PUBLIC-SECTOR UNIONS AND THE SIZE OF GOVERNMENT

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Public-sector unions are generally thought to increase the size of government through collective bargaining. This article challenges this idea for the case of teacher unions in the U.S., and argues that while collective bargaining institutions sometimes lead to increased education spending, this is not the norm. Using a new longitudinal dataset spanning all states before and after they granted collective bargaining rights to teachers, the article shows that although states that mandate districts to bargain with teachers have higher education expenditures than states that do not, the differences *precede* collective bargaining. Difference-in-differences analyses find no evidence that introducing collective bargaining rights led to average increases in the level of resources devoted to education. While existing theories cannot explain these null findings, the article shows one reason behind them is that most laws granting collective bargaining rights to teachers were not unambiguously pro-labor, but included both pro- *and anti-*union provisions.

Keywords: labor unions; teacher unions; public employees; education; collective bargaining; politics of policymaking; endogenous institutions.

A long theoretical tradition in political economy holds that organized bureaucrats exert pressure from inside the government to expand its size, and that collective bargaining is a key mechanism by which they attain this (e.g., Downs 1964; Niskanen 1971; Peterson 1976; Levi 1977; Freeman 1984; Shefter 1985; Swenson 1991a; Moe 2006; Anzia & Moe 2015). The argument that collective bargaining weakens politicians—who cannot afford to endure strikes and public service disruptions[1]—and empowers unions to extract sizable material benefits such as more jobs, better job protections, and higher compensation, lies at the heart of existing theories of what public-sector unions do and where their power stems from. It shapes how we think about interest groups, policymaking, education politics, and bureaucracies; and informs the debate on how to regulate these unions' legal rights. In the United States, conservatives have argued that cutting public employees' collective bargaining rights should help reduce fiscal deficits— think of Scott Walker in Wisconsin—, while liberals have argued that maintaining these rights enables public employees to obtain fair compensation.

Surprisingly, whether collective bargaining enables public employees to increase the size of government remains an open empirical question. Dozens of cross-sectional studies comparing U.S. governments *at a single point in time* demonstrate that those that have collective bargaining institutions employ more workers, pay higher salaries, and spend more than those that do not.[2] However, as emphasized by Moe (2011: 211), we cannot rely on these studies to make causal claims because they do not account for the role of unobservable factors that could have led both to the emergence of bargaining institutions and to greater public spending. Studies that employ longitudinal data and account for such characteristics represent a considerable improvement, but these studies are scant (Hoxby 1996; Lovenheim 2009; Anzia & Moe 2015; Frandsen 2016), they

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[1] Wellington and Winter (1969: 1125); Levi (1977: 11); Swenson (1991a); Anzia & Moe (2015) ("public sector unions … exercise power … through collective bargaining: formal negotiations with management, backed by implicit threats of strike" p. 116).
[2] Freeman (1984) provides a comprehensive literature review.

arrive at conflicting conclusions, and raise questions about their internal and/or external validity due to: (i) the use of difference-in-differences methods without examining if the pre-treatment trends support the identifying parallel trends assumption;[3] (ii) reliance on data from the U.S. Census of Governments,[4] which Lovenheim (2009) has shown produces upwardly-biased estimates of union effects, or data from the Current Population Survey,[5] which is not designed to provide a representative sample of local or state governments; and/or (iii) coverage of a minority of states with collective bargaining rights for public employees.[6]

This article uses a new longitudinal dataset constructed from historical sources to overcome the limitations of prior research and assess whether granting collective bargaining rights to public employees leads to increases in the size of government. Focusing on the case of education, and building on a literature that leverages state-level variation in the timing of institutional reforms (e.g., Besley & Case 2003; Folke, Hirano, & Snyder 2011; Fowler 2013), I examine what happened to the level of resources devoted to education when states passed laws ending prohibitions on teachers' collective bargaining and establishing districts' obligation to bargain with teacher unions. These mandatory collective bargaining laws, passed in thirty-three states from the 1960s on, have been persuasively identified as the main determinant of the emergence of modern teacher unions (Moe 2011). They led to a strong increase in union membership and collective bargaining agreements (Saltzman 1985; Moe 2011), and increased unions' capacity for political mobilization (Flavin & Hartney 2015), usually in favor of Democrats. But did these laws also increase the level of resources devoted to education?

The results, which are robust to various tests and specifications, indicate that the positive correlation between collective bargaining rights and the size of government is

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[3] e.g., Hoxby (1996); Anzia & Moe (2015).
[4] e.g., Hoxby (1996); Anzia & Moe (2015).
[5] e.g., Frandsen (2016).
[6] e.g., Hoxby (1996); Lovenheim (2009); Anzia & Moe (2015); Frandsen (2016).

spurious—at least for teachers, who comprise the largest group of unionized public employees. First, consistent with cross-sectional studies, I document that in 1990 states with mandatory collective bargaining laws had more teachers per student, higher salaries, and higher per-pupil education expenditures, but I show that *they already did so well before* the emergence of collective bargaining rights or modern teacher unions. Second, non-parametric difference-in-differences estimates that exploit variation in the timing of mandatory bargaining laws across states provide no support for the argument that, on average, the laws led to increases in the level of resources devoted to education. That is, the differences are historical and the introduction of mandatory bargaining with teachers on average did not exacerbate them. Why not?

I argue that a key reason why the laws did not lead to average increases in education spending is that, contrary to existing theories' assumption that the laws favored unions, in most states the content of the laws not only gave teachers collective bargaining rights, but also introduced provisions designed to limit unions' power. Important among these were provisions establishing costly penalties against strikes. The inclusion of a bundle of pro- and anti-union provisions is consistent with accounts of the design of private-sector bargaining institutions (Goldfield 1989; Swenson 1991b; Levi et.al. 2017), and with political economy and policy feedback theories that predict institutional capture and "counter-mobilization" (Schickler 2001; Swenson 2002, 2004; Acemoglu & Robinson 2008; Hacker & Pierson 2014; Jacobs & Weaver 2014) by organized groups who stood to lose from unions' empowerment. Because strikes typically constitute the primary weapon available to unions during collective negotiations, I hypothesize that in states whose laws included new strike penalties, unions had limited ability to extract material concessions from such negotiations.

The empirical analysis supports this argument. When we disaggregate the average null findings, we find that those mandatory bargaining laws that did not include new

strike penalties did lead to increased spending, as conventional wisdom predicts. This, however, was the case in fourteen of the thirty-three states that passed these laws. In the remaining nineteen states, the laws that granted teachers collective bargaining rights also included strike penalties designed to raise the cost of striking, and these laws did not lead to increases in education expenditures.

The findings challenge core beliefs about what public-sector unions do and where their power stems from, and help identify a set of empirically-substantiated conditions under which unions are more likely to obtain what they want. They also underscore the importance of future research on the historical determinants of current education policy and provision; and speak to the debate on who shapes policy in democratic regimes, and what that implies for outcomes we care about (here, fiscal outcomes).

Beyond their theoretical relevance, these findings inform the heated debate on how to regulate public-sector unions. Granting collective bargaining rights to teachers may have led to an increase in unions' membership and capacity for political organization—and precisely because of that, Republicans have *political* reasons to undo, and Democrats to maintain, these rights. But arguments that rely on an *economic* rationale to curtail or uphold these rights lack strong empirical evidence to support them.

Collective Bargaining Laws: Pro- and Anti-Labor Provisions

Following a long history of prohibitions against public employees' engagement in collective bargaining, beginning in the 1960s a wave of state laws established local and state governments' obligation to bargain with them. By 1990, thirty-three states had laws mandating school districts to bargain with teachers;[7] ten states allowed but did not require bargaining; and seven states, mostly Southern, prohibited it. Figure 1 maps these rights; Figure A1 shows the timing of the laws and the rising coverage of collective bargaining agreements (CBAs).

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[7] In mandatory-bargaining states, almost all teachers became covered by CBAs (Saltzman 1982, 1985, 1988; Moe 2011).

Figure 1. Legal Status of Collective Bargaining as of 1990, First Year when a Mandatory Collective Bargaining Law was Passed, and Type of Strike Penalty in the Law, by State
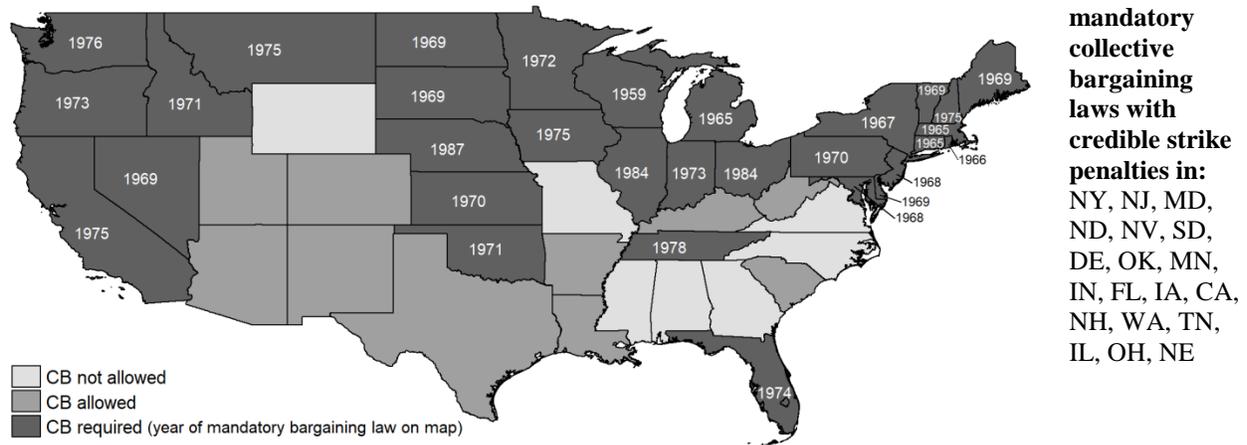
The mandatory bargaining laws of the 1960s and 1970s have usually been characterized as "pro-labor" (e.g., Levi 1977; Saltzman 1985; DiSalvo 2010; Moe 2011; Ahlquist 2012; Walker 2014; Flavin & Hartney 2015; Anzia & Moe 2015, 2016), and in some important respects, they were. In mandatory-bargaining states, almost all teachers became covered by CBAs (Saltzman 1982, 1985, 1988; Moe 2011). They triggered an upsurge in teacher union membership rates (Saltzman 1982, 1985, 1988; Freeman 1984; Moe 2011), which rose from 6% in the late 1950s to 60% in the early 1980s. This growth positioned teacher unions as large interest groups with ties to the Democratic Party and the ability to mobilize voters, lobby lawmakers, and campaign for their preferred candidates (Berkman & Plutzer 2005; Moe 2011).[8] Additionally, the laws conferred some organizational advantages to unions—e.g., access to teachers' contact information and automatic deduction of union dues from teachers' salaries—which facilitated unions' political mobilization of teachers (Flavin & Hartney 2015).

Notwithstanding their role in the emergence of teacher unions as relevant political actors in elections and legislatures, it is not obvious that mandatory bargaining laws

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[8] Unions' *success* in influencing policy is unclear. See the conflicting findings of Berkman & Plutzer (2005); Shen & Wong (2006); Moe (2006).

empowered unions *at the bargaining table*. Strikes are typically unions' main form of leverage during collective bargaining (Ashenfelter & Johnson 1969; Freeman 1984; Levi 2003), especially in the public sector, where they can paralyze public service provision, inconvenience voters, and thus pressure politicians to find "a quick end to the strike … with little concern for the cost of settlement" (Wellington & Winter 1969: 1125; see also footnote 1). When public-sector unions engage in collective bargaining "backed by implicit threats of strike," we should expect them to obtain more jobs, higher salaries, better benefits, and/or greater job stability, all of which requires increased public spending (Anzia & Moe 2015: 116).

