Flexible Wages, Bargaining, and the Gender Gap*

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Abstract

Does the introduction of flexible pay in industries historically characterized by rigid salaries penalize women? To study this question we analyze wages of male and female public school teachers in the aftermath of Wisconsin's Act 10, a state bill that gave school districts the autonomy to set teachers' pay more flexibly and on an individual basis. Using variation in the timing of expiration of collective bargaining agreements, which determined when districts could start using this freedom, we show that the introduction of flexible pay gave rise to a gap in wages of male and female teachers with the same credentials. This gap is not driven by gender differences in the propensity to change district, ability, or a higher demand for male teachers. The gap, however, is absent in schools with female principals and districts with female superintendents. Survey evidence suggests that the gap might be driven by female teachers being less likely to negotiate pay compared with males, especially when their superintendent is a man, because they do not feel comfortable doing so. We interpret these findings as evidence that environmental factors may play a significant role in shaping labor market outcomes of men and women in contexts where workers are required to negotiate.

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1 Introduction

There has long been a belief that women are reluctant to negotiate for higher salaries, giving a workplace advantage to men and exacerbating gender gaps (Sandberg, 2013).[1] A body of evidence, mainly from laboratory settings, generally supports this hypothesis, finding that women avoid situations in which they have to negotiate or bargain (Babcock and Laschever, 2003; Dittrich and Leipold, 2014; Exley et al., 2019). Whether the differences found in the lab translate to non-experimental settings has been difficult to study, though, as workers can sort into jobs based on whether negotiating is required.[2] At the same time, as individually based compensation becomes more prevalent, especially in labor markets traditionally characterized by rigid pay schemes (such as the public sector), understanding whether more flexible pay schemes disadvantage women is important to address the gender wage gap.

In this paper we use the passage of Wisconsin's Act 10, a state bill which dramatically redefined the rules of collective bargaining for public sector employees, to test whether and how individual pay negotiations affect gender gaps in pay. We focus our analysis on teachers, a class of workers for whom, before Act 10, pay was strictly determined on the basis of seniority and academic credentials using rigid salary schedules negotiated between each school district and the teachers' union. After Act 10 unions lost the authority to bargain over the schedule. Instead, upon the expiration of the pre-existing collective bargaining agreements (CBAs), districts became free to set teachers' pay more flexibly on an individual basis, without union consent. Some districts adopted flexible pay that allowed salaries to be set differently for each teacher ("flexible-pay" districts), giving rise to an increase in pay dispersion among teachers with the same seniority and credentials (Biasi, 2018). Others chose to keep a seniority-based pay schedule ("seniority-pay" districts); even in these districts, however, teachers could negotiate their placement on the schedule.

Using variation in the timing of expiration of CBAs pre-dating Act 10, driven by long-standing differences in districts' negotiation calendars, we estimate the effect of the introduction

[1] The "Lean In" movement advocated for women to promote themselves in the workplace and ask for promotions and pay raises.

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

of flexible pay on the difference in salaries of observationally similar male and female teachers. While no gender pay gap existed before Act 10, the introduction of flexible pay led to a one percent decline in women's salaries relative to their male counterparts. Although small in dollar terms, this gap is quite large compared with the limited pay variation among teachers with the same credentials prior to Act 10. For example, it corresponds to 10 percent of a standard deviation of pre-Act 10 conditional salaries and 10 percent of the average salary increase a teacher with 10 years of seniority would receive upon acquiring a Master's degree.

Our estimates of the gender wage gap are robust to controlling for teacher characteristics, teaching and grade assignments, as well as district and time effects. In addition, they are robust when accounting for changes in the composition of the teaching body across districts (driven, for example, by early retirement; Biasi, 2019) and for endogenous assignment to the policy change (driven by teachers moving across districts to contrast the effects of flexible pay). Perhaps surprisingly, the gap is present in both flexible-pay and seniority-pay districts. However, in seniority-pay districts, the gap is largely driven by male teachers being placed on higher steps of the salary schedule compared with observationally similar women.

Differences across teachers of different age and seniority indicate that flexible pay penalizes young and inexperienced teachers the most. While the gender wage gap is 0.7 percent for teachers with more than 15 years of seniority, it is larger at 1.5 percent and more persistent over time for teachers with less than five years of seniority. Similarly, the gap is smaller for teachers aged 45 and older and larger for teachers aged 30 and younger.

What explains the rise of a gender gap in pay following the introduction of flexible pay? We focus our attention on four classes of explanations which we consider plausible: differences in teaching quality, differences in teachers' propensities to move, higher demand for male teachers, and the role of workplace environment. First, we study whether the gap is explained by gender differences in teaching quality. If districts use their acquired flexibility to compensate teachers with higher value-added (Biasi, 2018), a gender gap in pay could arise if women are less effective than men at teaching. We do not find any evidence in support of this hypothesis: Women have a slightly higher value-added both before and after Act 10, and controlling for value-added leaves the estimate for the gender pay gap unchanged in all districts.[3] Furthermore, while the returns to having a high value-added become positive for men after the introduction of flexible pay they

[3] This is in contrast with evidence from three performance pay programs for teachers in North Carolina (Hill and Jones, 2020). There, female teachers' value-added declines with the introduction of performance pay, while men's remains relatively flat. We do not find evidence of this and argue that this does not appear to drive the gender pay gap in our setting.

remain virtually at zero for women, suggesting that women are not as able as men to have their teaching ability rewarded.

A second explanation involves differences in geographic mobility and selective sorting across districts. Biasi (2018) shows that the introduction of flexible pay after Act 10 was followed by an increase in cross-district movements of teachers and exits from the profession. If taking advantage of flexible pay requires teachers to be able to move across districts (from a seniority-pay to a flexible-pay district), the post-Act 10 gender pay gap might be driven by men's differential ability to move compared to women's. Our data indicate instead that men and women became equally more likely to change districts after Act 10. Furthermore, we do not find any evidence of differential sorting by gender or value-added across flexible-pay and seniority-pay districts. Of course, these results do not rule out the possibility that men are able to garner better outside offers than women (without actually moving at a higher rate), and as a result they have more bargaining power in the negotiations. However, this is a mechanism through which the gender wage gap is driven by differences in men's and women's abilities to bargain.

Third, the gender wage gap could reflect excess demand for male teachers. We test for this possibility in two ways. First, we check whether the gap is larger in elementary schools, where men are vastly underrepresented.[4] We find instead that the gap is significantly smaller for elementary school teachers (0.4 percent after five years) compared to high-school teachers (1.1 percent after five years). Second, we test whether the pay gap differs between districts and schools that had a large versus small share of male teachers before Act 10. Again, we find that the gap is largest in districts and schools where more than 30 percent of the teaching body is male. As districts have limited resources, this result might also be driven by the fact that higher pay for one (male) teacher may come at the expense of higher pay for another (female) teacher. Nevertheless, these findings do not seem to support the hypothesis of the gender gap being driven by a larger demand for male teachers.

Finally, we study whether the gender pay gap is related to workplace environment, and in particular to the gender composition of schools' and districts' leadership. We find that, five years after an expiration, the gender pay gap is 0.7 percent for teachers working in schools which had a male principal in the years immediately preceding Act 10; instead, no gap exists in schools with a female principal. Similarly, the gap is 0.8 percent larger in districts with a male superintendent, compared with districts with a female leader.

[4] Approximately 80 percent of male teachers teach in middle or high schools.

Our findings indicate that, while penalized on average, women do not always lose in contexts where wages are subject to bargaining, and they suggest that individual attributes and the workplace environment may matter for the outcomes of the negotiations. Nevertheless, the above results do not tell us whether women avoid bargaining (in some contexts more than others) or whether they do bargain, but obtain a lower payoff or are punished when doing so.

To distinguish between these mechanisms, we ran a survey with all current public school teachers in Wisconsin. We asked respondents whether they have ever negotiated their pay or plan on doing so in the future. We then asked why the respondents who had not negotiated chose not to do so, and for those who had negotiated, whether they were successful. We also collected information on teachers' knowledge about their colleagues' salaries and measures of socio-emotional skills as proxies for bargaining ability.

Teachers' answers to our survey indicate that women are between 12 and 20 percent less likely than men to have negotiated their pay in the past or to anticipate doing so in the future. Differences in information on colleagues' salaries and socio-emotional skills do not explain this difference. In line with our earlier results, however, we find that the gender difference in the likelihood of negotiating is entirely driven by teachers working under a male superintendent. Men and women who work under a female superintendent are instead equally likely to negotiate their salaries. When asked about the reasons for not negotiating, women are more likely than men to report that they do not feel comfortable doing so.

Taken together, our results suggest that the introduction of flexible pay, while beneficial for incentivizing workers to exert more effort under some conditions, can be detrimental for the outcomes of some subgroups of the workforce. Workforce environmental factors, rather than gender differences in bargaining ability, are likely explanations for the observed disparities in negotiating outcomes between men and women, even in a female-dominated occupation like public school teaching. Our findings also highlight how institutions, such as unions, can play a role in closing the gender wage gap.

Our paper contributes to several literatures on gender inequality in the labor force. A mainly experimental literature has highlighted the importance of bargaining for the gender pay gap, showing that women are less likely than men to negotiate and giving credence to the idea that women should bargain more (Babcock and Laschever, 2003; Leibbrandt and List, 2014; Dittrich and Leipold, 2014). One notable exception is Exley et al. (2019), who also find a gender bargaining gap but find that women select into bargaining when the returns from doing so are positive.

This finding implies that forcing women to bargain can perpetuate, rather than close, gender gaps in pay. Our paper confirms these findings by showing that a pay gap emerges when men and women are required to bargain over their salaries, and sheds light on the possible mechanisms behind it.

We also contribute to a growing body of evidence on the impact of the gender composition of firm leadership on women's career outcomes, which has so far found mixed results. While studies of the effects of gender quotas for firm boards generally do not find any positive impact for women in other parts of the organization (Bertrand et al., 2019; Maida and Weber, 2019), other works have 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). An advantage of our context is that we are able to look at different types of school leaders, which carry on different functions: School principals are responsible for evaluating and managing teachers, whereas district superintendents are involved in the negotiations and ultimately decide over teachers' pay. We find that women lose the most when they negotiate with male leaders, a result that points towards female representation in leadership as a way to combat gender inequality in the workplace (Matsa and Miller, 2011; Athey and Zemsky, 2000; Langan, 2019).

Our paper also relates to the literature on the effects of changes in pay schemes on workers' outcomes. Most of this literature (especially the one on teachers) has studied the effects of various forms of performance pay on employees' selection and incentives (for example Lazear, 2000a,b; Bandiera et al., 2005; Neal et al., 2011). We focus instead on the gender wage gap as a possibly unintended consequence of a new pay scheme, designed to allow employers to pay higher salaries to more productive workers, which also rewards behaviors and actions (such as negotiating) that men and women might be differentially more likely to undertake.

Lastly, our results speak to the literature on de-unionization and the gender pay gap. Studies of aggregate trends tend to find no impact (or small positive impacts) of unionization on this gap (Antonczyk and Sommerfeld, 2010; Blau and Kahn, 2006). However, these works are generally unable to fully control for worker sorting and productivity and lack a proper control group. Following teachers over several years allows us to account for sorting and teacher ability, and to estimate a precise and negative impact of de-unionization on the gender gap in this setting.

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 main results in section 4. We explore mechanisms in sections

5. Section 6 describes our survey and its results, and section 7 concludes.

2 Institutional Background: Teacher Pay and Act 10

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

In states where teachers are authorized to collectively bargain with school districts, these schedules are negotiated between each district and the teachers' union.[5] Collective-bargaining agreements (CBA) typically do not allow for individual pay adjustments; this implies that seniority and credentials (along with "overtime" or extra-curricular activities, for example coaching a sports team) are the only determinants of salaries and pay is unrelated (at least directly) to teacher effectiveness (Podgursky, 2006).

2.1 Wisconsin's Act 10

Until 2011, salaries of all teachers in Wisconsin were set on a schedule, which districts negotiated with the union. [6] These schedules were a key part of CBAs and listed in each district's employee handbook, a document that describes rights and duties of all district employees.

The rules disciplining teacher pay dramatically changed on June 29, 2011, when the state legislature passed the Wisconsin Budget Repair Bill in an attempt to close a projected $3.6 billion budget deficit. The bill, which became known as Act 10, introduced a series of changes to the powers and duties of all public sector unions, including teachers' unions. First and most importantly, the Act limits the scope of collective bargaining: While before Act 10 unions could negotiate the entire salary schedule, after the Act negotiations must be limited to base salaries. Second, Act 10 requires unions to recertify every year by obtaining the absolute majority of all members' votes in yearly elections. Third, it limits the validity of newly stipulated CBAs to one year; and lastly, it prohibits automatic collection of union dues from employees' paychecks.[7]

[5]In states without collective bargaining (such as Georgia or North Carolina), salary schedules are generally established at the state level.

[6]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.

[7]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

The Act also contained a number of budget-cutting rules for public school districts. It required them to stop paying the employees' share of retirement contributions (amounting to 5.8 each employee's annual salaries) and to reduce health insurance premiums by increasing employees' contributions and by choosing cheaper plans. An amendment to Act 10 (Act 32 of July 2011) also reduced state aid to school districts and decreased their revenue limit.[8]

Implications For Teacher Pay With the end of collective bargaining, school districts became free to set teacher pay more flexibly. While until 2011 pay depended exclusively on seniority and academic credentials, after Act 10 districts could reward teachers for other attributes without union consent. Using information collected from districts' employee handbooks, Biasi (2018) shows that different districts used this flexibility in different ways: As of 2015, approximately half of all districts (122 out of 224 for whom handbooks are available) were still setting pay using a schedule only based on experience and education, whereas the remaining half had discontinued the use of such a schedule. In the latter group, districts started paying high-quality, young teachers more and reduced the growth in pay for some high-seniority teachers (Biasi, 2018).

Differences In The Timing of The Introduction of Flexible Pay The provisions of Act 10 had immediate effect on all teacher unions and school districts starting from the academic year 2011-2012. Existing CBAs stipulated between unions and school districts before 2011, however, remained binding until their expiration. Pre-Act 10 CBAs fully regulated teacher pay with a salary schedule; as a result, districts could only use their freedom to flexibly set teacher pay after the expiration of their CBAs. In addition, after Act 10 some districts decided to extend the validity of their CBAs by one or two years.

Due to differences in electoral cycles, the expiration dates of pre-existing CBAs (and of their extensions) varied across districts. Figure I summarizes these cross-district differences. While the 76 percent of districts' CBAs expired in 2011 and were not extended, 18 percent expired in or were extended until 2012 (including the school district of Madison) and an additional 7 percent expired in or were extended until in 2013 (including the school district of Milwaukee). Thus, approximately half of all teachers were covered by districts with CBAs that expired or were extended after 2011. Cross-district differences in expiration and extension dates introduce plausibly random variation in the timing of introduction of flexible pay, which we use in our

https://www.wsj.com/articles/SB10001424052702304821304577436462413999718.

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

empirical analysis.

3 Data

Our main data set includes individual-level information on the universe of public school teachers in Wisconsin. We combine these data with hand-collected information on the school districts, including their post-Act 10 salary regimes and the expiration dates of their CBAs. We also link teacher records with student-level information on demographic characteristics and test scores in Math and Reading, which we use to calculate teacher value-added. Data are reported by academic 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 Wisconsin, and characteristics of job assignments (school identifiers, grades and subject taught, and full-time equivalency (FTE) units). The data set also includes total salaries and fringe benefits for each teacher. 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.[9] We express salaries in FTE units, so that the salary of each teacher corresponds to a full-time position regardless of her actual hours. One limitation of the salary information is that we do not observe extra duties (such as serving in committees or acting as sports coaches) for which teachers might be receiving extra pay (which would be included in total salaries). We discuss this data limitation along with our results. The characteristics of male and female teachers are summarized in Table I, separately for years preceding and following Act 10.

Pre-Act 10 CBAs We collected information on districts' CBAs from multiple sources. The first are districts' union contracts set to expire around 2011. The second are local newspaper articles from 2011. Many of these articles reported on the negotiations taking place and offered enough information to discern when the district's agreement was slated to expire. Several articles also

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

mentioned that the uncertainty surrounding Act 10 influenced many districts to simply extend their current contracts for one or two years. Our third source are school board meeting minutes from 2011, which describe whether the contract was set to expire in 2011, whether an extension was granted, and for how long. When possible, we prioritize data from union contracts, complementing it with the other two sources when unavailable. We were able to successfully find information on the expiration and extension dates for 211 out of 428 school districts, enrolling 78 percent of all teachers. For the remaining 217 districts with missing information, we assume the that the CBA expired in 2011 and no extension was granted; our main results are robust to this assumption.

Employee Handbooks and Salary Schedules To better understand how districts used their flexibility in terms of pay setting after the expirations of CBAs and their extensions, 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 enroll 83 percent of all students).[10] As in Biasi (2018), we classify a district as "seniority-pay" if its 2015 handbook contains a salary schedule and does not mention rewards for performance or merit, and as "flexible-pay" otherwise. If a handbook contains a schedule but mentions bonuses linked to performance, the district is classified as flexible-pay.

Student Test Scores and Demographic Information Test scores data are available for for all students in grades 3 to 8 and for the years 2006-2017, and 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 including gender, race and ethnicity, socio-economic (SES) status, migration status, English-learner status, and disability.[11]

3.1 Value-Added

We measure teachers' quality using value-added (Rivkin et al., 2005; Kane and Staiger, 2008; Chetty et al., 2014), an estimate of each teacher's contribution to the growth in achievement.

[10] Unclassified districts (i.e., those for which handbooks are not available) either do not have a website or do not make their handbook public. Biasi (2018) shows that districts without a website are smaller, enroll more disadvantaged students, pay lower salaries, and are disproportionately located in rural areas.

[11] The WKCE was administered in November of each school year, whereas the Badger test was administered in the spring, or 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.

