Flexible Wages, Bargaining, and the Gender Gap*

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May 9, 2021

Abstract

Does flexible pay increase the gender wage gap? To answer this question we analyze the wages of public school teachers in Wisconsin, where a 2011 reform allowed school districts to set teachers' pay more flexibly and engage in individual negotiations. Using quasi-exogenous variation in the timing of the introduction of flexible pay, driven by the expiration of pre-existing collective-bargaining agreements, we show that flexible pay lowered the salaries of women compared with men with the same credentials. This gap is larger for younger teachers and smaller for teachers working under a female principal or superintendent. Survey evidence suggests that the gap is partly driven by women engaging less frequently in negotiations over pay, especially when the counterpart is a man. This gap is not driven by gender differences in job mobility, ability, or a higher demand for male teachers. We conclude that environmental factors are an important determinant of the gender wage gap in contexts where workers are required to negotiate and that institutions, such as unions, might help to narrow the gender wage gap.

JEL Classification: J31, J71, J45

Keywords: Gender wage gap, Flexible pay, Teacher salaries, Bargaining

*We thank Jaime Arellano-Bover, Marianne Bertrand, Kirill Borusyak, Eric Budish, Brantly Callaway, Judy Chevalier, Nicole Fortin, Rob Jensen, Matt Notowidigdo, Jessica Pan, Ben Polak, Fiona Scott Morton, Pedro Sant'Anna, Isaac Sorkin, Liyang Sun, and participants at various seminars and conferences for useful comments. Rohan Angadi, Calvin Jahnke, Nidhaan Jain, Kate Kushner, and Hayden Parsley provided excellent research assistance.

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1 Introduction

Women are often believed to be reluctant to negotiate for higher pay. This could give a workplace advantage to men and exacerbate gender gaps in pay (Sandberg, 2013). Evidence from lab experiments generally supports this hypothesis, showing that women avoid situations in which they have to negotiate or bargain (Babcock and Laschever, 2003; Dittrich et al., 2014; Exley et al., 2019). Whether differences found in the lab translate to non-experimental settings, however, has been difficult to study because workers can sort into jobs based on whether negotiating is required.[1] Yet, as individually based compensation becomes more prevalent even in traditionally unionized sectors, due to the passage of right-to-work laws, understanding whether flexible pay penalizes women is key to understanding the sources of the gender wage gap.

In this paper we use the passage of Wisconsin's Act 10, a state bill that dramatically redefined the rules of collective bargaining for public sector employees, to test whether and how the introduction of flexible pay affects the gender wage gap. We focus our analysis on public school teachers, a class of workers whose pay before Act 10 was strictly based on seniority and academic credentials, using rigid schedules that school districts negotiated with teachers' unions. After Act 10, unions lost the authority to bargain over these schedules. Instead, upon the expiration of pre-existing collective bargaining agreements (CBAs), districts became free to adjust teacher pay on an individual basis and without union consent (Baron, 2018; Litten, 2016). Some districts opted to switch to entirely flexible pay, while others opted to keep a salary schedule; even in these districts, however, teachers could negotiate their place on the schedule.

We use variation in the timing of CBA expirations, due to long-standing differences in districts' negotiation calendars (as in Baron, 2018; Litten, 2016), which pre-dated Act 10, to estimate the effect of the introduction of flexible pay on the gender pay gap for teachers. While no gender pay gap existed before Act 10, the introduction of flexible pay led to a 0.8 percent decline in women's salaries relative to their male counterparts. Although small in absolute terms, this gap corresponds to 1.2 times the pre-Act 10 increase in pay associated with one additional year of seniority and 8 percent of the increase associated with obtaining a Master's degree. The gap is

[1] For example, Card et al. (2015) find that women are underrepresented in firms with a high bargaining surplus. Studying US real estate transactions, Goldsmith-Pinkham and Shue (ming) find that women pay more for housing properties and sell them for less than men. Using data from Denmark, Andersen et al. (2020) confirm that a gender gap in real estate negotiation outcomes exists; however, they find it is due to differences in the types of property men and women demand. In this paper, we are able to overcome some of the obstacles of measuring gender differences in negotiations by holding constant the employer-employee match (Wisconsin public schools) and testing for differences in outside options.

also 1.6 times the post-Act 10 difference in pay associated with a one-standard deviation higher value-added (Biasi, 2021).

Our estimates of the gender wage gap are robust to controlling for teacher characteristics, teaching assignment (school, grade, and subject), as well as district and time effects. In addition, they are robust to accounting for the possibility of heterogeneous treatment effects across units (Sun and Abraham, 2020), changes in the composition of the teaching body across districts, and endogenous assignment to the treatment (driven by teachers moving across districts).

Our aggregate estimates of the gender wage gap mask heterogeneity across teachers, schools, and districts. Flexible pay appears to penalize younger and less experienced teachers more than older teachers and those with more seniority. Larger estimates for young teachers imply that, if the gap persists over time, women would lose an entire year's pay over the course of a 35-year career relative to men.

The gender wage gap is also related to the gender composition of schools' and districts' leadership. In schools with a male principal the gap is 0.4 percent, whereas it is zero in schools with a female principal. Similarly, the gap is 0.5 percent in districts with a male superintendent and indistinguishable from zero in districts with a female superintendent. These findings are in line with recent evidence on the link between the gender composition of management and women's careers (Casarico and Lattanzio, 2019; Langan, 2019; Cullen and Perez-Truglia, 2020).

The emergence of a gender wage gap following the introduction of flexible pay suggests that gender differences in teachers' willingness to bargain or their bargaining ability could be driving part of the observed pay gap. In an attempt to test this mechanism, we ran a survey with all current Wisconsin public school teachers. We asked respondents whether they have ever negotiated their pay or plan to do so in the future. We then asked teachers who opted out of bargaining why they chose to do so; to those who did bargain, we asked whether they believed the negotiation was successful.

Survey responses indicate that women are between 12 and 23 percent less likely than men to have negotiated their pay at various points in their careers and 13 percent less likely to anticipate negotiating in the future. These estimates suggest that the observed gender differences in the propensity to bargain might be an important determinant of the gender wage gap. The magnitude of the estimates is significant: An 8 percentage point difference in the likelihood of negotiating, combined with an aggregate wage gap of one percent, suggests that differences in bargaining could lead to a wage gap as large as 12 percent.

In line with our earlier results, we also find that gender differences in negotiating behavior are entirely driven by men being more likely to bargain under a male superintendent, whereas men and women who work under a female superintendent are equally likely to negotiate their salaries. When asked why they did not negotiate, women are 31 percent more likely than men to report that they do not feel comfortable negotiating pay. Differences in the perceived returns to bargaining and beliefs about one's teaching ability do not explain our findings (Biasi and Sarsons, ming).

One limitation of our setting is the inability to link survey answers to administrative records, which prevents us from exactly estimating the portion of the post-Act 10 wage gap attributable to differences in bargaining. To make progress, we test three other possible determinants of the gap. First, we study whether the gap is explained by gender differences in teaching quality, as districts may have used flexibility to pay better teachers more. Our data do not support this hypothesis: Women's value added is slightly higher than men's, and controlling for value-added does not affect our estimate of the gender pay gap. Furthermore, the returns to a high value-added are positive after the introduction of flexible pay for men, but not for women. This suggests that women are not rewarded for their teaching ability at the same rate as men.[2]

A second explanation relates to differences in job mobility and the returns to moving.[3] If women are less likely than men to move, they might be unable to increase their pay by moving to a different school or district, or by garnering outside offers. We find that women are as likely as men to move, and differences in mobility cannot explain the total pay gap. Suggestive evidence indicates that men might be able to use outside job offers to bid up their salary at their current school, which points to bargaining as a primary channel driving the observed gender wage gap, but provides an alternative reason for why women might be less likely to bargain.

Finally, the gap could be driven by a higher demand for male teachers. To explore this hypothesis, we identify three instances in which this demand might be higher: (i) schools with fewer men, (ii) schools that lost male teachers immediately before Act 10, and (iii) schools enrolling a higher share of male students (where men could serve as role models for boys). In line with the hypothesis, the gap is larger in schools that lost more men or enroll more male students. Both of these variables, however, only explain a very small portion of the total gap.

[2]Evidence from three performance-pay programs for teachers in North Carolina shows that teachers' value-added declined for women with the introduction of performance pay, while it remained flat for men (Hill and Jones, 2020). In our data, we do not find any evidence of differential selection and retention of high- and low-value-added teachers by gender.

[3]Biasi (2021) shows that the introduction of flexible pay after Act 10 was followed by an increase in cross-district movements, associated with an increase in pay.

Our results indicate that flexible pay, while possibly beneficial to incentivize workers to exert more effort, can be detrimental for the outcomes of some subgroups of the workforce. Workplace environmental factors likely play a role in the observed disparities in negotiating outcomes between men and women, even in a female-dominated occupation like public school teaching. Our findings also suggest that institutions, such as unions, can mitigate the rise of gender wage gaps by setting rules that govern pay. Importantly, our findings do not necessarily imply that it is suboptimal to pay workers based on productivity. Rather, our results call for more exploration of policies which prevent some workers from taking advantage of performance pay, simply because they are less likely to negotiate.[4]

Our paper contributes to several literatures on gender inequality in the labor force. A mainly experimental literature has shown that women negotiate less than men (Babcock and Laschever, 2003; Leibbrandt and List, 2014; Dittrich et al., 2014) and ask for lower pay (Roussille, 2020).[5] Our paper confirms these findings by showing that a gap emerges when workers are allowed to negotiate their pay, and it sheds light on the mechanisms at play.

Several studies have analyzed the impact of the gender composition of firms' leadership on women's career outcomes, finding mixed results.[6] An advantage of our context is that we are able to look at different types of school leaders (principals and superintendents) who carry out different functions. We find that men gain more than women when they negotiate with a male superintendent. This suggests that female representation in leadership could combat gender inequality in the workplace (Matsa and Miller, 2011; Athey et al., 2000; Langan, 2019).

Our paper also relates to the literature on the effects of changes in pay schemes on a variety of outcomes. Most of this literature has studied the effects of various forms of performance pay on employees' selection and effort (for example Lazear, 2000a,b; Bandiera et al., 2005; Neal et al., 2011). We study gender gaps in wages as a possibly unintended consequence of a pay scheme that, while designed to allow employers to pay workers for performance, also rewards behaviors and actions (such as negotiating) women might be less likely to engage in. We do so by building on recent works on the impact of Wisconsin's Act 10 on teachers and students, such

[4]We show that high value-added women do not benefit from performance pay, suggesting that the differences in pay are not due to differences in productivity. Rather, they appear due to differences in one's ability or willingness to negotiate.

[5]Exley et al. (2019) also find that women correctly select into bargaining, suggesting that forcing women to bargain could result in suboptimal outcomes.

[6]Studies of the effects of gender quotas for firm boards have not found any positive impact on women in other parts of the organization (Bertrand et al., 2019; Maida and Weber, 2020). Other works have instead unveiled a positive impact of having a female non-board manager on women's careers (Sato and Ando, 2017; Casarico and Lattanzio, 2019; Bhide, 2019; Langan, 2019).

as Litten (2016), Baron (2018), Roth (2019), Biasi (2021), and Biasi et al. (2021).[7]

Lastly, our results speak to the literature on the relationship between unionization and collective bargaining and the gender pay gap.[8] Existing studies have found that countries with lower unionization rates (such as the US) have larger gender wage gaps (Blau and Kahn, 1992, 1996). This points to a relationship between the larger decline in unionization for men relative to women in the US and a decline of the gender gap (Even and Macpherson, 1993).[9] However, these studies are unable to fully control for worker sorting and productivity and lack a proper control group, which prevents them from establishing a causal link.[10] Following teachers over several years allows us to account for sorting and differences in teacher ability and to estimate the impact of de-unionization on the gender pay gap.

The remainder of the paper is organized as follows. Section 2 discusses the history of teacher pay in Wisconsin and how Act 10 affected teacher salary rules. We describe the data used in our analysis in section 3 and show the our main findings on the gender wage gap in section 4. Section 5 describes our survey and its results. We explore alternative mechanisms for the gender wage gap in Section 6. Section 7 concludes.

2 Institutional Background: Teacher Pay, Collective Bargaining, and Act 10

2.1 Teacher Pay and Collective Bargaining

Salaries of US public school teachers are generally determined using a salary schedule, which specifies each employee's pay based on their seniority and academic credentials. A schedule is designed as a matrix: Increases in pay arise from movements along its rows or "steps," which correspond to increases in seniority, and columns or "lanes," which correspond to the attainment of credentials such as a Master's degree or a PhD.

[7] Litten (2016) and Biasi (2021) study the effects on Act 10 on wages. Baron (2018) studies the impact of CBA expirations on students' outcomes. Roth (2019) and Biasi (2019) explore retirement effects. Lastly, Biasi (2021) and Biasi et al. (2021) study the impacts of the reform on teacher sorting.

[8] A large literature has documented a negative relationship between unionization and income inequality in the US (Card, 1996; Dinardo et al., 1996; Farber et al., 2018). Fortin and Lemieux (1997) argue that deunionization impacted pay inequality among men but that the minimum wage was more important for women's pay. See also Card et al. (2020) for a comparison of Canada and the U.S.

[9] Analyzing union wage premia by demographic groups, Wunnava and Peled (1999) also find that union membership could explain part of the observed gender wage gap.

[10] Controlling for variables like sorting is especially important given the recent work by Farber et al. (2018) that shows that sorting into unionized jobs has varied substantially over time. In this paper, we make use of the fact that Act 10 was relatively unanticipated to look at the impact on individuals who have already sorted into teaching. In addition, we can track individuals who leave teaching following Act 10.

In states where teachers are authorized to collectively bargain, these schedules are negotiated between school districts and the teachers' unions. CBAs typically do not allow for individual pay adjustments, implying that seniority and credentials (along with "overtime" or extra-curricular activities) are the only determinants of salaries, and that pay is not directly related to teacher effectiveness (Podgursky, 2006).

However, the past two decades have seen a decline of collective bargaining and union membership for public school teachers. Data from the Current Population Survey (CPS) show that union membership declined from 67 percent in 2005 to 61 percent in 2019 for public school teachers. At the same time, the gender wage gap for teachers increased from 9 to 11 percent (Figure I, panel (a)).

This pattern is not unique to public school teachers. Panel (b) of Figure I shows a positive relationship between union membership and the male-female gender wage gap, calculated separately for each industry-occupation-sector-state-year cell, for public school teachers (red squares) and for all other employees (blue circles).[11] While the overall gender gap declined in absolute terms during this time period (from 14 to 12 percent), it did so significantly more slowly in occupations, industries, sectors, and states that also experienced a decline in union membership (Appendix Figure AI, panel (a)) relative to those that experienced an increase (Appendix Figure AI, panel (b)).

While suggestive of a link between collective bargaining and the gender wage gap, these findings are not sufficient to establish a causal relationship between the two. In the remainder of the paper, we use the passage of Act 10 in Wisconsin to estimate the causal effect of a specific provision of the end of collective bargaining – flexible pay– on the gender pay gap.

2.2 Wisconsin's Act 10

Similar to other states, salaries of all Wisconsin public school teachers were, until 2011, determined using a schedule negotiated between districts and unions, and represent a key part of each district's CBA.[12]

The rules disciplining teacher pay dramatically changed on June 29, 2011. In an attempt to close a projected $3.6 billion budget deficit, the state legislature passed the Wisconsin Budget Repair Bill (Act 10). This bill introduced a series of changes to the powers and duties of all

[11] We estimate the gender gap controlling for a cubic polynomial in age and an indicator for having a college degree.

[12] In 1959, Wisconsin became the first state to introduce CB for public sector employees (Moe, 2013). Since then, teachers' unions have gained considerable power and have been involved in negotiations with school districts over key aspects of a teaching job.

public sector unions, including teachers' unions. First and most importantly, Act 10 limits the scope of collective bargaining: While before this law change unions could negotiate the entire salary schedule, after Act 10 negotiations were limited to base salaries. Second, Act 10 requires unions to recertify every year by obtaining the absolute majority of all members' votes. Third, it limits the validity of newly stipulated CBAs to one year. Lastly, it prohibits automatic collection of union dues from employees' paychecks.[13]

Act 10 also contained a number of budget-cutting rules for public school districts. First, it capped the the growth in base salaries to the rate of inflation.[14] Second, it required districts to stop paying the employees' share of retirement contributions (amounting to 5.8 of each employee's annual salaries), to increase employees' contributions to health care plans, and to choose cheaper plans in order to reduce premiums. An amendment to Act 10 (Act 32, passed in July 2011) also reduced state aid to school districts and decreased their revenue limit.[15]

Implications For Teacher Pay With the end of collective bargaining, school districts became free to set teachers' pay more flexibly. Until 2011, pay depended exclusively on seniority and academic credentials. After Act 10, districts could reward teachers for other attributes without union consent. Districts used this flexibility in a variety of ways. An analysis of districts' employee handbooks (documents that describe the rights and duties of all district employees in the post-Act 10 era) indicate that, as of 2015, approximately half of all districts were still setting pay using a schedule exclusively based on experience and education, whereas the remaining half had discontinued the use of a schedule (Kimball et al., 2016; Biasi, 2021).[16] Even within these two groups, the specific pay schemes adopted by the districts varied, with some districts linking pay to principal or peer evaluations and others negotiating raises and bonuses with each individual teacher to attract and retain employees. Regardless of how pay was set after Act 10, teachers in all districts could individually negotiate their salaries. For example, even in districts that continued the use of a salary schedule, some teachers were able to increase their pay by negotiating for a higher place on the schedule (Kimball et al., 2016).[17] In sum, individual wage

[13]Union membership dropped by nearly 50 percent in Wisconsin in the 5 years after the passage of Act 10. See D. Belkin and K. Maher, *Wisconsin Unions See Ranks Drop Ahead of Recall Vote*, The Wall Street Journal. Retrieved from https://www.wsj.com/articles/SB10001424052702304821304577436462413999718.

[14]Base wages are the lowest steps of a salary schedule, i.e., the wages of teachers with minimum experience and education.

[15]Revenue limits are the maximum level of revenues a district can raise through state aid and local property taxes.

[16]As a result of these changes, flexible-pay districts started paying high-quality, young teachers more and reduced the growth in pay for some high-seniority teachers (Biasi, 2021).

[17]Many districts explicitly stated in handbooks that placement on the schedule was up to the school district, indicating some flexibility in placement. See Appendix Figure AV, for an example.

negotiations became the common denominator among districts' post-Act 10 pay schemes.

Differences In The Timing of The Introduction of Flexible Pay The provisions of Act 10 had an immediate effect on all school districts starting from the 2011/2012 school year. Existing CBAs stipulated between unions and school districts before 2011, however, remained binding until their expiration. Since pre-Act 10 CBAs fully regulated teacher pay with a salary schedule, districts could begin to use their freedom to flexibly set teacher pay only after the expiration of these CBAs.

Due to differences in electoral cycles, the expiration dates of pre-existing CBAs and their extensions varied across districts (Litten, 2016; Baron, 2018). These differences reflect long-standing misalignments in the negotiation calendars. For example, while most districts typically negotiated agreements bi-yearly on odd years, the school district of Janesville negotiated contracts in March 2008 and September 2010.[18] Off-calendar districts (i.e., those with expiration dates after 2011) include both large, urban districts like Milwaukee and Madison, and smaller, suburban or rural districts like Clintonville and South Milwaukee. On average, these districts are more likely to be located in suburban areas and serve a larger share of Black students (Appendix Table AI, columns 1-3); the latter difference, however, is largely driven by the Milwaukee Public Schools district.

After the passage of Act 10, 100 school districts decided to extend the validity of their CBAs by one or two additional years, primarily to gain more time to design the new pay schemes. These cross-district differences in expiration and extension dates introduce plausibly random variation in the timing of the introduction of flexible pay, which we use in our empirical analysis.

3 Data

Our main data set includes individual-level information on the universe of Wisconsin public school employees. We combine these data with information on the school districts, including the expiration dates of their CBAs and their post-Act 10 salary regimes (i.e., the presence, or lack thereof, of a salary schedule in a district's post-Act 10 handbook). We also link teacher records with students' demographic characteristics and test scores in Math and Reading, which we use to calculate teacher value-added. Data are reported by school year and referenced using the

[18] See https://www.schoolinfosystem.org and https://www.tmcnet.com.

calendar year of the spring semester (e.g. 2007 for 2006-07).

Personnel Data We draw information on the population of Wisconsin teachers, district superintendents, and school principals from the *PI-1202 Fall Staff Report - All Staff Files* of the Wisconsin Department of Public Instruction (WDPI) for the years 2006-2016. These files contain individual-level records of all individuals employed by the WDPI in each year and include personal and demographic information, highest level of education, years of teaching experience in Wisconsin, and characteristics of job assignments (school identifiers, grades and subject taught, and full-time equivalency, or FTE, units).[19] The data set also includes total salaries for each worker. We restrict our teacher sample to non-substitute teachers and assign those employed in multiple districts and schools in a given year to the district-school with the highest FTE.[20] We express salaries in FTE units, so that the salary of each teacher corresponds to a full-time position regardless of her actual hours.[21] The characteristics of male and female teachers are summarized in Table I, separately for the years preceding and following Act 10.

Pre-Act 10 CBAs We collected information on districts' CBAs from multiple sources, including union contracts, districts' employee handbooks, school board meetings minutes, and local news sources. Meeting minutes describe whether the contract was set to expire in 2011, whether an extension was granted, and for how long. Employee handbooks allow us to establish when the post-CBA pay regime was introduced. We prioritize data from union contracts, school board minutes, and handbooks. When these are unavailable, we use information from online local news sources. These websites often reported on the negotiations taking place, offering enough information to discern when the CBA was slated to expire and, in some cases, mentioning an extension to this deadline.

Our data sources are listed in greater detail in Appendix Table EI. We were able to successfully find information on the expiration dates for 247 out of 428 school districts, employing 83 percent of all teachers. For 225 of these 247 districts, employing 80 percent of teachers, we also have information on the presence or absence of an extension. We exclude districts with missing expiration dates from our analysis and we assume that districts with an expiration date but no

[19] FTE equals 100 for a person employed full-time.

[20] We exclude long- and short-term substitute teachers, teaching assistants and other support staff, and contracted employees since salaries for these workers are calculated differently from those of permanent teachers. We were notified by the WDPI of mistakes in salary reporting for teachers in the district of Kenosha for all years and in Milwaukee for 2015. We therefore set these observations to missing.

[21] In robustness checks we restrict our attention to full-time teachers; the main estimates are largely unchanged (Table V, column 6).

information on an extension had no extension.[22]

Employee Handbooks and Salary Schedules To better understand how districts used their flexibility in setting teacher pay after a CBA expiration, we gathered information on post-Act 10 pay schemes from employee handbooks, available on districts' websites for 224 out of 428 districts for the year 2015 (in total, these districts employ 80 percent of all teachers).[23] We classify a district as a "schedule district" if its 2015 handbook contains a salary schedule and does not mention rewards for performance or merit, and as "non-schedule district" otherwise. If a handbook contains a schedule and mentions bonuses linked to performance, we classify the district as non-schedule.

Student Test Scores and Demographic Information Test-score data are available for for all students in grades 3 to 8 and for the years 2006-2017. They include math and reading scores from the Wisconsin Knowledge and Concepts Examination (WKCE, 2007-2014) and the Badger test (2015-2016), together with demographic information such as gender, race and ethnicity, socioeconomic status (SES), migration status, English-learner status, and disability.[24] We use test-score data to calculate teacher value-added.

3.1 Value-Added

We measure teachers' quality using value-added (VA), an estimate of a teacher's contribution to the growth in student achievement. We follow the canonical VA model of Kane and Staiger (2008) and estimate VA as the teacher-specific component of a standard achievement model using an empirical Bayes estimator.[25]

VA is usually estimated using datasets where teachers can be linked to the students they taught. The absence of classroom identifiers in the WDPI data implies that we can only link a

[22] Our results are robust to including districts with missing expiration dates and assigning them a 2011 expiration, as well as to excluding districts that have an expiration date but no extension date (see Appendix Figure AIII).

[23] Unclassified districts (i.e., those for which handbooks are not available) either do not have a website or do not make their handbook public. Biasi (2021) shows that districts without a handbook are smaller, enroll more disadvantaged students, pay lower salaries, and are disproportionately located in rural areas.

[24] The WKCE was administered in November of each school year, whereas the Badger test was administered in the spring. For this reason, for the years 2007-2014 we assign each student a score equal to the average of the standardized scores for the current and the following year.

[25] Our achievement model controls for the following determinants of achievement: school and grade-by-year fixed effects; cubic polynomials of past scores interacted with grade fixed effects; cubic polynomials of grade average past scores, interacted with grade fixed effects; student k's demographic characteristics (gender, race and ethnicity, disability, English-language earner status, and socioeconomic status); grade average demographic characteristics; and the student's socioeconomic status interacted with the share of low-socioeconomic status students in her grade and school in t.

teacher to students in her school and grade. To account for this data limitation, we follow Biasi (2021) and estimate a modified version of the estimator of Kane and Staiger (2008), adapted to reflect the structure of the data. This measure exploits teacher turnover across grades and schools (rather than classrooms) over time (as in Rivkin et al., 2005).[26] We allow a teacher's VA to differ before and after Act 10, to account for changes in effort in response to the reform, and we standardize it to have mean zero and variance equal to one. VA estimates are available for 23,581 teachers of Math and Reading in grades 4 to 8.

4 The Effect of Flexible Pay On The Gender Wage Gap

In the section, we begin by describing our identifying variation. We then analyze the impact of flexible pay on the gender gap in teachers' salaries. Finally, we explore heterogeneity in the gap based on teachers' age and seniority, districts' salary structures, and the gender composition of schools' and districts' leadership.

Identifying Variation As explained in Section 2, once Act 10 took effect, districts could only start to use flexible pay after their current CBAs expired (Litten, 2016; Baron, 2018). In addition, 100 districts extended the validity of their agreements by one or two years (Figure II). While the timing of expiration of the CBAs can be considered as good as random, the enactment of an extension was a deliberate choice of each district. Districts with an extension tend to be larger, located in urban and suburban areas, and have lower revenues (Appendix Table AI, columns 4-6).

In our analysis, we make use of variation in the timing of the introduction of flexible pay driven by the expiration of both the CBAs and their extensions. Although only the former can be considered random, our strategy still allows us to estimate the effects of flexible pay on the gender wage gap if the reasons that induced school districts to opt for an extension are unrelated to the differences in salaries between men and women. Our estimates are robust to ignoring extensions and instead only using variation from CBA expirations, as well as to using the timing of CBA expirations as an instrument for CBA extensions.

[26] With multiple years of data, turnover permits the identification of a single teacher's effect by comparing test score residuals $\bar{\nu}_{gst}$ of a grade g in school s and year t before and after her arrival in that grade and school. Turnover helps identify not only the effect of a teacher who switches, but also that of all teachers in her same grade and school at any point in time. In Appendix B we discuss this estimator in detail and show that, although noisier than the canonical estimator, it explains a substantial portion of the variance in test scores and is a forecast-unbiased estimator of standard VA estimates and future student achievement.

4.1 Evolution of Salaries for Men and Women Over Time

Before Act 10, teacher salaries were determined by attributes such as experience, academic credentials, and teaching assignment (i.e., grade and subject) and followed a strict pay schedule. On average, prior to Act 10 women earned 0.8 percent less than men (Appendix Table AII, panel A, column 1). This gap, however, can be entirely explained by observable differences and disappears when we control for experience, credentials, and teaching assignment (column 5).[27]

Following the expiration of the CBAs, districts acquired the freedom to pay different salaries to teachers with the same experience, credentials, and teaching assignment. Panel (a) of Figure III shows how men and women's salaries evolved after Act 10. Their salaries followed a similar trajectory until a CBA expiration; after this point a gender pay gap emerges and grows over time.[28]

This raw difference in pay, however, could be driven by observable differences between male and female teachers. To estimate the change in salaries of observationally similar men and women after the expiration of CBAs or their extensions, we begin with an event study of men's and women's conditional salaries. We first obtain residuals ($\widehat{\omega_{it}}$) from the following regression, estimated by pooling together data on men and women:

$$\begin{aligned}\ln(w_{it}) \quad = \quad & \beta_1' X_{it} + \beta_2' X_{it} \times postext_{j(it)t} + \gamma_1' T_{it} + \gamma_2' T_{it} \times postext_{j(it)t} \\ & + \theta_{j(it)} + \theta_{j(it)} \times postext_{j(it)t} + \tau_t + \tau_t \times Y_{j(it)}^{exp} + \tau_t \times Y_{j(it)}^{ext} + \omega_{it}\end{aligned}$$

Here, $\ln(w_{it})$ is the natural logarithm of the salary of teacher i, working in district $j(it)$ in year t. The vector X_{it} contains indicators for teacher i's highest education degree and for years of experience. Alone and interacted with an indicator for the years following a CBA expiration or extension ($postext_{jt}$), these fixed effects allow us to account for observable differences across genders and compositional changes in the sample of teachers over time, which could affect salaries. The vector T_{it} contains indicators for i's grade level (elementary, middle, and high school) and subject (Math, Reading, English, and Science); alone and interacted with $postext_{jt}$,

[27] In Panel A of Appendix Table AII, we first estimate the gender salary gap prior to Act 10 controlling only for district and year fixed effects (column 1). In columns 2-5, we progressively add controls for experience, credentials, and teaching assignment.

