $j>0$ are negative, none is statistically significant.

Given the significant increase in full-time teacher employment, why is there no commensurate decrease in student-teacher ratios? There are two possible explanations for this result: full-time-equivalent teacher employment does not change while full time employment does, or enrollment increases. To explore the first possibility, I estimate equation (2) using log full-time-equivalent teacher employment rather than log full-time teacher employment as the dependent variable.[33] The results are similar to those reported in Figure 4, suggesting unionization causes the same response in the two types of employment measures.

To investigate the second explanation, I run equation (2) using log student enrollment as the dependent variable. Results are reported in Figure 7. They indicate log enrollment is unaffected in the first two years following unionization but then increases to between 5 and 8 percent over the next three years and remains in this range for the remainder of the event window. Taken together, these estimates suggest teacher employment increases immediately upon unionization, but within four years after certification, enrollment expansion in treated relative to control districts undoes much of the impact on class sizes that occurs from increased teacher employment. That teachers' unions have little long-term effect on student-teacher ratios can be attributed largely to the relatively fast enrollment increases in newly unionized

[32]Figure 3 could also 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.

[33]In addition to instructional employees, "teachers" in the Census and Survey of Governments include educational support staff and school-level administrators, such as principals. Both full time and full time equivalent teacher employment include the same categories of staff members. The major difference between them is the proportion of each staff type that is full time or part time.

districts, which raises student-teacher ratios to near their pre-union levels.[34]

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.[35] Figures 5 and 6 examine this possibility by analyzing the effect of teachers' unions on log real current operating expenditures per student and log real total revenues per student, respectively.

Because current operating expenditures represent the bulk of total expenditures, it is not surprising Figures 5 and 6 are similar. In both figures, there is considerable variation in the estimates, with an upward spike of 3.8 to 4.8 percent respectively in Figures 5 and 6 for the first year of unionization, though the estimate is not statistically significant at the 5 percent level in Figure 5. After the first year, these estimates become negative, remain close to zero in magnitude, and are not statistically significant for the remainder of the event window. Note these graphs represent changes in per-student expenditures and revenues. As enrollment is increasing by between 5 to 8 percent over this period in unionized districts relative to control districts, total expenditures and revenues do increase, though not enough to keep up with the enrollment increases. One interpretation of Figures 5 and 6 is teachers' unions successfully guard against per-student revenue and 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 these outcomes.

[34] 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.

[35] 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.

4.2.2 Union Impact on the Distribution of Expenditures

The results shown in Figure 5 suggest teacher unionization has, at most, a small effect on the total level of current operating expenditures per student. This finding does not necessarily imply unions have no impact on resource allocation, because unions may change the composition of expenditures without affecting the level.

Figure 8 presents estimates from equation (2), where the dependent variable is the log share of total expenditures going to instruction. The results indicate unionization has little effect on this share. None of the point estimates in either panel is statistically distinguishable from zero at the 5 percent level, and they range from -2.1 to 1.9 percent. It is interesting that the share of expenditures on instruction is essentially unchanged by unionization, as Figure 3 suggests full-time employment is increasing. If the increase in the total teacher wage bill is not fully made up by revenue increases, unionization can cause reductions in other instructional expenditures. Such a redistribution can have either positive or negative effects on student achievement, depending on the efficiency of expenditures prior to unionization.

In addition to instruction, I estimate the effect of unions on the proportion of expenditures going to administration, attendance and health, transportation, plant operating and maintenance (O&M) and fixed charges. While I find little evidence of union effects on these proportions,[36] the confidence intervals are large and make it hard to draw strong conclusions. Full regression results can be found in Appendix B, Tables B-3 and B-4.

4.2.3 Discussion

Taken together, the results presented above suggest teachers' unions have little net effect on the level and allocation of resources per student. What theories of school district and union behavior might be consistent with my findings? One model that

[36] The exception is for transportation expenditures. However, this result is likely due to the fact the laws requiring school boards to negotiate with teachers' unions applied to all public sector employees, including bus drivers. The increase in the proportion of expenditures going to transportation is in all likelihood a result of transportation employees organizing, not teachers' unions.

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 consistent with more school resources leading 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. If districts that are expecting future enrollment changes are more likely to unionize in order to keep their per-student resource levels constant, this model of union behavior would produce the data patterns reported in Section 4.2.1. To test this 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 any 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 run equation (2) using only those districts that have no block points in an urban area at any point in the sample period. The results are both qualitatively and quantitatively indistinguishable from those presented above.[37] The

[37]It would be ideal to be able to control for secular trends in population mobility at the district level by including a measure of the change in the number of families without children in each district. Unfortunately, no such measure

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 population. That the relative enrollment increases in unionized districts negates any class size and per-student expenditure and revenue gains from rising employment, expenditure and revenue levels following unionization, however, is unambiguous in the data.

Another explanation for my results is teachers unions may simply be ineffective at influencing resource allocation. This could occur if teachers' 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 teachers' 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, rather than salary increases.[38]

A final explanation for union ineffectiveness is unions may be focusing their resources on negotiating over work rules and practices rather than wages. There is much anecdotal and historical evidence unions fundamentally change workplace practices (Moe, 2001; Murphy, 1990; Retsinas, 1982; and Johnson, 2004). What is poorly understood, however, is how these changes influence teacher productivity and the returns to teacher based inputs into education production. I next turn to an empirical analysis of the education production function in order to test for such effects.

exists at the school district level for intercensal years.

[38]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.

5 The Effect of Teachers' Unions on the Education Production Function

5.1 Empirical Methodology

The education production function describes how inputs are transformed into educational outputs. In order to test whether teachers' unions change this relationship, I estimate linear education production functions that include interaction terms of union status with educational inputs. I proxy the inputs to education production with log real average teacher pay and the student-teacher ratio. My measure of educational output is the high school dropout rate. This analysis will therefore 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 event study methodology given by equation (2) due to the fact there are relative years to union election with no observations. Thus, I will be unable to detect subtleties in the time pattern of union impacts on the education production function. Instead, I will estimate the average effect of unions on high school dropout rates and on the returns to education inputs over all relative time periods included in the analysis. I estimate linear education production functions of the form:

$$\begin{aligned}\text{Dropout Rate}_{ist} &= \beta_0 + \beta_1 \text{Union}_{ist} + \beta_2 \frac{\text{Student}}{\text{Teacher}}_{ist} + \beta_3 \frac{\text{Student}}{\text{Teacher}} * \text{Union}_{ist} \\ &\quad + \beta_4 (\text{Log Teacher Pay})_{ist} + \beta_5 (\text{Log Teacher Pay}) * \text{Union}_{ist} \qquad (4) \\ &\quad + \delta X_{ist} + \tau_i + \phi_{st} + \epsilon_{ist},\end{aligned}$$

where *Union* is a dummy variable equal to 1 if a district has successfully completed a teacher union election, X is a vector of demographic characteristics listed in Section

3.2, and all other variables are as previously defined.[39]

I use two different samples to identify the parameters in equation (4). The first sample includes all observation from 1970 and 1980, excluding the 1990 observations. I exclude 1990 because I want to identify the state-specific year effects only off of school districts that are not unionized. The results from this specification are presented in Columns (i) and (ii) of Table 2. I also estimate equation (4) using all three years of data. The state-specific year effects therefore include variation from unionized districts in this specification, but the sample size increases substantially.

I estimate equation (4) both with and without the union interaction terms. In the specifications that leave out these interaction terms, β_1 gives the average effect of teachers' unions on high school dropout rates. In the specifications that include the union interaction terms, estimates of β_3 and β_5 test whether teachers' unions change the returns to expenditures on class size and teachers, respectively. A positive β_3 will imply unionized districts have a higher return (as measured by dropout rates) to lowering class size. Similarly, $\beta_5 < 0$ means unionized districts have a higher return to increasing teacher pay than non-unionized districts. In other words, β_3 and β_5 measure the degree to which unionized districts differ in their ability to transform class sizes and teacher pay into reduced dropout rates.[40]

5.2 Results

Results from estimation of equation (4) are presented in Table 2. The coefficient on the Union variable in columns (i) and (iii) gives the net effect of teachers' unions on high school dropout rates (in percent). I estimate unions decrease high school dropout rates by 0.140 percent using the 1970 and 1980 data and raise dropout rates

[39] 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 teacher inputs, teachers' unions, and high school dropout rates.

[40] It is equally valid to interpret β_3 and β_5 as measuring heterogeneous treatment effects of unions. Under this interpretation, β_3 shows how union effects differ by student-teacher ratios, and β_5 describes the heterogeneity across the average teacher wage distribution.

by 0.202 percent using observations from all three years. Neither of these coefficients is significant at even the 10 percent level. These results are suggestive that, on average, teachers' unions do not influence high school dropout rates.

The Union interaction terms reported in columns (ii) and (iv) of Table 2 allow me to test for the effect of teachers' unions on the education production function. In both columns, the Union, Student-teacher Ratio interaction term is positive and statistically significant at the 5 percent level. Again, the magnitudes are small: in the specification using only 1970 and 1980 observations, a 1 unit decrease in the student-teacher ratio reduces the percent of high school dropouts by 0.44 in unionized districts relative to non-unionized districts. These estimates provide evidence lowering class size is more effective at reducing dropout rates in unionized than non-unionized school districts. The coefficient on the interaction between Log Teacher Pay and Union is negative and of sizeable magnitude, though it is only significant at the 5 percent level in column (ii). The estimates suggest increasing teacher pay has higher returns to reducing dropout rates in unionized as opposed to non-unionized districts.[41]

It is important to note the results from Table 2 measure the net change in the productivity of teacher-based inputs into education production. 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 2 is suggestive that the positive productivity effects of unionization outweigh the negative effects, on average. This finding is consistent with studies such as Eberts and Stone (1987), who find unions increase education productivity. However, this productivity

[41] Under the heterogeneous treatment effect interpretation of equation (4), these results suggest unions have a larger effect on reducing dropout rates relative to non-union districts when class sizes are lower and teacher pay is higher.

increase is achieved without the coincident increase in teacher pay that is typically associated with teacher unionization.

6 A Discussion and Interpretation of Different Union Measures and Varying Union Impact Estimates

6.1 A Comparison of Alternative Unionization Measures

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. However, in providing detailed union histories of each school district in the sample, my union measure differs from the cross-sectional measures used by previous researchers in ways that may yield insight into the robustness of those and my estimates.

In order to understand these differences more fully, it is instructive to first compare my union election certification data and the union measure constructed from the Census of Governments Labor Relations Survey used in Hoxby (1996) as this is the only other available district-level panel data union measure. The COG does not directly ask respondents about the existence of a teachers' union or a contract with that union. Instead, it contains three survey items related to labor relations that can be used to infer union status in a district:

1. Total number off full-time teachers who are members of an employee organization.
2. Does your agency engage in collective negotiations or meet and confer discussions with employee organizations for the purpose of reaching agreement on conditions of employment?
3. Total number of contractual agreements between your agency and employee organizations in effect as of October 15 of the survey year.

From these survey responses, one can construct a unionization measure using the following criteria: at least 50 percent of teachers are union members, the form of labor negotiations is collective bargaining, and the district has at least one contract or memorandum of understanding with *any* employee organization in effect as of October of the survey year. Note that this union measure is appropriately designed to identify teacher contracts that are collectively bargained with a school district rather than a contract with other employee unions.

While the above measure is the most sensible alternative in the COG, it has several drawbacks. The first is it effectively measures whether a district has a collectively bargained contract with the teachers' union, not whether a teachers' union exists. Given the short lag between certification and negotiation of a first contract, however, this discrepancy is likely small.

The second, more serious, problem is classification error in the COG union measure. Although the COG-based union measure is designed to reduce potential measurement error by making the definition of unionization relatively strict, there are significant differences between the COG and election certification measures of union status that suggest measurement error exists in the former data. Table 3 contains a comparison of district-level unionization rates from the Census of Governments and the election certifications for each state in the sample. I constructed the Census of Governments measure by a straightforward implementation of the above definition of unionization. Note the COG is conducted every 5 years and labor relations information was only included in the 1972, 1977, 1982 and 1987 surveys.

The table illustrates the substantial differences between the two union measures. In the table, each four-cell square sums to one, and each diagonal within a cell represents the observations for which the union measures agree. For example, in Iowa in 1977, the COG and election certification measures agree 49.89% of school districts were unionized and 26.61% were not. However, 9.31% of the school districts are classified as unionized by the COG measure but had not successfully completed a teachers' union election by that date. Conversely, 14.19% of districts had completed an election but were measured as not unionized by the constructed Census of Governments union measure.

