WAGE HYSTERESIS AND ENTITLEMENT EFFECTS: THE PERSISTENT IMPACTS OF A TEMPORARY OVERTIME POLICY[*]

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January 6, 2025

Abstract

This paper studies the unexpected retraction of a U.S. federal policy in 2016 that would have more than doubled the "overtime exemption threshold" from $455 to $913 per week and thereby grant overtime protection to an additional 20 percent of salaried workers. Although the policy was blocked by a federal court injunction a week before it was supposed to take effect, I show that it nevertheless had a persistent positive impact on workers' earnings. Leveraging a bunching design with administrative payroll data from ADP, I find that employers raised workers' salaries to the $913 threshold even after the policy was repealed. Over the following 18 months, difference-in-difference estimates reveal that employers did not slow the wage growth of workers affected by the policy relative to those already earning above $913 per week, nor did they hire new employees at a lower pay rate. Real wages remained persistently elevated relative to what they would have been absent the policy and separation rates decreased among workers bunched at the $913 threshold. Comparing highly exposed firms to unaffected firms, I find an increase in employers' wage bill but no change in aggregate employment. Taken together, the results indicate that temporary policies impacting wage levels can have permanent impacts on the labor market. Survey responses collected by the Department of Labor suggest that morale concerns play a key role in driving the wage hysteresis.

JEL codes: D63, E64, J31, J38

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1 Introduction

Conventional models of the labor market commonly assume that the effects of wage policies, like the minimum wage, can be easily reversed once the rule is retracted. However, such policies may do more than simply alter the constraints faced by employers and workers; they can also establish reference points that shape perceptions of fairness and influence long-term wage-setting behavior. Concerns about fairness, morale, and reputation may make it costly for firms to lower wages even after a policy is lifted. For instance, laboratory experiments have found that the temporary introduction of a minimum wage can permanently raise workers' reservation wages, even after the removal of the wage floor (Falk, Fehr and Zehnder, 2006). Outside the lab, it is still an open question whether rescinded wage policies in the real world generate similar wage hysteresis and entitlement effects.

In this paper, I study the labor market response to a temporary overtime policy and show that its wage and employment effects persist well after the policy is no longer legally binding. Empirically evaluating the effects of transient wage policies has proven difficult for at least two reasons. First, outside a laboratory setting, researchers are faced with a lack of variation as it is often politically unfavorable to repeal a policy that raised workers' earnings. Second, even in laboratory settings, it is impractical to measure outcomes over a sufficiently long time horizon to capture dynamic adjustments in wage growth, and changes to the composition and wages of new hires.

To overcome prior empirical challenges, my paper leverages a natural experiment from the unexpected termination of a major overtime reform that was intended to increase workers' salaries. In May 2016, the U.S. Department of Labor announced that starting December 2016, employers would have to monitor the hours of all salaried workers earning less than $913 per week and pay them a 1.5 times overtime premium for each hour they work above 40 in a week. This reform would have more than doubled the "overtime exemption threshold" from its previous level at $455 per week and was expected to affect about 20% of all salaried

workers. In anticipation of the policy change, many employers announced plans to raise workers' salaries to the $913 overtime exemption threshold. However, one week before the policy was supposed to go into effect, a federal court ordered an injunction on the new rule, effectively nullifying it. Since the policy was never binding, traditional models of the labor market predict that it should have no impact on wages or employment. On the other hand, if the $913 threshold permanently raised workers' reservation wages, employers may increase workers' salaries as promised to avoid reducing morale.

Analyzing anonymous monthly payroll data from ADP that covers a tenth of the U.S. labor force, I document four results. First, I show that employers raised workers' salaries and bunched them right above the $913 overtime exemption threshold, even when it is not legally binding. While firms started raising salaries five months before the policy's intended start date, I find that the largest increase in the share of workers earning precisely $913 per week occurred the month after the policy was already nullified. The median "bunched worker" received a 7% increase in their weekly income. This bunching persisted for over 18 months after the injunction of the policy, and nearly no bunched workers experienced a pay cut during that time. The absence of downward wage adjustments indicates that the hysteresis is not simply due to employers gradually learning about the injunction or awaiting the court's final decision in June 2017. Instead, the sustained bunching suggests that employers are unable to cut nominal wages even when there are no policy constraints.

Second, I find that employers do not slow down the wage growth of bunched workers in the year after their pay raise. To test whether firms offset the initial raise by slowing future wage growth, I use workers earning above the $913 threshold as a counterfactual. Applying a difference-in-difference design, I show that the wages of both groups followed similar trends before the policy announcement, but only bunched workers received a sharp pay increase in December 2016. Afterwards, both groups again exhibit similar wage trajectories in the following year. The rigidity in wage growth implies that not only are nominal wages downward rigid, but so are real wages. To explain the persistence in wage growth, I compare workers

within the same firm and find that in the year after the repeal of the policy, bunched workers tend to receive the same pay increase as the modal coworker in their firm. The similarity in wage growth within-firm suggests that employers have difficulty reversing the impact of temporary wage policies due to frictions against individualized pay increases.

Third, I show that firms likewise continued bunching the salaries of new hires at the $913 threshold after the overtime policy was nullified. To determine whether the bunching of entry wages reflects a real pay increase or simply a change in the composition of new hires, I compare the characteristics of workers hired at the $913 threshold before and after December 2016 relative to those hired above the threshold. My difference-in-difference estimates indicate that workers hired at the $913 threshold after December 2016 had lower salaries at their previous jobs compared to earlier cohorts. Given that employers are hiring workers with lower past salaries, the results suggest that the bunching reflects a real wage increase rather than positive selection of new hires. The observation that firms apply the $913 threshold to both new hires and incumbents further supports the idea that internal pay scales constrain firms from lowering wages over time.

Fourth, I show that monthly separation rates among workers who received a raise to the $913 threshold decreased relative to unaffected workers, suggesting that the quality of workers' jobs improved. Despite the decrease in separations, I find no significant impact on net employment levels. To identify aggregate employment effects, I leverage variation in firms' exposure to the policy driven by their baseline share of workers earning a salary below the $913 threshold. Comparing highly exposed firms to unexposed firms, I find that the temporary policy raised labor costs by 2% but had no significant impact on employment. The null employment effect suggests that while temporary policies may introduce downward wage rigidity, this friction does not necessarily reduce employment if firms have market power.

To explain the empirical results, I analyze survey data collected by the U.S. Federal Department of Labor in the year after the injunction of the overtime rule change. I find

that the most common reason employers cited for not reversing pay increases is concern about worker's morale. Using a simple model, I show that morale concerns can generate the empirical findings via two mechanisms. First, incumbents may feel entitled to the new wage so their morale would suffer if they do not receive their promised pay increase. This entitlement effect can explain why employers raised incumbents' salaries to the $913 threshold even though the policy was never binding. Second, workers' morale may depend on not only their own wage, but also on how it compares to that of their peers. These relative pay preferences can explain why firms do not mitigate the initial salary increase by reducing wage growth or new hires' wages. While morale concerns are likely not the only factor affecting employers' response to the policy, they provide a simple explanation for the empirical results that is consistent with the survey evidence.

My paper contributes to four areas of research. First, my paper adds to the growing literature on fairness norms in the labor market. Most related to my study is the paper by Falk, Fehr and Zehnder (2006), which showed in a laboratory experiment that a temporary minimum wage increase can create entitlement effects that permanently impact wages and employment. My study provides a real-world test of this hypothesis that transient wage policies can have persistent effects due to morale concerns.[1] By leveraging a natural experiment, I am able to examine outcomes over a longer time horizon than would be feasible in a laboratory setting, allowing me to study how wage growth and new hires' wages respond over time. The absence of dynamic adjustments along these margins provides new evidence that morale concerns impede wage adjustments to policy retractions, resulting in lasting shifts to the labor market equilibrium.

Second, my paper contributes to the literature on labor market hysteresis. Previous research have found that labor market impacts can persist long after a policy or shock has

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[1]The mechanism that links morale concerns to wages follows from the many empirical studies showing that perceived unfairness in pay practices negatively impacts job satisfaction, turnover, and productivity (Card et al., 2012; Mas, 2006; Breza, Kaur and Shamdasani, 2017; Cullen and Perez-Truglia, 2022; Dube, Giuliano and Leonard, 2019).

ended (Blanchard and Summers, 1986; Miller, 2017; Saez, Schoefer and Seim, 2021). Similar to my paper, contemporaneous work by Huet-Vaughn and Piqueras (2023) likewise find that wages remain elevated following the reversal of a county-level minimum wage increase. While prior studies focused on policies that were in effect for many years, I examine a reform that was blocked before its implementation. This distinction helps to isolate the role of morale concerns from other mechanisms, such as long-run changes in firms' capital and beliefs.

Third, my study relates to the literature on downward nominal wage rigidity. Consistent with existing survey evidence (Campbell and Kamlani, 1997; Kaur, 2019; Davis and Krolikowski, 2023), I provide empirical support for morale concerns as a potential mechanism for wage stickiness. In addition, I add to the ongoing debate on how firms respond when unable to lower nominal wages - whether by cutting new hires' wages (Pissarides, 2009), slowing wage growth (Elsby, 2009), or reducing employment (Gertler and Trigari, 2009). I find evidence against all three hypotheses. My results suggest that even if wages do not dynamically adjust, employment may not necessarily fall if firms possess market power to set wages (Sokolova and Sorensen, 2021; Card, 2022).

Fourth, my paper adds to a literature on the impact of wage and hour laws.[2] My findings reaffirm results from my previous work where I demonstrate that state increases to the overtime exemption threshold raise workers' earnings without reducing employment (Quach, 2024). The key innovation of my current study is that I examine an overtime policy that was never binding, thereby enabling me to test whether firms uphold their promises of pay increases. Given that firms face no binding policy constraints, conventional models of the labor market predict that none of the outcomes I observed in my other paper would occur in this setting. However, consistent with evidence from the minimum wage literature showing that firms seldom take up the subminimum wage for teens due to fairness concerns (Katz and Krueger, 1991, 1992), I find that overtime regulations similarly shape perceptions of a fair wage in the labor market.

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[2]See Brown and Hamermesh (2019) for a review.

The remainder of the paper is organized as follows. In section 2, I explain the history of the proposal in 2016 to expand overtime coverage for salaried workers. Section 3 describes the administrative payroll data from ADP LLC that I use in this study. Sections 4 and 5 present my analysis of wage rigidity for continuously employed workers and new hires, respectively. Section 6 reports the effects of wage rigidity on employment dynamics. Section 7 examines potential mechanisms. I conclude in section 8 with a discussion of the implications of my findings and areas for future research.

2 The 2016 FLSA Overtime Regulation

Under the Fair Labor Standards Act (FLSA), employers in the U.S. are required to record workers' hours and pay them an overtime premium of at least 1.5 times their regular rate of pay for each hour worked above 40 in a week. While nearly all hourly employees are covered under this provision, the FLSA permits employers to exempt salaried workers who primarily perform white-collar duties and earn at least the "overtime exemption threshold". Consequently, firms have incentive to bunch salaried employees' base pay right above the threshold to not only exempt them from overtime, but also to avoid the costs of monitoring their hours. Between 2004 and 2016, this threshold was set at $455 per week ($23,660 per year), or about the 10th percentile of the income distribution for salaried workers in 2016.

On May 18, 2016, the federal Department of Labor (DOL) announced that it would double the FLSA's overtime exemption threshold from $455 to $913 per week ($47,476 per year), effective December 1, 2016. The goal of the rule change was to expand overtime coverage to low-income white-collar salaried workers, such as managers of fast-food restaurants and retail establishments.[3] In preparation for the major reform, the DOL Wage and Hour Division, led

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[3]The new overtime rule was part of a broader federal policy agenda to reduce monopsony power and raise wages (CEA, 2016). Although other worker-friendly labor market policies such as minimum wage increases, regulations on non-compete agreements, and pay transparency measures were introduced during the Obama administration, none coincided with December 2016 or established $913 as an important reference point.

by David Weil, engaged with employers to help them comply with the new rule. As a result, large employers in targeted sectors like retail, fast-food, non-profit, and higher education began making plans in anticipation of the policy change.

However, the announcement of the upcoming regulation sparked legal opposition. On September 26, 2016, twenty-one states sued the Department of Labor in the Federal District Court for the Eastern District of Texas, arguing that such a large increase in the overtime exemption threshold exceeds the authority of the DOL and requires congressional approval.[4] From a review of newspaper articles at the time, I found that the case received little media attention, and the few reports that did cover it advised employers not to expect a ruling before the December 1st deadline.[5] Hence, it was a surprise to employers when Judge Amos L. Mazzant III issued a preliminary injunction on November 22, 2016, just ten days before the rule change was set to take effect, thereby preserving the overtime exemption threshold at $455 per week (State of Nevada et al. v. United States Department of Labor et al., 2016).

Despite initial uncertainty about the future of the policy, it quickly became clear that the $913 threshold was unlikely to ever go into effect. For example, less than two weeks after the injunction, the Society for Human Resource Management (SHRM) published an article noting that "the Justice Department appealed the injunction on Dec. 1, but many believe the Trump administration is unlikely to pursue the appeal" (Sammer, 2016). Consistent with that prediction, the incoming administration nominated Andrew Puzder, a fast-food executive and critic of the new overtime regulation, to be Labor Secretary on December 8, 2016. Although Puzder did not receive sufficient support from the Senate to be instated, the next nominee, Alexander Acosta, commented during his March 22, 2017 confirmation hearing that he believed the overtime exemption threshold should be updated to only around $634 per

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[4]Specifically, when establishing the FLSA during the Great Depression, Congress allowed exemptions for "executive, administrative, and professional" (EAP) employees. Instead of strictly defining those classes of workers, they gave the DOL authority to adjust definitions over time. The plaintiffs argued that while the DOL is permitted to set a salary threshold, it should not be so high that EAP status is determined solely by income rather than job duties.

[5]For example, see *Texas Judge Consolidates Challenges to Overtime Rule* (SHRM Oct. 21, 2016)

week. Ultimately, Acosta was confirmed as Labor Secretary and the DOL officially dropped its defense of the Obama-era rule on June 30, 2017. Thus, at the very latest, employers were certain by July 2017 that the overtime exemption threshold would not increase to $913 per week.[6]

Since the policy is never binding, neoclassical models of the labor market predict it would have no impact. However, anecdotal reports find that some companies promised workers pay raises in anticipation of the policy and chose not to rescind them after the injunction (Simon and Silverman, 2016). Such behavior suggests that morale concerns may constrain employers from reversing planned wage increases. As noted in the SHRM article, "regardless of the future legal status of the DOL regulations, the changes may end up becoming fixed in many organizations simply because many employers are uncomfortable walking them back for fear of damaging employee morale and engagement" (Sammer, 2016). My analysis will test whether the anecdotal evidence is representative of a broader shift in the labor market whereby firms raised workers' salaries and bunched them at $913 per week. Furthermore, since the rule change only targeted a specific segment of the salary distribution, I use jobs already paying above the $913 threshold as a control group to identify whether firms adjusted the future wage growth, composition, or employment of bunched workers.

3   ADP Data

I use anonymous administrative payroll data from ADP LLC, a global provider of human resources software and services for managing employers' payroll, benefits, and taxes. Their matched employer-employee panel records individuals' earnings at a monthly frequency from May 2008 to January 2020 for over a tenth of the U.S. labor force. Previous analyses have found that the ADP data closely matches the age, sex, and tenure distribution of workers in

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[6]The overtime exemption threshold was eventually raised to $684 per week by the Trump administration in January 2020. The Biden administration then increased it to $844 per week in July 2024, but that was again blocked by a Federal judge in Texas in November 2024.

the Current Population Survey.[7]

The dataset provides monthly aggregates of anonymized individual paycheck information including workers' salaried/hourly status, earnings, hours, pay frequency, industry, and state of employment.[8] In addition, the data records each worker's standard rate of pay from their last paycheck in the month. For hourly workers, their standard rate of pay is simply their wage, while for salaried workers, it is their base salary per paycheck. This variable allows me to precisely compute the measure of weekly base pay described in the Fair Labor Standards Act to determine employees' overtime exemption status. Following the Department of Labor's guidelines, I calculate salaried workers' weekly base pay by dividing their salary per paycheck by the number of weeks in the pay period.[9]

I create three samples from the data. First, to study the effect of the reform on the wages of stayers, I construct a sample of all workers continuously employed at the same firm between May 2015 and April 2018. Second, to study the evolution of entry wages, I create a sample of all new hires over the same period. For each new hire, I also merge on the job characteristics from their most recent employer observed in the data so that I can examine how the composition of new hires changed following the 2016 rule change. Third, I create an unbalanced panel of workers to study the impact of the policy on employment dynamics. This last dataset allows me to measure the effect of the policy on incumbents' separation rates and track how employment at firms changed based on the initial share of their workforce affected by the new overtime exemption threshold. In all samples, I restrict the data to continuously operating firms to avoid conflating business creation and destruction with changes in ADP's client composition.

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[7]However, the data under-represents very large firms with over 5000 employees. For a detailed discussion of the representativeness of the ADP data, see Grigsby, Hurst and Yildirmaz (2021).

[8]Consistent with high monitoring costs, hours are only recorded for hourly workers and non-exempt salaried workers. As such, I cannot observe the hours of affected salaried workers prior to the policy.

[9]For workers paid monthly and semi-monthly, the FLSA calculates weekly base pay by first converting the standard rate of pay into an annual salary and then dividing by 52.

4 Persistence in Stayers' Wages

In this section, I examine how the wages of continuously employed salaried workers evolve following the injunction of the FLSA overtime policy in December 2016. I define a worker as salaried based on their status in April 2016, the month before the announcement of the policy. As a benchmark, I compare my main sample to workers who are salaried in April 2014, and continuously employed from May 2013 to April 2016.

4.1 Immediate Effect of the Policy

To begin, I present graphical evidence of the immediate response to the reform. Figure Ia overlays the distribution of weekly base pays in April and December 2016. The figure highlights the stark shift in salaries from between the old and new overtime exemption thresholds to precisely above the $913 cutoff. Besides the bunching at the overtime exemption threshold, there appears to be minimal change to the rest of the income distribution.[10] To visualize the bunching more clearly, Figure Ib plots the change in the distribution between April and December 2016, and compares it to the change in 2014. While the 2014 distribution also shows a rightward shift due to natural wage growth, it does not exhibit the large bunching mass observed during the year of the FLSA reform.[11]

Before analyzing the persistence of the wage increase, I next show that complying with the defunct overtime exemption threshold had an economically significant impact on workers' salaries. Since employers had 7 months to prepare for the rule change and the injunction was issued only 10 days before the policy was supposed to go into effect, many firms already announced pay raises to their employees. If the cost of bunching workers' salaries is small, then firms may have raised employees' salaries simply to avoid the administrative cost of

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[10]Since annual salaries tend to bunch at multiples of $5,000, the density of weekly base pays exhibit periodic spikes along the distribution when plotted using $40 bins.

[11]I choose 2014 as the comparison year because my subsequent analysis uses the evolution of base pays from May 2013-2016 as a counterfactual for changes between May 2015-2018 in the absence of the policy.

changing their plans. However, my analysis suggests that this explanation is unlikely.

To estimate the cost of raising workers' salaries to the overtime exemption threshold, I use a similar identification strategy to Cengiz et al. (2019) by comparing the observed distribution of weekly base pay over time to a counterfactual distribution. Since the 2016 FLSA policy was a federal rule change that affected all states simultaneously, I do not have a clean control state. Instead, motivated by Figure Ib, I use the distribution from years prior to the policy change as a counterfactual to estimate the cost of raising salaries to the \$913 threshold.

Formally, for each year-month t, I compute the effect of the 2016 FLSA rule change and its subsequent injunction using the following equation:

$$\hat{\beta}_{tk} = \left(\bar{n}_{tk} - \bar{n}_{Apr2016,k}\right) - \left(\bar{n}_{t-24,k} - \bar{n}_{Apr2014,k}\right) \tag{1}$$

where $\bar{n}_{tk}$ represents the share of salaried workers in base pay bin k at time t, averaged across all firms in the sample. Standard errors are computed using the delta method and clustered by firm. Intuitively, equation 1 is a difference-in-difference that compares the change in the base pay distribution since April 2016 to the corresponding change over time same number of months since April 2014. A limitation of this identification strategy is that it accounts for seasonal trends that is common across years but not year-specific shocks. While the method can be extended to address year-specific confounders by using the right tail of the distribution as another control, I apply a straightforward cross-year comparison for simplicity and use the right tail as a placebo check.

Figure Ic plots the distribution of $\hat{\beta}_{kt}$ by base pay k for $t = $ December 2016. The figure is equivalent to the raw difference between the two lines in Figure Ib using \$20 bins of base pay. Three results emerge from the graph. First, the missing mass to the left of \$913 extends as far left as \$693 per week, indicating that employers were willing to raise workers' salaries by up to 32% (i.e. $\frac{913-693}{693}$) to comply with the new rule. Second, the median worker in the

missing mass would have earned $853 per week absent the policy change. In other words, the median bunched worker experienced a 7% (i.e. $\frac{913-853}{853}$) increase in their base salaries. Third, as a validation of my empirical strategy, I find no significant effects in either the left or right tail of the pay distribution. Overall, the evidence from Figure Ic suggests that raising salaries to the $913 threshold was a substantial cost for employers, making it unlikely that they did so merely to avoid the one-time administrative cost of readjusting their plans.

4.2 Who Gets Bunched?

While some workers received a large pay increase despite the court injunction, Figure I shows that the majority of workers experienced no change in their weekly base pay. About 20% of salaried workers earned between $455-913 per week before the reform, but only 1% received a raise above the $913 threshold in December 2016. In other words, about 5% (i.e. $\frac{1}{20}$) of all affected workers were bunched. There are two possible reasons for this modest response. First, employers may have been able to cancel their planned wage increases immediately after the injunction. If that were true, it suggests that wages are actually fairly flexible for most firms, and only a small subset are constrained by wage hysteresis. Alternatively, employers did not reverse pay increases but had promised raises for only a select group of workers. In that case, the bunching rejects the benchmark model of wage flexibility, but my results are primarily informative for the "complier" group of workers who received pay increases.

Appendix B presents two sets of analyses to test the above hypotheses. First, I summarize findings from six independent surveys conducted by various business associations and the Department of Labor on employers' responses to the court injunction. Across multiple industries, the surveys show that the limited number of workers bunched above the overtime exemption threshold is partly due to firms opting to cover them for overtime pay instead of raising salaries. In addition, while some firms delay their responses, the majority report that they proceeded with planned pay increases despite the injunction. Second, I benchmark the magnitude of the bunching effect to that of Quach (2024), which studies state-level changes

to the overtime exemption threshold that were actually binding. Even when the policy is binding, only 16% of affected workers received a raise above the overtime exemption threshold, which is remarkably similar to what I observe for the retracted FLSA rule change once I account for differences between the scale of the state and federal policies. Thus, the survey evidence and benchmark comparison suggest that while some firms canceled planned pay raises, the policy remained sticky for the majority of employers and the mild bunching response simply reflects firms' preference to cover workers for overtime than to increase their base salaries.

Given that employers only bunch a small share of affected workers, even in response to binding state policies, Appendix B proceeds to explore the types of jobs that firms select to bunch at the $913 threshold. I find evidence that employers tend to bunch jobs with long or flexible hours. First, survey data suggests the choice to either bunch workers or cover them for overtime is largely a within-firm decision, rather than across-firms, whereby the effect of the policy differs across workers within the same organization. Second, as corroborative evidence that firms are bunching workers who work long hours, I show that both the FLSA and state-level rule changes only had a minor impact on workers' overtime pay. In other words, workers who gain overtime coverage barely work enough hours to receive overtime compensation. Third, using administrative data of public sector employees, I show that employers prefer to bunch occupations for whom flexibility is vital to the job.

As a case study, Appendix B explores heterogeneous responses by job titles in three state university systems: Arizona, California, and Connecticut. I find that employees with job titles containing terms such as "postdoc", "lecturer", and "research" exhibit clear bunching at the $913 threshold after 2016, similar to the outcomes observed in the ADP data. In contrast, I find no bunching among administrative roles. This heterogeneity supports the hypothesis that employers prefer to bunch occupations with flexible hours that are difficult to monitor, such as those in teaching and research positions, but cover for overtime jobs with fixed hours like office administrators.

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4.3 Evidence of Wage Hysteresis

If raising workers' salaries to the overtime exemption threshold is expensive, why did firms not simply tell workers that the new rule was retracted? Laboratory evidence from Falk, Fehr and Zehnder (2006) suggests that temporary policies can create entitlement effects by permanently changing workers' reservation wages. While I am unable to directly measure workers' preferences, I can rule out other hypotheses for why firms behaved as if the policy was still binding.

To start, I consider three simple explanations for why firms raised workers' salaries. First, employers may have been unaware of the injunction and thought the policy was still binding. Second, even if firms knew about the injunction, they may be concerned that the policy would ultimately be upheld in a court appeal. Third, even if employers wanted to avoid raising salaries, they may have needed time to make adjustments - a behavior assumed by staggered bargaining models, where wages are updated periodically rather than immediately in response to economic shocks (Taylor, 1979). A common prediction across all three scenarios is that, in the absence of morale concerns, employers would gradually lower workers' salaries to pre-policy levels as they became informed about the injunction, receive more information about its legal status, and had time to adjust payrolls. In contrast, if the policy introduced entitlement effects, then salaries would exhibit downward rigidity and remain bunched at the $913 threshold.

