

PERSISTENT WAGE EFFECTS OF A TEMPORARY OVERTIME POLICY: THE ROLE OF FAIRNESS CONCERNS*

Simon Quach*

February 27, 2024

Abstract

This paper studies the unexpected retraction of a federal policy that would have granted overtime coverage to all salaried employees earning less than \$913 per week. Although the policy was blocked a week before it was supposed to go into effect, I show that it nevertheless had a persistent positive effect on workers' earnings. Leveraging administrative payroll data from ADP, I find that employers raised workers' salaries to the \$913 threshold even after the policy was repealed. In the long-run, employers do not slow the wage growth of workers who benefited from the policy, nor do they hire new workers at a lower pay rate. As a result, real wages remain permanently elevated relative to what they would have been absent the policy. The persistent wage effect contradicts models of downward nominal wage rigidity in which employers dynamically adjust to sticky wages, and is suggestive of a fundamental shift in fairness norms. Consistent with the existence of fairness concerns, I find that workers within the same firm tend to all receive the same pay increase in the year after the policy repeal. Despite the increased labor costs, I find a decline in separation rates among workers bunched at the \$913 threshold, which suggests that employers hold some monopsony power. Taken together, my findings show that even temporary policies can have permanent impacts on the labor market due to the presence of fairness norms.

JEL codes: E24, J23, J31

*I am extremely grateful to Alexandre Mas for his tremendous guidance and support on this project. I am thankful for the helpful comments and suggestions from David S. Lee, Henry Farber, Lawrence Katz, John Grisby, Jonathon Hazell, and the participants at the Industrial Relations Section labor lunch. I am indebted to Alan Krueger, Ahu Yildirmaz, and Sinem Buber Singh for facilitating access to ADP's payroll data, which I use in my analysis. The author is solely responsible for all errors and views expressed herein. This paper was previously a chapter in my PhD dissertation under the title "The Extent of Downward Nominal Wage Rigidity: Evidence from a Natural Experiment".

*University of Southern California, USC-Department of Economics 3620 S. Vermont Ave., KAP 364D Los Angeles, CA 90089-0253, simonqua@usc.edu

“If economic policies generate entitlement effects that respond asymmetrically to the introduction and the removal of a policy, much of what is taught in economics textbooks needs to be rewritten because the introduction of a policy may have effects that prevail even after it has been abolished” (Fehr *et al.*, 2009)

1 Introduction

Labor market policies frequently change due to shifts in political leadership.¹ Conventional models of the labor market commonly assume that the introduction and removal of such policies have symmetric effects. The reasoning is that policies impact economic behavior by altering incentives and constraints, elements that cease to exist once a policy is no longer in effect. However, such reasoning may not be true in practice. If the introduction of a policy also changes fairness norms, then its impacts may persist even after the policy is retracted. For instance, laboratory experiments have found that the temporary introduction of a minimum wage can permanently raise individuals’ reservation wages, even after the removal of the wage floor (Falk *et al.*, 2006; Owens and Kagel, 2010). Outside the lab, it is still an open question as to whether rescinded policies exhibit similar wage persistence in real-world settings, and how the labor market adapts in the long run.

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 testing for persistent labor market effects outside a laboratory setting has been difficult for at least two reasons. First, researchers are faced with a lack of variation as it is often rare and politically unfavorable to remove a policy that raised workers’ earnings. Second, even in instances when such policy variation exists, it is often a challenge to obtain data that can accurately measure wages for a large enough sample to evaluate the reform. For example, although some state preemption laws have reversed local minimum wage increases (National Employment Law Project, 2019), traditional survey data do not track enough low-income workers in these cities to be able to reach conclusive results.

To overcome prior empirical challenges, my paper leverages a natural experiment from the unexpected termination of a major overtime rule that was intended to raise workers’ salaries. In May 2016, the federal Department of Labor announced that starting December 2016, all salaried workers earning less than \$913 per week would be entitled to overtime

¹For instance, state preemption laws have reversed many local minimum wage increases (von Wilpert, 2017), the rules differentiating employees from independent contractors have changed with each administration since 2014 (Scheiber, 2023), and states have both passed and repealed right-to-work laws (Gardner, 2024).

compensation if they work above 40 hours in a week. However, one week before the \$913 “overtime exemption threshold” was supposed to go into effect, a federal court ordered an injunction on the new rule. In a separate paper on the labor market effects of expanding overtime coverage, I show that similar state policies cause firms to reduce employment, reclassify some workers from salaried to hourly, and raise other workers’ salaries to right above the cutoff needed to keep them exempt from overtime (Quach, 2024). The key distinction of this paper relative to Quach (2024) is that this study examines a policy that was never binding. Given that firms face no binding policy constraints, competitive models of the labor market predict that none of the outcomes I observed in my other paper would occur in this setting. Rather than evaluating the impacts of overtime coverage, the goal of this paper is to use the response to the temporary policy to gain new insights into how wages adjust in the presence of fairness concerns, and its implications for models of downward nominal wage rigidity.

Analyzing anonymous monthly administrative 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 workers’ salaries five months before the policy was supposed to go into effect, I find that the largest increase in the share of workers earning precisely \$913 a week occurred in the month after the policy was already nullified. The median “bunched worker” received a sizeable 7% increase in their weekly income. One simple explanation for why firms behaved as if the policy was still active is that given the short notice of the policy’s injunction, employers may have been temporarily unaware of the recent court decision. Alternatively, even if firms are aware, they may simply need time to make pay adjustments.² Contrary to those explanations, I find no evidence that employers are slowly adjusting wages back to their initial levels. The bunching in salaries persisted for over a year and a half after the injunction of the new overtime exemption threshold, and nearly no bunched workers received a pay cut during that period. Instead of a lack of information or delayed adjustments, media accounts at the time reported that many employers promised workers raises in anticipation of the new rule and did not renege on their promises after learning of the court ruling.

Second, I present evidence that employers do not slow down the wage growth of bunched workers in the year following their pay raise. Even though salaries do not revert back to their pre-policy levels, a dynamically optimizing firm could in principle reduce future wage growth

²For example, prior work has found that workers’ base pays tend to change exactly once per year (Grigsby *et al.*, 2019).

to recoup the cost of the initial pay increase. To test whether firms reduce the wage growth of workers who got bunched at the \$913 threshold, I use the wage trajectory of workers with salaries above that cutoff as a counterfactual. Applying a difference-in-difference design, I show that the wages of both groups were trending similarly prior to the announcement of the \$913 overtime exemption threshold. However, only bunched workers received a sharp increase in salaries on December 2016. After the pay raise, both groups again exhibit similar wage trajectories in the following year. The rigidity in wage growth implies that not only are nominal wages rigid, but so are real wages. To explain the persistence in wage growth, I find that employers tend to give all workers in their firm the same pay raise, regardless of whether or not the worker previously benefited from the FLSA rule change. Such behavior is consistent with models of fairness whereby workers' effort depends on not only their own wage, but also on how their wage compares to their peers (Fehr *et al.*, 2009).

Third, I show that firms likewise bunched the salaries of new hires at the \$913 threshold even after the overtime policy was nullified. The persistent bunching of entry wages is perhaps surprising given that workers hired after December 2016 were not yet employed with the firm during the time that it promised pay raises to incumbents. To determine whether the bunching of new hires' wages reflects a real pay increase or simply a change in the composition of new hires, I examine how the characteristics of workers hired at the \$913 threshold change after December 2016 relative to workers hired above the threshold. Using a difference-in-difference design with repeated cohorts, I show 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 estimates suggest that the bunching largely reflects a real wage increase instead of simply positive selection of new hires. The observation that firms apply the \$913 threshold to new hires is consistent with recent evidence that employers do not always personalize wages for each worker, but instead rely on salary benchmarking software (Cullen *et al.*, 2022) or national wage setting policies (Hazell *et al.*, 2022) to maintain internal pay scales.

Fourth, I show that monthly separation rates among workers who received a raise to the \$913 threshold decreased by 0.18 p.p. ($\sim 8.4\%$ of baseline) relative to workers unaffected by the policy. The reduction in separation rates is consistent with growing evidence of monopsony power in the labor market (Card, 2022). In particular, using variation from a firm-specific minimum wage, Emanuel and Harrington (2022) show that a 1% increase in firm's wage reduces turnover by nine percent. My estimate of the separation and wage effects imply a similar separations elasticity of 8% for each 1% increase in workers' salaries. Despite the decrease in separations, I find no significant impact on firms' net employment levels.

My paper contributes to three areas of research. Foremost, my paper adds to the growing literature on fairness norms in the labor market. Previous studies in this literature have shown that whether workers perceive they are paid a fair wage affects their job satisfaction (Card *et al.*, 2012), productivity (Mas, 2006; Breza *et al.*, 2018; Cullen and Perez-Truglia, 2022), and quit behavior (Dube *et al.*, 2019). Most related to my study is the paper by Falk *et al.* (2006), which showed in a laboratory experiment that a temporary minimum wage increase can change fairness perceptions and thereby have persistent effects on wages and employment. My study validates the results of Falk *et al.* (2006) in a real world setting and shows that transient policies can permanently raise wages even in a high-stakes environment with real economic agents. Moreover, by leveraging a natural experiment, I am able to examine responses over a much longer time horizon than would be feasible in a laboratory setting. Given the extended time frame, I show that fairness norms can persist for well over a year and, during that period, employers neither mitigate its impacts by reducing wage growth nor by decreasing new hires' wages.

Second, my paper contributes to the literature on labor market hysteresis. Previous research have found that even after a shock or policy has long passed, its impact on the labor market may still persist (Blanchard and Summers, 1986; Miller, 2017; Saez *et al.*, 2021). While these studies focused primarily on employment hysteresis, I show that wages likewise exhibit similar persistence in response to temporary policies. Moreover, since previous studies examined the retraction of policies that were already in effect for many years, the persistent effects of these policies could reflect changes in firms' investments rather than changes in social norms. For instance, prior work have attributed the persistence of labor market regulations to investments in screening capital (Miller, 2017) and changes in discriminatory beliefs (Saez *et al.*, 2021). By examining a policy that was never binding, I am able to rule out such long-term adjustments. Instead, my unique empirical setting provides stronger evidence for the role of fairness norms in explaining hysteresis in the labor market.³

Third, my study also relates to the literature on downward nominal wage rigidity. While prior research has consistently shown that firms seldom give workers wage cuts,⁴ there is less consensus as to the causes and impacts of wage rigidity. In regards to the causes of wage persistence, my paper shows that staggered bargaining (Taylor, 1979; Gertler and

³Outside the labor market, behavioral mechanisms have been used to explain why temporary interventions have persistent impacts on smoking (Giné *et al.*, 2010), exercise (Charness and Gneezy, 2009), energy consumption (Allcott and Rogers, 2014), and commuting routes (Larcom *et al.*, 2017).

⁴Examples include Card and Hyslop (1996); Kahn (1997); Altonji and Devereux (1999); Barattieri *et al.* (2014); Elsby and Solon (2019); Jardim *et al.* (2019); Kaur (2019); Grigsby *et al.* (2021).

Trigari, 2009) and implicit contracts (Beaudry and DiNardo, 1991) alone are insufficient to generate the observed wage dynamics. Instead, the results provide empirical support for morale concerns as a key mechanism, consistent with evidence from surveys of employers (Campbell and Kamlani, 1997) and workers (Kaur, 2019; Davis and Krolikowski, 2023). As for the impacts of wage stickiness, previous studies have argued that employers could either cut new hires’ wages (Pissarides, 2009) or slow down wage growth (Elsby, 2009) in response to downward nominal wage rigidity. I find evidence against both hypotheses. In addition, I provide new findings on the employment effects of wage rigidity. While previous wage rigidity papers have found negative employment effects of wage rigidity (Fehr and Goette, 2005; Kaur, 2019; Kurmann and McEntarfer, 2019), my results provide suggestive evidence that this may not be true if employers have market power to set wages.

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. I conclude in section 7 with a discussion of possible mechanisms and the role of fairness norms in explaining my results.

2 The 2016 FLSA Overtime Regulation

Under the Fair Labor Standard Act (FLSA), employers in the U.S. are required to record workers’ hours and pay them an overtime premium of at least one and a half 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 avoid the costs of monitoring their hours.⁵ 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 of salaried workers in 2016.

On May 18, 2016, the 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

⁵Comments by businesses in response to the 2016 FLSA reform studied in this paper suggests that tracking workers’ hours is a significant cost of the policy (U.S. Department of Labor, 2016).

to low-income white-collar salaried workers such as managers of fast food restaurants and retail establishments. In response to the upcoming regulation, twenty-one states sued the federal Department of Labor on September 26, 2016, arguing that such a large increase in the threshold overstepped the authority of the DOL and requires congressional approval.⁶ From a review of newspaper articles at the time, I found that little media attention was given to the development of the court case and those that did warned employers to not expect a ruling before the December 1st deadline.⁷

Hence, it was a surprise to employers when the court ordered a preliminary injunction ten days before the effective date of the rule change, thereby preserving the overtime exemption threshold at \$455 per week. By statute, granting an injunction meant the judge believed that the plaintiff was likely to succeed and would suffer irreparable loss without a temporary preservation of the status quo. Despite initial uncertainty about the future of the overtime exemption threshold, it soon became clear that it was highly unlikely for the \$913 proposal to ever go into effect. Following the 2016 election, the incoming administration nominated fast-food executive, and critic of the new overtime regulation, Andrew Puzder to be Labor Secretary on December 8, 2016. While Puzder did not receive sufficient support from the Senate to be instated, the next nominee, Alexander Acosta, commented in his confirmation hearing on March 22, 2017 that he believed the overtime exemption threshold should be updated to only around \$634 per week. Ultimately, Acosta was confirmed as Labor Secretary and the DOL officially dropped its defense of the Obama-proposed rule change 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.

Since the policy is never binding, neoclassical models of the labor market would predict zero impact of the policy. Anecdotally though, some companies reported that they promised their workers pay raises in anticipation of the new policy and would not rescind on those promises after the injunction (Wall Street Journal). Such behavior is suggestive that employers are constrained by fairness norms that prevent them from cutting wages. Using comprehensive data of the U.S. labor market, I show that the anecdotal evidence reflect a broader shift in the labor market whereby firms raised workers salaries and bunched them

⁶Specifically, when establishing the FLSA during the Great Depression, Congress allowed exemptions for “executive, administrative, and professional” employees. Instead of strictly defining those classes of workers, they gave the DOL authority to write and adjust definitions over time. The plaintiffs in the case argued that while the DOL is permitted to set a salary threshold, it should not be so high that “executive, administrative, and professional” employees are solely determined by their income rather than their duties.

