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 10 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.

KEYWORDS: Teachers' Unions, Public Sector Unions, Teacher Labor Markets, Education, Measurement Error, Event Study.
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1 Introduction

In 1959, conservative state legislators in Wisconsin amended a bill establishing collective bargaining rights for public employees to include public school teachers. Their hope was that such an amendment would cause moderates to vote against the legislation, as teacher organization was considered a radical idea at the time. Despite this effort, the bill passed, making Wisconsin the first state to establish the right of teachers to collectively bargain (Beilke (2001)).

Since its accidental beginnings, public school teacher collective bargaining has become a stable fixture in the American education system: by 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 over the past 40 years, teachers' unions remain controversial. Opponents of teachers' unions argue that 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) and Johnson (2004)).

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

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

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 or they can alter the production function itself. These effects will likely be related due to diminishing marginal productivity and the fact that 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]

[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.

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. Unlike previous teachers' union impact studies, this methodology allows me to estimate both the long and short term effect of unions on various resource levels by analyzing the change in district resources over time relative to union elections. By examining the pre-election trends, I also can determine whether there is any evidence that changes in educational inputs affect union election timing.

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.[3] While I find full-time teacher employment increases by between 5 and 10 percent, unions increase enrollment relative to non-union districts, which almost fully offsets any reductions in the student-teacher ratio due to the employment increase. Current operating expenditures and district revenues per student also 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 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. 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

[3]In his comprehensive review of the literature, Freeman (1986) reports that 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 premiums on the order of 5 to 10 percent for public sector protective services 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.[4] 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 that 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. She finds the presence of a teachers' union, as indicated by the existence of contracts combined with over 50 percent teacher union membership and the district reporting it engages in collective bargaining[5], 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 that unions increase high school dropout rates as well as reduce the returns to expenditures on teachers.

My results differ sharply from those reported in Hoxby (1996), despite the fact that I am able to reproduce many of her results on my sample of three states using the same data. I show the differences between my results and Hoxby's are due to pervasive classification error of unionization status of upwards of 45 percent in the

[4]See Freeman (1986) for an overview of the literature.
[5]Hoxby classifies a school district as unionized only if all three of these conditions are met.

Census of Governments. While these results suggest the accepted notions that unions reduce teacher productivity and drain district resources may not be robust to more accurate unionization data, they are based only on data from three states. To the extent the political and educational environments in Iowa, Indiana, and Minnesota in the 1970s and 1980s differed from the rest of the country, my results will not be generalizable.[6] However, the comparison between my results and those found by Hoxby (1996) underscores the importance of correctly measuring union status in union impact analyses and suggests collecting certification data from more states will allow a more general and robust analysis of teachers' union impacts in future research.

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 and discusses the election certification data in detail. 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

[6]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.

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.[7]

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,[8] 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

[7]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.

[8]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).

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 that these outcomes have differential effects on achievement, simple models of union behavior do not yield unique predictions about the impact of unionization. It is therefore necessary to analyze empirically the effect teachers' unions have on students and school districts in order to evaluate the claims made by both advocates and opponents of these unions.

3 Data

3.1 Measuring Teacher Unionization

3.1.1 Overview

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)).[9] There are, however, several problems with such measures that can bias impact estimates. The largest problem with union membership data is that 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.[10] The differences between union membership and the existence of a union for the purpose

[9] 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.

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

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)).[11] Absent measurement error, a collective bargaining agreement will accurately measure the presence of a union as long as all unions obtain contracts.[12] According to the NEA and AFT, which represent the vast majority of teachers' unions in the United States, it is rare for a unionized district to never obtain a contract, although there can be a lag between union formation and the culmination of collective bargaining in the form of a contract.

No previous union effects study has been based on data that accurately describe both the timing of unionization and the existence of a teachers' union in a given district. In order to obtain an improved measure of teacher unionization, I hand-collected teacher union certification dates from union election certifications housed in the Public Employment Relations Board/Commission (PERC) office in Indiana, Iowa 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.

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, if available, the

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

[12] 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.

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. However, for those UniServ districts that had copies of the original contracts, the union election certification data 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.[13]

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] Beginning in Minnesota in 1972 and followed by Indiana in 1973 and Iowa in 1975, these states passed "duty-to-bargain" laws, which mandated that 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. The data show that 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 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

[13] All of the discrepancies came either from early recognition or from the merger of two districts that necessitated a new union election. The certification date recorded from the PERC offices were incorrect for 3.8 percent of school districts.

[14] These contracts were all due to "voluntary recognition" of the union by the school district.

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

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 that, while the existence of a union and the existence of a negotiated contract are conceptually distinct, in practice they are similar. Analyzing the effect of winning a unionization election as opposed to negotiating a contract should yield comparable results.

Secondly, because the certification dates are obtained from official state documents, there will be less measurement error than in data based on survey responses. Finally, similar to the contract measure, 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 six 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.[16] 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.

3.1.2 A Comparison of Alternative Unionization Measures

How does the new election certification measure of teacher unionization compare to the only other district-level panel containing unionization information: the Census of Governments (COG) Labor Relations Survey used in Hoxby (1996)? The COG does

[16] 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.

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. This is the union measure utilized by Hoxby (1996) in her analysis of teacher unionization and 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, as previously discussed, is that 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 with the union measure in the COG is classification error. Although Hoxby's 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 1 contains a comparison of district-level unionization rates from the Census of Governments and the election certifications for each state in the sample. Note that 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 that there was little voluntary recognition occurring in these states in this period and that 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.

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. Further, in Indiana, 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.[17] This is suggestive evidence that the measure of the existence of contracts in the COG contains measurement error.

Taking the election certification data as the true measure of unionization status, Table 2 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.[18] However, the misclassification rate is as high as 46.5% in Indiana in 1987. 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. I will turn to the impact of this measurement error on union impact estimates in Section 6.

3.2 Other Data Sources

I combine my more accurate 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 employ-

[17] 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. Also, note 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."

[18] 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.. He 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.

ment, student-teacher ratios, real current operating expenditures (COE) and real total revenues for each district in the sample.[19] 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).[20] 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.

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.[21] I measure high school dropout rates using the following formula:[22]

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

[19] Given the errors in the Census of Governments labor relations data described in the previous section, 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, for example, the number of teachers belonging to the union. The latter information is not likely to be kept on file by the district administrative offices.

[20] 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.

[21] 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)).

[22] Hoxby (1996) utilizes a similar formula that includes those who are nineteen years old in the denominator. I exclude this group because the typical ages for high school attendance are 14 through 18. This change has no effect on the results.

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. These variables come directly from the Census files and are the same as those used in Hoxby (1996).[23]

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:

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

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

[23]Hoxby (1996) also includes a variable that measures the percentage of K-12 enrollment attributed to African Americans. Because this is difficult to measure in the 1970 Census and because the states in my sample have small African American populations, I exclude this variable from my analysis.

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[24] 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.[25] Thus, the full time pattern of union impacts over the event window allowed by the data will be estimated by equation (2), whereas standard models of union impacts are much more restrictive.

Another major advantage of equation (2) is that it includes district and time fixed effects 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 union-

[24]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.

[25]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.

ization, 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.[26]

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 movement 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 that, because the Census of Governments panel begins in 1972 and the collective bargaining laws were passed in 1972, 1973, and 1975 in Minnesota, Indiana, and Iowa, respectively, the relative time dummies with j<0 will be identified predominantly off of districts that unionize relatively later

[26]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.

in the sample.[27] 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 3 compare districts that never unionize to districts that do unionize as of 2004. The table indicates that 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 3 compare districts that unionized within a year of the passage of their state's duty-to-bargain law and those that unionized later. The comparison of means suggests districts that unionized immediately following passage of their state's duty to bargain law had a larger percentage of adults with a bachelor's degree, were larger, more urban, had higher median rent, unemployment rate and district enrollment, but had a lower poverty rate than those that unionized later. Overall, this exercise suggests districts in larger cities and suburbs organized earlier while the more rural districts unionized later or not at all.[28]

What effect can one expect these differences to have on the estimates from equa-

[27] 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 that 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.

[28] There are many explanations for this trend in the literature on the history of teachers' unions. The first is that 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.

tion (2)[29] 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. While this restriction does not hold for equation (2),[30] the main source of variation off of which the state-year effects are identified is the control group districts.[31]

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

[29]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 3. However, the union impact estimates do not change appreciably nor do the substantive conclusion from those estimates change when this restriction is imposed.

[30]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.

[31]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).

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.[32] 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 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}.[33]

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

[32]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.

[33]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.