The reason why unions' ability to exert pressure during collective bargaining should not be taken for granted is that rational politicians and interest groups who are concerned about public-sector strikes, or about the empowerment of unions generally, may try to influence the design of labor laws to limit unions' power. This expectation stems from political economy and policy feedback theories that argue that policy proposals with the potential to radically redistribute power toward one interest group will likely trigger a counter-mobilization by organized groups who stand to lose from such redistribution (Schickler 2001; Swenson 2002, 2004; Acemoglu & Robinson 2008; Hacker & Pierson 2014; Jacobs & Weaver 2014), leading to laws that look like a patchwork catering to multiple competing interests (Schickler 2001; Swenson 2002, 2004). Indeed, there is evidence from the U.S. and other advanced democracies that business groups seeking to deter labor militancy shaped private-sector bargaining institutions in ways that went against unions' interests (Goldfield 1989; Swenson 1991b; Levi et.al. 2017).

While past theories of how public-sector mandatory bargaining laws impacted the size of government assume that the laws were unambiguously pro-labor (Saltzman 1985; Moe 2009, 2011; Anzia & Moe 2015), I consider the content of these laws to inform

expectations about the conditions under which they favored unions at the bargaining table. Three facts suggest that the laws were neither passed by an unambiguously pro-labor coalition nor were unambiguously pro-labor in content: (1) the laws had extensive *bipartisan* support; (2) a key goal of many laws was to *deter public-sector strikes*; and (3) to attain this, the laws often introduced *new strike penalties* aimed at increasing the cost of going on strike. My goal here is not to provide a full account of the dynamics that shaped these laws, but to bring back to light important facts that have not informed existing theories of how mandatory bargaining laws impacted the size of government.

Bipartisan Support

Contrary to common perceptions that public-sector bargaining laws were passed by Democrats to empower unions (DiSalvo 2010: 8; McCartin 2007: 79; Moe 2009: 158), Anzia and Moe (2016) show that the laws had extensive bipartisan support. Table 1 complements their analysis of roll-call votes by classifying states depending on which party had the power to (a) veto legislation or (b) unilaterally pass legislation when states introduced mandatory bargaining laws covering teachers.

The analysis underscores that both parties supported these laws. First, in seventeen of the thirty-three states that introduced mandatory bargaining laws, Republicans could have vetoed these laws, but did not. Second, Democrats had the power to unilaterally pass new legislation in only ten of the thirty-three states that passed these laws; Republicans had this power in seven states; and, in the remaning states, both parties had to negotiate the content of the laws because neither had a legislative supermajority to override the veto of a governor from the opposite party. Why did both parties—especially, Republicans—support these laws?

Table 1. Partisan Balance when States Passed Mandatory Collective Bargaining Laws

State Partisan Balance	States	Party with power to veto legislation	Party with power to pass new laws
Unified Republican Government	SD, DE, VT, IN	Republicans	Republicans
Democratic Governor but veto-proof Republican Legislature	NJ, ND, KS		
Republican Governor and non-veto-proof Democratic Legislature	MI, NY, NV, AK, PA, OR, IA, NH, WA, IL		neither party
Democratic Governor and non-veto-proof Republican Legislature	WI, CT, ME, ID, MN		
Republican Governor but veto-proof Democratic Legislature	MA, RI, MD	Democrats	
Unified Democratic Government	HI, OK, FL, MT, CA, TN, OH		Democrats

Laws to Deter Strikes

Many mandatory bargaining laws sought to deter strikes by public employees (Levi 1977; Saltzman 1982; Miller & Canak 1991). Once uncommon, public-sector strikes increased in the early 1960s and reached 250 strikes per year in 1966-1968, leaving entire cities without schooling, public transportation, and other services. Politicians from both parties who were concerned about the reputational and electoral costs of service disruptions (Levi 1977), and business groups concerned about the economic costs and disorder caused by public-sector strikes (Miller & Canak 1988, 1991, 1995), became interested in new laws to deter them.

Almost all the mandatory bargaining laws of the 1960s and 1970s prohibited public-sector strikes—a major departure from the private sector's National Labor Relations Act of 1935. But legal prohibitions alone were insufficient to deter strikes. The problem

lawmakers needed to address was that the strike penalties inherited from the 1940s—dismissal or jail time for strikers—were not useful deterrents because they could not be credibly enforced by a government desperate to end a service shutdown (Levi 1977: 11).

Collective bargaining laws were conceived in this context as a new approach to deter strikes. On one hand, lawmakers hoped that bargaining rights would appease insurgent bureaucrats (Levi 1977: 18-19; also, Saltzman 1982: 161; Anzia & Moe 2016: 767). Simultaneously, in most states the laws introduced new strike penalties that could be more credibly enforced by governments.

The experience of New York State illustrates these dynamics. In 1966, following a twelve-day transit strike, Republican Governor Nelson Rockefeller appointed a Committee to "make legislative proposals for protecting the public against the disruption of vital public services by illegal strikes, while at the same time protecting the rights of public employees." The result was New York's mandatory collective bargaining law of 1967. George Taylor, who headed the Committee, explains the challenges they faced when drafting the law:

> "the ban on strikes becomes more or less meaningless in the absence of penalties which actually deter stoppages. The results are even worse when … impractical penalties cannot be invoked and the impotence of government is revealed. Not only are new negotiating procedures essential, but a difficult question must be faced: What penalties would serve as deterrents to strikes…?" (Taylor 1967: 618).

To deter strikes, the Taylor Law simultaneously established collective bargaining rights and strike penalties "harsh enough to deter strikes, but not too harsh to be imposed: strikers lose two days' pay for each day on strike, while unions lose dues deduction privileges" (Saltzman 1982: 167). Union officials criticized the law because its "focus was on … prohibitions against strikes by public employees [and] on penalty provisions" (New York Times 1968). Business applauded the stipulation of credible penalties, especially the use of fines:

"The vexatious problem of [strikes] … requires new approaches. This does not necessarily mean that public employee strikes should be banned even more. Less can sometimes work better than more… The law should impose heavy fines for each day the strike continues. *Where jail sentences make martyrs, fines threaten to make paupers of unions.*" (Business Week 1968, emphasis mine.)

New Strike Penalties

As shown in Figure 1, nineteen of the thirty-three mandatory bargaining laws introduced new penalties that, like New York's Taylor Law, imposed *monetary and organizational* sanctions for striking. These included: wage losses for employees; monetary fines for unions; union decertification; suspension of CBAs; and/or loss of automatic dues deduction. Inclusion of these penalties became widespread after the escalation of strikes in 1966: two-thirds of the laws passed after 1966 included the new penalties, compared to none prior to 1966. Consistent with the widespread concern about strikes, the new approach was equally endorsed by unified Republican and unified Democratic governments.

Prior research shows that mandatory bargaining laws reduced public-sector strikes (Currie & McConnell 1994; Stern & Olson 1982; Freeman 1984), especially where credible strike penalties were established (Olson 1986). Still, this reduction is also consistent with the argument that the laws *empowered* unions and thus reduced their need to strike. We therefore need to turn to the evidence to determine whether mandatory bargaining laws empowered unions at the bargaining table.

Expectations

Because strikes and threats of strike are unions' primary weapon during collective bargaining (e.g., Wellington & Winter 1969; Levi 2003: 56; Swenson 1991a: 382; Ahlquist 2012; Anzia & Moe 2015: 116), I hypothesize that, in states whose laws included the new strike penalties, unions were limited in their ability to extract material concessions from bargaining. Thus, it is not obvious that, on average, mandatory bargaining laws led to increases in the level of resources devoted to education: the

overall effect will depend on the effect of laws with new strike penalties (nineteen states) and without them (fourteen states).

Original Longitudinal Dataset

Which states adopted mandatory bargaining laws was not determined at random. Historically, these states were more industrialized, had fewer minorities, and had more favorable legislation for private-sector workers (Table A1), all predictive of greater redistribution and public spending. We need to account for these and other potential confounders to isolate the effect of mandatory bargaining laws on the size of government. While the cross-sectional literature nets out the role of observable factors such as GDP or private-sector unionization, studies that employ longitudinal evidence preceding and following the introduction of collective bargaining rights can additionally account for unobservable time-invariant factors such as cultural values and historical attitudes toward labor rights and redistribution.[9]

Longitudinal studies, however, are an oddity in the extant literature because of the limited availability of digitized data preceding public-sector collective bargaining. The few longitudinal studies that have been published arrive at conflicting conclusions (e.g., Hoxby 1996 and Anzia & Moe 2015 find positive effects; Lovenheim 2009 and Frandsen 2016 find no effect); and employ data that raise questions about their external and/or internal validity. First, existing studies rely on the U.S. Census of Governments (COG) to measure the size of government, using measures of public-sector employment, salaries, and spending collected by the COG from local governments. Reliance on these data raises questions about the generalizability of these studies' findings because COG data are only available since 1972, and thus cannot be used to examine the impact of collective bargaining institutions that emerged before that year. However, twenty of the

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[9] Including a dummy for Southern states is insufficient; the same pattern of historical differences exists *within* the South (Table A2).

thirty-three mandatory bargaining laws covering teachers were introduced *before* 1972.[10]
To overcome this limitation, I employ an original longitudinal dataset containing information on the level of resources devoted to education in *all* fifty states before and after any of them introduced collective bargaining rights for teachers.

Additionally, some studies rely on the COG to measure local-level unionization rates and the timing of locally-negotiated CBAs (e.g., Hoxby 1996; Anzia & Moe 2015). This raises internal validity questions, given Lovenheim's (2009) finding that these data contain non-classical measurement error that produces upwardly-biased estimates of unions' impact. To remedy this problem, Lovenheim (2009) hand-collects data on the timing of CBAs directly from Public Employment Relations Board offices, but understandably his data-collection effort is limited to three Midwestern states. In this article, as in Frandsen (2016), what is important is to measure accurately the timing of mandatory collective bargaining laws, which are determined at the state level. Frandsen (2016) relies on the NBER Public Sector Collective Bargaining Law Dataset, which contains annual information since 1959 on whether a state allows, requires or prohibits collective bargaining with teachers, police, and firefighters. As explained below, I use this source too, but combine it with additional sources to reduce the probability of measurement error.

Public Education Resources. To overcome the external validity limitations of extant research, I use official education statistics not previously digitized to construct a new longitudinal dataset containing annual data from 1959-1990 on the three main measures of public education resources examined in the extant literature—student-teacher ratios,[11]

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[10] Frandsen (2016) attempts to overcome this limitation with CPS data on teachers' salaries aggregated at the state level and beginning in 1962. However, the CPS is designed to provide a representative sample of households at the national level, not of teachers at the state level.
[11] Number of full-time-equivalent teachers employed in public schools divided by number of students enrolled in public schools in that state.

average teacher salaries,[12] and per-pupil current operational expenditures[13]—plus a fourth measure of interest, per-pupil non-wage current expenditures, which includes employer contributions to retirement systems or social security, administrative costs, and expenditures for miscellaneous school services. Additionally, the dataset includes decennial information on per-pupil current expenditures since 1919 and teacher salaries since 1939, enabling me to assess the role of mandatory bargaining laws in historical perspective—something not done previously. All measures are aggregated at the state level because the treatment of interest is the passage of state mandatory bargaining laws, which are what politicians can influence most directly. For 1959-1985, I hand-entered the data from thirty-three reports published by the U.S. Department of Education (DOE) under the following series: *Statistics of State School Systems*; *Statistical Summary of State School Systems*; *Fall Statistics of Public Schools: Pupils, Teachers, Instruction Rooms, and Expenditures*; and *Fall Statistics of Public Elementary and Secondary Day Schools*. These reports are the predecessors of the Common Core of Data (CCD), which today constitutes the DOE's primary database on K-12 education. CCD data from 1986-1990 were downloaded from the DOE's website. Further details about the dataset are included in Online Data Appendix.

Mandatory Collective Bargaining Laws. For each state, the NBER Public Sector Collective Bargaining Law Dataset (Valetta and Freeman 1988) provides annual data beginning in 1959 on whether collective bargaining with teachers was required, allowed but not required, or not allowed. I used this dataset as the starting point to identify whether or not a mandatory collective bargaining law was in place in each state at the beginning of each school year (which is when the dependent variables are measured), but in order to minimize measurement error on the timing of mandatory collective

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[12] Total amount paid in salaries to all public-school teachers divided by number of teachers in a state. Values are CPI-adjusted and expressed in 2010 dollars.