The starting point is the following model of achievement:

$$A_{kt} = \beta X_{kt} + \nu_{kt}, \text{ where } \nu_{kt} = \mu_{i(kt)} + \theta_{c(kt)} + \varepsilon_{kt} \tag{1}$$

A_{kt} is a standardized measure of test scores for student k in year t, X_{kt} is a vector of student and school-specific controls, and $i(kt)$ denotes student k's teacher in t.[12] Teacher value-added is the estimate of $\mu_{i(kt)}$, the teacher-specific component of test score residuals.

Value-added is usually estimated using datasets containing classroom identifiers, in which teachers can be linked to the students they taught. Until 2017, the WDPI did not record classroom identifiers but only teachers' and students' schools and grades. This implies that we cannot link a teacher to the actual students she taught, but only to those in her school and grade. To estimate value-added in the presence of this data limitation we follow the approach of Biasi (2018), which exploits teacher turnover across grades and schools over time.[13] With multiple years of data, turnover permits the identification of a single teacher's effect by comparing test score residuals $\bar{\nu}_{gst}$ before and after her arrival in a given grade and school. Importantly, turnover helps identify not only the effect of a teacher who switches, but also that of the teachers in her same grade and school at any point in time.

We construct value-added measures using an empirical Bayes estimator, modified to reflect the structure of the data and described in detail in Appendix B. The Appendix also shows that, although noisier than the standard estimators, our measure still explains a substantial portion of the variance in test scores, and it is a forecast-unbiased estimator of both standard estimates and future student achievement. We calculate the value-added of each teacher allowing it to differ before and after Act 10, to account for changes in effort. Value-added estimates are available for 23,581 teachers of math and reading in grades 4 to 8, including 19,187 teachers in 224 flexible-pay and seniority-pay districts.

[12]The vector X_{kt} includes the following: 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.

[13]Rivkin et al. (2005), who face a similar challenge using data from Texas, also use teacher turnover to identify the variance of teacher effects.

4 Flexible Pay and The Gender Gap

4.1 Empirical Strategy

To identify the effects of flexible pay on the differences in salaries between men and women we take advantage of the fact that, following Act 10, districts were only allowed to discontinue the use of salary schedules after the expiration of existing CBAs.

The timing of these expirations varied across districts (Figure I), reflecting 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.[14] 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. Table II (columns 1-3) shows no large differences in observable characteristics of on-schedule and off-schedule school districts (those with CBAs expiring in 2011 and after 2011, respectively); the latter are more likely to be located in suburban areas and serve a larger share of Black students (the latter being largely driven by the school district of Milwaukee).

After the CBAs expired, 100 school districts (23 percent) decided to extend the validity of their agreements by one or two years, presumably to gain more time to re-design the pay schemes. While the timing of expiration of CBAs can be considered as good as random, the enactment of an extension was the result of a choice made by the district. Table II (columns 4-6) shows that districts with an extension are larger, have lower revenues, and are more likely to be located in urban and suburban areas.

In our analysis we make use of variation in the timing of expiration of both CBAs and their extensions. While only the former can be considered random, as long as the reasons that induced school districts to opt for an extension are not directly related to the differences in salaries between men and women, this strategy allows us to estimate the effects of flexible pay on the gender wage gap. For completeness we also report our main results obtained using exclusively the variation in the timing of CBA expiration (i.e., ignoring the extensions), as well as using the timing of CBA expirations as an instrument for CBA extensions. Estimates are largely robust to this choice.

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

4.2 Evolution of Salaries for Men and Women Over Time

Before Act 10, salaries were determined by attributes such as experience, academic credentials, and teaching assignment (i.e., grade level and subject) and followed a strict pay schedule. Table AI shows the difference between men and women's salaries before Act 10. Without controlling for the variables that went into salary schedules, women earned 0.9 percent less than men (column 1). However, as we control for teacher characteristics such as experience, qualifications, and teaching assignment, this gap disappears (column 5).

Following the expiration of CBAs, districts acquired the freedom to pay different salaries to teachers with identical experience and credentials. To understand the implications of flexible pay for the gender wage gap, we start by studying the evolution of conditional salaries for men and women, after the expiration of CBAs or their extensions.[15] We employ the following event study design:

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

where $\ln(w_{ijt})$ is the natural logarithm of the salary of teacher i working in district j in year t. The vector X_{it} contains indicators for teacher i's highest education degree and for years of experience, both 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 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 allow us to 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 for time-specific factors that are common to all districts whose

[15] Panel A of Appendix Figure AI shows the evolution of raw nominal salaries of male and female teachers between 2007 and 2016. Salaries of men and women are similar and increase at a steady rate until 2011, when both men and women are earning roughly $54,000 per year. The growth in salaries slows abruptly with the passage of Act 10, likely due to the Act's imposition of a limit in the growth rate of base pay. After Act 10, salaries grow significantly less for female teachers (who earn just over $54,000 by 2016) compared with men (who earn $55,000). A similar pattern emerges if we plot raw salaries by time-to-expiration of a CBA, instead of by year (Panel B).

CBAs and extensions expired in the same year. The variable G_i^g is a gender indicator (where g denotes the gender), and it is interacted with indicators for years since the expiration or the end of an extension of a CBA. In this equation, the coefficient δ_s^g gives the relative change in salaries of individuals of gender g, conditional on all the other determinants of salaries, in a window around the expiration of a CBA.

Estimates of δ_s for men and women, shown in Figure II, indicate that conditional salaries of men and women were on similar, flat trends in the years leading to a CBA expiration. Five years after the expiration, however, salaries of women had fallen by 0.3 percent relative to the year prior to the expiration, whereas salaries of men had increased by 0.7 percent (both estimates are significant at 1 percent). The pattern is similar when we use CBA expirations instead of extensions (Figure AII). While small in an absolute sense, these changes appear significant when compared with the limited variation in conditional salaries among Wisconsin public school teachers prior to Act 10. In particular, a 0.7 percent increase in salaries for men corresponds to 7 percent of a standard deviation of pre-Act 10 conditional salaries and 6 percent of a standard deviation of post-Act 10 salaries. It also corresponds to 1.7 times the average increase in salaries associated with earning an additional year of seniority prior to Act 10, for teachers with 20 to 25 years of seniority and a Master's degree.

4.3 Gender Gaps 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 with an event study of the form:

$$\begin{aligned} \ln(w_{ijt}) &= \beta_1' X_{it} + \beta_2' X_{it} \times postext_{j(it)t} + \gamma_1' T_{it} + \gamma_2' T_{it} \times postext_{j(it)t} + \theta_{j(it)} \\ &\quad + \theta_{j(it)} \times postext_{j(it)t} + \tau_t + \tau_t \times Y_{j(it)}^{exp} + \sum_{s=-4}^{5} \delta_s Female_i \times \mathbb{1}(t - Y_{j(it)}^{ext} = s) + \epsilon_{it} \end{aligned} \tag{3}$$

where all variables are defined as before and the variable $Female_i$ equals one if the teacher is female. In this equation, estimates of the coefficients δ_s give the differential impact of flexible pay on the salaries of women relative to men.

Estimates of δ_s, shown in Figure III along with confidence intervals, indicate that a significant gender pay gap appeared right after the expiration of districts' CBAs. 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 one percent five years after the expiration. In dollar terms, this estimate implies that women earned \$540 per year less than men. While small in percentage terms, this gap corresponds to 10 percent of a standard deviation of conditional salaries prior to Act 10 (equal to \$5,302), and to 57 percent of the standard deviation increase following the CBA expiration (equal to \$670).

These results are summarized in Table III, where we pool all years together and estimate

$$\begin{aligned} \ln(w_{ijt}) &= \beta_1' X_{it} + \beta_2' X_{it} \times postext_{j(it)t} + \gamma_1' T_{it} + \gamma_2' T_{it} \times postext_{j(it)t} + \theta_{j(it)} \\ &\quad + \theta_{j(it)} \times postext_{j(it)t} + \tau_t + \tau_t \times Y^{exp}_{j(it)} + \delta_0 Female_i + \delta Female_i \times postext_{j(it)t} + \epsilon_{it} \end{aligned} \tag{4}$$

Estimates of δ_0 indicate that, before a CBA expiration, women and men earned similar salaries conditional on observables. In the five years following the expiration of a CBA or of its extension, however, salaries of women became 0.3 percentage points lower than salaries of men (Table III, column 1, significant at 5 percent). Allowing the coefficient δ to vary for each of the years following an extension indicates that the gap was largest five years after the expiration, at 0.8 percent (column 2). The gap is robust to only using the date of expiration of CBAs (as opposed to the the final date of its extension, columns 3 and 4), as well as to to instrumenting dates of CBA extensions with dates of CBA expiration (columns 5 and 6).

4.4 Flexible Pay, Seniority Pay, and Gender Differences in The Returns to Experience

While all districts became free to negotiate pay with individual teachers after the passage of Act 10, seniority-pay districts continued to use a salary schedule uniquely based on seniority and academic credentials. If the use of a schedule prevents districts from using discretion in setting teacher pay, we should not see any gender wage gaps in seniority-pay districts.

We test this hypothesis in Figure IV, which shows estimates of the coefficients in equation (3) obtained separately for flexible-pay and seniority-pay districts. The data does not support the hypothesis: The gender wage gap is similar in flexible-pay and in seniority-pay districts. The former see a 0.6 percent increase in the difference in salaries between male and female teachers five years after a CBA extension (significant at 1 percent), while the latter experience a 1.2 percent increase in this difference (the difference in these two estimates, however, is not statistically different from zero, as shown in column 3 of Table IV).[16]

[16] In columns 1-3 of Table IV we pool years before and after a CBA extension and re-estimate equation (4) separately for flexible-pay and seniority-pay districts. This exercise reveals an increase in the gender wage gap equal to 0.3

How to rationalize the appearance of a gender wage gap in districts that continued to use a salary schedule? Before Act 10, unions were fully involved in the negotiations on the schedules and guaranteed that no individual-level adjustments could take place. Act 10 prevented union involvement and left open the possibility, for individual teachers, to bargain even with seniority-pay districts, for example in order to obtain a placement on a higher "step" or "lane" of the salary schedule that would guarantee a higher pay. If this is what explains the gender wage gap for seniority-pay districts that emerges in Figure IV, we would observe the salary returns to (actual) seniority and education to be higher for men compared with women after a CBA expiration.

Figure AIII shows evidence in line with this pattern: In the years prior to an expiration, men and women with the same years of experience earn identical salaries, both in flexible-pay and in seniority-pay districts (top panel). After Act 10 and the expiration of CBA extensions, however, women earn less than men at almost all levels of experience (bottom panel).

Next, we test whether the gap that arose in seniority-pay districts can be explained by men being placed on a different salary schedule compared with observationally similar women. We do so by augmenting equation (3) to include fixed effects for the years of actual experience and for having a master's or higher degree, interacted with an indicator for female and an indicator for years following a CBA expiration or extension and allowing these effects to be different before and after a CBA extension. This allows us to separately estimate the returns to experience for men and women in the post-Act 10 period.

The results of this test are shown in the bottom panel of Figure IV; for exposition, we plot the gender gap for teachers with 3 or 4 years of experience and a master's degree (Figure AIV shows the same estimates for teachers with 19 or 20 years of experience). Allowing for gender-specific returns to experience and education completely closes the gender gap in seniority-pay districts. In flexible-pay districts, however, the gap remains large at 1.2 percent five years after an extension. Together these results suggest that, in seniority pay districts, men are able to negotiate different spots on the salary schedule while in flexible pay districts they negotiate on other factors, such as salary directly. Columns 4-6 of Table IV summarize these findings.[17]

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percent in flexible-pay districts and 0.6 in seniority-pay districts; the difference in these two estimates, however, is not statistically different from zero (column 3).

[17] It is possible that men are more likely to take part in highly-valued extra-curricular activities, such as being a football coach, and bargain a better spot on the salary schedule. However, extra-curricular participation already factored into teachers' salaries before Act 10. It would therefore have to still be a bargaining story in which men are able to negotiate for a higher salary for participating in extracurriculars. In addition, this would have to correlate with superintendent gender, discussed further in Section 5.

4.5 Gender Gaps in Salaries, Age, and Seniority

To better understand whether the gender wage gap is larger for certain subgroups of teachers, we study heterogeneity in our estimates by teacher age and seniority.

In Panel A of Figure V, we plot estimates of δ_s in equation (3) obtained separately for teachers with less than five (solid line) and more than 15 years of seniority (dashed line). While a gender gap in salaries appears for both seniority groups, it is larger and more persistent for less experienced teachers. For these teachers, the gap is 1.5 percent five years after the expiration of a CBA or its extension (significant at 1 percent); for more experienced teachers, it is smaller at 0.7 percent (significant at 1 percent). These estimates correspond to 12 and 8 of the pre-Act 10 standard deviation in salaries, respectively, and to 140 and 63 percent of its post-CBA expiration increase.

A similar same pattern holds in Panel B, where we plot estimates of δ_s for teachers aged 30 and younger (solid line) and 45 and older (dashed line). The gender pay gap is larger and more persistent among younger teachers. These results could suggest that young women are more likely to opt out of bargaining or to have lower returns to bargaining, possibly because there is less information about them for evaluation.

4.6 Additional Robustness Checks

Accounting for Compositional Changes The estimates presented so far control for fixed effects for the years of seniority. This allows us to always compare teachers with the same seniority, before and after a CBA expiration.[18] Following Act 10, however, retirement rates spiked among Wisconsin teachers (Biasi, 2019; Roth, 2017). To ensure that our results are not driven by changes in the overall composition of the male and female teaching body across districts with different CBA expiration dates, we conduct two additional checks.

First, we restrict our analysis to a balanced panel of teachers in the eight years surrounding each expiration. This allows us to only use, in estimation, teachers who do not retire nor leave the sample.[19] This restriction yields an estimate of the gender wage gap equal to 0.4 percent (Table V, column 1, significant at 1 percent). Second, we re-estimate equation (4) controlling for teacher fixed effects. The corresponding estimate is robust at 0.4 percent (Table V, column 2, significant at 1 percent).

[18]Recall that we are controlling for the number of years an individual has taught, not on the seniority used to place teachers on a salary schedule.

[19]Results are unchanged (although noisier due to a much smaller sample size) if we use the full ten years.

Accounting for Endogenous Switches Across Districts Biasi (2018) shows that the passage of Act 10 was followed by an increase in teacher movements across districts. If these movements are driven (entirely or partly) by teachers' responses to the rise of a gender wage gap after the introduction of flexible pay, the assignment of teachers to our treatment of interest would be endogenous. To gauge the impact of endogenous assignment on our main estimates, we perform intent-to-treat (ITT) tests by assigning teachers to the district they taught in the year prior to the passage of Act 10, even for the years after 2011. ITT estimates, shown in column 3 of Table V, are slightly larger than in our main specifications in Table III and equal to 0.6 percent, suggesting that cross-district movements lessen the impact of Act 10 on the gender wage gap. We study the role of teacher mobility for the gender wage gap in the next section.

Allowing for Different Salary Schedules Across Districts Lastly, we allow for the possibility that the gender wage gap that followed the expiration of districts' CBAs reflected changes in the salary schedules used by districts after Act 10. We do so by allowing the parameters β_1 and β_2 in equation (3) to be district-specific. These results, shown in column 4 of Table V, indicate that a gender gap remains (and becomes larger at 0.7 percent) even when controlling for district-specific schedules.

5 Possible Explanations for The Gender Wage Gap

What drove the increase in the pay gap between similar male and female teachers following the introduction of flexible pay? We focus here on four possible sets of explanations (aside from pure differences in bargaining): 1) gender differences in teaching quality, 2) a differential propensity to change district or exit for men and women, 3) differences in the demand for male/female teachers, and 4) differences in school leadership.

5.1 Gender Differences in Teaching Quality

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

Even if women appear to be better teachers on average, it is still possible that some men have higher quality and are compensated more after the introduction of flexible pay. We check for this possibility by testing whether the gender wage gap can be explained by differences in value-added across teachers. We do so by augmenting equation (3) to flexibly control for value-added, and we estimate:

$$\begin{aligned}\ln(w_{ijt}) \quad = \quad & \beta_1' X_{it} + \beta_2' X_{it} \times postext_{j(it)t} + \beta_3' VA_{it} + \beta_4' VA_{it} \times postext_{j(it)t} + \gamma_1' T_{it} \\ & + \gamma_2' T_{it} \times postext_{j(it)t} + \theta_{j(it)} + \theta_{j(it)} \times postext_{j(it)t} + \tau_t + \tau_t \times Y_{j(it)}^{exp} \\ & + \delta_0 Female_i + \delta Female_i \times postext_{j(it)t} + \epsilon_{it}\end{aligned} \tag{5}$$

where VA_{it} is the value-added of teacher i in year t. Estimates of this equation are shown in Table VI. Because value-added measures are available only for teachers in Math and Reading teaching grades 4-8, column 1 re-estimates equation (3) on this subsample. The post-extension gender wage gap remains is robust at 0.4 percent (significant at 10 percent). Column 2 shows estimates of equation (5): these indicate that the gender wage gap remains stable at 0.4 percent controlling for value-added (significant at 10 percent). A positive estimate on *VA * Post Expiration* (albeit imprecisely estimated) confirms that districts pay teachers with higher value-added more (column 2, p-value equal to 0.27). In line with Biasi (2018), the same result holds (and the estimate of the coefficient is larger) on the subsample of flexible-pay districts (column 5).

In columns 3, 6, and 9 Table VI we further explore whether the post-Act 10 returns to value-added are different among men and women. We do so by further interacting *VA * Post Expiration* with an indicator for *Female*. At -0.078, this estimate completely offset the positive estimate for *VA * Post Expiration*, equal to 0.068 (column 3, significant at 5 and 10 percent respectively). This indicates that, while men are compensated for having a high value-added, women are not. Importantly, however, in these equations the estimates for *Female * Post Expiration* are unchanged; this implies that even women with average value-added experience a wage penalty compared with men with the same value-added.

