EdWorkingPaper No. 20-316

Rent-Seeking through Collective Bargaining: Teachers Unions and Education Production

Jason Cook
University of Utah

Stéphane Lavertu
The Ohio State University

Corbin Miller
U.S. Treasury

We explore how teachers unions affect education production by comparing outcomes between districts allocating new tax revenue amidst collective bargaining negotiations and districts allocating tax revenue well before. Districts facing union pressure increase teacher salaries and benefits, spend down reserves, and experience no student achievement gains. Conversely, districts facing less pressure hire more teachers (instead of increasing compensation) and realize significant student achievement gains. We interpret these results as causal evidence of the negative impact of teacher rent seeking on education production, as the timing of district tax elections relative to collective bargaining appears to be as good as random.

VERSION: November 2020

Suggested citation: Cook, Jason B., Stéphane Lavertu, and Corbin Miller. (2020). Rent-Seeking through Collective Bargaining: Teachers Unions and Education Production. (EdWorkingPaper: 20-316). Retrieved from Annenberg Institute at Brown University: https://doi.org/10.26300/xwxt-jv66

Rent-Seeking through Collective Bargaining: Teachers Unions and Education Production*

Jason Cook†
David Eccles School of Business
University of Utah
jason.cook@eccles.utah.edu

Stéphane Lavertu†
John Glenn College of Public Affairs
The Ohio State University
lavertu.1@osu.edu

Corbin Miller
Office of Tax Analysis
U.S. Treasury
corbin.miller@treasury.gov

Abstract

We explore how teachers unions affect education production by comparing outcomes between districts allocating new tax revenue amidst collective bargaining negotiations and districts allocating tax revenue well before. Districts facing union pressure increase teacher salaries and benefits, spend down reserves, and experience no student achievement gains. Conversely, districts facing less pressure hire more teachers (instead of increasing compensation) and realize significant student achievement gains. We interpret these results as causal evidence of the negative impact of teacher rent seeking on education production, as the timing of district tax elections relative to collective bargaining appears to be as good as random.

Keywords: collective bargaining, teachers unions, student achievement, school finance
JEL Codes: H11, I21, I22, I26, J31, J45, J51, J52

*Note: The Spencer Foundation and the John Glenn College of Public Affairs provided generous funding that made this study possible. We thank Shaun Khurana and Coral Wonderly for research assistance. This draft has benefited from the insightful feedback of Aaron Churchill, Nathan Favero, Andrew Johnston, Vladimir Kogan, Terry Moe, Claudia Persico, Matthew Pesavento, and Alexander Willén, as well as seminar participants at the University of Missouri's Truman School of Public Affairs and at the O'Neil School of Public and Environmental Affairs at Indiana University-Purdue University.

†Please direct communications to the corresponding authors at jason.cook@eccles.utah.edu or lavertu.1@osu.edu.

1 Introduction

There is a growing consensus that increasing school district funding can lead to better education outcomes (Jackson, 2018). How teachers unions affect these returns to spending is unclear, however. Teachers may be committed to imparting knowledge and skills, but they also seek better salaries, benefits, and work conditions. Critics of teachers unions often cite such rent-seeking to argue that empowering teachers to bargain collectively for compensation undermines public education (Chubb and Moe, 1990; Moe, 2011) Whether this argument holds depends on teachers' and administrators' relative understanding of education production and the extent to which they prioritize student achievement (Hoxby, 1996). If teachers are more inclined to prioritize student learning or understand education production better than administrators, then teachers' influence through collective bargaining could increase efficiency (Retsinas, 1982).

Unfortunately, we lack conclusive empirical evidence on whether teacher rent-seeking impacts education production. Studies that leverage the enactment of duty-to-bargaining laws are limited because student achievement data are unavailable going that far back, whereas studies that estimate the more recent impact of collective bargaining on student achievement generally lack plausibly exogenous variation in union-district bargaining (Lovenheim and Willén, 2019). Even studies that largely overcome these limitations cannot speak to the efficiency of union-induced spending. For example, in their analysis of district responses to state finance reforms, Brunner et al. (2018) show that districts in states with strong unions spent more, directed more spending toward teacher salaries, and experienced greater student achievement gains than districts in states with weak unions. They cannot say, however, whether districts would have realized larger gains had they allocated money differently.

Our study addresses these limitations by estimating the contemporary impact of teacher collective bargaining on revenue allocation and student achievement, holding fixed all other district differences (e.g., revenue levels). Specifically, using data on thousands of tax referenda held across Ohio school districts from 1995 to 2019, we use a difference-in-regression-discontinuity design to estimate the impact of just passing a tax levy—as compared to just failing to pass a tax levy—

on collective-bargaining agreements (CBAs), resource allocation, and student achievement. In particular, we compare the effect of obtaining this new tax revenue just before a CBA is set to expire—in the midst of collective bargaining—to the effect of obtaining this tax revenue well before the next scheduled round of negotiations. Essentially, because districts largely commit revenue to operational functions in the summer immediately following a tax referendum, there are limited resources for unions to bargain over if collective bargaining is scheduled to take place at a time other than the summer immediately following the referendum. As we show below, the precise timing of local tax levies (relative to scheduled collective bargaining negotiations) is plausibly random. Thus, comparing the impact of tax elections held at different times relative to collective bargaining negotiations should reflect the causal effect of union pressure on resource allocation and student achievement.

The analysis of collective-bargaining agreements yields imprecise estimates but, as a whole, paints a coherent picture. Unions and districts agreed to higher teacher salaries if collective bargaining occurred while a district decided how to allocate new revenue, as opposed to well after a district allocated new revenue.[1] Similarly, collective bargaining agreements conferred more teacher benefits—such as dental coverage, extended meal times, and more sick and personal days—if districts secured and allocated new revenue in the midst of collective bargaining negotiations. On the other hand, tax passage was more likely to lead to changes in CBA text dealing with work conditions among districts that committed new revenue one year prior to collective bargaining. These results are consistent with research indicating that unions pursue higher salaries and benefits in times of abundance but settle for perks with few or no immediate financial implications when budgets are tight (see Retsinas, 1982; Strunk and Marianno, 2019) .

The analysis of school district spending and staffing corroborates the results of the CBA analysis. Districts in which tax levies generated funds in the year leading up to summer CBA negotiations spent more on teacher compensation, spent down their reserves to a greater extent,

[1]It appears salary increases are larger at the top of the pay scale, which is consistent with numerous studies that have found a positive relationship between unionization (or union strength) and collectively bargained wages (see Cowen and Strunk, 2015). In particular, it is consistent with Winters (2011) and Strunk and Grissom (2010), who find that salary increases tend to benefit relatively senior teachers.

and hired fewer teachers than districts that allocated new tax revenue well before negotiating new collective bargaining agreements. Specifically, although both sets of districts increased overall spending on instructor salaries, districts that allocated new revenue well before collective bargaining spent the money on 10-12 new teachers as opposed to salary increases. Moreover, districts subject to more union pressure in collective bargaining increased spending on teacher benefits to a greater extent (both in absolute terms and as a proportion of district revenues), experienced relative declines in their reserves, and were more likely to pass a new tax when the collective bargaining agreement expired.

Consistent with rent-seeking theory, additional revenue did not lead to student achievement gains among districts that allocated these new funds while in the midst of collective bargaining, but it did among districts that committed new revenue one year prior to collective bargaining. For these districts, relative spending increases of approximately $200 per pupil translated to an increase of annual achievement gains of 0.02 of a student-level standard deviation, for total accumulated gains of around 0.06 student-level standard deviations over the following three years (the typical duration of a collective bargaining agreement). That equates to cumulative gains of 0.2 district-level standard deviations by year 3. Put differently, districts that allocated funds relatively free of collective bargaining pressures were more efficient, realizing an extra 0.002 standard deviations in student-level achievement gains for every $1,000 in annual per pupil expenditures.

To our knowledge, this study provides the most direct test of the rent-seeking theory of Hoxby (1996) as it relates to collective bargaining over teacher compensation and its impact on student achievement, confirming more suggestive evidence from recent studies (e.g., Lott and Kenny, 2013; Marianno and Strunk, 2019; Moe, 2009; Shi and Singleton, 2020; Strunk, 2011). The study also illustrates a mechanism through which collective bargaining leads to greater educational spending, as districts subject to union pressure subsequently raised taxes further—ostensibly because they committed to unsustainable teacher compensation levels. Finally, the study contributes to a growing literature on school district collective bargaining with the novel coding and quasi-experimental analysis of decades of collective bargaining agreements, providing evidence that in the absence of available funds, unions and districts negotiate changes to work conditions instead of salaries and benefits.

The paper is organized as follows. First, we review some conceptual underpinnings of collective bargaining, particularly as it occurs in Ohio. Second, in the course of describing our data, we provide background on Ohio school district governance. Third, we present our empirical strategy and establish its validity. Fourth, we present the results and conclude with a brief discussion.

2 Conceptual Framework

As in thousands of school districts across the country, Ohio public-sector unions representing teachers (and other school employees) regularly bargain with district officials over wages, benefits, and rules governing the management of schools. Hoxby (1996) characterizes this as a bargaining game between teachers and parents, based on the assumption that administrators and parents share the goal of maximizing student achievement. The model implies that collective bargaining outcomes will lie somewhere between what is best for students and what is best for teachers. Additionally, if teachers and administrators are rational utility-maximizing actors with complete information, then one would expect negotiations to end immediately with an allocation of available funds that reflects the relative bargaining power of unions and districts (e.g., see Rubenstein, 1982).[2]

Such a bargaining model imperfectly characterizes bargaining even in a high-stakes, formalized, and professionalized process such as teacher collective bargaining, however. Although strikes are rare, extended collective-bargaining negotiations that last through the summer are common in Ohio. Incomplete information almost surely plays a role, as newspaper accounts of collective bargaining suggest that through the bargaining process, districts update their beliefs about

[2]For example, because Ohio is one of just a few states in which teachers are permitted to strike—an action that is very costly to parents and administrators (Jaume and Willén, 2019)—then the benefits teachers can secure through the bargaining process should be greater than they would be in other states.

union resolve and unions update their beliefs about the resolve and financial means of districts.[3] But differences between collective bargaining processes and the canonical bargaining model may be more extensive. Consistent with recent empirical research on bargaining processes and outcomes (see Backus et al., 2020), anecdotal evidence suggests that norms of fairness and mutual concession play a significant role in collective bargaining processes and outcomes. Alienating a district's teachers—its most important and expensive input—by refusing to compromise in a manner that is perceived as "fair" could significantly undermine school district effectiveness.[4] The empirical implication is that what teachers and district officials deem "fair" may not align with the relative bargaining power of districts and teachers unions.

Our study captures variation in relative bargaining power between teachers and districts by comparing situations in which districts do and do not have funds available due to the recent passage of a local tax referendum. Specifically, we assume that summer collective bargaining negotiations that occur immediately after the passage of a tax referendum involve more union influence over the allocation of funds than negotiations that occur over one year after the passage of a tax referendum—long after districts have committed these funds to difficult-to-change operational functions (e.g., hiring new faculty and staff). Unions of course are aware of windfalls regardless of how proximate they are to the collective bargaining process, and norms of fairness will likely compel districts to direct at least some of this new revenue toward union priorities during the collective bargaining process. But districts' superior information about their financial situations and their ability to commit funds such that they are unavailable when collective bargaining takes place should lessen the extent to which unions can direct the allocation of revenue contrary to the preferences of district officials when collective bargaining happens well after tax passage.

[3]Sometimes this learning occurs over the course of years. For example, in 2016 the Columbus City Schools's financial projections improved immediately after the district agreed to a collective bargaining agreement. Believing that the district had misrepresented its financial situation to avoid giving teachers a significant raise, during 2018 the new Columbus Education Association president dismissed district financial projections and threatened a strike if the district did not give teachers far larger raises than in 2016 (e.g., see Neese, 2019).

[4]For example, according to the Columbus Dispatch (August 25, 2019), after reaching a collective bargaining agreement in August 2019, Columbus City Schools Superintendent Talisa Dixon stated "Any time you have two parties in any kind of negotiations, by definition, there will always be some give and take...I believe this contract is a fair, strong one that respects teachers as professionals."

Researchers and political observers seem to agree that teachers unions most covet higher wages and benefits; that better work conditions—such as reductions in class size—are next on the list; and that, when such expensive options are off the table, they focus on perks such as personal leave, class preparation time, professional development, transfer rights, evaluation procedures, and other governance matters such as the number of faculty meetings a principal may convene (Moe, 2011; Retsinas, 1982; Strunk and Marianno, 2019). In principle, creating such a hierarchy is problematic, as a sufficiently large improvement in work conditions could be worth more to teachers than a salary increase. In practice, however, the feasible non-pecuniary options may be of lesser value to teachers than the salary increases we tend to observe. For example, Johnston (2019) finds that teachers are willing to forgo $320 in salary for a 10 percentage point reduction in the poverty rate of their students, but increasing teacher transfer rights would seldom allow for such a pronounced change in the population of students teachers educate.

The net cost to districts of these options vary significantly. For example, Johnston (2019) finds that teachers value the benefits of smaller class sizes far less than their costs to districts, but they value instructional aides more than their costs to districts.[5] Similarly, whereas teachers value future cash benefits significantly (Johnston, 2019), the immediate costs to districts are far more limited. Thus, districts may be most willing to increase salaries and the immediate share of benefits contributions when they have new revenue and unions have bargaining power, whereas support staff, workplace rules and perks, and changes to future benefits (e.g., the accrual of sick leave that teachers can cash in upon retirement) may be most desirable when budgets are tight and unions lack bargaining power.[6] Unless districts see smaller classes as desirable for meeting their own goals, however, that seems like an exceedingly expensive option for the purposes of meeting union demands.

Characterizing the preferences of district officials is more difficult.[7] Hoxby (1996) and

[5]In Ohio, where unions often represent other school employees such as instructional aides, nurses, counselors, and social workers, increasing non-instructional staff may be particularly desirable as a way of improving work conditions for teachers while pleasing other union members.

[6]Pensions are set by the state in Ohio, so only district contributions to teacher pensions are on the table in collective bargaining negotiations.

[7]District officials—namely, school boards and the top administrators they appoint, such as district superintendents

Moe (2011) assume that district officials pursue an allocation of revenue that will maximize student achievement. That assumption seems strong even in this era of test-based school district accountability. For example, teachers perceive (and research confirms) that administrators are inclined to game accountability systems at the expense of meeting the educational needs of students (Murnane and Papay, 2010). And for districts where accountability benchmarks are not binding, there is significant room for district officials to pursue noneducational goals (e.g., pleasing constituents by investing in a district's football team). Moreover, teachers' understanding of how to allocate revenue to improve educational functions may be superior to that of district officials—particularly due to the prominent role of non-expert school boards in education governance (Hess and Meeks, 2010; Howell, 2005; Retsinas, 1982). Thus, union rent-seeking through collective bargaining could in fact lead to better achievement returns to educational spending.

