Flexible Wages, Bargaining, and the Gender Gap*

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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. The gap is unlikely to be driven by observable gender differences in job mobility or teacher ability, although the threat of moving and a high demand for male teachers could exacerbate it. Our results suggest that pay discretion and wage bargaining are important determinants of the gender wage gap and that institutions, such as unions, might help narrow this gap.

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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 pay gaps (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, Knabe, and Leipold, 2014; Exley, Niederle, and Vesterlund, 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.

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. 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 (as in Baron, 2018; Litten, 2016; Biasi, 2021) , due to long-standing differences in districts' negotiation calendars which pre-dated Act 10, to estimate the effect of the introduction of flexible pay on the gender pay gap for teachers. While no 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. 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 also 1.6 times the post-Act 10

[1] For example, Card, Cardoso, and Kline (2016) find that women are underrepresented in firms with a high bargaining surplus. Studying US real estate transactions, Goldsmith-Pinkham and Shue (2021) find that women pay more for housing properties and sell them for less than men. Using data from Denmark, Andersen et al. (2021) 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.

difference in pay associated with a one-standard deviation higher value-added.

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 persisted 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, 2021).

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 gap as large as 12 percent.

In line with our wage 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,

2021).

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 garnering outside offers. We find that women are as likely as men to move, and differences in mobility alone cannot explain the 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.

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.

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

[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.

policies which prevent some workers from taking advantage of performance pay, simply because they are less likely to negotiate.

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, Knabe, and Leipold, 2014) and ask for lower pay (Roussille, 2020).[4] 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.[5] 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, Avery, and Zemsky, 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, Barankay, and Rasul, 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 as Litten (2016), Baron (2018), Roth (2019), Biasi (2021), and Biasi, Fu, and Stromme (2021).[6]

Lastly, our results speak to the literature on the relationship between unionization and collective bargaining and the gender pay gap.[7] Existing studies have found that countries with

[4] Exley, Niederle, and Vesterlund (2019) also find that women correctly select into bargaining, suggesting that forcing women to bargain could result in suboptimal outcomes.

[5] 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).

[6] 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, Fu, and Stromme (2021) study the impacts of the reform on teacher sorting.

[7] A large literature has documented a negative relationship between unionization and income inequality (Card, 1996; Dinardo, Fortin, and Lemieux, 1996; Farber et al., 2021; Card et al., 2020). Fortin and Lemieux (1997) argue that deunionization impacted pay inequality among men but that the minimum wage was more important for women's pay.

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).[8] 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.[9] 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, and Section 7 concludes. All appendix materials are in the Online Appendix.

2 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.

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

[8] 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.

[9] Controlling for variables like sorting is especially important given the recent work by Farber et al. (2021) that shows that sorting into unionized jobs has varied substantially over time. 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.

the same time, the gender wage gap for teachers increased from 9 to 11 percent (Figure I, panel (a)).

[Figure I here]

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 (public-private)-state-year cell, for public school teachers (red squares) and for all other employees (blue circles).[10] 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, which represented a key part of each district's CBA.[11]

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 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

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

[11] 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.

of union dues from employees' paychecks.[12]

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.[13] Second, it required districts to stop paying the employees' share of retirement contributions (amounting to 5.8 percent 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.[14]

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).[15] 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).[16] In sum, individual wage 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

[12] 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.

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

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

[15] 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).

[16] 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.

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. 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.[17] 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. 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 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 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

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

Wisconsin, and characteristics of job assignments (school identifiers, grades and subject taught, and full-time equivalency, or FTE, units).[18] 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.[19] We express salaries in FTE units, so that the salary of each teacher corresponds to a full-time position regardless of her actual hours. The characteristics of male and female teachers are summarized in Table I, separately for the years preceding and following Act 10.

[Table II here]

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 information on extensions had no extension.[20]

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). We

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

[19] We exclude long- and short-term substitute teachers, teaching assistants and other support staff, and contracted employees because 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 discard these observations.

[20] 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).

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 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, socio-economic status (SES), migration status, English-learner status, and disability.[21] 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 model of Kane and Staiger (2008) and estimate VA as the teacher-specific component of a standard achievement model using an empirical Bayes estimator.[22]

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 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 and described in detail in Appendix B. This measure exploits teacher turnover across grades and schools (rather than classrooms) over time (as in Rivkin, Hanushek, and Kain, 2005). 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.

[21] 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.

[22] Our achievement model controls for 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.

4 The Effect of Flexible Pay On The Gender Wage Gap

In this section, we begin by describing our identifying variation. We then analyze the impact of flexible pay on the gender gap in teachers' salaries and explore heterogeneity in the gap.

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. 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).

[Figure II here]

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.

