The Labor Market Effects of Expanding Overtime Coverage

Simon Quach*

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Abstract

This paper evaluates the impact of overtime coverage on the US labor market and offers new insights into the wage-hour setting process. While overtime was originally intended to raise employment by encouraging firms to hire more workers for fewer hours per worker, a competing theory predicts that employers would instead reduce base pay to offset the cost of the overtime premium. Leveraging recent changes in state and federal salary thresholds for overtime coverage of salaried employees, in conjunction with high-frequency administrative payroll data, I find evidence inconsistent with both views. On one hand, rather than increasing headcount, expansions in overtime coverage led to a net loss in employment. On the other hand, rather than reducing base pay, the increased overtime eligibility thresholds led firms to either raise base pays above the threshold to keep jobs exempt from the new overtime provisions, or pay an overtime premium without changing base salaries. Comparing the costs and benefits, these responses imply a large negative elasticity of employment with respect to wages. Moreover, the rise in income is largest for jobs paying near the new threshold, whereas the employment loss is greater among lower paying jobs. As a result, the policy amplifies, rather than reduces, inequality. Viewing these effects through the lens of a wage-bargaining model suggests that there are large costs to firms for offering overtime.

JEL codes: J23, J31, J33, J38

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1 Introduction

Concerns over income inequality and wage stagnation have motivated many states to strengthen labor market policies in support of low-income workers. Interest in these policies has sparked a large literature on the economic impacts of the minimum wage, its implications for labor market efficiency, and its role in the rise of income inequality (Autor, Manning and Smith, 2016; Harasztosi and Lindner, 2019; Cengiz et al., 2019). In contrast, although overtime regulation was likewise designed to protect low-wage workers, far less is known about its effects on the labor market. This is despite the fact that overtime protection covers over half the U.S. labor force U.S. Department of Labor (2019*a*), and results in a significant transfer from employers to workers. For instance, employers in the U.S. pay more in overtime compensation each year than they do in taxes to fund the unemployment insurance systems (U.S. Department of Labor, 2019*b*). Similar to the minimum wage, understanding the effects of this large cost to employers is central to ongoing debates surrounding the nature of the labor market, and the effectiveness of wage and hour regulations at reducing inequality.

This paper investigates the labor market impacts of expansions in overtime eligibility. Unlike the minimum wage, there is no single canonical theory of how expansions in overtime coverage should affect the labor market. Instead, there are multiple theories, the most prominent of which are the labor demand model and the compensating differentials model of overtime.[1] The labor demand model predicts that the premium for overtime hours would incentivize firms to reduce workers' hours, and in turn, either increase or decrease employment depending on the substitutability of hours across workers (Ehrenberg, 1971*a*). Historically, the original motivation for the introduction of overtime regulations during the Great Depression was the hypothesis that workers are highly substitutable so that by making hours more expensive, employers would hire more workers. In contrast, the compensating differentials model hypothesizes that in equilibrium, overtime coverage would have no real labor market effects since firms would simply lower workers' base wages to offset the costs of overtime (Trejo, 1991). Despite the long history of overtime regulation in the U.S., and the prominence of these two models in modern policy discussions (U.S. Department of Labor, 2019*a*), there is still no consensus as to the accuracy of these theories at describing the labor market effects of overtime eligibility.

Empirical studies of overtime coverage in the United States have been limited by a lack

[1] Other theories of overtime have examined it through the lens of workers' labor supply decision (Idson and Robins, 1991; Frederiksen, Graversen and Smith, 2008), firms' demand for labor in the presence of absenteeism (Ehrenberg, 1970), union-bargaining (Andrews and Simmons, 2001), and wage-hours contract with on-the-job training (Hart and Ma, 2010).

of policy variation and inadequate data.[2] Although there have been a few expansions in overtime coverage to additional industries and demographic groups over the past 80 years, these regulatory changes have often coincided with changes in the minimum wage. As a result, while previous papers have used these policies to evaluate the effect of overtime coverage on workers' hours, they were unable to isolate its effect on income and employment.[3] Furthermore, even if these studies had policy variation that did not overlap with minimum wage changes, it would still be difficult to estimate the income and employment effects using traditional household surveys. Few datasets in the U.S. distinguish between workers' base pay and overtime pay, and those that do often lack the sample size or panel structure to precisely estimate changes in aggregate employment. Given these empirical challenges, a recent review by Brown and Hamermesh (2019) finds that "no study presents estimates of effects [of overtime coverage] on employment, and none offers evidence on all outcomes: [wages, earnings, and hours]".

I fill this gap in the literature by leveraging anonymous administrative payroll data from the largest payroll processing company in the U.S. to evaluate the effect of recent federal and state expansions in overtime coverage for low-income salaried employees.[4] Unlike hourly workers, overtime eligibility for salaried employees is determined by their base pay relative to a legislated "overtime exemption threshold." All workers who earn below this threshold are guaranteed overtime protection whereas white-collared salaried employees who earn above it are legally exempt from overtime. Between 2014 and 2020, there were two federal rule changes and sixteen prominent state-level policies that raised this cutoff.[5] As a result, these rule changes expanded overtime coverage to salaried workers earning between the old and new thresholds, but did not directly affect workers paid outside this interval.

Following recent advancements in the minimum wage literature (Cengiz et al., 2019), I estimate the labor market effects of raising the overtime exemption threshold within increments of base pay across the salary distribution using an event-study design. My analysis exploits both the state and federal rule changes, but uses a separate approach to construct the counterfactual for each of the two policy variations. To identify the effect of the state

[2]See Hart (2004) and Brown and Hamermesh (2019) for an overview of the literature on overtime.
[3]Studies of expansions in overtime coverage have found a mix of negative (Costa, 2000; Hamermesh and Trejo, 2000) and zero significant effects (Johnson, 2003; Trejo, 2003) of overtime eligibility on the number of hours worked per week.
[4]See Goff (2020) for another recent study that uses administrative payroll data to study the effects of overtime. He finds that among hourly workers, the overtime premium leads to significant bunching at the 40 hours per week kink and reduced hours worked by at least 40 minutes per week.
[5]Although the 2016 federal rule change was overturned a week before it went into effect, I find that firms nevertheless responded to it as if it was binding.

policies, I compare the evolution of labor market outcomes in states that raised their thresholds to those in states that did not. However, since the federal policies affected all states simultaneously, I instead estimate their effects by comparing firms and workers in the year of each federal threshold change to similar firms and workers in the year prior to the reform. In both cases, the identifying assumption is that absent the increase to the threshold, the distribution of base pay in the treatment and control groups would have evolved similarly.

Using this approach, I find that while expanding overtime coverage succeeds in raising workers' income, it also reduces employment. After the overtime exemption threshold rises, I observe that the number of salaried workers between the old and new threshold falls by 18% (s.e. 0.7%). I document three responses that explain this phenomenon. First, half the decrease in jobs below the new threshold are accounted for by an increase in jobs right above it. This bunching in the distribution reflects firms' decision to raise workers' base pay above the new cutoff to keep them exempt from overtime.[6] Second, about a quarter of the missing salaried jobs were reclassified from salaried to hourly. Individuals in these jobs no longer receive a fixed salary, but are paid per hour of labor and qualify for overtime protection. Third, for every 100 affected workers, 4.3 (s.e. 2.2) jobs were lost due to a reduction in employment. In comparison to the large decrease in employment, I estimate that the income of affected workers only increased by 1.3% (s.e. 0.1%). The small increase in income implies a large elasticity of employment with respect to own wage of -3.25 (s.e. 1.71). This elasticity is an order of magnitude larger than estimates in the minimum wage literature, and I can reject elasticities more positive than -0.38 at the 90% confidence level.[7] These results suggest that expanding overtime coverage costs relatively more jobs for each percent increase in workers' income than raising the minimum wage.

I also estimate the effects of the federal and state rule changes separately to determine whether firms' responded differently to small changes in the threshold relative to larger ones. Relative to the major 2016 federal rule change, the incremental increases in the state thresholds induced a larger bunching effect and a smaller reclassification effect for each affected worker. This heterogeneity reflects variation in the base pay of affected workers relative to the new threshold. Since most of the workers impacted by the state rule changes

[6]A contemporaneous study by Cohen, Gurun and Ozel (2020) also finds bunching of managerial jobs at the overtime exemption threshold using a cross-sectional analysis of online job postings. However, while they interpret the bunching solely as firms strategically classifying jobs as managers to avoid paying overtime, I show that it is also due to some salaried workers receiving raises and others losing their jobs.

[7]See (Dube, 2019) and appendix A.4 of Harasztosi and Lindner (2019) for detailed reviews of the elasticities in the minimum wage literature. Dube (2019) finds a median elasticity of employment with respect to income of 0.17.

were already earning close to the new threshold, it is less costly for firms to bunch these workers than to reclassify them. With regards to employment, I observe that employers reduced hires in anticipation of the federal rule change months before it went into effect, but did not exhibit similar forward-looking behavior with respect to the state policies. This difference in anticipatory response suggests that firms are able to adjust quicker to small policy changes than to large shocks. However, despite these differences, the magnitude of the employment and income effects are similar between the state and federal policies. This implies that the overall costs and benefits to workers do not vary significantly with the size of the threshold increase.

In addition to the aggregate impacts, I also evaluate the redistributive implications of increasing the overtime exemption threshold by comparing the employment and income effects across the distribution of base pay. To accomplish this, I use the matched employer-employee panel structure of the data to determine how the number of new hires, separations, reclassifications, and bunched workers varied by base pay as a result of the 2016 federal reform. The results of my analysis indicate that raising the overtime exemption threshold was actually counter-redistributive. I show that the largest gain in income accrued to the 5% of affected workers who received a raise right above the new threshold but would otherwise have earned within \$180 below it. These bunched workers experienced a median income increase of 5.8% due solely to a rise in base pay. In comparison, reclassified workers saw no change in base pay but a small increase in overtime pay, and workers who stayed salaried but not bunched saw no additional compensation. However, while the largest beneficiaries of the policy earned within \$180 of the new threshold, the employment loss primarily fell onto lower paying jobs. Taken together, the distribution of income and employment effects imply that raising the federal overtime exemption threshold largely benefited a small group of workers earning close to the new threshold, but cost jobs paying further below it, thereby exacerbating inequality.

The negative employment response to overtime coverage is inconsistent with the original intent of the policy that overtime stimulates job creation by encouraging firms to substitute additional workers for a reduction in long hours (Ehrenberg, 1971*a*). This prediction relies on the assumption that the substitution effect from the change in relative prices between headcount and hours exceeds the negative scale effect from an increase in labor cost. Empirically, previous tests of this work-sharing hypothesis have generally found negative or zero employment effects from policies outside the U.S. that shortened the length of the standard workweek above which workers are entitled to overtime compensation (Hunt, 1999; Crépon and Kramarz, 2002; Skuterud, 2007; Chemin and Wasmer, 2009). My paper contributes to this literature on work-sharing by using an expansion in overtime coverage, rather than a reduction in the standard workweek, to study the employment effects of overtime protection within a modern U.S. context. My findings reinforce existing evidence that work-sharing policies implemented through the regulation of overtime are ineffective tools for creating jobs.

While my results do not support the work-sharing theory of overtime, they are also inconsistent with the prediction of the compensating differentials model that firms would reduce employees' base salaries to negate the costs of overtime pay (Trejo, 1991). Due to a lack of policy variation, prior tests of this prediction have relied on cross-sectional variation in overtime coverage to estimate the correlation between wages and overtime hours, by eligibility status (Trejo, 1991; Barkume, 2010). While the negative relationship identified in these studies is consistent with firms lowering wages to partially negate the costs of overtime requirements, it can also be driven by the selection of low skilled workers into jobs that demand long hours. My paper advances this literature by exploiting a natural experiment to provide causal evidence against the compensating differentials model of overtime. Not only do I find that average base pay increases following an increase in the overtime exemption threshold, but I also do not observe any change in the left tail of the base pay distribution that would indicate that at least some workers' base salaries were cut as a result of the policy.

Given that neither the labor demand model nor the compensating differentials model of overtime can explain the labor market effects of raising the overtime exemption threshold, I build upon a job-search and bargaining model from the minimum wage literature to interpret my results (Flinn, 2006). In this model, there are three underlying parameters that vary between jobs: workers' value of leisure, the relative cost of a job being salaried versus hourly, and the match quality of the job. Through Nash bargaining between the worker and firm, these parameters generate a distribution of weekly base pay, weekly hours, and salaried/hourly status. I show that by introducing overtime coverage as a fixed cost per worker, the predictions of the model can match the bunching, reclassification, dis-employment, and anticipatory effects that I find in the paper. Moreover, interpreting the bunching effect through the lens of the model suggests that, for some employees, firms capture enough surplus to raise workers' salaries by up to 25% to avoid the cost of offering overtime.

The remainder of this paper is organized as follows. In section 2, I explain the institutional details governing U.S. overtime regulations and the specific policies to expand coverage for salaried workers. Section 3 outlines the predictions of the two competing models of overtime and develops additional predictions within a job-search framework. In section 4, I describe the administrative payroll data from ADP LLC that I use in this study. Sections 5 and 6 report my results on the aggregate employment and income effects. In section 7, I examine how the labor market effects vary across the distribution of base pay. I conclude in section 8 by discussing the implications of my findings and areas for future research.

2 Federal and State Overtime Regulation

The Fair Labor Standards Act (FLSA) requires employers to record workers' hours, and pay them one and a half times their regular rate of pay for each hour worked above 40 in a week.[8] While this rule applies to nearly all hourly workers in the U.S., the FLSA exempts a large group of salaried workers from overtime coverage who are considered executive, administrative, or professional employees. To exempt a salaried employee under this provision, an employer must show that the worker performs primarily white-collared duties, and earns a salary equal to or greater than the "exemption threshold" set by the Department of Labor (DOL).[9] Since the FLSA's overtime exemption threshold is not adjusted for inflation, the share of salaried workers earning less than that threshold, and thereby guaranteed overtime coverage, fell from over 50% in 1975 to less than 10% in 2016 (see Appendix Figure A.1).[10] In an effort to restore overtime protection to low-income salaried workers, such as managers at fast food restaurants and retail stores, multiple Departments of Labors at both the federal and state level have recently raised their overtime exemption thresholds. My paper uses these policy changes in the exemption threshold as natural experiments to study the effects of expanding overtime coverage.

At the federal level, I examine two major policies to revise the FLSA's overtime exemption threshold. First, the Department of Labor announced in May 2016 that it would increase the federal exemption threshold from \$455 per week (\$23,660 per year) to \$913 per week (\$47,476 per year) effective December 1, 2016. According to the Current Population Survey, the new rule would effectively raise the threshold from the 10th percentile of the salaried

[8]For hourly workers, the regular rate of pay is simply their wage. For salaried workers, the regular rate of pay is defined as their weekly salary divided by the number of hours for which the salary is intended to compensate (29 C.F.R. § 778.113). In practice, firms typically calculate salaried workers' regular pay rate as their weekly salary divided by 40. For example, a worker paid a salary of \$450 per week has an implied wage of $\$11.25 = \frac{450}{40}$. If the worker is covered for overtime, she would receive $\$16.88 = 1.5 \cdot 11.25$ for each hour above 40 that she works in a given week, in addition to her regular salary of \$450.

[9]The law also makes exceptions for special occupations such as teachers and outside sale employees. For a detailed overview of all exemptions, refer to Face Sheet #17A published by the DOL.

[10]In appendix figure A.2, I show that over the same time period, the share of salaried workers who say they would be paid for working more than their usual hours per week dropped from 27% to 12%.

income distribution to the 35th percentile. However, to employers' surprise, a federal judge imposed an injunction on the policy on November 22, 2016, stating that such a large increase in the threshold oversteps the power of the DOL and requires Congressional approval. Given that this unexpected injunction occurred only one week before the policy was to go into effect, many companies at the time reported that they had either already responded to the policy, or made promises to their employees that they intended to keep.[11] Following the injunction of the 2016 rule change, the federal Department of Labor debated a smaller increase to the FLSA overtime exemption threshold and announced in September 2019 that it would raise the threshold to $684 per week effective January 1, 2020. For my analysis, I examine both the nullified 2016 proposal and the binding 2020 rule change to estimate the short-run effects of a federal expansion in overtime coverage for salaried workers.

To complement my evaluation of the federal rule changes, I also implement an event study analysis using 16 prominent state-level increases in the overtime exemption threshold between 2014 and 2020. Similar to the minimum wage, multiple states impose their own overtime exemption thresholds that exceed the one set by the FLSA. I present in figure 1 all state and federal overtime exemption thresholds from 2005 to 2020, along with the invalidated proposal in 2016.[12] My state-level analysis uses variation from four states: California, New York, Alaska, and Maine, all of which define their overtime exemption thresholds as a multiple of their respective minimum wages. Thus, each time these states raise their minimum wage, their overtime exemption threshold simultaneously increases following a known formula.[13] In all four states, the overtime exemption threshold is high enough such that the segment of the income distribution affected by changes in the threshold does interact with changes in the minimum wage, even after accounting for potential spillovers.[14]

[11]For example, WalMart and Kroger raised their managers' salaries above the $913 threshold and did not take back those raises after the injunction (*Some Employers Stick With Raises Despite Uncertainty on Overtime Rule* - Wall Street Journal Dec 20, 2016). For a detailed history of the events leading up to and following the injunction, refer to appendix section B.

[12]I exclude from my event study the four most recent rule changes in Alaska that cumulatively increased the exemption threshold by only $35 to adjust for inflation. I also exclude the January 2014 event in New York due to missing data.

[13]Starting in 2017, California and New York also passed legislation that generated variation within-state. California sets a lower threshold for employers with fewer than 26 employees, whereas New York varies its threshold by both employer size and location (i.e. in/near/away from NYC). Since the data I analyze only records geography at the state-level and contains few small firms, I do not exploit the within-state variation. In my main analysis, I assume that the highest threshold within each state is binding for all employers, and show that my results are robust to restricting the sample to only events without within-state variation.

[14]Of the four states, Maine has the smallest threshold-to-minimum-wage ratio of 58. This implies that salaried workers paid at the threshold earn 45% more than a minimum wage employee who

In addition to the increases in the overtime exemption thresholds, the nature of the overtime regulation also provides two other sources of variation that can be used as placebo checks. First, the rule changes only directly affect salaried workers earning between the old and new thresholds, and should therefore have little effect on workers with incomes much higher in the salary distribution. Second, the federal policies occur in 2016 and 2020, so any valid empirical strategy should detect zero effects of the federal rule changes in all the other years. Aside from these placebo checks, the policies also generate variation between workers by their initial exemption status, and between firms by the initial share of their workforce paid within the old and new thresholds. While workers already receiving overtime pay and firms with no directly affected employees could arguably serve as additional control groups, I show in appendix C that these workers nevertheless experience a small pay increase and these firms reduce their hires of newly covered workers. Given these effects, I consider all workers and firms as treated by the rule changes, regardless of their pre-policy characteristics.

3 Theoretical Predictions

To guide my empirical analysis, I examine multiple theories of how overtime coverage may affect the labor market. To begin, I discuss the two competing models developed in the literature. In the labor demand model, firms' take wages as given, and choose the number of workers (n) and hours per worker (h) to maximize profit (Ehrenberg, 1971b):

$$\max_{(n,h)} \underbrace{f(n,h)}_{\text{Output}} - \Big(\underbrace{wh + p \cdot w(h-40) \cdot 1[h>40]}_{\text{Cost for hours}} + \underbrace{F}_{\text{Fixed cost}} \Big) n$$

where w is the market wage, p is the overtime premium, and F is a fixed cost per worker. In this framework, overtime coverage has no effect on wages and simply increases the overtime premium p.[15] By raising the cost per hour of labor above 40, overtime coverage induces two responses: a substitution effect away from long hours for more employment, and a scale effect to reduce both factors of production. In theory, the direction of the employment effect can be negative if the scale effect exceeds the substitution effect. However, in practice, policymakers who instigated the Fair Labor Standards Act had intended for overtime regulations to raise employment. One condition under which the substitution effect would dominate is if the return to long hours diminishes quickly (i.e. $\frac{d^2 f(n,h)}{dh^2} \approx 0$), in which case the reduction in

works 40 hours per week (i.e. $\frac{58}{40}$), a range where minimum wage studies have found either small or zero (Aaronson, Agarwal and French, 2012; Gopalan et al., 2020) spillover effects.

[15]While the assumption that wages are fixed is highly restrictive, previous attempts to endogenize wages by integrating labor supply responses have generated intractable predictions (Hart, 2004).

hours does not greatly affect the productivity of the marginal hire. Although I cannot test this assumption directly, the literature on the relationship between work hours and productivity generally finds decreasing returns to working long hours among low and medium skilled jobs such as those likely affected by changes in the overtime exemption threshold (Pencavel, 2014; Collewet and Sauermann, 2017). Given that I do not observe hours or productivity in the data, I am unable to separately identify the scale and substitution effects. Nevertheless, my analysis of the effect of overtime coverage on employment has useful policy implications, and will be able to rule out at least one of the two potential predictions of the labor demand model.

To model overtime in a market equilibrium, a competing theory argues that within a compensating differentials framework, base wages would decrease in response to overtime coverage such that total income remains unchanged (Trejo, 1991).[16] Under this framework, overtime coverage would have no effect on real income, hours, or employment. Empirically though, previous estimates of the correlation between overtime coverage and wages suggest that base wages do not fully adjust to offset the entire cost of overtime (Trejo, 1991; Barkume, 2010). To allow for the possibility that there may be real labor market effects if wages only adjust partially, my primary test of the compensating differentials model is its core prediction that workers' base pay should decrease after gaining overtime coverage.

In addition to the two canonical models of overtime, in this section, I present a third model of overtime that interprets overtime coverage as simply an added fixed cost to the firm for each covered worker. The goal of this model is threefold. First, it aims to capture some of the specific institutional details of my setting such as the distinction between salaried and hourly workers, and the rule that overtime coverage depends on an exemption threshold. Second, it generates a rich set of testable predictions of how raising the overtime exemption threshold would impact the labor market if it raised the fixed cost of affected workers. Lastly, the model will help me interpret the results that I find in this paper that the two canonical models of overtime fail to predict. While I use the model to generate predictions that will guide my analysis, I will not estimate the specific parameters in the model. I summarize the testable predictions of the labor demand, compensating differentials, and job-search and bargaining models in table 1.

[16] For instance, suppose an employee initially works 50 hours for a salary of \$825 each week and receives no overtime. If this worker becomes covered for overtime, the firm can reduce the worker's base salary to \$600, so that with the 10 hours of overtime, the worker would continue to receive $\$600 \cdot (1 + 1.5\frac{50-40}{40}) = \$825$ per week.

3.1 Search and Bargaining with Exogenous Contract Rates

The basic structure of my model builds on the theory of minimum wage developed by Flinn (2006). Suppose unemployed workers continuously search for a job and match with potential employers at an instantaneous rate $\lambda \geq 0$. Each match is characterized by three parameters. As conventional, I assume each worker-firm match has a idiosyncratic productivity level, θ. To generate variation in hours and pay classification (i.e. salaried/hourly status) between jobs, I introduce two non-standard parameters: a disutility of labor that varies between workers $a \sim H(a)$, and a relative value of classifying the job as salary rather than hourly F. The match quality and salary-fit of jobs follow a joint distribution $G(\theta, F)$.[17]

When an individual and firm meet, they both observe (θ, F, a) and Nash-bargain over the weekly income (w), weekly hours (h), and pay classification (S) of the job. If the applicant's value of accepting the job, denoted by $V_e(w, h)$, exceeds the value of continued searching V_n, then the employment relationship is formed. While employed, I assume that workers do not engage in on-the-job search. If unemployed, the individual continues searching while receiving an instantaneous utility b. I assume jobs are exogenously destroyed at a rate $\delta \geq 0$. The instantaneous discount rate is $r > 0$. Given these parameters, I characterize the worker's value of employment and continued search by the following Bellman equations:

$$(r+\delta)V_e(w,h) = w - a^{-\frac{1}{\epsilon}} \frac{h^{1+\frac{1}{\epsilon}}}{1+\frac{1}{\epsilon}} + \delta V_n$$

$$rV_n = b + \lambda \int_{V_e(\theta,F) \geq V_n} [V_e(w(\theta,F), h(\theta,F)) - V_n] dG(\theta,F)$$

where ϵ is the worker's constant labor supply elasticity. Unlike common search and matching models, I assume that workers receive a disutility from working longer hours that is additively separable from their income.[18]

I model firms' production technology as a function of both the match quality and the hours of labor per week, $y = \theta h^{\beta}$. Given the parameters (θ, F) and wage contract (w, h, S), the firm's discounted stream of profits is denoted by

$$J = \frac{\theta h^{\beta} - w + F \cdot \text{sgn}(S)}{r+\delta}$$

[17] One can think of F as the difference between the benefits (e.g. more flexibility, no need to monitor hours, etc.) of paying a worker by salary and the costs (e.g. less incentive to work long hours, etc.). A distribution of salary-fit can be motivated by an agency problem where a firm chooses an occupation's pay classification depending on how informative the number hours worked predicts workers' effort and output (Fama, 1991).

[18] The predictions of the model are invariant to including a additive preference for pay classification.

where $S = 1$ if the position is salaried, and $S = -1$ if hourly, and $\text{sgn}(\cdot)$ equals the sign of its argument. The firm's production function assumes that the output of each employee is independent of the output of other employees. This modeling assumption thereby eliminates the ability of the firm to substitute between hours per worker and number of workers.

Given (θ, F, a), the job characteristics are determined by Nash bargaining:

$$(w, h, S) = \arg\max_{(w,h,S)} [V_e(w,h) - V_n]^\alpha \left[\frac{\theta h^\beta - w + F \cdot \text{sgn}(S)}{r + \delta}\right]^{1-\alpha}$$

where $\alpha \in (0,1)$ represents the worker's bargaining power. This problem has a unique closed-form solution, which I henceforth denote by (w_0, h_0, S_0). The weekly hours $h_0 = \left(a^{\frac{1}{\epsilon}} \beta\theta\right)^{\frac{1}{1+\frac{1}{\epsilon}-\beta}}$ equates the marginal product per hour of labor with the marginal disutility per hour, $\frac{\partial J}{\partial h} = \frac{\partial V_e(w,h)}{\partial h}$. Since the pay classification only enters the firm's production function, a job is salaried if and only if F is positive (i.e. $S_0 = \arg\max_S \{F \cdot \text{sgn}(S)\}$). Given h_0 and S_0, weekly income is set as a weighted average of the worker's surplus and the firm's surplus, similar to standard applications of the search and matching model:

$$w_0 = \alpha\Big(\theta h_0^\beta + F \cdot \text{sgn}(S_0)\Big) + (1-\alpha)\Big(a^{-\frac{1}{\epsilon}} \frac{h_0^{1+\frac{1}{\epsilon}}}{1+\frac{1}{\epsilon}} + rV_n\Big)$$

Intuitively, the weekly hours and pay classification maximize the total match surplus, whereas the weekly salary distributes it. Heterogeneity in (θ, a) generate a joint distribution of weekly income and hours, and the distribution of F generates the share of salaried and hourly jobs.

Since workers only accept jobs where $V_e(w,h) \geq V_n$, not all matches will result in employment. For each worker type a and salary-fit F, there exists a critical value $\theta_0^*(a, F)$ such that $V_e(w(\theta_0^*, F), h(\theta_0^*, F)) = V_n$ and the worker accepts the job if and only if $\theta \geq \theta_0^*$. Inputting θ^* into the worker's value of unemployment, I derive V_n as a function of model primitives:

$$rV_n = b + \lambda \int_{\theta \geq \theta_0^*(a,F)} \left[\frac{w_0(\theta, F, V_n) - a^{-\frac{1}{\epsilon}} \frac{h_0(\theta, V_n)^{1+\frac{1}{\epsilon}}}{1+\frac{1}{\epsilon}} - rV_n}{r + \delta}\right] dG(\theta, F)$$

3.2 Comparative Statics in Response to Overtime Policy

Equipped with the benchmark model, I explore how the job characteristics (w, h, S) and the share of matches that become employment contracts change once I introduce an overtime premium. For reference, I continue to denote the solution to the model without overtime by (w_0, h_0, S_0). Henceforth, I make a distinction between base pay and gross pay. Let w present workers' weekly base pay, prior to receiving overtime compensation. The worker and firm bargain over the weekly base pay, weekly hours, and pay classification. However, the worker's value of employment and the firm's profit depend on gross pay $g = (1 + \eta_{(w,h,S)})w$, where

$$\eta_{(w,h,S)} = \begin{cases} \frac{0.5(h-40)}{h} & \text{if } h > 40 \text{ and } S = -1 \\ \frac{1.5(h-40)}{40} & \text{if } h > 40,\ S = 1, \text{ and } w < \bar{w} \\ 0 & \text{otherwise} \end{cases} \tag{1}$$

and $\bar{w}$ represents the overtime exemption threshold.[19]

First, I consider the case with no fixed costs or wage rigidities. Since neither the firm's production technology nor the worker's preferences change, their agreed upon job characteristics is equivalent to the benchmark case without an overtime policy. Workers' base incomes are discounted by a factor of $(1 + \eta_{(w,h,S)})$ relative to w_0 so that their gross incomes remains the same: $g = w_0$. Weekly hours, pay classification, and employment are the same as the baseline case. This result is analogous to the predictions of the compensating differentials model of overtime where base income adjusts such that overtime coverage has no real labor market effects (Trejo, 1991).

Second, I examine the case where overtime coverage imposes a fixed cost on salaried workers. For instance, it could be costly for the firm to monitor workers' hours or hours become less flexible. Since firms already monitor the hours of hourly workers, this friction only raises the costs of salaried workers earning less than the overtime exemption threshold. The firm's discounted stream of profits is given by

$$J = \frac{\theta h^{\beta} - (1 + \eta_{(w,h,S)})w + F \cdot \text{sgn}(S) - C \cdot 1[S = 1, w < \bar{w}]}{r + \delta}$$

where C is a constant, and $1[S = 1, w < \bar{w}]$ is an indicator that equals 1 if the worker is salaried and earns less than $\bar{w}$.[20] The fixed cost does not affect the bargaining outcome of hourly jobs or salaried jobs that pay above the threshold in the baseline scenario. For newly covered jobs that would otherwise be salaried $S_0 = 1$ and pay $w_0 < \bar{w}$, the fixed cost has one of three possible effects on the Nash bargaining solution, depending on the match quality and salary-fit (θ, F):

[19] While hourly workers' overtime premium is calculated as 0.5 times their wage (i.e. $0.5\frac{w}{h}$ using my notation), under the DOL's rules, the overtime premium for salaried workers is calculated as 1.5 times their salary divided by 40 (i.e. $1.5\ \frac{w}{40}$).

[20] Instead of all matches experiencing the same fixed cost, one can also allow it to vary by job without affecting the predictions. For example, I can model the relative benefit of being salaried as $F = B - C$ and the fixed costs as ρC where $0 < \rho < 1$.

Prediction 1 (Bunching): If the job's income in the benchmark scenario is sufficiently close to the overtime exemption threshold (i.e. $\bar{w} - w_0$ is small), then the Nash bargaining solution is to raise the job's base income to the threshold and increase weekly hours.

Prediction 2 (Reclassification): If the job is not bunched and the cost of reclassifying is smaller than the monitoring cost (i.e. $0 < 2F \leq C$), then the firm would reclassify the job as hourly. Its base income becomes $w_2 = \frac{w_0 - 2\alpha F + (1-\alpha)(V_n^{OT} - V_n)}{1+\eta_{(w,h,S)}}$.

Prediction 3 (Remain salaried): If the job is not bunched and it is very costly to make the job hourly (i.e. $2F > C$), then the firm would keep the job as salaried, and change its base income to $w_2 = \frac{w_0 - \alpha C + (1-\alpha)(V_n^{OT} - V_n)}{1+\eta_{(w,h,S)}}$ to adjust for the overtime premium, the loss in surplus from the monitoring costs, and the change in the worker's outside option.

For a given type a worker, the sign and magnitude of the change in the worker's outside option, $V_n^{OT} - V_N$, depends on the distribution of (θ, F) and the proportion of matches affected by each of the above three responses. If all matches are reclassified or gain coverage, then $V_n^{OT} - V_N < 0$ since workers do not value their pay classification but the added cost to the employer reduces workers' weekly earnings. On the other hand, if all matches are bunched, then $V_n^{OT} - V_N > 0$ if and only if the worker values the increase in earnings more than the loss in leisure. This implies that the base and gross income of reclassified and newly covered employees can either increase or decrease, depending on the value of $V_n^{OT} - V_N$ and the worker's hours of work.

Define a job's total surplus as the sum of the firm's profits and the worker's surplus: $T = J + V_e - V_n$. If both the firm and worker accept a job offer, then the total surplus of the job must be positive. One can show that in the benchmark model, the total surplus at the acceptance cutoff $\theta^*(a, F)$ is equal to zero. By introducing the overtime exemption threshold with fixed costs, the total surplus of salaried jobs earning below the threshold decreases. Given a continuous distribution of (θ, a, F), there must exist matches close to the cutoff $\theta^*(a, F)$ that would have been accepted in the benchmark model, but result in a negative surplus in the model of overtime with fixed costs. These jobs, which are no longer incentive compatible for at least the firm or the worker, are dissolved. This gives a forth prediction of the effect of expanding overtime coverage for salaried workers:

Prediction 4 (Employment Loss): Firms and workers no longer accept some jobs with poor match quality (i.e. θ is small) that would have been accepted if there was no overtime coverage.

3.3 Labor Market Dynamics with Endogenous Contract Rates

Following the conventional approach in the macroeconomics literature, I endogenize the job match creation rate by modeling the firm's decision to create vacancies (see Pissarides (2000) for a review of this approach). Let v be the number of vacancies per worker in the labor force, and u the unemployment rate. Define market tightness as $k = \frac{v}{u}$. Suppose the job match rate follows a constant returns to scale technology

$$m(u, v) = vq(k)$$

where $q(k) = m(\frac{u}{v}, 1)$ is the vacancy filling rate from the perspective of the firm. The job arrival rate (λ in the previous subsection) from the perspective of the worker is $\frac{m(u,v)}{u} = kq(k)$.

Each employer can create a vacancy at a cost $\psi > 0$. The expected value of creating a vacancy, J_v, is characterized by

$$rJ_v = -\psi + q(k)\sigma(\Phi)(J_F - J_v)$$

where $\sigma(\Phi)$ is the probability that a match is accepted by both parties,[21] and J_F is the expected value of a filled vacancy. Suppose that prior to the announcement of the overtime policy, the labor market is in steady state where employers created vacancies until $J_v = 0$. After the announcement of the policy, the expected value of a match $\sigma(\Phi)J_F$ decreases, so that $rJ_v = -\psi + q(k)\sigma(\Phi)(J_F - J_v)$ at the current level of v. In response, firms reduce the number of vacancies, v, until $J_v = 0$.

To characterize the dynamics of u, I assume that the job loss rate equals the job finding rate prior to the announcement of the policy: $\delta(1 - u) = kq(k)\sigma(\Phi)u$. This implies a steady state unemployment rate of

$$u = \frac{\delta}{\delta + kq(k)\sigma(\Phi)}$$

The policy reduces the number of vacancies and the probability that a match is accepted, so the unemployment rate increases. Since firms and workers are forward-looking, the steady state adjusts immediately following the announcement of the policy:

Prediction 5 (Forward Looking): There will be fewer new hires of salaried workers earning between the old and new thresholds following the announcement of the new overtime exemption threshold, even before it goes into effect.

[21] In other words, it is the measure of the set $\Phi = \{(\theta, F, a) | \theta \geq \theta^*(F, a)\}$

Intuitively, this prediction holds even if the job destruction rate (i.e. δ) is an endogenous decision of the firm and incumbent workers have firm-specific human capital. Since layoffs are instantaneous, the firm would not layoff any workers until the policy goes into effect. Between the announcement of the policy and the date that it goes into effect, the firm can either continue hiring workers at the same rate as before, then fire them when the policy becomes binding, or reduce its hires to avoid the vacancy cost. Given large enough vacancy costs, firms would choose the latter and I would expect to observe a reduction in hires immediately following the announcement of the policy.

While the overtime model with fixed costs predicts no real labor market effects on hourly workers, I show in appendix D that by introducing wage rigidity, the model generates incentives to decrease the weekly hours of both hourly and salaried workers with overtime coverage. This nests a key prediction of the classic labor demand model and fits the empirical observation that there is a spike in the hours distribution at 40 hours per week (Ehrenberg, 1971*b*). Furthermore, even without a fixed cost of overtime coverage, the model with downward nominal wage rigidity generates qualitatively similar predictions to the four discussed above. To avoid the cost of overtime, the worker and firm either no longer agree upon an employment contract, or agree to bunch at the threshold, reclassify pay status, or cut hours. However, under the wage rigidity model, only employees initially working above 40 hours per week are affected. Given that I do not observe salaried workers' hours in the data, credibly distinguishing between the model with fixed costs and the model with wage rigidity is beyond the scope of this paper. Instead, I use the predictions of these models as useful guides to my empirical analysis.

