THE LABOR MARKET EFFECTS OF EXPANDING OVERTIME COVERAGE*

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Abstract

Estimating the labor market impact of overtime regulations has been hindered by a lack of policy variation and high-quality data. This paper uses exogenous changes in salary thresholds for overtime eligibility in the U.S. (2014-2020), along with high-frequency administrative payroll data covering over a tenth of the U.S. labor force, to empirically assess the effects of overtime coverage. While overtime was originally intended to raise employment by encouraging firms to replace long workweeks with more workers, a competing theory predicts that employers would instead reduce base pays to offset the additional labor costs. Contrary to the two theories, the analysis finds that expanding overtime coverage decreases employment and increases earnings. The positive income effect primarily benefits middle income workers, whereas the employment loss falls on lower paying jobs.

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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 wage and hour regulations has 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 despite its potentially large impact on workers' hours, earnings, and employment. In 2019, while only 2% of workers in the U.S. earn the federal minimum wage (BLS, 2019), over half of U.S. workers are guaranteed a 50% overtime wage premium for each hour worked above 40 per week (DOL, 2019a). The broad extent of overtime coverage induces a larger transfer from employers to workers each year, with firms paying \$53 billion in overtime compensation in 2019 - that is 60% more than the amount paid in taxes to fund the entire unemployment insurance systems (DOL, 2019b).

Despite the prevalence of overtime regulations, research on its effects have been hindered by a shortage of policy changes and the unavailability of data to accurately measure their impacts. In the U.S., expansions in overtime coverage to additional industries and demographic groups over the past 80 years often coincided with expansions in the minimum wage. As a result, previous papers are confronted with the challenge of separately identifying the effects of overtime on income and employment. Apart from a shortage of clean variation, 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. In light of these empirical challenges, a review by Brown and Hamermesh (2019) concludes that "no study presents estimates of effects [of overtime coverage] on employment, and none offers evidence on all outcomes: [wages, earnings, and hours]".

My paper overcomes prior empirical limitations by leveraging anonymous administrative payroll data that covers over one tenth of the U.S. labor force to evaluate the effect of recent federal and state expansions in overtime eligibility 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 workers who earn above it are legally exempt. Between 2014 and 2020, there were two federal rule changes and sixteen state reforms that raised the overtime exemption threshold.[1]

To estimate the labor market effects of raising the overtime exemption threshold, I imple-

[1] Although the 2016 federal rule change was overturned a week before it went into effect, I find that firms nevertheless responded to the policy as if it was binding.

ment an event-study difference-in-difference design that exploits the timing of the state and federal rule changes, along with the high-frequency nature of the data, to construct counterfactual outcomes. Intuitively, my analysis compares the evolution of employment and earnings in states that increased their thresholds to those that did not. However, given that the federal reforms affected all states simultaneously, I apply a complementary approach using observations in the year preceding each federal rule change as a counterfactual. In both cases, I estimate the impact of the policy along the entire pay distribution and, as a placebo test, show that the threshold changes had no effect outside the salary interval directly targeted by the policy (Cengiz et al., 2019).

I report four set of results. First, on the exact month that the overtime exemption threshold increases, the number of salaried workers earning between the old and new thresholds falls by 17% (s.e. 0.7%). Three responses explain the dramatic change in the pay distribution of salaried jobs. 1) Half the decrease in jobs below the new threshold is 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. 2) About a quarter of the missing mass were jobs 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. 3) The remaining missing mass, 4.3% (s.e. 2.2%) of affected jobs, were lost due to a reduction in employment.[2] In comparison to the decrease in employment, I estimate that the income of affected workers increased on average by 1.3% (s.e. 0.1%).

Second, the ratio of the employment and income effects imply that the number of affected jobs falls by 3.36% (s.e. 1.71%) for each percent increase in workers' incomes. In comparison, point estimates of the own-wage elasticity of employment across studies of the minimum wage range between -3 and 1.5, with a median elasticity of -0.17 (Dube, 2019). Taken together, the 95% confidence bound of my estimate suggests that relative to raising the minimum wage, expansions in overtime coverage cost at least as many jobs for similar wage increases. To explain the large elasticity, I simulate the mechanical costs of overtime using workers' reported hours and assuming no behavioral change. The simulation finds that the cost of overtime to the firm varies significantly across workers. In particular, if eliminated jobs are selected from the top of the hours distribution, then the ex-ante cost of overtime for displaced jobs is actually an order of magnitude larger than the realized gains for stayers.[3]

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

[3]As direct evidence that firms view overtime as costly for selected jobs, I show that among the jobs affected by the large 2016 federal rule change, firms were willing to raise some workers' salaries

Third, I show that raising the overtime exemption threshold is actually counter-redistributive, benefiting middle income workers at the expense of lower paying jobs.[4] Focusing on the large 2016 federal rule change, I show that the largest gain in income accrued to the 5% of affected workers who received a raise right above the new threshold but would otherwise have earned right below it. These bunched workers experienced a median pay increase of 7%. In comparison, the employment loss primarily fell onto jobs paying below the range that got bunched. As a result, raising the 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.

Fourth, to understand the mechanisms driving the labor market effects of overtime coverage, I use the empirical results to test three general theories of wage and hour determination and show that the evidence is consistent with a labor demand model whereby employers unilaterally set workers' hours. If wages and hours are determined jointly by the market, then compensating differential forces would reduce base pays to fully offset the costs of overtime coverage, leading to no real effects on the price or quantity of labor (Trejo, 1991). Due to a lack of policy changes, prior support for the compensating differentials model have relied on a negative cross-sectional correlation between wages and overtime hours (Trejo, 1991; Barkume, 2010). In contrast, using the threshold changes as a natural experiment, I find no evidence that firms cut salaries to negate the effects of overtime. Alternatively, if workers select their own hours, then a labor supply model predicts that no person eligible for overtime would ever choose to work exactly 40 hours per week (Idson and Robins, 1991). However, consistent with previous work, I find significant bunching in the distribution of hours at precisely 40 per week, suggesting there exists hour constraints within firms (Estevão et al., 2008; Goff, 2020).[5] Indeed, I find a labor demand model where firms choose hours and employment to maximize profits can explain the bunching, reclassification, and employment effects observed in the data. Within that framework, expansions in overtime coverage increase the cost of salaried jobs paid below the exemption threshold, thereby leading firms to reallocate away from those positions.

The empirical findings provide evidence against the historical policy intent that overtime coverage would encourage firms to create new jobs to substitute for a reduction in hours

by up to 25% to keep them exempt from overtime, despite most workers experiencing no change in base pay.

[4]In comparison, studies of the minimum wage generally find that it reduces inequality (Lee, 1999; Autor et al., 2016).

[5]Constraints by firms on workers' hours has also been proposed to explain the dynamics of hours among job-switchers (Altonji and Paxson, 1992), differences between micro and macro labor supply elasticities (Chetty et al., 2011), and the responsiveness of hours to tax reforms (Labanca and Pozzoli, 2022).

(Ehrenberg, 1971).[6] Through the lens of the labor demand model, the effect of overtime coverage on the number of jobs is ambiguous due to opposing substitution and scale effects (Trejo, 2003). Given limitations in variation and data, tests of the work-sharing hypothesis of overtime in the U.S. have focused solely on its effects on workers' hours (Costa, 2000; Hamermesh and Trejo, 2000; Johnson, 2003; Trejo, 2003). Outside the U.S., rather than expansions in coverage, papers have instead examined policies that shortened the length of the standard workweek, and found a mix of zero and negative employment effects (Hunt, 1999; Crépon and Kramarz, 2002; Skuterud, 2007; Chemin and Wasmer, 2009; Sanchez, 2013). However, studies of reforms that shortened the workweek often lack adequate data to compute precise elasticities or examine the redistributive properties of overtime. To my knowledge, my paper provides the first causal estimates of the employment and income effects of overtime coverage in the U.S., and the first evaluation of its efficiency and equity implications in any context.

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. 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 pay workers one and a half times their regular rate of pay for each hour worked above 40 in a week.[7] While the overtime premium applies to nearly all hourly workers in the U.S., the FLSA exempts a large group of salaried workers who are considered executive, administrative, or professional employees. To exempt a salaried employee, an employer must show that the worker performs primarily

[6] Similar arguments persist in modern proposals to promote work-sharing through the unemployment insurance system (Abraham and Houseman, 2014).

[7] 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.

white-collared duties, and earns a salary equal to or greater than the "exemption threshold" set by the Department of Labor (DOL).[8] Since the FLSA's overtime exemption threshold is not adjusted for inflation, the share of salaried workers earning less than that threshold, and thereby guaranteed overtime coverage, fell from over 50% in 1975 to less than 10% in 2016 (see Appendix Figure A.1).[9] 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 rule 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. 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 the unexpected ruling occurred only one week before the policy was to go into effect, many companies reported that they had either already responded to the policy, or made promises to employees that they intended to keep.[10] Following the retraction of the 2016 rule change, the DOL debated a smaller increase to the FLSA overtime exemption threshold and eventually raised the threshold to \$684 per week effective January 1, 2020. For my analysis, I examine both the nullified 2016 proposal and the binding 2020 rule change to estimate the short-run effects of a federal expansion in overtime coverage for salaried workers.

To complement my evaluation of the federal rule changes, I also implement an event study using 16 state-level increases to the overtime exemption threshold between 2014 and 2020. Similar to the minimum wage, multiple states impose their own exemption thresholds that exceed the one set by the FLSA. I present in Figure I all state and federal thresholds from 2005 to 2020, along with the invalidated proposal in 2016.[11] My state-level analysis

[8]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.

[9]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%.

[10]For example, Walmart and Kroger raised their managers' salaries above the \$913 threshold and did not take back those raises after the injunction (*WSJ Dec 20, 2016*). For a detailed history of the events leading up to and following the injunction, refer to appendix section B.

[11]I exclude from my event study the four most recent rule changes in Alaska that cumulatively

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.[12] 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 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 pay distribution. Second, since the federal policies occur in 2016 and 2020, a falsification test should detect zero effects of the federal rule changes in all other years.

III Theoretical Predictions

To guide my empirical analysis, I present a framework of wage and hour setting that nests three prominent models of overtime developed in the literature: a labor supply model, a labor demand model, and a compensating differentials model. I show in my framework that the effects of overtime differ depending on whether workers choose their own hours, employers set workers' hours, or earning-hour profiles are determined jointly by the market. I use my framework to generate testable predictions of each case.

III.a Set-up

Firm. The employer's production function depends on both the number of workers n and the number of hours per worker h. Following standard parametrizations (Ehrenberg, 1971; Hart, 2004), I characterize the firm's profit function by

$$\pi = xn^{\alpha}h^{\beta} - Y(h)n \tag{1}$$

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.

[12] 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, my analysis treats the highest threshold within each state as if it is binding for all employers. I show that my results are robust to restricting the sample to only events without within-state variation.

where the parameter α introduces decreasing returns to scale, β allows for nonlinear returns to long work weeks, x is the price of the firm's output, and $Y(h)$ is the cost of hiring a worker for h hours.

The cost structure of a job depends on whether it is a salaried or hourly position:

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

Hourly workers are paid a wage w for each hour of labor and receive a premium $p = 0.5 \cdot 1[h > 40]$ for each hour worked above 40 in a week. Salaried workers receive a fixed base pay S regardless of their hours and are only eligible for overtime compensation if their base pay is below the exemption threshold $\bar{S}$. I introduce the fixed costs F and R to capture the reduced-form value of classifying a job as salaried or hourly. For example, R encompasses the benefits (e.g. more flexibility, no need to monitor hours, etc.) and costs (e.g. easier to shirk, less control, etc.) of paying a worker by salary.[13] I assume the firm chooses whether a job is salaried or hourly.

Workers. There are M equally productive workers who differ only in their reservation wages, ordered by $r_1 < \cdots < r_M$. Workers' payoff from a job with weekly earnings Y and hours h equals the utility of consuming their income minus the disutility from working:

$$U(Y,h) = Y(h) - a^{-\frac{1}{\epsilon}} \frac{h^{1+\frac{1}{\epsilon}}}{1+\frac{1}{\epsilon}} \tag{3}$$

where ϵ represents the intensive labor supply elasticity and $Y(h)$ is the earnings from working h hours. I define extensive labor supply to be the number of workers for whom utility at (Y, h) is at least equal to their reservation wage: $N^s(Y, h) = j$ such that $r_j \leq U(Y, h) < r_{j+1}$.

Equilibrium. A market equilibrium is characterized by a set of hours, salaries, and wages (h^*, S^*, w^*) such that

1. Hours maximize workers' utility in the labor supply model (i.e. $\frac{\partial U}{\partial h} = 0$), and maximize profits in the labor demand model (i.e. $\frac{\partial \pi}{\partial h} = 0$).
2. Labor demand $N^d(Y^*, h^*)$ satisfies the firms' first order condition (i.e. $\frac{\partial \pi}{\partial n} = 0$).
3. The firm cannot increase profits by switching salaried/hourly classification.

[13]The salaried-hourly decision can be formally motivated by an agency problem where firms choose 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 on the costs of overtime.

4. The firm cannot increase profits by raising salaries (i.e. $\pi(S^*, h^*) \geq \pi(S, h^*)$ for every $S > S^*$). If decreasing base salaries reduces profits, then demand can exceed supply. Otherwise, the market clears: $N^d(Y^*, h^*) = N^s(Y^*, h^*)$.

The first three conditions ensure that agents have no incentive to deviate hours, employment, or job classification in equilibrium. The last condition determines the equilibrium salary level by moving along the labor demand curve until either the market clears, or it is no longer profitable for firms to cut base salaries, which may occur if bunching base pays at the overtime exemption threshold is cheaper than paying overtime. I now examine how the equilibrium outcomes respond to an expansion in overtime coverage under three different assumptions about the manner in which hours are determined.

III.b Labor Supply Model

Suppose workers unilaterally sets hours as in traditional labor supply models (Blundell and MaCurdy, 1999). Taking wages and salaries as given, workers choose weekly hours to satisfy the first order condition implied by equation 3.

$$a^{-\frac{1}{\epsilon}}h = \begin{cases} w(1+p) & \text{if hourly} \\ \frac{S}{40}p1[S < \bar{S}] & \text{if salaried} \end{cases}$$

Proposition 1. *If workers set their own hours, then workers eligible for overtime would never work exactly 40 hours per week.*

Proof. See Appendix C.a. ■

By introducing a kink in employees' budget constraint, workers are never indifferent to working exactly 40 hours per week. Although the policy variation targets salaried jobs, the first proposition nevertheless provides a clear null hypothesis that can be tested simply from cross-sectional data on hourly workers.

Proposition 2. *If workers set their own hours, an exogenous increase in p for salaried jobs will increase hours and reduce base pay. Effects on employment are ambiguous.*

Proof. See Appendix C.b. ■

An expansion in overtime coverage for salaried workers can be interpreted as an increase in the premium p from 0 to 1.5 for all jobs with base pays less than $\bar{S}$. Since workers choose hours, the larger overtime premium incentivizes them to work more hours. Moreover, workers now receive higher net earnings for any combination of base pay S and hours h, leading to an increase in extensive labor supply. On the other hand, labor demand falls due to both a scale effect from the increased cost of labor and a substitution effect from the increase in hours. The combination of an increased labor supply and decreased labor demand implies a fall in base salaries and ambiguous employment responses.

III.c Labor Demand Model

Next, suppose firms unilaterally set hours. This setting would be an extreme interpretation of the growing evidence that workers face hours constraints within firms (Altonji and Paxson, 1992; Chetty et al., 2011; Labanca and Pozzoli, 2022). Taking wages as given, the firm simultaneously chooses hours and employment to maximize equation 1. In the case of salaried workers, the first order conditions imply

$$n^D(S, h^*(S,p), p) = \Big[\frac{x\alpha h^*(S,p)^\beta}{S(1 + p\frac{h^*(S,p)-40}{40}1[S \leq \bar{S}]) + R}\Big]^{\frac{1}{1-\alpha}} \tag{4}$$

where

$$h^*(S,p) = \frac{40\beta(S(1 - p1[S \leq \bar{S}]) + R)}{(\alpha - \beta)Sp1[S \leq \bar{S}]} \tag{5}$$

Proposition 3. *If firms set hours and p exogenously increases for salaried jobs paying less than $\bar{S}$, then*

1. *Weekly hours will decrease*
2. *Employment will increase for sufficiently small β and decrease otherwise*
3. *Salaries will bunch at $\bar{S}$, with the missing mass coming from jobs paying directly below the exemption threshold*
4. *Jobs will be reclassified from salaried to hourly*

Proof. See Appendix C.c. ■

Following equation 5, increasing the marginal hourly cost of labor incentivizes employers to cut hours. Aside from increasing the absolute cost of labor, overtime coverage also distorts relative costs, making it relatively cheaper to hire workers than raise hours. From equation 4, the opposing scale and substitution effects lead to ambiguous employment responses.

$$\frac{dn^d}{dp} = \underbrace{\frac{\partial n^d}{\partial p}}_{\text{Scale Effect}<0} + \underbrace{\frac{\partial n^d}{\partial h}\frac{\partial h}{\partial p}}_{\text{Substitution Effect}>0}$$

Historically, when overtime was introduced during the Great Depression, policymakers had intended for the policy to raise employment, implicitly believing that the substitution effect exceeds the scale effect (Ehrenberg, 1971; Trejo, 2003). Appendix C.c shows that a sufficient condition for employment to rise is for β to be small. In effect, employers engage in work-sharing only if there is little value to employing workers for long hours so that it is inexpensive to reallocate hours across workers.

In addition to the possibility of work-sharing, the labor demand model also generates two addition predictions: bunching and reclassification. First, if paying workers a salary at the overtime exemption threshold (i.e. $\bar{S}$) is cheaper than paying them the market clearing rate plus overtime (i.e. $S^*(1+1.5\frac{h^*-40}{40})$), then firms would raise workers' salaries to exactly the threshold, leading to a bunching mass in the distribution of base pays. Second, since overtime increased the cost of salaried workers relative to hourly ones, jobs on the margin of being salaried are reclassified to hourly. Neither the bunching nor reclassification effects would occur in the labor supply model because if workers can set their own hours, firms would actually prefer that salaried jobs are covered for overtime to incentivize them to work longer.

Given that employers do not record the hours of salaried workers exempt from overtime, I am unable to empirically test whether hours fall in response to an expansion in overtime coverage. Nevertheless, the prediction that hours decrease generates a useful corollary that allows me to distinguish between the labor demand and labor supply models using only cross-sectional data for hourly workers.

Proposition 4. *If firms set hours, then there would be a bunching mass in the distribution of weekly hours at precisely 40 per week.*

Proof. See Appendix C.d. ∎

III.d Compensating Differentials Model

A criticism of the preceding models is that hours are unilaterally determined by one party. In reality, the preferences of workers and firms likely both play a role in setting hours. However, the discussion of the labor supply and labor demand models shows that regardless of the base wage, there is no weekly hours for which both the worker and firm are indifferent - workers would never choose a 40 workweek whereas employers would bunch hours at 40.

To reconcile the preferences of both parties, the literature has argued through the lens of a compensating differentials model that overtime would have no real labor market effects (Trejo, 1991). If the government mandates that employers pay workers a premium for each hour worked above 40 in a week, firms and workers could simply renegotiate a lower base pay such that after accounting for the overtime premium, total earnings remain unchanged. As a result, overtime would have no effect on either hours or employment.

In appendix C.e, I replicate the predictions of the compensating differentials model within the framework developed in section III.a. Unlike the labor supply and labor demand models wherein jobs are defined in equation 2 by either a salary S or wage w, I assume the market determines a general earnings-hour profile $Y(h)$. Taking $Y(h)$ as given, firms and workers solve equations 3 and 1, giving the following first order conditions:

$$\frac{d\pi}{dh} = x\beta n^{\alpha}h^{\beta-1} - Y'(h)n = 0 \tag{6}$$
$$\frac{d\pi}{dn} = x\alpha n^{\alpha-1}h^{\beta} - Y(h) = 0 \tag{7}$$
$$\frac{dU}{dh} = Y'(h) - a^{-\frac{1}{\epsilon}}h^{\frac{1}{\epsilon}} = 0 \tag{8}$$

Together with the market clearing condition $n^d(Y,h) = n^s(Y,h)$, the four equations define an equilibrium characterized by four outcomes: employment n^*, hours h^*, earnings $Y(h^*)$, and wage $Y'(h^*)$.

