

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

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## **Abstract**

This paper studies employers' wage responses to the unexpected suspension of a labor market reform that would have granted overtime coverage to all salaried employees earning less than \$913 per week. Leveraging detailed monthly administrative data, I find that although the policy was nullified one week before it was supposed to go into effect, employers nevertheless behaved as if it was binding. Firms raised incumbents' base pay right above the overtime eligibility threshold and continued bunching workers at that cutoff even up to two years after the policy was terminated. Employers also did not compress workers' future wage growth to offset the rise in labor costs. Similarly, firms continued to raise new hires' salaries to the threshold, and I show that this bunching of new hires cannot be explained by changes in worker composition. Overall, these results are consistent with the existence of downward nominal wage rigidity among both stayers and new hires.

**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 rigidity in wages inhibits market-clearance during recessions, and leads to high unemployment. Among contemporary economists, the idea that nominal wages cannot adjust downwards continues to be debated as an explanation for unemployment volatility over the business cycle within canonical job-search and matching models (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 both the literature and policy, there is no consensus on the degree of wage rigidity that exists and its effects on the labor market.

In this paper, I investigate the extent to which nominal wages are downward rigid using a natural experiment. Previous work have documented the existence of wage stickiness by showing that the distribution of nominal 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 not only on workers’ present wages, but also the expected future stream of wages (Becker, 1962). Second, given that firms can potentially replace incumbent workers with new employees at lower wages, the key allocative wage in job search models is the wage of new hires, not stayers (Pissarides, 2009). To test for stickiness in these additional concepts of wage, macroeconomic studies have primarily relied on variation in the unemployment rate over time as a source of variation (see Basu and House (2016) for a review). While estimating the cyclicalities of wages to aggregate economic conditions 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 not be clear whether wage rigidity has any real effects on employment.

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<sup>1</sup>Similarly, downward nominal wage rigidity continues to be a central feature of many models of business cycles (Dupraz *et al.*, 2019), 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.

To overcome these empirical challenges, my paper leverages variation from the unexpected termination of a policy that effectively created a wedge between some workers' wages and their market rate absent the policy. 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 (Quach, 2020). For the median worker who got bunched, I found that this raise was equivalent to a 5.8% increase in their weekly income. In this paper, I analyze the evolution of wages and employment in the months following the negation 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 their market wage.

Using detailed anonymous monthly administrative employer-employee matched payroll data, I first show that the spot wages of continuously employed workers is downward rigid. If current wages are perfectly flexible, then firms would simply reduce workers' salaries back to their pre-policy levels immediately after learning that the policy is no longer binding. Instead, I find that the bunching persisted for over two years 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. Plotting the distribution of salary changes for bunched workers who were continuously employed workers between December 2016 and December 2017, I document that nearly no salaried workers who were bunched at the \$913 per week threshold received a pay cut within the proceeding year. This is consistent with the conclusion of previous analyses of the distribution of wage changes in the literature - firms seldom cut workers' wages.

I then present evidence that employers also do not adjust the present discounted value of stayers' wages downwards by decreasing future wage growth. Elsby (2009) argued that even if spot wages are downward rigid, a dynamically optimizing firm would compress future wage increases without significantly affecting employment. Causally testing this hypothesis is difficult as it requires knowledge of the counterfactual evolution of wages in the absence of wage rigidity. As evidence that firms adjust dynamically, Elsby (2009) shows that employers compress the distribution of real wage increases during periods of low inflation, when downward

wage rigidity is more binding, than during periods of high inflation. However, the correlation between wage growth compression and inflation rate may be confounded by other changes in the labor market over time such as technological change, globalization, or weakening labor market institutions. I overcome these confounders by using the 2016 rule change to generate variation in whether a worker is being paid above their market rate. If firms can compress future wages to reduce labor costs, then the salary growth of bunched workers should slow down following the injunction in December 2016. However, using 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), this suggests that firms do not compress workers' wage growth to offset downward rigidity in spot wages.

Next, I show that entry wages are also rigid as firms continued to bunch the salaries of new hires at the \$913 threshold even after the proposed overtime policy was nullified. However, these changes in the wages of new hires can be driven by both real wage growth and changes in the productivity of new employees. To test whether the bunching is due solely to changes in composition, I examine how the gains from switching jobs changed after the announcement of the rule change. If firms are hiring more productive workers, then this should be reflected in the previous salaries of new hires. In the extreme case that the entire bunching is due to positive selection, then new hires at the threshold would not experience any additional pay increase from switching jobs compared to what they would gain absent the policy. Using a difference-in-difference design, I instead find that new hires with weekly salaries at the threshold experienced a larger pay increase from leaving their previous job compared to hires earning immediately above the cutoff. This indicates that employers indeed raised the wages of new hires, and were not simply becoming more selective.

My study contributes to the literature on the rigidity of wages of new hires. Previous studies on the cyclicalities of entry wages have tried to account for changes in the compositions of jobs and 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. My paper uses an alternative source of variation and adds to the more recent evidence that the wages of new hires are fairly rigid. An advantage of my approach is that I also use the policy experiment and long panel structure of the data to directly examine the stickiness of new hires' wage growth. Similar to the wage growth of stayers, I show that the base pay of new employees bunched at the threshold did not grow significantly

slower than that of non-bunched hires or stayers.<sup>3</sup> Another advantage of the policy variation is that I can determine how long it takes for firms to adjust the wages of new hires. While the bunching of stayers persisted for two years after the injunction, I find that firms no longer bunch new hires at the threshold one year after the injunction. This implies that while the wages of new hires are rigid, it is still more flexible than that of stayers.

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 employment effects may still persist. For instance, Miller (2017) finds that the share of black workers in an establishment regulated by affirmative 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.* (2019) 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. My paper presents evidence that wage effects likewise persist after the removal of a labor market policy.

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 6 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 employees 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 above 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.

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<sup>3</sup>This is in contrast to a paper by Kudlyak (2014) who found that the long term cost of new hires, indirectly estimated from worker characteristics and year interacted with date of hire fixed effects, is more procyclical than average wages and entry wages.

On May 18, 2016, the Department of Labor (DOL) announced that it would double the FLSA’s overtime exemption threshold from \$455 per week to \$913 per week (\$47,476 per year), effective December 1, 2016. This rule change would have expanded overtime coverage to all white-collared salaried workers earning less than \$913 per week 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>4</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>5</sup>

Hence, it came as a surprise to employers when the court ordered a preliminary injunction ten days before the date of the rule change that effectively preserved the overtime exemption threshold at \$455 per week. By statute, granting an injunction meant that the judge believed 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, 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

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<sup>4</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 permission to define and adjust their definitions as occupations change 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>5</sup>For example, see *Texas Judge Consolidates Challenges to Overtime Rule* (SHRM Oct. 21, 2016)

and \$913 per week by decreasing 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 a significant share of salaried workers at the \$913 threshold who otherwise would have earned between \$720 and \$913 per week. Of the workers who got bunched, the median person experienced a \$50 (or 5.8%) 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 after December 2016. Any persistence in bunching would therefore be evidence of wage rigidity.<sup>6</sup> Furthermore, since the rule change only directly affected a specific segment of the salary distribution, I am able to use the evolution of workers earning 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 contains detailed payroll information that lets me observe workers' incomes at a monthly frequency between May 2008 and January 2020. 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>7</sup>

Within the data, I observe monthly aggregates of anonymized individual paycheck information including their salaried/hourly status, income, hours, pay frequency, industry, and state of employment. In addition, the data records, without measurement error, each worker's standard rate of pay as of the last paycheck in the month separate from their overtime and gross compensation. 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

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<sup>6</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.

<sup>7</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).

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>8</sup> To be able to compare the standard rate of pay between salaried and hourly jobs, I define hourly workers’ weekly base pay as 40 times their wage.

For my main analyses, I create two subsamples 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. As a placebo group, I append to this dataset the set of continuously employed workers between May 2013 and April 2016. Second, to study the wage rigidity of new hires, I create a sample consisting of all hires into firms that remain in the dataset between May 2015 and April 2018.<sup>9</sup> To address concerns about the selection of new hires due to the 2016 policy, for each worker, I also merge on the job characteristics from their most recent employer that I observe in the data. Given the size of the ADP dataset, I am able to match 47% of new hires with at least one previous employer. For both samples, I drop all workers employed in California, New York, Alaska, and Maine, because these four states have their own overtime exemption thresholds that change after 2016.<sup>10</sup>

## 4 Wage Rigidity of Stayers

In this section, I examine how the wages of workers continuously employed in a salaried position evolve following the injunction of the FLSA overtime policy in December 2016. I organize my analysis through a series of intuitive and testable null hypotheses.

