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Union Reform and Teacher Turnover: Evidence from Wisconsin's Act

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Union Reform and Teacher Turnover: Evidence from Wisconsin's Act 10

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This paper studies teacher attrition in Wisconsin following Act 10, a policy change which severely weakened teachers' unions and capped wage growth for teachers. I document a sharp increase in turnover after the Act was passed, driven almost entirely by the exit of older teachers, who faced strong incentives to retire before the end of pre-existing collective bargaining agreements. I find that student academic performance increased in grades with teachers who retired following the reform, and I obtain similar results when instrumenting for retirement using the pre-existing age distribution of teachers. Differences in value-added between retirees and their replacements can potentially explain some, but not all, of the observed academic improvements.

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I. Introduction

Since 2010, eight states have passed legislation that has weakened teachers' unions by restricting the scope of collective bargaining, establishing right-to-work laws, or both.[1] The question of how such changes affect teacher recruitment and retention is an important component of the broader debate regarding the influence of teachers' unions on education quality. Critics argue that these policies reduce teacher compensation, thereby impairing the ability of school districts to recruit and retain high-quality teachers (e.g. Murnane et al., 1991; Dolton and Marcenaro-Gutierrez, 2011; Wisconsin Budget Project, 2017). Yet others contend that unions allow teachers to earn rents above their outside options (Hoxby, 1996), in which case the labor supply effects of reductions in union power or cuts to compensation may be small. Moreover, even if teachers are responsive to these policy changes, the effects on education quality will depend on whether high-quality or low-quality teachers are more elastic to the changes.

Until recently, however, there had been few sharp policy changes to curb union power or reduce teacher compensation. Thus, little empirical evidence exists on how teacher labor supply responds to such changes, and if so what are the effects of the resulting teacher turnover on student achievement. This paper addresses this gap in the literature by examining the labor supply response of teachers to one of the most extreme reforms in this area, Wisconsin's Act 10, a 2011 law which severely weakened teachers' unions, capped wage growth, and increased mandatory pension contributions for teachers.

I show that teacher attrition did, in fact, increase sharply following the school year during which Act 10 was passed, rising by 4.0 percentage points (58 percent) relative to the year before. However, this increase was driven almost entirely by an increase in teacher retirements: turnover for teachers over the minimum retirement age of 55 increased from 17 to 35 percent relative to the previous year, compared with an increase from 4.7 to 5.4 percent for teachers below age 55. Moreover, a comparison of attrition rates by age in 2010 versus 2011 reveals a sharp divergence precisely at age 55, the minimum retirement age.

Act 10 created strong short-run incentives for eligible teachers to retire prior to the expiration of their district's pre-existing collective bargaining agreement (CBA). Teachers who did so were guaranteed collectively-bargained supplementary retirement benefits, such as retiree heathcare, whereas teachers who waited to retire faced the risk that these benefits, which under Act 10 could no longer be collectively bargained, would be reduced or eliminated by the district. On its own the heterogenous labor supply response by older and younger teachers in 2011 could be explained by these short-run incentives or by differing elasticities to permanent changes, but additional evidence points to the short-run incentives as the primary factor. The statewide retirement rate in Wisconsin spiked in 2011, when collective bargaining agreements were set to expire in the

[1] Idaho, Iowa, and Tennessee passed legislation restricting (or eliminating) collective bargaining; West Virginia and Missouri established right-to-work laws, which prohibit unions from charging non-union members fees to cover the costs of collective-bargaining; and Wisconsin, Indiana, and Michigan did both.

vast majority of districts, and returned towards its pre-reform level the following year. Additionally, the Milwaukee Public School system, which had an usually long pre-existing CBA, experienced a spike in retirements in the final year of its pre-existing CBA, a pattern which was not seen in other districts whose CBAs had expired earlier.

The more permanent components of Act 10 appear to have had a more modest effect on teacher attrition. A simple calculation that takes the one-year percentage change in attrition for teachers under age 55, who were not influenced by the short-term retirement incentives, and divides by the one-year percentage change in average compensation, implies a separation elasticity of around 2. Similarly-constructed estimates using the changes in attrition for older teachers after all pre-existing CBAs had expired also produce estimates around 2. A handful of omitted factors likely bias these calculations upwards. The longer-term labor supply response is thus consistent with Ransom and Sims (2010), who estimate a district-level elasticity of 1.8, and lends some additional credibility to Rothstein (2015)'s simulations that assume occupation-wide separation elasticities are below 2.[2]

I then turn to investigating the effects of the wave of retirements in 2011 on education quality in Wisconsin. One might have expected the increase in retirements to have had a negative impact on education quality. Retirees are typically replaced by less experienced teachers, many of whom are novices, and a large literature suggests that there are returns to experience in teaching, particularly early in the career (e.g. Papay and Kraft, 2015; Wiswall, 2013; Chetty, Friedman and Rockoff, 2014). Additionally, turnover itself may be harmful for students, even holding teacher quality constant (Ronfeldt, Loeb and Wyckoff, 2013; Akhtari, Moreira and Trucco, 2017). On the other hand, it is possible that lower quality teachers are more responsive to incentives to retire (Fitzpatrick and Lovenheim, 2014), in which case the wave of retirements could have been beneficial. The increase in retirements could have benefited schools in other ways, as well. For instance, since older, more experienced teachers are generally paid more, retirements may have freed up resources in school budgets that could be spent more productively elsewhere.

I investigate the effects of the surge of retirements in 2011 on education quality using measures of value-added (VA) in math and reading at the school-by-grade level. I first show that school-grade-levels with a larger fraction of retirees in 2011 improved significantly in math VA in 2012, despite having similar trends to other grades prior to the reform. If the changes in school-grade-level VA were attributable entirely to the difference in value-added between retirees and their replacements, then my estimates would imply that retirees raised their students math test scores by 0.09 standard deviations (σ) less than their replacements. My point estimates also suggest that retirements are associated with improvements in reading VA, although the magnitudes are somewhat smaller (0.05σ versus 0.09σ) and not statistically significant across all specifications.

Of course, the OLS results will only identify the causal effect of retirements on VA if the fraction of retirements in 2011 is uncorrelated with other changes occurring at the school-grade level. The OLS estimates will be biased upwards if teachers retire in anticipation of negative shocks to student

[2]In various simulations, Rothstein (2015) uses elasticities varying from 0.5 to 1.5.

performance (e.g. departure of an effective principal), and will be biased downwards if new policies that improve education quality (e.g. the introduction of a new curriculum) also induce teachers to retire. To address the possible endogeneity of retirements, I employ an instrumental variables strategy in which I instrument for the fraction of retirees in 2011 using the fraction of teachers over the minimum retirement age of 55 in 2011. This approach, which is similar to that used by Fitzpatrick and Lovenheim (2014) to evaluate an Early Retirement Incentive (ERI) program in Illinois, will overcome the endogeneity issues discussed above if school-grade-levels with higher fractions of retirement-eligible teachers were no more likely to experience administrative changes or other shocks that influenced education quality. Using this IV approach, I once again find positive and significant effects of retirements on math VA, with point estimates slightly larger (although not significantly different) than those obtained using OLS. My point estimates in reading are also similar to those obtained using OLS, but the estimates are less precise and therefore not statistically significant.

The similar pattern across the OLS and IV specifications increases our confidence that the results for the impact of retirements on education quality are not confounded by other sudden shocks to student performance. I provide additional evidence in favor of a causal interpretation by showing that the results are robust to controls for principal turnover and changes in district funding, and via a placebo test involving teachers just below the minimum retirement age. Although some concerns remain related to confounding changes in policy at the school level, at minimum the results indicate that the wave of retirements in 2011 was not as detrimental to student achievement as the literature on teacher experience and turnover would suggest – either these retirements directly improved student performance, or schools were able to fully compensate for the retirements via other policies.

Finally, I assess what causal mechanisms might have contributed to the observed increases in education quality following teacher retirements in 2011. Fitzpatrick and Lovenheim (2014) suggest that teachers who respond to financial incentives to retire may have low value-added on average. My results are consistent with differences in teacher value-added between retirees and their replacements having played an important role, but I present evidence that suggests that other mechanisms were at play as well. In particular, I find a strong association between retirements in one grade-level and improvements in performance in other grade-levels in the same school. This association cannot easily be explained by teacher switching, since it persists across grades that have relatively few switchers between them (e.g. first to fourth grade). A possible explanation for this result is that since older teachers are generally paid more, retirements may free up resources that can be used to improve achievement school-wide. A back-of-the-envelope calculation using results from the Project STAR experiment (Krueger, 1999) suggests that if these savings were spent on something as effective as class size reduction, then this channel could account for over half of the observed increases in performance. Teacher peer effects could also have played a role (Jackson and Bruegmann, 2009).

This paper relates to the literature on how teachers' retirement decisions respond to financial incentives. While previous work has generally focused on how retirement decisions respond to discontinuities in the benefit formula embedded in existing pension systems (Costrell and Podgursky, 2009; Costrell and McGee, 2010), or to policy changes that increase the generosity of retiree benefits at various ages (Furgeson, Strauss and Vogt, 2006; Brown and Laschever, 2012; Brown, 2013; Fitzpatrick and Lovenheim, 2014; Fitzpatrick, 2014; Koedel and Xiang, 2017), I examine how retirement decisions respond to the impending loss of supplementary retiree benefits. Fitzpatrick and Lovenheim (2014) also examine the impacts of retirements induced by benefit changes on student achievement, and find similar improvements in the grade-levels previously taught by retirees. However, my paper sheds additional light on the mechanisms by which retirements may affect student achievement. In particular, the presence of cross-grade spillovers suggests that the improvements in student performance cannot be explained by differences in value-added alone, and indicates that other mechanisms, such as the impact of retirements on school budgets or teacher peer effects, may be important.

This paper also contributes to work evaluating the effects of recent legislation to curb the power of teachers' unions.[3] Quinby (2017) examines the end of collective bargaining in Tennessee and finds significant effects on compensation but not on test scores or teacher retention. Three other recent papers examine Act 10 in particular. Litten (2016) studies the effects of Act 10 on teacher compensation, but does not examine teacher turnover or student achievement. Baron (2017) finds decreases in student performance in districts whose prior CBAs expired in 2011 relative to districts with longer pre-existing CBAs, and proposes teacher retirements as a possible mechanism. He documents a fall in average teacher experience in districts with shorter pre-existing contracts, but does not directly measure teacher retirements or examine the incentives that induced them. The most closely related paper is Biasi (2017), who also examines teachers' labor supply responses to Act 10. Her primary focus is on teacher transfers between districts, although she also documents an increase in overall turnover rates and concludes that exiting teachers in 2011 were negatively selected using a quality measure derived from test scores in the school-grade-levels taught by teachers prior to the reform. My paper is distinctive in examining the differential incentives and labor-supply responses of older and younger teachers following Act 10, as well as the first to directly study the effects of retirements induced by the reform on subsequent student performance.

The paper is organized as follows. Section II provides additional context regarding Act 10. Section III describes the data used in my analysis. In Section IV, I examine changes in teacher turnover following Act 10. Section V presents the methodology and results regarding the relationship between the increase in retirements and education quality. Section VI concludes.

[3]A related literature focuses on the rise of public sector collective bargaining, primarily in the 1960s and 70s (Hoxby, 1996; Lovenheim, 2009; Lovenheim and Willen, 2016). Recent papers have also examined how teacher turnover is affected by changes to tenure policies (Strunk, Barrett and Lincove, 2017) and pay-for-performance schemes (Fryer, 2013).

II. Background on Act 10

A. The law's contents and timing

Wisconsin's Act 10 instituted five major changes that affected teachers' unions or teacher compensation directly.[4] First, the Act restricted the scope of the collective bargaining process to negotiation of base wages, thus excluding teacher benefits – including health care and pension contributions by the district – from the collective bargaining process. Second, Act 10 prohibited base wages for teachers to rise faster than the rate of price inflation (CPI-U), unless authorized by a referendum. Third, the Act required that employees make a mandatory contribution to the state-wide pension system, the Wisconsin Retirement System (WRS), which in 2011 amounted to roughly 5.8 percent of salary. Prior to the reform, there nominally had been an "employee" contribution, but this was paid by the district in the vast majority of cases – in 2010, employees in the WRS actually contributed less than 0.7% of the so-called employee contributions. The WRS benefit formula was left unchanged by the Act, however. Fourth, the Act instituted a number of other measures that weakened teachers' unions, including a prohibition on the collection of agency fees,[5] a restriction that collective bargaining agreements be no more than one year in length, and a requirement that unions hold recertification elections every year. Fifth, the biennial budget proposed concurrently with Act 10 reduced state general aid to public schools by 8.4 percent, and cut revenue limits, which cap the total amount of combined revenue that school districts can receive from state general aid and local property taxes, by 5.5 percent.[6]

All of the aforementioned changes, besides for the changes to education funding, came into effect at the expiration of each district's pre-existing collective bargaining agreement (CBA). In the vast majority of districts, the pre-existing CBA expired in the summer of 2011, since the usual practice was to negotiate two-year CBAs in lockstop with the state's biennial budget. Only 16 of the state's 424 districts had pre-existing CBAs with later expiration dates (Litten, 2016).[7] The most notable of these was the Milwaukee Public Schools (MPS), which had a pre-existing contract that expired at the end of June 2013. This contract arose after MPS and the union were unable to come to an agreement in 2009, and then ultimately agreed to a contract in 2010 that lasted through the end of the subsequent biennial budget.

[4]I focus here on the main impacts of the Act as it relates to teachers, although it should be noted that the Act applied to all public sector unions in Wisconsin (excluding police and fire). For more details about the Act, see the 2011 summary by the Wisconsin Legislative Reference Bureau.

[5]Agency fees are mandatory charges to non-union members meant to cover the cost of collective bargaining. Laws banning such fees are often referred to as right-to-work laws.

[6]Federal aid and certain state categorical expenditures, such as aid targeted for students with special needs or for transportation, are not included in the revenue limits.

[7]Litten (2016) acquired the universe of union contracts from an anonymous organization. I was unfortunately unable to obtain access to comprehensive data on districts with extended CBAs and their timing. However, newspaper accounts suggest that all pre-existing contracts expired by the summer of 2013 or earlier (e.g. Richards, 9/12/13).

B. Effects on compensation

Figure 1 plots the time series of average real salary and fringe benefits for teachers in Wisconsin. The value of fringe benefits is estimated by the district for each of its employees as part of annual reports to the Department of Public Instruction (DPI), and incorporates employer contributions to health insurance and the pension system, as well as other benefits such as life and disability insurance. The change in teacher compensation around the introduction of Act 10 is notable. After rising slowly in the pre-period, average real compensation fell by $5813 (7.2 percent) in 2012, the first year after the reform, and continued to fall somewhat afterwards. The drop was driven primarily by a fall in teacher benefits, which dropped by an average of $4944 (18.1 percent) in 2012. This decrease in benefits is 9.2 percent of the pre-reform average salary, so the mandated pension contributions discussed above can explain roughly half of the drop in fringe benefits. Changes in healthcare benefits likely explain most of the remainder of the drop, as many districts either switched over to cheaper health plans or increased the employee share of the premiums. Litten (2016) investigates changes in teacher compensation following Act 10 in more detail, concluding that the Act reduced compensation by roughly 8 percent.

C. Incentives for retirement-eligible teachers

In Section IV, I show that attrition increased markedly for teachers over the minimum retirement age of 55 following the school year during which Act 10 was passed, with much smaller changes for other teachers. The features of the Act that created incentives for teachers to retire therefore deserve particular attention.

Prior to the Act, former teachers received two forms of retirement benefits – pension payments from the centralized Wisconsin Retirement System (WRS), which were determined at the state level, and supplementary retirement benefits provided by the district as part of the local collective bargaining agreement. These supplementary benefits typically included retiree health insurance, and in some cases included other benefits such as supplemental pensions and life insurance.[8] As mentioned in Section II.A, Act 10 required working teachers to contribute a fraction of salary to the pension system, but it did not make any changes to the pension benefits for retirees.

Act 10 did, however, have a profound impact on the supplementary retirement benefits provided by school districts. Under the Act, the district was required to honor these benefits for teachers who retired prior to the end of the pre-existing collective bargaining agreement, but determination of these benefits was left solely at the discretion of the districts once their pre-existing CBA had expired. Thus, teachers who retired before the end of their districts' pre-existing CBA were guaranteed their collectively-bargained retirement benefits, whereas teachers who waited past this point faced the risk of these benefits being reduced or eliminated by the school district. An anonymous survey of district administrators conducted by the Milwaukee Journal Sentinel suggests

[8] I thank David Umhoefer of the Milwaukee Journal Sentinel and Pat Deklotz, Superintendent for the Kettle Moraine School District, for helpful conversations about the range of benefits provided prior to the Act.

that approximately two-thirds of districts reduced or eliminated their post-employment benefits packages in the five years after the Act's passage (Umhoefer and Hauer, 2016). Moreover, substantial uncertainty existed in 2011 as to what districts would do, and the threat of losing benefits may therefore have influenced teachers' retirement decisions even in districts where benefits were not ultimately reduced or where grandfathering clauses were established.

