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 and state-level threshold changes from 2014-2021, I find evidence against conventional models of overtime. Contrary to the historical intent of policymakers, firms do not increase employment by substituting more workers for fewer hours. However, contrary to compensating differential models, firms also do not offset the costs of overtime by lowering workers' base pays. Instead, employers raised salaries above the threshold to keep workers exempt from overtime, indicating that monitoring and adjusting workers' hours is costly for firms. Taken together, these results suggest that expanding overtime coverage increases workers' earnings without negatively impacting 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. While overtime legislation has been central to US employment standards for over 80 years, research on its effects has been hindered by a shortage of policy changes and a lack of data to accurately measure their impacts. Expansions in overtime coverage are rare and often coincide with changes in other labor laws. Moreover, 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-collar salaried workers who earn above it are legally exempt. Between 2014 and 2021, there were nineteen state reforms that raised the overtime exemption threshold. 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 thresholds 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, about half of the remaining missing mass was 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 corresponds to a statistically insignificant reduction in employment.

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.

Third, workers targeted by the policy experience a 1.4% increase in their labor income. Although the policy expanded overtime coverage, 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 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 8.5%. 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 the 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).[1] To my knowledge, my paper provides the first causal estimates of the employment and income effects of overtime coverage.[2] More-

[1] 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 restrict total working hours without affecting employment, suggesting that firms cannot flexibly reallocate hours across workers.

[2] Although Quach (2025) also examines the labor market effects of an overtime policy, that paper uses a non-binding federal rule change to test competing theories of wage rigidity. In contrast, the present paper exploits an entirely different set of state-level reforms to study the effects of overtime policies that are actually binding.

over, I study overtime coverage for *salaried* workers, a group for which there are no existing papers but is at the center of modern policy debates. Since the analysis focuses on changes in overtime eligibility for salaried workers, the results do not speak directly to the effects of overtime coverage on hourly workers. Nevertheless, my paper provides new 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 has 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 costs 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 for 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 (Bureau of Labor Statistics, 2019). My paper provides the first evidence of the ex-ante cost of overtime. I show that firms' willingness to raise workers' base pay to avoid overtime coverage is an order of magnitude larger than the observed increase in overtime compensation.

Third, I contribute to a broader literature on wage and hour laws (Brown and Hamermesh, 2019). Rather than implementing 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 policies tend to have only small employment effects (Cengiz et al., 2019; Harasztosi and Lindner, 2019; Derenoncourt and Montialoux, 2021; Dustmann et al., 2022).[3] 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 distribution 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

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

details governing U.S. overtime regulations and the specific policies that expanded coverage for salaried workers. Section III outlines the predictions of competing models of overtime. In section IV, I describe the administrative payroll data. Section V reports my main results on the aggregate employment and income effects of overtime. I conclude in section VI by summarizing 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.[4] 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-collar duties, and earns a salary equal to or greater than the "overtime exemption threshold" set by the Department of Labor (DOL).[5] 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).[6] 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.

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

[4]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.

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

[6]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%.

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 ($35,568 per year) on January 1, 2020. The federal Department of Labor later 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 policy was again blocked by federal courts so the binding threshold as of June 2026 is $684 per week. Given that the federal policies affect all states simultaneously, I drop these reforms from my analysis.[7]

My analysis uses an event-study design to evaluate 19 state-level increases to the overtime exemption threshold between 2014 and 2021.[8] 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.[9] 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 threshold simultaneously increases following a known formula. Although 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 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 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.

[7] In a separate paper, I use the retraction of the 2016 reform as a natural experiment to study employers' response to downward nominal wage rigidity. Although the federal proposal was never binding, I nevertheless find that firms raised workers' salaries above the overtime exemption threshold, and this effect was not offset by either slower wage growth or lower wages for new hires (Quach, 2025).

[8] 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.

[9] 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.

III Theoretical Predictions

There is no theoretical consensus on how the labor market responds to overtime coverage. In this section, I summarize the predictions of the two canonical models of overtime: a labor demand model and a compensating differentials model. I then present an alternative model that nests key features of the previous models and captures the main institutional details of my setting.