[28] Panel (a) of Figure III show men and women's raw salaries by time-to-expiration; panels (b)-(d) show salaries by gender and year, separately for CBAs that expired in 2011, 2012, and 2014-2016. Throughout the paper, when we refer to "time-to-expiration", we are referring to the final extension expiration date for districts that received an extension, and otherwise to the expiration date. Salaries of teachers in districts with missing expiration data are shown in Appendix Figure AII.

they account for the possibility that districts used their flexibility to raise pay for teachers in certain subjects or grades. The vector θ_j contains district fixed effects, allowing us to account for district-specific components of salaries that are fixed in the periods before (θ_j) and after a CBA expiration or extension ($\theta_j \times postext_{jt}$). Year fixed effects τ_t, alone and interacted with expiration and extension year fixed effects Y_j^{exp} and Y_j^{ext}, control flexibly for time-specific factors that are common to all districts whose CBAs and extensions expired in the same year.[29]

We then estimate the following equation separately for men ($g = m$) and women ($g = f$):

$$\widehat{\omega_{it}} = \sum_{s=-5}^{5} \delta_{g(i)s} \mathbb{1}(t - Y_{j(it)}^{ext} = s) + \tilde{\varepsilon}_{it} \tag{1}$$

Estimates of δ_{ms} and δ_{fs} are shown in panel (a) of Figure IV. In the years leading to a CBA expiration, the conditional salaries of men and women were on similar, flat trends. Five years after the expiration, however, women's salaries fell by 0.2 percent relative to the year prior to the expiration (although this difference is indistinguishable from zero), whereas men's salaries increased by 0.6 percent (significant at 5 percent). While small in an absolute sense, these changes are significant when compared with the limited variation in conditional salaries among Wisconsin public school teachers prior to Act 10. In particular, a 0.6 percent increase in salaries for men corresponds to 6 percent of a standard deviation of pre-Act 10 conditional salaries and 5 percent of a standard deviation of post-Act 10 salaries, and it is roughly equivalent to the pre-Act 10 salary increase associated with an additional year of seniority.

4.2 The Gender Gap in Salaries

The differential trends in the salaries of men and women following the expiration of districts' CBAs gave rise to a gender gap in pay. We quantify this gap using a dynamic difference-in-differences design:

$$\begin{aligned} \ln(w_{it}) \quad = \quad & \beta_1' X_{it} + \beta_2' X_{it} \times postext_{j(it)t} + \gamma_1' T_{it} + \gamma_2' T_{it} \times postext_{j(it)t} + \theta_{j(it)} \\ & + \theta_{j(it)} \times postext_{j(it)t} + \tau_t + \tau_t \times Y_{j(it)}^{exp} + \tau_t \times Y_{j(it)}^{ext} + \sum_{s=-4}^{5} \delta_s F_i \times \mathbb{1}(t - Y_{j(it)}^{ext} = s) + \varepsilon_{it} \end{aligned} \tag{2}$$

In this equation, all variables are defined as before and the variable F_i equals one if the teacher is female. Estimates of the coefficients δ_s represent the differential impact of flexible pay on the

[29] Year fixed effects also control for possible direct effects of the additional provisions of Act 10 on salaries of male and female teachers and the gender pay gap.

salaries of women relative to men.

These estimates, shown in the solid series in panel (b) of Figure IV, indicate that a significant gender pay gap appeared after the introduction of flexible pay. Two years after the expiration of a CBA or its extension, women earned 0.4 percent less than men with equivalent years of experience and qualifications; this gap widened over time time, reaching 0.8 percent five years after the expiration. This lagged response could be a function of several factors. For example, teachers might have learned about the possibility of negotiating, or what they can negotiate over, gradually over time.[30] In addition, because not all districts adopted flexible pay at the same time, teachers in early adopter districts may have had fewer outside options and less bargaining power.

Overall, these estimates imply that women earned $440 per year less than men. While small in percentage terms, this difference corresponds to 8 percent of a standard deviation of conditional salaries prior to Act 10 (equal to $5,302) and 66 percent of the increase in the standard deviation of salaries that followed Act 10 (equal to $670). The results are summarized in Table II, where we re-estimate equation (2) pooling together the years before and after a CBA expiration. These estimates indicate that, prior to the introduction of flexible pay, women and men earned similar salaries conditional on observables. In the five years following the expiration of a CBA or its extension, however, women's salaries became 0.3 percentage points lower than men's salaries (Table II, column 1). Allowing the post-expiration gap to vary for each of the years following an extension indicates that the gap was largest four and five years after the expiration, at 0.7 percent (column 2). The gap is robust to only using the variation from CBA expirations, ignoring the extensions (columns 3 and 4), and to instrumenting the dates of CBA extensions with the dates of CBA expirations (columns 5 and 6).[31]

A recent literature has pointed to issues with dynamic difference-in-differences designs, including the possibility that, in the presence of heterogeneous treatment effects, some units might receive negative weights when their outcomes are aggregated to form treatment effects. This could bias the estimates (Borusyak et al., 2021; Callaway and Sant'Anna, 2020; De Chaisemartin and d'Haultfoeuille, 2020). To check for this possibility we replicate our results using the estimation method proposed by Sun and Abraham (2020), devised to obtain unbiased estimates in the

[30] In our survey, we find that in 2020 over 40 percent of teachers still believe that it is not possible to negotiate pay (see Section 5).

[31] In Panel B of Appendix Table AII, we replicate the estimates in Panel A to show how the gender salary gap changes as we progressively control for variables that entered into salary schedules, using data after a CBA expiration. In this time period, controlling observable teacher characteristics no longer closes the gender salary gap.

presence of heterogeneous treatment effects and outlined in greater detail in Appendix C. These estimates are indistinguishable from standard OLS estimates (Figure IV, panel (b), dashed series). We use standard OLS in the remainder of the paper; for completeness, Appendix C shows estimates from all the event studies included in the paper obtained using Sun and Abraham (2020)'s method, as well as estimates of our main event study using the procedures outlined by Borusyak et al. (2021) and Cengiz et al. (2019).[32]

4.3 Gender Gaps And The Use of A Salary Schedule

Districts varied in how they changed their pay following Act 10. While many discontinued the use of a salary schedule as soon as their CBAs expired, others continued to use one (Kimball et al., 2016). Whether districts that continued to use a schedule also introduced some pay flexibility influences the interpretation of the results in Table II and Figure IV. If all districts, even those with schedules, used some flexibility and allowed for individual negotiations, our results should be interpreted as the average treatment effect (ATE) of flexible pay. If only some districts allowed for negotiations, however, the results should be interpreted as an intent-to-treat (ITT).

To better understand these mechanisms, we explore differences in the gender salary gap by district type using information from employee handbooks, documents that describe the human resource policies in place for all district employees. Roughly half of all district handbooks continued to reference a salary schedule after CBAs had expired (we call these "schedule" districts). The other half of districts did not mention a schedule (we call these "non-schedule" districts). If schedule districts continued to set pay after Act 10 as they did before (i.e., strictly basing it on experience and education and without any flexibility), then we should not see a gender gap emerging after a CBA expiration in these districts, and the estimates in Table II would capture the ITT.

The data, however, do not support this hypothesis. Estimates of equation (2), obtained separately for schedule and non-schedule districts pooling together years before and after a CBA expiration, reveal a significant increase in the gender wage gap in both groups of districts (Table III, columns 1-3). No-schedule districts experience a 0.32 percent increase in the gap after a CBA extension (column 1), whereas schedule districts see a 0.29 percent increase (column 2). The

[32]The approach of Callaway and Sant'Anna (2020) is very similar to Sun and Abraham (2020); while Sun and Abraham (2020) use the last-treated cohorts as controls (effectively treating these cohort as never-treated), Callaway and Sant'Anna (2020) use all not-yet-treated cohorts as controls. The method devised by Cengiz et al. (2019) consists in aggregating event-by-event analyses and can be implemented by (i) creating event-specific datasets for each treated group and the corresponding set of "clean controls," and (ii) estimating the main event study on this stacked datasets, using dataset fixed effects.

difference between these two estimates is small and indistinguishable from zero (column 3).[33]

What explains the rise of a gender wage gap in districts that continued to use a salary schedule? Before Act 10, unions were fully involved in the negotiations of the schedules and guaranteed that no individual-level adjustments could take place. Without union involvement in the design and use of a schedule, after Act 10 even schedule districts could use flexibility by placing people with similar credentials on different steps and lanes of the schedule.[34] If this is the case, allowing for the returns to (actual) experience and education to differ across genders in equation (2) should close the gender gap in schedule districts.

The results of this exercise are shown in columns 4-6 of Table III. For exposition, we show the gender gap for teachers with 3 or 4 years of experience and a Master's degree. In line with our hypothesis, allowing for gender-specific returns to experience and education completely closes the gender gap in schedule districts, leaving it unchanged in no-schedule districts.[35] The results from this test confirms that even schedule districts used some flexibility in setting teachers' pay after Act 10, which in turns implies that the estimates in Table II represent the ATE of flexible pay.

4.4 Differences by Age and Seniority

Existing works have shown that the gender wage gap tends to grow across workers' careers, arguably due to child-bearing and family commitments that lead women to decrease their work hours (see Zeltzer, 2020, for a study of physicians). To see whether this hypothesis holds in our data, we test whether older or more experienced teachers see a larger gender salary gap following Act 10.

Panel (a) of Figure V shows estimates of δ_s in equation (2), obtained separately for teachers with six or fewer years and more than 20 years of seniority (the bottom and top quartiles of the seniority distribution). Panel (b) shows estimates for those aged 32 and younger and those older than 50 (the bottom and top quartiles of the age distribution). The data do not confirm the hypothesis. The gender wage gap is larger for less experienced teachers, and equal to 1.5

[33] Event study estimates are shown in panel (a) of Appendix Figure AIV and confirm these results.

[34] Some schedule districts explicitly state that the district has the discretion to determine teachers' placements on the schedule. An example is Madison Public Schools: Although its 2020 handbook contains a salary schedule, in Section 1.2 it states that "The District has the sole discretion to determine initial placement on the salary schedule" (Appendix Figure AV). It is possible that this differential placement was the result of differences in bargaining across genders. Survey data (presented in detail in Section 5) indicate that teachers bargain at the same rate in schedule and non-schedule districts. In the former, 37 percent of teachers report having bargained; in the latter, this share is 39 percent. These two shares are indistinguishable from each other.

[35] In non-schedule districts, however, the gap remains large at 0.8 percent. Event study estimates are shown in panel (b) of Appendix Figure AIV.

percent four years after the expiration of a CBA or its extension (panel (a), significant at the 1 percent level). For more experienced teachers, the gap is smaller at 0.5 percent (significant at 10 percent). These estimates correspond to 12 and 8 percent of the pre-Act 10 standard deviation in salaries, respectively. The gender pay gap is also larger and more persistent among younger teachers compared to older ones (panel (b)).

Although wide confidence intervals do not allow us to reject the null hypothesis of equality of the gender gap between younger and older teachers, and no significant gap appears when using the SA method (Appendix Figure CI), the point estimates suggest that young women might be more likely to opt out of bargaining or have lower returns to bargaining. It is unlikely for the salary gap to be driven by women with children working fewer hours or going on maternity leave, since our estimates account for a teacher's full or part-time status.[36] In Section 4.6, we also show that our estimates hold when we restrict the sample to teachers observed at least four years before and after a CBA expiration, to exclude women who are on maternity leave in a given year.

4.5 The Role of School and District Leadership

Studies across a variety of workplaces have found a positive correlation between the presence of female management and women's career outcomes (Casarico and Lattanzio, 2019; Langan, 2019; Cullen and Perez-Truglia, 2020). To explore whether the gender composition of schools' and districts' leadership is related to women's success, we test whether the gender wage gap varies with the gender of school principals and district superintendents. Principals and superintendents serve distinct roles in the public school system. Superintendents are district administrators in charge of hiring all staff, and they ultimately decide on employees' pay.[37] Principals manage individual schools, perform human resource leadership tasks related to the recruitment and selection of teachers, assign them to classes, evaluate their performance, and are in charge of their professional development. They also tend to have closer interactions with teachers relative to superintendents (Kimball et al., 2016).[38]

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[36] Since hours are set in K-12 teaching, all full-time teachers work the same number of hours. Part-time teachers work 50 percent of a FTE.

[37] A superintendent "[...] works for the school board and translates the policy into action. [...] the superintendent and staff make the day-to-day decisions that affect the operation of the school district, deploying board-approved resources, assigning staff and documenting results (see https://wasb.org/legal-human-resources-services/basic-resources/new-school-board-member-handbook/chapter-1-beginning-your-school-board-service/).

[38] See also https://dpi.wi.gov/sites/default/files/imce/ee/pdf/principalprocessmanual.pdf.

Principals Columns 1 and 2 of Table IV show estimates of equation (2), obtained separately for teachers who work under male and female principals in each year, pooling together years before and after a CBA expiration or its extension.[39] The change in the gender pay gap is larger in schools with a male principal, and equal to 0.41 on average across the five years following an expiration (column 1). In schools with a female principal, the change in the gap is small and indistinguishable from zero (column 2). In column 3, we pool male and female teachers and find that the pay gap for teachers working in schools with male principals is 0.44 percentage points larger than in schools with a female principal. Because principals are largely responsible for evaluating teachers and less involved in salary negotiations, this result suggests that male principals evaluate women more negatively than men.

Superintendents Next, we re-estimate equation (2) separately for teachers in districts with male and female superintendents in a given year. The estimates reveal a larger gender gap for teachers in districts with a male superintendent, equal to 0.45 percent (Table IV, column 4). In districts with a female superintendent, on the other hand, the change in the gap is positive and indistinguishable from zero (column 5). The difference in the gap between teachers with male and female superintendents is equal to 0.7 percentage points (column 6).[40]

This finding suggests that women are not just earning less than men everywhere; rather, the gender of the other negotiating party matters.[41] In particular, the fact that no salary gap exists when the superintendent is female suggests that women are either better able to negotiate with other women (or men are worse at negotiating with women), or that they experience backlash when they try to negotiate with men. We explore these possibilities in Section 5.

A caveat to the interpretation of these results is that principals and superintendents are not allocated randomly to schools and districts. It is possible that schools headed by female principals or districts headed by female superintendents differ on the basis of observable and unobservable characteristics related to the gender pay gap. For example, a school district located in a community with more gender-equal social norms could be more likely to have both a female superintendent and a smaller gender pay gap. As a partial test, in Appendix Table AIV we check

[39] Assigning teachers to the gender of their leader in any given year allows us to leverage variation from changes in a school's principal. Estimates are also robust to assigning teachers to the gender of their principal in the years prior to Act 10 (Appendix Table AIII).

[40] Event study estimates are shown in panel B of Appendix Figure AVI.

[41] Perhaps surprisingly, the gender of the superintendent seems to matter more than that of the principal (i.e., the coefficient estimate for "Female teacher $\times$ male superintendent $\times$ post extension" is larger than that for "Female teacher $\times$ male principal $\times$ post extension" in Table IV). This could be explained by the fact that, while school principals evaluate teachers and can provide recommendations to the superintendent on pay raises, the ultimate decision on teacher pay rests with the superintendent.

whether the gender of school and district leaders is correlated with teacher quality, attrition, and a set of proxies for social norms (such as the socio-demographic and political make-up of the community and female labor force participation). These correlations are small and mostly indistinguishable from zero.[42]

4.6 Additional Robustness Checks

Accounting for Compositional Changes Following Act 10, retirement rates spiked among Wisconsin teachers (Roth, 2019; Biasi, 2019). We conduct three robustness checks to ensure that our results are not driven by compositional changes to the pool of teachers. First, we restrict our analysis to a balanced panel of teachers in the eight years surrounding each expiration. This restriction yields an estimate of the gender wage gap equal to 0.7 percent five years after an expiration and 0.13 percent overall (panel (a) of Figure VI and column 1 of Table V, p-values equal to 0.001 and 0.17 percent respectively). Second, we re-estimate equation (2) on the subsample of teachers who entered Wisconsin public schools between 2007 and 2011. In line with the evidence in panel (b) of Figure V, the gap for this younger subsample is larger at 1.8 percent four years after an expiration and 0.52 overall (panel (a) of Figure VI and column 2 of Table V, significant at 1 and 10 percent respectively). Third, we re-estimate the same equation controlling for teacher fixed effects. The corresponding estimate is 0.6 percent four years after an expiration and 0.16 percent overall (panel (b) of Figure VI and column 3 of Table V, p-values equal to 0.001 and 0.10 respectively).

Accounting for Endogenous Switches Across Districts Biasi (2021) shows that an increase in teacher movements across districts followed the passage of Act 10. If these movements were driven at least in part by teachers' responses to the rise of a gender wage gap, the assignment of teachers to the policy change would be endogenous. To address this issue, we estimate the intent-to-treat (ITT) by assigning teachers to the district they taught in the year prior to the passage of Act 10. A teacher is then considered exposed to flexible pay the year their original district's CBA expires, regardless of whether they have moved from that district.[43] ITT estimates, shown in panel (b) of Figure VI and column 4 of Table V, are comparable to those in panel (b) of

[42]Appendix Table AIV shows estimates of OLS regressions of either an indicator for a district having a female superintendent in 2011 (column 1) or the share of principals in the district's schools who are women in 2011 (column 2) and variables listed on each row. Standard errors are reported in parenthesis and the mean of each variable is shown in square brackets.

[43]This strategy is similar to Yagan (2019), who estimates the effects of local unemployment rates on employment during the Great Recession (2007-2009) assigning rates to workers based on workers' location in January 2007.

Figure IV and Table II. We study the role of teacher mobility for the gender wage gap more in detail in Section 6.2.

Allowing for Different Salary Schedules Across Districts Next, we allow for the possibility that the gender wage gap that followed the expiration of districts' CBAs reflected changes in the salary schedules used by districts after Act 10. We do so by allowing the parameter vectors β_1 and β_2 in equation (2) to be district-specific. Our estimates of the gender gap remain robust when controlling for district-specific schedules (panel (c) of Figure VI and column 5 of Table V).

Controlling for Grade-Specific Effects Our main analyses control for grade group effects (elementary, middle, or high school), in part because most middle and high school teachers teach multiple grades, making it difficult to include specific grade fixed effects. However, even within elementary school, women are more likely to teach lower grades than men. If teachers teaching higher grades are more highly compensated, this could mechanically lead to a gender salary gap. To test for this, we control for fixed effects for a teacher's lowest and highest grade taught, alone and interacted with an indicator for years after a CBA expiration. The results, shown in panel (c) of Figure VI and column 6 of Table V, are largely unchanged.

Controlling for Extra Duties Data from the DPI include information on all additional non-teaching duties performed by teachers in each year, including (but not limited to) serving as a department head, program coordinator, or sports coach. Four percent of men and 2.7 percent of women take on at least one of these extra duties, generally associated with additional pay. If compensation for these duties increased with the introduction of flexible pay, this might drive part or all of the estimated gender gap. We test for this possibility by controlling for whether teachers perform any other duties besides teaching in equation (2).[44] The data confirm that extra duties are associated with an 11 percent pay premium (column 1 of Appendix Table AV). Our estimates of the gender gap, however, are nearly unchanged when we flexibly control for these duties (panel (d) of Figure VI and column 2 of Appendix Table AV), indicating that extra duties cannot explain the gender differences in pay that followed Act 10.[45]

[44]The most frequent extra duties include being a Department Head, Other Professional (which includes Head of Athletics), Non-Teaching Support Role (which includes all coaches), Program Coordinator, Other Support Staff, and Subject Coordinator. Together, these comprise 75 percent of all extra duties.

[45]Column 3 of Table AV also indicate that the returns to extra duties might be smaller for women, although estimates are imprecise (*Female* $\times$ *Other duty* $\times$ *Post expiration* equal to -0.908, p-value equal to 0.32).

Alternative Inference Approaches To test the sensitivity of our estimates' precision to alternative inference approaches, Table AVI shows estimates from the first four columns of Table II, along with t-statistics obtained using a Wild cluster bootstrap (where the clusters are the school districts, Cameron and Miller, 2015) (in brackets) and p-values obtained using permutation tests, where we randomly permute the timing of the expiration of a CBA (columns 1 and 2) or its extension (columns 3 and 4, in parentheses).[46] Our conclusions in terms of statistical significance of the estimates are largely unchanged.

5 Avoiding Bargaining or Being Punished? A Survey

We have shown that a salary gap emerged between male and female Wisconsin teachers following the introduction of flexible pay. Gender differences in bargaining might play an important role in driving this gap. Administrative staff and salary data, however, do not allow us to directly test whether women chose not to bargain following Act 10 or whether they bargained, but were less successful at it or penalized for doing so. Distinguishing between these explanations is crucial for policy. For example, if women chose not to bargain because they underestimated the returns to doing so, providing them with information on these returns could close part or all of the gender pay gap.[47] Alternatively, if women have worse negotiating skills than men, providing them with the appropriate training could help close the gender wage gap (Ashraf et al., 2020).

To discern among these hypotheses, we surveyed current Wisconsin public school teachers. We asked teachers whether they had ever bargained their salary in their current and past positions and about their intention to bargain over pay and other aspects of their job in the future. If a respondent reported having negotiated their salaries, we asked them whether they believed the negotiation was successful; if they instead reported not negotiating, we asked the reason for this choice. To measure beliefs about the returns from bargaining, we also asked teachers whether they knew their colleagues' salaries or had colleagues who negotiated pay. Finally, we used questions from social psychology to create a measure of negotiating skills, and we asked respondents to rate their performance relative to that of their colleagues to measure their confidence. Answers to these questions allow us to study the mechanisms underlying the salary gap.

[46] We assign dates such that the timing distribution that we observe in the data is preserved.

[47] Roussille (2020) shows that while women in tech ask for a much lower initial salary compared with men, they raise their bid when informed about the median salary for their position.

Survey Details and Sample Description The survey questionnaire is shown in Appendix D. We sent an email invitation to fill in the survey (shown in Appendix Figure DI) to 39,081 teachers employed in the 276 Wisconsin districts which make teachers' emails available on their websites.[48] A total of 3,156 teachers responded to our survey, with a response rate of 13 percent. The gender and age distributions of the respondents closely resemble those of the teacher population (Appendix Figure AVII).[49]

5.1 Gender Differences in Negotiation Experiences and Attitudes

Table VI summarizes men's and women's responses to the the survey questions. The main result is that women are less likely to have negotiated their pay with previous and current employers. For example, 37.9 percent of men and 29.5 percent of women report having negotiated with past employers (a 21 percent difference). Women are also 8.3 percentage points less likely to have negotiated at the start of their current job and 4.0 percentage points less likely to have negotiated after the start of their current job.

Conditional on having negotiated at the beginning of their current contract, women are 10.5 percentage points less likely than men to state that the negotiation with the current employer at the start of the relationship was successful. Among the reasons for not negotiating, three answers stand out: Women are more likely than men to state that they were not comfortable negotiating (with a gender difference of 10.5 percentage points or 83 percent), that they thought it would be useless (2.2 percentage points or 35 percent), and that they were already satisfied with their pay (3.6 percentage points or 24 percent).[50]

Most of our questions concern negotiations over salaries. It is possible, however, that women are more inclined to negotiate other job aspects beyond pay. To explore this possibility, we asked teachers about the likelihood that they will negotiate salaries, classroom assignment, and non-teaching duties in the future. The data confirm that gender differences in bargaining dis-

[48]These include 69 districts with CBA or extension expiration dates in 2011, 61 in 2012, 26 in 2013, one in 2014, and one in 2016, as well as 62 non-schedule districts and 78 schedule districts. We did not explicitly ask teachers to disclose their school district; we obtained this information by sending out different surveys to teachers in different districts. The survey was sent out on March 5, 2020; two reminder emails were sent in the following 14 days to the teachers who had not responded. The survey was closed on May 7th, 2020.

[49]Appendix Table AVII tests whether administrative district variables, such as the number of teachers, average teacher salary and experience, the share of teachers who are female, the post-CBA expiration/extension conditional gender pay gap, an indicator for the superintendent being male, the share of school principals who are male, and indicators for the CBA or its extension expiring in either 2012, 2013, 2014, or 2016, can explain whether a district had at least one survey response (column 1) or a district's response rate (column 2). The F-statistics of joint significance of these variables are both below 2, suggesting that these variables cannot explain response rates.

[50]It is worth noting that men are more likely to state that they did not negotiate pay *after* the start of their current contract because they are satisfied with pay, suggesting that salary satisfaction does not explain the entire difference in bargaining.

proportionately affect wage negotiations. While women are 19 percent less likely than men to report that they will negotiate their pay, they are only 5 percent less likely to plan on negotiating non-teaching duties and slightly more likely to plan on negotiating their classroom assignment.

Turning to other possible determinants of willingness to bargain, we find that women are 29 percent less likely than men to know their colleagues' salaries and 14 percent less likely to know someone who negotiated their pay. This could lead women to underestimate the returns to bargaining.[51] No gender differences exist in measures of socio-emotional skills, such as the ability to assess how people feel and to read subtle signals in other people's behavior, which we use as proxy for bargaining ability (Sharma et al., 2013).[52] Women are, however, 13 percent less likely to state that they are confident talking to people they don't know. Lastly, women in our data tend to value themselves less than their male colleagues, and they are 12 percent less likely than men to report that their performance is above average.[53]

Controlling for Teachers' and Districts' Attributes A simple comparison of men's and women's answers indicates that women are less likely than men to negotiate their pay. We now test whether these differences remain once we control for teachers' and districts' observable characteristics. Specifically, we control for district fixed effects to account for potential differences in the negotiating environment across districts. We also control for a set of teacher attributes such as age, knowledge of colleagues' salaries, and measures of socio-emotional skills, to gauge the extent to which the observed gaps in the propensity to negotiate is explained by teachers' bargaining ability, confidence, or their expected returns to negotiating.

Table VII presents our main results. Panel A confirms that, even controlling for district fixed effects and teacher attributes, women are 7.1 percentage points (or 23 percent) less likely to have negotiated at the start of their tenure with their current employer (column 1). They are also 2.8 percentage points (or 11 percent) less likely to have negotiated after the start of their tenure, although this difference is estimated imprecisely (column 2, p-value equal to 0.13).

Among teachers who have negotiated in the past, the likelihood of success is lower for women than for men. Controlling for district fixed effects and teacher attributes and conditional on having negotiated, women are 13 percentage points less likely to report that salary

[51]In our survey, less than one-third of all teachers state that they know their colleagues' pay. This is in spite of the fact that this information is publicly available on the WDPI's website (available at https://dpi.wi.gov).

[52]These skills are drawn from the literature on individual differences in negotiating behaviors and outcomes. For an overview, see Sharma et al. (2013).

[53]This finding is in line with Exley and Kessler (2019), who show that women are less likely to self-promote themselves in professional contexts, in part because they underestimate their performance.

negotiations with their current employer, at the start of the relationship, were successful (19 percent, Table VII, panel B, column 1).

In columns 2-6 of panel B we test for gender differences in the reasons for the choice of not negotiating at the beginning of the current employment relationship.[54] Controlling for district effects and teacher attributes, we find that women are 6.5 percentage points more likely than men to state that they were not comfortable negotiating (column 2), but 4 percentage points less likely to state that they are satisfied with their pay (column 5). Women are also slightly more likely than men to claim that they thought negotiating was useless (2.4 percentage points, column 3), although this difference is not statistically different from zero.

Lastly, in columns 5-8 of panel A we explore the likelihood that women will negotiate in the future. Our estimates confirm that women are 12 percent less likely than men to plan on negotiating their pay in the future (with an estimate for *Female* equal to -0.475, column 5, significant at 1 percent). Women are also slightly more likely to negotiate their teaching assignment (column 7) and as likely as men to negotiate other non-teaching duties (column 8). These results indicate that the reluctance of women to bargain is limited to negotiations over pay.

5.2 The Role of Superintendents' Gender

In Section 4.5, we showed that the gender wage gap is larger among teachers who work under a male principal or superintendent. We now investigate whether the propensity to negotiate is related to the gender of the district's management.[55] We find that the observed gender differences in bargaining are largely driven by teachers working under a male superintendent. Simple comparisons of means indicate that women are 19 percent less likely to negotiate their pay in the future under a male superintendent, while men and women are equally likely to negotiate when the superintendent is a woman (Table VI). Controlling for district and teacher attributes, we confirm that women working under a male superintendent are 8.3 percentage points (27 percent) less likely than men to have negotiated their pay with their current employer at the start of the work relationship (estimate for *Female*, Table VII, panel A, column 2, significant at 1 percent), 5.7 percentage points (23 percent) less likely to have negotiated after the start (column 4), and 18 percent less likely to plan to negotiate in the future (column 6). Women and men working under a female superintendent are instead equally likely to have negotiated their pay at the start of the

[54] The results are similar if we instead look at reasons for not negotiating with a past employer.

[55] We assign superintendents' genders to districts using information from 2016. To ensure confidentiality, we did not collect information on respondents' schools. This prevents us from investigating the role of the gender of school principals.

current job (estimate for *Female* + *Female* × *F super* in panel (a), column 2 of Table VII, panel A), to have negotiated after the start (column 4) or to plan to negotiate in the future (column 6).

However, we do not find evidence that women are more likely to report that their negotiations were successful under a female superintendent (Table VII, panel B, column 3). We also do not find any association between the gender of the superintendent and the reasons teachers give for not negotiating (Table AVIII). It should be noted, however, that the coefficients for *Female* × *Female super* in these tables are estimated imprecisely, which prevents us from ruling out large positive or negative values for the point estimates.

5.3 Additional Results

In Biasi and Sarsons (ming) we show that gender differences in confidence and information on the returns to bargaining cannot fully explain the gender gap in bargaining. In Appendix Table AIX we further investigate whether the gender gap remains when we control for individual attributes (such as knowing the salaries of colleagues, measures of self-confidence, and socio-emotional skills), separately for men and women. While some of these attributes (such as measures of self-confidence and confidence in talking to strangers) are positively correlated to the likelihood of negotiating, this relationship does not differ across genders: Estimates for the interaction coefficients in Appendix Table AIX (columns 1-3) are small and statistically insignificant. Furthermore, controlling for these variables leaves the estimate for *Female* largely unchanged. We also investigate the role of these attributes in the likelihood of reporting that past negotiations were successful and of stating that they felt uncomfortable negotiating (columns 4 and 5). Measures of socio-emotional skills and self-confidence are associated with a lower likelihood that women report feeling uncomfortable negotiating; however, controlling for these attributes leaves the estimated gender gaps largely unchanged. Taken together, these results do not show evidence that individual attributes related to beliefs about the returns to bargaining, confidence, and bargaining ability have a large impact on the gender gap in the propensity to negotiate.

5.4 Survey Results: Summing Up

The results from our survey indicate that women are less likely than men to have negotiated their pay at several stages of their careers. This difference cannot be explained by a lower bargaining ability or differences in the perceived returns from negotiating, and it does not appear to be driven by whether teachers know their colleagues' salaries or other people who have negotiated their pay. Our results instead suggest that the bargaining environment might play an important role in determining whether teachers choose to negotiate or not.

