The Labor Market Effects of Expanding Overtime Coverage

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Abstract

This paper estimates the employment, income, and reclassification effects of expanding overtime coverage for salaried workers. In the United States, employers are required to pay salaried employees who earn less than the “overtime exemption threshold” an additional one-and-a-half times their implied wage for each hour worked above 40 within a week. I evaluate the impact of 18 state and federal increases in this threshold between 2014 and 2020 using detailed administrative payroll data that covers over one-tenth of the U.S. workforce. Contrary to the two prevailing theories of overtime, I find both negative employment and positive income effects. First, while the labor demand model predicts that expanding overtime coverage would raise employment as firms substitute workers for hours, I estimate that 4 jobs are lost and 10 are reclassified from salaried to hourly for every one hundred workers who would gain coverage under a new threshold. Second, inconsistent with the predictions of the compensating differentials model of overtime, I find no evidence that firms reduced workers’ base pay to offset the increase in overtime pay. Instead, workers directly affected by a rule change experienced a 1.2% increase in their total income due to both a rise in overtime pay and a bunching effect whereby employers raised workers’ salaries to the new threshold to keep them exempt. I show that these labor market effects can be explained in a job search and bargaining model whereby overtime eligibility imposes a fixed cost per worker covered.

JEL codes: J23, J31, J33, J38

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1 Introduction

A key factor in the determination of employment, wages, and hours in the economy is the extent to which labor market regulations affect labor demand. Interest in this topic has sparked a large literature on the labor market impacts of the minimum wage, its implications for labor market efficiency, and its role in the rise of income inequality (e.g. see Brown, 1999). In contrast, although overtime regulations could potentially impose similarly large distortions on firms’ employment decisions, far less is known about its effects on the labor market, especially in the United States. The original motivation for imposing an overtime premium was the theory that by raising the cost of weekly hours above forty, it would encourage firms to reduce the prevalence of long workweeks and increase employment (Ehrenberg, 1971). However, a competing economic theory argues that in equilibrium, overtime coverage would have no real labor market effects if straight-time wages can lower such that workers earn the same weekly income with or without coverage (Trejo, 1991). Underlying these models are core assumptions about the flexibility of wages and hours. Despite the long history of overtime regulation in the U.S., there is still no consensus on the effects of overtime eligibility on the labor market.

Empirical studies of overtime coverage in the United States have been limited by a lack of clean policy variation and inadequate data.¹ Previous studies that exploit variation from the expansion of overtime coverage to additional industries and demographic groups have found a mix of negative (Costa, 2000; Hamermesh and Trejo, 2000) and zero significant effects (Johnson, 2003; Trejo, 2003) of overtime eligibility on the number of hours worked per week. However, since these expansions in coverage coincided with changes in the minimum wage, they were not suitable for isolating the income and employment effects of overtime coverage. Tests of the income effect have instead relied on cross-sectional variation in overtime coverage to estimate the correlation between log-wages and overtime hours, by eligibility status (Trejo, 1991; Barkume, 2010).² While the negative relationship identified in these studies is consistent with firms lowering wages to partially negate the costs of overtime requirements, it can also be driven by the selection of low skilled workers into jobs that demand long hours. In addition to the shortage of clean policy variation, existing studies have also been limited by insufficient data. Few datasets in the U.S. distinguish between workers’ base pay and overtime pay, and those that do often lack the sample size to precisely

¹See Hart (2004) and Brown and Hamermesh (2019) for an overview of the literature on overtime.
²An exception is Johnson (2003), which studies both the hours and wage effects of a Supreme Court ruling that extended coverage to public state and local employees. However, the direction of the estimated wage effects vary depending on the control group.
estimate changes in aggregate employment. Given these empirical challenges, “no study presents estimates of effects [of overtime coverage] on employment, and none offers evidence on all outcomes: [wages, earnings, and hours]” (Brown and Hamermesh, 2019).

I fill this gap in the literature by using detailed anonymous monthly administrative payroll data from the largest payroll processing company in the U.S. to evaluate the effect of recent federal and state expansions in overtime coverage for low-income salaried workers between 2014 and 2020. Labor regulations in the U.S. stipulate that salaried workers are legally covered for overtime if their weekly base pay is less than an “overtime exemption threshold” set by the Department of Labor (DOL). Since 2014, there have been two federal rule changes and sixteen state-level policies that expanded overtime eligibility for salaried workers by raising this threshold. Limited by the dearth in research on this topic, the federal Department of Labor’s cost-benefit analyses of its 2016 and 2020 rule changes relied heavily on strong assumptions that incorporated elasticities from the literature and a combination of predictions from the two theories of overtime to forecast the labor market effects of their proposed reforms. In contrast, I directly estimate the impact of raising the overtime exemption threshold on employment, income, and worker’s salaried/hourly classification without relying on these assumptions. The results of my study inform not only the debates surrounding upcoming increases to state thresholds, but also discussions about expanding overtime coverage to other traditionally exempt personnel such as gig workers.

To identify the employment effect of increasing the overtime exemption threshold, I compare the change in the observed distribution of workers across weekly base pay to the change in a counterfactual distribution that assumes the threshold stayed constant. I model the counterfactual distribution of the state and federal policies using two different approaches. For the state policies, I simply use the distribution in states that did not raise their overtime exemption thresholds as the counterfactual. However, since the federal policies affected all

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3See Goff (2020) for another recent study that uses administrative payroll data to study the effects of overtime. He finds that among hourly workers, the overtime premium leads to significant bunching at the 40 hours per week kink and reduced hours worked by at least 40 minutes per week.

4Although the 2016 federal rule change was overturned a week before it went into effect, I find that firms nevertheless responded to it as if it was binding.

5For instance, the DOL assumes that of the workers who occasionally work overtime, half would have both their base salary and hours decreased, and half would receive overtime compensation with no change in base pay or hours. 84 FR 10900

6Effective January 1, 2021, Washington and Colorado will raise their thresholds to $965 and $778.85 per week, respectively. Meanwhile, Michigan and Pennsylvania are evaluating proposals to raise their own exemption thresholds.

7See Cengiz et al. (2019) for a similar analysis that studies how the minimum wage impacts employment at different parts of the wage distribution.
states simultaneously, I instead apply a linear transformation to the distribution in the year prior to each rule change to construct the counterfactual distributions. This linear transformation is designed to match the employment growth at the right tail of the true and counterfactual base pay distributions, where the overtime exemption threshold is inconsequential.

Evaluating the effect of increasing the threshold on the distribution of salaried and hourly workers separately, I estimate that for every one hundred salaried workers initially earning between the old and new thresholds, 7.9 (s.e. 0.6) salaried jobs are lost and 3.6 (s.e. 2.2) hourly jobs are created, resulting in a net employment loss of 4.3 (s.e. 2.3) jobs. The 95% confidence interval rules out employment gains greater than 0.3% and employment losses greater than 8.9%. Focusing specifically on the distribution of salaried workers, I also find clear evidence of firms bunching employees base pay at the new exemption threshold to avoid paying them overtime. These results and their magnitudes are similar for both the federal and state policies, and are robust to restricting the sample to firms that operate in both the treatment and control states.

The negative employment response to expansions in overtime coverage is inconsistent with the prediction of the “labor demand model” that overtime stimulates job creation by encouraging firms to substitute additional workers for a reduction in long hours (Ehrenberg, 1971). Previous tests of this hypothesis have generally found imprecise negative and zero employment effects of policies outside the U.S. that reduced the length of the standard workweek above which workers are entitled to overtime compensation (Hunt, 1999; Crépon and Kramarz, 2002; Skuterud, 2007; Chemin and Wasmer, 2009). However, unlike an expansion in overtime coverage, shortening the standard workweek does not raise firms’ marginal cost per hour of employing workers who are already working overtime. For these workers, a shorter legislated workweek only increases their average cost and would therefore induce firms to reduce employment. Thus, an expansion in overtime coverage to a new group could potentially have a larger positive effect on employment than simply shortening the standard workweek. Nevertheless, I find that the overtime expansions for salaried workers in the U.S. yielded negative employment effects, reinforcing existing evidence that work-sharing policies implemented through the regulation of overtime are ineffective tools for creating jobs.

While my results do not support the labor demand theory of overtime, they are also inconsistent with the prediction of the compensating differentials model of overtime that

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For example, suppose a firm employs two workers: one works 38 hours per week, and the other works 42 hours. If the standard workweek decreases from 40 hours to 35, then the firm’s marginal hourly cost of employing the first worker would increase by 50%, whereas the marginal cost of the second worker remains constant but her average cost increases.
firms would reduce employees’ base salaries to negate the costs of overtime pay (Trejo 1991). Reviewing the effect of raising the overtime exemption threshold on the distribution of base pay, I do not observe any change in the number of workers earning below the initial threshold, which I would expect to see if firms were cutting workers’ base salaries. To directly measure the income effect, I use a difference-in-difference design that compares salaried employees affected by a threshold increase to similar workers who were unaffected. For the state policies, I define the control group as individuals earning similar base pays as the treatment group, but worked in a state that did not change its threshold. For the federal policies, I use workers with similar base pays in the year before the rule change. In both cases, I show that the parallel trends assumption holds.

I estimate that raising the overtime exemption threshold increased the income of directly affected workers by 1.2% (s.e. 0.1%), where I define a directly affected worker as a salaried employee earning between the old and new thresholds prior to a rule change. Of this positive income effect, 83% is due to an increase in base pay, and only 17% is due to an increase in overtime pay. The positive effect on base pay is direct evidence against the compensating differentials model of overtime. Taken together with the employment effect, these results imply a large, but imprecise, elasticity of employment with respect to own wage of -3.69 (s.e. 1.92).

Given that neither the labor demand model nor the compensating differentials model of overtime can explain the employment and income effects of raising the overtime exemption threshold, I develop a job-search and matching model to rationalize my results. In this model, firms and workers bargain over jobs’ weekly base income, weekly hours and salaried/hourly status. I show within this framework that my results are consistent with a scenario where overtime coverage imposing a fixed cost per worker that is independent of the number of hours worked, such as a cost to monitor workers’ hours. Alternatively, I also show that a model with wage rigidity would fit the results. Without the existence of either of these frictions though, my model predicts that overtime coverage would simply cause workers and firms to agree on a lower base pay like in the compensating differentials model.

To understand which workers benefit and lose from an increase in the overtime exemption threshold, I use the matched employer-employee panel structure of the data to decompose the effect on the number of salaried workers along the base pay distribution into an employment effect and a reclassification effect. Since the state rule changes were too small for these outcomes to vary significantly by base pay, I focus specifically on the large 2016 federal policy that attempted to raise the overtime exemption threshold from $455 to $913 per week. For each margin of adjustment, I use a linear transformation of its distribution in
2015 as a counterfactual for its distribution in 2016. I estimate that for every one hundred workers directly affected by the change in the overtime exemption threshold, 3.8 (s.e. 0.7) salaried jobs were lost due to a reduction in employment, and 10 (s.e. 0.7) salaried jobs were reclassified as hourly. These effects explain nearly the entire fall in aggregate employment and the increase in hourly employment, respectively. Further decomposing these effects, I find that the employment loss is driven primarily by a decrease in new hires rather than an increase in separations, whereas the reclassification effect is due to both an increase in salaried-to-hourly conversions and a decrease in hourly-to-salaried conversions.

The magnitudes of these margins of adjustment and the income effect vary along the distribution of base pay. First, while the share of employment lost is uniformly distributed between the old and new thresholds, the decline in employment between $817 and $913 is entirely offset by the increase in hires bunched at the new threshold. This suggests that the employment loss disproportionately fell on jobs earning less than $817 per week. Second, I find that 4.9% of directly affected workers were bunched at the new threshold but would otherwise have earned between $721 and $913 per week. The median bunched worker experienced a 5.8% increase in their income as a result of the policy. In other words, the policy largely benefited workers earning close to the new threshold, and cost jobs paying below it. Third, there are spillover effects onto jobs paying up to $1200 per week. A small number of jobs above the $913 threshold were reclassified from salaried to hourly and experienced an increase in income from overtime compensation.

To my knowledge, this is the first paper to show the importance of labor market regulations in determining whether a job is paid by salary or by hour, the two most common pay structures in labor contracts. The growing literature on alternative work arrangements has found that working conditions are strongly correlated with workers’ salaried/hourly status. Relative to hourly workers, salaried employees tend to be more educated, work longer hours, and are more likely to have flexible work schedules and locations (Mas and Pallais 2020). To explain the existence of these two pay structures, the personnel economics literature argues that firms choose salaried contracts for jobs where workers’ hours are uninformative about their effort or output, and the cost of monitoring hours is high (Fama 1991, Garen 1998). The reclassification effect that I estimate in this paper suggests that at least two of the properties often associated with salaried jobs - higher pay and longer hours - may not simply be an intrinsic feature of these jobs’ duties, but also a result of firms’ response to the overtime exemption threshold by classifying low-income covered employees as hourly.

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

2 Federal and State Overtime Regulation

The Fair Labor Standards Act (FLSA) requires employers to record workers’ hours, and pay them one and a half times their regular rate of pay for each hour worked above 40 in a week. While this rule applies to nearly all hourly workers in the U.S., the FLSA exempts a large group of salaried workers from overtime coverage who are considered executive, administrative, or professional employees. To exempt a salaried employee from overtime, an employer must show that the worker performs primarily white-collared duties, and earns a salary equal to or greater than the “exemption threshold” set by the DOL. 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 Figure A.1). In an effort to restore overtime protection for low-income salaried workers such as managers at fast food restaurants and retail stores, multiple Departments of Labor at both the federal and state level have recently increased their overtime exemption thresholds. My paper uses these policy changes in the exemption threshold as natural experiments to study the effects of expanding overtime coverage.

At the federal level, I examine two major policies to revise the FLSA’s overtime exemption threshold. First, the Department of Labor announced in May 2016 that it would increase the federal exemption threshold from $455 per week ($23,660 per year) to $913 per week. 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. 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 = $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.

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.