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).

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 that men and women's salaries followed a similar trajectory until a CBA expiration; after this point a gender pay gap emerged and grew over time.[23]

[23] Panel (a) of Figure III shows 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. When we refer to "time-to-expiration" throughout the paper, we indicate the final extension date for districts that received an extension, and

[Figure III here]

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:

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

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 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 $postext_{jt}$, 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^{exp}_j and Y^{ext}_j, control flexibly for time-specific factors that are common to all districts whose CBAs and extensions expired in the same year.[24]

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

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

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 date otherwise. Salaries of teachers in districts with missing expiration data are shown in Appendix Figure AII.

[24] 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.

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.

[Figure IV here]

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:

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

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 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.[25] 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

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

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, to 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).[26]

[Table II here]

A recent literature has pointed to issues with dynamic difference-in-differences designs, and in particular to 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, which could bias the estimates (Borusyak, Jaravel, and Spiess, 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), outlined in greater detail in 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, 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, Jaravel, and Spiess (2021) and Cengiz et al. (2019).[27]

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.,

[26] 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.

[27] 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 in Cengiz et al. (2019) consists of aggregating event-by-event analyses and is further discussed in Appendix C.

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 difference between these two estimates is small and indistinguishable from zero (column 3).[28]

[Table III here]

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.[29] If this is the case, allowing for

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

[29] 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.

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.[30] 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

Gender wage gaps have been shown to grow across workers' careers (see Zeltzer, 2020, for a study of physicians). Motivated by these findings, we now test for heterogeneity in the gap by age and seniority. In contrast with the existing evidence, we find that estimates of δ_s in equation (2) are larger at 1.5 percent four years after the expiration for teachers with six or fewer years of experience (the bottom quartile of the distribution) and smaller at 0.5 percent for those with 20 or more years (the top quartile, Figure V, panel a). They are also larger for 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), point estimates suggest that young women gain less from flexible pay. Importantly, the salary gap is unlikely 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.[31]

[Figure V here]

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;

[30] 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.

[31] 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. 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.

Cullen and Perez-Truglia, 2021). 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.[32] 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).[33]

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, poo2ling together years before and after a CBA expiration or its extension.[34] 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.

[Table IV here]

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).[35]

This finding suggests that women are not just earning less than men everywhere; rather, the gender composition of district leadership matters.[36] The absence of a salary gap when the

[32] See https://wasb.org/legal-human-resources-services/basic-resources/new-school-board-member-handbook/chapter-1-beginning-your-school-board-service/ for a sample of summary of superintendents' duties.

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

[34] Estimates are also robust to assigning teachers to the gender of their principal in the years prior to Act 10 (Appendix Table AIII).

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

[36] Perhaps surprisingly, the gender of the superintendent seems to matter more than that of the principal (i.e., the coefficient estimate for "Female teacher × male superintendent × post extension" is larger than that for "Female

superintendent is female could occur if women are better able to negotiate with other women (or men are worse at negotiating with women) or if they experience backlash as 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 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.[37]

4.6 Additional Robustness Checks

Accounting for Compositional Changes Following Act 10, retirement rates spiked among Wisconsin teachers (Roth, 2019; Biasi, 2019). To ensure that our results are not driven by compositional changes to the pool of teachers, in panels a-c of Appendix Figure AVII and columns 1-3 of Appendix Table AV we show that our results are robust to (i) restricting our analysis to a balanced panel of teachers in the eight years surrounding each expiration; (ii) restricting it to teachers who entered Wisconsin public schools between 2007 and 2011; (iii) controlling for teacher fixed effects in our main specifications. Our estimates are robust to these tests.

Accounting for Endogenous Switches Across Districts If the teacher movements across districts that followed after Act 10 (Biasi, 2021) happened in response 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. Estimates are comparable using this approach (panel (b) of Appendix Figure AVII and column 4 of Appendix Table AV).

teacher × male principal × 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.

[37] 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.

Allowing for Different Salary Schedules Across Districts To account for the possibility that districts changed the salary schedule after Act 10 in a way that impacted the gender wage gap, we allow the parameter vectors β_1 and β_2 in equation (2) to be district-specific. Our estimates of the gender gap remain robust (panel (c) of Appendix Figure AVII and column 5 of Appendix Table AV).

Controlling for Grade-Specific Effects Our main analyses control for grade group effects (elementary, middle, or high school). However, sorting based on gender and differences in pay could occur within grade groups, giving rise to a gender gap. Our results are robust to controlling for fixed effects for a teacher's lowest and highest grade taught, alone and interacted with an indicator for years after a CBA expiration (panel (c) of Appendix Figure AVII and column 6 of Appendix Table AV).

