THE LABOR MARKET EFFECTS OF EXPANDING OVERTIME COVERAGE*

Simon Quach†

January 23, 2022

Abstract

Analysis of the impact of overtime regulations on the labor market has been hindered by a lack of high quality data that can take advantage of useful policy variation. This paper uses changes in state and federal salary thresholds for overtime coverage of salaried employees in the U.S. (2014-2020), in conjunction with high-frequency administrative payroll data, to empirically assess two leading predictions of the effects of overtime: 1) that firms will substitute away from hours per worker in favor of more workers, and 2) a "compensating differentials" mechanism whereby firms would neutralize the effect of overtime via reductions in base pay. The evidence is inconsistent with both views. Using a difference-in-difference design, I show that expansions in overtime coverage lead to a net loss in headcount. At the same time, firms raise base pays in order to keep workers exempt from the new overtime provisions. Compared to effects found from studies on the minimum wage, the gains in income from an expansion in overtime are small relative to the loss in employment. Moreover, the rise in income is largest for jobs paying near the new exemption threshold, whereas the employment loss is greater among lower paying jobs. As a result, expansions in overtime amplify, rather than reduce, inequality.

*I am extremely grateful to David S. Lee for his tremendous guidance and support on this project. I thank Alexandre Mas, Henry Farber, Felix Koenig, and the participants at the Industrial Relations Section labor seminar for their many helpful comments and suggestions. I also benefited from feedback by Stephen Trejo and the audiences at the 2021 NBER Summer Institute, University of Southern California, National University of Singapore, Queen's University, Boston College, Hong Kong University of Science and Technology, City University of Hong Kong, Federal Reserve Board, and Federal Reserve Bank of Dallas. I am indebted to Alan Krueger, Ahu Yildirmaz, and Sinem Buber Singh for facilitating access to ADP's payroll data, which I use in my analysis. The author is solely responsible for all errors and views expressed herein.

†University of Southern California, USC-Department of Economics 3620 S. Vermont Ave., KAP 300 Los Angeles, CA 90089-0253, simonqua@usc.edu

I Introduction

In nearly every OECD country, basic employment standards are regulated through at least two major rules: a minimum wage to set a hard floor on workers' earnings, and an overtime premium to set a soft ceiling on workers' hours (OECD, 2021; ILO, 2021). While interest in these policies have sparked a large literature on the economic impacts of the minimum wage (Autor et al., 2016; Harasztosi and Lindner, 2019; Cengiz et al., 2019), far less is known about the effects of overtime regulations. This is despite the fact that overtime may have as large an impact on the labor market. For instance, whereas only 2% of workers in the United States earn the federal minimum wage (BLS, 2019), over half the U.S. labor force are covered for overtime (U.S. Department of Labor, 2019a).[1] This broad coverage arguably leads to labor market distortions, perhaps the most apparent being the bunching of weekly work hours - nearly half of all respondents in the Current Population Survey report working exactly 40 hours per week.[2] In spite of behavioral responses by firms to avoid the overtime penalty, it nevertheless induces a large transfer from employers to workers. Each year, employers in the U.S. pay more in overtime compensation than they do in taxes to fund the entire unemployment insurance systems (U.S. Department of Labor, 2019b). This labor cost is expected to grow as a recent wave of policies have expanded overtime protection for salaried workers at both the state and federal levels.[3]

Despite the wide prevalence of overtime regulation in the United States, the relative infrequency of policy changes and the unavailability of data needed to accurately measure

[1]Even at its height in 1980, the federal minimum wage was binding for an estimated 15% of the labor force (BLS, 2019).

[2]Recent work using administrative payroll data of a selected sample of firms likewise finds that 12% of hourly workers are paid for exactly 40 hours per week.(Goff, 2020). As evidence that this is due to overtime regulation, Estevão et al. (2008) shows that the bunching in France shifted from 39 to 35 hours per week following a reduction in the standard workweek.

[3]In response to these policies, 12 prominent economists signed a letter to the Secretary of Labor in 2015 in favor of expanding overtime under the belief that it would "improve the well-being of the affected employees without harm to the economy." (Economic Policy Institute, 2015)

their impacts have hindered research on overtime.[4] 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, previous papers that have used these policies are confronted with the challenge of separately identifying the effects of overtime on income and employment.[5] Even apart from this issue, 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.[6] In light of these empirical challenges, a 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. 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 salary 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 state-level policies that raised this cutoff.[7]

To estimate the labor market effects of raising the overtime exemption threshold, I implement an event-study difference-in-difference design that exploits the timing of the state

[4]See Hart (2004) and Brown and Hamermesh (2019) for an overview of the literature on overtime.

[5]These studies have instead focused on the impact of overtime coverage on workers' weekly hours, finding a mix of negative (Costa, 2000; Hamermesh and Trejo, 2000) and zero significant effects (Johnson, 2003; Trejo, 2003).

[6]Outside the U.S., researchers have studied reforms from the 1980s to the early 2000s that changed the length of the standard workweek (Brown and Hamermesh, 2019). While they consistently find a decrease in weekly hours, the significance of the employment and income effects vary. My paper differs from these studies along four dimensions: the country (i.e. United States), the time period (i.e. post 2010), the variation (i.e. expanded eligibility instead of a shorter workweek), and the treated population (i.e. salaried workers).

[7]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.

and federal rule changes, along with the high-frequency nature of the data, to construct counterfactual outcomes. Following recent advancements in the minimum wage literature (Cengiz et al., 2019), I estimate the effects of overtime within increments of base pay along the entire wage distribution, leveraging the fact that the expansions in overtime targeted salaried workers earning between the old and new exemption thresholds.

My analysis shows that while expansions in overtime coverage succeed in raising workers' incomes, they also reduce aggregate employment. Following an increase in the overtime exemption threshold, the number of salaried workers earning between the old and new thresholds falls by 17% (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. 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, 4.3% (s.e. 2.2%) of affected jobs were lost due to a reduction in employment that was driven primarily by a reduction in hires.[8] 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%). Together, these estimates imply that affected employment falls by 3.36% (s.e. 1.71%) for every percent increase in affected workers' earnings.

Although the ratio of the employment and income effects suggests that expanding overtime coverage is a relatively less efficient means of raising workers' incomes than simply increasing the minimum wage, I cannot precisely rule out small negative elasticities.[9] Nevertheless, I show that this difference is not unreasonable given the unequal distribution of

[8]A contemporaneous study by Cohen et al. (2020) also finds bunching of managerial jobs at the overtime exemption threshold using a cross-sectional analysis of online job postings. While they interpret the bunching solely as firms strategically classifying jobs as salaried to avoid paying overtime, I show that it is also due to a combination of pay increases and job loss.

[9]In a review of 36 U.S. studies, (Dube, 2019) finds a median elasticity of employment with respect to own wage of 0.17, which I can rule out only with 93% confidence.

the costs of overtime. While the average stayer does not see a large pay increase from overtime coverage, heterogeneity in baseline hours implies that labor costs could have increased substantially among jobs that were eliminated.

In further contrast to the minimum wage, I find that expansions in overtime coverage are actually counter-redistributive, benefiting middle income workers at the expense of lower paying jobs. To evaluate the redistributive properties of overtime expansion, I use the matched employer-employee panel structure of the data to determine where along the income distribution were there changes in hires, separations, reclassifications, and bunching as a result of the major 2016 federal reform. This analysis shows 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 increase in income of 5.8% due solely to a rise in base pay. In comparison, reclassified workers saw no change in base pay and only a small increase in overtime pay, whereas 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 increased the salaries of a small group of workers earning close to the new threshold but cost jobs paying further below it, and thereby exacerbated inequality.

I use these empirical results along with a simple theoretical framework to test three competing models of overtime - a compensating differentials model, a labor supply model, and a labor demand model - and show the evidence is consistent with the latter.[10] The compensating differentials model predicts that firms would reduce employees' base salaries

[10]Unlike the case of the minimum wage in a competitive labor market, there is no single benchmark theory of how expansions in overtime coverage should affect labor market outcomes. While the labor demand model and the compensating differentials model of overtime are the most prominent in the literature, other theories of overtime have examined it through the lens of workers' labor supply decision (Idson and Robins, 1991; Frederiksen et al., 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).

to offset the costs of overtime (Trejo, 1991). Due to a lack of policy changes, prior tests of this theory 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 that firms do not cut base salaries in response to overtime coverage. I show using my framework that the the lack of a negative wage response suggests the existence of rigid wage contracts whereby employers cannot immediately adjust salaries to shifts in product demand.

The inadequacy of the labor supply model at explaining the empirical results adds to the growing evidence on hours constraints within firms (Altonji and Paxson, 1990; Chetty et al., 2011; Labanca and Pozzoli, 2021). I show that if workers dictate their own hours, then no hourly worker would ever choose to work exactly 40 hours per week when covered for overtime. Moreover, an expansion in coverage would lead to a reduction in salaried workers' base pay. In contrast, I find bunching at the 40 hours cutoff among hourly workers and bunching at the exemption threshold among salaried workers, suggesting that hours of labor are not determined solely by labor supply decisions.

My empirical findings also contribute to the literature on work-sharing and the ability of hours regulations at spurring job creation. Historically, overtime was introduced during the Great Depression under the belief that by making hours more expensive, employers would substitute away from long workweeks in favor of more workers (Ehrenberg and Schumann, 1981). The negative employment response to overtime coverage that I observe is inconsistent with this original policy intent, and reinforces existing evidence from studies outside the U.S. that have generally found negative or zero employment effects of policies that shortened the length of the standard workweek (Hunt, 1999; Crépon and Kramarz, 2002; Skuterud, 2007; Chemin and Wasmer, 2009). Interpreting this result through the lens of the labor demand model suggests that employers highly value long work hours, consistent with evidence from the gender gap literature (Goldin, 2014).

The remainder of this paper is organized as follows. In section II, I explain the institutional details governing U.S. overtime regulations and the specific policies that expanded coverage for salaried workers. Section III outlines the predictions of the competing models of overtime. In section IV, I describe the administrative payroll data from ADP LLC that I use in this study. Sections V and VI report my results on the aggregate employment and income effects. In section VII, I compare the effects of overtime coverage to the minimum wage, and examine how the labor market effects of overtime vary across the income distribution. I conclude in section VIII by discussing the implications of my findings and areas for future research.

II 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.[11] 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).[12] 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

[11]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.

[12]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.

coverage, fell from over 50% in 1975 to less than 10% in 2016 (see Appendix Figure A.1).[13] In an effort to restore overtime protection to low-income salaried workers, such as managers at fast food restaurants and retail stores, Departments of Labors at both the federal and state levels 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 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 more than double 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 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.[14] Following the retraction of the 2016 rule change, the DOL 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

[13] 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%.

[14] 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.

study 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 I all state and federal overtime exemption thresholds from 2005 to 2020, along with the invalidated proposal in 2016.[15] 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.[16] 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 not interact with changes in the minimum wage, even after accounting for potential spillovers.

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 other years.

[15] 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.

[16] 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.

III Theoretical Predictions

To guide my empirical analysis, I present a theory of overtime that nests the predominant models in the literature, extends their implications to a scenario where overtime coverage depends on workers' base pay, and explains under which circumstances would each model be expected to prevail. I show that if weekly earnings can vary flexibly as a nonlinear function of workers' hours, then overtime coverage would have no real labor market effects via a compensating differentials mechanism. However, if earnings must be defined via a rigid salary contract, then the impact of overtime coverage will depend on whether the firm or worker has greater bargaining power in determining weekly hours, and the value of long workweeks to the firm. Detailed derivations are in appendix C.

III.a Flexible contracts and compensating differentials

To begin, suppose firms solve the following profit maximization problem:

$$\max_{(n,h)} \pi = xn^{\alpha}h^{\beta} - Y(h)n$$

where x represents demand in the product market, the firm's production function depends on both the number of workers n and the hours per worker h, and weekly labor costs $Y(h)$ is a flexible function of workers' hours. The parameter β allows for the possibility of nonlinear returns to long work weeks, and α allows for decreasing returns to scale. In this general form, any value of h where

$$Y(h) = C_x h^{\frac{\beta}{\alpha}} \tag{1}$$

and C_x is a function of x would satisfy the firm's first order conditions.

I introduce the workers' side of the market to determine equilibrium values for h, n, and

Y. Suppose workers solve the following intensive labor supply problem:

$$\max_h U(h) = Y(h) - a^{-\frac{1}{\epsilon}} \frac{h^{1+\frac{1}{\epsilon}}}{1+\frac{1}{\epsilon}}$$

The intersection between the workers' problem and the profit maximization condition in equation 1 defines the number of weekly hours in terms of C_x. To close the model, the level of employment, earnings, and hours is determined by selecting C_x such that the employer's extensive labor demand, derived from the first order condition $\frac{d\pi}{dn} = 0$, equals workers' extensive labor supply $N^s(Y, h)$.[17]

Within this general framework, overtime coverage would have no effects on any labor market outcomes. For instance, suppose in practice, the earnings schedule $Y(h) = C_x h^{\frac{\beta}{\alpha}}$ is implemented by defining a wage profile $w(h)$ such that

$$w(h)[h + p(h-40)] = C_x h^{\frac{\beta}{\alpha}}$$

where p is the overtime premium for hours worked above 40. In this case, if the overtime premium rises from 0 to 0.5, the wage $w(h)$ would simply decrease to achieve the same level of gross earnings as before. A analogous result holds if the worker is paid by salary rather than per hour: $S(h)[1 + p\frac{(h-40)}{40}] = C_x h^{\frac{\beta}{\alpha}}$.[18] Ultimately, labor market outcomes depend only on product demand, the technology of the firm, and workers' labor supply elasticities.

The independence of labor market outcomes to the overtime premium mirrors the predictions of the compensating differentials model developed by Trejo (1991). In that framework, the ability to offset the cost of overtime depends on the ability to jointly contract on both

[17] One way to motivate $N^s(Y, h)$ is to assume a distribution of reservation utilities $r_1 < \cdots < r_M$. Let $N^s(Y, h) = j$ where $r_j \leq U(Y, h) < r_{j+1}$ be the function that maps earnings and hours to the index of the reservation utilities.

[18] 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.

gross earnings and hours. In effect, even if the marginal wage per hour increases from w to $1.5w$, firms and workers commit to maintaining weekly hours at a lower base wage. Within my model, I show that such a contract can be enforced through an agreement that base wages is a nonlinear function of hours. While a nonlinear wage profile may be reasonable over the span of a person's career, in the very short-run, it may be difficult to maintain if wages are rigid and cannot quickly respond to fluctuations in product demand.

III.b Fixed wage contracts - labor supply vs labor demand

Next, consider the scenario where workers' earning profiles are limited to either a linear function of their hours or a fixed amount independent of weekly hours. The employer is able to decide whether a job is to be paid per hour or via a salary. The cost of employing each type of worker is as follows:

$$Y(h) = \begin{cases} w(h + p(h-40)) + F & \text{if hourly} \\ S(1 + p\frac{h-40}{40}1[S \leq \bar{S}]) + R & \text{if salaried} \end{cases} \tag{2}$$

For salaried workers, they are only eligible for overtime pay if their base pay is below the overtime exemption threshold $\bar{S}$. I have introduced the fixed costs F and R to capture not only usual hiring expenses, but also the value of classifying a job as salaried or hourly. For example, the reduced-form measure R encompass the benefits (e.g. more flexibility, no need to monitor hours, etc.) and the costs (e.g. easier to shirk, less control, etc.) of paying a worker by salary as opposed to hourly.[19] A joint distribution of earnings and hours for both salaried and hourly jobs, can be generated from underlying distributions in the parameters x, β, ϵ, and R. For my comparative statics, I focus on jobs that work at least 40 hours per week when exempt from overtime, since those are the ones directly affected by the policy.

[19] The salaried-hourly decision can be formally motivated by an agency problem where a firm chooses an occupation's pay classification depending on whether the number hours worked is informative of workers' effort and output (Fama, 1991). I take a reduced-form approach to focus primary on the costs of overtime.

Given the above restrictions on the structure of the wage contract, there is no value of h that can satisfy both the firm's and worker's preferences. To see this, notice for any given wage w, employers have an incentive to reduce hourly employees' hours and bunch them at 40. On the other hand, it is never in the workers' interest to work at the 40 hour kink point. Similarly, when a salaried job is not covered for overtime, employers want workers to work as many hours as possible whereas workers want to work as little as possible. One way to reconcile this is to determine weekly hours through a bargaining process. For clarity, I consider the cases where one party has all the bargaining power.

Case 1: Labor Supply Dominates

Suppose workers unilaterally sets hours as in traditional labor supply models (Blundell and MaCurdy, 1999). Among hourly jobs, overtime coverage would incentivize employees to increase their hours, leading to a missing mass at precisely 40 hours per week. However, among salaried jobs, overtime coverage leads to a reduction in base pay similar to the compensating differentials model. Intuitively, since salaried workers' earnings are independent of their hours prior to coverage, they would already be extracting all surplus from the employment relationship by working the minimum hours necessary for the firm to continue operating. As a result, overtime cannot increase salaried workers' surplus and would simply bid down base pays until gross earnings return to their pre-policy levels.

Case 2: Labor Demand Dominates

On the other hand, suppose firms unilaterally set hours. This would be an extreme interpretation of the growing empirical evidence that workers face constraints on their hours within firms (Altonji and Paxson, 1990; Chetty et al., 2011; Labanca and Pozzoli, 2021). In the case of hourly workers, this is analogous to the labor demand model presented in Ehrenberg (1971b). Since the marginal cost per hour of labor has gone up, firms will substitute away from hours per worker in favor of more bodies. At the same time, since labor has become more expensive, there is a counteracting scale effect that reduces employment. This tension between the substitution and scale effects imply that the impact of overtime coverage on employment and income are both theoretically ambiguous.

A similar mechanism also drives the effect of overtime coverage for salaried workers. Appendix C shows that a sufficient condition for employment to rise is if β is sufficiently small. In order words, employers engage in work-sharing if there is little value to employing workers for long hours. Aside from the possibility of work-sharing, the model also generates two addition predictions. First, since only salaried workers with base pays below the exemption threshold are covered, firms would raise workers' salaries to exactly that cutoff if doing so is cheaper than paying them overtime. This bunching effect drives a wedge between extensive labor demand and labor supply, thereby reducing the total number of jobs. Second, since overtime has reduced the profitability of salaried workers relative to hourly workers, jobs that were on the margin of being salaried would be reclassified to hourly.

III.c Testing the predictions

I summarize the testable predictions of the model in Table I. In all cases, an expansion in coverage mechanically increases overtime pay. In response to this, if wage contracts are fully flexible or if wage-hour contracts are enforceable, then base pay would decrease to fully offset the costs of overtime. The same would be true under the labor supply model, except among hourly jobs, I would expect a missing mass at 40 hours of labor per week. In contrast, if firms control hours, then I expect an increase in the overtime exemption threshold to lead to a bunching mass at the threshold coming from jobs that would of otherwise been paid right below it, and reclassification of jobs from salaried to hourly. Employment may rise or fall in this model depending on the productivity returns of working long hours.

IV 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), and state of employment.[20]

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, 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.[21] 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 their wage.

In addition to workers' pay rate, the data also records employees' monthly gross pay and monthly overtime pay.[22] 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

[20] 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 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.

[21] 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.

[22] 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 D for more details.

per pay-period.[23] 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 employer datasets. For each of my analyses, I restrict the sample to a balanced panel of employers since the entry and exit of firms in the data reflect both real business formations and the decision of existing firms to partner with ADP.

In Appendix E, I explore the characteristics of firms affected by the changes in the overtime exemption threshold.[24] Among employers, I find that these expansions in overtime coverage primarily impact large firms. Employers with at least one salaried worker in the interval of base pay targeted by the rule changes are over twice as large as the average firm in the sample. In practice, my main empirical specification retains firms not directly affected by the rule changes to account for potential spillovers such as the reallocation of workers from large to small firms, but I show the results are robust to restricting the sample to only directly affected firms. In terms of the industry mix, I find that while treated firms are more likely to be in retail and restaurant relative to the overall sample, and less likely to be in manufacturing, there are a sizeable share of directly affected employers across all sectors.

[23]I only observe workers' number of paychecks per month starting in 2016. Prior to 2016, I impute the number of paychecks at the employer-month level using a process described in Appendix D. In short, I assume biweekly and weekly paid workers received one more paycheck than usual if the average gross pay for employees in that employer-month far exceeds the average gross pay in the median month for that employer.

[24]For a detailed analysis of the representativeness of the ADP data in general, refer to (Grigsby et al., 2021). They find that while the data closely matches the demographics of workers in the Current Population Survey, it underrepresents employment in firms with over 5000 employees relative to the Business Dynamic Statistics.

V 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 distribution of base pay. Using these estimates, I evaluate the impact of the rule changes on aggregate employment, determine 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.

V.a Federal Policies

Graphical Evidence. Since changes to the Fair Labor Standards Act affect all employers in the US simultaneously, I am unable to implement a traditional cross-state difference-in-difference design to identify the effect of the federal rule changes. Instead, I apply cross-year analysis in the spirit of Saez et al. (2019, 2021) where I compare the evolution of base pays in the year of the reform relative to the evolution in previous years. This empirical strategy will rely on the stability of the base pay distribution over time. As such, my preferred sample only counts employment across the 22 states that did not change their state or local minimum wages after 2014.[25] Moreover, I drop the largest 0.1% of firms since even small year-specific changes in employment at these firms can have very pronounced effects on aggregate employment that is unaccounted for in the cross-year comparison.[26] I will test the robustness of my results to relaxing these restrictions in my analysis.

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 IIa, I

[25] These states are AL, GA, ID, IN, IA, KS, KY, LA, MS, NH, NM, NC, NY, OK, PA, SC, TN, TX, UT, VA, WI, WY. They account for 35% of all workers in the data. Incidentally, given the wave of minimum wage changes since 2015, this sample only includes states that are bounded by the federal minimum wage.

[26] 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.

overlay the frequency distribution of salaried workers' base pay in April 2016 and December 2016, averaged over the balanced panel of firms observable in both months. Reviewing figure IIa from left to right, three features stand out. First, there are very few workers below the old threshold of $455 per week in either month and a noticeable increase in the distribution at exactly the old threshold. This suggests that firms were already cognizant of the initial overtime exemption threshold and adjusted their operations to have very few salaried workers earning below that cutoff.[27] Second, there was a large drop in the number of workers with base pays between the old and new thresholds from April to 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 IIb where I plot the difference between the two distributions in figure IIa. As a placebo check, I also overlay the difference-in-distributions between April and December of each year from 2012 to 2015. Consistent with the labor demand 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 IIc 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 IId. Two features of this figure are worth noting. First, there is a large drop in the number of hourly

[27] 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.

workers at the left tail of the distribution each year and an increase in employment to the right of it. This simply reflects wages growth over time, leading to fewer people earning precisely the federal minimum wage. Second, there is a progressively larger volume of workers within the $455-913 interval each year. This trend of increasing employment growth in fact follows the same pattern as that of salaried jobs. To see this more clearly, appendix figure A.4 replicates the difference-distribution of salaried jobs from figure IIb, but omits the 2016 line to provide a clearer picture of changes in previous years. The similarity between the growth of salaried and hourly jobs over time will motivate my empirical strategy of using changes in the right tail of the salaried distribution to infer the counterfactual hourly distribution.

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 III plots the average salaried distribution for 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 III is similar to the blue line in Figure IIb, 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 after January 2017.[28] I plot a similar graph in Appendix figure A.5 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

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

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.[29] Following recent advancements in the minimum wage literature, I compute 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:

$$n_{ijk,Dec,t} = n_{ijk,Apr,t} + \alpha_{jkt} + \beta_{jk} \cdot D_{t=16} + \varepsilon_{ijkt} \tag{3}$$

where α_{jkt} represents the average change in the number of jk-type workers 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

[29] Graphically, this is equivalent to vertically stretching/compressing and shifting the 2015 difference-distribution in figures IIb and IId to fit the right tail of the 2016 distribution.

above a cutoff bin k^*. This claim is supported empirically by the lack of movement in the upper tail of the difference-distribution between November and December 2016 in the time series profile depicted in figure III. 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 IIb and IId that the difference-distributions have similar shapes each year aside from 2016.[30]

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

$$\begin{aligned}\hat{\beta}_{jk} &= \left(\bar{n}_{jk,Dec,t} - \bar{n}_{jk,Apr,t}\right) - \hat{\gamma}_1\left(\bar{n}_{jk,Dec,t-1} - \bar{n}_{jk,Apr,t-1}\right) - \hat{\gamma}_0 \\ &= \Delta\bar{n}_{jkt} - \hat{\gamma}_1\Delta\bar{n}_{jk,t-1} - \hat{\gamma}_0 \end{aligned} \tag{4}$$

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

$$\Delta\bar{n}_{sal,kt} = \gamma_1\Delta\bar{n}_{sal,k,t-1} + \gamma_0 + \epsilon_{sal,kt} \tag{5}$$

using only salaried workers with bins $k \geq k^*$. I restrict the sample to only salaried workers when estimating equation 5 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.[31]

To develop an intuition for equation 4, notice that if $\hat{\gamma}_1 = 1$ and $\hat{\gamma}_0 = 0$, then the regression is simply a difference-in-difference using the year prior to the policy change as a control group. On the other hand, if employment growth in year $t-1$ is uninformative about

[30] These assumptions are similar to the ones made by Defusco et al. (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%.

[31] A concern with using only salaried workers to estimate equation 5 is that if $\hat{\gamma}_0$ is large, this method would imply a sizeable increase in the counterfactual number of hourly workers in the right tail despite there being very few workers in that region. In practice, this is not an issue as $\hat{\gamma}_0$ is small. In my preferred specification, I estimate $\hat{\gamma}_0 = -0.002$ and $\hat{\gamma}_1 = 0.697$. Thus, the 2016 counterfactual is primary a multiplicative shrinkage of the employment growth in 2015.

the growth in year t (i.e. $\hat{\gamma}_1 = 0$), then $\hat{\gamma}_0$ is the average employment growth at the top of the distribution in year t. In that case, equation 4 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 4 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.

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 5. 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^*.

Before proceeding with the results, first consider the strengths and weaknesses of this empirical approach. To start, note that even without the linear transformation, the difference-in-difference across years is able to account for seasonality between April and December. Yet it is clear from figures II and A.4 that employment differs for reasons aside from seasonality - some years simply have larger employment growth than others. A benefit of applying the linear transformation to the counterfactual year is that it can account for annual differences in aggregate employment growth that affect the entire distribution. However, unlike a traditional difference-in-difference, it is unable to control for year-specific shocks that target specific segments of base pay. Nevertheless, the placebo checks will indicate that any such bin-year specific shocks between 2012-2015 are small relative to the effect of the 2016 policy. Moreover, the event study with the state rule changes will fully address this limitation.

Estimates of Employment Effect Across Distribution of Base Pay. I plot in figure IVa the bin-by-bin treatment effects estimated from equation 4 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. However, 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 4 using adjacent years of data between 2011 and 2015, and plot their respective integrals in figure IVb. 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 IVc 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. Applying the model to the frequency distributions of hourly workers in the four years prior to 2016, I show in figure IVd 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 II, 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 IV, 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 20.7% (s.e. 1%). This reduction in low paying salaried jobs is attributed to three margins of 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, 11.4%

(3.7%) of jobs were reclassified from salaried to hourly.[32] Third, 4.1% (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. Column (2) calculates firms' employment over all states covered by the FLSA overtime exemption policy, rather than just states without a state-specific minimum wage. Column (3) includes the largest 0.1% of firms in the sample. Column (4) allows 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. Column (5) estimates the parameters of the linear transform from equation 5 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.6 and A.7 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.[33]

In column (6) of table II, I apply my baseline specification to estimate the employment

[32] While the rise in hourly jobs is also consistent with firms laying off salaried workers and hiring new hourly ones, I show in section VII.b.1 that nearly the entire increase in hourly jobs is explained by the reclassification of continuously employed workers and not changes in hiring.

[33] As another robustness check, I also estimate equation 4 using only firms in California and New York. I present these estimates graphically in appendix figure A.8. 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.

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 month that the new threshold went into effect (January 2020).[34] 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.9. 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 in support of the labor demand model in section III. Contrary to the predictions of the compensating differentials and labor supply models, base salaries do not decrease following an expansion in overtime coverage. Rather, the two main findings thus far: the bunching of salaries above the overtime exemption threshold and the reclassification of jobs from salaried to hourly fit a model where employers have some control over hours. As a final piece of evidence that workers do not unilaterally set hours, appendix figure A.10 shows bunching at 40 hours of labor per week among hourly workers.[35] While the bunching in hours by itself does not imply that employers are the principle determinants of hours, it does lend credence against the notion that hours are driven solely by labor supply responses. Despite these insights, the results from the federal analysis do not offer enough statistical power to establish the direction of the employment effects.

V.b State Policies

Methodology. To precisely estimate 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

[34] 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

[35] This is consistent with recent evidence of bunching in weekly hours using other sources of administrative payroll data (Goff, 2020).

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 employment is defined in terms of levels rather than shares, even if firms in the treatment and control groups grow at the same rate, the control group will nevertheless gain more jobs simply because it had a higher level of initial employment. To account for this difference 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:

$$\tilde{n}_{ikstv} = \begin{cases} n_{ikstv} & \text{if } s = \text{treatment} \\ n_{ikstv} \cdot \frac{\bar{n}_{k,treat,t=-2,v}}{\bar{n}_{k,control,t=-2,v}} & \text{if } s = \text{control} \end{cases} \tag{6}$$

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:

$$\tilde{n}_{ikstv} = \sum_{\substack{t=-6 \\ t\neq -2}}^{5} \sum_{k=-6}^{15} \beta_{kt} \cdot I_{kst} + \alpha_{ksv} + \delta_{ktv} + \varepsilon_{ikstv} \tag{7}$$

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

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] 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 7 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 correlation between changes in employment within firm.