4 ADP Data

I use anonymized monthly administrative payroll data provided by ADP LLC, a global provider of human resource services that helps employers manage their payroll, taxes, and benefits. As part of their business operations, ADP processes paychecks for 1 in 6 workers in the United States. Their matched employer-employee panel allows me to observe monthly aggregates of anonymous individual paycheck information between May 2008 and January 2020. The data contains detailed information on each employee's salaried/hourly status, income, hours, pay frequency (i.e. weekly, bi-weekly, or monthly), sex, industry, and state of employment.[22]

[22]For observations prior to 2016, I use workers' state of residence to proxy for their state of employment. This approximation is often implicitly assumed in papers that use the Current Population

A significant advantage of the ADP data over commonly used survey data or other administrative datasets is that it records each worker's standard rate of pay as of the last paycheck in the month, separate from other forms of compensation and without measurement error. This enables me to calculate precisely the measure of weekly base pay that the Department of Labor uses to determine employees' exemption status. For salaried workers, the standard pay rate is the fixed salary they receive per pay-period irrespective of their hours or performance. Following the DOL's guidelines, I compute salaried workers' weekly base pay as the ratio between their salary per pay-period and the number of weeks per per-period.[23] For hourly workers, the standard pay rate is simply their wage. As a simple benchmark to compare the rate of pay for workers who transition between salaried and hourly status, I define the weekly base pay of hourly jobs as 40 times the wage.

In addition to workers' pay rate, the data also records employees' monthly gross pay and monthly overtime pay.[24] For a given worker-month, the gross earnings variable is defined as the total pre-tax remuneration paid over all paychecks issued to the worker in that month, including overtime pay, bonuses, cashed-out vacation days, and meal and travel reimbursements. To express gross pay and overtime pay in the same weekly denominator as base pay, I scale them by the number of paychecks received each month and the number of weeks per pay-period.[25] While the ADP data also has a variable for the total number of hours worked per month, employers only accurately record this information for hourly employees. The hours of salaried workers are often either missing or set to 40 per week. Since employers are not required to keep track of the hours of salaried workers who are not covered for overtime, this limitation is likely endemic to all administrative firm datasets.

For each of my analyses, I restrict the sample to a balanced panel of employers. The entry and exit of firms in the data reflect both real business formations and the decision of existing firms to partner with ADP. I find that the flow of firms into the ADP sample deviates from

Survey. Testing the validity of this assumption in the post-2016 ADP data, I find that 95.5% of workers work in the same state that they live.

[23] For example, a salaried worker with a statutory pay of \$3000 per month would have a weekly base pay of $\$3000 * \frac{12}{52}$=\$692.31.

[24] I impute overtime pay from a variable that often reports overtime earnings, but may occasionally include other forms of compensation. I consider the variable to capture overtime pay if the implied overtime rate ($\frac{\text{OT Pay}}{\text{OT Hours}}$) is no greater than 2 times the regular pay rate ($\frac{\text{Base Pay}}{40}$). See Appendix E for more details.

[25] I only observe workers' number of paychecks per month starting in 2016. Prior to 2016, I impute the number of paychecks at the employer-level by comparing each firm's average gross pay each month to the median of their average monthly pay over a year, as described in detail in Appendix E

the Business Formation Statistics published by the U.S. Census.[26] To prevent the selection of firms from biasing my estimates, I focus specifically on continuously operating employers.

5 Firm Outcomes: Employment, Bunching, and Reclassification

In this section, I estimate the effect of raising the overtime exemption threshold on the number of salaried and hourly workers along the base pay distribution. This will allow me to measure the change in total employment, identify whether firms bunched workers above the new threshold, and test if firms substituted away from salaried jobs for hourly jobs. I start with a case study of the federal rule changes, and then implement an event-study using the state-level variation.

5.1 Federal Policies

Graphical Evidence. To begin, I present evidence that although the 2016 policy was never legally binding, companies nevertheless responded to the proposed overtime exemption threshold. In figure 2a, I overlay the frequency distribution of salaried workers' base pay in April 2016 and December 2016, averaged over the balanced panel of firms that are observable in both months. Since my empirical strategy will rely on the stability of the base pay distribution over time, I only count employees in the 22 states that did not change their state or local minimum wages after 2011.[27] Moreover, I drop the largest 0.1% of firms since small percent changes in their employment levels across years has very pronounced effects on the average change in employment across all firms.[28] I will test the robustness of my results to relaxing these restrictions in my analysis.

Reviewing figure 2a from left to right, three features stand out. First, there are very few workers below the old threshold of $455 per week and a noticeable increase in the distribution at exactly the old threshold. This suggests that firms were cognizant of the initial overtime

[26]For a detailed analysis of the representativeness of the ADP data, refer to (Grigsby, Hurst and Yildirmaz, 2021). In short, they find that while the data closely matches the demographics of workers in the Current Population Survey, it over represents mid-sized firms with 50 to 5000 employees relative to the Business Dynamic Statistics.

[27]These states are Alabama, Georgia, Idaho, Indiana, Iowa, Kansas, Kentucky, Louisiana, Mississippi, New Hampshire, New Mexico, North Carolina, North Dakota, Oklahoma, Pennsylvania, South Carolina, Tennessee, Texas, Utah, Virginia, Wisconsin, and Wyoming. They account for 35% of all workers in the data.

[28]This restriction drops 41 firms, accounting for 11% of all workers in the sample in 2016. I do not make this restriction in my analysis of the state policies where large firms in control states adequately control for the year-specific fluctuations of the same firms in the treatment states.

exemption threshold and adjusted their operations to have very few salaried workers earning below that cutoff.[29] Second, there was a large drop in the number of workers with base pays between the old and new thresholds between April and December. Firms employed on average 8.4 salaried workers with base pays between \$455 and \$913 in April 2016, and only 6.8 such workers in December 2016 - a decrease of 19%. Third, there is a large spike in the distribution at \$913 that appears in December but not April, indicating that firms raised some workers' salaries above the new threshold. These features are even more evident in figure 2b where I plot the difference between the two distributions in figure 2a. As a placebo check, I also overlay the difference-in-distributions between April and December of each year from 2012 to 2015. Consistent with prediction 1 of the job-search model, firms bunched workers' base salaries at the new \$913 overtime exemption threshold in 2016, but not in any of the four preceding years. Furthermore, the lack of any spikes in the left tail of the distribution suggests that firms did not reduce workers' base pay to offset the cost of overtime, contrary to the prediction of the compensating differentials model.

Replicating the same graphs for hourly workers, figure 2c depicts the frequency distribution of hourly workers' base pay in April and December 2016. Compared to salaried workers, there are twice as many hourly workers and the distribution of their base pay is heavily right-skewed. To distinguish the effect of the policy from natural employment growth, I compare the change in hourly employment in 2016 to its growth in previous years in figure 2d. Consistent with prediction 2 that firms reclassify newly covered workers from salaried to hourly, I find that the number of hourly jobs earning between \$455 and \$913 increased more in 2016 than any previous year.

As further evidence that the change in the distributions of base pay reflect a behavioral response to the nullified policy, I examine their evolution over time. Figure 3 plots the average salaried distribution of each month in 2016 and 2017, subtracted by the distribution in April 2016, for a balanced panel of firms. For example, the December 2016 graph in Figure 3 is similar to the blue line in Figure 2b, but for employers that remain in the sample until December 2017. I find that the timing of the growth and decay of the spike at \$913 corresponds precisely with the history of the FLSA policy. After the announcement of the policy in May 2016, firms start reducing the number of salaried employees between the old and new thresholds, and bunching workers at the new cutoff. This bunching experiences a large increase in December 2016 when the rule change was supposed to go into effect. Since the new threshold was not binding, firms slowly stopped bunching base pay at \$913 per week

[29] To see the bunching at the initial threshold more clearly, figure A.3 plots the distribution of salaried jobs using finer increments of base pay.

after January 2017.[30] I plot a similar graph in Appendix figure A.4 to examine the evolution of the hourly distribution, but it is difficult to distinguish the effect of the policy on this distribution from natural wage and employment growth.

Constructing the Counterfactual Distribution. To identify the effect of the 2016 FLSA policy on the frequency distribution of base pay, I use the change in the distribution between April and December 2015 as a counterfactual. I account for year-specific aggregate employment growth by applying a linear transformation to the difference-distribution in 2015 so that the counterfactual employment growth for jobs paying well above the new threshold closely matches the observed change in employment in 2016.[31] Following recent advancements in the minimum wage literature, I estimate the aggregate employment effect of raising the OT exemption threshold by first estimating its effect on the number of workers within each bin along the distribution of weekly base pay, and then integrating these effects across all bins (Cengiz et al., 2019; Derenoncourt and Montialoux, 2019; Harasztosi and Lindner, 2019; Gopalan et al., 2020). In my analysis, I treat the salaried and hourly distributions within each firm as independent observations and cluster estimates at the firm-level.

Formally, let n_{ijkmt} be the number of workers employed at firm i, with pay classification j and base pay in bin k, during month m of year t. I model the number of workers within each firm-classification-bin in December of year t as follows:

$$n_{ijk,Dec,t} = n_{ijk,Apr,t} + \alpha_{jkt} + \beta_{jk} \cdot D_{t=16} + \varepsilon_{ijkt} \tag{2}$$

where α_{jkt} represents the average change in the number of workers with classification j and bin k between April and December of year t, absent the policy. The variable $D_{t=16}$ is a dummy variable for the year 2016 and the coefficient β_{jk} is the causal effect of increasing the overtime exemption threshold on the number of workers in classification-bin jk.

To separately identify the β_{jk}'s from the α_{jkt}'s, I make two modeling assumptions:

$$\beta_{jk} = 0 \text{ for every } k \geq k^*$$

$$\alpha_{jkt} = \gamma_1 \alpha_{jk,t-1} + \gamma_0$$

The first assumption states that the policy has no effect on the number of workers earning above a cutoff bin k^*. This claim is supported empirically by the lack of movement in

[30] Refer to Quach (2020) for an analysis of the persistence of the 2016 FLSA policy and its implications for wage rigidity.

[31] Graphically, this is equivalent to vertically stretching/compressing and shifting the 2015 difference-distribution in figures 2b and 2d to fit the right tail of the 2016 distribution.

the upper tail of the difference-distribution between November and December 2016 in the time series profile depicted in figure 3. The second condition states that the distribution of changes in employment between April and December is similar across years, up to a linear transformation. This assumption is supported by the observation in figures 2b and 2d that the difference-distributions have similar shapes each year aside from 2016.[32] The stability of the change in distribution each year suggests that the difference-distribution in 2015 is a reasonable approximation for how the distribution in 2016 would have evolved if not for the new overtime exemption threshold.

Under these assumptions, I show in appendix F that an unbiased estimator of β_{jk} for any $k < k^*$ is

$$\begin{aligned}\hat{\beta}_{jk} &= \left(\bar{n}_{jk,Dec,t} - \bar{n}_{jk,Apr,t}\right) - \hat{\gamma}_1\left(\bar{n}_{jk,Dec,t-1} - \bar{n}_{jk,Apr,t-1}\right) - \hat{\gamma}_0 \\ &= \Delta\bar{n}_{jkt} - \hat{\gamma}_1\Delta\bar{n}_{jk,t-1} - \hat{\gamma}_0 \end{aligned} \tag{3}$$

where $\bar{n}_{jkmt}$ is the average n_{ijkmt} across all firms, and $\hat{\gamma}_1$ and $\hat{\gamma}_0$ are estimated from

$$\Delta\bar{n}_{sal,kt} = \gamma_1\Delta\bar{n}_{sal,k,t-1} + \gamma_0 + \epsilon_{sal,kt} \tag{4}$$

using only salaried workers with bins $k \geq k^*$. I restrict the sample to only salaried workers when estimating equation 4 since changes in employment in the right tail of the hourly distribution, where there is very little mass, reflect more noise than aggregate employment fluctuations.

To develop an intuition for equation 3, notice that if $\hat{\gamma}_1 = 1$ and $\hat{\gamma}_0 = 0$, then the treatment effect of the policy is simply a difference-in-difference using the year prior to the policy as the control group. On the other hand, if employment growth in year $t-1$ is uninformative about the growth in year t (i.e. $\hat{\gamma}_1 = 0$), then $\hat{\gamma}_0$ is simply the average employment growth at the top of the distribution in year t. In that case, equation 3 is akin to a difference-in-difference between low and high income jobs within the same year. The estimator nests both these models, and selects the parameters that best predicts the change in employment at the upper tail of the base pay distribution in year t. To test the validity of this model, I run a series of placebo tests by estimating equation 3 using each pair of adjacent years from 2011 to 2015. Since the policy did not occur prior to 2016, the estimates of the β_{jk}'s in these placebo tests should be close to zero.

[32]These assumptions are similar to the ones made by Defusco, Johnson and Mondragon (2019) to generate the counterfactual number of loans along the distribution of debt-to-income (DTI) ratios absent a regulatory rule that made it more difficult to give mortgages to individuals with a DTI above 43%.

In practice, I choose bins of width \$96.15 $\approx \frac{5000}{52}$ because the base pay of salaried workers exhibit bunching at values corresponding to annual salaries of multiples of \$5000. I use the 9 bins greater than or equal to $k^* =$ \$1778 to estimate equation 4. A benefit of selecting a large k^* is that it allows me to test the accuracy of the model by seeing whether it eliminates the spikes between the new threshold of \$913 and k^*.

Estimates of Employment Effect Across Distribution of Base Pay. I plot in figure 4a the bin-by-bin treatment effects estimated from equation 3 for the frequency distribution of salaried workers, and the integral of these treatment effects over the entire distribution. By construction, the identification strategy minimizes the magnitudes of the treatment effects above \$1778. As a falsification test, the model also estimated small effects right below \$1778 where the new overtime exemption threshold of \$913 is unlikely to have any effect. Examining the integral of the bin-specific treatment effects, I find that the large drop in the number of workers between the old and new threshold exceeds the spike in the number of workers above the new threshold, implying a net loss in the number of salaried employees. As a placebo check, I estimate equation 3 using adjacent years of data between 2011 and 2015, and plot their respective integrals in figure 4b. Compared to the estimate of the causal effect for 2016, the placebo effects are relatively small, indicating that the econometric model successfully generates the counterfactual distribution for each year prior to 2016.

Repeating the analysis for hourly employees, figure 4c shows the effect of the policy on the number of hourly workers within each bin of weekly base pay. Firms decreased the number of hourly workers in the bin immediately below the old threshold, and increased the number of workers between \$432 and \$1009. Cumulatively, there is a net increase in the number of hourly workers, but it appears smaller in magnitude than the decrease in salaried workers. Applying the model to the frequency distributions of hourly workers in the four years prior to 2016, I show in figure 4d that the cumulative effect is relatively flat in each of the placebo years, and do not exhibit the sharp increase in hourly workers between the old and new threshold that is present in 2016.

In table 2, I report estimates of the bunching, reclassification, and employment effects of the 2016 FLSA policy. The estimates in column (1) correspond to sums of the bin-specific treatment effects graphically depicted in figure 4, divided by the number of salaried workers between the old and new thresholds in April 2016. In this benchmark specification, I find that the 2016 FLSA rule change decreased the number of salaried jobs paying below the new threshold by 20.5% (s.e. 1%). The finding that most salaried workers remain in the affected interval is consistent with prediction 3 of the job-search and bargaining model that it may be too costly for firms to adjust certain jobs. Following the other predictions of the model, I find that the reduction in low paying salaried jobs is due to three adjustments by the firm. First, 5.2% (s.e. 0.8%) of affected workers were given raises above the new cutoff, thereby keeping them exempt from overtime. Second, 11.2% (3.7%) of jobs were reclassified from salaried to hourly.[33] Third, 4.2% (s.e. 4.2%) of jobs are unaccounted for by either an increase in salaried jobs above the threshold or an increase in hourly jobs, and were therefore lost via a change in employment.

To test the robustness of my results, I run four additional specifications using different sample selections and model parameters. Each specification changes one property relative to the baseline specification. In column (2), I calculate firms' employment over all states covered by the FLSA overtime exemption policy, rather than just states without a state-specific minimum wage change. In column (3), I include the largest 0.1% of firms in the sample. In column (4), I allow for firm entry and exit into the data by keeping all firms that appear in either April or December, and filling in any missing month's employment as 0. In column (5), I estimate the parameters of the linear transform from equation 4 using all bins greater than or equal to \$1393. Overall, the estimates from these alternative specifications are similar to the baseline estimate. In all cases, there is a significant reduction in the number of salaried jobs below the threshold, bunching above the threshold, and an increase in the number of hourly jobs. Moreover, the net change in employment is negative, except for when I include the largest 0.1% of firms. However, while the estimates of the treatment effects are fairly stable across specifications, the placebo tests can vary substantially. In particular, I show in appendix figures A.5 and A.6 that the placebo effects deviate from zero if I include states that change their minimum wages, the largest 0.1% of firms, or firm entry and exit, indicating that the identification assumptions require the sample restrictions that I imposed in the benchmark model.[34]

In column (6) of table 2, I apply my baseline specification to estimate the employment effects of the 2020 federal policy. These estimates represent the change in employment between the month before the announcement of the new threshold (August 2019) and the

[33]While the rise in hourly jobs is also consistent with firms laying off salaried workers and hiring new hourly ones, I show in section 7.1 that nearly the entire increase in hourly jobs is explained by the reclassification of continuously employed workers and not changes in hiring.

[34]As another robustness check, I also estimate equation 3 using only firms in California and New York. I present these estimates graphically in appendix figure A.7. Unlike the FLSA states, California and New York already had overtime exemption thresholds of \$800 and \$675 per week, respectively, so I would expect to see smaller employment effects. Consistent with this prediction, I find that the decline in salaried employment in these two states is concentrated above the initial state thresholds.

month that the new threshold went into effect (January 2020).[35] Since the 2020 policy targeted far fewer people than the 2016 policy, the estimated effect per exposed worker is less precise. Nevertheless, I find clear evidence that firms raised some salaried workers' base pays above the new overtime exemption threshold. I also verify this graphically in appendix figure A.8. However, it is less clear how the 2020 FLSA policy affected the hourly distribution or aggregate employment.

Overall, the results from my analysis of the federal rule changes provide strong evidence that firms bunched workers' salaries above the overtime exemption threshold to keep them exempt from overtime, and in the case of the 2016 policy, also reclassify a significant share of affected workers from salaried to hourly. While the bunching effect contradicts the predictions of the compensating differentials model of overtime, the main results that some affected workers remain salaried, some receive raises to above the threshold, and some are reclassified from salaried to hourly are all consistent with prediction 1 to 3 of the job search and bargaining model presented in section 3.

5.2 State Policies

Methodology. To estimate more precise measures of the employment effect, I execute an event-study analysis using the state rule changes. An advantage of this approach over the cross-year comparison is that I am able to account for bin-specific confounders that vary over time by using the states covered by the FLSA as a control group. For each of the 16 events, I create a dataset that decomposes firms' employment by treatment-control group, where each firm in the control group consists of all its employees across the 46 FLSA states. To average my estimates across events, I normalize base pay in each event relative to the new threshold, and time relative to the date of the rule change. Appending the 16 datasets together, I estimate a event-study stacked regression.

Formally, for each event v, let n_{ikstv} be the number of workers in firm i, with base pay between $40k$ and $40(k+1)$ of the new threshold, in treatment-control state s, at t months from the date of the rule change.[36] Since firms in the control group have, on average, larger initial employment levels compared to firms in the treatment group, they experience larger fluctuations in the number of workers even if employment grows at the same rate in both groups. Moreover, even if firms in the treatment and control groups were the same size,

[35] Unlike the analysis for the 2016 FLSA policy, I only sum the bin-specific estimates up to \$876 to avoid capturing any "unbunching" at the \$913 threshold instigated by the nullified 2016 policy

[36] To simplify notation, I drop the index for pay classification. In my analysis, I estimate each regression separately for salaried and hourly employees.

a similar problem arises if the share of employment differs across the pay distribution. To account for these differences in initial employment, I scale the employment of firms in the control state by the ratio of the average firm size between the treatment and control states two months prior to the event, separately for each bin of base pay:

$$\tilde{n}_{ikstv} = \begin{cases} n_{ikstv} & \text{if } s = \text{treatment} \\ n_{ikstv} \cdot \frac{\bar{n}_{k,treat,t=-2,v}}{\bar{n}_{k,control,t=-2,v}} & \text{if } s = \text{control} \end{cases} \tag{5}$$

In effect, the rescaling transforms the distribution of base pay in the control group to exactly match the distribution of base pay in the treatment group two months before the threshold change.

Taking the scaled employment variable as the outcome, I estimate the following stacked regression:

$$\tilde{n}_{ikstv} = \sum_{\substack{t=-6 \\ t\neq-2}}^{5} \sum_{k=-6}^{15} \beta_{kt} \cdot I_{kst} + \alpha_{ksv} + \delta_{ktv} + \varepsilon_{ikstv} \tag{6}$$

where the treatment dummy I_{kst} equals 1 for the treatment state at normalized bin k, and event time t. I set the reference date as two months prior to the rule change to capture any anticipatory responses, which may be important given the early responses of firms to the 2016 FLSA policy.[37] Since all states increase their thresholds with at least one year between each event, I define the event window to include 6 months before the rule change and 5 months after so that none of the events overlap each other within the same state. My benchmark specification includes bin-state-event (α_{ksv}) and bin-month-event (δ_{ktv}) fixed effects to control for state-specific differences in the base pay distribution and nationwide changes in inequality, respectively. Intuitively, equation 6 is equivalent to estimating 16 individual differences-in-differences and then taking a weighted average of the treatment effects to compute β_{kt}.[38] The identifying assumption is that absent the state threshold changes, the frequency distribution of base pay in the treated states would have evolved the same as the scaled control states. I cluster standard errors at the firm-level to account for the correlation between changes in employment across bins within firm.

[37] For the three state policies that went into effect on Jan 1, 2017, I set the reference month as event time -1 because otherwise the estimate will capture both the effect of the 2016 FLSA policy and the state policy.

[38] Relative to event study designs that organize the data in calendar time, this model avoids contaminating estimates of the pre-trend with effects from the post-period across events (Sun and Abraham, 2020).

Estimates of Employment Effect Across Distribution of Base Pay. Figure 5 shows the estimates of the treatment effect from equation 6, separately for the distribution of salaried and hourly workers. In figure 5a, I plot the effect of raising the overtime exemption threshold on the number of salaried workers at event time 0 when the new threshold first becomes binding. Similar to the effect of the federal policies, there is a net decrease in the number of salaried employees below the new threshold and a spike in workers right above it. Aside from two events in New York that raised the overtime exemption threshold by about \$150, all other rule changes were no more than \$80. Consequently, most of the decrease in salaried employment is concentrated within \$80 below the new threshold.[39] As a placebo check, I find little effect on any individual bin of base pay above the new threshold. To measure the total employment effect, I will aggregate the estimates across base pays between -160 and 80 dollars relative to the new threshold.

In figure 5b, I plot the change in the number of salaried workers paid below and above the new threshold over event time, relative to the employment level two months before the rule change.[40] Examining the figure from left to right, four features stand out. First, there is little evidence of a pre-trend for either graph prior to the month that the policy goes into effect, indicating that employment in the control groups was evolving at the same rate as the treatment group. Second, there is a sharp drop in the number of jobs below the threshold and a sharp increase in the number of jobs above it at precisely the month of the rule change, consistent with the bunching from the cross-sectional estimates in figure 5a. Third, the magnitude of the decrease in employment below the threshold is visibly larger than the increase in employment above it. Fourth, the number of salaried workers above the new threshold remains relatively stable after it goes into effect, whereas the number of workers below it continues to decrease.

Plotting analogous figures for hourly workers, I find that the base pay distribution for hourly employees responded in a qualitatively similar fashion to the distribution for salaried employees. I show in figure 5c that raising the state overtime exemption threshold cut hourly jobs earning between the old and new thresholds, and increased the number of hourly jobs above it. This is in contrast to the effect of the 2016 FLSA policy, which increased hourly jobs across the entire affected interval of base pay. I confirm the bunching effect in figure 5d where I plot the evolution of the hourly employment estimates below and above the threshold over event time. Mirroring the estimates for the salaried distribution, I find no pre-trend prior to

[39] I drop observations with normalized base pays equal to or less than -160 for the events in Maine because those bins coincided with income levels affected by Maine's minimum wage changes.

[40] I drop the California 2020 event from the sample because the data ends on January 2020.

the rule change, and a sharp divergence in employment between these two groups at exactly the month of the rule change. This bunching of hourly employees is consistent with growing evidence that workers care about their pay relative to their peers (Card et al., 2012; Dube, Giuliano and Leonard, 2019).

While the total number of hourly workers barely changed as a result of the state reforms, this small net effect masks an employment and a reclassification effect that cancel each other out. To directly observe these margins of response, I estimate equation 6 using monthly employment flows (i.e. hires minus separations) and monthly reclassification flows as the outcome variable. Figure 6 plots the estimated effects on these flows over time. First, I confirm in figure 6a that there was indeed a drop in the employment flow of salaried workers at precisely the month of the rule change, and this was concentrated solely among jobs with base pays in the treated interval. Second, I find in figure 6b that there was also a decrease in the employment flow of hourly employees on the month that the threshold increased. Third, counterbalancing the employment loss of hourly jobs, figure 6c shows that there was a sharp increase in the number jobs being reclassified from salaried to hourly, and this reclassification effect persists even past the month that states raised their thresholds. This is consistent with the persistent drop in salaried jobs and rise in hourly jobs below the threshold depicted in figure 5. Lastly, in figure 6d, I show that after a policy change, workers who are reclassified from hourly to salaried are more likely to earn above the new threshold. Together, these results indicate that underlying the net changes in salaried and hourly employment, firms are responding via both the employment and classification margins.[41]

Table 3 summarizes the aggregate employment effects across the salaried and hourly base pay distributions. In column (1), I report sums of the bin-specific estimates from my baseline specification at event time 0, scaled by the average number of salaried workers between the old and new thresholds two months prior to the rule change. Similar to the effect of the federal threshold changes, I find that the number jobs in the affected interval of base pay fell by 20.8% (s.e. 1.2%). However, what happened to these jobs differs significantly between the large 2016 federal policy and the smaller state policies. In comparison to the federal policy, firms did not increase the number of hourly workers in response to the state policies, and bunched more workers at the new threshold. These differences can be explained by the difference in the size of the threshold change between the federal and state reforms. Since the state threshold increases were much smaller, a greater share of affected workers had initial

[41] In table A.1, I show that more than half the decrease in salaried jobs is due to reclassification. For each affected salaried job, 0.033 jobs are reclassified from salaried to hourly. This is less than the 0.112 reclassified jobs for each worker affected by the 2016 FLSA policy.

base pays close to the new threshold and were therefore cheaper to bunch. After accounting for the movement of affected jobs to above the threshold and to the hourly distribution, I find that 5.9% (s.e. 2.0%) of jobs were lost due to a reduction in employment. This effect is similar in magnitude to the employment loss of the 2016 FLSA policy, but more precisely estimated. The 95% confidence interval implies that at least 2 jobs were lost for every 100 workers directly affected by the rule change. This negative employment effect is consistent with prediction 4 of the job search and bargaining model.

I assess the robustness of my results to additional controls and alternate samples in columns (2)-(5) of table 3. I estimate in column (2) the employment effect five months after the threshold increase. In column (3), I restrict the sample within each event to firms that employ workers in both the treatment and control states. This controls for any differences in employment driven by differences in the composition of firms between the treated and control states. In columns (4), I drop the three state threshold increases that take place on January 1, 2017 to eliminate any confounding effects from firms' response to the nullified 2016 FLSA rule change. Column (5) further restricts the sample to only the six threshold increases that occurred prior to 2016. This removes any biases from not accounting for the geographical variation in the threshold that was introduced in New York and California after 2016. In general, the magnitude of the bunching, reclassification, and employment effects are similar across all specifications.

In column (6) of table 3, I average the employment effects across all the state and federal policies, by estimating the following stacked difference-in-difference regression:

$$\tilde{n}_{ikstv} = \alpha_{kv} \cdot After_t + \alpha_{kv} \cdot Treat_s + \beta_k \cdot After_t \cdot Treat_s + \varepsilon_{ikstv}$$

This regression is similar to equation 6 except I collapse the data to only two time periods and two bins of base pay: one for below the new threshold and one for above. That way, the reference period and the bin-widths can vary between the federal and state rule changes. For the federal policies, the time dummy $After_t$ equals 0 on the month before the announcement of the reform, and the bins span from \$215 less than the old threshold to \$192 above the new one. For the state policies, $After_t$ equals 0 two months before the new threshold goes into effect, and the bins range between \$160 less than the new cutoff and \$80 above it. In both cases, $After_t$ equals one on the month that the threshold increases and $Treat_s$ equals 1 for the treatment group, where the treatment and control groups are defined as in the individual state and federal analyses. As in equation 6, I allow each event-bin to have its own time and treatment group fixed effects.

As expected, the estimates of the pooled regression imply that after an increase in the overtime exemption threshold, firms raise some workers salaries above the new cutoff, reclassify other workers, and reduce employment. The aggregate effects are not exactly equal to a weighted average of the previous estimates since the pooled regression cuts the base pay distribution into fewer bins. Nevertheless, the effects are relatively the magnitudes that I would expect. The point estimate of the employment effect implies that for every one hundred workers directly affected by an increase in the overtime exemption threshold, 4.9 (s.e. 2.3) jobs are lost. The 95% confidence interval rules out any employment losses less than -0.3% or greater than 8.9%. Thus, contrary to the prediction of the standard labor demand model with small scale effects, the evidence suggests that raising the overtime exemption threshold likely decreased aggregate employment.

6 Worker Outcomes: Base Pay and Overtime Pay

In this section, I estimate the effect of raising the overtime exemption threshold on workers' incomes using a difference-in-difference design where I compare continuously employed workers initially earning a salary between the old and new thresholds to similar workers unaffected by the new policy. My baseline regression is

$$y_{ivt} = \sum_{t=T_0}^{T_1} \beta_t \cdot I_{st} + \alpha_{vs} + \delta_{vt} + \varepsilon_{ist} \tag{7}$$

where y_{ivt} is worker i's compensation at event time t for event v, and I_{st} is an indicator that equals 1 at month t for workers in the treatment group. I control for event-group (α_{vs}) and event-month (δ_{vt}) fixed effects. The identifying assumption is that absent the reforms, workers' income in the treatment and control groups would have evolved similarly.

As in section 5, I identify the counterfactual to the federal and state policies using two different methods. For my evaluation of the 2016 and 2020 FLSA policies, I consider a worker to be in the treatment group if they are paid a salary between the old and new thresholds on the month before the new rule is announced. I define the control group to be salaried workers who earn within the same interval on the same month in the preceding year. As earlier, I only consider states without state-specific minimum wages when evaluating the federal FLSA policies. In my event-study analysis of the state policies, the treatment group consists of salaried employees with base pays in the treated interval two months before a state raised its overtime exemption thresholds.[42] The control group consists of workers within

[42]To separately identify the effect of the three state policies that went into effect on January 1,

the same income bracket working in the 46 states where the FLSA threshold is binding. In all cases, I restrict the sample to workers who are continuously employed at the same firm in all months of the event window.

While restricting the sample to stayers is necessary since I cannot observe the income of job switchers who leave the ADP sample, it may introduce selection bias because post-policy employment is an endogenous outcome. For instance, if the policy causes firms to disproportionately layoff workers with low expected wage growth, then my empirical strategy would over-estimate the true income effect. To address this, in appendix figure A.9, I compare the probability that workers in the treatment and control groups remain with their employer following the announcement and enactment of a higher overtime exemption threshold. In general, I find no trend break in the survival function of workers in the treatment group relative to the control group due to the federal policy changes, but a small increase in separations from the state policies.

Estimates of Income Effect. Figure 7 plots the difference-in-difference estimates for all three policy evaluations: the cross-year analyses of the 2016 and 2020 rule changes, and the event-study of the state reforms. For the federal policies, I indicate both the month that the new threshold is announced and the month that it went into effect, whereas for the event-study, I only indicate the latter. The dependent variable is weekly base pay in the top three figures, and weekly overtime pay in the bottom three figures.

Reviewing all six graphs in figure 7, there are five key features to highlight. First, in all cases, the treatment and control groups were trending similarly prior to the announcement of the new rule, suggesting that the identification assumption holds. Second, following the announcement of the federal policies, workers' income begin to rise even before the new FLSA threshold goes into effect. Third, unlike the federal policies, workers' income only begin to rise one month prior to an increase in the state threshold. A possible explanation for this difference in anticipatory response is that it simply costs less for firms to quickly adjust to a small change in the threshold relative to a large change. Forth, in all cases, workers experience a sharp jump in their base pay and overtime pay at precisely the month that the threshold increases, and this raise remains fairly stable after the policy goes into effect. Lastly, across all three sources of policy variation, I find that the average worker experiences a larger increase in base pay than overtime pay. These results reject the primary prediction of the compensating differentials model that firms would cut workers' base salary to nullify

2017 from the nullified 2016 FLSA policy, I define the treatment and control groups of those state policies using workers' income on December 2016 rather than November 2016.

the costs of overtime coverage.

I summarize the income effect of expanding overtime coverage for salaried workers in table 4. The first two rows report the increase in base pay and overtime pay, respectively, as of the first month that the new threshold goes into effect. I compute the sum of these two estimates in row (3), which I denote as the effect on workers' total income. Dividing the change in total income by the average income at baseline, I show that average total income increased by 1.2% (s.e. 0.1%) due to the 2016 FLSA policy, 2.1% (0.5%) due to the 2020 FLSA policy, and 1.4% (0.1%) due to the state policies. Alternatively, I also compare the change in log total income between the treatment and control group over time, which measures the average percent change in income rather than the percent change in the average income, and find similar magnitudes.

By construction, if a worker is reclassified from salaried to hourly, I defined the weekly base pay component of their total income as forty times their wage. While this is a useful benchmark for comparing the income of jobs with different weekly hours, it overstates the actual earnings of workers who work less than forty hours per week. To measure the effect of expanding overtime coverage on workers' realized earnings, I also estimate equation 7 using log gross pay as the outcome variable.[43] While these numbers suggest that the 2016 FLSA policy had no effect on gross pay, this is likely due to imputation error in translating monthly gross pay to an average weekly amount without observing the number of paychecks that workers received each month before 2016.[44] As evidence of this, I report in column (4) the income effect of the six state threshold increases that occurred prior to 2016. Similar to the estimates in column (1), I find positive significant effects on base pay and overtime pay, but no effect on gross pay. In contrast, the gross pay effect is twice as large in column (3) when I include all sixteen state policies. Focusing on just the 2020 FLSA policy where weekly gross pay is defined without imputation error, the estimate suggests that gross pay increased by 1.2% (s.e. 0.4), which is less than but similar to the estimated effect on total pay.

In column (5) of table 4, I report the estimates of the income effect averaged across all

[43] I censor gross pay at two times total pay. The estimates of the effect on both censored and uncensored gross pay are presented graphically in appendix figure A.10. While the magnitudes of the estimates are similar, the latter is more volatile. Note also that the event-study estimates exhibits a spike in gross pay the month prior to the event date. This is entirely driven by workers in New York, and exists even for individuals earning well above the interval treated by the change in the threshold. Since this spike does not appear in base or overtime pay, it likely reflects larger end of year bonuses in New York compared to other states.

[44] See appendix E for the imputation procedure.

18 policy changes.[45] As expected, there is a positive effect on total income that is primarily driven by an increase in base pay. In column (6), I show that these estimates are robust to restricting the sample to only firms that employ workers in both the treatment and control group within each event. Lastly, as a placebo check, I examine the effect of the threshold changes on workers who were already earning \$40 to \$80 above the new threshold. I show in column (7) that while the placebo workers experienced a small increase in their earnings, the percent change in their income is an order of magnitude smaller than for directly affected workers.

To compare the employment and income effects of raising the overtime exemption threshold to those of other labor market policies, I divide the percent change in employment in column (6) of table 3 by the percent change in total pay in column (5) of table 4 to compute an elasticity of employment with respect to own wage of -3.25 (s.e. 1.71). The magnitude of this elasticity is very large relative to previous estimates of labor demand elasticities in the literature. For comparison, a meta-analysis by (Dube, 2019) finds a median elasticity of -0.17 across 36 studies of the minimum wage in the U.S, with only 2 studies observing elasticities less than -2. I can rule out elasticities more positive than -0.17 at the 93% confidence level. However, given the large standard errors, I am unable to reject that raising the overtime exemption threshold has the same elasticity as modest estimates from the minimum wage literature.

I estimated the above elasticity with respect to the income and employment of directly affected salaried workers in all firms. Instead, if I drop firms that had no salaried workers in the treated interval prior to the policy change, I get a more precise estimate of -2.85 (s.e. 1.14) and can reject elasticities more positive than -0.62 at the 95% level. To account for spillovers, I also compute the elasticity with respect to all workers (i.e. salaried and hourly) in the affected interval of base pay. Since this includes workers not impacted by the reforms, I find a smaller elasticity of -0.90 (s.e. 0.54). In all cases, the elasticity of employment with respect to earnings are within the range of larger estimates in the minimum wage literature.