⁷For example, see *Texas Judge Consolidates Challenges to Overtime Rule* (SHRM Oct. 21, 2016)

at \$913 per week. Furthermore, since the rule change only targeted a specific segment of the salary distribution, I am able to use jobs that were 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 data lets me observe individuals' earnings at a monthly frequency between May 2008 and January 2020 for over a tenth of the U.S. labor force. Previous analyses of the ADP data have found that it closely matches the age, sex, and tenure distribution of workers in the Current Population Survey.⁸

Within the data, I observe monthly aggregates of anonymized individual paycheck information including workers' salaried/hourly status, earnings, hours, pay frequency, industry, and state of employment. In addition, the data records each worker's standard rate of pay as of the last paycheck in the month. For hourly workers, their standard rate of pay is simply their wage and 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 employee's overtime exemption status. Following the Department of Labor's guidelines, I define salaried workers' weekly base pay as their salary per paycheck divided by the number of weeks between each paycheck.⁹

I create three sub-samples from the data. First, to study the effect of the reform on the wages of stayers, I construct a sample of all workers who are continuously employed at the same firm between May 2015 and April 2018. Second, to study the evolution of entry wages, I create a sample consisting 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 effect of the policy on employment dynamics. Using the last dataset, I am able to determine the effect of the policy on incumbents' separation rates and test how employment at firms changed depending on the share of their workers impacted by the new overtime exemption threshold. In all data

⁸However, the data under-represents very large firms with over 5000 employees. For a detailed discussion of the representativeness of the ADP data, refer to Grigsby *et al.* (2021).

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

samples, I restrict the samples to continuously operating firms since the entry and exit of firms reflect both business creation/destruction and changes in the composition of ADP’s clientele.

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.

4.1 Immediate Effect of the Policy

To begin, I present graphical evidence of the immediate response to the reform. In figure 1a, I overlay the distribution of weekly base pays in April and December 2016. The key highlight from the figure is the stark shift in salaries from between the old and new overtime exemption thresholds to preciously right above the \$913 cutoff. Besides the bunching at the overtime exemption threshold, there appears to be little change to the rest of the income distribution.¹⁰ To visualize the bunching more clearly, figure 1b plots the change in the distribution between April and December 2016, and compares it to the change in 2014. While jobs in 2014 likewise experience a shift towards the right of the income distribution due to natural wage growth, it does not exhibit the large bunching behavior observed during the year of the overtime policy 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. Given that employers had 7 months to prepare for the rule change and the injunction only occurred 10 days before the policy was supposed to go into effect, many firms already announced pay raises to their employees (Wall Street Journal). If the cost of bunching workers is small, then firms may have raised employees’ salaries simply to avoid the administrative cost of changing their plans and communicating the new strategy with their workers. My analysis suggests that this is unlikely the story.

To estimate the cost of raises workers’ salaries to the overtime exemption threshold, I follow a similar identification strategy to Cengiz *et al.* (2019) by comparing the observed density of weekly base pays over time to a counterfactual distribution. However, since the

¹⁰Given that annual salaries tend to cluster at \$5,000 intervals, the density of weekly base pays exhibit periodic spikes along the distribution when plotted using \$40 bins.

¹¹I choose 2014 as the comparison year because in my subsequent analysis, I use the evolution of base pays from May 2013-2016 as a counterfactual for the change in the distribution of base pays between May 2015-2018 absent the policy.

2016 FLSA policy was a federal rule change that affected all states simultaneously, I am unable to implement a cross-state comparison like those commonly used in studies of the minimum wage. Instead, motivated by figure 1b, I estimate the cost raising workers' salaries to the \$913 threshold by using the distribution in the years prior to the policy change as a counterfactual.

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

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

where \bar{n}_{tk} is the share of salaried workers in the bin of base pay 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 evolution of the base pay distribution since April 2016 to the evolution of the distribution over the same number of months since April 2014. A limitation of the identification strategy is that it only accounts for seasonal trends that is common across years, but not year-specific shocks. While the method can be easily adapted 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 1c plots the distribution of $\hat{\beta}_{kt}$ by base pay k for $t = \text{December 2016}$. The figure is equivalent to the raw difference between the two lines in figure 1b using \$20 bins of base pay. Four results are worth noting from the graph. First, the share of salaried workers earning right above \$913 per week increased by approximately about 0.9 percentage points from April to December 2016. In comparison, figure 1a finds that less than 3% of all salaried workers earned \$913 in April 2016. As a result, about 1 in 4 workers earning \$913 per week in December 2016 had been impacted by the FLSA overtime proposal. Second, the missing mass mass to the left of \$913 extends as far left as \$693 per week. In other words, employers were willing to raise workers' salaries by as much as 32% (i.e. $\frac{913-693}{693}$) to comply with the new rule. Third, the median worker in the missing mass would have earned \$853 per week absent the policy change. Instead, the median bunched worker experienced a 7% (i.e. $\frac{913-853}{853}$) increase in their base salaries. Fourth, 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 1c suggests that it was fairly costly for employers to raise workers' salaries to the \$913 overtime exemption threshold. As such, it is unlikely that firms raised workers' salaries simply to avoid the one-time administrative costs of adjusting their plans.

4.2 Evidence of Wage Persistence

If raising workers’ salaries to the overtime exemption threshold was so expensive, why did firms not simply tell workers that the new rule was retracted? Laboratory evidence from Falk *et al.* (2006) suggests the announcement of the FLSA policy may have changed workers’ view on what constitutes a “fair wage”, so that employers would raise workers’ salaries to avoid reducing morale (Akerlof and Yellen, 1990). While I am unable to directly measure workers’ sentiments, I will rule out other hypotheses for why firms behaved as if the policy was still binding, and provide evidence consistent with the existence of fairness norms.

I rule out three simple explanations for why firms raised workers’ salaries. First, employers could simply have been unaware of the injunction and thought the policy was still binding. Second, even if firms knew of the injunction, they may be concerned that the new rule will ultimately be upheld in a court appeal. Third, even if employers wanted to avoid raising workers’ salaries, they may simply need time to make adjustments. The third point would be an outcome of staggered bargaining models where employers do not instantaneously adjust wages to shocks, but only change wages periodically (Taylor, 1979). All three of these scenarios have a common prediction: in a environment without fairness concerns, employers would gradually reduce workers’ salaries back to their pre-policy levels over time as they learn about the injunction, receive more information about its legal status, and make adjustments to their payroll. In contrast, if the policy changed fairness norms, then salaries would be downward rigid and remain bunched at the \$913 threshold.

To test whether wages were downward rigid, Figure 2 plots the distribution of $\hat{\beta}_{kt}$ across base pay, for each month from January 2016 to December 2017. Four features of the graph are worth noting. First, I observe very little movement in the distribution prior to the announcement of the new overtime exemption threshold in May 2016. That serves as a useful placebo check to show that my empirical strategy is capturing primarily the effects of the policy and not other contemporaneous shocks. Second, a bunching mass starts gradually growing at the \$913 threshold after May 2016, consistent with the view that employers were preparing in anticipation of the rule change, and some firms even made adjustments prior to when the policy was supposed to go into effect. Third, the largest increase in the bunching mass occurred on December 2016, after the court injunction. Fourth, the bunching gradually shrinks in 2017, but is still about half its original size by December 2017.

Given that the bunching persisted for well over a year, it would be surprising if employers did not learn of the injunction during this time. As evidence of firms’ knowledge of the court ruling, appendix figure A.1 plots the Google search popularity for the term “FLSA Overtime”

from Jan 2015 to Dec 2017. I find that the spike in Google searches for “FLSA Overtime” on the week of November 20-26 was even larger than the spike in May when the policy was first announced. If firms could reverse the wage increases immediately after learning about the injunction, it is likely that the bunching mass would have disappeared within a year.

Next, I show that the persistence in bunching is also inconsistent with the hypothesis that firms are worried about the legality surrounding the new threshold and do not want to risk a costly law suit. To test whether employers’ beliefs about the potential risks of non-compliance caused them to avoid reducing workers’ wages, I use the final court decision in June 2017 as a discrete breakpoint in firms’ beliefs. Namely, if wages are flexible and the bunching is simply a reflection of firm’s risk aversion, then it should unravel quicker following the final court decision in June 2017.

In figure 3, I plot the evolution of the bunching mass at \$913 per week over time (estimated from equation 1) and find 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 features. First, the estimated size of the bunching mass is close to 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 interval rose by about 1 p.p by January 2017. For comparison, only about 2.5% of workers earned between \$913 and \$953 per week in May 2016. The policy therefore increased the number of workers within this interval by over 25%. Third, the magnitude of the bunching mass shrank at a constant rate following its retraction. The absence of any discontinuous change in the share of salaried workers earning \$913 per week after key developments surrounding the legality of the rule change suggests that the persistence in the bunching is not due to employers’ expectations and uncertainties about the future of the policy.

The constant rate of decrease in the bunching mass also suggests that the persistence of the spike is not due to staggered bargaining contracts. These type of contracts are often used in macroeconomic models to introduce wage rigidity by assuming that salaries are adjusted periodically, rather than instantaneously (Taylor, 1979, 1980; Gertler and Trigari, 2009). Empirically, recent work by Grigsby *et al.* (2019) finds that firms indeed tend to adjust the wages of all workers at the same month each year. I replicate their findings in appendix B where I show that workers tend to only receive one pay increase per year, and these wage adjustments tend to occur in either January or April. Given that a large share of wage

adjustments occur in January, if the persistence in bunching is due to staggered bargaining, then I would expect to see a sharp drop in the bunching mass on January 2018. While figure 3 exhibits a small kink in January 2018, it is followed by zero change in the bunching mass in the following month. The results imply that while staggered bargaining may exist, it alone does not explain the type of rigidity observed after the retraction of the overtime policy.¹²

While the persistent bunching in the pay distribution exists in aggregate, two questions remain: is the shrinking in the bunching due to wage decreases or increases, and is there heterogeneity in wage rigidity across firms and workers.¹³ To answer these questions, figure 4 plots the distribution of one-year wage changes for job-stayers who earned between \$913-953 per week in December 2016. If wage flexibility is heterogeneous across workers or firms, then at least some workers bunched at the \$913 threshold would experience a large pay decrease in 2017. Similarly, heterogeneity in the timing of wage adjustments due to staggered bargaining would also aggregate to a large contingent of wage cuts over the year. In contrast, figure 4 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 of workers even received a raise. This stark asymmetry in the wage change distribution is similar to the result in Grigsby *et al.* (2021) where they plot the distribution of wage changes for a random sample of job-stayers in the ADP data. The rarity of negative wage changes implies that not only is there little heterogeneity in wage flexibility, but also that the decline in the bunching over time is due to workers receiving raises rather than decreases in their base pay.

To summarize the results thus far, I find that even though the 2016 FLSA rule change was never binding, firms nevertheless raised workers' salaries to the retracted overtime exemption threshold and the bunching persisted for over 1.5 years after the injunction. The strong persistence in bunching is consistent with existing laboratory evidence that temporary labor market policies can have permanent effect on wages by changing fairness norms (Falk *et al.*, 2006). However, while firms do not reduce workers' wages back to their pre-policy levels, they

¹²In appendix B, I also show that staggered bargaining cannot explain the gradual increase in the bunching mass prior to the date it was supposed to go into effect. One hypothesis is that some workers receive an early raise to the overtime exemption threshold simply because they customarily receive a pay raise every 12 months. Counter to that argument, I find no clear correlation between the month that a worker received a pay increase in the year prior to the policy announcement, and the month they are bunched in 2016. Overall, the evidence suggests that while staggered bargaining is a feature of the data, it does not explain the wage dynamics surrounding the temporary change in overtime regulations.

¹³For example, Kurmann and McEntarfer (2019) found that between 2004-2007, about two-thirds of employers exhibited an excess spike at zero in the distribution of annual wage changes, while one-third did not.

may respond in other ways to the increased labor costs. In the remainder of the paper, I investigate three potential margins of response: wage growth, entry wages, and employment. These outcomes coincide with a broader debate concerning the impacts of downward nominal 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, many modern theories of wage rigidity have argue that rigidity in spot wages may have little effect on aggregate employment if employers can reduce long-run wage growth (Elsby, 2009) or replace incumbent workers with new employees at a lower wage (Barro, 1977; Pissarides, 2009). Empirically testing for stickiness in either wage growth or entry wages has been particularly 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 overtime rule in 2016 as a natural experiment to test whether employers cut wage growth, entry wages, or employment when fairness norms prevent them from reducing nominal wages.

4.3 No Dynamic Wage Adjustments

In this subsection, I test the hypothesis that employers respond to downward nominal spot wages by reducing future wage increases. My analysis compares workers who likely received a raised as a result of the 2016 FLSA policy relative to those earning above the new overtime exemption threshold, and were less affected by the rule change. In contrast to the theory that firms adjust wages dynamically, I show that the wages of bunched workers continue to grow at the same rate relative to other counterfactual groups.

My analysis estimates a difference-in-difference regression of the form

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

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 a worker fixed effect α_i , and a month fixed effect α_t .

Figure 5 plots the estimates from my difference-in-difference analysis using two different definitions for the treatment and control groups. To start, I define worker’s treatment status based on their salaries in April 2016, the month before the policy announcement. Given the results in section 4.2, I define the treatment group to be 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 5a plots the average weekly base pay over time for these two groups. It is visually difficult to observe a clear jump in base pay among the treatment group because many of the workers earning \$700-913 per week in April 2016 were unaffected by the policy.¹⁴ To more closely visualize the impact of the reform, I next plot the regression estimates of equation 2.

The difference-in-difference estimates in figure 5b 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, I find 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, I find that firms gradually raised workers' salaries, consistent with the preemptive effects in figure 2. Third, there is a sharp increase in workers' salaries at precisely December 2016, the month that the new threshold was supposed to go into effect. Fourth, there is no slow down in wage growth following the injunction of the policy. Even 16 months after the injunction, workers impacted by the reform earn 1% more than the counterfactual group of workers.

As noted, one limitation of defining the treatment and control groups based on workers' 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 on workers likely affected by the FLSA rule change, I define 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". These non-bunched workers were not directly affected by the policy or its injunction since they are paid above the overtime exemption threshold.¹⁵

Figure 5c plots the raw evolution of bunched and non-bunched workers' salaries over 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

¹⁴Given the distributions in figure 1, about 10% of salaried workers earned between \$700 and \$913 per week in April 2016, but firms only bunched 1% of salaried workers at the threshold. As a result, only about 10% of the "treatment" group actually received a raise to the \$913 threshold.