Estimates of Employment Effect Across Distribution of Base Pay. Figure V shows the estimates of the treatment effect from equation 7, separately for the distribution of salaried and hourly workers. In figure Va, 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 no effect on any bins of base pay above the new threshold.

In figure Vb, I plot the change in the number of salaried workers paid below and above

[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).

[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.

the new threshold over 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 group 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 Va. 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 Vc 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 Vd 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 the increase in the threshold, 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 et al., 2019).

Table III summarizes the aggregate employment effects below and above the new threshold, separately by salaried and hourly jobs. In column (1), I report the employment effect from my baseline specification at event time 0, scaled by the average number of salaried

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

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 20.9% (s.e. 1.2%). However, what happened to these jobs differs from the 2016 federal policy. In comparison to the federal rule change, firms bunched more workers at the new threshold, and did not increase the number of hourly workers in response to the state policies. After accounting for the movement of jobs to above the threshold and to the hourly distribution, I find that 6% (s.e. 2.0%) of affected jobs were lost due to a reduction in employment. The 95% confidence interval implies that at least 2 jobs were lost for every 100 workers directly affected by the rule change.

I assess the robustness of my results to additional controls and alternative samples in columns (2)-(5) of table III. I estimate in column (2) the labor market effects 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 variation in the composition of firms across 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 III, 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 7 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 7, 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, 4.3 (s.e. 2.2) jobs are lost. While I can rule out positive employment effects, the 95% confidence interval suggests that employment losses can be as small as 0% to as large as 8.6%. Overall, I interpret the negative employment response as evidence against the work-sharing hypothesis and in favor of a production function that values long workweeks.

Why Do the Effects of the State and Federal Policies Differ? Relative to the 2016 federal rule change, employers appear to bunch a larger share of affected workers and reclassified fewer workers in response to the state thresholds changes. I argue that this is consistent with the labor demand model. The theory predicts that the likelihood a worker receives a raise above the new exemption threshold increases as their initial salary approaches that cutoff. Since the state rule changes were much smaller, a greater share of affected workers had base pays close to the new threshold, so more workers should be bunched and fewer reclassified.

To understand why the total number of hourly workers barely changed as a result of the state reforms, I estimate equation 7 using net employment flows (i.e. hires minus separations) and net reclassification flows as the outcome variable. This decomposes changes in the aggregate number of workers into the responses that drive these changes. Figure VI plots the estimates of these responses over time. First, I confirm in figure VIa 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 VIb that there was also a decrease in the employment flow of hourly employees on the month that the threshold increased, albeit the magnitude of the effect is imprecisely estimated. Third, counterbalancing the employment loss of hourly jobs, figure VIc 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 V. Lastly, in figure VId, I show that after a policy change, workers who are reclassified from hourly to salaried are more likely to earn above the new threshold.[41]

Together, these results indicate that the state rule changes indeed caused workers to be reclassified from salaried to hourly, as predicted by the labor demand model and consistent with the response to the federal policy change. This reclassification effect was obscured by a simultaneous negative employment flow out of hourly positions, hence why there appeared to be no change in the number of hourly jobs. The reclassification effect was clearer from the analysis of the federal policy simply because the response was much larger due to the substantial increase in the threshold. However, the federal analysis failed to explain increase

[41]To see the estimates corresponding to the figures along with a more detailed decomposition, refer to appendix table A.1 where I report the immediate impact of the state rule change on employment and reclassification flows, by their direction. One insight from this decomposition is that the decrease in employment was driven more by a decline in hires than an increase in separations.

in the number of hourly workers right above the new overtime exemption threshold, as it was overshadowed by the broader increase in hourly employment (see figure IVc). The state analysis clarifies that this small increase in fact reflects a decision by employers to raise hourly workers' salaries above the threshold.

VI 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 workers initially earning between the old and new thresholds to similarly paid workers unaffected by the new policy. As in section V, I identify the counterfactual to the federal and state policies using two different methods. To evaluate the 2016 and 2020 FLSA rule changes, I compare directly affected workers in the year of the reform to similarly paid salaried workers in the preceding year. In my event-study of the state policies, I compare salaried employees in the states that raised their overtime exemption thresholds to those in the 46 states bound by the FLSA. My baseline regression is

$$y_{ivt} = \sum_{t=T_0}^{T_1} \beta_t \cdot I_{st} + \alpha_{vs} + \delta_{vt} + \varepsilon_{ist} \tag{8}$$

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. 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.11, I compare the probability that workers in the treatment and control groups remain with their employer following the 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 VII plots the difference-in-difference estimates for all three policy evaluations. 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 VII, there are four 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, workers' incomes begin to rise even before the new FLSA thresholds go into effect, but show no such anticipatory response to the state reforms. A possible explanation for this difference is that it costs less for firms to quickly adjust to a small change in the threshold relative to a large change. Third, 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 workers experiences a larger increase in base pay than overtime pay. The rise in base salaries reject the prediction of the compensating differentials model that firms would cut workers' base salary to nullify the costs of overtime coverage.

I summarize the income effect of expanding overtime coverage for salaried workers in table IV. 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' pre-tax earnings, I estimate equation 8 using log gross pay as the outcome variable.[42] While these estimates 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 received before 2016.[43] As evidence, notice that the effect on gross pay in column (4) is half that in column (3) when I restrict the state variation to only the six threshold changes that occurred prior to 2016. 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. Overall, the estimates of the income effects are robust to using the gross pay variable.

In summary, column (5) of table IV reports the estimated income effects averaged across all 18 policy changes.[44] As expected, there is a positive effect on total income that is primarily driven by an increase in base pay. I find that workers' incomes increased on average by 1.3%

---

[42] 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.12. While the magnitudes of the estimates are similar, the latter is more volatile.

[43] See appendix D for the imputation procedure.

[44] These are estimated using equation 8 but keeping only two months of data per policy change: the reference month and the month that the threshold increased.

across all sources of variation. 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.

VII Policy Implications

VII.a Jobs vs. Wage Growth

To evaluate the costs and benefits of raising the overtime exemption threshold relative to other labor market policies, I compute the ratio of the employment and income effects in table V. Column (1) presents the ratio of the percent change in employment from column (6) of table III and the percent change in total pay from column (5) of table IV. I find that for every one percent increase in affected workers' incomes, the number of affected jobs falls by 3.36% (s.e. 1.71%). If overtime coverage not only raises employers' labor costs through workers' earnings, but also through a fixed administrative cost that is not captured in the income effect, then the ratio of the employment and income effects alone does not represent a labor demand elasticity.[45] Nevertheless, this ratio is still a policy-relevant statistic for gauging the cost and benefits of overtime coverage from the perspective of workers. Interpreting the ratio in this way, I find that expansions in overtime coverage cost an order of magnitude more jobs for each percent increase in earnings relative to previous estimates in the minimum wage literature. For comparison, a meta-analysis by Dube (2019) finds a median elasticity of employment with respect to own wage of -0.17 across 36 studies of the minimum wage in the U.S, with only two studies observing elasticities less than -2. However, I can only rule out

[45] I thank Steve Trejo for this clarification.

values more positive than -0.17 with 93% confidence, and I cannot reject half the existing elasticities at the 95% level.

One way of estimating more precise elasticities is by removing "unaffected" firms that add noise to the estimates. If I drop firms that employed no salaried workers in the treated interval prior to the policy change, column (2) of table V shows that I obtain a smaller, more precise estimate of -2.93 (s.e. 1.12) and can reject values more positive than -0.72 at the 95% confidence level.[46] This specification can be interpreted as the entire effect of the rule change only under the assumption that the dropped employers do not experience spillover effects from either absorbing workers from directly affected firms or reducing their own hires within the affected region. Taken together, the estimates rule out with confidence any positive work-sharing effects of overtime, but the exact magnitude of the employment-to-income ratio is inconclusive.

Despite the imprecision of the estimates, they do weakly suggest that the job loss associated with overtime coverage more likely than not exceeds that of the minimum wage for the same increase in income. I discuss two potential counterarguments to this finding - challenges to the identifying assumptions and selection of firms in the data - and present a rationale for why it may be reasonable: nonuniform distribution of the costs of overtime.

First, while there may be reasons to be concerned about the validity of the identifying assumptions, the elasticity is robust to a series of placebo checks and stricter specifications, suggesting that the negative elasticity is likely not driven by unobserved shocks. Perhaps the most natural challenge to the empirical strategy would be against the unconventional cross-year analysis of the federal policies. However, note from the estimates of the employment and income effects in tables III and IV that using only the variation from the state threshold

[46]As a placebo check, it makes sense to compare the employment effect in these directly affected firms to unaffected firms. However, by construction, there is no measure of $\frac{\%\Delta\text{Employment}}{\text{Affected Worker}}$ in firms with no treated salaried workers. Instead, to compare between directly and indirectly affected firm, I normalize the employment effect by the total number of workers (salaried and hourly) between the old and new thresholds. Using this broader measure of treated workers, I find that affected firms lose 0.8 (s.e. 0.3) jobs per 100 treated worker, whereas unaffected firms only lose 0.03 (s.e. 0.7) jobs, but the standard errors are too large to distinguish them with confidence.

changes would actually lead to an even larger negative elasticity. Putting aside the federal reforms, the difference-in-difference analysis of the state policies relies on the assumption that absent changes in the overtime exemption threshold, employment in the treated and control states would have evolved at the same rate. I have supported this claim by showing that the parallel trends assumption holds, and that there were no large employment losses outside the pay interval affected by the policy. Nevertheless, there could still have been a shock that targeted only jobs in the affected income range, at exactly the month of the reform, and differentially impacted firms in the treated states from those in the control states. For example, the negative employment response could be confounded by correlation between the timing of industry specific shocks and the composition of industries across states. To account for this, column (3) of table V estimates the ratio of the employment and income effects using only the state variation and restricting the sample to firms that operate in both the treated and control states. Even in this stricter specification where national industry shocks would have affected both states equally, I find a similarly sized elasticity, albeit with less precision.

Second, since the data is from a selected sample of all firms in the US, workers no longer employed in the sample could in principle find a job at firms that do not partner with ADP. In order for this to alter the interpretation of the results, it must be the case that overtime coverage has drastically different effects on firms inside and outside the sample. That is, while overtime causes firms using ADP's software to decrease employment, it causes firms outside the sample to increase employment. This seems unlikely given that the data covers a large share of the US labor force across every industry. Nevertheless, strictly speaking, the results should be interpreted with the understanding that firms outside the sample may respond differently from those in the sample, similar to how studies of the minimum wage in the restaurant industry (Card and Krueger, 2000; Dube et al., 2010), online labor market (Horton, 2017), and other administrative data (Gopalan et al., 2020) are informative foremost for the employers in the sample.[47]

[47]It should be noted that Gopalan et al. (2020) do not find drastically larger elasticities from minimum wage changes than previous estimated in the literature despite using similar administrative

Another concern with the selection of firms is that the results could be driven by a few large employers. Since I compute the employment effect in levels rather than logs to allow for zero employment within a bin, the response of large employers have a pronounced effect on the magnitude of the estimates. While the behavior of these firms is important from a policy perspective, their disproportionate impact on the estimates raises concerns that shocks unrelated to the reforms that affect only a few employers may be driving the results. To address this possibility, I show in column (4) of table V that the estimates are robust to dropping the largest 1% of employers while also using only the state-level variation.

To rationalize the seemingly large employment loss from overtime, I show in appendix E.b that while the increase in income among stayers is small, the cost of overtime among jobs that were never created was potentially sizeable. I confirm previous evidence from Grigsby et al. (2020) that overtime pay makes up only a small fraction of hourly workers' total compensation. However, this is an endogenous outcome as employers have already adjusted their behavior to minimize overtime hours, as evident by the bunching at 40 hours per week in appendix figure A.10. To consider the ex-ante cost of overtime, I make three calculations. To begin, I use the upper tail of the distribution of overtime pay among hourly workers as a benchmark. If I assume that the employment loss came from jobs that would have saw the largest increase in overtime compensation, then hourly workers who earn the most overtime pay would be a more appropriate counterfactual than the average worker. Using this benchmark, I find that for a tenth of hourly workers, overtime comprised at least 8% of their total earnings. This is six times larger than the average ex-post income effect. As a second counterfactual, I use data from the CPS to simulate the expected cost of granting salaried workers overtime given their reported hours and weekly earnings. From this exercise, I find that while 75% of salaried workers would have gained no additional compensation, a tenth of them would have experienced a 10% or greater increase in their earnings.[48] As a

payroll data.

[48]The 10% estimate is using the "fluctuating workweek" method of calculating overtime pay for salaried workers. In the data, most employers use the "fixed workweek" method, which would imply a 30% increase in earnings. This more expensive method is also required by law in California

final exercise, I simulate the cost of simply raising all affected workers above the overtime exemption threshold rather than paying them overtime. This would be an upper bound on the cost of the policy to employers. Presenting these estimates in column (5) of table V, I find an elasticity of employment with respect to own wage of -0.447 (s.e. 0.224), an estimate within range of commonly found elasticities in the minimum wage literature. In summary, these simulations of the ex-ante cost of overtime suggests that it would not be unreasonable for expansions in coverage to cost more jobs than the minimum wage for each percent increase in income. Nevertheless, it is important to recall that such a statement cannot be made with strong confidence given the imprecision of the estimates, so the magnitude of the elasticities should be interpreted with caution.

VII.b 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 which workers' earnings went up.

In this section, I examine how the margins of adjustments vary along the distribution of base pay to determine whether high or low income workers benefit more from expansions 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. By partitioning the sample in this way, I am able to decompose the aggregate changes found in section V.a to changes in employment flows, reclassification flows, and within-classification flows along the income distribution.

and Alaska.

VII.b.1 Which Workers Benefited?

I begin by documenting which workers experienced the largest increase in income. In figure VIII, I plot the evolution of affected workers' income separately by their classification and base pay in December 2016. 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 VI. 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 VI), and fewer workers were bunched than reclassified (see section V), this figure suggests that bunched workers received the largest increase in earnings. 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 V to estimate the effect of the 2016 overtime proposal on the distribution of always-salaried workers.[49] Since I condition the sample on workers being salaried post-reform, I require two identifying assumptions in addition to those described in section V. 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 in the subsequent analysis that they are reasonable given my analysis of the separation and reclassification effects.

[49] 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. $\gamma_0 = 0$).

I present the estimates of my analysis in figure IX, 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.[50] 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 for the average worker estimated in section VI. 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 firms initially captured fairly large rents from the employment relationship. It also supports the argument 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 Xa and Xb, I plot the distribution of reclassifications out of and into the salaried distribution, respectively.[51] Visually, there is a clear increase in the number of reclassifications from salaried

[50] As a placebo check, I do not observe any bunching in the years prior to 2016 in figure A.13. Repeating the same analysis for always-hourly workers, I show in appendix figure A.14 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.

[51] For the equivalent graphs from the perspective of the hourly distribution, see appendix figure

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.

To estimate the net reclassification effect of the 2016 FLSA policy, I make a minor adjustment to the procedure outlined in section V.a. 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.16).

Figure Xc overlays the estimates of the net reclassification effects into the salaried and hourly distributions. There are three findings 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, in aggregate, firms are paying 0.84 (s.e. 0.057) more workers by hour rather than by salary. 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) jobs are reclassified from salaried to hourly. This estimate accounts for nearly the entire rise in hourly jobs described in section V. 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 figure Xd. 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.

A.15.

VII.b.2 Which Workers Lose?

I now turn to the question of where along the income distribution were jobs displaced. In figures XIa and XIb, I plot the distribution of separations and new hires into the salaried distribution between April and December of each year from 2012 to 2016. Examining the distribution of these employment flows, the negative employment effect appears to be driven primarily by a reduction in hires rather than an increase in separations. This is consistent with the previous observation in section VI that the increase in the threshold had no effect on the probability that workers remain employed at the same firm, and reaffirms evidence from the minimum wage literature of firms cutting employment 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 estimate equation 4 using net employment flows (i.e. hires minus separations) as the outcome variable. These estimates, presented in figure XIc, indicate that the employment loss was spread across the entire interval of weekly base pays affected by the increase in the threshold.[52] However, under the assumption that, absent the policy, 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 ones.

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.19 that the identification strategy

[52] To validate the econometric model, I run two falsification tests. First, appendix figure A.17 plots the cumulative sum of the estimates for each year prior to 2016. Although the placebo effects deviate slightly from zero, they are small compared to the effect in 2016 and do not show systematic bias in either direction. Second, appendix figure A.18 plots the cumulative employment effect over time from January to December 2016. Broadly, the picture shows that employment flows did not change relative to the counterfactual until September 2016. The drop in employment starting in October implies that employers are forward-looking and slowed down their hiring of affected workers even before the new overtime exemption threshold went into effect.

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 identity:

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

The change in the total number of workers within each pay classification (Δn) estimated in section V can be decomposed into the employment and reclassification effects estimated in this section. I report each of these components in table VI, scaled by the number of directly affected salaried workers.[53]

The decomposition provides two pieces of suggestive evidence that firms did not change their employment decisions regarding hourly employees in response to the 2016 FLSA policy. First, the 4% (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 2.6 to 5.4 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 V.b, I found that changes in the state thresholds had a negative employment effect on hourly jobs.

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

VIII 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 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 literature on overtime and used existing labor demand elasticities to infer from the Current Population Survey the expected effects of their upcoming reform.

My empirical results differ from the conclusions of the Department of Labor in four ways. First, the DOL believed 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," 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 affected. Second, while the DOL accurately predicted that average weekly earnings would rise, they calculated an income effect of only 0.7%, whereas I show that earnings 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 a small group of 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 decrease in 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. Fourth, 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 (see appendix G). Another area that deserves further attention is the long-run 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.

UNIVERSITY OF SOUTHERN CALIFORNIA

References

Altonji, Joseph G and Christina Paxson, "Labor supply, hours constraints and job mobility," (1990).

Andrews, M. J. and R. Simmons, "Friday May Never be the Same Again: Some Results on Work Sharing from Union–Firm Bargaining Models," *Scottish Journal of Political Economy*, 48 (2001), 488–516.

Autor, David H., Alan Manning, and Christopher L. Smith, "The Contribution of the Minimum Wage to US Wage Inequality over Three Decades: A Reassessment," *American Economic Journal: Applied Economics*, 8 (2016), 58–99.

Barkume, Anthony, "The Structure of Labor Costs with Overtime Work in US Jobs," *Industrial and Labor Relations Review*, 64 (2010), 128–142.

BLS, "Characteristics of minimum wage workers, 2019,"Technical report, Bureau of Labor Statistics (2019).

Blundell, Richard and Thomas MaCurdy, "Labor supply: A review of alternative approaches," *Handbook of labor economics*, 3 (1999), 1559–1695.

Brown, Charles C. and Daniel S. Hamermesh, "Wages and Hours Laws: What Do We Know? What Can Be Done?" *RSF: The Russell Sage Foundation Journal of the Social Sciences*, 5 (2019), 68–87.

Card, David and Alan B. Krueger, "Minimum Wages and Employment: A Case Study of the Fast-Food Industry in New Jersey and Pennsylvania: Reply," *The American Economic Review*, 90 (2000), 1397–1420.

Card, David, Alexandre Mas, Enrico Moretti, and Emmanuel Saez, "Inequality at Work: The Effect of Peer Salaries on Job Satisfaction," *American Economic Review*, 102 (2012), 2981–3003.

Cengiz, Doruk, Arindrajit Dube, Attila Lindner, and Ben Zipperer, "The Effect of Minimum Wages on Low-Wage Jobs," *The Quarterly Journal of Economics*.

Chemin, Matthieu and Etienne Wasmer, "Using Alsace-Moselle Local Laws to Build a Difference-in-Differences Estimation Strategy of the Employment Effects of the 35-Hour Workweek Regulation in France," *Journal of Labor Economics*, 27 (2009), 487–524.

Chetty, Raj, John N Friedman, Tore Olsen, and Luigi Pistaferri, "Adjustment costs, firm responses, and micro vs. macro labor supply elasticities: Evidence from Danish tax records," *The quarterly journal of economics*, 126 (2011), 749–804.

Cohen, Lauren, Umit G. Gurun, and N. Nugra Ozel, "Too Many Managers: Strategic Use of Titles to Avoid Overtime Payments," November (2020).

Collewet, Marion and Jan Sauermann, "Working hours and productivity," *Labour Economics*, 47 (2017), 96 – 106, EALE conference issue 2016.

Costa, Dora L., "Hours of Work and the Fair Labor Standards Act: A Study of Retail and

Wholesale Trade, 1938-1950," *Industrial and Labor Relations Review*, 53 (2000), 648–664.

Crépon, Bruno and Francis Kramarz, "Employed 40 Hours or Not Employed 39: Lessons from the 1982 Mandatory Reduction of the Workweek," *Journal of Political Economy*, 110 (2002), 1355–1389.

Defusco, Anthony A, Stephanie Johnson, and John Mondragon, "Regulating Household Leverage," *The Review of Economic Studies*, 87 (2019), 914–958.

Derenoncourt, Ellora and Claire Montialoux, "Minimum Wages and Racial Inequality," Working Paper (2019).

Dube, Arindrajit, "Impacts of minimum wages: review of the international evidence,"Technical report (2019).

Dube, Arindrajit, Laura Giuliano, and Jonathan Leonard, "Fairness and Frictions: The Impact of Unequal Raises on Quit Behavior," *American Economic Review*, 109 (2019), 620–63.

Dube, Arindrajit, T. William Lester, and Michael Reich, "MINIMUM WAGE EFFECTS ACROSS STATE BORDERS: ESTIMATES USING CONTIGUOUS COUNTIES," *The Review of Economics and Statistics*, 92 (2010), 945–964.

Economic Policy Institute, "Prominent Economists Call on Department of Labor to Set Overtime Pay Threshold No Lower than $50,000 a Year," January (2015).

Ehrenberg, Ronald G., "Absenteeism and the Overtime Decision," *American Economic Review*, 60 (1970), 352–357.

– *Fringe Benefits and Overtime Behavior*: Lexington Books (1971a).

– "The Impact of the Overtime Premium on Employment and Hours in U.S. Industry," *Economic Inquiry*, 9 (1971b), 199–207.

Ehrenberg, Ronald G and Paul L Schumann, "The overtime pay provisions of the fair labor standards act."

Estevão, Marcello, Filipa Sá, and Barbara Petrongolo, "The 35-Hour Workweek in France: Straightjacket or Welfare Improvement? [with Discussion]," *Economic Policy*, 23 (2008), 417–463.

Fama, Eugene F., "Time, Salary, and Incentive Payoffs in Labor Contracts," *Journal of Labor Economics*, 9 (1991), 25–44.

Flood, Sarah, Miriam King, Renae Rodgers, Steven Ruggles, and J. Robert Warren., "Integrated Public Use Microdata Series, Current Population Survey: Version 6.0 [dataset].," Minneapolis, MN: IPUMS, https://doi.org/10.18128/D030.V6.0 (2018).

Frederiksen, Anders, Ebbe Krogh Graversen, and Nina Smith, "Overtime work, dual job holding, and taxation," in Polachek, Solomon W. and Konstantinos Tatsiramos eds. *Work, Earnings and Other Aspects of the Employment Relation*, 28 of Research in Labor Economics, 25–55: Emerald Publishing Ltd (2008).

Goff, Leonard, "You're done for the week: using bunching in hours to infer effects of overtime policy," *Job Market Paper.*

Goldin, Claudia, "A Grand Gender Convergence: Its Last Chapter," *American Economic Review*, 104 (2014), 1091–1119.

Gopalan, Radhakrishnan, Barton H. Hamilton, Ankit Kalda, and David Sovich, "State Minimum Wages, Employment, and Wage Spillovers: Evidence From Administrative Payroll Data," *Journal of Labor Economics, Forthcoming.*

Grigsby, John, Erik Hurst, and Ahu Yildirmaz, "Aggregate Nominal Wage Adjustments: New Evidence from Administrative Payroll Data," Working Paper 25628, National Bureau of Economic Research (2020).

– "Aggregate Nominal Wage Adjustments: New Evidence from Administrative Payroll Data," *American Economic Review*, 111 (2021), 428–71.

Hamermesh, Daniel S. and Stephen J. Trejo, "The Demand for Hours of Labor: Direct Evidence from California," *The Review of Economics and Statistics*, 82 (2000), 38–47.

Harasztosi, Peter and Attila Lindner, "Who Pays for the Minimum Wage?" *American Economic Review*, 109 (2019), 2693–2727.

Hart, Robert A., *The Economics of Overtime Working*: Cambridge University Press (2004).

Hart, Robert A. and Yue Ma, "Wage–hours contracts, overtime working and premium pay," *Labour Economics*, 17 (2010), 170 – 179.

Horton, John J, "Price floors and employer preferences: Evidence from a minimum wage experiment," *Available at SSRN 2898827.*

Hunt, Jennifer, "Has Work-Sharing Worked in Germany?" *The Quarterly Journal of Economics*, 114 (1999), 117–148.

Idson, Todd L. and Philip K. Robins, "DETERMINANTS OF VOLUNTARY OVERTIME DECISIONS," *Economic Inquiry*, 29 (1991), 79–91.

ILO, *Global Wage Report 2020-21: Wages and minimum wages in the time of COVID-19*, 212 (2021).

Johnson, John H., "The Impact of Federal Overtime Legislation on Public Sector Labor Markets," *Journal of Labor Economics*, 21 (2003), 43–69.

Labanca, Claudio and Dario Pozzoli, "Constraints on Hours within the Firm."

OECD, *OECD Employment Outlook 2021*, 401 (2021).

Pencavel, John, "The Productivity of Working Hours," *The Economic Journal*, 125 (2014), 2052–2076.

Quach, Simon, "The Extent of Downward Nominal Wage Rigidity: Evidence from a Natural Experiment."

Saez, Emmanuel, Benjamin Schoefer, and David Seim, "Payroll Taxes, Firm Behavior, and

Rent Sharing: Evidence from a Young Workers' Tax Cut in Sweden," *American Economic Review*, 109 (2019), 1717–63.

– "Hysteresis from employer subsidies," *Journal of Public Economics*, 200 (2021), 104459.

Skuterud, Mikal, "Identifying the Potential of Work-Sharing as a Job-Creation Strategy," *Journal of Labor Economics*, 25 (2007), 265–287.

Sun, Liyang and Sarah Abraham, "Estimating Dynamic Treatment Effects in Event Studies with Heterogeneous Treatment Effects," (2020).

Trejo, Stephen J., "The Effects of Overtime Pay Regulation on Worker Compensation," *The American Economic Review*, 81 (1991), 719–740.

– "Does the Statutory Overtime Premium Discourage Long Workweeks?" *ILR Review*, 56 (2003), 530–551.

U.S. Department of Labor, "Defining and Delimiting the Exemptions for Executive, Administrative, Professional, Outside Sales and Computer Employees," *Federal Register*, 81 (2016), 32391–32552.

– "Defining and Delimiting the Exemptions for Executive, Administrative, Professional, Outside Sales and Computer Employees," *Federal Register*, 84 (2019a), 51230–51308.

– "Employer Costs for Employee Compensation - December 2019," *News Release Bureau of Labor Statistics*.

Wasserman, Melanie, "Hours constraints, occupational choice, and gender: Evidence from medical residents," *Occupational Choice, and Gender: Evidence from Medical Residents (March 19, 2019)*.