When interpreting the survey results, a few caveats bear mention. First, we cannot rule out selection into the survey, based on unobservables correlated with gender and/or attitudes towards bargaining. Second, since we cannot link our survey answers to administrative records, we are unable to exactly estimate the portion of the post-Act 10 gender wage gap generated by men's and women's different propensities to negotiate. However, the results from the survey provide suggestive evidence that women's reluctance to bargain is likely an important driver of these salary differences.

6 Alternative Explanations for the Gender Wage Gap

To obtain a better understanding of the importance of bargaining vis à vis other explanations for the gender wage gap, we test here for three alternative mechanisms: 1) gender differences in teaching quality, 2) differences in mobility, and 3) differences in the demand for male and female teachers.

6.1 Gender Differences in Teaching Quality

A possible explanation for the observed wage gap is that districts used their post-Act 10 flexibility to reward teachers for their quality, and men are better teachers than women. A simple comparison of VA between men and women does not support this hypothesis: Women's average VA is close to zero both before and after Act 10, whereas men's VA is equal to -0.002 before Act 10 and -0.001 afterwards. The gender difference in VA is significant at the 1 and 5 percent levels before and after Act 10, respectively (Table I).

Even if women appear to be better teachers on average, it is still possible that some men have higher quality and are compensated more after the introduction of flexible pay. To test whether the gender wage gap can be explained by differences in VA across teachers, we augment equation (2) to control for *VA* and *VA * Post extension*.

Because VA is only available for teachers in tested grades (4-8) and subjects, we begin by first estimating equation 2 on the subsamples of teachers with and without VA.[56] After a CBA expiration, the increase in the gender pay gap is smaller at 0.6 and indistinguishable from zero

[56] As we explain in Section 3, students in Wisconsin get tested in grades 3-8 and 11 in math and reading. Since our VA model controls for past test scores, we cannot calculate VA for teachers in grades 3 and 11.

for teachers with VA (with a p-value of 0.79, Table VIII, column 1), while it is larger at 0.33 percent for teachers without VA (significant at 1 percent, column 2). This is a finding we confirm in subsection 6.3 (where we show that the gender salary gap is primarily driven by teachers in high schools, or schools with a larger share of men, for whom we cannot calculate VA). Importantly, however, the estimated gender gap on the subsample of teachers with VA remains unchanged at 0.07 when we control for *VA* and *VA* $\times$ *Post extension* (with a p-value of 0.78, Table VIII, column 2).[57]

A possible explanation for the absence of a gender gap for teachers with VA is that the availability of an objective quality measure limits management's use of discretion in setting pay. Under this hypothesis, the returns to a higher VA should be similar for men and women.[58] To test this, in column 4 of Table VIII we allow the post-Act 10 returns to VA to differ among men and women, interacting *VA * Post Expiration* with indicators for men and women. An estimate of 0.56 for *Male* $\times$ *VA* $\times$ *Post Extension* indicates that a one standard deviation higher VA is associated with a 0.6 percent higher pay for men after a CBA expiration (significant at the 5 percent level). An estimate of *Female* $\times$ *VA* $\times$ *Post Extension* equal to 0.03 (with a p-value equal to 0.82) indicates instead that the return to a higher VA is zero for women.

Taken together, these estimates indicate that men are compensated for having a high VA while women are not. Furthermore, in columns 3-4 of Table VIII the estimates for *Female * Post Expiration* are the same as in column 2; this implies that, at least on the subsample of teachers with VA, the gender pay gap is not related to teacher quality. Admittedly, the relationship between quality and pay for men and women could differ on the subsample of teachers without a VA measure, for whom the gender pay gap following the introduction of flexible pay is larger and significant. Nevertheless, these results suggest that the emergence of a gender pay gap is not solely due to the increased variance in pay (Juhn et al., 1993; Blau and Kahn, 1996).

6.2 Gender Differences in Job Mobility

Gender differences in cross-district mobility could influence the gap in several ways. First, female teachers might be less likely to relocate than men. In this case, they would not be able to

[57] We remove observations with VA in the top and bottom one percent of the distribution and we standardize VA to have mean zero and variance one.

[58] To test whether the results in Table VIII are driven by changes in the composition of teachers in tested positions (with VA), we test whether the likelihood of changing teaching assignment (i.e., grade, subject, or school) or switching from a tested to a non-tested position differed between men and women after a CBA expiration relative to before. The data do not show evidence of this (Appendix Figure AVIII). Compositional changes are thus unlikely to explain our results.

take advantage of higher salaries offered in other districts.[59] Second, if employers know that women are unlikely to move, women may receive fewer outside offers and enjoy less bargaining power in negotiations with their current district or with any prospective employer (Caldwell and Danieli, 2018).

A simple plot of the share of male and female teachers who change district in each year, by time-to-expiration of each district's CBA, indicates that women are only slightly less likely to move throughout the period of analysis (Panel A of Appendix Figure AIX). To more rigorously test for differences in mobility, we estimate

$$Moves_{it} = \beta_1 Female_i + \beta_2 postext_{j(it)} + \beta_3 Female_i \times postext_{j(it)} + \alpha X_{it} + \theta_{j(it)} + \tau_t + \varepsilon_{it} \quad (3)$$

where $Moves_{it}$ is a dummy indicating that teacher i moved to a different school district in year t. In column 1 of Table IX we estimate this equation without teacher controls (X_{it}) and fixed effects ($\theta_{j(it)}$, and τ_t). The estimate for *Female* × *post-extension* is small and indistinguishable from zero. Estimates remain robust when we control for district and year fixed effects (column 2) and for teachers' observables, such as experience and education (column 3).[60] In column 4 the dependent variable indicates movements across districts within a commuting zone (CZ), which do not require a relocation, while in column 5 it indicates movements across CZs. These results indicate no gender differences in the likelihood of moving across districts, both within and across CZs.[61]

As a further test for the role of mobility, we estimate event studies of the gender wage gap around a CBA expiration separately for three groups of teachers: (i) those who never move, (ii) those who move at least once between 2007 and 2016, and (iii) those who move at least once after a CBA expiration. While the gap is largest for teachers who move post-expiration, it is still significant at 0.7 percent for teachers who never move (Figure VII, panel (a)). These results suggest that *observed* mobility plays at most a small role in explaining the gender gap.[62]

[59]Existing research suggests that women have a lower willingness-to-commute than men, possibly because of family obligations (LeBarbanchon et al., 2020; Caldwell and Danieli, 2018; Manning, 2003). A similar argument can be applied when thinking about moving. Using survey data from a set of European countries, Hospido (2009) finds no gender differences in moving rates. Although they find no differences in moving rates across gender, Keith and McWilliams (1999) show that women are less likely to quit or change jobs for family reasons.

[60]The district fixed effect is the district the teacher works in each year.

[61]Women might also be less likely to move to no-schedule districts if they anticipate being rewarded less than men (especially given their lower return to value-added). However, we do not find strong evidence of gender differences in propensity to move when splitting our sample by the type of district of origin/destination (schedule or no-schedule), by teacher VA, or by the combination of the two (Appendix Table AX).

[62]In Panel B of Appendix Figure AIX we also test whether the returns to moving differ for men and women who actually move. Specifically, we estimate an event study of conditional salaries around each move, separately for men and women who move at least once, and focusing on moves that happen after a CBA expiration in the destination

It is still possible, however, that *unobserved* mobility plays a role: Men might receive more outside offers than women because they can more credibly threaten to move. Our data does not allow us to observe outside offers that teachers do not accept. To make progress, we test whether the salary gap is larger in CZs with more schools, where a teacher should in principle have more options. We find that the salary gap is largest for teachers in CZs with a number of schools in the top quartile of the distribution, suggesting that outside options may play a role in determining men and women's bargaining power (Figure VII, panel (b). These results are in line with our main hypothesis that differences in bargaining influence the gender wage gap once flexible pay is adopted. Differences in mobility could be an additional driver of differences in bargaining outcomes.

6.3 Higher Demand for Male Teachers

Men are underrepresented in the teaching profession. A higher demand for male teachers could result in men having their salaries bid up once Act 10 allowed for individual negotiations. Under this hypothesis, we would expect the gender wage gap to be larger in schools or districts with a higher demand for men. Since teachers' demand is unobserved, we conjecture three instances in which the demand for men could be higher and test whether the gender wage gap is larger in these cases.

First, demand might be higher in schools and grades where men are scarcer, such as elementary schools (where men are only 20 percent of the teacher population, compared with 40 percent for middle and high schools). When we look at differences by school type, however, the gap is significantly smaller for teachers in elementary schools compared with those in high schools (Appendix Figure AX, panel (a)). The gap is also smaller in schools with a share of men in the top half of the distribution, relative to the bottom half (Appendix Figure AX, panel (b)).[63]

It is possible that schools that employ more men do so precisely due to a higher demand for male teachers. We thus identify a second instance where the demand for male teachers might be higher: schools that lost and gained men immediately before Act 10. However, we find comparable gender gaps among schools where the share of male teachers declined by 2 percentage points or more relative to those where this share increased; if anything, the gap is

district. The estimates indicate that the returns from moving are larger for men: Immediately following a move, salaries of men increase by 4.2 percent whereas salaries of women only increase by 2.8 percent.

[63] Appendix Figure AX shows an event study of the gender wage gap for schools where the share of male teachers was above and below the pre-Act 10 median. These estimates indicate that the gap is large five years after a CBA expiration in schools where more than 30 percent of teachers are men (solid line) compared with schools with less than 30 percent male teachers (dashed line).

larger in the latter group (Appendix Figure AX, panel (c)).

The third instance are schools enrolling a higher share of boys. If male teachers act as role models for male students, these schools should have a higher demand for men and a larger gap. Our data confirm this hypothesis: The gap is significantly larger in schools with 54 percent or more male students (the top 5 percent of the distribution) compared with those with 48 percent or fewer males (the bottom 5 percent, Appendix Figure AXI, panel (a)).[64] Because the variation in the share of male students is rather limited, however, controlling for this variable does not change the gender wage gap (Appendix Figure AXI, panel (b)).

Although imperfect, these results provide suggestive evidence that, while a higher demand for male teachers is associated with a larger gap, it explains at most a very small portion of our overall estimated gender gap. Another possible reason why demand might be higher for men is discrimination against women. If male principals or superintendents either believe that men are higher quality teachers, or simply prefer to work with male teachers, they could increase men's salaries after Act 10 to attract them. A direct test for of this hypothesis is not possible in our context but should be kept in mind when interpreting our estimates, and it represents an avenue for future research.

7 Conclusion

Differences in willingness to negotiate is often discussed as a potential contributor to the gender wage gap. At the same time, the erosion of union power and the rise of flexible pay could open the possibility that pay differences emerge based on willingness to negotiate. This paper uses data from a large public-sector employer, the Wisconsin public school system, to shed light on these issues. Wisconsin's Act 10 replaced the traditional bargaining system, in which teacher unions bargain with the school district, with a system that involves individual bargaining between teachers and school districts. The staggered timing of the introduction of the bill's provisions allows us to quantify the impact of flexible pay on the gender wage gap, as teachers became allowed to individually bargain over their salaries.

In line with previous experimental work, we find that women lose relative to men when bargaining becomes an option. When school districts adopted flexible pay, a gap emerged in the salaries of men and women. The gap is largest among new, inexperienced teachers, and among

[64]This result holds and the difference becomes more pronounced using schools in the top and bottom 1 percent of the distribution of the share of male students. Thirty-nine percent of schools with more than 54 percent male students are high schools.

teachers working in schools or districts run by men. These results suggest that bargaining might play an important role in shaping the gender wage gap.

Responses to a survey administered to all Wisconsin teachers confirm this hypothesis. Women are less likely to have negotiated their salary in the past or to expect to do so in the future, especially if they work in a district with a male superintendent. Survey responses further suggests that women chose not to negotiate because they felt uncomfortable doing so, rather than because they underestimate the returns to it or are worse at bargaining. We also explore possible alternative explanations for the gap, unrelated to bargaining. The gap is not explained by gender differences in teacher ability or job mobility and is unlikely to be driven by a higher demand for men in certain schools.

Our results call for further exploration into policies that might prove successful in reducing the gender wage gap when flexible pay is adopted. The evaluation of policies that train women to negotiate, that have women negotiate with other women, or that improve salary transparency (Baker et al., 2019) or transparency regarding salary gaps represent important topics for further research. In addition, more research is needed to understand why the gender gap in salaries is driven by districts headed by a female superintendent or principal. If this is a sign of discrimination against women, policies such as providing negotiations training may be ineffective. Finally, we bring causal evidence to questions related to unionization and wage inequality, corroborating earlier evidence of a negative correlation between unionization and the gender wage gap (Blau and Kahn, 1996).

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Tables

Table I: Mean observable characteristics, male and female teachers

	2007-2011			2012-2016		
	Males	Females	Diff.	Males	Females	Diff.
Experience (yrs)	15.0	14.5	0.51***	14.2	13.9	0.28***
			(0.042)			(0.039)
Age	42.9	43.3	-0.38***	42.4	42.5	-0.053
			(0.047)			(0.045)
Value-added	-0.0023	-0.00023	-0.0021***	-0.0012	-0.000067	-0.0011**
			(0.00069)			(0.00048)
Salary ($1,000)	51.3	51.2	0.063	54.2	53.9	0.33***
			(0.051)			(0.053)
Full-Time Equivalents	98.0	97.1	0.90***	97.1	97.0	0.096*
			(0.057)			(0.054)
Full-Time	0.94	0.92	0.015***	0.92	0.92	-0.0036***
			(0.0011)			(0.0012)
Ever moves	0.11	0.10	0.013***	0.16	0.14	0.016***
			(0.0013)			(0.0015)
Leaves sample	0.066	0.064	0.0028***	0.074	0.073	0.00073
			(0.0011)			(0.0013)
BA	0.49	0.48	0.0036*	0.47	0.46	0.0072***
			(0.0022)			(0.0021)
Master	0.50	0.51	-0.0049**	0.52	0.53	-0.0098***
			(0.0022)			(0.0021)
PhD	0.0022	0.00095	0.0012***	0.0039	0.0014	0.0025***
			(0.00016)			(0.00020)
Grade level						
Elementary	0.21	0.49	-0.28***	0.22	0.49	-0.27***
			(0.0021)			(0.0021)
Middle	0.30	0.25	0.043***	0.29	0.25	0.046***
			(0.0019)			(0.0019)
High	0.56	0.25	0.31***	0.54	0.24	0.30***
			(0.0020)			(0.0019)
Subject						
Math	0.12	0.057	0.061***	0.12	0.062	0.062***
			(0.0011)			(0.0011)
Science	0.79	0.82	-0.038***	0.78	0.83	-0.046***
			(0.0017)			(0.0017)
Reading	0.011	0.041	-0.029***	0.011	0.041	-0.030***
			(0.00077)			(0.00077)
Observations	267962			279443		

Note: The table shows mean characteristics of male and female teachers, and the differences in means (standard errors in parentheses) for the years 2007–2011 (columns 1-3) and 2012–2016 (columns 4–6). *Salary* is a teacher's yearly salary. *Value-added* is a measure of teacher quality, described in Section 3.1. *Ever moves* is an indicator equal to 1 if a teacher has moved to a new school from $t-1$ to t. *Leaves sample* is an indicator equal to 1 if a teacher no longer appears in the sample in $t+1$. *Elementary*, *Middle*, and *High* are indicators for a teacher's grade level. Similarly, *Math*, *Science*, and *Reading* are indicators for a teacher's subject. *Full-Time Equivalents* equal 100 for teachers employed full-time. $^* \leq 0.1$, $^{**} \leq 0.05$, $^{***} \leq 0.01$.

Table II: Gender pay gap after a CBA expiration/extension: OLS and 2SLS

	Extensions		Expirations		2SLS, Extensions	
	(1)	(2)	(3)	(4)	(5)	(6)
Female	-0.121	-0.121	-0.097	-0.097	-0.097	-0.097
	(0.120)	(0.120)	(0.116)	(0.116)	(0.116)	(0.116)
Female × Post Extension	-0.286***				-0.348***	
	(0.094)				(0.111)	
Female × 1 Year(s) Post		-0.062		-0.014		-0.071
		(0.117)		(0.114)		(0.298)
Female × 2 Year(s) Post		-0.285*		-0.269**		-0.672*
		(0.148)		(0.136)		(0.358)
Female × 3 Year(s) Post		-0.073		-0.248*		-0.015
		(0.156)		(0.143)		(0.328)
Female × 4 Year(s) Post		-0.705***		-0.347*		-0.566
		(0.191)		(0.186)		(0.566)
Female × 5 Year(s) Post		-0.718***		-0.600***		-0.843
		(0.228)		(0.179)		(0.736)
Female × Post Expiration			-0.281***			
			(0.089)			
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Yr × Exp yr	Yes	Yes	Yes	Yes	Yes	Yes
N	444111	444111	444111	444111	444111	444111
# districts	247	247	247	247	247	247

Note: The dependent variable is the natural logarithm of salary per year, in full-time equivalency units and multiplied by 100. The variable *Female* equals one for female workers, the variable *Post Expiration* equals one for years following the expiration of a CBA, and the variable *Post Extension* equals one for years following the expiration of a CBA or its extension. The variables *X Year(s) × Post* equal one for observations X years after an extension (in columns 2 and 6) or an expiration (column 4). Columns 1-4 estimate OLS; columns 5 and 6 estimates 2SLS, with *Post expiration* as an instrument for *Post extension*. *Controls* include fixed effects for the district, number of years of experience, highest education degree, grade level (elementary, middle, high), and subject (math, reading, and others), alone and interacted with an indicator for years after the extension of a CBA. All specifications also include year fixed effects interacted with CBA expiration and extension year effects. Standard errors in parentheses are clustered at the district level. $^{*} \leq 0.1$, $^{**} \leq 0.05$, $^{***} \leq 0.01$.

Table III: Gender pay gap after a CBA expiration/extension, by district type

	Baseline			Gender-specific schedule		
	(1)	(2)	(3)	(4)	(5)	(6)
	No schedule	Schedule	Diff	No schedule	Schedule	Diff
Female	-0.156	-0.100	-0.103	0.715	-0.146	0.144
	(0.162)	(0.178)	(0.178)	(0.527)	(0.337)	(0.324)
Female × Post Extension	-0.318**	-0.290**	-0.288**	-0.820*	0.680	0.390
	(0.139)	(0.144)	(0.143)	(0.483)	(0.582)	(0.628)
Female × No sched			-0.055			0.108
			(0.239)			(0.249)
Female × No sched × Post			-0.029			-0.746
			(0.199)			(0.741)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Yr × Exp yr	Yes	Yes	Yes	Yes	Yes	Yes
Exper * Female * Post Ext	No	No	No	Yes	Yes	Yes
N	176917	220414	397439	176917	220414	397331
# districts	81	99	180	81	99	180

Note: The dependent variable is the natural logarithm of salary per year, in full-time equivalency units and multiplied by 100. The variable *Female* equals one for female workers and the variable *Post Extension* equals one for years following the expiration of a CBA or its extension. Columns 1 and 4 are estimated on teachers in districts that do not use a salary schedule after Act 10, and columns 2 and 5 are estimated on teachers in districts that keep a salary schedule. Columns 4-6 allow for the gender-specific returns to experience and education, by including fixed effects for years of experience and highest education degree, interacted with *Female* and an indicator for years after a CBA expiration/extension. *Controls* include fixed effects for the district, number of years of experience, highest education degree, grade level (elementary, middle, high), and subject (math, reading, and others), alone and interacted with an indicator for years after the extension of a CBA. All specifications also include year fixed effects interacted with CBA expiration and extension year effects. All columns present OLS estimates. Standard errors in parentheses are clustered at the district level. $^{*} \leq 0.1$, $^{**} \leq 0.05$, $^{***} \leq 0.01$.

Table IV: Gender pay gap after a CBA expiration/extension, by principal and superintendent gender

	Principal			Superintendent		
	(1)	(2)	(3)	(4)	(5)	(6)
	Male	Female		Male	Female	
Female	-0.091	-0.200	-0.195	-0.037	-0.386**	-0.450**
	(0.114)	(0.182)	(0.176)	(0.132)	(0.186)	(0.184)
Female × Post Extension	-0.413***	0.018	0.014	-0.453***	0.134	0.246
	(0.103)	(0.162)	(0.167)	(0.151)	(0.243)	(0.260)
Female × Male princ			0.095			
			(0.175)			
Female × Male princ × Post			-0.440**			
			(0.187)			
Female × Male super						0.426*
						(0.224)
Female × Male super × Post						-0.736**
						(0.365)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Yr × Exp yr	Yes	Yes	Yes	Yes	Yes	Yes
N	261528	173703	435234	322513	115796	438312
# districts	244	232	247	239	107	247

Note: The dependent variable is the natural logarithm of salary per year, in full-time equivalency units and multiplied by 100. The variable *Female* equals one for female workers and the variable *Post Extension* equals one for years following the expiration of a CBA or its extension. The variables *Male princ* and *Male super* equal one for teachers in schools with a male principal and in districts with a male superintendent, respectively, in any given year. *Controls* include fixed effects for the district, number of years of experience, highest education degree, grade level (elementary, middle, high), and subject (math, reading, and others), alone and interacted with an indicator for years after the extension of a CBA. All specifications also include year fixed effects interacted with CBA expiration and extension year effects. All columns present OLS estimates. Column 1 is estimated on teachers in schools with a male principal, column 2 on teachers in schools with a female principal, column 4 on teachers in districts with a male superintendent, and column 5 on teachers in districts with a female superintendent. Standard errors in parentheses are clustered at the district level. $^{*} \leq 0.1$, $^{**} \leq 0.05$, $^{***} \leq 0.01$.

Table V: Gender pay gap after a CBA expiration/extension: Robustness checks

	Balanced	Entrants 2007-11	Teacher FE	ITT	Distr-spec schedule	Full-time	Grade FE
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Female	-0.043	0.146		-0.185	-0.195*	-0.081	-0.045
	(0.110)	(0.203)		(0.116)	(0.116)	(0.120)	(0.111)
Female × Post Extension	-0.134	-0.520*	-0.155	-0.206**	-0.300***	-0.239**	-0.260***
	(0.098)	(0.267)	(0.094)	(0.101)	(0.095)	(0.101)	(0.096)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Yr × Exp yr	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	267805	49365	434742	374442	442559	410965	444105
# districts	247	247	247	247	247	247	247

Note: The dependent variable is the natural logarithm of salary per year, in full-time equivalency units and multiplied by 100. The variable *Female* equals one for female workers and the variable *Post Extension* equals one for years following the expiration of a CBA or its extension. *Controls* include fixed effects for the district, number of years of experience, highest education degree, grade level (elementary, middle, high), and subject (math, reading, and others), alone and interacted with an indicator for years after the extension of a CBA. All specifications also include year fixed effects interacted with CBA expiration and extension year effects. Column 1 is estimated on a balanced sample of teachers in the 4 years before and after each expiration; column 2 is estimated on the subsample of teachers who enter Wisconsin public schools between 2007 and 2011; column 3 includes teacher fixed effects; column 4 assigns teachers to the districts where they were teaching in 2011; column 5 controls for indicators for years of experience and highest education degree, interacted with district fixed effects and for an indicator for years after the extension of a CBA; column 6 is estimated on the subsample of full-time teachers (FTE = 100); and column 7 controls for a teacher's highest and lowest grade, alone and interacted for an indicator for the years following a CBA expiration. All columns present OLS estimates. Standard errors in parentheses are clustered at the district level. $^{*} \leq 0.1$, $^{**} \leq 0.05$, $^{***} \leq 0.01$.

Table VI: Survey answers: Means, women vs men, and differences in means

	Women	Men	Difference	Std. Error
Have you ever negotiatied...				
w/prev employer	0.295	0.379	-0.084***	(0.019)
w/current employer, at start	0.223	0.306	-0.083***	(0.018)
w/current employer, after start	0.205	0.245	-0.040**	(0.017)
Have you ever negotiatied, male superintendent				
w/current employer, at start	0.223	0.315	-0.093***	(0.021)
w/current employer, after start	0.204	0.273	-0.069***	(0.020)
Have you ever negotiatied, female superintendent				
w/current employer, at start	0.216	0.285	-0.069**	(0.032)
w/current employer, after start	0.200	0.191	0.008	(0.031)
If yes, was the negotiation successful?				
w/prev employer	0.819	0.904	-0.085***	(0.025)
w/current employer, at start	0.709	0.814	-0.105***	(0.034)
w/current employer, after start	0.455	0.572	-0.117***	(0.042)
Why did you not negotiate? (current employer, at start)				
it was not possible	0.419	0.451	-0.032	(0.020)
I was not comfortable doing so	0.233	0.128	0.105***	(0.016)
It was useless	0.084	0.063	0.022**	(0.011)
I feared backlash	0.065	0.055	0.011	(0.010)
I was satisfied w/pay	0.186	0.149	0.036**	(0.015)
Average likelihood that you will negotiate...				
salary	3.365	3.889	-0.524***	(0.121)
classroom assignment	4.752	4.539	0.213	(0.130)
non-teaching duties	4.347	4.579	-0.232*	(0.124)
Average likelihood that you will negotiate, male superintendent				
salary	3.233	3.996	-0.764***	(0.143)
classroom assignment	4.652	4.449	0.202	(0.157)
non-teaching duties	4.215	4.509	-0.293**	(0.148)
Average likelihood that you will negotiate, female superintendent				
salary	3.556	3.667	-0.110	(0.229)
classroom assignment	4.922	4.714	0.209	(0.237)
non-teaching duties	4.581	4.724	-0.143	(0.231)
Share agreeing w/statements				
I worked in other industries	0.476	0.503	-0.027	(0.020)
I know someone who negotiated their pay	0.505	0.590	-0.085***	(0.020)
I know my colleagues' pay	0.275	0.387	-0.111***	(0.019)
I am confident talking to people I don't know	0.728	0.839	-0.110***	(0.017)
I can read subtle signals	0.890	0.884	0.006	(0.013)
I can read people's feelings	0.871	0.861	0.010	(0.014)
I have good people's skills	0.888	0.883	0.006	(0.013)
My performance is above the mean	0.321	0.364	-0.044**	(0.019)
N (teachers)	2190	843		

Note: Mean survey answers by genders and differences between men and women, along with standard errors (in parentheses). The options under *Why did you not negotiate* are non-mutually exclusive choices offered to the respondents. Appendix D contains the full list of questions. $^{*} \leq 0.1$, $^{**} \leq 0.05$, $^{***} \leq 0.01$.

Table VII: Survey answers: Negotiating behavior

Panel A) Past and future negotiations

	Current employer				Likelihood future negotiations			
	(1) At start	(2) At start	(3) After	(4) After	(5) Salary	(6) Salary	(7) Classroom assgn	(8) Non-teaching duties
Female	-0.071***	-0.083***	-0.028	-0.057***	-0.475***	-0.718***	0.273*	-0.135
	(0.022)	(0.025)	(0.018)	(0.020)	(0.162)	(0.151)	(0.139)	(0.133)
Female * F super		0.038		0.086**		0.746**		
		(0.046)		(0.035)		(0.354)		
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	2836	2784	2836	2784	2836	2784	2836	2836
Y mean, males	0.306		0.245		3.889		4.539	4.579

Panel B) Negotiated successfully or reasons for not negotiating

	Successful negotiation			Reasons for not negotiating				
	(1) Cond	(2) Uncond	(3) Cond	(4) Not possible	(5) Not comfortable	(6) Useless	(7) Fear backlash	(8) Satisfied w/pay
Female	-0.132**	-0.082***	-0.087	-0.023	0.065**	0.024	0.005	-0.040*
	(0.052)	(0.020)	(0.057)	(0.028)	(0.029)	(0.025)	(0.019)	(0.022)
Female * F super			-0.134					
			(0.119)					
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	700	2836	682	2222	2222	2222	2222	2222
Y mean, males	0.904	0.249	0.814	0.565	0.210	0.215	0.131	0.189

Note: In panel A, the dependent variable equals one if a teacher negotiated their salary with the current employer at the start of the work relationship (columns 1-2), or after the start (columns 3-4); or the likelihood, measured as a number from 0 to 1, that the teacher will negotiate either salaries (columns 5-6), classroom assignment (column 7), or non-teaching duties (column 8) in the future. In panel B the dependent variable equals one if a teacher believed the negotiation with her current employer (at the start of the work relationship or after the start) was successful, conditional on negotiating (columns 1 and 3) or unconditionally (column 2); or if a teacher gives the corresponding reason as a motive for not negotiating (conditional on not doing so, columns 4-8). *Female* is an indicator for female teachers. *F super* equals one for teachers in districts run by female superintendents. All specifications include controls for age class, self-reported job performance (above/below average), measures of people skills, an indicator for whether the respondent knows someone who negotiated salary, an indicator for whether the respondent knows his/her colleagues' salaries, and district fixed effects. Standard errors in parentheses are clustered at the district level. $^{*} \leq 0.1$, $^{**} \leq 0.05$, $^{***} \leq 0.01$.

Table VIII: Gender pay gap and teacher value-added

	Without VA	With VA		
	(1)	(2)	(3)	(4)
Female	-0.225*	0.281	0.281	0.283
	(0.131)	(0.188)	(0.188)	(0.188)
Female × Post Extension	-0.326***	-0.062	-0.066	-0.070
	(0.117)	(0.235)	(0.235)	(0.235)
VA (sd)			0.016	-0.100
			(0.060)	(0.115)
VA × Post Extension			0.131	
			(0.118)	
Female × VA				0.145
				(0.119)
Male × VA × Post Extension				0.556**
				(0.240)
Female × VA × Post Extension				0.027
				(0.123)
Controls	Yes	Yes	Yes	Yes
Yr × Exp yr	Yes	Yes	Yes	Yes
N	341608	102499	102499	102499
# districts	247	247	247	247

Note: The dependent variable is the natural logarithm of salary per year, in full-time equivalency units and multiplied by 100. The variables *Female* and *Male* equal one for female and male workers, respectively, and the variable *Post Extension* equals one for years following the expiration of a CBA or its extension. *VA* is teacher value-added, described in Section 3.1 and standardized to have mean zero and variance one. *Controls* include fixed effects for the district, number of years of experience, highest education degree, grade level (elementary, middle, high), and subject (math, reading, and others), alone and interacted with an indicator for years after the extension of a CBA. All specifications also include year fixed effects interacted with CBA expiration and extension year effects. All columns present OLS estimates. Column 1 is estimated on the subsample of teachers without a VA estimate; columns 2-4 are estimated on the subsample of teachers with VA estimates, where we remove teachers with VA in the top and bottom one percent of the distribution. Standard errors in parentheses are clustered at the district level. $^{*} \leq 0.1$, $^{**} \leq 0.05$, $^{***} \leq 0.01$.