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 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 that 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 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;[34] none of the point estimates is statistically distinguishable from zero at the 5 percent level, and most are less than 1% in both

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

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 1 year. 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.[35] 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 10 percent. Further, the majority of these estimates are statistically distinguishable from zero at the 5 percent level and 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, causing more teachers to be hired.[36]

The employment results in Figure 3 suggest class sizes, as measured by student-teacher ratios, should decrease. Figure 4, however, illustrates that 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

[35]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.

[36]However, 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.

log full-time teacher employment as the dependent variable.[37] 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.[38] 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 districts, which raises class sizes to near their pre-union levels.

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.[39] 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,

[37] 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.

[38] The enrollment increases due to unionization are an interesting result because no other study has found such effects. This result, however, could be due to relative increases in the population of unionizing areas that coincide with, but are unrelated to, unionization. While my results are robust to dropping urban districts from the analysis, they are only suggestive that unions cause enrollment increases. Collecting certification data from other states that passed duty-to-bargain laws at different times than in my sample will allow me to test the robustness of this finding in future research.

[39] 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.

it is not surprising that Figures 5 and 6 are similar. In both figures, there is considerable variation in the estimates, with an upward spike of 3.6 to 5 percent respectively in Figures 5 and 6 for the first year of unionization, though the estimates are 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 that 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 that teachers' unions successfully guard against per-student revenue and expenditure losses in the face of rising enrollment.[40] However, an equally plausible interpretation is that unions have little effect, especially in the long-run, on these outcomes.

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 9 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 expendi-

[40] Note that this enrollment increase is relative to districts in the control group. Total enrollment is decreasing for both treated and untreated districts, but is declining slower in the former than in the latter.

tures. 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,[41] 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 expenditures. What theories of school district and union behavior might be consistent with my findings? One model that fits into the context of the above results is Tiebout sorting (Tiebout (1956)). Tiebout sorting could occur due to the increases in teacher employment and current operating expenditures per student directly after unionization. To the extent parents value these increased resource levels, enrollment in unionized districts should increase, which is what the data show. This enrollment increase negates any class size and per-student expenditure and revenue gains from rising employment, expenditure and revenue levels following unionization. This explanation assumes the enrollment increase is caused by the district-level unionization decision. Conversely, unions may be reacting to expected relative enrollment increases in their district and force the administration to keep class sizes and expenditures per student roughly constant.

Finally, 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

[41] The exception is for transportation expenditures. However, this result is likely due to the fact that 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.

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.[42]

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 that 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.

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.

[42]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.

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 that 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:

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

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.

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 4. 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.

5.2 Results

Results from estimation of equation (4) are presented in Table 4. 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 increase high school dropout rates by 0.007 percent using the 1970 and 1980 data and reduce dropout rates by 0.44 percent using observations from all three years. Neither of these coefficients issignificant 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 4 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 it is statistically significant at the 5 percent level in Column (ii). 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 0.46 in unionized districts relative to non-unionized districts. These estimates provide evidence that 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 though not significant at the 5 or 10 percent level in either column. However, the negative and large point estimates suggest increasing teacher pay has higher returns to reducing dropout rates in unionized as opposed to non-unionized districts.

It is important to note that the results from Table 4 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, union 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 4 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 increase is achieved without the coincident increase in educational costs, such as teacher pay, that is typically associated with teacher unionization.

My education production function estimates contrast with those of Hoxby (1996), who utilizes the Census of Governments labor relations data to measure teacher unionization. In explaining dropout rates, Hoxby finds a negative coefficient on the Student-teacher Ratio, Union interaction term and a positive coefficient on the Log Teacher Pay, Union interaction term. She takes these results as evidence of teachers' union rent seeking behavior. I obtain similar results to hers on my sample when I employ her measure, but when I utilize the certification data to measure unionization, Table 4 illustrates one can draw the opposite conclusion. The central difference between the two sets of estimates is due to the measurement error in the Census of Governments measure discussed in Section 3.1.2.. In the proceeding section, I conduct a formal comparison of my results and those reported in Hoxby (1996) in order to understand the effect of measurement error on union impact estimates.

6 The Effect of Different Union Measures on Union Impact Estimates

In order to understand more fully the differences between my results from Sections 4 and 5 and those from the existing literature, it is instructive to undertake a comparison of union impact estimates using my new certification data and the previously used Census of Governments constructed union measure. Specifically, I replicate estimates from Hoxby (1996) using both union measures. I focus on this paper 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 that 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.[43]

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 that the standard error on the union coefficient increases by a factor of 4.2 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.

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 3.5. 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.[44]

[43]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.

[44]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 that the differences between the estimates in columns (ii) and (iii) of Tables 5-7 are due

What is most interesting about the form of the measurement error bias is that 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 that 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 that 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.[45]

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 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 that both forms of bias are present and reinforce each other for teacher pay and current operating expenditures but work in opposite directions for student-teacher ratios and high school dropout rates.[46]

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 districts fail 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.

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

[46]While I am able to decompose the various sources of the misclassification bias that occurs from the Census of Governments union measure, I am unable to determine why the measurement error is occurring. There are no clear trends in the classification error to suggest certain types of districts are systematically misunderstanding survey questions or certain schools are filling out the forms incorrectly in a systematic manner. The reasons why the

The central conclusion from Tables 5-7 is that the classification error reported in Tables 1 and 2 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 that 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 that teacher unionization causes an increase in full–time teacher employment of between 5 and 10 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 that 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 that teachers' unions have no effect on high school dropout rates: the point estimates are small and negative in both specifications, and in neither case are the coefficients statistically differentiable from zero. I do find evidence consistent with unions increasing teacher productivity in the form

measurement error in the COG data documented here takes the form that it does remain an open question.

of higher returns to lower class sizes and higher teacher pay, but only the class size estimates are significant at even the 10 percent level. 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 that 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 that 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 that 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 that 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. Further, because these states all passed duty-to-bargain laws in the early to mid-1970s, I am not able to disentangle confounding relative trends in unionized districts from actual treatment effects. It is possible that such spurious trends are responsible for producing the enrollment increases and the lack of wage, expenditure, and class size effects reported above. 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. Collecting union election certification data from other states will allow a more general and robust analysis of teachers' unions than I am able to produce in this paper. The main implication of this study is that 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 definition of most of these variables is straightforward and comes 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 that 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 will only 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 1 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 that is directly due to measurement error and the part that is 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 that $\tilde{\beta}$ will be less than β in absolute value. In other words, classical measurement error will cause an attenuation bias. Note that 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 that 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:

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

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 that 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 that 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 that a district is reported as unionized in the COG when it had successfully completed a union election and the probability that 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:

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

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

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

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

where the last line follows from the fact that 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 and log real COE per student 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 and COE per student. This result occurs because non-unionized districts have higher pay and expenditures than unionized districts, so mis-classifying non-unionized districts as unionized will bias upward the estimated impact of teachers' unions on both teacher pay and expenditures. That the classification error is positively correlated with the regression error for teacher pay and COE is due to the fact that school districts incorrectly classified as unionized tend to have higher levels of teacher pay and expenditures than school districts for which union classification is correct. Thus, the misclassification of union status will serve to further bias upward the union impact estimates on these two variables.

For the student-teacher ratio and high school dropout rate decompositions, the biases offset each other somewhat, but the relatively large positive correlation between the measurement error and the regression error dominates the negative bias due to measurement error in the union variable. In these cases, districts wrongly classified as unionized have higher dropout rates and student-teacher ratios, which is partially offset by the fact that non-unionized school districts tend to have lower student-teacher ratios and dropout rates.