Controlling for Extra Duties Four percent of men and 2.7 percent of women take on at least one extra duty besides teaching (e.g., serving as department head, program coordinator, or sports coach), generally associated with additional pay.[38] If compensation for these duties increased with the introduction of flexible pay and women are less likely than men to take over these duties, a gender gap might arise. However, our results are unchanged when we control for whether teachers perform any other duties besides teaching in equation (2) (panel (d) of Appendix Figure AVII and column 2 of Appendix Table AVI).[39]

Alternative Inference Approaches In our analysis, we conduct inference using standard errors clustered at the district level. Our conclusions are largely unchanged if we instead use t-statistics obtained using a Wild cluster bootstrap (where the clusters are the school districts, Cameron and Miller, 2015, Appendix Table AVII, in brackets) or 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).[40].

[38] Employment records from the DPI include information on all additional non-teaching duties performed by teachers in each year.

[39] Extra duties are associated with an 11 percent pay premium (column 1 of Appendix Table AVI). Column 3 of Table AVI 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).

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

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 gained less from it. Distinguishing between these explanations is important for policy. 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.[41] 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.

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.[42] A total of 3,156 teachers responded to our survey (a 13 percent response rate). The gender and age distributions of the respondents closely resemble those of the teacher population (Appendix Figure AVIII).[43]

[41] 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.

[42] 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. The survey was sent out on March 5, 2020. The survey was closed on May 7th, 2020.

[43] Districts' response rates cannot be explained by 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 (with an F-statistics of joint significance of these variables below 2, Appendix Table AVIII).

5.1 Gender Differences in Negotiation Experiences and Attitudes

Average men's and women's responses to the the survey questions indicate that women are less likely to have negotiated their pay with previous and current employers (Table V). For example, 37.9 percent of men and 29.5 percent of women report having negotiated with past employers (a 22 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.

[Table V here]

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).[44]

Most of our questions concern negotiations over salaries. It is possible, though, that women are more inclined to negotiate job aspects other than 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 disproportionately 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 only 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. 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, Bottom, and Elfenbein, 2013).[45] Women are, however, 13 percent less likely to state that they are confident talking to people they

[44] 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.

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

don't know. Lastly, women in our data are 12 percent less likely than men to report that their performance is above average.[46]

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 VI 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).

[Table VI here]

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 negotiations with their current employer, at the start of the relationship, were successful (19 percent, Table VI, 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.[47] 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

[46] This finding is in line with Exley and Kessler (2019), who show that women assess their own performance more negatively than men.

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

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. In line with these findings, survey data show that the observed gender differences in bargaining are largely driven by teachers working under a male superintendent. 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 V). Controlling for district and teacher attributes, we confirm that women working under a male superintendent are 8.3 percentage points less likely than men to have negotiated their pay with their current employer at the start of the work relationship (estimate for *Female*, Table VI, panel A, column 2), 5.7 percentage points 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 equally likely to have negotiated their pay at the start of the current job (estimate for *Female* + *Female* × *F super*, Table VI, panel A, column 2), to have negotiated after the start (column 4) or to plan to negotiate in the future (column 6).[48]

Women are equally likely to report that their negotiations were successful under female and male superintendent (Table VI, panel B, column 3). The gender of the superintendent is also unrelated to the reported reasons for not negotiating (Appendix Table AIX), although imprecisely estimated coefficients for *Female* × *Female super* prevent us from ruling out large positive or negative values for the point estimates.

5.3 Additional Results

In Biasi and Sarsons (2021) 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

[48] Because we did not ask teachers when past negotiations took place and only have information about superintendents in 2020, we believe the most informative variable to study differences in bargaining by superintendent gender is the likelihood of future negotiations.

AX we further show that controlling for individual attributes (such as knowing the salaries of colleagues, measures of self-confidence, and socio-emotional skills) separately for men and women does not eliminate the gender gap: Estimates for *Female* are largely unchanged. Measures of socio-emotional skills and self-confidence are also associated with a lower likelihood that women report feeling uncomfortable negotiating, but controlling for these attributes does not change the gender differences in answering these questions. Together, these results do not show evidence that 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 and Caveats

Responses to 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 is not driven by knowledge of colleagues' salaries or other people who have negotiated their pay.

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, although the survey provides suggestive evidence that women's reluctance to bargain is an important driver of the gender wage gap, our inability to link survey answers to administrative records prevents us from exactly estimating the portion of the post-Act 10 gender wage gap generated by men's and women's different propensities to negotiate. For this reason, we now explore other explanations for the gender wage gap.