In 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%.
($47,476 per year) starting December 1, 2016. According to the Current Population Survey, the new rule would effectively raise the threshold from the 10th percentile of the salaried income distribution to the 35th percentile. However, to employers’ surprise, a federal judge imposed an injunction on the policy on November 22, 2016, stating that such a large increase in the threshold oversteps the power of the DOL and requires Congress’s approval. Given that this unexpected injunction occurred only one week before the policy was to go into effect, many companies at the time reported that they had either already responded to the policy, or made promises to their employees that they intended to keep. Following the injunction of the 2016 rule change, the federal Department of Labor debated a smaller increase to the FLSA overtime exemption threshold and announced in September 2019 that it would raise the threshold to $684 per week effective January 1, 2020. I examine both the nullified 2016 proposal and the binding 2020 rule change to estimate the short-run effects of a federal expansion in overtime coverage for salaried workers.

To complement my evaluation of the federal rule changes, I also implement an event study analysis using 16 prominent state-level increases in the overtime exemption threshold between 2014 and 2020. Similar to the minimum wage, multiple states impose their own overtime exemption thresholds that exceed the one set by the FLSA. I present in figure 1 all state and federal overtime exemption thresholds since 2005, along with the invalidated proposal in 2016. My state-level analysis uses variation from four states: California, New York, Maine, and Alaska, 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.

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 overlap with that of

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

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

\[ \text{Starting in 2017, California and New York also passed legislation that generated variation within-state. California sets a lower threshold for employers with fewer than 26 employees, whereas New York varies its threshold by both employer size and location (i.e. in/near/away from NYC). Since the data I analyze only records geography at the state-level and consists of firms with at least 50 workers, I do not exploit the within-state variation. In my main analysis, I assume that the highest threshold within each state is binding for all employers, and show that my results are robust to restricting the sample to only events without within-state variation.)} \]
the minimum wage, even after accounting for potential spillovers.\textsuperscript{15}

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

### 3 Theoretical Predictions

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

\[
\begin{align*}
\max_{(n,h)} & \quad f(n,h) - \left( wh + p \cdot w(h - 40) \cdot 1[h > 40] + F \right) n
\end{align*}
\]

where \(w\) is the market wage, \(p\) is the overtime premium, and \(F\) is a fixed cost per worker. In this framework, overtime coverage has no effect on the wages and simply increases the overtime premium \(p\).\textsuperscript{16} By raising the cost per hour of labor above 40, overtime coverage induces two responses: a substitution effect away from long hours for more employment, and a scale effect to reduce both factors of production. In theory, the direction of the employment effect can be negative if the scale effect exceeds the substitution effect. However, in practice, I

\textsuperscript{15}Of the four states, Maine has the smallest threshold-to-minimum-wage ratio of 58. This implies that salaried workers paid at the threshold earn 44\% more a minimum wage employee who works 40 hours per week, well above the range that the minimum wage literature has found spillover effects (Brochu et al., 2015; Cengiz et al., 2019).

\textsuperscript{16}While the assumption that wages are fixed is highly restrictive, previous attempts to endogenize wages by integrating labor supply responses have generated intractable predictions (Hart, 2004).
do not find any reduction in employment among workers not directly affected by a change in
the overtime exemption threshold that would indicate a large scale effect. In light of this, my
primary test of the labor demand model is the prediction that employment should increase
when the scale effects are small.\footnote{This prediction also consistent with the original motivation for introducing overtime regulation
during the Great Depression to reduce unemployment.}

To model overtime in a market equilibrium, a competing theory argues that within a
compensating differentials framework, base wages would decrease in response to overtime
coverage such that total income remains unchanged (Trejo 1991).\footnote{For instance, suppose an employee initially works 50 hours for a salary of $825 each week and
receives no overtime. If this worker becomes covered for overtime, the firm can reduce the worker’s
base salary to $600, so that with the 10 hours of overtime, the worker would continue to receive
$600 \cdot (1 + 1.5 \frac{50 - 40}{40}) = $825 per week.} Under this framework, overtime coverage would have no effect on real income, hours, or employment. Empirically
though, previous estimates of the correlation between overtime coverage and wages suggest
that base wages do not adjust fully to offset the costs of overtime (Trejo 1991; Barkume
2010). To allow for the possibility that there may be real labor market effects if wages only
adjust partially, my primary test of the compensating differentials model is its core prediction
that workers’ base pay should decrease after gaining overtime coverage.

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

\[ \text{table 1} \]

\section*{3.1 Search and Bargaining with Exogenous Contract Rates}

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

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

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

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

where $\epsilon$ is the worker’s constant labor supply elasticity. Unlike common search and matching models, I assume that workers receive a disutility from working longer hours that is additively separable from their income.\textsuperscript{20}

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

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

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

\textsuperscript{19}One can think of $F$ as the difference between the benefits (e.g. more flexibility, no need to monitor hours, etc.) of paying a worker by salary and the costs (e.g. less incentive to work long hours, etc.). A distribution of salary-fit can be motivated by an agency problem where a firm chooses an occupation’s pay classification depending on how informative the number hours worked predicts workers’ effort and output (Fama, 1991).

\textsuperscript{20}The predictions of the model are invariant to including a additive preference for pay classification.
Given \((\theta, F, a)\), the job characteristics are determined by Nash bargaining:

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

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

\[
w_0 = \alpha \left( \theta h_0^\beta + F \cdot \text{sgn}(S_0) \right) + (1 - \alpha) \left( a^{-\frac{1}{\beta}} h_0^{\frac{1}{1+\frac{1}{\beta}}} + rV_n \right)
\]

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

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

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

### 3.2 Comparative Statics in Response to Overtime Policy

Equipped with the benchmark model, I explore how the job characteristics \((w, h, S)\) and the share of matches that become employment contracts change once I introduce an overtime premium. For reference, I continue to denote the solution to the model without overtime by \((w_0, h_0, S_0)\). Henceforth, I make a distinction between base pay and gross pay. Let \(w\) present workers’ weekly base pay, prior to receiving overtime compensation. The worker and firm bargain over the weekly base pay, weekly hours, and pay classification. However, the worker’s
value of employment and the firm’s profit depend on gross pay \( g = (1 + \eta(w,h,S))w \), where
\[
\eta(w,h,S) = \begin{cases} 
0.5(h-40) \\ 1.5(h-40) \\ 0 
\end{cases} \quad \text{if } h > 40 \text{ and } S = -1 \\
\frac{h}{40} \quad \text{if } h > 40, \ S = 1, \text{ and } w < \bar{w} \\
\text{otherwise}
\] (1)
and \( \bar{w} \) represents the overtime exemption threshold.\(^{21}\)

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

Second, I examine the case where overtime coverage imposes a fixed cost on salaried workers. For instance, it could be costly for the firm to monitor workers’ hours or hours become less flexible. Since firms already monitor the hours of hourly workers, this friction only raises the costs of salaried workers earning less than the overtime exemption threshold. The firm’s discounted stream of profits is given by
\[
J = \frac{\theta h^\beta - (1 + \eta(w,h,S))w + F \cdot \text{sgn}(S) - C \cdot 1[S = 1, w < \bar{w}]}{r + \delta}
\]
where \( C \) is a constant, and \( 1[S = 1, w < \bar{w}] \) is an indicator that equals 1 if the worker is salaried and earns less than \( \bar{w} \).\(^{22}\) The fixed cost does not affect the bargaining outcome of hourly jobs or salaried jobs that pay above the threshold in the baseline scenario. For newly covered jobs that would otherwise be salaried \( S_0 = 1 \) and pay \( w_0 < \bar{w} \), the fixed cost has one of three possible effects on the Nash bargaining solution, depending on the match quality and salary-fit \( (\theta, F) \):

**Prediction 1 (Bunching):** If the job’s income in the benchmark scenario is sufficiently...

---

\(^{21}\)While hourly workers’ overtime premium is calculated as 0.5 times their wage (i.e. 0.5 \( \frac{w}{h} \) using my notation), under the DOL’s rules, the overtime premium for salaried workers is calculated as 1.5 times their salary divided by 40 (i.e. 1.5 \( \frac{w}{40} \)).

\(^{22}\)Instead of all matches experiencing the same fixed cost, one can also allow it to vary by job without affecting the predictions. For example, I can model the relative benefit of being salaried as \( F = B - C \) and the fixed costs as \( \rho C \) where \( 0 < \rho < 1 \).
close to the overtime exemption threshold (i.e. \( \bar{w} - w_0 \) is small), then the Nash bargaining solution is to raise the job’s base income to the threshold and increase weekly hours.

**Prediction 2 (Reclassification):** If the job is not bunched and the cost of reclassifying is smaller than the monitoring cost (i.e. \( 0 < 2F \leq C \)), then the firm would reclassify the job as hourly. Its base income becomes

\[
w_2 = \frac{w_0 - 2\alpha F + (1-\alpha)(V_{n}^{OT} - V_n)}{1+\eta(w,h,S)}
\]

**Prediction 3 (Remain salaried):** If the job is not bunched and it is very costly to make the job hourly (i.e. \( 2F > C \)), then the firm would keep the job as salaried, and change its base income to

\[
w_2 = \frac{w_0 - \alpha C + (1-\alpha)(V_{n}^{OT} - V_n)}{1+\eta(w,h,S)}
\]

to adjust for the overtime premium, the loss in surplus from the monitoring costs, and the change in the worker’s outside option.

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

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

**Prediction 4 (Employment Loss):** Firms and workers no longer accept some jobs with poor match quality (i.e. \( \theta \) is small) that would have been accepted if there was no overtime coverage.
3.3 Labor Market Dynamics with Endogenous Contract Rates

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

\[
m(u, v) = vq(k)
\]

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

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

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

where \( \sigma(\Phi) \) is the probability that a match is accepted by both parties,\(^{23}\) and \( J_F \) is the expected value of a filled vacancy. Suppose that prior to the announcement of the overtime policy, the labor market is in steady state where employers created vacancies until \( J_v = 0 \). After the announcement of the policy, the expected value of a match \( \sigma(\Phi)J_F \) decreases, so that \( rJ_v = -\psi + q(k)\sigma(\Phi)(J_F - J_v) \) at the current level of \( v \). In response, firms reduce the number of vacancies, \( v \), until \( J_v = 0 \).

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

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

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

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

\(^{23}\)In other words, it is the measure of the set \( \Phi = \{ (\theta, F, a) | \theta \geq \theta^*(F, a) \} \)
Intuitively, this prediction holds even if the job destruction rate (i.e. $\delta$) is an endogenous decision of the firm and incumbent workers have firm-specific human capital. Since layoffs are instantaneous, the firm would not lay off any workers until the policy goes into effect. Between the announcement of the policy and the date that it goes into effect, the firm can either continue hiring workers at the same rate as before, then fire them when the policy becomes binding, or reduce its hires to avoid the vacancy cost. Given large enough vacancy costs, firms would choose the latter and I would expect to observe a reduction in hires immediately following the announcement of the policy.

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

4 ADP Data

I use anonymized monthly administrative payroll data provided by ADP LLC, a global provider of human resource services that helps employers manage their payroll, taxes, and benefits. As part of their business operations, ADP processes paychecks for 1 in 6 workers in the United States. Their matched employer-employee panel allows me to observe monthly aggregates of anonymous individual paycheck information between May 2008 and January 2020. The data contains detailed information on each employee’s salaried/hourly status, income, hours, pay frequency (i.e. weekly, bi-weekly, or monthly), sex, industry, and state of employment.\footnote{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.} Given that the overtime exemption threshold varies across states, I partition
the sample by workers in California, New York, Maine, Alaska, and the rest of the United States (henceforth called FLSA states).

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

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

I make two restrictions to the sample of firms in my analysis. First, the entry and exit of firms in the data reflect both real business formations and the decision of existing firms to partner with ADP. I find that the flow of firms into the ADP sample deviates from Survey, which only records respondents’ state of residence. 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.

For example, a salaried worker with a statutory pay of $3000 per month would have a weekly base pay of $3000 * $12 = $692.31.

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 \( \frac{OT Pay}{OT Hours} \) is no greater than 2 times the regular pay rate \( \frac{Base Pay}{40} \). See Appendix E for more details.

I only observe workers’ number of paychecks per month starting in 2016. Prior to 2016, I impute the number of paychecks at the firm-level by comparing each firm’s average gross pay per month to the median of their average monthly pay over a year, as described in detail in Appendix E.
the Business Formation Statistics published by the U.S. Census. To prevent the selection of firms from biasing my estimates, I restrict each of my analyses to a balanced panel of employers. Second, ADP offers two payroll products, one designed for firms with at least 50 employees and one for smaller firms. The monthly payroll data that I use in my main analysis is derived from the former and is therefore by construction, restricted to businesses with 50 or more employees.

5 Employment Effects

In this section, I estimate the aggregate employment effect of raising the overtime exemption threshold. I begin with a case study of the federal rule changes, followed by an event-study using the state-level variation. Since the 2016 FLSA proposal affected all states simultaneously and the 2020 policy coincided with minimum wage changes in states unaffected by the federal overtime policy, I do not include them in the event-study. Instead, I evaluate the effects of each of the federal policies by comparing the evolution of the base pay distribution in the year of the policy to its evolution in the same months in previous years.

5.1 Federal Policies

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

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28 For a detailed analysis of the representativeness of the ADP data, refer to (Grigsby et al., 2019).
29 These states are Alabama, Georgia, Idaho, Indiana, Iowa, Kansas, Kentucky, Louisiana, Mississippi, New Hampshire, New Mexico, North Carolina, North Dakota, Oklahoma, Pennsylvania, South Carolina, Tennessee, Texas, Utah, Virginia, Wisconsin, and Wyoming. They account for 35% of all workers in the data.
30 This restriction drops 41 firms, accounting for 11% of all workers in the sample in 2016. I do not make this restriction in my analysis of the state policies as large firms in control states adequately control for the year-specific fluctuations of the same firms in the treatment state.
First, there are very few workers below the old threshold of $455 per week and a noticeable increase in the distribution at exactly the old threshold. Second, there was a decrease in the number of workers with base pays between the old and new thresholds between April and December. The average firm employed 8.4 salaried workers with base pays between $455 and $913 in April 2016, and only 6.8 such workers in December 2016 - a decrease of 19%. Third, there is a large spike in the distribution at $913 that appears in December but not April. These features are even more evident in figure 2b where I plot the difference between the two distributions in figure 2a. As a placebo check, I also overlay the difference-in-distributions between April and December of each year from 2012 to 2015. Consistent with prediction 1 of the job-search model, firms bunched workers’ base salaries at the new $913 overtime exemption threshold in 2016, but not in any of the four preceding years. Furthermore, the lack of any spikes in the left tail of the distribution suggests that firms did not reduce workers’ base pay to offset the cost of overtime as predicted by the compensating differentials model.

