When Individual Politics Become Public: Do Civil Service Protections Insulate Government Workers?

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Abstract

This paper examines whether the civil service system protected state bureaucrats from political interference following a recall petition against the governor of Wisconsin. I find that most classified workers, who were covered by the state civil service laws, were paid equally by signing status following the public disclosure of the petition list. Conversely, signers in the unclassified service, a smaller set of government positions, were paid about 3 percent less annually relative to non-signers in the post-disclosure period. These results indicate that the civil service insulated qualified bureaucrats, while uncovered workers faced retribution.

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Protecting public-sector workers from politics is a staple of an effective bureaucracy and widespread across countries, including in the US (Besley et al. 2022). For instance, Rauch and Evans (2000) discuss how merit-based determinants of selection and promotion of state officials is a necessary condition for a professional, "Weberian" bureaucracy. Accordingly, many countries have enacted legislation and created independent agencies to ensure that partisanship does not interfere with state personnel decisions (OECD 2020).

However, while an independent bureaucracy is considered a tenet of productive governance, it remains unclear whether *de jure* mandates translate into *de facto* protections for civil service workers. To this point, research from developing countries finds that politicians may use subtle ways to circumvent the civil service system, either through job reassignments (Iyer and Mani 2012) or by reclassifying positions as exempt from the civil service (Colonnelli, Prem, and Teso 2020). The effectiveness of civil service rules in the US is particularly not well understood, in part, because we rarely observe measures of bureaucrats' connectedness to a political administration besides what can be gleaned from ideological leanings seen in campaign finance reports or party registration data. It is important to evaluate whether civil service protections are effective — namely, whether government workers are protected from political retribution — because recent administrations have sought to pare back the civil service system, arguing that the rigidities of the civil service are too cumbersome.[1]

In this paper, I examine whether US civil servants who oppose the administration in power are retaliated against following public disclosure of their opposition. To do this, I study the unique case of the 2011–2012 Wisconsin recall election of Governor Scott Walker. This special election was triggered after almost one million citizens signed a recall petition to remove Governor Walker from office following a controversial piece of legislation. Known as Act 10, the bill curtailed public-sector workers' ability to collectively bargain as part of a labor union. However, to people's surprise, a searchable database was posted online with the names of all the recall petition signers prior to the special election. Debate ensued about the ethics of signing the petition, especially for those working in certain occupations such as journalists and judges.[2]

[1] E.g., see https://finance.yahoo.com/news/brewer-signs-bill-revamp-state-224535841.html.

[2] E.g., see https://madison.com/wsj/news/local/govt-and-politics/article_0f28272c-7865-11e1-a779-001871e3ce6c.html and https://fox6now.com/2012/04/27/judges-investigated/.

Civil service protections — which often require that hiring, promotion, and salary decisions be independent of politics — are in place to prevent politicians from using the government bureaucracy as a tool to reward supporters and punish detractors. This setting provides a useful natural experiment to investigate whether the civil service system does in fact protect public-sector workers from politics because it provides a shock disclosure of state officials' political views that is differentiated from a normal election cycle. Previous studies often examine personnel changes following elections that switch the political party holding office (e.g., Spenkuch, Teso, and Xu 2023). In this instance, the Wisconsin recall drive resulted in a mass disclosure of bureaucrats' opposing views towards the administration, separate from any natural churn that accompanies election turnover.

To determine whether government employees faced retribution for signing the petition, I match the list of signers to the universe of state employees using 2006–2017 government administrative salary data. I then use a difference-in-differences framework, comparing signers to non-signers, before versus after the 2012 disclosure of the petition list. I break out the analysis by whether a government worker was part of the *classified* service, who are granted full civil service protections, or the *unclassified* service, who are not given full protections. I find that, for most classified workers, which represented the majority of the state workforce, there is no difference in pay following the petition disclosure. Conversely, for unclassified workers, a smaller group of highly-paid employees, signers experienced a 3 percent decline in annual earnings in the years following the release of the petition list, relative to non-signers. Importantly, however, I examine heterogeneity within the classified service by looking at whether there were differential effects for administrators employed in upper-level management positions. While the majority of the civil service was paid equally by signing status, classified administrators who signed the petition also experienced a relative pay decline of about 2–3 percent. These results suggest that there were two channels through which a salary gap emerged: (1) a lack of formal civil service protections, or (2) being close to the top of the government bureaucracy.

Beyond the curtailment of collective bargaining, Act 10 changed other aspects of state employment, including the requirement that workers pay a greater share of their retirement and health insurance contributions. While these provisions should in theory affect all state employees, one identification concern is that workers signed the petition because they believed that their careers would benefit from union coverage or that their specific jobs would be in jeopardy under the Walker administration. To this point, workers in jobs covered by a labor union were more likely to sign the petition. Therefore, it is important to not conflate any signer effect with any differential changes by job or union coverage. To alleviate this concern, I first control for time-varying baseline job fixed effects, in order to estimate the average signing effect within a particular occupation. Next, I use the coarsened exact matching algorithm (CEM) developed by Iacus, King, and Porro (2012) to more precisely find comparable sets of workers by signing status. I match workers on union status, state agency, job group, and years of experience in the year prior to the release of the petition list. The primary assumption is that, if a worker was going to be differentially harmed by the new legislation, then other workers with the same job attributes should be similarly affected. Difference-in-difference regressions using job fixed effects and matched samples still show a sizable negative effect for unclassified workers and classified administrators. However, the effect for classified workers in non-administrative positions is a very precise zero.

The post-disclosure salary gaps in the unclassified sector cannot be explained by compositional changes within the state workforce as the results are robust to using individual fixed effects as well as to using a balanced panel of workers. Therefore, to understand the driving mechanisms for these findings, I first assess whether the differences in average pay are explained by hierarchical movements. For instance, perhaps employees were passed over for promotions because they signed the petition. Yet, I find that signers in the unclassified service and at the top of the bureaucracy were not more or less likely to be promoted or demoted, nor were they transferred across state agencies at a different rate.

Next, I examine differences in base hourly pay, which must conform to a fixed range on occupation-specific salary schedules, versus a measure of bonus pay above what would be predicted given someone's hourly rate. More specifically, I define residual bonus pay as any gross salary in excess of the base hourly rate multiplied by a standard work year (and net of any overtime pay). I see a small difference in terms of base hourly pay; however, most of the decline is due to differences in my constructed measure of residual bonus pay. Therefore, it seems that, when discretion over pay was possible, non-signers were rewarded with more additional pay relative to signers.

A contrasting explanation for the salary gap is that signers exerted less effort after Governor Walker's successful re-election to office, and so differences in pay reflected differences in productivity. While productivity measures are notoriously hard to capture in the public sector (e.g., Dixit 2002, Besley et al. 2022), the State of Wisconsin gave out discretionary merit compensation awards over this period to recognize good performance. I find no differences in the rate of merit award recognition, though this is merely suggestive as the DMC program was mostly administered in the years after the recall petition. While not an ideal measure of performance, the similar rates of merit recognition do not accord with effort or differential expectations from signers as the driving mechanism for the pay differences. Additionally, I examine quality measures for the state attorneys general and public defenders, all of whom lacked classified civil service status. However, signers and non-signers attended equivalently ranked law schools and were not differentially likely to have faced disciplinary proceedings. Finally, looking at a subset of bureaucrats who donated to political campaigns, I find suggestive evidence that signers were negatively affected regardless of their political affiliation. This strengthens the argument that the pay gap was a deliberate response to the individual decision to sign the recall petition.

Lastly, I examine changes to the composition of the workforce such as whether signers were differentially likely to exit or enter the government following the petition disclosure. I find evidence that classified administrators who signed were more likely to leave the government than non-signers, though it is not clear if this was due to voluntary departures or forced exits. Likewise, I see suggestive evidence that signers were less likely to enter the workforce, though I cannot separate whether this was due to a decline in applications or a deliberate factor in the hiring process.

This paper builds on a growing literature in public-sector personnel economics studying how the US civil service system has affected the quality of governance and employment in the public sector. For instance, several studies examine historical changes in state and local spending after sub-national governments switched from the spoils-type system, where patronage was allowed, to the merit system where officials had to hire based off objective qualifications (Rauch 1995, Ujhelyi 2014, Bostashvili and Ujhelyi 2019). Likewise, Ornaghi (2019), Aneja and Xu (2022b), and Moreira and Pérez (2022) examine how the transition to the merit system affected the quality and performance of government bureaucrats, generally finding positive effects.[3] Recent work at the federal level examines how presidential turnover affects public-sector employment (Bolton, de Figueiredo, and Lewis 2021; Spenkuch, Teso, and Xu 2023). With a similar upshot to this research, Spenkuch, Teso, and Xu (2023) find that presidential changes lead to turnover for political appointees, but the rank-and-file civil service is largely protected from political cycles. This literature examines cases where the entire personnel system was overhauled dramatically or where there was a concurrent change to the political party holding office. My paper adds to this research by considering an event where the civil service system and political regime remained intact, but where there was a sudden revelation of employees' opposition to the government administration. This allows me to analyze whether the purpose of the civil service system — insulation from political discretion — works as intended in a modern setting.

Recent work in other settings also studies whether politicians fill government positions with political supporters or pay politically aligned bureaucrats more following changes in leadership. For example, research in developing countries finds that political turnover after elections leads to public-sector jobs going to politically connected individuals, often resulting in lower quality public services (Brassiolo, Estrada, and Fajardo 2020; Colonnelli, Prem, and Teso 2020; Akhtari, Moreira, and Trucco 2022; Barbosa and Ferreira 2023). Likewise, Xu (2018) finds evidence of patronage following turnover in the British colonial administration, which also led to lower-quality bureaucrats. While much of this work focuses on developing countries, my study explores a setting in the US, where institutional protections may be stronger.[4] Moreover, while much of this research studies whether election turnover affects the selection process into government jobs, my study examines whether career civil servants are negatively affected for exercising their political rights.[5]

In a similar event to this paper, Hsieh et al. (2011) examine whether employment and wages were affected following a petition drive in Venezuela to remove Hugo Chávez from office. They find that Venezuelan petition signers experienced a 5 percent decline in earn-

[3] Looking at elections, Folke, Hirano, and Snyder Jr. (2011) find that the former patronage system helped political parties retain power.