Proposition 5. *If the market determines a wage-hour profile $Y(h)$, then expansions in overtime coverage would decrease base salaries but have no effect on total earnings, hours, or employment.*

Proof. See Appendix C.e. ∎

An insight from the demand-supply framework relative to the compensating differentials model is that it provides equilibrium conditions under which neither workers nor firms have an incentive to deviate the weekly number of hours. To illustrate, suppose prior to the rule change, a job was working 55 hours per week for a wage of \$15 per hour. While the compensating differentials model predicts that wages would fall to entirely offset the new overtime premium, it does not define the conditions under which such an equilibrium would satisfy each party's marginal decision per hour. In the above example, it is often assumed that wages would decrease to \$13.2 per hour, so that gross earnings are constant (i.e. $15 \cdot 55 = 13.2 \cdot 40 + 1.5 \cdot 13.2 \cdot (55 - 40)$). However, under this arrangement, the marginal cost of the 55th hour has increased to \$19.8, incentivizing firms to cut hours and workers to increase hours. In the demand-supply framework, I show that the market will adjust the earnings-hour profile $Y(h)$ such that not only are hours and earnings held constant relative to the pre-policy values, but the wage at the equilibrium hours $Y'(h^*)$ is held constant too. To accomplish that while also satisfying the requirements of the law, firms could reduce wages by a factor of 1.5 and then pay a lump sum bonus to compensate for the reduction in wages.

In the above example, wages would fall to \$10 per hour and firms would give workers a \$200 bonus for each week of labor (i.e. $15 \cdot 55 = 10 \cdot 40 + 1.5 \cdot 10 \cdot (55 - 40) + 200$).

III.e Testing the predictions

I summarize the testable predictions of the three models in Table I. In all cases, an expansion in coverage mechanically increases overtime pay. In response to this, if wages and hours are determined jointly, then base pay would decrease to fully offset the costs of overtime. Base pays would likewise fall in the labor supply model, but I would also expect a missing mass at 40 hours per week among hourly jobs. 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 otherwise be paid right below it, and reclassification of jobs from salaried to hourly. Employment may rise or fall in this model depending on the marginal productivity of 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. 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 employee's salaried/hourly status, income, hours, pay frequency (i.e. weekly, bi-weekly, or monthly), and state of employment for over a tenth of the U.S. labor force.[14]

A significant advantage of the ADP data over traditional survey data and 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 variable enables me to precisely calculate the measure of weekly base pay that the DOL uses to determine employees' exemption status. 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.[15] 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.

[14] 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.

[15] 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.

In addition to workers' pay rate, the data also records employees' monthly gross pay and monthly overtime pay.[16] 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 reimbursements. To express gross pay and overtime pay in the same weekly denominator as base pay, I scale them by the number of paychecks received each month and the number of weeks per pay-period.[17] While the data also records workers' total number of hours worked per month, employers only accurately track this information for hourly employees. Since employers are not required to record 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.[18] 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 about twice as large as the average firm in the sample. In practice, my main empirical specification retains employers not directly affected by the rule changes to account for potential spillovers such as the reallocation of workers across 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.

[16] 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.

[17] 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.

[18] 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 under-represents 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 income distribution of salaried and hourly jobs. I start with a case study of the federal policies, and then implement an event-study using the state reforms.

V.a Federal Policies

Graphical Evidence. Since changes to the FLSA 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 a cross-year analysis where I compare the evolution of base pays in the year of the reform relative to the evolution in previous years. The empirical strategy relies on the stability of the base pay distribution over time. As such, my preferred specification restricts the sample to the 22 states that did not change their state or local minimum wages after 2014.[19] 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 not adequately controlled for in the cross-year comparison.[20] My estimates are robust to relaxing these restrictions.

To begin, I show that although the 2016 policy was never legally binding, companies nevertheless responded to the proposed overtime exemption threshold. In figure IIa, I overlay the frequency distribution of salaried workers' base pay in April and December 2016, averaged over a balanced panel of firms. Reviewing figure IIa from left to right, three features stand out. First, there are 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 exemption threshold and adjusted their operations to avoid salaried jobs earning below that cutoff.[21] Second, there was a large drop in the number of workers with base pays between the old and new thresholds. Firms employed on average 8.4 salaried workers in the affected interval in April 2016, and only 6.8 such workers in December - a decrease of 19%. Third, there is a large spike in the distribution at $913 that appears only in December, indicating that firms raised some salaries above the new threshold.

The bunching of workers above the threshold is 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

[19] These states account for 35% of all workers in the data. Given the wave of minimum wage changes since 2015, the sample only includes states that are bounded by the federal minimum wage.

[20] The size restriction drops 41 firms, accounting for 11% of all workers in the sample.

[21] 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.

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 preceding years. Furthermore, the mediocre growth 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 predictions of the labor supply and compensating differentials models.

Replicating the same graphs for hourly jobs, figure IIc depicts the distribution of hourly workers' base pay in April and December 2016. Compared to salaried jobs, there are twice as many hourly workers and the distribution of their base pay is more right skewed. To visualize the change in employment, figure IId plots the difference-in-distributions between April and December of each year from 2013 to 2016. Two features of the figure warrant discussion. First, there is a large drop in the number of hourly workers at the left tail of the distribution in every year and an increase in employment to the right of it. This pattern simply reflects natural wage growth leading to fewer people earning precisely the federal minimum wage. Second, there is a progressively larger volume of workers entering the \$455-913 interval over time, making it difficult to visually distinguish the effects of the policy from aggregate employment shocks. A natural control group to account for year-specific shocks are jobs paid far above the new threshold, but few hourly jobs satisfy such a criterion. Instead, my empirical strategy will use changes to the number of high-income salaried positions to infer the counterfactual growth for both salaried and hourly employment, noting a strong similarity in the trend of employment growth between these two types of jobs.[22]

Constructing the Counterfactual Distribution.

Intuitively, the stability of the shape of the difference-distributions over time motivates the potential for a cross-year difference-in-difference. However, the growth in the distribution across years, among both salaried and hourly jobs, suggests a need to account for year-specific shocks. Following these observations, I identify the effect of the 2016 FLSA policy by using the change in the distribution between April and December 2015 as a counterfactual. To account for year-specific aggregate employment growth, I apply a linear transformation to the difference-distribution in 2015 to match the right tail of the distribution in 2016.[23]

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:

$$n_{ijk,Dec,t} = n_{ijk,Apr,t} + \alpha_{jkt} + \beta_{jk} \cdot D_{t=16} + \varepsilon_{ijkt} \tag{9}$$

[22]To see the evolution of employment growth for salaried jobs more clearly, appendix figure A.4 omits the 2016 line from figure IIb to shrink the vertical axis.

[23]Graphically, this is equivalent to vertically compressing and shifting the 2015 distribution in figures IIb to fit the right tail of the 2016 distribution.

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. Following recent advancements in the minimum wage literature, I compute the aggregate employment effect by first estimating each β_{jk}, and then integrating these effects across all bins (Cengiz et al., 2019; Derenoncourt and Montialoux, 2019; Harasztosi and Lindner, 2019; Gopalan et al., 2020).

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

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

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

The first assumption states that the policy has no effect on the number of workers earning above a cutoff bin k^*. Following the intuition from the graphical evidence, the second condition states that the distribution of employment growth between April and December is similar across years, up to a linear transformation.[24]

Under these assumptions, appendix G shows that an unbiased estimator of β_{jk} for any $k < k^*$ is

$$\begin{aligned} \hat{\beta}_{jk} &= \left(\bar{n}_{jk,Dec,t} - \bar{n}_{jk,Apr,t}\right) - \hat{\gamma}_1 \left(\bar{n}_{jk,Dec,t-1} - \bar{n}_{jk,Apr,t-1}\right) - \hat{\gamma}_0 \\ &= \Delta \bar{n}_{jkt} - \hat{\gamma}_1 \Delta \bar{n}_{jk,t-1} - \hat{\gamma}_0 \end{aligned} \tag{10}$$

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

$$\Delta \bar{n}_{sal,kt} = \gamma_1 \Delta \bar{n}_{sal,k,t-1} + \gamma_0 + \epsilon_{sal,kt} \tag{11}$$

using only salaried workers with bins $k \geq k^*$. I restrict the sample to only salaried workers when estimating equation 11 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.[25] My preferred specification uses $k^* = \$1778$, and bins of width $\$96.15 \approx \frac{5000}{52}$ to account for bunching at annual salaries of multiples of $5000. I treat the salaried and

[24] These assumptions mirror the ones made by Defusco et al. (2019) to generate a counterfactual distribution of debt-to-income (DTI) ratios absent a rule change that raised the difficulty of giving mortgages to individuals with a DTI threshold.

[25] A concern with using only salaried workers to estimate equation 11 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.

hourly distributions within each firm as independent observations and cluster estimates at the firm-level.

To develop an intuition for equation 10, 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 the growth in year t (i.e. $\hat{\gamma}_1 = 0$), then equation 10 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 checks by estimating equation 10 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.

Before proceeding with the results, first consider the strengths and weaknesses of the empirical approach. Note that even without the linear transformation, the difference-in-difference across years accounts 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 is that it accounts for year-specific differences in aggregate employment growth that affect all income groups. However, unlike a traditional difference-in-difference, it is unable to control for year-specific shocks that target a particular segment 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.

Figure IIIa plots the estimates of the treatment effects on salaried jobs by bin, and the integral of these estimates over the 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 effects, I find that the large drop in the number of workers below the new threshold exceeds the spike in the number of workers right above it, implying a net loss in the number of salaried jobs. As a placebo check, I do not find any significant effects when I estimate equation 10 using adjacent years of data between 2011 and 2015 (see figure IIIb).

Repeating the analysis for hourly employees, figure IIIc shows that 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. In comparison, the placebo test in figure IIId finds relatively flat cumulative employment effects in preceding years.

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 depicted in figure III, 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%). The 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. 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.[26]

In column (2) of table II, I apply my baseline specification to estimate the employment effects of the 2020 federal policy. These estimates represent the change in employment between the month before the announcement of the new threshold (August 2019) and the month that the new threshold went into effect (January 2020).[27] 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.5.

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 control workers' hours and therefore value keeping jobs exempt from overtime. As a final piece of evidence that workers do not unilaterally set hours, appendix figure A.6 shows bunching at 40 hours of labor per week among hourly workers. Despite these insights, the results from the federal analysis do not offer enough statistical power to establish the direction of the employment effects.

[26] Appendix table A.1 shows that these responses are robust to 1) keeping all states, 2) keeping the largest 0.1% of firms, 3) allowing for firm entry and exit into the data by attributing firms an employment of zero in months where they are not observed, and 4) using all bins $k^* \geq \$1393$ to estimate the parameters of the linear transform.

[27] 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 previous \$913 threshold.

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 can account for bin-specific confounders that vary over time by using the states covered by the FLSA as a control group. Intuitively, for each of the 16 events, I compare the evolution of employment between the treated and control states. However, since employment is measured in levels rather than logs, even if firms grow at the same rate in both groups, the control will nevertheless gain more jobs simply because it had higher baseline employment. To account for this, I rescale the distribution of base pay in the control group to exactly match the distribution in the treatment group two months before each threshold change.

Following this adjustment, I append the 16 datasets together and estimate the following stacked difference-in-difference regression:

$$n_{ikstv} = \sum_{\substack{t=-6 \\ t\neq -2}}^{5} \sum_{k=-6}^{15} \beta_{kt} \cdot I_{kst} + \alpha_{ksv} + \delta_{ktv} + \varepsilon_{ikstv} \tag{12}$$

where the treatment dummy I_{kst} equals 1 for the treatment state at event time t and bin k, which I normalize to equal 0 at the new threshold. I set the reference date as two months prior to the rule change to capture any anticipatory responses.[28] 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 12 is equivalent to estimating 16 individual differences-in-differences and then taking a weighted average of the treatment effects to compute β_{kt}.[29] The identifying assumption is that absent the state threshold changes, the distribution of base pay in the treated states would have evolved the same as the control states. I cluster standard errors at the firm-level, and show the results are robust to clustering by state.

Estimates of Employment Effect Across Distribution of Base Pay. Figure IV plots the estimates of the treatment effect, separately for the distribution of salaried and hourly workers. Similar to the federal policies, figure IVa shows a net decrease in the number of salaried employees below the new threshold and a spike in workers right above it at the month of the reform. As a placebo check, I find no effect on any bins of base pay above the new threshold. To further validate my empirical strategy, figure IVb plots the change in the

[28] For the three state policies that went into effect on Jan 1, 2017, I set the reference month as event time -1 to avoid capturing the effect of the 2016 FLSA policy.

[29] Relative to organizing the data in calendar time, this model avoids contaminating pre-trend estimates by heterogeneous treatment effects across events (Sun and Abraham, 2020).

number of salaried workers paid below and above the new threshold over time.[30] Examining the figure from left to right, three features stand out. First, there is little evidence of a pre-trend prior to the policy change, indicating that the parallel trends assumption holds. 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. Third, the magnitude of the decrease in employment below the threshold is visibly larger than the increase in employment above it.

Plotting analogous figures for hourly workers, I find that the base pay distribution for hourly jobs responded in a qualitatively similar fashion. Figure IVc shows 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 interval of affected base pay. I confirm the bunching effect in figure IVd where I plot the effect on hourly employment over time. Mirroring the estimates for the salaried distribution, I find no significant pre-trends, followed by a sharp divergence in the number of jobs below and above the threshold at exactly the month of the rule change. The bunching of hourly employees who were unaffected by the policy is consistent with growing evidence of relative pay concerns within firms (Card et al., 2012; Dube et al., 2019).

Column (3) of table II summarizes the aggregate employment effects at event time 0, scaled by the average number of affected workers two months prior to the rule change. Similar to the 2016 FLSA proposal, I find that the number of jobs in the affected interval of base pay falls by 20.9% (s.e. 1.2%). However, in comparison to the federal rule change, firms bunched a larger share of affected workers at the new threshold and did not increase the number of hourly workers. In total, 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 directly affected workers.

Appendix table A.2 assesses the robustness of my estimates to different specifications. Overall, I find that the magnitude of the bunching, reclassification, and employment effects are stable even after 1) five months post-event, 2) using only within-firm variation, 3) restricting the sample to pre-2016 events that have yet to introduce within-state variation in the threshold, or 4) clustering the data at the state-level.[31]

[30] I drop the California 2020 event in this figure because the data ends in January 2020.

[31] Given the size of the data, it is infeasible to cluster by state while using firm-level observations. To reduce computational burden, I instead aggregate employment to the state-level first, and then scale each state in the control group to match the treated state at the baseline period. The regression coefficients and baseline values therefore differ slightly from my preferred specification because it weighs each state equally, independent of the number of firms in the state.

To summarize my results, the last column of table II averages the employment effects over 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 12 except I collapse the data to only two months 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 reforms. The point estimate of the employment effect from this pooled regression 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 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. This actually fits the predictions of the labor demand model. Since the state rule changes raised the threshold by a smaller amount, a greater share of affected workers had base pays close to the new threshold, making it cheaper for firms to bunch them, rather than reclassify them.

To further explore why the total number of hourly workers barely changed as a result of the state reforms, appendix figure A.7 plots the effect of the state policies on net employment flows (i.e. hires minus separations) and net reclassification flows. I show that the state rule changes indeed caused workers to be reclassified from salaried to hourly. The reclassification effect was simply obscured by a simultaneous negative employment flow out of hourly positions, leading to a net zero change in the number of hourly workers. In comparison, the reclassification effect was more noticeable from the federal policy change simply because the larger increase in the threshold led a greater share of affected workers being reclassified. Overall, the state and federal responses are internally consistent, and any differences between them can be explained by the magnitude of the threshold change.

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. 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

$$y_{ivt} = \sum_{t=T_0}^{T_1} \beta_t \cdot I_{st} + \alpha_{vs} + \delta_{vt} + \varepsilon_{ist} \quad (13)$$

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. Since I do not observe the wages of workers in firms not using ADP's software, I restrict the sample to workers who are continuously employed at the same firm in all months of the event window.[32]

Figure V plots the difference-in-difference estimates for all three policy evaluations, separately by base and overtime pay. Four key features stand out from this analysis. First, in all cases, the treatment and control groups were trending similarly prior to the announcement of the new rule, thereby satisfying the parallel trends assumption. 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 base pay and overtime pay at precisely the month that the threshold increases, and this raise remains fairly stable afterwards. Lastly, workers experience a larger increase in base pay than overtime pay, suggesting that bunched workers gained the most from the policy. The rise in salaries further rejects the prediction of the compensating differentials model that firms would cut workers' base pay to nullify the costs of overtime.

I report the estimates of the income effect from expanding overtime coverage in table III. Summing the effects on base and overtime pay, then dividing by baseline incomes, I show

[32]To determine whether this restriction introduces selection bias, appendix figure A.8 compares 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 workers' retention due to the federal policy changes, but a small increase in separations from the state policies.

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. Results are similar if I simply compare the change in log incomes. In appendix figure A.9, I also estimate the effect on workers' actual gross pay, without assuming a 40 hour workweek for workers who are reclassified from salaried to hourly. Although the estimates are noisier due to imputation error from translating monthly gross pay to an average weekly amount, the magnitude of the effects are largely consistent with the impact on total pay.

To summarize, column (5) of table III reports the estimated income effects averaged across all 18 policy changes. As expected, there is a positive income effect driven primarily by an increase in base pay. I find that workers' earnings increased on average by 1.3%. 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 show in column (7) that the effect on workers whose baseline salary was already \$40 to \$80 above the new threshold was an order of magnitude smaller than for directly affected workers.

VII Policy Implications

VII.a Jobs vs. Wage Growth

To benchmark the costs and benefits of raising the overtime exemption threshold relative to other labor market policies, table IV computes the ratio of the employment and income effects. Column (1) presents this estimate using the pooled sample described earlier. I find that for each percent increase in affected workers' incomes, the number of affected jobs falls by 3.36% (s.e. 1.71%).[33] Given that no other overtime study estimates an employment elasticity, I compare my result to those in the minimum wage literature. 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 a range of -3 to 1.5. While I can rule out a ratio more positive than -0.17 with 93% confidence, that still means I cannot reject half the existing elasticities. As such, I interpret the evidence to weakly suggest that overtime coverage costs at least as many jobs per percent increase in earnings when compared to the minimum wage.

The remaining columns of table IV show that the estimates are robust to a series of

[33]If overtime coverage also imposes additional administrative costs, then the ratio of the employment and income effects alone does not represent a labor demand elasticity. Nevertheless, this ratio is still a policy-relevant statistic for gauging the cost and benefits to workers. I thank Steve Trejo for this clarification.

stricter specifications checks. First, I drop firms that employed no salaried workers in the treated interval prior to the policy change. While this specification reduces noise, it implicitly assumes that "unaffected" employers do not reduce future hires within the affected region or absorb displaced workers from treated firms.[34] Second, I estimate the elasticity using only the state variation and restrict the sample to firms that operate in both treated and control states. The stricter sample eliminates challenges to the unconventional cross-year analysis of the federal policies as well as concerns that the effects are driven by differences in industry composition across states. Given the strict sample restriction and the nature of the policy, the estimates are immune to a wide range of unobservable shocks, except for those that specifically targeted jobs in the affected income range, at exactly the month of the reform, and differentially impacted establishments in the treated states from those of the same firms in the control states. Third, to determine whether the results are driven by a few large firms, I compute the elasticity after dropping the largest 1% of employers while again using only the state-level variation. Overall, the magnitude of the elasticity is fairly stable across all specifications.

Aside from imprecision, a possible explanation for the large elasticity is that workers no longer employed in the sample could in principle find a job at firms not partnered with ADP. However, in order for this to alter the interpretation of the results, it must be 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 do the opposite. 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 employers in the sample.[35]

To explain the seemingly large employment loss from overtime, I show in appendix F that while the increase in income among stayers is small, the ex-ante cost of overtime among

[34] To check for spillovers, I have also estimated the impact of the policies on "unaffected" firms. Since these firms have no directly affected salaried workers, I normalize the employment effect by the sum of salaried and hourly jobs in the treated interval. Using this measure, I find that affected firms lose 0.8% (s.e. 0.3) of treated jobs, whereas unaffected firms only lose 0.03% (s.e. 0.7).

[35] Using similar payroll data, Gopalan et al. (2020) find minimum wage elasticities comparable to those in the literature, suggesting that firms choosing to use a payroll software are not more responsive to wage increases than firms in other samples.

affected jobs that were lost was potentially sizeable. Given that I do not observe the counterfactual earnings of jobs that were eliminated, I run two simulations to measure their expected cost of overtime. First, I use hourly workers in the upper tail of the distribution of overtime hours as a benchmark.[36] I find that overtime comprised at least 8% of total earnings for a tenth of hourly workers. 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 the simulation, 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 earnings under conservative parameters, and at least a 30% increase under a more realistic specification.[37] Taken together, the simulations suggest that the unobserved cost of overtime among destroyed jobs may be an order of magnitude larger than the estimated income effect among stayers. In the next section, I provide additional evidence that employers are willing to raise some workers' salaries by up to 25% to keep them exempt from overtime, suggesting that the regulation can indeed be very costly for select jobs. It is therefore not implausible for expansions in overtime coverage to cost more jobs than the minimum wage, especially given that overtime compliance may also entail additional managerial costs from monitoring workers hours which in turn reduces worker flexibility. Nevertheless, the magnitude of the elasticities should be interpreted with caution given their imprecision.

VII.b Implications for Redistribution

I next explore the heterogeneous impacts of overtime coverage and its implications for redistribution. To do this, I leverage the matched employer-employee panel structure of the data to separately identify bunched workers, reclassified workers, new hires, and separations. I focus specifically on the 2016 FLSA policy, as it increased the threshold by a large enough amount for the effects of the policy to vary substantially along the income distribution.

