

# The Extent of Downward Nominal Wage Rigidity: Evidence from a Natural Experiment

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## Abstract

This paper studies the repeal of a policy that had raised workers' salaries above their market rates. Leveraging detailed administrative payroll data, I find evidence of wage rigidity whereby employers increase turnover instead of reduce wages back to their pre-policy levels. First, I show that nearly no stayers received a pay cut after the policy was terminated. Second, firms continue to hire new workers at the elevated rate despite no evidence of increased selectivity in hiring. Third, in the long run, firms do not compress the wage growth of incumbents who benefited from the policy. Fourth, incumbents who received an exogenous raise due to the policy are more likely to separate from their employer within a year relative to unaffected workers. Taken together, the results provide new evidence of long-run wage rigidity and its effects on employment dynamics.

**JEL codes: E24, E31, J31**

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# 1 Introduction

The extent to which wages adjust to economic shocks is central for understanding the fluctuation of earnings and employment over the business cycle. In his seminal work, Keynes (1936) theorized that downward wage rigidity inhibits market-clearance during recessions, thereby leading to high unemployment. Among modern economists, the idea that nominal wages cannot adjust downwards continues to be debated as an explanation for the volatility of employment over the business cycle (Shimer, 2004; Hall, 2005; Hall and Milgrom, 2008; Kennan, 2010).<sup>1</sup> Furthermore, the nature of downward nominal wage rigidity has received considerable attention by policymakers concerned with the dynamics of recessions and the ability of inflation targeting to “grease the wheels of the labor market” (Tobin, 1972; Akerlof *et al.*, 1996; Yellen, 2016). Despite its prominence in economic theory and policy, there is no consensus on the degree to which wages are rigid nor its effects on the labor market.

In this paper, I use a natural experiment to investigate the extent of downward nominal wage rigidity and its impact on employment. Previous microeconomic studies have alluded to the existence of wage stickiness by showing that the distribution of annual wage changes among job stayers exhibits significant asymmetry at zero.<sup>2</sup> However, modern theories of wage rigidity argue that the spot wage of job stayers is not the relevant measure of the cost of labor for determining aggregate employment. First, since employment is often a long-term relationship, firms’ employment decisions depend on not only workers’ spot wages, but also the expected future stream of wages (Becker, 1962; Kudlyak, 2014). Second, given that firms can potentially replace incumbent workers with new employees at a lower pay, the key allocative wage in job search models is the wage of new hires, not stayers (Barro, 1977; Pissarides, 2009).<sup>3</sup> To test for stickiness in these additional measures of wage, macroeconomic studies have primarily relied on changes in the unemployment rate over time as a source of variation (see Basu and House (2016) for a review). While the cyclicalities of wages is an intuitive test of wage rigidity, it is often difficult to identify a counterfactual for the present value of wages and to control for changes in the composition of new hires over the business cycle. Furthermore, even after addressing these issues, it would still be unclear whether wage rigidity has any real effects on employment.

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<sup>1</sup>Downward nominal wage rigidity is also a central feature of many models of monetary economics (Benigno and Ricci, 2011) and international macroeconomics (Schmitt-Grohé and Uribe, 2016).

<sup>2</sup>Examples include Card and Hyslop (1996); Kahn (1997); Altonji and Devereux (1999); Barattieri *et al.* (2014); Jardim *et al.* (2019); Grigsby *et al.* (2021). See Elsby and Solon (2019) for a review.

<sup>3</sup>However, a recent argument by Schoefer (2021) finds that the wages of incumbents is allocative if firms face financial constraints.

To overcome prior empirical challenges, my paper leverages a natural experiment from the unexpected termination of a policy that had elevated workers' wages above their market rates. In May 2016, the federal Department of Labor announced that starting December 2016, salaried workers earning less than \$913 per week would be entitled to overtime compensation if they work above 40 hours in a week. Media reports at the time documented that many employers promised raises to their employees in anticipation of the new rule. However, one week before the rule was to go into effect, a federal court ordered an injunction on the policy. In a separate paper on the labor market effects of expanding overtime coverage, I show that firms nevertheless raised some workers' salaries to the \$913 threshold, leading to a bunching mass in the pay distribution (Quach, 2020). For the median worker who got bunched, I found that the raise was equivalent to a 7% increase in their weekly income. In this paper, I analyze the evolution of wages and employment in the months following the repeal of the policy. Given that firms no longer have a policy incentive to bunch workers' salaries, the sudden suspension of the overtime rule offers a unique natural experiment to study how firms behave when they are paying workers above market level.

Analyzing anonymous monthly administrative payroll data covering a tenth of the U.S. labor force, I document four results. First, I show that the spot wages of continuously employed workers is downward rigid. If spot wages are perfectly flexible, then firms would simply reduce workers' salaries back to their pre-policy levels immediately after learning that the reform is no longer binding. Instead, I find that the bunching persisted for over a year and a half after the injunction of the new overtime exemption threshold. The persistent effect of the policy is consistent with previous work by Falk *et al.* (2006), which showed in a laboratory experiment that the temporary introduction of a minimum wage can have long-term effects due to fairness perceptions. In addition, I find little heterogeneity in wage stickiness across workers. Plotting the distribution of annual salary changes for continuously employed bunched workers, I show that nearly no bunched worker received a pay cut within a year of the court ruling. My result is consistent with the conclusion of previous analyses of the distribution of wage changes - firms seldom cut workers' wages (Elsby and Solon, 2019).

Second, I present evidence that employers do not reduce wage growth to adjust the present discounted value of wages downwards. Elsby (2009) argued that even if spot wages are downward rigid, a dynamically optimizing firm would compress future wage increases and thereby mitigate any employment effects. Causally testing this hypothesis is difficult as it requires knowledge of the counterfactual evolution of wages in the absence of wage rigidity.<sup>4</sup> I am able to infer a reasonable counterfactual by using the 2016 rule change to

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<sup>4</sup>As evidence that firms adjust dynamically, Elsby (2009) shows that the distribution of real wage

generate worker-level variation in the degree to which they are paid above their market rate. Applying a difference-in-difference design, I show that the wage growth of incumbent workers who received a raise to the \$913 threshold did not change relative to those who were already earning above that cutoff. In contrast to the hypothesis by Elsby (2009), the analysis suggests that firms do not compress workers' wage growth to offset downward rigidity in spot wages.

Third, I show that entry wages are likewise rigid as firms continue to bunch the salaries of new hires at the \$913 threshold even after the overtime policy was nullified. As recognized in previous studies, fluctuations in wages can be driven by both wage growth and changes in the selection of new hires (Solon *et al.*, 1994).<sup>5</sup> To test whether the bunching is due solely to changes in composition, I use the wages of new hires at their last observed employer as a proxy for productivity. As a counterfactual, I compare workers hired at the threshold to those hired immediately above it. I find that the productivity of bunched hires actually went down following the policy change, indicating that the bunching largely reflects a real wage increase instead of simply positive selection of new hires.

Fourth, having established that employers do not adjust wages downward following the injunction of the new overtime rule, I next show that separation rates increased by 10% (1.7 p.p) among workers earning a higher salary as a result of the policy. Applying a difference-in-difference design with repeated cross-sections, I find that workers who received a raise to exactly the overtime exemption threshold at the time of the court ruling are more likely to be displaced within a year relative to workers who paid well above that cutoff. As a placebo check, I find no reduction in separation rates among jobs paid below the new threshold, indicating that firms did not simply bunch individuals who were likely to leave anyways. The increase in job displacements adds to a growing body of evidence that wage rigidity has allocative effects on employment dynamics. In the context of rural India, Kaur (2019) finds that agricultural employers would rather cut workers than reduce wages in response to negative rain shocks. In developed countries, papers have examined how firms' adjustments

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changes is more compressed during periods of low inflation, when downward wage rigidity is especially binding, than during periods of high inflation. However, the correlation between wage growth and inflation may be confounded by other changes in the labor market over time such as technological change, globalization, or weakening labor market institutions.

<sup>5</sup>Prior studies on the cyclicalities of entry wages have tried to account for changes in the compositions of new hires over the business cycle by matching job-switchers on observables to stayers (Grigsby *et al.*, 2021), focusing on hires from unemployment (Haefke *et al.*, 2013; Gertler *et al.*, 2020), or following the same employer-job over time (Martins *et al.*, 2012; Hazell and Taska, 2019). These studies have found mixed results depending on their data and sample selection. An advantage of the natural experiment in my study is that it provides a clear counterfactual for the wages and composition of new hires absent wage rigidity.

to aggregate demand shocks differ by their baseline propensity to reduce wages (Pischke, 2018; Kurmann and McEntarfer, 2019a; Funk and Kaufmann, 2021). Compared to studies that rely on firm-level variation in pay practices, this paper benefits from exogenous variation in wage rigidity that is generated from a government policy outside firms' direct control.

I use the wage dynamics observed in the study to test various explanations for wage rigidity raised in the literature. First, I find that the persistent bunching in the distribution cannot be explained solely by staggered bargaining, a common mechanism used in macro models to introduce wage rigidity by assuming that salaries are only renegotiated periodically (Taylor, 1979, 1980; Gertler and Trigari, 2009). While prior work has found that workers' base pays tend to change exactly once per year (Grigsby *et al.*, 2019), that alone cannot explain the rigidity in this study as the bunching in base pays endures for at least a year and a half, during which time there is no systematic decline in bunched workers' salaries. Second, the results also cannot be explained solely by implicit contracts. Similar to micro studies on the enduring effect of past labor market conditions on workers' wages, I too find a persistent effect of the initial policy shock (Beaudry and DiNardo, 1991; Schmieder and Von Wachter, 2010). However, it is unclear from an implicit contract perspective why the impact of the policy would also apply to new hires for whom there is no prior expectation of insurance. Instead, the results are suggestive of fairness concerns where workers' reference point depends on both their expectations of a fair annual wage increase (Abeler *et al.*, 2011) as well as the wages of their colleagues (Akerlof and Yellen, 1990; Card *et al.*, 2012; Breza *et al.*, 2017; Dube *et al.*, 2019).

In addition to the literature on wage rigidity, my paper also contributes to the literature on labor market hysteresis (Blanchard and Summers, 1986). Research in this field has found that even after a shock or labor market policy has long passed, its effects may still persist. For instance, Miller (2017) finds that the share of black workers in an establishment regulated by affirmation action requirements continues to grow even after its deregulation, which he argues is due to improved screening methods for new hires. A recent paper by Saez *et al.* (2021) likewise finds persistent increases to youth employment following the reversal of a payroll tax cut for workers aged 26 or younger, which they attribute to a decrease in youth discrimination. In the product market, Benzarti *et al.* (2020) finds that prices respond more to increases in a value added tax than to decreases. Similar to these studies, my paper shows that employers respond asymmetrically to the expansion and retreat of an overtime regulation, and that the effects of the policy persists even after the rule is no longer binding.

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. 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 pay workers 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-collared duties and earn at least the “overtime exemption threshold”. Consequently, firms have incentive to bunch salaried employees’ base pay right at the threshold to exempt them from overtime. 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 to low-income white-collared salaried workers such as many 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.<sup>6</sup> 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.<sup>7</sup>

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

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<sup>6</sup>Specifically, 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.

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

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, stated 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.

Although the \$913 threshold was never legally binding, Quach (2020) showed that between April and December 2016, firms behaved as if the policy went into effect. In particular, he found that employers responded along three margins of adjustment. First, in anticipation of the rule change, employers reduced employment of workers with base pay between \$455 and \$913 per week by reducing hires. Second, employers reclassified one in ten workers within the treated interval from salaried to hourly. Third, and particularly important for my study, firms bunched 5% of affected workers at the \$913 threshold who otherwise would have earned less than that cutoff. Of the workers who got bunched, the median person experienced a \$60 (or 7%) increase in their weekly base pay.

In this paper, I examine the long-run effects of the 2016 FLSA overtime regulation in the months following its injunction. Since the policy initially caused firms to raise workers' salaries above their market rates, one would expect that in a frictionless environment, firms would simply reverse workers' salaries back to their pre-policy levels immediately after December 2016. Any persistence in bunching would therefore be evidence of wage rigidity.<sup>8</sup> 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.