To test whether wages were downward rigid, Figure II plots the distribution of $\hat{\beta}_{kt}$ (estimated from equation 1) across base pay for four months between January 2016 and December 2017.[12] Four key features stand out. First, I observe little movement in the distribution before the announcement of the new overtime exemption threshold in May 2016. That serves as a useful placebo check to show that my empirical strategy primarily captures the effects of the policy and no other contemporaneous shocks. Second, a bunching mass starts gradually growing at the $913 threshold even before December 2016, consistent with employers prepar-

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[12]To see all 24 months in that time period, refer to Appendix Figure A.I.

ing in anticipation of the rule change and some making early adjustments. Third, the largest increase in the bunching mass occurred in December 2016, after the court injunction. Fourth, although the bunching gradually shrinks throughout 2017, it is still about half its original size by December 2017.[13] The persistence of the bunching mass for over a year suggests that it is unlikely due to a lack of awareness.[14]

Next, I show that the persistence in wages is not driven by concerns about the legality of the new threshold. To test whether the potential risk of lawsuits deterred employers from reducing wages, I use the final court decision in June 2017 as a discrete breakpoint in the legal environment. Figure III plots the evolution of the bunching mass at $913 per week over time and finds that neither the confirmation hearing of Alexander Acosta in March 2017 nor the final ruling on the FLSA rule change in June 2017 had a noticeable impact on its magnitude. Examining the figure from left to right, I highlight three key features. First, the estimated size of the bunching mass is near zero for the entire year prior to the announcement of the FLSA rule change in May 2016. The parallel pre-trends suggests that the identification strategy uses a reasonable counterfactual for the share of salaried workers earning between $913 and $953 per week absent the policy. Second, the share of salaried workers in that pay interval rose by about 1 p.p by January 2017. For comparison, only 2.5% of workers earned between $913 and $953 per week in May 2016, so the policy increased the number of workers within this interval by over 25%. Third, the magnitude of the bunching mass shrank at a constant rate after the policy retraction. The absence of any discontinuous change after key court decisions suggests that the persistence in the bunching is not due to legal uncertainties surrounding the policy.

While the persistent bunching in the pay distribution exists in aggregate, two questions

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[13]Appendix C shows there is still a missing mass by December 2019. However, the bunching disappears by early-2019, consistent with my results in section 4.4 that bunched workers continue to experience wage growth over time.

[14]As further evidence of firms' knowledge of the court ruling, Appendix Figure A.II finds that Google searches for the term "FLSA Overtime" was more popular in November 2016 on the week of the injunction than in May 2016 when the policy was first announced.

remain: is the decline in the bunching due to wage decreases or increases, and is there heterogeneity in wage rigidity across firms and workers? To answer these questions, Figure IV plots the distribution of one-year wage changes for job-stayers who earned between $913 and $953 per week in December 2016. If wage flexibility is heterogeneous across workers or firms, then at least some workers bunched at the $913 threshold should experience a large pay decrease in 2017. Similarly, if staggered bargaining leads to heterogeneity in the timing of wage adjustments, I should also observe a large number of wage cuts over the year.[15] In contrast, Figure IV shows that very few workers experienced a wage cut. Instead, 27% of workers had no change to their base pay in the year following the injunction, and the majority even received raises. The rarity of negative wage changes implies not only little heterogeneity in wage flexibility but also that the decline in the bunching over time is primarily due to raises rather than wage reductions.[16]

The results thus far imply that the persistent impact of the 2016 FLSA rule change is not due to lack of information, concerns over future court appeals, or staggered bargaining. Instead, the evidence points to the presence of downward nominal wage rigidity, potentially driven by entitlement effects. In the remainder of the paper, I examine how employers respond to this rigidity in the long-run. Although firms do not reduce workers' wages back to pre-policy levels, they may adjust labor costs by slowing wage growth, lowering entry wages, or reducing employment. These outcomes relate to a broader debate about the impacts of wage rigidity. In his seminal work, Keynes (1936) theorized that downward nominal wage rigidity inhibits market-clearance during recessions, thereby leading to high unemployment. However, modern theories argue that rigidity in spot wages may have little effect on aggregate

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[15]In Appendix D, I show that while firms tend to adjust wages the same month each year as predicted by staggered bargaining, that behavior does not explain the increase in the bunching mass before December 2016.

[16]A concern with focusing on stayers in Figure IV is that I do not observe workers who leave the firm after a wage cut. To mitigate this bias, Appendix Figure A.III plots the change in base pay over a two-month period for bunched workers who stayed at the same firm from Dec 2016 to Jan 2017. This larger sample captures wage cuts in January 2017, even if workers subsequently left the firm. The figure nevertheless finds no evidence of pay cuts.

employment if firms can slow wage growth over time (Elsby, 2009) or replace incumbent workers with new employees at a lower wage (Pissarides, 2009). Empirically testing for stickiness in either wage growth or entry wages has been challenging since studies often rely on changes in the unemployment rate over time as the key source of variation, making it difficult to identify counterfactual outcomes without strong assumptions. I use the retraction of the 2016 overtime rule as a natural experiment to investigate how employers adapt in the long-run when they cannot cut nominal wages in response to a temporary policy.

4.4 Do firms reduce wage growth?

In this subsection, I test the hypothesis that employers respond to downward nominal wages by reducing future wage growth. My analysis compares workers who likely received a raise due to the 2016 FLSA policy relative to those who were already earning above the new overtime exemption threshold and were less affected by the rule change. I estimate a difference-in-difference regression of the form

$$\log(y_{it}) = \sum_{\tau \neq -8}^{16} \beta_\tau D_{b=1,t=\tau} + \alpha_i + \alpha_t + \varepsilon_{it} \tag{2}$$

where y_{it} is the base pay of individual i at month t, and $D_{b=1,t=\tau}$ is a dummy that equals one for bunched workers τ months since December 2016. I omit April 2016 as the reference period. I control for worker fixed effects α_i, and month fixed effects α_t.

Figure V plots the estimates from my difference-in-difference analysis using two different definitions for the treatment and control groups. To start, I define treatment status based on workers' salaries in April 2016, the month before the policy announcement. Given the results in Section 4.3, the treatment group comprises of salaried workers earning between \$700 and \$913 per week in April 2016, as this is the group most likely to receive a raise to the new overtime exemption threshold. As a counterfactual, I define the control group to be workers already earning between \$913 and \$953 per week. Figure Va plots the average weekly base

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pay over time for these two groups. A clear jump in base pay among the treatment group is not immediately visible because many of the workers earning $700-913 per week in April 2016 were unaffected by the policy.[17] To more closely visualize the impact of the reform, I next plot the regression estimates from equation 2.

The difference-in-difference estimates in Figure Vb suggests that firms permanently increased the salaries of the treated group by 1%. I note four findings from the figure. First, prior to the announcement of the new overtime exemption threshold, there are no differences in pre-trends between workers earning below and above $913 per week. The similarity in pre-trends supports the identification assumption that the wages of treated and control workers would have evolved at the same rate absent the policy. Second, after the announcement of the reform, firms gradually raised workers' salaries, consistent with the preemptive effects in Figure II. Third, there is a sharp increase in workers' salaries precisely in December 2016, the month that the new threshold was supposed to take effect. Fourth, there is no slowdown in wage growth following the injunction of the policy. Even 16 months after the injunction, workers impacted by the reform continue to earn 1% more than they would have had their wage growth stayed on the same trajectory as the control group.

One limitation of defining the treatment and control groups based on pre-policy wages is that it dilutes the treatment group with workers who do not receive a raise from the overtime exemption rule. To focus more closely on workers likely affected by the FLSA rule change, I redefine the treatment group to be salaried workers who earn between $913 and $953 in December 2016, which I will refer to as "bunched workers". I model the counterfactual wage growth of bunched workers using workers who earned between $953 and $993 per week on December 2016, henceforth called "non-bunched workers".

Figure Vc plots the raw evolution of bunched and non-bunched workers' salaries over

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[17]Given the distributions in Figure I, about 14% of all salaried workers earned between $700 and $913 per week in April 2016, but firms only bunched 1% of all salaried workers at the threshold. As a result, only about 7% of the "treatment" group (i.e. $\frac{1}{14}$) actually received a raise to the $913 threshold.

time. Even though some of the bunched workers would have earned $913 per week regardless of the policy, it is apparent from the figure that, on average, bunched workers experienced a large one-time increase in base pay in December 2016. In contrast, workers earning between $953 and $993 per week were unaffected by the nullified FLSA rule change.[18] To visualize changes in wage growth, Figure Vd plots the equivalent difference-in-difference estimates. By focusing on workers likely affected by the policy, I find a larger increase in weekly base pay relative to the analysis in Figure Vb. On average, bunched workers experienced a nearly 3% increase in salary between April and December 2016. While their wages grew a little slower than that of non-bunched workers after the injunction, bunched workers still earn over 2% more than they would have otherwise, even 16 months after the policy was rescinded.

To formally test whether wage growth changed following the injunction, I estimate the difference in wage growth between the treatment and control groups over three periods: pre-announcement, post-announcement but pre-injunction, and post-injunction:

$$\log(y_{it}) = \sum_{p=1}^{3} (\lambda_{0p} + \lambda_{1p} \cdot time) D_{bp} + \alpha_i + \alpha_t + \varepsilon_{it} \tag{3}$$

where $time$ is a continuous time variable and D_{bp} is a dummy that equals one for bunched workers during period p. The index p equals 1 for months prior to May 2016, 2 for months between May and December 2016, and 3 for months after December 2016. The coefficients λ_{1p} therefore represent the difference in average monthly wage growth between treated and control workers during each period. To measure the change in wage growth relative to its levels pre-policy announcement, I also compute the difference $\lambda_{13} - \lambda_{11}$.

Table I reports the estimates of equation 3. The estimates in column (1) show that before the policy announcement, the wages of workers earning $700-913 per week in April 2016 were growing at the same rate as those earning $913-953 per week. During the transition

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[18]While Figure Ic finds a small spillover effect for jobs paying up to $1013 per week, the spillovers are small relative to the bunching mass and do have no significant impact on the diff-in-diff analysis.

period when employers thought the new overtime exemption threshold was coming into effect, the treated group's wages grew 0.13% per month faster than the control group. After the injunction, the two groups again had statistically and economically indistinguishable wage growth. A similar pattern emerges in column (2) when I control for firm-month fixed effects, which compares workers employed within the same firm over time. The results are also robust to the alternative definition of the treatment and control groups. Columns (3) and (4) repeats the analysis with workers earning \$913-953 per week in December 2016 as the treated group, and workers earning \$953-993 per week as the counterfactual. By conditioning on post-injunction salaries, the treatment group is more likely to comprise of workers who benefited from the FLSA rule change, resulting in a larger wage increase from May to December 2016 relative to columns (1) and (2). Nevertheless, even with a refined treatment group, I observe no economically significant change in bunched workers' wage growth following the injunction.[19]

My analysis implies that not only are nominal wages downward rigid, but so are real wages. To illustrate, note that while the Consumer Price Index grew by 2.1% in 2017, the median annual pay increase for workers earning \$913-953 per week in December 2016 was 2.8%, meaning real wages for this group actually went up. However, the median person at the overtime exemption threshold may not be representative of the specific workers who actually received a raise as a result of the overtime policy. While I cannot directly isolate the wage growth of the "treatment-on-the-treated" (TOT) group, equation 3 identifies an "intent-to-treat" (ITT) effect on real wages by using the wages of non-bunched workers as a proxy for inflation. The null ITT effect implies that there was no decrease in real wages among the TOT workers.

Even my largest negative estimates suggest a high degree of wage rigidity. Taking the

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[19]While my results show no slowdown in the wage growth of affected workers relative to unaffected workers, firms may have implemented slower pay raises across all employees. In contrast to that hypothesis, section 6.2 finds that firms heavily impacted by the policy saw a persistent increase in their wage bill relative to firms with few affected workers, suggesting that wage growth was not reduced at the firm level either.

estimates in column (3) of Table I as an extreme benchmark, the analysis implies that the weekly base pay of bunched workers grew by about 0.28 p.p. per month over the 8 month period between May and December 2016. Weekly pay therefore cumulatively increased by 2.3% (i.e. 8 times 0.28). In contrast, after the injunction, bunched workers' salaries decreased by only 0.03 p.p. per month quicker than non-bunched workers. At that rate, it would take over 6 years to eliminate the initial 2.3% raise and even longer to equalize the net present value of wages. In comparison, if employers simply froze the nominal wages of bunched workers, a back-of-the-envelope calculation suggests that it would take less than 4 years to bring salaries back to the same path as before the policy.[20]

While employers may have initially promised workers a raise to the $913 threshold, what explains why firms did not simply keep bunched workers' nominal wages stagnant after 2016? One possibility is that workers became more productive after the wage increase (Katz, 1986; Coviello, Deserranno and Persico, 2022). However, if firms discovered that the productivity gains from higher wages outweighed the costs, then I would expect them to raise the wages of other workers at the firm as well. In that case, the wage gap between treated and control workers would narrow over time, not because the wages of bunched workers have stagnated, but because the wage growth of the control group is accelerating. In contrast, I find no indication that the salary gap between the two groups shrinks even when comparing employees within the same firm over time.

Instead, I find evidence supporting the hypothesis that real wages are rigid due to concerns over horizontal pay equity. If workers have relative pay concerns, whereby effort is negatively impacted by increases in inequality that are perceived as unfair, then employers would want to give similar pay increases to all workers within the same firm. For example, Dube, Giuliano and Leonard (2019) finds that uneven pay raises lead to higher quit rates using non-experimental data from a US employer. To test for internal equity concerns, Ap-

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[20]According to Figure I, about 1 in 3.5 workers earning $913 per week in December were bunched due to the policy. That implies a TOT effect of 2.3% · 3.5 = 8%. At an inflation rate of 2.1%, it would take 3 years and 10 months to eliminate the 8% raise.

pendix Figure A.IV plots the distribution of annual wage increases for bunched workers, relative to the modal wage increase among non-bunched salaried workers at their employer. I find that about 40% of bunched workers receive the same pay raise as their modal coworker and are unlikely to receive a smaller raise than the mode. The stark asymmetry in the distribution suggests that workers care about how their pay increases compare to that of their peers.[21]

5 Persistence in New Hires' Wages

In this section, I investigate how firms adjust the wages of new hires following the injunction of the 2016 FLSA rule change. It is important to distinguish between stayers' and new hires' wages, as it is often rigidity in the latter that determines aggregate employment in standard job-search and bargaining models (Pissarides, 2009).[22] Moreover, whether the bunching extends to new hires provides insights into the causes of wage rigidity. For example, even if incumbents feel entitled to the $913 weekly base pay they were promised, it is unclear that new hires would be bound by the same constraints.

I begin by examining whether firms continue to bunch new hires at the invalidated $913 threshold after the injunction. If the wages of new hires are flexible, then firms would immediately cease hiring excess workers at the new overtime exemption threshold once the policy is rescinded. To examine the bunching of entry workers, Figure VI plots the distribution of base pay for new hires over four months between January 2016 and December 2017, relative to the distribution in April 2016.[23] In anticipation of the policy change, firms are already

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[21]To show that the asymmetry is not simply due to firms that have a modal pay increase of 0, I drop such firms and find similar bunching at the modal pay raise. In general, across all firms in the data with at least 50 employees, I find that 29% of all workers received the same percent pay increase between 2016 to 2017 as their modal coworker.

[22]A notable exception is if firms face liquidity constraints, in which case Schoefer (2021) shows that wage rigidity among incumbent workers can have allocative effects on employment.

[23]Unlike with stayers, I divide the weekly base pay distribution into increments of $96.15 (i.e. $\frac{5000}{52}$) to smooth over firms' tendency to bunch annual salaries at multiples of $5,000 (e.g. $50,000/year). To reduce noise, I also do not compare the distributions to that of previous years. Appendix Figure A.V plots the change in the distribution for every month in 2016 and 2017.

bunching new hires at the $913 threshold in November 2016, when the share of workers hired at the threshold was 3 p.p greater than in April 2016. For comparison, only 9.1% of new hires earned within $96.15 above the threshold in April 2016. While the bunching effect diminishes over time, it still persists until at least December 2017, a full year after the injunction of the threshold and six months after the final court ruling.

There are two potential explanations for the persistence in the bunching mass among new hires: either firms increased entry wages, or they simply hired more productive workers. If the bunching is due to selection, then workers hired at $913 per week in December 2016 should be at least as productive as those hired before the rule change was announced. Conversely, if the bunching reflects real wage growth rather than composition changes, then I would expect firms to hire less productive workers at the new overtime exemption threshold. Since I cannot directly measure workers' productivity, I instead use the salary of new hires at their previous employer as a proxy for productivity.[24]

To test for changes in worker composition, I compare the characteristics of new hires at the $913 threshold over time to that of new hires earning between $953 and $993 per week. My analysis assumes that, absent the rule change, the productivity of workers hired at the threshold would have evolved similarly relative to workers hired above the cutoff. Formally, I estimate

$$log(y_{it}) = \sum_{\tau \neq -2}^{3} \beta_\tau D_{b=1, \tau-1 \leq \frac{t}{6} < \tau} + \alpha_b + \alpha_t + \varepsilon_{it} \tag{4}$$

using monthly cross-sections of new hires. My primary outcome, y_{it}, is the wages of new hire i at their last observed employer. I control for a month fixed effects α_t, and a dummy α_b for workers hired at a weekly base pay of $913-953. The variable $D_{b=1, \tau-1 \leq \frac{t}{6} < \tau}$ is a dummy that equals one for bunched workers hired between $6(\tau-1)$ and 6τ months since May 2016. Given that the sample size of new hires is sparse relative to the number of stayers, I aggregate the data into six-month intervals for statistical power.

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[24]Although wages may fall below marginal revenue productivity if firms have market power, workers' earnings would nevertheless be positively correlated with productivity.

Figure VIIa plots the difference-in-difference estimates over time. Reviewing the figure from left to right, I highlight three points. First, the productivity (i.e. past wages) of workers hired at $913 per week and those hired above the threshold follow the same trend in the months leading up to the announcement of the new FLSA rule change and during the immediate months afterwards when firms have yet to fully respond to the policy. Second, there is a sharp drop in the productivity of workers hired between November 2016 and April 2017, the period when the bunching of new hires was most pronounced. The decline in productivity, measured by the salary of workers at their last place of employment, suggests that the persistence in new hires' wages reflects real wage growth and not simply compositional changes. Third, I find that the productivity of workers hired at the $913 threshold partially recovers at the end of 2017 relative to new hires earning $953-993 per week.

Similar to the case of stayers, the gradual reduction in the bunching mass of new hires may simply reflect natural wage growth causing salaries to increase above the overtime exemption threshold. In the long-run, new hires who would have earlier earned the overtime exemption threshold may be earning above it and thereby included in the control group of the difference-in-difference regression. As a result, I cannot conclude whether the convergence in productivity at the end of 2017 is due to wage growth or if employers are becoming more selective in screening new hires. Instead, I focus on the Nov 2016 - April 2017 estimate as the clearest indicator of employers' response to the reform.

To quantify the increase in new hires' wages, Appendix Figure A.VI plots the difference-in-difference estimates using the percent change in workers' salaries from their previous job as the outcome variable. I find that employers are paying new hires 7% more than they would have had the policy not been announced, consistent with the argument that the bunching reflects real wage growth and not simply changes in composition.

Next, I examine whether firms slowed the wage growth of new employees initially hired at the $913 threshold. While firms continued hiring workers at the overtime exemption threshold after the policy was terminated, they could have offset their labor costs by raising

new employees' wages at a slower rate than if the policy had never been announced. To test this, I estimate equation 4 using the weekly base pay of workers 18 months after their hire date as the outcome. My analysis restricts the sample to individuals who stay with the same employer for all 18 months. To maximize sample size, I include new hires even if I do not have data on their past job's salaries. Figure VIIb plots the regression estimates over time and finds that the difference in wage growth between entrants hired at and above the \$913 threshold was constant over the entire study period. In particular, workers hired in the 6 months starting from November 2016 did not suffer any long-run penalty in wage growth, despite receiving a wage premium when they were initially hired.

Table II summarizes my empirical findings on the rigidity of entry wages. Column (1) reports the difference-in-difference estimates corresponding to Figure VII where I compare the past work history of new hires at and above the overtime exemption threshold. The estimates imply that bunched workers hired between November 2016 and April 2017 earned, on average, 5.1% (s.e. 1.7%) less per week at their last observed job and earned 7.1% (s.e. 2.6%) more from their job transition compared to workers hired above the bunching mass. The analysis thus suggests that firms are hiring less productive individuals to fill jobs bunched at the threshold. To address concerns that the last observed employer in the data may be from the distant past and not a good measure of workers' current productivity, column (2) repeats the analysis using only hires with less than a 6-month gap between jobs. The direction and magnitude of the estimates remain similar to the main specification, albeit the estimates are less precise with the restricted sample. In comparison, column (3) finds small statistically insignificant effects on the wages of bunched hires 18 months after their employment, indicating that initial wage premiums did not lead to long-term wage penalties. Column (4) shows that the magnitude and direction of the estimates are robust to restricting the sample to only workers for whom I can observe both their past and future wages.

The difference-in-difference analysis assumes that the FLSA rule change did not affect the selection of workers hired in the \$953-993 weekly pay range, making these workers a valid

control group for bunched hires. While the reform does not directly target workers earning above the overtime exemption threshold, there may nevertheless be spillover effects elevating wages higher up the distribution. To address concerns that new hires earning right above the overtime exemption threshold are a contaminated control group, Appendix E shows that my results are robust to a regression discontinuity type design using entrants within a wider income interval to infer the counterfactual productivity of workers hired at the bunching mass. Intuitively, even if the policy had spillover effects right above the $913 threshold, these impacts are likely minimal for jobs paid well below and above that cutoff. While the complementary regression discontinuity approach is not without its limitations, which I describe in the appendix, it is nevertheless reassuring that both the difference-in-difference and regression discontinuity analyses reach the same conclusion.

To summarize, although the overtime exemption threshold was nullified, it nevertheless had persistent effects on new hires' wages that cannot be accounted for by changes in worker composition nor by reductions in their future wage growth. While the policy announcement may have prompted firms to make pay promises to incumbents, why did it also affect the wages of new hires? One possibility is that new hires were also aware of the reform and had higher reservation wages as a result. For example, Falk, Fehr and Zehnder (2006) show that temporary minimum wage increases permanently raise the reservation wages of workers who previously benefited from the policy. Alternatively, similar to why employers did not slow down the wage growth of incumbents, new hires may also exhibit relative pay concerns and reduce their effort if they are paid less than their new colleagues. Relatedly, employers may uphold internal pay structures that dictate equal pay for all workers in the same occupation, such as when firms have national wage policies (Hazell et al., 2022). In either case, the results suggest that temporary wage shocks can persist even among new cohorts of workers.

6 Changes in Employment Dynamics

This section examines changes in employment dynamics following the retraction of the new overtime exemption threshold in December 2016. Given that firms do not adjust wages downward after the injunction, employment levels may become more sensitive to negative demand shocks. For instance, Bils (1991) finds that rigid bargaining contracts lead to excessive variability in employment that is partially corrected for after wage renegotiations. To test whether wage hysteresis impacted employment, I estimate changes in worker separations and firm-level employment.

6.1 Effect on Worker Separation Rates

Using a difference-in-differences approach, I compare separation rates between "affected" workers (i.e. weekly base pay of \$700–\$913 in April 2016) and "unaffected" workers (base pay of \$913–\$993). Figure VIIIa plots the probability that each of the two groups are employed over time. By construction, all incumbent workers were employed in April 2016. While affected workers are more likely to separate from their employer after the announcement of the FLSA rule change, this difference in employment rates is true throughout the entire study period. Affected workers are simply less likely to be employed at any point in time. To more clearly visualize the divergence in separation rates, Figure VIIIb plots the analogous difference-in-difference estimates, computed using equation 2. Separation rates exhibit a sharp trend-break in December 2016, the month that the new threshold was supposed to take effect. The precise timing of the kink suggests that separation rates for bunched workers decreased after they received a raise.

To summarize the reduction in separation rates, Table III reports the change in the slope over time, estimated from equation 3. Column (1) shows that between May and November 2016, affected workers separated at a rate 0.2 percentage points higher per month than unaffected workers, but this gap shrinks to only 0.04 points post-December. Together, the

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estimates suggest that the FLSA rule change reduced monthly separation rates by 0.017 percentage points. Column (2) shows that the result is robust to even comparing incumbents within the same firm over time. Appendix F provides additional tests to show the robustness of my results against threats to the SUTVA assumption.

To benchmark my results to previous studies, I use my estimates to compute an elasticity of separations with respect to wages. Recall that Figure Vb shows that the salaries of workers initially earning right below the $913 threshold increased by approximately 1% as a result of the policy. In comparison, Table III suggests that monthly separation rates fell by 8% (i.e. $\frac{0.0017}{0.0214}$). Together, the results imply a separations elasticity of 8, which is on the higher end of estimates in the literature. Separation elasticities from the minimum wage literature tend to be less than 0.5 for the fast-food industry (Dube, Lester and Reich, 2016; Wursten and Reich, 2023; Wiltshire et al., 2023). More broadly, a recent survey article by Sokolova and Sorensen (2021) finds an average firm-level separations elasticity of 1.5, albeit some estimates can range as high as 9 (e.g. Emanuel and Harrington, 2022). Overall, my estimates are in the upper end of elasticities from previous studies, but still within the range of existing estimates.

6.2 Effect on Firm Employment Levels

Next, I explore whether the persistent wage response to the FLSA rule change had an impact on aggregate employment. My identification strategy leverages variation across firms by the share of their workforce affected by the change to the overtime exemption threshold. Specifically, I first define

$$\text{Share Treated}_j = \frac{\text{No. of Salaried Workers Earning \$455-913 per Week in Apr 2016}}{\text{Total Number of Employees}}$$

as the share of firm j's workforce in April 2016 who are paid by salary and earn between the old and new overtime exemption thresholds. Appendix Figure A.VII plots the distribution

of Share Treated$_j$. I find that 27% of firms had no salaried workers with weekly base pays between the old and new overtime exemption thresholds in April 2016.

I partition firms in the sample into three tercile groups based on their share of workers directly affected by the change in the overtime exemption threshold. Appendix Table A.I provides a description of the characteristics of each group in April 2016. I note two features of the data. First, there is significant variation in exposure to the FLSA rule change across the three groups. On average, the first tercile has less than 1% treated workers, the second tercile has 4% treated, and the third tercile has 21% of workers affected by the policy. Second, workers in the first tercile group are more likely to be in the West Census region and less likely to be in the South compared to the other two groups. In my analysis, I will show that it is important that I control for state fixed effects to account for differences in geography-specific trends.

To estimate the firm-level impact of the overtime exemption policy, I compare firms in the third (i.e. highly exposed) and second (i.e. medium exposed) terciles of the share treated distribution to firms in the bottom tercile. Formally, I estimate regression models of the form

$$\log(y_{jt}) = \sum_{\tau \neq -8}^{16} \beta_\tau D_{\text{high},t=\tau} + \sum_{\tau \neq -8}^{16} \gamma_\tau D_{\text{med},t=\tau} + \alpha_j + \alpha_t + \varepsilon_{jt} \qquad (5)$$

where y_{jt} is an outcome in firm j at time t, α_j is a firm fixed effect and α_t is a time fixed effect. The coefficients of interest are β_τ and γ_τ, which represent the effect of the reform on high and medium exposed firms, respectively, relative to low exposed firms.