The case of the Milwaukee Public Schools (MPS), Wisconsin's largest school district, provides a useful illustration of the magnitude of the changes to supplementary retirement benefits. Under the pre-existing CBA in Milwaukee, retiring teachers could remain on their district health plan until they became eligible for Medicare at age 65, and the district would pay a subsidy equal to that in place for employees at the time of their retirement.[9] This benefit was available to teachers age 55 and older with at least 15 years of experience at the time of their retirement. MPS made two policy changes that greatly reduced the generosity of its retiree health benefits for teachers retiring after the expiration of its pre-existing CBA. First, the district eliminated retiree health benefits for teachers who were under the age of 60 or had fewer than 20 years of experience at the date of their retirement.[10] Second, the district required active employees to contribute a higher fraction towards healthcare premiums. This led to a fall in the subsidies for retirees, which are based on the district's subsidy at the time of retirement. Table 1 shows the impact that these changes had on the retiree health benefits received by a representative teacher in Milwaukee before and after the expiration of the pre-existing CBA. The table underscores the strong incentives to retire before the expiration of the pre-existing CBA: a representative teacher who was age 55 in the final year of the pre-existing CBA would lose over $18k in annual health benefits for each of the next 10 years if she chose to postpone retirement by one year.

III. Data

A. *Staffing Data*

The Wisconsin Department of Public Instruction (DPI) annually publishes on its website an individual-level dataset containing information on all staff members working in the Wisconsin public school system as of the first week in September. I use these All Staff files for the 1995-6 through 2015-6 school years. The covariates include staff member first and last name, and basic demographic information such as year of birth, gender, race, level of education, and local and total experience in education. The year of birth information allows me to determine whether a teacher is above or below the minimum retirement age of 55.[11] The All Staff files also contain information on each teacher's base salary, an estimate of the value of the total value of the benefits that they receive from the district, as well as a description of the teacher's position in the school and the range of

[9] At age 65, retirees were required to enroll in Medicare, and the district would pay for supplementary insurance benefits.

[10] A two-year grandfathering period was established for teachers age 55 and older with 30 or more years of experience. By my calculations, less than 10% of teachers aged 55-59 had the requisite experience to qualify.

[11] I calculate a teacher's age at the end of a particular school year as the calendar year minus the teacher's year of birth. Since I do not see month of birth, some teachers born in the latter part of the year will be characterized as retirement-eligible when they were only 54 at the end of the relevant school.

grades they serve (e.g. Regular Education - English - Grades 6 to 8). In my primary analysis, I restrict the sample to staff members categorized as regular education teachers.

I use the All Staff files to determine whether each teacher was retained from one year to the next. I say that a teacher was retained between year t and year $t + 1$ if they worked as a regular education teacher anywhere in the Wisconsin public school system in both years; otherwise I say that the teacher attrited.[12] Note that under this definition, a person who works as a teacher in one year and transitions to an administrative role the next will be classified as having exited teaching. This approach concords with the definition of turnover used by the National Center for Education Statistics (Goldring, Taie and Riddles, 2014).

For the 2008-09 school year onwards, the All Staff files contain individual-level teacher identifiers that are constant across years, allowing me to accurately measure whether a teacher was retained for those years. There is no common identifier across years for the files prior to 2008-09, and so I match individuals across years on first name, last name, and year of birth.[13] Appendix Figure A5 shows the imputed and true turnover rates for the years that unique identifiers are available. Turnover rates follow very similar patterns using both methods, although attrition is somewhat higher using the matching method; the primary reason for this appears to be that teachers' last names may change if they get married, which explains why the gap is smaller for retirement-eligible teachers and for men, who are less likely to get married and change their names. In figures that show the time series of turnover rates for a particular group of teachers, I adjust the turnover rates for years before 2009 by the average measurement error for the relevant population between 2009 and 2014.[14] For instance, between 2009 and 2014, the matching procedure produced a turnover rate for teachers under the age of 55 that was on average 1.9 percentage points higher than the true turnover rate. Thus, when I plot aggregate turnover rates for teachers below 55 (Figure 3), I subtract 1.9 percentage points from the turnover rate for teachers under 55 calculated using the matching procedure for years prior to 2009.

I also use the individual-level staffing data to compute turnover rates and other aggregates (e.g. the fraction over age 55) at the school-by-grade level, since my measures of student performance are at the school-by-grade level. When computing aggregates at the school-by-grade level, I weight teachers by the number of full-time equivalent (FTE) units of their assignment (i.e., a full-time teacher gets weight 1, a half-time teacher gets weight 0.5). I focus my analysis of turnover and student performance on elementary school teachers in third through fifth grade, since over 85 percent of such teachers teach only one grade, and it is therefore relatively straightforward to match teachers to the school-grade-level which they teach. In the instances where a teacher teaches multiple grades, I do not see how their time is split between grades, and I therefore assume that

[12] I will use the terms teacher "turnover" and "attrition" interchangeably to mean the opposite of teacher retention.

[13] There are a small number of first name, last name, year of birth combinations that appear multiple times in the same year (e.g. Alison Johnson, born 1953). I drop all observations for such combinations, since I am unable to determine which observations to match across years. Roughly 0.6% of observations are dropped because of this.

[14] In 2015, DPI switched to a new data management system, which standardized names between 2015 and 2016, effectively eliminating measurement error. I therefore do not include the measurement error for 2015 in computing the adjustment.

their FTE units are split evenly across the grades that they teach; a full-time teacher teaching both 3rd and 4th grade will thus be counted as half a teacher in each of those grades.

B. School-Grade-Level Value-Added Data

I evaluate the impact of retirements on education quality using two measures of value-added in reading and mathematics at the school-grade-year level. Both measures of value-added are based on student performance on the Wisconsin Knowledge and Concepts Examination (WKCE), a state-wide examination in math and reading administered to students in the Fall of the 2005-06 through 2013-14 school years.[15] Again, I focus on value-added for elementary school (3rd through 5th grade), since elementary school teachers typically teach only one grade, making it more straightforward to link teachers to the grade-level that they taught.

Constructing value-added from WKCE scores. I construct the primary measure of value-added used in my analysis using publicly available data on the mean WKCE score at the school-grade-year level. Intuitively, my school-grade-level value-added metric is based on the growth in scores for a cohort of students within a school. For instance, the value-added metric for third grade in Hamilton Elementary school in the 2010-11 school year is derived based on how well students at Hamilton scored on the test administered at the beginning of fourth grade in 2011, relative to what we would have expected given the scores for the same cohort of students on the test administered at the beginning of third grade in 2010. Formally, I normalize the school-grade-year mean scores by the mean and standard deviation of the state-wide distribution for students in each grade, so that the units of school-grade-year means are z-scores of the student distribution for each grade. I then predict (normalized) math test scores using the following regression:

$$\bar{Y}^{math}_{s,g+1,t+1} = \beta_0 + \bar{Y}^{math}_{s,g,t}\,\beta_1 + \bar{Y}^{reading}_{s,g,t}\,\beta_2 + X_{s,g,t}\,\beta_3 + \epsilon_{s,g,t}$$

where $\bar{Y}^{math}_{s,g,t}$ and $\bar{Y}^{reading}_{s,g,t}$ are the normalized mean test scores for students in math and reading for grade g in school s in year t. $X_{s,g,t}$ is a vector of covariates describing the demographics of the students in grade g in school s in year t – namely, the fraction of students who are (non-hispanic) black, hispanic, receive free or reduced price lunch, are characterized as English language learners, and are characterized has having disabilities. I attribute the difference between the predicted score and the actual score, i.e. $\epsilon_{s,g,t}$, as the math value-added of grade g in school s in year t. I follow an analogous procedure for reading.

During the time period studied, the WKCE examination was administered in October, close to the beginning of the school year, and so I construct the value-added for the fourth grade, for instance, using the growth in test scores between the beginning of 4th grade and the beginning of 5th grade the following year (and likewise for other grades). This is consistent with the practice used

[15]After conducting the WKCE examination in the Fall from 2005 to 2013, Wisconsin switched to a spring test administration in 2015, effectively going 2 school years between tests. Additionally, they implemented a new test, called the Badger Exam in 2015, before switching to a third test, the Wisconsin Forward Exam, in 2016. It is thus difficult to construct a consistent value-added series past 2013.

by DPI in constructing its internal value-added estimates. Since my value-added metric requires me to match scores for cohorts in adjacent grades, I am unable to construct a value-added measure for a particular school-grade unit if it is the highest grade offered in the school. This is generally not an issue for third and fourth grade, but leads to missing value-added for the majority of fifth grades, since elementary schools often end in fifth grade (see Appendix Table A2). I obtain very similar results when excluding fifth grade classrooms from the analysis.

VARC Value-Added. In addition to the school-grade-level value-added metric constructed above, I also obtained access to a school-grade-level value-added metric constructed by the Wisconsin Value-Added Research Center (VARC) for internal evaluation by DPI. The primary advantage of the VARC value-added measure is that it covers nearly the universe of third through fifth grades (Appendix Table A2). This is because the VARC value-added measure is based on a student-level dataset (which I do not have access to), which allows them to track students across years even if they change schools. The student microdata also allows them to include student-level controls which are not available to me.

Unfortunately, the VARC data also have a few shortcomings. First and foremost, the value-added metrics provided by VARC have had a complex shrinkage procedure applied to them which cannot be reversed given the information available to me. It is problematic to use shrunk measures of value-added as an outcome variable since they will typically change less than one-for-one with changes in true effectiveness, and thus we would expect the results to be attenuated towards zero (see Schwartz, 2015 for a discussion). In addition, the control variables used by VARC vary somewhat across years, and for some years VARC reports a value-added metric in the unit of student test scores, whereas in other years they report only a z-score of the school-grade-level value-added.

As a result, I focus on the school-grade-level value-added discussed above in my main analysis. Nonetheless, in Appendix A.A1 I reproduce my primary analysis using a standardized version of the VARC value-added measures. For the most part, the results follow quite similar patterns to those with my main value-added measure – both when using the full VARC sample, as well as the VARC value-added measure for the subsample for which I am able to construct my main analysis. This gives some confidence that the main results are not driven by the exact specification or sample for the value-added, and I note in the text the few places where the results appear to be sensitive to these choices.

School-Grade-Level versus Individual-Teacher Value-Added. Wisconsin does not have student-teacher linkages for the WKCE examination, and so it is not possible to construct individual teacher-level value-added for the period studied. However, even if individual TVA were available, school-grade-level value-added may be a more appropriate metric for measuring the impacts of teacher retirements on education quality. First, there may be spillover effects across teachers (Jackson and Bruegmann, 2009) or negative impacts of turnover per se (Ronfeldt, Loeb and Wyckoff, 2013), which will be captured by changes in school-grade-level value-added but not by the pure differences in TVA between retirees and their replacements. Along similar lines, as I discuss in Section V.E, it is also possible that retirements affect education quality by freeing up resources, which again will not be captured by differences in individual TVA. Moreover, concerns about bias in individual TVA owing to endogenous sorting of students to teachers (Rothstein, 2009) may be particularly strong following teacher retirements, as principals may assign particular types of students to novice teachers. Grade-level value-added, which examines the growth of an entire cohort of students, alleviates these concerns.

IV. Teacher Attrition Following Act 10

Figure 2 plots the time series of aggregate turnover rates for regular education teachers in Wisconsin's public schools. The figure shows a sharp increase in turnover (from 7.0 to 11.0 percentage points) following the 2010-2011 school year – that is, following the school year during which Act 10 was passed.[16] Aggregate turnover rates come back down after 2011, but remain above their 2010 levels from 2012 through 2015.

The increase in aggregate turnover rates in 2011 was driven almost entirely by teachers of retirement-eligible age. This can be seen in Figure 3, which plots attrition separately for teachers above and below the age of 55, which is the minimum age at which one is eligible to receive pension benefits from the statewide Wisconsin Retirement System. The differences between the two groups are striking: between 2010 and 2011, attrition increases from 17 to 35 percent for teachers of retirement-eligible age, and from 4.7 to 5.4 percent for teachers below the retirement age. The importance of retirement-eligibility for attrition in 2011 is further highlighted in Figure 4, which plots attrition by age following the 2010 and 2011 school years. Attrition rates in 2010 and 2011 appear fairly similar at all ages under 55, and begin to diverge sharply precisely at the minimum retirement age of 55.

On its own, the sharply heterogenous labor-supply response by retirement-eligibility could have been the result of two different channels. First, as discussed in Section II.C, retirement-eligible teachers faced strong incentives to retire before the expiration of their district's pre-existing CBA in order to guarantee that they received collectively-bargained supplementary retirement benefits. Second, it is possible that teachers with the option of taking a generous retirement package are more elastic to permanent changes to compensation and union status than are other teachers.

However, additional evidence suggests that the incentives to retire earlier to receive collectively-bargained retirement benefits were the predominant factor behind the surge in retirements in 2011. First, after rising to 35 percent in 2011, when the vast majority of districts' pre-existing CBAs were set to expire, attrition rates for teachers over 55 fell to 23 percent in 2012. By 2014, when all of the pre-existing collective bargaining agreements had expired, retirement rates had fallen somewhat further, to 21 percent, just 3.5 percentage points above their 2010 levels. One might worry that the fall in retirement rates after the initial spike in 2011 is the result of a selection effect in which only

[16] The turnover rate plotted for the year 2011 in Figure 2 represents the fraction of regular education teachers in the 2010-2011 school year who did not teach the following year, i.e. those who appear to have left the system in the summer of 2011. I will henceforth refer to school years by the calendar year in which they end (e.g. 2010-2011 as 2011).

the most attached older teachers remained in the system. However, since the changes in attrition for teachers under the age of 55 were relatively small, we would not expect much of a selection effect for teachers age 55, i.e. those who are newly eligible to retire. Appendix Figure A6 shows that retirement rates for newly-eligible teachers follow similar patterns to those for all retirees, indicating that the drop in retirement rates in 2012 is likely not explained by selection.

Further evidence on the importance of the short-run incentives comes from a comparison of turnover rates for older teachers in the Milwaukee Public Schools (MPS) versus the rest of Wisconsin. As mentioned in Section II.A, MPS had an unusually long pre-existing CBA that expired in 2013, whereas the vast majority of districts had contracts expiring in 2011. Figure 5 shows that MPS experienced a large spike in retirements in 2013, when its pre-existing CBA was set to expire, a pattern which was not seen in other districts whose CBAs had primarily expired earlier.[17]

Although the predominant factor in the rise in attrition following Act 10 appears to have been the short-run incentives for teachers to retire, the evidence is consistent with the more permanent components of the Act having had a modest effect on teacher attrition. After experiencing declines every year since 2003, attrition for teachers under the age of 55 rose 0.7 percentage points (16 percent) in 2011 and continued to rise over the next 3 years (Figure 3). Additionally, as discussed above, retirement rates remained about 3 percentage points above their pre-reform levels even once all pre-existing CBAs had expired. A naive calculation that takes the percent change in turnover for teachers under the age of 55 between 2011 and 2010 and divides by the percent change in average total real compensation for these teachers in the first year after the reform yields a separation elasticity of 2.2. Similarly, if we take the percent change in turnover for older teachers between 2010 and 2014, once all pre-existing CBAs had expired, and divide by the percent change in compensation for older teachers, we obtain a naive separation elasticity estimate of 1.7. Appendix Table A1 shows these calculations for other years as well.

One ought to be cautious in interpreting these naive elasticities as the causal impact of Act 10, since other concurrent changes in Wisconsin could also have had modest effects on teacher turnover rates. For instance, Wisconsin's economy was generally improving in the period following Act 10's introduction, and the state implemented a new teacher evaluation program in the 2014-2015 school year.[18] Additionally, in the absence of any confounding factors, we would generally expect these naive estimates to exceed the elasticity of separation with respect to a permanent change in compensation, since Act 10 appears to have reduced not just the level of compensation but also its growth rate. All of these omitted factors likely bias the naive elasticity estimates upwards, and so it seems reasonable to view these as an upper bound on the elasticity of separation

[17]Figure 5 shows that Milwaukee also experienced a spike in retirements in 2011, following the school year during which Act 10 was announced. Two factors may have contributed to this spike: first, some forward-looking teachers may have responded to the announcement of Act 10, even if it did not come into effect immediately in their district. Second, as part of the pre-existing CBA in Milwaukee, beginning in the 2011-2012 school year teachers were required to contribute 1-2% of their salary towards their health insurance. Since retiree healthcare benefits depend on the district contribution at the time of retirement, teachers who retired in 2011 received more generous retiree healthcare benefits than those who retired later.