I interpret the disagreement between the two data sources as measurement error, with true union status measured by the election certifications. Given there was little voluntary recognition occurring in these states in this period and the validation study made every attempt to find such districts, measurement error in the Census of Governments is a natural explanation for why there are districts that had not completed a unionization election yet were measured as unionized by the COG. Further, since most districts achieve a contract within a year of certification, the lag between certification and successfully negotiating a contract cannot explain why so many districts that had certified unions were not measured as unionized by the Census of Governments.

The accuracy of the COG unionization construct is also called into question by the differential time trends in union status within states across measures. Because there are no decertifications, unionization as measured by election certifications weakly increases over time. Thus, conditional on completing a successful election, a district will always be classified as unionized. In contrast, after 1977, unionization rates decline over time in the Census of Governments: while 788 districts were measured as unionized by the Census of Governments in 1977, this number fell to 771 districts in 1982 and to 742 districts in 1987. These declines are not consistent with the lack of any evidence of decertification or cessation of collective bargaining in this time period in these three states.

To investigate further the source of the discrepancy, I look at which of the three criteria used in the COG union measure "fail" when a district has completed a successful unionization election but is not classified as unionized in the COG. I find for such districts in all three states, the provision that the percentage of teachers who are union members must be greater than 50 fails at higher rates over time. It is likely this variable constitutes the main source of measurement error in the COG union measure. First, the Census of Governments is filled out by district administrators, who have no way of knowing how many teachers are union members. Second, even if the union membership rate were accurately measured, Iowa, Indiana, and Minnesota are agency shop states, meaning employees are covered by the contract and must pay union dues even when they are not union members. That union membership is reducing over time in these states thus creates measurement error in the COG-based union measure, but this decline in membership, even if accurate, has not translated into changes in collective bargaining status. The problems surrounding the measurement of teacher union membership in the COG are the main source of measurement error in the COG union measure.

Another potential source of measurement error is an increasing number of districts report having no negotiated contracts over time, despite the fact that, conditional on obtaining a first contract, it is rare the teachers are ever without a negotiated contract with the district.[42] Some of this discrepancy could be due to the fact that even if a contract expires, teachers typically continue to work under that contract until a new one is negotiated with the district. One explanation for the decrease in unionization rates apparent in the COG is expired contracts are coded as "no contract," despite the fact that negotiated work rules and wage schedules are in place in these districts. These results provide suggestive evidence that the measure of the existence of contracts in the COG contains measurement error.[43]

Taking the election certification data as the true measure of unionization status, Table 4 reports the misclassification rates by state and year in the Census of Governments. Aside from 1972, the average misclassification rate remains relatively constant at between 31 and 36 percent in the sample. However, the misclassification

[42]While there are no available credible aggregate statistics on this assertion, lawyers I have spoken to at both the AFT and NEA agree with this generalization.

[43]Given the errors in the Census of Governments labor relations data, one must be skeptical of the accuracy of the financial and employment information in these surveys as well. However, since the survey is filled out by the central administrative offices that have access to payroll records and budgets, it is reasonable to expect such data will be supplied with greater accuracy than the number of teachers belonging to the union.

rate is as high as 47.2% in Indiana in 1987. Saltzman (1986) provides some outside validation for these misclassification rates. He validates the 1977 Census of Governments union measure for 1000 districts in the U.S. and finds a misclassification rate of 30% for the U.S., which is similar to the 31% misclassification rate I report for my sample of 3 states in that year.

6.2 The Effect of Different Union Measures on Union Impact Estimates

The high misclassification rates from the union measure constructed from the Census of Governments suggest this measure does not accurately characterize the history and state of collective bargaining in the school districts in the sample. In order to understand more fully the differences between my results and those from the existing literature, it is instructive to undertake a comparison of union impact estimates using the two available district-level union measures. Specifically, I replicate estimates from Hoxby (1996) using both union measures because it is the most comprehensive and empirically sophisticated study of teachers' unions in the literature and because our studies use similar data and time periods.

The empirical specifications in Hoxby (1996) are of the form:

$$Y_{it} = \beta_0 + \beta_1 U_{it} + \delta X_{it} + \tau_i + \phi_t + \psi_i * t + \epsilon_{it}, \tag{5}$$

where Y_{it} is an outcome variable of interest, U_{it} is an indicator variable equal to 1 if district i is unionized at time t, X_{it} is a vector of demographic characteristics listed in the previous section, τ_i are district fixed effects, ϕ_t are year fixed effects, $\psi_i * t$ are district-specific linear time trends, and ϵ_{it} is a normally distributed error term.

Tables 5-7 present the results of the regressions when Y_{it} is, alternatively, log real teacher pay, log real current operating expenditures per student, and student-teacher ratios, as these are the dependent variables analyzed by Hoxby. Each regression contains three years of data from the 1970, 1980 and 1990 U.S. Census school district files. Column (i) in Tables 5-7 presents the results taken directly from Hoxby (1996).

Column (ii) contains estimates using Hoxby's methodology on my sample of three states and allows me to determine how much of the difference in our estimates is due to the fact I use only three states and she uses all districts in the U.S.. Column (iii) is identical to column (ii), except unionization is measured using the election certification data. This last column thus will yield insight into the effect of the measurement error on union impact estimates.[44]

Hoxby identifies union effects by using only within-state variation through the use of district fixed effects and by an IV strategy that uses only cross-state variation over time. The instruments she uses are passage of state-level public sector bargaining laws. As I only have data for three states, I am restricted to a within-state analysis. While this restriction is justified by my more accurate union data, I will only compare my results to the within-state estimates from Hoxby (1996). However, her IV and fixed effects estimates are both qualitatively and quantitatively similar.

Changing the unionization measure has a large impact on estimates of union effects in all three tables. In Table 5, the union impact estimates on log real teacher pay using the COG union measure are similar in magnitude, sign and statistical significance for the national sample and the Midwest sample. However, when I employ the election certification definition of unionization, the coefficient on the union variable becomes negative, smaller in magnitude, and not statistically significant at even the 10 percent level. Note the standard error on the union coefficient increases by a factor of 3 between Columns (ii) and (iii) in Table 5. This increase occurs because there is variation in the Census of Governments measure that is due to measurement error and is correlated with the dependent variable (see Appendix C). Eliminating this variation increases the standard error estimate substantially. While the union estimate in Column (iii) does not allow one to rule out the verity of the union estimate in Column (i), it illustrates the fragility of the estimate to correcting for measurement error.

[44]Typically, one would run "horse race" regressions to compare the two measures, but since the measurement error is correlated with the regression errors (see Appendix C), such a methodology is not appropriate.

A similar pattern emerges in Table 6, which presents results for current operating expenditures per student. Switching from the national to the midwest sample reduces the magnitude of the union coefficient, but the signs are the same across columns (i) and (ii). However, in column (iii), the union impact estimate becomes negative when I use the election certification measure and the standard error increases by a factor of 3.8. Table 7 is more problematic because there is a marked difference between the estimates in the first two columns; the union impact on student-teacher ratios in the 3 midwestern states is of a different sign than for the nation as a whole. However, the difference in union coefficients and the increase in the size of the standard error of these coefficients between columns (ii) and (iii) in Table 7 is consistent with the sensitivity of the results reported in Hoxby (1996) to measurement error.[45]

What is most interesting about the form of the measurement error bias is it is not attenuating, which is the form of bias one would expect from classical measurement error. Classical measurement error occurs when the error is uncorrelated with the dependent variable, the independent variables, the regression error, and the true value of the variable. Despite the fact the measurement error must be correlated with the true measure of union status as union status is a binary variable, Bound, Brown and Mathiowetz (2001) show as long as the misclassification is what Carroll, Ruppert and Stefanski (1995) term "non-differential," the bias in the coefficient will still be attenuating as long as the rest of the classical measurement error assumptions hold.[46]

Appendix C contains a detailed discussion of measurement error issues and an analysis of the properties of this measurement error, treating the election certification data as the true measure of union status for each school district. I find the

[45]Because the Census of Governments union construct measures whether a district has a contract with a teachers' union and the election certification data measure whether a teachers' union exists for the purpose of collective bargaining, one could argue the differences between the estimates in columns (ii) and (iii) of Tables 5-7 are due to the difference between having a union and having a negotiated contract. As previously discussed, my validation study suggests most districts achieve a contract within one year of certification, and no district fails to achieve a contract conditional on certifying a union. While this difference may cause some attenuation in the results, it cannot account for the sign change in coefficient estimates and is likely to be small.

[46]Non-differential classification error occurs when, conditional on the true classification, reporting errors are independent of the dependent variable.

measurement error in the Census of Governments is differential; the classification error is correlated with the dependent variable in all regressions. Thus, the misclassification bias is not guaranteed to attenuate the coefficient estimates. I also find the classification error is correlated with the demographic characteristics included in equation (5). Finally, I perform Bound, Brown, Duncan and Rodgers (1994) decompositions of the measurement error. The BBDR decompositions 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, current operating expenditures, and student-teacher ratios but work in opposite directions for high school dropout rates.

The central conclusion from Tables 5-7 is the classification error reported in Tables 3 and 4 in the COG union measure is not innocuous. My results using the Midwest sample are similar to those in Hoxby (1996) for two of the three comparisons, but switching the union measure illustrates those results are not robust to correcting for measurement error. These comparisons underscore the importance of accurately measuring union status in an analysis of teachers' union impacts.

7 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 and the Elementary and Secondary General Information System (ELSEGIS), I investigate the impact of teachers' unions on the level and allocation of 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 between

5 and 9 percent, a negligible decrease in student-teacher ratios, and has a short-run positive effect on current operating expenditures per student and total revenue per student, but this positive effect disappears after the first year following certification. Further, one cannot reject the null hypothesis that teachers' unions have no influence on the allocation of expenditures with my results.

I also estimate the impact of unions on high school dropout rates and on the education production function using 1970-1990 U.S. Census school district summary data. I cannot reject the null hypothesis teachers' unions have no effect on high school dropout rates: the point estimates are small in both specifications, and in neither case are the coefficients statistically differentiable from zero. I find evidence consistent with unions increasing teacher productivity in the form of higher returns to lower class sizes and higher teacher pay. A topic for further research will be to determine whether such effects exist for other student achievement measures, especially those that include more students from higher portions of the ability distribution.

My findings contrast markedly with those of the literature, most notably Hoxby (1996). I argue the basic reason for the differences between my analysis and Hoxby's is the accuracy of the union certification data I use relative to the union measure constructed from the Census of Governments Labor Relations survey she uses. These differences highlight the importance of correctly measuring unionization status in union impact studies.

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), and the production function estimates reported in Section 5 are consistent with such effects. Finally, unionization may increase non-wage benefits such as pensions or health care that are valued by teachers. Freeman (1981) finds private sector unions increase non-wage benefits more than they increase wages, and Freeman (1986) reports many previous studies on public sector unions in general have found similar effects. It is a topic for further study whether teachers' unions in particular have such an impact on these benefits.

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 are only available electronically starting in 1972. The survey contains expenditure, revenue 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 states such as Maryland and Massachusetts, there are a large number of dependent school districts. 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, current operating expenditures (COE) and total revenues for each district in the sample. All financial variables are inflated to real 2004 dollars. 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. Note in the COG/SOG data, administrators such as principals and guidance counselors are included in the definition of full-time instructional staff. 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). While it 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. I calculate the student-teacher ratio by dividing total enrollment by the number of full time equivalent teachers in each school district.

Appendix C: Classification Error in the Constructed Census of Governments Teachers' Union Measure

Table 3 in the text presents the non-parametric identification of the measurement error in the Census of Governments union measure. This appendix investigates some properties of this classification error and includes a decomposition that breaks the bias due to the error into the part directly due to measurement error and the part due to the correlation of the measurement error with the regression error.

C-1 Non-Differential Classification Error

Let U be union status as measured by the Census of Governments variables and let U^* be true union status as indicated by the election certification data. If μ is the measurement error, then

$$U = U^* + \mu. \tag{C-1}$$

If one can only observe U instead of U^*, then instead of estimating the true model given by:

$$Y = \alpha + \beta U^* + \delta X + \epsilon, \tag{C-2}$$

one must estimate

$$Y = \tilde{\alpha} + \tilde{\beta} U + \tilde{\delta} X + \tilde{\epsilon}, \tag{C-3}$$

where ϵ is the regression error, X is a vector of demographic characteristics assumed to be measured without error, and Y is the outcome variable of interest that contains no measurement error. The standard result under the classical measurement error assumption in which μ is uncorrelated with U^*, X, Y, or ϵ is $\tilde{\beta}$ will be less than β in absolute value. In other words, classical measurement error will cause an attenuation bias. Note this result holds regardless of the number of independent variables measured with error as long as the classical measurement error assumptions hold.

