THE LABOR MARKET EFFECTS OF EXPANDING OVERTIME COVERAGE*

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Abstract

This paper examines the labor market effects of overtime coverage in the United States, where salaried workers are covered for overtime if their base pay falls below a legislated salary threshold. Using an event-study design with administrative payroll data from ADP and state-level threshold changes from 2014-2021, I find evidence of frictions in hours reallocation and wage adjustments. Contrary to the historical intent of overtime, firms do not increase employment by substituting more workers for fewer hours. Firms also do not offset the costs of overtime by lowering workers' base pays, as predicted by models of compensating differentials. Instead, employers raised salaries above the threshold to keep workers exempt from overtime. Taken together, these findings suggest that overtime coverage increases workers' earnings without negatively impacting hours or employment.

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I Introduction

Overtime is a core feature of labor regulations in nearly every OECD country (OECD, 2021). In the US, it is the primary way by which the government regulates workers' hours. More than half of all workers are legally covered for overtime, and there is ongoing debate to expand overtime eligibility to additional workers. Proponents of expanding overtime argue that it can reduce workers' hours, increase earnings, and potentially even lower unemployment by reallocating hours across workers. Despite its widespread adoption and modern policy relevance, there is scarce research on the labor market effects of overtime coverage. Most studies on the topic have focused on its impact on workers' hours, but little is known about its other effects. In fact, a recent review by Brown and Hamermesh (2019) concludes that "no study presents estimates of [the] effects [of overtime coverage] on employment, and none offers evidence on all outcomes: [wages, earnings, and hours]".

My paper studies the impact of recent expansions in overtime coverage for salaried workers in the United States. Despite the long history of overtime regulations, research on its effects have been hindered by a shortage of policy changes and a lack of data to accurately measure their impacts. While overtime legislation has been central to US employment standards for over 80 years, expansions in overtime coverage are rare and often coincide with changes in other labor laws. Apart from a shortage of clean variation, few datasets in the US distinguish between workers' base pay and overtime pay, and those that do often lack the sample size or panel structure to precisely estimate changes in aggregate employment.

I overcome prior empirical limitations by exploiting recent state expansions in overtime eligibility for low-income salaried employees. Unlike hourly workers, overtime eligibility for salaried employees is determined by their base pay relative to a legislated "overtime exemption threshold." All workers who earn below this salary threshold are guaranteed overtime protection, whereas white-collared salaried workers who earn above it are legally exempt. Between 2014 and 2021, there were two federal rule changes and nineteen state reforms that raised the overtime exemption threshold.[1] Leveraging anonymous administrative payroll data covering over one tenth of the US labor force, I implement an event-study difference-in-difference design that compares the evolution of employment and earnings in states that increased their thresholds to those that did not.

I report three sets of results. First, on the exact month that the overtime exemption threshold increases, the number of salaried workers earning between the old and new thresh-

[1] A similar policy is currently being debated in federal court with the Department of Labor attempting to raise the threshold to $58,656 per year - a policy that is expected to impact 3.6 million salaried workers (DOL Wage and Hour Division, 2023).

olds falls by 21%. Three responses explain this dramatic change in the pay distribution for salaried jobs. To start, three quarters of the decrease in jobs below the new threshold is accounted for by an increase in jobs right above it. This bunching in the distribution reflects firms' decision to raise workers' base pay above the new cutoff to keep them exempt from overtime. Next, half of the remaining missing mass were jobs reclassified from salaried to hourly. Individuals in these jobs no longer receive a fixed salary, but are paid per hour of labor and qualify for overtime protection. Lastly, the remaining missing mass was lost due to a statistically insignificant reduction in employment. Firms' decision to raise workers' base pays above the overtime exemption threshold, rather than increase employment, suggests that it is costly for employers to substitute workers for hours.

Second, the expansion in overtime protection for salaried workers had positive spillover effects onto hourly jobs. Similar to the impact on the distribution of salaried workers, I find that firms raised the wages of hourly workers such that their weekly base pay, defined as 40 times their wage, is bunched right above the overtime exemption threshold. As a placebo check, I show that the threshold changes had no effect on either salaried or hourly workers outside the pay interval directly targeted by the policies. Despite the pay increase, I also find no decrease in employment among hourly workers. The spillover effects suggest that firms face internal pay equity constraints that prevent them from giving individualized pay increases.

Third, workers targeted by the policy experience a 1.3% increase in their labor income. Although the policy expanded overtime coverage, I find that the majority of the rise in earnings is attributed to an increase in base pay, rather than an increase in overtime compensation. Since only about 16% of affected workers actually get bunched above the overtime exemption threshold, the benefit of the reform is unevenly distributed across workers and is concentrated primarily among a small group of bunched workers. Under the assumption that only bunched workers receive an increase in base pay, I show that the salaries of this group increased by approximately 8.3%. At the same time, using data from the Current Population Survey, I find zero impact of raising the overtime threshold on workers' hours. Taken together, the evidence suggests that expansions in overtime coverage benefit workers by raising average salaries with no significant impact on either hours or employment.

My paper contributes to three areas of research. First, my paper adds to a literature on the impact of overtime regulations. Historically, overtime was introduced during the Great Depression to encourage firms to create new jobs by substituting workers for hours (Ehrenberg, 1971). Given limitations in variation and data, tests of this work-sharing hypothesis in the US have focused solely on its effects on workers' hours (Costa, 2000; Hamermesh and Trejo, 2000; Johnson, 2003; Trejo, 2003; Goff, 2020). Outside the US, rather than expansions in coverage, papers have instead examined policies that regulate the minimum and maximum hours of a standard workweek (Hunt, 1999; Crépon and Kramarz, 2002; Skuterud, 2007; Chemin and Wasmer, 2009; Sanchez, 2013; Carry, 2022). In general, studies find that such policies tend to reduce total working hours without increasing employment, suggesting that firms cannot flexibly reallocated hours across workers. To my knowledge, my paper provides the first causal estimates of the employment and income effects of overtime coverage. Moreover, I study overtime coverage for *salaried* workers, a group for which there are no existing estimates but is at the center of modern policy debates. Consistent with existing studies, I find evidence against the hypothesis that overtime coverage leads to work-sharing.

Second, my paper contributes to the literature on compensating differentials and work amenities (Lavetti, 2023). In contrast to the work-sharing theory of overtime, a competing model argues that firms would fully offset the costs of overtime coverage by lowering base wages, leading to no real effects on hours, earnings, or employment (Trejo, 1991). Due to a lack of policy changes, prior support for the compensating differentials model have relied solely on a negative cross-sectional correlation between wages and overtime hours across workers (Trejo, 1991; Barkume, 2010). In contrast, using the changes in overtime eligibility for salaried workers as a natural experiment, I find no evidence that firms cut salaries to negate the effects of overtime. Relatedly, while many studies in the compensating differentials literature have measured workers' willingness to pay for alternative work arrangements such as flexible scheduling or long workweeks (Mas and Pallais, 2017; Maestas et al., 2023), less is known about the costs of such arrangements to firms. Government surveys tend to underestimate the costs of overtime regulations since they are calculated after employers have already adjusted hours and salaries to minimize overtime expenses (DOL, 2019). My paper provides the first estimate of the ex-ante cost of overtime by documenting how much firms are willing to raise workers' base salaries to avoid overtime requirements.

Third, I contribute to a broader literature on wage and hour laws (Brown and Hamermesh, 2019). Rather than implement work-sharing or compensating wage differentials, firms respond to increases in the overtime exemption threshold in a similar way to how they respond to the minimum wage - by bunching workers' salaries above the legislated cutoff. The literature on the minimum wage generally finds that such pay increases tend to have only small employment effects (Cengiz et al., 2019; Harasztosi and Lindner, 2019; Derenoncourt and Montialoux, 2021; Dustmann et al., 2022).[2] While there is a large literature on how employment responds to mandated pay increases at the bottom of the income distribution, less is known about the effect of policy-induced pay increases on middle-income workers. My paper provides new evidence that exogenous pay increases in the middle of the income dis-

[2] See Dube and Lindner (2024) for a recent review.

tribution likewise have no significant employment effects, consistent with growing evidence of monopsony power in the labor market (Azar et al., 2024).

The remainder of this paper is organized as follows. In section II, I explain the institutional details governing U.S. overtime regulations and the specific policies that expanded coverage for salaried workers. Section III outlines the predictions of the competing models of overtime. In section IV, I describe the administrative payroll data from ADP. Sections V reports my main results on the aggregate employment and income effects of overtime. In section VI, I discuss the policy implications of raising the overtime exemption threshold for salaried workers. I conclude in section VII by discussing the implications of my findings and areas for future research.

II Federal and State Overtime Regulation

The Fair Labor Standards Act (FLSA) requires employers to pay workers one and a half times their regular rate of pay for each hour worked above 40 in a week.[3] While the overtime premium applies to nearly all hourly workers in the U.S., the FLSA exempts a large group of salaried workers who are considered executive, administrative, or professional employees. To exempt a salaried employee, a firm must show that the worker performs primarily white-collared duties, and earns a salary equal to or greater than the "overtime exemption threshold" set by the Department of Labor (DOL).[4] Since the overtime exemption threshold is not adjusted for inflation, the share of salaried workers earning less than the threshold, and thereby guaranteed overtime coverage, fell from over 50% in 1975 to less than 10% in 2016 (see Appendix Figure A.1).[5] In an effort to restore overtime protection to low-income salaried workers, such as managers at fast food restaurants and retail stores, Departments of Labor at both the federal and state levels have recently raised their overtime exemption thresholds. My paper uses these rule changes in the exemption threshold as natural experiments to study the effects of overtime coverage.

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

[4] 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.

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

At the federal level, there have been three major policy proposals to increase the FLSA's overtime exemption threshold. First, the Department of Labor announced in May 2016 that it would more than double the federal exemption threshold from \$455 per week (\$23,660 per year) to \$913 per week (\$47,476 per year) effective December 1, 2016. The new rule would effectively raise the threshold from the 10th percentile of the salaried income distribution to the 35th percentile. However, to employers' surprise, a federal judge imposed an injunction on the policy on November 22, 2016, stating that such a large increase in the threshold oversteps the power of the DOL and requires Congressional approval. Following the retraction of the 2016 rule change, the DOL debated a smaller increase to the FLSA overtime exemption threshold and eventually raised the threshold to \$684 per week on January 1, 2020. More recently, the federal Department of Labor announced in August 2023 that it plans to increase the threshold to \$1,128 per week (\$58,656 per year) in January 2025. However, this latest policy is again facing challenges in court, and the DOL is currently appealing a judge's decision to halt the rule change. Given that the federal policies affect all states simultaneously, I drop these reforms from my analysis.[6]

My analysis uses an event-study design to evaluate 19 state-level increases to the overtime exemption threshold between 2014 and 2021.[7] Similar to the minimum wage, multiple states impose their own exemption thresholds that exceed the one set by the FLSA. I present in Figure 1 all state and federal thresholds from 2005 to 2021, along with the invalidated proposal in 2016.[8] My event-study analysis uses variation from six states: California, New York, Colorado, Washington, Alaska, and Maine. With the exception of Colorado, all six states define their overtime exemption thresholds as a multiple of their minimum wage. Thus, each time these states raise their minimum wage, the overtime exemption thresholds simultaneously increases following a known formula. While these policies happen concurrently, they target very different population groups. For example, in New York and California, which are the states with the most policy variation, the overtime exemption threshold is set at 75 and 80 times the minimum wage, respectively. Thus, a minimum wage worker would have to

[6] In a separate paper, I use the retraction of the 2016 reform as a natural experiment to study the interaction between morale concerns and wage rigidity. Although the 2016 federal proposal was never binding, I nevertheless find evidence that firms raised workers' salaries above the overtime exemption threshold, and this impact persisted even a year after the court injunction (Quach, 2020).

[7] Although the data ends in 2021, California, Colorado, and Washington state have continued to increase their overtime exemption thresholds. For instance, Washington passed a law to gradually raise its threshold to \$1,780 per week by 2028, which is equivalent to a salary of \$92,560 per year.

[8] I exclude from my event study the five most recent rule changes in Alaska, which cumulatively increased the exemption threshold by only \$47 to adjust for inflation. I also exclude the January 2014 event in New York due to missing data.

work at least 75 hours in New York to earn the overtime exemption threshold. I will show empirically that the overtime exemption threshold is high enough such that the segment of the income distribution affected by changes in the threshold does not interact with changes in the minimum wage, even after accounting for potential spillovers.

III Theoretical Predictions

There are multiple ways in which firms can respond to increases in the overtime exemption threshold. In this section, I summarize the predictions of the two canonical models of overtime in the literature: a labor demand model and a compensating differentials model.

Historically, when the FLSA was first passed during the Great Depression, policymakers had intended for the policy to create jobs. The intuition was that by making long workweeks more expensive, employers would reduce hours and hire more workers. This work-sharing hypothesis is encapsulated by the labor demand model of overtime outlined by Ehrenberg (1971). Under this framework, firms take wages w as given, and choose employment n and hours h to solve the following profit maximization problem:

$$\max_{(n,h)} f(n,h) - wh - pw(h-40)1[h > 40]$$

where $f(n,h)$ is production, p is the overtime premium, and $1[h > 40]$ is an indicator for whether hours exceed 40.

Overtime coverage, modeled by an increase in p from 0 to 1.5, leads to two effects. First is a substitution effect - since hours have become more expensive, firms would want to reduce hours and increase employment. Second is a scale effect - since an input of production has become more expensive, firms would want to cut output, leading to both fewer hours and employment. Thus, while the intent of the policy is to create new jobs, it is theoretically ambiguous whether such work-sharing would occur. Ultimately, it depends on the suggestibility of hours across workers. If hours and workers are perfectly substitutable, then overtime coverage would be costless to the firm.[9] If there is imperfect sustainability, then it is unclear what happens to employment. Moreover, specific to my context, if it is costly for firms to adjust hours and employment in response to overtime coverage, they may simply choose to raise salaries above the overtime exemption threshold.

One limitation of the labor demand model is that it treats wages as exogenous and fixed. Incorporating labor supply responses has been a challenge in the literature since there is no equilibrium level of hours that satisfies both the firm and workers' first order conditions.

[9]For example, a firm can replace two 60 hours workers with three 40 hour workers.

While employers want to bunch workers hours at 40, no worker would be indifferent at exactly 40 hours per week. To capture both labor demand and labor supply incentives, Trejo (1991) modeled the labor market in a compensating differentials framework where an equilibrium consists of a bundle of hours and wages. This model predicts that base wages would fall in equilibrium such that with the addition of overtime, workers' net earnings remain unchanged.[10] As a result, overtime coverage would have no effect on hours, employment, or gross income. The same predictions hold if firms and workers simultaneously Nash bargain over wages and hours, or even if employers are monopsonists that can choose both outcomes. Essentially, if earnings Y^* and hours h^* were an equilibrium prior to the policy, and they are still achievable afterwards, then many models predict that the labor market would converge back to the same equilibrium.

I summarize the testable predictions of the two models in Table I. In both cases, an expansion in coverage mechanically increases overtime pay. In response to this, if wages and hours are determined jointly, then base pay would decrease to fully offset the costs of overtime. In contrast, if firms can only adjust wages upwards or reallocate hours and employment, then I expect an increase in the overtime exemption threshold to lead to a bunching mass at the threshold coming from jobs that would otherwise be paid right below it, bunching in the distribution of hours, and ambiguous employment effects. In addition, if one of the reasons that employers classify some workers as salaried to begin with is to avoid overtime pay, then I would also expect to see some workers being reclassified from salaried to hourly.

