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

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December 12, 2022

Abstract

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

JEL codes: E24, J23, J31

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 nominal wage rigidity inhibits market-clearance during recessions, thereby leading to high unemployment. Among modern economists, the idea that nominal wages seldom adjust downwards continues to be debated as an explanation for the volatility of employment over the business cycle (Shimer, 2004; Hall, 2005; Hall and Milgrom, 2008; Kennan, 2010).[1] Furthermore, the nature of downward nominal wage rigidity has received considerable attention by policymakers concerned with the dynamics of recessions and the ability of inflation targeting to "grease the wheels of the labor market" (Tobin, 1972; Akerlof *et al.*, 1996; Yellen, 2016). Despite its prominence in economic theory and policy, there is no consensus on the degree to which wages are rigid nor its effects on the labor market.

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

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[1]Downward nominal wage rigidity is also a central feature of models in monetary economics (Benigno and Ricci, 2011) and international macroeconomics (Schmitt-Grohé and Uribe, 2016).

[2]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.

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

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

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

Second, I present evidence that employers do not compress wage growth to reduce the present discounted value of wages. Elsby (2009) argued that even if spot wages are downward rigid, a dynamically optimizing firm could compress future wage increases and thereby mitigate any employment effects. Causally testing this hypothesis is difficult as it requires knowledge of the counterfactual evolution of wages in the absence of wage rigidity.[4] I am able

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[4]As evidence that firms adjust dynamically, Elsby (2009) shows that the distribution of real wage changes is more compressed during periods of low inflation, when downward wage rigidity is

to infer a reasonable counterfactual by using the 2016 rule change to generate worker-level variation in the degree to which different employees are paid above their market rate. Applying a difference-in-difference design, I show that the wage growth of incumbent workers who received a raise to the $913 threshold did not change relative to those who were already earning above that cutoff. In contrast to the hypothesis by Elsby (2009), the analysis suggests that firms do not compress workers' wage growth to offset downward rigidity in spot wages.

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

Fourth, having established that employers do not adjust wages downward following the injunction of the new overtime rule, I next show that annual separation rates among bunched workers increased by 1.7 p.p (s.e. 0.6 p.p) from a baseline of 17%. Applying a difference-in-difference design with repeated cross-sections, I find that workers who received a raise to exactly the overtime exemption threshold at the time of the court ruling are more likely to be displaced within a year relative to workers paid above that cutoff. As a placebo check, I find no reduction in separation rates among jobs paid below the new threshold, indicating that firms did not simply bunch individuals who were likely to leave anyways. The increase in job displacements adds to a growing body of evidence on the effects of wage rigidity on employment dynamics. In the context of rural India, Kaur (2019) finds that agricultural employers would rather reduce employment than cut wages in response to negative rain shocks. In de-

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especially binding, than during periods of high inflation. However, the correlation between wage growth and inflation may be confounded by other changes in the labor market over time such as technological progress, globalization, and weakening labor market institutions.

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

veloped countries, papers have examined how firms' adjustments to aggregate demand shocks differ by their baseline propensity to reduce wages (Pischke, 2018; Kurmann and McEntarfer, 2019; Funk and Kaufmann, 2021). At the worker level, Schmieder and Von Wachter (2010) finds that tight labor market conditions during a job-spell is associated with both persistent wage gains at the job and a greater likelihood of layoffs. Compared to previous studies that rely on macro-economic shocks, this paper benefits from an exogenous government policy that generates worker-level variation in wage rigidity and clear counterfactual comparison groups to identify the wage and employment responses to a temporary positive wage shock.

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

In addition to the wage rigidity literature, my paper contributes to studies of labor market hysteresis (Blanchard and Summers, 1986). Research on firm behavior has found that even after a shock or labor market policy has long passed, its effects may still persist. For instance, Miller (2017) finds that the share of black workers in establishments regulated by affirmation action requirements continues to grow even after its deregulation. In the product market, Benzarti *et al.* (2020) shows that prices respond more to increases in value added taxes than to decreases. Perhaps conceptually most similar to my paper is a recent series of studies examining the introduction of a payroll tax cut for young workers in Sweden (Saez *et al.*, 2019), and the persistent increase in youth employment following its retraction (Saez *et al.*, 2021). Relative to these studies, my paper focuses on wages instead of employment, and

shows that it too responds asymmetrically to the expansion and retreat of labor market regulations.

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

2 The 2016 FLSA Overtime Regulation

Under the Fair Labor Standard Act (FLSA), employers in the U.S. are required to pay workers an overtime premium of at least one and a half times their regular rate of pay for each hour worked above 40 in a week. While nearly all hourly employees are covered under this provision, the FLSA permits employers to exempt salaried workers who primarily perform white-collared duties and earn at least the "overtime exemption threshold". Consequently, firms have incentive to bunch salaried employees' base pay right above the threshold to exempt them from overtime. Between 2004 and 2016, this threshold was set at $455 per week ($23,660 per year), or about the 10th percentile of the income distribution of salaried workers in 2016.

On May 18, 2016, the Department of Labor (DOL) announced that it would double the FLSA's overtime exemption threshold from $455 to $913 per week ($47,476 per year), effective December 1, 2016. The goal of the rule change was to expand overtime coverage to low-income white-collared salaried workers such as managers of fast food restaurants and retail establishments. In response to the upcoming regulation, twenty-one states sued the federal Department of Labor on September 26, 2016, arguing that such a large increase in the threshold overstepped the authority of the DOL and requires congressional approval.[6] From a review of newspaper articles at the time, I found that little media attention was given to the development of the court case and those that did warned employers to not expect a

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

ruling before the December 1st deadline.[7]

Hence, it was a surprise to employers when the court ordered a preliminary injunction ten days before the effective date of the rule change, thereby preserving the overtime exemption threshold at $455 per week. By statute, granting an injunction meant the judge believed that the plaintiff was likely to succeed and would suffer irreparable loss without a temporary preservation of the status quo. Despite initial uncertainty about the future of the overtime exemption threshold, it soon became clear that it was highly unlikely for the $913 proposal to ever go into effect. Following the 2016 election, the incoming administration nominated fast-food executive, and critic of the new overtime regulation, Andrew Puzder to be Labor Secretary on December 8, 2016. While Puzder did not receive sufficient support from the Senate to be instated, the next nominee, Alexander Acosta, commented in his confirmation hearing on March 22, 2017 that he believed the overtime exemption threshold should be updated to only around $634 per week. Ultimately, Acosta was confirmed as Labor Secretary and the DOL officially dropped its defense of the Obama-proposed rule change on June 30, 2017. Thus, at the very latest, employers were certain by July 2017 that the overtime exemption threshold would not increase to $913 per week.

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

In this paper, I examine the long-run effects of the 2016 FLSA overtime regulation in the months following its injunction. Since the policy initially caused firms to raise workers' salaries above their market rates, one would expect that in a frictionless environment, firms would simply reverse workers' salaries back to their pre-policy levels immediately after December 2016. Any persistence in bunching would therefore be evidence of wage rigidity.[8] Furthermore, since the rule change only targeted a specific segment of the salary distribution,

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

[8]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.

I am able to use jobs that were already paying above the \$913 threshold as a control group to identify whether firms adjusted the future wage growth, composition, or employment of bunched workers.

3 ADP Data

I use anonymous administrative payroll data from ADP LLC, a global provider of human resources software and services for managing employers' payroll, benefits, and taxes. Their matched employer-employee panel data lets me observe individuals' incomes at a monthly frequency between May 2008 and January 2020 for over a tenth of the U.S. labor force. Previous analyses of the ADP data have found that it closely matches the age, sex, and tenure distribution of workers in the Current Population Survey.[9]

Within the data, I observe monthly aggregates of anonymized individual paycheck information including workers' salaried/hourly status, earnings, hours, pay frequency, industry, and state of employment. In addition, the data records each worker's standard rate of pay, without measurement error, as of the last paycheck in the month. For hourly workers, their standard rate of pay is simply their wage and for salaried workers, it is their base salary per paycheck. This variable allows me to precisely compute the measure of weekly base pay described in the Fair Labor Standards Act to determine employee's overtime exemption status. Following the Department of Labor's guidelines, I define salaried workers' weekly base pay as their salary per paycheck divided by the number of weeks between each paycheck.[10]

I create three sub-samples from the data for my analysis. First, to study the wage rigidity of stayers, I construct a sample of all workers who are continuously employed at the same firm between May 2015 and April 2018. Second, over the same period, I create a sample consisting of all new hires to study the evolution of entry wages. For each new hire, I also merge on the job characteristics from their most recent employer observed in the data so that I can examine how the composition of new hires changed following the 2016 rule change. Given the size of the ADP dataset, I am able to match 47% of new hires with at least one previous employer. Third, to study the effect of the policy on employment dynamics, I create a panel of repeated cross-sections of "wage-changers," defined as stayers who receive a change in their base pay within the past month. Using the last dataset, I am able to determine whether workers who received a raise to the \$913/week overtime exemption threshold experience an

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[9]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).

[10]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.

increase in job-separation rates relative to other wage-changers. In all data samples, I drop workers employed in California, New York, Alaska, and Maine, because these four states have their own overtime exemption thresholds that change after 2016. Moreover, I restrict the samples to continuously operating firms since the entry and exit of firms reflect both business creation/destruction and changes in the composition of ADP's clientele.

4   Wage Rigidity of Stayers

In this section, I examine how the wages of continuously employed salaried workers evolve following the injunction of the FLSA overtime policy in December 2016.

4.1   Are Spot Wages Rigid?

To begin, I present graphical evidence of the immediate response to the reform. In figure 1a, I overlay the distribution of weekly base pays in April and December 2016. The key highlight from the figure is the stark shift in salaries from between the old and new overtime exemption thresholds to preciously right above the $913 cutoff. Besides the bunching at the overtime exemption threshold, there appears to be little change to the rest of the income distribution.[11] To visualize the bunching more clearly, figure 1b plots the change in the distribution between April and December 2016, and compares it to the change in 2014. While jobs in 2014 likewise experience a shift towards the right of the income distribution due to natural wage growth, it does not exhibit the large bunching behavior observed during the year of the policy change.[12]

Next, I use the retraction of the $913 overtime exemption threshold to test for rigidity in spot wages. If spot wages are fully flexible, then employers would decreased workers' salaries back to their pre-policy levels immediately after the injunction of the new threshold. To estimate firms' response to the injunction, I follow a similar identification strategy to Cengiz *et al.* (2019) where I compare the observed density of weekly base pays over time to a counterfactual distribution. However, since the 2016 FLSA policy was a federal rule change that affected all states simultaneously, I am unable to implement a cross-state comparison like those commonly used in studies of the minimum wage. Instead, motivated by figure 1b, I use the distribution in the years prior to the policy change as a counterfactual.

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

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

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

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

where $\bar{n}_{tk}$ is the share of salaried workers in the \$40 bin of base pay k at time t, averaged across all firms in the sample. Standard errors are computed using the delta method and clustered by firm. Intuitively, equation 1 is a difference-in-difference that compares the evolution of the base pay distribution since April 2016 to the evolution of the distribution over the same number of months since April 2014. A limitation of the identification strategy is that it only accounts for seasonal trends that is common across years, but not year-specific shocks. While the method can be easily adapted to address year-specific confounders by using the right tail of the distribution as another control (see Quach, 2020), for simplicity, I apply a straightforward cross-year comparison to focus on the qualitative persistence of the bunching.

Figure 2 shows the distribution of $\hat{\beta}_{kt}$ across base pay, by date. As expected, firms bunched the salaries of stayers at the \$913 threshold in December 2016. However, contrary to the prediction implied by perfect wage flexibility, the spike at the nullified threshold does not immediately dissipate in the proceeding months and is clearly visible even one year after the injunction. While the persistent bunching could indicate that it is costly for firms to cut wages, it is also consistent with firms being uncertain about the legality surrounding the new threshold and do not want to risk a costly law suit.

To test whether employers' beliefs about the potential risks of non-compliance caused them to avoid reducing workers' wages, I use the final court decision in June 2017 as a discrete breakpoint in firms' beliefs. Namely, if wages are flexible and the bunching is simply a reflection of firm's risk aversion, then it should unravel quicker following the final court decision in June 2017.

In figure 3, I plot the evolution of the bunching mass at \$913 per week over time (estimated from equation 1) and find that neither the confirmation hearing of Alexander Acosta in March 2017 nor the final ruling on the FLSA rule change in June 2017 had a noticeable impact on its magnitude. Examining the figure from left to right, I highlight three features. First, the estimated size of the bunching mass is close to zero for the entire year prior to the announcement of the FLSA rule change in May 2016. The parallel pre-trends suggests that the identification strategy uses a reasonable counterfactual for the share of salaried workers earning between \$913 and \$953 per week absent the policy. Second, the share of salaried workers in that interval rose by nearly 1 p.p by January 2017. For comparison,

only about 2% of workers earned between \$913 and \$953 per week in May 2016. The policy therefore increased the number of workers within this interval by 50%. Third, the magnitude of the bunching mass shrank at a constant rate following its retraction. The absence of any discontinuous change in the share of salaried workers earning \$913 per week after key developments surrounding the legality of the rule change suggests that the persistence in the bunching is not due to employers' expectations and uncertainties about the future of the policy.

The constant rate of decrease in the bunching mass also suggests that the persistence of the spike is not due to staggered bargaining contracts. These type of contracts are often used in macroeconomic models to introduce wage rigidity by assuming that salaries are adjusted periodically, rather than instantaneously (Taylor, 1979, 1980; Gertler and Trigari, 2009). Empirically, recent work by Grigsby *et al.* (2019) finds that firms indeed tend to adjust the wages of all workers at the same month each year. I replicate their findings in appendix B where I show that workers tend to only receive one pay increase per year, and these wage adjustments tend to occur in either January or April. Given that a large share of wage adjustments occur in January, if the persistence in bunching is due to staggered bargaining, then I would expect to see a sharp drop in the bunching mass on January 2018. However, figure 3 exhibits no such kink, implying that while staggered bargaining may exist, it alone does not explain the type of rigidity observed after the retraction of the overtime policy.[13]

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.[14] To answer these questions, figure 4 plots the distribution of one-year wage changes for job-stayers who were bunched at the \$913 threshold in December 2016. If wage flexibility is heterogeneous across workers or firms, then at least some workers bunched at the \$913 threshold would experience a large pay decrease in 2017. Similarly, heterogeneity in the timing of wage ad-

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

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

justments due to staggered bargaining would also aggregate to a large contingent of wage cuts over the year. In contrast, figure 4 shows that very few workers experienced a wage cut. Instead, 27% of workers had no change to their base pay in the year following the injunction and the majority of workers even received a raise. This stark asymmetry in the wage change distribution is similar to the result in Grigsby *et al.* (2021) where they plot the distribution of wage changes for a random sample of job-stayers in the ADP data. The rarity of negative wage changes implies that not only is there little heterogeneity in wage flexibility, but also that the decline in the bunching over time is due to workers receiving raises rather than decreases in their base pay.

4.2 Is the Discounted Present Value of Wages Rigid?

In this subsection, I test the hypothesis from Elsby (2009) that employers respond to downward nominal spot wages by compressing future wage increases. In contrast to that theory, I show that the wages of bunched workers continue to grow at the same rate relative to other counterfactual groups. In effect, the discounted present value of labor costs is also downward rigid. For simplicity, I will refer to salaried workers who earn between $913 and $953 in December 2016 as "bunched workers" regardless of their wages afterwards.