Table 1: 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%	0.22%	0.00%	0.00%	53.58%	17.78%
Non-Union	5.76%	94.01%	13.86%	86.14%	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%	56.44%	22.44%	55.89%	20.79%
Non-Union	9.31%	26.61%	12.54%	8.58%	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%	54.79%	26.40%	58.20%	22.17%
Non-Union	8.43%	22.39%	8.91%	9.90%	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%	39.93%	64.67%	16.86%
Non-Union	9.76%	18.18%	6.60%	11.22%	13.63%	4.85%

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

Table 2: 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	5.99%	13.86%	37.18%	19.38%
1977	23.50%	34.98%	36.95%	31.34%
1982	26.39%	35.31%	35.33%	31.93%
1987	35.03%	46.53%	30.48%	36.31%

[1] Source: Authors' 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 1. 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 3: 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 4: 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	0.007	38.146	-0.442	26.670
	(0.875)	(29.573)	(0.700)	(29.397)
$\frac{\text{Student}}{\text{Teacher}}$ Ratio	0.037	0.016	0.047*	0.034
	(0.022)	(0.018)	(0.029)	(0.026)
$\frac{\text{Student}}{\text{Teacher}}$*Union	.	0.463**	.	0.175
	.	(0.196)	.	(0.106)
Log Real Full Time Teacher Pay	1.616	2.674	-2.801	-1.328
	(2.172)	(2.679)	(1.797)	(2.222)
(Log Real Full Time Teacher Pay)*Union	.	-5.510	.	-3.618
	.	(3.671)	.	(3.649)
Log Population	27.829**	27.706**	31.447**	31.055**
	(4.137)	(4.018)	(3.233)	(3.187)
Percent Urban	0.651	0.470	0.479	0.439
	(0.687)	(0.688)	(0.709)	(0.703)
Log Average Income	-5.805	-5.561	-21.940**	-21.107**
	(4.190)	(4.230)	(4.125)	(4.146)
Log Median Gross Rent	0.187	0.226	0.308	0.244
	(1.827)	(1.791)	(1.793)	(1.716)
Percent Below Poverty	-0.082	-0.074	-0.300*	-0.282*
	(0.114)	(0.123)	(0.160)	(0.167)
Percent Unemployed	0.362	0.366	0.024	0.041
	(0.228)	(0.225)	(0.110)	(0.148)
Percent Black	-0.252**	-0.220**	0.200*	0.220**
	(0.126)	(0.143)	(0.105)	(0.108)
Percent Hispanic	0.086	-0.025	0.069	0.079
	(0.554)	(0.645)	(0.192)	(0.182)
Percent 12–15 Years School	0.210**	0.187**	0.166**	0.156**
	(0.073)	(0.073)	(0.077)	(0.080)
Percent 16+ Years School	0.033	0.016	0.449**	0.441**
	(0.103)	(0.106)	(0.065)	(0.065)
Percent Private Enrollment	-0.259**	-0.258**	-0.369**	-0.362**
	(0.053)	(0.089)	(0.076)	(0.078)
Log Public School Enrollment	-24.952**	-25.823**	-29.905**	-29.905**
	(3.550)	(3.541)	(2.738)	(2.718)
Constant	-63.224**	-65.839**	-17.390	-24.838
	(29.609)	(29.729)	(23.912)	(26.292)

[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. Standard errors are clustered at the district level: ** indicates significance at the 5 percent level and * indicates significance at the 10 percent level.

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**	0.054**	-0.033
	(0.008)	(0.024)	(0.101)
Log Population	-0.015**	0.029	0.026
	(0.004)	(0.063)	(0.058)
Percent Urban	0.0005**	0.0004	0.0003
	(0.0002)	(0.0007)	(0.0007)
Log Average Income	0.199**	-0.066	-0.049
	(0.022)	(0.186)	(0.188)
Log Median Gross Rent	-0.021**	0.064	0.081
	(0.010)	(0.103)	(0.102)
Percent Below Poverty	-0.0001	-0.009	-0.009
	(0.0006)	(0.006)	(0.007)
Percent Unemployed	-0.003**	-0.009*	-0.009*
	(0.001)	(0.005)	(0.005)
Percent Black	-0.004**	0.0001	0.003
	(0.001)	(0.010)	(0.009)
Percent Hispanic	-0.004**	0.002	0.0002
	(0.001)	(0.010)	(0.011)
Percent 12–15 Years School	-0.002**	-0.002	-0.004
	(0.0003)	(0.004)	(0.004)
Percent 16+ Years School	0.004**	-0.005	-0.007
	(0.0004)	(0.005)	(0.005)
Percent Private Enrollment	0.001**	0.003	0.004
	(0.0002)	(0.004)	(0.004)
Log Public School Enrollment	0.041**	-0.050	-0.051
	(0.002)	(0.057)	(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 of equation (5) in the text using the COG-based union measure on the IA, IN, and MN sample. Column (iii) presents estimates of equation (5) in the text 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
	(i)	(ii)	(iii)
Independent Variable	U.S. Estimation Sample	IA,IN,MN Estimation Sample	IA,IN,MN Estimation Sample
Union	0.029**	0.017	-0.024
	(0.007)	(0.017)	(0.059)
Log Population	0.029**	0.013	0.011
	(0.004)	(0.068)	(0.068)
Percent Urban	-0.001**	-0.0004	-0.0004
	(0.0001)	(0.0007)	(0.0007)
Log Average Income	0.116**	0.146	0.151
	(0.019)	(0.149)	(0.150)
Log Median Gross Rent	0.232**	-0.032	-0.026
	(0.008)	(0.105)	(0.106)
Percent Below Poverty	-0.007	-0.009*	-0.009*
	(0.001)	(0.005)	(0.005)
Percent Unemployed	-0.005**	-0.006	-0.007
	(0.001)	(0.004)	(0.005)
Percent Black	0.005**	-0.005	-0.004
	(0.001)	(0.006)	(0.005)
Percent Hispanic	0.003**	-0.005	-0.006
	(0.001)	(0.008)	(0.009)
Percent 12–15 Years School	0.005**	-0.0001	-0.0006
	(0.001)	(0.003)	(0.003)
Percent 16+ Years School	0.004**	-0.007	-0.008*
	(0.001)	(0.005)	(0.005)
Percent Private Enrollment	0.003**	-0.001	-0.001
	(0.001)	(0.003)	(0.003)
Log Public School Enrollment	-0.409**	-0.024	-0.024
	(0.011)	(0.031)	(0.031)
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 of equation (5) in the text using the COG-based union measure on the IA, IN, and MN sample. Column (iii) presents estimates of equation (5) in the text 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	0.066
	(0.338)	(0.547)	(0.865)
Log Population	-0.841**	1.154	1.158
	(0.071)	(2.618)	(2.605)
Percent Urban	0.029**	0.020	0.020
	(0.003)	(0.015)	(0.014)
Log Average Income	-1.170**	-4.698	-4.661
	(0.367)	(4.100)	(4.150)
Log Median Gross Rent	-1.167**	0.024	0.053
	(0.161)	(2.519)	(2.462)
Percent Below Poverty	0.149	-0.038	-0.038
	(0.012)	(0.136)	(0.137)
Percent Unemployed	0.123**	-0.101	-0.100
	(0.015)	(0.103)	(0.103)
Percent Black	-0.143**	0.285	0.291
	(0.012)	(0.478)	(0.461)
Percent Hispanic	-0.065**	-0.162	-0.164
	(0.014)	(0.233)	(0.227)
Percent 12–15 Years School	-0.129**	0.071	0.068
	(0.011)	(0.098)	(0.106)
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	-3.001
	(0.217)	(4.769)	(4.741)
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 of equation (5) in the text using the COG-based union measure on the IA, IN, and MN sample. Column (iii) presents estimates of equation (5) in the text 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 and Greater Than -6

	Dependent Variable: Log of				
Independent Variable	Real Monthly Full Time Teacher Pay	Full Time Teacher Employment	Student-Teacher Ratio	Real COE Per Student	Real Revenue Per Student
Relative Time = -5 Years	0.028*	0.009	0.009	-0.028	-0.023
	(0.025)	(0.034)	(0.040)	(0.025)	(0.027)
Relative Time = -4 Years	0.017	0.003	-0.011	-0.010	-0.009
	(0.012)	(0.019)	(0.017)	(0.012)	(0.015)
Relative Time = -3 Years	0.007	0.004	-0.007	-0.031**	-0.036*
	(0.010)	(0.017)	(0.014)	(0.013)	(0.020)
Relative Time = -2 Years	0.006	-0.030	-0.014	-0.003	-0.019
	(0.010)	(0.021)	(0.017)	(0.013)	(0.017)
Relative Time = 0 Years	0.015	0.035*	-0.033	0.038*	0.050**
	(0.011)	(0.025)	(0.019)	(0.021)	(0.020)
Relative Time = 1 Years	0.013	0.036*	-0.018	-0.008	0.017
	(0.011)	(0.020)	(0.020)	(0.020)	(0.020)
Relative Time = 2 Years	-0.003	0.042*	-0.015	-0.045	-0.049
	(0.012)	(0.021)	(0.019)	(0.029)	(0.031)
Relative Time = 3 Years	0.011	0.045**	-0.008	-0.024*	-0.001
	(0.013)	(0.023)	(0.020)	(0.015)	(0.017)
Relative Time = 4 Years	0.008	0.058**	-0.025	-0.003	0.008
	(0.013)	(0.025)	(0.021)	(0.015)	(0.016)
Relative Time = 5 Years	0.006	0.044*	-0.013	-0.011	-0.004
	(0.014)	(0.024)	(0.021)	(0.015)	(0.017)
Relative Time = 6 Years	0.011	0.054**	-0.022	-0.027	-0.020
	(0.013)	(0.025)	(0.022)	(0.017)	(0.018)
Relative Time = 7 Years	0.008	0.052*	-0.029	-0.018	-0.032
	(0.004)	(0.028)	(0.026)	(0.019)	(0.019)
Relative Time = 8 Years	0.014	0.064**	-0.024	-0.007	-0.014*
	(0.015)	(0.030)	(0.024)	(0.017)	(0.018)
Relative Time = 9 Years	0.024	0.073**	-0.029	-0.006	-0.022
	(0.016)	(0.031)	(0.024)	(0.016)	(0.021)
Relative Time = 10 Years	0.018	0.035	-0.022	-0.001	-0.003
	(0.016)	(0.028)	(0.024)	(0.019)	(0.023)
Constant	8.156**	4.560**	2.479**	8.720**	8.217**
	(0.020)	(0.043)	(0.031)	(0.017)	(0.043)
N	8515	8515	7500	12225	12229
Number of 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 are collinear with the district fixed effects in equation (2).