### 4.1 Are Spot Wages Rigid?

First, I test whether firms decreased workers’ salaries back to their pre-policy levels after it became clear that the proposed \$913 threshold was not legally binding.

**Null Hypothesis 1a:** If spot wages are flexible and employers believed that the injunction on December 2016 was permanent, then the bunching would unravel immediately in January 2017.

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<sup>8</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.

<sup>9</sup>I focus on continuously operating firms since they may enter or exit the dataset due to both business formation/creation and changes in continuing firm’s partnership with ADP.

<sup>10</sup>Unlike Quach (2020), I do not drop states that change their minimum wage since this paper which is primarily focused on the bunching at \$913 rather than the shape of the entire income distribution.



To estimate the effect of the injunction on the bunching of workers' salaries, 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 policy that affected all states simultaneously, I am unable to compare treated and untreated states like Cengiz *et al.* (2019) did for the minimum wage. Instead, similar to Quach (2020), I use the distribution in the years prior to the policy change as a counterfactual.

Formally, I model the density of base pay within each firm relative to its density in April 2016 as follows:

$$n_{ikt} = n_{ik, Apr, 2016} + \alpha_{kt} + \beta_{kt} + \varepsilon_{ikt} \quad (1)$$

where the index  $k$  divides the base pay distribution into bins of \$40 increments. The variable  $n_{ikt}$  is the share of salaried workers employed at firm  $i$  with base pay in bin  $k$ , at  $t$  months from April 2016. I denote the counterfactual increase in the share of workers in bin  $k$  at the average firm after  $t$  months by  $\alpha_{kt}$ , and the effect of the 2016 FLSA overtime policy by  $\beta_{kt}$ . To separately identify the causal effect  $\beta_{kt}$  from the counterfactual effect  $\alpha_{kt}$ , I express the density of base pay in 2014 by a similar equation:

$$n_{ik\tau} = n_{ik, Apr, 2014} + \alpha_{k\tau} + \varepsilon_{ik\tau}$$

where the index  $\tau$  refers to the number of months since April 2014.

If  $\alpha_{kt} = \alpha_{k\tau}$ , then I can isolate  $\beta_{kt}$  by simply comparing the change in the density in 2016 to the change in 2014 (i.e.  $\Delta \bar{n}_{k, 2016} - \Delta \bar{n}_{k, 2014}$ ). However, there may be year-specific shocks that make 2014 a poor counterfactual for 2016. To account for these year-specific differences, I scale the 2014 distribution to match the right tail of the 2016 distribution by making the following two assumptions:

$$\beta_{kt} = 0 \text{ if base pay} \geq \$1760$$

$$\alpha_{kt} = \gamma \alpha_{k\tau}$$

The first statement assumes that the policy has no effect on the right tail of the base pay distribution, while the second statement assumes that changes to the distribution of base pay is similar across years up to a scalar transformation. Under these assumptions, I compute the causal effect of the 2016 FLSA policy as

$$\begin{aligned}\hat{\beta}_{kt} &= (\bar{n}_{kt} - \bar{n}_{k, Apr, 2016}) - \hat{\gamma}(\bar{n}_{k\tau} - \bar{n}_{k, Apr, 2014}) \\ &= \Delta\bar{n}_{kt} - \hat{\gamma}\Delta\bar{n}_{k\tau}\end{aligned}\tag{2}$$

where  $\bar{n}_{kt}$  and  $\bar{n}_{k\tau}$  are the average  $n_{ikt}$  and  $n_{ik\tau}$  across all firms, respectively. I estimate  $\hat{\gamma}$  from equation 3 using only bins where the base pay is greater than \$1760.

$$\Delta\bar{n}_{kt} = \gamma\Delta\bar{n}_{k\tau} + \epsilon_{kt}\tag{3}$$

I test the validity of my identification strategy through a series of placebo tests in Quach (2020) where I estimate equation  $\beta_{kt}$  for each year between 2012 and 2015 using the changes in the preceding adjacent year as a counterfactual.<sup>11</sup> However, the approach in this paper differs from that of Quach (2020) in two ways. First, to ensure that the counterfactual density of base pay sums to 1, I use a scalar transformation to construct the counterfactual rather than a linear transformation. Second, to be able to estimate the effect of the FLSA overtime policy at least a year after its injunction, I set the reference date for the control group as April 2014 rather than April 2015. As a simple visual test of my identifying assumptions, I show in appendix figure A.2 that at 9 months past the reference dates, the shape of the counterfactual and actual difference-in-densities are fairly similar to the right of the \$913 threshold.

Figure 1 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 null hypothesis 1a, the spike at the nullified threshold did not immediately disappear in the proceeding months and is clearly visible even one year after the injunction.<sup>12</sup> This is an indication that it is either costly for firms to cut workers' wages, or that firms expected the Department of Labor to win the appeal. Even if the \$913 was going to be reinstated though, it would still be in the firm's interest to reduce workers' salaries in the meantime and bunch them again after the threshold becomes binding. Thus, for firms' expectations about the outcome of the injunction to affect the spot wages of stayers, it must still be the case that there are adjustment costs to modifying workers' wages. Nevertheless, to see if employers' expectations about the future of the policy affected their behavior, I use the final court decision in June 2017 as a discrete point in time that should have adjusted firms'

<sup>11</sup>For instance, the placebo test for 2014 would be  $\hat{\beta}_{kt} = (\bar{n}_{kt} - \bar{n}_{k, Apr, 2014}) - \hat{\gamma}_1(\bar{n}_{k\tau} - \bar{n}_{k, Apr, 2013} - \hat{\gamma}_0)$ .

<sup>12</sup>In appendix figure A.3, I show that the bunching persists until at least December 2018. Since the control group overlaps with the announcement of the new threshold if I try to estimate the effect of the policy past April 2018, figure A.3 instead simply plots the raw difference in the base pay distribution relative to April 2016 using all salaried workers, including hires and separations.

beliefs about the probability of the \$913 threshold being binding to zero.

**Null Hypothesis 1b:** If spot wages are flexible and employers' behavior are affected by the belief that the proposed threshold might be upheld, then the bunching would unravel quicker following the final decision by the courts in June 2017.

In figure 2, I plot the evolution of the bunching at \$913 per week over time (estimated from equation 2) 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. The share of all salaried workers at the cutoff rose by nearly 1 p.p in January 2017 and then shrank at a constant rate afterwards.<sup>13</sup> The absence of any discontinuous change in the share of salaried workers earning \$913 per week after key developments in the proceedings 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. Furthermore, the constant rate of decrease in the spike alleviates concerns that the persistence is a result of time dependence in wage setting where firms only adjust workers' wages once per year. However, 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>14</sup>

**Null Hypothesis 1c:** If there is heterogeneity in wage flexibility, then some workers bunched at the \$913 threshold would experience a salary decrease between Dec 2016 and Dec 2017.

To examine the variation in wage changes across workers, figure 3 plots the distribution of one-year wage changes for job-stayers who were bunched at the \$913 threshold in December 2016. Reviewing the figure from left to right, the figure shows that very few workers experienced a wage cut, about 27% of workers had no change to their base pay in the year following the injunction, and the majority of workers 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 pay cuts.

<sup>13</sup>For comparison, in May 2016, about 2% of workers in the sample earned between \$913 and \$953 per week. The policy therefore increased the number of workers within this interval by 50%.

<sup>14</sup>For example, Kurmann and McEntarfer (2019) found that between 2004-2007, about two-thirds of employers exhibited an excess spike at 0 in the distribution of annual wage changes, while one-third did not.

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

In this subsection, I show that in addition to not cutting bunched workers' salaries after the injunction of the 2016 FLSA policy, firms also did not slow down their wage growth. 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.

**Null Hypothesis 2:** If the present discounted value of wages is flexible, then workers earning \$913 in December 2016 would experience slower wage growth after the injunction.

