Compensation and Employment Policies in the U.S. Public Sector

Compensation and Employment Policies in the U.S. Public Sector

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Abstract

How do compensation and employment policies distinct to the public sector affect the quality of public services in the United States? I explore labor-market reactions to de-unionization, defined-benefit pensions, and statutory expenditure limits. I find that a Tennessee ban on collective bargaining in school districts modestly reduced both teacher compensation and student-teacher ratios, but had no significant effect on student test scores or per-pupil expenditure. Similarly, local referendums to override a tax and expenditure limit in Wisconsin prompted school-district administrators to decrease class sizes without altering teacher compensation. Parents responded by enrolling their children in funded schools. Lastly, I show that transitioning from a defined-benefit to a defined-contribution pension increased mid-career mobility among state employees in Michigan. Older workers were willing to remain an additional four years in government service to receive pension benefits worth twice their salary at separation. Younger workers did not persist to earn benefits equal to their final salary.

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Introduction

In 2014, state and local governments in the United States spent nearly two trillion dollars on compensation for past and current public employees – 53 percent of total expenditure (U.S. Census Bureau, 2014; and U.S. Bureau of Labor Statistics, 2004-16). Meanwhile, the financial crisis of 2008 and ensuing recession placed significant strain on state and local finances. General revenue rose by only four percent (nominal) between 2007 and 2009, in stark contrast to 15 percent growth from 2005 to 2007. As revenue growth fell, so did the value of assets set aside to pre-fund contractually promised pension benefits. In 2009, the major state-administered pension plans were responsible for unfunded liabilities valued at 2.9 trillion dollars (Munnell et al, 2010). In response to budgetary pressure, government managers around the country sought to reduce personnel costs without sacrificing the quality of public services.

Personnel policies in the U.S. public sector differ noticeably from those in the private sector. Labor unions negotiate wages and benefits for nearly 40 percent of state and municipal employees, but only seven percent of private-sector workers (Hirsch and Macpherson, 2016). Fringe benefits comprise a larger share of total compensation. While nearly all full-time government employees participate in a defined-benefit pension that rewards longevity, only 60 percent of private-sector workers earn a pension from their employer, and most employer-sponsored private plans operate as portable, tax-deferred savings accounts (Munnell, 2012; and Munnell and Bleckman, 2014). Complementing and perhaps underlying these differences, public-sector employees often exert political pressure on their managers. Government personnel policies may transfer rents from private-sector

taxpayers to bureaucrats when civil servants help elect the administrators who set tax rates and negotiate compensation. In response, thirty states around the country restrict the size of municipal government with statutory tax and expenditure limits (Gordon and Rueben, 2009).

Recent attempts to enhance efficiency have aligned labor-market policies in the public sector with those in the private sector. Several states passed laws to hinder teachers' unions from negotiating with local school boards. Proponents of public-education reform debate the merits of increased funding for residence-based school districts or establishing a competitive market for students who would pay state-subsidized tuition. Newly hired public employees increasingly participate in a defined-contribution or hybrid pension, rather than a traditional defined-benefit plan. Despite substantial challenge to traditional government employment policies, few studies have accessed the detailed data necessary to evaluate compensation, turnover, and performance across many occupations in the public sector.

I examine three personnel policies prevalent in the public sector: unionization, revenue caps, and defined-benefit pensions. "From School Boards to State Politics: De-Unionization and the Labor Market for Teachers" compares teachers in Tennessee who lost bargaining rights in 2011 to their colleagues within the state whose districts never negotiated, within a differences-in-differences framework. De-unionization modestly reduced teacher compensation and promoted smaller classes. Salaries grew one percentage-point less, cumulatively over five years, while employer-paid health insurance premiums grew five percentage-points less. School administrators reduced class sizes by half a student on average. Nevertheless, de-unionization had no impact on student test scores in the short run. In contrast, teachers' unions lost at least 25 percent of pre-prohibition revenue because contracts no longer stipulate automatic payroll deduction of union dues.

"How Tax Overrides Affect Teacher Employment and School Choice" examines the effect of local referendums to override state-legislated tax and expenditure limits on teacher employment and student enrollment in Wisconsin public-school districts. Regression

discontinuity estimates based on close votes indicate that district administrators used override revenue to decrease class sizes and that parents responded by enrolling their children in funded schools. Conversely, teacher compensation did not benefit substantially from relaxing the budget constraint. These results help arbitrate between a model of tax and expenditure limits where administrators protect instruction in favor of one where budget constraints restrict all activities proportionately. They also explain why tax caps are more salient in some school districts than others.

"Pay for Seniority: Do Defined-Benefit Pensions Retain Government Employees?" estimates the impact of seniority pay on workers' career trajectories in the U.S. public sector. State government employees in Michigan participate in either a defined-benefit or a defined-contribution pension plan depending on their date of hire. Defined-benefit members must remain at least a decade in state government to receive any payments, whereas defined-contribution members vest immediately. Regression discontinuity estimates show that workers are eight percentage points more likely to remain at least a decade in state service if they have a defined-benefit pension. The effect rises to 30 percentage points among older workers with occupations requiring a college degree. In contrast, the prospect of additional retirement wealth does not induce younger workers to prolong their careers in state government. Policymakers contemplating pension reform should consider the benefits and costs of a younger workforce with less firm-specific human capital.

Together, these essays suggest that the cost of compensating public employees affects the size of the workforce and that the structure of remuneration determines whether productive workers enter the government and exert maximum effort. Administrators prefer a larger workforce with lower salaries than fewer workers at higher pay. Union efforts to protect compensation induce public managers to hire fewer employees. The same lesson may apply to rising pension costs in the public sector. Additionally, deferred compensation strongly affects employee behavior, either encouraging talented mid-career workers to enter the public sector and persist until retirement, or preventing dissatisfied

employees from pursuing private-sector opportunities. Future research should explore the relationship between employee mobility and government productivity by linking detailed personnel records to the agency performance data increasingly reported by state and local governments.

Chapter 1

From School Boards to State Politics: De-Unionization and the Labor Market for Teachers

Once a prominent feature of public education in the United States, teachers' ability to collectively bargain is increasingly limited. Since 2011, seven state governments have passed legislation to reduce union leverage over local school boards.[1] For example, Utah prohibited paid leave for association activities, Illinois allowed school boards to cease negotiating over pension and health insurance benefits, and Tennessee banned collectively bargained contracts outright. In 2015, the Supreme Court of the United States considered a challenge to California's agency shop policy, which allows the California Education Association to collect dues from non-member teachers who are covered by union contracts (*Friedrichs v. California Teachers' Association*).[2] These recent events reinforce a longer-term trend toward the de-unionization of education. In 2015, 45 percent of K-12 professionals were covered by collectively bargained employment contracts, whereas 61

[1]National Conference of State Legislatures (2016). Specifically: Idaho (H 261 of 2012), Illinois (S 1 of 2014), Indiana (S 575 of 2011), Michigan (H 4628 and S 158 of 2011, H 5387 of 2016), Tennessee (S 113 of 2011), Utah (H 183 of 2011), and Wisconsin (Act 10 of 2011).

[2]The case was affirmed by an equally divided court in 2016.

percent were covered in 1983 (Hirsch and Macpherson, 2016).

The literature has yet to reach consensus on the causal relationship between union contracts, teacher labor markets, and student achievement. Studies of unionization in the U.S. private sector often find wage premiums on the order of 15 percent.[3] Yet, union differentials in the education sector vary greatly depending on the research design and context. For example, recent observational studies find either no effect (Strunk, 2011) or a four-percent premium increasing with seniority (Lamm West and Mykerezi, 2011).[4] Quasi-experimental analyses typically focus on the passage of state laws, beginning in the 1960s, that permitted collective bargaining in school districts. Here again, conclusions diverge. Hoxby (1996) documents a five percentage-point salary premium from unionization, whereas Lovenheim (2009) and Frandsen (2016) see none. Hoxby shows a two-student decrease in the student-teacher ratio and a three percentage-point increase in the dropout rate, while Lovenheim finds no change. Nevertheless, Lovenheim and Willén (2016) argue that collective bargaining hurts student achievement in the long run.

Even if the lessons from historical studies were clear, they need not apply to de-unionization today. Cutting existing benefits might be more difficult than granting new ones, as school boards risk a deterioration of teacher morale that undermines performance (Mas, 2006). National trends toward salary transparency, pension reform, and teacher evaluation based on high-stakes testing have changed the terms of labor negotiations.[5] Two recent studies examine Wisconsin's 2011 package of policies that simultaneously limited collective bargaining to wages, capped wage growth at cost-of-living, and reduced pension and retiree health insurance benefits. Litten (2016) documents an eight percentage-point reduction in compensation due to the law, while Biasi (2016) shows that higher-quality teachers

[3]Blanchflower and Bryson (2004) review the observational literature; DiNardo and Lee (2004) and Frandsen (2012) offer regression discontinuity estimates based on union certification elections.

[4]Strunk (2011) examines variation in union strength across districts in California. Lamm West and Mykerezi (2011) compare (large) districts that collectively bargain throughout the United States to those that do not.

[5]See Mas (2014), Munnell (2012), and National Council on Teacher Quality (2011) for a discussion of these developments.

sorted into the districts that subsequently adopted performance pay. Yet, neither studies how school boards react to de-unionization absent direct legislative control over wages and benefits. Thus, recent initiatives to curb collective bargaining serve as both the motivation for this essay and its research design.

I explore how a prohibition of collective bargaining in public-school districts impacts teacher compensation, student-teacher ratios, and student test scores. Specifically, I evaluate the effects of a 2011 law in Tennessee that replaced teachers' previously held right to collectively bargain with a "meet and confer" arrangement. The law allows teachers and administrators to formally discuss working conditions, but school districts are no longer required to negotiate contracts with the Tennessee Education Association. Importantly, not all school districts engaged in collective bargaining prior to 2011. In 2010, Tennessee hosted 136 traditional K-12 public-school districts, 91 of which collectively bargained, while 45 did not. Within-state variation in bargaining status allows for a differences-in-differences (DID) research design. I compare outcomes in districts that once bargained – the "treated" districts – to outcomes in districts that never bargained – the "control" districts – before and after the policy change.

I draw on the personnel records of nearly 70,000 classroom teachers over a period of six years, combined with district-aggregate data from 2006 to 2015 on staffing, student test scores, and financial characteristics. I find that the ban on collective bargaining promoted smaller classes at the expense of teacher compensation, and had large effects on the unions themselves. Teacher salaries grew one percentage-point less, cumulatively over five years, while employer-paid health insurance premiums grew five percentage-points less over the same period. As school-district administrators slowed the growth of compensation, they also decreased student-teacher ratios by half a student on average. Whereas the effect on compensation had stabilized by 2015, student-teacher ratios are still on a downward trajectory. Student test scores appeared unchanged as of 2014, but the estimates are too imprecise to rule out small effects. Meanwhile, Tennessee's teachers' unions suffered a rapid loss of revenue since they can no longer require school boards to automatically

deduct union dues from teacher paychecks. If smaller coffers reduce union lobbying, then the prohibition of bargaining may affect local school board elections and policies set by the State Legislature.

The remainder of this chapter proceeds as follows. Section 1.1 sets the context for de-unionization in Tennessee. Section 1.2 outlines the conceptual framework underlying my analysis. Section 1.3 introduces the data and formalizes my differences-in-differences research design. Section 1.4 presents the main results: union revenue, teacher salaries, health insurance premiums, and class size. Section 1.5 reports suggestive results: student test scores, total expenditure, and teacher turnover. Section 1.6 discusses how waning political influence could affect teachers in the future.

For ease of exposition, I refer to districts that collectively bargained in 2010 as "unionized." Tennessee is a right-to-work state where teachers elect to join the union irrespective of their employer. Most public-school data in Tennessee are reported over school years rather than calendar years. I indicate school years by the calendar year of the fall semester. Hence, the 2009-10 school year becomes 2009.

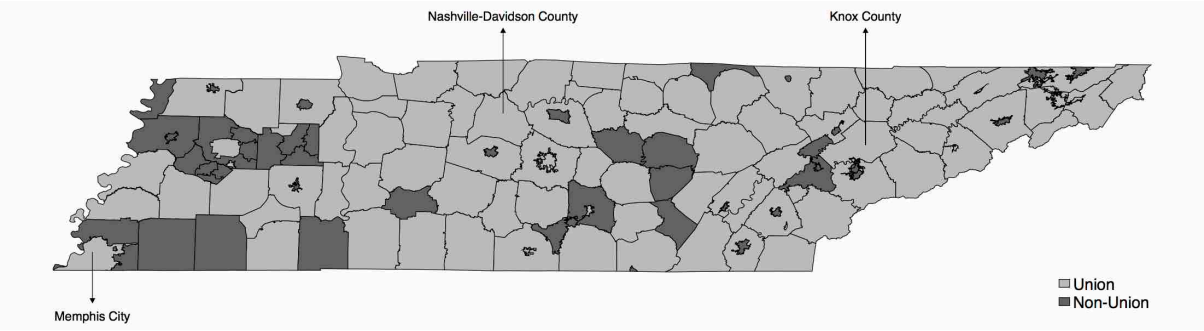
1.1 A Natural Experiment in Tennessee

For 32 years, teachers in Tennessee determined their own bargaining status with district-specific union certification elections (Education Professional Negotiations Act of 1978). Certification persisted until the local school board could demonstrate union membership below 50 percent. If Tennessee resembled Midwestern states, most districts would have held elections during the 1980s and 1990s, the results of which still determined collective bargaining status years later (Lovenheim, 2009). Indeed, certification data from the Tennessee School Boards' Association shows nearly constant bargaining status after 2007.[6]

[6]Neither the Tennessee Department of Education nor the Tennessee School Boards' Association possess historical data on union certification. The Tennessee Education Association did not respond to my requests for assistance. The Tennessee School Boards' Association provided data on bargaining status from the 2007-08 school year to the 2010-11 school year. Two districts changed status between 2007-08

As a result, two-thirds of Tennessee's 136 traditional public-school districts collectively bargained in 2010, while the remaining one-third did not.[7] Figure 1.1 shows how the unionized and non-unionized districts were scattered across the state.

Figure 1.1: Location of School Districts by Bargaining Status, 2010



Source: data provided by the Tennessee School Boards' Association.

The 1978 collective bargaining law mandated a procedure for labor negotiations. District managers and union representatives each proposed an ideal contract and engaged in "reasonable efforts to reach agreement." Teachers were prohibited from going on strike. If compromise proved elusive, either side could request assistance from the Federal Mediation and Conciliation Service. After mediation, either side could ask the American Arbitration Association to designate a non-binding, fact-finding, advisory arbitrator. The arbitrator was permitted to publicly announce his/her recommendations. The law required district administrators to negotiate salaries and fringe benefits (with the exception of the Tennessee Consolidated Retirement System, a State-administered defined-benefit pension) working conditions, leaves of absence, student disciplinary procedures, grievance procedures, and payroll deduction of union dues. Union contracts remained in effect for a maximum of three years. I examined 15 union contracts that were still readily available online in the fall of 2016.[8] All stipulated that negotiations should recommence before the

and 2008-09: Fayette County Schools de-certified (student enrollment around 3,500) and Rogersville City Schools unionized (student enrollment around 600). Changing the classification of these two districts does not affect the results.

[7]The district count excludes charter school districts and schools that exclusively serve children with disabilities.

[8]Blount County (2009-11), Cumberland County (2010-13), Dickson County (2010-13), Franklin

current expiration date. However, one also indicated that the current agreement hadn't been reached until after the prior contract had expired.

Teacher compensation and working conditions were still fairly rigid in the non-unionized districts, even absent negotiated contracts. In 2010, all school districts in Tennessee paid teachers according to salary schedules (matrices) that rewarded teaching experience and highest-degree earned.[9] Wage rigidity is partly due to a legislated, state-wide Minimum Salary Schedule set annually by the Tennessee State Board of Education. Similarly, the Tennessee Legislature determines pension benefits, tenure laws, minimum class sizes, and school performance benchmarks.

1.1.1 Collective Bargaining Replaced with Meet and Confer

In 2011, the Tennessee State Legislature prohibited collective bargaining in traditional K-12 public-school districts (Professional Educators' Collaborative Conferencing Act). As a result, school-district administrators became the sole arbiters of labor disputes. The prohibition of bargaining still provides a formal mechanism through which teachers and administrators meet to discus salaries and benefits. Teachers within each district decide by referendum to confer with administrators, electing a representative body that may include traditional union negotiators. However, teacher representatives are not allowed to discuss school staffing or payroll deduction of union dues. Existing union contracts persist until their scheduled expiration dates, with the exception of "reopener" clauses, which faced legal dispute (White, 2011).[10] As of September 2015, the Tennessee School Boards' Association knew of collaborative conferencing in 16 districts.

County (2010-13), Hawkins County (2010-13), Heywood County (2009-12), Macon County (2011-14), McMinn County (2010-13), Metropolitan Nashville (2009-10), Obion County (2010-13), Rhea County (2009-11), Sevier County (2005-09), Sullivan County (2010-11), Sumner County (2009-12), and Trenton SSD (2008-11). Contracts available from the author upon request.

[9]The Tennessee Education Association posts these schedules on its website. Historical schedules from 2005 through 2009 are recoverable using the Internet Archive.

[10]Re-opener clauses require annual renegotiation of a multi-year contract.

The Tennessee State Legislature introduced its ban on bargaining in January of 2011. Republicans had just gained control of the Governorship, State House and State Senate for the first time since the late 1800s, running on a platform of fiscal restraint (Santos, 2010). The new governor, Bill Haslam, prioritized implementation of First to the Top education reforms enacted under the previous administration (discussed in Section 1.1.2).[11] This environment emboldened the Tennessee School Boards' Association to suggest de-unionization – a long-standing desire – to state senators (Gibbons, 2012). Governor Haslam signed the law prohibiting bargaining in June of 2011, effective immediately.

Political actors describe the prohibition as a conflict between children's interests and politicians' careers. Proponents of de-unionization claim that rigid contracts prevented school boards from implementing performance-enhancing compensation and management practices: "Reform after reform has been refused or dismantled. The barrier that has prevented us from putting the best possible teacher in every classroom will soon be removed" (Tennessee Lt. Governor Ron Ramsey, 2011). Opponents argue that anti-union laws help election coffers rather than teachers and students: "Its sponsors only care that TEA endorsed legislators [...] who happen to be Democrats" (Al Mance, Executive Director of the Tennessee Education Association, 2011). Supporters and opponents alike rallied around the Tennessee State Capitol in 2011 amid significant local media coverage.

The prohibition of bargaining likely affected some outcomes with a time lag, while others could have changed in anticipation of the law. Wages were relatively inflexible until existing collectively-bargained contracts expired. School boards could not reduce salaries below those specified in the union contract, but could freeze salaries by refusing to reopen grandfathered contracts. In contrast, teachers might have altered their pedagogy and turnover at the very threat of de-unionization. Anticipation effects may therefore appear as early as November, 2010, when the state political climate shifted against the Tennessee Education Association.

[11] "Bill's Priorities" official profile on `www.tn.gov`, archived by the Wayback Machine.

1.1.2 Concurrent Changes to State Education Policy

The prohibition of bargaining took effect in a newly high-stakes environment for teachers. In 2010, the Tennessee State Legislature enacted education reforms to comply with a Federal Race to the Top grant (Tennessee First to the Top Act). The reforms provide for State oversight of persistently low-achieving schools, and mandate annual teacher performance evaluations based on Teacher Value-Added Scores (35 percent), school-aggregate performance (15 percent), and qualitative classroom observations (50 percent).[12] Teacher tenure became contingent on these evaluations in 2011 (Chapter 70 of the Public Acts of 2011). To receive tenure, novice teachers must wait five years, rather than three, and earn positive evaluations in the last two years. Teachers granted tenure prior to 2011 enter probation if they receive negative evaluations for two consecutive years.

So as not to conflate the effects of First to the Top with de-unionization, I exclude districts at risk of state intervention from my analysis. Specifically, the Tennessee Department of Education designated "priority" schools in six districts between 2010 and 2014 – five bargaining districts and one non-bargaining.[13] Teacher evaluations are less of a concern because aggregate district performance was already well known, and few teachers lose tenure due to poor test scores. School- and district-aggregate Value-Added scores have been calculated by the Tennessee Department of Education since the late 1990s and are widely disseminated on *District Report Cards*. Meanwhile, district administrators use the qualitative portion of teacher evaluations to reduce disparities in the quantitative portion (Tennessee Department of Education 2012, 2014, and 2016). Kraft and Gilmour (2016) indicate that 11 percent of teachers received negative evaluations in 2014-15. This is an upper-bound during the period of my analysis because teacher evaluations have been worsening over time (Tennessee Department of Education, 2016).

[12]Teacher Value-Added measures student test-score gains over time, and ascribes systematic gains within a class to teacher quality. See Chetty et al. (2014a and 2014b) and Tennessee Department of Education (2015) for a detailed explanation.

[13]Hamilton County, Hardeman County, Knox County, Jackson-Madison County, Memphis City, and Nashville-Davidson County. "Priority" designations are posted on the website of the Tennessee Department of Education.

The most recent change to State education policy occurred in 2013, when the Tennessee State Board of Education revised its State Minimum Salary Schedule to reduce salaries for senior teachers and those with graduate degrees. The State Legislature responded by allocating fewer equalization funds for teacher salaries. I will show in Section 1.4.2 that state funding dropped equally in unionized and non-unionized districts.

1.2 Conceptual Framework

What changes should we expect from a prohibition of collective bargaining? The Tennessee Education Association and its local affiliates once engaged in three primary activities. First, they negotiated labor contracts with local school boards. Second, they supported members with legal assistance in cases of contract dispute. And, third, they engaged in political activities to advance members' interests with local school boards and the State Legislature. In 2009, the four largest expenditures of the Tennessee Education Association were: compensation and travel for employees (77 percent, including negotiators, lobbyists, and the Association President), legal services (three percent), government relations (three percent), and activity by its Political Action Committee (two percent).[14]

By banning union contracts, de-unionization eliminates the first of these activities and largely obviates the second.[15] Prior studies typically expect union contracts to increase the monetary compensation of covered teachers, but disagree as to magnitude. Three quasi-experiments examine the passage of state laws permitting collective bargaining in school districts during the 1960s, 1970s, and 1980s. Hoxby (1996) finds that unionization increased teacher salaries by five percent, with a 95-percent confidence range of one to nine percent. In contrast, Lovenheim (2009) and Frandsen (2016) see no increase

[14]Tennessee Education Association (2010).

[15]Of course, the union may still take legal action over non-contracted grievances. For example, the Tennessee Education Association recently lost a suit against the Knox County School Board over the use of Teacher Value-Added scores in evaluations (Sawchuk, 2016).

in teacher salaries.[16] Glaeser and Ponzetto (2014) expect union contracts to enhance pensions and health insurance because benefit costs are opaque to taxpayers. Lacking historical data, neither Hoxby, Lovenheim, nor Frandsen consider benefits. Similarly, studies of the private sector often assume that union negotiators cater to the preferences of the median voter in representation elections (Farber, 1978; Grossman, 1983; and Lee and Mas, 2012). This assumption implies that senior teachers capture the benefits of association. Unfortunately, the aggregate salary data employed by prior studies do not differentiate based on seniority.

Efforts to pin down the causal relationship between union contracts and employment protections face greater challenges. If teachers negotiate class size, then union contracts should increase the number of teachers and decrease the student-teacher ratio.[17] Yet, districts could also respond to elevated union salaries by hiring fewer teachers. Hoxby (1996) shows a nearly two-student decrease in the student-teacher ratio (with a 95-percent confidence range of one to three students), while Lovenheim (2009) finds no change.[18] Union grievance procedures may prevent school boards from terminating low-productivity teachers, or force districts to rigorously screen novice, un-tenured teachers who are often less protected (Han, 2013).

The effect of union contracts on student achievement is also ambiguous, ex-ante. Rising compensation should attract talented teachers, but the effect may be mitigated if raises only accrue to senior colleagues (Nagler et al., 2015). Small classes improve student test scores and long-run outcomes, but protecting low-quality educators from termination hurts achievement (Chetty et al., 2014b; Angrist and Lavy, 1999; Chetty et al., 2011; Fredriksson et al., 2013). Collective bargaining leads to a three-percentage-point increase in the dropout rate in Hoxby's (1996) analysis, whereas Lovenheim (2009) reports no

[16]However, the standard errors on both sets of estimates are too large to rule out very small increases.

[17]The 2010-13 Memorandum of Understanding negotiated between Cumberland County Schools and the Cumberland County Education Association is an example in Tennessee.

[18]Lovenheim (2009) estimates the percent change in the student-teacher ratio. The 95-percent confidence interval on his estimate ranges from a two-percent increase to a five-percent decrease.

change. In subsequent work, Lovenheim and Willén (2016) argue that unionization reduces students' long-run earnings.

Many studies document how teachers' unions attempt to sway education policy beyond the negotiating table. Local school boards and the State Legislature are all elected, prompting union campaign contributions, get-out-the-vote initiatives, and lobbying (Freeman, 1986; Hess and Leal, 2005; Moe, 2011; and Zax and Ichniowski, 1988). Political influence may be an important determinant of labor-market outcomes. For example, union contracts in California are more likely to restrict district management when the union is also involved in local politics (Strunk and Grissom, 2010). Participatory local budgeting favors civil servants because they are more likely to vote than private-sector taxpayers (Saiz, 2011). Police unions obtain larger wage increases than teachers, although the two professions negotiate in similar institutional environments (Frandsen, 2016). Lastly, teachers enjoy tenure and defined-benefit pensions even in southern states where bargaining has never been allowed.

The Tennessee Education Association could lose its largest source of revenue as negotiated contracts expire. De-unionization reduced teachers' incentive to join the union and simultaneously increased the time cost of paying dues, since many union contracts provide for automatic payroll deduction.[19] I present evidence of a decline in union revenue that is consistent with a political role for negotiated contracts. Smaller, weaker unions may lose influence over local school boards and state legislators.

I hope to help arbitrate between conflicting stories of union influence in public schools. Of course, any research design has limitations. I only examine the short run, and my control group is not entirely isolated from de-unionization. In addition to experiencing political fallout, the non-unionized districts no longer face a union threat, and may freeze compensation alongside unionized districts. Relatedly, all districts compete in a local labor market for teachers. School districts located in the same region might set compen-

[19]Of the 15 collectively bargained agreements that I examined, all require employers to allow payroll deduction of union dues, and several default teachers into automatic deduction.

sation in relation to each other so as to recruit and retain talented staff. As wages decline in unionized districts, non-unionized neighbors follow suit. For these reasons, I may underestimate the "true" causal effect of de-unionization on teacher labor markets.

1.3 Data and Summary Statistics

1.3.1 Sample Selection and Data Sources

My analysis sample contains all traditional public-school districts in Tennessee that: 1) operated continuously between 2006 and 2014, and 2) did not contain any "priority" schools that could become subject to State oversight under First to the Top.[20] Since priority schools are more likely to be located in metropolitan areas, this sample restriction excludes the four largest cities in Tennessee – Nashville, Memphis, Knoxville, and Chattanooga. Ultimately, my analysis sample tracks 87 unionized and 42 non-unionized districts from 2006 to 2015.

The Tennessee Department of Education (TDE) provided most of the data for this study. Teacher personnel records follow 68,240 classroom teachers in the analysis districts from 2009 to 2014. These records contain: salary received in a given year (including base pay, supplements for extra duties, and bonus pay), years of full-time-equivalent teaching experience credited on the State teaching license, highest academic degree earned, grade assignment, and district where employed.[21] I supplement the personnel records with district-aggregate data from multiple sources, available from 2006 to 2015. The Tennessee

[20]The main sample omits Davidson County Schools (Nashville), Hamilton County Schools (Chattanooga), Hardeman County Schools, Knox County Schools (Knoxville), Jackson-Madison County Schools, Memphis City Schools, and Shelby County Schools. I drop Carroll County Schools because the district enrolls between two and ten students over this period. The Tennessee Department of Education provides an annual list of "priority" schools on its website.

[21]I collapse the data to observe each teacher only once within a district in a given year. Teachers filling multiple positions within a district in a given year (administrative or teaching) are assigned their total salary across all positions, but are categorized by the position that contributes most to their total salary. In the analysis sample, only 619 teachers fill multiple positions within a district in the same year.

State Board of Education (TSBE) publishes annual compensation data in *Basic Education Program Review Committee Annual Reports*. The TSBE collects official salary schedules from each district and calculates the average scheduled salary in that district, weighting the individual salary "cells" by the fraction of teachers across the entire state who have the requisite experience and education. Similarly, the TSBE calculates the average health insurance premium paid by a local school board, on behalf of its teachers, across three plan types: PPO, POS, and HMO. The individual premiums are weighted by the fraction of teachers who participate in each plan type among all teachers in the state.

The Tennessee School Boards' Association supplied a list of the districts that collectively bargained in 2010. The TDE publishes *Annual Statistical Reports* with basic financial, demographic, and staffing information for each school district. I rely particularly on average daily membership (ADM), which measures the average number of students enrolled in the district over the school year. I also frequently reference the number of full-time-equivalent classroom teachers, which sums total teaching hours in a district and divides by the number of teaching hours clocked by a typical full-time teacher, as determined by the State. Full-time-equivalent teacher counts correlate quite strongly with a simple count of teaching staff in the personnel records.[22] Likewise, the TDE provided aggregate student test scores from 2006 to 2014.[23] These data record mean and median scores on the standardized Tennessee Comprehensive Achievement Program by district, grade, and subject. The Tennessee Comptroller of the Treasury publishes *Tax Aggregate Reports of Tennessee* that list total appraised residential property value in each district. Lastly, the U.S. Census Bureau tabulates school-district demographic information for all residents of Tennessee based on the decennial Census. To judge the level of urban development in a school district, I divide total population by the land area measured in 2010.

[22]The correlation is 0.999. I replace Fayetteville City's full-time-equivalent count in 2010 with personnel records because of an error in the *Annual Statistical Report*.

[23]Unfortunately, Tennessee did not administer the exam in 2015.

Large non-profit organizations, including labor unions, report revenues and expenditures to the Internal Revenue Service on Form 990. Non-profits with fewer than $50,000 in gross receipts may also choose to file. I gathered information on unions in Tennessee from Form 990 Reports published online by the National Center for Charitable Statistics at the Urban Institute. My sample includes public-sector unions that filed Form 990 every year between 2006 and 2013 (the last year of data available): 12 affiliates of the National Education Association or American Federation of Teachers (bargaining teachers' unions); 9 professional associations for K-12 management; 17 police unions, and 12 fire-fighter unions.

1.3.2 Summary Statistics

Table 1.1 compares mean characteristics of the unionized and non-unionized districts – weighted by size – while Appendix Section A.2 presents detailed summary statistics. The two groups appear to be similar with a few notable exceptions. The unionized districts enrolled substantially more students, but are located in less urban areas. Unionized districts are more likely to serve an entire county, whereas non-unionized districts mostly serve individual cities or special service areas within a county. The unionized districts enjoyed slightly fewer financial resources, largely due to Tennessee's tax structure, which allows cities to levy property taxes on top of county-wide taxes. Aggregate student test scores were also slightly lower in the unionized districts. I convert the raw exam results to z-scores by subtracting the state-wide mean in 2010 from the district-aggregate score, within each grade and subject, and then dividing by the state-wide standard deviation in 2010.