[4] One study that examines a developed context is Fiva et al. (2021), who find that bureaucrats in Norway are paid more when a politically aligned politician wins office.

[5] My findings regarding the merit compensation program also relate more broadly to a large literature on performance pay in the public sector. For example, see Lavy 2009; Muralidharan and Sundararaman 2011; Khan, Khwaja, Olken 2016; Burgess et al. 2017; Leaver et al. 2021.

ings following the disclosure of the petition list. My research builds on this work by examining a setting where institutional safeguards should in theory protect government workers from any political retribution. I find that the civil service system seems to have worked largely as intended — most classified workers were not retaliated against and did not exit the workforce at a differential rate. Interestingly, though, I find a decline in earnings similar in magnitude to Hsieh et. al (2011) for unclassified workers, suggesting that, even in a non-authoritarian government, opposing the administration in office can affect one's career when civil service protections are not present.

I. Context and Setting

A. The Wisconsin Civil Service

Many US states outlawed patronage in state government following the 1883 Pendleton Act, which mandated that federal hiring be determined by merit. In 1905, Wisconsin became the third state to pass such legislation, creating a merit-based civil service system requiring that public-sector workers be hired by competitive examinations. In addition to the requirement that hiring be based off objective metrics, the law prohibited firing employees because of their political affiliation. Addressing the legislature, the then Governor of Wisconsin, Robert LaFollette, said, "The fundamental idea of democracy is that all men are equal before the law. What proposition is plainer than that every citizen should have an equal opportunity to aspire to serve the public, and that when he does so aspire the only test applied should be that of merit. Any other test is undemocratic. To say that the test of party service should be applied is just as undemocratic as it would be to apply the test of birth or wealth or religion" (OSER 2005, pg. 1). As in other states, the Wisconsin civil service system also required that people working in similar jobs be paid equivalently as defined by uniform salary schedules to prevent salary manipulation and produce predictable budget expenditures (OSER 2005).

Since the enactment of the civil service system, Wisconsin government employment is largely broken into two groups: the classified civil service and the unclassified civil service. According to the current state employment relations guide, "Selection of classified state employees shall be based on merit and no employment recommendation shall be based on political or religious affiliation or on membership in associations not primarily related to merit in employment" (Wisconsin Statute Chapter 230). The unclassified service, meanwhile, is not considered to be covered under the civil service protection laws. In the period of study, the unclassified service includes people in departmental leadership positions, the State of Wisconsin Investment Board (which manages the finances of the Wisconsin Retirement System), all state attorneys general and public defenders, and a few other less common positions. Upon creation of the Wisconsin civil service, 960 employees were in the classified service and 600 were considered the unclassified service. However, in 2011, 96 percent of full-time employees were part of the classified service.

B. The 2011–2012 Recall Drive and Election

In the fall of 2010, Wisconsin held a gubernatorial election between Republican Scott Walker and Democrat Tom Barrett after the previous governor, Democrat Jim Doyle, decided not to seek re-election. Walker won the 2010 election, taking over as Governor of Wisconsin in January 2011.

Shortly after taking office, Governor Walker introduced legislation known as Act 10, a typical budget bill that became contentious because it included provisions limiting public-sector workers' ability to collectively bargain as part of a labor union. It also made it harder for unions to remain active by disallowing the collection of "fair-share" fees from non-members and requiring that unions hold elections in order to maintain their right to collectively bargain over base wages. Additionally, the bill required that public-sector workers pay more into their pension contributions. Immediately after Act 10 was proposed, thousands of demonstrators protested outside of the state capitol building in Madison. The Democratic state legislators attempted to stall voting on the bill by fleeing the state, but eventually the law was passed via Republican majority in June of 2011.

As a result of the legislation, a group of grassroots organizers started the process of initiating a special recall election in the fall of 2011. Under Wisconsin law, organizers had sixty days to collect physical signatures amounting to 25 percent of the people who voted in the previous gubernatorial election. Given the 2010 election vote totals, this meant that organizers needed roughly 500,000 signatures, a threshold that was easily surpassed, as around 900,000 Wisconsin residents signed the petition. This triggered a special election in July of 2012, which Walker won, again defeating Democratic candidate Tom Barrett.

In January 2012, the Government Accountability Board (GAB), the state agency in charge of facilitating the petition verification process, posted the 153,000 scanned petition pages on their website as PDF documents. The GAB maintained at the time that the lists would not be searchable.[6] However, two conservative organizations digitized the thousands of petition pages with the help of volunteers and posted the list of names on a website called iverifytherecall.com in March of 2012. Visitors to the site could type in a person's first name, last name, or address to see whether someone had signed the petition. The website included links to scanned images of each individual petition page. Likely in response to the iverifytherecall.com site, the GAB revised their decision, creating a searchable online database of petition signers in late March of 2012.

This set off a debate about whether it was inappropriate for people working in certain occupations to have signed the petition. As an example, a complaint was filed against several Wisconsin state judges who signed, claiming that the judges violated their ethical duties to remain non-partisan. A state ethics commission formally addressed the issue and stated that the judges did not act impartially in signing the petition. More broadly, after the petition drive was completed, there was confusion as to whether the petition signatures would become publicly available. Kevin Kennedy, the director of the GAB, stated that signing a petition, unlike voting, was a public act and therefore open to the state's open records law. However, Kennedy acknowledged that most petition signers likely did not expect that their name and address would be made public online when they signed.[7]

II. Data

To estimate whether public workers' career and pay trajectories were affected by the petition disclosure, I link the list of signers with Wisconsin state employment records. The next section describes the data sources, after which I explain the matching procedure.

A. Recall Petition Lists

The recall petition was collected via thousands of paper forms, as each individual petition page had a maximum of ten signatures per page. Two entities digitized these forms independently and I have data from both digitization efforts.

[6] For instance, see https://youtube.com/watch?v=oXgP53qJNpc.

[7] See https://jsonline.com/news/opinion/posting-signatures-stirs-controversy-k441iqs-138685999.html

The first digitized list of petition signers comes from the Wisconsin Elections Commission (WEC), a non-partisan division of the Wisconsin government responsible for administering election law (Wisconsin Elections Commission 2012). Formerly known as the Government Accountability Board (GAB) until 2015, the agency handled the processing of the 2011 recall petitions. From the WEC, I received scanned copies of the individual petition lists as well as a database containing the digitized names of all petition signers. The WEC did not enter a person's address information into their database, which is available both on the scanned PDFs as well as on the iverifytherecall.com site. There are 930,911 individual names in the WEC database.

The second petition list is from the iverifytherecall.com website (Verify the Recall 2012).[8] The site became live to the public on March 8, 2012, and news outlets immediately ran stories covering the creation of the website.[9] In addition to being able to search someone's name, the iverify site allowed for searches by address, municipality, and zip code. There are some petition pages which appear on the WEC list but not in the iverify list — I estimate that the iverify list has about 826,000 names.

B. Wisconsin Employment Data

In 2018, I requested salary information from the Wisconsin Department of Administration (DOA) for all state employees since 2005 (Wisconsin Department of Administration 2017). The DOA fulfilled my request for every year except 2005, as records were not maintained going back that far. They also redacted names of certain employees where their health, safety, or financial security would be at risk by inclusion in the data. Additionally, the DOA withheld the information of certain staff employed in a law enforcement capacity.

In total, I have annual employee salary data for all state agencies in the executive branch from 2006 to 2017 with the exception of the University of Wisconsin system as their payroll records are maintained separately (see Appendix Table A1 for a list of the most common job titles by agency). I also do not observe anyone employed in either the legislative or judicial branches of the Wisconsin government. Furthermore, the data do not include any local government employees such as police officers or school teachers.

[8] Henceforth, I will refer to this as the "iverify" data for ease of exposition.

[9] E.g., see https://madison.com/daily-cardinal/news/new-website-provides-searchable-database-of-recall-petition-signees/article_c5f2aafc-69ad-11e1-a3a5-0019bb2963f4.html.

In each year, there are between roughly 36,000 to 44,000 records. Of the over 460,000 combined records, fewer than 1,000 have their name redacted or are missing either their first or last name. The data include a person's name, job title, civil service classification status, hourly rate, gross pay, start date in the Wisconsin government, start date in their current job, overtime pay, work county location, and the state agency they worked for. Some people appear more than once in a particular year if the person worked more than one job in that year. The data do not contain individual identifiers, so I construct them using a person's full name and start date in the government workforce.

I separately received data pertaining to discretionary merit compensation (DMC) awards, which were given out from 2006 to 2008 and then again starting in 2012. Recipients of the DMC awards were given either a lump sum pay bonus or an hourly pay raise.

C. Matching Procedure

There are some discrepancies between the WEC and iverify databases given that the original petitions were collected via paper forms and needed to be digitized by hand. For instance, misspellings are possible and, as mentioned, the iverify list is missing some pages. However, the iverify list has the added benefit of including geographic information. I therefore incorporate both datasets to come up with a measure of who signed the petition.