Movements Between Tested and Non-Tested Positions In Section 4.6 we have shown that our main results are not driven by compositional changes among male and female teachers over time. In the same spirit we also test whether the distribution of men and women across "tested" teaching posts (for whom value-added measures can be obtained) changed after a CBA expiration. The data indicates that neither the likelihood of changing teaching assignment (i.e., grade, subject, or school, Appendix Figure AVII, top panel) nor that of switching from a tested to a non-tested position (bottom panel) changed differentially for men and women around a CBA expiration.

Taken together, these estimates unveil two findings. The first is that a gender wage gap exists even when holding teaching quality fixed. The second is that low-quality women receive lower salaries compared to both high-quality teachers (both men and women) and low-quality men. This last finding can be explained either by a difference in women's abilities to negotiate by value-added, or in their returns to negotiating regardless of ability.[20] It remains unclear, however, why low value-added men are able to gain relatively more from negotiating. In the absence of mobility differences, it would have to be the case that low value-added women are especially unskilled in negotiating their salaries, more so than high value-added women. It could also be the case, though, that low value-added women are less able than their male counterparts to move out of flexible pay districts (where their low value-added would be most penalized) and into seniority pay ones. We explore this possibility next.

5.2 Gender Differences in the Propensity to Move Across Districts

Gender differences in the ability to relocate to a new school district could influence the gender pay gap in several ways. First, if women are less mobile than men, they may have fewer outside options and a lower bargaining power in negotiating, resulting in a gender wage gap (Caldwell and Danieli, 2018). Second, while some workers (for example low-ability ones) may find it profitable to move from flexible-pay to seniority-pay districts to "escape" flexible pay or to avoid bargaining (especially if they face low or negative returns to negotiating), women may be more likely to get stuck in flexible-pay districts if they face higher costs of moving (for example because they are second-earners in the household). Even in the absence of differences in bargaining ability, mobility differences might lead to gender differences in sorting into seniority-pay and flexible-pay districts. The average ability of female teachers in flexible pay districts would then be lower than that of male teachers, leading to a pay gap.

We first look at sorting by value-added. If both male and female teachers can easily move across districts, we would expect low value-added men and women to sort into seniority-pay districts where they can benefit from a set pay scale. In such instances, the pay gap should

[20] The result is roughly in line with Exley et al. (2019) who find that the gap in negotiating is driven by women opting out of bargaining when they anticipate negative returns to doing so. Forcing low value-added women to bargain when they may have negative returns to doing so could hurt them.

disappear. However, if there are differences in men and women's ability to move, a pay gap may persist. Mobility differences could also explain why the gender pay gap disappears over time: If low value-added women are slower to move out of flexible pay districts, the pay gap will persist until they are able to fully sort.

We test whether women are less mobile than men by estimating

$$Moves_{ijt} = \beta_1 Female_i + \beta_2 postext_{jt} + \beta_3 Female_i \times postext_{jt} + \alpha E_{it} + \theta_j + \epsilon_{ijt} \tag{6}$$

where $Moves_{ijt}$ is a dummy indicating that teacher i moved to another school district j in year t and E_{it} is teacher experience. If women are less likely to move after the introduction of flexible pay, either because they are unable to move or choose not to, we would expect to find $\hat{\beta}_3 < 0$.

Estimates of this equation, shown in column 1 of Table VIII, do not show differences in mobility rates by gender after the introduction of flexible pay. Prior to a CBA expiration, mobility rates are 1.49 percent for women and 1.58 percent for men; these rates are not statistically different from each other. While rates of mobility increase after a CBA expiration (with a coefficient on Post Extension equal to 1.5 percentage points), women are not differentially likely to move; the estimate for *Female* × *Post Extension* is equal to a small 0.2 percentage points and it is indistinguishable from zero. In column 2 we test whether women are more or less likely to move within their commuting zone and find a similar result, with men and women being equally likely to move within a CZ both before and after the policy change.

In columns 3-6 of Table VIII we test for differential mobility by teacher value-added. If the pay gap among low value-added teachers is driven by low value-added women being less able to move, we would expect to see significant differences in mobility among these teachers. We find little evidence for this. In fact, teachers do not seem to sort by value-added. High and low value-added teachers are equally likely to move to seniority-pay districts (Table VIII, columns 3 and 4) and to flexible-pay districts (Table VIII, columns 5 and 6). Importantly, though, there are no significant differences by gender: The estimate for *Female* × *Post Extension* is close to zero (ranging from -0.002 to 0.002) in both flexible-pay and seniority-pay districts.

As an additional check, we also compare the average value-added of male and female teachers in seniority-pay and flexible pay-districts following Act 10. If low value-added women are "stuck" in flexible-pay districts, we would expect male teachers to have on average a higher value-added in those districts. In flexible-pay districts the average value-added for female teachers is -0.0001 (with a standard error of 0.0003) and for male teachers is -0.0022 (with a standard error of 0.0006, with the difference being significant at the 1 percent level). In seniority pay districts, women again have a higher average value-added (0.0001 for women vs. -0.002 for men, the difference being significant at the 5 percent level). It is therefore unlikely that sorting by ability is driving the differences in pay.

As last check, we also test for the possibility that the salary gap is larger in districts or labor markets with a greater concentration of schools a teacher could work in, which would represent a valid outside option. For example, if a teacher teaches high school, we measure the number of other high schools in her commuting zone and in her district and correlate it with the gap. Results from this test, shown in Appendix Table AII, indicate that the number of schools in a teacher's commuting zone does not correlate with the size of the gender salary gap (column 1). This is also true when we look at the number of other high schools in a high school teacher's commuting zone (column 2) and the number of elementary schools in an elementary school teacher's commuting zone (column 3).

Taken together, these results indicate that a differential propensity to move across genders can explain the gender wage gap that arose after the introduction of flexible pay. It is of course possible that, while we do not see differences in mobility by gender, men are better able to threaten to move or to garner outside offers. However, we show in Section 5.4 that the salary gap is almost entirely concentrated in districts with male superintendents. For this explanation to hold, men's ability to obtain outside offers would therefore have to correlate with superintendent gender.

5.3 "Excess" Demand for Male Teachers

Men are underrepresented in the teaching profession, especially in elementary schools. A high demand for male teachers could have bid up their salaries once Act 10 allowed for individual negotiations. If this explanation holds, we would expect the gender wage gap to be larger in schools or districts where men are scarcer.

As a first test, we check whether the salary gap is larger in elementary schools, where men represent only 20 percent of the teacher population (approximately 80 percent of male teachers teach in middle or high schools). Figure VI plots the coefficients δ_s in equation (3), estimated separately for elementary and high school teachers. The gap is significantly smaller for teachers in elementary schools (0.4 percent after five years) compared with those in high schools (1.1 percent after five years). This finding does not align with the hypothesis of the gap being driven by a larger demand of men.

We also estimate the salary gap for districts that had a large versus small share of male teachers before Act 10. If higher demand for male teachers drives the gender wage gap, we would expect the gap to be larger in districts with fewer male teachers before Act 10. Estimates of δ_s in schools or districts with a share of male teachers in their schools or districts above and below 30 percent (the 75th percentile in the distribution) are shown in Figure VII. Panel A shows that the gender gap is large at 1.1 percent five years after the expiration of CBAs in schools where more than 30 percent of teachers are men (solid line); the gap is instead indistinguishable from zero for teachers in schools with less than 30 percent male teachers (dashed line). Panel B shows that the gap is instead unrelated to the share of men in the district (rather than in the school), and it is similar across schools with fewer and more men. Table VII (columns 3 and 4) summarizes these results.

We take these results as suggestive evidence that demand for male teachers is not driving the salary gap. It is of course possible that the demand for male teachers is not evenly distributed across schools and it is higher among those with an already larger share of male teachers before Act 10. A larger salary gap in schools with more men could also indicate that men are more successful than women at bargaining, and are in turn are able to secure a larger share of school resources for themselves, or that having more men around changes women's ability to bargain. We explore these mechanisms in Section 6. We further explore heterogeneity in the salary gap by school environment in the next section and again in Section 6.

5.4 The Role of School and District Leadership

Evidence from other work environments shows the existence of a positive correlation between the presence of female management and women's progression in the work place (Sato and Ando, 2017; Casarico and Lattanzio, 2019; Bhide, 2019; Langan, 2019). Here, we test whether salaries as a bargaining outcome differ across genders under male and female school principals and district superintendents, as a potential mechanism through which leadership influences women's success.

Principals Biasi (2018) shows that salaries of Wisconsin teachers became correlated with teacher quality after Act 10, which indicates that schools and districts value teachers' quality. As a consequence, a teacher's bargaining power could depend on how her quality is perceived by the school system. As in many other states, in Wisconsin school principals are mainly responsible for assessing teachers through a combination of objective and subjective measures (Biasi, 2018). If principals tend to give more favorable evaluations to teachers of their same gender, the gender gap in salaries should be larger for teachers in schools with male principals. We test this by re-estimating equation (2) for teachers with male and female principals in the years preceding the expiration of a CBA or of its extension.

Figure VIII, which shows estimates of equation (2) for teachers with male and female principals in the years preceding Act 10, confirms that the change in the gender pay gap is larger in schools with a male principal, and equal to 0.7 percent five years after the expiration (solid line, significant at 1 percent). In schools with a female principal, on the other hand, the change in the gap is more contained and indistinguishable from zero (dashed line). These results are summarized in column 1 of Table VII, and indicate that teachers in schools with a male superintendent prior to a CBA expiration have a 0.4 percentage points larger gap (significant at 10 percent). Because principals are largely responsible for evaluating teachers and less involved in salary negotiations, we take this as suggestive evidence that male principals evaluate women more negatively than men, even conditional on value-added. This could have been true before Act 10 as well; it however only affects salary afterward, once principal evaluations can feed into how salaries are set.

Superintendents District superintendents administer districts' finances and negotiate and set salaries. We look for heterogeneity in the pay gap by superintendent gender by re-estimating equation (2) separately for teachers in districts with male and female superintendents int he years before a CBA expiration.

Figure IX indicates that the gender gap is larger for teachers in districts with a male superintendent, and it becomes more than 0.8 percent larger five years after a CBA expiration compared with before (solid line). In districts with a female superintendent, on the other hand, the change in the gap is indistinguishable from zero (Figure IX, dashed line). These results are summarized in columns 4-6 of Table VII, and indicate that teachers in districts with male superintendents prior to a CBA expiration experience a 0.6 percentage point larger gap (significant at 5 percent).

This finding suggests that women are not just on average worse at bargaining than their male counterparts; rather, the gender of the person they are negotiating with matters, a finding that has not been tested in earlier literature. In particular, the fact that no salary gap exists when the superintendent is female suggests that women are either better able to negotiate with other women (or men are worse at negotiating with women), or that they experience backlash when they try to negotiate with men. We explore these possibilities next.

6 Avoiding Bargaining or Being Punished? A Survey

So far we have shown that a pay gap emerged between male and female Wisconsin teachers after the introduction of flexible pay. The salary gap is concentrated among young and less experienced teachers, as well as in districts with male superintendents. Analyses based on administrative data allow us to rule out several potential mechanisms for this finding, such as differences in ability, mobility, and demand. However, these data do not allow us to test whether women are less willing to bargain (possibly because they feel uncomfortable, fear backlash, or underestimate the returns to bargaining), or whether they bargain but are either less successful in doing so or are penalized for reasons unrelated to their negotiating ability.[21]

Distinguishing between these explanations, however, is crucial for policy. If women have worse negotiating skills than men, for example, providing them with training could close part or all of the observed pay gap (Ashraf et al., 2020). If instead they underestimate the returns to bargaining, a possible solution would be to provide them with this piece of information.

To make progress, we ran a survey of teachers' negotiating experiences and attitudes with current Wisconsin public school teachers. We asked teachers whether they had ever bargained their salary in both their current and past position (and if not, why) and about their intention to bargain over several aspects of their job in the future. We also asked about their knowledge of their colleagues' salaries and whether they know someone who has negotiated their pay. Finally, we used questions from social psychology to create a measure of negotiating skills. These questions allow us to further shed light on the mechanisms underlying the salary gap.

6.1 Survey Details and Sample Description

The survey questionnaire is in Appendix C. An invitation to fill in the survey (shown in Appendix Figure CI) was sent via email to 39,081 teachers employed in the 284 Wisconsin districts which make teachers' emails available on their websites.[22] A total of 3,156 teachers responded

[21] The literature has found that women are less likely to negotiate their salaries than men (Babcock and Laschever, 2003; Cardoso and Winter-Ebmer, 2010; Leibbrandt and List, 2014), but that they may be correct in doing so: an experiment by Exley et al. (2019) shows that women correctly select into bargaining, choosing to do so only when they know they will have a positive payoff.

[22] These districts include 215 with CBA expiration dates in 2011, 46 in 2012, and 22 in 2013, as well as 65 flexible-pay and 80 seniority-pay districts. We kept track of each respondent's school district by sending out different surveys to teachers in different districts.

to our survey, with a response rate of 13 percent.[23] The gender and age distributions of the respondents closely resemble those of the teacher population (Appendix Figures AVIII and AIX).

6.2 Gender Differences in Negotiation Experiences and Attitudes

Table IX presents men and women's mean responses to the the survey questions. The first striking finding is that women are less likely to have negotiated their pay with previous and current employers. For example, they are 8.3 percentage points less likely than men to have negotiated pay at the beginning of their current employment relationship (where 30.6 percent of male respondents report negotiating pay). Conditional on having negotiated, women are also 13 percentage points less likely to state that the negotiation was successful.[24] When asked why they did not negotiate, women are more likely than men to state that they were not comfortable doing so (10.5 percentage points, or 83 percent), that they thought it would be useless (2.2 percentage points or 35 percent), or that they were already satisfied with their pay (3.6 percentage points or 24 percent).

We also asked women about the likelihood that they will negotiate different aspects of their job in the future. While women appear slightly more likely than men to negotiate classroom assignment, they are 19 percent less likely to negotiate their pay, and only 5 percent less likely to negotiate non-teaching duties. In line with our earlier findings, these differences are almost entirely driven by women working under a male superintendent. When the superintendent is female, the likelihood of negotiating is instead indistinguishable between men and women.[25]

In terms of knowledge of others's salaries, women are 29 percent less likely to know their colleagues' salaries and 14 percent less likely to know someone who negotiated their pay.[26] Finally, we ask teachers a set of questions targeted at assessing socio-emotional skills, which we treat as proxies for bargaining skills.[27] We do not observe any differences in self-reported measures of socio-emotional skills (such as the ability to read subtle signals or assess how people feel); women are, however, 13 percent less likely to claim that they are confident talking to people they don't know. Lastly, women tend to value themselves less than their male colleagues:

[23]The survey was sent out on March 5, 2020; two reminder emails were sent in the following 14 days to the teachers who had not responded. The survey was closed on May 7th, 2020.

[24]This could be due to women having different beliefs about what a successful negotiation entails.

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

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

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

They are 12 percent less likely than men to report that their performance is above average.[28]

Controlling for Teachers' and Districts' Attributes A simple comparison of men's and women's answers suggests 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 teachers' 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 might be explained by teachers' bargaining ability or by their expected returns to negotiating.[29]

Table X presents our main results. Panel A confirms that, even controlling for district fixed effects and teacher attributes, women are 6.8 percentage points less likely to having negotiated pay with their previous employer (column 1) and 7.1 percentage points less likely to have negotiated at the start of their tenure with their current employer (column 2).

We also find that, 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, women are 8 percentage points less likely to report that salary negotiations with their current employer at the start of the relationship were successful (Table X, panel B, column 3, significant at 1 percent). Unfortunately we do not know what men and women consider to be a successful negotiation, so some of this result could be driven by differences in such beliefs.

In Panel C we test for gender differences in the reasons teachers give for not having negotiated at the beginning of their current employment relationship.[30] Based on the findings of other papers showing that women tend to be less confident in their abilities and less likely to self promote (Exley and Kessler, 2019), we ask a series of questions about one's comfort with negotiating, beliefs about backlash, and beliefs about the possibility and scope of bargaining. Controlling for district effects and teacher attributes, two answers stand out: Women are 6.5 percentage points (31 percent) more likely than men to say that they were not comfortable negotiating (column 2, significant at 1 percent), but 4 percentage points less likely to state that they are satisfied with their pay (or 21 percent, column 5, significant at 10 percent). Women are also slightly more likely than men to claim that they thought negotiating was useless (2.4 percentage

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

[29] Estimates obtained controlling only for district fixed effects, but not for teacher attributes, are shown in Table AIV.

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

points or 11 percent, column 3), although this difference is not statistically different from zero.

Lastly, in Panel D we explore the likelihood that women will negotiate in the future. Our estimates confirm that women are 12 percent less likely than men to negotiate their pay in the future (with an estimate for *Female* equal to -0.475, column 1, significant at 1 percent). Women are also slightly more likely to negotiate their teaching assignment (column 2) as likely as men to negotiate other non-teaching duties (column 3). Taken together, these results indicate that the reluctance of women to bargain is limited to negotiations over pay.

The Role of Superintendents' Gender Next, we investigate whether gender differences in the likelihood of negotiating are related to the gender of districts' leadership. Compared with men and women with a male superintendent, women who work under a female superintendent are 8.6 percentage points more likely to have negotiated with their current employer after the start of the employment relationship (Table XI, column 2) and 7.5 percentage points more likely to state that they will negotiate their pay in the future (column 3).

We do not find evidence that negotiating with a female superintendent is associated with a higher likelihood of women stating that their negotiations were successful (Panel B). We also do not find any association between the gender of the superintendent and the reasons teachers give for not negotiating (Panel C). It should be noted, however, that the coefficients for *Female * Female super* in these tables are estimated imprecisely, which prevents us from ruling out large positive or negative values for the point estimates.