¹⁵While figure 1c finds a small spillover effect up the pay distribution to around \$1013 per week, the spillover effects are small relative to the bunching mass and do not have any economically significant impact on the subsequent analysis.

a large one-time increase in their base pay on December 2016. In contrast, workers earning between \$953 and \$993 per week were largely unaffected by the nullified FLSA rule change. Furthermore, there does not appear to be any indication of a slow down in wage growth for either group following the injunction. To see this more clearly, figure 5d plots the equivalent difference-in-difference estimates computed from equation 2. Given that I condition the sample on workers likely to be affected by the policy change, I find a larger increase in weekly base pay relative to the analysis in figure 5b. On average, bunched workers experienced a nearly 3% increase in salary between April and December 2016. While the wages of bunched workers grew a little slower than that of non-bunched workers post-injunction, they 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 calculate the slope of the wage growth over three periods: pre-announcement, post-announcement but pre-injunction, and post-injunction using the following regression:

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

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, it equals 2 for months between May and December 2016, and it equals 3 for months after December 2016. The estimates of λ_{1p} therefore represent the difference in average monthly wage increases between treated and control workers.

Table 1 reports the estimates of equation 3. The estimates in column (1) suggest that before the announcement of the policy, 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 period when employers thought the new overtime exemption threshold was coming into effect, the wages of workers in the treated pay interval were growing at a rate of 0.13% per month faster than the control group. Then 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.

Note that the robustness of my results to comparing workers within the same firm suggests that the wage persistence is not driven by improvements in productivity via efficiency wages (Katz, 1986; Coviello *et al.*, 2022). If firms are discovering that the productivity gains from higher wages exceed their costs, then I would expect employers to also raise the wages of other workers at the firm. In that case, the wage difference between treated and control

workers would close over time, not because the wages of bunched workers has stagnated, but rather 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 over time.

My results are also robust to an 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. As evidence that this alternative specification captures a greater share of workers whose salaries got bunched at the new overtime exemption threshold, I find a much larger wage increase from May to December 2016 relative to columns (1) and (2). Despite refining the treatment group, I nevertheless observe no economically significant change in bunched workers' wage growth following the injunction.

Even my largest negative estimates suggest a high degree of wage rigidity. Taking the estimates in column (3) 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%. In contrast, bunched workers' salaries only decreased by 0.3 p.p. (s.e. 0.02 p.p.) per month more than non-bunched workers relative to before the announcement of the policy. At that rate, it would take over 6 years to eliminate the initial 2.3% raise that firm gave bunched workers and even longer to equalize the net present value of wages. Overall, the evidence do not support the hypothesis that firms respond to downward nominal wage rigidity by cutting wage growth.

As another benchmark, note that the annual inflation rate in 2017 was 2.1%. The results in section 4.1 suggest that the policy had a median treatment on the treated (TOT) effect of 7%.¹⁶ As a result, if firms simply kept bunched workers' wages constant over time, it would take 3-4 years to bring salaries back to the amount they would have been absent the reform. Given that my estimates suggest it would take between 6 years to infinite years for wages to converge to their counterfactual equilibrium, the evidence suggest that not only are nominal wages downward rigid, but so are real wages.

While the announcement of the overtime exemption threshold may have set \$913 weekly base pay as a new reference point for workers, what explains why firms could not simply keep bunched workers' nominal wages stagnant after 2016? One theory is that workers may have relative pay concerns so that their effort is negatively impacts by increases in inequality that

¹⁶The estimates in table 1 imply a similar TOT effect. According to figure 1, about 1 in 3.5 workers earning \$913 per week in December were affected by the policy. That implies a TOT of $2.3\% \cdot 3.5 = 8\%$.

are perceived as unfair (Breza *et al.*, 2018). In that case, firms would want to give similar pay increases to all workers. To test that prediction, appendix figure A.2 plots the distribution of annual wage increases for bunched workers, relative to the modal wage increase at each workers' employer. I find that nearly 30% of bunched workers receive the same pay raise as their peers, and they are very unlikely to receive a smaller pay increase than the mode in the firm. The stark asymmetry in the distribution is highly suggestive that workers care about how much their pay increases relative to their coworkers.¹⁷ The evidence suggests that while the retracted overtime exemption threshold raised workers immediate pay expectations, it did not deviate their expectations for future pay raises.

One potential interpretation of my results is that a targeted inflation rate of 2% may not be sufficient to “grease the wheels of the labor market” and eliminate frictions from downward nominal wage rigidity (Card and Hyslop, 1997). While I do find rigidity in real wages, I am cautious to extrapolate from my results to a macroeconomic setting. Whether high inflation leads to more flexibility in real wages depends on the source of downward nominal wage stickiness. If workers are reference dependent with respect to their real wages, then nominal wages would simply grow with inflation and high inflation would not add greater real wage flexibility. On the other hand, if real wages in my setting are rigid because of relative pay concerns, then inflation can still reduce real wages. The reasoning is that firms may not want to give differential pay wages across coworkers, but they may be more willing to slow real wage growth if it applies to all employees. Given these nuances, my natural experiment provides some evidence on the limitations of inflation to increase wage flexibility, but may be more applicable to settings where pay differences is readily apparent.

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 make a distinction 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).¹⁸ Moreover, even if the wages of incumbents are downward rigid, it is unclear that the wages of new hires would be bound by the same fairness norms.

¹⁷To show that the asymmetry is not simply due to firms that have a modal pay increase of 0, I drop such firms and find that the bunching at the modal pay increase is even greater.

¹⁸A notable exception is if firms face liquidity constraints, in which case Schoefer (2021) shows that wage rigidity among incumbent workers can have allocative employment effects.

5.1 Are Entry Wages Rigid?

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 retracted. To examine the bunching of entry workers, figure 6 plots the base pay distribution of new hires for each month between January 2016 and December 2017, relative to the distribution in April 2016.¹⁹ In anticipation of the policy change, firms began bunching new hires' salaries at the \$913 threshold starting in June 2016. The spike of new hires then reaches its peak 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 it diminishes over time, the bunching of entry wages persists until at least December 2017, a year after the injunction of the threshold and six months after the final court decision.

There are two potential explanations for the persistence in the bunching mass of new hires: either firms increased entry wages or they are simply hiring more productive workers. If the bunching of new hires is explained by selection, then workers hired at \$913 per week in December 2016 should be at least as productive as those hired before the announcement of the rule change. On the other hand, if the bunching is not simply due to compositional changes but reflects real wage growth, then I would expect to see firms hiring less productive workers at the new overtime exemption threshold. Since I am unable to measure workers' productivity directly, I instead use the salary of new hires at their previous employer as a proxy for their marginal revenue product.

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 paid between \$953 and \$993 per week. My analysis assumes that absent the rule change, the productivity of workers hired at the cutoff would have evolved similarly relative to workers hired above the cutoff. Formally, I estimate

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

using monthly cross-sections of new hires. My primary outcome, y_{it} , is the wages of the new hire i at their last observed employer. I control for a month fixed effect α_t , and a dummy

¹⁹Unlike the distribution of stayers, I divide the base pay distribution into increments of \$96.15 to avoid the bunching at annual salaries of \$5,000. To reduce noise, I also do not compare the distributions to that of previous years.

α_b for workers hired with \$913-953 weekly base pay. 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 stayer, I aggregate the data into six month intervals for statistical power.

Figure 7a plots the difference-in-difference estimates over time. Reviewing the figure from left to right, I highlight three points. First, the difference in productivity between workers hired at \$913 per week and those hired above the threshold follows the same trend in the months leading up to the announcement of the new FLSA rule change, and even in 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 months when the bunching of new hires was most prevalent. 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 in the latter half 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 can simply reflect natural wage growth causing salaries to increase above the overtime exemption threshold. As such, in the long-run, new hires who would have otherwise earned the overtime exemption threshold may be earning well above it and thereby included in the control group of the difference-in-difference regression. I therefore cannot conclude whether the convergence in productivity in the latter half of 2017 is due to wage growth or if employers are becoming better at screening new hires. Instead, I focus on the Nov 2016 - April 2017 estimate as the clearest indicator of employers' behavioral response to the overtime reform.

To measure the amount by which new hires' wages increased, appendix figure A.3 plots the difference-in-difference estimates using the percent change in workers' salaries from their previous job as the outcome variable. I find that workers hired at the overtime exemption threshold experienced about a 7% larger pay increase when switching from their previous job relative to hires earning above the threshold. In other words, employers are paying entrants 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.

5.2 Is the Wage Growth of New Hires Rigid?

Next, I investigate whether firms slowed the wage growth of new employees who were initially hired at the \$913 threshold. While firms continued hiring workers at the overtime exemption

threshold even after it was terminated, they could have offset their labor costs by raising new employees' wages at a slower rate compared to if the policy was never announced. Whether the wage growth of new hires is rigid is important as firms' hiring decisions depend on not only the initial wage of new hires, but also their expected discounted stream of wages (Kudlyak, 2014).

Using the same difference-in-difference framework as above, I test for rigidity in wage growth by comparing the evolution in base pay between workers hired at \$913-953 per week to those hired at \$953-993 per week. In particular, I estimate equation 4 using the weekly base pay of workers 18 months after their hire date as the outcome variable. To start, I restrict the sample to individuals who stay with the same employer for all 18 months after being hired. To maximize sample size, I allow the sample to differ from the hires for whom I can observe historical wages at past jobs.

Figure 7b plots the regression estimates over time. I find that the difference in wage growth between entrants hired at and above the \$913 threshold was constant from May 2015 to December 2017. In particular, workers hired in the 6 months starting from November 2016 did not suffer any long-run penalty in terms of wage growth, despite receiving a wage premium when they were initially hired.

I summarize the empirical evidence on rigidity of entry wages in table 2, and test the robustness of my results to alternative specifications. Column (1) reports the difference-in-difference estimates corresponding to figures 7 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 \$45 (s.e. \$26) 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, and the wage increase does not simply represent compositional changes. To reduce 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 after I restrict the sample, albeit the estimates are less precise. In comparison, column (3) finds small statistically insignificant effects on the wages of bunched hires 18 months after their employment. Column (4) shows that the magnitude and direction of the estimates are robust to restricting the sample to workers for whom I can observe both their past and future wages.

The above difference-in-difference analysis assumes that the FLSA rule change had no

impact on the selection of workers hired in the \$953-993 weekly pay range, thereby enabling those workers to be a valid control group for bunched hires. While the reform does not directly target workers paid above the overtime exemption threshold, there may nevertheless be spillover effects from the policy elevating wages higher up in the distribution.²⁰ To address concerns that new hires earning right above the overtime exemption threshold are a contaminated control group, appendix C implements a regression discontinuity type design using entrants within a wider income interval to infer the counterfactual productivity of workers hired at the bunching cutoff. Intuitively, even if the policy had spillover effects right above the \$913 overtime exemption threshold, these impacts are likely diminished for jobs paid well below and above that threshold.

In appendix C, I find that prior to May 2016, there is a near linear relationship between new hires' salary at their previous job and their current salary. In contrast, after May 2016, the past salary of bunched hires falls below the level predicted by the past salaries of other workers hired within the \$600-1200 per week pay range. Moreover, from estimating regression discontinuities by month, I show that the decrease in new hires' productivity occurs at precisely the months that the bunching mass is largest, and the magnitude of the decrease is comparable to the difference-in-difference estimates above. 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, even though the overtime exemption threshold was nullified, it nevertheless had persistent effects on new hires' wages that cannot be accounted for by changes in the composition of workers nor by reductions in their future wage growth. While the policy announcement may have caused firms to make pay promises to incumbents, why did it also affect the wages of new hires? One possibility is that the policy could have increased new hires' expectations. For example, Falk *et al.* (2006) finds in a laboratory setting that a temporary minimum wage policy can permanently raise workers' reservation wages. Alternatively, similar to why employers did not slow down the wage growth of incumbents, new hires may exhibit relative pay concerns and reduce their effort if they are paid less than their peers. Relatedly, employers may keep internal pay structures and simply pay all workers in the same occupation the same wage, such as firms that have national wage policies (Hazell *et al.*, 2022).

²⁰The smooth increase in base pay around December 2016 among workers paid \$40 above the overtime exemption threshold in figure 5a provides suggestive evidence that the zero-spillovers assumption holds for at least stayers.

6 Changes in Employment Dynamics

In this section, I examine whether employment dynamics changed after the retraction of the new overtime exemption threshold in December 2016. Given that firms do not adjust wages downward following the injunction of the FLSA rule change, employment levels may be more sensitive to negative demand shocks afterwards compared to if the policy was never announced. To test whether wage hysteresis decreased aggregate employment, I conduct two analyses: a worker-level analysis to examine the effect on separations and a firm-level analysis on net employment.

6.1 Effect on Worker Separation Rates

I start by comparing the trend in separation rates between incumbent workers affected by the FLSA rule change relative to unaffected workers. Following the approach in section 4, I define “affected” workers as those whose weekly base pay was between \$700 and \$913 per week in April 2016. These are workers who are earning right below the new overtime exemption threshold in the month before the announcement of the policy reform. I define “unaffected” workers as those whose weekly base pay was between \$913 and \$993 during the same month. These incumbent workers were already earning above the overtime exemption threshold and therefore not directly affected by the new policy.

Figure 8a 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 9b plots the analogous difference-in-difference estimates, computed using equation 2. As noted, there is a trend-difference both before and after the announcement of the new FLSA overtime exemption threshold. However, there is also a sharp trend-break in December 2016, the month that the new threshold was supposed to go into effect. The precise timing of the kink suggests that separation rates for bunched workers decreased after they received a raise.

To formally measure the change in separation rates, I estimate the slope of the difference-in-difference estimates using equation 3. In particular, I am interested in whether there is a significant change in the rate at which workers separate from their jobs before and after December 2016. Table 3 reports the difference in separation rates between affected and unaffected workers. Column (1) shows that between May and November 2016, incumbent

workers affected by the FLSA rule change were separating from their employers at a rate that is 0.2 p.p. per month quicker than unaffected workers. In comparison, that difference in monthly separation rates shrinks to only 0.04 p.p. per month after December 2016. Together, the estimates suggest that the FLSA rule change reduced separation rates by 0.017 (s.e. 0.002) percentage points per month. Column (2) shows that the result is robust to even comparing incumbents within the same firm over time.

The results thus far indicate that wage persistence actually reduces separation rates rather than increase them. While employers' labor costs increased as a result of the overtime policy, any added incentives to layoff workers appears weaker than the incentives for workers to stay with their employers. This suggests that employers are facing an upward labor supply curve and possess wage-setting power. To benchmark my results to previous studies, I use my estimates to compute an elasticity of separations with respect to wages. Column (1) of table 1 suggests that the salaries of workers initially earning right below the \$913 threshold increased by approximately 1% as a result of the policy from May to December 2016. In comparison, table 3 suggests that monthly separation rates fell by 8% (i.e. $\frac{0.0017}{0.0215}$). Together, the estimates imply an elasticity of 8, which is remarkably close to the elasticity estimated by Emanuel and Harrington (2022).

6.2 Effect on Firm Employment Levels

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

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

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.4 plots the distribution of Share Treated_j . I find that among the 42,619 firms in the sample, 27% of them 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.1 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 show that it is important that I control for state fixed effects to account for 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

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

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 9 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 worker (i.e. salaried and hourly workers), 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) shows that the increase in the wages of salaried workers is reflected in the overall wage bill for medium exposed firms, but not highly exposed firms. Including hourly workers into firms' total labor costs introduces more noise such that the effect on highly exposed firms' wage bill is not statistically significant.

