

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

Simon Quach\*

October 1, 2020

[Please click here for most recent version](#)

## Abstract

This paper evaluates the extent of downward nominal wage rigidity in the United States by using the retraction of a federal policy that raised some workers' wages above their market rate as a source of variation. In November 2016, a court order nullified an anticipated rule change that would have granted overtime coverage to all salaried employees in the US earning less than \$913 per week. However, in preparation for the new rule, employers had already promised raises to certain employees. Consistent with the existence of downward rigidities, I find that firms bunched workers' pay at the \$913 threshold in December 2016, and this bunching persisted for over two years after the policy was terminated. Comparing stayers who got bunched to unaffected workers above the cutoff, I show that bunched workers continued to experience the same rate of wage growth before and after their raise. In addition, I find similar effects for new hires: they continued to be bunched at the \$913 cutoff and their wages grew at the same rate as new hires who were not bunched. These results provide causal evidence that the present discounted value of wages for both stayers and new hires is downward rigid.

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

---

\*I am extremely grateful to Alexandre Mas for his tremendous guidance and support on this project. I am thankful for the helpful comments and suggestions from David S. Lee and Henry Farber. I am indebted to Alan Krueger, Ahu Yildirmaz, and Sinem Buber Singh for facilitating access to ADP's payroll data, which I use in my analysis. The author is solely responsible for all errors and views expressed herein.

# 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 modern economists, the idea that nominal wages cannot adjust downwards to achieve full employment 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>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 existence of downward nominal wage rigidity and its effect on employment 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.<sup>3</sup> First, since employment is often a long-term relationship, firms’ employment decision depends on the discounted present value of workers’ wages (Becker, 1962). Second, the key allocative wage in job search models is the wage of new hires, not stayers (Pissarides, 2009). Empirical studies of the stickiness of hiring wages have found mixed results from comparing the cyclicalities of entry wages to that of existing workers.<sup>4</sup> While estimating the responsiveness of wages to aggregate economic conditions is an intuitive test of wage rigidity, it is often a challenge to control for changes in the composition of new hires over the business cycle and to identify a counterfactual for the present value of wages.

---

<sup>1</sup>In a similar spirit, many papers assume symmetric wage rigidity to generate large enough unemployment volatility in the canonical search and matching model Mortensen and Pissarides (1994) to fit the data (Shimer, 2004; Hall, 2005; Hall and Milgrom, 2008; Kennan, 2010).

<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.* (2019). See Elsby and Solon (2019) for a review.

<sup>3</sup>See Basu and House (2016) for a review.

<sup>4</sup>Depending on the data and sample selection, some papers find that new hires’ wages are as cyclical as job stayers’ (Gertler and Trigari, 2009; Grigsby *et al.*, 2019; Hazell and Taska, 2019; Gertler *et al.*, 2020), whereas others find that it is more procyclical (Martins *et al.*, 2012; Haefke *et al.*, 2013; Kudlyak, 2014).

Furthermore, even if one fully addresses these issues, it would still not be clear whether wage rigidity has any real effects on employment.

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 1, 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 reclassified some workers from salaried to hourly, and bunched other workers' salaries at the \$913 threshold (Quach, 2020). For the median worker who got bunched, I found that this raise was equivalent to a 5.8% increase to 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 proposed 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. Previous papers have argued that even if spot wages are downward rigid, a dynamically optimizing firm would compress future wage increases without significantly affecting employment (Elsby, 2009). Causally

testing this hypothesis is difficult as it requires one to develop a counterfactual evolution of wages in the absence of wage rigidity. I overcome this difficulty by using worker-level variation in whether a worker is being paid above their market wage or not, as indicated by whether their salary is bunched at \$913 per week. If firms do compress future wages in response to wage rigidity, then one would expect the salary growth of bunched workers to slow down following the injunction in December 2016. However, using a difference-in-difference design, I show that the salaries of individuals bunched at the \$913 threshold grew at the same rate before and after the injunction compared to those earning slightly above that threshold. These results also hold if I use individuals earning \$913 per week in December 2014 as the control group.

Consistent with the existence of rigidity in the wages of new hires, I next show that even a year after the proposed overtime policy was nullified, firms continued to bunch the salaries of new hires at the \$913 exemption threshold. This result supports the growing body of evidence that workers care not only about their own wages, but also about the wages of their colleagues (Akerlof and Yellen, 1990; Card *et al.*, 2012; Breza *et al.*, 2017; Dube *et al.*, 2019). While new hires could be receiving higher salaries to match their coworkers' salaries, it is also possible that their composition has changed following the termination of the overtime policy. To test whether new hires bunched at the threshold were positively selected, I compare the salaries of new hires at their previous jobs across the base pay distribution and over time. I find that after the \$913 threshold was supposed to go into effect, new hires with weekly salaries at the threshold experienced a larger pay increase from leaving their previous job compared to hires earning right below and above the cutoff. This indicates that the bunching of entry wages represents a real pay increase and is not driven solely by firms hiring more productive workers. Furthermore, I find evidence that this rigidity in new hires' wages persists in the long-run. Following the evolution of workers' salaries over time, I show that the base pay of new employees bunched at the threshold did not grow more slowly than that of non-bunched hires or stayers.

My results contribute to a growing literature that estimates the employment effects of downward nominal wage rigidity using variation in its restrictiveness over time and between firms. In a developing country context, Kaur (2019) uses rainfall shocks as exogenous variation to workers' marginal revenue product, and finds movement in wages and employment consistent with downward nominal wage rigidity. In a US context, Kurmann and McEntarfer (2019) finds that firms bound by downward rigidity, as defined from bunching in their distribution of wage changes prior to 2008, suffered larger employment losses during the Great Recession relative to firms that had more flexible wages. Compared to these studies, I

evaluate the employment effects of wage rigidity using variation induced by a labor market policy rather than negative productivity shocks.

In addition, my paper is related to a large literature on employment 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 affirmation action requirements continues to grow even after its deregulation, which he argues is due to improved screening methods for new hires. A working 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 he attributes to a decrease in youth discrimination. My paper presents evidence that negative employment effects can continue to persist due to downward negative wage rigidity.

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. In section ??, I report the effect of the increased labor costs on employment. 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.

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>5</sup> From a review of newspaper articles at the time, I found that few paid attention to the development of the court case and those that did warned employers to not expect a ruling before the December 1st deadline.<sup>6</sup>

Hence, it came as a surprise to employers when the court ordered a preliminary injunction on November 22, 2016, ten days before the scheduled 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 that the \$913 proposal would 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 around \$634 per week. Ultimately, Acosta was confirmed as Labor Secretary and the DOL dropped its defense of the 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 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,

---

<sup>5</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>6</sup>For example, see *Texas Judge Consolidates Challenges to Overtime Rule* (SHRM Oct. 21, 2016)

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 rate, 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>7</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.<sup>8</sup>

### 3 ADP Data

I use anonymous monthly administrative payroll data accessed through ADP LLC. ADP is 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 on over 50 million unique individuals and 170,000 firms 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>9</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 whereas 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

---

<sup>7</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>8</sup>While Quach (2020) finds reclassification effects for workers earning up to \$1200 per week, these effects were small and unlikely to introduce selection bias when I restrict the sample to continuously-salaried employees.

<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.* (2019).

of weeks between each paycheck.<sup>10</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. This sample consists of 35,806 firms and 10,182,805 workers, 16% of whom are always classified as salaried. Second, to study the wage rigidity of new hires, I create a sample consisting of all hires into firms that remain continuously in the dataset over the same time period. This sample has 37,603 firms and 20,145,618 new hires, 14% of which are salaried. To address concerns about changes in the composition of new hires following the 2016 policy, I also merge on the job characteristics from each new hire's 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 threshold that changes after 2016.<sup>11</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 testable null hypotheses.