Figure IX plots the estimates of equation 5 over time for three outcome variables: the log average weekly base pay of salary workers, the log average wage bill per hourly worker, and log employment. Panel (a) shows that the average labor cost of salary workers was trending similarly between high, medium, and low exposed firms prior to April 2016. The wage bill of salaried workers then increases after the announcement of the FLSA rule change, with a larger impact in highly exposed firms relative to medium exposure firms. Panel (b) repeats

the same exercise for hourly workers. Consistent with the presence of relative pay concerns, I find that highly exposed firms also saw an increase in hourly workers' wage bill.[25] However, there are no significant spillover effects on hourly workers in medium exposed firms.

Looking at employment, panel (c) finds that while medium and highly exposed firms were trending similarly before the announcement of the FLSA rule change, their employment trajectories differ significantly from low exposure firms. To define a better control group, I show in panel (d) that including state-month fixed effects eliminates the pre-trend differences between high and low exposed firms. After adding the controls, I find no immediate impact of the policy on employment levels. The null employment effect, coupled with the decrease in separations observed above, suggests that hiring rates fell. It is possible that firms slowly adjust employment downward over time by decreasing the number of new hires. However, recent work shows that although minimum wage increases in the U.S. cause both a reduction in hires and an increase in retention among small and medium firms, these effects cancel out on average, leading to no meaningful employment effects (Rao and Risch, 2024). Consistent with that evidence, I likewise find that even 1.5 years after the injunction, employment fell by only 1%, which is small relative to the increase in firms' wage bill.

I summarize my results and test for robustness to alternative specifications in Table IV. I aggregate the event-study estimates by collapsing the time indicators in equation 5 into two post-treatment dummies, by exposure level:

$$log(y_{jt}) = \beta_1 D_{\text{high},May2016<t<Nov2016} + \beta_2 D_{\text{high},t>Dec2016}$$

$$+ \gamma_1 D_{\text{med},May2016<t<Nov2016} + \gamma_2 D_{\text{med},t>Dec2016} + \alpha_j + \alpha_t + \varepsilon_{jt} \quad (6)$$

For conciseness, Table IV only reports the estimates for the impact on highly exposed firms. Column (1) of Table IV corresponds to the estimates in Figure IX with only firm and month fixed effects. I find an increase in both firms' wage bill and employment. However, as noted,

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[25]Quach (2024) shows that firms also bunch some hourly workers' wages at the overtime exemption threshold, even though they are not directly targeted by the policy.

I am cautious to interpret the employment estimates as the causal impact of the policy given the strong pre-trends. In column (2), I introduce state-month fixed effects to remove the pre-trends, and find that while the wage bill effects persist, there is no longer a statistically significant impact on firms' employment. I can rule out employment loss greater than 1.12% and employment gains greater than 0.30% with 95% confidence. Column (3) finds similar null effects after controlling for state-naics-month fixed effects, so that the estimates are identified from firms operating within the same state-industry. I report similar results for medium exposed firms in Appendix Table A.II.

The lack of a negative employment response is perhaps surprising given that I find a persistent increase in labor costs. However, my results are consistent with the broader literature on the impact of wage and hour laws (Brown and Hamermesh, 2019). Previous studies have found that state level changes to the overtime exemption threshold (Quach, 2024) and minimum wage (Cengiz et al., 2019) have similarly small employment effects. A common explanation for this result is that employers possess monopsony power (Azar et al., 2023). Taken together, my analysis implies that although temporary policies may lead to wage hysteresis, it can nevertheless have little impact on aggregate employment.[26]

7   Mechanisms

In this section, I explore potential explanations for why the temporary overtime policy had such persistent effects despite never being legally binding.

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[26]In contrast, some studies find that downward nominal wage rigidity exacerbates job loss after large macroeconomic shocks (Fehr and Goette, 2005; Kaur, 2019; Kurmann and McEntarfer, 2019). However, as explained in Beaudry, Green and Sand (2018), small policy changes and large aggregate shocks may have different employment elasticities due to search and entrepreneurship externalities.

7.1 Morale Concerns

To start, I review responses submitted by employers to the Department of Labor's information request in September 2017. From these responses, I find two pieces of evidence that employers care considerably about workers' morale. First, many employers reported a reduction in morale as a cost of the policy. Appendix Table B.II shows that among firms that say they reduced workers' flexibility in response to the reform, 41% claim it damaged morale. Although these comments focused on workers' discontent with being asked to track their hours, and not on why firms did not revert salaries, they nevertheless highlight the frequency with which employers think about workers' job satisfaction. Second, as part of the information request, the DOL specifically asked employers whether they made "any additional changes, such as reverting salaries of exempt employees to their prior (pre-rule) levels, after the preliminary injunction was issued". Appendix Table A.III summarizes the responses of employers who initially gave workers a pay raise. Among the 39 responses, 23 did not mention how they responded after the injunction, 4 say they did not reverse the pay raise, 6 claim they "cannot" reverse the pay increase, 1 was concerned about "legal obligations" and 5 specifically mentioned employee "morale". Even the one firm that was worried about legal responsibilities was also concerned about "the messaging it would send to workers". Although only a small sample of firms provided insights into why they did not reverse pay increases, it is striking that nearly every response that gave a rationale for the wage persistence mentions employee morale.

Motivated by the survey evidence, Appendix G develops a simple model to see whether morale concerns could qualitatively explain the main results of the paper. The model illustrates two channels through which morale concerns could affect wage dynamics. First, if the announcement of the policy made workers feel entitled to a \$913 weekly base pay, then their morale may decrease if firms do not follow through with the pay increase. Motivated by the findings of Falk, Fehr and Zehnder (2006), I introduce entitlement effects as an increase in workers' reservation wages and show that it can generate persistent bunching at

the overtime exemption threshold even after the policy is removed. Second, if workers have relative pay concerns, then their morale will depend on how their wage compares to their peers. The model finds that concerns over horizontal pay equity can prevent employers from stagnating wage growth or decreasing the wages of new hires. Although morale concerns can explain the core empirical findings, it may not be the only mechanism driving the results. In fact, the model shows that any factor that increases workers' reservation wages can generate the persistent bunching in incumbents' salaries. I next explore other mechanisms that may concurrently be driving firms' behavior.

7.2   Other Potential Mechanisms

Monopsony - I find some evidence that employers have market power over workers affected by the change in the overtime exemption threshold. As outlined by the model in Appendix G, in a perfectly competitive model, workers are paid their marginal revenue product so an exogenous increase in compensation would lead to a decrease in labor demand and employment. Contrary to that benchmark, I find no negative employment effects, despite a sizable increase in workers' salaries. The fact that firms were able to absorb a large increase in labor costs without significantly cutting jobs suggests that these workers' wages were initially marked down relative to their marginal productivity. Moreover, I find that incumbents are less likely to separate after their salaries increased. Strictly speaking, the degree of monopsony power depends on how separation rates respond to wage changes in a firm, *holding outside options constant*. Although I do not estimate a firm-specific labor supply elasticity, the decrease in separation rates nevertheless provides evidence that the quality of workers' jobs improved, and firms did not undo the pay increases with reductions in amenities (Holzer, Katz and Krueger, 1991).

   Firm Pay Policies - Multiple pieces of evidence also support the view that employers maintain internal pay scales and that it is costly to deviate from these benchmarks. First, in the year after the policy change, employers seem to provide roughly uniform wage increases

to workers at the $913 threshold and to other workers in the firm. Second, the observation that new hires' wages are also similarly affected suggests that position-level pay is set equally across workers. Together, these results indicate that employers may use simple heuristics to set pay scales within-firm across occupations.

The existence of internal pay scales is not mutually exclusive from fairness constraints or monopsony power. In fact, results from previous studies suggest that these factors may even be mechanisms that lead to internal pay scales. For example, Dube, Manning and Naidu (2018) find that workers' wages are often bunched at round integer numbers. They note that this behavior "could reflect internal fairness constraints or administrative costs internal to the firm". They then show that the ability of firms to systematically mis-optimize without being driven out of the market is an indication of labor market power. As another example of firm pay scales, Hazell et al. (2022) find that wages tend to be the same within the same firm-occupation, even across establishments located in very different cost of living areas. To model this phenomenon, they simply assume that a subset of "firms must pay the same nominal wage in all establishments, regardless of the revenue product or markdown of the establishment". My results support the view that employers maintain internal pay scales and I show that these constraints affect how firms respond to policy changes.

Learning and Bargaining - Another explanation is that announcing a pay raise acted as an information shock, revealing the amount of available rents to workers and enabling them to bargain for higher wages. For example, a class of union strike models rationalize labor disputes by assuming that unions use strikes to gather new information about the amount of rents available (Kennan, 1986). In our setting, a raise to the $913 threshold would inform workers that the *minimum* amount of rents available is at least that amount. However, bargaining models with imperfect information predict that the final wage would fall between the minimum and maximum perceived rents (Tracy, 1987). Unless workers have zero bargaining power, the new information should have pushed salaries above $913 per week. Moreover, it is unclear why new information might influence new hires but not existing

higher-paid workers. Thus, while learning may be occurring, it alone does not explain the key results.

Efficiency Wages - An alternative reason why firms kept workers' wages elevated above the $913 threshold may be due to an increase in productivity. Models of efficiency wages argue that higher pay can reduce turnover and increase workers' productivity (Katz, 1986). While I observe a decrease in turnover, a revealed preference argument suggests that efficiency wages alone do not explain the persistent wage effects. If improvements in turnover and productivity exceeded the cost of complying with the policy, then firms would raise wages for other employees as well, although there is uncertainty about whether the productivity of other workers will respond similarly. In contrast, I find no pay increases among those earning above $913 per week. Thus, while efficiency wages may play a role, they do not fully account for the observed wage patterns.

Reputation - Employers may also have kept their promises to maintain their reputation. Models of relational contracts argue that employers honor non-enforceable agreements when the present value of the relationship outweighs the short-term benefits of noncompliance (MacLeod, 2007; Macchiavello and Morjaria, 2023). For example, employers and workers may implicitly agree that the worker will exert high effort if the employer keeps its promises. Neither party will want to renege since doing so would only benefit them immediately but reduce all future earnings. Building on this example, if employers raised workers' salaries to preserve their reputation, then the bunching mass would be more prevalent among firms with low baseline turnover rates where reputation can earn returns over a longer time horizon. However, Appendix Figure A.VIII shows no significant difference in bunching of new hires between high- and low-turnover firms. The lack of heterogeneity does not necessarily imply that employers do not care about their reputation, but rather, the expectation of a long-term relationship is not a predictive factor. Firms with high turnover may still raise salaries if reputation affects effort or recruitment.

In summary, employers' letters to the Department of Labor support the hypothesis that

morale concerns play a central role in explaining the persistent wage effects. Other factors might also affect firms' decision, such as monopsony power and reputation value. These various mechanisms are not mutually exclusive, so while morale concerns appear to be a key driver of the observed wage dynamics, it can also be interacting with other factors in determining employers' response.

8 Discussion and Conclusion

Do labor market policies shape perceptions of a fair wage, and if so, what are the economic impacts of this phenomenon and how does the labor market adjust in the long run? My paper answered these questions by examining the wage and employment response to the unexpected retraction of a major overtime reform. I show that temporary wage policies can permanently change the long-run labor market equilibrium. Although the new overtime exemption threshold was rescinded, employers nevertheless raised workers' salaries above the nullified cutoff. This salary premium persisted for at least 1.5 years after the removal of the policy. During that time, the labor market does not appear to adjust either future wage growth, the wages of new hires, or employment levels. Taken together, these results provide the first real-world evidence that labor market policies can interact with morale concerns to generate wage hysteresis that endures long after the policies are abolished.

To explain my results, I presented novel evidence on the role of morale concerns in directing the impact of wage policies. Previous laboratory experiments have found that temporary policies can raise workers' reservation wages through entitlement effects (Falk, Fehr and Zehnder, 2006). Another body of work has shown that perceptions of unfair pay differences among peers reduces workers' job satisfaction (Card et al., 2012), retention (Dube, Giuliano and Leonard, 2019), and effort (Breza, Kaur and Shamdasani, 2017). My paper highlights survey evidence suggesting that the interaction of these two forces - a change in reservation wages and a preference for horizontal pay equity - can lead to long-run wage hysteresis. Consistent with that hypothesis, I show new evidence that employers tend to

give similar annual pay increases to all workers within the same firm. While many factors jointly influence employers' response to the overtime policy, morale concerns appears to play an important role. A fruitful area for future research would be to quantify the relative importance of morale concerns to other mechanisms, such as reputation value, in driving wage rigidity.

The interaction between morale concerns and labor market regulations has several potential policy implications. First, as discussed in this paper, the introduction and removal of wage policies may have asymmetric effects due to reference point formation. Second, changes in social norms may cause policies to have spillover effects beyond their intended targets. For example, recent reports indicate that in response to state-level pay transparency laws, companies in states without such regulations have also started disclosing salaries in online job postings. Third, besides setting wage expectations, labor market policies may also introduce reference points for workers' hours. For instance, even though the majority of salaried workers in the U.S. are not covered for overtime, it is common for salaried workers to report working exactly 40 hours a week in the CPS. Whether these reports are accurate and the extent to which they reflect fairness norms versus economic considerations, such as team production, remains an open question. Future research can continue to explore these types of questions to determine under what circumstances do policies change fairness norms and whether behavioral concerns in turn impact the effects of labor market policies.

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Table I: Effect on Stayers' Monthly Wage Growth Over Time

	(1)	(2)	(3)	(4)
Pre-trend	-.0001	.0001*	.0001	.0001
	(.0001)	(.0001)	(.0001)	(.0001)
Anticipation	.0013***	.0015***	.0028***	.0022***
	(.0001)	(.0001)	(.0002)	(.0001)
Post-trend	0.0000	.0002***	-.0002	$-.0002^*$
	(.0001)	(0.0000)	(.0002)	(.0001)
Change in Slope	.0001	.0001	-.0003	$-.0002^*$
	(.0001)	(.0001)	(.0002)	(.0001)
Worker FE	Y	Y	Y	Y
Month FE	Y	Y	Y	Y
Firm-Month FE	-	Y	-	Y
N	11,214,315	10,998,155	4,280,210	4,012,604
Sample	Apr	Apr	Dec	Dec

Notes: This table reports the change in the wage growth of stayers affected by the 2016 FLSA rule change (see equation 3). Estimates are reported for three time periods: May 2015 - April 2016 (Row 1); May 2016 - December 2016 (Row 2); and Jan 2016 - April 2017 (Row 3). The fourth row reports the difference in estimates between rows 1 and 3, computed using the delta method. Columns (1) and (2) compare workers earning \$700-913 per week in April 2016 to those earning \$913-953 per week. Columns (3) and (4) compare workers earning \$913-953 per week in December 2016 to those earning \$953-993 per week. All workers in the sample are continuous employed throughout the study period. All robust standard errors in parentheses are clustered by firm. *10%,** 5%,*** 1% significance level.

Table II: Effect on the Composition and Wage Growth of New Hires

	(1)	(2)	(3)	(4)
Log Base Pay in Previous Job	−.051***	−.047*		−.048**
	(.017)	(.025)		(.023)
% Δ Base Pay from Prev. Job	.071***	.041		.064*
	(.026)	(.029)		(.034)
Log Base Pay After 18 Months			-.007	-.004
			(.007)	(.006)
% Δ Base Pay After 18 Months			-.003	.004
			(.011)	(.008)
Bin FE	Y	Y	Y	Y
Hire Date FE	Y	Y	Y	Y
N	60,937	29,225	103,150	32,469
Sample	Any Past	6 Month Past	Any Post	Balanced

Notes: This table reports difference-in-difference estimates that compare cross-sections of new hires with a base pay of \$913-953 per week to those with \$953-993 (see equation 4). The estimates are reported for the 6 months starting from November 2016. The outcome variables in the regression are log base pay in the last observed job prior to hire (row 1), the percent change in base pay relative to the last observed job (row 2), log base pay 18 months after being hired (row 3), and the percent change in base pay 18 months after being hired (row 4). Columns (1) restricts the sample to new hires for whom I observe any past employment. Columns (2) restricts the sample to only those with employment in the past 6 months. Column (3) keeps all hires that stay employed for at least 18 months. Column (4) keeps only new hires for whom I can observe both their salary at their previous job and their salary 18 months after hire. All robust standard errors in parentheses are clustered by firm. *10%,** 5%,*** 1% significance level.

Table III: Effect on the Probability that Incumbents are Employed

	(1)	(2)
Pre-trend	.0012***	.0028***
	(.0002)	(.0002)
Anticipation	−.002***	−.002***
	(.0002)	(.0002)
Post-trend	−.0004***	−.0006***
	(.0001)	(.0001)
Change in Slope	.0017***	.0014***
	(.0002)	(.0002)
Baseline Monthly Separations	.0214	.0214
Worker FE	Y	Y
Month FE	Y	Y
Firm-Month FE	-	Y
N	23,212,504	23,041,898

Notes: This table reports difference-in-difference estimates that compare the employment rate of incumbent workers earning $700-913 per week in April 2016 to those earning $913-993 per week (see equation 3). Estimates are reported for three time periods: May 2015 - April 2016 (Row 1); May 2016 - December 2016 (Row 2); and Jan 2016 - April 2017 (Row 3). The fourth row reports the difference between rows 2 and 3, computed using the delta method. Baseline separation rate is the average monthly separation rate between May to November 2016. All robust standard errors in parentheses are clustered by firm. *10%,** 5%,*** 1% significance level.

Table IV: Effect on Highly Exposed Firm's Employment and Wage Bill

	(1)	(2)	(3)
Log Employment			
May - Nov 2016	.0311***	-.0012	-.0018
	(.002)	(.0022)	(.0029)
Post Dec 2016	.0282***	-.0041	-.0045
	(.0031)	(.0036)	(.0048)
Log Avg. Salaried Wage Bill			
May - Nov 2016	.0066***	.0049***	.004*
	(.0011)	(.0018)	(.0023)
Post Dec 2016	.0207***	.0264***	.0268***
	(.0017)	(.0025)	(.0031)
Log Avg. Hourly Wage Bill			
May - Nov 2016	-.0001	.0031	-.0009
	(.0054)	(.0056)	(.0075)
Post Dec 2016	.0204***	.0275***	.0245***
	(.006)	(.0065)	(.0084)
Firm FE	Y	Y	Y
Month FE	Y	Y	Y
State-Month FE	-	Y	Y
State-Month-NAICS FE	-	-	Y
N	2,761,113	1,568,393	1,283,271

Notes: This table reports the estimates from a difference-in-difference that compares firms highly exposed to the 2016 FLSA rule change relative to little exposed firms (see equation 6). The estimates show the effect of the policy after December 2016, relative to before April 2016. Column (1) compares firms over time, controlling for firm and month specific fixed effects. Column (2) compares firms within the same state, and column (3) compares firms within the same state and 6-digit industry. All robust standard errors in parentheses are clustered by firm. *10%,** 5%,*** 1% significance level.

Figure I: Change in the Density of Base Pay Between April and December 2016
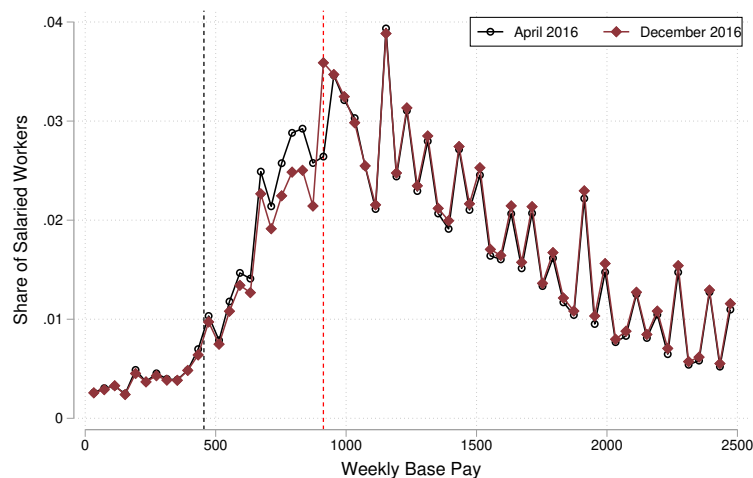
(a) Raw Averages
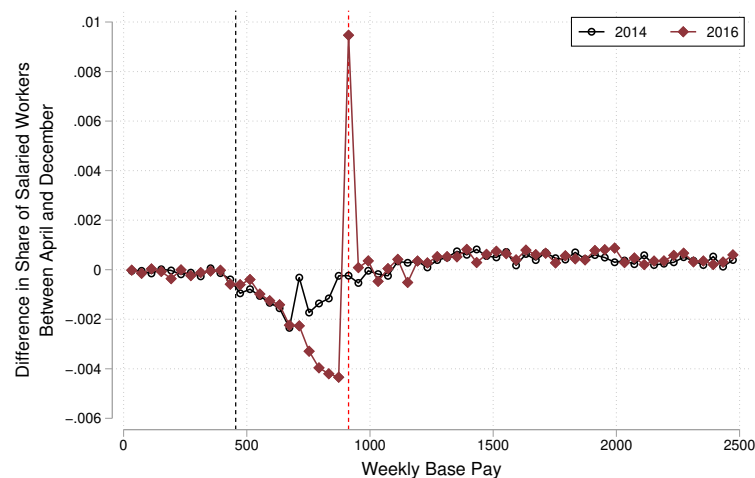
(b) Difference in Distribution
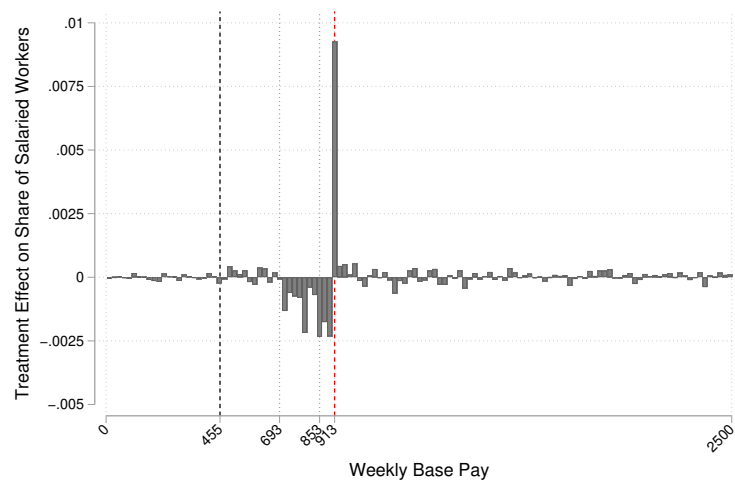
(c) Diff-in-Diff Distribution

Notes: In panel (a), the blue and red lines show the density of base pay in April and December 2016, respectively. The sample is restricted to salaried workers who are continuously employed at the same firm from May 2015 to April 2018. In panel (b), the blue (red) line shows the difference in the density between April and December of 2016 (2014), where the 2014 sample is continuously employed from May 2013 to April 2016. Panel (c) shows the difference between the two lines in panel (b), but using $20 bins of base pay. The dotted line at $853 is the median salary within the missing mass to the left of $913. In all figures, the black vertical dashed line is at $455 and the red vertical dashed line is at $913 per week.

Figure II: Effect on the Base Pay Distribution of Salaried Workers Relative to April 2016
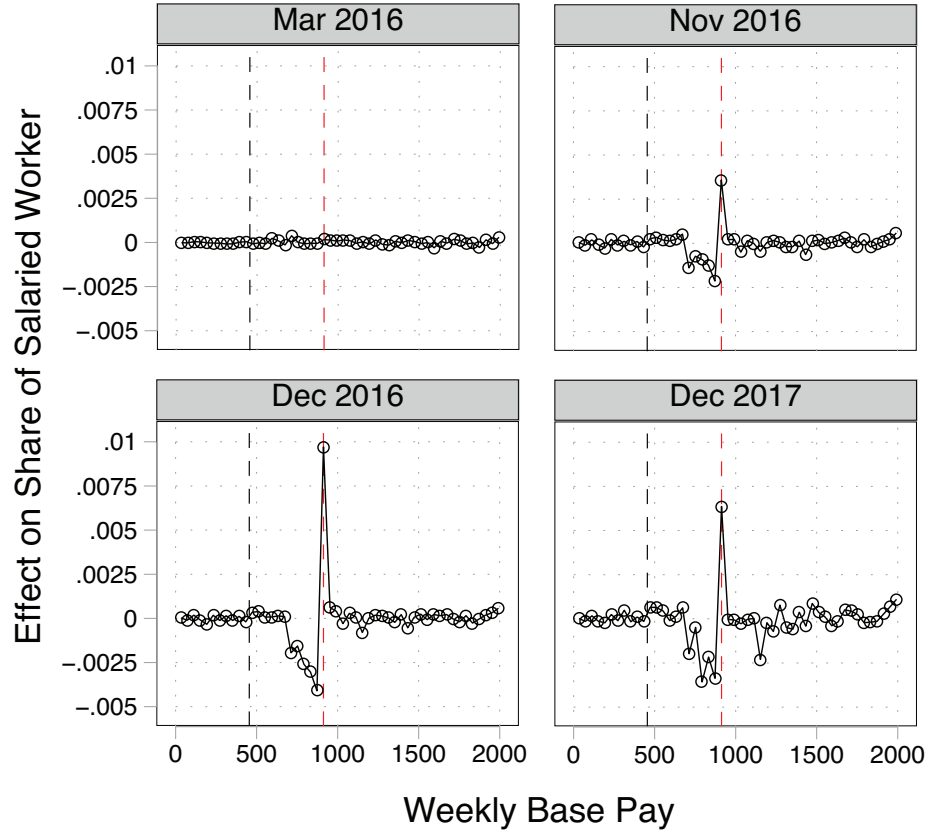
Notes: This figure shows difference-in-difference estimates that compare changes to the base pay distribution since April 2016 to changes over the same number of months since April 2014 (see equation 1). The x-axis is divided into $40 increments, where the black vertical dashed line is at $433 and the red vertical dashed line is at $913 per week. The treatment sample consists of workers who are salaried in April 2016 and continuously employed at the same firm between May 2015 and April 2018. The control sample is similarly defined with all reference dates shifted back two years (i.e. employed May 2013-April 2016, with baseline at April 2014).