Table 1.1: Mean Characteristics of School Districts and Teachers in 2009

Main Analysis Sample

VARIABLES	Unionized	Non-Unionized
District Characteristics, Identically Weighted		
Average Daily Student Membership (ADM)	6,715	2,321
Full-Time-Equivalent Teachers	454	164
District Characteristics, Student-Weighted		
District Serves Entire County	0.952	0.370
Fraction Students White	0.788	0.816
Frac. Students Eligible for Fed. Title I Funds	0.539	0.666
Per-Student Operating Expenditure	7,929	8,490
Fraction Expenditure State-Funded	0.502	0.493
Per-ADM Residential Property Value*	344,971	334,845
District Population Per Square Mile*	316	515
ADM / Full-Time-Equivalent Teachers	14.87	14.25
Average Employer Health Premium	7,071	6,256
Student-Mean Math Score (Z-Score)	-0.177	-0.069
Student-Mean RLA Score (Z-Score)	-0.064	0.009
Teacher Characteristics, Teacher-Weighted		
Total Annual Salary	43,373	44,591
Years of Licensed Teaching Experience	13.01	13.96
Fraction Highest Degree Bachelor's	0.455	0.432
Fraction Highest Degree Master's	0.394	0.422
Fraction Highest Degree Master's +30 Hours	0.078	0.060
Fraction Highest Degree Education Specialist	0.065	0.079
Fraction Leave District At End of Year	0.083	0.087
Number of Districts	87	42

* property value measured over the calendar year and district population from 2010 U.S. Census.

Source: author's calculations.

Note: see Appendix Section A.1 for variable definitions and data sources.

My DID design assumes that outcomes in unionized and non-unionized districts would have trended similarly absent the ban on bargaining. Of particular concern in this context, large urban districts and small rural districts may experience the financial crisis of 2008 differently. To illustrate, Appendix Figure A.1 highlights the positive relationship between 10-year growth in student enrollment (2005-15) and average daily membership in 2000. Districts in the top third of the enrollment distribution grew by two percent, on average, while those in the bottom third contracted by two percent. Migration from small to large school districts could produce a spurious correlation between unionization and student enrollment. For this reason, all of my analyses control flexibly for average daily membership in 2000. I test the sensitivity of my DID estimates to these controls and show that the results do not change if I also control for district population density. As a further robustness check, I replicate the analysis on a "small" robustness sample that excludes all districts enrolling more than 6,500 students in 2000 – the largest non-unionized enrollment. Appendix Tables A.1 through A.5 display summary statistics for this smaller sample; small unionized districts are increasingly rural.

Table 1.1 reveals that teachers in the unionized districts had one fewer year of licensed teaching experience in 2009, on average, and earned slightly lower salaries. School districts in Tennessee receive annual equalization funds from State appropriations.[24] The formula to fund teacher salaries is based on the State Minimum Salary Schedule. Most school districts pay teachers with a combination of State equalization revenue and local property taxes. In 2009, districts supplemented each teacher's State-paid salary with \$5,000-\$6,000 of local funds, on average (Appendix Table A.3). Appendix Figure A.2 shows that most teachers earned at least \$30,000 in 2009, but that some received significantly less. Since a first-time teacher holding a Bachelor's degree was entitled to a legislated minimum salary of \$29,215 in 2009, I infer that teachers earning below this amount work part-time or part-year. Appendix Figure A.3 supports this supposition: teachers who earn less than the State Minimum Salary tend to be novices or seniors preparing for retirement.

[24]The Basic Education Program.

Turnover is high among novice teachers, decreasing notably around 10 years of experience, and rising again as teachers gradually become eligible for retirement (Appendix Figure A.5). Teachers participate in the Tennessee Consolidated Retirement System, a state-administered defined-benefit pension that permits retirement at age 60 with 10 years of tenure, or at any age with 30 years of tenure (Tennessee Consolidated Retirement System, 2009-15). Benefit levels and pension contributions (both employer and employee) are mandated by the State Legislature. Districts, however, choose health insurance plans for currently employed teachers.

1.3.3 Dynamic Differences-in-Differences Model

Equation 1.1 below serves as a baseline specification for my empirical analysis.

$$Y_{i,d,t} = \alpha + \boldsymbol{I_t} + \boldsymbol{D_d} + \boldsymbol{\beta}(\boldsymbol{I_t U_d}) + \boldsymbol{I_t A_d} + \epsilon_{i,d,t} \tag{1.1}$$

$Y_{i,d,t}$ is the outcome of interest for teacher i in district d in year t. $\boldsymbol{I_t}$ denotes a vector of year fixed effects that includes 2006 through 2014, with 2010 as the omitted year. $\boldsymbol{D_d}$ represents a vector of district fixed effects; I identify the impact of de-unionization from within-district changes in outcomes over time. To this end, define U_d as a dichotomous indicator for bargaining status pre-2011; it varies across districts, but not over time. The interaction of $\boldsymbol{I_t}$ and U_d allows for a union premium (or penalty) that changes over time. I label these DID estimators $\boldsymbol{\beta}$. As discussed above, the interaction of average daily membership in 2000, A_d, with the year fixed effects controls flexibly for urbanization.[25] I estimate Equation 1.1 using Ordinary Least Squares; all regressions report robust standard errors clustered at the district level.

I periodically examine how the effect of de-unionization varies by subgroup. In these instances, I introduce an additional set of multiplicative terms to Equation 1.1, multiplying

[25]The results are unchanged if I control instead for the log of average daily membership.

an indicator variable for subgroup participation with the DID estimators and year fixed effects. Linear combinations of $\boldsymbol{\beta}$ and the resulting differences-in-differences-in-differences (DDD) estimators test for an effect of prohibition among unionized subgroup teachers relative to their non-unionized subgroup peers. Results Section 1.4 explains each of these subgroup regressions in detail.

1.4 Main Results

1.4.1 Union Revenue

A sharp reduction in union revenue signals that the prohibition of bargaining took effect as intended. As discussed in Section 1.3.1, union revenue in Tennessee is reported to the Internal Revenue Service on Form 990. For ease of interpretation, I adjust revenue to account for different fiscal year-ends across the sample of unions – year-ends occur in nearly every month. I put all unions on a similar timeline by creating a weighted average of current and prior-year revenue, where the weights reflect the number of months that the union's fiscal year exceeds March (the earliest fiscal year-end in the data). For example, a union whose fiscal year ends in May receives 10/12 of current-year revenue and 2/12 of prior-year revenue. Table 1.2 summarizes the size of the unions in my sample.

Table 1.2: Union Revenue in Tennessee, Fiscal-Year 2009-10

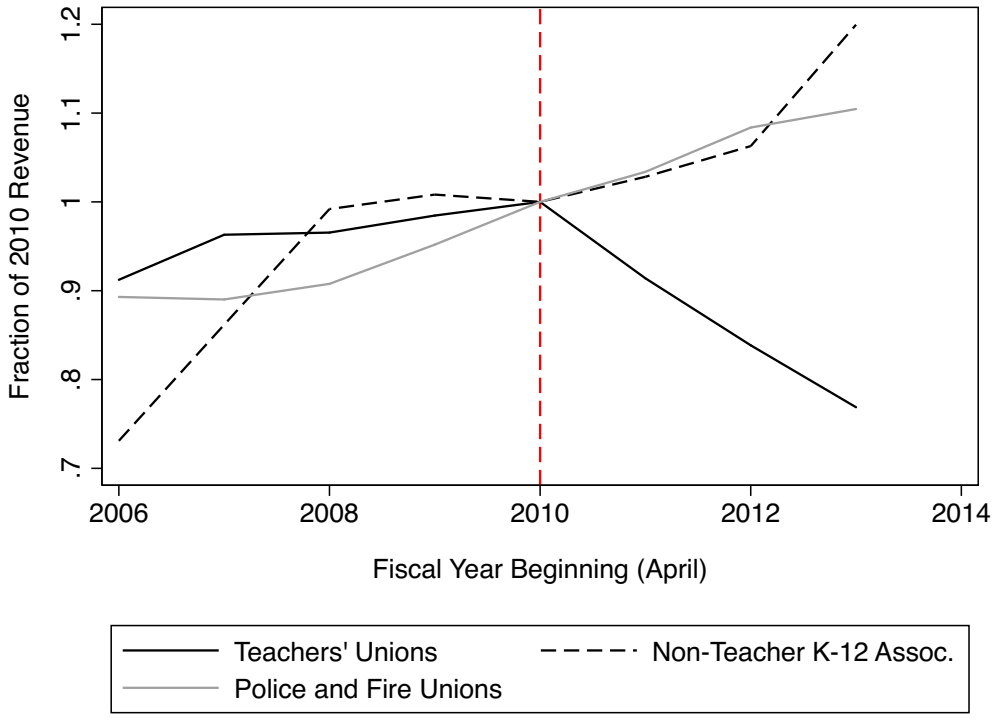
Union	N	Mean	SD	Min	Max
Teachers' Unions	12	1.591e+06	3.488e+06	45,194	1.251e+07
Other K-12 Professional Associations	9	354,392	608,571	18,184	1.909e+06
Police and Fire Unions	29	548,180	930,738	13,858	4.811e+06

Source: author's calculations from IRS Form 990 data provided by the National Center for Charitable Statistics.

Figure 1.2 plots total revenue across different sectors, as a fraction of total revenue earned by the sector in 2010. Teachers' unions steadily increased revenue collection until 2011, when receipts began to decline. By 2013, teachers' unions collected approximately 25 percent less revenue than they received in 2010. The prohibition of collective bargaining probably caused this sharp decline, since other sectors experienced 10 to 20 percent growth in total revenue over the same period. The Tennessee Education Association and its affiliates charged approximately $250 per year in dues between 2010 and 2016 (Tennessee Education Association, 2010-16). A 25 percent drop in revenue implies that more than 10,000 teachers quit union membership between 2010 and 2014.

Figure 1.2: Total Revenue as a Fraction of Total 2010 Revenue

Large Public Unions in Tennessee



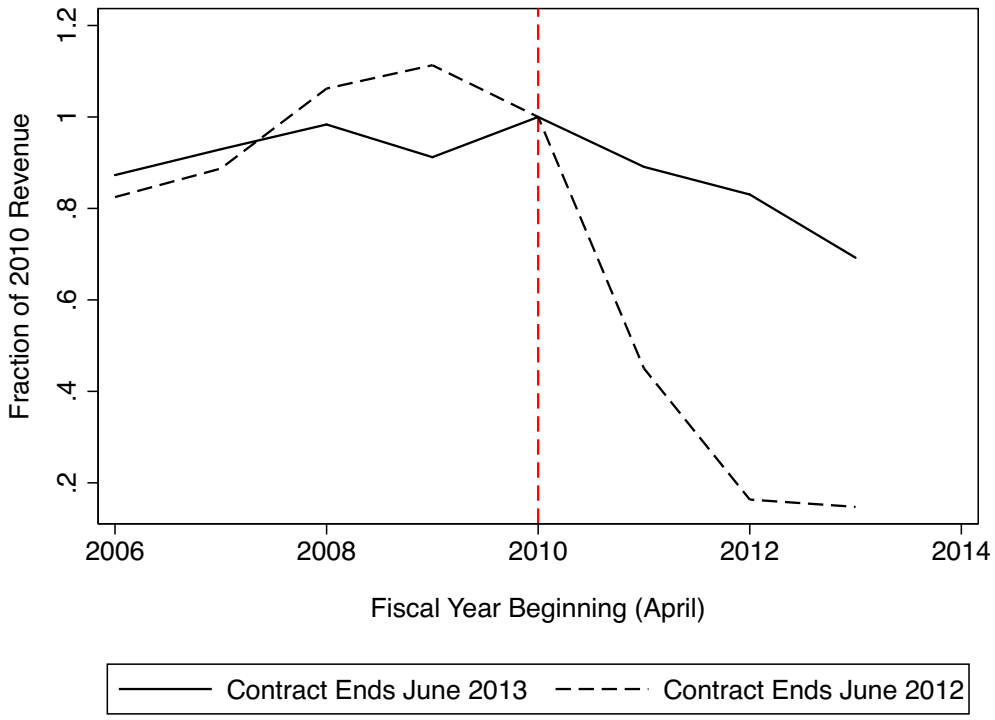
Source: author's calculations from IRS Form 990 data provided by the National Center for Charitable Statistics.

Notes: totals sum over 12 teachers' unions, 39 police and fire unions, and nine associations for K-12 management.

Figure 1.3 replicates the analysis for two teachers' unions whose last collectively bargained contracts I was able to find online.[26] Both unions experienced a substantial reduction in real revenue after 2011, but contract expiration dates alone do not fully explain the setback. The union whose contract expired on June 30, 2013 lost 20 percent of real revenue by March, 2014. The union whose contract expired on June 30, 2012 lost 80 percent of real revenue by March, 2013. Union funding appears to be influenced by union-employer relations. For example, the union whose contract expired in 2012 was engaged in legal conflict with the local school board.[27]

Figure 1.3: Total Revenue as a Fraction of Total 2010 Revenue

Two Large Teachers' Unions in Tennessee



Source: author's calculations from IRS Form 990 data provided by the National Center for Charitable Statistics.

[26]Union names are withheld at the request of the National Center for Charitable Statistics.

[27]References withheld to protect confidentiality, but available from the author upon request.

1.4.2 Teacher Salaries

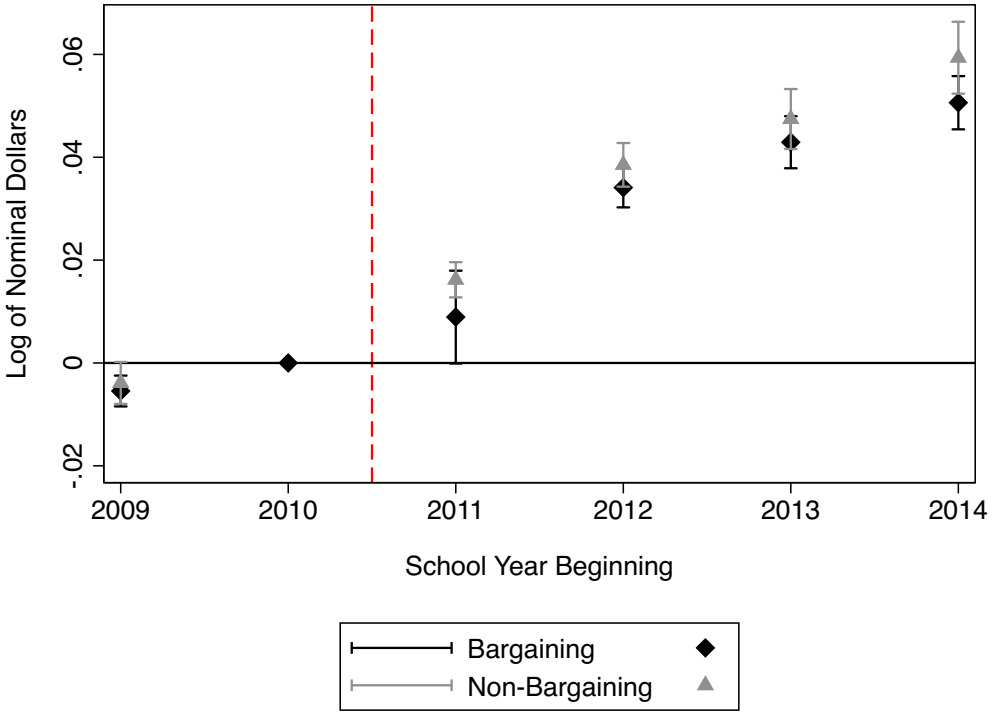
This section examines the generosity of teacher salaries. I estimate Equation 1.1 with log of annual salary as the dependent variable. Recall that salary includes not only base pay, but also supplements and bonuses. The district's salary schedule is typically announced during the spring or summer before the school year begins, so that de-unionization should gradually affect outcomes beginning in 2011. I focus on full-time classroom teachers by removing those earning below the State Minimum Salary.[28] Experience dummy variables and highest-degree-earned dummy variables removes bias from changes in demographic composition. I drop teachers with more than 30 years of experience since Appendix Figure A.4 shows that so few persist beyond this point.[29]

Figure 1.4 displays cumulative salary growth as predicted by Equation 1.1, controlling for teacher demographics and district size. By 2014, nominal salaries in the non-unionized districts had risen by six percent, whereas salaries in the unionized districts had risen by five percent. DID estimates of the difference in growth are presented in the first column of Table 1.3. The one-percentage-point difference in cumulative growth is both statistically significant and economically small. Ninety-five percent confidence intervals rule out even a three-percentage-point difference in cumulative growth. Replacing district with teacher fixed effects in column 2 does not change the story, but columns 3 and 4 show that the effect of de-unionization depends on district size. Removing the enrollment controls or limiting the sample to districts with 6,500 or fewer students in 2000 substantially attenuates my estimate.

[28]I exclude principals, supervisors of instruction, vocational teachers, substitute teachers, and home/hospital instructors.

[29]Teaching experience is occasionally unreliable in the data. A few teachers are credited zero years of experience in the year that they are hired, only to receive more than one the following year. Additionally, experience is missing for 2,835 teacher-year observations. I infer the correct experience from years prior or post. I also exclude teachers whose highest degree I cannot determine or who do not hold a Bachelor's degree – less than half of a percent of all teacher-year observations.

Figure 1.4: Teacher Salaries Relative to 2010, by Pre-Prohibition Bargaining Status



Source: author's estimates from data provided by the Tennessee Department of Education.

Notes: sample excludes districts with "priority" schools, Shelby County Schools, and Carroll County Schools. Salaries are paid during the school year indicated. The regression controls for licensed teaching experience, highest degree achieved, and average daily student membership in 2000 interacted with year dummies. Non-classroom teachers and those with more than 30 years of experience are excluded from the analysis. Standard errors are clustered at the district level.

Table 1.3: Effect of De-Unionization on Nominal Teacher Salaries

VARIABLES	(1) Log Salary	(2) Log Salary	(3) Log Salary	(4) Log Salary
Year 2009	-0.0039*	-0.0099***	-0.0035*	-0.0019
	(0.0021)	(0.0029)	(0.0021)	(0.0029)
Year 2011	0.0162***	0.0228***	0.0167***	0.0304**
	(0.0017)	(0.0024)	(0.0017)	(0.0132)
Year 2012	0.0385***	0.0515***	0.0392***	0.0389***
	(0.0021)	(0.0038)	(0.0022)	(0.0033)
Year 2013	0.0474***	0.0659***	0.0478***	0.0479***
	(0.0029)	(0.0058)	(0.0030)	(0.0037)
Year 2014	0.0594***	0.0853***	0.0603***	0.0633***
	(0.0035)	(0.0072)	(0.0036)	(0.0043)
Union * Year 2009	-0.0016	-0.0009	0.0001	0.0013
	(0.0025)	(0.0031)	(0.0027)	(0.0024)
Union * Year 2011	-0.0073	-0.0083	-0.0050	-0.0064
	(0.0047)	(0.0053)	(0.0037)	(0.0060)
Union * Year 2012	-0.0044	-0.0057*	-0.0011	-0.0005
	(0.0028)	(0.0032)	(0.0034)	(0.0026)
Union * Year 2013	-0.0045	-0.0054	-0.0028	-0.0015
	(0.0039)	(0.0049)	(0.0039)	(0.0036)
Union * Year 2014	-0.0088**	-0.0116**	-0.0043	-0.0054
	(0.0043)	(0.0053)	(0.0046)	(0.0045)
Observations	294,151	294,151	294,151	126,709
R-squared	0.793	0.969	0.793	0.798
Number of Districts	129	129	129	104
District FE	X		X	X
Experience Dummies	X	X	X	X
Education Dummies	X	X	X	X
ADM * Year Dummies	X	X		X
Teacher FE		X		

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Source: author's estimates from data provided by the Tennessee Department of Education.

Notes: columns 1-3 exclude districts with "priority" schools, Shelby County Schools, and Carroll County Schools. Column 4 also excludes districts with an average daily student membership greater than 6,500 in 2000. Salaries are paid during the school year indicated. "Experience" refers to years of teaching experience on the State teaching license, earned in Tennessee or elsewhere. "Education" denotes highest degree earned. "ADM" controls for average daily student membership in 2000. Non-classroom teachers and those with more than 30 years of experience are excluded from the analysis. Standard errors are clustered at the district level.

To better understand the relationship between de-unionization and district size, define an indicator variable – $S_{d,t}$ – equal to one if the district enrolled fewer than 4,000 students in 2000. In the main sample, 43 of 87 unionized districts enrolled so few students, while 36 of 42 non-unionized districts did so. In the "small" robustness sample, 43 of 62 unionized districts enrolled 4,000 or fewer. Equation 1.2 interacts the small-district indicator variable with the year dummy variables and DID estimators in Equation 1.1[30]

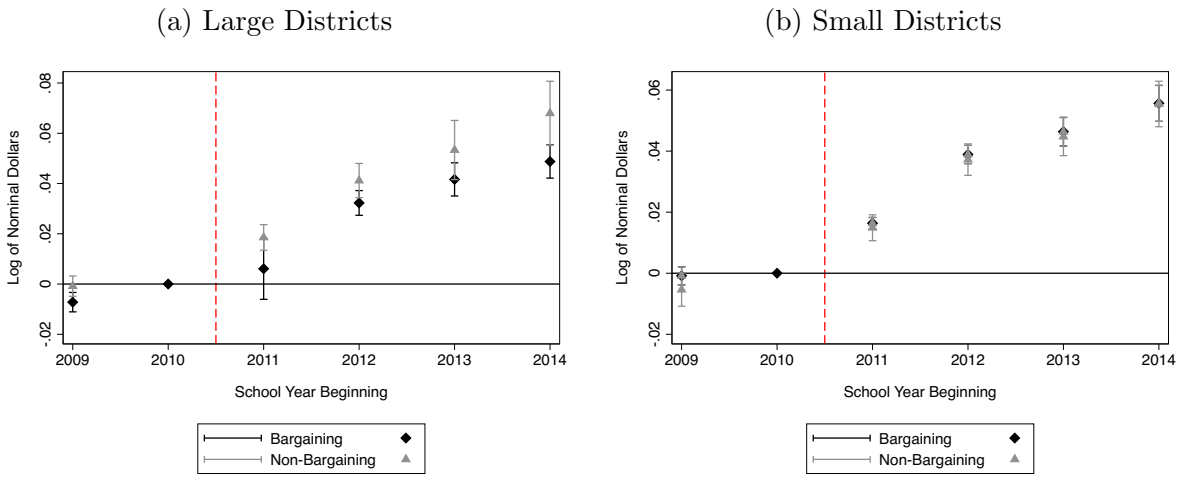
$$\begin{aligned} Y_{i,d,t} =& \alpha + \boldsymbol{I_t} + \boldsymbol{D_d} + S_{d,t} + \boldsymbol{I_t S_{d,t}} + U_d S_{d,t} + \boldsymbol{\beta(I_t U_d)} + \boldsymbol{\gamma(I_t U_d S_{d,t})} \\ &+ \boldsymbol{I_t A_d} + \epsilon_{i,d,t} \end{aligned} \tag{1.2}$$

The vector of DID coefficients $\boldsymbol{\beta}$ compares large unionized districts to their large non-unionized counterparts, while the linear combination of $\boldsymbol{\beta}$ and $\boldsymbol{\gamma}$ contrasts outcomes among small unionized and non-unionized districts.

Figure 1.5 shows clearly that de-unionization had a larger effect on salaries in the large districts. Salaries in the large non-unionized districts increased by seven percent between 2010 and 2014, whereas salaries only increased by five percent in the large unionized districts. Meanwhile, salaries in the small districts grew by five percent over this period, regardless of unionization. Columns 1 and 2 of Table 1.4 confirm that the two-percentage-point penalty among large districts is both statistically significant and statistically different from the effect of de-unionization on small districts. This result persists in the "small" robustness sample, suggesting that the relationship between de-unionization and size is not driven entirely by the very largest districts. Appendix Table A.6 reveals similar results if I replace the level enrollment controls with either log enrollment, level or log district population density, or a combination of enrollment and density. The results are also similar if I remove the enrollment controls altogether.

[30]The regression drops $S_{d,t}$ and $U_d S_{d,t}$ because they are co-linear with the district fixed effects. I leave them in the written version of Equation 1.2 for consistency with later subgroup regressions.

Figure 1.5: Teacher Salaries Relative to 2010, by Pre-Prohibition Bargaining Status



Source: author's estimates from data provided by the Tennessee Department of Education.

Notes: Large districts enrolled at least 4,000 students in 2000. Small districts enrolled fewer than 4,000 students that year. The sample excludes districts with "priority" schools, Shelby County Schools, and Carroll County Schools. Salaries are paid during the school year indicated. The regression controls for licensed teaching experience, highest degree achieved, and average daily student membership in 2000 interacted with year dummies. Standard errors are clustered at the district level.

Small average effects could mask a larger penalty among senior teachers. I test this hypothesis with two variants of Equation 1.2. In the first, I change the subgroup indicator, $S_{i,d,t}$, to equal one if the teacher has no more than 10 years of teaching experience – the median level of experience in the data. The results are displayed in columns 3 and 4 of Table 1.4. I find that senior teachers fare similarly to their junior colleagues in both the main sample and the "small" robustness sample. While one might worry about bias due to the selection of senior (or junior) teachers into urban school districts, Appendix Figure A.7 reveals no substantial experience differences across large and small districts.

As an additional test of the seniority hypothesis, I redefine the subgroup indicator to equal one if the teacher's current salary falls in the bottom half of her district's annual distribution of salaries. Equation 1.2 now compares the effect of de-unionization at different points of the district's wage distribution. Results in columns 5 and 6 of Table 1.4 are similar to those by absolute seniority; de-unionization seems to penalize most teachers in large districts, consistent with a district-wide wage freeze.

Table 1.4: Effect of De-Unionization on Nominal Teacher Salaries, by Subgroup

	Subgroup: Small		Subgroup: Junior		Subgroup: Low-Paid	
	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	Log Sal.	Log Sal.	Log Sal.	Log Sal.	Log Sal.	Log Sal.
Union * Year 2009	-0.006**	-0.004	-0.002	0.002	-0.003	0.002
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Union * Year 2011	-0.012**	-0.020	-0.008	-0.007	-0.007	-0.007
	(0.006)	(0.015)	(0.005)	(0.007)	(0.005)	(0.007)
Union * Year 2012	-0.009**	-0.005	-0.007*	-0.001	-0.005	0.002
	(0.004)	(0.004)	(0.004)	(0.003)	(0.004)	(0.003)
Union * Year 2013	-0.012*	-0.008	-0.007	-0.002	-0.006	-0.000
	(0.007)	(0.007)	(0.005)	(0.004)	(0.005)	(0.004)
Union * Year 2014	-0.019***	-0.017**	-0.012**	-0.005	-0.009*	-0.003
	(0.007)	(0.008)	(0.005)	(0.005)	(0.005)	(0.005)
Subgroup * Union * 2009	0.011**	0.009**	0.001	-0.002	0.001	-0.001
	(0.004)	(0.004)	(0.002)	(0.002)	(0.002)	(0.002)
Subgroup * Union * 2011	0.014**	0.022	0.001	0.001	0.001	0.002
	(0.007)	(0.015)	(0.002)	(0.002)	(0.002)	(0.003)
Subgroup * Union * 2012	0.011**	0.007	0.006	0.001	0.004	0.001
	(0.005)	(0.005)	(0.004)	(0.003)	(0.004)	(0.003)
Subgroup * Union * 2013	0.013*	0.010	0.005	0.001	0.003	0.001
	(0.008)	(0.008)	(0.004)	(0.003)	(0.004)	(0.003)
Subgroup * Union * 2014	0.019**	0.018*	0.006	-0.002	0.003	-0.001
	(0.009)	(0.009)	(0.005)	(0.003)	(0.005)	(0.003)
Observations	294,151	126,709	294,151	126,709	294,151	126,709
R-squared	0.793	0.798	0.793	0.799	0.810	0.814
Number of Districts	129	104	129	104	129	104
District FE	X	X	X	X	X	X
Subgroup * Year Dummies	X	X	X	X	X	X
Experience Dummies	X	X	X	X	X	X
Education Dummies	X	X	X	X	X	X
ADM * Year Dummies	X	X	X	X	X	X

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Source: author's estimates from data provided by the Tennessee Department of Education.

Notes: in columns 1-2, "subgroup" refers to districts enrolling fewer than 4,000 students in 2000; in columns 3-4, "subgroup" refers to teachers with fewer than 11 years of licensed experience; in columns 5-6, "subgroup" refers to teachers who earn salaries in the bottom half of their district's annual distribution. Columns 1, 3, and 5 exclude districts with "priority" schools, Shelby County Schools, and Carroll County Schools. Columns 2, 4, and 6 also exclude districts with an average daily student membership greater than 6,500 in 2000. Salaries are paid during the school year indicated. "Experience" refers to years of teaching experience on the State teaching license, earned in Tennessee or elsewhere. "Education" denotes highest degree earned. "ADM" controls for average daily student membership in 2000. Non-classroom teachers and those with more than 30 years of experience are excluded from the analysis. Standard errors are clustered at the district level.

The remainder of this section reports the results of two placebo tests. First, I confirm that the "de-unionization penalty" is not a preexisting trend toward lower salaries in districts that once bargained. Instead of personnel records, I analyze district-level data on average scheduled salaries published by the Tennessee State Board of Education. Each year, the TSBE collects official salary schedules from every district. The individual salary "steps" that contribute to the district mean are weighted by the percent of teachers across the state who have the requisite experience and education. I estimate Equation 1.1 at the district level, with log of average scheduled salary as the dependent variable. For comparability with the teacher-level analysis, I weight the regressions by the number of full-time-equivalent teachers in the district. Appendix Table A.7 is reassuring: scheduled salaries trend similarly before 2011, estimates are similar to those found using personnel records, and the de-unionization penalty appears to stabilize by 2015.

As a second placebo test, I look for an effect of de-unionization on the portion of teacher salaries that is paid by State equalization funds. The loss of union contracts could have an effect on State salaries, since, with minor exceptions, equalization funds are not strictly earmarked. However, the State does suggest uses for the funds it allocates, and I hypothesize that the State-funded portion of teacher salaries is much less sensitive to collective negotiation. I set the log of State-paid salary as the dependent variable in a teacher-level version of Equation 1.1. As before, I control for the experience and education of teachers, since these characteristics determine the level of State funding. The placebo DID estimates in Appendix Table A.8 are economically small and statistically insignificant regardless of sample or controls for district size.