IV ADP Data

I use anonymized monthly administrative payroll data provided by ADP LLC, a global provider of human resource services that helps employers manage their payroll, taxes, and benefits. Their matched employer-employee panel allows me to observe monthly aggregates of anonymous individual paycheck information between May 2008 and July 2021. The data contains detailed information on employee's salaried/hourly status, income, hours, pay frequency (i.e. weekly, bi-weekly, or monthly), and state of employment for over a tenth of the U.S. labor force.[11] Since ADP manages both W-2 and 1099-MISC tax forms for employers,

[10] To illustrate, suppose prior to the rule change, a job was working 55 hours per week for a wage of \$15 per hour. The compensating differentials model predicts that overtime coverage would reduce wages to \$13.2 per hour, so that gross earnings are constant (i.e. $15 \cdot 55 = 13.2 \cdot 40 + 1.5 \cdot 13.2 \cdot (55 - 40)$).

[11] 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

the data contains data on both regular employees and independent contractors. I aggregate across both types of workers to estimate the effect of overtime coverage on total employment and average earnings across all workers.

A significant advantage of the ADP data over traditional survey data and other administrative datasets is that it precisely records each worker's standard rate of pay, separate from other forms of compensation. This variable enables me to precisely calculate the measure of weekly base pay that the DOL uses to determine employees' exemption status. Following the DOL's guidelines, I compute salaried workers' weekly base pay as the ratio between their salary per pay-period and the number of weeks per per-period.[12] As a simple benchmark to compare the rate of pay for workers who transition between salaried and hourly status, I define the weekly base pay of hourly jobs as 40 times their wage.

In addition to workers' pay rate, the data also records employees' monthly overtime pay and monthly gross 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 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. Appendix B provides more detail on how I construct the two variables. For my analysis, I restrict the sample to a balanced panel of employers because the entry and exit of firms in the data reflect both real business formations and the decision of existing firms to partner with ADP.

While the data also records workers' total number of hours worked per month, employers only accurately track this information for hourly employees. Since employers are not required to record the hours of salaried workers who are not covered for overtime, this limitation is likely endemic to all administrative employer datasets, including Census or IRS data. To overcome this limitation, I will supplement my analysis with survey data from the CPS Outgoing Rotation Group, which asks workers their weekly earnings, usual weekly hours, and the number of hours they worked in the previous week. I distinguish between salaried and hourly workers in the CPS ORG based on respondents' answer to whether or not they are paid by the hour.

In Appendix C, I explore the characteristics of firms affected by the changes in the overtime exemption threshold.[13] I show three descriptive results. First, I find that these ex-

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

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

[13] For a detailed analysis of the representativeness of the ADP data in general, refer to Grigsby

pansions in overtime coverage affect workers across all industries, but primarily impacts large firms. Employers with at least one salaried worker in the interval of base pay targeted by the rule changes are about twice as large as the average firm in the sample. Second, plotting the distribution of weekly base pay, I find evidence of employers bunching workers' salaries right at the overtime exemption threshold, thereby keeping them exempt from overtime. Third, the probability that salaried workers receive overtime pay is highly responsive to the overtime exemption threshold. In California and New York, salaried employees earning right below the threshold have a 10-20% probability of earning overtime pay. However, the probability of receiving OT pay discontinuously drops by half for workers right above the cutoff, suggesting that employers are complying with the overtime regulations. In the next section, I use the state policy changes in the thresholds to identify the causal impact of raising the overtime exemption threshold on employment, earnings, hours, and salaried/hourly classification.

V Results

V.a Employment, Bunching, and Reclassification Effects

Empirical Strategy.

To start, I estimate the impact of the labor reforms on the distribution of weekly base pay. My analysis uses an event-study difference-in-difference design that leverages the 19 state rule changes. Intuitively, I compare the evolution of employment and earnings between states that raised their overtime exemption thresholds to states that did not. Formally, I estimate the following stacked difference-in-difference regression:

$$n_{jstkv} = \sum_{\substack{t=-6 \\ t\neq -1}}^{5} \sum_{k=-6}^{15} \beta_{kt} \cdot I_{stk} + \alpha_{skv} + \delta_{ktv} + \varepsilon_{jstkv} \tag{1}$$

where n_{jstkv} is the number of workers employed in firm j from state s at event-time t, with base pay in bin k for event v. I define each bin as a \$40 interval of weekly base pay, normalized to 0 at the new threshold. The treatment dummy I_{stk} equals 1 for the treatment state at event time t and bin k. I omit the month before the policy change as a reference period. My benchmark specification includes state-bin-event (α_{skv}) and month-bin-event (δ_{tkv}) fixed effects to control for state-specific differences in the base pay distribution and nationwide changes in inequality, respectively. Standard errors are clustered by state.

et al. (2021). They find that while the data closely matches the demographics of workers in the Current Population Survey, it under-represents employment in firms with over 5000 employees relative to the Business Dynamic Statistics.

There are three features of the regression that differ from traditional difference-in-difference designs. First, rather than aggregating employment at the firm level, I follow recent advancements in the minimum wage literature and measure employment at the firm-bin level (Cengiz et al., 2019; Derenoncourt and Montialoux, 2019; Harasztosi and Lindner, 2019; Gopalan et al., 2020). The extra level of disaggregation allows me to estimate changes in employment along the entire income distribution, which provides two useful advantages over a single aggregate statistic: 1) I am able to test whether employers bunch workers' salaries right above the overtime exemption threshold, and 2) I can use jobs at the right tail of the income distribution, where I expect no effect of the policy, as a placebo check of the parallel trends assumption.

Second, instead of a two-way fixed effects model, I estimate a stacked difference-in-difference model by creating a distinct dataset for each of the 19 state reforms. Each dataset comprises of the treated state along with all the control states that have never increased their overtime exemption threshold yet up to the that point in time. I then append the 19 datasets together to estimate equation 1. By organizing the data in this way and interacting each of the fixed-effects with an event indicator, equation 1 is equivalent to estimating 19 individual differences-in-differences and then taking a weighted average of the treatment effects to compute β_{kt}. The advantage of estimating a stacked difference-in-difference relative to a traditional two-way fixed-effects model is that I avoid biasing my estimates from heterogeneous treatment effects over varying time periods (Goodman-Bacon, 2021; Baker et al., 2022; Roth et al., 2023; Freedman et al., 2023).

Third, for each event, I rescale the distribution of base pay for each state in the control group to exactly match the distribution in the treatment group in the month before the threshold change. Since employment is measured in levels rather than logs, even if firms grow at the same rate in both groups, the state with the largest population will nevertheless gain more jobs simply because it had higher baseline employment levels. To account for this, I apply the following transformation to the observations in the control group:

$$\tilde{n}_{jstkv} = n_{jstkv} \cdot \frac{\bar{n}_{s=treat,t=-1,k,v}}{\bar{n}_{s,t=-1,k,v}}$$

where $\bar{n}$ is the average employment across all firms within a state. Given that the transformation is only applied to the control states and uses baseline characteristics, it is uncorrelated with the change in employment levels post-reform and will therefore not bias my results.

I estimate equation 1 separately for salaried and hourly workers. The aggregate change in employment is simply the sum of the β_k estimates across both class of workers. For interpretability, unless otherwise noted, I scale all estimates by the number of "affected salaried workers", which I define as the number of salaried workers between the old and new thresholds in the baseline month.

My identification strategy relies on the assumption that absent the state threshold changes, the distribution of base pay in the treated states would have evolved the same as the control states. I test my identification assumption in two ways. First, I show that treated and control states do not exhibit differential trends prior to the reform. Second, I show that treated and control states exhibit similar trends *post-reform* for workers paid well above the overtime exemption threshold who are unlikely to be affected by the policy. Given these two validation checks and the high frequency nature of the data, my estimates would only be biased if there is a shock that occurs at precisely the month of the policy change, in precisely the treated states, and affects solely jobs within the interval targeted by the overtime threshold, but not jobs higher in the income distribution.

Estimates of the Employment Effect Along the Distribution of Base Pay.

Figure 2 plots the estimates of the treatment effect, β_k, separately for the distribution of salaried and hourly workers. Panel (a) shows that immediately in the month a new threshold goes into effect, there is a decrease in the number of salaried employees below the threshold and a spike in workers right above it.[14] As a placebo check, I find no effect on any bins of base pay above the new threshold. To further validate my empirical strategy, panel (b) plots the change in the number of salaried workers paid below and above the new threshold over time. Examining the figure from left to right, three features stand out. First, there is little evidence of a pre-trend prior to the policy change, indicating that the parallel trends assumption holds. Second, there is a sharp drop in the number of jobs below the threshold and a sharp increase in the number of jobs above it at precisely the month of the rule change, consistent with the bunching from the cross-sectional estimates. Third, the magnitude of the decrease in employment below the threshold is visibly larger than the increase in employment above it.

Plotting analogous figures for hourly workers, I find that the base pay distribution for hourly jobs responded in a qualitatively similar fashion. Panel (c) of Figure 2 shows that raising the state overtime exemption threshold cut hourly jobs earning between the old and new thresholds, and increased the number of jobs above it. In panel (d), I confirm that the bunching effect is statistically significant by plotting the effect on hourly employment over time. Mirroring the estimates for the salaried distribution, I find no significant pre-trends, followed by a sharp divergence in the number of jobs below and above the threshold at exactly

[14] The largest impact occurs within -80 to 40 above the new threshold simply because the majority (15 out of 19) of the policy changes raised the overtime exemption threshold by at most $80 weekly base pay.

the month of the rule change. However, the number of jobs below the threshold appear to trend upwards in the months following the reform. The bunching of hourly employees who were unaffected by the policy is consistent with growing evidence of relative pay concerns within firms (Card et al., 2012; Dube et al., 2019; Quach, 2020).

Contrary to the compensating differentials model of overtime, I find no evidence that employers reduced workers' base pay to offset the costs of overtime. The majority of the policy reforms only increased the overtime exemption threshold by $80 of weekly base pay, and yet I find no increase in the number of salaried workers even $240 below the new threshold. For hourly workers, I do see a small change in the number of jobs earning in the left tail of the distribution in Figure 2, but this can be attributed to changes in the minimum wage. In particular, if a state increased both its overtime exemption threshold and minimum wage, then spillover effects of the minimum wage may be captured in the left tail of the distribution. To avoid any possible contamination with the minimum wage and other differences in state trends, I only aggregate the estimates from -$160 to $80 of normalized base pay when computing the employment effects.

In the appendix, I present two pieces of evidence that my preferred estimate of the employment effect is not confounded by changes in the minimum wage. First, appendix figure A.3 shows the distributional impacts of the overtime rule after omitting all states with a new minimum wage within $200 of the old overtime exemption threshold.[15] As expected, I no longer observe a change in the left tail of the hourly workers' distribution. Nevertheless, the sum of my estimates are very similar to the earlier results with the full sample. Second, appendix figure A.4 extends the estimates further to the left and right of the pay distribution. Similar to the truncated results, I find no statistically significant impact at any point above the new overtime exemption threshold, suggesting that the treatment and control states are following similar post-trends after the reform. In contrast, there is clear bunching from the minimum wage in the lower end of the pay distribution, particularly for hourly jobs. While the effect of the minimum wage is multiple times larger than the impact of the overtime reforms, its impacts are concentrated primarily in the far left tail of the distribution, away from the bounds affected by the change in the overtime exemption threshold.

Table II summarizes my results for the employment effects separately for salaried and hourly jobs. The estimates in column 1 are computed from equation 1 and correspond to those presented earlier in figure 2. Panel (a) reports the change in the number of jobs along the income distribution for each affected salaried worker. I find that the number of salaried jobs in the affected interval of base pay falls by 20.9% (s.e. 6.1%). Among those jobs, 15.8

[15] This restriction excludes 5 events and is equivalent to dropping Colorado, Maine, and Washington from the data.

p.p. (s.e. 5.6) or about three-quarters of the missing mass can be accounted for by employers bunching salaried workers right above the new overtime exemption threshold. Similarly, I find that for each affected salaried worker, the policy reduces the number of hourly jobs paid between the old and new thresholds by 0.07 (s.e. 0.026) workers, all of which can be accounted for by bunching above the cutoff.

Panel (b) reports two measures of the aggregate employment effect based on the four estimates in panel (a). First, the sum of the above estimates imply that employment falls by 2.3 (s.e. 2.3) jobs for every 100 salaried workers earning between the old and new thresholds at baseline. However, while the number of affected salaried jobs is a useful denominator for thinking about the share of salaried workers that receive a raise above the threshold, it makes more sense to include hourly workers in the denominator when calculating a labor demand elasticity. There are at least three reasons for this distinction. First, it is clear from Figure 2 that the policy affected the wages of not only salaried workers, but also hourly ones. Second, the change in aggregate employment (i.e. the numerator in $\frac{\Delta\text{Employment}}{\text{Employment}_0}$) is calculated as not only the change in salaried employment, but also hourly. Lastly, previous studies in the minimum wage literature also consider both salaried and hourly workers as treated, so to make my results comparable, I use a similar denominator.

Given the above rationale, panel B also reports the change in employment as a percentage of "all affected workers", which I define to be the total number of salaried and hourly workers with base pays between the old and new thresholds. The increase in the overtime exemption threshold had no statistically significant impact on aggregate employment, and the 95% confidence bounds can rule out employment losses greater than 1.5% and employment gains above 0.5%. Thus, while the policy did not achieve one of its original intent of creating new jobs, it did succeed in increasing workers' salaries without significantly affecting employment.

To assess the robustness of my results, I report a series of alternative specifications that control for geographically localized shocks and variation in firm composition across states. In column (2), I show that the estimate of the employment effect is similar even if I compare states within the same Census division. This specification eliminates any bias from spatial shocks that target only specific regions of the U.S. (Dube et al., 2010). In column (3), I introduce firm-interacted fixed effects to compare only firms that operate in both the treated and control states. This stricter specification controls for differences in time-trends across states that may arise due to industry or even firm-specific shocks. While the sign of the employment effect flips, the estimates nevertheless suggests only minor changes in employment at most. In column (4), I restrict the sample to only policy changes where the old overtime exemption threshold is at least \$200 weekly base pay above the new minimum wage. The sample restriction thereby eliminates concerns regarding interactions between the two policies. Column (5) imposes all the restrictions and controls from columns (2)-(4). Lastly, column (6) estimates a traditional two-way fixed effects model where the outcome variable is simply the total number of jobs between -$160 and +$80 weekly base pay relative to the new threshold. Overall, the culmination of evidence suggests that the aggregate employment was small relative to the total number of affected workers.[16]

Decomposing the Changes in the Number of Workers.

The results thus far are consistent with the predictions of the labor demand model whereby firms set workers' hours. However, multiple questions remain. First, aside from the bunching effect, the model predicts that firms would also reclassify workers from salaried to hourly - did that happen? Second, it takes time for firms to adjust the stock of employment. Did firms instead change their hiring or separation rates?

