Unions, Monitoring, and Deferred Compensation: Evidence from California School Districts

Paul Bruno
University of Illinois at Urbana-Champaign

337 Education Building
1310 S. 6th Street
Champaign, IL 61820

(e) pbruno@illinois.edu

Abstract

Public agencies vary considerably in the extent to which they defer compensation until later in workers' careers and often heavily backload compensation even when frontloaded compensation would likely be more efficient. I use two-way fixed effects models and detailed longitudinal data on collective bargaining agreements, salaries, and fringe benefits in public school districts in California to test two common theories about the prevalence of deferred public sector compensation. I find no evidence that stronger unions bargain for more backloaded compensation on average. However, I find suggestive evidence that unions may prefer to bargain for more backloaded compensation when their members are more veteran. I find no support for the theory that administrators prefer to defer compensation when employee performance is more difficult to monitor. These results suggest that other explanations for the backloadedness of public sector compensation may hold more promise, though they also call for additional empirical investigation.

Keywords: personnel administration; compensation; employee monitoring; public sector unions; seniority

Uncorrected Accepted Manuscript (*Review of Public Personnel Administration*)
December 6, 2022

Unions, Monitoring, and Deferred Compensation: Evidence from California School Districts

Public sector compensation is more backloaded than in the private sector (i.e., deferred until later in workers' careers; Glaeser & Ponzetto, 2014; Kelley, 2014). However, theory and evidence provide reasons to believe that frontloaded compensation – allowing for rapid early-career increases in earnings – can be more effective because early-career workers are more sensitive to incentives and make more rapid productivity gains (Ballou & Podgursky, 2002; Hendricks, 2014, 2015). Researchers often rely on two theories to explain why backloaded compensation persists in the public sector. First, backloaded compensation may reflect rents extracted by labor unions (Glaeser & Ponzetto, 2014). Second, imperfect information about workers may incentivize administrators to defer compensation until their quality can be assessed through performance monitoring (Heutel, 2009; Lazear, 1981; Prendergast, 1999). However, studies testing these theories are few, have mixed results, and are hampered by data limitations.

Public school districts are perhaps the public agencies where issues of deferred compensation, personnel monitoring, and union influence are most hotly contested. As with many public sector workers, retiree benefit costs for teachers have placed intense pressure on both state and school district budgets in recent years (e.g., Bruno, 2019; Moody & Randazzo, 2020), and backloaded teacher salaries and benefits have been criticized as inefficient and as exacerbating teacher turnover (e.g., Vigdor, 2008). These debates are all the more heated because they are commonly interconnected with arguments that administrators monitor public school teachers' performance ineffectively, if at all (e.g., Aldeman & Chuong, 2014). The persistence of these personnel practices is commonly – and often contentiously – attributed to the influence of teachers' unions over state policy and local administration and said to have detrimental impacts on students (e.g., achievement; Grissom & Strunk, 2012; Vigdor, 2008). This has motivated

recent state-level reforms restricting public sector workers' collective bargaining rights, often focused on teachers (Marianno, 2015).

It is in this context that I empirically test the theories that deferred public sector compensation reflects either union influence or difficulties associated with monitoring workers. I use a long panel of data on how school districts in California compensate teachers. The detail of these data allows me to assess the extent to which compensation is deferred in a variety of ways often not observed in prior work, including salary levels across the entire experience distribution and the generosity of health benefits for both active employees and retirees. I also use longitudinal data on the content of collective bargaining agreements (CBAs) between teachers' unions and administrators in a subset of districts to obtain a proxy for overall union strength (Strunk & Grissom, 2010; Strunk & Reardon, 2010) as well as a novel proxy for monitoring intensity: the extent to which the CBA restricts the ability of administrators to evaluate teachers. These proxies allow more direct tests of the aforementioned compensation theories than in previous work, and longitudinal data allow me to address many potential sources of endogeneity.

I do not find that unions prefer deferred compensation on average. However, I find evidence that unions' behavior is moderated by the experience levels of their members. In particular, when teachers in the district are more experienced, union influence predicts more deferred compensation in the form of later-career salaries and health benefits for retirees. I find no evidence that administrators backload compensation due to imperfect information about workers. Neither administrator/teacher ratios nor restrictions on the teacher evaluation process predict deferred compensation. Given the importance of compensation to public administration (Fowler & Birdsall, 2020; Llorens, 2008), these results will be of interest to researchers studying public sector labor markets and contribute to our understanding of administrative dynamics in

public agencies. They will also be of interest to policymakers seeking to understand why public agencies have not consistently adopted many of the compensation reforms (e.g., performance pay or pay banding) that are often recommended by research or reformers (Kellough & Lu, 1993; Nelson, 2004).

THEORY AND EVIDENCE ON DEFERRED PUBLIC SECTOR COMPENSATION

Union Influence

Perhaps the most common theory of deferred public sector compensation is that it reflects rent extraction by labor unions, made possible because administrators are conflict-averse, weakly accountable to taxpayers, or have their own compensation linked to employee salaries, and veteran workers are often more influential in unions (Hek & Vuuren, 2011; Kelley, 2014; Lankford & Wyckoff, 1997). Also, public sector unions can influence their compensation through political processes such as election campaigns (Kelley, 2014; Moe, 2006). Fringe benefits are thought to be particularly amenable to this rent seeking because it is difficult for voters to assess their costs and hold officials accountable for them (Glaeser & Ponzetto, 2014).

Unions may also drive deferred compensation through psychological impacts on their members. Unions are sometimes thought to increase worker dissatisfaction, for example by drawing attention to the least attractive elements of the job or by raising members' expectations beyond what can be delivered (Hammer & Avgar, 2005). Unions may also increase workers' commitment to the organization or public sector, increasing their willingness to self-sacrifice (Davis, 2011). More influential unions may therefore have fewer costs in terms of job satisfaction (e.g., because they can deliver on more promises) and more benefits in terms of member commitment (e.g., because they are more effective at socializing members). If so, unions may reduce the need to incentivize workers to stay in the short term and increase workers'

willingness to wait for (i.e., defer) compensation.

While the preponderance of evidence suggests that public sector unions advocate for higher wages (Llorens, 2008), empirical work on their advocacy for deferred compensation focuses mostly on pensions and has very mixed results (Aronson et al., 2009; Chaney et al., 2002; Hoang & Goodman, 2018; Kelley, 2014; Mitchell & Smith, 1994; Thom, 2013, 2017). Studies of teachers' unions has sometimes considered non-pension compensation but is similarly mixed. Grissom and Strunk (2012) find significantly more backloading of teacher salary schedules in jurisdictions that collectively bargain salaries than in those that do not. Similarly, Winters (2011) finds that collective bargaining is associated with higher salaries, but more so for veterans than for novices. However, other studies find that unionization is associated with more salary frontloading (Gustman & Segal, 1977; West & Mykerezi, 2011).

One way to reconcile these results comes from median voter models that predict that union leaders will advocate for the interests of the representative member (Farber, 1986). Thus, the extent to which unions advocate for early- vs. later-career compensation will depend on the experience levels of the union's membership. This view has been tested in the case of teachers but again finds only mixed support. Babcock and Engberg (1999) find that the difference in salaries between more- and less-experienced teachers is larger in districts where the median teacher is more experienced, but only among districts with high local union support, suggesting an interaction between union influence and member seniority. However, Ballou and Podgursky (2002) do not find that differences in teacher salary increases over time for novices and veterans are related to collective bargaining, raising questions about the causal role of unions.

Imperfect Information about Workers

Another explanation for deferred compensation is that it may result from principal-agent

problems arising from imperfect information about workers. If employers do not know who the best employees are or are constrained in their ability to directly incentivize effort (Miller & Whitford, 2007), high early-career compensation will result in inefficient amounts of compensation going to less productive staff. Deferred compensation can incentivize workers who risk losing that compensation if caught shirking (Lazear, 1981; Prendergast, 1999). This implies that deferred compensation is a substitute for monitoring. This could be why frontloading is more common in jobs where performance is easier to observe, like sales (Prendergast, 1999).

There is some evidence that employers use deferred compensation to motivate workers when supervision is difficult (Adams & Heywood, 2011; Barth, 1997; Groshen & Krueger, 1990), though these studies typically use indirect proxies of monitoring (e.g., supervisor/staff ratios) and use cross-sectional data that may result in endogeneity issues. They are also not specific to the public sector, where considerations may differ because shirking may be less likely to result in termination even if caught (Ballou & Podgursky, 2002), deferred compensation is not highly valued by workers (Fitzpatrick, 2015), or workers are less motivated by extrinsic incentives (Fowler & Birdsall, 2020). I am aware of only one study that tests the possibility that deferred compensation serves as a substitute for monitoring the performance of teachers specifically. Heutel (2009) finds that a district's administrator/teacher ratio is unrelated to the ratios of tenth- or twentieth-year salary to starting salaries. This suggests that public agencies may be unlikely to use deferred compensation to disincentivize shirking, though these proxies for monitoring and backloading are both relatively indirect measures of the constructs of interest.

Research examining whether unions or monitoring intensity can explain backloaded public sector compensation is far from definitive. The number of studies testing these theories is small, and the studies discussed do not allow for easy causal interpretations. They typically rely

on indirect proxies for the constructs of interest, and because they use cross-sectional data they mostly cannot control for unobserved factors as is possible, to varying degrees, with longitudinal data. This may explain why their results are mixed and leave important questions unanswered about why backloaded compensation is common in the public sector.

School districts are important public agencies and are a useful context to study these issues because they often heavily backload salaries (Grissom & Strunk, 2012; Vigdor, 2008) and benefits (Bruno, 2019; Fitzpatrick, 2015), even as evidence suggests that teachers can be recruited and retained more effectively with frontloaded compensation (Ballou & Podgursky, 2002; Grissom & Strunk, 2012; Hendricks, 2014, 2015; Lankford & Wyckoff, 1997). I also extend the literature methodologically in two ways. First, by using the actual contents of CBAs, I observe more detailed compensation information than in previous work, as well as more direct measures of the extent to which the CBA constrains district administrators generally (a proxy for union power) or on matters of teacher evaluation specifically (a proxy for the ability to monitor employees). Second, because I observe districts over many years I can control for unobservable between-district factors that might otherwise bias estimates.

I address two research questions, motivated by the theory and literature discussed above. First, *Do districts defer compensation due to teachers' union influence?* If backloaded salaries indicate rents being captured by unions, union strength should be associated with more backloading. Moreover, if unions advocate in the interests of their members, union strength should be associated with more backloading when union members are more experienced. Second, *Do districts defer compensation for teachers as a substitute for monitoring?* If districts substitute backloaded compensation for monitoring, frontloaded compensation should be more prevalent when administrators can more easily monitor teacher performance.

DATA

I answer these questions using data from California school districts. California contains roughly 1,000 school districts, virtually all of which are unionized and report detailed data to the state that is eventually made public. Moreover, state law requires that a wide range of topics be collectively bargained between the district and the teachers' union at least every three years, such as compensation, evaluation, hours of employment, and working conditions (Ed-Data, 2021). This results in substantial variation in CBAs, and state-level requirements have remained essentially unchanged during the period under consideration (Strunk et al., 2018), bolstering the interpretation that variation over time is driven by local dynamics.

I use annual data on 910 unique California school districts released by the California Department of Education (CDE), including the 2004-2005 through 2018-2019 school years. Staff data files link teachers and administrators to schools and include their years of experience in their current district. The CDE also releases enrollment and student demographic data at the district level each year, such as student race, free- or reduced-price lunch (FRL) eligibility (a proxy for their income level), and English learner status. I additionally use data from the National Center for Education Statistics to associate districts with labor markets (Taylor & Fowler, 2006).

Measures of Deferred Compensation

My teacher compensation data come from annual "Salary and Benefits Schedule for the Certificated Bargaining Unit" (or "J-90") surveys submitted by districts to the CDE. These include the number of service days for teachers, the salary offered at each step (experience level) of each lane (level of education) of the salary schedule, and the healthcare plans available to active teachers and retirees. This is more compensation detail than is available in data sets used

in similar prior work, which often observes salaries at just a few combinations of experience and education or cannot observe fringe benefits. I take advantage of this detail to consider salaries at each of the first 30 steps since teachers in California's teacher pension system become eligible for full benefits with 30 years of service credit, changing retirement incentives.