### 4.1 Rigid Spot Wages

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.

To estimate the effect of the injunction on the density of weekly base pay, I apply a similar identification strategy to Quach (2020) where I use the change in the distribution before the

---

<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.

<sup>11</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.



policy as a counterfactual for the change afterwards. To account for year specific differences, I scale the counterfactual distribution to match the right tail of the actual distribution. Formally, I model the density of base pay within each firm relative to its density in April 2016 as follows:

$$n_{ikt} = n_{ik, Apr, 2016} + \alpha_{kt} + \beta_{kt} \cdot D_{t>0} + \varepsilon_{ikt} \quad (1)$$

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 by  $\alpha_{kt}$  the counterfactual increase in the share of workers in bin  $k$  at the average firm after  $t$  months. The variable  $D_{t>0}$  is an indicator that equals one after April 2016, and the coefficient  $\beta_{kt}$  is the causal effect of the 2016 FLSA overtime policy on the share of workers in bin  $k$  at time  $t$ . Similarly, I express changes in the density of base pay relative to April 2014 by

$$n_{ik\tau} = n_{ik, Apr, 2016} + \alpha_{k\tau} + \beta_{k\tau} \cdot D_{\tau>24} + \varepsilon_{ik\tau}$$

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

To separately identify the causal effect  $\beta_{kt}$  from the counterfactual effect  $\alpha_{kt}$  in equation 1, I make the following 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 can use the change in the density relative to April 2014 as a counterfactual for the change relative to April 2016:

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

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.

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

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>12</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 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, the average firm 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>13</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 also be the case that there are adjustment costs each time they modify workers' wages, whether up or down.

**Null Hypothesis 1b:** If spot wages are flexible, employers believe with nonzero probability that the proposed threshold might be upheld, and it is costly to adjust wages in either direction then the bunching would unravel quicker following the final decision by the courts in June 2017.

I show in figure 2 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 the magnitude of the bunching in the base pay distribution. The size of the bunching rose by nearly 1% of all salaried workers in January 2017 and then shrank at a constant rate afterwards.<sup>14</sup> The lack

<sup>12</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>13</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 plots the raw difference in the base pay distribution relative to April 2016 for all salaried workers, not just stayers.

<sup>14</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%.

of any discontinuous changes 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 \$913 threshold. 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, it could be masking significant heterogeneity in wage rigidity across firms and workers.<sup>15</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.* (2019) 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 the decline in the bunching over time is due to workers receiving raises rather than pay cuts.

## 4.2 Rigid Discounted Present Value of Wages

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

---

<sup>15</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.

December 2016 (henceforth called non-bunched workers).<sup>16</sup> To compare these two groups, I estimate a difference-in-difference regression of the form

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

where  $y_{it}$  is the base pay of individual  $i$  at month  $t$ , and  $D_{s=1,t=\tau}$  is a dummy that equals one for bunched workers  $\tau$  months since December 2016. I control for a bunched worker fixed effect  $\alpha_s$ , and a month fixed effect  $\alpha_t$ .

Figure 4 shows the evolution of bunched and non-bunched workers' salaries over time, as well as their difference-in-difference estimates. Even though not some of the workers earning within \$40 of the threshold would have been there regardless of the policy, it is still 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. In panel (b), I plot the difference-in-difference estimates relative to April 2016. While the wages of bunched workers grew more slowly than that of non-bunched workers post-injunction, this wage-growth differential was also present before the announcement of the FLSA rule change in May 2016.<sup>17</sup>

To statistically test whether the relative wage growth changed following the injunction, I estimate the following regression:

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

where  $time$  is a continuous time variable and  $D_{sp}$  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 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-

---

<sup>16</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>17</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 faster wage growth in 2014 as well.

bunched. This is not statistically different from the negative \$0.35 (s.e. 0.03) wage-growth differential after December 2016.

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 1st, 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 to an implicit contract 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.

### 5.1 Rigid Hiring Wages

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.

Figure 5 plots the base pay distribution of hires between January 2016 and December 2017, relative to the distribution in April 2016.<sup>18</sup> In anticipation of the policy change, firms began bunching new hires' salaries at the \$913 threshold starting in June 2016. At the spike's highest point in November 2016, the month before the rule change was supposed to go into effect, the share of workers hired at the threshold increased by 3% relative to April 2016.<sup>19</sup> While it diminishes over time, the bunching of entry wages persists to at least November 2017,

---

<sup>18</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>19</sup>For comparison, 7.7% of new hires earned within \$96.15 above the threshold in April 2016.

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 absent the policy, 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.

I argue that based on the distribution of new hires' previous salaries, the persistence in bunching cannot be explained by compositional changes alone.

**Null Hypothesis 3b:** If firms are hiring more productive workers in response to the policy, then the observed and counterfactual new hires at the \$913 threshold should have the same salary at their previous job.

Intuitively, if the productivity of workers bunched at the \$913 threshold is equal to the productivity of workers that would earn \$913 per week even in the absence of the FLSA policy change, then their previous salaries should be equal. In contrast, under the alternative hypothesis that the wages of new hires is downward rigid, I would instead expect actual hires bunched at the threshold to have had lower previous salaries, and to experience a large pay increase from switching jobs, compared with counterfactual workers.

To test this, figure 6 plots the percent change in new hires' base pay from their last observed job, by the entry salary at their new job. For workers hired prior to May 2016, I find that the change in base pay from switching jobs is continuous across the \$913 cutoff. However, for workers hired after the announcement of the FLSA rule change, workers hired at the \$913 cutoff experienced a discontinuously larger increase in base pay from switching jobs, relative to workers hired above that threshold. Consistent with the existence of wage rigidity, this discontinuity suggests that firms actually raised the pay of new hires, and were not simply becoming more selective.

I present additional tests in appendix figures A.6 and A.7 to show that the discontinuity in the percent change in base pay is not driven by sample selection. 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. It is in principle possible that firms are hiring more productive workers in response to the 2016 FLSA policy, but I 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.6 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. In contrast, figure A.7 shows a noticeable discontinuity in previous

base pay with respect to the hiring wage at the \$913 cutoff that is more pronounced after May 2016. Taken together, these graphs support the hypothesis that after the announcement of the FLSA rule change, firms increased the pay of new hires without hiring more productive workers.

Although firms increased the base pay of new hires after the announcement of the 2016 FLSA rule change, they could have become more selective over time following the injunction of the policy. To determine whether this is the case, I compare over time the change in base pay from switching jobs for workers hired at \$913-953 per week to the gains from job-switching for workers hired at \$953-993 per week:

$$\Delta y_i = \sum_{\substack{\tau=-3 \\ \tau \neq -1}}^4 \beta_\tau D_{s=1, \text{floor}(\frac{t}{6})=\tau} + \alpha_s + \alpha_t + \varepsilon_i \quad (6)$$

Let  $\Delta y_i$  be the percent change in new hire  $i$ 's base pay relative to the base pay at their previous job. I categorize 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> I control for treatment/control group and month of hire fixed effects with  $\alpha_s$  and  $\alpha_t$ , respectively. The dummy variable  $D_{s=1, \text{floor}(\frac{t}{6})}$  equals 1 if worker  $i$  is in the treatment group, and hired between  $6\tau$  and  $6(\tau + 1)$  months of May 2016.