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<sup>8</sup>While persistent effects could also indicate that employers were unaware of the injunction, I find that this is highly unlikely. I show in appendix figure A.1 that the spike in Google searches for the term "FLSA Overtime" on the week of November 20-26 was even larger than the spike in May when the policy was first announced.

### 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' incomes 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.<sup>9</sup>

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, without measurement error, 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 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.<sup>10</sup>

For my main analyses, I create three sub-samples from the data. First, to study the wage rigidity 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 wage rigidity of new hires, I create a sample consisting of all workers hired over the same time frame. 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. Given the size of the ADP dataset, I am able to match 47% of new hires with at least one previous employer. Third, to study the effect of the policy on employment dynamics, I create a sample of repeated monthly cross-sections and identify the workers in each cross-section that remain employed at the same firm after 12 months. In all data samples, I drop workers employed in California, New York, Alaska, and Maine, because these four states have their own overtime exemption thresholds that change after 2016. Moreover, 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.

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<sup>9</sup>However, 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).

<sup>10</sup>For workers paid on a monthly and semi-monthly basis, I first translate the standard rate of pay into an annual salary and then divide by 52.



## 4 Wage Rigidity of Stayers

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 Are Spot Wages Rigid?

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.<sup>11</sup> 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 policy change.

Next, I use the retraction of the \$913 overtime exemption threshold to test for rigidity in spot wages. If spot wages are fully flexible, then employers would decreased workers' salaries back to their pre-policy levels immediately after the injunction of the new threshold. To estimate firms' response to the injunction, I follow a similar identification strategy to Cengiz *et al.* (2019) where I compare the observed density of weekly base pay over time to a counterfactual distribution. However, since the 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 use 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 \$40 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 time

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<sup>11</sup>Given 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.

period two years prior. The identification strategy can only account 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 (see Quach, 2020), for simplicity, I apply a straightforward cross-year comparison to focus on the qualitative persistence of the bunching.

Figure 2 shows the distribution of  $\hat{\beta}_{kt}$  across base pay, by date. As expected, firms bunched the salaries of stayers at the \$913 threshold in December 2016. However, contrary to the prediction implied by perfect wage flexibility, the spike at the nullified threshold does not immediately dissipate in the proceeding months and is clearly visible even one year after the injunction. The persistent bunching indicates that it is either costly for firms to cut wages or they were uncertain of the legality surrounding the new threshold.

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 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 any effect 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, suggesting that the identification strategy uses a reasonable counterfactual for the share of salaried workers earning between \$913 and \$953 per week. Second, the share of salaried workers in that interval rose by nearly 1 p.p by January 2017.<sup>12</sup> 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.

While the persistence in bunching indicates that wage rigidity exists at the aggregate level, 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.<sup>13</sup> To answer

<sup>12</sup>For comparison, only about 2% of workers earned between \$913 and \$953 per week in May 2016. The policy therefore increased the number of workers within this interval by 50%.

<sup>13</sup>For example, Kurmann and McEntarfer (2019b) found that between 2004-2007, about two-thirds of employers exhibited an excess spike at 0 in the distribution of annual wage changes, while

these questions, figure 4 plots the distribution of one-year wage changes for job-stayers who were bunched at the \$913 threshold in December 2016. If wage flexibility is heterogeneous across workers, then some workers bunched at the \$913 threshold would experience a large pay decrease in 2017. In contrast, the figure 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 a decrease in their base pay.

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 finding in appendix figure A.2 where I show that within the sample of continuously employed salaried workers, a disproportionately large share of firms in 2015 changed wages in January over any other month. 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. However, figure 3 exhibits no such kink, implying that while staggered bargaining may exist, it alone does not explain the type of rigidity observed from the policy change.

## 4.2 Is the Discounted Present Value of Wages Rigid?

In this subsection, I test the hypothesis by Elsby (2009) that employers respond to downward nominal spot wages by compressing future wage increases. In contrast to that theory, I show that the wages of bunched workers continue to grow at the same rate relative to multiple counterfactual groups. In effect, workers’ discounted present value of wages is also downward rigid. For simplicity, I will refer to salaried workers that earn between \$913 and \$953 in December 2016 as “bunched workers” even after they no longer earn within that interval.

I model the counterfactual wage growth of bunched workers in the absence of the 2016 FLSA policy by the wage growth of workers who earned between \$953 and \$993 per week

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one-third did not.

on December 2016, henceforth called "non-bunched workers".<sup>14</sup> These non-bunched workers were not directly affected by the policy or its injunction since they are already paid above the threshold. To compare these two groups, I estimate a difference-in-difference regression of the form

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

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  $\tau$  months since December 2016. I control for a bunched worker fixed effect  $\alpha_b$ , and a month fixed effect  $\alpha_t$ .

Figure 5a 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 panel (a) that on average, bunched workers experienced a large one-time increase in their base pay on December 2016.<sup>15</sup> In contrast, workers earning between \$953 and \$993 per week were 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 5b plots the equivalent difference-in-difference estimates computed from equation 2. While the wages of bunched workers grew more slowly than that of non-bunched workers post-injunction, this wage-growth differential was already present before the announcement of the FLSA rule change in May 2016.<sup>16</sup>

In figures 5c and 5d, I show that the persistence in wage growth is robust to alternative selection of the treatment and control groups. First, figure 5c plots the estimates of a difference-in-difference that compares bunched workers in 2016 to similarly defined workers in 2014 (i.e. base pay between \$913 and \$953 in December 2014). An advantage of the cross-year specification is that the treatment and control groups have similar pre-trends prior to the announcement of the rule change in May 2016, making it easier to visually detect any negative trends after the injunction in December. Nevertheless, I find no indication of a negative slope change after the court ruling. Second, figure 5d plots the estimates of equation 2 using a narrowly defined treatment group that excludes individuals who were unaffected by the new overtime policy. Specifically, I partition the set of bunched workers into two groups:

<sup>14</sup>I do not use workers who earned less than the \$913 threshold in December 2016 as a control group as they were also affected by the FLSA rule change.

<sup>15</sup>Given the magnitude of the bunching observed in figure 3, approximately a third of bunched workers would earn less than \$913 per week if not for the policy.

<sup>16</sup>As a placebo check, I repeat a similar analysis using workers earning [913,953) and [953,993) per week in December 2014, two years prior to the rule change. Figure A.3 shows that higher income workers experienced faster wage growth in 2014 as well.

a treatment group comprising of those who earned less than \$913 per week in April 2016 and a control group of those who earned at least \$913. By focusing on workers who are most likely to have received a raise as a result of the FLSA rule change, any response to the policy should be more pronounced than in the previous specifications. However, while the estimate of the initial pay raise in December 2016 is more than double that of the previous regressions, there is still no evidence of compressed future wage increases.

To statistically test whether wage growth changed following the injunction, I calculate the slope of the wage growth pre-announcement, post-announcement but before the injunction, and post-injunction using the following regression:

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

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  $\lambda_{1p}$  therefore represent the difference in average monthly wage increases between the treated and control groups.

Table 1 reports the estimates of equation 3. The estimates in column (1) suggest that the weekly base pay of bunched workers grew \$0.33 (s.e. 0.06) less per month than non-bunched workers prior to the announcement of the policy. The pre-trend is statistically indistinguishable from the negative \$0.35 (s.e. 0.07) wage-growth differential after December 2016. Moreover, the magnitude of the post-injunction wage differential is incredibly small. At a rate of \$0.35 change in weekly salary per month, it would take nearly 4 years for firms to eliminate the raise they had given workers and even longer to equalize the net present value of wages. The persistent in wage growth is robust to a series of alternative specifications. In column (2), I compare workers within the same firm over time and find estimates of a similar magnitude. Following the graphical analysis in figure 5, the third and forth column estimate equation 2 using workers earning between \$913 and \$953 per week in December 2014 as the control group. The last two columns refines the treatment group to only include workers who were not already earning \$913 per week in April 2016 and uses those who were as a control. In all cases, I find no significant decrease in workers' wage growth post-injunction relative to their pre-announcement trend. Overall, the evidence do not support the predictions raised by Elsbey (2009), and suggests that even the discounted present value of wages is rigid.

## 5 Wage Rigidity of New Hires

In this section, I investigate the wage response of new hires following the injunction of the FLSA rule change that was supposed to go into effect on December 1, 2016. As highlighted by Pissarides (2009), it is the wages of new hires that determine aggregate employment in standard job-search and bargaining models, not stayers. Moreover, even if the wages of stayers are rigid due to fairness norms or implicit contracts between employers and employees, it is unclear that the wages of new hires would be bound by the same rules.

### 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.<sup>17</sup> 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 reached its peak in November 2016, the share of workers hired at the threshold was 3 p.p greater than in April 2016.<sup>18</sup> While it diminishes over time, the bunching of entry wages persists to at least November 2017, a year after the injunction of the threshold and five months after the final court decision.

There are two potential explanations for the persistence in the bunching mass of new hires: either firms are paying new hires above their market wage or firms 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 due to the policy. 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 the characteristics of new hires paid right above that cutoff

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<sup>17</sup>Unlike the distribution of stayers, I divide the base pay distribution into increments of \$96.15 to aggregate over more observations per bin.

<sup>18</sup>For comparison, 7.7% of new hires earned within \$96.15 above the threshold in April 2016.

earning between \$953 and \$993 per week. The assumption is that absent the rule change, the productivity of workers hired at and above the cutoff would have evolved similarly over time. Formally, I estimate equation 3 using monthly cross-section of new hires. My primary outcomes are the wages of new hires at their last observed employer and the percent change in base pay from switching jobs. Given that the sample size of new hires is sparse relative to the number of stayer, I aggregate the data into six month intervals centered around the announcement of the new overtime exemption threshold in May 2016.

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 and above \$913 per week 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. The parallel pre-trends leads empirical support for the identifying assumption of the empirical strategy. 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, as a validation check, I find that the productivity of workers hired at the \$913 threshold recovers in the latter half of 2017 when the bunching of new hires has diminished.

To corroborate the claim that the bunching in new hires reflects downward wage rigidity, appendix figure A.4 examines two additional characteristics of new hires over time within the same difference-in-difference framework. First, figure A.4a plots the impact of the FLSA rule change on new hires' salaries and finds that workers hired at the overtime exemption threshold experienced a larger pay increase from leaving their previous job relative to the expected counterfactual absent the rule change. In other words, the estimates suggest that employers are paying entrants more than they would have had the policy not been announced, despite the fact that the rule change was never actually binding. Second, figure A.4b tests whether the share of workers for whom I can observe any previous employment changed following the injunction. Since the data only follows workers across firms if they move between ADP clients, I am unable to observe the previous salary of about half of new hires in my sample. In principle, it is possible that firms are hiring more productive workers in response to the 2016 FLSA policy, but I simply do not observe them because they transferred from non-ADP clients. However, the evidence suggests otherwise as the policy had no significant effect on the probability that a new employee's previous salary is observable.

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

on the which workers are hired in the \$953-993 weekly pay range. While the policy does not directly targeted workers in that interval, there may nevertheless be spillover effects from the policy. To address this issue, figure A.5 uses entrants within a wider income interval to non-parametrically infer the counterfactual productivity of workers hired at the overtime exemption threshold. Prior to the announcement of the FLSA rule change in May 2016, I find a near linear relationship between new hires' salary at their previous job and their current salary. After May 2016, the previous salary of bunched hires appears to fall significantly below the prediction line implied by the other jobs locally within the \$500-1300 per week pay range. Although the visual evidence reinforces the argument that the bunching in new hires' wages cannot be explained solely by compositional changes, I am cautious to over rely on this analysis. The non-parametric approach assumes that absent the rule change, the distribution of pay increases from switching jobs would be locally continuous with respect to workers' new salaries. While that appears to be true prior to May 2016, the overtime policy by construction affected jobs to the left of the \$913 per week threshold. Despite that limitation, it is reassuring that both the difference-in-difference and non-parametric analyses supports the same hypothesis that after the announcement of the FLSA rule change, firms increased the pay of new hires rather than simply hire more productive workers.

## 5.2 Is the Wage Growth of New Hires Rigid?

Next, I investigate whether firms compressed the wage growth of new employees who were initially hired at an elevated rate. 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). If the present discounted value of new hires' wages is flexible, then the salary of hires bunched at \$913 should grow at a slower rate relative to workers hired above that threshold.