Figure III: Share of Workers Bunched Over Time
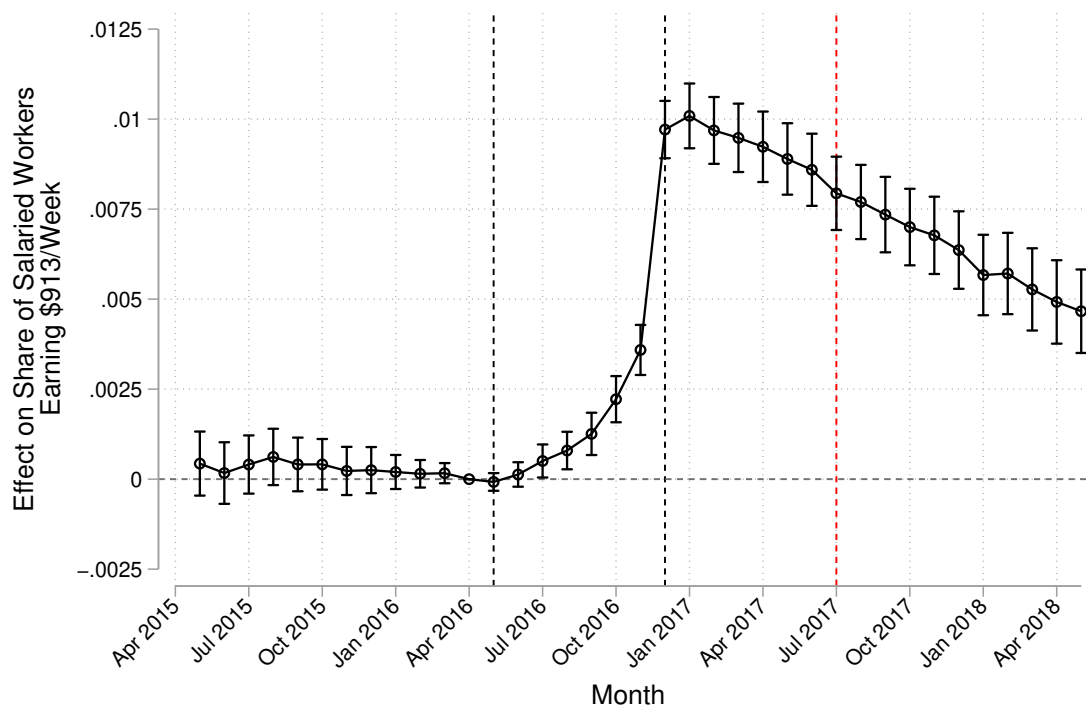
Notes: This figure shows the effect of the 2016 FLSA policy on the share of salaried workers earning between \$913 and \$953 per week, estimated from equation 1. The treatment sample consists of salaried workers who are continuously employed at the same firm between May 2015 and April 2018, whereas the control group is similarly defined from May 2013 to April 2016. The first vertical dash line is at May 2016 when the \$913 threshold was first announced, the second vertical dash line is at December 2016 when the threshold was supposed to go into effect, and the third red dashed line is at July 2017 which is the first month after the Department of Labor dropped their defense of the \$913 threshold.

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Figure IV: Distribution of Annual Wage Change for Workers Bunched in December 2016
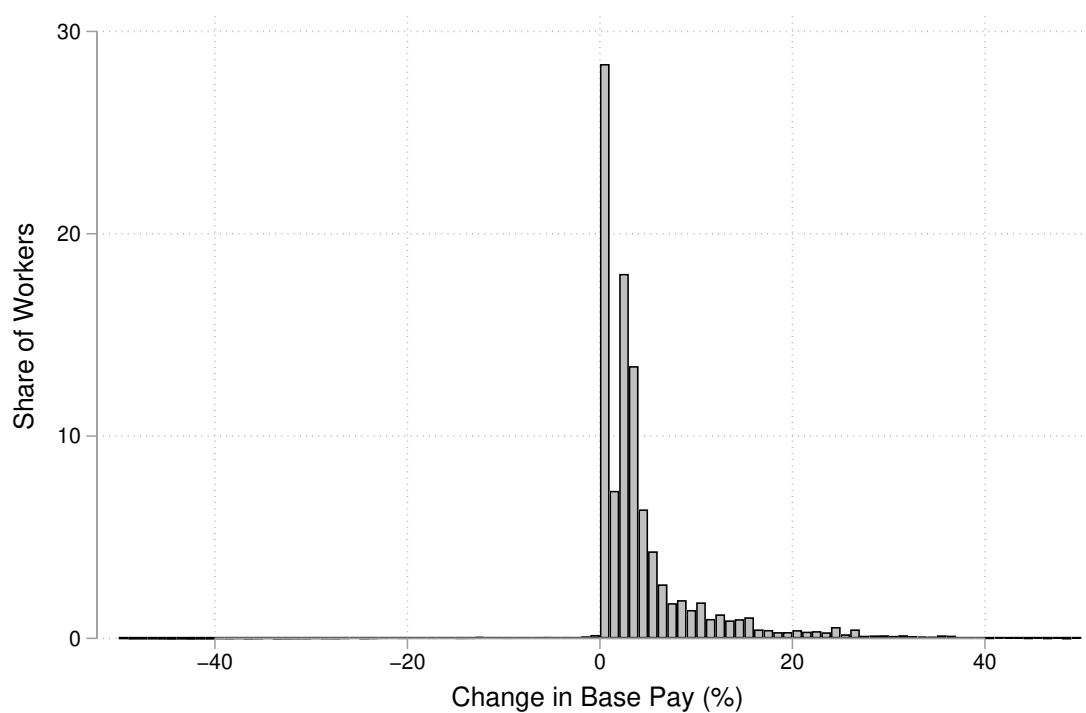
Notes: This figure shows the distribution of the percent change in weekly base pay between December 2016 and 2017. The sample consists of salaried workers who are continuously employed at the same firm from May 2015 to April 2018, and earned between $913 and $953 in December 2016.

Figure V: Difference-in-Difference Comparing Base Pay Between Bunched and Non-Bunched Workers

(a) Base Pay given Apr 2016 Salary

(b) Diff-in-Diff given Apr 2016 Salary

(c) Base Pay given Dec 2016 Salary

(d) Diff-in-Diff given Dec 2016 Salary
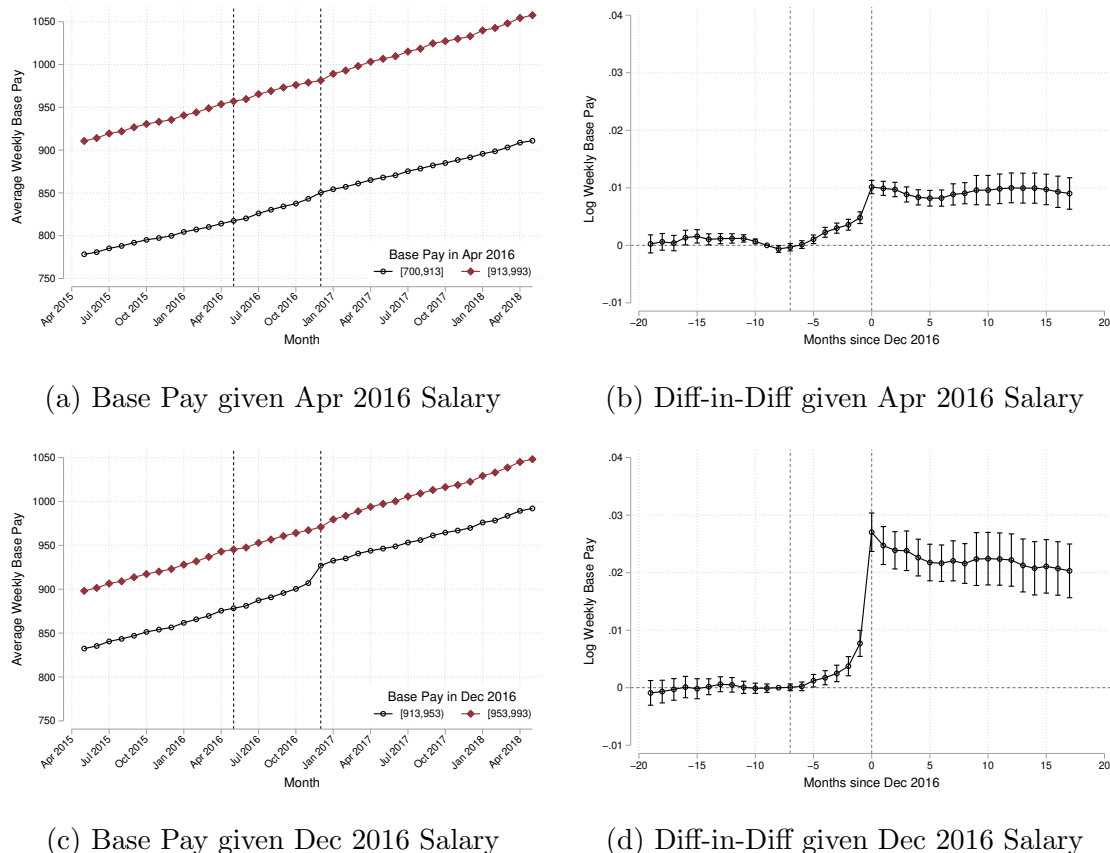
Notes: Panel (a) shows the evolution of weekly base pay over time for salaried workers who earned within [700,913) and [953,993) per week in April 2016. Panel (b) plots the difference-in-difference estimates of the two lines in panel (a), with the treatment group being workers in the [700,913) bin. Panels (c) and (d) presents analogous figures using workers earning within [913,953) and [953,993) per week in December 2016 as the treatment and control groups, respectively. In all figures, the left vertical dashed line is at May 2016, and the right vertical dashed line is at December 2016.

Figure VI: Distribution of New Hires' Base Pay Relative to April 2016
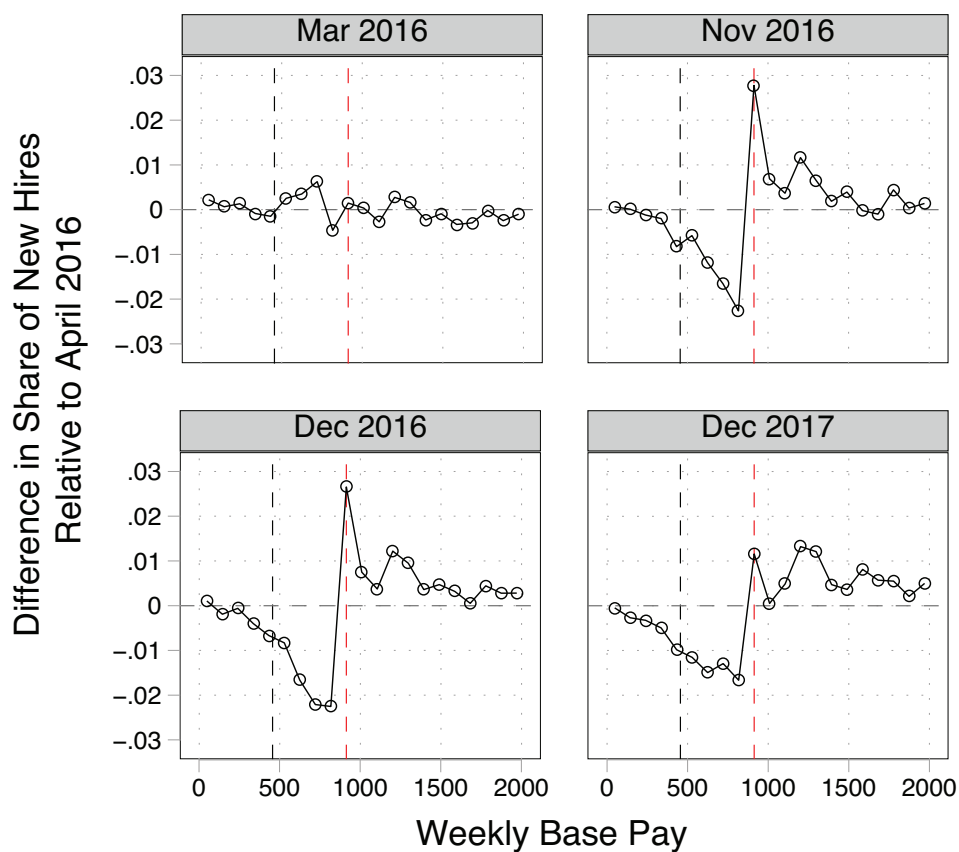
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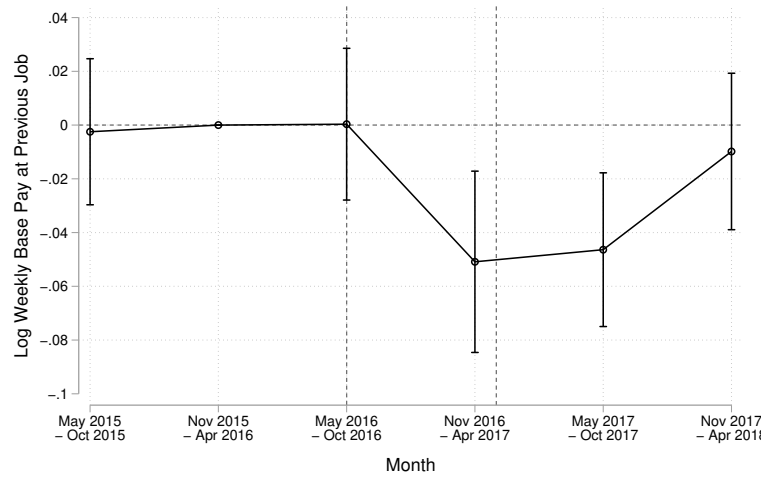

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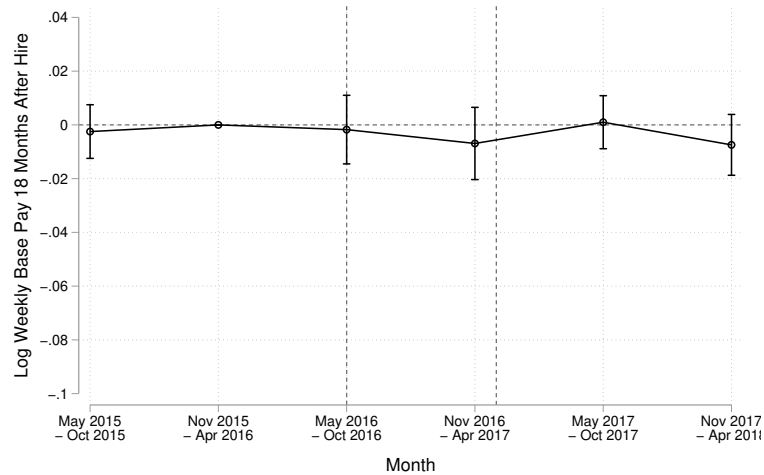
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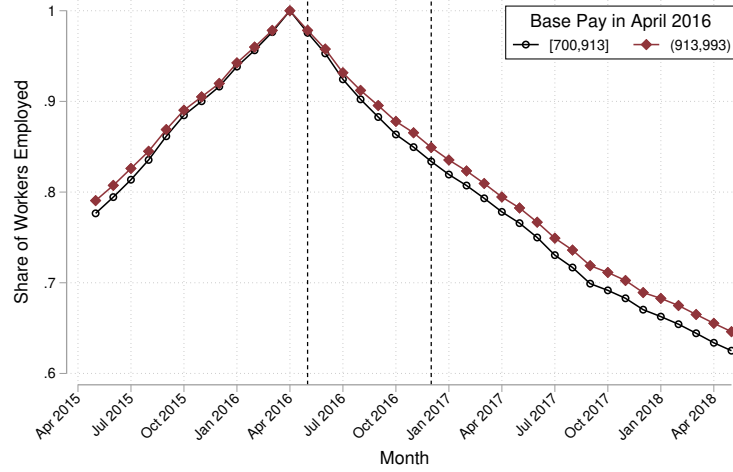
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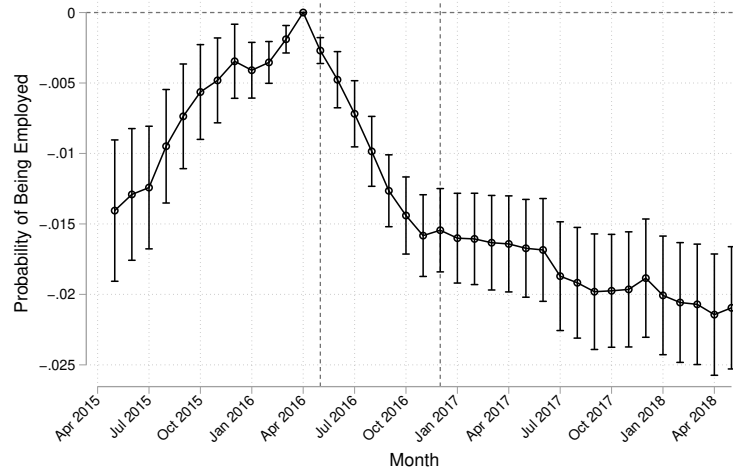
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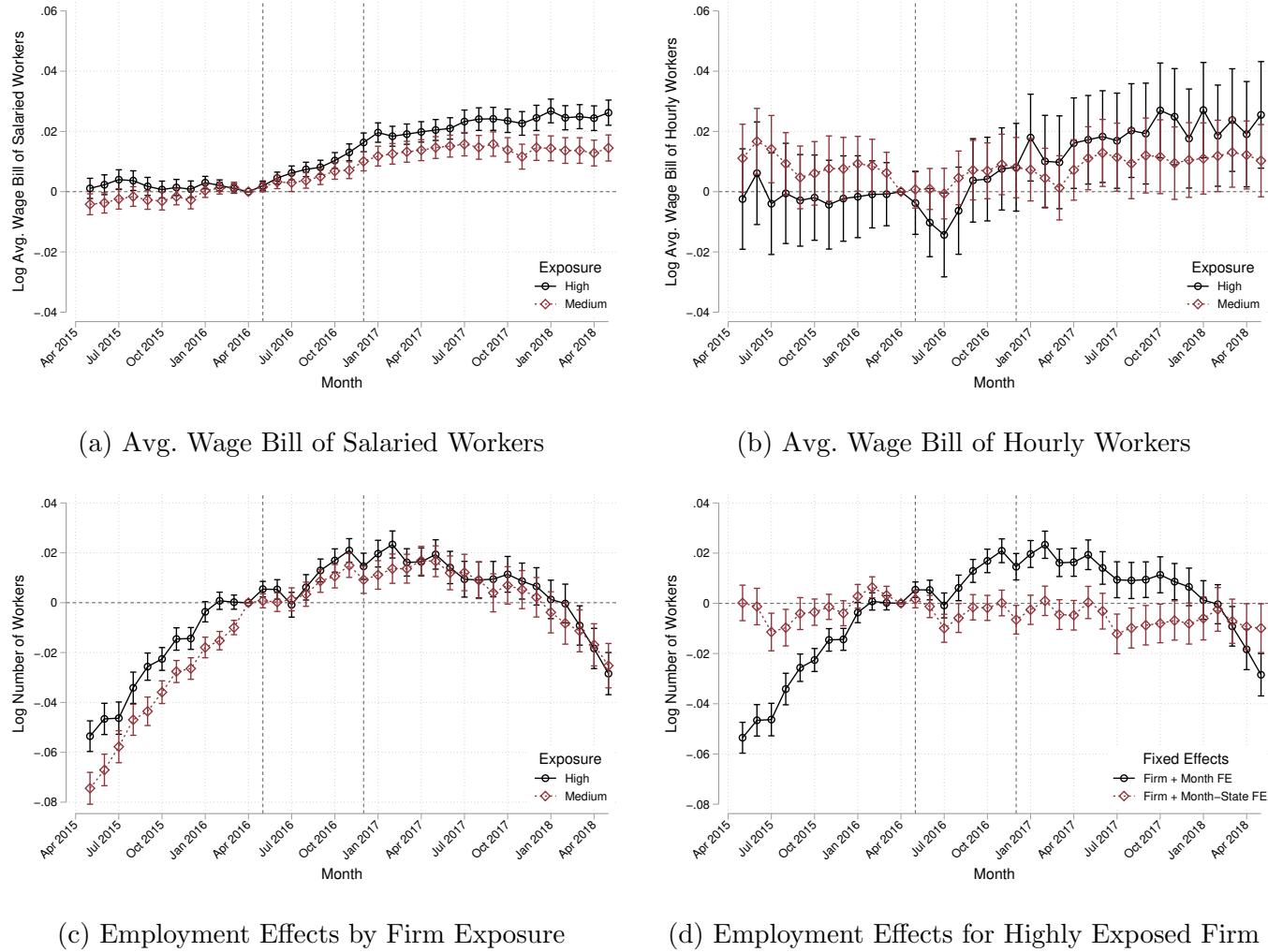
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WAGE HYSTERESIS AND ENTITLEMENT EFFECTS: THE PERSISTENT IMPACTS OF A TEMPORARY OVERTIME POLICY - ONLINE APPENDIX

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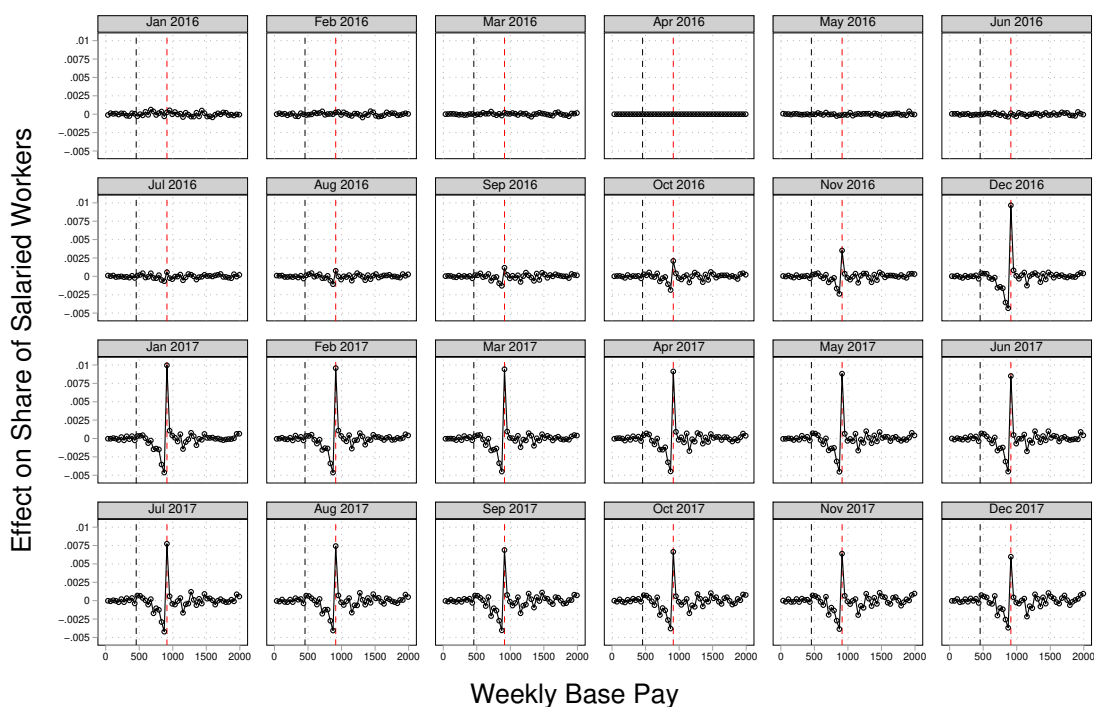
Notes: This figure shows difference-in-difference estimates that compare changes to the base pay distribution since April 2016 to changes over the same number of months since April 2014 (see equation 1). The x-axis is divided into \$40 increments, where the black vertical dashed line is at \$433 and the red vertical dashed line is at \$913 per week. The treatment sample consists of workers who are salaried in April 2016 and continuously employed at the same firm between May 2015 and April 2018. The control sample is similarly defined with all reference dates shifted back two years (i.e. employed May 2013-April 2016, with baseline at April 2014).

Appendix Figure A.II: Google Search Popularity for the Term "FLSA Overtime"
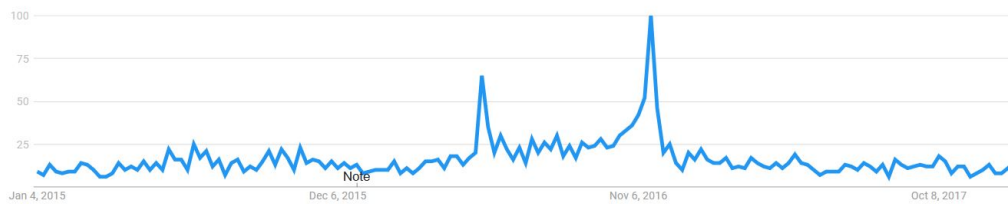
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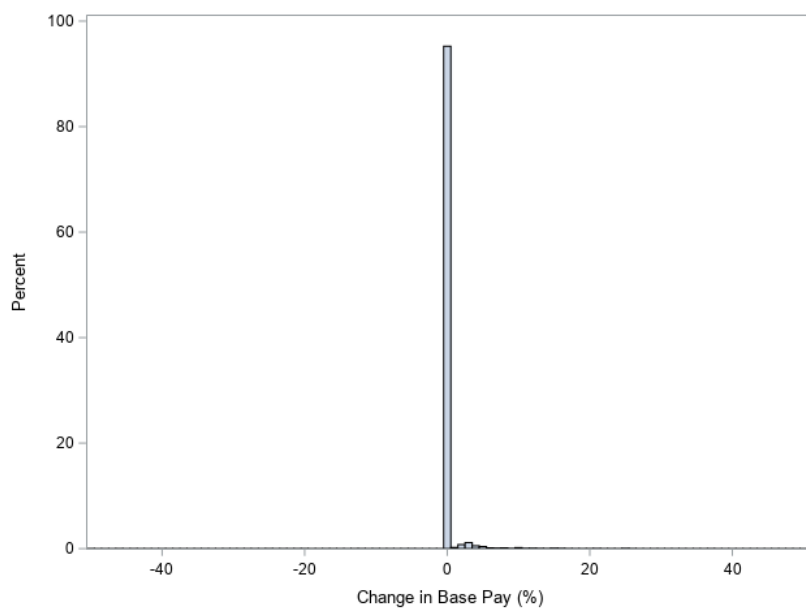
Notes: This figure plots the change in weekly base pay among individuals earning \$913-953 per week in December 2016 and stayed in the same firm until January 2017.