Knowing Colleagues' Salaries We also investigate whether the observed gender differences in negotiating vary according to whether teachers know their colleagues' pay. Unfortunately, the results of this test (shown in Appendix Table AIII) are imprecise and do not allow us to rule out large differences of either sign.

6.3 Survey Results: Summing Up

The results from our survey indicate that women are less likely than men to have negotiated their pay in the past and to do so in the future. This behavior does not appear to be explained by a lower bargaining ability or by different perceived returns to negotiating. Even if women who negotiate are less likely than men to report that the negotiation was successful, the gender difference in the propensity to negotiate holds even when controlling for socio-emotional skills as a proxy for bargaining ability. Similarly, the gap in the likelihood to negotiate does not seem to be explained by whether teachers know other people's salaries or other people who have negotiated their pay.

We instead interpret our results as outlining an important role for the bargaining environment in determining whether teachers choose to negotiate or not. First, we find that women are significantly more likely than men to report that they chose not to negotiate because they felt uncomfortable doing so. Second, the gender gap in the likelihood of negotiating pay in the future is entirely driven by teachers working under a male superintendent; male and female teachers who work under a female superintendent are equally likely to negotiate.

While our inability to link our survey answers to administrative records prevents us from exactly estimating the portion of the post-Act 10 gender wage gap generated by the different propensity to negotiate across genders, the results from the survey suggest that women's reluctance to bargain, particularly with male superintendents, is likely an important driver of these salary differences. Creating an environment in which all teachers feel comfortable discussing their pay could potentially close a significant part of the gender wage gap.

7 Conclusion

There has been much debate about the role that bargaining plays in the gender wage gap. This paper uses data from a large public-sector employer, the Wisconsin public school system, to shed light on this debate. Wisconsin's Act 10 replaced the traditional bargaining system in which teacher unions bargain with the school district and instead allowed for 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 "forced" to bargain over their salaries.

In line with previous experimental work, we find that women lose relative to men when they are required to bargain. After Act 10, school districts that adopted flexible pay schemes see a gap of 0.8 percent emerge between men and women's salaries. The gap is not explained by differences in teacher experience or ability, nor can it be explained by differences in mobility. The gap is largest among new, inexperienced teachers.

Aside from documenting this fact, we also shed light on *why* women fare worse than men when forced to bargain. We find that the gender pay gap is largest when women are working in school with male principals and in districts governed by male superintendents. These results suggest that differences in evaluations as well as the gender of the person one bargains with are important, and point to a finding of the literature – that women do better under female management – as a potential mechanism underlying our results.

Responses to a survey administered to all Wisconsin teachers corroborate this evidence: women are less likely to have negotiated their salary or to expect to do so in the future, especially if they work in a district with a male superintendent. The survey further suggests that women are choosing not to negotiate because they feel uncomfortable doing so, not because they do not believe it is possible or that the are satisfied with their pay.

Taken together, our results support the hypothesis that forcing women to bargain could perpetuate the wage gap. Policies that train women to negotiate or that have women negotiate with other women could prove successful and represent important topics for further research.

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Tables

Table I: Male and female teachers: Mean observable characteristics

	2007-2011			2012-2016		
	Males	Females	Diff.	Males	Females	Diff.
experience (years)	14.9	14.3	0.6***	14.2	13.9	0.3***
			(0.04)			(0.04)
age	43.0	43.3	-0.3***	42.4	42.5	-0.06
			(0.04)			(0.05)
highest ed = BA	0.5	0.5	-0.003	0.5	0.5	0.003
			(0.002)			(0.002)
highest ed = Master	0.5	0.5	0.0006	0.5	0.5	-0.005**
			(0.002)			(0.002)
highest ed = PhD	0.003	0.001	0.002***	0.004	0.001	0.002***
			(0.0002)			(0.0002)
salary ($)	51213.2	51019.6	193.5***	53971.9	53611.5	360.5***
			(48.2)			(59.1)
value-added	-0.002	-0.00005	-0.002***	-0.001	-0.00003	-0.001*
			(0.0007)			(0.0005)
ever moves	0.1	0.09	0.01***	0.1	0.1	0.01***
			(0.001)			(0.002)
leaves sample	0.07	0.06	0.003***	0.08	0.08	-0.0005
			(0.001)			(0.001)
elementary T	0.2	0.5	-0.3***	0.2	0.5	-0.3***
			(0.002)			(0.002)
middle school T	0.2	0.2	0.05***	0.2	0.2	0.05***
			(0.002)			(0.002)
high school T	0.6	0.2	0.3***	0.5	0.2	0.3***
			(0.002)			(0.002)
math T	0.1	0.06	0.06***	0.1	0.06	0.06***
			(0.001)			(0.001)

Note: This table shows the mean characteristics of males 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). ** ≤ 0.1, ** ≤ 0.05, *** ≤ 0.01.

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

	Expiration post 2011 vs in 2011			W/ extension vs w/out		
	(1)	(2)	(3)	(4)	(5)	(6)
District chars.	Difference	SE	P-value	Difference	SE	P-value
Enrollment	13116.29	8153.50	0.11	2618.90	791.06	0.00
N teachers	957.31	569.04	0.09	177.26	54.38	0.00
Per pupil expenditure	0.08	0.88	0.92	-1.42	0.38	0.00
Share black students	0.14	0.06	0.03	0.01	0.01	0.07
Share disadvantaged students	0.06	0.06	0.36	-0.03	0.02	0.05
In urban area	0.23	0.17	0.19	0.09	0.05	0.05
In suburban area	0.34	0.17	0.05	0.07	0.03	0.02

Note: The table shows the estimates ("Differences"), robust standard errors, and p-values from OLS regressions in which we separately regress each district characteristic listed in the first column on a dummy variable indicating that a CBA expiration occurred after 2011 (columns 1-3) and a dummy variable indicating that a district received an extension (columns 4-6). Each observation is a school district.

Table III: Gender salary gap after CBA expiration and extension (OLS and 2SLS)

Dep Var: Log Salary	Expirations		Extensions		2SLS, Extensions	
	(1)	(2)	(3)	(4)	(5)	(6)
Female	-0.0024**	-0.0024**	-0.0024**	-0.0024**	-0.0024**	-0.0024**
	(0.0012)	(0.0012)	(0.0011)	(0.0011)	(0.0011)	(0.0011)
Female × Post Extension	-0.0033***				-0.0031***	
	(0.0009)				(0.0010)	
Female × 1 Year(s) Post		-0.0006		0.0005		0.0006
		(0.0011)		(0.0010)		(0.0019)
Female × 2 Year(s) Post		-0.0030*		-0.0018		-0.0038*
		(0.0015)		(0.0013)		(0.0020)
Female × 3 Year(s) Post		-0.0012		-0.0016		-0.0012
		(0.0012)		(0.0014)		(0.0019)
Female × 4 Year(s) Post		-0.0062***		-0.0036**		-0.0050
		(0.0017)		(0.0015)		(0.0031)
Female × 5 Year(s) Post		-0.0091***		-0.0078***		-0.0105***
		(0.0020)		(0.0017)		(0.0033)
Female × Post Expiration			-0.0027***			
			(0.0009)			
Distr × Post exp	Yes	Yes	Yes	Yes	Yes	Yes
Educ, Exper, Teaching Assign × Post exp	Yes	Yes	Yes	Yes	Yes	Yes
Yr × Exp yr	Yes	Yes	Yes	Yes	Yes	Yes
N	579596	579596	579596	579596	579596	579596
# districts	428	428	428	428	428	428

Note: The dependent variable is the natural logarithm of salary per year, in full-time equivalency units. 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 1, 2, 5, and 6) or after an expiration (columns 3 and 4). Columns 1-4 estimate OLS; columns 5 and 6 estimates 2SLS, with *Post expiration* as an instrument for *Post extension*. All specifications 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 extension year effects. Standard errors in parentheses are clustered at the district level. $^{**} \leq 0.1$, $^{**} \leq 0.05$, $^{***} \leq 0.01$.

Table IV: Gender salary gap after CBA extension by district type

	Baseline			W/ gender-specific schedule, 3-4 yrs seniority, master's		
	(1)	(2)	(3)	(4)	(5)	(6)
	FP	SP	Difference	FP	SP	Difference
=1 if Female	-0.0031**	-0.0015	-0.0019	0.0064	0.0006	0.0005
	(0.0015)	(0.0018)	(0.0019)	(0.0047)	(0.0033)	(0.0035)
Female × Post Extension	-0.0028*	-0.0048***	-0.0046***	-0.0088*	0.0018	0.0044
	(0.0014)	(0.0013)	(0.0013)	(0.0045)	(0.0058)	(0.0063)
Female × FP			-0.0014			-0.0011
			(0.0024)			(0.0023)
Female × FP × Post Extension			0.0020			-0.0084
			(0.0019)			(0.0104)
Distr × Post exp	Yes	Yes	Yes	Yes	Yes	Yes
Educ, Exper, Teaching Assign × Post exp	Yes	Yes	Yes	Yes	Yes	Yes
Exper * Female * Post Ext	No	No	No	Yes	Yes	Yes
N	203157	259956	463255	203157	259816	462973
# districts	102	122	224	102	122	224

Note: The dependent variable is the natural logarithm of salary per year, in full-time equivalency units. The variable *female* equals one for female workers, the variable *Post Extension* equals one for years following the expiration of a CBA or its extension. Columns 1 and 3 are estimated on teachers in flexible-pay districts, and columns 2 and 4 are estimated on teachers working in seniority-pay districts. All specifications include fixed effects for the district, number of years of seniority, 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. Columsn 4-6 also include years of experience fixed effects, interacted with *Female* and for *Post Extension*. All columns present OLS estimates. All specifications also include year fixed effects interacted with extension year effects. Standard errors in parentheses are clustered at the district level. $^{**} \leq 0.1$, $^{**} \leq 0.05$, $^{***} \leq 0.01$.

Table V: Gender salary gap after a CBA expiration: Robustness checks. OLS, dependent variable is log(salaries)

	Balanced	Teacher FE	ITT	District sched.
	(1)	(2)	(3)	(4)
Female	-0.0007	0.0016	-0.0011	-0.0007
	(0.0010)	(0.0050)	(0.0010)	(0.0010)
Female × Post Extension	-0.0043***	-0.0047***	-0.0060***	-0.0067***
	(0.0012)	(0.0012)	(0.0012)	(0.0012)
Distr × Post exp	Yes	Yes	Yes	Yes
Educ, Exper, Teaching Assign × Post exp	Yes	Yes	Yes	Yes
Yr × Exp yr	Yes	Yes	Yes	Yes
N	327687	569111	490644	576135
# districts	428	428	428	428

Note: The dependent variable is the natural logarithm of salary per year, in full-time equivalency units. The variable *Female* equals one for female workers, the variable *Post Extension* equals one for years following the expiration of a CBA or its extension. All specifications include fixed effects for the district, number of years of seniority, 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. Column 1 is estimated on a balanced sample of teachers in the 3 years before and after each expiration; column 2 includes teacher fixed effects; column 3 assigns teachers to the districts where they were teaching in 2011; and column 4 controls for indicators for years of experience and highest education degree, interacted with district fixed effects and for an indicator for years after the extension of a CBA. All specifications also include year fixed effects interacted with extension year effects. Standard errors in parentheses are clustered at the district level. ** ≤ 0.1, ** ≤ 0.05, *** ≤ 0.01.

Table VI: Gender salary gap and teacher value-added

Dep Var: Log Salary	All districts			FP			SP		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Female	0.0022	0.0022	0.0022	0.0052*	0.0052*	0.0052*	0.0013	0.0013	0.0013
	(0.0016)	(0.0016)	(0.0016)	(0.0030)	(0.0030)	(0.0030)	(0.0021)	(0.0021)	(0.0021)
Female × Post Extension	-0.0036*	-0.0036*	-0.0036*	-0.0103***	-0.0102***	-0.0102***	-0.0007	-0.0007	-0.0009
	(0.0021)	(0.0021)	(0.0021)	(0.0032)	(0.0032)	(0.0032)	(0.0026)	(0.0026)	(0.0026)
VA		0.0148	0.0148		0.0110	0.0110		0.0080	0.0080
		(0.0093)	(0.0093)		(0.0172)	(0.0172)		(0.0123)	(0.0123)
VA × Post Extension		0.0025	0.0677*		0.0344	0.1093		-0.0107	0.0521
		(0.0175)	(0.0368)		(0.0312)	(0.0689)		(0.0225)	(0.0497)
Female × VA × Post Extension			-0.0780**			-0.0879			-0.0751
			(0.0367)			(0.0717)			(0.0493)
Distr, Educ, Exper, Teaching Assign × Post exp	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Yr × Exp yr	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	134620	134620	134620	47288	47288	47288	60007	60007	60007
# districts	425	425	425	102	102	102	121	121	121

Note: The dependent variable is the natural logarithm of salary per year, in full-time equivalency units. The variable *Female* equals one for female workers, the variable *Post-extension* equals one for years following the time limit for an extension to a CBA (for districts without an extension, this variable equals one for years after expiration), and the variable *VA* is equal to teachers' value-added. Columns 1 and 4 are estimated on teachers in all districts, columns 2 and 5 are estimated on teachers in flexible-pay districts, and columns 3 and 6 are estimated on teachers in seniority-pay districts. All columns present OLS estimates. All specifications include fixed effects for the district, number of years of seniority, and highest education degree, alone and interacted with an indicator for years after the extension of a CBA. Specifications also include year fixed effects. Standard errors in parentheses are clustered at the district level. $^{**} \leq 0.1$, $^{**} \leq 0.05$, $^{***} \leq 0.01$.

Table VII: Gender salary gap after CBA extension, By principal and superintendent gender and by share of men in the district or school

	Principal	Super.	Share Men	
	(1)	(2)	(3)	(4)
			In school	In district
Female	-0.0026	-0.0036	-0.0015	-0.0026*
	(0.0019)	(0.0026)	(0.0015)	(0.0014)
Female × Post	-0.0002	0.0028	0.0010	-0.0017
	(0.0015)	(0.0028)	(0.0015)	(0.0012)
Female × Male princ	-0.0002			
	(0.0017)			
Female × Male princ × Post	-0.0035**			
	(0.0015)			
Female × Male super		0.0011		
		(0.0029)		
Female × Male super × Post		-0.0063**		
		(0.0031)		
Female × High share men			-0.0007	-0.0006
			(0.0016)	(0.0024)
Female × High share men × Post			-0.0081***	-0.0043*
			(0.0019)	(0.0025)
Distr, Educ, Exper, Teaching Assign × Post exp	Yes	Yes	Yes	Yes
Yr × Exp yr	Yes	Yes	Yes	Yes
N	538434	540533	579493	579496
# districts	428	428	428	428

Note: The dependent variable is the natural logarithm of salary per year, in full-time equivalency units. The variable *female* equals one for female workers, the variable *Post Expiration* 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 at least one male principal and districts with at least one male superintendent in the years prior to the CBA expiration or extension, respectively. The variable *High share men* equals one for teachers with an average share of male colleagues in their school/district and year higher than 30 percent in the years prior to the CBA expiration or extension. All specifications include fixed effects for the district, number of years of seniority, 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. Specifications also include year fixed effects interacted with 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 VIII: Gender Differences in Mobility

	Moves District	Moves within CZ	Moves to SP District		Moves to FP District	
	(1)	(2)	(3)	(4)	(5)	(6)
			High VA	Low VA	High VA	Low VA
Post Extension	0.015***	0.013***	0.006***	0.007***	0.009***	0.008***
	(0.002)	(0.001)	(0.002)	(0.002)	(0.003)	(0.003)
Female × Post	-0.002	-0.000	0.002	0.000	-0.002	-0.001
	(0.001)	(0.001)	(0.002)	(0.002)	(0.003)	(0.003)
Female	-0.000	0.001***	0.001	0.001	-0.001	-0.001
	(0.001)	(0.000)	(0.001)	(0.001)	(0.001)	(0.001)
Experience FE	Y	Y	Y	Y	Y	Y
District FE	Y	Y	Y	Y	Y	Y
Mean of Dep. Var., Women	1.49	0.72	0.63	0.63	0.77	0.77
Mean of Dep. Var., Men	1.58	0.61	0.64	0.64	0.66	0.66
Observations	349,735	354,070	43,362	45,286	43,362	45,286
R-squared	0.119	0.069	0.032	0.032	0.034	0.041

Note: This table looks at differences in mobility rates between men and women before and after the expiration of a CBA. 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 1-2 are estimated on all teachers, columns 3-4 are estimated on teachers working in districts with seniority pay, and columns 5-6 are estimated on teachers working in flexible pay districts. Columns 3 and 5 also only include teachers with VA in the top quartile while column 4 only includes teachers with VA in the bottom three quartiles. All specifications 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. Specifications also include year fixed effects interacted with extension year effects. Standard errors in parentheses are clustered at the district level. $^{**} \leq 0.1$, $^{**} \leq 0.05$, $^{***} \leq 0.01$.

Table IX: Survey Answers: Means, Women vs Men, and Differences in Means

	Women	Men	Difference	Std. Error
Have you ever negotiatied...				
w/prev employer	0.295	0.379	-0.084***	(0.019)
w/current employer, at start	0.223	0.306	-0.083***	(0.018)
w/current employer, after start	0.205	0.245	-0.040**	(0.017)
If yes, negotiation was 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)
I didn't know it was possible	0.000	0.000	0.000	(0.000)
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: This table presents the average shares of female and male teachers answering "yes" to a given survey question, as well as the differences in means and standard deviations (in parentheses). ** ≤ 0.1, ** ≤ 0.05, *** ≤ 0.01.