[18]DPI also ran smaller pilots of the evaluation program in 2012-3 and 2013-4, covering roughly 600 and 1200 teachers respectively (out of over 50,000 total teachers in the state).

with respect to permanent changes in compensation at the state level. As a point of comparison, Ransom and Sims (2010) estimate that the elasticity of separation with respect to changes in compensation at the district level is 1.8. The naive estimates are thus of a fairly similar magnitude, suggesting that the observed rise in attrition for younger teachers, and the longer-run rises in attrition among retirement-eligible teachers, might plausibly have resulted from Act 10's impact on teacher compensation.

In sum, the short-run incentives to retire created by Act 10 appear to have caused a large spike in retirements, with attrition doubling from 17 to 35 percent for teachers of retirement-eligible age in the first year after the reform. Identifying the longer-run effects of the Act is more difficult, although the results are consistent with Act 10 having contributed to a more modest rise in the longer-run turnover rates for both younger and older teachers, with effects plausibly as large as 2 or 3 percentage points.

V. The Effects of Teacher Retirements on Education Inputs and Quality

In this Section, I examine how the large increase in teacher retirements in 2011 affected the composition of teachers and the quality of education in the grade-levels (and schools) that these retirees left behind.

A. Empirical Strategy

I first describe the main empirical strategy and required assumptions for estimating the impacts of teacher retirements in 2011 on subsequent outcomes. For clarity, I focus the discussion on the strategy and required assumptions for the effects of retirements on value-added, which will be the primary subject of this section, although I use an analogous methodology with parallel required assumptions to analyze the effects of retirements on other outcomes such as teacher experience, compensation, and student-teacher ratios.

I begin with an "event study" approach in which I compare the performance over time of school-grade-levels with higher and lower fraction of retirees in 2011. Formally, I estimate OLS regressions of the form:

$$\begin{aligned} y_{gst} = &\sum_{\tau \neq 2011} (frac\_retire2011)_{gs}\, \mathbb{1}\left[t=\tau\right] \beta_{1,\tau} + \\ &\sum_{\tau} \mathbb{1}\left[t=\tau\right] \beta_{0,\tau} + \phi_{gs} + \epsilon_{gst} \end{aligned} \tag{1}$$

where y_{gst} is value-added (or another outcome of interest) in grade g in school s at time t; $frac\_retire2011$ is the fraction of teachers in the school-grade-level that retired in 2011; and ϕ_{gs} is a school-grade-level fixed effect, which absorbs time invariant differences across school-grade-levels with higher and lower fractions of retirees in 2011.

The main coefficient of interest is $\beta_{1,2012}$, which indicates how value-added changed between 2011 and 2012 in grades with higher fractions of retirees in 2011 relative to other grades. $\beta_{1,2012}$ will identify the causal impact of retirements in 2011 on performance in 2012 under the assumption that, absent any retirements in 2011, school-grade-levels with higher and lower fractions of retirees would have had parallel trends.

I find that prior to 2011, school-grade-levels with higher and lower fractions of retirees had similar trends in effectiveness – i.e. the $\beta_{1,\tau}$ coefficients are generally close to 0 and insignificant. Nonetheless, there is reason to be concerned that $\beta_{1,2012}$ may not capture the causal impact of having higher fractions of retirees in 2011 on school-grade-level performance, although the direction of the bias is not obvious. If teachers choose to retire in anticipation of a negative shock to their school's performance – for instance, the retirement of a talented principal or disruptive construction outside of the school building – then $\beta_{1,2012}$ will be biased downwards. On the other hand, it is possible that retirements in 2011 were correlated with policies that improved student achievement, in which case the $\beta_{1,2012}$ will be biased upwards. For instance, following the passage of Act 10 certain principals may have attempted to exert more effort from their teachers or to impose a standardized curriculum, simultaneously improving value-added and inducing a larger number of teachers to retire. These endogeneity concerns would arise in any year, but they may be particularly sharp for retirements following 2011, since the introduction of Act 10 may have given school administrations more latitude to introduce new policies.

To address the potential endogeneity of retirements in 2011, I employ an instrumental variables strategy in which I instrument for the fraction of teachers retiring in 2011 with the fraction of teachers over the minimum retirement age of 55 in 2011.[19] This strategy will overcome the endogeneity issues discussed above under the assumption that school-grade-levels with higher fractions of retirement-eligible teachers are no more likely to experience policy changes or other shocks that influence effectiveness. Although this assumption is plausible, there still may be some concern that older teacher sorts to different types of schools (or grade-levels within a school), which may react differently to the introduction of a reform like Act 10. I employ two strategies to address this concern. First, I add controls for observable time-varying characteristics – such as principal turnover and district-level funding – to see whether these affect the results. Second, I conduct a falsification test in which I include the fraction of teachers just below the retirement-eligible age (age 45 to 54) in addition to the fraction 55 and over. If the age distribution affects trends in performance through the channel of retirements, then we would not expect to see sharp changes in VA for school-grade-levels with higher fractions of teachers just below the minimum retirement age. Conversely, if other factors that are correlated with age are driving the results, we would expect to see similar patterns for near-retirement and retirement-eligible teachers.

[19]More precisely, since I observe birth-year but not birthdate, I use the fraction of teachers born in 1956 or earlier as the instrument. A small number of teachers born in the latter part of 1956 will therefore be characterized as having been retirement-eligible in 2011 despite not having reached the minimum retirement age by the summer of 2011, when retirement decisions were made.

B. Results for Teacher Composition

Before proceeding to the main results using value-added, I first present evidence on how the composition of teachers in a school-grade-level changed following the retirement of a teacher in 2011 (Figure 6 and Table 2). Perhaps unsurprisingly, school-grade-levels with higher fractions of retirees in 2011 experience a drop in average teacher experience and compensation in 2012, and a rise in the fraction of novice teachers, relative to other school-grade-levels. They also experience a small rise in student-teacher ratios, as would occur if not all retirees are replaced, although the changes are relatively small and statistically insignificant. Table 2 shows that the OLS and IV methods produce fairly similar results for these outcomes.

C. Event-Study Results for School-Grade-Level Value-Added

The top panel of Figure 7 shows the OLS event study results for math value-added. The figure shows that school-grade-levels with higher fractions of retirees in 2011 seemed to have fairly stable value-added prior to the policy change – with, if anything, perhaps a slightly negative trend – before improving significantly in the year following the policy change. The bottom panel of Figure 7 shows the analogous results using the instrumental variables strategy. The patterns are similar to the OLS results, and the coefficient for 2012 is in fact even larger than using OLS. Similarly, Figure 8 shows the OLS and IV results for reading VA. The results are qualitatively similar to those for math, although the magnitude of the effects is smaller, and the IV estimate for 2012 is not significant (although it is similar in magnitude to the OLS coefficient).[20] The similarity between the IV and OLS results gives some confidence that the improvements in value-added in grades with retirements are not driven by the endogeneity of retirements to school policies that also improve performance.

To interpret the magnitudes of the coefficients, it is useful to consider the following simplified model. Suppose that school-grade-level value-added is merely the average of the value-added of the individual teachers in that school-grade-level, and that all of the changes in effectiveness between school-grade-levels with and without retirees were driven by differences in effectiveness between retirees and their replacements. Under these assumptions, $\beta_{1,2012}$ would identify the average difference in value-added between retirees in 2011 and their replacements.[21] Thus, the coefficient of 0.09 for math value-added would suggest that retirees raised their students' test scores by 0.09 standard deviations less than their replacements. Although these assumptions may not hold in practice – teacher retirements may influence school-grade-level effectiveness through other mechanisms, for

[20] In Appendix A.A1, I reproduce analogous figures using school-grade-level value-added estimates constructed by VARC for internal DPI review. The findings for math are quite similar to those presented here. In reading, the OLS results are again positive but no longer significant, and the IV coefficient for reading flips sign, but again is insignificant. See the Appendix for a more detailed discussion.

[21] To see why this is the case, consider the case of a school-grade-level with 2 teachers in 2011, one of whom retired in 2011 and was replaced by a teacher who raised student test scores by 0.1 standard deviations more than the retiree. If the second teacher remained and had the same effectiveness as before, then overall school-grade-level value-added would rise by 0.05 standard deviations – precisely the fraction of teachers who retired (0.5) times the difference in value-added between the retiree and his replacement (0.1).

instance – the model described above provides a useful framework for interpreting the magnitude of the effects.

Table 4 summarizes the results when adding time-varying controls or time-varying fixed effects to the main event-study specification. The coefficients are quite similar when adding controls for district-level funding and for whether a school has a new principal, suggesting that district-level changes in funding and principal turnover are not driving the observed increases in effectiveness in school-grade-levels with more retirees. It is important to note, however, that the estimated effects are smaller and not statistically significant (albeit less precise) when including school-by-year fixed effects. The implication is that school-grade-levels with retirees in 2011 experience a sudden increase in value-added in 2012, but so do nearby grades in the same school.[22] This intuition is confirmed in Figure 9, which shows estimates of equation (1) when collapsing the data to the school-by-year level.

These patterns may contribute to concerns that the fraction of older teachers is correlated with confounding schoolwide changes in 2012, although it is also possible that retirements have causal spillover effects across school-grade-levels, as I discuss extensively in Section V.E. To try to distinguish between the causal effects of retirements and unobserved confounding factors at the school level that are correlated with the age distribution, I run placebo tests in which I regress the growth in school-grade-level value-added between 2011 and 2012 on the fraction of teachers above the retirement age (55-plus) and the fraction of teachers just below the retirement age (45 to 54). The results are shown in Table 5. In math, the coefficient on the fraction age 55-plus is positive and significant, whereas the coefficient on teachers 45 to 54 is smaller and not significant, as we would expect if retirements, rather than other changes correlated with the age distribution, were driving the results. However, I cannot rule out that the two coefficients are equal at conventional levels (p-value = 0.17). Likewise, I do not find that either coefficient is significant for reading, which is unsurprising given that the IV results were not significant for reading. Additionally, I show in Appendix Table A3 that the coefficients for the placebo group are somewhat sensitive to the choice of value-added and sample used. Thus, while the math results using the primary analysis sample are suggestive in favor of a causal interpretation, I cannot fully rule out some role for omitted variables correlated with the teacher age distribution.

D. *Comparison to Previous Years and Implied Effects for Marginal Retirees*

In Section V.C, I focused on the average effects of retirements in 2011 on subsequent education quality. However, since some teachers would have retired regardless of the reform, we may be more interested in the effects of the *marginal* retirement in 2011. Under certain additional assumptions, we can estimate the average effect of a marginal retirement in 2011 by comparing the effects for 2011 shown above to similar estimates constructed in the pre-period. In particular, suppose that i)

[22] Recall that my analysis uses value-added for grades 3 to 5, so using school-by-year fixed effects compares, for instance, 3rd grade in a school to 4th and 5th grades in the school in the same year.

all teachers who would have retired in 2011 absent the reform retired anyway (monotonicity), and ii) retirements of such always-takers had the same average effect on education quality as retirements in the pre-period. It follows from these assumptions that the average effect of retirements in 2011 is just the weighted-average of the effects for marginal retirees and for retirees in the pre-period. That is,

$$\beta_{2011} = \alpha\beta_{2011}^{marginal} + (1-\alpha)\beta_{pre} \tag{2}$$

where α is the fraction of marginal retirees in 2011, and β_{2011}, $\beta_{2011}^{marginal}$ and β_{pre} are respectively the average effects of retirements for all teachers in 2011, for marginal teachers in 2011, and for teachers in the pre-period. Equation (2) allows us to solve for the effect for marginal retirees in terms of the average effects of retirements in 2011 and in the pre-period:

$$\beta_{2011}^{marginal} = \frac{1}{\alpha}\beta_{2011} - \frac{1-\alpha}{\alpha}\beta_{pre} \tag{3}$$

I jointly estimate β_{2011} and β_{pre} by estimating the regression equation:

$$\begin{aligned} VA_{t+1} - VA_t = & frac\_retire_t \times \mathbb{1}[pre_t]\,\beta_{pre} + \beta_{pre}^0 \mathbb{1}[pre_t] + \\ & frac\_retire_t \times \mathbb{1}[2011_t]\,\beta_{2011} + \beta_{2011}^0 \mathbb{1}[2011_t] \end{aligned} \tag{4}$$

I estimate (4) both using OLS and by instrumenting for the fraction retired using the fraction of teachers in the school-grade-level over age 55 (interacted with indicators for pre and 2011). The parallel trends assumptions needed for identification are the same as those discussed in the previous sections, except they must now hold for both 2011 and prior years. To calculate α, I assume that absent Act 10 the retirement rate in 2011 would have been equal to that in 2010.

Table 6 shows the results for β_{pre} and β_{2011} as well as the implied effects for marginal retirees in 2011. The point estimates indicate that in the pre-period, retirements were associated with modest improvements in school-grade-level value-added, but the estimated effects are smaller than those for 2011 and are significant only in the OLS specification for math. As a result, the estimated effects for marginal retirees in 2011 are larger than the estimated average effects for 2011, although they are also somewhat less precise. Nonetheless, the OLS and IV estimates of the marginal effects are statistically significant in math, and the OLS results for reading allow us to rule out all but very small negative marginal effects. The IV results for reading are too imprecise to draw strong conclusions.

If the primary mechanism behind the changes in value-added was differences in teacher effectiveness between retirees and their replacements, then these results would suggest that the marginal retiree was particularly ineffective relative to the norm. This is consistent with Fitzpatrick and Lovenheim (2014)'s suggestion that low value-added teachers may be the most responsive to changes in teacher retirement incentives. The marginal effects estimates should be viewed with some caution, however, because there are other plausible causal mechanisms that would lead to violations of the assumptions needed to estimate the marginal effects. For instance, since retirees generally earn less than their replacements, retirements may benefit education quality by freeing up resources. Because the 2011 biennial budget reduced education funding, these effects may have been larger in 2011 for *all* retirees, which would violate the assumption that "always-takers" had the same effects as in previous years.

E. Possible Mechanisms

In this section, I consider what causal mechanisms might plausibly have driven the positive effects of retirements in 2011 on subsequent value-added. One candidate explanation is that the retirees had lower value-added than their replacements, which is the primary explanation offered by Fitzpatrick and Lovenheim (2014) for similar increases in school-grade-level effectiveness in Illinois following an Early Retirement Incentive program. This might have occurred if less-effective older teachers are more responsive to a potential loss in benefits, or if teacher effectiveness generally declines in the years just before retirement.[23] The smaller improvements following retirements observed in previous years could also be the result of differences in teacher effectiveness if teachers nearing retirement have below-average effectiveness.

I assess whether differences in teacher value-added alone could explain the results by examining the relationship between retirements in one grade and improvements in value-added in other grades in the same school. Under such a story, we would only expect retirements in one grade to be strongly associated with improvements in another grade to the extent that there is a game of "teacher musical chairs" in which replacement teachers enter the school in a different grade from the retirees and other teachers switch grades to balance things out. Table 7 and 8 show that teacher switching is in fact more common following a teacher retirement, but teacher switches primarily occur across adjacent grades. If differences in teacher effectiveness is the dominant mechanism, the effects of teacher retirements on performance should therefore be concentrated primarily in the grade-level and adjacent grades of the retiree, with more negligible effects for grades across which switching is more rare.

I test this hypothesis by regressing the growth in value-added between 2011 and 2012 in a particular school-grade-level on retirement rates in that school-grade-level as well as those for the nearby grades in the same school. In particular, since my value-added measures are for grades 3 through 5, which are often the highest grades in an elementary school, I include covariates for

[23]While many papers have shown returns to experience early in a teacher's career, there is less consensus on the experience-effectiveness profile for teachers nearing the end of their career (see, e.g., Papay and Kraft, 2015; Wiswall, 2013; Clotfelter, Ladd and Vigdor, 2006; Rivkin, Hanushek and Kain, 2005; and Rockoff, 2004). Additionally, most of the literature has focused on the *average* effectiveness of teachers at a given experience level. It is possible that, on average, experienced teachers are more effective, but teacher effectiveness declines in the few years before retirement.

the retirement rates in the grade-levels one, two, and three grades below the grade-level for which value-added is measured.