1.4.3 Employer Cost of Teachers' Health Insurance

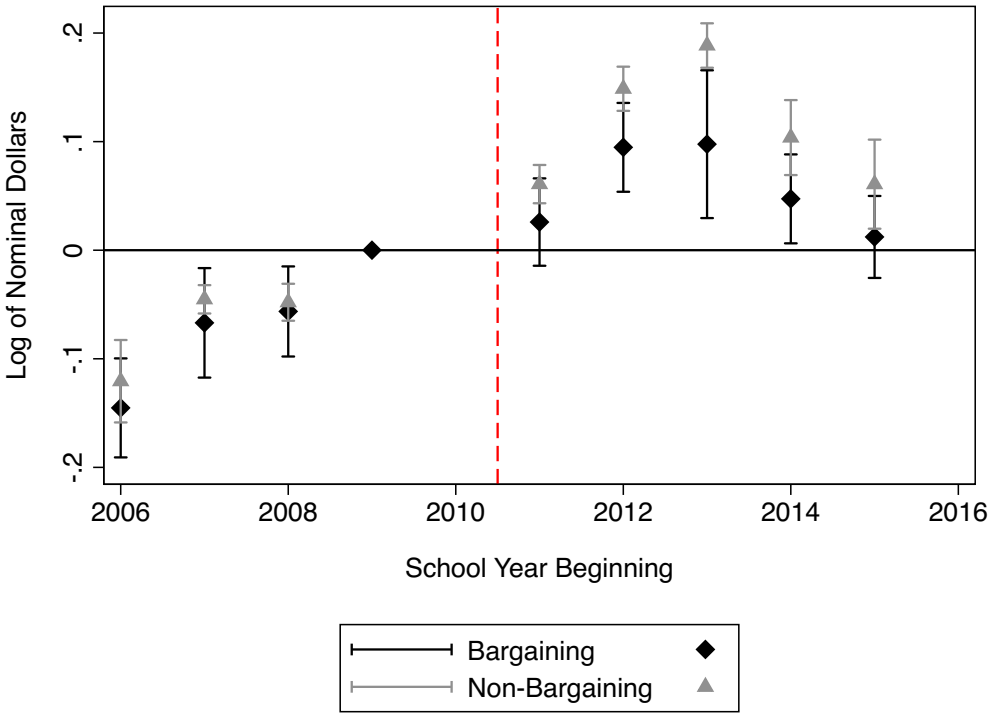
Many observers of collective bargaining contend that teachers' unions enhance fringe benefits more effectively than salaries. Data limitations compel me to focus on health insurance premiums as a measure of benefit generosity; specifically, I consider the weighted average premium paid by local school boards on behalf of teachers. If school boards decrease the employer cost, then teachers must either pay more for the same benefits or receive reduced benefits. Figure 1.6 displays the average effects estimated by Equation 1.1. For comparability with the salary regressions reported in Section 1.4.2, I weight the insurance regressions by the number of full-time-equivalent teachers employed by the district in each year.[31]

Employer premiums were rising rapidly in the non-unionized districts until 2014, when they began a steady decline. However, premiums in the unionized districts stabilized earlier, so that by 2015, de-unionization had reduced employer costs by five percentage points relative to the non-unionized districts. Since the average employer paid $6,308 for health insurance in 2009, a five-percentage-point reduction implies that employers saved around $300 per teacher. Although insufficient variation in the premiums data prevents me from estimating separate DID coefficients by district size, I infer differential effects by re-estimating Equation 1.1 with each district granted equal weight. Column 2 of Table 1.5 reveals that small districts again reacted much less to de-unionization. However, we should note that the very largest unionized districts drive much of this result, and these districts have no clear non-unionized counterparts for comparison. Restricting the sample to districts with 6,500 or fewer students in 2000 weakens the estimated effect considerably, even with teacher weights (column 3).

[31]The employer premiums are missing in 2010 because the Tennessee State Board of Education did not published updated data.

Figure 1.6: Employer-Paid Premiums for Teachers' Health Insurance

Relative to 2009, by Pre-Prohibition Bargaining Status



Source: author's estimates from data provided by the Tennessee Department of Education and the Tennessee State Board of Education.

Notes: sample excludes districts with "priority" schools, Shelby County Schools, and Carroll County Schools. The regression controls for average daily student membership in 2000 interacted with year dummies. Observations are weighted by the number of full-time-equivalent teachers in the district that year. Standard errors are clustered at the district level.

Table 1.5: Effect of De-Unionization on Nominal Employer Health Insurance Premiums

VARIABLES	(1) Log Prem.	(2) Log Prem.	(3) Log Prem.
Union * Year 2006	-0.025	0.008	0.012
	(0.029)	(0.017)	(0.024)
Union * Year 2007	-0.022	0.017	0.019
	(0.025)	(0.012)	(0.012)
Union * Year 2008	-0.008	0.023*	0.033**
	(0.022)	(0.013)	(0.015)
Union * Year 2011	-0.035	0.010	0.009
	(0.021)	(0.012)	(0.012)
Union * Year 2012	-0.054**	0.003	0.006
	(0.022)	(0.016)	(0.014)
Union * Year 2013	-0.091***	-0.009	0.002
	(0.034)	(0.017)	(0.015)
Union * Year 2014	-0.056**	-0.011	-0.014
	(0.026)	(0.023)	(0.026)
Union * Year 2015	-0.049*	-0.015	-0.012
	(0.028)	(0.025)	(0.031)
Observations	1,161	1,161	936
R-squared	0.873	0.904	0.887
Number of Districts	129	129	104
Teacher Weighted	X		X
District FE	X	X	X
Year Dummies	X	X	X
ADM * Year Dummies	X	X	X

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Source: author's estimates from data provided by the Tennessee Department of Education and the Tennessee State Board of Education.

Notes: columns 1 and 2 exclude districts with "priority" schools, Shelby County Schools, and Carroll County Schools. Column 3 also excludes districts with an average daily student membership greater than 6,500 in 2000. "ADM" controls for average daily student membership in 2000. Standard errors are clustered at the district level.

1.4.4 Student-Teacher Ratios

Since teachers' unions can enhance the size of the workforce by contracting on student-teacher ratios, we might expect school boards to reduce the number of teachers after union contracts expire. Alternately, local school boards might respond to lower salaries with increased demand for teachers. I define the student-teacher ratio as the average daily membership divided by the number of full-time-equivalent classroom teachers. In order to examine trends prior to 2009, I rely on district-aggregate data published in the *Annual Statistical Report of the Department of Education*. I estimate Equation 1.1 at the district level, setting student-teacher ratio as the dependent variable. Again, I weight the regressions to determine whether large and small districts react differently. Weighting by the average daily student membership in 2000 asks how an average pupil experienced de-unionization instead of an average school board.

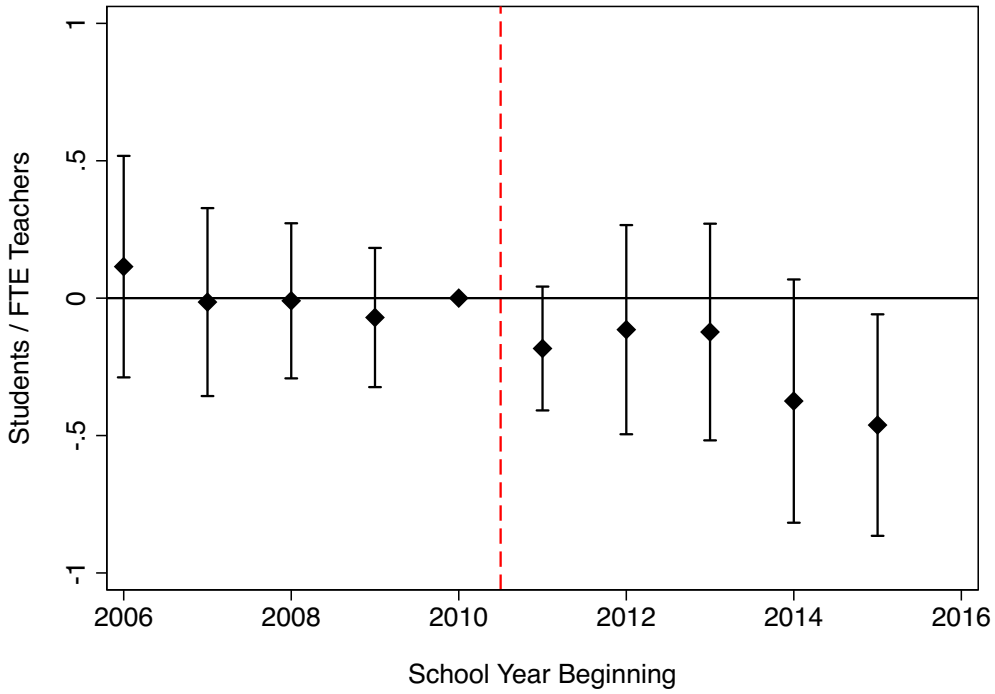
By 2015, most pupils in the unionized districts enjoyed classes that were half a student smaller, on average, relative to counterparts in the non-unionized districts (Figure 1.7). Unlike teacher compensation, class sizes were still reacting to de-unionization in 2015 and may decrease further. Unsurprisingly, the very largest districts appear to drive this result. The district-weighted regression in column 2 of Table 1.6 and its neighbor in column 3, which restricts the sample to districts with fewer than 6,500 pupils in 2000, both show limited effects of de-unionization.

One can decompose trends in the student-teacher ratio into trends in student enrollment and trends in teacher hiring. Since staffing strongly depends on the number of students, I first examine whether enrollment reacts to de-unionization. Conclusions should be considered suggestive because the DID estimates are imprecisely estimated. I re-estimate Equation 1.1 with the log of average daily membership as the dependent variable. Unlike the analysis of student-teacher ratio, my preferred specification grants each district equal weight. Columns 1 and 2 of Appendix Table A.9 reveal a two percentage-point decline in student enrollment, relative to the non-unionized districts, that is statistically

indistinguishable from zero. In a few years, we may observe a five-percent decrease corresponding to Lovenheim (2009), or the trend may reverse if parents react positively to smaller class sizes.[32] Conversely, large districts appear to have preserved their full-time equivalent teaching staff despite the dip in enrollment (columns 3 and 4).

Figure 1.7: Effect of De-Unionization on the Student-Teacher Ratio

Differences-in-Differences Estimates



Source: author's estimates from data provided by the Tennessee Department of Education.

Notes: sample excludes districts with "priority" schools, Shelby County Schools, and Carroll County Schools. The regression controls for average daily student membership in 2000 interacted with year dummies. Observations are weighted by the average daily student membership in 2000 and standard errors are clustered at the district level.

[32]If parents did withdraw their children from unionized districts, then we could see a corresponding increase in private-school enrollment. Unfortunately, the *Private School Universe Survey* administered by the National Center for Education Statistics has not been updated beyond the 2011-12 school year. To my knowledge, this is the only comprehensive database on private schools in Tennessee.

Table 1.6: Effect of De-Unionization on the Student-Teacher Ratio

VARIABLES	(1) Student-Teacher Ratio	(2) Student-Teacher Ratio	(3) Student-Teacher Ratio
Union * Year 2006	0.115	0.186	0.253
	(0.204)	(0.196)	(0.215)
Union * Year 2007	-0.014	0.191	0.173
	(0.173)	(0.181)	(0.174)
Union * Year 2008	-0.010	0.154	0.129
	(0.143)	(0.129)	(0.145)
Union * Year 2009	-0.070	0.070	0.067
	(0.128)	(0.124)	(0.126)
Union * Year 2011	-0.183	-0.183	0.001
	(0.114)	(0.154)	(0.180)
Union * Year 2012	-0.115	0.049	0.082
	(0.192)	(0.258)	(0.326)
Union * Year 2013	-0.123	0.177	0.176
	(0.199)	(0.249)	(0.300)
Union * Year 2014	-0.375*	-0.060	-0.156
	(0.224)	(0.220)	(0.260)
Union * Year 2015	-0.462**	-0.185	-0.251
	(0.204)	(0.189)	(0.212)
Observations	1,290	1,290	1,040
R-squared	0.766	0.742	0.715
Number of Districts	129	129	104
ADM Weighted	X		X
District FE	X	X	X
Year Dummies	X	X	X
ADM * Year Dummies	X	X	X

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Source: author's estimates from data provided by the Tennessee Department of Education.

Notes: columns 1 and 2 exclude districts with "priority" schools, Shelby County Schools, and Carroll County Schools. Column 3 also excludes districts with an average daily student membership greater than 6,500 in 2000. "ADM" controls for average daily student membership in 2000. Standard errors are clustered at the district level.

1.5 Supplemental Analyses

The results presented above raise two questions that I attempt to answer in this section. First, does de-unionization allow districts to maintain student achievement at lower cost? And second, do teachers separate at a higher rate after the loss of bargaining rights? Claims to follow should be considered suggestive since the regression results are either less precise than earlier estimates or limited to the very short run.

1.5.1 Student Test Scores

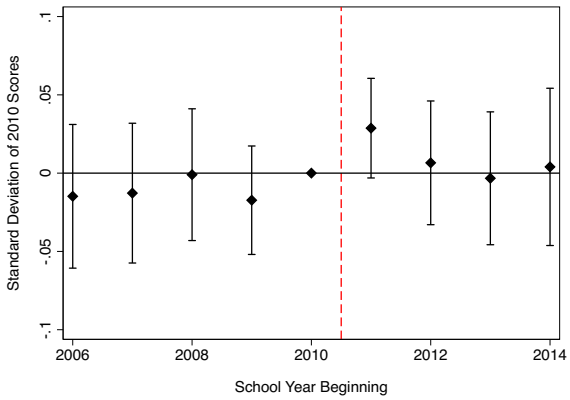
I examine student performance on a standardized exam as a proxy for overall productivity in the school district. Specifically, I consider math and reading scores of children in grades three through eight on the Tennessee Comprehensive Assessment Program. The Tennessee Department of Education aggregates individual scores to the level of district, grade, subject, and year. For ease of interpretation, I normalize the raw scores by subtracting the mean of the state-wide distribution in 2010 and dividing by the standard deviation of the state-wide distribution in 2010, for each grade and subject. Thus, the unit of analysis is a standard deviation of the 2010 distribution of student scores. Since the exam was graded on a different scale prior to 2009, we may be wary of comparing 2008 with 2009, but we can still assess trends from 2006 to 2008 and from 2009 to 2014.

I re-estimate Equation 1.1 including grade-by-year fixed effects for grades six through eight, since not all districts offer the higher grades. Table 1.7 compares the vector of DID estimates from three specifications, each of which aggregates the individual student scores differently. Column 1 considers mean scores, by district-grade-subject-year, with each observation receiving equal weight. Column 2 switches the dependent variable to median score, while column 3 reverts to mean scores, but weights the regression by the number of students in each district-grade-year. For ease of interpretation, panels (a) through (c) of Figure 1.8 depict the DID estimates graphically.

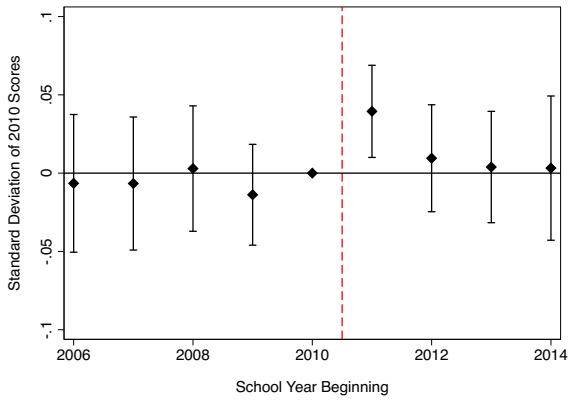
Figure 1.8: Effect of De-Unionization on District-Aggregate Standardized Test Scores

Math and Reading in Grades 3-8

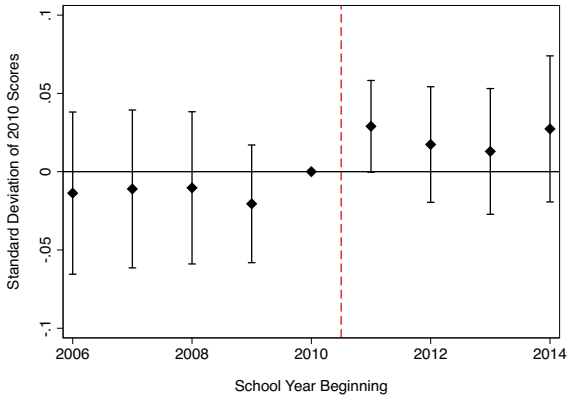
(a) Mean Score within Grade and Subject



(b) Median Score within Grade and Subject



(c) Mean Weighted by Number of Students



Source: author's estimates from data provided by the Tennessee Department of Education.

Notes: sample excludes districts with "priority" schools, Shelby County Schools, and Carroll County Schools. Individual student scores are aggregated within each combination of district, grade, and subject. Z-scores subtract the mean score across the entire state, within grade and subject, from the district-aggregate score and divide by the state-wide standard deviation within grade and subject. Regressions control for grade dummies and average daily student membership in 2000, both interacted with year dummies. Standard errors are clustered at the district level.

Table 1.7: Effect of De-Unionization on District-Aggregate Math and Reading Scores in Grades 3-8

VARIABLES	(1) Z-Score (Mean)	(2) Z-Score (Median)	(3) Z-Score (Mean)	(4) Z-Score (Mean)	(5) Z-Score (Median)	(6) Z-Score (Mean)
Union * Year 2006	-0.015	-0.007	-0.014	-0.008	0.004	-0.001
	(0.023)	(0.022)	(0.026)	(0.025)	(0.025)	(0.027)
Union * Year 2007	-0.013	-0.007	-0.011	-0.008	0.001	-0.002
	(0.023)	(0.021)	(0.025)	(0.025)	(0.024)	(0.027)
Union * Year 2008	-0.001	0.003	-0.010	0.007	0.012	0.001
	(0.021)	(0.020)	(0.025)	(0.024)	(0.023)	(0.027)
Union * Year 2009	-0.017	-0.014	-0.021	-0.015	-0.010	-0.020
	(0.018)	(0.016)	(0.019)	(0.019)	(0.018)	(0.021)
Union * Year 2011	0.029*	0.039***	0.029*	0.027	0.042**	0.021
	(0.016)	(0.015)	(0.015)	(0.018)	(0.017)	(0.016)
Union * Year 2012	0.007	0.010	0.017	0.016	0.019	0.019
	(0.020)	(0.017)	(0.019)	(0.023)	(0.020)	(0.021)
Union * Year 2013	-0.003	0.004	0.013	0.007	0.014	0.022
	(0.021)	(0.018)	(0.020)	(0.025)	(0.021)	(0.022)
Union * Year 2014	0.004	0.003	0.027	0.005	0.008	0.020
	(0.025)	(0.023)	(0.024)	(0.029)	(0.026)	(0.024)
Observations	13,770	13,770	13,770	11,070	11,070	11,070
R-squared	0.993	0.993	0.995	0.993	0.993	0.994
Number of Districts	129	129	129	104	104	104
District FE	X	X	X	X	X	X
Year Dummies	X	X	X	X	X	X
Grade Dummies * Year Dummies	X	X	X	X	X	X
ADM * Year Dummies	X	X	X	X	X	X
Student-Weighted			X			X

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Source: author's estimates from data provided by the Tennessee Department of Education.

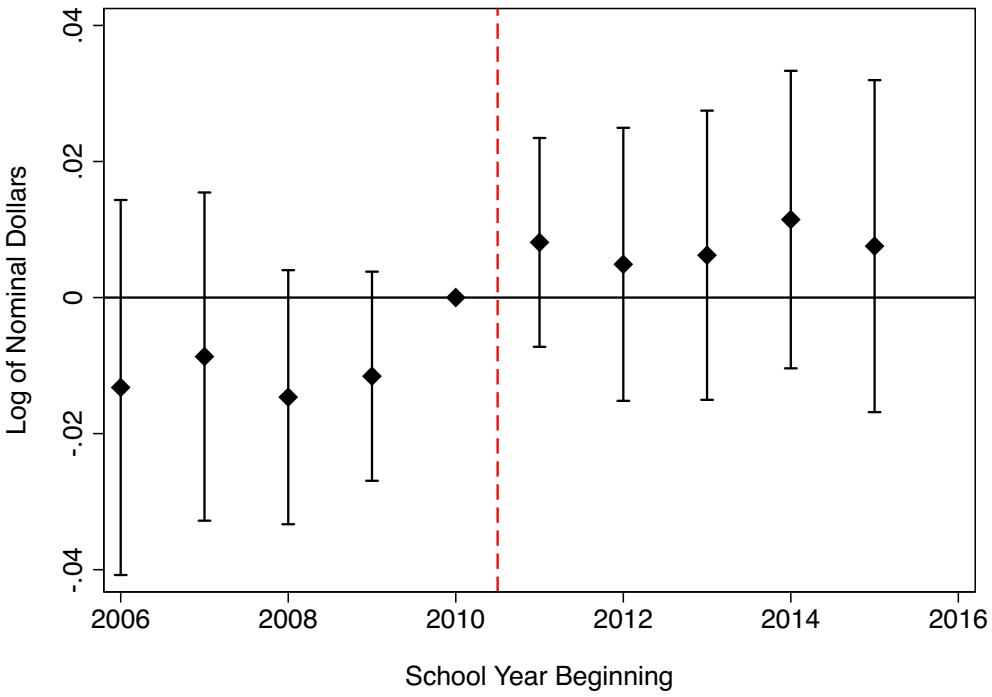
All three specifications suggest that de-unionization did not alter student achievement in the short run. (Columns 4 through 6 of Table 1.7 repeat this exercise on the "small" robustness sample of districts, with similar results). However, the point estimates are insufficiently precise to rule out small improvements. As a benchmark, consider the Tennessee STAR experiment that ran between 1985 and 1989. This project randomly assigned young students in Tennessee to classes with seven fewer students than normal. Participation in a small class for one year increased student test scores by approximately 0.2 standard deviations (Krueger, 1999; and Mosteller, 1995). Based on these results, we might expect test scores to improve by 0.01 to 0.02 standard deviations after de-unionization, since class sizes decreased by half a student, on average. Effects of this magnitude are within the confidence intervals depicted in Figure 1.8, particularly when the DID estimates are weighted by the number of students taking the exam.

Appendix Table A.10 performs an additional series of sensitivity tests. As before, the first three columns report on the main analysis sample, while the last three limit the sample to districts with 6,500 or fewer students in 2000. Columns 1, 2, 4, and 5 examine math and reading scores separately to check for differential trends by subject matter. Columns 3 and 6 control for the annual percent change in the number of test takers, lest trends in enrollment subtly alter the socioeconomic composition of union districts. None of these alternate specifications contradict the main finding.

1.5.2 Total Expenditure

Overall, de-unionization appears to reduce teacher compensation with no short-run loss of staff or student achievement. Are school districts becoming more efficient? I use Equation 1.1 to examine the log of total current operating expenditure per-pupil, weighting each observation by the average daily membership in that year. Instead of implying efficiency gains, Figure 1.9 shows that per-pupil expenditure has either remained constant or increased after de-unionization (Appendix Table A.11). Reductions in class size counteracted the cost savings from salary and benefit freezes. Since this result is insensitive to both the weighting of the regression and the sample of districts, I conclude that de-unionization probably had no effect on per-pupil expenditure in the short run.

Figure 1.9: Effect of De-Unionization on Per-Pupil Operating Expenditure



Source: author's estimates from data provided by the Tennessee Department of Education.

Notes: per-pupil expenditure divides total current operating expenditure by the average daily student membership in that year. The sample excludes districts with "priority" schools, Shelby County Schools, and Carroll County Schools. The regression controls for average daily student membership in 2000 interacted with year dummies. Observations are weighted by the average daily membership in the district that year. Standard errors are clustered at the district level.

1.5.3 Teacher Turnover

Several factors suggest that de-unionization should increase teacher turnover in Tennessee. Teachers in unionized districts may seek alternate uses for their time as salaries decline relative to non-unionized districts. Additionally, with termination and grievance procedures no longer subject to union oversight, district managers can more easily remove low-productivity staff. I create a binary, teacher-level indicator for leaving one's current district at the end of the school year, and estimate Equation 1.1 as a linear probability model using Ordinary Least Squares.

I find no short-run effect of de-unionization on the probability of separation. The DID estimates in column 1 of Table 1.8 never exceed one percentage point, are precisely estimated, and are quite stable across specifications. Column 2 of Table 1.8 removes the student enrollment controls while column 3 limits the sample to districts with 6,500 or fewer students in 2000. Neither of these sensitivity tests contradict the null finding.

Table 1.8: Effect of De-Unionization on the Probability of Leaving Employment

VARIABLES	(1) Pr(Leave)	(2) Pr(Leave)	(3) Pr(Leave)
Year 2009	-0.0192**	-0.0202**	-0.0267**
	(0.0084)	(0.0084)	(0.0112)
Year 2011	-0.0048	-0.0037	-0.0184*
	(0.0062)	(0.0062)	(0.0098)
Year 2012	0.0037	0.0044	-0.0082
	(0.0075)	(0.0075)	(0.0104)
Year 2013	0.0046	0.0054	-0.0071
	(0.0082)	(0.0081)	(0.0101)
Union * Year 2009	0.0068	0.0019	-0.0037
	(0.0095)	(0.0100)	(0.0095)
Union * Year 2011	-0.0068	-0.0018	-0.0144*
	(0.0067)	(0.0068)	(0.0077)
Union * Year 2012	-0.0068	-0.0030	-0.0076
	(0.0080)	(0.0080)	(0.0088)
Union * Year 2013	-0.0030	0.0007	-0.0059
	(0.0088)	(0.0087)	(0.0101)
Observations	252,949	252,949	109,554
R-squared	0.030	0.030	0.031
Number of Districts	129	129	104
District FE	X	X	X
Experience Dummies	X	X	X
Education Dummies	X	X	X
ADM * Year Dummies	X		X

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Source: author's estimates from data provided by the Tennessee Department of Education.

Notes: teachers separate at the end of the current school year by transferring to another district or exiting the dataset. Columns 1-2 exclude districts with "priority" schools, Shelby County Schools, and Carroll County Schools. Column 3 also excludes districts with an average daily student membership greater than 6,500 in 2000. "Experience" refers to years of teaching experience on the State teaching license, earned in Tennessee or elsewhere. "Education" denotes highest degree earned. "ADM" controls for average daily student membership in 2000. Non-classroom teachers and those with more than 30 years of experience are excluded from the analysis. Standard errors are clustered at the district level.

1.6 Discussion

Political efforts to de-unionize education are gaining momentum in many states. Yet, the effect of union activity on K-12 public schools remains poorly understood. In this paper, I examine how teacher labor markets in Tennessee reacted to a 2011 prohibition of collective bargaining. I employ a differences-in-differences research design, comparing districts that once bargained to those that never did, before and after the policy change.

The loss of union contracts caused a modest reduction in teacher compensation and school administrators responded with increased demand for teachers. Between 2011 and 2015, salaries in unionized districts grew one percentage-point less, cumulatively, while employer contributions for health insurance premiums grew five percentage-points less, cumulatively. Class sizes fell simultaneously by half a student, relative to those in non-unionized districts. Student test scores have yet to show a response, either positive or negative, although the results are insufficiently precise to rule out small effects.

What explains such modest changes? Working conditions may adjust over a long time horizon, with correspondingly slow effects on student achievement. The prohibition of collective bargaining took effect during the 2011-12 school year, with grandfathered contracts expiring as late as 2013. Although salaries and health insurance premiums had stabilized by 2015, relative class sizes may continue to decline today. Additionally, I assume that districts were unaffected by de-unionization if they were not bargaining prior to 2011. Yet, school districts in Tennessee might all interact in equilibrium, competing for workers and students by enacting similar compensation and workforce policies.

Alternately, state politics may be an important mechanism through which public unions influence the civil service. Tennessee's teachers' unions lost at least 30 percent of their annual pre-prohibition revenue. While much of this revenue is no longer needed to support collective bargaining, some of it funds political activism at the state and local levels. Recall the official statement of the Tennessee Education Association regarding the prohibition of bargaining: "Its sponsors only care that TEA endorsed legislators [...] who

happen to be Democrats" (Mance, 2011). With fewer resources in 2016, the TEA is in an undoubtedly weaker position to lobby legislative committees and donate to friendly campaigns.

Actions by the State Legislature contribute significantly to teacher salaries and working conditions across all districts in Tennessee. State appropriations fund approximately 50 percent of public-school budgets each year. The Legislature mandates teacher tenure laws and evaluation policies, along with maximum class sizes, teacher certification requirements, and performance targets on standardized exams. Assume that the prohibition of collective bargaining reduced union influence over the State Legislature by removing the funds needed to lobby. The resulting changes to State policy impact all teachers, regardless of their district's bargaining status. Yet, my differences-in-differences research design misses these effects. Political changes often phase in gradually, so that it is still too early to judge outcomes in Tennessee. There is much scope for future research on the political role of union contracts in the public sector.

Chapter 2

How Tax Overrides Affect Teacher Employment and School Choice

In voting to allow the city to raise property taxes beyond the limits imposed by state law, residents said yes to an additional tax burden that may save 18 jobs, including 14 in the city schools and four police officers. (MA local newspaper: Cain and Crowley, 2013)

The combination of the State-imposed revenue cap, increases in operating costs, and the requirements of the QEO [Wisconsin's duty to collectively bargain] have resulted in ... even more permanent cuts in staff, curriculum, programs and services. (Newsletter: WI Prescott School District, 2007)

2.1 Introduction

Do legal restrictions on the growth of municipal expenditures in the United States restrict the employment of public-school teachers? If so, do parents respond to larger classes by removing their children from affected schools? Precipitated by the "Tax Revolt" of the 1980s, tax and expenditure Limits (TELs) are state-level statutory restrictions on municipal revenue or expenditure. Thirty state governments had a self-imposed TEL

as of 2008 (Gordon and Rueben, 2009), often permitting growth based on an index of population, personal income, and inflation. Proponents of TELs argue that weak oversight of government officials combined with (presumed) bureaucratic tastes for higher spending creates a principal-agent problem (Cutler et al., 1999; and Ladd and Wilson, 1982). Municipal administrators often counter that overly-stringent budget constraints impair the quality of public services by forcing reductions in staff – rhetoric reflected in the quotes above from local news outlets. Since recent academic work confirms that small classes improve children's short-run test scores and long-run earnings, expenditure limits that constrict teacher hiring could result in lower-quality schools (Chetty et al., 2011; and Fredriksson et al., 2013).

Surprisingly, studies of tax and expenditure limits in school districts find inconsistent effects on instructional spending, of which teacher payroll comprises a large portion. For example, Dye and McGuire (1997) show no impact in Illinois, whereas Shadbegian (2003) looks at TELs across the nation and finds a slight increase in the student-teacher ratio, but no effect on teacher salaries. Local referendums to override TELs are one explanation for the divergence. Bradbury et al. (2000) argue that property-tax caps in Massachusetts only reduced total spending in school districts that could not pass an override. Similarly, Downes et al. (1998), extending the Illinois analysis to include student test scores, discover that some districts fared much worse under the revenue limit than others, and mention that override referendums might have caused the variance.[1]

Relaxing the budget constraint with an override need not translate to payroll enhancements. School-district administrators may regard instructional spending as a fixed cost and react to TELs by adjusting administrative expenditure or capital investments (see Downes and Figlio, 2007, for a review of this literature). Imagine that competition between school districts for students and teachers forces administrators to preserve class sizes and teacher compensation or endure rapidly declining enrollments that lead to still

[1]Zycher (2013) argues that local governments relax the budget constraint by developing alternate sources of revenue and shifting expenditures to off-balance-sheet debt (such as public pensions).

smaller budgets (Downes and Figlio, 2001). Alternately, administrators may face strict collective bargaining laws that prevent teachers from experiencing significant hardship. In this model, a state-imposed budget constraint compels administrators to allocate a larger share of total spending to instruction than they would have preferred. Overrides enable administrators to achieve the optimal budget allocation by purchasing non-instructional inputs. Figlio and O'Sullivan (2001) argue that district administrators publicly threaten (and sometimes enact) staff cuts to secure an override even when requested funds are intended for alternate purposes. Their model requires local voters to be imperfectly informed about public budgets both before and after an override referendum – an assumption that often seems reasonable.