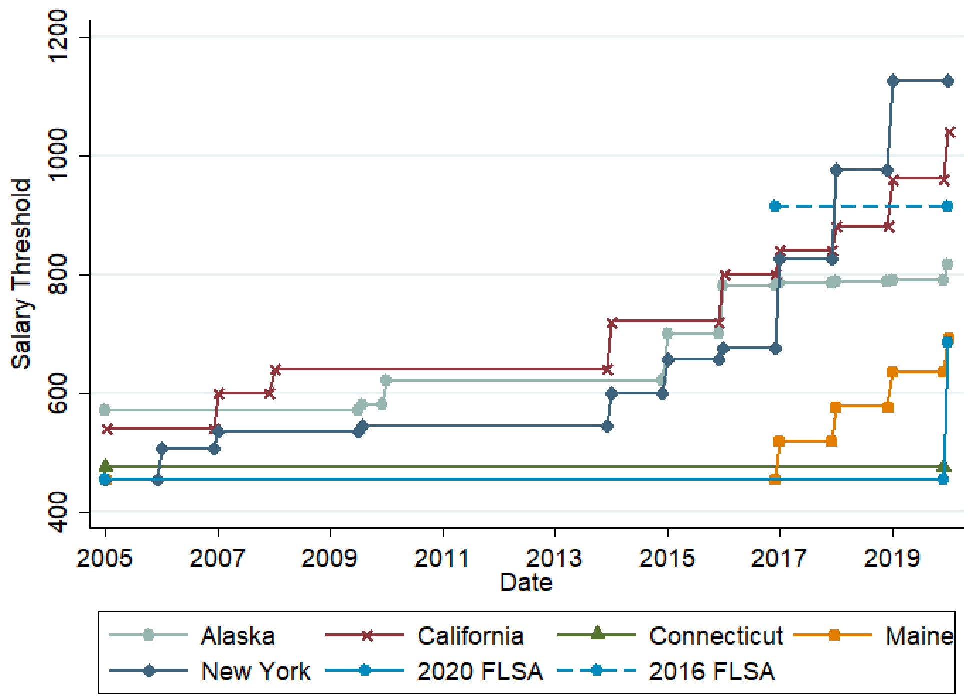
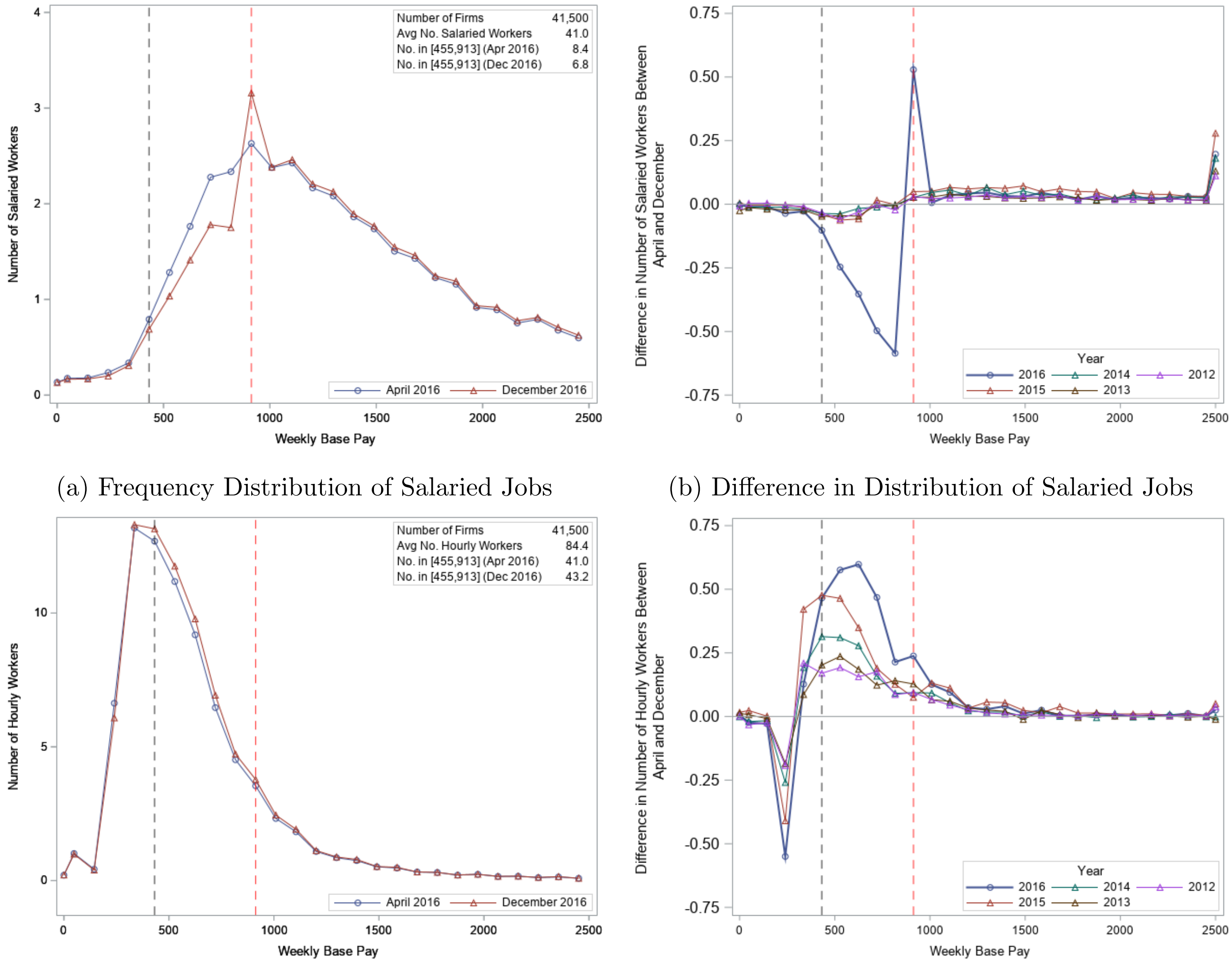


Figure I
Variation in State-Specific Overtime Exemption Thresholds

Notes. This figure shows the binding overtime exemption threshold in each state between 2005 and 2020. All states not explicitly included in the graph are covered by the Fair Labor Standards Act (FLSA). 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.



(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

Figure II

Changes in the Frequency Distribution of Base Pay, by Salaried/Hourly Status

Notes. Panel (a) shows the frequency distribution of weekly base pay of salaried workers in April and December 2016, scaled by the number of firms in the balanced sample. The left vertical 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 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 in the frequency distribution of salaried workers' base pay between December and April, by year. The last bin in Panel (b) counts all workers with base pays$\geq$\$2500. Panels (c) and (d) are the hourly worker analog to Panels (a) and (b), respectively.

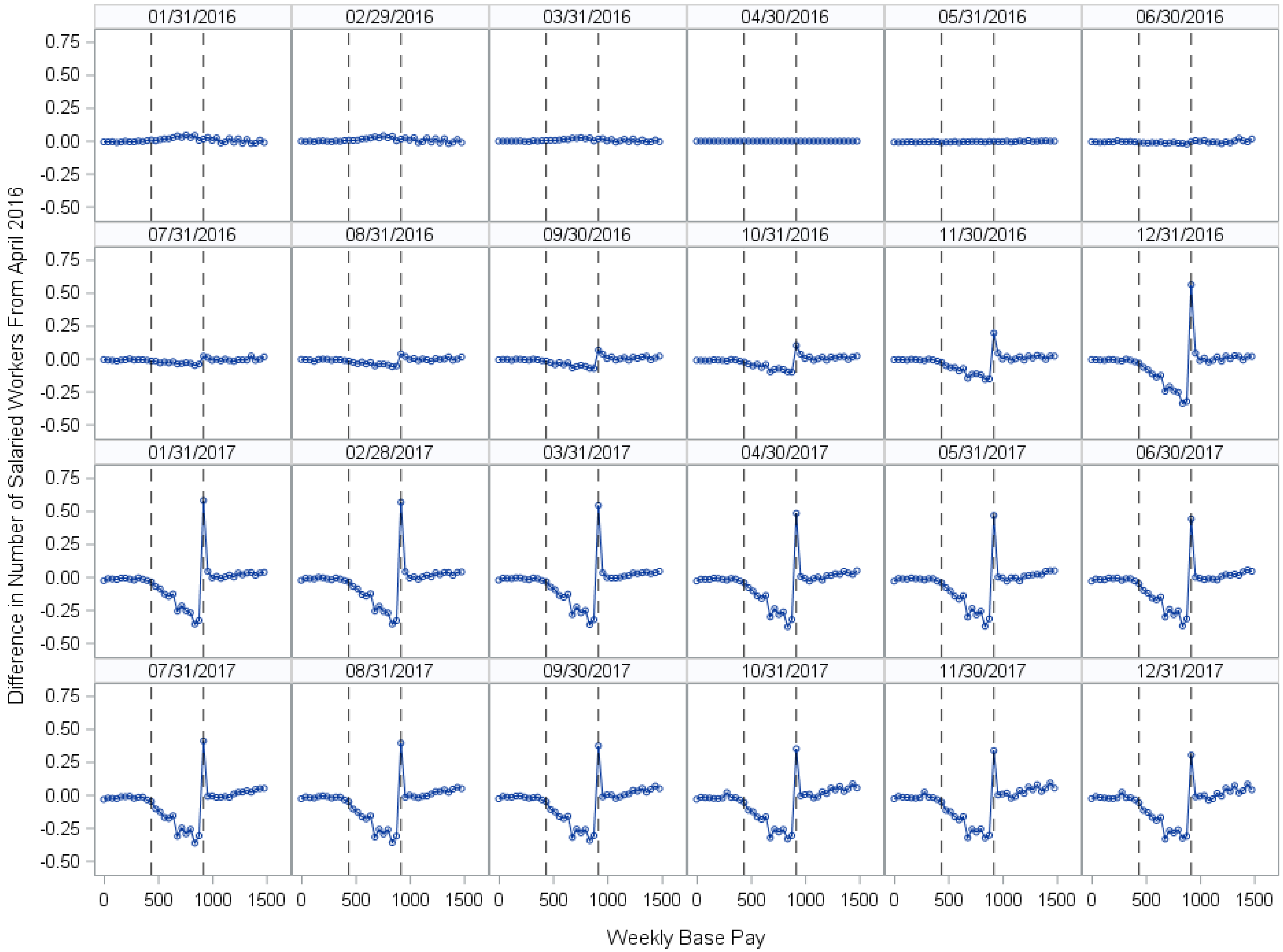
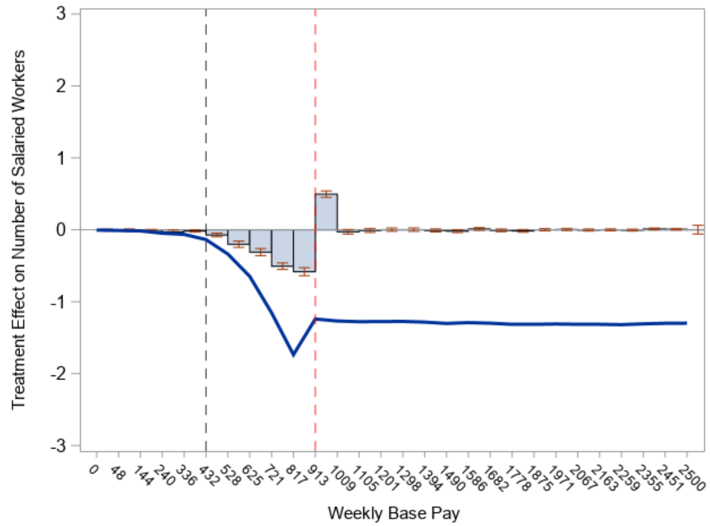


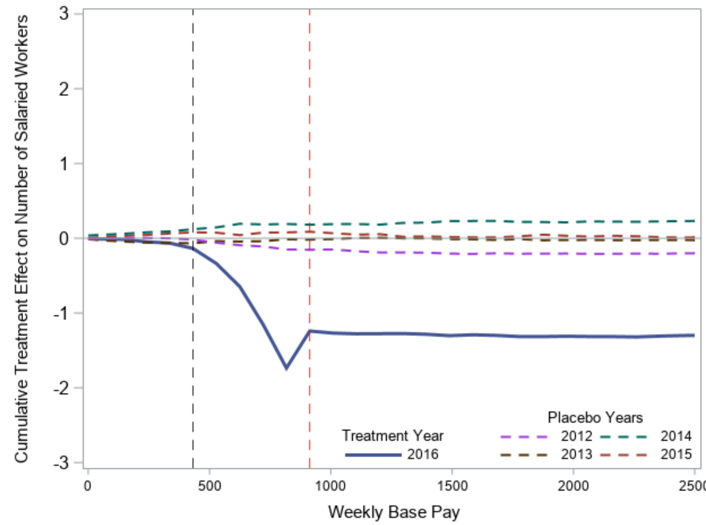
Figure III

Change in Pay Distribution of Salaried Workers since April 2016

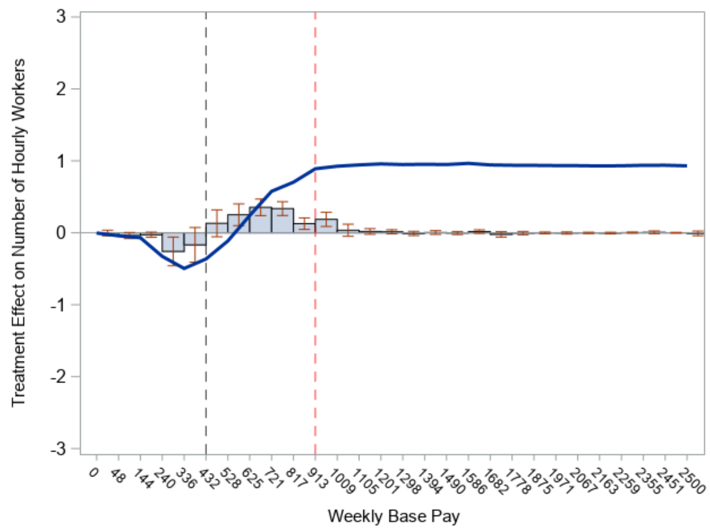
Notes. The figure shows the frequency distribution of weekly base pays in each month of 2016 and 2017, subtracted by the frequency distribution in April 2016. For each month, I scale the distribution by the number of firms that I continuously observe over the 24 months. The bins are $40 wide, shifted so that one bin starts at exactly $913. The left vertical 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.



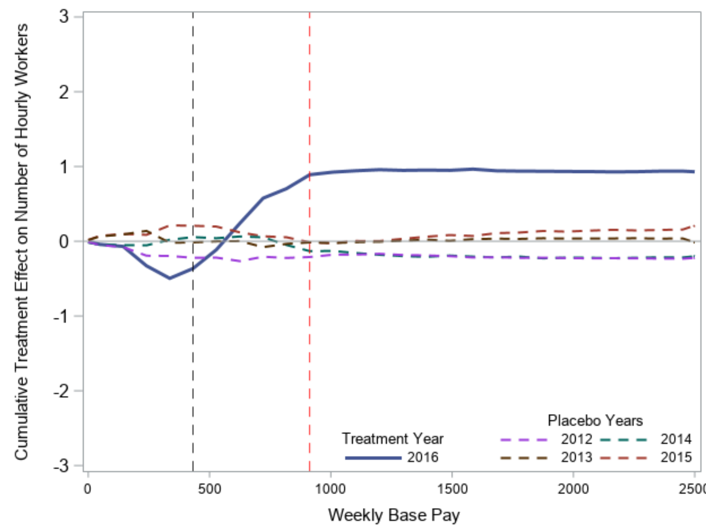
(a) Estimates of Effect on Number of Salaried Jobs



(b) Placebo Test of Effect on Salaried Distribution



(c) Estimates of Effect on Number of Hourly Jobs

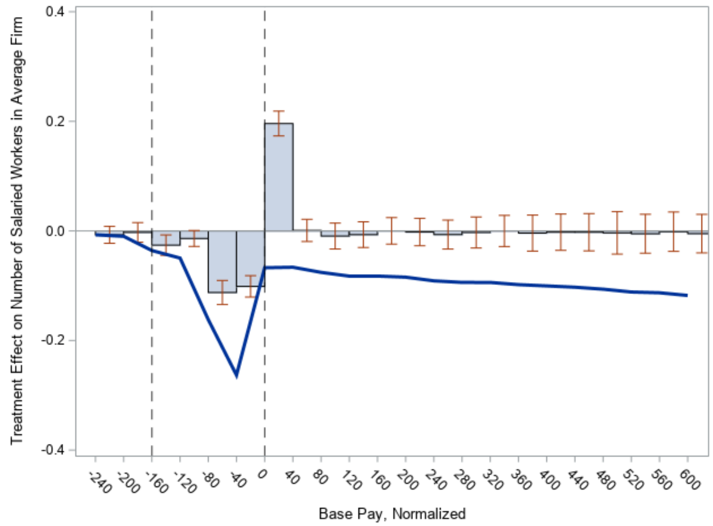


(d) Placebo Test of Effect on Hourly Distribution

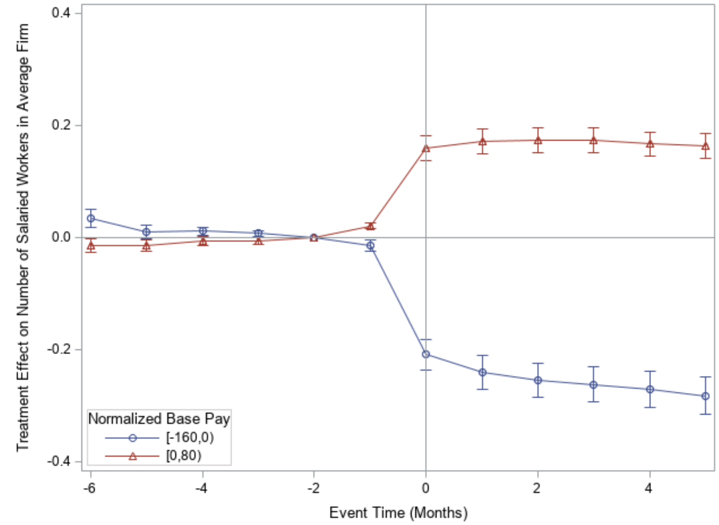
Figure IV

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

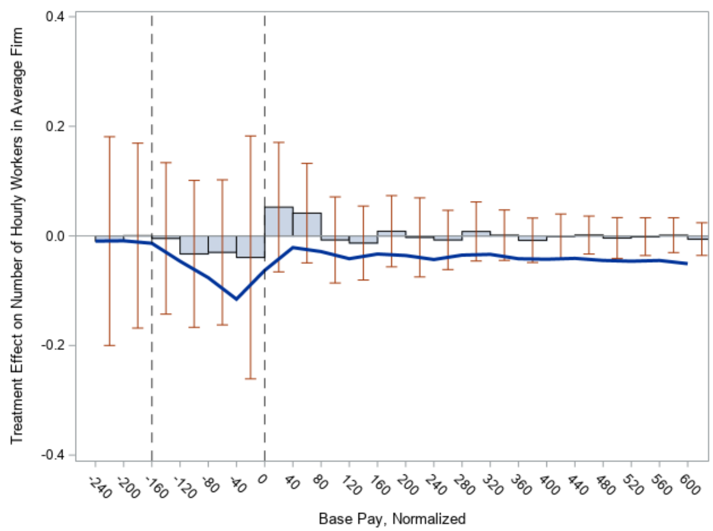
Notes. Panel (a) shows the effect of the 2016 FLSA policy on the number of salaried jobs in each \$96.15 bin of base pay in Dec 2016. The treatment effects are estimated using equation 4. The solid blue line is the running sum of these effects. The solid line in Panel (b) is the same as the solid blue line in Panel (a), whereas the dotted lines are similarly defined running sums estimated using adjacent years of data prior to 2016. Panels (c) and (d) are analogous to Panels (a) and (b) for the distribution of hourly jobs. In all graphs, the left and right vertical lines are at the bins that contain the old and new OT exemption thresholds (\$455 and \$913), respectively.



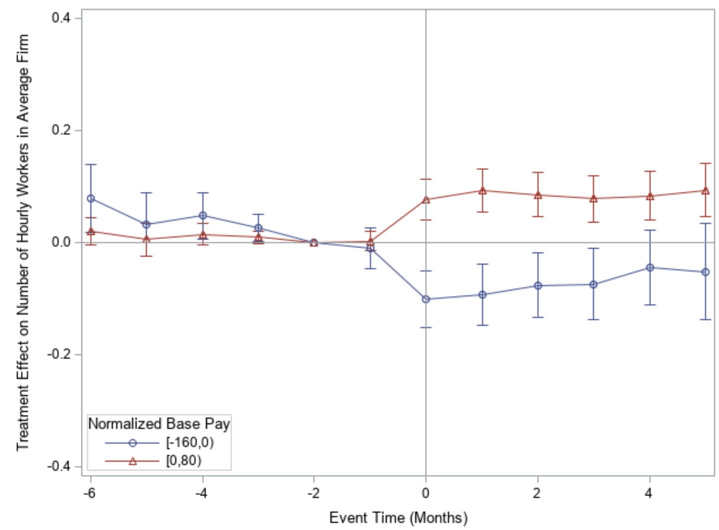
(a) Effect on Number of Salaried Jobs, at Time 0



(b) Effect on Salaried Distribution Over Time



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

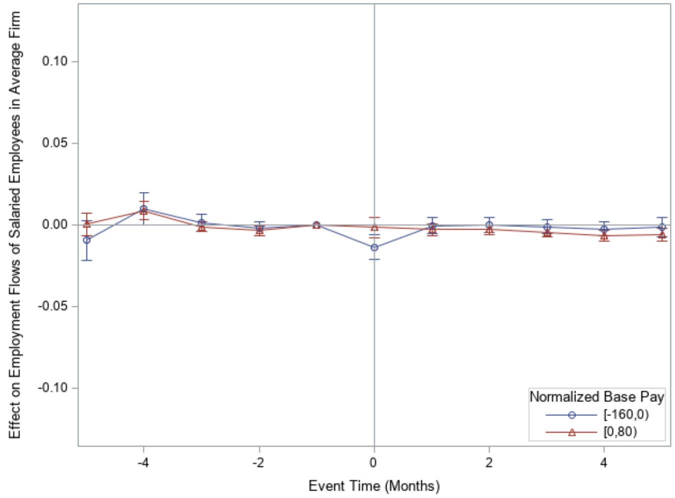


(d) Effect on Hourly Distribution Over Time

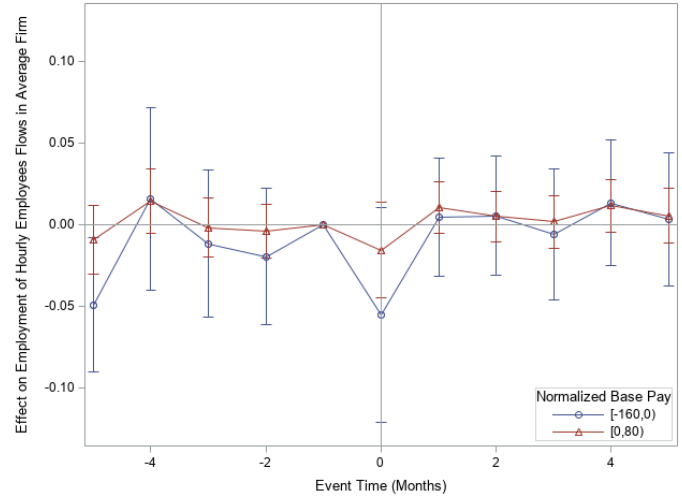
Figure V

Effect of Raising States' OT Exemption Thresholds on the Frequency Distribution of Base Pay

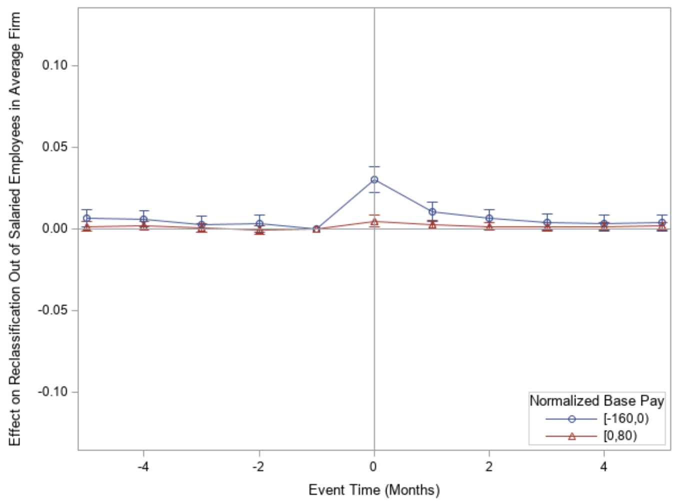
Notes. Panel (a) shows the event study estimates from equation 7. The height of each bar indicates the effect of raising the OT exemption threshold on the number of salaried jobs in each $40 bin of base pay on the month that the the new threshold becomes binding. The solid blue line is the running sum of these estimates. The bins are normalized so that the new threshold for each event is 0. The left vertical dashed line is set at the smallest baseline threshold across all the events. Panel (b) shows the sum of the estimates over time, separately for bins between the old and new thresholds and bins above the new threshold. 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.



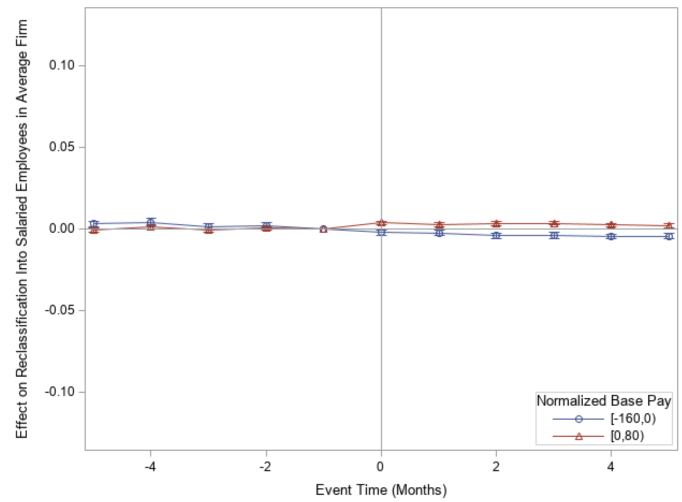
(a) Net Salaried Employment Flows



(b) Net Hourly Employment Flows



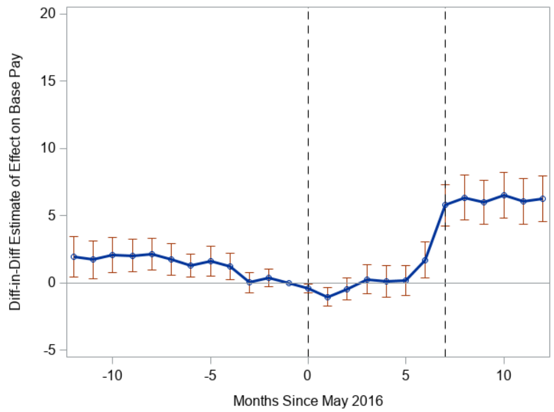
(c) Reclassification Flows Out of Salaried



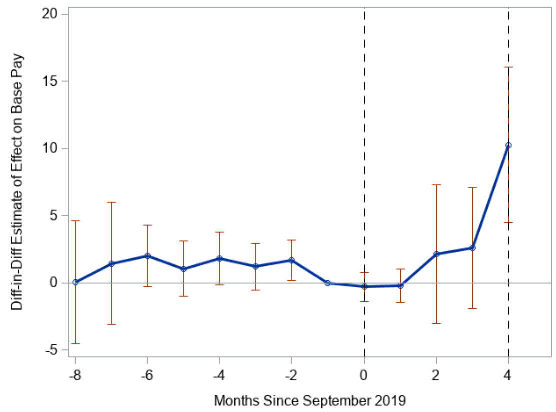
(d) Reclassification Flows Into Salaried

Figure VI
Effect of State Threshold Changes on the Flow of Workers Into, Out of, and Within Firms

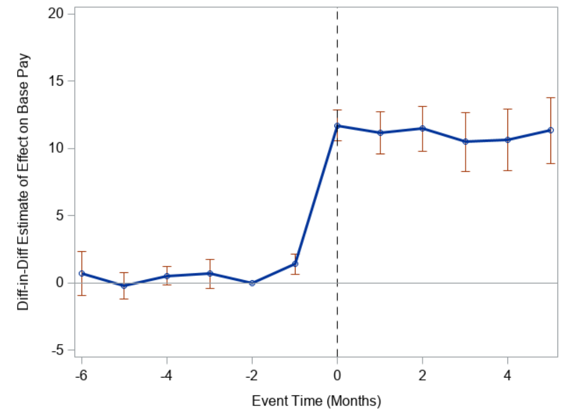
Notes. Panel (a) plots the effect of the state threshold changes on the net employment flow of salaried employees for each month since the threshold increased. Panel (b) plots the analogous figure for net employment flows of hourly employees. Panel (c) plots the effect on the number of salaried workers 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 7, and aggregated separately for bins between the old and new thresholds (circles) and bins above the new threshold (triangles). For each estimate, I show the 95% confidence interval using standard errors clustered by firm.



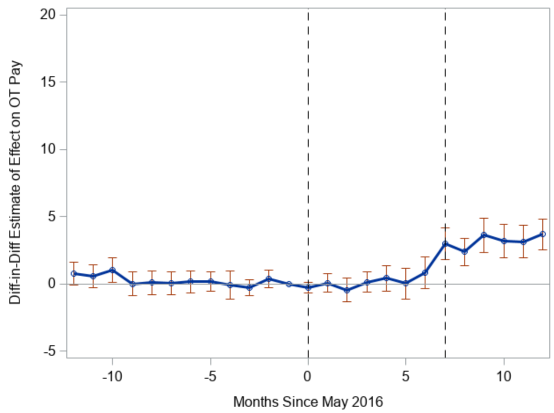
(a) 2016 FLSA: Base Pay



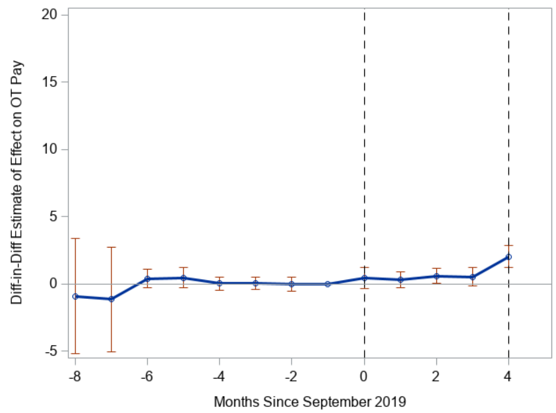
(b) 2020 FLSA: Base Pay



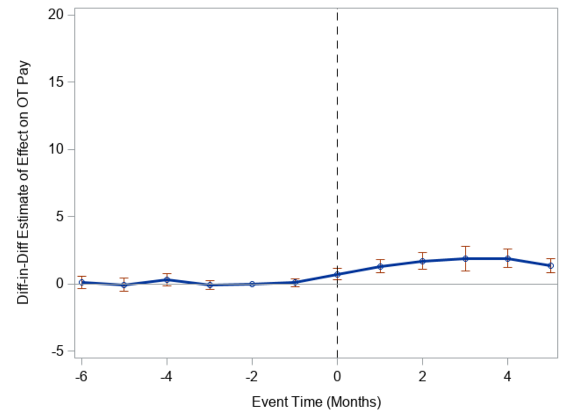
(c) Event Study: Base Pay



(d) 2016 FLSA: OT Pay



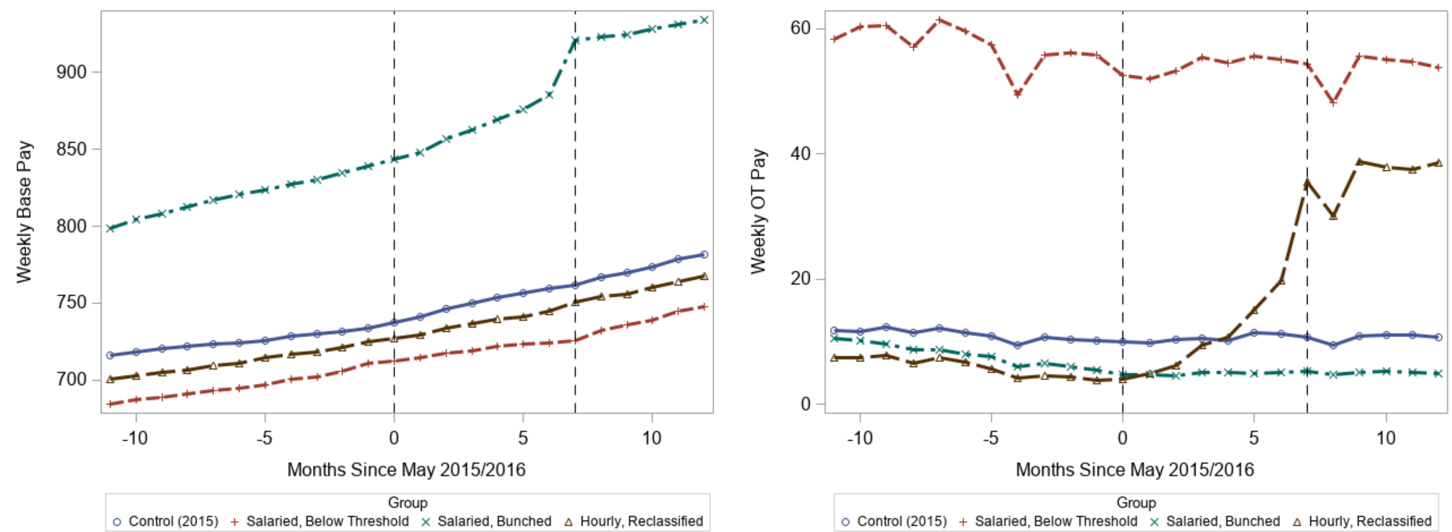
(e) 2020 FLSA: OT Pay



(f) Event Study: OT Pay

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

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 8, 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.