7 Implications for Redistribution

The results thus far suggest that in response to an increase in the overtime exemption threshold, firms decreased aggregate employment, reclassified jobs from salaried to hourly, and raised average incomes. However, it is still unclear precisely which jobs were lost and

[45] These are estimated using equation 7 but keeping only two months of data per policy change: the reference month and the month that the threshold increased.

which workers' earnings went up.

In this section, I examine how the margins of adjustments vary along the distribution of base pay to determine which workers benefited and which workers lost as a result of the expansion in overtime coverage. Given that the state policies were too small for the labor market responses to vary significantly along the intervals of affected base pay, my analysis will focus on the large 2016 FLSA policy that attempted to double the federal exemption threshold from $455 per week to $913 per week. Leveraging the matched employer-employee matched panel structure of the data, I categorize workers in April and December 2016 as either stayers, new hires, or separations. Among stayers, I further partition the sample into those who switched pay classifications and those who maintained the same salaried/hourly status in both months. Collapsing each of these subsamples by pay classification and base pay, I measure the employment flows, reclassification flows, and within-classification flows along the income distribution. Following the discussion in section 5.1, I estimate the effect on each of these measures by comparing the distribution in 2016 to a linear transformation of the distribution in 2015 (see equation 3).

7.1 Which Workers Benefited?

I begin by documenting which workers experienced the largest increase in income. In figure 8, I plot the evolution of affected workers' income separately by their salaried/hourly status and base pay in December 2016 after the policy change. For comparison, I also include the income of salaried workers in the year before the rule change. From this figure, I infer that the bulk of the positive base pay effect accrued to workers who received a raise above the new threshold. Although part of the increase in base pay among this group is simply mechanical from conditioning the sample on individuals' post-policy income, no other group of workers experience the sharp rise in base pay on December 2016 that matches the results in section 6. By a similar argument, the figure implies that most of the increase in overtime pay is attributed to reclassified workers. Given that the policy had a larger effect on base pay than overtime pay (see section 6), and fewer workers were bunched than reclassified (see section 5), this figure suggests that bunched workers received the largest increase in earnings. Interestingly, it appears that workers who remain salaried but did not get bunched were already likely to be receiving overtime compensation before the reform, and experienced no change in income as a result of the policy. Next, I will determined from where along the base pay distribution were workers bunched and reclassified.

Within-Classification Flows

To identify which workers were given raises above the new threshold, I apply the empirical strategy from section 5 to estimate the effect of the 2016 overtime proposal on the distribution of always-salaried workers.[46] Since I condition the sample on workers being salaried post-reform, I require two identifying assumptions in addition to those described in section 5. First, I assume that the policy has little effect on the distribution of separations from employment. Second, I assume that workers who were reclassified as a result of the policy would have earned a similar base pay in the absence of the policy. I discuss these assumptions in detail in appendix F, and show that they are reasonable given my analysis of the separation and reclassification effects in the subsequent analysis. Under these assumptions, my empirical strategy identifies the causal effect of the policy on the distribution of base pay of always-salaried workers.

I present the estimates of my analysis in figure 9, using \$20 increments of base pay. The figure shows that the spike at the new threshold comes from workers who would have otherwise earned between \$733 and \$913 per week.[47] These workers bunched at the new threshold experienced a much larger income effect compared to the average worker directly affected by the 2016 FLSA policy. The median base pay in the hole to the left of \$913 is between \$853 and \$873. A back of the envelope calculation (i.e. $\frac{913-863}{863}$) implies that the median bunched worker earned 5.8% more per week due to the rule change, nearly five times the effect on the average worker estimated in section 6. Taking the ratio of the size of the spike (0.412) to the number of workers directly affected by the 2016 policy (8.45), I find that only 4.9% (s.e. 0.2%) of workers benefit from the bunching, not counting new hires or reclassified workers. Taken together, these results suggest that raising the overtime exemption threshold greatly benefited a small share of workers who received raises to the new threshold.

Under the strong assumption that firms did not adjust the hours of bunched workers, the range of the missing mass suggests that employers were willing and able to raise workers' salaries by up to \$180 to avoid the cost of offering overtime. This translates to a 25% raise for the marginal worker that was bunched above the threshold, and implies that firms initially captured fairly large rents from the employment relationship. While this is a large increase

[46] To ensure that the cumulative effects sum to zero so that there is no change in the total number of always-salaried workers, I assume that the constant in the linear transformation used to construct the control group equals zero (i.e. $\gamma_0 = 0$).

[47] As a placebo check, I do not observe any bunching in the years prior to 2016 in figure A.11. Repeating the same analysis for always-hourly workers, I show in appendix figure A.12 that the 2016 FLSA policy also had negligible effects on always-hourly workers and that the effects do not differ significantly from those of the placebo years.

in earnings, without knowledge of workers' initial hours, it is difficult to ascertain how this compares to the mechanical effect of simply paying these workers an overtime premium.[48] Nevertheless, it does indicate that overtime imposes a large cost on employers.

Reclassification Flows

Next, I repeat a similar analysis to determine where along the distribution of base pay were jobs reclassified, and what happened to the base pay of these jobs. In figures 10a and 10b, I plot the distribution of reclassifications out of and into the salaried distribution, respectively.[49] Visually, there is a clear increase in the number of reclassifications from salaried to hourly status in 2016 compared to previous years, and a decline in reclassifications in the opposite direction. Moreover, individuals who do transition from hourly to salary are more likely to become bunched at the new threshold. Interestingly, the clear increase in reclassifications out of the salaried distribution extends past the proposed $913 threshold.

To estimate the net reclassification effect of the 2016 FLSA policy, I make a minor adjustment to the procedure outlined in section 5.1. Since there are very few reclassifications in the right rail of the base pay distribution, small differences in reclassifications across years leads to large deviations in the parameters used to construct the control group. Given the stability of the distribution of reclassifications over time, I instead assume that $\gamma_1 = 1$ and $\gamma_0 = 0$. To validate my identification assumptions, I estimate the cumulative reclassification effects for 2012-2015 as a placebo test and find very small estimates relative to the change in 2016 (see appendix figure A.14).

Figure 10c overlays the estimates of the net reclassification effects into the salaried and hourly distributions. There are three findings that I would like to highlight. First, jobs across the entire range of affected base pays are reclassified, including those right below the threshold and even those right above it. Second, firms are paying 0.84 (s.e. 0.057) more workers by hour rather than by salary due to the increase in the FLSA overtime exemption threshold. Scaling this estimate by the number of salaried workers initially between the old and new thresholds, I find that for every one hundred workers directly affected by the reform, 10 (s.e. 0.7) workers are reclassified from salaried to hourly. This estimate accounts for nearly the entire rise in hourly jobs described in section 5. Third, the distribution of net reclassifications into hourly jobs has a very similar shape to the negative of the net reclassifications into salaried jobs. For a clearer comparison between these these two distributions, I also plot their difference in

[48]Using the formula to compute overtime pay, an employee would need to receive 7.5 hours of overtime pay to earn a 25% increase in their earnings relative to their base pay.

[49]For the equivalent graphs from the perspective of the hourly distribution, see appendix figure A.13.

figure 10d. Aside from a small bunching effect, the difference is relatively flat across the base pay distribution. This reaffirms the earlier claim that firms did not raise reclassified workers' base pay, but instead paid them a wage roughly equal to their previous salary divided by 40.

7.2 Which Workers Lose?

I now turn to the question of where along the income distribution were jobs displaced. In figures 11a and 11b, I plot the distribution of separations from and new hires into the salaried distribution, respectively, between April and December of each year from 2012 to 2016. Examining the distribution of these employment flows, I find that the negative employment effect is driven primarily by a reduction in hires rather than an increase in separations. This is consistent with the previous observation in section 6 that the increase in the threshold had no effect on the probability that workers remain employed at the same firm. In contrast, there are noticeably fewer new hires between the old and new thresholds in 2016 compared to previous years. Furthermore, workers that do get hired are more likely to be bunched at the new threshold. Consistent with prediction 5 of the job search model, this implies that employers are forward-looking and slowed down their hiring of affected workers before the new overtime exemption threshold went into effect, hence why the 2016 FLSA policy decreased employment even though it never was never binding. This result is similar to recent findings that firms cut employment in response to the minimum wage via a reduction in hires rather than an increase in layoffs (Gopalan et al., 2020).

To understand how the employment effect varies by income, I first compute the difference between the number of salaried hires and separations within each bin of base pay, and then estimate the effect of the 2016 rule change on this distribution using equation 3.[50] These estimates, presented in figure 11c, indicate that the employment loss was spread across the entire interval of weekly base pays affected by the increase in the threshold. However, under the reasonable assumption that, absent the policy, new hires bunched at the new threshold would have earned right below it, most of the employment effect is actually borne by workers earning less than $100 below the new threshold. This is in contrast to the gains from the policy, which mainly benefited workers earning within $180 of the threshold and received a raise above it. Taken together, these results suggest that the 2016 rule change was counter-redistributive: the policy benefited higher paying jobs at the expense of lower paying jobs.

[50] To validate the econometric model, appendix figure A.15 tests how well it fits the distribution of employment flows in the years prior to 2016. Although the placebo effects deviate slightly from zero, these effects are small compared to the effect in 2016 and do not show systematic bias in either direction.

In principle, the policy may have also had an effect on the hiring and separations of hourly workers. However, I show in appendix figure A.16 that the identification strategy fails to satisfy the placebo test when applied to the employment flow of hourly jobs. Instead, I indirectly test for the significance of changes in hourly workers' employment flows by applying the following accounting identify:

$$\Delta n = (\text{Hires - Separations}) + \text{Net Reclassifications}$$

The change in the total number of workers within each pay classification (Δn) estimated in section 5 can be decomposed into the employment and reclassification effects estimated in this section. I report each of these components in table 5, scaled by the number of directly affected salaried workers.[51]

The decomposition provides two suggestive evidence that firms did not change their employment decisions regarding hourly employees in response to the 2016 FLSA policy. First, the 3.8% (s.e. 0.7%) fall in employment flows to salaried jobs explains nearly the entire decline in aggregate employment, leaving little room for responses along the hourly distribution. Second, the decomposition indicates that the reclassification effect explains two-thirds of the decline in salaried jobs, and accounts for nearly the entire rise in hourly jobs. The size of the reclassification effect relative to the increase in hourly jobs likewise suggests that the magnitude of the employment effect for hourly jobs is small. Overall, the evidence suggests that most of the job loss is through the salaried distribution. Furthermore, if one is willing to accept the strong assumption that the entire decline in aggregate employment is driven by the hiring and separation of salaried workers, then the employment flow estimate implies a tight 95% confidence interval on the aggregate employment effect of 2.4 to 5.2 jobs lost per one hundred workers directly affected by the expansion in overtime coverage in 2016. Alternatively, this estimate can be considered a lower bound on the number of jobs lost considering that in section 5.2, I found that changes in the state thresholds had a negative employment effect on hourly jobs.

8 Discussion and Conclusion

This paper presents new facts about the labor market effects of expanding overtime coverage that inform the policy debate surrounding recent initiatives to raise the overtime exemption threshold. In this section, I summarize my findings by comparing the estimates of the effects

[51] The decomposition is not exact since I used a different linear transformation to construct the counterfactual when estimating each component.

of the 2016 FLSA policy to the predictions in the Department of Labor's cost-benefit assessment. To generate these predictions, the DOL conducted a thorough review of the economic literature on overtime and used existing labor demand elasticities to infer from the Current Population Survey the expected effects of their upcoming reform. While the DOL relied heavily on the two predominant models of overtime in their analysis, I find in this paper that neither the labor demand model nor the compensating differentials model of overtime adequately explain the effects of recent expansions in overtime coverage for salaried workers.

My empirical results differ from the conclusions of the Department of Labor in four ways. First, the DOL believed from the classic labor demand model of overtime that by increasing the marginal cost of labor per hour, "employers have an incentive to avoid overtime hours worked by newly overtime-eligible workers, spreading work to other employees (which may increase employment)," though they do not attempt to quantify the magnitude of those effects (U.S. Department of Labor, 2016). In contrast, I estimate that the 2016 FLSA policy actually reduced employment by about four jobs per hundred workers initially between the old and new thresholds. To substantiate this result, I find further evidence of negative employment effects from the event-study of the state policies and from directly estimating the effect of the 2016 policy on the net employment flows into the salaried distribution. Second, while the DOL's assessment accurately predicted that the average affected worker would experience a increase in their weekly earnings, they calculated that average income would rise by only 0.7%, whereas I show that it increased by nearly twice that amount. I also show that this positive income effect was not uniformly distributed across the range of affected base pays, and primarily benefited workers who receive a raise above the threshold. Third, drawing from previous studies of the contract model of overtime, the DOL calculated that 18% of workers would experience a 5.3% decrease in their base pay to partially offset the increase in their overtime pay. However, I find no evidence that firms reduced workers base pays in response to being covered for overtime. Instead, the clearest effect on the distribution of base pay among stayers is the bunching of some workers who would otherwise earn between $733 and $913 per week to above the new threshold. Forth, the DOl considered the reclassification effects of the policy negligible given the available evidence at the time. In contrast, I find that the reclassifications are large: for every one hundred workers directly affect by the 2016 reform, ten are reclassified from salaried to hourly.

Although my paper offers the most comprehensive evaluation of the overtime exemption policy to date, there exist many avenues for future research that are beyond the scope of this study. One particular fruitful endeavor would be to estimate the effect of raising the overtime exemption threshold on workers' hours. To that end, Brown and Hamermesh (2019) finds from the Current Population Survey that jobs that likely lost overtime coverage since the 1980's due to the deterioration of the FLSA threshold experienced a larger increase in weekly hours than jobs whose exemption status did not change. Unfortunately, I cannot estimate the hours response to the recent increases in the overtime exemption threshold using administrative payroll data as firms seldom record the hours of salaried workers. To address this issue, I attempted to estimate the hours effect using self-reported data from the Current Population Survey. However, due to the small sample size, I am unable to even replicate any of the bunching, income, or reclassification effects from the main analysis of the paper (see appendix G). Another area that deserves further attention is the redistribution consequences of the overtime exemption threshold. Similar to the minimum wage literature, it would be worth exploring the relationship between the depreciation in the real value of FLSA overtime exemption threshold over the past 30 years and the growing wage inequality over the same period. Lastly, it would be an interesting avenue of research to estimate workers' value of being paid by salary, and to connect the various effects of raising the overtime exemption threshold within a normative framework to evaluate its welfare impacts.

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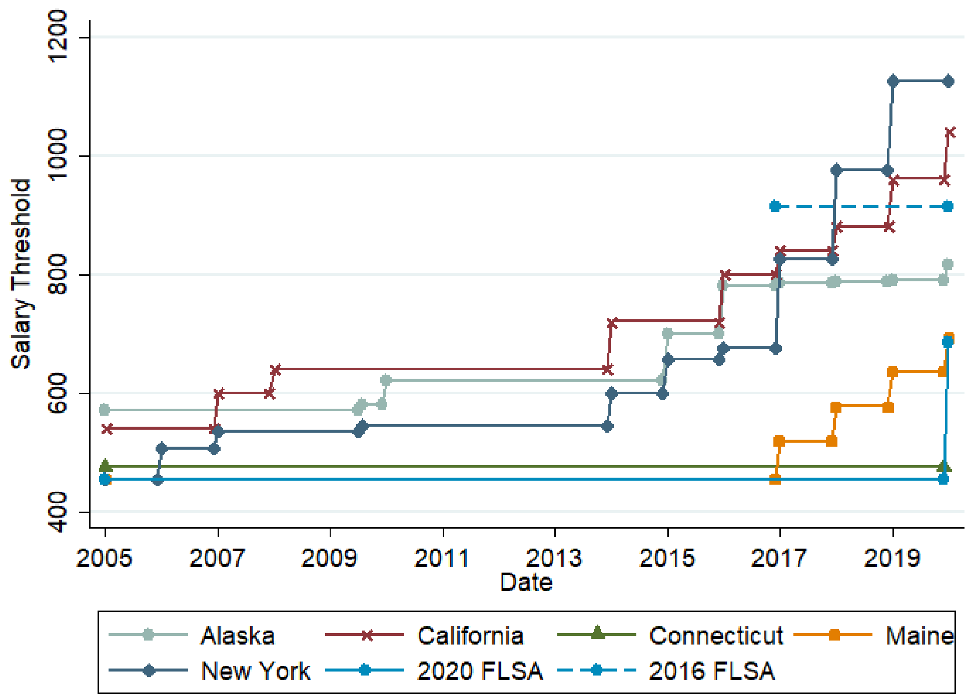
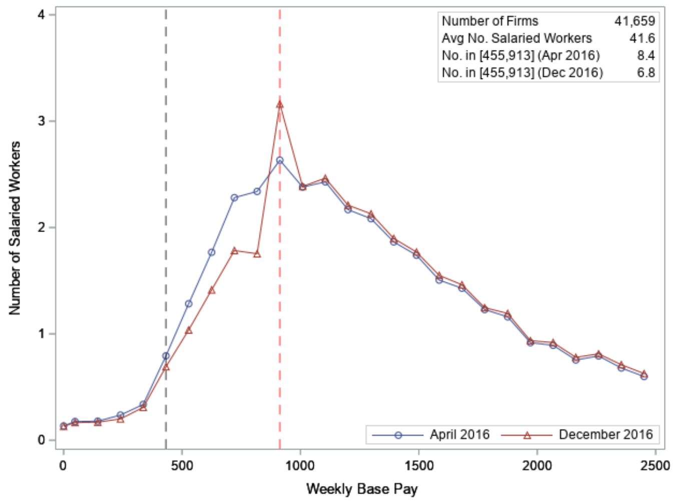
Figure 1: Variation in State-Specific Overtime Exemption Thresholds

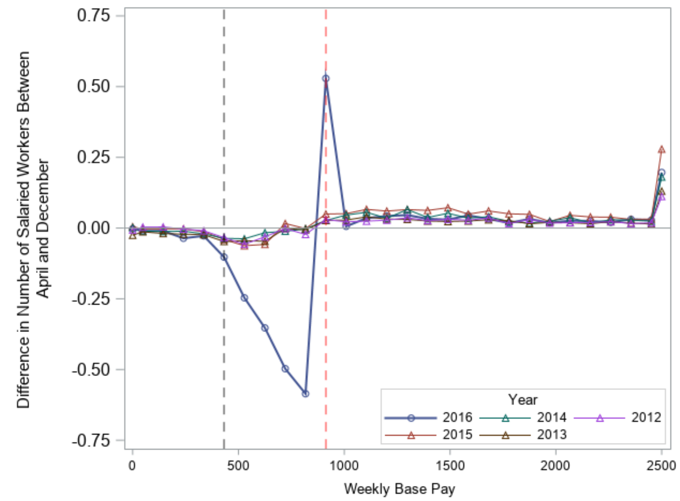


Notes: This figure shows the binding overtime exemption threshold in each state between 2005 and 2020. All states not included in the graph are covered by the Fair Labor Standards Act (FLSA), depicted by the line labeled "Federal". The line "2016 FLSA" represents the federal threshold that was supposed to go into effect on December 1, 2016 but was nullified in November 2016. In Alaska and California, the threshold equals 80 times the state minimum wage. In New York, the threshold equals 75 times the minimum wage. In Maine, the threshold equals 3000/52 times the minimum wage. Starting in January 2017, the minimum wage and threshold varies by firm size in CA, and county and firm size in NY. When the threshold varies within-state, I plot the highest threshold faced by any employer in the state.

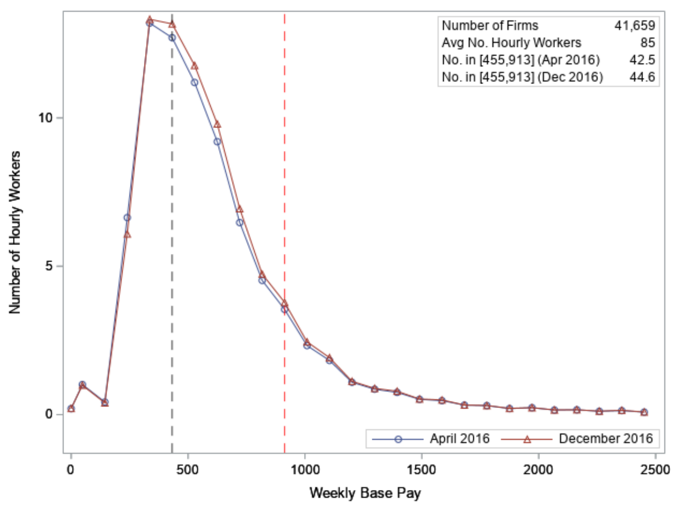
Figure 2: Frequency Distribution of Base Pay, by Pay Classification



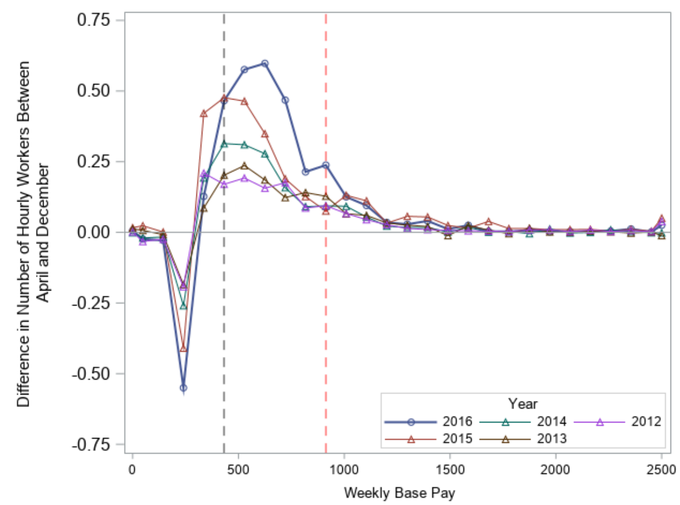
(a) Frequency Distribution of Salaried Jobs



(b) Difference in Distribution of Salaried Jobs



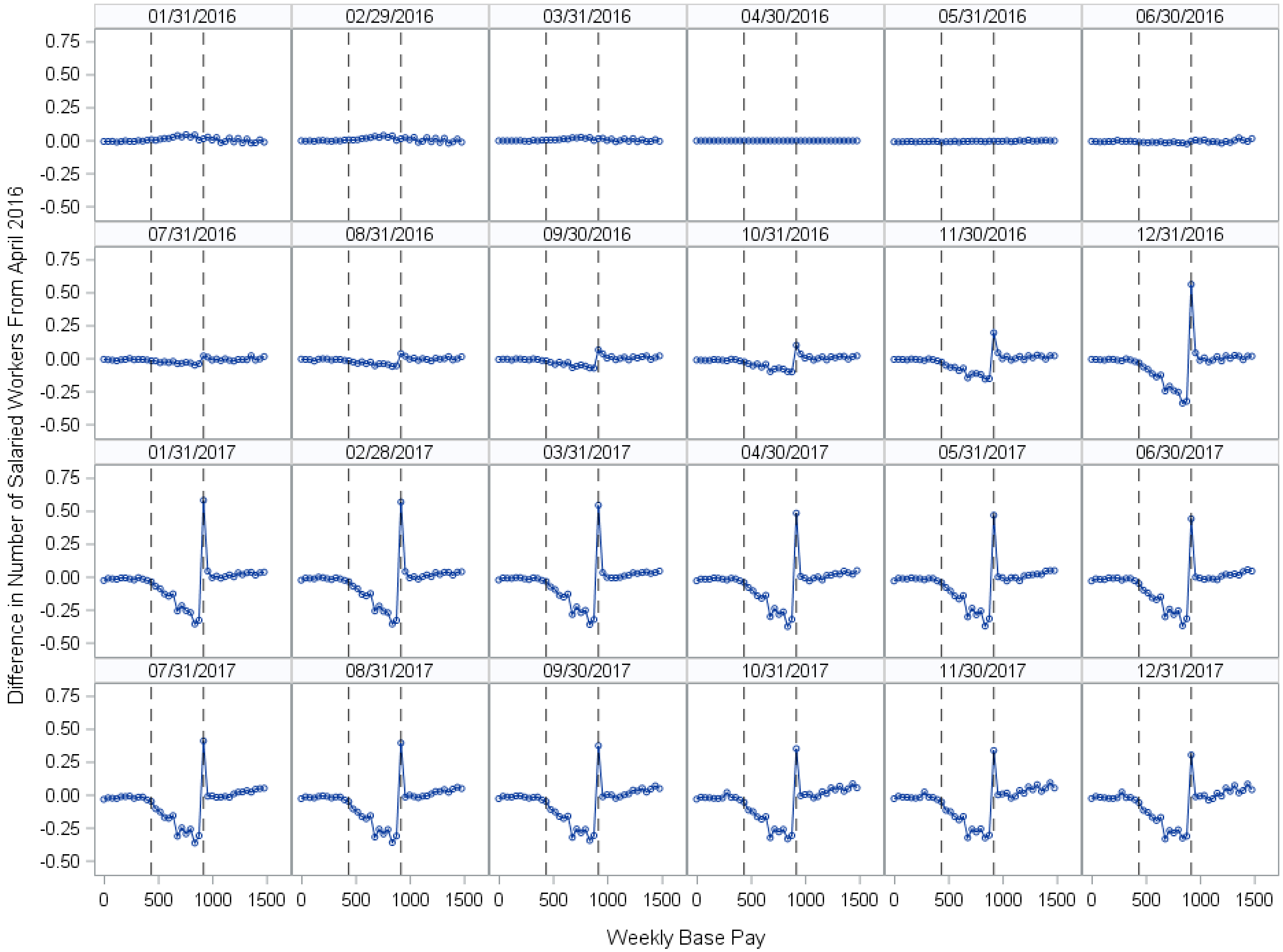
(c) Frequency Distribution of Hourly Jobs



(d) Difference in Distribution of Hourly Jobs

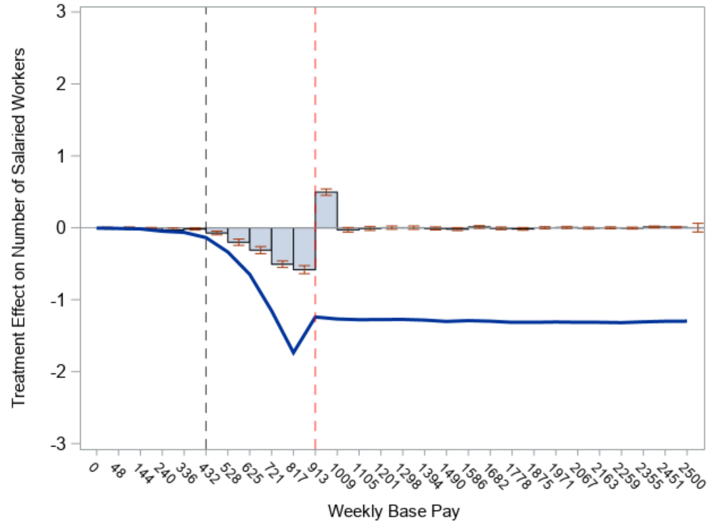
Notes: Panel (a) shows the frequency distribution of weekly base pay (as defined in section 4) of salaried workers in April and December 2016, scaled by the number of firms in the balanced sample. The blue line represents the distribution in April and the red line represents the distribution in December. The vertical black dashed line is at the bin containing the overtime exemption threshold in April (\$455), while the red dashed line is at the bin containing the proposed threshold for December (\$913). The bins have width \$96.15, shifted such that \$913 is the start of a bin. The distribution is truncated at \$2500. Panel (b) shows the difference between the frequency distribution of salaried workers' base pay in December and April, by year. The last bin in panel (b) counts all workers with base pays≥\$2500. Panels (c) and (d) are the hourly worker analog to panels (a) and (b), respectively.

Figure 3: Frequency Distribution of Salaried Workers by Month, Differenced by the Distribution in April 2016

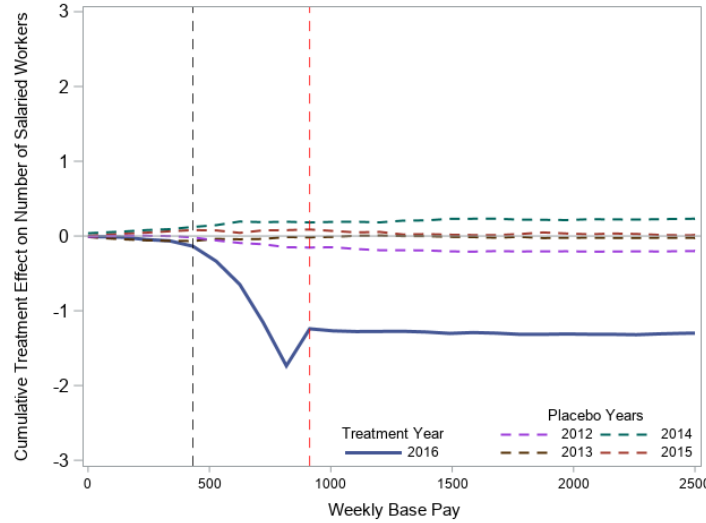


Notes: The figure shows the frequency distribution of weekly base pays in each month of 2016 and 2017, subtracted by the frequency distribution in April 2016. For each month, I scale the distribution by the number of firms that I continuously observe over the 24 months. The bins are $40 wide, shifted so that one bin starts at exactly $913. The left vertical black dashed line is at the bin containing the overtime exemption threshold in April ($455), while the right dashed line is at the bin containing the threshold ($913) that was supposed to go into effect on December 1, 2016.

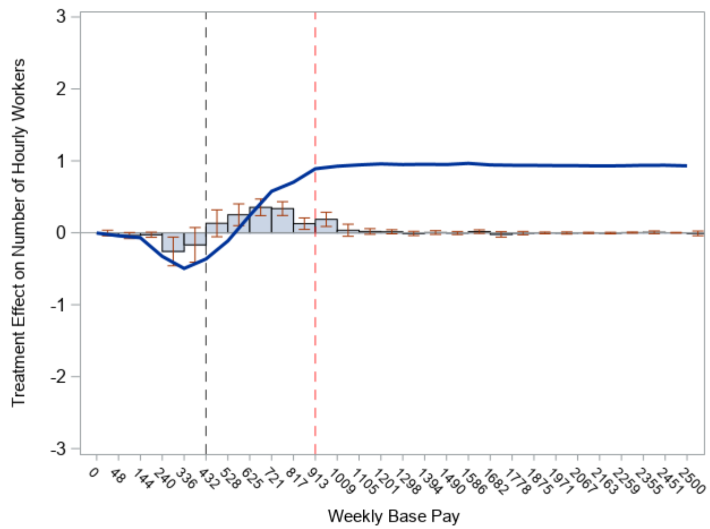
Figure 4: Effect of Raising the 2016 OT Policy on the Frequency Distribution of Base Pay, by Salaried/Hourly Status



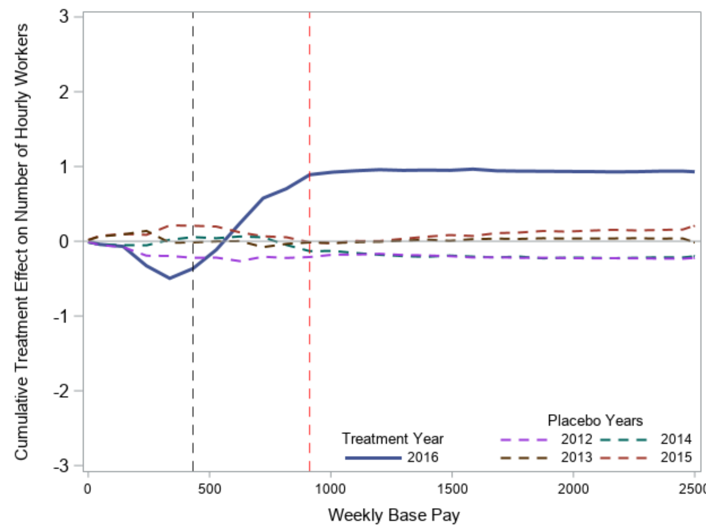
(a) Estimates of Effect on Number of Salaried Jobs



(b) Placebo Test of Effect on Salaried Distribution



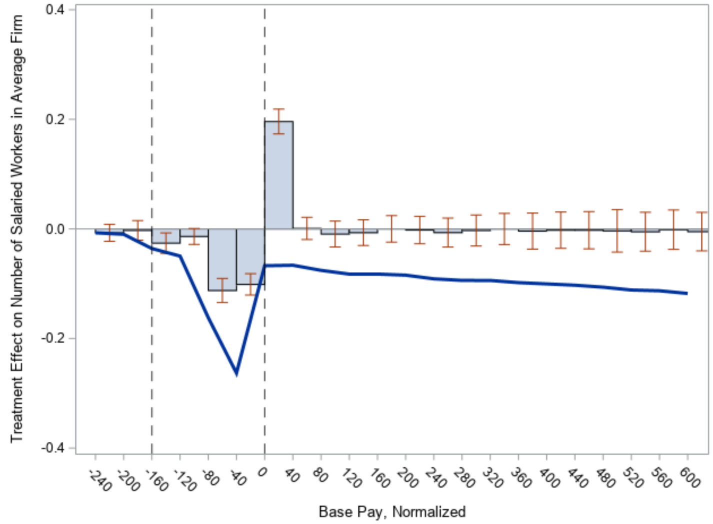
(c) Estimates of Effect on Number of Hourly Jobs



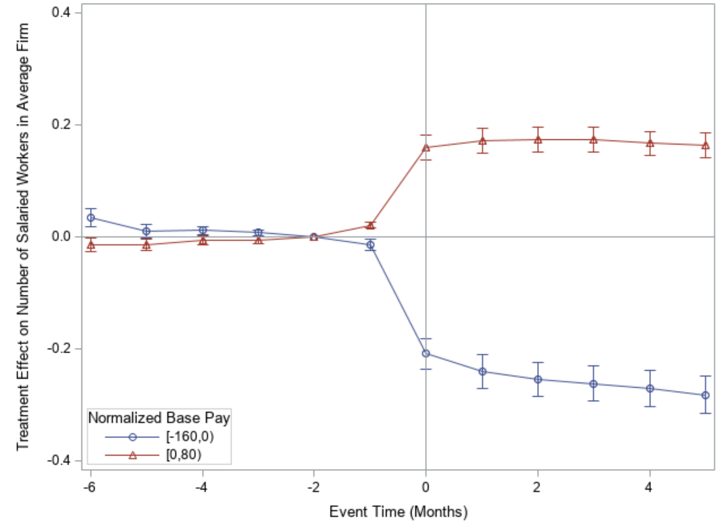
(d) Placebo Test of Effect on Hourly Distribution

Notes: Panel (a) shows the effect of the 2016 FLSA policy on the number of salaried jobs in each \$96.15 bin of base pay in Dec 2016. The treatment effects are estimated using equation 3. The solid blue line is the running sum of these effects. The solid blue line in panel (b) is the same as the solid blue line in panel (a). The dotted graphs in panel (b) are similarly defined running sums, where the effect of the policy is estimated using the December and April distributions of the labeled year and the preceding adjacent year. Panels (c) and (d) are analogous to panels (a) and (b) for the distribution of hourly jobs. In all graphs, the vertical black and red lines are at the bins that contain the old and new OT exemption thresholds (\$455 and \$913), respectively.