Assuming there are no changes to the composition of hires over time as I have argued, and the 2016 FLSA policy had no effect on the control group, then  $\beta_\tau$  represents the effect of the rule change on the difference between bunched hires' actual entry wages and their counterfactual entry wages where the policy was never announced. I plot the estimates of  $\beta_\tau$  in figure 7. I would like to highlight three characteristics of the graph. First, the difference between the treatment and control groups' gains from switching jobs follows a similar trend prior to the announcement of the \$913 threshold in May 2016, suggesting that new hires earning \$953-\$993 per week are a valid counterfactual for hires who earn within \$913-953 per week. Second, the percent change in base pay from switching jobs was 6.7% (s.e. 2.7%) larger for workers hired between November 2016 and April 2017, relative to the counterfactual. Third, while this elevated raise decreases over time, new hires between May and October 2017 still experienced a 4.3% (s.e. 2.7%) larger gain from switching jobs. It is only starting in November 2017 that I see new hires' base wages returning to the level it would be if the policy never occurred. This suggests that the entry wages of new hires were

---

<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.

sticky for a full year after the injunction of policy.

## 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 the counterfactual where 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 if the FLSA policy did not occur.

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  $i$ , at  $t$  months after their hiring date. I control for whether or not the worker's entry wage was bunched at the threshold with the treatment/control group fixed effect  $\alpha_s$ , and for time fixed effect  $\alpha_t$ . 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. Even at the 95% lower bound, I can rule out negative effects on new hires' wage growth that are larger than \$12.78 over 16 months.

---

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



## 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 with the existence of downward sticky wages, I show that firms do not revert stayers' wages back to their pre-policy levels over time. Moreover, firms 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. These results provide evidence to show that the present discounted wages of both stayers and new hires are 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.

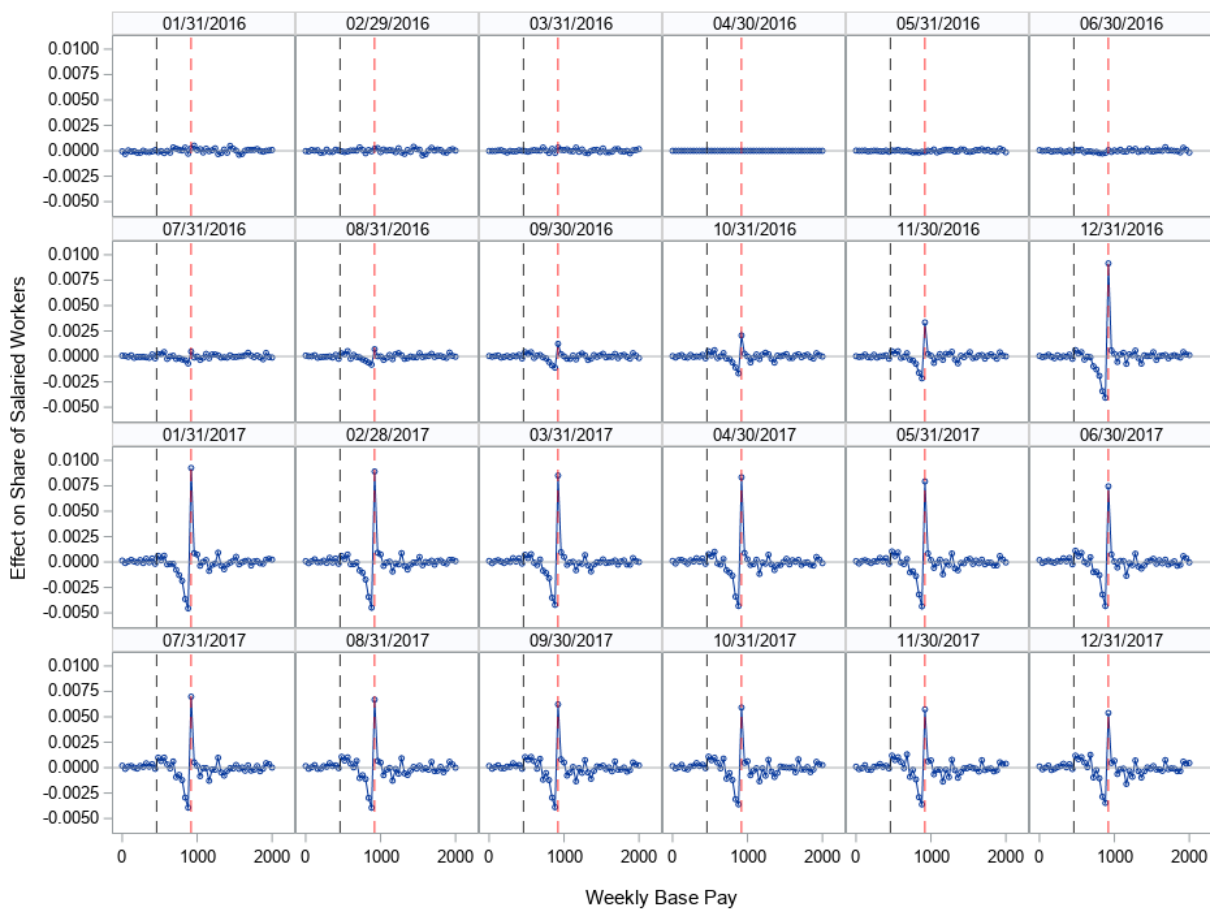
## References

- AKERLOF, G., DICKENS, W. R. and PERRY, G. (1996). The macroeconomics of low inflation. *Brookings Papers on Economic Activity*, **27** (1), 1–76.
- AKERLOF, G. A. and YELLEN, J. L. (1990). The Fair Wage-Effort Hypothesis and Unemployment\*. *The Quarterly Journal of Economics*, **105** (2), 255–283.
- ALTONJI, J. G. and DEVEREUX, P. J. (1999). *The Extent and Consequences of Downward Nominal Wage Rigidity*. Working Paper 7236, National Bureau of Economic Research.
- BARATTIERI, A., BASU, S. and GOTTSCHALK, P. (2014). Some evidence on the importance of sticky wages. *American Economic Journal: Macroeconomics*, **6** (1), 70–101.
- BASU, S. and HOUSE, C. L. (2016). Chapter 6 - allocative and remitted wages: New facts and challenges for keynesian models. In J. B. Taylor and H. Uhlig (eds.), *Handbook of Macroeconomics Volume 2, Handbook of Macroeconomics*, vol. 2, Elsevier, pp. 297 – 354.
- BECKER, G. S. (1962). Investment in human capital: A theoretical analysis. *Journal of Political Economy*, **70** (5), 9–49.
- BENIGNO, P. and RICCI, L. A. (2011). The inflation-output trade-off with downward wage rigidities. *American Economic Review*, **101** (4), 1436–66.
- BLANCHARD, O. J. and SUMMERS, L. H. (1986). *Hysteresis and the European Unemployment Problem*. Working Paper 1950, National Bureau of Economic Research.
- BREZA, E., KAUR, S. and SHAMDASANI, Y. (2017). The Morale Effects of Pay Inequality\*. *The Quarterly Journal of Economics*, **133** (2), 611–663.
- CARD, D. and HYSLOP, D. (1996). *Does Inflation Grease the Wheels of the Labor Market?* Working Paper 5538, National Bureau of Economic Research.
- , MAS, A., MORETTI, E. and SAEZ, E. (2012). Inequality at work: The effect of peer salaries on job satisfaction. *American Economic Review*, **102** (6), 2981–3003.
- DUBE, A., GIULIANO, L. and LEONARD, J. (2019). Fairness and frictions: The impact of unequal raises on quit behavior. *American Economic Review*, **109** (2), 620–63.
- DUPRAZ, S., NAKAMURA, E. and STEINSSON, J. (2019). *A Plucking Model of Business Cycles*. Working Paper 26351, National Bureau of Economic Research.
- ELSBY, M. W. (2009). Evaluating the economic significance of downward nominal wage rigidity. *Journal of Monetary Economics*, **56** (2), 154 – 169.
- ELSBY, M. W. L. and SOLON, G. (2019). How prevalent is downward rigidity in nominal wages? international evidence from payroll records and pay slips. *The Journal of Economic Perspectives*, **33** (3), 185–201.