When the mismeasured variable is binary, such as union status, the measurement error (i.e., the classification error) cannot be classical. This result is due to the fact U^* and μ will have to be negatively correlated. For example, if $U^* = 1$, $\mu \in -1, 0$, but if $U^* = 0$, $\mu \in 0, 1$. Thus, the typical attenuation result does not necessarily hold.

Bound, Brown and Mathiowetz (2001) show as long as the misclassification is non-differential and none of the other classical measurement error assumptions are violated, the bias in the coefficient will still be attenuating. Non-differential classification error occurs when reporting errors are independent of the dependent variable. More formally, this can be written:

$$\mathrm{P(U{=}i|U^*{=}i,Y)} = \mathrm{P(U{=}i|U^*{=}i)}, \tag{C-4}$$

where i $\in$ 0,1. I use a linear probability model to test for non-differential classification error for log real teacher pay, log real current operating expenditures per student, student-teacher ratios, and high school dropout rates. Specifically, I run models of the form:

$$U = \alpha_0 + \alpha_1 Y + \eta, \tag{C-5}$$

where U is an indicator variable that equals 1 if the school district is measured as unionized in the Census of Governments, Y are the dependent variables used in the analysis in the main text, and η is an error term. I perform this test separately for the probability of correctly classifying a district as unionized conditional on being unionized and for the probability of correctly classifying a district as non-unionized conditional on not being unionized. The estimates of α_1 test for the existence of differential classification error. These estimates are presented in Table C-1.

Assuming the election certification data accurately represent true union status, the data strongly reject that the measurement error from the Census of Governments is non-differential. In each row of Table C-1, the estimates of α_1 are statistically different from zero for at least one of the misclassification types. The implication of Table C-1 is the misclassification of union status in the Census of Governments is correlated with the dependent variables of interest; the classification error is differential. The bias due to the error in variables is therefore not guaranteed to be attenuating. This result is consistent with the positive biases in absolute value reported in Tables 5–7 of union effects when the imperfectly measured union measure is utilized.

C-2 Misclassification as a Function of X

Thus far, I have established the intuition about the effect of measurement error on parameter estimates when the error in variables is classical does not hold because μ is correlated with Y (as the error is differential) and with U^* (as the variable is binary). It is also instructive to determine whether the assumption holds that the measurement error is uncorrelated with the observable Xs. To test the relationship between misclassification and the Xs, I estimate the probability a district is reported as unionized in the COG when it had successfully completed a union election and the probability a district is reported as non-union in the COG when no union election certification was on file, conditional on observables. More formally, I estimate the following models using a linear probability model:

$$\mathrm{P(U{=}1|U^*{=}1,X)} \tag{C-6}$$

$$\mathrm{P(U{=}0|U^*{=}0,X)} \tag{C-7}$$

Table C-2 contains the results from these regressions from the pooled 1970, 1980 and 1990 U.S. Census and Census of Governments data described in the main text. Each cell in the table represents a separate regression. As Table C-2 illustrates, the probability of misclassifying a district's union status is correlated with the observable demographic characteristics of the district. Some general trends do emerge from Table C-2: smaller, less urbanized districts with lower public school enrollment are less likely to be correctly classified as unionized, while those districts with lower average income, lower average rent, and a smaller proportion of BA recipients are more likely to be misclassified as unionized. School districts with a higher percentage of residents with 12 or more years of schooling are less likely to be classified as unionized regardless of true union status, and conversely, districts with a higher percentage of private enrollment have a higher probability of being classified as unionized regardless of true union status. Lastly, those districts with higher poverty and unemployment rates have a higher probability of being misclassified conditional on their true union status. The assumption necessary for classical measurement error that the error is independent of the correctly measured observables clearly does not hold in the data.

C-3 BBDR Decompositions

Since the misclassification error is correlated with both the dependent variables and the independent variables in the union impact regressions, it is interesting to determine the extent to which each of these correlations cause the observed differences in the estimated union effects. Bound, Brown, Duncan and Rodgers (1994) propose a decomposition of the difference between the biased coefficient and the unbiased coefficient into the difference directly due to measurement error and the difference due to the correlation of the measurement error with the regression error.[47] More formally, let

[47] See Black, Sanders and Taylor (2003) for an implementation of the BBDR decomposition similar to the one presented here.

$$Z = [U|X]' \tag{C-8}$$

be a matrix of all the data. Then

$$\begin{aligned}
\tilde{\beta} &= (Z'Z)^{-1}Z'Y && \text{(C-9)}\\
&= (Z'Z)^{-1}Z'[Z^{*}\beta + \epsilon]\\
&= (Z'Z)^{-1}Z'[(Z-\mu)\beta + \epsilon]\\
&= (Z'Z)^{-1}Z'Z\beta + (Z'Z)^{-1}Z'[-\mu\beta + \epsilon]\\
&= \beta - (Z'Z)^{-1}Z'\mu\beta + (Z'Z)^{-1}Z'\epsilon\\
&\Longleftrightarrow\\
\tilde{\beta} - \beta &= -(Z'Z)^{-1}Z'\mu\beta + (Z'Z)^{-1}Z'\epsilon\\
&= -(\mathrm{E}[\mu \mid \mathrm{U{=}1, X}] - \mathrm{E}[\mu \mid \mathrm{U{=}0, X}])\beta + (\mathrm{E}[\epsilon \mid \mathrm{U{=}1, X}] && \text{(C-10)}\\
&\quad - \mathrm{E}[\epsilon \mid \mathrm{U{=}0, X}]),
\end{aligned}$$

where the last line follows from the fact only union status is assumed to be measured with error in the data. The first term on the right-hand side of Equation C-10 gives the part of the total difference that is due to measurement error, while the second term shows the part of the total difference that is due to the correlation between the measurement error and the regression error. I perform this decomposition separately for each of the four dependent variables used above in a model that includes district fixed effects, year fixed effects and district-specific linear time trends. The coefficient estimates are thus identical to those reported in Tables 5–7 in the main text.

Table C-3 presents the results of the BBDR decompositions. As is evident from the table, both forms of bias are present. These biases reinforce each other for log real teacher pay, log real current operating expenditures per student, and student-teacher ratios in this sample. The bias due to measurement error implies the direct effect of wrongly classifying a district as unionized is to increase the estimated union effect on teacher pay, expenditures per student, and student-teacher ratios. This result occurs because non-unionized districts have higher pay, expenditures, and class sizes than unionized districts, so mis-classifying non-unionized districts as unionized will bias upward the estimated impact of teachers' unions on all three measures. That the classification error is positively correlated with the regression error for the three inputs is due to the fact school districts incorrectly classified as unionized tend to have higher levels of teacher pay, expenditures, and student-teacher ratios than school districts for which union classification is correct. Thus, the misclassification of union status will serve to bias further upward the union impact estimates on these variables.

For the high school dropout rate decompositions, the biases offset each other somewhat, but the relatively large negative effect from measurement error dominates the positive correlation between the measurement error and the regression error. Non-union schools tend to have lower dropout rates than union schools, which is partially offset by the fact districts wrongly classified as unionized have higher dropout rates.

Table 1: Comparison of School District-Level 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	0.55	-0.40	0.60	0.35	0.25
	(1.00)	(3.12)	(0.27)	(3.40)	(1.49)	(0.25)
Percent Hispanic	0.10	0.16	-0.05	0.17	0.12	0.04
	(0.32)	(0.78)	(0.07)	(0.85)	(0.39)	(0.06)
Percent Some High School	14.83	16.15	-1.32**	16.22	15.89	0.33
	(3.57)	(4.98)	(0.44)	(5.01)	(4.86)	(0.40)
Percent High School Graduate	56.34	54.45	1.89**	54.27	55.19	-0.91*
	(6.07)	(6.24)	(0.57)	(5.01)	(4.88)	(0.50)
Percent Some College	16.95	16.52	0.44	16.45	16.78	-0.33
	(3.87)	(4.16)	(0.38)	(4.22)	(3.90)	(0.33)
Percent BA	11.88	12.88	-1.00*	13.06	12.14	0.91**
	(4.88)	(5.66)	(0.51)	(5.93)	(4.26)	(0.45)
Percent Urban	4.44	9.70	-5.27**	10.73	5.40	5.33**
	(19.10)	(27.53)	(2.43)	(22.82)	(20.87)	(2.20)
Percent Private Enrollment	5.86	7.66	-1.76**	7.66	7.49	0.17
	(6.74)	(7.83)	(0.70)	(7.39)	(9.49)	(0.63)
Log Average Income	9.59	9.59	0.01	9.59	9.57	0.02
	(0.17)	(0.17)	(0.02)	(0.18)	(0.17)	(0.01)
Log Median Rent	6.08	6.12	-0.04**	6.13	6.08	0.06**
	(0.19)	(0.19)	(0.02)	(0.19)	(0.18)	(0.02)
Percent Unemployed	2.63	2.86	-0.23*	2.90	2.66	0.24**
	(1.36)	(1.39)	(0.13)	(1.38)	(1.40)	(0.11)
Percent Below Poverty	5.28	4.81	0.48*	4.70	5.25	-0.54**
	(3.26)	(2.86)	(0.27)	(2.81)	(3.03)	(0.23)
Public School Enrollment/100	10.73	22.31	-11.58**	23.99	15.28	8.72**
	(32.63)	(42.95)	(3.81)	(45.07)	(31.74)	(3.42)
N	137	1006	.	812	194	.

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 2: Education Production Function Estimates

	Dependent Variable: High School Dropout Rate in Percent			
	1970 and 1980		1970, 1980, and 1990	
Independent Variable	(i)	(ii)	(iii)	(iv)
Union	-0.140	48.613**	0.202	26.262
	(0.576)	(20.924)	(0.656)	(18.593)
$\frac{\text{Student}}{\text{Teacher}}$ Ratio	0.037**	0.015	0.049**	0.029
	(0.016)	(0.015)	(0.022)	(0.021)
$\frac{\text{Student}}{\text{Teacher}}$*Union	.	0.438**	.	0.227**
	.	(0.116)	.	(0.092)
Log Real Full Time Teacher Pay	1.604	4.026**	-2.752**	-1.125
	(1.567)	(1.889)	(1.392)	(1.711)
(Log Real Full Time Teacher Pay)*Union	.	-6.793**	.	-3.612
	.	(2.584)	.	(2.317)
Log Population	27.790**	27.913**	31.475**	31.009**
	(2.942)	(2.857)	(2.411)	(2.386)
Percent Urban	0.641	0.486	0.511	0.465
	(0.487)	(0.487)	(0.590)	(0.588)
Log Average Income	-5.789*	-5.259*	-21.965**	-21.005**
	(2.978)	(3.019)	(3.179)	(3.238)
Log Median Gross Rent	0.193	0.025	0.277	0.219
	(1.130)	(1.271)	(1.516)	(1.498)
Percent Below Poverty	-0.082	-0.048	-0.303**	-0.282**
	(0.081)	(0.088)	(0.120)	(0.123)
Percent Unemployed	0.365**	0.371**	0.022	0.041
	(0.162)	(0.160)	(0.085)	(0.084)
Percent Black	-0.256**	-0.204**	0.204	0.208
	(0.090)	(0.089)	(0.222)	(0.216)
Percent Hispanic	0.094	0.239	0.069	0.072
	(0.393)	(0.377)	(0.179)	(0.167)
Percent 12–15 Years School	0.209**	0.180**	0.168**	0.154**
	(0.052)	(0.052)	(0.061)	(0.062)
Percent 16+ Years School	0.033	0.019	0.450**	0.441**
	(0.074)	(0.074)	(0.054)	(0.054)
Percent Private Enrollment	-0.258**	-0.254**	-0.369**	-0.360**
	(0.066)	(0.063)	(0.057)	(0.058)
Log Public School Enrollment	-24.915**	-26.239**	-29.904**	-29.919**
	(2.530)	(3.545)	(1.993)	(1.987)
Constant	-62.921**	-74.439**	13.733	4.550
	(21.159)	(21.267)	(20.499)	(22.077)

[1] Source: Author's calculation as described in the text from the 1970, 1980, and 1990 U.S. Census School District Files. Columns (i) and (ii) use only observations from 1970 and 1980, while columns (iii) and (iv) include observations from all three years.

[2] All models include year and school district fixed effects. Robust standard errors are in parentheses: ** indicates significance at the 5 percent level and * indicates significance at the 10 percent level.