Overall, based on the literature (as well as anecdotal newspaper accounts), we would expect unions to be more successful in increasing salaries and benefits when collective bargaining occurs immediately after districts pass a tax referendum, as opposed to one year later. But, because unions observe tax passage regardless of the timing of collective bargaining—and perhaps because of norms of fairness that may guide collective-bargaining negotiations—we should expect unions to secure at least some compensation (financial or not) even when collective bargaining occurs well after tax passage. Whether district officials' preferred budget allocations differ significantly from those of unions—and whether these allocations have differential impacts on achievement—is an empirical question.

3 Ohio Context and Data

We use novel data on Ohio school district collective bargaining agreements and tax referenda combined with data on Ohio school district finances, demographics, and student achievement that are generally publicly available. We describe these data in turn and provide important context

and treasurers—often contract with professionals to advise them or negotiate on their behalf.

about Ohio school districts along the way.

3.1 Ohio Collective Bargaining Agreements

Like most states, Ohio has a duty-to-bargain law that requires school districts to negotiate with a union elected for the purpose of collective bargaining. As we show below, teachers in the vast majority of Ohio districts appointed or elected a union to represent them in negotiations and reached collective bargaining agreements during our period of study, as is the case in other collective bargaining states (Lovenheim, 2009).[8] These agreements deal with virtually all aspects of school district management that involve teachers, including salary schedules based on teacher credentials and experience; benefits such as insurance coverage and personal leave (pension benefits are set by the state); work conditions such as class sizes, evaluation procedures, preparation time, and meal time; job protections related to transfer, separation, and grievance procedures; and collective-bargaining procedures and union rights (e.g., with respect to strikes) that affect the bargaining power of teachers unions. The majority of districts negotiate new agreements in the summer every three years (see Table B1 in Appendix B) and the negotiations of these agreements are generally staggered, so that about one third of Ohio's 610 school districts negotiate agreements each year.[9]

Table 1 provides a snapshot of agreements for unique districts with CBA start dates between 2004 and 2006, which is at the very beginning of the period we analyze. The table reveals that we have CBA data for up to 548 of the 611 Ohio school districts in operation at the time and that these agreements are typically scheduled to last three years. These documents are long on average (19,601 words), but there is significant variation in length across districts. Comparisons of the text between expiring and new district CBAs suggests that most of this text is stable over time. Specifically, in spite of errors in our text extraction that might make CBAs appear dissimilar

[8]Specifically, we found a peak of 548 districts negotiated a new agreement at some time between 2004-06. The National Center for Education Statistic's Schools and Staffing Survey suggests that only about 76 percent of Ohio districts (about 464) had agreements in 2007-08.

[9]Figure B1 in Appendix B illustrates the pattern. Interestingly, the pattern is disrupted in 2009, as fewer districts negotiate agreements that year (around 150) and more districts (around 275) start new agreements in 2011. As we detail below, we limit the analysis to districts that do not deviate from pre-determined collective bargaining schedules to avoid this apparent recession-induced endogeneity in the timing of collective bargaining.

when they are not, the average Jaro-Winkler dissimilartiy score is 0.19 with a standard deviation of 0.03. That indicates that roughly 19 percent of text characters in a CBA differ from those of the prior CBA in that district.[10] Finally, Table 1 provides some insights into CBA content. It indicates a minimum salary of $24,897 for entry-level positions in one district to a high of $101,695 for top-level positions in another (all in 2012 dollars). Whereas all CBAs provide health insurance, around 50 percent provide prescription and optical coverage.

Our analysis of CBA text in Appendix A indicates that CBAs are quickly increasing in length (from an average of around 17,000 words in 1999 to over 24,000 in 2018) while text is increasing in stability from year to year.[11] Variation in length and content is significant across districts, however. As we show in Table 1, there appears to be significant variation across districts in terms of salaries and certain teacher benefits (e.g., prescriptions, optical, attendance bonuses, retirement incentives, parking, and tuition reimbursement). These findings are consistent with those from other states in that CBAs are quite stable over time—gradually becoming more restrictive—but that there is significant variation across districts in their restrictiveness (Strunk et al., 2018).

3.2 Ohio School District Tax Referenda

Ohio school finance is typical in that, on average, school districts get around 40 percent of their revenue from local sources—primarily property taxes.[12] In order to raise local taxes above a state-set millage limit, districts' elected school boards must first get approval from voters who reside within district boundaries by placing an issue on the ballot. School boards typically choose to hold referenda during November elections (about 50 percent of the time) and May primaries (about 30 percent of the time), but they sometimes hold them during special elections in February or August,

[10] This interpretation is not entirely correct, of course, as the Jaro-Winkler calculation entails averaging proportions of matching characters for both the prior and current CBA, as well as adjusting for characters that are not in the same position in both documents (see Winkler, 1990, 2006).

[11] There is a period after the Great Recession that is associated with major changes to collective bargaining agreements, but the general trend is toward more stability.

[12] Some districts also levy local income taxes, but even in these districts property tax revenues are far larger.

Table 1: Features of collective-bargaining agreements (CBAs) from 2004-06

	CBA/District Count	Mean	Standard Deviation	Min.	Max.
Scheduled Duration (years) (end date minus start date)	548	2.8	0.8	1.0	7.9
Length (word count)	533	19,601	7,556	2,783	64,827
Change in text from prior CBA (Jaro-Winkler dissimilarity score)	532	0.19	0.03	0.14	0.44
Teacher Salary Schedule (2012$)					
Entry-level, with bachelor's	548	36,347	3,802	24,897	50,204
Top-level, with master's	548	67,936	9,887	26,489	101,695
Teacher Benefits (yes/no)					
Prescriptions	548	0.53	0.50	0	1
Dental	548	0.85	0.36	0	1
Optical	548	0.48	0.5	0	1
Attendance bonus	548	0.41	0.49	0	1
Tuition reimbursement	496	0.77	0.42	0	1
Retirement incentives	496	0.44	.50	0	1
Parking	496	0.31	0.46	0	1
Teacher Benefits (days of leave)					
Personal days	491	3.03	0.49	1	6
Sick days	495	15.02	0.23	15	18
Bereavement days	471	3.86	1.95	1	23
Max. accumulation of days	397	254.14	46.53	27	450
Work Conditions					
Time for meals	327	30.349	2.465	25	60
Bargaining					
Arbitration	476	1	0	1	1
Union leave	548	0.63	0.48	0	1

Notes: The table provides descriptive statistics on CBAs that took effect in 2004-06, for up to 548 unique school districts. Word counts and Jaro-Winkler dissimilarity scores (which capture differences in a CBA's text compared to the text of the prior CBA in that district) are based on analyses of text for CBAs we were able to obtain. We constructed all other variables using data from the Ohio State Employment Relations Board (SERB).

or in March primaries (see Table C1 in Appendix C).[13]

Districts may request approval to issue bonds for capital expenditures—which typically feature tax levies to pay off the debt over many years (typically over 20 years)—or they may request a tax levy to fund district operations. Over 80 percent of referenda between 1995 and 2019 were tax (non-bond) referenda and there were over 6,000 such elections (250 per year) across Ohio's approximately 610 districts.[14] These referenda occur frequently because, typically, tax levies are short-term (over 60 percent expire within 5 years) and they are for a set dollar value that does not increase with inflation or when property values increase. School districts also often have multiple tax levies in effect with staggered expiration dates. Together, these institutional features lead most Ohio districts to put tax referenda on the ballot frequently to maintain revenue growth.[15]

Table 2 provides descriptive statistics for the samples of referenda we use in the analysis below—including the "full sample" of all elections and a "restricted" sample of elections in which the vote in favor of passage is within 15 percentage points of the 50 percent threshold for passage. We limit the analysis and descriptive statistics to referenda we could link to district CBAs that had start dates within four years of the election, as that is typically the maximum scheduled length of a collective-bargaining agreement.[16] As we discuss below, we further limit the analysis to samples of referenda with an upcoming CBA start date at the end of the school year in which the election is held ("close CBA") and those for which the next CBA will start after the following school year ("distant CBA"). Finally, as we also discuss below, we limit the sample to districts on a 3- or 4-year CBA cycle that occurs during summers and is on schedule (e.g., the CBA that starts after the election takes effect exactly three years after the scheduled start date of the prior CBA) and

[13]Primaries are typically in May. March primaries are in presidential election years. School districts' elected school boards may put an issue on the ballot up to three times each year, provided that two-thirds of board members agree.

[14]This count is likely low because our dataset is based on election results from the Secretary of State that may be missing some elections. For example, the Ohio School Boards Association has a database for more recent years that suggests we may be missing 20-30 elections in some years.

[15]According to state law, districts can only place a tax levy on the ballot if they anticipate falling short of funding district operations in the future. However, districts can project expenses in a way that enables them to make such a claim. Thus, the requirement that districts need funds to avoid an operation shortfall likely has little impact on the frequency with which districts place referenda on the ballot.

[16]Allowing a greater temporal distance between a referendum and a CBA increases the odds that we are missing data for the most proximate CBA, which would introduce significant measurement error in our analysis.

Table 2: Descriptive statistics for Ohio tax and bond referenda tied to CBAs (2003-2019 elections)

	Ref. Count	Percent Passed	Mean Pct Yes Vote	Mean Vote Count	Percent 3-yr CBAs	Mean days to CBA
Full Sample	1,484	62.33	53.22	5,621	90.70	368
Close CBA	736	64.13	53.42	5,702	93.07	182
Distant CBA	748	60.56	53.03	5,541	88.37	551
Restricted Sample	1,234	59.64	51.87	5,967	90.11	374
Close CBA	611	62.52	52.42	6,167	92.47	187
Distant CBA	623	56.82	51.33	5,771	87.80	557

Notes: The table provides descriptive statistics for referenda used in the estimation of the impact of tax levy passage on collective-bargaining, budget allocations, staffing, and student achievement. The "full sample" includes all tax and bond referenda. The "restricted" sample includes all tax and bond referenda for which the vote in favor of passage was within 15 percentage points. The "close CBA" subsample includes referenda held in the year leading up to the next collective bargaining agreement, whereas the "distant CBA" includes referenda held more than one year prior to the next collective bargaining agreement.

to elections held between school years 2003-04 and 2018-19, for which we can observe student achievement and other outcomes at least three years prior to the election (which is important for examining changes in CBA content). This time span has the added benefit of capturing the No Child Left Behind accountability era, during which districts have incentives to focus on student achievement in mathematics and reading.

Table 2 reveals that, regardless of the sample, the average vote in favor of passage is just above 50 percent, the average number of votes cast approaches 6,000, and the proportion of referenda that pass hovers around 60 percent. These statistics are consistent with a Romer and Rosenthal (1979) bargaining model in which a district (the agenda setter) asks voters for just enough to pass a referendum.[17] Recognizing that, by random chance, they may not pass a referendum in any given election, districts have indicated that their strategy involves putting tax proposals on the ballot

[17]Because the reversion tax rate is endogenous—the result of prior referenda, particularly in districts with prior approval of bonds or permanent levies, or staggered tax levies—the dynamics are better captured by the Barseghyan and Coate (2014) model. But, for our purposes, the implications are similar.

repeatedly until they pass. As we discuss below, this feature is likely to contribute to the randomness of referendum timing from year to year. That the various samples yield similar descriptive statistics—particularly those limited to referenda with "close" and "distant" CBAs—is consistent with this notion of as-if random election timing.

3.3 Ohio School District Staffing, Finance, and Achievement

District finance, staffing, and student data are from the National Center for Education Statistics's (NCES's) Common Core of Data (CCD) repository. As Table 3 reveals, our preferred sample features 425 unique districts with average enrollments of approximately 2,700 students and 160 full-time-equivalent (FTE) teachers. Average operational spending in constant 2012 dollars is approximately $9,500 per pupil and capital spending is approximately $1,200 per pupil. About one-third of students qualify for free or reduced-price lunches, 5 percent are Black, and 2 percent are Hispanic. There are quite a few more unique districts with passing referenda than failing referenda, which is consistent with districts placing their tax levies on the ballot repeatedly until they pass. Finally, once again, descriptive statistics for the "close CBA" and "distant CBA" samples are very similar.

District-level student achievement data are from the Ohio Department of Education. We employ two different measures. Our primary measure is a "performance index" that the state has used continuously in its district accountability system from 2001 through 2019. The index runs from 0 to 120 and is based on point assignments for the proportion of students who reach various proficiency thresholds (e.g., below basic, basic, proficient, and advanced) on all subject tests (mathematics, reading, social studies, and science) across all tested grades. The exact calculation of this index varies over time as the state changed the tests that are included and the cutoffs for various proficiency levels. We standardized this measure by year, so that it captures district-level achievement differences for each year. The last column of Table 3 reveals that student achievement is similar between districts with "close" and "distant" referenda, but that districts that are able to

Table 3: Characteristics of districts that held referenda

	Unique District Count	Oper. Expnd. (2012$)	Cap. Outlay (2012$)	Stdnt Count	Pct. FRL Stdnts	Pct. Hisp. Stdnts	Pct. Black Stdnts	Teach. FTE	Achiev. (Dist. SDs)
Full Sample	425	9,551	1,188	2,711	30.60	2.13	5.22	161	0.11
Close CBA	338	9,566	1,193	2,757	30.90	2.20	4.95	164	0.13
Distant CBA	337	9,537	1,183	2,666	30.30	2.07	5.50	158	0.10
Passing refs.	386	9,639	1,115	2,761	30.71	2.45	5.56	165	0.18
Failing refs.	251	9,406	1,309	2,628	30.42	1.61	4.67	156	-0.01
Restricted Sample	387	9,533	1,230	2,796	30.40	2.09	5.28	167	0.08
Close CBA	293	9,571	1,226	2,859	30.60	2.17	5.10	171	0.11
Distant CBA	301	9,497	1,235	2,735	30.22	2.01	5.46	163	0.06
Passing refs.	345	9,613	1,191	2,878	30.70	2.42	5.80	172	0.13
Failing refs.	236	9,416	1,288	2,675	29.97	1.60	4.52	159	0.02

Notes: The table provides descriptive statistics for districts that held referenda. The “full sample” includes all tax and bond referenda. The “restricted” sample includes all tax and bond referenda for which the vote in favor of passage was within 15 percentage points. The “close CBA” subsample includes referenda held in the year leading up to the next collective bargaining agreement, whereas the “distant CBA” includes referenda held more than one year prior to the next collective bargaining agreement.

pass referenda have substantially higher achievement levels. That is why the analysis below must employ a regression discontinuity design–to estimated impacts for districts nearly identical in their likelihood of generating additional revenue.

One limitation of the standardized performance index is that it captures district-level standard deviations in achievement, whereas common achievement benchmarks in the economics of education literature are based on student-level standard deviations. To supplement our analysis of the performance index, in the analysis below we also use Ohio's district "value added" estimates as a measure of student achievement.[18] Specifically, we use estimates of annual student gains in mathematics and reading in grades 4-8, which the SAS Institute has calculated since 2006 based on student-level test scores standardized by subject, grade, and year.[19] They estimate annual, student-level gains based on mathematics and reading achievement because Ohio has administered these subject tests annually in grades 3-8 since 2007.[20] Although these estimates are available for fewer years, they capture within-student changes in achievement.[21] To generate value-added estimates that are comparable to benchmarks common in the literature, we convert average student-level gains from Normal Curve Equivalent (NCE) units to standard deviations by dividing gain estimates by the NCE standard deviation (21.063).