Table 9 shows the results of these cross-grade regressions. In reading, the results look as one would expect if differences in teacher effectiveness were the driving mechanism: the coefficients on other grades are smaller than the own-grade coefficient and are (both individually and jointly) insignificant. I cannot, however, rule out that all of the retirement coefficients, including the own-grade coefficient, are jointly 0. Moreover, in math there is a large positive and significant coefficient on the retirement rate 3 grades below the grade for which value-added is measured, in contrast to the predictions of the pure teacher effectiveness model discussed above. Table 10 shows the results of a similar exercise in which I regress changes in value-added on the fraction of older teachers in the reference grade and nearby grades (i.e. I run the reduced form of the IV approach). Again, for math there is a positive and significant coefficient on the measure three grades below, as well as positive coefficients for retirements one and two grades below.[24]

The association between improvements in math value-added in one grade and retirements in the grade-level three grades below suggests that changes in teacher VA likely do not fully account for the observed improvements in value-added. This does not necessarily imply, however, that differences in teacher effectiveness did not contribute at all to the observed effects in combination with other mechanisms that affected other grades. To test whether this is the case, we would like to know whether the grade-levels with retirees improved more than the surrounding grades, as would likely occur if retirees were less effective than their replacements. Table 11 shows the results of regressions in which I regress grade-level value-added on both the own-grade and schoolwide retirement rates in 2011. The results are unfortunately quite imprecise: while neither own-grade coefficient is statistically significant at the 5 percent level, the coefficients are positive in both math and reading (and significant at the 10 percent level in math), and I cannot rule out that retirements raise VA by $.07\sigma$ more in their own-grade than in other grades in the school in both subjects. I thus cannot rule out that differences in teacher effectiveness were a substantial component of the observed increases in value-added, but the correlations across grades indicates that they were not the only factor.

What other mechanisms might have contributed to these patterns in performance? One possibility is that teacher retirements free up money in school budgets, since retirees earn more than their replacements, and this money is then re-allocated to more productive sources, with similar effects on all grades. Figure 6 suggests that replacement teachers are paid almost $20,000 less than the retirees that they replace on average. I do not have access to school-level budgeting data, so it is difficult to measure how these savings are used. However, a useful benchmark to assess the potential importance of this mechanism is how much would test scores have improved if all of the

[24]Interestingly, Fitzpatrick and Lovenheim (2014) do not find significant cross-grade effects of retirements following an early retirement incentive program in Illinois. However, they are only able to estimate such effects for a subset of their sample, and they also are unable to detect any significant impacts of retirements on own-grade performance within this sample. It is thus not clear whether the cross-grade effects of retirements differ across contexts, or whether Fitzpatrick and Lovenheim (2014) are merely underpowered to detect such effects.

cost savings had been used to reduce student-teacher ratios.[25] If we assume as in Rothstein (2015) that a 1 percent decrease in student-teacher ratios is associated with a 0.004σ improvement in test-scores – an extrapolation of Kruger (1999)'s results from the Project STAR experiment – then a 10 percentage point increase in the retirement rate at the school level would be associated with a 0.01σ improvement in performance.[26] By comparison, my OLS results suggest that a 10 pp increase in the retirement rate at the school level is associated with a 0.016σ improvement in student performance. Thus, if the resources formerly spent on retiree compensation were reallocated to something equally as productive as student-teacher ratios, then this channel could plausibly have accounted for over half of the observed increases in value-added.

Interestingly, however, there does not appear to have been a strong association between retirements in nearby grades and improvements in value-added in years prior to 2011 (Table 12). If reallocation of resources drove this correlation following Act 10, we might have expected to see a similar correlation in previous years, since retirees also tended to receive higher compensation in previous years. It is possible, though, that the resource savings mattered more following Act 10 than beforehand, possibly because schools were operating under tighter budgets following Act 10, or because the expiration of collective bargaining enabled schools to reallocate the resources in different ways than beforehand. The results are thus consistent with a reallocation of resources having played a role following Act 10 if the returns to school resources are context-specific.

Another possibility is that the teachers retiring following Act 10 may have had negative peer effects on other teachers in the same school. Older teachers may have particularly large effects on other teachers within a school, since they are more likely to be placed in leadership positions, such as department head or head of teacher training. Additionally, since preserving retiree health insurance was one of the main incentives for teachers to retire following Act 10, it is possible that teachers in poor health were the most responsive to the reform. Such teachers are likely to be absent frequently, which may have disruptive effects on other teachers within the school.

It is of course possible that other mechanisms were at play as well, and pinning down the exact channels by which retirements affect education quality seems a promising avenue for future research.

VI. Conclusion

This paper examines changes in teacher attrition in Wisconsin following Act 10, a policy that severely weakened teachers' unions and reduced teacher compensation. I find a sharp increase in teacher attrition the year after the Act was passed, and I show that this was driven primarily by the exit of older teachers, who faced incentives to retire prior to the end of pre-existing union

[25] Figure 6 shows that student-teacher ratios actually increased slightly following teacher retirements. Reductions in student teacher ratios should therefore be viewed only as a benchmark for how effective changes in spending are likely to be, rather than a statement of how cost savings were allocated.

[26] In 2011, average total teacher compensation was approximately \$80,000. If a school saved \$20,000 on 10 percent of its teachers, this would reduce average compensation by 2.5% (\$2,000), and hence allow the school to reduce its student-teacher ratio by 2.5%.

contracts in order to secure collectively-bargained retirement benefits. Somewhat surprisingly, I find that retirements in the year following the Act are associated with subsequent improvements in school-grade-level value-added, especially in mathematics. A combination of differences in teacher value-added, changes in school resources, and teacher peer effects could explain the results, although the exact mechanisms are not entirely clear. Nonetheless, this paper suggests that the exodus of a large number of experienced teachers following Act 10 was not as detrimental as the existing literature on teacher experience and turnover would suggest – these retirements either directly caused improvements in education quality, or schools were able to more than compensate for their departure with other changes.

A few caveats are in order. First, while I find no evidence that the turnover induced by Act 10 adversely affected students, it is important to note that Act 10 may have affected overall education quality through other channels, and so the results here do not fully resolve the question of how restrictions on collective bargaining affect education quality. Second, I am only able to address the short-run impacts of changes in teacher labor supply following Act 10, and it is certainly possible that the long-run impacts will differ. For instance, while my results suggest that in the immediate aftermath of Act 10 schools were able to find adequate replacements for retirees, it is possible that the long-run supply of new teachers is more elastic, in which case schools may eventually face teacher shortages. Finding adequate replacement teachers may also become more difficult as labor markets tighten. Third, while value-added estimates similar to those used in this paper have been shown to be correlated with long-run outcomes (Chetty, Friedman and Rockoff, 2014), they are by no means a perfect measure of education quality. If young replacement teachers in 2012 were more motivated to "teach to the test" than the retirees they replaced, then the observed growth in value-added may not reflect true differences in human capital acquisition for students. Future research that explores the longer-term impacts of Act 10 on teacher quality or student outcomes, such as college attendance and earnings in adulthood, could therefore prove quite fruitful.

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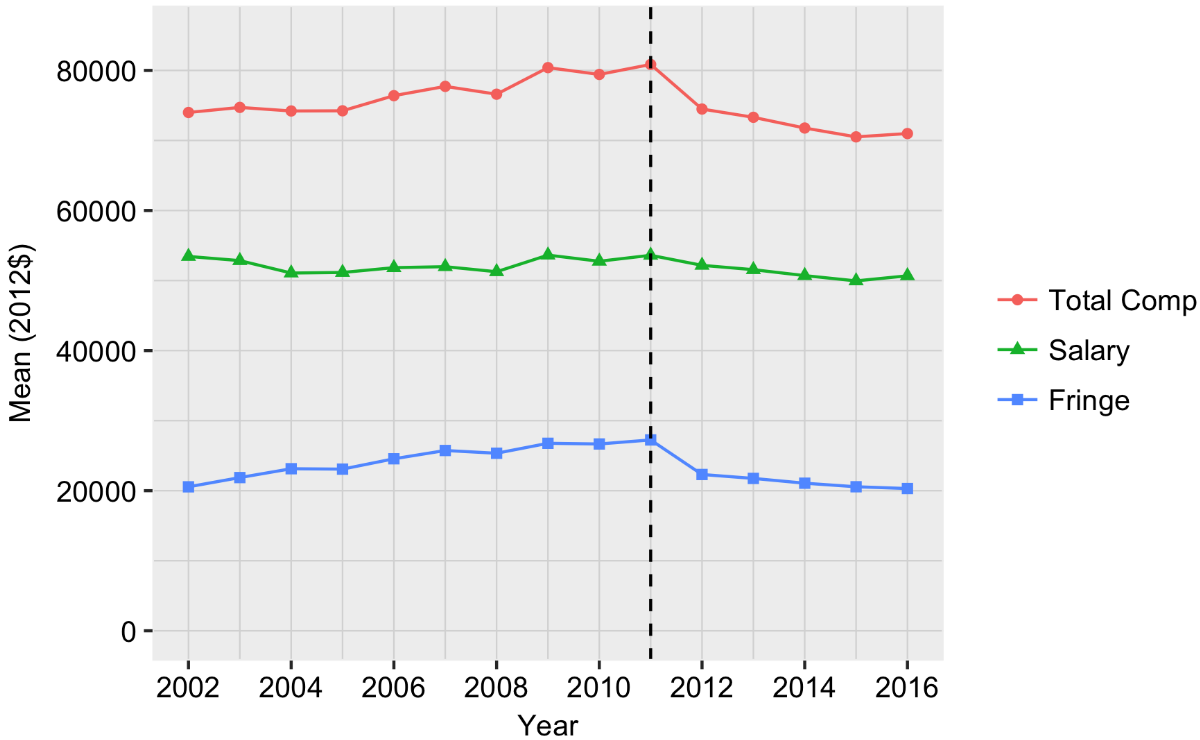
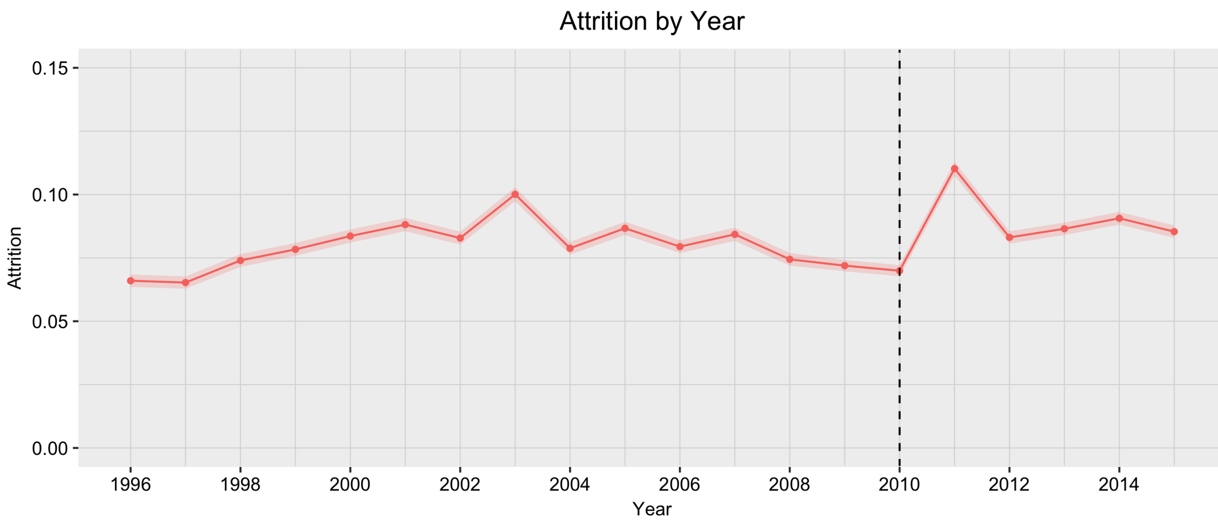
Figure 1: Changes in Real Compensation Following Act 10



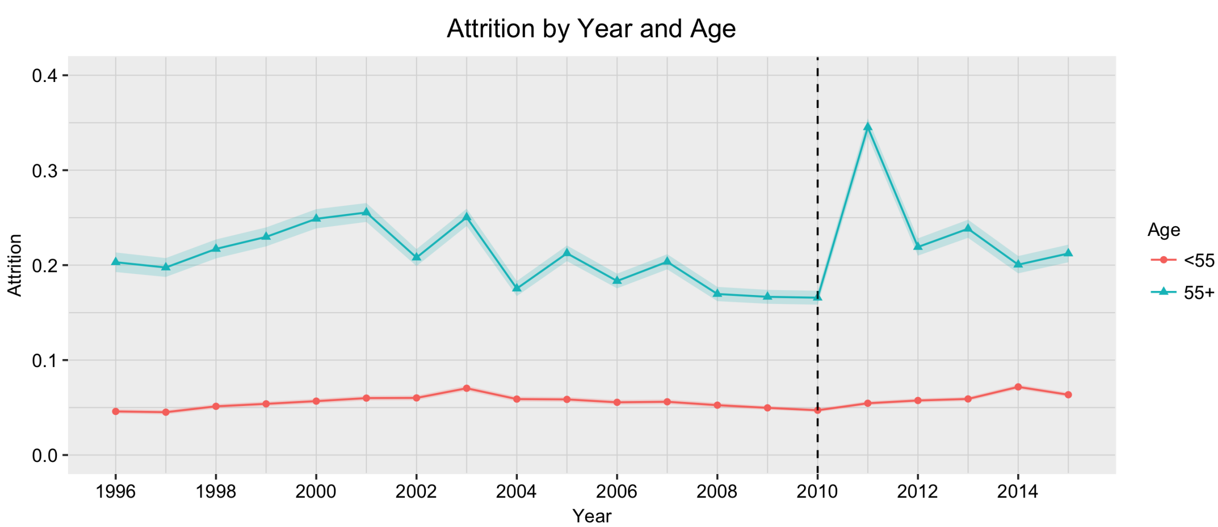
Note: This figure shows mean real salary, fringe benefits, and total compensation (salary + fringe) for regular education teachers in Wisconsin. All values are converted to 2012 dollars using the CPI-U-RS. The value of fringe benefits is estimated by the district for each of its employees as part of annual reports to the Wisconsin Department of Public Instruction (DPI), and incorporates employer contributions to health insurance and the pension system, as well as other benefits such as life and disability insurance. Data come from the annual All Staff files provided by DPI.

Figure 2



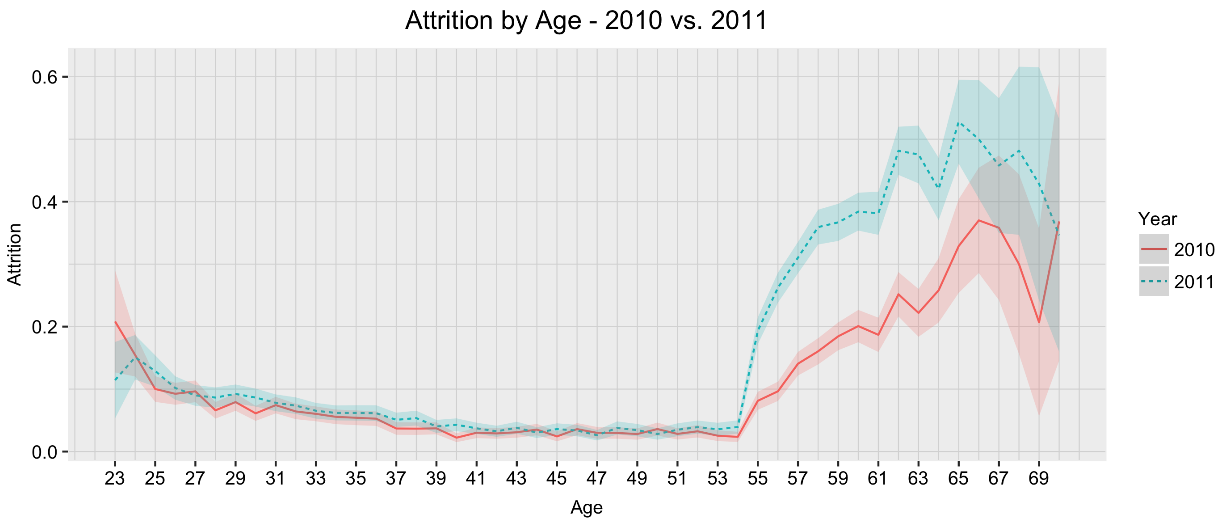
Note: This figure shows attrition rates by year and the corresponding 95% confidence intervals for regular education teachers in the Wisconsin public school system. Attrition rates are calculated using the All Staff files from the Wisconsin Department of Public Instruction. A teacher is said to have attrited between year t and year $t+1$ if they were a regular education teacher anywhere in the Wisconsin public schools in year t and were not a regular education teacher anywhere in the Wisconsin public schools in year $t+1$. See Section III.A for additional details on how attrition is calculated. The attrition rate plotted for a given year represents the fraction of teachers who attrited following the school year ending with that year – for instance, the attrition rate for 2007 represents the fraction of teachers in the 2006-2007 school year who did not teach in 2007-2008.