Outside of the statewide pension system, which I do not consider, there are few statewide constraints on teacher compensation in California. Thus, districts can, through local collective bargaining, establish a wide range of health and welfare benefits packages for active employees or retirees, or elect not to provide such benefits. Similarly, districts can establish different lanes on their salary schedules for different levels of education, each with an arbitrary number of steps for different experience levels. This autonomy allows me to explore variation in compensation over time, but also complicates comparisons between salary schedules. These comparisons are more straightforward if I focus on wage profiles for teachers with a specific education level, and this also allows me to distinguish returns to experience from returns to education. I focus on the lane for teachers with a bachelor's degree and 60 additional semester units of education, as the J-90 identifies this lane in all years. However, other lanes may also be important, so I additionally consider the minimum and maximum salaries offered anywhere on the salary schedule.

As noted above, non-salary benefits are thought to be particularly likely to be deferred by rent-seeking public sector actors because they are harder for the public to observe. I consider two measures of deferred benefit compensation. First, I consider the maximum employer contribution to health benefit plans, included in J-90 responses since 2004-2005. Because later-career teachers are more likely to be married and have children, I consider contributions to both single-party and family plans. To accommodate observations where no contributions are offered by the district, I subject maximum contributions to an inverse hyperbolic sine (IHS) transformation (Burbidge et

al., 1988). Additionally, I use a dichotomous measure of whether post-employment (i.e., retiree) health benefits are offered to at least some teachers, information recorded since 2011-2012.

Proxies for Union Strength and Monitoring Intensity

My proxy for union strength is a measure of the extent to which CBAs restrict district administrators, a plausibly more direct measure of union influence than other common proxies, such as local unionization rates or political party affiliation (e.g., Babcock & Engberg, 1999). This measure comes from a content analysis of actual CBAs collected from approximately 500 of the largest districts in California every three years from 2005-2006 through 2014-2015. This is the minimum frequency with which state law requires CBAs to be renegotiated, substantially standardizing negotiation frequency, though as I show in appendix B results are not sensitive to the exclusion of districts that appear to have unusual renegotiation frequencies. Contracts were dichotomously coded for the presence of 253 items that impose constraints on administrators (e.g., the imposition of maximum class sizes). These 253 items were reduced to 34, presented in appendix table A1, via alpha item analysis, and were then entered into a partial independence item response (PIIR) model to estimate latent contract restrictiveness, much as test responses are used to estimate students' latent subject knowledge. The PIIR model allows for interdependence between CBA provisions such that one provision cannot be present unless another "gate item" is present. For example, a CBA can only guarantee the union president at least 20 days of leave to conduct association business if it also guarantees the president at least 10 days of such leave. This makes the latter restrictive provision a gate item for the former. If $\boldsymbol{Y_{ktd}}$ is defined as a dummy indicator of whether restrictive provision k is present in the CBA in year t in district d, and $\boldsymbol{\varphi_{ktd}}$ is the probability that $\boldsymbol{Y_{ktd}}$ equals one conditional on whether its gate item is present, CBA restrictiveness can be estimated using the hierarchical random effects model:

$$log\left[\frac{\varphi_{ktd}}{1-\varphi_{ktd}}\right] = \theta_{td} + \sum_{j=1}^{K} \gamma_k D_{ktd} + \tau_t \quad (1)$$

D is a dummy variable indicating whether provision *k* is present in a contract so that γ_k captures the conditional restrictiveness of item *k*. The latent restrictiveness of the CBA is estimated by the sum of $\hat{\tau}_t$ (a year random effect) and $\hat{\theta}_{td}$ (a CBA random effect). I standardize this continuous, interval measure of restrictiveness across all districts in each year. This measure has been shown to correlate with school board members' perceptions of union power and with union involvement in board elections (Strunk & Grissom, 2010), suggesting that it is a valid proxy for union strength. Moreover, the longitudinal restrictiveness measure used here has been shown to be associated with district spending and student achievement, suggesting that it is substantively related to districts' operations (Marianno et al., 2021). For additional details about the estimation of CBA restrictiveness, see Marianno and Strunk (2018) and Strunk and Reardon (2010).

I consider two proxies for monitoring intensity. First, like Heutel (2009), I use the administrator/teacher ratio. Second, I use a measure of the extent to which the CBA restricts administrators in the evaluation of teachers that is like the overall CBA restrictiveness measure, with two differences. First, the evaluation restrictiveness measure uses only CBA provisions that place limits on the ability of administrators to evaluate teachers (e.g., specifying the minimum advanced notice for teachers prior to classroom observations). Second, while the overall measure includes only items with a sufficiently high scale reliability, due to a smaller number of available items the evaluation restrictiveness measure includes all 21 evaluation-related provisions collected in all years. These items are presented in appendix table A2. The CBA's restrictions on evaluation are plausibly a more direct measure of the intensity with which teacher performance is monitored and have not to my knowledge been used in this way in previous research. Summary statistics for all variables are presented in appendix table A3.

EMPIRICAL APPROACH

First, to explore the relationship between compensation profiles and union strength, I predict the natural log of salary at each step *s* (sal^s) for teachers in the BA+60 lane (or the minimum and maximum salaries on the schedule) in district *d* in labor market *l* in year *t*:

$$sal^s_{dlt} = \theta_1 res_{dlt} + \theta_2 xp_{dlt-1} + \theta_3[res_{dlt} \times xp_{dlt-1}] + \boldsymbol{D}_{dlt-1}\boldsymbol{\Omega} + \delta_d + \gamma_{lt} + \delta_d t + \varepsilon_{dlt} \quad (2)$$

Alternatively, I predict the IHS of maximum employer health benefit contributions or the availability of retiree benefits. *res* is the CBA restrictiveness measure, which I interact with the median in-district experience level of teachers (*xp*). I use experience in the prior year since it is likely that the CBA was negotiated prior to its effective date. A major advantage of longitudinal data on districts is that I can control for district fixed effects (FEs) (δ_d). This mitigates concerns that results will be biased by unobserved differences between districts related to both union influence and compensation. Still, estimates of the effect of union influence could be biased by factors varying within districts over time. I account for such potential factors in three ways. First, I account for year-to-year shocks common to an economic region with labor market-by-year FEs (γ_{lt}). Second, I control for a vector of time-varying district characteristics (***D***) that have been found in previous research to be related to teacher compensation levels or backloadedness (Grissom & Strunk, 2012; Winters, 2011): shares of students who are black, Hispanic, and eligible for free or reduced-price lunch; the natural log of enrollment; an indicator for declining enrollment; and the number of teaching days. All variables in ***D*** are measured in *t*-1 except for number of teaching days, which is typically bargained concurrently with salaries. Finally, to account for unobserved factors varying linearly within districts over time I again take advantage of the longitudinal nature of my data to control for district-specific linear time trends ($\delta_d t$). ε is an error term. Since I observe districts repeatedly over time, I cluster standard errors on districts.

Second, to test whether backloaded compensation substitutes for monitoring, I estimate:

$$sal^{S}_{dlt} = \theta_1 monitoring_{dlt} + \boldsymbol{D}_{dl,t-1}\boldsymbol{\Omega} + \delta_d + \gamma_{lt} + \delta_d t + \varepsilon_{dt} \quad (3)$$

Model 3 is like model 2 but replaces the CBA restrictiveness and teacher experience predictors with *monitoring*, one of my two monitoring proxies described above. For consistency with the administrator/teacher ratio, I multiply evaluation restrictiveness by negative one so it indicates administrators' evaluation flexibility. Both higher administrator/teacher ratios and fewer restrictions on evaluation will tend to make supervision easier and will be associated with relatively more frontloading if backloading is a substitute for employee monitoring.

My models do not include controls for community ideology or student outcomes. Though these are potentially important determinants of teacher compensation, they are somewhat difficult to measure and controlling for them could obscure relationships of interest if they impact – or are impacted by – union influence or monitoring practices. Appendix B discusses these issues further and presents results showing that my estimates of interest are nearly identical if I additionally control in ***D*** for measures of local political partisanship and student achievement.

RESULTS

RQ1: Do districts defer compensation due to teachers' union influence?

Figure 1 presents average marginal effects from model 2; i.e., the difference in log salary associated with a one standard deviation (SD) increase in CBA restrictiveness. I plot results from models where restrictiveness and teacher experience are not interacted (black markers) as well as from models with interactions where the median district teacher has either 4 years of experience (gray markers) or 13 years (blue markers; the 10th and 90th percentiles in my data, respectively).

Insert Figure 1 about here

Across models, CBA restrictiveness is associated with at most only slightly higher

salaries on average; a SD increase in restrictiveness rarely predicts an increase in salary of more than 1% and estimates mostly fail to reach statistical significance at the 5% level. This is perhaps surprising given the evidence above that union activity is generally associated with higher compensation. However, much of that evidence comes from cross-sectional studies, and results may therefore be biased by unobserved differences between contexts. Additionally, my results are consistent with related work finding that modest increases in salary spending associated with more restrictive CBAs are mostly explicable in terms of lower student/teacher ratios as opposed to higher salaries (Marianno et al., 2021). Teachers in California may prefer smaller classes over higher salaries at the margin because California public schools have relatively high student/teacher ratios (23 vs. 16 nationwide; U.S. Department of Education, 2018).

The average relationship between CBA restrictiveness and salary masks heterogeneity across contexts with more- or less-experienced teachers. While teacher experience moderates the relationship little on the earliest steps of the salary schedule, beginning roughly at step 15 teacher experience moderates the relationship in more substantively – and statistically – significant ways, and to a greater extent higher up the salary schedule. In districts where the median teacher has 4 years of prior experience, CBA restrictiveness is if anything slightly negatively related to salaries above step 15 (or maximum salaries). Conversely, in districts where the median teacher has 13 years of experience a SD increase in CBA restrictiveness is associated with small – and increasing – increments in salary, up to roughly one percent. These relationships are often significantly different both from zero and from relationships in the districts with more novice teachers. This is consistent with union interest in backloaded compensation driven by veterans.

Results predicting benefits (table 1) are similarly suggestive of unions advocating for slightly more deferred compensation in the form of retiree benefits when workers are more

experienced. Columns 7 and 8 show results from models that exclude district FEs and time trends since retiree benefits are observed across at most two CBA cycles. A SD increase in CBA restrictiveness predicts a marginally significant increase in the probability that a district offers retiree benefits of 3 percentage points (pp, column 7). As with higher-step salaries, this is driven by districts where teachers have had longer district tenure. When the relationship is allowed to vary with teacher in-district experience (column 8), a SD increase in restrictiveness predicts a decrease in the probability that retiree benefits are offered of 5.3pp when the median teacher has no prior experience ($p = .324$), but each subsequent year of experience makes that relationship more positive by nearly 1pp ($p = .097$). This implies that a SD increase in restrictiveness predicts an insignificant *decrease* in the probability that retiree benefits are offered of 1.9pp ($p = .59$) when the median teacher has 4 years of experience, but an *increase* of 5.8pp ($p < .01$) when the median experience is 13 years. These relationships increase slightly in magnitude and significance in a logistic specification (not shown). Columns 9 and 10 present results controlling for district FEs. These models identify results from just 16 districts for which the availability of benefits changes between 2011-2012 and 2014-2015, so should be interpreted with caution. Still, results are qualitatively robust, losing statistical significance but shrinking only slightly.

Insert Table 1 about here

I find no evidence indicating that union influence is related to compensation deferred in the form of health benefits. The relationship between CBA restrictiveness and maximum employer contributions to these benefits is insignificant whether considering one-party plans (column 1) or family plans that might be more valuable for veterans (column 4). Teacher experience fails to moderate these relationships (columns 2, 3, 4, and 6). Because California districts on average during this time paid the large majority of all health benefit costs for their

employees (Bruno, 2019), changes in employer contributions may have been driven much more by changes in health insurance costs than by changes in bargained contribution rates.

RQ2: Do districts defer compensation for teachers as a substitute for monitoring?

I find little evidence that districts substitute deferred compensation for monitoring teachers. Figure 2 is like figure 1 but predicts salary levels as a function of the administrator/teacher ratio (black markers) or the flexibility afforded to administrators during the evaluation process (gray markers). Both proxies are standardized for comparability; a SD change in the administrator/teacher ratio is about 4.2 administrators per 100 teachers. The relationship between administrator/teacher ratios and salaries is uniformly small and insignificant across the schedule. I get similar results when I allow the district time trends to vary cubically (not shown). My results are consistent with Heutel (2009), but as noted above the administrator/teacher ratio may say little about how teachers are evaluated or held accountable.