One limitation of the data is that the petition list does not include all people in Wisconsin by definition. This means that an unmatched worker is either a non-signer or a signer who was mistakenly not linked. It is therefore important to carefully devise a matching procedure that limits mismatches. To do this, I first start with the most stringent matching procedure, incorporating the iverify geographic information. More specifically, the iverify data includes individual addresses, while the DOA salary data includes a person's county of work. This allows me to rule out implausible cases where a potential match lives in an opposite part of the state from the listed work location. Hence, for each residential zip code, I tabulate each county within 25 miles to account for possible commuting distances. I then match the DOA salary data to the iverify data by an individual's full name and whether someone with the same name lives within 25 miles of the work county.

Next, I repeat the process using the WEC petition list. As there is no geographic information in this data, I simply match the WEC list to the DOA salary data by first, middle, and last name. By construction, this procedure results in a higher match rate than when matching using the iverify data, but the added benefit is that it also matches people who may be missing or misspelled in the iverify data.

To devise a singular measure of who signed the petition, I first code a worker as a signer if they match to the petition list using the more restrictive procedure with the iverify geographic information. I then add in anyone who matches to the WEC list but whose name does not appear in the iverify list. This accounts for anyone who is missing or misspelled in the iverify list. Finally, I exclude from the sample any workers with a name that appears on the recall petition list more than once in a particular county, as these names are so common that there are likely other people in the area with that name who did not sign the petition. See appendix section C for more details.

In Section IV, I validate that the results are robust with regards to the matching procedure. Given the large number of signers, and lack of other identifying information, there may be instances where a DOA record is matched to the petition list incorrectly. To alleviate this concern, I first make a more stringent cut, further dropping people with common first and last names. I also re-analyze the results using only the iverify list or only the WEC list, implement a fuzzy matching algorithm, and vary the 25 mile commuting distance.

III. Sample and Empirical Strategy

A. Analysis Sample

This paper seeks to understand whether state employees' pay and employment status were affected by the fact that they signed the petition to recall the governor. One complication is that people who were hired after 2011–2012 may not have been living in the state at the time of the recall, leading to an overestimate of non-signing. As such, the analysis focuses on people who were employed in the state government at the time of the recall, assuming that those individuals had the choice to sign given that they worked in the state. More specifically, I limit the sample to state employees who, at a minimum, have salary data in years 2011 and 2012, as these people were present in the state in both the pre- and post-disclosure periods. I further drop individuals who worked fewer than an estimated 20 hours per week to focus on people whose primary occupation was likely in the public sector.[10] Lastly, I drop elected officials, temporary workers, and limited-term employees to focus on people working as career civil servants.

The analysis sample contains 24,561 unique individuals, 53 percent of whom are matched as signers. This rate is much higher than the state average, given that roughly one in four Wisconsin adults signed the petition. However, it seems reasonable that state workers signed at a significantly higher rate relative to the average population. For one, many of the Wisconsin state employees work in the Democratic-leaning capitol of Madison. Additionally, Act 10 curtailed collective bargaining rights and altered employees' benefit packages specifically for public-sector workers.

On average, signers were more likely to come from the middle range of the pre-2012 income distribution as seen in Figure 1. The plot shows that the distribution of signers was relatively higher between annual salaries of about \$30,000 and \$50,000. Conversely, the distribution of non-signers is skewed towards the upper income part of the pay scale.

Table 1 presents employment characteristics by signing status in 2011, the year before the petition database disclosure. The left half of the table shows characteristics of the 11,472 individuals who did not sign the petition, while the right side of the table shows characteristics for the 13,089 signers. The right-most columns present the difference in means and associated p-values between the two groups. There are statistically significant differences at baseline, an unsurprising fact given that signing was of course not randomly assigned. For instance, as seen in the first row, there exists about a \$2,800 annual pay difference between the two groups, with signers earning slightly less on average relative to non-signers. The two groups are similar in terms of years of experience with a mean difference of less than one year (row 2). There is a larger difference in the rate of union coverage between the two groups as 79 percent of non-signers were covered by a labor union pre-Act 10, while 87 percent of signers were covered by a union (row 3). The fact that people working in unionized jobs were more likely to sign accords with the fact that Act 10 diminished the power of unionization and collective bargaining.

[10] I do not observe hours worked in the data, so I make this determination by dividing one's gross pay by their hourly rate and then dividing by the number of weeks in a year.

Signers were also more likely to be members of the classified service (row 4). In general, this aligns with the fact that signers were also more likely to be covered by a union, as 84 percent of the classified service was covered by a union pre-Act 10, whereas 60 percent of the unclassified service was unionized. Lastly, signers were less likely to be employed in administrative positions, indicating that people at the top of the bureaucracy were less likely to sign the petition.

As previously mentioned, the unclassified service is primarily made up of department administrators, the state public defenders and attorneys general, and the state investment board members. Because the departmental leadership positions are a relatively small number of the total workforce, about 77 percent of the unclassified service are attorneys or investment board members. The average unclassified worker earned about $84,000 per year in 2011, whereas the average classified worker earned $52,000 (see Appendix Table A2 for mean differences between the two sectors).[11]

The highest-ranking administrators are likely to be in the unclassified service as appointees, but many other upper-level administrators are in classified positions. For example, Appendix Figure A1 displays an organizational chart of the Department of Revenue (DOR), where each box represents at least one administrative position. Near the top of the department, there are ten unclassified positions (in red) including the secretary and deputy secretaries who lead the agency, as well as most DOR division leaders (e.g., Division of Lottery). Within the various DOR divisions there are many more middle-level administrators who are members of the classified service (in gold).

In a preview of the empirical results, Appendix Figure A2 plots the raw trends in average pay by signing status. Panel A displays trends for unclassified employees; panel B plots the same trends for classified employees. In both classes, signers (blue) earn less on average relative to non-signers (gray). The trends are on similar trajectories throughout the sample period for classified employees. For unclassified workers, however, the pay gap widens considerably in 2012 when the petition list was released.

[11] In Section V, I discuss the differences between the two schemes in more detail; in particular, I look at a subsample of high-earning professionals.

B. Empirical Strategy

To formally assess whether signing the petition affected workers' pay and employment status, I use a simple difference-in-differences estimation strategy, comparing signers to non-signers, before versus after the 2012 petition disclosure. The regression specification is as follows:

$$Y_{it} = \alpha + \beta \mathbb{1}(Post_t \text{ x } Signer_i) + \gamma_i + \theta_t + \epsilon_{it}, \tag{1}$$

where Y_{it} is an employment outcome such as annual gross pay for individual i in year t, θ_t are year fixed effects, and γ_i are individual fixed effects, which are included in my preferred specification. I include individual fixed effects to absorb any time-invariant confounders that differ across individuals. The $\mathbb{1}(Post_t \text{ x } Signer_i)$ term is an indicator variable for the year being after 2011 and the individual having signed such that β is the regression coefficient indicating whether signers were differentially affected as a result of signing the petition. Note that the year and individual fixed effects absorb the indicator variables for $Post_t$ and $Signer_i$, but I include an indicator variable for having signed in specifications that do not include individual fixed effects. Standard errors are clustered at the individual level.

There are a few concerns with identification that arise with this setting and empirical strategy. First, as with any difference-in-differences, the design relies on the parallel trends assumption, such that, absent the database disclosure, pay would have evolved similarly for both groups prior to and after 2012. To examine whether signers and non-signers pay evolved similarly prior to 2012, I also analyze differences in pay using a corresponding event study approach. This regression specification is

$$Y_{it} = \alpha + \sum_{j=-6}^{5} \beta_j \mathbb{1}(Year_{t=j} \text{ x } Signer_i) + \gamma_i + \theta_t + \epsilon_{it}, \tag{2}$$

where the terms are as before, but now the β_j coefficients denote the pay difference relative to 2011, the year before the petition disclosure.

The other primary concern is that there exists a confounding shock that differentially affects a worker characteristic that is correlated with signing the petition. This is possible in this setting given that the Act 10 legislation, enacted in 2011, largely eliminated the state's public-sector unions. Therefore, the design may be picking up reverse causality if signers knew that their pay would be affected under a Walker administration and thus supported a recall election. To this point, Table 1 shows that there is a sizable difference in the rate of union membership by signing status.

I do two things to address this concern. First, I control for baseline covariates interacted with year fixed effects. Specifically, I interact 2011 job fixed effects and years of experience bins with year indicators to control for observable characteristics that may be correlated with signing the petition. This rules out the possibility that any post-disclosure effect was not because of signing directly but because of how Act 10 affected certain occupations. Of particular concern would be if any post-2011 pay differences were correlated with union coverage, given that Act 10 reduced the power of public sector unions. The baseline job fixed effects control for this given that specific job titles were all either covered or uncovered by a union.

Secondly, I use coarsened exact matching to create a more comparable sample of signers and non-signers (Iacus, King, and Porro 2012).[12] In this analysis, I match workers by their pre-disclosure characteristics. In other words, I find comparable workers using their 2011 baseline data, matching individuals by union coverage, Equal Employment Opportunity (EEO) job group code, state agency, and years of experience. I then use the baseline regression model (Equation 3) but reweight the sample using the corresponding CEM weights, which effectively drops any observations that do not have a matched individual in the opposite treatment group. When assessing robustness, I also add a fixed effect for each CEM strata interacted with year effects; formally, this estimating equation is

$$Y_{it} = \alpha + \beta \mathbb{1}(Post_t \text{ x } Signer_i) + \gamma_i + \lambda_{st} + \epsilon_{it}, \tag{3}$$

[12]For other papers using coarsened exact matching, see Azoulay, Fons-Rosen, and Graff Zivin (2019); Sarsons (2019); and Aneja and Xu (2022a).

where the terms are as before except that the strata-by-year effects (λ_{st}) replace the year fixed effects. Mechanically, the CEM procedure balances the sample by the joint distribution of the pre-disclosure covariates, such that the primary observable difference between the two groups is whether they signed the petition or not. The primary assumption is that individuals with similar levels of tenure, and working comparable jobs in the same agency, should equally benefit if there exists some advantage to changing administrations.