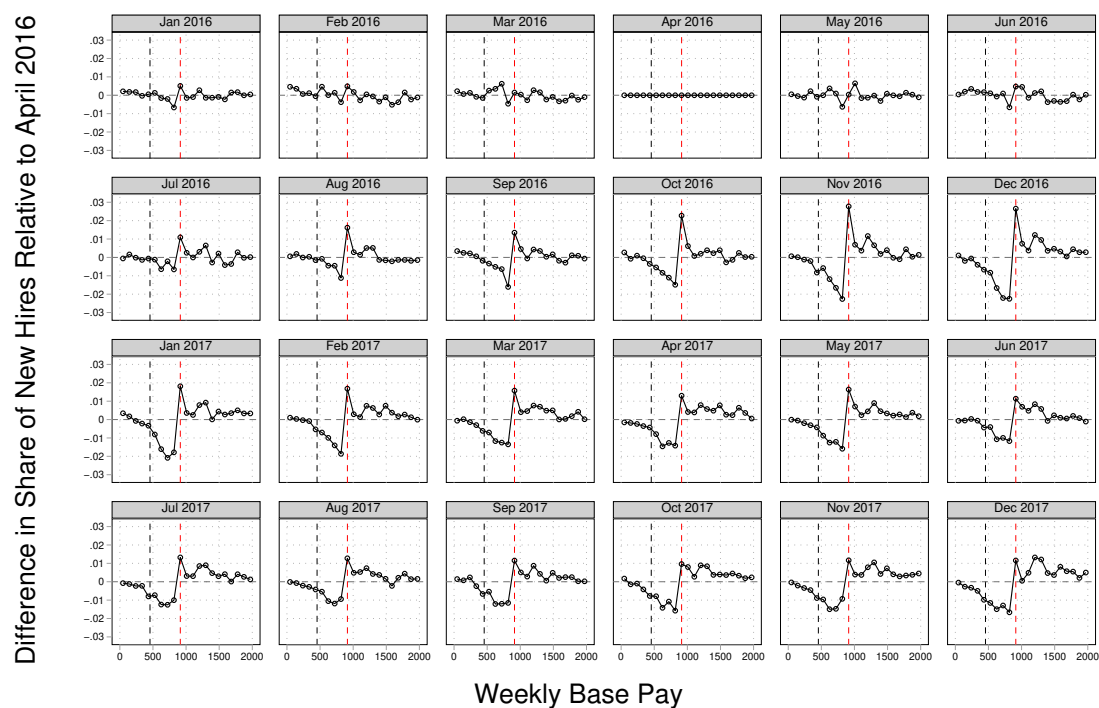Appendix Figure A.IV: Change in Base Pay Relative to Modal Change within Firm
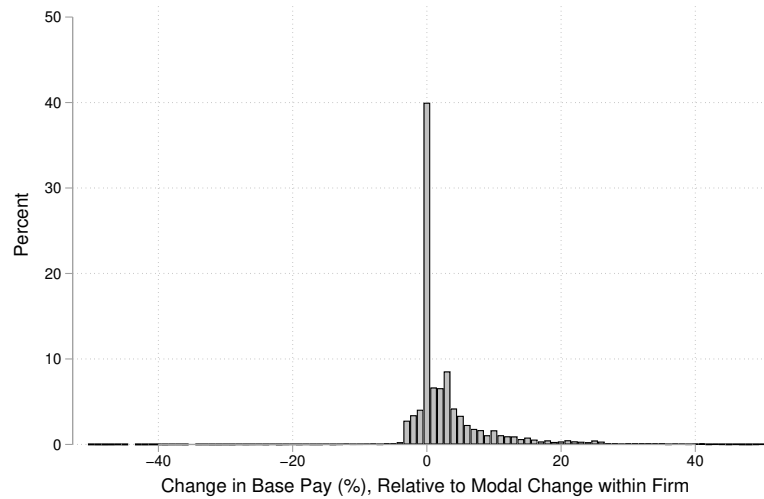
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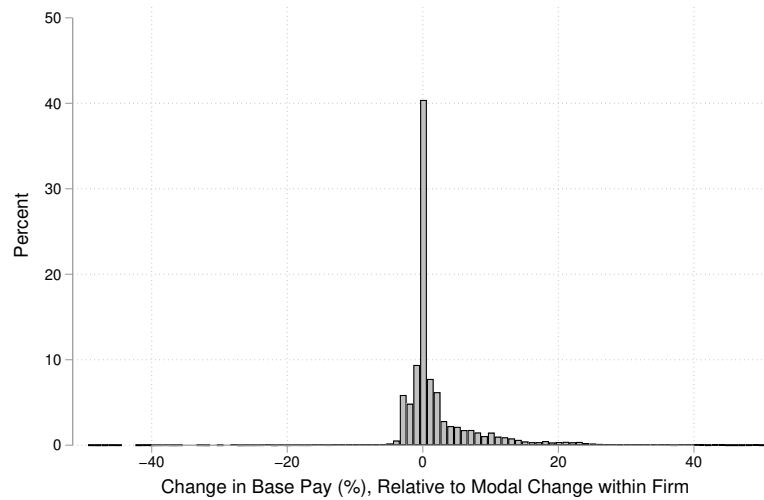

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Notes: This figure shows the distribution of workers' annual pay increase between December 2016 and 2017, relative to the modal change within each workers' employer. The sample consists of salaried workers who are continuously employed at the same firm from May 2015 to April 2018, and earned between \$913 and \$953 in December 2016. The mode is computed using all salaried workers within each firm who were not bunched at the \$913 threshold in Dec 2016. Panel (a) restricts the sample to firms with at least 50 salaried employees when computing the mode. Panel (b) further restricts the sample to firms where the modal wage change is non-zero.

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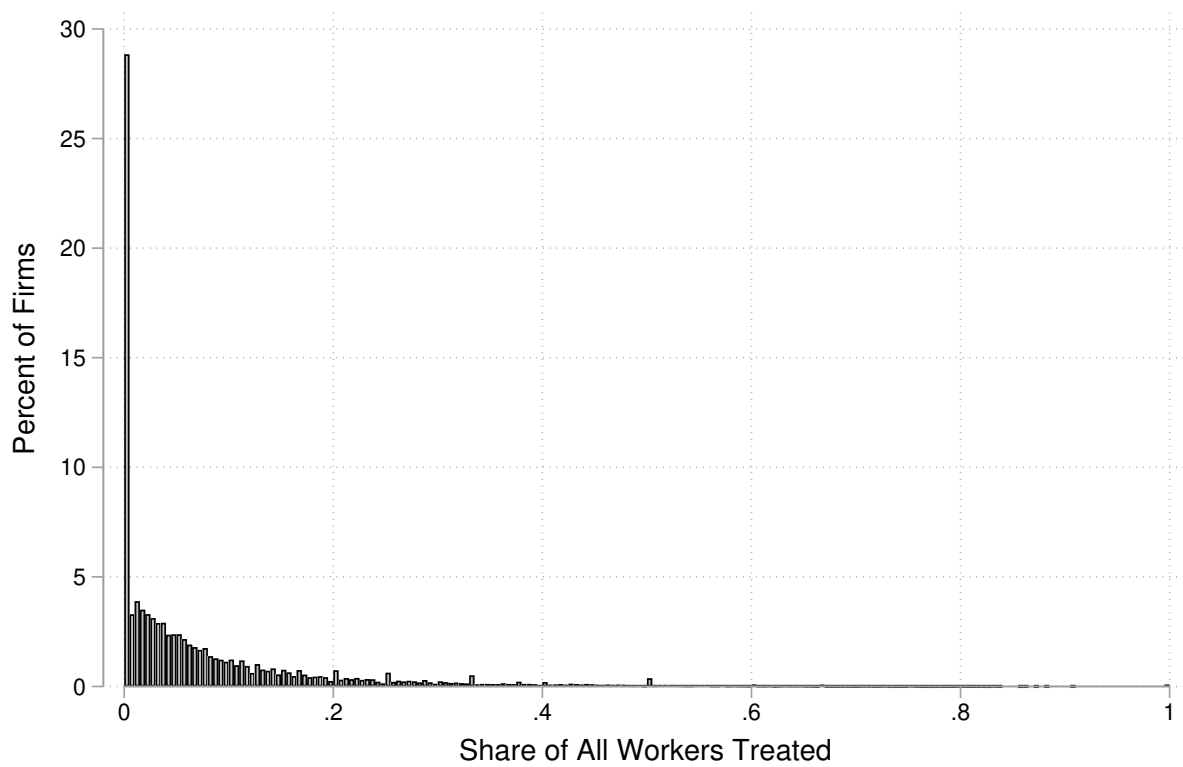

Notes: This figure shows the share of new salaried hires within $96.15 increments of weekly base pay relative to the share in April 2016. In each panel, the black vertical dashed line is at $432 and the red vertical dashed line is at $913.

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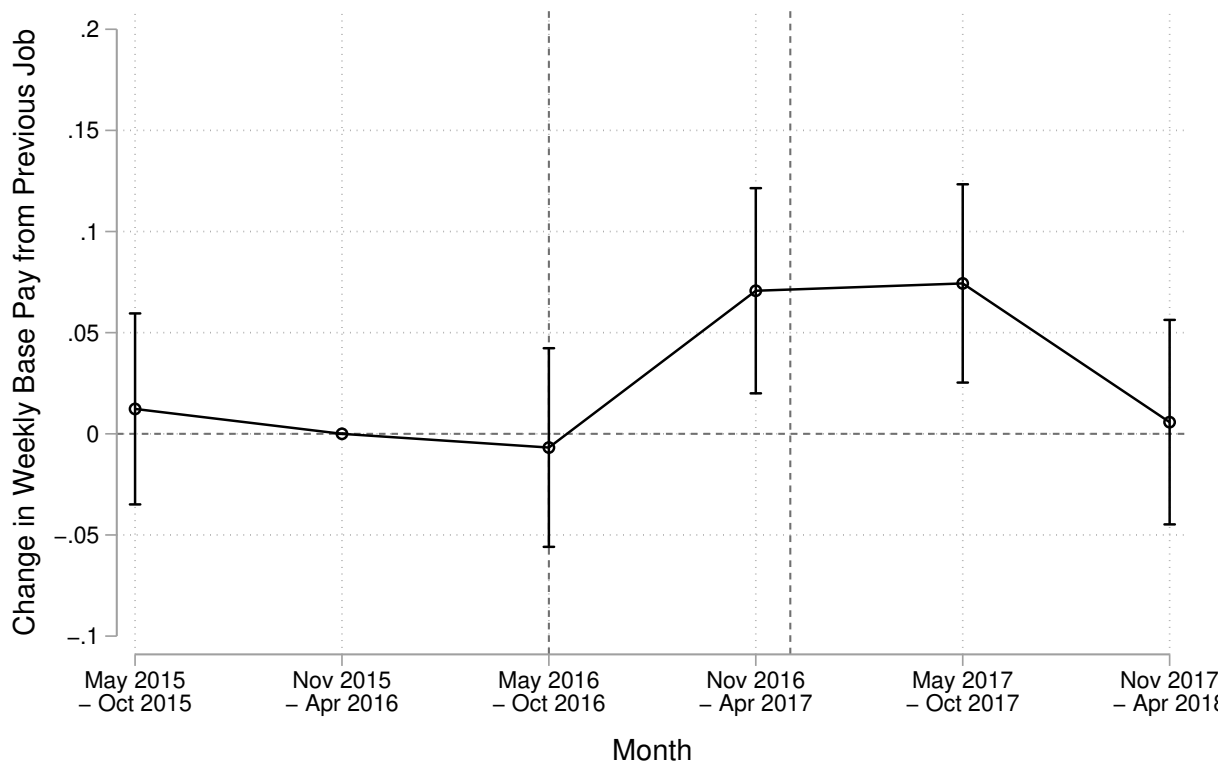
Notes: This figure plots the estimates from a difference-in-difference that compares repeated cross-sections of new hires employed at a base pay of $913-953 per week to those hired at $953-993 per week. Each estimate is averaged over 6 months. The outcome is the percent change in base pay from each worker's last observed employer to their current one. The sample is restricted to workers hired between May 2016 and May 2018. The left vertical dashed line is at May 2016, and the right vertical dashed line is at December 2016.

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Notes: This figure plots the distribution of firms in April 2016 by the share of all workers who are salaried and have a base pay between $455 and $913 per week.

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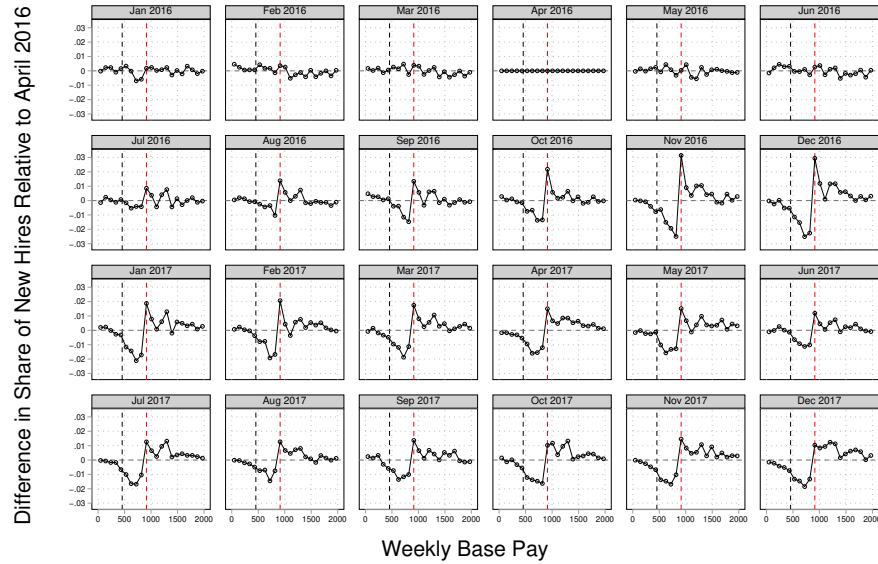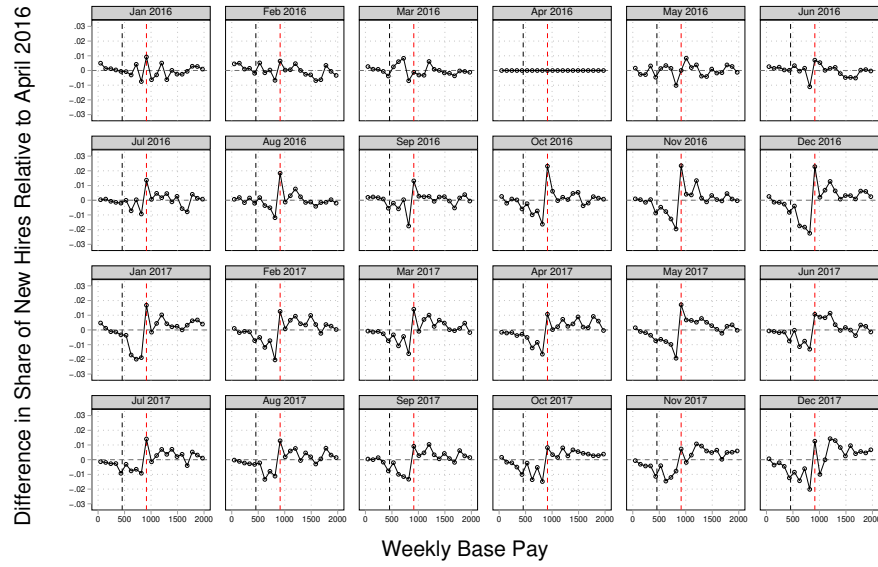
(a) Low Turnover Firms

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Notes: This figure shows the share of new salaried hires within $96.15 increments of weekly base pay relative to the share in April 2016. The black vertical dashed line is at $432 and the red vertical dashed line is at $913. Panels (a) and (b) restrict the sample to firms with below and above median worker separation rate from May 2015 to April 2014, respectively.

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Appendix Table A.I: Firm Descriptive Statistics by Share Treated, April 2016

	Share Treated		
	Treat ≤ .01	.01<Treat ≤ .07	Treat>.07
Fraction Treated	0	.04	.21
Number of Workers	328.71	389.96	190.87
Avg. Weekly Base Pay	1284.2	1031.25	1008.85
Share Male	.61	.59	.54
Share Salaried	.28	.34	.59
West	.27	.23	.19
Midwest	.18	.21	.21
South	.24	.28	.31
Northeast	.29	.26	.28
Agriculture & Mining	.01	.01	.01
Construction	.07	.04	.03
Manufacturing	.19	.2	.1
Retail and Wholesale	.09	.14	.19
Transportation	.02	.03	.02
Professional Services	.31	.28	.32
Education	.01	.02	.07
Health	.14	.09	.08
Restaurants	.04	.09	.05
Public Services	.02	.02	.02
Other	.06	.05	.09
No. Firms	14067	14067	14485

Notes: This table reports statistics for a balanced panel of firms active from May 2015-2018. The sample is partitioned into three tercile groups based on the share of workers within each firm that is salaried and earns between $455-913 per week in April 2016.

Appendix Table A.II: Effect on Medium Exposed Firm's Employment and Wage Bill

	(1)	(2)	(3)
Log Employment			
May - Nov 2016	.0406***	.0081***	.0058**
	(.0021)	(.0022)	(.0027)
Post Dec 2016	.0387***	.0054	.0049
	(.0033)	(.0037)	(.0047)
Log Avg. Salaried Wage Bill			
May - Nov 2016	.0066***	.0047***	.0046**
	(.0012)	(.0018)	(.0023)
Post Dec 2016	.0153***	.0204***	.02***
	(.0017)	(.0025)	(.003)
Log Avg. Hourly Wage Bill			
May - Nov 2016	-.0038	-.001	-.0022
	(.0032)	(.0035)	(.0051)
Post Dec 2016	.0011	.0075*	.0064
	(.0038)	(.0044)	(.0063)
Firm FE	Y	Y	Y
Month FE	Y	Y	Y
State-Month FE	-	Y	Y
State-Month-NAICS FE	-	-	Y
N	2,761,113	1,568,393	1,283,271

Notes: This table reports the estimates from a difference-in-difference that compares medium exposed firms to the 2016 FLSA rule change relative to little exposed firms (see equation 5). The estimates show the effect of the policy after December 2016, relative to before April 2016. Column (1) compares firms over time, controlling for firm and month specific fixed effects. Column (2) compares firms within the same state, and column (3) compares firms within the same state and 6-digit industry. All robust standard errors in parentheses are clustered by firm. *10%,** 5%,*** 1% significance level.

Appendix Table A.III: Response to DOL Request for Information

Type of Response	Example	N
"Gave a Raise"	"Our company, for example, raised salaries at the beginning of a pay period that began a few days before the injunction was issued." - Bloomin Brands	23
"Did not reverse pay raise"	"For a variety of reasons, most of these employers have not reverted back to their previous practices even after it became clear that the Final Rule would not take effect." - National Association of Truckstop Operators	4
"Cannot reverse pay raise"	"To the extent that the increases were already communicated or implemented, many businesses had no choice but to keep any salary increases intact." - Center for Workplace Compliance	6
Legal	"Few of our members, if any, reverted these changes after the injunction was issued with concern to both legal obligations and the messaging it would send to workers that they work hard to retain." - Alliance (nonprofit)	1
Morale	"We have heard that some nonprofits increased salaries effective December 1, 2016 in anticipation of the Final Rule and have found it difficult to undo the change, since the resulting salary reduction would hurt employee morale." - North Carolina Center for Nonprofits	5

Notes: This table reports a count of the types of responses given to the DOL by employers who specifically mentioned raising workers' salaries above the $913 threshold. Column 1 lists the 5 categories of responses, where the last two categories are rationales for why the firm did not reverse the pay increase. Column 2 are quotes from employers as examples of responses to the DOL's request for information. Column 3 reports the number of each type of response.

Appendix B. Heterogeneous Response to the 2016 FLSA Rule Change

There are essentially three ways in which employers could have reacted to the injunction of the 2016 overtime exemption threshold: raise workers' base pay to $913 per week; start paying workers overtime or cut their hours; and cancel any planned changes. Given that base pay is by far the largest component of most workers' incomes, the main text has focused primarily on the first margin of response and whether employers can reverse salary increases in the long-run. Moreover, base pay is the relevant measure of labor costs in most models of downward nominal wage rigidity. After all, overtime pay is simply a function of base pay and hours of work. However, to put my results in context, this Appendix explores the other margins by which employers responded to the injunction of the overtime exemption threshold.

B.1 Why Did Most Workers Not Get Bunched?

In this section, I identify three reasons why the majority of affected workers did not receive a raise above the $913 overtime exemption threshold: some workers were already covered for overtime, some firms retracted their planned pay increases, and some workers gain overtime coverage. To start, I calculate the share of salaried workers who were already earning overtime compensation. Recall from section 2 that employers are only allowed to exempt a worker from overtime if their job fulfills two criteria: 1) they perform white collared duties and 2) they earn a salary above the overtime exemption threshold. If a salaried worker primarily engages in non-white collared tasks, then legally, they would already have been covered for overtime compensation. Moreover, even if employers are not legally required to pay workers overtime, they may do so anyways to incentivize them to work longer hours. In either case, workers already eligible for overtime pay would not be impacted by the policy.

Figure B.I plots the probability that someone earns any overtime pay in April 2016, as

a function of their base pay. I find that about 10% of salaried workers earning $455-913 per week received overtime pay in April 2016. However, that is likely an underestimate of the true share of salaried workers who were covered for overtime since many workers do not work enough hours to claim overtime compensation in a given week. For example, even though all hourly workers are covered for overtime, Grigsby, Hurst and Yildirmaz (2021) documents that in a given month, only 37% of hourly workers in the ADP data actually receive overtime pay.[1] Under the strong assumption that the ratio of workers who receive overtime and those who are eligible for overtime is the same between hourly and salaried workers, I expect around 27% (i.e. $\frac{10\%}{0.37}$) of salaried workers were already covered for overtime prior to the 2016 FLSA rule change.

Next, I examine how much of the remaining share of non-bunched workers can be explained by firms reversing planned pay increases. To understand employers' thought process following the injunction of the FLSA rule change, I review six surveys conducted in the months after the Nov 22, 20216 court ruling. Appendix table B.I summarizes the results of these surveys.[2] Column (1) reports the results of a survey by Littler Mendelson, the largest employment and labor law firm in the US. In a survey of almost 900 employers in their network, Littler found that 50% of firms had already implemented changes in response to the policy and 39% did not implement their plans. Similarly, in column (2), a survey by the National Association of Independent Schools found that about half of schools already made adjustments in anticipation of the overtime rule change. Conditional on making an adjustment, 55% report increasing some workers' salaries above the threshold and nearly all of them report reclassifying at least one employee from exempt to non-exempt from overtime. Column (3) shows that 37% of respondents to the International Franchise Association likewise report already implementing their payroll changes, whereas 46% did not move forward

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[1]Similarly, although all salaried workers earning less than $455 per week are covered for overtime, the majority do not earn any overtime compensation.

[2]Each survey asked a different set of questions. I grouped the questions so that their responses would be comparable across surveys. Cells with no entry means the question was not asked in the survey.

with their plans.[3] Again, the most common response was to reclassify workers, rather than increase their pay. Overall, the results from the first three surveys suggest that about half of firms responded in anticipation of the FLSA rule change before it was retracted.

In columns (4) to (7) of table B.I, I find that not only did firms implement changes in anticipation of the rule change but many firms even made changes after the injunction, consistent with the wage hysteresis observed in the main text. In column (4), I report the results from a survey of 68 major retailers by the consulting firm Korn Ferry. I find that 31% of retailers already implemented their planned changes and another 25% said they intend to proceed as if the policy was still binding. Moreover, an additional 21% report that they will implement some, but not all, of their planned changes. In column (5), Korn Ferry narrows their question specifically to what firms intend to do about the employees to whom they were planning on bunching above the $913 threshold. In this case, I find that 65% of respondents say they will move forward as planned. Column (6) reports a similar pattern from a survey by the College and University Professionals Association for Human Resources (CUPA HR). I find that 28% of schools proceeded as planned and 32% only implemented changes for a portion of their employees. Conditional on colleges saying that they are only partially implementing planned changes, I find that 70% are moving forward with pay raises, and most of the delay is coming from reclassifications and hours adjustments. Similarly, in the 8% of schools that reported reversing some change they already made, only 9 schools report cutting salaries. Most retractions are for overtime coverage and salary/hourly classification. Aggregating across all responses (i.e. 28% + 0.7·32% + 0.77·8%), I find that 57% of colleges followed through with planned pay raises. Together, the surveys find that firms are more willing to retract reclassifications, but not pay increases.

Aside from the surveys conducted by various business associations, the Department of Labor also submitted a public Request for Information (RFI) in September 2017. The DOL received over 140,000 letters from companies, workers, and the general public. In column (7),

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[3]The remaining 17% gave no response.

I summarize the responses of the letters, where I hand coded each *informative* letter for any information on how their organization responded to the FLSA rule change.[4] Again, I find that a large share of firms (i.e. 37%) gave workers pay increases above the \$913 overtime exemption threshold. Only 2% of letters made any mention that they reversed a pay increase. Overall, across all six surveys, I consistently find no more than half of firms say they will wait until the final resolution of the court case to implement changes (row 9 in table B.I). The surveys thus indicate that at least 50% of firms were unable to retract salary increases. There is surprisingly little heterogeneity across the industries covered by the surveys, but the persistence of the policy appears stronger for planned pay increases than for reclassifications.

A limitation of the surveys is that employers have incentive to exaggerate the harm of the policy to their business and downplay its benefits to workers in an attempt to influence the Department of Labor's decision on future overtime rule changes. As another way to gauge the extent to which firms followed-through with planned pay increases, I benchmark my results to Quach (2024), which studies state-level changes to the overtime exemption threshold that were actually binding. The majority of state policies raised the threshold by only \$40 to \$80. In that setting, I find that about 16% of affected workers are bunched above the threshold. In comparison, even though the 2016 FLSA policy change was reversed, about 13% of workers within \$80 below the \$913 threshold are bunched.[5] There is no reason to expect state and federal changes in the overtime exemption threshold to have the same effect, even if they are both binding. Nevertheless, if I assume that the entire difference in the bunching mass is driven by firms delaying their planned pay increases, then the estimates suggest that 80% (i.e. $\frac{13}{16}$) of workers who were expected to receive raises actually got them.