(a) Evolution of Base Pay

(b) Evolution of Overtime Pay

Figure VIII

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

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 paid workers in April 2016, separated by their pay rate and classification 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.

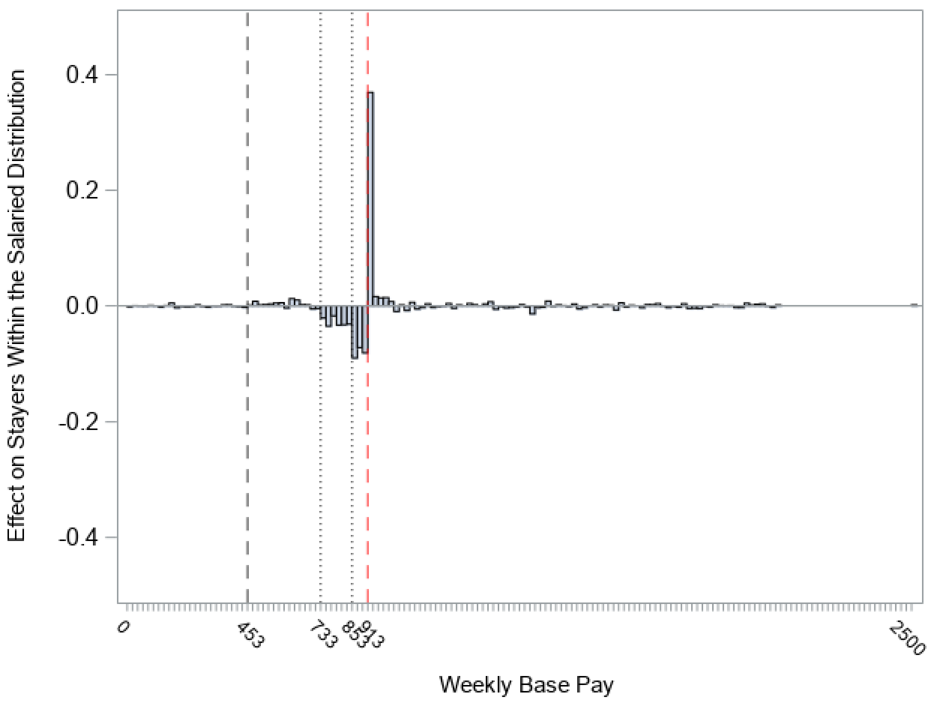
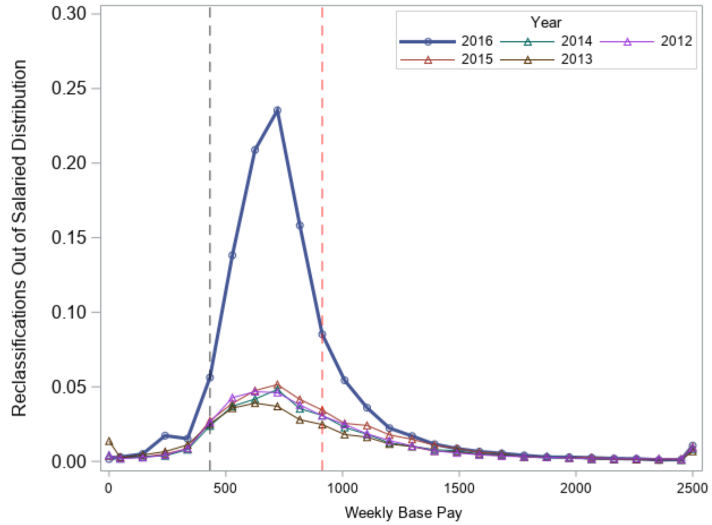
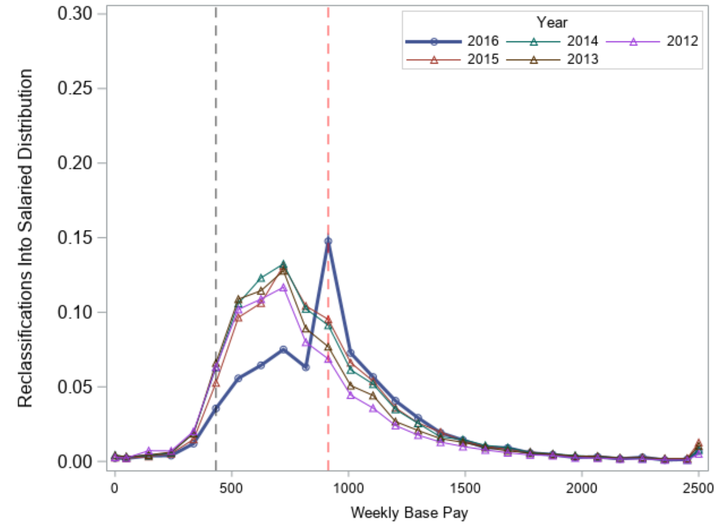


Figure IX
Effect of Raising the OT Exemption Threshold on the Distribution of Stayers

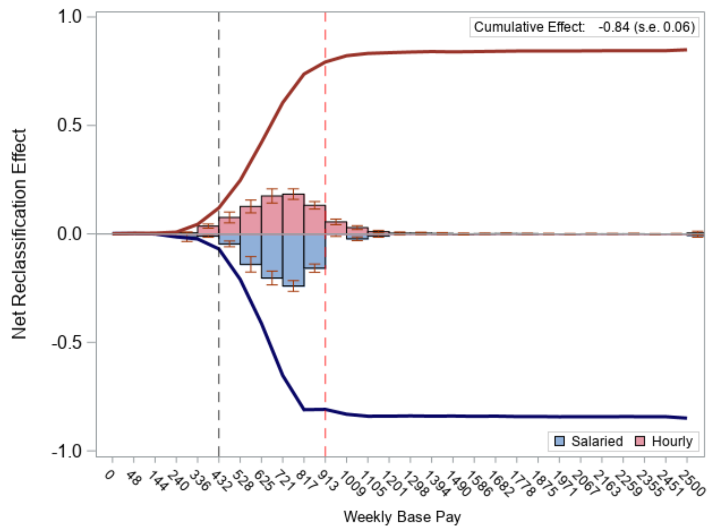
Notes. This figure shows the effect of the 2016 OT policy on the distribution of workers who stay at the same firm between April and December 2016, and are paid by salary in both months. The height of each bar is estimated using equation 4, assuming $\gamma_0 = 0$ and using \$20 bins of base pay. The left and right most vertical dashed 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.



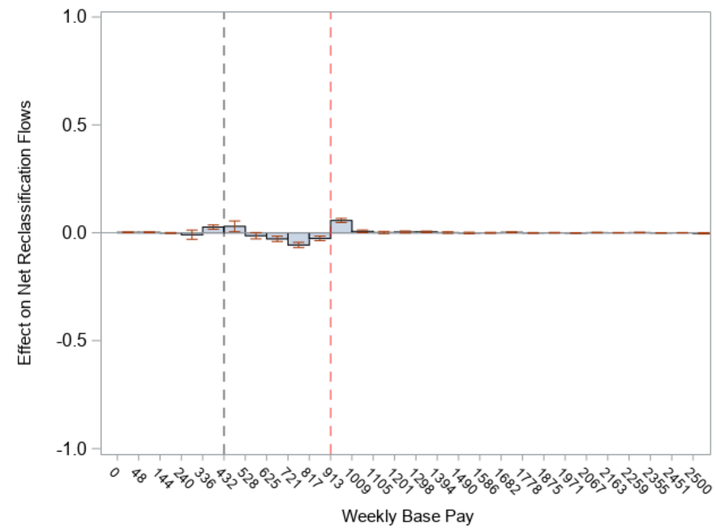
(a) Reclassifications Out of Salaried



(b) Reclassifications into Salaried



(c) Effect on Net Reclassifications

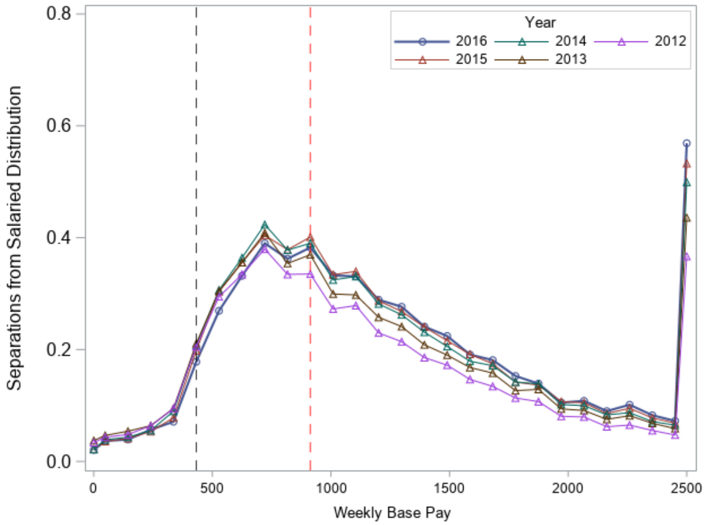


(d) Effect on Base Pay of Reclassified Workers

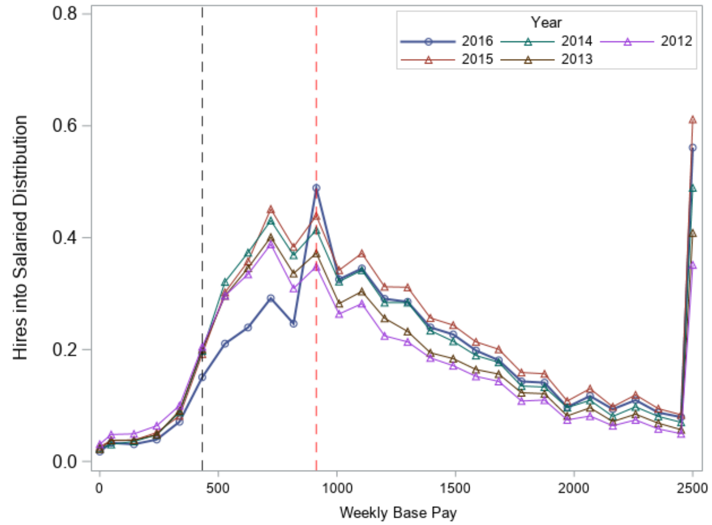
Figure X

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

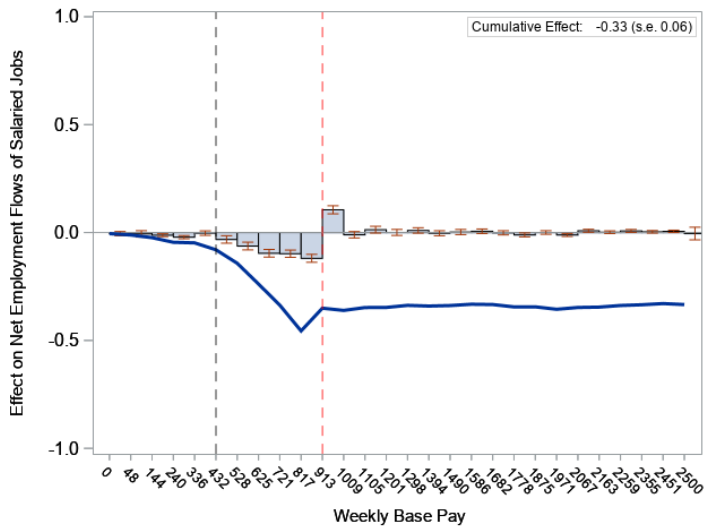
Notes. Figure (a) shows the average firm's frequency distribution of base pays in April among stayers who are reclassified from salaried to hourly between April and December. Figure (b) shows the frequency distribution of base pays in December for stayers who are reclassified from hourly to salaried. Figure (c) plots the effect of the 2016 FLSA policy on the net number of reclassifications into the salaried distribution and the net number of reclassifications into the hourly distribution, estimated from equation 4 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 two distributions in Panel (c). The left and right vertical lines are at the initial and proposed 2016 FLSA thresholds, respectively.



(a) Distribution of Separations from Salaried Jobs



(b) Distribution of New Hires into Salaried Jobs



(c) Effect on Net Employment Flows

Figure XI
Effect of Raising the 2016 FLSA OT Exemption Threshold on the Distribution of 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 net employment flows (i.e. difference between the number of hires and separations), estimated from 4. The solid blue line is the cumulative sum of the bin-specific effects. In all figures, the left and right vertical lines are at the initial and proposed 2016 FLSA thresholds, respectively.

Table I
Summary of Theoretical Predictions

Prediction	Compensating Differentials	Labor Supply	Labor Demand
Base Pay	$\downarrow$	$\downarrow$	Bunching
Overtime Pay	$\uparrow$	$\uparrow$	$\uparrow$
Employment	-	-	?
Pay structure	-	-	Reclass from salaried to hourly
Hours	-	Missing mass at 40	Bunching at 40

Notes. This table summarizes the predictions of the three models of overtime discussed in Section III. The first four rows refer to the effect on each outcome from an expansion in overtime coverage for salaried workers. THe last row refers to the effect of overtime among hourly workers. In the labor demand model, the employment effect is positive if the marginal productivity of additional hours diminishes sufficiently quickly, and negative otherwise.

Table II
Effect of Raising the FLSA OT Exemption Threshold on the Pay Distribution

	(1)	(2)	(3)	(4)	(5)	(6)
Jobs Below Threshold	−0.207***	−0.186***	−0.208***	−0.222***	−0.205***	−0.144***
	(0.01)	(0.007)	(0.011)	(0.012)	(0.01)	(0.026)
Bunched	0.052***	0.041***	0.050***	0.028**	0.054***	0.134***
	(0.008)	(0.007)	(0.009)	(0.013)	(0.008)	(0.038)
Hourly Jobs	0.114***	0.079***	0.178***	0.078	0.119***	0.013
	(0.037)	(0.028)	(0.054)	(0.066)	(0.037)	(0.218)
Employment	−0.041	−0.073**	0.020	−0.116	−0.032	0.002
	(0.042)	(0.033)	(0.058)	(0.076)	(0.042)	(0.231)
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 4. 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 the scaling factor in equation 5. 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 5 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. *10%,** 5%,*** 1% significance level.

Table III
Effect of Raising States' OT Exemption Thresholds on the Pay Distribution

	(1)	(2)	(3)	(4)	(5)	(6)
Jobs Below Threshold	−0.209***	−0.238***	−0.218***	−0.231***	−0.219***	−0.172***
	(0.012)	(0.014)	(0.017)	(0.012)	(0.017)	(0.007)
Bunched	0.162***	0.138***	0.18***	0.178***	0.16***	0.092***
	(0.010)	(0.009)	(0.014)	(0.011)	(0.013)	(0.004)
Hourly Jobs	−0.013	0.036	−0.010	−0.003	−0.010	0.037*
	(0.02)	(0.037)	(0.029)	(0.020)	(0.045)	(0.021)
-Below	−0.091***	−0.043	−0.071**	−0.090***	−0.110***	−0.009
	(0.023)	(0.037)	(0.033)	(0.023)	(0.043)	(0.020)
-Above	0.078***	0.079***	0.061**	0.087***	0.100***	0.045***
	(0.018)	(0.020)	(0.027)	(0.019)	(0.026)	(0.008)
Employment	−0.060***	−0.064*	−0.048	−0.057***	−0.069	−0.043**
	(0.020)	(0.037)	(0.030)	(0.020)	(0.044)	(0.022)
Treatment Group						
Affected Salaried	1.20	1.18	1.10	1.20	0.77	2.46
Affected Hourly	4.18	4.25	3.8	3.96	3.74	14.21
Avg. Firm Size	110	108	62	109	99	117
No. Firm-Events	183,673	164,106	126,777	150,901	68,735	262,107
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 and above the new threshold, 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 effects of increasing a state's OT exemption threshold at 0 and 5 months after the date of the rule change, respectively, estimated using equation 7. 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) presents the event-study estimates that include both the state and federal overtime reforms. All robust standard errors in parentheses are clustered by firm. *10%,** 5%,*** 1% significance level.

Table IV
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 ($)	5.79***	10.260***	11.710***	11.769***	9.608***	10.176***	1.507***
	(0.774)	(2.901)	(0.571)	(0.944)	(0.519)	(0.686)	(0.434)
OT Pay ($)	2.968***	2.042***	0.706***	0.369	1.589***	2.053***	0.335*
	(0.590)	(0.410)	(0.214)	(0.319)	(0.238)	(0.332)	(0.197)
Total Pay ($)	8.759***	12.303***	12.416***	12.141***	11.197***	12.229***	1.842***
	(1.01)	(2.924)	(0.621)	(0.987)	(0.582)	(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***	0.004	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	586.78	864.39	692.94	771.52	746.78	909.37
N (treatment)	159,408	51,408	166,892	38,232	377,708	274,317	122,491
N (control)	192,912	56,885	1,838,371	591,075	2,088,168	562,423	1,071,995
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 baseline 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 VI.

Columns (1) reports the income effect of the 2016 FLSA policy estimated from equation 8, 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. $^{*}10\%,^{**}5\%,^{***}1\%$ significance level.

Table V
Ratio of Employment and Income Effects

	(1)	(2)	(3)	(4)	(5)
%Δ Employment	-0.043^{*}	-0.038^{**}	-0.047	-0.049^{**}	-0.043^{*}
	(0.022)	(0.015)	(0.029)	(0.019)	(0.022)
%Δ Income	0.013^{***}	0.012^{***}	0.014^{***}	0.011^{***}	0.097^{***}
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Ratio	-3.362^{*}	-2.927^{**}	-3.397	-4.346^{**}	-0.447^{**}
	(1.705)	(1.124)	(2.148)	(1.708)	(0.224)
Sample					
Income	Realized	Realized	Realized	Realized	Simulated
No. Firm-Events	262,107	75,211	126,777	181,844	262,107

Notes. Row (1) reports the change in employment scaled by the number of affected workers. Row (2) reports the percent change in income of affected workers. Row (3) reports the ratio of the estimates in rows (1) and (2).

Column (1) reports the employment effect from column (6) of table III, the income effect in column (5) of table IV, and their ratio. This specification uses both the state and federal policy variations, keeps all firms in the sample, and treats only salaried workers between the old and new overtime exemption thresholds as directly affected workers. Column (2) restricts the sample in column (1) to only firms that had at least one salaried worker between the old and new thresholds. Column (3) uses only the state variation and restricts the sample to firms that operate in both treated and control states. Column (4) uses only the state variation and drops the largest 1% of firms. Column (5) uses the full sample but simulates the income effect as the cost of bunching all affected salaried workers above the new exemption threshold. All robust standard errors in parentheses are clustered by firm. $^{*}10\%,^{**}5\%,^{***}1\%$ significance level.

Table VI
Decomposition of the Effect of the 2016 FLSA Policy on the Number of Workers

	Δn	Employment	Reclassification
Salaried	-0.155^{***}	-0.040^{***}	-0.100^{***}
	(0.013)	(0.007)	(0.007)
Hourly	0.114^{***}		0.100^{***}
	(0.037)		(0.007)
	-0.041		0.000
	(0.042)		(0.000)

Notes. Column (1) reports the effect of the 2016 FLSA policy on the number of salaried employees (row 1), the number of hourly employees (row 2), and the total number of employees (row 3) in the average firm. These numbers correspond to the estimates in the first column of table II, 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, discussed in section VII.b.2. Column (3) shows the effect of the policy on the number of reclassifications, discussed in section VII.b.1. All estimates are reported in terms of "per directly affected workers in April 2016". All robust standard errors in parentheses are clustered by firm. *10%,** 5%,*** 1% significance level.

Appendix A. Additional figures and tables

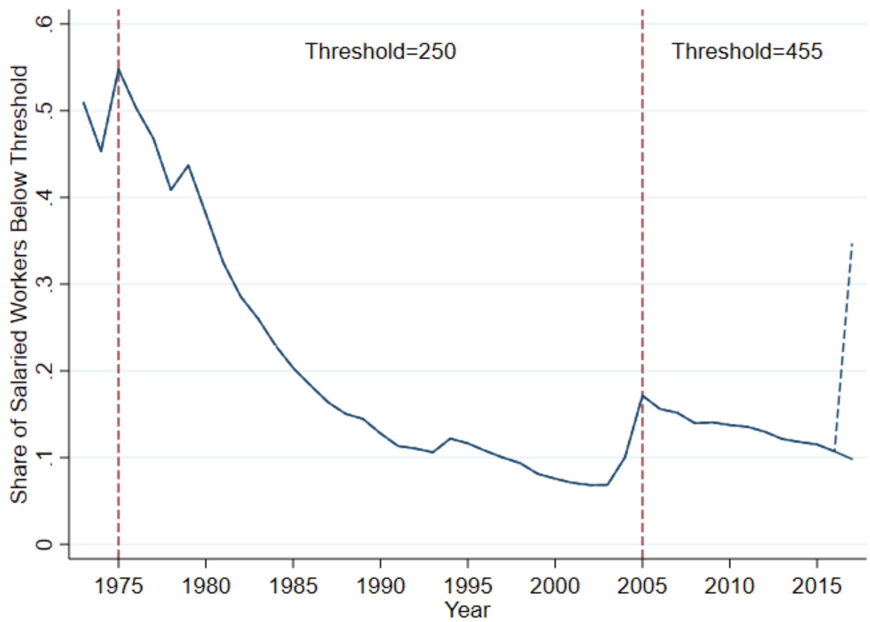


Figure A.1
Percent of Salaried Workers Below the FLSA OT Exemption Threshold

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.

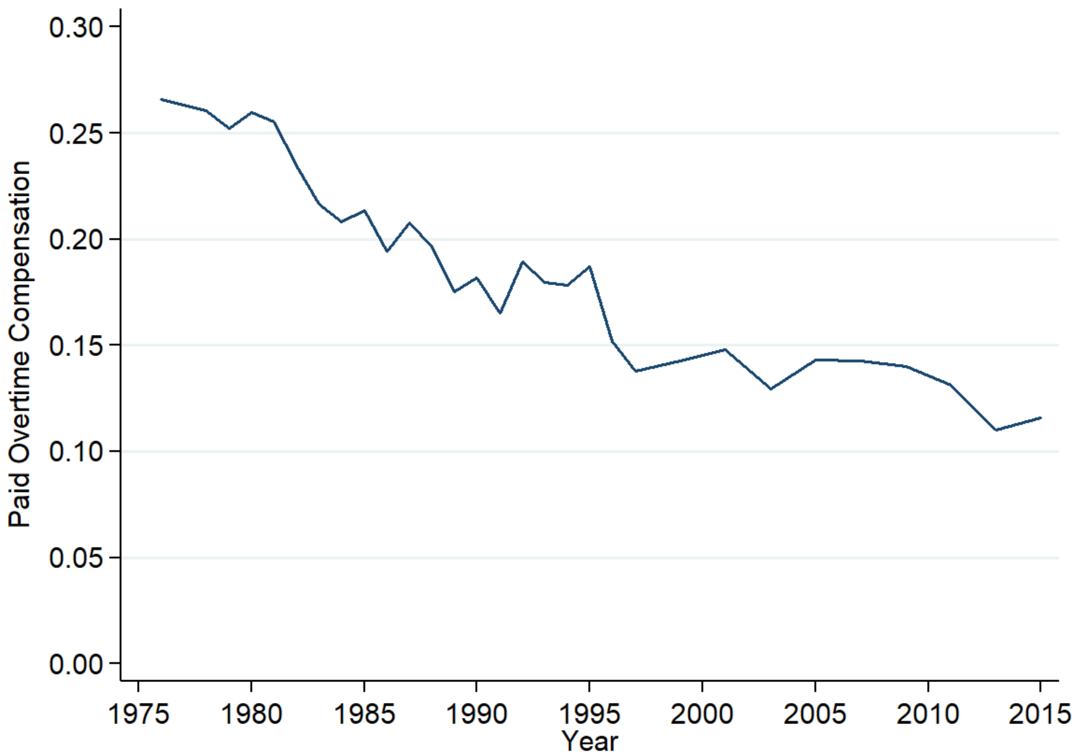


Figure A.2
Percent of Salaried Workers Eligible for Overtime

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".

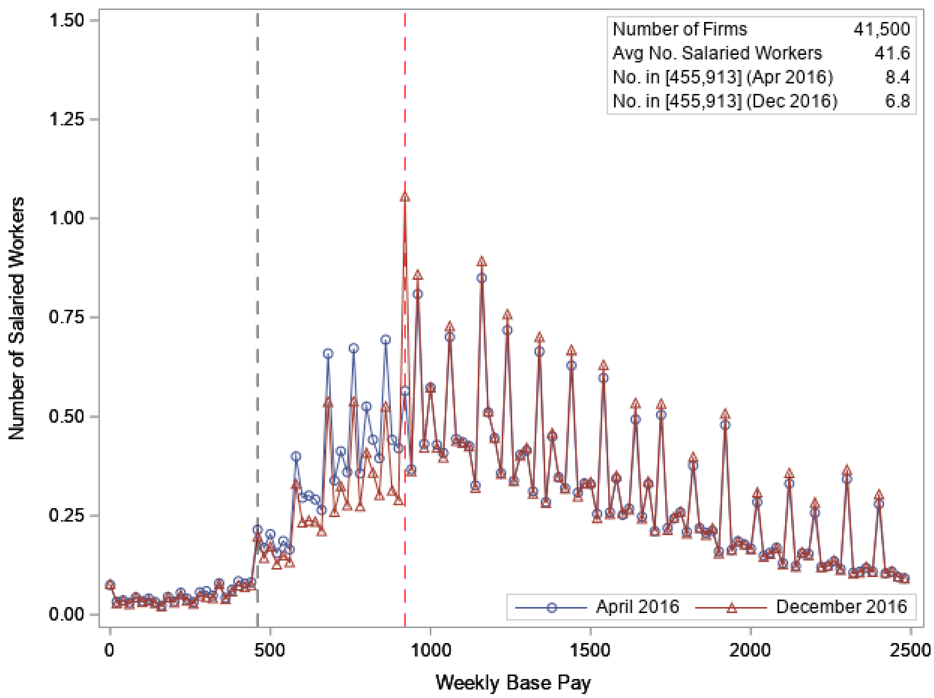


Figure A.3
Frequency Distribution of Salaried Workers' Base Pay using $20 Bins

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

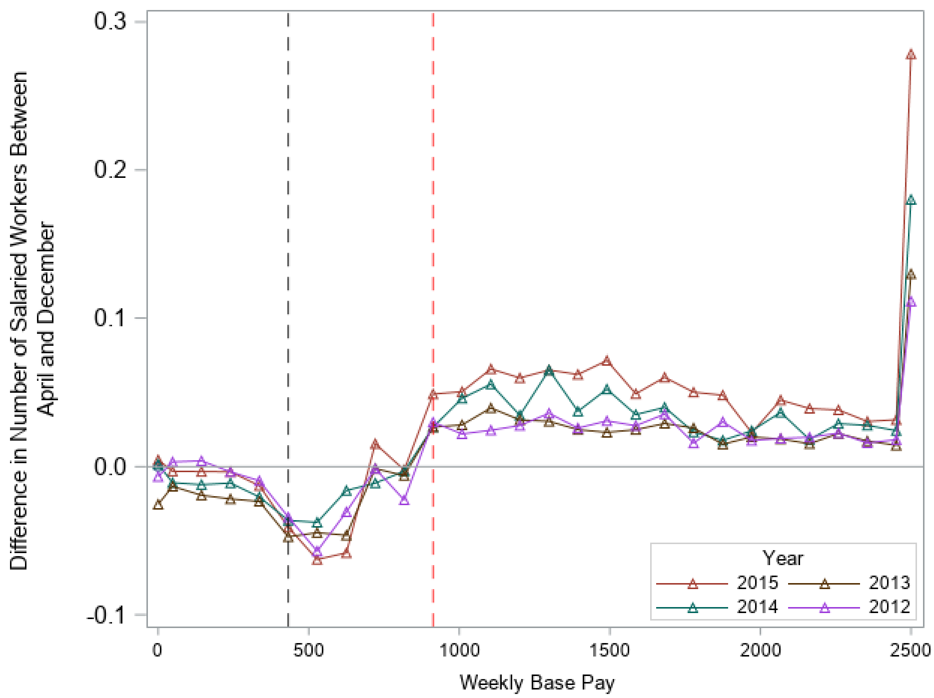


Figure A.4
Difference in Distribution of Salaried Jobs, Omitting 2016

Notes. This figure shows the difference in the frequency distribution of salaried workers' base pay between December and April, by year. This figure is equivalent to figure IIb, except 2016 is omitted to provide a closer look at the previous years.