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

	Dependent Variable: Log of				
Independent Variable	Real Monthly Full Time Teacher Pay	Full Time Teacher Employment	Student-Teacher Ratio	Real COE Per Student	Real Revenue Per Student
Relative Time = -5 Years	0.007	-0.016	0.019	-0.041	-0.025
	(0.023)	(0.048)	(0.060)	(0.033)	(0.034)
Relative Time = -4 Years	0.002	-0.001	-0.006	-0.018	-0.017
	(0.018)	(0.028)	(0.029)	(0.016)	(0.022)
Relative Time = -3 Years	-0.003	-0.0004	-0.005	-0.037**	-0.042*
	(0.015)	(0.024)	(0.026)	(0.015)	(0.025)
Relative Time = -2 Years	-0.004	-0.035	-0.011	-0.010	-0.022
	(0.014)	(0.028)	(0.025)	(0.015)	(0.021)
Relative Time = 0 Years	0.007	0.039	-0.046	0.038	0.051**
	(0.014)	(0.025)	(0.035)	(0.023)	(0.024)
Relative Time = 1 Years	0.006	0.045**	-0.012	-0.015	0.011
	(0.014)	(0.022)	(0.026)	(0.021)	(0.025)
Relative Time = 2 Years	-0.008	0.055**	-0.014	-0.043**	-0.040
	(0.015)	(0.025)	(0.026)	(0.026)	(0.030)
Relative Time = 3 Years	0.007	0.055**	-0.003	-0.031**	-0.011
	(0.015)	(0.027)	(0.026)	(0.014)	(0.020)
Relative Time = 4 Years	0.011	0.065**	-0.025	-0.011	0.002
	(0.016)	(0.028)	(0.028)	(0.014)	(0.019)
Relative Time = 5 Years	0.006	0.061**	-0.020	-0.014	-0.006
	(0.016)	(0.029)	(0.027)	(0.015)	(0.021)
Relative Time = 6 Years	0.006	0.081**	-0.032	-0.027*	-0.017**
	(0.016)	(0.028)	(0.028)	(0.016)	(0.022)
Relative Time = 7 Years	0.003	0.080**	-0.041	-0.023	-0.033**
	(0.018)	(0.033)	(0.032)	(0.018)	(0.023)
Relative Time = 8 Years	0.016	0.079**	-0.020	-0.019	-0.020*
	(0.018)	(0.032)	(0.029)	(0.016)	(0.022)
Relative Time = 9 Years	0.029	0.101**	-0.037	-0.012	-0.027**
	(0.020)	(0.035)	(0.029)	(0.016)	(0.025)
Relative Time = 10 Years	0.019	0.079**	-0.048	-0.010	-0.015
	(0.020)	(0.035)	(0.031)	(0.018)	(0.027)
Constant	8.108**	4.602**	2.529**	8.719**	8.184**
	(0.023)	(0.049)	(0.043)	(0.019)	(0.046)
N	7549	7549	6633	10822	10825
Number of 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] Relative year -1 is omitted in order to identify the model: a complete set of relative time dummy variables are 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 and Greater Than -6

	Dependent Variable: Log Proportion of Total Expenditures on:					
Independent Variable	Administration	Instruction	Attendance & Health	Trans-portation	Plant O & M	Fixed Charges
Relative Time = -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)
Relative Time = -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)
Relative Time = -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)
Relative Time = -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)
Relative Time = 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)
Relative Time = 1 Years	-0.007	-0.012*	0.219	0.088**	0.011	0.004
	(0.039)	(0.007)	(0.156)	(0.033)	(0.021)	(0.024)
Relative Time = 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)
Relative Time = 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)
Relative Time = 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)
Relative Time = 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)
Relative Time = 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)
Relative Time = 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
Number of 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 are collinear with the district fixed effects in equation (2).

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

	Dependent Variable: Log Proportion of Total Expenditures on:					
Independent Variable	Administration	Instruction	Attendance & Health	Trans-portation	Plant O&M	Fixed Charges
Relative Time = -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)
Relative Time = -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)
Relative Time = -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)
Relative Time = -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)
Relative Time = 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)
Relative Time = 1 Years	-0.054	-0.005	0.097	0.051	-0.002	-0.006
	(0.055)	(0.009)	(0.154)	(0.036)	(0.027)	(0.031)
Relative Time = 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)
Relative Time = 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)
Relative Time = 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)
Relative Time = 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)
Relative Time = 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)
Relative Time = 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
Number of 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] Relative year -1 is omitted in order to identify the model: a complete set of relative time dummy variables are 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	0.368**	-0.093
	(0.052)	(0.064)
Log Real COE per Student	0.022	0.024**
	(0.026)	(0.004)
Student-Teacher Ratio	0.018**	0.006**
	(0.004)	(0.002)
High School Dropout Rate	-0.0003	0.004**
	(0.001)	(0.001)

[1] Source: Authors' 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	0.038**	0.003
	(0.010)	(0.012)
Percent Urban	0.072**	0.001
	(0.037)	(0.039)
Log Average Income	-0.028	-0.042**
	(0.028)	(0.007)
Log Median Rent	-0.048	-0.127**
	(0.037)	(0.024)
Percent Poverty	-0.765**	-0.525**
	(0.269)	(0.223)
Percent Unemployed	-0.002**	-0.008**
	(0.001)	(0.001)
Percent Black	-0.001	-0.002
	(0.003)	(0.006)
Percent Hispanic	-0.011	-0.008
	(0.006)	(0.013)
Percent 12–15 Years School	-0.001**	0.008**
	(0.001)	(0.002)
Percent 16+ Years School	0.002	-0.010**
	(0.001)	(0.002)
Percent Private Enrollment	0.004**	-0.003**
	(0.001)	(0.001)
Log Public School Enrollment	0.042**	0.005
	(0.010)	(0.012)