This study seeks to arbitrate between conflicting views of override referendums with a natural experiment in Wisconsin. In 1995, the state government legislated that school-district revenue cannot grow faster than an annual, per-pupil dollar amount specified by statute. However, residents can vote to override the cap in their local district. With detailed teacher personnel records and data on district finances, I observe how the three components of teacher payroll – employment, salaries, and the employer cost of fringe benefits – differ among districts that barely won and barely lost an override referendum.

I find that districts spread override funds evenly across their budgets, increasing large expenditure categories by a constant percentage. Teacher hiring drove payroll enhancements, rather than salaries or fringe benefits. Specifically, overrides that increased the growth of total revenue by three percentage points, on average, caused a two percentage-point increase in the growth of the number of full-time equivalent teachers. Yet, the effect on class size was smaller than that on hiring because the growth of district enrollment also grew by one percentage point. Unsurprisingly, limited local control over school spending further disadvantaged districts with already low levels of per-pupil spending because these districts were less able to secure override funds.

My findings indicate that tax and expenditure limits do constrain teacher employment

and that overrides at least partially explain the variance in saliency across school districts. Parents believe that overrides enhance school quality, although it is not clear whether they value small classes above other inputs. If parents prefer schools with non-instructional amenities (Cellini et al., 2010), then overrides intended for capital improvements may attract students and force district administrators to hire more teachers. Nevertheless, my results are consistent with arguments that school spending enhances student achievement by improving the teaching staff (Figlio and Rueben, 2001; and Jackson et al., 2016).[2]

2.2 Empirical Context

Funding for Wisconsin schools comes primarily from state aid, distributed progressively according to property values in the district, and local property taxes. In 1995, the Wisconsin State Legislature stipulated that annual increases in school-district revenue cannot exceed a per-pupil dollar amount specified by statute.[3] For example, districts could collect an additional $275 per pupil during the 2008-09 school year (Kava and Olin, 2011). Districts desiring more revenue can override the cap with a majority vote of local residents (Kava and Merrifield, 2013). Three distinct types of overrides may be requested: 1) a debt override permits a bond issuance with service payments excluded from the revenue limit; 2) a non-recurring override triggers property tax increases for a specific number of years; and 3) a recurring override increases property taxes in perpetuity. I focus on recurring and non-recurring overrides because these types of referendums typically request funds for operating purposes, rather than to enable large construction projects.

Historically, most public-school employees in Wisconsin were members of a labor union.[4] School districts, which were required to collectively negotiate salaries and benefits, could

[2]Whether public-school budgets affect student achievement is a matter of some debate. See Betts (1995 and 1996) and Hanushek (2001) for an opposition to Figlio and Rueben (2001) and Jackson et al. (2016).

[3]Act 16, which legislated temporary revenue limitations, was renewed permanently in 1997.

[4]Act 16 of 1993 codified bargaining procedures.

avoid binding arbitration if they made a "Qualified Economic Offer" (QEO) to their teachers. The QEO set a minimum increase in total compensation of 3.8 percent. This increase was split between the rising cost of fringe benefits, of which pension contributions and health insurance premiums are the largest components, and salary adjustments. Thus, if fringe-benefit costs increased by 1.7 percent (of total compensation), salaries rose by 2.1 percent. All contracts expired on June 30 of odd-numbered years, causing short-term rigidity in the labor market for teachers (Wisconsin Legislative Reference Bureau, 1998). Until recently, teacher salaries followed strict schedules based on position, highest degree achieved, and total years of experience.

In 2011, the state legislature limited public-sector collective bargaining to salaries, the growth in which is now capped based on the Consumer Price Index.[5] Legislators simultaneously reduced the generosity of pension and retiree health insurance benefits. Full-time teachers all participate in the Wisconsin Retirement System, a defined-benefit pension plan controlled by the State Legislature. Although teachers could never bargain over key pension provisions, districts were permitted to choose health insurance providers. This enabled benefit enhancements when budgets grew quickly and retractions when premiums grew faster than property values could sustain (Zimmerman, 2011).

2.2.1 Regression Discontinuity Design

I identify the effect of relaxing the revenue limit by assuming that referendums determined by a close vote randomly assign districts to receive override funds. Although local economic conditions and unobserved political activism are likely to influence the probability that voters favor an override, differences should be negligible among districts where referendums are determined by one vote. To implement this research design, I rely on a

[5] Act 10 of 2011, first effective during the 2011-12 school year.

standard local-linear regression discontinuity equation:

$$\begin{aligned} Y_{d,r,t} =& \alpha + \beta_1 VoteShare_{d,r} + \beta_2 Passed_{d,r} + \beta_3 (VoteShare_{d,r} * Passed_{d,r}) \\ &+ Year_t + \epsilon_{d,r,t} \end{aligned} \tag{2.1}$$

The level of observation is a referendum (r) held in district d and the impact of which is observed in year t. The sample only contains districts that held at least one referendum, with those that held multiple referendums appearing as many times. $VoteShare_{d,r}$ denotes the number of "yes" votes divided by the total number of votes cast, normalized to equal zero at the 50 percent threshold for passing an override. $Passed_{d,r}$ is a binary indicator equal to one if the override passed and zero otherwise. The regression coefficient β_2 measures whether $Y_{d,r,t}$ changes discontinuously and systematically when the normalized vote share passes from negative to zero. I consider β_2 to be the causal effect of passing an override. To control for trends in override passage and outcomes over time, $Year_t$ represents a vector of school-year fixed effects. I estimate Equation 2.1 with Ordinary Least Squares, clustering standard errors at the district level.

2.2.2 Data and Summary Statistics

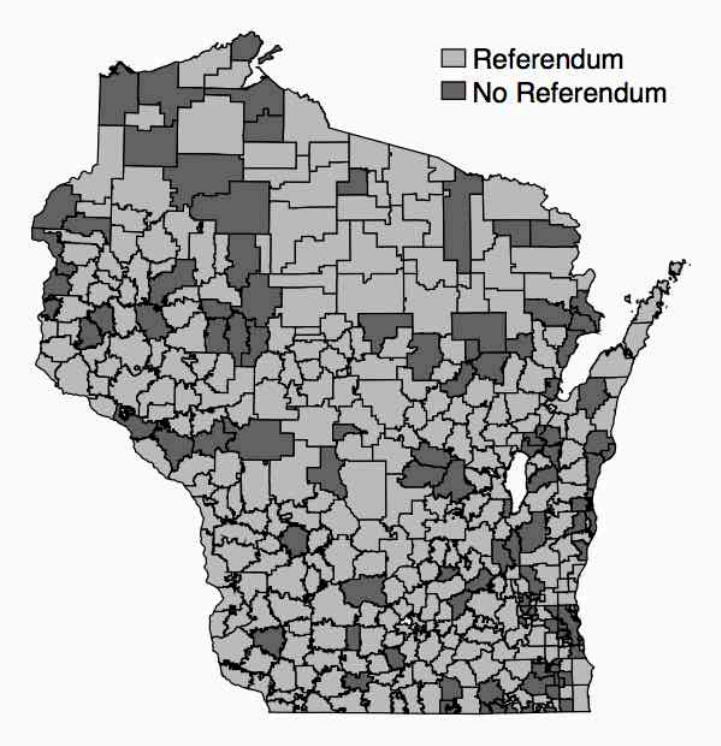
This project employs data publicly released by the Wisconsin Department of Public Instruction. School districts' *Annual Financial Reports* contain detailed budgets and enrollment per district. Additionally, the Department of Public Instruction publishes a database of historical personnel records for all Wisconsin public school employees. Unfortunately, the Department does not assign each employee a unique, time-invariant identification number, so it is difficult to track individual salary growth over time.[6] For this reason, I aggregate key variables to the district level including: mean teacher salary, employer cost of fringe benefits, the percent of teachers with a Master's Degree or higher, the mean number of years of experience in education, and the number of teachers employed

[6]One can develop a matching algorithm by name, birth year, sex and race. However, female employees who marry and change their name appear as two distinct observations in the resulting dataset.

by the district. Teacher counts and mean salaries are based on the concept of full-time equivalency, where two teachers who work half time equal one full-time employee.

Crucially, the Wisconsin Department of Public Instruction also publishes the results of local override referendums. This database includes the number of "yes" and "no" votes cast, the amount of revenue requested, the first school year in which the district intends to collect the funds, and a brief description of how the funds will be spent. Between 1995 and 2013, 312 school districts – approximately 70 percent of the total number of districts in Wisconsin – asked residents to vote on 940 tax override measures. Figure 2.1 shows that these referendums were evenly geographically distributed across the state and Appendix Table B.1 reveals the same dispersion over time. While most non-debt override requests sought to increase "operating revenue" (often defined in vague terms), 175 were earmarked for specific capital expenditures. I retain these overrides in the analysis because necessary capital investments could crowd out payroll enhancements.

Figure 2.1: Wisconsin School Districts by Referendum Status, 2013



Source: data provided by the Wisconsin Department of Public Instruction.

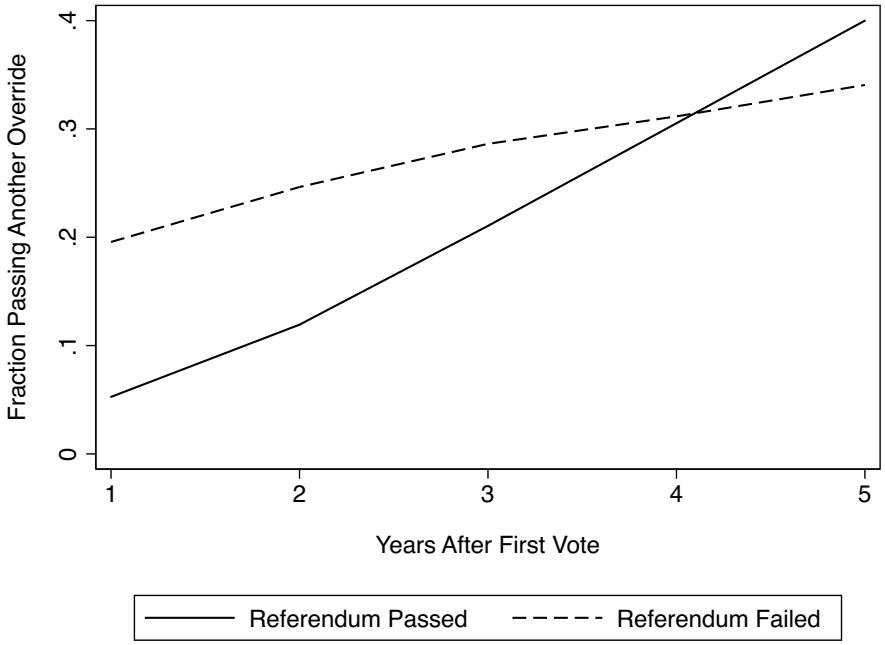
Note: map displays the boundaries of elementary school districts.

The median referendum asked taxpayers to approve a six-percent increase in the district's total expenditure. Approximately 45 percent of override referendums asked for recurring revenue. The remainder requested additional funds for four years on average. Half of referendums — mostly recurring overrides – failed to list a date on which the district would first access the additional funds; in these cases, I assume that the district planned to access override funds the following school year since 84 percent of referendums with non-missing dates applied funds to the next school year. I limit the sample to referendums held between the 1995-96 and 2007-08 school years, since so few were held before this point and so as not to overlap the fundamental shifts in state education policy that occurred in 2011.

Districts in the analysis sample not only held multiple referendums over many years, but sometimes in the same year or even the same day. The district of Rhinelander in 2008 serves as a good example. In April, it failed to pass a $334,000 recurring override. It reduced the amount of the override to $225,000 and tried again, unsuccessfully, in September. It raised the issue one last time in November, still failing to obtain a tax increase. Although Rhinelander was unusually persistent, districts that failed to pass an override were 15 percentage points more likely to pass one the following year (Figure 2.2). Not until four years after the first override were the districts that had been originally successful again more likely to have passed at least one override.

I make several additional sample restrictions to account for districts that obtain override funds even after losing the first vote. I only consider the first referendum that a district holds in a given calendar year. Referendums held as a third attempt are unlikely to resolve randomly, even conditional on a close vote, because persistent administrators have better knowledge of taxpayer preferences and can fine-tune the amount of funds requested to pass by a bare majority (Young et al., 2007). In addition, I improve the explanatory power of the first stage by dropping all referendums held on the same date as others in the same district (approximately 20 percent of all referendums). Lastly, I exclude referendums that requested funds in excess of 20 percent of total operating expenditure

Figure 2.2: Probability of Passing Another Override After Initial Referendum



Source: author's calculations from data provided by the Wisconsin Department of Public Instruction.

Note: graph shows the fraction of school districts passing at least one additional override in the years after an initial override referendum.

– five percent of the sample – since these districts clearly face extraordinary budgetary pressures or plan to undertake major capital investments.

The worst-funded school districts in Wisconsin were less likely to override the revenue cap than their better-funded counterparts. For this reason, override referendums may have exacerbated existing resource inequalities in Wisconsin, relative to a revenue cap with no local control. Table 2.1 reports the results of a simple probit regression, where the outcome equals one if the district ever held a referendum (column 1) or passed an override (columns 2 and 3) between the 1995-96 and 2007-08 school years. With one observation per district, the regression predicts future override passage based on minimal school-district characteristics during the 1994-95 school year – before the state-imposed revenue cap.

Table 2.1: Probability of Obtaining At Least One Override, 1996-2008 School Years

Marginal Effects from a Probit Regression

VARIABLES (1995 Values)	(1) Held Referendum	(2) Passed Override	(3) Passed Override
Log Pupils	0.0511**	0.0152	0.0358
	(0.0253)	(0.0244)	(0.0264)
Middle Third Per-Pupil Exp.	0.178***	0.199***	
	(0.0513)	(0.0589)	
Top Third Per-Pupil Exp.	0.0414	0.0831	
	(0.0547)	(0.0598)	
Middle Third Class Size			-0.0557
			(0.0608)
Top Third Class Size			-0.125**
			(0.0612)
Observations	427	427	427

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Source: author's estimates from data provided by the Wisconsin Department of Public Instruction.

Notes: marginal effects from a one-unit increase, holding all other variables constant at their means. Regressions are at the school district level. Explanatory variables reflect the 1994-95 school year, while the dependent variables reflect cumulative experience during the 1995-96 through 2007-08 school years. "Middle third" and "top third" refer to the district's position in the 1994-95 distribution.

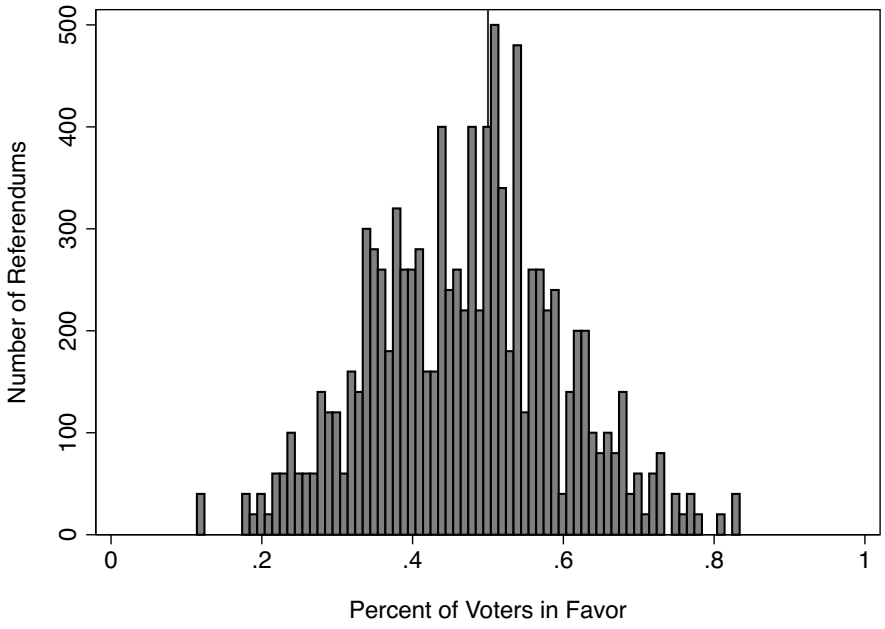
Strikingly, districts that fell in the middle third of the distribution of per-pupil instructional expenditure were 20 percentage points more likely to pass at least one future override than districts in the bottom third.[7] Those in the top third were also more likely to eventually obtain override funds, but the effect is smaller in magnitude and not statistically significant.[8] This pattern holds when I substitute the student-teacher ratio in 1994-95 for per-pupil instructional spending (calculating the ratio by dividing the number of enrolled students by the number of full-time equivalent teachers). Column 3 of Table 2.1 shows that districts with the largest class sizes were 13 percentage points less likely to relax the revenue cap than those in the bottom third of the class-size distribution. Larger districts were more likely to hold referendums, but not more likely to win them.

[7] I report marginal effects holding all other variables constant at their means.

[8] Maher and Skidmore (2008) attribute this particular hump-shaped propensity to Wisconsin's progressive formula for allocating state equalization aid.

Although override funds were not randomly assigned, close wins and losses appear quasi-random in the data. Figure 2.3 plots the distribution of referendums by the percent of voters in favor of passage. Narrow wins occur with comparable frequency to narrow losses, assuaging concerns that school district administrators or election officials could be manipulating the vote share. For additional reassurance, Table 2.2 compares the characteristics of school districts within a narrow bandwidth of the 50 percent "yes" requirement for override passage. All financial variables are adjusted to reflect constant September 2012 dollars using the "all urban" series of the Consumer Price Index. In general, districts holding near-loss "treatment" and near-win "control" referendums appear statistically similar. However, overrides do appear more likely to fail when the funds requested are large relative to the current budget. Fortunately, the robustness checks in Section 2.3.1 show that controlling for the size of the request does not alter the results. Appendix Table B.2 presents detailed summary statistics for the full analysis sample of referendums.

Figure 2.3: Distribution of Referendums, by Percent of Voters in Favor of Passage



Source: author's calculations from data provided by the Wisconsin Department of Public Instruction.

Table 2.2: Characteristics of School Districts the Year Referendums Were Held

Where 40-60 Percent of Voters Favored Passage

Weighted by the Number of Referendums

Variable Means	Override Passed	Override Failed	P-Value
Funds Requested Relative to Expenditure	0.048	0.056	0.057
Log Total Revenue	16.401	16.469	0.588
Log Total Expenditure	16.396	16.467	0.570
Log Instructional Expenditure	15.781	15.869	0.483
Log Student Enrollment	7.229	7.119	0.406
Log Number of FTE Teachers	4.490	4.547	0.643
Log Average FTE Teacher Salary	11.128	11.116	0.555
Log Average Employer Fringe Cost	10.269	10.223	0.128
	Growth from Prior Year		
Growth in Total Revenue	0.006	0.003	0.441
Growth in Total Expenditure	0.006	0.001	0.267
Growth in Instructional Expenditure	-0.014	-0.015	0.978
Growth in Student Enrollment	-0.006	-0.004	0.627
Growth in Number of FTE Teachers	-0.005	0.003	0.149
Growth in FTE Teacher Salary	-0.002	-0.008	0.390
Growth in Employer Fringe Cost	0.028	0.018	0.349

Source: author's calculations from data provided by the Wisconsin Department of Public Instruction.

Notes: the unit of observation is a referendum. Standard errors for P-value calculation are clustered at the district level. All financial variables are transformed to reflect real September 2012 values using the Consumer Price Index. "Fringe" measures the employer cost of providing non-wage compensation including Social Security, Medicare, the Wisconsin Retirement System, health insurance (including retiree), and paid time off. "FTE" refers to full-time equivalent teachers: two teachers working half-time equal one full-time equivalent teacher.

2.3 Results

Before examining teacher payroll in detail, I first confirm that passing an override increased school-district revenue and examine the effect on student enrollment. Each panel of Table 2.3 presents the regression discontinuity estimate, β_2, for three different outcome variables at different points in time. The first, third, and fifth columns of Table 2.3 examine each outcome during the first school year that districts were supposed to gain access to override funds, as indicated by district administrators during the referendum. Since negotiated teacher contracts were sticky over two-year intervals, the second, fourth, and sixth columns examine outcomes one school year after districts first gained access to funds. The three panels of Table 2.3 alter the number of referendums admitted to the sample, first focusing only on those decided by a relatively close vote, then opening the sample to increasingly more referendums. The regression discontinuity estimates are consistent across bandwidths, although small sample sizes tend to decrease statistical power. Panels (a), (b), and (c) of Figure 2.4 offer graphical depictions of the formal regression results.

I examine the cumulative percent growth in outcomes, from the year before districts were supposed to gain access to funds to the year under consideration. Focusing on growth, rather than levels, reduces noise from variation in district size pre-override. Column 1 of Table 2.3 shows that passing an override increased the growth of total revenue by three percentage points. This "first stage" effect is less than the average value of funds requested – five percent of total expenditure – because some of the districts that failed to pass an initial override won subsequent referendums.[9] Non-compliance by the "control" districts also weakened the difference in revenue growth over time, as demonstrated by the slightly smaller coefficients in column 2.

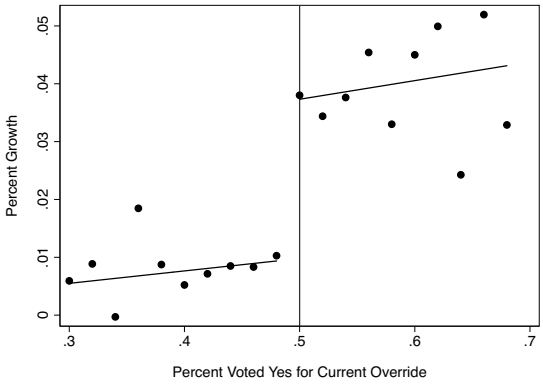
[9]State aid for education may have also decreased as local expenditures increased.

Figure 2.4: Effect of Winning an Override Referendum on Growth Rates (Percent)

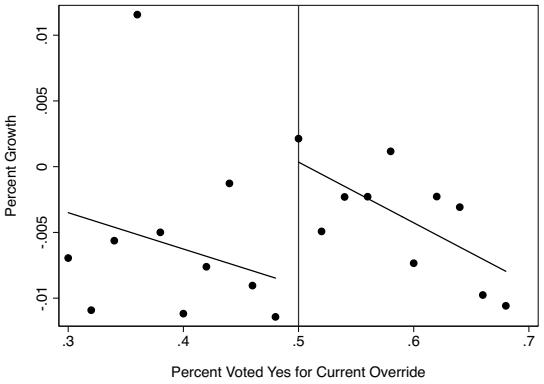
Growth From Year Before Funds Available to Year First Available

Residuals Controlling for Year Fixed Effects (Binscatter)

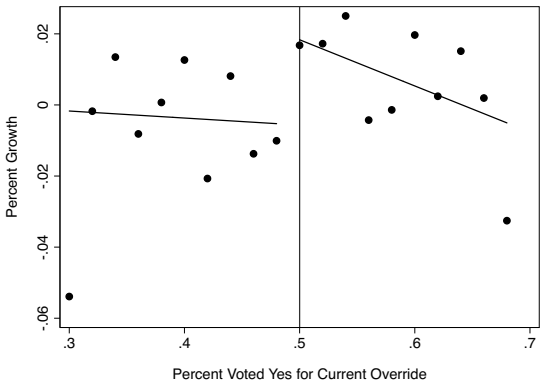
(a) Total Revenue



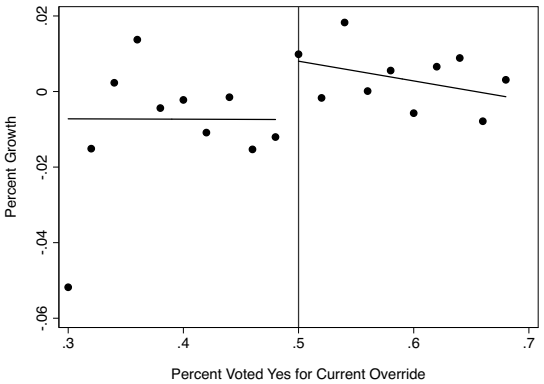
(b) Student Enrollment



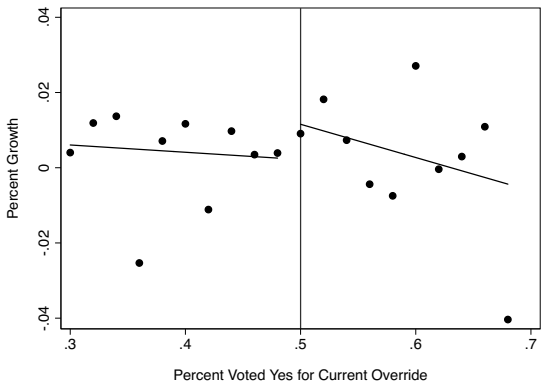
(c) Teacher Payroll (Including Fringe)



(d) Number of FTE Teachers



(e) Average Teacher Compensation



Source: author's calculations from data provided by the Wisconsin Department of Public Instruction.

Notes: regressions are at the referendum level. Districts holding multiple referendums enter the regressions as many times. All financial variables have been transformed to reflect real September 2012 values using the Consumer Price Index. "Fringe" measures the employer cost of providing non-wage compensation including Social Security, Medicare, the Wisconsin Retirement System, health insurance (including retiree), and paid time off. "FTE" refers to full-time equivalent teachers: two teachers working half-time equal one full-time equivalent teacher.

Table 2.3: Effect of Passing an Override on Revenue, Enrollment, and Aggregate Teacher Compensation

	Cum. % Growth Total Revenue		Cum. % Growth Pupils		Cum. % Growth Agg. Comp.	
VARIABLES	(1) First Year	(2) Second Year	(3) First Year	(4) Second Year	(5) First Year	(6) Second Year
			Vote-Share Bandwidth = 10 Points			
Vote-Share Threshold	0.029***	0.026**	0.009	0.006	0.038***	0.032
	(0.007)	(0.010)	(0.006)	(0.010)	(0.013)	(0.027)
Observations	275	275	275	275	275	275
R-squared	0.496	0.326	0.094	0.094	0.182	0.176
			Vote-Share Bandwidth = 20 Points			
Vote-Share Threshold	0.028***	0.027***	0.010**	0.010	0.026**	0.038*
	(0.005)	(0.008)	(0.005)	(0.008)	(0.010)	(0.020)
Observations	438	438	438	438	438	438
R-squared	0.368	0.217	0.059	0.039	0.132	0.123
			All Votes in Sample			
Vote-Share Threshold	0.028***	0.025***	0.008*	0.005	0.021**	0.032*
	(0.005)	(0.007)	(0.004)	(0.008)	(0.008)	(0.018)
Observations	501	501	501	501	501	501
R-squared	0.382	0.223	0.056	0.044	0.113	0.102
% Voted Yes	X	X	X	X	X	X
% Voted Yes * Passed	X	X	X	X	X	X
Year FE	X	X	X	X	X	X

Standard errors in parentheses, clustered at the district level
*** p<0.01, ** p<0.05, * p<0.1

Source: author's estimates from data provided by the Wisconsin Department of Public Instruction.
Notes: aggregate compensation equals the district's wage bill and total cost of fringe benefits. Financial variables are transformed to reflect real September 2012 values. Growth rates are measured from the year before school districts first collect override funds to the first and second years after districts collect funds. "Vote-share bandwidth" refers to the number of referendums admitted to the sample based on the fraction of voters supporting override passage.

Interestingly, column 3 of Table 2.3 shows that student enrollment grew one percentage point faster in the districts that gained access to override funds. The effect is short lived and statistical significance quickly fades (column 5). Nevertheless, some parents acted as if override revenue enhanced the quality of local schools. Override funds also enhanced aggregate teacher compensation, the growth rate of which immediately increased by three percentage points (column 5). Since the growth of revenue and aggregate compensation increased by the roughly same amount, I infer that districts spent override funds proportionately to their existing budget allocations. In support of this conclusion, Appendix Table B.3 examines the effect of passing an override on administrative expenditure. As before, a three percentage-point increase in the growth of district revenue lead to a three percentage-point boost in the growth of outlays for administration. Together, teacher compensation and administrative costs accounted for 90 percent of total operating expenditure in all years, on average.

Is payroll growth driven by teacher employment or compensation? The first two columns of Table 2.4 display regression discontinuity estimates of the cumulative percent growth in the number of full-time equivalent teachers, from the year before access to funds to the first and second years of access. Panel (d) of Figure 2.4 depicts this regression graphically. Employment growth during the first year of access was two percentage-points higher as a result of override funds, and this higher growth rate persisted slightly into the second period. Nevertheless, hiring did not translate to an appreciable decrease in the student-teacher ratio because students also enrolled in response to the override (regressions not shown). As before, these results are insensitive to the choice of vote-share bandwidth.

Table 2.4: Effect of Passing an Override on Teacher Employment and Compensation

	Cum. % Growth FTE Teachers		Cum. % Growth Avg. Comp.	
VARIABLES	(1) First Year	(2) Second Year	(3) First Year	(4) Second Year
	Vote-Share Bandwidth = 10 Points			
Vote-Share Threshold	0.025**	0.029*	0.012	-0.004
	(0.010)	(0.016)	(0.011)	(0.027)
Observations	275	275	275	275
R-squared	0.099	0.117	0.293	0.285
	Vote-Share Bandwidth = 20 Points			
Vote-Share Threshold	0.016**	0.022*	0.010	0.008
	(0.007)	(0.012)	(0.009)	(0.020)
Observations	438	438	438	438
R-squared	0.081	0.097	0.213	0.249
	All Votes in Sample			
Vote-Share Threshold	0.012*	0.018*	0.008	0.005
	(0.006)	(0.010)	(0.008)	(0.017)
Observations	501	501	501	501
R-squared	0.082	0.104	0.191	0.246
% Voted Yes	X	X	X	X
% Voted Yes * Passed	X	X	X	X
Year FE	X	X	X	X
Δ % Advanced Degree			X	X
Δ Avg. Years Experience			X	X

Standard errors in parentheses, clustered at the district level
*** p<0.01, ** p<0.05, * p<0.1

Source: author's estimates from data provided by the Wisconsin Department of Public Instruction.

Notes: "FTE" refers to full-time equivalent teachers. Average compensation equals the district's wage bill and cost of fringe benefits divided by the number of FTE teachers. Financial variables are transformed to reflect real September 2012 values. Growth rates are measured from the year before school districts first collect override funds to the first and second years after districts collect funds. "Vote-share bandwidth" refers to the number of referendums admitted to the sample based on the fraction of voters supporting override passage. Hence, a 10-point bandwidth admits referendums where 40 to 60 percent of voters favored passage.