Table 3: A Comparison of Union Status from the Census of Governments and the Union Election Certifications by State and Year

	Census of Governments Union Measure					
	1972					
Election Certification	**Iowa**		**Indiana**		**Minnesota**	
Union Measure	Union	Non-Union	Union	Non-Union	Union	Non-Union
Union	0.00%	0.67%	3.63%	1.98%	53.58%	17.78%
Non-Union	5.99%	93.35%	10.23%	84.16%	19.40%	9.24%
	1977					
Election Certification	**Iowa**		**Indiana**		**Minnesota**	
Union Measure	Union	Non-Union	Union	Non-Union	Union	Non-Union
Union	49.89%	14.19%	57.43%	22.77%	55.89%	20.79%
Non-Union	9.31%	26.61%	11.55%	8.25%	16.17%	7.16%
	1982					
Election Certification	**Iowa**		**Indiana**		**Minnesota**	
Union Measure	Union	Non-Union	Union	Non-Union	Union	Non-Union
Union	51.22%	17.96%	55.12%	26.73%	58.20%	22.17%
Non-Union	8.43%	22.39%	8.58%	9.57%	13.16%	6.47%
	1987					
Election Certification	**Iowa**		**Indiana**		**Minnesota**	
Union Measure	Union	Non-Union	Union	Non-Union	Union	Non-Union
Union	46.78%	25.28%	42.24%	40.59%	64.67%	16.86%
Non-Union	9.76%	18.18%	6.60%	10.56%	13.63%	4.85%
Number of Districts	435		297		431	

Source: Author's calculations from the 1972, 1977, 1982, and 1987 Census of Governments and the teachers' union election certification data described in the text. The number of districts represent the total number of districts in the sample in 1987.

Table 4: Misclassification Rates in the Census of Governments by State and Year, Treating the Election Certifications as the True Measure of Unionization Status

Year	Iowa	Indiana	Minnesota	Average
1972	6.65%	12.21%	37.18%	19.21%
1977	23.50%	34.32%	36.95%	31.17%
1982	26.39%	35.33%	35.33%	31.93%
1987	35.03%	47.19%	30.48%	36.48%

[1] Source: Author's calculations from the 1972, 1977, 1982, and 1987 Census of Governments and the teachers' union election certification data described in the text.
[2] The misclassification rate is the sum of the total number of times the Census of Governments and the election certification union measures disagree for each state and year. Each state-level misclassification rate is calculated by taking the sum of the off-diagonal entries from the appropriate four-cell square in Table 3. The average misclassification rate is a weighted average of the state-level misclassification rates, where the weight is the number of school districts in each state.

Table 5: Comparison of the Effect of Different Union Measures and Estimation Samples on Estimates of the Union Impact on Teacher Pay

	Dependent Variable: Ln(Real Monthly Average Teacher Pay)		
	COG/Hoxby (1996)		Election Certification
Independent Variable	(i) U.S. Estimation Sample	(ii) IA,IN,MN Estimation Sample	(iii) IA,IN,MN Estimation Sample
Union	0.051** (0.008)	0.054** (0.024)	-0.019 (0.072)
Log Population	-0.015** (0.004)	0.029 (0.063)	0.029 (0.057)
Percent Urban	0.0005** (0.0002)	0.0004 (0.0007)	0.0003 (0.0007)
Log Average Income	0.199** (0.022)	-0.066 (0.186)	-0.049 (0.187)
Log Median Gross Rent	-0.021** (0.010)	0.064 (0.103)	0.078 (0.107)
Percent Below Poverty	-0.0001 (0.0006)	-0.009 (0.006)	-0.009 (0.007)
Percent Unemployed	-0.003** (0.001)	-0.009* (0.005)	-0.009* (0.005)
Percent Black	-0.004** (0.001)	0.0001 (0.010)	0.002 (0.009)
Percent Hispanic	-0.004** (0.001)	0.002 (0.010)	0.001 (0.011)
Percent 12–15 Years School	-0.002** (0.0003)	-0.002 (0.004)	-0.004 (0.004)
Percent 16+ Years School	0.004** (0.0004)	-0.005 (0.005)	-0.007 (0.005)
Percent Private Enrollment	0.001** (0.0002)	0.003 (0.004)	0.004 (0.004)
Log Public School Enrollment	0.041** (0.002)	-0.050 (0.057)	-0.053 (0.057)
R^2	NR	0.9366	0.9337

[1] Source: Estimates in column (i) come from Hoxby (1996) Table (IV) Column 6. Column (ii) contains estimates using the COG-based union measure on the IA, IN, and MN sample. Column (iii) presents estimates using the election certification union data on the IA, IN and MN sample.

[2] Hoxby (1996) utilizes median household income, whereas I utilize mean household income because median household income is not included in the 1970 Census school district summary files.

[3] All regressions include district and year fixed effects as well as district-specific linear time trends. Standard errors are clustered at the district level: ** indicates significance at the 5 percent level and * indicates significance at the 10 percent level.

Table 6: Comparison of the Effect of Different Union Measures and Estimation Samples on Estimates of the Union Impact on Current Operating Expenditures

	Dependent Variable: Ln(Real Current Operating Expenditure Per Student)		
	COG/Hoxby (1996)		Election Certification
Independent Variable	(i) U.S. Estimation Sample	(ii) IA,IN,MN Estimation Sample	(iii) IA,IN,MN Estimation Sample
Union	0.029** (0.007)	0.017 (0.017)	-0.010 (0.064)
Log Population	0.029** (0.004)	0.013 (0.068)	0.013 (0.068)
Percent Urban	-0.001** (0.0001)	-0.0004 (0.0007)	-0.0004 (0.0007)
Log Average Income	0.116** (0.019)	0.146 (0.149)	0.151 (0.150)
Log Median Gross Rent	0.232** (0.008)	-0.032 (0.105)	-0.027 (0.106)
Percent Below Poverty	-0.007 (0.001)	-0.009* (0.005)	-0.009* (0.005)
Percent Unemployed	-0.005** (0.001)	-0.006 (0.004)	-0.006 (0.005)
Percent Black	0.005** (0.001)	-0.005 (0.006)	-0.004 (0.005)
Percent Hispanic	0.003** (0.001)	-0.005 (0.008)	-0.005* (0.009)
Percent 12–15 Years School	0.005** (0.001)	-0.0001 (0.003)	-0.001 (0.003)
Percent 16+ Years School	0.004** (0.001)	-0.007 (0.005)	-0.008* (0.005)
Percent Private Enrollment	0.003** (0.001)	-0.001 (0.003)	-0.001 (0.003)
Log Public School Enrollment	-0.409** (0.011)	-0.024 (0.031)	-0.025 (0.030)
R^2	NR	0.9997	0.9997

[1] Source: Estimates in column (i) come from Hoxby (1996) Table (III) Column 6. Column (ii) contains estimates using the COG-based union measure on the IA, IN, and MN sample. Column (iii) presents estimates using the election certification union data on the IA, IN and MN sample.

[2] Hoxby (1996) utilizes median household income, whereas I utilize mean household income because median household income is not included in the 1970 Census school district summary files.

[3] All regressions include district and year fixed effects as well as district-specific linear time trends. Standard errors are clustered at the district level: ** indicates significance at the 5 percent level and * indicates significance at the 10 percent level.

Table 7: Comparison of the Effect of Different Union Measures and Estimation Samples on Estimates of the Union Impact on Student-Teacher Ratios

	Dependent Variable: Student-Teacher Ratio		
	COG/Hoxby (1996)		Election Certification
	(i)	(ii)	(iii)
Independent Variable	U.S. Estimation Sample	IA,IN,MN Estimation Sample	IA,IN,MN Estimation Sample
Union	-1.112**	0.117	-0.189
	(0.338)	(0.547)	(0.836)
Log Population	-0.841**	1.154	1.158
	(0.071)	(2.618)	(2.604)
Percent Urban	0.029**	0.020	0.020
	(0.003)	(0.015)	(0.014)
Log Average Income	-1.170**	-4.698	-4.660
	(0.367)	(4.100)	(4.150)
Log Median Gross Rent	-1.167**	0.024	0.056
	(0.161)	(2.519)	(2.483)
Percent Below Poverty	0.149	-0.038	-0.039
	(0.012)	(0.136)	(0.137)
Percent Unemployed	0.123**	-0.101	-0.102
	(0.015)	(0.103)	(0.103)
Percent Black	-0.143**	0.285	0.286
	(0.012)	(0.478)	(0.455)
Percent Hispanic	-0.065**	-0.162	-0.161
	(0.014)	(0.233)	(0.225)
Percent 12–15 Years School	-0.129**	0.071	0.067
	(0.011)	(0.098)	(0.105)
Percent 16+ Years School	-0.082**	0.165	0.162
	(0.015)	(0.154)	(0.161)
Percent Private Enrollment	-0.098**	-0.250	-0.249
	(0.009)	(0.344)	(0.345)
Log Public School Enrollment	7.334**	-2.990	-2.997
	(0.217)	(4.769)	(4.745)
R^2	NR	0.9612	0.9612

[1] Source: Estimates in column (i) come from Hoxby (1996) Table (V) Column 6. Column (ii) contains estimates using the COG-based union measure on the IA, IN, and MN sample. Column (iii) presents estimates using the election certification union data on the IA, IN and MN sample.

[2] Hoxby (1996) utilizes median household income, whereas I utilize mean household income because median household income is not included in the 1970 Census school district summary files.

[3] All regressions include district and year fixed effects as well as district-specific linear time trends. Standard errors are clustered at the district level: ** indicates significance at the 5 percent level and * indicates significance at the 10 percent level.

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

	Dependent Variable: Log of				
Relative Time	Real Monthly Full Time Teacher Pay	Full Time Teacher Employment	Student-Teacher Ratio	Real COE Per Student	Real Revenue Per Student
-5 Years	0.027*	0.009	0.005	-0.028	-0.016
	(0.016)	(0.036)	(0.039)	(0.026)	(0.027)
-4 Years	0.018	0.004	-0.010	-0.010	-0.009
	(0.012)	(0.020)	(0.017)	(0.012)	(0.015)
-3 Years	0.008	0.006	-0.004	-0.031**	-0.037*
	(0.010)	(0.017)	(0.014)	(0.013)	(0.020)
-2 Years	0.006	-0.028	-0.010	-0.001	-0.017
	(0.010)	(0.021)	(0.017)	(0.013)	(0.017)
0 Years	0.020*	0.036*	-0.023	0.036*	0.048**
	(0.011)	(0.020)	(0.022)	(0.020)	(0.020)
1 Year	0.013	0.040**	-0.016	-0.006	0.012
	(0.011)	(0.019)	(0.018)	(0.020)	(0.020)
2 Years	0.002	0.046**	-0.009	-0.040	-0.045
	(0.012)	(0.021)	(0.018)	(0.029)	(0.030)
3 Years	0.019	0.054**	-0.005	-0.016	0.004
	(0.012)	(0.022)	(0.018)	(0.015)	(0.016)
4 Years	0.017	0.061**	-0.016	0.002	0.008
	(0.013)	(0.024)	(0.019)	(0.015)	(0.016)
5 Years	0.011	0.050**	-0.010	-0.005	-0.001
	(0.013)	(0.023)	(0.020)	(0.016)	(0.017)
6 Years	0.010	0.064**	-0.022	-0.021	-0.014
	(0.013)	(0.025)	(0.022)	(0.018)	(0.019)
7 Years	0.013	0.054*	-0.024	-0.017	-0.027
	(0.014)	(0.028)	(0.025)	(0.019)	(0.019)
8 Years	0.021	0.059**	-0.013	-0.010	-0.015
	(0.015)	(0.029)	(0.025)	(0.017)	(0.019)
9 Years	0.027*	0.067**	-0.018	-0.010	-0.024
	(0.016)	(0.030)	(0.023)	(0.017)	(0.021)
10 Years	0.024	0.029	-0.013	-0.004	-0.004
	(0.016)	(0.028)	(0.023)	(0.019)	(0.023)
Constant	8.095**	4.602**	2.463**	8.759**	8.833**
	(0.022)	(0.049)	(0.032)	(0.018)	(0.019)
N	7549	7549	6633	10822	10825
# Clusters	1112	1112	1104	1137	1137
R^2	0.753	0.979	0.630	0.923	0.756

[1] Source: Parameter estimates from estimation of equation (2) in the text.
[2] 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.
[3] A complete set of relative time dummy variables is collinear with the district fixed effects in equation (2) on the sample that excludes observations with relative time to union election less than -6. Relative year -1 is omitted to make the parameter estimates consistent with that model.