In summary, the survey evidence and the cross-study benchmark show that even though

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[4]Over 134,000 of the letters were duplicates of the same few messages, driven by campaigns both in favor of and against the policy. Among the non-duplicates, the majority of letters were people voicing what they think the DOL should do, rather than explaining how their company responded to the DOL's policy. I only keep letters that had some information on how an employer reacted to the 2016 FLSA rule change.

[5]In April 2016, 5.5% of all salaried workers earned between \$833 and \$913 per week, of which 0.73 p.p. were bunched.

some employers reversed their planned pay increases, the majority behaved as if the policy was still binding. The estimates suggest that around 50-80% of firms followed-through with their intended response to the policy change. The reasons why only a small share of affected workers are bunched is because the majority either were supposed to gain overtime coverage or were already covered for overtime. One potential explanation for why some firms were able to retract pay increases while others were not is that the employers that were more proactive and made promises to their workers in advance could not renege on their promises without lowering morale. Anecdotally, a report by CUPA HR stated that "colleges and universities said they were reluctant to reverse increases to salaries after those changes were implemented or announced for reasons related to morale and fairness" (College and University Professional Association for Human Resources, 2017).[6] Thus, if one trusts the anecdote, then an interpretation of the heterogeneity is that the temporary overtime policy only had persistent impacts if firms already made promises to workers. For a more conservative interpretation, the surveys still imply that regardless of the mechanism, wage hysteresis affects at least half of all employers.

B.2 Firm's Selection Criteria for Bunching

Who then are the workers who get bunched above the threshold? From the perspective of the firm, it makes sense to cover a salaried worker for overtime if

$$\text{Cost of paying OT} + \text{Cost of monitoring and adjusting hours} < \text{Cost of bunching} \quad (7)$$

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[6]On the other hand, employees tend to prefer being an exempt salaried worker, hence why Table B.I saw more delays and reversals for reclassifications. In the same report, CUPA HR writes "in contrast, many colleges and universities reported waiting until closer to the compliance deadline to reclassify employees as employees were generally not enthusiastic about this change. Institutions also informed us that they tended to reverse more reclassifications than pay increases that were implemented for the same reason" (College and University Professional Association for Human Resources, 2017).

For example, a salaried employee who always works exactly 9am-5pm on weekdays would be costless to cover for overtime. Even if they tend to work 41 hours per week, it may be cheaper for the employer to simply cut their hours than to raise their base pay. On the other hand, firms would want to bunch workers who either work long hours or have difficult to monitor hours.

As direct evidence that monitoring costs matter, Table B.II reports the responses of firms to the types of costs they incurred or expect to incur when complying with the FLSA rule change. In column (1), the survey by Littler Mendelson finds that while the majority of firms experienced an increase in overtime costs, the second most common cost is for tracking workers' hours, with over half of firms reporting an increase in timekeeping costs. Similarly, column (2) shows in a separate survey by the International Franchise Association that 63% of franchises report an increase in "timekeeping and/or payroll administration costs". Lastly, in column (3), I report summary statistics from data that I manually coded from reviewing the letters received in response to the DOL's request for information. I keep all responses in which the firm either reported adopting the FLSA rule change or simply complained about an increase in costs.[7] Although firms are primarily concerned about something related to the monetary costs of bunching workers or paying overtime, a comparable share of employers also report incurring a cost related to a decrease in flexibility. Among those concerned with a loss in flexibility, 41% of employers claim that monitoring workers' hours lowers morale, 22% say it is administratively burdensome to monitor hours, and a smaller share of firms worry about either a decrease in productivity, difficulty in managing hours for highly seasonal work, and challenges responding to unexpected events. Together, the multiple surveys suggest that monitoring and adjusting hours play an important role in firms' costs of compliance with the overtime regulation.

If firms are choosing to bunch workers according to equation 7, then I expect four predictions to be true. First, firms are more likely to bunch workers with salaries close to the $913

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[7]The sample is larger than the one used in table B.I, which only kept firms that explicitly described how they responded to the reform.

overtime exemption threshold. Second, heterogeneity in bunching across workers is driven by within-firm variation, rather than just across-firm variation. Third, firms are more likely to bunch workers who work long hours. Fourth, occupations with flexible hours are more likely to be bunched. The first prediction is already validated from figure I and I test the remaining predictions below.

Prediction: Selection within-firm

To start, I show that the decision to bunch workers is a within-firm decision. That is, even within the same firm, employers may decide to bunch some occupations but not others. As evidence, I show in Figure B.II the response of employers to a survey conducted by World at Work in September 2016 that aimed specifically to understand how firms planned to adjust to the upcoming overtime rule change (World at Work, 2016). The survey covered a wide range of industries and included major firms such as FedEx, Dollar General, T-Mobile, and Nestle. In general, the survey finds that the majority of firms plan to both raise some workers' salaries above the $913 threshold and also reclassify other workers to non-exempt.[8] This suggests that the heterogeneity in responses to the policy is largely a within-firm phenomenon. The result that firms may only bunch some workers but not others suggests that if peer comparisons is an important feature of internal labor markets, it is likely stronger within-occupations than across.

Prediction: Bunch workers with long hours

Next, I test the prediction that firms are more likely to bunch workers with long hours. Unfortunately, I cannot directly test this prediction since the ADP data does not record the hours of workers at baseline when they were exempt from overtime. Instead, I test a corollary to the prediction: if firms are bunching employees who work long hours, then the policy should have minimal impact on the average amount of overtime compensation that

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[8]The same survey also found that half of firms report a decrease in workplace flexibility, consistent with the idea that firms face a monitoring cost to comply with the policy.

workers receive, since the workers who gain overtime coverage are selected to have shorter hours. I use the ADP data to provide direct empirical evidence of the impact of the 2016 FLSA rule change on overtime pay.

Figure B.III replicates the difference-in-difference analysis from section 4.3 using the amount of weekly overtime pay as the outcome variable. Specifically, I plot the estimates of equation 2, where I compare all salaried workers with base pays between $455 and $913 per week in April 2016 to a control group with base pays between $913 and $993. Since many salaried workers earn 0 overtime pay, I estimate the regression in levels rather than logs. The figure shows that in the year prior to the announcement of the FLSA rule change, overtime pay was trending fairly similarly between workers targeted and not targeted by the policy. However, exposed workers see an increase in overtime pay in the months leading up to the reform, culminating in an average increase of about $2 per week after December 2016. Similar to the findings of the main text, the increased earnings from overtime pay continue to persist even 1.5 years after the injunction of the reform.

The $2 increase in overtime pay is about a third of the cost relative to the increase in base pay. Figure V finds that base pay increased on average by 1%, which translates to approximately a $7 increase in weekly earnings. Quach (2024) similarly finds that binding state policies have only a small impact on overtime pay. The relatively small increase in overtime pay suggests that even though the majority of affected workers become eligible for overtime, they do not work enough hours in a week to receive overtime compensation. One reason for the small effect on overtime pay is that firms are choosing to bunch employees who work long hours, as predicted above. A second reason could be that employers are also cutting workers' hours to avoid paying overtime. These mechanisms are not mutually exclusive, but without data on workers' baseline hours, I am unable to distinguish their relative importance.

Together, the increase in base pay and overtime pay suggests that the cost of employing affected workers would have increased by about 2% had the policy remain binding. At

baseline, the average affected worker was earning about $700 per week. After the injunction, base pay and overtime cumulatively increased by about $9, translating to a 1.3% increase in labor costs. However, the previous section found that only 50-80% of firms complied with the policy. As a result, if the policy was fully binding, payroll costs would have increased by approximately 1.6-2.6%.[9] This calculation is only a lower bound for the total cost of the policy though, as it does not account for the cost of monitoring or restricting workers' hours, which surveys suggest is also a large component of the cost of compliance.

Prediction: Bunch workers with flexible hours

To determine whether firms bunch workers with flexible hours, I would ideally like data on salaried workers' hours before the policy intervention, their occupation titles, and their exemption status, none of which are observed in the ADP data. Instead, I use data on public sector workers from ten states to examine exactly which occupations are being bunched above the $913 threshold. Specifically, I draw on public employee records from Arizona, California, Connecticut, Florida, Maryland, Minnesota, New Jersey, New York, Texas, and Vermont. Table B.III summarizes the characteristics of the different datasets. I aggregate all data to the annual level and restrict the data to solely salaried employees whenever possible. Importantly, all the datasets contain a job title for each employee so that I can examine heterogeneity in bunching by occupation.

To start, I validate that I observe a similar bunching effect among public sector employees. Figure B.IV plots the change in the distribution of weekly base pay over time for Arizona, which only contains data on Arizona State University. Similar to the analysis in the main text, I observe no major changes in the pay distribution in the years prior to 2016, and then a sharp bunching mass at exactly the $913 threshold after 2016 that persists for at least 4 years.[10] Figure B.V repeats the same analysis for each of the 10 states and NYC, plotting

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[9]For comparison, Quach (2024) finds that binding state policies increased workers' earnings by about 1.5%.

[10]The data is published before the end of the year so there is no bunching in 2016.

the change in the distribution of weekly base pay between 2015 and 2017. Interestingly, I only observe clear bunching at the $913 threshold among three states: Arizona, Maryland, and Texas. These are precisely the three states for which the data only covers workers employed by the public university system. It appears that there is something unique about the higher education sector that makes raising workers' salaries less costly than covering them for overtime.

Focusing on university employees, I next apply textual analysis to identify the occupations that are being bunched above the $913 overtime exemption threshold. I first train a predictive model using the ASU data and then I test my model out-of-sample using University of California (UC) and University of Connecticut (UConn) employees. To predict bunching by occupation title, I first partition affected salaried workers in ASU into two groups: those who earn above $913 per week after 2016 (i.e. bunched workers) and those who earn below (i.e. non-bunched workers). Next, I use an n-gram model to measure the probability that different words appear in the title of bunched and non-bunched workers. Finally, I compute the difference in these probabilities between bunched and non-bunched workers to remove words that appear very frequently across all occupations. For instance, the word "specialist" is by far the most common word in job titles across all workers. I take the difference in the probability that "specialist" appears in bunched and non-bunched job titles to focus on the relative probability of the word.

Figure B.VI plots the top 20 most common terms among bunched and non-bunched workers in ASU. In panel (a), I find that bunched workers are often in research or teaching roles with terms like "postdoc", "assoc" (i.e. associate), "lecturer", and "research". In contrast, panel (b) shows that non-bunched workers tend to be in more administrative roles with job titles that contain "administrative", "office", and "sr" (i.e. the equivalent of associate for promotion in admin roles). This dichotomy is consistent with my hypothesis that firms would bunch workers that have a high cost of monitoring or adjusting hours. In general, research and teaching positions tend to have unpredictable hours, with employees possibly

doing much of their work off campus, whereas administrative jobs are more often a standard 9am-5pm position with less variation in hours.

To validate my predictive model out-of-sample, I test for bunching among UC and UConn employees separately by research and admin roles, which I define using the 20 top and bottom most common terms from the n-grams in Figure B.VI.[11] In panel (a) of figure B.VII, I first demonstrate that the model predicts bunching within the ASU sample. As expected, only research/teaching positions experience a bunching mass above the overtime exemption threshold. In panels (b) and (c), I repeat my analysis using UC and UConn employees and find a similar result, albeit the bunching mass in these states are smaller. This bunching among researchers and lecturers in the UC and UConn systems stands in contrast to the average effect across all public sector employees in their respective states presented earlier in figure B.V.

In summary, evidence from company surveys, the ADP data, and a case study of university employees are all consistent with the hypothesis that employers choose to bunch individuals who work long or flexible hours. As a result, the estimates in the main text should be interpreted as measuring the wage dynamics of white collared salaried workers with flexible hours for whom it would be cheaper for firms to bunch at the $913 threshold than cover for overtime.

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[11]I drop "specialist" when defining admin roles because 20% of all roles in universities, academic or admin, are called a specialist.

Appendix Figure B.I: Probability of Overtime Conditional on Base Pay, April 2016
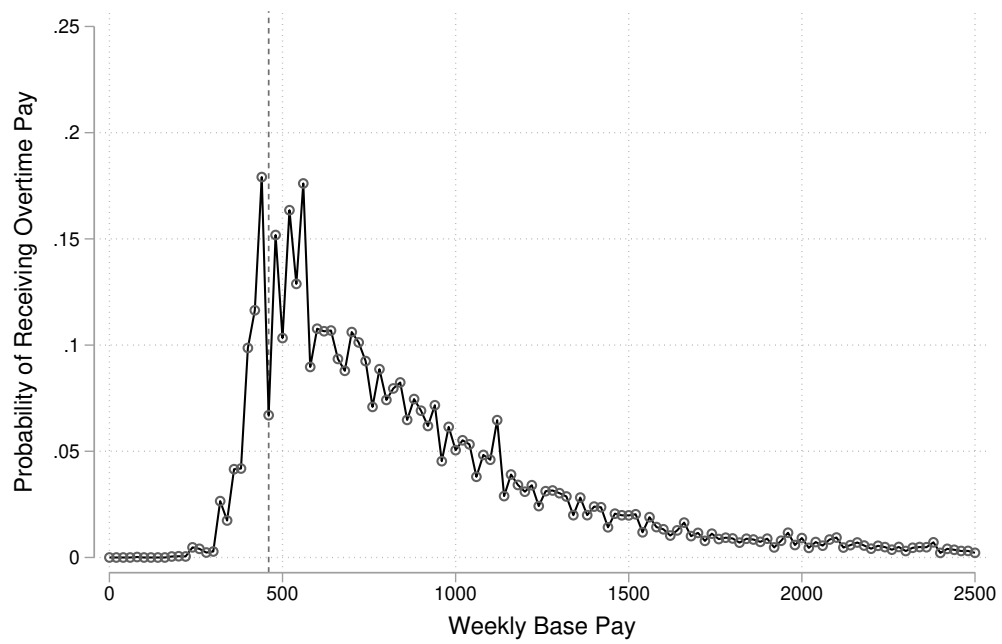
Notes: This figure plots the probability that an individual receives overtime pay as a function of their base pay. The dotted vertical line is at $455 per week. The figure is replicated from Quach (2024).

Appendix Table B.I: Surveys of Employers' Response to FLSA Injunction

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Already implemented changes	50%	49%	37%	31%			
Pay raises		55%	28%				37%
Reclassifications (i.e. cover for overtime)		98%	48%				58%
Go forward as planned				25%	65%	28%	
Implement some changes				21%		32%	
Go ahead with pay raises						70%	
Go ahead with reclassifications						41%	
Go ahead with changing hours						15%	
Wait until final resolution of legal case	39%		46%	24%	35%	32%	5%
No plans	11%						
Reversing some changes						8%	
Reverse some or all pay raises						23%	2%
Reverse some or all reclassifications						95%	13%
Date	Post Injunction	Post Injunction	Post Injunction	Nov 25-29, 2016	Nov 25-29, 2016	Dec 2016	Sept 2017
Sample	Multiple industries	Independent Schools	Franchises	Retail	Retail, conditional on planned to raise salaries	Colleges and Universities	Open Forum
Survey	Littler Mendelson	NAIS and NBOA	IFA	Korn Ferry	Korn Ferry	CUPA HR	DOL RFI
N	∼900	369	∼300	68	68	495	105

Notes: This table reports the responses of employers to six surveys conducted specifically to ask employers how they plan to respond to the injunction of the 2016 FLSA overtime rule change. Rows (1)-(3) show the share of firms that already implemented their planned changes before the injunction occurred, and the actions they took conditional on making a change. Row (4) shows the share of firms that report they plan to go forward with their planned changes despite the injunction. Rows (5)-(8) report the share of firms that plan to implement only some of their plans, and which of these plans they will implement conditional on implementing at least one. Row (9) shows the share of firms that plan to wait until the final resolution of the court case. Row (10) shows the share of firms that made no plans to adjust to the FLSA rule change. Rows (11) to (13) show the share of firms that reversed changes they already made, and if they did, the types of reversals. An empty cell indicates that the question was not asked in the survey.

Appendix Figure B.II: Survey of Planned Response to 2016 FLSA Rule Change
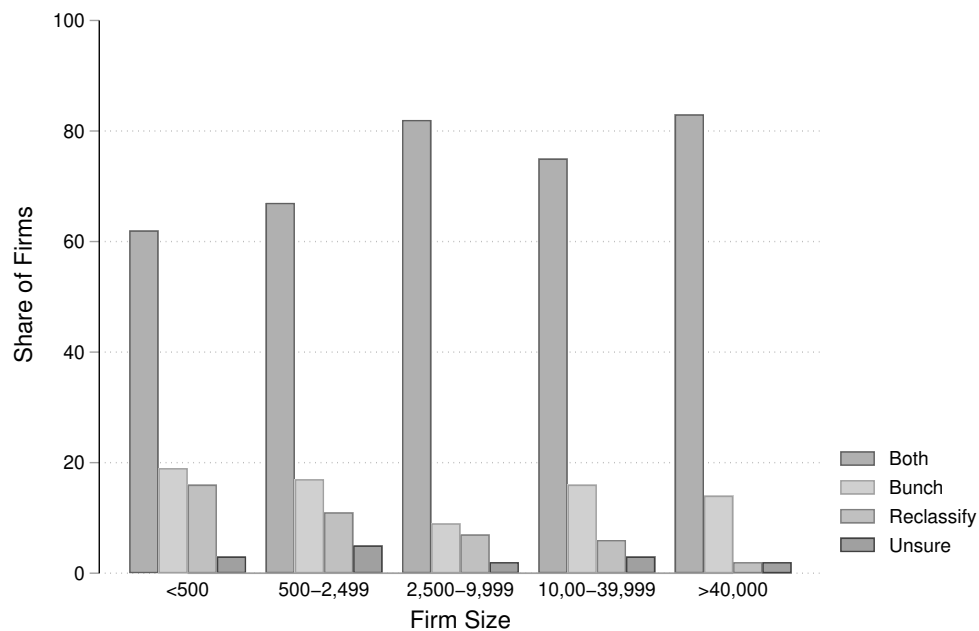
Notes: This figure plots employers' response to the question "How are you addressing or planning to address employees that were exempt under the old rules who fall below the new standard salary level threshold?". The survey was conducted by World at Work from Sept 23-30, 2016. There were 89 respondents with <500 employees, 172 with 500-2,499 employees, 216 with 2,500-9,999 employees, 141 with 10,000-39,999 employees, and 59 with over 40,000 employees.

Appendix Table B.II: Costs of Complying with the 2016 FLSA Overtime Rule Change

	(1)	(2)	(3)
Overtime Costs	73%	75%	
Training Costs	17%	17%	
Timekeeping	53%	63%	
Managerial Cost of supervising non-exempt	38%	38%	
Modifying policies	53%	49%	
Change benefits	19%	26%	
Travel time	24%		
Budget			37%
Flexibility			29%
Productivity			12%
Seasonal			12%
Unexpected Events			10%
Monitoring			22%
Morale			41%
Date	Post Injunction	Post Injunction	Sept 2017
Sample	All	Franchises	Anyone
Survey	Littler Mendelson	IFA	RFI
N	∼900	∼ 300	238

Notes: This table reports the response of firms to three surveys asking about firms' cost of complying with the 2016 FLSA rule change. In column (1), I report the response to the question "Did your organization incur, or anticipate incurring any of the following costs?". In column (2), I report the share of firms that reported a "substantial increase in operational costs" due to different factors asked by the International Franchise Association. In column (3), I report the share of firms that reported a budget or flexibility cost to the Department of Labor's request for information. Conditional on reporting some flexibility cost, I disaggregate the response by the source of the cost.

Appendix Figure B.III: Difference-in-Difference Comparing OT Pay Between Affected and Unaffected Salaried Workers
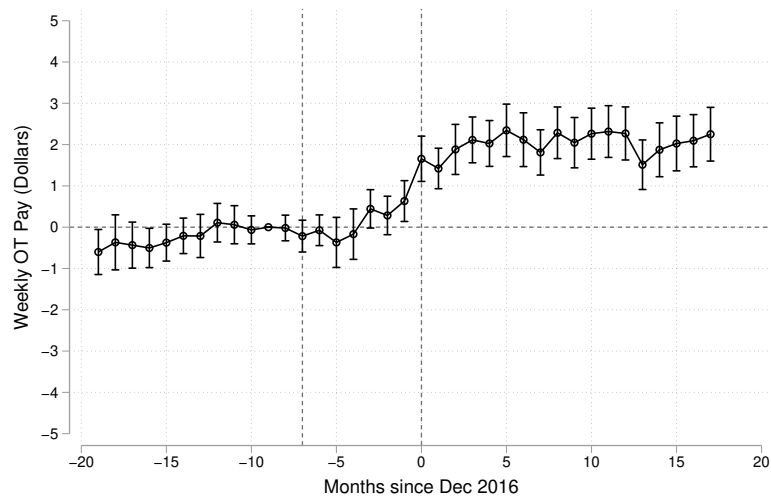
Notes: This figure plots the difference-in-difference estimates from equation 2 to show the effect of the 2016 FLSA rule change on the amount of weekly overtime pay. The treatment group in the regression are salaried workers earning $455-913 per week in April 2016. The control group are salaried workers earning $913-993 per week in April 2016.

Data	Frequency	Sample	Job Title	Base Pay	Salary/Hourly	Years
AZ	Annual	Arizona State University	Yes	Annual Salary/52	No	2012-2022
CA	Annual	All Public Employees	Yes	Regular pay/52	No	2013-2022
CT	Paystub	All Public Employees	Yes	Biweekly/2	Yes*	2015-2024
FL	Annual	Executive Branch Agencies	Yes	Annual Salary/52	Yes	2016-2018, 2024
MD	Annual	University of Maryland	Yes	Biweekly/2	Yes	2013-2019
MN	Annual	Executive and Judicial Branch	Yes	Biweekly/2	Yes	2011-2023
NJ	Quarterly	Executive and Judicial Branch	Yes	Annual Salary/52	Yes	2010-2024
NY	Annual	Executive and Judicial Branch	Yes	Annual Salary/52	Yes	2011-2018
NYC	Annual	Municipal Employees	Yes	Annual Salary/52	Yes	2015-2020
TX	Annual	Texas A&M University	Yes	Annual Salary/52	No	2015, 2018
VT	Annual	Executive and Judicial Branch	Yes	Annual Salary/52	No	2015-2020

Notes: This table describes the public sector datasets used in my analysis. "Frequency" refers to the aggregation level of the data. "Sample" refers to which employees are contained in the data. "Job title" refers to whether the data contains occupation titles of employees. "Base Pay" refers to how I define weekly base pay for the purchase of the overtime exemption rule. "Salary/Hourly" refers to whether the data contains a salary/hourly indicator. For CT, I define a worker as salaried if their "standard rate of pay" exceeds $200.

Appendix Figure B.IV: Difference-in-Distribution of Weekly Base Pay Relative to 2015, ASU

Notes: This figure plots change in the distribution of weekly base pay relative to 2015. The sample consists of all ASU employees, including salaried and hourly. The two vertical dashed lines are at $455 and $913 weekly base pay, respectively.

Appendix Figure B.V: Difference-in-Distribution of Weekly Base Pay from 2015 to 2017, Public Sector Workers
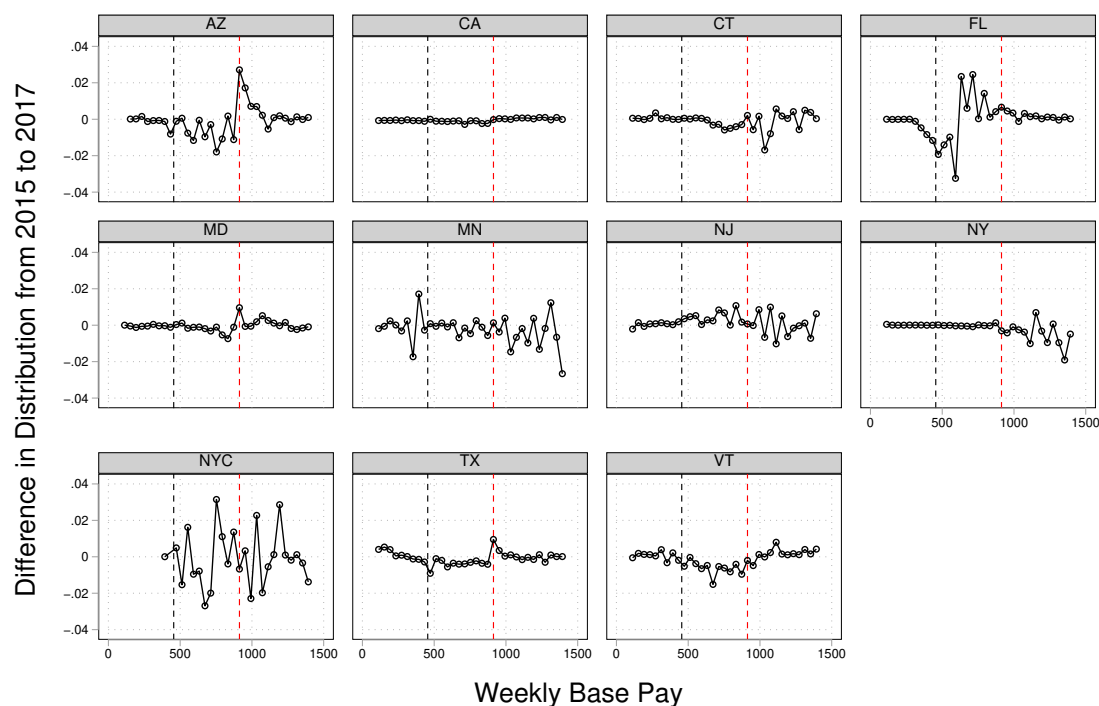
Notes: This figure plots the change in the distribution of weekly base pay between 2015 and 2017 among public sector workers across ten states and NYC. Since Florida does not have data starting in 2015, I use 2016 as the base.