Table IX: Gender differences in job mobility

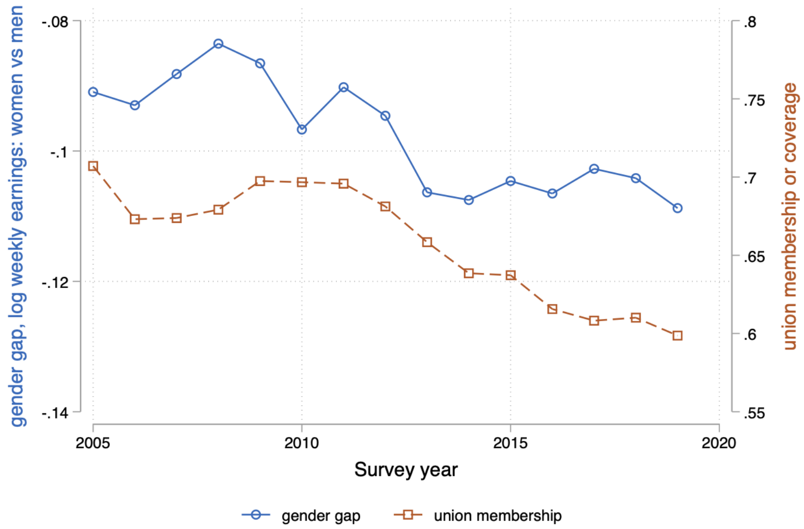
	All moves			Within CZ	Across CZ
	(1)	(2)	(3)	(4)	(5)
Female	-0.0014**	-0.0012**	-0.0009	0.0004	-0.0013**
	(0.0006)	(0.0005)	(0.0007)	(0.0003)	(0.0005)
Post Extension	0.0179***	0.0036	0.0007	0.0024*	-0.0012
	(0.0012)	(0.0023)	(0.0028)	(0.0014)	(0.0018)
Female × Post Extension	-0.0004	-0.0004	-0.0010	-0.0006	-0.0007
	(0.0011)	(0.0012)	(0.0011)	(0.0008)	(0.0008)
District FE	No	Yes	Yes	Yes	Yes
Year FE	No	Yes	Yes	Yes	Yes
Exp, edu FE	No	No	Yes	Yes	Yes
N	413920	413920	413801	418383	412931
# districts	247	247	247	247	247
Mean of dep. var.	0.0236	0.0236	0.0236	0.0102	0.0112

Note: The dependent variable is an indicator for a teacher changing district (columns 1-3), changing district within the same CZ (column 4), and changing district *and* CZ in a given year (column 5). The variable *Female* equals one for female teachers and the variable *Post Extension* equals one for years following the expiration of a CBA or its extension. Columns 2-5 include district and year fixed effects; columns 3-5 also include fixed effects for years of experience and for the highest education degree. Standard errors in parentheses are clustered at the district level. $^{*} \leq 0.1$, $^{**} \leq 0.05$, $^{***} \leq 0.01$.

Figures

Figure I: Gender wage gap and unionization

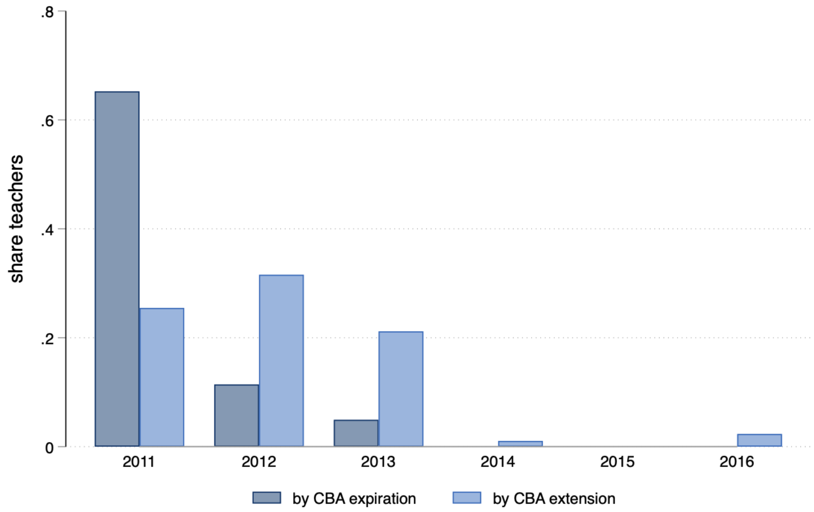
(a) The gender wage gap and union membership over time (public school teachers)



(b) Gender wage gaps and union membership (public school teachers and other employees)

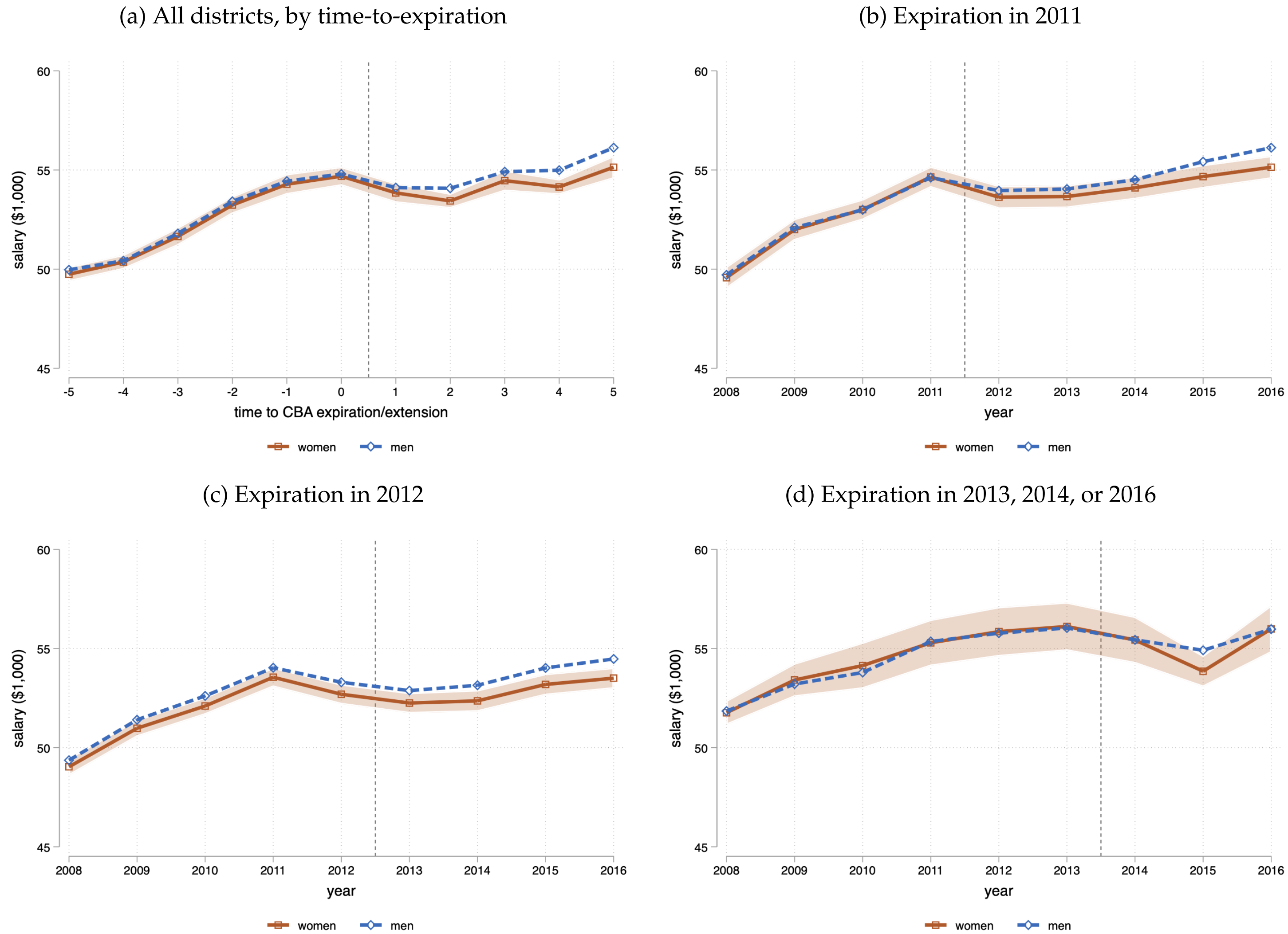
Note: Panel (a): the solid line shows the gender wage gap for public school teachers, estimated as the coefficient on an indicator for *Female* in yearly regressions of the log of weekly earnings on age-by-education fixed effects (where age is measured in two-year bins and education is measured with indicators for having a BA or a Master's degree) and state-by-year fixed effects. The dashed line shows the share of public school teachers who are either members of or covered by a union. Panel (b): binned scatterplot of the gender gap and union membership. The former is the male-female difference in salary residuals, obtained conditioning on industry-by-occupation-by-sector-by-state-by-year and age-by-education fixed effects and calculated within each industry-occupation-sector-state-year cell; the latter is the share of workers in each cell which are either members of or covered by a union. Estimates are obtained using data from the Current Population Survey.

Figure II: Share of teachers, by expiration and extension dates of CBAs



Note: The figure shows the share of teachers covered by collective bargaining agreements (CBAs) with different expiration dates. The darker bars show the share of teachers covered by a CBA that was originally supposed to expire in 2011, 2012, and 2013. The lighter bars show the share of teachers covered by a CBA whose validity was extended until 2011, 2012, 2013, 2014, or 2016 (for districts that did not extend the validity of the CBA, we use the expiration date).

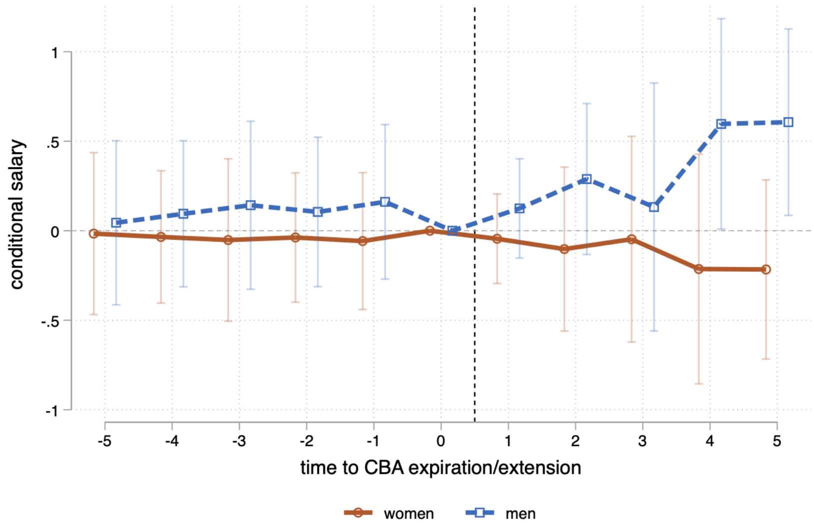
Figure III: Raw salaries of men and women



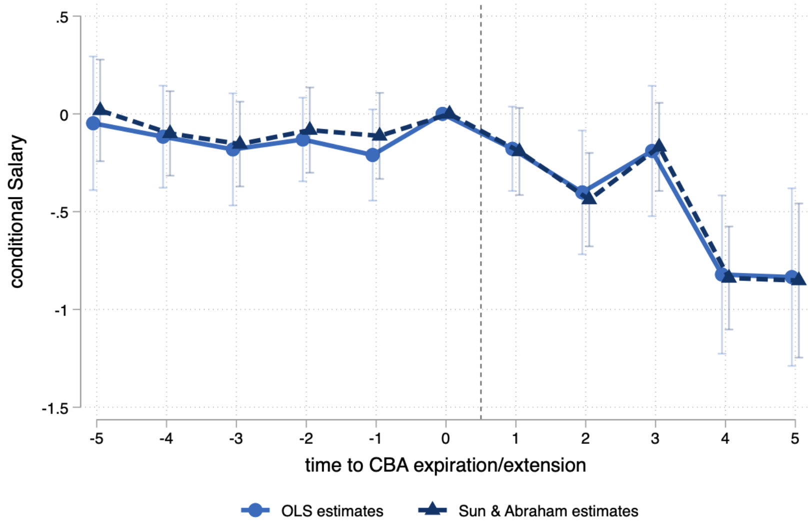
Note: Panel (a) shows the unconditional salaries of male and female teachers relative to the year a CBA or its extension expired (denoted by $t = 0$.) Panels (b)-(d) show unconditional salaries of male and female teachers, by year and separately by time of expiration/extension of districts' CBAs. Shaded areas represent confidence intervals for the female-male difference in salaries.

Figure IV: Flexible pay and the gender wage gap

(a) Salaries of men and women, by time-to-expiration/extension of CBAs



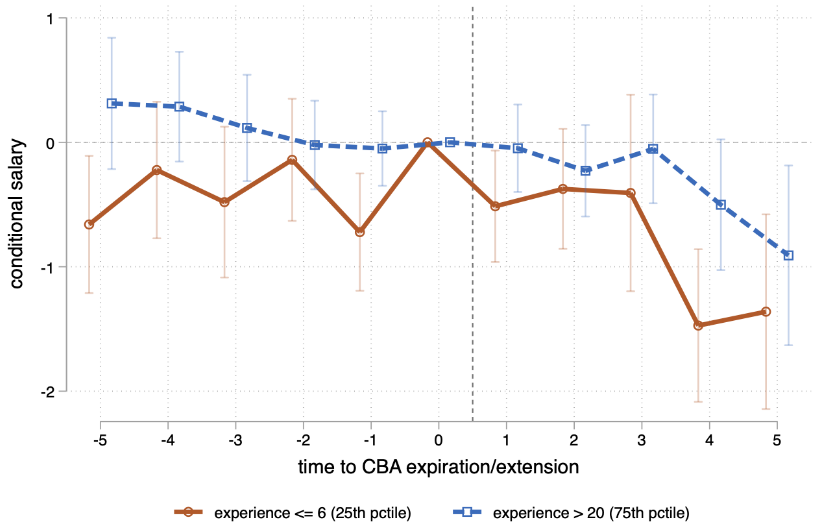
(b) Gender gap in salaries, by time-to-CBA expiration/extension



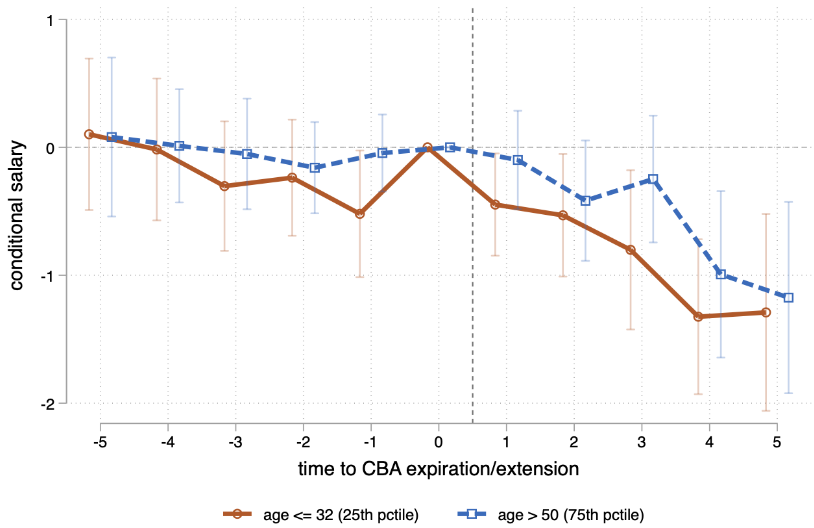
Note: Panel (a) shows OLS point estimates and 90% confidence intervals of the coefficients δ_s^g in equation (1), for g =female (solid line) and g =male (dashed line). Panel (b) shows point estimates and 90% confidence intervals of the coefficients δ_s in equation (2). The solid line shows OLS estimates. The dashed line shows estimates obtained using the method outlined in Sun and Abraham (2020). The procedure used to obtain these estimates is outlined in Appendix C. All coefficients are plotted relative to the year a CBA or its extension expired ($t = 0$). Standard errors are clustered at the district level.

Figure V: Gender pay gap, by seniority and age

(a) By Seniority

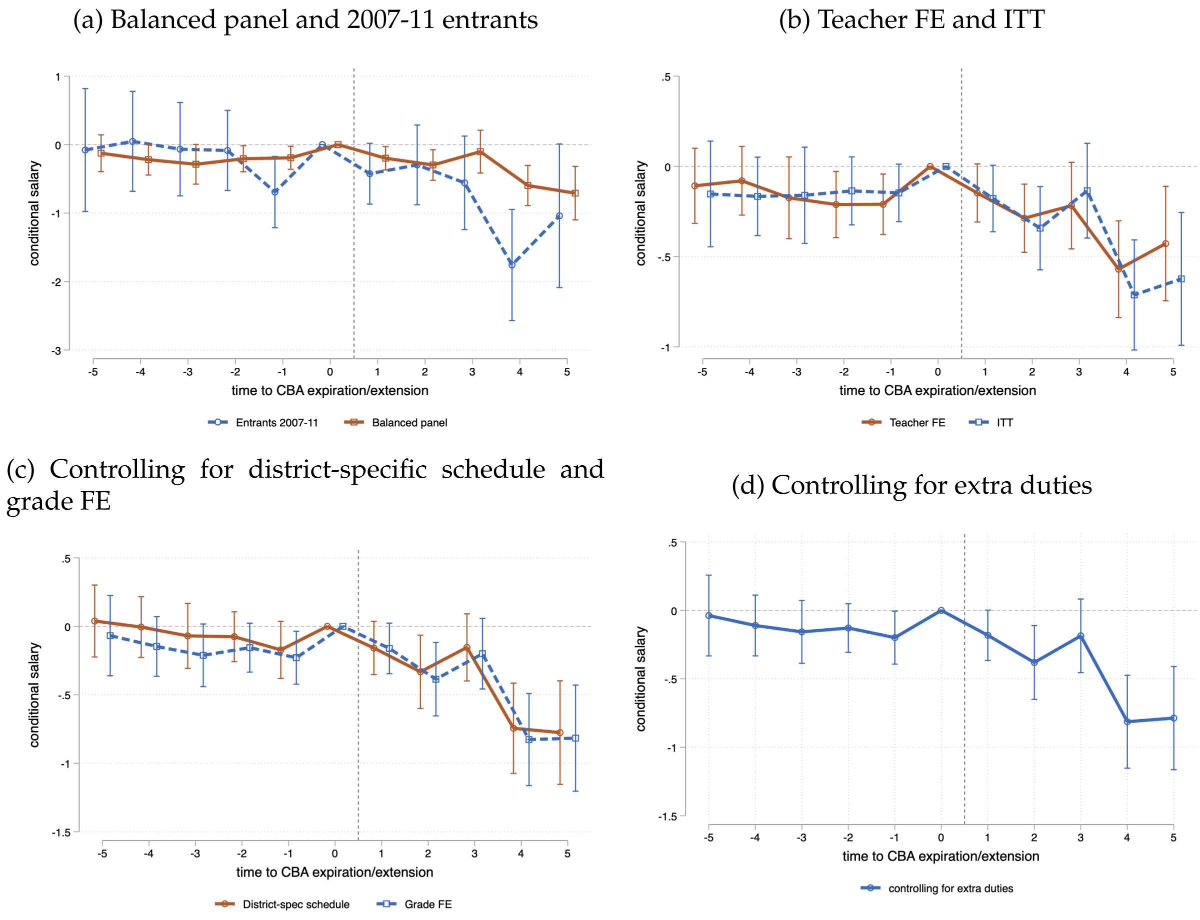


(b) By Age



Note: Panel (a) shows OLS point estimates and 90% confidence intervals of the coefficients δ_s in equation (2), estimated separately for teachers with six or fewer (solid line) and more than 20 years of seniority (dashed line). Panel (b) shows OLS point estimates and 90% confidence intervals of the coefficients δ_s in equation (2), estimated separately for teachers aged 32 and younger (solid line) and those older than 50 (dashed line). All coefficients are plotted relative to the year a CBA or its extension expired ($t = 0$). Standard errors are clustered at the district level.

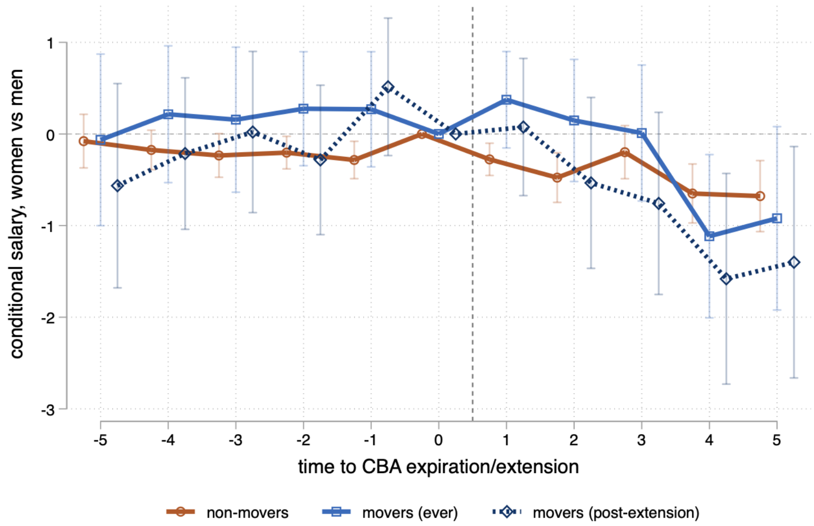
Figure VI: Gender gap in salaries: Robustness tests



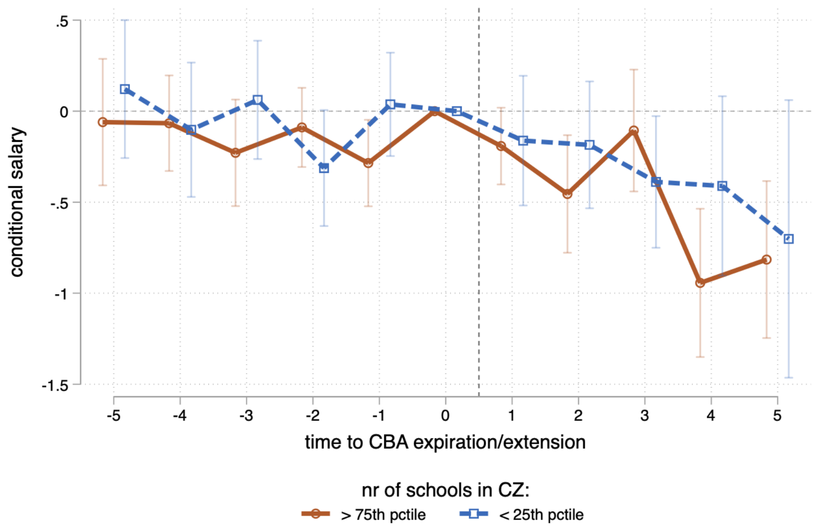
Note: OLS point estimates and 90% confidence intervals of the coefficients δ_s in equation (2) obtained using a balanced panel (panel (a), solid line); a panel of teachers who entered Wisconsin public schools between 2007 and 2011 (panel (a), dashed line); teacher fixed effects (panel (b), solid line); assigning teachers to their pre-CBA expiration district (panel (b), dashed line); controlling for district-specific experience and education indicators (panel (c), solid line); controlling for grade fixed effects, alone and interacted for an indicator for years following a CBA expiration (panel (c), dashed line); and controlling for an indicator for extra duties, alone and interacted for an indicator for years following a CBA expiration (panel (d)). All coefficients are plotted relative to the year a CBA or its extension expired ($t = 0$). Standard errors are clustered at the district level.

Figure VII: Gender pay gap and job mobility

(a) Movers vs non-movers



(b) Gender pay gap and and outside options



Note: Panel (a) shows OLS point estimates and 90% confidence intervals of the coefficients δ_s in equation (2), estimated separately for teachers who never move between 2007 and 2016 ("non-movers"), those who move at least once ("movers (ever)"), and those who move at least once after a CBA expiration ("movers (post-extension)"). Panel (b) shows OLS point estimates and 90% confidence intervals of the coefficients δ_s in equation (2), estimated separately for teachers in commuting zones with a small number of schools (below the 25th percentile of the distribution) and a large number of schools (above the 75th percentile). All coefficients are plotted relative to the year a CBA or its extension expired ($t = 0$). Standard errors are clustered at the district level.

Flexible Wages, Bargaining, and the Gender Gap

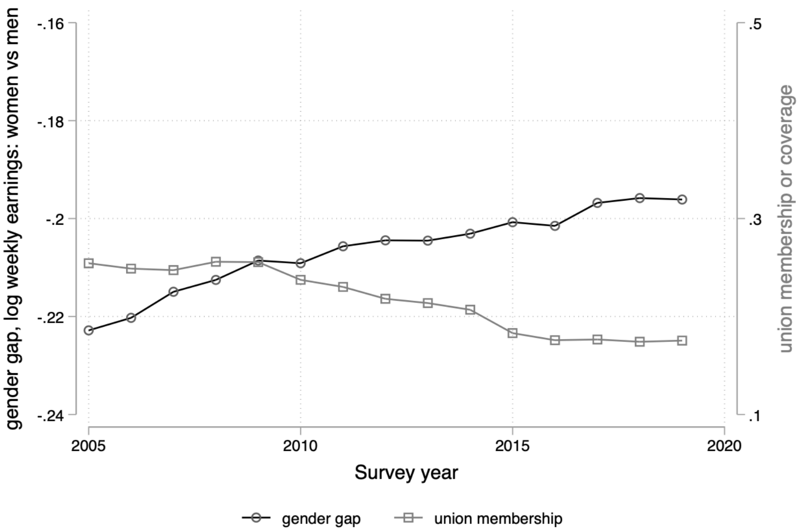
Barbara Biasi and Heather Sarsons

Appendix – For online publication only

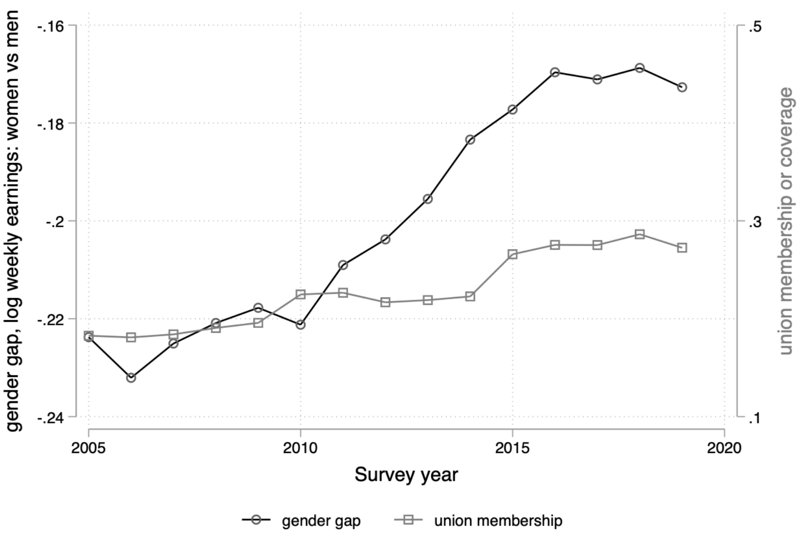
Appendix A Additional Tables and Figures

Figure AI: Trends in the gender wage gap and union membership

(a) Industries, occupations, sectors, and states with a unionization decline

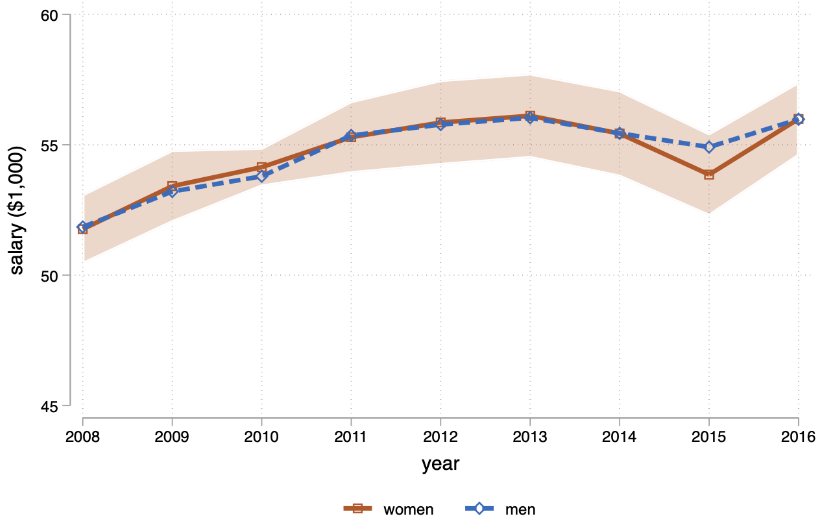


(b) Industries, occupations, sectors, and states with a unionization increase



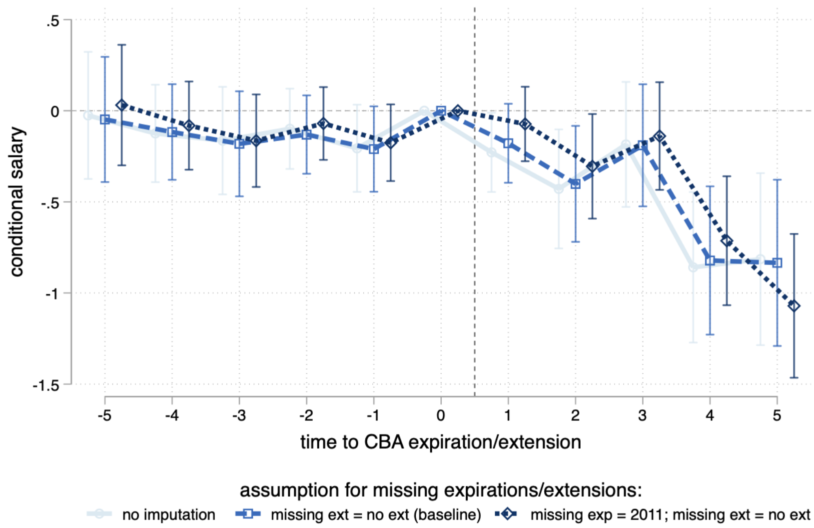
Note: In all panels, the solid line shows the gender wage gap for all workers, estimated as the coefficient on an indicator for *Female* in yearly regressions of the log of weekly earnings on age-by-education fixed effects (where age is measured in two-year bins and education is measured with indicators for having a BA or a Master's degree) and state-by-occupation-by-sector-by-industry-by-year fixed effects, where occupations and industries are grouped using one-digit SOC and NAICS codes, respectively, and sector is either public or private. The dashed line shows the share of workers who are either covered by a union or members of a union. Panel (a) shows estimates for occupation-industry-sector-state cells that experienced at least a 5 percent decline in unionization between the 2005-10 period and the 2015-19 period; panel (b) shows estimates for occupation-industry-sector-state cells that experienced at least a 5 percent increase in unionization over the same time period. Estimates are obtained using data from the Current Population Survey.