Historically, when the FLSA was first passed during the Great Depression, policymakers had intended for the regulation to create new 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 in Ehrenberg (1971). Under this framework, firms take wages w as given, and choose employment n and hours per worker h to solve the following profit maximization problem:

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

where $f(n,h)$ is employers' production function, p is the overtime premium, $1[h > 40]$ is an indicator for whether hours exceed 40, and F is a fixed cost per worker.

Overtime coverage, modeled by an increase in p from 0 to 0.5, has two effects. First is a substitution effect - since hours have become more expensive, firms want to trade hours for employment. Second is a scale effect - since an input of production has become more expensive, firms want to cut output, leading to a reduction in both hours and employment. Thus, while the intent of the policy is to create new jobs, it is theoretically ambiguous whether such work-sharing occurs and the outcome depends on the substitutability of hours across workers.

One limitation of the labor demand model is that it treats wages as exogenous and fixed. 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.

In the context of my paper, the labor demand model suggests that raising the overtime exemption threshold could potentially increase employment, whereas the compensating differentials model predicts that firms would cut workers' base pay. However, both models miss

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

key institutional features of my setting. First, neither model makes a distinction between salaried and hourly workers. Second, the labor demand model only predicts the partial equilibrium effect of employment. If the economy was already at full-employment, firms would be unable to hire more workers. Third, both models treat overtime coverage as a policy that affects all workers, whereas in my setting, it is contingent on salaried workers' base pay.

To incorporate these institutional features, I build a job search and bargaining model in the style of Carry (2022). For brevity, I summarize its intuition and predictions in this section, and provide the mathematical details in Appendices B and C. The framework follows a standard Pissarides (1985) model with random search, exogenous job loss, and endogenous vacancy postings. The difference is that when workers and firms match, they observe four random parameters instead of one: the productivity of the match, the maximum number of productive hours, the cost of monitoring hours, and the worker's disutility of labor. The distribution of these four parameters in turn generate a distribution of base pays, hours, and salaried/hourly classification through a bargaining process. Covering salaried workers for overtime imposes two costs on the firm: an overtime premium for long hours and a monitoring cost to track hours.[11]

The key insight from the model is that the effects of overtime coverage depend on which outcomes are determined by Nash bargaining and which are determined solely by the employer. First, if both parties jointly bargain over all outcomes (i.e. hours, pay, and classification), then the overtime premium has no real effect and the only burden from the policy is the monitoring cost. Both parties would agree to a lower base pay to exactly offset the premium, identical to the compensating differentials model. However, since the monitoring cost reduces the surplus of the job, it leads to either bunching above the new threshold, an additional decrease in base pay, or job destruction, depending on the parameters of the match. In any case, hours do not change in response to the overtime premium or the monitoring cost, and are always selected to maximize total surplus.

Second, if pay and salaried/hourly status are determined through bargaining, but the firm unilaterally sets hours afterwards, then hours may not be socially optimal because employers do not internalize workers' disutility of labor. When there is no overtime for salaried workers, employers always set hours as high as possible since the marginal cost per hour of labor is zero once salaries are fixed.[12] If the firm is very productive and the worker has low disutility of labor, then these long hours are socially efficient and maximize total surplus. However, if

[11] Once a salaried employee is covered for overtime, their employer is legally required to either record the worker's exact hours or give them a fixed weekly schedule (U.S. Department of Labor, 2026). I model this loss in flexibility as a fixed monitoring cost that randomly varies across jobs.

[12] To prevent hours from going to infinity, I follow Carry (2022) and assume that each job has a maximum number of productive hours, reflecting the time it takes to complete a task.

the marginal disutility of labor exceeds the marginal productivity per hour, the total surplus could be increased by cutting hours.