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 above between $953 and $993 per week on December 2016, henceforth called "non-bunched workers".[15] These non-bunched workers were not directly affected by the policy or its injunction since they are paid above the overtime exemption threshold. To compare these two groups, I estimate a difference-in-difference regression of the form

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

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

Figure 5a plots the raw evolution of bunched and non-bunched workers' salaries over time. Even though some of the bunched workers would have earned $913 per week regardless of the policy, it is apparent from panel (a) that on average, bunched workers experienced a large

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[15]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.

one-time increase in their base pay on December 2016.[16] In contrast, workers earning between $953 and $993 per week were unaffected by the nullified FLSA rule change. Furthermore, there does not appear to be any indication of a slow down in wage growth for either group following the injunction. To see this more clearly, figure 5b plots the equivalent difference-in-difference estimates computed from equation 2. While the wages of bunched workers grew more slowly than that of non-bunched workers post-injunction, this wage-growth differential was already present before the announcement of the FLSA rule change in May 2016.[17]

In figures 5c and 5d, I show that the persistence in wage growth is robust to alternative choices for the treatment and control groups. First, figure 5c plots the estimates of a difference-in-difference that compares bunched workers in 2016 to similarly defined workers in 2014 (i.e. base pay between $913 and $953 in December 2014). An advantage of the cross-year specification is that the treatment and control groups have similar pre-trends prior to the announcement of the rule change in May 2016, making it easier to visually detect any negative trends after the injunction in December. Nevertheless, I find no indication of a negative change in slope after the court ruling. Second, figure 5d plots the estimates of equation 2 using a narrowly defined treatment group that excludes individuals who were unaffected by the new overtime policy. Specifically, I partition the set of bunched workers into two groups: a treatment group comprising of those who earned less than $913 per week in April 2016 and a control group of those who earned at least $913. By focusing on workers who are most likely to have received a raise as a result of the FLSA rule change, any response to the policy should be more pronounced than in the previous specifications. However, while the estimate of the initial pay raise in December 2016 is more than double that of the previous regressions, there is still no evidence of compressed future wage increases.[18]

To statistically test whether wage growth changed following the injunction, I calculate the slope of the wage growth over three periods: pre-announcement, post-announcement but

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[16]Given the magnitude of the bunching observed in figure 3 and the baseline share of workers earning $913-953 per week, approximately a third of bunched workers would earn less than $913 per week if not for the policy.

[17]As a placebo check, I repeat a similar analysis using workers earning [913,953) and [953,993) per week in December 2014, two years prior to the rule change. Figure A.2 shows that higher income workers experienced faster wage growth in 2014 as well.

[18]The estimates in figure 5d exhibit seasonality that did not exist in the previous specifications. The cyclicality over the calendar year suggests bunched workers affected by the policy change receive pay increases in the second to fourth quarters of the year, whereas those already earning $913 before the announcement of the new overtime exemption threshold receive pay increases in the first quarter. I control for seasonality with calendar month-treatment group fixed effects in appendix figure A.3, and again find no decrease in wage growth following the injunction of the overtime exemption threshold.

pre-injunction, and post-injunction using the following regression:

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

where $time$ is a continuous time variable and D_{bp} is a dummy that equals one for bunched workers during period p. The index p equals 1 for months prior to May 2016, it equals 2 for months between May and December 2016, and it equals 3 for months after December 2016. The estimates of λ_{1p} therefore represent the difference in average monthly wage increases between the bunched and non-bunched workers.

Table 1 reports the estimates of equation 3. The estimates in column (1) suggest that the weekly base pay of bunched workers grew \$0.34 (s.e. 0.06) less per month than non-bunched workers prior to the announcement of the policy. The pre-trend is statistically indistinguishable from the negative \$0.35 (s.e. 0.07) wage-growth differential after December 2016. Moreover, the magnitude of the post-injunction wage differential is economically small. At a rate of \$0.35 decrease in weekly salary per month, it would take nearly 4 years for firms to eliminate the initial raise they had given workers and even longer to equalize the net present value of wages.[19] The persistence in wage growth is robust to a series of alternative specifications. In column (2), I compare workers within the same firm over time and find estimates of a similar magnitude. Following the graphical analysis in figure 5, the third and fourth columns estimate equation 2 using workers earning between \$913 and \$953 per week in December 2014 as the control group. The last two columns refines the treatment group to only include workers earning less than \$913 per week in April 2016 and uses those who earned greater than that amount as a control. In all cases, I find no significant decrease in workers' wage growth post-injunction relative to their pre-announcement trend. Overall, the evidence do not support the predictions raised by Elsby (2009), and suggests that even the discounted present value of wages is downward rigid.

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[19]To compute the initial raise, I multiply the estimate of the differences in time trend during the post-injunction period by the number of months before the injunction. The estimate in the first specification suggests an intent to treat effect on the weekly salaries of bunched workers of about \$1.7 per month. Over the 8 month period pre-injunction, weekly pay therefore cumulatively increased by \$14, whereas the post-injunction trend implies weekly base pay only decreases by \$0.35 per month.

5 Wage Rigidity of New Hires

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

5.1 Are Entry Wages Rigid?

I begin by examining whether firms continue to bunch new hires at the invalidated $913 threshold after the injunction. If the wages of new hires are flexible, then firms would immediately cease hiring excess workers at the new overtime exemption threshold once the policy is retracted. To examine the bunching of entry workers, figure 6 plots the base pay distribution of new hires for each month between January 2016 and December 2017, relative to the distribution in April 2016.[20] In anticipation of the policy change, firms began bunching new hires' salaries at the $913 threshold starting in June 2016. The spike of new hires then reached its peak in November 2016, when the share of workers hired at the threshold was 3 p.p greater than in April 2016. For comparison, only 7.7% of new hires earned within $96.15 above the threshold in April 2016. While it diminishes over time, the bunching of entry wages persists to at least November 2017, a year after the injunction of the threshold and five months after the final court decision.

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

To test for changes in worker composition, I compare the characteristics of new hires at

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

the \$913 threshold over time to that of new hires paid between \$953 and \$993 per week. My analysis assumes that absent the rule change, the productivity of workers hired at the cutoff would have evolved similarly over time relative to workers hired above the cutoff. Formally, I estimate equation 2 using monthly cross-sections of new hires. My primary outcomes are the wages of new hires at their last observed employer and the percent change in base pay from switching jobs. Given that the sample size of new hires is sparse relative to the number of stayer, I aggregate the data into six month intervals centered around the announcement of the new overtime exemption threshold in May 2016.

Figure 7a plots the difference-in-difference estimates over time. Reviewing the figure from left to right, I highlight three points. First, the difference in productivity between workers hired at and above \$913 per week follows the same trend in the months leading up to the announcement of the new FLSA rule change, and even in the immediate months afterwards when firms have yet to fully respond to the policy. The parallel pre-trends lends empirical support for the identifying assumption of the empirical strategy. Second, there is a sharp drop in the productivity of workers hired between November 2016 and April 2017, the months when the bunching of new hires was most prevalent. The decline in productivity, measured by the salary of workers at their last place of employment, suggests that the persistence in new hires' wages reflects real wage growth and not simply compositional changes. Third, I find that the relative productivity of workers hired at the \$913 threshold recovers in the latter half of 2017 when the bunching of new hires has diminished.

Similar to the case of stayers, the gradual reduction in the bunching mass of new hires can simply reflect natural wage growth driving salaries above the overtime exemption threshold. As such, in the long-run, new hires who would have otherwise earned less than the overtime exemption threshold may be earning well above it and thereby included in the control group of the difference-in-difference regression. I therefore do not interpret the convergence in productivity in figure 7a as an indication that employers are becoming better at screening new hires. Instead, given the diminishing relevance of the overtime exemption threshold over time, I focus on the Nov 2016 - April 2017 estimate as the clearest indicator of employers' behavioral response to the overtime reform.

To corroborate the claim that the bunching in new hires reflects downward wage rigidity, appendix figure A.4 examines two additional characteristics of new hires over time within the same difference-in-difference framework. First, figure A.4a plots the impact of the FLSA rule change on new hires' wage gains and finds that workers hired at the overtime exemption threshold experienced a larger pay increase from leaving their previous job relative to hires earning above the threshold. In other words, employers are paying entrants more than they

would have had the policy not been announced, consistent with the argument that the bunching reflects real wage growth and not simply changes in composition. Second, figure A.4b tests whether the share of workers for whom I can observe a previous employment changed following the injunction. Since the data only follows workers across firms if they move between ADP clients, I am unable to observe the previous salary of about half of new hires in my sample. In principle, it is possible that firms are hiring more productive workers in response to the 2016 FLSA policy, but I simply do not observe them because they transferred from non-ADP clients. However, the evidence suggests otherwise as the policy had no discernible effect on the probability that a new employee's previous salary is observable.

5.2   Is the Wage Growth of New Hires Rigid?

Next, I investigate whether firms compressed the wage growth of new employees who were initially hired at an elevated rate. While firms continued hiring workers at the overtime exemption threshold even after it was terminated, they could have offset their labor costs by raising new employees' wages at a slower rate compared to if the policy was never announced. Whether the wage growth of new hires is rigid is important as firms' hiring decisions depend on not only the initial wage of new hires, but also their expected discounted stream of wages (Kudlyak, 2014).

Using the same difference-in-difference framework as above, I test for rigidity in wage growth by comparing the evolution in base pay between workers hired at $913-953 per week to those hired at $953-993 per week. In particular, I estimate equation 2 using the weekly base pay of workers 18 months after their hire date as the outcome variable. To start, I restrict the sample to individuals who stay with the same employer for all 18 months after being hired. To maximize sample size, I allow the sample to differ from the hires for whom I can observe historical wages at past jobs.

Figure 7b plots the regression estimates over time. As validation for the empirical strategy, I find that that the difference in wage growth between entrants hired at and above the $913 threshold was constant from May 2015 to October 2016. Starting in November 2016, it appears that the long-run salary of bunched hires slightly dropped relative to non-bunched hires, and this change persisted for the year afterwards. However, the reduction in wage growth is not only statistically insignificant but also economically small. Even after 18 months, the decrease in base pay is still less than half the initial wage premium implied by figure 7a.

I summarize the empirical evidence on rigidity of entry wages in table 2, and test their

robustness to alternative specifications. Column (1) reports the difference-in-difference estimates corresponding to figures 7a and A.4a where I compare the past work history of new hires at and above the overtime exemption threshold. The estimates imply that bunched workers hired between November 2016 and April 2017 earned on average $42 (s.e. $15) less at their last observed job and earned 8.2% (s.e. 3.2%) more from their job transition compared to workers who would have been hired absent the policy change. The analysis thus suggests that firms are hiring less productive individuals to fill jobs bunched at the threshold, and the wage increase does not simply represent a compositional changes. In column (2), I show that the negative selection of workers is robust to comparing hires within the same firm, albeit the estimates are less precise. To reduce concerns regarding the ability of the data to follow workers transitioning across employers, columns (3) and (4) repeats the analysis using only hires with a short gap between jobs. The direction and magnitude of the estimates remain the similar to the main specification when I restrict the sample to workers who left their last observed employer within the past 6 months. In comparison, column (5) finds small significant effects on the wages of bunched hires 18 months after their employment. Column (6) shows that the rigidity in wage growth is robust to coding zero wages for workers who left before 18 months.

The above difference-in-difference analysis assumes that the FLSA rule change had no impact on the selection of workers hired in the $953-993 weekly pay range, thereby enabling those workers to be a valid control group for bunched hires. While the reform does not directly target workers paid above the overtime exemption threshold, there may nevertheless be spillover effects from the policy elevating wages higher up in the distribution.[21] The bias that arises from such spillovers will depend on which workers benefit from the indirect income effect of the policy. To address concerns that new hires earning right above the overtime exemption threshold are a contaminated control group, appendix C implements a regression discontinuity type design using entrants within a wider income interval to infer the counterfactual productivity of workers hired at the bunching cutoff. Intuitively, even if the policy had spillover effects right above the $913 overtime exemption threshold, these impacts are likely diminished for jobs paid well above that threshold.

In appendix C, I find that prior to May 2016, there is a near linear relationship between new hires' salary at their previous job and their current salary. In contrast, after May 2016, the past salary of bunched hires falls significantly below the level predicted by the past

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

salaries of other jobs within the $500-1300 per week pay range. Moreover, from estimating regression discontinuities by month, I show that the decrease in new hires' productivity occurs at precisely the months that the bunching mass is largest, and the magnitude of the decrease is comparable to the difference-in-difference estimates above. While the complementary regression discontinuity approach is not without its limitations, which I describe in the appendix, it is nevertheless reassuring that both the difference-in-difference and regression discontinuity analyses reach the same conclusion that the rigidity in new hires' wages cannot be accounted for by changes in the composition of workers nor by compressions in future wage growth.

6   Changes in Employment Dynamics

In this section, I examine whether employment dynamics changed after the retraction of the new overtime exemption threshold in December 2016. Given that employers do not adjust wages downward following the injunction of the FLSA rule change, workers who received an exogenous pay increase from the policy may be more sensitive to negative demand shocks afterwards compared to if they had never received the raise. To test whether sticky wages increased the likelihood that workers lose their jobs, I compare the separation rates of bunched workers to non-bunched workers over time.

I start by plotting the monthly separation rates for repeated cross-sections of workers from May 2015 to 2018 in figure 8a. For each month, the figure indicates the probability that workers earning $913-953 per week leaves their employer in that month, and compares it to the separation probability for workers earning $953-993 per week. Visually, there appears to be a 3-month drop in the probability of separations among workers at the bunching threshold starting in December 2016. To formally test the significance of the change in separations, I estimate equation 2 using 3-month time intervals for statistical power. The corresponding difference-in-difference estimates in figure 8b show that while the decline in monthly separation rates in December 2016 is the largest drop in the 3 year sample period, its statistical significance is highly dependent on the choice of the reference period. The short-run employment response to the rule change is therefore negligible relative to usual fluctuations in job displacement rates.

However, there are two limitations to simply comparing monthly separation rates across workers with different base pays in repeated cross-sections. First, as discussed in section 4, the salaries of workers who benefited from the overtime exemption rule continue to grow even after the bunching response in December 2016. As a result, it is unclear that workers

earning \$913-953 per week in 2017 would even be the same group of workers who received the exogenous pay increase and are now being paid above market rate. Second, employment responses may take longer than one month to materialize which, coupled with the first limitation, means increases in longer-run separations may not be observed in the monthly cross-sections. In light of these empirical challenges, I repeat my analysis using annual separation rates as the outcome, and repeated cross-section of "wage-changers" as the sample. Specifically, I restrict each month of the data to only individuals who received a raise from the previous month. If the persistent increase in workers' wages did in fact increase layoffs, then I would expect separation rates to increase for precisely workers who received a raise in December 2016 to \$913-953 per week.

Figure 8c plots the probability that a worker is no longer employed with the same firm after a year, conditional on receiving a raise in the previous month. Prior to November 2016, I find that wage-changers earning \$913-953 per week had very similar separation rates as those earning \$40 above that range. The separation rates then diverged in November and December 2016 when wage-changers bunched at the overtime exemption threshold experienced an increase in annual separations. The two groups' separation rates converge immediately in January 2017 and follow a relatively similar trend afterwards, with the exception of an outlier in July 2017. I show in appendix figure A.5 that the outlier is driven by firms that experience a 100% annual turnover rate. In order to account for the outliers while keeping the full sample, I add firm-date fixed effects to the difference-in-difference regression in equation 2.[22] Plotting the estimates over time, figure 8d shows that the increase in job displacement is unique to the months when firms were bunching workers' salaries at the overtime exemption threshold in anticipation of the FLSA rule change. Taken together, the precise overlap between the timing of the FLSA rule change and the months in which job displacement increased among workers earning \$913-953 per week highly suggests a causal relationship between the two events.

Table 3 reports the difference-in-difference estimates of the increase in separations from November 2016 - January 2017, and tests their robustness to alternative specifications. In column (1), I report the effect of the FLSA rule change on monthly separation rates using the full sample of workers, analogous to figure 8b. The estimates suggest that job displacements between November 2016 and January 2017 increased between -0.23 p.p to 0.03 p.p, where average separation rates was 1.9% in the year leading up to May 2016. While the estimates are

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[22] Appendix figure A.5 shows that the result is robust to excluding firm-date fixed effects, albeit the estimates are noisier. Alternatively, the appendix also shows that the separation effect is precisely estimated and fairly similar if I simply drop firms that experience a 100% annual separation rate at any point in the sample.

suggestive of a fall in short-run separation rates, column (2) shows that they are not robust to restricting the sample to only wage-changers who just received a pay increase. Overall, the evidence is inconclusive as to the short-run employment response to wage rigidity. In contrast, the analysis provides a clear picture for the medium-run employment effects. Column (3) finds that annual separation rates increased by 0.52-2.88 percentage points on a baseline rate of 16.6%, equivalent to a 3-17% increase in separations. Column (4) shows that the results are robust to comparing workers within the same firms, as illustrated in figure 8d.