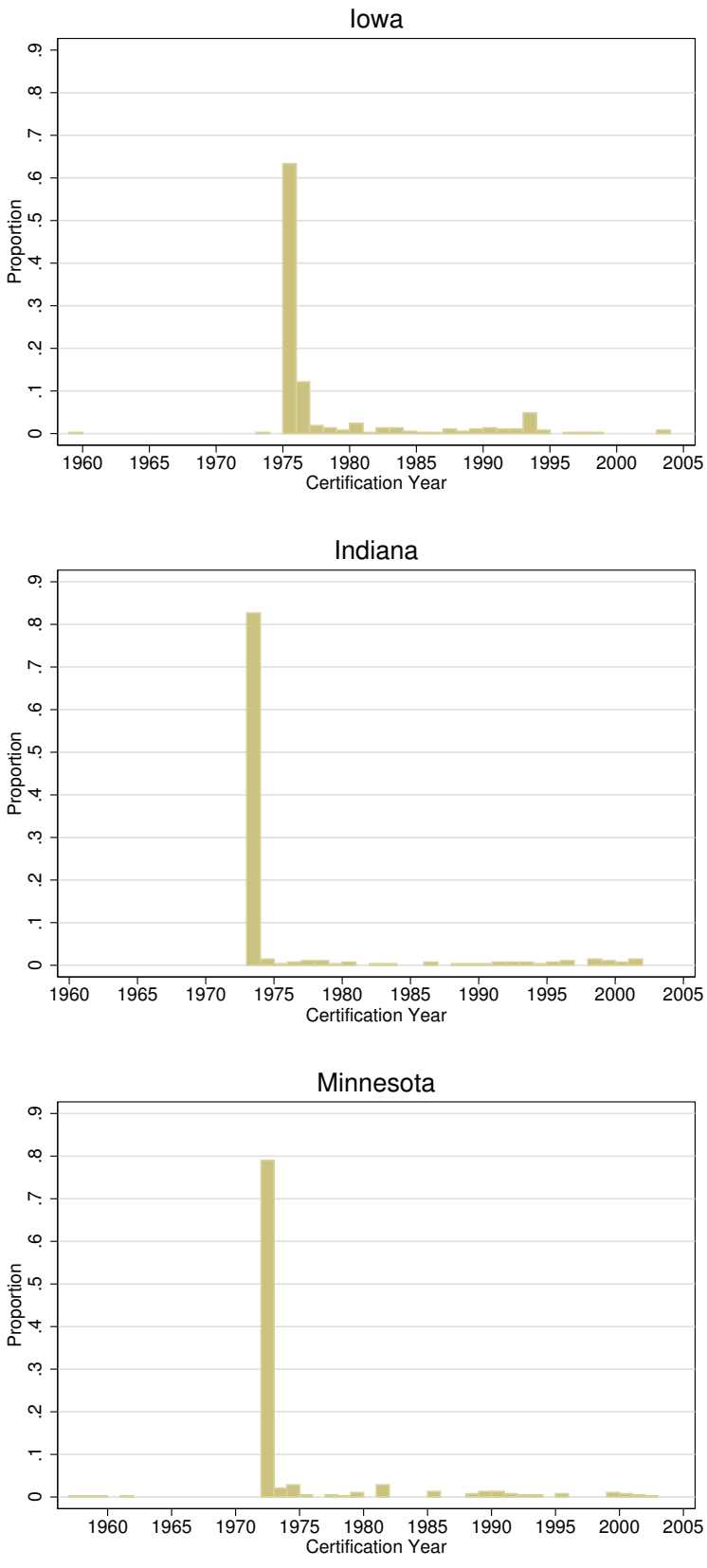
[1] Source: Authors' calculations from the 1972, 1982, and 1987 Census of Governments, the 1970, 1980 and 1990 U.S. Census, and the teachers' union election certification data described in the text.
[2] Each cell above represents a separate pooled linear probability model regression. Standard errors 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	-0.033	0.087	0.032	0.054
Log Real COE per Student	0.017	-0.024	0.041	0.024	0.017
Student-Teacher Ratio	0.117	0.066	0.051	-0.066	0.117
High School Dropout Rate	0.589	0.325	0.264	-0.323	0.587

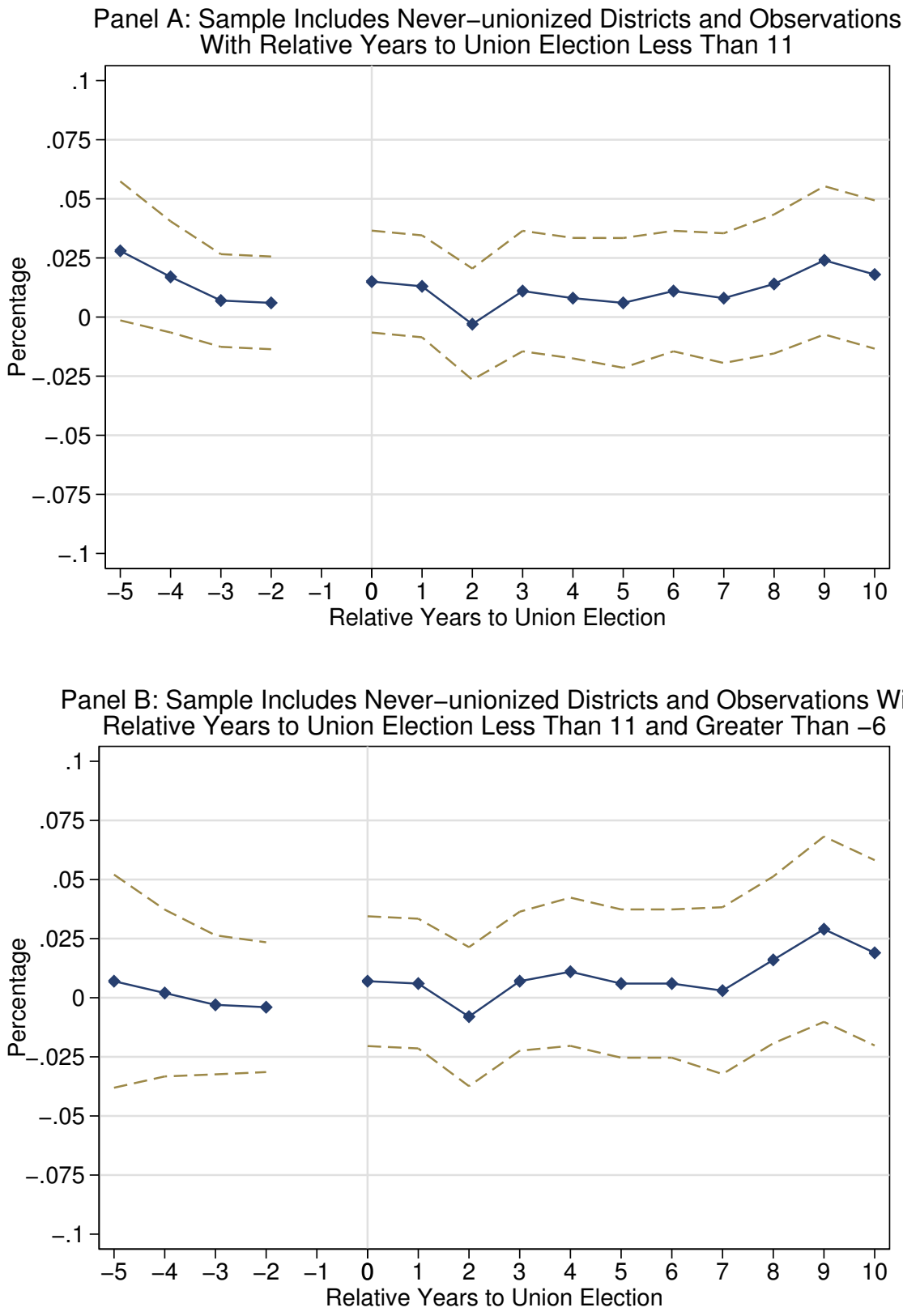
[1] Source: Authors' calculations from the 1972, 1982, and 1987 Census of Governments, the 1970, 1980 and 1990 U.S. Census, and the teachers' union election certification data described in the text.
[2] Each regression includes district and year fixed effects as well as district-specific linear time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

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



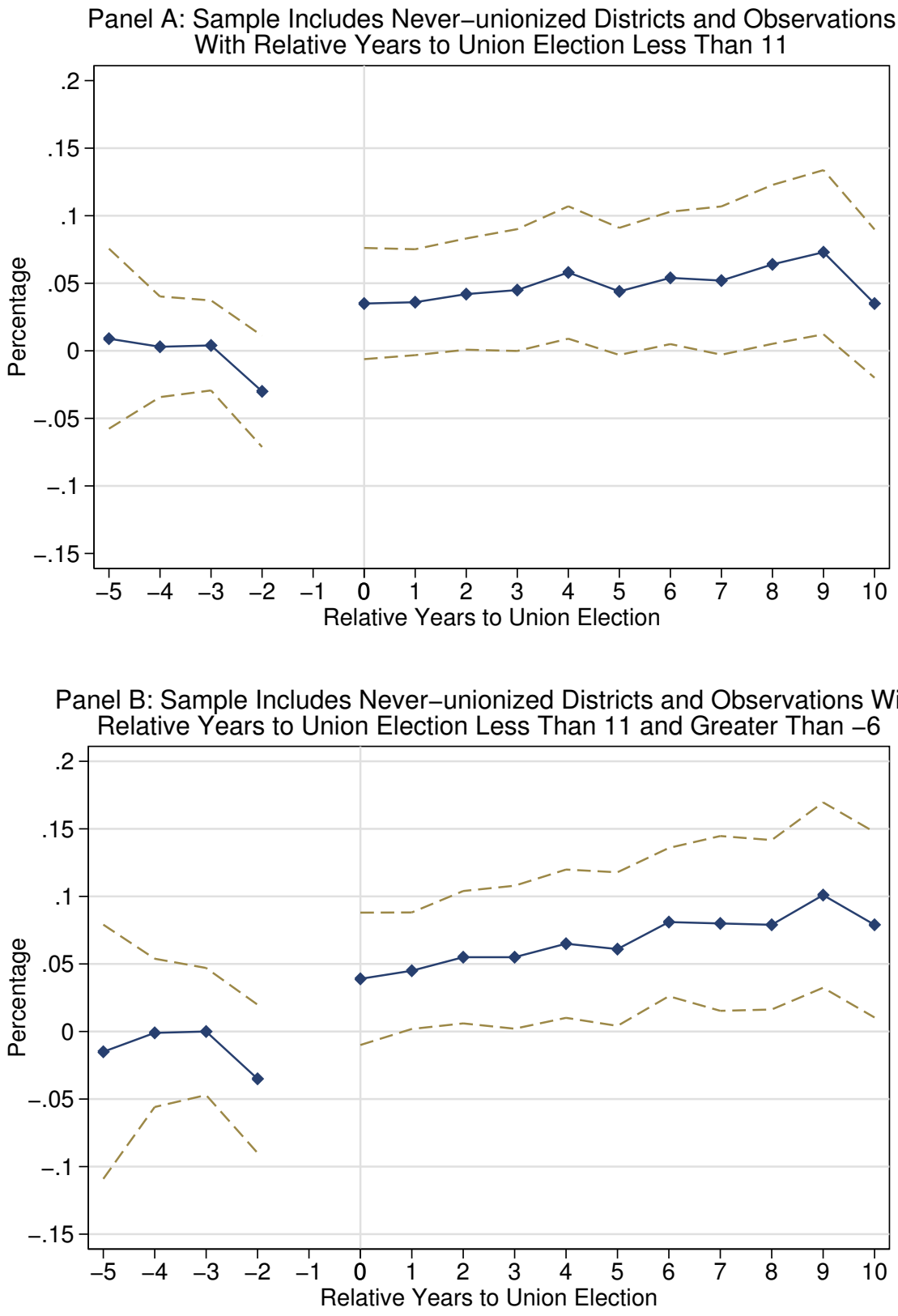
Source: Teachers' union election certification data described in the text.