Figure AII: Unconditional salaries of men and women - Districts with missing CBA expiration date



Note: Unconditional salaries of male and female teachers by year, for districts with missing CBA expiration dates. Shaded areas represent confidence intervals for the female-male difference in salaries.

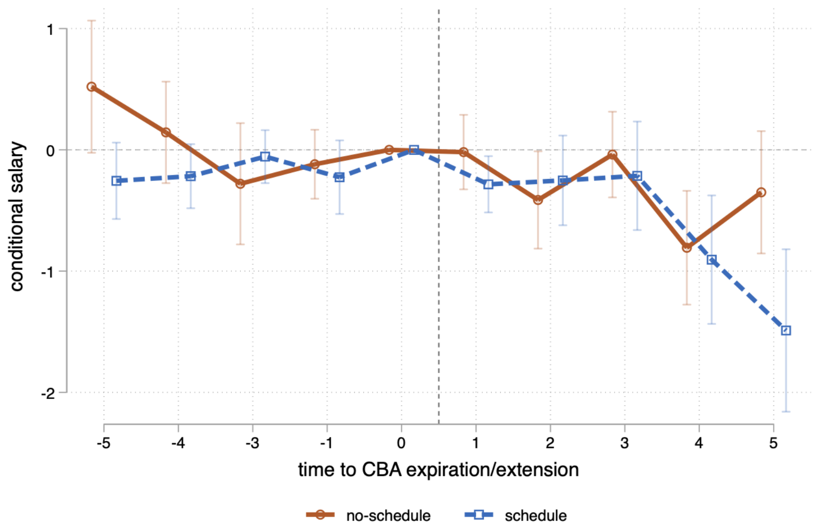
Figure AIII: Gender gap in salaries: Assumptions on districts with missing CBA expiration/extension dates



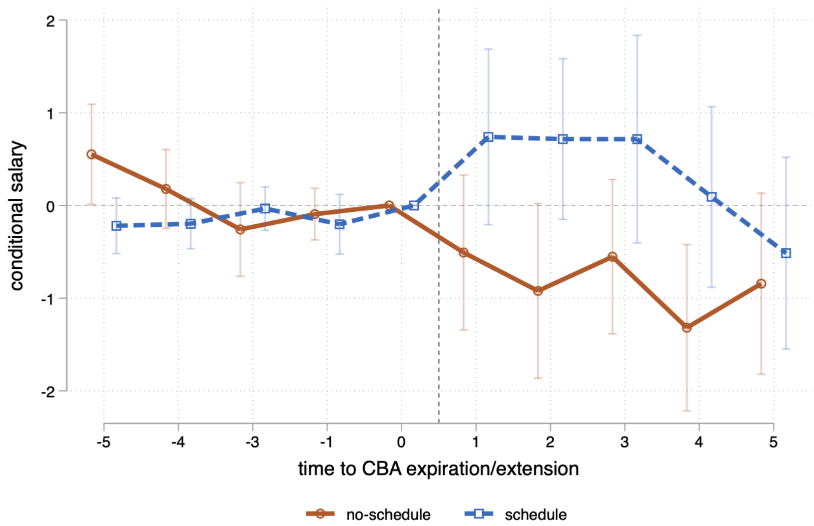
Note: The figure shows OLS point estimates and 90% confidence intervals of the coefficients δ_s in equation (2), obtained either (a) excluding districts with missing CBA expiration/extension dates ("no imputation"); (b) excluding districts with missing CBA expiration dates and assuming districts with missing extension did not have an extension ("missing ext = no ext (baseline)"; and (c) assuming that districts with missing CBA expiration had an agreement expiring in 2011, and that districts with missing extension did not have an extension ("missing exp = 2011; missing ext = no ext"). All coefficients are plotted relative to the year a CBA or its extension expired ($t = 0$). Standard errors are clustered at the district level.

Figure AIV: Gender gap in salaries, by time to expiration/extension of CBAs and district type

(a) Baseline



(b) With gender-specific experience returns, for teachers with 3-4 years of experience and a master's degree



Note: OLS point estimates and 90% confidence intervals of the coefficients δ_s in equation (2), estimated and shown separately for schedule and no-schedule districts. In the bottom panel, we further control for experience and education fixed effects interacted with $Female$ and with an indicator for years following a CBA expiration/extension; the coefficients refer to teachers with 3 or 4 years of experience and a master's degree. All coefficients are plotted relative to the year a CBA or its extension expired ($t = 0$). Standard errors are clustered at the district level.

Figure AV: Employee handbooks and teacher pay: Madison Public Schools, 2019-20 employee handbook

1.1 Salary Schedule

1. The basic salaries of employees covered by this Handbook are set forth in Appendix 1 which is attached to and incorporated in this Handbook.

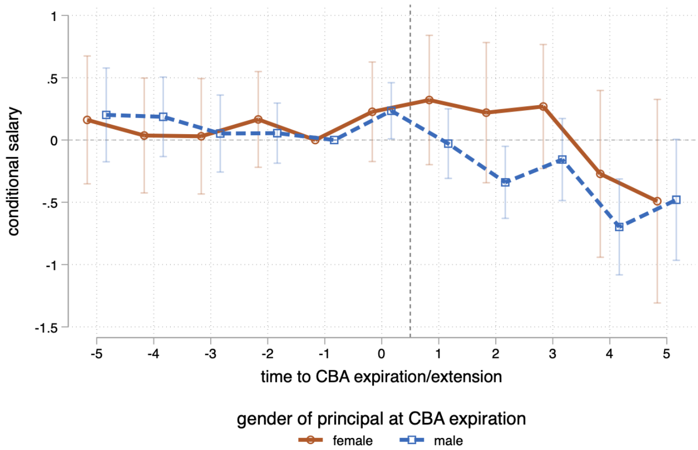
1.2 Initial Salary Schedule Placement/Signing Bonuses

The District has the sole discretion to determine initial placement on the salary schedule.

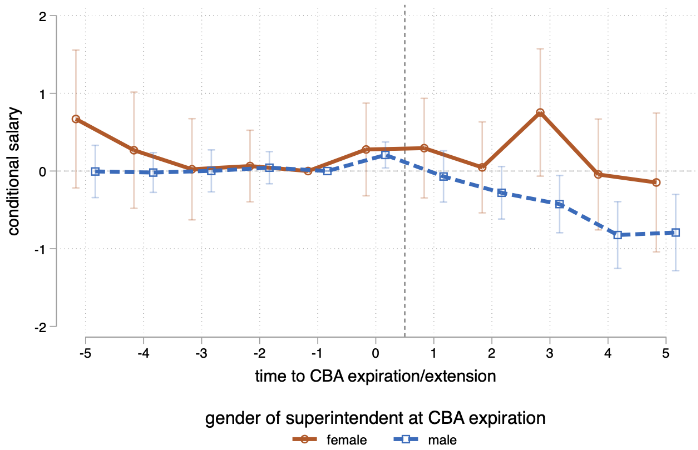
Note: Snapshots from the Madison Public School District's 2019-2020 employee handbook, available on page 85 of Madison Metropolitan School District (2019).

Figure AVI: Gender gap in salaries and the gender of school and district management

(a) Gender of school principals



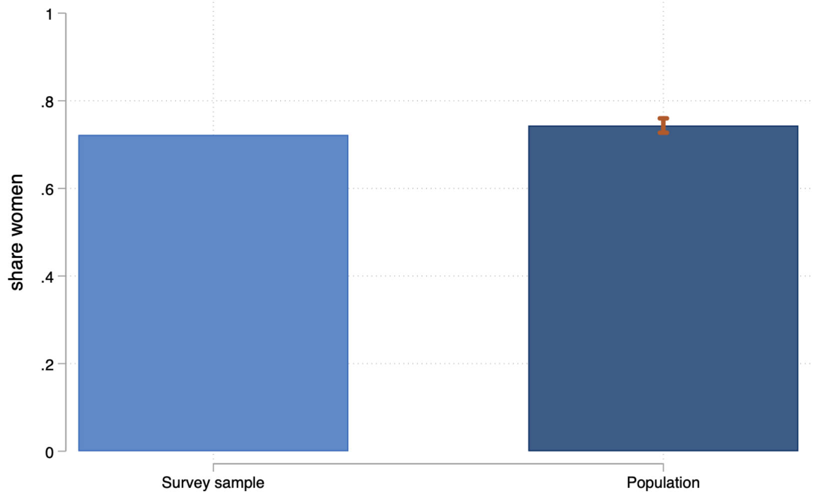
(b) Gender of district superintedents



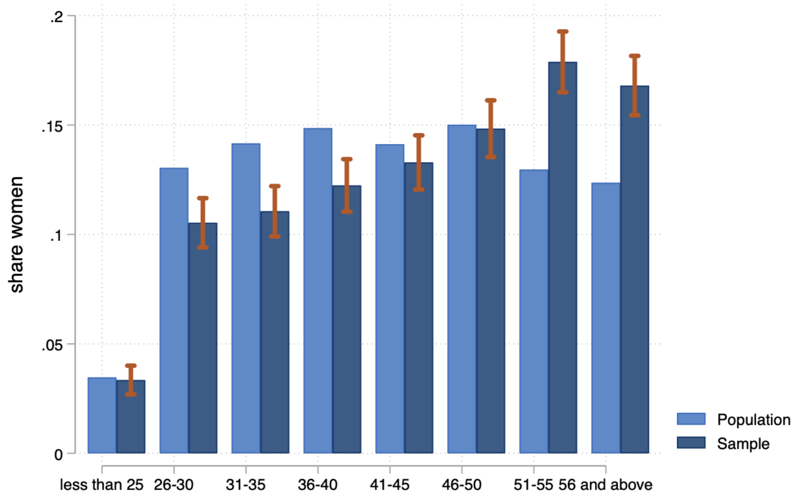
Note: OLS point estimates and 90% confidence intervals of the coefficients δ_s in equation (2), estimated separately for teachers in a school with a female vs male principal (panel (a)) and teachers in a district with a female vs male superintendent (panel (b)) in any given year. All coefficients are plotted relative to the year a CBA or its extension expired ($t = 0$). Standard errors are clustered at the district level.

Figure AVII: Survey sample vs. population

(a) Gender (share female)



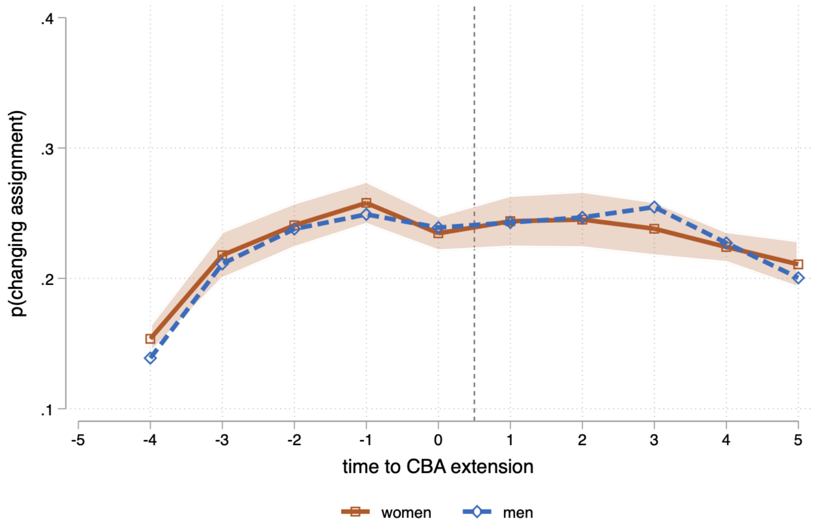
(b) Age groups



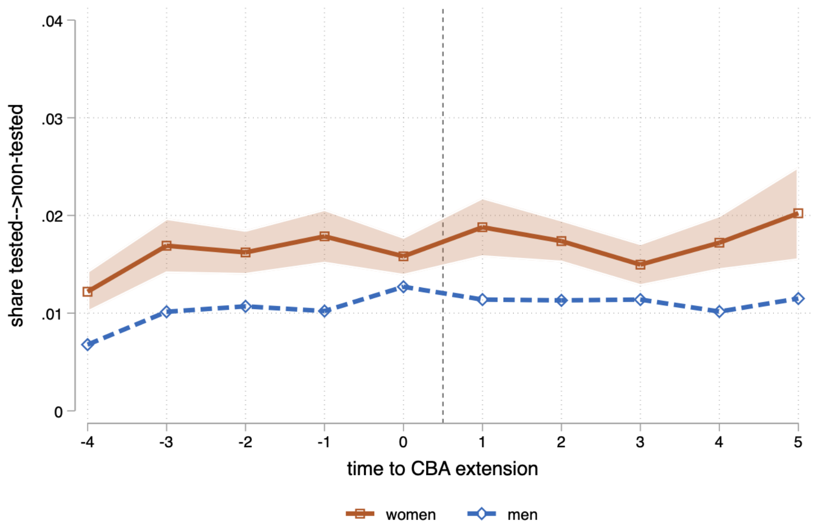
Note: Panel (a) shows the share of female teachers in the survey sample and in the population in 2016. Panel (b) shows the share of teachers in each age group, in the survey sample and in the population in 2016. Spikes represent confidence intervals for the difference in mean shares across groups. Standard errors are clustered at the district level.

Figure AVIII: Switches across teaching posts, by gender

(a) Share of teachers who switch teaching post, by gender



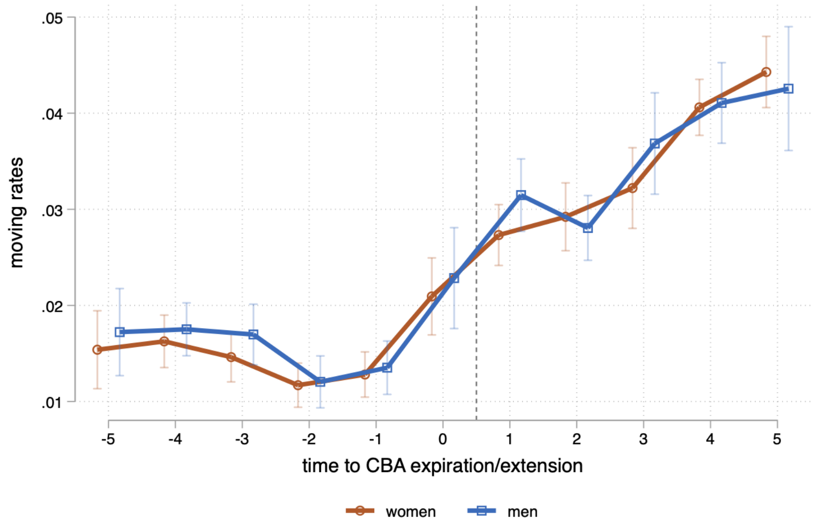
(b) Share of teachers who switch from a tested to a non-tested post, by gender



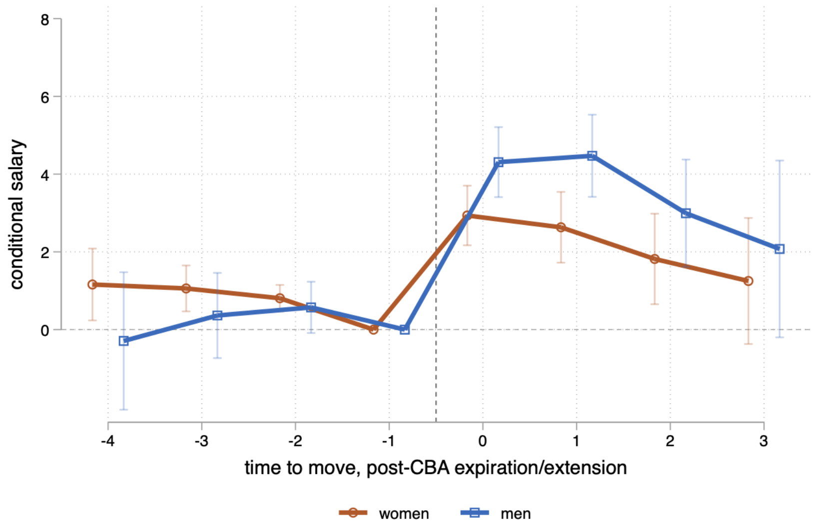
Note: Panel (a) shows the share of teachers who switch teaching position (i.e., grade or subject), by time-to-CBA expiration and gender. Panel (b) panel shows the share of teachers who switch from a tested to a non-tested position, by time-to-CBA expiration and gender. Shaded areas represent confidence intervals for the female-male difference in the shares.

Figure AIX: Gender differences in mobility

(a) Mobility rates, men and women



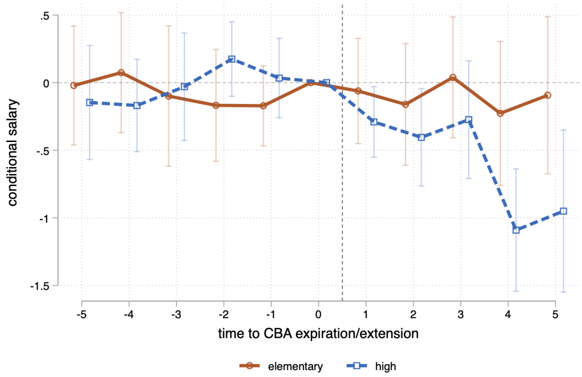
(b) Conditional salaries around a district move



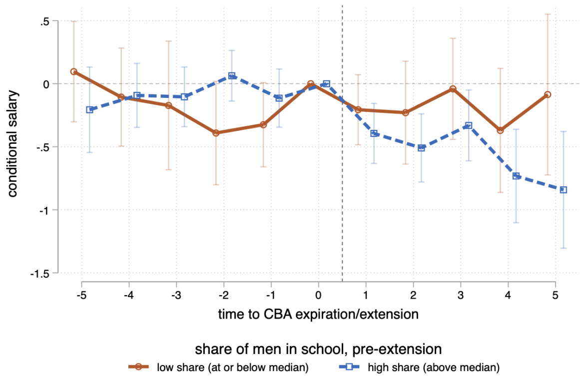
Note: Panel (a) shows the share of teachers who change district in a given year, by time-to-expiration of a district's CBA or its extension and separately for men and women. Panel (b) shows OLS point estimates and 90% confidence intervals of event study coefficients of conditional salaries around each move, separately for male and female teachers. All coefficients are plotted relative to the year a CBA or its extension expired ($t = 0$). Standard errors are clustered at the district level.

Figure AX: Gender gap in salaries: by proxies for demand for men

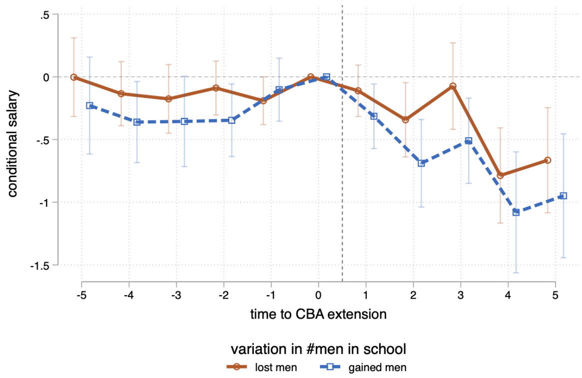
(a) Elementary vs. high school teachers



(b) By share of men in a school



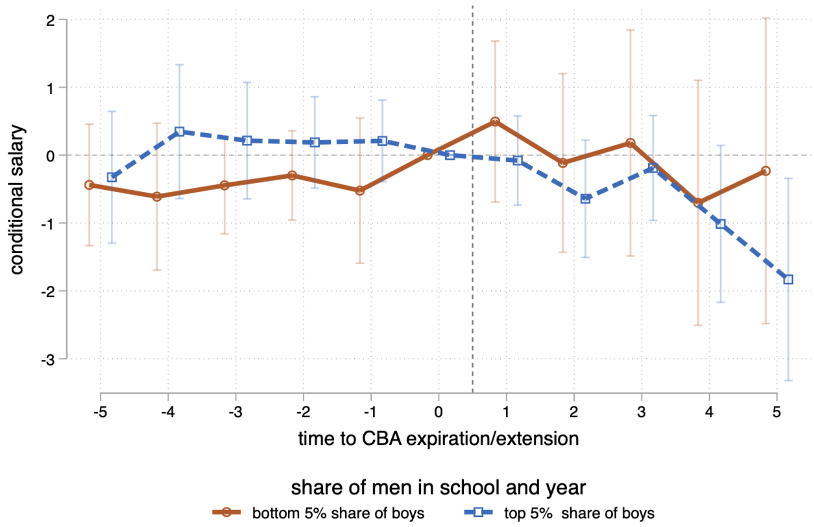
(c) Schools that gained vs. lost men before Act 10



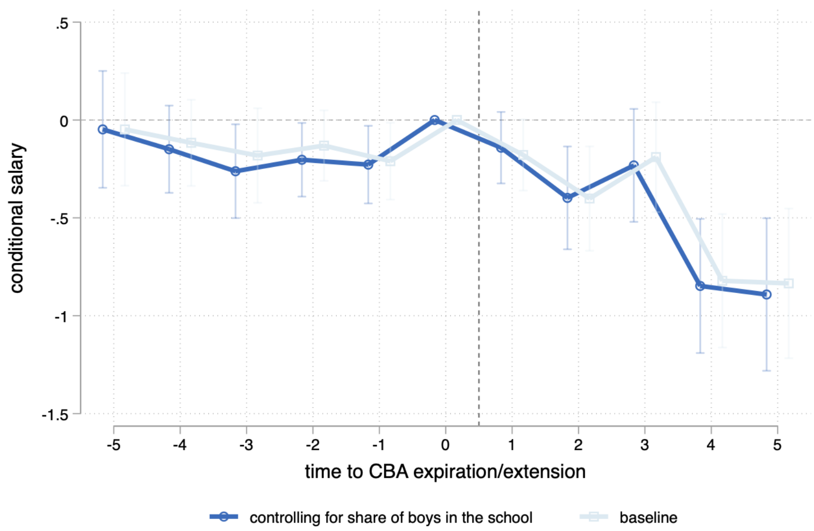
Note: Panel (a) shows OLS point estimates and 90% confidence intervals of the coefficients δ_s in equation (2), estimated separately for teachers in elementary school (solid line) and in high school (dashed line). Panel (b) shows OLS point estimates and 90% confidence intervals of the coefficients δ_s, estimated separately for teachers in schools in the top quartile of the share of men (i.e., with more than 30 percent of men, solid line), and teachers in all other schools (dashed line). Panel (c) shows OLS point estimates and 90% confidence intervals of the coefficients δ_s in equation (2), estimated separately for teachers in schools whose share of male teachers declined by more than 2 percentage points (schools which "lost men") and schools whose share of male teachers increased (schools which "gained men") between 2007 and 2011. All coefficients are plotted relative to the year a CBA or its extension expired ($t = 0$). Standard errors are clustered at the district level.

Figure AXI: Gender gap in salaries and share of boys in school

(a) Gender gap by share of boys in school



(b) Gender gap controlling for share of boys in school



Note: Panel A: OLS point estimates and 90% confidence intervals of the coefficients δ_s in equation (2), estimated separately for teachers in schools in the top and bottom 5 percent of the share of boys. Panel B: OLS point estimates and 90% confidence intervals of the coefficients δ_s in equation (2), controlling for the share of boys in each school (alone and interacted with an indicator for years after a CBA expiration). "Baseline" refers to the gap estimated on all schools. All coefficients are plotted relative to the year a CBA or its extension expired ($t = 0$). Standard errors are clustered at the district level.

Table AI: District characteristics, CBA expiration dates, and extensions: Differences

	Expiration post 2011 vs in 2011			W/ extension vs w/out			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
District chars.	Difference	SE	P-value	Difference	SE	P-value	N
Enrollment	2490.919	2391.640	0.299	2016.876	751.330	0.008	245
N teachers	181.118	161.522	0.263	140.510	50.305	0.006	247
Per pupil expenditure	0.333	0.528	0.528	-0.211	0.358	0.557	245
Share black students	0.027	0.021	0.195	0.011	0.008	0.141	245
Share disadv. students	0.040	0.030	0.181	0.014	0.018	0.440	245
In urban area	-0.029	0.077	0.709	-0.037	0.053	0.482	247
In suburban area	0.078	0.062	0.211	0.070	0.032	0.030	247

Note: The table shows the estimates ("Difference"), robust standard errors, and p-values from OLS regressions of each district characteristic listed in the first column (measured in 2011) on a dummy variable indicating that a CBA expiration occurred after 2011 (columns 1-3) and a dummy variable indicating that a district received an extension (columns 4-6). Each observation is a school district. The number of observations are shown in column 8 and are the same for the post 2011 comparisons and the extensions comparisons.

Table AII: Gender gap in salaries before and after a CBA expiration/extension

Panel A: Before a CBA expiration/extension

	(1)	(2)	(3)	(4)	(5)
Female	-0.756*	-0.486***	-0.392***	-0.104	-0.074
	(0.398)	(0.155)	(0.134)	(0.113)	(0.115)
Distr and year FE	Yes	Yes	Yes	Yes	Yes
Experience FE	No	Yes	Yes	Yes	Yes
Education FE	No	No	Yes	Yes	Yes
Grade level FE	No	No	No	Yes	Yes
Subject	No	No	No	No	Yes
N	230334	230330	230218	230218	230218
# districts	247	247	247	247	247

Panel B: After a CBA expiration/extension

	(1)	(2)	(3)	(4)	(5)
Female	-1.343***	-0.685***	-0.699***	-0.355***	-0.327***
	(0.352)	(0.142)	(0.128)	(0.120)	(0.122)
Distr and year FE	Yes	Yes	Yes	Yes	Yes
Experience FE	No	Yes	Yes	Yes	Yes
Education FE	No	No	Yes	Yes	Yes
Grade level FE	No	No	No	Yes	Yes
Subject	No	No	No	No	Yes
N	172808	172803	172802	172802	172802
# districts	245	245	245	245	245

Note: The table shows how the gender salary gap changes as we control for observable characteristics that enter districts' salary schedules, separately for years before (panel A) and after (panel B) each CBA expiration/extension. The dependent variable is the natural logarithm of salary per year, in full-time equivalency units and multiplied by 100. The variable *Female* equals one for female workers. All specifications include district and year fixed effects; columns 2-5 include years of experience fixed effects, columns 3-5 include fixed effects for the highest education degree, columns 4-5 include fixed effects for the school level (elementary, middle, high school), and column 5 includes fixed effects for subjects taught. Standard errors in parentheses are clustered at the district level. $^{*} \leq 0.1$, $^{**} \leq 0.05$, $^{***} \leq 0.01$.

Table AIII: Gender pay gap after a CBA expiration/extension, by principal and superintendent gender. Assigning teachers to principals and superintendents in 2011

	Principal			Superintendent		
	(1)	(2)	(3)	(4)	(5)	(6)
	Male	Female		Male	Female	
Female	-0.109	-0.043	-0.092	-0.085	-0.182	-0.276
	(0.115)	(0.205)	(0.183)	(0.127)	(0.187)	(0.207)
Female × Post Extension	-0.348***	0.119	0.106	-0.238**	-0.095	-0.108
	(0.108)	(0.199)	(0.196)	(0.111)	(0.205)	(0.232)
Female × Male princ			-0.000			
			(0.181)			
Female × Male princ × Post			-0.447**			
			(0.207)			
Female × Male super						0.206
						(0.251)
Female × Male super × Post						-0.140
						(0.263)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Yr × Exp yr	Yes	Yes	Yes	Yes	Yes	Yes
N	253854	132329	386204	335066	53369	388487
# districts	247	234	247	247	198	247

Note: The dependent variable is the natural logarithm of salary per year, in full-time equivalency units and multiplied by 100. The variable *Female* equals one for female workers and the variable *Post Extension* equals one for years following the expiration of a CBA or its extension. The variables *Male princ* and *Male super* equal one for teachers in schools with a male principal and in districts with a male superintendent, respectively, in 2011. *Controls* include fixed effects for the district, number of years of experience, highest education degree, grade level (elementary, middle, high), and subject (math, reading, and others), alone and interacted with an indicator for years after the extension of a CBA. All specifications also include year fixed effects interacted with CBA expiration and extension year effects. All columns present OLS estimates. Column 1 is estimated on teachers in schools with a male principal, column 2 on teachers in schools with a female principal, column 4 on teachers in districts with a male superintendent, and column 5 on teachers in districts with a female superintendent. Standard errors in parentheses are clustered at the district level. $^{*} \leq 0.1$, $^{**} \leq 0.05$, $^{***} \leq 0.01$.