Finally, I split the sample by whether someone was a member of the classified civil service or the unclassified civil service as determined by Wisconsin statute. This is an interesting source of variation given that civil service protections for the classified sector should, in theory, disallow discretionary employment or pay changes as a result of political motives. Conversely, there may be more leeway for political discretion with regards to the unclassified sector. Therefore, the classified service provides a counterfactual for whether political interference in the government bureaucracy occurs in the absence of civil service protections.

The limitation of this heterogeneity analysis is that positions are of course not randomly assigned to one of the two civil service classifications either. As previously discussed, the unclassified sector is largely made up of high-ranking administrators, as well as high-skilled departments such as the office of public defenders. As a result, the unclassified sector mostly lacks any employees who are not highly-paid professionals or working in management roles. Accordingly, unclassified workers earn substantially more than the average classified worker (Appendix Table A2), opening up the possibility that unclassified workers were negatively affected, not because of their civil service status, but because they were high earners. I address this concern when looking at alternative mechanisms in Section V, examining a subsample of the workforce who were in the upper half of the pay distribution prior to the release of the petition list.

IV. Results

A. Salary Comparisons

In Table 2, I present the results from the difference-in-differences regressions, examining whether gross pay differed by signing status in the post-disclosure period. I split the sample by whether an employee was a member of the classified civil service in 2011 (panel A) or whether the employee worked in the unclassified civil service (panel B). Column 1 displays the results with year fixed effects, but no individual fixed effects. In this specification, the coefficient on *Signed x Post* is close to zero and insignificant for classified employees but indicates that a pay gap of about 7.0 percent emerges for unclassified signers relative to non-signers. Column 2 adds individual fixed effects, which reduces the effect for unclassified signers to a statistically significant -4.9 percent. Meanwhile, classified signers experience a 0.5 percent decline. Column 3 adds controls for baseline job fixed effects and years of experience bins interacted with year fixed effects.[13] After controlling for occupation- and tenure-specific trends, the coefficient in the classified sector is very close to zero, yet a 3 percent pay gap remains in the unclassified sector.

To further balance the samples by baseline observables, I match signers to non-signers based off their pre-Act 10 employment characteristics using the CEM algorithm. I then weight the regressions using the corresponding CEM weights, which places a weight of zero on people outside the area of common support (Blackwell et al. 2009).

I separately match individuals in each classification group exactly on an indicator variable for union coverage, state agency, and EEO job group code.[14] Lastly, I coarsely match the two groups on the number of years since the person first started in the Wisconsin state workforce, dividing workers into bins of ten years of experience.[15] For the matched sample, each person is grouped into one of 47 unique strata for the unclassified group and 526 unique strata for the classified group. Table 2 column 4 presents the difference-in-difference results using the CEM weights, again broken out by classified (panel A) versus unclassified employees (panel B).

The samples are slightly smaller than in columns 1–3, as some workers do not have a comparable individual in the opposite treatment group, with the exact same covariate levels, and in the same classification scheme. Hence, these workers are given zero weight in the regressions. It is important to note that the people who are dropped in the unclassified

[13]Specifically, baseline jobs are defined as one's job title as of 2011; there are about 1,300 unique jobs. Years of experience bins are divided into 10-year bins as of 2011.

[14]In 2011, there were 40 state agencies and eight EEO job groupings: officials/administrators, professionals, technicians, protective services, paraprofessionals, administrative support, skilled crafts, and service/maintenance.

[15]The results are robust to binning years of experience into other intervals as well.

sector are largely administrators at the top of their respective agency.[16] Of the unclassified workers in the full sample, 82 percent are considered "professionals" and 17 percent are "officials/administrators" according to their EEO classification. Yet after dropping the unmatched individuals, 91 percent and 8 percent are professionals and administrators, respectively. This change in the distribution occurs because each agency usually has fewer than ten unclassified administrators, 28 percent of whom signed the petition. Therefore, many administrators are dropped as the CEM procedure requires that, within each agency, there needs to be both signers and non-signers with similar levels of experience. However, there are still some administrative positions that remain after matching; for instance, "deputy district attorney supervisor" and "investment board–executive" are administrative positions that are included in the matched sample.

In panel A of column 4, the point estimate on classified employees is again a very precise zero using the CEM weights. Specifically, the estimates imply that with 95 percent confidence the post-signing salary gap is smaller than 0.4 percent. Yet even in this restrictive specification, the unclassified group effect is still a statistically significant -3.2 percent (panel B). This indicates that a significant salary gap exists post-signing even for individuals working in the exact same job group, agency, and with similar levels of experience. Moreover, this implies that unclassified workers not at the top of the bureaucracy were negatively impacted as a result of signing the petition.[17]

Visual Evidence: To understand the dynamic effects, Figure 2 presents the associated event study plots for the classified service (panel A) and the unclassified service (panel B). The figure displays results using the more restrictive CEM specification (corresponding to column 4 of Table 2) but Appendix Figure A3 displays event studies without using the CEM-weighted sample. Each event study coefficient is relative to 2011, the year before the searchable petition list was posted online. I plot both figures on the same axis scale to clearly see the differences between the two classification groups. In Figure 2 panel A, the plot for the classified service shows no discernible pre-trends as the regression coefficients are close to zero for each year from 2006–2010. After the petition files are released, the

[16]In total, there are 758 unique people in the matched sample for the unclassified sector, 323 of whom are signers.

[17]In Appendix Table A3, I show the results from a triple differences specification in which I interact *Signed x Post* with an indicator variable for being in the unclassified service. Appendix Table A4 column 3 shows the difference-in-difference results when interacting the CEM stratas with year effects.

coefficients are still at a precise zero, indicating that classified employees were not paid differently because of their signing status.[18] Turning to the unclassified employees in panel B, once again the pre-2011 trends are flat and statistically insignificant. However, in concert with the difference-in-differences estimates, the post-2011 coefficients start trending downwards in 2012, settling in at about a 4 percent pay difference the year after the disclosure. In summary, the event study plots alleviate the concern that signers and non-signers' gross pay was trending differently prior to the petition disclosure. Additionally, the evidence accords with the previous findings that the unclassified service was negatively impacted, while most of the classified service was not.

These effect sizes are economically important as the state workforce only received general pay increases in five of the sample years, at an average annual rate of about 0.7 percent. Therefore, an annual 3 percent pay decrease was at least three times as large as the expected general wage increase in a given year.

Heterogeneity: Next, I assess heterogeneity within the classified service. In Table 3, I show results for classified administrators, defined as people working in the "officials or administrators" EEO job grouping. Recall that these people worked in upper management positions but held classified civil service protections. Across all specifications, there exists a statistically significant pay gap of about 2–3 percent, indicating that, even within the classified service, administrators earned less due to signing the petition.[19] As with the unclassified sector, classified administrator salaries exhibit no pretrends but diverge starting in 2012 as seen in Appendix Figure A4. This suggests an additional channel through which people were affected: working in a job position that was close to the executive.

Figure 3 presents more detailed heterogeneity analysis, comparing differences across the classified and unclassified civil service schemes but holding the level of the government hierarchy fixed. Each "row" of the figure plots regression coefficients based off workers' hierarchical position prior to the release of the petition list, where the positions are ranked from top to bottom. In particular, the first row shows coefficients for "administrators" who

[18] The 2006 and 2007 point estimates are significantly different than 0 suggesting there could be a small pre-trend in the classified sector. To formally test whether this is a significant trend, I conducted a Wald test for the pre-2012 coefficients (Freyaldenhoven et al. 2021). The Wald test of joint significance cannot reject that the pre-2011 coefficients are jointly equal to 0 at the 10 percent level.

[19] In the matched sample, there are 464 unique classified administrators, 148 of whom are signers.

were employed in top-level management roles. The second row shows results for managers that were below the top level, which I refer to as "supervisors." Corresponding to Table 3 column 4, there exists a 2 percent salary gap for classified administrators that is significant at the 10 percent level. However, this negative effect does not seem to have leaked down to the supervisors level within the classified sector as the coefficient is close to 0 and not statistically significant (as seen in the second row). For both the administrators and supervisors in the unclassified sector, there is a large, negative coefficient that is imprecisely estimated owing to the fact that there are only a few unclassified managerial workers within each agency.

The bottom two rows of Figure 3 show the associated results for workers in non-management positions. Specifically, rows 3 and 4 show the regression coefficients for "professionals" and "non-professionals," respectively. The key difference here is that there exists a negative effect of about 3 percent for unclassified professionals who were not employed as managers. Conversely, the coefficient for classified professionals is a precise zero effect. This indicates that the unclassified salary gaps are due to the civil service scheme rather than being explained simply by a worker's ranking within the government bureaucracy. Finally, there is no estimate for unclassified non-professionals as the agencies with unclassified workers are predominately made up of highly-skilled workers. In the classified sector, there is again a precise zero effect for non-professionals.

To summarize, the figure shows that classified workers are more protected even when holding constant seniority levels, suggesting that the results are indeed capturing differences due to civil service protections. The coefficients within the unclassified group are consistently negative and the mean effect is driven by professionals who were not employed in managerial positions. In the classified sector, there is some evidence of a salary gap for the very top officials, but this negative effect did not seep down into the supervisory or professional job groupings.

B. Robustness

In Appendix Table A5, I assess whether the results are robust to altering the sample or specification.[20] In panel A, I report the *Signed x Post* coefficients for the unclassified sector;

[20] I report the robustness checks using the CEM-weighted regressions as this is the most restrictive specification. Results are robust to the other specifications as well.

panel B reports results for the classified administrators subsample.