- FALK, A., FEHR, E. and ZEHNDER, C. (2006). Fairness Perceptions and Reservation Wages—the Behavioral Effects of Minimum Wage Laws\*. *The Quarterly Journal of Economics*, **121** (4), 1347–1381.
- GERTLER, M., HUCKFELDT, C. and TRIGARI, A. (2020). Unemployment Fluctuations, Match Quality and the Wage Cyclicity of New Hires. *The Review of Economic Studies*. — and TRIGARI, A. (2009). Unemployment fluctuations with staggered nash wage bargaining. *Journal of Political Economy*, **117** (1), 38–86.
- GRIGSBY, J., HURST, E. and YILDIRMAZ, A. (2019). *Aggregate Nominal Wage Adjustments: New Evidence from Administrative Payroll Data*. Working Paper 25628, National Bureau of Economic Research.
- HAEFKE, C., SONNTAG, M. and VAN RENS, T. (2013). Wage rigidity and job creation. *Journal of Monetary Economics*, **60** (8), 887 – 899.
- HALL, R. E. (2005). Employment fluctuations with equilibrium wage stickiness. *American Economic Review*, **95** (1), 50–65.
- and MILGROM, P. R. (2008). The limited influence of unemployment on the wage bargain. *American Economic Review*, **98** (4), 1653–74.
- HAZELL, J. and TASKA, B. (2019). *Downward Rigidity in the Wage for New Hires*. Tech. rep., Working Paper.
- JARDIM, E. S., SOLON, G. and VIGDOR, J. L. (2019). *How Prevalent Is Downward Rigidity in Nominal Wages? Evidence from Payroll Records in Washington State*. Working Paper 25470, National Bureau of Economic Research.
- KAHN, S. (1997). Evidence of nominal wage stickiness from microdata. *The American Economic Review*, **87** (5), 993–1008.
- KAUR, S. (2019). Nominal wage rigidity in village labor markets. *American Economic Review*, **109** (10), 3585–3616.
- KENNAN, J. (2010). Private Information, Wage Bargaining and Employment Fluctuations. *Review of Economic Studies*, **77** (2), 633–664.
- KEYNES, J. M. (1936). *The General Theory of Employment, Interest, and Money*. London: Macmillan.
- KUDLYAK, M. (2014). The cyclicity of the user cost of labor. *Journal of Monetary Economics*, **68** (C), 53–67.
- KURMANN, A. and MCENTARFER, E. (2019). *Downward Nominal Wage Rigidity in the United States: New Evidence from Worker-Firm Linked Data*. School of Economics Working Paper Series 2019-1, LeBow College of Business, Drexel University.

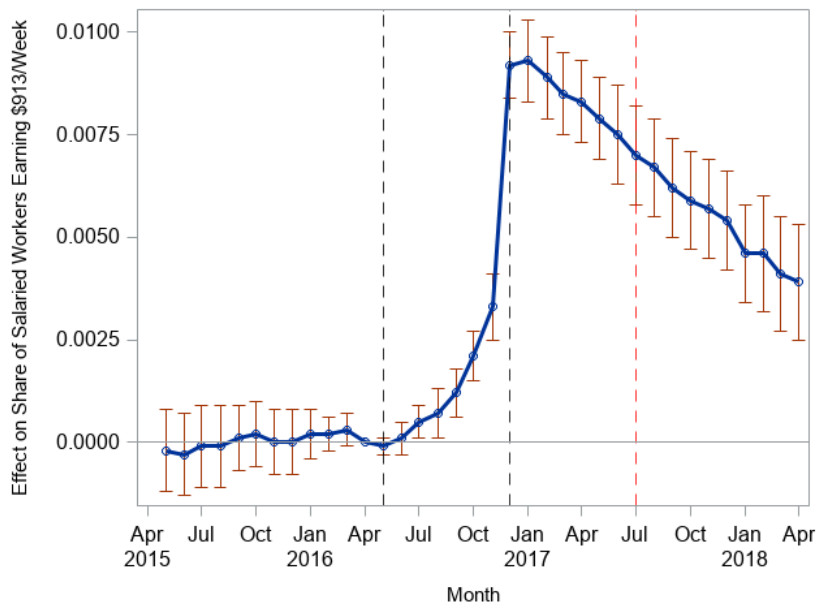
- MARTINS, P. S., SOLON, G. and THOMAS, J. P. (2012). Measuring what employers do about entry wages over the business cycle: A new approach. *American Economic Journal: Macroeconomics*, **4** (4), 36–55.
- MILLER, C. (2017). The persistent effect of temporary affirmative action. *American Economic Journal: Applied Economics*, **9** (3), 152–90.
- MORTENSEN, D. T. and PISSARIDES, C. A. (1994). Job creation and job destruction in the theory of unemployment. *The Review of Economic Studies*, **61** (3), 397–415.
- PISSARIDES, C. A. (2009). The unemployment volatility puzzle: Is wage stickiness the answer? *Econometrica*, **77** (5), 1339–1369.
- QUACH, S. (2020). The labor market effects of expanding overtime coverage. *Job Market Paper*.
- SAEZ, E., SCHOEFER, B. and SEIM, D. (2019). *Hysteresis from Employer Subsidies*. Tech. rep., National Bureau of Economic Research.
- SCHMITT-GROHÉ, S. and URIBE, M. (2016). Downward nominal wage rigidity, currency pegs, and involuntary unemployment. *Journal of Political Economy*, **124** (5), 1466–1514.
- SHIMER, R. (2004). The consequences of rigid wages in search models. *Journal of the European Economic Association*, **2** (2-3), 469–479.
- TOBIN, J. (1972). Inflation and unemployment. *American Economic Review*, **62** (1), 1–18.
- YELLEN, J. L. (2016). *Macroeconomic Research After the Crisis: a speech at "The Elusive 'Great' Recovery: Causes and Implications for Future Business Cycle Dynamics" 60th annual economic conference sponsored by the Federal Reserve Bank of Boston, Boston, Massachusetts, October 14, 2016*. Speech 915, Board of Governors of the Federal Reserve System (U.S.).