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

As further evidence that the change in the frequency distributions of base pay reflect a behavioral response to the nullified policy, I examine their evolution over time. Figure 3 plots the average salaried distribution of each month in 2016 and 2017, subtracted by the distribution in April 2016, for a balanced panel of firms. For example, the December 2016 graph in Figure 3 is similar to the blue line in Figure 2b, but for employers that remain in the sample until December 2017. I find that the timing of the growth and decay of the spike at $913 corresponds precisely with the history of the FLSA policy. After the announcement of the policy in May 2016, firms start reducing the number of salaried employees between the old and new thresholds, and bunching workers at the new threshold. This bunching experiences a large increase in December 2016, which is when the new threshold was supposed to go into effect. Since the new threshold was not binding, firms slowly stopped bunching workers at the threshold after January 2017.31 Unlike the effect of the policy on the salaried distribution, its

31Refer to Quach (2020) for an analysis of the persistence of the 2016 FLSA policy and its implications for wage rigidity.
harder to distinguish between its effect on the hourly distribution and natural wage growth
from simply plotting the evolution of the frequency distribution over time (see Appendix
figure A.3).

**Constructing the Counterfactual Distribution.** Following recent advancements in the
minimum wage literature, I estimate the aggregate employment effect of raising the OT
exemption threshold by first estimating its effect on the number of workers within each bin
along the distribution of weekly base pay, and then integrating these effects across all bins
(Cengiz et al. 2019; Derenoncourt and Montialoux 2019; Harasztosi and Lindner 2019;
Gopalan et al. 2020). To identify the employment effects of the 2016 FLSA policy, I use the
change in the frequency distribution of base pay in 2015 as a counterfactual. My empirical
strategy stems from the observation that the shape of the difference-distributions in figures
2b and 2d are remarkably similar in each year prior to 2016. This suggests that the difference-
distribution in 2015 is a reasonable approximation for how the distribution in 2016 would
have evolved if not for the new overtime exemption threshold. To account for year-specific
aggregate employment growth, I apply a linear transformation to the difference-distribution
in 2015 such that the counterfactual employment growth for jobs paying well above the new
threshold closely matches the observed change in employment in 2016.32 In my analysis, I
treat the frequency distributions (i.e. salaried and hourly) within each firm as independent
observations and cluster estimates at the firm-level.

Formally, let $n_{ijkt}$ 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:

$$n_{ijk,\text{Dec.},t} = n_{ijk,\text{Apr.},t} + \alpha_{jkt} + \beta_{jk} \cdot D_{t=16} + \epsilon_{ijkt}$$

(2)

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

To separately identify the $\beta_{jk}$’s from the $\alpha_{jkt}$’s, I make two modeling assumptions:

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

32Visually, this is equivalent to vertically stretching/compressing and shifting the 2015 difference-
distribution to fit the right tail of the 2016 distribution.
\[
\alpha_{jkt} = \gamma_1 \alpha_{jk,t-1} + \gamma_0
\]

The first assumption states that the policy has no effect on the number of workers earning above a cutoff bin \( k^* \). This claim is supported empirically by the lack of movement in the upper tail of the difference-distribution between November and December 2016 in the time series profile depicted in figure 3. The second condition states that the distribution of changes in employment between April and December is similar across years, up to a linear transformation. This assumption is supported by the observation in figures 2b and 2d that the difference-distributions have similar shapes each year aside from 2016.33

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

\[
\hat{\beta}_{jk} = \left(\bar{n}_{jk,Dec,t} - \bar{n}_{jk,Apr,t}\right) - \hat{\gamma}_1 \left(\bar{n}_{jk,Dec,t-1} - \bar{n}_{jk,Apr,t-1}\right) - \hat{\gamma}_0
\]

\[
= \Delta\bar{n}_{jkt} - \hat{\gamma}_1 \Delta\bar{n}_{jk,t-1} - \hat{\gamma}_0
\]

(3)

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

\[
\Delta\bar{n}_{sal,kt} = \gamma_1 \Delta\bar{n}_{sal,k,t-1} + \gamma_0 + \epsilon_{sal,kt}
\]

(4)

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

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

33These assumptions are similar to the ones made by Defusco et al. (2019) to generate the counterfactual number of loans along the distribution of debt-to-income (DTI) ratios, absent a regulatory rule that made it more difficult to give mortgages to individuals with a DTI above 43%.
In practice, I choose bins of width $96.15 \approx \frac{5000}{52}$ because the base pay of salaried workers exhibit bunching at values corresponding to annual salaries of multiples of $5000. I use the 9 bins greater than or equal to $k^* = $1778 to estimate equation 4. A benefit of selecting a large $k^*$ is that it allows me to test the accuracy of the model by seeing whether it eliminates the spikes between the new threshold of $913 and $k^*$.

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

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

In table 2 I present estimates of the net employment effect of the 2016 FLSA rule change, calculated as the sum over all the bin-specific treatment effects below the cutoff $k^*$. The first two rows of column (1) report the estimates of the policy’s effect on the number of salaried and hourly workers, respectively, corresponding to the graphs in figure 4. Consistent with the figures, I find that firms on average lose 1.299 (s.e. 0.111) salaried employees and gain 0.943 (s.e. 0.310) hourly employees. The sum of these two estimates, which I report in the third row, implies a net employment loss of 0.356 (s.e. 0.352) workers. To compare this number...
across specifications and with different policy evaluations, I normalize it by the number of salaried workers between the old and new threshold in April 2016. I find that for each worker directly affected by the increase in the overtime exemption threshold, 0.042 (s.e. 0.042) jobs are lost.

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

As another robustness check, I also estimate equation 3 using only firms in California and New York. I present these estimates graphically in appendix figure A.6. Unlike the FLSA states, California and New York already had overtime exemption thresholds of $800 and $675 per week, respectively, so I would expect to see smaller employment effects. Consistent with this prediction, I find that the decline in salaried employment in these two states is concentrated above their initial thresholds. However, since these states also increased their minimum wages in 2016, I am unable to disentangle the employment effect of the proposed change to the FLSA overtime exemption threshold from the effect of the minimum wage, as evident from the estimated effects on the hourly distribution.

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

\[^{34}\]Unlike the estimates for the 2016 FLSA policy, I only sum over the estimates up to $876 to avoid
similar to those of the 2016 policy, the magnitude of these effects are much smaller since the 2020 rule change affected fewer people. Although the standard errors of the aggregate effects are large relative to their estimates, the bin-by-bin treatment effects in appendix figure A.7 precisely show that firms bunched salaried workers’ base pays to right above the new threshold. It is less clear how the 2020 FLSA policy affected the hourly distribution as even the bin-specific effects are imprecisely estimated.

Using the same empirical strategy, I also estimate the effect of the increases in the federal overtime exemption threshold on employers’ wage bill. In this case, I replace the outcome variable in equations 3 and 4 with the sum of workers’ base pay within classification \( j \) and bin \( k \).\(^{35} \) I report these estimates in rows (4) to (8) of table 2. Mirroring the employment effects, there is a decrease in the wage bill paid to salaried workers and an increase in the wage bill paid to hourly workers across all specifications. Unlike the employment effects though, the net change in the wage bill is positive in my preferred specification, although estimated imprecisely. Scaling the net wage bill effect by the initial wage bill of directly affected workers, I find that the wage bill increased by 1.7% in my preferred specification. However, I cannot rule out changes between -6.9% and 10.3%.

5.2 State Policies

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

Formally, for each event \( v \), let \( n_{ikstv} \) be the number of workers at firm \( i \), with base pay between \( 40k \) and \( 40(k + 1) \) of the new threshold, in treatment-control state \( s \), at \( t \) months from the date of the rule change.\(^{36} \) Since firms in the control group have, on average, larger capturing any “unbunching” residual effect of the nullified 2016 policy
\(^{35} \)Alternatively, I can sum workers’ gross pay but this causes the placebo estimates to deviate from zero. This is to be expected since my empirical strategy controls for differences in employment growth across years, but does not account for differences in hours.
\(^{36} \)To simplify notation, I drop the index for pay classification. In my analysis, I estimate each regression separately for salaried and hourly employees.
initial employment levels compared to firms in the treatment group, even if employment in both groups grow at the same rate, the control group will experience larger changes in the number of workers. To account for this, I scale the employment of firms in the control state by the ratio of the average firm size between the treatment and control states two months prior to the event:

\[
\tilde{n}_{ikstv} = \begin{cases} 
  n_{ikstv} & \text{if } s = \text{treatment} \\
  n_{ikstv} \cdot \frac{n_{v,\text{treat},t=-2}}{n_{v,\text{control},t=-2}} & \text{if } s = \text{control}
\end{cases}
\]

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

\[
\tilde{n}_{ikstv} = \sum_{t=-6}^{5} \sum_{k=-6}^{15} \beta_{kt} \cdot I_{kst} + \alpha_{ks} + \delta_{kt} + \varepsilon_{ikstv}
\]

where the treatment dummy \( I_{kst} \) equals 1 for the treatment state at normalized bin \( k \), and event time \( t \). I set the reference date as two months prior to the rule change to capture any anticipatory responses, which may be important given the early responses of firms to the 2016 FLSA policy. Since all states increase their thresholds with at least one year between each event, I define the event window to only include 6 months before the rule change and 5 months after so that none of the events overlap each other within the same state. My benchmark specification includes bin-state \( (\alpha_{ks}) \) and bin-month \( (\delta_{kt}) \) fixed effects to control for state-specific differences in the base pay distribution and nationwide changes in inequality, respectively. I cluster standard errors at the firm-level to account for the correlation between the changes in employment across bins within firm.

**Estimates of Employment Effect.** Figure 5 shows the estimates of the treatment effect from equation 5, separately for the distribution of salaried and hourly workers. In figure 5a, I plot the effect of raising the overtime exemption threshold on the number of salaried workers at event time 0 when the new threshold first becomes binding. Similar to the effect of the federal policies, there is a net decrease in the number of salaried employees below the new threshold and a spike in workers right above it. Aside from two events in New York that raised the overtime exemption threshold by about $150, all other rule changes were no more than $80. Consequently, most of the decrease in salaried employment is concentrated within $80 below the new threshold.\(^{37}\) As a placebo check, I find little effect on any individual bin.

\(^{37}\)I drop observations with normalized base pays equal to or less than -160 for the events in Maine.
of base pay above the new threshold. However, the cumulative treatment effect does exhibit a minor downward trend as a function of base pay, suggesting that my estimates could potentially be downward biased. To minimize the effect of this trend, my preferred measure of the employment effect will only aggregate the estimates across base pays between -160 and 120 dollars relative to the new threshold.

In figure 5b I plot the change in the number of salaried workers paid below and above the new threshold over event time, relative to the employment level two months before the rule change. Examining the figure from left to right, four features stand out. First, there is little evidence of a pre-trend for either graph prior to the month that the policy goes into effect, indicating that firms did not respond in anticipation of the new threshold. Second, there is a sharp drop in the number of jobs below the threshold and a sharp increase in the number of jobs above it at precisely the month of the rule change, consistent with the bunching from the cross-sectional estimates in figure 5a. Third, the magnitude of the decrease in employment below the threshold is visibly larger than the increase in employment above it. Fourth, the number of salaried workers above the new threshold remains relatively stable after it goes into effect, whereas the number of workers below it continues to decrease.

Plotting analogous figures of the cross-sectional estimates and the longitudinal estimates for hourly workers, I find that the base pay distribution for hourly employees responded in a qualitatively similar fashion to the distribution for salaried employees. I show in figure 5c that raising the overtime exemption threshold cut hourly jobs between the old and new thresholds, and increased the number of hourly jobs above it. Similar to the effect of the federal rule changes, there is a net increase in hourly employment. I confirm this bunching effect in figure 5d where I plot the evolution of the hourly employment estimates below and above the threshold over event time. Mirroring the estimates for the salaried distribution, I find no pre-trend prior to the rule change, and a sharp drop in the number of hourly employees below the new threshold along with a jump in the number of above it exactly on the month of the rule change. However, unlike the changes in salaried employment, the number of hourly jobs below the threshold increases over time following the event. Coupled with the earlier observation that there was continued loss of salaried employment within that range of base pay, this suggests that firms substituted away from salaried jobs for hourly jobs in the long run.

In table 3 I present the estimates of the aggregate employment effect due to the state increases in the overtime exemption threshold. I report the estimates of the baseline specifi-

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38 I drop the California 2020 event from the sample because the data ends on January 2020.
cation (see equation 5) for event time 0 in column (1). The employment effects equal the sum of the bin-specific estimates from -160 to 120 dollars relative to the new threshold. I find that there is a statistically significant loss in salaried employment of 0.068 jobs (s.e. 0.010), and a smaller positive effect on hourly employment of 0.021 (s.e. 0.028) jobs. The sum of these two effects imply that firms lost on average 0.047 (s.e. 0.029) jobs. These effects are an order of magnitude smaller than the effect of the 2016 FLSA policy because the state policies were designed to be gradual increases in the threshold over multiple years such that each policy only targets a small share of workers. Normalizing the aggregate employment loss by the average number of salaried workers between the old and new thresholds two months prior to the rule change, I estimate that 0.040 (s.e. 0.025) jobs are lost per affected worker. This effect is similar in magnitude to the employment loss of the 2016 FLSA policy, but more precisely estimated. The 95% confidence interval can reject any employment gain above ten jobs per one hundred workers affected. To examine the medium-run impact of raising the overtime exemption threshold, I estimate in column (2) the employment effect at five months after the threshold increase. Consistent with the graphical evidence from figure 5, I find that the substitution from salaried jobs to hourly jobs grows over time. While the estimate of the employment loss shrinks in the medium-run, I cannot rule out that it remains the same over time.

I assess the robustness of my results to additional controls and alternate samples in columns (3)-(6) of table 3. First, in column (3), I interact the bin-state and bin-month fixed effects with indicators for each events. This is equivalent to estimating a difference-in-difference regression within each event, and computing a weighted average of the treatment effects. Of all the specifications, this is the most precisely estimated and can reject employment losses of less than 1.6 jobs per one hundred affected worker at the 95% confidence level. Second, in column (4), I restrict the sample within each event to firms that employ workers in both the treatment and control states. This controls for any differences in employment driven by differences in the composition of firms between the treated and control states. In column (5), I drop the three state threshold increases that take place on January 1, 2017 to eliminate any confounding effects from differences in states’ response to the nullified 2016 FLSA rule change. Column (6) restricts the sample further to only the six threshold increases that occurred prior to 2016. This removes any biases from not accounting for the geographical variation in the threshold that was introduced in New York and California after 2016. In general, the number of jobs lost per affected worker is similar across all specifications.