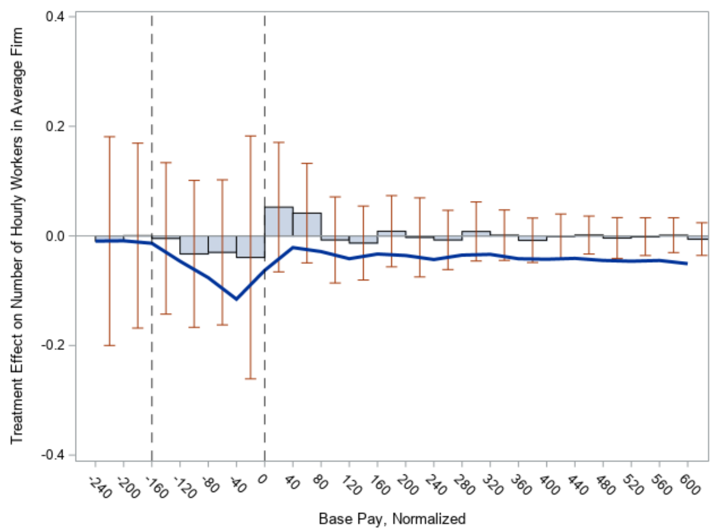
Figure 5: Event Study of Raising State's OT Exemption Threshold on the Frequency Distribution of Base Pay



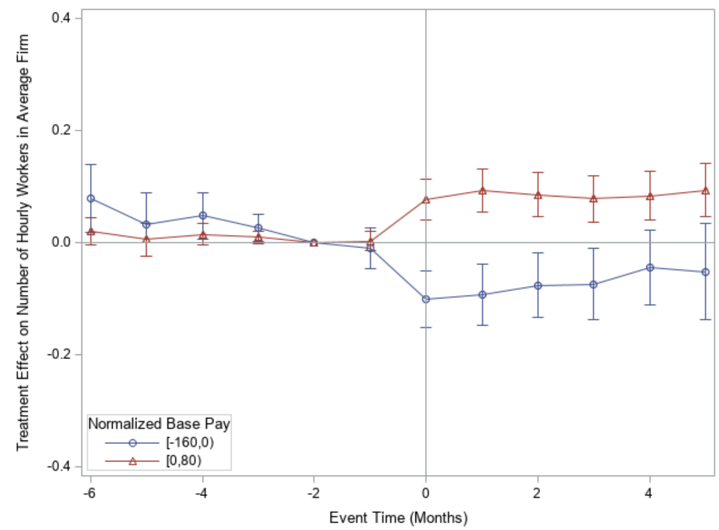
(a) Effect on Number of Salaried Jobs, at Time 0



(b) Effect on Salaried Distribution Over Time



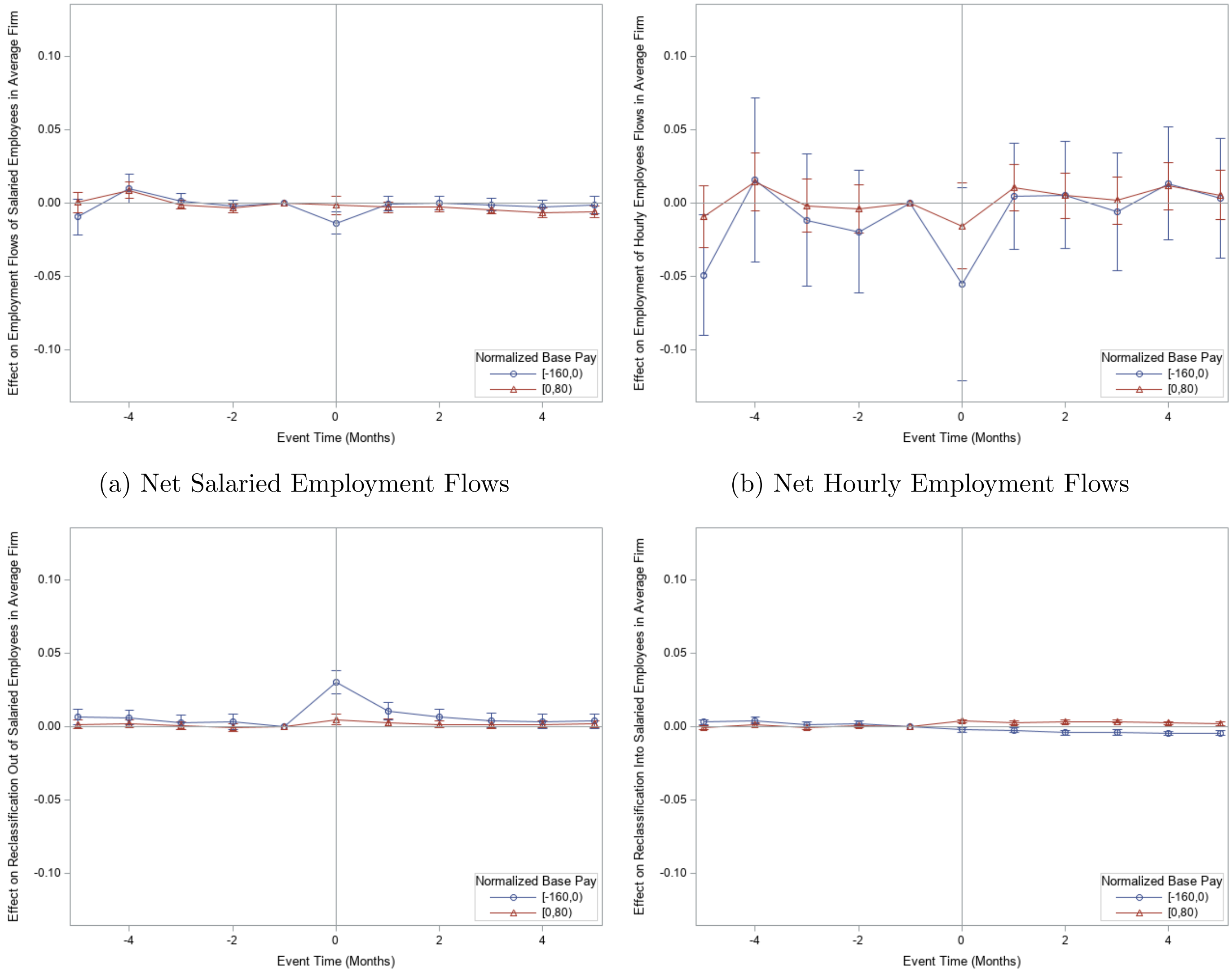
(c) Effect on Number of Hourly Jobs, at Time 0



(d) Effect on Hourly Distribution Over Time

Notes: Panel (a) shows the event study estimates of the effect of raising the OT exemption threshold on the number of salaried jobs in each $40 bin of base pay on the month that the the new threshold goes into effect. The bins are normalized so that the new threshold for each event is 0. The left vertical dashed line is set at the lowest old threshold of all the events. The effects are estimated using equation 6. The solid blue line is the running sum of these effects. Panel (b) shows the employment effects over time, separately for bins between the old and new thresholds (blue line) and bins above the new threshold (red line). Panels (c) and (d) are analogous to panels (a) and (b) for the distribution of hourly jobs. For each estimate, I show the 95% confidence interval using standard errors clustered by firm.

Figure 6: Effect of State Threshold Changes on Flow of Workers Into, Out of, and Within Firms



(a) Net Salaried Employment Flows

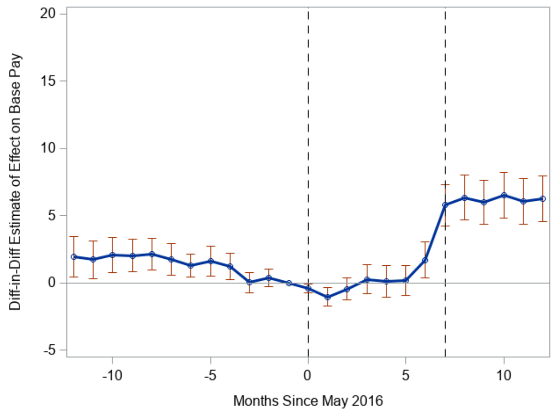
(b) Net Hourly Employment Flows

(c) Reclassification Flows Out of Salaried

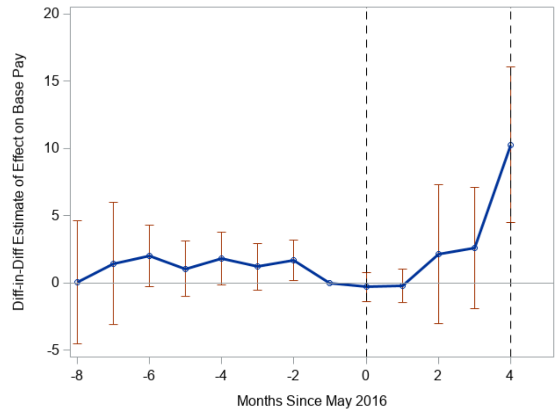
(d) Reclassification Flows Into Salaried

Notes: Panel (a) plots the effect of the state threshold changes on the net employment flow of salaried employees for each month since the month of the reform. Panel (b) plots the analogous figure for net employment flows of hourly employees. Panel (c) plots the effect on the number of salaried workers being reclassified to hourly each month and panel (d) plots the effect on the number of salaried employees that were reclassified from hourly since the preceding month. All estimates are computed using equation 6, and plotted separately for bins between the old and new thresholds (blue line) and bins above the new threshold (red line). For each estimate, I show the 95% confidence interval using standard errors clustered by firm.

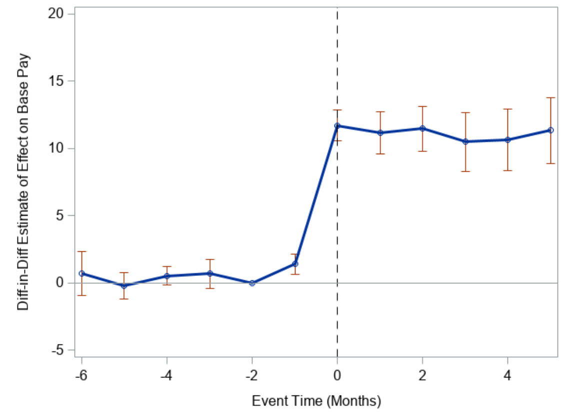
Figure 7: Difference-in-Difference Estimates of the Income Effect of Raising the OT Exemption Threshold



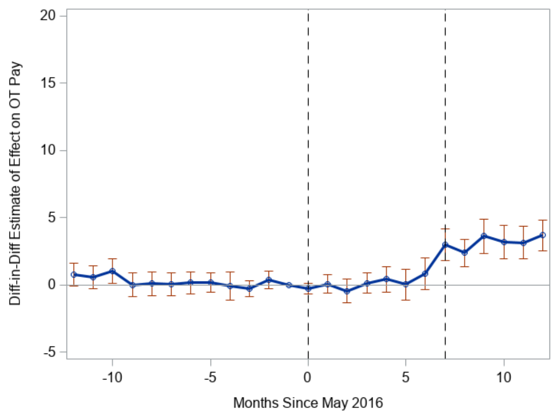
(a) 2016 FLSA: Base Pay



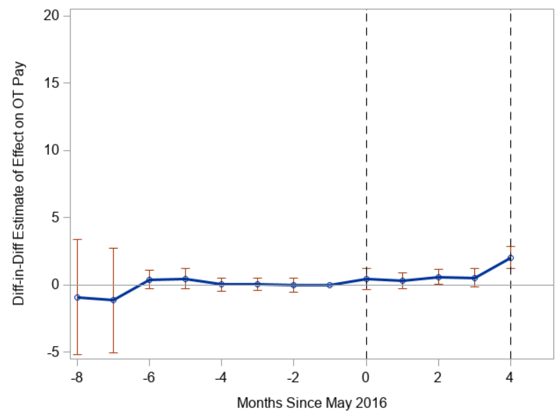
(b) 2020 FLSA: Base Pay



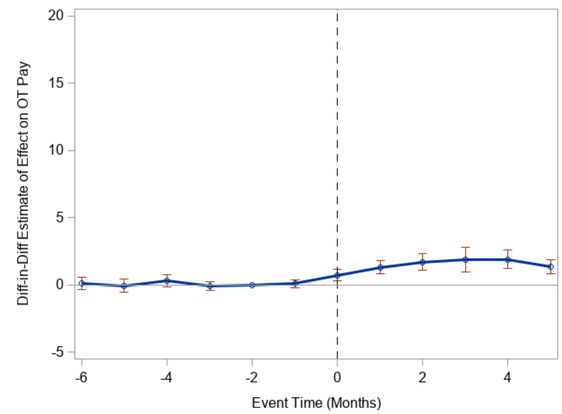
(c) Event Study: Base Pay



(d) 2016 FLSA: OT Pay



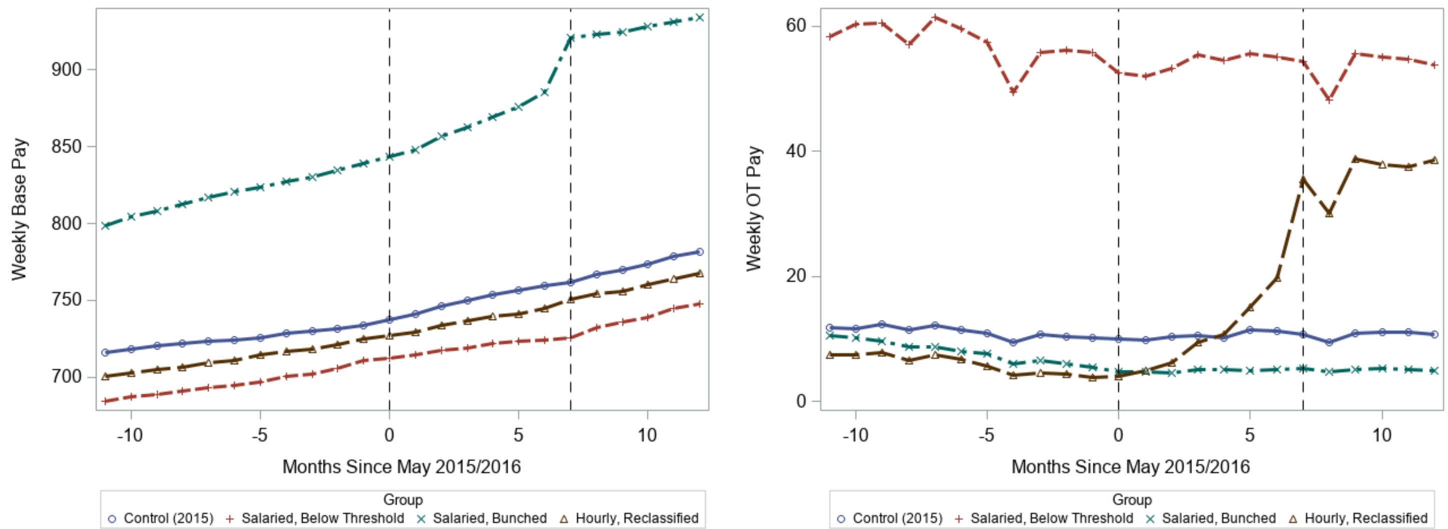
(e) 2020 FLSA: OT Pay



(f) Event Study: OT Pay

Notes: Panels (a)-(c) and (d)-(f) show the effect of raising the overtime exemption threshold on base pay and overtime pay, respectively, for salaried workers initially earning between the old and new thresholds. All estimates are computed from equation 7, where the four panels on the left compares workers in the year of the FLSA rule change to similar workers in the preceding year, and the two right panels compare workers in states that raise their thresholds to similar workers in states that do not. In the four panels on the left, the first dotted vertical line at 0 indicates the month that the rule change is announced, and the second indicates the month that the new threshold actually goes into effect. In the two panels on the right, the vertical line indicates the month that the new threshold goes into effect.

Figure 8: Evolution of Income, by Status of Worker after the 2016 FLSA Rule Change

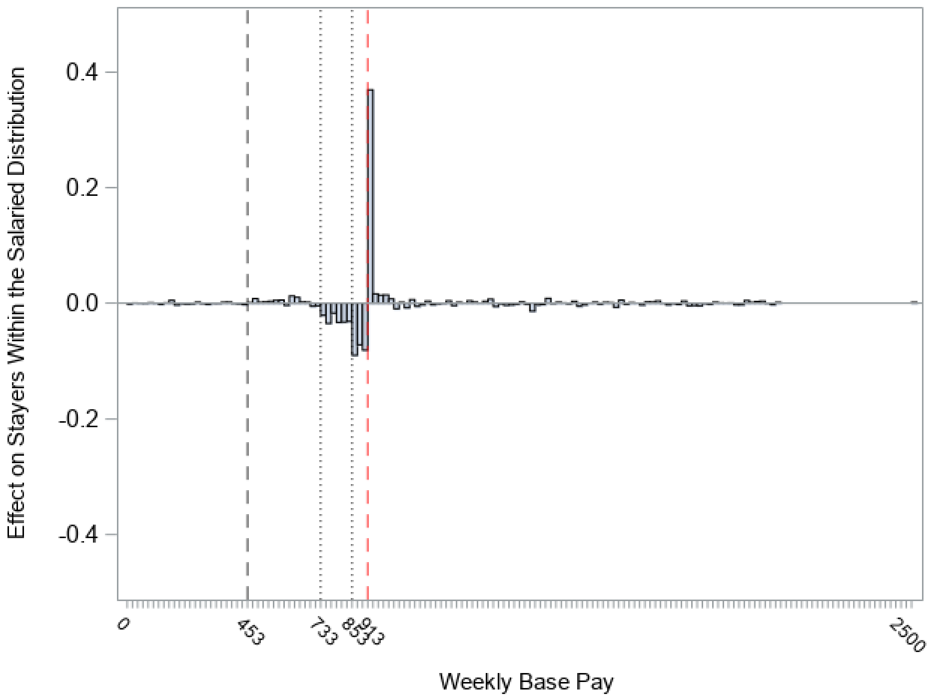


(a) Evolution of Base Pay

(b) Evolution of Overtime Pay

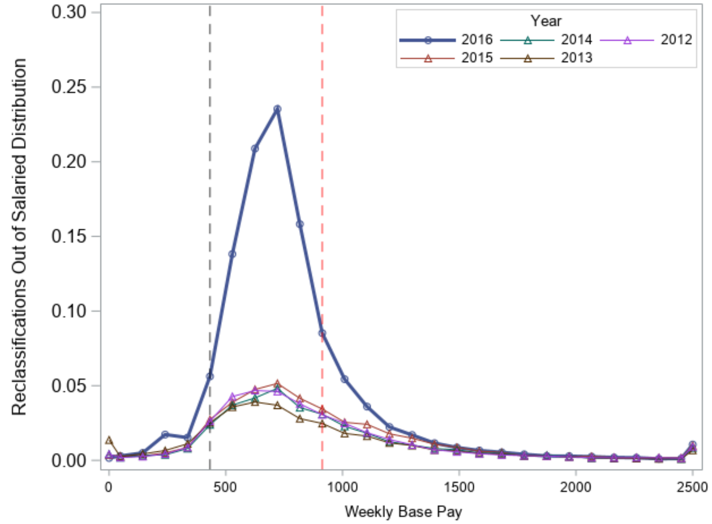
Notes: Figure (a) and (b) compares the base pay and overtime pay, respectively, of four different groups of workers over time. The "Control" group consists of individuals who were paid by salary in April 2015 with a base pay between \$455 and \$913 per week. The remaining groups were similarly defined workers in April 2016, but each satisfies a different condition in December 2016. The "Salaried Below Threshold" group are workers who remain salaried with a base pay less than \$913, the "Salaried Bunched" group remain salaried but with a base pay between \$913-953 after the rule change, and the "Hourly Reclassified" group are workers who were reclassified from salaried to hourly.

Figure 9: Effect of Raising the OT Exemption Threshold on the Frequency Distribution of Always-Salaried Workers, Computed using $20 Bins of Base Pay

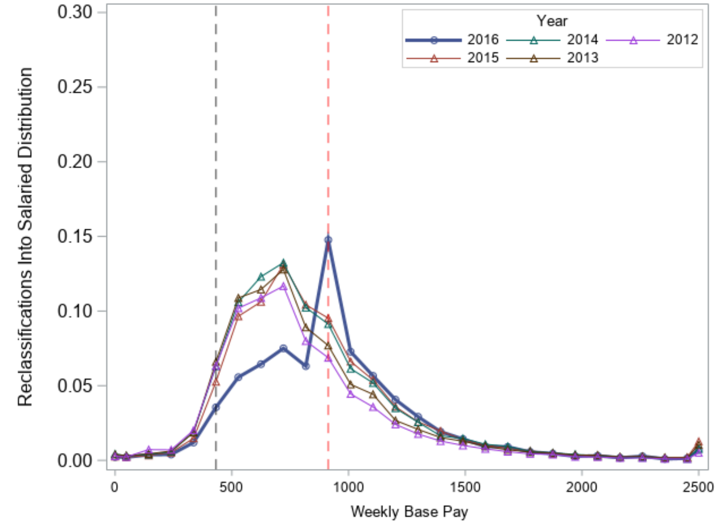


Notes: This figure shows the effect of the 2016 OT policy on the distribution of workers who stay at the same firm between April and December 2016, and are paid by salary in both months, estimated using equation 3 assuming $\gamma_0 = 0$. The vertical dashed black and red lines are at the initial and proposed 2016 FLSA thresholds ($455 and $913), respectively. The dotted line at $853 is located at the median of the base pays in the hole to the left of the new threshold. The dotted line at $733 indicates lowest counterfactual base pay among jobs that got bunched above the new threshold as a result of the policy.

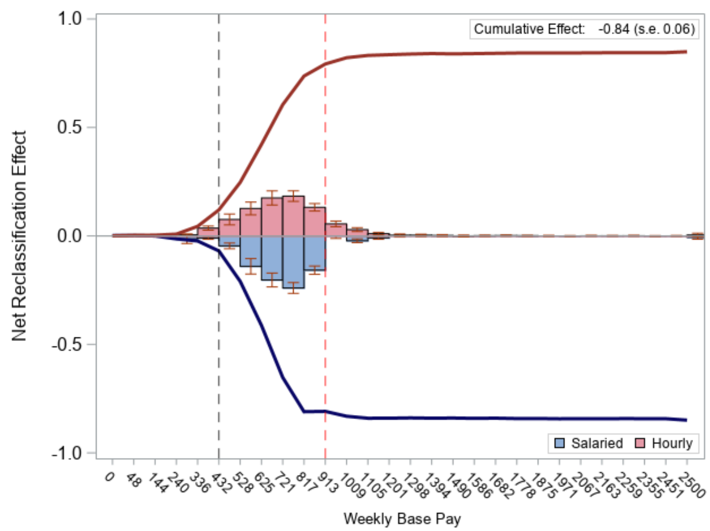
Figure 10: Effect of Raising the 2016 FLSA OT Exemption Threshold on the Distribution of Reclassification Flows



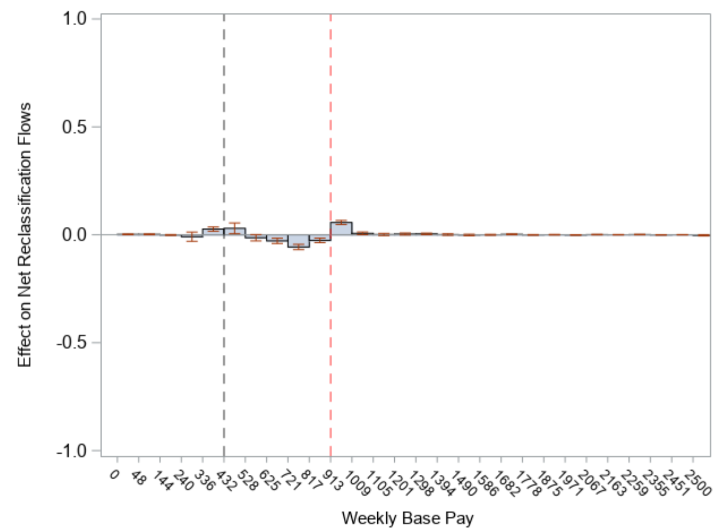
(a) Reclassifications Out of Salaried



(b) Reclassifications into Salaried



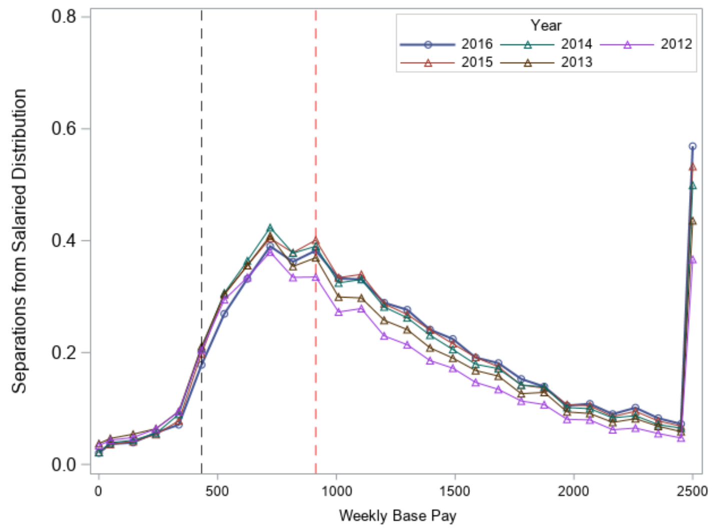
(c) Effect on Net Reclassifications



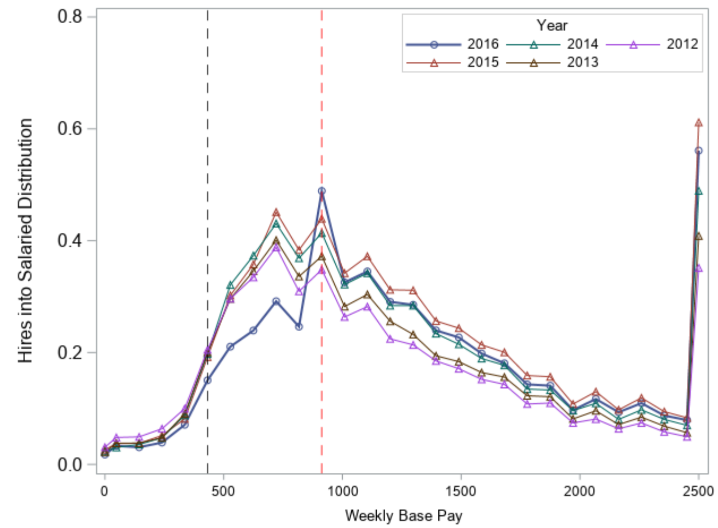
(d) Effect on Base Pay of Reclassified Workers

Notes: Figure (a) shows the average firm's frequency distribution of base pays in April of each year between 2012 and 2016 for workers who are salaried in April, hourly in December, and stayed at the same firm. Figure (b) shows the frequency distribution of base pays in December of each year for workers who are hourly in April, salaried in December, and employed in the same firm in both months. Figure (c) plots the effect of the 2016 FLSA policy on the net number of reclassifications into the salaried distribution (in blue) and the net number of reclassifications into the hourly distribution (in red), estimated by equation 3 assuming $\gamma_1 = 1$ and $\gamma_0 = 0$. The solid lines are the cumulative sum of these bin-specific effects. Panel (d) shows the difference between the blue and red bars in panel (c). The vertical black and red lines are at the initial and proposed 2016 FLSA thresholds, respectively.

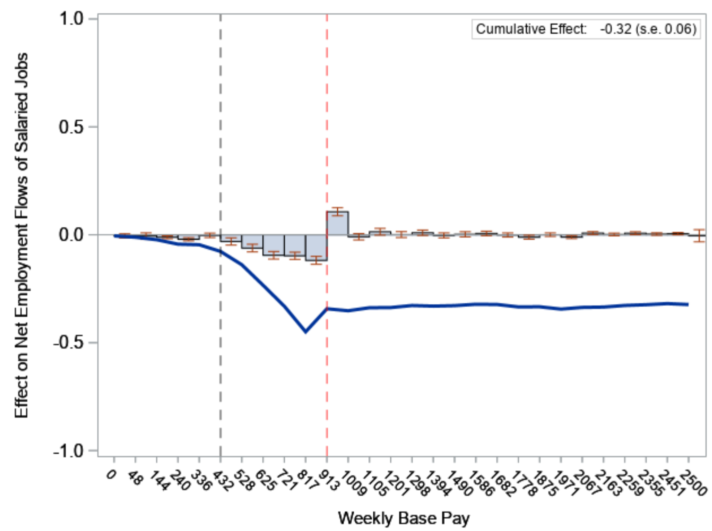
Figure 11: Effect of Raising the 2016 FLSA OT Exemption Threshold on the Distribution of Employment Flows



(a) Distribution of Separations from Salaried Jobs



(b) Distribution of New Hires into Salaried Jobs



(c) Effect on Net Employment Flows

Notes: Panel (a) shows the average firm's frequency distribution of base pays in April of each year between 2012 and 2016 for salaried workers who separate from their employer by December. Panel (b) shows the frequency distribution of base pays in December of each year for salaried workers hired between April and December. Panel (c) shows the effect of raising the 2016 overtime exemption threshold on the difference between the number of hires and separations, estimated from 3. The solid blue line is the cumulative sum of the bin-specific effects. In all figures, the vertical black and red lines are at the initial and proposed 2016 FLSA thresholds, respectively.

Table 1: Summary of Theoretical Predictions

Prediction	Labor Demand	Compensating Differentials	Job Search & Bargaining
Base Pay	-	$\downarrow$	Bunching
Overtime Pay	$\uparrow$	$\uparrow$	$\uparrow$
Employment	$\uparrow$	-	$\downarrow$
Pay structure	-	-	Reclassification from salaried to hourly
Dynamics	-	-	Anticipatory response

Notes: This table summarizes the predictions of the three models of overtime discussed in section 3. The third column of the table refers to the job-search and bargaining model with fixed costs.

Table 2: Employment Effect of Raising the FLSA OT Exemption Threshold

	(1)	(2)	(3)	(4)	(5)	(6)
Jobs Below Threshold	-0.206^{***}	-0.183^{***}	-0.209^{***}	-0.221^{***}	-0.204^{***}	-0.163^{***}
	(0.01)	(0.007)	(0.011)	(0.012)	(0.01)	(0.029)
Bunched	0.052^{***}	0.040^{***}	0.050^{***}	0.028^{**}	0.054^{***}	0.154^{***}
	(0.008)	(0.006)	(0.009)	(0.013)	(0.008)	(0.043)
Hourly Jobs	0.113^{***}	0.078^{***}	0.176^{***}	0.077	0.116^{***}	0.022
	(0.037)	(0.028)	(0.054)	(0.065)	(0.037)	(0.249)
Employment	-0.041	-0.072^{**}	0.018	-0.115	-0.034	0.013
	(0.042)	(0.033)	(0.058)	(0.075)	(0.042)	(0.264)
Treatment Group						
Affected Workers	8.37	13.14	8.81	7.49	8.37	2.09
Avg. Firm Size	125	203	144	109	125	147
Number of Firms	41,500	58,456	41,565	49,413	41,500	36,934
Sample						
States	No MW	FSLA	No MW	No MW	No MW	No MW
Firms Size (%)	99.9	99.9	100	99.9	99.9	99.9
Balanced	Yes	Yes	Yes	No	Yes	Yes
Cutoff	1776	1776	1776	1776	1393	1776
Policy Variation	2016	2016	2016	2016	2016	2020

Notes: Rows (1) and (2) report the effect of raising the OT exemption threshold on the number of salaried workers below and above the new threshold, respectively, scaled by the number of affected workers. Affected workers are defined as salaried employees with base pay between the old and new threshold on the month before the announcement of the rule change. Row (3) reports the effect on the total number of hourly workers for each affected salaried worker. Row (4) reports the sum of rows (1) to (3), and is the effect on aggregate employment for each affected worker.

Columns (1)-(5) report the effects of the 2016 FLSA policy, estimated using equation 3. Column (1) calculates each firm's employment across states with no minimum wage changes, dropping the 0.1% largest firms, for a balanced sample of firms, and using a cutoff of \$1776 to estimate equation 4. Relative to column (1), column (2) calculates firms' employment across all states, column (3) keeps the largest 0.1% of firms, column (4) uses an unbalanced sample of firms where employment in missing firms is set to zero, and column (5) estimates equation 4 using a cutoff of \$1393. Column (6) estimates the same specification as column (1) for the 2020 federal FLSA policy. All robust standard errors in parentheses are clustered by firm. $^{*}p < .1,^{**}p < .05,^{***}p < .01$

Table 3: Employment Effect of Raising States' OT Exemption Threshold

	(1)	(2)	(3)	(4)	(5)	(6)
Jobs Below Threshold	-0.208^{***}	-0.141^{***}	-0.217^{***}	-0.231^{***}	-0.221^{***}	-0.175^{***}
	(0.012)	(0.019)	(0.017)	(0.012)	(0.017)	(0.007)
Bunched	0.162^{***}	0.108^{***}	0.179^{***}	0.178^{***}	0.161^{***}	0.094^{***}
	(0.01)	(0.011)	(0.014)	(0.011)	(0.014)	(0.004)
Hourly Jobs	-0.012	-0.033^{*}	-0.009	-0.003	-0.015	0.038^{*}
	(0.02)	(0.019)	(0.029)	(0.019)	(0.044)	(0.022)
-Below	-0.090^{***}	-0.141^{***}	-0.070^{**}	-0.090^{***}	-0.115^{***}	-0.008
	(0.023)	(0.019)	(0.033)	(0.023)	(0.042)	(0.021)
-Above	0.078^{***}	0.108^{***}	0.062^{**}	0.087^{***}	0.100^{***}	0.046^{***}
	(0.018)	(0.011)	(0.027)	(0.019)	(0.026)	(0.008)
Employment	-0.059^{***}	-0.066^{*}	-0.047	-0.056^{***}	-0.076^{*}	-0.043^{*}
	(0.020)	(0.038)	(0.029)	(0.020)	(0.044)	(0.022)
Treatment Group						
Affected Salaried	1.21	1.18	1.1	1.21	0.77	2.47
Affected Hourly	4.2	4.25	3.82	3.97	3.74	14.25
Avg. Firm Size	110	108	113	109	99	118
No. Firm-Events	182,909	164,106	126,283	150,319	68,735	261,343
Controls						
Bin-State-Event FE	Y	Y	Y	Y	Y	Y
Bin-Month-Event FE	Y	Y	Y	Y	Y	Y
Sample						
Event Time	0	5	0	0	0	0
No. Events	16	15	16	13	6	18
Balanced Firms	No	No	Yes	No	No	No

Notes: Rows (1) and (2) report the effect of raising the OT exemption threshold on the number of salaried workers below and above the new threshold, respectively, scaled by the number of affected workers. Affected workers are defined as salaried employees with base pay between the old and new threshold on the month before the announcement of the rule change. Row (3) reports the effect on the total number of hourly workers for each affected salaried worker. Rows (4) and (5) decomposes the effect on hourly employment to its effect on the number of hourly workers below the new threshold and above it, respectively. Row (6) reports the sum of rows (1) to (3), and is the effect on aggregate employment for each affected worker.

Columns (1) and (2) reports the average employment effect of increasing a state's OT exemption threshold at 0 and 5 months after the date of the rule change, respectively, estimated using equation 6. Column (3) restricts the sample within each event to only firms that employ workers in both the treatment and control states. Column (4) drops the three threshold increases that occurred on January 1, 2017. Column (5) restricts the sample to only threshold increases that went into effect prior to 2016. Column (6) pools the All robust standard errors in parentheses are clustered by firm. $^{*}p < .1,^{**}p < .05,^{***}p < .01$

Table 4: Income Effect of Raising the OT Exemption Threshold

	FLSA 2016	FLSA 2020	Event-Study		Pooled		Placebo
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Base Pay ($)	5.789***	10.273***	11.718***	11.769***	9.61***	10.185***	1.506***
	(0.774)	(2.901)	(0.572)	(0.943)	(0.519)	(0.687)	(0.434)
OT Pay ($)	2.968***	2.037***	0.713***	0.369	1.589***	2.052***	0.334*
	(0.59)	(0.41)	(0.215)	(0.319)	(0.238)	(0.332)	(0.197)
Total Pay ($)	8.759***	12.31***	12.431***	12.141***	11.199***	12.24***	1.841***
	(1.01)	(2.924)	(0.621)	(0.987)	(0.583)	(0.773)	(0.467)
%Δ Total Pay	0.012***	0.021***	0.014***	0.017***	0.014***	0.016***	0.002***
	(0.001)	(0.005)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Log Total Pay	0.012***	0.017***	0.013***	0.015***	0.013***	0.014***	0.001**
	(0.001)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)	(0.0004)
Log Gross Pay	0.005	0.012***	0.009***	0.005	0.008***	0.010***	−0.002
	(0.004)	(0.004)	(0.002)	(0.003)	(0.002)	(0.003)	(0.002)
State FE	Y	Y	-	-	-	-	-
Time FE	Y	Y	-	-	-	-	-
Event-State FE	-	-	Y	Y	Y	Y	Y
Event-Time FE	-	-	Y	Y	Y	Y	Y
Balanced Firms	-	-	-	-	-	Y	-
Initial Income	734.19	586.77	864.71	703.22	771.54	746.81	909.37
N (treatment)	159,406	51,359	166,882	38,228	377,646	274,267	122,480
N (control)	192,910	56,885	1,838,323	591,064	2,088,118	562,373	1,071,977
Events	1	1	16	6	18	18	18

Notes: Rows (1) and (2) report the effect of raising the OT exemption threshold on continuously employed workers' base pay and overtime pay, respectively. Row (3) equals the sum of rows (1) and (2). Row (4) scales row (3) by the average initial income of the treatment group. Row (5) and (6) report the estimate of the policy's effect on log total pay and log gross pay, respectively, as defined in section 6.