Insert Figure 2 about here

I check this in two ways. First, I consider extent to which the CBA allows administrators flexibility during the teacher evaluation process. Figure 2 presents results from models using this plausibly more direct measure of monitoring intensity, but still provides little evidence that CBAs substitute deferred compensation for monitoring. The fact that the estimates are negative even at the lowest steps of the BA+60 salary lane is not what the monitoring theory predicts; rather, more intense monitoring should result in higher early career salaries. I also find no evidence that either of my proxies for monitoring intensity are predictive of deferred health benefit compensation. In the interests of brevity, I present those results in appendix table B2. These results do not simply reflect that evaluation restrictiveness in the CBA just mirrors overall union influence. Appendix B also presents results showing that results are not significantly

moderated by administrator autonomy; estimates are quantitatively and qualitatively similar if I interact my monitoring proxies with overall CBA restrictiveness. Second, as Ballou and Podgursky (2002) argue, it could be that monitoring makes little difference to teachers because they are unlikely to face serious consequences even if caught shirking. Also in appendix B, I present results examining whether the monitoring/backloading tradeoff is more salient at schools serving lower grade levels, where there is a plausible case that teachers are subject to more accountability pressure. However, the results provide no evidence that administrators substitute deferred compensation for monitoring even for elementary teachers.

In sum, districts in California do not frontload compensation more heavily when monitoring teachers is easier. This may be because, as discussed above, deferred compensation is not an effective motivator in this context. It may also be that teachers face little accountability pressure from their administrators in practice (Ballou & Podgursky, 2002), consistent with relatively high levels of job security in the public sector generally (Delfgaauw & Dur, 2008) or with teachers specifically accumulating important skills with experience that make monitoring less of a priority for administrators than retention (Williamson, 1981).

DISCUSSION

Using detailed, longitudinal data from education agencies in California, including detailed data on the contents of CBAs typically not observed in previous work, I do not find evidence that on average teachers' unions prefer more deferred compensation. However, I find suggestive evidence that union influence is moderated by members' seniority: my proxy for union strength is associated with more deferred compensation when the membership is more experienced. I find no evidence that administrators adopt backloaded compensation when monitoring workers is more difficult. This may reflect the relatively weak incentive power of

deferred compensation, or relatively high levels of job security for public sector workers.

California school districts are distinctive in important ways; because I focus on a state where the political context for unions has remained largely steady, I am unable to speak to state-level political or economic factors that might be relevant for the persistence of deferred public sector compensation. For example, other states have more limited collective bargaining rights for teachers, and choices about compensation may vary for both teachers and administrators in contexts where bargaining rights have recently been aggressively curtailed (e.g., Wisconsin and Michigan; Strunk et al., 2018) or where unions have been able to achieve monopoly power in the labor market to different degrees (Barling et al., 1992). And, as noted above, California schools have relatively high student/staff ratios, which may affect the salience of trade-offs made during bargaining. Therefore, while the results here extend previous work in important ways, they should also be generalized only cautiously. Similar work in other contexts remains potentially valuable.

Still, my results suggest that the deferred nature of public sector compensation may be due less to factors like union influence and monitoring challenges than has often been proposed, though each may play a role. Other factors, such as sensitivity to competition in the labor market (Langbein & Roberts, 2022; Winters, 2011) or efforts to induce worker self-selection (Salop & Salop, 1976) may be more important. Future work should explore such explanations, which may be important for understanding what kinds of public sector compensation reforms are needed in various contexts and whether efforts to impose those reforms are likely to succeed.

By the same token, my results provide reasons to doubt that recent efforts to restrict public sector collective bargaining rights will have the intended effects in high-profile areas like performance monitoring and deferring compensation. At least in California schools, the roles of

unions and collectively bargained constraints on these aspects of administration appear modest at most. Efforts to change those practices may do better to focus on levers besides collective bargaining (e.g., funding for financially constrained agencies) or by focusing more directly on the practices of concern (e.g., salary frontloading or retiree benefit pre-funding requirements).

REFERENCES

Adams, S. J., & Heywood, J. S. (2011). Does Deferred Compensation Increase Worker Effort? *The Manchester School*, *79*(3), 381–404.

Aronson, J. R., Dearden, J. A., & Munley, V. G. (2009). The impact of surplus sharing on the portfolio mix of public sector defined benefit pension plans: A public choice approach. *Public Choice*, *140*(1), 161–184.

Babcock, L., & Engberg, J. (1999). Bargaining Unit Composition and the Returns to Education and Tenure. *ILR Review*, *52*(2), 163–178.

Ballou, D., & Podgursky, M. (2002). Returns to Seniority among Public School Teachers. *The Journal of Human Resources*, *37*(4), 892–912.

Barling, J., Fullagar, C., & Kelloway, K. K. (1992). *The Union and Its Members: A Psychological Approach*. Oxford University Press.

Barth, E. (1997). Firm-Specific Seniority and Wages. *Journal of Labor Economics*, *15*(3), 495–506.

Bruno, P. (2019). *The Challenges of Health and Welfare Benefit Costs for California Districts* [Report]. Policy Analysis for California Education.

Burbidge, J. B., Magee, L., & Robb, A. L. (1988). Alternative Transformations to Handle Extreme Values of the Dependent Variable. *Journal of the American Statistical Association*, *83*(401), 123–127.

Chaney, B. A., Copley, P. A., & Stone, M. S. (2002). The effect of fiscal stress and balanced budget requirements on the funding and measurement of state pension obligations. *Journal of Accounting and Public Policy*, *21*(4), 287–313.

Davis, R. S. (2011). Blue-Collar Public Servants: How Union Membership Influences Public Service Motivation. *The American Review of Public Administration*, *41*(6), 705–723.

Delfgaauw, J., & Dur, R. (2008). Incentives and Workers' Motivation in the Public Sector. *The Economic Journal*, *118*(525), 171–191.

Ed-Data. (2021). *Negotiating Teachers' Contracts in California*. EdData. https://www.ed-data.org/article/Negotiating-Teachers'-Contracts-in-California

Farber, H. S. (1986). Chapter 18: The analysis of union behavior. In O. Ashenfelter & R. Layard (Eds.), *Handbook of Labor Economics* (Vol. 2, pp. 1039–1089). Elsevier.

Fitzpatrick, M. D. (2015). How Much Are Public School Teachers Willing to Pay for Their Retirement Benefits? *American Economic Journal: Economic Policy*, *7*(4), 165–188.

Fowler, L., & Birdsall, C. (2020). Are the Best and Brightest Joining the Public Service? *Review of Public Personnel Administration*, *40*(3), 532–554.

Glaeser, E. L., & Ponzetto, G. A. M. (2014). Shrouded costs of government: The political economy of state and local public pensions. *Journal of Public Economics*, *116*, 89–105.

Grissom, J. A., & Strunk, K. O. (2012). How Should School Districts Shape Teacher Salary Schedules? Linking School Performance to Pay Structure in Traditional Compensation Schemes. *Educational Policy*, *26*(5), 663–695.

Groshen, E. L., & Krueger, A. B. (1990). The Structure of Supervision and Pay in Hospitals. *ILR Review*, *43*(3), 134-S.

Gustman, A. L., & Segal, M. (1977). Teachers' Salary Structures—Some Analytical and

Empirical Aspects of the Impact of Collective Bargaining. *Proceedings of the Thirtieth Annual Winter Meeting of the Industrial Relations Research Association*, 437–445.

Hammer, T. H., & Avgar, A. (2005). The impact of unions on job satisfaction, organizational commitment, and turnover. *Journal of Labor Research*, *26*(2), 241–266.

Hek, P. de, & Vuuren, D. van. (2011). Are older workers overpaid? A literature review. *International Tax and Public Finance*, *18*(4), 436–460.

Hendricks, M. D. (2014). Does it pay to pay teachers more? Evidence from Texas. *Journal of Public Economics*, *109*, 50–63.

Hendricks, M. D. (2015). Towards an optimal teacher salary schedule: Designing base salary to attract and retain effective teachers. *Economics of Education Review*, *47*, 143–167.

Heutel, G. (2009). Testing implications of a tournament model of school district salary schedules. *Economics of Education Review*, *28*(1), 143–151.

Hoang, T., & Goodman, D. (2018). Public Pensions and Collective Bargaining Rights: Evidence from State and Local Governments. *Public Administration Review*, *78*(5), 772–784.

Kelley, D. G. (2014). The political economy of unfunded public pension liabilities. *Public Choice*, *158*(1), 21–38.

Kellough, J. E., & Lu, H. (1993). The Paradox of Merit Pay in the Public Sector: Persistence of a Problematic Procedure. *Review of Public Personnel Administration*, *13*(2), 45–64.

Langbein, L., & Roberts, F. W. (2022). Money Matters: Sector Differences, Competition, and the Public Personnel System. *The American Review of Public Administration*, *52*(1), 61–86.

Lankford, H., & Wyckoff, J. (1997). The changing structure of teacher compensation, 1970–1994. *Economics of Education Review*, *16*(4), 371–384.

Lazear, E. P. (1981). Agency, Earnings Profiles, Productivity, and Hours Restrictions. *The*

American Economic Review, *71*(4), 606–620.

Llorens, J. J. (2008). Uncovering the Determinants of Competitive State Government Wages. *Review of Public Personnel Administration*, *28*(4), 308–326.

Marianno, B. D. (2015). Teachers' Unions on the Defensive?: How Recent Collective Bargaining Laws Reformed the Rights of Teachers. *Journal of School Choice*, *9*(4), 551–577.

Marianno, B. D., Bruno, P., & Strunk, K. O. (2021). The Effect of Teachers' Union Contracts on School District Efficiency: Longitudinal Evidence From California. *SAGE Open, 11*(1).

Marianno, B. D., & Strunk, K. O. (2018). The Bad End of the Bargain?: Revisiting the Relationship between Collective Bargaining Agreements and Student Achievement. *Economics of Education Review*, *65*, 93–106.

Miller, G. J., & Whitford, A. B. (2007). The Principal's Moral Hazard: Constraints on the Use of Incentives in Hierarchy. *Journal of Public Administration Research and Theory*, *17*(2), 213–233.

Mitchell, O. S., & Smith, R. S. (1994). Pension Funding in the Public Sector. *The Review of Economics and Statistics*, *76*(2), 278–290.

Moe, T. M. (2006). Political Control and the Power of the Agent. *Journal of Law, Economics, and Organization*, *22*(1), 1–29.

Nelson, S. (2004). The State of the Federal Civil Service Today: Aching for Reform. *Review of Public Personnel Administration*, *24*(3), 202–215.

Prendergast, C. (1999). The Provision of Incentives in Firms. *Journal of Economic Literature*, *37*(1), 7–63.

Salop, J., & Salop, S. (1976). Self-Selection and Turnover in the Labor Market. *The Quarterly Journal of Economics*, *90*(4), 619–627.

Strunk, K. O., Cowen, J., Goldhaber, D., Marianno, B. D., Kilbride, T., & Theobald, R. (2018). *Collective Bargaining and State-Level Reforms: Assessing Changes to the Restrictiveness of Collective Bargaining Agreements across Three States* (Working Paper No. 210-1218-1). National Center for Analysis of Longitudinal Data in Education Research.

Strunk, K. O., & Grissom, J. A. (2010). Do Strong Unions Shape District Policies? Collective Bargaining, Teacher Contract Restrictiveness, and the Political Power of Teachers' Unions. *Educational Evaluation and Policy Analysis*, *32*(3), 389–406.

Strunk, K. O., & Reardon, S. F. (2010). Measuring the Strength of Teachers' Unions: An Empirical Application of the Partial Independence Item Response Approach. *Journal of Educational and Behavioral Statistics*, *35*(6), 629–670.

Taylor, L. L., & Fowler, W. J. Jr. (2006). *A Comparable Wage Approach to Geographic Cost Adjustment* (NCES 2006-321). U.S. Department of Education, National Center for Education Statistics.

Thom, M. (2013). Politics, Fiscal Necessity, or Both? Factors Driving the Enactment of Defined Contribution Accounts for Public Employees. *Public Administration Review*, *73*(3), 480–489.

Thom, M. (2017). The Drivers of Public Sector Pension Reform Across the U.S. States. *The American Review of Public Administration*, *47*(4), 431–442.

U.S. Department of Education. (2018). *State Nonfiscal Survey of Public Elementary/Secondary Education, 2000-01 through 2015-16*. National Center for Education Statistics.