Table AIV: Gender of school and district leaders and district characteristics

	Male super	Share of male principals
Share female teachers	0.020**	0.018
	(0.008)	(0.013)
	[0.712]	[0.712]
Avg teacher experience	-1.148***	-0.583
	(0.295)	(0.388)
	[15.778]	[15.766]
Share teachers w/Master's	0.013	0.007
	(0.021)	(0.029)
	[0.489]	[0.491]
Value-added	0.001	0.001
	(0.002)	(0.002)
	[-0.002]	[-0.002]
Salary ($1,000)	1.384*	1.815**
	(0.745)	(0.875)
	[50.988]	[51.092]
Avg income in district ($1,000)	4.258	3.434
	(2.629)	(3.054)
	[55.540]	[55.629]
Share w/ college degree in district	0.016*	0.015
	(0.009)	(0.011)
	[0.149]	[0.149]
Poverty rate	-0.001	-0.003
	(0.005)	(0.007)
	[0.066]	[0.066]
Share democratic votes (2012 Presidential)	-0.001	0.022
	(0.013)	(0.016)
	[0.511]	[0.512]
Female LFP	-0.009*	-0.013**
	(0.005)	(0.007)
	[0.796]	[0.796]

Note: Each coefficient and standard error correspond to estimates of a district-level OLS regression of each row variable on the share of district superintendents (column 1) or school principals (column 2) who are women in 2011. Data on district sociodemographic composition (income, share of people with college degree, poverty rate, female labor force participation) are from the American Community Survey (2007-2011). Means of each variable are shown in brackets. Robust standard errors in parentheses. $^{*} \leq 0.1$, $^{**} \leq 0.05$, $^{***} \leq 0.01$.

Table AV: Gender pay gap and extra duties

	(1)	(2)	(3)
Other duty	10.692***	10.691***	10.419***
	(1.035)	(1.034)	(1.023)
Other duty × Post Ext	1.284	1.274	1.884*
	(0.928)	(0.927)	(0.998)
Female		-0.072	-0.087
		(0.116)	(0.125)
Female × Post Extension		-0.284***	-0.252**
		(0.095)	(0.100)
Female × Other duty			0.410
			(0.831)
Female × Other duty × Post Ext			-0.908
			(0.964)
Controls	Yes	Yes	Yes
N	444111	444111	444111
R^2	0.809	0.809	0.809

Note: The dependent variable is the natural logarithm of salary per year, in full-time equivalency units and multiplied by 100. *Female* equals one for female workers. *Post Extension* equals one for years following the expiration of a CBA or its extension. *Other duty* equals one for teachers performing extra duties. *Controls* include fixed effects for the district, number of years of experience, highest education degree, grade level (elementary, middle, high), and subject (math, reading, and others), alone and interacted with an indicator for years after the extension of a CBA. All specifications also include year fixed effects interacted with CBA expiration and extension year effects. All columns present OLS estimates. Standard errors in parentheses are clustered at the district level. $^{*} \leq 0.1$, $^{**} \leq 0.05$, $^{***} \leq 0.01$.

Table AVI: Gender pay gap after a CBA expiration/extension: OLS, Wild cluster bootstrap and permutation tests

	Expirations		Extensions	
	(1)	(2)	(3)	(4)
Female	-0.100	-0.097	-0.121	-0.121
	[-1.401]	[-1.401]	[-1.596]	[-1.596]
	(0.500)	(0.500)	(0.494)	(0.196)
Female × Post Expiration	-0.275			
	[-3.234]***			
	(0.005)***			
Female × 1 Year(s) Post		-0.014		-0.062
		[-0.300]		[-0.646]
		(0.499)		(0.295)
Female × 2 Year(s) Post		-0.269		-0.285
		[-2.037]**		[-2.010]**
		(0.000)***		(0.155)
Female × 3 Year(s) Post		-0.248		-0.073
		[-1.792]*		[-0.579]
		(0.043)**		(0.499)
Female × 4 Year(s) Post		-0.347		-0.705
		[-2.019]**		[-3.725]***
		(0.488)		(0.046)**
Female × 5 Year(s) Post		-0.600		-0.718
		[-3.370]**		[-3.155]***
		(0.077)*		(0.141)
Female × Post Extension			-0.287	
			[-3.128]***	
			(0.135)	
Controls	Yes	Yes	Yes	Yes
N	444120	444120	444120	444120

Note: The dependent variable is the natural logarithm of salary per year, in full-time equivalency units and multiplied by 100. The variable *Female* equals one for female workers, the variable *Post Expiration* equals one for years following the expiration of a CBA, and the variable *Post Extension* equals one for years following the expiration of a CBA or its extension. The variables *X Year(s) Post* equal one for observations X years after an expiration (in column 2) or an extension (column 4). *Controls* include fixed effects for the district, number of years of experience, highest education degree, grade level (elementary, middle, high), and subject (math, reading, and others), alone and interacted with an indicator for years after the extension of a CBA. All specifications also include year fixed effects interacted with CBA expiration and extension year effects. T-statistics in brackets are obtained using a Wild cluster bootstrap. P-values in parentheses are obtained randomly permuting the date of each CBA expiration (columns 1 and 2) or extension (columns 3 and 4), preserving the distribution observed in the data. $^{*} \leq 0.1$, $^{**} \leq 0.05$, $^{***} \leq 0.01$.

Table AVII: Survey responses and district characteristics

	=1 if district has responses	District response rate
	(1)	(2)
Nr teachers (hundreds)	-0.0623	-25.3312
	(0.1343)	(17.5661)
Avg salary ($1,000)	-0.0096	-1.0192
	(0.0069)	(1.0285)
Avg teacher experience	0.0143	4.7572
	(0.0165)	(3.7313)
Share female teachers	-0.1873	40.3676
	(0.5520)	(93.0863)
Gender pay gap (post-expiration)	0.0123*	1.3991
	(0.0064)	(1.3084)
Male superintendent	-0.0429	-1.4968
	(0.0688)	(10.1366)
Share principals who are male	-0.0033	-5.4091
	(0.1182)	(19.4189)
CBA/extension expires in 2012	-0.0145	-13.6233
	(0.0697)	(10.0372)
CBA/extension expires in 2013	0.0987	1.3467
	(0.0913)	(15.0888)
CBA/extension expires in 2014	-0.3013	-46.0949
	(0.2760)	(29.8561)
N	241	241
F-stat of joint significance	1.410	1.297

Note: OLS estimates. In column 1, the dependent variable equals one if the district has any survey responses. In column 2, the dependent variable is a district's response rate. All variables are measured in 2016 except for the gender pay gap, estimated on each district's post CBA expiration/extension period. Robust standard errors in parentheses. $^{*} \leq 0.1$, $^{**} \leq 0.05$, $^{***} \leq 0.01$.

Table AVIII: Survey answers: Reasons for not negotiating and superintendent gender

	Reasons for not negotiating				
	(1)	(2)	(3)	(4)	(5)
	Not possible	Not comfortable	Useless	Fear backlash	Satisfied w/pay
[1em] Female	-0.001	0.074**	0.024	-0.004	-0.059**
	(0.033)	(0.035)	(0.030)	(0.023)	(0.023)
Female * F super	-0.070	-0.024	0.017	0.034	0.061
	(0.057)	(0.058)	(0.054)	(0.042)	(0.048)
Controls	Yes	Yes	Yes	Yes	Yes
N	2183	2183	2183	2183	2183
Y mean, males	0.565	0.210	0.215	0.131	0.189

Note: The dependent variable equals one if a teacher gave the corresponding reason for not negotiating, conditional on not having negotiated. *Female* is an indicator for female teachers. *F super* indicates teachers in school districts with a female superintendent. All specifications include controls for age class, self-reported job performance (above/below average), measures of people skills, an indicator for whether the respondent knows someone who negotiated salary, an indicator for whether the respondent knows his/her colleagues' salaries, and district fixed effects. Standard errors in parentheses are clustered at the district level. $^{*} \leq 0.1$, $^{**} \leq 0.05$, $^{***} \leq 0.01$.

Table AIX: Survey answers: People skills, knowledge of colleagues' salaries, and confidence. OLS estimates, no controls

	Neg. beginning (1)	Neg. after (2)	Neg. future (3)	Successful neg (4)	Not confident (5)
Female	-0.077***	-0.026	-0.383**	-0.105**	0.112***
	(0.025)	(0.020)	(0.162)	(0.042)	(0.021)
Knows colleague pay	0.013	0.083***	0.230	0.066	-0.094***
	(0.035)	(0.030)	(0.238)	(0.052)	(0.023)
Female × knows colleague pay	-0.001	-0.006	-0.335	0.005	-0.033
	(0.041)	(0.036)	(0.262)	(0.070)	(0.029)
Female	-0.151**	-0.063	-0.914*	-0.190	0.200**
	(0.076)	(0.076)	(0.494)	(0.184)	(0.079)
People skills	0.028	0.025	0.084	-0.045	-0.074
	(0.067)	(0.062)	(0.426)	(0.098)	(0.057)
Female × People skills	0.074	0.025	0.415	0.084	-0.088
	(0.081)	(0.077)	(0.497)	(0.190)	(0.079)
Female	0.005	-0.046	-0.672**	-0.225***	0.130***
	(0.037)	(0.043)	(0.320)	(0.086)	(0.045)
Confident talking	0.179***	0.045	0.149	-0.114	-0.104**
	(0.039)	(0.045)	(0.324)	(0.076)	(0.043)
Female × Confident talking	-0.083*	0.017	0.231	0.122	-0.037
	(0.043)	(0.048)	(0.333)	(0.090)	(0.046)
Female	-0.088	0.028	-0.307	-0.111	0.230***
	(0.062)	(0.057)	(0.459)	(0.129)	(0.056)
Understand feelings	0.048	0.107**	0.088	-0.016	0.049
	(0.054)	(0.049)	(0.400)	(0.092)	(0.038)
Female × Understand feelings	0.010	-0.073	-0.219	-0.001	-0.127**
	(0.063)	(0.060)	(0.488)	(0.131)	(0.058)
Female	-0.093***	-0.019	-0.577***	-0.110**	0.151***
	(0.033)	(0.023)	(0.184)	(0.055)	(0.027)
Perf > avg	0.016	0.130***	-0.121	-0.028	-0.024
	(0.036)	(0.033)	(0.221)	(0.049)	(0.026)
Female × Perf > avg	0.023	-0.038	0.112	0.004	-0.059*
	(0.040)	(0.033)	(0.249)	(0.071)	(0.033)
N	2810	2809	2801	701	2810
Y mean, males	0.306	0.245	3.889	0.814	0.128

Note: The dependent variable is an indicator for whether a teacher negotiated with the current employer at the beginning or after the start of the work relationship (columns 1, 2, respectively); whether the teacher plans to negotiate pay in the future (column 3); whether past negotiations were successful (column 4); and whether a teacher did not negotiate in the past because she did not feel comfortable doing so (column 5). Each column and panel show estimates from a separate regression. Standard errors in parentheses are clustered at the district level. $^{*} \leq 0.1$, $^{**} \leq 0.05$, $^{***} \leq 0.01$.

Table AX: Gender differences in mobility, by type of district and value-added

	Move to no-schedule			Move to schedule		
	(1)	(2)	(3)	(4)	(5)	(6)
	All teachers	High VA	Low VA	All teachers	High VA	Low VA
Female	-0.0005	-0.0017	-0.0015	0.0001	0.0024*	0.0015
	(0.0005)	(0.0011)	(0.0015)	(0.0004)	(0.0015)	(0.0015)
Post Extension	0.0002	0.0012	-0.0021	0.0018	0.0011	-0.0019
	(0.0018)	(0.0030)	(0.0040)	(0.0015)	(0.0037)	(0.0030)
Female $\times$ Post Extension	-0.0008	-0.0021	0.0000	-0.0008	0.0019	0.0014
	(0.0008)	(0.0027)	(0.0033)	(0.0008)	(0.0022)	(0.0028)
District FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Experience, education FE	Yes	Yes	Yes	Yes	Yes	Yes
N	370185	43050	45019	370185	43050	45019
# districts	180	180	180	180	180	180
Mean of dep. var.	0.0114	0.0112	0.0126	0.0114	0.0096	0.0111

	Move from no-schedule			Move from schedule		
	(1)	(2)	(3)	(4)	(5)	(6)
	All teachers	High VA	Low VA	All teachers	High VA	Low VA
Female	-0.0002	0.0007	0.0006	-0.0004	0.0005	-0.0016
	(0.0004)	(0.0011)	(0.0010)	(0.0005)	(0.0012)	(0.0014)
Post Extension	0.0003	0.0014	-0.0090***	0.0008	0.0027	0.0018
	(0.0017)	(0.0036)	(0.0033)	(0.0011)	(0.0030)	(0.0031)
Female $\times$ Post Extension	0.0003	-0.0018	0.0046**	-0.0005	0.0017	0.0015
	(0.0007)	(0.0025)	(0.0022)	(0.0008)	(0.0022)	(0.0027)
District FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Experience, education FE	Yes	Yes	Yes	Yes	Yes	Yes
N	369111	42884	44827	369111	42884	44827
# districts	243	198	198	243	198	198
Mean of dep. var.	0.0090	0.0075	0.0074	0.0108	0.0086	0.0117

Note: The dependent variable is an indicator for a teacher moving to a flexible-pay district (top panel, columns 1-3), to a seniority-pay district (top panel, columns 4-6), from a flexible-pay district (bottom panel, columns 1-3), and from a seniority-pay district (bottom panel, columns 4-6), and separately for all teachers (columns 1 ad 4), teachers with value-added above the median ("High VA", columnns 2 and 5), and teachers with value-added below the median ("Low VA", columns 3 and 6). The variable *Female* equals one for female teachers and the variable *Post Extension* equals one for years following the expiration of a CBA or its extension. All columns include district and year fixed effects, as well as fixed effects for years of experience and for the highest education degree. Standard errors in parentheses are clustered at the district level. $^{*} \leq 0.1$, $^{**} \leq 0.05$, $^{***} \leq 0.01$.

Appendix B Estimating Teacher Value-Added With Grade-School Links

Teacher value-added (VA) is defined as the contribution of each teacher to achievement (or achievement growth), once all other determinants of student learning have been taken into account. The starting model is the following (Kane and Staiger, 2008):

$$A_{kt} = \beta X_{kt} + \nu_{kt} \tag{B1}$$
$$\text{where } \nu_{kt} = \mu_{i(kt)} + \theta_{c(kt)} + \varepsilon_{kt}$$

and where A_{kt} is a standardized measure of test scores (or test score gains) for student k in year t, and X_{kt} is a vector of student, grade, and school observables which could affect achievement, including: school and grade-by-year fixed effects; cubic polynomials of past scores interacted with grade fixed effects; cubic polynomials of average past scores for the students in the same grade and school, interacted with grade fixed effects; student k's demographic characteristics, including gender, race and ethnicity, disability, English-language earner status, and socioeconomic status; the same demographic characteristics, averaged for all students in the same grade and school as student k in year t; and the student's socioeconomic status interacted with the share of low-socioeconomic status in her grade and school in t.[65] The residual ν_{kt} can be decomposed into three parts: The error term component $\mu_{i(kt)}$ is the individual effect of teacher i, teaching student k in year t; the component $\theta_{c(kt)}$ is an exogenous classroom shock; and ε_{kt} is an idiosyncratic student-specific component which varies over time. VA is an estimate of the teacher effect μ_i.

A range of techniques have been proposed to estimate μ_i, including fixed effects (Aaronson et al., 2007) and two-steps procedures based on the decomposition of test score residuals (Kane and Staiger, 2008; Chetty et al., 2014). Here, we consider the two-steps estimator of Kane and Staiger (2008), a special case of the more general framework of Chetty et al. (2014) which allows for the correction of noise in the estimates using a Bayes "shrinkage" approach. The estimation procedure can be summarized as follows:

1. Estimate β in equation (B1) via OLS;
2. Construct residuals $\hat{\nu}_{kt} = A^*_{kt} - \hat{\beta} X_{kt}$, where $\hat{\beta}$ is the OLS estimate of β;
3. Estimate VA as $\bar{\nu}_i \left(\frac{\sigma_\mu}{Var(\bar{\nu}_i)} \right)$, where
   (a) $\bar{\nu}_i = \sum_t w_{it} \bar{\nu}_{it}$ is a weighted average of average test score residuals $\bar{\nu}_{it}$ for teacher i in year t;
   (b) $w_{it} = \frac{h_{it}}{\sum_t h_{it}}$, with $h_{it} = \frac{n_{it}}{n_{it}\sigma^2_\theta + \sigma^2_\varepsilon}$, are the weights, function of class size n_{it}, the variance of the classroom component σ^2_θ and of the residual component σ^2_ε;
   (c) the variance of the teacher effect is $\sigma^2_\mu = Cov(\bar{\nu}_{it}, \bar{\nu}_{it-1})$; the variance of the residual component is $\sigma^2_\varepsilon = Var(\nu_{kt} - \bar{\nu}_{it})$; the variance of the classroom component is $\sigma^2_\theta = Var(\nu_{kt}) - \sigma^2_\varepsilon - \sigma^2_\mu$.

Constructing an estimate of teacher VA thus requires correctly estimating $\bar{\nu}_{it}$, which in turn requires linking each teacher with the students she taught in each year. The WDPI started to record classroom identifiers, which allow to link students to teachers, only in 2017; data from previous years only contain identifiers for schools and grades. This means that, in a given year, a student can be linked to all the teachers in her school and grade, but not to the specific teacher who taught her (and conversely, a teacher can be linked to all students attending her grade in her school, but not to her own pupils). The lack of information on classroom identifiers is common

[65] This specification largely follows Chetty et al. (2014).

to teacher-student datasets from several other states and/or districts (Rivkin et al., 2005, for example, face a similar issue with data from Texas).

How to identify teacher effects in the absence of classroom links? A simple approximation is given by grade-level average test score residuals. Rivkin et al. (2005), however, show that in the presence of teacher turnover across grades or schools one can obtain a more accurate measure of teacher effects than grade residuals. The intuition behind the identification of these effects is as follows. In the absence of teacher turnover, teachers in grade g and school s would have the same $\bar{\nu}_{it}$ for every t, and separately identifying their individual effects would be impossible. With data on test scores for multiple years and in the presence of turnover, teachers switches across schools or within schools and grades allow to isolate the effect of the individual teacher through the comparison of test score residuals before and after her arrival in a given grade and school. Importantly, teacher turnover allows a more precise identification of the effects not only of the teacher who switches school or grade, but also of the teachers teaching in her same grade and school at any point in time.

To incorporate this feature of the data, we proceed as follows.

a. We calculate the grade-school-year average residuals $\bar{\nu}_{gst}$ for each g, s, and t;

b. We construct the "teams" of teachers in each grade and school in each year;

c. Given these teams, we identify teachers or groups of teachers whose value added can be separately identified, either because they move or because other teachers in their team move. For these teachers we can identify a $\bar{\nu}_{it}$; in the Wisconsin data, these teachers represent 70 percent of the whole sample. For 10 percent of the sample, $\bar{\nu}_{it}$ will not be separately identifiable from that of another teacher, and for 20 percent of the sample $\bar{\nu}_{it}$ will not be separately identifiable from that of two or more teachers.

d. Given these $\bar{\nu}_{it}$, we can calculate VA from step 3 above. This strategy does not allow to separately identify θ_c; we therefore assume θ_c and σ_θ to be zero.

Two features of this identification strategy are worth highlighting:

1. While my VA estimates are more precise than grade-school residuals, they contain more noise relative to estimates obtained with teacher-student links: Even in the presence of turnover, teachers always teaching the same grade-school would have the same $\bar{\nu}_{it}$ for every t, and hence the same estimate.

2. The aggregation of teacher effects at the grade level overcomes a problematic form of selection, which occurs within schools and grades and across classrooms when some parents manage to have their children assigned to specific teachers. The (forced) use of grade-school estimates circumvents this form of selection, and is in practice equivalent to an instrumental variable estimator based on grade rather than on classroom assignment (Rivkin et al., 2005).

Identification of Teacher Value-Added With Turnover

To understand the identification argument, consider a simple example of 3 teachers (A, B, C) observed in 3 periods $(t = 1, 2, 3)$ and in 2 possible grades $(g = 4, 5)$. The teaching assignments are as follows.

period	grade	
1	A,B	C
2	B,C	A
3	A,C	B

The objective is to calculate VA of the three teachers in period 3. We define A_{kt} as the average test score residual for students of teacher k in period t, and $\bar{A}_t^g$ the average test score residuals of students in grade g in period t. Following Chetty et al. (2014) we can write the VA estimate for each teacher as follows (we suppress the hats on the VA estimates for ease of notation and we consider 3 lags):

$$\mu_{A3} = \begin{bmatrix} A_{A1}^2 & A_{A1}A_{A2} \\ A_{A1}A_{A2} & A_{A2}^2 \end{bmatrix}^{-1} \begin{bmatrix} A_{A1}A_{A3} \\ A_{A2}A_{A3} \end{bmatrix} \tag{B2}$$

$$\mu_{B3} = \begin{bmatrix} A_{B1}^2 & A_{B1}A_{B2} \\ A_{B1}A_{B2} & A_{B2}^2 \end{bmatrix}^{-1} \begin{bmatrix} A_{B1}A_{B3} \\ A_{B2}A_{B3} \end{bmatrix} \tag{B3}$$

$$\mu_{C3} = \begin{bmatrix} A_{C1}^2 & A_{C1}A_{C2} \\ A_{C1}A_{C2} & A_{C2}^2 \end{bmatrix}^{-1} \begin{bmatrix} A_{C1}A_{C3} \\ A_{C2}A_{C3} \end{bmatrix} \tag{B4}$$

Assuming a constant number of students in each classroom, one can write:

$$\bar{A}_1^4 = \frac{1}{2}(A_{A1} + A_{B1}) \tag{B5}$$

$$\bar{A}_1^5 = A_{C2} \tag{B6}$$

$$\bar{A}_2^4 = \frac{1}{2}(A_{B2} + A_{C2}) \tag{B7}$$

$$\bar{A}_2^5 = A_{A2} \tag{B8}$$

$$\bar{A}_3^4 = \frac{1}{2}(A_{A3} + A_{C3}) \tag{B9}$$

$$\bar{A}_3^5 = A_{B3} \tag{B10}$$

My VA estimator implies:

$$\mu_{A3} = \begin{bmatrix} (\bar{A}_1^4)^2 & \bar{A}_1^4\bar{A}_2^5 \\ \bar{A}_1^4\bar{A}_2^5 & (\bar{A}_2^5)^2 \end{bmatrix}^{-1} \begin{bmatrix} \bar{A}_1^4\bar{A}_3^4 \\ \bar{A}_2^5\bar{A}_3^4 \end{bmatrix} \tag{B11}$$

$$\mu_{B3} = \begin{bmatrix} (\bar{A}_1^4)^2 & \bar{A}_1^4\bar{A}_2^4 \\ \bar{A}_1^4\bar{A}_2^4 & (\bar{A}_2^4)^2 \end{bmatrix}^{-1} \begin{bmatrix} \bar{A}_1^4\bar{A}_3^5 \\ \bar{A}_2^4\bar{A}_3^5 \end{bmatrix} \tag{B12}$$

$$\mu_{C3} = \begin{bmatrix} (\bar{A}_1^5)^2 & \bar{A}_1^5\bar{A}_2^4 \\ \bar{A}_1^5\bar{A}_2^4 & (\bar{A}_2^4)^2 \end{bmatrix}^{-1} \begin{bmatrix} \bar{A}_1^5\bar{A}_3^4 \\ A_{C2}\bar{A}_3^4 \end{bmatrix} \tag{B13}$$

Equations (B2)-(B13) represent a system of 12 equations in 12 unknowns: μ_{A3}, μ_{B3}, μ_{B3}, A_{A1}, A_{A2}, A_{A3}, A_{B1}, A_{B2}, A_{B3}, A_{C1}, A_{C2}, A_{C3}. In this case, VA can be perfectly identified for all teachers because at least one teacher switches grade each year.

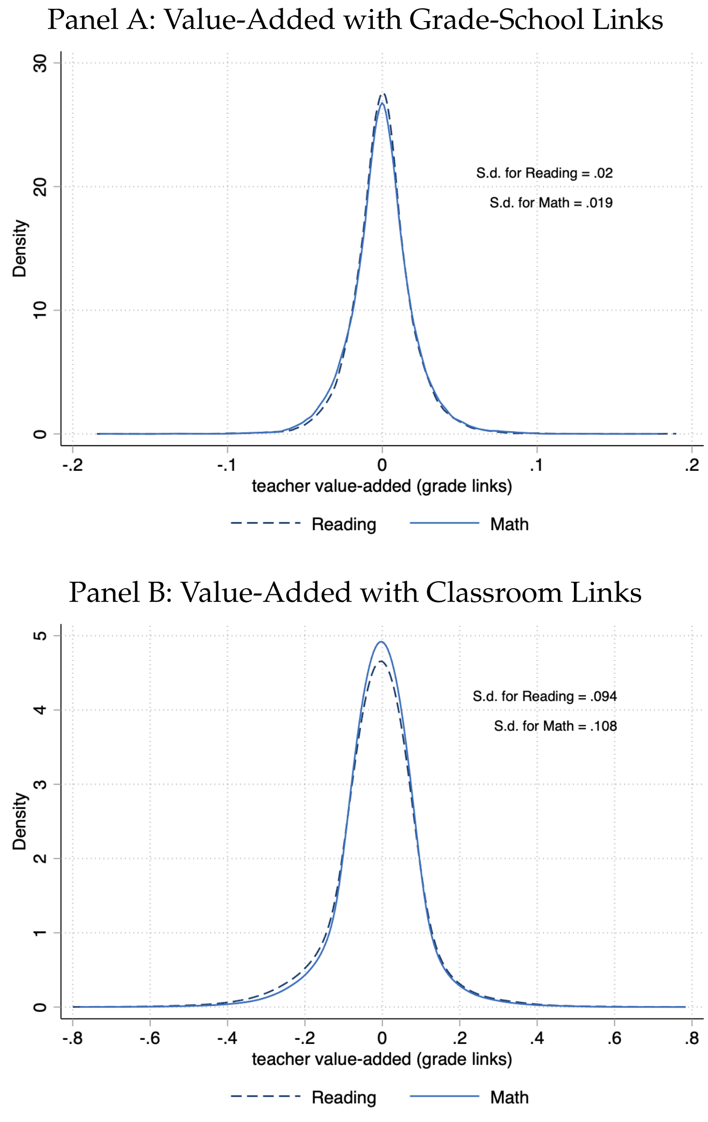
Validation Exercise: Value-Added with Classroom Links and with Grade-School Links in the NYC data

To validate the VA estimator with grade-school links described above (which we call GL) against the standard Kane and Staiger estimator with classroom links (CL), we use teacher and student data from the New York City Department of Education (NYCDOE) from the years 2006-07 to 2009-10. This dataset contains classroom, grade, and school identifiers, which allow me to estimate both CL and GL measures. We estimate teacher VA for 15,469 teachers of Math and English-Language-Arts (ELA) using the procedure of Kane and Staiger (2008).

Measurement Error The main limitation of GL relative to CL is measurement error. Since students are linked to teachers at the grade-school level, the VA of a teacher will also be a function of test scores of students she never taught.

Classic measurement error will push VA estimates towards zero. To quantify the extent of this problem, Figure BI shows the kernel density of the distribution of GL (top panel) and CL (bottom panel). As expected, the distribution of GL is more concentrated around zero compared to CL. In spite of this, GL is able to explain a significant amount of variance in test scores. Its standard deviation (measured in test scores standard deviation units) is equal to 0.02 for Math teachers; by comparison, the standard deviation of CL is equal to 0.11. Figure BII shows the density of GL for Wisconsin teachers. Its standard deviation is equal to 0.10 for Math teachers.

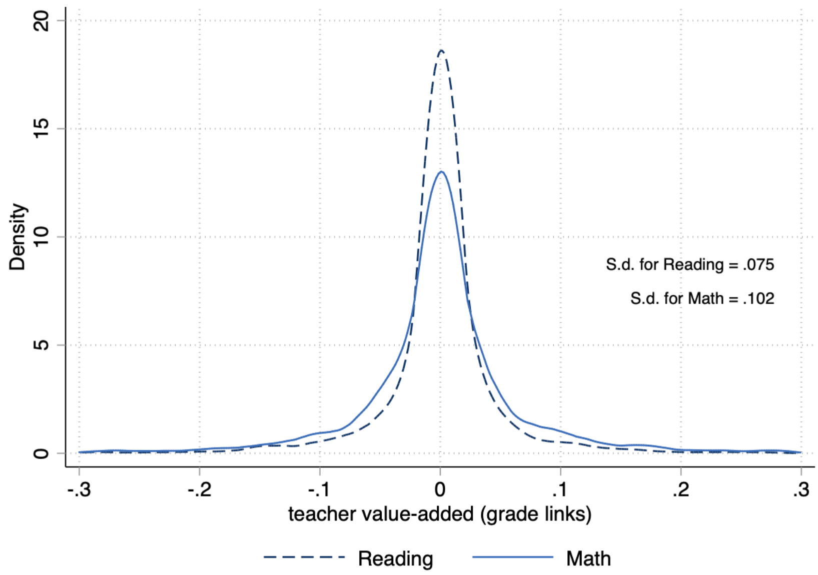
Figure BI: Empirical Distribution of Value-Added Estimates: New York City, 2007-2010



Notes: Kernel densities of the empirical distribution of VA estimates for NYC math and ELA teachers, for each subject. Estimates are averaged across years for each teacher. Each density is weighted by the number of student test scores observations used to estimate each teacher's VA, and estimated using a bandwidth of 0.05. The figure also reports the standard deviations of these empirical distributions.

Forecast Bias of GL as a Proxy for CL Next, we test whether GL is a forecast-unbiased estimate for CL. Figure BIII shows a binned scatterplot of the two estimates in the NYC data, averaged across the four years for each teacher. Their correlation is 0.62. The forecast bias of $\hat{\mu}_i^{GL}$ as a

Figure BII: Empirical Distribution of Value-Added Estimates: Wisconsin, 2007-2015



Notes: Kernel densities of the empirical distribution of VA estimates for Wisconsin math and reading teachers, for each subject. Estimates are averaged across years for each teacher, separately for years before and after Act 10. Each density is weighted by the number of student test scores observations used to estimate each teacher's VA, and estimated using a bandwidth of 0.05. The figure also reports the standard deviations of these empirical distributions.

proxy for $\hat{\mu}_i^{CL}$ can be defined based on the best linear predictor of $\hat{\mu}_i^{CL}$ given $\hat{\mu}_i^{GL}$:

$$\hat{\mu}_i^{CL} = \alpha + \gamma \hat{\mu}_i^{GL} + \chi_i \tag{B14}$$

Assuming χ_i to be uncorrelated with $\hat{\mu}_i^{GL}$, the forecast bias f is zero if $\gamma = 1$: $f = 1 - \gamma$. We can estimate the slope coefficient γ via OLS on equation (B14). The 95% confidence interval for γ, whose point estimate is equal to 0.99, includes 1, which implies that the forecast bias f is equal to 0.01 and it is indistinguishable from zero (Figure BIII).