Figure 3



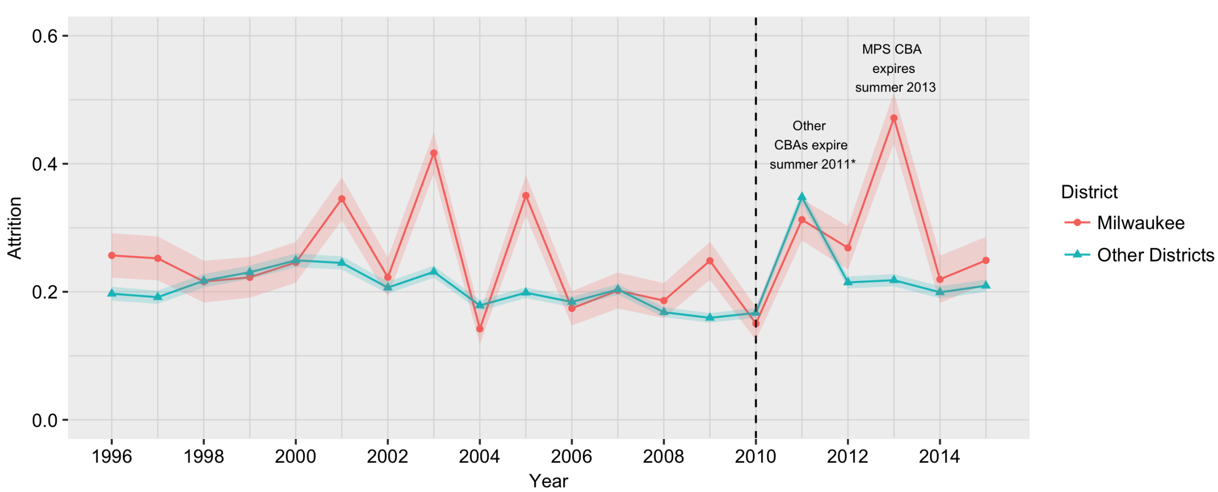
Note: This figure shows attrition rates by year and the corresponding 95% confidence intervals for regular education teachers in the Wisconsin public school system. Separate series are shown for teachers above and below the minimum retirement age of 55 in the relevant year. Attrition rates are calculated using the All Staff files from the Wisconsin Department of Public Instruction. A teacher is said to have attrited between year t and year $t+1$ if they were a regular education teacher anywhere in the Wisconsin public schools in year t and were not a regular education teacher anywhere in the Wisconsin public schools in year $t+1$. See Section III.A for additional details on how attrition is calculated. The attrition rate plotted for a given year represents the fraction of teachers who attrited following the school year ending with that year – for instance, the attrition rate for 2007 represents the fraction of teachers in the 2006-2007 school year who did not teach in 2007-2008.

Figure 4



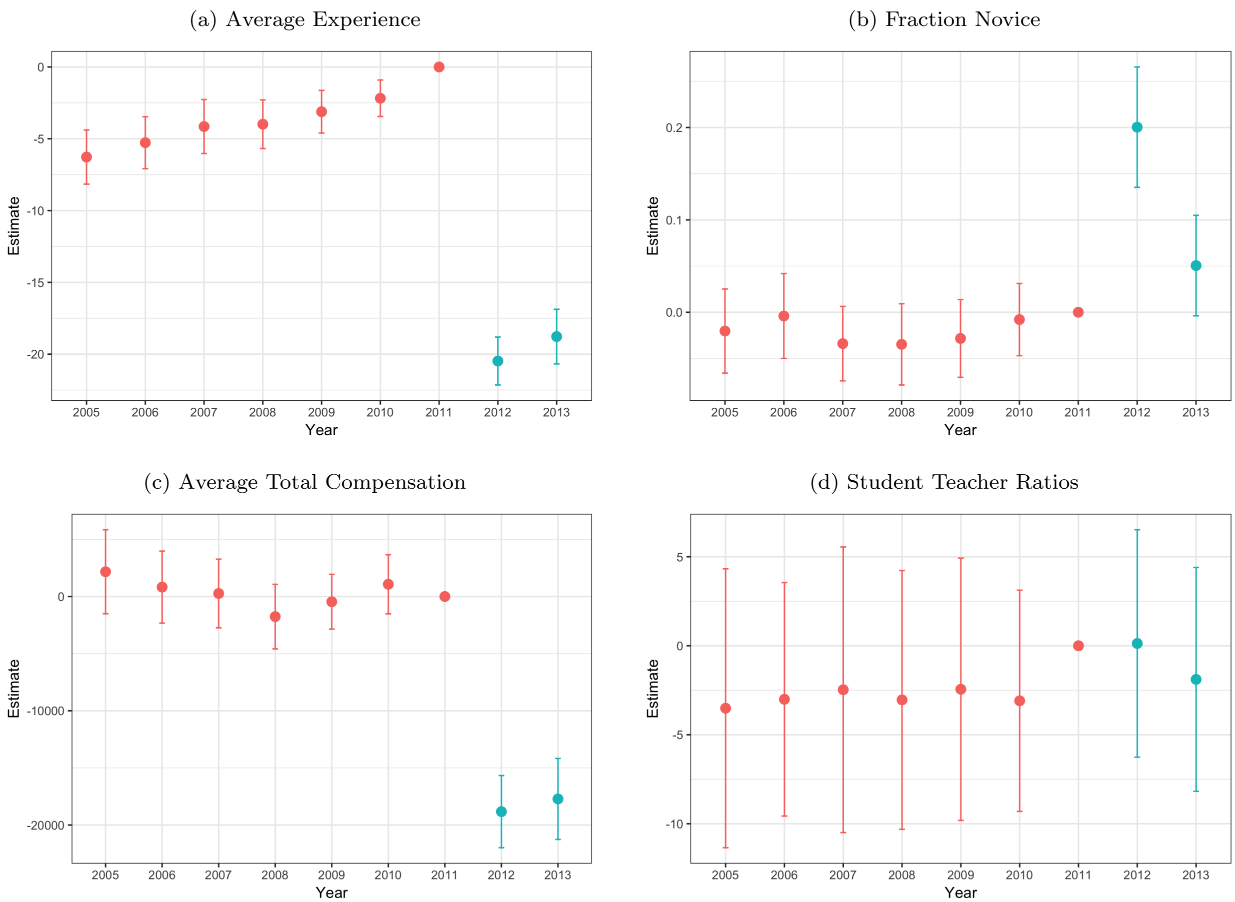
This figure shows attrition rates (and associated 95% confidence intervals) by age for regular education teachers in Wisconsin in 2010 and 2011. Attrition is calculated as described in the note to Figure 3. A small number of teachers younger than age 23 or older than age 70 are excluded (less than 0.2 percent of teachers are excluded).

Figure 5: Comparison of Attrition for Teachers Age 55+ in Milwaukee and Elsewhere



Note: This figure shows attrition rates by year for regular education teachers in the Wisconsin Public School system who were age 55 or older in the relevant year. See the notes to Figure 3 on how attrition is calculated. Separate series are shown for teachers in the Milwaukee Public Schools (MPS) and all other districts. As discussed in Section II.A, the MPS had an unusually long pre-existing CBA, ending in the summer of 2013, whereas the vast majority of other districts had a pre-existing CBA ending in summer of 2011.

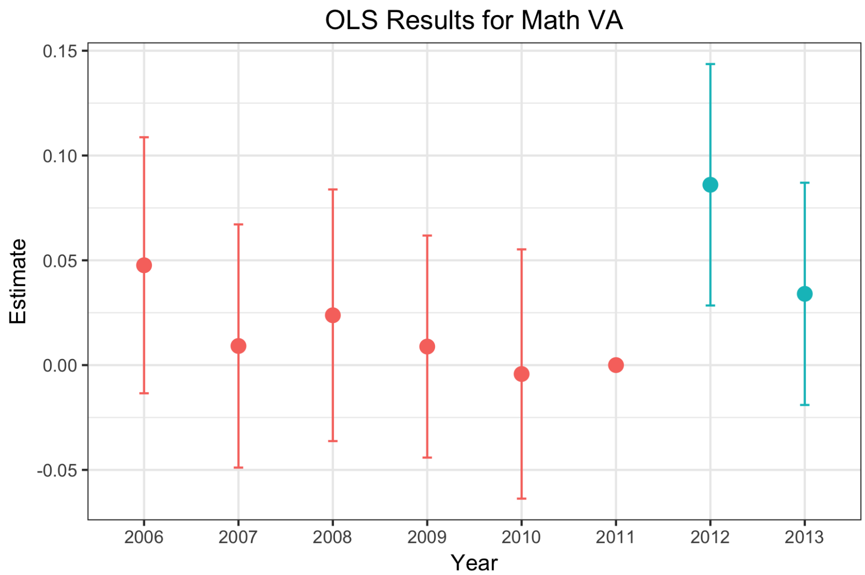
Figure 6: Event Study Results for the Effects of Retirements in 2011 on Teacher Composition



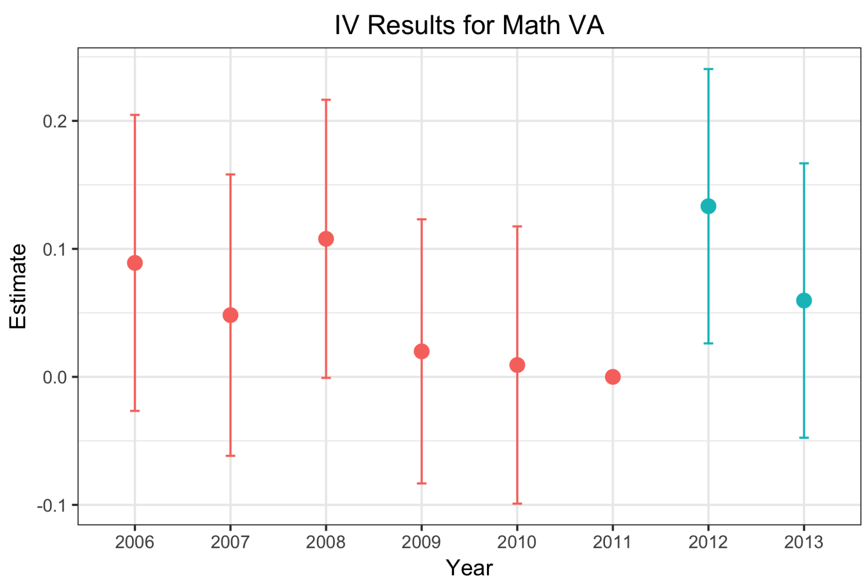
Note: This figure displays OLS coefficient estimates and the associated 95% confidence intervals for the $\beta_{1,\tau}$ in regression (1), i.e. the coefficients on $frac\_retire2011$ interacted with each year. The four panels show results using different outcome variables. In panel a), the dependent variable is the fraction of teachers in the grade-level who are new to the Wisconsin public school system. In panel b), it is the average experience of teachers in the grade-level, and in panel c) it is the average real total compensation (salary + benefits). In column (4), the dependent variable is the student-teacher ratio at the grade-level, i.e. the number of students in the grade-level, divided by the number of full-time-equivalent teachers. The coefficient for the year 2011 is normalized to 0. All standard errors are clustered at the school level, and regressions are weighted by the number of students in the grade-level taking the WKCE exam. For comparability with the value-added results, the regression sample contains grade-levels between 3rd and 5th grade for which I am able to construct value-added (see Section III.B for details).

Figure 7: Event Study Results for the Effects of Retirements in 2011 on Math Value-Added

(a) OLS



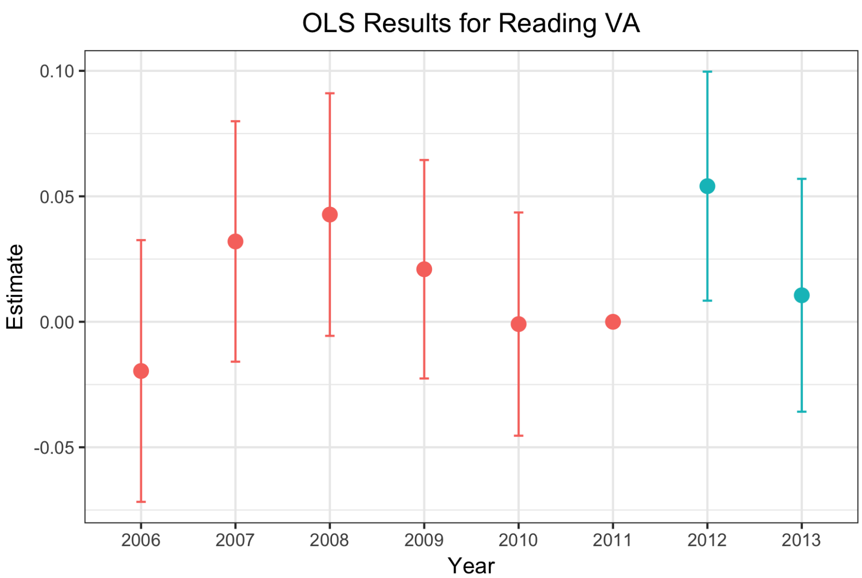
(b) IV



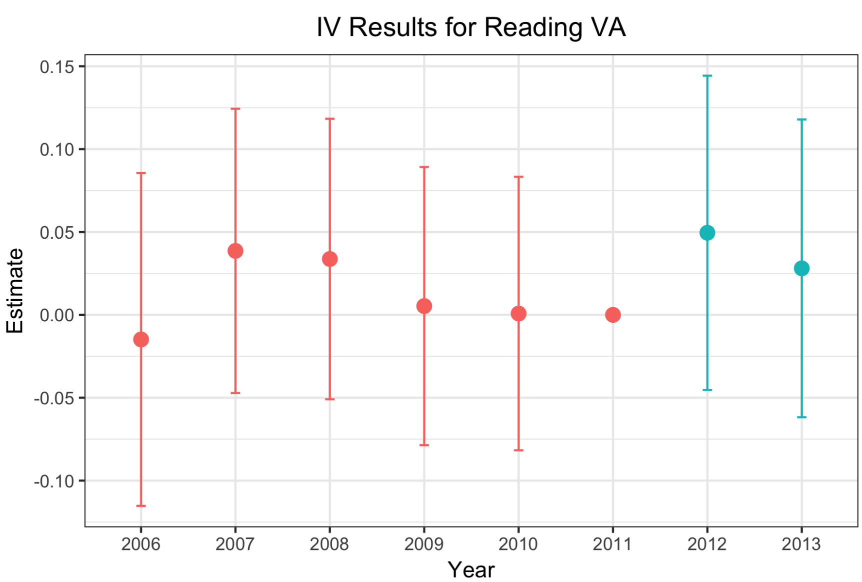
Note: This figure displays OLS and IV coefficient estimates and the associated 95% confidence intervals for the $\beta_{1,\tau}$ in regression (1), i.e. the coefficients on $frac\_retire2011$ interacted with each year. The dependent variable is school-by-grade-level value-added in mathematics. The coefficient for the year 2011 is normalized to 0. In panel b), I instrument for the fraction of teachers retiring in 2011 using the fraction of teachers age 55+. All standard errors are clustered at the school level, and regressions are weighted by the number of students taking the WKCE exam in math in the reference year. The regression sample contains grade-levels between 3rd and 5th grade for which I am able to construct math value-added (see Section III.B for details).