To answer these questions, this section studies the impact of increasing the overtime exemption threshold on the flow of workers. Intuitively, changes in the stock of workers within a bin of base pay for a particular hourly/salaried classification can be decomposed into three margins of response: flows across bins within the same classification (i.e. wage growth), flows across hourly/salary status (i.e. reclassifications), and flows in/out of employment. I measure each of these flows directly using the employer-employee panel structure of the data, and use the number of such flows at the firm level as the outcome variable to equation 1.

Figure 3 plots the effect of the state policies on net employment flows (i.e. hires minus separations) and net reclassification flows. In panel (a), I find a decrease in employment flows among salaried workers paid between the old and new thresholds at precisely the month of the reform, and an increase in flows above the threshold. Interestingly, the change in employment flows does not persists over time. Turning to hourly jobs, the estimates in panel (b) are far noisier and exhibit a downward pre-trend. In either case, there does not appear to be a persistent change in employment in the month of the reform, nor afterward. The lack of an effect on salaried employment flows suggests that the decline in salaried jobs observed earlier in figure 2 is due to the reclassification of jobs from salaried to hourly.

To directly measure the reclassification effect, panels (c) and (d) plot the impact of the overtime reform on the flow of jobs out of and into the salaried distribution, respectively. Consistent with the labor demand model, I observe a sharp increase in the number of jobs being reclassified from salaried to hourly at precisely the month that the new overtime

[16]Since the stacked event study is a weighted sum of the individual difference-in-difference estimates, I also test whether the results are driven by any outlier events. Appendix figure 2 plots the difference-in-difference estimates separately for each policy change. I drop Maine from the figure since the estimates are very noisy given the small sample size in the state. While there appears to be one outlier in New York 2016 with a very large negative response, the other estimates tend to hover around 0, with no particular pattern towards a negative or positive employment effect.

exemption threshold goes into effect. This impact solely affects jobs paid below the threshold. On the other hand, there does not appear to be a large impact on the number of jobs being reclassified from hourly to salaried, which usually occurs following a promotion. However, jobs moving from hourly to salaried are now more likely to be paid above the overtime exemption threshold. Overall, I find clear evidence of a reclassification effect that was obscured when examining the stock of workers.

Table III summarizes the effect of raising the overtime exemption threshold on employment and reclassification flows. Given that the bulk of the flow responses occurred at precisely the month of the policy reform, I report the β_{tk} estimates from equation 1 for $t = 0$. All estimates are scaled by the number of affected salaried workers. In columns (1)-(3), I present the change in hires, separations, and net employment flows, respectively. The first row finds that there is a precisely estimated decrease in net employment flows to salaried jobs paid below the new overtime exemption threshold, with a confidence bound between -1 to -0.2 jobs per 100 affected salaried position. Half of the reduction is driven by a decrease in hires and half is from an increase in separations. However, the second row shows that about two-thirds of the drop in employment flows below the threshold can be account for by new jobs above it.

As for hourly jobs, the third and forth rows report a negative impact on hourly employment concentrated primarily among jobs paid below the new threshold. The response appears to be driven more by an increase in separations than a decrease in hires. However, as noted in the figures, there is a already pre-trend prior to the policy reform so I am cautious to interpret the change in hourly employment flows as causal. Nevertheless, column 3 shows that on net, the estimates imply a cumulative decrease in employment flow of approximately 0.021 (s.e. 0.028) jobs per affected salaried worker. Despite using a different method to compute the employment effect, my estimate from examining changes in employment flows is very close to the estimate in table II calculated using changes in the stock of workers.

Relative to the employment effects, the change in reclassification flows are very precisely estimated even for small responses. The first two rows correspond to panel (c) and (d) from figure 3 where I observe a net flow of jobs from salaried to hourly primarily among jobs being paid below the new threshold. The magnitude of the estimates suggest that employers reclassify approximately 2.2% of affected salaried workers from salaried to hourly. While reclassifying workers has no impact on their overtime eligibility, it enables employers to reduce workers' pay if they work less than 40 hours per week instead of simply paying a fixed weekly salary. In the third and fourth rows, I show that there is a corresponding increase in the number of hourly positions both below and above the threshold. This suggests that newly reclassified workers are paid a weekly base pay similar to what they earned in their previous title.

Overall, the evidence from the analysis on flows suggests that raising the overtime exemption threshold has no significant impact on employment, but leads to a reclassification of jobs from salaried to hourly. In terms of magnitudes, the reclassification effect is much smaller than the bunching effect. Taken together with the results from table II, the estimates suggest that three quarter of the reduction in affected salaried jobs below the threshold is due to bunching, half of the remainder is accounted for by reclassification, and the residual noise is due to employment loss.

V.b Income Effects: Base Pay and Overtime Pay

In this section, I estimate the effect of raising the overtime exemption threshold on workers' incomes using a difference-in-difference design. As in section V, my identification strategy compares workers in states that raised their overtime exemption thresholds to similar workers in states that did not. My baseline regression is

$$y_{isvt} = \sum_{t=T_0}^{T_1} \beta_t \cdot I_{st} + \alpha_{vs} + \delta_{vt} + \varepsilon_{isvt} \tag{2}$$

where y_{isvt} is the weekly earnings of worker i in state s 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-state (α_{vs}) and event-month (δ_{vt}) fixed effects. My identification strategy assumes that the wages of workers in the treated states would have evolved at the same rate as the control states absent the policy. As before, I validate my empirical strategy by checking that the parallel trend assumption holds pre-reform and that my results are robust to a series of alternative specifications. Standard errors are clustered by state.

I make two sample restrictions. First, to focus on workers directly targeted by the policies, I restrict the sample for each event to workers who are paid a weekly base pay between the old and new thresholds in the month prior to the policy change. I make the same restriction for the control groups to create a comparable counterfactual. Second, since I do not observe the wages of workers in firms not using ADP's software, I restrict the sample to workers who are continuously employed at the same firm in all months of the event window. I acknowledge that the sample restriction may introduce selection bias if the policies had an impact on the separation of workers. Given that I did not find strong employment effects, this is likely a minor issue. Nevertheless, I will show that despite the potential differences in composition, workers in the treated and control states still had very similar pre-trends.

Figure 4 plots the difference-in-difference estimates for base and overtime pay, separately by whether the worker was salaried or hourly at baseline. I estimate all regressions in levels rather than logs to be able to compare the dollar increase in weekly base pay relative to weekly overtime pay. Starting with salaried workers, three key features stand out from this analysis. First, consistent with the parallel trends assumption, both weekly base pay and overtime pay were trending similarly between the treatment and control states prior to the announcement of the new rule. Second, workers experience a sharp jump in base pay and a minor increase in overtime pay at precisely the month that the threshold increases, and this raise remains fairly stable afterwards. Lastly, workers experience a far larger increase in base pay than overtime pay, suggesting that bunched workers gained the most from the policy. The rise in salaries further rejects the prediction of the compensating differentials model that firms would cut workers' base pay to nullify the costs of overtime.

Turning to hourly workers, I find a similar pattern of parallel pre-trends, a sharp increase in earnings at the month of the reform, and continued elevated earnings afterwards. However, there are two differences to note from salaried workers. First, the increase in base pay is far smaller on average for hourly workers than salaried workers. This is to be expected as not only did fewer hourly workers receive a raise above the threshold, but there are also more hourly workers than salaried workers at baseline. Second, the increase in overtime pay is more volatile than for base pay. At most, the overtime reforms had a small positive impact on hourly workers' overtime earnings.

Table IV summarizes the estimates of the income effects from expanding overtime coverage. All estimates are computed from equation 2, and reported for the first month of the reform to ease computational burden. In the first column, I show that the base pay impact is an order of magnitude larger than the increase in overtime pay, suggesting that most of the benefits of the policy for workers is realized through the bunching effect. Summing the effects on base and overtime pay, then dividing by baseline incomes, the overtime reforms increased average total income of salaried workers by 1.4% (s.e. 0.2%). In the last row, I show that results are similar if I simply estimate the difference-in-difference using log total incomes as the outcome.

Column (2) reports analogous estimates for hourly workers. In this case, I find a smaller increase in weekly base pay, but a larger increase in overtime compensation. Overall, the earnings of hourly workers between the old and new thresholds increased by about 0.9% (s.e. 0.2%). Although the magnitude of the income effect for hourly workers is smaller than that of salaried workers, it nevertheless highlights that raising the overtime exemption threshold has strong spillover effects onto hourly workers too.

I implement three tests of the robustness of my results. First, in columns (3) and (4), I control for Census-division interacted with time fixed effects, thereby only comparing states within the same Census division over time. Second, columns (4) and (5) control for event-firm-month and event-firm-state fixed effects. This specification compares workers who work at the same firm but reside in different states. Lastly, columns (7) and (8) drops events where a state simultaneously raised the minimum wage at the same time it increased the overtime exemption threshold, and the new minimum wage is within \$200 weekly base pay of the old threshold. Across all specifications, I find a clear increase in salaried workers' earnings by around 1%. I find a slightly smaller increase in hourly workers' earnings, and the estimate shrinks significantly once I compare workers within the same firm. That perhaps suggest that the initial spike in overtime in panel (d) of figure 4 is driven by firm-specific changes and the composition of firms across states.

Taken together, the evidence shows that expanding overtime coverage increases the earnings of both salaried and hourly workers. However, the increase in base pay is far larger than the increase in overtime pay, suggesting that employers would rather pay to keep workers exempt from overtime than simply pay them overtime compensation. I conduct a simple calculation to understand the magnitude of the bunching effect and the share of workers who are benefit from such a raise. In theory, the only reason firms would raise base pays in response to the overtime policies is to pay workers above the new overtime exemption threshold. Assuming that the rise in base pay accrues entirely to bunched workers, then the previous analysis in table II shows that about 16% of affected salaried workers drive the entire \$11.54 increase in weekly base pay. Together, the estimates imply that the weekly base pay of bunched workers increase by $\frac{\$11.54}{0.16} = \$72.13$. Given a baseline weekly salary of \$870, the simple calculation suggests that the earnings of the 16% of affected workers increased by approximately 8.3%, whereas remaining affected workers only experienced minor increases to their earnings from overtime pay.

V.c Hours Effects

In this section, I analyze the effect of raising the overtime exemption threshold on workers' hours. My analysis proceeds in two parts. First, I explore the distribution of hours in the ADP data. However, given that employers are not required to record the hours of salaried workers exempt from overtime, this preliminary analysis with the ADP data is limited to only hourly workers. Second, I supplement my analysis using data from the CPS Outgoing Rotation Survey, which asks respondents' their usual weekly work hours and salaried/hourly status.

To start, I use the ADP data to test one of the fundamental predictions of the labor demand model of overtime: bunching in the distribution of weekly hours. Similar to overtime pay and gross pay, the variable for hours is aggregated over all hours worked in a month and I convert it to an average weekly value following the procedure in appendix B. Appendix

Figure A.6 plots the distribution of average weekly hours for all hourly workers in the baseline month. Despite the imprecision from converting monthly hours to an average weekly value, there is a clear bunching mass at precisely 40 hours per week. The bunching is consistent with recent evidence from Goff (2020) that finds a similar result using administrative payroll data at the weekly level. Such bunching at the overtime kink point is contrary to labor supply models of hours determination and adds to growing evidence that employers play a pivotal role in setting workers' hours (Labanca and Pozzoli, 2022).

While the cross-sectional distribution of hours provides a useful test of competing models of hours determination, it does not contain any information on the impact of raising the overtime exemption threshold. In theory, the response can be in either direction. Newly covered salaried workers may see a decrease in hours leading to a bunching mass at 40. However, those who receive a raise to above the overtime exemption threshold may actually be asked to work more hours to compensate. Given the theoretical ambiguity, I next test for the hours response in the CPS. Specifically, I estimate the following regression:

$$\log(Y_{it}) = \alpha_{iv} + \alpha_{tv} + \sum_{q=-4}^{3} \beta_q I_{sq} + \varepsilon_{it} \tag{3}$$

where Y_{it} is the outcome variable for respondent i at month t. I estimate the stacked difference-in-difference regression controlling for event-individual (α_{iv}) and event-month (α_{tv}) fixed effects. The treatment interacted with event-time indicators (I_{sq}) are aggregated at the quarterly level, q. For statistical power, I expand my sample period to include all 30 state overtime threshold changes from 2008 to 2023. I cluster estimates at the state level.

Note that by controlling for individual fixed effects, my preferred specification follows the same individuals over time. As such, the regression is identified from individuals who are employed both pre and post reform. Since the CPS only surveys respondents over 16 months, some individuals may not exist throughout the entire panel. Moreover, respondents in the CPS are asked their salaried/hourly status and weekly earnings only in the 4th and 16th month they are surveyed. As such, the sample of workers used to identify each β_q changes each quarter. Workers are however asked their weekly hours in every wave of the survey, so the sample changes less often when examining the effect of the policy on hours. In either case, β_q is interpreted as the percent change in Y_{it} within-individual between the treated states relative to the control states.

My main specification restricts the sample to workers who reported working a salaried job during the 4th wave of the survey, with weekly earnings between the old and new overtime exemption thresholds. As a placebo check, I also estimate the difference-in-difference for workers earning up to $100 per week above the new threshold.

Figure 5 presents my estimates of equation 3 for two outcome variables: log weekly earnings and log weekly hours worked last week. First, as a validity check on the quality of the data, I replicate my earlier analysis of the income effect of expanding overtime coverage for salaries workers. One concern with the CPS is that the weekly earnings variable is measured with self-reporting error, so I cannot perfectly identify workers directly affected by the reforms. Despite the measurement error, panel (a) shows similar pre-trends between the treated and control states, and an increase in average weekly earnings immediately following the expansion in overtime coverage. As a placebo check, panel (b) finds no such immediate impact for salaried workers already earning above the overtime exemption threshold. The successful replication of the income effect, although imprecisely estimated, builds confidence that the earlier results in the ADP data are externally valid.[17]

After confirming that the CPS data appears adequate enough to identify workers affected by the overtime reforms, I next estimate the effect on workers' hours. Panel (c) shows that weekly hours increase in the 2nd and 3rd quarters after the expansion in overtime coverage. However, while hours increased relative to the quarter immediately before the policy change, it is not significantly different from even earlier quarters. Hours also return back to their baseline levels 4 quarters after the expansions in overtime. Overall, I find weak evidence of an increase in hours, but nothing persistent. Panel (d) shows that hours did no change for the placebo group relative to the year before the policy change.

Table V assesses the robustness of my analysis of the CPS data. First, the estimates in column (1) are analogous to figure 5 except I aggregate the dynamic effects using a simple indicator for the treated states post-reform. Consistent with the figure, I find a 3.7% (s.e. 1%) increase in weekly earnings among the treatment group, no impact on income of the placebo group, and no effects on hours for either sample. The estimates of the hours effect are precise enough that I can rule out any positive increase in hours above 1.2%. Column (2) adds demographic controls for gender, race, age, and age-squared. Lastly, in column (3), I interact the month fixed effects with the Census division of the survey respondent. This stricter specification accounts for region-specific time trends so that I am comparing states in the same Census division over time. I find that the magnitude of the estimates all remain fairly stable over the three specifications. Taken together, the evidence suggests that raising the overtime exemption threshold increases workers' earning, without significantly impacting workers' hours.

[17] Given the relatively small sample size of the CPS, I do not have enough statistical power to replicate the bunching results.