[13] Total current operational expenditures (regardless of funding source) that are devoted to public schools in a state divided by number of public-school students in that state. Values are CPI-adjusted and expressed in 2010 dollars.

bargaining laws, I cross-checked the NBER dataset with at least two additional sources per state: the *Negotiation Research Digest* (NEA 1972) and Saltzman's (1982) Ph.D. dissertation. Whenever the timing of the laws coincides across all three sources, I assume the information they provide is correct. When the information from at least one source is missing, or when they do not all coincide—typically because of disagreements on whether a statute implies a duty to bargain—, I use additional state-specific sources, such as law journals and information from states' Public Employment Relations Boards. Of the thirty-three states that adopted mandatory bargaining laws, the final coding coincides with Valetta and Freeman (1988) in twenty-four cases; with NEA (1972) and Saltzman (1982) in six cases; and with information from state Public Employment Relations Boards in three cases. All sources and coding decisions are detailed in the Online Data Appendix.

Strike Penalties. To test the proposed mechanism, I classified mandatory bargaining laws by whether they established credible strike penalties or not. Following the earlier discussion, a state law was considered not to establish credible penalties if it established no penalties whatsoever or if it only established penalties that a government wanting to end a shutdown could not enforce (i.e., dismissal and/or jailing). A law was considered to establish credible penalties if it established any monetary or organizational penalties for striking (e.g., wage loss, fines, union decertification, suspension of CBAs, and/or suspension of automatic union dues deduction).

Differences in Spending Precede Collective Bargaining

I begin by inspecting the historical evolution of public education resource levels across states from 1959-1990, grouping states by whether in 1990 they required collective bargaining, allowed but did not require it, or did not allow it. The analysis, shown in Figure 2, reveals that in 1990, many more resources were devoted to education in states that required collective bargaining than in states that did not: more teachers were

employed per student; salaries were 18% higher; per-pupil current expenditures, 29% higher; and per-pupil non-wage current expenditures, 32% higher. However, *these differences were already present in 1959*—before collective bargaining rights emerged.

Were more resources devoted to education in 1959 in states that eventually required collective bargaining because of the presence of stronger teacher unions in those states even prior to formal bargaining? To assess this, I examine salary data from 1939, when less than 5% of teachers belonged to unions; and per-pupil current expenditures data from 1919, when the AFT was barely beginning to operate and the NEA was dominated by school administrators, not teachers. Figure A4 reveals that expenditures in 1919 were 68% higher, and salaries in 1939, 41% higher, in states that eventually required collective bargaining. That is, the differences predate both the introduction of bargaining rights *and* the existence of modern teacher unions.

The differences are also not just capturing differences between non-Southern and Southern states.[14] To see this, we can leverage the fact that, within the South (but not within the non-South), there is considerable variation in the legal status of collective bargaining: five states required it by 1990, six allowed it but did not require it, and five did not allow it. When we restrict the analysis to Southern states, the patterns remain: states that by 1990 required collective bargaining had lower student-teacher ratios, higher teacher salaries, and higher education expenditures well before they required bargaining (Figure A2 and Table A3).

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[14] I follow the extant literature and use the Census definition of South.

Figure 2. Average Level of Resources Devoted to Public Education in 1959-1990, by Legal Status of Collective Bargaining in 1990
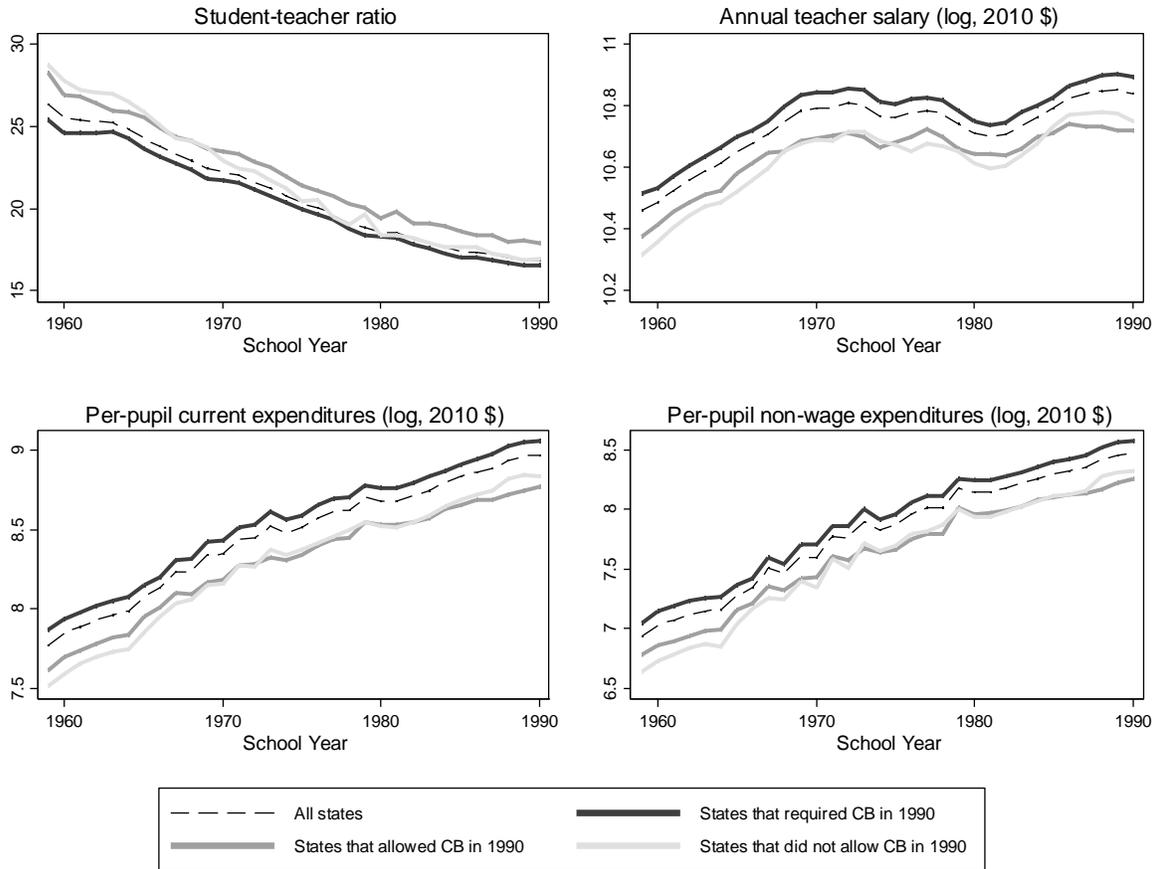
One possible interpretation for why the historical differences depicted in Figure 2 did not widen over time as mandatory bargaining laws were introduced is that the laws' passage in some states led other states to match the material improvements granted to teachers to prevent unionization. However, three observations suggest this interpretation may not be quite right. First, no sharp breaks in trend took place in states that passed mandatory bargaining laws (Figure 3 and Figure A3), so it is unclear why states that did not introduce these laws would have felt compelled to make special concessions to teachers. Second, the trends of states that eventually introduced mandatory bargaining and states that did not were parallel throughout the entire period from 1959 through 1990. If threat effects had been in place, the non-bargaining states should have followed

the bargaining states not simultaneously but with a lag. Third, the more likely reason why the trends of these different groups of states moved closely together is that they responded similarly to common economic shocks, as suggested by the fact that their trends moved together even before the 1960s, when public-sector bargaining was inconceivable (Figure A4).

Overall, Figure 2 illustrates why we cannot assume that the current differences in public education resources between states that require collective bargaining and states that do not were *caused by* bargaining. States that were historically inclined to devote more resources to education were eventually more likely to require collective bargaining with teachers. Cross-sectional comparisons that do not account for these historical differences will likely overestimate the impact of collective bargaining rights.

Still, perhaps mandatory bargaining laws exacerbated these pre-existing differences— a possibility that the preceding graphs cannot rule out. To determine the laws' impact, we can examine the evolution of public education resources within states that passed mandatory bargaining laws, to control for time-invariant state characteristics that may be correlated with both the level of resources and the passage of these laws, while also netting out the influence of secular forces that affected all states regardless of the presence of these laws. Longitudinal data enable me to do this.

Collective Bargaining Laws Did Not, On Average, Increase Spending

To assess the effect of mandatory collective bargaining laws on the level of public education resources among states that passed these laws, I estimate the following non-parametric difference-in-differences model:

$$(1)\ Y_{s,t} = \gamma_s + \phi_t + \sum_{n=-6}^{10} \beta_n I_{s,t}^n + \epsilon_{s,t}$$

$Y_{s,t}$ is an outcome of interest (student-teacher ratios, log of real teacher salaries, log of real per-pupil current expenditures, or log of real per-pupil non-wage current

expenditures) in state s at the beginning of school year t; γ_s are dummies that account for time-invariant state characteristics;[15] ϕ_t are dummies that account for year fixed effects; and $I_{s,t}^n$ takes a value of 1 if state s at the beginning of school year t is n years away from the coming into effect of a mandatory bargaining law, and 0 otherwise.[16] For any given n, β_n is the average difference in Y between states that were n years away from passing a mandatory bargaining law and states that at that time were not subject to a law, net of the state and year fixed effects. Under the identifying assumption that this difference would have been zero in the post-treatment period had a mandatory bargaining law not been passed, then the β_n parameters for $n \geq 0$ represent the average effect of a law n years after its passage. If mandatory bargaining laws led to an increase in the level of public education resources, then we should find clear evidence that $\beta_n > 0$ for $n \geq 0$.

This model, similar to Lovenheim's (2009), has three main advantages over a standard (linear) difference-in-differences model. First, it does not require an assumption about the shape of the trends. Second, it assesses the impact of the laws over time, allowing for gradual effects. Third, it enables us to assess the plausibility of the identifying assumption by testing whether the pre-treatment trends of the treatment and control groups were parallel. If they were, β_n should equal zero for all $n < 0$, but if there was selection into the treatment based on pre-treatment outcomes, we would find that $\beta_n \neq 0$ for some $n < 0$.

Indeed, we can take advantage of this feature of the model to make an informed decision about how to construct the counterfactual trends. In the main analysis, I estimate the counterfactual at post-treatment time n using information only from states

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[15] Time-varying variables may be endogenous to the introduction of the laws and therefore do not enter the model.

[16] Because the dependent variables are measured at the beginning of each SY, during September and October, I define the timing of the treatment as the first SY in which a mandatory bargaining law was already effective on September 1st.

that later did pass such a law but that at time n had not yet done so. This approach is informed by prior research showing that which states adopted a law was not random, but that among those that did pass a law, the specific timing of adoption was driven by idiosyncratic factors (Farber 1988; Saltzman 1988).[17] Specifically, I estimate equation 1 with data from thirty-two of the thirty-three states that ever adopted a mandatory bargaining law.[18,19] Alternatively, we could construct the counterfactual using information from *all* states that at time n did not have a mandatory bargaining law, including also states that never passed a law. With this approach, the results reported below hold, but there is some evidence of non-parallel pre-treatment trends (Figures A5-A6).[20]

Baseline Results

Figure 3 preempts the main findings, displaying, in black, the average level of public education resources among states that passed mandatory bargaining laws in the six years before and the ten years after their passage; and, in grey, the average control group trend.[21] Both trends moved together before and after the introduction of mandatory bargaining laws. If the laws, and the increase in CBAs they triggered, had led to an increase in teacher employment, salaries, or expenditures, we should observe a clear and permanent divergence in these trends after the laws were introduced. That we do not suggests the laws did not increase the level of resources devoted to education.

Consistent with this visual evidence, the difference-in-differences estimates do not support the claim that mandatory bargaining laws led to generalized increases in the number of teachers per student, teacher salaries, per-pupil current expenditures, or per-

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[17] Because I estimate the average treatment effect on the treated (ATT), selection into mandatory bargaining laws based on expectations of how such laws might impact the outcomes of interest will not bias the causal estimates.

[18] Wisconsin is excluded because only one year of pre-treatment data are available.