I begin by documenting which workers experienced the largest increase in income. In

[36] I choose hourly workers with the most overtime hours as a comparison for two reasons. First, the salaried jobs eliminated by the reforms were likely also those with the largest cost of overtime. Second, the average hourly worker may not receive much overtime compensation because employers have constrained their hours. In the extreme, if firms constrained every employee's hours to only 40 per week, then no one receives overtime compensation but that does not imply such a constraint is costless to the firm. To approximate the ex-ante cost of overtime before any behavioral responses, I choose hourly workers with long weeks as a benchmark since their hours may be less constrained by firms.

[37] 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. The fixed workweek method is also required by law in California and Alaska.

figure VII, I plot the evolution of affected workers' incomes 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. The figure shows that the bulk of the positive base pay effect accrued to workers who received a raise above the new threshold. Although this is partly mechanical from conditioning the sample on individuals' post-policy income, no other group experiences 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, and fewer workers were bunched than reclassified (see table II), this analysis suggests that bunched workers received the largest increase in earnings.

To identify which workers were bunched, I apply the empirical strategy from section V to the sub-sample of always-salaried workers.[38] I present the estimates of this analysis in figure VIIa. The figure shows that the spike at the new threshold comes from workers who would have otherwise earned within \$180 of the cutoff. A back of the envelope calculation implies that the median bunched worker experienced a 7.0% wage increase due to the rule change, nearly five times the effect for the average affected worker. Moreover, firms gave the marginal bunched worker a 25% raise to keep them exempt from overtime, consistent with the simulation in the previous section that showed the cost of overtime can be very large for a small subset of jobs despite it having little impact on the majority of affected workers.[39].

I now turn to the question of where along the income distribution were jobs displaced. Figures VIIb plots the estimated effects of the FLSA 2016 threshold on the distribution of employment flows (i.e. hires minus separations). These estimates indicate that the employment loss was spread across the entire interval of affected weekly base pays.[40] 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 accrued to workers earning closer to the new threshold and received a raise above it. Taking the ratio of the size of the spike in figure VIIa to the number of workers directly affected by the 2016 policy, I find that only 4.9% (s.e. 0.2%) of affected workers benefit from the bunching. Taken together, these results suggest that the 2016 rule change

[38] This approach assumes that the policy has little effect on the distribution of separations from employment, and 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 provide evidence in support of these assumptions in appendix H.

[39] Appendix figure A.10 plots the effect on stayers using finer bins of base pay to clearly identify the counterfactual salaries of bunched workers. The median bunched worker would have earned \$853 per week and the marginal bunched worker would have earned \$733.

[40] See appendix H for placebo checks and the dynamics of the employment loss.

was counter-redistributive: the policy greatly benefited a small share of middle-income jobs at the expense of lower paying ones.

VIII Discussion and Conclusion

This paper presents new facts about the labor market effects of expanding overtime coverage and informs policy debates 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" (U.S. Department of Labor, 2016). In contrast, I estimate that expansions in overtime coverage actually have the opposite effect and reduce employment. 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 compensating differentials model of overtime, the DOL calculated that 18% of workers would experience a decrease in base pay to partially offset the increase in overtime pay. However, I find no evidence that firms reduced base pays in response to overtime coverage. 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 affected 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 are many avenues for future research that are beyond the scope of this study. Similar to the minimum wage, overtime can potentially have important implications for consumers (Harasztosi and Lindner, 2019), inequality (Lee, 1999; Autor et al., 2016), and productivity (Coviello et al., 2021). Unlike the minimum wage though, there did not even exist estimates of the employment effects of overtime eligibility prior to my study (Brown and Hamermesh, 2019). This paper adds to the scarce literature on overtime coverage by providing the first causal estimates of its employment and income effects. In future endeavors, it would be fruitful to explore the effects of overtime coverage on additional margins of responses and compare them to estimates from the minimum wage literature.

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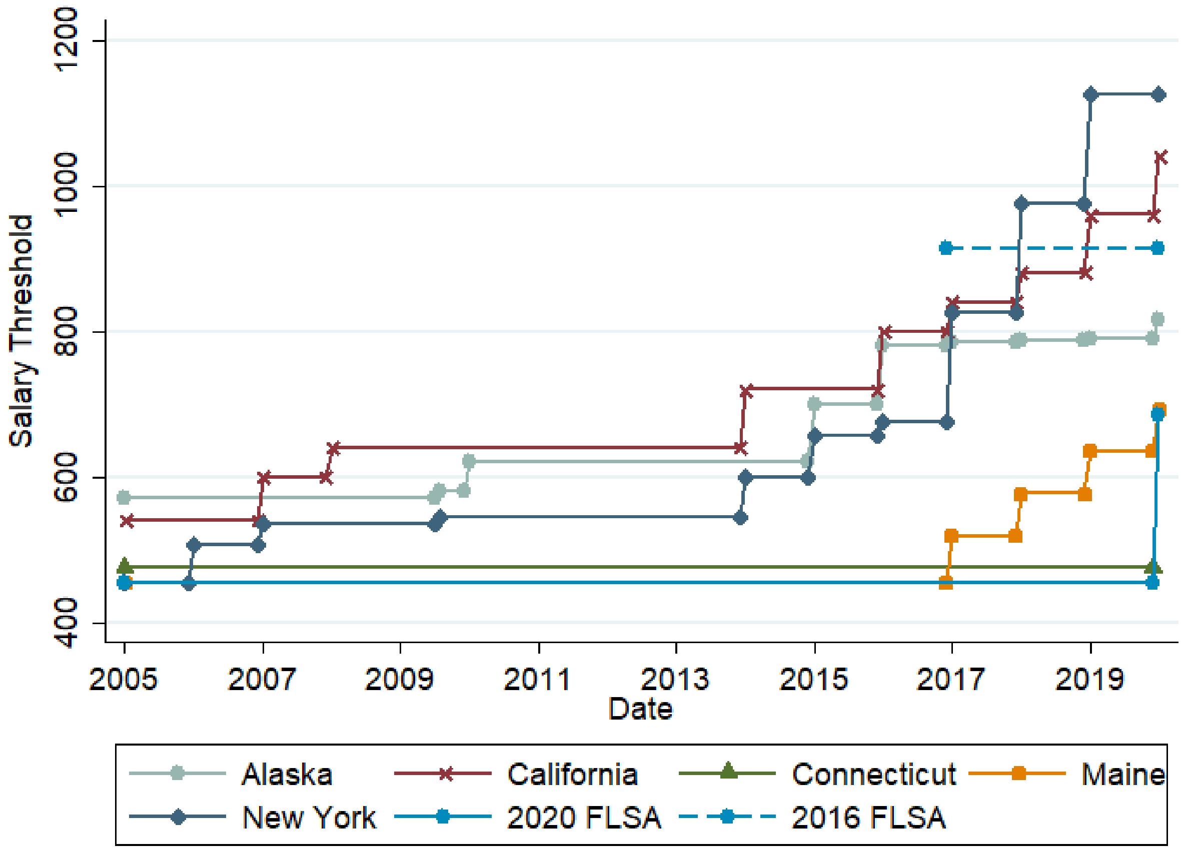
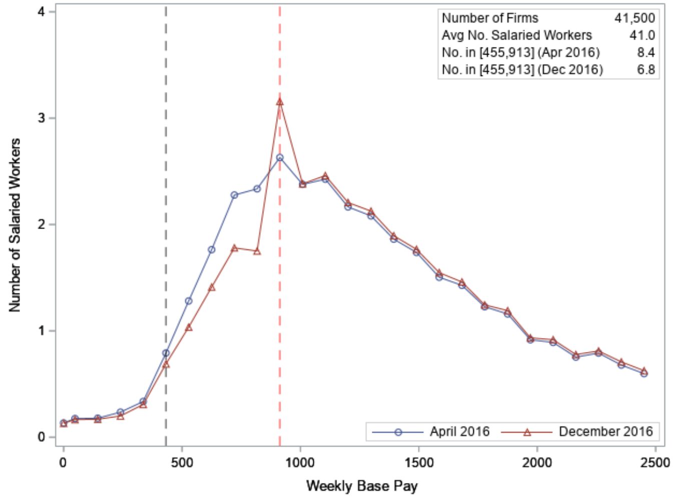
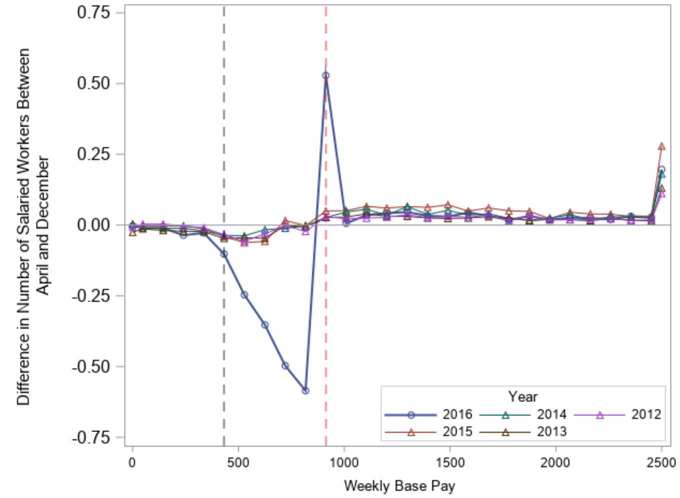


Figure I
Variation in State-Specific Overtime Exemption Thresholds

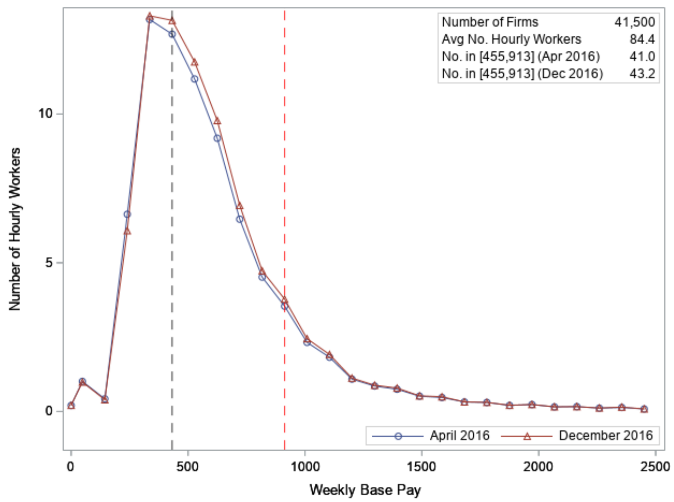
Notes. This figure shows the binding overtime exemption threshold in each state between 2005 and 2020. All states not explicitly included in the graph are covered by the Fair Labor Standards Act (FLSA). The line "2016 FLSA" represents the federal threshold that was supposed to go into effect on December 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.



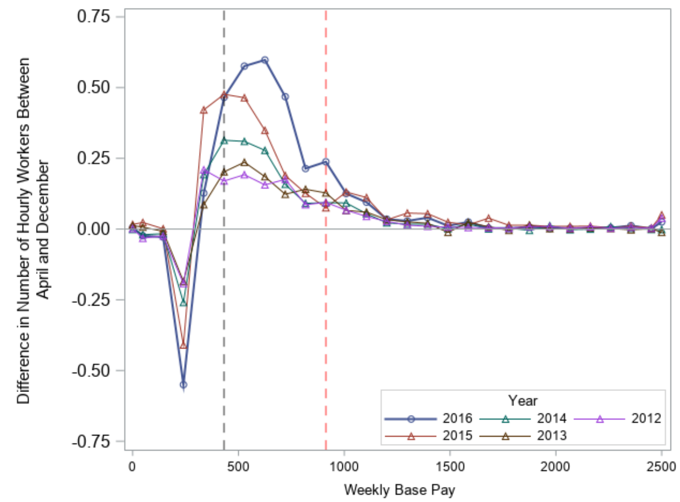
(a) Frequency Distribution of Salaried Jobs



(b) Difference in Distribution of Salaried Jobs



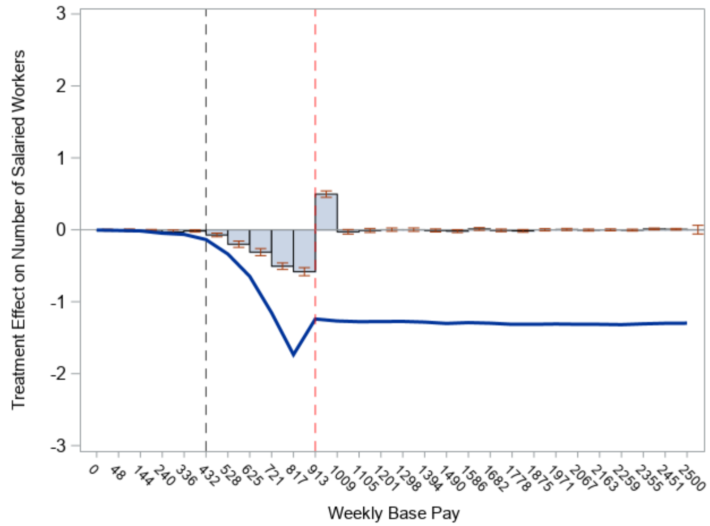
(c) Frequency Distribution of Hourly Jobs



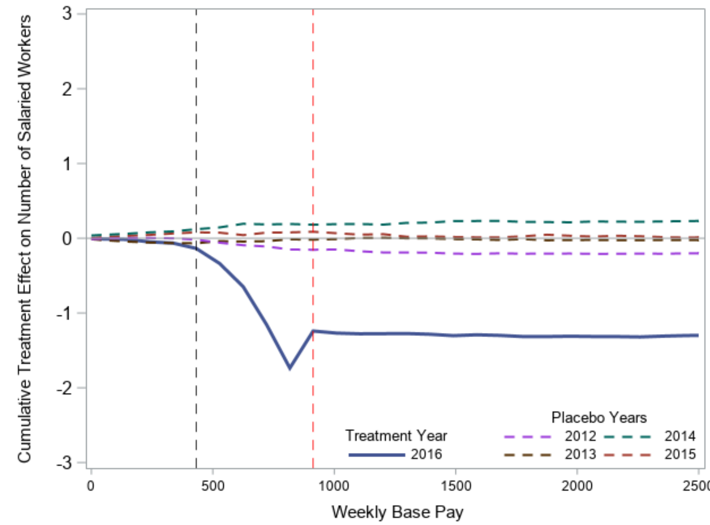
(d) Difference in Distribution of Hourly Jobs

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

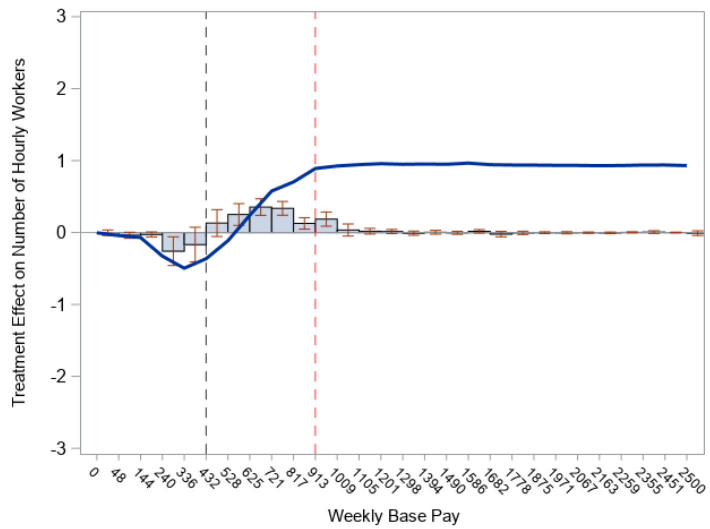
Notes. Panel (a) shows the frequency distribution of weekly base pay of salaried workers in April and December 2016, scaled by the number of firms in the balanced sample. The left vertical dashed line is at the bin containing the overtime exemption threshold in April (\$455), while the right dashed line 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.



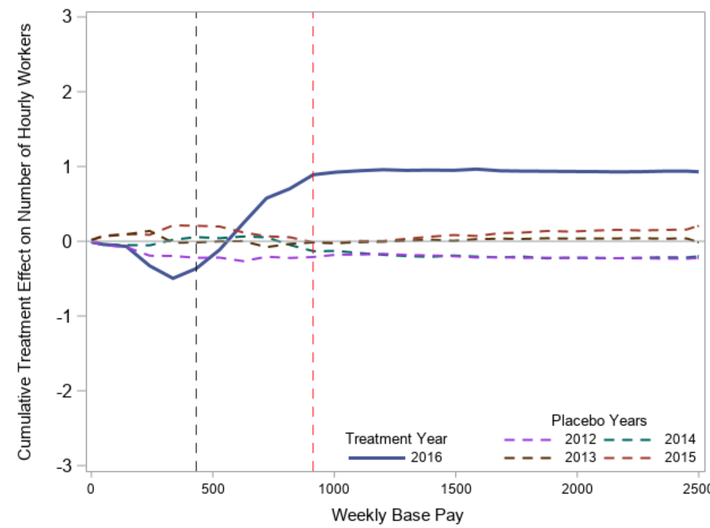
(a) Estimates of Effect on Number of Salaried Jobs



(b) Placebo Test of Effect on Salaried Distribution



(c) Estimates of Effect on Number of Hourly Jobs

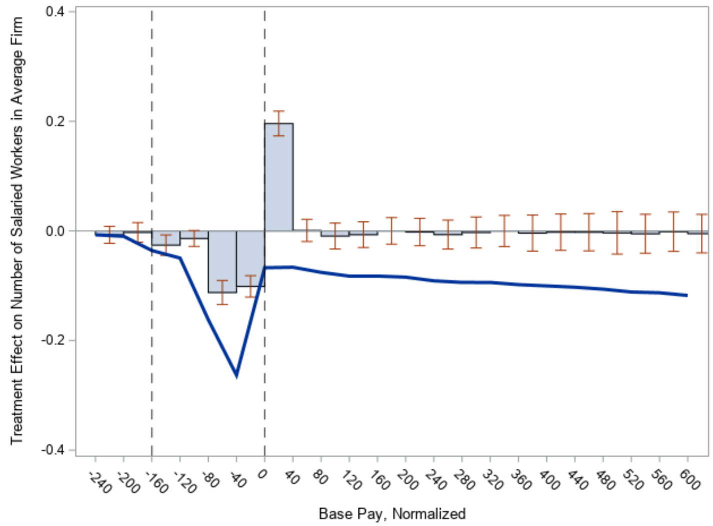


(d) Placebo Test of Effect on Hourly Distribution

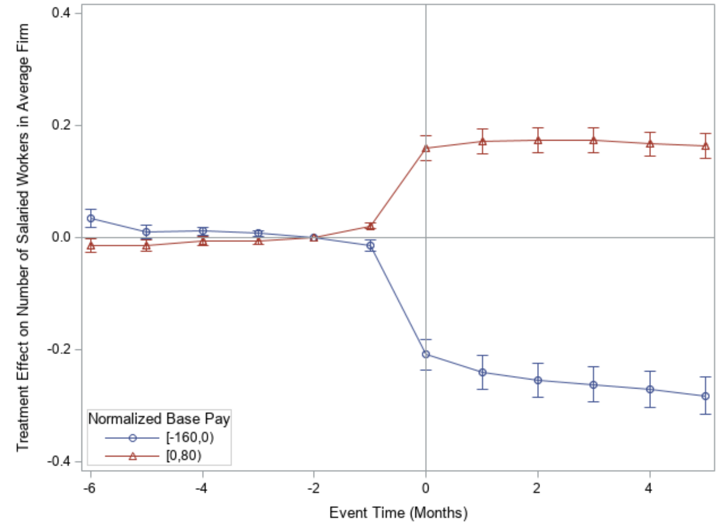
Figure III

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

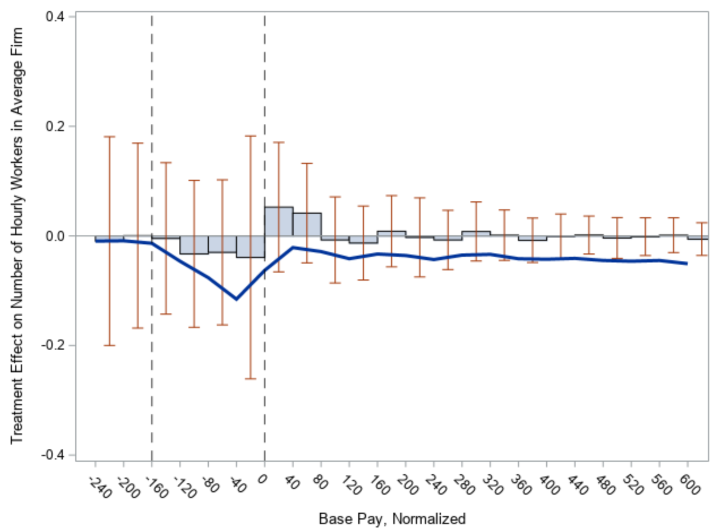
Notes. Panel (a) shows the effect of the 2016 FLSA policy on the number of salaried jobs in each $96.15 bin of base pay in Dec 2016. The treatment effects are estimated using equation 10. The solid blue line is the running sum of these effects. The solid line in Panel (b) is the same as the solid blue 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.