First, I use a balanced panel of individuals who are present in the data for every year from 2006 to 2017. Even though many people worked for the government over this entire time frame, this cut drops about 95,000 observations (38 percent of the sample). This procedure completely shuts down any effect arising from compositional changes in and out of the state workforce. For the unclassified service (panel A) the pay gap is about 5.1 percent, providing strong evidence that career civil servants were negatively impacted. In the classified administrators sample (panel B) the salary difference is a qualitatively similar, though less precise, 2.5 percent.

One concern is the measurement error that may occur when matching the petition signers to the administrative salary data on only a person's name and county. Recall that I already drop any state workers whose name combination appears on the petition list more than once within a local area. However, there still may be people who are unique to both datasets but are not the same person, given that the petition list is not an inclusive list of all adults in Wisconsin. While it is not obvious that this would introduce bias in a certain direction, I re-estimate the results by dropping people with common names from the sample. Specifically, I take the 100 most common forenames and surnames in Wisconsin from Forebears.io (Forebears n.d.). I then drop anyone who has a first and last name that are both ranked in the top 100 names in Wisconsin. This procedure drops roughly 10,000 observations from the sample. The coefficients are very similar to those in Table 2 with a 3.2 percent decline in earnings for the unclassified sector (panel A) and a 1.8 percent decline for the classified administrators (panel B).

Columns 3 and 4 show that results are similar when only matching the salary data to one of the two petition list databases. Column 3 shows results from only matching names using the Elections Commission list, while column 4 shows results using only the iverify list.

Lastly, the results are robust to tweaking the matching procedure more generally. Recall that I only consider a potential match if a person's work county is within 25 miles of a possible residential county to allow for possible commuting distances. Appendix Table A6 reexamines the main results for the unclassified sector when matching exactly on county (column 1), or redefining the cutoff to different distances (10 miles in column 2, 40 miles in column 3).

I also test whether the results are robust to using a fuzzy matching algorithm, which allows for possible misspellings. Recall that my primary matching procedure relies on two separate petition lists: the one digitized by iverify and the one digitized by the elections commission. Therefore, a misspelling that was a true match would have to be misspelled or missing on both petition lists. The exact matching procedure is strict by design as it is trading off type I and type II errors given that the petition data is not a list of all residents of Wisconsin. Nevertheless, in Appendix Table A7 I use the Stata `reclink` package to test this assumption. Specifically, I run a fuzzy match for each county in the data separately to make the process tractable. The reclink algorithm then assigns a score from 0 to 1 denoting the likelihood of a match. Appendix Table A7 column 1 displays the coefficient from the exact matching procedure for reference (corresponding to Table 2 panel B column 4). In columns 2 and 3, I find similar results when using various cutoffs from the fuzzy matching procedure. Specifically, column 2 designates a match if the `reclink` score is greater or equal to 0.98 and column 3 denotes a match if the score is greater or equal to 0.95. The matches below this threshold appear to be very low quality.

V. Discussion of Mechanisms

The evidence presented thus far indicates that state employees' pay was negatively impacted by signing the petition for those working in unclassified or administrative positions. The results are not explained by compositional changes given that the pay gap is present in specifications using individual fixed effects and when using a balanced panel of workers. However, I return to the question of whether the composition of the workforce changed in Section VI. But first, I explore mechanisms for the results by examining whether there were differential rates of promotions or discretionary bonuses. I then discuss other potential explanations for the salary differences. I present evidence most consistent with signing as the primary explanation, rather than other possibilities such as differential effort rates or political party status.

A. Hierarchical Changes

One potential explanation for the differences in salaries would be that signers were either demoted to lower positions or were passed over for promotions. Alternatively, perhaps signers were more likely to be transferred to lower-quality agencies. The DOA data does not explicitly state when someone is promoted or demoted so, as a coarser measure, I define a promotion as when an individual changes job classifications and receives a base pay increase above the state's general wage increase.[21] Likewise, I denote a demotion as when someone changes jobs but does not receive a pay increase. Finally, I examine whether someone transferred between agencies, which may be a form of reward or punishment given the rigidities of wage changes in the public bureaucracy (e.g., Iyer and Mani 2012; Khan, Khwaja, and Olken 2019).

I report the results from this exercise in Table 4. For simplicity, panel A now reports regression results for the classified non-administrators; panel B shows results for a pooled sample of both the unclassified sector and classified administrators. Results are similar when looking at the unclassified sector and classified administrators separately. For classified non-administrators (panel A), there is a point estimate of -0.3 percentage points on the likelihood of promotion (a 4.2 percent difference from the mean) that is just outside the range of statistical significance (column 1, p-value = 0.103). Appendix Table A8 varies the definition of a promotion and finds a small effect in some cases but the estimate is not consistently significant across specifications. This may suggest that there was a small impact on the likelihood of promotion within the classified sector, but it is hard to say definitively given the imprecision and uncertainty regarding what constitutes a promotion. However, the difference in promotion rates corresponds to a very small monetary difference, which is why there is such a precise null effect on gross pay (though promotions may be important in their own right if they come with other job amenities).[22] In columns 2 and 3, there are precise null effects on demotions and transfers.

For the unclassified sector and classified administrators (panel B), the coefficients on all

[21] All state workers receive the same general wage adjustments for a particular year, as determined by the state budget. Over the sample period, workers received a 2.25 percent general wage increase in 2007, a 2 percent increase in 2008, and a 1 percent increase in 2009, 2014, and 2015. There were no general pay increases from 2009–2013 and 2016–2017.

[22] Specifically, a 4.2 percent reduction in the probability of promotion implies that signers' promotion rate is about 6.9 percent, given a mean promotion rate of about 7.2 percent. Moreover, the average gross pay increase upon promotion is only 7 percent after conditioning on year, experience, and job fixed effects. Converting the promotion differences into monetary terms therefore implies that the average signer can expect a 0.48 percent increase in pay from a promotion in a typical year (.069 * .07), while a non-signer can expect a 0.50 percent increase in pay from a promotion (.072 * .07). This assumes that the quality of promotions is equal between signers and non-signers.

three measures are close to zero and not statistically significant. Note that promotions are quite rare, as fewer than 5 percent of employees are promoted each year. For many of these workers, there are not many avenues for career advancement given that they are already near the top of the bureaucracy. Likewise, demotions and transfers are extremely uncommon, as fewer than 2 percent of people are demoted or switch agencies per year. Therefore, vertical or horizontal movements through the government do not explain the salary gaps seen in Tables 2 and 3.

B. Discretionary Pay Changes

What explains the differences in salary if it cannot be explained by compositional or hierarchical shifts? In this section, I explore whether the observed effects are operating through base wage changes or discretionary bonus pay.

I first examine whether pay differences exist in terms of workers' hourly pay rate. Public-sector workers are often paid via rigid salary schedules (Finan, Olken, and Pande 2017). Wisconsin is similar in this regard, placing people within specified pay ranges using an hourly base rate, even if the person is a full-time, salaried employee. Table 5 column 1 shows regression coefficients where the outcome variable is the base hourly rate on a logarithmic scale. As seen in panel B, there exists a significant effect of about 1.0 percent for the pooled sample. This suggests that there was some ability to influence pay within the salary schedule ranges, though it seems this was not the primary channel through which the pay gaps emerged.

Instead, I find that the main effects for the unclassified sector and classified administrators are largely due to differences in bonus pay above what would be predicted given the base pay rates. More specifically, I define a measure of residual bonus pay as the difference between someone's gross annual salary and their predicted salary, defined as the base hourly rate multiplied by a standard work year plus any overtime pay.[23] Table 5 column 2 reports results where the outcome variable is an indicator for a person earning any residual bonus pay in a given year greater than 0.5 percent of their total salary. In panel A, there is a null effect on the bonus pay measure for classified non-administrators. Despite the rigid salary schedules in the classified sector, the average rate of residual bonus pay receipt is

[23]The predicted salary also includes lump sum merit compensation as discussed in the next section. In unreported results, I find no differences in overtime pay by signing status.

still significant for this group as 27 percent of classified non-administrators received bonus pay in a given year. Turning to panel B, there exists a significant 3.1 percentage point gap by signing status for the pooled sample of unclassified workers and classified administrators, corresponding to a 15 percent difference given a mean rate of bonus receipt of about 21 percent. This suggests that, when discretion over pay was possible, it was used to reward non-signers relative to signers in the post-disclosure period. One interpretation of the bonus pay difference is that it reflects differences in productivity, a point I turn to in the next section.

C. Alternative Explanations

The prior evidence shows that a pay gap emerges for petition signers, largely because of differences in residual bonus pay. While the analysis points to the decision to sign the recall as the causation for the salary gap, a competing explanation is that signers became discouraged with the Walker administration and exerted less effort, specifically during the post-disclosure period. Under this rationale, signers were not retaliated against for signing per se, but rather paid according to their output. Alternatively, given the endogeneity of signing the petition, perhaps signers differed on some unobservable characteristic such as skill or quality. Next, I examine these possibilities, but fail to find evidence supporting either conclusion.

Differential Performance: Measures of effort or productivity are usually difficult to capture in the public sector, especially since state bureaucrats work across a wide range of occupations. In Wisconsin, the closest measure of performance is the discretionary merit compensation (DMC) program, which provides bonuses of "recognition for superior or meritorious performance." A form of DMC existed from 2006 to 2008 but was discontinued from 2009 to 2011. The Walker administration brought back DMC in 2012. In addition to the lack of pre-2012 data, this is a relatively weak measure for effort given that it only informs us of workers on the high end of the performance distribution. Despite those caveats, the DMC program provides the closest proxy for effort, given that workers across departments and occupations can receive the award.

Table 5 column 3 examines whether DMC receipt differed by signing status.[24] Given the

[24] DMC came in two forms: lump sum and base pay increases. In this table, the outcome variable is whether an individual received any DMC in year t.

gap in the DMC program from 2009–2011, I use the base difference-in-differences estimator but on a sample of people who worked in the state government in both the 2006–2008 period, as well as the 2012–2017 period. The coefficients are near zero and insignificant for both the unclassified/administrators sample and the classified non-administrators. While only suggestive, the lack of differences in DMC pay is not consistent with notions of effort as the driving factor for the salary gaps, despite this compensation program being discretionary by definition.