Table X: Survey Answers: Likelihood of Negotiating, OLS Estimates

Panel A) Ever negotiated with:

	Previous employer	Current empl., at start	Current empl, after start
Female	-0.068***	-0.071***	-0.028
	(0.020)	(0.022)	(0.018)
Controls	Yes	Yes	Yes
N	2836	2836	2836
Y mean, males	0.379	0.306	0.245

Panel B) Negotiated successfully conditional on negotiating, with:

	Previous employer	Current empl., at start	Current empl., after start
Female	-0.080***	-0.132**	-0.107*
	(0.029)	(0.052)	(0.062)
Controls	Yes	Yes	Yes
N	902	700	614
Y mean, males	0.904	0.814	0.572

Panel C) Reasons for not negotiating (current employer, at start)

	Not possible	Not comfortable	Useless	Fear backlash	Satisfied w/pay
Female	-0.023	0.065**	0.024	0.005	-0.040*
	(0.028)	(0.029)	(0.025)	(0.019)	(0.022)
Controls	Yes	Yes	Yes	Yes	Yes
N	2222	2222	2222	2222	2222
Y mean, males	0.565	0.210	0.215	0.131	0.189

Panel D) Likelihood of negotiating in the future, over:

	Salary	Classroom assignment	Non-teaching duties
Female	-0.475***	0.273*	-0.135
	(0.162)	(0.139)	(0.133)
Controls	Yes	Yes	Yes
N	2836	2836	2836
Y mean, males	3.889	4.539	4.579

Note: Panel A shows whether a teacher negotiated her salary at the start of her contract with her current employer (column 1) or after the start of her contract with her current employer (column 2). Columns 3-5 ask whether a teacher plans to negotiate her salary, classroom assignments, or non-teaching duties in the future. Panel B shows whether female teachers are more likely to state that they were unsuccessful conditional on negotiating relative to men. Panel C presents the reasons respondents gave for not negotiating. *Female* is an indicator for female teachers. All regressions include controls for age class, self-reported job performance (above/below average), a measure of people skills, an indicator for whether the respondent knows someone who negotiated his/her 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 XI: Survey Answers, By Superintendent's Gender. OLS Estimates

Panel A) Ever negotiated with:

	Current empl.		In the future		
	At start	After start	Salary	Class assgn	Non-teach. duties
Female	-0.083***	-0.057***	-0.718***	0.272*	-0.186
	(0.025)	(0.020)	(0.151)	(0.161)	(0.151)
Female * F super	0.038	0.086**	0.746**	0.009	0.156
	(0.046)	(0.035)	(0.354)	(0.334)	(0.278)
Controls	Yes	Yes	Yes	Yes	Yes
N	2784	2784	2784	2784	2784
Y mean, males	0.306	0.245	3.889	4.539	4.579

Panel B) Negotiated successfully conditional on negotiating, with:

	Current employer, at start	Current employer, after start
Female	-0.087	-0.082
	(0.057)	(0.069)
Female * F super	-0.134	-0.119
	(0.119)	(0.139)
Controls	Yes	Yes
N	682	601
Y mean, males	0.814	0.572

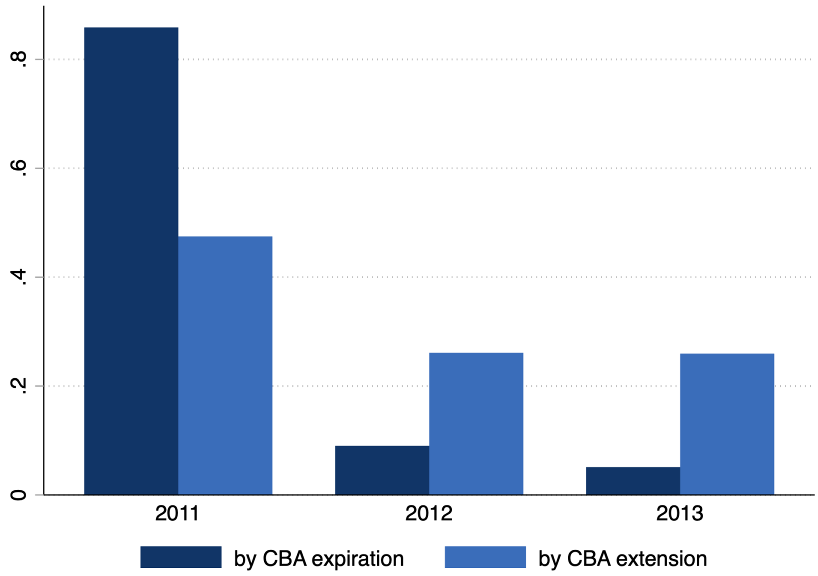
Panel C) Reasons for not negotiating:

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

Note: Panel A shows whether a teacher negotiated her salary at the start of her contract with her current employer (column 1) or after the start of her contract with her current employer (column 2). Columns 3-5 ask whether a teacher plans to negotiate her salary, classroom assignments, or non-teaching duties in the future. Panel B shows whether female teachers are more likely to state that they were unsuccessful conditional on negotiating relative to men. Panel C presents the reasons respondents gave for not negotiating. *Female* is an indicator for female teachers. *F super* is an indicator that takes the value one if a teacher currently works in district with a female superintendent. All regressions include controls for age class, self-reported job performance (above/below average), a measure of people skills, an indicator for whether the respondent knows someone who negotiated his/her 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. ** ≤ 0.1, ** ≤ 0.05, *** ≤ 0.01.

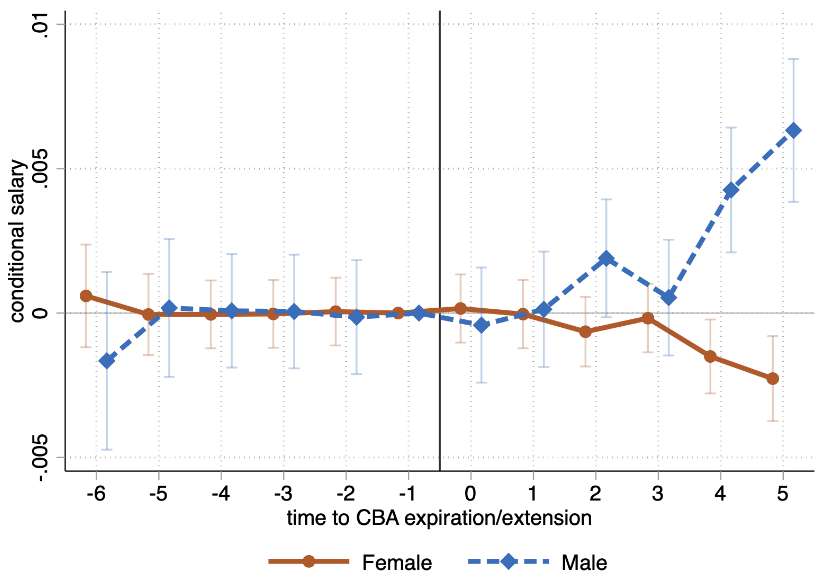
Figures

Figure I: Share of Teacher-Year Observations, by Expiration and Extension Dates of CBAs



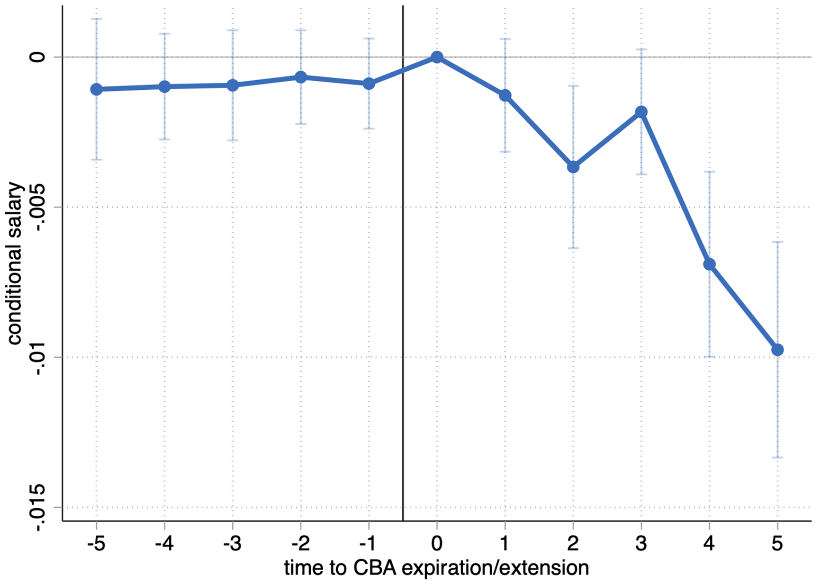
Note: The darker bars show the share of teachers covered by a CBA that expires in 2011, 2012, and 2013. The lighter bars show the share of teachers covered by a CBA that had its expiration date extended to 2011, 2012, and 2013.

Figure II: Salaries of Men and Women, by Time to Expiration/Extension of CBA



Note: The figure shows OLS point estimates and 90% confidence intervals of the coefficients δ_s in equation (2), for g =female (solid line) and g =male (dashed line). Standard errors are clustered at the district level.

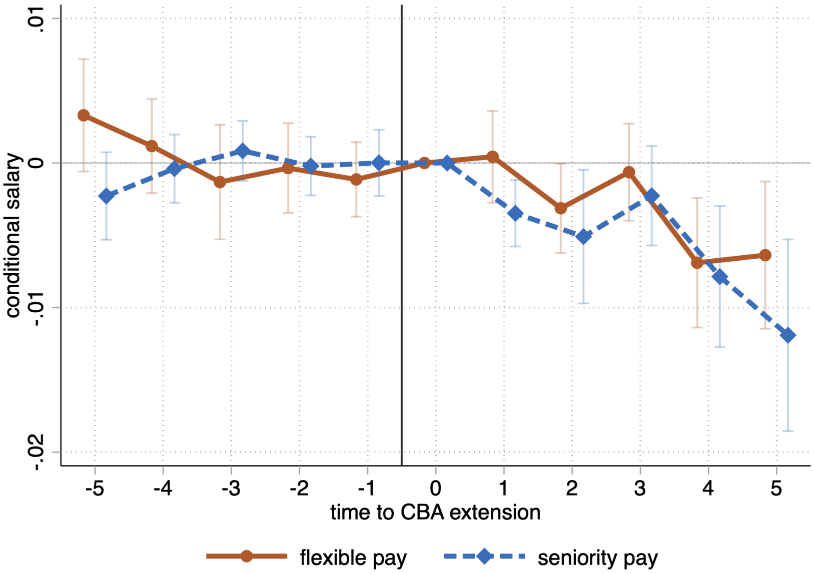
Figure III: Gender Gap in Salaries, by Time to Expiration/Extension of CBA



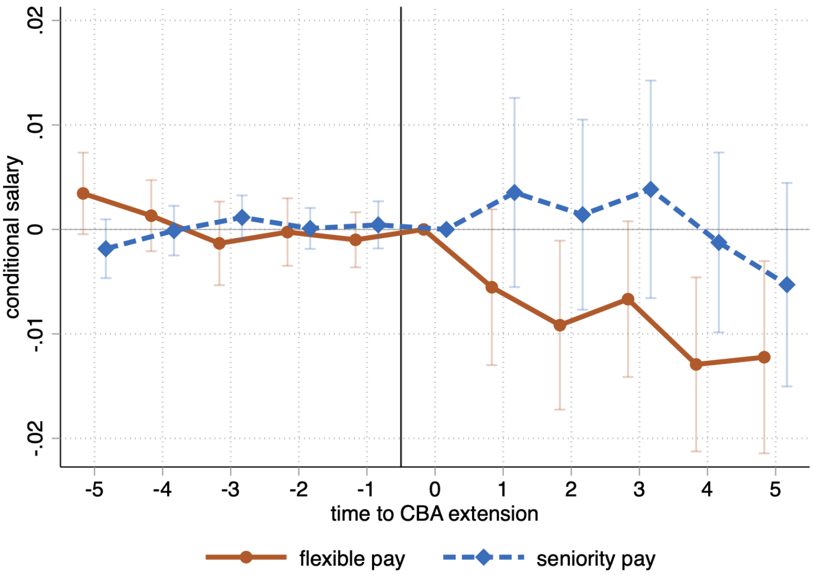
Note: The figure shows OLS point estimates and 90% confidence intervals of the coefficients δ_s in the equation $\ln(w_{ijt}) = \sum_{s=-4}^{3} \delta_s Female_i \mathbb{1}(t - Y_j = s) + \beta X_{it} + \varepsilon_{ijt}$, where $\ln(w_{ijt})$ is the natural logarithm of salaries for teacher i, working in district j in year t; $Female_i$ equals 1 for women; Y_j is either the year of expiration of district j's CBA or the year in which the extension to the CBA ended; the vector X_{it} contains district, seniority, and education fixed effects (alone and interacted for an indicator for years after a CBA expiration), and year fixed effects interacted with extension year fixed effects. Standard errors are clustered at the district level.

Figure IV: Gender Gap in Salaries, by Time to Expiration/Extension of CBA and District type

Panel a) Baseline



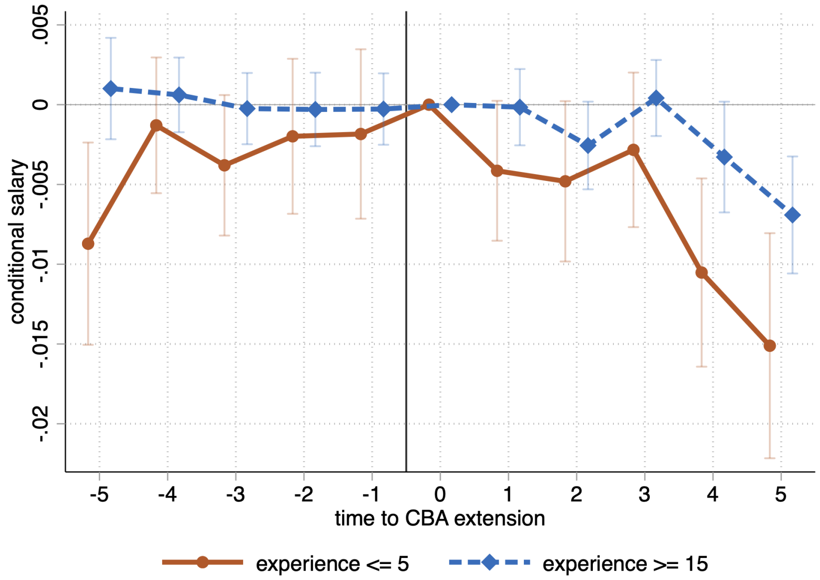
Panel b) With gender-specific experience returns, for teachers with 3-4 years of experience and a master's degree



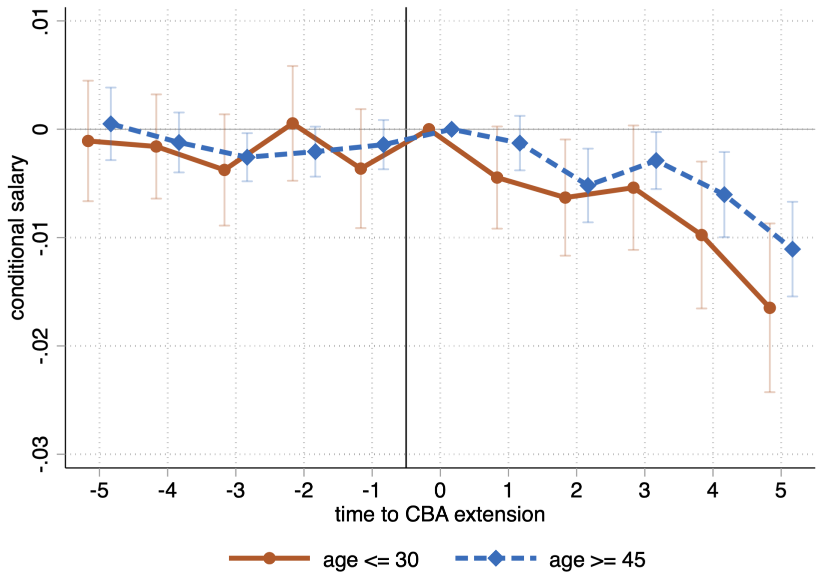
Note: The figure shows OLS point estimates and 90% confidence intervals of the coefficients δ_s in the equation $\ln(w_{ijt}) = \sum_{s=-4}^{3} \delta_s Female_i \mathbb{1}(t - Y_j = s) + \beta X_{it} + \varepsilon_{ijt}$, where $\ln(w_{ijt})$ is the natural logarithm of salaries for teacher i, working in district j in year t; $Female_i$ equals 1 for women; Y_j is either the year of expiration of district j's CBA or the year in which the extension to the CBA ended; the vector X_{it} contains district, seniority, and education fixed effects (alone and interacted for an indicator for years after a CBA expiration), and year fixed effects interacted with extension year fixed effects. The coefficients are estimated and shown separately for flexible-pay (FP) and seniority-pay (SP) districts. In the bottom panel, we further control for seniority and education fixed effects interacted with $Female_i$ and with an indicator for years following Y_j; the plotted coefficients refer to teachers with 3 or 4 years of experience and a master's degree. Standard errors are clustered at the district level.