To test for rigidity in wage growth, I compare the change in base pay between workers hired at \$913-953 per week to those hired at \$953-993 per week using a difference-in-difference framework. In particular, I estimate equation 2 using the weekly base pay of workers 18 months after their hire date as the outcome variable. For my first specification, I restrict the sample to individuals who stay with the same employer for all 18 months after being hired.

Figure 7b plots the regression estimates over time. To validate the empirical strategy, I find that that the difference in wage growth between entrants hired at and above the



\$913 threshold was constant from May 2015 to October 2016. Starting in November 2016, it appears that the salary of bunched workers 18 months after hire dropped relative to non-bunched hires, and this change persisted for the year afterwards. However, the reduction in wage growth is not only statistically insignificant but also economically small. Even after 18 months, the decrease in base pay is still less than half the initial wage premium implied by figure 7a.

I summarize the empirical evidence on rigidity of entry wages in table 2, and test their robustness to alternative specifications. Column (1) reports the difference-in-difference estimates corresponding to figures 7a and A.4a 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 \$42 (s.e. \$15) less at their last observed job and earned 8.2% (s.e. 3.2%) more from their job transition compared to the set of workers who would have been hired absent the policy change. The analysis thus suggests that firms are hiring less productive individuals to fill jobs bunched at the threshold. The bunching thus represents a true wage increase and not simply compositional changes. In column (2), I show that the negative selection of workers is robust to comparing hires within the same firm, albeit the estimates are less precise. Similarly, the direction of the estimates remain the same in columns (3) and (4) when I restrict the same to workers who left their last observed employer within the past 6 months. In comparison, column (5) finds small significant effects on the wages of bunched hires 18 months after their employment. Column (6) shows that the rigidity in wage growth is robust to coding zero wages for workers who left before 18 months.

## 6 Changes in Employment Dynamics

In this section, I examine whether employment dynamics changed as a result of the proposed increase to the overtime exemption threshold. Given that employers do not adjust wages downward following the injunction of the FLSA rule change, workers who received an exogenous pay increase from the policy may be more sensitive to negative demand shocks compared to if they had never received the raise.

To test whether the sticky wages increased the likelihood that workers lose their jobs, I compare the separation rate of bunched workers to non-bunched workers over time using a sample of repeated cross-sections. I start by plotting the monthly separation rate of these two groups from May 2015 to 2018 in figure 8a. Visually, there appears to be a 3-month drop in the probability of separation starting in December 2016. However, the corresponding difference-

in-difference estimates in figure 8b show that while the decline in monthly separation rates in December 2016 is the largest drop in the 3 years of the sample, its statistical significance is highly dependent on the choice of the reference period. The short-run employment response to the rule change is therefore small relative to usual fluctuations in job displacement rates.

In the medium run, figures 8c and 8d suggest that workers who received an exogenous pay increase are more likely to separate from their employers. In figure 8c, I plot the probability that a worker is no longer employed with the same firm after 12 months. To isolate the workers directly affected by the overtime policy from those who would have earned \$913-953 per week regardless, I restrict the sample of workers in each month to only individuals who just received a pay increase from the month prior. Given the sample restriction, I find that workers who receive a raise to the overtime exemption threshold in November and December 2016 experience a larger increase in separation rates compared to workers whose raised earnings are between \$953 and \$993 per week.

As a falsification test, figure 8d shows that the increase in job displacement is unique to the months when firms were bunching workers' salaries at the overtime exemption threshold in anticipation of the FLSA rule change.<sup>19</sup> Prior to the announcement of the policy in May 2016, annual job separation rates were trending the same between workers at and above the upcoming threshold. Similarly, workers who receive a raise to the threshold after the injunction of the policy are no more likely to separate within a year compared with counterfactual workers, consistent with the previous observation that the majority of workers affected by the policy received a raise in December 2016. Taken together, the precise overlap between the timing of the FLSA rule change and the months in which job displacement among bunched workers highly suggest a causal relationship between the two events.

Table 3 reports estimates of the difference-in-difference analysis and tests their robustness to alternative specifications. In column (1), I report the effect of the FLSA rule change on monthly separation rates using the full sample of workers, analogous to figure 8d. The estimates suggest that job displacements between November 2016 and January 2017 increased between -0.23 p.p to 0.03 p.p, where average separation rates was 1.9% in the year before May 2016. While the estimates are suggestive of a fall in short-run separation rates, column (2) shows that they are not robust to restricting the sample each month to only individuals who just received a pay increase. Overall, the evidence is inconclusive as to the short-run

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<sup>19</sup>Figure 8d controls for firm-date fixed effects to account for large turnover by some employers. Appendix figure A.6 shows that the result is robust to excluding firm-date fixed effects, albeit the estimates are noisier. Alternatively, I show that the separation effect is precisely estimated and fairly similar if I drop firms that experience a 100% annual separation rate for any cohort in the sample.

employment response to wage rigidity. However, the analysis provides a clearer picture for the medium-run effects. Column (3) finds that annual separation rates increased by 0.52-2.88 percentage points, or 3-17% on a baseline rate of 16.6%. Column (4) shows that the results are robust to comparing workers within the same firms.

Similar to the analysis of new hires in section 5, one concern with studying repeated cross-section is that the composition of workers changes over time. For example, appendix figure A.7a shows that the number of workers who received a raise to \$913-953 per week increased drastically in December 2016. As a result, the rise in annual separations among the December 2016 cohort could simply be due to a compositional change whereby employers bunched workers who already have a higher propensity to separate. To explore the effect of the FLSA rule change on worker composition, I use the fact that absent the policy, bunched workers would have earned between \$750 to \$913 per week. If employers bunch workers who already have above average likelihood of leaving the firm, then the separation rate of the remaining workers who did not get bunched should go down. In contrast, appendix figure A.7b shows that the separation rate of workers paid below the nullified overtime exemption threshold did not fall relative to that of workers earning \$953-993 per week. In fact, the estimates reported in the last column of table 3 rules out decreases in separation rates greater than 0.18 p.p from a baseline rate of 18%. The placebo test therefore suggests that the increase in separations is not driven by selection in the type of workers who received a raise to the overtime exemption threshold.

## 7 Discussion and Conclusion

This paper studies the extent of downward nominal wage rigidity by examining firms' response to the retraction of an overtime policy that had raised affected workers' salaries above their market rates. The policy, was was set to go into effect on December 2016, would have granted overtime coverage to all salaried workers earning less than \$913 per week. Although the reform was never binding, employers nevertheless bunched workers' salaries at the anticipated threshold. Consistent with the existence of downward sticky wages, I show that firms do not revert stayers' wages back to their pre-policy levels over time. Firms also did not compress 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 without slowing their wage growth. 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 evidence that the present discounted wages

of both stayers and new hires are both highly rigid. Instead of adjusting wages, the analysis finds that employers increased the separation rate of workers who received an exogenous pay increase from the retracted policy.

To conclude, I relate the wage dynamics observed in this paper to theories of wage rigidity presented in the literature. 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 a 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, implicit contracts alone do not rationalize why new hires are affected by a non-binding policy when their implicit agreement should only depend on the labor market conditions at hire.

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 equity reduces workers' job satisfaction (Card *et al.*, 2012), retention (Dube *et al.*, 2019), and effort (Breza *et al.*, 2018). Survey evidence suggests that these fairness concerns lead employers to abstain from cutting wages since it would also decrease worker productivity (Akerlof and Yellen, 1990; Campbell and Kamlani, 1997; Kaur, 2019).

I present two pieces of evidence that such relative pay concerns can explain the rigidity in wage growth and the rigidity in entry wages. First, if workers' effort is more greatly affected by increases in pay inequality than decreases, then firms would want to give similar pay increases to all workers. As evidence that pay raises are highly correlated within firm, appendix figure A.8 plots the distribution of annual wage increases for bunched workers, relative to the modal wage increase in each workers' employer. I find that over 30% of workers receive the same pay raise as their peers, and they are very unlikely to receive a pay increase that is smaller 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.<sup>20</sup> Second, relative pay concerns would also incentivize employers to pay new hires

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<sup>20</sup>To show that the asymmetry is not simply due to the majority of firms having a modal pay

at the same rate as existing employees. In support of that theory, appendix figures A.9 and A.10 show that the bunching of new hires at the overtime exemption threshold only occurs in firms that are also bunching incumbents. Overall, the empirical evidence is consistent with the argument that horizontal pay equity may drive the wage dynamics observed in this study.

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increase of 0, I find that the distribution is remarkably similar even after dropping such firms.

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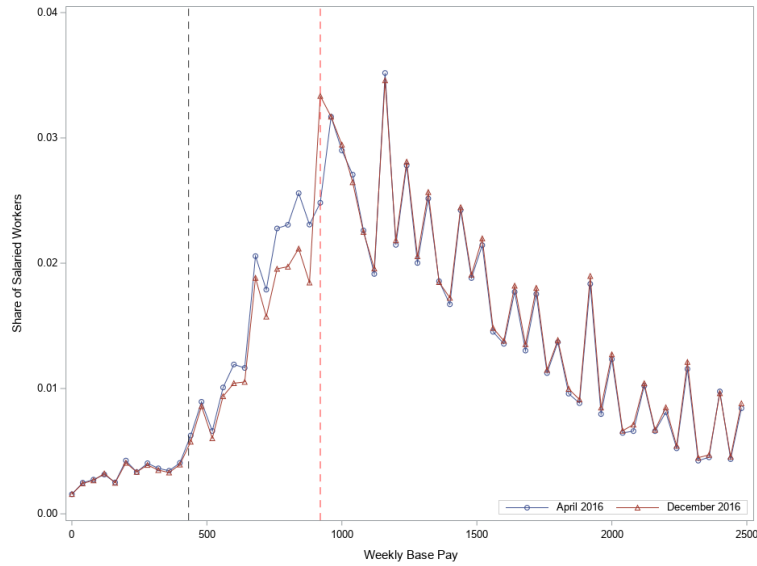
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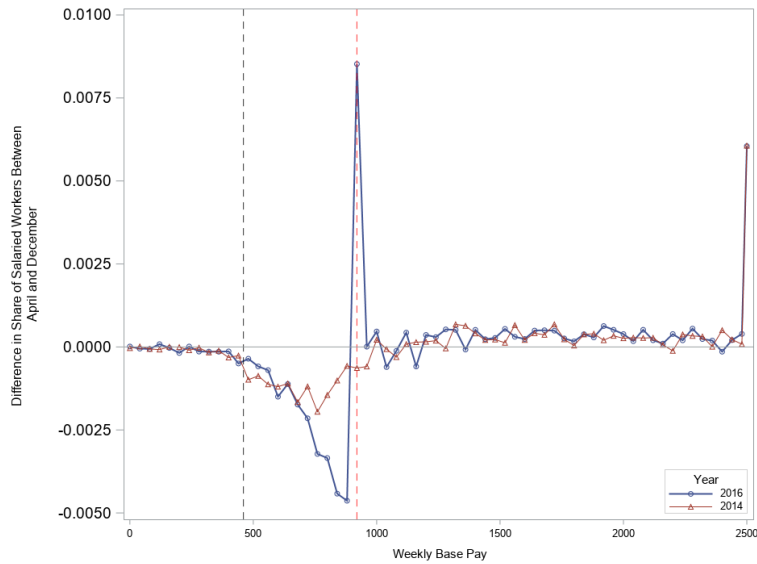
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Figure 1: Change in the Density of Base Pay Between April and December