Vigdor, J. (2008). Scrap the Sacrosanct Salary Schedule. *Education Next*, *8*(4), 37–42.

West, K. L., & Mykerezi, E. (2011). Teachers' unions and compensation: The impact of collective bargaining on salary schedules and performance pay schemes. *Economics of*

Education Review, *30*(1), 99–108.

Williamson, O. E. (1981). The Economics of Organization: The Transaction Cost Approach. *American Journal of Sociology*, *87*(3), 548–577.

Winters, J. V. (2011). Teacher Salaries and Teacher Unions: A Spatial Econometric Approach. *Industrial & Labor Relations Review*, *64*(4), 747–764.

TABLES

Table 1 – Teacher Benefits as a Function of Collective Bargaining Agreement (CBA) Restrictiveness

	IHS of Maximum District Healthcare Contribution						Probability Retiree Benefits Offered			
	One-Party Plans			Family Plans						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
CBA Restrictiveness	0.040	0.229	0.441	0.038	0.221	0.413	0.030^{+}	-0.053	0.021	-0.045
	(0.122)	(0.217)	(0.399)	(0.122)	(0.220)	(0.399)	(0.016)	(0.054)	(0.020)	(0.052)
Median Prior Teacher Experience	-0.028	-0.029	-0.057	-0.040	-0.040	-0.071	0.006	0.007	0.003	0.004
	(0.040)	(0.040)	(0.067)	(0.040)	(0.041)	(0.067)	(0.006)	(0.006)	(0.008)	(0.008)
CBA Restrictiveness x Median Experience		-0.025	-0.053		-0.024	-0.050		0.009^{+}		0.007
		(0.024)	(0.043)		(0.024)	(0.044)		(0.005)		(0.005)
% Hispanic	0.020	0.020	0.057	0.023	0.023	0.067	-0.000	-0.000	-0.002	-0.002
	(0.020)	(0.020)	(0.051)	(0.020)	(0.020)	(0.046)	(0.001)	(0.001)	(0.002)	(0.002)
% Black	0.035	0.032	0.085	0.043	0.040	0.108	0.003	0.003	0.009	0.008
	(0.056)	(0.056)	(0.225)	(0.055)	(0.055)	(0.227)	(0.004)	(0.004)	(0.010)	(0.009)
% FRL	-0.001	-0.001	0.011	0.000	0.001	0.016	0.001	0.001	0.000	-0.000
	(0.010)	(0.010)	(0.018)	(0.010)	(0.010)	(0.019)	(0.001)	(0.001)	(0.002)	(0.002)
Enrollment (Natural Log)	0.004	-0.005	-1.888	-0.307	-0.306	-2.471	0.048*	0.046*	0.008	0.020
	(0.802)	(0.805)	(2.525)	(0.801)	(0.803)	(2.474)	(0.019)	(0.019)	(0.281)	(0.280)
=1 if Declining Enrollment	-0.028	-0.035	0.112	-0.052	-0.059	0.099	-0.063*	-0.064*	0.003	0.004
	(0.128)	(0.128)	(0.222)	(0.131)	(0.131)	(0.228)	(0.029)	(0.029)	(0.018)	(0.018)
Service Days for Returning Teachers	0.027	0.026	0.015	0.031	0.030	0.022	-0.008	-0.008	0.003	0.003
	(0.027)	(0.027)	(0.035)	(0.028)	(0.028)	(0.037)	(0.006)	(0.006)	(0.004)	(0.004)
District FEs	X	X	X	X	X	X			X	X
Linear District Trends			X			X				
Labor Market x Year FEs	X	X	X	X	X	X	X	X	X	X
Observations	966	966	966	974	974	974	875	875	778	778
Districts	282	282	282	285	285	285	486	486	389	389
Adj. R-sq.	0.72	0.72	0.75	0.74	0.74	0.77	0.09	0.09	0.82	0.82

Note. Standard errors clustered on districts in parentheses. All predictors are lagged by one year except service days and CBA restrictiveness. IHS = Inverse Hyperbolic Sine. FEs = Fixed Effects.
+ p<.1, * p<.05, ** p<.01, *** p<.001

FIGURES

Figure 1 – Difference in Log Salary Associated with +1 SD in CBA Restrictiveness

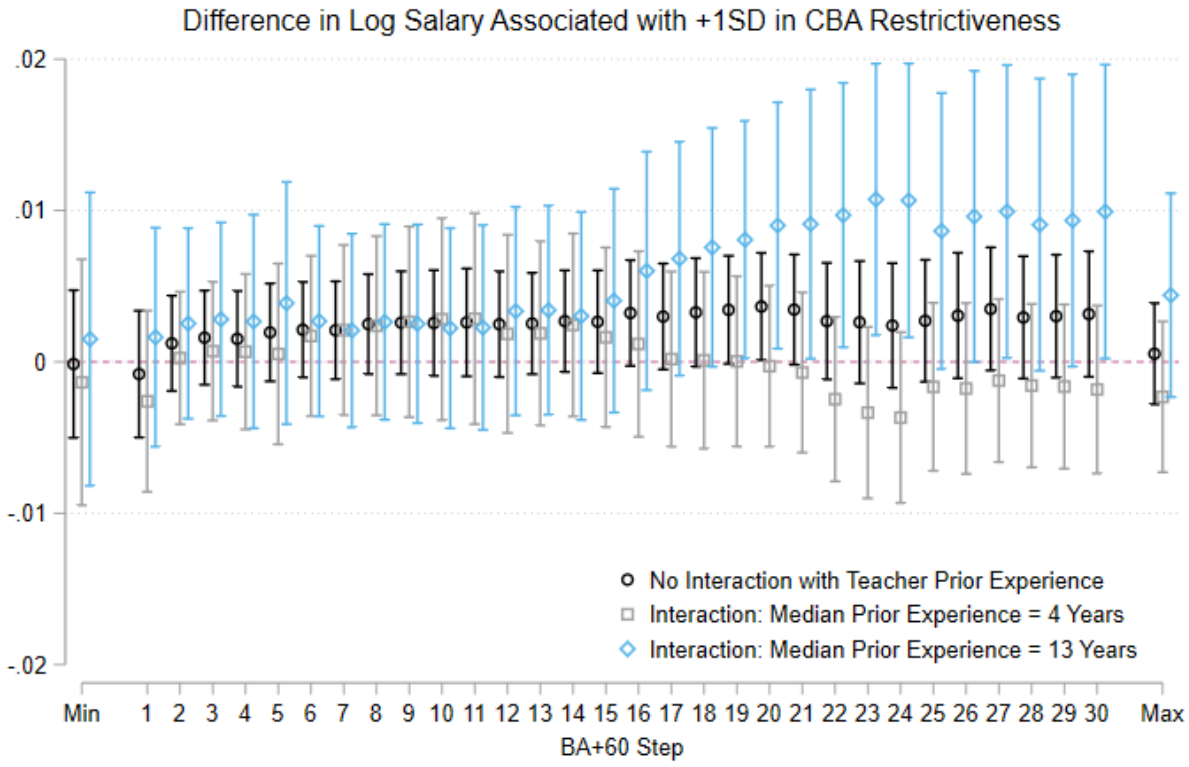

Figure 1. Difference in natural log of salary associated with a one standard deviation increase in collective bargaining agreement (CBA) restrictiveness. Black markers come from models without interactions between CBA restrictiveness and median teacher experience. Gray and blue estimates are separate average marginal effects of CBA restrictiveness at different levels of median teacher experience from single models interacting CBA restrictiveness with median teacher experience. All estimates include 1,726 observations of 495 districts, each observed at least twice, and are accompanied by 95% confidence intervals based on standard errors clustered on districts.

Figure 2 – Difference in Log Salary Associated with +1SD in Monitoring Intensity

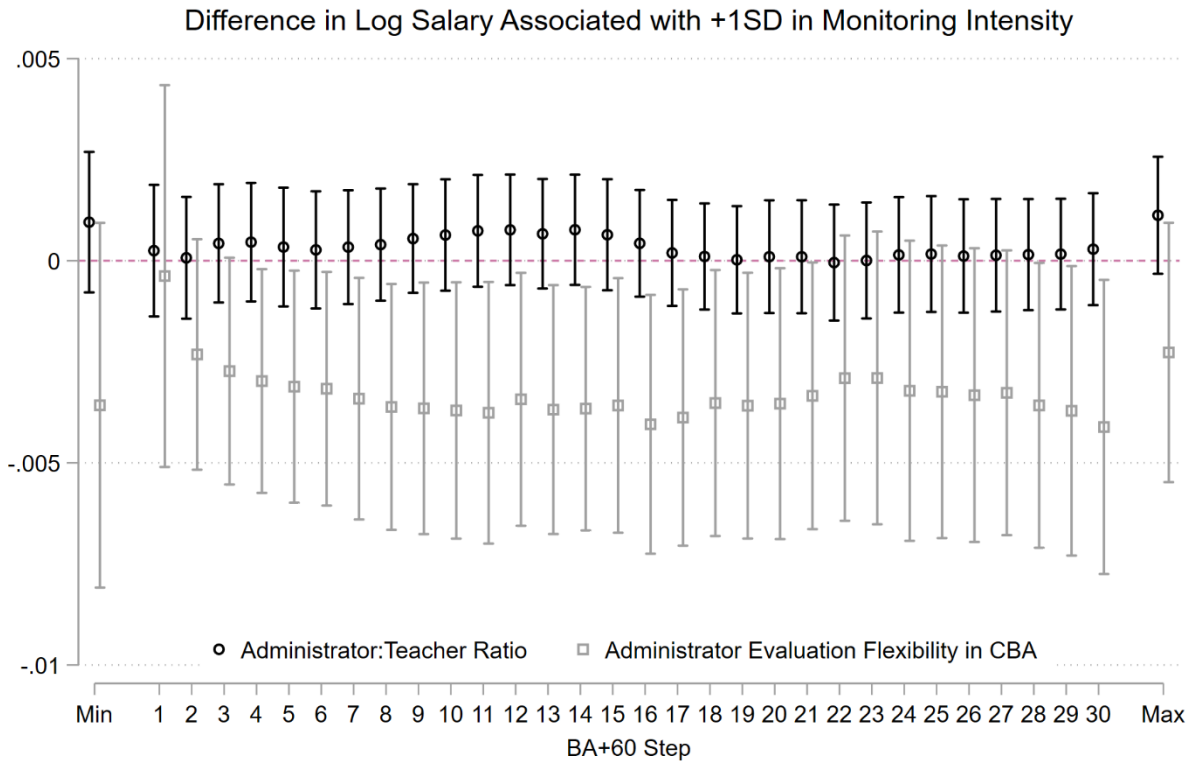

Figure 2. Difference in natural log of salary associated with a one standard deviation increase in monitoring intensity. Black markers come from models where monitoring intensity is proxied by the administrator/teacher ratio (9,489 observations of 850 districts). Gray markers are from models where the proxy for monitoring intensity is administrator flexibility in evaluating teaches as captured in the collective bargaining agreement (CBA; 1,686 observations of 489 districts). Estimates include 95% confidence intervals based on standard errors clustered on districts.

APPENDIX A: INFORMATIONAL TABLES

Table A1 – CBA Items Used to Estimate Overall Restrictiveness

Association Rights

1. Association members or presidents are promised leave.
2. The contract specifies an amount of release time for the association per year.
3. The contract specifies who pays for general association release time.
4. The association president (or designee) gets additional leave time.
5. The contract specifies who pays for the association president's leave.
6. The contract specifies the total number of days of release time the association president receives per year.
7. $\geq$ 10 days
8. $\geq$ 20 days
9. $\geq$ 40 days
10. The association president receives full-time leave.

Compensation

11. Members receive a bonus for having a PhD/EdD.

Class Size

12. The contract addresses class size.
13. The contract specifies a particular class size.
14. The district must balance class sizes within a specific period of time.
15. The district must balance class sizes within three weeks of the stat of the year or semester.
16. The district must take action if the class size is exceeded.
17. The district must take action by a specific time if class size is exceeded.
18. Class size actions be taken within three weeks
19. Specific actions must be taken if class size is exceeded.

Evaluation

20. Permanent members to use an alternative evaluation process with satisfactory prior performance.
21. Permanent members can use an alternative evaluation process.

Grievances

22. The board does not make final/binding decisions on grievances.
23. Grievances do not go to the board.
24. Grievances can go to arbitration.
25. Arbitration is the final stage in the grievance process.
26. Grievance arbitration is binding.