VI Policy Implications

To benchmark the costs and benefits of raising the overtime exemption threshold relative to other labor market policies, Table VI computes the ratio of the employment and income effects, otherwise known as the "own-wage employment elasticity" in the minimum wage literature (Dube and Lindner, 2024). The elasticity is computed by jointly estimating the employment and income effects, and then calculating their ratio using the delta method.

In column (1), I report the estimates from my baseline firm-level regression. The elasticity in panel (A) is equivalent to taking the ratio of the employment effect with respect to affected salaried workers in column 1 of Table II and the income effect for salaried jobs in column 1 of Table IV. The ratio suggests that for each percent increase in affected salaried workers' earnings, the number of affected jobs falls by 1.9% (s.e. 2.4%). However, this statistic likely overstates the percent change in employment because it assumes that the entire employment loss comes from salaried jobs. As evident from the earlier analysis, there is significant spillover effects onto hourly jobs in terms of both income and employment. To account for such spillovers, panel (B) reports the estimates once I treat all workers between the old and new thresholds as affected by the policy, regardless of their salary/hourly status. As expected, both the percent change in employment and income fall significantly. Taken together, the estimates imply that the number of affected jobs fall by -0.59% (s.e. 0.55%) for each percent increase in workers' earnings.[18] My 95% confidence bound can rule out elasticities smaller than -1.66 and larger than 0.48.

The remaining columns of Table VI assess the robustness of my estimate to a series of stricter specifications checks. Focusing on panel (B), I find that the magnitude of the elasticity is stable to including Census-division fixed effects, dropping events where a concurrent change in the minimum wage risked interacting with the change in the overtime exemption threshold, or simply estimating a stacked two-way fixed effects model. However, the elasticity flips signs and becomes much noisier if I only use within-firm variation. Overall, the point estimates of the elasticity appear fairly small, ranging from -0.75 to 1.1.

To my knowledge, this is the first study to compare the employment effects of overtime to its income effects. As such, there are no estimates in the literature with which I can make a one-to-one comparison. Given the bunching in the pay distribution, the most similar benchmark for reference would be from studies of the minimum wage. A meta-analysis by Dube (2019) finds a median elasticity of employment with respect to own wage of -0.17 across

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

36 studies of the minimum wage in the U.S, with a range of -3 to 1.5. Relative to these studies, I examine a higher-income population where the implied hourly wage of affected salaried workers is about \$21.75/hour. As such, there is no reason why the overtime exemption policy should have comparable effects. Overall, I find slightly more negative elasticities than in the minimum wage literature, but still below the -0.8 threshold that Dube and Lindner (2024) uses to classify "large negative" employment impacts.

VII Discussion and Conclusion

This paper presents new facts about the labor market effects of expanding overtime coverage and informs policy debates surrounding recent initiatives to raise the overtime exemption threshold. In this section, I summarize my findings by comparing my estimates to the predictions of the Department of Labor in their cost-benefit assessment for their 2016 proposal. To generate these predictions, the DOL conducted a thorough review of the literature on overtime and used existing labor demand elasticities to infer from the Current Population Survey the expected effects of their proposed reform.

My empirical results differ from the conclusions of the Department of Labor in four ways. First, the DOL believed that by increasing the marginal cost of labor per hour, "employers have an incentive to avoid overtime hours worked by newly overtime-eligible workers, spreading work to other employees" (U.S. Department of Labor, 2016). In contrast, I estimate that expansions in overtime coverage actually have little effect on employment. Second, while the DOL accurately predicted that average weekly earnings would rise, they calculated an income effect of only 0.7%, whereas I show that earnings increased by nearly twice that amount for salaried workers. I also show that this positive income effect was not uniformly distributed across the range of affected base pays, and primarily benefited a small group of workers who receive a raise above the threshold. Third, drawing from previous studies of the compensating differentials model of overtime, the DOL calculated that 18% of workers would experience a decrease in base pay to partially offset the increase in overtime pay. However, I find no evidence that firms reduced base pays in response to overtime coverage. Fourth, the DOL considered the reclassification effects of the policy negligible given the available evidence at the time. In contrast, I find that approximate 2.6% of affected workers are reclassified from salaried and hourly, and the policy appears to have noticeable spillover effects on the income and employment of hourly workers.

Although my paper offers the most comprehensive evaluation of the overtime exemption policy to date, there are many avenues for future research that are beyond the scope of this study. Similar to the minimum wage, overtime can potentially have important implications for consumers (Harasztosi and Lindner, 2019), inequality (Lee, 1999; Autor et al., 2016), and productivity (Coviello et al., 2021). Unlike the minimum wage though, there did not even exist estimates of the employment effects of overtime eligibility prior to my study (Brown and Hamermesh, 2019). This paper adds to the scarce literature on overtime coverage by providing the first causal estimates of its employment and income effects. In future endeavors, it would be fruitful to explore the effects of overtime coverage on additional margins of responses.

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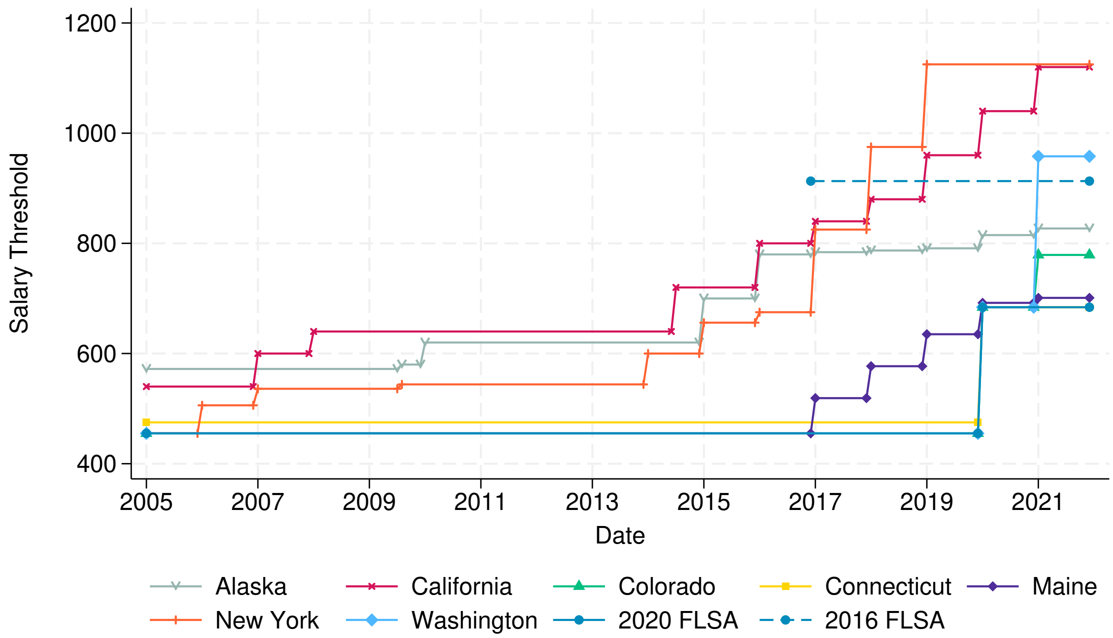
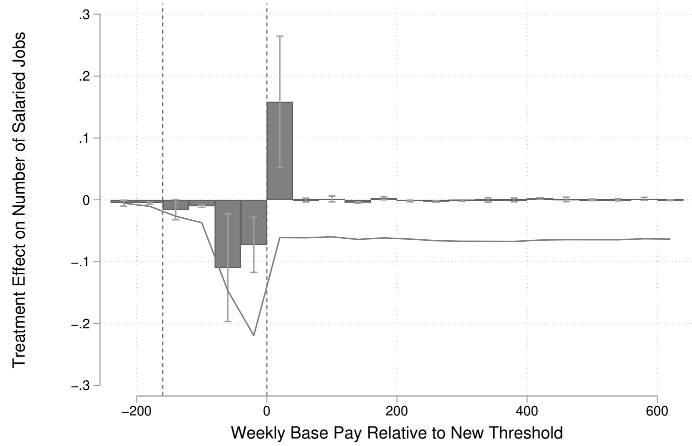
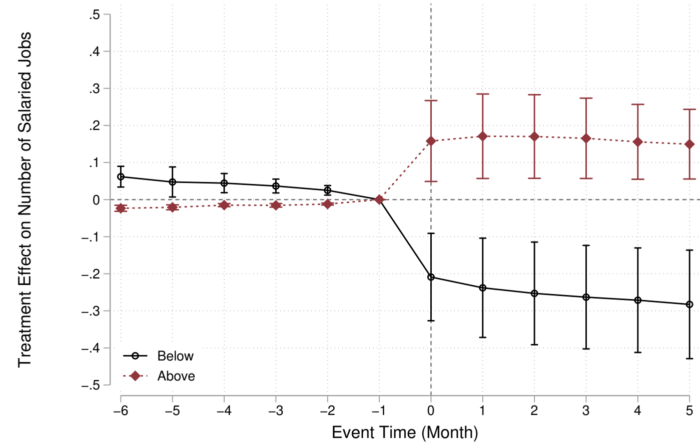


Figure 1
Variation in Overtime Exemption Thresholds Across States

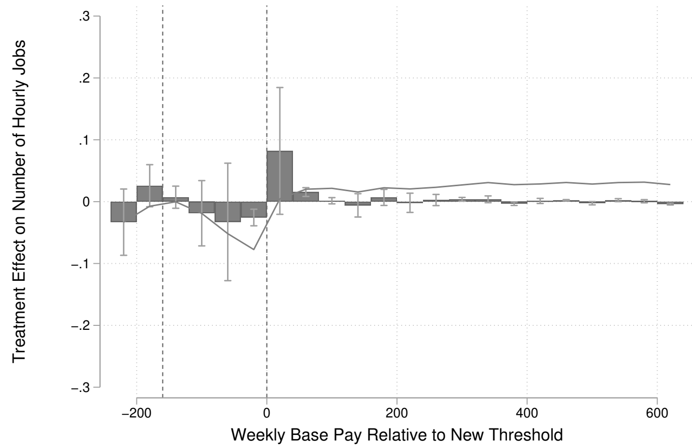
Notes. This figure shows the binding overtime exemption threshold in each state between 2005 and 2021. All states not explicitly included in the graph are covered by the Fair Labor Standards Act (FLSA). The line "2016 FLSA" represents the federal threshold that was supposed to go into effect on December 1, 2016 but was nullified in November 2016. In Alaska and California, the threshold equals 80 times the state minimum wage. In New York, the threshold equals 75 times the minimum wage. In Washington, the overtime exemption threshold equals 70 times the minimum wage starting in 2021. In Maine, the threshold equals 3000/52 times the minimum wage. Colorado's overtime exemption threshold is not pegged to the minimum wage. Starting in January 2017, the minimum wage and threshold varies by firm size in CA, and county and firm size in NY. When the threshold varies within-state, I plot the highest threshold faced by any employer in the state.



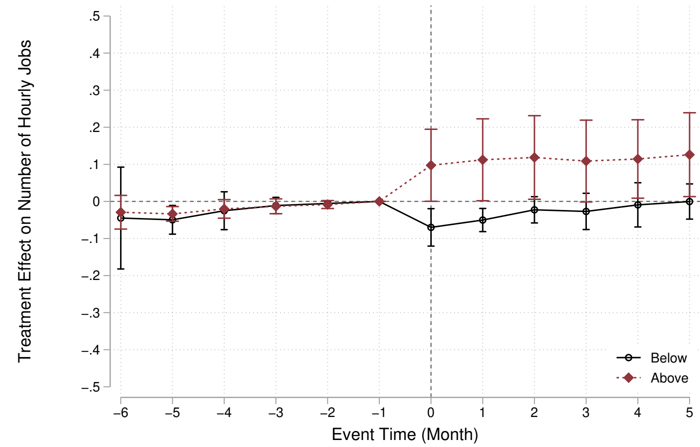
(a) Effect on Number of Salaried Jobs, at Time 0



(b) Effect on Salaried Distribution Over Time



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

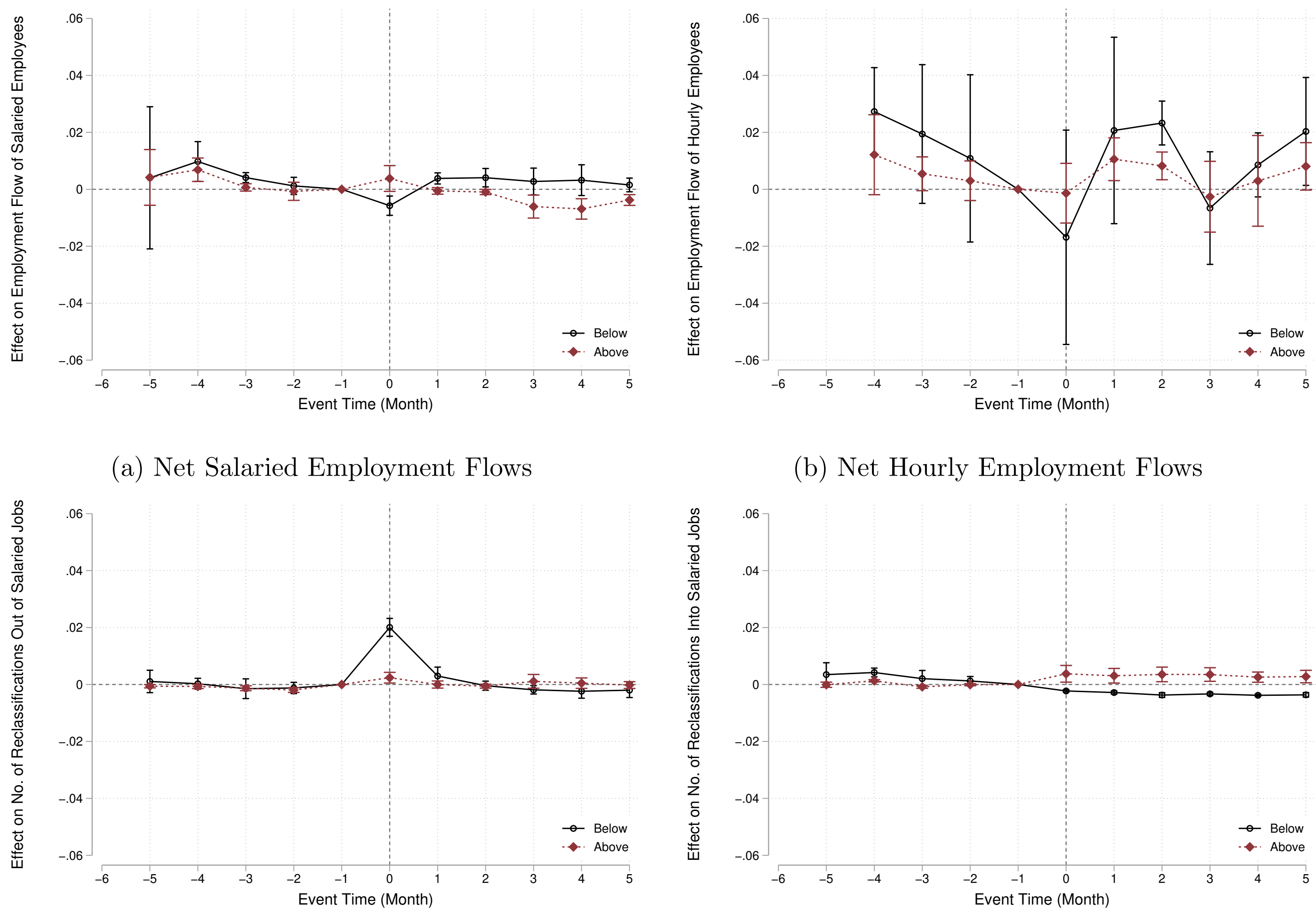


(d) Effect on Hourly Distribution Over Time

Figure 2

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

Notes. Panel (a) shows the event study estimates from equation 1. The height of each bar indicates the effect of raising the OT exemption threshold on the number of salaried jobs in each \$40 bin of base pay on the month that the new threshold becomes binding, scaled by the baseline number of salaried workers between the old and new thresholds. The solid line is the running sum of these estimates. The bins are normalized so that the new threshold for each event is 0. The left vertical dashed line is set at the smallest baseline threshold across all the events. Panel (b) shows the sum of the estimates over time, separately for bins between the old and new thresholds and bins up to \$80 above the new threshold. Panels (c) and (d) are analogous to Panels (a) and (b) for the distribution of hourly jobs. For each estimate, I show the 95% confidence interval using standard errors clustered by state.