Finally, in some additional analyses below we use data that we formally requested from

[18]It is important for researchers to use caution when compiling these estimates, as the Department sometimes reports a gain "index" that essentially captures a t-statistic indicating whether a district's performance is less than or greater than baseline expectations. Confusingly, they sometimes label the index as a "gain score," which is a term they use in some years to distinguish the index from the actual gain estimate. Moreover, in some years the "composite" gain "score" reflects the index, whereas the by-grade gain scores in mathematics and reading reflect the actual value-added estimates of interest to us. To avoid these problems, we focus on by-grade reading and math gain scores, which we average at the district level.

[19]The method of standardization depends on the year and, thus, the comparisons it captures depend on the year. In some periods, SAS estimated student-level gains based on a baseline student cohort's distribution of test scores. In years in which there were significant changes in the tests the state administered, test scores were standardized each year, so that student year-to-year gains are based on students' changing position in the statewide distribution of scores from year to year.

[20]Estimates for 2006 are based on a single grade.

[21]We are unable to use precision weights in the estimation of our models below because the standard errors of annual gain estimates are unavailable for some years. Although ODE reports standard errors with all publicly available estimates, in some years ODE reports three-year averages of annual gains. We backed out one-year gain estimates based on these averages and one-year estimates from adjacent years, but we could not back out standard errors for these estimates. We do show, however, that our results are a bit more pronounced if we weight by district enrollment counts, which are highly correlated with the standard errors of value-added estimates.

the Ohio Department of Education. First, we obtained restricted-use data that enables us to track all Ohio teachers over time (1995-2019). These data include teacher pay, credentials, and building assignments. Second, we obtained school districts' five-year budget forecasts (2008-2019), which they submit to the Department in May so that the state can monitor their financial health. We describe the variables we created based on these data when discussing the results.

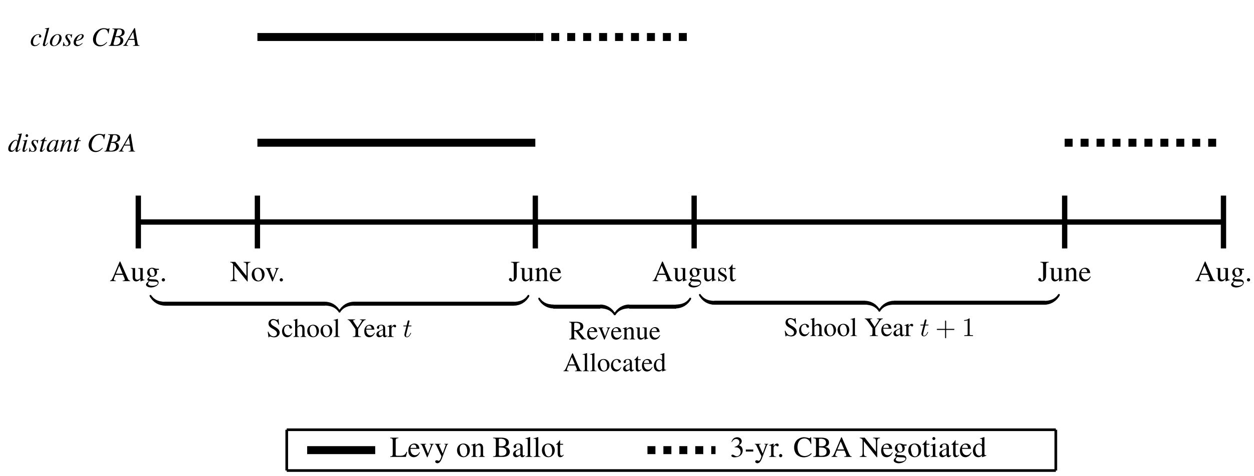
4 Empirical Strategy

We use a difference-in-regression-discontinuity design to estimate the impact of just passing a tax levy—as compared to just failing to pass a tax levy—on district collective-bargaining agreements (CBAs), budgeting and staffing, and student achievement. We compare these estimated impacts between tax levies that generated revenue just before a CBA is set to expire—in the midst of collective-bargaining negotiations—and tax levies that generated revenue well before the next scheduled round of negotiations. Because the timing of referenda is plausibly random (more on that below), these comparisons should reflect the causal effect of union bargaining power.

Specifically, as Figure 1 illustrates, we compare the impact of levy passage during the school year (elections held from November to May) for districts with CBAs expiring immediately after the school year (between June and August of the upcoming summer) and those with CBAs expiring the following summer (over one year later). As we note above, we refer to these two samples of referenda as those with "close" CBAs and those with "distant" CBAs, respectively. Unions should have more influence over revenue allocation during summer negotiations immediately after tax passage because districts likely allocate new revenue during that summer. Unions that negotiate over the following summer (over one year later), however, will be bargaining well after new revenue has been committed to district functions—typically by hiring teachers and support staff. As we discuss below, the analysis provides evidence consistent with these claims.

Importantly, the timing of referenda should essentially be random. By chance, districts near the passage threshold will fail to pass a referendum, and these districts are likely to keep

Figure 1: CBAs negotiated in summers every three or four years



putting a measure on the ballot until it passes.[22] Additionally, as we discuss above, Ohio districts must frequently put levies on the ballot to keep up with rising costs, in part because a large fraction of referenda expire after five years—a pre-determined schedule that cannot consistently align with the expiration of three-year collective bargaining agreements. These institutional features should make the timing of referenda essentially random across districts and over time within districts, and our empirical tests below provide evidence consistent with this notion. Thus, our design appears to provide quasi-random variation in both the probability of tax passage and the probability that a referendum falls into the "close CBA" or "distant CBA" subsample.

We limit our preferred analytic sample in a number of ways to ensure that we are comparing the impact of tax levy passage between districts that differ primarily in the extent to which they face collective-bargaining pressures while deciding how to spend new revenue. First, we limit the analysis to tax elections held in districts under three- or four-year CBAs that are set to expire after the current school year or after the subsequent school year. We omit tax elections held in districts under a CBA with a scheduled duration of two years or less, as the tax levy may have been put on the ballot explicitly as part of a collective-bargaining process. For example, if negotiations

[22]As we show below, districts in which referenda just fail to pass put referenda on the ballot in subsequent years, until they pass. Our conversation with district administrators suggest that this is often an explicit strategy, as districts ask for just enough to get around 50 percent support and recognize that they need not alter the content of the proposal if they are sufficiently close to the passage threshold.

prior to the election involved a stop-gap, one-year CBA that districts would revise to meet teacher demands should they succeed in passing a tax levy during the school year, then negotiations have largely occurred prior to the upcoming summer. Similarly, because we limit our sample to CBAs with a scheduled length of three or four years, focusing on districts where CBAs expire within two years leads us to omit cases in which current CBAs were negotiated in the summer just prior to a tax election. Finally, we omit tax elections held in August—the only election date that does not occur during the school year—because bargaining and budgeting may take place concurrently. These restrictions enable us to cleanly capture the comparisons depicted in Figure 1.

4.1 Statistical Models

We use a regression discontinuity design to estimate the impact of tax levies on teacher collective bargaining agreements, school district budgeting, and student achievement. For district outcomes we observe yearly (budgets, staffing, and student achievement) we implement the regression discontinuity design using primarily the following Ordinary Least Squares (OLS) model:

$$\begin{aligned}Y_{i(t-t^*)} = \tau_1 * Distant_i * Pass_i + \tau_2 * Pass_i + \tau_3 * Distant_i \\ + f(Vote_i) + Pass_i * f(Vote_i) \\ + Distant_i * f(Vote_i) + Distant_i * Pass_i * f(Vote_i) \\ + \beta_1 Y_{i(t^*-1)} + \beta_2 Y_{i(t^*-5)} + \mathbf{X}_i'\gamma + \lambda_t + \epsilon_{it}\end{aligned} \tag{1}$$

The outcome Y for tax election i in school year t relative to the election year t^* is a function of the following: the variable $Pass_i$, indicating whether (1) or not (0) a tax levy passed; the variable $Distant_i$, indicating whether (1) or not (0) the tax referendum occurred over one year prior to a new collective bargaining agreement (i.e., revenue allocation occurred relatively free of union pressure); a linear function of the percentage of the vote in favor of passage centered at the 50 percent vote threshold for passage ($Vote_i$); lags of the outcome one year prior to the election ($Y_{i(t^*-1)}$) and five years prior to the election ($Y_{i(t^*-5)}$); covariates capturing student counts, the proportion of students who qualify for free or reduced-price lunch, the proportion of students who are Black, total expenditures per pupil, and enty-level and top-level collectively bargained salaries in the year before the election ($\mathbf{X}_i$); school year fixed effects (λ_t); and some error.[23] Our parameter of interest is τ_1, which captures the difference in the impact of tax levy passage between districts with "distant" as opposed to "close" CBAs. We include baseline covariates, lagged outcomes, and year fixed effects to increase precision. As we show below, their inclusion does not affect the estimates in substantively significant ways. We also cluster standard errors by district, as the election-level observations are stacked and, thus, include multiple elections from the same districts held at different times.

For the analysis of CBA provisions, which we observe every three or four years, we estimate a model in which the outcome captures the difference between the CBA with a post-election start date and the prior CBA with a pre-election start date:

$$\begin{aligned}\Delta Y_{it} = \tau_1 * Distant_i * Pass_i + \tau_2 * Pass_i + \tau_3 * Distant_i \\ + f(Vote_i) + Pass_i * f(Vote_i) \\ + Distant_i * f(Vote_i) + Distant_i * Pass_i * f(Vote_i) \\ + \mathbf{X}_i'\gamma + \lambda_t + \epsilon_{it}\end{aligned} \tag{2}$$

In most models, we calculate the change in the outcome (e.g., teacher salary) using a simple difference between the value associated with the post-election CBA and the value associated with the pre-election CBA ($Y_{it} - Y_{i(t^*-1)}$). As we describe above, the change in CBA provisions is also sometimes captured by a Jaro-Winkler (JW) "distance" calculation capturing the overall dissimilarity between the post- and pre-election CBAs, as well as the dissimilarity of sections dealing with issues such as benefits, worker protections, and work conditions. Once again, we cluster the errors by district and the inclusion of baseline covariates and year fixed effects does not affect the substantive interpretation of the results.

[23] We include a three-year lag of the achievement variables (instead of a five-year lag) as this variable is available dating back to 2001 only.

The models feature a linear specification of $Vote$ and use a sample of referenda within 10 or 20 percentage points of the 50 percent vote threshold necessary for passage—or 15 percentage points if we report a single estimate in the interest of space (e.g., we use the average of the two bandwidths when presenting figures or conducting additional calculations in the appendix). The 10 percentage point bandwdith is around the mean of the bandwidths generated by the Calonico et al. (2014) mean-squared-error (MSE) procedure, although this procedure does not take into account model fixed effects. We also report estimates for models using the wider, 20-point bandwidth in order to examine the sensitivity of the estimates.

Finally, it is important to note that we are estimating an "intent to treat" effect of levy passage. We know that districts in which levies fail are 25 percentage points more likely to pass a levy in the following school year than districts in which a levy passed (see Table E3 in Appendix E). We are not scaling the estimates by the probability of passage to get a "treatment on the treated" effect (as per Cellini et al., 2010) because we are interested in the impact of immediate budgeting decisions—those made in the summer just after an election. We merely cluster standard errors by district to address the fact that the same district is likely to have multiple observations in our samples, which may involve both passing and failing referenda as well as referenda that have close and distant CBAs.

4.2 Validity of research design

Our design entails comparing the impact of tax levy passage between referenda held in the school year leading up to summer CBA negotiations (referenda with close CBAs) and referenda held over one year prior to summer CBA negotiations (referenda with distant CBAs). As is common in the literature, we test the validity of the regression discontinuity design for estimating the impact of levy passage by testing for discontinuities in the density of the running variable (the centered vote share) and testing for discontinuities in the levels and trends of pre-election district covariates. McCrary (2008) density tests reveal no discontinuities for the full sample of tax and bond referenda, the sample of tax referenda, or the close and distant subsamples (see Figure D1 in Appendix D).

Balance tests for pre-election variables—as well as for trends in those variables from five years before the election to one year before the election—also generally fail to detect significant differences between districts on either side of the threshold. As Table D1 in Appendix D illustrates, there are no significant differences in the pre-election characteristics of districts (in terms of levels or five year trends) that would go on to pass or fail a referendum—nor is there a difference in these differences between districts with "distant" or "close" CBAs at the time of the election. Similarly, Table D2 in Appendix D reveals no such differences in these districts' prior collective bargaining agreements, the timing of past agreements relative to the election, and the timing of future agreements relative to the election. Finally, Table D3 reveals that there are no statistically significant differences in the characteristics of districts with "distant" as opposed to "close" referenda. Joint significance tests using seemingly unrelated regression methods return p values in excess of 0.2 across all models.

Thus, near the threshold for referendum passage, it appears that both the passage of tax levies and the timing of CBA negotiations are as good as random. The final requirements for identifying the causal impact of collective bargaining are that 1) districts largely commit revenues in the summer after the election and 2) collective bargaining has a greater impact on resource allocation if a CBA expires in the summer after a tax levy passes as opposed to two summers after levy passage. The analysis below provides evidence consistent with these stipulations.

5 Results

We begin by documenting the impact of tax levy passage on district revenues and collective-bargaining agreements, paying particular attention to differences in effects between referenda with CBA start dates in the summer after the current school year (the "close CBA" sample) and referenda with CBA start dates in the following summer (the "distant CBA" sample). We then turn to estimates of the impact of tax levy passage and CBA timing on district budgets, staffing, and achievement outcomes.