Even in this setting, overtime coverage would not improve labor market efficiency. For roles where long hours are efficient, overtime coverage decreases total surplus by incentivizing firms to reduce hours. For jobs where long hours are inefficient, the overtime premium would decrease hours towards the socially efficient allocation, but firms and workers already had that option even before the policy change by making the job hourly. The fact that they agreed to classify the job as salaried implies that the monitoring costs are too high for it to be optimal to track workers' hours. Thus, in both cases, overtime coverage leads to a reduction in match surplus.

In response to the threat of a loss in surplus, the bargaining solution is to either bunch base pay above the overtime exemption threshold to avoid the cost, accept that the job is covered for overtime and lower base pay, or dissolve the job. The main distinction between this scenario and the case where firms and workers jointly bargain hours is in the selection of workers into each margin of response. Since overtime can distort hours in this model, the workers with the longest hours are more likely to be bunched above the threshold. As a result, individuals who remain below the threshold are selected to have relatively short workweeks and may only receive a small increase in overtime pay and a small decrease in base pay.

Third, expanding overtime coverage for salaried workers could increase total surplus if the firm determines both hours and the salaried/hourly status, and only the pay is determined by bargaining. For instance, a firm decides whether a job is salaried, then the two parties would negotiate the pay during the interview, and then the employer sets hours each week. In this setting, firms continue to make salaried employees work the maximum hours. Similar to the previous scenario, some of these jobs work efficiently long hours whereas some are working inefficiently long hours but have high monitoring costs. However, by giving firms control over workers' classification, there will also be jobs where long hours are inefficient and monitoring costs are low, but the firm chooses to make the job salaried anyway to increase the worker's hours. Overtime coverage would cause firms to reclassify such jobs as hourly, thereby decreasing hours towards their efficient level. Unlike the previous two cases, the employment effect is now ambiguous since overtime coverage increases the total surplus for some jobs and decreases it for others.

The model thus captures the tradeoffs that policymakers faced when designing the FLSA. Policymakers wanted to exempt jobs whose "work ... was difficult to standardize to any time frame and could not be easily spread to other workers", which I model as jobs where either the monitoring costs are high or long hours are efficient (U.S. Department of Labor, 2016a).

However, they added the overtime exemption threshold to prevent employers from using the salaried classification to bypass overtime protections for hourly workers who are working inefficiently long hours (Stein, 1940).

To summarize, the predictions of the job search and bargaining model differ depending on employers' "market power" over workers' hours and salaried/hourly status. In all cases, the model predicts a bunching mass at the overtime exemption threshold and a reduction in base pay. However, the decrease in base pay is most pronounced if firms and workers jointly bargain on all outcomes. If employers unilaterally set hours, then the model predicts that workers with long hours are most likely to receive a raise above the overtime exemption threshold rather than a decrease in base pay. This selection effect also suggests that the increase in overtime pay will be small because those who gain overtime coverage are chosen to have short hours. Lastly, if employers also control workers' salaried/hourly status, then I would expect firms to reclassify some jobs from salaried to hourly after they gain overtime coverage. Thus, through the lens of the model, the effects of expanding overtime coverage are informative about how wages, hours, and pay structure are determined in the labor market.

IV Data

I use anonymized monthly administrative payroll data provided by a large payroll processor. The company is 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 January 2014 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.[13] Since the data provider manages both W-2 and 1099-MISC tax forms for employers, the data contains information 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 payroll 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

[13] For observations prior to 2016, I use workers' state of residence to proxy for their state of employment. This approximation is often implicitly assumed in papers that use the Current Population Survey. Testing the validity of this assumption in the post-2016 data, I find that 95.5% of workers work in the same state that they live.

salary per pay-period and the number of weeks per pay-period.[14] 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.

The data also records employees' monthly overtime pay separate from their pay rate. To express monthly overtime pay in the same weekly denominator as base pay, I scale it by the number of paychecks received each month and the number of weeks per pay-period. Appendix D provides more detail on how I construct my measure of weekly overtime pay. 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 the data provider.