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



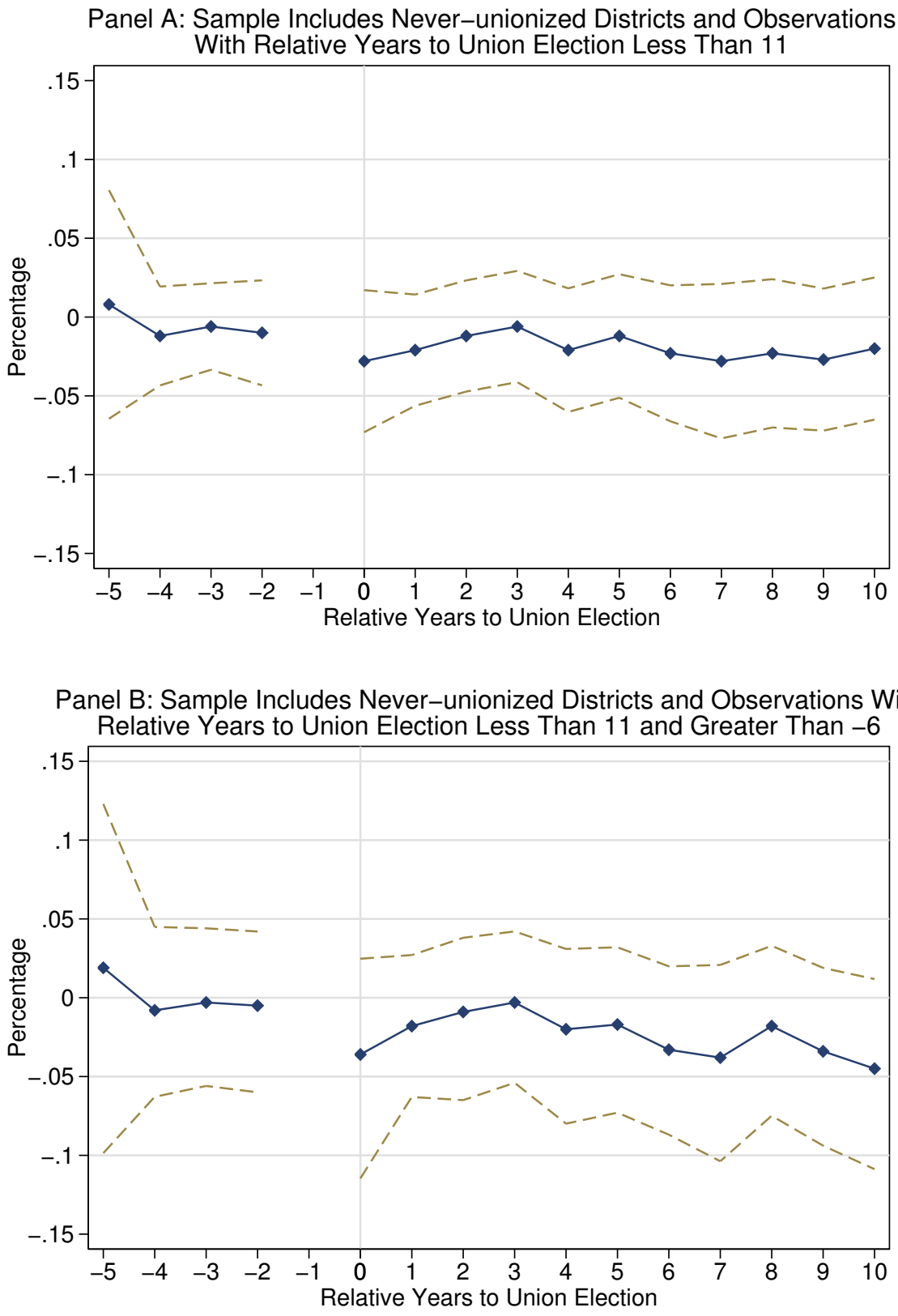
[1] Source: Author's calculations from the 1972–1991 Census/Survey of Governments as described in the text.
[2] Solid lines represent coefficient estimates from estimation of equation (2) in the text. Dotted lines are the bounds of the 95 percent confidence interval calculated from standard errors that 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).

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



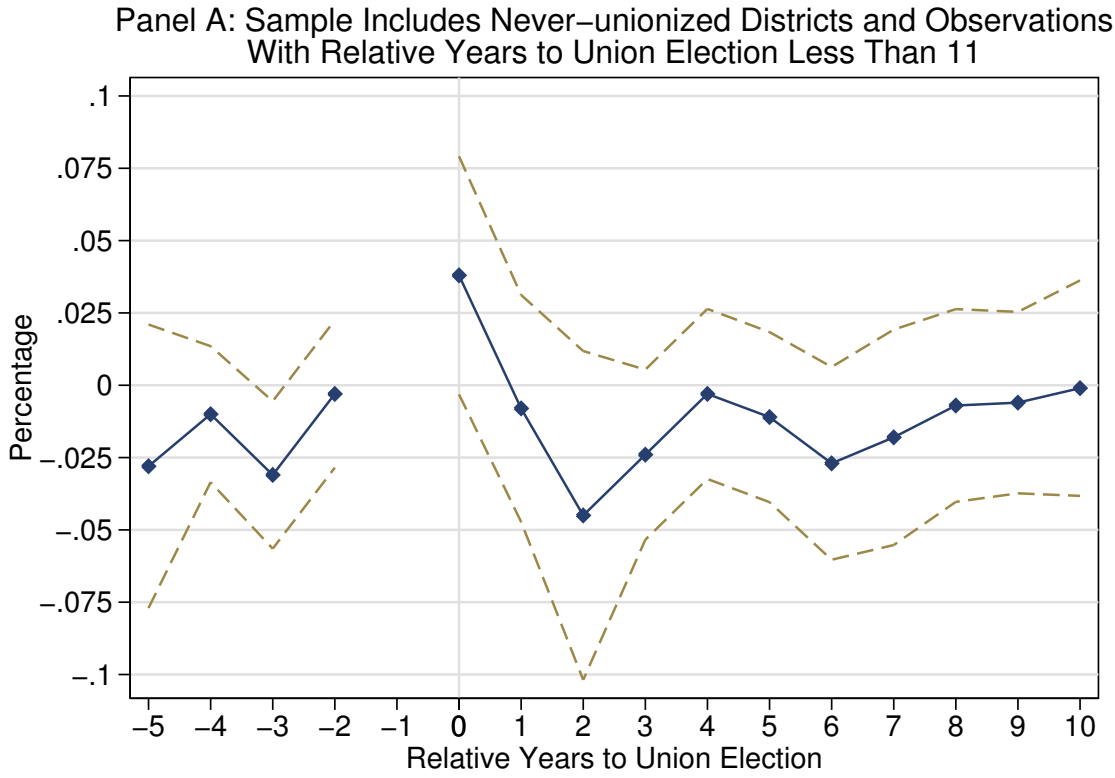
[1] Source: Author's calculations from the 1972–1991 Census/Survey of Governments as described in the text.
[2] Solid lines represent coefficient estimates from estimation of equation (2) in the text. Dotted lines are the bounds of the 95 percent confidence interval calculated from standard errors that 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).