Table B-2: 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 and Greater Than -6

	Dependent Variable: Log of				
Relative Time	Real Monthly Full Time Teacher Pay	Full Time Teacher Employment	Student-Teacher Ratio	Real COE Per Student	Real Revenue Per Student
-5 Years	0.015	-0.015	0.010	-0.035	-0.015
	(0.023)	(0.051)	(0.056)	(0.034)	(0.035)
-4 Years	0.005	0.000	-0.009	-0.013	-0.015
	(0.018)	(0.028)	(0.026)	(0.017)	(0.021)
-3 Years	-0.001	0.002	-0.004	-0.033**	-0.040*
	(0.015)	(0.023)	(0.023)	(0.016)	(0.025)
-2 Years	-0.003	-0.033	-0.007	-0.005	-0.019
	(0.013)	(0.028)	(0.023)	(0.015)	(0.021)
0 Years	0.012	0.039*	-0.032	0.038*	0.048**
	(0.013)	(0.023)	(0.028)	(0.022)	(0.023)
1 Year	0.007	0.048**	-0.017	0.010	0.008
	(0.013)	(0.021)	(0.022)	(0.020)	(0.024)
2 Years	-0.003	0.059**	-0.010	-0.033	-0.035
	(0.013)	(0.023)	(0.022)	(0.020)	(0.029)
3 Years	0.017	0.063**	-0.003	-0.018	-0.004
	(0.014)	(0.025)	(0.021)	(0.014)	(0.018)
4 Years	0.020	0.068**	-0.016	-0.001	0.008
	(0.014)	(0.026)	(0.024)	(0.014)	(0.018)
5 Years	0.010	0.065**	-0.017	-0.004	-0.001
	(0.015)	(0.028)	(0.023)	(0.015)	(0.020)
6 Years	0.005	0.089**	-0.033	-0.018	-0.011
	(0.016)	(0.028)	(0.025)	(0.016)	(0.022)
7 Years	0.009	0.081**	-0.034	-0.018	-0.028
	(0.017)	(0.032)	(0.029)	(0.018)	(0.023)
8 Years	0.021	0.073**	-0.009	-0.018	-0.021
	(0.018)	(0.032)	(0.025)	(0.017)	(0.022)
9 Years	0.029	0.092**	-0.025	-0.014	-0.030
	(0.019)	(0.034)	(0.027)	(0.016)	(0.025)
10 Years	0.021	0.069	-0.037	-0.010	-0.015
	(0.020)	(0.034)	(0.028)	(0.018)	(0.027)
Constant	8.070**	4.633**	2.447**	8.758**	8.828**
	(0.028)	(0.059)	(0.043)	(0.021)	(0.024)
N	8515	8515	7500	12225	12229
# Clusters	1165	1165	1157	1165	1165
R^2	0.735	0.977	0.622	0.923	0.754

[1] Source: Parameter estimates from estimation of equation (2) in the text.
[2] 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.
[3] Relative year -1 is omitted in order to identify the model: a complete set of relative time dummy variables is collinear with the district fixed effects in equation (2).

Table B-3: Regression Results from Fixed Effects Estimates of Teachers' Union Impacts on Expenditure Allocation from the ELSEGIS Survey Using the Sample Comprised of Never-Unionized Districts and Observations With Relative Years to Union Election Less Than 8

	Dependent Variable: Log Proportion of Total Expenditures on:					
Relative Time	Administration	Instruction	Attendance & Health	Transportation	Plant O&M	Fixed Charges
-5 Years	-0.150**	0.014	0.059	-0.060	-0.013	-0.016
	(0.076)	(0.013)	(0.270)	(0.066)	(0.052)	(0.044)
-4 Years	-0.108*	0.005	0.215	-0.056	0.012	0.009
	(0.061)	(0.011)	(0.171)	(0.050)	(0.032)	(0.037)
-3 Years	-0.065	-0.0005	0.162	-0.037	0.019	-0.043
	(0.060)	(0.009)	(0.166)	(0.039)	(0.032)	(0.035)
-2 Years	-0.056	0.003	0.114	-0.008	-0.005	-0.035
	(0.044)	(0.007)	(0.133)	(0.028)	(0.024)	(0.024)
0 Years	-0.152*	0.019	0.099	0.013	0.016	-0.005
	(0.080)	(0.018)	(0.157)	(0.038)	(0.033)	(0.037)
1 Year	-0.054	-0.005	0.097	0.051	-0.002	-0.006
	(0.055)	(0.009)	(0.154)	(0.036)	(0.027)	(0.031)
2 Years	-0.074	0.004	-0.048	0.041	-0.068*	0.009
	(0.058)	(0.013)	(0.170)	(0.037)	(0.041)	(0.031)
3 Years	-0.089	-0.007	0.032	0.045	0.003	-0.010
	(0.068)	(0.015)	(0.179)	(0.039)	(0.047)	(0.033)
4 Years	-0.068	-0.009	-0.094	0.082**	-0.041	-0.042
	(0.068)	(0.020)	(0.179)	(0.041)	(0.042)	(0.031)
5 Years	-0.029	-0.015	-0.218	0.080*	-0.045	-0.018
	(0.078)	(0.019)	(0.201)	(0.045)	(0.039)	(0.040)
6 Years	-0.083	-0.021	-0.030	0.056	-0.079*	-0.022
	(0.098)	(0.028)	(0.242)	(0.054)	(0.044)	(0.046)
7 Years	-0.025	-0.016	-0.107	0.076	-0.047	-0.037
	(0.068)	(0.021)	(0.214)	(0.051)	(0.041)	(0.048)
Constant	1.404**	4.246**	-1.084**	1.806**	2.546**	1.209**
	(0.074)	(0.016)	(0.131)	(0.053)	(0.036)	(0.057)
N	4142	4185	3960	4135	4139	4155
# Clusters	1072	1074	1042	1073	1074	1075
R^2	0.756	0.655	0.846	0.919	0.715	0.904

[1] Source: Parameter estimates from estimation of equation (2) in the text.
[2] 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.
[3] A complete set of relative time dummy variables is collinear with the district fixed effects in equation (2) on the sample that excludes observations with relative time to union election less than -6. Relative year -1 is omitted to make the parameter estimates consistent with that model.

Table B-4: Regression Results from Fixed Effects Estimates of Teachers' Union Impacts on Expenditure Allocation from the ELSEGIS Survey Using the Sample Comprised of Never-Unionized Districts and Observations With Relative Years to Union Election Less Than 8 and Greater Than -6

Relative Time	Administration	Instruction	Attendance & Health	Transportation	Plant O&M	Fixed Charges
-5 Years	-0.066*	-0.007	0.021	0.029	0.016	-0.011
	(0.039)	(0.007)	(0.184)	(0.053)	(0.031)	(0.033)
-4 Years	-0.054	-0.011	0.254*	0.029	0.036	0.007
	(0.041)	(0.007)	(0.150)	(0.047)	(0.020)	(0.029)
-3 Years	-0.036	-0.011	0.235	0.009	0.033	-0.038
	(0.041)	(0.007)	(0.135)	(0.034)	(0.023)	(0.028)
-2 Years	-0.026	-0.005	0.167	0.039	0.008	-0.035
	(0.036)	(0.006)	(0.113)	(0.027)	(0.023)	(0.022)
0 Years	-0.112*	0.006	0.220	0.061*	0.023	-0.004
	(0.060)	(0.013)	(0.159)	(0.037)	(0.024)	(0.029)
1 Year	-0.007	-0.012*	0.219	0.088**	0.011	0.004
	(0.039)	(0.007)	(0.156)	(0.033)	(0.021)	(0.024)
2 Years	-0.034	-0.002	0.113	0.075**	-0.048	0.020
	(0.043)	(0.009)	(0.167)	(0.033)	(0.038)	(0.026)
3 Years	-0.036	-0.011	0.212	0.084**	0.019	0.008
	(0.049)	(0.010)	(0.181)	(0.038)	(0.033)	(0.027)
4 Years	-0.022	-0.011	0.097	0.111**	-0.023	-0.017
	(0.049)	(0.012)	(0.178)	(0.035)	(0.030)	(0.026)
5 Years	0.002	-0.017	-0.007	0.120**	-0.016	-0.006
	(0.059)	(0.012)	(0.195)	(0.038)	(0.029)	(0.034)
6 Years	-0.093	-0.019	0.178	0.096**	-0.034	0.015
	(0.092)	(0.017)	(0.217)	(0.047)	(0.032)	(0.035)
7 Years	-0.004	-0.018	0.154	0.128**	-0.015	-0.021
	(0.058)	(0.013)	(0.220)	(0.042)	(0.032)	(0.040)
Constant	1.391**	4.276**	-1.154**	1.777**	2.434**	1.360**
	(0.033)	(0.005)	(0.106)	(0.035)	(0.016)	(0.024)
N	4796	4844	4570	4788	4792	4808
# Clusters	1190	1192	1157	1191	1192	1193
R^2	0.756	0.669	0.836	0.920	0.714	0.905

[1] Source: Parameter estimates from estimation of equation (2) in the text.
[2] 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.
[3] Relative year -1 is omitted in order to identify the model: a complete set of relative time dummy variables is collinear with the district fixed effects in equation (2).

Table C-1: Tests of Non-Differentiation in COG Misclassification

Independent Variable	P(U=1|U*=1,Y)	P(U=0|U*=0,Y)
Log Real Teacher Pay	0.356**	-0.076
	(0.051)	(0.064)
Log Real Expenditures per Student	-0.002	0.024**
	(0.015)	(0.004)
Student-Teacher Ratio	0.017**	0.007**
	(0.004)	(0.002)
High School Dropout Rate	-0.0003	0.004**
	(0.001)	(0.001)

[1] Source: Author's calculations from the 1972, 1977, 1982, and 1987 Census of Governments and the teachers' union election certification data described in the text.

[2] Each cell above represents a separate pooled linear probability model regression. Standard errors are in parentheses: ** indicates significance at the 5 percent level.

Table C-2: Relationship Between Misclassification and the Observables

Independent Variable	P(U=1|U*=1,X)	P(U=0|U*=0,X)
Log Population	0.035**	0.009
	(0.010)	(0.013)
Percent Urban	0.067**	0.026
	(0.036)	(0.041)
Log Average Income	-0.007	-0.042**
	(0.022)	(0.007)
Log Median Rent	-0.041	-0.126**
	(0.036)	(0.024)
Percent Poverty	-0.755**	-0.570**
	(0.268)	(0.224)
Percent Unemployed	-0.002**	-0.008**
	(0.001)	(0.001)
Percent Black	-0.001	0.022*
	(0.003)	(0.012)
Percent Hispanic	-0.07	0.026
	(0.006)	(0.020)
Percent 12–15 Years School	-0.001	0.008**
	(0.001)	(0.002)
Percent 16+ Years School	0.001	-0.010**
	(0.001)	(0.002)
Percent Private Enrollment	0.004**	-0.003**
	(0.001)	(0.001)
Log Public School Enrollment	0.039**	0.010
	(0.010)	(0.013)