6 Alternative Explanations for the Gender Wage Gap

To better understand the importance of bargaining relative to 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. The data do 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 (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 first re-estimate equation 2 on the subsample of teachers with and without VA.[49] After a CBA expiration, the increase in the gender pay gap is smaller at 0.06 and indistinguishable from zero for teachers with VA (with a p-value of 0.79, Table VII, column 1), while it is larger at 0.33 percent for teachers without VA (significant at 1 percent, column 2).[50]. 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 VII, column 2).[51]

[Table VII here]

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.[52] To test this, in column 4 of Table VII 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.

These estimates indicate that men are compensated for having a high VA, but women are not. Furthermore, in columns 3-4 of Table VII the estimates for *Female * Post Expiration* are the same as in column 2; this implies that, at least in the subsample of teachers with VA, the gender pay gap is not related to 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

[49] 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.

[50] 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, for whom we cannot calculate VA

[51] 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.

[52] To test whether the results in Table VII 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 AIX). Compositional changes are thus unlikely to explain our results.

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, Murphy, and Pierce, 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 than men to relocate and take advantage of higher salaries offered in other districts.[53] 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 AX). To more rigorously test for differences in mobility, we estimate equations of an indicator for mobility as a function of *Female*, *Post extension*, and the interactions between the two. Estimates for *Female* × *post extension*, either without controls (X_{it}, Table VIII, column 1), with district and year fixed effects (column 2), and with controls for experience and education (column 3) are small and indistinguishable from zero. [54] No differences in mobility can be registered either across districts within a commuting zone (CZ, which do not require a relocation, column 4) or across CZs (column 5).[55]

[Table VIII here]

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 VI, panel (a)). These results suggest that *observed* mobility plays at most a small role in explaining the gender gap.[56]

[53] Women have a lower willingness-to-commute than men, possibly because of family obligations (LeBarbanchon, Rathelot, and Roulet, 2021; Caldwell and Danieli, 2018; Manning, 2003). Using survey data from a set of European countries, Hospido (2009) finds no gender differences in moving rates, while Keith and McWilliams (1999) show that women are less likely to quit or change jobs for family reasons.

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

[55] 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 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 AXI).

[56] In Panel B of Appendix Figure AX we also test whether the returns to moving differ for men and women who

[Figure VI here]

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 do 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 VI, panel (b)). These results are in line with the 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's salaries to bid up once Act 10 allowed for individual negotiations. Since demand for teachers 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, the gap is significantly smaller for teachers in elementary schools compared with those in high schools (Appendix Figure AXI, 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 AXI, panel (b)).[57]

It is possible that schools that employ more men do so because they have a higher demand for male teachers. We therefore look at schools that lost and gained men immediately before Act 10. 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 larger in the latter group (Appendix Figure AXI, panel (c)).

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 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.

[57] Appendix Figure AXI 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.

The third instance are schools that enrol a high 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 AXII, panel (a)).[58] 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 AXII, panel (b)).

These results are suggestive of a limited role for a higher demand for male teachers in explaining the gender wage gap. Another possible reason for a high demand 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 it should be kept in mind when interpreting our estimates and represents an avenue for future research.

7 Conclusion

A difference in men and women's willingness to negotiate is often discussed as a potential contributor to the gender wage gap. The erosion of union power and the rise of flexible pay could open the possibility that pay differences emerge based in part on willingness to negotiate. This paper uses data from a large set of public-sector employers, the Wisconsin public school districts, to shed light on these questions. 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. We find that when school districts adopted flexible pay, a salary gap emerged between men and women.

Responses to a survey sent to all Wisconsin teachers suggest that women are less likely to have negotiated their salary. We find no evidence that women underestimate the returns to or are worse at bargaining. We also do not find observable differences in mobility between men and women, though it is possible that men have more outside job offers that they use to negotiate

[58] 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.

their salaries in their current district.

Our results 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). Our findings 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 male superintendent or principal. If this is a sign of discrimination against women, policies such as providing negotiations training may be ineffective.

Supplementary Material

An Online Appendix for this article can be found at the Quarterly Journal of Economics online.

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

	(1) Women	(2) Men	(3) Difference	(4) 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. **??** contains the full list of questions. $^{*} \leq 0.1$, $^{**} \leq 0.05$, $^{***} \leq 0.01$.