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 on December 2016, henceforth called "non-bunched workers".<sup>15</sup> These non-bunched workers were not directly affected by the policy or its injunction since they were already paid above the threshold. To compare these two groups, I estimate a difference-in-difference regression of the form

$$y_{it} = \sum_{\tau \neq -8}^{16} \beta_{\tau} D_{b=1,t=\tau} + \alpha_b + \alpha_t + \varepsilon_{it} \quad (4)$$

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 4 plots the raw evolution of bunched and non-bunched workers' salaries over time, as well as their difference-in-difference estimates. 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. 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, I plot the difference-in-difference estimates in panel (b). 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>

<sup>15</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>16</sup>To verify that firms did not reduce bunched workers' wage growth in anticipation of the announcement, I repeat a similar analysis using workers earning [913,953) and [953,993) per week in December 2014 as a placebo test. Figure A.4 shows that higher income workers experienced

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:

$$y_{it} = \sum_{p=1}^3 (\lambda_{0p} + \lambda_{1p} \cdot time) D_{bp} + \alpha_b + \alpha_t + \varepsilon_{it} \quad (5)$$

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 months after December 2016. The estimates of  $\lambda_{1p}$  reported in the top left of figure 4b imply that prior to the announcement of the policy, the weekly base pay of bunched workers grew at \$0.33 (s.e. 0.04) less per month than non-bunched workers. This is statistically indistinguishable from the negative \$0.35 (s.e. 0.03) wage-growth differential after December 2016. Moreover, the magnitude of the wage differential is incredibly small. At a rate of \$0.35 change in weekly salary per month, it would take over 4 years for firms to eliminate the raise they had given workers and even longer to equalize the net present value of wages.

As an alternative test, I also estimate equation 4 using workers earning between \$913 and \$953 per week in December 2014 as the control group. In this case, I define the months as relative to December 2016 and December 2014 for the treatment and control groups, respectively. Figure A.5 shows that not only do these two groups have similar pre-trends, but they also had similar wage growth after December of their respective reference years. Overall, the evidence is consistent with wage rigidity in the discounted present value of stayers' wages.

## 5 Wage Rigidity of New Hires

In this section, I investigate how the wages of new hires respond after the injunction of the FLSA rule change that was supposed to go into effect on December 1, 2016. As discussed in Pissarides (2009), within standard job-search and bargaining models, it is the wage of new hires that determines aggregate employment, not stayers. Moreover, even if the wages of stayers are rigid due to fairness norms or implicit contracts between the employer and employee to insure against negative shocks, it is not clear that the wages of new hires would be bound by the same rules.

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faster wage growth in 2014 as well.

## 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.

**Null Hypothesis 3a:** If the wages of new hires is flexible and firms did not change the type of workers hired in response to the 2016 FLSA proposal, then there should be no bunching in the distribution of new hires after the injunction.

Figure 5 plots the base pay distribution of hires 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 bunching of new hires then reached its peak in November 2016, the month before the rule change was supposed to go into effect. In November, 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 ruling. There are two potential explanations for this persistence: either firms are paying new hires above their market wage, or firms are simply hiring more productive workers. 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.

**Null Hypothesis 3b:** If the bunching of new hires is fully explained by selection on worker productivity, then new hires earning \$913/week should have the same salary at their previous job as workers who would have been hired at that rate even absent the policy. In that case, bunched hires would not experience any additional pay increase from switching jobs relative to the counterfactual hire.

To test this, I compare new hires at the \$913 threshold to new hires paid around that cutoff. The assumption is that absent the rule change, the distribution of pay increases from switching jobs would be locally continuous with respect to workers' new salaries. I verify this in figure 6 where I plot the percent change in new hires' base pay relative to the pay at their last observed job, conditional on the pay at their new employer. For workers hired prior to the policy announcement in April 2016, I find that the change in base pay from switching jobs is continuous across the \$913 cutoff. However, after the announcement of the FLSA rule change, workers hired at the \$913 threshold experienced a discontinuously larger increase

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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.

in base pay from switching jobs relative to workers hired above or below that threshold. Assuming that the distribution of pay increases would have remained continuous absent the policy, the discontinuity supports the alternative hypothesis that firms actually raised the pay of new hires, and were not simply hiring more productive workers.<sup>19</sup>

I present an additional test in appendix figure A.7 to show that the discontinuity in the percent change in base pay is not driven by selection into the sample. Since the ADP dataset only follows workers who 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. In that case, I would expect a negative discontinuity in the probability that I observe a new hires' past wages at the \$913 threshold. Instead, appendix figure A.7 shows that the probability that a new employee's previous salary is observable within the data is not statistically different between bunched hires and those paid above the bunching cutoff. This implies that sample selection is not driving the observed increase in the wages of job-switchers paid at the \$913 threshold.

Next, I investigate how long the increase in new hires' wages persists after the injunction of the FLSA rule change. To understand the persistence of rigidity in entry wages, I compare the change in base pay between job-switchers hired at \$913-953 per week to those hired at \$953-993 per week within a difference-in-difference framework:

$$\% \Delta y_{it} = \sum_{\substack{\tau=-3 \\ \tau \neq -1}}^4 \beta_{\tau} D_{s=1,\tau} + \alpha_s + \alpha_t + \varepsilon_i \quad (6)$$

Let  $\% \Delta y_{it}$  be the percent change in new hire  $i$ 's base pay relative to the base pay at their previous job. I classify workers by their initial salary and the date that they were hired. If a new employee's initial base pay was within \$913-953 per week (\$953-\$993 per week), then I consider the worker as part of the treatment (control) group.<sup>20</sup> For statistical power, I group the date of hire into 6 month intervals denoted by  $\tau$ . The dummy variable  $D_{s=1,\tau}$  equals 1 if worker  $i$  is in the treatment group and hired  $\tau$  months from May 2016. The regression controls for treatment/control group fixed effects ( $\alpha_s$ ) and month of hire fixed effects ( $\alpha_t$ ). My identifying assumption is that absent the policy, firms are not changing the types of workers they hire at the threshold relative to hires above the threshold. If this holds, then

<sup>19</sup>Similarly, if I plot workers' previous pay with respect to their hiring wage, I find no discontinuity prior to the policy announcement but a downward discontinuity at \$913 after the policy (see appendix figure A.6).

<sup>20</sup>I do not use workers earning less than \$913 as a control group because those workers were also affected by the 2016 FLSA policy.

$\beta_\tau$  represents the effect of the rule change on the salaries of new hires, which I argued above is due to a real wage effect and not solely the hiring of more productive workers.

I plot the estimates of  $\beta_\tau$  over time in figure 7. I would like to highlight three features from the graph. First, the pay increase of new hires in the treated and control groups followed a similar trend prior to the announcement of the new threshold in May 2016. This parallel pre-trend gives credibility to the identification assumption that these two groups would have continued to evolve similarly absent the policy. Second, the percent change in base pay from switching jobs was 6.7 p.p (s.e. 2.7 p.p) larger for workers hired between November 2016 and April 2017, relative to the counterfactual. This is consistent with the earlier evidence that firms raised the salaries of new hires. Third, while this elevated raise decreases over time, new hires between May and October 2017 still experienced a 4.3 p.p (s.e. 2.7 p.p) larger gain from switching jobs. It is only starting in November 2017 that I see new hires' base wages return to the level it would be if the policy never occurred. This is consistent with the bunching observed in figure 5 and implies that the entry wages of new hires were sticky for a full year after the injunction of policy. It is worth noting that while firms stopped bunching new hires a year after the injunction, I found in section 4 that it took two years for employers to stop bunching incumbents. This suggests that the while entry wages are rigid, they are still more flexible than the wages of incumbents.

## 5.2 Rigid Wage Growth of Hires

While firms continued to pay new hires an elevated wage even after the 2016 FLSA policy was terminated, they could have compensated 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.

**Null Hypothesis 4:** 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 whether the discounted present value of new hires' wages is rigid, I compare the evolution of bunched hires' salaries over time to that of workers who initially earned between \$953 and \$993 per week when they were hired. Formally, I estimate the following regression:

$$y_{it} = \sum_{\tau \neq 0}^{18} \beta_\tau D_{s=1, t=\tau} + \alpha_s + \alpha_t + \varepsilon_{it} \quad (7)$$



where  $y_{it}$  is the base pay of worker  $i$ , after  $t$  months at their new job. I control for a tenure fixed effect ( $\alpha_t$ ), and for whether or not the worker’s entry wage was bunched at the threshold ( $\alpha_s$ ). I restrict the sample to workers who were hired between November 2016 and April 2017, and remain employed at the same firm for at least 16 months afterwards. The coefficient  $\beta_\tau$  represents the difference in base pay between bunched and non-bunched hires,  $\tau$  months after the date of hire relative to their initial hire date.<sup>21</sup> The identifying assumption is that absent the policy, the base pay of bunched hires would have grown at a similar rate to the base pay of non-bunched workers.