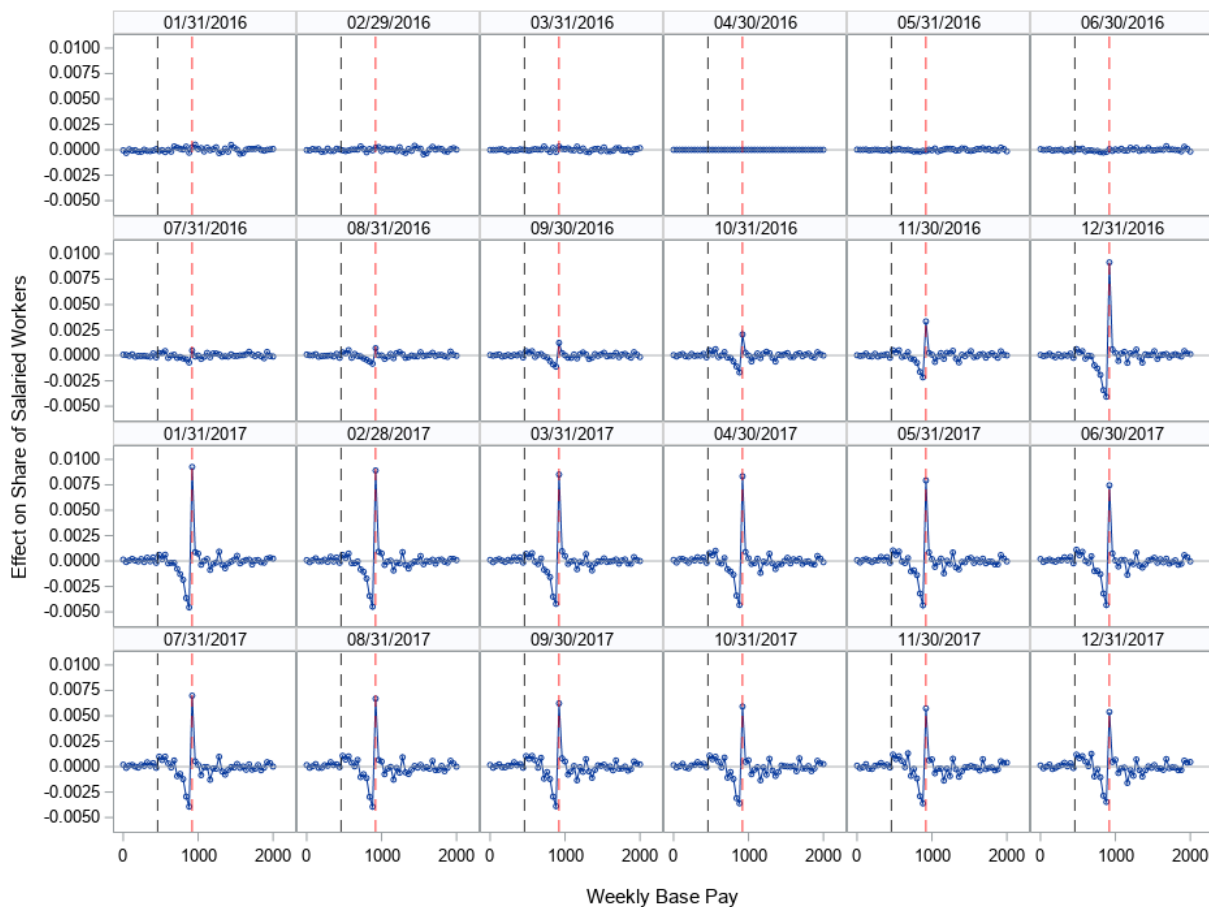
(a) Raw Averages



(b) Difference in Distribution

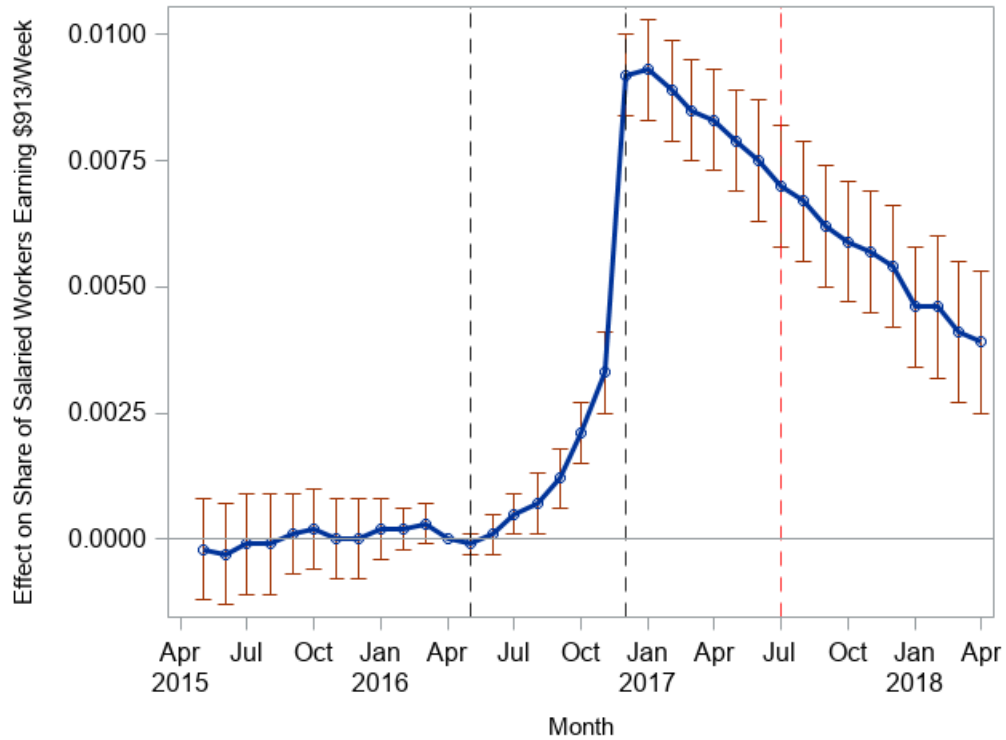
Notes: In panel (a), the blue and red lines show the density of base pay in April and December 2016, respectively. The sample is restricted to salaried workers who are continuously employed at the same firm from May 2015 to April 2018. In panel (b), the blue (red) line shows the difference in the density between April and December of 2016 (2014). The black vertical dashed line is at \$433 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



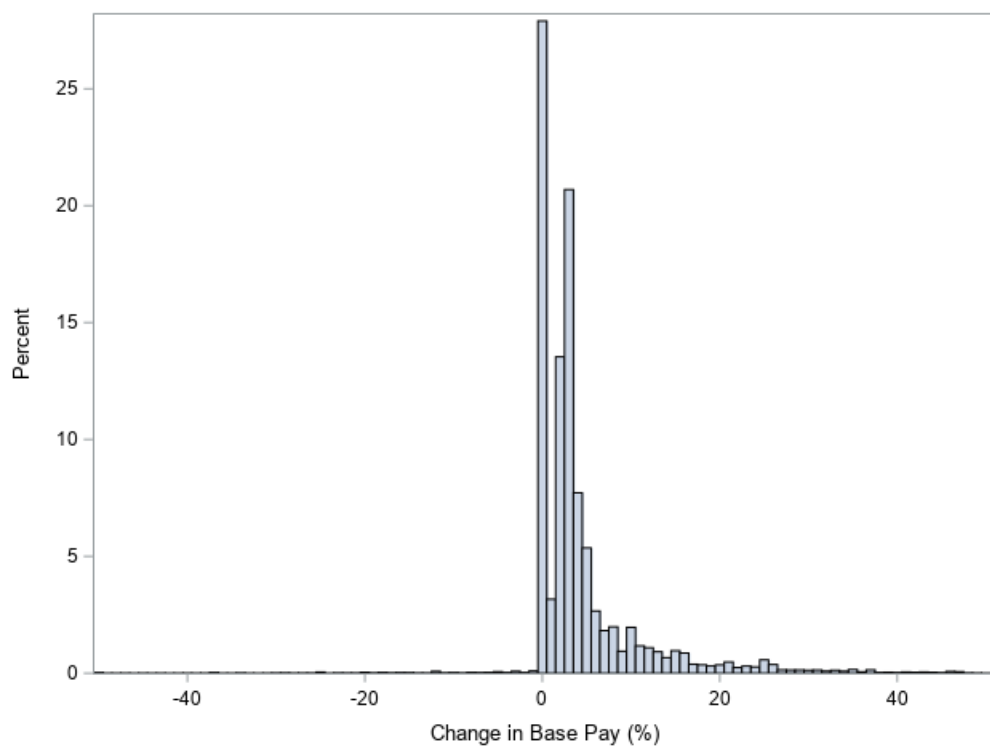
Notes: This figure shows difference-in-difference estimates that compare the changes to the base pay distribution since April 2016 to changes over the same number of months since April 2014. The x-axis is divided into \$40 increments, where the black vertical dashed line is at \$433 and the red vertical 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



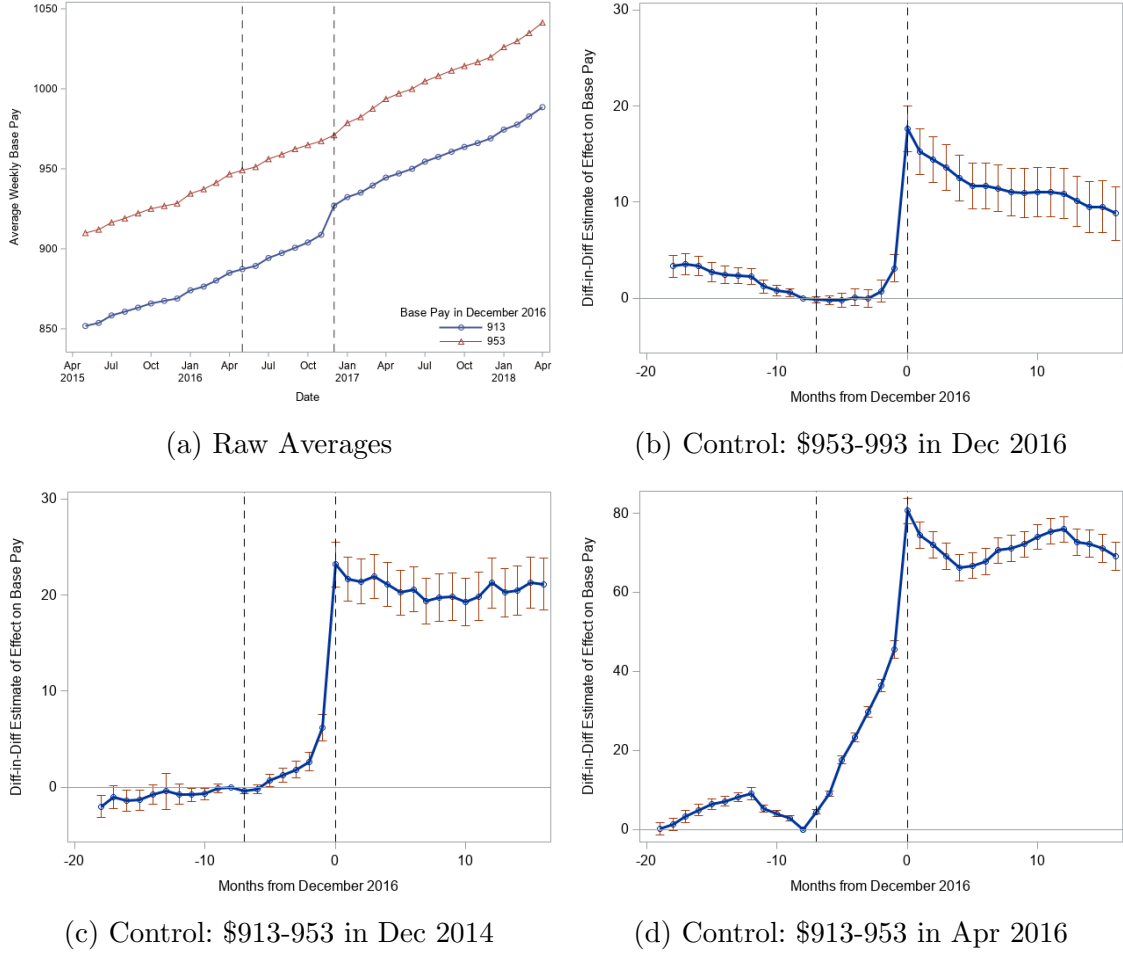
Notes: This figure shows the effect of the 2016 FLSA policy on the share of salaried workers earning between \$913 and \$953 per week, estimated from equation 1. The sample consists of workers who are always salaried, and continuously employed at the same firm between May 2015 and April 2018. The first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