(a) Net Salaried Employment Flows

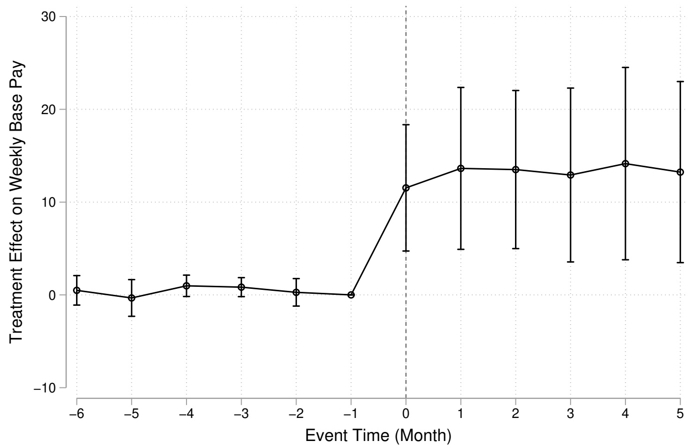
(b) Net Hourly Employment Flows

(c) Reclassification Flows Out of Salaried

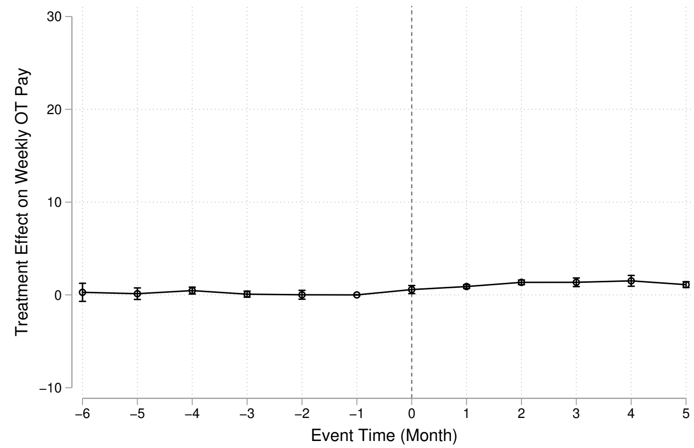
(d) Reclassification Flows Into Salaried

Figure 3
Effect of State Threshold Changes on the Flow of Workers Into, Out of, and Within Firms

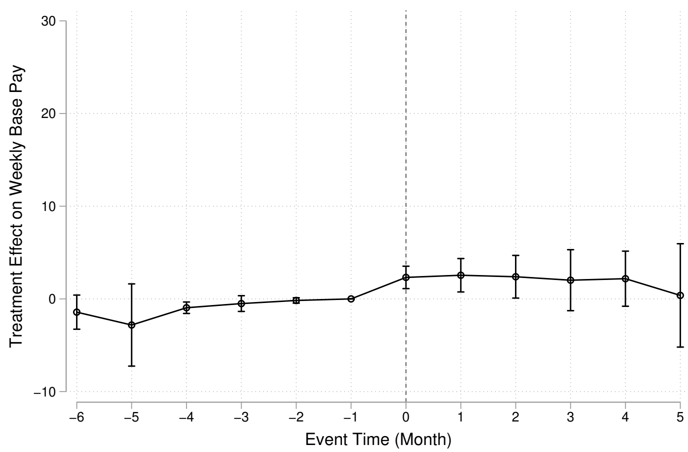
Notes. Panel (a) plots the effect of the state threshold changes on the net employment flow of salaried employees for each month since the threshold increased, scaled by the baseline number of salaried workers between the old and new thresholds. Panel (b) plots the analogous figure for net employment flows of hourly employees. Panel (c) plots the effect on the number of workers being reclassified from salaried to hourly each month and Panel (d) plots the effect on the number of employees that were reclassified from hourly to salaried. All estimates are computed using equation 1, and aggregated separately for bins between the old and new thresholds (circles) and bins up to $80 above the new threshold (diamonds). For each estimate, I show the 95% confidence interval using standard errors clustered by state.



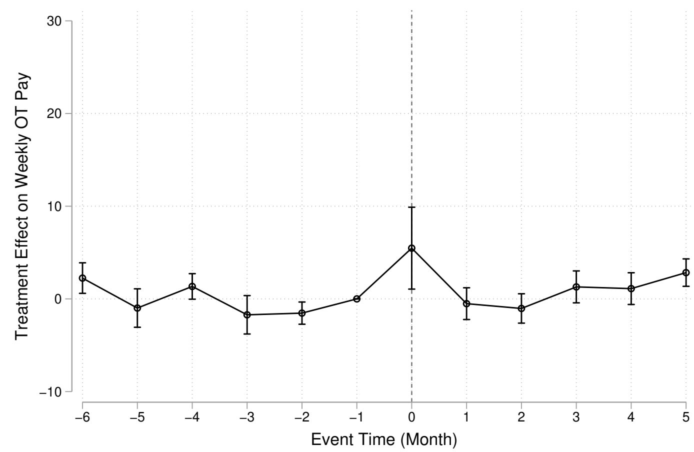
(a) Salaried: Weekly Base Pay



(b) Salaried: Weekly OT Pay



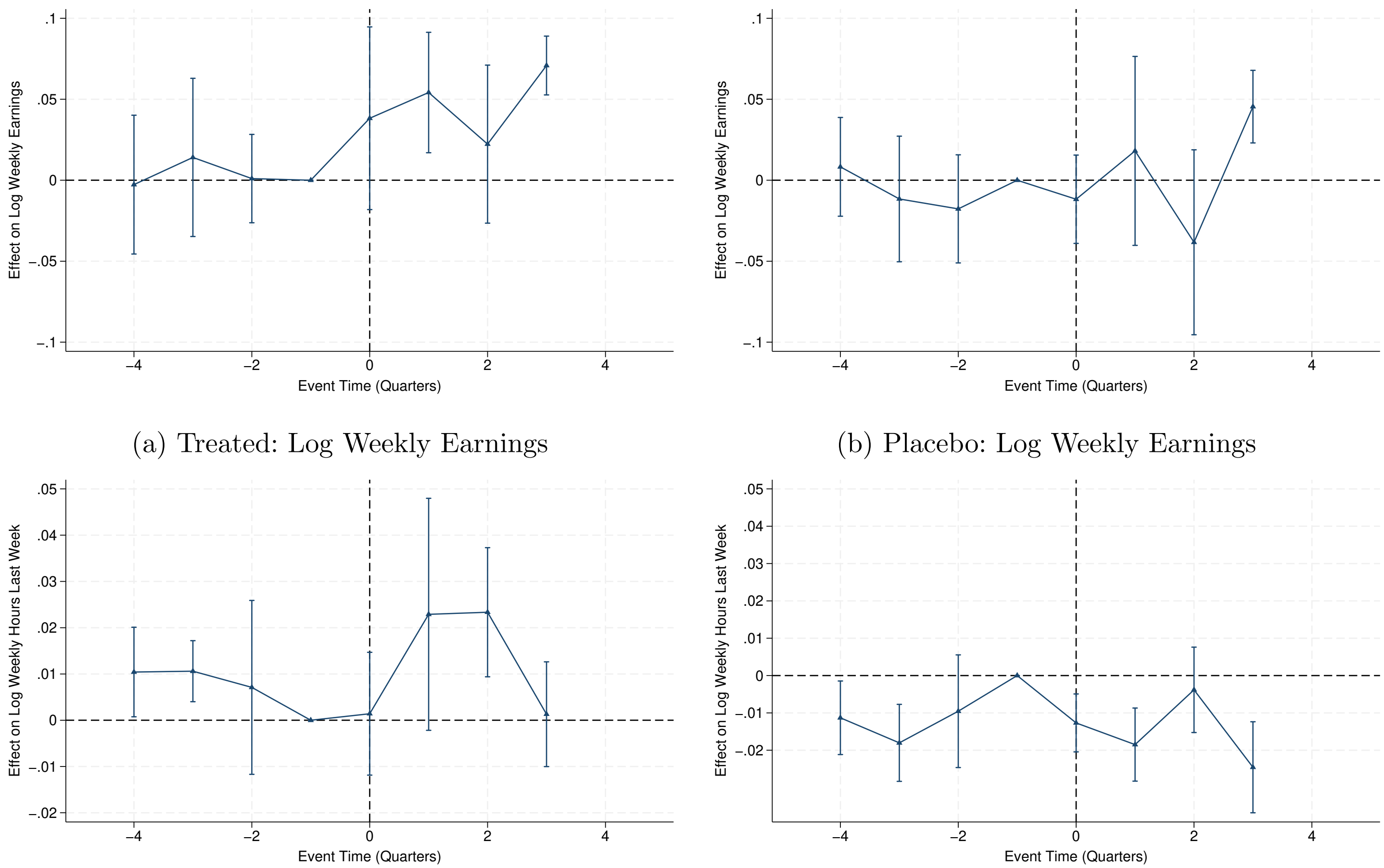
(c) Hourly: Weekly Base Pay



(d) Hourly: Weekly OT Pay

Figure 4
Difference-in-Difference Estimates of the Income Effect of Raising the OT Exemption Threshold

Notes. Panels (a)-(d) show the effect of raising the overtime exemption threshold on base pay and overtime pay, separately for salaried and hourly workers initially earning between the old and new thresholds. All estimates are computed from equation 2. Standard errors are clustered by state.



(a) Treated: Log Weekly Earnings

(b) Placebo: Log Weekly Earnings

(c) Treated: Log Weekly Hours

(d) Placebo: Log Weekly Hours

Figure 5
Difference-in-Difference Estimates of the Income and Hours Effect using the CPS

Notes. This figure shows the estimates of equation 3, which compares the outcomes of CPS respondents in states that raised the overtime exemption threshold to respondents in states that did not. Panels (a) and (c) uses the sample of salaried workers with reported weekly earnings between the old and new thresholds. Panels (b) and (d) uses salaried workers with reported weekly earnings up to $100 above the new threshold.

Table I
Summary of Theoretical Predictions

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

Notes. This table summarizes the predictions of the two models of overtime discussed in Section III. The first four rows refer to the effect on each outcome from an expansion in overtime coverage for salaried workers. The last row refers to the effect of overtime among hourly workers.

Table II
Effect of Raising the Overtime Exemption Threshold on the Pay Distribution

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A						
Salaried Jobs						
Below Threshold	$-.209^{***}$	$-.149^{***}$	$-.203^{***}$	$-.216^{***}$	$-.209^{***}$	
	(.061)	(.032)	(.063)	(.056)	(.062)	
Above Threshold	$.158^{***}$	$.107^{***}$	$.157^{***}$	$.164^{***}$	$.153^{**}$	
	(.056)	(.03)	(.057)	(.054)	(.062)	
Hourly Jobs						
Below Threshold	$-.07^{***}$	$-.042^{*}$	$-.028$	$-.077^{***}$	-.009	
	(.026)	(.023)	(.021)	(.023)	(.017)	
Above Threshold	$.098^{**}$	$.065^{**}$	$.088^{**}$	$.102^{**}$	$.08^{*}$	
	(.05)	(.026)	(.042)	(.049)	(.046)	
Panel B						
Emp. w.r.t. Aff. Salaried	-.023	-.019	.015	-.027	.015	-.024
	(.023)	(.013)	(.018)	(.025)	(.035)	(.024)
Emp. w.r.t. All Affected	-.005	-.004	.003	-.006	.003	-.005
	(.005)	(.003)	(.004)	(.005)	(.008)	(.005)
Number of Affected Salaried	1.075	1.075	1.096	1.15	1.198	1.075
Number of Affected Hourly	4.212	4.212	4.272	4.233	4.378	4.212
Event-Bin-Month FE	Y	Y	Y	Y	Y	
Event-Bin-State FE	Y	Y	Y	Y	Y	
Event-Bin-Division-Month FE		Y			Y	
Event-Bin-Month-Firm FE			Y		Y	
Event-Bin-State-Firm FE			Y		Y	
Event-State						Y
Event-Month						Y
Number of event-firms (treatment)	237,285	237,285	184,560	213,356	162,454	237,285
Number of event-firms (control)	5,124,260	5,124,260	2,225,276	3,770,451	1,854,267	5,124,260

Notes. Rows (1) and (2) report the effect of raising the OT exemption threshold on the number of salaried jobs below and above the new threshold, respectively, scaled by the number of affected salaried jobs. Affected jobs are defined as salaried employees with base pay between the old and new threshold on the month before the announcement of the rule change. Rows (3) and (4) report similar estimates for the number of hourly jobs. Row (5) reports the sum of rows (1) to (4), and represents the effect on the total number of jobs, scaled by the number of affected salaried jobs. Row (6) scales the change in total employment by the total number of jobs (i.e. salaried and hourly) with base pays between the old and new thresholds before the rule change, instead of just salaried jobs.

Column (1) reports the effects of the state threshold increases on the exact month of the rule change, estimated using equation 1. Column (2) estimates the effects using within-census division variation. Column (3) uses variation within firms that operate in both the treatment and control groups. Column (4) drops the five events where the old overtime exemption threshold was within $200 of the new minimum wage. Column (5) estimates a saturated model with the full sample. Column (6) aggregates employment across bins of base pay and then estimates a two-way fixed-effects model. Standard errors are clustered by state. *10%,** 5%,*** 1% significance level.