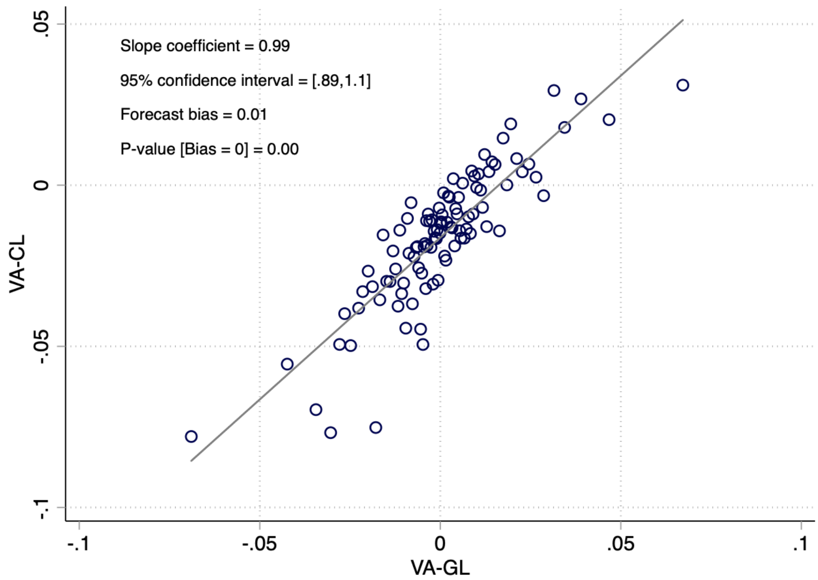
Teacher Switches as a Quasi-Experiment As an additional test for the unbiasedness of GL estimates we exploit teacher switches across grades as a quasi-experiment, as in Chetty et al. (2014). If VA is an unbiased measure of teacher quality, changes in average VA of teachers in a given school and grade (driven by teacher switches) should predict changes in average student test score residuals one-by-one. To understand the rationale behind this test suppose that, in a given school with three 4th-grade classrooms (and hence three 4th-grade math teachers), one of these teachers leaves and is replaced by a teacher with a 0.3 higher VA (measured in standard deviations of test scores). If VA is an unbiased measure of teacher effectiveness, test scores should raise by 0.3/3 = 0.1 standard deviations due to this switch (Chetty et al., 2014).

We estimate the degree of forecast bias for the Wisconsin GL measures by estimating the following first-differences equation (we restrict attention to the years 2007 to 2011 to parse out any changes in teacher effort, as done in the paper):

$$\Delta A_{gst}^* = a + b \Delta Q_{gst} + \Delta \chi_{gst} \tag{B15}$$

where A_{gst}^* are test score residuals of students in grade g, school s, and year t, Q_{gst} is average

Figure BIII: Binned scatterplot: $\hat{\mu}_i^{CL}$ and $\hat{\mu}_i^{GL}$



Notes: The figure shows the relationship between $\hat{\mu}_i^{CL}$, estimate of teacher VA obtained using the procedure of Kane and Staiger (2008) and teacher-student links, and $\hat{\mu}_{GL}$, its analogous obtained discarding these links. Estimates are obtained using data from New York City students and teachers of math and ELA for the years 2007-2010.

teacher VA, and $\Delta W_{gst} = W_{gst} - W_{gst-1}$ for any variable W_{gst}. The forecast bias is defined as $\lambda = 1 - b$. Table BIII shows estimates of b and λ, obtained using either mean residual test scores or mean actual test scores, and controlling for school-by-year fixed effects (as in Chetty et al., 2014).[66] Estimates of b are all close to 1 both over the full sample period and in the years after Act 10. While slightly larger than Chetty et al. (2014), who estimate it to be between 0.003 and 0.026, estimates of b are small and indistinguishable from zero, both over the full sample period and in the years after Act 10.

Non-Classical Measurement Error A possible concern with the GL version of VA is non-classical measurement error, which occurs when the precision of the estimates is related to characteristics of the teachers or the students. This issue could arise, for example, if teachers who switch across schools or grades (and, analogously, the grades and schools employing these teachers) are selected on the basis of observable and/or unobservable characteristics.

In Table BII we use the GL and CL estimates of VA from the NYC data to investigate the extent of measurement error. Specifically, we correlate the difference between GL and CL (a proxy for measurement error) with a range of student and teacher observable characteristics. These estimates reveal no discernible relationship between the error and these characteristics, with the exception of the share of special education students. Importantly, the measurement error does not appear to be systematically different between teachers who switch across grades (i.e., those with "switcher" equal to 1) and teachers who do not switch. While only suggestive of the lack of non-classical measurement error, this evidence reassuringly shows no systematic patterns of correlations between VA and student and teacher observables.

[66] The fact that using test scores as a regressor instead of test score residuals yields similar results further confirms that selection of students across teachers is unlikely to generate substantial bias in the estimates (Chetty et al., 2014).

Table BI: Forecast bias in teacher VA

	Δ test scores	Δ test score residuals
	(1)	(2)
ΔVA_{gst}	0.978	1.055
	(0.290)	(0.377)
School-by-year FE	Yes	Yes
Observations	13684	13684
# districts	414	414
λ	0.022	-0.055
p-value λ=0	0.94	0.88

Notes: The dependent variable is the first difference in grade-school average test score residuals (from a regression of test scores on student characteristics, school, and grade fixed effects, column 1) or in average test scores at the grade, school, and year level (column 2). The variable ΔVA_{gst} is the first difference in average teacher VA in school s and grade g. VA is calculated using data from Wisconsin for the years 2007-2011. All regressions include school-by-year fixed effects, and observations are weighted by the number of students. Standard errors in parentheses are clustered at the district level.

Table BII: Correlations Between the Difference [GL-CL] and Student and Teacher Observables

	(1)
experience	-0.0003
	(0.0002)
switcher	0.0013
	(0.0024)
Black	-0.0014
	(0.0026)
Hispanic	0.0033
	(0.0029)
% low SES students	-0.0042
	(0.0028)
% Black students	0.0052
	(0.0035)
% Hispanic students	0.0009
	(0.0037)
% special Ed students	-0.0060
	(0.0107)
% disabled students	-0.0414***
	(0.0103)
Observations	8077

Notes: OLS regression of the difference between GL and CL and a range of student and teacher characteristics, averaged at the teacher-year level. VA is calculated using data from NYC. Robust standard errors in parentheses.

Appendix C Robustness Checks for Dynamic Difference-in-Differences Estimators

Appendix C.1 Dynamic difference-in-differences design

A number of recent articles have highlighted issues that can arise when using a dynamic difference-in-differences design where all units are eventually treated. A common concern is that treatment effects may be heterogenous across time periods or across agents within a time period. This can result in some units receiving non-convex or negative weights when their outcomes are aggregated to produce treatment effects, which can bias the estimates. Here, we outline some of the proposed solutions to this issue in recent literature and show that our results are robust to the use of these methods.

Appendix C.1.1 Abraham and Sun method

Our main alternative approach consists in the use of the "interaction-weighted estimator" put forth in Sun and Abraham (2020, SA henceforth). This approach provides estimates that are robust to treatment effect heterogeneity.

SA define a *cohort-specific average treatment effect (CATT)* as

$$CATT_{e,l} = \mathbb{E}(Y_{i,e+l} - Y^{\infty}_{i,e+l}|E_i = e) \tag{C1}$$

where e represents a cohort (the set of units that are all first exposed to treatment in the same time period), l indexes relative time periods (in our setting, the number of years surrounding treatment), and E_i is the time period of the initial treatment for unit i.

Following this setup, we estimate our parameters as follows.

1. We define a cohort e as the set of teachers working under a CBA that expires in a given year. This gives us five cohorts: 2011, 2012, 2013, 2014, and 2016. The last treated cohort is used as the control group. We index teachers with i and a relative time period as l (so that l represents the periods before or after treatment).

2. We estimate the cohort-specific average treatment effect using a linear two-way fixed effects specification that interacts relative period indicators with cohort indicators:

$$ln(wage_{i,d,t}) = \alpha_d + \lambda_t + \sum_{e \notin C} \sum_{l \neq -1} \delta_{e,l}(I \cdot E_i = e) \cdot D^l_{i,t}) + \epsilon_{i,d,t} \tag{C2}$$

   where C is the last-treated cohort (treated after 2016).

3. We calculate the weights for each period, $\mathbb{P}(E_i = e|E_i \in (-l, T-l))$, using the sample shares of each cohort in the given relative time period l.

4. We combine the $CATT_{e,l}$ estimates and the weights to calculate what Abraham and Sun call the "interaction-weighted" estimate:

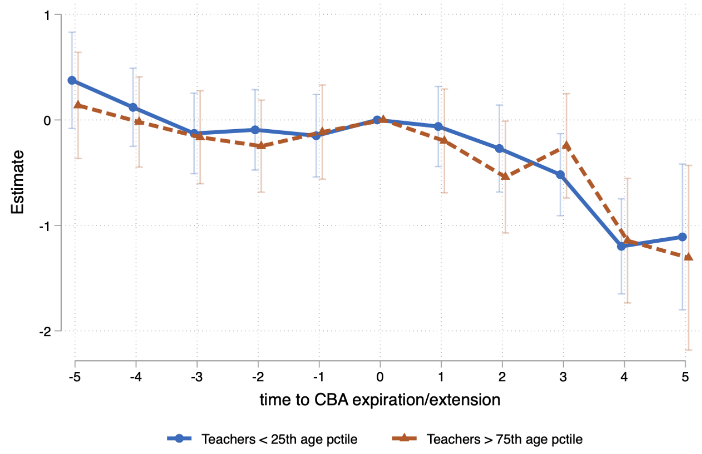
$$\hat{v}_g = \frac{1}{|g|} \sum_{l \in g} \sum_{e} \hat{\delta}_{ee,l} \hat{\mathbb{P}}(E_i = e|E_i \in (-l, T-l)) \tag{C3}$$

Sun and Abraham (2020) show that the difference-in-differences estimator for CATT, $\hat{\delta}_{e,l}$, is consistent in the presence of parallel trends (an assumption we discuss and test for below).

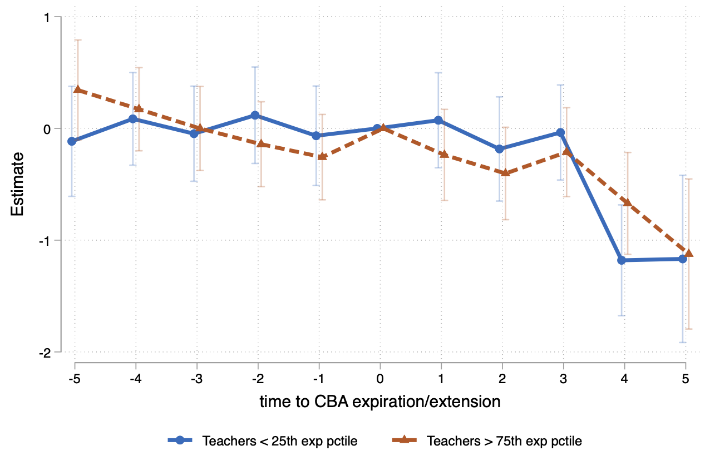
We apply these methods to the event studies shown in our paper. The main specification is shown in Figure IV (panel (b)); the remaining ones are in Figures CI to CIII.

Figure CI: Gender gap in salaries using SA method: By age and experience

Panel a) Age



Panel b) Experience



Note: Panel A shows point estimates and confidence intervals from estimating equation (2) using the SA method. The equations is estimated separately for teachers with in the lowest 25th percentile in terms of age (solid line) and in the upper 75th percentile in terms of age (dashed line). Panel B does the same but splits by the number of years of teacher experience, with less-experienced teachers being represented by the solid line and more experienced teachers being represented by the dashed line.

Appendix C.1.2 Borusyak, Jaravel, and Spiess (2021)

Next, we construct the "imputation estimator" proposed by Borusyak et al. (2021) and used in Borusyak and Schönberg (2021). We adapt their approach to our setting as follows:

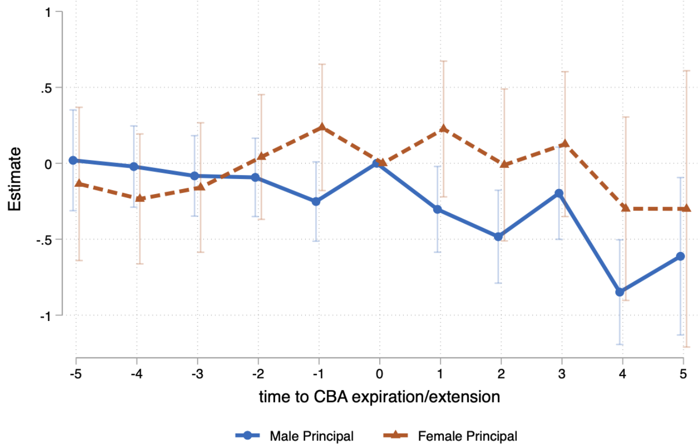
1. We estimate β_1, β_2, γ_1, γ_2, θ_j and τ_t in equation (2) using only not-yet-treated units in each period;

2. We construct conditional outcomes $\log(\tilde{w_{it}})$ as

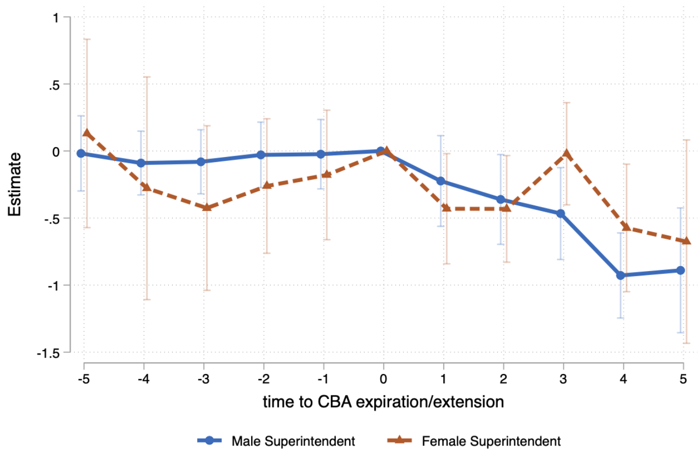
$$\begin{aligned} \log(\tilde{w_{it}}) \quad = \quad & \ln(w_{it}) - \beta_1' X_{it} - \beta_2' X_{it} \times postext_{j(it)t} - \gamma_1' T_{it} - \gamma_2' T_{it} \times postext_{j(it)t} - \theta_{j(it)} \\ & - \theta_{j(it)} \times postext_{j(it)t} - \tau_t - \tau_t \times Y^{exp}_{j(it)} - \tau_t \times Y^{ext}_{j(it)} \end{aligned}$$

Figure CII: Gender gap in salaries using the SA method: By gender of school principals and district superintendents

Panel a) Principal Gender



Panel b) Superintendent Gender



Note: The figure shows the point estimates and 90% confidence intervals of the coefficients δ_s in equation (2), obtained using the method in Sun and Abraham (2020). Estimates are obtained separately for teachers in schools with a female principal and teachers in schools with a male principal (Panel A), and for teachers in districts with a female superintendent and teachers in districts with a male superintendent (Panel B). All coefficients are plotted relative to the year a CBA or its extension expired ($t = 0$). Standard errors are clustered at the district level.

3. We aggregate these outcomes by time-to-treatment, separately for men and women:

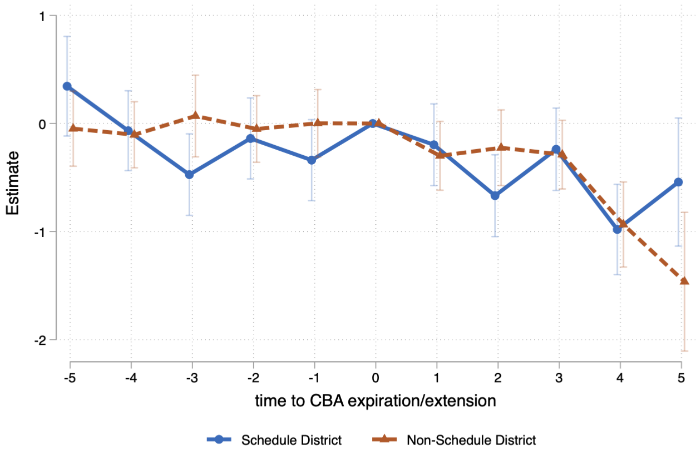
$$\tau_h^g = \frac{1}{|N_{gsh}|} \sum_{k,s:s-Y_{j(ks)}^{ext}=h} \log(\tilde{w}_{ks}) \mathbb{1}(Female_k = g)$$

where $g = 1$ for women and 0 for men and N_{gsh} is the set of teachers of gender g in districts h years after an expiration in year s.

4. We estimate δ_1 to δ_5 in equation (2) as $\tau_h^1 - \tau_h^0$ for $h \in [1, 5]$.

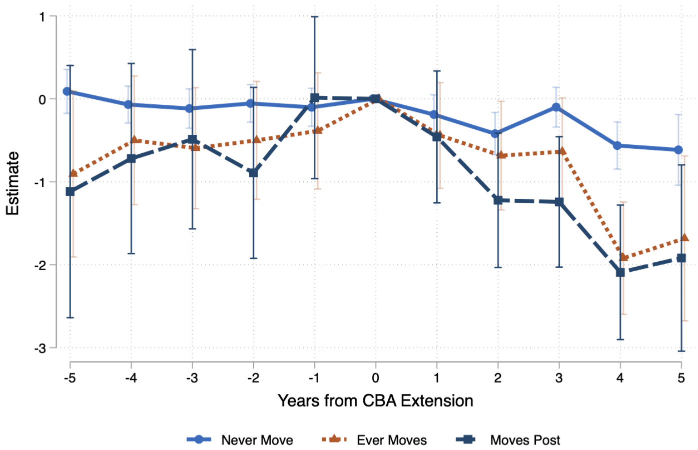
Estimates using this procedure are shown in Figure CV, along with bootstrapped confidence intervals.

Figure CIII: Gender gap in salaries using the SA method: By district type



Note: The figure shows the point estimates and 90% confidence intervals of the coefficients δ_s in equation (2), obtained using the method in Sun and Abraham (2020). Estimates are obtained separately for teachers working in districts that discontinued the use of a salary schedule (solid line) and those that continued to use a salary schedule (dashed line). The estimates are obtained using the methods in Sun and Abraham (2020). All coefficients are plotted relative to the year a CBA or its extension expired ($t = 0$). Standard errors are clustered at the district level.

Figure CIV: Gender gap in salaries using the SA method: Mobility



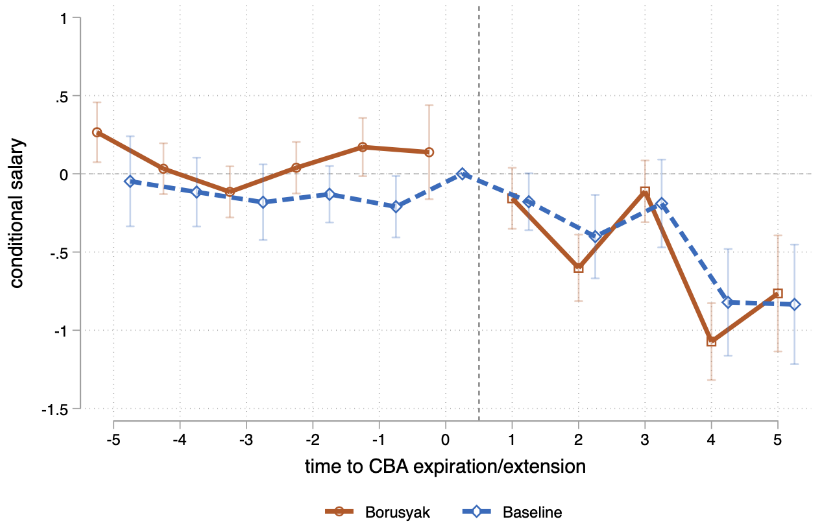
Note: This figure shows the point estimates and 90% confidence intervals of the coefficients δ_s in equation (2), obtained using the method in Sun and Abraham (2020), and estimated separately for teachers who never move between 2007 and 2016 ("non-movers"), those who move at least once ("movers (ever)"), and those who move at least once after a CBA expiration ("movers (post-extension)"). All coefficients are plotted relative to the year a CBA or its extension expired ($t = 0$). Standard errors are clustered at the district level.

Appendix C.1.3 Cengiz et al., (2019): Event-by-event analysis

As a final check of the robustness of our results, we also perform a stacked regression analysis, following the method outlined by Cengiz et al. (2019). This approach consists of the following steps:

1. We create cohort-specific panel datasets, where each dataset contains a single treated cohort along with all other cohorts that do not experience a CBA expiration in the relevant

Figure CV: Gender gap in salaries, by time-to-expiration/extension of CBAs - Borusyak, Jaravel, and Spiess (2021) approach



Note: Point estimates and 90% confidence intervals of the coefficients δ_s in equation (2). The solid lighter line shows standard OLS estimates. The darker dashed line shows the estimates using the method outlined in Borusyak, Jaravel, and Spiess (2021). OLS standard errors are clustered at the district level; standard errors in the Borusyak, Jaravel, and Spiess (2021 are bootstrapped, clustering at the district level.

time period surrounding the treated cohort's expiration year. These untreated cohorts make up the "clean controls". For example, the first treated cohort is the set of teachers working under a CBA that expires in 2011. If we want to look at the impact of flexible pay in the five years following the 2011 expirations, we can only use the 2016 cohort as the clean control as it is the only cohort that remains untreated during that time. If we want to look at the impact of flexible pay in only the three years following the 2011 expirations, we could use both the 2014 and the 2016 cohorts as controls.

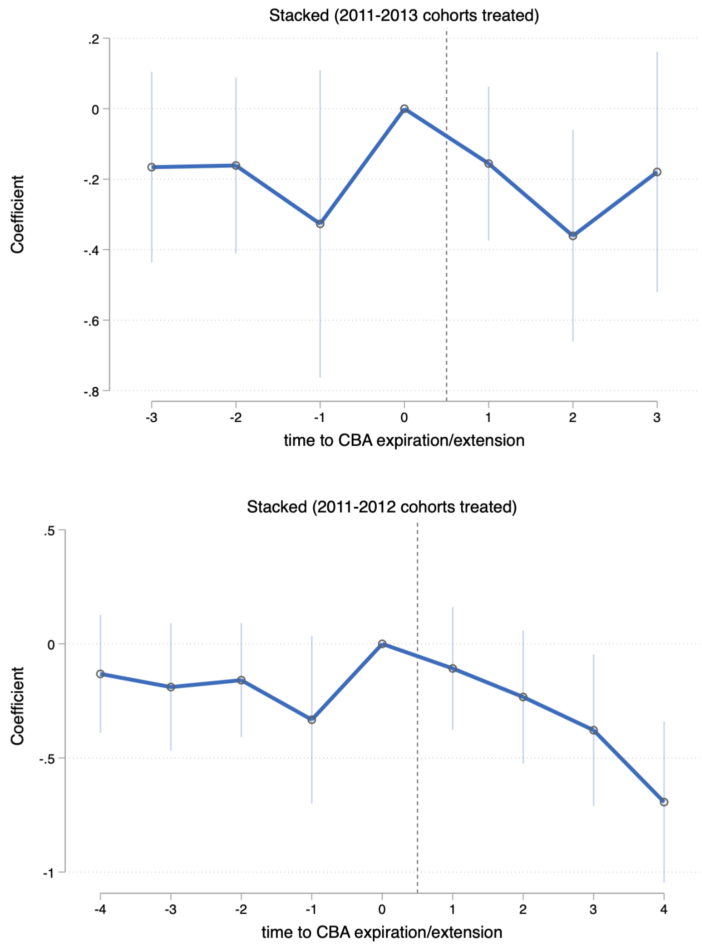
2. We stack these datasets and line them up according to the relative time indicators.
3. We estimate equation (2) on this stacked dataset, interacting fixed effects and controls with dataset indicators and using only data for the years for which we have a clean control.

Compared with Cengiz et al. (2019), our data cover much fewer event times and years and contain only two districts which are never treated. We therefore show two stacked event studies in Figure CVI below, where we restrict our sample to be able to include as many pre- and post-treatment periods as possible. The top panel uses the 2011 treated cohort with 2014 and 2016 as controls, the 2012 treated cohort with 2016 as a control, and the 2013 treated cohort with 2016 as a control. The bottom panel uses the 2011 and 2012 cohorts as treated cohorts, each with 2016 as a control. We do not use the 2014 treated cohort as this would only allow us to study the gender gap two years after a CBA expiration. With this approach our estimates are noisier than OLS, especially when we use the 2011-2013 cohorts as the treated ones. Nevertheless, estimates paint a similar picture to OLS.

Appendix C.2 Violations of Parallel Trends Assumption: Rambachan and Roth (2020) and Borusyak, Jaravel, and Spiess (2021)

Rambachan and Roth (2020, R&R henceforth) propose an approach to test for violations of the parallel trends assumption and study their impacts on the point estimates and confidence inter-

Figure CVI: Stacked Dataset Analysis (Cengiz et al, 2019): Gender Salary Gap



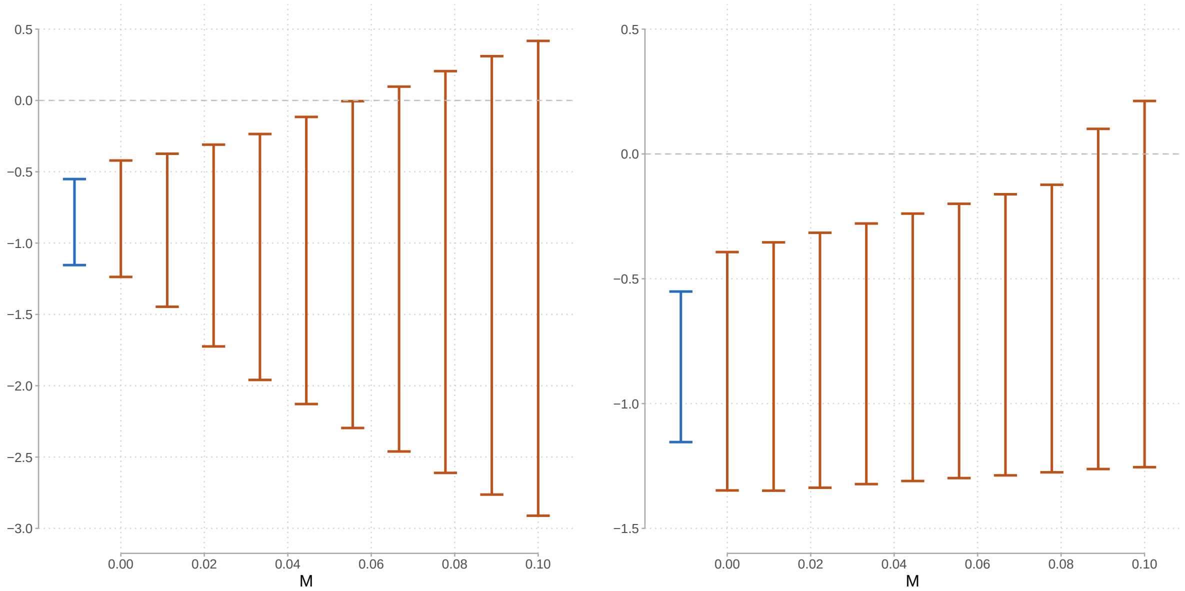
Note: This figure shows the gender wage gap in the years surrounding a CBA expiration/extension obtained via a stacked event study, as in Cengiz et al. (2019). The top panel shows the result when using the 2011, 2012, and 2013 cohorts as treated cohorts and the 2016 cohort as the control. The bottom panel shows the result when using the 2011 and 2012 cohorts as treated cohorts and the 2016 cohort as the control.

vals of interest. Specifically, their proposed test consists in (a) constructing a set Δ of possible deviations from the parallel trends assumption, and (b) constructing the confidence intervals associated with these deviations. We adopt R&R's main robustness test, which involves constructing confidence intervals that allow for deviations from linearity up to a parameter M: defining δ as the trend, $\Delta^{SD}(M) := \{\delta : |(\delta_{t+1} - \delta_t) - (\delta_t - \delta_{t-1})| \leq M, \forall t\}$. We allow M to range from zero (linear pre-trends) to the standard error of the coefficient of interest (R&R use 0.5*standard error as a default; we attempt to be conservative). Estimates of the confidence interval for the parameter δ_4 in equation (3) are shown in panel (a) of Figure CVII (which replicates Figure 8 on page 13 of R&R). The significance of the estimate is robust to deviations for M up to 60 percent of the standard deviation. In panel (b) we also show confidence intervals for deviations in $\Delta^{SDD}(M)$, analogous to $\Delta^{SD}(M)$ but with the additional assumption that the pre-trend be decreasing. In this case, the significance of the estimate is robust to deviations for M up to 80 percent of the standard deviation.

Borusyak et al. (2021) also propose a test for the absence of parallel pre-trends, which consists in (i) estimating δ_{-5} to δ_{-1} in equation (2), and (ii) testing for the individual and joint significance of these coefficients (this test is used in Borusyak and Schönberg, 2021). We perform this test in

Figure CVII: Analysis of sensitivity of confidence intervals for δ_4 to non-linear pre-trends, following Rambachan and Roth (2021)

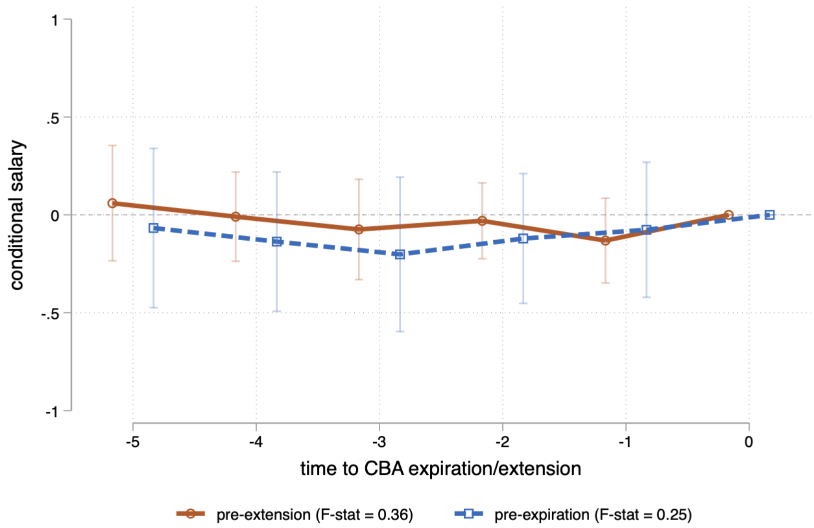
(a) Without imposing monotonicity ($\Delta^{SD}(M)$) (b) Without imposing monotonicity ($\Delta^{SDD}(M)$)



Note: Sensitivity plots of the confidence intervals of δ_4 in equation (3), constructed following the approach of Rambachan and Roth (2020). The blue series represents our OLS confidence intervals.