Appendix Figure B.VI: Likelihood of Being Bunched or Non-Bunched within ASU
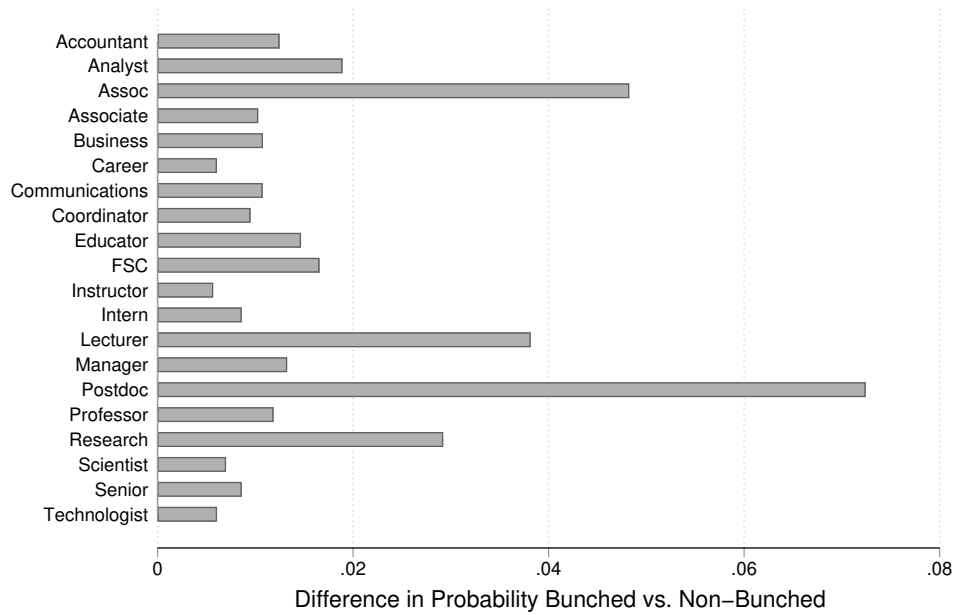
(a) Most likely to be bunched
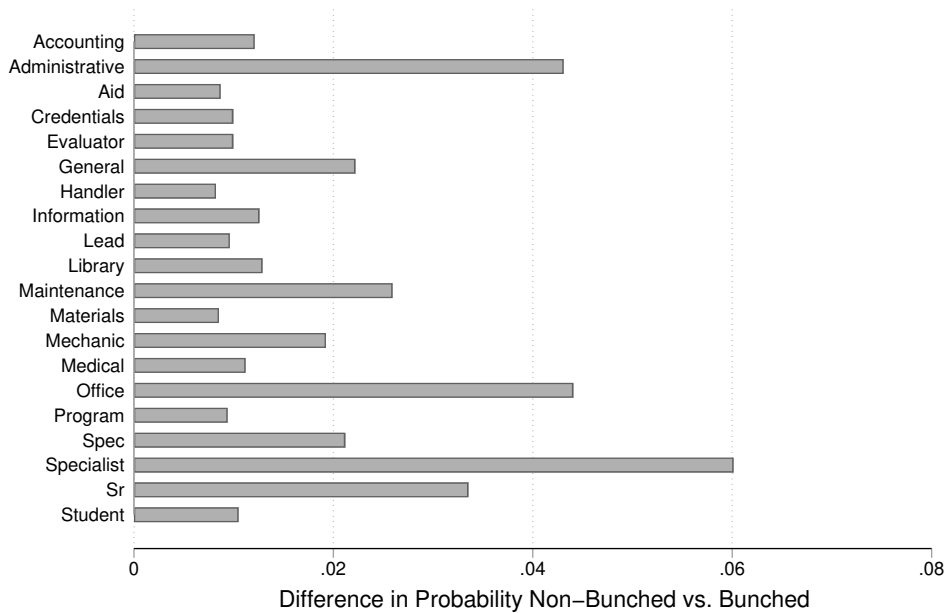
(b) Least likely to be bunched

Notes: Panel (a) plots the top 20 words with the largest difference in probability that the word appears in the job title of bunched workers relative to non-bunched workers. Panel (b) plots the bottom 20 words. Bunched workers are defined as individuals affected by the FLSA rule change given their 2015 base pay but earned above $913 per week in 2017. Non-bunched workers are affected by the FLSA rule change, but did not earn above the $913 threshold in 2017. The sample is restricted to employees at ASU.

Appendix Figure B.VII: Difference-in-Distribution of Weekly Base Pay Between 2015 and 2017, by Admin and Research Employees
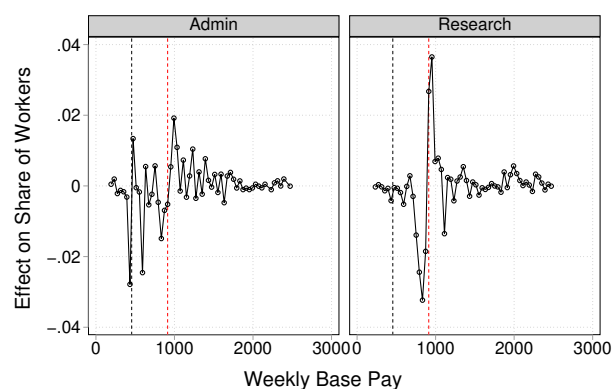
(a) Arizona State University
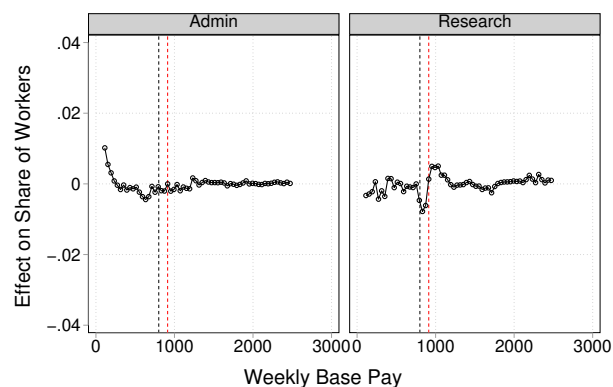

(b) University of California
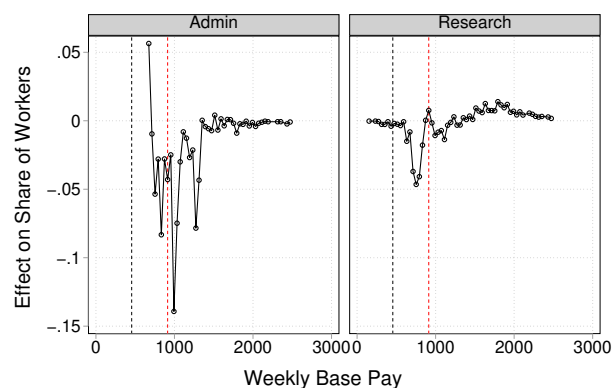
(c) University of Connecticut

Notes: This figure shows the change in the distribution of weekly base pay between 2015 and 2017 for employees at Arizona State University, the University of California, and the University of Connecticut separately by administrative and research positions. The black and red lines mark the change in the overtime exemption threshold. Note that California had a higher initial threshold than other states.

Appendix C.   Long-run Wage Persistence

Figure C.I plots the change in the distribution of salaried workers over time relative to April 2016. This figure spans February 2016 to December 2019. Note three methodological differences between figure C.I and figure II in the main text. First, the outcome variable in figure C.I is the difference in the *number* of salaried workers, rather than the difference in shares. This alternative measure does not have any qualitative impact on the figure, but makes the magnitude of the bunching mass harder to interpret without knowing the baseline number of workers at the threshold. Second, the sample consists of repeated cross-sections of salaried workers, rather than a balanced panel of workers who were salaried in April 2016. Analyzing repeated cross sections exaggerates the missing mass in December 2016, because some affected salaried workers were reclassified from salaried to hourly. While the decrease in salaried jobs appears larger in December 2016 since it drops reclassified workers, the repeated cross-section has little impact on the persistence of that missing mass or the bunching effect. Third, I do not use pre-2016 data as a control in figure C.I. In the main text, I used stayers from April 2013-2016 as a control to eliminate trends in wage growth, which would naturally shift workers from the left tail of the distribution to the right tail. Without this control, the missing mass in figure C.I naturally grows over time as wages increase to the right of the distribution. Despite these methodological differences, figure C.I shows a persistent impact of the 2016 FLSA rule change, similar to the main text. The missing mass in the $455-913 pay interval persists even three years after the policy injunction. In comparison, by early-2019, there was no difference in the number of workers earning $913 per week relative to April 2016. However, that is to be expected given that section 4.4 finds bunched workers continued receiving pay raises following the injunction, rather than have their salaries stagnant at $913 per week. Overall, the evidence suggests that the impact of the 2016 overtime reform persisted even 3 years after its injunction.

Appendix Figure C.I: Distribution of Salary Workers Over Time, Relative to April 2016
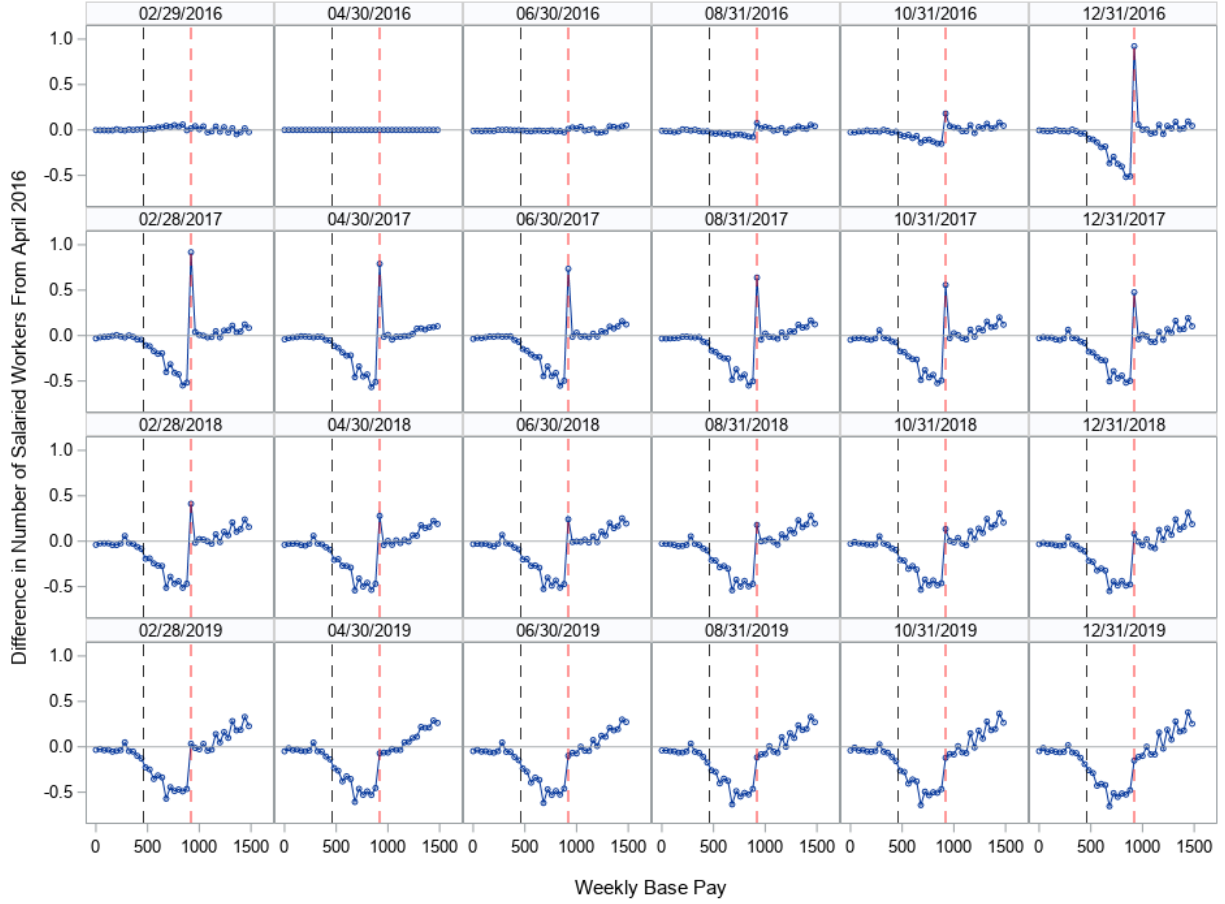
Notes: This figure plots the change in the average number of workers over time relative to April 2016. The x-axis is divided into $40 increments of weekly base pay. The sample consists of repeated cross-section of salaried workers for firms that stay in the ADP data from April 2015 to December 2019.

Appendix D.   Role of Staggered Bargaining

In this section, I first replicate the evidence for staggered bargaining highlighted by Grigsby, Hurst and Yildirmaz (2021), but then show that this evidence does not explain the dynamics in bunching at the overtime exemption threshold observed in the data.

To begin, I present two pieces of evidence that wages are adjusted on a yearly basis. First, figure D.I plots the distribution of the number of wage adjustments that workers receive between each pair of consecutive months from May 2015-2016. Over 80% of salaried workers experience no more than 1 pay increase in the one year interval. Second, figure D.II shows that for each time a worker receives a pay change, there is a 50% chance that the last wage adjustment occurred exactly 12 months prior. Together, the figures suggest that workers' salaries adjust on the exact same month each year, if they adjust at all.

Next, I argue that the periodicity of wage adjustments does not explain the observed wage dynamics before and after the proposed change to the overtime exemption threshold announced in 2016. First, if the only cause for the persistence in bunching at the threshold after December 2016 is due to staggered bargaining, then the bunching mass should experience a sharp drop exactly a year after its injunction. After all, not only do wage changes occur in 12 month intervals, but figure D.III shows that the majority of wage changes also occur in January. Thus, one might expect to see a large decline in the bunching mass on January 2018. In contrast, figure III finds a constant trend in the magnitude of the bunching mass over time. As a result, the persistence in the bunching mass cannot be solely explained by periodicity in wage bargaining.

Moreover, there is no evidence that the anticipatory bunching before December 2016 is due to workers' wages adjusting on exactly the same calendar month that they usually change. For example, given that workers tend to receive wage changes once every 12 months, one might expect that the early-bunching in October 2016 is driven by workers who also received a pay increase in October 2015. To test that hypothesis, figure D.IV plots the

magnitude of the \$913 bunching mass over time, estimated from equation 1, separately by the month that workers received a pay increase in the year prior to the policy announcement. I find that leading up to the first pay increase before May 2016, the evolution of the share of firms' workers at the \$913 threshold was perfectly parallel to the evolution in 2014, consistent with the evidence that workers only receive 1 pay increase per year so neither treated nor control group experienced any pay changes. After the first change in base pay, workers continue to experience no wage increases, leading again to incredibly straight parallel trends. However, after the announcement of the new overtime exemption threshold in May 2016, the share of bunched workers started rising even before December 2016. This anticipatory response does not appear to be stronger on the month that workers received a pay increase prior to the announcement of the policy. For instance, workers who received a raise in October 2015 were already being bunched in July 2016, and the magnitude of the bunching mass in October 2016 was not significantly larger than for other types of workers. Overall, there does not appear to be any correlation between the month of workers' usual pay increase and the month their wages adjusted to the new overtime exemption threshold.

While the primary focus of my paper is the persistence in wages post-injunction, I nevertheless provide a brief analysis into the understanding which firms are bunching workers early pre-injunction. Interestingly, the data suggests that small firms are less likely to react early relative to large firms. Figure D.V plots estimates of the magnitude of the bunching mass over time, separately for firms below and above the median firm size in the data. While I do not have enough statistical power to make a conclusive statement, the coefficients suggest that large firms already saw an increasing trend in the number of workers at the overtime exemption threshold starting in July 2016. On the other hand, the share of bunched workers in small firms remain relatively more constant until at least October 2016. A more detailed examination into why large firms tend to increase wages even before the month that the new threshold was supposed to go into effect is beyond the scope of this paper, and a potentially interesting question for future work.

Appendix Figure D.I: Distribution of Workers by Number of Wage Changes per Year
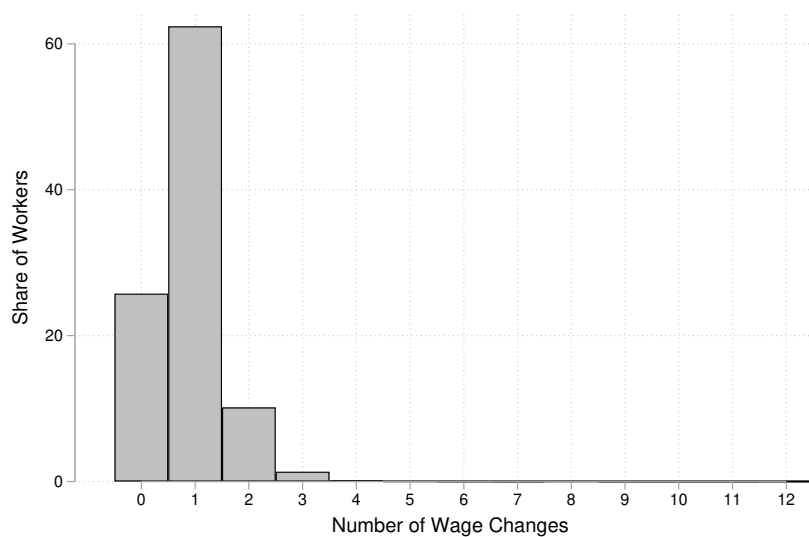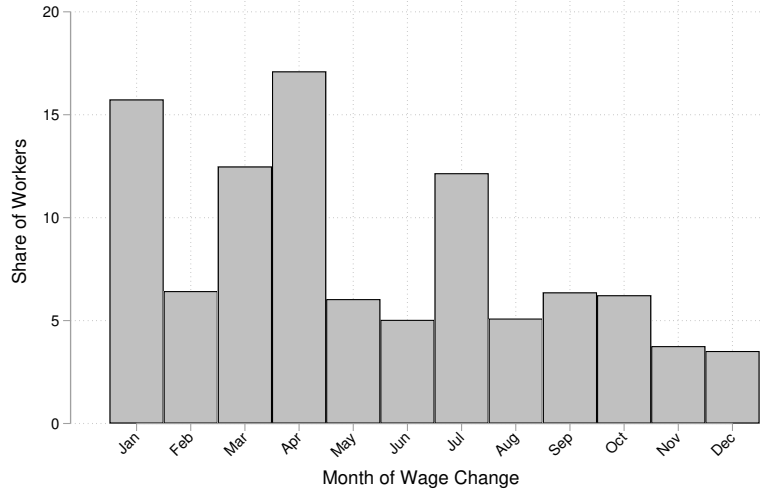

Notes: This figure shows the distribution of workers by the number of months they experienced a change in base pay relative to the previous month, for each pair of months from May 2015-2016. The sample comprises of salaried workers who are continuously employed between May 2015-2018.

Appendix Figure D.II: Distribution of Months Since Last Pay Increase Among all Wage Changes
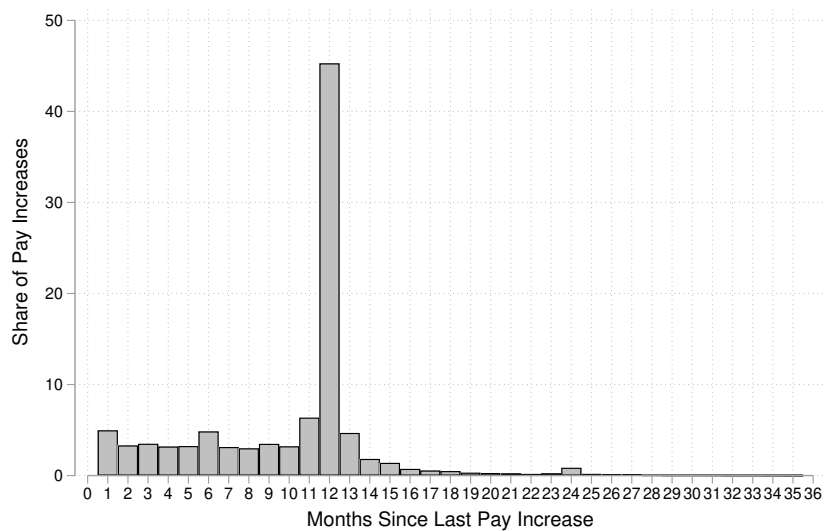
Notes: This figure restricts the sample to all wage changes among salaried stayers between May 2015-2018, and plots the distribution of the number of months since the previous pay increase.

Appendix Figure D.III: Distribution of Month of Pay Increases

(a) Distribution of Workers by Month of Wage Change

(b) Distribution of Firms by Month of Most Wage Changes

Notes: Panel (a) shows the share of wage-changes by the month of pay increase between June 2015 and May 2016. Panel (b) plots the distribution of firms by the month for which the majority of their workers receive a pay increase. In both cases, the sample comprises of workers who are continuously employed between May 2015-2018.

Appendix Figure D.IV: Share of Workers Bunched Over Time, by Month of Last Pay Increase Before May 2016
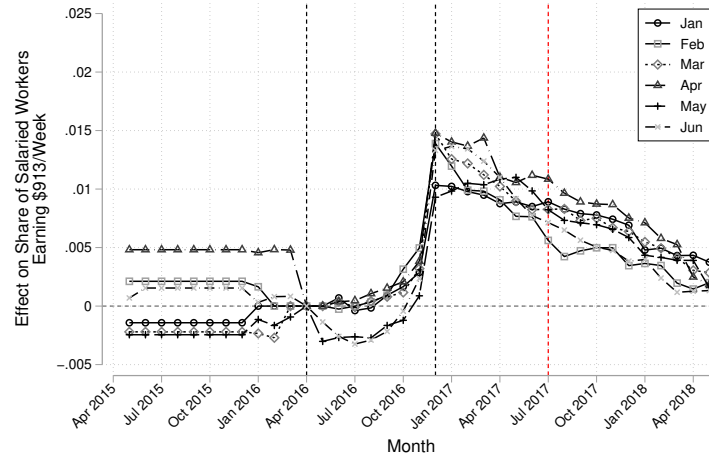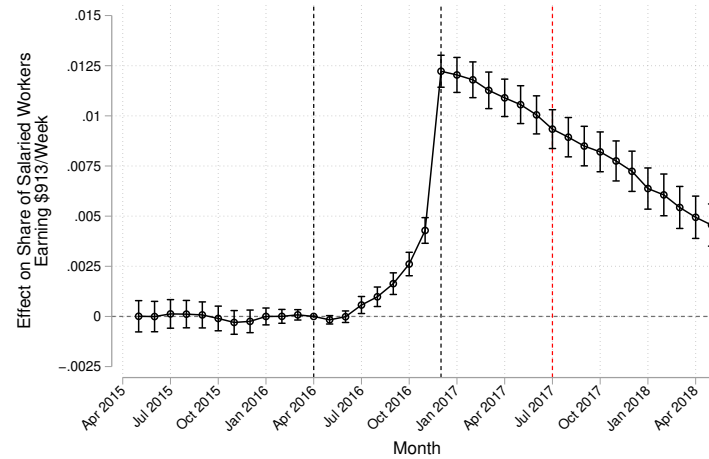

(a) January to June Wage Changers
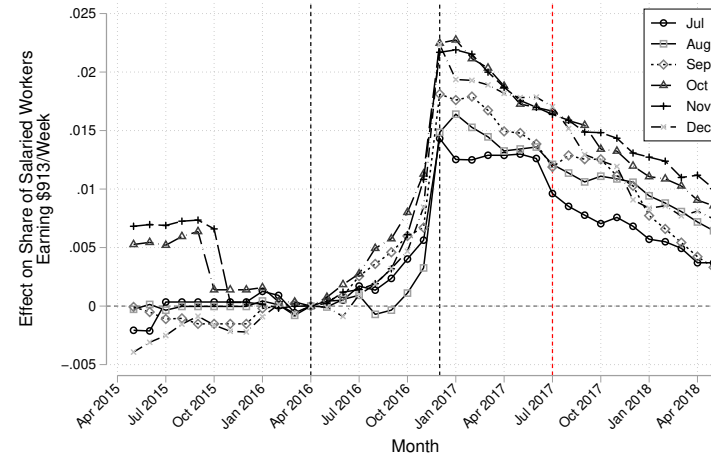
(b) July to December Wage Changers

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Notes: These figures plot the effect of the 2016 FLSA policy on the share of salaried workers earning between $913 and $953 per week, estimated from equation 1, separately by the month that workers received a pay increase in the year before the policy announcement. The treatment sample consists of workers who are always salaried, continuously employed at the same firm between May 2015 and April 2018, and received a pay change in the year prior to May 2016. The first vertical dash line is at May 2016 when the $913 threshold was first announced, the second vertical dash line is at December 2016 when the threshold was supposed to go into effect, and the third red dashed line is at July 2017 which is the first month after the Department of Labor dropped their defense of the $913 threshold.

Appendix Figure D.V: Share of Workers Bunched Over Time, by Firm Size
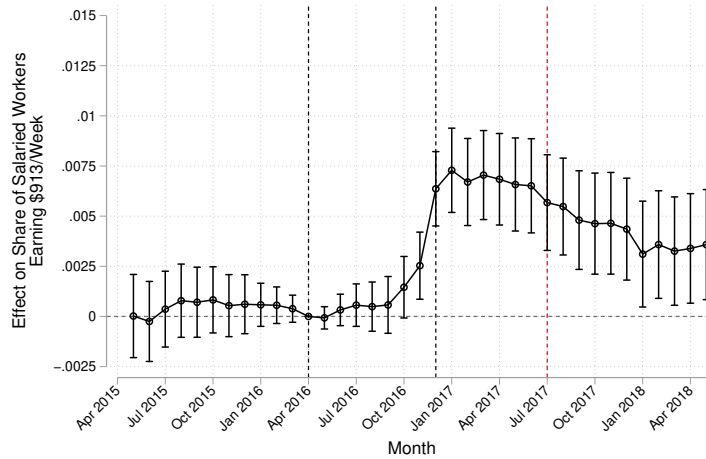
(a) Firms Below Median Employment Size

(b) Firms Above Median Employment Size

Notes: These figures plot the effect of the 2016 FLSA policy on the share of salaried workers earning between $913 and $953 per week, estimated from equation 1, separately for small and large firms. The treatment sample consists of workers who are always salaried, continuously employed at the same firm between May 2015 and April 2018. The control group are similarly defined workers from two years prior. The first vertical dash line is at May 2016 when the $913 threshold was first announced, the second vertical dash line is at December 2016 when the threshold was supposed to go into effect, and the third red dashed line is at July 2017 which is the first month after the Department of Labor dropped their defense of the $913 threshold.