Table III
Event-Study Estimates of Employment Flow and Reclassification Effects

	Employment			Reclassification		
	Hires	Separations	Net	Into	Out of	Net
Salaried Below	-.003***	.003	-.006***	-.002***	.02***	-.022***
	(.001)	(.003)	(.002)	(0)	(.002)	(.002)
Salaried Above	.006***	.002**	.004**	.004***	.002**	.001
	(.001)	(.001)	(.002)	(.001)	(.001)	(.001)
Hourly Below	-.001	.017*	-.018	.019***	.002**	.018***
	(.01)	(.01)	(.019)	(.002)	(.001)	(.002)
Hourly Above	.003	.005**	-.001	.005***	0	.005***
	(.005)	(.002)	(.005)	(.001)	(0)	(.001)
Cumulative	.005	.027**	-.021	.026***	.024***	.001
	(.015)	(.014)	(.028)	(.004)	(.003)	(.001)
Treatment Group						
Number of Affected Salaried	1.075	1.075	1.075	1.075	1.075	1.075
Number of Affected Hourly	4.212	4.212	4.212	4.212	4.212	4.212
Event-Bin-Month FE	Y	Y	Y	Y	Y	Y
Event-Bin-State FE	Y	Y	Y	Y	Y	Y
Number of event-firms	237,285	237,285	237,285	237,285	237,285	237,285

Notes. The first column reports the effect of raising the state OT exemption threshold on the number of new hires among salaried jobs paying within $160 below the new threshold (row 1), salaried jobs paying within $80 above the new threshold (row 2), hourly jobs within the same ranges (rows 3 & 4), and the sum of salaried and hourly jobs within those ranges (row 5). Each estimate is scaled by the number of affected salaried jobs, reported in row 6. Column (2) reports estimates for the effect on separations by the same groups. Column (3) is the difference between columns (1) and (2). Columns (4) and (5) report the effect on the number of reclassifications into and out of each group, respectively. Column (6) is the difference between columns (4) and (5). All values are estimated from equation 1, and robust standard errors in parentheses are clustered by state. $^{*}p < .1,^{**}p < .05,^{***}p < .01$

Table IV
Income Effect of Raising the OT Exemption Threshold

	Main Spec		Within-Census		Within-Firm		No Minimum Wage	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Weekly Base Pay	11.536***	2.319***	7.359***	1.528***	9.602***	1.551***	11.405***	2.379***
	3.473	.616	1.851	.299	2.38	.239	3.619	.677
Weekly OT Pay	.574***	5.474**	.282***	4.647***	.285*	1.485*	.564**	5.395**
	.222	2.254	.099	1.557	.162	.849	.232	2.467
Weekly Total Pay	12.11***	7.793***	7.641***	6.175***	9.887***	3.036***	11.968***	7.774***
	3.68	1.69	1.88	1.272	2.444	.793	3.836	1.836
% Δ Total Pay	.014***	.009***	.009***	.007***	.011***	.003***	.014***	.009***
	.004	.002	.002	.001	.003	.001	.004	.002
Log Total Pay	.013***	.008***	.008***	.006***	.01***	.003***	.012***	.007***
	.004	.001	.002	.001	.003	.001	.004	.002
Baseline Weekly Base Pay	870	815	870	815	878	820	874	821
Baseline Weekly OT Pay	7	79	7	79	10	86	7	81
Event-Month FE	Y	Y	Y	Y	Y	Y	Y	Y
Event-State FE	Y	Y	Y	Y	Y	Y	Y	Y
Event-Division-Month FE			Y	Y			Y	Y
Event-Month-Firm FE					Y	Y	Y	Y
Event-State-Firm FE					Y	Y	Y	Y
N (treatment)	189,241	708,375	189,241	708,375	97,002	402,593	92,796	362,043
N (control)	2,322,226	9,195,386	2,322,226	9,195,386	1,447,181	6,711,793	1,172,638	4,307,095
Sample	Salaried	Hourly	Salaried	Hourly	Salaried	Hourly	Salaried	Hourly

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

Columns (1) reports the effect of increasing the state overtime exemption threshold on salaried workers' earnings, estimated from equation 2. The sample consists of salaried workers who were earning between the old and new threshold prior to the rule change. Column (2) reports the income effect for hourly workers with weekly base pay within the affected salary interval. Columns (3) and (4) report analogous estimates comparing workers within the same Census division. Columns (5) and (6) compare workers within the same firm, across states. Columns (7) and (8) drops the five events where the old overtime exemption threshold was within \$200 of the new minimum wage. Robust standard errors in parentheses are clustered by state. *10%,** 5%,*** 1% significance level.

Table V

Effect of Raising the OT Exemption Threshold on Workers' Earnings and Hours

	(1)	(2)	(3)
Treatment (n=161,804)			
Log Weekly Earnings	.037***	.038***	.049***
	(.01)	(.011)	(.016)
Log Weekly Hours	.004	.003	-.011
	(.004)	(.003)	(.003)
Placebo (n=188,576)			
Log Weekly Earnings	.009	.009	.011
	(.005)	(.005)	(.011)
Log Weekly Hours	.001	.001	.001
	(.002)	(.002)	(.004)
Event-Individual	Y	Y	Y
Event-Month	Y	Y	
Event-Month-Region			Y
Controls		Y	Y

Notes. This table presents stacked difference-in-difference estimates comparing states that raised their overtime exemption thresholds to states that did not. Column (1) reports the estimates for two outcome variables (i.e. log weekly earnings and log weekly hours) separately for a treatment and placebo sample constructed from the CPS. Column (2) includes controls for gender, race, age, age-squared, and marital status. Column (3) adds fixed effects for Census division-event-month fixed effects. The treatment panel restricts the data to only salaried workers who report earning between the old and new overtime exemption threshold before the policy change. The placebo panel restricts the sample to those earning between the new threshold and $100 per week above it. I report besides each group name the number of observations over the two year event window for all events. All robust standard errors in parentheses are clustered by state. *10%,** 5%,*** 1% significance level.

Table VI
Ratio of Employment and Income Effects

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A						
% Δ Emp. w.r.t. Aff. Salaried	-.023	-.019	.015	-.027	.015	-.024
	(.023)	(.013)	(.018)	(.025)	(.035)	(.024)
% Δ Salaried Income	.013	.008	.01	.012	.004	.013
	(.004)	(.002)	(.003)	(.004)	(.001)	(.004)
Elasticity w.r.t. Aff. Salaried	-1.855	-2.25	1.452	-2.152	3.503	-1.905
	(2.357)	(1.688)	(1.49)	(2.654)	(7.335)	(2.453)
Panel B						
% Δ Emp. w.r.t. All Affected	-.005	-.004	.003	-.006	.003	-.005
	(.005)	(.003)	(.004)	(.005)	(.007)	(.005)
% Δ All Income	.008	.006	.004	.008	.003	.008
	(.001)	(.001)	(0)	(.001)	(.001)	(.001)
Elasticity w.r.t. Income	-.592	-.622	.745	-.722	1.097	-.608
	(.545)	(.393)	(.93)	(.611)	(2.487)	(.571)
Number of Affected Salaried	1.075	1.075	1.096	1.15	1.198	1.075
Number of Affected Hourly	4.212	4.212	4.272	4.233	4.378	4.212
Event-Bin-Month FE	Y	Y	Y	Y	Y	
Event-Bin-State FE	Y	Y	Y	Y	Y	
Event-Bin-Division-Month FE		Y			Y	
Event-Bin-Month-Firm FE			Y		Y	
Event-Bin-State-Firm FE			Y		Y	
Event-State						Y
Event-Month						Y
Number of event-firms	5,361,545	5,361,545	2,409,836	3,983,807	2,016,721	5,361,545
Number of event-workers (sal)	2,679,244	2,679,244	1,676,655	2,256,403	1,435,846	2,679,244
Number of event-workers (sal + hr)	9,388,099	9,388,099	6,453,514	6,571,030	4,502,730	9,388,099

Notes. Row (1) reports the change in employment scaled by the number of affected salaried workers, defined as salaried workers earning between the old and new overtime exemption thresholds in the month before the policy change. Row (2) reports the percent change in income of affected salaried workers. Row (3) reports the ratio of the estimates in rows (1) and (2). Rows (4)-(6) repeats the same analysis, but defines the treated group as all jobs (i.e. both salaried and hourly) with base pays between the old and new thresholds before the rule change.

Column (1) reports the employment effect from column (1) of table II, the income effect in column (1) of table IV, and their ratio. Column (2) estimates the effects using within-census division variation. Column (3) uses variation within firms that operate in both the treatment and control groups. Column (4) drops the five events where the old overtime exemption threshold was within $200 of the new minimum wage. Column (5) estimates a saturated model with the full sample. Column (6) aggregates employment across bins of base pay and then estimates a two-way fixed-effects model. Standard errors are clustered by state. *10%,** 5%,*** 1% significance level.

Appendix: For Online Publication

Appendix A. Additional figures and tables

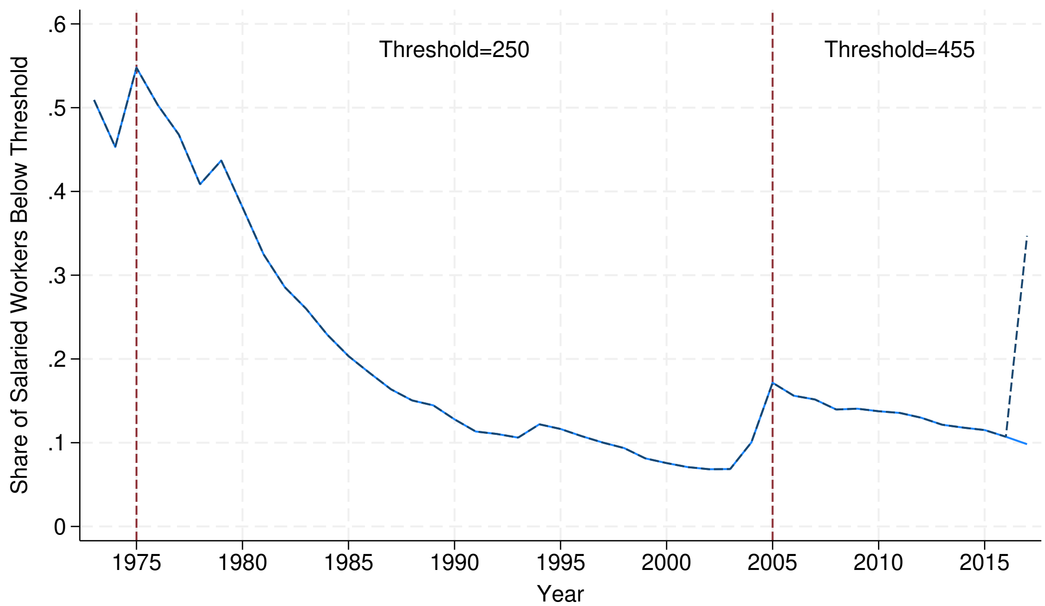


Figure A.1
Percent of Salaried Workers Earning Below the FLSA OT Exemption Threshold

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

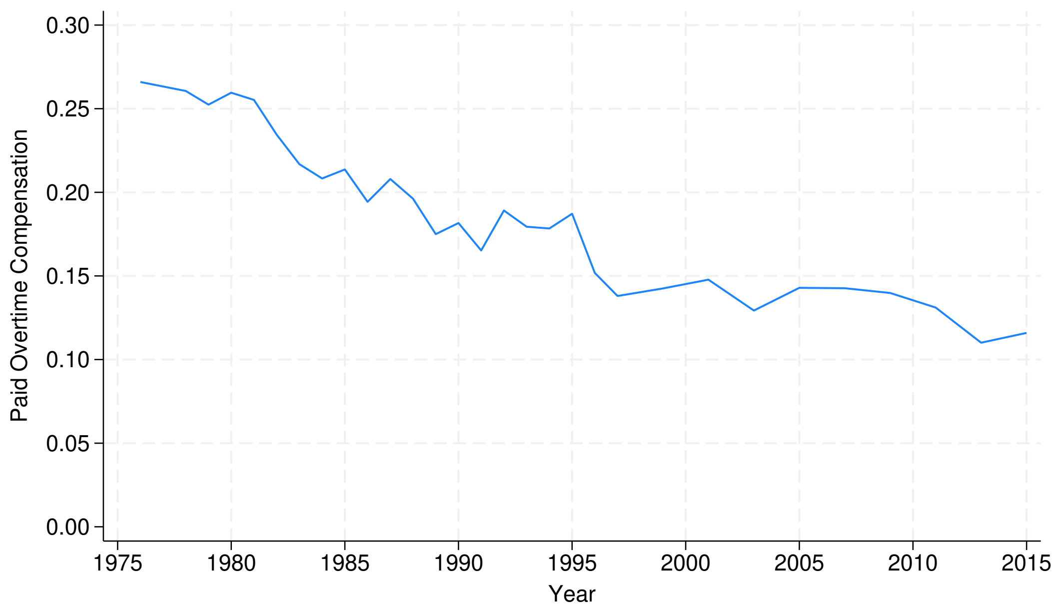
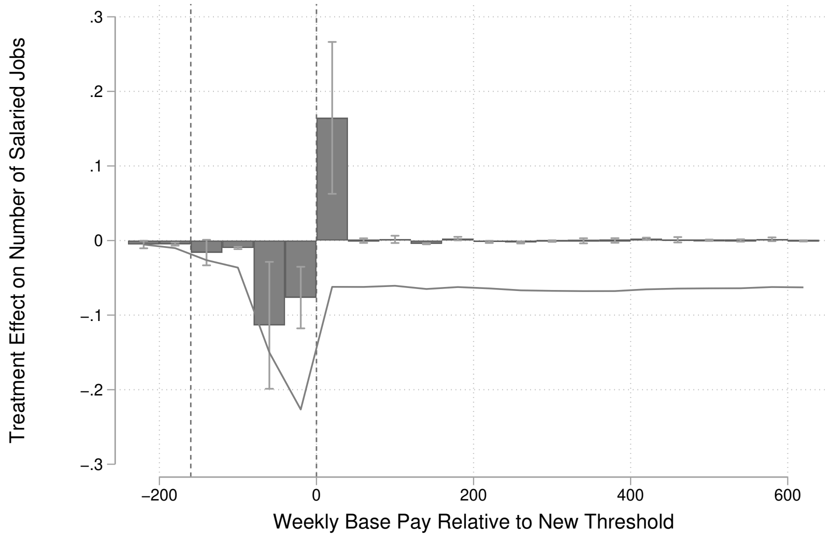
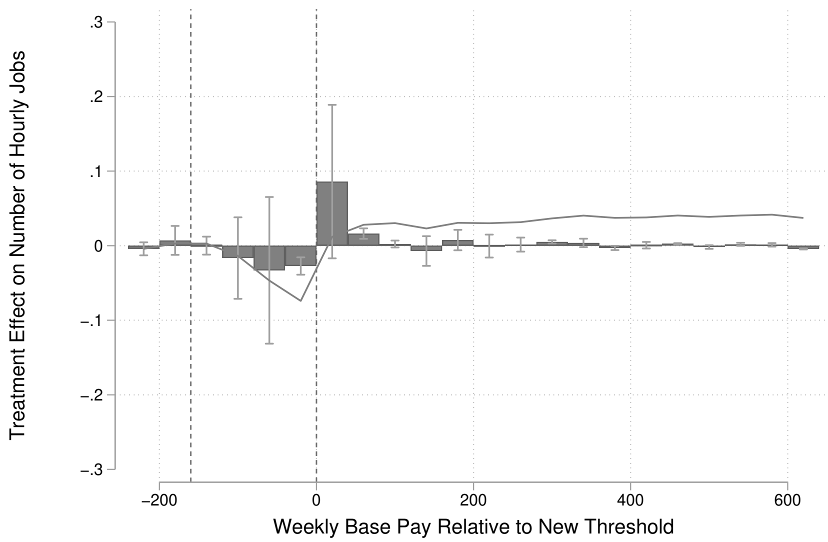


Figure A.2
Percent of Salaried Workers Eligible for Overtime

Notes. This figure shows the percent of salaried workers in the PSID who work at least 40 hours a week and 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".