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 legally 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 supplement my analysis with survey data from the CPS Outgoing Rotation Group (ORG), 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 E, I explore the characteristics of firms affected by changes in the overtime exemption threshold and discuss the external validity of my estimates. I show four descriptive results. First, I find that these expansions in overtime coverage affect workers across all industries, but primarily impact large firms. Employers with at least one salaried worker in the interval of base pay targeted by the rule changes are about twice as large as the average firm in the sample. Second, previous work by Grigsby et al. (2021) shows that the data closely matches the demographics of workers in the Current Population Survey, but under-estimates employment in firms with under 50 and over 5000 employees relative to the Business Dynamic Statistics. Although the data is not perfectly representative of the US labor market, I show that at least 40% of workers are employed at a firm that uses a payroll software. Third, plotting the distribution of weekly base pay, I find that although the share of workers affected by any single policy change is small, the cumulative exposure to these multi-state threshold increases is quite sizable. Fourth, 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-

[14]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.

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 comply with the overtime regulations.

V Results

V.a Employment, Bunching, and Reclassification Effects

Empirical Strategy.

In this section, I estimate the impact of the overtime reforms on the distribution of weekly base pay. My analysis uses an event-study difference-in-difference design, leveraging 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.

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 three ways. First, I will show that outcomes in the treated and control states exhibit only small differential trends prior to the reform. Second, Appendix F implements the test by Roth and Sant'Anna (2023) and cannot reject the null that parallel trends holds throughout nearly the entire distribution of weekly base pay. Third, 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 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 specifically jobs within the interval targeted by the overtime threshold, but not jobs higher in the income distribution.

Formally, I estimate the following stacked difference-in-difference regression:

$$n_{jktv} = \sum_{\substack{t=-6 \\ t\neq -1}}^{5} \sum_{k=-6}^{15} \beta_{kt} \cdot I_{s(j)kt} + \alpha_{s(j)kv} + \delta_{ktv} + \varepsilon_{jktv} \tag{1}$$

where n_{jktv} is the number of workers employed in establishment j at event-time t, with base pay in bin k for event v. Since the data does not contain an establishment identifier, I define an establishment as a firm-state. I define each bin as a \$40 interval of nominal weekly base pay, normalized to 0 at the new threshold. The treatment dummy $I_{s(j)kt}$ equals 1 if establishment j is located in 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 ($\alpha_{s(j)kv}$) and month-bin-event (δ_{ktv}) 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.

There are three features of the regression that differ from traditional difference-in-difference designs. First, rather than defining employment at the employer level, I follow recent advancements in the minimum wage literature and measure employment at the establishment-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 consists of the treated state along with all the control states that have never increased their overtime exemption threshold yet up to 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 employers 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 observations in the control group:

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

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

For interpretability, after computing equation (1), I divide the β_k estimates by the average number of "affected salaried workers" across all event-establishments, which I define as the count of salaried workers between the old and new thresholds in the baseline month. The scaled estimates thus represent the change in employment relative to the number of workers targeted by the policy. To compute the aggregate employment effect, I sum the β_k estimates across both salaried and hourly jobs.

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.[15] 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, although there is a negative pre-trend prior to the policy change, it is relatively small compared to the broader policy effect. 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 salaried jobs, I find minor pre-trends, followed by a sharp divergence in the number of jobs below and above the threshold at exactly the month of the rule change. I discuss potential explanations for this spillover effect in Section V.d. In addition to the bunching effect, the figure also shows that the total number of hourly workers increased following the reform, suggestive of reclassification responses.

Contrary to a setting where firms and workers jointly bargain earnings and hours, or the compensating differentials model of overtime, I find no evidence that employers reduced workers' base pay to offset the overtime premium. The majority of the policy reforms only

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

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. Although I see a small increase in the number of jobs in the left tail of the distribution for hourly workers, this is due 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. Appendix G provides a detailed analysis of the potential interactions between the overtime exemption threshold and minimum wage, and shows that my results are robust to accounting for these interactions.