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



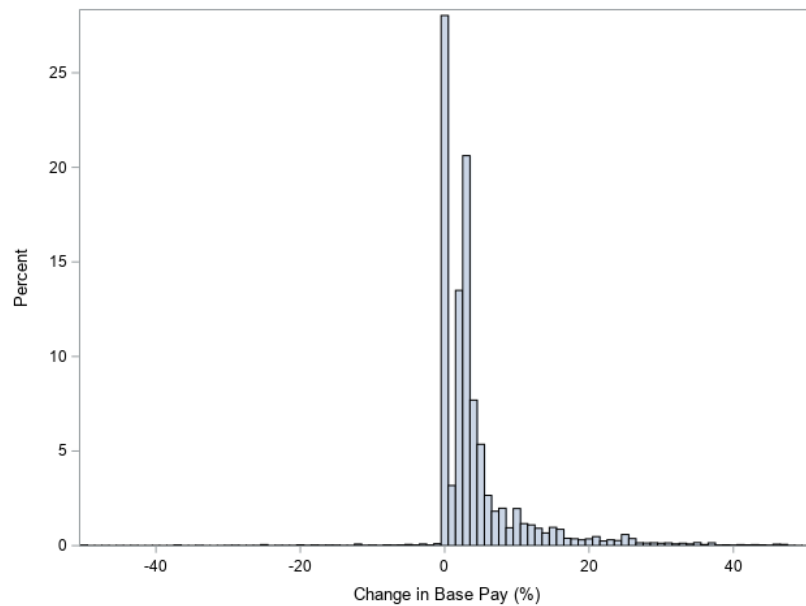
Notes: This figure shows the effect of the 2016 FLSA policy on the share of salaried workers across weekly base pay from January 2016 to December 2017, estimated using equation 2. The x-axis is divided into \$40 increments, where the black vertical dashed line is at \$433 and the red vertical dashed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



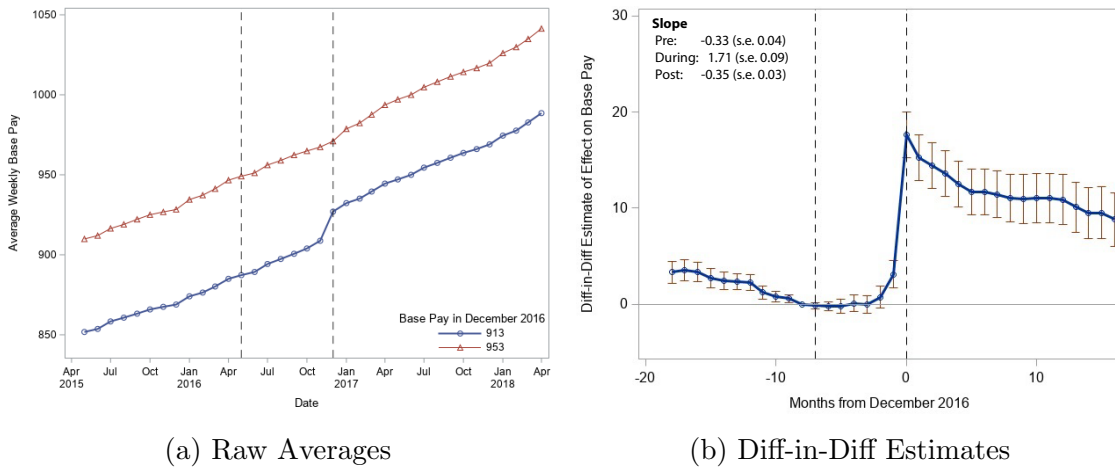
Notes: This figure shows the effect of the 2016 FLSA policy on the share of salaried workers earning between \$913 and \$953 per week, estimated from equation 2. Stayers. The sample consists of workers who are always salaried, and continuously employed at the same firm between May 2015 and April 2018. 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.

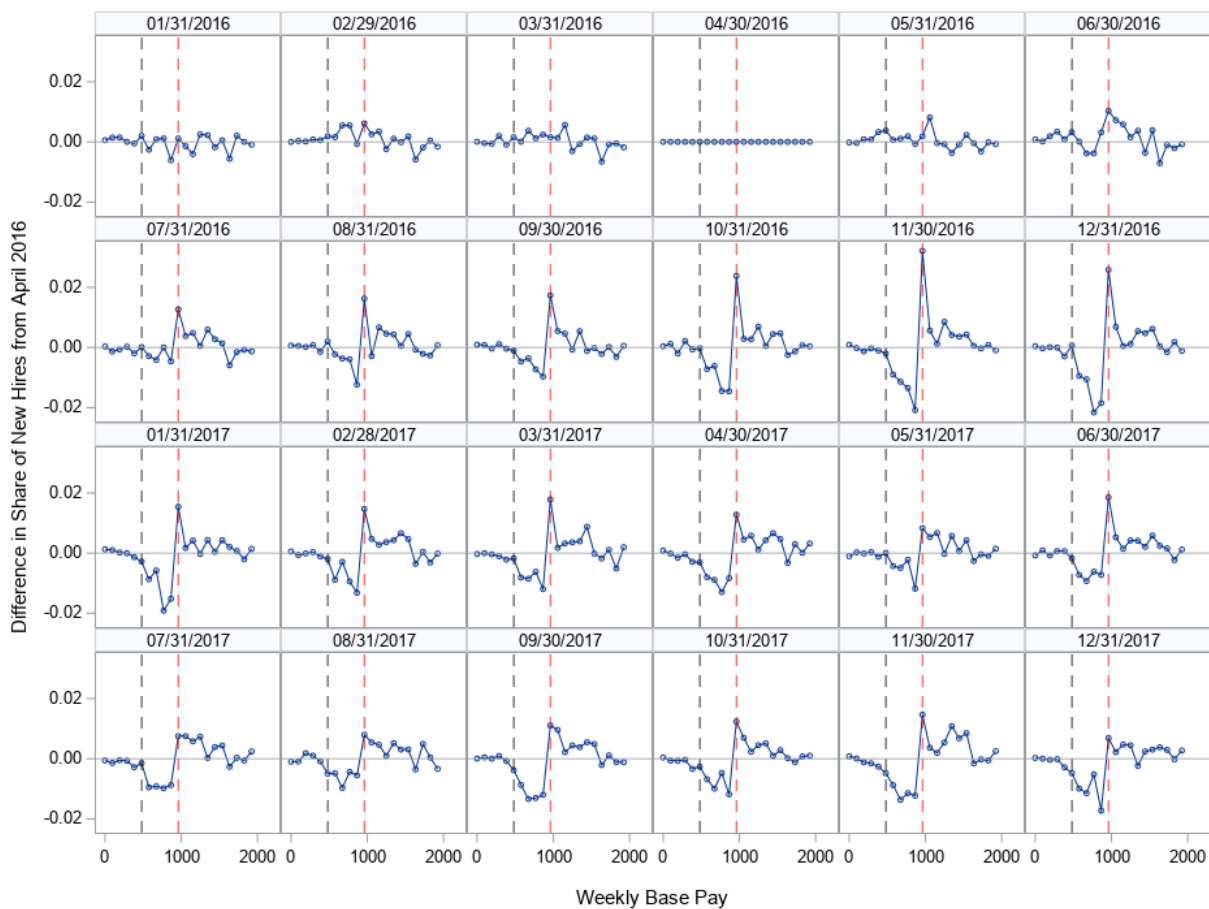
Figure 4: Difference-in-Difference of Base Pay Between Bunched and Non-Bunched Workers



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. In both figures, the left vertical dashed line is at May 2016, and the right vertical dashed line is at December 2016.