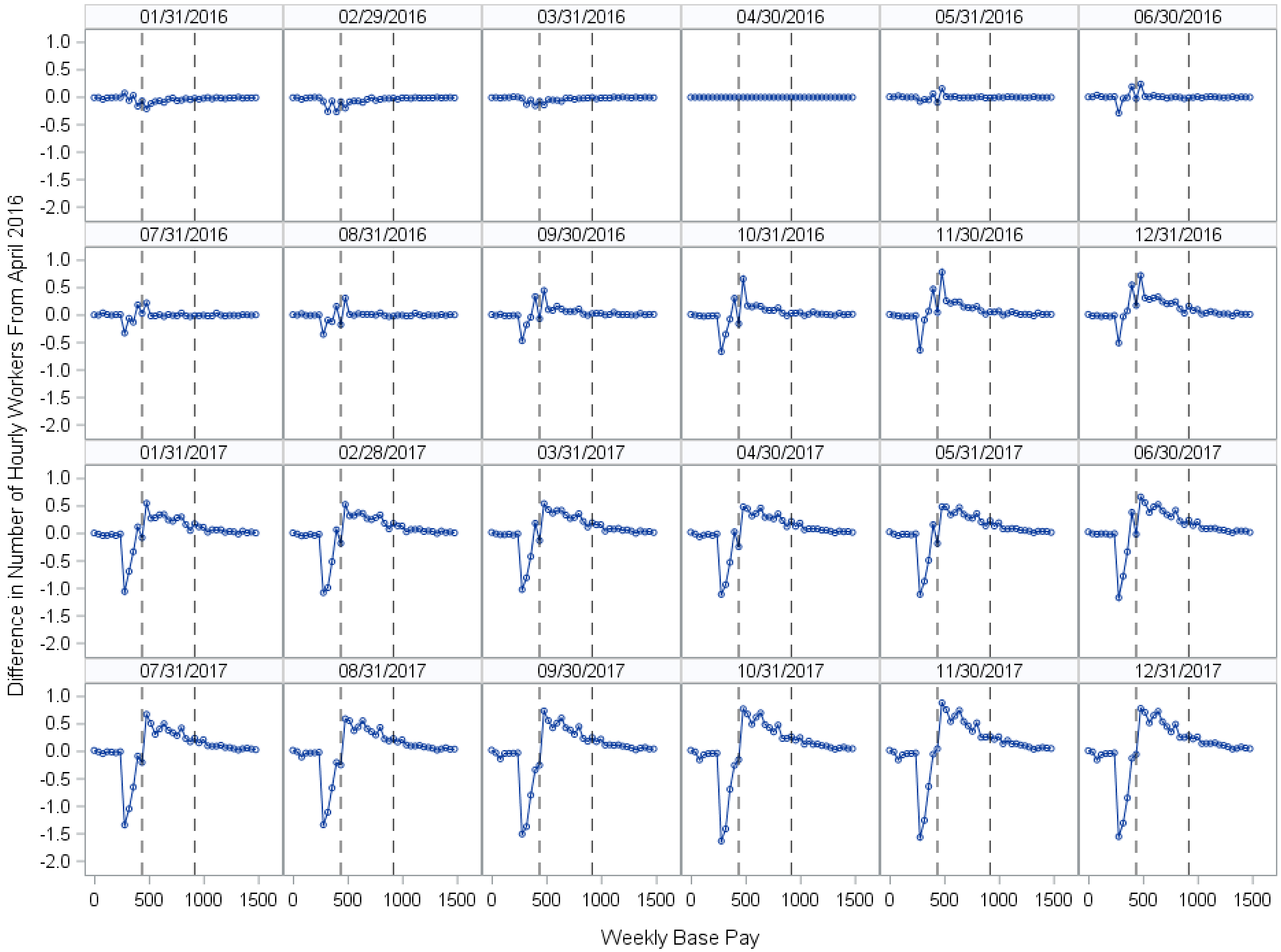
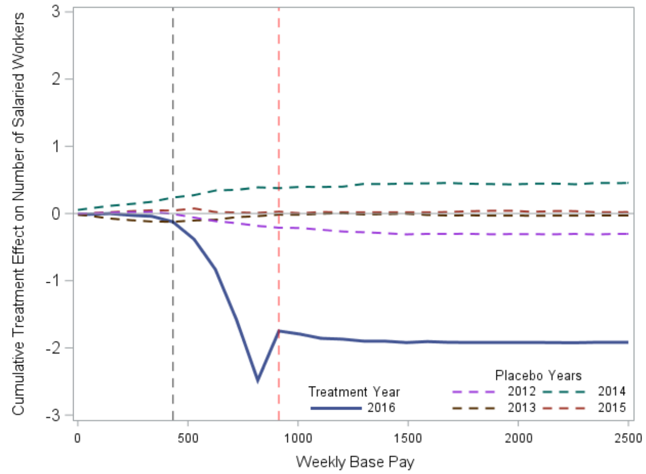


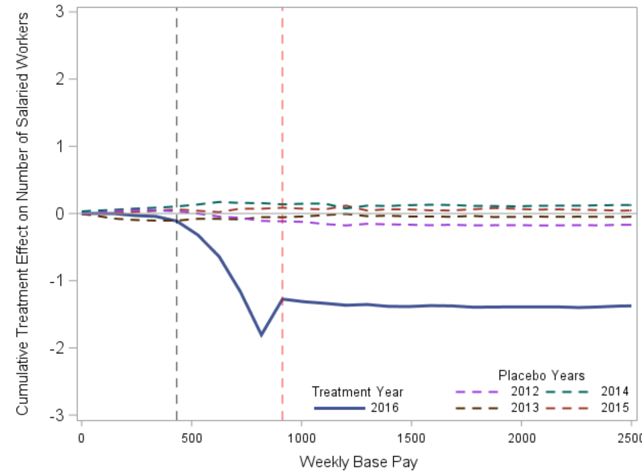
Figure A.5

Frequency Distribution of Hourly Workers by Month, Differenced by the Distribution in April 2016

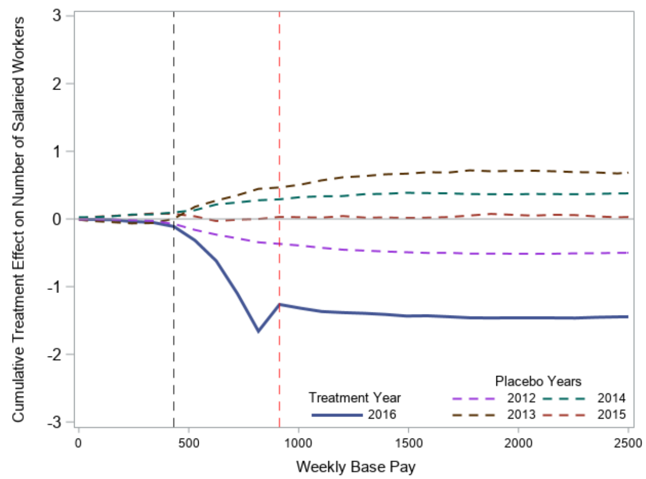
Notes. The figure shows the frequency distribution of weekly base pays of hourly workers in each month of 2016 and 2017, subtracted by the frequency distribution in April 2016. For each month, I scale the distribution by the firms that I continuously observe over the 24 months. Within each graph, the bins are \$20 wide except for the first bin which goes from \$0 to \$12.99. The left vertical dashed line is at the bin containing the overtime exemption threshold in April (\$455), while the right vertical dashed line is at the bin containing the threshold (\$913) that was supposed to go into effect on December 1, 2016.



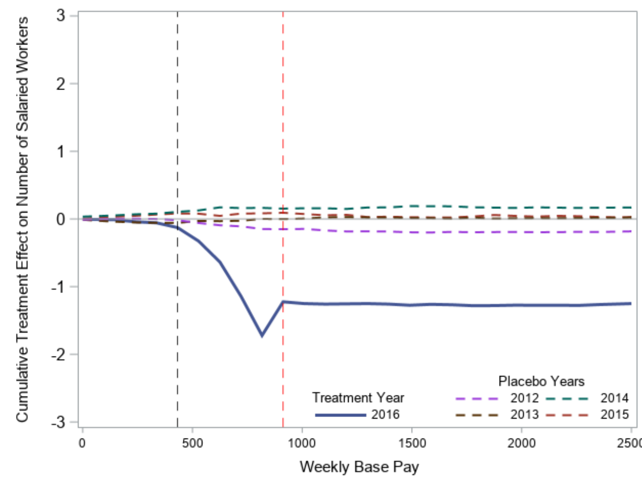
(a) Include all FLSA States



(b) Include Largest 0.1% of Firms



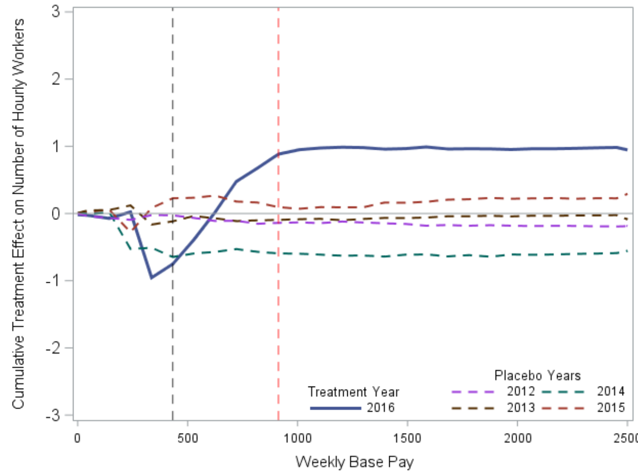
(c) Include Firm Entry and Exit



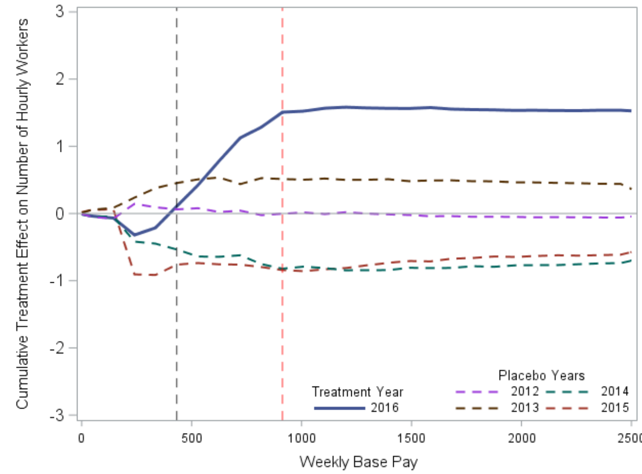
(d) Use Smaller Cutoff

Figure A.6
Robustness of the Estimated Effects on the Frequency Distribution of Salaried Workers

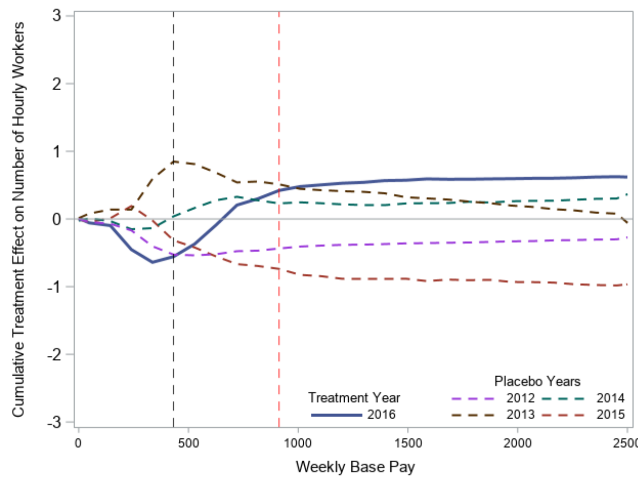
Notes. Each figure presents the cumulative treatment effect of raising the OT threshold in 2016 on the number of salaried workers and the cumulative placebo effect for years prior to 2016. The estimates are computed using equation 4 with different samples or parameters. Figure (a) estimates the bin-by-bin 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 4.



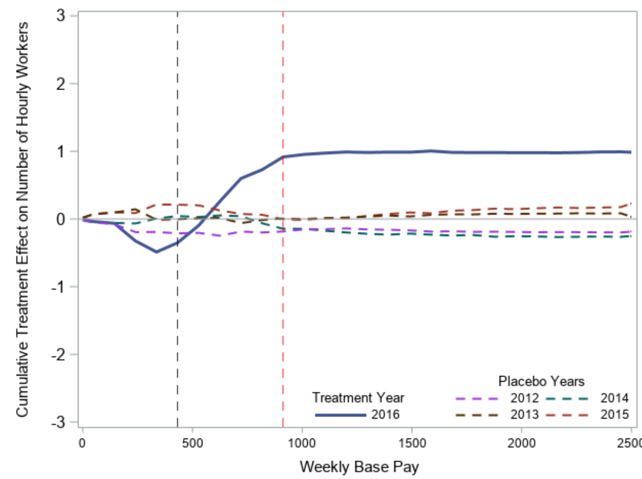
(a) Include all FLSA States



(b) Include Largest 0.1% of Firms



(c) Include Firm Entry and Exit

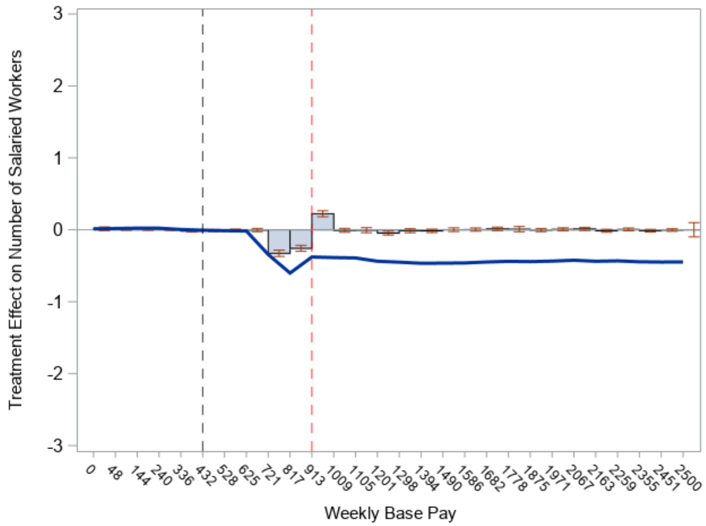


(d) Use Smaller Cutoff

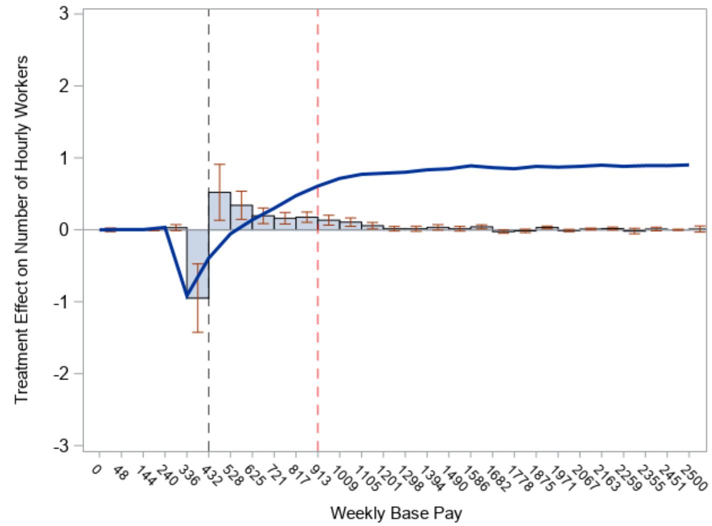
Figure A.7

Placebo Tests of the Cumulative Effects on the Frequency Distribution of Hourly Workers

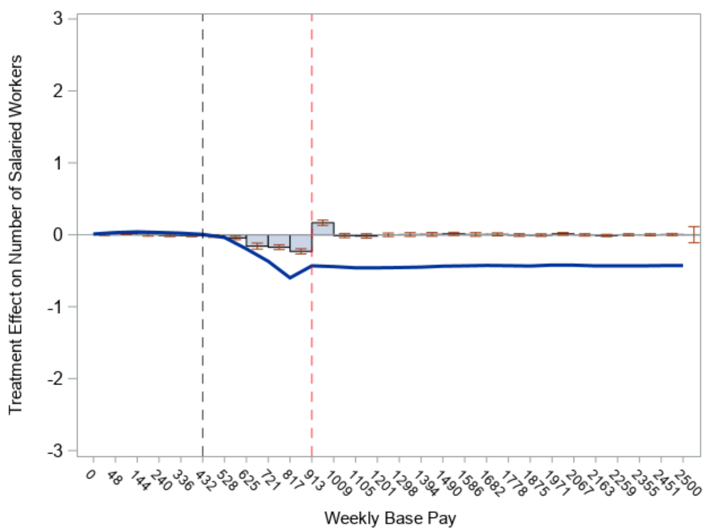
Notes. Each figure presents the cumulative treatment effect of raising the OT threshold in 2016 on the number of hourly workers and the cumulative placebo effect for years prior to 2016. The estimates are computed using equation 4 with different samples or parameters. Figure (a) estimates the bin-by-bin 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 4.



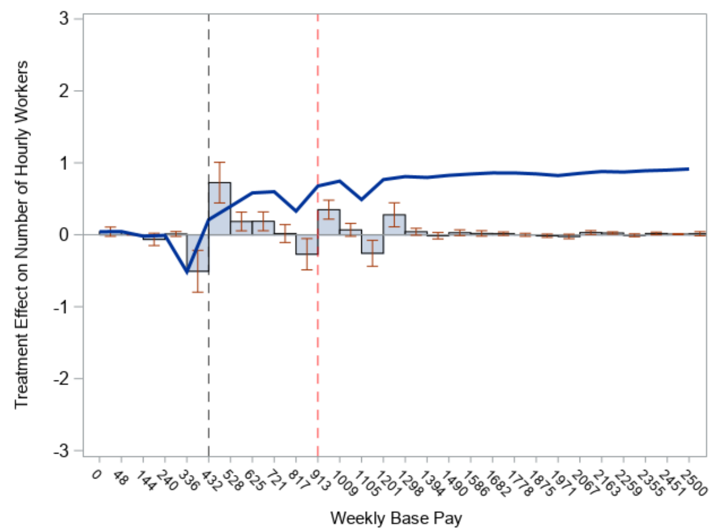
(a) Effect on Number of Salaried Jobs in CA



(b) Effect on Number of Hourly Jobs in CA



(c) Effect on Number of Salaried Jobs in NY

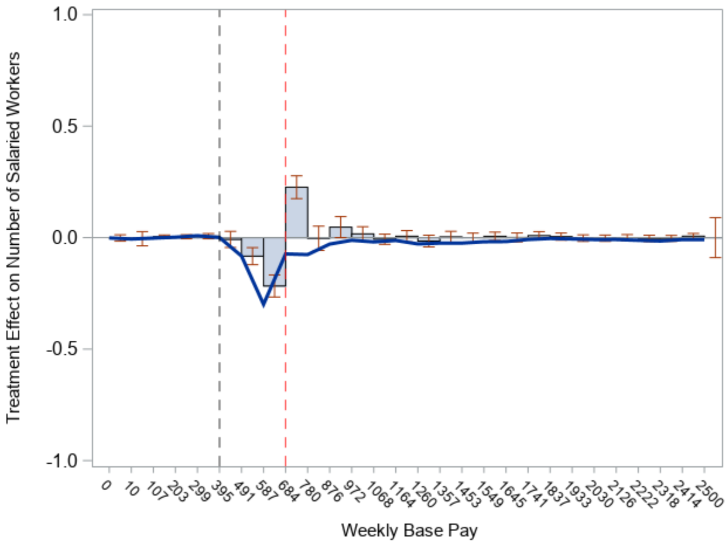


(d) Effect on Number of Hourly Jobs in NY

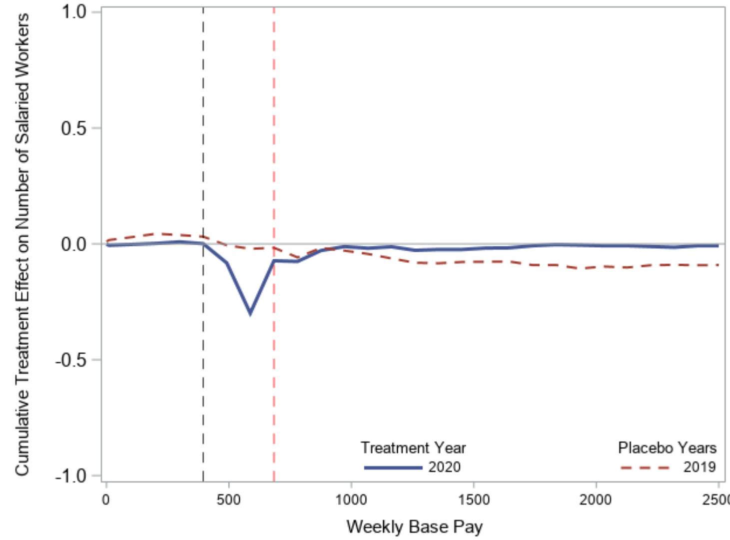
Figure A.8

Effect of Raising the 2016 OT Policy on the Frequency Distribution of Base Pay in CA and NY

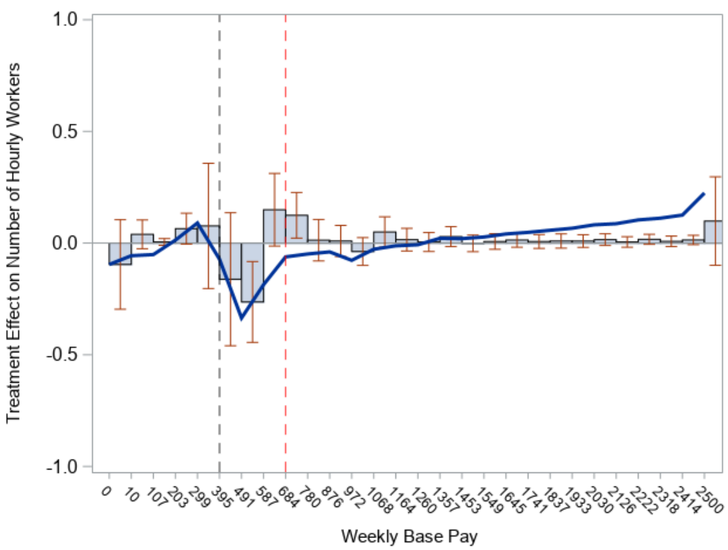
Notes. Panel (a) shows the effect of the 2016 FLSA policy on the number of salaried jobs in California, within each \$96.15 bin of base pay in Dec 2016, estimated using equation 4. The solid blue line is the running sum of these effects. Panel (b) depicts the same estimates for the number of hourly workers. Panels (c) and (d) are analogous to Panels (a) and (b) for firms in New York. In all graphs, the left and right vertical dashed lines are at the bins that contain the old and new FLSA OT exemption thresholds (\$455 and \$913), respectively.



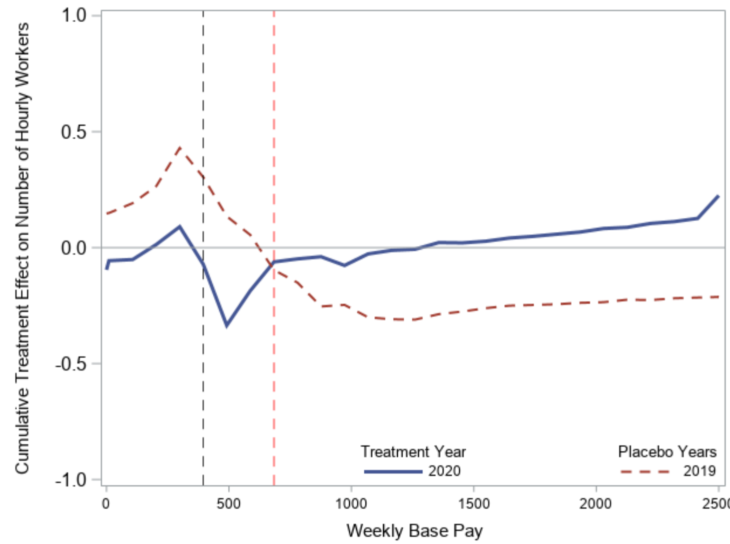
(a) Estimates of Effect on Number of Salaried Jobs



(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

Figure A.9

Effect of Raising the 2020 OT Policy on the Frequency Distribution of Base Pay

Notes. Panel (a) shows the effect of raising the OT exemption threshold on the number of salaried jobs in each \$96.15 bin in Jan 2020, estimated using equation 4. The solid blue line is the running sum of these effects. Panel (b) overlays the cumulative effects in 2020 with a placebo test of the cumulative effects in 2019. Panels (c) and (d) are analogous to Panels (a) and (b) for the distribution of hourly jobs. In all graphs, the left and right vertical dashed lines are at the old and new OT exemption thresholds (\$455 and \$684), respectively.

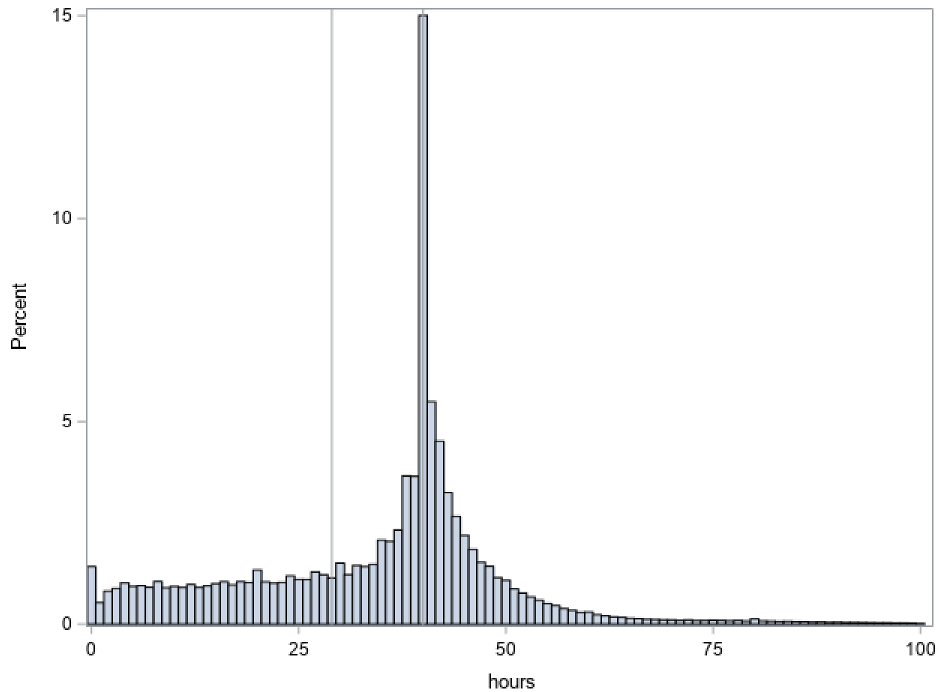
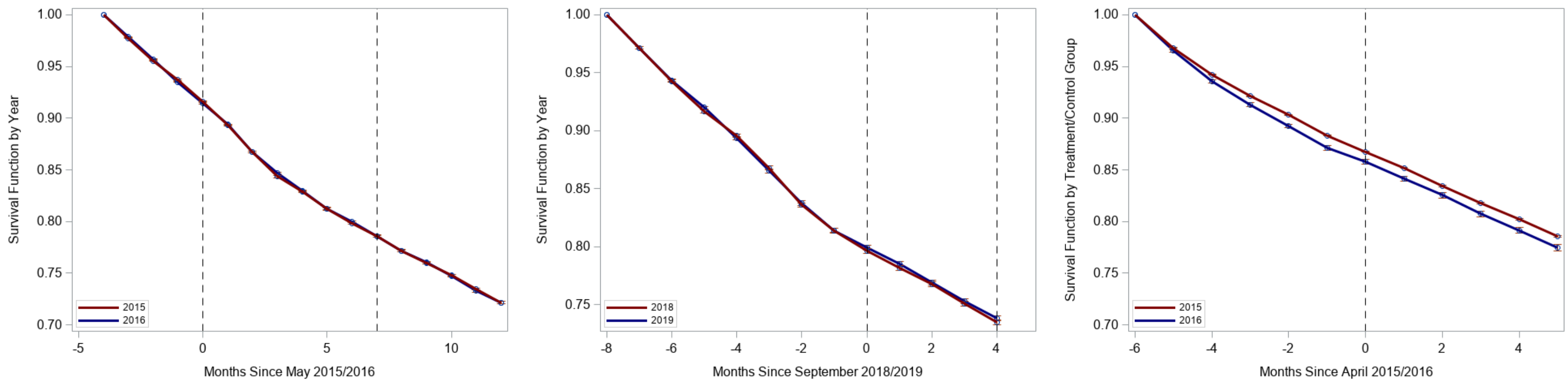


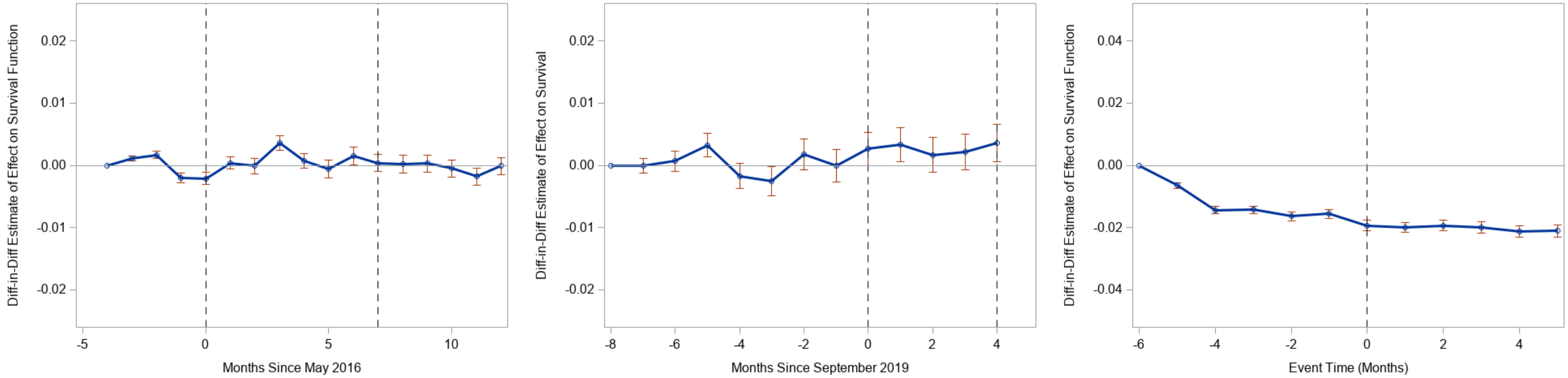
Figure A.10

Distribution of Average Weekly Hours Among Hourly Workers in April 2016

Notes. The figure shows the distribution of average workweeks in April 2016 among all hourly workers. Average weekly hours is imputed from the total hours worked in a month following the methodology in Appendix D. Each bin is a one hour increment. The left vertical line is at 29 hours and the right vertical line is at 40 hours.