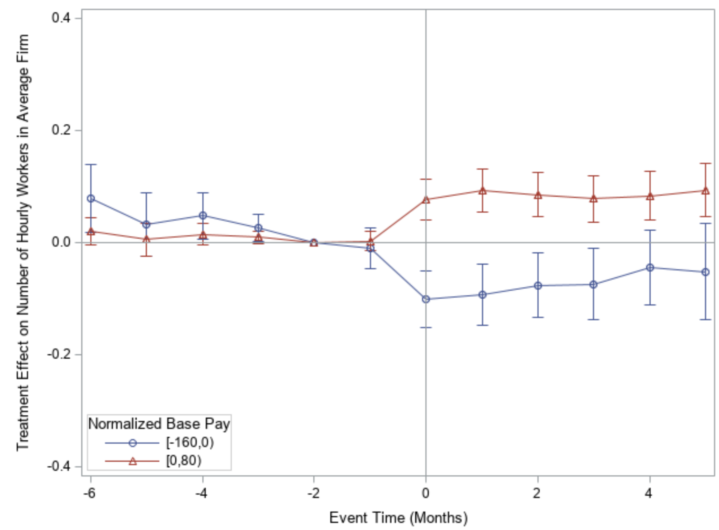
(a) Effect on Number of Salaried Jobs, at Time 0



(b) Effect on Salaried Distribution Over Time



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

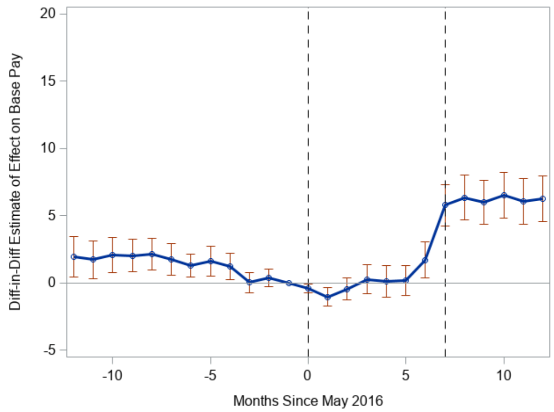


(d) Effect on Hourly Distribution Over Time

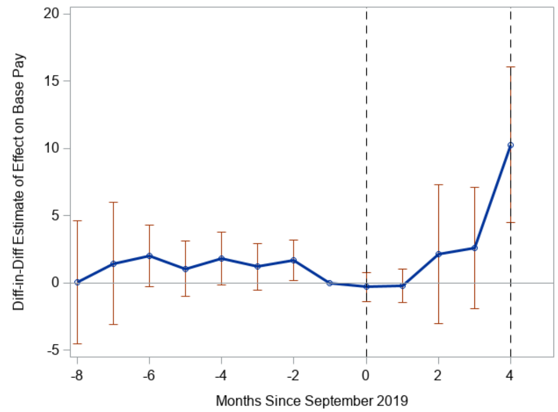
Figure IV

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

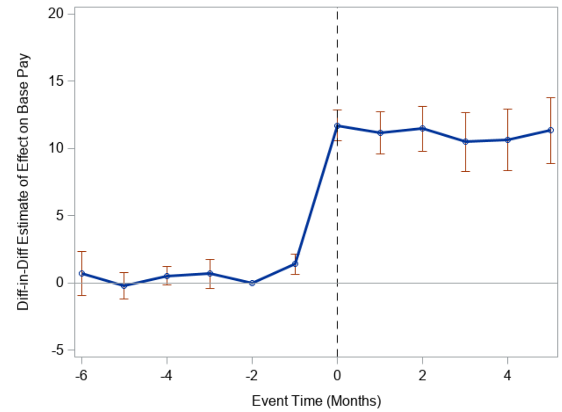
Notes. Panel (a) shows the event study estimates from equation 12. The height of each bar indicates the effect of raising the OT exemption threshold on the number of salaried jobs in each \$40 bin of base pay on the month that the new threshold becomes binding. The solid blue line is the running sum 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.



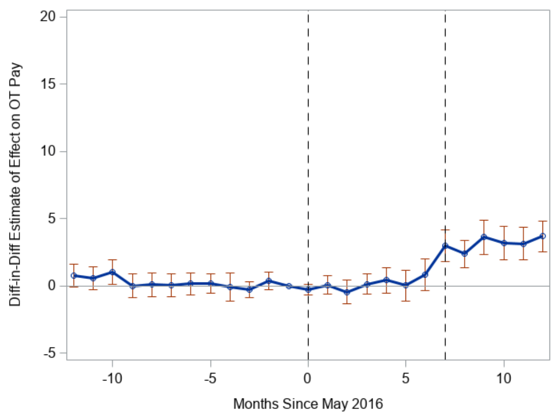
(a) 2016 FLSA: Base Pay



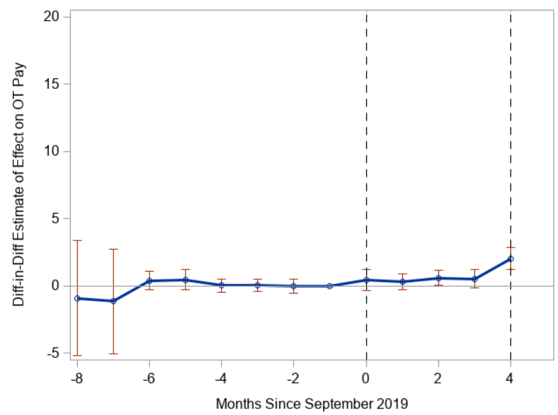
(b) 2020 FLSA: Base Pay



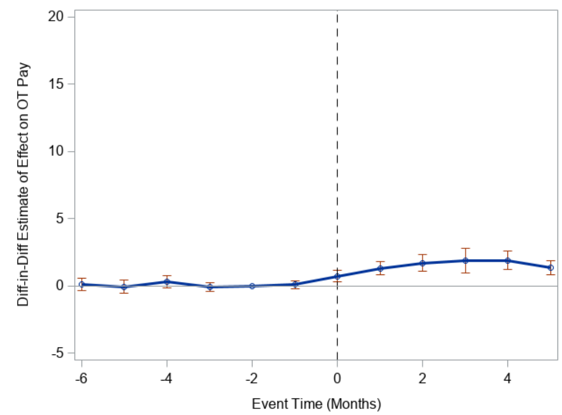
(c) Event Study: Base Pay



(d) 2016 FLSA: OT Pay



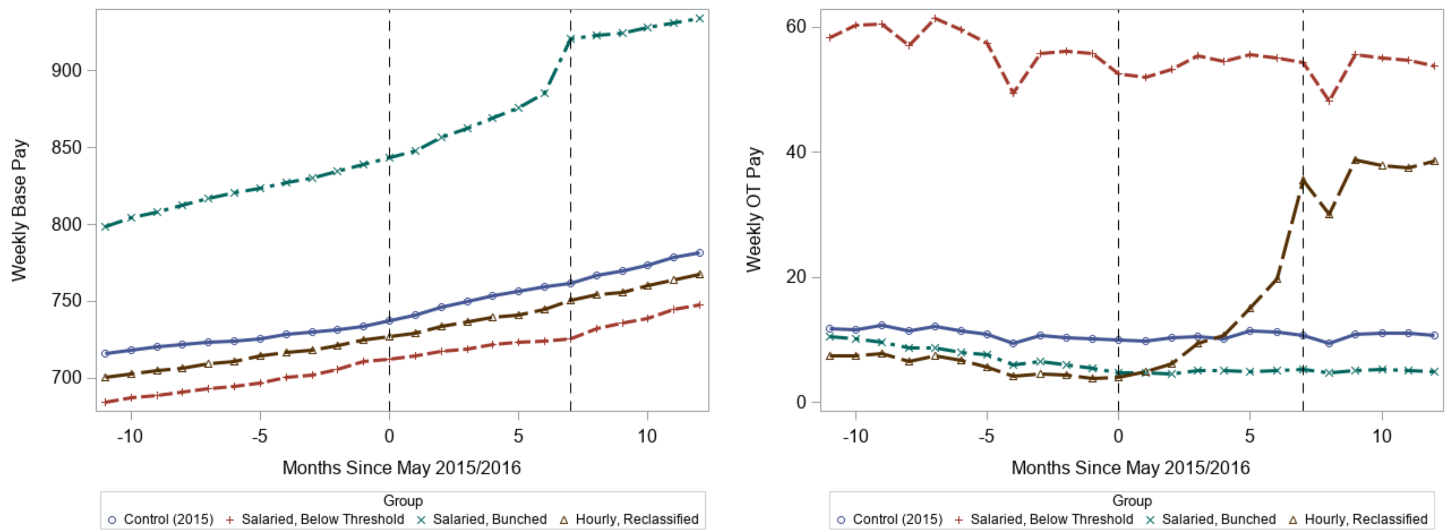
(e) 2020 FLSA: OT Pay



(f) Event Study: OT Pay

Figure V
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 13, 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.



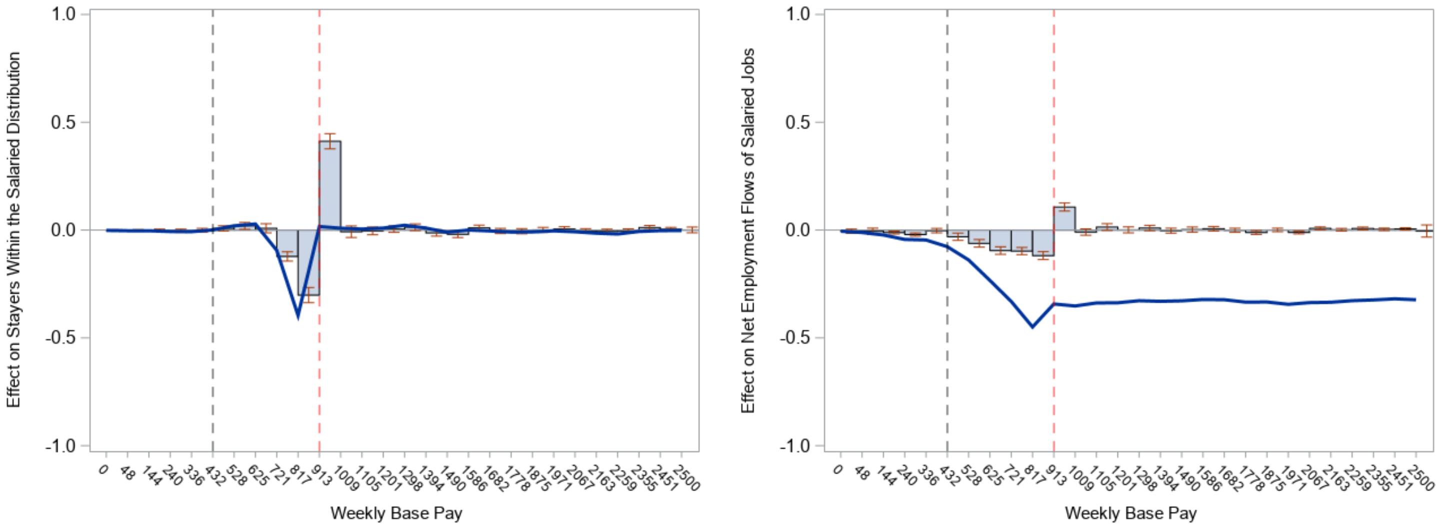
(a) Evolution of Base Pay

(b) Evolution of Overtime Pay

Figure VI

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

Notes. Figure (a) and (b) plots the evolution of base pay and overtime pay, respectively, for four different groups. 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.



(a) Distribution of Stayers

(b) Distribution of Employment Flows

Figure VII
Effect of the 2016 FLSA Policy on the Distribution of Stayers vs. Employment FLows

Notes. Figure (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 salary in both months. The height of each bar is estimated using equation 10, assuming $\gamma_0 = 0$. Figure (b) shows the effect of raising the 2016 overtime exemption threshold on net employment flow of salaried jobs (i.e. number of hires minus separations). In both figures, the solid blue line is the cumulative sum of the bin-specific effects. 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 Overtime Exemption Threshold on the Pay Distribution

	(1)	(2)	(3)	(4)
Jobs Below Threshold	−0.207***	−0.144***	−0.209***	−0.172***
	(0.01)	(0.026)	(0.012)	(0.007)
Bunched	0.052***	0.134***	0.162***	0.092***
	(0.008)	(0.038)	(0.010)	(0.004)
Hourly Jobs	0.114***	0.013	−0.013	0.037*
	(0.037)	(0.218)	(0.02)	(0.021)
Employment	−0.041	0.002	−0.060***	−0.043**
	(0.042)	(0.231)	(0.020)	(0.022)
Treatment Group				
Affected Workers	8.37	2.09	1.20	2.46
Avg. Firm Size	125	147	110	117
No. Firm-Events	41,500	36,934	183,673	262,107
Sample				
No. Events	1	1	16	18
Policy Variation	2016	2020	State	Pooled

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) and (2) report the effects of the 2016 and 2020 FLSA policies, respectively, estimated from equation 10 using a cutoff of $1776 to estimate the scaling factor in equation 11. The FLSA sample is restricted to a balanced panel of firms in states with no minimum wage changes, dropping the 0.1% largest firms. Column (3) reports the effects of the state threshold increases on the exact month of the rule change, estimated using equation 12. Column (4) 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 III
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)
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) reports the estimate of the policy's effect on log total pay.

Columns (1) reports the income effect of the 2016 FLSA policy estimated from equation 13, 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 IV
Ratio of Employment and Income Effects

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

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 (4) of table II, the income effect in column (5) of table III, and their ratio. This specification uses both the state and federal policy variations and keeps all firms in the sample. Column (2) restricts the sample in column (1) to only firms that had at least one salaried worker between the old and new thresholds in the baseline period. 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. All robust standard errors in parentheses are clustered by firm. *10%,** 5%,*** 1% significance level.

Appendix: For Online Publication

Appendix A. Additional figures and tables

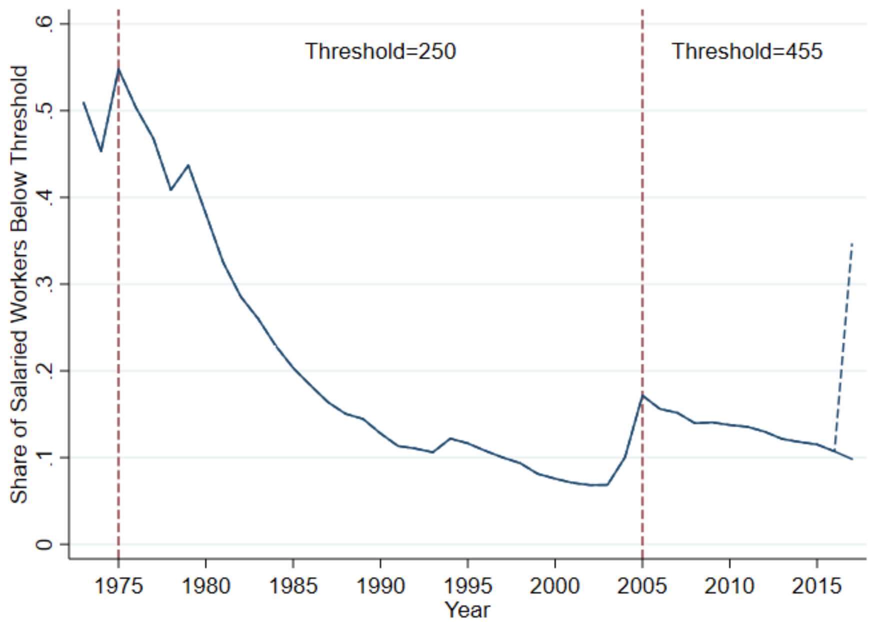


Figure A.1
Percent of Salaried Workers Below the FLSA OT Exemption Threshold

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.

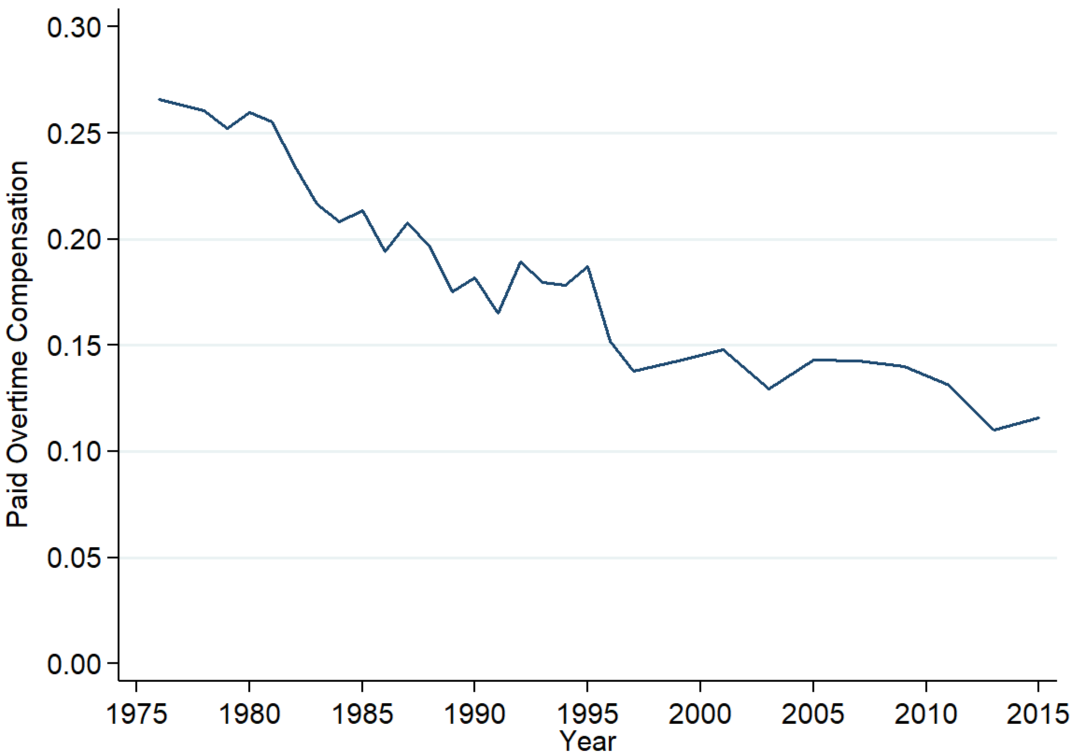


Figure A.2
Percent of Salaried Workers Eligible for Overtime

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".

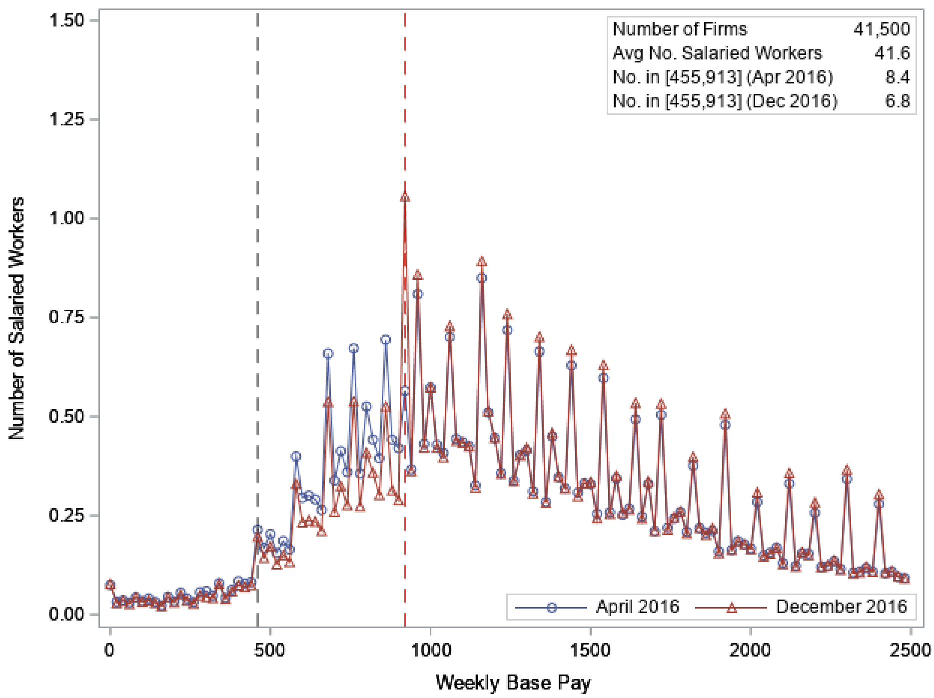


Figure A.3
Frequency Distribution of Salaried Workers' Base Pay using $20 Bins

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.