I plot the estimates of  $\beta_\tau$  in figure 8. I find that the base pay of bunched workers evolves similarly to the base pay of non-bunched workers for the first 5 months, and then slows down afterwards. However, in no month is the difference in the change in base pay between the two groups statistically significant. I report the estimate at 16 months after the hire date in column (1) of table 1. As a robustness check, I also interact the fixed effects with workers’ date of hire in column (2). In column (3), instead of comparing workers hired at the threshold to workers hired above the threshold, I compare them to incumbent workers already earning \$913 per week. In all cases, I find that bunched hires earn less after 16 months, but the difference is statistically insignificant. Even at the 95% confidence level, my main specification in column (1) rules out negative effects on bunched hires’ wage growth greater than \$12.78 over 16 months. A back of the envelop calculation suggests that at this rate, it would take at least 4.5 years for the wages of bunched hires to converge back to its counterfactual level, and even longer for firms to recuperate the cost of initially paying workers the elevated wage.<sup>22</sup> Taken together, the evidence supports the argument that firms cannot easily adjust the wage growth of new hires.

## 6 Discussion and Conclusion

This paper tests for downward nominal wage rigidity by examining firms’ response to the retraction of a change to the FLSA overtime exemption threshold that was set to go into effect on December 1, 2016. Although the rule change was never binding, employers nevertheless bunched workers’ salaries at the anticipated threshold, above their market rates. Consistent

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<sup>21</sup>While equation 6 compares cross-section of new hires over time, equation 7 follows the same new hires over time.

<sup>22</sup>I calculated the number of years to convergence by assuming that the base pay of bunched hires grows slower at a rate of \$12.78 every 16 months. Extrapolating from the earlier result that these hires experienced a 6.7 p.p increase in their entry wage, I infer that they would have earned \$855 (i.e.  $\frac{913}{1.067}$ ) per week if they did not get bunched. Instead, they earn \$913 per week, so for their pay to converge back to the level it would be absent the policy, it would take 4.5 years (i.e.  $\frac{913-855}{12.78}$ ).

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, I find that firms continued to bunch the salaries of new hires at the nullified threshold, and did not slow down their wage growth either. Comparing the previous wages of bunched and non-bunched hires, I show that the bunching cannot be explained solely by selection on worker productivity, and reflects a real increase in entry wages. Taken together, these results provide evidence that the present discounted wages of both stayers and new hires are both highly rigid.

Frictions in adjusting wages to shocks and policies in the labor market has important implications for unemployment over the business cycle. In subsequent drafts of this paper, I plan use firm-level variation in the share of workers exposed to the bunching to identify the effect of wage rigidity on employment. In addition, I plan to connect the empirical findings to theories of the underlying causes of wage rigidity.

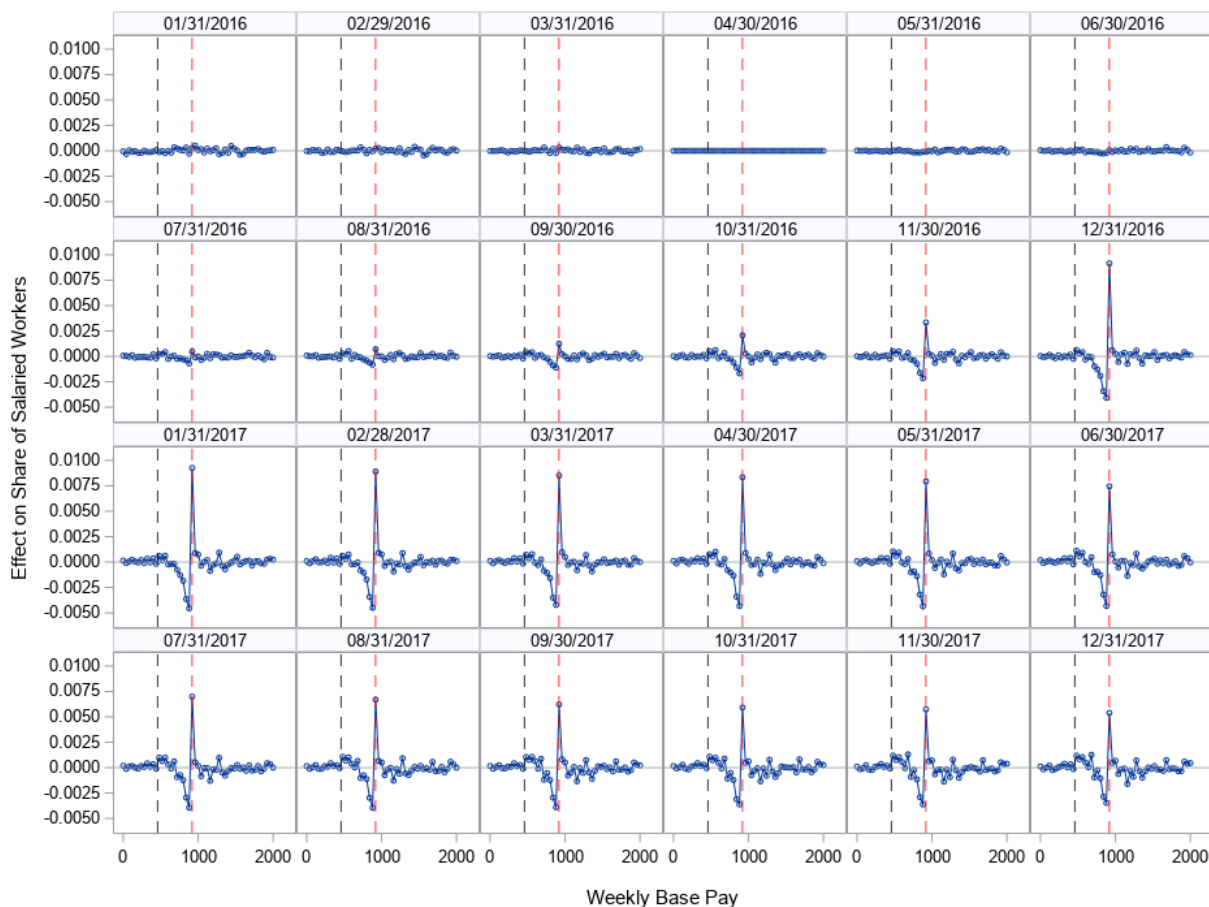
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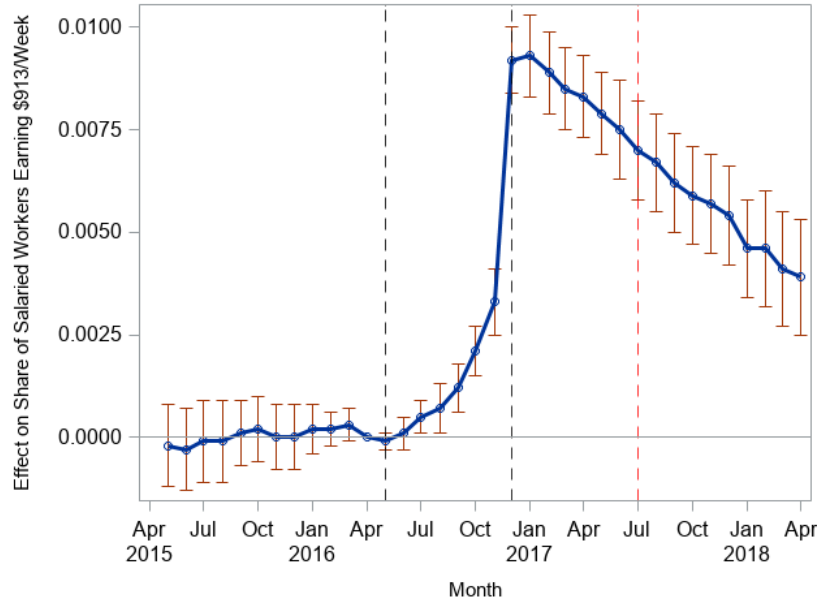
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Figure 1: Effect on Distribution of Continuously Employed Salaried Workers Relative to April 2016