Similar to the analysis of new hires in section 5, one concern with comparing repeated cross-sections is that the composition of workers changes over time. For example, appendix figure A.6a shows that the number of workers who received a raise to $913-953 per week increased drastically in December 2016. As a result, the rise in annual separations among the December 2016 cohort could simply be due to a compositional change whereby employers bunched workers who already have a high propensity to separate. To explore the effect of the FLSA rule change on worker composition, appendix figure A.6b compares the separation rate of wage-changers earning $750-913 per week to those earning $953-993 per week. Intuitively, if bunched workers were selected from those who already had above average likelihood of leaving the firm, then the average separation rate among the remaining workers who did not receive a raise should go down. In contrast, appendix figure A.6b shows that the separation rate of workers paid below the nullified overtime exemption threshold did not fall relative to that of workers earning $953-993 per week.[23] In fact, the estimates, which I report in the last column of table 3, rules out decreases in separation rates greater than 0.18 p.p from a baseline rate of 18%. The placebo test therefore suggests that the increase in separations is not driven by selection in the type of workers who received a raise to the overtime exemption threshold.

7   Discussion and Conclusion

I conclude by relating the wage dynamics observed in this paper to theories of wage rigidity presented in the literature. First, economists have incorporated wage rigidity into macro models by assuming that contracts are renegotiated periodically (Taylor, 1979, 1980; Christiano *et al.*, 2005; Gertler and Trigari, 2009). While I find evidence that employers indeed adjust the wages of workers only once per year, the bunching mass persists for at least 1.5 years with no indication of convergence. The long-run rigidity in both spot wages and wage

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[23]While my analysis uses the finding from Quach (2020) that, absent the policy, bunched workers would have earned between $750 to $913 per week, the conclusion is robust to restricting the sample to a smaller interval below the $913 threshold.

growth suggests that staggered bargaining alone cannot explain the wage dynamics observed in the data. Second, another source of wage rigidity discussed in the literature is the existence of implicit contracts (Beaudry and DiNardo, 1991; Schmieder and Von Wachter, 2010). In long-term employment relationships, initial labor market conditions may have persistent effects as employers and workers agree to a future stream of payments that insures risk-averse workers against wage declines. However, since implicit contracts depend on the initial conditions at hire, and not on contemporaneous conditions, they alone do not rationalize why I observe firms continue bunching new hires at the overtime exemption threshold even after it is no longer binding.

One argument for the persistence in wages is simply that firms increased the hours of bunched workers to compensate for the additional labor costs. Unfortunately, the hours of salaried workers are not observed in the data. However, even if hours did increase, it does not take away from the broader implications that wages are rigid and that this rigidity contributed to increased layoffs. Instead, the hours adjustment is simply another margin of response to wage rigidity, whereby employers were initially maximizing profits at $h^*(w^*)$ and, as a result of the policy, their optimal hours changed to $h^*(w^* + p)$. Given that separation rates of bunched workers nevertheless increased, any changes to hours did not prevent the displacement of jobs that presumably had positive surplus to employers prior to the policy.

A simple explanation for the persistence in spot wages, wage growth, and entry wages is the existence of relative pay concerns among workers. A growing body of work has shown that perceptions of unfair pay differences among peers reduces workers' job satisfaction (Card *et al.*, 2012), retention (Dube *et al.*, 2019), and effort (Breza *et al.*, 2018). Survey evidence suggests that these fairness concerns lead employers to abstain from lowering wages since pay cuts would also decrease worker productivity (Akerlof and Yellen, 1990; Campbell and Kamlani, 1997; Kaur, 2019). In the context of this paper, equity considerations could explain why employers do not revert salaries back to their pre-policy levels immediately after the injunction.

I present two pieces of evidence that such relative pay concerns can also explain the rigidity in wage growth and the rigidity in entry wages. First, if workers' effort is negatively impacted by increases in inequality (Breza *et al.*, 2018), then firms would want to give similar pay increases to all workers. In particular, bunched workers would expect to receive the same pay increase as non-bunched workers in 2017, even though they received an above normal raise in the previous year. As evidence that pay raises are highly correlated within firm, appendix figure A.7 plots the distribution of annual wage increases for bunched workers, relative to the modal wage increase in each workers' employer. I find that over 30% of

workers receive the same pay raise as their peers, and they are very unlikely to receive a smaller pay increase than the mode in the firm. The stark asymmetry in the distribution is highly suggestive that workers care about how much their pay increases relative to their coworkers.[24] Second, relative pay concerns would incentivize employers to pay new hires at the same rate as existing employees. In support of that theory, appendix figures A.8 and A.9 show that the bunching of new hires at the overtime exemption threshold only occurs in firms that are also bunching incumbents. Overall, the empirical evidence is consistent with the argument that horizontal pay equity may drive the wage dynamics observed in this study.

To summarize, this paper studies the extent of downward nominal wage rigidity by examining firms' response to the retraction of an overtime policy that had raised affected workers' salaries above their market rates. The policy, which was set to go into effect on December 2016, would have granted overtime coverage to all salaried workers earning less than $913 per week. Although the reform was never binding, employers nevertheless raised and bunched workers' salaries at the anticipated threshold. Consistent with the existence of downward sticky wages, I show that firms do not revert stayers' wages back to their pre-policy levels following the injunction of the policy, nor did they compress bunched workers' future wage growth relative to workers unaffected by the rule change. Similarly, employers continued to bunch the salaries of new hires at the nullified threshold without slowing their wage growth. Comparing the work history of bunched and non-bunched hires, I find that the bunching cannot be explained by changes in the composition of new entrants. Taken together, these results provide evidence that the present discounted wages of both stayers and new hires are both highly rigid. Instead of adjusting wages, the analysis finds that employers increased the separation rate of workers who received an exogenous pay increase from the retracted policy. The results in this study are consistent with a micro-economic view that workers carry relative pay concerns, and supports the macro-economic view that wage rigidity contributes to increased layoffs.

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[24]To show that the asymmetry is not simply due to firms that have a modal pay increase of 0, I find that the distribution is remarkably similar even after dropping such firms.

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Figure 1: Change in the Density of Base Pay Between April and December 2016
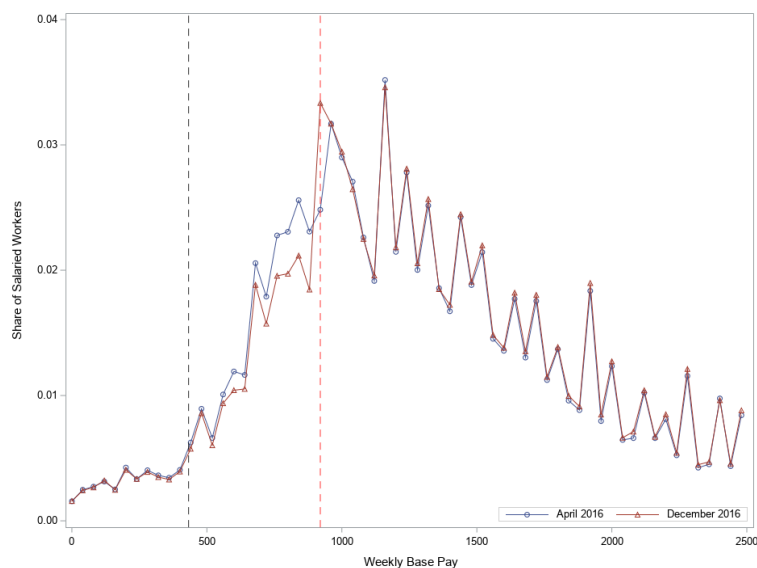
(a) Raw Averages
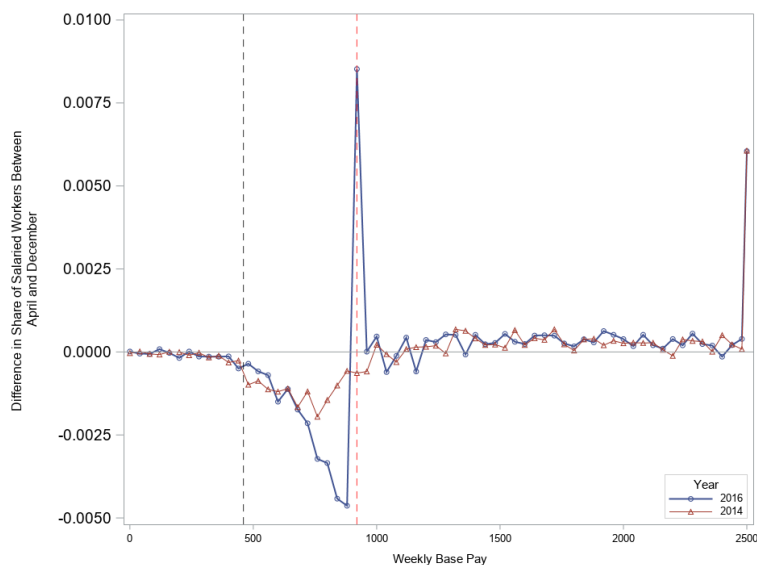
(b) Difference in Distribution

Notes: In panel (a), the blue and red lines show the density of base pay in April and December 2016, respectively. The sample is restricted to salaried workers who are continuously employed at the same firm from May 2015 to April 2018. In panel (b), the blue (red) line shows the difference in the density between April and December of 2016 (2014). The black vertical dashed line is at $433 and the red vertical dashed line is at $913 per week.

Figure 2: Effect on Distribution of Continuously Employed Salaried Workers Relative to April 2016
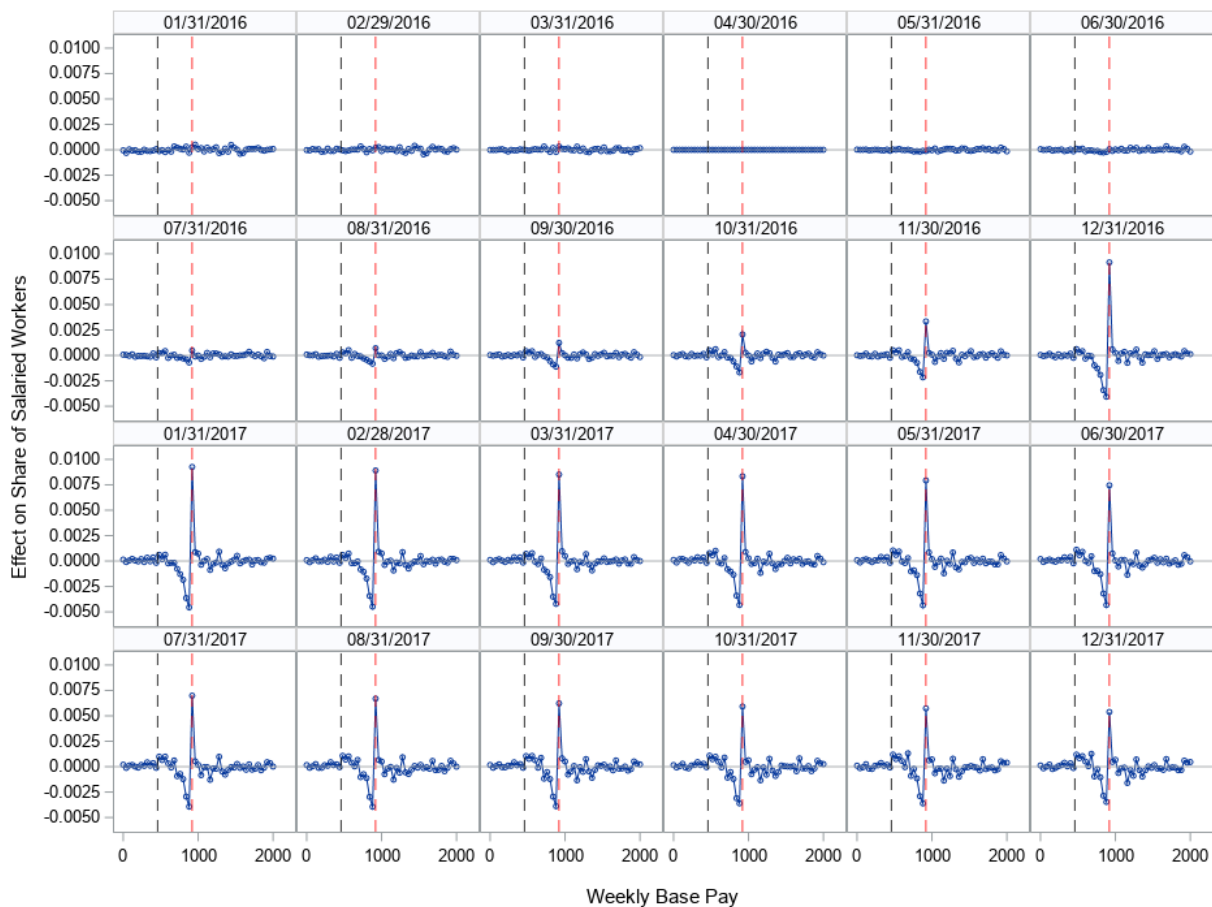
Notes: This figure shows difference-in-difference estimates that compare the changes to the base pay distribution since April 2016 to changes over the same number of months since April 2014. The x-axis is divided into \$40 increments, where the black vertical dashed line is at \$433 and the red vertical dashed line is at \$913 per week. The sample consists of workers who are always salaried and continuously employed at the same firm between May 2015 and April 2018.

Figure 3: Share of Workers Bunched Over Time
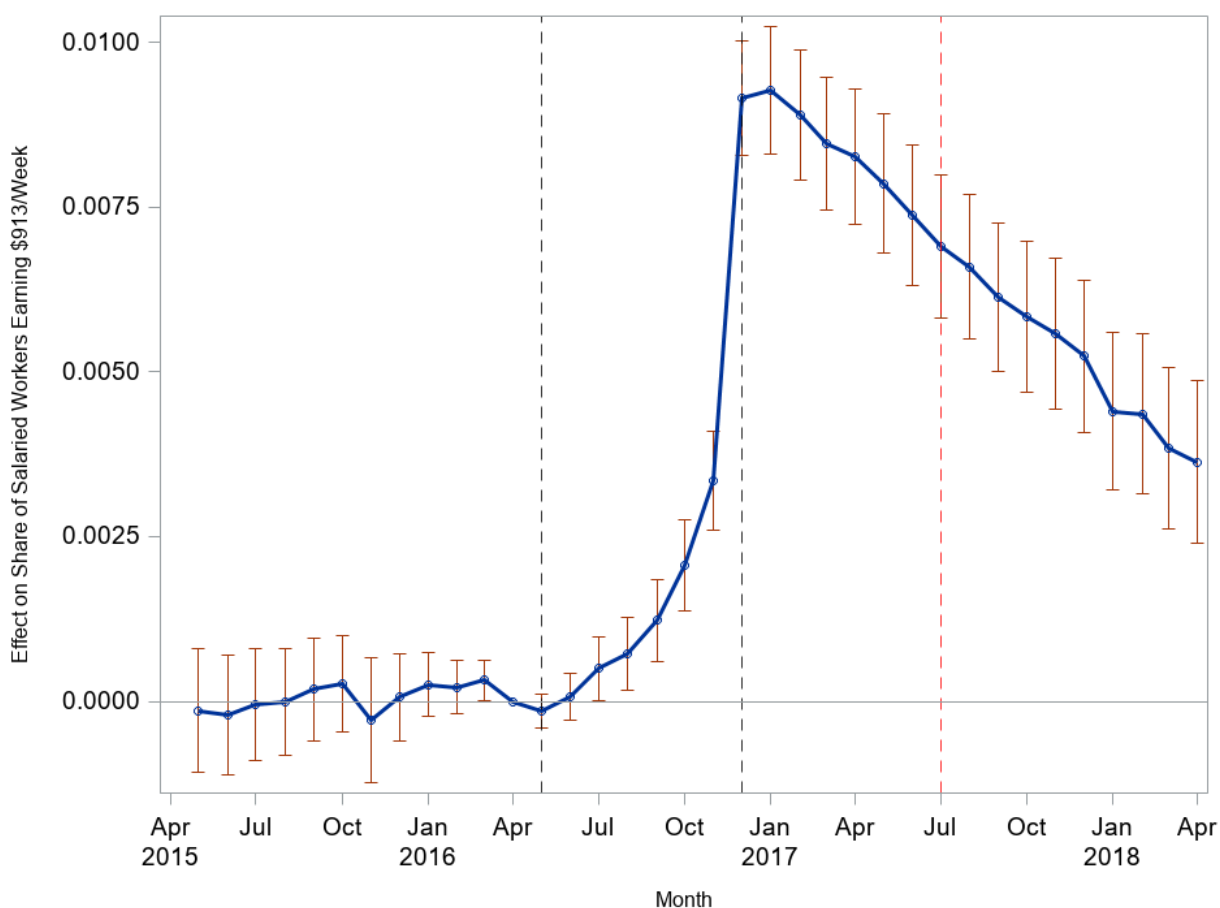
Notes: This figure shows the effect of the 2016 FLSA policy on the share of salaried workers earning between \$913 and \$953 per week, estimated from equation 1. The sample consists of workers who are always salaried, and continuously employed at the same firm between May 2015 and April 2018. The first vertical dash line is at May 2016 when the \$913 threshold was first announced, the second vertical dash line is at December 2016 when the threshold was supposed to go into effect, and the third red dashed line is at July 2017 which is the first month after the Department of Labor dropped their defense of the \$913 threshold.