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 significant 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 the high and low exposed firms. After adding the controls, I find no statistically significant impact of the policy on employment levels.

I summarize my results and test for robustness to alternative specifications in table 4. I aggregate the event-study estimates by collapsing the time indicators in equation 5 into a

two post-treatment dummies, by exposure level:

$$\begin{aligned} \log(y_{jt}) = & \beta_1 D_{\text{high}, \text{May}2016 < t < \text{Nov}2016} + \beta_2 D_{\text{high}, t > \text{Dec}2016} \\ & + \gamma_1 D_{\text{med}, \text{May}2016 < t < \text{Nov}2016} + \gamma_2 D_{\text{med}, t > \text{Dec}2016} + \alpha_j + \alpha_t + \varepsilon_{jt} \quad (6) \end{aligned}$$

For conciseness, table 4 only reports the estimates for the impact on highly exposed firms in the months after December 2016 (i.e. β_2). Column (1) of table 4 corresponds to the estimates in figure 9. I find an increase in both firms' salaried wage bill and employment. However, as noted, 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 effect persists, albeit noisily, there is no longer a statistically significant impact on firms' employment. I can rule out employment loss greater than 0.18% and employment gains greater than 1.27% 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.2.

7 Discussion and Conclusion

To summarize, this paper studies the wage and employment response to the unexpected retraction of a major overtime reform. The policy, which was set to go into effect in December 2016, would have granted overtime coverage to all salaried workers earning less than \$913 per week. In anticipation of the reform, employers promised workers a raise to the new overtime exemption threshold. Although the reform was rescinded, employers nevertheless raised and bunched workers' salaries at the anticipated threshold. Consistent with changes in fairness norms, I show that firms do not revert stayers' wages back to their pre-policy levels following the injunction of the policy, nor did they slow down bunched workers' future wage growth relative to workers unaffected by the rule change. Similarly, employers continued to bunch the salaries of new hires at the nullified threshold. Comparing the work history of bunched and non-bunched hires, I find that the bunching cannot be explained by changes in the composition of new entrants. Taken together, these results provide real-world evidence that labor market policies can alter wage-setting norms, leading to persistent impacts of policies even after they are rescinded.

The results of my study sheds new insights into the sources of downward nominal wage rigidity. First, economists have incorporated wage rigidity into macro models by assuming

that contracts are renegotiated periodically (Taylor, 1979, 1980; Christiano *et al.*, 2005; Gertler and Trigari, 2009). While I find evidence that employers indeed adjust the wages of workers only once per year, the bunching mass persists for at least 1.5 years with no indication of convergence. The long-run rigidity in both spot wages and wage growth suggests that staggered bargaining alone cannot explain the wage dynamics observed in the data. Second, another source of wage rigidity discussed in the literature is the existence of implicit contracts (Beaudry and DiNardo, 1991; Schmieder and Von Wachter, 2010). In long-term employment relationships, initial labor market conditions may have persistent effects as employers and workers agree to a future stream of payments that insures risk-averse workers against wage declines. However, since implicit contracts depend on the initial conditions at hire, they alone do not rationalize why I observe firms continue bunching new hires at the overtime exemption threshold even after it is no longer binding.

A simple explanation for the persistence in spot wages, wage growth, and entry wages is the existence of relative pay concerns among workers. A growing body of work has shown that perceptions of unfair pay differences among peers reduces workers' job satisfaction (Card *et al.*, 2012), retention (Dube *et al.*, 2019), and effort (Breza *et al.*, 2018). Consistent with the presence of equity concerns, previous laboratory experiments have shown that temporary policies can have persistent effects on workers' reservation wages (Falk *et al.*, 2006). My paper provides the first real-world evidence that fairness norms may lead to wage hysteresis, causing salaries to remain elevated even years after a labor market policy has been retracted. To better understand the interaction between fairness concerns and labor market policies, future research could explore under what circumstances do policies change fairness norms, and how might behavioral concerns impact the effects of policies that are still active.

References

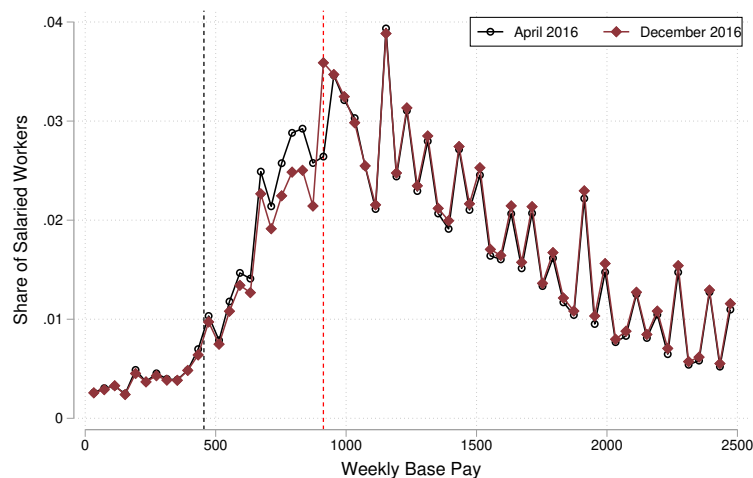
- AKERLOF, G. A. and YELLEN, J. L. (1990). The Fair Wage-Effort Hypothesis and Unemployment*. *The Quarterly Journal of Economics*, **105** (2), 255–283.
- ALLCOTT, H. and ROGERS, T. (2014). The short-run and long-run effects of behavioral interventions: Experimental evidence from energy conservation. *American Economic Review*, **104** (10), 3003–37.
- ALTONJI, J. G. and DEVEREUX, P. J. (1999). *The Extent and Consequences of Downward Nominal Wage Rigidity*. Working Paper 7236, National Bureau of Economic Research.
- BARATTIERI, A., BASU, S. and GOTTSCHALK, P. (2014). Some evidence on the importance of sticky wages. *American Economic Journal: Macroeconomics*, **6** (1), 70–101.
- BARRO, R. J. (1977). Long-term contracting, sticky prices, and monetary policy. *Journal of Monetary Economics*, **3** (3), 305–316.
- BEAUDRY, P. and DINARDO, J. (1991). The effect of implicit contracts on the movement of wages over the business cycle: Evidence from micro data. *Journal of Political Economy*, **99** (4), 665–88.
- BLANCHARD, O. J. and SUMMERS, L. H. (1986). *Hysteresis and the European Unemployment Problem*. Working Paper 1950, National Bureau of Economic Research.
- BREZA, E., KAUR, S. and SHAMDASANI, Y. (2018). The morale effects of pay inequality. *The Quarterly Journal of Economics*, **133** (2), 611–663.
- CAMPBELL, I., CARL M. and KAMLANI, K. S. (1997). The Reasons for Wage Rigidity: Evidence from a Survey of Firms*. *The Quarterly Journal of Economics*, **112** (3), 759–789.
- CARD, D. (2022). Who set your wage? *American Economic Review*, **112** (4), 1075–90.
- and HYSLOP, D. (1996). *Does Inflation Grease the Wheels of the Labor Market?* Working Paper 5538, National Bureau of Economic Research.
- and — (1997). Does inflation” grease the wheels of the labor market”? In *Reducing inflation: Motivation and strategy*, University of Chicago Press, pp. 71–122.
- , MAS, A., MORETTI, E. and SAEZ, E. (2012). Inequality at work: The effect of peer salaries on job satisfaction. *American Economic Review*, **102** (6), 2981–3003.
- CENGIZ, D., DUBE, A., LINDNER, A. and ZIPPERER, B. (2019). The effect of minimum wages on low-wage jobs. *The Quarterly Journal of Economics*.
- CHARNESS, G. and GNEEZY, U. (2009). Incentives to exercise. *Econometrica*, **77** (3), 909–931.
- CHRISTIANO, L., EICHENBAUM, M. and EVANS, C. (2005). Nominal rigidities and the

- dynamic effects of a shock to monetary policy. *Journal of Political Economy*, **113** (1), 1–45.
- COVIELLO, D., DESERRANNO, E. and PERSICO, N. (2022). Minimum wage and individual worker productivity: Evidence from a large us retailer. *Journal of Political Economy*, **130** (9), 2315–2360.
- CULLEN, Z. and PEREZ-TRUGLIA, R. (2022). How much does your boss make? the effects of salary comparisons. *Journal of Political Economy*, **130** (3), 766–822.
- CULLEN, Z. B., LI, S. and PEREZ-TRUGLIA, R. (2022). *What’s My Employee Worth? The Effects of Salary Benchmarking*. Working Paper 30570, National Bureau of Economic Research.
- DAVIS, S. J. and KROLIKOWSKI, P. M. (2023). *Sticky Wages on the Layoff Margin*. Working Paper 31528, National Bureau of Economic Research.
- DUBE, A., GIULIANO, L. and LEONARD, J. (2019). Fairness and frictions: The impact of unequal raises on quit behavior. *American Economic Review*, **109** (2), 620–63.
- ELSBY, M. W. (2009). Evaluating the economic significance of downward nominal wage rigidity. *Journal of Monetary Economics*, **56** (2), 154 – 169.
- ELSBY, M. W. L. and SOLON, G. (2019). How prevalent is downward rigidity in nominal wages? international evidence from payroll records and pay slips. *The Journal of Economic Perspectives*, **33** (3), 185–201.
- EMANUEL, N. and HARRINGTON, E. (2022). *Firm frictions and the payoffs of higher pay: Labor supply and productivity responses to a voluntary firm minimum wage*. Tech. rep., Working Paper.
- FALK, A., FEHR, E. and ZEHNDER, C. (2006). Fairness Perceptions and Reservation Wages—the Behavioral Effects of Minimum Wage Laws*. *The Quarterly Journal of Economics*, **121** (4), 1347–1381.
- FEHR, E. and GOETTE, L. (2005). Robustness and real consequences of nominal wage rigidity. *Journal of Monetary Economics*, **52** (4), 779–804, sNB.
- , — and ZEHNDER, C. (2009). A behavioral account of the labor market: The role of fairness concerns. *Annual Review of Economics*, **1** (1), 355–384.
- GARDNER, P. (2024). Michigan right-to-work repeal: What workers, businesses need to know.
- GERTLER, M. and TRIGARI, A. (2009). Unemployment fluctuations with staggered nash wage bargaining. *Journal of Political Economy*, **117** (1), 38–86.
- GINÉ, X., KARLAN, D. and ZINMAN, J. (2010). Put your money where your butt is: A com-

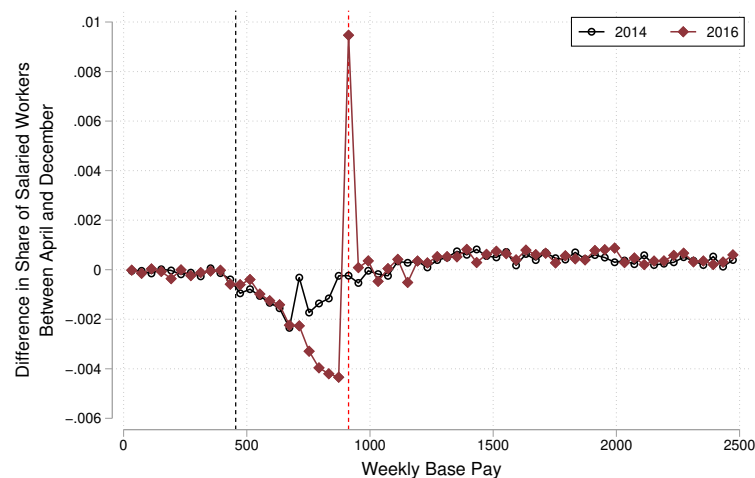
- mitment contract for smoking cessation. *American Economic Journal: Applied Economics*, **2** (4), 213–35.
- GRIGSBY, J., HURST, E. and YILDIRMAZ, A. (2019). *Aggregate Nominal Wage Adjustments: New Evidence from Administrative Payroll Data*. Working Paper 25628, National Bureau of Economic Research.
- , — and — (2021). Aggregate nominal wage adjustments: New evidence from administrative payroll data. *American Economic Review*, **111** (2), 428–71.
- HAZELL, J., PATTERSON, C., SARSONS, H. and TASKA, B. (2022). *National Wage Setting*. Working Paper 30623, National Bureau of Economic Research.
- JARDIM, E. S., SOLON, G. and VIGDOR, J. L. (2019). *How Prevalent Is Downward Rigidity in Nominal Wages? Evidence from Payroll Records in Washington State*. Working Paper 25470, National Bureau of Economic Research.
- KAHN, S. (1997). Evidence of nominal wage stickiness from microdata. *The American Economic Review*, **87** (5), 993–1008.
- KATZ, L. F. (1986). Efficiency wage theories: A partial evaluation. *NBER Macroeconomics Annual*, **1**, 235–276.
- KAUR, S. (2019). Nominal wage rigidity in village labor markets. *American Economic Review*, **109** (10), 3585–3616.
- KEYNES, J. M. (1936). *The General Theory of Employment, Interest, and Money*. London: Macmillan.
- KUDLYAK, M. (2014). The cyclical of the user cost of labor. *Journal of Monetary Economics*, **68** (C), 53–67.
- KURMANN, A. and MCENTARFER, E. (2019). Downward nominal wage rigidity in the united states: new evidence from worker-firm linked data. *Drexel University School of Economics Working Paper Series WP*, **1**.
- LARCOM, S., RAUCH, F. and WILLEMS, T. (2017). The Benefits of Forced Experimentation: Striking Evidence from the London Underground Network*. *The Quarterly Journal of Economics*, **132** (4), 2019–2055.
- MAS, A. (2006). Pay, reference points, and police performance. *The Quarterly Journal of Economics*, **121** (3), 783–821.
- MILLER, C. (2017). The persistent effect of temporary affirmative action. *American Economic Journal: Applied Economics*, **9** (3), 152–90.
- NATIONAL EMPLOYMENT LAW PROJECT (2019). Report: Workers lose billions thanks to corporate campaign to block local raises. World Scientific.