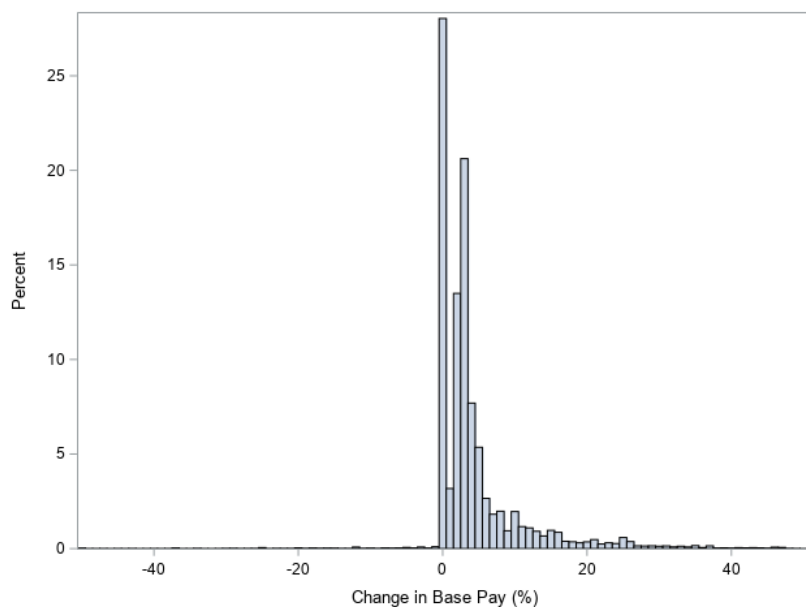
Notes: This figure shows the effect of the 2016 FLSA policy on the share of salaried workers across weekly base pay from January 2016 to December 2017, estimated using equation 2. The x-axis is divided into \$40 increments, where the black vertical dashed line is at \$433 and the red vertical dashed 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 2: 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 2. 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.

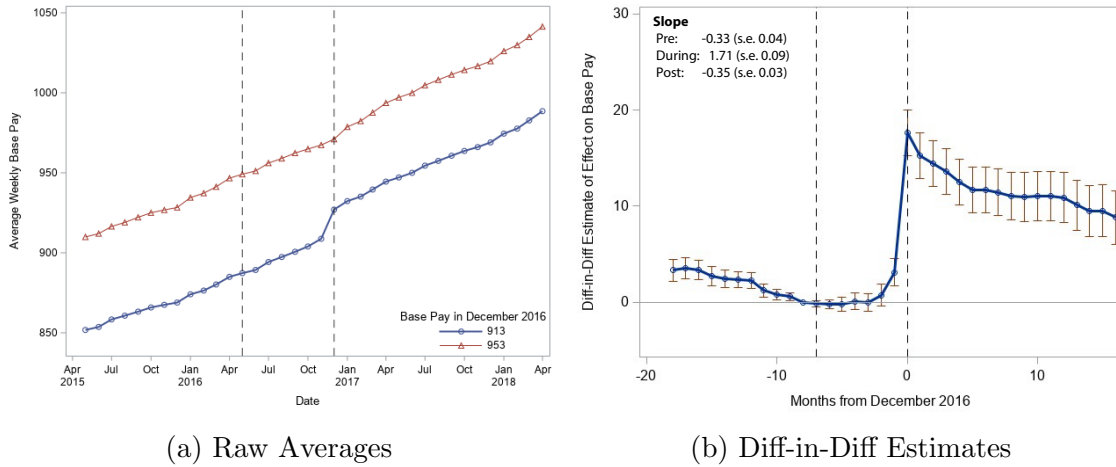
Figure 3: Distribution of One-Year Change in Base Pay for Workers Bunched in December 2016



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.

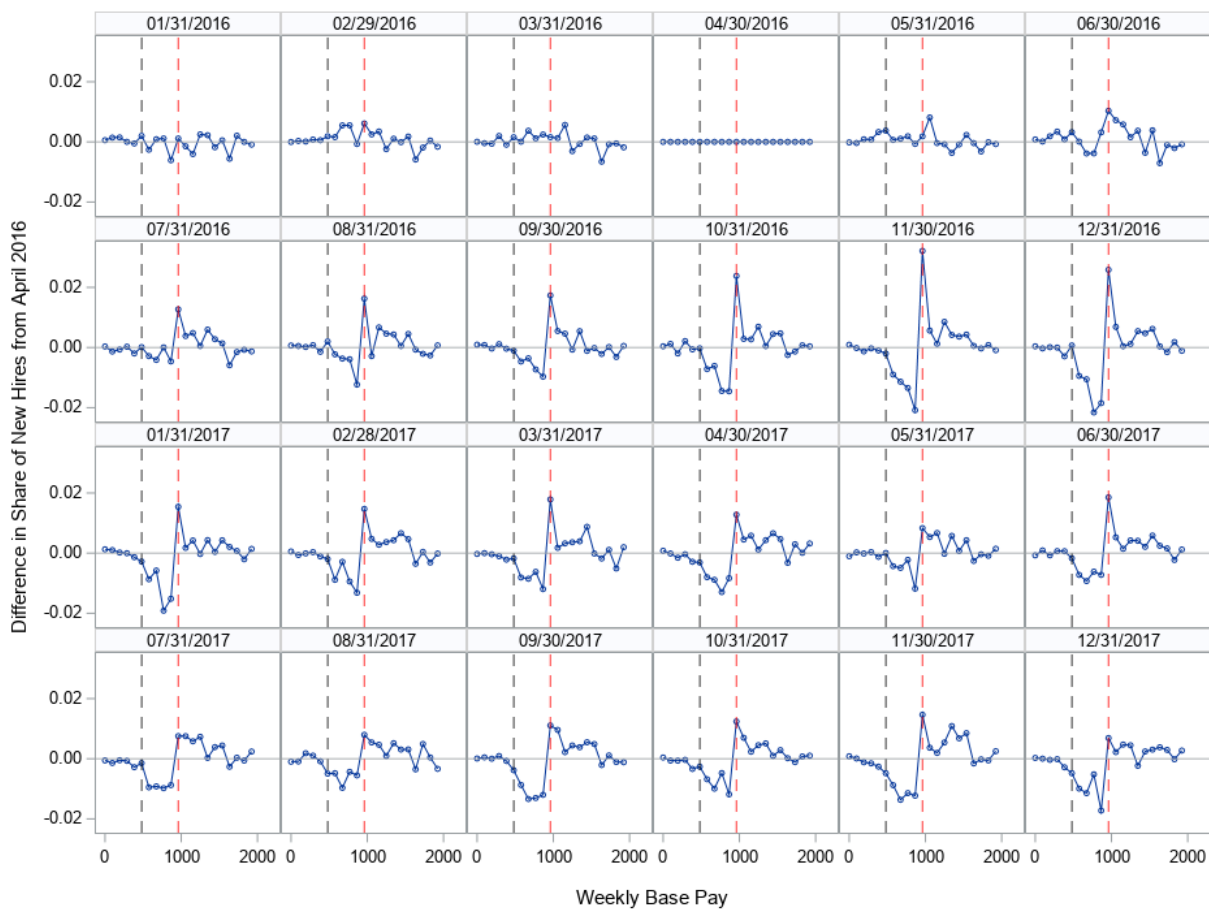


Figure 4: Difference-in-Difference of Base Pay Between Bunched and Non-Bunched Workers



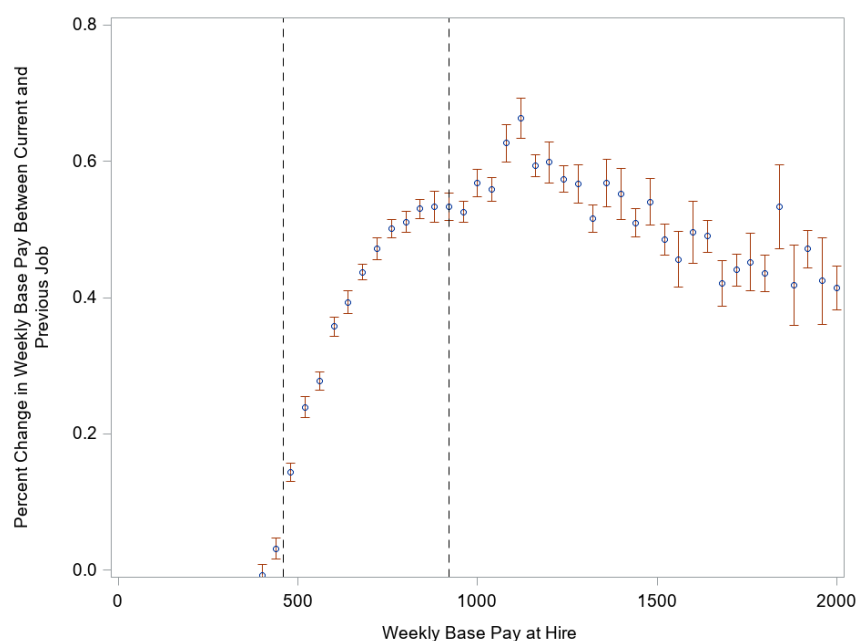
Notes: Panel (a) shows the evolution of workers' weekly base pay over time for salaried workers who earned within [913,953) and [953,993) per week in December 2016. Panel (b) plots the difference in difference estimates of the two lines in panel (a), with the difference at April 2016 being the reference point. The estimates in the top left corner of panel (b) represent the slope of the coefficients leading up to May 2016, between May and December 2016, and after December 2016. In both figures, the left vertical dashed line is at May 2016, and the right vertical dashed line is at December 2016.