Differential Quality: The other primary concern revolves around the endogeneity of signing the petition, though, reassuringly, there are no differential pre-trends in gross pay leading up the release of the petition list. Table 1 showed that there existed baseline differences in observables by signing status, which motivated the use of the coarsened exact matching. Nevertheless, one might be concerned that the sample still remains unbalanced on unobservables such as quality.

To address this concern, I examine proxies for quality in a sample of unclassified attorneys, who worked as either public defenders or attorneys general. The State Bar of Wisconsin and Wisconsin Court System maintain records of attorneys qualified to work in the state, which include the law school the attorney attended and whether the person faced any disciplinary history (State Bar of Wisconsin n.d., Wisconsin Court System n.d.).[25] In Appendix Table A9, I examine whether the 586 attorneys in the data differed on proxies for quality by signing status. In row 1, signers and non-signers attended similarly ranked law schools, with signers attending schools ranked 67th and non-signers attending schools ranked 72nd on average (where 1 is the top ranking). There were also no mean differences in the number of years since the attorney graduated law school (row 2) or in the likelihood that the person attended one of the two Wisconsin law schools, UW-Madison or Marquette University (row 3). Finally, the last two rows report whether there were differences in the likelihood that the attorney had a public disciplinary record either before the 2012 petition disclosure (row 4) or after it (row 5).[26] Examples of public disciplinary records are either receiving a public reprimand or a suspension of one's license. Fewer than 2 percent of attorneys had some sort of discipline record, but the mean rate did not differ by signing status.

[25] See data appendix for more details.

[26] I show both before and after as it could be possible that discipline would be an outcome variable in this context.

High-Earner Comparison: Thus far, I have shown that unclassified signers were paid less following the release of the petition list, while no pay difference appeared for non-administrators within the classified service. One possibility is that the pay gap difference seen for unclassified workers was not due to the civil service scheme directly, but rather a characteristic correlated with the civil service classifications. For instance, Appendix Table A2 established that unclassified workers were paid substantially more on average. Hence, perhaps it was not the classification per se, but the fact that an individual worker was highly paid. This explanation would also accord with the Table 3 results showing that the pay gap by signing status was present for classified administrators, which are also highly-paid positions. To address this critique, I re-examine the results with a subsample of non-administrators who earned above median pay in 2011, presented in Appendix Table A10. The results are very similar to Table 2 as there is no mean difference for high-earning professionals within the classified sector (panel A column 4). Conversely, a statistically significant pay gap is present across all specifications for unclassified professionals (panel B). It is hard to shut down all other characteristics correlated with the civil service groups given that entire departments and specific positions are unclassified, but these findings imply that it was not simply high earners who were targeted.

Role of Party Affiliation: Lastly, one might think that the pay gap is not due to signing the petition, but rather to the person's chosen political party. For instance, perhaps employees were flagged as political supporters or opponents through another process, such as voter registration records or campaign contributions, and were rewarded or punished on this margin. In this final analysis, I examine whether government workers who revealed their preference for Democratic or Republican politicians via other means were differentially affected. If Republicans who signed also were paid less, then this would provide additional evidence that the pay gap was because of a deliberate effort to reward people who did not sign the petition. Unfortunately for the purpose of this research, Wisconsin does not require voters to register with a certain political party when voting, which would be the most suitable test of this mechanism. Instead, I use the Database on Ideology, Money in Politics, and Elections (Bonica 2016) to match individuals in my sample to whether they donated to political campaigns.[27] Specifically, I use the subset of government workers who donated to any federal or Wisconsin state campaign that identified with either the Demo-

[27] See the data appendix for details of this data.

cratic or Republican parties from 2000 to 2010. The downside of this approach is that few state workers donated according to my matching technique; I estimate that about 5 percent of workers contributed to a political campaign over this period. Unsurprisingly, Democratic contributors were significantly more likely to have signed the petition: I estimate that 70 percent of Democratic contributors and 30 percent of Republican contributors signed, respectively.

Appendix Table A11 presents the salary results separated by whether someone contributed to a Republican or Democratic politician over the preceding decade. In column 1, the salary gap for people who had previously donated to Republican campaigns is about 2.4 percent, which is significant at the 10 percent level. Column 2 shows a more modest point estimate of 1.1 percent for Democratic contributors. Finally, in column 3, I pool the two samples and report the results from a triple differences estimation where I interact the *Signed x Post* indicator with an indicator for someone having donated to a Republican campaign. The triple differences coefficient is negative, aligning with the results from the first two columns, but not statistically significant, indicating that neither group was differentially affected. Therefore, I do not find that the effect of signing the petition varied significantly by party status, though it is hard to draw definitive conclusions from this exercise given the small set of workers who donated to political campaigns. Nevertheless, this provides supporting evidence that the post-2011 pay gap was a direct result of signing the petition rather than some alternative political mechanism that may be correlated with signing.

Unaffiliated Agencies: I have shown that the results are not explained by proxies for quality, effort, or political party affiliation. Therefore, the most likely explanation for the salary gaps appears to be that the Walker administration deliberately incorporated the petition lists into personnel decisions. Another suitable test of this mechanism would be if there were parts of the state government that did not fall under the umbrella of the Walker executive administration. In support of this conclusion, in Appendix Table A12 I examine salary differences within the Department of Public Instruction (DPI), an agency which, at the time, was led by Superintendent Tony Evers, who publicly acknowledged that he signed the recall petition.[28] There is no negative salary difference for signers post-disclosure in

[28] See https://jsonline.com/news/wisconsin/walker-says-recall-wont-sway-school-chief-endorsement-os83n1n-184176821.html/. Unlike with the heads of most state agencies, the superintendent is an elected position.

the DPI; in fact, the coefficient for all workers in column 4 is positive though insignificant. This evidence is only suggestive given that these results are from a single agency, but it accords with the idea that the DPI staff were unaffected given that they worked under an administrator who was not aligned with the governor.

VI. Broader Effects

The preceding analysis found that the post-disclosure salary differences were not explained by compositional changes. However, it may also be the case that signing the petition affected the flow of workers in and out of the state service. In general, this is a more difficult question to answer conclusively, given that I do not see the reason why someone left the workforce and I cannot see who applied for a job into the state service. Nevertheless, in this final analysis I examine measures of exit and entry from the workforce, as the trends show suggestive patterns that signing may have played a role.

First, I examine whether exit from the government differed by signing status. Because I cannot directly observe whether a person quit voluntarily or was fired, as a coarser measure, I define an exit as the last year a person appears in the data. In Figure 4, I plot Kaplan-Meier survival functions separated by signing status, where an event is defined as the year an individual exits the workforce.[29] The x-axis displays the number of years relative to 2011. In panel A, I plot the survival functions for unclassified workers. It does not appear that unclassified workers exited the workforce at a differential rate, as the two distributions are quite similar.

In panel B, I plot the corresponding distributions for the classified administrators. Here, there is a significant gap with signers being more likely to exit the workforce relative to non-signers. More precisely, the log-rank test for equality rejects that the two functions are the same at the 5 percent level. Again, it is not entirely clear whether this difference is a result of forced exits or voluntary quits, though in theory the civil service protections should protect workers from being fired. In Appendix Figure A5, I report the survival functions for the classified non-administrators; as with the unclassified group, no differences exist by signing status.

[29]For the survival function analysis, I do not restrict the sample to those working more than 20 hours a week. This is required because someone who leaves early in calendar year t will appear to have worked very few hours if estimated over an entire year.

Whether signing affected the hiring process is more challenging to measure. I only observe people who signed the petition in 2011; hence, people who are hired after 2011 may not have been living in Wisconsin for the petition drive. Moreover, there may have been fewer applicants amongst signers given their opinions on the Walker administration. With those caveats in mind, I plot the raw number of new hires into the government workforce by signing status in Appendix Figure A6.[30] Prior to 2012, the trends of new entrants for signers and non-signers are very similar. However, starting in 2012, the trend line for signers flattens out, while hires of non-signers continue to trend upwards. This strongly suggests that signing affected the entry process as well, but again, it is difficult to say whether this was a demand- or a supply-side effect.

VII. Conclusion

In this paper, I examine how the release of the 2011–2012 Wisconsin recall petition signatures affected signers' future career earnings within the state workforce. In theory, most of the civil service should have been protected from political retaliation, given the state civil service statute and rigid salary schedules. By and large, the lack of a post-signing salary gap in the classified sector indicates that the civil service regulations worked as intended. In contrast, non-signers in the unclassified sector received 3 percent more in annual earnings relative to signers following the release of the recall petition list. One exception within the classified sector was for administrators near the top of the government hierarchy, where a 2–3 percent pay gap emerges between signers and non-signers. This implies that affected workers either lacked civil service protections or were in high-level management positions. The main salary effects are not driven by compositional changes or job movements in the post-disclosure period. Moreover, the results are not consistent with measures of worker effort or quality. Rather, it appears non-signers were more likely to receive discretionary pay increases, implying that workers were rewarded for not signing the petition.

Seemingly, the findings show that classified workers were insulated from retribution. And

[30] I use an alternative measure of signing in this plot because the work counties and residential information are more likely to be dissimilar the farther one is from 2012. Instead, I use a more liberal estimate of signing, denoting an entrant as a signer if their name appeared on either of the two petition lists. I then drop anyone whose name is among the more common names in Wisconsin as described in section IV. The plot looks similar when taking a more conservative approach and dropping any names that appear more than once on the petition lists.

yet, the evidence suggests that classified workers near the top of the bureaucracy were negatively impacted. One conclusion is therefore that the rank-and-file civil service at lower levels of the government were simply too far from the top of the executive branch to merit consideration. On the other hand, recent research in Brazil finds patronage extending to many different occupations across all levels of the public bureaucracy (Colonnelli, Prem, and Teso 2020; Akhtari, Moreira, and Trucco 2022), implying that perhaps effects would have been more severe in the absence of civil service protections.