- OWENS, M. F. and KAGEL, J. H. (2010). Minimum wage restrictions and employee effort in incomplete labor markets: An experimental investigation. *Journal of Economic Behavior and Organization*, **73** (3), 317–326.
- PISSARIDES, C. A. (2009). The unemployment volatility puzzle: Is wage stickiness the answer? *Econometrica*, **77** (5), 1339–1369.
- QUACH, S. (2024). The labor market effects of expanding overtime coverage. *Job Market Paper*.
- SAEZ, E., SCHOEFER, B. and SEIM, D. (2021). Hysteresis from employer subsidies. *Journal of Public Economics*, **200**, 104459.
- SCHEIBER, N. (2023). Labor board, reversing trump-era ruling, widens definition of employee.
- SCHMIEDER, J. F. and VON WACHTER, T. (2010). Does wage persistence matter for employment fluctuations? evidence from displaced workers. *American Economic Journal: Applied Economics*, **2** (3), 1–21.
- SCHOEFER, B. (2021). *The Financial Channel of Wage Rigidity*. Working Paper 29201, National Bureau of Economic Research.
- TAYLOR, J. B. (1979). Staggered wage setting in a macro model. *The American Economic Review*, **69** (2), 108–113.
- (1980). Aggregate dynamics and staggered contracts. *Journal of Political Economy*, **88** (1), 1–23.
- U.S. DEPARTMENT OF LABOR (2016). Defining and delimiting the exemptions for executive, administrative, professional, outside sales and computer employees. *Federal Register*, **81** (99), 32391–32552.
- VON WILPERT, M. (2017). Missouri’s new preemption law cheats 38,000 workers out of a raise.
- WALL STREET JOURNAL (). Some employers stick with raises despite uncertainty on overtime rule.

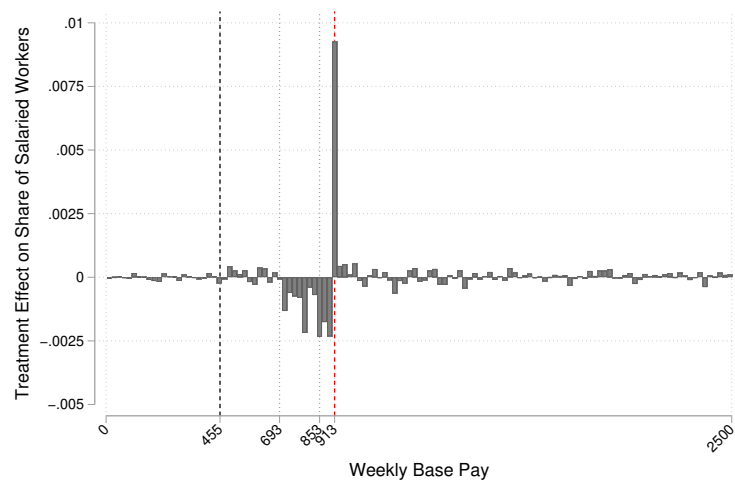
Figure 1: Change in the Density of Base Pay Between April and December 2016



(a) Raw Averages



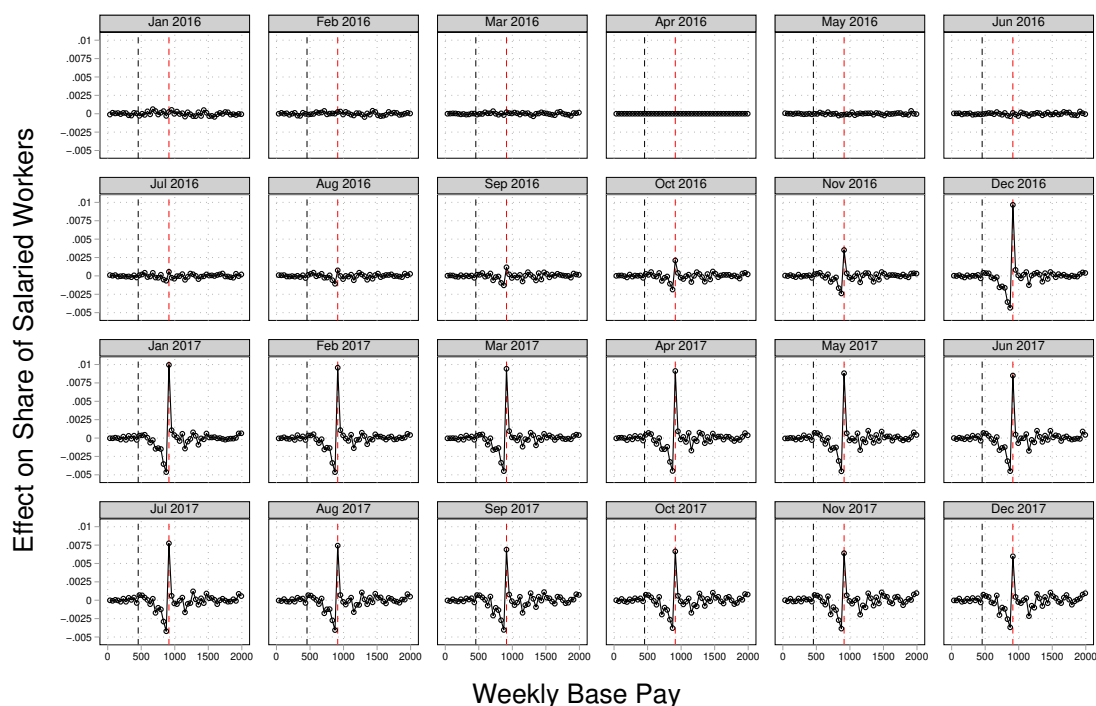
(b) Difference in Distribution



(c) Diff-in-Diff Distribution

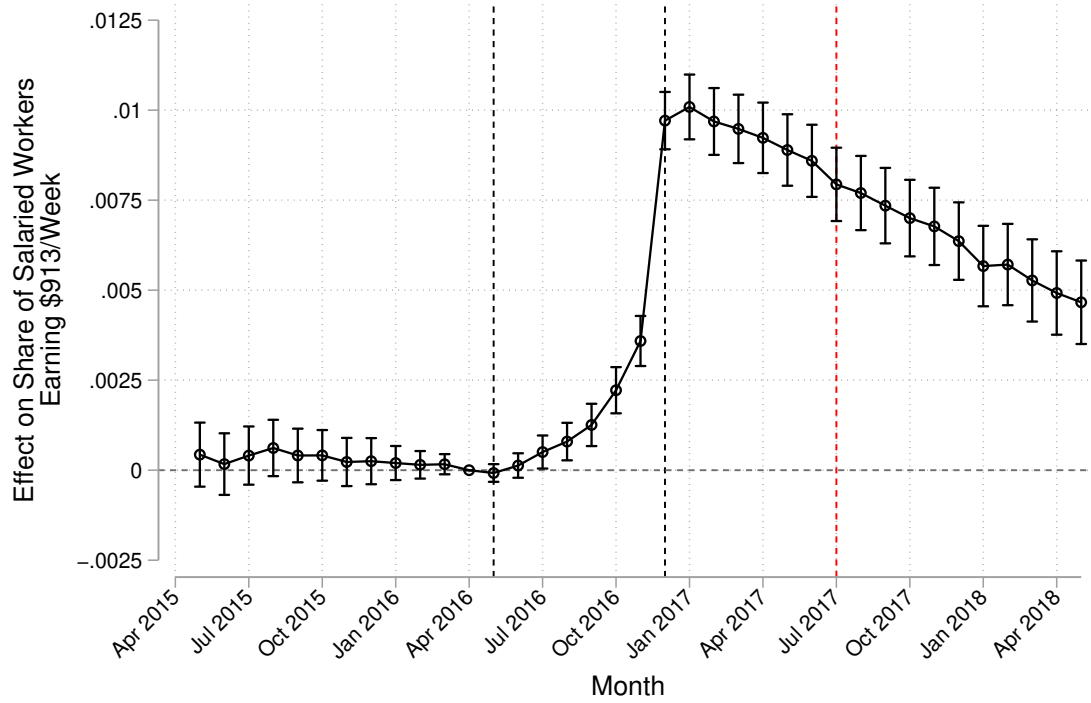
Notes: In panel (a), the blue and red lines show the density of base pay in April and December 2016, respectively. The sample is restricted to salaried workers who are continuously employed at the same firm from May 2015 to April 2018. In panel (b), the blue (red) line shows the difference in the density between April and December of 2016 (2014). 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 2: Effect on Distribution of Continuously Employed Salaried Workers Relative to April 2016



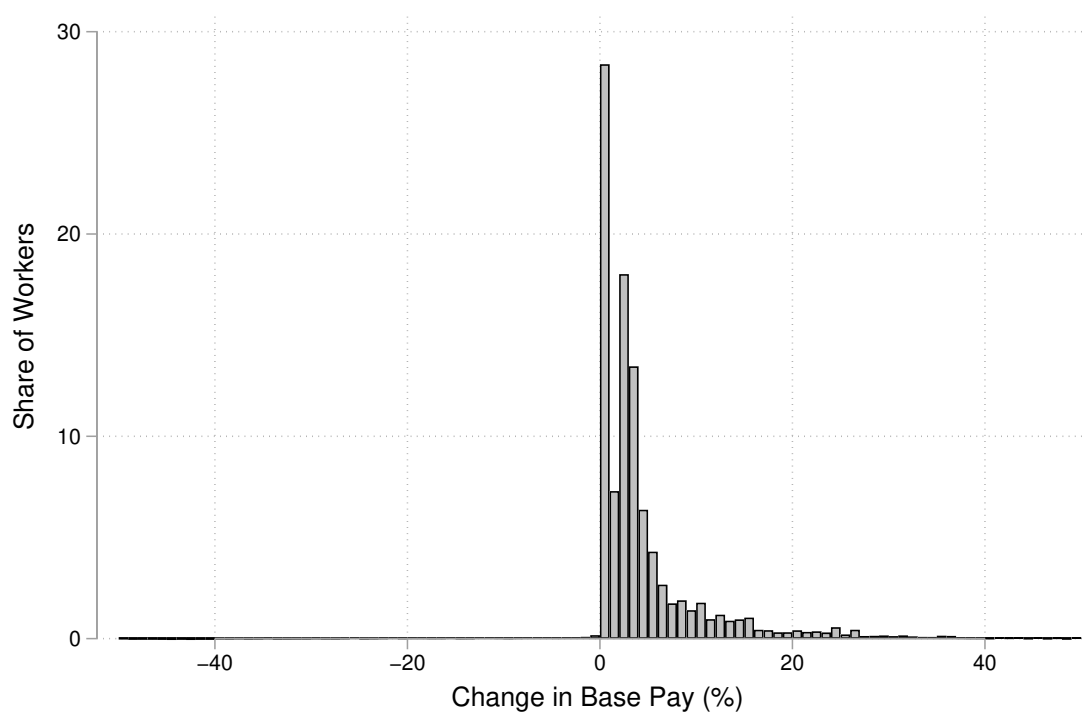
Notes: This figure shows difference-in-difference estimates that compare changes to the base pay distribution since April 2016 to changes over the same number of months since April 2014 (see equation 1). The x-axis is divided into \$40 increments, where the black vertical dashed line is at \$433 and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

Figure 3: Share of Workers Bunched Over Time



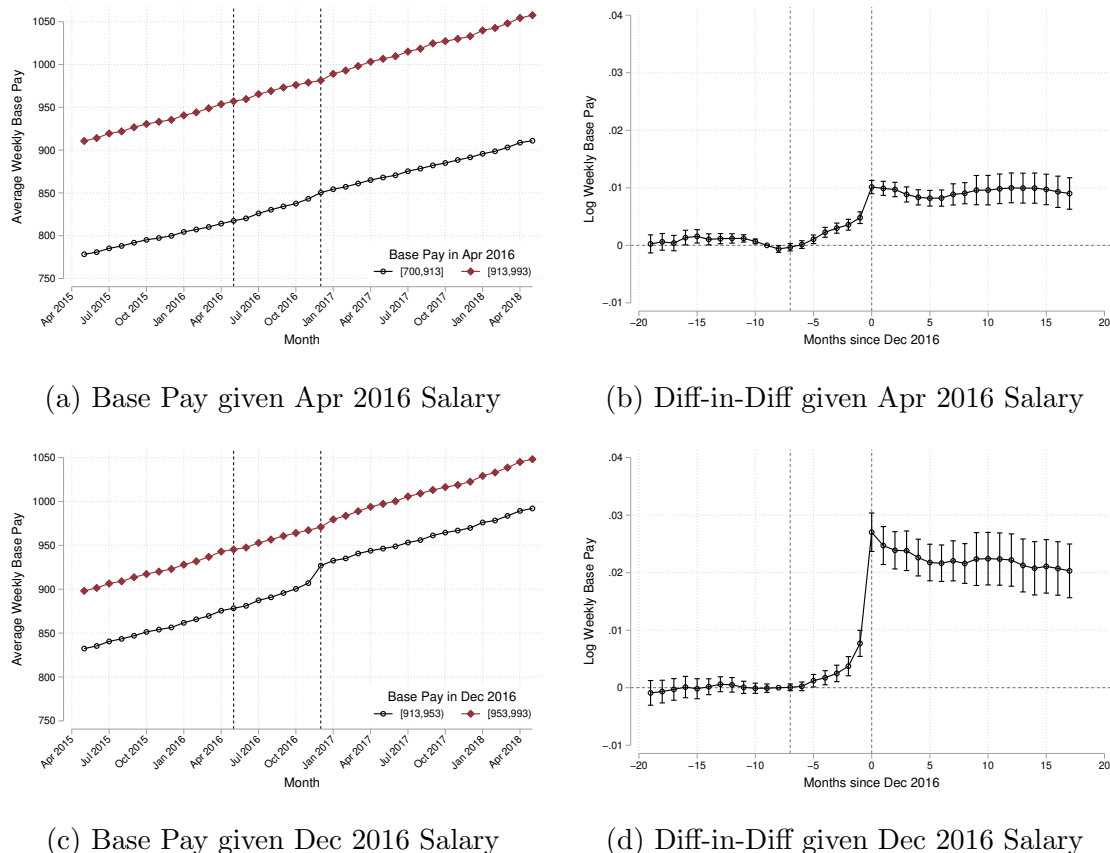
Notes: This figure shows the effect of the 2016 FLSA policy on the share of salaried workers earning between \$913 and \$953 per week, estimated from equation 1. The sample consists of workers who are always salaried, and continuously employed at the same firm between May 2015 and April 2018. The first 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.