Figure 5: Distribution of New Hires Over Time Relative to Hire in April 2016

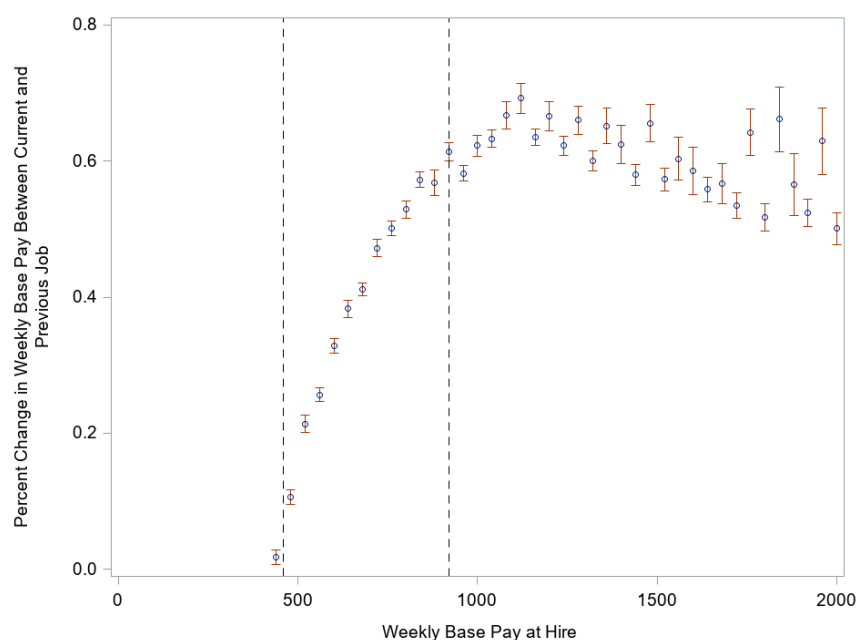


Notes: This figure shows the share of new salaried hires within each \$96.15 increment of weekly base pay between January 2016 and December 2017, relative to the share in April 2016. In each panel, the black vertical dashed line is at \$432 and the red vertical dashed line is at \$913.

Figure 6: Base Pay at Previous Job Given Base Pay at New Job



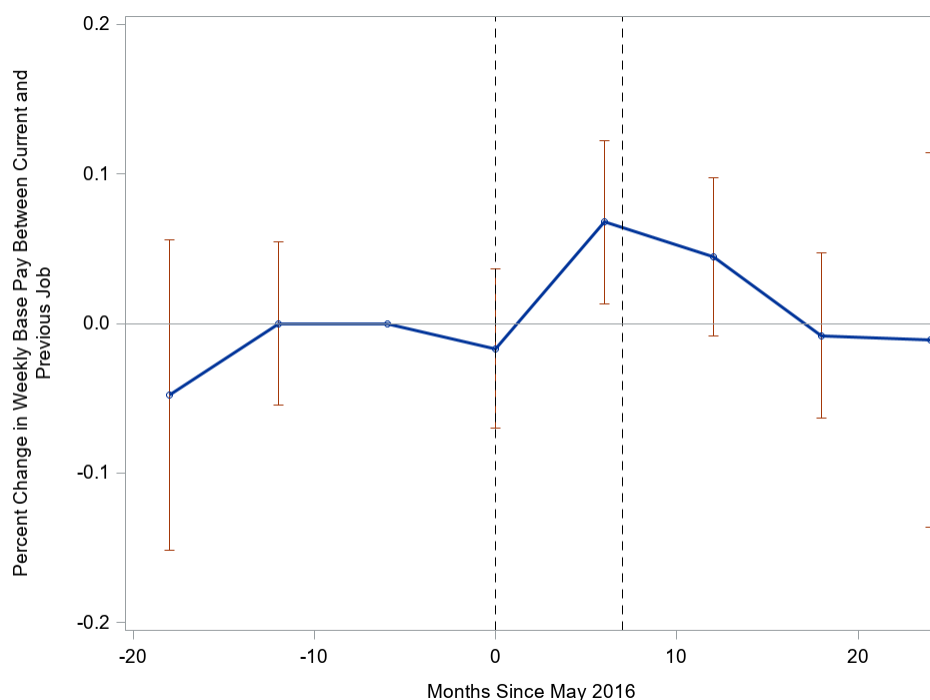
(a) Hired Between May 2015 and April 2016



(b) Hired Between May 2016 and May 2018

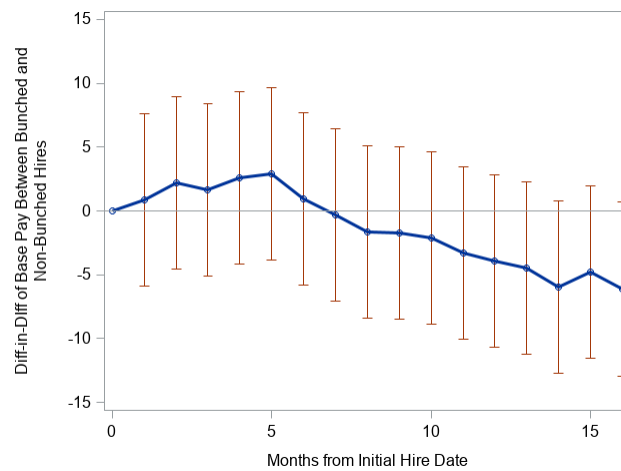
Notes: This figure plots the percent change between the base pay of new hires' entry salary and the salary at their last observable employer, by the salary at their new job. Panel (a) restricts the sample to workers hired between May 2015 and April 2016. Figure (b) restricts the sample to workers hired between May 2016 and May 2018. In both figures, the left vertical dashed line is at \$432 per week, and the right vertical dashed line is at \$913 per week.

Figure 7: Difference in Percent Change in Base Pay from Previous Job Between Bunched and Non-bunched Workers



Notes: This figure shows the difference in the percent change in new hires' base pay from their previous job, between bunched and non-bunched hires, over time. The coefficients are estimated via equation 6, where I define bunched workers as hires earning between \$913 and \$953, and non-bunched workers as hires in the bin \$40 greater. Each point is averaged over 6 months, where 0 is for May 2016 to October 2016. The left vertical dashed line is at May 2016, and the right line is at December 2016.

Figure 8: Difference-in-Difference in Base Pay Between Bunched and Non-bunched Workers



Notes: This figure shows the difference in the evolution of base pay between bunched and non-bunched hires, relative to the difference in the month of their initial hire date. I restrict the sample to workers hired between November 2016 and April 2017, who remain with their new employer for at least 16 months. The coefficients are estimated via equation 7, where I define bunched workers as hires earning between \$913 and \$953, and non-bunched workers as hires in the bin \$40 greater.

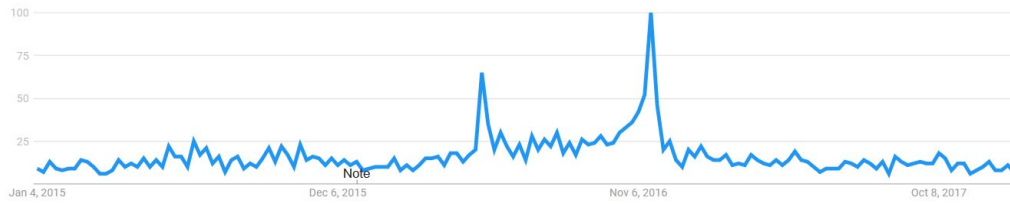
Table 1: Effect on Workers' Base Pay 16 Months After Hire

	(1)	(2)	(3)
Base Pay	-6.10 (3.41)	-6.39 (3.41)	-5.67* (3.08)
Treatment FE	Y	-	Y
Time FE	Y	-	Y
Treatment-Hire Date FE	-	Y	-
Time-Hire Date FE	-	Y	-
N	171,444	171,444	786,255
Control Group	Non-bunched	Non-bunched	Stayers

Notes: This table reports the change in the wages of workers hired between November 2016 and April 2017 with an entry wage of \$913-953 per week relative to the change in the wages of a control group of workers. In columns (1) and (2), the control group are workers hired during the same time period, but had entry wages of \$953-993 per week. Column (1) controls for time from hire and treatment/control group fixed effects, as described in equation 7. Column (2) interacts the fixed effects for workers' date of hire. In column (3), the control group consists of incumbent workers earning \$913-953 per week on the same month that the treatment group was hired. All workers are employed at the same firm for at least 16 months from the reference date.