Figure 4: Distribution of One-Year Change in Base Pay for Workers Bunched in December 2016
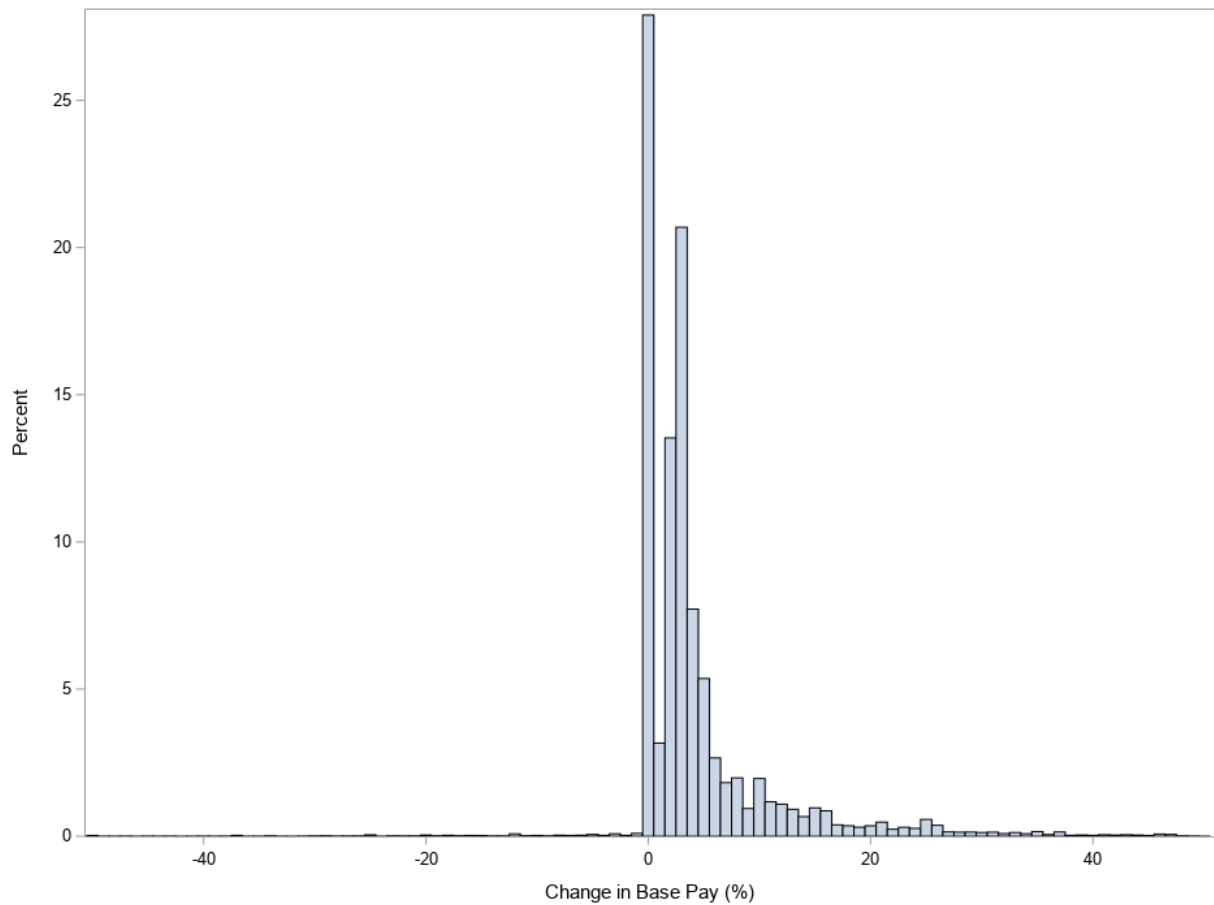
Notes: This figure shows the distribution of the percent change in weekly base pay between December 2016 and 2017. The sample consists of salaried workers who are continuously employed at the same firm from May 2015 to April 2018, and earned between $913 and $953 in December 2016.

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

(a) Raw Averages

(b) Control: $953-993 in Dec 2016

(c) Control: $913-953 in Dec 2014

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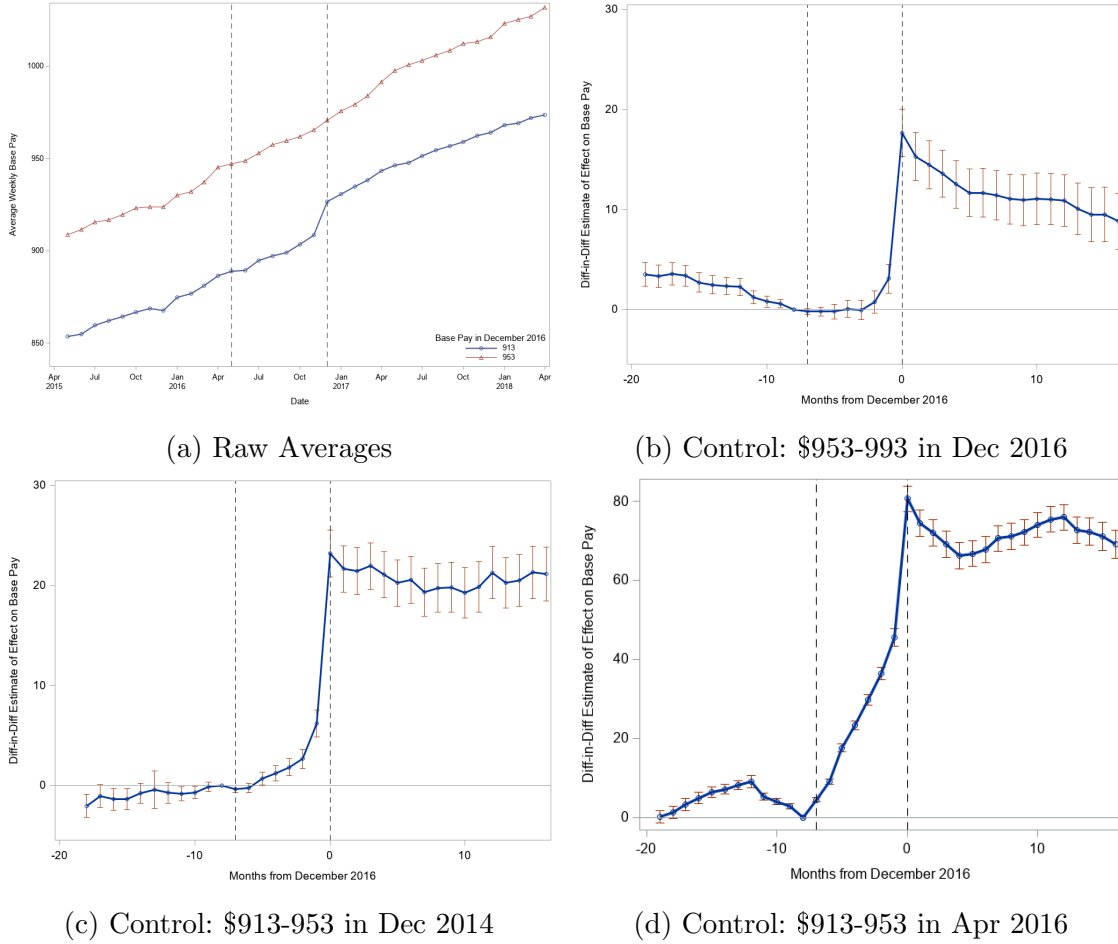
Notes: Panel (a) shows the evolution of weekly base pay over time for salaried workers who earned within [913,953) and [953,993) per week in December 2016. Panel (b) plots the difference-in-difference estimates of the two lines in panel (a), with the treatment group being workers in the [913,953) bin.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

Figure 6: Distribution of New Hires Over Time Relative to April 2016
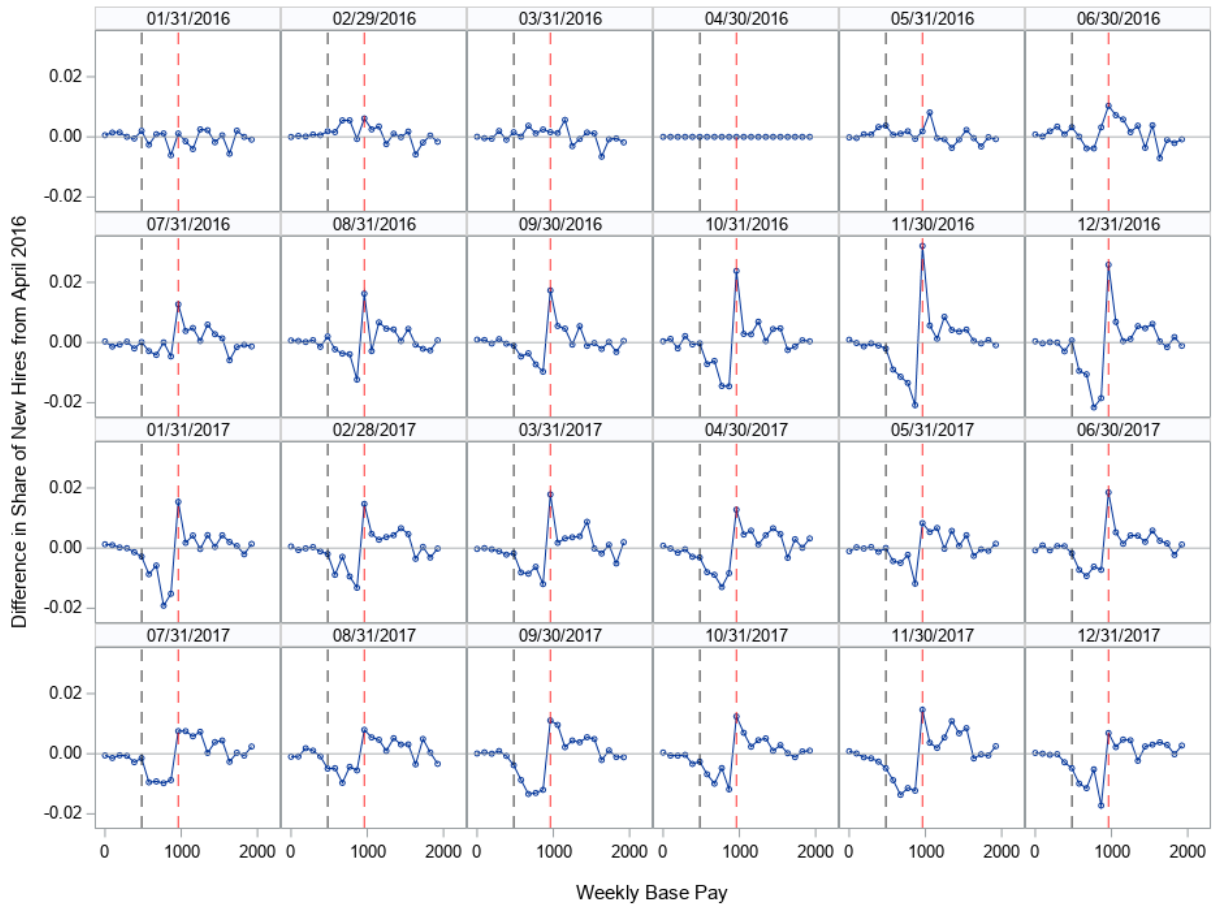
Notes: This figure shows the share of new salaried hires within $96.15 increments of weekly base pay relative to the share in April 2016. In each panel, the black vertical dashed line is at $432 and the red vertical dashed line is at $913.

Figure 7: Difference-in-Difference Estimates Comparing New Hires Earning At and Above the Overtime Exemption Threshold
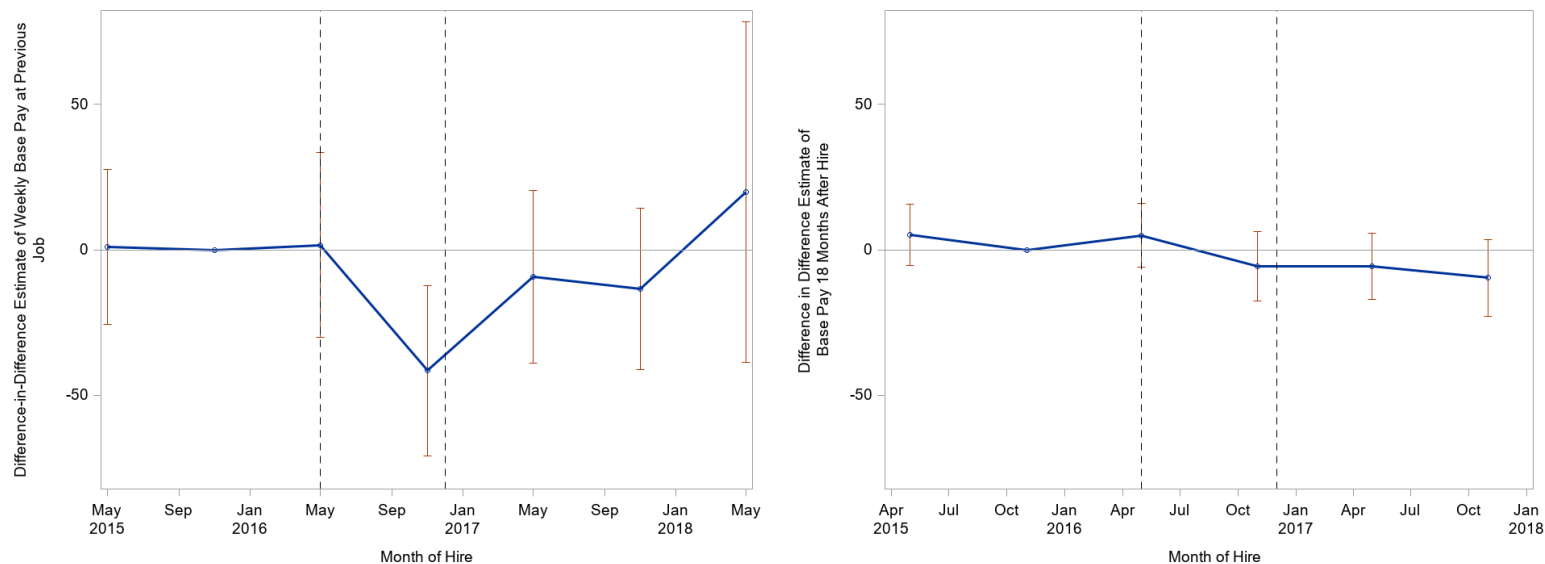
(a) Outcome: Pay at Previous Job

(b) Outcome: Pay 18 Months After Hire

Notes: This figure plots the estimates from a difference-in-difference regression that compares repeated cross-sections of new hires employed at a base pay of $913-953 per week to those hired at $953-993 per week. Each estimate is averaged over 6 months of data. Panel (a) uses workers' base pay at their last observed employer as the outcome, and panel (b) uses workers' base pay 18 months after hire conditional on continuous employment. The sample is restricted to workers hired between May 2016 and May 2018. In both figures, the left vertical dashed line is at May 2016, and the right vertical dashed line is at December 2016.