Columns (1) reports the income effect of the 2016 FLSA policy estimated from equation 7, column (2) reports the estimates for the 2020 FLSA policy, and columns (3)-(4) report the estimates of the event-study. Column (4) restricts the sample to only threshold increases that went into effect prior to 2016. Column (5) is estimated from a difference-in-difference that pools the two federal policies and the 16 state policies together. Column (6) restricts the pooled regression to firms that employ workers in both the treatment and control groups. Column (7) reports the estimates of the pooled regression for workers initially earning above the new exemption threshold. All estimates are reported for the month that the new threshold goes into effect. The treatment sample consists of workers who were paid by salary, and earning between the old and new threshold prior to the rule change. All robust standard errors in parentheses are clustered by firm. $^{*}p < .1,^{**}p < .05,^{***}p < .01$

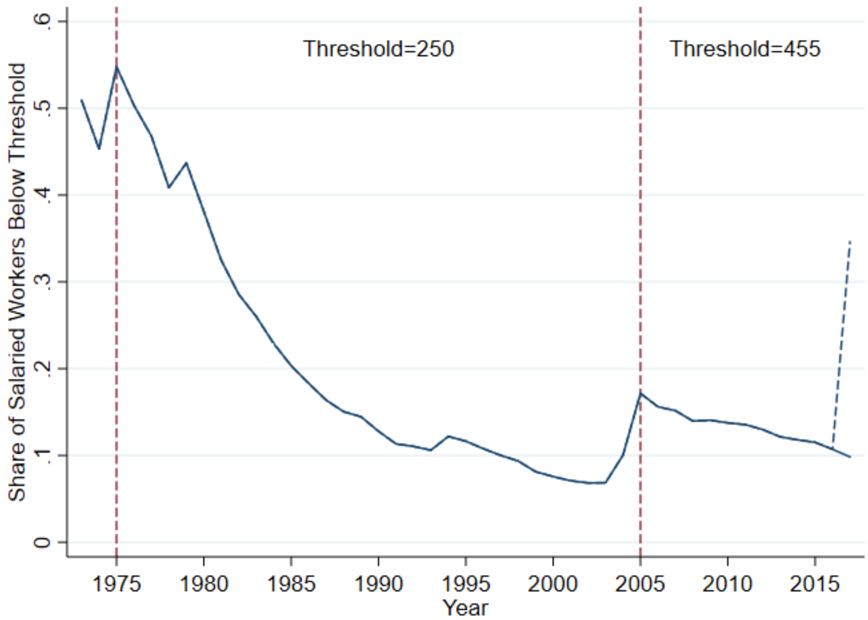
Table 5: Decomposition of the Effect of the 2016 FLSA Policy on the Number of Salaried and Hourly Workers

	Δn	Employment	Reclassification
Salaried	-0.154	-0.038	-0.099
	(0.013)	(0.007)	(0.007)
Hourly	0.112		0.099
	(0.037)		(0.007)
	-0.042		0.000
	(0.042)		(0.000)

Notes: Column (1) reports the effect of the 2016 FLSA policy on the number of salaried employees, the number of hourly employees, and the total number of employees in the average firm estimated in section 5.1. These numbers correspond to the estimates in the first column of table 2, where the total change in salaried employment is the sum of the loss in jobs below the threshold and the bunching above it. Column (2) shows the effect of the policy on the number of hires minus separations, estimated in section 7.2. Column (3) shows the effect of the policy on the number of reclassifications, estimated in section 7.1. All estimates are reported in terms of "per directly affected workers in April 2016".

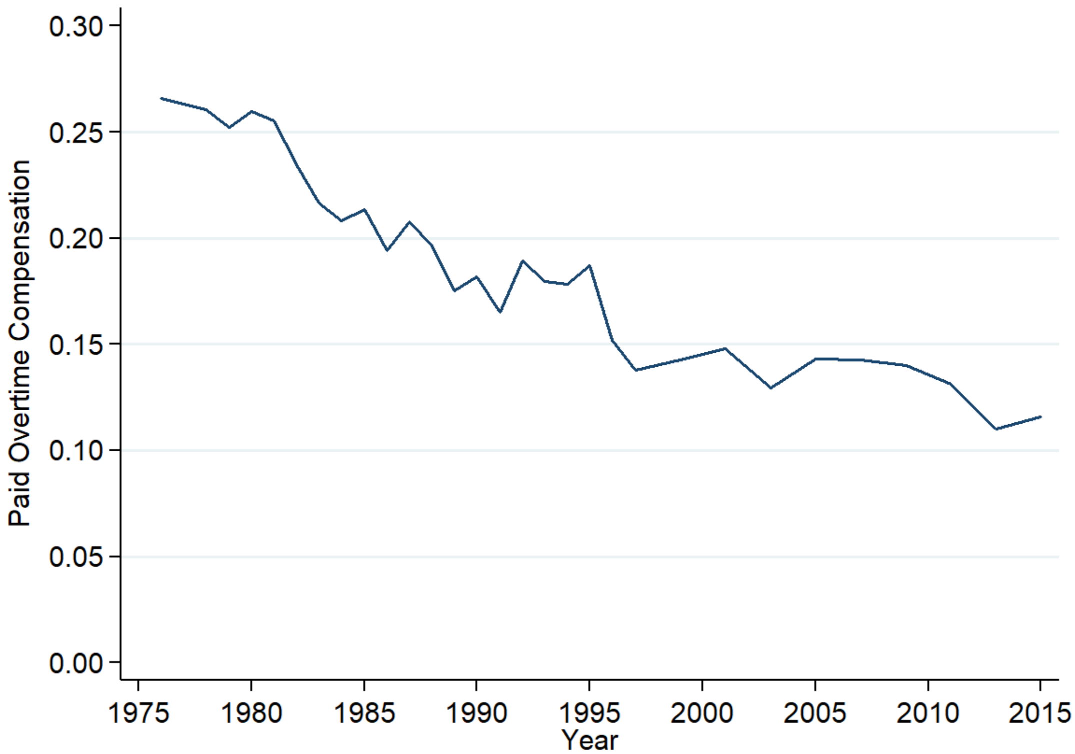
Appendix A. Additional figures and tables

Appendix Figure A.1: Percent of Salaried Workers Below the FLSA OT Exemption Threshold



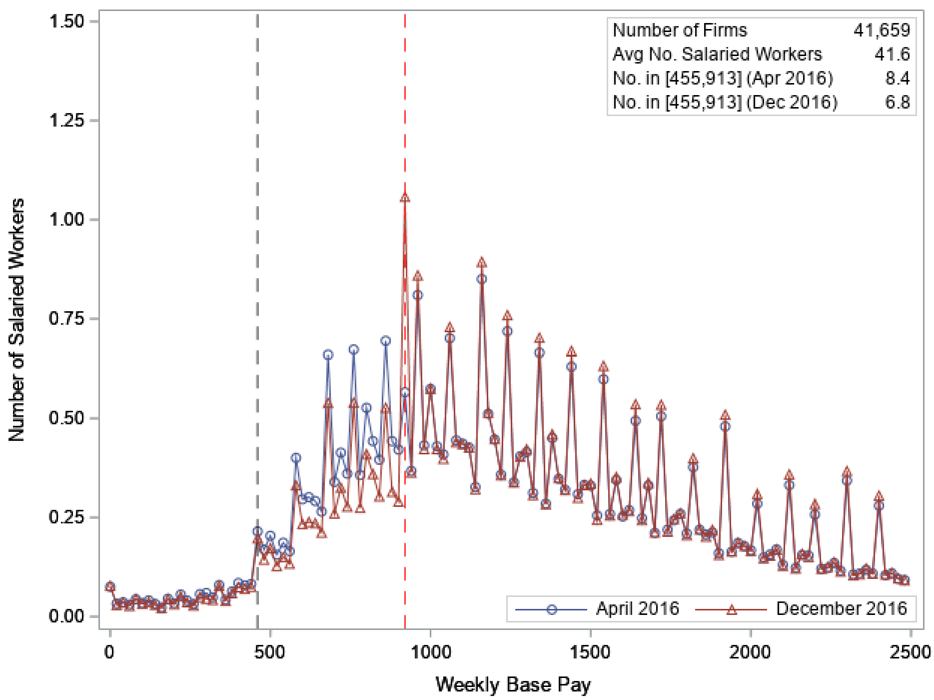
Notes: The figure shows the share of all salaried workers in the May extracts of the CPS who report usual weekly earnings below the effective FLSA overtime exemption threshold from 1973 to 2017. The threshold increased from \$200 per week to \$250 per week in January 1975, and then to \$455 in August 2004. The dotted blue line shows the percent of salaried workers with usual weekly earnings below the \$913 per week threshold announced in the 2016 policy.

Appendix Figure A.2: Percent of Salaried Workers Eligible for Overtime



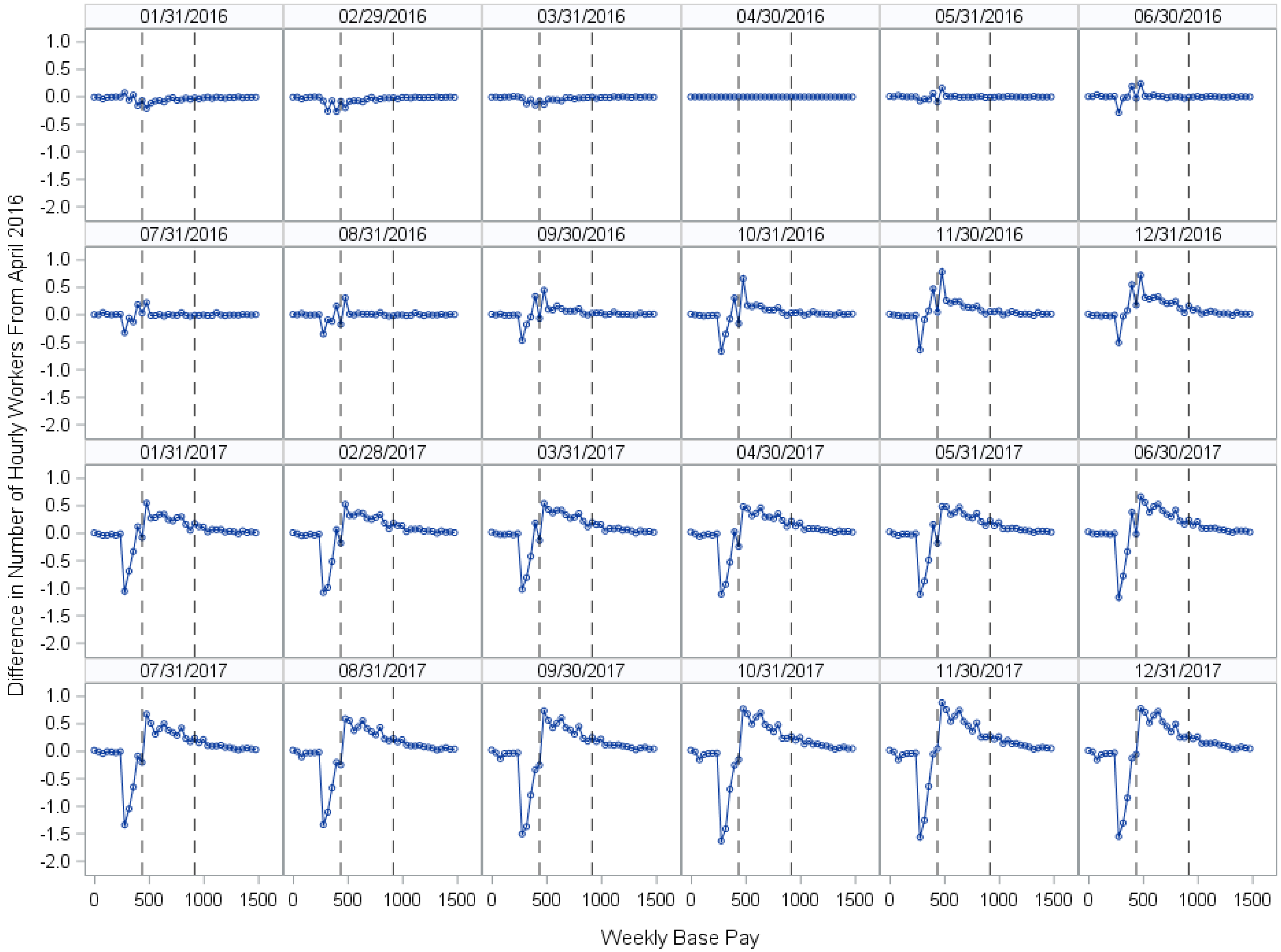
Notes: This figure shows the percent of salaried workers in the PSID who respond yes to the question ”If you were to work more hours than usual during some week, would you get paid for those extra hours of work”.

Appendix Figure A.3: Frequency Distribution of Salaried Workers' Base Pay using $20 Bins



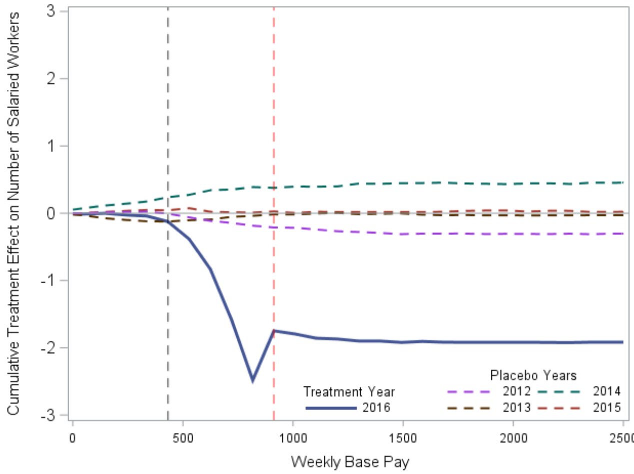
Notes: This figure shows the number of salaried workers across the base pay distribution in April and December 2016. It is analogous to figure 2a but aggregates employment across $20 increments of base pay.

Appendix Figure A.4: Frequency Distribution of Hourly Workers by Month, Differenced by the Distribution in April 2016

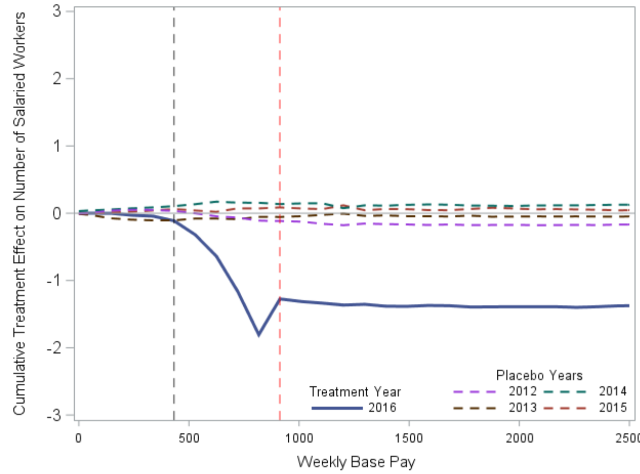


Notes: The figure shows the frequency distribution of weekly base pays of hourly workers in each month of 2016 and 2017, subtracted by the frequency distribution in April 2016. For each month, I scale the distribution by the number of firms in the superset of all firms that appear in the data that month and in April 2016. Within each graph, the bins are \$20 wide except for the first bin which goes from \$0 to \$12.99. The vertical black dashed line is at the bin containing the overtime exemption threshold in April (\$455), while the red dashed line is at the bin containing the threshold (\$913) that was supposed to go into effect on December 1, 2016.

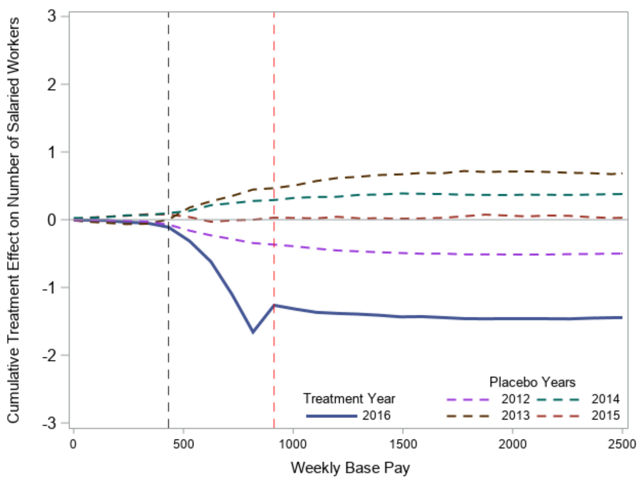
Appendix Figure A.5: Robustness of the Estimated Effects on the Frequency Distribution of Salaried Workers



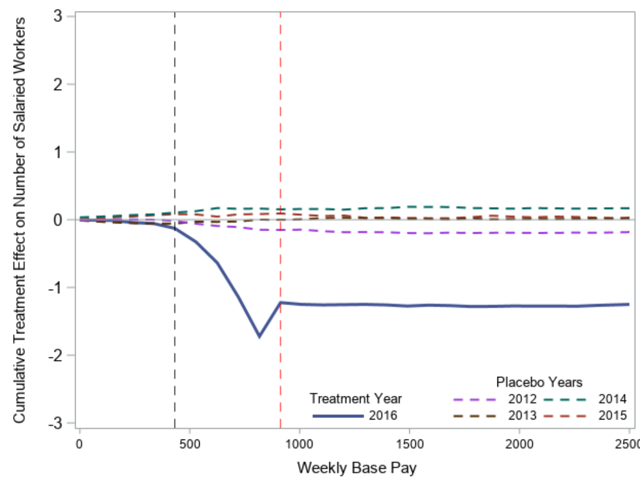
(a) Include all FLSA States



(b) Include Largest 0.1% of Firms



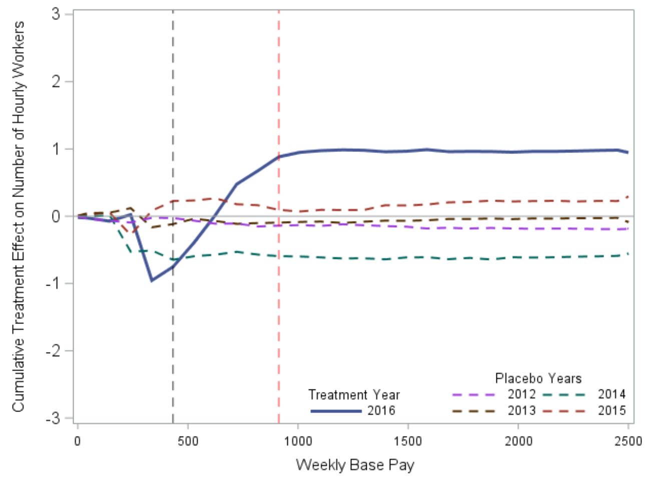
(c) Include Firm Entry and Exit



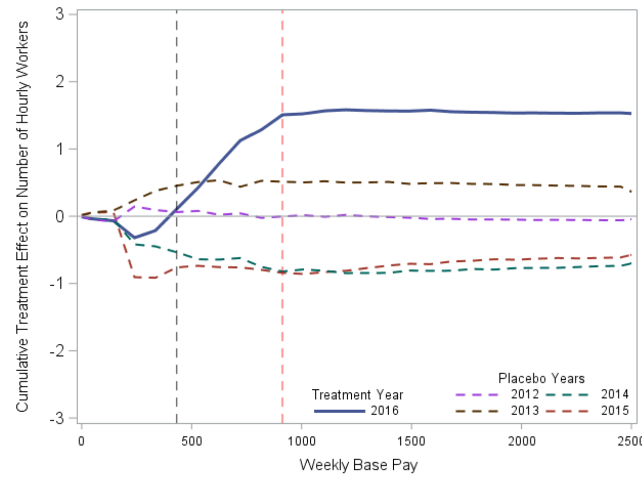
(d) Use Smaller Cutoff

Notes: Each figure presents the cumulative treatment effect of raising the OT threshold in 2016 on the number of salaried workers and the cumulative placebo effect for years prior to 2016, estimated from a different sample or specification of equation 3. Figure (a) estimates the bin-by-bin treatment effects for employment over all states covered by the FLSA overtime exemption threshold. Figure (b) includes in the sample the largest 0.1% of firms. Figure (c) uses an unbalanced panel whereby if a firm is missing in one month, its employment is coded as 0 in every bin. Figure (d) uses bins greater than or equal to \$1393 to estimate the γ_1 and γ_0 in equation 3.

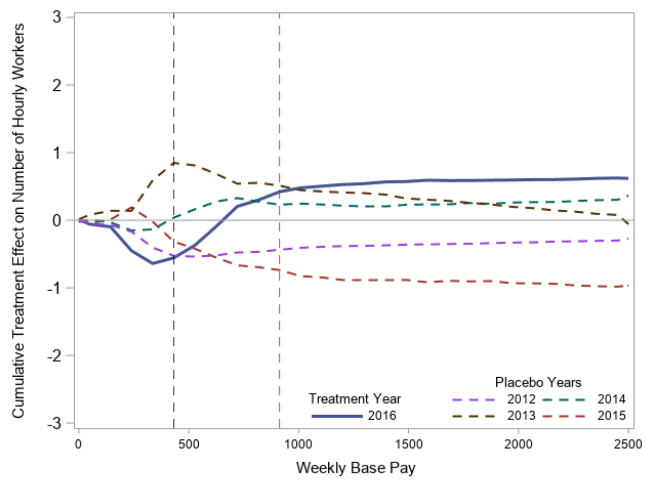
Appendix Figure A.6: Placebo Tests of the Cumulative Effects on the Frequency Distribution of Hourly Workers



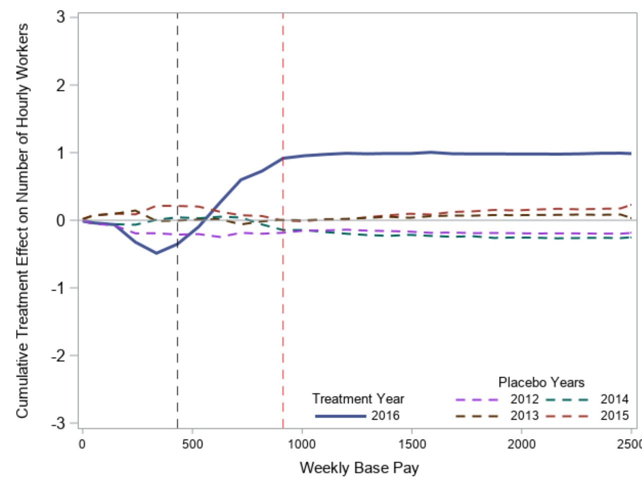
(a) Include all FLSA States



(b) Include Largest 0.1% of Firms



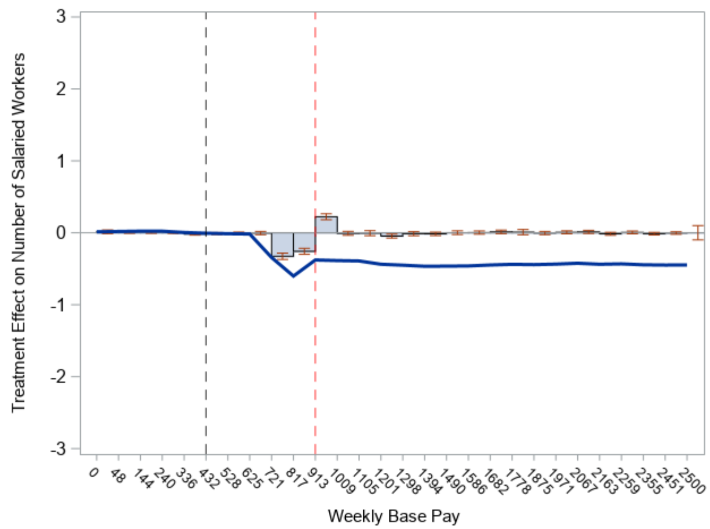
(c) Include Firm Entry and Exit



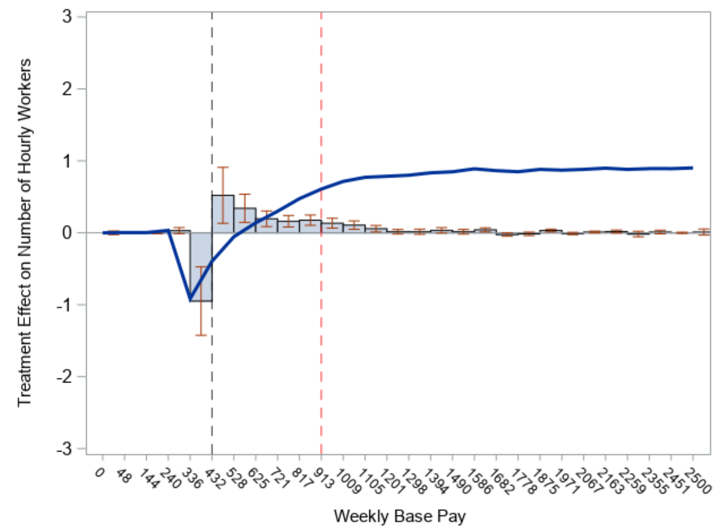
(d) Use Smaller Cutoff

Notes: Each figure presents the cumulative treatment effect of raising the OT threshold in 2016 on the number of hourly workers and the cumulative placebo effect for years prior to 2016, estimated from a different sample or specification of equation 3. Figure (a) estimates the bin-by-bin treatment effects for employment over all states covered by the FLSA overtime exemption threshold. Figure (b) includes in the sample the largest 0.1% of firms. Figure (c) uses an unbalanced panel whereby if a firm is missing in one month, its employment is coded as 0 in every bin. Figure (d) uses bins greater than or equal to \$1393 to estimate the γ_1 and γ_0 in equation 3.

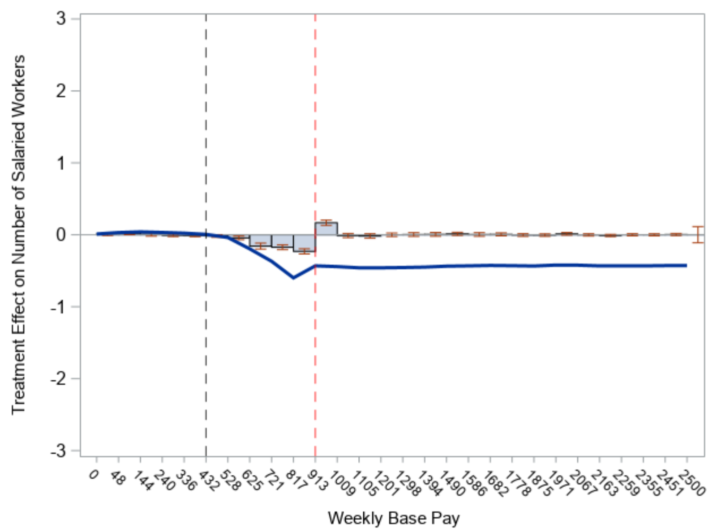
Appendix Figure A.7: Effect of Raising the 2016 OT Policy on the Frequency Distribution of Base Pay, by Salaried/Hourly Status



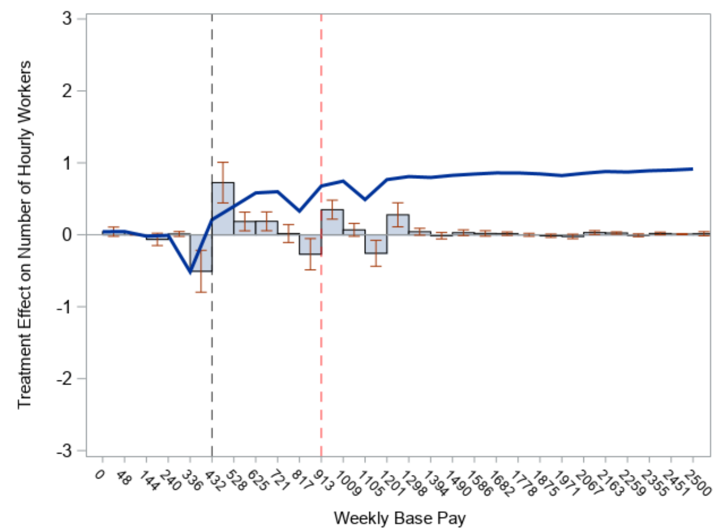
(a) Effect on Number of Salaried Jobs in CA



(b) Effect on Number of Hourly Jobs in CA



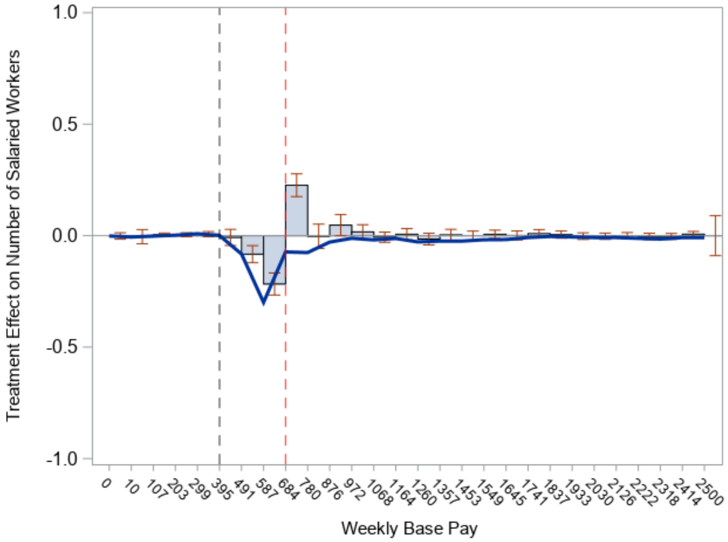
(c) Effect on Number of Salaried Jobs in NY



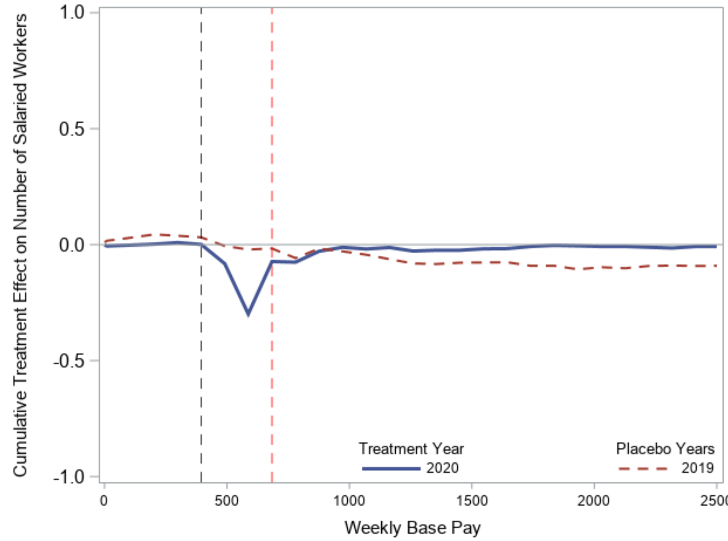
(d) Effect on Number of Hourly Jobs in NY

Notes: Panel (a) shows the effect of the 2016 FLSA policy on the number of salaried jobs in California, within each \$96.15 bin of base pay in Dec 2016, estimated using equation 3. The solid blue line is the running sum of these effects. Panel (b) depicts the same estimates for the number of hourly workers. Panels (c) and (d) are analogous to panels (a) and (b) for firms in New York. In all graphs, the vertical black and red lines are at the bins that contain the old and new FLSA OT exemption thresholds (\$455 and \$913), respectively.

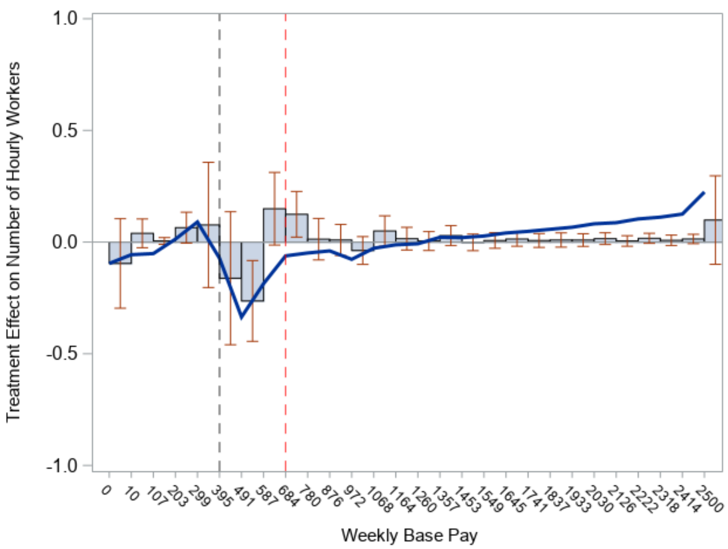
Appendix Figure A.8: Effect of Raising the 2020 OT Policy on the Frequency Distribution of Base Pay



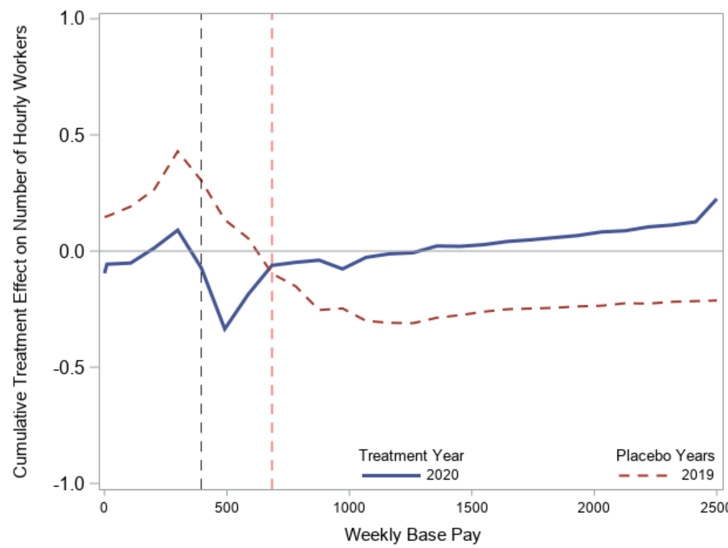
(a) Estimates of Effect on Number of Salaried Jobs



(b) Placebo Test of Effect on Salaried Distribution



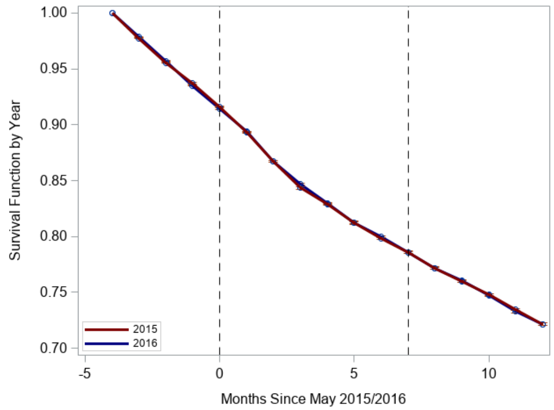
(c) Estimates of Effect on Number of Hourly Jobs



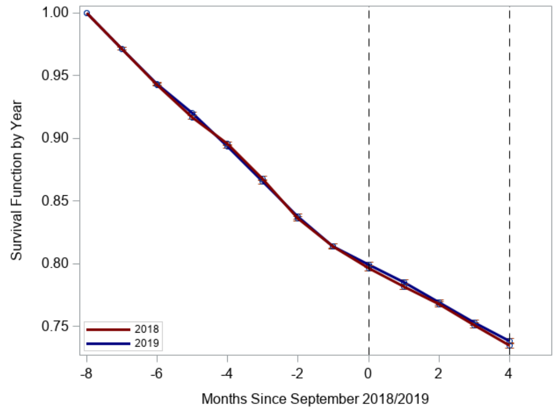
(d) Placebo Test of Effect on Hourly Distribution

Notes: Panel (a) shows the effect of the FLSA policy on the number of salaried jobs in each \$96.15 bin in Jan 2020. The treatment effects are estimated using equation 3. The solid blue line is the running sum of these effects. The solid blue line in panel (b) is the same as the solid blue line in panel (a). The dotted graphs in panel (b) are similarly defined running sums, where the effect of the policy is estimated using the December and April distributions of the labeled year and the preceding adjacent year. Panels (c) and (d) are analogous to panels (a) and (b) for the distribution of hourly jobs. In all graphs, the vertical black and red lines are at the bins that contain the old and new OT exemption thresholds (\$455 and \$684), respectively.