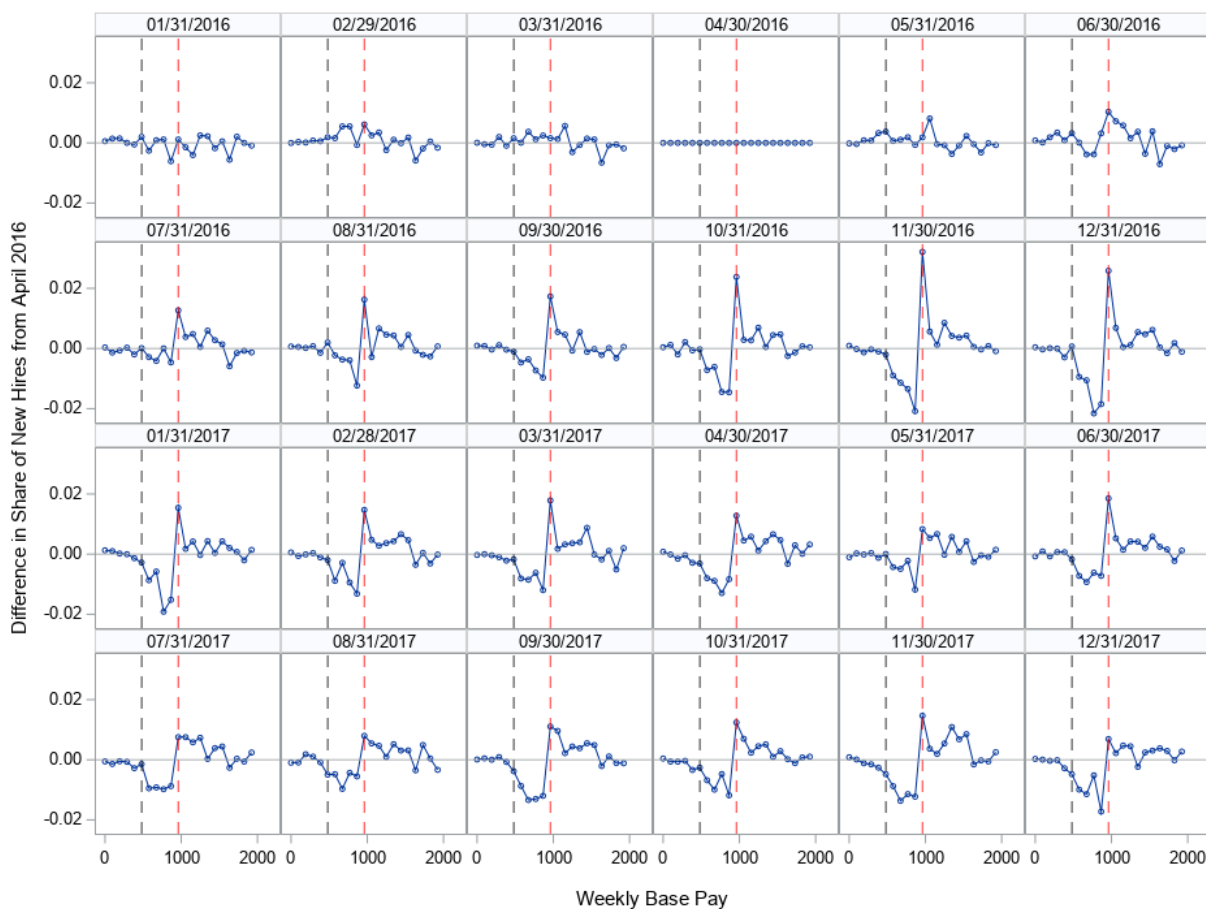
Notes: This figure shows the distribution of the percent change in weekly base pay between December 2016 and 2017. The sample consists of salaried workers who are continuously employed at the same firm from May 2015 to April 2018, and earned between \$913 and \$953 in December 2016.

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



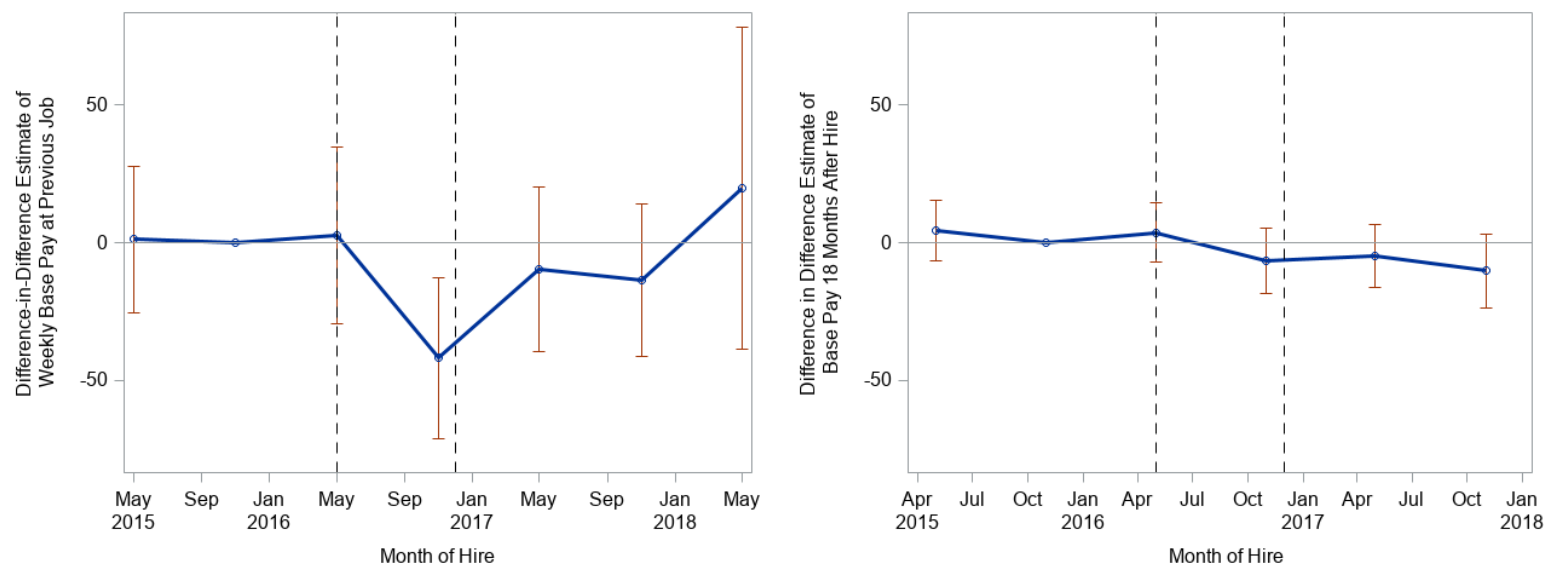
Notes: Panel (a) shows the evolution of weekly base pay over time for salaried workers who earned within [913,953) and [953,993) per week in December 2016. Panel (b) plots the difference-in-difference estimates of the two lines in panel (a), with the treatment group being workers in the [913,953) bin. Panel (c) plots differences-in-difference estimates using the same treatment group but with workers earning \$913-953 in December 2014 as a control. Panel (d) restricts the treatment group to only workers who earned less than \$913 per week in April 2016, and uses those already earning at least that amount as a control.

Figure 6: Distribution of New Hires Over Time Relative to Hire in 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



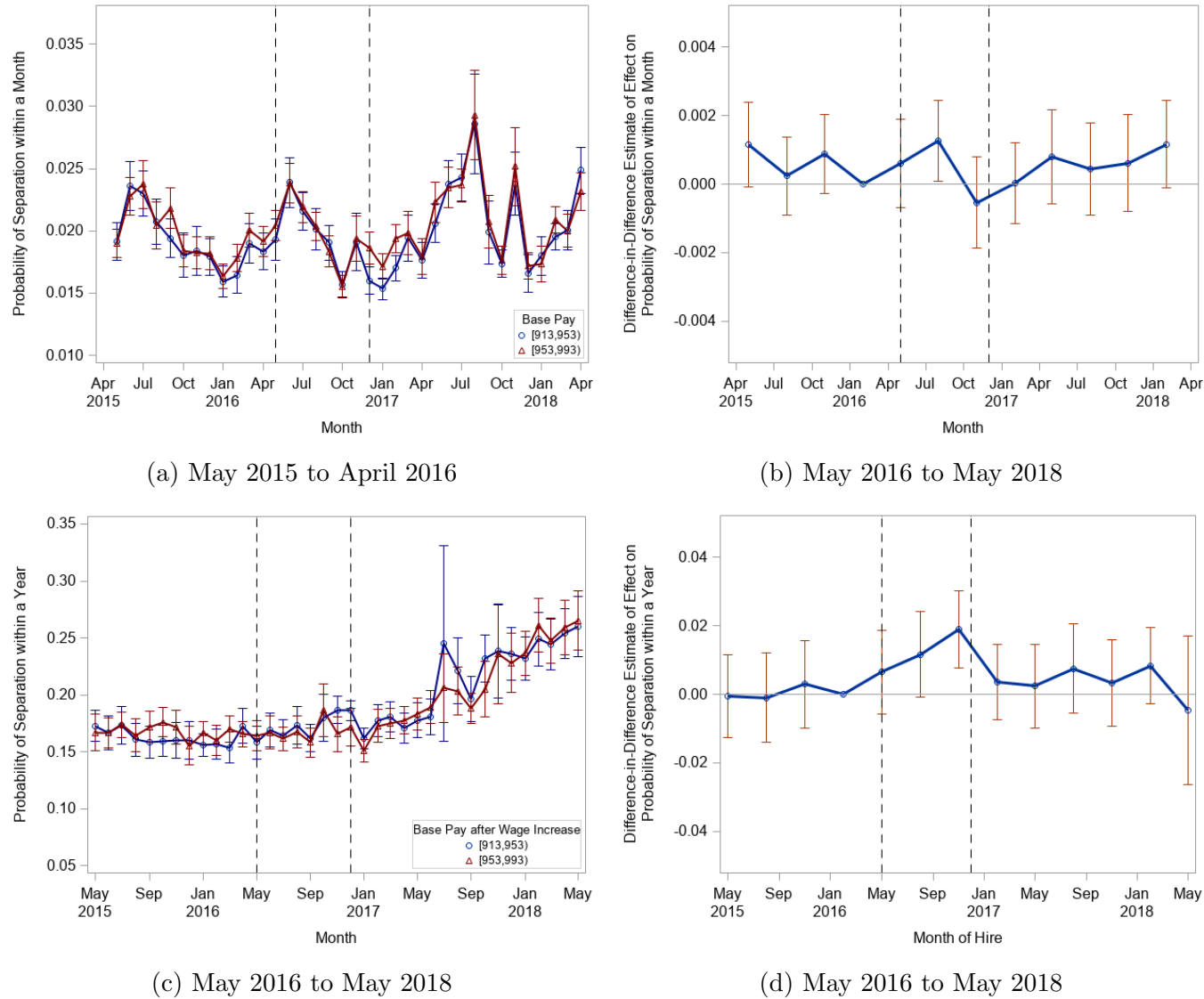
(a) Outcome: Pay at Previous Job

(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: Comparing the Probability of Separation by Workers' Base Pay Using Repeated Cross-sections



Notes: Panel (a) plots the monthly job separation rate between May 2016-2018 for workers earning \$913-953 and \$953-993 per week. Panel (b) plots the equivalent difference-in-difference estimates aggregated at the quarterly level, using workers earning \$953-993 as a control group. Panel (c) plots the annual job separation rate over time, where the sample in each month is restricted to only workers who received a pay increase from the preceding month. Panel (d) plots the difference-in-differences estimates after controlling for firm-date fixed effects. In all panels, the left vertical line is at May 2016 and the right is at December 2016.