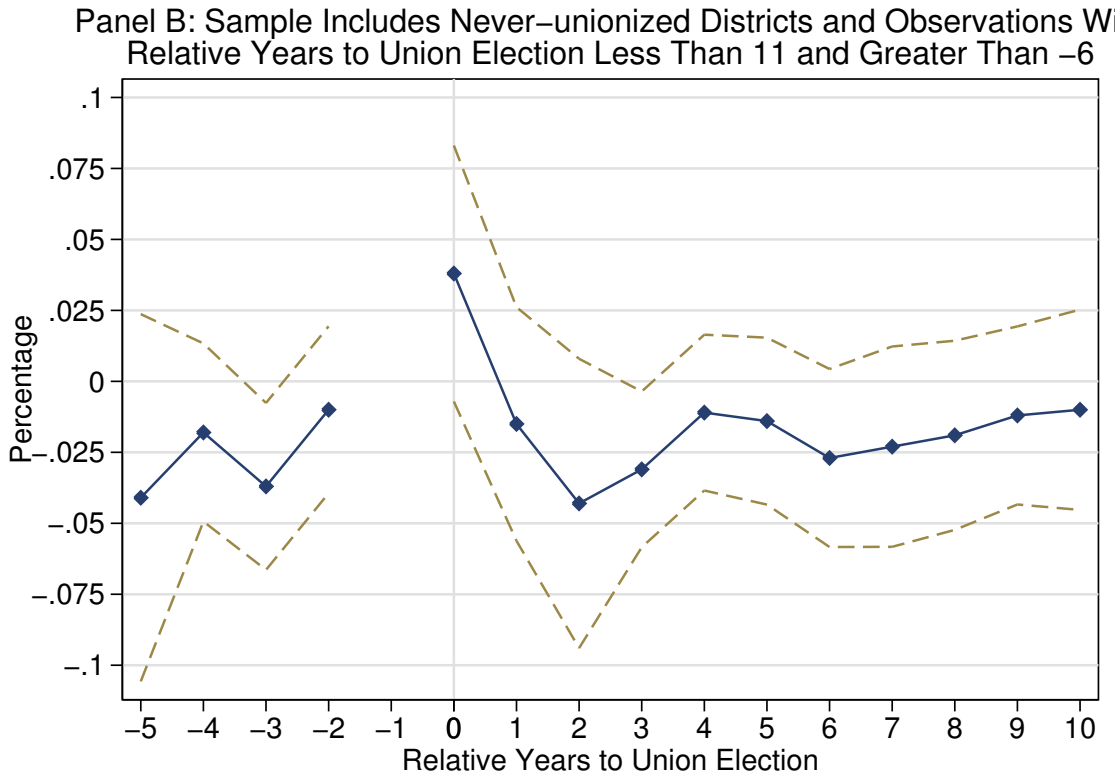
Figure 4: The Effect of Teachers' Unions on Log Student-Teacher Ratios



[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).

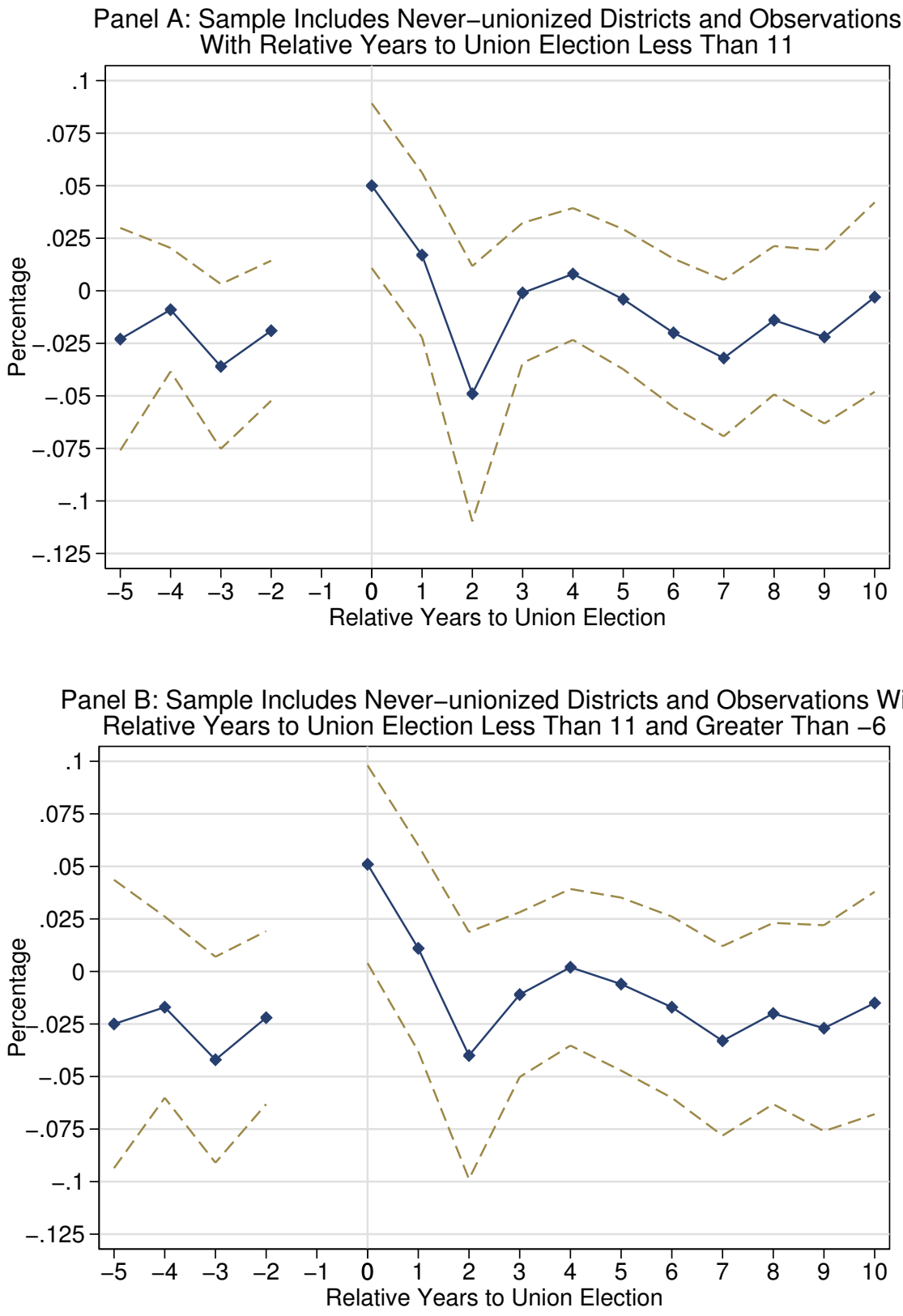
Figure 5: The Effect of Teachers' Unions on Log Real Current Operating Expenditure Per Student