Non-Teaching Duties

27. There are restrictions on the length and/or number of faculty meetings.
28. There are time constraints on faculty meetings.
29. There are constraints in the number of faculty meetings.

Transfers and Vacancies

30. Seniority is addressed as a factor in who is voluntarily transferred.
31. Seniority is a factor in who is voluntarily transferred at least when all else is equal.
32. There are limits on the frequency with which members may be involuntarily transferred.
33. The CBA outlines specific causes for which a member may be involuntarily transferred.

School Days and Hours

34. The CBA specifies the length of the school day in instructional minutes.

Table A2 – Items Used in Evaluation Subarea Contract Restrictiveness Measure

1. CBA specifies that probationary/non-tenured teachers must have a pre-observation meeting with their evaluator
2. CBA requires probationary teachers to have advance notice for any of their formal evaluation observations
3. CBA requires greater than 2 days' notice
4. CBA requires greater than 1 weeks' notice or agree upon in advance
5. If they get advance notice, there is NO clause permitting additional *unannounced* observations
6. CBA specifies the length of formal observations
7. CBA specifies a time limit within post-observation meetings must occur after observations
8. CBA specifies that post-observation meetings must occur within 5 days
9. CBA specifies how many formal observations the district can have for tenured faculty
10. CBA specifies that district can have greater than one observation of tenured faculty
11. CBA specifies that district can have greater than two observations of tenured faculty
12. CBA allows for Education Code standard evaluation period for NCLB highly qualified teachers of every 5 years, and no more frequently
13. CBA allows permanent/tenured members in the district to use an alternative evaluation process for satisfactory evaluation and performance
14. CBA allows permanent/tenured members in the district to use an alternative evaluation process
15. CBA specifies how many formal observations the district can have for probationary/non-tenured faculty
16. CBA outlines a minimum number of observations that must occur before a member receives an unsatisfactory evaluation
17. CBA puts a time limit on a teacher's reply rights
18. Reply time limit is greater than or equal to 10 days
19. Reply time limit is greater than or equal to 15 days
20. CBA does not restrict teachers with negative evaluation from gaining a salary step in the following year
21. CBA specifies that negative evaluations can be removed from personnel files

Table A3 – Summary Statistics

	N	Mean	SD	Min	Max
=1 if Elementary District	10750	0.50	0.50	0	1
=1 if High School District	10750	0.09	0.29	0	1
=1 if Unified District	10750	0.40	0.49	0	1
% Hispanic	10750	43.41	28.26	0	100
% Black	10750	3.30	5.34	0	76.19
% FRL	10722	52.86	26.02	0	100
Enrollment	10750	7547.91	24382.42	5	717176
=1 if Declining Enrollment	10748	0.56	0.50	0	1
Service Days for Returning Teachers	10750	183.88	2.17	153	198
Median Prior Teacher Experience in District	10747	9.60	3.55	1	33
Overall Contract Restrictiveness	1777	0.01	0.99	-3.45	2.80
Evaluation Restrictiveness	1777	-0.00	1.00	-2.80	3.26
Administrators per 100 Teachers	10748	8.45	4.15	0	100
Minimum Salary	10750	46.78	5.44	24.82	84.54
Maximum Salary	10750	92.73	12.57	48.36	159.74
BA+60 Salaries					
Step 1	10750	53.57	6.42	32.62	116.59
Step 2	10750	55.45	6.79	34.33	116.86
Step 3	10750	57.53	7.17	36.04	119.25
Step 4	10750	59.68	7.62	37.75	121.92
Step 5	10750	61.86	8.04	38.99	121.92
Step 6	10750	64.06	8.46	41.18	121.92
Step 7	10750	66.28	8.87	41.80	121.92
Step 8	10750	68.52	9.32	41.80	126.76
Step 9	10750	70.80	9.77	41.80	131.77
Step 10	10750	73.13	10.29	41.80	136.62
Step 11	10750	75.27	10.77	41.80	141.56
Step 12	10750	77.18	11.15	41.80	146.49
Step 13	10750	78.46	11.29	41.80	146.49
Step 14	10750	79.38	11.35	41.80	146.49
Step 15	10750	80.33	11.54	41.80	146.49
Step 16	10750	81.05	11.67	41.80	146.49
Step 17	10750	81.52	11.80	41.80	149.19
Step 18	10750	82.10	11.95	41.80	149.19
Step 19	10750	82.50	12.05	41.80	149.19
Step 20	10750	83.19	12.28	41.80	149.19
Step 21	10750	83.68	12.52	41.80	151.90
Step 22	10750	83.97	12.61	41.80	151.90
Step 23	10750	84.22	12.76	41.80	151.90
Step 24	10750	84.53	12.93	41.80	151.90
Step 25	10750	85.09	13.21	41.80	151.90
Step 26	10750	85.34	13.36	41.80	151.90
Step 27	10750	85.52	13.50	41.80	151.90
Step 28	10750	85.66	13.56	41.80	151.90
Step 29	10750	85.72	13.60	41.80	151.90
Step 30	10750	86.01	13.75	41.80	151.90
Maximum District Health & Welfare Benefit Contribution					
One-Party Plan	5929	7.36	2.81	0	20.38
Family Plan	5876	12.40	6.28	0	43.02
=1 if Retiree Benefits Offered	5686	0.74	0.44	0	1

Note. This table combines annual observations of 910 districts from 2003-2004 through 2018-2019. Salaries and benefit contributions are in thousands of 2018-2019 dollars.

Online Supplementary Materials

APPENDIX B: SUPPLEMENTAL ANALYSES

CBA Renegotiation Frequency

Because my analyses rely on within-district comparisons over time, one possible concern is that CBAs will tend to be monotonically increasing in length and restrictiveness over time as they are renegotiated (e.g., Fuller & Mitchell, 2006), and that this is a somewhat distinct phenomenon from the union exercising greater influence. I suspect that this is less of a concern in my context than it might be in other contexts for three reasons. First, my use of district fixed effects means I am effectively comparing districts to themselves over time. For example, the average rate at which districts renegotiate their CBAs, even if unobserved, would be accounted for by the district fixed effects. Second, to the extent that CBA renegotiation is a mechanism by which teachers' unions exercise influence, more frequent renegotiation within the district may reflect genuine union influence that I would not want to "control away". Third, and perhaps most importantly, under California law CBAs are supposed to be renegotiated at least every three years. Districts and their labor partners may choose to renegotiate more frequently than that, and negotiations may drag on too long or otherwise be delayed, but in practice this state requirement means that renegotiation cycles are substantially standardized across the state.

Nevertheless, CBA renegotiation rates are not perfectly standardized. This may mean that my results are driven disproportionately by districts that renegotiate CBAs more frequently (thus generating a disproportionate amount of CBA variation). This could be particularly concerning if renegotiation frequency is driven by unobservable factors that are distinct from "union influence" but correlated with my compensation measures within districts over time (e.g., unexpected local economic shocks).

I cannot rule those possibilities out, but I take advantage of the fact that my CBA data include some information about the years spanned by the CBA (i.e., the year the CBA was first supposed to be effective and the year in which the CBA was officially supposed to expire). These data are imperfect in a variety of ways. These span dates are missing altogether for roughly one quarter of the observations used to estimate model 2 (i.e., for the first research question, focused on union influence), including for all observations from 2005-2006 (i.e., the first year CBAs were collected). The observed dates may also not perfectly reflect the true start and end dates of the CBA (e.g., if the contract was reopened early or renegotiation was delayed), and I do not observe CBAs at all that may have been bargained in between data collections. Still, the dates allow me to at least roughly approximate the rate at which districts are renegotiating contracts.

Table B1 and figure B1 present results like those in table 1 and figure 1, except that I exclude districts for which the mean CBA span dates I observe are two years or less or five years or more. This corresponds to roughly the top and bottom decile of mean CBA spans. (There are no districts for which I observe no CBA spans in any year.) Thus, I include in these models only districts that appear to renegotiate their CBAs at a relatively "normal" rate on average (i.e., less frequently than every two years but more frequently than every five years). Despite substantially reducing my estimation sample (i.e., by roughly a quarter), results are essentially unchanged.

The Relationship Between Monitoring Intensity and Health Benefits

Table B2 presents results referenced in the main manuscript where my proxies for monitoring intensity are used to predict health benefits. There is no significant relationship between administrator:teacher ratios and employer healthcare contributions for either one party plans or family plans (columns 1, 2, 4, and 5). The only estimate consistent with the monitoring intensity

hypothesis is found in column 7: a SD increase in the administrator:teacher ratio is associated with a roughly 3 percentage point lower probability that a district offers retiree benefits. Yet even this estimate is highly sensitive to controlling for unobserved heterogeneity between districts (column 8). And since I observe these ratios for far more districts and in more years than I do for CBAs, this sensitivity is not simply driven by minimal within-district variation; these models include 92 districts with at least some variation in their retiree benefit offerings. And in no case do I find a statistically significant relationship between administrators' evaluation flexibility and the provision of health benefits, either for active employees (columns 3 and 6) or for retirees (columns 10 and 11).

Ideology and Achievement Controls

Despite the fact that union influence and teacher compensation have both been shown to be related to political ideology in the local community (e.g., Babcock & Engberg, 1999) and to student outcomes (e.g., Cowen & Strunk, 2015), the models presented as my primary specifications do not include as controls variables related to either ideology or school outputs. This is to avoid controlling away the impacts of unions on compensation. For example, if more left-wing electorates increase support for unions and those more powerful unions are in turn able to bargain for higher compensation levels, the effect of unions on compensation will be obscured by controlling for political ideology in the community. Similarly, to the extent that union power impacts both student achievement and teacher compensation, controlling for achievement will obscure the effects of unions on compensation.

Nevertheless, it is possible that political ideology and student outcomes may have impacts on compensation *over and above* their relationships to union influence (or how teachers are monitored). To assess this possibility, I estimate models like those described in the main text but

that additionally control for measures of local political ideology and student achievement. As a measure of political ideology, I include Democratic party vote share in the most recent U.S. House of Representatives election. I accomplish this by linking Congressional district information on districts from the Common Core of Data to election results from the MIT Election Lab (MIT Election Data and Science Lab, 2017). Though Congressional district boundaries do not perfectly align with school district attendance boundaries, this provides at least a rough proxy of the local political climate.

As two measures of student outcomes, I include student proficiency rates on state-administered standardized tests in (i) English language arts (ELA) and (ii) mathematics, using data publicly available from the California Department of Education. Like my political ideology measure, these measures are imperfect, both because student test scores are an incomplete measure of students' educational outcomes and because during the time under consideration California made substantial changes to its statewide standardized testing regime. Nevertheless, student proficiency rates are an outcome that is commonly of interest and they can provide a reasonable sense of students' educational outcomes and advantages.

Results are shown in tables B3 and B4 and figures B2 and B3. Results are essentially unchanged in these models, even when the new predictors are themselves statistically significant.

Monitoring and Compensation at Different Levels of Overall CBA Restrictiveness

One premise of testing for substitution between deferred compensation and monitoring is that administrators have autonomy to make such substitutions in the first place. As noted in the main text, school districts in California enjoy substantial autonomy over compensation, with relatively few external (e.g., state) constraints. However, compensation is within the scope of local collective bargaining processes, and so is not unilaterally determined by administrators. Thus,

administrators may lack the ability to strategically defer compensation if unions dominate the collective bargaining process. Such a lack of administrator autonomy could explain my null results when answering my second research question. If so, then the results should be substantively and significantly different when administrators have more autonomy in setting compensation. I cannot definitively identify the presence of such autonomy, but as a simple test I take advantage of the fact that I am already using a proxy for overall union influence to answer my first research question: overall CBA restrictiveness. Table B5 and figure B4 present results where my proxies for monitoring intensity are interacted with overall CBA restrictiveness. These results provide little reason to think that overall CBA restrictiveness – and thus union influence – moderates to a meaningful degree the relationships between monitoring and compensation: coefficients on interaction terms are small and statistically insignificant, and so estimated average marginal effects are quite similar regardless of whether unions' influence in bargaining is relatively high or relatively low.