(a) FLSA 2016: Survival Function (b) FLSA 2020: Survival Function (c) Event Study: Survival Function

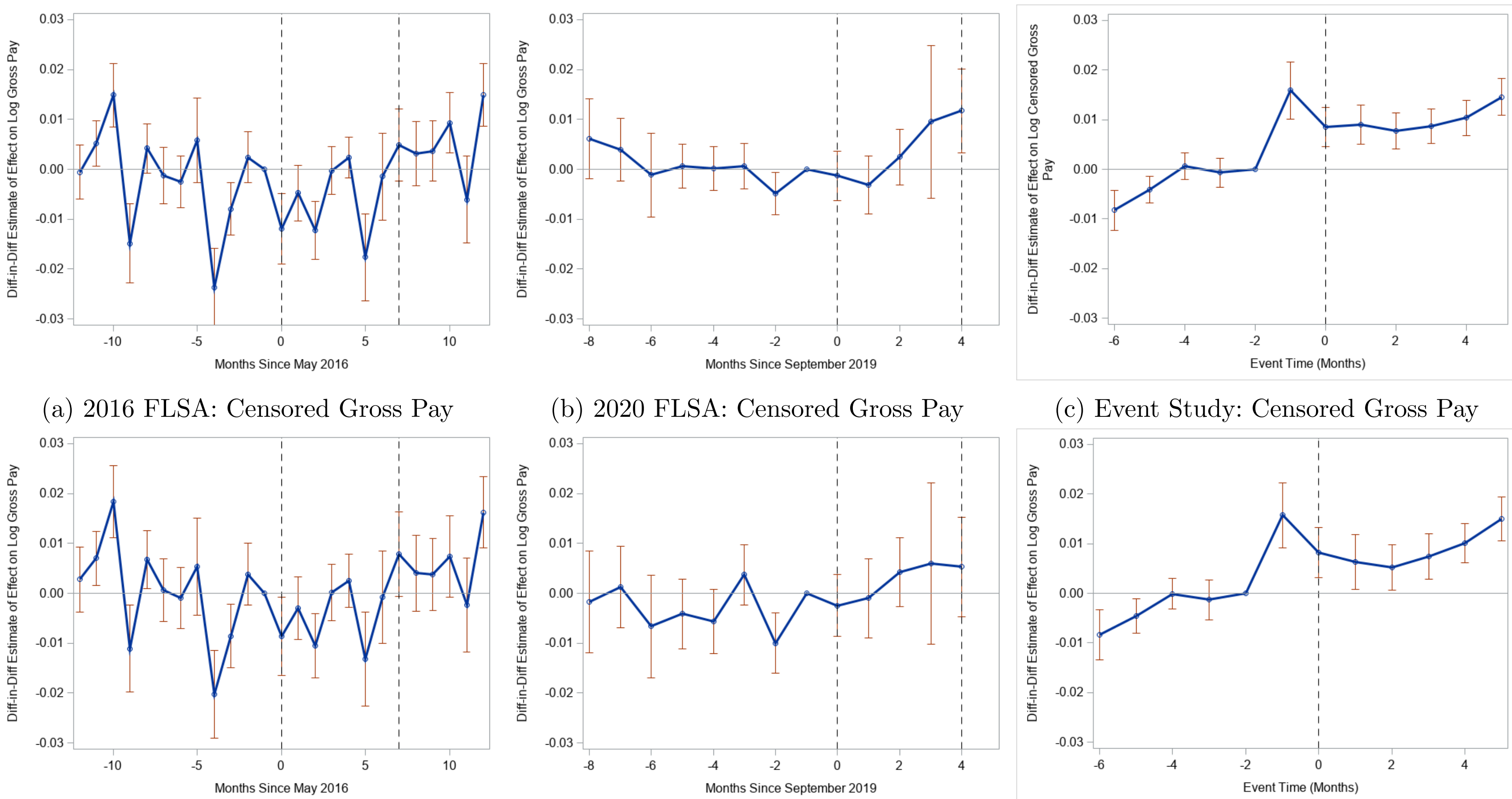


(d) FLSA 2016: Effect on Survival Function (e) FLSA 2020: Effect on Survival Function (f) Event Study: Effect on Survival Function

Figure A.11

Effect of Raising the Overtime Exemption Threshold on the Probability of Remaining in the Same Firm

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 VI. 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, whereas the second vertical line is on the month the new threshold goes into effect. For the event-study figures, the vertical line indicates the month that the new threshold becomes binding.



(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

Figure A.12
Difference-in-Difference Estimates of the Effect of Raising the OT Exemption Threshold on 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 8, 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.

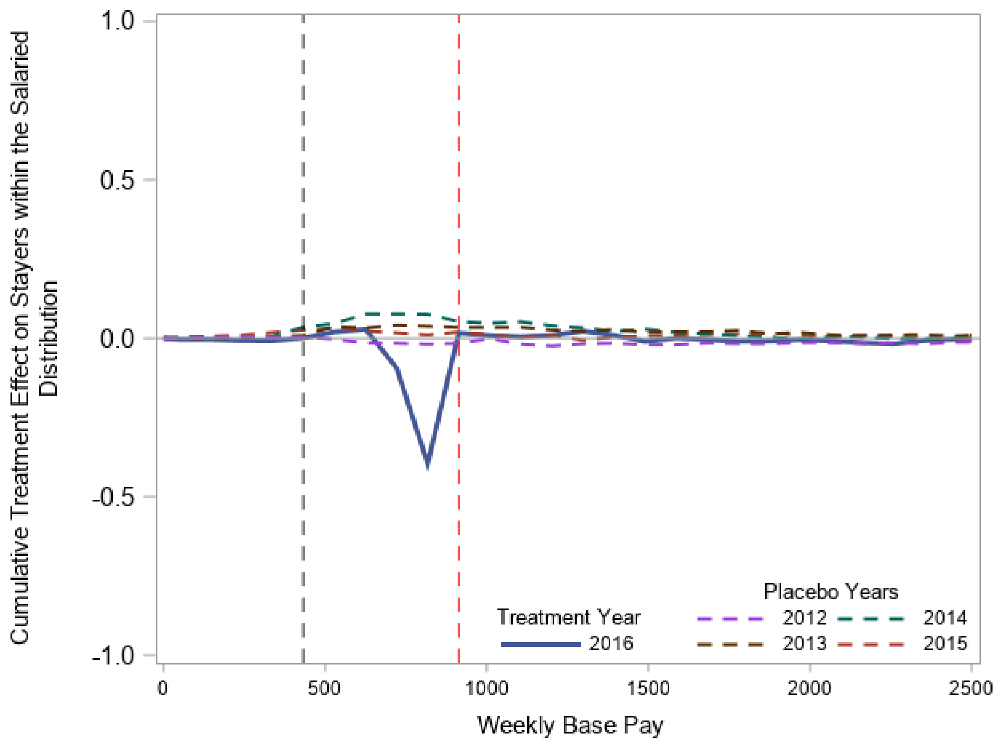
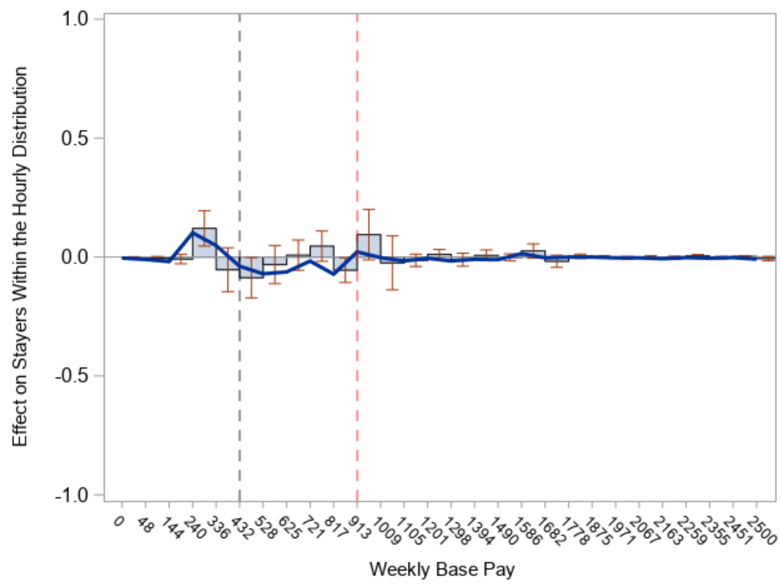
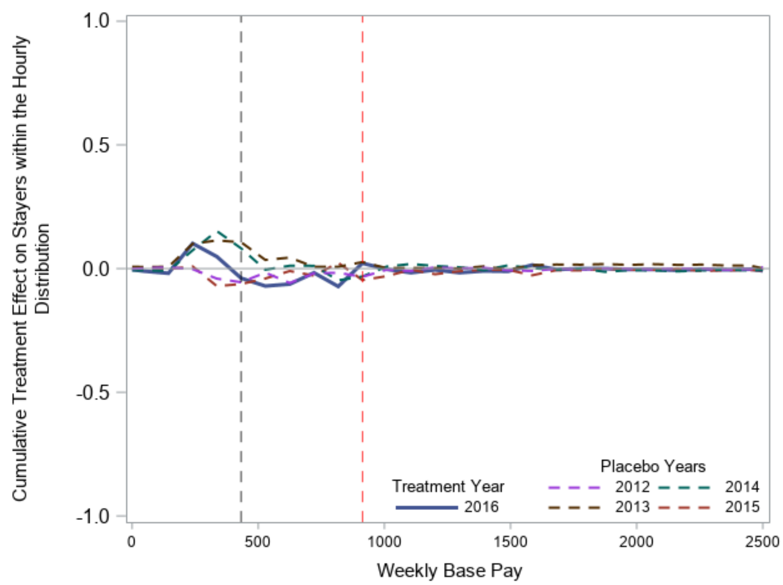


Figure A.13
Placebo Test of Effect of 2016 FLSA Policy on Always-Salaried Workers

Notes. This figure shows the cumulative effect of the 2016 FLSA policy across the base pay distribution of continuously employed workers who are salaried before and after the policy. For each year, the sum is estimated from equation 4, assuming γ_0=0 and using the workers in the preceding year as a control. The left and right vertical dashed lines are at the old and new OT exemption thresholds ($455 and $913), respectively.



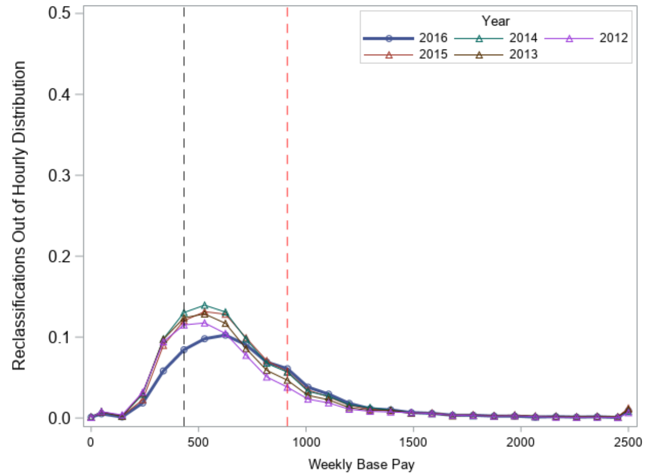
(a) Estimates of the Effect on Number of Salaried Workers



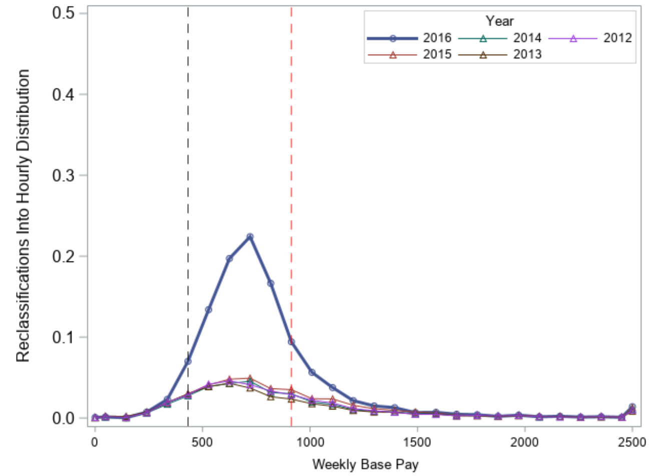
(b) Placebo Test of Effect on Salaried Distribution

Figure A.14
Effect on Flows Within the Hourly Distribution

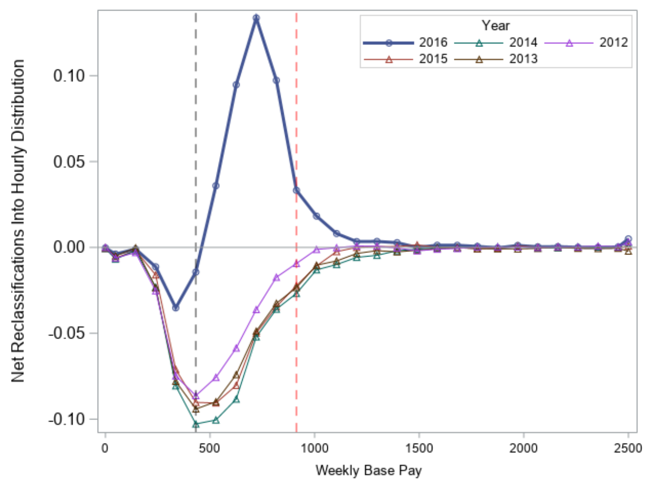
Notes. Panel (a) shows the effect of the 2016 OT policy on the distribution of workers who stay at the same firm between April and December 2016, and are paid by hour in both months, estimated using equation 4 while assuming $\gamma_0 = 0$. The solid blue line is the cumulative sum of the bin-specific 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), whereas the dotted lines 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 left and right vertical lines are at the old and new OT exemption thresholds (\$455 and \$913), respectively.



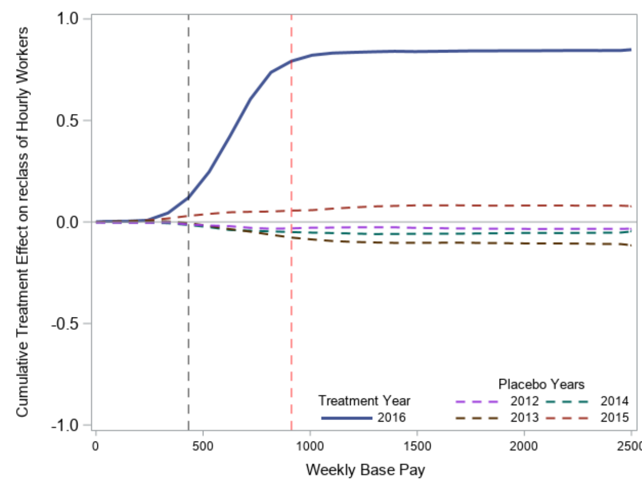
(a) Reclassifications from Hourly



(b) Reclassifications into Hourly



(c) Distribution of Net Reclassifications by Year



(d) Placebo Test of Net Reclassification Effects

Figure A.15

Effect on Reclassifications in and out of the Hourly Distribution

Notes. Figure (a) shows the frequency distribution of base pays in April of each year between 2012 and 2016, averaged across firms, for stayers who are hourly in April and salaried in December. Figure (b) shows the frequency distribution of base pays in December of each year for stayers who are salaried in April and hourly in December. Panel (c) shows the difference between Panel (b) and (a). Panel (d) presents the cumulative effects estimated from equation 4, assuming $\gamma_1 = 1$ and $\gamma_0 = 0$ for each year from 2012 to 2016 In all figures, the left and right vertical lines are at the initial and proposed FLSA thresholds, respectively.

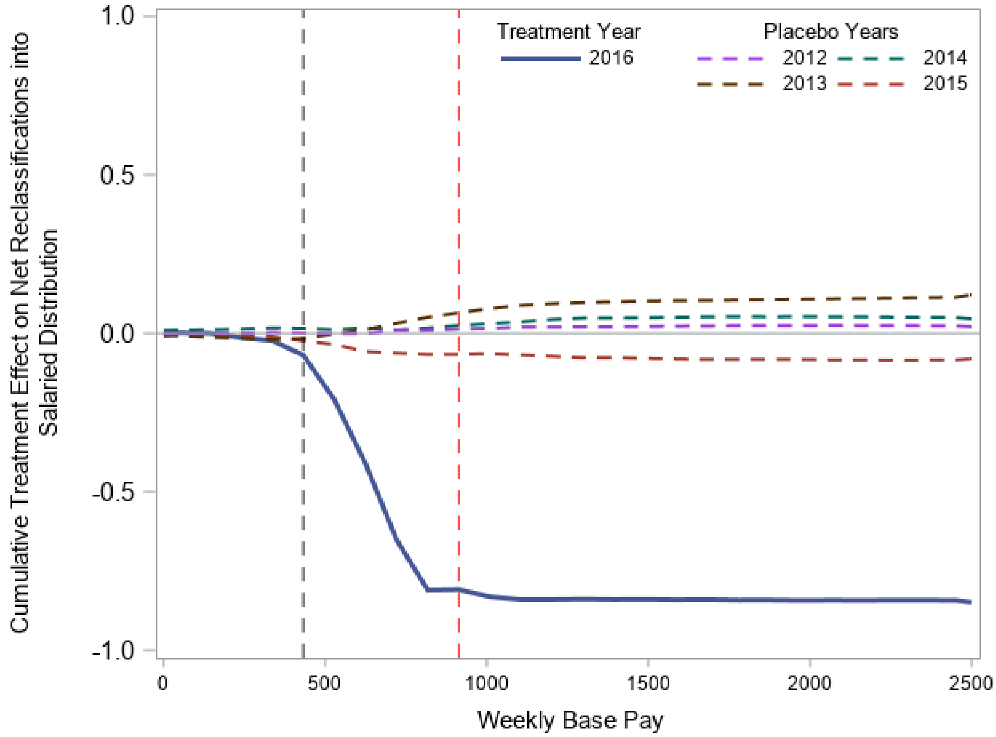


Figure A.16
Placebo Test of Net Reclassification Flows into the Salaried Distribution

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 4 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 left and right vertical dashed lines are at the old and new OT exemption thresholds ($455 and $913), respectively.

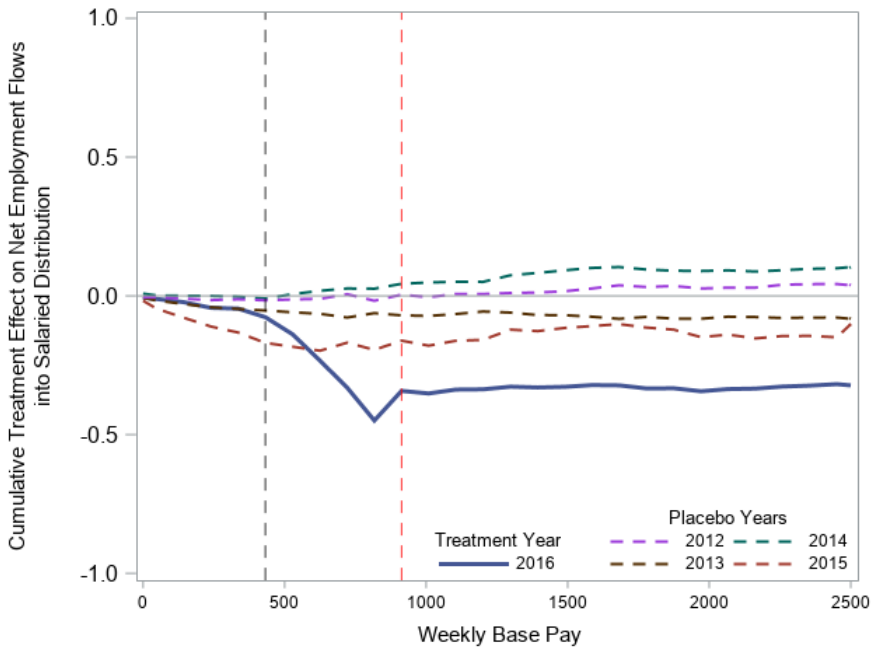


Figure A.17
Placebo Test of Net Employment Flows into the Salaried Distribution

Notes. This figure shows the cumulative sum of the estimates of equation 4 using the number of hires minus separations within each bin as the outcome variable.

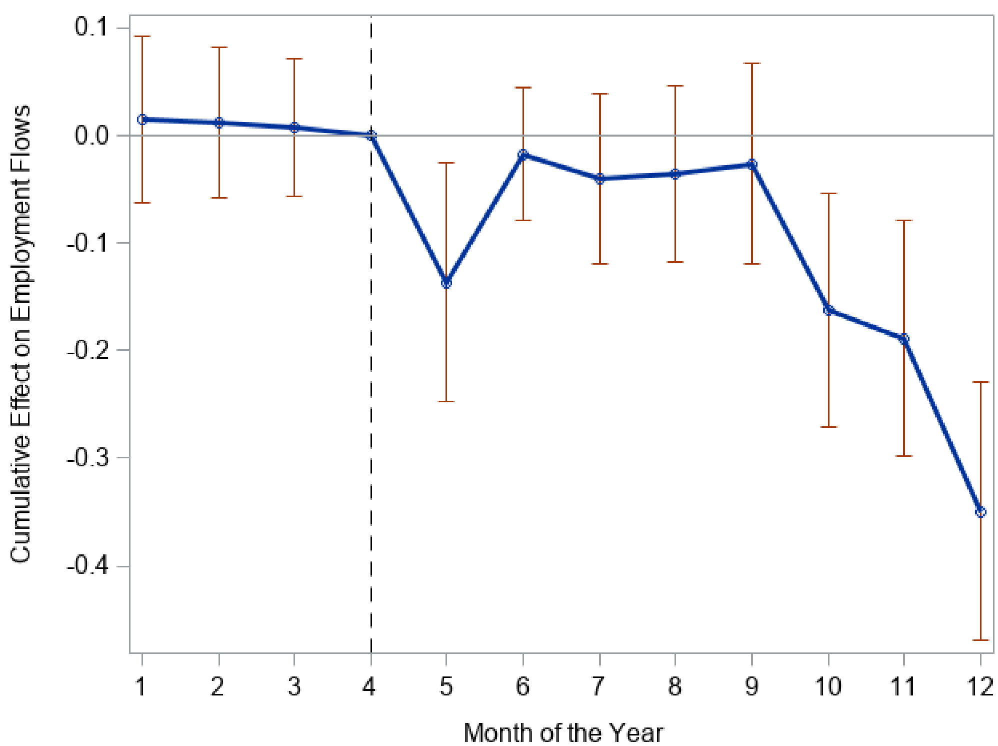
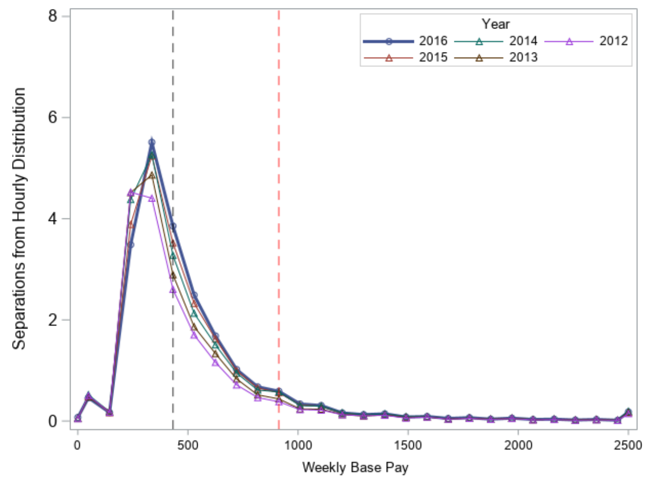
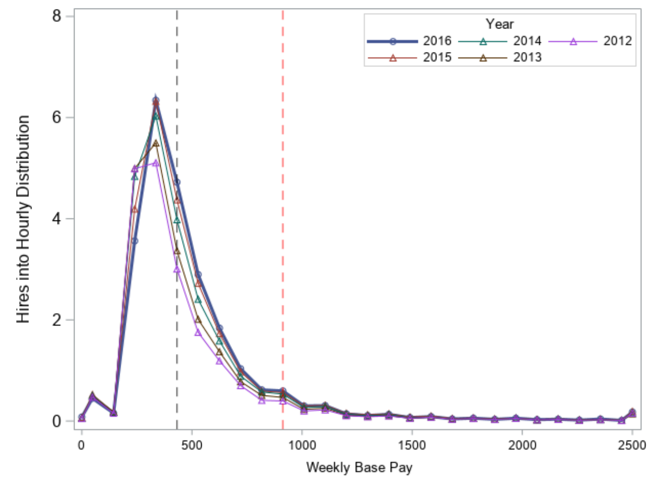


Figure A.18
Net Employment Flows into the Salaried Distribution Over Time, 2016

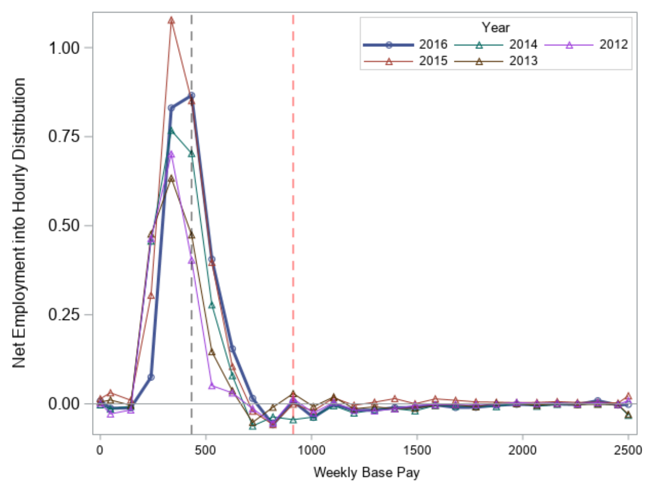
Notes. This figure shows the effect of the 2016 FLSA policy on the net employment flow since April 2016, estimated from equation 4.



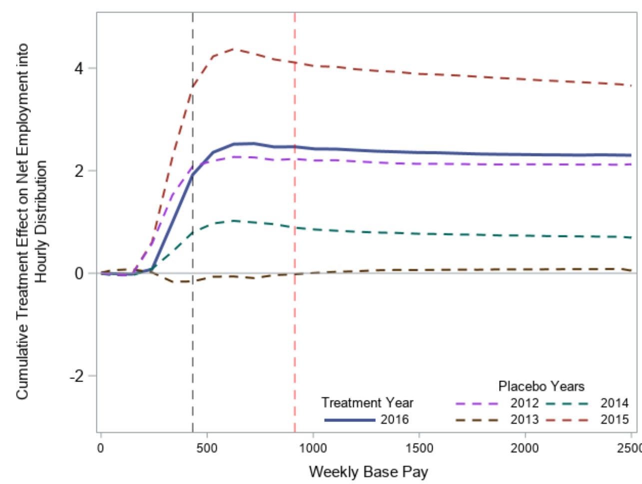
(a) Separations Between April and December



(b) New Hires Between April and December



(c) Distribution of Net Employment by Year



(d) Placebo Test of the Net Employment Effect

Figure A.19
Analysis of the Employment Flows into the Hourly Distribution

Notes. Panel (a) shows the 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 4 for each year from 2012 to 2016. In all figures, the left and right vertical dashed lines are at the initial and proposed FLSA thresholds, respectively.