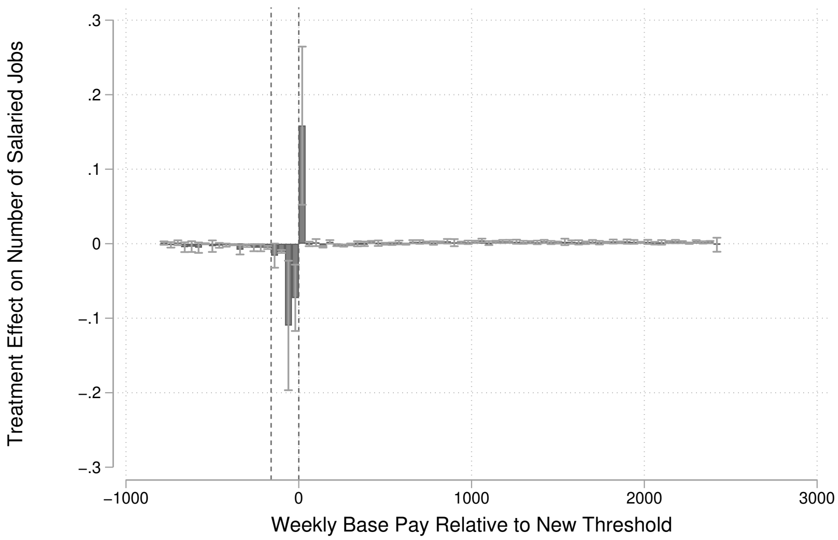
(a) Effect on Number of Salaried Jobs, at Time 0



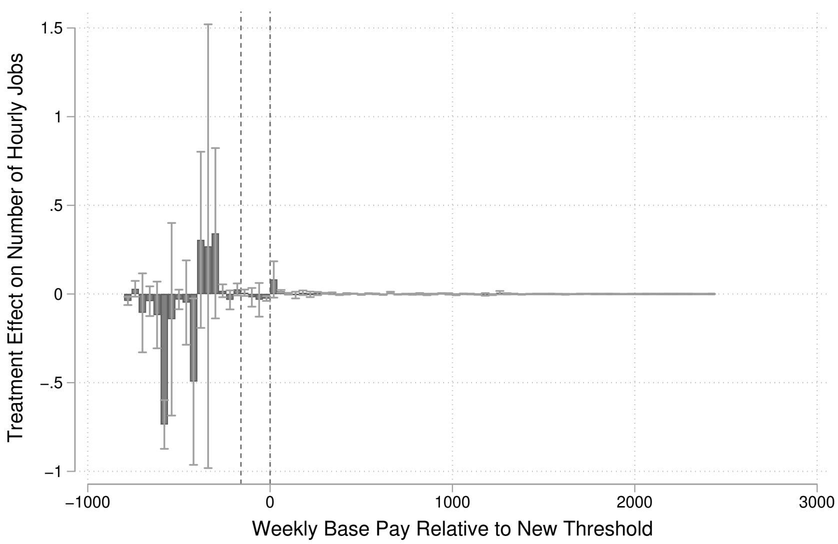
(b) Effect on Number of Hourly Jobs, at Time 0

Figure A.3
Effect of Raising States' OT Exemption Thresholds on the Distribution of Base, No Interaction with Minimum Wage Pay

Notes. The figures show the event study estimates from equation 1, separately for salaried and hourly jobs. The sample is restricted to the 14 events where the the old overtime exemption threshold is at least $200 weekly base pay above a new minimum wage. The height of each bar indicates the effect of raising the OT exemption threshold on the number jobs in each $40 bin of base pay on the month that the new threshold becomes binding, scaled by the baseline number of salaried workers between the old and new thresholds. The solid line is the running sum of these estimates. The bins are normalized so that the new threshold for each event is 0. The left vertical dashed line is set at the smallest baseline threshold across all the events. For each estimate, I show the 95% confidence interval using standard errors clustered by state.



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



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

Figure A.4
Effect of Raising States' OT Exemption Thresholds on the Distribution of Base Pay

Notes. The figures show the event study estimates from equation 1, separately for salaried and hourly jobs. The height of each bar indicates the effect of raising the OT exemption threshold on the number jobs in each \$40 bin of base pay on the month that the new threshold becomes binding, scaled by the baseline number of salaried workers between the old and new thresholds. The bins are normalized so that the new threshold for each event is 0. The left vertical dashed line is set at the smallest baseline threshold across all the events. For each estimate, I show the 95% confidence interval using standard errors clustered by state.

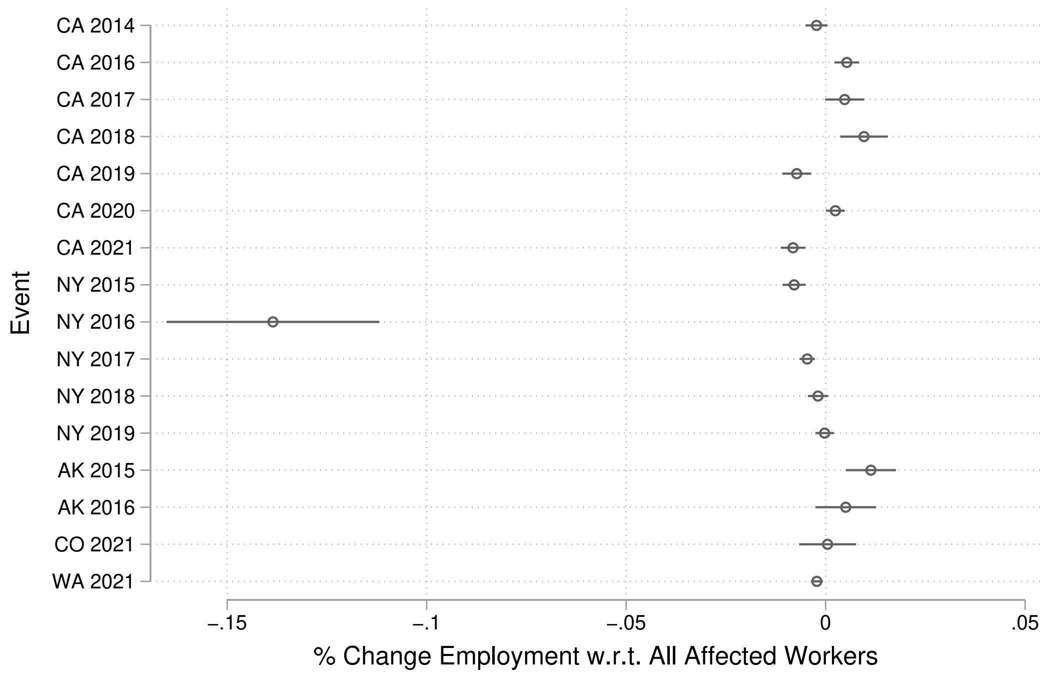


Figure A.5
Employment Effect Scaled by Affected Workers, by Event

Notes. The figure shows the difference-in-difference estimates from equation 1, separately for 16 events. All regression estimates are scaled by the total number of salaried and hourly workers between the old and new overtime exemption threshold in the month before the policy change. The bars around each estimate represent 95% confidence intervals. Standard errors are clustered by state.

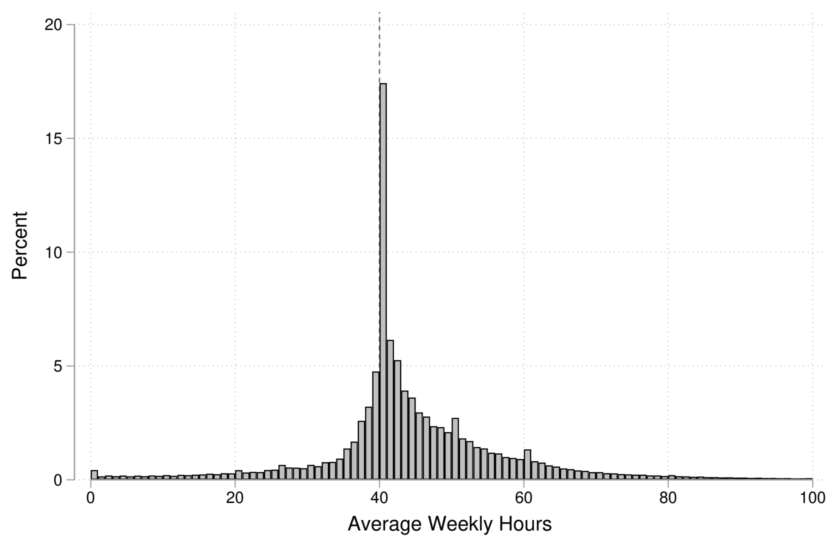


Figure A.6
Distribution of Average Weekly Hours Among Hourly Workers in April 2016

Notes. The figure shows the distribution of average workweeks among hourly workers with base pays between the old and new overtime exemption threshold in the month before a policy change. Average weekly hours is imputed from the total hours worked in a month following the methodology in Appendix B. Each bin is a one hour increment. The left vertical line is at 40 hours per week.

Appendix B. Defining the Compensation Variables

B.a Overtime Pay

In this subsection, I present the procedure I use to determine each individual's overtime pay from the "OT earnings" variable, when available. There are two challenges to inferring workers' overtime pay from the ADP data.

First, firms are not required to input a value into the "OT earnings" field. Although the ADP data contains four separate earnings variables and four corresponding hours variables, each capturing a different component of gross compensation, firms are only required to report employees' gross pay and standard rate of pay. Thus, it is uncertain whether a missing value for overtime earnings means that the firm does not record the value or the worker did not receive any overtime pay. To test how often firms separately record workers' overtime pay, I calculate the sum of workers' four components of pay and find that it matches the measure of gross pay 99.8% of the time. This suggests that most employers are indeed properly recording the multiple aspects of individuals' incomes.

The second challenge with measuring workers' overtime pay is that the type of compensation included in the "OT earnings" field is at the discretion of the firm. Thus, some employers may use the field to record other forms of compensation than overtime pay. To account for this, I impute overtime pay following the methodology described by Grigsby et al. (2021). First, I define an implied overtime wage as the ratio between the "OT earnings" and "OT hours" variables. Next, I divide the implied wage by workers' actual wage to compute an implied overtime premium (i.e. $\frac{\text{OT earnings}}{\text{OT hours}\cdot\text{wage}}$), where a salaried worker's "wage" for overtime purposes is defined by the Department of Labor as $\frac{\text{weekly base pay}}{40}$. I consider the "OT earnings" variable to represent true overtime pay if the implied overtime premium is less than or equal to 2. I find that the distribution of the implied overtime premium exhibits significant bunching at 1.5, and 2, indicating that the variable usually captures true overtime earnings. Among workers with non-missing "OT earnings", 94% of hourly workers and 86% of salaried workers have implied overtime premiums within either 1.4-1.6 or 1.9-2.1. To validate my measure of overtime for salaried workers, appendix figure C.2 plots the probability that a salaried worker receives overtime as a function of their weekly base pay, and finds clear discontinuity at the overtime exemption threshold.

B.b Computing Weekly Measure of Income

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

	Share of Workers			
Pay Frequency	Hourly	Salaried	Base Pay	Gross & OT Pay
Weekly	.276	.079	S	$\frac{1}{N}$ Y
Biweekly	.653	.570	$\frac{1}{2}$S	$\frac{1}{2N}$Y
Semimonthly	.067	.311	$\frac{24}{52}$S	$\frac{12}{52}$Y
Monthly	.004	.04	$\frac{12}{52}$S	$\frac{12}{52}$Y
All workers	.762	.238		

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 the baseline month across all 19 events studied in the paper. 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 amount conditional on receiving N paychecks in the month.

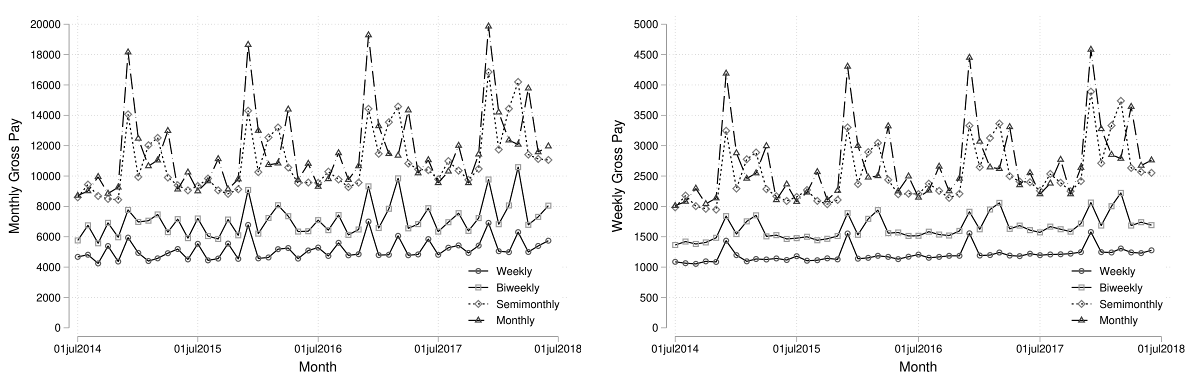
To derive workers' weekly base pay from their standard rate of pay, I follow the rules set by the Department of Labor and scale each worker's standard rate of pay by their pay frequency (i.e. $\frac{\text{standard pay}}{\text{week}} = \frac{\text{standard pay}}{\text{paycheck}} \cdot \frac{\text{paycheck}}{\text{week}}$). 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:

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

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

I define $\frac{\text{paychecks}}{\text{month}} = 1$ for workers paid monthly and $\frac{\text{paychecks}}{\text{month}} = 2$ for workers paid semi-monthly. For weekly and biweekly paid workers, the number of paychecks received each month depends on both the day of the week that each worker gets paid, and the number of times that day appears in the month. For instance, if a worker gets paid on a Thursday every two weeks, then the worker's gross pay includes 3 paychecks in December 2016 when there were 5 Thursdays, but only 2 paychecks in April 2016. To illustrate this problem, I plot in figure B.1a the monthly gross pay for all continuously employed workers from July 2014 to July 2018, by their pay frequency. Not only do biweekly and weekly paid workers experience spikes in their gross pay, the peaks and troughs do not occur on the same months between years. In contrast, monthly and semi-monthly paid workers only experience a large spike in December of each year, likely reflecting bonuses.



(a) Monthly Gross Pay

(b) Normalized Weekly Gross Pay

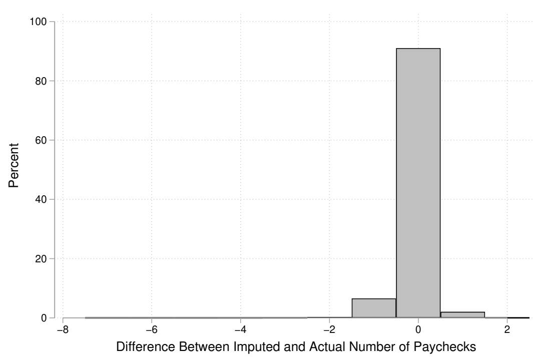
Appendix Figure B.1
Gross Income, by Pay Frequency

Notes. Panel (a) shows the average monthly gross pay for a balanced panel of workers from July 2014 to July 2018. Panel (b) shows the average weekly gross pay for the same panel of workers.