Table VI: Survey answers: Negotiating behavior

Panel A) Past and future negotiations

	Current employer				Likelihood future negotiations			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	At start	At start	After	After	Salary	Salary	Classroom assgn	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)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Cond	Uncond	Cond	Not possible	Not comfortable	Useless	Fear backlash	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 VII: 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

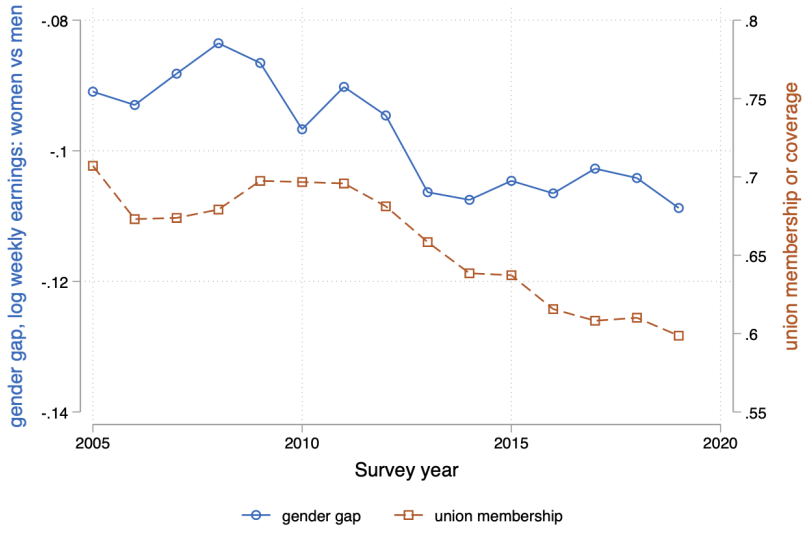
Note: The dependent variable is the natural logarithm of salary per year, in full-time equivalency units and multiplied by 100. The variables *Female* and *Male* equal one for female and male workers, respectively, and the variable *Post Extension* equals one for years following the expiration of a CBA 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 VIII: Gender differences in job mobility

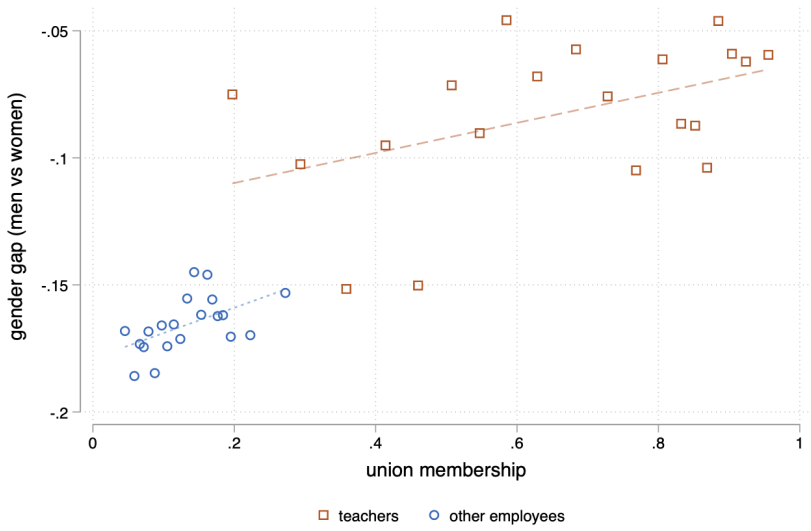
	All moves			Within CZ	Across CZ
	(1)	(2)	(3)	(4)	(5)
Female	-0.0014**	-0.0012**	-0.0009	0.0004	-0.0013**
	(0.0006)	(0.0005)	(0.0007)	(0.0003)	(0.0005)
Post Extension	0.0179***	0.0036	0.0007	0.0024*	-0.0012
	(0.0012)	(0.0023)	(0.0028)	(0.0014)	(0.0018)
Female × Post Extension	-0.0004	-0.0004	-0.0010	-0.0006	-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



(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)

Figure I:
Gender wage gap and unionization

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 (public vs private)-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. We classify industries using one-digit codes of the IND1990 variable from the Census Bureau and occupations using one-digit codes of the OCC1990 variable from the Census Bureau. Estimates are obtained using data from the Current Population Survey, Merged Outgoing Rotation Groups for 2005 to 2019. The sample is restricted to full-time workers.