[19] I use a window of six pre-treatment years to maximize the number of states that remain in the analysis, and ten post-treatment years to detect if there were non-immediate effects.
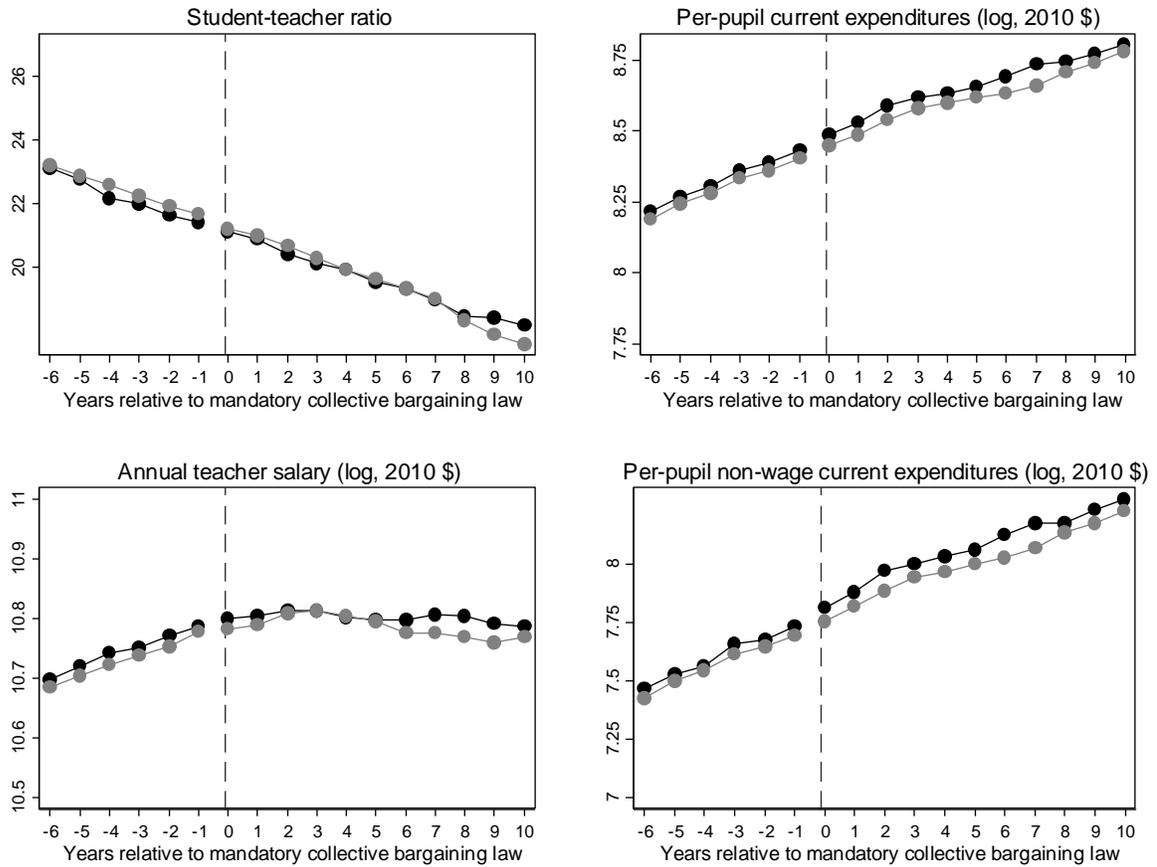
[20] This alternative appproach would probably underestimate the impact of the laws because never-treated states benefitted disproportionately from federal funding for education under the Elementary and Secondary Education Act of 1965.

[21] For each treated state in each year, the control group consists of states that had not yet passed a mandatory bargaining law in that year but that did so later.

pupil non-wage current expenditures. Figure 4 plots the β_n coefficients from equation 1 and 95% confident intervals with standard errors clustered at the state level. First, none of the β_n coefficients for $n < 0$ are statistically different from zero, which combined with the visual evidence in Figure 3 helps support the identifying assumption that states that introduced mandatory bargaining laws early on would have had, absent those laws, similar trends in outcomes as states that were not yet subject to a mandatory bargaining law.

Further, none of the β_n coefficients for $n \geq 0$ are statistically different from zero. Some coefficients have the opposite sign of what dominant theories predict, pointing to increases in student-teacher ratios and decreases in salaries after the passage of mandatory bargaining laws. The results echo Lovenheim (2009) and Frandsen's (2016) null findings, and extend their generalizability to all states.

Figure 3. Average Level of Resources Devoted to Public Education Before and After Mandatory Collective Bargaining Laws, in Treatment and Control Groups, 1959-1990

Student-teacher ratio

Per-pupil current expenditures (log, 2010 $)

Annual teacher salary (log, 2010 $)

Per-pupil non-wage current expenditures (log, 2010 $)

NOTE: Black trend for treated group; grey for control (states not yet subject to a mandatory bargaining law).

Figure 4. Non-parametric Difference-in-Differences Effect of Mandatory Collective Bargaining Laws on the Level of Resources Devoted to Education, 1959-1990
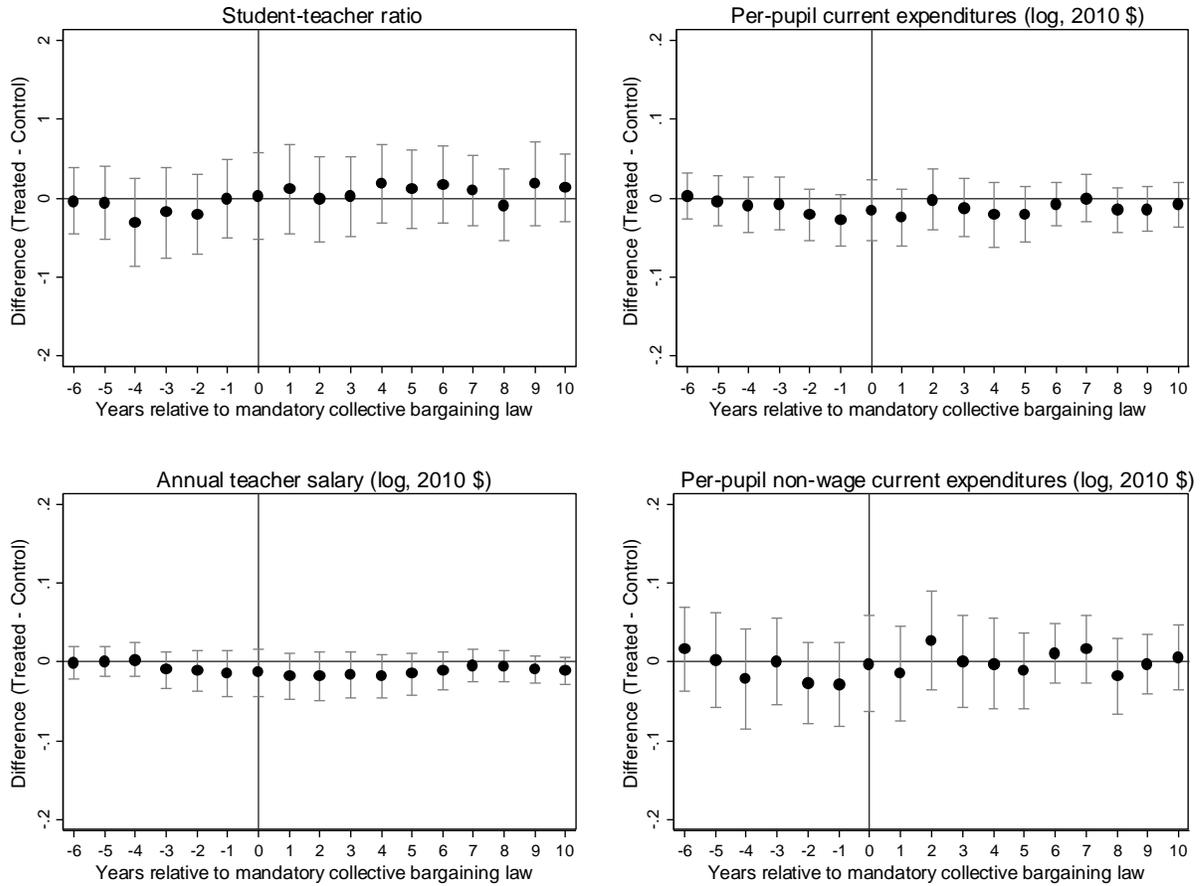
NOTE: β_n coefficients from equation 1 and 95% confidence intervals (standard errors clustered at state level).

Robustness

The null findings are not driven by a lack of power: the confidence intervals are narrow and centered around zero. Still, we can estimate a more parsimonious linear difference-in-differences model that is more likely to identify significant effects:

$$(2)\ Y_{s,t} = \gamma_s + \phi_t + \delta T_{s,t} + \epsilon_{s,t}$$

$Y_{s,t}$, γ_s and ϕ_t are defined as before; $T_{s,t}$ takes a value of 1 if a mandatory bargaining law was effective in state s at the beginning of school year t, and a value of 0 otherwise.

The identifying assumption is the same as before, but δ provides a single measure of the average effect of bargaining laws.

The results again provide evidence against the claim that the introduction of mandatory bargaining laws led to increases in the level of education resources (Table 2). This is true for all four dependent variables. Again, most coefficients' signs imply effects in the opposite direction and the confidence intervals are small. Adding a state-specific linear time trend to control for time-varying state characteristics also yields precisely-estimated zero coefficients (Table A6).

Table 2. Linear Difference-in-Differences Effect of Mandatory Collective Bargaining Laws on the Level of Resources Devoted to Education, 1959-1990

	Dependent variable			
	Student-teacher ratio	Average teacher salary (log, 2010$)	Per-pupil current expenditures (log, 2010$)	Per-pupil non-wage current expenditures (log, 2010$)
	(1)	(2)	(3)	(4)
CB required at the beginning of the SY	0.389	-0.028 *	-0.021	0.006
	(0.2637)	(0.0133)	(0.0159)	(0.0210)
	[-0.148; 0.927]	[-0.055; - 0.001]	[-0.053; 0.012]	[-0.037; 0.048]
Constant	24.498 **	10.529 **	7.936 **	7.135 **
	(0.2893)	(0.0113)	(0.0168)	(0.0230)
Observations	1248	1248	1248	1248
Adj. R-Square	0.88	0.759	0.954	0.949
No. of clusters	32	32	32	32

NOTE: Equation 2 estimates. All regressions include state and year fixed effects. Standard errors clustered at state level in parentheses; 95% confidence intervals in brackets. Stars denote statistical significance at the *0.05 or **0.01 level.

The null results are also unlikely to be driven by weak compliance with mandatory bargaining laws by districts, because compliance was high (Saltzman 1982; Flavin & Hartney 2015): within five (ten) years of a state passing a mandatory bargaining law, 85% (99%) of teachers in that state were covered by a collective bargaining agreement (Figure 5). Using mandatory bargaining laws as an instrument for CBA coverage, we can estimate the effect of increasing the share of teachers covered by a CBA from 0 to

100%. The results provide little support for the claim that collective bargaining increases the size of government: the coefficients for student-teacher ratios and salaries have the opposite sign; and while the coefficients for expenditures are positive, they do not reach statistical significance (Table A7). Finally, it is unlikely that the absence of positive average effects in Table 2 reflects a redistribution of resources within mandatory-bargaining states, from districts in that state that did not unionize to districts in the same state that did. Suppose that the salaries of teachers who became covered by a CBA as a result of a mandatory bargaining law increased by 5% (10%); for this assumption to be true, given the average effect of -2.8%, the salaries of teachers who did not unionize would have had to *decline by 29% (39%)*—an implausible scenario.