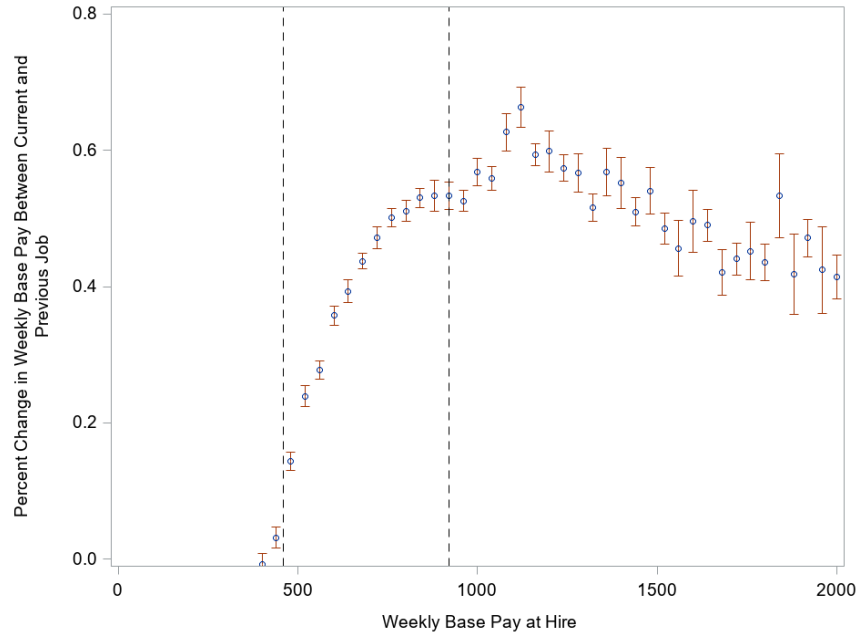


Figure 5: Distribution of New Hires Over Time Relative to Hire in April 2016

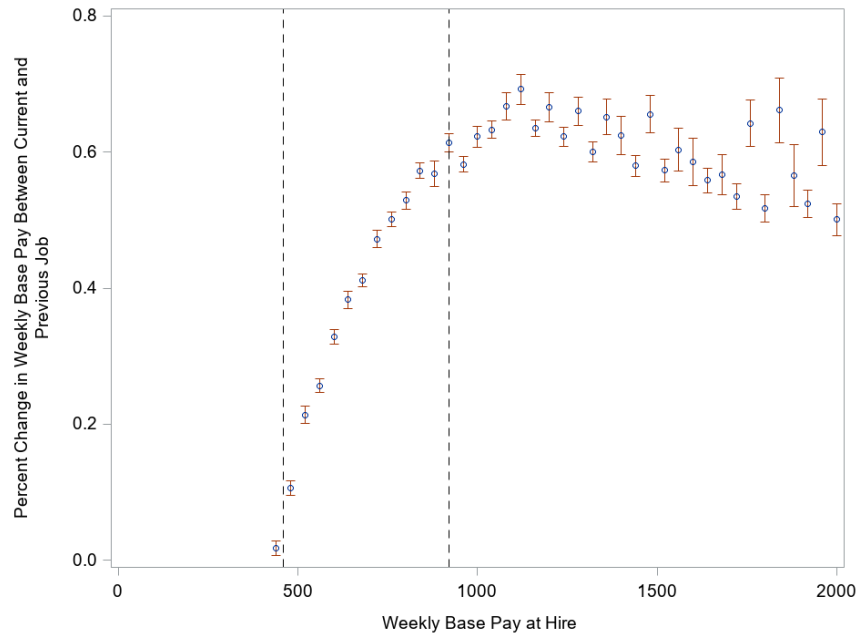


Notes: This figure shows the share of new salaried hires within each \$96.15 increment of weekly base pay between January 2016 and December 2017, relative to the share in April 2016. In each panel, the black vertical dashed line is at \$432 and the red vertical dashed line is at \$913.

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



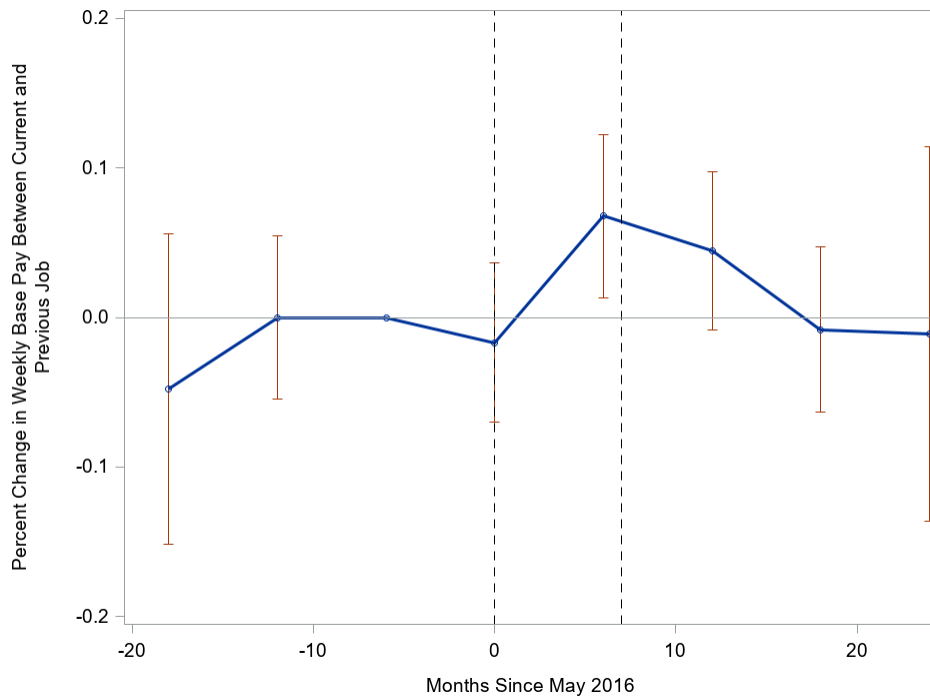
(a) Hired Between May 2015 and April 2016



(b) Hired Between May 2016 and May 2018

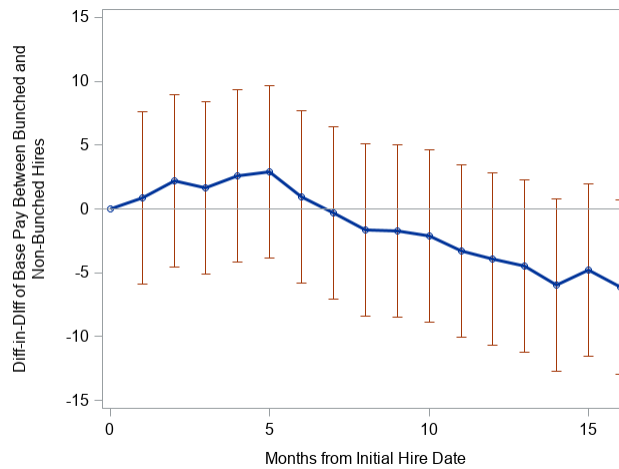
Notes: This figure plots the percent change between the base pay of new hires' entry salary and the salary at their last observable employer, by the salary at their new job. Panel (a) restricts the sample to workers hired between May 2015 and April 2016. Figure (b) restricts the sample to workers hired 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.

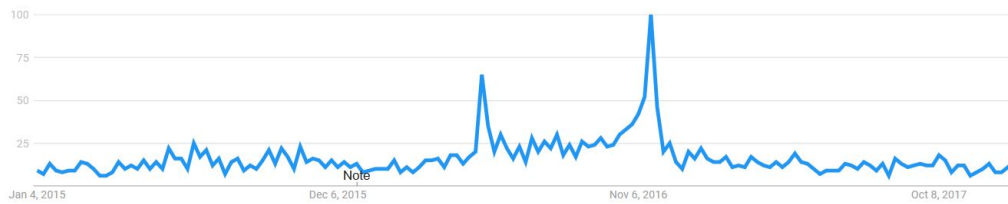
Table 1: Effect on Workers' Base Pay 18 Months After Hire

	(1)	(2)	(3)
Base Pay	-6.10 (3.41)	-6.39 (3.41)	-5.67* (3.08)
Treatment Group FE	Y	-	Y
Event 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