Figure V: Gender Gap in Salaries, by Seniority and Age

Panel A: Pay Gap by Seniority

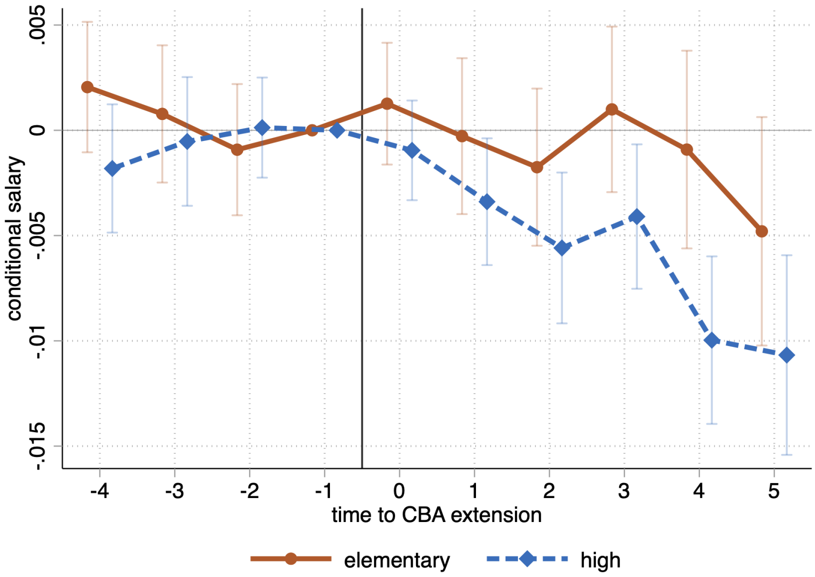


Panel B: Pay Gap by Age



Note: Panel A shows OLS point estimates and 90% confidence intervals of the coefficients δ_s in equation (3), estimated separately for teachers with less than five (solid line) and more than 15 years of experience (dashed line). Panel B shows OLS point estimates and 90% confidence intervals of the coefficients δ_s in equation (3), estimated separately for teachers who are younger than 30 (solid line) and older than 45 (dashed line). Standard errors are clustered at the district level.

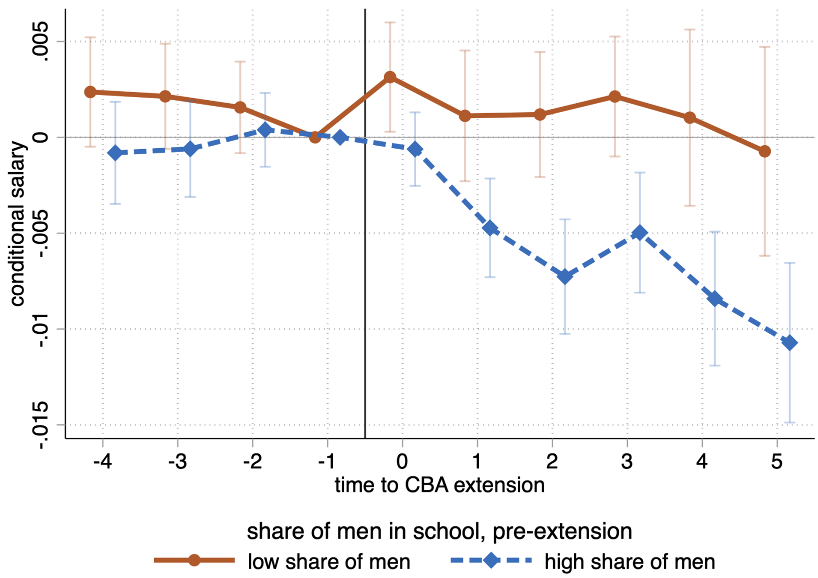
Figure VI: Gender Gap in Salaries, for Elementary vs High School Teachers



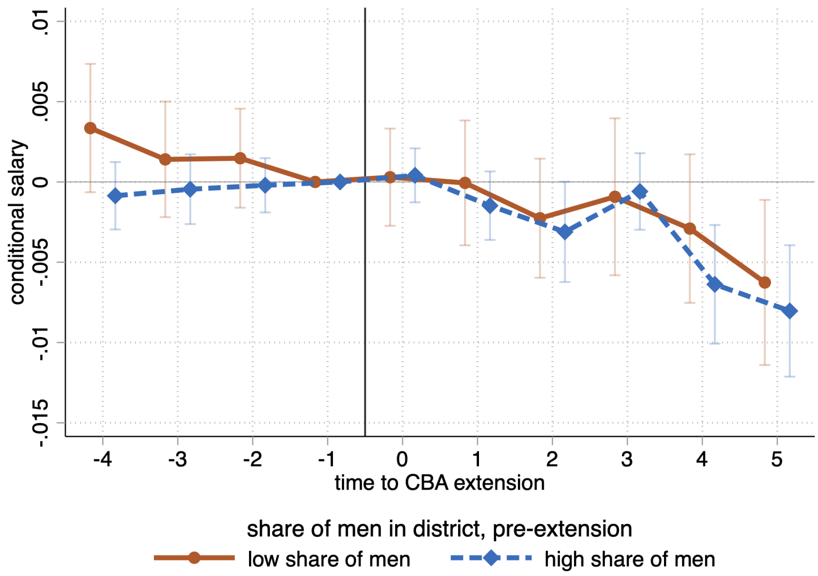
Note: OLS point estimates and 90% confidence intervals of the coefficients δ_s in equation (3), estimated separately for teachers in elementary school (solid line) and in high school (dashed line). Standard errors are clustered at the district level.

Figure VII: School Environment and the Gender Gap in Salaries: Gender of Colleagues

Panel A: By Share of Male Colleagues in School

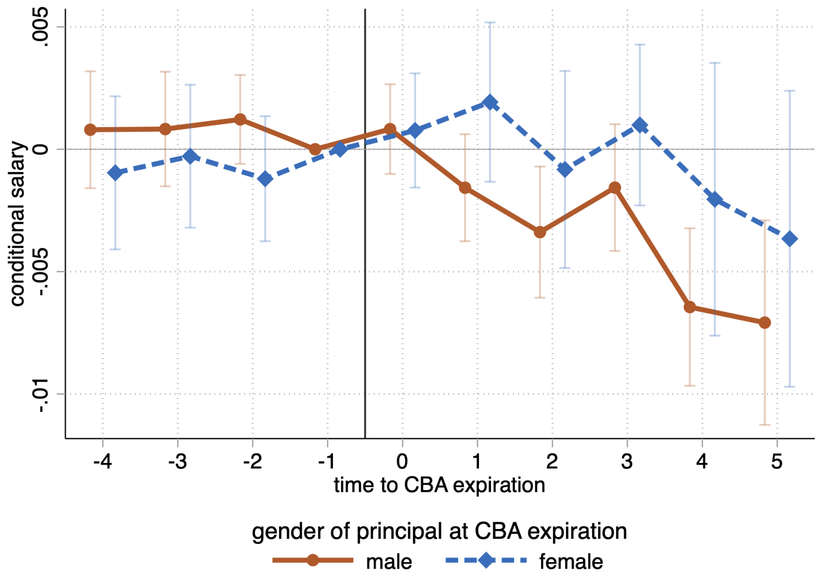


Panel B: By Share of Male Colleagues in District



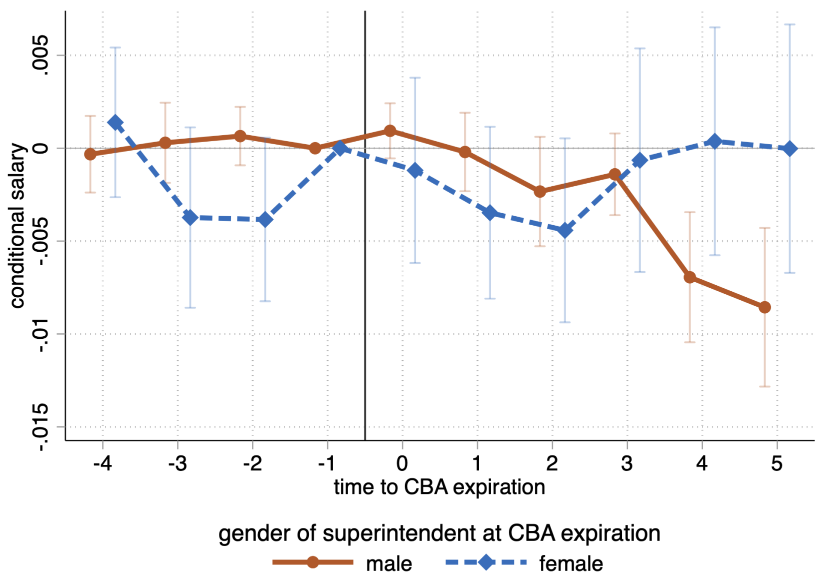
Note: Panel A shows OLS point estimates and 90% confidence intervals of the coefficients δ_s in equation (3), estimated separately for teachers in schools in the top quartile of the share of men (i.e., with more than 30 percent of men, solid line), and teachers in all other schools (dashed line). Panel B shows OLS point estimates and 90% confidence intervals of the coefficients δ_s in the equation (3), estimated separately for teachers in districts in the top quartile of the share of men (i.e., with more than 30 percent of men, solid line), and teachers in all other districts (dashed line). Standard errors are clustered at the district level.

Figure VIII: School Environment and the Gender Gap in Salaries: Gender of School Principals



Note: The figure shows OLS point estimates and 90% confidence intervals of the coefficients δ_s in equation (3), estimated and shown separately for teachers in schools with at least one female principal and teachers in schools with all male principals in the years before a CBA expiration. Standard errors are clustered at the district level.

Figure IX: School Environment and the Gender Gap in Salaries: Gender of District Superintendents



Note: The figure shows OLS point estimates and 90% confidence intervals of the coefficients δ_s in equation (3), estimated and shown separately for teachers in districts with at least one female superintendent and teachers in districts with all male superintendents in the years before a CBA expiration. Standard errors are clustered at the district level.

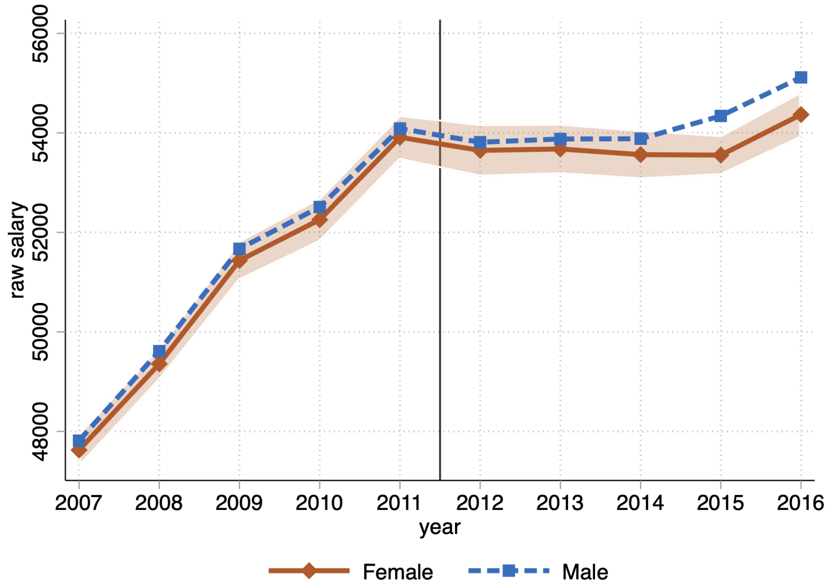
Appendix

For online publication only

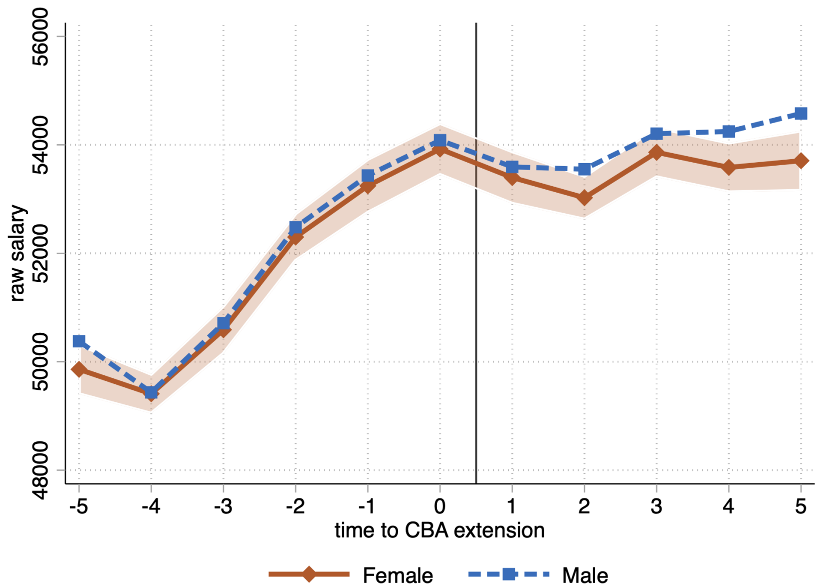
Appendix A Additional Tables and Figures

Figure AI: Raw Salaries of Men and Women

Panel A) Raw Salaries by Year

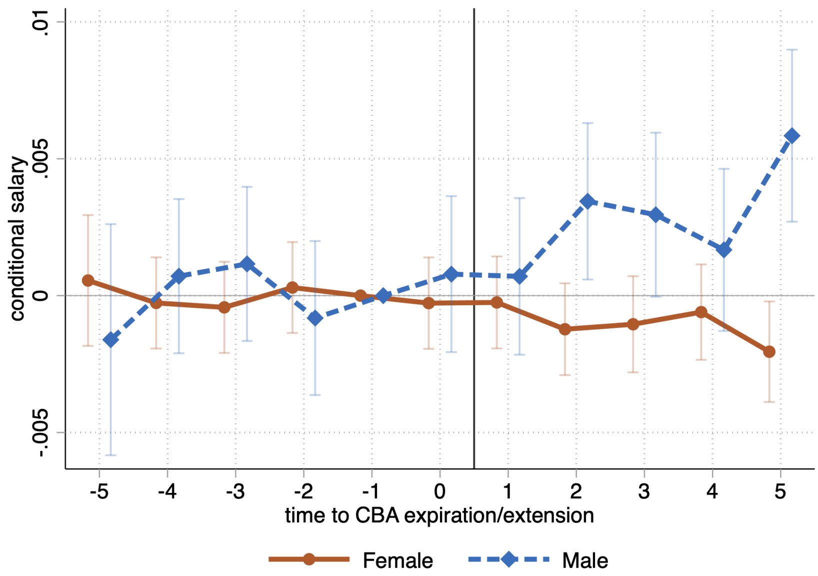


Panel B) Raw Salaries around Extension Date



Note: The figure shows the unconditional salaries of male and female teachers by calendar year (Panel A) and by the years surrounding a district's CBA extension year (Panel B).

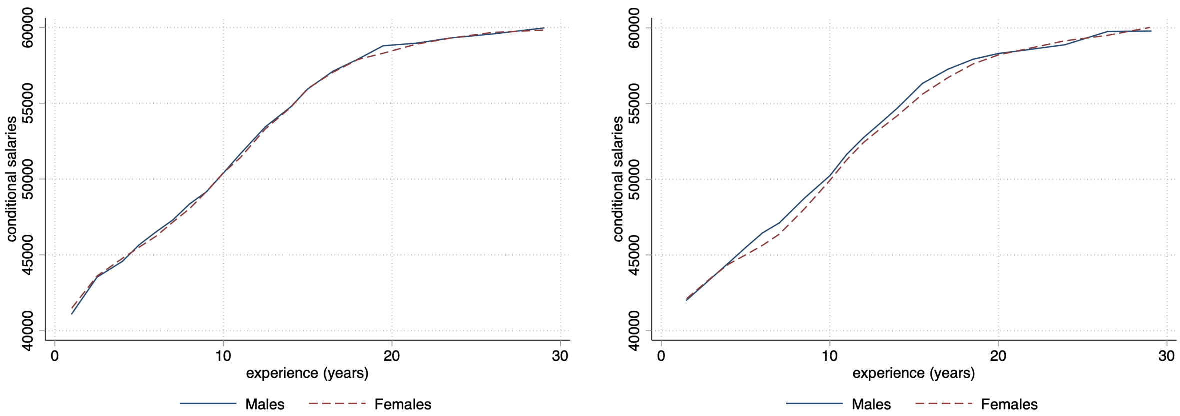
Figure AII: Salaries of Men and Women, by Time to Expiration of CBA



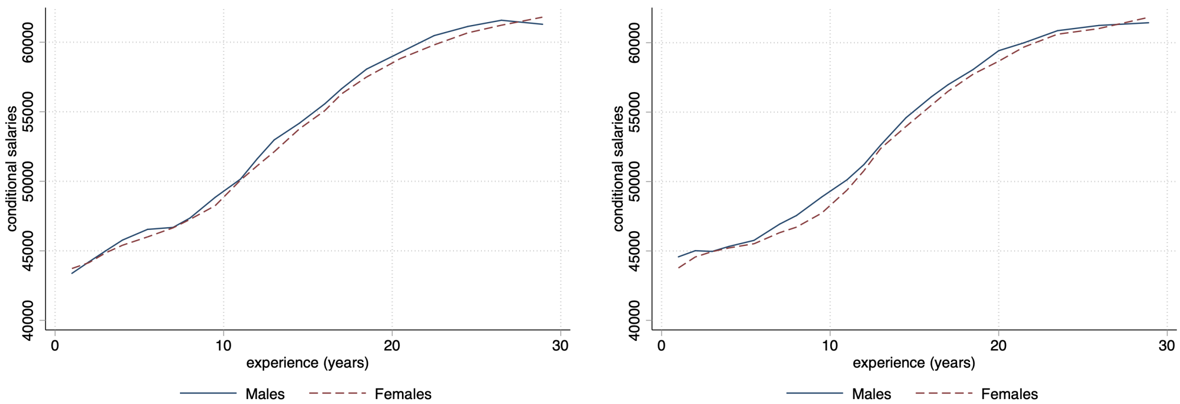
Note: The figure shows OLS point estimates and 90% confidence intervals of the coefficients δ_s in equation (2) in the paper, for g =female (solid line) and g =male (dashed line), and using CBA expiration dates (rather than extensions). Standard errors are clustered at the district level.