Figure 5. Average Percentage of Teachers Covered by CBAs Before and After Mandatory Collective Bargaining Laws, in Treatment and Control Groups, 1958-1977
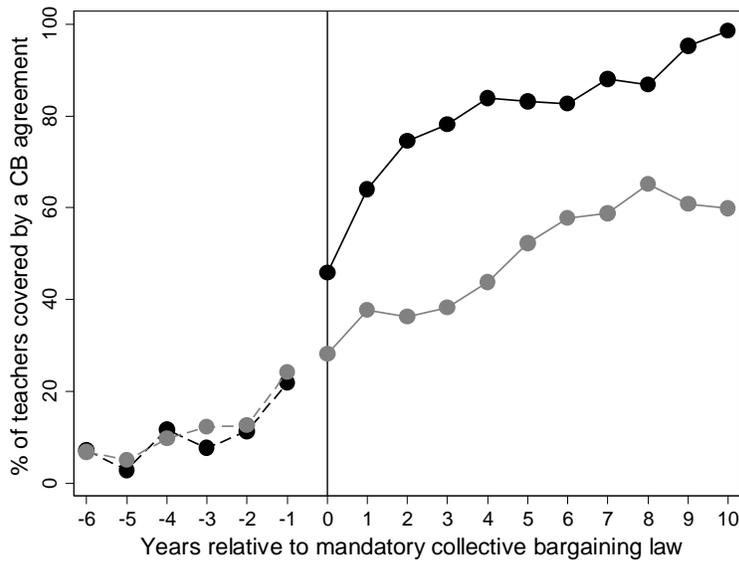
NOTE: Black trend for treated group; grey for control (states not yet subject to a mandatory bargaining law). Data on CBA coverage are from Saltzman (1982).

Protection against Shocks?

Perhaps collective bargaining rights serve not to increase material benefits but to protect existing jobs and benefits during adverse shocks. I assess the plausibility of this argument by exploring whether the adjustment of public education resources during the 1980-82 recession—back then, the most severe recession since the Great Depression—varied depending on the legal status of collective bargaining. Specifically, I estimate:

$$(3)\ Y_{s,t} = \gamma_s + \sum_{n=-10}^{10} \phi_n I_t^n(year - 1980 = n) + \sum_{n=-10}^{10} \beta_n I_t^n(year - 1980 = n).T_s + \epsilon_{s,t}$$

$Y_{s,t}$ and γ_s are defined as before; I_t^n is an indicator variable that takes a value of 1 in year t if t is n years away from the onset of the 1980 crisis, and a value of 0 otherwise; and T_s equals 1 if state s had a mandatory bargaining law before 1980, and 0 if it never passed such a law. The β_n coefficients indicate whether there was a difference in the trend of mandatory-bargaining and non-mandatory-bargaining states before (β_n for $n < 0$), during ($\beta_0, \beta_1, \beta_2$), and after ($\beta_n$ for $n \geq 3$) the recession. If mandatory bargaining laws protect the status quo, then during the recession (for $0 \leq n \leq 2$) we should see teacher dismissals ($\phi_n^{student-teacher\ ratio} > 0$) and/or reductions in salaries and education expenditures across the board ($\phi_n^{salaries;\ expenditures} < 0$), but less so in mandatory-bargaining states ($\beta_n^{student-teacher\ ratio} < 0; \beta_n^{salaries;\ expenditures} > 0$).

The results provide no evidence of a differential response to the recession in states that required collective bargaining versus states that did not (Table A8). Consistent with Freeman and Han's (2012) analysis of state budgets during the Great Recession, these findings cast doubt on the argument that collective bargaining provides job protection during tough times.

Mechanism

Why did mandatory bargaining laws not lead to a generalized increase in the level of education resources? I argue that one reason lies in the fact that most laws contained pro- *and anti-*union provisions, including among the latter provisions that raised the cost of striking. To test this argument, I estimate a difference-in-differences model that allows for heterogeneous treatment effects depending on whether mandatory bargaining laws established credible strike penalties or not. Specifically, I re-estimate equation 2 adding an interaction term between $T_{s,t}$ and a dummy variable $NoCrediblePenalties_s$ that equals 1 if state *s* introduced a mandatory bargaining law that did not establish credible strike penalties:

$$(4)\ Y_{s,t} = \gamma_s + \phi_t + \delta T_{s,t} + \alpha T_{s,t}.NoCrediblePenalties_s + \epsilon_{s,t}$$

Here, δ is the effect of mandatory bargaining laws among states where the laws established credible strike penalties, and $(\delta + \alpha)$ is the effect among states where the laws did *not* establish such penalties.

The results provide support for the proposed theory (Table 3). Mandatory bargaining laws that did not establish credible strike penalties led to increases in per-pupil expenditures (+2.6%). Fourteen of thirty-three laws fall in this category. The remaining nineteen laws established new penalties designed to raise the cost of striking. The results suggest these laws had no effect on student-teacher ratios or non-wage expenditures, and led to *declines* in teacher salaries (-4.3%) and total expenditures (-4.1%).

Table 3. Heterogeneous Difference-in-Differences Effect of Mandatory Collective Bargaining Laws, by Type of Strike Penalty, 1959-1990

	Dependent variable						
	Student-teacher ratio		Average teacher salary (log, 2010$)		Per-pupil current expenditures (log, 2010$)		Per-pupil non-wage current expenditures (log, 2010$)
CB required at beginning of SY	0.509		-0.043 **		-0.041 *		-0.010
	(0.3344)		(0.0148)		(0.0197)		(0.0248)
CB required at beginning of SY * No credible strike penalties	-0.389		0.048 *		0.067 *		0.050
	(0.5757)		(0.0183)		(0.0324)		(0.0392)
Constant	24.200 **		10.659 **		8.079 **		7.275 **
	(0.2356)		(0.0100)		(0.0157)		(0.0273)
Observations	1248		1248		1248		1248
Adj. R-Square	0.881		0.767		0.955		0.950
No. of clusters	32		32		32		32

NOTE: Equation 4 estimates. Standard errors clustered at state level in parentheses. Stars denote statistical significance at the *0.05 or **0.01 level.

These results raise a puzzle: why did teachers demand collective bargaining rights if, at best, they led to small increases in expenditures? While addressing this question is beyond the scope of this article, we can suggest some explanations. First, observing that states with bargaining rights had higher salaries than those without them, teachers may have concluded that the relationship was causal. Second, the introduction of collective bargaining rights led to increases in union membership and in teachers' capacity for political activism. That alone may have been sufficient reason to want bargaining rights. Finally, teachers may have demanded these rights not just to pursue material ends but also in search of procedural fairness (Goldfield & Bromsen 2013). In 1961-62, only 21% of school districts followed systematic promotion procedures, and only 31% had formal grievance procedures (NEA 1963). CBAs appear to have reverted this: about 90% of

contracts negotiated between 1967-1971 established such procedures (NEA 1968, 1970, 1971a, 1971b). Whether such grievance procedures reduced patronage, protected underperforming teachers, led districts to recruit and retain better teachers, or had no impact on teacher quality remains an important empirical question.

Summary & Implications

How does granting collective bargaining rights to public employees impact the size of government? Using an original longitudinal dataset that encompasses all U.S. states before and after they passed laws mandating districts to engage in collective bargaining with teachers, and leveraging variation in the timing of these laws across states, this article shows that: (1) states where districts are required to bargain with unions have lower student-teacher ratios, higher teacher salaries, and higher expenditures than states where bargaining is not required, but the differences *precede* collective bargaining rights or modern teacher unions; (2) the introduction of mandatory collective bargaining laws did not, on average, increase the level of resources devoted to education; (3) many mandatory bargaining laws contained provisions designed to limit unions' ability to strike; (4) laws that did not contain these provisions did lead to increased education spending; and (5) even then, the impact was small in historical perspective.

The results have implications for several literatures in American and comparative politics. First, they challenge mainstream theories about public-sector unions. Much research argues that collective bargaining is a key mechanism by which public-sector unions increase the size of government (e.g., Peterson 1976; Levi 1977; Freeman 1984; Shefter 1985; Swenson 1991a; Moe 2006; Anzia & Moe 2015), but few studies examine this argument with data and methods that warrant causal claims. The findings suggest that for teacher unions—the largest unions in the U.S.—, collective bargaining is not as effective a mechanism to increase the size of government as commonly argued: sometimes it is, but this is not the norm. One reason is that the collective bargaining

process is regulated by state laws that, instead of being "pro-labor" as past theories assume (e.g., Levi 1977; Saltzman 1985; DiSalvo 2010; Moe 2009, 2011; Anzia & Moe 2015), often contain both pro- *and anti*-union provisions. Taking these laws' content into consideration, the article identifies a set of empirically-substantiated conditions under which public-sector unions are more likely to obtain what they want. It shows that collective bargaining is more likely to increase the size of government when labor laws do not limit unions' ability to strike.

Even if collective bargaining is not a key mechanism by which unions influence the size of government, unions may influence governments' payroll and budget through other mechanisms, including electoral mobilization and legislative lobbying (Levi 1977; Moe 2006). There is direct evidence that teacher unions engage in these activities (Moe 2011; Hartney & Flavin 2011; Flavin & Hartney 2015), and some evidence suggests that unions' electoral mobilization and lobbying activities help boost salaries and spending (Berkman & Plutzer 2005; Anzia 2011). Additionally, teachers mobilize politically to pursue a broad range of education policies and to advance other social policies related, for instance, to civil rights, healthcare, or immigration.[22] A question that emerges from this study is whether public-sector unions became politically active *because* their collective bargaining power was limited.

In many ways, the preceding findings underscore how much room there is to expand our understanding of public-sector unions—what they do, where their power stems from, and with what effects on policy. What other factors besides the content of state labor laws affect unions' power during collective bargaining? Why did teachers demand bargaining rights if their impact on jobs, salaries, and spending was, at best, small? What would we conclude about public-sector unions' impact on other outcomes (e.g.,

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[22] See http://www.nea.org/home/18526.htm

quality of service provision, income inequality, policy responsiveness, pension or health benefits) if we had longitudinal data on these outcomes?

Second, while much research on the politics of education focuses on teacher unions' influence (e.g., Murillo 1999; Hecock 2006; Moe 2009, 2011; Hartney & Flavin 2011), the findings presented underscore the importance of future research on other actors and on more historical determinants of current education policy and provision. They show that even in those cases where mandating bargaining with teachers led to greater education spending, the bulk of the differences in spending we see today date back at least to the early-twentieth century and thus precede modern teacher unions.

Third, the article contributes to a growing literature that studies the question of who shapes policy in democratic regimes by paying attention to specific policies and to the role organized interest groups in shaping these policies (Hacker & Pierson 2014). In the case of U.S. public-sector collective bargaining laws, their inclusion of pro- and anti-union provisions supports the view that policies are often shaped by multiple competing interests (Dahl 1961; Schickler 2001; Swenson 2002, 2004) rather than being primarily shaped by either business or labor organizations. Accordingly, the same policy can benefit a group in some respects but limit it in others (e.g., strengthen unions' capacity for electoral mobilization but limit their strike power). That no single interest group dominates policymaking is consistent with the well-documented prevalence of the status quo (Gilens & Page 2014). What this article shows is that the status quo can prevail in democracies not only because of the difficulty to agree on policy reforms but also because new laws that look like reform on paper may help institutionalize the status quo of *outcomes*. For instance, new labor laws were introduced, but the interests that shaped them—with some groups pushing for bargaining rights and others, for strike penalties—led to laws that helped sustain the status quo of fiscal outcomes in education.

Fourth, the article highlights the value of historical approaches in political economy research. Part of the value is methodological, as historical datasets can help disentangle the impact of institutions that were not introduced at random. But equally important is the theoretical value of considering the history behind institutional reforms to understand their consequences—a perhaps obvious point that nonetheless is largely absent in past theories of how public-sector labor laws impacted the size of government.

Finally, the findings have implications for policy debates. The evidence presented challenges both conservatives and liberals who attack or defend teachers' collective bargaining rights arguing that they "raise the costs of" or "improve investments in" education. Republicans and Democrats may have political and ideological reasons to undo or maintain these rights, but arguments that rely on an economic rationale lack strong empirical evidence to support them.