Varying Levels of Stakes for Monitoring

One explanation for why backloaded compensation does not appear to serve as a substitute for monitoring intensity is that teachers are unlikely to face serious consequences even if their performance is deemed to be lacking (Ballou & Podgursky, 2002). One implication of this is that in circumstances where teachers face more accountability for their evaluated performance, the monitoring/backloading trade off could be more salient for administrators. If so, the monitoring/backloading trade off could be more salient in schools serving lower grade levels because that is where (due to less teacher subject specialization) administrators are most comfortable evaluating teachers (Donaldson, 2013), and incentives to terminate teachers may be greatest due to school accountability pressure (Grissom, Kalogrides, & Loeb, 2017; Lavigne,

2020) and a greater supply of replacement teachers from which to hire (Donaldson, 2013; Goldhaber, et al., 2018). To test for this possibility, I estimate models that interact my proxies for monitoring intensity with indicators of whether the district serves primarily high school grades (i.e., 9-12) or is unified (i.e., K-12), with districts serving elementary grades (i.e., K-8) as the comparison group. This allows relationships between monitoring intensity and compensation to vary across district type. However, as shown in figure B5 and table B6, these results do not provide evidence of a monitoring/backloading substitution by administrators, even in elementary school districts.

REFERENCES

Babcock, L., & Engberg, J. (1999). Bargaining Unit Composition and the Returns to Education and Tenure. *ILR Review*, *52*(2), 163–178.

Ballou, D., & Podgursky, M. (2002). Returns to Seniority among Public School Teachers. *The Journal of Human Resources*, *37*(4), 892–912.

Cowen, J. M., & Strunk, K. O. (2015). The impact of teachers' unions on educational outcomes: What we know and what we need to learn. *Economics of Education Review*, *48*, 208–223.

Donaldson, M. L. (2013). Principals' Approaches to Cultivating Teacher Effectiveness: Constraints and Opportunities in Hiring, Assigning, Evaluating, and Developing Teachers. *Educational Administration Quarterly*, *49*(5), 838–882.

Fuller, H., & Mitchell, G. (2006, June 22). A Culture of Complaint. *Education Next*, *6*(3), 18–22.

Goldhaber, D., Strunk, K. O., Brown, N., Chambers, A., Naito, N., & Wolff, M. (2018). *Teacher Staffing Challenges in California: Exploring the Factors That Influence Teacher Staffing and Distribution* (Getting Down to Facts II) [Technical Report]. Policy Analysis for California Education. https://eric.ed.gov/?id=ED594738

Grissom, J. A., Kalogrides, D., & Loeb, S. (2017). Strategic Staffing? How Performance Pressures Affect the Distribution of Teachers Within Schools and Resulting Student Achievement. *American Educational Research Journal*, *54*(6), 1079–1116.

Lavigne, A. L. (2020). Examining individual- and school-level predictors of principal adaptation to teacher evaluation reform in the United States: A two-year perspective. *Educational Management Administration & Leadership*, *48*(2), 379–395.

MIT Election Data and Science Lab. (2017). *U.S. House 1976–2020* (MIT Election Data and Science Lab, Ed.; V11 ed.). Harvard Dataverse.

TABLES

Table B1 – Teacher Benefits as a Function of Collective Bargaining Agreement (CBA) Restrictiveness, Excluding High- and Low-Frequency Renegotiation Districts

	IHS of Maximum District Healthcare Contribution						Probability Retiree Benefits Offered			
	One-Party Plans			Family Plans						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
CBA Restrictiveness	-0.053	0.114	0.181	-0.055	0.093	0.163	0.034^{+}	-0.071	0.028	-0.045
	(0.134)	(0.250)	(0.375)	(0.134)	(0.252)	(0.380)	(0.018)	(0.055)	(0.026)	(0.061)
Median Prior Teacher Experience	-0.043	-0.044	-0.072	-0.066	-0.066	-0.090	0.005	0.005	0.003	0.003
	(0.052)	(0.052)	(0.064)	(0.053)	(0.053)	(0.065)	(0.007)	(0.007)	(0.010)	(0.010)
CBA Restrictiveness x Median Experience		-0.022	-0.033		-0.020	-0.033		0.011*		0.008
		(0.031)	(0.051)		(0.031)	(0.052)		(0.005)		(0.006)
% Hispanic	0.025	0.026	0.094	0.026	0.027	0.090	0.001	0.001	-0.001	-0.001
	(0.035)	(0.035)	(0.069)	(0.035)	(0.036)	(0.070)	(0.002)	(0.002)	(0.002)	(0.002)
% Black	0.083	0.084	0.202	0.094	0.094	0.224	0.006^{+}	0.006^{+}	0.013	0.012
	(0.067)	(0.067)	(0.192)	(0.065)	(0.065)	(0.193)	(0.004)	(0.004)	(0.011)	(0.011)
% FRL	-0.012	-0.012	0.001	-0.011	-0.011	0.006	0.000	0.000	-0.003	-0.003
	(0.008)	(0.008)	(0.016)	(0.008)	(0.008)	(0.017)	(0.002)	(0.002)	(0.002)	(0.002)
Enrollment (Natural Log)	-0.320	-0.347	-0.437	-0.825	-0.838	-1.062	0.043*	0.040^{+}	0.031	0.033
	(0.918)	(0.933)	(2.517)	(0.906)	(0.917)	(2.431)	(0.022)	(0.022)	(0.277)	(0.281)
=1 if Declining Enrollment	0.020	0.012	0.314	-0.002	-0.009	0.303	-0.073*	-0.076*	0.003	0.004
	(0.137)	(0.137)	(0.212)	(0.142)	(0.142)	(0.221)	(0.032)	(0.032)	(0.022)	(0.022)
Service Days for Returning Teachers	0.050	0.050	0.043	0.050	0.050	0.047	-0.004	-0.004	0.005	0.005
	(0.033)	(0.033)	(0.045)	(0.036)	(0.036)	(0.048)	(0.007)	(0.006)	(0.005)	(0.005)
District FEs	X	X	X	X	X	X			X	X
Linear District Trends			X			X				
Labor Market x Year FEs	X	X	X	X	X	X	X	X	X	X
Observations	758	758	758	760	760	760	693	693	618	618
Districts	221	221	221	222	222	222	384	384	309	309
Adj. R-sq.	0.73	0.73	0.77	0.75	0.75	0.79	0.06	0.06	0.79	0.79

Note. Standard errors clustered on districts in parentheses. All predictors are lagged by one year except service days and CBA restrictiveness. These models exclude districts with mean observed CBA spans of 2 years or less or 5 years or more, roughly the top and bottom deciles. IHS = Inverse Hyperbolic Sine. FEs = Fixed Effects.
+ p<.1, * p<.05, ** p<.01, *** p<.001

Table B2 – Teacher Benefits as a Function of Monitoring Intensity

	IHS of Maximum District Healthcare Contribution						Probability Retiree Benefits Offered				
	One-Party Plans			Family Plans							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Administrator to Teacher Ratio (Standardized)	-0.044 (0.050)	-0.038 (0.063)		-0.077 (0.052)	-0.030 (0.080)		-0.029** (0.011)	0.006 (0.005)	0.004 (0.006)		
Administrator Evaluation Flexibility (Standardized)			0.220 (0.174)			0.228 (0.188)				0.011 (0.015)	-0.022 (0.015)
Time-Varying District Controls	X	X	X	X	X	X	X	X	X	X	X
District FEs	X	X	X	X	X	X		X	X		X
Labor Market x Year FEs	X	X	X	X	X	X	X	X	X	X	X
Linear District Trends	X		X	X		X			X		
Cubic District Trends		X			X						
Observations	5223	5223	966	5161	5161	974	5401	5375	5375	875	778
Districts	531	531	282	511	511	285	816	790	790	486	389
Adj. R-sq.	0.74	0.60	0.75	0.76	0.64	0.77	0.23	0.87	0.91	0.08	0.82

Note. Standard errors clustered on districts in parentheses. All predictors are lagged by one year except service days and CBA restrictiveness. IHS = Inverse Hyperbolic Sine. FEs = Fixed Effects.
+ p<.1, * p<.05, ** p<.01, *** p<.001

Table B3 – Teacher Benefits as a Function of Collective Bargaining Agreement (CBA) Restrictiveness, Controlling for Democratic Vote Share and Student Proficiency

	IHS of Maximum District Healthcare Contribution						Probability Retiree Benefits Offered			
	One-Party Plans			Family Plans						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
CBA Restrictiveness	0.048	0.272	0.493	0.047	0.265	0.462	0.024	-0.053	0.023	-0.046
	(0.123)	(0.218)	(0.420)	(0.123)	(0.220)	(0.417)	(0.016)	(0.054)	(0.020)	(0.052)
Median Prior Teacher Experience	-0.019	-0.020	-0.042	-0.029	-0.030	-0.054	0.005	0.006	0.003	0.003
	(0.039)	(0.039)	(0.072)	(0.039)	(0.039)	(0.072)	(0.006)	(0.006)	(0.008)	(0.008)
CBA Restrictiveness x Median Experience		-0.029	-0.063		-0.029	-0.060		0.008		0.007
		(0.024)	(0.048)		(0.024)	(0.048)		(0.005)		(0.005)
% Hispanic	0.017	0.017	0.051	0.020	0.020	0.061	-0.001	-0.001	-0.002	-0.002
	(0.021)	(0.021)	(0.047)	(0.021)	(0.021)	(0.042)	(0.001)	(0.001)	(0.002)	(0.002)
% Black	0.029	0.025	0.063	0.035	0.031	0.086	0.002	0.002	0.009	0.009
	(0.056)	(0.056)	(0.227)	(0.055)	(0.055)	(0.227)	(0.004)	(0.004)	(0.009)	(0.009)
% FRL	-0.000	0.000	0.008	0.001	0.002	0.013	-0.001	-0.001	0.000	-0.000
	(0.011)	(0.011)	(0.019)	(0.011)	(0.011)	(0.019)	(0.002)	(0.002)	(0.002)	(0.002)
Enrollment (Natural Log)	-0.114	-0.119	-0.926	-0.386	-0.379	-1.509	0.047*	0.046*	0.009	0.023
	(0.801)	(0.804)	(2.428)	(0.801)	(0.804)	(2.370)	(0.019)	(0.019)	(0.274)	(0.272)
=1 if Declining Enrollment	-0.052	-0.060	0.116	-0.077	-0.085	0.102	-0.057+	-0.058*	0.002	0.003
	(0.129)	(0.129)	(0.224)	(0.132)	(0.132)	(0.231)	(0.029)	(0.029)	(0.019)	(0.019)
Service Days for Returning Teachers	0.027	0.025	0.012	0.030	0.028	0.020	-0.008	-0.007	0.003	0.004
	(0.027)	(0.027)	(0.036)	(0.029)	(0.029)	(0.038)	(0.006)	(0.006)	(0.004)	(0.004)
House Democratic Vote Share	-0.007	-0.008	-0.017	-0.008	-0.008	-0.019	0.002**	0.002**	0.000	0.000
	(0.007)	(0.007)	(0.014)	(0.007)	(0.007)	(0.015)	(0.001)	(0.001)	(0.001)	(0.001)
% Proficient in Math	0.037*	0.038*	0.032	0.039*	0.040**	0.031	0.002	0.002	-0.000	-0.000
	(0.015)	(0.015)	(0.027)	(0.015)	(0.015)	(0.027)	(0.002)	(0.002)	(0.002)	(0.002)
% Proficient in ELA	-0.038+	-0.040+	-0.054	-0.040+	-0.042*	-0.051	-0.005+	-0.005+	0.002	0.002
	(0.021)	(0.021)	(0.043)	(0.021)	(0.021)	(0.042)	(0.003)	(0.003)	(0.002)	(0.002)
District FEs	X	X	X	X	X	X			X	X
Linear District Trends			X			X				
Labor Market x Year FEs	X	X	X	X	X	X	X	X	X	X
Observations	965	965	965	973	973	973	874	874	778	778
Districts	282	282	282	285	285	285	485	485	389	389
Adj. R-sq.	0.72	0.72	0.75	0.75	0.75	0.77	0.10	0.10	0.82	0.82

Note. Standard errors clustered on districts in parentheses. Models are as described in table 2 except for the inclusion of three additional control variables. FEs = Fixed Effects.