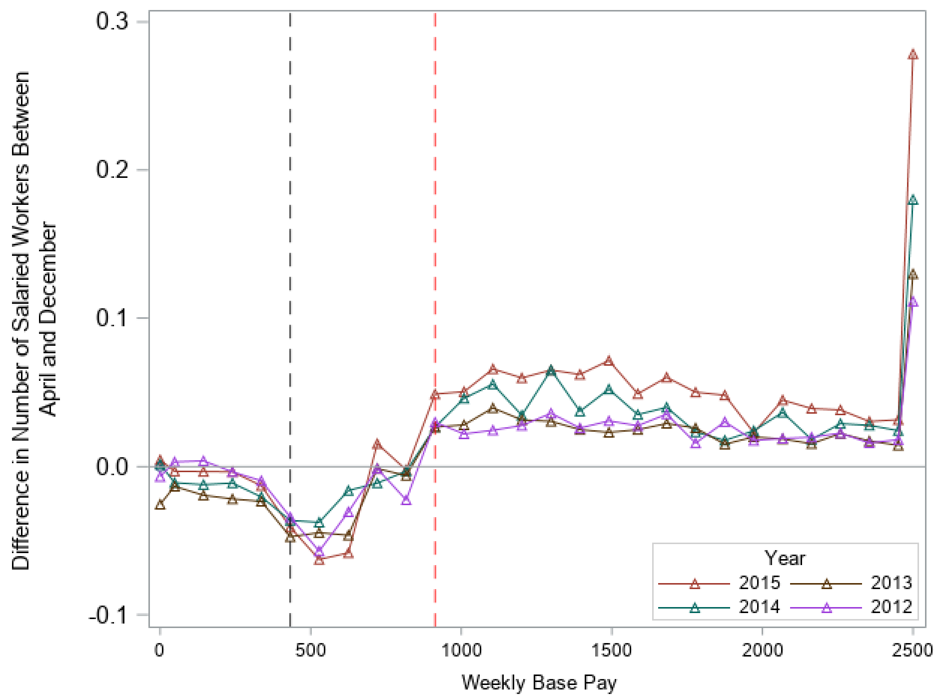
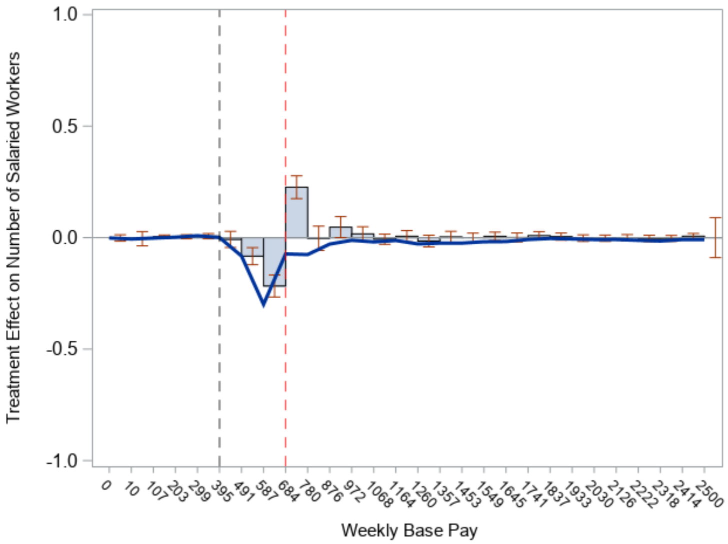
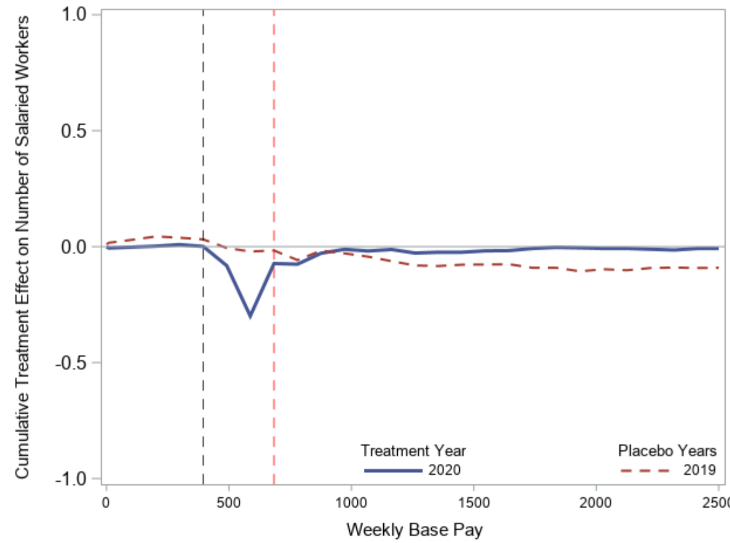


Figure A.4
Difference in Distribution of Salaried Jobs, Omitting 2016

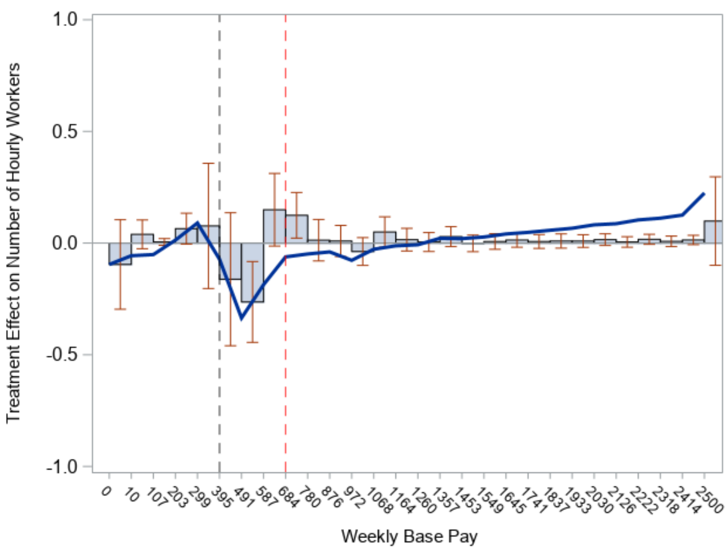
Notes. This figure shows the difference in the frequency distribution of salaried workers' base pay between December and April, by year. This figure is equivalent to figure IIb, except 2016 is omitted to provide a closer look at the previous years.



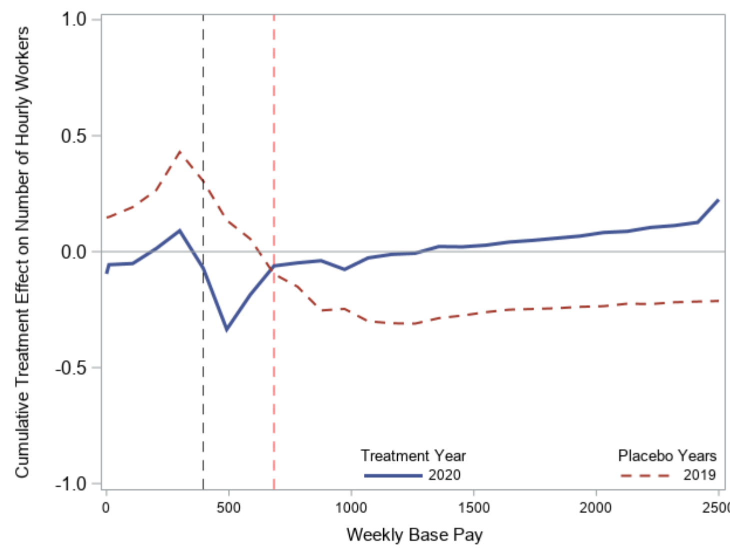
(a) Estimates of Effect on Number of Salaried Jobs



(b) Placebo Test of Effect on Salaried Distribution



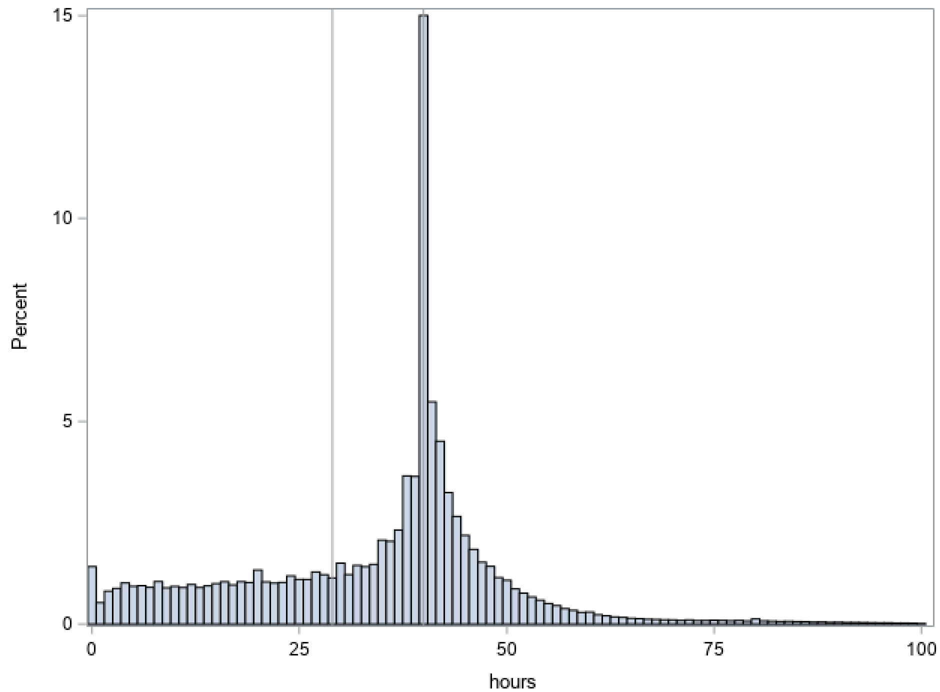
(c) Estimates of Effect on Number of Hourly Jobs



(d) Placebo Test of Effect on Hourly Distribution

Figure A.5
Effect of Raising the 2020 OT Policy on the Frequency Distribution of Base Pay

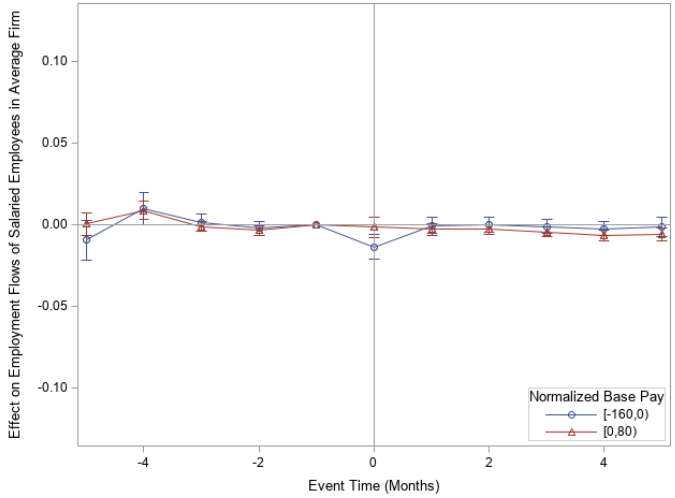
Notes. Panel (a) shows the effect of raising the OT exemption threshold on the number of salaried jobs in each $96.15 bin in Jan 2020, estimated using equation 10. The solid blue line is the running sum of these effects. Panel (b) overlays the cumulative effects in 2020 with a placebo test of the cumulative 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.



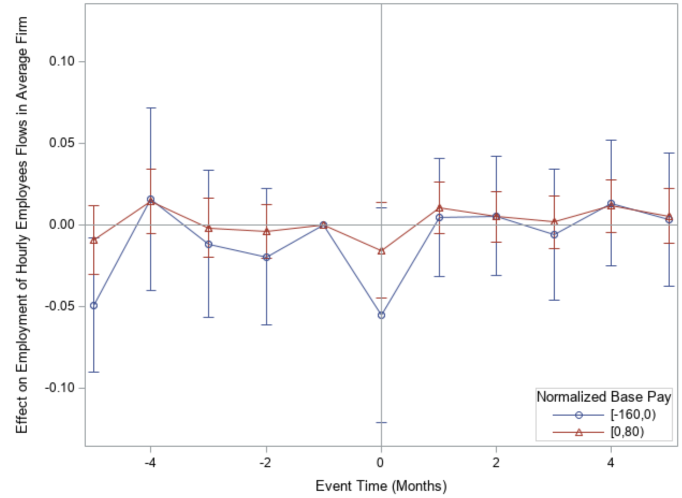
Hours/img0.png

Figure A.6
Distribution of Average Weekly Hours Among Hourly Workers in April 2016

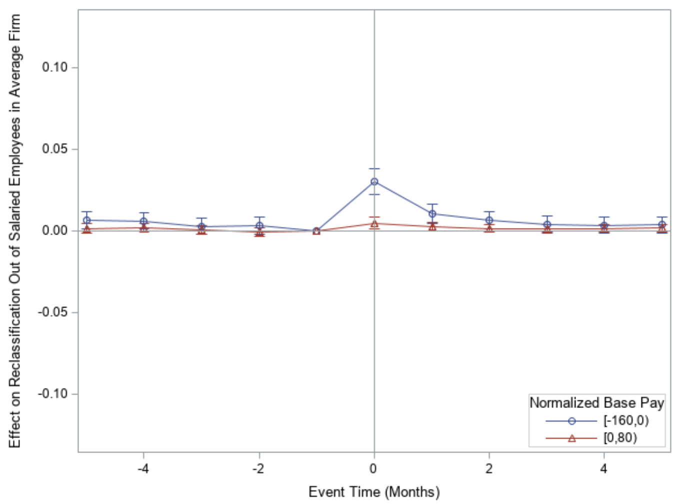
Notes. The figure shows the distribution of average workweeks in April 2016 among all hourly workers. Average weekly hours is imputed from the total hours worked in a month following the methodology in Appendix D. Each bin is a one hour increment. The left vertical line is at 29 hours and the right vertical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



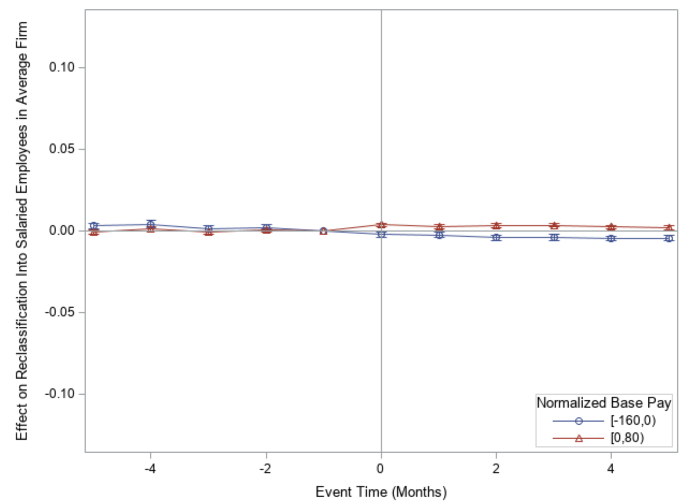
(a) Net Salaried Employment Flows



(b) Net Hourly Employment Flows



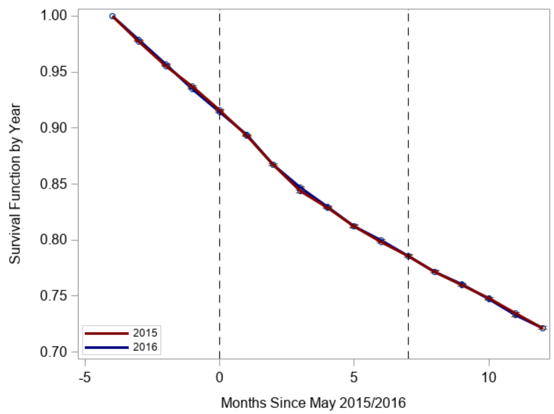
(c) Reclassification Flows Out of Salaried



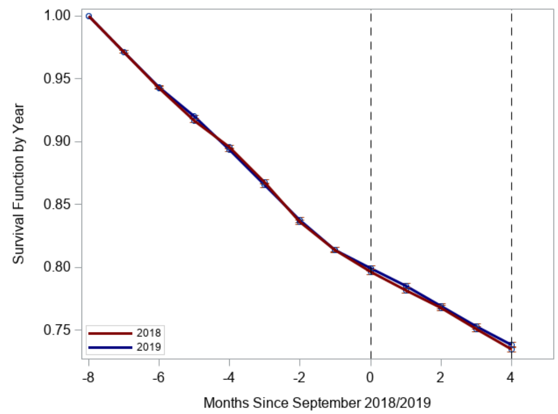
(d) Reclassification Flows Into Salaried

Figure A.7
Effect of State Threshold Changes on the Flow of Workers Into, Out of, and Within Firms

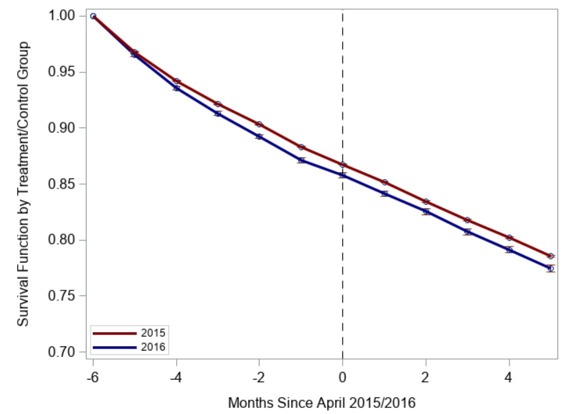
Notes. Panel (a) plots the effect of the state threshold changes on the net employment flow of salaried employees for each month since the threshold increased. Panel (b) plots the analogous figure for net employment flows of hourly employees. Panel (c) plots the effect on the number of salaried workers 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 12, 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.



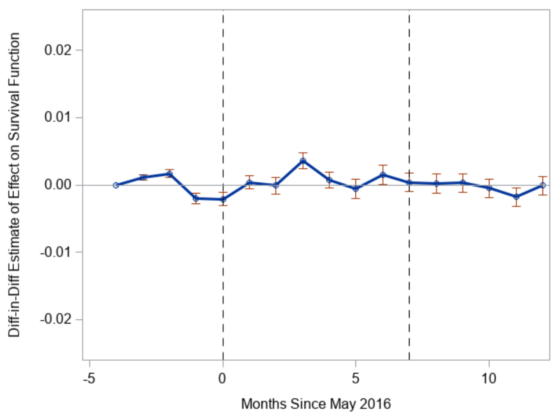
(a) FLSA 2016: Survival Function



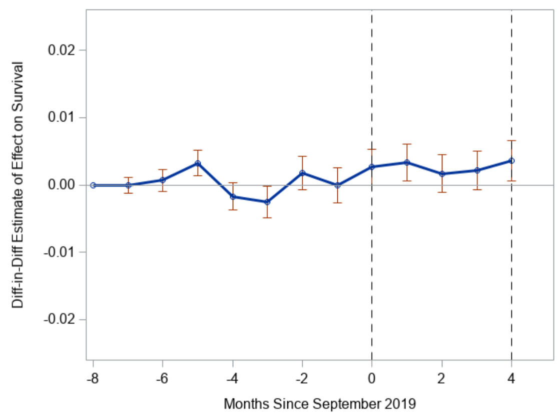
(b) FLSA 2020: Survival Function



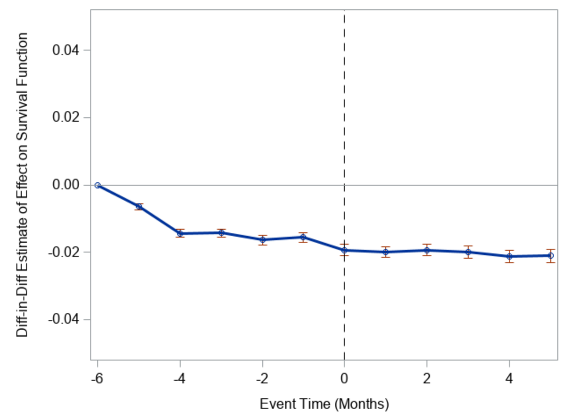
(c) Event Study: Survival Function



(d) FLSA 2016: Effect on Survival Function



(e) FLSA 2020: Effect on Survival Function

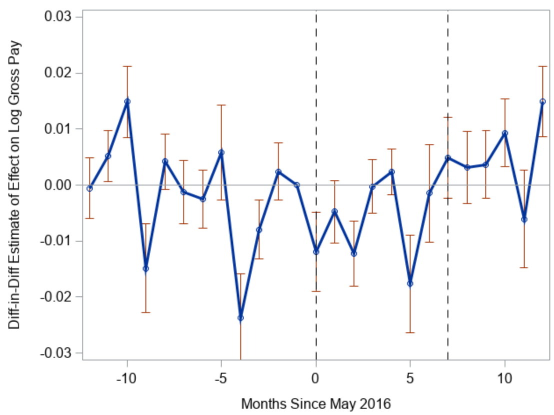


(f) Event Study: Effect on Survival Function

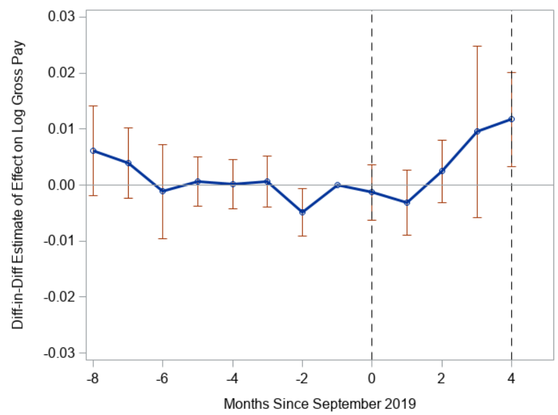
Figure A.8

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

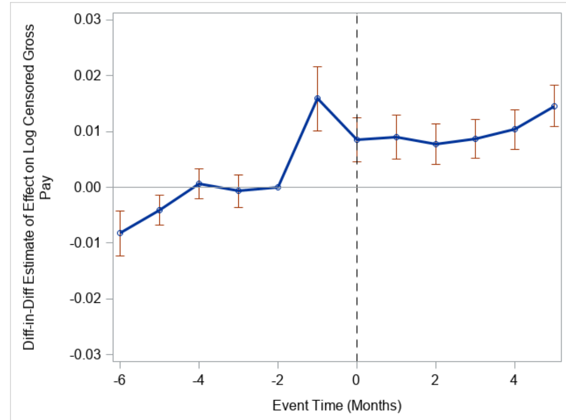
Notes. Panels (a)-(c) plots the survival function of workers directly affected by the 2016 FLSA policy, 2020 FLSA policy, and 16 state policies, respectively, along with the survival function of workers in their respective control groups defined in section VI. Panels (d)-(f) shows the difference in survival 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.