Table A.1
Event-Study Estimates of Employment Flow and Reclassification Effects

	Employment			Reclassification		
	Hires	Separations	Net	Into	Out of	Net
Salaried, Below	-0.008^{***}	0.004^{***}	-0.013^{***}	-0.002^{***}	0.025^{***}	-0.027^{***}
	(0.002)	(0.002)	(0.003)	(0.001)	(0.003)	(0.003)
Salaried, Above	0.002	0.003^{***}	-0.001	0.003^{***}	0.004^{***}	0.00
	(0.002)	(0.001)	(0.002)	(0.0005)	(0.001)	(0.001)
Hourly, Below	-0.025^{*}	0.018	-0.043^{*}	0.026^{***}	0.001	0.025^{***}
	(0.014)	(0.013)	(0.025)	(0.003)	(0.001)	(0.003)
Hourly, Above	-0.006	0.005	-0.011	0.007^{***}	0.000	0.007^{***}
	(0.006)	(0.006)	(0.011)	(0.001)	(0.000)	(0.001)
Cumulative	-0.037^{*}	0.031	-0.068^{*}	0.034^{***}	0.029^{***}	0.005^{***}
	(0.020)	(0.019)	(0.036)	(0.004)	(0.004)	(0.001)
Treatment Group						
Affected Salaried	1.20	1.20	1.20	1.20	1.20	1.20
Affected Hourly	4.18	4.18	4.18	4.18	4.18	4.18
Avg. Firm Size	110	110	110	110	110	110
No. Firm-Events	183,673	183,673	183,673	183,673	183,673	183,673

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 7, 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).

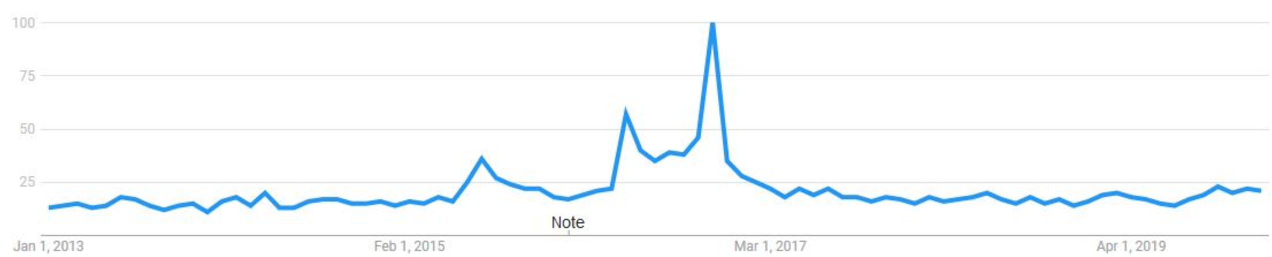


Figure B.1
Google Search Popularity for the Term "FLSA Overtime"

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 IIb).

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.[54] 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

[54]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.

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 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. Derivation of the Conceptual Framework

C.a Flexible Contracts

Firm:

$$\max_{(n,h)} \pi = xn^{\alpha}h^{\beta} - Y(h)n$$

FOC:

$$\frac{d\pi}{dh} = x\beta n^{\alpha}h^{\beta-1} - Y'(h)n = 0$$
$$\frac{d\pi}{dn} = x\alpha n^{\alpha-1}h^{\beta} - Y(h) = 0$$

Take the ratio: $\frac{\beta}{\alpha h} = \frac{Y'(h)}{Y(h)}$

Solve differential equation: $Y(h) = Ch^{\frac{\beta}{\alpha}}$

Worker:

$$\max_{h} U(h) = Y(h) - a^{-\frac{1}{\epsilon}}\frac{h^{1+\frac{1}{\epsilon}}}{1+\frac{1}{\epsilon}}$$

FOC:

$$\frac{dU}{dh} = Y'(h) - a^{-\frac{1}{\epsilon}}h^{\frac{1}{\epsilon}} = 0$$
$$\Rightarrow \frac{\beta}{\alpha}Ch^{\frac{\beta}{\alpha}-1} = a^{-\frac{1}{\epsilon}}h^{\frac{1}{\epsilon}}$$
$$\Rightarrow h = a^{-\frac{\alpha}{\epsilon(\beta-\alpha)-\alpha}}(\frac{\alpha}{\beta C})^{\frac{\alpha\epsilon}{\epsilon(\beta-\alpha)-\alpha}}$$

Equilibrium: Suppose extensive labor supply follows a function $N^s(Y,h)$. One way to motivate this is to suppose there is a distribution of reservation utilities $r_1 < r_2 < \cdots < r_M$. Let $N^s(Y,h) = j$ where $r_j \leq U(Y,h) < r_{j+1}$ be the function that maps earnings and hours to the index of the reservation utilities. Let labor demand be the function $N^d(Y,h)$ that satisfies the first order condition $\frac{d\pi}{dn} = 0$. The equilibrium condition is $N^d(Y,h) = N^s(Y,h)$. Since Y and h are both functions of C, solve for C to satisfy the equilibrium condition.

C.b Fixed Contract - Labor Supply Dominates

Hourly Workers:

To determine h, solve

$$\max_h U(h) = w[h + p(h-40)] - a^{-\frac{1}{\epsilon}} \frac{h^{1+\frac{1}{\epsilon}}}{1+\frac{1}{\epsilon}}$$

$$\frac{dU}{dh} = w(1+p) - a^{-\frac{1}{\epsilon}} h^{\frac{1}{\epsilon}} = 0$$
$$\Rightarrow h = a[w(1+p)]^{\epsilon} \tag{9}$$

To determine labor demand n^d, solve

$$\max_n \pi = x n^{\alpha} h^{\beta} - [w[h + p(h-40)] + F]n$$

$$\frac{d\pi}{dn} = x\alpha n^{\alpha-1} h^{\beta} - [w[h + p(h-40)] + F] = 0$$
$$\Rightarrow N^d(w,h,p) = \Big[\frac{x\alpha h^{\beta}}{w[h+p(h-40)]+F}\Big]^{\frac{1}{1-\alpha}} \tag{10}$$

In equilibrium, select w^* to satisfy $N^d(w, h(w,p), p) = N^s(w, h(w,p), p)$ and then input w^* into the formula for h and n.

Comparative statics: Suppose p increases from 0 to 0.5

Proposition 1. *Hours will increase, and no one will work exactly 40 hours per week.*

Proof. This follows from equation 9 and the factor that the overtime premium p only applies for hours greater than 40. ∎

Proposition 2. *Extensive labor supply rises. If there is diminishing returns to scale (i.e. $\alpha < 1$) and the fixed cost per worker F is small relative to the wage bill, then labor demand will fall. As such. wages will decrease.*

Proof. For a given w, workers' utility $U(h,p)$ increases with p so $N^s(w)$ increases.

To understand the effect on labor demand, take the derivative of equation 10 with respect to p,

$$\frac{dn^d}{dp} = \underbrace{\frac{\partial n^d}{\partial p}}_{\text{Scale Effect}<0} + \underbrace{\frac{\partial n^d}{\partial h}\frac{\partial h}{\partial p}}_{\text{Substitution Effect}} \tag{11}$$
$$= \underbrace{\frac{1}{1-\alpha}\Big[\frac{x\alpha h^\beta}{w[h+p(h-40)]+F}\Big]^{\frac{\alpha}{1-\alpha}}}_{>0}\Big[\frac{\beta x\alpha h^{\beta-1}\frac{\partial h}{\partial p}[w[h+p(h-40)]+F] - w[\frac{dh}{dp}+(h-40)+p\frac{\partial h}{\partial p}]x\alpha h^\beta}{[w[h+p(h-40)]+F]^2}\Big]$$

The numerator in the second term simplifies to

$$\begin{aligned}
&x\alpha h^{\beta-1}\Big[\Big(\beta w[h+p(h-40)]+\beta F - wh[1+p]\Big)\frac{\partial h}{\partial p} - wh(h-40)\Big]\\
=&x\alpha h^{\beta-1}\Big[\Big(\beta wh[1+p]-\beta 40wp+\beta F - wh[1+p]\Big)\frac{\partial h}{\partial p} - wh(h-40)\Big]\\
=&x\alpha h^{\beta-1}\Big[\underbrace{\Big(\beta(F-40wp)-(1-\beta)wh[1+p]\Big)\frac{\partial h}{\partial p}}_{\text{Substitution Effect}}\underbrace{-wh(h-40)}_{\text{Scale Effect}}\Big]
\end{aligned}$$

The scale effect is always negative. If we assume $F < 40wp$ (i.e. the majority of the cost of labor is via their wages) then the substitution effect is in the opposite direction as $\frac{\partial h}{\partial p}$. In the case where workers select hours, $\frac{\partial h}{\partial p} > 0$ so $\frac{dn^d}{dp} < 0$. Given that supply increases and demand fall, to satisfy the equilibrium condition $N^d(w) = N^s(w)$, it must be that w decreases.[55] ■

Salaried Workers:

To determine h, solve

$$\max_h U(h) = S(1+p\frac{h-40}{40}1[S\le\bar{S}]) - a^{-\frac{1}{\epsilon}}\frac{h^{1+\frac{1}{\epsilon}}}{1+\frac{1}{\epsilon}}$$

[55]Note that w will not decrease enough to offset the effects of overtime. From the labor supply equation, to achieve the same hours, w would need to decrease to $\frac{w}{1+p}$, but then workers' level of utility fell relative to when $p=0$ because they are being paid less per hour before 40. At this point, while N^d does not change because hours are constant, N^s has decreased, creating upward pressure on w.

such that

$$\pi = xn^{\alpha}h^{\beta} - [S(1 + p\frac{h-40}{40}1[S \leq \bar{S}]) + R]n \geq \max(\pi^{hourly}, 0)$$

where π^{hourly} would be the equilibrium profit of offering the job as hourly. If $p = 0$, then workers will work as little as possible while keeping the job salaried: $h = \left[\frac{\pi^{hourly}+(S+R)n}{xn^{\alpha}}\right]^{\frac{1}{\beta}}$. Note that this point is no greater than the firm's profit maximizing level of weekly hours, because if it were greater, the worker can simply engage in fewer hours and still achieve the same level of profits for the firm.

When $p = 0$, labor demand is perfectly elastic since no matter the S, workers choose h such that profits are fixed at $\max(\pi^{hourly}, 0)$. In equilibrium, workers continue entering the market until the reservation wage of the marginal worker is indifferent between working and not working. That is, the equilibrium salary S^* is chosen to maximize $N^S(S, h(S))$.[56] Intuitively, since firms are indifferent about the level of employment, everyone who wants a job can get one. This solution is equivalent to the worker having monopoly power to set hours and wages to maximize utility, while respecting the employer's profit constraint.

Proposition 3. *Overtime coverage (i.e. p increases from 0 to 1.5) will reduce base salaries such that there will be no effects on hours, earnings, or employment.*

Proof. Workers choose hours to maximize their utility. They can respond in one of two ways to an expansion in overtime coverage:

1. Increase their hours from the corner solution to the interior solution

$$\frac{dU}{dh} = \frac{Sp}{40}1[S \leq \bar{S}] - a^{-\frac{1}{\epsilon}}h^{\frac{1}{\epsilon}} = 0$$
$$\Rightarrow h = a\left[\frac{Sp}{40}1[S \leq \bar{S}]\right]^{\epsilon}$$

[56] Unlike the case of hourly workers where $N^S(S)$ is strictly increasing, labor supply is concave in S for salaried workers because to satisfy the firms' indifference condition, hours must rise with salaries..

2. Or decrease hours to the new corner solution $h = \left[\frac{\pi^{hourly} + (S(1+p\frac{h-40}{40}) + R)n}{xn^{\alpha}}\right]^{\frac{1}{\beta}}$. In this case, since firms' profits are again independent of their choice of employment, the equilibrium salary is chosen to maximize employment. However, the initial equilibrium was already chosen to maximize employment, in the new equilibrium, base salaries simply fall to the point where there is no change in gross earnings or hours.

Of these two options, workers will select the one that maximizes their utility. Given that employment depends on the number of individuals for whom working at S^* and h^* exceeds their reservation utility, maximizing employment is equivalent to maximizing utility. As such, workers will arrive at the second option in equilibrium. Intuitively, workers already extracted all surplus from the employment relationship to begin with, and since they behave as a monopolist in this framework, introducing the overtime premium will not change their behavior.

■

C.c Fixed Contract - Labor Demand Dominates

Hourly Workers:

To determine h and n^d, solve

$$\max_{(n,h)} \pi = xn^{\alpha}h^{\beta} - [w(h + p(h-40)) + F]n$$

First order conditions:

$$\frac{d\pi}{dh} = x\beta n^{\alpha}h^{\beta-1} - w(1+p)n = 0$$
$$\frac{d\pi}{dn} = x\alpha n^{\alpha-1}h^{\beta} - [w(h + p(h-40)) + F] = 0$$

Take the ratio of FOCs: $\frac{\beta}{\alpha h} = \frac{w(1+p)}{w(h+p(h-40))+F}$

Rearrange: $h = \frac{\beta(F - wp40)}{w(\alpha-\beta)(1+p)}$

Note that F is necessary because otherwise firms would choose $h = 0$ if $p = 0$.

Proposition 4. *If p increases from 0 to 0.5, there will be a bunching mass at 40 hours per week.*

Proof.

$$\begin{aligned} \frac{\partial h}{\partial p} &= \frac{-w^2 40\beta(\alpha-\beta)(1+p) - w(\alpha-\beta)\beta(F - wp40)}{[w(\alpha-\beta)(1+p)]^2} \\ &= \frac{-w40\beta(1+p) - \beta(F - wp40)}{w(\alpha-\beta)(1+p)^2} \\ &= \frac{-w40\beta p - \beta F}{w(\alpha-\beta)(1+p)^2} \\ &< 0 \end{aligned}$$

∎

What happens to n^d? From equation 11, the substitution and scale effects will go in opposite directions, so the effect on labor demand is ambiguous. The effect on labor supply $n^s(w, h)$ is also ambiguous since it is unclear whether the initial hours was above or below workers' preferred optimum. That said, the effect on base wages and employment are undetermined.

Salaried Workers:

To determine h, solve

$$\max_{(n,h)} \pi = xn^\alpha h^\beta - [S(1 + p\frac{h-40}{40}1[S \leq \bar{S}]) + R]n$$

such that

$$U(h) = S(1 + p\frac{h-40}{40}1[S \leq \bar{S}]) - a^{-\frac{1}{\epsilon}}\frac{h^{1+\frac{1}{\epsilon}}}{1+\frac{1}{\epsilon}} \geq r_n$$

where r_n is the reservation utility for the n^{th} worker.

Taking first order conditions:

$$\frac{d\pi}{dh} = x\beta n^{\alpha} h^{\beta-1} - \frac{Sp}{40} 1[S \leq \bar{S}]n = 0$$
$$\frac{d\pi}{dn} = x\alpha n^{\alpha-1} h^{\beta} - [S(1+p\frac{h-40}{40}1[S \leq \bar{S}]) + R] = 0$$

Taking the ratio of the FOCs: $\frac{\beta}{\alpha h} = \frac{Sp1[S\leq\bar{S}]}{40[S(1+p\frac{h-40}{40}1[S\leq\bar{S}])+R]}$

Rearrange: $h = \frac{40\beta(S(1-p1[S\leq\bar{S}])+R)}{(\alpha-\beta)Sp1[S\leq\bar{S}]}$

If $p = 0$, then for a given S and n, the firm will have workers work as many hours as possible because the marginal cost per hour of labor is zero. Firms therefore choose the corner solution where the n^{th} worker's indifference condition is just satisfied.

$$h = \left[a^{\frac{1}{\epsilon}}(1+\frac{1}{\epsilon})(S-r_n)\right]^{\frac{\epsilon}{1+\epsilon}}$$

To determine n, rearrange $\frac{d\pi}{dn} = 0$:

$$n^D(S, h(S,p), p) = \left[\frac{x\alpha h(S,p)^{\beta}}{S(1+p\frac{h(S,p)-40}{40}1[S\leq\bar{S}])+R}\right]^{\frac{1}{1-\alpha}}$$

The above equations implicitly define a downward sloping extensive labor demand function with respect to S. By definition of the corner solution for h, any point along the labor demand curve will satisfy the extensive labor supply curve $N^S(S, h(S))$. Employers therefore have monopsonistic power to choose the level of S to maximize profits.

Comparative statics: Suppose p increases from 0 to 1.5. The following statements apply to workers initially working over 40 hours per week:

Proposition 5. *If salary $S < \bar{S}$ after rule change, then hours will decrease (i.e. $\frac{\partial h}{\partial p} < 0$)*

Proof. Even though the previous equilibrium is achievable through a reduction in base pay, it is no longer incentive compatible for the firm since the marginal cost per hour of labor has increased. It is clear from the interior solution to the firm's first order condition, $h = \frac{40\beta(S(1-p1[S\leq\bar{S}])+R)}{(\alpha-\beta)Sp1[S\leq\bar{S}]}$, that hours decrease with the overtime premium. ■

Proposition 6. *If salary $S < \bar{S}$ after rule change, then employment increases as β approaches 0 and decreases as β increases, with guaranteed negative employment effects if $\beta \geq 1$.*

Proof. To determine the change in labor demand, take the derivative of $n^D(S, h(S,p), p)$ with respect to p:

$$\frac{dn^d}{dp} = \underbrace{\frac{\partial n^d}{\partial p}}_{\text{Scale Effect}<0} + \underbrace{\frac{\partial n^d}{\partial h}\frac{\partial h}{\partial p}}_{\text{Substitution Effect}}$$
$$= \underbrace{\frac{1}{1-\alpha}\Big[\frac{x\alpha h^\beta}{S(1+p\frac{h-40}{40})+R}\Big]^{\frac{\alpha}{1-\alpha}}}_{>0}\Big[\frac{x\beta\alpha h^{\beta-1}\frac{\partial h}{\partial p}[S(1+p\frac{h-40}{40})+R] - S[\frac{h-40}{40}+\frac{p}{40}\frac{\partial h}{\partial p}]x\alpha h^\beta}{[S(1+p\frac{h-40}{40}1[S\leq\bar{S}])+R]^2}\Big]$$

where $1[S \leq \bar{S}]$ is attached to every p but omitted for brevity.

The numerator in the second term simplifies to

$$x\alpha h^{\beta-1}\Big[\big(\beta[S(1-p)+R] - (1-\beta)\frac{ph}{40}S]\big)\frac{\partial h}{\partial p} - \frac{h-40}{40}Sh\Big]$$

Similar to the case for hourly workers, there are opposing substitution and scale effects. If $\beta \geq 1$, then $\frac{dn}{dp} < 0$ and no work-sharing can occur. On the other hand, if $\beta = 0$ then $\frac{dn}{dp} \propto -[p\frac{\partial h}{\partial p} + h - 40]\frac{Sh}{40}$, implying that work-sharing would occur if the the reduction in hours exceeds the amount of overtime hours. However, this must be true if $\beta = 0$ since firms would simply choose 0 hours considering that it is irrelevant to production. As a result, if β is sufficiently small, then prior to coverage, firms will extract as many hours from workers as possible but after coverage, they will reduce hours by more than the number of overtime hours. Hence, employment goes up as β approaches 0. ■

Proposition 7. *Among workers with salary $S < \bar{S}$ after rule change, the change in employ-*

ment and salary is ambiguous.

Proof. In equilibrium, salary equates extensive labor demand and supply, $n^D(S, h(S,p), p) = n^S(S, h(S,p), p)$. An increase in p decreases h and rewards workers for hours of overtime. As a result, extensive labor supply $N^S(S) = n^S(S, h(S,p), p)$ has increased for each value of S. Labor demand may either increase or decrease depending on the magnitudes of the scale and substitution effects. If labor demand increases, then equilibrium employment must rise, and the effect on base salary is ambiguous. If labor demand falls, then equilibrium employment is ambiguous while base salaries will fall. ■

Proposition 8. *Workers initially earning sufficiently close to the exemption threshold $\bar{S}$ will receive a raise to right above that threshold.*

Proof. In equilibrium, it must be that $\pi(n, S, h) \geq \pi(n, \bar{h}, \bar{S})$. If only employees earning less than $\bar{S}$ are covered for overtime, then for any n and h, there exists a $\underline{S}$ such that

$$\underline{S}(1 + 1.5\frac{h-40}{40}) = \bar{S}$$

That is, it costs the firm as much to pay each worker at a base pay of $\underline{S}$ with overtime as it does to simply pay them $\bar{S}$ and work hours $\bar{h}$. In this case, no $S \in (\underline{S}, \bar{S})$ can exist in equilibrium because firms would simply raise salaries for these jobs to the threshold. This bunching is stable because while workers are willing to accept a lower base salary to bid wages down, that is not incentive compatible for the firm. By increasing salaries, we move down the labor demand curve so the bunching effect thereby adds to any disemployment effects of overtime. ■

Proposition 9. *Some jobs will be reclassified from salaried to hourly.*

Proof. In equilibrium, for a job to be salaried, it must be that $\pi^{salaried} \geq \pi^{hourly}$. Since overtime coverage reduces the profitability of salaried jobs, occupations on the margin of being salaried will be reclassified. On the other hand, jobs that benefit greatly from being salaried (i.e. $R << 0$) will remain salaried. ■

Appendix D. Defining the Compensation Variables

D.a 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.

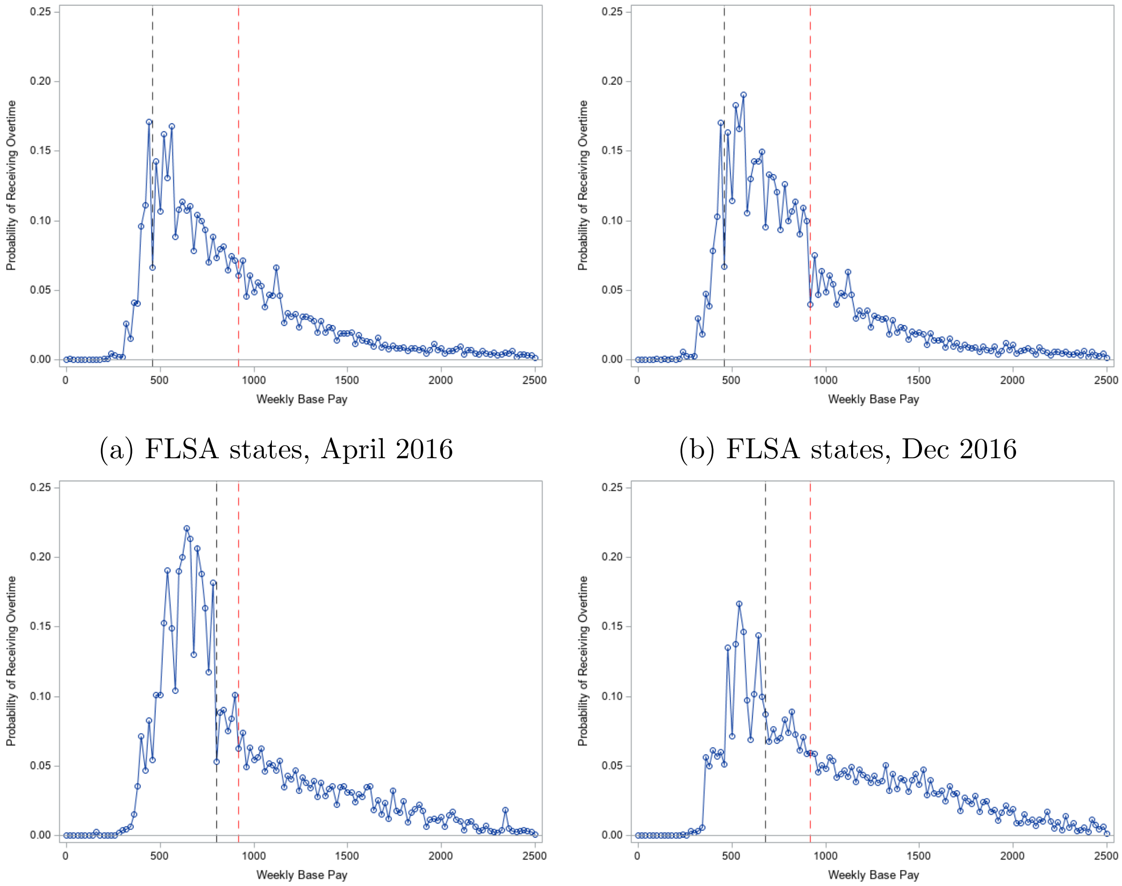
To test how often firms separately record workers' overtime pay, I calculate the sum of workers' four components of pay and find that it matches the measure of gross pay 99.8% of the time. This suggests that most employers are indeed properly recording the multiple aspects of individuals' incomes. As another test and to gauge the frequency of overtime usage, 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. This suggests that 15% of hourly employees always work overtime, while 4% only work overtime one week per month. Under that assumption, I would expect around 31% (i.e. $15+4\cdot4$) 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 in 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. To account for this, I impute overtime pay following the methodology described by Grigsby et al. (2021). 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. $\frac{\text{OT earnings}}{\text{OT hours} \cdot \text{wage}}$), 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”, 94% of hourly workers and 86% of salaried workers have implied overtime premiums within either 1.4-1.6 or 1.9-2.1.

To validate my measure of overtime for salaried workers, I plot in figure D.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 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 at exactly the threshold. The lack of a discontinuity at the $455 threshold among FLSA states in April 2016 is confounded by the fact that very few salaried workers earned below that threshold (see Appendix FIgure A.3).

D.b 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 D.1 shows the share of



(a) FLSA states, April 2016

(b) FLSA states, Dec 2016

(c) California, April 2016

(d) New York, April 2016

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

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, Maine, or Alaska, in 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.

workers with each pay frequency in April 2016, and the formula used to compute their weekly base pay, gross pay, and overtime pay.

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

Appendix Table D.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.

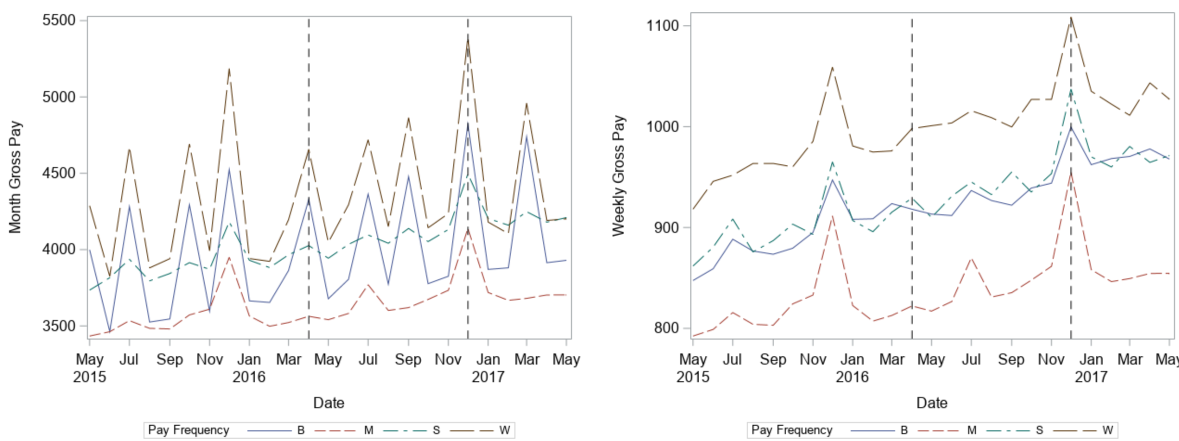
that each month is 1/12 of the year and each year has 52 weeks. Thus, weekly base pay equals standard rate of pay times $\frac{24}{52}$ for workers paid semimonthly, and standard rate of pay times $\frac{12}{52}$ for workers paid monthly.

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 semimonthly. 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 D.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.



(a) Monthly Gross Pay (b) Normalized Weekly Gross Pay

Appendix Figure D.2
Gross Income, by Pay Frequency

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.

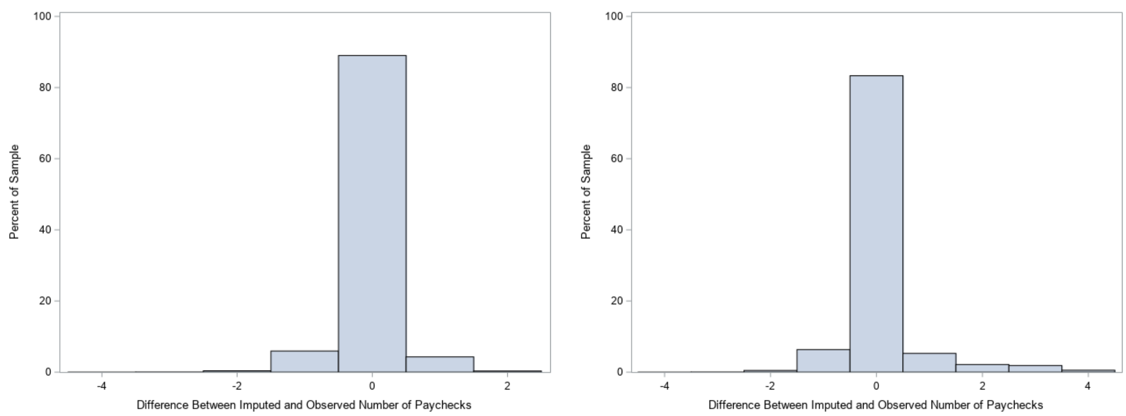
2. Within each year, for each firm-frequency, compute the median of the average gross pays across the 12 months.

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 D.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 D.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.