+ p<.1, * p<.05, ** p<.01, *** p<.001

Table B4 – Teacher Benefits as a Function of Monitoring Intensity, Controlling for Democratic Vote Share and Student Proficiency

	IHS of Maximum District Healthcare Contribution						Probability Retiree Benefits Offered				
	One-Party Plans			Family Plans							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Administrator to Teacher Ratio	-0.028	-0.109		-0.025	-0.067		-0.030*	0.009	-0.000		
(Standardized)	(0.068)	(0.073)		(0.073)	(0.092)		(0.014)	(0.007)	(0.007)		
Administrator Evaluation			0.209			0.216				0.017	-0.022
Flexibility (Standardized)			(0.173)			(0.186)				(0.015)	(0.015)
House Democratic	-0.003	-0.007	-0.016	-0.002	-0.005	-0.018	0.002*	0.000	0.001	0.003**	0.000
Vote Share	(0.004)	(0.005)	(0.014)	(0.004)	(0.004)	(0.014)	(0.001)	(0.000)	(0.000)	(0.001)	(0.001)
% Proficient or	-0.005	0.002	0.032	-0.007	-0.002	0.031	0.002	-0.001	-0.001	0.003	-0.000
Advanced in Math	(0.006)	(0.007)	(0.027)	(0.005)	(0.008)	(0.027)	(0.002)	(0.001)	(0.001)	(0.002)	(0.002)
% Proficient or	0.007	-0.000	-0.050	0.005	0.001	-0.049	-0.003^{+}	0.001	0.001	-0.007*	0.002
Advanced in ELA	(0.008)	(0.009)	(0.041)	(0.009)	(0.011)	(0.041)	(0.002)	(0.001)	(0.001)	(0.003)	(0.002)
Time-Varying District Controls	X	X	X	X	X	X	X	X	X	X	X
District FEs	X	X	X	X	X	X		X	X		X
Labor Market x Year FEs	X	X	X	X	X	X	X	X	X	X	X
Linear District Trends	X		X	X		X			X		
Cubic District Trends		X			X						
Observations	4356	4356	965	4322	4322	973	4001	3965	3965	874	778
Districts	501	501	282	486	486	285	788	752	752	485	389
Adj. R-sq.	0.75	0.49	0.75	0.78	0.56	0.78	0.22	0.87	0.91	0.10	0.82

Note. Standard errors clustered on districts in parentheses. Models are as described in table 2 except for the inclusion of three additional control variables. IHS = Inverse Hyperbolic Sine. FEs = Fixed Effects.
+ p<.1, * p<.05, ** p<.01, *** p<.001

Table B5 – Teacher Benefits as a Function of Monitoring Intensity at Varying Levels of Overall CBA Restrictiveness

	IHS of Maximum District Healthcare Contribution				Probability Retiree Benefits Offered			
	One-Party Plans		Family Plans					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Administrator to Teacher Ratio	-0.084		-0.066		0.010	-0.062^{+}		
(Standardized)	(0.272)		(0.271)		(0.036)	(0.032)		
Administrator to Teacher Ratio	0.014		-0.034		0.001	0.009		
x CBA Restrictiveness	(0.229)		(0.238)		(0.034)	(0.026)		
Administrator Evaluation		0.187		0.190			0.017	-0.021
Flexibility (Standardized)		(0.131)		(0.143)			(0.015)	(0.015)
Evaluation Flexibility		0.238		0.243			-0.024^{+}	-0.015
x CBA Restrictiveness		(0.149)		(0.152)			(0.013)	(0.014)
CBA Restrictiveness	0.041	0.121	0.028	0.118	0.030^{+}	0.021	0.035^{*}	0.016
	(0.210)	(0.181)	(0.215)	(0.183)	(0.017)	(0.020)	(0.016)	(0.018)
Time-Varying District Controls	X	X	X	X	X	X	X	X
District FEs	X	X	X	X		X		X
Labor Market x Year FEs	X	X	X	X	X	X	X	X
Linear District Trends	X	X	X	X				
Observations	966	966	974	974	873	778	875	778
Districts	282	282	285	285	484	389	486	389
Adj. R-sq.	0.75	0.75	0.77	0.78	0.09	0.82	0.09	0.82

Note. Standard errors clustered on districts in parentheses. Models are as described in figure 2 except that monitoring proxies are interacted with overall CBA restrictiveness. All predictors are lagged by one year except service days and CBA restrictiveness. IHS = Inverse Hyperbolic Sine. FEs = Fixed Effects.
+ p<.1, * p<.05, ** p<.01, *** p<.001

Table B6 – Teacher Benefits as a Function of Monitoring Intensity by District Grade Level

	IHS of Maximum District Healthcare Contribution						Probability Retiree Benefits Offered				
	One-Party Plans			Family Plans							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Administrator to Teacher Ratio (Standardized)	0.022	0.008		-0.019	0.004		-0.026*	0.012+	0.004		
	(0.062)	(0.077)		(0.053)	(0.083)		(0.013)	(0.006)	(0.006)		
Administrator Evaluation Flexibility (Standardized)			0.256			0.284				-0.006	-0.005
			(0.287)			(0.327)				(0.022)	(0.018)
x Unified	-0.180	-0.187	-0.047	-0.221	-0.264	-0.080	-0.004	-0.023+	-0.009	0.006	-0.027
	(0.147)	(0.160)	(0.376)	(0.164)	(0.193)	(0.412)	(0.024)	(0.013)	(0.012)	(0.030)	(0.033)
x High School	-0.136	0.265	-0.189	-0.101	0.650+	-0.244	-0.023	-0.003	0.036	0.163**	-0.054
	(0.234)	(0.234)	(0.350)	(0.236)	(0.390)	(0.385)	(0.091)	(0.022)	(0.030)	(0.057)	(0.056)
Unified							-0.042			-0.039	
							(0.034)			(0.041)	
High							-0.004			-0.080	
							(0.051)			(0.070)	
Time-Varying District Controls	X	X	X	X	X	X	X	X	X	X	X
District FEs	X	X	X	X	X	X		X	X		X
Labor Market x Year FEs	X	X	X	X	X	X	X	X	X	X	X
Linear District Trends	X		X	X		X			X		
Cubic District Trends		X			X						
Observations	5223	5223	966	5161	5161	974	5401	5375	5375	875	778
Districts	531	531	282	511	511	285	816	790	790	486	389
Adj. R-sq.	0.74	0.60	0.75	0.76	0.65	0.77	0.23	0.87	0.91	0.10	0.82

Note. Standard errors clustered on districts in parentheses. All predictors are lagged by one year except service days and CBA restrictiveness. IHS = Inverse Hyperbolic Sine. FEs = Fixed Effects.

+ p<.1, * p<.05, ** p<.01, *** p<.001

FIGURES

Figure B1 – Difference in Log Salary Associated with +1SD in CBA Restrictiveness, Excluding High- and Low-Frequency Renegotiation Districts

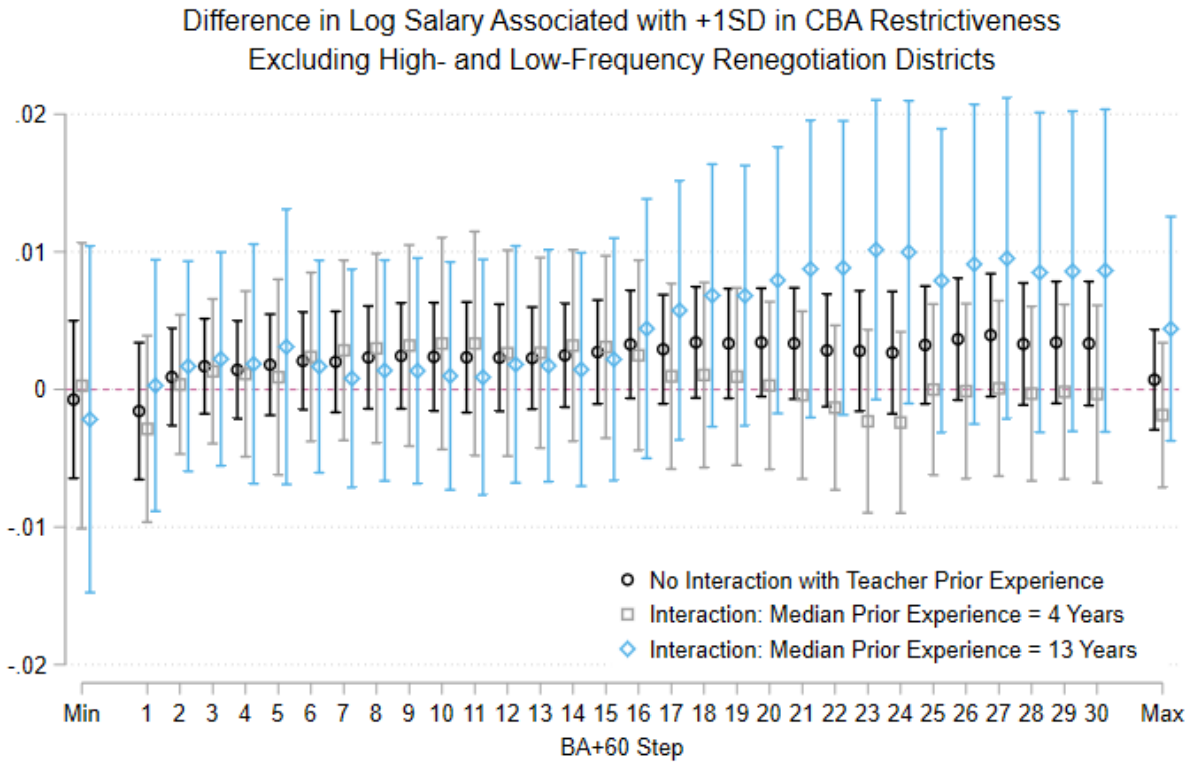

Figure B1. Difference in natural log of salary associated with a one standard deviation increase in collective bargaining agreement (CBA) restrictiveness excluding high- and low-frequency renegotiation districts. Black markers come from models without interactions between CBA restrictiveness and median teacher experience. Gray and blue estimates are separate average marginal effects of CBA restrictiveness at different levels of median teacher experience from single models interacting CBA restrictiveness with median teacher experience. Models are as described in figure 1 except that districts are excluded if their mean observed CBA spans two or fewer years or five or more years. All estimates include 1,327 observations of 384 districts, each observed at least twice, and are accompanied by 95% confidence intervals based on standard errors clustered on districts.

Figure B2 – Difference in Log Salary Associated with +1SD in CBA Restrictiveness, Controlling for Democratic Vote Share and Student Proficiency

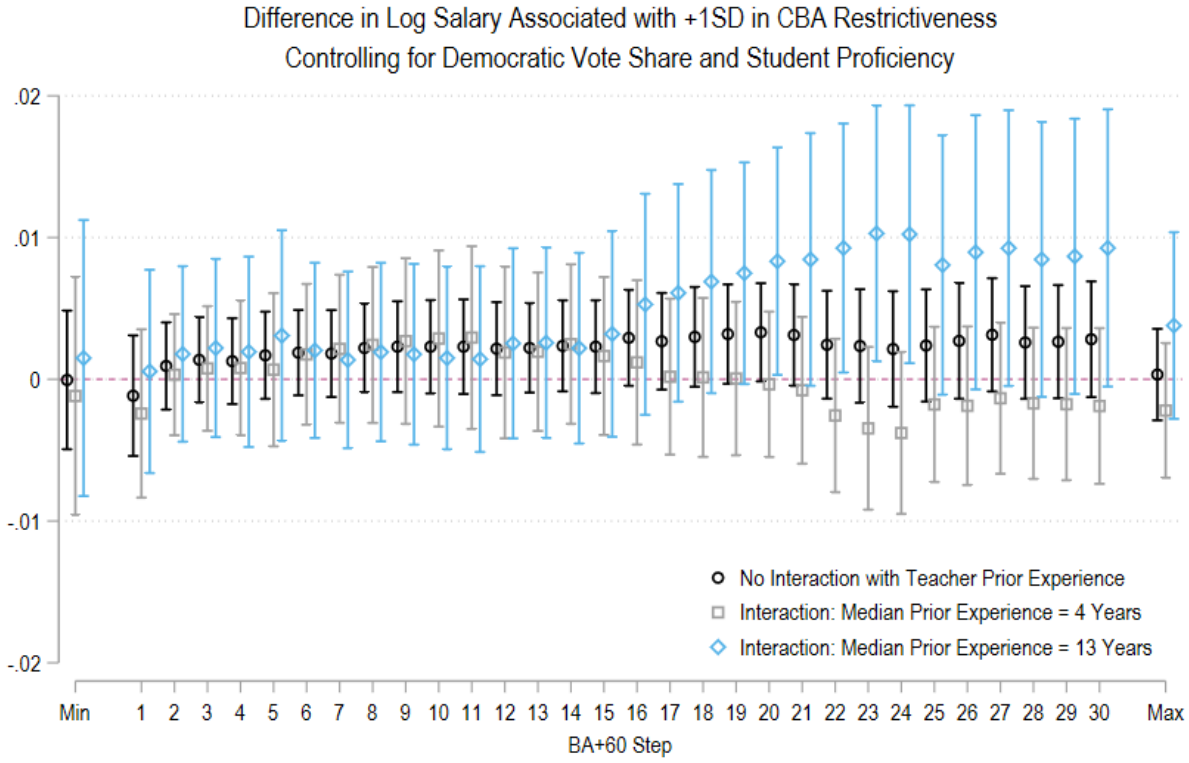

Figure B2. Difference in natural log of salary associated with a one standard deviation increase in collective bargaining agreement (CBA) restrictiveness controlling for Democratic vote share and student proficiency. Black markers come from models without interactions between CBA restrictiveness and median teacher experience. Gray and blue estimates are separate average marginal effects of CBA restrictiveness at different levels of median teacher experience from single models interacting CBA restrictiveness with median teacher experience. Models are as described in figure 1 except that they additionally control for Democratic party vote share in the Congressional district in the most recent House election as well as the percentages of students in the district who were proficient (or above) on statewide standardized tests in math and English language arts. All estimates include 1,685 observations of 489 districts, each observed at least twice, and are accompanied by 95% confidence intervals based on standard errors clustered on districts.