Figure 8: Comparing the Probability of Separation by Workers' Base Pay Using Repeated Cross-sections

(a) May 2015 to April 2016

(b) May 2016 to May 2018

(c) May 2016 to May 2018

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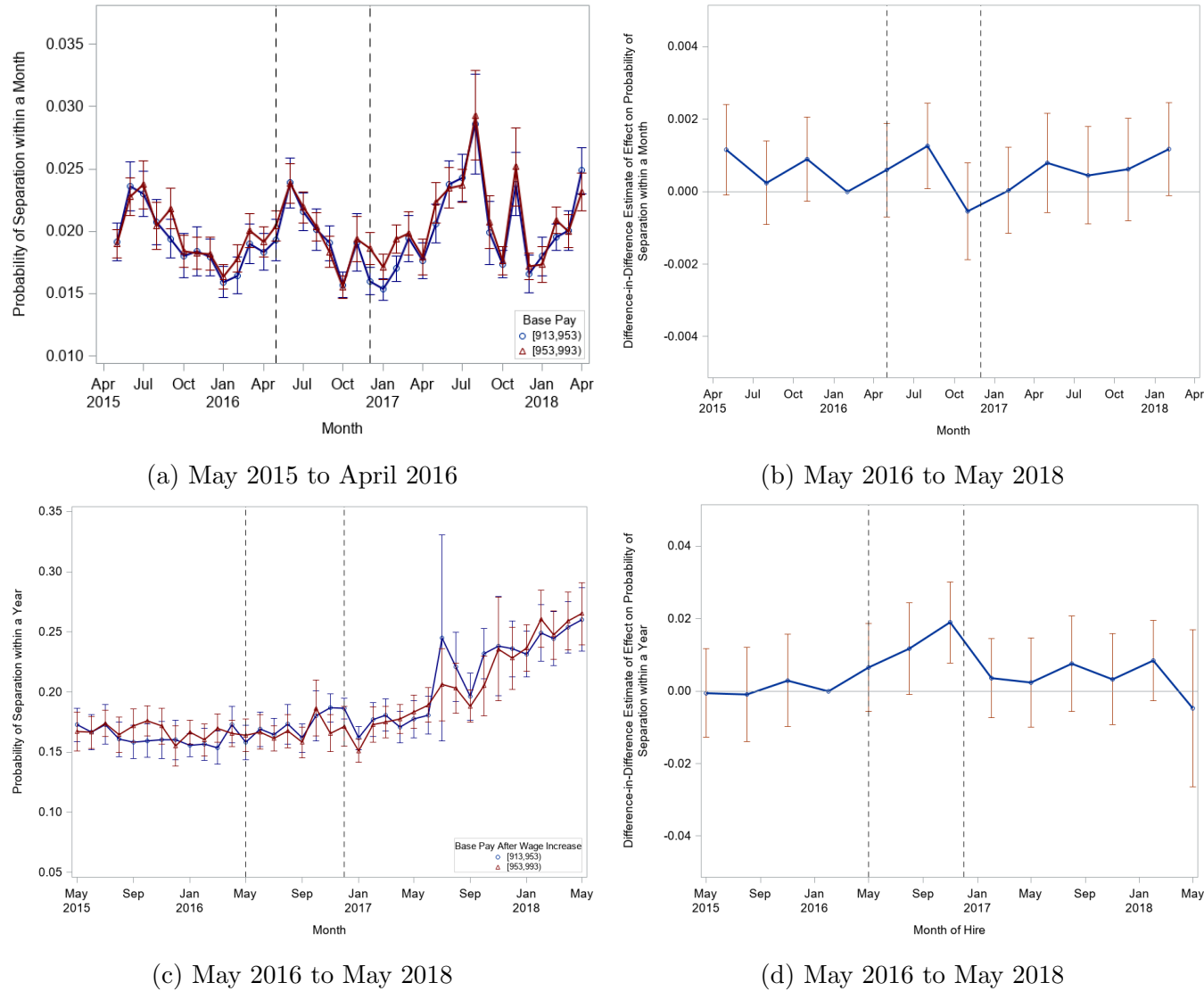
Notes: Panel (a) plots the monthly job separation rate between May 2016-2018 for workers earning $913-953 and $953-993 per week. Panel (b) plots the equivalent difference-in-difference estimates aggregated at the quarterly level, using workers earning $953-993 as the control group. Panel (c) plots the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

Table 1: Effect on Stayers' Base Pays Over Time

	(1)	(2)	(3)	(4)	(5)	(6)
Pre-trend	-0.341	-0.348	0.148	0.020	0.114	-0.165
	(0.057)	(0.047)	(0.058)	(0.050)	(0.079)	(0.070)
Anticipation	1.718	1.243	2.426	1.707	9.271	8.618
	(0.133)	(0.086)	(0.125)	(0.092)	(0.164)	(0.129)
Post-trend	-0.348	-0.313	-0.031	0.058	0.115	0.056
	(0.066)	(0.028)	(0.066)	(0.032)	(0.091)	(0.042)
Bunched FE	Y	Y	Y	Y	Y	Y
Date FE	Y	-	Y	-	Y	-
Firm-Date FE	-	Y	-	Y	-	Y
N	3,085,171	3,085,171	2,896,568	2,896,568	1,624,448	1,624,448
Control Group	$953	$953	Y2014	Y2014	$913 Apr2016	$913 Apr2016

Notes: This table reports the change in the weekly base pay of stayers bunched at the $913 threshold in December 2016 over three time periods: May 2015 - April 2016 (Row 1); May 2016 - December 2016 (Row 2); and Jan 2016 - April 2017 (Row 3). Columns (1) and (2) compare bunched workers to workers who earn between $953-993 per week in December 2016. Columns (3) and (4) compare bunched workers in December 2016 to workers who earned $913-953 per week two years prior in December 2014. Columns (5) and (6) compare bunched workers who earned less than $913 per week in April 2016 to workers already earning at least $913 in that month. All workers in the sample are continuous employed throughout the study period. Estimates are computed from equation 3. All robust standard errors in parentheses are clustered by firm. *10%,** 5%,*** 1% significance level.

Table 2: Effect on the Composition and Wage Growth of New Hires

	(1)	(2)	(3)	(4)	(5)	(6)
Base Pay in Previous Job	−41.520**	−38.056*	−35.757*	−52.268		
	(14.667)	(23.045)	(18.680)	(34.152)		
%Δ Base Pay	0.082**	0.053	0.027	0.012		
	(0.032)	(0.048)	(0.037)	(0.071)		
Base Pay After 18 Months					−5.639	14.325
					(5.945)	(16.968)
Bin FE	Y	Y	Y	Y	Y	Y
Hire Date FE	Y	Y	Y	Y	Y	Y
Firm-Date FE	-	Y	-	Y	-	-
N	49,017	49,017	18,375	18,375	40,863	66,803
Sample	Any Past	Any Past	6 Month Past	6 Month Past	Any Post	All

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

Table 3: Effect on the Probability of Separations

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

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

Appendix A. Supplementary figures and tables noted in the text

Appendix Figure A.1: Google Search Popularity for the Term "FLSA Overtime"
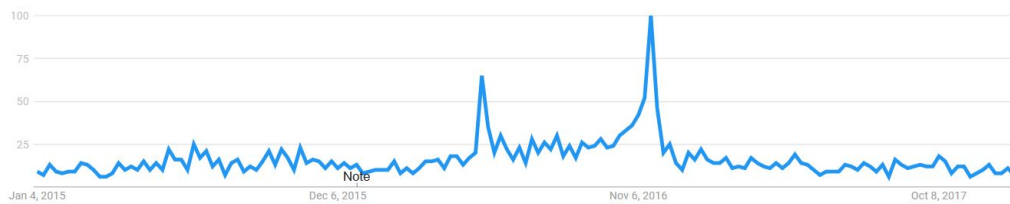
Notes: This figure shows the relative popularity of "FLSA Overtime" as a Google search term between January 2015 and December 2017. A value of 100 indicates its highest popularity level, and the measure of popularity is scaled proportional to that instance.

Appendix Figure A.2: Placebo Test of Difference-in-Difference Estimates of Base Pay Effect
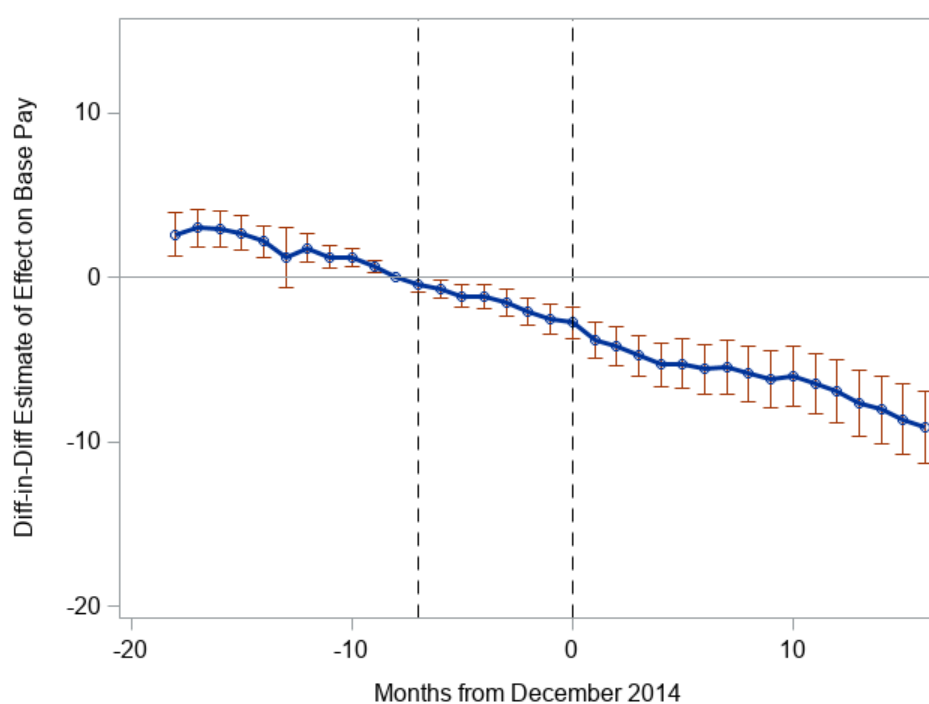
Notes: This figure shows difference-in-difference estimates that compares salaried workers earning within [$913,953) in December 2014 to those earning [$953,993). The sample consists of workers who are continuously employed in a salaried position at the same firm between May 2013 and April 2016.

Appendix Figure A.3: Difference-in-Difference Estimates of Base Pay Effect, Controlling for Calendar Month-Bunched Group Fixed Effects
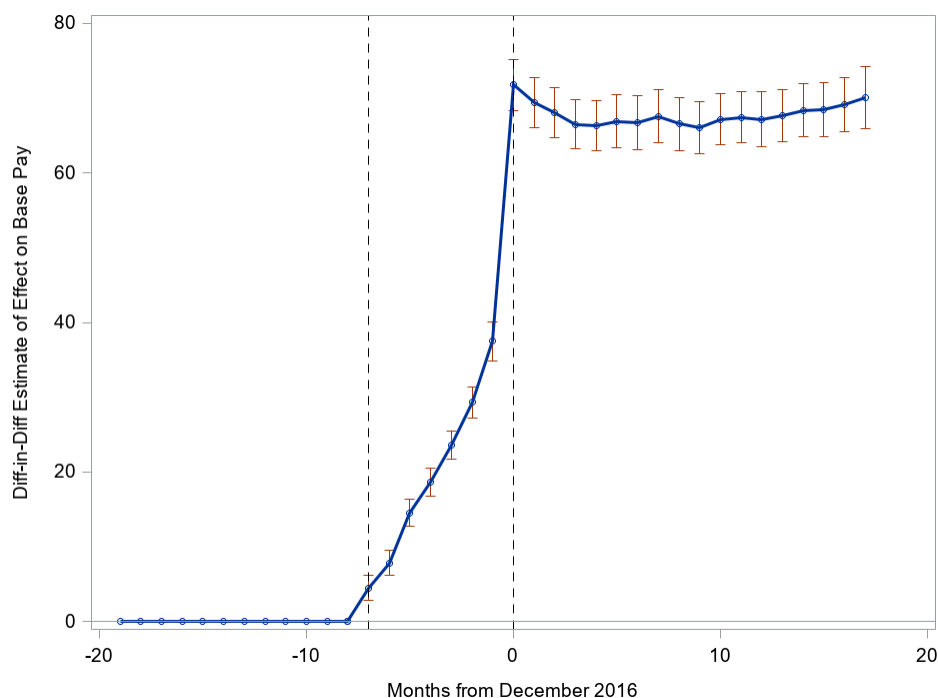
Notes: This figure shows difference-in-difference estimates that restricts the sample to salaried workers earning within [$913,953) in December 2016, and compares those earning less than $913 in April 2016 to those earning more. The sample consists of workers who are continuously employed in a salaried position at the same firm between May 2013 and April 2016. Estimates are computed from equation 2 with the inclusion of calendar month-treatment group fixed effects.

Appendix Figure A.4: Difference in Difference Estimates for New Hires: Change in Base Pay from Job Transition and Probability of Observing Previous Employment
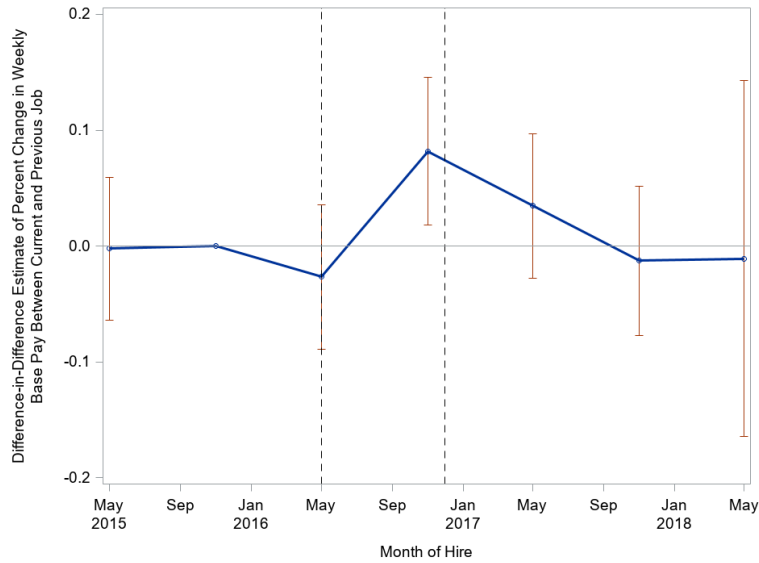
(a) May 2016 to May 2018
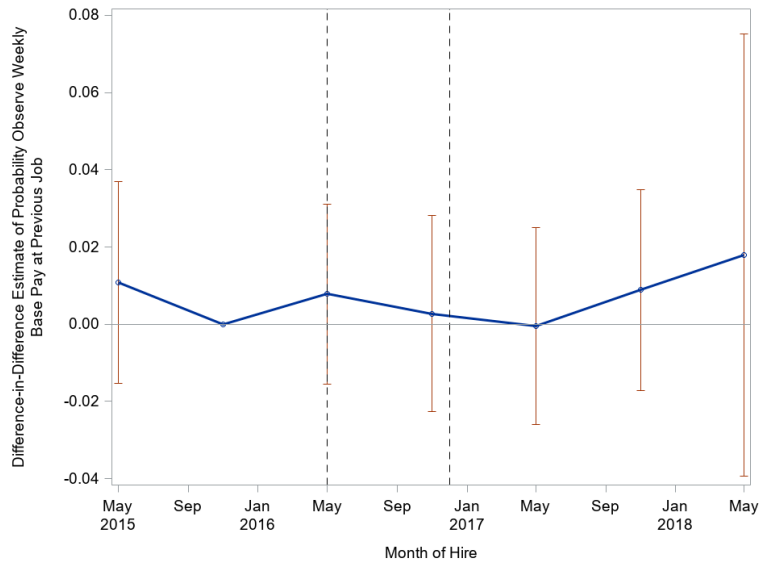
(b) May 2015 to April 2016

Notes: This figure plots the estimates from a difference-in-difference that compares repeated cross-sections of new hires employed at a base pay of $913-953 per week to those hired at $953-993 per week. Each estimate is averaged over 6 months. The outcome in panel (a) is the percent change in base pay from each worker's last observed employer to their current one. The outcome in panel (b) is an indicator for whether the data contains any information on a new hire's previous employer. The sample is restricted to workers hired between May 2016 and May 2018. In both figures, the left vertical dashed line is at May 2016, and the right vertical dashed line is at December 2016.