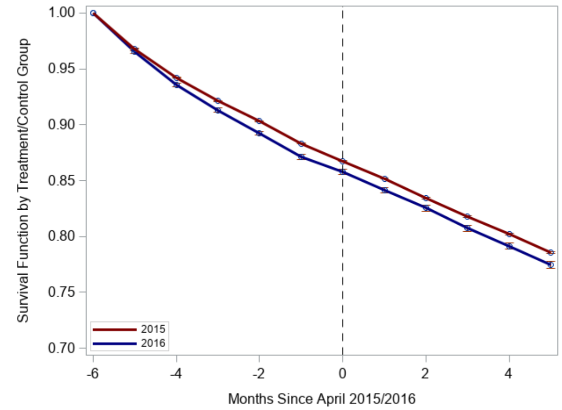
Appendix Figure A.9: Effect of Raising the Overtime Exemption Threshold on the Probability of Remaining in the Same Firm



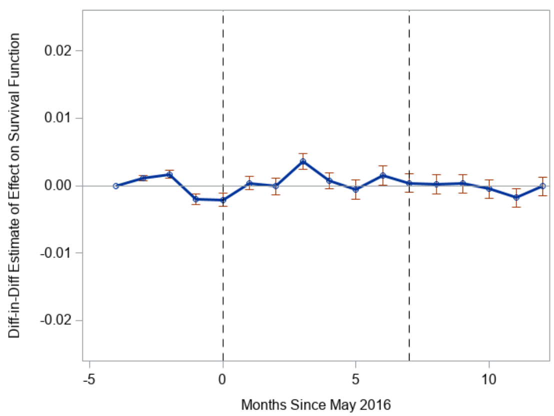
(a) FLSA 2016: Survival Function



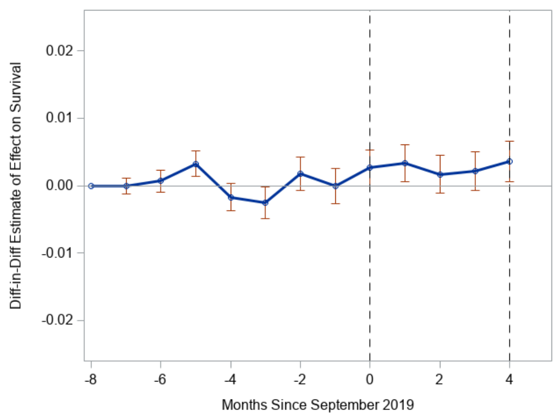
(b) FLSA 2020: Survival Function



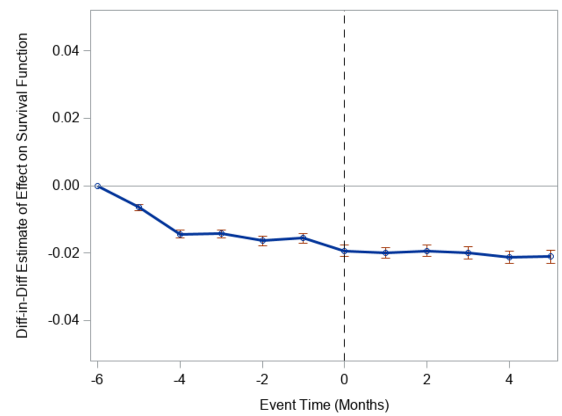
(c) Event Study: Survival Function



(d) FLSA 2016: Effect on Survival Function



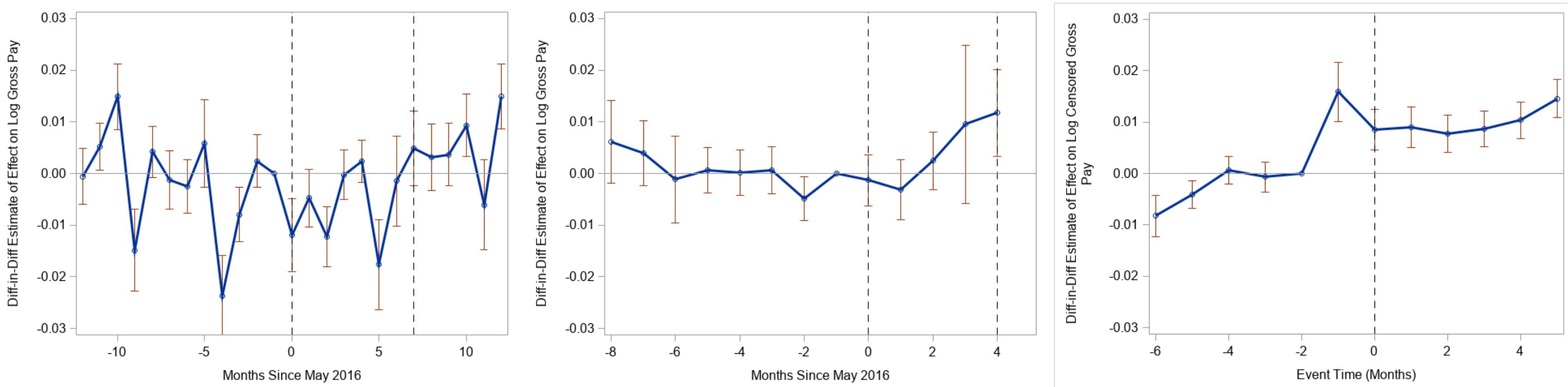
(e) FLSA 2020: Effect on Survival Function



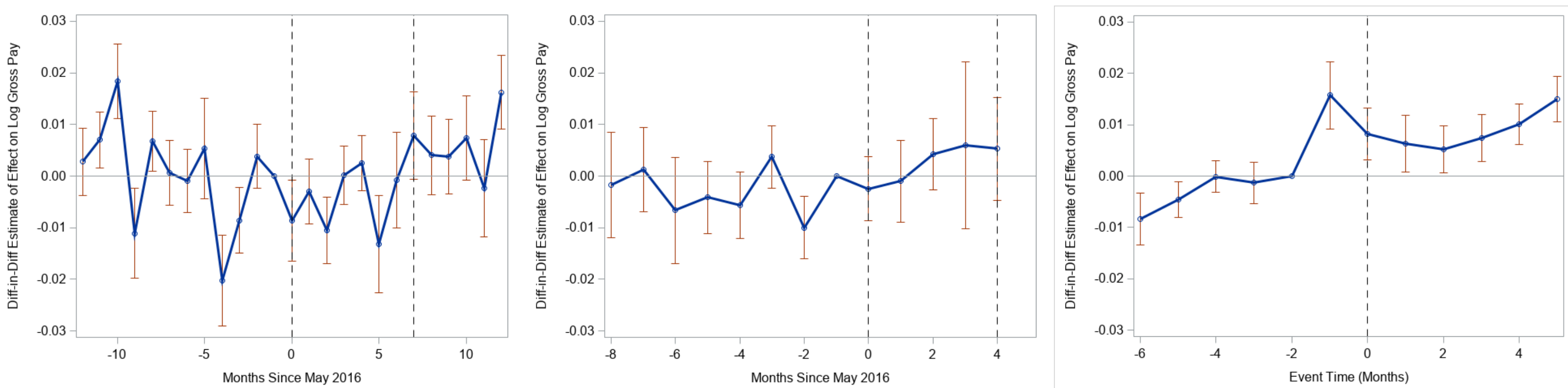
(f) Event Study: Effect on Survival Function

Notes: Panels (a)-(c) plots the survival function of workers directly affected by the 2016 FLSA policy, 2020 FLSA policy, and 16 state policies, respectively, along with the survival function of workers in their respective control groups defined in section 6. Panels (d)-(f) shows the difference in survival function between the treatment and control groups, corresponding to each of their above graphs. For the FLSA figures, the first dotted vertical line at 0 indicates the month that the rule change is announced. In panels (a) and (d), the second dotted line is at seven months after the announcement when the policy was supposed to go into effect. For the FLSA 2020 figures, the new threshold goes into effect four months after the announcement. For the event-study figures, the vertical line indicates the month that the new threshold goes into effect.

Appendix Figure A.10: Difference-in-Difference Estimates of the Effect of Raising the OT Exemption Threshold on Gross Pay



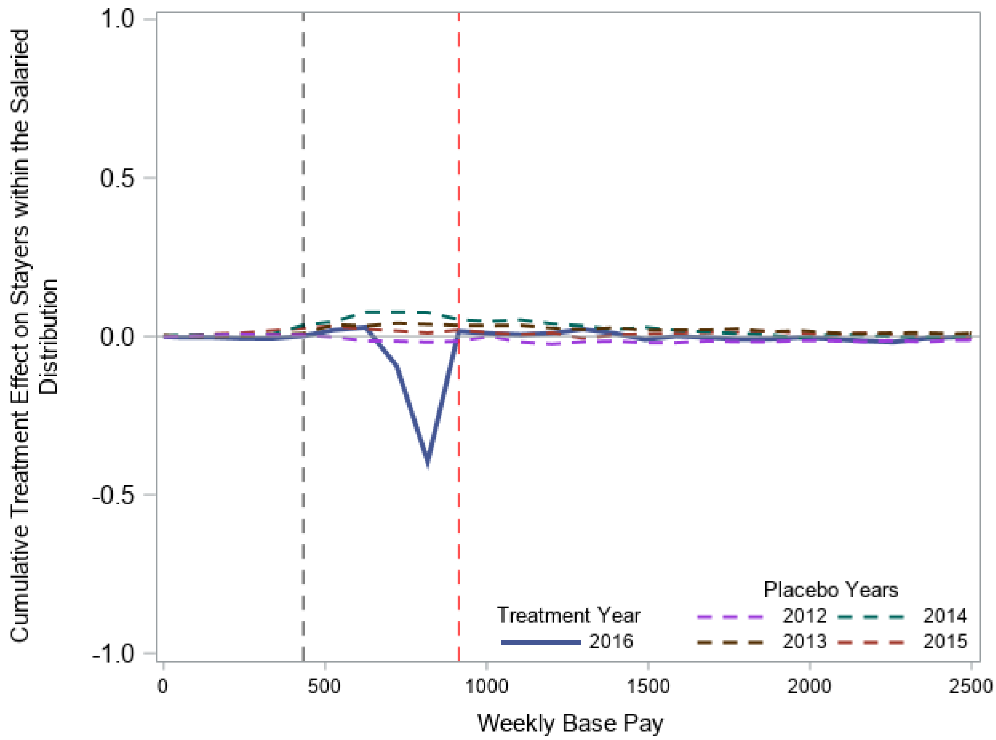
(a) 2016 FLSA: Censored Gross Pay (b) 2020 FLSA: Censored Gross Pay (c) Event Study: Censored Gross Pay



(d) 2016 FLSA: Uncensored Gross Pay (e) 2020 FLSA: Uncensored Gross Pay (f) Event Study: Uncensored Gross Pay

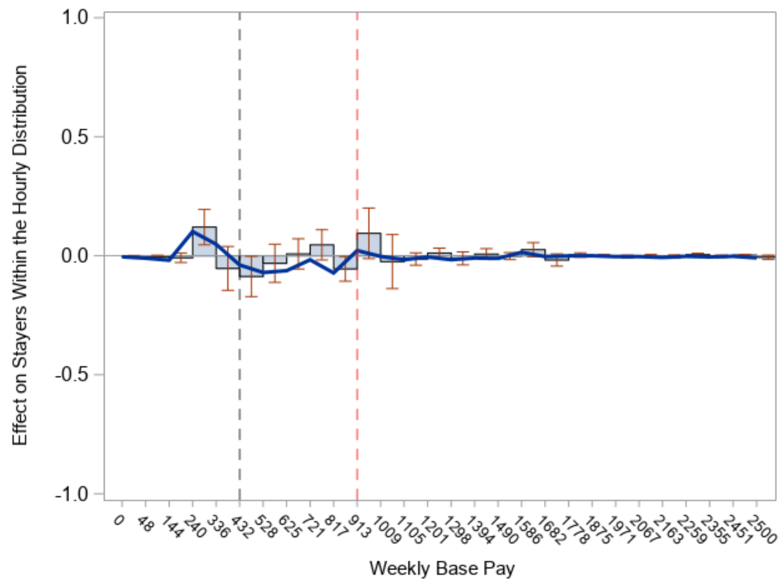
Notes: Panels (a)-(c) show the effect of raising the overtime exemption threshold on gross pay for salaried workers initially earning between the old and new thresholds, where gross pay is censored at two times total pay. Panels (d)-(f) report the estimates using uncensored gross pay. All estimates are computed from equation 7, where the four panels on the left compares workers in the year of the FLSA rule change to similar workers in the preceding year whereas the right panels compare workers in states that raise their thresholds to similar workers in all states that do not. For the FLSA rule changes, the dotted vertical line at 0 indicates the month that the rule change is announced, whereas the second dotted line shows the month that the threshold actually goes into effect. For the state rule changes, the vertical line indicates the month that the new threshold goes into effect.

Appendix Figure A.11: Placebo Test of Effect of 2016 FLSA Policy on Always-Salaried Workers

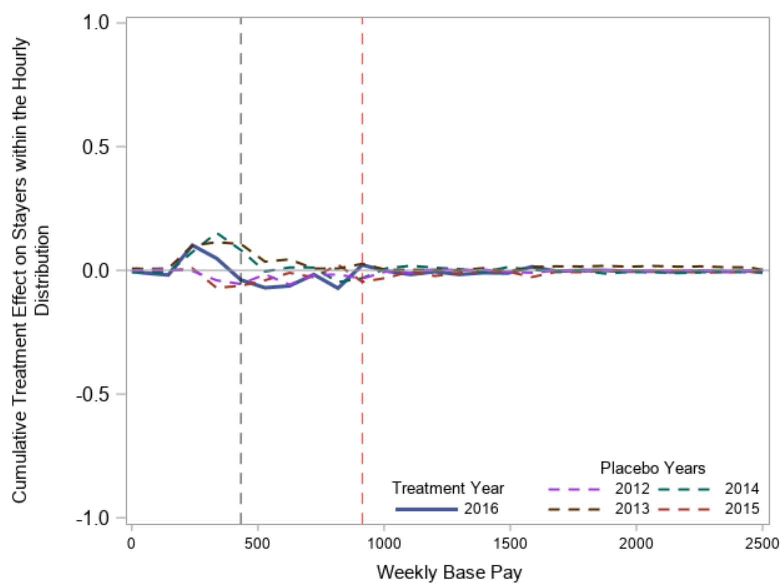


Notes: This figure shows the cumulative sum of the effect of the 2016 FLSA policy on the base pay distribution of continuously employed workers who are salaried before and after the policy, estimated using equation 3 assuming γ_0=0. Each line is the estimated effect for the year indicated in the legend, using the workers in the previous years as a control. The vertical black and red lines are at the old and new OT exemption thresholds ($455 and $913), respectively.

Appendix Figure A.12: Analysis of Flows Within the Hourly Distribution Between April and December



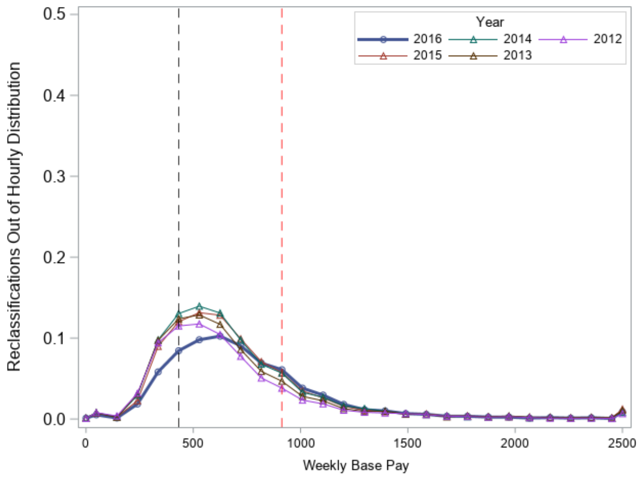
(a) Estimates of the Effect on Number of Salaried Workers



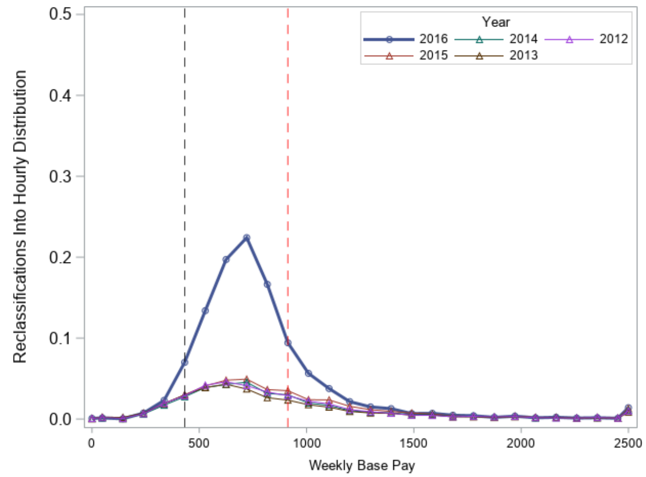
(b) Placebo Test of Effect on Salaried Distribution

Notes: Panel (a) shows the effect of the 2016 OT policy on the distribution of workers who stay at the same firm between April and December 2016, and are paid by hour in both months, estimated using equation 3, assuming $\gamma_0 = 0$. The solid blue line is the cumulative sum of the bin-specific effects. Panel (b) shows the cumulative effect of raising the OT exemption threshold on the number of job-stayers in December of each year between 2012 and 2016. The solid blue line in panel (b) is the same as the solid blue line in panel (a). The dotted graphs are similarly defined running sums, except estimated using the December and April distributions of the labeled year and the preceding adjacent year. In both graphs, the vertical black and red lines are at the old and new OT exemption thresholds (\$455 and \$913), respectively.

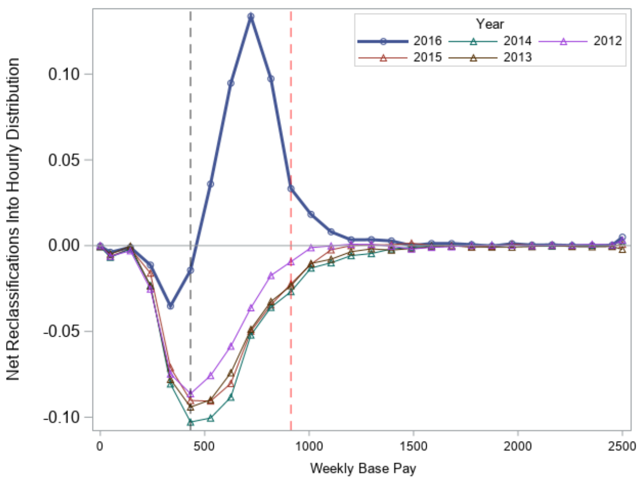
Appendix Figure A.13: Analysis of Reclassifications in and out of the Hourly Distribution



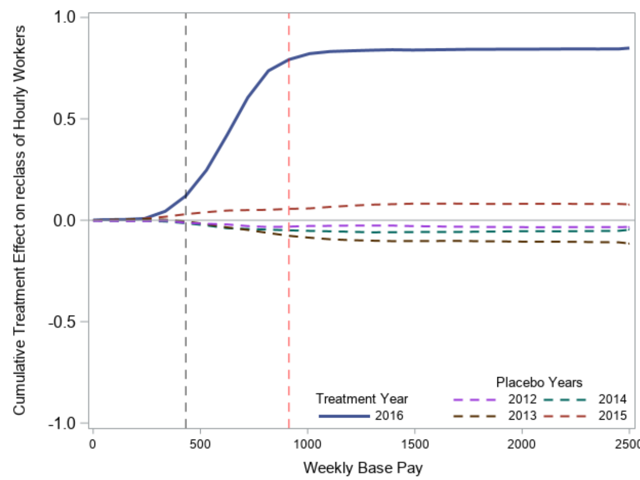
(a) Reclassifications from Hourly



(b) Reclassifications into Hourly



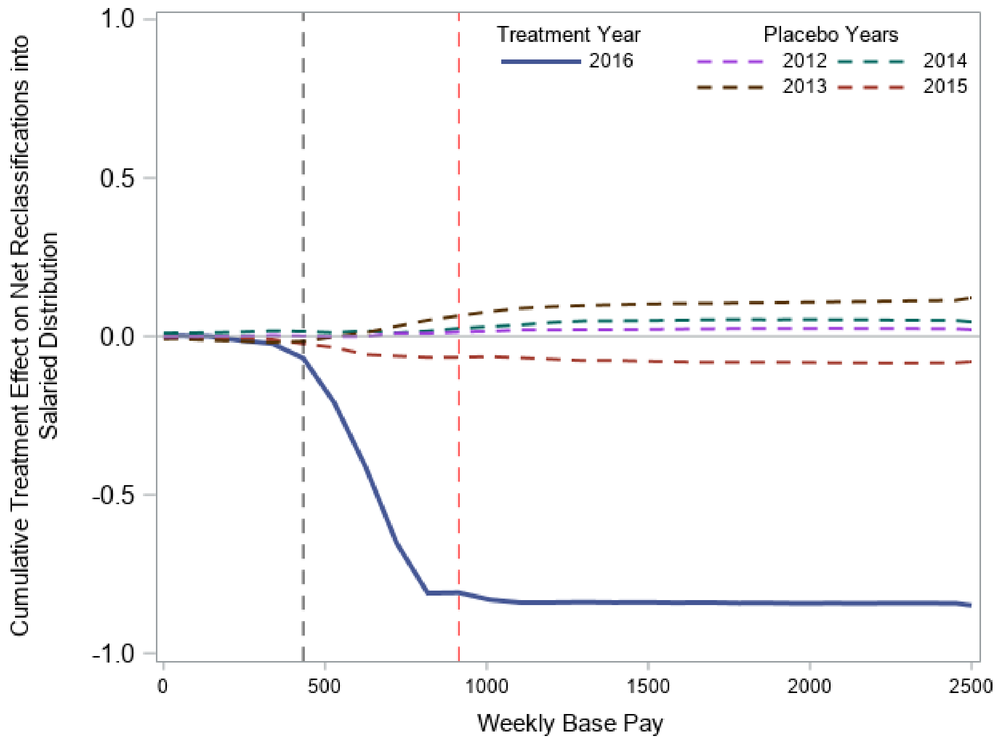
(c) Distribution of Net Reclassifications by Year



(d) Placebo Test of Net Reclassification Effects

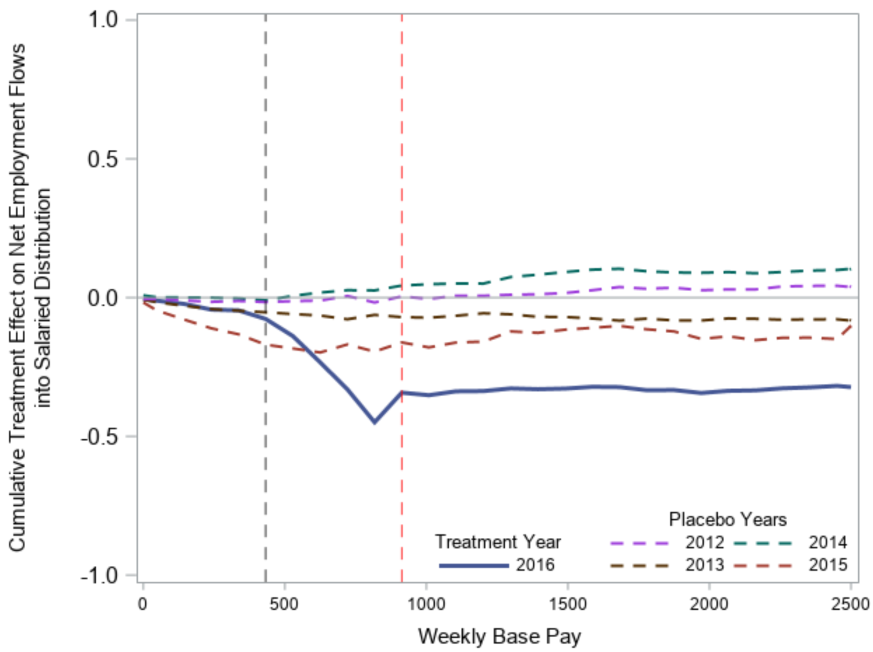
Notes: Figure (a) shows the frequency distribution of base pays in April of each year between 2012 and 2016, averaged across firms, for workers who are hourly in April, hourly in December, and employed in the same firm in both months. Figure (b) shows the frequency distribution of base pays in December of each year for workers who are hourly in April, hourly in December, and employed in the same firm in both months. Panel (c) shows the difference between panel (b) and (a). Panel (d) presents the cumulative effects estimated from equation 3, assuming $\gamma_1 = 1$ and $\gamma_0 = 0$ for each year from 2012 to 2016 In all figures, the vertical black and red lines are at the initial and proposed FLSA thresholds, respectively.

Appendix Figure A.14: Placebo Test of Net Reclassification Flows into the Salaried Distribution



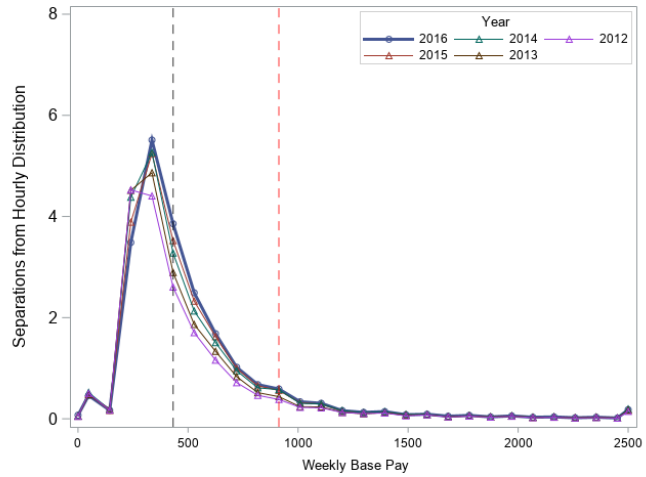
Notes: This figure shows the cumulative sum of the effect of the 2016 FLSA policy on net reclassifications into the salaried distribution, estimated using equation 3 assuming γ_1=1 and γ_0=0. Each line is estimated as the reclassification flows in the year indicated in the legend minus the reclassification flows in the adjacent previous year. The vertical black and red lines are at the old and new OT exemption thresholds ($455 and $913), respectively.

Appendix Figure A.15: Placebo Test of Net Employment Flows into the Salaried Distribution

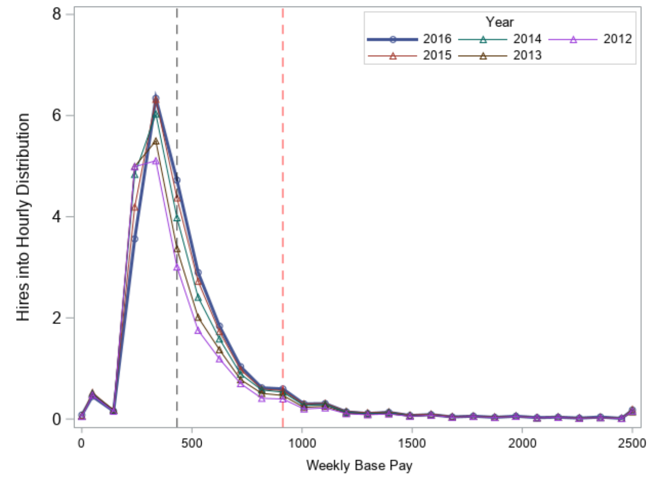


Notes: This figure shows the cumulative sum of the estimates of equation 3 using the number of hires minus separations within each bin as the outcome variable. Each line is estimated using the employment flows from the year indicated in the legend and the employment flows in the adjacent previous year.

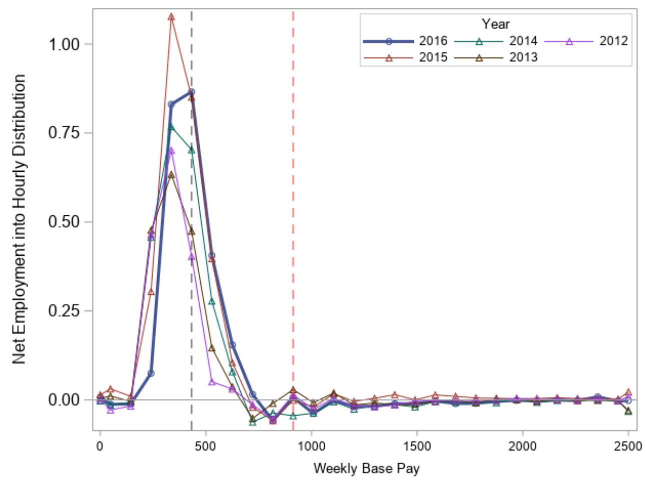
Appendix Figure A.16: Analysis of the Employment Flows into the Hourly Distribution



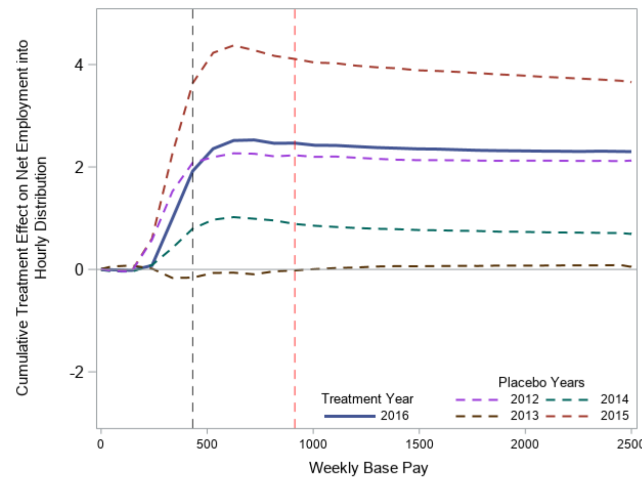
(a) Separations Between April and December



(b) New Hires Between April and December



(c) Distribution of Net Employment by Year



(d) Placebo Test of the Net Employment Effect

Notes: Panel (a) shows frequency distribution of base pays in April of each year between 2012 and 2016, averaged across firms, for hourly workers who separate from their employer between April and December. Panel (b) shows the frequency distribution of base pays in December of each year for hourly workers hired between April and December. Panel (c) shows the difference between panel (b) and (a). Panel (d) presents the cumulative effects estimated from equation 3 for each year from 2012 to 2016. In all figures, the vertical black and red lines are at the initial and proposed FLSA thresholds, respectively.

Appendix Table A.1: Effect of Raising the State OT Exemption Threshold on Employment and Reclassification

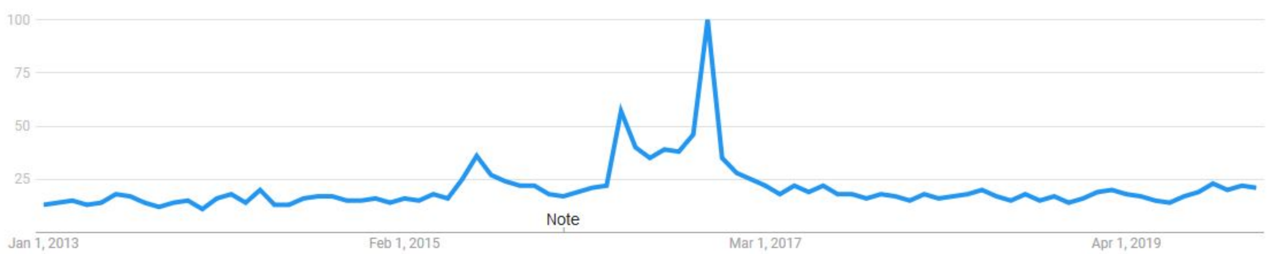
	Employment			Reclassification		
	Hires	Separations	Net	Into	Out of	Net
Salaried, Below	-0.008^{***}	0.004^{**}	-0.012^{***}	-0.002^{***}	0.025^{***}	-0.027^{***}
	(0.002)	(0.002)	(0.002)	(0.001)	(0.003)	(0.003)
Salaried, Above	0.002	0.003^{***}	-0.001	0.003^{***}	0.004^{***}	-0.001
	(0.002)	(0.001)	(0.002)	(0.001)	(0.001)	(0.001)
Hourly, Below	-0.025^{*}	0.018	-0.042^{*}	0.026^{***}	0.001	0.025^{***}
	(0.014)	(0.013)	(0.025)	(0.003)	(0.001)	(0.003)
Hourly, Above	-0.007	0.005	-0.012	0.007^{***}	0.000	0.007^{***}
	(0.006)	(0.006)	(0.011)	(0.001)	(0.000)	(0.001)
Cumulative	-0.037^{*}	0.031	-0.067^{*}	0.033^{***}	0.030^{***}	0.004^{***}
	(0.020)	(0.019)	(0.036)	(0.004)	(0.004)	(0.001)
Treatment Group						
Affected Salaried	1.21	1.21	1.21	1.21	1.21	1.21
Affected Hourly	4.21	4.21	4.21	4.21	4.21	4.21
Avg. Firm Size	109	109	109	109	109	109
No. Firm-Events	182,136	182,136	182,136	182,136	182,136	182,136

Notes: The first column reports the effect of raising the OT exemption threshold on the number of new hires among salaried jobs paying within $160 below the new threshold, salaried jobs paying within $80 above the new threshold, hourly jobs within the same ranges, and the sum of salaried and hourly jobs within those ranges. Each estimate is scaled by the number of affected salaried worker, reported in row 6. The second column reports estimates for the effect on separations by the same groups. The third column is the difference between the first two columns. The forth column reports the effect on the number of reclassifications into each respective group from the alternative pay classification (e.g. row 1 is the number of reclassifications into salaried jobs paying below the new threshold from any hourly jobs). The fifth column reports reclassifications away from each respecive group into the alternative pay classification. The sixth column is the difference between the fouth and fifth columns. All values are estimated from equation 6, and robust standard errors in parentheses are clustered by firm. $^{*}p < .1,^{**}p < .05,^{***}p < .01$

Appendix B. History of the 2016 FLSA Policy

The first public announcement of the Department of Labor's intent to update the FLSA overtime exemption threshold occurred on March 13, 2014. After identifying problems with the existing threshold, President Obama declared "I'm directing Tom Perez, my Secretary of Labor, to restore the common-sense principle behind overtime... we're going to consult with both workers and businesses as we update our overtime rules" (White House Archives - March 13, 2014). The reaction from the press was that "Mr. Obama's decision to use his executive authority to change the nation's overtime rules is likely to be seen as a challenge to Republicans in Congress, who have already blocked most of the president's economic agenda" (NYT March 14, 2014). However, while there was an expectation of resistance from Congress, Google search trends suggest that the FLSA overtime exemption policy did not receive much attention from the public at this time (see figure B.1).

Appendix Figure B.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 2013 and January 2020. A value of 100 indicates its highest popularity level, and the measure of popularity is scaled proportional to this instance.

Interest in the the FLSA grew in 2015 following the DOL's announcement on June 26th that it would like to "raise the threshold under which most salaried workers are guaranteed overtime to equal the 40th percentile of weekly earnings for full-time salaried workers. As proposed, this would raise the salary threshold from \$455 a week (\$23,660 a year) – below the poverty threshold for a family of four – to a projected level of \$970 a week (\$50,440 a year) in 2016" (White House Archives June 30, 2015). Consistent with the normal rulemaking process, the Department of Labor stated that it would release a finalized rule the next year after reviewing comments from the public regarding its current proposal. Similar to the initial announcement in 2014, new articles at the time believed that the policy would face challenges in the courts (NYT June 30, 2015). There were also some reports that companies were already investing in new software to comply with the policy (WSJ Jul 21, 2015), though I do not observe any evidence of this adjustment in the data (compare the difference-distributions in figure 2b).

The finalized threshold of $913 per week was announced on May 18, 2016, and was set to go into effect on December 1, 2016 with automatic updating every three years to adjust for inflation.[52] This announcement received considerable attention from employers, as evident from the spike in Google searches for "FLSA Overtime". In response to the new regulation, "Republican lawmakers, who are close to many of the industries that oppose the new rule, have vowed to block it during a mandated congressional review period". However, given Donald Trump's presidential campaign, there was an understanding that repealing the regulation would be a risky political move for the Republican party as it "could exacerbate an already palpable split between Mr. Trump's blue-collar supporters and the party's establishment donors and politicians" (NYT May 18, 2016). Hence, it was not clear at this point that the rule would be repealed.

On September 20, 2016, twenty-one States sued the Department of Labor in federal court in Sherman, Texas. They argued that the new regulation should be nullified for two reasons. First, they claimed that "the FLSA's overtime requirements violate the Constitution by regulating the States and coercing them to adopt wage policy choices that adversely affect the States' priorities, budgets, and services". Second, the states argued that the magnitude of the proposed overtime exemption threshold conflicted with Congress' original intent in the FLSA to exempt "any employee employed in a bona fide executive, administrative, or professional capacity" (State of Nevada et al v. United States Department of Labor et al, Filing 60). While the DOL has historically used both a duties test and a salary test to define these occupations, the States argued that the language of the text indicates that Congress intended for a duties test to be the primary determinant of overtime exemption status, and a salary threshold of $913 effectively supplants the duties test. Under the Chevron deference principle, the new rule would therefore exceed the power given to the Department of Labor by Congress.

Given the lack of media coverage over the court proceedings, it came as a surprise to employers when Judge Amos L. Mazzant III placed a preliminary injunction on the new overtime exemption threshold on November 22, 2016, after agreeing with the plaintiffs' second argument. From a review of newspaper articles at the time, I find no reports on the court case in the Wall Street Journal or New York Times between the date of the initial court filing and the date of the injunction. While I do find mentions of the lawsuit as part of

[52]The final rule also raised the threshold for "highly compensated employees" from $100,000 per year to $134,004. Workers above this threshold are subject to a less stringent duties test to be exempt from overtime. I do not find any bunching in response to this component of the policy.

broader news on the FLSA overtime exemption threshold, none go into any more detail than stating that a case is under way (eg. USA Today Oct. 12, 2016). Consistent with the lack of awareness of the appeal against the new overtime exemption threshold, I see no increase in Google traffic for the term "FLSA Overtime" in September when the initial case was filed, but a large spike in November after its injunction.

Even among individuals aware of the lawsuit, there was the belief that employers should be ready for the December 1st deadline. For example, a story by the Washington Post quoted a senior executive at the National Federation of Independent Business that "employers can't count on a reprieve, and playing chicken with the Dec. 1 deadline 'could be a very expensive mistake"' (Washington Post Oct 20, 2016). Similarly, an attorney interviewed by the Society of Human Resource Management stated that "although it's possible,... employers shouldn't expect a miracle before the Dec. 1 implementation deadline." (SHRM Oct 21, 2016). Overall, there is no indication that employers expected the injunction.