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Online Appendix: Supplementary Graphs and Tables

Figure A1. Number of States with Mandatory Collective Bargaining Laws and % of Teachers Covered by a Collective Bargaining Agreement, 1959-1995
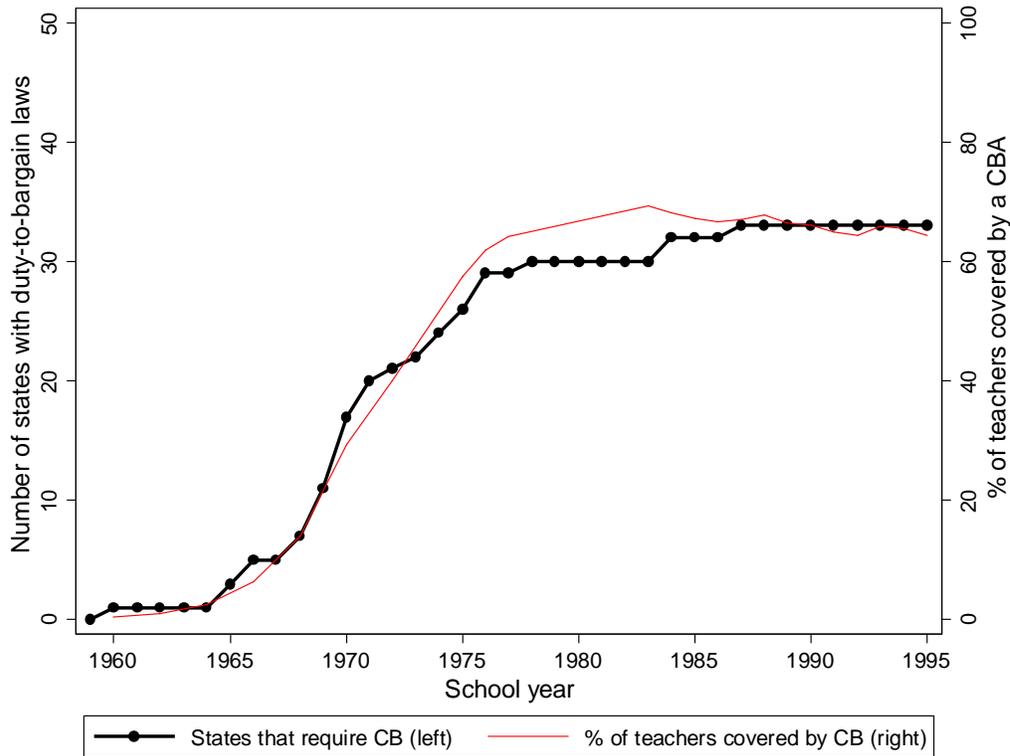
NOTE: Data on collective bargaining coverage from Saltzman (1982).

Figure A2. Average Level of Resources Devoted to Public Education in 1959-1990, by Legal Status of Collective Bargaining in 1990 – Southern States Only

Student-teacher ratio

Annual teacher salary (log, 2010 $)

Per-pupil current expenditures (log, 2010 $)

Per-pupil non-wage expenditures (log, 2010 $)

– – – All states

States that required CB in 1990

States that allowed CB in 1990

States that did not allow CB in 1990
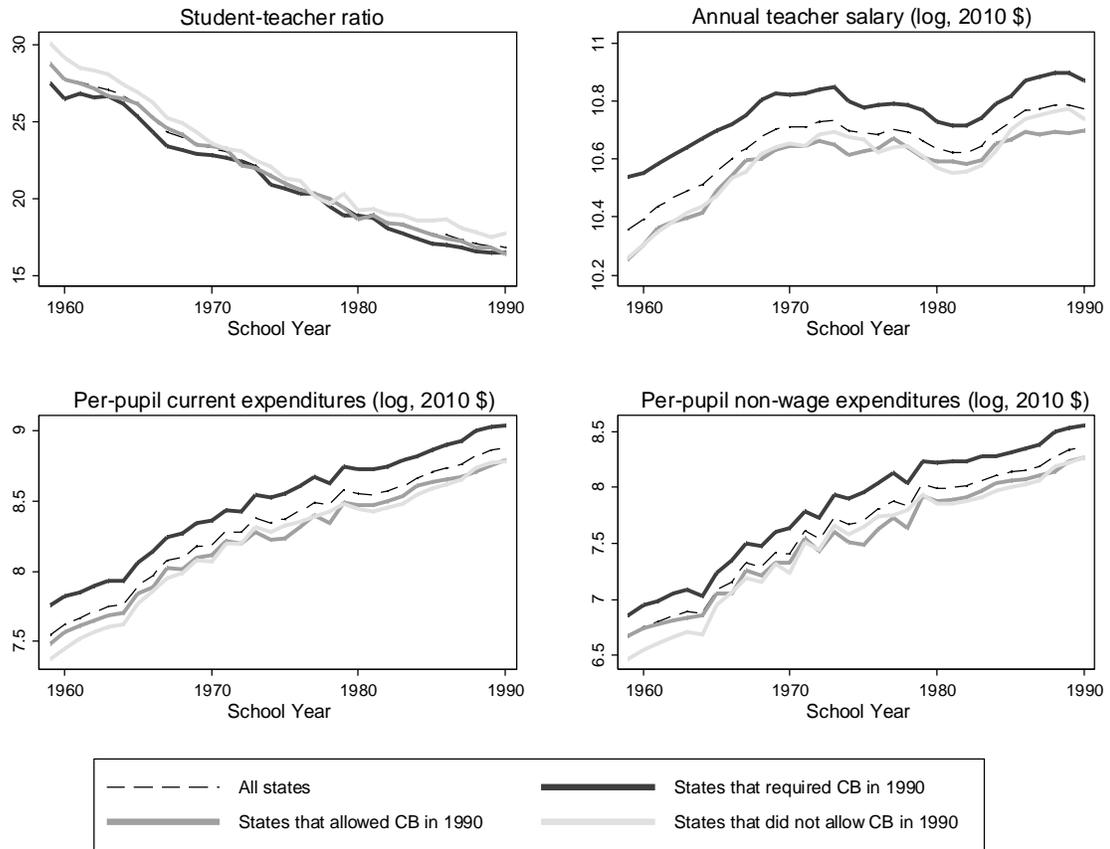
Figure A3. Resources Devoted to Public Education Before and After the Introduction of a Mandatory Collective Bargaining Law, by State, 1959-1990
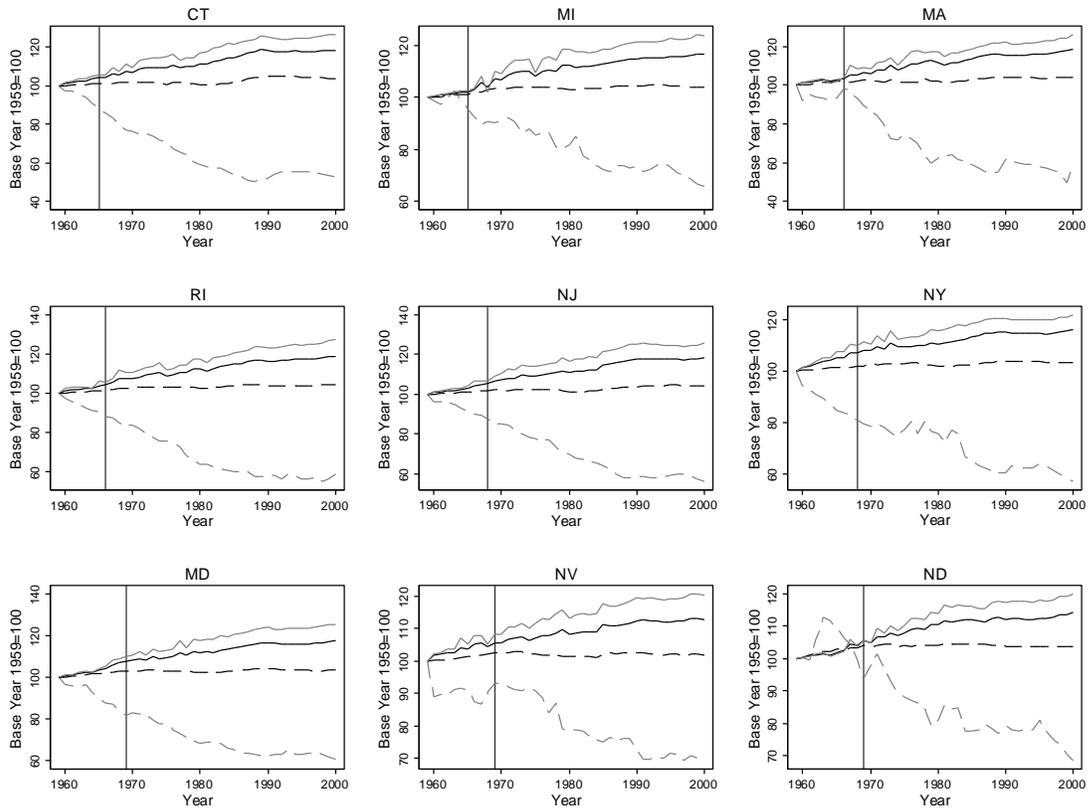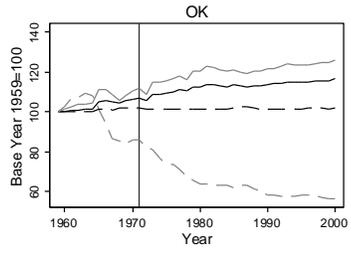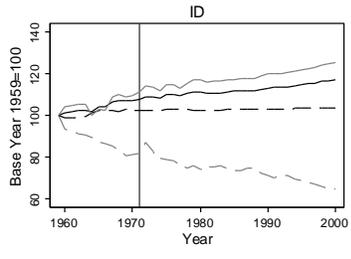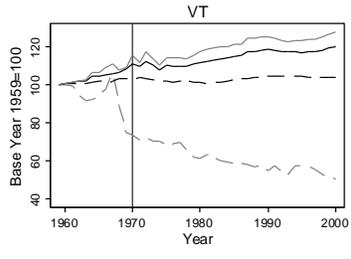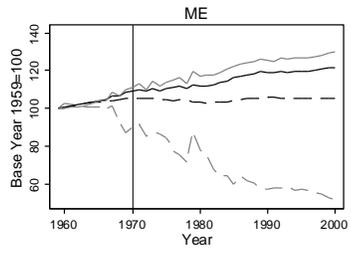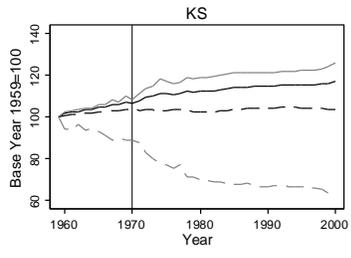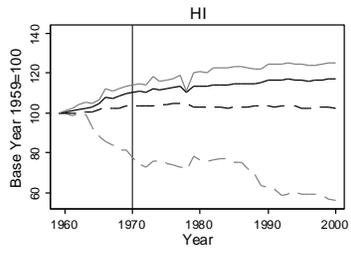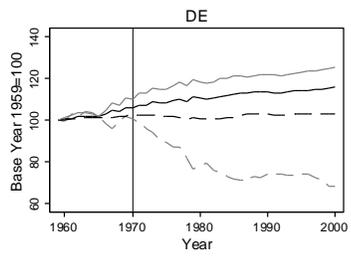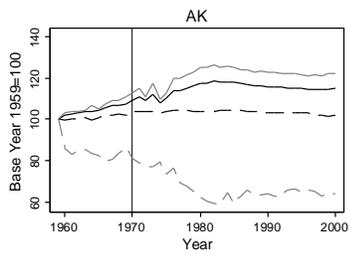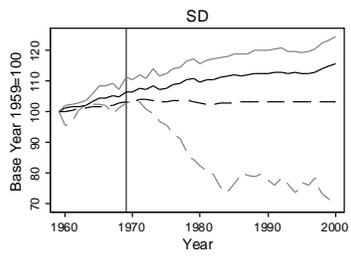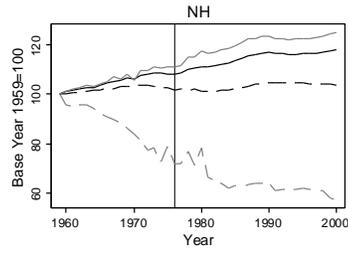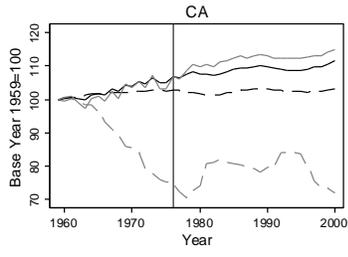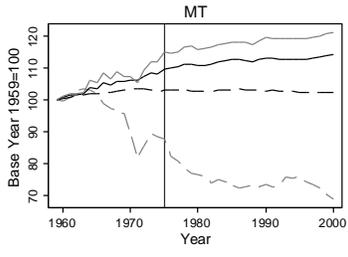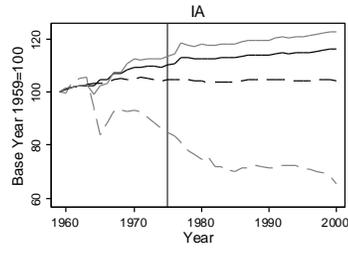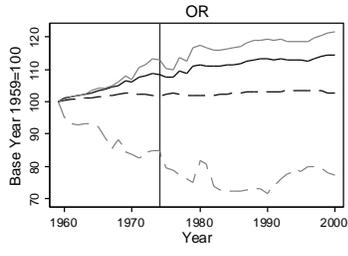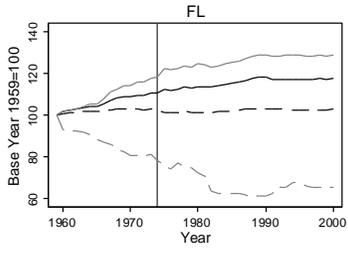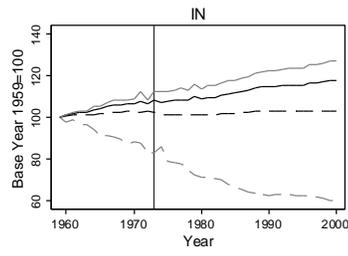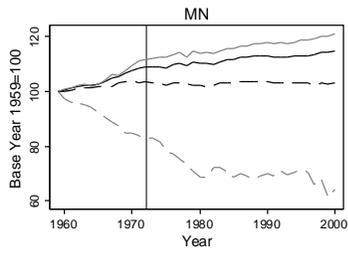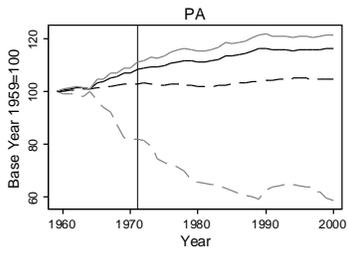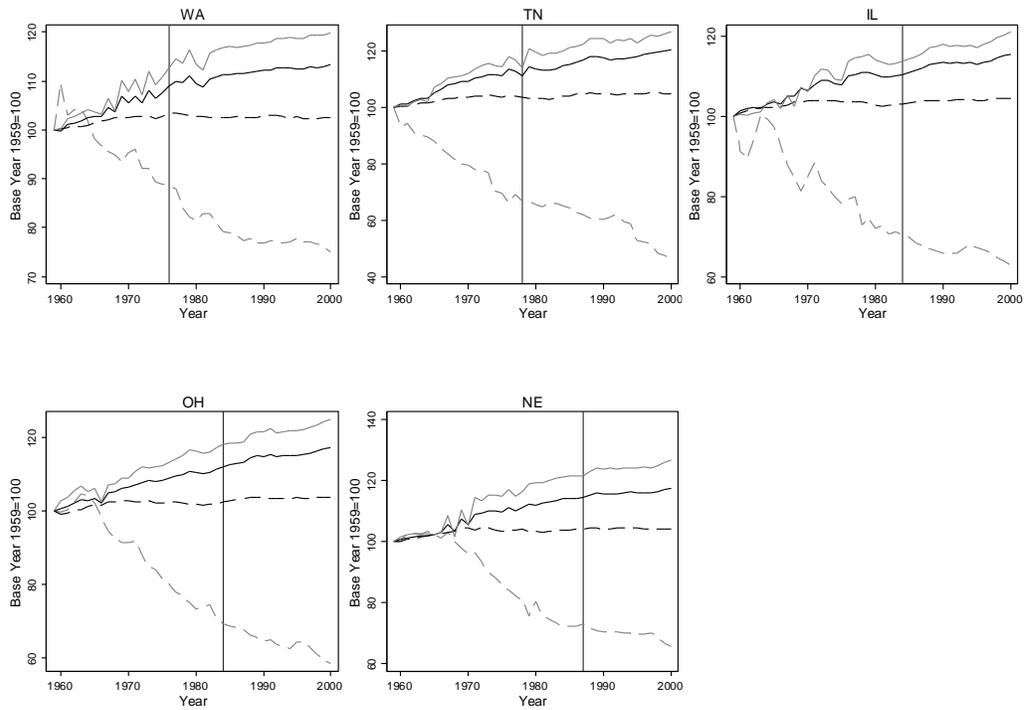
NOTE: Solid black lines indicate per-pupil current expenditure in education; solid grey line, per-pupil non-wage current expenditures; dashed black lines indicate average annual teacher salaries, and dashed grey lines, student-teacher ratios, all with respect to the base (beginning) year, 1959. The vertical lines indicate the first school year in which a duty-to-bargain law was effective in the state.