Appendix Figure A.5: Difference in Difference Estimates for Separations of Wage Changers
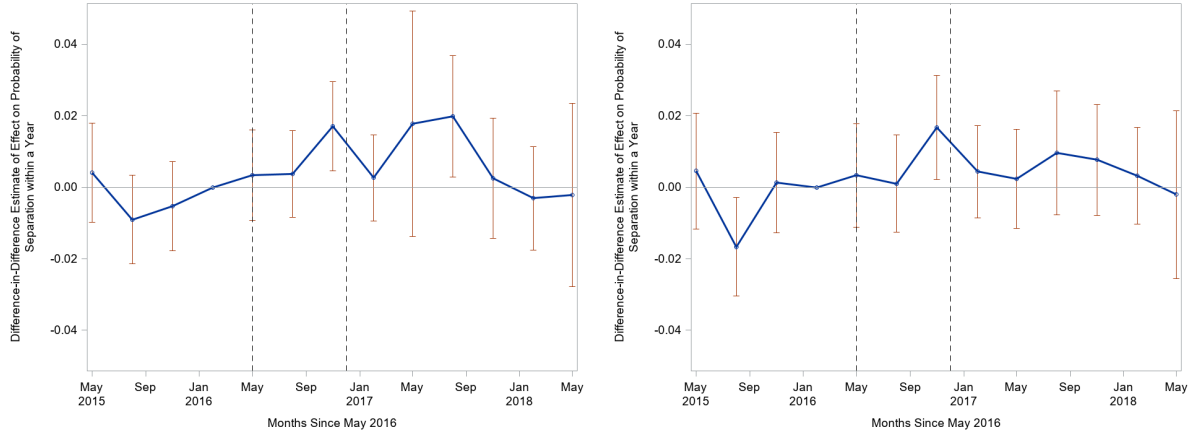
(a) All Firms

(b) Low Turnover Firms
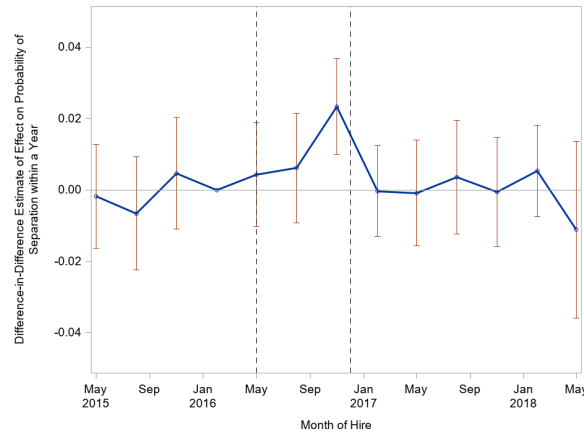
(c) Low Turnover Firms, Firm-Date FE

Notes: Panel (a) plots the estimates from a difference-in-difference that compares the annual job separation rate of workers who received a raise to $913-953 per week relative to those earning $953-993 per week using repeated cross-sections. Panel (b) plots the analogous figure after dropping firms that experience a month in which no worker in that month stays for another year. Panel (c) uses the same sample as panel (b) and controls for firm-date fixed effects. In all panels, the left vertical line is at May 2016 and the right is at December 2016.

Appendix Figure A.6: Placebo Test for Selection of Wage Changers
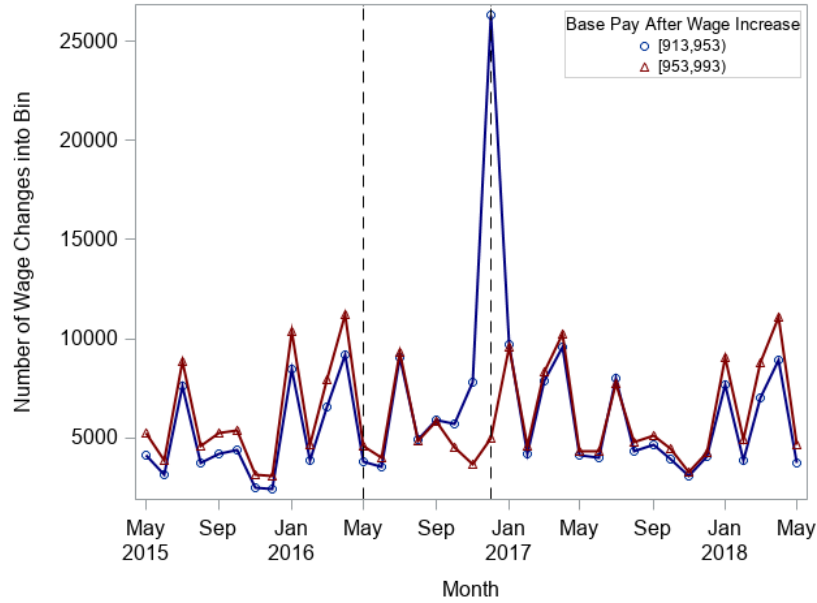
(a) May 2015 to April 2016
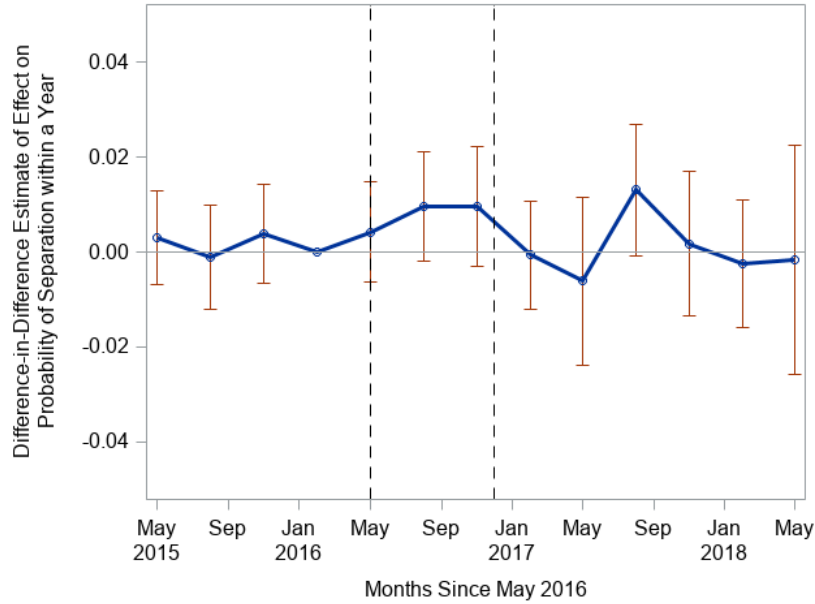
(b) May 2015 to April 2016

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

Appendix Figure A.7: Change in Base Pay Relative to Modal Change within Firm
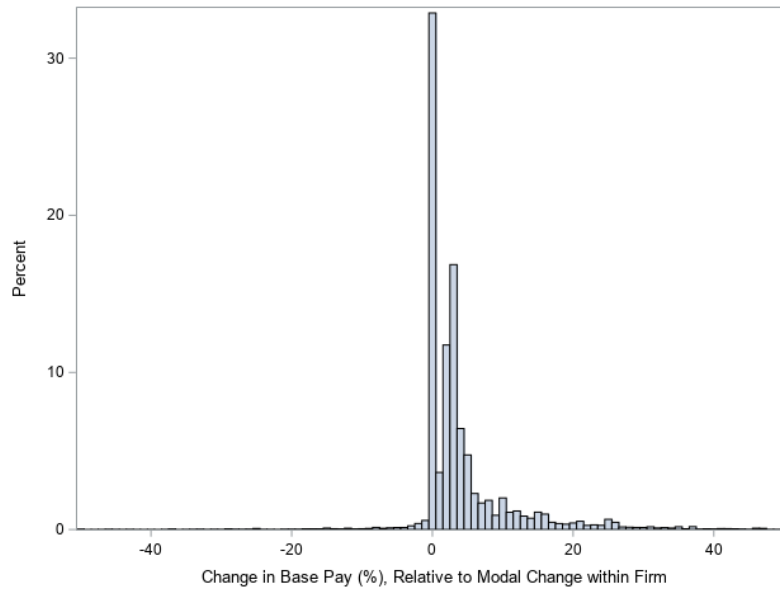
(a) Sample: Minimum 50 Stayers in Firm
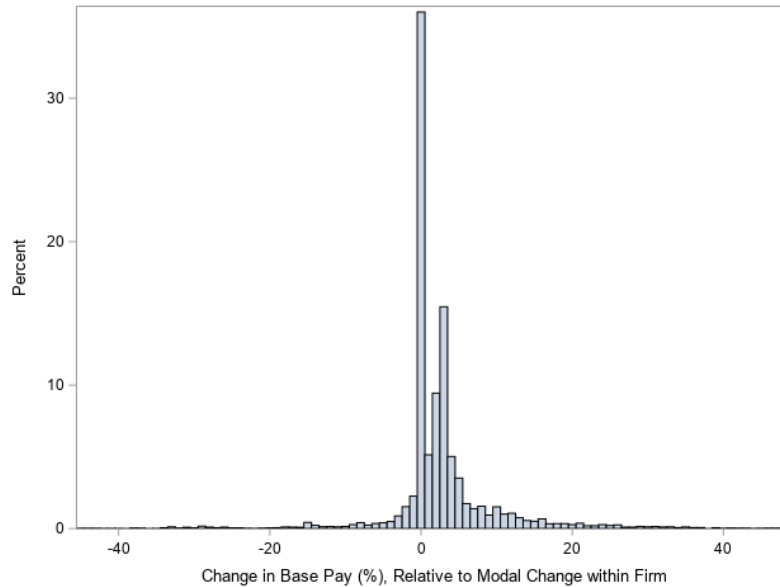
(b) Sample: Minimum 50 Stayers, Mode $\neq$ 0

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

Appendix Figure A.8: Distribution of New Hires Over Time Relative to Hire in April 2016, Sample: Bunching Firms
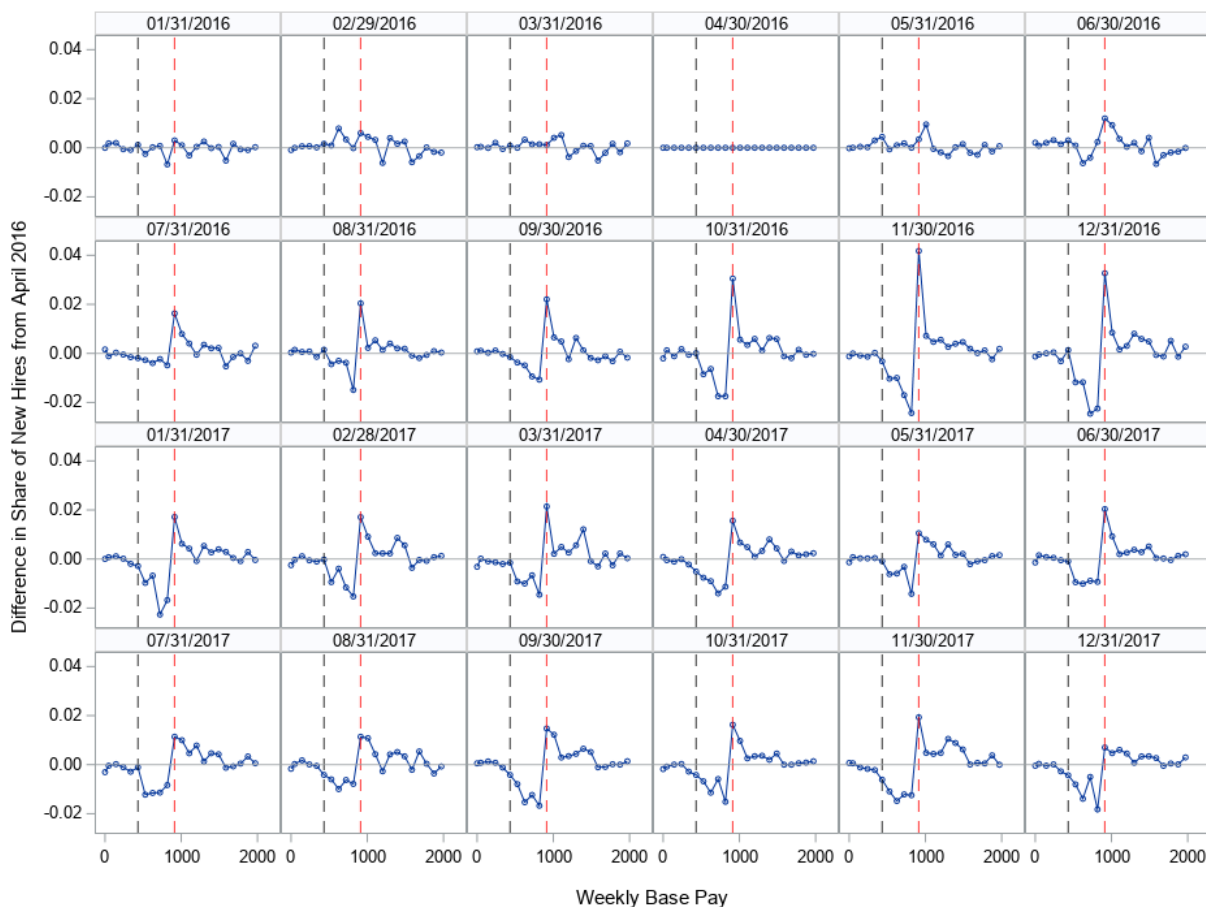
Notes: This figure shows the share of new salaried hires within each \$96.15 increment of weekly base pay between January 2016 and December 2017, relative to the share in April 2016. In each panel, the black vertical dashed line is at \$432 and the red vertical dashed line is at \$913. The sample is restricted to firms where there is at least 1 salaried worker earning between \$913 and \$953 per week in December 2016.