Since employers did not foresee the injunction, many had already implemented changes in anticipation of the policy or followed through with their promises to their workers. For instance, Wal-Mart and Kroger both raised their managers' salaries above the new overtime exemption threshold and did not retract them after the injunction (WSJ Dec 20, 2016). On the other hand, Burger King announced that it would defer its initial plan to convert its salaried manager to hourly in light of the injunction Slate Jan 16, 2017]. Aside from retail and fast food restaurants, anecdotally, the policy also had a large effect on institutions of higher education. The National Institutes of Health (NIH) and many large universities also gave their post-docs raises above the proposed overtime exemption threshold (Science Jan 4, 2017). On the other hand, some institutions such as the University of Maryland and Arizona State University retracted their promises to either pay their employees overtime or increase their salaries (Huffpost June 7, 2017).

Following the preliminary injunction, there was a general belief from judge Mazzant's language that the $913 exemption threshold would not survive. However, it was uncertain how long the judicial process would take and whether the new Trump administration would propose a smaller increase to the overtime exemption threshold (NYT Nov 22, 2016). It became clearer that the new administration had no desire to defend the overtime policy in courts after the nomination of fast-food executive, and critic of overtime regulation, Andrew Puzder as Labor Secretary on December 8, 2016 (Forbes March 18, 2016). In the end, Andrew Puzder did not receive enough support from the Senate for his confirmation on February 15, 2016 and the position ultimately went to Alexander Acosta. Nevertheless, Acosta reaffirmed employers' priors that the overtime threshold proposed by Obama would never go into effect.

When asked about the overtime exemption threshold during his confirmation hearing on March 22, 2017, Acosta stated that “if you were to apply a straight inflation adjustment, I believe the figure if it were updated would be somewhere around $33,000”. The Department of Labor officially dropped its defense of the $913 threshold in June 2017.

After the DOL abandoned its defense of the $913 threshold in June 2017, they submitted a new Request for Information on June 27 (DOL June 27, 2017), allowing the public an opportunity to submit their opinions of the overtime exemption threshold. In December 2017, the DOL announced that it plans to propose a new threshold by October 2018, and most employers believed that it would be within the $30,000-35,000 per year range SHRM March 2018. The DOL officially proposed a new threshold of $679 per week ($35,308 per year) on March 7, 2019. After a period of public comments, on September 24, 2019, the DOL finalized the new threshold at $684 per week. This new threshold went into effect on January 1, 2020 without as much coverage as the 2016 policy (see figure B.1).

Appendix C. Additional Sources of Variation

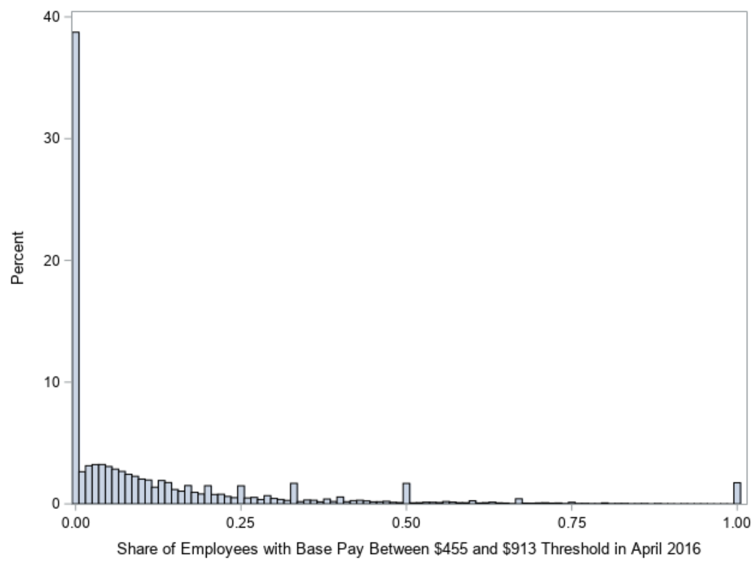
In this section, I explore how the 2016 FLSA policy affected firms that initially did not employ any workers between the old and new thresholds, and workers who initially already received overtime pay. I find that despite their initial characteristics, these firms and workers respond to the rule change in 2016, and therefore fail the Stable Unit Treatment Value Assumption (SUTVA) that is needed to qualify them as control groups for identifying the effect of the policy.

To begin, I plot in figure C.1 the distribution of firms by the percent of their workforce directly affected by the 2016 policy proposal. I find that nearly 40% of firms employed no salaried workers between the old and new thresholds in April 2016. Restricting the sample to only these firms, I identify the effect of the 2016 policy using the same strategy I applied in section 5.1. I compare the change in the frequency distribution of base pays between April and December 2016, to the change in the distribution in previous years. Since these firms never employed any workers between the old and new thresholds in April, they can only gain new employees within that range. As evident from figure C.2, the number of workers hired with salaries between \$455 and \$913 per week in 2016 is noticeably smaller than in previous years. This graphical evidence suggests that as long as firms have the option of hiring workers who are affected by the federal rule change, their behavior will be affected by the policy regardless of whether or not they were initially employing any such workers.

Similarly, I find that the 2016 attempt to increase the overtime exemption threshold also had an effect on workers who appear to already be covered for overtime before the announcement of the policy. Since I am unable to observe employees' exemption status in the data, I instead focus my attention on the sample of salaried workers who received positive overtime compensation in April 2016 and remain employed in December. Plotting the number of workers reclassified between April and December of each year between 2012 and 2016 by base pay, I show in figure C.3 that far more workers were reclassified between the old and new thresholds in 2016 than in any previous years. This difference disappears past the \$913 threshold, indicating that the 2016 rule change affected the pay classification of workers who were already receiving overtime pay in April.

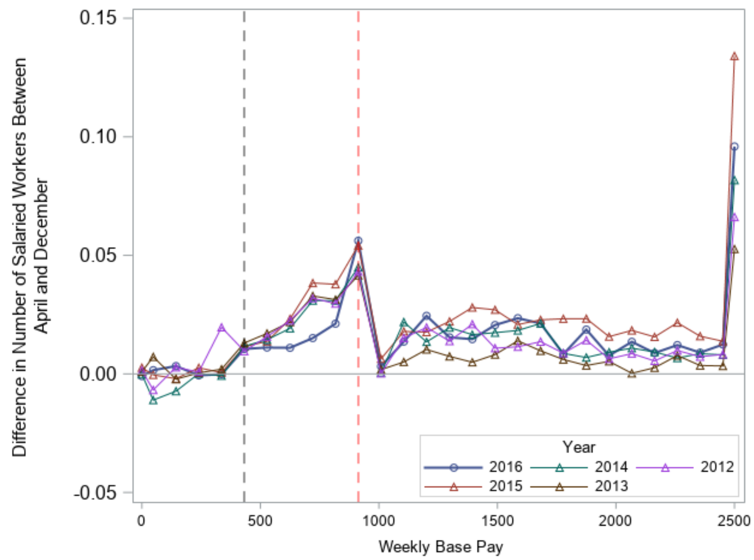
While salaried workers who never work above 40 hours per week could arguably also act as a control, in my analysis, I consider these workers as part of the treatment group. From a labor demand perspective, one of the main concerns that businesses raised to the DOL in response to the 2016 rule change is the cost of monitoring salaried workers' hours. Thus, covering employees who never engage in overtime work may nevertheless raise their cost to the firm. Furthermore, from a labor supply perspective, workers who engage in no more than 40 hours of labor per week may want to increase their hours once they are covered for overtime. As a practical matter, I also do not observe the hours of salaried workers in the data if they are not covered for overtime.

Appendix Figure C.1: Distribution of Firms by Share of Employees Directly Affected by the 2016 FLSA Policy



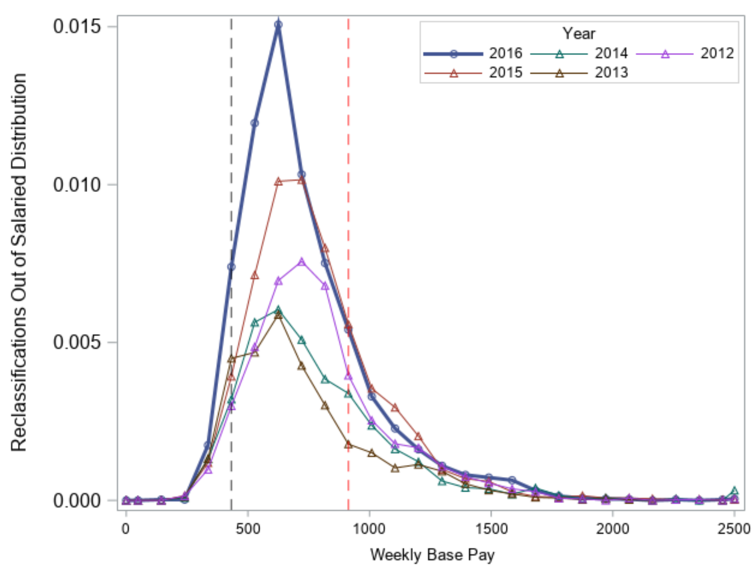
Notes: The figure shows the distribution of firms in April 2016 by the share of workers who are paid by salary, and earn between $455 and $913 per week.

Appendix Figure C.2: Difference in the Base Pay Distribution of Salary Workers in "Unaffected" Firms



Notes: The figure shows the difference between the frequency distribution of salaried workers' base pay between April and December of each year between 2012 and 2016. The sample is restricted to firms that did not employ any salaried workers with base pays between \$455 and \$913 in April.

Appendix Figure C.3: Base Pay Distribution of "Already Covered" Salaried Workers Reclassified from Salaried to Hourly



Notes: The figure shows the number of workers reclassified from salaried to hourly in the average firm for each year between 2012 and 2016. Within each firm, the sample is restricted to only employees who were salaried and earned nonzero overtime pay in April, and remain employed in December.

Appendix D. Derivation of the Conceptual Framework

D.1 Solving the baseline model

The worker's utility from employment and searching are characterized by the following Bellman equations:

$$(r+\delta)V_e(w,h) = w - a^{-\frac{1}{\epsilon}}\frac{h^{1+\frac{1}{\epsilon}}}{1+\frac{1}{\epsilon}} + \delta V_n$$

$$rV_n = b + \lambda\int_{V_e(\theta,F)\geq V_n}[V_e(w(\theta,F),h(\theta,F)) - V_n]dG(\theta,F)$$

The firm's present value of profits is given by

$$J = \frac{\theta h^\beta - w + F\cdot \mathrm{sgn}(S)}{r+\delta}$$

I want to express $w_0, h_0, S_0,\ \theta_0^*$, and V_n in terms of (θ, a, F) and model primitives. First, rearrange the worker's Bellman equation for the value of employment as

$$V_e(w,h) - V_n = \frac{w - a^{-\frac{1}{\epsilon}}\frac{h^{1+\frac{1}{\epsilon}}}{1+\frac{1}{\epsilon}} - rV_n}{r+\delta} \tag{8}$$

Substitute (8) into the Nash bargaining problem and take first order conditions:

$$(w_0,h_0,S_0) = \arg\max_{(w,h,S)}[V_e(w,h)-V_n]^\alpha\Big[\frac{\theta h^\beta - w + F\cdot\mathrm{sgn}(S)}{r+\delta}\Big]^{1-\alpha}$$

$$\mathrm{FOC}_w = \alpha[h^\beta\theta - w - F_S] - (1-\alpha)[w - a^{-\frac{1}{\epsilon}}\frac{h^{1+\frac{1}{\epsilon}}}{1+\frac{1}{\epsilon}} - rV_n] \tag{9}$$

$$\mathrm{FOC}_h = -\alpha a^{-\frac{1}{\epsilon}}h^{\frac{1}{\epsilon}}(h^\beta\theta - w - F_S) + (1-\alpha)\beta h^{\beta-1}\theta(w - a^{-\frac{1}{\epsilon}}\frac{h^{1+\frac{1}{\epsilon}}}{1+\frac{1}{\epsilon}} - rV_n) \tag{10}$$

Rearrange (9) for w_0, then substitute w_0 into (10) to solve for h_0. S_0 is simply the corner solution that maximizes the Nash product.

$$w_0 = \alpha\Big(\theta h_0^\beta + F\cdot\mathrm{sgn}(S_0)\Big) + (1-\alpha)\Big(a^{-\frac{1}{\epsilon}}\frac{h_0^{1+\frac{1}{\epsilon}}}{1+\frac{1}{\epsilon}} + rV_n\Big)$$

$$h_0 = \Big(a^{\frac{1}{\epsilon}}\beta\theta\Big)^{\frac{1}{1+\frac{1}{\epsilon}-\beta}}$$

$$S_0 = \arg\max_{S\in\{-1,1\}} F\cdot\mathrm{sgn}(S)$$

Note that at h_0, $\frac{\partial J}{\partial h} = \frac{\partial V_e(w,h)}{\partial h}$. In other words, h_0 maximizes the total surplus of the employment relationship. To solve for $\theta_0^*(a, F)$, substitute (w_0, h_0) into (8) and solve for the value of θ such that the expression equals 0.

$$\begin{aligned}
V_e(w_0, h_0) - V_n &= 0 && \Leftrightarrow \\
w_0 - a^{-\frac{1}{\epsilon}} \frac{h_0^{1+\frac{1}{\epsilon}}}{1+\frac{1}{\epsilon}} - rV_n &= 0 && \Leftrightarrow \\
\alpha\Big(\theta h_0^\beta + F \cdot \text{sgn}(S_0) - a^{-\frac{1}{\epsilon}} \frac{h_0^{1+\frac{1}{\epsilon}}}{1+\frac{1}{\epsilon}} - rV_n\Big) &= 0 && \Leftrightarrow \\
\theta_0^* = \left[\frac{rV_n - \alpha F_S}{(a^{\frac{1}{\epsilon}}\beta)^{\frac{\beta}{1+\frac{1}{\epsilon}-\beta}}(1 - \frac{\beta}{1+\frac{1}{\epsilon}})}\right]^{\frac{1+\frac{1}{\epsilon}-\beta}{1+\frac{1}{\epsilon}}}
\end{aligned}$$

Note from the third line that at θ_0^*, the total surplus of the employment relationship equals 0. I will use this property when computing the comparative statics. Lastly, from the Bellman equation representing the worker's value of searching, I numerically solve for V_n

$$\begin{aligned}
rV_n &= b + \lambda \int_{V_e(\theta,F)\geq V_n} [V_e(w(\theta,F), h(\theta,F)) - V_n] dG(\theta, F) \\
&= b + \lambda \int_{\theta \geq \theta_0^*(F,V_n),F} \left[\frac{w_0(\theta, F, V_n) - a^{-\frac{1}{\epsilon}} \frac{h_0(\theta,V_n)^{1+\frac{1}{\epsilon}}}{1+\frac{1}{\epsilon}} - rV_n}{r+\delta}\right] dG(\theta, F)
\end{aligned}$$

D.2 Solving the model with overtime

Case 1: No Fixed Costs or Wage Rigidities

Given job characteristics (w, h, S), the firm's profit is given by

$$J = \frac{\theta h^\beta - (1 + \eta_{(w,h,S)})w + F \cdot \text{sgn}(S)}{r + \delta}$$

where

$$\eta_{(w,h,S)} = \begin{cases} \frac{0.5(h-40)}{40} & \text{if } h > 40 \text{ and } S = -1 \\ \frac{1.5(h-40)}{40} & \text{if } h > 40,\ S = 1, \text{ and } w < \bar{w} \\ 0 & \text{otherwise} \end{cases} \tag{11}$$

The worker's value of employment follows

$$(r+\delta)V_e(w,h) = (1+\eta_{(w,h,S)})w - a^{-\frac{1}{\epsilon}}\frac{h^{1+\frac{1}{\epsilon}}}{1+\frac{1}{\epsilon}} + \delta V_n$$

Following the same steps as in the model without overtime, I solve the Nash bargaining problem

$$\begin{aligned}
(w_1,h_1,S_1) &= \arg\max_{(w,h,S)} [V_e(w,h) - V_n]^{\alpha}\Big[\frac{\theta h^{\beta} - (1+\eta_{(w,h,S)})w + F\cdot \mathrm{sgn}(S)}{r+\delta}\Big]^{1-\alpha} \\
\mathrm{FOC}_w &= \alpha[h^{\beta}\theta - (1+\eta)w - F_S] - (1-\alpha)[(1+\eta)w - a^{-\frac{1}{\epsilon}}\frac{h^{1+\frac{1}{\epsilon}}}{1+\frac{1}{\epsilon}} - rV_n] \qquad (12) \\
\mathrm{FOC}_h &= -\alpha a^{-\frac{1}{\epsilon}}h^{\frac{1}{\epsilon}}\big(h^{\beta}\theta - (1+\eta)w - F_S\big) + (1-\alpha)\beta h^{\beta-1}\theta\big((1+\eta)w - a^{-\frac{1}{\epsilon}}\frac{h^{1+\frac{1}{\epsilon}}}{1+\frac{1}{\epsilon}} - rV_n\big) \qquad (13)
\end{aligned}$$

The Nash bargaining solution is

$$\begin{aligned}
w_1 &= \frac{1}{1+\eta_{(w,h,S)}}\left[\alpha\Big(\theta h_1^{\beta} + F\cdot\mathrm{sgn}(S_1)\Big) + (1-\alpha)\Big(a^{-\frac{1}{\epsilon}}\frac{h_1^{1+\frac{1}{\epsilon}}}{1+\frac{1}{\epsilon}} + rV_n\Big)\right] \\
h_1 &= \Big(a^{\frac{1}{\epsilon}}\beta\theta\Big)^{\frac{1}{1+\frac{1}{\epsilon}-\beta}} \\
S_1 &= \arg\max_{S\in\{-1,1\}} F\cdot\mathrm{sgn}(S)
\end{aligned}$$

Notice that $h_1 = h_0$, $S_1 = S_0$, and $(1+\eta_{(w,h,S)})w_1 = w_0$. In other words, gross pay, weekly hours, and pay classification all remain the same. By extension, θ_* and V_n are the same as in the baseline model.

Case 2: Fixed Costs

Given job characteristics (w,h,S), the firm's profit is given by

$$J = \frac{\theta h^{\beta} - (1+\eta_{(w,h,S)})w + F\cdot\mathrm{sgn}(S) - C\cdot 1[S=1, w<\bar{w}]}{r+\delta}$$

where C is a constant and $1[S=1,w<\bar{w}]$ is an indicator that equals 1 if $S=1$ and $w<\bar{w}$. The worker's value of employment and search follow the same formulation as case 1. To solve the Nash bargaining problem, I need to compare the Nash product of the interior solution and the corner solution where $w_2 = \bar{w}$.

Interior solution:

$$
\begin{aligned}
w_2 &= \frac{1}{1+\eta_{(w,h,S)}}\left[\alpha\Big(\theta h_1^{\beta} + F_S - C_{1[S=1,w<\bar{w}]}\Big) + (1-\alpha)\Big(a^{-\frac{1}{\epsilon}}\frac{h_0^{1+\frac{1}{\epsilon}}}{1+\frac{1}{\epsilon}} + rV_n^{OT}\Big)\right] \\
h_2 &= \Big(a^{\frac{1}{\epsilon}}\beta\theta\Big)^{\frac{1}{1+\frac{1}{\epsilon}-\beta}} \\
S_2 &= \arg\max_{S\in\{-1,1\}} F_S - C_{1[S=1,w<\bar{w}]}
\end{aligned}
$$

where V_n^{OT} is the value of unemployment given the overtime policy.

Corner solution:

$$
\begin{aligned}
& w_2 = \bar{w} \\
& h_2 \text{ solves } \mathrm{FOC}_h|_{w=\bar{w}} = 0 \\
& \mathrm{FOC}_h|_{h=h_0,w=\bar{w}} = a^{-\frac{1}{\epsilon}} h_0^{\frac{1}{\epsilon}}[\bar{w} - w_0] > 0 \\
& S_2 = 1
\end{aligned}
$$

All salaried jobs with $w_0 \geq \bar{w}$ and all hourly jobs are unaffected by the monitoring cost C. As such, the hours of these jobs are equivalent to that in case 1, and earnings is likewise similar except for an adjustment of $(1-\alpha)(rV_n^{OT} - rV_n)$. For salaried workers with base incomes less than the overtime exemption threshold, one of three outcomes can occur:

1. Interior solution, where $F > 0$ and $2F - C < 0$. This implies that without overtime, the job is salaried $S_0 = 1$, but with overtime, it becomes reclassified as hourly $S_2 = -1$. The hours remain constant $h_2 = h_0$, but the base income becomes $w_2 = \frac{w_0 - 2\alpha F + (1-\alpha)r(V_n^{OT} - V_n)}{1+\eta_{(w,h,S)}}$, and gross income $g = (1+\eta)w_2 = w_0 - 2\alpha F + (1-\alpha)r(V_n^{OT} - V_n)$.

2. Interior solution, where $2F - C \geq 0$. The job's salaried status and hours are the same as in a world without overtime $(h_2, S_2) = (h_0, S_0)$. However, base income decreases to $w_2 = \frac{w_0 - \alpha C + (1-\alpha)r(V_n^{OT} - V_n)}{1+\eta_{(w,h,S)}}$, and gross income to $g = w_0 - \alpha C + (1-\alpha)r(V_n^{OT} - V_n)$.

3. Corner solution. This creates bunching in the base income distribution of salaried workers. Weekly hours cannot be expressed as a closed form solution, but by evaluating the first order condition at $w = \bar{w}$ and $h = h_0$, I show that weekly hours should increase relative to the baseline scenario, and to a first order approximation, the increase in hours is proportional to the increase in income.

The sign and magnitude of $V_n^{OT} - V_N$ for a given worker type a depends on the distribution of (θ, F) and the proportion of workers affected by each of the above three responses. From the worker's Bellman equation, one can express $V_n^{OT} - V_N$ as:

$$[\frac{r+\delta}{\lambda} + \sigma(\Phi_{unaff}) + \alpha(\sigma(\Phi_{rec}) + \sigma(\Phi_{cov})) + \sigma(\Phi_{bun})]r(V_n^{OT} - V_N)]$$
$$= -\int_{\Phi_{rec}} 2\alpha F dG - \int_{\Phi_{cov}} \alpha C dG + \int_{\Phi_{bun}} (\bar{w} - w_0) - \frac{a^{-\frac{1}{\epsilon}}}{1+\frac{1}{\epsilon}}(h_2^{1+\frac{1}{\epsilon}} - h_0^{1+\frac{1}{\epsilon}})dG + \int_{\Phi_{emp}} \Delta V dG$$

where σ is a measure, $\Phi_{unaff} = \{(\theta, F)|w_0 \geq \bar{w} \text{ or } S_0 = -1\}$ is the set of jobs not directly affected by the overtime exemption threshold. Similarly, Φ_{rec} is the set of jobs that are reclassified, Φ_{cov} is the set of jobs that gain coverage, Φ_{bun} is the set of jobs that get bunched, and Φ_{emp} is the set of matches that become jobs in only one of the two scenarios. ΔV is the difference between the value of employment and unemployment. Abstracting away from the last term, if all workers are reclassified or gain coverage, then $V_n^{OT} - V_N < 0$ since workers do not value their pay classification but the added cost to the employer reduces workers' earnings. On the other hand, if all workers are bunched, then $V_n^{OT} - V_N$ is positive if and only if workers value the increase in earnings more than the loss in leisure. To simplify the subsequent discussion, I assume $V_n^{OT} - V_n = 0$, though the predictions hold for a wider range of values.

To determine whether an interior or corner solution solves the Nash bargaining problem for a given (θ, F, a), I compare the Nash products between the two solutions.

$$NP = \frac{1}{r+\delta}[(1+\eta)w_2 - a^{-\frac{1}{\epsilon}}\frac{h_2^{1+\frac{1}{\epsilon}}}{1+\frac{1}{\epsilon}} - rV_n^{OT}]^{\alpha}\Big[\theta h_2^{\beta} - (1+\eta)w_2 + F_S - C_{1[S=1,w<\bar{w}]}\Big]^{1-\alpha}$$

Without a closed form for h_2 in the corner solution, one cannot directly compare the two quantities. However, I can predict when the corner solution is more likely to be the optimum given the baseline income w_0. Notice that at the corner solution, the Nash product simplifies to

$$NP_{corner} = \frac{1}{r+\delta}[\bar{w} - a^{-\frac{1}{\epsilon}}\frac{h_2^{1+\frac{1}{\epsilon}}}{1+\frac{1}{\epsilon}} - rV_n^{OT}]^{\alpha}\Big[\theta h_2^{\beta} - \bar{w} + F\Big]^{1-\alpha}$$

From the first order condition that determines h_2, I know that the optimal hours with monitoring costs approaches the optimal hours in the baseline case as the baseline income approaches the overtime threshold (i.e. $\lim_{w_0 \to \bar{w}} h_2 = h_0$). Assuming $V_n^{OT} = V_n$, this implies that $NP_{corner} \to NP_0$ as $w_0 \to \bar{w}$, where NP_0 is the Nash product in the baseline case. Since $NP_0 \geq NP$ for every (θ, F, a), it is the case that for each (θ, F, a), there exists a ε such that

$NP_{corner} \geq NP_{interior}$ for every $w_0 \in \{w_0 | \bar{w} - w_0 < \varepsilon\}$.[53] In other words, jobs initially paying close to the threshold are more likely to be bunched.

Given the Nash bargaining solutions, do more or fewer matches get accepted and become employment relative to the benchmark case without overtime? Recall that in the benchmark case, given a (a, F), all matches with quality greater than or equal to $\theta_0^*(a, F)$ are accepted. Furthermore, the total surplus at θ_0^* equaled zero:

$$\theta_0^* h_0^\beta + F \cdot \text{sgn}(S_0) - a^{-\frac{1}{\epsilon}} \frac{h_0^{1+\frac{1}{\epsilon}}}{1+\frac{1}{\epsilon}} - rV_n = 0$$

where $h_0 = \left(a^{\frac{1}{\epsilon}} \beta \theta_0^*\right)^{\frac{1}{1+\frac{1}{\epsilon}-\beta}}$ and S_0 maximizes the surplus. Given parameters (θ_0^*, a, F) such that $S_0 = 1$ and $w < \bar{w}$, consider how the total surplus with overtime and monitoring costs (TS_{OT}) compare to the total surplus in the benchmark case $(TS_0 = 0)$:

1. If the job is reclassified, then $TS_{OT} = TS_0 - 2F - r(V_n^{OT} - V_n)$
2. If the job remains salaried but not bunched, then $TS_{OT} = TS_0 - C - r(V_n^{OT} - V_n)$
3. If the job is bunched, then $TS_{OT} = TS_0 + \theta_0^*(h_2^\beta - h_0^\beta) - a^{-\frac{1}{\epsilon}} (\frac{h_2^{1+\frac{1}{\epsilon}}}{1+\frac{1}{\epsilon}} - \frac{h_0^{1+\frac{1}{\epsilon}}}{1+\frac{1}{\epsilon}}) - r(V_n^{OT} - V_n)$

If $r(V_n^{OT} - V_n) \geq 0$, then it must be that $TS > TS_0$.[54] A negative match surplus implies that if the job is agreed upon, then either the firm will receive negative profits or the value of searching exceeds the value of the job. Thus, jobs with negative surplus are not accepted. Since the total surplus is monotonically increasing with respect to θ, for each (a, F), there exists a $\hat{\theta} > \theta_0^*$ such that matches are accepted if and only if $\theta \geq \hat{\theta}$.

Case 3: Downward Nominal Wage Rigidity

For the case with downward nominal wage rigidity, it is instructive to study the effects of imposing overtime on hourly and salaried workers in sequence.[55] First, suppose all hourly workers are covered for overtime, and firms cannot offer a wage, $\frac{w}{h}$, lower than in the benchmark case. The Nash bargaining problem is

[53] If $V_n^{OT} \neq V_n$, then $NP_{corner} \to NP_{max}$ as $w_0 \to \bar{w} - (1-\alpha) r(V_n^{OT} - V_n)$, where $NP_{max} \geq NP$ for every every (θ, F, a). NP_{max} is the solution to the benchmark Nash bargaining problem with no monitoring cost, assuming a search value of V_n^{OT}.

[54] The inequality is true for the third line since h_0 maximizes $\theta h^\beta - a^{-\frac{1}{\epsilon}} \frac{h^{1+\frac{1}{\epsilon}}}{1+\frac{1}{\epsilon}}$.

[55] In contrast to recent methods developed in the search literature to generate wage rigidity (see Rogerson and Shimer (2011) for review), I abstract from modeling the cause of wage rigidity and focus specifically on its effects by exogenously imposing that $\frac{w}{h} \geq \frac{w_0}{h_0}$ for hourly workers and $w \geq w_0$ for salaried workers.

$$(w_3, h_3, S_3) = \arg \max_{(w,h,S)} [V_e(w,h) - V_n]^\alpha \Big[\frac{\theta h^\beta - (1 + \eta_{(w,h,S)})w + F \cdot \text{sgn}(S)}{r + \delta}\Big]^{1-\alpha}$$

where

$$\eta_{(w,h,S)} = \begin{cases} \frac{0.5(h-40)}{40} & \text{if } h > 40 \text{ and } S = -1 \\ 0 & \text{otherwise} \end{cases} \tag{14}$$

and $\frac{w}{h} \geq \frac{w_0}{h_0}$ if $S = -1$.

Since the overtime policy only affects hourly workers working above 40 hours, it has no effect on the job characteristics of matches (θ, F, a) that do not meet this criteria in the benchmark case, aside from the indirect effect of changes to the value of continued search, V_n. For matches where $S_0 = -1$ and $h_0 > 40$, the bargaining solution can either adjust along the hours margin (interior solution) or the salaried/hourly margin (corner solution).

The corner solution sets $S_3 = 1$, and solves for (w_3, h_3). The solution to the Nash bargaining problem is similar to that of case 1:

$$w_3 = \frac{1}{1 + \eta_{(w,h,S)}} \Big[\alpha\Big(\theta h_3^\beta - F\Big) + (1-\alpha)\Big(a^{-\frac{1}{\epsilon}} \frac{h_3^{1+\frac{1}{\epsilon}}}{1 + \frac{1}{\epsilon}} + rV_n\Big)\Big]$$
$$h_3 = \Big(a^{\frac{1}{\epsilon}} \beta\theta\Big)^{\frac{1}{1+\frac{1}{\epsilon}-\beta}}$$

This solution is optimal for matches where the relative cost to being salaried, F, is small.

To solve for the interior solution of (w_3, h_3, S_3), substitute $w = \frac{w_0}{h_0} h$ into the Nash bargaining problem and take first order conditions with respect to h:

$$FOC_h = \alpha(1.5\frac{w_0}{h_0} - a^{-\frac{1}{\epsilon}} h^{\frac{1}{\epsilon}})[\theta h^\beta - (1.5 - \frac{20}{h})\frac{w_0}{h_0} h - F_S]$$
$$+ (1-\alpha)(\theta\beta h^{\beta-1} - 1.5\frac{w_0}{h_0})[(1.5 - \frac{20}{h})\frac{w_0}{h_0} h - a^{-\frac{1}{\epsilon}} \frac{h_0^{1+\frac{1}{\epsilon}}}{1 + \frac{1}{\epsilon}} - rV_n]$$

Although there is no closed form solution for h_3, I can determine whether h_3 is larger or smaller relative to the case without overtime by evaluating the first order condition at h_0, recalling that $a^{-\frac{1}{\epsilon}} h_0^{\frac{1}{\epsilon}} = \theta\beta h_0^{\beta-1}$:

$$FOC_h|_{h=h_0} = \underbrace{(1.5\frac{w_0}{h_0} - \theta\beta h_0^{\beta-1})}_{>0 \text{ if } \beta<1.5\alpha} \underbrace{(\frac{20}{h_0} - 0.5)}_{<0 \text{ since } h_0>40} \underbrace{w_0}_{>0}$$

Substituting in w_0, I show that $\beta < 1.5\alpha$ is a sufficient condition for the first order condition to be negative. Intuitively, the overtime premium raises the income of the worker, so the firm is able to demand longer hours. However, if the worker receives a sufficiently large portion of the surplus relative to the gain in production, then the firm would rather decrease hours to reduce costs.

Assuming that $\beta < 1.5\alpha$, the model predicts a spike at 40 hours per week among hourly workers. Since this model relies on wages being downward rigid, one might expect there to be a higher propensity to bunch among minimum wage workers. In addition, the model predicts that some jobs will be reclassified from hourly to salaried. Following the same argument in case 2, some matches will also fail to become employment.

Next, I extend overtime coverage to salaried workers with weekly base incomes less than an overtime exemption threshold $\bar{w}$. I introduce "wage" rigidity by restricting salaried workers' weekly base pay to be greater than or equal to the agreed upon amount in the benchmark scenario (i.e. $w_3 \geq w_0$). Following the same argument as above, weekly hours decreases for covered salaried workers if $\beta < 1.5\alpha$. Similar to the argument in case 2, the Nash product for individuals initially earning just below the threshold ($\bar{w} - w_0$ is small) is maximized by bunching their weekly income at the threshold. Without a fixed monitoring cost though, the bunching in hour and weekly income only affects salaried workers who are initially working over 40 hours per week ($h_0 > 40$). If reducing hours or increasing base income results in a negative surplus, the job is dissolved.

In contrast to case 2, there are no longer any incentives to reclassify workers who are salaried in the benchmark case ($S_0 = 1$) since the costs of overtime are the same regardless of their classification. However, given that salaried workers gain coverage after hourly workers, the comparative statics should be made relative to the scenario where only hourly workers are eligible for overtime. In that case, workers who are salaried as a result of the asymmetric overtime costs ($S_0 = -1$ and $S_3 = 1$) will be reclassified back to hourly status with shorter hours, or bunched at the threshold, or unemployed. I expect to observe this reclassification effect only for workers earning below the overtime exemption threshold. The asymmetric overtime costs for those above the threshold still incentivizes firms to reclassify hourly workers as salaried.

Appendix E. Defining the Compensation Variables

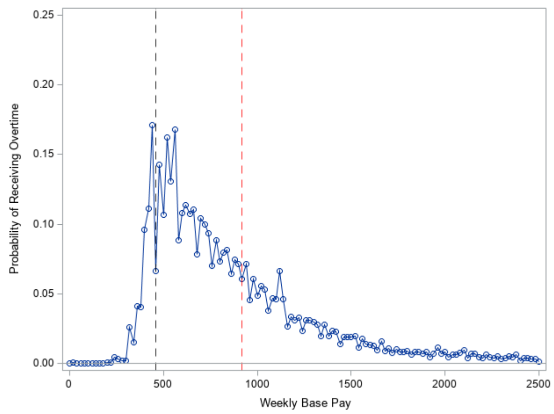
E.1 Overtime Pay

In this subsection, I present the procedure I use to determine each individual's overtime pay from the "OT earnings" variable and its corresponding hours, when available. There are two challenges to inferring workers' overtime pay from the ADP data. First, firms are not required to input a value into the "OT earnings" field. Although the ADP data contains four separate earnings variables and four corresponding hours variables, each capturing a different component of gross compensation, firms are only required to report employees' gross pay and standard rate of pay. Thus, it is uncertain whether a missing value for overtime earnings means that the firm does not record the value or the worker did not receive any overtime pay. To test how often firms separately record workers' overtime pay, I compare the probability that a worker receives overtime pay in the ADP data to the probability that a worker works overtime in the Current Population Survey (CPS). In the ADP data, I find that the overtime earnings variable is non-zero for 45% of hourly workers and 3.5% of salaried workers in April 2016. For the same month, only 19% of hourly workers in the CPS report working over 40 hours in the previous week, and 15% report usual weekly hours exceeding 40. Assuming that 15% of hourly employees always work overtime, and 4% work overtime one week per month, I would expect around 31% of hourly employees to receive positive overtime compensation per month. Given that this is even smaller than the probability of overtime pay in ADP, it is likely that most firms separately record overtime pay from gross pay.

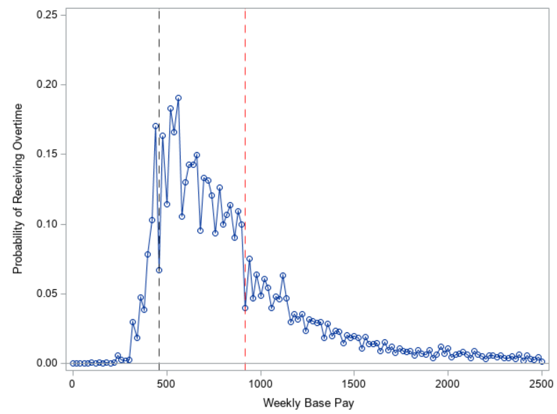
The second challenge with measuring workers' overtime pay is that the type of compensation included into the "OT earnings" variable is at the discretion of the firm. Thus, some employers may use the variable to record other forms of compensation than overtime pay. I impute overtime pay following the methodology described by Grigsby, Hurst and Yildirmaz (2020). First, I define an implied overtime wage as the ratio between the "OT earnings" and "OT hours" variables. Next, I divide the implied wage by workers' actual wage to compute an implied overtime premium (i.e. $\frac{\text{OT earnings}}{\text{OT hours}*\text{wage}}$), where a salaried worker's "wage" for overtime purposes is defined by the Department of Labor as $\frac{\text{weekly base pay}}{40}$. I consider the "OT earnings" variable to represent true overtime pay if the implied overtime premium is less than or equal to 2. I find that the distribution of the implied overtime premium exhibits significant bunching at 1.5, and 2, indicating that the variable usually captures true overtime earnings. Among workers with non-missing "OT earnings", 75% of hourly workers and 79% of salaried workers have implied overtime premiums within 1.4-1.6 and 1.9-2.1.