Table I summarizes the employment effects separately by salaried and hourly jobs. To minimize the effects of pre-trends, my preferred estimates focus on the impact in the month immediately after the reform.[16] Panel (a) shows that the number of salaried jobs in the affected interval of base pay falls by 21.2% (s.e. 5.9%). Among those jobs, 16 p.p. (s.e. 5.5) 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 below the new threshold by 0.07 (s.e. 0.026) job per affected salaried worker, 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 implies that employment falls by 2.4 (s.e. 2.4) 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 an employment 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 $\frac{\Delta \text{Employment}}{\text{Employment}_{t-1}}$) 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

[16] Appendix Table A.1 reports the estimates 5 months post-treatment. The results are qualitatively similar, but I observe a larger reclassification of jobs from salaried to hourly.

above 0.5%. Thus, while the policy did not achieve one of its original goals 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 employer 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 establishments within 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 suggest 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. Taken together, the evidence suggests that the aggregate employment effect was small relative to the total number of affected workers.

Appendix Table A.2 tests the robustness of my results to alternative methods of inference. Column (1) shows that the cluster wild bootstrap rejects a null bunching mass with 88% confidence (Cameron et al., 2008). However, MacKinnon and Webb (2018) show that while the restricted wild cluster bootstrap improves on conventional clustered standard errors when there are a small number of clusters, it tends to under-reject when there are only a few *treated* clusters, as in my setting. Column (2) instead uses the asymptotic method of Ibragimov and Müller (2016), estimating a separate difference-in-differences for each event and applying a t-test to the mean of the coefficients. This approach yields precise evidence of bunching for both salaried and hourly jobs. Column (3) repeats this procedure but computes stacked difference-in-differences estimates for each treated state to account for correlated errors across events in the same state. The estimates become less precise, but I remain 85% confident of a positive bunching mass. In all cases, I find fairly small employment effects. Overall, the bunching and employment estimates are robust across inference procedures, albeit with varying degrees of precision.

Decomposing the Changes in the Number of Workers.

The results thus far indicate that raising the overtime exemption threshold raises workers' base pay with little net employment effects. However, changes in the stock of workers can mask changes in separation and hiring rates. Moreover, it is unclear from the employment counts alone how much of the decrease in salaried jobs can be explained by employers reclassifying salaried workers to hourly.

In this section, I exploit the panel structure of the data to decompose the change in the number of workers at each firm-classification-pay bin into three flows: net hires from outside the firm, net reclassifications across salaried and hourly status within the firm, and base pay growth within the firm-classification. Let n_{jrkt} represent the number of workers at a firm j, with an hourly/salaried classification r, and a bin of base pay k, in time t. The change in the stock of workers between time t and $t+1$ can be decomposed into three margins of response:

$$\Delta n_{jrk} = \underbrace{\sum_{j' \neq j} (m_{j' \to jrk} - m_{jrk \to j'})}_{\text{Hires minus Separations}} + \underbrace{\sum_{r' \neq r} (m_{jr' \to jrk} - m_{jrk \to jr'})}_{\text{Reclassification}} + \underbrace{\sum_{k' \neq k} (m_{jrk' \to jrk} - m_{jrk \to jrk'})}_{\text{Wage Growth}}$$

where $m_{j' \to jrk}$ denotes the number of workers who moved from firm j' or unemployment into a job at firm-classification-pay jrk. Likewise, $m_{jr' \to jrk}$ is the number of reclassifications from r' to rk within the same firm j, and $m_{jrk' \to jrk}$ are changes in base pay from k' to k within the same firm-classification jr. The flows in reverse are defined similarly. Intuitively, if I observe an increase in the number of workers at a specific firm-bin-classification, then those workers must have either come from outside the firm, another worker classification within the same firm, or a different bin within the same firm-classification. I measure each of these flows directly using the employer-employee panel structure of the data, and use the number of such flows as the outcome variable in equation (1).