Figure CVIII (solid line): Estimates are individually and jointly indistinguishable from zero (the F-statistic of joint significance is equal to 0.32). For completeness, we also estimate δ_{-5} to δ_{-1} using the date of expiration of the CBAs (instead of the extension). These estimates, shown in the dashed series in Figure CVIII, are also individually and jointly indistinguishable from zero, with an F-statistic of joint significance equal to 0.24.

Figure CVIII: Gender gap in salaries, by time-to-expiration/extension of CBAs - Borusyak, Jaravel, and Spiess (2021) approach



Note: Point estimates and 90% confidence intervals of the coefficients $\delta_{-5} - \delta_{-1}$ in equation (2), estimated only on pre-treatment observations as specified in Borusyak, Jaravel, and Spiess (2021). The solid line uses CBA expirations or extensions as the treatment; the dashed line uses only CBA expirations, ignoring the extensions. Standard errors are clustered at the district level.

Appendix D Survey Details

Survey Questionnaire

General Questions

1. What is your age? (select one)
   - less than 25
   - 25-30
   - 31-35
   - 36-40
   - 41-45
   - 46-50
   - 51-55
   - 56 and above
2. What is your gender?
   - Male
   - Female
   - Other
3. Did you work in another industry before teaching in public schools?
   - Yes
   - No
4. Which industry did you work in before teaching in public schools?
   - Other job in public sector
   - Other job in private education
   - Other job in different sector

Negotiation

5. Have you ever negotiated your pay with any of your past employers?
   - Yes, successfully
   - Yes, unsuccessfully
   - No, it was not a possibility
   - No, it was a possibility but I chose not to
   - No, it was a possibility but I did not feel I could negotiate without repercussions
6. When you started your current job, did you negotiate your pay?
   - Yes, successfully
   - Yes, unsuccessfully
   - No
7. (If no to previous question) Why didn't you negotiate your pay? [choose all that apply]

- It was not a possibility
- I would not have gotten anything out of it
- I was worried about backlash
- I didn't feel comfortable negotiating
- I was satisfied with my offered salary

8. Since starting your current job, have you ever asked for a pay increase?
    - Yes, successfully
    - Yes, unsuccessfully
    - No
9. (If no to previous question) Why haven't you asked for a pay increase? [choose all that apply]
    - I would not have gotten anything out of it
    - It is not a possibility
    - I am worried about backlash
    - I don't feel comfortable asking
    - I am satisfied with my salary
10. How likely is it that you will negotiate any of the following in the future? [for each item, choose a number from 1 (not at all likely) and 10 (very likely))
    - Salary
    - Classroom assignment
    - Non-teaching duties
11. Do you know what your colleagues earn?
    - Yes
    - Only some of them
    - No
12. Do you know any public sector teachers who have negotiated their salary?
    - Yes, among my colleagues
    - Yes, outside of my colleagues
    - Yes, both among and outside of my colleagues
    - No
13. How would you rate your performance relative to your colleagues' performance?
    - Below average
    - Average
    - Above average
14. Are you confident about talking to people you don't know?
    - Yes
    - No

Please state whether you agree or disagree with the following statements.

15. I pick up the subtle signals of feelings from another person.
    - Agree
    - Disagree
16. I am astute at reading people's reactions and feelings.
    - Agree
    - Disagree
17. I have good people skills.
    - Agree
    - Disagree

Figure DI: Survey Email

From: Heather Sarsons

To: [TEACHER'S EMAIL]

Subject: A short survey for a Yale and UChicago study

Good evening,

We are a team of researchers at The University of Chicago and Yale University, and we are conducting a research study on public sector employees' perceptions about their jobs. As part of this study, we would like to ask you to fill in a **very short survey (length < 5 mins).** This survey is confidential, completely anonymous, and has been approved by the Institution Review Boards at Yale and the University of Chicago. Your participation is invaluable for our research.

If you would like to take the survey, please click here:

Follow this link to the Survey:
[LINK]

Or copy and paste the URL below into your internet browser:
[URL]

We sincerely appreciate your time and participation, and please feel free to contact us with any questions. Thank you!

Best regards,

Barbara Biasi
(email: barbara.biasi@yale.edu, website: www.barbarabiasi.com)

Heather Sarsons
(email: heather.sarsons@chicagobooth.edu, website: sites.google.com/view/sarsons/)

Follow the link to opt out of future emails:
(click here to unsubscribe}

Table EI: CBA data sources

Code	Name	CBA Expiration	Extension?	Extension until	Sources	Links			
63	Albany Sch Dist	2011	Yes	2016	MoA district-union	source (1)			
70	Algoma Sch Dist	2011	No	--	handbook	source (1)			
84	Alma Sch Dist	2011	no info	no info	online source (WILL)	source (1)			
105	Almond-Bancroft Sch Dist	2011	Yes	2013	online source (MacIver Inst)	source (1)			
112	Altoona Sch Dist	2011	Yes	2013	board minutes	source (1)	source (2)	source (3)	
119	Amery Sch Dist	2011	Yes	2012	district website; handbook	source (1)	source (2)		
147	Appleton Area Sch Dist	2011	Yes	2012	district website; handbook	source (1)	source (2)	source (3)	source (4)
2450	Arrowhead UHS Sch Dist	2011	Yes	2012	online source (WILL); district website	source (1)	source (2)		
170	Ashland Sch Dist	2011	No	--	online source (governor website)	source (1)	source (2)		
182	Ashwaubenon Sch Dist	2011	Yes	2012	district website	source (1)	source (2)	source (3)	source (4)
203	Auburndale Sch Dist	2011	no info	no info	online source	source (1)			
217	Augusta Sch Dist	2012	No	--	handbook	source (1)			
231	Baldwin-Woodville Area Sch Dis	2011	Yes	2012	online news source (MJS)	source (1)			
245	Bangor Sch Dist	2011	No	--	online source	source (1)			
280	Baraboo Sch Dist	2011	Yes	2012	online source (MacIver Inst)	source (1)	source (2)	source (3)	source (4)
308	Barron Area Sch Dist	2011	Yes	2013	online source (MacIver Inst)	source (1)			
315	Bayfield Sch Dist	2011	Yes	2012	handbook	source (1)			
336	Beaver Dam Unified Sch Dist	2011	Yes	2013	handbook	source (1)	source (2)		
350	Belleville Sch Dist	2011	no info	no info	union contract	source (1)			
413	Beloit Sch Dist	2011	Yes	2013	union contract	source (1)	source (2)		
422	Beloit Turner Sch Dist	2011	Yes	2012	online news source (Beloit Daily News)	source (1)			
427	Benton Sch Dist	2011	No	--	district website	source (1)			
434	Berlin Area Sch Dist	2011	Yes	2012	online news source (PolitiFact)	source (1)			
476	Black River Falls Sch Dist	2011	No	--	handbook	source (1)			
485	Blair-Taylor Sch Dist	2011	Yes	2012	handbook	source (1)			
497	Bloomer Sch Dist	2011	No	--	handbook	source (1)			
602	Bonduel Sch Dist	2011	Yes	2013	online source (MacIver Inst)	source (1)	source (2)		
700	Brodhead Sch Dist	2011	Yes	2012	online source (MacIver Inst)	source (1)	source (2)		
721	Brown Deer Sch Dist	2011	No	--	handbook	source (1)			
777	Burlington Area Sch Dist	2011	Yes	2013	online news source (Journal Times)	source (1)			
870	Cadott Community Sch Dist	2011	No	--	handbook	source (1)			
910	Campbellsport Sch Dist	2011	No	--	handbook	source (1)			
1015	Cedarburg Sch Dist	2011	No	--	handbook	source (1)			
1080	Chetek-Weyerhaeuser Area Sch D	2011	No	--	board minutes	source (1)	source (2)		
1085	Chilton Sch Dist	2011	Yes	2012	district website	source (1)			
1092	Chippewa Falls Area Unified Sc	2011	Yes	2012	online news source (Chippewa Herald)	source (1)	source (2)		
1141	Clintonville Sch Dist	2012	No	--	online news source (Waupaca now)	source (1)	source (2)		
1155	Cochrane-Fountain City Sch Dis	2011	no info	no info	online source (EAG)	source (1)			
1162	Colby Sch Dist	2011	Yes	2012	handbook	source (1)			
1183	Columbus Sch Dist	2011	Yes	2012	online news source (MJS)	source (1)	source (2)		
1232	Crivitz Sch Dist	2013	No	--	handbook	source (1)	source (2)		
1253	Cudahy Sch Dist	2011	No	--	online news source (Cudahy Now); handbook	source (1)	source (2)		

4970	D C Everest Area Sch Dist	2011	Yes	2012	board minutes	source (1)	source (2)		
1295	Darlington Community Sch Dist	2011	No	--	handbook	source (1)			
1316	De Forest Area Sch Dist	2011	Yes	2012	board minutes	source (1)	source (2)		
1414	De Pere Sch Dist	2011	Yes	2013	handbook	source (1)	source (2)	source (3)	
1421	De Soto Area Sch Dist	2011	Yes	2012	board minutes	source (1)			
1309	Deerfield Community Sch Dist	2011	Yes	2012	board minutes	source (1)	source (2)		
1380	Delavan-Darien Sch Dist	2011	No	--	district website	source (1)	source (2)		
1407	Denmark Sch Dist	2011	No	--	board minutes	source (1)			
2744	Dodgeland Sch Dist	2011	No	--	board minutes	source (1)	source (2)		
1428	Dodgeville Sch Dist	2011	Yes	2012	district website; handbook	source (1)	source (2)		
1499	Durand Sch Dist	2011	Yes	2012	online source (MacIver Inst)	source (1)	source (2)	source (3)	
1540	East Troy Community Sch Dist	2011	No	--	district website	source (1)			
1554	Eau Claire Area Sch Dist	2011	Yes	2012	handbook; online source (MacIver Inst)	source (1)	source (2)	source (3)	source (4)
1568	Edgerton Sch Dist	2011	Yes	2013	online source (MacIver Inst)	source (1)	source (2)	source (3)	
1638	Elkhorn Area Sch Dist	2011	Yes	2013	handbook	source (1)	source (2)		
1659	Ellsworth Community Sch Dist	2011	Yes	2012	online source (MacIver Inst)	source (1)	source (2)		
714	Elmbrook Sch Dist	2011	No	--	online news source (Patch)	source (1)	source (2)	source (3)	source (4)
1666	Elmwood Sch Dist	2011	No	--	online source (WILL)	source (1)			
1694	Evansville Community Sch Dist	2011	Yes	2013	online news source (GazettExtra)	source (1)	source (2)		
1729	Fall Creek Sch Dist	2012	No	--	handbook	source (1)			
5757	Flambeau Sch Dist	2011	no info	no info	online source (WILL)	source (1)			
1862	Fond du Lac Sch Dist	2011	No	--	online news source (MJS)	source (1)	source (2)		
1883	Fort Atkinson Sch Dist	2012	No	--	union contract	source (1)	source (2)		
1900	Franklin Public Sch Dist	2011	No	--	district document	source (1)			
2009	Galesville-Ettrick-Trempealeau	2011	No	--	online source (WEA)	source (1)			
2058	Germantown Sch Dist	2011	No	--	union contract	source (1)	source (2)	source (3)	source (4)
2114	Gibraltar Area Sch Dist	2012	Yes	2013	handbook	source (1)			
2184	Glendale-River Hills Sch Dist	2012	No	--	board meeting minutes	source (1)			
2212	Goodman-Armstrong Creek Sch	2011	No	--	handbook	source (1)			
2217	Grafton Sch Dist	2011	No	--	handbook	source (1)			
2233	Grantsburg Sch Dist	2011	No	--	handbook	source (1)			
2289	Green Bay Area Public Sch Dist	2011	Yes	2013	online source (MacIver Inst); board minutes	source (1)	source (2)		
2310	Green Lake Sch Dist	2011	Yes	2013	online source (MacIver Inst)	source (1)	source (2)		
2296	Greendale Sch Dist	2011	No	--	online news source (Patch)	source (1)			
2303	Greenfield Sch Dist	2011	No	--	handbook	source (1)			
2420	Hamilton Sch Dist	2011	No	--	district website	source (1)	source (2)		
2443	Hartford J1 Sch Dist	2011	No	--	online source (WILL)	source (1)	source (2)		
2460	Hartland-Lakeside J3 Sch Dist	2011	No	--	online source (EAG)	source (1)	source (2)		
2478	Hayward Community Sch Dist	2011	No	--	handbook	source (1)			
2527	Highland Sch Dist	2011	Yes	2012	online news source (The Dodgeville Chronicle)	source (1)			
2562	Holmen Sch Dist	2011	Yes	2012	online news source (The Lacrosse Tribune)	source (1)	source (2)	source (3)	source (4)
2576	Horicon Sch Dist	2011	No	--	online source (EAG)	source (1)			
2583	Hortonville Area Sch Dist	2011	Yes	2013	handbook	source (1)	source (2)		
2604	Howard-Suamico Sch Dist	2011	No	--	online source (EAG)	source (1)	source (2)		

2605	Howards Grove Sch Dist	2011	Yes	2012	handbook	source (1)				
2611	Hudson Sch Dist	2011	No	--	online source (EAG)	source (1)	source (2)	source (3)		
2632	Independence Sch Dist	2012	No	--	handbook	source (1)				
2639	Iola-Scandinavia Sch Dist	2011	Yes	2012	handbook	source (1)	source (2)			
2646	Iowa-Grant Sch Dist	2011	Yes	2012	handbook	source (1)	source (2)			
2695	Janesville Sch Dist	2013	No	--	online news source (Channel3000)	source (1)	source (2)	source (3)		
2730	Johnson Creek Sch Dist	2011	Yes	2012	school board minutes	source (1)				
2758	Kaukauna Area Sch Dist	2011	No	--	handbook	source (1)	source (2)			
1376	Kettle Moraine Sch Dist	2011	No	--	handbook	source (1)	source (2)	source (3)		
2800	Kewaskum Sch Dist	2011	Yes	2012	handbook	source (1)	source (2)	source (3)		
2814	Kewaunee Sch Dist	2011	Yes	2013	school board minutes	source (1)				
2828	Kiel Area Sch Dist	2011	No	--	handbook	source (1)				
2835	Kimberly Area Sch Dist	2011	Yes	2012	board minutes	source (1)	source (2)	source (3)		
2842	Kohler Sch Dist	2011	no info	no info	online source (EAG)	source (1)				
2849	La Crosse Sch Dist	2011	Yes	2012	online news source (The Lacrosse Tribune)	source (1)	source (2)	source (3)	source (4)	
3862	Lake Country Sch Dist	2011	no info	no info	online source (EAG)	source (1)				
3647	Lakeland UHS Sch Dist	2011	No	--	online source (WEA)	source (1)				
3129	Little Chute Area Sch Dist	2011	Yes	2012	school board minutes	source (1)	source (2)	source (3)		
3150	Lodi Sch Dist	2012	No	--	handbook	source (1)	source (2)	source (3)		
3220	Luxemburg-Casco Sch Dist	2011	Yes	2012	handbook	source (1)				
3269	Madison Metropolitan Sch Dist	2013	Yes	2016	union contract; district website	source (1)	source (2)	source (3)	source (4)	source (5)
3276	Manawa Sch Dist	2011	no info	no info	online news source (Waupaca County News)	source (1)				
3290	Manitowoc Sch Dist	2011	No	--	handbook	source (1)				
3297	Maple Sch Dist	2011	Yes	2012	school board minutes	source (1)	source (2)			
3304	Marathon City Sch Dist	2011	No	--	handbook	source (1)				
3311	Marinette Sch Dist	2011	Yes	2012	handbook	source (1)				
3332	Marshall Sch Dist	2011	Yes	2012	district press release	source (1)				
3339	Marshfield Unified Sch Dist	2011	Yes	2012	handbook	source (1)	source (2)			
3360	Mauston Sch Dist	2011	No	--	online news source (Wiscnews); handbook	source (1)				
3381	McFarland Sch Dist	2011	Yes	2013	online news source (WI State Journal); handbook	source (1)	source (2)			
3409	Medford Area Public Sch Dist	2012	no info	no info	handbook	source (1)	source (2)			
3428	Melrose-Mindoro Sch Dist	2011	No	--	online news source (The Lacrosse Tribune)	source (1)				
3430	Menasha Joint Sch Dist	2011	Yes	2012	school board minutes	source (1)	source (2)			
3437	Menomonee Falls Sch Dist	2011	Yes	2013	online source (EAG)	source (1)	source (2)			
3444	Menomonie Area Sch Dist	2011	Yes	2013	handbook	source (1)	source (2)			
3479	Mequon-Thiensville Sch Dist	2011	No	--	online source (EAG); district website	source (1)	source (2)			
3500	Merrill Area Sch Dist	2011	Yes	2012	online news sources	source (1)	source (2)	source (3)		
3549	Middleton-Cross Plains Area Sc	2013	Yes	2014	online news source (Channel 3000)	source (1)	source (2)	source (3)		
3612	Milton Sch Dist	2011	No	--	online news source (GazettExtra)	source (1)	source (2)			
3619	Milwaukee Sch Dist	2012	Yes	2013	online sources (EAG, MJS)	source (1)	source (2)	source (3)		
3633	Mineral Point Unified Sch Dist	2011	No	--	handbook	source (1)				
3640	Minocqua J1 Sch Dist	2011	No	--	online source (WEA)	source (1)				
3661	Mishicot Sch Dist	2011	No	--	online source (WEA)	source (1)				

3668	Mondovi Sch Dist	2011	no info	no info	online source (WILL)	source (1)				
3675	Monona Grove Sch Dist	2011	No	--	handbook	source (1)	source (2)			
3682	Monroe Sch Dist	2011	No	--	handbook	source (1)	source (2)			
3689	Montello Sch Dist	2011	No	--	online news source (Portage Daily Register)	source (1)				
3794	Mount Horeb Area Sch Dist	2011	No	--	handbook	source (1)				
3822	Mukwonago Sch Dist	2011	No	--	handbook	source (1)				
3857	Muskego-Norway Sch Dist	2011	No	--	school board minutes	source (1)	source (2)			
3871	Necedah Area Sch Dist	2011	No	--	online news source (Juneau County Star Times)	source (1)				
3892	Neenah Joint Sch Dist	2011	No	--	union contract	source (1)	source (2)	source (3)		
3899	Neillsville Sch Dist	2011	No	--	handbook	source (1)				
3906	Nekoosa Sch Dist	2011	Yes	2012	online source (MacIver)	source (1)				
3925	New Berlin Sch Dist	2011	Yes	2012	handbook	source (1)				
3941	New Holstein Sch Dist	2011	No	--	handbook	source (1)				
3948	New Lisbon Sch Dist	2011	No	--	online news source (Juneau County Star Times)	source (1)				
3955	New London Sch Dist	2011	Yes	2012	online news source (Mercury News)	source (1)				
3962	New Richmond Sch Dist	2011	No	--	handbook	source (1)	source (2)			
3983	North Fond du Lac Sch Dist	2011	Yes	2012	news source (The Reporter Fond du Lac)	source (1)	source (2)			
1526	Northland Pines Sch Dist	2011	No	--	union-district arbitration documents	source (1)	source (2)			
4018	Oak Creek-Franklin Joint Sch D	2011	Yes	2012	online news source (Patch)	source (1)				
4025	Oakfield Sch Dist	2013	No	--	school board minutes	source (1)				
4060	Oconomowoc Area Sch Dist	2011	No	--	handbook	source (1)	source (2)			
4074	Oconto Falls Public Sch Dist	2011	Yes	2014	handbook; online news source (Oconto Times Herald)	source (1)	source (2)			
4067	Oconto Unified Sch Dist	2011	no info	no info	handbook	source (1)	source (2)			
4088	Omro Sch Dist	2011	Yes	2012	handbook	source (1)	source (2)			
4095	Onalaska Sch Dist	2011	Yes	2012	handbook	source (1)	source (2)	source (3)		
4144	Oregon Sch Dist	2011	Yes	2012	online news source (WSJ)	source (1)	source (2)			
4165	Osceola Sch Dist	2011	No	--	online news source (PressPubs)	source (1)				
4179	Oshkosh Area Sch Dist	2011	Yes	2012	online source (EAG)	source (1)	source (2)	source (3)	source (4)	source (5)
4186	Osseo-Fairchild Sch Dist	2012	No	--	board minutes	source (1)				
4207	Owen-Withee Sch Dist	2011	No	--	board minutes	source (1)				
4221	Palmyra-Eagle Area Sch Dist	2011	No	--	handbook	source (1)				
4228	Pardeeville Area Sch Dist	2011	Yes	2013	online news source (Portage Daily Register)	source (1)	source (2)			
4151	Parkview Sch Dist	2011	No	--	board minutes	source (1)	source (2)			
4305	Peshtigo Sch Dist	2011	no info	no info	online news source (Peshtigo Times)	source (1)				
4312	Pewaukee Sch Dist	2011	No	--	handbook	source (1)				
4368	Pittsville Sch Dist	2011	No	--	online source (WEA)	source (1)				
4389	Platteville Sch Dist	2011	Yes	2012	handbook	source (1)				
4473	Plymouth Joint Sch Dist	2011	Yes	2013	online source (MacIver)	source (1)	source (2)			
4508	Port Edwards Sch Dist	2012	No	--	handbook	source (1)				
4515	Port Washington-Saukville Sch	2011	Yes	2012	online news source (Patch)	source (1)				
4501	Portage Community Sch Dist	2011	Yes	2012	handbook	source (1)	source (2)			
4529	Potosi Sch Dist	2012	no info	no info	handbook	source (1)				
4543	Prairie du Chien Area Sch Dist	2011	No	--	handbook	source (1)				

4557	Prairie Farm Public Sch Dist	2011	No	--	online source (MacIver)	source (1)				
4578	Prescott Sch Dist	2011	No	--	online source (EAG)	source (1)	source (2)			
4613	Pulaski Community Sch Dist	2011	No	--	online source (EAG)	source (1)	source (2)	source (3)		
4620	Racine Unified Sch Dist	2011	Yes	2013	union contract; online news source (Journal Times)	source (1)	source (2)	source (3)	source (4)	source (5)
4641	Random Lake Sch Dist	2011	Yes	2012	board minutes	source (1)				
4753	Reedsburg Sch Dist	2011	No	--	online news source (Reedsburg Time Press)	source (1)	source (2)			
4781	Rhinelander Sch Dist	2011	No	--	handbook	source (1)				
4865	Rio Community Sch Dist	2011	no info	no info	online source (The Wisconsin Taxpayer)	source (1)				
4872	Ripon Area Sch Dist	2011	Yes	2012	online source (EAG); handbook	source (1)	source (2)			
4893	River Falls Sch Dist	2011	Yes	2012	board minutes	source (1)	source (2)			
4904	River Ridge Sch Dist	2011	Yes	2012	board minutes	source (1)				
5523	River Valley Sch Dist	2011	Yes	2013	budget hearing documents	source (1)				
4956	Rosendale-Brandon Sch Dist	2011	no info	no info	online source (The Wisconsin Taxpayer)	source (1)				
4963	Rosholt Sch Dist	2013	no info	no info	board minutes	source (1)				
4998	Rubicon J6 Sch Dist	2011	no info	no info	online source (The Wisconsin Taxpayer)	source (1)				
5019	Saint Croix Falls Sch Dist	2011	No	--	handbook	source (1)				
5026	Saint Francis Sch Dist	2011	No	--	board minutes	source (1)				
5100	Sauk Prairie Sch Dist	2011	Yes	2012	online news source (The Sauk Prairie Eagle)	source (1)	source (2)			
5138	Seymour Community Sch Dist	2011	Yes	2012	online news source	source (1)				
5264	Shawano Sch Dist	2011	Yes	2012	online news source (Shawano Leader)	source (1)	source (2)			
5271	Sheboygan Area Sch Dist	2011	Yes	2012	district documents	source (1)	source (2)	source (3)	source (4)	
5278	Sheboygan Falls Sch Dist	2011	Yes	2012	handbook	source (1)	source (2)			
5306	Shell Lake Sch Dist	2011	No	--	board minutes	source (1)				
5348	Shiocton Sch Dist	2012	No	--	board minutes	source (1)				
5355	Shorewood Sch Dist	2011	No	--	handbook	source (1)				
5376	Siren Sch Dist	2011	Yes	2012	online news source (PressPubs)	source (1)				
5390	Slinger Sch Dist	2013	No	--	online news source (Journal Sentinel)	source (1)	source (2)	source (3)		
5432	Somerset Sch Dist	2011	Yes	2012	online news source (RiverTowns)	source (1)	source (2)			
5439	South Milwaukee Sch Dist	2013	No	--	online news source (South Milwaukee)	source (1)	source (2)			
4522	South Shore Sch Dist	2012	No	--	handbook	source (1)				
5457	Southern Door County Sch Dist	2011	Yes	2012	board minutes	source (1)	source (2)			
5460	Sparta Area Sch Dist	2011	no info	no info	online source (EAG)	source (1)				
5474	Spooner Area Sch Dist	2011	No	--	handbook	source (1)				
5593	Stanley-Boyd Area Sch Dist	2011	Yes	2012	district documents	source (1)				
5607	Stevens Point Area Public Sch	2011	Yes	2012	union contract	source (1)	source (2)			
5621	Stoughton Area Sch Dist	2011	Yes	2012	handbook	source (1)	source (2)			
5628	Stratford Sch Dist	2011	no info	no info	online source (EAG)	source (1)				
5642	Sturgeon Bay Sch Dist	2011	Yes	2013	online source (MacIver, EAG)	source (1)	source (2)			
5656	Sun Prairie Area Sch Dist	2011	Yes	2012	handbook	source (1)				
5663	Superior Sch Dist	2011	Yes	2012	online news source (Superior Telegraph)	source (1)	source (2)	source (3)	source (4)	
3510	Swallow Sch Dist	2011	No	--	online source (WILL)	source (1)				
5726	Thorp Sch Dist	2012	No	--	handbook	source (1)				

5747	Tomah Area Sch Dist	2011	Yes	2012	handbook	source (1)	source (2)	source (3)	source (4)	
5754	Tomahawk Sch Dist	2011	No	--	online news source (Tomahawk Leader; EAG)	source (1)	source (2)			
126	Tomorrow River Sch Dist	2011	Yes	2012	online source (MacIver)	source (1)	source (2)			
4375	Tri-County Area Sch Dist	2011	No	--	online source (MacIver)	source (1)				
5824	Two Rivers Public Sch Dist	2011	No	--	online source (MacIver)	source (1)	source (2)	source (3)		
5901	Verona Area Sch Dist	2011	No	--	handbook	source (1)	source (2)			
5985	Viroqua Area Sch Dist	2011	Yes	2012	online news source (Vernon County Broadcaster)	source (1)	source (2)	source (3)		
6027	Washburn Sch Dist	2012	No	--	handbook	source (1)				
6113	Waterford Graded J1 Sch Dist	2011	Yes	2012	online news source (The Journal Times)	source (1)				
6118	Waterloo Sch Dist	2011	No	--	handbook	source (1)				
6125	Watertown Unified Sch Dist	2011	Yes	2012	handbook	source (1)	source (2)	source (3)		
6174	Waukesha Sch Dist	2011	Yes	2012	handbook	source (1)	source (2)	source (3)		
6181	Waunakee Community Sch Dist	2011	Yes	2012	board minutes, handbook	source (1)	source (2)			
6195	Waupaca Sch Dist	2011	No	--	online news source (Waupaca County Post)	source (1)				
6216	Waupun Sch Dist	2011	Yes	2013	online souce (MacIver)	source (1)	source (2)			
6223	Wausau Sch Dist	2011	Yes	2013	online news source (Wsaw)	source (1)				
6230	Wausaukee Sch Dist	2011	no info	no info	union contract	source (1)				
6244	Wauwatosa Sch Dist	2011	Yes	2012	online news sources (Patch, MJS)	source (1)	source (2)			
6293	Webster Sch Dist	2012	no info	no info	online source (WTA)	source (1)				
6300	West Allis-West Milwaukee Sch	2011	Yes	2012	online news source (MJS)	source (1)	source (2)			
6307	West Bend Sch Dist	2011	No	--	online news source (Heartland)	source (1)				
6370	West Salem Sch Dist	2011	Yes	2012	board minutes	source (1)	source (2)			
6321	Westby Area Sch Dist	2011	Yes	2012	online news source (Westby Times)	source (1)				
6354	Weston Sch Dist	2011	No	--	online source (EAG)	source (1)				
6384	Weyauwega-Fremont Sch Dist	2011	no info	no info	online source (Waupaca County News)	source (1)				
6412	Wheatland J1 Sch Dist	2011	No	--	handbook	source (1)				
6440	White Lake Sch Dist	2011	No	--	district documents	source (1)				
6419	Whitefish Bay Sch Dist	2011	No	--	online news source (Patch)	source (1)	source (2)	source (3)		
6461	Whitewater Unified Sch Dist	2011	Yes	2013	online source (EAG)	source (1)				
6470	Whitnall Sch Dist	2011	No	--	online source (EAG, Patch)	source (1)	source (2)			
6678	Wisconsin Dells Sch Dist	2011	Yes	2014	online source (MacIver)	source (1)	source (2)			
469	Wisconsin Heights Sch Dist	2011	Yes	2013	union contract; online news source (WSJ)	source (1)	source (2)			
6685	Wisconsin Rapids Sch Dist	2011	Yes	2013	online source (MacIver)	source (1)	source (2)	source (3)	source (4)	source (5)