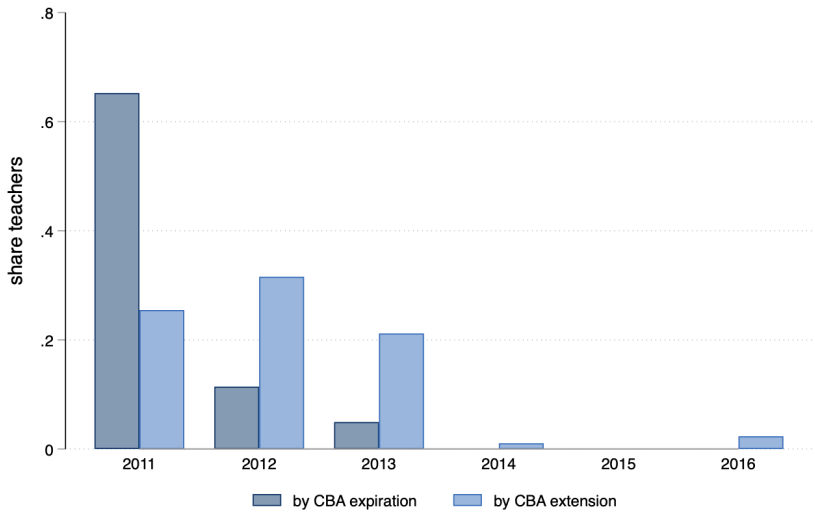
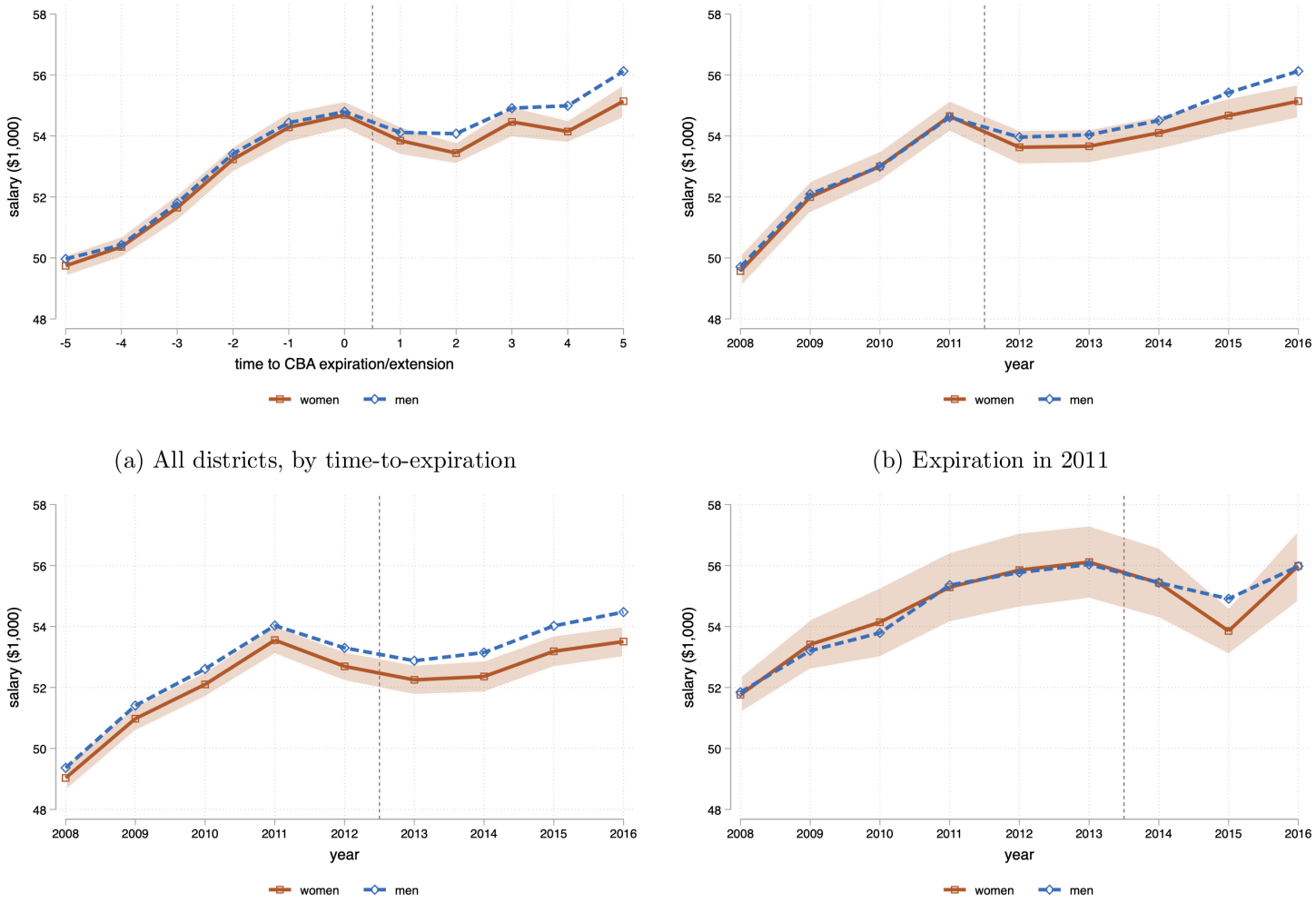


Figure II:
Share of teachers, by expiration and extension dates of CBAs

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).