To validate my measure of overtime for salaried workers, I plot in figure E.1 the probability that a salaried worker receives overtime as a function of their weekly base pay. Consistent with compliance with the overtime regulation, and potentially selection into bunching, salaried workers earning less than the overtime exemption are far more likely to receive overtime pay compared to those earning above it. Furthermore, the probability of receiving overtime in FLSA states in December 2016, and California and New York in April 2016, exhibits a discontinuous drop in at exactly the threshold.

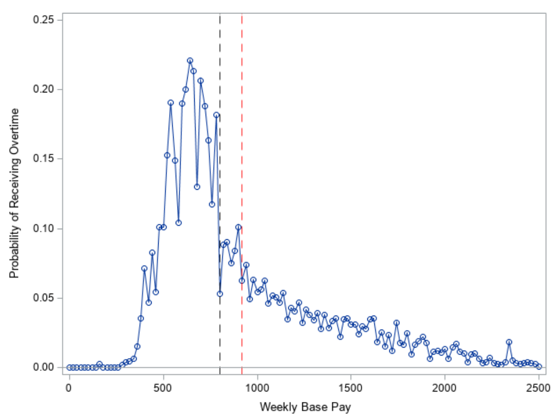
Appendix Figure E.1: Probability of Receiving Overtime Pay, Conditional on Base Pay



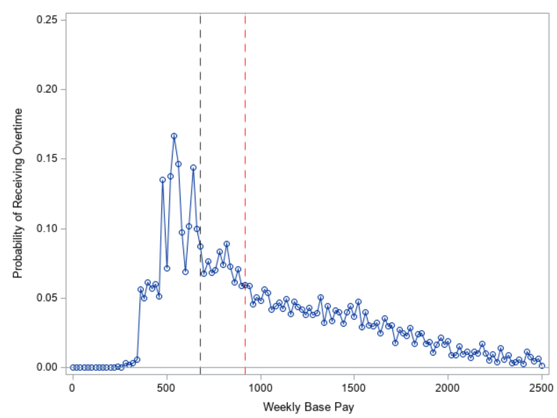
(a) FLSA states, April 2016



(b) FLSA states, Dec 2016



(c) California, April 2016



(d) New York, April 2016

Notes: Each graph shows the probability that salaried workers receive non-zero overtime pay in the month, as a function of their weekly base pay. The sample in figure (a) is restricted to salaried workers not living California, New York or Alaska, in the month April 2016. The sample in figure (b) is restricted to salaried workers in the same states as figure (a) in December 2016. The sample in figure (c) is restricted to salaried workers in California in April 2016. The sample in figure (d) is restricted to salaried workers in New York in April 2016.

E.2 Computing Weekly Measure of Income

While the measure of base pay that the Department of Labor uses to determine overtime eligibility is denominated at the weekly level, workers' gross pay and overtime pay are recorded at the monthly level in the data. In this section, I explain the procedure I use to standardize these two key measures of compensation to the weekly level. Table E.1 shows the share of workers with each pay frequency in April 2016, and the formula used to compute their weekly base pay, gross pay, and overtime pay.

Appendix Table E.1: Normalizing Compensation to Weekly Level, by Pay Frequency

	Share of Workers			
Pay Frequency	Hourly	Salaried	Base Pay	Gross/Overtime Pay
Weekly	0.24	0.06	S	$\frac{1}{N}Y$
Biweekly	0.66	0.53	$\frac{1}{2}S$	$\frac{1}{2N}Y$
Semimonthly	0.09	0.35	$\frac{24}{52}S$	$\frac{12}{52}Y$
Monthly	0.01	0.06	$\frac{12}{52}S$	$\frac{12}{52}Y$
All workers	0.66	0.34		

Notes: The first column shows the four frequencies at which individuals can receive their paycheck. Columns 2 and 3 show the share of hourly and salaried workers with each pay frequency, respectively, in April 2016 who are paid according to each pay frequency. Column 4 shows the formula to normalize salaried workers' standard rate of pay, denoted by S, to weekly base pay for each pay frequency. Column 5 shows the formula to normalize monthly gross pay and overtime pay, denoted by Y, to an average weekly gross pay conditional on receiving N paychecks in the month.

To derive workers' weekly base pay from their standard rate of pay, I follow the rules set by the Department of Labor and scale each worker's standard rate of pay by their pay frequency (i.e. $\frac{\text{standard pay}}{\text{week}} = \frac{\text{standard pay}}{\text{paycheck}} \cdot \frac{\text{paycheck}}{\text{weeks}}$). For workers paid weekly or biweekly, I simply multiply the standard rate of pay by 1 and 0.5, respectively, to compute their weekly base pay. For workers paid semimonthly or monthly, the DOL's formula makes the approximation that each month is 1/12 of the year and each year has 52 weeks. Thus, weekly base pay equals standard rate of pay times $\frac{24}{52}$ for workers paid semimonthly, and standard rate of pay times $\frac{12}{52}$ for workers paid monthly.

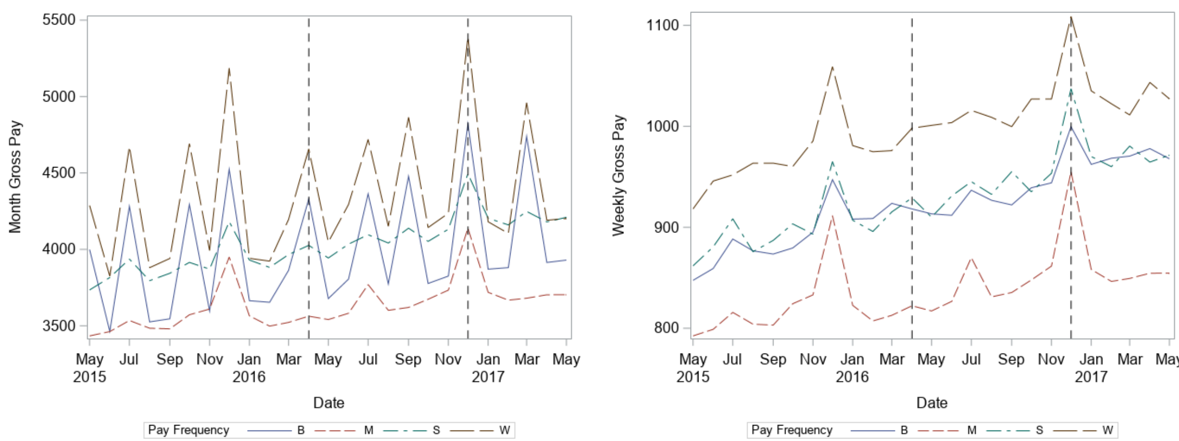
To express the monthly gross and overtime pay variables at the weekly level, I normalize it by the number of paychecks they receive each month and the number of weeks covered per paycheck:

$$\frac{\text{gross pay}}{\text{week}} = \frac{\text{gross pay}}{\text{month}} \Big/ \left(\frac{\text{paychecks}}{\text{month}} \cdot \frac{\text{weeks}}{\text{paycheck}}\right)$$

This scaling calculation is simple to compute for observations after 2016 since I observe the number of paychecks per month, and the term $\frac{\text{paycheck}}{\text{weeks}}$ is equivalent to the scaling factor used to translate the standard rate of pay to weekly base pay. For observations prior to 2016 though, I have to impute the number of paychecks per month.

I define $\frac{\text{paychecks}}{\text{month}} = 1$ for workers paid monthly and $\frac{\text{paychecks}}{\text{month}} = 2$ for workers paid semi-monthly. For weekly and biweekly paid workers, the number of paychecks received each month depends on both the day of the week that each worker gets paid, and the number of times that day appears in the month. For instance, if a worker gets paid on a Thursday every two weeks, then the worker's gross pay includes 3 paychecks in December 2016 when there were 5 Thursdays, but only 2 paychecks in April 2016. To illustrate this problem, I plot in figure E.2a the monthly gross pay for a balanced panel of workers who earn between \$455 and \$913 base pay in April 2016, by their pay frequency. Not only do biweekly and weekly paid workers experience spikes in their gross pay, the peaks and troughs do not occur on the same months between years. In contrast, monthly and semi-monthly paid workers only experience a large spike in December of each year, likely reflecting bonuses.

Appendix Figure E.2: Gross Income, by Pay Frequency



(a) Monthly Gross Pay

(b) Normalized Weekly Gross Pay

Notes: Panel (a) shows the average monthly gross pay for a balanced panel of workers who earned between \$455 and \$913 per week in April 2016. The pay frequencies from left to right are biweekly, monthly, semi-monthly, and weekly. Panel (b) shows the average weekly gross pay for the same panel of workers.

While different workers may receive an extra paycheck in different months, employees of the same firm tend to receive a paycheck on the same day of the month, conditional on their pay frequency. To impute the number of paychecks per month that each firm issues in a month, I apply the following algorithm:

1. Compute the average gross pay across all workers of the same pay frequency within each firm-month.

2. Within each year, for each firm-frequency, compute the median of the average gross pays across the 12 months.

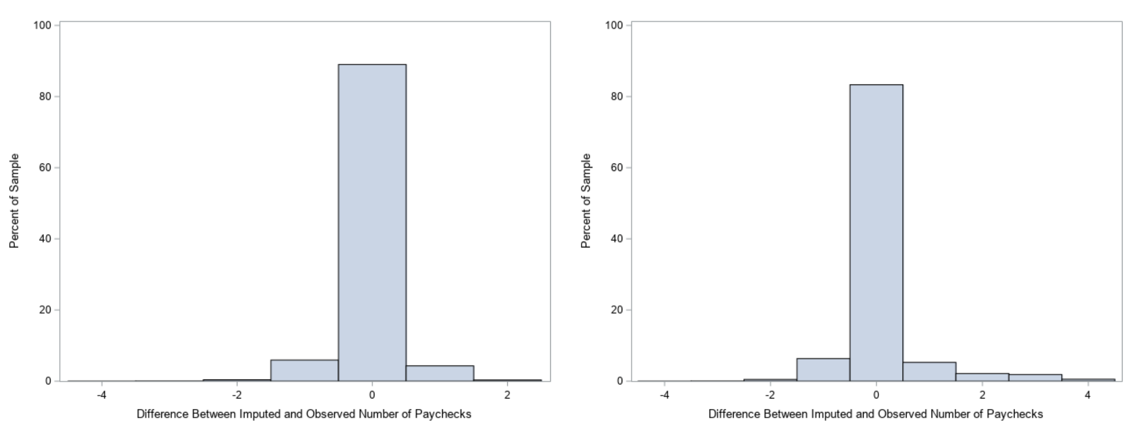
3. I record biweekly workers as receiving 3 paychecks in months where the average gross pay in their firm-frequency exceeds 1.2 times the firm's median gross pay in that year, and 2 otherwise.

4. I record weekly workers as receiving 5 paychecks in months where the average gross pay in their firm-frequency exceeds 1.075 time the firm's median gross pay in that year, and 4 otherwise.

By computing the number of paychecks at the firm level, I can impute the number of paychecks received by newly employed workers. Plotting workers' gross pay, scaled to a weekly level using their imputed number of paychecks, I show in figure E.2b that the periodic spikes in gross pay among biweekly and weekly paid workers disappear.

To validate the imputation, I compare the imputed number of paychecks per month to the actual number of paychecks per month using data post-2016 (see figure E.3). I find that I am able to match the actual number of paychecks for nearly 90% of biweekly paid worker-months and 80% of weekly paid worker-months.

Appendix Figure E.3: Impute Number of Pay Checks, by Pay Frequency



(a) Biweekly (b) Weekly

Notes: Panel (a) shows distribution of the difference between imputed and actual number of paychecks per month, for all worker-months in 2016 where the worker is paid biweekly. Panel (b) shows a similar distribution for workers who are paid weekly.

Appendix F. Derivation of Estimators

F.1 Derivation of Equation 3

If the coefficients in equation 2 satisfy

$$\beta_{jk} = 0 \text{ for every } k \geq k^*$$

$$\alpha_{jkt} = \gamma_1 \alpha_{jk,t-1} + \gamma_0$$

then for every $k < k^*$, an unbiased estimator of β is

$$\begin{aligned}\hat{\beta}_{jk} &= \left(\bar{N}_{jk,Dec,t} - \bar{N}_{jk,Apr,t}\right) - \hat{\gamma}_1\left(\bar{N}_{jk,Dec,t-1} - \bar{N}_{jk,Apr,t-1}\right) - \hat{\gamma}_0 \\ &= \Delta\bar{N}_{jkt} - \hat{\gamma}_1 \Delta\bar{N}_{jk,t-1} - \hat{\gamma}_0\end{aligned}$$

where $\bar{N}_{jkmt}$ is the average N_{ijkmt} across all firms, and $\hat{\gamma}_1$ and $\hat{\gamma}_0$ are estimated using all salaried workers in bins $k \geq k^*$ from

$$\Delta\bar{N}_{sal,kt} = \gamma_1 \Delta\bar{N}_{sal,k,t-1} + \gamma_0 + \epsilon_{sal,kt}$$

Proof. For every $k \geq k^*$,

$$\begin{aligned} \bar{N}_{jk,Dec,t} &= \bar{N}_{jk,Apr,t} + \alpha_{jkt} \\ \Rightarrow \quad \Delta\bar{N}_{jkt} &= \alpha_{jkt} \\ \Rightarrow \quad \Delta\bar{N}_{jkt} &= \gamma_1 \alpha_{jk,t-1} + \gamma_0 \\ \Rightarrow \quad \Delta\bar{N}_{jkt} &= \gamma_1 \Delta\bar{N}_{jk,t-1} + \gamma_0 \end{aligned}$$

This implies that I can estimate γ_1 and γ_0 by regressing $\Delta\bar{N}_{sal,kt}$ on $\Delta\bar{N}_{sal,k,t-1}$ using all bins $k \geq k^*$. Given the γ's, I can then predict the α_{jkt}'s for both salaried and hourly workers with bins $k < k^*$.

$$\hat{\alpha}_{jtk} = \hat{\gamma}_1 \Delta\bar{N}_{jk,t-1} + \hat{\gamma}_0$$

From equation 2, I estimate the β_{jk}'s as the difference between $\Delta\bar{N}_{jkt}$ and $\hat{\alpha}_{jkt}$. ∎

F.2 Identifying Assumptions for Estimating the Causal Effect on Always-Salaried Workers

Consider the sample of incumbent workers who were salaried in April 2016. Let N^j_{Dec} and N^j_{Apr} be the number of these workers in bin of base pay j, on December and April, respectively. In this case, N^j sums over both incumbent salaried and hourly workers with base pay in bin j. However, by construction, the workers in April are all salaried. The difference in the number of workers between these two months can be decomposed as follows:

$$N^j_{Dec} - N^j_{Apr} = \underbrace{N^{kj}_{S_0,S_1} - N^{jk}_{S_0,S_1}}_{\text{Within Classification } (\Delta N_{S_0,S_1})} + \underbrace{N^{kj}_{S_0,H_1} - N^{jk}_{S_0,H_1}}_{\text{Reclassifications } (\Delta N_{S_0,H_1})} - \underbrace{N_{S_0,u_1}}_{\text{Separations}} \tag{15}$$

where the N_{x_0,y_1} denotes the number of workers with status x in April and status y in December. The three statuses are S for salaried, H for hourly, and u for unemployed. The superscript kj denote flows from bin k to bin j, and vice versa for jk superscript.

To identify the effect of the 2016 FLSA policy on within classification flows (i.e. workers who stay salaried in April and December), I use a scalar transformation of the within classification flows in 2015. In other words,

$$\begin{aligned} E[\Delta N_{16,S_0,S_1}] - \gamma E[\Delta N_{15,S_0,S_1}] &= E[\Delta N^T_{16,S_0,S_1}] - E[\Delta N^C_{16,S_0,S_1}] \\ &\quad + (E[\Delta N^C_{16,S_0,S_1}] - \gamma E[\Delta N^C_{15,S_0,S_1}]) \end{aligned}$$

where the superscripts T and C refer to whether the policy passed (T) or it is the number of people in the counterfactual (C). For an unbiased estimator of the causal effect, I need the selection bias in the brackets to equal zero. I next present conditions where that would hold. Substituting in equation 15 into the selection bias term:

$$\begin{aligned} E[\Delta N^C_{16,S_0,S_1}] - \gamma E[\Delta N^C_{15,S_0,S_1}\Big] &= E\Big[\Delta N^C_{16,S_0}\Big] - \gamma E\Big[\Delta N^C_{15,S_0}\Big] && \text{(All Incumbents)} \\ &\quad - (E\Big[\Delta N^C_{16,S_0,H^T_1}\Big] - \gamma E\Big[\Delta N^C_{15,S_0,H^C_1}\Big]) && \text{(Reclassifications)} \\ &\quad + (E\Big[N^C_{16,S_0,u^T_1}\Big] - \gamma E\Big[N^C_{15,S_0,u^C_1}\Big]) && \text{(Separations)} \end{aligned}$$

Given the assumptions in section 5.1, the control group is a reasonable counterfactual for the change in the total number of workers across the base pay distribution: $E\Big[\Delta N^C_{16}\Big] - \gamma E\Big[\Delta N^C_{15}\Big] = 0$. I assume that under the same assumptions, the control group is also a reasonable control for the change in the number of incumbents across the distribution: $E\Big[\Delta N^C_{16,S_0}\Big] - \gamma E\Big[\Delta N^C_{15,S_0}\Big] = 0$. This eliminates the first component of the selection bias.

To remove the selection bias from separations, I assume that the policy had no effect on separations. This appears reasonable from the analysis in section 7 (see figure 11a). In that case, the observed number of separations in 2016 would be equal to the counterfactual number of separations:

$$\begin{aligned} E\Big[N^C_{16,S_0,u^T_1}\Big] - \gamma E\Big[N^C_{15,S_0,u^C_1}\Big] &= E\Big[N^C_{16,S_0,u^C_1}\Big] - \gamma E\Big[N^C_{15,S_0,u^C_1}\Big] \\ &= 0 \end{aligned}$$

where the second line follows from the assumption that 2015 is a good counterfactual of 2016.

To remove the selection bias from reclassifications, I assume that the policy had no effect on the distribution of base pay among reclassified workers relative to the counterfactual. This appears reasonable given the analysis in section 7. Workers reclassified as a result of the policy earned a similar base pay pre-and-post policy (see figure 10c). Given that the policy tended to raise workers' salaries, the fact that these workers' base pay did not rise suggest that they would also have not experienced a large increase in base pay in the absence of the policy. If this holds, then:

$$\begin{aligned} E\Big[\Delta N^C_{16,S_0,H^T_1}\Big] - \gamma E\Big[\Delta N^C_{15,S_0,H^C_1}\Big] &= E\Big[\Delta N^C_{16,S_0,H^C_1}\Big] - \gamma E\Big[\Delta N^C_{15,S_0,H^C_1}\Big] \\ &= 0 \end{aligned}$$

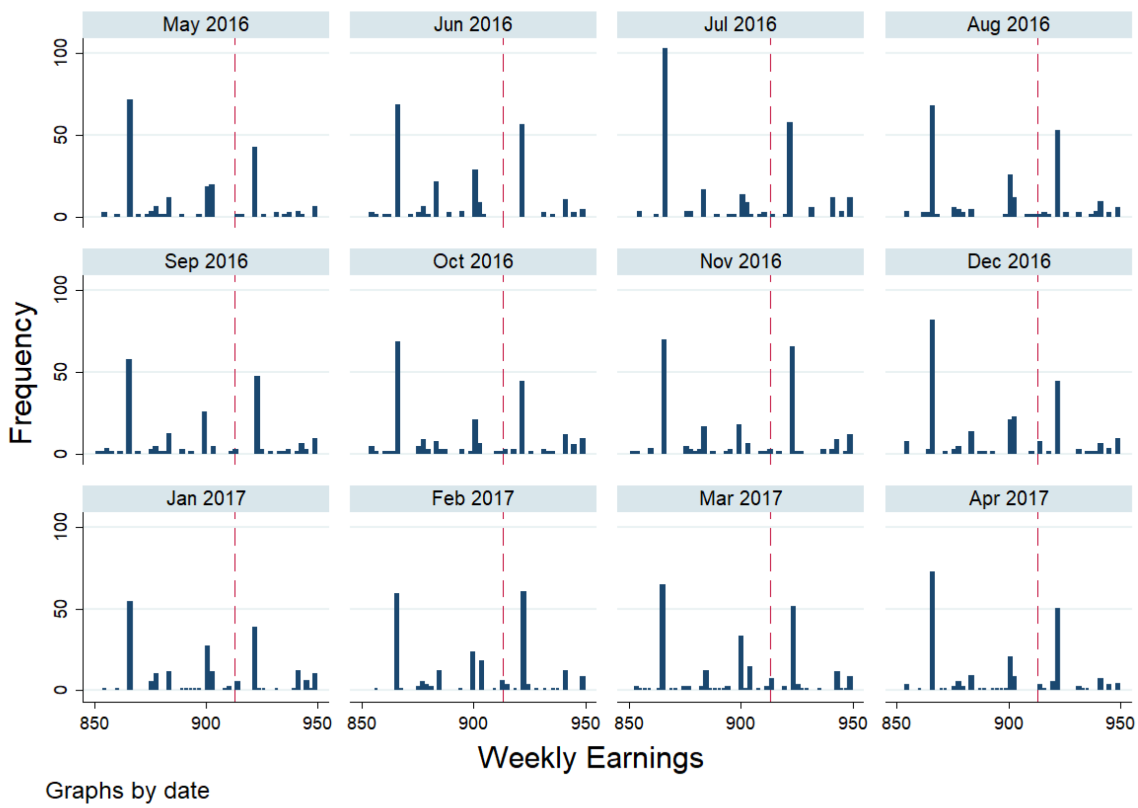
where the second line again follows from the assumption that 2015 is a good counterfactual of 2016.

If all these assumptions hold, then $\gamma E[\Delta N^C_{15,S_0,S_1}\Big]$ is an unbiased estimator of the effect of the policy on the distribution of always-salaried workers.

Appendix G. Analysis using the Current Population Survey

There are many advantages of the ADP data over traditional survey data. Foremost for the purposes of studying the overtime exemption policy is that it records workers' base salaries without measurement error, for a very large sample of workers. These features make it possible to compare the distribution of salaries over time with minimal concern that the differences are driven by measurement error or changes in the sample population. A limitation of the ADP data though is that it does not record the hours worked by salaried workers. Hence, a natural response would be to supplement the main analysis by using survey data, such as the Current Population Survey (CPS), to estimate the effect of raising the overtime exemption threshold on workers' weekly hours. However, I show that the CPS is unable to even pick up the clear bunching and reclassifications effects identified from the ADP data.

Appendix Figure G.1: Frequency Distribution of Salaried Workers in \$2 Bins of Weekly Earnings, by Date



Notes: This figure shows the frequency distribution of respondents' usual weekly earnings in the CPS. The sample is restricted to individuals who are not paid an hourly wage, and earn between \$851 and \$950 per week. The dotted vertical red line is at \$913 per week.

To begin, I plot the frequency distributions of weekly earnings of salaried workers for each month between May 2016 and April 2017 in figure G.1. The number of respondents earning within a dollar of $913 per week experiences a visibly small jump between November and December 2016 that persists after December. In the year prior to December 2016, 0.09% of salaried workers report earning within a dollar of $913, whereas in the year after, 0.37% report earning within that interval. However, the "bunching" at the threshold is considerably smaller than the other spikes in the distribution.

Replicating figures 2a and 2b, I try to isolate the dip and bunching by taking the difference in the earnings distributions before and after the policy. Given that there are on average only 4,470 salaried workers surveyed per month, I construct the post-policy distribution by pooling all observations between December 2016 and April 2017, and the pre-policy distribution using all observations in the analogous months in the previous year. The two distributions, overlaid in figure G.2 look very similar. Furthermore, the difference between the distributions do not exhibit the clear dip and bunching observed using the ADP data. While there is a drop in the number of salaried workers earning between $455 and $912 and an increase in the number of workers earning exactly $913 from 2015 to 2016, the same is also true from 2014 to 2015. Overall, I am unable to find definitive evidence of large bunching using the CPS data.

The absence of bunching in the CPS data may be attributed to measurement error in the weekly earnings variable. For example, respondents may tend to round their reported earnings to the nearest $1000 annual income or $100 weekly income. Alternatively, when asked their "usual" weekly earnings, respondents may report their most common weekly earnings over the past year, rather than their weekly earnings in the month that they are surveyed. Given these concerns over measurement error in reported earnings, the CPS may be more suited to identifying reclassification effects.

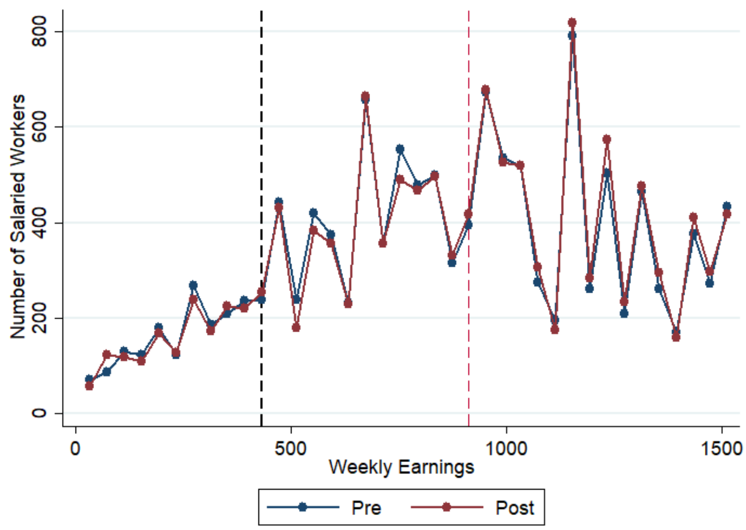
In figure G.3, I plot the proportion of respondents earning who report being paid per hour. I find no visible evidence of a trend break in the probability of hourly status between May 2016 and December 2016 for those earning between $400 and $1000 per week. To control for time-specific effects, I estimate a difference-in-difference where I assume that the proportion of hourly workers among those earning between $1000 and $1200 per week follows the same trend as those earning between $400 and $1000 per week. I do not find any effect of the policy on the share of hourly workers under this specification.

One concern with restricting the sample within each cross-section to only workers who earn between $400 and $1000 per week is that the policy might affect the selection of workers into this sample. To address this issue, I leverage the panel structure of the CPS data to identify the change within-worker over one year. First, I restrict the sample to workers who, in their first survey, report being non-hourly, and earning between \$455 and \$913. Given the timing of the 2016 FLSA policy, there should be a jump in the share of hourly workers among those who completed their second survey between December 2016 and February 2016. While figure G.4 shows no trend break in the share of workers who transition to hourly status in December 2016, I do find a large jump in hourly workers among the September to November 2016 respondents. Comparing salaried workers initially earning between \$455 and \$913 per week to salaried workers initially earning between \$913 and \$1200, I find no statistically significant differences in their probabilities of becoming hourly in December 2016. However, the confidence intervals are very large such that I cannot rule out the estimate in the main text that 10% of workers were reclassified. While not reported, I also find no earnings effect from the cohort-by-cohort difference-in-difference. These observations are inconsistent with the results from the main analysis using administrative payroll data.

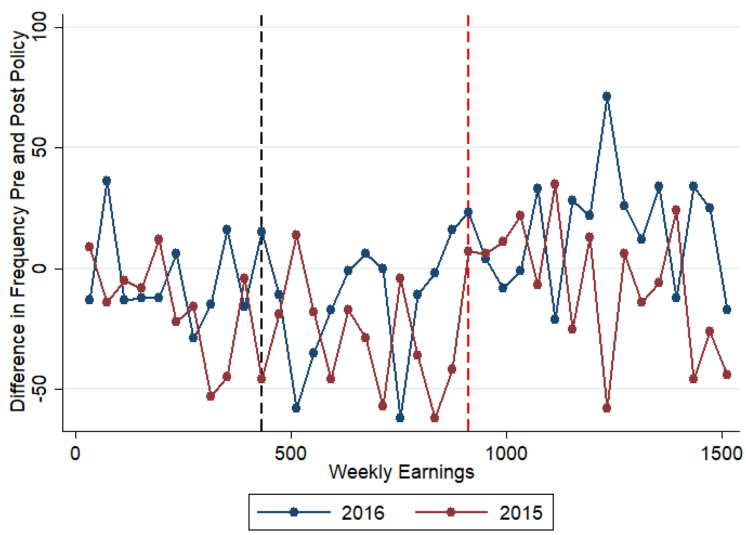
In summary, I am unable to replicate the key results found in the ADP data using the CPS, due to a combination of measurement error and small sample size. For instance, there are only 317 salaried workers not living in California or New York, with weekly earnings between \$455 and \$913 per week, who completed their first Outgoing Group Rotation Survey in April 2016. In comparison, as reported in table 4, there are 372,772 such workers in the ADP data. The small policy changes at the individual state level also do not offer me many more observations. Given that the CPS cannot identify the bunching or reclassification effects, it is not surprising that I also do not find any significant changes to weekly hours worked among salaried workers around the time of the policy.[56] Overall, the affected population in the CPS is simply too small to precisely study the effects of raising the overtime exemption threshold on the labor market.

[56]Graphs available upon request.

Appendix Figure G.2: Difference in Distribution of Salaried Workers Before and After Raising the OT Exemption Threshold, Using CPS



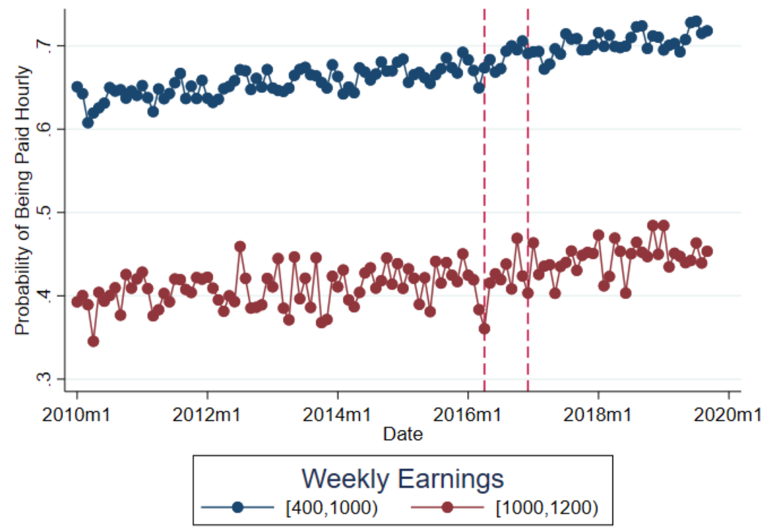
(a) Distribution of Salaried Workers' Weekly Earnings



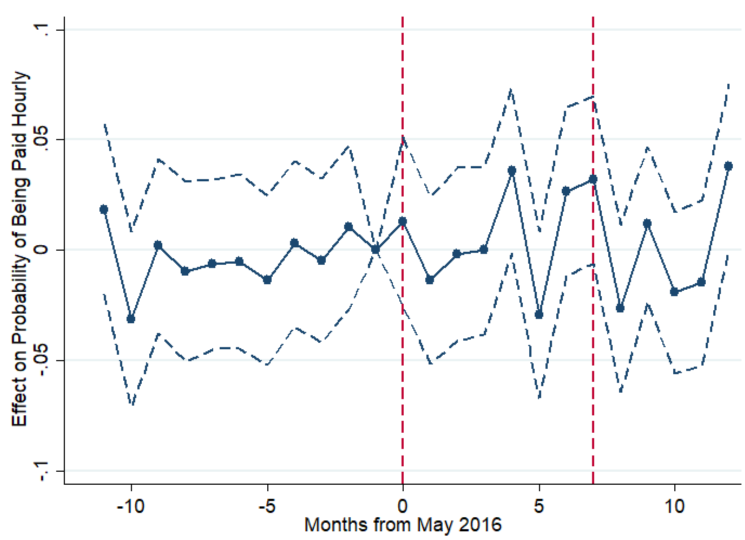
(b) Difference in Distribution Pre and Post Policy

Notes: Panel (a) shows the frequency distribution of salaried workers' weekly earnings in $40 bins, reported in the CPS. The distribution in the pre-period is constructed using all respondents between December 2015 and April 2016. The post-period is constructed using all respondents between December 2016 and April 2015. The "2016" line in panel (b) shows the difference between the pre and post distributions in panel (a). The "2015" line shows the difference between the pre-distribution and the analogous distribution of salaried workers from December 2014 and April 2015.

Appendix Figure G.3: Difference in Difference of Probability of Being Paid Hourly Using Repeated Cross Sections, Using CPS



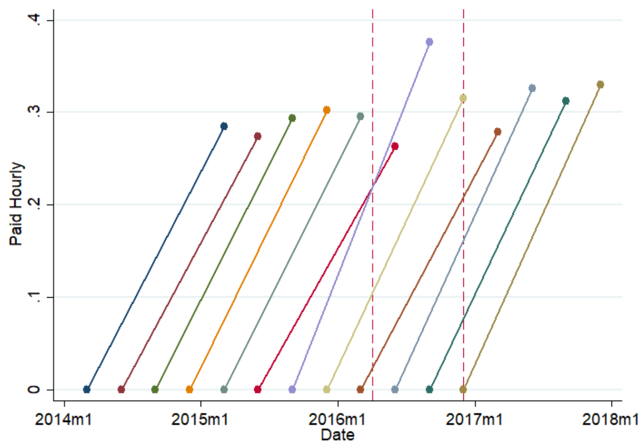
(a) Probability of Being Paid Hourly, by Date



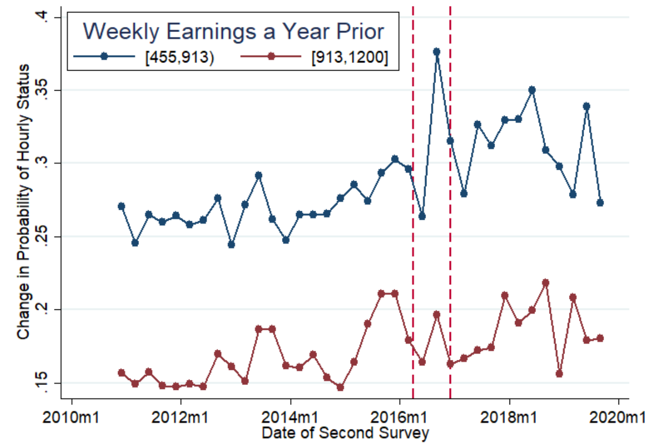
(b) Diff-in-diff for Hourly Status Indicator

Notes: Panel (a) shows the probability that an individual in the CPS is paid an hourly wage for each month between January 2010 and September 2019, conditional on weekly earnings. The two dotted vertical lines are at May 2016 and December 2016, respectively. Panel (b) shows the difference in difference estimates where I compare workers earnings earning between \$400 and \$1000 per week to workers earning between \$1000 and \$1200 per week.

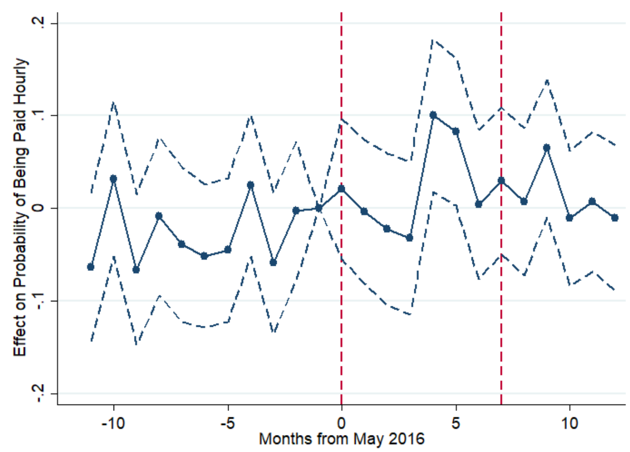
Appendix Figure G.4: Annual Change in Hourly/Salaried Status, Conditional on Initially Earning Between $455 and $913 per Week as a Salaried Worker



(a) Hourly Status, by Cohort



(b) Change in Hourly Status, by Earnings



(c) Difference-in-Difference of Hourly Status

Notes: In panel (a), the sample is restricted to workers who answered both outgoing rotation group surveys, and in their first CPS ORG survey, reported earning between $455 and $913 per week, and paid non-hourly. Each point represents the average response across all respondents in three consecutive surveys, starting with the month on the x-axis corresponding to that point. Each line connects the average response answered by the same panel of workers. In panel (b), the blue line is the difference between each pair of points in panel (a), plotted against the date of the second survey. The red line is the analogous graph for workers earning between $913 and $1200 in their first survey. Panel (c) plots the difference-in-difference estimates corresponding to the normalized difference between the two graphs in panel (b), computed using monthly data.