Figure A4. Average Salaries and Per-Pupil Current Expenditures in 1919-1990, By Legal Status of Collective Bargaining in 1990
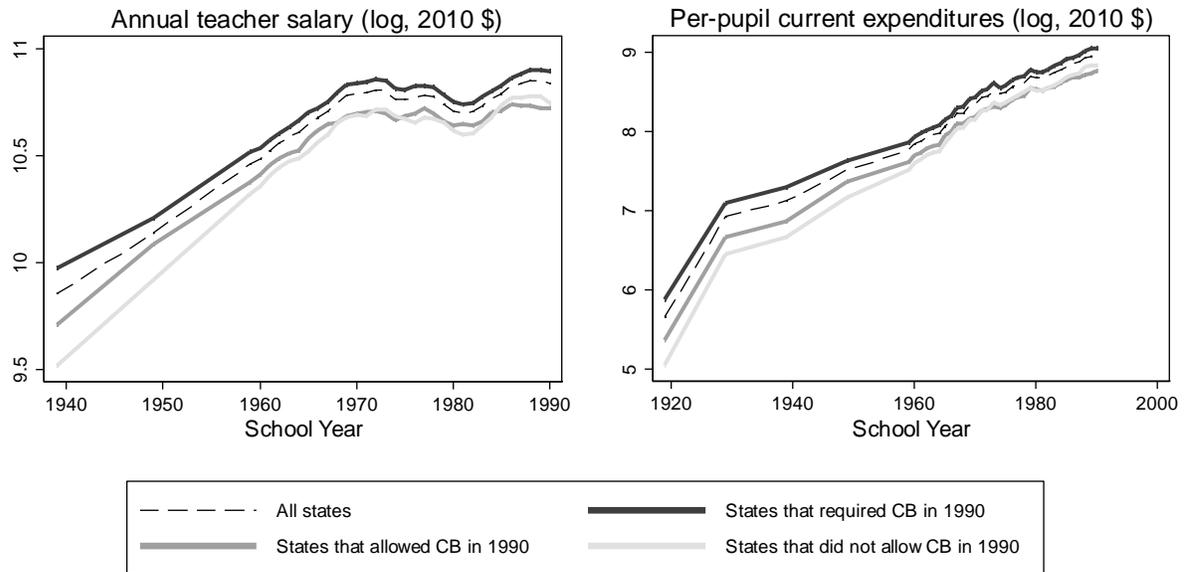
Figure A5. Non-Parametric Difference-in-Differences Effect of Mandatory Collective Bargaining Laws on the Level of Resources Devoted to Education, 1959-1990 – using information from all fifty states
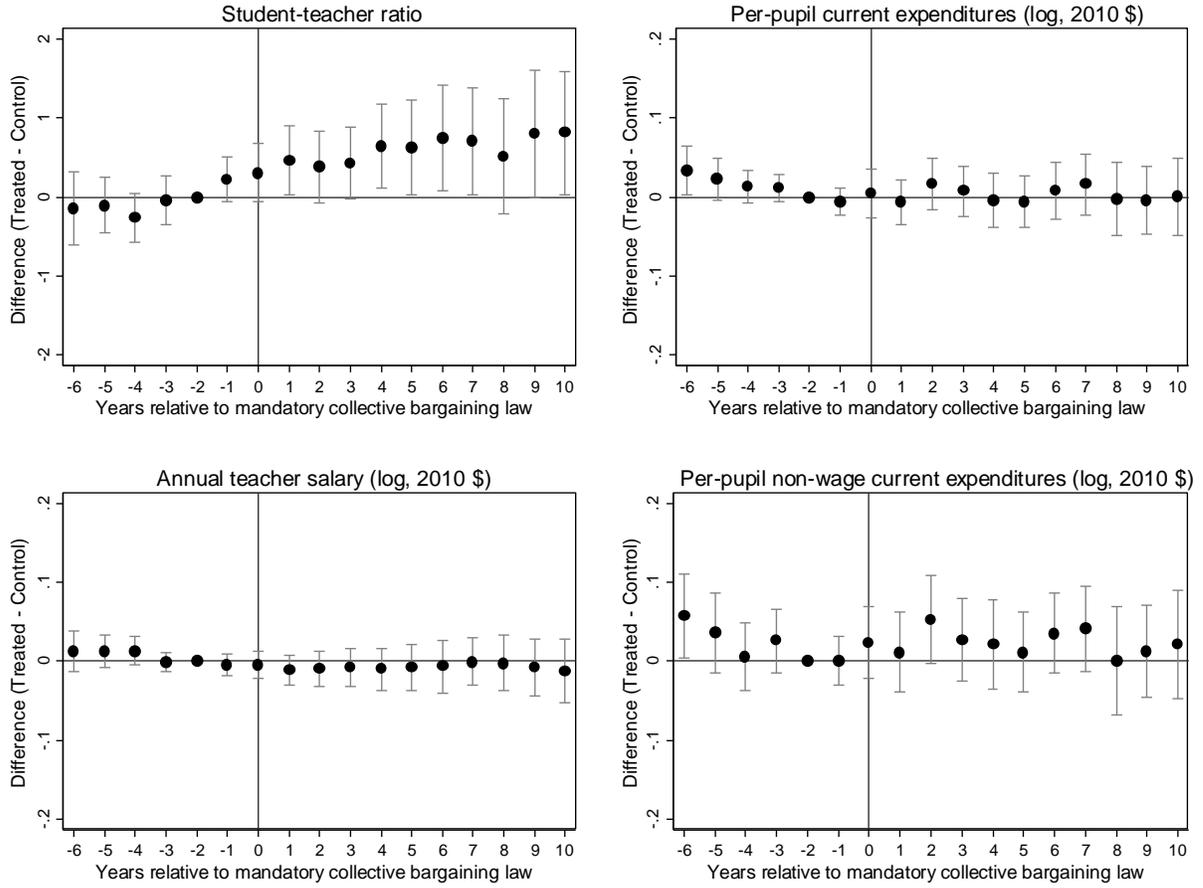
NOTE: the estimated positive effect on student-teacher ratios implies that there was a negative effect on the number of teachers per student.

Figure A6. Average Percentage of Teachers Covered by CBAs Before and After Mandatory Collective Bargaining Laws, in Treatment and Control Groups, 1958-1977 – using information from all fifty states
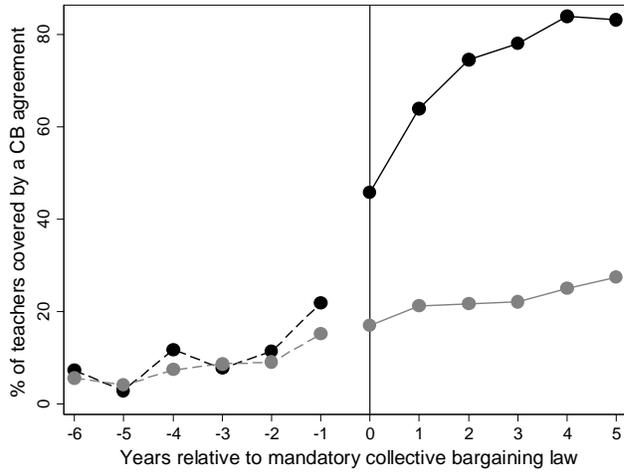
NOTE: Black trend for treated group; grey for control (all states that did not have a mandatory bargaining law). Data on the coverage of collective bargaining agreements are from Saltzman (1982).