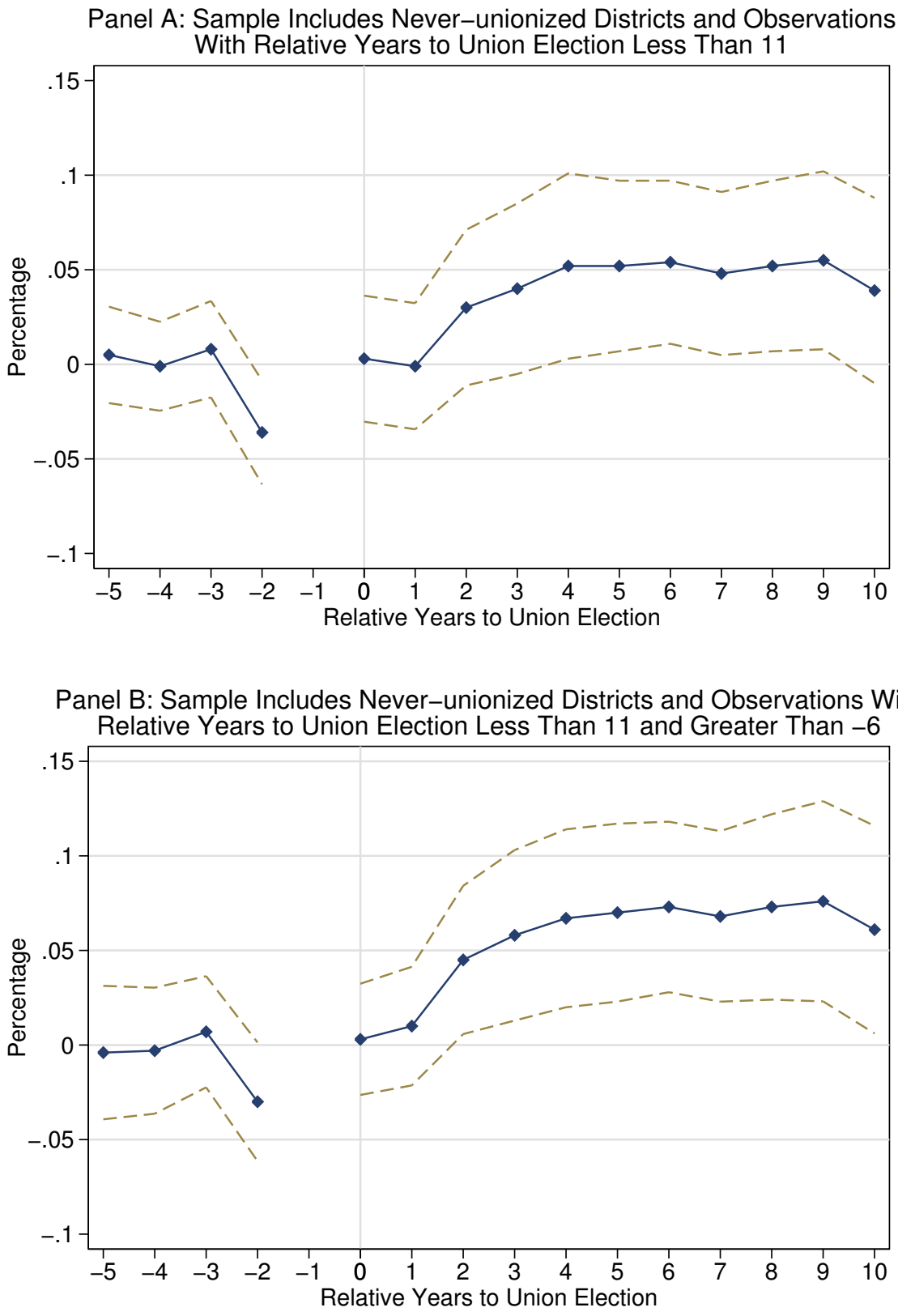
[1] Source: Author's calculations from the 1972–1991 Census/Survey of Governments as described in the text.
[2] Solid lines represent coefficient estimates from estimation of equation (2) in the text. Dotted lines are the bounds of the 95 percent confidence interval calculated from standard errors that 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).

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



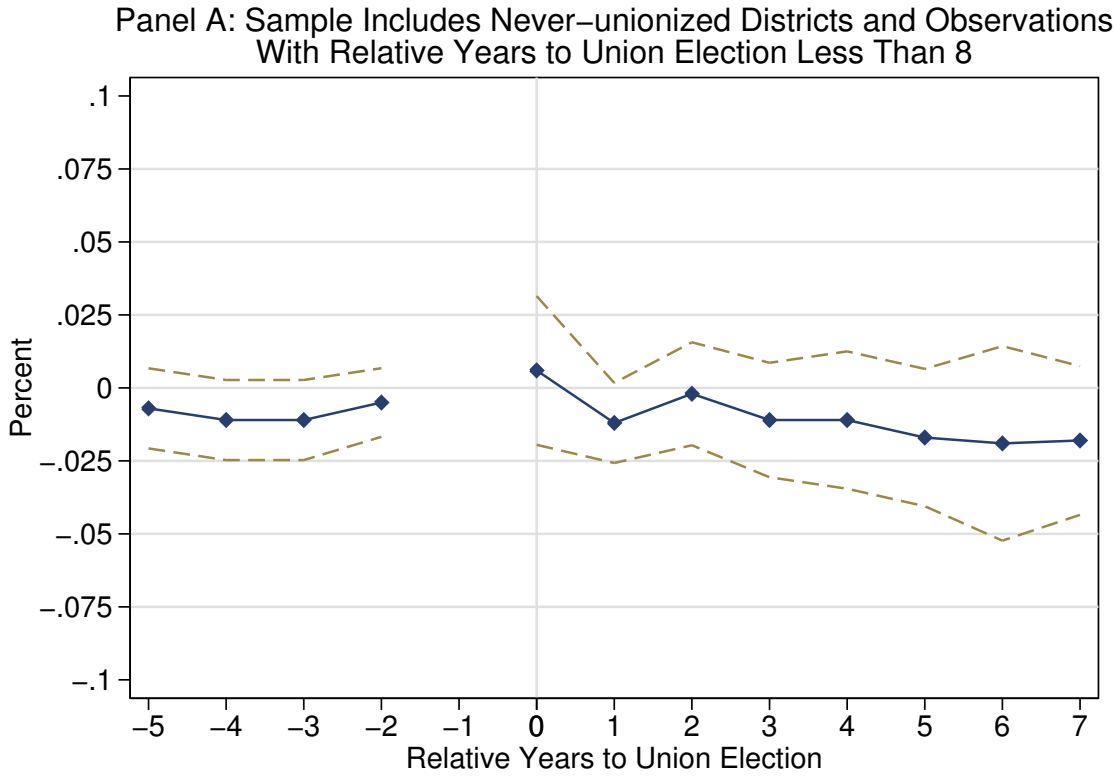
[1] Source: Author's calculations from the 1972–1991 Census/Survey of Governments as described in the text.
[2] Solid lines represent coefficient estimates from estimation of equation (2) in the text. Dotted lines are the bounds of the 95 percent confidence interval calculated from standard errors that 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).

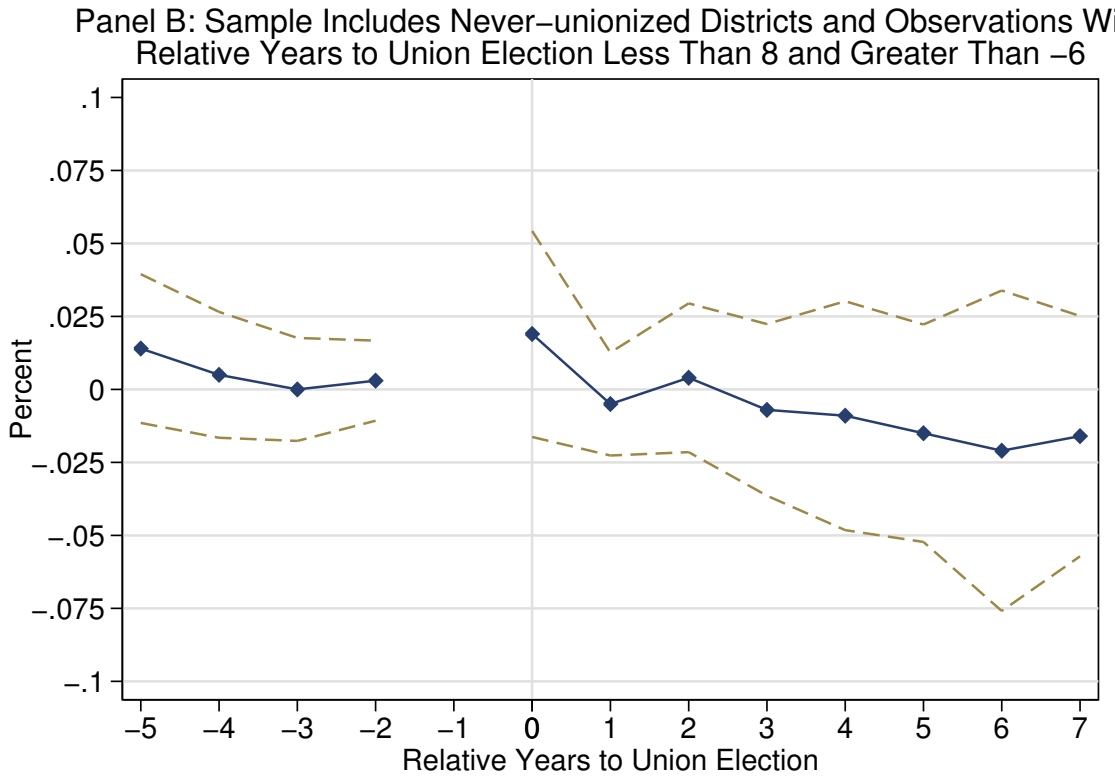
Figure 7: The Effect of Teachers' Unions on Log Student Enrollment



[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).

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).