Appendix Figure A.9: Distribution of New Hires Over Time Relative to Hire in April 2016, Sample: Non-bunching Firms
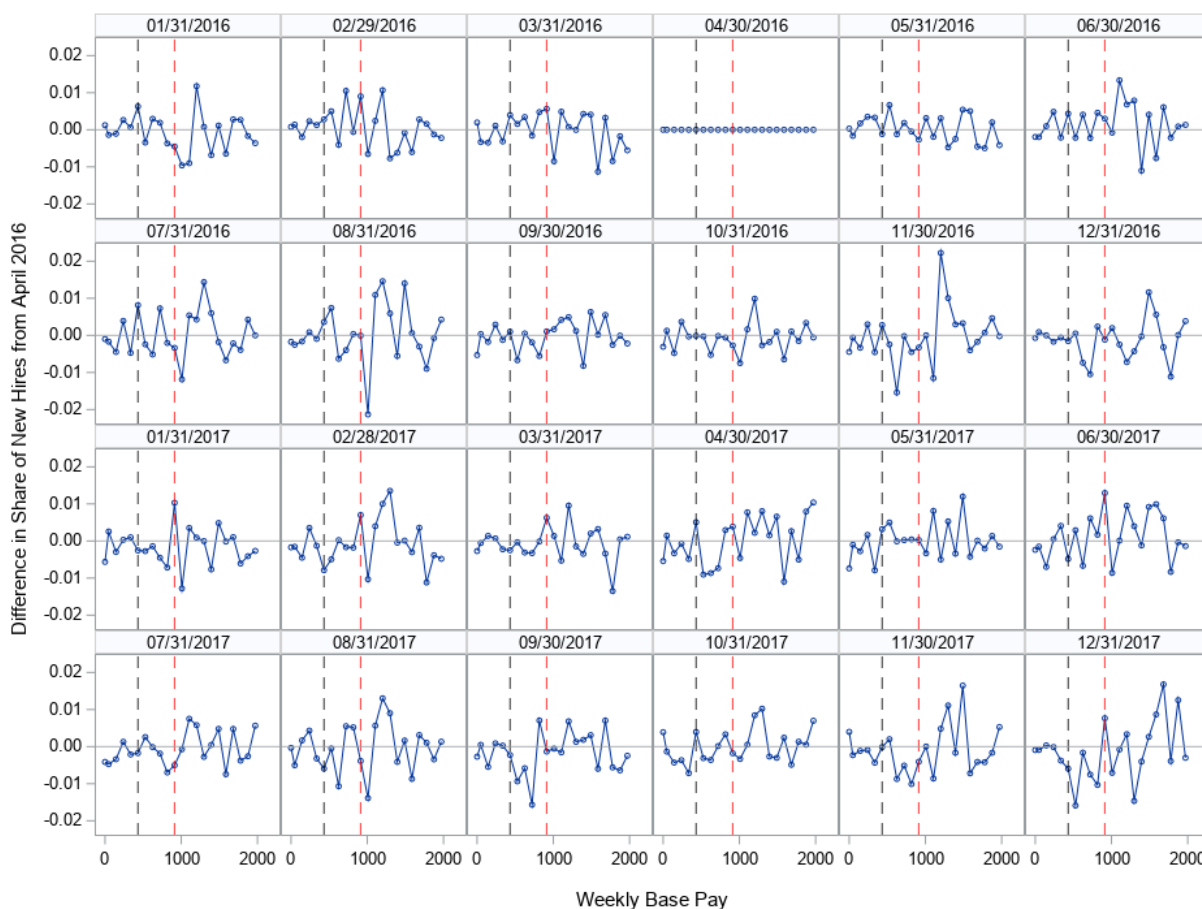
Notes: This figure shows the share of new salaried hires within each $96.15 increment of weekly base pay between January 2016 and December 2017, relative to the share in April 2016. In each panel, the black vertical dashed line is at $432 and the red vertical dashed line is at $913. The sample is restricted to firms where there are no salaried workers earning between $913 and $953 per week in December 2016.

Appendix B.   Role of Staggered Bargaining

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

To begin, I present two pieces of evidence that wages are adjusted on a yearly basis. First, figure B.1 plots the distribution of the number of wage adjustments that workers receive between each pair of consecutive months from May 2015-2016. Conditional on receiving at least one pay change, over 80% of salaried workers experience no more than 1 pay increase in the one year interval. Second, figure B.2 shows that for each time a worker receives a pay change, there is a 50% chance that the last time wage adjustment occurred exactly 12 months prior. Together, the figures suggest that workers' salaries adjust on the exact same month each year, if thy adjust at all.

Next, I argue that the periodicity of wage adjustments does not explain the observed wage dynamics before and after the proposed change to the overtime exemption threshold announced in 2016. First, if the only cause for the persistence in bunching at the threshold after December 2016 is due to staggered bargaining, then the bunching mass should experience a sharp drop exactly a year after its injunction. After all, not only do wage changes occur in 12 month intervals, but figure B.3 shows that the majority of wage changes also occur in January. Thus, one might expect to see a large decline in the bunching mass on January 2018. In contrast, 3 finds a constant trend in the magnitude of the bunching mass over time. As a result, the nature of the persistence cannot be solely explained by periodicity in wage bargaining.

Second, there is no evidence that the anticipatory bunching before December 2016 is due to workers' wages adjusting on exactly the same calendar month that they usually change. For example, given that workers tend to receive wage changes once every 12 months, one might expect that the early-bunching in October 2016 is driven by workers who also received a pay increase in October 2015. To test that hypothesis, figure B.4 plots the magnitude of the $913 bunching mass over time, estimated from equation 1, separately by the month that workers received a pay increase in the year prior to the policy announcement in May 2016. Focusing on panel (b), I find that leading up to the first pay increase, the evolution of the share of firms' workers at threshold in 2016 was perfectly parallel to the evolution in 2014, consistent with the evidence that workers only receive 1 pay increase per year so neither treated nor control group experienced any pay changes. After the first change in base pay, workers continue to experience no wage increases, leading again to incredibly straight

parallel trends. However, after the announcement of the new overtime exemption threshold in May 2016, I find that the share of bunched workers started rising even before December 2016. This anticipatory response does not appear to be stronger on the month that workers received a pay increase prior to the announcement of the policy. For instance, workers who received a raise in October 2015 were already being bunched in September 2015, and the magnitude of the bunching mass in October 2016 was not significantly larger than for other types of workers. Overall, there does not appear to be any correlation between the month of workers' usual pay increase and the month their wages adjusted to the new overtime exemption threshold.

While the primary focus of my paper is the persistence in wages post-injunction, I nevertheless provide a brief analysis into the understanding which firms are bunching workers early pre-injunction. Interestingly, the data suggests that small firms are more likely to react early relative to large firms. Figure B.5 plots estimates of the magnitude of the bunching mass over time, separately for firms below and above the median firm size in the data. While I do not have enough statistical power to make a conclusive statement, the coefficients suggest that small firms saw an increasing trend in the number of workers at the overtime exemption threshold starting in August 2016. On the other hand, the share of bunched workers in large firms remain relatively more constant until at least November 2016. A more detailed examination into why small firms tend to increase wages even before the month that the new threshold was supposed to go into effect is beyond the scope of this paper, and a potentially interesting question for future work.

Appendix Figure B.1: Distribution of Workers by Number of Wage Changes per Year
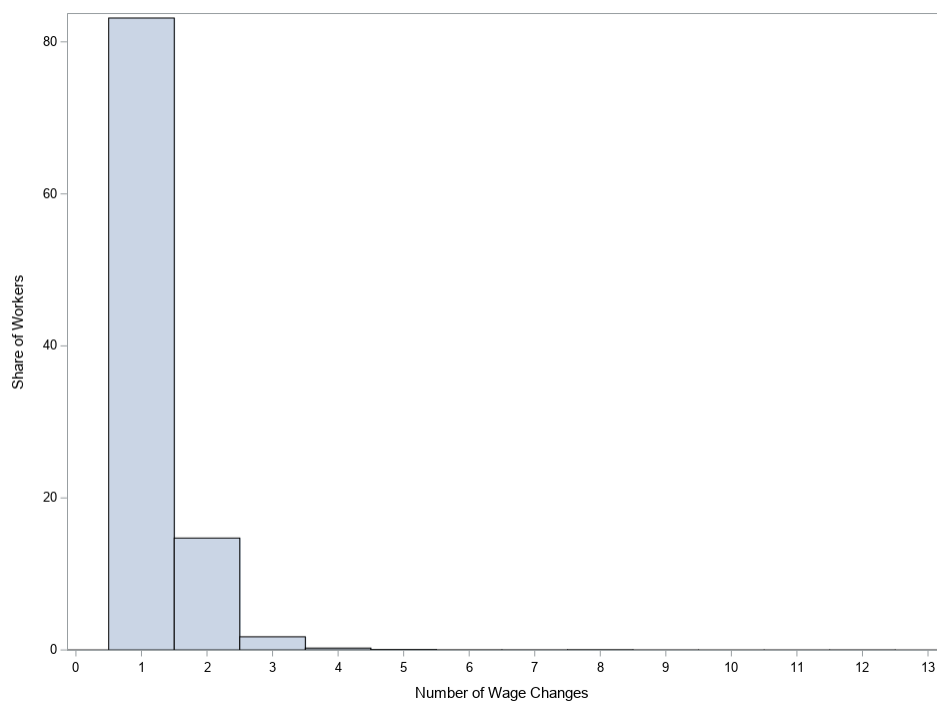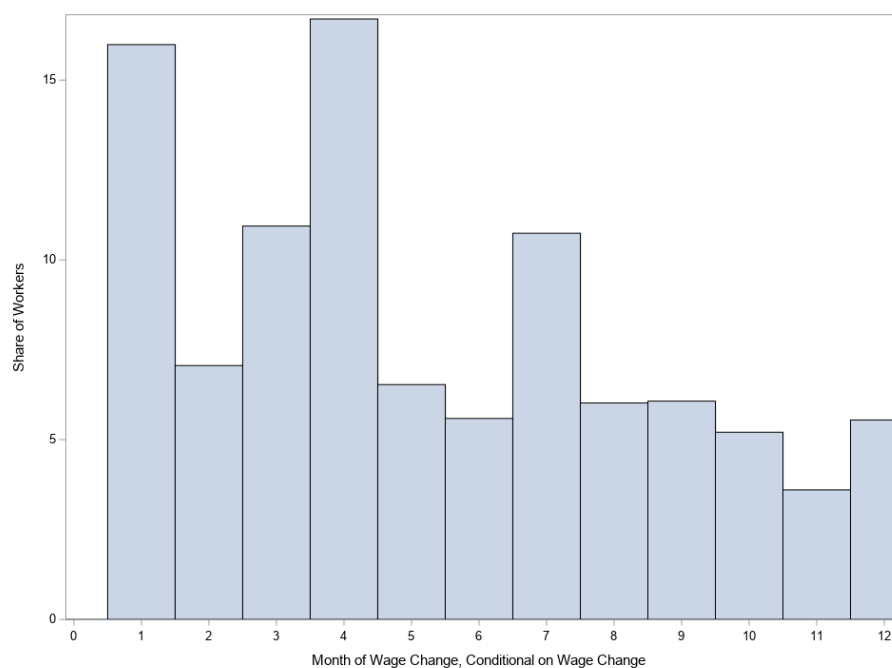

Notes: This figure shows the distribution of workers by the number of months they experienced a change in base pay relative to the previous month, for each pair of months from May 2015-2016, conditional on receiving at least one pay increase. The sample comprises of salaried workers who are continuously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

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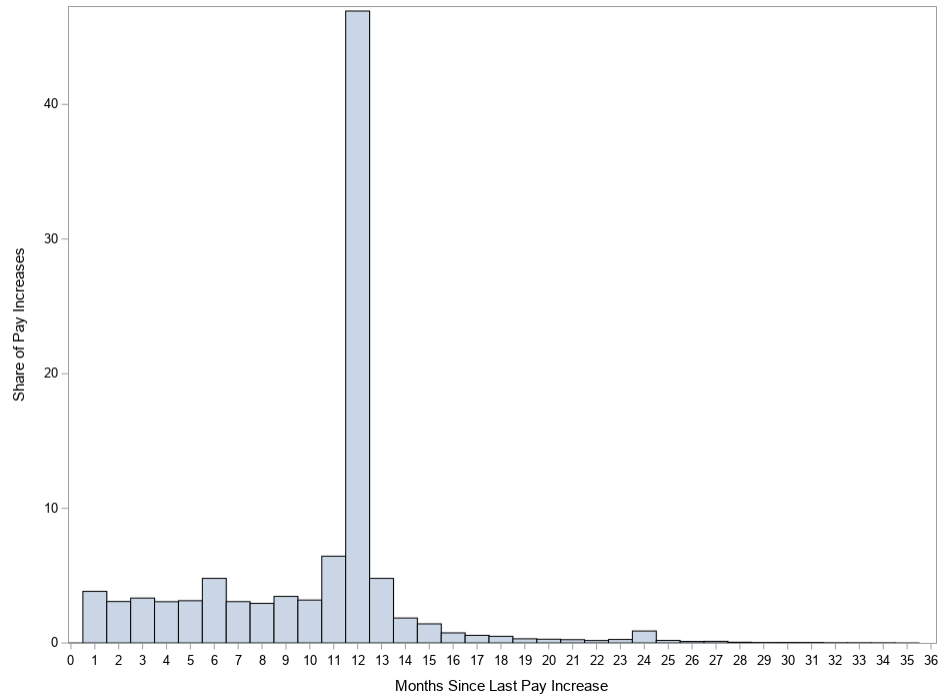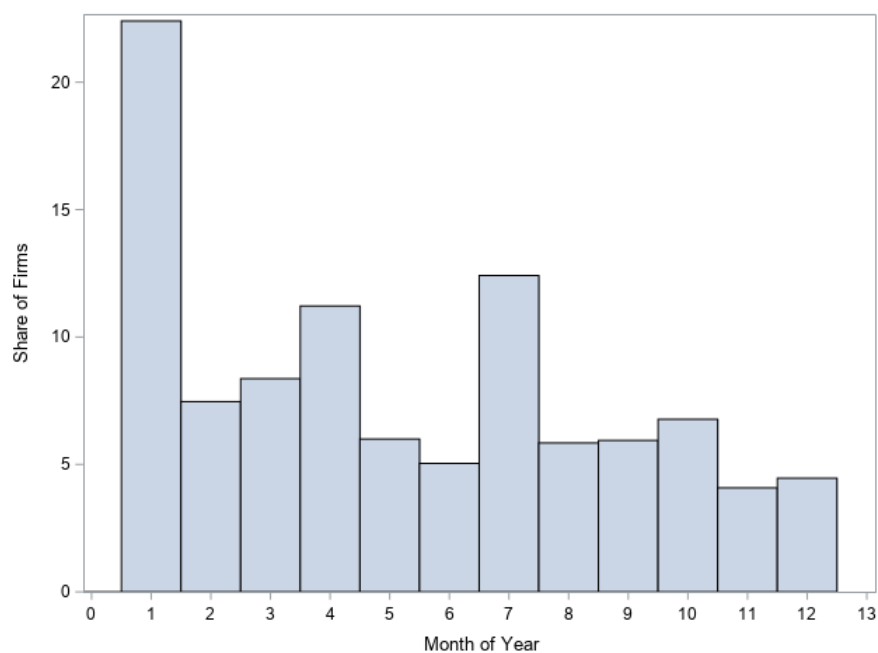

Notes: This figure restricts the sample to all wage changes among salaried stayers between May 2015-2018, and plots the distribution of the number of months since the previous pay increase.