Figure 3 plots the effect of raising the overtime exemption threshold on net employment flows (i.e. hires minus separations) and net reclassification flows. In panel (a), I find that in the month of the reform, employment flow of salaried jobs moves from below the new threshold to above it. However, this effect does not persist over time, suggesting that the increase in separations below the threshold was only a temporary adjustment response. 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 flows after the reform. The minor effect on salaried employment flows suggests that the decrease in salaried jobs observed earlier in Figure 2 is at least partially 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 model when firms having control over workers' pay structure, I observe a sharp increase in the number of jobs being reclassified from salaried to hourly at precisely the month that the new overtime exemption threshold goes into effect. This impact only 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 II summarizes the effect of raising the overtime exemption threshold on employment and reclassification flows. Given that most of the flow responses occurred precisely in the month of the policy reform, I report the β_{tk} estimates from equation 1 for $t = 0$, divided by the number of affected salaried workers. Columns (1)-(3) 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 (column 3), with a confidence bound between -1 and -0.2 jobs per 100 affected salaried positions. Half of the reduction is driven by a decrease in hires and half by an increase in separations. However, the second row shows that about two-thirds of the drop in employment below the threshold can be accounted for by new jobs above it.

As for hourly jobs, the third and fourth 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 by the figures, there is already a 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 0.022 (s.e. 0.029) 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 I calculated using changes in the stock of workers. An advantage of decomposing the flows though is that I show that the net employment flow to salaried jobs is a precisely estimated zero, and the entire negative employment response is driven by noisy turnover among hourly workers.

Relative to the employment effects, the change in reclassification flows is very precisely estimated even for small responses. The first two rows correspond to panel (c) and (d) of Figure 3 where I observe a net flow of jobs from salaried to hourly primarily among jobs being paid below the new threshold. 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. The magnitude of the estimates suggest that employers reclassify approximately 2.3% of affected workers from salaried to hourly. This reclassification effect is consistent with the aggregate estimates in Table I, which found that hourly employment increased by 2.7 jobs per 100 affected salaried workers. This decomposition thus suggests that the reclassification effect explains most of the increase in hourly employment and approximately half the decrease in salaried jobs. Although reclassifications have no impact on the overtime eligibility of workers earning below the overtime exemption threshold, they enable employers to deduct workers' pay if they work less than 40 hours per week instead of simply paying a fixed weekly salary.

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. The model suggests that such reclassifications are surplus enhancing if employers were initially classifying hourly workers as salaried to avoid paying overtime for inefficiently long hours. In terms of magnitudes, the reclassification effect is much smaller than the bunching effect. Taken together with the results from Table I, the estimates suggest that three quarters 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. As in Section V.a, 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

$$y_{ivt} = \sum_{t=-6}^{5} \beta_t \cdot I_{st} + \alpha_{vs} + \delta_{vt} + \varepsilon_{ivt} \tag{2}$$

where y_{ivt} is the weekly earnings of worker i 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 treatment and control groups 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. Second, since I do not observe the wages of workers in firms not using payroll processing 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 only found small separation effects, this is likely a minor issue. Nevertheless, I show that despite potential differences in composition, workers in the treated and control states still had 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 the increase in weekly overtime pay. I note three features from the analysis for salaried workers. 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.

Through the lens of the job search and bargaining model, the rise in salaries rejects the case where firms and workers jointly bargain over hours. Similar to the compensating differentials framework, joint bargaining predicts that base pay would decrease to nullify the overtime premium. Instead, the relatively small increase in overtime pay is consistent with a model in which employers control workers' hours. In that scenario, individuals who work long hours are bunched above the overtime exemption threshold to avoid distorting their hours. As a result, the policy is predicted to have little effect on overtime pay as workers who gain coverage are selected to have short workweeks.

Turning to hourly workers, I find a similar pattern of parallel pre-trends, followed by a sharp increase in earnings that persists for 3 months after the reform. 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 temporary positive impact on hourly workers' overtime earnings.