Figure 8: Event Study Results for the Effects of Retirements in 2011 on Reading Value-Added

(a) OLS

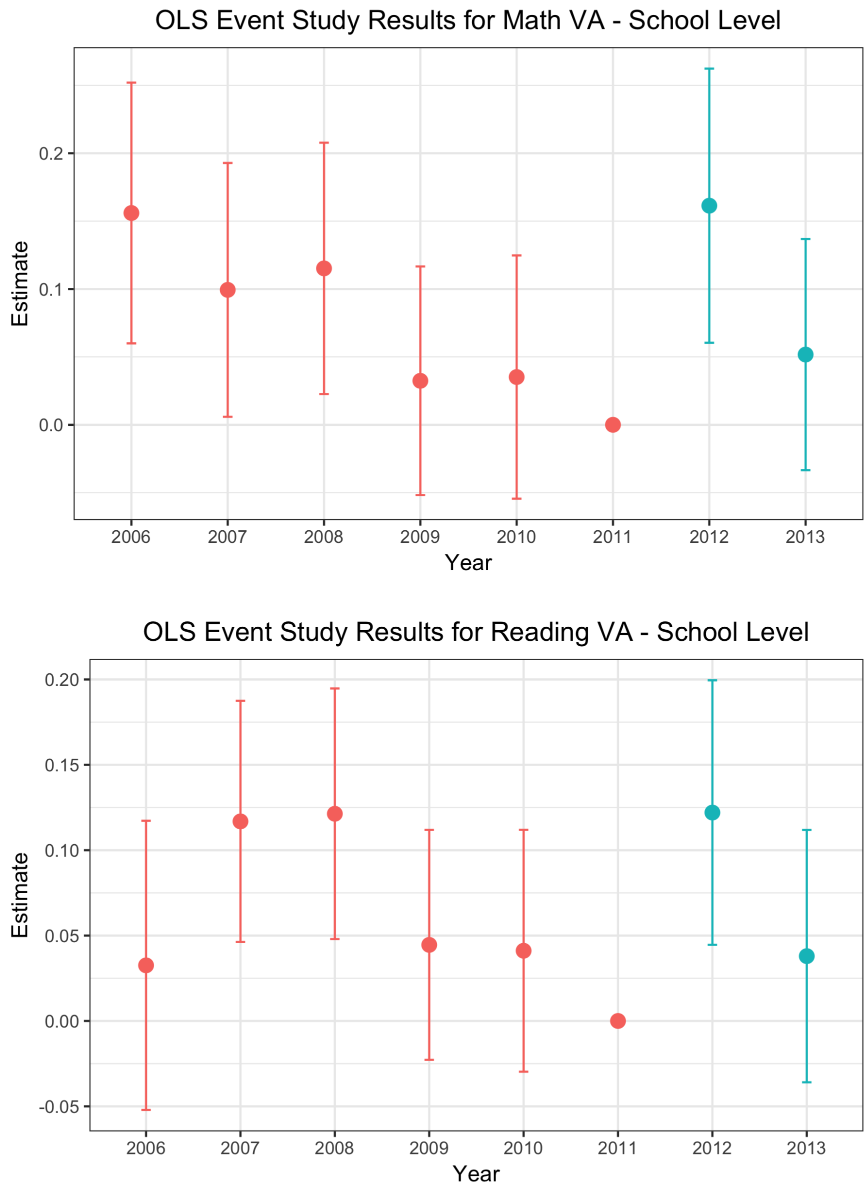


(b) IV



Note: This figure displays OLS and IV coefficient estimates and the associated 95% confidence intervals for the $\beta_{1,\tau}$ in regression (1), i.e. the coefficients on $frac\_retire2011$ interacted with each year. The dependent variable is school-by-grade-level value-added in reading. The coefficient for the year 2011 is normalized to 0. In panel b), I instrument for the fraction of teachers retiring in 2011 using the fraction of teachers age 55+. All standard errors are clustered at the school level, and regressions are weighted by the number of students taking the WKCE exam in reading in the reference year. The regression sample contains grade-levels between 3rd and 5th grade for which I am able to construct reading value-added (see Section III.B for details).

Figure 9: School-Level Event Study Results for the Effects of Retirements in 2011 on Value-Added



Note: This figure displays OLS coefficient estimates and the associated 95% confidence intervals for the $\beta_{1,\tau}$ in a modified version of regression (1), in which all observations are collapsed to the school-level. The dependent variable is the average of all the school-by-grade-level value-added estimates available for a given school in mathematics (top panel) or reading (bottom panel). The dependent variable, $frac\_retire2011$, represents the share of teachers retiring in the grades-levels for which value-added is available. The coefficient for the year 2011 is normalized to 0. All standard errors are clustered at the school level, and regressions are weighted by the number of students taking the WKCE exam in math in the reference year in the grade-levels for which value-added is available. The regression sample contains schools for which I am able to construct a math value-added estimate for at least one grade-level between third and fifth grade (see Section III.B for details).

Table 1

Retiree Healthcare Benefits

For 'Typical' Retiree in Milwaukee ($70k Salary, 20 years experience)

Time of Retirement	Pre-existing CBA 2013		Post-CBA 2014	
Age at Retirement	55-64		55-59	60-64
Annual Premium Subsidy until Age 65	$18,953		$0	$16,502
		Difference from 2013:	-$18,953	-$2,451

Note: This table shows the annual healthcare subsidy for a ‘typical’ retiring teacher in the Milwaukee Public Schools (MPS) at two points in time: the summer of 2013, just prior to the expiration of Milwaukee’s CBA in place from before Act 10, and the summer of 2014, after the pre-existing CBA had expired. The typical teacher is assumed to have a salary of $70,000 and 20 years of experience, roughly the median for a 55 year-old teacher in Milwaukee in 2013. The typical teacher is also assumed to choose a family (rather than individual) health plan, which is the more common plan choice.

Under the pre-existing CBA, retirees could continue on the health plan that they were on at the time of their retirement, and the district would continue to contribute towards the premium the amount that it had contributed towards the employee PPO health plan at the time of the teacher’s retirement. This arrangement would be in place until the retiree turned 65, at which point the retiree would be required to enroll in Medicare, and the district would contribute towards secondary coverage. This benefit was available to teachers who retired at age 55 or older with 15 or more years of experience. The following changes came into effect upon the expiration of the pre-existing CBA: First, the eligibility requirements to receive retiree health benefits were increased to 60 years of age and 20 years of experience, effectively eliminating the benefit for teachers age 55-59. (There was a grandfathering period of two years for teachers age 55-59 with 30 or more years of experience, although fewer than 10% of teachers in that age-range in 2014 qualified). Second, active employees were required to contribute more for their healthcare (with rates depending on teacher salary), thus reducing the amount the district would continue to subsidize in retirement. Lastly, the district modified the funding formula to depend on the average of the district’s contribution to its PPO and EPO health plans, rather than basing the subsidy only on the PPO plan.

Source: Milwaukee Public Schools Retiree Healthcare and Life Insurance Programs, Actuarial Valuation as of July 1, 2011; Milwaukee Public Schools Summary of Benefits (2012, 2013)

Table 2: OLS and IV Event-Study Results for the Effects of Retirements in 2011 on Teacher Composition in 2012

	Fraction Novice	Average Experience	Average Compensation	Student-Teacher Ratio
	(1)	(2)	(3)	(4)
OLS				
Coefficient for 2012	0.200***	−20.479***	−18,827.160***	0.129
	(0.033)	(0.850)	(1,611.064)	(3.260)
IV				
Coefficient for 2012	0.267***	−14.985***	−23,904.920***	2.630
	(0.047)	(1.399)	(2,788.531)	(2.710)

Note: Each element in the table represents an estimate of $\beta_{1,2012}$ from a separate specification of regression equation (1). In column (1), the dependent variable is the fraction of teachers in the school-grade-level who are new to the Wisconsin public school system. In column (2), it is the average experience of teachers in the school-grade-level, and in column (3) it is the average real total compensation. In column (4), the dependent variable is the student-teacher ratio at the school-grade-level, i.e. the number of students divided by the number of full-time-equivalent teachers. The first row in the table shows estimates from OLS regressions using the fraction of teachers in each school-grade-level who retired in 2011, whereas the second row shows coefficients from instrumental variables regressions in which I instrument for the fraction of teachers retiring in 2011 using the fraction age 55 and above in 2011. All standard errors are clustered at the school level, and regressions are weighted by the number of students taking the WKCE exam. For comparability with the value-added results, the regression sample contains school-grade-levels between 3rd and 5th grade for which I am able to construct value-added (see Section III.B for details).

Table 3: First Stage IV and F-Statistics

	Dependent variable:	
	Fraction Retiring in 2011	
	(1)	(2)
Fraction 55+ in 2011	0.362***	0.351***
	(0.019)	(0.032)
Constant	0.001	
	(0.002)	
School FE	No	Yes
Partial F-stat	364	119
Observations	2,321	2,321
Note:	*p<0.1; **p<0.05; ***p<0.01	

Note: This table shows the results for first-stage regressions in which I regress the fraction retiring in 2011 in a school-grade-level on the fraction of teachers age 55+ in 2011 in the school-grade-level. Column 2 contains school fixed effects. All standard errors are clustered at the school level, and regressions are weighted by the number of students taking the WKCE exam. The regression sample contains school-grade-levels between 3rd and 5th grade for which I am able to construct value-added (see Section III.B for details).

Table 4: OLS and IV Event-Study Results for the Effects of Retirements in 2011 on School-Grade-Level Value-Added in 2012

	Math VA			*Reading VA*		
	(1)	(2)	(3)	(4)	(5)	(6)
OLS						
Coefficient for 2012	0.086***	0.088***	0.008	0.054**	0.052**	−0.023
	(0.029)	(0.031)	(0.046)	(0.023)	(0.025)	(0.040)
IV						
Coefficient for 2012	0.133**	0.144**	0.047	0.050	0.055	−0.041
	(0.055)	(0.058)	(0.082)	(0.048)	(0.051)	(0.076)
School x Grade FE	X	X	X	X	X	X
Time-Varying Controls		X			X	
School x Year FE			X			X

Note: Each element in the table represents an estimate of $\beta_{1,2012}$ from a separate specification of regression equation (1). In columns (1)-(3), the dependent variable is school-grade-level math value-added, whereas in columns (4)-(6) it is reading value-added. The first row in the table shows estimates from OLS regressions using the fraction of teachers in each school-grade-level who retired in 2011, whereas the second row shows coefficients from instrumental variables regressions in which I instrument for the fraction of teachers retiring in 2011 using the fraction age 55 and above in 2011. The regressions in columns (2) and (5) include time-varying controls for whether the school had a new principal and district-level expenditure per pupil taken from the Common Core of Data School District Finance Survey. All standard errors are clustered at the school level, and regressions are weighted by the number of students taking the WKCE exam in the relevant subject. The regression sample contains school-grade-levels between 3rd and 5th grade for which I am able to construct value-added (see Section III.B for details).

Table 5: Placebo Tests Using Teachers Age 45 to 54

	Change in VA, 2011-2			
	Math		Reading	
	(1)	(2)	(3)	(4)
Fraction 55+ in 2011	0.041**	0.047**	0.016	0.023
	(0.018)	(0.019)	(0.015)	(0.017)
Fraction 45-54 in 2011		0.019		0.021
		(0.017)		(0.015)
Constant	−0.005	−0.012	−0.0004	−0.008
	(0.007)	(0.009)	(0.006)	(0.009)
P-value for equality		0.165		0.934
Observations	2,252	2,252	2,252	2,252
Note:		*p<0.1; **p<0.05; ***p<0.01		

Note: This table shows the results of OLS regressions in which the growth in school-grade-level value-added between 2011 and 2012 is regressed on the fraction of teachers over the retirement age of 55 in 2011, and the fraction of teachers age 45-54. The regression sample contains school-grade-levels between 3rd and 5th grade for which I am able to construct value-added (see Section III.B for details). All standard errors are clustered at the school level, and regressions are weighted by the number of students taking the WKCE exam in 2011.

Table 6: Comparison of Retirement Effects in the Pre-Period versus 2011, and Implied Estimates of the Effects of Marginal Retirees

(a) OLS

	Change in VA	
	Math	Reading
	(1)	(2)
Frac Retire x Pre	0.040**	0.012
	(0.018)	(0.016)
Frac Retire x 2011	0.073***	0.046**
	(0.027)	(0.022)
Pre	−0.0005	−0.0005
	(0.001)	(0.001)
2011	−0.002	−0.001
	(0.006)	(0.005)
Effects for Marginal Retirees		
Estimate	0.096	0.071
s.e.	(0.047)	(0.039)
Lower Bound	0.003	-0.005
Observations	13,467	13,467
Note:	*p<0.1; **p<0.05; ***p<0.01	

(b) IV

	Change in VA	
	Math	Reading
	(1)	(2)
Frac Retire x Pre	0.022	−0.007
	(0.037)	(0.032)
Frac Retire x 2011	0.114**	0.042
	(0.049)	(0.043)
Pre	0.0001	0.0001
	(0.002)	(0.002)
2011	−0.005	−0.0004
	(0.007)	(0.006)
Effects for Marginal Retirees		
Estimate	0.180	0.078
s.e.	(0.092)	(0.081)
Lower Bound	0.000	-0.081
Observations	13,467	13,467
Note:	*p<0.1; **p<0.05; ***p<0.01	

Note: Panel a) shows the results of OLS regressions in which the growth in school-grade-level value-added between year t and $t+1$ is regressed on the fraction of teachers retiring in year t, interacted with indicators for whether t is prior to the policy change ($t < 2011$) or the year following the policy change ($t = 2011$). I compute the effects of a marginal retirement in 2011 under the assumption that retirees who would have retired anyway in 2011 (always-takers) have the same impact on value-added as retirees in the pre-period. See Section V.D for details. Panel b) shows analogous results where the fraction of teachers retiring is instrumented for using the fraction of teachers age 55-plus. The regression sample includes data for years 2006-2011 on all school-grade-levels between 3rd and 5th grade for which I am able to construct math value-added. All regressions are weighted by the fraction of students taking the WKCE exam in year t, and standard errors are clustered at the school level.

Table 7: Grade Composition in 2012 by Whether Had Retiree in 2011

Had Retiree in:			Composition in 2012			
Grade?	School?	Frac Retired	Stayer	Novice	Grade Switcher	School Switcher
Y	Y	0.383	0.6	0.112	0.155	0.133
N	Y	0	0.801	0.049	0.088	0.062
N	N	0	0.786	0.053	0.104	0.057

Note: This table shows the mean teacher composition in 2012 for the school-grade-levels in the primary analysis sample. School-grade-levels are broken into three different groups: those that had a teacher retire at the end of 2011; those that did not have a teacher retire at the end of 2011, but were in a school that had an elementary school teacher retire in a different grade in 2011; and school-grade-levels that did not have a teacher retire at the end of 2011 and were not in a school with a elementary school retiree in 2011 (row 3). The teachers in each grade-level in a given school in 2012 are partitioned into 4 distinct groups: teachers who taught in the same grade-level in the same school in 2011 (Stayers); teachers new to the Wisconsin public school system (Novice); teachers who taught in a different school in the previous year (School Switcher); and teachers who taught in the same school the previous year but did not teach the given grade-level (Grade Switcher). The sample in this table is restricted to school-grade-levels in third through fifth grade for which value-added estimates are available (see Section III for more details).

Table 8: Number of Grades Moved for Grade-Switchers in 2012

Grades Moved	N	Percent
1	513	54.69
2	241	25.69
3	127	13.54
4	44	4.69
5	13	1.39

Note: This table shows the number of grades moved for elementary school teachers who switched within school into a 3rd, 4th, or 5th grade classroom in 2012. In the small fraction of cases in which a teacher previously taught multiple grades, I use the largest absolute difference between the old grade and the new grade.

Table 9: Growth in VA and Teacher Retirements in Nearby Grades in the Same School

	Change in VA, 2011-2			
	Math		Reading	
	(1)	(2)	(3)	(4)
Frac Retire in 2011, Own-grade	0.078***	0.069**	0.043*	0.041*
	(0.029)	(0.028)	(0.023)	(0.023)
Frac Retire in 2011, 1 Grade Down		0.055*		0.022
		(0.033)		(0.026)
Frac Retire in 2011, 2 Grades Down		−0.020		0.002
		(0.033)		(0.029)
Frac Retire in 2011, 3 Grades Down		0.093***		0.008
		(0.034)		(0.030)
Constant	0.001	−0.007	0.001	−0.001
	(0.006)	(0.008)	(0.006)	(0.007)
P-value for joint test:				
All Other-Grade Coeffs = 0		0.006		0.866
All Retirement Coeffs = 0		0.001		0.442
Observations	2,037	2,037	2,037	2,037
Note:				*p<0.1; **p<0.05; ***p<0.01

Note: This table shows the relationship between growth in school-grade-level value-added between 2011 and 2012 and the fraction of teachers retiring in the reference grade and surrounding grades in 2011. The dependent variable is the change in school-grade-level value-added in math or reading between 2011 and 2012. The regressors are respectively the fraction of teachers retiring in 2011 in the grade for which value-added is measured, as well as the fraction retiring in the grades in the school 1, 2, and 3 grades below that for which VA is measured. The regression sample includes all 3rd through 5th school-grade-levels for which I am able to construct value-added and for which the school also serves the grade-levels 1 to 3 grades below. All regressions are weighted by the number of students taking the WKCE examination in the grade for which value-added is calculated. All standard errors are clustered at the school level.