Figure AIII: Conditional Salaries of Men and Women, by Experience

Panel A) Years before a CBA expiration. Seniority pay (left) and flexible pay (right)



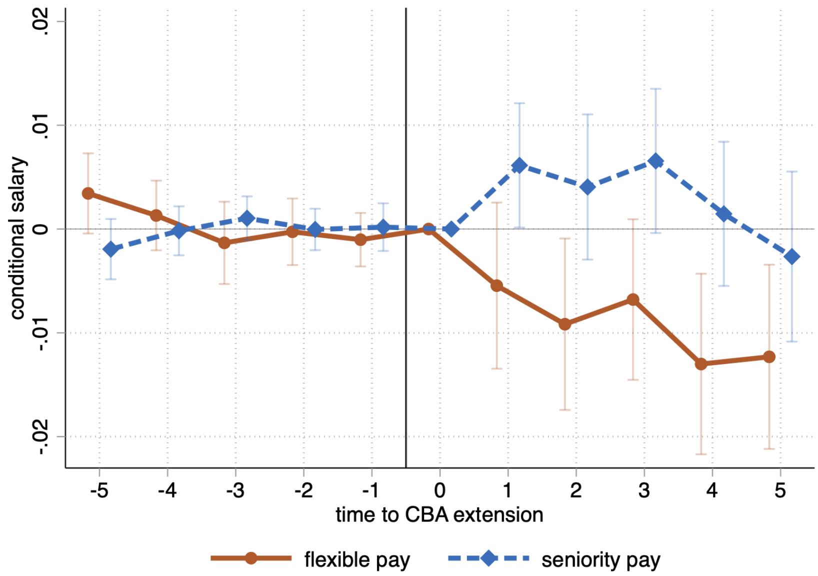
Panel B) Years after a CBA expiration. Seniority pay (left) and flexible pay (right)



Note: The figure shows conditional salaries per years of experience, separately for males and females; the top panel uses data prior to (and including) 2011, the bottom panel uses data after CBA extensions. Conditional salaries are obtained as residuals of a regression of salaries on education, district, and teaching assignment fixed effects, alone and interacted with an indicator for years following extensions, as well as year effects interacted by extension years.

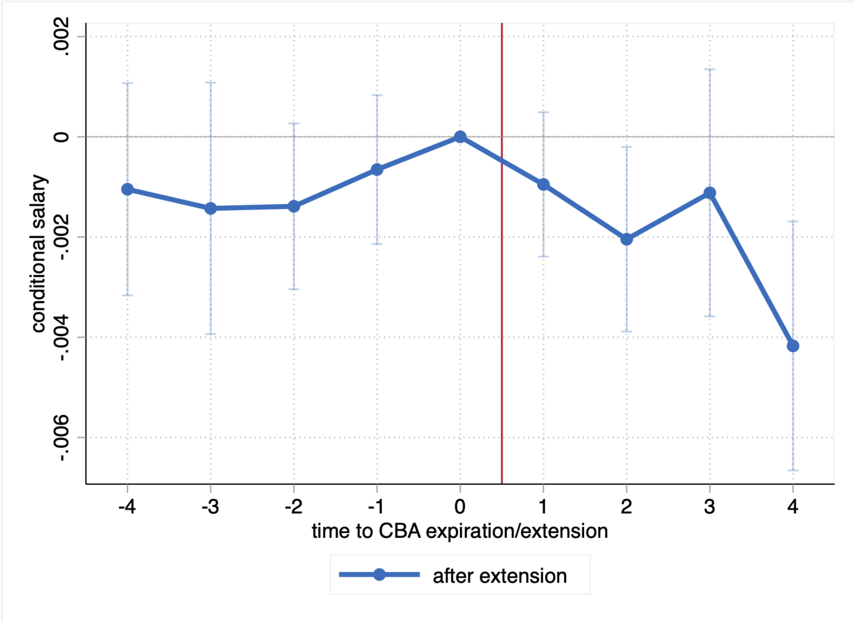
Figure AIV: Gender Gap in Salaries, by Time to Expiration/Extension of CBA and District type. Teachers with 19-20 years of experience and a master's degree

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



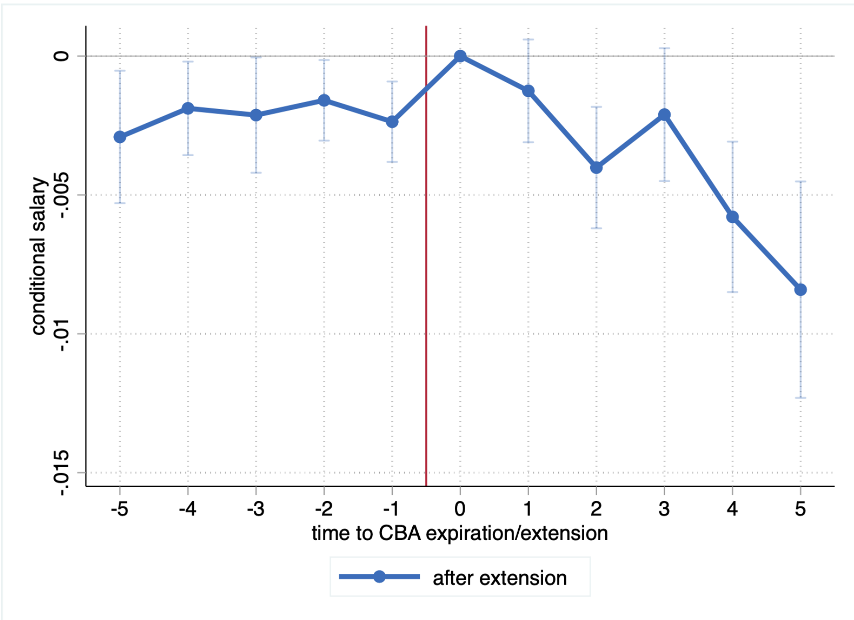
Note: The figure shows OLS point estimates and 90% confidence intervals of the coefficients δ_s in the equation $\ln(w_{ijt}) = \sum_{s=-4}^{3} \delta_s Female_i \mathbb{1}(t - Y_j = s) + \beta X_{it} + \varepsilon_{ijt}$, where $\ln(w_{ijt})$ is the natural logarithm of salaries for teacher i, working in district j in year t; $Female_i$ equals 1 for women; Y_j is either the year of expiration of district j's CBA or the year in which the extension to the CBA ended; the vector X_{it} contains district, seniority, and education fixed effects (alone and interacted for an indicator for years after a CBA expiration), and year fixed effects interacted with extension year fixed effects. We also control for seniority and education fixed effects interacted with $Female_i$ and with an indicator for years following Y_j; the plotted coefficients refer to teachers with 19 or 20 years of experience and a master's degree. The coefficients are estimated and shown separately for flexible-pay (FP) and seniority-pay (SP) districts. Standard errors are clustered at the district level.

Figure AV: Gender Gap in Salaries, by Time to Expiration/Extension of CBA. Balanced Panel



Note: This figure estimates equation 3 using a balanced panel. Teachers in this sample are working in the Wisconsin public school district three years before and three years after their district's extension date. Standard errors are clustered at the district level.

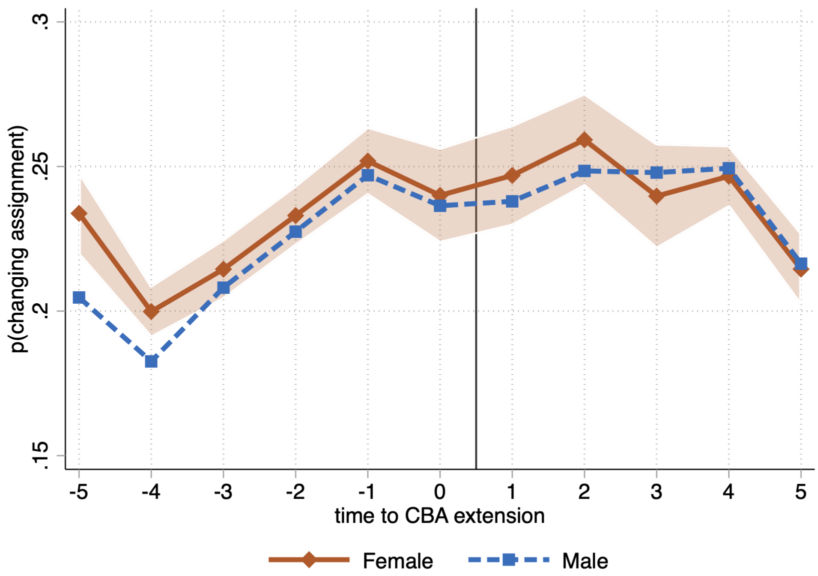
Figure AVI: Gender Gap in Salaries, by Time to Expiration/Extension of CBA. Intent-to-Treat Estimates



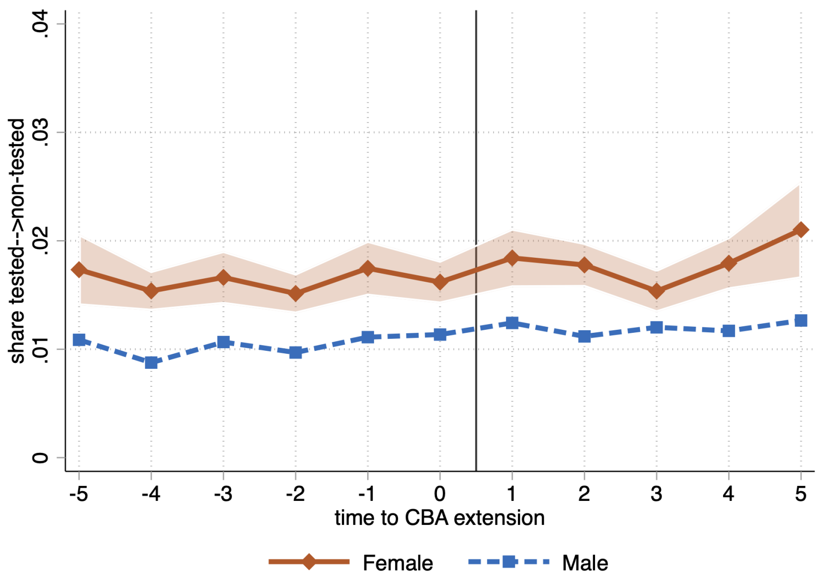
Note: This figure shows the ITT estimates from equation 3. We assign teachers to the district they taught in the year before Act 10 and hold this constant. Standard errors are clustered at the district level. Standard errors are clustered at the district level.

Figure AVII: Switches Across Teaching Posts, By Gender

Panel A) Share of teachers who switch teaching post, by gender

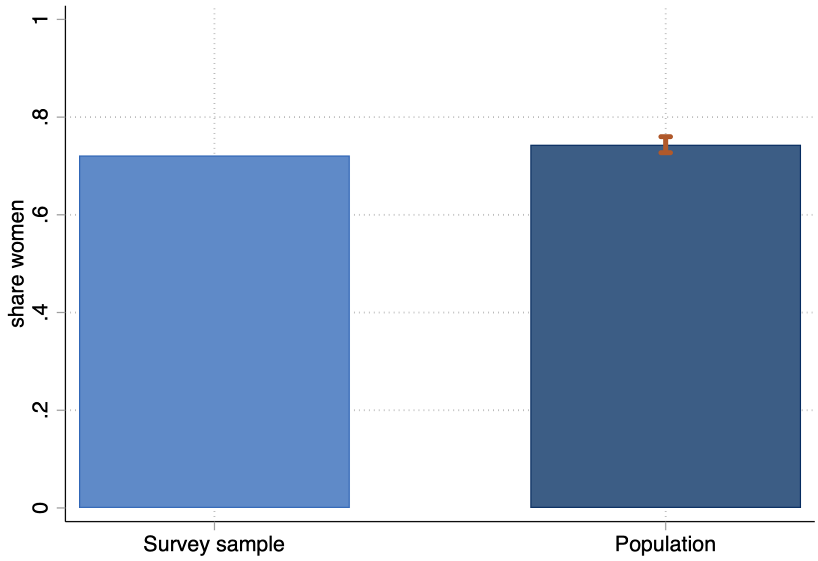


Panel B) Share of teachers who switch from a tested to a non-tested post, by gender



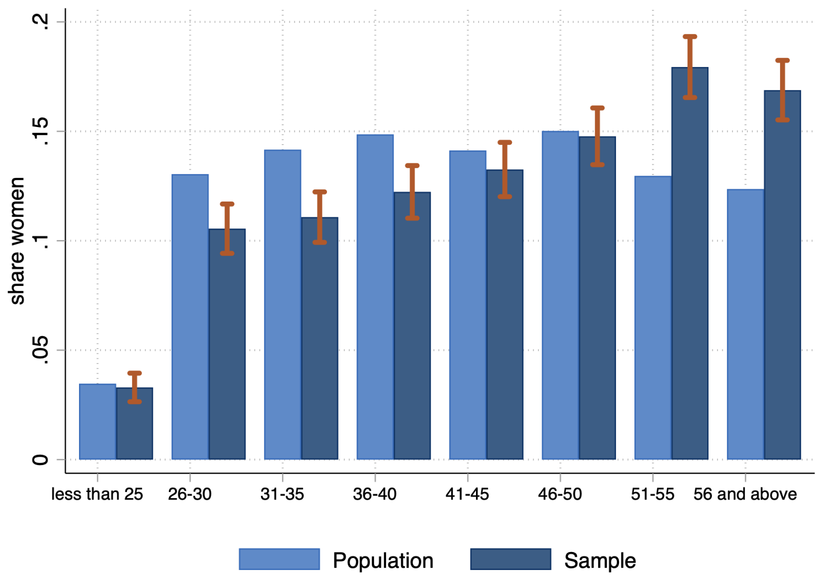
Note: The top panel shows the share of teachers who switch teaching position (i.e., grade or subject), by time-to-CBA expiration and gender. The bottom panel shows the share of teachers who switch from a tested to a non-tested post, by time-to-CBA expiration and gender. Shaded areas represent confidence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

Figure AVIII: Share of Women: Survey Sample vs. Population



Note: Share of female teachers in the survey sample and in the 2016 population. Spikes represent confidence intervals for the difference in mean shares across the two groups. Standard errors are clustered at the district level.

Figure AIX: Age Distribution: Survey Sample vs. Population



Note: Share of teachers in each age group, in the survey sample and in the 2016 population. Spikes represent confidence intervals for the difference in mean shares across the two groups.

Table AI: Gender Gap in Salaries, Prior to CBA Expirations/Extensions

	(1)	(2)	(3)	(4)	(5)
Female	-0.0087**	-0.0055***	-0.0046***	-0.0011	-0.0011
	(0.0037)	(0.0015)	(0.0013)	(0.0010)	(0.0010)
Distr and year FE	Yes	Yes	Yes	Yes	Yes
Experience FE	No	Yes	Yes	Yes	Yes
Education FE	No	No	Yes	Yes	Yes
Teaching assignm	No	No	No	Yes	Yes
Subject	No	No	No	Yes	Yes
N	307525	307522	307355	307355	307355
# districts	428	428	428	428	428

Note: This table shows how the pre-Act 10 gender salary gap changes as we control for observable characteristics that go into district salary schedules. Estimates are obtained using data on years prior to each district's CBA expiration. The dependent variable is the natural logarithm of salary per year, in full-time equivalency units. The variable *Female* equals one for female workers. All specifications include district and year fixed effects; columns 2-5 include years of experience fixed effects, columns 3-5 include fixed effects for the highest education degree, columns 4-5 include fixed effects for the school level (elementary, middle, high school), and column 5 includes fixed effects for subjects taught. Standard errors in parentheses are clustered at the district level. $^{**} \leq 0.1$, $^{**} \leq 0.05$, $^{***} \leq 0.01$.

Table AII: Outside Options and the Gender Gap in Salaries

	Log Salary		
	(1)	(2)	(3)
Female	-0.004***	-0.004**	-0.001
	(0.001)	(0.002)	(0.002)
Post Extension	-0.012***	-0.012***	-0.013***
	(0.004)	(0.004)	(0.004)
Female × Post Ext	-0.002	-0.005**	-0.001
	(0.001)	(0.002)	(0.002)
Female × Post Ext × Num Schools	-0.001		
	(0.001)		
Female × Num Schools		0.001	
		(0.001)	
Female × Post Ext × Num High Schools		-0.001	
		(0.001)	
Female × Num Elem Schools			0.001
			(0.001)
Female × Post × Num Elem Schools			-0.001
			(0.001)
Distr × Post Exp FE	Yes	Yes	Yes
Educ, Exper, Teaching Assign × Post Exp FE	Yes	Yes	Yes
Observations	579,331	184,060	247,500
R-squared	0.801	0.791	0.810

Note: The variable $NumSchools$ is the number of schools in a teacher's commuting zone. In column 2, $NumHighSchools$ is the number of high schools in a teacher's commuting zone and the sample is restricted to high school teachers. In column 3, $NumElemSchools$ is the number of elementary schools in a teacher's commuting zone and the sample is restricted to elementary school teachers. Standard errors in parentheses are clustered at the district level. $^{**} \leq 0.1$, $^{**} \leq 0.05$, $^{***} \leq 0.01$.

Table AIII: Survey Answers, By Knowledge of Colleagues' Pay. OLS Estimates

Panel A) Ever negotiated with:

	Current empl.			In the future	
	At start	After start	Salary	Class assgn	Non-teach. duties
Female	-0.066**	-0.030	-0.394**	0.292**	0.117
	(0.028)	(0.020)	(0.180)	(0.141)	(0.158)
Fem * knows coll. pay	-0.014	0.006	-0.217	-0.051	-0.681***
	(0.045)	(0.038)	(0.257)	(0.275)	(0.262)
Controls	Yes	Yes	Yes	Yes	Yes
N	2836	2836	2836	2836	2836
Y mean, males	0.306	0.245	3.889	4.539	4.579

Panel B) Negotiated successfully conditional on negotiating, with:

	Current employer, at start	Current employer, after start
[1em] Female	-0.149**	-0.058
	(0.062)	(0.086)
Fem * knows coll. pay	0.048	-0.113
	(0.114)	(0.137)
Controls	Yes	Yes
N	700	614
Y mean, males	0.814	0.572

Panel C) Reasons for not negotiating:

	Not possible	Not comfortable	Useless	Fear backlash	Satisfied w/pay
[1em] Female	-0.027	0.066*	0.022	-0.013	-0.032
	(0.037)	(0.038)	(0.035)	(0.024)	(0.026)
Fem * knows coll. pay	0.011	-0.006	0.007	0.053	-0.023
	(0.055)	(0.048)	(0.055)	(0.037)	(0.042)
Controls	Yes	Yes	Yes	Yes	Yes
N	2222	2222	2222	2222	2222
Y mean, males	0.565	0.210	0.215	0.131	0.189

Note: All tables control for district fixed effects. Controls include age classes, self-reported performance, measures of people skills, and for whether each person knows someone who negotiated or the salary of their colleagues. ** ≤ 0.1, ** ≤ 0.05, *** ≤ 0.01.