Appendix E. Regression Discontinuity Design to Evaluate Wage Rigidity of New Hires

In this section, I implement a regression discontinuity type design to test whether workers hired after May 2016 with a base pay at the $913 overtime exemption threshold were more positively selected or experienced slower wage growth relative to their predicted productivity and wage growth absent the policy. Intuitively, I use the characteristics of new hires in parts of the income distribution away from the $913 cutoff to predict the counterfactual outcome of workers hired at the threshold. I then use deviations from the prediction as an indication of the effect of the policy on bunched hires' outcomes. The analysis relies on the assumption that the outcomes of new hires follow a continuous linear trend as a function of their entry wage, which I validate using data from before the proposed change to the overtime exemption threshold.

To start, figure E.I plots new hires' weekly base pay at their last observed employer as a function of their base pay at their current employer, separately for workers hired before and after the FLSA policy change. In panel (a), I find that a linear line does a reasonably good job of predicting the past wage of workers hired from May 2015-2016. Crucial for my analysis is the observation that past salaries are continuous and fairly linear locally around the $913 per week threshold. If workers' past salaries are a good proxy for their productivity and employers become more selective after the announcement of the new overtime rule, then I would expect the cohort of new hires after May 2016 to have prior wage histories that lie on or above the linear prediction line. In contrast, panel (b) finds that after May 2016, workers hired at the threshold have lower past salaries than predicted by the linear fit. Moreover, workers' hired to the left of the $913 threshold also have lower than predicted salaries. Together, the plot suggests that firms are choosing the most productive workers among those who would usually be paid to the left of the $913 cutoff and moving their salaries up to the new overtime exemption threshold. The results therefore suggest that employers are

not changing the composition of new hires, but are instead raising entry wages relative to what they would have paid absent the policy.

Inspired by the linearity in entry base pay from figure E.I, I estimate the following regression for 3-month cohorts of new hires:

$$y_{it} = \beta_t + \beta_{1t}v_{it} + \beta_{2t}D_{it} + \varepsilon_{it} \tag{8}$$

where y_{it} is the last observed weekly base pay of worker i hired at time t. I control for a time specific constant β_t and a linear trend in entry base pay v_{it}, normalized to zero at \$913. The coefficient of interest is β_{2t}, which measures the deviation of outcomes from the linear trend for new hires earning \$913-953 per week, indicated by the dummy D_{it}. I restrict the sample for the local linear regression to new hires paid between \$600 and \$1200 per week.

Figure E.II plots the estimates of β_{2t} over time for two outcome variables: new hires' prior salary and workers' salaries 18 months after hire. In panel (a), I highlight three main points. First, there is no detectable discontinuity in new hires' prior base pay at the \$913 threshold in any quarter from May 2015 to October 2016. Second, there is a large discontinuity starting in precisely November 2016 when the bunching mass at the overtime exemption threshold is largest. Third, the discontinuity persists for a year after the court injunction of the policy. Taken together, the timing and location of the discontinuity aligns closely with the conclusion that firms did not positively select more productive hires at the \$913 threshold, but instead gave real pay increases as a direct result of the FLSA rule change. In contrast, although panel (b) finds a negative discontinuity in future salaries, this discontinuity is relatively constant through the study period, suggesting that employers did not compensate for the elevated initial salary with slower wage growth.

I show the robustness of my results to alternative specifications in table E.I. The first estimate in column (1) corresponds to the Nov 2016 - Jan 2017 estimate in figure E.IIa, which implies that workers hired at the \$913 threshold had prior weekly salaries that are \$30

lower than predicted given the salaries of other new hires in the cohort. In row two, I find that the excess initial salary translates to a 5.2% larger pay increase from job transition than otherwise predicted. The real wage increase for new hires at the threshold is robust to an alternative specification in column (2) where I allow for different slopes to the left and right of the threshold, and to a further restriction in column (3) where I only use workers earning above the $913 threshold to predict bunched hires' expected past salaries. The restricted sample alleviates concerns that the FLSA rule change affected the selection of jobs paying below the new overtime exemption threshold. In all three specifications, I find consistent evidence that workers hired at $913-953 per week had lower past wages than predicted, leading me to conclude that firms are not selecting more productive workers in response to the elevated wage.

In columns (4) to (6), I repeat my analysis using new hire's future wages as the outcome. Given that the regression discontinuity systematically understates workers' future wages, even in the months before the rule change, I use a difference-in-discontinuity design to adjust my estimates by the magnitude of the discontinuity in the quarter prior to the announcement of the new threshold. Consistent with the graphical evidence, I find no effect on the future wages of new hires after employment.

Overall, the regression discontinuity analysis rejects the claims that 1) employers are positively selecting more productive hires after raising entry wages, and 2) employers are reducing wage growth of workers who receive a premium on their initial pay. While the findings in this section reinforces the argument in the main text, I am cautious not to over-rely on this analysis. The discontinuity approach assumes that absent the rule change, the distribution of pay increases from switching jobs would be continuous with respect to workers' new salaries. While that appears to be true prior to May 2016, it is unlikely to hold after the rule change since, by construction, the overtime policy affected jobs to the left of the $913 per week threshold. I have tried to account for that critique by showing that my results are robust to using only workers on the right tail of the distribution as a control. Nevertheless,

the type of analysis in this section does not have the same econometric properties as a traditional regression discontinuity design where the assumption of continuity is more plausible. Despite that limitation, it is reassuring that both the difference-in-difference and regression discontinuity analyses reach the same conclusions.

Appendix Figure E.I: New Hires' Salary at Previous Job Conditional on Current Base Pay, Pre and Post May 2016
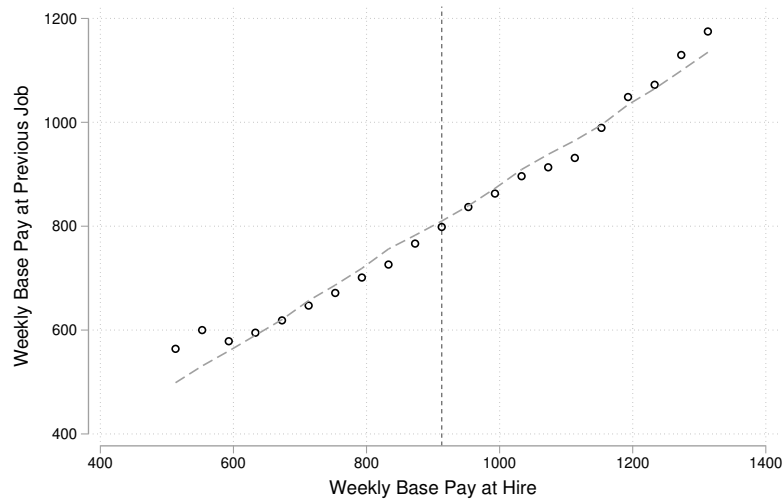
(a) May 2015-2016
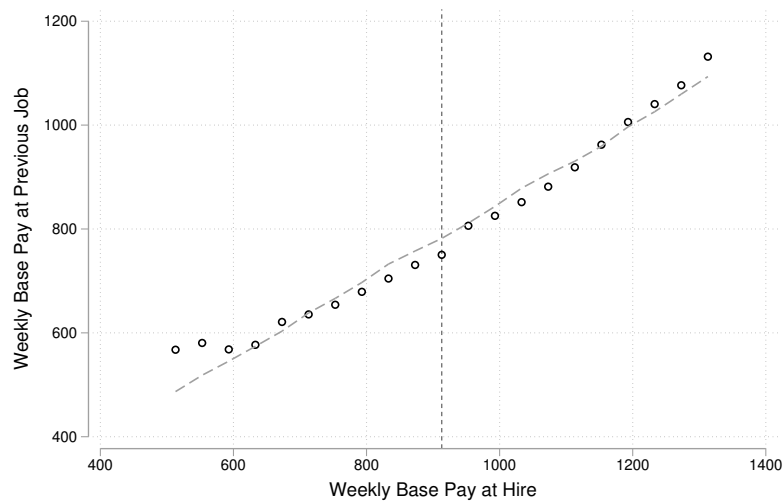
(b) May 2016-2018

Notes: This figure plots the average base pay of new hires at their last observed employer as a function of their current pay, averaged to $40 bins. Panel (a) plots the relationship between past and current pay for workers hired between May 2015-2016, and panel (b) repeats the same analysis for workers hired May 2016-2018. The fitted line is the predicted values from a linear regression. The vertical line is at $913 per week.

Appendix Figure E.II: Discontinuity in Outcomes as a Function of Current Base Pay among New Employees Hired at $913-953/week
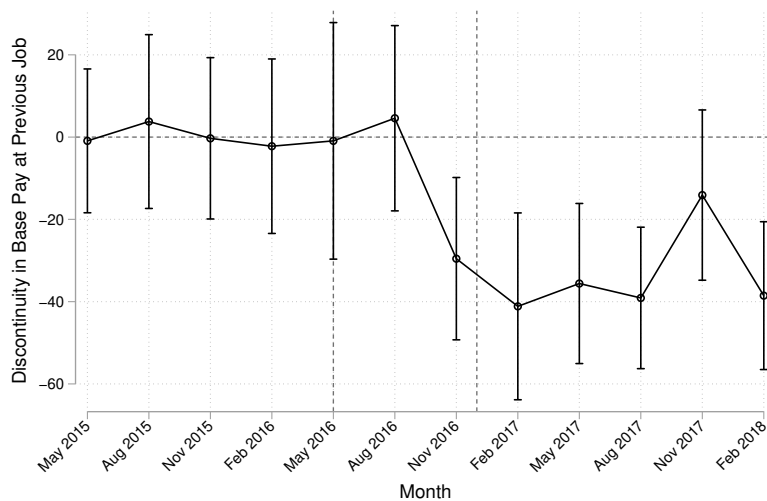
(a) Base Pay at Last Employer
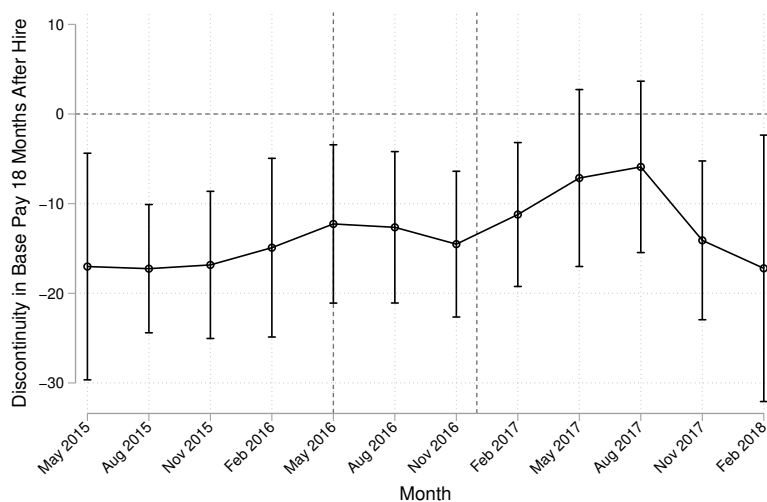
(b) Base Pay After 18 Months

Notes: This figure plots the discontinuity in last observe base pay (panel a) and discontinuity in wages 18 month after hire (panel b) at the $913 overtime exemption threshold, estimated from equation 8. Each discontinuity is estimated using 3 months of data, starting with May 2015.

Appendix Table E.I: Discontinuity in Outcomes Among Workers Hired Between $913-953 per Week

	(1)	(2)	(3)	(4)	(5)	(6)
Base Pay in Previous Job	−29.551***	−39.794***	−21.488			
	(10.065)	(11.232)	(13.115)			
% Δ Base Pay from Prev. Job	.052***	.076***	.047			
	(.021)	(.021)	(.031)			
Base Pay After 18 Months				.394	-2.485	-9.134
				(6.156)	(8.897)	(14.341)
% Δ Base Pay After 18 Months				.002	0	-.01
				(.007)	(.01)	(.015)
N	25,105	25,105	15,233	95,336	95,336	55,731
Sample	One Slope	Two Slope	Right Tail	One Slope	Two Slope	Right Tail

Notes: This table reports regression discontinuity estimates that calculates the deviation in outcome between new hires earning $913-953 per week and the level predicted by a linear regression from other hires with similar base pays. All estimates are reported for workers hired between November 2016-January 2017. The estimates in columns (1)-(3) are computed from equation 8. The estimates in columns (3)-(6) are computed from a difference-in-discontinuity version of equation 8 that differences the discontinuity in Nov 2016 - Jan 2017 by the discontinuity in Feb-Apr 2016. The prediction in columns (1) and (4) uses one linear slope for jobs paying between $600 and $1200 per week. Columns (2) and (5) uses a different slope for each side of the discontinuity. Columns (3) and (6) only uses workers paid $913-1200 per week for the prediction. *10%,** 5%,*** 1% significance level.

Appendix F. Robustness of Decrease in Separations

The analysis in section 6 of the main text found that raising workers' salaries decreased separation rates. This section tests the robustness of that result to relaxing two SUTVA assumptions. First, the treatment group in my difference-in-difference comprises of a mix of workers who received raises and those who did not. I assumed that quit rates did not rise among the latter group after they were left out of the pay increases. If this assumption fails, then my estimates would be biased towards zero. This would actually strengthen my finding, and suggests that the increase in base pay not only reduced the turnover of bunched workers, but reduced it by so much that it more than made up for any rise in quits among treated workers who did not get a raise.

Second, I also assume that workers in the control group (i.e. those earning above $913 per week) did not increase their separations in response to a reduction in relative pay. If this assumption fails, then my estimates are biased upwards and what appears to be a reduction in separations among bunched workers may simply reflect an increase in quits among the control group. In support of my assumption, evidence from previous studies find that job satisfaction (Card et al., 2012) and separation rates (Dube, Giuliano and Leonard, 2019) respond to comparisons with higher-paying peers, but not lower paid ones. As such, those earning above the threshold are unlikely to quit due to reductions in relative pay from below.

Nevertheless, to empirically mitigate concerns that the control group is contaminated, I estimate a cross-year difference-in-difference where I compare workers earning $700-913 per week in April 2016 to similarly defined workers from April 2014. This alternative specification is immune to SUTVA violations but can be biased by year-specific shocks so should be interpreted with caution. I plot the estimates in Figure F.I and Table F.I summarizes the estimates. While there is a statistically significant fall in separation rates, it is half the magnitude of the estimates from the main text and no longer robust to comparing workers within the same firm. Across all specifications though, I find no evidence of a rise in separations.

Appendix Figure F.I: Difference-in-Difference of Separation Rates Between Salaried Workers in 2016 and 2014
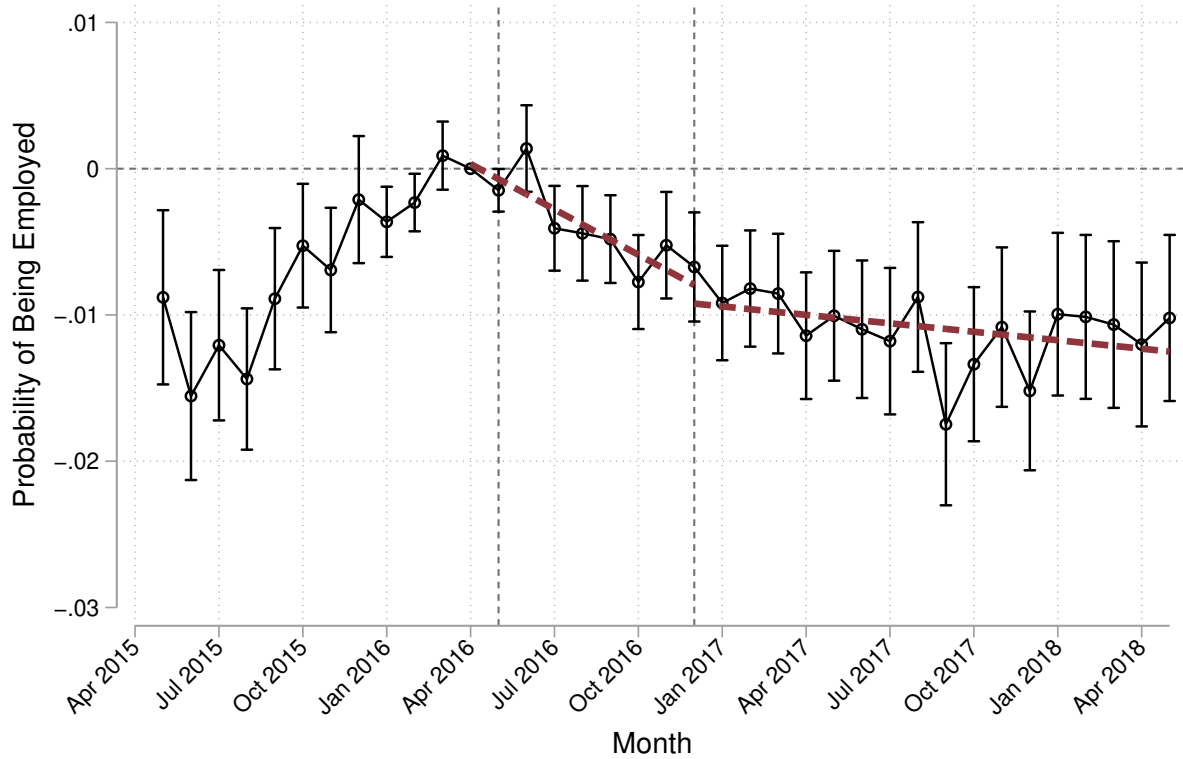
Notes: This figure plots difference-in-difference estimates where the outcome is an indicator for being employed at the same firm since April 2016. The treatment group comprises of salaried workers earning $700-913 in April 2016. The control group are similarly defined workers in April 2014. The left vertical dashed line is at May 2016, and the right vertical dashed line is at December 2016. The dashed red lines are the slopes of the estimates from April to Nov 2016, and Dec 2016 to May 2018.

Appendix Table F.I: Cross-Year Diff-in-Diff Estimates of the Effect on the Probability that Incumbents are Employed

	(1)	(2)
Pre-trend	.0013***	0
	(.0003)	(.0002)
Anticipation	−.001***	−.0011***
	(.0003)	(.0003)
Post-trend	-.0001	−.0008***
	(.0001)	(.0001)
Change in Slope	.0008***	.0003
	(.0003)	(.0003)
Baseline Monthly Separations	.0215	.0215
Worker FE	Y	Y
Month FE	Y	-
Firm-Month FE	-	Y
N	34,013,804	33,794,800

Notes: This table reports difference-in-difference estimates that compare the employment rate of incumbent workers earning $700-913 per week in April 2016 to similarly defined workers in April 2014 (see equation 3). Estimates are reported for three time periods: May 2015 - April 2016 (Row 1); May 2016 - December 2016 (Row 2); and Jan 2016 - April 2017 (Row 3). The fourth row reports the difference between rows 2 and 3, computed using the delta method. Baseline separation rate is the average monthly separation rate between May to November 2016. All robust standard errors in parentheses are clustered by firm. *10%,** 5%,*** 1% significance level.

Appendix G.   Model of Morale Concerns

In this section, I provide a model of the labor market to illustrate how wages and employment would evolve following the injunction of the FLSA rule change under different assumptions. In particular, I generate testable predictions to distinguish a simple neoclassical model of the labor market from one with entitlement effects, monopsony power, or relative pay concerns.

Consider a monopsony framework with a single firm facing an upward sloping labor supply curve.[12] The firm's revenue $P \cdot Q(l)$ depends on their units of labor l. Their costs are simply wages times the number of workers $w(l)l$, where $w(l)$ is the firm's inverse labor supply curve. The firm solves the following profit maximization problem:

$$\max_l \pi = P \cdot Q(l) - w(l)l$$

The solution to the problem gives the standard wage markdown equation

$$\frac{P \cdot Q'(l) - w}{w} = \frac{1}{\epsilon}$$

where ϵ is the labor supply elasticity facing the firm.

I model labor supply by a distribution of reservation wages across workers in the labor market.[13] Suppose reservation wages r have a cumulative distribution $F(r)$. For simplicity, I assume that workers' utility is simply equal to the wage and individuals choose to work if their utility exceeds the reservation wage (i.e $u(w) > r$). The labor supply facing the firm is then given by $l(w) = F(w)$ and is simply the number of people with reservation wages below w. The labor supply function and the firm's markdown equation together characterize an equilibrium wage and employment level.

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[12]The predictions are the same in a Cournot model where each firm faces an upward sloping labor supply curve. For simplicity, I present the model with one firm.

[13]A market in this setting can be thought of as a particular occupation rather than all jobs in the economy. Implicitly, I assume each occupation has its own equilibrium market wage.

Suppose the equilibrium wage is initially below the \$913 overtime exemption threshold and consider comparative statics whereby the employer promises workers a pay increase to the new threshold.[14] In a benchmark scenario, the promise is non-binding so the equilibrium does not change.

Lemma 1 (Benchmark). *If the promise of a pay increase had no impact on workers' expectations, utility, or preferences, then wages would remain constant.*

However, suppose workers feel "entitled" to the promise. Motivated by the findings of Falk, Fehr and Zehnder (2006), I introduce entitlement effects as an increase in workers' reservation wages such that their new reservation wage r_{new} is at least the promised \$913 per week salary threshold: $r_{new} = \max(r_{old}, 913)$. In this case, if the firm maintains the previous equilibrium wage, it would hire no workers and make no profits. As a result, an increase in reservation wage leads to the following predictions:

Proposition 1 (Entitlement Effect). *Suppose the promise of a pay increase raised workers' reservation wages to at least the \$913 per week threshold.*

1. *Weekly base pay will increase to \$913 per week.*

2. *If markets are perfectly competitive (i.e. $\epsilon = \infty$), then employment falls.*

3. *If firms possess enough market power (i.e. ϵ is sufficiently small), then employment can remain constant or even increase.*

The first prediction follows from workers' new reservation wages. If workers are employed, they must be paid at least \$913 per week. While I am using the model to represent a single occupation, more generally, if there are multiple occupations with different wages and they each raise weekly salaries to \$913 per week, then I would expect to observe bunching in the wage distribution. The second prediction follows from the wage markdown equation.

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[14]The model takes firms' promise as a given. For a more detailed discussion of *why* a firm would choose to raise salaries as opposed to simply pay overtime, refer to section 4.2 and Quach (2024).

In a perfectly competitive market, firms are already paying workers their marginal revenue product, so an increase in wages must reduce the number of jobs. On the other hand, if firms were marking down wages to begin with, then employment could actually increase with the wage, analogous to imposing a minimum wage in a standard monopsony model.

G.1 Dynamic Wage Adjustments

Thus far, the model examines the impact of an increase in reservation wages within a static framework. I next introduce wage and employment dynamics into the firm's decision, assuming that they raised incumbents' salaries to $913 in the short-run. Moreover, I test how the predictions change once I also add horizontal pay equity concerns.

First, suppose wages in the economy naturally increase over time, which I model by indexing prices in the firm's profit function by the month t. As P_t increases over time, the firm's wage markdown equation implies that the equilibrium wage would also increase. However, if salaries are constrained at $913 per week, above the firm's profit maximizing level, then nominal wages would not change. This implies that workers who initially receive a pay increase to the $913 threshold would experience no wage growth over time until their real wages once again satisfy the firm's markdown equation.

Second, suppose firms can replace existing workers with new hires following the dynamic monopsony framework of Boal and Ransom (1997):

$$l_{t+1} = [1 - s(w^i_{t+1})]l_t + r(w^h_{t+1})$$

where employment in period $t+1$ depends on the retention of workers from period t and the recruitment of new workers. I assume that separation rates $s(w^i_{t+1})$ decrease with incumbents' wages and recruitment $r(w^h_{t+1})$ increases with the wage of new hires.[15] The firm's profit

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[15]The ability to offer new hires' different wages from stayers is analogous to the distinction that Pissarides (2009) makes between the rigidity of incumbents' and new hires' wages.

maximization problem at time t is given by

$$\max_{s,r} \pi_{t+1} = P_{t+1} \cdot Q\Big([1 - s_{t+1}]l_t + r_{t+1})\Big) - w^i_{t+1}(s_{t+1})[1 - s]l_t - w^h_{t+1}(r_{t+1})r_{t+1}$$

If firms are able to price discriminate between incumbents and new hires, then the firm's first order condition with respect to recruitment implies that the wages of new hires does not depend on incumbents' wages. It only needs to satisfy the wage markdown formula. Thus, new hires would not receive $913 per week, but rather, the optimal salary in the absence of the reform:

Proposition 2 (Dynamic Wage Response with No Relative Pay Concerns). *Assume the overtime policy increased salaries to $913 per week. Suppose workers' utility only depend on their own wage and not that of their peers.*

1. *If the reservation wages of incumbents do not change over time, then incumbents' weekly base pay will stagnate at $913 per week leading to a decrease in real wages.*

2. *If the reservation wages of new hires is not impacted by the policy and firms can price discriminate, then employers would replace incumbents with new hires at a lower wage.*

The above predictions depend crucially on the stated assumptions. For example, if incumbents' reservation wages increase with inflation, then employers would also have to raise incumbents' wages. Similarly, if new hires' reservation wages are the same as incumbents', then employers would not be able to pay new hires a lower wage. One condition in which both predictions of proposition 2 fail is if workers care about how their wage compares to their peers. Consider the following utility function $u(w) = w + \lambda(w - w_{peer})$, where λ captures workers' relative pay concern.[16] In that case, a reservation wage of $913 actually implies a reservation utility of $r = 913 + \lambda(913 - w_{peer})$. Notice that individuals' decision to work now depends on the wages of their peers. As a result, employers cannot give a pay increase

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[16]To connect with the data, I assume employers hire a peer group of workers who earn above the $913 overtime exemption threshold and are unaffected by the policy.

to only workers unaffected by the FLSA rule change but leave affected incumbents' wages stagnant. Likewise, if new hires care about how they are paid relative to incumbents doing the same job, then employers cannot offer them significantly lower wages.

In summary, the model hypothesizes that temporary policies can generate persistent pay increases due to entitlement effects that permanently raise workers' reservation wages. In response to this rigidity, employers can try to reduce future wage growth or the wages of new hires. However, other frictions might prevent firms from making these adjustments, such as concerns over horizontal pay equity. It is important to note that while my empirical analysis shows evidence of wage persistence, the rejection of the benchmark model does not necessarily imply that my alternative hypotheses are correct. In section 7, I discuss alternative mechanisms that also explain my results and provide survey evidence to suggest that morale concerns nevertheless play an important role in firms' response to the reform.