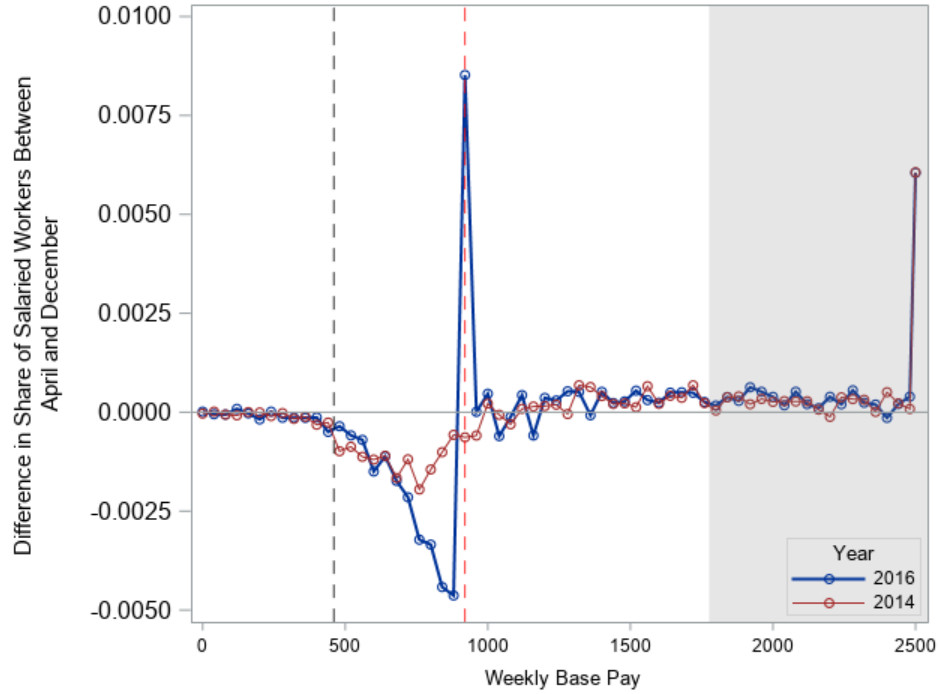
## Appendix A. Supplementary figures and tables noted in the text

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.

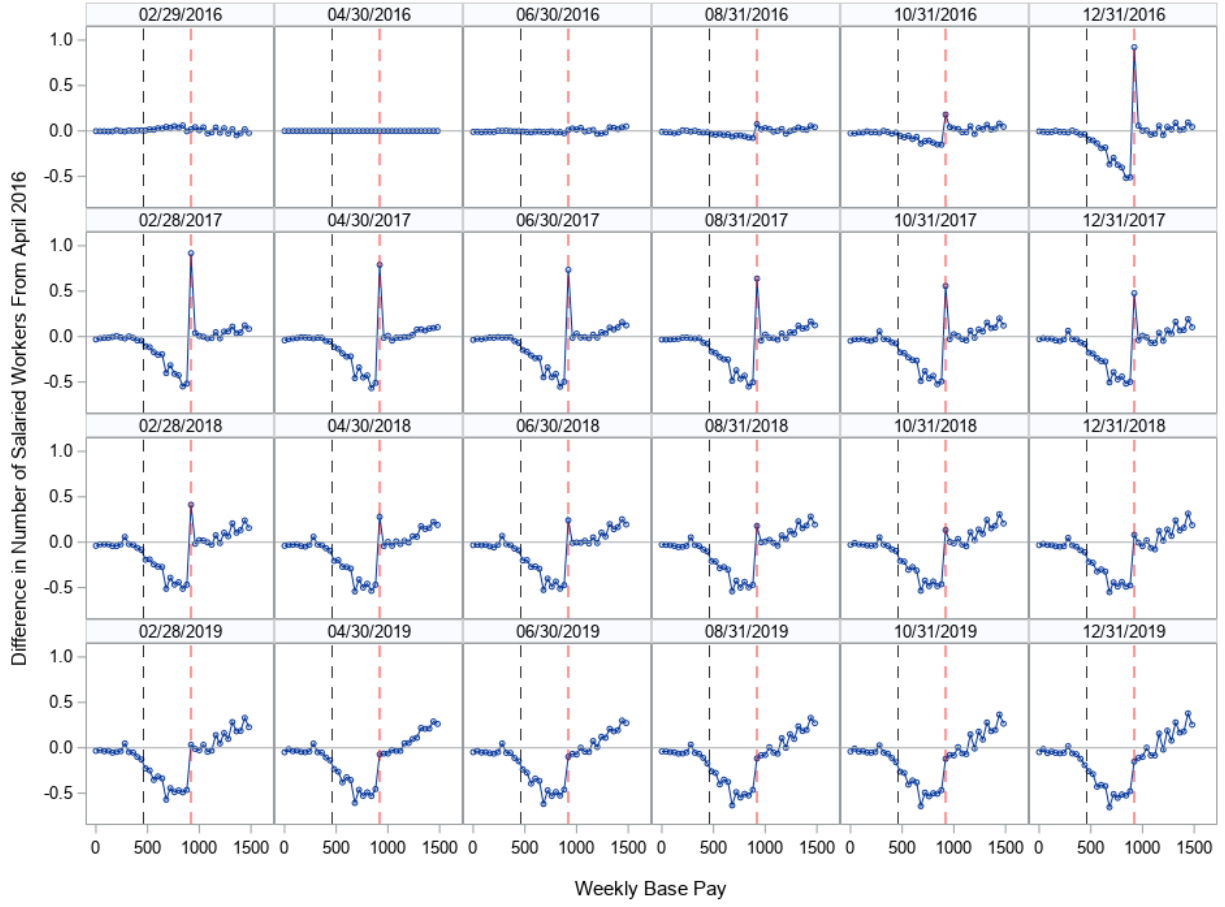
Appendix Figure A.2: Change in the Density of Base Pay Between April and December



Notes: The blue (red) line show the difference in the density of base pay between April and December of 2016 (2014) among salaried workers who are continuously employed at the same firm from May 2015 to April 2018 (May 2013 to April 2016). The black vertical dashed line is at \$433 and the red vertical dashed line is at \$913 per week. The shaded region shows the bins of base pay that I use to estimate equation 3.

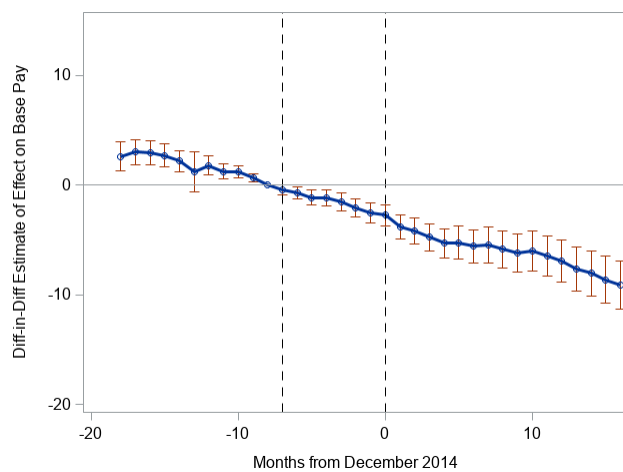


Appendix Figure A.3: Difference in Distribution of Salaried Workers from April 2016