Conversely, I find no compelling evidence that districts used override funds to enhance teacher compensation. I consider the average full-time equivalent compensation of teachers in the district, which is calculated by summing payroll and the total employer cost of fringe benefits, then dividing by the number of full-time equivalent teachers. Because I am interested in the generosity of compensation offered by the district, I control for changes in the demographic composition of teachers – specifically, the level change in the fraction of all teachers holding advanced degrees and the level change in the average years of teaching experience. As before, I focus on the percent growth in average compensation from the year before districts accessed funds to the first and second years after receiving override revenue. Columns 3 and 4 of Table 2.4 suggest that cumulative growth is no more than one percentage-point higher after passing an override, although standard errors on the point estimates cannot rule out very small effects. Analyzing salaries and fringe benefits separately produces similar results.

2.3.1 Robustness Checks

The summary statistics in Table 2.2 suggest that districts were more likely to barely lose a referendum when they asked for generous funding. What if the funding request is systematically related to underlying conditions that also affect enrollment, hiring, and teacher salaries? I rule out this possibility by re-estimating the main results controlling for funds requested relative to total expenditure (measured during the school year prior to receiving override revenue). To simplify the presentation, I focus on the first school year that districts should have gained access to override funds according to their referenda requests. Although the effect on enrollment attenuates somewhat after controlling for funds requested, columns 1 through 3 of Appendix Table B.4 reveal no substantive alteration to the main results.

Which came first: the teachers or the students? Even if school-district administrators had no desire to enhance teacher payroll, they may have been forced to preserve class sizes

if parents responded to non-instructional amenities by enrolling children in well-funded schools. Columns 4 and 5 of Appendix Table B.4 re-estimate the effects of override funds on the growth of payroll and staff, controlling for the percent growth in student enrollment. As expected, comparing districts on similar enrollment trajectories attenuates the effect of override funds on staff size: a three percentage-point increase in the growth of total revenue causes a one percentage-point increase in the growth of full-time equivalent staff and a two percentage-point increase in payroll growth. Nevertheless, these statistically-significant estimates suggest that district administrators would have used override funds to support teachers even absent the influx of pupils.

Did statistical noise produce the small effects documented above? To check this hypothesis, I re-estimate regression discontinuities for the three main variables of interest: student enrollment, payroll, and the number of full-time equivalent teachers. I assume that referendums pass with alternate (placebo) percentages of "yes" votes, and compare the placebo "effects" with the discontinuities that I found at the 50 percent vote margin. I choose placebo thresholds at 30, 40, 60, and 70 percent of the vote, and limit the vote-share bandwidth to 10 percentage points so as not to overlap the actual threshold for passing an override. Panels (a) through (c) of Appendix Figure B.1 reveal that the placebo regression discontinuity estimates are typically zero or negative in magnitude and never statistically significant.

2.4 Conclusion

This study examines how local referendums to override a state-legislated cap on school-district revenue in Wisconsin affect teacher employment and parents' perceptions of school quality. Proponents of tax and expenditure limits hope to ameliorate the principal-agent problem that could arise when public administrators prefer large budgets and experience little oversight from taxpayers. However, these laws may also hurt student achievement if they enlarge classes and repel high-quality teachers. District administrators often

threaten to eliminate staff if residents do not relax the expenditure constraint with a local referendum. Absent close taxpayer monitoring of administrator behavior, such threats might be a bluff designed to obtain funds for non-instructional purposes.

Regression discontinuity estimates based on close votes reveal that the growth of aggregate teacher compensation (salaries and fringe benefits) increased by three percentage points for every three percentage-point gain in the growth of total revenue. Additions to the full-time-equivalent teaching staff drove this effect, rather than enhancements to salaries or fringe benefits. Parents responded to successful overrides by enrolling their children in funded schools. Specifically, a three-percentage point increase in the growth of total revenue increased the growth of student enrollment by one percentage point. Of course, these are short-run effects in a context of repeated interaction. Were districts unable to constantly request override revenue, relaxing the budget constraint by such a small amount might not alter payroll.

Why were district administrators more likely to adjust staff size than teacher salaries or benefits? Wisconsin maintained a strong duty-to-bargain law during the analysis period, including mandated pay raises, which may have prevented administrators from reducing salaries or benefits after a failed override. Compensation recruits high-quality teachers, but the effect on student achievement likely differs in magnitude from the effect of small classes. Identifying the margin along which districts adjust payroll will prove important for predicting the effect of tax and expenditure limits on school quality.

Nevertheless, these results help explain a puzzle in the previous literature: why do state-mandated caps constrict instructional resources much more in some school districts than others? In Wisconsin, overrides added teachers to districts that already had relatively high levels of per-pupil expenditure and relatively small class sizes. Allowing for limited local control narrowed the resource gap between students in affluent and middle-income communities, but further disadvantaged those in the very worst-funded schools.

Chapter 3

Pay for Seniority: Do Defined-Benefit Pensions Retain Government Employees?

Most civil servants in the United States participate in generous, employer-provided defined-benefit pensions, but must remain on the job for many years in order to receive benefits. Since the mid 1990s, nearly 20 state governments partially or totally converted to defined-contribution retirement savings accounts that travel with employees to all future employers (Munnell et al., 2014). The financial crisis of 2008 and ensuing recession intensified the debate around defined benefit versus defined contribution. Although financial health varies enormously across the more than 6,000 state and local defined-benefit pensions (U.S. Census Bureau, 2015), a number of government employers face large unfunded liabilities.[1] For example, pension costs could exceed 15 percent of own-source revenue in Illinois, New Jersey, Connecticut, and Kentucky if these states calculated their liabilities with a semi-conservative discount rate and adhered to a strict funding schedule (Munnell

[1]Immediately after the financial crisis of 2008, analysts valued the nation-wide unfunded pension liability at 0.5 trillion (eight percent discount rate) to 2.9 trillion (four percent discount rate). See Munnell et al. (2010) and Pew Center on the States (2010).

and Aubry, 2016).[2] Many county governments, particularly in California, face a much greater cost burden. In contrast, defined-contribution plans do not guarantee benefit levels and thus are always fully funded.

The recent debate has paid little attention to the effect of pension design on the composition of the civil service, despite implications for employer cost and employee productivity. Workers who participate in a defined-benefit pension, but who are not yet eligible for retirement, earn benefits that increase exponentially with each additional year of tenure at the firm (Beshears et al., 2011; Costrell and Podgursky, 2009; and Poterba et al., 2007). In contrast, workers who save for retirement in a defined-contribution account often accrue a substantial portion of the final account balance in the early years of employment, since contributions earn interest over a long time horizon regardless of the worker's career trajectory. Hence, defined-benefit pensions are deferred compensation, encouraging workers to remain with one employer (Coile and Gruber, 2001; Lazear, 1986; Stock and Wise, 1990; and Yellen, 1984). If workers value this deferred compensation, then defined-benefit pensions should not only increase early and mid-career retention in the civil service, but attract workers who are pre-disposed to long careers in government.

The empirical relationship between pension design and employee behavior is much less clear. Workers who are covered by defined-benefit pensions are also more likely to persist in their jobs (Allen et al., 1988; Ippolito, 1991; Mitchell, 1982; Munnell et al., 2012; and Stock and Wise, 1990). Government employers with generous defined-benefit pensions are better able to attract and retain highly skilled workers (Munnell et al., 2015). However, pension design may correlate with unobserved aspects of personnel policy that also influence recruitment and retention.[3] Meanwhile, reduced-form research designs are scarce. Brown (2013), Costrell and Podgursky (2009), and Koedel et al. (2013) show that defined-benefit pensions encourage teachers to separate at the normal retirement

[2]The authors calculate pension costs assuming a six-percent nominal discount rate and amortized unfunded liabilities over a closed 30-year period.

[3]For example, Gustman and Steinmeier (1993) find no difference in retention after controlling for detailed firm characteristics.

age.[4] Similarly, Liebman et al. (2009) show that older workers adjust their labor supply in response the Social Security benefit formula. While suggestive, none of these studies examine employee decision making at the beginning of the career and all focus on older workers.[5]

This paper examines the effect of deferred pension benefits on employee retention over the long run. On April 1, 1997, the state government of Michigan closed its defined-benefit pension to new entrants and transitioned to a defined-contribution system. Members of the traditional pension were only eligible for benefits after earning 10 years of tenure in state government, whereas defined-contribution participants owned the contents of their savings accounts almost immediately. Since membership in the defined-benefit pension is based on date of hire, I identify the effect of back-loaded compensation with a regression discontinuity design. I assume that workers hired within a few months of each other would turnover similarly if they faced the same pension incentives.

From the detailed personnel records of all state government employees in Michigan, I find that defined-benefit membership substantially decreased turnover among older workers, but had no effect on their younger colleagues. Workers who fell in the top half of the age distribution of new hires – 13 years away from retirement, on average – were 17 percentage points more likely to remain at least a decade in state service if they had a defined-benefit pension. The effect rose to 30 percentage points among those with occupations requiring a college degree, and dropped to 10 percentage points among their less highly skilled peers. A simple accounting of the pension wealth earned in each of the two retirement plans suggests that older workers were willing to remain for an additional four years in order to receive (discounted) pension wealth equal to twice their salary at separation. Younger workers were unwilling to persist an additional four years to earn pension wealth worth a year's salary. Once vested in the pension, older workers persisted

[4]In a similar vein, Fitzpatrick (2014), Gruber and Madrian (1995), and Leiserson (2013) examine how access to health insurance influences labor mobility and retirement decisions.

[5]Fitzpatrick (2015) estimates that teachers are only willing to trade 20 cents in wages for an additional dollar of pension benefits.

until eligible to receive benefit payments.

These results suggest that governments contemplating pension reform should anticipate a younger workforce with increased turnover and less employer-specific human capital. Governments are likely to incur productivity losses as they recruit and train new employees, particularly in highly skilled occupations. However, governments may also realize productivity gains as they encourage dissatisfied civil servants to leave for the private sector (Fitzpatrick and Lovenheim, 2014). The effect of deferred compensation on agency-level productivity is thus an open empirical question that merits investigation.

3.1 Conceptual Framework

Observers have long recognized that traditional pensions contain strong retention and retirement incentives. Defined-benefit pensions promise workers an annuity upon retirement – the value of which is set by plan provisions – contingent on a minimum period of employment. The tenure requirement for eligibility is called the "vesting period," and ranges from zero to 10 years in the public sector.[6] Vested workers may collect an annuity once they reach the normal retirement age, which is usually age 65.[7] The annual annuity payment is calculated:

$$A_t = \begin{cases} 0 & \text{if } s_t < v \\ b * w_t * s_t & \text{if } s_t \geq v \end{cases} \tag{3.1}$$

where b is a "benefit multiplier," typically around 2 percent, w_t is the employee's salary in year t, s_t is the total number of years that the employee served with the pension-granting employer as of year t, and v is the vesting period.[8]

[6]The modal state/local vesting period is five years. Center for Retirement Research at Boston College and Center for State and Local Government Excellence (2014).

[7]Depending on the plan, workers may retire earlier if they satisfy age and service requirements. For example, several public plans allow workers to collect full benefits at age 50 if the workers attain 30 years of service.

[8]In practice, public pensions often define w_t as the average of the employee's five largest annual

Define W_t as the present discounted value of retirement wealth were the worker to quit her job in year t. In a defined-benefit pension, W_t equals the sum of a series of annual annuity payments, each of which is adjusted for mortality, receives a cost-of-living adjustment, and is discounted to the reference year t:

$$W(DB)_t = A_t \sum_{x=y}^{\infty} (Cola^{x-y} * Pr(alive)_x)\delta^{-(x-t)} \tag{3.2}$$

Here y is the retirement date, $Pr(alive)_x$ is the probability that the worker is alive in year x, $Cola$ is the annual cost-of-living adjustment, and δ is the discount rate.

Most public-sector employees make annual contributions to pre-fund their pension benefits.[9] Define C_t as the worker's contribution to the pension in year t. Non-vested workers who choose to leave the pension may reclaim their contributions, having earned a statutorily-set, annual interest rate of $1 + r$. Thus, defined-benefit pension wealth is determined by a step function that changes suddenly at the service requirement for vesting:

$$W(DB)_t = \begin{cases} \sum_{x=t-s_t}^{t} C_x(1+r)^{t-x} & \text{if } s_t < v \\ \\ A_t \sum_{x=y}^{\infty} (Cola^{x-y} * Pr(alive)_x)\delta^{-(x-t)} & \text{if } s_t \geq v \end{cases} \tag{3.3}$$

In contrast, defined-contribution plans operate like a savings account. Workers and employers contribute a set percentage of salary each year, which grows over time with the return to a portfolio of investments chosen by the employee. Contributions cannot be removed from the account without incurring a significant tax penalty, so the present value of pension wealth in year t equals:

$$W(DC)_t = \sum_{x=t-s_t}^{t} \left[(C_x + M_x) \prod_{z=t-s_t}^{y} (1 + m_z) \right] \delta^{-(y-t)} \tag{3.4}$$

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salaries – typically those earned in the last five years of employment.

[9] Center for Retirement Research at Boston College and Center for State and Local Government Excellence (2014).

where C_x is the employee's contribution in year x, M_x is the employer's contribution in year x, and m_z is the return on the investment portfolio in that year.

When making career decisions, employees compare the pension wealth they could earn by quitting in year t to the wealth they could earn by continuing to work for the pension-granting employer. Consider two public-sector workers who are identical except that one participates in a traditional defined-benefit pension while the other participates in a defined-contribution account. Assume that both workers would be covered by the same defined-contribution retirement account were they to quit their jobs and move to the private sector. If the defined-benefit member continues in the public sector, she earns total wealth from compensation: $T(DB)_y$, where wealth equals the present discounted value of all current and future income from salary, retirement benefits, and health insurance benefits. Similarly, the defined-contribution member earns total wealth from compensation: $T(DC)_y$.[10] In contrast, if the defined-benefit member transitions to the private sector, she earns total wealth from compensation:

$$T(transition) = T(DB)_t + I[T(DB)_t] + T(private) \tag{3.5}$$

The first term on the right-hand side of this equation is wealth accrued to date (including pension benefits calculated according to Equation 3.3) and the second term is future interest earnings on pension wealth accrued to date. If the defined-benefit member vests and leaves her contributions within the pension system, then $I[T(DB)_t] = 0$. However, workers who withdraw their contributions may earn interest in the future. The third term in the equation represents future wealth from compensation earned in the private sector, including savings in a private-sector defined-contribution account (calculated according to Equation 3.4).

For simplicity, assume that both the defined-benefit and the defined-contribution member quit their jobs if they can earn greater total compensation by doing so than by remaining

[10]The worker may know that she will not stay in the public sector beyond a certain number of years; in this case, potential final wealth is calculated as of the known quit date, rather than the retirement date.

for a full career in the public sector:[11]

$$T(DB)_y \leq T(DB)_t + I[T(DB)_t] + T(private) \tag{3.6}$$

$$T(DC)_y \leq T(DC)_t + T(private) \tag{3.7}$$

To compare the difference in quit probability between the defined-benefit and defined-contribution members, re-arrange Equations 3.6 and 3.7 to yield:

$$T(DB)_y - T(DB)_t - I[T(DB)_t] \leq T(private) \tag{3.8}$$

$$T(DC)_y - T(DC)_t \leq T(private) \tag{3.9}$$

Assume that compensation in the defined-benefit pension is deferred relative to the defined-contribution plan: $T(DB)_y - T(DB)_t - I[T(DB)_t] \geq T(DC)_y - T(DC)_t$. This assumption holds if the defined-benefit pension calculates benefits based on final salary (as in Equation 3.1) or requires a longer vesting period than the defined-contribution plan.[12] In this case, relative quit probabilities depend on the generosity of private-sector compensation. If the private sector earnings do not provide at least as much total compensation as what could be earned by staying in the public sector: $T(private) < T(DC)_y - T(DC)_t$, then no one quits and the difference in separation probability is zero. If private-sector earnings exceed future public-sector earnings for the defined-contribution member but not the defined-benefit member: $W(DC)_y - W(DC)_t \leq W(private) < W(DB)_y - W(DB)_t - I(W(DB)_t)$, then the defined-contribution member quits while the defined-benefit member stays. Lastly, if private-sector earnings exceed future public-sector earnings for the defined-benefit member: $T(private) > T(DB)_y - T(DB)_t - I[T(DB)_t]$, then both workers quit irrespective of pension type. The value of pension wealth decreases as the worker's discount rate increases, so workers with very high discount rates may value neither already-earned nor future pension benefits, and thus not react to pension incentives.

[11] In reality, most workers also consider non-pecuniary benefits when making turnover decisions. However, the analysis is greatly simplified by an indifference assumption along this dimension.

[12] I support this claim with a simulation exercise in the next section.

Older, educated workers may be more likely to consider the pension when making turnover decisions. Among employees with the same salary and tenure at the firm, those hired at older ages accrue more defined-benefit pension wealth because they earn the same nominal annuity payment (Equation 3.1), but must wait fewer years until the normal retirement age to start receiving benefits. Additionally, the distribution of wages in the U.S. public sector is compressed relative to the private sector. Less-skilled employees, in manual and clerical occupations, often earn a wage premium relative to similarly skilled counterparts in the private sector, while highly skilled workers in professional and administrative occupations often suffer a wage penalty (Belman and Heywood, 1989; Borjas, 2002; and Katz and Krueger, 2000).[13] Highly skilled workers in the private sector are nearly twice as likely to participate in a retirement plan than their low-skilled private-sector counterparts, whereas nearly all full-time public-sector employees participate in a defined-benefit pension (Munnell and Quinby, 2009; Munnell, 2012). Among highly skilled workers, defined-benefit members members are less likely than defined-contribution members to receive private-sector compensation that exceeds future wealth from public-sector compensation. Low-skilled workers, in contrast, are likely to receive greater wealth in the public sector regardless of pension type.

Poor understanding of pension provisions could reinforce the differences in sensitivity discussed above. Mitchell (1988) shows that employees are often significantly mis-informed (or not informed) about basic provisions of their employer-provided pension, but that senior, more educated, higher paid, and female workers have a better understanding. In contrast, Starr-McCluer and Sundén (1999), looking six years after Mitchell, find only weak relationships between employee characteristics and knowledge of pension provisions (particularly regarding portability).[14] Nevertheless, the addition of partial information to

[13]Mas (2014) shows that taxpayers are averse to paying high wages to public administrators irrespective of the administrators' productivity.

[14]Starr-McCluer and Sundén note that workers in their sample are better informed, overall, than the workers in Mitchell's earlier sample from the same longitudinal survey. Liebman and Luttmer (2012) demonstrate that older workers are well-informed about the basic provisions of Social Security, but do not survey workers younger than 50.

the model of labor supply developed above only strengthens the prediction of considerable effect heterogeneity.

3.2 Empirical Context

In 1997, the state government of Michigan closed its traditional defined-benefit pension and enrolled new hires in a defined-contribution retirement savings plan. The new defined-contribution plan significantly reduced the financial rewards for long tenure. In particular, members of the old defined-benefit pension needed to remain employed with the state government for at least 10 years in order to receive any retirement benefits. In contrast, defined-contribution participants vest incrementally over a four-year period. Otherwise, the two plans are fairly representative of their types; Table 3.1 details their provisions. All state employees are eligible for employer-provided health insurance after retirement. The state subsidizes retiree health insurance premiums after 10 years of tenure, with defined-benefit members generally receiving a larger subsidy.[15] Defined-benefit and defined-contribution members enjoy similar workplace and pre-retirement compensation policies.[16]

[15]The Michigan Civil Service Commission administers the retiree health insurance plan. Vested defined-benefit members need pay only 40 percent of the cost of their premiums, whereas defined-contribution members pay 70 percent after 10 years of tenure, decreasing linearly to 40 percent after 20 years of tenure.

[16]Employees of the state of Michigan belong to collective bargaining units that negotiate occupation-specific labor contracts. The contracts in effect for 2016 do not appear to differentiate between workers based on pension participation. Contracts are available on the website of the Office of the State Employer.

Table 3.1: Provisions of Defined-Benefit Pension Vs. Defined Contribution Plan (2016)

	Defined-Benefit	**Defined-Contribution**
Benefit Multiplier	1.5%	NA
Final Average Salary	3 highest years	NA
Vesting Period	10 years	• Full vesting after 4 years • Partial vesting after two years
Contribution Rate	• Employer: set by legislature, varies across years depending on pension fund performance • Employee: None	• Employer: 7% • Employee: varies depending on desired DC savings rate. Default is 3%
Normal Retirement	• Age 60 with 10 years tenure • Age 55 with 30 years tenure	NA
COLA	3% annually, non-compounded	NA
Eligibility	Hired before March 31, 1997	Hired after March 31, 1997

Source: Michigan Office of Retirement Services (2015 and 2016).

Figure 3.1 demonstrates state pension incentives in Michigan by simulating the retirement wealth of two hypothetical workers.[17] Although they enter state service in the same year, one participates in the defined-benefit pension and the other in the defined-contribution plan. Each year of employment, the workers debate whether they should leave for the private sector. They calculate the present value (as of the current year) of the total retirement wealth earned to date under their respective plans. The workers vary in their age at hire: the first worker is hired at age 30 and the second at age 40, both with a $20,000 salary.[18] Each year, salaries increase with inflation, an additional year of tenure is credited to the defined-benefit pension, and 10 percent of salary is contributed to the defined-contribution plan (the default employee and employer total contribution rate).[19] The defined-contribution member invests her savings in safe investments yielding a two-percent real return. I assume that inflation is three percent and that workers discount future wealth by a nominal five percent rate.[20] Figure 3.1 displays retirement wealth as a fraction of annual salary, net of employee contributions. Given the assumptions, workers who separate with 9 years of tenure earn approximately one year's salary from their defined contribution account, but nothing from their defined-benefit pension. However, the defined-benefit plan is substantially more generous to long-tenure workers; those who persist with the State of Michigan until age 60 earn twice as much from the defined-benefit pension as from the defined-contribution account.[21]

[17]See Poterba et al. (2007) for a sophisticated simulation of defined-benefit and defined-contribution wealth that accounts for investment risk.

[18]Changing the workers' starting salary does not greatly affect the simulation.

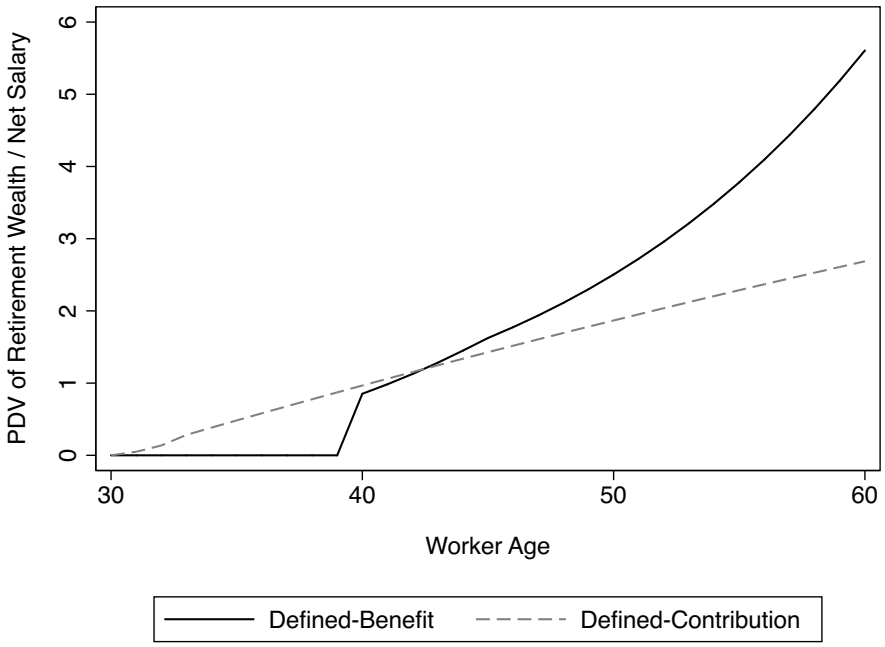
[19]Employees do not contribute to Michigan's defined-benefit pension, but do fund the defined-contribution account.

[20]I calculate the present value of an annuity using the RP-2000 mortality tables – specifically, a 50-50 combination of the male and female healthy annuitant tables.

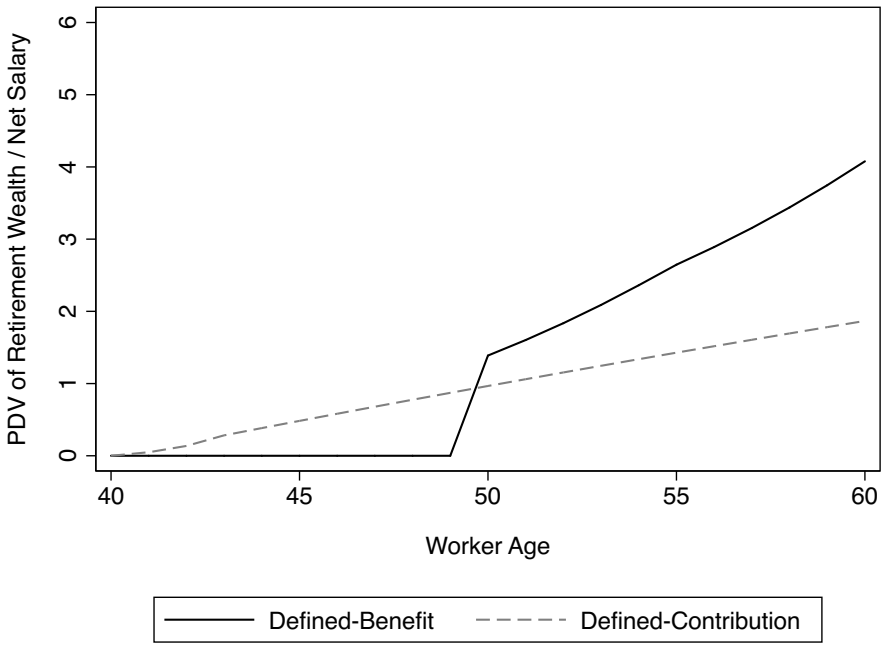
[21]The results of this exercise are similar to Diamond et al. (2010).

Figure 3.1: PDV of Pension Wealth at Time of Separation, by Age at Separation Relative to Salary Net Pension Contributions

(a) Hired at Age 30, $20,000 Starting Salary



(b) Hired at Age 40, $20,000 Starting Salary



Source: author's calculations from provisions cataloged in Michigan Office of Retirement Services (2015).

Notes: calculations assume a three-percent inflation rate, two-percent real return to defined-contribution savings, and five-percent nominal discount rate. Salaries increase annually with inflation and employees contribute three percent of salary to the defined-contribution plan, but nothing to the defined-benefit plan. Mortality probabilities are calculated according to a 50-50 split of the RP-2000 male/female healthy annuitant tables.

3.2.1 Regression Discontinuity Design

In practice, state employees in Michigan only participate in one type of retirement plan, depending on their date of hire. Employees hired before April 1, 1997 default into the defined-benefit pension, while those hired on or after April 1, 1997 receive the defined-contribution plan. Defined-benefit members could switch plan types during a four-month period in 1998, and a few workers hired after April 1, 1997 participate in the traditional pension due to past service with the state. Nevertheless, this statutory threshold for defined-contribution participation enables a fuzzy regression discontinuity design, ascribing differential turnover among workers hired a few months apart to the design of their pension plans.

Specifically, I estimate the equation:

$$\begin{aligned} Y_i =& \alpha + \beta_1 HireDate_i + \beta_2 Threshold_i + \beta_3 (HireDate_i * Threshold_i) \\ &+ \boldsymbol{\beta_4 X_i} + \epsilon_i \end{aligned} \tag{3.10}$$

Y_i is a dichotomous variable equal to one if worker i remains employed by the state of Michigan for at least 10 consecutive years. I focus on a binary measure of persistence to avoid top-coding tenure among workers who remain employed at the end of my sample period. $HireDate_i$ measures the distance, in days, from the employee's date of hire to April 1, 1997. Employees hired prior to April 1, 1997 have negative values of $HireDate_i$, while those hired after have positive values. Coefficients β_1 and β_3 allow the linear relationship between hire date and persistence to change slope after April 1, 1997. β_2 captures any discontinuous change in the quit probability at $HireDate_i = 0$. Hence, β_2 identifies the causal impact of pension design from the behavior of workers hired in close proximity, but who face different financial incentives. To enhance statistical power and remove potential bias from cyclical hiring, the regression controls flexibly for employee age, gender, total annual salary in 2001, job category, and employer (agency) in 2001.[22]

[22]I control for total annual salary with brackets that increase by $5,000. Job categories are determined by the Michigan Civil Service Commission. "Highly skilled" occupations include those classified as

I estimate the regression with Ordinary Least Squares, observing each employee only once.

Equation 3.11 explores how the effect of hire date on persistence varies according to demographic characteristics. Define an indicator variable, D_i, that equals one if worker i falls into a particular demographic category. The interaction of D_i with $Threshold_i$ tests whether the regression discontinuity estimate differs when separately estimated on each worker category:

$$\begin{aligned} Y_i = \alpha &+ \beta_1 HireDate_i + \beta_2 Threshold_i + \beta_3 (HireDate_i * Threshold_i) + \beta_4 D_i \\ &+ \beta_5 (HireDate_i * D_i) + \beta_6 (Threshold_i * D_i) \\ &+ \beta_7 (HireDate_i * Threshold_i * D_i) + \boldsymbol{\beta_8 X_i} + \epsilon_i \end{aligned} \tag{3.11}$$

This empirical design assumes that workers hired within a few months of each other would turnover similarly if they faced the same pension incentives. Since the political and legislative process to revise Michigan's pension statutes lasted for more than a year, well-informed job candidates could have strategically altered hire dates to qualify for their preferred pension type. Workers who anticipated a short tenure may have opted into the defined-contribution system, while those who expected to persist should have preferred the defined-benefit plan. I mitigate bias from strategic hiring with a "donut-hole" discontinuity design: excluding workers hired within a narrow window of the participation threshold. I assume that new hires are willing to speed up or postpone employment by several weeks, but are reluctant to do so by several months. In Section 3.2.2, I present empirical evidence of (limited) sorting to validate the donut-hole approach. Fortunately, dropping observations around the threshold does not alter any of the results.

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administrative, professional, and technical.

3.2.2 Data and Descriptive Statistics

The State of Michigan Office of Retirement Services provided longitudinal personnel records for members of the State Employees Retirement System. The data include all defined-benefit and defined-contribution members employed by state government agencies between 2001 and 2014.[23] Personnel information includes: age, gender, employer name, position title, total salary, date of hire (by employer and position title), date of separation (by employer and position title), and the retirement plan in which the employee participates. All told, the dataset tracks nearly 120,000 employees in 52 State Government agencies. Figure 3.2 reveals that the size of State Government in Michigan has been declining over time, as measured by the number of job positions generating payroll (either part or full-time employment). To reduce noise from part-time and short-duration employment spells, I exclude workers filling multiple positions, earning a total salary below $10,000 in 2001, or employed by the State Legislature (Senate or House of Representatives).[24] The fraction of these employees participating in the defined-contribution plan rose from 25 to 70 percent between 2000 and 2014 (Figure 3.3).