To average the employment effect across the state and federal policies, I estimate the
following stacked difference-in-difference regression:

\[ \tilde{n}_{ikstv} = \alpha_k \cdot \text{After}_t + \alpha_{kv} \cdot \text{Treat}_s + \beta_k \cdot \text{After}_t \cdot \text{Treat}_s + \varepsilon_{ikstv} \]

where the outcome variable \( \tilde{n}_{ikstv} \) is the scaled number of workers in firm \( i \), base pay bin \( k \), treatment/control group \( s \), time \( t \), and event \( v \). I define the scaling factor and the indicator variables differently depending on whether event \( v \) is a federal or state policy. First, the scaling factor and treatment/control groups are defined as in the above analyses.\(^{39}\) The indicator \( \text{Treat}_s \) equals 1 if the observation is in the treatment group. Second, the time dummy \( \text{After}_t \) equals 0 on the month before the announcement of rule change for the federal policies, and two months before the new threshold actually goes into effect for the state policies. In both cases though, \( \text{After}_t \) equals 1 on the first month that the new threshold becomes binding. Third, the bin \( k \) takes on two values: one for base pays above the new threshold and one for base pays below. For the federal policies, the two bins include all base pays from $215 less than the old threshold to $192 above the new one, whereas the bins for the state policies span a shorter range between $160 less than the new threshold to $80 above it. The parameter of interest is the sum of \( \beta_k \) across the two bins.

I report the estimates of the pooled regression in column (7) of table 3. The fall in salaried employment and the rise in hourly employment are not exactly equal to a weighted average of the previous estimates since the pooled regression cuts the base pay distribution into fewer bins. Nevertheless, the net employment effect scaled by the average number of worker affected is of a similar magnitude as the other estimates. The point estimates imply that for every one hundred workers directly affected by an increase in the overtime exemption threshold, 7.9 (s.e. 0.6) salaried jobs are lost and 3.6 (s.e. 2.2) hourly jobs are created, leading to a net loss of 4.3 (s.e. 2.3) jobs. The standard errors rule out any employment gains greater than 0.3%, and employment losses greater than 8.9%. Thus, consistent with prediction 4 of the job-search model and contrary to the prediction of the standard labor demand model, the evidence suggests that raising the overtime exemption threshold likely decreased employment.\(^{40}\)

In panel B of table 3, I estimate equation 5 using the sum of workers’ gross pay as the outcome variable. I continue to scale the control group by the ratio of the treatment and control groups’ average firm size two months before the threshold increase. Similar to the

\(^{39}\)In this case, for the federal policies, I only scale the control group by \( \gamma_1 \) in equation 4.

\(^{40}\)Although employment loss is compatible with the labor demand model if the scale effect of the increased labor costs is sufficiently large, this condition would imply that there would be employment loss across the entire income distribution, which I do not observe.
effect of the federal policies on firms’ wage bill, I report a positive but noisy estimate of the state rule changes on firms’ wage bill across all my specifications. The most precise estimate of the wage bill effect, presented in column (7), is from the pooled regression that utilizes both the variation from the state and federal policies. Under that specification, I find that the wage bill increased by 2.1% relative to the initial wage bill paid to directly affected workers, with a 95% confidence interval ranging between -2.7% and 6.9%.

6 Income Effect on Affected Workers

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

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

(6)

where \( y_{ivt} \) is worker \( i \)'s income 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 (\( \alpha_{vs} \)) and event-month (\( \delta_{vt} \)) fixed effects. The identifying assumption is that absent the reforms, workers’ income in the treatment and control groups would have evolved similarly.

For my evaluation of the 2016 and 2020 FLSA policies, I consider a worker to be in the treatment group if they are paid a salary between the old and new thresholds on the month before the new rule is announced. I define the control group to be salaried workers who earn within the same interval on the same month in the preceding year. In my event-study analysis of the state policies, the treatment group consists of salaried workers living in states that raised their thresholds two months before a rule change, and earning between the old and new thresholds. The control group consists of workers within the same income bracket working in the 46 FLSA states. To separately identify the effect of the three state policies that went into effect on January 1, 2017 from the nullified 2016 FLSA policy, I define the treatment and control groups of those state policies using workers’ income on December 2016 rather than November 2016. In all cases, I restrict the sample to workers who are continuously employed at the same firm in all months of the event window.

One concern with restricting the sample to workers who remain employed between April and December is that employment in endogenous to the effect of the policy, so the treatment and control groups may not be comparable. For instance, if the policy causes firms to dis-
proportionately layoff workers with low expected wage growth, then my empirical strategy would over-estimate the true income effect. To address this, in appendix figure A.8 I compare the probability that workers in the treatment and control groups remain with their employer following the announcement and enactment of a higher overtime exemption threshold. While I find no trend break in the survival function of workers in the treatment group relative to the control group due to the federal rule changes, workers in the treated group experience a one-time increase in separations of 0.7 p.p on the month of a state threshold change. Given that the effect of increasing the threshold on separations is fairly small, restricting the sample to continuously employed workers is unlikely to significantly bias my estimates of the income effect.

**Estimates of Income Effect.** Figure 6 plots the difference-in-difference estimates for all three policy evaluations: the cross-year analyses of the 2016 and 2020 rule changes, and the event-study of the state reforms. For the federal policies, I indicate both the month that the new threshold is announced and the month that it went into effect, whereas for the event-study, I only indicate the latter. The dependent variable is weekly base pay in the top three figures, and weekly overtime pay in the bottom three figures.

Reviewing all six graphs in figure 6 there are five key features that I would like to highlight. First, in all cases, the treatment and control groups have similar base pay and overtime pay prior to the announcement of the new rule, which suggests that the identification assumption holds. Second, following the announcement of the federal policies, workers’ income begin to rise even before the new FLSA threshold goes into effect. Third, unlike the federal policies, workers’ income only begin to rise one month prior to an increase in the state threshold. A possible explanation for this difference in anticipatory response is that it simply takes less time for firms to fully adjust to a small change in the threshold relative to a large change. Forth, in all cases, workers experience a sharp jump in their base pay and overtime pay at precisely the month that the threshold increases, and this raise remains fairly stable after the policy goes into effect. Lastly, across all three sources of policy variation, I find that the average worker experiences a larger increase in base pay than overtime pay. These results reject the primary prediction of the compensating differentials model that firms would cut workers’ base salary to nullify the costs of overtime coverage.

I summarize the income effect of expanding overtime coverage for salaried workers in table 4. The first two rows report the increase in base pay and overtime pay, respectively, as of the first month that the new threshold goes into effect. I compute the sum of these two estimates in row (3), which I denote as the effect on workers’ total income. Dividing
the change in total income by the average income in the baseline, I show in row (4) of columns (1)-(3) that average total income increased by 1.3% (s.e. 0.1%) due to the 2016 FLSA policy, 2.1% (0.3%) due to the 2020 FLSA policy, and 1.4% (0.1%) due to the state policies. Alternatively, I also compare the change in log total income between the treatment and control group over time, which measures the average percent change in income rather than the percent change in the average income. I present these estimates in row (5), and find that their magnitudes are fairly similar to those in row (4).

By construction, if a worker is reclassified from salaried to hourly, I defined the weekly base pay component of their total income as forty times their wage. While this is a useful benchmark for comparing the income of jobs with different weekly hours, it overstates the actual earnings of workers who work less than forty hours per week. To directly measure the effect of expanding overtime coverage on workers’ weekly earnings, I also estimate equation 6 using log gross pay as the outcome variable. While these numbers suggest that the 2016 FLSA policy had no effect on gross pay, this is likely due to imputation error in translating monthly gross pay to an average weekly amount without observing the number of paychecks that workers received each month before 2016. As evidence of this, I report in column (4) the income effect of the six state threshold increases that occurred prior to 2016. Similar to the estimates in column (1), I find positive significant effects on base pay and overtime pay, but no effect on gross pay. In contrast, the gross pay effect is twice as large in column (3) when I include all sixteen state policies. Focusing on just the 2020 FLSA policy where weekly gross pay is defined without imputation error, the estimate suggests that gross pay increased by 0.5 p.p less than total pay.

In column (5) of table 4, I report the estimates of the income effect averaged across all 18 policy changes. As expected, there is a positive effect on total income that is primarily driven by an increase in base pay. In column (6), I show that these estimates are robust to restricting the sample to only firms that employ workers in both the treatment and control

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41 I censor gross pay at two times total pay. The estimates of the effect on both censored and uncensored gross pay are presented graphically in appendix figure A.9. While the magnitudes of the estimates are similar, the latter is more volatile. Note also that the event-study estimates exhibits a spike in gross pay the month prior to the event date. This is entirely driven by workers in New York, and exists even for individuals earning well above the interval treated by the change in the threshold. Since this spike does not appear in base or overtime pay, this spike likely reflects larger end of year bonuses in New York compared to other states.

42 Similarly, the total pay effect is 0.4 p.p larger than the gross pay effect if I restrict the event study to only policies post-2016.

43 These are estimated using equation 6 but keeping only the two months of data per policy change: a reference month and the month that the threshold increased.
group within each event. Lastly, as a placebo check, I examine the effect of the threshold changes on workers who were already earning $40 to $80 above the new threshold. I show in column (7) that while the placebo workers in the treated states experienced a larger increase in income compared to similar workers in the control states, the percent change in total income is an order of magnitude less than for directly affected workers.

Dividing the percent change in employment in column (7) of table 3 by the percent change in total pay in column (5) of table 4, I estimate an elasticity of employment with respect to own wage of -3.69 (s.e. 1.92). In comparison, previous studies in the minimum wage literature estimate an elasticity of no less than -2 with 95% confidence. However, given my large standard errors, I am unable to reject that raising the overtime exemption threshold has the same elasticity as raising the minimum wage.

7 Heterogeneous Effects of the 2016 FLSA Proposal

The results thus far suggest that in response to an increase in the overtime exemption threshold, firms decreased aggregate employment, reclassified jobs from salaried to hourly, and raised workers’ incomes. In this section, I examine how these margins of adjustments vary along the distribution of base pay. Given that the state policies are too small for the labor market responses to vary significantly along the intervals of base pay affected, my analysis will focus on the large 2016 FLSA policy that attempted to double the federal exemption threshold from $455 per week to $913 per week. Leveraging the matched employer-employee matched panel structure of the data, I categorize workers in April and December 2016 as either stayers, new hires, or separations. Among stayers, I further partition the sample into those who switched pay classifications and those who maintained the same salaried/hourly status in both months. Collapsing each of these subsamples by pay classification and base pay, I construct the distribution of employment flows, cross-classification flows, and within-classification flows. Following the discussion in section 5.1, my identification strategy to estimate the effect on each of these transitions is to compare the distribution in 2016 to a linear transformation of the distribution in 2015 (see equation 3).

7.1 Employment Flows

Examining the distribution of employment flows, I find the negative employment effect is driven primarily by a reduction in hires rather than an increase in separations. In figures 44 See appendix A.4 of Harasztosi and Lindner (2019) for a detailed review of the elasticities in the minimum wage literature.
7a and 7b, I plot the distribution of separations and new hires into the salaried distribution, respectively, for each year between 2012 and 2016. Compared to previous years, there appears to be fewer separations in 2016 within the $455 and $913 range, though the difference is small. This is consistent with the previous observation that the increase in the threshold had no effect on the probability that workers remain employed at the same firm. In contrast, there are noticeably fewer new hires between the old and new thresholds in 2016 compared to previous years. Furthermore, workers that do get hired are more likely to be bunched at the new threshold. Consistent with prediction 5 of the job search model, this implies that employers are forward-looking and slowed down their hiring of affected workers before the new overtime exemption threshold went into effect, hence why the 2016 FLSA policy decreased employment even though it never was never binding. This result is similar to recent findings that firms cut employment in response to the minimum wage via a reduction in hires rather than an increase in layoffs (Gopalan et al., 2018).

To understand how the employment effect varies by income, I compute the difference between the number of hires and separations within each bin of base pay. I estimate the effect of the 2016 rule change on this distribution using equation 3, which compares the net employment flows in 2016 to that of 2015. These estimates, presented in figure 7c, indicate that the employment loss was spread across the entire interval of weekly base pays affected by the increase in the threshold. However, if one assumes that absent the policy, new hires bunched at the new threshold would have earned right below it, then most of the employment effect is borne by workers earning less than $817 per week. Scaling the decline in employment within each bin by their respective number of workers in April, figure 7d shows that the each bin lost relatively the same share of its employment. To validate the econometric model, appendix figure A.10 tests how well it fits the distribution of employment flows in the years prior to 2016. Although the placebo effects deviate slightly from zero, these effects are small compared to the effect in 2016 and do not show systematic bias in either direction.

In principle, the effect of the policy on the number of employees should be equivalent to its effect on the number of hires minus separations. To measure the aggregate employment effect using the latter method, I would also need to estimate the effect of the policy on the net employment flows of hourly jobs. However, I show in appendix figure A.11 that the identification strategy fails to satisfy the placebo test when applied to the hourly distribution. Nevertheless, it is still informative to compare the fall in salaried employment flows to the fall in total employment estimated in section 5. Summing across the bin-specific treatment

45 I omit the estimates below $332 of base pay since very few salaried workers earned within that range in April 2016.
I find that total employment flows fell by 0.32 jobs (s.e. 0.06) as a result of the 2016 FLSA policy, or 3.8 (s.e. 0.7) jobs per one hundred directly affected workers. Remarkably, this drop explains nearly the entire decline in aggregate employment (see column (1) of table 2).

### 7.2 Reclassification Flows

Repeating a similar analysis for the flow of workers between pay classifications, I plot in figures 8a and 8b 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. Similar to the effect on new hires, I see that individuals who do transition from hourly to salary are more likely to become bunched at the new threshold. Interestingly, the clear increase in reclassifications out of the salaried distribution extends past the proposed $913 threshold.