Figure B3 – Difference in Log Salary Associated with +1SD in Monitoring Intensity, Controlling for Democratic Vote Share and Student Proficiency

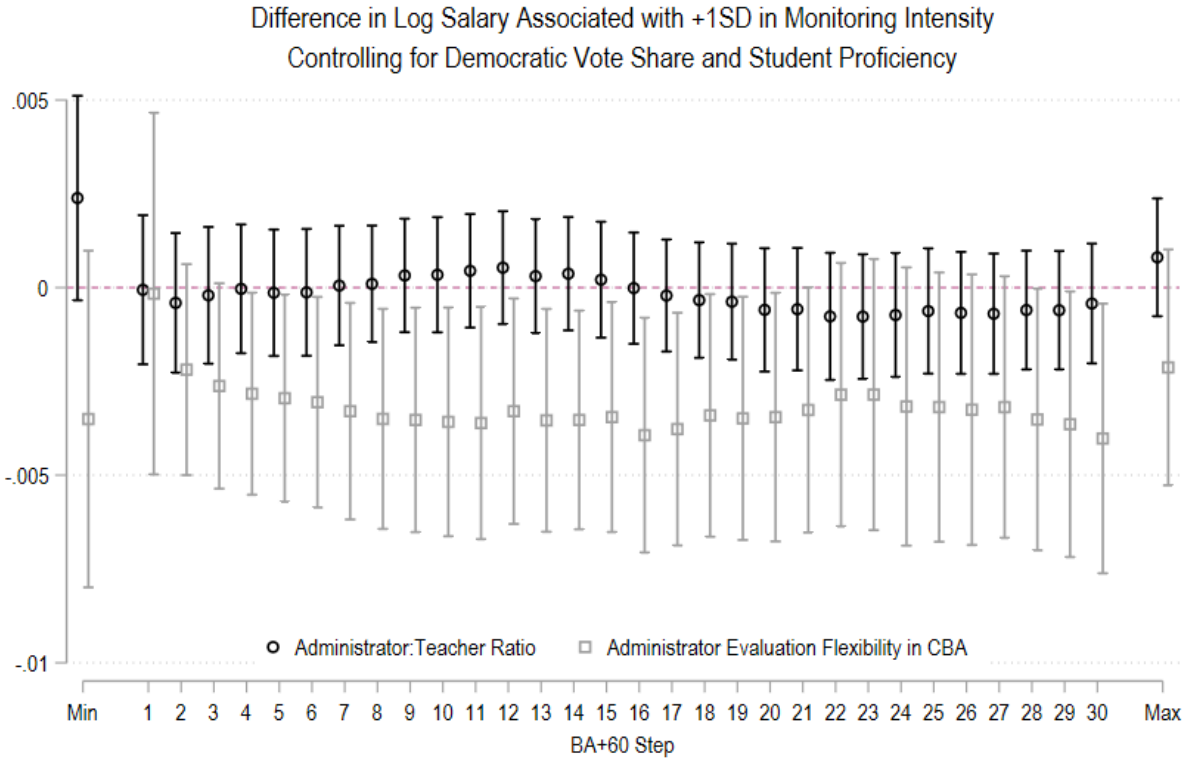

Figure B3. Difference in natural log of salary associated with a one standard deviation increase in monitoring intensity. Models are as described in figure 1 except that they additionally control for Democratic party vote share in the Congressional district in the most recent House election as well as the percentages of students in the district who were proficient (or above) on statewide standardized tests in math and English language arts. Black markers come from models where monitoring intensity is proxied by the administrator:teacher ratio (8,007 observations of 823 districts). Gray markers are from models where the proxy for monitoring intensity is administrator flexibility in evaluating teaches as captured in the collective bargaining agreement (CBA; 1,685 observations of 489 districts). Estimates include 95% confidence intervals based on standard errors clustered on districts.

Figure B4 – Differences in Log Salary Associated with +1 SD in Monitoring Intensity at Varying Levels of Overall CBA Restrictiveness

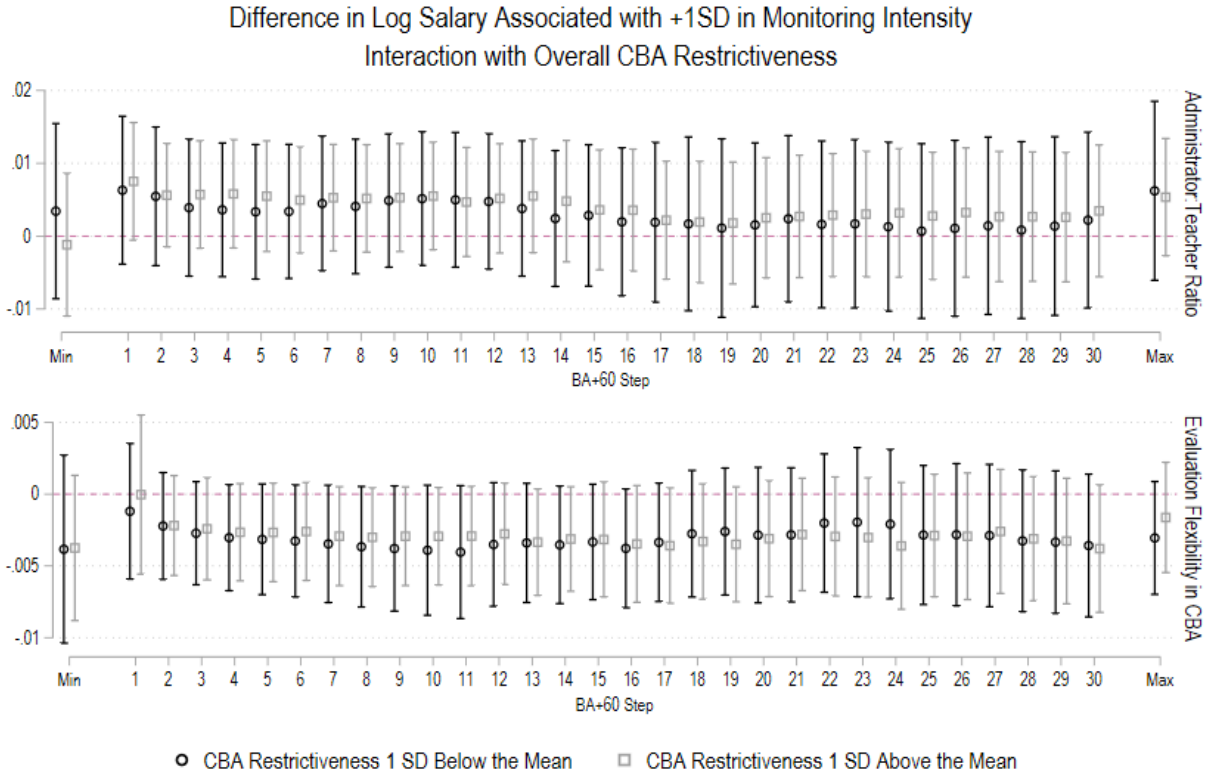

Figure B4. Difference in natural log of salary associated with a one standard deviation increase in monitoring intensity at varying levels of overall collective bargaining agreement (CBA) restrictiveness. Models are as described in figure 2 except that monitoring proxies are interacted with overall CBA restrictiveness. Coefficients are average marginal effects at the indicated levels of overall CBA restrictiveness and associated 95% confidence intervals. Models include 1,684 (for administrator:teacher ratios) or 1,686 (for evaluation flexibility in CBA) observations of 489 districts.

Figure B5 – Difference in Log Salary Associated with +1 SD in Monitoring Intensity by District Grade Level

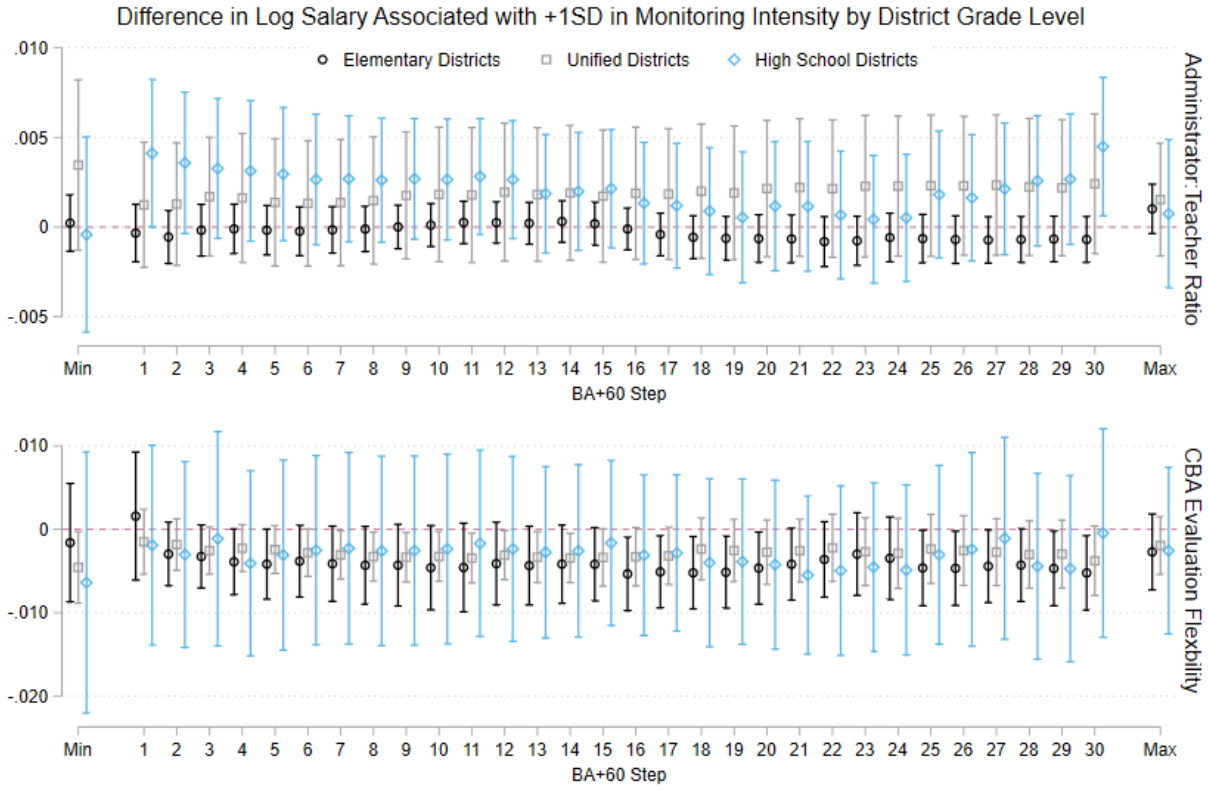

Figure B5. Difference in natural log of salary associated with a one standard deviation increase in monitoring intensity, estimated separately by district grade level. Models are as described in figure 2 except that relationships with proxies for monitoring intensity are allowed to vary between elementary (K-8), unified (K-12), and high school (9-12) districts. Coefficients are average marginal effects and associated 95% confidence intervals.