Table 1: Effect on Stayers' Base Pays Over Time

	(1)	(2)	(3)	(4)	(5)	(6)
Pre-trend	-0.333 (0.057)	-0.334 (0.045)	0.152 (0.058)	0.023 (0.049)	0.100 (0.0791)	-0.167 (0.068)
Anticipation	1.714 (0.133)	1.234 (0.084)	2.424 (0.1248)	1.701 (0.090)	9.269 (0.1628)	8.590 (0.126)
Post-trend	-0.350 (0.066)	-0.309 (0.029)	-0.0431 (0.0657)	-0.052 (0.032)	0.170 (0.0914)	0.0546 (0.044)
Bunched FE	Y	Y	Y	Y	Y	Y
Date FE	Y	-	Y	-	Y	-
Firm-Date FE	-	Y	-	Y	-	Y
N	3,060,792	3,060,792	2,885,172	2,885,172	1,613,052	1,613,052
Control Group	\$953	\$953	Y2014	Y2014	\$913 Apr2016	\$913 Apr2016

Notes: This table reports the change in the weekly base pay of stayers bunched at the \$913 threshold in December 2016 over three time periods: May 2015 - April 2016 (Row 1); May 2016 - December 2016 (Row 2); and Jan 2016 - April 2017 (Row 3). Columns (1) and (2) compare bunched workers to workers who earn between \$953-993 per week in December 2016. Columns (3) and (4) compare bunched workers in December 2016 to workers who earned \$913-953 per week two years prior in December 2014. Columns (5) and (6) compare bunched workers who earned less than \$913 per week in April 2016 to workers already earning at least \$913 in that month. All workers in the sample are continuous employed throughout the study period. Estimates are computed from equation 3. 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)	(5)	(6)
Base Pay in Previous Job	-41.520** (14.667)	-38.056* (23.045)	-35.757* (18.680)	-52.268 (34.152)		
%Δ Base Pay	0.082** (0.032)	0.053 (0.048)	0.027 (0.037)	0.012 (0.071)		
Base Pay After 18 Months					-6.320 (5.945)	14.625 (16.968)
Bin FE	Y	Y	Y	Y	Y	Y
Date FE	Y	Y	Y	Y	Y	Y
Firm-Date FE	-	Y	-	Y	-	-
N	49,017	49,017	18,375	18,375	79,110	129,605
Sample	Any Past	Any Past	6 Month Past	6 Month Past	Any Post	All

Notes: This table reports estimates from difference-in-difference regressions that compare cross-sections of new hires with a base pay of \$913-953 per week to those with \$953-993. The estimates are 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), and the base pay 18 months after being hired (row 3). Columns (1) and (2) restricts the sample to new hires for whom I observe any past employment. Columns (3) and (4) restrict the sample to only those with employment in the past 6 months. Column (5) keeps all hires that stay employed for at least 18 months. Column (6) keeps all workers and set missing wages to zero. All robust standard errors in parentheses are clustered by firm. \*10%, \*\* 5%, \*\*\* 1% significance level.

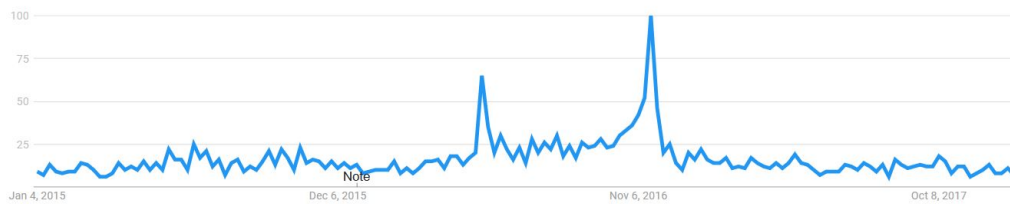
Table 3: Effect on the Probability of Separations

	(1)	(2)	(3)	(4)	(5)
Separations	−0.001 (0.0007)	0.0013 (0.0015)	0.017*** (0.006)	0.019*** (0.006)	0.010 (0.006)
Time frame	1 month	1 month	12 months	12 months	12 months
Baseline	0.019	0.010	0.166	0.166	0.177
Firm-Date FE	-	-	-	-	Y
N	6,193,791	450,877	450,877	450,877	692,147
Sample	All	Wage Changers	Wage Changers	Wage Changers	Placebo

Notes: This table reports the estimates from difference-in-difference regressions that compares cross-sections of workers with a base pay of \$913-953 per week to those with \$953-993. The estimates are for the 3 months starting from November 2016. The outcome variable is an indicator for separating from the employer within the time frame indicator in the second row. Column (1) reports estimates using all workers in the sample. Column (2)-(4) restrict the sample to incumbents who received a wage increase. Column (5) considers individuals with a base pay of \$753-913 as the treatment group, and keeps the same control group of workers with base pays in \$953-993. All robust standard errors in parentheses are clustered by firm. \*10%, \*\* 5%, \*\*\* 1% significance level.

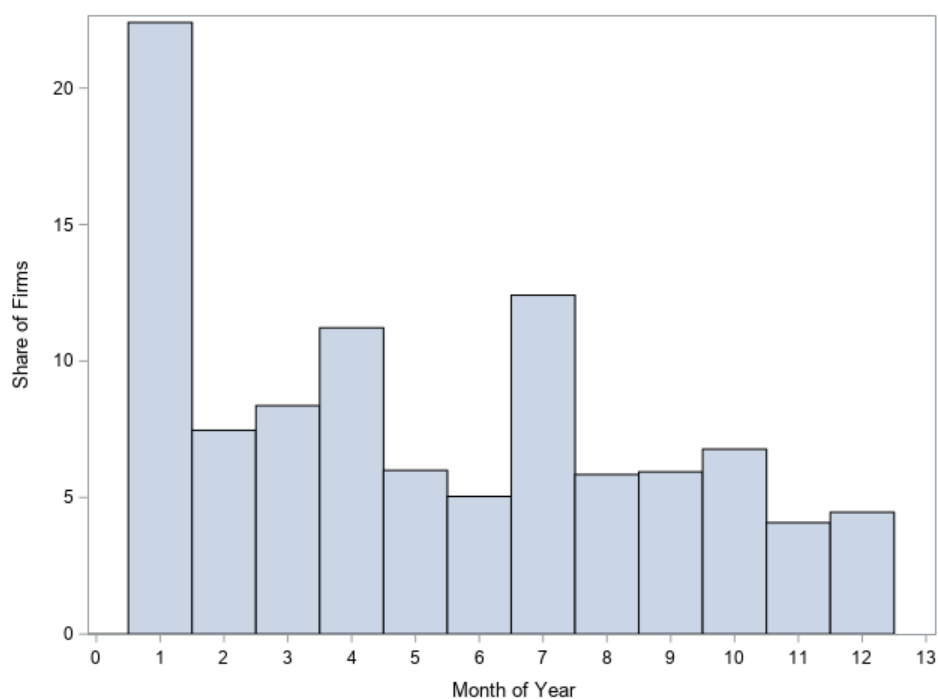
## Appendix A. Supplementary figures and tables noted in the text

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



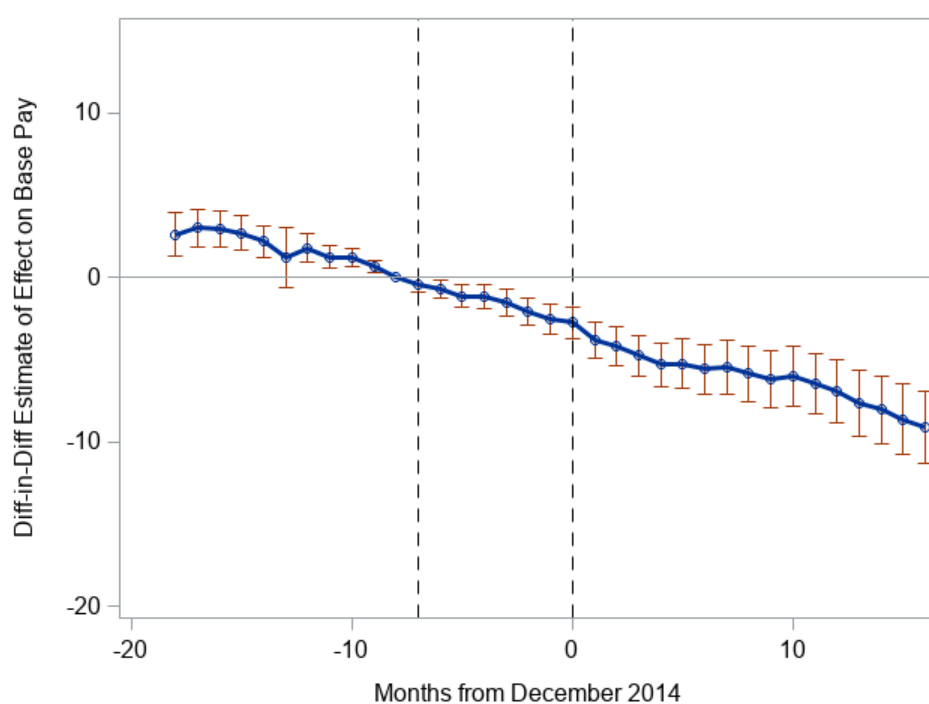
Notes: This figure shows the relative popularity of “FLSA Overtime” as a Google search term between January 2015 and December 2017. A value of 100 indicates its highest popularity level, and the measure of popularity is scaled proportional to this instance.

Appendix Figure A.2: Placebo Test of Difference-in-Difference Estimates of Base Pay Effect



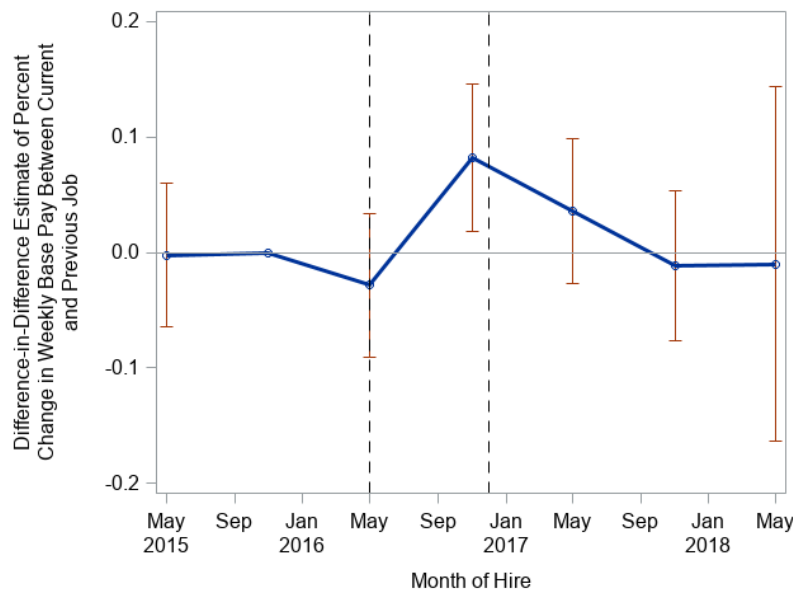
Notes: This figure shows the distribution of firms by the month in 2015 that they adjust the most workers' base pays.

Appendix Figure A.3: Placebo Test of Difference-in-Difference Estimates of Base Pay Effect

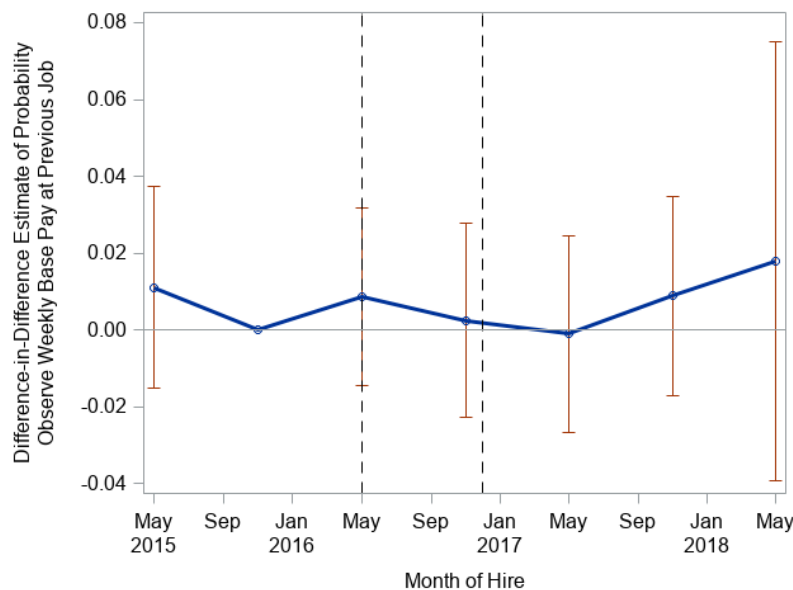


Notes: This figure shows difference-in-difference estimates that compares salaried workers earning within [\$913,953) in December 2014 to those earning [\$953,993). The sample consists of workers who are continuously employed in a salaried position at the same firm between May 2013 and April 2016.