To estimate the net reclassification effect of the 2016 FLSA policy, I make a minor adjustment to the procedure outlined in section 5.1. Since there are very few reclassifications in the right rail of the base pay distribution, small differences in reclassifications across years leads to large deviations in the parameters used to construct the control group. Given the stability of the distribution of reclassifications over time, I instead assume that $\gamma_1 = 1$ and $\gamma_0 = 0$. To validate my identification strategy, in appendix figure A.13, I estimate the cumulative reclassification effects for 2012-2015 as a placebo test and find very small estimates relative to the change in 2016.

Figure 8c overlays the estimates of the net reclassification effects into the salaried and hourly distributions. There are three findings that I would like to highlight. First, jobs across the entire range of affected base pays are reclassified, including those right below the threshold and even those right above it. This is in contrast to the employment flows which mainly affected workers earning below $817 per week. Second, the distribution of net reclassifications into hourly jobs has a very similar shape to the negative of the net reclassifications into salaried jobs. Given the width of the bins, this suggests that firms are paying reclassified workers a wage within two dollars of their previous salary divided by 40.

Recall from section 5.1 that on average, 8.45 salaried workers were initially between the old and new thresholds.

For the equivalent graphs from the perspective of the hourly distribution, see appendix figure A.12.

For a clearer comparison between these two distributions, I plot their difference in figure 8d. Aside from a small bunching effect, the difference is relatively flat across the base pay distribution.
Third, in total, firms are paying 0.84 (s.e. 0.057) more workers by hour rather than by salary due to the increase in the FLSA overtime exemption threshold. Scaling this estimate by the number of salaried workers initially between the old and new thresholds, I find that for every one hundred workers directly affected by the reform, 10 (s.e. 0.7) workers are reclassified from salaried to hourly.

To compare the reclassification effects to the results in section 5.1, I use the following accounting identity:

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

The change in the total number of workers within a given pay classification ($\Delta n$) comprises of an employment effect and a reclassification effect. I report each of these components in table 5, where I scale the estimates by the number of directly affected workers.\(^{49}\) The first column of the table corresponds to column (1) of table 2. The decomposition indicates that the reclassification effect explains 65% of the decline in salaried jobs estimated in section 5 and accounts for nearly the entire rise in hourly jobs. Although I am unable to credibly estimate the effect of the FLSA proposal on the employment flow of hourly workers, the relative size of the reclassification effect suggests that the magnitude of the employment effect for hourly jobs is likely to be small. If one is willing to accept the strong assumption that the entire decline in aggregate employment is driven by the hiring and separation of salaried workers, then the employment flow estimate implies a tight 95% confidence interval on the aggregate employment effect of 2.4 to 5.2 jobs lost per one hundred workers directly affected by the expansion in overtime coverage.

### 7.3 Within-Classification Flows

In figure 9, I plot the effect of the 2016 overtime proposal on the number of always-salaried workers across the base pay distribution, estimated from equation 3. The sample consists of continuously employed workers who stay within the salaried distribution so by definition, the total change in the number of workers equals zero.\(^{50}\) The figure shows that the spike at the new threshold comes from workers who would have otherwise earned between $720 and $913 per week. As a placebo check, I do not observe any bunching in the years prior to 2016.

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\(^{49}\) The decomposition is not exact since I used a different linear transformation to construct the counterfactual when estimating each component.

\(^{50}\) To ensure that the total effect on the counterfactual distribution also sums to zero, I assume that the constant in the linear transformation used to construct the control group equals zero (i.e. $\gamma_0 = 0$).
Workers bunched at the new threshold experienced a much larger income effect compared to the average worker directly affected by the 2016 FLSA policy. In appendix figure A.16, I re-estimate the distributional effects from figure 9 using smaller increments of base pay of $20, and show that the median base pay in the hole to the left of $913 is between $853 and $873. A back of the envelope calculation (i.e. $\frac{913-863}{863}$) implies that the median bunched worker earned 5.8% more per week due to the rule change, nearly five times the effect on the average worker estimated in section 6. Taking the ratio of the size of the spike (0.412) to the number of workers directly affected by the 2016 policy (8.45), I find that only 4.9% (s.e. 0.2%) of workers benefit from the bunching, not counting new hires or reclassified workers. Taken together, these results suggest that raising the overtime exemption threshold greatly benefited a small share of workers who received raises to the new threshold but would have otherwise earned within $200 below it.

7.4 Income Effects

While the results in section 6 imply that workers directly affected by the 2016 FLSA policy experienced a 1.2% increase in their total pay, these gains may vary across the base pay distribution. To determine which workers benefited most from the 2016 proposal, figure 10 presents estimates of equation 6 by workers’ base pay in April 2016. The figure shows that increasing the overtime exemption threshold had a statistically significant positive effect on the total pay of salaried workers initially earning as low as $336 to those earning as high as $1200. However, this positive income effect is largest between the old and new thresholds. Within the directly affected interval, workers initially earning between $721 and $817 experienced a 0.4 p.p larger raise relative to other affected workers. This is consistent with the earlier observation that these are the workers farthest away from the new threshold who still get bunched at the threshold.

To understand the source of the income effect, in appendix figure A.17 I decompose the effect of the 2016 FLSA policy on total pay to its effect on base pay and overtime pay. As expected, workers within the $721-817 bin experienced the largest increase in their base pay. Firms also raised the base pay of workers who do not get bunched at the new threshold. Above the $913 threshold though, I find that the spillover effect on total pay is entirely attributed to an increase in overtime pay rather than base pay.

51 Repeating the same analysis for always-hourly workers, I show in appendix figure A.15 that the 2016 FLSA policy had negligible effects on hourly stayers and that the effects do not differ significantly from those of the placebo years.
8 Discussion and Conclusion

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

My empirical results differ from the conclusions of the Department of Labor in four ways. First, the DOL believed from the classic labor demand model of overtime that by increasing the marginal cost of labor per hour, expanding overtime coverage would spread employment to more workers, though they do not attempt to quantify the magnitude of those effects. In contrast, I estimate that the 2016 FLSA policy actually reduced employment by about four jobs per hundred workers initially between the old and new thresholds. To substantiate this result, I find further evidence of negative employment effects from the event-study of the state policies and from directly estimating the effect of the 2016 policy on the net employment flows into the salaried distribution. In the latter analysis, I also find that the reduction in labor was primarily due to a slow down of hires rather than an increase in separations. Second, while the DOL’s assessment accurately predicted that the average affected worker would experience an increase in their weekly earnings, they calculated that average income would rise by only 0.7%, whereas I show that it increased by nearly twice that amount. I also show that this positive income effect uniformly distributed across the range of affected base pays. Third, drawing from previous studies of the contract model of overtime, the DOL calculated that 18% of workers would experience a 5.3% decrease in their base pay to partially offset the increase in their overtime pay. However, I find no evidence that firms reduced workers base pays in response to being covered for overtime. Instead, the clearest effect on the distribution of base pay among stayers is the bunching of some workers who would otherwise earn between $720 and $913 per week to above the new threshold. Forth, the DOL considered the reclassification effects of the policy negligible given the available evidence at the time. In contrast, I find that the reclassifications are large: for every one
hundred workers directly affect by the reform, ten are reclassified from salaried to hourly.

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


Notes: This figure shows the binding overtime exemption threshold in each state between 2005 and 2020. All states not included in the graph are covered by the Fair Labor Standards Act (FLSA), depicted by the line labeled "Federal". The line "2016 FLSA" represents the federal threshold that was supposed to go into effect on December 1, 2016 but was nullified in November 2016. In Alaska and California, the threshold equals 80 times the state minimum wage. In New York, the threshold equals 75 times the minimum wage. In Maine, the threshold equals 3000/52 times the minimum wage. Starting in January 2017, the minimum wage and threshold varies by firm size in CA, and county and firm size in NY. When the threshold varies within-state, I plot the highest threshold faced by any employer in the state.
Notes: Panel (a) shows the frequency distribution of weekly base pay (as defined in section 4) of salaried workers in April and December 2016, scaled by the number of firms in the balanced sample. The blue line represents the distribution in April and the red line represents the distribution in December. The vertical black dashed line is at the bin containing the overtime exemption threshold in April ($455), while the red dashed line is at the bin containing the proposed threshold for December ($913). The bins have width $96, shifted such that $913 is the start of a bin. The distribution is truncated at $2500. Panel (b) shows the difference between the frequency distribution of salaried workers’ base pay in December and April, by year. The last bin in panel (b) counts all workers with base pays $\geq$ $2500. Panels (c) and (d) are the hourly worker analog to panels (a) and (b), respectively.
Notes: The figure shows the frequency distribution of weekly base pays in each month of 2016 and 2017, subtracted by the frequency distribution in April 2016. For each month, I scale the distribution by the number of firms that I continuously observe over the 24 months. The bins are $40 wide, shifted so that one bin starts at exactly $913. The left vertical black dashed line is at the bin containing the overtime exemption threshold in April ($455), while the right dashed line is at the bin containing the threshold ($913) that was supposed to go into effect on December 1, 2016.
Figure 4: Effect of Raising the 2016 OT Policy on the Frequency Distribution of Base Pay, by Salaried/Hourly Status

(a) Estimates of Effect on Number of Salaried Jobs

(b) Placebo Test of Effect on Salaried Distribution

(c) Estimates of Effect on Number of Hourly Jobs

(d) Placebo Test of Effect on Hourly Distribution

Notes: Panel (a) shows the effect of the 2016 FLSA policy on the number of salaried jobs in each $96.15 bin of base pay in Dec 2016. The treatment effects are estimated using equation 3. The solid blue line is the running sum of these effects. The solid blue line in panel (b) is the same as the solid blue line in panel (a). The dotted graphs in panel (b) are similarly defined running sums, where the effect of the policy is estimated using the December and April distributions of the labeled year and the preceding adjacent year. Panels (c) and (d) are analogous to panels (a) and (b) for the distribution of hourly jobs. In all graphs, the vertical black and red lines are at the bins that contain the old and new OT exemption thresholds ($455 and $913), respectively.
Figure 5: Event Study of Raising State’s OT Exemption Threshold on the Frequency Distribution of Base Pay

Notes: Panel (a) shows the event study estimates of the effect of raising the OT exemption threshold on the number of salaried jobs in each $40 bin of base pay on the month that the the new threshold goes into effect. The bins are normalized so that the new threshold for each event is 0. The left vertical dashed line is set at the lowest old threshold of all the events. The effects are estimated using equation 5. The solid blue line is the running sum of these effects. Panel (b) shows the employment effects over time, separately for bins between the old and new thresholds (red line) and bins above the new threshold (blue line). Panels (c) and (d) are analogous to panels (a) and (b) for the distribution of hourly jobs. For each estimate, I show the 95% confidence interval using standard errors clustered by firm.
Figure 6: 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 6, where the four panels on the left compares workers in the year of the FLSA rule change to similar workers in the preceding year, and the two right panels compare workers in states that raise their thresholds to similar workers in states that do not. In the four panels on the left, the first dotted vertical line at 0 indicates the month that the rule change is announced, and the second indicates the month that the new threshold actually goes into effect. In the two panels on the right, the vertical line indicates the month that the new threshold goes into effect.
Figure 7: Effect of Raising the 2016 FLSA OT Exemption Threshold on the Distribution of Employment Flows

Notes: Panel (a) shows the average firm’s frequency distribution of base pays in April of each year between 2012 and 2016 for salaried workers who separate from their employer by December. Panel (b) shows the frequency distribution of base pays in December of each year for salaried workers hired between April and December. Panel (c) shows the effect of raising the 2016 overtime exemption threshold on the difference between the number of hires and separations, estimated from 3. The solid blue line is the cumulative sum of the bin-specific effects. Panel (d) scales the estimates in panel (c) by the initial employment within each bin in April 2016. In all figures, the vertical black and red lines are at the initial and proposed 2016 FLSA thresholds, respectively.
Figure 8: 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 of each year between 2012 and 2016 for workers who are salaried in April, hourly in December, and stayed at the same firm. Figure (b) shows the frequency distribution of base pays in December of each year for workers who are hourly in April, salaried in December, and employed in the same firm in both months. Figure (c) plots the effect of the 2016 FLSA policy on the net number of reclassifications into the salaried distribution (in blue) and the net number of reclassifications into the hourly distribution (in red), estimated by equation 3 assuming $\gamma_1 = 1$ and $\gamma_0 = 0$. The solid lines are the cumulative sum of these bin-specific effects. Panel (d) shows the difference between the blue and red bars in panel (c). The vertical black and red lines are at the initial and proposed 2016 FLSA thresholds, respectively.
Figure 9: Effect of Raising the OT Exemption Threshold on the Frequency Distribution of Always-Salaried Workers

Notes: This figure shows the effect of the 2016 OT policy on the number of workers along the base pay distribution who stay at the same firm between April and December 2016 and are paid by salary in both months. The estimates are computed using equation 3 assuming $γ_0 = 0$. The solid blue line is the cumulative sum of the bin-specific effects. The vertical black and red lines are at the initial and proposed 2016 FLSA thresholds ($455 and $913), respectively.
Notes: This figure shows the income effect of the 2016 FLSA proposal by salaried workers initial base pay in April 2016, estimated with equation (6). Each point compares the change in total income (base pay + OT pay) from April to December between workers in 2016 and workers in 2015 within the same $96.15 bin of base pay. The red bars at each point are 95% confidence intervals. The vertical dashed lines are at the initial and proposed 2016 FLSA thresholds ($455 and $913).
Table 1: Summary of Theoretical Predictions

<table>
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<tr>
<th>Prediction</th>
<th>Labor Demand</th>
<th>Compensating Differentials</th>
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<td>↑</td>
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<td>Employment</td>
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<td>-</td>
<td>↓</td>
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<tr>
<td>Pay structure</td>
<td>-</td>
<td>-</td>
<td>Reclassification from salaried to hourly</td>
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<tr>
<td>Dynamics</td>
<td>-</td>
<td>-</td>
<td>Anticipatory response</td>
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Notes: This table summarizes the predictions of the three models of overtime discussed in section 3. The third column of the table refers to the job-search and bargaining model with fixed costs.
Table 2: Employment Effect of Raising the FLSA OT Exemption Threshold