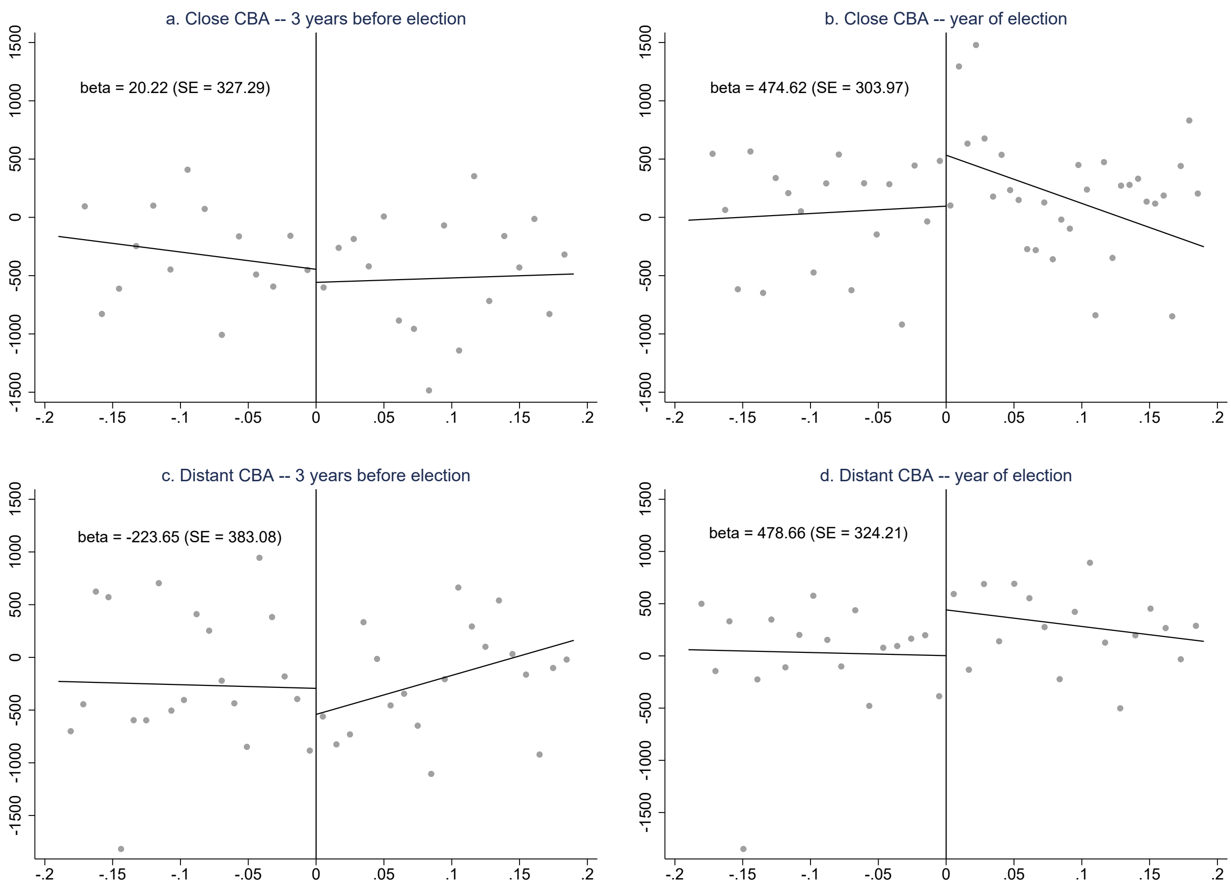
5.1 Timing of New Revenue

As we discuss above, the research design requires that districts obtain revenue in the year of the election, so that these revenues are allocated during the budgeting process over the upcoming summer—which will either involve collective bargaining ("close CBA") or not ("distant CBA"). Figure 2 presents the impact of tax passage on revenue three years prior to the election and by the end of the election year. Specifically, the y axis captures the difference in revenue between the indicated year (the year of the election or three years prior) and revenue in the year prior to the election. The x axis is the vote share in favor of passage centered at the 50 percent vote threshold. The estimates are noisy, as these estimates are from models that include no covariates except baseline revenue in the prior year to the election—but they confirm that the timing of revenue is indeed as the design requires. Specifically, the figures reveal no statistically significant differences in revenue three years prior to the election but, by the end of the election year, tax passage leads to a revenue advantage of $475 in the close-CBA sample and $479 in the distant-CBA sample. Thus, the important takeaway for now is that the revenue effects of tax passage are immediate, such that districts can allocate funds in the summer following the election held during the school year.

5.2 Impact of Revenue-CBA Timing on CBA Provisions

Table 4 presents the results of models estimating the impact of levy passage on CBA text, salary schedules, and benefits based on the specification in equation 2. The coefficient for $Distant*Pass$ captures the difference in the impact of levy passage between districts with a "distant CBA" (those we argue are subject to relatively less union pressure) and those with a "close CBA" (those we argue are subject to relatively more union pressure). The coefficient for $Pass$ is the impact of passing a tax levy in districts with a close CBA. To increase precision, some of the models include data from elections held from 1995 to 2003, prior to our primary sample spanning 2004-2019 (for which we observe all outcomes). As in all results tables below, we present estimates based on samples of referenda with vote shares within 10 or 20 percentage points of the 50 percent vote threshold needed for referendum passage. As we discuss above, a bandwidth of 10 percentage points is in

Figure 2: Impact of Tax Passage on Changes in Revenue Per Pupil



Note: The figure compares changes in revenues per pupil between districts in which tax referenda passed (vote margin >0) and those in which referenda failed (vote margin <0) as of three years before the election (figures a and c) and through three years after the election (the average across post-election years 1-3, presented in figures b and d). Specifically, the y axes capture the difference in revenues per pupil between each of these years (three years prior and the year of the election) and revenues in the year before the election. The x axes capture the centered vote variable—the difference between the fraction of votes in favor of passage and the 0.5 threshold needed for passage. The dots are local means and the regression lines are fitted using all referenda types and years for which we observe student achievement. We present the results separately for districts with close CBAs (figures a and b) and those with distant CBAs (figures c and d).

the neighborhood of the MSE-optimal bandwidth across the various models. We include estimates using a bandwidth of 20 percentage points primarily as a sensitivity check.

The top portion of Table 4 provides results of models estimating the extent to which the text of collective bargaining agreements changed after a tax election. The dependent variables are Jaro-Winkler dissimilarity scores that, roughly, indicate the fraction of CBA text that changed.[24] The results in the second column indicate that the overall CBA dissimilarity score increased by an average of 0.013—almost half of the district-level standard deviation we report in Table 1—when a district with a close CBA passed a tax referendum. Crudely, one might state that about 1 percent more text changed if a tax levy passed than if it failed. The results in the first column indicate, however, that there was no such effect in districts with distant CBAs. The difference in effects between districts with distant instead of close CBAs is -0.018. That is, districts with distant CBAs (those we argue are subject to relatively less union pressure during collective bargaining) changed approximately 2 percent less text than districts with close CBAs (those we argue were subject to more union pressure). On the other hand, about 4 percent more text changed in CBA sections dealing with work conditions—such as class sizes, evaluation procedures, and preparation time—for districts with distant CBAs. The estimates decline in magnitude and fail to attain conventional levels of statistical significance when we use a bandwidth of 20 percentage points.

The next section examines changes to salary schedules—particularly pay for entry-level teachers with a bachelor's degree and pay for the most experienced teachers with a master's degree. The estimates are very imprecise—only those based on the larger 1995-2019 sample and the 20 point bandwidth reach conventional levels of statistical significance—but they consistently indicate that tax passage in districts with close CBAs led to a relative increase in salary of $1,000 at the top of the pay scale. But there is no such impact in districts with distant CBAs. The bottom third of the table provides similar results for benefits. Although we have data for the years 1997-2010 only, the estimates indicate that teachers are more likely to acquire new benefits after districts with close

[24] As we note above, this characterization is not entirely accurate as the JW score essentially captures a weighted average of the proportion of text that is not common to both documents, including a penalty for characters that are not in the same position.

Table 4: Impact of tax referenda on Collective Bargaining Agreements (2004-2019)

	Distant*Pass	Pass	Distant*Pass	Pass
Wording Changes (Jaro-Winkler)				
All Text	-0.018*	0.013*	-0.0097	0.0029
	(0.0092)	(0.0071)	(0.0065)	(0.0044)
Benefits	0.0089	-0.0032	0.010	0.00071
	(0.015)	(0.011)	(0.012)	(0.0090)
Work Conditions	0.044**	-0.0077	0.020	-0.0026
	(0.021)	(0.015)	(0.017)	(0.012)
Work Protections	0.020	0.0087	0.0070	0.011
	(0.022)	(0.014)	(0.017)	(0.011)
Pay schedule (2012$)				
Entry pay - BA	468	-226	43.6	137
	(416)	(313)	(276)	(192)
Entry pay - BA (1995-2019)	152	-29.4	-138	236*
	(282)	(214)	(191)	(137)
Top pay - MA	-1,156	1,056	-1,241	1,229
	(1,236)	(1,246)	(880)	(957)
Top pay - MA (1995-2019)	-883	765	-1,083**	1,070*
	(728)	(708)	(543)	(556)
Benefits (1997-2010)				
Benefits Gain (count)	-0.25*	0.19*	-0.21*	0.11
	(0.15)	(0.11)	(0.13)	(0.090)
Benefits Loss (count)	-0.0036	0.13	-0.029	0.038
	(0.18)	(0.11)	(0.13)	(0.091)
Sick/Personal Leave (days)	0.065	-0.072	0.043	-0.039
	(0.13)	(0.10)	(0.091)	(0.085)
Max Leave Accrual (days)	-1.17	-0.30	-3.99	3.37
	(5.83)	(4.51)	(5.94)	(4.53)
Meal time (minutes)	-4.23	4.84**	-3.81*	4.73**
	(2.78)	(2.26)	(2.29)	(1.95)
Baseline Covariates	Yes		Yes	
Year Fixed Effects	Yes		Yes	
Bandwidth (percentage points)	10		20	

Notes: Each pair of coefficients is from a separate OLS regression. "Pass" captures the impact of passing (as opposed to failing to pass) a tax or bond referendum as a district is engaging in collective bargaining, and "Distant*Pass" captures the difference in the impact of passing a tax or bond referendum among districts not engaged in collective bargaining. The dependent variables capture changes between the first post-election CBA and the CBA negotiated prior to the election. All models control for baseline district characteristics (student count, total expenditures per pupil, proportion of students who are free/reduced-lunch eligible, and the proportion of students who are Black) in the year prior to the election. They also include variables capturing entry and top salaries from the prior CBA. Standard errors clustered at the district level appear in parentheses below coefficient estimates. $*p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

CBAs secure new revenue, but that is not the case for districts with distant CBAs. Specifically, the results are statistically significant for the variable $Benefits\ Gain$, which is a count of new benefits among this list: dental coverage, prescription drug coverage, attendance bonuses, tuition coverage, retirement incentives, and parking. Similarly, teachers get approximately 5 more minutes for lunch when a district negotiates a CBA in the midst of receiving new tax revenue, as opposed to one year after committing new revenue.

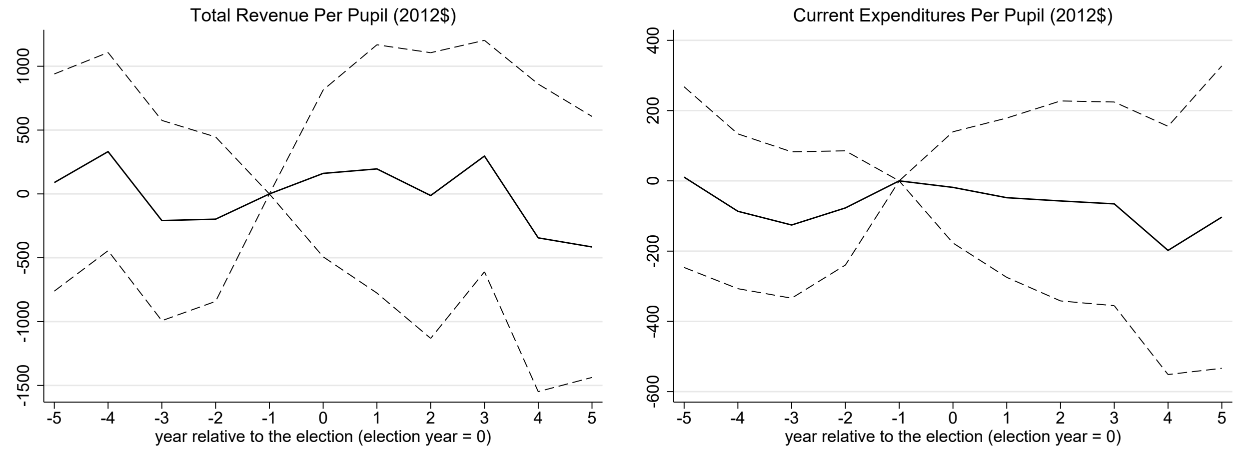
Although the estimates are imprecise, the results paint a coherent picture consistent with the argument that districts with close CBAs experienced more union pressure during collective bargaining than those with distant CBAs. Districts with close CBAs changed more language in their collective bargaining agreements and agreed to greater teacher compensation—in terms of both salary and benefits. On the other hand, it appears that districts with distant CBAs—those that likely committed new funds well before collective bargaining took place—may have provided unions with concessions related to work conditions.

5.3 Impact of Revenue-CBA Timing on Expenditures

Figure 3 plots the estimated difference in the impact of tax passage between districts with distant CBAs and those with close CBAs (i.e., it plots the coefficient for $Distant * Pass$) for overall revenues and current expenditures. These estimates are from a parsimonious model that includes lagged revenues or expenditures from the year prior to the election as the sole covariate. The figure indicates no significant differences in revenues or expenditures in the five years preceding and the five years after a tax referendum, although these estimates are notably imprecise.

Table 5 presents more precisely estimated models of revenues and expenditures, which feature the specification in equation 1. Additionally, these models pool observations for post-election years 1-3, as 90 percent of observations are for CBAs with that duration. (Appendix E provides estimates separately for each of the first five post-election years.) The results in Table 5 are much like those in Figure 3. Although the revenue estimates are very noisy, the results indicate that regardless of CBA timing, districts that passed a tax levy spent approximately \$200 more

Figure 3: Impact of Tax Passage Prior to (vs. During) CBA Negotiations



Note: The figure illustrates the expenditure impacts of passing a tax referendum over one year prior to a new collective bargaining agreement (distant CBA) as opposed to passing a referendum in the year leading up to a new collective bargaining agreement (close CBA). Specifically, it plots the coefficient estimates for the interaction variable *distant*pass*—along with 95 percent confidence intervals—from five years prior to the election (a placebo test) to five years after the election, using the year prior to the election as the baseline. Data are from the Common Core of Data and are reported in constant 2012 dollars.

per pupil over the first three post-election years. The results in Figure 3 and Table 5 suggest that districts with distant CBAs may have spent somewhat less than those with close CBAs, but that difference does not approach conventional levels of statistical significance.