(a) All districts, by time-to-expiration

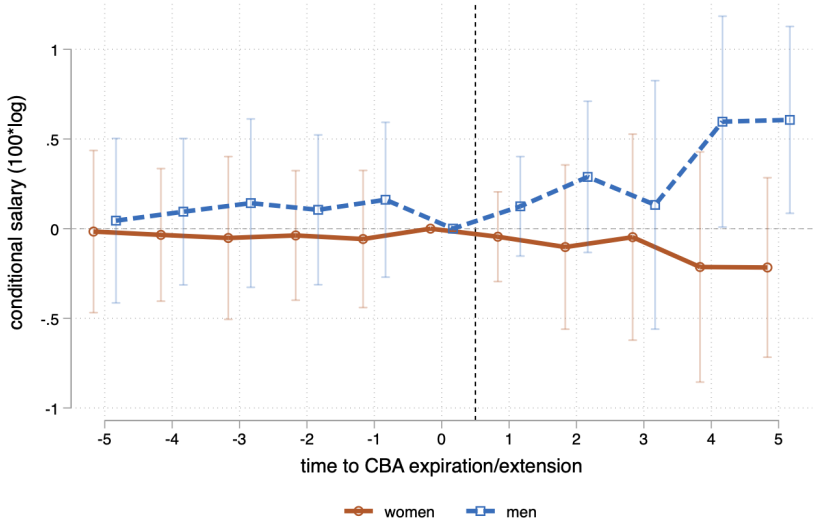
(b) Expiration in 2011

(c) Expiration in 2012

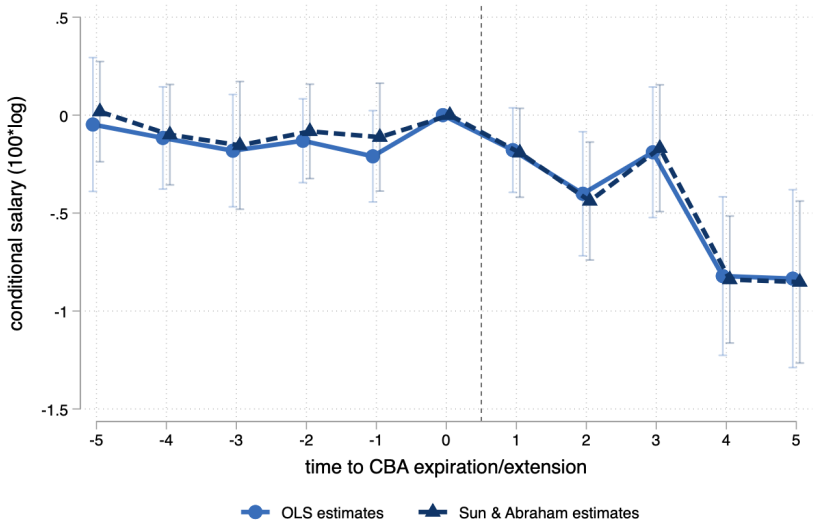
(d) Expiration in 2013, 2014, or 2016

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.



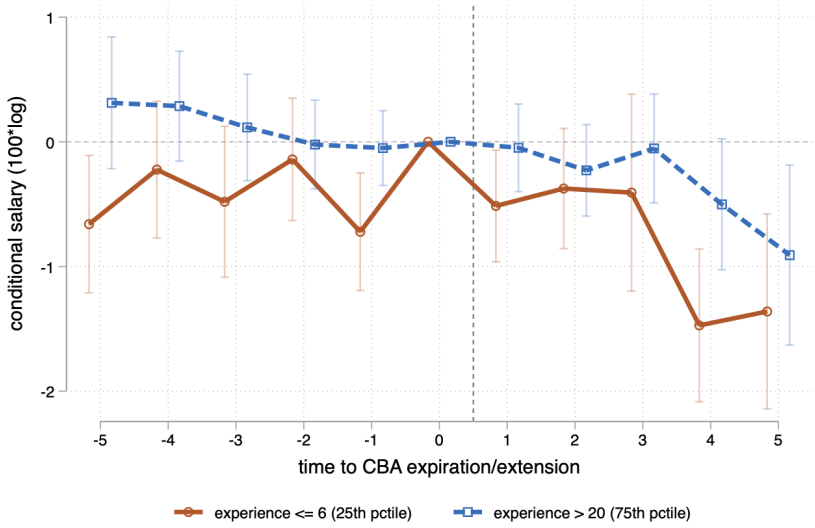
(a) Salaries of men and women, by time-to-expiration/extension of CBAs