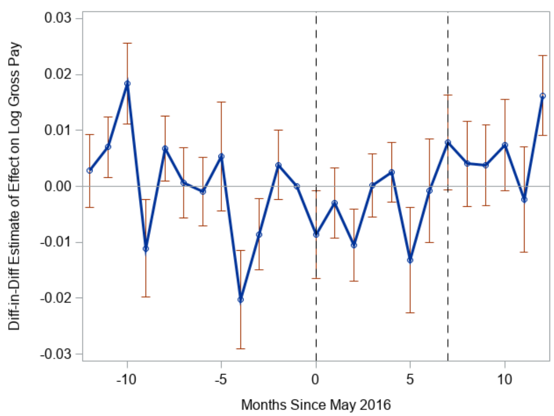
(a) 2016 FLSA: Censored Gross Pay



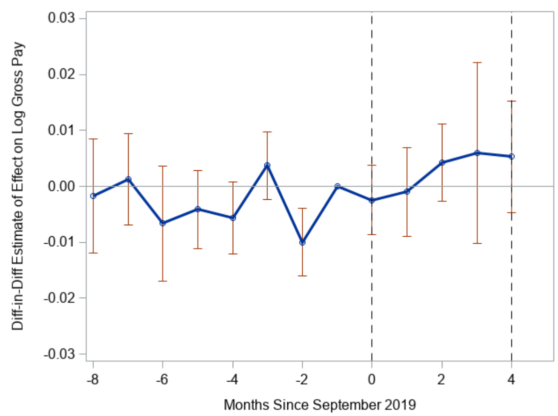
(b) 2020 FLSA: Censored Gross Pay



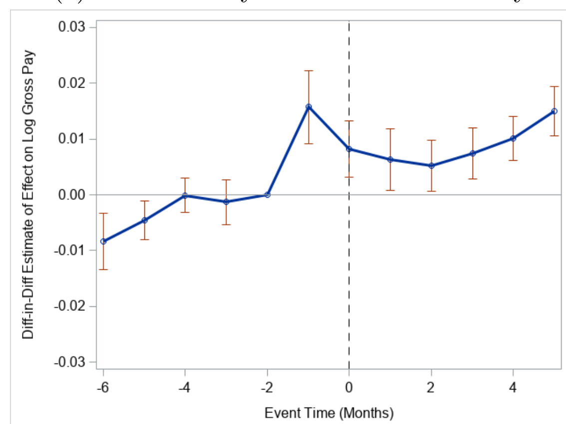
(c) Event Study: Censored Gross Pay



(d) 2016 FLSA: Uncensored Gross Pay



(e) 2020 FLSA: Uncensored Gross Pay



(f) Event Study: Uncensored Gross Pay

Figure A.9
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 13, 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.

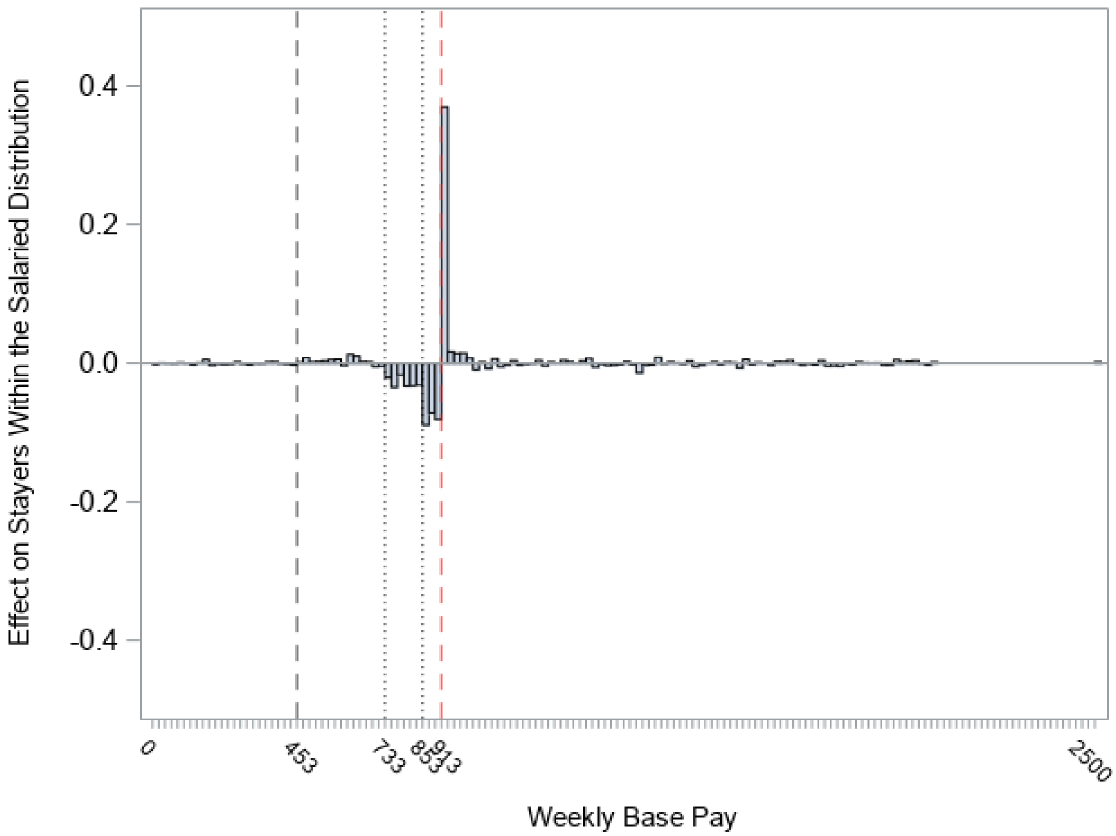


Figure A.10
Effect of Raising the OT Exemption Threshold on the Distribution of Stayers

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 10, 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.

Table A.1
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 10. 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 11. 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 11 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 A.2
Effect of Raising States' OT Exemption Thresholds on the Pay Distribution

	(1)	(2)	(3)	(4)	(5)
Jobs Below Threshold	−0.209***	−0.238***	−0.218***	−0.219***	−0.203**
	(0.012)	(0.014)	(0.017)	(0.017)	(0.088)
Bunched	0.162***	0.138***	0.18***	0.16***	0.150**
	(0.010)	(0.009)	(0.014)	(0.013)	(0.067)
Hourly Jobs	−0.013	0.036	−0.01	−0.01	−0.012
	(0.020)	(0.037)	(0.029)	(0.045)	(0.029)
-Below	−0.091***	−0.043	−0.071**	−0.11***	−0.097**
	(0.023)	(0.037)	(0.033)	(0.043)	(0.048)
-Above	0.078***	0.079***	0.061**	0.100***	0.085
	(0.018)	(0.02)	(0.027)	(0.026)	(0.063)
Employment	−0.060***	−0.064*	−0.048	−0.069	−0.059**
	(0.020)	(0.037)	(0.030)	(0.044)	(0.026)
Treatment Group					
Affected Workers	1.20	1.18	1.10	0.77	14,574
Avg. Firm Size	110	108	62	99	499,394
No. Firm-Events	183,673	164,106	126,777	68,735	16
Sample					
Event Time	0	5	0	0	0
No. Events	16	15	16	6	16
Balanced Firms	No	No	Yes	No	No
Cluster	Firm	Firm	Firm	Firm	State

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 12. Column (3) restricts the sample within each event to only firms that employ workers in both the treatment and control states. Column (4) restricts the sample to only threshold increases that went into effect prior to 2016. Column (5) presents the estimates of an event-study that aggregates firms to the state level. *10%,** 5%,*** 1% significance level.

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).

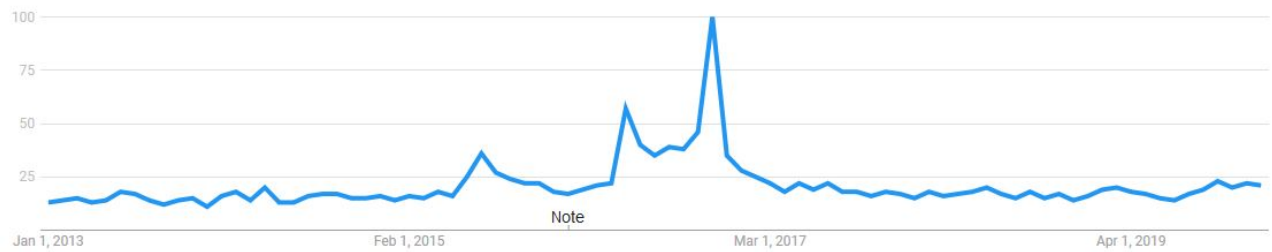


Figure B.1
Google Search Popularity for the Term "FLSA Overtime"

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 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.[41] This announcement received considerable attention from employers, as evident from the spike in Google searches for "FLSA Overtime". In response to the new regulation, "Republican lawmakers, who are close to many of the industries that oppose the new rule, have vowed to block it during a mandated congressional review period". However, given Donald Trump's presidential campaign, there was an understanding that repealing the regulation would be a risky political move for the Republican party as it "could exacerbate an already palpable split between Mr. Trump's blue-collar supporters and the party's establishment donors and politicians" (NYT May 18, 2016). Hence, it was not clear at this point that the rule would be repealed.

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

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

[41] 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.

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 Proof of Proposition 1

Proof. To determine h, hourly workers solve

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

The first order conditions $w(1+p) - a^{-\frac{1}{\epsilon}} h^{\frac{1}{\epsilon}} = 0$ imply that

$$h^* = a[w(1+p)]^\epsilon$$

The overtime premium $p = 0.5 \cdot 1[h > 40]$ creates a kink in the budget constraint, causing workers with $40 = aw^\epsilon$ to increase their hours until $h^* = a[1.5w]^\epsilon > aw^\epsilon = 40$. ■

C.b Proof of Proposition 2

Proof. To determine h, salaried workers solve

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

Suppose p increases from 0 to 1.5. First, I show that hours increase with respect to p. The first order condition to the worker's problem in equation 1 is

$$\begin{aligned} \frac{dU}{dh} &= 0 \\ \Rightarrow \frac{Sp}{40} 1[S \leq \bar{S}] &= a^{-\frac{1}{\epsilon}} h^{\frac{1}{\epsilon}} \\ \Rightarrow h^* &= a\Big[\frac{Sp}{40} 1[S \leq \bar{S}]\Big]^\epsilon \end{aligned} \tag{2}$$

Hours is thus an increasing function of the overtime premium p.

Second, I show that increasing p decreases labor demand. Given h^*, the firm solves

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

giving the first order condition

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

Rearranging for labor demand:

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

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

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

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

The numerator in the second term simplifies to

$$x\alpha h^{\beta-1}\Big[\big(\underbrace{\beta[S(1-p)+R] - (1-\beta)\frac{ph}{40}S]}_{<0 \text{ because } p=1.5}\big)\underbrace{\frac{\partial h}{\partial p}}_{>0} - \frac{h-40}{40}Sh\Big]$$

The term is always negative, implying that labor demand decreases. Intuitively, an increase in the overtime premium induces a scale and substitution effect. In this case, since workers are increasing hours, both these effects point in the same direction, leading to a negative impact on labor demand.

$$\frac{dn^d}{dp} = \underbrace{\frac{\partial n^d}{\partial p}}_{\text{Scale Effect}<0} + \underbrace{\frac{\partial n^d}{\partial h}\frac{\partial h}{\partial p}}_{\text{Substitution Effect}<0}$$

Third, raising p increases labor supply since for any given S, the worker can receive more for the same h. The market clearing salary S^* equates demand and supply: $n^d(S^*) = n^S(S^*)$. A drop in labor demand and an increase in labor supply imply that wages will fall, and the employment effect is ambiguous.

Forth, there will be no bunching at the overtime exemption threshold. Since workers control hours, they will work as few hours as possible if exempt from overtime, leaving firms with zero surplus from the employment relationship. As such firms have no incentives to raise salaries above the market clearing rate to the exemption threshold. ■

C.c Proof of Proposition 3

Proof. The firm take salary S as given. It chooses hours and employment to maximize profits:

$$\max_{(n,h)} \pi = xn^\alpha h^\beta - [S(1 + p\frac{h-40}{40}1[S \leq \bar{S}]) + R]n \tag{3}$$

If $p = 0$, then for any S, the firm will have workers work as many hours as possible because the marginal cost per hour of labor is zero. However, increasing hours will reduce the number of workers willing to supply labor to the firm. Given the firm's ability to control hours, it will therefore always be on the worker's extensive labor supply curve. As such, when $p = 0$, the firm behaves as a monopsonist, maximizing equation 3 subject to the following constraint:

$$U(S,h) = S(1 + p\frac{h-40}{40}1[S \leq \bar{S}]) - a^{-\frac{1}{\epsilon}}\frac{h^{1+\frac{1}{\epsilon}}}{1+\frac{1}{\epsilon}} \geq r_n \tag{4}$$

Rearranging equation 4 for S, substituting it into equation 3 and taking first order conditions, the firm's decision solves

$$\frac{d\pi}{dh} = x\beta n^\alpha h^{\beta-1} - a^{-\frac{1}{\epsilon}}h^{\frac{1}{\epsilon}} = 0$$
$$\frac{d\pi}{dn} = x\alpha n^{\alpha-1}h^\beta - [r_n + a^{-\frac{1}{\epsilon}}\frac{h^{1+\frac{1}{\epsilon}}}{1+\frac{1}{\epsilon}} + R] - r'(n)n = 0$$

The first order conditions implicitly define n and h, which can be substituted back into the worker's labor supply function to solve for S.

Next, suppose p increases from 0 to 1.5. The market for a specific job can respond in one of three ways, depending on the fundamental parameters of the job (i.e. α, β, R):

Case 1: Firms cut hours (i.e. $\frac{\partial h}{\partial p} < 0$). If β is small, then employment increases and effect on salary is ambiguous. If β is large, then salaries fall and employment is ambiguous.

First, I show that firms cut hours as p increases. The first order conditions to equation 3 are

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

Ratio of the first order conditions:

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

Solve for hours:

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

Hours is a decreasing function of the premium p.

Second, I show that the effect on labor demand is ambiguous and depends on the magnitude of β. Intuitively, an increase in p induces opposing scale and substitution effects on labor demand:

$$\frac{dn^d}{dp} = \underbrace{\frac{\partial n^d}{\partial p}}_{\text{Scale Effect}<0} + \underbrace{\frac{\partial n^d}{\partial h}\frac{\partial h}{\partial p}}_{\text{Substitution Effect}>0}$$

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

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

To determine the change in labor demand, I follow the same argument as in proof C.b. Take the derivative of $n^D(S, h(S,p), p)$ with respect to p:

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

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

The numerator in the second term simplifies to

$$x\alpha h^{\beta-1}\Big[\underbrace{\big(\beta[S(1-p)+R]-(1-\beta)\frac{ph}{40}S]\big)\frac{\partial h}{\partial p}}_{\text{Substitution Effect>0}}\underbrace{-\frac{h-40}{40}Sh}_{\text{Scale Effect<0}}\Big]$$

Since p=1.5, the substitution effect is positive for all β. On the other hand, the scale effect is negative. To show that the sum can be positive or negative, I show that it is positive at $\beta = 0$ and strictly decreasing in β. 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 1.5 times the reduction in hours exceeds the amount of overtime hours. 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 all overtime hours. The derivative of the sum of the substitution and scale effects is $[S+R+\frac{pS(h-40)}{40}]\frac{\partial h}{\partial p} < 0$ so the employment effect is strictly decreasing in β. At a sufficiently large β, the employment effect will therefore be negative.

Third, I show that in equilibrium, one of the effects on employment and salary is ambiguous, depending on what happened to labor demand. Equilibrium (n^*, S^*, h^*) are characterized by the firm's two first order conditions and workers' extensive labor supply equation. 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. As shown before, 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.

Case 2 (Bunching): Some jobs receive a raise to exactly the overtime exemption threshold $\bar{S}$. Jobs initially paid close to the threshold are more likely to receive a raise.

The equilibrium outcome (n^*, S^*, h^*) must satisfy $\pi(n^*, S^*, h^*) \geq \pi(n^*, S, h^*)$ for every salary $S > S^*$, otherwise the firm would increase salaries. Since only employees earning less than $\bar{S}$ are covered for overtime, then at the value of n^* and h^* in case 1, 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 $\bar{S}$. 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. For these jobs, a stable equilibrium is characterized by $S = \bar{S}$, and (n, h) are determined by the firm's first order conditions as functions of $\bar{S}$. At this equilibrium, firms have no incentives to deviate hours or employment. Even though workers are willing to accept a lower base salary to work, that is not incentive compatible for the firm. Similar to a minimum wage, employment is no longer on the worker's extensive labor supply curve.

Note that by raising salaries, the bunching effect leads firms to reduce employment relative to baseline levels. Nevertheless, it is still true that for small values of β, employment would rise. If firms do not value long work hours, they would simply reduce hours to 40. In that case, raising salaries to the threshold would only increase labor costs.

Case 3 (Reclassification): Some jobs are reclassified from salaried to hourly.

For jobs with large values of R, the value of classifying the job salaried was initially only marginally better than having the job hourly (i.e. $\pi^{sal} - \pi^{hr} = \epsilon > 0$). After p increases, the cost of overtime reduces the profitability of salaried jobs so that it is now more profitable to classify these jobs as hourly: $\pi^{sal} - \pi^{hr} < 0$. ∎

C.d Proof of Proposition 4

Proof. The firm takes wage w as given. It chooses hours and employment to maximize profits:

$$\max_{(n,h)} \pi = xn^{\alpha}h^{\beta} - [w(h + p(h - 40)) + F]n \tag{5}$$

The first order conditions are

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

Taking the ratio of the first order conditions implies

$$\frac{\alpha}{\beta}h = \frac{w(h + p(h - 40)) + F}{w(1 + p)}$$

Isolating h, I show that hours is a decreasing function in p, which is the premium that goes into effect for each hour worked above 40 per week:

$$h = \frac{\beta}{\alpha + \beta} \frac{F - 40wp}{w(1+p)}$$

■

C.e Proof of Proposition 5

Proof. **Market:** Suppose the market determines a earnings-hour profile $Y(h)$ where $Y'(h) > 0$ for every h.
Firm: The firm takes the function $Y(h)$ as given and solves

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

The first order conditions are

$$\frac{d\pi}{dh} = x\beta n^{\alpha}h^{\beta-1} - Y'(h)n = 0 \tag{6}$$
$$\frac{d\pi}{dn} = x\alpha n^{\alpha-1}h^{\beta} - Y(h) = 0 \tag{7}$$

Worker: Workers take the function $Y(h)$ as given and solve

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

The first order condition is

$$\frac{dU}{dh} = Y'(h) - a^{-\frac{1}{\epsilon}}h^{\frac{1}{\epsilon}} = 0 \tag{8}$$

Equilibrium: Equilibrium consists of values of h^*, n^*, Y^*, and $Y'(h^*)$ such that

1. Both parties are indifferent to switching their hours: h^*, n^* and $Y'(h^*)$ satisfy equations 6 and 8. Together, the two first order conditions imply

$$h^*(n) = \left[\frac{a^{\frac{1}{\epsilon}}x\beta}{n^{1-\alpha}}\right]^{\frac{1}{1+\frac{1}{\epsilon}-\beta}}$$

2. The firm is indifferent to hiring more or fewer workers: n^*, h^*, and Y^* satisfy equation 7. Substituting $h^*(n)$ into equation 7 and solving for n^* implies the following labor demand function:

$$n^D(Y^*) = \left[a^{\frac{1}{\epsilon}} x\beta\right]^{(1-\alpha)(1+\frac{1}{\epsilon})} \left[\frac{x\alpha}{Y^*}\right]^{\frac{1+\frac{1}{\epsilon}-\beta}{(1-\alpha)(1+\frac{1}{\epsilon})}}$$

Subbing $n^D(Y^*)$ back into $h^*(n)$ gives hours as a function of Y^*

3. The labor market clears: $n^D(Y^*) = n^S(Y^*)$ where extensive labor supply $n^S(Y, h)$ can be written only in terms of Y^* and model primitives. Given the Y^* that clears the market, I can write n^* and h^* in terms of model parameters, and then the worker's first order condition defines $Y'(h^*)$.