Figure 4: Distribution of One-Year Change in Base Pay for Workers Bunched in December 2016



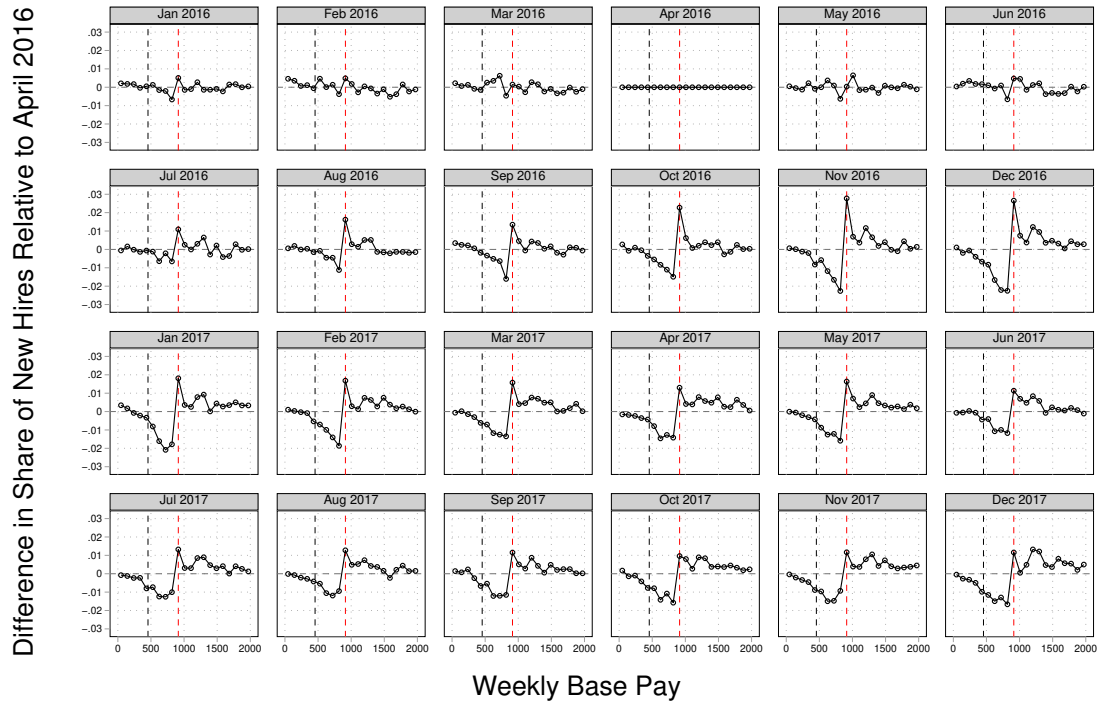
Notes: This figure shows the distribution of the percent change in weekly base pay between December 2016 and 2017. The sample consists of salaried workers who are continuously employed at the same firm from May 2015 to April 2018, and earned between \$913 and \$953 in December 2016.

Figure 5: Difference-in-Difference of Base Pay Between Bunched and Non-Bunched Workers



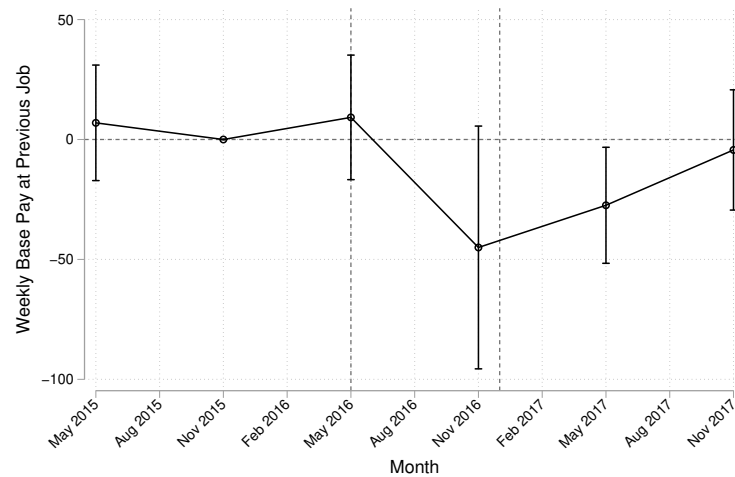
Notes: Panel (a) shows the evolution of weekly base pay over time for salaried workers who earned within [700,913) and [953,993) per week in April 2016. Panel (b) plots the difference-in-difference estimates of the two lines in panel (a), with the treatment group being workers in the [700,913) bin. Panels (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 6: Distribution of New Hires Over Time Relative to April 2016

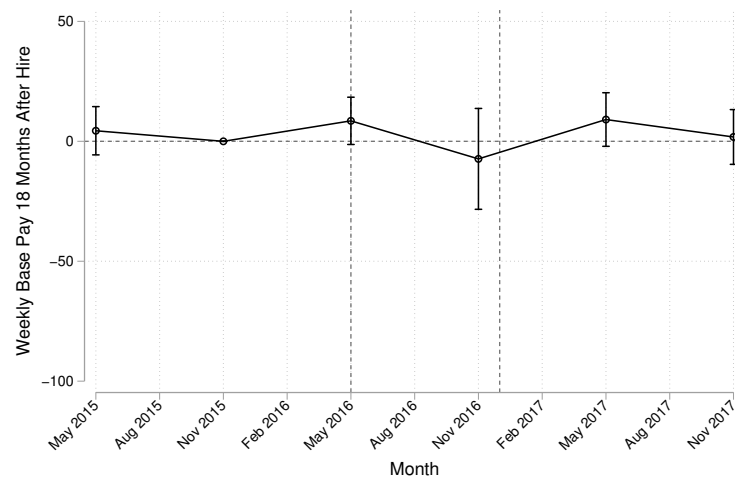


Notes: This figure shows the share of new salaried hires within \$96.15 increments of weekly base pay relative to the share in April 2016. In each panel, the black vertical dashed line is at \$432 and the red vertical dashed line is at \$913.

Figure 7: Difference-in-Difference Estimates Comparing New Hires Earning At and Above the Overtime Exemption Threshold



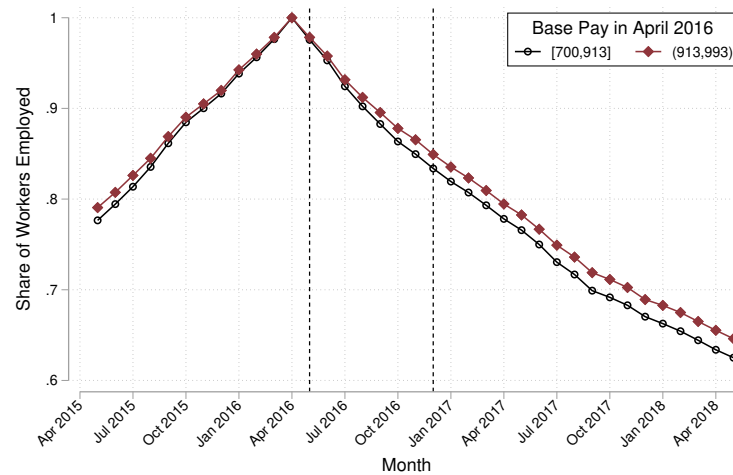
(a) Outcome: Pay at Previous Job



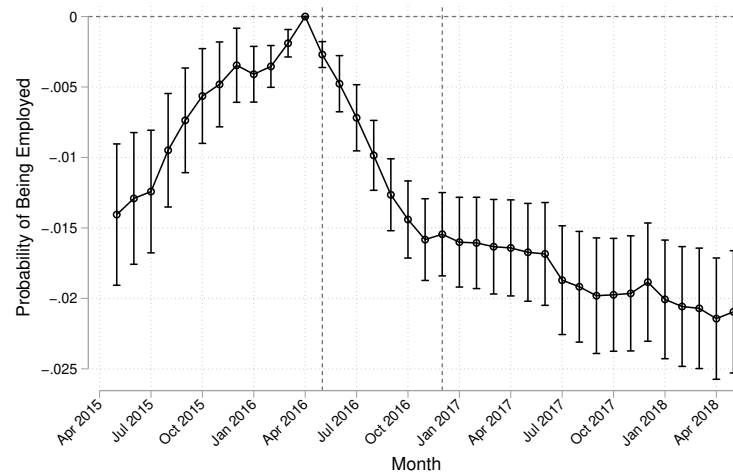
(b) Outcome: Pay 18 Months After Hire

Notes: This figure plots the estimates from a difference-in-difference regression 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 of data. Panel (a) uses workers' base pay at their last observed employer as the outcome, and panel (b) uses workers' base pay 18 months after hire conditional on continuous employment. The sample is restricted to workers hired between May 2016 and May 2018. In both figures, the left vertical dashed line is at May 2016, and the right vertical dashed line is at December 2016.

Figure 8: Difference-in-Difference of Separation Rates Between Bunched and Non-Bunched Workers



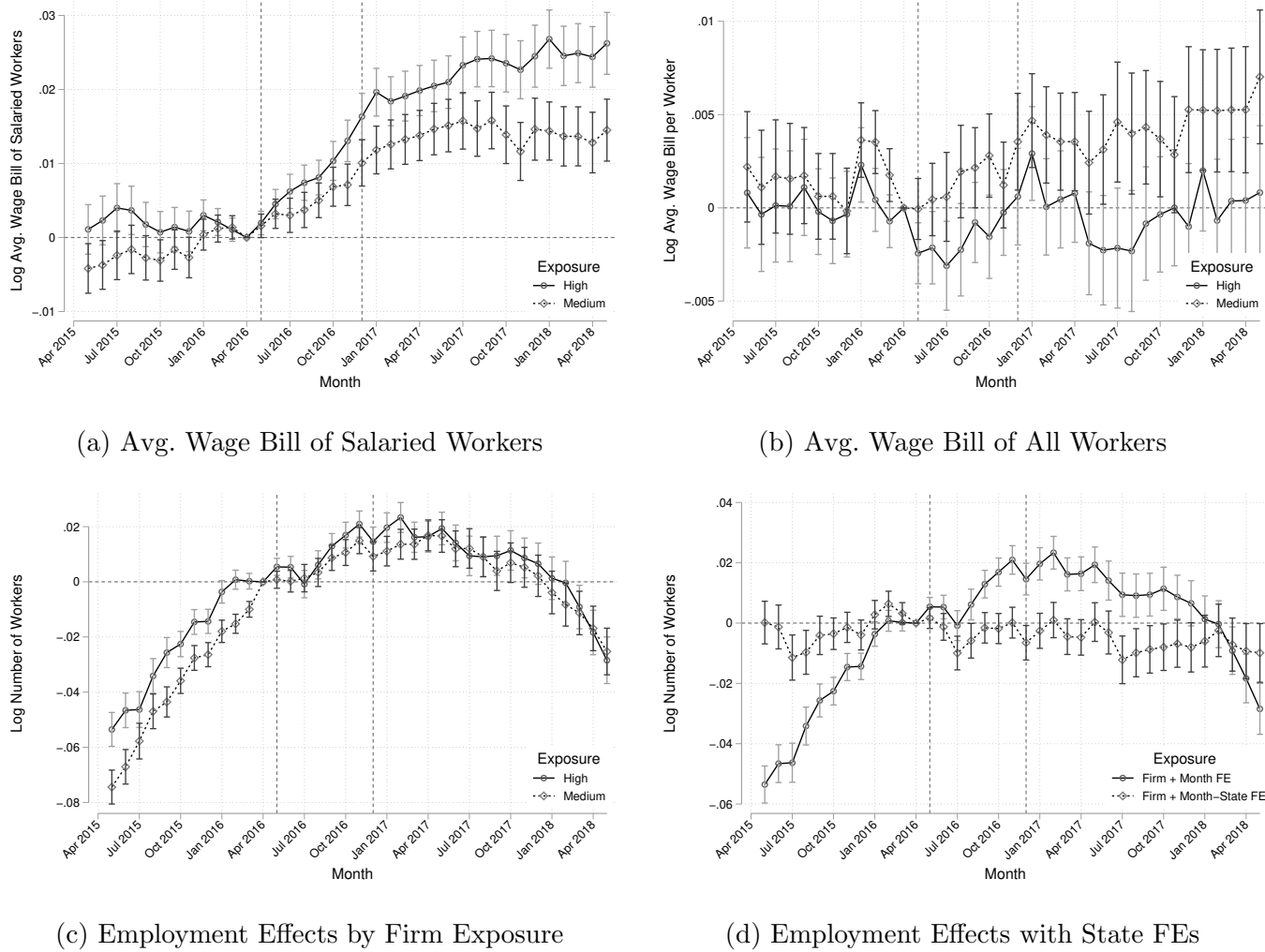
(a) Prob. Employed, by Salary in Apr 2016



(b) Diff-in-Diff, by Salary in Apr 2016

Notes: Panel (a) shows the probability of employment 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. In all figures, the left vertical dashed line is at May 2016, and the right vertical dashed line is at December 2016.

Figure 9: Difference-in-Difference Estimates Comparing Firms by Exposure Level



Notes: This figure plots difference-in-difference estimates that compares firms that had a high or medium share of workers impacted by the 2016 FLSA rule change, relative to firms that had few such workers (see equation 5). Panels (a)-(c) plots the effect on avg. salaried workers' wage bill, avg. wage bill for all workers, and firm employment, respectively. Panel (d) presents the employment effects for highly exposed firms with and without state-month fixed effects. In all figures, the left vertical dashed line is at May 2016, and the right vertical dashed line is at December 2016.

Table 1: Effect on Stayers' Base Pays Over Time

	(1)	(2)	(3)	(4)
Pre-trend	-.0001 (.0001)	.0001* (.0001)	.0001 (.0001)	.0001 (.0001)
Anticipation	.0013*** (.0001)	.0015*** (.0001)	.0028*** (.0002)	.0022*** (.0001)
Post-trend	0.0000 (.0001)	.0002*** (0.0000)	-.0002 (.0002)	-.0002* (.0001)
Change in Slope	.0001 (.0001)	.0001 (.0001)	-.0003 (.0002)	-.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 weekly base pay 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 slope 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 2: Effect on the Composition and Wage Growth of New Hires

	(1)	(2)	(3)	(4)
Base Pay in Previous Job	-45.01* (25.824)	-71.40 (49.798)		-52.437 (38.672)
% Δ Base Pay from Prev. Job	.071*** (.026)	.041 (.029)		.064* (.034)
Base Pay After 18 Months			-7.322 (10.726)	-.232 (7.605)
% Δ Base Pay After 18 Months			-.003 (.011)	.004 (.008)
Bin FE	Y	Y	Y	Y
Hire Date FE	Y	Y	Y	Y
N	60,937	29,225	103,643	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 the 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), the 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 3: Effect on the Probability that Incumbents are Employed

	(1)	(2)
Pre-trend	.0012*** (.0002)	.0028*** (.0002)
Anticipation	−.002*** (.0002)	−.002*** (.0002)
Post-trend	−.0004*** (.0001)	−.0006*** (.0001)
Change in Slope	.0017*** (.0002)	.0014*** (.0002)
Baseline Monthly Separations	.0214	.0214
Worker FE	Y	Y
Month FE	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 in slope 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.