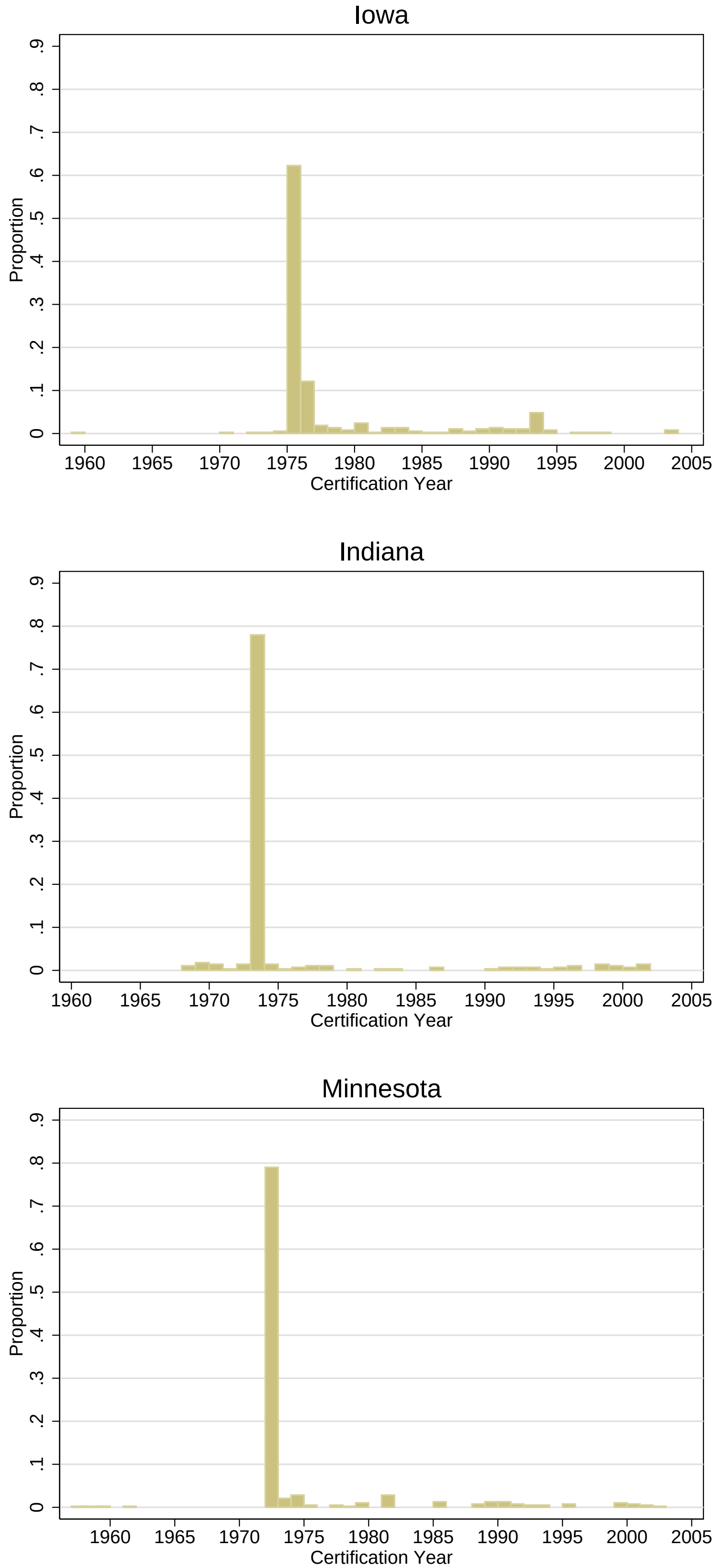
[1] Source: Author's calculations from the 1972, 1982, and 1987 Census of Governments, the 1970, 1980 and 1990 U.S. Census, and the teachers' union election certification data described in the text.
[2] Each cell above represents a separate pooled linear probability model regression. Standard errors are in parentheses: ** indicates significance at the 5 percent level and * indicates significance at the 10 percent level.

Table C-3: BBDR Decompositions

Dependent Variable	COG Estimate	Union Election Estimate	Total Difference	Difference From Measurement Error	Difference From Correlation of Measurement Error and Regression Error
Log Real Teacher Pay	0.054	-0.019	0.073	0.018	0.054
Log Real Expenditures per Student	0.017	-0.010	0.027	0.010	0.017
Student-Teacher Ratio	0.117	-0.189	0.306	0.183	0.124
High School Dropout Rate	0.589	1.385	-0.796	-1.332	0.536

[1] Source: Author's calculations from the 1972, 1982, and 1987 Census of Governments, the 1970, 1980 and 1990 U.S. Census, and the teachers' union election certification data described in the text.
[2] Each regression includes district and year fixed effects as well as district-specific linear time trends.

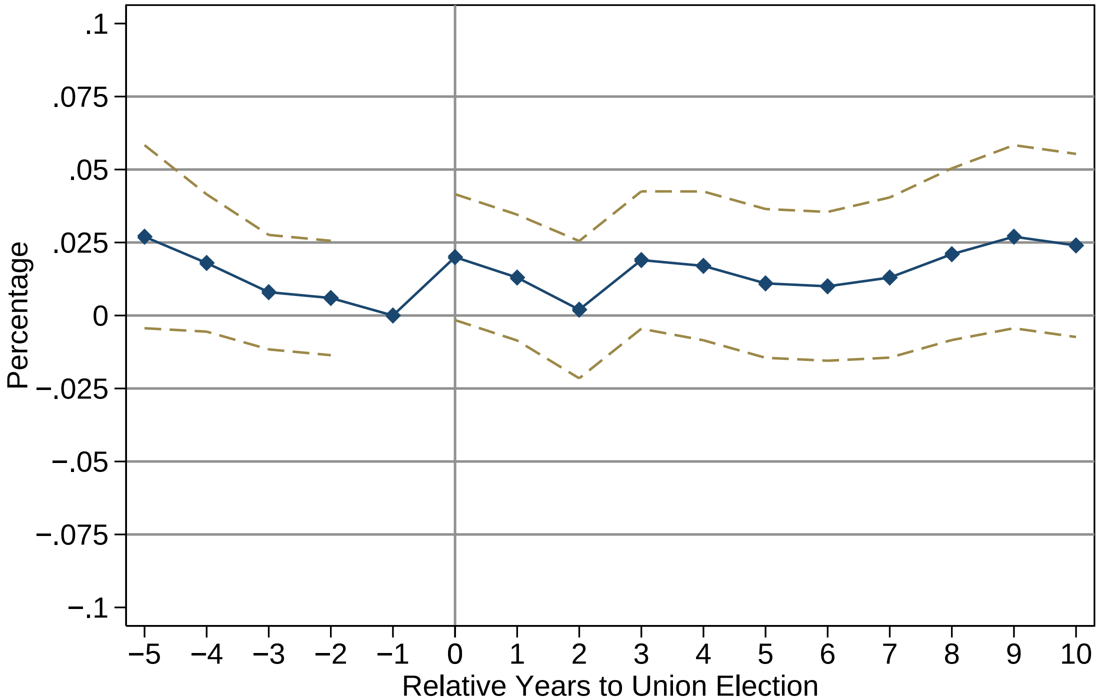
Figure 1: Distribution of Teachers' Union Election Certifications by State



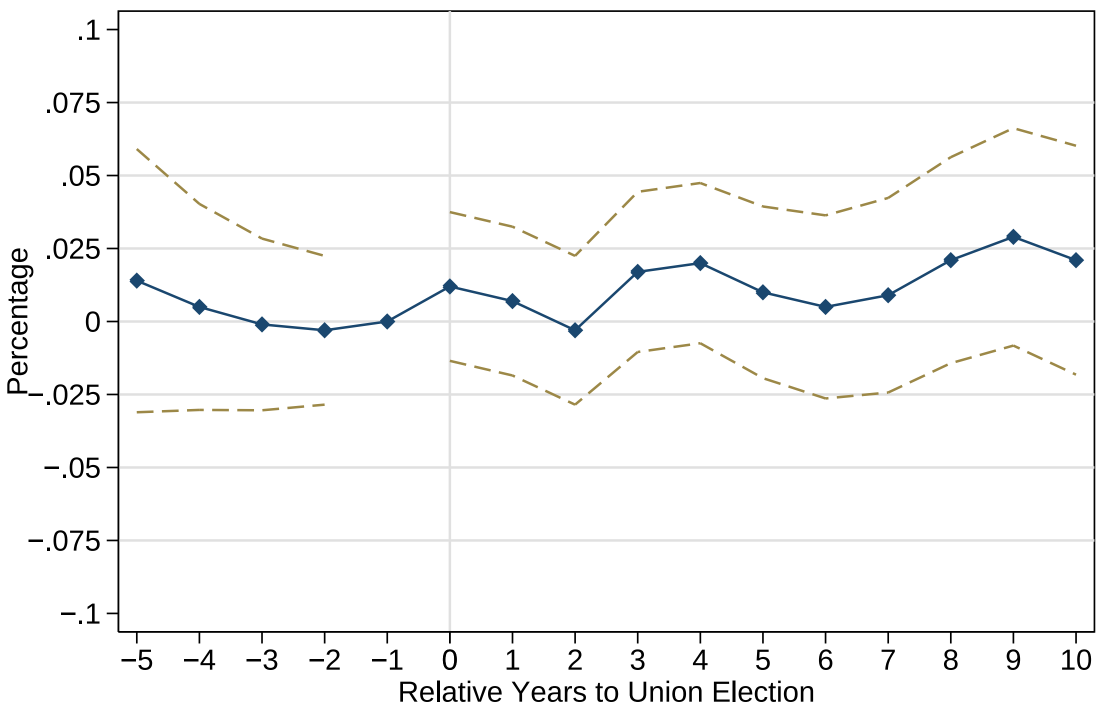
Source: Teachers' union election certification data described in the text.

Figure 2: The Effect of Teachers' Unions on Log Real Monthly Full Time Teacher Pay

Panel A: Sample Includes Never−unionized Districts and Observations With Relative Years to Union Election Less Than 11



Panel B: Sample Includes Never−unionized Districts and Observations With Relative Years to Union Election Less Than 11 and Greater Than −6

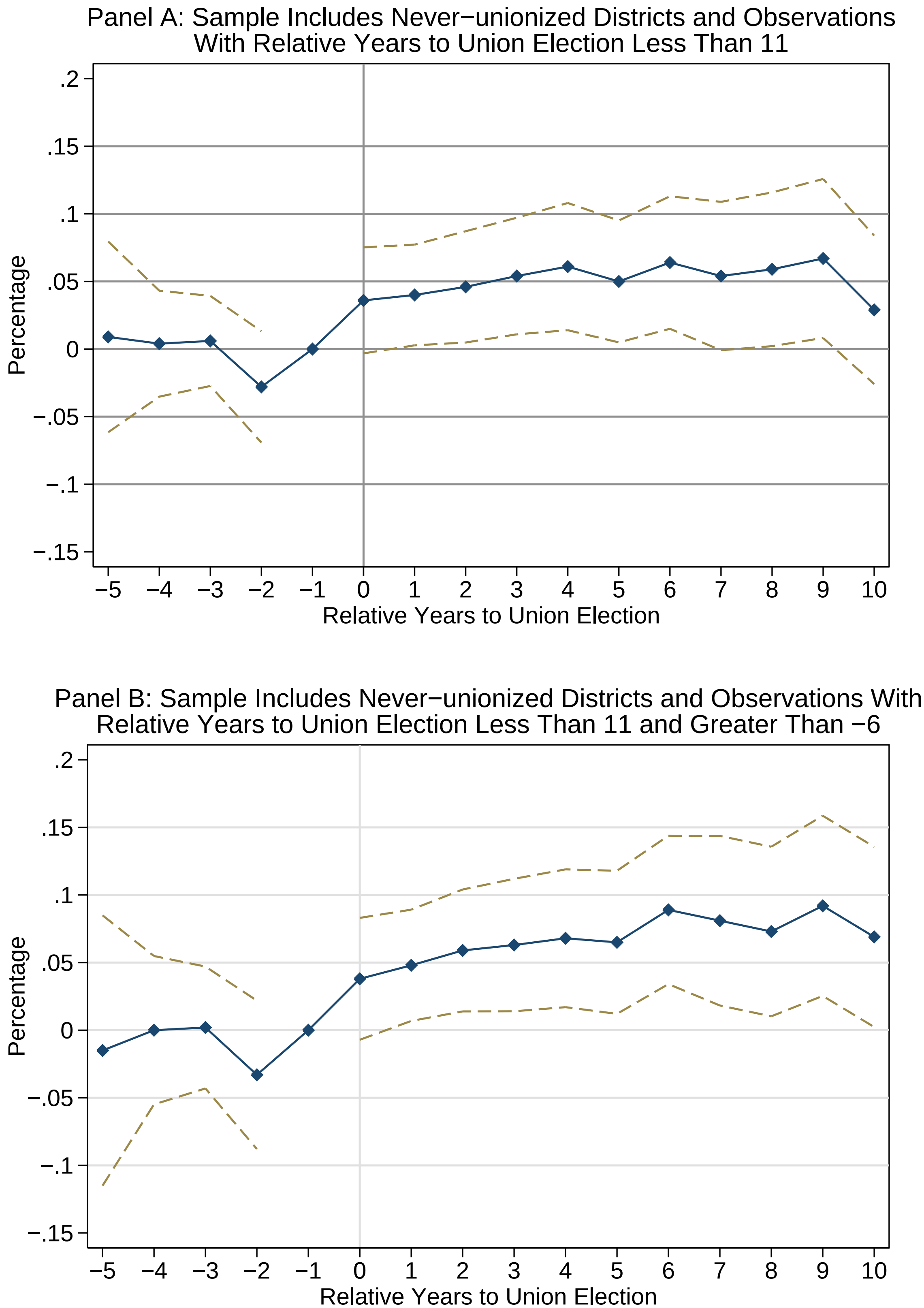


[1] Source: Author's calculations from the 1972–1991 Census/Survey of Governments as described in the text.

[2] Solid lines represent 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.