Comparative Statics:

Case 1: Hourly jobs

To start, suppose earning-hour profiles are defined in terms of only an hourly wage contract. In that case, without overtime coverage, $Y'(h)$ is constant for every h. An increase in the overtime premium from $p = 0$ to $p = 0.5$ imposes a new constraint on the earnings-hour profile. Namely, suppose $Y'(h) = w$ is constant for every $h < 40$, then $Y'(h) \geq 1.5w$ for every $h > 40$. Initially, the equilibrium outcomes are independent of the overtime premium. Thus, as long as the earnings-hour profile adjusts in a way that the new constraint holds without violating any of the previous first order conditions, the equilibrium will not change.

First, I show that the initial equilibrium cannot be maintained by simply reducing the wage rate $Y'(h)$. Suppose the equilibrium was initially at $h^* > 40$, n^*, Y^*, and $Y'(h^*)$. Also suppose $Y'(h) = w$ for every h. In that case, I can express weekly earnings Y^* as follows:

$$Y^* = \int_0^{h^*} w dh \tag{9}$$

After overtime coverage, the law requires that

$$Y^* = \int_0^{40} Y'(h) + \int_4 0^{h^*} 1.5Y'(h) dh \tag{10}$$

However, equation 10 cannot hold if the slope of the earnings-hour profile still satisfies the first order conditions. For the FOC to be satisfied, the slope at h^* must remain the same as it was in the initial equilibrium: $Y'(h^*) = w$. That leads to a contradiction:

$$
\begin{aligned}
Y^* &= \int_0^{40} Y'(h) + \int_4 0^{h^*} 1.5Y'(h)dh \\
&= \int_0^{40} \frac{w}{1.5} + \int_4 0^{h^*} wdh \\
&< \int_0^{h^*} wdh \\
&= Y^*
\end{aligned}
$$

In order for Y^* to stay the same, the wage rate at h^* must be higher than its initial value.

Second, I present a simple solution to satisfy the overtime regulation for hourly jobs, while also maintaining the initial equilibrium. Let $D = \int_0^{h^*} wdh - \int_0^{40} \frac{w}{1.5} + \int_4 0^{h^*} wdh$ be the difference between the total earnings at baseline and the total earning if the same wage rate remained constant at the point h^*. To satisfy the law while keeping earnings and wages constant at h^*, the new earnings profile can offer hourly workers a wage $\frac{w}{1.5}$ for the first 40 hours, a bonus D for working above 40 hours, and then a wage w for all hours above 40.

Case 2: Salaried jobs

Now consider the case of salaried jobs. I interpret $Y(h)$ as a *market* level earnings-hour profile. That is, jobs that work more hours are paid a higher salary. Within a job, salaried workers simply work hours h for a salary S that implicitly depends on h at the market level, but does not explicitly depend on hours in the employment contract. Define the implied wage $w = \frac{S}{40}$, be the salary divided by 40. The law requires that $Y'(h) = 1.5w$ for every $h > 40$. Given the notation, the same proof that showed wages would decrease for hourly jobs would show that implied wages, and by tension salaries, would decrease for salaried jobs. However, in this case, each hour-salary pair should be interpreted as a job, so that to change hours, a worker needs to switch employment. ∎

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+4·4) 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

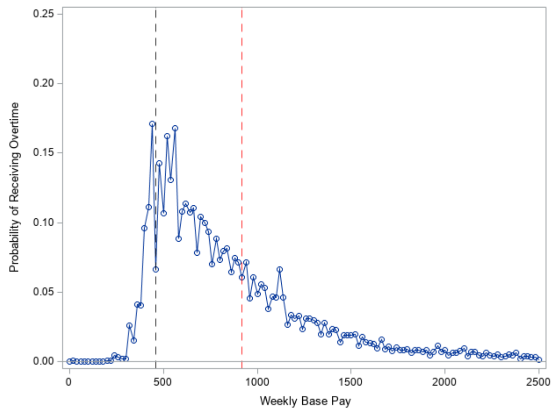
While the measure of base pay that the Department of Labor uses to determine overtime eligibility is denominated at the weekly level, workers' gross pay and overtime pay are recorded at the monthly level in the data. In this section, I explain the procedure I use to standardize these two key measures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

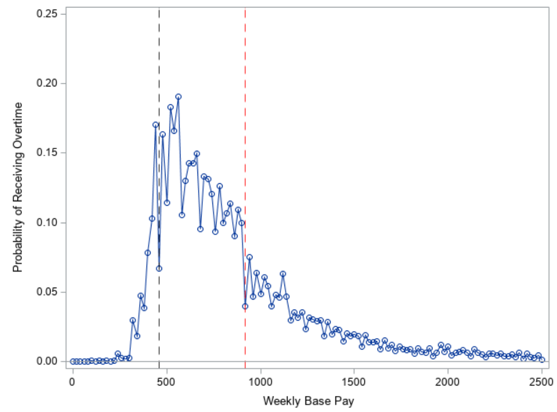
To express the monthly gross and overtime pay variables at the weekly level, I normalize it by the number of paychecks they receive each month and the number of weeks covered per paycheck:

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

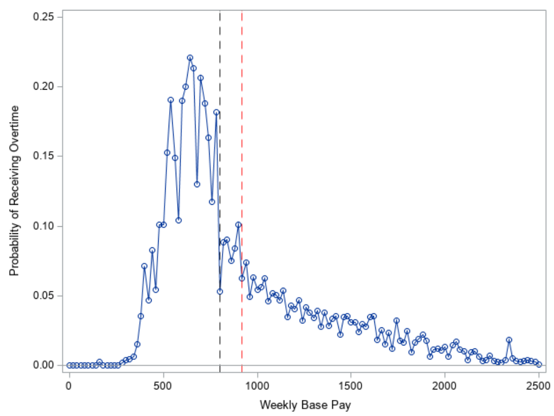
This scaling calculation is simple to compute for observations after 2016 since I observe the



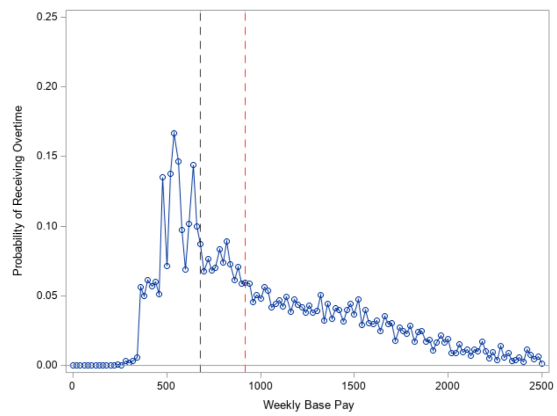
(a) FLSA states, April 2016



(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 in 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.

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

I define $\frac{\text{paychecks}}{\text{month}} = 1$ for workers paid monthly and $\frac{\text{paychecks}}{\text{month}} = 2$ for workers paid semi-monthly. For weekly and biweekly paid workers, the number of paychecks received each month depends on both the day of the week that each worker gets paid, and the number of times that day appears in the month. For instance, if a worker gets paid on a Thursday every two weeks, then the worker's gross pay includes 3 paychecks in December 2016 when there were 5 Thursdays, but only 2 paychecks in April 2016. To illustrate this problem, I plot in

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.

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.

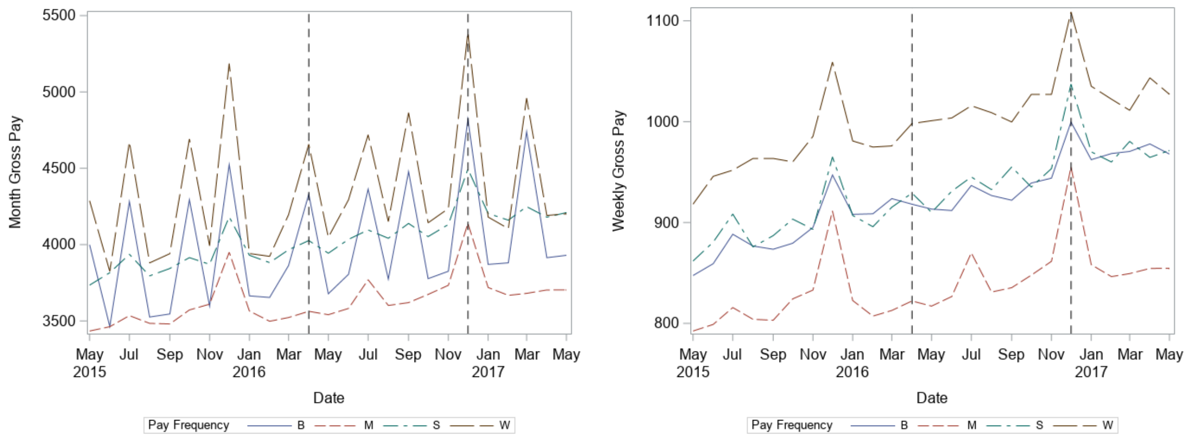
While different workers may receive an extra paycheck in different months, employees of the same firm tend to receive a paycheck on the same day of the month, conditional on their pay frequency. To impute the number of paychecks per month that each firm issues in a month, I apply the following algorithm:

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,



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

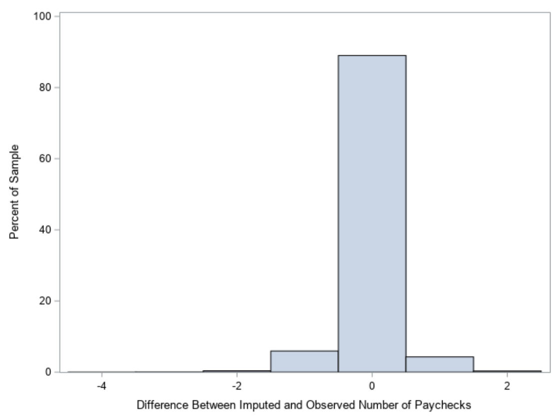
Appendix Figure D.2
Gross Income, by Pay Frequency

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.

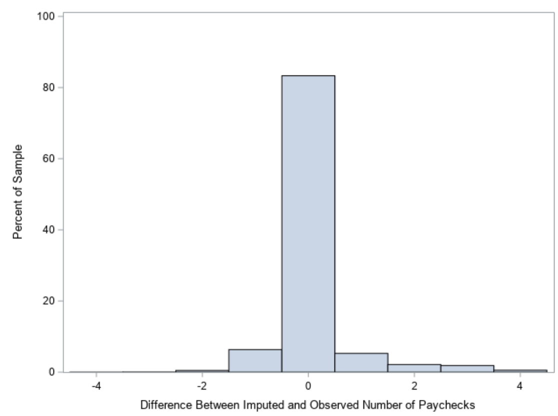
and 4 otherwise.

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

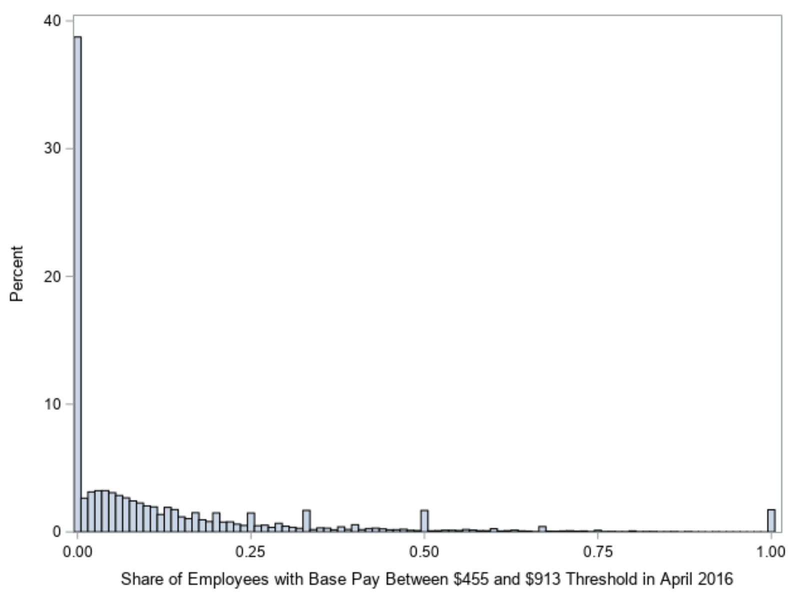
Notes. Panel (a) shows the distribution of the difference between imputed and actual number of paychecks per month, for all worker-months in 2016 where the worker is paid biweekly. Panel (b) shows a similar distribution for workers who are paid weekly.

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 April 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, the measure of firm size 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 is driven by large firms, there may be concern about the representativeness 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 is 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 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.

Appendix F. 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 find 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 (DOL, 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 F.1, 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 may not fully capture the cost of the policy to employers. Overtime is a small component of workers' earnings partly due to the actions of employers to reduce weekly hours, such as the bunching of workers' hours at 40 as depicted in appendix figure A.6. This behavioral response may also entail costs if employers value long work hours. To capture these costs, an arguably more relevant comparison population is the group of hourly workers in the right tail of overtime pay distribution, since they may be less affected by employers' adjustments. Moreover, if one is willing to assume that any employment loss from the policy stems from workers who would have saw the largest increase in overtime pay, then the right tail of hourly workers' overtime pay is a better reflection of the cost of these marginal jobs.

I show in figure F.1 that while a third of hourly workers do not earn overtime, the distribution of overtime pay exhibits a long right tail indicating that there exist some workers for whom overtime is a considerable share of their income. Column (1) of table F.1 confirms that for the top 10% of the distribution, overtime comprises at least 8% of hourly 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. 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 as an endogenous outcome that already accounts for actions by employers to reduce hours.

If one is concerned that hourly workers' overtime pay is not a good counterfactual for those of salaried workers, I next show that my results are robust to a benchmark that uses salaried workers' hours. 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 the ADP data. In column (3) of table F.1, 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 is clear that workers who are not covered for overtime are significantly less likely to receive overtime compensation.[42] Even among those who are already covered, the amount of overtime pay is attenuate by employers' decisions 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, 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 formula. Nevertheless, I compute the implied cost of overtime coverage using both standards. 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%).

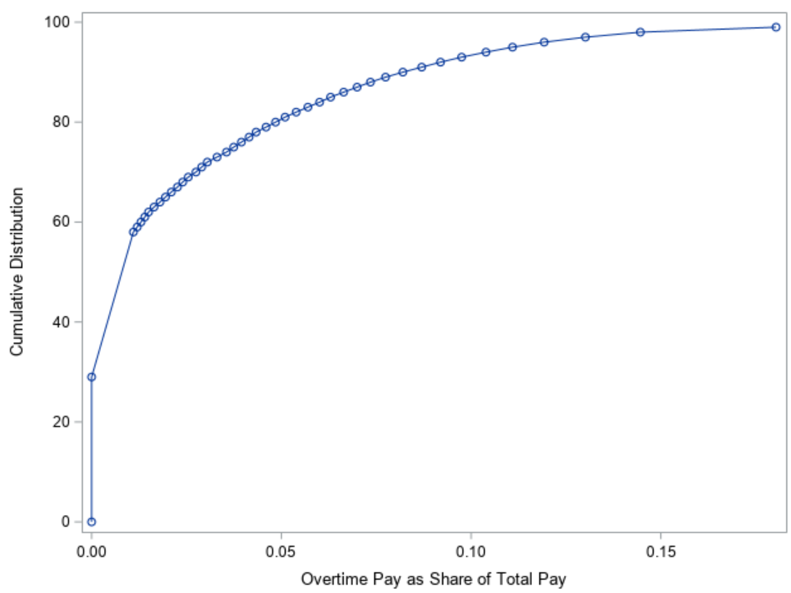
In summary, while overtime appears to be relatively cheap on average, I expect it to be very expensive for at least a tenth to a fifth of affected salaried workers. These are the jobs that would be expected to disappear from an expansion in overtime coverage.

[42] 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 F.1
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	Salaried	Salaried
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.



Appendix Figure F.1
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 G. Derivation of FLSA Estimator

If the coefficients in equation 9 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 9, I estimate the β_{jk}'s as the difference between $\Delta\bar{N}_{jkt}$ and $\hat{\alpha}_{jkt}$. ■

Appendix H. Analysis of 2016 FLSA policy on Flows

This appendix explains the assumptions used in the heterogeneity analysis in section VII.b, and presents some evidence in support of these assumptions.

H.a 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. Although by construction, N^j_{Apr} only includes salaried jobs, N^j_{Dec} may include both salaried and hourly jobs as long as they were salaried in April 2016. The difference in the number of workers between these two months, within a bin j, can be decomposed as follows:

$$N^j_{Dec} - N^j_{Apr} = \underbrace{N^{kj}_{S_0,S_1} - N^{jk}_{S_0,S_1}}_{\text{Within Classification } (\Delta N_{S_0,S_1})} + \underbrace{N^{kj}_{S_0,H_1} - N^{jk}_{S_0,H_1}}_{\text{Reclassifications } (\Delta N_{S_0,H_1})} - \underbrace{N_{S_0,u_1}}_{\text{Separations}} \tag{11}$$

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. The above identity states that changes in the number of workers from a bin j can be decomposed into three components: the flow of jobs between all other bins k while remaining salaried, the flow of jobs between all other bins k while switching to hourly, and separations to unemployment.

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 11 into the selection bias term:

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

To remove the selection bias from changes among incumbents, I impose similar assumptions as section V.a. That is, 1) the policy had no effect above a cutoff bin j^*, and 2) changes in the distribution each year differ by a scalar multiple. Following the same argument as earlier, $E\Big[\Delta N^C_{16,S_0}\Big] - \gamma E\Big[\Delta N^C_{15,S_0}\Big] = 0$

To remove the selection bias from separations, I assume that the policy had no effect on separations. This appears reasonable given the distribution of separations in 2016 relative to previous years from figure H.5a. 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. I provide evidence in the next section that workers reclassified as a result of the regulation earned a similar base pay pre-and-post rule change (see figure H.3c). 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, the transformation of 2015's distribution models the counterfactual distribution in 2016:

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

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

H.b Within-Classification Flows

I presented estimates of the effects on always-salaried workers in figure A.10, and discuss their implications in section VII.b. In this section, I present some evidence of the validity of this empirical strategy. First, as a placebo test, figure H.1 repeats the same analysis using previous adjacent years of data and do not observe any bunching in the years prior to 2016. This suggests that in a usual year, the model successfully approximates changes to the distribution of incumbent workers. As a second placebo check, figure H.2 reveals that the 2016 FLSA policy had negligible effects on always-hourly workers and that the effects do not differ significantly from those of the placebo years. This is to be expected since the policy never targeted workers who were already covered for overtime. I check the effects of the rule change on reclassifications and separations in the following sections.

H.c Reclassification Flows

In figures H.3a and H.3b, I plot the distribution of reclassifications out of and into the salaried distribution, respectively. Visually, there is a clear increase in the number of reclassifications from salaried to hourly status in 2016 compared to previous years, and a decline in reclassifications in the opposite direction. Moreover, individuals who do transition from hourly to salary are more likely to become bunched at the new threshold.

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 $\gamma_0 = 0$ (i.e. this is a cross-year difference-in-difference with no scaling adjustments). 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 figure H.4).