Disaggregating the results according to types of current expenditures, it appears that districts with distant CBAs spent less on instruction—including instructor salaries—and more on support services. But, once again, these results do not generally approach conventional levels of statistical significance. The results clearly indicate, however, that following tax passage, districts with distant CBAs spent about $50 less per pupil on instructor benefits than those with close CBAs. The results also indicate that districts with distant CBAs spent 0.46 percent less of their budgets on benefits than districts with close CBAs. In other words, districts that allocated new revenue in the midst of collective bargaining had a larger share of their budgets dedicated to instructor benefits. Finally, consistent with the notion that districts with close CBAs spent a greater fraction of their new revenues, the results indicate that they experienced a relative decline in their fund balances. Specifically, by the third year after passing a new tax levy, districts with distant CBAs had 6.8

Table 5: Impact of tax referenda on annual revenues and expenditures, years 1-3 (2004-2019)

	Distant*Pass	Pass	Distant*Pass	Pass
Revenue Per Pupil	52.2	355	312	289
	(525)	(329)	(397)	(258)
Current Expenditures Per Pupil	-82.1	204**	-43.9	231***
	(131)	(100)	(112)	(83.7)
Instructional	-113	124*	-52.0	139**
	(83.6)	(65.7)	(70.4)	(54.6)
Support Services	47.4	59.6	15.8	81.4*
	(72.6)	(56.0)	(61.7)	(43.5)
Inst. Salary/Ben. Expend. P.P	-91.9	94.7*	-53.0	105**
	(69.6)	(54.1)	(57.9)	(43.9)
Inst. Salary Expend.	-28.2	58.9	-0.17	67.3**
	(51.7)	(41.0)	(42.5)	(32.3)
Salary/Total Expend.	0.00026	-0.0016	0.00069	-0.0014
	(0.0039)	(0.0032)	(0.0032)	(0.0023)
Inst. Ben. Expend.	-54.7*	25.6	-50.5**	35.5**
	(29.6)	(20.5)	(24.9)	(16.8)
Benefits/Total Exp.	-0.0046*	0.00099	-0.0047**	0.0011
	(0.0025)	(0.0019)	(0.0020)	(0.0014)
Capital Expenditures Per Pupil	-220	893**	261	649**
	(607)	(439)	(440)	(330)
Total Expenditures Per Pupil	-83.3	858*	435	752**
	(658)	(487)	(486)	(359)
Reserves/Expenditures (Year 3)	0.060	-0.048	0.068**	-0.041*
	(0.041)	(0.030)	(0.031)	(0.023)
Lags	Yes		Yes	
Baseline Covariates	Yes		Yes	
Year Fixed Effects	Yes		Yes	
Bandwidth (percentage points)	10		20	

Notes: Each pair of coefficients is from a separate OLS regression. "Pass" captures the impact of passing (as opposed to failing to pass) a tax or bond referendum as a district is engaging in collective bargaining, and "Distant*Pass" captures the difference in the impact of passing a tax or bond referendum among districts not engaged in collective bargaining. The dependent variables capture spending per pupil (unless otherwise indicated). All models include lags of the DV one and three years before the election. They also include controls for district characteristics (student count, total expenditures per pupil, proportion of students who are free/reduced-lunch eligible, and the proportion of students who are Black) and collectively bargained salaries in the year prior to the election. Standard errors clustered at the district level appear in parentheses below coefficient estimates. $*p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

percentage points more in reserves as a fraction of their current expenditures.[25]

5.4 Impact of Revenue-CBA Timing on Staffing

Teachers are school districts' most important and most expensive asset. The salaries and job protections of Ohio teachers are based primarily on seniority.[26] However, barring a significant financial need to reduce a district's teaching force, hiring a teacher essentially commits district funding to that teacher for the foreseeable future regardless of seniority. As we argue above, this is the sort of financial commitment a district with a distant CBA might take on, thereby limiting the financial resources a union can bargain over when it comes time to negotiating a new collective bargaining agreement. Table 6 provides estimates of tax levy passage on staffing during the first three years after a referendum, once again comparing these effects for districts with close and distant CBAs.

Table 6 reveals that districts with distant CBAs had about 12 more teachers during the first three post-election years. As Table E2 in Appendix E reveals, the staffing difference occurs immediately in the first-post-election year and disappears entirely in the fourth post-election year—after CBAs expire for the close-CBA sample. Those results further suggest that the difference in staffing levels comes from two combined effects: districts with distant CBAs hiring more teachers than districts that did not pass a tax and districts with close CBAs hiring fewer teachers than districts that did not pass a tax. Table 6 reveals no average difference in teacher experience, however. This finding is consistent with further analyses indicating that districts hired both experienced and inexperienced teachers. Finally, the table suggests that average teacher pay was between $450 and $625 lower in districts with distant CBAs—which is right at the midpoint of the salary differences in entry- and top-level pay in Table 4—but these estimates do not attain conventional levels of statistical significance. Similarly, the table indicates that districts paid $1,000-$2,000 less in

[25] Models estimating a district's reserves as a fraction of expenditures do not include one- and five-year lags, as that variable is available for 2005-2019 only. Estimates that include a single lag—and, thus, cut the sample size—yield nearly identical but less precisely estimated coefficients.

[26] Although in 2011 Ohio enacted laws that forbid the use of seniority in staffing decisions and that increase pre-tenure probationary periods from 3 to 7 years (with at least three years in the tenuring district), tenure is based on seniority (not performance) and Ohio explicitly identifies it as the primary criterion in layoffs related to fiscal stress.

Table 6: Impact of tax referenda on annual staffing, years 1-3 (2004-2019)

	Distant*Pass	Pass	Distant*Pass	Pass
Teacher count	11.6**	-4.87	11.8***	-7.95*
	(4.80)	(4.29)	(4.49)	(4.17)
Student/teacher ratio	-0.58	0.30	-0.70**	0.15
	(0.42)	(0.34)	(0.35)	(0.28)
Teacher experience (years)	0.26	-0.31	0.25	-0.20
	(0.44)	(0.36)	(0.35)	(0.29)
Teacher pay (2012$)	-625	362	-448	414
	(485)	(406)	(391)	(317)
Salaries/benefits per teacher fte	-2,100	1,537	-1,042	1,212
	(1,374)	(1,093)	(1,230)	(870)
Lags	Yes		Yes	
Baseline Covariates	Yes		Yes	
Year Fixed Effects	Yes		Yes	
Bandwidth (percentage points)	10		20	

Notes: Each pair of coefficients is from a separate OLS regression. "Pass" captures the impact of passing (as opposed to failing to pass) a tax or bond referendum as a district is engaging in collective bargaining, and "Distant*Pass" captures the difference in the impact of passing a tax or bond referendum among districts not engaged in collective bargaining. The dependent variables capture spending per pupil (unless otherwise indicated). All models include lags of the DV one and three years before the election. They also include controls for district characteristics (student count, total expenditures per pupil, proportion of students who are free/reduced-lunch eligible, and the proportion of students who are Black) and collectively bargained salaries in the year prior to the election. Standard errors clustered at the district level appear in parentheses below coefficient estimates. *$p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

salary and benefits per teacher if they secured new revenue well before collective bargaining took place, but, once gain, this difference does not generally approach conventional levels of statistical significance.

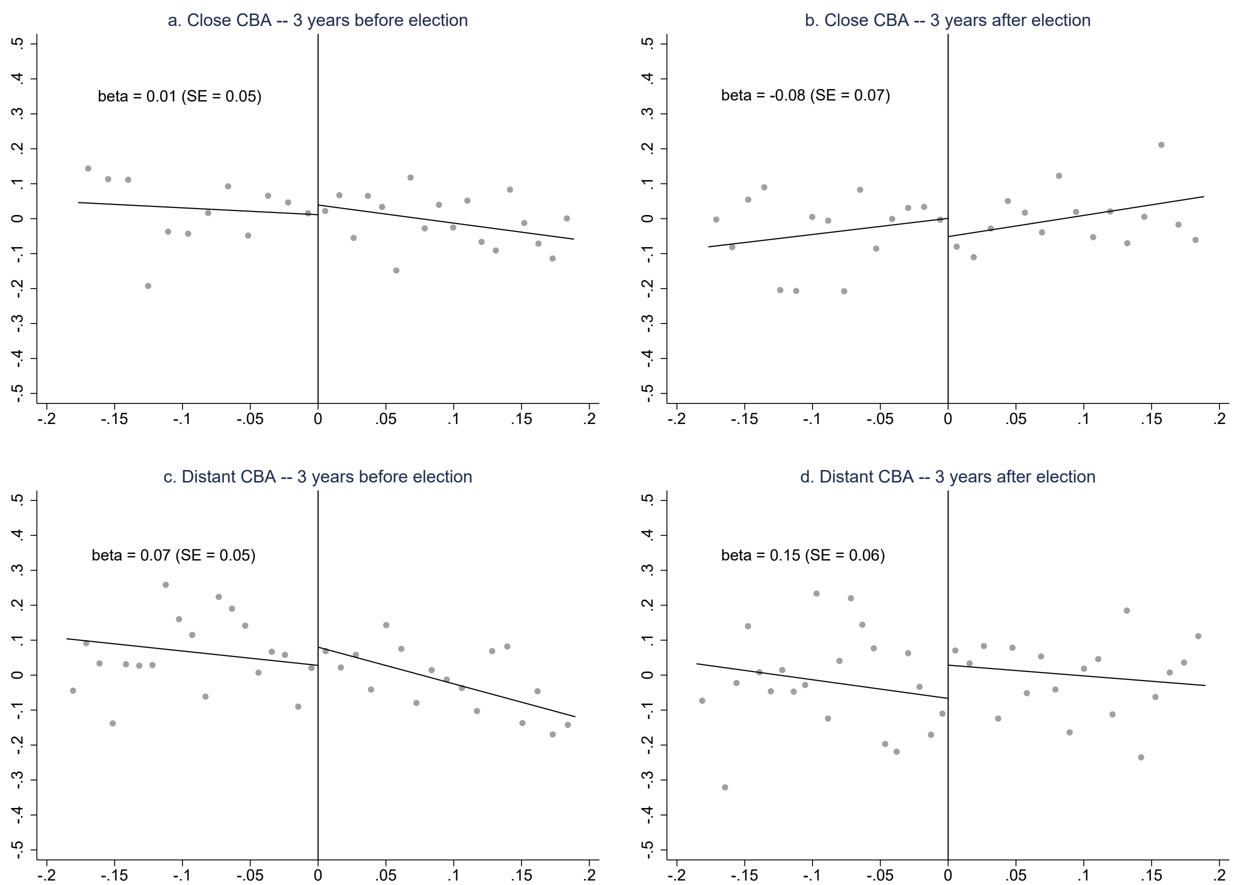
Overall, these results are consistent with the notion that new tax revenues were directed toward hiring more teachers only in districts relatively removed from collective bargaining pressures. Those districts subject to immediate collective bargaining pressures—those that increased collectively bargained salaries—experienced no net increase in teacher staffing levels. Indeed, in some models—particularly those with wider bandwidths—it appears that districts that secured new revenue in the midst of collective bargaining actually hired fewer teachers than those that did not secure new revenue.

5.5 Impact of Revenue-CBA Timing on Student Achievement

We now turn to the effects of tax passage and collective-bargaining agreements on student achievement. Figure 4 illustrates the impact of tax passage on changes in achievement using the standardized performance index. Specifically, the y axis captures the difference in achievement between the indicated year (three years before the election and three years after) and achievement in the year prior to the election. The figures reveal no statistically significant differences in achievement three years prior to the election. Three years after the election, however, tax passage leads to achievement "gains" of -0.08 district-level standard deviations in the close-CBA sample and 0.15 district-level standard deviations in the distant-CBA sample, although only the latter estimate is statistically significant.

Figure 5 plots the estimated difference in the impact of tax passage between districts with distant CBAs and those with close CBAs (i.e., it plots the coefficient for $Distant * Pass$) for teacher counts and student achievement, in order to compare how the two correspond. As in the other figures above, these estimates are from parsimonious models that include lagged teacher counts or student achievement from the year prior to the election as the sole covariate. The figure indicates no significant differences in staffing or achievement in the five years preceding a tax

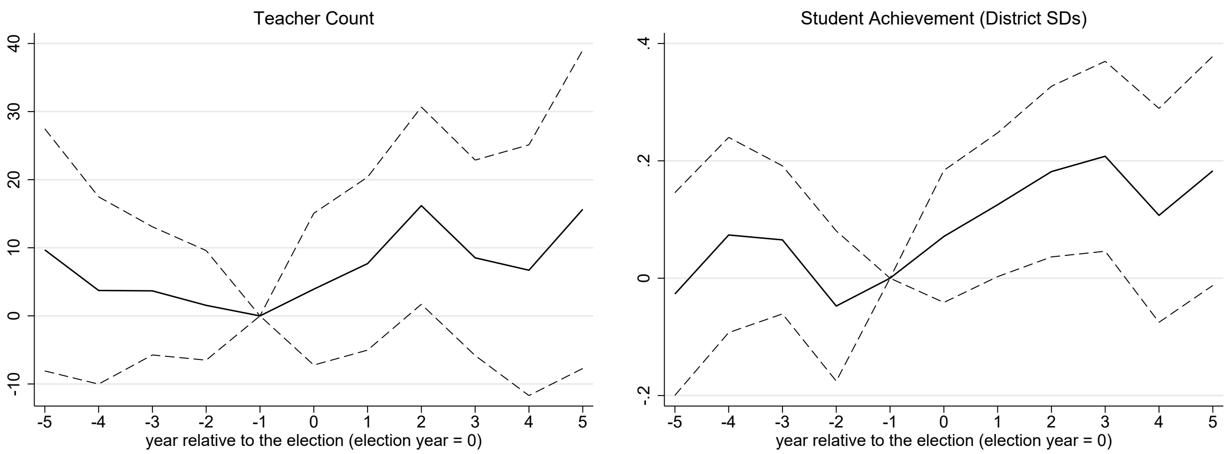
Figure 4: Impact of Tax Passage on Changes in Achievement



Note: The figure compares changes in achievement between districts in which tax referenda passed (vote margin > 0) and those in which referenda failed (vote margin < 0) as of three years before the election (figures a and c) and three years after the election (figures b and d). Specifically, the y axes capture the difference in the performance index (standardized at the district level) between each of these years and the performance index in the year before the election. The x axes capture the centered vote variable—the difference between the fraction of votes in favor of passage and the 0.5 threshold needed for passage. The dots are local means and the regression lines are fitted separately on either side of the vote threshold. We present the results separately for districts with close CBAs (figures a and b) and those with distant CBAs (figures c and d).

referendum, but they reveal positive trajectories in both staffing and student achievement in the years after an election. Indeed, the peak difference in teacher counts occurs in the second post-election year and the peak difference in achievement occurs in the third post-election year, which is consistent with staffing changes being the cause of improvements in student achievement. In other words, it appears that districts subjected to collective bargaining pressures while allocating new revenue experienced lower student achievement because they directed those funds toward salaries instead of new hires.

Figure 5: Impact of Tax Passage Prior to (vs. During) CBA Negotiations



Note: The figure illustrates the expenditure impacts of passing a tax referendum over one year prior to a new collective bargaining agreement (distant CBA) as opposed to passing a referendum in the year leading up to a new collective bargaining agreement (close CBA). Specifically, it plots the coefficient estimates for the interaction variable *distant*pass*—along with 95 percent confidence intervals—from five years prior to the election (a placebo test) to five years after the election, using the year prior to the election as the baseline. Teacher staffing and achievement data are from the Ohio Department of Education.