I define separation as ceasing to receive pay for a given position. Workers who separate often return to state service later in time; I consider returns to be independent employment spells. The rate of separation from employment positions averaged around five percent between 2001 and 2014, with noticeable increases in 2002, 2009, and 2010 (Figure 3.4). Late-career separations drove these increases, spurred by early retirement incentive programs.[25] Figure 3.5 shows that workers are more likely to separate within the first two years of employment than at any subsequent point, except after becoming eligible for retirement.[26] The average age of new hires hovers consistently around 35.

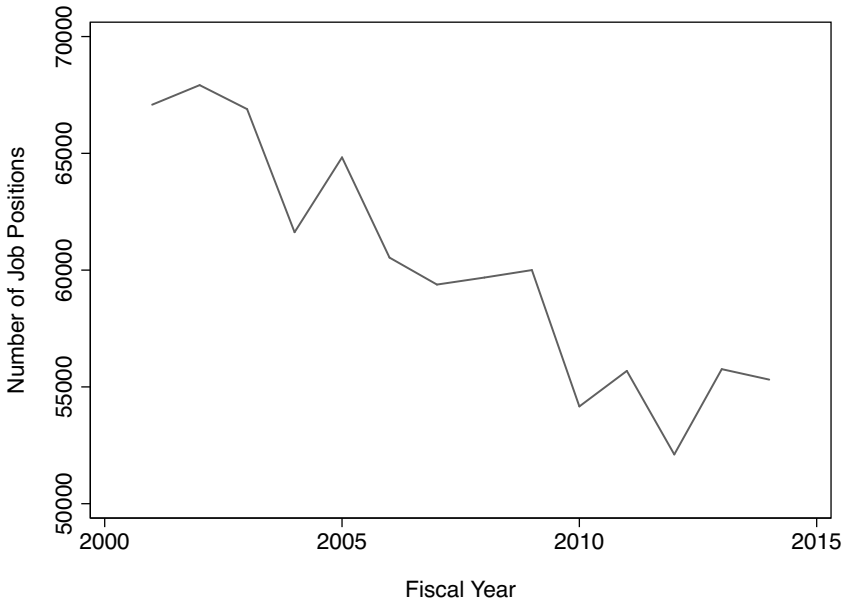
[23]The Office of Retirement Services no longer maintains records prior to 2001.

[24]The analysis sample includes protective services and corrections workers, but excludes university faculty and staff who participate in the Public School Employees' Retirement System.

[25]National Association of State Retirement Administrators (2013).

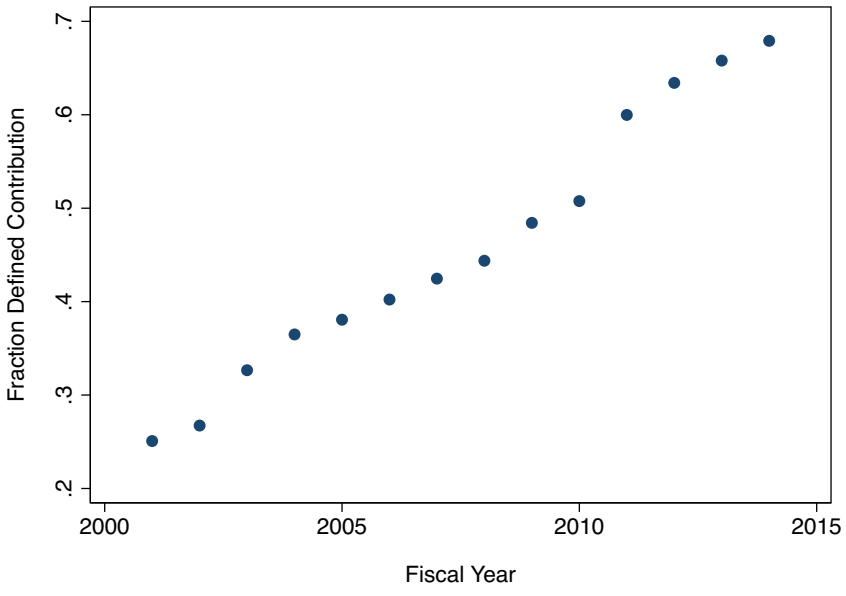
[26]Current tenure is measured from the date of hire to the end of the current fiscal year (September 30). Total tenure is measured from the date of hire to the date of separation. Note that I underestimate the

Figure 3.2: Number of State Employment Positions on Payroll



Source: author's calculations from data provided by the Michigan Office of Retirement Services.

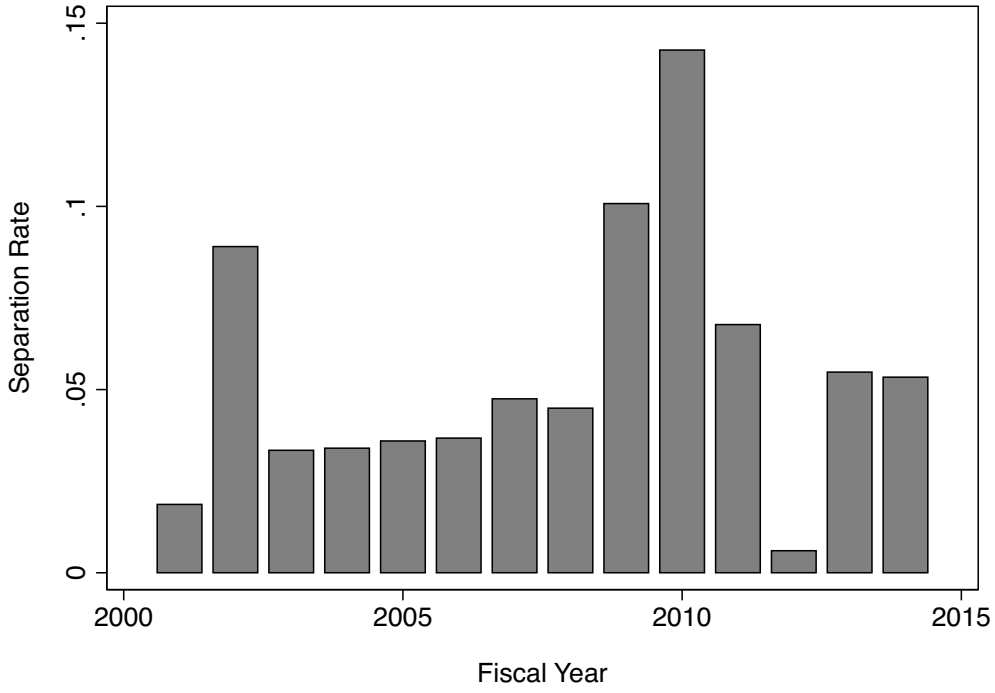
Figure 3.3: Fraction of State Employees Participating in the Defined-Contribution Plan



Source: author's calculations from data provided by the Michigan Office of Retirement Services.

tenure of workers who transitioned in and out of government service before 2001, since I do not observe their previous employment spells. Occasional errors in the hire and separation dates produce negative tenure; between 2001 and 2014, fewer than one percent of person-year observations are awarded negative tenure.

Figure 3.4: Separation Rate from State Government Positions

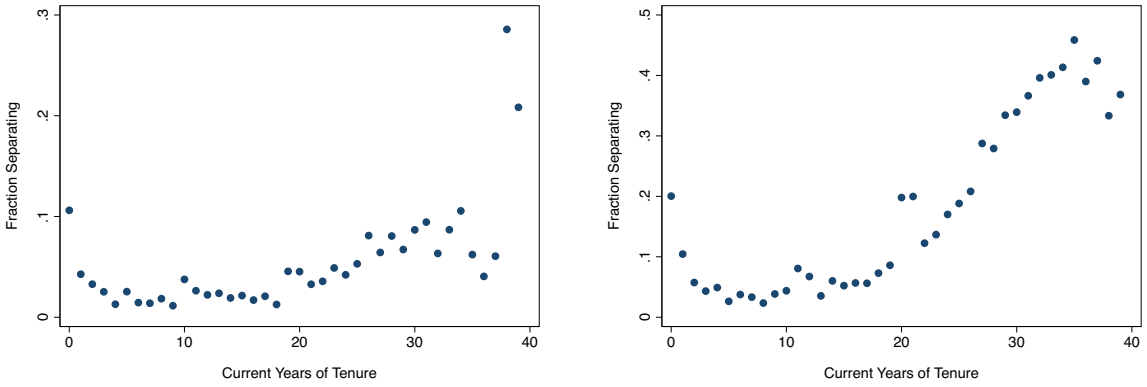


Source: author's calculations from data provided by the Michigan Office of Retirement Services.

Notes: separation is defined as ceasing to receive payroll for a given position. Temporary increases in the separation rate are due to state policies encouraging early retirement.

Figure 3.5: Fraction of Workers Separating, by Current Tenure

(a) Fiscal Year 2001 (b) Fiscal Year 2002



Source: author's calculations from data provided by the Michigan Office of Retirement Services.

Notes: separation is defined as ceasing to receive payroll for a given position. Tenure is estimated by subtracting the employee's date of hire from their date of separation.

The regression discontinuity analysis makes two additional sample restrictions. I focus on workers hired within two years of the threshold for participation in the defined-contribution plan (April 1, 1995 through April 1, 1999) and who were between 30 and 55 years of age in 2001. Workers hired near the threshold for defined-contribution participation have already accrued four years of service credit when I begin observing them in 2001. This sample restriction, necessitated by the data, could bias the estimated effect of defined-benefit membership on persistence toward zero, since defined-contribution members who persist despite incentives to quit may enjoy a higher-quality "match" with their employer, or possess unobserved traits that promote longevity (Jovanovic, 1979; and Topel and Ward, 1992). However, the bias is likely minor because defined-contribution members must also remain employed for four years to vest fully in their benefits.

Table 3.2 displays summary statistics for the main analysis sample, approximately 7,000 workers in total.[27] Defined-benefit and defined-contribution members have similar ages, gender, and salaries. Note that salaries in 2001 appear unusually low. I suspect an error in the personnel records because the median employee received a 180-percent raise between 2001 and 2002. Fortunately, there is a strong correlation between 2001 and 2002 salaries, and substituting 2002 salaries for the faulty 2001 records does not alter any conclusions.[28] As expected, defined-benefit members were hired earlier, on average, and show signs of enhanced persistence, being six percentage points more likely to remain with the state for 10 consecutive years.

[27]The sample size drops precipitously due to the restriction on hire dates.

[28]The correlation coefficient exceeds 0.85.

Table 3.2: Characteristics of Michigan State Government Employees in 2001

Those Hired Between April 1, 1995 and April 1, 1999

VARIABLES	N	Mean	SD	Min	Max
		Defined-Benefit Participants			
Current Age	2,795	40.39	7.466	30	55
Year Hired	2,795	1996	0.976	1995	1999
Persist 10 Years	2,795	0.843	0.364	0	1
Total Current Salary	2,795	23,487	6,620	10,117	100,715
Female	2,795	0.513	0.500	0	1
		Defined-Contribution Participants			
Current Age	4,016	39.67	7.318	30	55
Year Hired	4,016	1998	0.964	1995	1999
Persist 10 Years	4,016	0.781	0.414	0	1
Total Current Salary	4,016	22,386	6,903	10,043	97,857
Female	4,016	0.525	0.499	0	1
		Officials, Professionals, Technicians			
Current Age	2,681	40.44	7.680	30	55
Year Hired	2,681	1997	1.271	1,995	1,999
Persist 10 Years	2,681	0.770	0.421	0	1
Total Current Salary	2,681	26,042	7,925	10,043	100,715
Defined-Contribution Plan	2,681	0.637	0.481	0	1
Female	2,681	0.553	0.497	0	1
		Police, Para-Prof., Support, Craft, Service			
Current Age	4,065	39.61	7.155	30	55
Year Hired	4,065	1997	1.224	1,995	1,999
Persist 10 Years	4,065	0.836	0.370	0	1
Total Current Salary	4,065	20,527	4,228	10,093	51,025
Defined-Contribution Plan	4,065	0.558	0.497	0	1
Female	4,065	0.500	0.500	0	1

Source: author's calculations from data provided by the Michigan Office of Retirement Services.

Since the regression analysis in Section 3.3 will consider the effect of deferred compensation by profession, Table 3.2 also compares worker characteristics across two skill groups. The first group, which I label "high skilled," contains workers whose position titles in 2001 were classified as Officials/Administrators, Professionals, or Technicians by the Michigan Civil Service Commission.[29] Typically requiring at least a college degree, the most common occupations in this group include: Social Services Specialist, Parole Officer, Family Independence Agency Specialist, Information Technology Programmer/ Analyst, Nurse, and Attorney. The second group encompasses workers considered to be Protective Service (police and fire), Para-Professionals, Administrative Support, Skilled Craft, and Service/Maintenance. These occupations require less formal training, and include: Corrections Officer, Word Processing Assistant, Assistant Payments Worker, and Secretary. Although employees in the two groups are similarly aged in Table 3.2, the high-skilled group earned around $6,000 more, on average, with the highest-earner receiving $50,000 more than her/his counterpart in the less-skilled group.

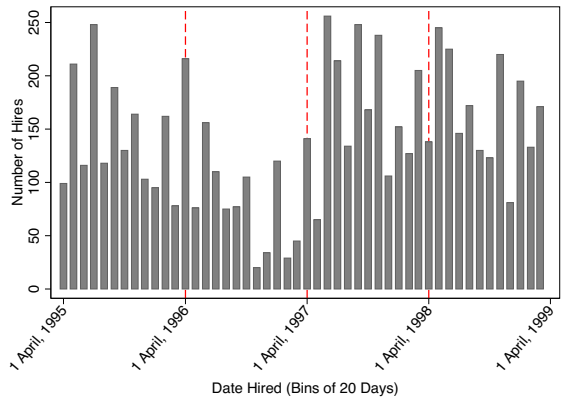
If job candidates strategically alter their hire dates to participate in a preferred pension, then worker characteristics might vary discontinuously around the April 1, 1997 threshold (McCrary, 2008). For example, younger candidates might expect a shorter tenure and wait to apply for state employment until eligible for the defined-contribution plan. Figure 3.6 plots the total number, mean age, mean salary, and average skill group of employees in the main analysis sample against date of hire. Although hiring was clearly restrained in the months leading up to the pension legislation, there is no abnormal bunching right before or after April 1, 1997 (panel a). Similarly, the distribution of employee age is smooth around the threshold (panel b). In contrast, panels (c) and (d) reveal a clear spike in the average salary of those hired right before April 1, 1997 as well as in the probability that these workers are administrators/professionals/technicians. Either highly skilled workers prefer the defined-benefit pension, or the state limited its

[29]I merged position titles in the Retirement Systems data with job classification codes provided on the website of the Michigan Civil Service Commission. Archived job codes only extend to 2002, forcing me to drop 65 workers with missing codes in the analyses that control for position type.

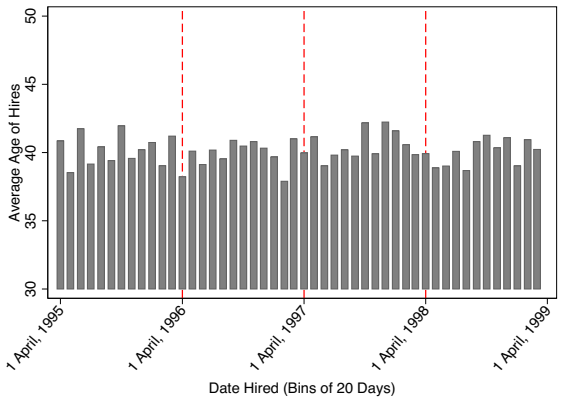
hiring to these positions. Based on the histograms in Figure 3.6, my donut-hole regression discontinuity estimates drop workers who were hired within three months of April 1, 1997.

Figure 3.6: Distribution of Worker Characteristics in 2001, by Hire Date

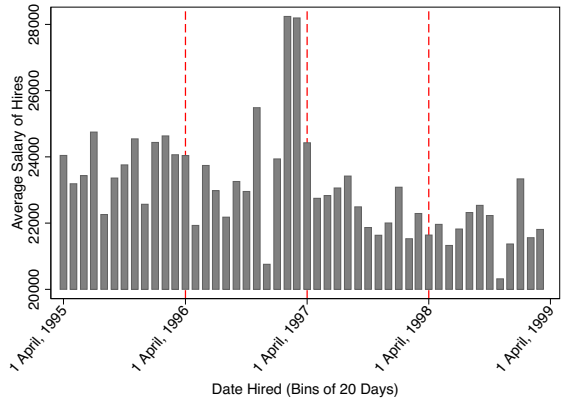
(a) Number of Employees



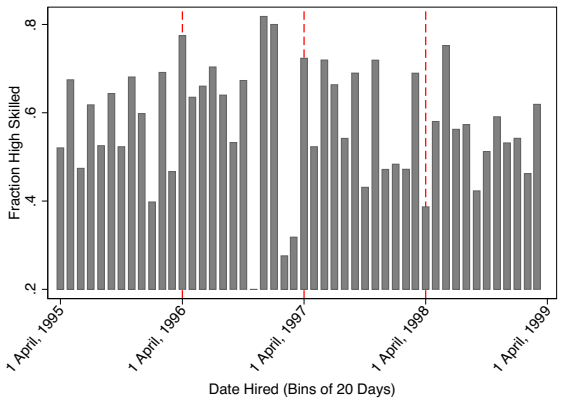
(b) Mean Age of Employees



(c) Mean Salary of Employees



(d) Fraction Officials/Professionals/Technicians



Source: author's calculations from data provided by the Michigan Office of Retirement Services.

Notes: workers ages 30 to 55, earning at least $10,000, only employed in one position, and excluding Legislative staff.

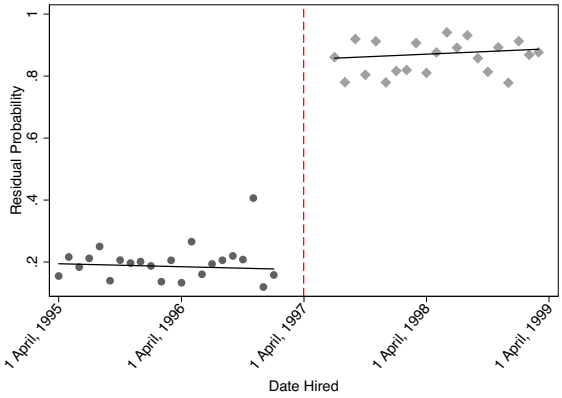
3.3 Results

To confirm that defined-contribution membership depends on hire date, panel (a) of Figure 3.7 plots the residuals from a linear regression that predicts pension type based on employee age, gender, earnings, job category, and agency. Workers hired after April 1, 1997 are 70 percentage points more likely to participate in the defined-contribution plan than the defined-benefit pension. Table 3.3 formalizes this picture by reporting the regression-discontinuity estimate β_2 from Equation 3.10. Table 3.3 presents two sets of estimates for each outcome variable: the first from a donut-hole sample that drops all workers hired between January 1, 1997 and July 1, 1997 (three months on either side of the threshold), and a second from the full sample. Unsurprisingly, column 1 of Table 3.3 reveals that crossing the hire-date threshold increased the probability of defined-contribution membership by a highly statically significant 70 percentage points.

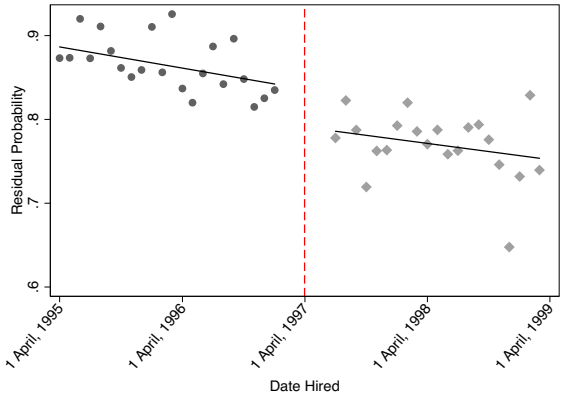
Similarly, panels (b) through (f) Figure 3.7 predict the probability of persisting with the state of Michigan for at least a decade, based on employee characteristics, and plot the residuals of these equations against hire date. On average, persistence appears to drop by about five percentage points when workers cross the threshold for defined-contribution participation: from an 85 percent probability to 80 percent (panel b). The second column of Table 3.3 formalizes this result, while column 3 accounts for defined-benefit members who switch plans by instrumenting for defined-contribution membership with date of hire. The instrumental variables estimate suggests that workers with a portable pension were eight percentage points less likely to remain employed with the state of Michigan for a decade. In Section 3.3.1, I confirm that this result is not sensitive to the choice of hire-date bandwidth or control variables.

Figure 3.7: Residuals Controlling for Worker Characteristics

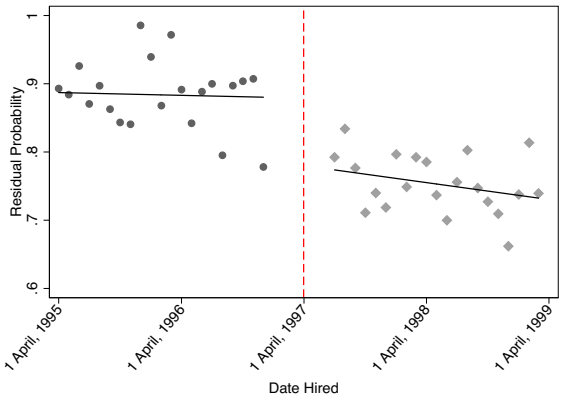
(a) Probability of DC Membership



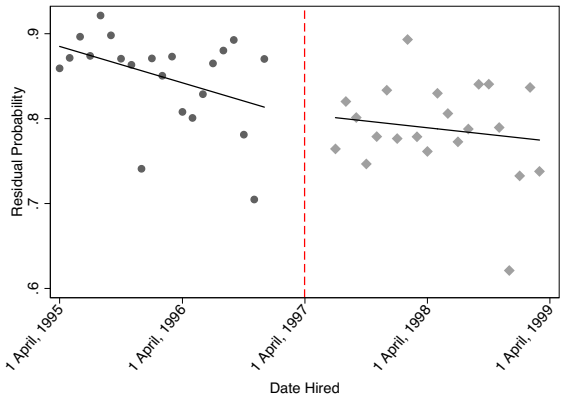
(b) Probability of Persisting 10 Years



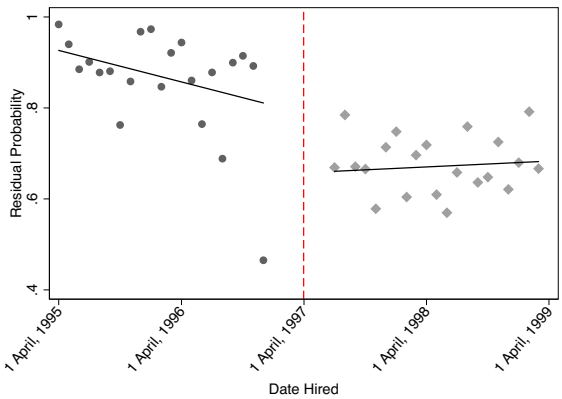
(c) Probability of Persisting 10 Years
Workers 5-20 Years to Retirement



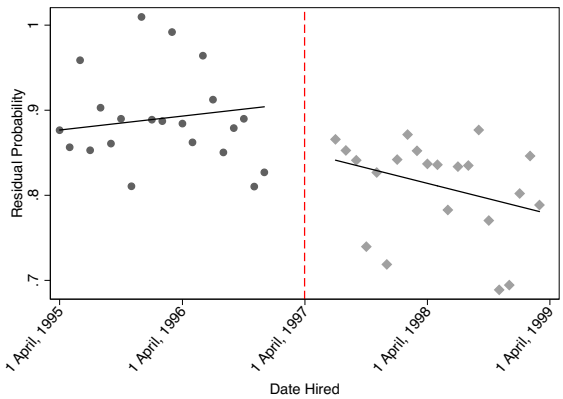
(d) Probability of Persisting 10 Years
Workers 21-30 Years to Retirement



(e) Probability of Persisting 10 Years
Highly Skilled 5-20 Years to Ret.



(f) Probability of Persisting 10 Years
Less Skilled 5-20 Years to Ret.



Source: author's estimates from data provided by the Michigan Office of Retirement Services.

Notes: each point represents the mean outcome among all workers with a given hire date. Sample includes workers ages 30 to 55, earning at least $10,000, only employed in one position, and excluding Legislative staff. Figures depict the residuals from regressions controlling for worker age, gender, salary in 2001, job category (high/low skill), and employer (agency). Each worker is observed only once.

Table 3.3: Effect of Defined-Contribution Membership on 10-Year Persistence

VARIABLES	(1) DC Member (First Stage)	(2) Persist 10 Years (Reduced Form)	(3) Persist 10 Years (IV)
		Donut-Hole Sample	
Hired After 4/1/97 (DC)	0.708***	-0.0579**	-0.0818**
	(0.0256)	(0.0257)	(0.0363)
Observations	6,093	6,093	6,093
R-squared	0.559	0.078	0.068
		Full Sample	
Hired After 4/1/97 (DC)	0.696***	-0.0554***	-0.0795***
	(0.0206)	(0.0211)	(0.0302)
Observations	6,745	6,745	6,745
R-squared	0.542	0.071	0.061
Number of Agencies	29	29	29
Hire Date	X	X	X
Hire Date * Threshold	X	X	X
Employer FE	X	X	X
Age FE	X	X	X
Gender	X	X	X
Salary Bins	X	X	X
Les Skilled Group	X	X	X

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Source: author's estimates from data provided by the Michigan Office of Retirement Services.

Notes: sample includes workers ages 30 to 55, earning at least $10,000, only employed in one position, and excluding Legislative staff. Each worker is observed only once.

To test for differences in persistence by age, occupation, salary, and gender, I estimate four versions of Equation 3.11. The first interacts the $HireDate_i$ and $Threshold_i$ variables with an indicator variable equal to one if the worker was between 30 and 39 years of age in 2001. Column 1 of Table 3.4 reports the results of this specification, which are also displayed graphically in panels (c) and (d) of Figure 3.7. Older workers were nearly 20 percentage points more likely to persist in state service for a decade when covered by the defined-benefit plan, but younger workers did not adjust their labor supply in response to pension incentives. The average worker who leaves with fewer than 10 years of tenure has earned 6 years of tenure regardless of age group. Thus, older workers seem willing to persist an additional four years in state service, on average, to earn benefits that equal approximately twice their salary at separation (Figure 3.1). Younger workers are unwilling to persist an additional four years to earn benefits equal to a year's salary.

Column 2 of Table 3.4 exchanges the "younger" indicator for a binary variable equal to one if the worker's occupation classified as highly skilled in 2001: administrative, professional, or technical. We see weak evidence that highly skilled workers were more sensitive to pension portability than their less-skilled colleagues. Conversely, there is no indication that workers who earned above the median salary of all employees in 2002 reacted differently (column 3 of Table 3.4).[30] High-earners include not only highly educated workers, but also managers in less-skilled occupations. Similarly, women adjusted their labor supply similarly to men (column 4 of Table 3.4).

[30]I limit the sample to workers still employed in 2002 so as not to rely on seemingly faulty 2001 salary data. Workers holding positions in two agencies are counted twice in the median salary calculation.

Table 3.4: Effect of Hire Date on Persistence, by Worker Characteristics

VARIABLES	(1) Stay 10 Yrs	(2) Stay 10 Yrs	(3) Stay 10 Yrs	(4) Stay 10 Yrs
		Donut-Hole Sample		
Hired After 4/1/97	-0.119***	-0.0794*	-0.0683*	-0.0523
	(0.0357)	(0.0445)	(0.0372)	(0.0358)
Younger * After	0.115**			
	(0.0510)			
Less Skilled * After		0.0282		
		(0.0545)		
Above Median Salary * After			0.0149	
			(0.0512)	
Female * After				-0.0118
				(0.0510)
Observations	6,093	6,093	6,006	6,093
R-squared	0.079	0.081	0.076	0.078
		Full Sample		
Hired After 4/1/97	-0.0982***	-0.0890**	-0.0450	-0.0660**
	(0.0302)	(0.0362)	(0.0344)	(0.0280)
Younger * After	0.0805*			
	(0.0420)			
Less Skilled * After		0.0483		
		(0.0445)		
Above Median Salary * After			-0.00444	
			(0.0428)	
Female * After				0.0211
				(0.0420)
Observations	6,745	6,745	6,651	6,745
R-squared	0.072	0.074	0.071	0.071
Number of Agencies	29	29	29	29
Hire Date	X	X	X	X
Hire Date * Threshold	X	X	X	X
Gender	X	X	X	X
Less Skilled Group	X	X	X	X
Above Median Salary 2002			X	

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Source: author's estimates from data provided by the Michigan Office of Retirement Services.

Notes: "younger" refers to workers ages 30 to 39 in 2001. "Less skilled" refers to workers in protective service, para-professional, support, craft, and service/maintenance occupations as categorized by the Michigan Civil Service Commission. Above median salary is calculated relative to all other employees of the state in 2001. The regressions also control flexibly for employee age, salary in 2001, and employer (agency). The sample includes workers ages 30 to 55, earning at least $10,000, only employed in one position, and excluding Legislative staff. Each worker is observed only once.

Older workers clearly drive any occupational differences in the effect of pension incentives on labor supply. For this reason, I reestimate the regression in column 2 of Table 3.4 on the sub-sample of workers ages 40 to 55 in 2001. Table 3.5 presents the first stage, reduced form, and instrumental variables estimates for this specification, while panels (e) and (f) of Figure 3.7 display the reduced form graphically. Among older highly skilled workers, defined-benefit membership caused a 30 percentage point increase in the probability of remaining in state service for a decade or more. Less-skilled older workers were only 10 percentage points more likely to persist. The difference in magnitude between these two effects is not quite statistically significant due to the small number of observations in each age-skill group.

How much longer do older workers remain in state service because of defined-benefit membership? Those who are more than 10 years away from the normal retirement age should not only persist to vest, but also be more likely to persist conditional on vesting. Consider the shape of the curve in Figure 3.1 – the benefit accrual pattern. The additional defined-benefit wealth earned from remaining with the state for another year increases exponentially as the worker gains tenure, even relative to salary: $\frac{\partial \frac{W(DB)}{Salary_t}}{\partial t} > 0$. In contrast, the additional pension wealth gained in the defined-contribution plan due to an additional year of tenure remains constant over the worker's career: $\frac{\partial \frac{W(DC)}{Salary_t}}{\partial t} = 0$. However, many workers may not understand the exponential growth of defined-benefit wealth, and react solely to the vesting period – a plan provision that is clearly explained in pension documentation provided by the Michigan Office of Retirement Services. Similarly, employees who become eligible for normal retirement immediately after vesting are not likely to persist longer in state service. For each year that the retirement-eligible worker remains to earn salary and future benefits, she loses a year's annuity payment.