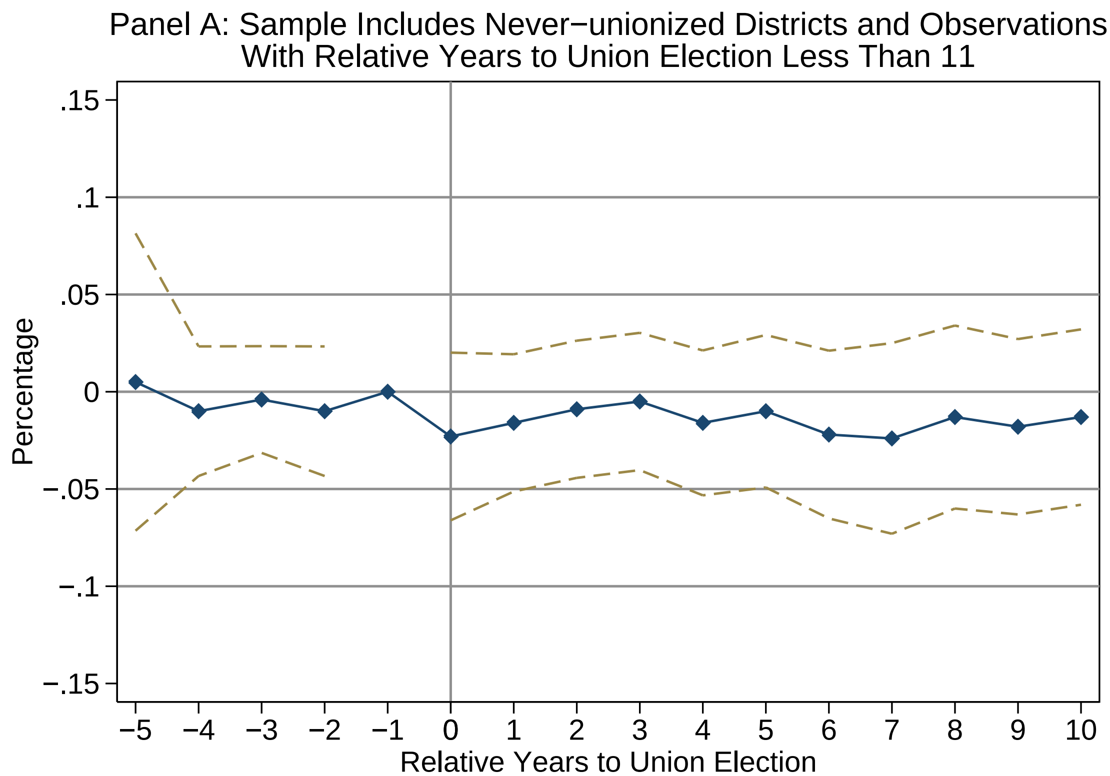
[3] Relative year -1 is omitted in order to identify the model: a complete set of relative time dummy variables is collinear with the district fixed effects in equation (2). I include a zero for the point estimates in relative year j=-1, but the gaps in the standard error bounds reflect that this zero is imposed rather than estimated.

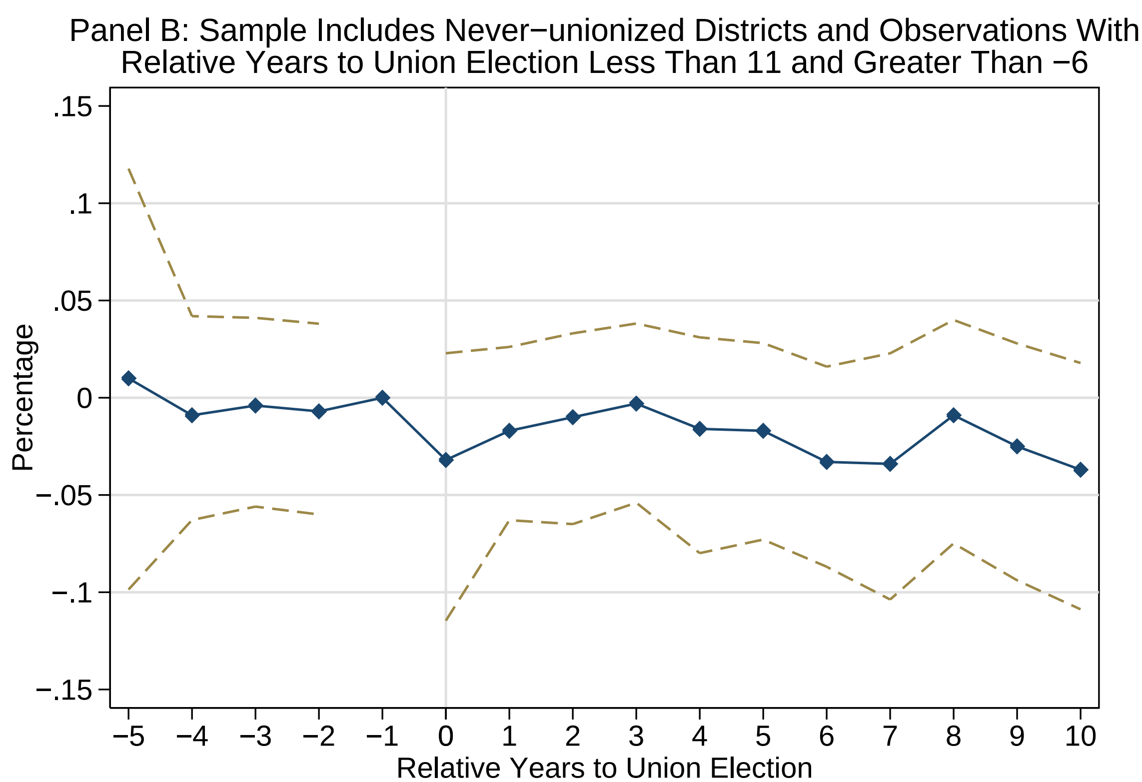
Figure 3: The Effect of Teachers' Unions on Log Full Time Teacher Employment



[1] Source: Author's calculations from the 1972–1991 Census/Survey of Governments as described in the text.
[2] Solid lines represent 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.
[3] Relative year -1 is omitted in order to identify the model: a complete set of relative time dummy variables is collinear with the district fixed effects in equation (2). 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.

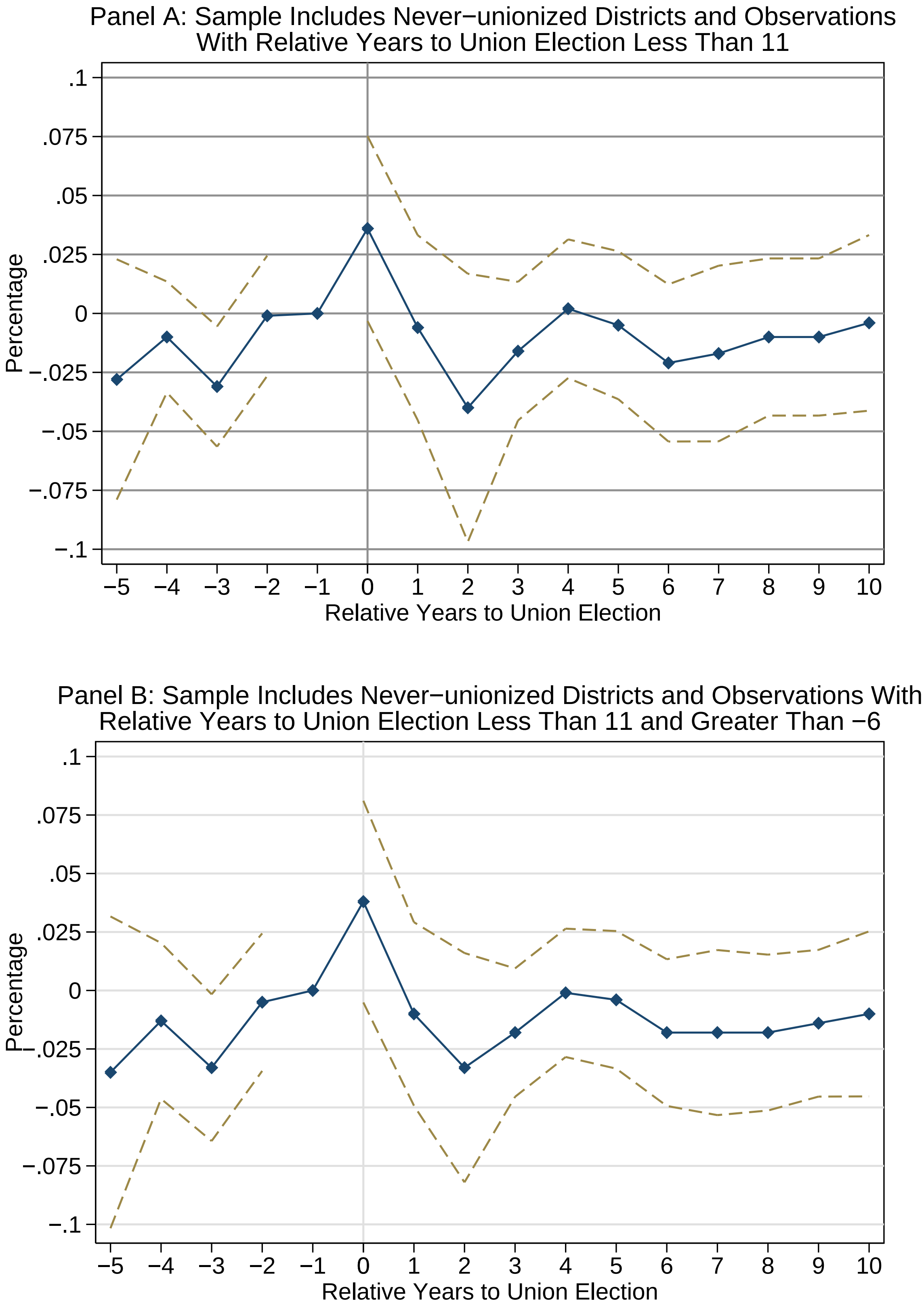
Figure 4: The Effect of Teachers' Unions on Log Student-Teacher Ratios





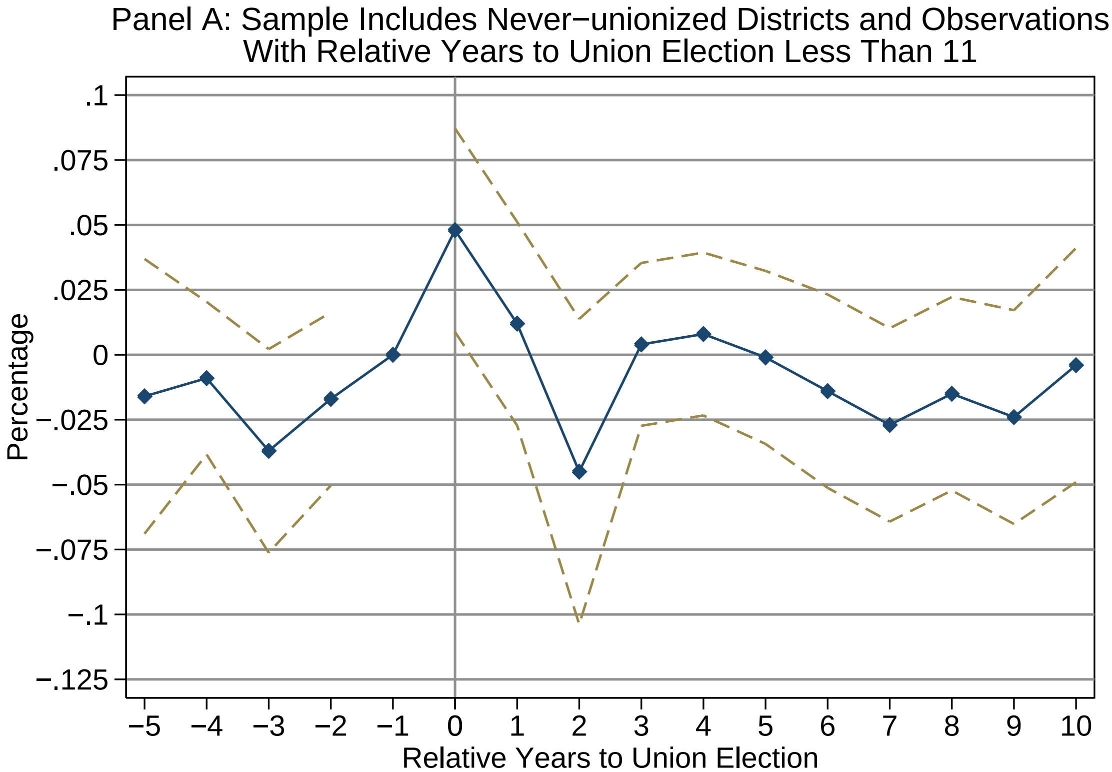
[1] Source: Author's calculations from the 1972–1991 Census/Survey of Governments as described in the text.
[2] Solid lines represent coefficient estimates from estimation of equation (2) in the text. Dotted lines are the bounds of the 95 percent confidence interval calculated from standard errors that are clustered at the district level.
[3] Relative year -1 is omitted in order to identify the model: a complete set of relative time dummy variables is collinear with the district fixed effects in equation (2). 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.