(a) Biweekly (b) Weekly

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

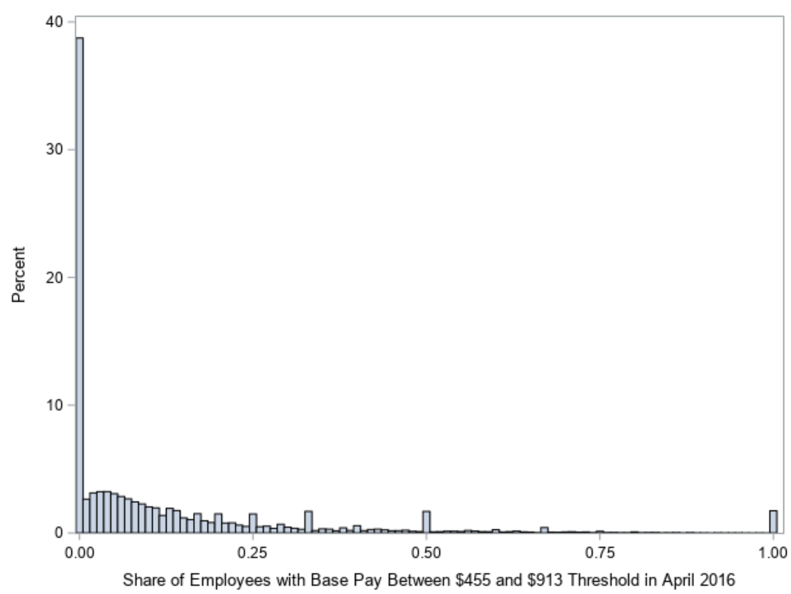
Notes. Panel (a) shows the distribution of the difference between imputed and actual number of paychecks per month, for all worker-months in 2016 where the worker is paid biweekly. Panel (b) shows a similar distribution for workers who are paid weekly.

Appendix E. Descriptive Statistics

In this section, I describe the characteristics of the firms and workers in the ADP data.

E.a Directly Affected Firms vs. Entire Sample

Figure E.1 plots the distribution of firms by their share of salaried employees between the old and new FLSA thresholds in 2016. The first feature to note is that nearly half of all firms had no salaried workers with base pays between the old and new thresholds. These firms would therefore only respond to the policy through changes in hiring decisions or spillovers from the reallocation of jobs between the directly affected firms. Aside from the large mass of firms that had no worker within the treated pay interval, most firms had between 1-25% of their workforce impacted. Reassuringly, this suggests that my estimates of the effect of the policy are determined by the large share of firms in this range and not by a couple heavily impacted employers.



Appendix Figure E.1
Distribution of Share 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.

Table E.1 describes in more detail the characteristics of firms and workers affected by each of the increases in the overtime exemption threshold. In column (1), I record the size distribution, industry mix, and worker composition among the sample of firms referenced in section V.a that were impacted by the FLSA 2016 rule change. I find that the sample comprises primarily of small and medium size firms, a quarter of firms are in professional services, and 14% of employers hire only hourly workers. Since this sample defines a firm as comprising of only employees in states that did not raise their minimum wage, the "size" of the firm is understated relative to if I included employees in all states. Nevertheless, this statistic is comparable internally to subsequent restrictions of the sample.

In column (2), I restrict the sample to only firms with at least one salary worker between the old and new thresholds. Relative to the average employer, directly affected firms are 62% larger and have a greater share of salaried workers, but follow a similar industry mix. The observation that larger firms are more susceptible to reforms in the exemption threshold follows from purely a probabilistic standpoint - firms with more employees are more likely to have at least one worker paid within any fixed interval of base pay. Given that the direct response to the rule changes are driven by large firms, there may be concern about the representiveness of my estimates. However, it should be noted that although the large firms affected by the 2016 policy only make up half the sample, they employ 84% of workers. Thus, the response of these large firms are highly relevant to the evaluation of the policy.

I repeat a similar comparison between the general sample and the directly treated firms in columns (3)-(6) for the 2020 FLSA policy and state reforms, respectively. I find a similar pattern that the policy inadvertently targets larger firms. In terms of the industry mix, I find that treated firms are much less likely to be in tradeable industries (i.e. manufacturing) and more likely to be in nontradeables such as retail and restaurant. Overall though, both samples span a variety of industries and are fairly comparable. One final point to note across all comparisons is that the vast majority of workers directly affected by changes in the exemption threshold had not previously received overtime compensation (i.e. last two rows).

Appendix Table E.1
Firms Affected by Changes in the Overtime Exemption Threshold

	FLSA 2016		FLSA 2020		State	
	(1)	(2)	(3)	(4)	(5)	(6)
Firms						
Average Size	125	202	147	260	110	231
% Firm Size: < 50	61.5	40.1	54.5	31.4	63.7	33.5
% Firm Size: 50-499	33.6	51.7	40	58.1	32.3	57.2
% Firm Size: 500-999	2.9	4.8	3.2	5.9	2.3	5.4
% Firm Size: 1000-4999	1.9	3.2	2	4.1	1.4	3.5
% Firm Size: ≥ 5000	0.1	0.2	0.2	0.6	0.1	0.3
% Tradeable	16.5	16	16.2	11.3	17.6	12.7
% Nontradeable	4.8	5.7	4.6	7.8	4.6	6.3
% Construction	11	9.9	11	9.1	10.2	8.2
% Services	27.1	26.6	27	24.4	25.5	25.7
% Education and Health	11.8	11.1	9.9	12.3	12.9	13.7
% Other	25.6	27.9	23.7	28.1	25.9	28.3
% Missing	3.2	2.7	7.6	6.9	3.2	3
Only Salaried	19.8	12.1	18.9	8.5	27.3	11.6
Only Hourly	14.1	0	10.1	0	11.9	0
Both Salaried and Hourly	62.87	87.9	70.3	91.5	60.8	88.4
Workers						
% Salaried	32.7	35.3	32.6	35.7	33.3	36.7
% Hourly	67.3	64.7	67.4	64.3	66.7	63.3
% Treated	6.7	7.9	1.4	3.0	1.1	2.2
% Treated, did not receive OT	6	7.1	1.3	2.7	1	2
No. Event-Firms	41,500	21,723	36,934	9,776	183,673	43,712
No. Event-Workers	5,203,480	4,381,227	5,414,684	2,537,314	20,138,911	10,077,154
Sample	All	Treated	All	Treated	All	Treated

Notes. The table reports the characteristics of firms and workers in the baseline month prior to each threshold change, separately for all firms and for only firms that employed salaried workers directly affected by the reform. Columns (1)-(4) report these statistics for the sample of firms described in section V.a, where employment within a firm is defined at the announcement of the threshold change and summed over all states with no minimum wage changes. Columns (5)-(6) counts employment at the state-firm level two months before a state raised its threshold, as described in section V.b. The first three group of rows report the distribution of firm sizes, industry mix, and worker composition of firms. The last group of rows report the share of workers that are salaried, hourly, treated salaried (i.e. base pay between the old and new thresholds), and treated salaried who did not previously receive overtime pay.

E.b Inferring the Cost of Expanding Overtime Coverage

In this section, I calculate the ex-ante cost of expanding overtime coverage for salaried workers. Since I do not observe the hours of salaried workers in the data, I am unable to directly compute the expected additional compensation from overtime. Instead, I will infer this expense in two ways: using the cost of overtime among hourly workers with similar income levels, and using the hours reported by salaried workers from the Current Population Survey.

To start, I argue that on average, overtime only comprises a small percent of the cost of hiring a worker. According to the BLS, overtime makes up 0.8% of total worker compensation (U.S. Department of Labor, 2019b). Similarly, Grigsby et al. (2020) find that overtime accounts for no more than 2% of gross earnings among hourly workers within the ADP data. In column (1) of table E.2, I confirm that a similar result holds true for hourly workers earning between \$455 and \$913 per week in April 2016: on average, overtime was only 2.3% of workers' total earnings in that month.

However, the small average cost of overtime masks significant heterogeneity across workers. In figure E.2, I show that a third of hourly workers do not even earn overtime and even conditional on those who do, the cost of overtime is small. Nevertheless, the long right tail indicates that there exist some workers for whom overtime is a considerable share of their income. Column (1) of table E.2 shows that for the top 10% of the distribution, overtime comprises at least 8% of workers' total monthly earnings. To show that this is not simply a result of hours having higher variance over a month than over a year, column (2) computes the cost of overtime as a share of annual income and finds very similar results.[57] Taken together, overtime appears to be a sizeable share of earnings for a small segment of hourly workers. This is despite the fact that the cost of overtime among hourly workers is calculated

[57]For example, one may be worried that if the workers who do and do not engage in long overtime hours switch each month, then within a particular month, there will be a group of workers where overtime is a large component of total earnings, but over a year, overtime is only a small part of any workers' earnings. I find that this is not the case.

as an endogenous outcome that already accounts for actions by employers to reduce hours, such as the bunching of workers' hours at 40 as depicted in appendix figure A.10. Thus, the ex-ante cost of expanding overtime coverage to salaried workers is presumably greater.

A naive way of predicting the cost of expanding overtime coverage is to calculate the share of earnings derived from overtime among existing salaried workers. In column (3) of table E.2, I do this for salaried workers with base pays between \$455 and \$913 per week in April 2016, and find that even at the 95th percentile, overtime only amounts to 6% of total compensation. However, this greatly underestimates the actual expected cost of overtime from an expansion in coverage. From appendix figure D.1, it clear that workers who are not covered for overtime are significantly less likely to receive overtime compensation.[58] Even among those who are already covered, the amount of overtime pay is attenuate by employers' decision to limit overtime hours.

To calculate how much overtime would cost if employers do not adjust their behaviors, I instead use the Current Population Survey to impute salaried workers' implied overtime pay if they maintain the same weekly earnings and hours after gaining coverage. To show that the cost of overtime in the CPS data is comparable to that in the ADP data, I first calculate the share of earnings from overtime for hourly workers. In both datasets, overtime makes up a little more than 2% of total earnings among hourly workers, but it is concentrated within a smaller share of individuals in the CPS. Next, I calculate the implied cost of overtime for salaried workers in two ways: the "fluctuating workweek" and "fixed workweek" methods. According to the FLSA, if an employer and worker agree that the base salary is intended to compensate any amount of hours worked, then overtime pay is computed using the "fluctuating workweek" method as $OT = 0.5\frac{S}{h}(h-40)$ where S is the weekly base pay and h are weekly hours. However, if such an agreement does not exist, then the base salary is assumed to only cover the first 40 hours of labor and overtime pay is calculated using the "fixed week" method as $OT = 1.5\frac{S}{40}(h-40)$. In practice, some states such as California,

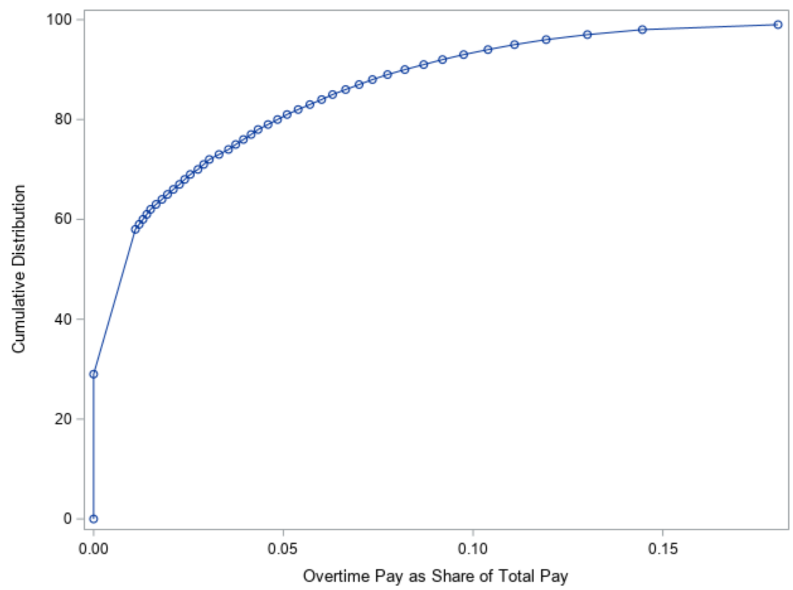
[58]In figure D.1a, there is no clear discontinuity at the \$455 threshold because the covered workers are a highly selected group with very few individuals earning less than the threshold (see A.3).

Appendix Table E.2
Overtime as a Share of Gross Earnings

	(1)	(2)	(3)	(4)	(5)	(6)
75th percentile (%)	3.0	3.7	0.0	0.0	0.0	0.0
80th percentile (%)	5.0	4.6	0.0	1.3	5.6	17
90th percentile (%)	8.0	7.2	1.4	8.3	10	30
95th percentile (%)	11	9.5	5.8	14	17	50
Mean (%)	2.3	2.3	0.8	2.2	3.0	8.0
Data	ADP	ADP	ADP	CPS	CPS	CPS
Sample	Hourly	Hourly	Salaried	Hourly	Salaired	Salaired
Time Frame	Month	Annual	Month	Week	Week	Week

Notes. The table shows the average the share of gross earnings derived from overtime, along with the 75th, 80th, 85th, 90th, and 95th percentiles. Column (1) reports the share of earnings from overtime in April 2016 for hourly workers earning between \$455 and \$913 per week. Column (2) calculates the overtime as a share of annual income from April 2015-2016, for continuously employed hourly workers in the same income bracket. Column (3) reports the share of earnings from overtime in April 2016 for salaried workers in the treated interval. Column (4) infers the cost of overtime as a share of weekly earnings for hourly workers in the CPS. Column (5) and (6) computes the expected increase in weekly earnings once treated salaried workers gain overtime, using the flexible and fixed workweek methods, respectively.

Alaska, New Mexico, and Pennsylvania require employers to apply the more expensive fixed week method, and the data suggests that employers seldom use the fluctuating workweek method. Nevertheless, I compute the implied cost of overtime coverage using both formulas. Under the fluctuating (fixed) workweek method, average labor costs would rise by 3% (8%), but for the top 10% of overtime users, income would increase by at least 10% (30%). In summary, while overtime appears to be relatively cheap on average, I expect it to be very expensive for at least a tenth to a fifth of affected salaried workers. These are the jobs that would be expected to disappear from an expansion in overtime coverage.



Appendix Figure E.2
Share of Earnings from Overtime, Hourly Workers

Notes. The figure shows the cumulative distribution of the share of earnings in April derived from overtime among hourly workers with base pays between \$455 and \$913 per week.

Appendix F. Derivation of Estimators

F.a Derivation of Equation 4

If the coefficients in equation 3 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 3, I estimate the β_{jk}'s as the difference between $\Delta \bar{N}_{jkt}$ and $\hat{\alpha}_{jkt}$. ■

F.b 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:

$$N^j_{Dec} - N^j_{Apr} = \underbrace{N^{kj}_{S_0,S_1} - N^{jk}_{S_0,S_1}}_{\text{Within Classification } (\Delta N_{S_0,S_1})} + \underbrace{N^{kj}_{S_0,H_1} - N^{jk}_{S_0,H_1}}_{\text{Reclassifications } (\Delta N_{S_0,H_1})} - \underbrace{N_{S_0,u_1}}_{\text{Separations}} \tag{12}$$

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 the counterfactual absent the policy (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 12 into the selection bias term:

$$\begin{aligned} E[\Delta N^C_{16,S_0,S_1}] - \gamma E[\Delta N^C_{15,S_0,S_1}\Big] &= E\Big[\Delta N^C_{16,S_0}\Big] - \gamma E\Big[\Delta N^C_{15,S_0}\Big] && \text{(All Incumbents)} \\ &- (E\Big[\Delta N^C_{16,S_0,H_1}\Big] - \gamma E\Big[\Delta N^C_{15,S_0,H_1}\Big]) && \text{(Reclassifications)} \\ &+ (E\Big[N^C_{16,S_0,u_1}\Big] - \gamma E\Big[N^C_{15,S_0,u_1}\Big]) && \text{(Separations)} \end{aligned}$$

Given the assumptions in section V.a, 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 VII.b (see figure XIa). In that case, from the same argument as above, the transformed distribution of separations in 2015 is equivalent to the counterfactual distribution in 2016:

$$\begin{aligned} E\Big[N^C_{16,S_0,u_1}\Big] - \gamma E\Big[N^C_{15,S_0,u_1}\Big] &= E\Big[N^C_{16,S_0,u_1}\Big] - E\Big[N^C_{16,S_0,u_1}\Big] \\ &= 0 \end{aligned}$$

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 VII.b. Workers reclassified as a result of the policy earned a similar base pay pre-and-post policy (see figure Xc). 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 by the above argument where the transformation of 2015's distribution models the distribution in 2016:

$$E\Big[\Delta N^C_{16,S_0,H_1}\Big] - \gamma E\Big[\Delta N^C_{15,S_0,H_1}\Big] = E\Big[\Delta N^C_{16,S_0,H_1}\Big] - E\Big[\Delta N^C_{16,S_0,H_1}\Big]$$
$$= 0$$

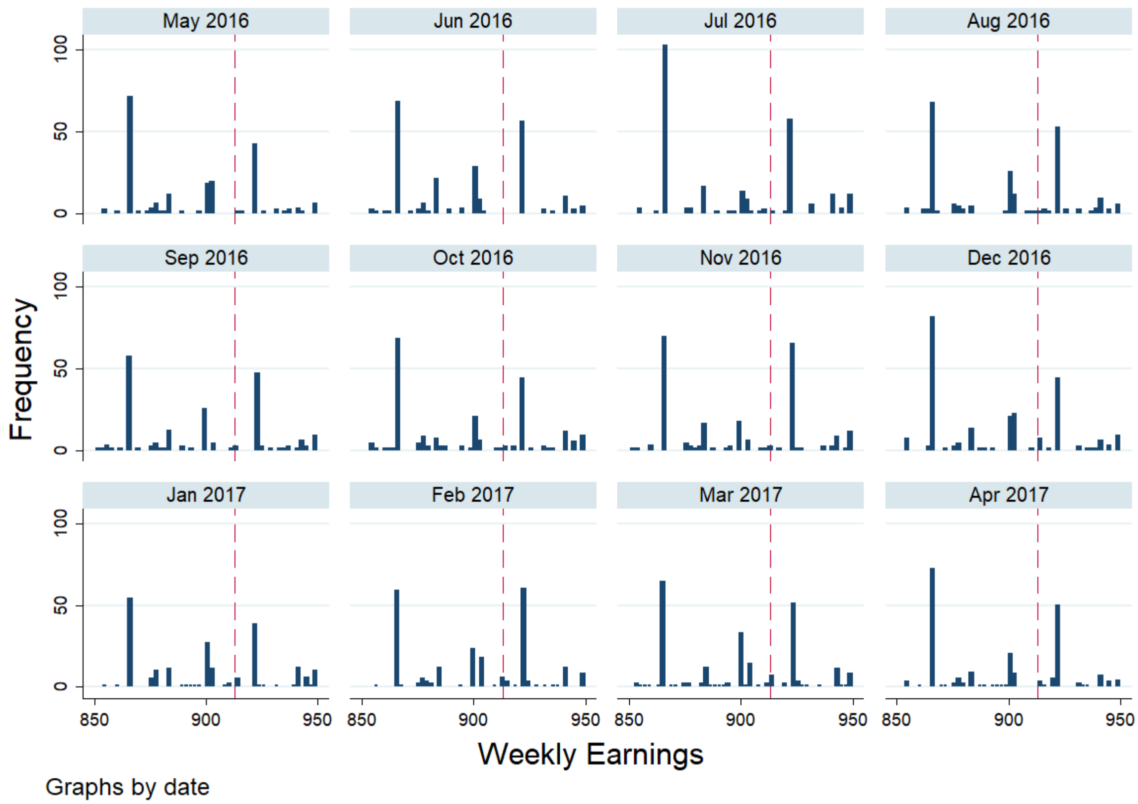
If all these assumptions hold, then $E\Big[N^C_{16,S_0,u_1^T}\Big] - \gamma E[\Delta N^C_{15,S_0,S_1}\Big]$ is an unbiased estimator of the effect of the policy on the distribution of always-salaried workers.

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.

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 IIa and IIb, 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



Appendix Figure G.1
Frequency Distribution of Salaried Workers' Weekly Earnings

Notes. This figure shows the frequency distribution of respondents' usual weekly earnings reported in the CPS, aggregated into $2 Bins. 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.

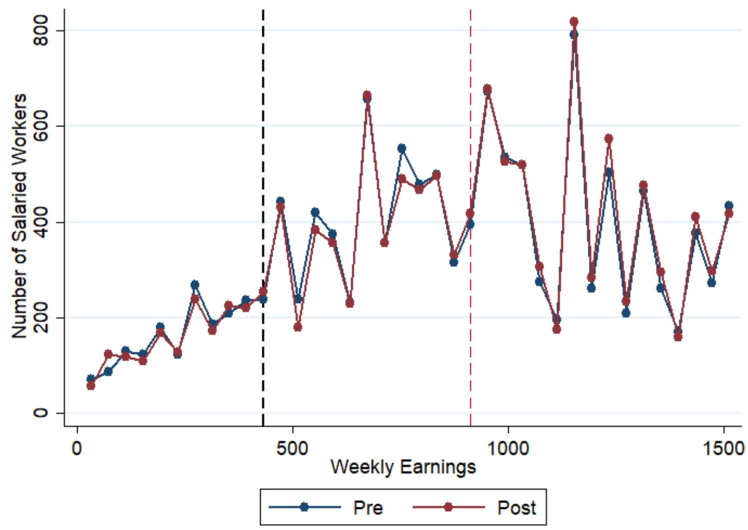
$912 and an increase in the number of workers earning exactly $913 from 2015 to 2016, the same is also true from 2014 to 2015. Overall, I am unable to find definitive evidence of large bunching using the CPS data.

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.

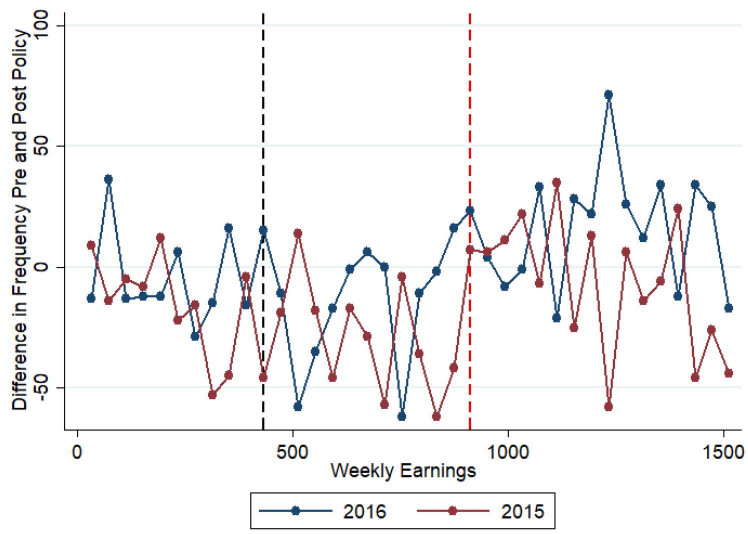
In figure G.3, I plot the proportion of respondents earning who report being paid per hour. I find no visible evidence of a trend break in the probability of hourly status between May 2016 and December 2016 for those earning between \$400 and \$1000 per week. To control for time-specific effects, I estimate 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. However, while figure G.4 shows a large jump in hourly workers among the September to November 2016 respondents, there is no trend break in the share of workers who transition to hourly status in December 2016. 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. Nevertheless, 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 or hours effects from the cohort-by-cohort difference-in-difference.

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 IV, 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. 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.



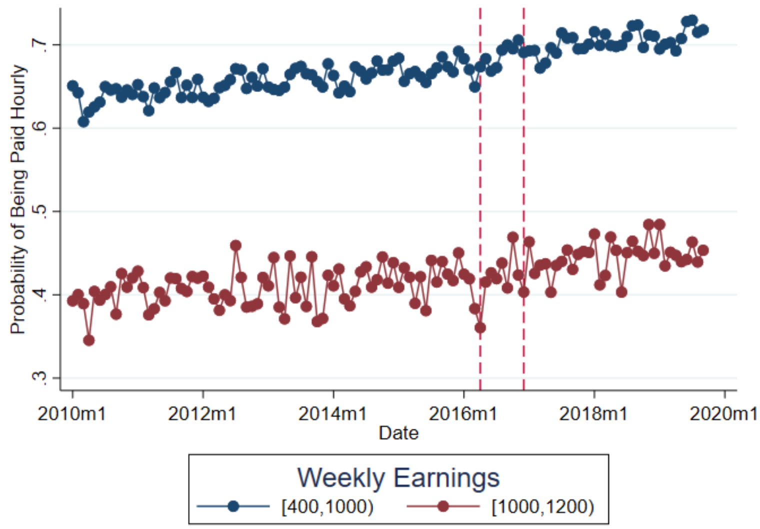
(a) Distribution of Salaried Workers' Weekly Earnings



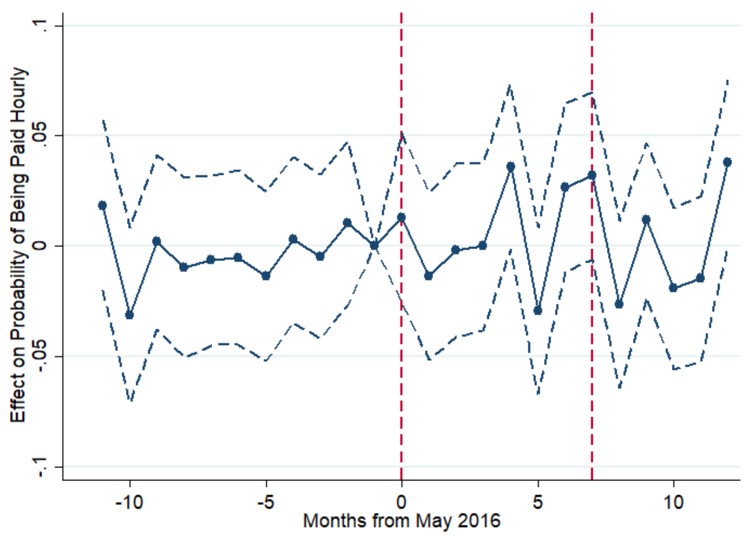
(b) Difference in Distribution Pre and Post Policy

Appendix Figure G.2
Change in the Pay Distribution of Salaried Workers in the CPS

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.



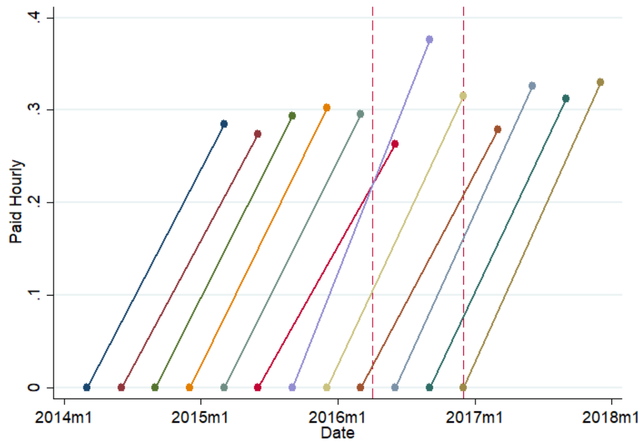
(a) Probability of Being Paid Hourly, by Date



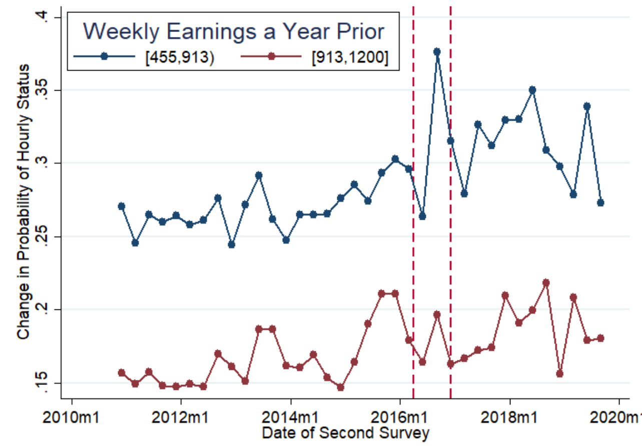
(b) Diff-in-diff for Hourly Status Indicator

Appendix Figure G.3
Diff-in-Diff of Probability of Being Paid Hourly

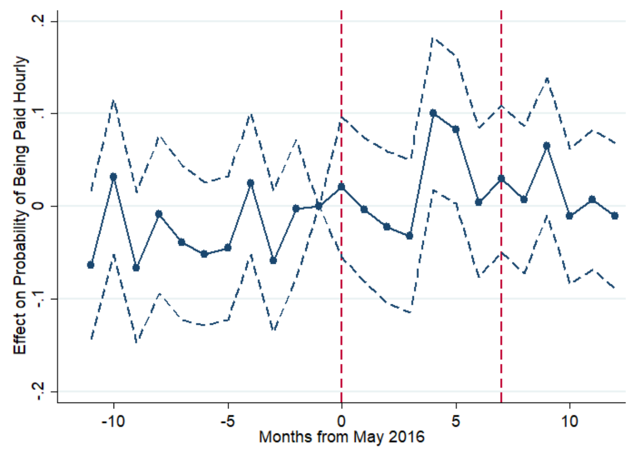
Notes. Panel (a) shows the probability that an individual in the CPS is paid an hourly wage for each monthly survey between January 2010 and September 2019, conditional on weekly earnings. The two dotted vertical lines are at May 2016 and December 2016, respectively. Panel (b) shows the difference in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



(a) Hourly Status, by Cohort



(b) Change in Hourly Status, by Earnings



(c) Difference-in-Difference of Hourly Status

Appendix Figure G.4
Panel Analysis on the Probability of Changing Hourly/Salaried 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.