The 2011–2012 Wisconsin recall drive and subsequent election was a unique political event, but the paper's findings have broader implications given that recent legislation at both the state and federal levels has moved to remove civil service protections for certain employees. The evidence presented here suggests that this may have negative consequences for employees whose political views do not align with the administration in power. From a broad perspective, the limitation of this paper is that it does not speak to whether the common, two-tiered classification system is optimal or to which positions should be covered under which classification scheme. It is possible that a certain amount of executive discretion is warranted to efficiently implement the government's objectives, even if it results in personnel decisions that can be influenced by politics. However, the results imply that civil service protections still serve a purpose, as classified rank-and-file workers were not retaliated against, while unclassified professionals experienced reprisals.

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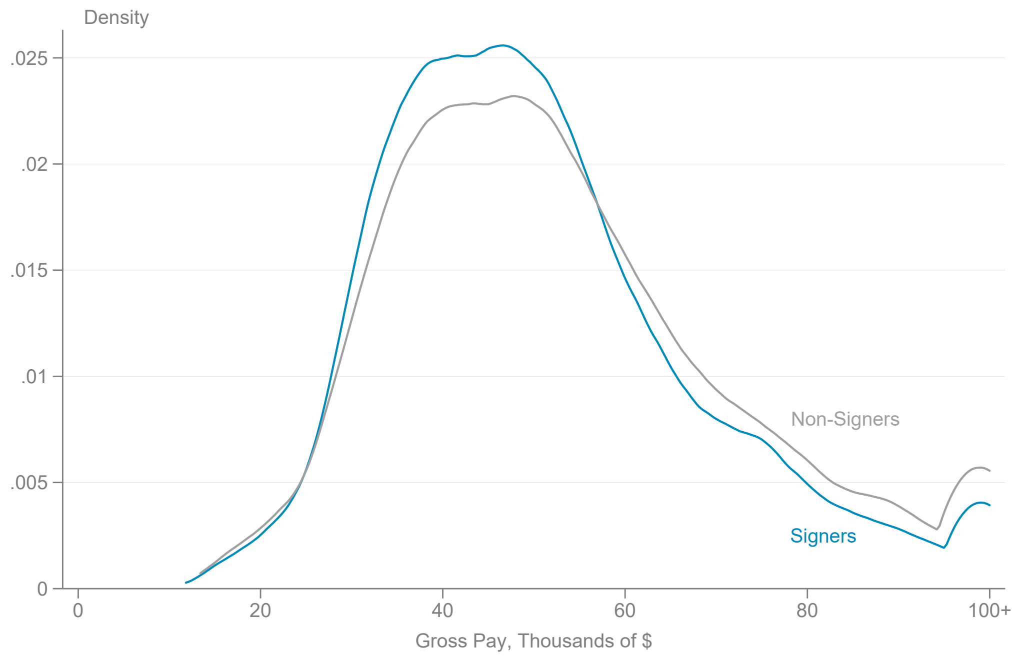
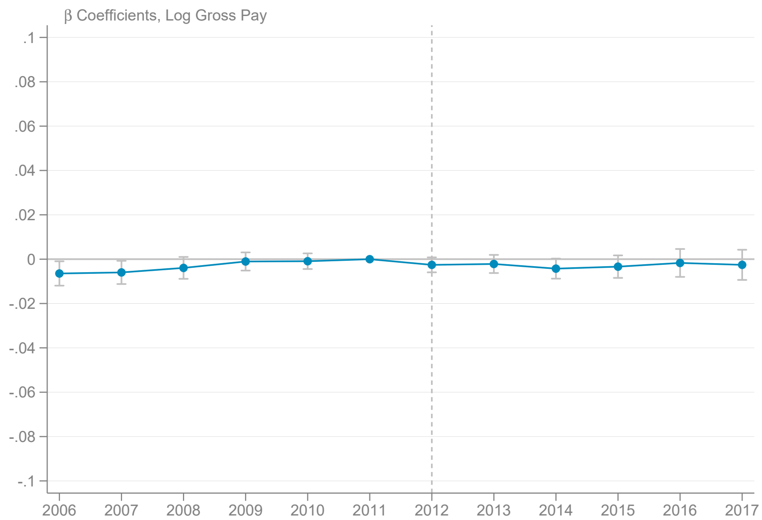
Figure 1: Annual Gross Pay Kernel Densities by Signing Status

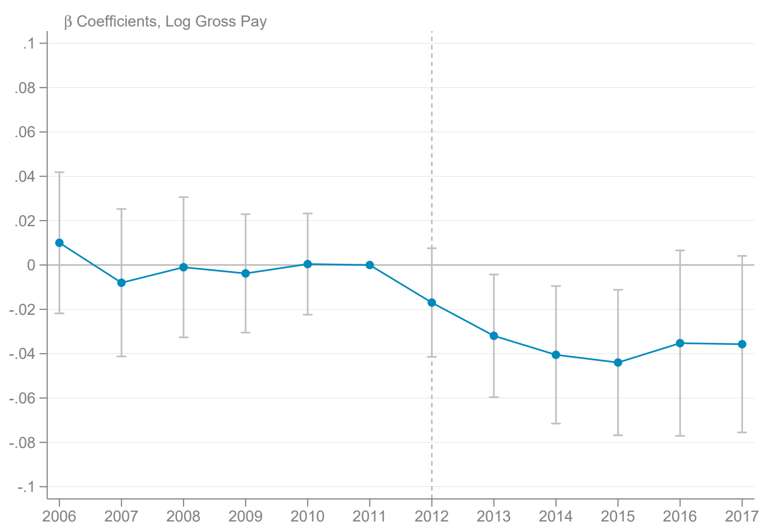


Note: The figure plots kernel density estimates of annual gross pay in 2011. The blue line is the density for signers; the gray line is the density for non-signers. The x-axis is annual gross pay in thousands of dollars. The data in this plot is winsorized at $100,000 for clarity.

Figure 2: Gross Pay Event Studies by Classification Status



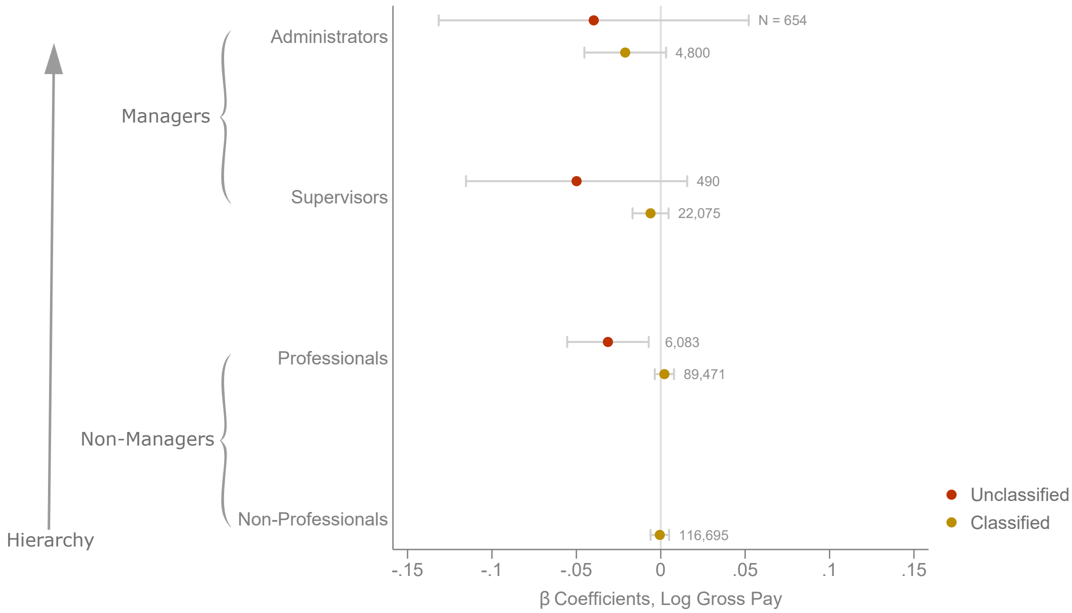
(a) Classified Employees



(b) Unclassified Employees

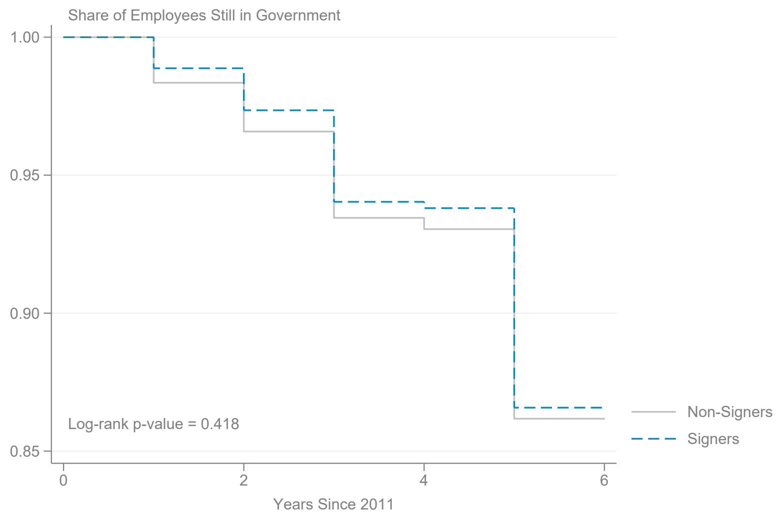
Note: The dependent variable is *log gross pay*. Panel A uses the sample of classified service employees in 2011; Panel B uses the sample of unclassified service employees in 2011. Both specifications include individual fixed effects, baseline job fixed effects and years of experience bins interacted with year fixed effects, and are weighted using the CEM matching procedure as in Table 2 column 4. The coefficients are normalized relative to 2011, the year before the disclosure of the petition list. The dashed line represents the year the petition list was released. The gray bars represent 95 percent confidence intervals based on standard errors that are clustered by individual.