Table 10: Growth in VA and the Fraction of Older Teachers in Nearby Grades in the Same School

	Change in VA, 2011-2			
	Math		Reading	
	(1)	(2)	(3)	(4)
Frac Age 55+ in 2011, Own-grade	0.043**	0.038**	0.018	0.017
	(0.019)	(0.018)	(0.016)	(0.016)
Frac Age 55+ in 2011, 1 Grade Down		0.037*		0.018
		(0.021)		(0.017)
Frac Age 55+ in 2011, 2 Grades Down		0.014		−0.017
		(0.020)		(0.017)
Frac Age 55+ in 2011, 3 Grades Down		0.041**		0.017
		(0.021)		(0.018)
Constant	−0.002	−0.019*	0.001	−0.003
	(0.007)	(0.010)	(0.006)	(0.009)
P-value for joint test:				
All Other-Grade Coeffs = 0		0.062		0.427
All Frac 55+ Coeffs = 0		0.028		0.442
Observations	2,039	2,039	2,039	2,039
Note:		*p<0.1; **p<0.05; ***p<0.01		

Note: This table shows the relationship between growth in school-grade-level value-added between 2011 and 2012 and the fraction of teachers of retirement age (55+) in the reference grade and surrounding grades in 2011. The dependent variable is the change in school-grade-level value-added in math or reading between 2011 and 2012. The regressors are respectively the fraction of teachers retiring in 2011 in the grade for which value-added is measured, as well as the fraction retiring in the grades in the school 1, 2, and 3 grades below that for which VA is measured. The regression sample includes all 3rd through 5th school-grade-levels for which I am able to construct value-added and for which the school also serves the school-grade-levels 1 to 3 grades below. All regressions are weighted by the number of students taking the WKCE examination in the grade for which value-added is calculated. All standard errors are clustered at the school level.

Table 11: Changes in VA and Schoolwide and Grade-Specific Retirement Rates

	Change in VA, 2011-2			
	Math		Reading	
	(1)	(2)	(3)	(4)
Frac Retire in 2011, Own-Grade	0.073***	0.049*	0.046**	0.024
	(0.027)	(0.028)	(0.022)	(0.025)
Frac Retire in 2011, School		0.105		0.099
		(0.073)		(0.066)
Constant	−0.002	−0.008	−0.001	−0.006
	(0.006)	(0.007)	(0.005)	(0.006)
Observations	2,250	2,250	2,250	2,250
Note:	*p<0.1; **p<0.05; ***p<0.01			

Note: Each column reports coefficients and standard errors from a separate OLS regression. The outcome variable is the change in school-grade-level value-added in math or reading between 2011 and 2012. The explanatory variables are respectively the fraction of teachers who retired in 2011 in the school-grade-level for which value-added is measured, and the fraction of elementary school teachers that retired in the school, pooling all grades (including the one for which VA is calculated). All regressions are weighted by the number of students who took the WKCE examination in 2011. All standard errors are clustered at the school level.

Table 12: Growth in VA and Teacher Retirements in Nearby Grades in the Same School in the Pre-Period

	Change in VA			
	Math		Reading	
	(1)	(2)	(3)	(4)
Frac Retire, Own-grade	0.050***	0.050***	0.016	0.016
	(0.019)	(0.019)	(0.016)	(0.016)
Frac Retire, 1 Grade Down		−0.003		0.011
		(0.019)		(0.017)
Frac Retire, 2 Grades Down		0.005		0.009
		(0.018)		(0.016)
Frac Retire, 3 Grades Down		−0.023		−0.015
		(0.019)		(0.017)
Constant	−0.001	−0.001	−0.001	−0.001
	(0.002)	(0.002)	(0.001)	(0.002)
P-value for joint test:				
All Other-Grade Coeffs = 0		0.668		0.685
All Retirement Coeffs = 0		0.059		0.667
Observations	10,464	10,464	10,464	10,464
Note:	*p<0.1; **p<0.05; ***p<0.01			

Note: This table shows results analogous to Table 9 for the pre-period (2006-2010). Instead of regressing value-added growth between 2011 and 2012 on retirement rates in 2011, I regress value-added growth between year t and $t+1$ on retirement in year t, pooling all years prior to the policy change. The unit of observation is the school-grade-level-year. In columns (1)-(2), the outcome variable is the growth in school by grade-level value-added in math from the observation year to the next year; in columns (2)-(4), it is the analogous change in value-added for reading. The regressors are respectively the fraction of teachers retiring in year t in the grade for which value-added is measured, as well as the fraction retiring in the grades in the school 1, 2, and 3 grades below that for which VA is measured. The regression sample includes all 3rd through 5th school-grade-levels for which I am able to construct value-added and for which the school also serves the grade-levels 1 to 3 grades below. All regressions are weighted by the number of students taking the WKCE examination in the grade for which value-added is calculated. All standard errors are clustered at the school level.

Appendix

A1. VARC Value-Added

As discussed in Section III.B, in my primary analysis I use a school-grade-level value-added metric derived from school-grade-year level means on the WKCE exam. In this section, I reproduce the main event-study results from Section V.C using an alternative measure of school-grade-level value-added, constructed by the Wisconsin Value-Added Research Center (VARC) for internal use by DPI.

Recall from Section III.B that the VARC value-added have both advantages and disadvantages relative to the value-added metric used in the primary analysis. The main advantage of the VARC value-added is that it covers a more complete set of school-grade levels than the primary analysis VA (Appendix Table A2). The most severe disadvantage of the VARC value-added measures is that they have had complex shrinkage procedure applied to them which cannot be reversed given the information available to me. It is problematic to use shrunk measures of value-added as an outcome variable since they will typically change less than one-for-one with changes in true effectiveness, and thus we would expect the results to be attenuated towards zero (see Schwartz, 2015 for a discussion). In addition, the control variables used by VARC vary somewhat across years, and for some years VARC reports a value-added metric in the unit of student test scores, whereas in other years they report only a z-score of the school-grade-level value-added.

To address the inconsistencies in units across years, I convert of all of the VARC value-added estimates to have mean 0 and standard deviation 1 within each grade in each year. To some extent, this also addresses the issue of shrinkage, since if the same shrinkage factor were applied to the value-added for all school-grade levels, then the z-scores of the shrunk value-added would be equivalent to the z-scores of the unshrunk value-added. However, this procedure does not fully reverse the shrinkage procedure since in reality some school-grade levels are larger than others or have noisier test scores, and thus the shrinkage factors will not be the same across school-grade levels.

In order to make the results for the VARC sample as comparable as possible to the primary analysis value-added measure, I also convert the value-added metric used in the primary analysis (which is in z-scores of the *student* test score distribution) to have mean 0 and standard deviation 1 within each grade in each year. I then re-run my main analysis using i) the primary analysis value-added standardized as above, ii) the standardized VARC value-added metric on the full sample of third to fifth grade school-grade units, and iii) the standardized VARC value-added metric on the sub-sample of third to fifth grade school-grade units used in the primary analysis. This allows us to assess how sensitive the results are to both changes in the value-added metric as well as the sample used.

Figures A1 to A4 respectively show the OLS and IV event study results for math, and the OLS and IV event study results for reading for each of the three specifications discussed in the preceding paragraph. For math, both the OLS and IV results are quantitatively and qualitatively quite similar across specifications. The OLS results for reading also follow similar patterns, although when using the full VARC sample the coefficient for 2012 is somewhat smaller and no longer statistically significant. When using the IV strategy for reading, the coefficient for 2012 flips sign when using the VARC VA, although in none of the specifications is the coefficient significant. On the whole, the event study results are fairly similar across specifications, particularly in math, where the results are stronger.

As mentioned in Section V.E, however, the results of the placebo tests using teachers near retirement age are less consistent across specifications (Table A3). In particular, while the coefficient for the placebo group is not significant and smaller than that for retirees in the primary analysis, in a number of cases the placebo coefficient is significant when using the VARC value-added. The results for the VARC sample thus raise some concern that the observed increases in value-added are driven by other factors correlated with the pre-existing age distribution.

A2. Appendix Tables and Figures

Table A1: Naive Separation Elasticities

(a) Teachers Below Age 55

	Cumulative Percent Change In:		
Year	Compensation	Turnover	Implied Elasticity
2011	-7.097	15.582	-2.195
2012	-8.650	21.923	-2.535
2013	-10.509	25.395	-2.416
2014	-12.174	52.304	-4.296
2015	-11.638	34.603	-2.973

(b) Teachers Age 55+

	Cumulative Percent Change In:		
Year	Compensation	Turnover	Implied Elasticity
2011	-9.212	108.310	-11.757
2012	-9.911	32.213	-3.250
2013	-11.610	43.821	-3.774
2014	-12.697	20.982	-1.653
2015	-11.781	28.105	-2.386

Note: This table shows the calculations for the naive elasticity estimates discussed in Section IV. Each row of the table shows the percentage change in real compensation and turnover t years after the reform was passed, relative to the year before the reform. Thus, for instance, the first row of panel a) shows the change in compensation in the 2011-2012 school year relative to the year before, as well as the change in turnover in the summer of 2011 relative to the previous summer. These two numbers are divided to compute the implied naive separation elasticity. The top panel shows results for teachers below the minimum retirement age of 55, whereas the bottom shows the results for teachers 55 and older.

Table A2: Fraction of Schools Covered By Each Value-Added Measure

Grade	Primary Analysis VA	VARC VA
3	0.914	0.997
4	0.846	0.998
5	0.329	0.993

Note: This table shows the fraction of schools for which each value-added metric is available in 2011, by grade. As discussed in Section III.B, the primary analysis VA measure requires matching scores across adjacent grades within a school. This results in a larger share of missing values for grade 5, since many schools end in 5th grade.

Table A3: Comparison of Results Using Different VA Measures - Placebo Tests Using Teachers Age 45 to 54

(a) Primary Analysis VA and Sample

	Change in VA, 2011-2			
	Math		Reading	
	(1)	(2)	(3)	(4)
Fraction 55+ in 2011	0.198**	0.229***	0.096	0.136
	(0.082)	(0.088)	(0.087)	(0.095)
Fraction 45-54 in 2011		0.096		0.121
		(0.080)		(0.084)
Constant	−0.048	−0.082*	−0.056	−0.099**
	(0.031)	(0.044)	(0.034)	(0.049)
P-value for equality		0.16		0.882
Observations	2,252	2,252	2,252	2,252
Note:				*p<0.1; **p<0.05; ***p<0.01

(b) VARC VA, Full Sample

	Change in VA, 2011-2			
	Math		Reading	
	(1)	(2)	(3)	(4)
Fraction 55+ in 2011	0.127*	0.156**	−0.094	−0.034
	(0.073)	(0.078)	(0.081)	(0.088)
Fraction 45-54 in 2011		0.091		0.186**
		(0.078)		(0.086)
Constant	−0.052*	−0.084**	−0.024	−0.090*
	(0.030)	(0.041)	(0.034)	(0.049)
P-value for equality		0.459		0.018
Observations	3,104	3,104	3,104	3,104
Note:				*p<0.1; **p<0.05; ***p<0.01

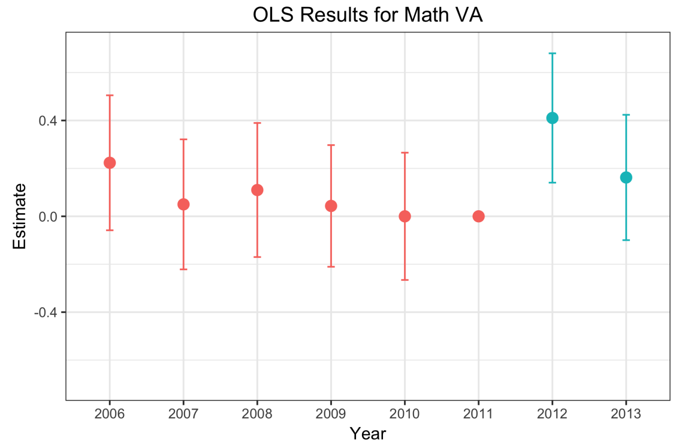
(c) VARC VA, Primary Analysis Sample

	Change in VA, 2011-2			
	Math		Reading	
	(1)	(2)	(3)	(4)
Fraction 55+ in 2011	0.147*	0.200**	−0.067	−0.0004
	(0.085)	(0.091)	(0.097)	(0.106)
Fraction 45-54 in 2011		0.166*		0.205**
		(0.089)		(0.100)
Constant	−0.059*	−0.117**	−0.038	−0.110*
	(0.034)	(0.047)	(0.041)	(0.058)
P-value for equality		0.737		0.06
Observations	2,250	2,250	2,250	2,250
Note:				*p<0.1; **p<0.05; ***p<0.01

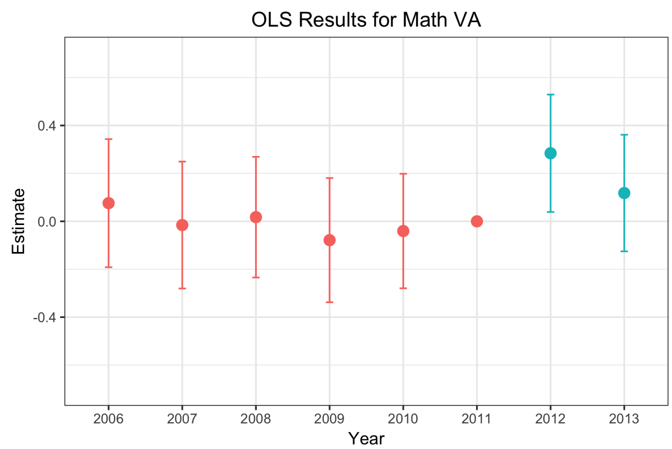
Note: This table compares results of placebo tests using teachers age 45 to 54, similar to Table 5, across different value-added specifications and samples. Panel a) uses the same sample and value-added metric as the main analysis in the text, except VA has been standardized to have mean 0 and standard deviation one within each grade-year. Panel b) uses the standardized VARC VA measure for the full sample in which it is available. Panel c) uses the standardized VARC VA measure for the sample of school-grades used in the primary analysis. See Appendix A.A1 for details on the different value-added measures and samples.

Figure A1: OLS Event Study Results for Math - Comparison Using Primary Analysis and VARC VA

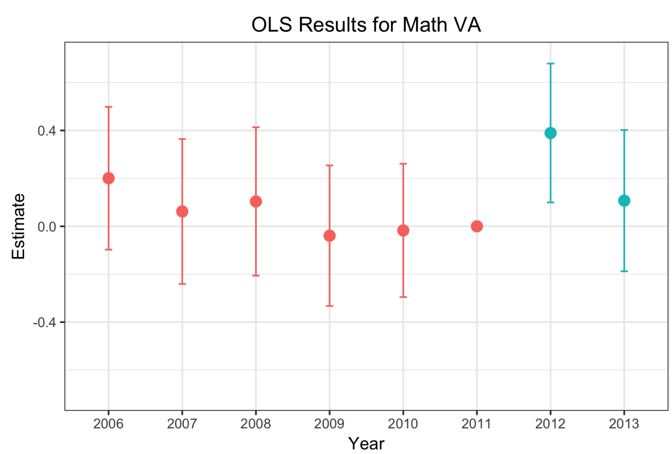
(a) Primary Analysis VA and Sample



(b) VARC VA, Full Sample



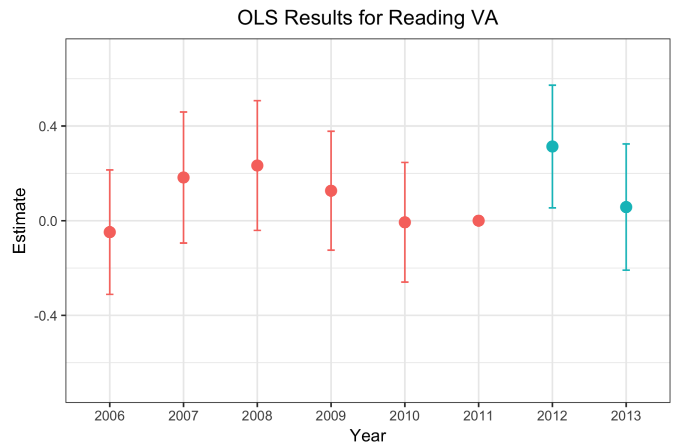
(c) VARC VA, Primary Analysis Sample



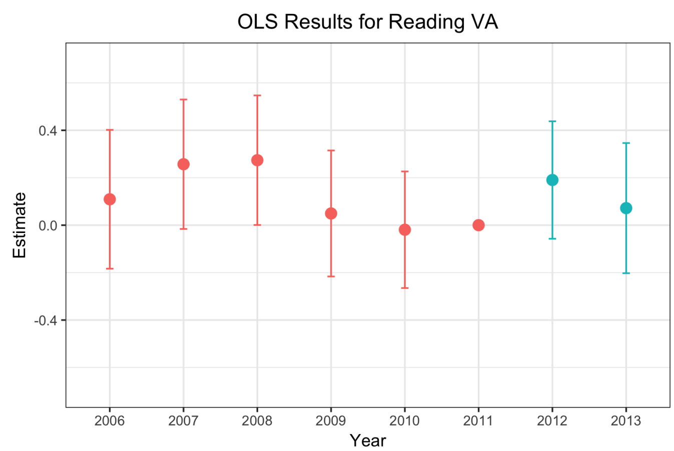
Note: This figure shows OLS event study results for math value-added, analogous to those in Figure 6a. Panel a) uses the same sample and value-added measure as in the text, except the VA measure has been standardized to have mean 0 and standard deviation 1 within each grade-year. Panel b) uses the standardized VARC VA measure for the full sample in which it is available. Panel c) uses the standardized VARC VA measure for the sample of school-grades used in the primary analysis. See Appendix A.A1 for details on the different value-added measures and samples.