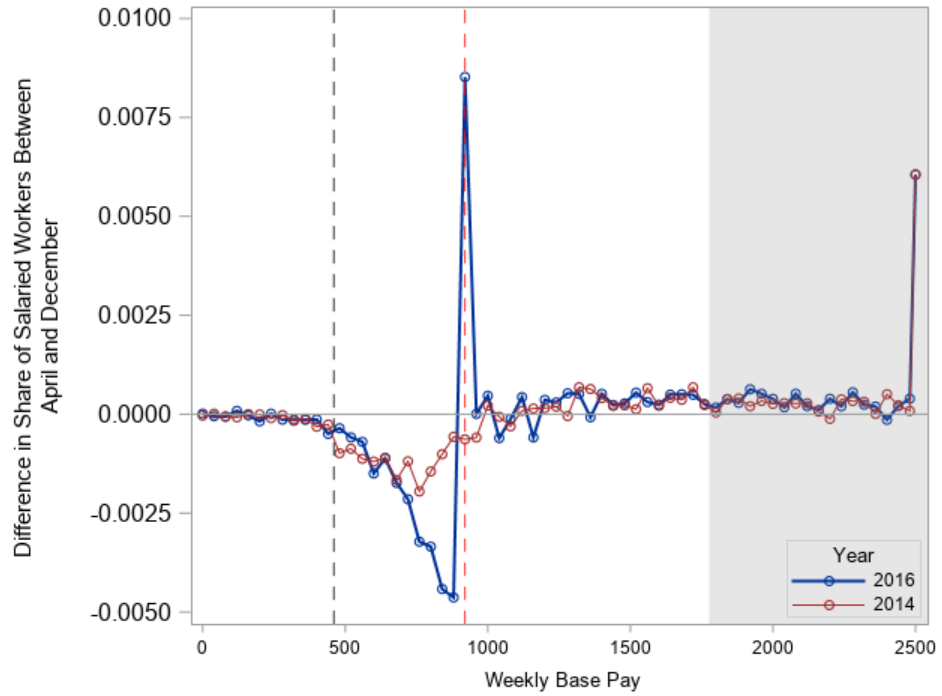
## Appendix A. Supplementary figures and tables noted in the text

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



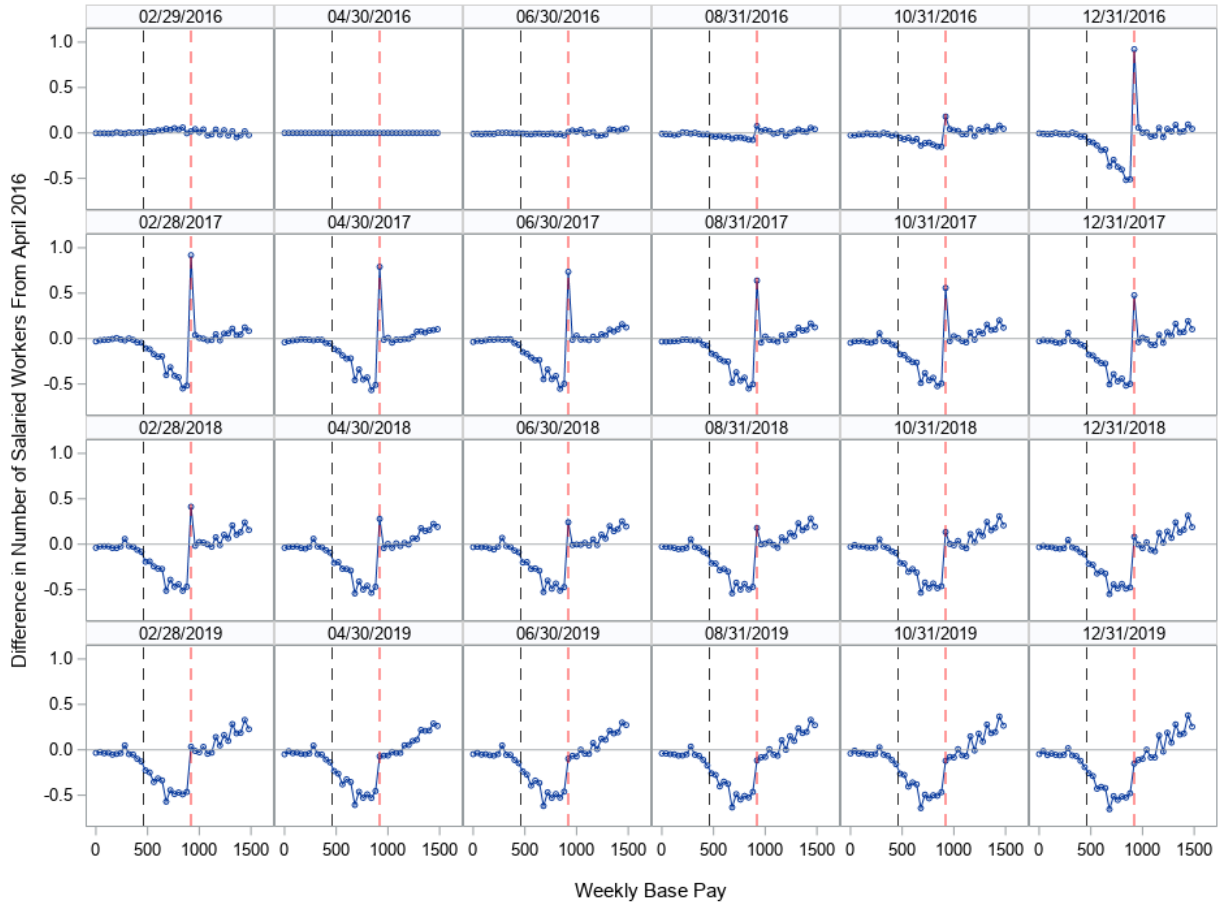
Notes: This figure shows the relative popularity of “FLSA Overtime” as a Google search term between January 2015 and December 2017. A value of 100 indicates its highest popularity level, and the measure of popularity is scaled proportional to this instance.

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.

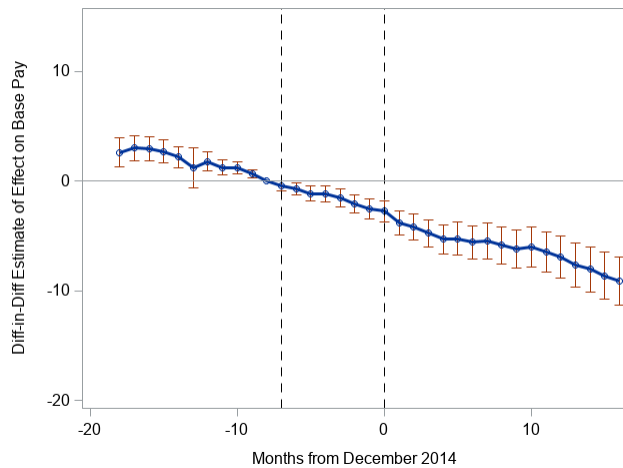
Appendix Figure A.3: Difference in Distribution of Salaried Workers from April 2016



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.