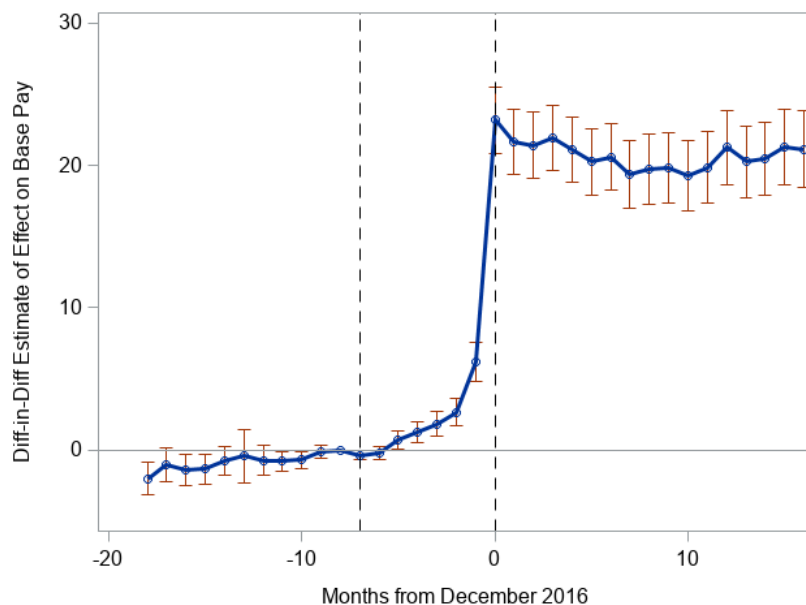
Notes: This figure shows the difference in the number of salaried workers across weekly base pay relative to April 2016 at the average firm. The x-axis is divided into \$40 increments, where the black vertical dashed line is at \$433 and the red vertical dashed line is at \$913 per week. The sample is restricted to firms that are continuously in the data between January 2016 and December 2019.

Appendix Figure A.4: Placebo Test of Difference-in-Difference Estimates of Base Pay Effect



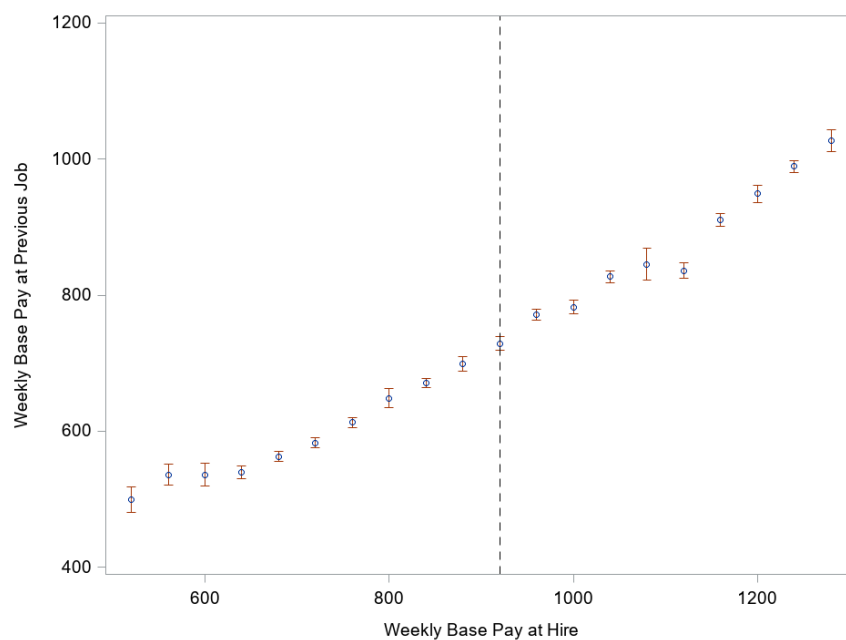
Notes: This figure shows difference-in-difference estimates that compares salaried workers earning within [\$913,953) in December 2014 to those earning [\$953,993). The sample consists of workers who are continuously employed in a salaried position at the same firm between May 2013 and April 2016.

Appendix Figure A.5: Difference-in-Difference Estimates of Base Pay Effect with Previous Year of Workers as Control

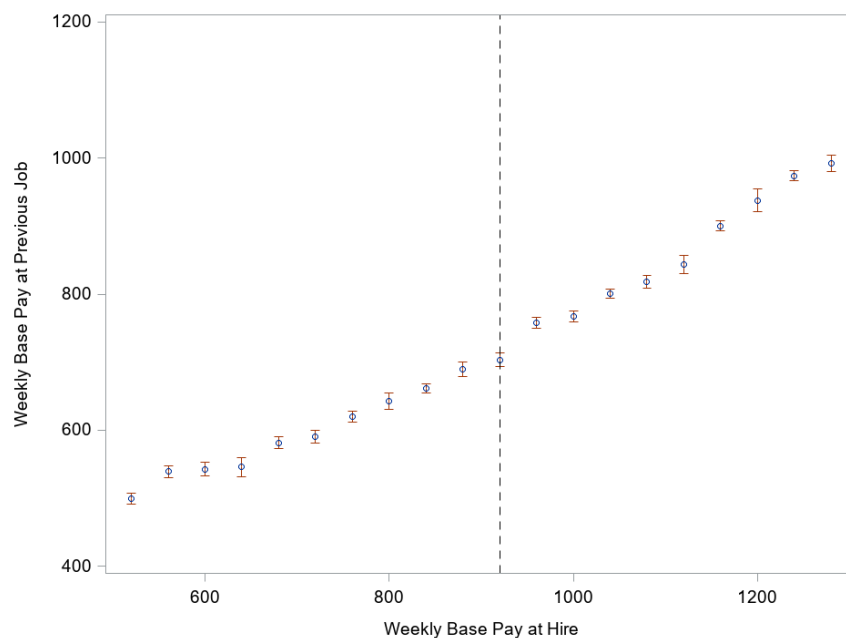


Notes: This figure plots the difference in difference estimates that compares the weekly base pay of salaried job-stayers in December 2016 to salaried-job stayers in December 2014, both of which earned between \$913 and \$955 per week in their respective months. The left vertical dashed line is at May 2016, and the right vertical dashed line is at December 2016.

Appendix Figure A.6: Base Pay at Previous Job Given Base Pay at New Job



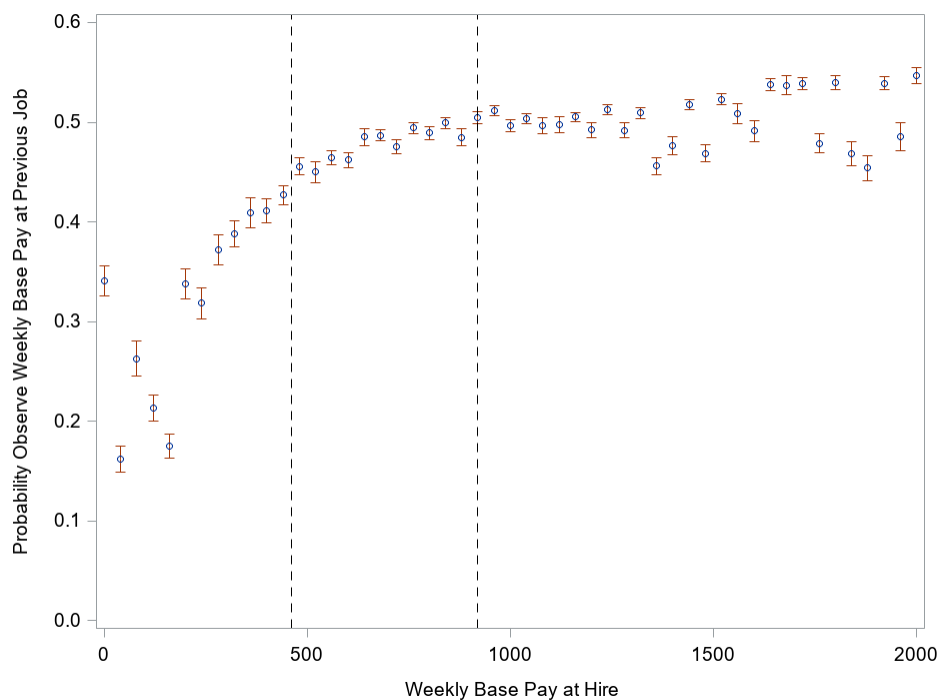
(a) May 2015 to April 2016



(b) May 2016 to May 2018

Notes: Each figure plots the base pay of new hires at their last observable employer by the base pay they receive in the first month at their new employer. Panel (a) restricts the sample to salaried workers hired between May 2015 and April 2016. Panel (b) restricts the sample to salaried workers hired between May 2016 and May 2018. In both figures, the vertical dashed line is at \$913 per week.

Appendix Figure A.7: Probability Observe Base Pay from Previous Job Given Base Pay at New Job



Notes: This figure shows the probability that I observe the salary of a new hire in at least one previous employer, by the entry wages of the new hire. The sample consists of all salaried 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.