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<tr>
<th>Panel A: Employment</th>
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<td>-1.901***</td>
<td>-1.389***</td>
<td>-1.440***</td>
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<td></td>
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<td>(0.140)</td>
<td>(0.122)</td>
<td>(0.146)</td>
<td>(0.111)</td>
<td>(0.060)</td>
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<tr>
<td>Hourly</td>
<td>0.943***</td>
<td>1.042**</td>
<td>1.546***</td>
<td>0.583</td>
<td>0.983***</td>
<td>-0.050</td>
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<tr>
<td></td>
<td>(0.310)</td>
<td>(0.374)</td>
<td>(0.470)</td>
<td>(0.490)</td>
<td>(0.310)</td>
<td>(0.390)</td>
</tr>
<tr>
<td>Total</td>
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<td>-0.965**</td>
<td>0.157</td>
<td>-0.866</td>
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<td></td>
<td>(0.352)</td>
<td>(0.435)</td>
<td>(0.511)</td>
<td>(0.566)</td>
<td>(0.351)</td>
<td>(0.400)</td>
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<td>Δ Jobs per Aff. Worker</td>
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<td>-0.073**</td>
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<td>-0.068</td>
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<td>(0.042)</td>
<td>(0.033)</td>
<td>(0.058)</td>
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<th>Panel B: Wage Bill</th>
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<td>-764***</td>
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<td>(108)</td>
<td>(139)</td>
<td>(136)</td>
<td>(151)</td>
<td>(108)</td>
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<tr>
<td>Hourly</td>
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<td>1,742***</td>
<td>914***</td>
<td>992***</td>
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<td>(212)</td>
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<td>(367)</td>
<td>(297)</td>
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<td>227</td>
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<td>(265)</td>
<td>(327)</td>
<td>(421)</td>
<td>(393)</td>
<td>(266)</td>
<td>(94)</td>
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<td>%Δ Wage Bill</td>
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<td>0.008</td>
<td>0.108</td>
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<td>0.037</td>
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<td>(0.043)</td>
<td>(0.034)</td>
<td>(0.066)</td>
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<td>8.78</td>
<td>7.53</td>
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<td>Wage Bill of Aff. Workers</td>
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<td>9.630</td>
<td>6.374</td>
<td>5.459</td>
<td>6.120</td>
<td>1.075</td>
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<td>Avg. Firm Size</td>
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<td>204</td>
<td>142</td>
<td>112</td>
<td>127</td>
<td>127</td>
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<tr>
<td>Number of Firms</td>
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<td>58,469</td>
<td>41,700</td>
<td>49,710</td>
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<td>42,445</td>
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<td>No MW</td>
<td>No MW</td>
<td>No MW</td>
<td>No MW</td>
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<td>Firms Size (%)</td>
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<td>100</td>
<td>99.9</td>
<td>99.9</td>
<td>99.9</td>
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<td>Yes</td>
<td>Yes</td>
<td>No</td>
<td>Yes</td>
<td>Yes</td>
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<tr>
<td>Cutoff</td>
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<td>1776</td>
<td>1776</td>
<td>1776</td>
<td>1393</td>
<td>1776</td>
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</table>

Notes: Rows (1) and (2) report the effect of raising the OT exemption threshold on the number of salaried and hourly jobs, respectively, on the month that the new threshold goes into effect. Row (3) equals the sum of rows (1) and (2). Row (4) scales row (3) by the number of affected workers, defined as the average number of workers between the old and new thresholds in the month prior to the announcement of the new rule. Rows (5) to (7) report the wage bill effects analogous to rows (1) to (3). Row (8) scales the net change in the wage bill in row (7) by the initial wage bill of affected workers.

Columns (1)-(5) report the effects of the 2016 FLSA policy, estimated using equation 3. Column (1) calculates each firm’s employment across states with no minimum wage changes, dropping the 0.1% largest firms, for a balanced sample of firms, using a cutoff of $1776 to estimate equation 4. Relative to column (1), column (2) calculates firms’ employment across all states, column (3) keeps the largest 0.1% of firms, column (4) uses an unbalanced sample of firms, and column (5) estimates equation 4 using a cutoff of $1393. Column (6) estimates the same specification as column (1) for the 2020 federal FLSA policy. All robust standard errors in parentheses are clustered by firm. *p < .1, **p < .05, ***p < .01
Table 3: Employment Effect of Raising States’ OT Exemption Threshold

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<tr>
<th>(1)</th>
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<th>(7)</th>
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<tr>
<td><strong>Panel A: Employment</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
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</tr>
<tr>
<td>Salaried</td>
<td>$-0.068^{***}$</td>
<td>$-0.135^{***}$</td>
<td>$-0.050^{***}$</td>
<td>$-0.042^{***}$</td>
<td>$-0.065^{***}$</td>
<td>$-0.056^{***}$</td>
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<tr>
<td></td>
<td>(0.010)</td>
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<td>(0.010)</td>
<td>(0.014)</td>
<td>(0.008)</td>
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<td>Hourly</td>
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<td>$0.097^{**}$</td>
<td>$-0.021$</td>
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<td>Total</td>
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<td>$-0.071^{***}$</td>
<td>$-0.054$</td>
<td>$-0.054$</td>
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<td>(0.029)</td>
<td>(0.043)</td>
<td>(0.026)</td>
<td>(0.034)</td>
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<tr>
<td>$\Delta$ Jobs per Aff. Worker</td>
<td>$-0.040^{*}$</td>
<td>$-0.024$</td>
<td>$-0.060^{***}$</td>
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<td>(0.032)</td>
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<tr>
<td><strong>Panel B: Wage Bill</strong></td>
<td></td>
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<td></td>
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<tr>
<td></td>
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<td>(139)</td>
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<td>Hourly</td>
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<td>$204^{***}$</td>
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<td>$%\Delta$ Wage Bill</td>
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Treatment Group

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<td>1.18</td>
<td>1.18</td>
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<td>1.251</td>
<td>1.251</td>
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<td>Avg. Firm Size</td>
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<td>186,105</td>
<td>186,105</td>
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Controls

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Sample

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</tbody>
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Notes: Rows (1) and (2) report the effect of raising the OT exemption threshold on the number of salaried and hourly jobs, respectively. Row (3) equals the sum of rows (1) and (2). Row (4) scales row (3) by the number of affected workers, reported in row (5), defined as the average number of workers between the old and new thresholds two months before the new threshold goes into effect. Rows (5) to (7) report the wage bill effects analogous to rows (1) to (3). Row (8) scales the net change in the wage bill in row (7) by the initial wage bill of affected workers.

Columns (1) and (2) reports the average employment effect of increasing a state’s OT exemption threshold at 0 and 5 months after the date of the rule change, respectively, estimated using equation 5. Column (3) controls for bin-state-event and bin-month-event fixed effects. Column (4) restricts the sample within each event to only firms that employ workers in both the treatment and control states. Column (5) drops the three threshold increases that occurred on January 1, 2017. Column (6) restricts the sample to only threshold increases that went into effect prior to 2016. All robust standard errors in parentheses are clustered by firm. *$p < .1$, **$p < .05$, ***$p < .01$
Table 4: Income Effect of Raising the OT Exemption Threshold

<table>
<thead>
<tr>
<th></th>
<th>FLSA 2016</th>
<th>FLSA 2020</th>
<th>Event-Study</th>
<th>Pooled</th>
<th>Placebo</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
<td>(3)</td>
<td>(4)</td>
<td>(5)</td>
</tr>
<tr>
<td>Base Pay ($)</td>
<td>6.72***</td>
<td>10.74***</td>
<td>11.61***</td>
<td>11.84***</td>
<td>9.09***</td>
</tr>
<tr>
<td></td>
<td>(0.59)</td>
<td>(1.74)</td>
<td>(0.58)</td>
<td>(0.95)</td>
<td>(0.44)</td>
</tr>
<tr>
<td>OT Pay ($)</td>
<td>2.73***</td>
<td>1.60***</td>
<td>0.71***</td>
<td>0.57*</td>
<td>1.81***</td>
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<tr>
<td></td>
<td>(0.35)</td>
<td>(0.33)</td>
<td>(0.22)</td>
<td>(0.31)</td>
<td>(0.19)</td>
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<tr>
<td>Total Pay ($)</td>
<td>9.45***</td>
<td>12.34***</td>
<td>12.32***</td>
<td>12.22***</td>
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</tr>
<tr>
<td></td>
<td>(0.70)</td>
<td>(1.78)</td>
<td>(0.63)</td>
<td>(0.10)</td>
<td>(0.49)</td>
</tr>
<tr>
<td>%Δ Total Pay</td>
<td>0.013***</td>
<td>0.021***</td>
<td>0.014***</td>
<td>0.017***</td>
<td>0.014***</td>
</tr>
<tr>
<td></td>
<td>(0.001)</td>
<td>(0.003)</td>
<td>(0.001)</td>
<td>(0.001)</td>
<td>(0.001)</td>
</tr>
<tr>
<td>Log Total Pay</td>
<td>0.012***</td>
<td>0.017***</td>
<td>0.013***</td>
<td>0.015***</td>
<td>0.012***</td>
</tr>
<tr>
<td></td>
<td>(0.001)</td>
<td>(0.002)</td>
<td>(0.001)</td>
<td>(0.001)</td>
<td>(0.001)</td>
</tr>
<tr>
<td>Log Gross Pay</td>
<td>0.003</td>
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<td>0.008***</td>
<td>0.004</td>
<td>0.006***</td>
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<tr>
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<td>(0.003)</td>
<td>(0.002)</td>
<td>(0.003)</td>
<td>(0.002)</td>
</tr>
<tr>
<td>State FE</td>
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<td>Y</td>
<td>-</td>
<td>-</td>
<td>-</td>
</tr>
<tr>
<td>Time FE</td>
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<td>Y</td>
<td>-</td>
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<td>-</td>
</tr>
<tr>
<td>Event-State FE</td>
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<td>-</td>
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<td>Y</td>
<td>Y</td>
</tr>
<tr>
<td>Event-Time FE</td>
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<td>-</td>
<td>Y</td>
<td>Y</td>
<td>Y</td>
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<tr>
<td>Balanced Firms</td>
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</tr>
</tbody>
</table>

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

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

<table>
<thead>
<tr>
<th></th>
<th>( \Delta n )</th>
<th>Employment</th>
<th>Reclassification</th>
</tr>
</thead>
<tbody>
<tr>
<td>Salaried</td>
<td>-0.154 (0.013)</td>
<td>-0.038 (0.007)</td>
<td>-0.099 (0.007)</td>
</tr>
<tr>
<td>Hourly</td>
<td>0.111 (0.037)</td>
<td>0.099 (0.007)</td>
<td>0.000 (0.000)</td>
</tr>
</tbody>
</table>

Notes: Column (1) reports the effect of the 2016 FLSA policy on the number of salaried employees, the number of hourly employees, and the total number of employees in the average firm estimated in section 5.1. Column (2) shows the effect of the policy on the number of hires minus separations, estimated in section 7.1. Column (3) shows the effect of the policy on the number of reclassifications, estimated in section 7.2. All estimates are scaled by 8.45, which was the number of directly affected workers in April 2016.
Appendix A. Additional figures and tables

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

Notes: The figure shows the share of all salaried workers in the May extracts of the CPS who report usual weekly earnings below the effective FLSA overtime exemption threshold from 1973 to 2017. The threshold increased from $200 per week to $250 per week in January 1975, and then to $455 in August 2004. The dotted blue line shows the percent of salaried workers with usual weekly earnings below the $913 per week threshold announced in the 2016 policy.
Appendix Figure A.2: Percent of Salaried Workers Eligible for Overtime

Notes: This figure shows the percent of salaried workers in the PSID who respond yes to the question "If you were to work more hours than usual during some week, would you get paid for those extra hours of work".
Appendix Figure A.3: Frequency Distribution of Hourly Workers by Month, Differenced by the Distribution in April 2016

Notes: The figure shows the frequency distribution of weekly base pays of hourly workers in each month of 2016 and 2017, subtracted by the frequency distribution in April 2016. For each month, I scale the distribution by the number of firms in the superset of all firms that appear in the data that month and in April 2016. Within each graph, the bins are $20 wide except for the first bin which goes from $0 to $12.99. The vertical black dashed line is at the bin containing the overtime exemption threshold in April ($455), while the red dashed line is at the bin containing the threshold ($913) that was supposed to go into effect on December 1, 2016.
Appendix Figure A.4: Robustness of the Estimated Effects on the Frequency Distribution of Salaried Workers

Notes: Each figure presents the cumulative treatment effect of raising the OT threshold in 2016 on the number of salaried workers and the cumulative placebo effect for years prior to 2016, estimated from a different sample or specification of equation 3. Figure (a) estimates the bin-by-bin treatment effects for employment over all states covered by the FLSA overtime exemption threshold. Figure (b) includes in the sample the largest 0.1% of firms. Figure (c) uses an unbalanced panel whereby if a firm is missing in one month, its employment is coded as 0 in every bin. Figure (d) uses bins greater than or equal to $1393 to estimate the $\gamma_1$ and $\gamma_0$ in equation 3.
Appendix Figure A.5: Placebo Tests of the Cumulative Effects on the Frequency Distribution of Hourly Workers

Notes: Each figure presents the cumulative treatment effect of raising the OT threshold in 2016 on the number of hourly workers and the cumulative placebo effect for years prior to 2016, estimated from a different sample or specification of equation 3. Figure (a) estimates the bin-by-bin treatment effects for employment over all states covered by the FLSA overtime exemption threshold. Figure (b) includes in the sample the largest 0.1% of firms. Figure (c) uses an unbalanced panel whereby if a firm is missing in one month, its employment is coded as 0 in every bin. Figure (d) uses bins greater than or equal to $1393 to estimate the $\gamma_1$ and $\gamma_0$ in equation 3.
Appendix Figure A.6: Effect of Raising the 2016 OT Policy on the Frequency Distribution of Base Pay, by Salaried/Hourly Status

Notes: Panel (a) shows the effect of the 2016 FLSA policy on the number of salaried jobs in California, within each $96.15 bin of base pay in Dec 2016, estimated using equation 3. The solid blue line is the running sum of these effects. Panel (b) depicts the same estimates for the number of hourly workers. Panels (c) and (d) are analogous to panels (a) and (b) for firms in New York. In all graphs, the vertical black and red lines are at the bins that contain the old and new FLSA OT exemption thresholds ($455 and $913), respectively.
Appendix Figure A.7: Effect of Raising the 2020 OT Policy on the Frequency Distribution of Base Pay