Appendix Figure A.4: Difference in Difference Estimates for New Hires: Probability of Observing Previous Employment and Change in Base Pay from Job Transition



(a) May 2016 to May 2018

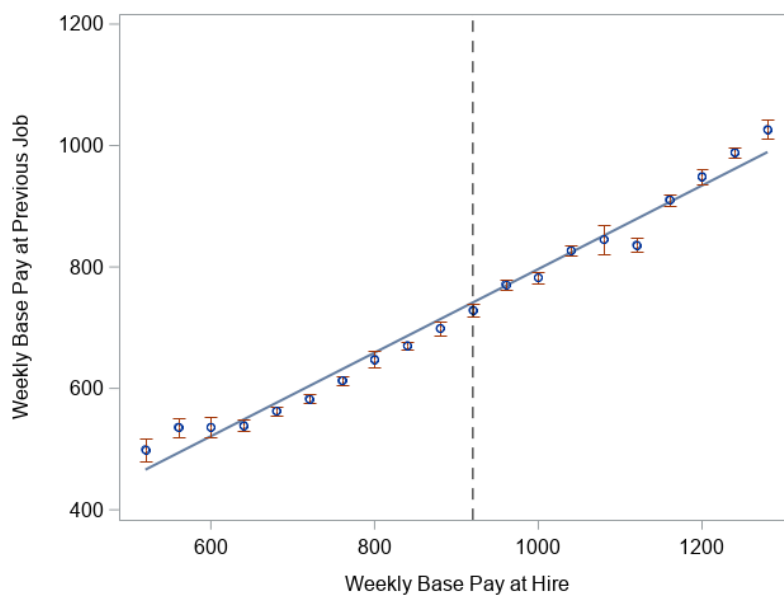


(b) May 2015 to April 2016

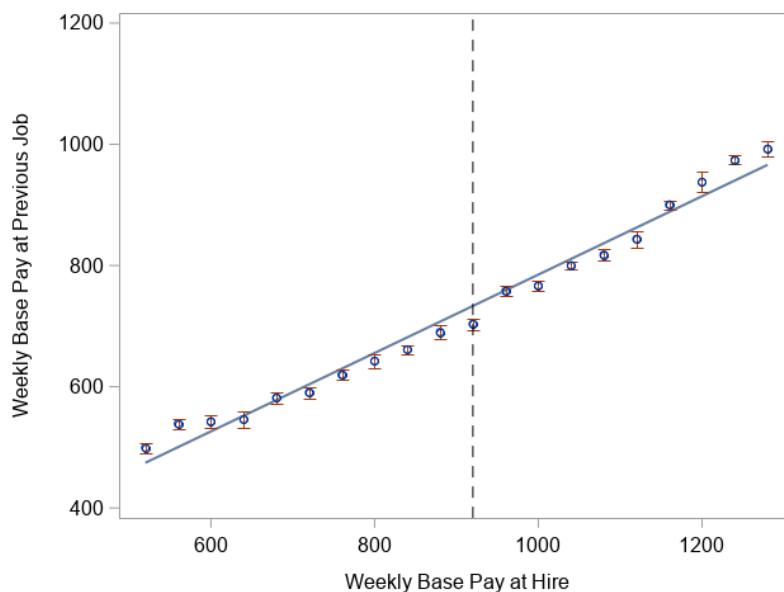
Notes: This figure plots the estimates from a difference-in-difference that compares repeated cross-sections of new hires employed at a base pay of \$913-953 per week to those hired at \$953-993 per week. Each estimate is averaged over 6 months. The outcome in panel (a) is the percent change in base pay from each worker's last observed employer to their current one. The outcome in panel (b) is an indicator for whether the data contains any information on a new hire's previous employer. 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.



Appendix Figure A.5: New Hires' Base Pay at Previous Job Conditional on Current Base Pay, Pre and Post May 2016



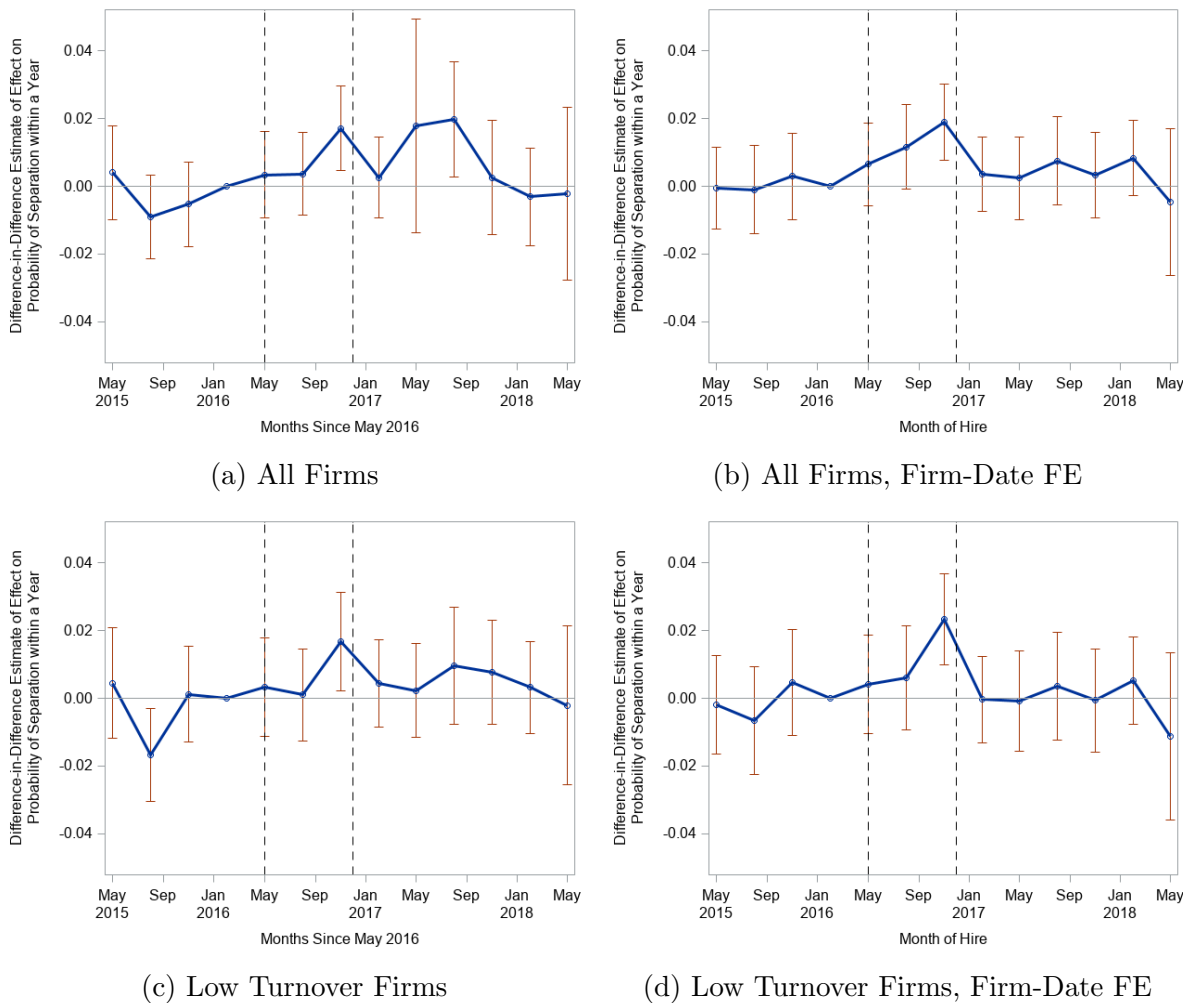
(a) Before May 2016



(b) After May 2016

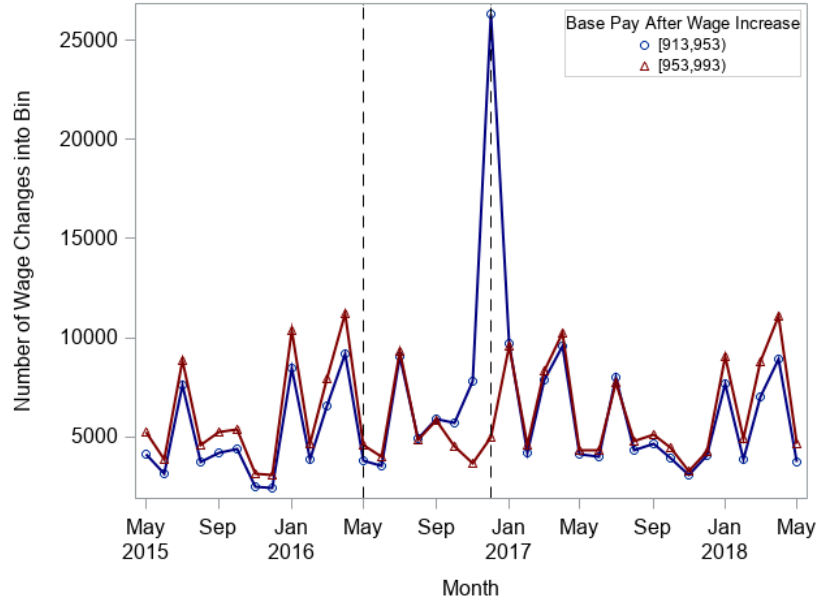
Notes: This figure plots the average base pay of new hires at their last observed employer as a function of their current pay, averaged to \$40 bins. Panel (a) plots the relationship between past and current pay for workers hired between May 2015-2016, and panel (b) repeats the same analysis for workers hired May 2016-2018. The fitted line is the predicted values from a linear regression. The vertical line is at \$913-953 per week.

Appendix Figure A.6: Difference in Difference Estimates for Separations of Wage Changers

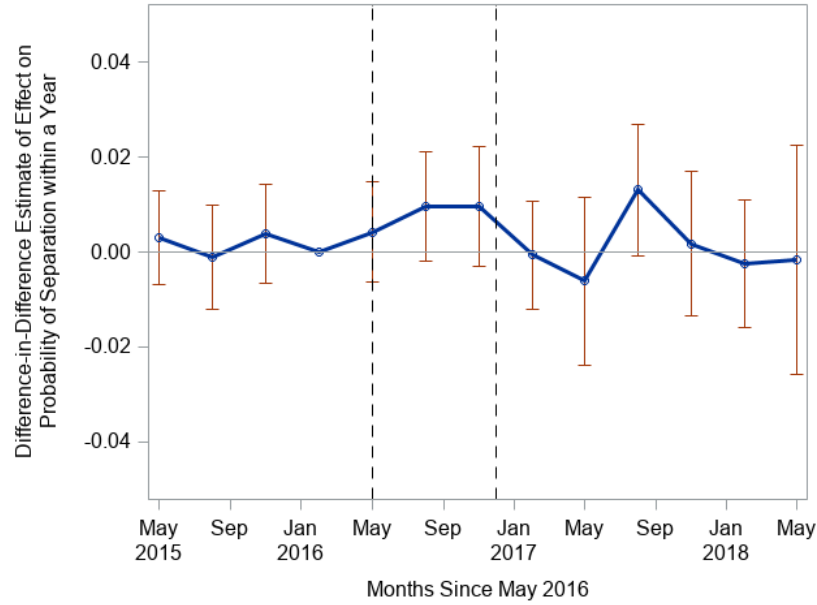


Notes: Panel (a) plots the estimates from a difference-in-difference that compares the annual job separation rate of workers who received a raise to \$913-953 per week relative to those earning \$953-993 per week using repeated cross-sections. Panel (b) plots the estimates from a similar regression that also controls for firm-date fixed effects. Panel (c) and (d) presents analogous figures while dropping any firm that experiences a month in which no worker in that month stays for another year. In all panels, the left vertical line is at May 2016 and the right is at December 2016.