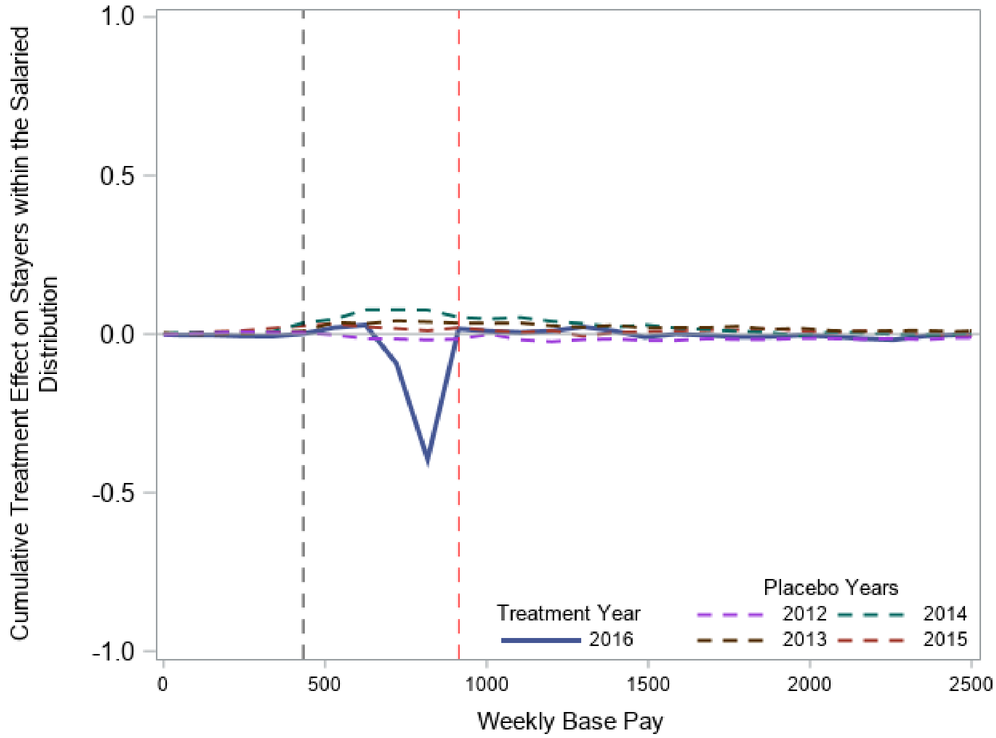
Figure H.3c overlays the estimates of the net reclassification effects into the salaried and hourly distributions. There are three findings to highlight. First, jobs across the entire range of affected base pays are reclassified, including those right below the threshold and even those right above it. 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 two distributions, I also plot their difference in figure H.3d. 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.

H.d Employment Flows

I now turn to the question of how the 2016 FLSA rule change affected employment flows. Figures H.5a and H.5b 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 appendix figure A.8 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).

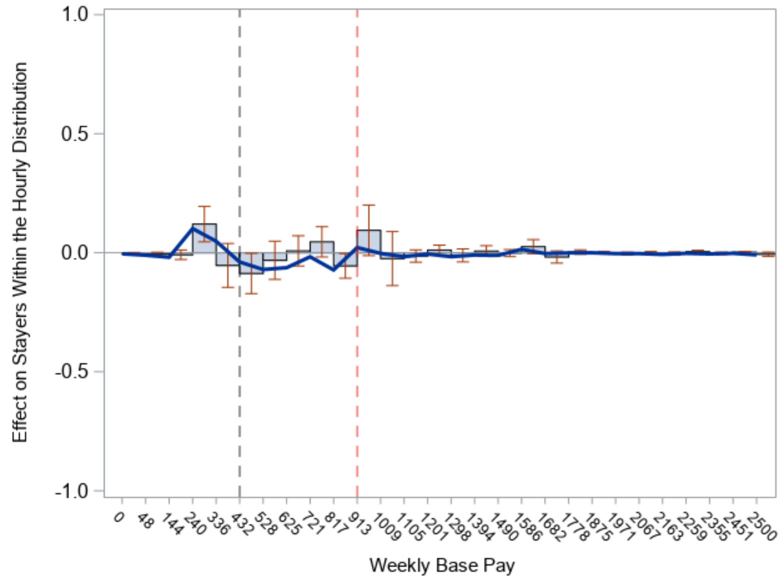
Given that approximately 4.1% (s.e. 4.2%) of affected job were lost from the FLSA rule change (see table II), it may seem surprising such a large employment response can be driven solely by hires since monthly hiring rates are only around 3-5%. Putting aside the imprecision of the employment estimates, this claim can be reconciled by noting that the reduction in employment took place over multiple months. To show this, figure H.6 plots the cumulative effect on net employment flows over time from January to December 2016.[43] Broadly, the figure 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.

[43] The estimate for December corresponds to the cumulative sum in figure VIIb. To validate this approach, figure H.7 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.

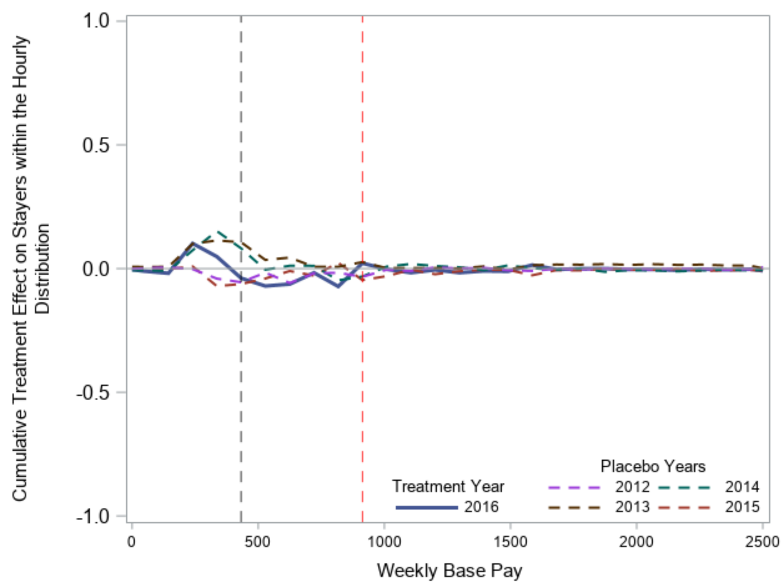


Appendix Figure H.1
Placebo Test of Effect of 2016 FLSA Policy on Always-Salaried Workers

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 10, 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.



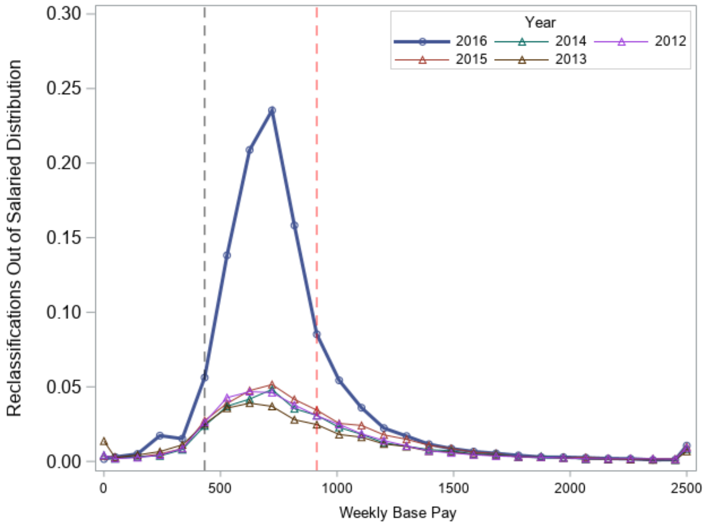
(a) Estimates of the Effect on Number of Salaried Workers



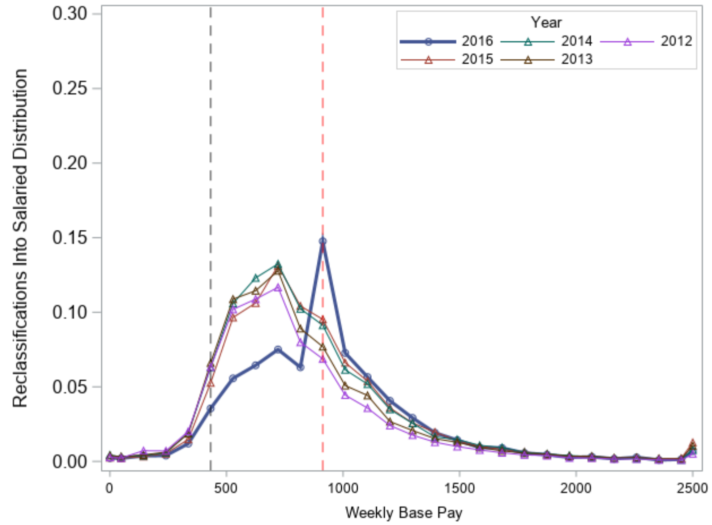
(b) Placebo Test of Effect on Salaried Distribution

Appendix Figure H.2
Effect on Flows Within the Hourly Distribution

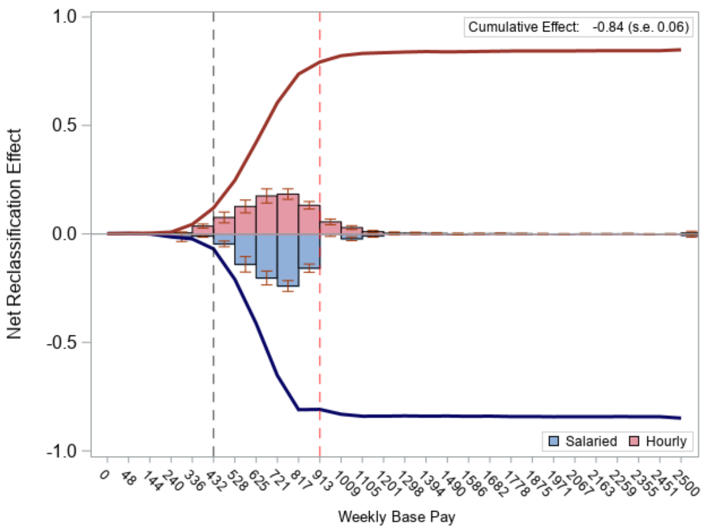
Notes. Panel (a) shows the effect of the 2016 OT policy on the distribution of workers who stay at the same firm between April and December 2016, and are paid by hour in both months, estimated using equation 10 while assuming $\gamma_0 = 0$. The solid blue line is the cumulative sum of the bin-specific 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.



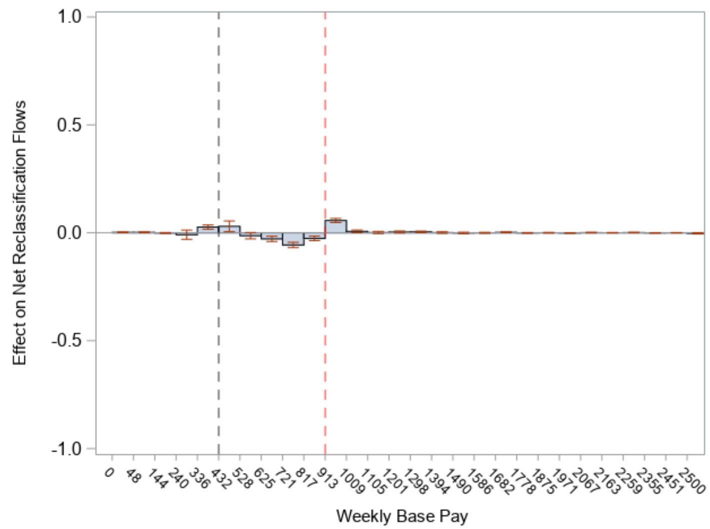
(a) Reclassifications Out of Salaried



(b) Reclassifications into Salaried



(c) Effect on Net Reclassifications

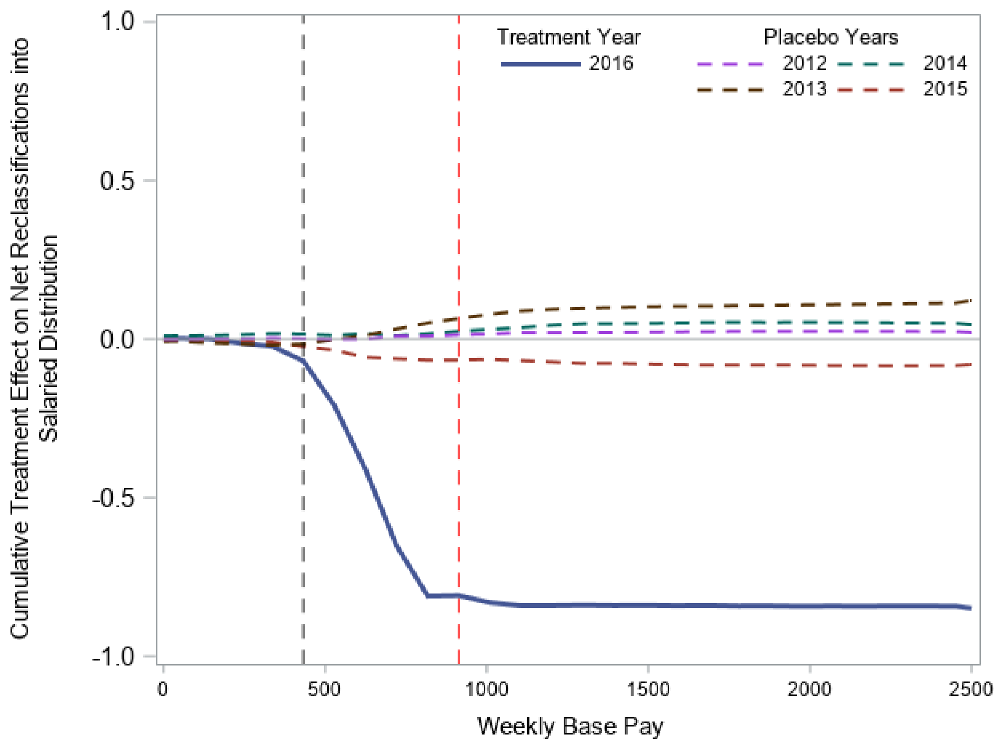


(d) Effect on Base Pay of Reclassified Workers

Appendix Figure H.3

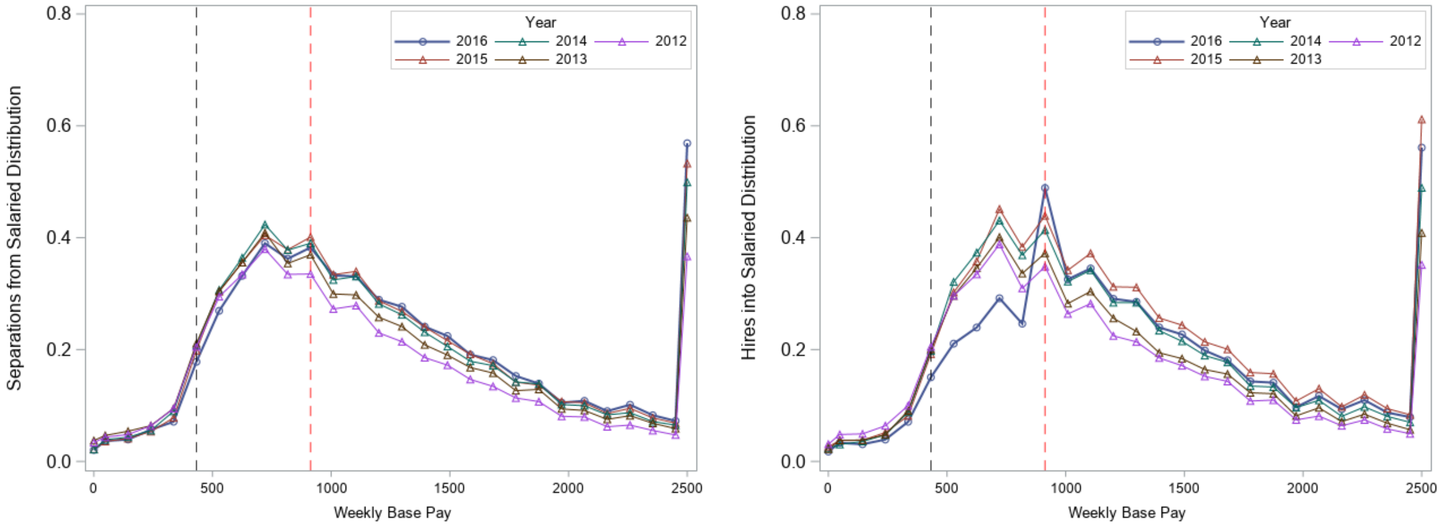
Effect of Raising the 2016 FLSA OT Exemption Threshold on the Distribution of Reclassification Flows

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 10 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.



Appendix Figure H.4
Placebo Test of Net Reclassification Flows into the Salaried Distribution

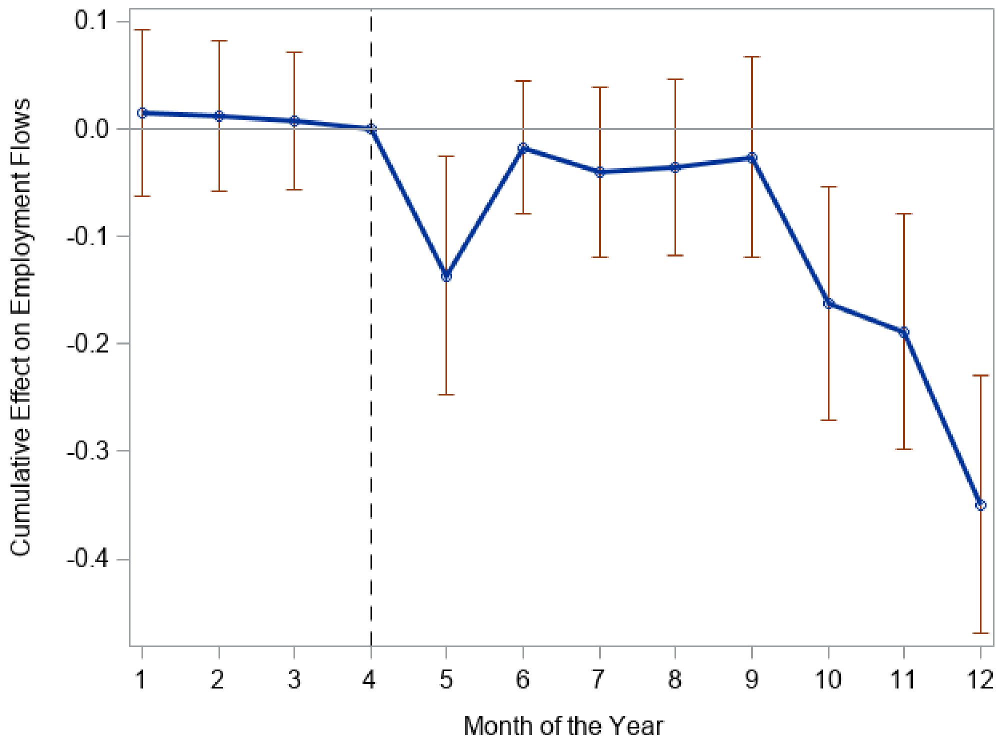
Notes. This figure shows the cumulative sum of the effect of the 2016 FLSA policy on net reclassifications into the salaried distribution, estimated using equation 10 assuming γ_1=1 and γ_0=0. Each line is estimated as the reclassification flows in the year indicated in the legend minus the reclassification 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.



(a) Distribution of Separations from Salaried Jobs (b) Distribution of New Hires into Salaried Jobs

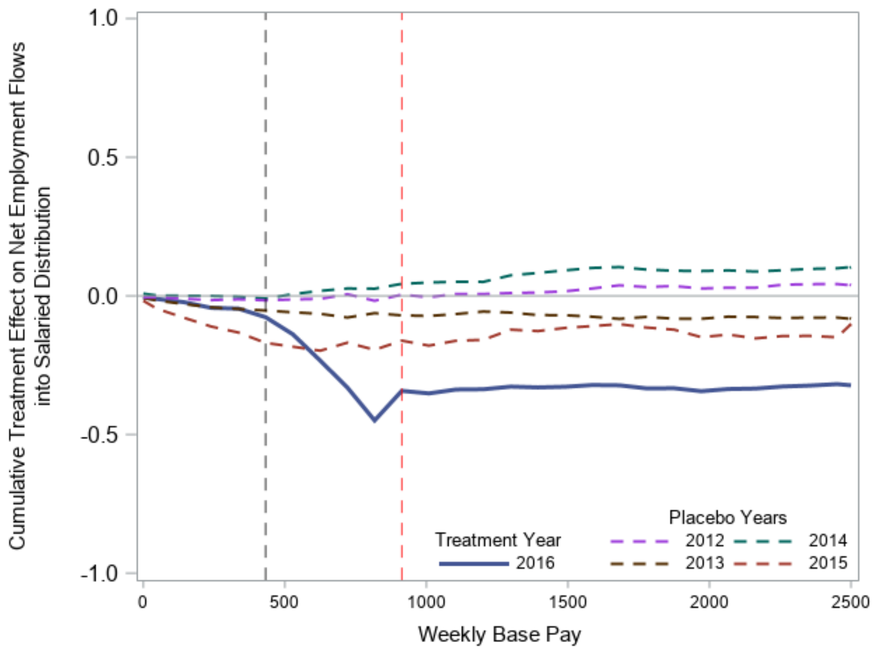
Appendix Figure H.5
Separations and Hires Between April and December, 2012-2016

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. In both figures, the left and right vertical lines are at the initial and proposed 2016 FLSA thresholds, respectively.



Appendix Figure H.6
Net Employment Flows into the Salaried Distribution Over Time, 2016

Notes. This figure shows the effect of the 2016 FLSA policy on net employment flow into the salaried distribution since April 2016, estimated from equation 10.



Appendix Figure H.7
Placebo Test of Net Employment Flows into the Salaried Distribution

Notes. This figure shows the cumulative sum of the estimates of equation 10 using the number of hires minus separations within each bin as the outcome variable.