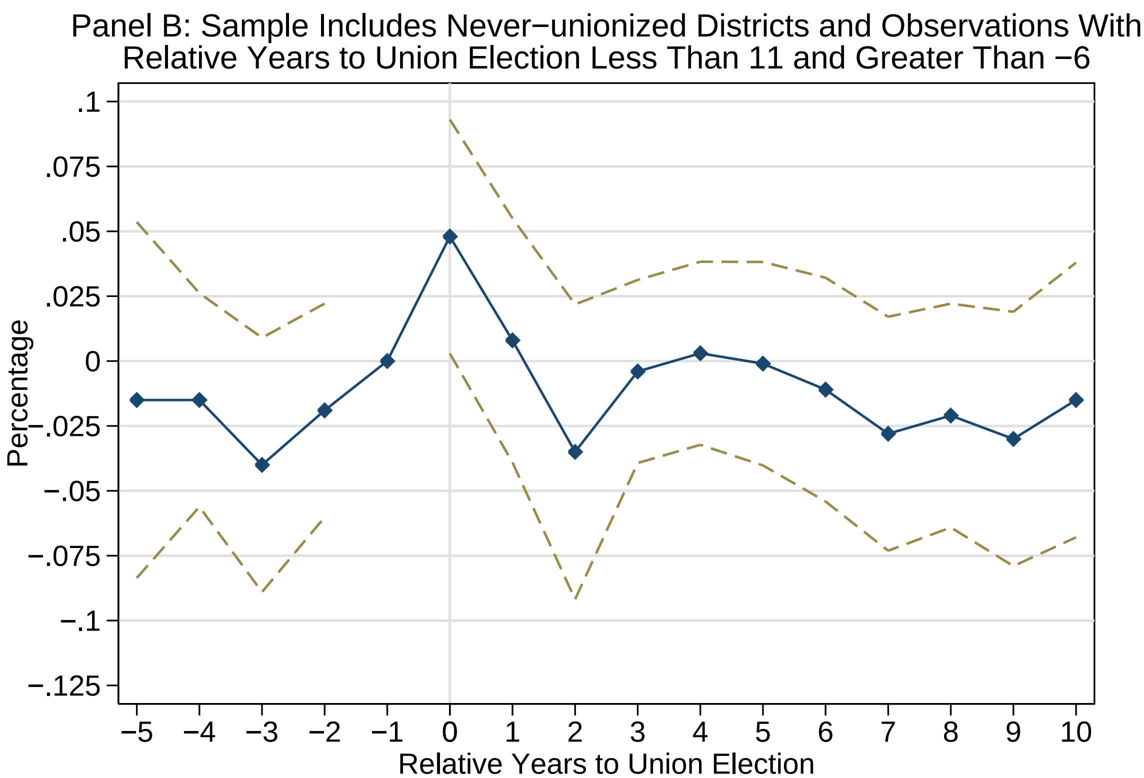
Figure 5: The Effect of Teachers' Unions on Log Real Current Operating Expenditures Per Student



[1] Source: Author's calculations from the 1972–1991 Census/Survey of Governments as described in the text.
[2] Solid lines represent 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.
[3] Relative year -1 is omitted in order to identify the model: a complete set of relative time dummy variables is collinear with the district fixed effects in equation (2). 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.

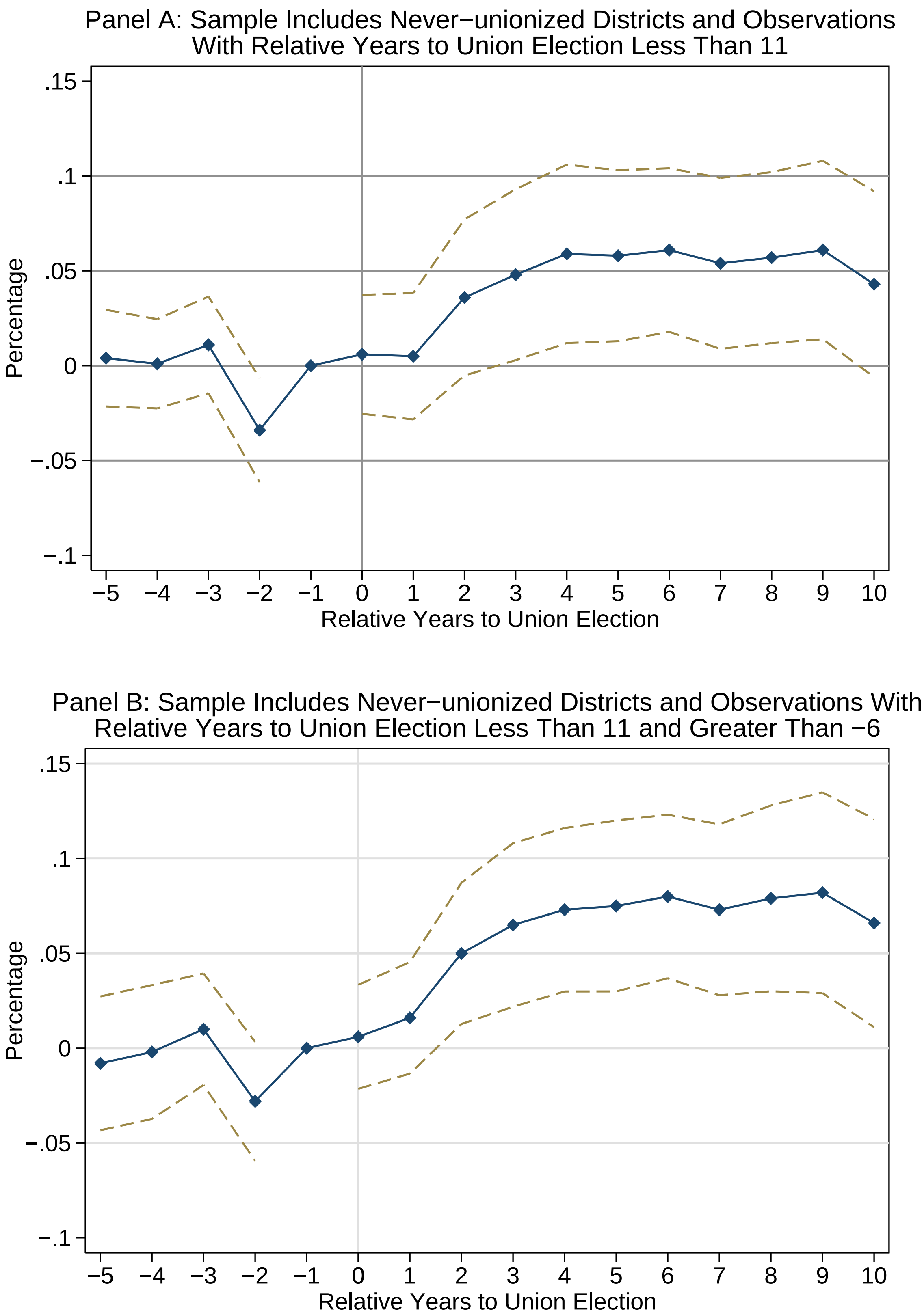
Figure 6: The Effect of Teachers' Unions on Log Real Total Revenues Per Student





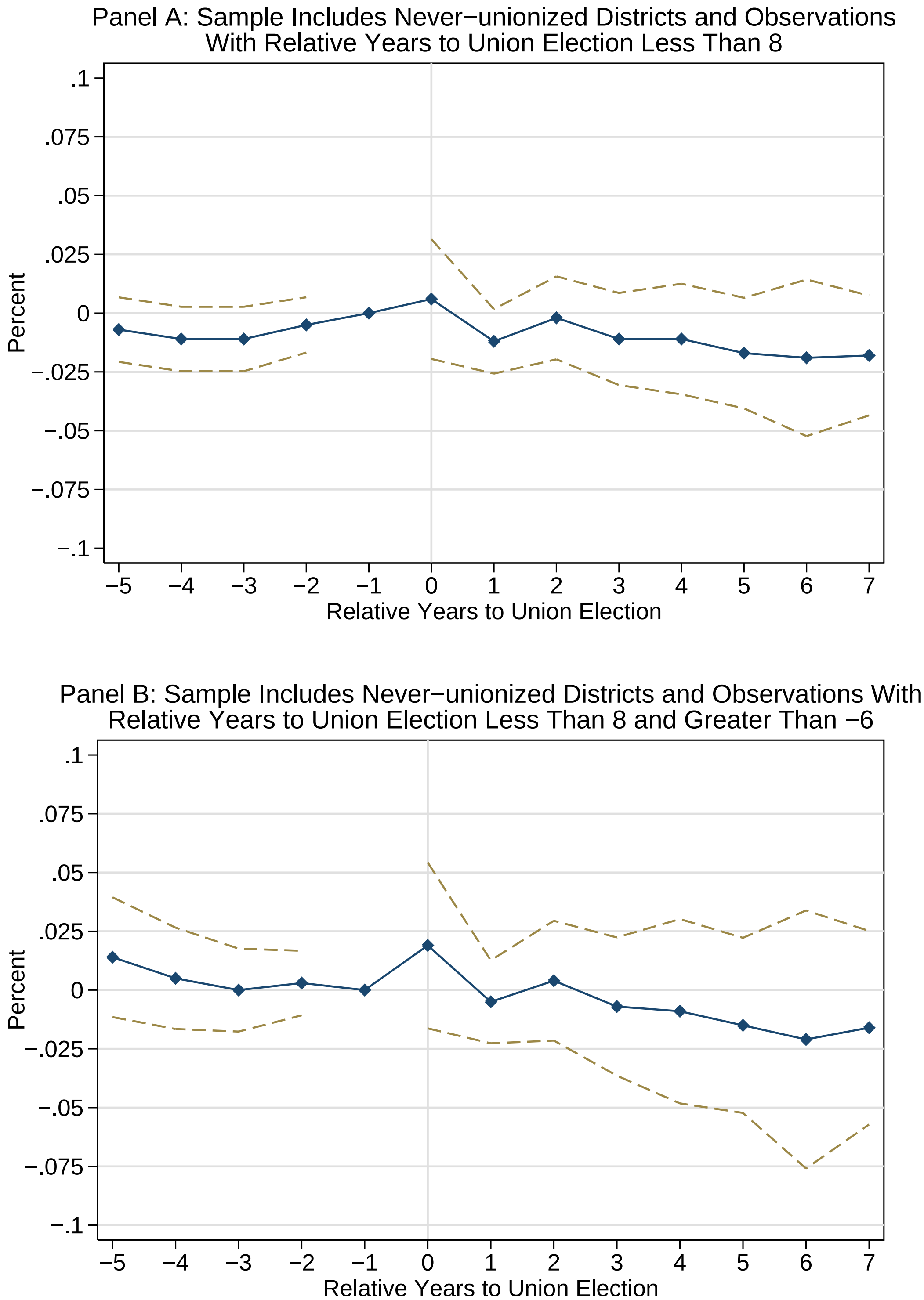
[1] Source: Author's calculations from the 1972–1991 Census/Survey of Governments as described in the text.
[2] Solid lines represent coefficient estimates from estimation of equation (2) in the text. Dotted lines are the bounds of the 95 percent confidence interval calculated from standard errors that are clustered at the district level.
[3] Relative year -1 is omitted in order to identify the model: a complete set of relative time dummy variables is collinear with the district fixed effects in equation (2). 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 Student Enrollment



[1] Source: Author's calculations from the 1972–1991 Census/Survey of Governments as described in the text.
[2] Solid lines represent 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.
[3] Relative year -1 is omitted in order to identify the model: a complete set of relative time dummy variables is collinear with the district fixed effects in equation (2). 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 Proportion of Total Expenditures on Instruction



[1] Source: Author's calculations from the 1967–1970, 1973–1974, 1976–1977, and 1979 ELSEGIS as described in the text.
[2] Solid lines represent 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.
[3] Relative year -1 is omitted in order to identify the model: a complete set of relative time dummy variables is collinear with the district fixed effects in equation (2). 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.