Table 3.5: Effect of Defined-Contribution Membership on Persistence

Workers 13 Years Away from Retirement on Average

by Occupational Category

VARIABLES	(1) DC Member (First Stage)	(2) Persist 10 Years (Reduced Form)	(3) Persist 10 Years (IV)
		Donut-Hole Sample	
Hired After 4/1/97 (DC)	0.634***	-0.192***	-0.300***
	(0.0646)	(0.0609)	(0.0989)
Less Skilled * After (DC)	0.0578	0.119	0.193*
	(0.0796)	(0.0746)	(0.115)
Observations	2,909	2,909	2,909
R-squared	0.491	0.106	0.056
		Full Sample	
Hired After 4/1/97 (DC)	0.573***	-0.162***	-0.283***
	(0.0540)	(0.0488)	(0.0896)
Less Skilled * After (DC)	0.125*	0.104*	0.198*
	(0.0659)	(0.0620)	(0.104)
Observations	3,209	3,209	3,209
R-squared	0.471	0.095	0.046
Number of Agencies	29	29	29
Hire Date	X	X	X
Hire Date * Threshold	X	X	X
Employer FE	X	X	X
Age FE	X	X	X
Gender	X	X	X
Salary Bins	X	X	X
Less Skilled Group	X	X	X

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Source: author's estimates from data provided by the Michigan Office of Retirement Services.

Notes: "less skilled" refers to workers in protective service, para-professional, support, craft, and service/maintenance occupations as categorized by the Michigan Civil Service Commission. The sample includes workers ages 30 to 55, earning at least $10,000, only employed in one position, and excluding Legislative staff. Each worker is observed only once.

I reestimate Equation 3.11 three times, changing the dependent variable to equal one if the worker leaves state service with fewer than 10 years of tenure, 10 to 11 years of tenure, or more than 12 years. Table 3.6 presents the results adjusted for take-up by instrumenting for defined-contribution participation with date of hire. Older workers are 17 percentage points *less* likely to leave with four to nine years of tenure under the defined-benefit plan, eight percentage points *more* likely to leave with 10 to 11 years, and nine percentage points *more* likely to leave with 12 or more years. These results are only suggestive, since 75 percent of the main analysis sample leaves with 12 or more years of tenure, and only six percent leaves with 10 to 11. Yet, the results are consistent with expectations, since these older workers are 13 years away from the normal retirement age on average. Workers who are induced to persist by the defined-benefit plan most likely remain until eligible to start receiving pension payments.

3.3.1 Robustness Checks

Reassuringly, the regression discontinuity estimates are insensitive to the specification of Equation 3.10. As shown in Figure 3.7, the relationship between persistence and hire date is quite linear on either side of the threshold for defined-contribution membership. To check formally for the effect of bandwidth on the regression discontinuity estimates, Appendix Table C.1 replicates the main result three times, instrumenting for plan type with date of hire. Each iteration limits the sample to workers hired within a growing window of the April 1, 1997 threshold. Despite losing statistical significance with small sample sizes, the effect of defined-contribution membership on persistence is always negative, ranging from a seven percentage-point decrease with a bandwidth of three years to a 16 percentage-point decrease with a bandwidth of one year.

Table 3.6: Effect of Defined-Contribution Membership on Years of Tenure at Separation by Employee Age in 2001, Donut-Hole Specification

VARIABLES	(1) 4-9 Years	(2) 10-11 Years	(3) 12+ Years
Defined-Contribution Member	0.173***	-0.0767*	-0.0965
	(0.0533)	(0.0395)	(0.0611)
Younger * Defined-Contribution Member	-0.167**	0.0738	0.0931
	(0.0716)	(0.0472)	(0.0801)
Observations	6,094	6,094	6,094
R-squared	0.060	0.025	0.079
Number of Agencies	29	29	29
Hire Date	X	X	X
Hire Date * Threshold	X	X	X
Employer FE	X	X	X
Age FE	X	X	X
Gender	X	X	X
Salary Bins	X	X	X
Less Skilled Group	X	X	X

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Source: author's estimates from data provided by the Michigan Office of Retirement Services.

Notes: the regressions instrument for defined-contribution membership with date of hire and drop workers hired during the three months before and three months after April 1, 1997. "Younger" refers to workers ages 30 to 39 in 2001. The sample includes workers ages 30 to 55, earning at least $10,000, only employed in one position, and excluding Legislative staff. Each worker is observed only once.

Similarly, removing or changing the control variables in Equation 3.10 does not affect the story. The first column of Appendix Table C.2 reestimates the main result without any demographic controls, while the second column switches the salary bins to log salary in 2001. The third column replaces potentially faulty 2001 salary data with log salary in 2002, limiting the sample to workers who were still employed in that year. None of these specifications alter the estimated eight percentage-point decrease in persistence from defined-contribution participation.

Lastly, I conduct two placebo tests to confirm that the discontinuity around April 1, 1997 is not simply due to seasonal patterns in hiring and turnover. I reestimate the donut-hole specification in Table 3.3, but change the threshold for defined-contribution participation to April 1, 1995 and April 1, 1999. As before, I limit the sample to workers hired within

a two-year window on either side of the placebo thresholds. As expected, Appendix Table C.3 reveals no effect of the placebo thresholds on the probability of persisting in state service for a decade or more.

3.4 Conclusion

Large unfunded liabilities in state and local defined-benefit pensions have prompted calls for a transition to the fully funded defined-contribution plans prevalent in the private sector. In addition being pre-funded, defined-contribution plans are designed to permit labor mobility across employers. In contrast, defined-benefit plans encourage long tenure at a single firm by granting generous benefits only to senior workers. Employee turnover imposes costs on employers and proffers benefits. In addition to the direct cost of recruiting and training new workers to replace those who separate, the loss of firm-specific human capital may hurt productivity. However, in a setting with strong employment protections, like the civil service, encouraging less-productive employees to leave for the private sector should also increase aggregate productivity. While an examination of productivity is outside the scope of this paper, understanding the effect of seniority pay on turnover is a necessary first step to fully accounting for workforce composition in a cost-benefit analysis of pension reform.

I examine the case of Michigan, which in 1997 closed its defined-benefit pension to newly hired state employees and transitioned to a purely defined-contribution system. Members of the old defined-benefit pension needed to persist in state service for a decade or more in order to receive any benefits, whereas defined-contribution participants can switch to the private sector without any significant pension penalty. Workers who quit state service immediately after accruing 10 years of tenure earned pension wealth equal to at least their parting salary, in present value terms, in either plan.[31] Membership in the different plans depends on the employee's date of hire, allowing me to identify the impact of defined-

[31] Assuming that defined-contribution members invest in safe assets.

benefit membership with a regression discontinuity design. I assume that workers hired within a few months of each other would turnover similarly if they faced the same pension incentives.

I find that participation in the defined-benefit pension increased the probability of persisting in state service for a decade or more by eight percentage points, on average. The effect is concentrated among older workers in occupations requiring a college degree, who were 30 percentage points more likely to persist. Older workers in less-skilled occupations were 10 percentage points more likely to persist, while younger workers did not respond to pension incentives. Workers who altered their behavior to vest in the pension remained in state service until eligible to collect benefit payments.

These findings indicate that the career decisions of older civil servants are influenced by seniority pay, particularly in highly skilled occupations. Nearly half of newly hired workers in the Michigan state government fall into the age range affected by pension incentives. Nearly a quarter of state employees classify as both older and highly skilled. If government employers replace workers who separate with new hires of average age, then defined-benefit pensions create an older workforce with more firm-specific human capital. This effect is magnified if seniority pay also recruits older workers to the civil service more effectively than their younger counterparts.

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Appendix A

Appendix to Chapter 1

A.1 Variable Definitions and Sources

Source: teacher personnel records provided by the Tennessee Department of Education.

- *Total Salary:* annual salary received, including base pay, bonuses, and supplements.
- *Salary Funded with Local Revenue:* portion of annual salary that is paid with revenue from local (district) sources.
- *Salary Funded with State Revenue:* portion of annual salary that is paid with revenue from State appropriations.
- *Years of Licensed Teaching Experience:* number of years of teaching experience, earned in Tennessee or elsewhere, credited on the State teaching license.
- *Highest Degree Bachelor's:* dichotomous variable equal to one if the teacher does not have a Master's degree.
- *Highest Degree Master's:* dichotomous variable equal to one if the teacher has a Master's degree, but does not have 30 hours of additional coursework, an Education Specialist degree, or a Ph.D.
- *Part-Time / Part-Year:* dichotomous variable equal to one if the teacher's total annual salary is less than the State-mandated minimum salary, given education and experience.

Source: *Annual Statistical Reports* published by the Tennessee Department of Education.

- *Average Daily Student Membership:* sum of student enrollment on each school day divided by the number of school days.
- *Full-Time-Equivalent Classroom Teachers:* total number of classroom teachers em-

ployed by the district, where two half-time teachers sum to one full-time-equivalent teacher.

- *Per-Student Operating Expenditure:* total current expenditure divided by average daily membership.

- *Fraction Expenditure State-Funded:* total revenue received from the Basic Education Program divided by total operating expenditure.

- *Student-Teacher Ratio:* average daily membership divided by the number of full-time-equivalent classroom teachers.

Source: *District Report Cards* published by the Tennessee Department of Education.

- *Fraction of Students White:* fraction of all students enrolled on October 1st who are identified as Caucasian by the Tennessee Department of Education.

- *Fraction of Students Eligible for Federal Title I Funds:* fraction of students enrolled on October 1st who qualify for Federal Title I funding.

Source: district-aggregate data provided by the Tennessee Department of Education.

- *Student Math and Reading Test Scores:* the mean or median score, within a district, grade, and subject, on the Tennessee Comprehensive Assessment Program. Students are tested in grades three through eight. Z-scores subtract the state-wide mean score in 2010 (per grade and subject) and divide by the state-wide standard deviation in 2010 (per grade and subject).

Source: Tennessee Comptroller of the Treasury.

- *Residential Property Value:* total value of residential property, appraised over the calendar year.

Source: U.S. Census Bureau Gazetteer Files.

- *District Population Per Square Mile:* number of people living within the boundaries of a school district divided by the total land area in that school district. Population counts are from the 2000 or 2010 Decennial Census.

Source: National Center for Charitable Statistics at the Urban Institute.

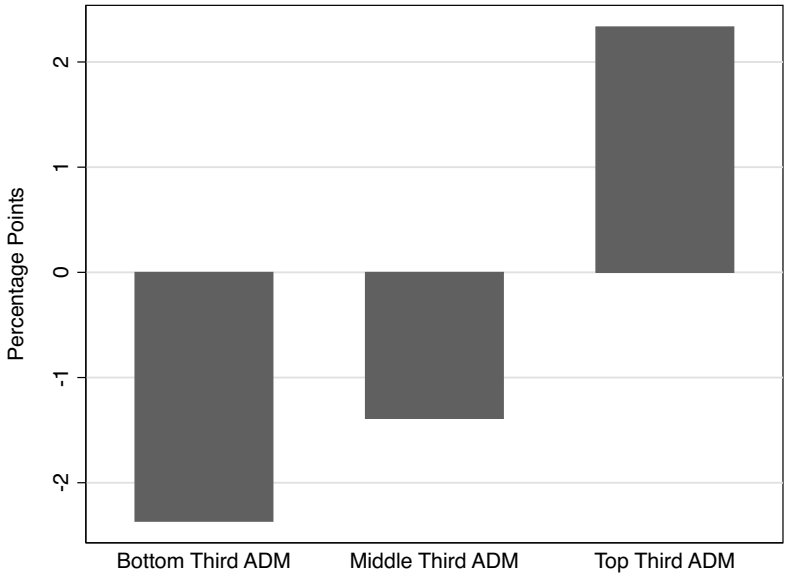
- *Union Revenue:* total revenue reported on Internal Revenue Service Form 990.

Source: miscellaneous.

- *District Serves Entire County:* dichotomous variable equal to one if all children living within the county are eligible to enroll. This variable equals zero if the district only serves a municipality or special service area, and is inferred from school-district names.

A.2 Detailed Summary Statistics

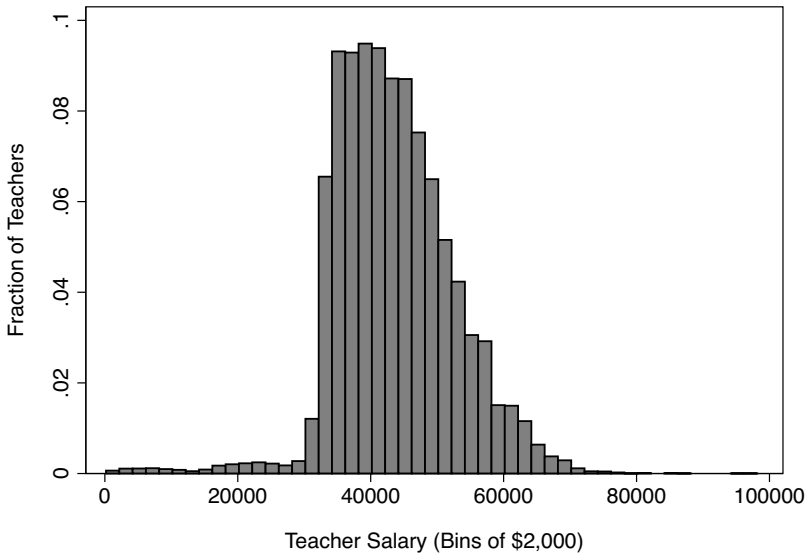
Figure A.1: Percent Growth in Average Daily Student Membership (2005-15)



Source: author's calculations from data provided by the Tennessee Department of Education.

Note: sample excludes districts with "priority" schools, Shelby County Schools, and Carroll County Schools. "ADM" denotes average daily student membership. Growth is calculated from the 2005-06 to the 2015-16 school year.

Figure A.2: Distribution of Salaries Paid to Classroom Teachers, 2009-10 School Year

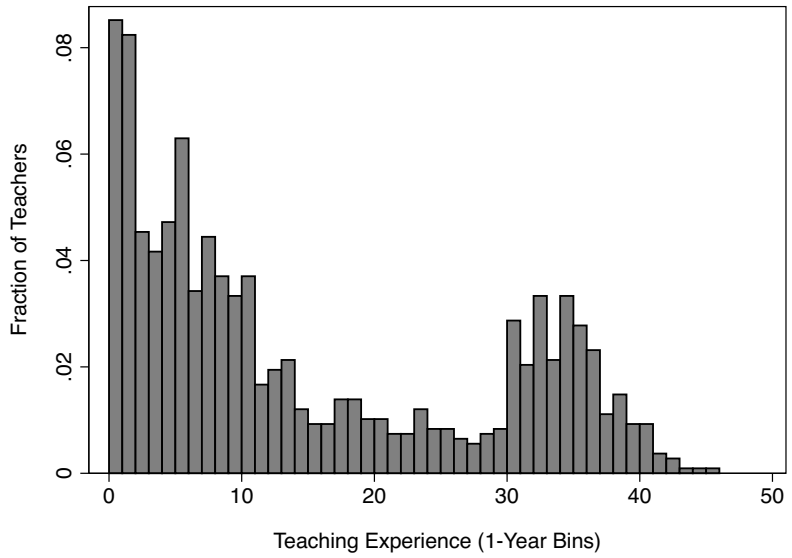


Source: author's calculations from data provided by the Tennessee Department of Education.

Note: sample excludes districts with "priority" schools, Shelby County Schools, and Carroll County Schools.

Figure A.3: Distribution of Licensed Experience Among Part-Time Classroom Teachers

2009-10 School Year

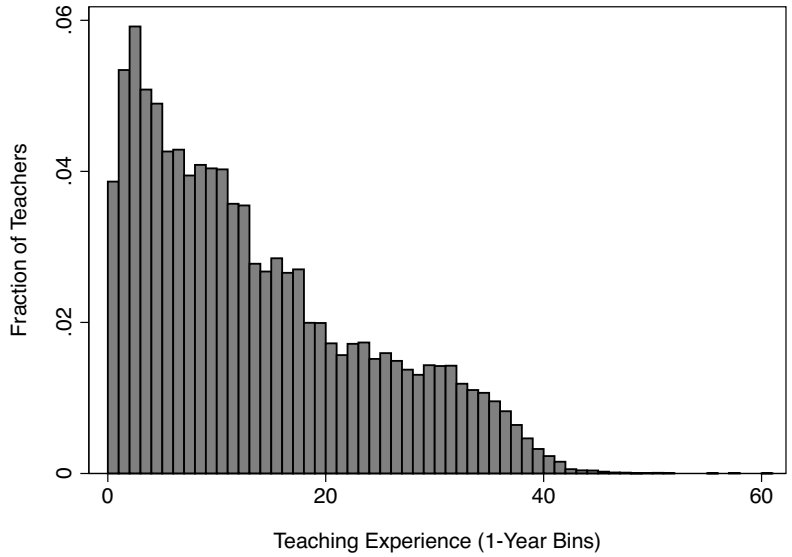


Source: author's calculations from data provided by the Tennessee Department of Education.

Note: sample excludes districts with "priority" schools, Shelby County Schools, and Carroll County Schools. Part-time teachers earn less than the State Minimum Salary, given education and experience.

Figure A.4: Distribution of Licensed Experience Among Full-Time Classroom Teachers

2009-10 School Year

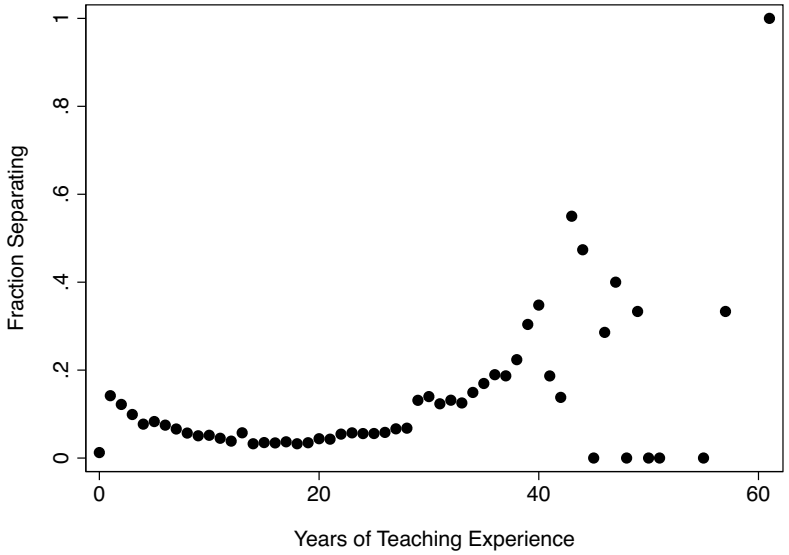


Source: author's calculations from data provided by the Tennessee Department of Education.

Note: sample excludes districts with "priority" schools, Shelby County Schools, and Carroll County Schools. Full-time teachers earn at least the State Minimum Salary, given education and experience.

Figure A.5: Fraction of Classroom Teachers Separating at the End of the Year

2009-10 School Year

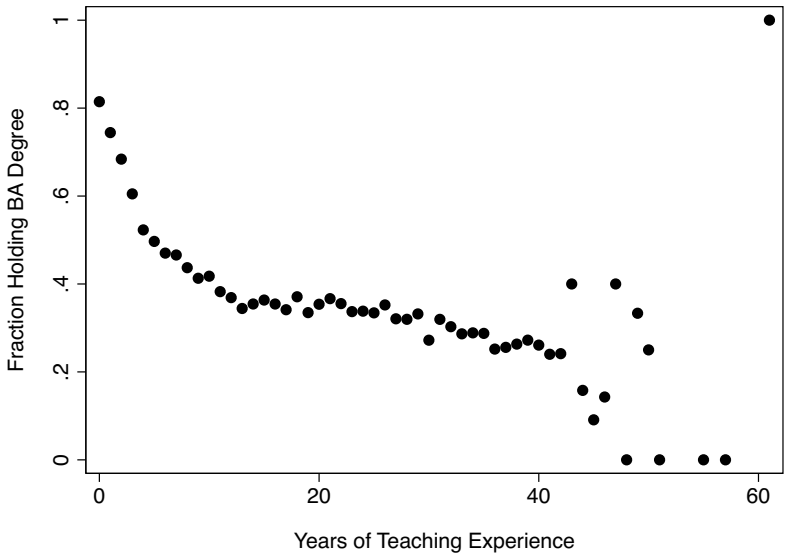


Source: author's calculations from data provided by the Tennessee Department of Education.

Note: sample excludes districts with "priority" schools, Shelby County Schools, and Carroll County Schools. Teachers leave a district by transferring or exiting the dataset.

Figure A.6: Fraction of Classroom Teachers Whose Highest Degree Is a Bachelor's

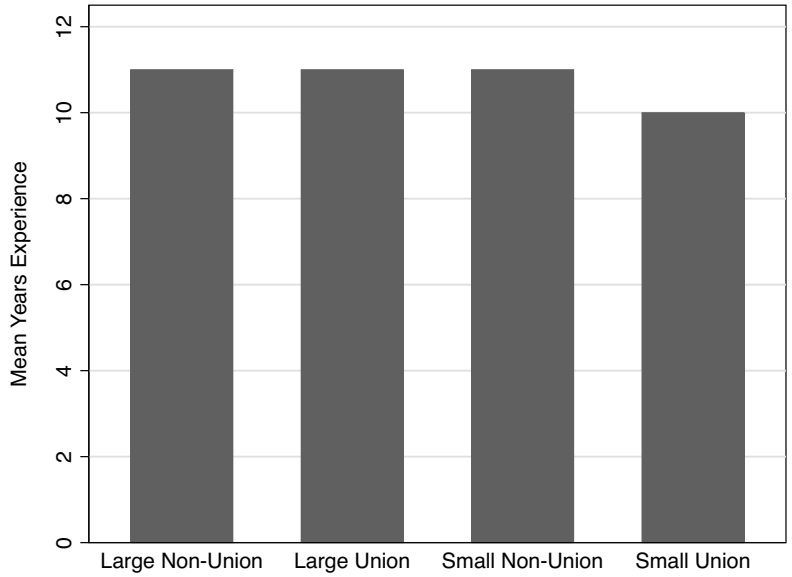
2009-10 School Year



Source: author's calculations from data provided by the Tennessee Department of Education.

Note: sample excludes districts with "priority" schools, Shelby County Schools, and Carroll County Schools.

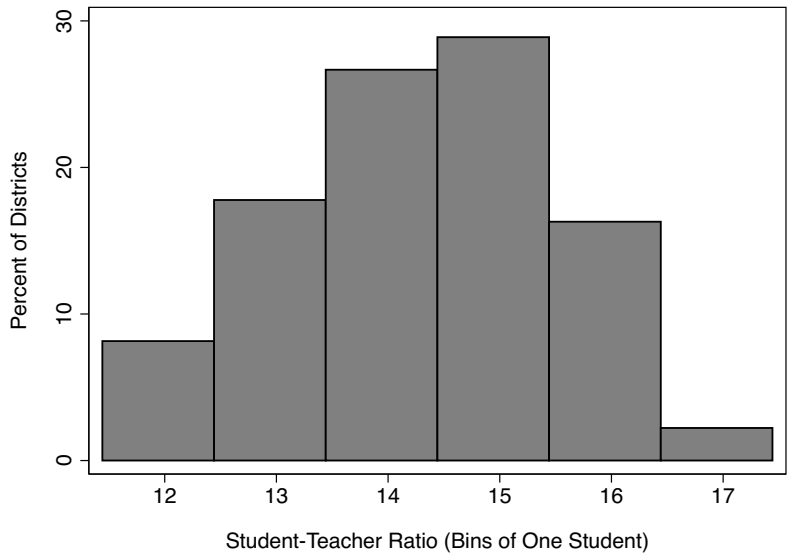
Figure A.7: Mean Years of Licensed Teaching Experience, 2009-10 School Year by School District Size and Bargaining Status



Source: author's calculations from data provided by the Tennessee Department of Education.

Note: sample excludes districts with "priority" schools, Shelby County Schools, and Carroll County Schools. "Large" districts had an average daily student membership of at least 4,000 in 2000. "Union" districts collectively bargained in 2010.

Figure A.8: Distribution of Student-Teacher Ratios, 2009-10 School Year



Source: author's calculations from data provided by the Tennessee Department of Education.

Note: sample excludes districts with "priority" schools, Shelby County Schools, and Carroll County Schools.

Table A.1: Characteristics of School Districts in Tennessee, 2009-10 School Year

District-Weighted

VARIABLES	N	mean	sd	min	max
Main Analysis Sample: Bargaining Districts					
Average Daily Student Membership (ADM)	87	6,715	9,707	329	73,447
District Serves Entire County	87	0.851	0.359	0	1
Fraction Students White	87	0.875	0.146	0.232	0.995
Frac. Students Eligible for Fed. Title I Funds	87	0.662	0.279	0.005	1
Per-Student Operating Expenditure	87	7,796	660.7	6,733	10,154
Fraction Expenditure State Funded	87	0.571	0.0883	0.266	0.752
Per-ADM Residential Property Value*	87	272,298	123,457	125,145	747,365
District Population per Square Mile*	87	220.6	358.7	18.16	1,424
ADM / Full-Time-Equivalent Teachers	87	14.47	1.230	11.48	17.08
Full-Time-Equivalent Teachers	87	453.6	654.5	26.85	5,064
Average Employer Health Premium	87	6,501	1,192	3,934	12,063
Main Analysis Sample: Non-Bargaining Districts					
Average Daily Student Membership (ADM)	42	2,321	1,597	312	6,439
District Serves Entire County	42	0.333	0.477	0	1
Fraction Students White	42	0.822	0.142	0.369	0.990
Frac. Students Eligible for Fed. Title I Funds	42	0.761	0.279	0.174	1
Per-Student Operating Expenditure	42	8,321	1,085	6,308	11,525
Fraction Expenditure State Funded	42	0.536	0.113	0.291	0.696
Per-ADM Residential Property Value*	42	284,675	178,162	65,791	871,225
District Population per Square Mile*	42	403.5	463.5	20.01	1,691
ADM / Full-Time-Equivalent Teachers	42	13.93	1.160	11.44	16.27
Full-Time-Equivalent Teachers	42	164.1	110.4	26	441.0
Average Employer Health Premium	42	5,908	1,127	3,934	8,077
Districts with 6,500 or Fewer Students 2000: Bargaining Districts					
Average Daily Student Membership (ADM)	62	3,166	1,789	329	7,761
District Serves Entire County	62	0.806	0.398	0	1
Fraction Students White	62	0.885	0.145	0.232	0.995
Frac. Students Eligible for Fed. Title I Funds	62	0.752	0.232	0.091	1
Per-Student Operating Expenditure	62	7,851	636.7	6,733	9,446
Fraction Expenditure State Funded	62	0.599	0.069	0.436	0.752
Per-ADM Residential Property Value*	62	237,172	97,220	125,145	747,365
District Population per Square Mile*	62	206.5	374.8	18.16	1,361
ADM / Full-Time-Equivalent Teachers	62	14.21	1.259	11.48	17.08
Full-Time-Equivalent Teachers	62	220.0	118.9	26.85	518.5
Average Employer Health Premium	62	6,206	994.7	3,934	8,019

Source: author's calculations. See Appendix section A.1 for variable definitions and sources.

Table A.2: Characteristics of School Districts in Tennessee, 2009-10 School Year

Student-Weighted

VARIABLES	N	mean	sd	min	max
Main Analysis Sample: Bargaining Districts					
District Serves Entire County	87	0.952	0.215	0	1
Fraction Students White	87	0.788	0.211	0.232	0.995
Frac. Students Eligible for Fed. Title I Funds	87	0.539	0.307	0.005	1
Per-Student Operating Expenditure	87	7,929	955.9	6,733	10,154
Fraction Expenditure State Funded	87	0.502	0.120	0.266	0.752
Per-ADM Residential Property Value*	87	344,971	156,189	125,145	747,365
District Population per Square Mile*	87	316.4	389.2	18.16	1,424
ADM / Full-Time-Equivalent Teachers	87	14.87	0.938	11.48	17.08
Average Employer Health Premium	87	7,071	1,596	3,934	12,063
Main Analysis Sample: Non-Bargaining Districts					
District Serves Entire County	42	0.370	0.489	0	1
Fraction Students White	42	0.816	0.142	0.369	0.990
Frac. Students Eligible for Fed. Title I Funds	42	0.666	0.280	0.174	1
Per-Student Operating Expenditure	42	8,490	1,248	6,308	11,525
Fraction Expenditure State Funded	42	0.493	0.119	0.291	0.696
Per-ADM Residential Property Value*	42	334,845	196,524	65,791	871,225
District Population per Square Mile*	42	514.7	522.7	20.01	1,691
ADM / Full-Time-Equivalent Teachers	42	14.25	1.172	11.44	16.27
Average Employer Health Premium	42	6,256	1,000	3,934	8,077
Districts with 6,500 or Fewer Students 2000: Bargaining Districts					
District Serves Entire County	62	0.894	0.311	0	1
Fraction Students White	62	0.882	0.138	0.232	0.995
Frac. Students Eligible for Fed. Title I Funds	62	0.740	0.219	0.091	1
Per-Student Operating Expenditure	62	7,719	544.4	6,733	9,446
Fraction Expenditure State Funded	62	0.595	0.063	0.436	0.752
Per-ADM Residential Property Value*	62	255,376	122,676	125,145	747,365
District Population per Square Mile*	62	156.0	306.4	18.16	1,361
ADM / Full-Time-Equivalent Teachers	62	14.48	1.165	11.48	17.08
Average Employer Health Premium	62	6,394	946.8	3,934	8,019

Source: author's calculations. See Appendix section A.1 for variable definitions and sources.

Table A.3: Characteristics of Classroom Teachers in Tennessee, 2009-10 School Year

Teacher-Weighted

VARIABLES	N	mean	sd	min	max
Main Analysis Sample: Bargaining Districts					
Total Annual Salary	40,041	43,373	8,658	176	96,883
Salary Funded with Local Revenue	40,041	5,366	3,952	0	53,108
Years of Licensed Teaching Experience	39,723	13.01	10.36	0	61
Fraction Highest Degree Bachelor's	40,041	0.455	0.498	0	1
Fraction Highest Degree Master's	40,041	0.394	0.489	0	1
Fraction Highest Degree Master's +30 Hours	40,041	0.078	0.269	0	1
Fraction Highest Degree Education Specialist	40,041	0.065	0.246	0	1
Fraction Part-Time / Part-Year	40,041	0.023	0.150	0	1
Fraction Leave District at End of Year	40,041	0.083	0.275	0	1
Main Analysis Sample: Non-Bargaining Districts					
Total Annual Salary	7,025	44,591	9,838	234	96,144
Salary Funded with Local Revenue	7,025	6,158	5,072	0	31,356
Years of Licensed Teaching Experience	6,979	13.96	10.70	0	57
Fraction Highest Degree Bachelor's	7,025	0.432	0.495	0	1
Fraction Highest Degree Master's	7,025	0.422	0.494	0	1
Fraction Highest Degree Master's +30 Hours	7,025	0.060	0.237	0	1
Fraction Highest Degree Education Specialist	7,025	0.079	0.270	0	1
Fraction Part-Time / Part-Year	7,025	0.033	0.177	0	1
Fraction Leave District at End of Year	7,025	0.087	0.281	0	1
Districts With 6,500 or Fewer Students 2000: Bargaining Districts					
Total Annual Salary	13,722	41,242	7,339	176	73,410
Salary Funded with Local Revenue	13,722	3,021	2,509	0	33,518
Years of Licensed Teaching Experience	13,625	13.84	10.70	0	61
Fraction Highest Degree Bachelor's	13,722	0.476	0.499	0	1
Fraction Highest Degree Master's	13,722	0.375	0.484	0	1
Fraction Highest Degree Master's +30 Hours	13,722	0.058	0.234	0	1
Fraction Highest Degree Education Specialist	13,722	0.088	0.283	0	1
Fraction Part-Time / Part-Year	13,722	0.026	0.160	0	1
Fraction Leave District at End of Year	13,722	0.076	0.266	0	1

Source: author's calculations. See Appendix section A.1 for variable definitions and sources.