(a) Estimates of Effect on Number of Salaried Jobs
(b) Placebo Test of Effect on Salaried Distribution
(c) Estimates of Effect on Number of Hourly Jobs
(d) Placebo Test of Effect on Hourly Distribution

Notes: Panel (a) shows the effect of the FLSA policy on the number of salaried jobs in each $96.15 bin in Jan 2020. The treatment effects are estimated using equation 3. The solid blue line is the running sum of these effects. The solid blue line in panel (b) is the same as the solid blue line in panel (a). The dotted graphs in panel (b) are similarly defined running sums, where the effect of the policy is estimated using the December and April distributions of the labeled year and the preceding adjacent year. Panels (c) and (d) are analogous to panels (a) and (b) for the distribution of hourly jobs. In all graphs, the vertical black and red lines are at the bins that contain the old and new OT exemption thresholds ($455 and $684), respectively.
Appendix Figure A.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 6. Panels (d)-(f) shows the difference in survival function between the treatment and control groups, corresponding to each of their above graphs. For the FLSA figures, the first dotted vertical line at 0 indicates the month that the rule change is announced. In panels (a) and (d), the second dotted line is at seven months after the announcement when the policy was supposed to go into effect. For the FLSA 2020 figures, the new threshold goes into effect four months after the announcement. For the event-study figures, the vertical line indicates the month that the new threshold goes into effect.
Appendix Figure A.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 [6] where the four panels on the left compares workers in the year of the FLSA rule change to similar workers in the preceding year whereas the right panels compare workers in states that raise their thresholds to similar workers in all states that do not. For the FLSA rule changes, the dotted vertical line at 0 indicates the month that the rule change is announced, whereas the second dotted line shows the month that the threshold actually goes into effect. For the state rule changes, the vertical line indicates the month that the new threshold goes into effect.
Appendix Figure A.10: Placebo Test of Net Employment Flows into the Salaried Distribution

Notes: This figure shows the cumulative sum of the estimates of equation 3 using the number of hires minus separations within each bin as the outcome variable. Each line is estimated using the employment flows from the year indicated in the legend and the employment flows in the adjacent previous year.
Appendix Figure A.11: Analysis of the Employment Flows into the Hourly Distribution

Notes: Panel (a) shows frequency distribution of base pays in April of each year between 2012 and 2016, averaged across firms, for hourly workers who separate from their employer between April and December. Panel (b) shows the frequency distribution of base pays in December of each year for hourly workers hired between April and December. Panel (c) shows the difference between panel (b) and (a). Panel (d) presents the cumulative effects estimated from equation 3 for each year from 2012 to 2016. In all figures, the vertical black and red lines are at the initial and proposed FLSA thresholds, respectively.
Appendix Figure A.12: Analysis of Reclassifications in and out of the Hourly Distribution

Notes: Figure (a) shows the frequency distribution of base pays in April of each year between 2012 and 2016, averaged across firms, for workers who are hourly in April, hourly in December, and employed in the same firm in both months. Figure (b) shows the frequency distribution of base pays in December of each year for workers who are hourly in April, hourly in December, and employed in the same firm in both months. Panel (c) shows the difference between panel (b) and (a). Panel (d) presents the cumulative effects estimated from equation 3, assuming $\gamma_1 = 1$ and $\gamma_0 = 0$ for each year from 2012 to 2016. In all figures, the vertical black and red lines are at the initial and proposed FLSA thresholds, respectively.
Appendix Figure A.13: Placebo Test of Net Reclassification Flows into the Salaried Distribution

Notes: This figure shows the cumulative sum of the effect of the 2016 FLSA policy on net reclassifications into the salaried distribution, estimated using equation 3 assuming $\gamma_1=1$ and $\gamma_0=0$. Each line is estimated as the reclassification flows in the year indicated in the legend minus the reclassification flows in the adjacent previous year. The vertical black and red lines are at the old and new OT exemption thresholds ($455$ and $913$), respectively.
Appendix Figure A.14: Placebo Test of Effect of 2016 FLSA Policy on Always-Salaried Workers

Notes: This figure shows the cumulative sum of the effect of the 2016 FLSA policy on the base pay distribution of continuously employed workers who are salaried before and after the policy, estimated using equation 3 assuming $\gamma_0=0$. Each line is the estimated effect for the year indicated in the legend, using the workers in the previous years as a control. The vertical black and red lines are at the old and new OT exemption thresholds ($455$ and $913$), respectively.
Appendix Figure A.15: Analysis of Flows Within the Hourly Distribution Between April and December

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

(b) Placebo Test of Effect on Salaried Distribution

Notes: Panel (a) shows the effect of the 2016 OT policy on the distribution of workers who stay at the same firm between April and December 2016, and are paid by hour in both months, estimated using equation 3, assuming $\gamma_0 = 0$. The solid blue line is the cumulative sum of the bin-specific effects. Panel (b) shows the cumulative effect of raising the OT exemption threshold on the number of job-stayers in December of each year between 2012 and 2016. The solid blue line in panel (b) is the same as the solid blue line in panel (a). The dotted graphs are similarly defined running sums, except estimated using the December and April distributions of the labeled year and the preceding adjacent year. In both graphs, the vertical black and red lines are at the old and new OT exemption thresholds ($455$ and $913$), respectively.
Appendix Figure A.16: Effect of Raising the OT Exemption Threshold on the Frequency Distribution of Always-Salaried Workers, Computed using $20 Bins of Base Pay

Notes: This figure shows the effect of the 2016 OT policy on the distribution of workers who stay at the same firm between April and December 2016, and are paid by salary in both months, estimated using equation $3$ assuming $\gamma_0 = 0$. The vertical dashed black and red lines are at the initial and proposed 2016 FLSA thresholds ($455 and $913), respectively. The dotted line at $853 is located at the median of the base pays in the hole to the left of the new threshold, between $733 and $913.
Appendix Figure A.17: Difference-in-Difference Estimates of Income Effect by Initial Base Pay

Notes: Figures (a) and (b) show the effect of the 2016 FLSA proposal on workers’ base pay and overtime pay, respectively, by salaried workers initial base pay in April 2016. Each point compares the change in income from April to December between workers in 2016 and workers in 2015 with similar base pays, estimated using equation 6. The vertical dashed lines are at the initial and proposed 2016 FLSA thresholds ($455 and $913).
Appendix B. History of the 2016 FLSA Policy

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

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

Notes: This figure shows the relative popularity of “FLSA Overtime” as a Google search term between January 2013 and January 2020. A value of 100 indicates its highest popularity level, and the measure of popularity is scaled proportional to this instance.

Interest in the the FLSA grew in 2015 following the DOL’s announcement on June 26th that it would like to “raise the threshold under which most salaried workers are guaranteed overtime to equal the 40th percentile of weekly earnings for full-time salaried workers. As proposed, this would raise the salary threshold from $455 a week ($23,660 a year) – below the poverty threshold for a family of four – to a projected level of $970 a week ($50,440 a year) in 2016” (White House Archives June 30, 2015). Consistent with the normal rulemaking process, the Department of Labor stated that it would release a finalized rule the next year after reviewing comments from the public regarding its current proposal. Similar to the initial announcement in 2014, new articles at the time believed that the policy would face challenges in the courts (NYT June 30, 2015). There were also some reports that companies were already investing in new software to comply with the policy (WSJ Jul 21, 2015), though I do not observe any evidence of this adjustment in the data (compare the difference-distributions in
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. This announcement received considerable attention from employers, as evident from the spike in Google searches for “FLSA Overtime”. In response to the new regulation, “Republican lawmakers, who are close to many of the industries that oppose the new rule, have vowed to block it during a mandated congressional review period”. However, given Donald Trump’s presidential campaign, there was an understanding that repealing the regulation would be a risky political move for the Republican party as it “could exacerbate an already palpable split between Mr. Trump’s blue-collar supporters and the party’s establishment donors and politicians” [NYT May 18, 2016]. Hence, it was not clear at this point that the rule would be repealed.

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

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

52 The final rule also raised the threshold for “highly compensated employees” from $100,000 per year to $134,004. Workers above this threshold are subject to a less stringent duties test to be exempt from overtime. I do not find any bunching in response to this component of the policy.
broader news on the FLSA overtime exemption threshold, none go into any more detail than stating that a case is under way (eg. USA Today Oct. 12, 2016). Consistent with the lack of awareness of the appeal against the new overtime exemption threshold, I see no increase in Google traffic for the term “FLSA Overtime” in September when the initial case was filed, but a large spike in November after its injunction.

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

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

Following the preliminary injunction, there was a general belief from judge Mazzant’s language that the $913 exemption threshold would not survive. However, it was uncertain how long the judicial process would take and whether the new Trump administration would propose a smaller increase to the overtime exemption threshold (NYT Nov 22, 2016). It became clearer that the new administration had no desire to defend the overtime policy in courts after the nomination of fast-food executive, and critic of overtime regulation, Andrew Puzder as Labor Secretary on December 8, 2016 (Forbes March 18, 2016). In the end, Andrew Puzder did not receive enough support from the Senate for his confirmation on February 15, 2016 and the position ultimately went to Alexander Acosta. Nevertheless, Acosta reaffirmed employers’ priors that the overtime threshold proposed by Obama would never go into effect. 
When asked about the overtime exemption threshold during his confirmation hearing on March 22, 2017, Acosta stated that “if you were to apply a straight inflation adjustment, I believe the figure if it were updated would be somewhere around $33,000”. The Department of Labor officially dropped its defense of the $913 threshold in June 2017.

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

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

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

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

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

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

Notes: The figure shows the distribution of firms in April 2016 by the share of workers who are paid by salary, and earn between $455 and $913 per week.
Appendix Figure C.2: Difference in the Base Pay Distribution of Salary Workers in “Unaffected” Firms

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

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

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

D.1 Solving the baseline model

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

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

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

The firm’s present value of profits is given by

\[J = \theta h^\beta - w + F \cdot \text{sgn}(S)\]

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

\[V_e(w, h) - V_n = \frac{w - a^{\frac{1}{\epsilon}}h^{1 + \frac{1}{\epsilon}} - rV_n}{r + \delta} \quad (7)\]

Substitute (7) into the Nash bargaining problem and take first order conditions:

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

\[\text{FOC}_w = \alpha[h^\beta \theta - w - FS] - (1 - \alpha)[w - a^{\frac{1}{\epsilon}}h^{1 + \frac{1}{\epsilon}} - rV_n] \quad (8)\]

\[\text{FOC}_h = -\alpha a^{-\frac{1}{\epsilon}}h^{\frac{1}{\epsilon}}(h^{\beta} \theta - w - FS) + (1 - \alpha)\beta h^{\beta - 1}\theta(w - a^{\frac{1}{\epsilon}}h^{1 + \frac{1}{\epsilon}} - rV_n) \quad (9)\]

Rearrange (8) for \(w_0\), then substitute \(w_0\) into (9) to solve for \(h_0\). \(S_0\) is simply the corner solution that maximizes the Nash product.

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

\[h_0 = \left(a^\frac{1}{\epsilon} \beta \theta \right)^{\frac{1}{1 + \frac{1}{\epsilon} - \beta}}\]

\[S_0 = \arg\max_{S \in \{-1, 1\}} F \cdot \text{sgn}(S)\]
Note that at $h_0$, $\frac{\partial J}{\partial h} = \frac{\partial V_e(w,h)}{\partial h}$. In other words, $h_0$ maximizes the total surplus of the employment relationship. To solve for $\theta_0^*(a, F)$, substitute $(w_0, h_0)$ into (7) and solve for the value of $\theta$ such that the expression equals 0.

$$V_e(w_0, h_0) - V_n = 0 \quad \Leftrightarrow \quad w_0 - a^{-\frac{1}{\epsilon}} h_0^{\frac{1+\frac{1}{\epsilon}}{1+\frac{1}{\epsilon}}} - r V_n = 0 \quad \Leftrightarrow \quad \alpha \left( \theta h_0^\beta + F \cdot \text{sgn}(S_0) - a^{-\frac{1}{\epsilon}} h_0^{\frac{1+\frac{1}{\epsilon}}{1+\frac{1}{\epsilon}}} - r V_n \right) = 0 \quad \Leftrightarrow \quad \theta_0^* = \left[ \frac{r V_n - \alpha F S}{(a^{\frac{1}{\beta}})^{1+\frac{1}{\beta}} \beta (1 - \beta) 1 + \frac{1}{\beta}} \right]^{1+\frac{1}{\beta} - \beta}$$

Note from the third line that at $\theta_0^*$, the total surplus of the employment relationship equals 0. I will use this property when computing the comparative statics. Lastly, from the Bellman equation representing the worker’s value of searching, I numerically solve for $V_n$

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

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

### D.2 Solving the model with overtime

**Case 1: No Fixed Costs or Wage Rigidities**

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

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

where

$$\eta_{(w,h,S)} = \begin{cases} 0.5 \frac{(h-40)}{40} & \text{if } h > 40 \text{ and } S = -1 \\ 1.5 \frac{(h-40)}{40} & \text{if } h > 40, S = 1, \text{ and } w < \bar{w} \\ 0 & \text{otherwise} \end{cases} \quad (10)$$
The worker’s value of employment follows

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

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

\[(w_1, h_1, S_1) = \arg \max_{(w,h,S)} [V_e(w, h) - V_n]^\alpha \left(\frac{\theta h^\beta - (1 + \eta(w,h,S))w + F \cdot \text{sgn}(S)}{r + \delta}\right)^{1-\alpha}\]

\[
\text{FOC}_w = \alpha [h^\beta \theta - (1 + \eta)w - F_S] - (1 - \alpha) [(1 + \eta)w - a^{-\frac{1}{\epsilon}}h^{1+\frac{1}{\epsilon}} + \delta V_n]
\]

\[
\text{FOC}_h = -\alpha a^{-\frac{1}{\epsilon}}h^{\frac{1}{\beta} - (1 + \eta)w - F_S} + (1 - \alpha) \beta h^{\beta - 1} \theta [(1 + \eta)w - a^{-\frac{1}{\epsilon}}h^{1+\frac{1}{\epsilon}} - r V_n]
\]

The Nash bargaining solution is

\[w_1 = \frac{1}{1 + \eta(w,h,S)} \left[\alpha \left(\theta h^\beta + F \cdot \text{sgn}(S)\right) + (1 - \alpha) \left(a^{-\frac{1}{\epsilon}}h^{1+\frac{1}{\epsilon}} + r V_n\right)\right]\]

\[h_1 = \left(\frac{a^{-\frac{1}{\epsilon}}h^\beta}{\theta} \right)^{1+\frac{1}{\epsilon} - \sigma}\]

\[S = \arg \max_{S \in \{-1, 1\}} F \cdot \text{sgn}(S)\]

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

**Case 2: Fixed Costs**

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

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

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

\[ w_2 = \frac{1}{1 + \eta(w,h,S)} \left[ \alpha \left( \theta h_1^\beta + F_S - C_1[S=1,w<\bar{w}] \right) + \left( 1 - \alpha \right) \left( a^{-\frac{1}{\epsilon}} h_0^{\frac{1}{1+\frac{1}{\epsilon}}} + r V_{n}^{OT} \right) \right] \]

\[ h_2 = \left( a^{\frac{1}{\epsilon}} \beta \theta \right)^{\frac{1}{1+\frac{1}{\epsilon}}} \]

\[ S_2 = \arg \max_{S \in \{-1,1\}} F_S - C_1[S=1,w<\bar{w}] \]

where \( V_{n}^{OT} \) is the value of unemployment given the overtime policy.