Figure A2: OLS Event Study Results for Reading - Comparison Using Primary Analysis and VARC VA

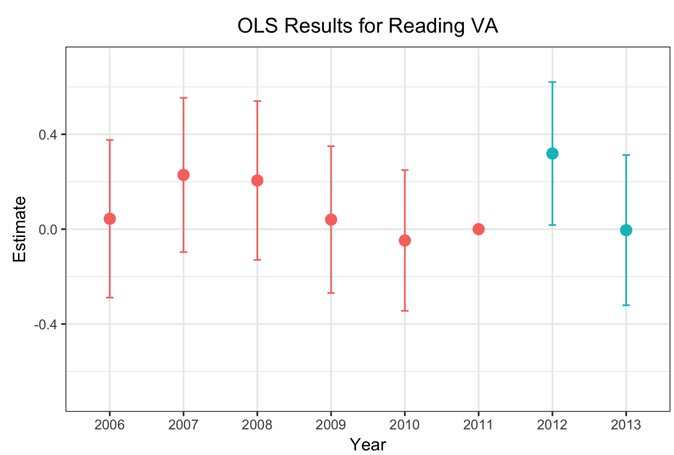
(a) Primary Analysis VA and Sample



(b) VARC VA, Full Sample



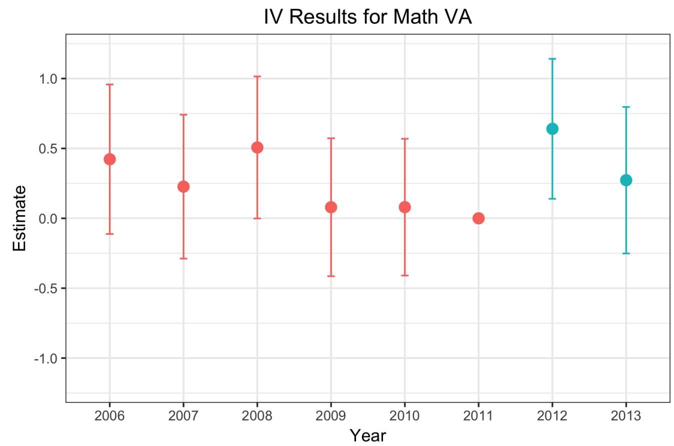
(c) VARC VA, Primary Analysis Sample



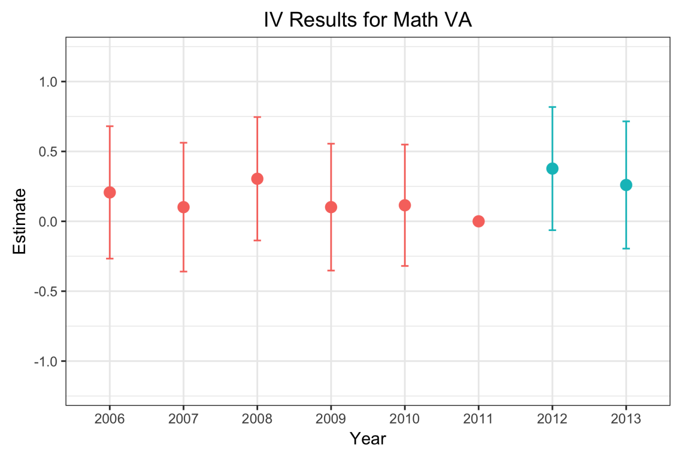
Note: This figure shows OLS event study results for reading value-added, analogous to those in Figure 7a. Panel a) uses the same sample and value-added measure as in the text, except the VA measure has been standardized to have mean 0 and standard deviation 1 within each grade-year. Panel b) uses the standardized VARC VA measure for the full sample in which it is available. Panel c) uses the standardized VARC VA measure for the sample of school-grades used in the primary analysis. See Appendix A.A1 for details on the different value-added measures and samples.

Figure A3: IV Event Study Results for Math - Comparison Using Primary Analysis and VARC VA

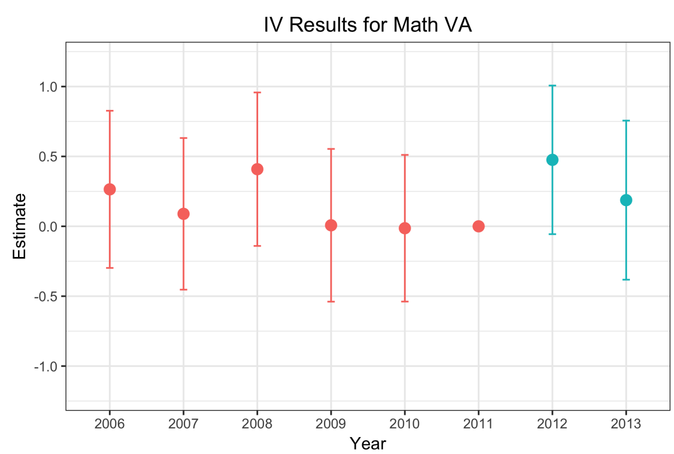
(a) Primary Analysis VA and Sample



(b) VARC VA, Full Sample



(c) VARC VA, Primary Analysis Sample

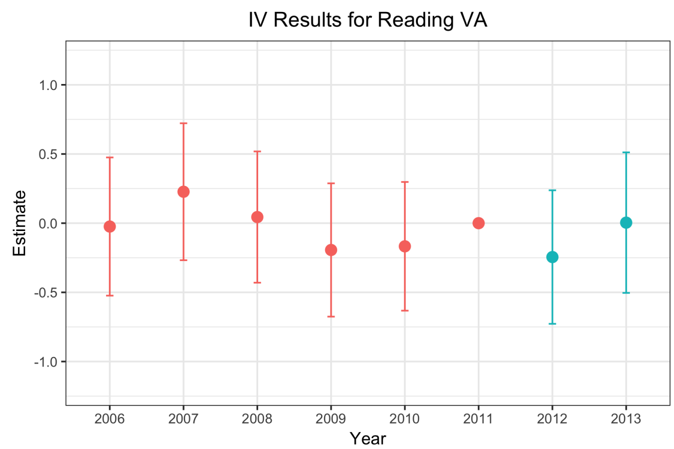


Note: This figure shows IV event study results for math value-added, analogous to those in Figure 6b. Panel a) uses the same sample and value-added measure as in the text, except the VA measure has been standardized to have mean 0 and standard deviation 1 within each grade-year. Panel b) uses the standardized VARC VA measure for the full sample in which it is available. Panel c) uses the standardized VARC VA measure for the sample of school-grades used in the primary analysis. See Appendix A.A1 for details on the different value-added measures and samples.

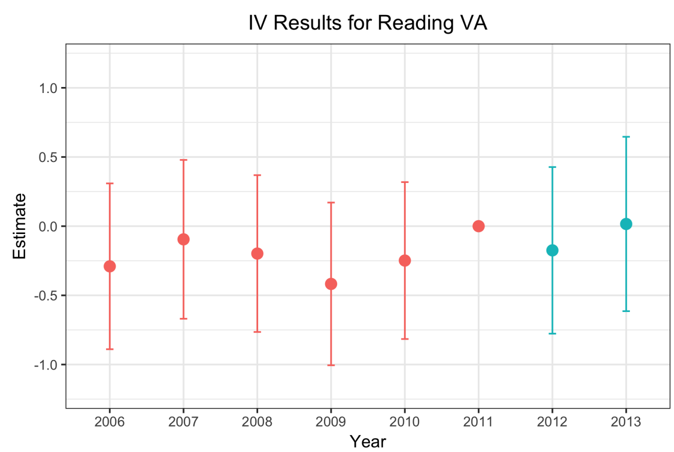
Figure A4: IV Event Study Results for Reading - Comparison Using Primary Analysis and VARC VA

(a) Primary Analysis VA and Sample

(b) VARC VA, Full Sample

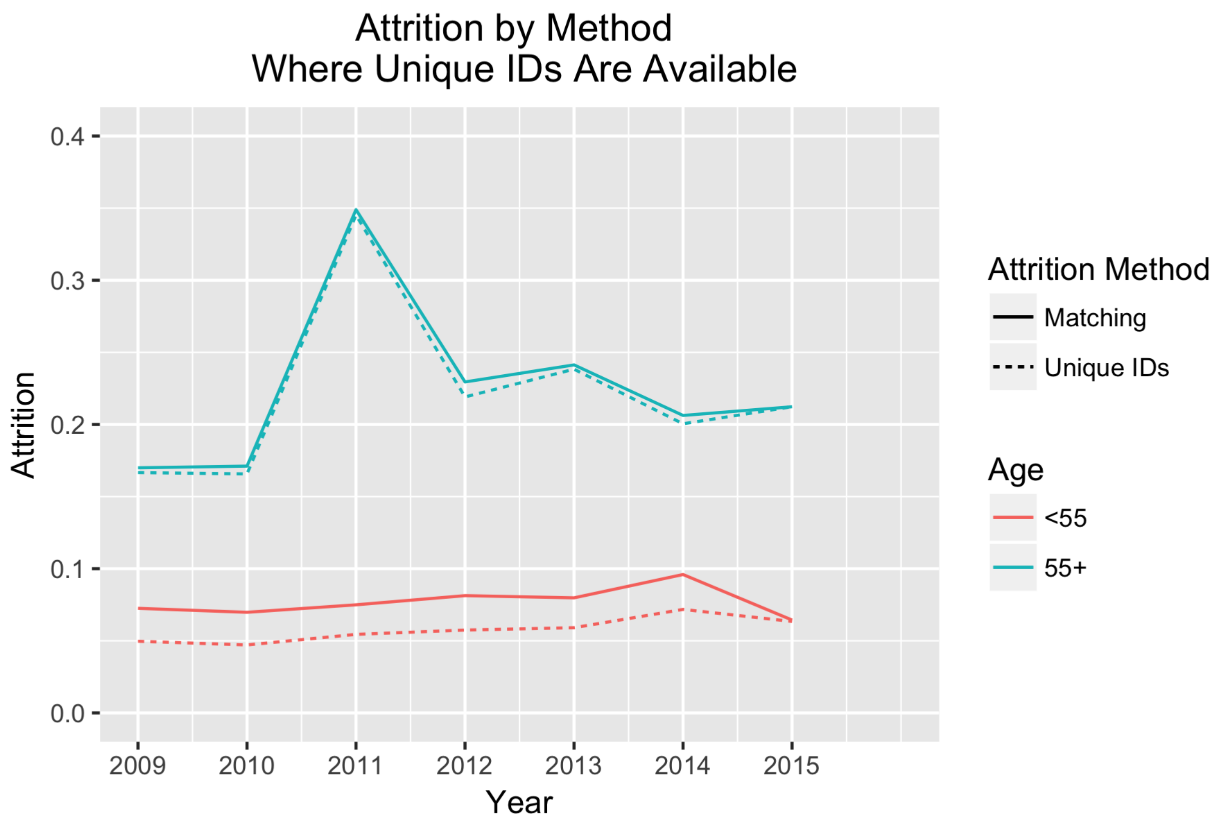


(c) VARC VA, Primary Analysis Sample



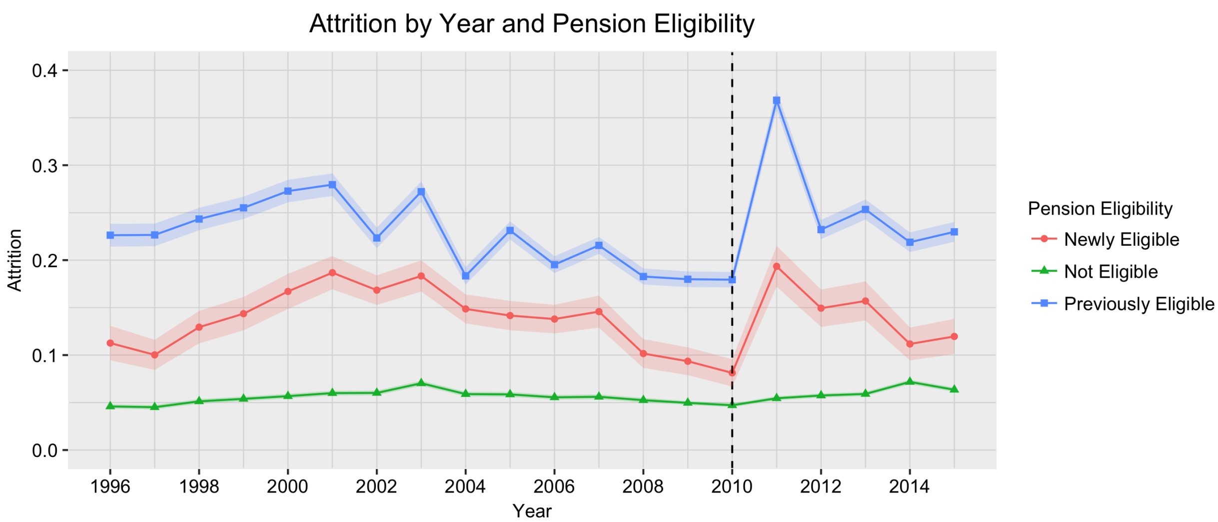
Note: This figure shows IV event study results for reading value-added, analogous to those in Figure 7b. Panel a) uses the same sample and value-added measure as in the text, except the VA measure has been standardized to have mean 0 and standard deviation 1 within each grade-year. Panel b) uses the standardized VARC VA measure for the full sample in which it is available. Panel c) uses the standardized VARC VA measure for the sample of school-grades used in the primary analysis. See Appendix A.A1 for details on the different value-added measures and samples.

Figure A5



Note: This figure compares attrition rates calculated using unique identifiers with those calculated using a matching procedure on name and year of birth, for the years where unique identifiers are available. In figures that show the time series of attrition for a particular population (e.g. Figure 3), I use the unique IDs to calculate turnover for years where they are available. For earlier years, I adjust the turnover rate using the matching procedure by the average measurement error between the two methods for the relevant population between 2009 and 2014. In 2015, DPI implemented a new data system with better standardization of names, effectively eliminating the measurement error in the matching procedure; I thus exclude 2015 from the calculation of the measurement error correction for earlier years.

Figure A6



Note: This figure shows attrition rates by year and the corresponding 95% confidence intervals for regular education teachers in the Wisconsin public school system. Teachers are classified as newly eligible for a pension if they are age 55, the minimum retirement age, in the relevant year; they are characterized as previously eligible if they are above age 55, and ineligible if they are below 55. Attrition rates are calculated using the All Staff files from the Wisconsin Department of Public Instruction. A teacher is said to have attrited between year t and year $t+1$ if they were a regular education teacher anywhere in the Wisconsin public schools in year t and were not a regular education teacher anywhere in the Wisconsin public schools in year $t+1$. See Section III.A for additional details on how attrition is calculated. The attrition rate plotted for a given year represents the fraction of teachers who attrited following the school year ending with that year – for instance, the attrition rate for 2007 represents the fraction of teachers in the 2006-2007 school year who did not teach in 2007-2008.