The top panel of Table 7 presents achievement estimates based on the performance index for the third post-election year, as well as estimates of average effects across the first three post-election years. Once again, the estimates are based on the specification in equation 1, which includes a variety of covariates. The results are robust to the inclusion of these covariates. By the third post-election year, student achievement was 0.15-0.20 district-level standard deviations greater among districts with a distant CBA than among districts with a close CBA at the time they passed a new tax. The average achievement level across the three post-election years is lower than achievement in the third year, which is to be expected because achievement levels increase over the first three post-election years. Finally, as the third row in the table reveals, these results hold even if we scale the achievement measure by a district's expenditures per pupil, indicating that districts subjected to more union pressure while allocating new revenue were less efficient for the duration of their collective bargaining agreements.

The bottom panel of Table 7 presents results of models that estimate the impact of tax passage on annual, student-level "value added" achievement gains averaged across the first three post-election years. These estimates do not capture cumulative achievement like the performance index. Consequently, as we show in Table E4 of Appendix E, there is a large initial gain in the first post-election year and no correspondingly negative estimate after that, indicating a sustained increase. The advantage of this measure is that it captures student-level achievement gains—which allows one to benchmark the effect sizes with those in the literature—and it enables us to characterize how much annual spending corresponds to annual student learning.

Table 7 reveals that spending an extra $200 per year translates to an extra 0.02 standard deviations in annual student achievement gains for districts that were relatively free of union pressure and invested in teachers instead of salaries. This estimate implies cumulative gains of 0.06 student-level standard deviations by the third post-election year. These results are on the higher-end of estimated returns to educational spending (e.g., see Abott et al., 2020; Jackson et al., 2018). As Table 7 reveals, however, there are no positive achievement effects of increased spending for districts engaged in collective bargaining at the time they allocated new revenue. Indeed, although statistically insignificant, the estimates are negative for the models using our preferred bandwidth of 10 percentage points.

It is worth noting that the achievement results disappear if we use a bandwidth of 20 percentage points, instead of our preferred bandwidth of 10 percentage points. However, the results are largest and most precise if we use a bandwidth of 15 percentage points, as we do in Appendix E.

Table 7: Impact of tax referenda on student achievement, years 1-3 (2004-2019)

	Distant*Pass	Pass	Distant*Pass	Pass
Achievement Level (Dist.-level SDs)				
Year 3	0.21**	-0.053	0.14*	-0.049
	(0.099)	(0.075)	(0.071)	(0.051)
Years 1-3	0.15*	-0.030	0.085	-0.032
	(0.083)	(0.061)	(0.059)	(0.041)
Years 1-3 (per $1,000)	0.013*	-0.0032	0.0067	-0.0017
	(0.0074)	(0.0056)	(0.0053)	(0.0040)
Annual Gains (Student-level SDs)				
Years 1-3	0.021*	-0.0073	0.0034	0.0053
	(0.012)	(0.0083)	(0.0099)	(0.0072)
Years 1-3 (per $1,000)	0.0023**	-0.00084	0.00072	0.00003
	(0.0011)	(0.0008)	(0.00096)	(0.0007)
Lags	Yes		Yes	
Baseline Covariates	Yes		Yes	
Year Fixed Effects	Yes		Yes	
Bandwidth (percentage points)	10		20	

Notes: Each pair of coefficients is from a separate OLS regression. "Pass" captures the impact of passing (as opposed to failing to pass) a tax or bond referendum as a district is engaging in collective bargaining, and "Distant*Pass" captures the difference in the impact of passing a tax or bond referendum among districts not engaged in collective bargaining. The dependent variables capture spending per pupil (unless otherwise indicated). All models include lags of the DV one and three years before the election. They also include controls for district characteristics (student count, total expenditures per pupil, proportion of students who are free/reduced-lunch eligible, and the proportion of students who are Black) and collectively bargained salaries in the year prior to the election. Standard errors clustered at the district level appear in parentheses below coefficient estimates. *$p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

6 Conclusion

This study takes advantage of quasi-random variation in school district revenue and the timing of collective bargaining to estimate how the influence of teachers unions affects education production in Ohio school districts. The results indicate that when districts have revenue available, collective bargaining leads districts to increase teacher compensation as opposed to hiring more teachers. During our period of study—which, notably, includes the Great Recession—such union-induced spending is associated with declining reserves and a higher likelihood of new taxes four years later. Districts subject to less collective bargaining pressure when allocating new revenue, however, hired new teachers and did not experience such fiscal stress. Among these districts, increases in current expenditures of $200 per pupil were also associated with annual achievement gains of 0.02 of a student-level standard deviation for the first three post-election years (the length of over 90 percent of collective bargaining agreements in our sample). In contrast, districts subject to relatively high collective-bargaining pressure—those that directed spending toward higher teacher salaries—experienced no achievement gains.

Since the impact of spending on district performance is likely asymmetric—cuts in spending seem to have a more pronounced impact on achievement than increases (see Downes and Figlio, 2015; Jackson et al., 2018; Lavertu and St. Clair, 2018)—that our period of study includes the Great Recession is notable.[27] Teachers are known to have a significant impact on student learning and lifetime outcomes (Chetty et al., 2014) and teacher compensation accounts for the vast majority of school district operational expenditures. Committing such a large fraction of district revenues through salary schedules that prioritize seniority over effectiveness can lead to a significant misallocation of resources (e.g., see Biasi, 2019; Kraft, 2015)—particularly when districts must respond to unexpected fiscal stress. Thus, our period of study may be ideal for testing the worst-case scenario of rent-seeking theory, which may explain the divergence of our results from those of Brunner et al. (2018). Because they focus on the impact of large increases in spending,

[27] Our estimated achievement effects are roughly in line with averages from the literature, including the Jackson et al. (2018) study focusing on the Great Recession. Interestingly, based on an analysis like Jackson et al.'s, Ju (2018) finds no difference in achievement effects between districts with strong and weak unions.

Brunner et al. (2018) may capture a best-case scenario. Allocating new funds to teacher salaries could still have been inefficient, but in their context it nonetheless led to positive achievement effects.

Our study does not speak to the entire bundle of policies linked to collective bargaining and union activity, however. For example, we do not examine the impact of changes in work conditions—such as removing benefits and work protections—which could affect teacher recruitment, retention, morale, and effort (Freeman and Medoff, 1984). Studies that leverage the 2011 rollback of collective bargaining in Wisconsin provide some insights on multiple features of collective-bargaining using convincing research designs. In particular, although there were short-term disruptions (Baron, 2018), it appears that rolling back health benefits and moving away from collective bargaining over seniority-based wage schedules led to improvements in teacher composition and student outcomes (Biasi, 2019; Roth, 2019)—although these effects would likely be muted if all districts had moved away from centralized, seniority-based schedules (Biasi, 2019).[28]

Overall, to our knowledge, our study provides the most direct test of the Hoxby (1996) rent-seeking theory by examining the efficiency implications of collective bargaining. It also confirms the negative impacts on achievement that other scholars have found using different research designs (e.g., Lott and Kenny, 2013; Marianno and Strunk, 2019; Moe, 2009; Strunk, 2011) and it serves as a counterweight to studies that imply positive effects (e.g., Brunner et al., 2018). Finally, it complements the Lovenheim and Willén (2019) analysis that focuses on the attainment and labor market impacts of duty-to-bargain laws by providing a contemporary estimate of collective bargaining's impact on education quality.

[28]Research on wage decentralization in other contexts finds no impacts on teacher composition and student outcomes (see Willén, 2019).

References

Abott, Carolyn, Vladimir Kogan, Stéphane Lavertu, and Zachary Peskowitz (2020). "School District Operational Spending and Student Outcomes: Evidence from Tax Elections in Seven States," *Journal of Public Economics*, 183: 104142.

Backus, Matthew Thomas Blake, Brad Larsen, and Steven Tadelis. (2020). "Sequential Bargaining in the Field: Evidence from Millions of Online Bargaining Interactions," *Quarterly Journal of Economics*, advance access.

Baron, E. Jason. (2018). "The Effect of Teachers' Unions on Student Achievement in the Short Run: Evidence from Wisconsin's Act 10," *Economics of Education Review* 67:40-67.

Barseghyan, Levon and Stephen Coate. (2014). "Bureaucrats, Voters, and Public Investment," *Journal of Public Economics* 119:35-48.

Biasi, Barbara. (2019). "The Labor Market for Teachers Under Different Pay Schemes," Working Paper, dated December 13, 2019.

Brunner, Eric J., Joshua Hyman, and Andrew Ju. (2018). "School Finance Reforms, Teachers' Unions, and the Allocation of School Resources," *Review of Economics and Statistics*.

Calonico, S., Cattaneo, M. D., and Titiunik, R. (2014), "Robust Nonparametric Confidence Intervals for Regression-Discontinuity Designs," *Econometrica*, 82, 2295-2326.

Cellini, Stephanie Riegg, Fernando Ferreira, and Jesse Rothstein. (2010). "The Value of School Facility Investments: Evidence from a Dynamic Regression Discontinuity Design." *Quarterly Journal of Economics* 125(1):215–261.

Chetty, Raj, John N. Friedman, and Jonah E. Rockoff. (2014). "Measuring the Impacts of Teachers II: Teacher Value-Added and Student Outcomes in Adulthood." *American Economic Review* 104(9): 2633-2679.

Chubb, John and Terry Moe. (1990). *Politics, Markets, and America's Schools*. Washington, D.C.: Brookings Institution Press.

Cowen, Joshua M. and Katharine O. Strunk. (2015). "The Impact of Teachers' Unions on Educational Outcomes: What We Know and What We Need to Learn," *Economics of Education Review* 48: 208-223.

Downes, Thomas A. and David N. Figlio. (2015). "Tax and Expenditure Limits, School Finance, and School Quality." In Helen F. Ladd and Margaret E. Goertz, eds., *Handbook of Research in Education Finance and Policy*, 2nd Edition, New York, NY: Routledge, pp 392-408.

Freeman, Richard B. and James L. Medoff. (1984). *What Do Unions Do?* New York: Basic Books.

Hess, Frederick M. and Olivia Meeks. (2010). *School Boards Circa 2010: Governance in the Accountability Era*. Washington, DC: National School Boards Association, Thomas B. Fordham Institute, and Iowa School Boards Foundation.

Howell, William (Ed.). (2005). *Besieged: School Boards and the Future of Education Politics*. Washington, D.C.: Brookings Institution Press.

Hoxby, Carolyn Minter. (1996). "How Teachers' Unions Affect Education Production" *The Quarterly Journal of Economics* 111(3): 671–718.

Jackson, C. Kirabo. (2018). "Does School Spending Matter? The New Literature on an Old Question," National Bureau of Economic Research, Cambridge, MA, Working Paper No. 25368.

Jackson, C. Kirabo, Cora Wigger, and Heyu Xiong. (2018). "Do School Spending Cuts Matter? Evidence from the Great Recession." National Bureau of Economic Research, Cambridge, MA, Working Paper No. 24203.

Jaume, David and Alexander Willén. (2019). "Oh Mother: The Neglected Impact of School Disruptions." CEDLAS Working Paper No. 2019:243.

Johnston, Andrew C. (2019). "Teacher Preferences, Working Conditions, and Compensation Structure," Working paper, November 18, 2019.

Ju, Andrew. (2018). "Teachers' Unions During the Great Recession and its Impact on School District Finance and Student Achievement," Working Paper, Download December 11, 2019 from http://andrewju2.weebly.com/

Kraft, Matthew A. (2015)."Teacher Layoffs, Teacher Quality, and Student Achievement: Evidence from a Discretionary Layoff Policy," *Education Finance and Policy* 10(4): 467-507.

Lavertu, Stéphane and Travis St. Clair. (2018). "Beyond Spending Levels: Revenue Uncertainty and the Performance of Local Governments," *Journal of Urban Economics* 106: 59-80.

Lott, Jonathan, and Lawrence W. Kenny. (2013). "State Teacher Union Strength and Student Achievement," *Economics of Education Review* 35: 93–103.

Lovenheim, Michael F. (2009). "The Effect of Teachers' Unions on Education Production: Evidence from Union Election Certifications in Three Midwestern States." *Journal of Labor Economics* 27(4): 525–87.

Lovenheim, Michael F. and Alexander Willén. (2019). "The Long-Run Effects of Teacher Collective Bargaining," *American Economic Journal: Economic Policy* 11(3): 292–324.

Marianno, Bradley D. and Katharine O. Strunk. (2019). "The Bad End of the Bargain?: Revisiting the Relationship between Collective Bargaining Agreements and Student Achievement," *Economics of Education Review* 65: 93-106.

McCrary, Justin. (2008). "Manipulation of the running variable in the regression discontinuity design: A density test," *Journal of Econometrics* 142(2): 698-714.

Moe, Terry M. (2009). "Collective Bargaining and the Performance of the Public Schools," *American Journal of Political Science* 53 (1): 156–74.

Moe, Terry M. (2011). *Special Interest: Teachers Unions and America's Public Schools*. Washington, D.C.: The Brookings Institution.

Murnane, Richard J. and John P. Papay. (2010) "Teachers' Views on No Child Left Behind: Support for the Principles, Concerns about the Practices," *Journal of Economic Perspectives* 24(3): 151–166.

Neese, Alissa. (2019). "Columbus school board hires Michigan firm to prepare plans in event of teachers strike," *The Columbus Dispatch*. https://www.dispatch.com/news/20190618/columbus-school-board-hires-michigan-firm-to-prepare-plans-in-event-of-teachers-strike.

Retsinas, Joan. (1982). "Teachers: Bargaining for Control," *American Educational Research Journal* 19(3): 353-372.

Romer, Thomas and Howard Rosenthal. (1979). "Bureaucrats versus voters: On the political economy of resource allocation by direct democracy," *Quarterly Journal of Economics* 93(4):563–87.

Roth, Jonathan. (2019)."Union Reform and Teacher Turnover: Evidence from Wisconsin's Act 10," Working Paper, Downloaded October 28, 2019 from https://scholar.harvard.edu/files/jroth/files/roth_act10_20190329.pdf

Rubenstein, Ariel. (1982)."Perfect Equilibrium in a Bargaining Model," *Econometrica* 50(1): 97-109.

Shi, Ying and John D. Singleton. (2020). "School Boards and Education Production: Evidence from Randomized Ballot Order," Working Paper, Downloaded October 24, 2020 from http://www.johndsingleton.com/research

Strunk, Katharine O. (2011). "Are Teachers' Unions Really to Blame? Collective Bargaining Agreements and Their Relationships with District Resource Allocation and Student Performance in California." *Education Finance and Policy* 6(3): 354–98.

Strunk, Katharine O. , Joshua Cowen, Dan Goldhaber, Bradley D. Marianno, Tara Kilbride, Roddy Theobald. (2018). "Collective Bargaining and State-Level Reforms: Assessing Changes to the Restrictiveness of Collective Bargaining Agreements across Three States" CALDER Working Paper No. 210-1218-1, Downloaded October 30, 2019 from https://caldercenter.org/sites/default/files/CALDER%20WP%20210-1218-1.pdf

Strunk, Katharine O. and Jason A. Grissom (2010). "Do Strong Unions Shape District Policies? Collective Bargaining, Teacher Contract Restrictiveness, and the Political Power of Teachers' Unions," *Educational Evaluation and Policy Analysis* 32(3):389-406.