Appendix Figure A.7: Placebo Test for The Selection of Wage Changers



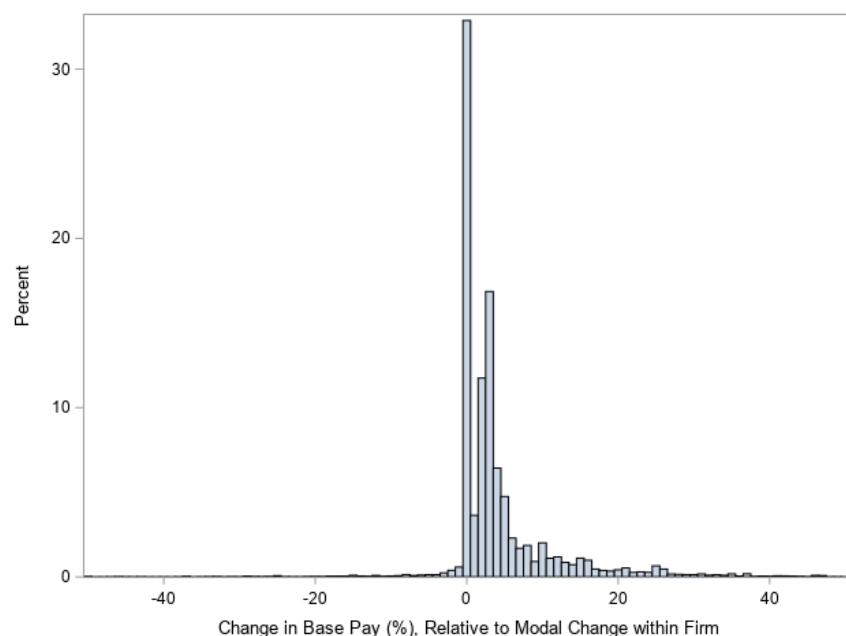
(a) May 2015 to April 2016



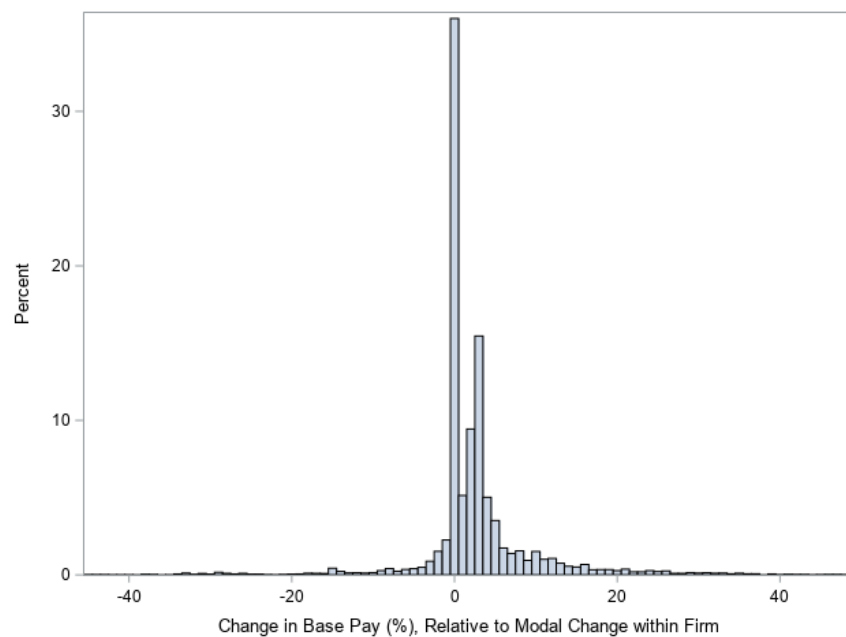
(b) May 2015 to April 2016

Notes: Panel (a) plots the number of workers who receive a wage increase to either \$913-953 or \$953-993 in each month between May 2015 and May 2018. Panel (b) plots the estimates from a difference-in-difference that compares the annual job separation rate of workers who received a raise to \$753-913 per week relative to those earning \$953-993 per week using repeated cross-sections.

Appendix Figure A.8: Change in Base Pay Relative to Modal Change within Firm



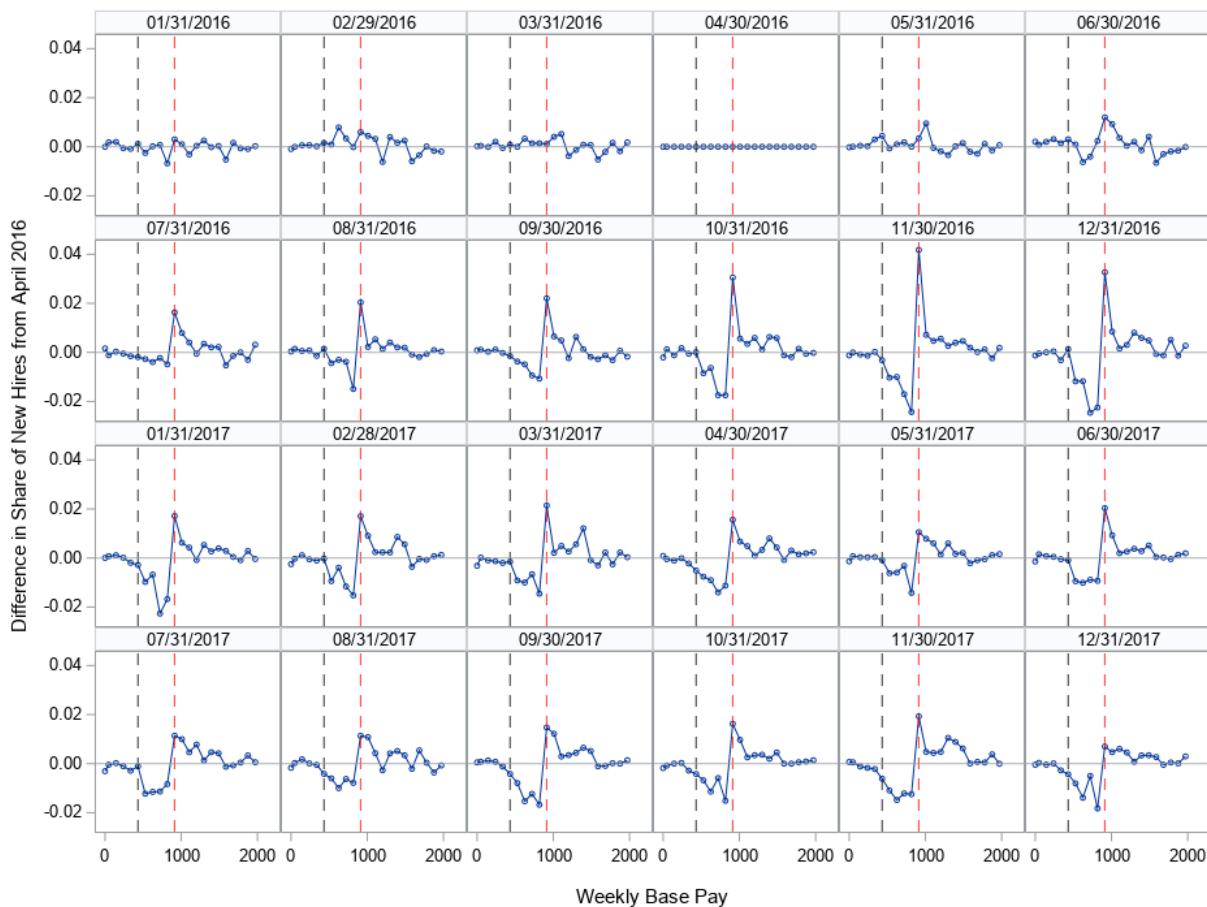
(a) Firm Sample: Minimum 50 Stayers



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

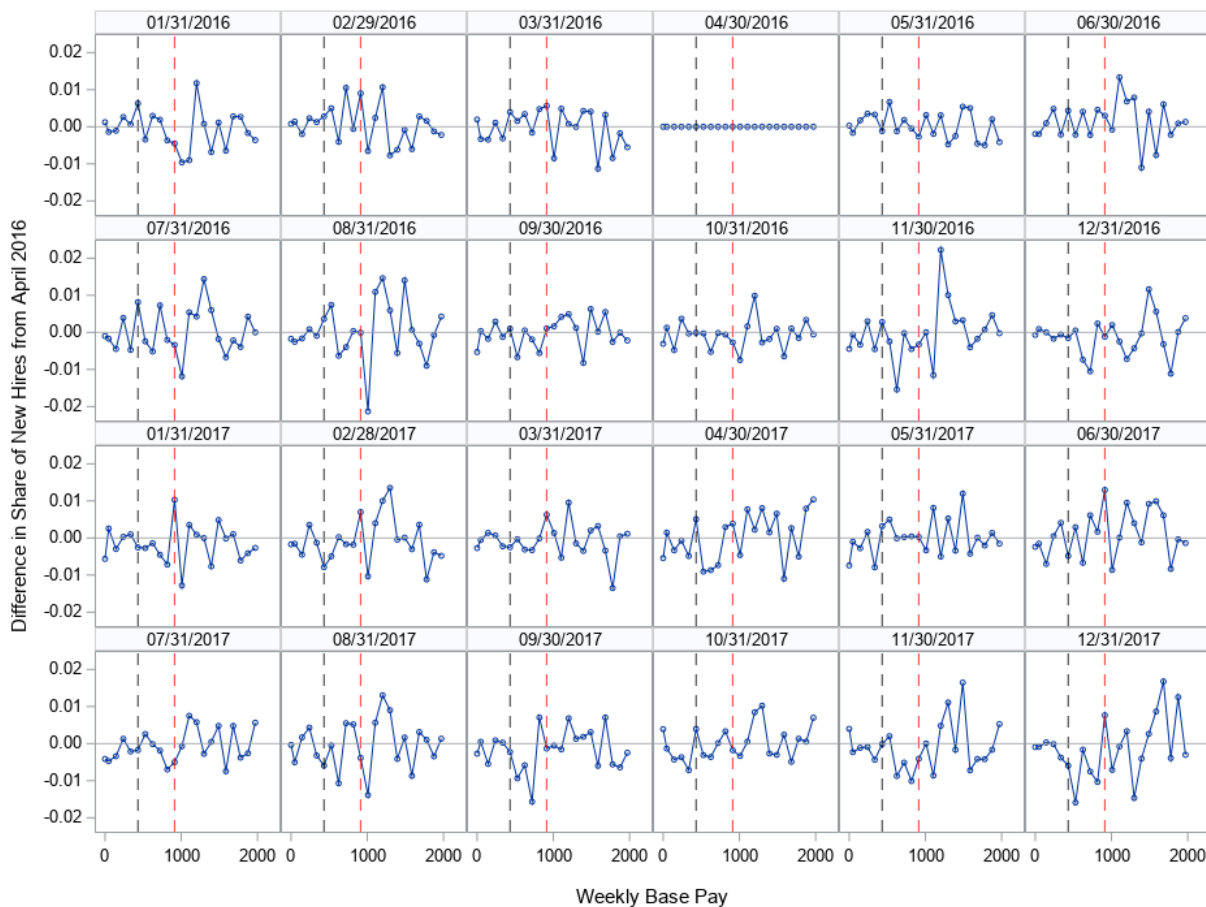
Notes: This figure shows the distribution of the percent change in weekly base pay between December 2016 and 2017, relative to the modal change within each workers' employer. The sample consists of salaried workers who are continuously employed at the same firm from May 2015 to April 2018, and earned between \$913 and \$953 in December 2016. The mode is computed using all salaried workers within each firm. Panel (a) restricts the sample to firms with at least 50 employees. Panel (b) further restricts the sample to firms where the modal wage change is non-zero.

Appendix Figure A.9: Distribution of New Hires Over Time Relative to Hire in April 2016,  
Sample: Bunching Firms



Notes: This figure shows the share of new salaried hires within each \$96.15 increment of weekly base pay between January 2016 and December 2017, 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. The sample is restricted to firms where there is at least 1 salaried worker earning between \$913 and \$953 per week in December 2016.

Appendix Figure A.10: Distribution of New Hires Over Time Relative to Hire in April 2016,  
Sample: Nonbunch Firms



Notes: This figure shows the share of new salaried hires within each \$96.15 increment of weekly base pay between January 2016 and December 2017, 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. The sample is restricted to firms where there are no salaried workers earning between \$913 and \$953 per week in December 2016.