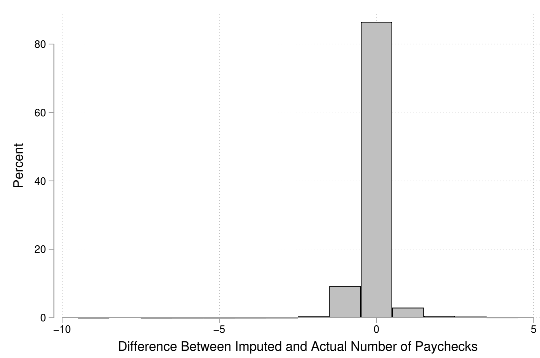
While different workers may receive an extra paycheck in different months, employees of the same firm tend to receive a paycheck on the same day of the month, conditional on their pay frequency. To impute the number of paychecks per month that each firm issues in a month, I apply the following algorithm:

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 12 average monthly gross pays computed in step 1.
3. Record biweekly workers as receiving 3 paychecks in months where the average gross pay in their firm-frequency exceeds 1.25 times the firm's median gross pay in that year, and 2 otherwise.
4. Record weekly workers as receiving 5 paychecks in months where the average gross pay in their firm-frequency exceeds 1.25 times the firm's median gross pay in that year, and 4 otherwise.

In effect, I assume a firm issued one additional paycheck in the month if the average worker's gross pay for that month far exceeds the median pay in the year. I implement two validation tests of my imputation strategy. First, plotting workers' weekly gross pay implied by their imputed number of paychecks, I show in figure B.1b that the periodic spikes in gross pay among biweekly and weekly paid workers completely disappear. Second, I compare the imputed number of paychecks per month to the actual number of paychecks per month using data post-2016 (see figure B.2). I find that I am able to match the actual number of paychecks for nearly 90% of biweekly paid worker-months and 85% of weekly paid worker-months.



(a) Biweekly



(b) Weekly

Appendix Figure B.2
Impute Number of Pay Checks, by Pay Frequency

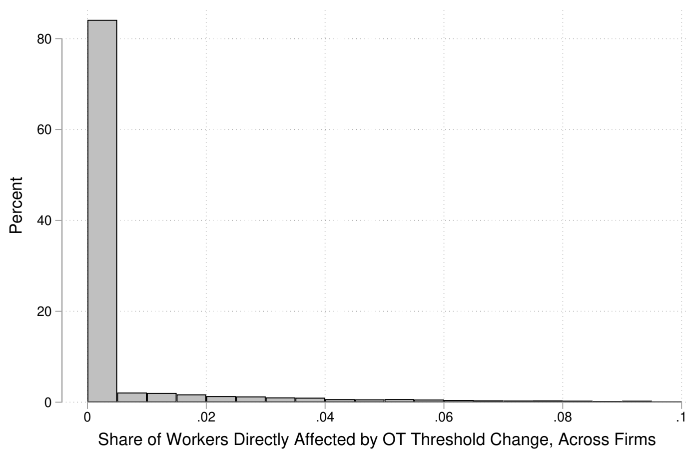
Notes. Panel (a) shows the distribution of the difference between imputed and actual number of paychecks per month, for all worker-months in between July 2016 to July 2018 where the worker is paid biweekly. Panel (b) shows a similar distribution for workers who are paid weekly.

Appendix C. Descriptive Statistics

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

C.a Directly Affected Firms vs. Entire Sample

Figure C.1 plots the distribution of firms by their share of salaried employees with base pays between the old and new state overtime exemption thresholds in the month before the policy change. The first feature to note is that the majority of firms had less than 0.5% of salaried workers with base pays between the old and new thresholds. Nevertheless, firms with no directly affected workers can still respond to the policy through changes in hiring decisions or spillovers from the reallocation of jobs between firms. Thus, I keep the full sample of firms in my main analysis.



Appendix Figure C.1
Distribution of Share Directly Affected by the State Overtime Policy

Notes. The figure shows the distribution of firms by the share of workers who are paid by salary, and earn between the old and new overtime exemption threshold at baseline.

Table C.1 describes in more detail the characteristics of firms and workers affected by each of the increases in the overtime exemption threshold. In column (1), I record the size distribution, industry mix, and worker composition among firms in the treated states. I find that the sample comprises primarily of small and medium size firms across a range of industries, albeit the data has a smaller share in non-tradeable and a larger share in "other" relative to representative statistics (Mian and Sufi, 2014). Of the firms in the treated states, 17% hire only hourly workers. In column (2), I restrict the sample to only firms with at least one salary worker between the old and new thresholds at baseline. Relative to the average employer, directly affected firms are over twice as large and have a greater share of salaried workers, but follow a similar industry mix. The observation that larger firms are more susceptible to reforms in the exemption threshold follows from purely a probabilistic standpoint - firms with more employees are more likely to have at least one worker paid within any fixed interval of base pay. Given that the direct response to the rule changes is driven by large firms, there may be concern about the representativeness of my estimates. However, it should be noted that although directly affected firms only make up 21% of the sample, they employ nearly half of all workers. Thus, the response of these large firms is highly relevant to the evaluation of the policy. One final point to note is that the majority of the treated firms are located in California and New York, so my firm-level analyses primarily capture the response of firms located in these states.

Columns (3) and (4) repeat the summary statistics for firms in the states that did not raise their overtime exemption thresholds. I included Colorado and Washington in the control group for all events prior to 2021 as these states simply followed the FLSA's overtime exemption threshold at that time. A "firm" in the control state should be understood as a "firm-state" combination. Overall, I find very similar industry and worker distribution as firms in the treated states. To account for any differences in trends by industry, the main analysis will test for the robustness of my results to keeping only firms that operate in both treated and control states.

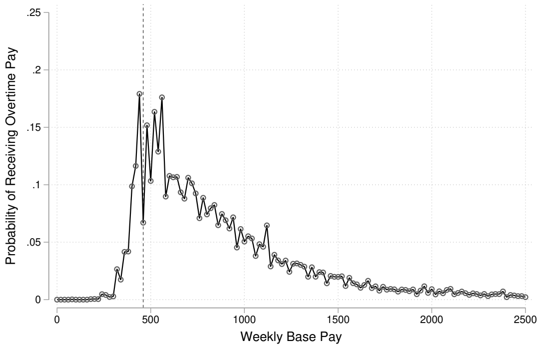
C.b Cross-sectional Evidence of Compliance

Next, I explore whether employers comply with the overtime rules. Figure C.2 plots the probability that a salaried worker receives overtime pay as a function of their weekly base pay. Consistent with compliance to the overtime regulation, salaried workers earning less than the overtime exemption threshold are far more likely to receive overtime pay compared to those earning above it. In particular, the probability of receiving overtime in California and New York in April 2016, exhibits a discontinuous drop at exactly the overtime exemption threshold. The discontinuity likely represents compliance to the policy and not simply the selection of workers who get bunched above the threshold. For example, firms likely prefer to bunch worker who work long hours above the threshold to avoid paying them overtime. In that case, those below the threshold should actually have lower hours than average, and should be *less* likely to earn overtime. Despite the bias against finding a discontinuity, I nevertheless observe that workers paid below the overtime exemption threshold are more likely to earn overtime in California and New York. However, this composition effect could explain the lack of a discontinuity in FLSA states in April 2016.

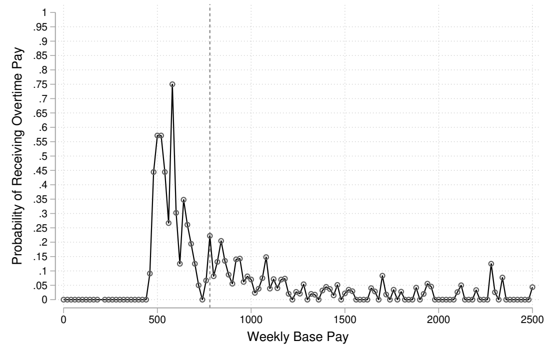
Appendix Table C.1
Firms Affected by Changes in the Overtime Exemption Threshold

	Treated States		Control States	
	(1)	(2)	(3)	(4)
Firms				
Average Size	87.135	208.169	42.258	151.144
% Firm Size: < 50	.703	.366	.842	.463
% Firm Size: 50-499	.266	.549	.144	.476
% Firm Size: 500-999	.019	.051	.009	.041
% Firm Size: 1000-4999	.011	.031	.004	.019
% Firm Size: ≥ 5000	.001	.002	0	.001
Tradeable	.168	.129	.191	.148
Nontradeable	.044	.061	.04	.063
Construction	.098	.085	.093	.09
Other	.65	.693	.643	.671
Restaurant	.023	.028	.016	.026
Retail	.048	.065	.045	.07
Manufacturing	.168	.135	.189	.156
Share Salaried	.521	.514	.545	.527
Only Salaried	.297	.125	.388	.169
Only Hourly	.174	0	.233	0
Both Salaried and Hourly	.529	.875	.379	.831
Share Treated	.017	.08	.016	.123
Variation				
California	.553	.568	0	0
New York	.333	.385	0	0
Alaska	.007	.002	0	0
Maine	.036	.005	0	0
Colorado	.036	.011	.023	.018
Washington	.036	.029	.024	.022
No. Event-Firms	253,139	52,918	5,698,548	754,045
No. Event-Workers	22,057,267	11,015,864	240,811,385	113,969,262
Sample	All	≥ 1 aff. worker	All	≥ 1 aff. worker

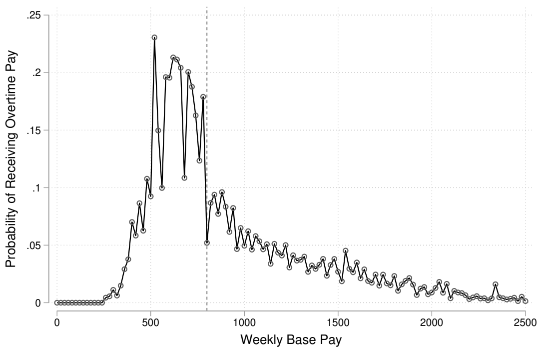
Notes. The table reports the characteristics of firms and workers in the baseline month prior to each threshold change, separately for all firms and for only firms that employed at least one salary worker directly affected by the reform. Columns (1)-(2) report these statistics for firms in states that raised their overtime exemption threshold. Columns (3)-(4) reports similar statistics for firms in the control states. The first three group of rows report the distribution of firm sizes, industry mix, and worker composition of firms. The fourth group of rows captures the distribution of firms across the states that ever raised the overtime exemption threshold from 2014-2021.



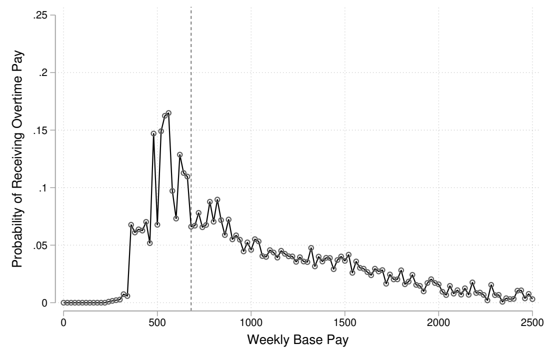
(a) FLSA states, April 2016



(b) Alaska, April 2016



(c) California, April 2016

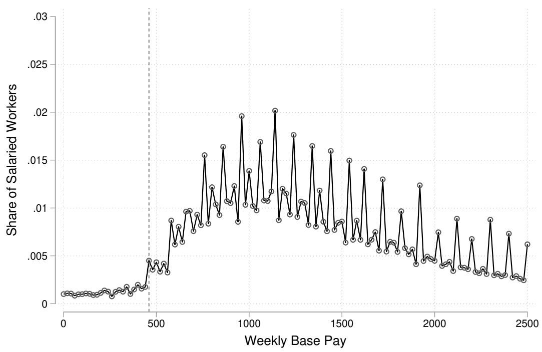


(d) New York, April 2016

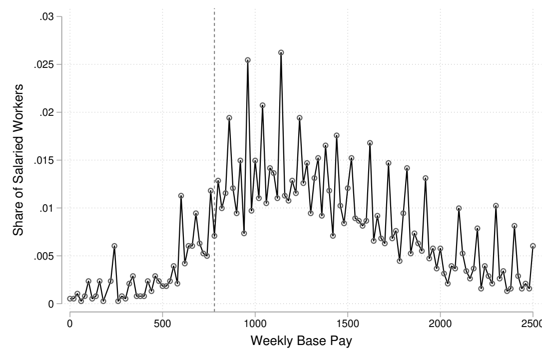
Appendix Figure C.2
Probability of Receiving Overtime Pay, Conditional on Base Pay

Notes. Each graph shows the probability that salaried workers receive non-zero overtime pay in April 2016, as a function of their weekly base pay. The sample in figure (a) is restricted to salaried workers not living in California, New York, Maine, or Alaska. The sample in figure (b) is restricted to salaried workers in Alaska, figure (c) is restricted to California, and figure (d) is restricted to New York. The dotted vertical black line is the overtime exemption threshold in effect for each respective sample.

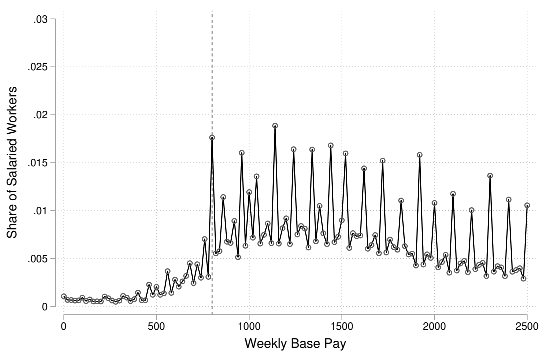
To provide preliminary evidence of a bunching effect, figure C.3 finds clear spikes in the distribution of weekly base pay at the overtime exemption threshold. Since I divided the distribution into $20 bins of weekly base pay, the distribution exhibits spikes at regular intervals that represent annual salaries in multiples of $5,000. Nevertheless, there is evidence of bunching at precisely each state's overtime exemption threshold. and the bunching does not exist at the same salary bin in states where the threshold is not binding. One feature worth pointing out is that there are rarely any workers to the left of the FLSA threshold. That not only provides evidence that few workers are covered for overtime, but also explains why there is no discontinuity in overtime pay at the cutoff in appendix figure C.2a.



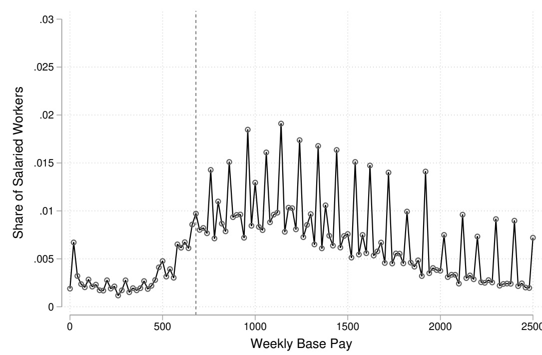
(a) FLSA states, April 2016



(b) Alaska, April 2016



(c) California, April 2016



(d) New York, April 2016

Appendix Figure C.3
Distribution of Weekly Base Pay

Notes. Each graph shows the distribution of weekly base pay for salaried workers in April 2016. The sample in figure (a) is restricted to salaried workers not living in California, New York, Maine, or Alaska. The sample in figure (b) is restricted to salaried workers in Alaska, figure (c) is restricted to California, and figure (d) is restricted to New York. The dotted vertical black line is the overtime exemption threshold in effect for each respective sample.