Strunk, Katharine O. and Bradley D. Marianno. (2019). "Negotiating the Great Recession: How Teacher Collective Bargaining Outcomes Change in Times of Financial Duress," *AERA Open* 5(2):1-18.

Willén, Alexander. (2019). "Decentralization of Wage Determination: Evidence from a National Teacher Reform," Working paper, Institute for Evaluation of Labor Market and Education Policy.

Winkler, William E. (1990). "String Comparator Metrics and Enhanced Decision Rules in the Fellegi-Sunter Model of Record Linkage," *Proceedings of the Section on Survey Research Methods of the American Statistical Association*, 354-359.

Winkler, William E. (2006). "Overview of Record Linkage and Current Research Directions," Research Report Series. Downloaded Friday, October 18, 2019, from https://www.census.gov/srd/papers/pdf/rrs2006-02.pdf

Winters, John V. (2011). "Teacher Salaries and Teacher Unions: A Spatial Econometric Approach," *Industrial and Labor Relations Review* 64(4): 747-764.

APPENDIX

A Coding Collective Bargaining Agreements

The Ohio State Employment Relations Board (SERB) provided us with a data file on school district collective bargaining agreements (CBAs) since the 1980s. This file includes the scheduled start and end dates of agreements, as well as some salary and benefits information. SERB also provided us with PDF copies of CBAs from 1999 through 2019 (with spotty coverage for earlier years). Except for very recent CBAs, PDFs were generally based on scans of paper copies of agreements. We extracted text from these scans using optical character recognition software.[29] We then calculated Jaro-Winkler (JW) scores to determine the extent to which the text of a given district CBA differs from the text of the prior CBA in that district (see Winkler, 1990 and Winkler, 2006). The JW metric essentially captures the proportion of characters that are common to both the current and prior CBA; imposes a penalty based on the proportion of characters that are not in the same position in both documents; and then subtracts this fraction from 1 so that 0 indicates identical text between CBAs and 1 indicates completely dissimilar text. We calculated this measure of CBA change for the overall document, as well as for sections dealing with teacher benefits (insurance, retirement, sick leave, and college course-taking); job protections (seniority, reduction-in-force, transfers, and grievance procedures), work conditions (class sizes, evaluation procedures, and recognition); and union-related activities such as collective-bargaining procedures and strikes.

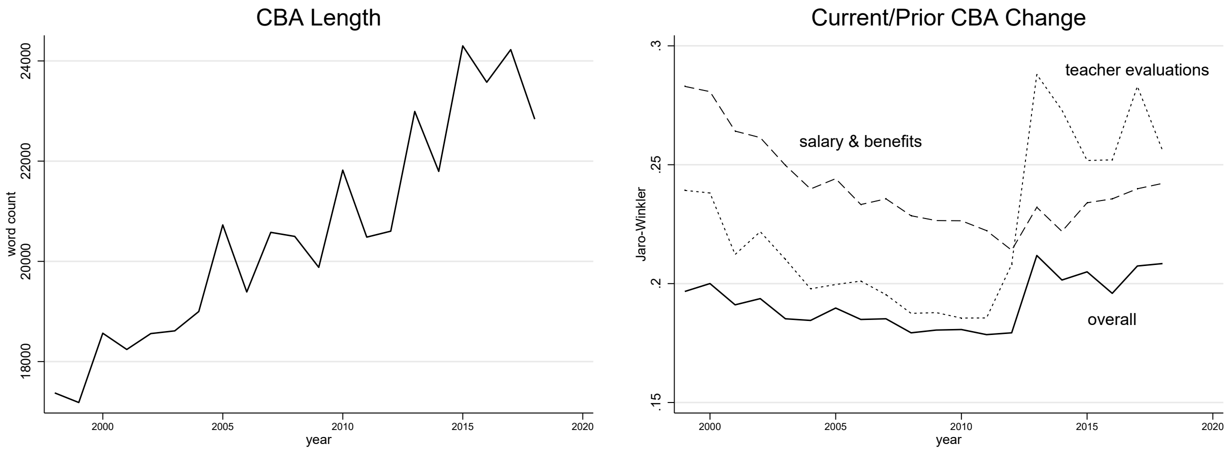
As Figure A1 below reveals, CBAs have rapidly increasing word counts. By 2015, the average CBA length was 24,000 words. The figure also reveals that the average JW score decreases from 1999 to 2011, indicating greater stability over time. This trend seems to be driven by sections dealing with salaries and benefits, which greatly increase in stability over this period. It also could be driven in part by our enhanced ability to extract text and tables (for pay schedules) over time. However, the trend reverses in 2011. As the figure illustrates, the reversal in the overall JW score appears to be due primarily to changes in provisions dealing with teacher evaluations. Further analysis indicates that the *addition* of text related to teacher evaluation drives this change in CBA

[29] Specifically, we used ABBYY FineReader. Text recognition became increasingly difficult as we went back in time and the quality of scans declined, but we were able to extract the vast majority of text from all documents.

text (the word count of evaluation sections increases dramatically), which is consistent with the implementation of Ohio's Race to the Top teacher evaluation plan. But increases in the JW score for salary and benefits sections suggest that the Great Recession also may be responsible for greater changes to CBA text after 2011, as this is when federal stimulus funds run out (affecting funds distributed using the state formula) and when revised housing valuations begin to significantly affect district revenues from local property taxes.

Figure A2 below indicates that average salaries and benefits also vary significantly over time. For example, there is a widening gap from 1990 to 2019 between top-level and entry-level salaries (both of which increase relative to inflation), and the average number of sick or personal days that teachers can accrue increases from about 225 to 275. On the other hand, there is no change over time and little variation across districts in the average number of personal, bereavement, and sick days (3, 4, and 15, respectively) or the average minimum meal time of 30 minutes. Overall, CBAs appear to be remarkably stable in terms of benefits over time, but there are steady yet significant increases in salary schedules—particularly from 1990 to 2007.

Figure A1. Changes in the Text of Collective-Bargaining Agreements, 1999-2019



Note: The figure on the left presents the average word count for collective-bargaining agreements that go into effect in each year, 2000-2019. The figure on the right presents estimates of the extent to which a district CBA's text differs from the text of the prior agreement in that district. Specifically, it plots the average Jaro-Winkler dissimilarity score for the entire CBA ("overall"), sections that deal with salary and benefits, and sections that deal with teacher evaluations. Higher values indicate that CBAs are relatively constant over time in terms of content, and lower values indicate that there are more changes from one CBA to another.

Figure A2. Collectively Bargained Salary Schedules and Benefits, 1990-2019

Note: The figures plot trends in CBA salary schedules and benefits over time, 1990-2019. The top-left figure plots average entry-level and top-level salaries in 2012 dollars for teachers with bachelor's and master's degrees. The top-right figure plots changes in the average number of unused personal days teachers can accumulate, the average number of sick and personal days they receive annually, and the average time (in minutes) they get for lunch. The bottom-left figure indicates the proportion of CBAs that provide teachers with dental and prescription benefits, and the proportion that provide teachers with a cash bonus for unused personal days. The bottom-right figure indicates the proportion of CBAs in a given year that guarantee teachers retirement incentives, parking and tuition benefits, and the right to arbitration.

B CBA Timing

Figure B1. New collective-bargaining agreements by year

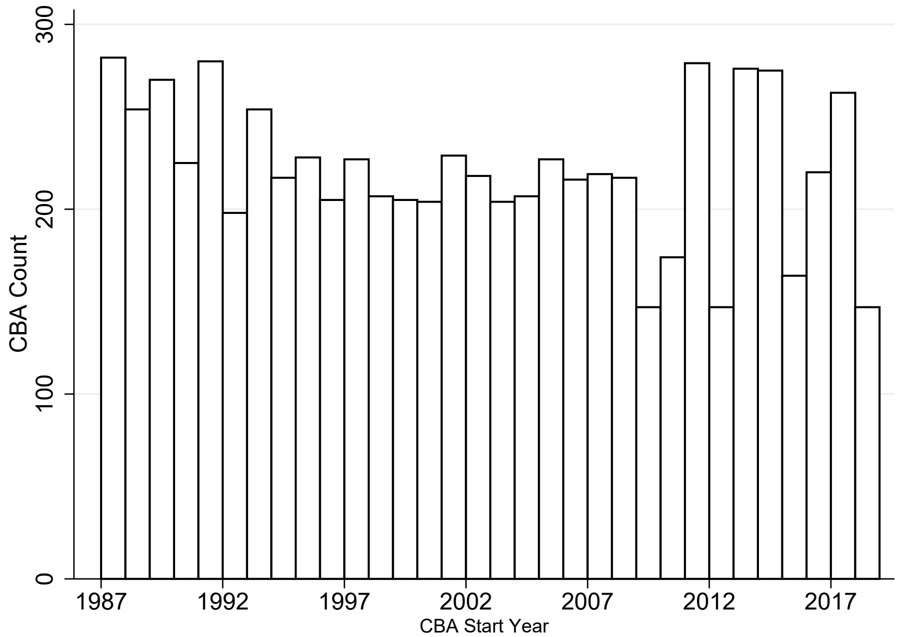


Table B1. Timing of CBAs, 1987-2019

		Obs. Count	Percent
CBA Length			
	Exactly 2 years	1,425	20.13
	Exactly 3 years	4,295	60.66
	Exactly 4 years	297	4.19
	1 year or less	607	8.57
	Irregular (1-8 years)	456	6.44
		7,080	100.00
CBA Start Month			
	January	314	4.56
	June	250	3.63
	July	3,811	55.39
	August	1,459	21.21
	September	1,046	15.20
	Other	200	2.82
		7,080	100.00
CBA End Month			
	June	4,048	58.71
	July	1,116	16.19
	August	1,473	21.36
	December	258	3.74
	Other	185	2.61
		7,080	100.00

Note: The table summarizes the scheduled duration of collective-bargaining agreements (CBAs), as well as the months of their typical start and end dates. Duration is labeled as "exactly" 2, 3, or 4 years if the CBA's formal end date is within two weeks of the 2th, 3rd, or 4th anniversary of the formal start date.

C Timing of Referenda

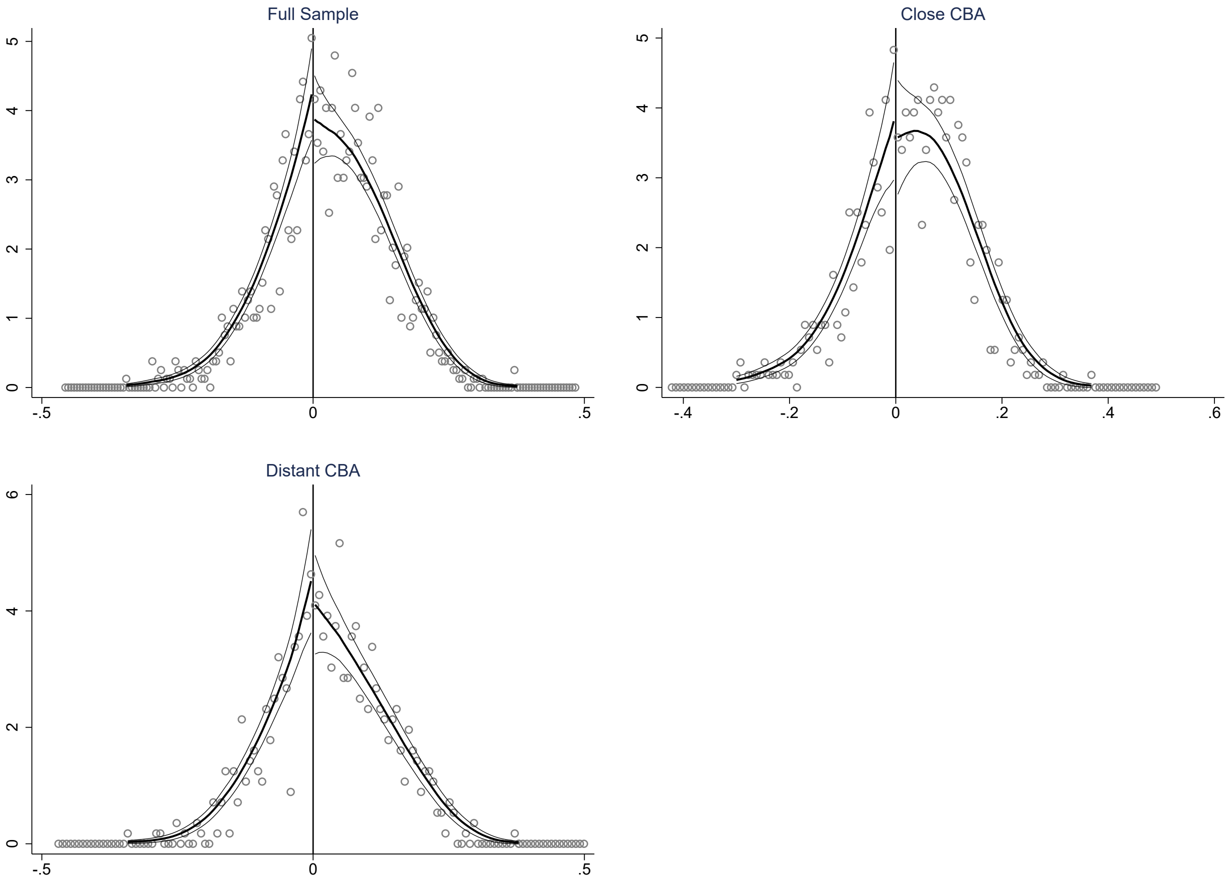
Table C1. Descriptive statistics for Ohio tax and bond referenda, 1995-2019

		Obs. Count	Percent
Referendum Type			
	Tax	6,062	80.46
	Bond	1,472	19.54
		7,534	100.00
Election Month			
	February	244	3.24
	March	650	8.63
	May	2,311	30.67
	August	512	6.80
	November	3,817	50.66
		7,534	100.00
Tax Ref. Length			
	$\leq$5 years	3,777	62.31
	$>$5 years	2,285	37.69
		6,062	100.00
Bond Ref. Length			
	$\geq$20 years	1,405	95.45
	$<$20 years	67	4.55
		1,472	100.00

Note: The table presents counts of referenda by type, by the month in which the election was held, and by the length in which tax levies are in effect. Referendum length is based on the first length provided in the ballot language. Values of tax length greater than 5 years includes cases in which our data contain missing values. Referendum counts are not limited to those for which we can identify a proximate collective-bargaining agreement.

D Validity Checks

Figure D1. Density Tests



Note: The figure presents the results of McCrary (2008) density tests for samples used in the analysis.