Figure 3: Heterogeneous Effects by Civil Service Scheme

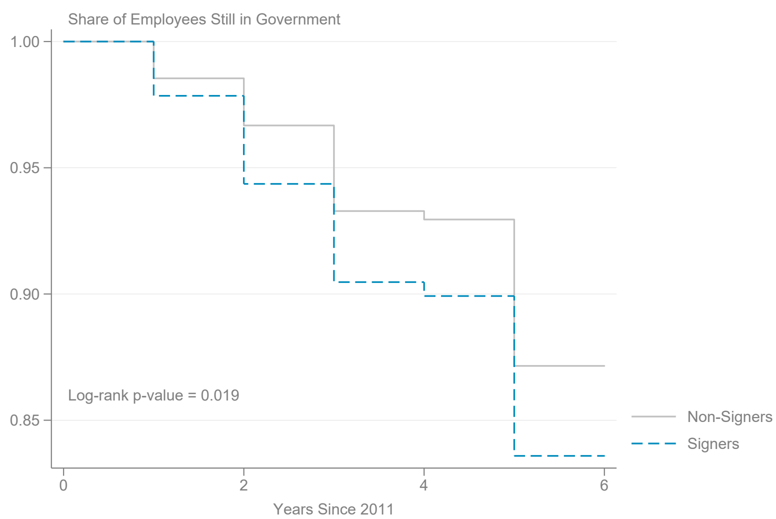


Note: The figure plots regression coefficients broken out by employment type and civil service classification scheme. The regressions use CEM weights and baseline controls as in Table 2 column 4. The plot is arranged from top to bottom to match the hierarchical structure of the government bureaucracy. The *administrators* row is restricted to people who were in the administrators/officials EEO job classification code in 2011. The *supervisors* row includes people who worked in a supervisory or management role, but were not members of the top-level administrators group. The *professionals* group includes members of the professionals EEO code who did not work in a supervisory or management capacity. The *Non-professionals* category includes all remaining workers. The number of observations are listed to the right of the coefficients. The gray bars represent 95 percent confidence intervals based on standard errors that are clustered by individual.

Figure 4: Survival Functions



(a) Unclassified Employees



(b) Classified Administrators

Note: The figure plots Kaplan-Meier survival functions by signing status. The y-axis displays the share of state employees still in the Department of Administration data in year t. The x-axis is the number of years since 2011, the year before the disclosure of the petition list. Panel A uses the sample of unclassified employees; Panel B uses the classified administrators sample.

Table 1: Descriptive Statistics

	Non-Signers			Signers				
	N	Mean	SD	N	Mean	SD	Difference	p-value
Gross Pay (000s)	11,472	54.42	24.51	13,089	51.57	20.89	-2.852	<.01
Years Experience	11,472	12.96	9.16	13,089	13.80	9.25	0.843	<.01
Union Coverage	11,472	0.79	0.41	13,089	0.87	0.33	0.081	<.01
Classified	11,472	0.95	0.22	13,089	0.97	0.16	0.022	<.01
Administrator	11,472	0.04	0.20	13,089	0.02	0.13	-0.027	<.01

Note: This table displays differences in average baseline characteristics by signing status using the Department of Administration data in 2011, the year before the petition list disclosure. *Gross Pay (000s)* is the annual gross salary in thousands of dollars. *Years Experience* is the number of years someone has worked for the state. *Union Coverage* is an indicator variable for being employed in a job classification that was represented by a union. *Classified* is an indicator variable for an employee being a member of the classified civil service. *Administrator* is an indicator variable for an employee being a member of the officials/administrators job classification code. N, Mean, SD represent the number of observations, mean, and standard deviation, respectively. The “Difference” column shows the difference in means between the two groups and the “p-value” column shows the associated p-value for the difference in means.

Table 2: Annual Pay Differences by Job Classification Status

	(1)	(2)	(3)	(4)
	Log Gross Pay			
Panel A: Classified Employees				
Signed	-0.040			
	(0.004)			
Signed x Post	-0.002	-0.005	0.0001	-0.00004
	(0.003)	(0.002)	(0.002)	(0.002)
Observations	240,430	240,430	237,091	233,668
Panel B: Unclassified Employees				
Signed	-0.016			
	(0.029)			
Signed x Post	-0.070	-0.049	-0.031	-0.032
	(0.018)	(0.014)	(0.011)	(0.011)
Observations	8,567	8,567	8,180	7,230
Year FEs	Yes	Yes	Yes	Yes
Individual FEs	No	Yes	Yes	Yes
Controls	No	No	Yes	Yes
CEM	No	No	No	Yes

Note: The dependent variable in all regressions is *log gross pay*. Column 1 includes year fixed effects, column 2 adds individual employee fixed effects, and column 3 adds controls for 2011 job title and 2011 years of experience bins interacted with year fixed effects. Column 4 includes the baseline controls from column 3 and uses the coarsened exact matching algorithm from Iacus, King, and Porro (2012), exactly matching on union coverage, agency, and EEO job code, and coarsely matching on years of experience. Panel A is restricted to employees who were classified in 2011; Panel B is restricted to employees who were unclassified in 2011. Standard errors are clustered by individual.

Table 3: Annual Pay Differences, Classified Administrators

	(1)	(2)	(3)	(4)
	Log Gross Pay			
Signed x Post	-0.030	-0.028	-0.027	-0.021
	(0.013)	(0.011)	(0.011)	(0.012)
Observations	5,620	5,620	5,248	4,800
Year FEs	Yes	Yes	Yes	Yes
Individual FEs	No	Yes	Yes	Yes
Controls	No	No	Yes	Yes
CEM	No	No	No	Yes

Note: The sample is restricted to employees who were in the classified service and members of the administrators/officials EEO job classification code in 2011. The dependent variable in all regressions is *log gross pay*. Column 1 includes year fixed effects, column 2 adds individual employee fixed effects, and column 3 adds controls for 2011 job title and 2011 years of experience bins interacted with year fixed effects. Column 4 includes the baseline controls from column 3 and uses the coarsened exact matching algorithm from Iacus, King, and Porro (2012), exactly matching on union coverage, agency, and EEO job code, and coarsely matching on years of experience. Standard errors are clustered by individual.

Table 4: Hierarchical Changes

	(1) Promotion	(2) Demotion	(3) Transfer
Panel A: Classified Non-Administrators			
Signed x Post	-0.003	-0.00003	-0.0005
	(0.002)	(0.001)	(0.001)
Observations	209,449	209,449	208,897
Mean	0.072	0.012	0.013
Panel B: Unclassified/Administrators			
Signed x Post	0.003	-0.0004	0.001
	(0.009)	(0.004)	(0.004)
Observations	11,460	11,460	11,393
Mean	0.044	0.010	0.016
Year FEs	Yes	Yes	Yes
Individual FEs	Yes	Yes	Yes
Controls	Yes	Yes	Yes
CEM	Yes	Yes	Yes

Note: The dependent variables in columns 1, 2, and 3 are an indicator variable for whether an employee was promoted, an indicator variable for whether an employee was demoted, and an indicator variable for whether someone transferred to a different state agency, respectively. See data appendix for details. All regressions include individual fixed effects, baseline controls interacted with year effects, and are weighted using the CEM matching procedure as in Table 2 column 4. This sample does not use data from 2006, as it is unclear what position someone was in before then, given that 2006 is the first year of the dataset. Panel A is restricted to employees who were classified but not in administrative positions in 2011. Panel B is restricted to the pooled sample of workers who were either unclassified in 2011 or members of the administrators/officials EEO job classification code in 2011. Standard errors are clustered by individual.

Table 5: Base vs. Discretionary Pay Changes

	(1) Base Pay, Log Scale	(2) Bonus Pay	(3) Merit Award (DMC)
Panel A: Classified Non-Administrators			
Signed x Post	-0.001	0.003	-0.002
	(0.001)	(0.004)	(0.002)
Observations	228,868	228,868	150,580
Mean	3.135	0.272	0.067
Panel B: Unclassified/Administrators			
Signed x Post	-0.010	-0.031	-0.009
	(0.005)	(0.017)	(0.009)
Observations	12,544	12,544	7,807
Mean	3.693	0.210	0.067
Year FEs	Yes	Yes	Yes
Individual FEs	Yes	Yes	Yes
Controls	Yes	Yes	Yes
CEM	Yes	Yes	Yes

Note: The dependent variable in column 1 is the base hourly rate on a logarithmic scale. The dependent variable in column 2 is an indicator variable for whether an employee received gross pay exceeding what would be predicted from their hourly pay rate. This variable is independent of any overtime or discretionary merit award pay. The dependent variable in column 3 is an indicator variable for whether an employee received any discretionary merit compensation (DMC) awards in that year. See data appendix for details. Panel A is restricted to employees who were classified but not in administrative positions in 2011. Panel B is restricted to the pooled sample of workers who were either unclassified in 2011 or members of the administrators/officials EEO job classification code in 2011. All regressions include individual fixed effects, baseline controls interacted with year effects, and are weighted using the CEM matching procedure as in Table 2 column 4. Column 3 does not use data from 2009–2011, as there were no discretionary merit awards in those years. Furthermore, I restrict the sample in column 3 to only those workers who were present in both the 2006–2008 and 2012–2017 periods. Standard errors are clustered by individual.