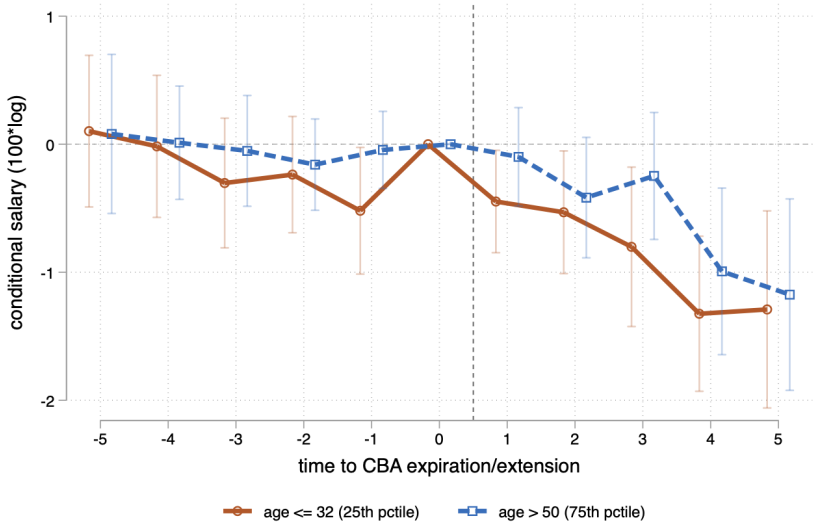
(b) Gender gap in salaries, by time-to-CBA expiration/extension

Figure IV:
Flexible pay and the gender wage gap

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 **??**. All coefficients are plotted relative to the year a CBA or its extension expired ($t = 0$). Standard errors are clustered at the district level.



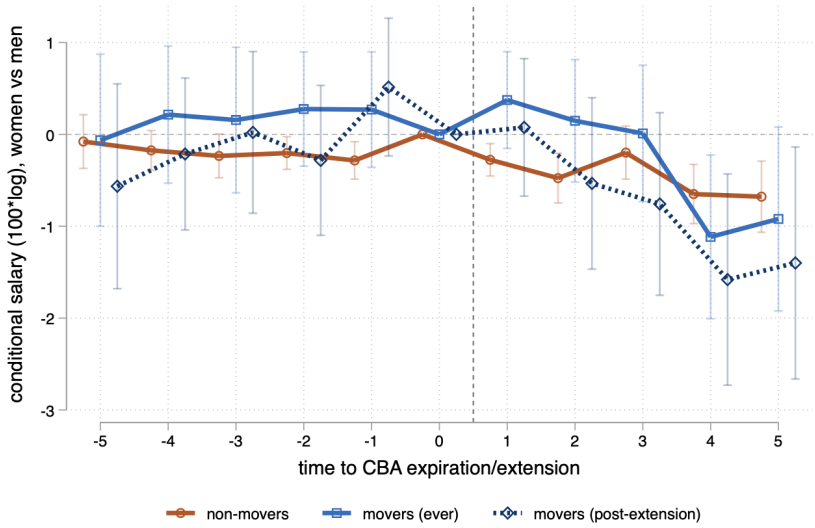
(a) By Seniority



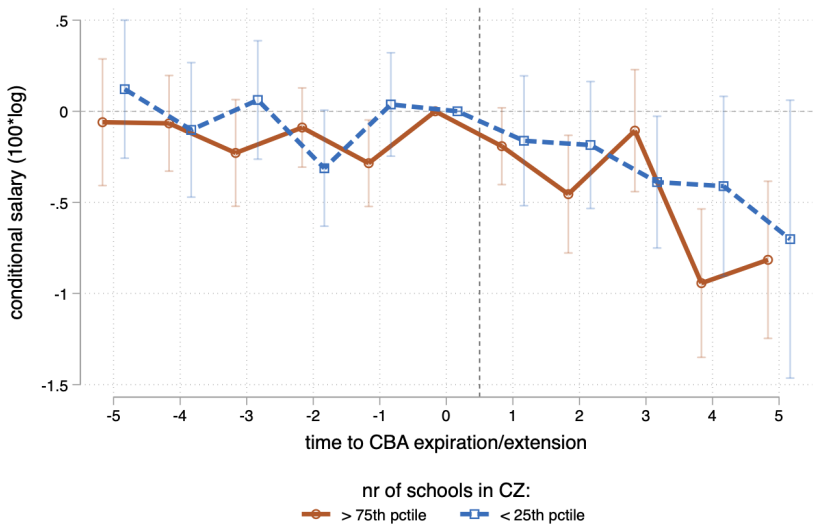
(b) By Age

Figure V:
Gender pay gap, by seniority and 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.



(a) Movers vs non-movers



(b) Gender pay gap and and outside options

Figure VI:
Gender pay gap and job mobility

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.