Table D1. Pre-election Covariate Balance for Passing/Failing Referenda with Distant/Close CBAs

	(1) Levels (t_1)		(2) Trends	(t_1-t_5)
	Distant*Pass	Pass	Distant*Pass	Pass
Revenue Per Pupil	353	305	477	181
	(485)	(332)	(445)	(305)
Current Expenditures Per Pupil	-47.4	103	-20.9	-54.0
	(264)	(196)	(147)	(119)
Capital Outlays Per Pupil	-106	310	-113	299
	(542)	(411)	(540)	(410)
Salary Expenditures Per Teacher	-102	95.7	-75.4	36.2
	(172)	(131)	(90.7)	(76.6)
Benefits Expenditures Per Teacher	39.2	3.05	49.1	-46.0
	(73.4)	(53.4)	(50.1)	(36.2)
Percnt Teachers Exiting/Entering	-0.34	-1.91	-0.35	-1.86
	(4.45)	(2.85)	(4.45)	(2.86)
Student/Teacher Ratio	0.48	-0.22	0.20	-0.11
	(0.41)	(0.30)	(0.34)	(0.24)
Student/Staff Ratio	0.59	-1.19*	0.36	-0.76
	(0.92)	(0.69)	(0.71)	(0.54)
Teacher Experience (years)	1.22**	-0.62	0.56	-0.098
	(0.57)	(0.44)	(0.42)	(0.36)
Fraction Free/Reduced Lunch	-0.045	0.053*	-0.0034	0.0044
	(0.035)	(0.028)	(0.012)	(0.010)
Fraction Black Students	-0.017	0.034*	-0.0026	0.0048
	(0.025)	(0.020)	(0.0037)	(0.0040)
Student Count	264	123	60.0	-70.0
	(543)	(509)	(73.4)	(77.9)
Student Achievement (SDs)	0.12	-0.20	0.057	-0.052
	(0.18)	(0.14)	(0.097)	(0.077)
Joint Hyp. Test				
Chi2 (26)	15.67	14.90	12.48	13.44
Prob>Chi2	0.2672	0.3138	0.4887	0.4144

Note. Each coefficient is from a separate regression and captures the difference in pre-election characteristics between districts in which referenda passed and those in which referenda failed. Differences in levels are estimated based on the year prior to the election, and differences in pre-treatment trends do the same based on within-district differences in levels between one year prior and five years prior. All models estimated with a bandwidth of +/- 20 percentage points of the vote in favor of passage. Standard errors clustered at the district level appear in parentheses below coefficient estimates. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table D2. Pre-election Covariate Balance for Passing/Failing Referenda with Distant/Close CBAs

	Levels (t_1)	
	Distant*Pass	Pass
Proximity of Prior CBA (days)	12.4	-10.7
	(38.8)	(24.3)
Proximity of Next CBA (days)	-0.59	1.62
	(17.3)	(12.5)
Duration of Prior CBA (years)	0.0097	-0.0094
	(0.054)	(0.041)
Duration of Next CBA (years)	-0.0016	0.0044
	(0.047)	(0.034)
Next CBA Delay (days)	16.5	-15.7
	(27.9)	(15.7)
Entry Salary (Prior CBA)	63.7	-115
	(708)	(520)
Top Salary (Prior CBA)	1,063	280
	(1,797)	(1,245)
Meal Time (Prior CBA)	0.96	0.16
	(0.86)	(0.68)
Work Hours (Prior CBA)	-0.24	0.40
	(0.32)	(0.26)
Max Accrual Days (Prior CBA)	15.5	-3.80
	(11.1)	(9.80)
Personal/Sick Days (Prior CBA)	0.083	-0.012
	(0.12)	(0.087)
Attendance Bonus (Prior CBA)	-0.022	-0.048
	(0.12)	(0.098)
Retirement Incent. (Prior CBA)	0.20	-0.13
	(0.13)	(0.099)
Tuition Benefits (Prior CBA)	-0.085	-0.050
	(0.095)	(0.080)
Parking Benefits (Prior CBA)	0.051	-0.21**
	(0.12)	(0.097)
Joint Hyp. Test		
Chi2 (26)	7.51	9.81
Prob>Chi2	0.8739	0.7093

Notes: Each coefficient is from a separate regression and captures the difference in pre-election characteristics between districts in which referenda passed and those in which referenda failed. Differences in levels are estimated based on the year prior to the election, and differences in pre-treatment trends do the same based on within-district differences in levels between one year prior and five years prior. All models estimated with a bandwidth of +/- 20 percentage points of the vote in favor of passage. Standard errors clustered at the district level appear in parentheses below coefficient estimates. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table D3. **Pre-election Covariate Balance for Distant vs. Close CBAs**

	Levels (t_1)	Trends (t_1-t_5)
	Distant	Distant
Revenue Per Pupil	-167	-66.7
	(147)	(132)
Current Expenditures Per Pupil	-74.0	-39.4
	(78.4)	(44.2)
Capital Outlays Per Pupil	8.79	8.79
	(152)	(152)
Salary Expenditures Per Teacher	-10.3	-15.3
	(51.2)	(27.0)
Benefits Expenditures Per Teacher	-11.3	-2.44
	(22.1)	(14.3)
Percnt Teachers Exiting/Entering	3.33***	3.02**
	(1.27)	(1.27)
Student/Teacher Ratio	-0.0071	-0.013
	(0.11)	(0.092)
Student/Staff Ratio	0.58**	0.61***
	(0.26)	(0.21)
Teacher Experience (years)	-0.16	-0.13
	(0.16)	(0.12)
Fraction Free/Reduced Lunch	-0.0038	-0.0025
	(0.0098)	(0.0036)
Fraction Black Students	0.0036	0.00019
	(0.0071)	(0.00100)
Student Count	-124	-2.54
	(168)	(23.2)
Student Achievement (SDs)	-0.051	-0.029
	(0.052)	(0.029)
Joint Hyp. Test		
Chi2 (26)	16.02	16.59
Prob>Chi2	0.2483	0.2186

Notes: Each coefficient is from a separate regression and captures the difference in pre-election characteristics between referenda/districts with CBAs during the summer after the election ("close" CBAs) and those with CBAs the following summer ("distant" CBAs). Differences in levels are estimated based on the year prior to the election, and differences in pre-treatment trends do the same based on within-district differences in levels between one year prior and five years prior. Standard errors clustered at the district level appear in parentheses below coefficient estimates. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

E Additional Results

Table E1. Impact of referenda on achievement (Years: 2004-2019; Bandwidth: 15 percentage pts)

	Election Year	Post-El. Year 1	Post-El. Year 2	Post-El. Year 3	Post-El. Year 4	Post-El. Year 5
Current Expenditures						
distant*pass	10.2	-25.6	-15.2	-17.8	-142	-137
	(83.4)	(116)	(144)	(146)	(179)	(222)
pass	25.2	204**	224**	191*	260*	257*
	(65.2)	(83.3)	(100)	(109)	(136)	(156)
Instructional Expenditures						
distant*pass	-20.6	-99.3	-63.3	-21.5	-66.1	-1.23
	(51.5)	(71.3)	(84.9)	(86.4)	(106)	(133)
pass	22.3	152***	135**	95.5	156**	100
	(41.7)	(55.8)	(64.8)	(66.4)	(78.4)	(93.9)
Support Services Expenditures						
distant*pass	31.9	82.7	60.0	28.4	-47.6	-109
	(44.9)	(68.4)	(88.9)	(86.6)	(99.0)	(115)
pass	2.69	37.5	72.7	72.4	74.2	126
	(36.8)	(45.6)	(55.2)	(67.8)	(79.9)	(80.4)
Inst. Salary Expenditures						
distant*pass	15.1	0.16	-9.77	18.2	37.4	68.5
	(32.9)	(42.3)	(51.9)	(54.7)	(69.4)	(87.1)
pass	-16.6	54.1	65.8	60.2	35.2	22.9
	(24.2)	(33.3)	(41.7)	(43.2)	(55.4)	(64.2)
Inst. Benefits Expenditures						
distant*pass	-14.6	-43.8	-48.3	-38.1	-57.2	-38.8
	(20.7)	(28.0)	(31.1)	(33.3)	(39.3)	(46.9)
pass	-7.97	25.7	43.4**	27.9	41.4	28.6
	(13.4)	(19.2)	(21.4)	(23.7)	(26.5)	(31.8)

Notes: Each pair of coefficients is from a separate OLS regression. "Pass" captures the impact of passing (as opposed to failing to pass) a tax referendum as a district is engaging in collective bargaining, and "Distant*Pass" captures the difference in the impact of passing a tax referendum among districts not engaged in collective bargaining. The dependent variables capture spending per pupil (unless otherwise indicated). All models include lags of the DV one and three years before the election. They also include controls for teacher salaries in the CBA prior to the election and district characteristics (student count, total expenditures per pupil, proportion of students who are free/reduced-lunch eligible, and the proportion of students who are Black) in the year prior to the election. Standard errors clustered at the district level appear in parentheses below coefficient estimates. $*p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table E2. Impact of referenda on staffing (Years: 2004-2019; Bandwidth: 15 percentage pts)

		Election Year	Post-El. Year 1	Post-El. Year 2	Post-El. Year 3	Post-El. Year 4	Post-El. Year 5
Student/Teacher Ratio							
	distant*pass	-0.37	-0.59	-1.06**	-0.64	-0.0015	20.5
		(0.43)	(0.43)	(0.45)	(0.42)	(0.44)	(16.6)
	pass	0.14	0.27	0.30	0.46	-0.021	-0.59
		(0.34)	(0.36)	(0.33)	(0.33)	(0.32)	(2.53)
Teacher Count							
	distant*pass	4.59	12.1*	15.0**	11.1*	2.69	2.92
		(5.80)	(6.62)	(6.02)	(6.04)	(7.04)	(9.55)
	pass	-3.29	-11.2*	-10.9*	-9.05*	-5.71	-3.26
		(4.55)	(6.24)	(5.73)	(5.11)	(5.67)	(5.22)
Teacher – Last Year In Dist. (percent)							
	distant*pass	0.63	0.21	1.48	0.61	-0.17	1.79
		(1.03)	(1.26)	(1.03)	(1.08)	(1.08)	(1.10)
	pass	-0.63	-0.48	-0.019	-0.73	0.70	-0.87
		(0.79)	(1.13)	(0.73)	(0.71)	(0.70)	(0.82)
Teacher – First Year In Dist. (percent)							
	distant*pass	1.81**	1.73	0.96	1.08	-0.31	1.22
		(0.86)	(1.43)	(1.10)	(0.93)	(1.00)	(0.94)
	pass	-1.07*	-1.37	0.23	-1.05	0.28	-1.51**
		(0.64)	(1.26)	(0.86)	(0.71)	(0.73)	(0.66)

Note: Each pair of coefficients is from a separate OLS regression. “Pass” captures the impact of passing (as opposed to failing to pass) a tax referendum as a district is engaging in collective bargaining, and “Distant*Pass” captures the difference in the impact of passing a tax referendum among districts not engaged in collective bargaining. All models include lags of the DV one and three years before the election. They also include controls for teacher salaries in the CBA prior to the election and district characteristics (student count, total expenditures per pupil, proportion of students who are free/reduced-lunch eligible, and the proportion of students who are Black) in the year prior to the election. Standard errors clustered at the district level appear in parentheses below coefficient estimates. *$p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table E3. Impact of referenda on balances, surplus, and future levies (2004-2019; Band: 15 pct. pts)

		Election Year	Post-El. Year 1	Post-El. Year 2	Post-El. Year 3	Post-El. Year 4	Post-El. Year 5
Fund balance							
	distant*pass	0.026	0.041	0.040	0.068**	0.064*	0.062
		(0.030)	(0.030)	(0.030)	(0.033)	(0.035)	(0.042)
	pass	0.0031	-0.0052	-0.020	-0.059**	-0.058**	-0.070**
		(0.023)	(0.022)	(0.022)	(0.025)	(0.027)	(0.035)
Future levy passage							
	distant*pass		-0.021	-0.065	0.074	-0.14	0.033
			(0.10)	(0.11)	(0.099)	(0.11)	(0.11)
	pass		-0.25***	-0.0058	-0.078	0.18**	-0.065
			(0.077)	(0.080)	(0.075)	(0.083)	(0.078)

Note: Each pair of coefficients is from a separate OLS regression. "Pass" captures the impact of passing (as opposed to failing to pass) a tax referendum as a district is engaging in collective bargaining, and "Distant*Pass" captures the difference in the impact of passing a tax referendum among districts not engaged in collective bargaining. All models include lags of the Performance Index (standardized at the district level) one and three years before the election. They also include controls for teacher salaries in the CBA prior to the election and district characteristics (student count, total expenditures per pupil, proportion of students who are free/reduced-lunch eligible, and the proportion of students who are Black) in the year prior to the election. Standard errors clustered at the district level appear in parentheses below coefficient estimates. *$p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table E4. Impact of referenda on achievement (Years: 2004-2019; Bandwidth: 15 percentage pts)

		Election Year	Post-El. Year 1	Post-El. Year 2	Post-El. Year 3	Post-El. Year 4	Post-El. Year 5
Performance Index							
	distant*pass	0.018	0.11*	0.15**	0.17**	0.091	0.15*
		(0.057)	(0.062)	(0.074)	(0.078)	(0.087)	(0.086)
	pass	0.024	-0.032	-0.048	-0.046	0.025	-0.070
		(0.042)	(0.051)	(0.056)	(0.059)	(0.076)	(0.067)
P. Index (per $1,000 per pup.)							
	distant*pass	-0.00012	0.0072	0.0097	0.018**	0.0072	0.013
		(0.0057)	(0.0063)	(0.0070)	(0.0072)	(0.0085)	(0.0088)
	pass	0.0031	-0.0023	-0.0024	-0.0056	0.0018	-0.0053
		(0.0041)	(0.0051)	(0.0055)	(0.0056)	(0.0074)	(0.0069)
Annual Value-Added							
	distant*pass	0.027	0.030*	0.011	-0.00036	-0.0037	0.032
		(0.017)	(0.018)	(0.022)	(0.018)	(0.024)	(0.028)
	pass	-0.019	-0.010	0.00024	0.00029	0.0040	-0.047**
		(0.013)	(0.014)	(0.016)	(0.013)	(0.021)	(0.020)
Ann. Val.-Add. (per $1,000)							
	distant*pass	0.0022	0.0031*	0.0011	0.00037	-0.00003	0.00068
		(0.0016)	(0.0018)	(0.0021)	(0.0016)	(0.0026)	(0.0030)
	pass	-0.0018	-0.0016	-0.00018	-0.00039	0.00024	-0.0022
		(0.0012)	(0.0014)	(0.0016)	(0.0012)	(0.0024)	(0.0020)

Note: Each pair of coefficients is from a separate OLS regression. "Pass" captures the impact of passing (as opposed to failing to pass) a tax referendum as a district is engaging in collective bargaining, and "Distant*Pass" captures the difference in the impact of passing a tax referendum among districts not engaged in collective bargaining. All models include lags of the Performance Index (standardized at the district level) one and three years before the election. They also include controls for teacher salaries in the CBA prior to the election and district characteristics (student count, total expenditures per pupil, proportion of students who are free/reduced-lunch eligible, and the proportion of students who are Black) in the year prior to the election. Standard errors clustered at the district level appear in parentheses below coefficient estimates. *$p < 0.10$, ** $p < 0.05$, *** $p < 0.01$