Corner solution:

\[ w_2 = \bar{w} \]

\[ h_2 \text{ solves } FOC_{h}|_{w=\bar{w}} = 0 \]

\[ FOC_{h}|_{h=h_0,w=\bar{w}} = a^{-\frac{1}{\epsilon}} h_0^{\frac{1}{\epsilon}} [\bar{w} - w_0] > 0 \]

\[ S_2 = 1 \]

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

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

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

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

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

\[
\left[ \frac{r + \delta}{\lambda} + \sigma(\Phi_{unaff}) + \alpha(\sigma(\Phi_{rec}) + \sigma(\Phi_{cov})) + \sigma(\Phi_{ban}) \right] r(V_{n}^{OT} - V_{N})
\]

\[
= -\int_{\Phi_{rec}} 2\alpha F dG - \int_{\Phi_{cov}} \alpha C dG + \int_{\Phi_{ban}} (\bar{w} - w_0) - \frac{a^{\beta - 1}}{1 + \frac{1}{\epsilon}} (h_2^{\beta - 1} - h_0^{\beta - 1}) dG + \int_{\Phi_{emp}} \Delta V dG
\]

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

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

\[
NP = \frac{1}{r + \delta} [(1 + \eta)w_2 - a^{\beta - 1} h_2^{\beta - 1} - rV_{n}^{OT}]^\alpha \left[ \theta h_2^\beta - (1 + \eta)w_2 + F_S - C_{1[S=1,w<\bar{w}]} \right]^{1-\alpha}
\]

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

\[
NP_{corner} = \frac{1}{r + \delta} [\bar{w} - a^{\beta - 1} h_2^{\beta - 1} - rV_{n}^{OT}]^\alpha \left[ \theta h_2^\beta - \bar{w} + F \right]^{1-\alpha}
\]

From the first order condition that determines $h_2$, I know that the optimal hours with monitoring costs approaches the optimal hours in the baseline case as the baseline income approaches the overtime threshold (i.e. $\lim_{w_0 \to \bar{w}} h_2 = h_0$). Assuming $V_{n}^{OT} = V_{n}$, this implies that $NP_{corner} \to NP_0$ as $w_0 \to \bar{w}$, where $NP_0$ is the Nash product in the baseline case. Since $NP_0 \geq NP$ for every $(\theta, F, a)$, it is the case that for each $(\theta, F, a)$, there exists a $\epsilon$ such that
Given the Nash bargaining solutions, do more or fewer matches get accepted and become employment relative to the benchmark case without overtime? Recall that in the benchmark case, given a \((a, F)\), all matches with quality greater than or equal to \(\theta^*_0(a, F)\) are accepted. Furthermore, the total surplus at \(\theta^*_0\) equaled zero:

\[
\theta^*_0 h_0^\beta + F \cdot \text{sgn}(S_0) - a^{-\frac{1}{\epsilon}} \frac{h_0^{1+\frac{1}{\epsilon}}}{1 + \frac{1}{\epsilon}} - r V_n = 0
\]

where \(h_0 = \left(a^{-\frac{1}{\beta}} \theta^*_0\right)^{\frac{1+\frac{1}{\epsilon}}{1-\beta}}\) and \(S_0\) maximizes the surplus. Given parameters \((\theta^*_0, a, F)\) such that \(S_0 = 1\) and \(w < \bar{w}\), consider how the total surplus with overtime and monitoring costs \(TS_{OT}\) compare to the total surplus in the benchmark case \((TS_0 = 0)\):

1. If the job is reclassified, then \(TS_{OT} = TS_0 - 2F - r(V_{OT} - V_n)\)
2. If the job remains salaried but not bunched, then \(TS_{OT} = TS_0 - C - r(V_{OT} - V_n)\)
3. If the job is bunched, then \(TS_{OT} = TS_0 + \theta^*_0 (h_0^\beta - h_0^\beta) - a^{-\frac{1}{\epsilon}} \left(\frac{h_0^{1+\frac{1}{\epsilon}}}{1+\frac{1}{\epsilon}} - \frac{h_0^{1+\frac{1}{\epsilon}}}{1+\frac{1}{\epsilon}}\right) - r(V_{OT} - V_n)\)

If \(r(V_{OT} - V_n) \geq 0\), then it must be that \(TS > TS_0\). A negative match surplus implies that if the job is agreed upon, then either the firm will receive negative profits or the value of searching exceeds the value of the job. Thus, jobs with negative surplus are not accepted. Since the total surplus is monotonically increasing with respect to \(\theta\), for each \((a, F)\), there exists a \(\hat{\theta} > \theta^*_0\) such that matches are accepted if and only if \(\theta \geq \hat{\theta}\).

### Case 3: Downward Nominal Wage Rigidity

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

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

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

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

In contrast to recent methods developed in the search literature to generate wage rigidity (see Rogerson and Shimer (2011) for review), I abstract from modeling the cause of wage rigidity and focus specifically on its effects by exogenously imposing that \(\frac{w}{n} \geq \frac{w_0}{h_0}\) for hourly workers and \(w \geq w_0\) for salaried workers.
(w_3, h_3, S_3) = \arg \max_{(w, h, S)} [V_e(w, h) - V_n]^\alpha \left[ \frac{\theta h^\beta - (1 + \eta_{(w, h, S)})w + F \cdot \text{sgn}(S)}{r + \delta} \right]^{1-\alpha}

where

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

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

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

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

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

$$h_3 = \left( a^{\frac{1}{\epsilon}} \theta \right)^{1+\frac{1}{\epsilon}-\beta}$$

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

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

$$FOC_h = \alpha \left( 1.5 \frac{w_0}{h_0} - a^{-\frac{1}{\epsilon}} h_3^\frac{1}{\epsilon} \right) \left[ \theta h_3^\beta - (1.5 - \frac{20}{h}) \frac{w_0}{h_0} h - F \right]$$

$$+ (1 - \alpha) (\theta \beta h_3^{\beta-1} - 1.5 \frac{w_0}{h_0}) \left[ (1.5 - \frac{20}{h}) \frac{w_0}{h_0} h - a^{-\frac{1}{\epsilon}} \frac{h_3^{1+\frac{1}{\epsilon}}}{1 + \frac{1}{\epsilon}} - r V_n \right]$$

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

$$FOC_h|_{h=h_0} = (1.5 \frac{w_0}{h_0} - \theta \beta h_0^{\beta-1}) \left( \frac{20}{h_0} - 0.5 \right) \frac{w_0}{h_0} \frac{h_0}{h}$$

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

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

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

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

E.1 Overtime Pay

In this subsection, I present the procedure I use to determine each individual’s overtime pay from the “OT earnings” variable and its corresponding hours, when available. There are two challenges to inferring workers’ overtime pay from the ADP data. First, firms are not required to input a value into the “OT earnings” field. Although the ADP data contains four separate earnings variables and four corresponding hours variables, each capturing a different component of gross compensation, firms are only required to report employees’ gross pay and standard rate of pay. Thus, it is uncertain whether a missing value for overtime earnings means that the firm does not record the value or the worker did not receive any overtime pay.

To test how often firms separately record workers’ overtime pay, I compare the probability that a worker receives overtime pay in the ADP data to the probability that a worker works overtime in the Current Population Survey (CPS). In the ADP data, I find that the overtime earnings variable is non-zero for 45% of hourly workers and 3.5% of salaried workers in April 2016. For the same month, only 19% of hourly workers in the CPS report working over 40 hours in the previous week, and 15% report usual weekly hours exceeding 40. Assuming that 15% of hourly employees always work overtime, and 4% work overtime one week per month, I would expect around 31% of hourly employees to receive positive overtime compensation per month. Given that this is even smaller than the probability of overtime pay in ADP, it is likely that most firms separately record overtime pay from gross pay.

The second challenge with measuring workers’ overtime pay is that the type of compensation included into the “OT earnings” variable is at the discretion of the firm. Thus, some employers may use the variable to record other forms of compensation than overtime pay. I impute overtime pay following the methodology described by Grigsby et al. (2019). 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. \( \frac{\text{OT earnings}}{\text{OT hours} \times \text{wage}} \)), where a salaried worker’s “wage” for overtime purposes is defined by the Department of Labor as \( \frac{\text{weekly base pay}}{40} \). I consider the “OT earnings” variable to represent true overtime pay if the implied overtime premium is less than or equal to 2. I find that the distribution of the implied overtime premium exhibits significant bunching at 1.5, and 2, indicating that the variable usually captures true overtime earnings. Among workers with non-missing ”OT earnings”, 75% of hourly workers and 79% of salaried workers have implied overtime premiums within 1.4-1.6 and 1.9-2.1.

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

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

Notes: Each graph shows the probability that salaried workers receive non-zero overtime pay in the month, as a function of their weekly base pay. The sample in figure (a) is restricted to salaried workers not living California, New York or Alaska, in the month April 2016. The sample in figure (b) is restricted to salaried workers in the same states as figure (a) in December 2016. The sample in figure (c) is restricted to salaried workers in California in April 2016. The sample in figure (d) is restricted to salaried workers in New York in April 2016.
E.2 Computing Weekly Measure of Income

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

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

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

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

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

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

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

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

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

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

![Monthly Gross Pay](image)

![Normalized Weekly Gross Pay](image)

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

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

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

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

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

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

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

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

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

Notes: Panel (a) shows distribution of the difference between imputed and actual number of paychecks per month, for all worker-months in 2016 where the worker is paid biweekly. Panel (b) shows a similar distribution for workers who are paid weekly.
Appendix F. Derivation of Estimator in Equation 3

If the coefficients in equation 2 satisfy

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

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

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

$$\hat{\beta}_{jk} = (\bar{N}_{jk,Dec,t} - \bar{N}_{jk,Apr,t}) - \hat{\gamma}_1 (\bar{N}_{jk,Dec,t-1} - \bar{N}_{jk,Apr,t-1}) - \hat{\gamma}_0$$

$$= \Delta \bar{N}_{jkt} - \hat{\gamma}_1 \Delta \bar{N}_{jkt-1} - \hat{\gamma}_0$$

where $\bar{N}_{jkm}$ is the average $N_{ijkm}$ 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 $k \geq k^*$,

$$\bar{N}_{jk,Dec,t} = \bar{N}_{jk,Apr,t} + \alpha_{jkt}$$

$$\implies \Delta \bar{N}_{jkt} = \alpha_{jkt}$$

$$\Rightarrow \Delta \bar{N}_{jkt} = \gamma_1 \alpha_{jkt-1} + \gamma_0$$

$$\Rightarrow \Delta \bar{N}_{jkt} = \gamma_1 \Delta \bar{N}_{jkt-1} + \gamma_0$$

This implies that I can estimate $\gamma_1$ and $\gamma_0$ by regressing $\Delta \bar{N}_{sal,kt}$ on $\Delta \bar{N}_{sal,k,t-1}$ using all bins $k \geq k^*$. Given the $\gamma$’s, I can then predict the $\alpha_{jkt}$’s for both salaried and hourly workers with bins $k < k^*$.

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

From equation 2 I estimate the $\beta_{jk}$’s as the difference between $\Delta \bar{N}_{jkt}$ and $\hat{\alpha}_{jkt}$. ■
Appendix G. Analysis using the Current Population Survey

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

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

Notes: This figure shows the frequency distribution of respondents’ usual weekly earnings in the CPS. The sample is restricted to individuals who are not paid an hourly wage, and earn between $851 and $950 per week. The dotted vertical red line is at $913 per week.
To begin, I plot the frequency distributions of weekly earnings of salaried workers for each month between May 2016 and April 2017 in figure G.1. The number of respondents earning within a dollar of $913 per week experiences a visibly small jump between November and December 2016 that persists after December. In the year prior to December 2016, 0.09% of salaried workers report earning within a dollar of $913, whereas in the year after, 0.37% report earning within that interval. However, the “bunching” at the threshold is considerably smaller than the other spikes in the distribution.

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

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

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

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

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

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

Notes: Panel (a) shows the frequency distribution of salaried workers’ weekly earnings in $40 bins, reported in the CPS. The distribution in the pre-period is constructed using all respondents between December 2015 and April 2016. The post-period is constructed using all respondents between December 2016 and April 2015. The “2016” line in panel (b) shows the difference between the pre and post distributions in panel (a). The “2015” line shows the difference between the pre-distribution and the analogous distribution of salaried workers from December 2014 and April 2015.
Appendix Figure G.3: Difference in Difference of Probability of Being Paid Hourly Using Repeated Cross Sections, Using CPS

Notes: Panel (a) shows the probability that an individual in the CPS is paid an hourly wage for each month between January 2010 and September 2019, conditional on weekly earnings. The two dotted vertical lines are at May 2016 and December 2016, respectively. Panel (b) shows the difference in difference estimates where I compare workers earnings earning between $400 and $1000 per week to workers earning between $1000 and $1200 per week.
Appendix Figure G.4: Annual Change in Hourly/Salaried Status, Conditional on Initially Earning Between $455 and $913 per Week as a Salaried Worker

(a) Hourly Status, by Cohort

(b) Change in Hourly Status, by Earnings

(c) Difference-in-Difference of Hourly Status

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