Table A1. Social, Economic, and Political Differences between States in the Early-Twentieth Century, by Legal Status of Collective Bargaining in 1990

	States that required collective bargaining in 1990	States that did not require collective bargaining in 1990
Income per capita (at 2010 $), 1929	724	442
Labor force in agriculture (%), 1910	29	51
Urban population (%), 1910	46	25
White population (%), 1910	95	78
Favorable labor legislation (EWSI Index x 100), 1909	30	18

NOTE: Author's calculations based on data from Goldin (1998), "America's Graduation from High School: The Evolution and Spread of Secondary Schooling in the Twentieth Century," *Journal of Economic History* 58(2): 345-374; Fishback, Holmes, and Allen (2009), "Lifting the Curse of Dimensionality: Measures of the States' Labor Legislation Climate in the United States during the Progressive Era," *Labor History* 50(3): 313-346.

Table A2. Social, Economic, and Political Differences between States in the Early-Twentieth Century, by Legal Status of Collective Bargaining in 1990 – Southern States Only

	States that required collective bargaining in 1990	States that did not require collective bargaining in 1990
Income per capita (at 2010 $), 1929	732	366
Labor force in agriculture (%), 1910	34	60
Urban population (%), 1910	45	19
White population (%), 1910	78	66
Favorable labor legislation (EWSI Index x 100), 1909	20	14

NOTE: Author's calculations based on data from Goldin (1998), "America's Graduation from High School: The Evolution and Spread of Secondary Schooling in the Twentieth Century," *Journal of Economic History* 58(2): 345-374; Fishback, Holmes, and Allen (2009), "Lifting the Curse of Dimensionality: Measures of the States' Labor Legislation Climate in the United States during the Progressive Era," *Labor History* 50(3): 313-346.

Table A3. Salaries in 1939 and Per-Pupil Current Expenditures in 1919, By Legal Status of Collective Bargaining as of 1990 – Southern States Only

	States that required collective bargaining in 1990	States that did not require collective bargaining in 1990
Per-pupil current expenditures (at 2010 $), 1919	254	134
Average teacher salaries (at 2010 $), 1939	22,391	13,300

Table A4. First School Year in Which a Mandatory Collective Bargaining Law Was Effective

State	Year (timing of treatment)
WI	1960
CT, MI	1965
MA, RI	1966
NJ, NY	1968
MD, ND, NV, SD	1969
AK, DE, HI, KS, ME, VT	1970
ID, OK, PA	1971
MN	1972
IN	1973
FL, OR	1974
IA, MT	1975
CA, NH, WA	1976
TN	1978
IL, OH	1984
NE	1987

Table A5. Non-Parametric Difference-in-Differences Effect of Mandatory Collective Bargaining Laws on the Level of Resources Devoted to Education, 1959-1990

	Dependent variable			
	Student-teacher ratio	Average teacher salary (log, 2010$)	Per-pupil current expenditures (log, 2010$)	Per-pupil non-wage current expenditures (log, 2010$)
	(1)	(2)	(3)	(4)
Years relative to CB requirement:				
6 years before	-0.036	-0.001	0.003	0.015
	(0.2158)	(0.0109)	(0.0149)	(0.0271)
5 years before	-0.051	0.000	-0.004	0.002
	(0.2379)	(0.0097)	(0.0163)	(0.0303)
4 years before	-0.309	0.003	-0.009	-0.021
	(0.2825)	(0.0110)	(0.0181)	(0.0325)
3 years before	-0.182	-0.010	-0.007	0.001
	(0.2940)	(0.0119)	(0.0170)	(0.0278)
2 years before	-0.207	-0.011	-0.021	-0.027
	(0.2588)	(0.0132)	(0.0164)	(0.0264)
1 year before	-0.008	-0.015	-0.028	-0.029
	(0.2521)	(0.0147)	(0.0169)	(0.0271)
0 years after	0.033	-0.014	-0.015	-0.002
	(0.2823)	(0.0155)	(0.0196)	(0.0311)
1 years after	0.115	-0.019	-0.025	-0.014
	(0.2885)	(0.0149)	(0.0188)	(0.0307)
2 years after	-0.011	-0.018	-0.001	0.027
	(0.2747)	(0.0155)	(0.0196)	(0.0317)
3 years after	0.027	-0.016	-0.012	0.001
	(0.2594)	(0.0146)	(0.0189)	(0.0298)
4 years after	0.181	-0.018	-0.021	-0.002
	(0.2539)	(0.0137)	(0.0210)	(0.0294)
5 years after	0.114	-0.015	-0.021	-0.011

	(0.2534)		(0.0137)		(0.0181)		(0.0243)
6 years after	0.173		-0.012		-0.007		0.011
	(0.2525)		(0.0124)		(0.0143)		(0.0190)
7 years after	0.104		-0.005		0.000		0.016
	(0.2288)		(0.0104)		(0.0154)		(0.0217)
8 years after	-0.086		-0.005		-0.015		-0.019
	(0.2295)		(0.0099)		(0.0148)		(0.0245)
9 years after	0.186		-0.010		-0.014		-0.003
	(0.2709)		(0.0091)		(0.0144)		(0.0194)
10 years after	0.130		-0.012		-0.008		0.006
	(0.2170)		(0.0086)		(0.0145)		(0.0210)
Constant	25.267	**	10.512	**	7.866	**	7.036	**
	(0.3022)		(0.0119)		(0.0174)		(0.0247)
Observations	1248		1248		1248		1248
Adj. R-Square	0.878		0.755		0.953		0.949
No. of clusters	32		32		32		32

NOTE: Results from equation 1 reported. All regressions include year and state fixed effects. All standard errors are clustered at the state level and are shown in parentheses. Stars denote statistical significance at the *0.05 or **0.01 level.

Table A6. Linear Difference-in-Differences Effect of Mandatory Collective Bargaining Laws on the Level of Resources Devoted to Education, with State-Specific Linear Time Trends, 1959-1990

	Dependent variable			
	Student-teacher ratio	Average teacher salary (log, 2010$)	Per-pupil current expenditures (log, 2010$)	Per-pupil non-wage current expenditures (log, 2010$)
	(1)	(2)	(3)	(4)
CB required at the beginning of the school year	0.246	-0.012	0.003	0.026
	(0.2336)	(0.0117)	(0.0146)	(0.0224)
Constant	493.485 **	-8.692 **	-53.936 **	-72.140 **
	(21.0131)	(0.9691)	(1.1022)	(1.4578)
Observations	1248	1248	1248	1248
Adj. R-Square	0.932	0.810	0.972	0.962
No. of clusters	32	32	32	32

NOTE: All regressions include state and year fixed effects and a state-specific linear time trend. Standard errors clustered at state level in parentheses. Stars denote statistical significance at the *0.05 or **0.01 level.

Table A7. Instrumental Variables Difference-in-Differences Estimates of the Effect of Being Covered by a Collective Bargaining Agreement, 1959-1977

	Dependent variable			
	Student-teacher ratio	Average teacher salary (log, 2010$)	Per-pupil current expenditures (log, 2010$)	Per-pupil non-wage current expenditures (log, 2010$)
	(1)	(2)	(3)	(4)
% of teachers covered by CB agreement (*predicted*)	0.0123 (0.0111) [0.2658]	-0.0003 (0.0004) [0.5219]	0.0004 (0.0006) [0.5275]	0.0013 (0.0010) [0.1812]
Constant	24.51 ** (0.4913) [0.0000]	10.53 ** (0.0301) [0.0000]	7.93 ** (0.0326) [0.0000]	7.13 ** (0.0379) [0.0000]
Observations	549	549	549	549
No. of states	32	32	32	32

NOTE: All regressions include state and year fixed effects. Bootstrapped standard errors in parentheses; p-values in brackets. Stars denote statistical significance at the *0.05 or **0.01 level. The first-stage regression is equation is $Y_{st} = \gamma_s + \phi_t + \delta T_{s,t} + \epsilon_{s,t}$ where Y_{st} is the percentage of teachers covered by a collective bargaining agreement; and $T_{s,t}$ equals 1 if state s in year t has a mandatory collective bargaining law in place, and 0 otherwise.

Table A8. Differential Response to the 1980 Crisis in States that Required Collective Bargaining by 1980 vs. States that Never Required Collective Bargaining

	Student-teacher ratio		Average teacher salary (log, 2010$)		Per-pupil current expenditures (log, 2010$)		Per-pupil non-wage current expenditures (log, 2010$)	
	(1)		(2)		(3)		(4)	
β_{-10}	-0.054		0.031		0.039		0.085	
	(0.5388)		(0.0219)		(0.0362)		(0.0654)	
β_{-9}	0.025		0.030		0.010		0.015	
	(0.5399)		(0.0222)		(0.0249)		(0.0380)	
β_{-8}	0.022		0.024		0.026		0.057	
	(0.5282)		(0.0213)		(0.0349)		(0.0644)	
β_{-7}	0.101		0.026		0.037		0.065	*
	(0.5110)		(0.0196)		(0.0211)		(0.0293)	
β_{-6}	0.319		0.013		0.008		0.033	
	(0.4929)		(0.0197)		(0.0309)		(0.0519)	
β_{-5}	0.537		0.007		0.009		0.052	
	(0.4825)		(0.0180)		(0.0279)		(0.0506)	
β_{-4}	0.351		0.020		0.019		0.046	
	(0.5007)		(0.0185)		(0.0191)		(0.0401)	
β_{-3}	0.640		-0.003		0.019		0.073	*
	(0.4408)		(0.0175)		(0.0181)		(0.0315)	
β_{-2}	0.459		0.011		0.000		0.018	
	(0.4410)		(0.0121)		(0.0278)		(0.0525)	
β_0	0.652	*	-0.003		0.010		0.045	
	(0.2689)		(0.0059)		(0.0228)		(0.0393)	
β_1	0.408		-0.005		0.018		0.050	
	(0.3841)		(0.0107)		(0.0253)		(0.0392)	
β_2	0.487		0.005		0.023		0.052	
	(0.3711)		(0.0085)		(0.0259)		(0.0394)	
β_3	0.394		0.014		0.028		0.049	

	(0.4306)		(0.0104)		(0.0256)		(0.0380)
β_4	0.284		-0.011		0.011		0.034
	(0.4459)		(0.0135)		(0.0283)		(0.0416)
β_5	0.204		-0.011		0.020		0.047
	(0.4326)		(0.0151)		(0.0294)		(0.0434)
β_6	0.378		-0.009		0.019		0.051
	(0.4372)		(0.0165)		(0.0337)		(0.0474)
β_7	0.420		0.015		0.035		0.061
	(0.4509)		(0.0210)		(0.0381)		(0.0487)
β_8	0.520		0.028		0.029		0.045
	(0.4569)		(0.0251)		(0.0412)		(0.0500)
β_9	0.448		0.041		0.032		0.037
	(0.4788)		(0.0297)		(0.0456)		(0.0559)
β_{10}	0.514		0.047		0.023		0.022
	(0.4706)		(0.0291)		(0.0457)		(0.0542)
Constant	18.836	**	10.724	**	8.693	**	8.165	**
	(0.1737)		(0.0051)		(0.0099)		(0.0148)
Observations	882		882		882		882
Adj. R-Square	0.828		0.456		0.850		0.829
No. of clusters	46		46		46		46

NOTE: Results from equation 3 reported. All regressions include year and state fixed effects (not reported). All standard errors are clustered at the state level and are shown in parentheses. Stars denote statistical significance at the *0.05 or **0.01 level.