Table A.4: Aggregate Student Test Scores in Tennessee, 2009-10 School Year by District-Grade-Subject, Identically Weighted

VARIABLES	N	mean	sd	min	max
Main Analysis Sample: Bargaining Districts					
Mean Student Math Score (Z-Score)	520	-0.219	0.266	-1.126	0.603
Mean Student RLA Score (Z-Score)	520	-0.108	0.209	-0.827	0.631
Median Student Math Score (Z-Score)	520	-0.166	0.256	-0.960	0.613
Median Student RLA Score (Z-Score)	520	-0.061	0.214	-0.866	0.647
Main Analysis Sample: Non-Bargaining Districts					
Mean Student Math Score (Z-Score)	245	-0.118	0.294	-1.222	0.530
Mean Student RLA Score (Z-Score)	245	-0.041	0.239	-0.791	0.596
Median Student Math Score (Z-Score)	245	-0.067	0.288	-1.128	0.583
Median Student RLA Score (Z-Score)	245	0.006	0.244	-0.693	0.647
Districts with 6,500 or Fewer Students 2000: Bargaining Districts					
Mean Student Math Score (Z-Score)	370	-0.259	0.264	-1.126	0.429
Mean Student RLA Score (Z-Score)	370	-0.142	0.200	-0.827	0.545
Median Student Math Score (Z-Score)	370	-0.200	0.255	-0.960	0.489
Median Student RLA Score (Z-Score)	370	-0.097	0.208	-0.866	0.613

Source: author's calculations. See Appendix section A.1 for variable definitions and sources.

Table A.5: Aggregate Student Test Scores in Tennessee, 2009-10 School Year by District-Grade-Subject, Student-Weighted

VARIABLES	N	mean	sd	min	max
Main Analysis Sample: Bargaining Districts					
Mean Student Math Score (Z-Score)	520	-0.177	0.269	-1.126	0.603
Mean Student RLA Score (Z-Score)	520	-0.064	0.241	-0.827	0.631
Median Student Math Score (Z-Score)	520	-0.132	0.259	-0.960	0.613
Median Student RLA Score (Z-Score)	520	-0.017	0.240	-0.866	0.647
Main Analysis Sample: Non-Bargaining Districts					
Mean Student Math Score (Z-Score)	245	-0.069	0.286	-1.222	0.530
Mean Student RLA Score (Z-Score)	245	0.009	0.243	-0.791	0.596
Median Student Math Score (Z-Score)	245	-0.018	0.274	-1.128	0.583
Median Student RLA Score (Z-Score)	245	0.057	0.244	-0.693	0.647
Districts with 6,500 or Fewer Students 2000: Bargaining Districts					
Mean Student Math Score (Z-Score)	370	-0.259	0.231	-1.126	0.429
Mean Student RLA Score (Z-Score)	370	-0.144	0.166	-0.827	0.545
Median Student Math Score (Z-Score)	370	-0.202	0.225	-0.960	0.489
Median Student RLA Score (Z-Score)	370	-0.099	0.175	-0.866	0.613

Source: author's calculations. See Appendix section A.1 for variable definitions and sources.

A.3 Robustness Checks

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Table A.6: Effect of De-Unionization on Nominal Teacher Salaries

Alternate Controls for District Size and Density

VARIABLES	(1) Log Salary	(2) Log Salary	(3) Log Salary	(4) Log Salary
Union * Year 2009	-0.0066**	-0.0021	-0.0029	-0.0050*
	(0.0028)	(0.0025)	(0.0028)	(0.0027)
Union * Year 2011	-0.0143*	-0.0111	-0.0102	-0.0276
	(0.0080)	(0.0093)	(0.0075)	(0.0222)
Union * Year 2012	-0.0086*	-0.0013	-0.0028	-0.0060*
	(0.0044)	(0.0041)	(0.0047)	(0.0036)
Union * Year 2013	-0.0107	-0.0068	-0.0078	-0.0075
	(0.0068)	(0.0064)	(0.0067)	(0.0066)
Union * Year 2014	-0.0198***	-0.0102	-0.0112	-0.0182**
	(0.0075)	(0.0077)	(0.0076)	(0.0078)
Small District * Union * 2009	0.0109***	0.0083**	0.0090**	0.0102***
	(0.0042)	(0.0040)	(0.0041)	(0.0038)
Small District * Union * 2011	0.0154*	0.0107*	0.0098**	0.0217
	(0.0081)	(0.0064)	(0.0048)	(0.0156)
Small District * Union * 2012	0.0100*	0.0058	0.0071	0.0091*
	(0.0051)	(0.0049)	(0.0050)	(0.0046)
Small District * Union * 2013	0.0122	0.0106	0.0117	0.0110
	(0.0078)	(0.0076)	(0.0078)	(0.0076)
Small District * Union * 2014	0.0196**	0.0135	0.0151*	0.0189**
	(0.0088)	(0.0088)	(0.0087)	(0.0089)
Observations	294,151	294,151	294,151	294,151
R-squared	0.793	0.793	0.793	0.794
Number of Districts	129	129	129	129
District FE	X	X	X	X
Experience Dummies	X	X	X	X
Education Dummies	X	X	X	X
Log ADM * Year Dummies	X			
Population Density * Year Dummies		X		X
ADM * Year Dummies				X
Log Population Density * Year Dummies			X	

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Source: author's estimates from data provided by the Tennessee Department of Education and U.S. Census Bureau.

Notes: sample excludes districts with "priority" schools, Shelby County Schools, and Carroll County Schools. Salaries are paid during the school year indicated. "Experience" refers to years of teaching experience on the State teaching license, earned in Tennessee or elsewhere. "Education" denotes highest degree earned. "ADM" controls for average daily student membership in 2000. "Population density" controls for district residential population divided by land area in 2000. Non-classroom teachers and those with more than 30 years of experience are excluded from the analysis. Standard errors are clustered at the district level.

Table A.7: Effect of De-Unionization on Average Scheduled Teacher Salary

VARIABLES	(1) Log Scheduled Salary	(2) Log Scheduled Salary
Union * Year 2006	-0.00632	0.00147
	(0.00457)	(0.00388)
Union * Year 2007	-0.00429	0.00202
	(0.00378)	(0.00340)
Union * Year 2008	-0.00198	0.00086
	(0.00330)	(0.00280)
Union * Year 2009	-0.00176	0.00020
	(0.00196)	(0.00109)
Union * Year 2011	-0.00024	-0.00004
	(0.00190)	(0.00134)
Union * Year 2012	-0.00146	-0.00033
	(0.00217)	(0.00141)
Union * Year 2013	-0.00433	-0.00446
	(0.00321)	(0.00281)
Union * Year 2014	-0.00756	-0.00505
	(0.00460)	(0.00466)
Union * Year 2015	-0.00616	-0.00609
	(0.00556)	(0.00585)
Observations	1,290	1,040
R-squared	0.981	0.978
Number of Districts	129	104
Teacher Weighted	X	X
District FE	X	X
Year Dummies	X	X
ADM * Year Dummies	X	X

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Source: author's estimates from data provided by the Tennessee Department of Education and Tennessee State Board of Education.

Notes: column 1 excludes districts with "priority" schools, Shelby County Schools, and Carroll County Schools. Column 2 also excludes districts with an average daily student membership greater than 6,500 in 2000. Scheduled Salaries are weighted by the fraction of teachers across the entire state with the requisite highest degree earned and licensed experience, then averaged across districts. Observations are weighted by the number of full-time-equivalent classroom teachers in the district that year. "ADM" controls for average daily student membership in 2000. District and year fixed effects explain most of the variation in the data. Standard errors are clustered at the district level.

Table A.8: Effect of De-Unionization on State-Paid Teacher Salary

VARIABLES	(1) Log Salary	(2) Log Salary	(3) Log Salary	(4) Log Salary
Year 2009	0.0001	-0.0053***	-0.0008	-0.0012
	(0.0015)	(0.0019)	(0.0015)	(0.0022)
Year 2011	0.0167***	0.0214***	0.0166***	0.0168***
	(0.0013)	(0.0017)	(0.0013)	(0.0021)
Year 2012	0.0356***	0.0428***	0.0354***	0.0341***
	(0.0019)	(0.0025)	(0.0019)	(0.0026)
Year 2013	-0.0159***	-0.0072	-0.0156***	-0.0145***
	(0.0040)	(0.0048)	(0.0040)	(0.0044)
Year 2014	-0.0133***	-0.0038	-0.0126***	-0.0121**
	(0.0042)	(0.0054)	(0.0041)	(0.0049)
Union * Year 2009	0.0031*	0.0040*	-0.0011	0.0009
	(0.0017)	(0.0021)	(0.0027)	(0.0018)
Union * Year 2011	-0.0012	-0.0021	-0.0018	-0.0014
	(0.0016)	(0.0018)	(0.0015)	(0.0017)
Union * Year 2012	0.0031	0.0023	0.0022	0.0023
	(0.0022)	(0.0024)	(0.0021)	(0.0023)
Union * Year 2013	0.0048	0.0030	0.0063	0.0021
	(0.0045)	(0.0048)	(0.0043)	(0.0046)
Union * Year 2014	0.0011	-0.0017	0.0046	0.0001
	(0.0045)	(0.0050)	(0.0046)	(0.0048)
Observations	294,151	294,151	294,151	126,709
R-squared	0.784	0.938	0.783	0.798
Number of Districts	129	129	129	104
District FE	X		X	X
Experience Dummies	X	X	X	X
Education Dummies	X	X	X	X
ADM * Year Dummies	X	X		X
Teacher FE		X		

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Source: author's estimates from data provided by the Tennessee Department of Education.

Notes: columns 1-3 exclude districts with "priority" schools, Shelby County Schools, and Carroll County Schools. Column 4 also excludes districts with an average daily student membership greater than 6,500 in 2000. The dependent variable is the amount of State funding for individual teacher salaries in the school year indicated. "Experience" refers to years of teaching experience on the State teaching license, earned in Tennessee or elsewhere. "Education" denotes highest degree earned. "ADM" controls for average daily student membership in 2000. Non-classroom teachers and those with more than 30 years of experience are excluded from the analysis. Standard errors are clustered at the district level.

Table A.9: Effect of De-Unionization on Student Enrollment and FTE Teachers

VARIABLES	(1) Log ADM	(2) Log ADM	(3) Log Teachers	(4) Log Teachers	(5) Log Teachers
Union * Year 2006	0.008	0.011	-0.010	-0.004	-0.005
	(0.013)	(0.015)	(0.015)	(0.014)	(0.014)
Union * Year 2007	0.008	0.009	0.001	-0.006	-0.003
	(0.011)	(0.013)	(0.013)	(0.013)	(0.012)
Union * Year 2008	0.009	0.009	0.003	-0.002	-0.001
	(0.009)	(0.010)	(0.009)	(0.010)	(0.009)
Union * Year 2009	0.010	0.009	0.011	0.005	0.004
	(0.006)	(0.007)	(0.007)	(0.009)	(0.007)
Union * Year 2011	-0.008	-0.006	0.007	0.007	-0.005
	(0.008)	(0.009)	(0.007)	(0.008)	(0.008)
Union * Year 2012	-0.007	-0.006	0.002	-0.008	-0.009
	(0.012)	(0.014)	(0.012)	(0.012)	(0.013)
Union * Year 2013	-0.008	-0.006	-0.001	-0.019	-0.024
	(0.013)	(0.015)	(0.015)	(0.014)	(0.015)
Union * Year 2014	-0.017	-0.019	0.008	-0.014	-0.019
	(0.013)	(0.015)	(0.021)	(0.017)	(0.021)
Union * Year 2015	-0.019	-0.020	0.012	-0.008	-0.021
	(0.014)	(0.016)	(0.022)	(0.018)	(0.021)
Observations	1,290	1,040	1,290	1,290	1,040
R-squared	0.998	0.996	0.999	0.998	0.994
Number of Districts	129	104	129	129	104
District FE	X	X	X	X	X
Year Dummies	X	X	X	X	X
ADM * Year Dummies	X	X	X	X	X
ADM Weighted			X		X

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Source: author's estimates from data provided by the Tennessee Department of Education.

Notes: student enrollment is measured with average daily student membership. "FTE" refers to full-time equivalent teachers. Columns 1, 3, and 4 exclude districts with "priority" schools, Shelby County Schools, and Carroll County Schools. Columns 2 and 5 also exclude districts with an average daily student membership greater than 6,500 in 2000. "ADM" controls for average daily student membership in 2000. District and year fixed effects explain most of the variation in the data. Standard errors are clustered at the district level.

Table A.10: Effect of De-Unionization on District-Aggregate Student Test Scores by Subject and Controlling for Enrollment Growth

VARIABLES	(1) Math	(2) Reading	(3) All	(4) Math	(5) Reading	(6) All
Union * Year 2006	-0.014	-0.016	-0.015	-0.006	-0.009	-0.009
	(0.032)	(0.020)	(0.023)	(0.034)	(0.023)	(0.025)
Union * Year 2007	-0.020	-0.006	-0.011	-0.015	-0.002	-0.007
	(0.032)	(0.019)	(0.022)	(0.034)	(0.022)	(0.024)
Union * Year 2008	-0.014	0.012	0.001	-0.009	0.023	0.008
	(0.030)	(0.018)	(0.021)	(0.033)	(0.020)	(0.023)
Union * Year 2009	-0.024	-0.011	-0.018	-0.024	-0.007	-0.016
	(0.023)	(0.016)	(0.017)	(0.026)	(0.018)	(0.019)
Union * Year 2011	0.054**	0.003	0.027*	0.057**	-0.002	0.026
	(0.023)	(0.014)	(0.015)	(0.026)	(0.015)	(0.017)
Union * Year 2012	0.029	-0.016	0.009	0.044	-0.013	0.017
	(0.027)	(0.018)	(0.019)	(0.031)	(0.020)	(0.022)
Union * Year 2013	0.011	-0.017	-0.004	0.023	-0.010	0.006
	(0.029)	(0.019)	(0.022)	(0.033)	(0.022)	(0.025)
Union * Year 2014	0.017	-0.009	0.005	0.019	-0.009	0.006
	(0.032)	(0.024)	(0.025)	(0.038)	(0.026)	(0.029)
Observations	6,885	6,885	13,770	5,535	5,535	11,070
R-squared	0.994	0.997	0.993	0.993	0.996	0.993
Number of Districts	129	129	129	104	104	104
District FE	X	X	X	X	X	X
Year Dummies	X	X	X	X	X	X
Grade Dummies * Year Dummies	X	X	X	X	X	X
ADM * Year Dummies	X	X	X	X	X	X
% Growth Number Test Takers			X			X

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Source: author's estimates from data provided by the Tennessee Department of Education.

Notes: columns 1-3 exclude districts with "priority" schools, Shelby County Schools, and Carroll County Schools. Columns 4-6 also exclude districts with an average daily student membership greater than 6,500 in 2000. Individual student scores are aggregated within each combination of district, grade, and subject. Z-scores subtract the mean score across the entire state, within grade and subject, from the district-aggregate score and divide by the statewide standard deviation within grade and subject. Exams are scored on a different scale beginning in 2009, so that year dummies absorb most of the variation in the data. "ADM" controls for average daily student membership in 2000. "Percent growth" refers to annual growth in the number of test-takers within a district, grade, and subject. Standard errors are clustered at the district level.

Table A.11: Effect of De-Unionization on Per-Pupil Current Operating Expenditure

VARIABLES	(1) Log Per-Pupil Exp.	(2) Log Per-Pupil Exp.	(3) Log Per-Pupil Exp.
Union * Year 2006	-0.013	-0.008	-0.018
	(0.014)	(0.011)	(0.014)
Union * Year 2007	-0.009	-0.007	-0.012
	(0.012)	(0.011)	(0.013)
Union * Year 2008	-0.015	-0.014	-0.017*
	(0.009)	(0.009)	(0.010)
Union * Year 2009	-0.012	-0.007	-0.007
	(0.008)	(0.007)	(0.008)
Union * Year 2011	0.008	0.015*	0.009
	(0.008)	(0.008)	(0.009)
Union * Year 2012	0.005	0.011	0.002
	(0.010)	(0.010)	(0.013)
Union * Year 2013	0.006	0.006	0.008
	(0.011)	(0.010)	(0.011)
Union * Year 2014	0.011	0.011	0.013
	(0.011)	(0.010)	(0.011)
Union * Year 2015	0.008	0.007	0.009
	(0.012)	(0.011)	(0.012)
Observations	1,290	1,290	1,040
R-squared	0.969	0.937	0.946
Number of Districts	129	129	104
ADM Weighted	X		X
District FE	X	X	X
Year Dummies	X	X	X
ADM * Year Dummies	X	X	X

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Source: author's estimates from data provided by the Tennessee Department of Education.

Notes: per-pupil expenditure divides total current operating expenditure by the average daily student membership that year. columns 1 and 2 exclude districts with "priority" schools, Shelby County Schools, and Carroll County Schools. Column 3 also excludes districts with an average daily student membership greater than 6,500 in 2000. "ADM" controls for average daily student membership in 2000. Observations are weighted by the average daily membership that year. Standard errors are clustered at the district level.

Appendix B

Appendix to Chapter 2

B.1 Detailed Summary Statistics

Table B.1: Number of Override Referendums by Calendar Year

Year	Referendums	School Districts
1995	3	3
1996	2	2
1997	24	21
1998	72	58
1999	58	45
2000	81	64
2001	82	58
2002	44	35
2003	50	38
2004	46	35
2005	53	45
2006	75	62
2007	63	57
2008	84	64
2009	49	38
2010	47	38
2011	37	32
2012	35	32
2013	35	38
Total	942	312*

Source: author's calculations from data provided by the Wisconsin Department of Public Instruction.

Note: the total number of districts holding referendums is less than the total number of referendums because some districts asked for override funds in multiple years.

Table B.2: Characteristics of School Districts the Year Referendums Were Held

Weighted by the Number of Referendums

VARIABLES	N	Mean	SD	Min	Max
			Referendum Passed		
Number Pupils	216	2,114	3,708	95	25,247
Total Expenditure	216	2.228e+07	4.171e+07	1.331e+06	3.228e+08
Instructional Expenditure	216	1.191e+07	2.123e+07	780,472	1.572e+08
Total Revenue	216	2.244e+07	4.191e+07	1.297e+06	3.159e+08
Funds Requested / Expen.	216	0.052	0.035	0.001	0.198
Number FTE Teachers	216	145.9	260.9	10.49	2,029
Average Teacher Salary	216	70,156	13,531	49,697	135,194
Average Teacher Fringe	216	30,311	8,672	8,828	75,188
% Growth Expenditure	216	-0.0003	0.041	-0.137	0.122
% Growth Instruc. Expen.	216	-0.018	0.064	-0.268	0.126
% Growth Revenue	216	0.004	0.037	-0.137	0.127
% Growth Pupils	216	-0.006	0.033	-0.135	0.089
% Growth Teacher Salary	216	-0.004	0.056	-0.247	0.288
% Growth Teacher Fringe	216	0.021	0.102	-0.668	0.509
% Growth FTE Teachers	216	-0.006	0.043	-0.180	0.101
			Referendum Failed		
Number Pupils	285	1,899	2,742	65	21,384
Total Expenditure	285	1.933e+07	2.798e+07	1.200e+06	2.235e+08
Instructional Expenditure	285	1.065e+07	1.555e+07	690,221	1.404e+08
Total Revenue	285	1.948e+07	2.828e+07	1.206e+06	2.244e+08
Funds Requested / Expen.	285	0.054	0.039	0.001	0.189
Number FTE Teachers	285	126.6	175.3	10.04	1,444
Average Teacher Salary	285	67,081	9,581	48,892	106,716
Average Teacher Fringe	285	28,040	6,433	11,209	49,790
% Growth Expenditure	285	0.001	0.049	-0.444	0.205
% Growth Instruc. Expen.	285	-0.013	0.062	-0.268	0.143
% Growth Revenue	285	0.004	0.040	-0.407	0.112
% Growth Pupils	285	-0.003	0.028	-0.092	0.074
% Growth Teacher Salary	284	-0.007	0.050	-0.157	0.220
% Growth Teacher Fringe	284	0.025	0.097	-0.485	0.409
% Growth FTE Teachers	284	0.0057	0.048	-0.133	0.433

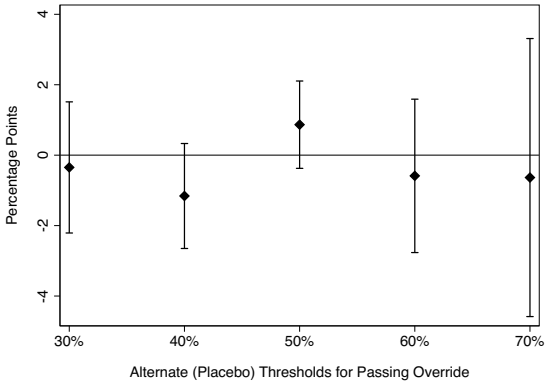
Source: author's calculations from data provided by the Wisconsin Department of Public Instruction.

Notes: the unit of observation is a referendum. Financial variables are transformed to reflect real September 2012 values using the Consumer Price Index. "Fringe" measures the employer cost of providing non-wage compensation including Social Security, Medicare, the Wisconsin Retirement System, health insurance (including retiree), and paid time off. "FTE" refers to full-time equivalent teachers: two teachers working half-time equal one full-time equivalent teacher. A couple of districts are missing the relevant historical personnel records.

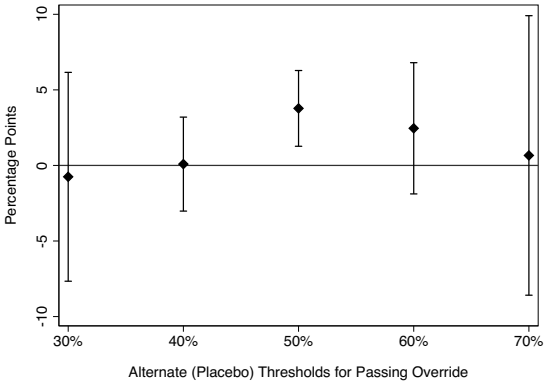
B.2 Robustness Checks

Figure B.1: Effect of Passing an Override on Cumulative Percent Growth Assuming that Overrides Pass With Placebo "Yes" Vote Margins

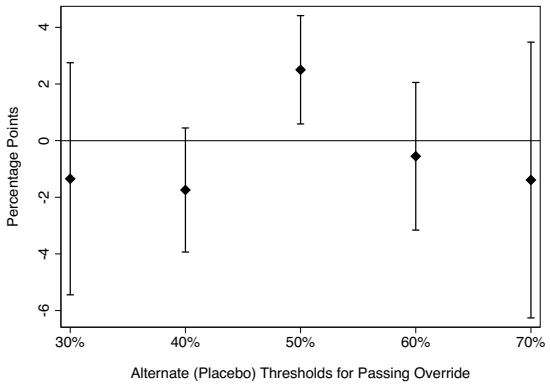
(a) Student Enrollment



(b) Aggregate Teacher Compensation



(c) Number of FTE Teachers



Source: author's estimates from data provided by the Wisconsin Department of Public Instruction.

Notes: growth is measured from the year before school districts first collect override funds to the first year that districts collect funds. "FTE" refers to full-time equivalent teachers. Aggregate compensation equals the district's wage bill and total cost of fringe benefits. Regression discontinuity estimates are based on a 10-point vote-share bandwidth and include year fixed effects, with standard errors clustered at the district level.

Table B.3: Effect of Passing an Override on Administrative Expenditure

	Cum. % Growth Admin. Expenditure	
VARIABLES	(1) First Year	(2) Second Year
	Vote-Share Bandwidth = 10 Points	
Vote-Share Threshold	0.036*	0.016
	(0.020)	(0.026)
Observations	275	275
R-squared	0.195	0.205
	Vote-Share Bandwidth = 20 Points	
Vote-Share Threshold	0.030**	0.034*
	(0.015)	(0.019)
Observations	438	438
R-squared	0.110	0.127
	All Votes in Sample	
Vote-Share Threshold	0.024*	0.023
	(0.013)	(0.017)
Observations	501	501
R-squared	0.116	0.151
% Voted Yes	X	X
% Voted Yes * Passed	X	X
Year FE	X	X

Standard errors in parentheses, clustered at the district level
*** p<0.01, ** p<0.05, * p<0.1

Source: author's estimates from data provided by the Wisconsin Department of Public Instruction.

Notes: Administrative expenditure is transformed to reflect real September 2012 values. Growth rates are measured from the year before school districts first collect override funds to the first and second years after districts collect funds. "Vote-share bandwidth" refers to the number of referendums admitted to the sample based on the fraction of voters supporting override passage. Hence, a 10-point bandwidth admits referendums where 40 to 60 percent of voters favored passage.

Table B.4: Effect of Passing an Override on Cumulative Percent Growth from the Year Before Accessing Funds to the Year of First Access

	(1) Pupils	(2) Aggregate Comp.	(3) FTE Teachers	(4) Aggregate Comp.	(5) FTE Teachers
			Vote-Share Bandwidth = 10 Points		
Vote-Share Threshold	0.005	0.036***	0.022**	0.034***	0.021**
	(0.006)	(0.013)	(0.010)	(0.013)	(0.009)
Observations	275	275	275	275	275
R-squared	0.150	0.184	0.113	0.212	0.185
			Vote-Share Bandwidth = 20 Points		
Vote-Share Threshold	0.006	0.026**	0.014*	0.022**	0.011*
	(0.005)	(0.010)	(0.008)	(0.010)	(0.007)
Observations	438	438	438	438	438
R-squared	0.096	0.132	0.089	0.160	0.181
			All Votes in Sample		
Vote-Share Threshold	0.007	0.020**	0.011*	0.018**	0.008
	(0.004)	(0.008)	(0.006)	(0.008)	(0.006)
Observations	501	501	501	501	501
R-squared	0.082	0.113	0.088	0.134	0.172
% Voted Yes	X	X	X	X	X
% Voted Yes * Passed	X	X	X	X	X
Year FE	X	X	X	X	X
Funds Requested / Expenditure	X	X	X		
Percent Growth Pupils				X	X

Standard errors in parentheses, clustered at the district level
*** p<0.01, ** p<0.05, * p<0.1

Source: author's calculations from data provided by the Wisconsin Department of Public Instruction.

Notes: aggregate compensation equals the district's wage bill and total cost of fringe benefits. "FTE" refers to full-time equivalent teachers. "Funds requested" equals the dollar amount of override funds requested by the district divided by total expenditure the year before the district desired to access the funds. Financial variables are transformed to reflect real September 2012 values. Growth rates are measured from the year before school districts first collect override funds to the first year after districts collect funds. A 10-point vote-share bandwidth admits referendums where 40 to 60 percent of voters favored passage.

Appendix C

Appendix to Chapter 3

C.1 Robustness Checks

Table C.1: Effect of Defined-Contribution Membership on Persistence

Varying the Number of Workers Admitted to the Sample Based on Hire Date (Bandwidth)

Donut-Hole Specification

	(1) One Year 4/1/96 to 4/1/98	(2) Eighteen Months 10/1/95 to 10/1/98	(3) Three Years 4/1/94 to 4/1/00
VARIABLES	Persist 10 Years	Persist 10 Years	Persist 10 Years
Defined-Contribution Member	-0.155	-0.122**	-0.0678***
	(0.104)	(0.0536)	(0.0262)
Observations	2,434	4,206	10,446
R-squared	0.069	0.066	0.079
Number of Agencies	28	28	29
Hire Date	X	X	X
Hire Date * Threshold	X	X	X
Employer FE	X	X	X
Age FE	X	X	X
Gender	X	X	X
Salary Bins	X	X	X
Less Skilled Group	X	X	X

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Source: author's estimates from data provided by the Michigan Office of Retirement Services.

Notes: the regressions instrument for defined-contribution membership with date of hire and drop workers hired during the three months before and three months after April 1, 1997. Column (1) includes workers hired between 4/1/96 and 4/1/98. Column (2) includes workers hired between 10/1/96 and 10/1/98. Column (3) includes workers hired between 4/1/94 and 4/1/00. The sample includes workers ages 30 to 55, earning at least $10,000, only employed in one position, and excluding Legislative staff. Each worker is observed only once.

Table C.2: Effect of Defined-Contribution Membership on Persistence

Alternate Demographic Control Variables

Donut-Hole Specification

VARIABLES	(1) Persist 10 Years	(2) Persist 10 Years	(3) Persist 10 Years
Defined-Contribution Member	-0.0835**	-0.0775**	-0.0804**
	(0.0367)	(0.0366)	(0.0342)
Observations	6,155	6,094	6,007
R-squared	0.013	0.054	0.118
Number of Agencies	29	29	29
Hire Date	X	X	X
Hire Date * Threshold	X	X	X
Employer FE		X	X
Gender		X	X
Less Skilled Group		X	X
Age FE		X	X
Gender FE		X	X
Log Salary in 2001		X	
Log Salary in 2002			X

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Source: author's estimates from data provided by the Michigan Office of Retirement Services.

Notes: the regressions instrument for defined-contribution membership with date of hire and drop workers hired during the three months before and three months after April 1, 1997. The sample includes workers ages 30 to 55, earning at least $10,000, only employed in one position, and excluding Legislative staff. Each worker is observed only once.

Table C.3: Effect of Hire Date on Defined-Contribution Membership and Persistence

Placebo Thresholds

Donut-Hole Specification

	Placebo Policy Year = 1995		Placebo Policy Year = 1999	
	(1)	(2)	(3)	(4)
VARIABLES	DC (First Stage)	Persist 10 Yrs (Reduced Form)	DC (First Stage)	Persist 10 Yrs (Reduced Form)
Hired After 4/1/Year	0.0108	0.0108	-0.0440**	0.0104
	(0.0211)	(0.0194)	(0.0175)	(0.0260)
Observations	5,219	5,219	7,537	7,537
R-squared	0.115	0.051	0.068	0.070
Number of Agencies	29	29	29	29
Hire Date	X	X	X	X
Hire Date * Threshold	X	X	X	X
Employer FE	X	X	X	X
Age FE	X	X	X	X
Gender	X	X	X	X
Salary Bins	X	X	X	X
Less Skilled Group	X	X	X	X

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Source: author's estimates from data provided by the Michigan Office of Retirement Services.

Notes: the regressions drop workers hired during the three months before and three months after each placebo threshold. The sample includes workers ages 30 to 55, earning at least $10,000, only employed in one position, and excluding Legislative staff. Each worker is observed only once.