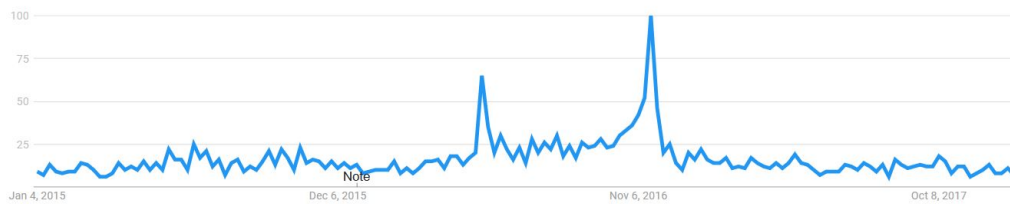
Table 4: Effect on Highly Exposed Firm's Employment and Wage Bill

	(1)	(2)	(3)
Log Employment	.0282*** (.0031)	-.0041 (.0036)	-.0045 (.0048)
Log Avg. Wage Bill	-.0005 (.0013)	.0001 (.0018)	.0004 (.0024)
Log Avg. Salaried Wage Bill	.0207*** (.0017)	.0264*** (.0025)	.0268*** (.0031)
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 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.

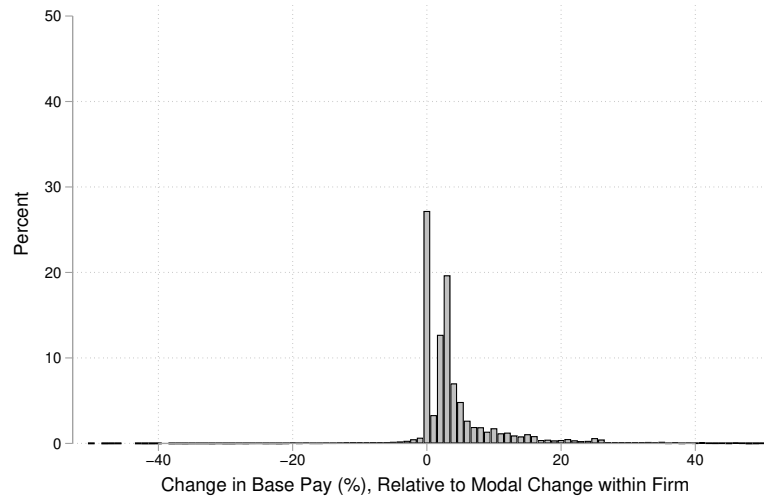
Appendix A. Supplementary figures and tables noted in the text

Appendix Figure A.1: Google Search Popularity for the Term “FLSA Overtime”

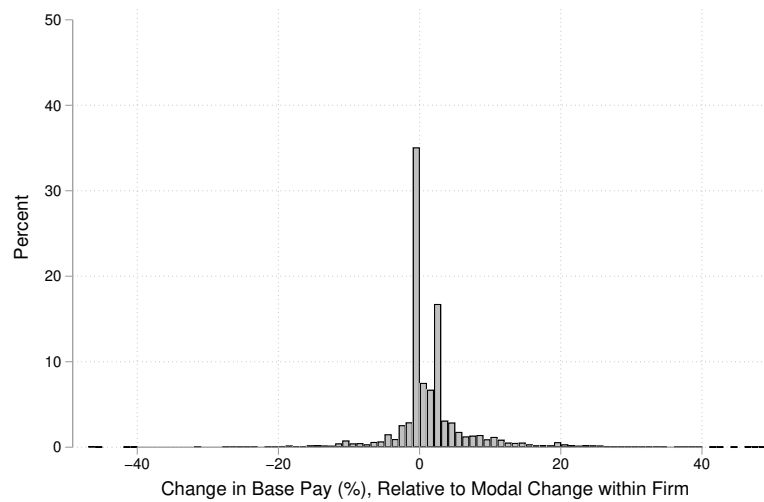


Notes: This figure shows the relative popularity of “FLSA Overtime” as a Google search term between January 2015 and December 2017. A value of 100 indicates its highest popularity level, and the measure of popularity is scaled proportional to that instance.

Appendix Figure A.2: Change in Base Pay Relative to Modal Change within Firm



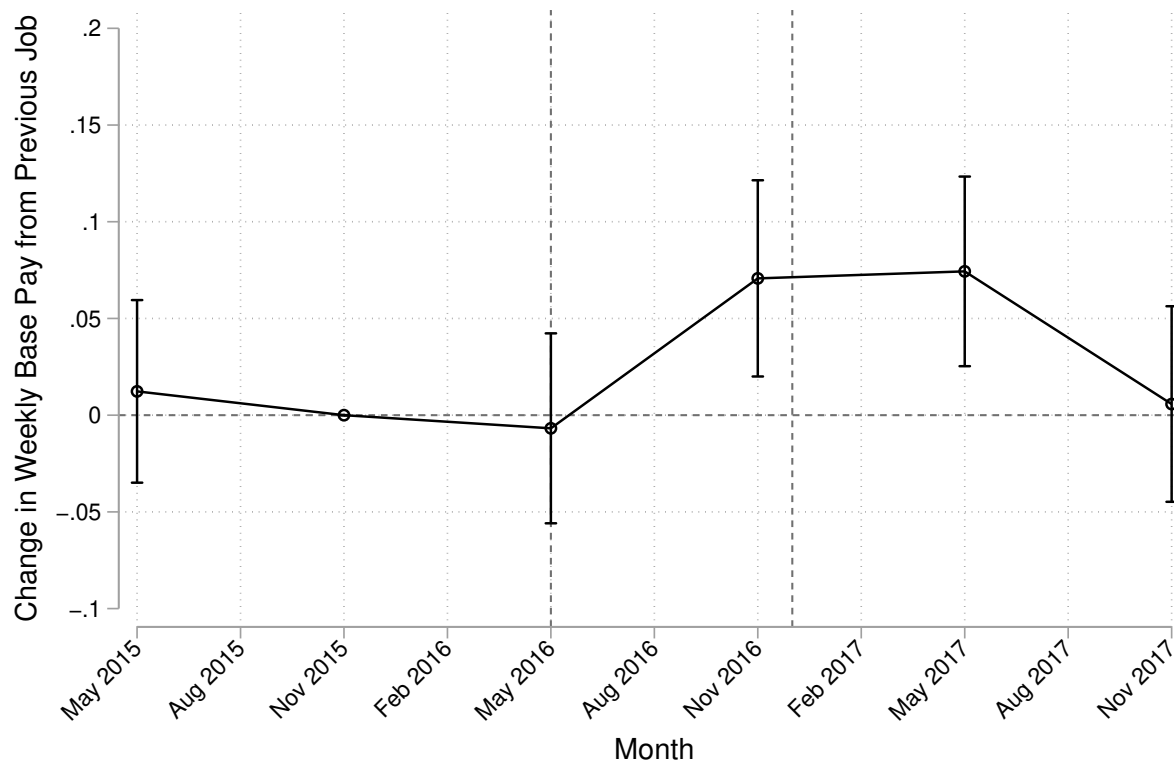
(a) Sample: Minimum 50 Stayers in Firm



(b) Sample: Minimum 50 Stayers, Mode $\neq 0$

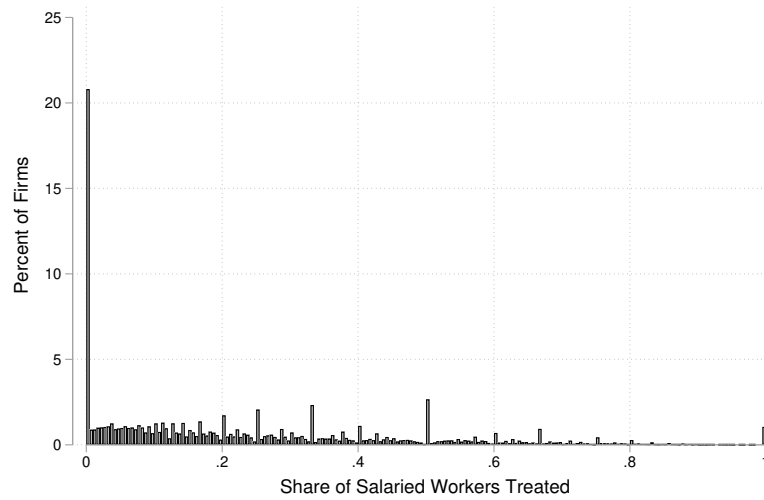
Notes: This figure shows the distribution of workers by their percent change in weekly base pay between December 2016 and 2017, relative to the modal change within each workers' employer. The sample consists of salaried workers who are continuously employed at the same firm from May 2015 to April 2018, 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.

Appendix Figure A.3: Difference in Difference Estimates for New Hires: Change in Base Pay from Job Transition

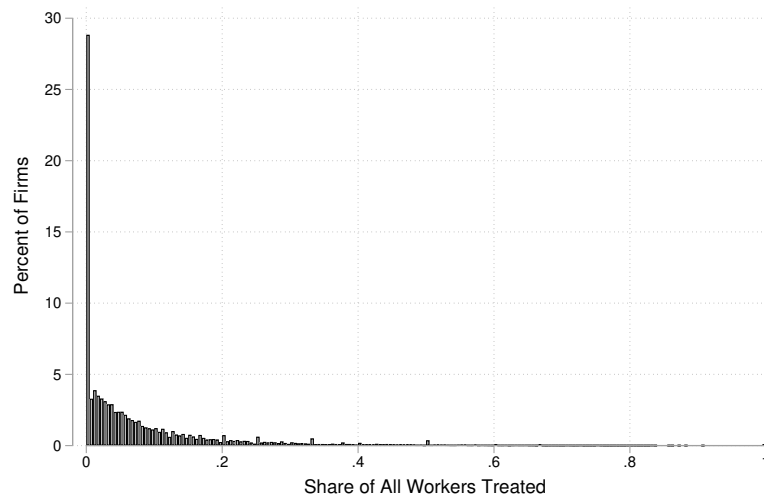


Notes: This figure plots the estimates from a difference-in-difference that compares repeated cross-sections of new hires employed at a base pay of \$913-953 per week to those hired at \$953-993 per week. Each estimate is averaged over 6 months. The outcome is the percent change in base pay from each 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.

Appendix Figure A.4: Distribution of Firms by Share of Salaried Workers' Affected by FLSA Rule Change



(a) Share of Salaried Workers



(b) Share of All Workers

Notes: Panel (a) plots the distribution of firms in April 2016, by the share of salaried workers with base pays between \$455 and \$913 per week. Panel (b) plots the distribution by the share of all workers who are salaried and have a base pay within the treated interval.

Appendix Table A.1: Firm Descriptive Statistics by Share Treated, April 2016

	Share Treated		
	Treat $\leq .01$	$.01 < \text{Treat} \leq .07$	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.2: Effect on Medium Exposed Firm's Employment and Wage Bill

	(1)	(2)	(3)
Log Employment	.0387*** (.0033)	.0054 (.0037)	.0049 (.0047)
Log Avg. Wage Bill	.0028** (.0014)	.0027 (.0018)	.0006 (.0023)
Log Avg. Salaried Wage Bill	.0153*** (.0017)	.0204*** (.0025)	.02*** (.003)
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 B. Role of Staggered Bargaining

In this section, I first replicate the evidence for staggered bargaining highlighted by Grigsby *et al.* (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 B.1 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 B.2 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.

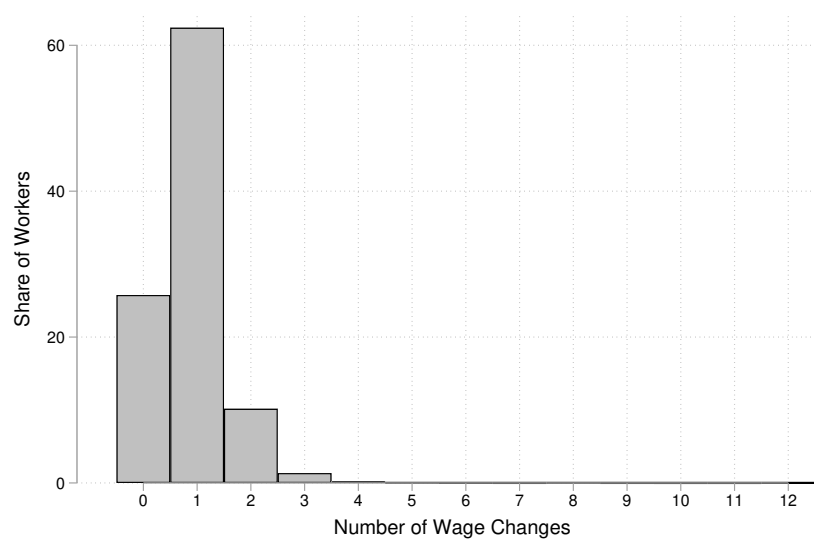
Next, I argue that the periodicity of wage adjustments does not explain the observed wage dynamics before and after the proposed change to the overtime exemption threshold announced in 2016. First, if the only cause for the persistence in bunching at the threshold after December 2016 is due to staggered 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 B.3 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 3 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 B.4 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, the evolution of the share of firms' workers at the threshold in 2016 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 September 2015, 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.

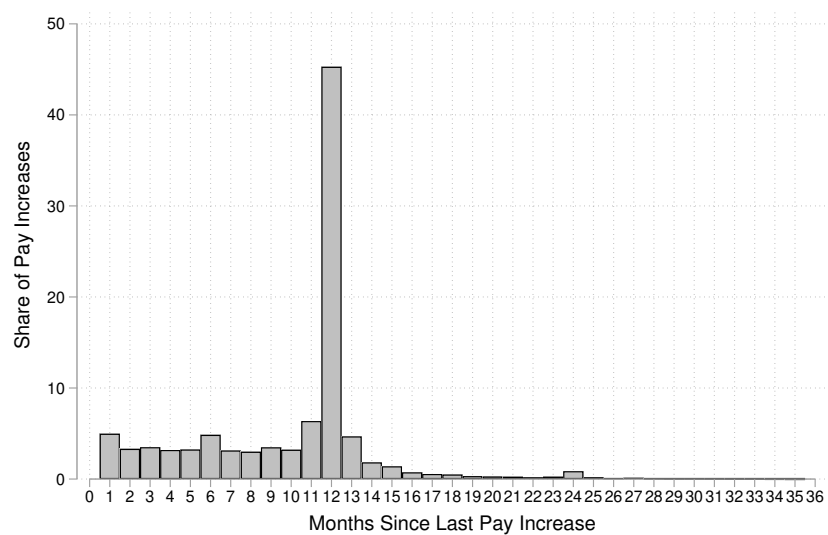
While the primary focus of my paper is the persistence in wages post-injunction, I nevertheless provide a brief analysis into the understanding which firms are bunching workers early pre-injunction. Interestingly, the data suggests that small firms are more likely to react early relative to large firms. Figure B.5 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 B.1: Distribution of Workers by Number of Wage Changes per Year



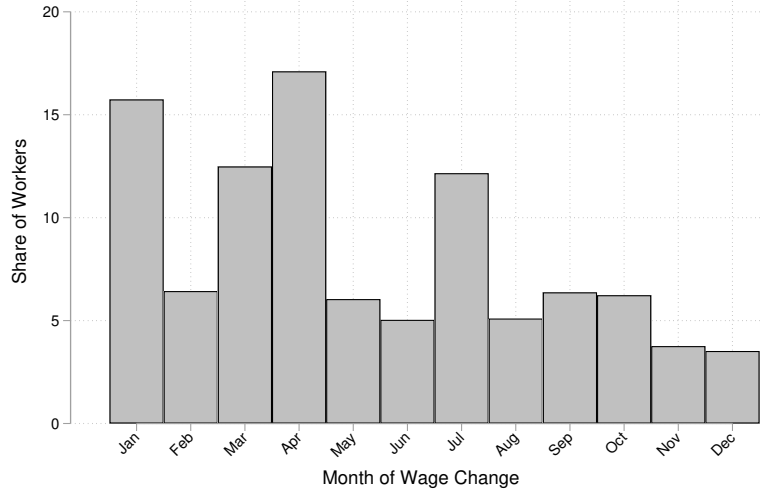
Notes: This figure shows the distribution of workers by the number of months they experienced a change in base pay relative to the previous month, for each pair of months from May 2015-2016. The sample comprises of salaried workers who are continuously employed between May 2015-2018.