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


(a) Distribution of Workers

(b) Distribution of Modal Month within Firm

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

Appendix Figure B.4: Share of Workers Bunched Over Time, by Month of Last Pay Increase Before May 2016
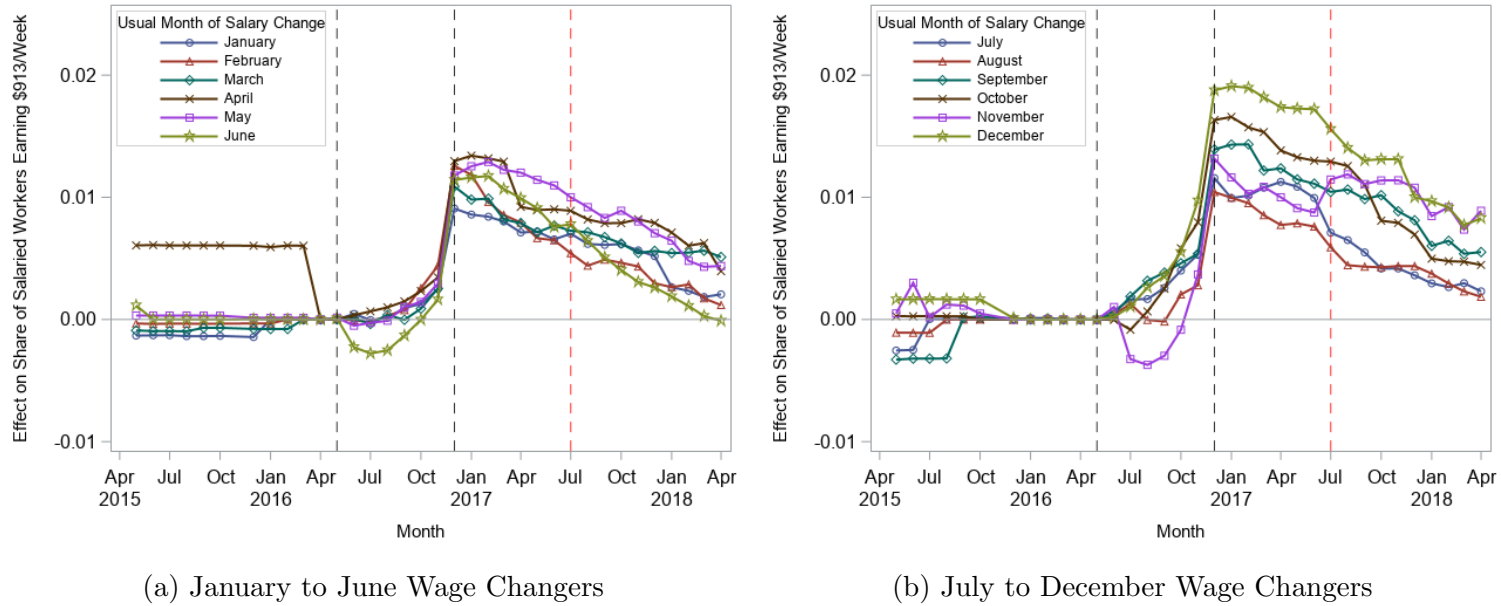
(a) January to June Wage Changers

(b) July to December Wage Changers

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

Appendix Figure B.5: Share of Workers Bunched Over Time, by Firm Size
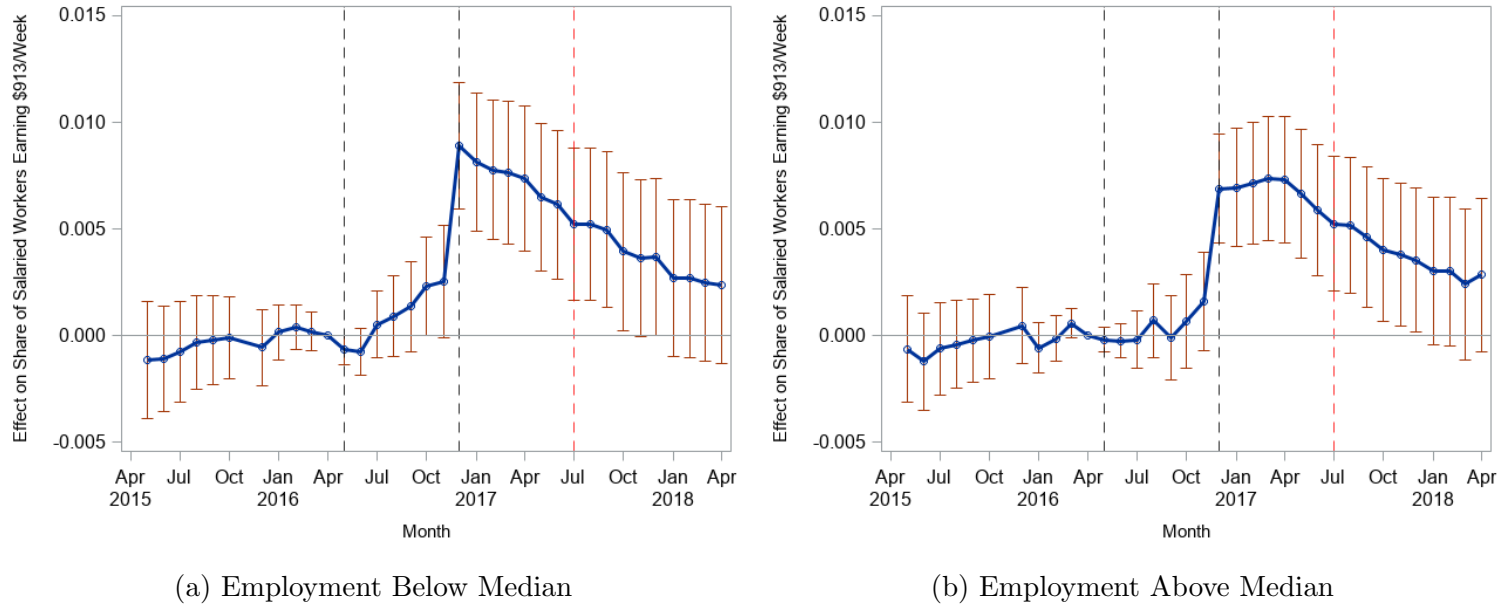
(a) Employment Below Median

(b) Employment Above Median

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

Appendix C. Regression Discontinuity Design to Evaluating Wage Rigidity of New Hires

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

To start, figure C.1 plots new hires' weekly base pay at their last observed employer as a function of their base pay at their current employer, separately for workers hired before and after May 2016. In panel (a), I find that a linear line does a reasonably good job of predicting the average past wage of new hires. Crucial for my analysis is the observation that past salaries are continuous and fairly linear locally around the $913 per week threshold. If workers' past salaries are a good indication of their productivity and employers become more selective after bunching new hires' salaries at the threshold, then I would expect the cohort of new hires after May 2016 to have prior wage histories that lie on or above the linear prediction line. In contrast, panel (b) find that after May 2016, workers hired at the threshold have lower past salaries than predicted by the linear fit. The result therefore suggests that employers are actually hiring workers of lower productivity at the threshold then they otherwise would have absent the policy.

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

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

where y_{it} is the last observed weekly base pay of worker i hired at time t. I control for a time specific constant β_t and a linear trend in entry base pay v_{it}, normalized to zero at $913. The coefficient of interest is β_{2t}, which measures the deviation of outcomes from the linear trend for new hires earning $913-953 per week, indicated by the dummy D_{it}. Given that workers' past wages tend to diverge from the linear trend in the left and right tails of figure C.1, I

restrict the sample for the local linear regression to new hires paid between \$600 and \$1200 per week. Figure C.2 plots the estimates of β_{2t} over time for two different outcome variables. In panel (a), I highlight three main points. First, there is no detectable discontinuity in new hires' prior base pay at the \$913 threshold in any quarter from May 2015 to October 2016. Second, there is a large discontinuity starting in precisely November 2016 when the bunching mass at the overtime exemption threshold is largest. Third, the discontinuity persists afterwards at smaller magnitudes. Taken together, the timing and location of the discontinuity aligns closely with the conclusion that new hires' productivity lowered as a direct result of the FLSA rule change. In contrast, panel (b) finds no discontinuity in the base pays of new employees 18 months after their hire date, suggesting that employers did not compensate for the elevated initial salary with slower wage growth.

I show the robustness of my results to alternative specifications in table C.1. The first estimate in column (1) corresponds to the Nov 2016 - Jan 2017 estimate in figure C.2a, which implies that workers hired at the \$913 threshold had prior weekly salaries that are \$54 lower than predicted given the salaries of other new hires in the cohort. In row two, I find that the excess initial salary translates to a 7.7% larger pay increase from job transition than otherwise predicted. The real wage increase for new hires at the threshold is robust to an alternative specification in column (2) where I allow for different slopes to the left and right of the threshold, and to a further restriction in column (3) where I only use workers earning \$1000-1500 per week to predict bunched hires' expected past salaries. The restricted sample alleviates concerns that the FLSA rule change affected not only jobs paying below the new overtime exemption threshold, but may have even resulted in spillovers right above it. One downside of using only workers on the right tail to predict bunched hires' outcomes is that it systemically underestimates their past wages, even in the months before the rule change, and so column (3) presents a difference-in-discontinuity estimate where I adjust for the underestimation using the magnitude of the discontinuity in the quarter prior to the announcement of the new threshold. In all three specifications, I find consistent evidence that workers hired at \$913-953 per week had lower past wages than predicted, leading me to conclude that firms are not selecting more productive workers in response to the elevated wage. Repeating the same regression specifications in columns (4) to (6), I find consistently no effect on the future wages of new hires after employment.

Overall, the regression discontinuity analysis rejects the claims that 1) employers are positively selecting more productive hires after raising entry wages, and 2) employers are reducing wage growth of workers who receive a premium on their initial pay. While the findings in this section reinforces the argument in the main text, I am cautious to over rely on

this analysis. The discontinuity approach assumes that absent the rule change, the distribution of pay increases from switching jobs would be continuous with respect to workers' new salaries. While that appears to be true prior to May 2016, it is unlikely to hold after the rule change since, by construction, the overtime policy affected jobs to the left of the $913 per week threshold. I have tried to account for that critique by showing that my results are robust to using only workers on the right tail of the distribution as a control. Nevertheless, the type of analysis in this section does not have the same econometric properties as a traditional regression discontinuity design where the assumption of continuity is more plausible. Despite that limitation, it is reassuring that both the difference-in-difference and regression discontinuity analyses reach the same conclusions.

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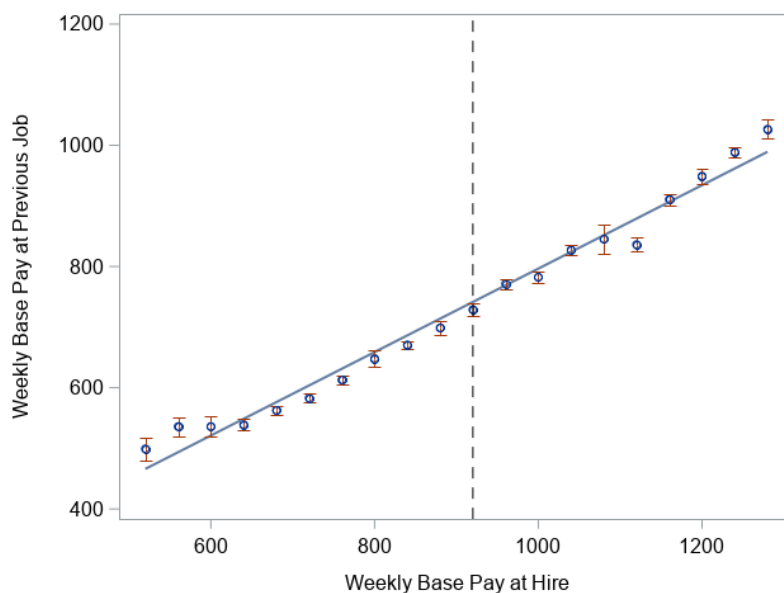

(a) Before May 2016
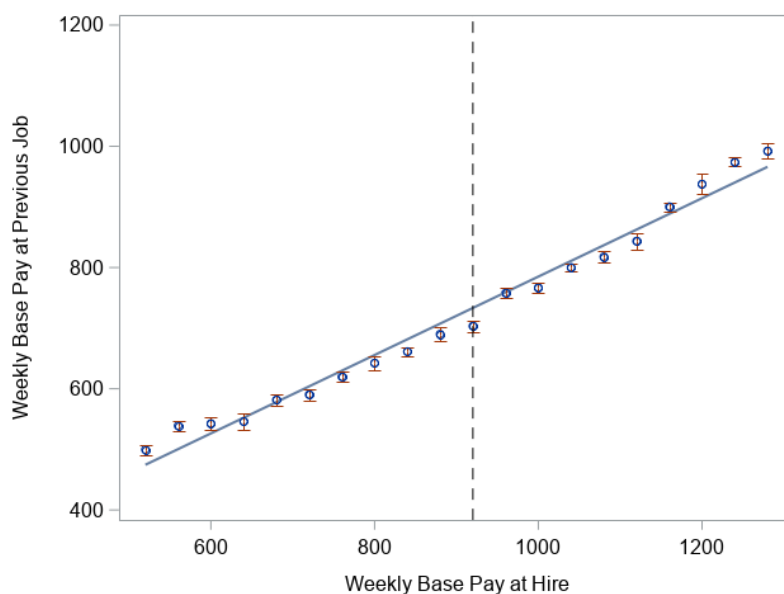
(b) After May 2016

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

Appendix Figure C.2: Discontinuity in Outcomes as a Function of Current Base Pay among New Hires Currently Earning $913-953/week
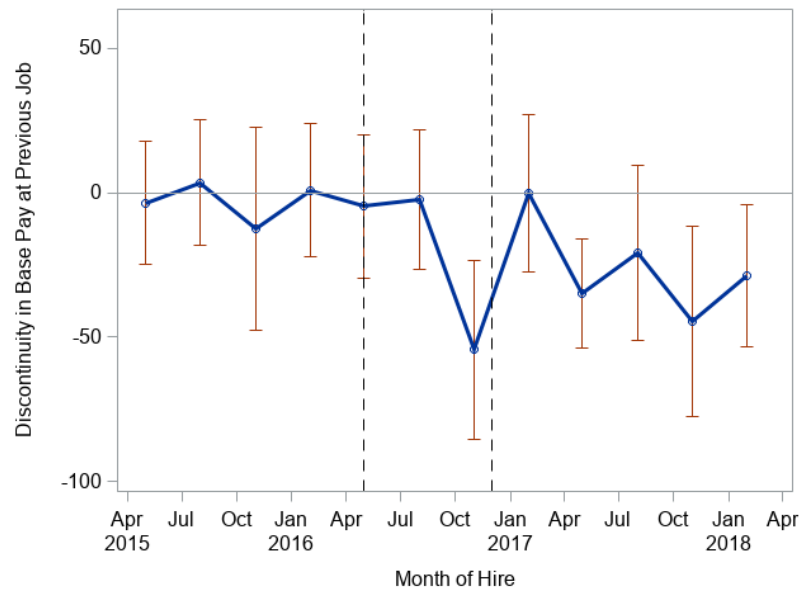
(a) Base Pay at Last Employer
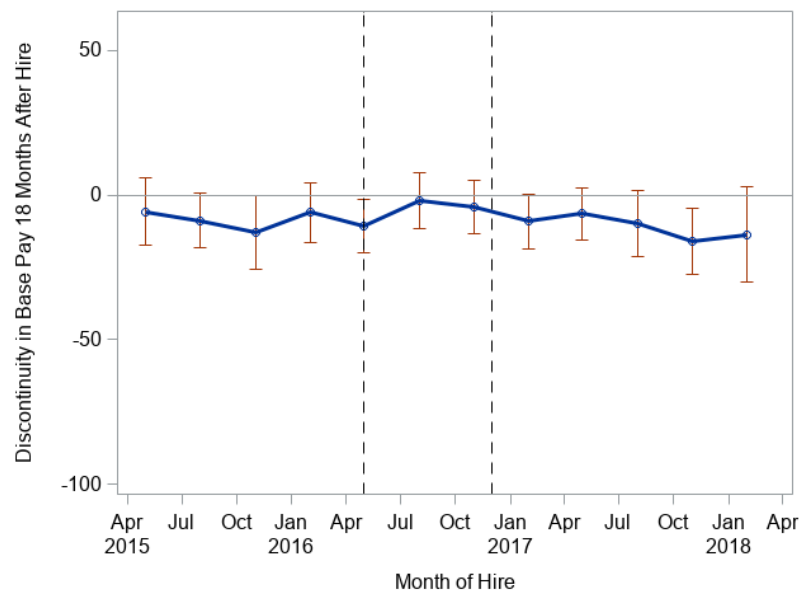
(b) Base Pay After 18 Months

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

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

	(1)	(2)	(3)	(4)	(5)	(6)
Base Pay in Previous Job	−54.427***	−51.616**	−47.478**			
	(15.820)	(20.463)	(23.457)			
%Δ Base Pay	0.077***	0.081***	0.052			
	(0.019)	(0.024)	0.041			
Base Pay After 18 Months				-4.245	0.139	-2.061
				(4.674)	(5.985)	(9.502)
Separations (18 Months)				−0.009	0.018	-0.013
				(0.003)	(0.013)	(0.018)
N	19,693	19,693	257,925	17,005	17,005	17,186
Sample	One Slope	Two Slopes	Right Tail	One Slope	Two Slopes	Right Tail

Notes: This table reports estimates from equation C that calculates the deviation in outcome between new hires earning $913-953 per week and the level predicted by a linear regression from other hires with similar base pays. The prediction in columns (1) and (4) uses one linear slope for jobs paying between $600 and $1200 per week. Columns (2) and (5) uses a different slope for each side of the discontinuity. Columns (3) and (6) only uses workers paid $1000-1500 per week for the prediction. *10%,** 5%,*** 1% significance level.