Table AIV: Survey Answers: Likelihood of Negotiating, OLS Estimates. No controls

Panel A) Ever negotiated with:

	Previous employer	Current empl., at start	Current empl, after start
[1em] Female	-0.090***	-0.085***	-0.043**
	(0.020)	(0.021)	(0.019)
N	2836	2836	2836
Y mean, males	0.379	0.306	0.245

Panel B) Negotiated successfully conditional on negotiating, with:

	Previous employer	Current empl., at start	Current empl., after start
[1em] Female	-0.080***	-0.080***	-0.106*
	(0.028)	(0.028)	(0.056)
N	902	902	614
Y mean, males	0.904	0.814	0.572

Panel C) Reasons for not negotiating (current employer, at start)

	Not possible	Not comfortable	Useless	Fear backlash	Satisfied w/pay
[1em] Female	-0.028	0.083***	0.038	0.010	-0.051**
	(0.027)	(0.029)	(0.025)	(0.018)	(0.021)
N	2222	2222	2222	2222	2222
Y mean, males	0.565	0.210	0.215	0.131	0.189

Panel D) Likelihood of negotiating in the future, over:

	Salary	Classroom assignment	Non-teaching duties
[1em] Female	-0.563***	0.271*	-0.160
	(0.165)	(0.148)	(0.131)
N	2836	2836	2836
Y mean, males	3.889	4.539	4.579

Note: All regressions include controls for age class, self-reported job performance (above/below average), a measure of people skills, an indicator for whether the respondent knows someone who negotiated his/her salary, an indicator for whether the respondent knows his/her colleagues' salaries, and district fixed effects. ** ≤ 0.1, ** ≤ 0.05, *** ≤ 0.01.

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

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

$$A_{kt} = \beta X_{kt} + \nu_{kt} \qquad (7)$$
$$\text{where } \nu_{kt} = \mu_{i(kt)} + \theta_{c(kt)} + \varepsilon_{kt}$$

and where A_{kt} is a standardized measure of test scores (or test score gains) for student k in year t, and X_{kt} is a vector of student, grade, and school observables which could affect achievement, including: school and grade-by-year fixed effects; cubic polynomials of past scores interacted with grade fixed effects; cubic polynomials of average past scores for the students in the same grade and school, interacted with grade fixed effects; student k's demographic characteristics, including gender, race and ethnicity, disability, English-language earner status, and socioeconomic status; the same demographic characteristics, averaged for all students in the same grade and school as student k in year t; and the student's socioeconomic status interacted with the share of low-socioeconomic status in her grade and school in t.[31] The residual ν_{kt} can be decomposed into three parts: The error term component $\mu_{i(kt)}$ is the individual effect of teacher i, teaching student k in year t; the component $\theta_{c(kt)}$ is an exogenous classroom shock; and ε_{kt} is an idiosyncratic student-specific component which varies over time. VA is an estimate of the teacher effect μ_i.

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

1. Estimate β in equation (7) via OLS;
2. Construct residuals $\hat{\nu}_{kt} = A^*_{kt} - \hat{\beta} X_{kt}$, where $\hat{\beta}$ is the OLS estimate of β;
3. Estimate VA as $\bar{\nu}_i \left(\frac{\sigma_\mu}{Var(\bar{\nu}_i)} \right)$, where
   (a) $\bar{\nu}_i = \sum_t w_{it} \bar{\nu}_{it}$ is a weighted average of average test score residuals $\bar{\nu}_{it}$ for teacher i in year t;
   (b) $w_{it} = \frac{h_{it}}{\sum_t h_{it}}$, with $h_{it} = \frac{n_{it}}{n_{it}\sigma_\theta^2 + \sigma_\varepsilon^2}$, are the weights, function of class size n_{it}, the variance of the classroom component σ_θ^2 and of the residual component σ_ε^2;
   (c) the variance of the teacher effect is $\sigma_\mu^2 = Cov(\bar{\nu}_{it}, \bar{\nu}_{it-1})$; the variance of the residual component is $\sigma_\varepsilon^2 = Var(\nu_{kt} - \bar{\nu}_{it})$; the variance of the classroom component is $\sigma_\theta^2 = Var(\nu_{kt}) - \sigma_\varepsilon^2 - \sigma_\mu^2$.

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

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

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

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

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

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

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

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

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

Two features of this identification strategy are worth highlighting:

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

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

Identification of Teacher Value-Added With Turnover

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

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

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

$$\mu_{A3} = \begin{bmatrix} A_{A1}^2 & A_{A1}A_{A2} \\ A_{A1}A_{A2} & A_{A2}^2 \end{bmatrix}^{-1} \begin{bmatrix} A_{A1}A_{A3} \\ A_{A2}A_{A3} \end{bmatrix} \quad (8)$$

$$\mu_{B3} = \begin{bmatrix} A_{B1}^2 & A_{B1}A_{B2} \\ A_{B1}A_{B2} & A_{B2}^2 \end{bmatrix}^{-1} \begin{bmatrix} A_{B1}A_{B3} \\ A_{B2}A_{B3} \end{bmatrix} \quad (9)$$

$$\mu_{C3} = \begin{bmatrix} A_{C1}^2 & A_{C1}A_{C2} \\ A_{C1}A_{C2} & A_{C2}^2 \end{bmatrix}^{-1} \begin{bmatrix} A_{C1}A_{C3} \\ A_{C2}A_{C3} \end{bmatrix} \quad (10)$$

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

$$\bar{A}_1^4 = \frac{1}{2}(A_{A1} + A_{B1}) \quad (11)$$

$$\bar{A}_1^5 = A_{C2} \quad (12)$$

$$\bar{A}_2^4 = \frac{1}{2}(A_{B2} + A_{C2}) \quad (13)$$

$$\bar{A}_2^5 = A_{A2} \quad (14)$$

$$\bar{A}_3^4 = \frac{1}{2}(A_{A3} + A_{C3}) \quad (15)$$

$$\bar{A}_3^5 = A_{B3} \quad (16)$$

My VA estimator implies:

$$\mu_{A3} = \begin{bmatrix} (\bar{A}_1^4)^2 & \bar{A}_1^4\bar{A}_2^5 \\ \bar{A}_1^4\bar{A}_2^5 & (\bar{A}_2^5)^2 \end{bmatrix}^{-1} \begin{bmatrix} \bar{A}_1^4\bar{A}_3^4 \\ \bar{A}_2^5\bar{A}_3^4 \end{bmatrix} \quad (17)$$

$$\mu_{B3} = \begin{bmatrix} (\bar{A}_1^4)^2 & \bar{A}_1^4\bar{A}_2^4 \\ \bar{A}_1^4\bar{A}_2^4 & (\bar{A}_2^4)^2 \end{bmatrix}^{-1} \begin{bmatrix} \bar{A}_1^4\bar{A}_3^5 \\ \bar{A}_2^4\bar{A}_3^5 \end{bmatrix} \quad (18)$$

$$\mu_{C3} = \begin{bmatrix} (\bar{A}_1^5)^2 & \bar{A}_1^5\bar{A}_2^4 \\ \bar{A}_1^5\bar{A}_2^4 & (\bar{A}_2^4)^2 \end{bmatrix}^{-1} \begin{bmatrix} \bar{A}_1^5\bar{A}_3^4 \\ A_{C2}\bar{A}_3^4 \end{bmatrix} \quad (19)$$

Equations (8)-(19) represent a system of 12 equations in 12 unknowns: μ_{A3}, μ_{B3}, μ_{B3}, A_{A1}, A_{A2}, A_{A3}, A_{B1}, A_{B2}, A_{B3}, A_{C1}, A_{C2}, A_{C3}. In this case, VA can be perfectly identified for all teachers because at least one teacher switches grade each year.

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

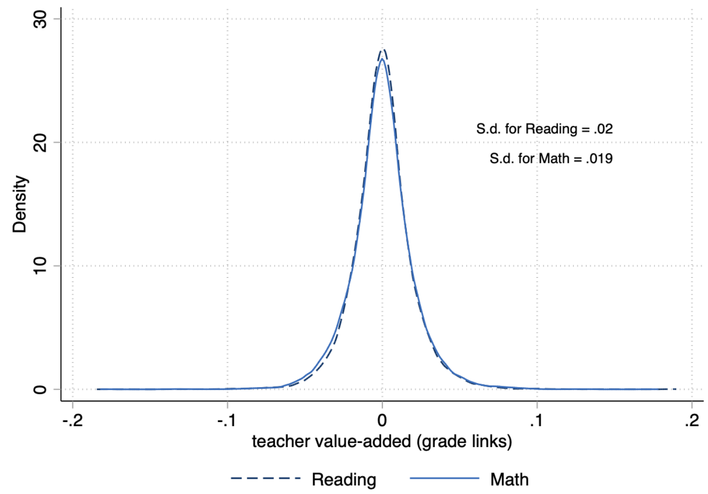
To validate the VA estimator with grade-school links described above (which we call GL) against the standard Kane and Staiger estimator with classroom links (CL), we use teacher and student data from the New York City Department of Education (NYCDOE) from the years 2006-07 to 2009-10. This dataset contains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

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

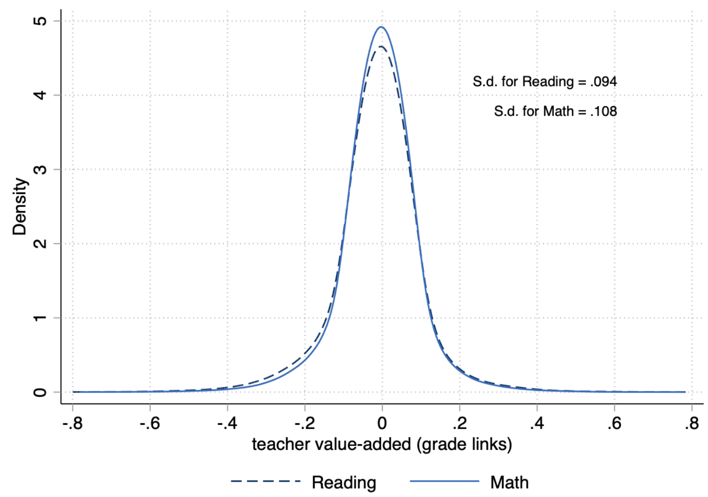
Classic measurement error will push VA estimates towards zero. To quantify the extent of this problem, Figure BI shows the kernel density of the distribution of GL (top panel) and CL (bottom panel). As expected, the distribution of GL is more concentrated around zero compared to CL. In spite of this,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

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

Panel A: Value-Added with Grade-School Links



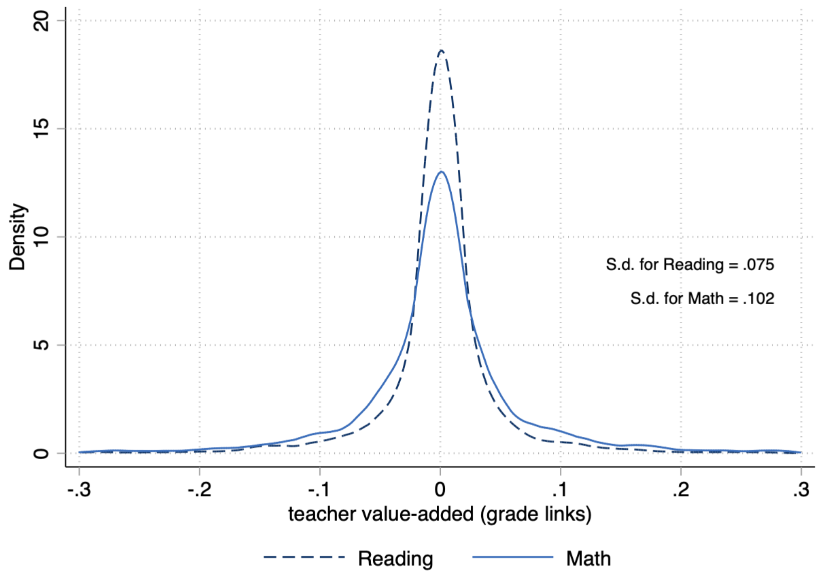
Panel B: Value-Added with Classroom Links



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

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

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



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

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

$$\hat{\mu}_i^{CL} = \alpha + \gamma\hat{\mu}_i^{GL} + \chi_i \tag{20}$$

Assuming χ_i to be uncorrelated with $\hat{\mu}_i^{GL}$, the forecast bias f is zero if $\gamma = 1$: $f = 1 - \gamma$. We can estimate the slope coefficient γ via OLS on equation (20). The 95% confidence interval for γ, whose point estimate is equal to 0.99, includes 1, which implies that the forecast bias f is equal to 0.01 and it is indistinguishable from zero (Figure BIII).

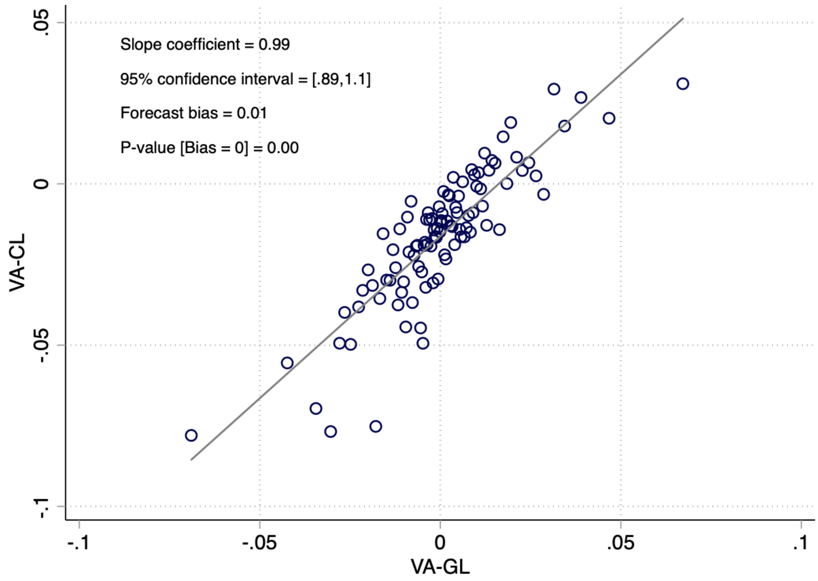
Teacher Switches as a Quasi-Experiment As an additional test for the unbiasedness of GL estimates we exploit teacher switches across grades as a quasi-experiment, as in Chetty et al. (2014). If VA is an unbiased measure of teacher quality, changes in average VA of teachers in a given school and grade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

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

$$\Delta A^*_{gst} = a + b\Delta Q_{gst} + \Delta\chi_{gst} \tag{21}$$

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

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



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

teacher VA, and $\Delta W_{gst} = W_{gst} - W_{gst-1}$ for any variable W_{gst}. The forecast bias is defined as $\lambda = 1 - b$. Table BIII shows estimates of b and λ, obtained using either mean residual test scores or mean actual test scores, and controlling for school-by-year fixed effects (as in Chetty et al., 2014).[32] Estimates of b are all close to 1 both over the full sample period and in the years after Act 10. While slightly larger than Chetty et al. (2014), who estimate it to be between 0.003 and 0.026, estimates of b are small and indistinguishable from zero, both over the full sample period and in the years after Act 10.

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

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

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

Table BI: Forecast bias in teacher VA

	Δ test scores	Δ test score residuals
(lr)2-2(lr)3-3	(1)	(2)
ΔVA_{gst}	0.978	1.055
	(0.290)	(0.377)
School-by-year FE	Yes	Yes
Observations	13684	13684
# districts	414	414
λ	0.022	-0.055
p-value λ=0	0.94	0.88

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

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

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

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

Appendix C Survey Details

Survey Questionnaire

General Questions

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

Negotiation

6. Have you ever negotiated your pay with any of your past employers?
   - Yes, successfully
   - Yes, unsuccessfully
   - No, it was not a possibility
   - No, it was a possibility but I chose not to
   - No, it was a possibility but I did not feel I could negotiate without repercussions
7. When you started your current job, did you negotiate your pay?
   - Yes, successfully

- Yes, unsuccessfully
- No

8. Why didn't you negotiate your pay? [choose all that apply]

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

9. Since starting your current job, have you ever asked for a pay increase?

   - Yes, successfully
   - Yes, unsuccessfully
   - No

10. Why haven't you asked for a pay increase? [choose all that apply]

    - I would not have gotten anything out of it It is not a possibility
    - I am worried about backlash
    - I don't feel comfortable asking
    - I am satisfied with my salary

11. How likely is it that you will negotiate any of the following in the future? [for each item, choose a number from 1 (not at all likely) and 10 (very likely))

    - Salary
    - Classroom assignment
    - Non-teaching duties

12. Do you know what your colleagues earn?

    - Yes
    - Only some of them
    - No

13. Do you know any public sector teachers who have negotiated their salary?

    - Yes, among my colleagues
    - Yes, outside of my colleagues
    - Yes, both among and outside of my colleagues
    - No

14. How would you rate your performance relative to your colleagues' performance?

    - Below average
    - Average
    - Above average

15. Are you confident about talking to people you don't know?

- Yes
- No

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

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

Figure CI: Survey Email

From: Heather Sarsons

To: [TEACHER'S EMAIL]

Subject: A short survey for a Yale and UChicago study

Good evening,

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

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

Follow this link to the Survey:
[LINK]

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

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

Best regards,

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

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

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