Appendix Figure B.2: Distribution of Months Since Last Pay Increase Among all Wage Changes

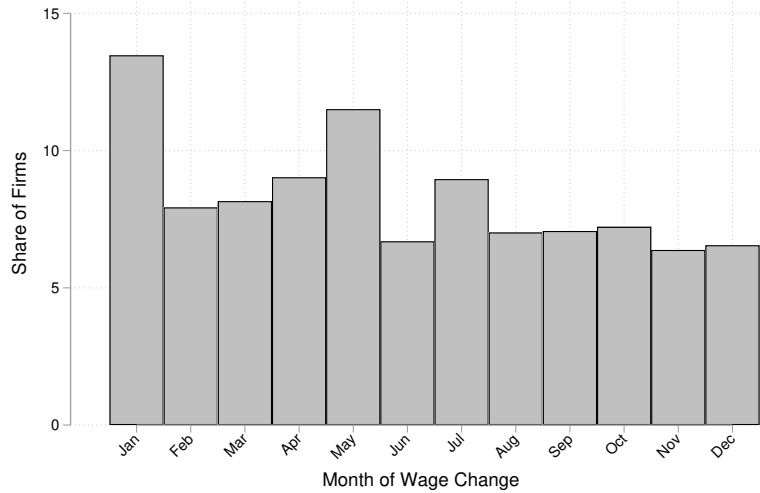


Notes: This figure restricts the sample to all wage changes among salaried stayers between May 2015-2018, and plots the distribution of the number of months since the previous pay increase.

Appendix Figure B.3: Distribution of Month of Pay Increases



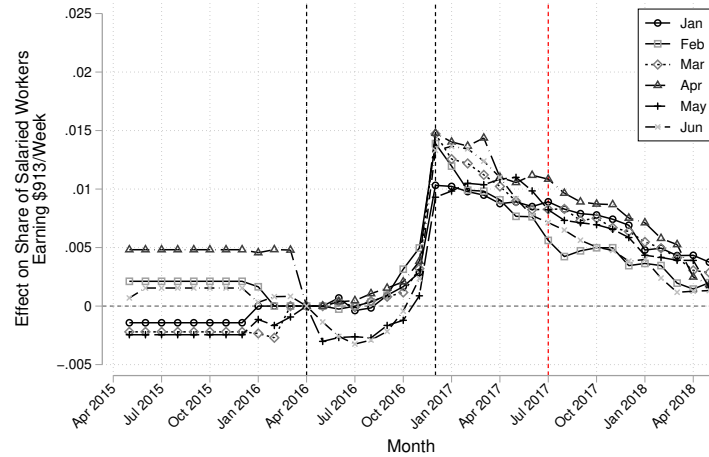
(a) Distribution of Workers by Month of Wage Change



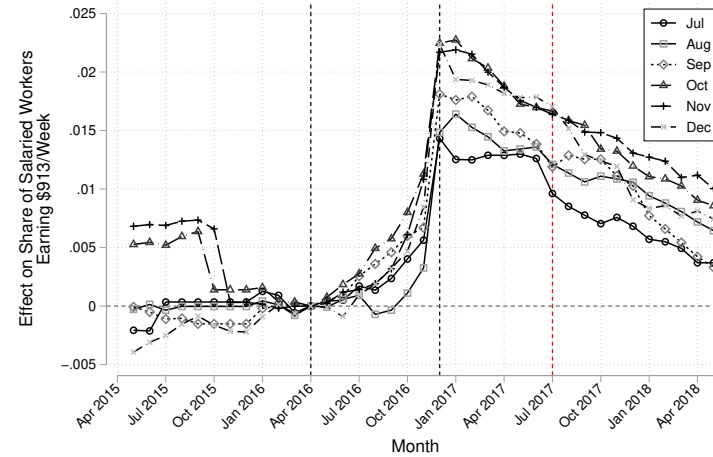
(b) Distribution of Modal Month of Wage Changes within Firm

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 B.4: Share of Workers Bunched Over Time, by Month of Last Pay Increase Before May 2016



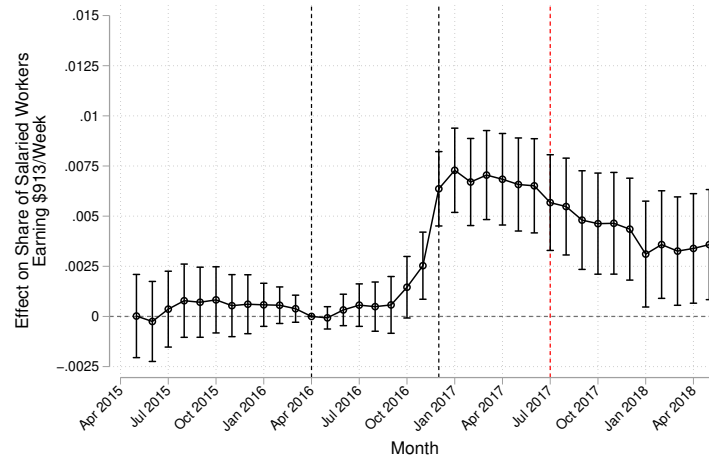
(a) January to June Wage Changers



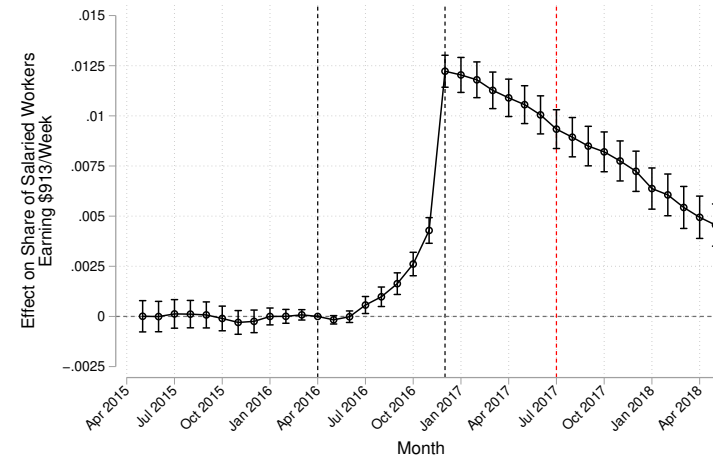
(b) July to December Wage Changers

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 B.5: 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 C. Regression Discontinuity Design to Evaluating Wage Rigidity of New Hires

In this section, I implement a regression discontinuity type design to study whether workers hired with a base pay at the \$913 overtime exemption threshold after May 2016 were more positively selected or experienced slower wage growth relative to the predicted productivity and wage growth of hires absent the policy. Intuitively, I use the characteristics of new hires in parts of the income distribution away from the \$913 threshold 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 C.1 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 average past wage of new hires 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 indication of their productivity and employers become more selective after bunching new hires' salaries at the threshold, then I would expect the cohort of new hires after November 2016 to have prior wage histories that lie on or above the linear prediction line. In contrast, panel (b) find that after May 2016, workers hired at the threshold have slightly 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 result therefore suggests that employers are actually hiring workers of lower productivity at the threshold than they otherwise would have absent the policy.

Inspired by the linearity in entry base pay from figure C.1, I estimate the following regression for 3-month cohorts of new hires:

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

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 C.2 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 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, while 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.

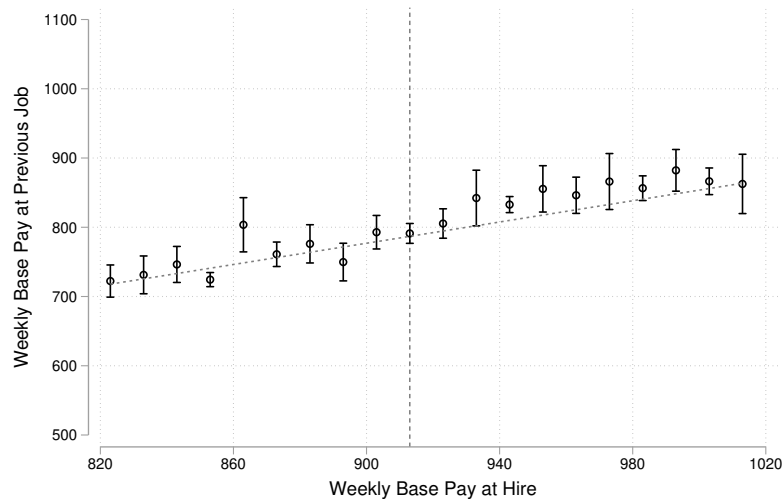
I show the robustness of my results to alternative specifications in table C.1. The first estimate in column (1) corresponds to the Nov 2016 - Jan 2017 estimate in figure C.2a, which implies that workers hired at the \$913 threshold had prior weekly salaries that are \$30 lower than predicted given the 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

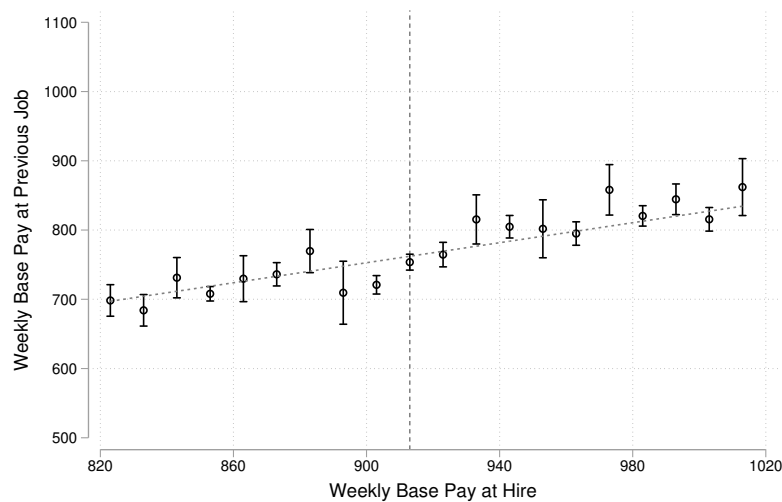
wages of new hires after employment.

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 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 C.1: New Hires' Base Pay at Previous Job Conditional on Current Base Pay, Pre and Post May 2016



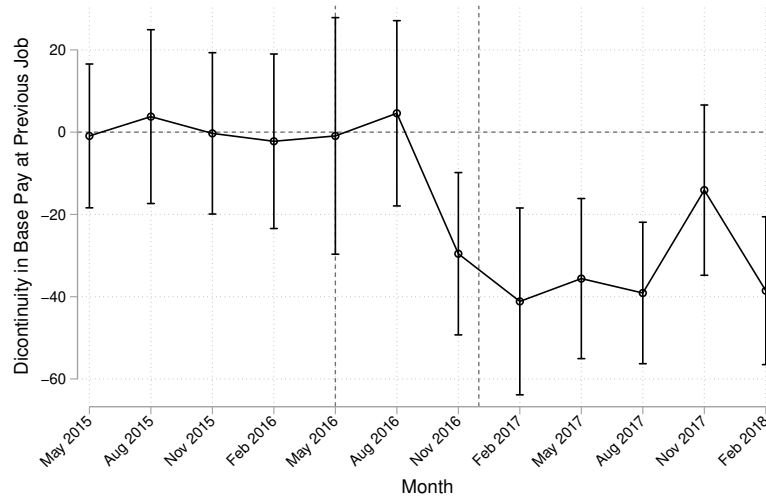
(a) May 2015-2016



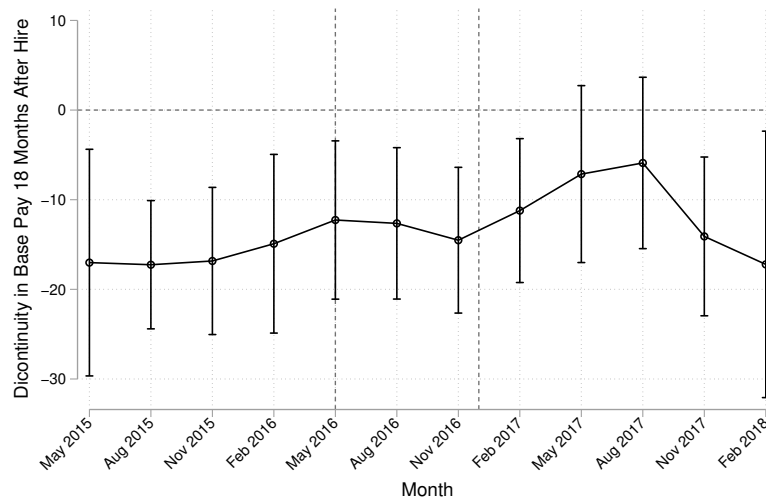
(b) Nov 2016-2017

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 \$10 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 Nov 2016-2017. The fitted line is the predicted values from a linear regression. The vertical line is at \$913 per week.

Appendix Figure C.2: Discontinuity in Outcomes as a Function of Current Base Pay among New Hires Currently Earning \$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 C. Each discontinuity is estimated using 3 months of data, starting with May 2015.

Appendix Table C.1: Discontinuity in Outcomes Among Workers Hired Between \$913-953 per Week

	(1)	(2)	(3)	(4)	(5)	(6)
Base Pay in Previous Job	-29.551*** (10.065)	-39.794*** (11.232)	-21.488 (13.115)			
% Δ Base Pay from Prev. Job	.052*** (.021)	.076*** (.021)	.047 (.031)			
Base Pay After 18 Months				.394 (6.156)	-2.485 (8.897)	-9.134 (14.341)
% Δ Base Pay After 18 Months				.002 (.007)	0 (.01)	-.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 C. The estimates in columns (3)-(6) are computed from a difference-in-discontinuity version of equation C that differences the discontinuity in Nov 2016 - Jan 2017 by the discontinuity in Feb-Apr 2016. The prediction in columns (1) and (3) uses one linear slope for jobs paying between \$600 and \$1200 per week. Columns (2) and (4) 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.