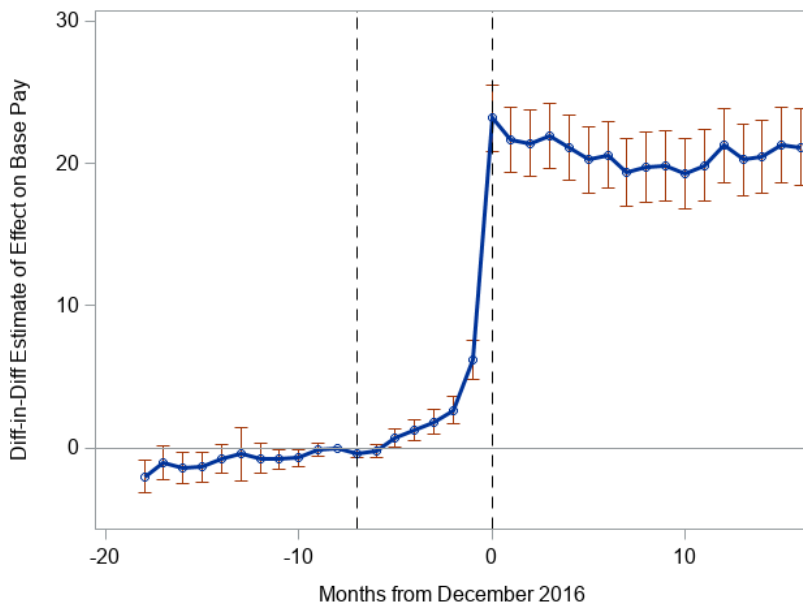


Appendix Figure A.4: Placebo Test of Difference-in-Difference Estimates of Base Pay Effect



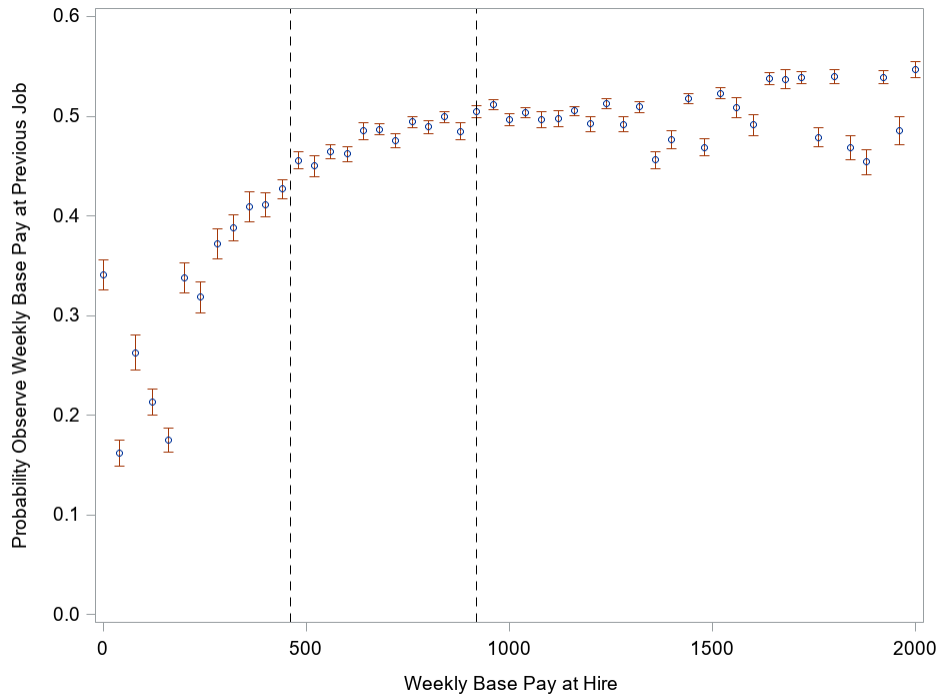
Notes: This figure shows difference-in-difference estimates that compares salaried workers earning within [\$913,953) in December 2014 to those earning [\$953,993). The sample consists of workers who are continuously employed in a salaried position at the same firm between May 2013 and April 2016.

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



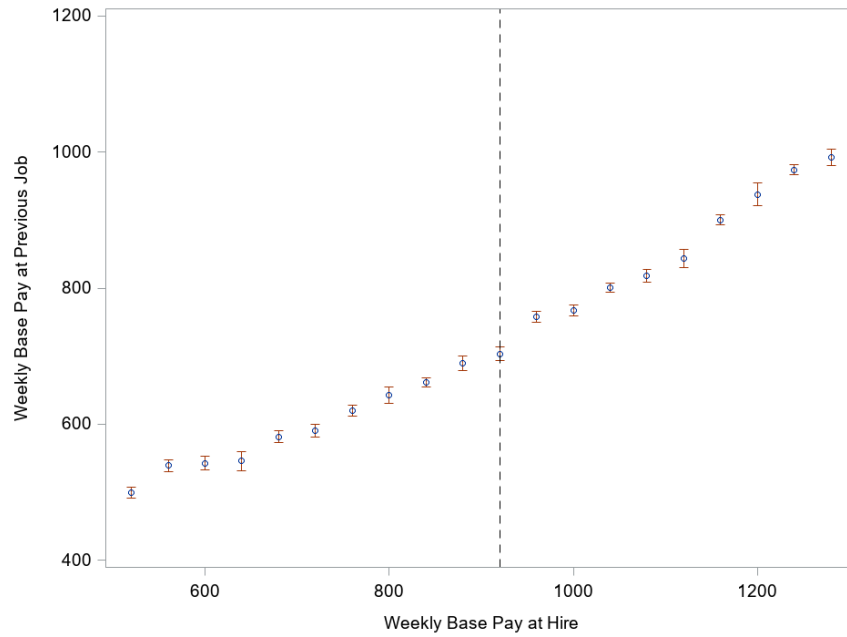
Notes: This figure plots the difference in difference estimates that compares the weekly base pay of salaried job-stayers in December 2016 to salaried-job stayers in December 2014, both of which earned between \$913 and \$955 per week in their respective months. The left vertical dashed line is at May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

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

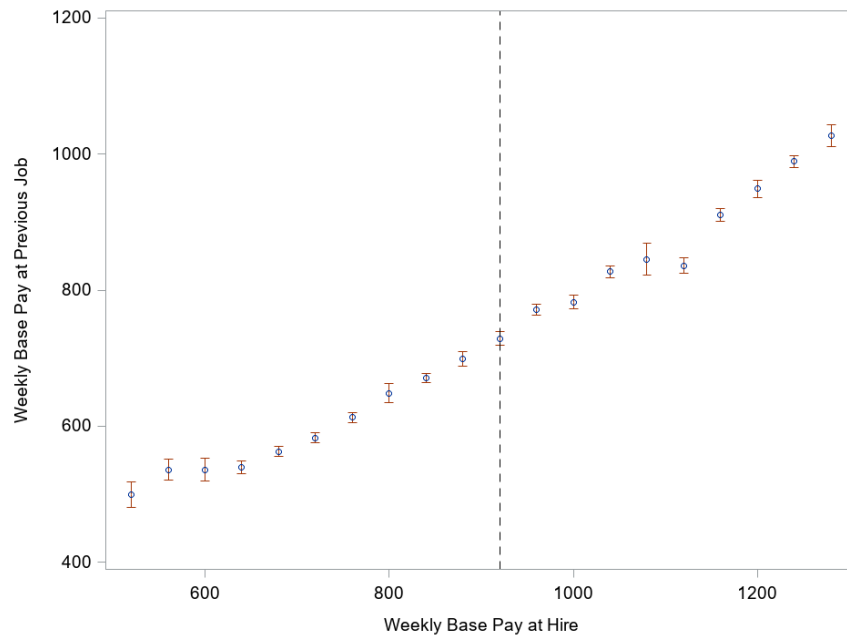


Notes: This figure shows the probability that I observe the salary of a new hire in at least one previous employer, by the entry wages of the new hire. The sample consists of all salaried workers hired between May 2016 and May 2018. The left vertical dashed line is at May 2016, and the right vertical dashed line is at December 2016.

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



(a) May 2016 to May 2018



(b) May 2015 to April 2016

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 2016 and May 2018. Panel (b) restricts the sample to salaried workers hired between May 2015 and April 2016. In both figures, the vertical dashed line is at \$913 per week.