To validate the firm level results, Appendix Figure A.3 plots the effect of raising the overtime exemption threshold on indicator variables for workers' salaried/hourly status and their income in relation to the old and new thresholds. Consistent with the firm-level results, I find that incumbent workers are 19% less likely to be within the treated interval, 17% more likely to earn above it, and 2% more likely to be reclassified as hourly.[17]

Table III summarizes the estimates of the income effects of expanding overtime coverage. All estimates are computed from equation (2), and reported for the first month of the reform to ease computational burden. In column (1), 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 to workers are 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. 0.4%). 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 clear 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 (5) and (6) 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) drop 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 suggests 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. 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. The increase in base pay also implies that government statistics often underestimate the cost of overtime coverage to employers. For instance, surveys by the Department of Labor find that overtime pay is only about 0.8% of total labor costs (Bureau of Labor Statistics, 2019). However, this figure

[17] The analysis focuses solely on stayers so there are no separations. Section V.e will discuss separations in more detail.

is calculated ex-post after employers already adjusted base pay and hours to minimize the costs of overtime. In my setting, employers' labor costs only increased by 0.07% from the rise in overtime pay (i.e. $\frac{0.648}{868}$). In comparison, average workers' earnings increased by 1.4%, suggesting that firms would be willing to pay *at least* that much to avoid overtime coverage, nearly twice the ex-post cost of overtime reported by the BLS and 20 times larger than the policy's effect on overtime pay.

Moreover, firms' willingness to pay varies significantly across workers, as not all workers received a raise above the overtime exemption threshold. I conduct a simple calculation to quantify the increase in income among workers who got bunched. In theory, the only reason firms would raise base pay in response to the overtime policies is to keep 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 I shows that about 16% of affected salaried workers explain the entire \$11.87 increase in weekly base pay. Together, the estimates imply that the weekly base pay of bunched workers increased by $\frac{\$11.87}{0.16} = \$74.19$. Given a baseline weekly salary of \$868, the simple calculation suggests that the earnings of the 16% of affected workers increased by approximately 8.5%, while the remaining affected workers only experienced minor increases in 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. As descriptive evidence, Appendix H uses the payroll data to test a key prediction of models in which firms dictate hours: bunching in the distribution of weekly hours. Plotting the distribution of average weekly hours for all hourly workers in the baseline month, I find a clear bunching mass at precisely 40 hours per week. The bunching is consistent with recent evidence from Goff (2020) who finds a similar result using administrative payroll data at the weekly level. This 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; Lachowska et al., 2023).[18]

Although 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. On the other hand, those who receive a raise above the overtime exemption threshold may actually

[18]In contrast to the bunching at 40 hours, I find no bunching mass below the 30-hour cutoff that triggers employer health insurance obligations under the Affordable Care Act (ACA), suggesting the ACA is unlikely to factor into employers' response to expansions in overtime coverage.

be asked to work more hours to compensate. However, if there is imperfect competition and workers consider higher earnings and fewer hours as sufficient complements, then it is even possible that hours fall after firms increase workers' salaries (Dube et al., 2022).

To empirically examine the effect on workers' hours, Appendix I supplements the analysis using the CPS Outgoing Rotation Survey, which measures respondents' weekly hours worked. To increase my sample size, I expand the stacked difference-in-difference analysis to 49 state overtime threshold changes between 2008 and 2026. Despite the smaller sample size and less precise estimates, the CPS successfully replicates the positive earnings effect identified in the payroll data. However, I cannot reject that raising the overtime exemption threshold had no effect on workers' hours, suggesting that firms may not have increased hours to offset the increase in workers' earnings. Nevertheless, a direct test of the effect on hourly wages using the CPS is too imprecise to draw meaningful conclusions.

V.d Heterogeneity

This section examines how the effects of raising the overtime exemption threshold vary across policies. Figure 5 plots the difference-in-difference estimates of the employment effects, separately by event. I exclude Maine from the plot as its estimates are imprecise, but I keep it when computing statistics in the top right corner of each panel. Consistent with the results from the stacked difference-in-difference design, Panel (a) finds a net decrease in salaried employment across all events, except for the two in Alaska. I report an average treatment effect that weights each event equally by collapsing the data to the state level before estimating a stacked difference-in-difference. The estimate indicates that about 3.8 salaried jobs are lost for every 100 salaried workers between the old and new overtime exemption thresholds at baseline. A joint F-test rejects with over 99% confidence that all the estimates equal 0. I also report the bias-corrected variance across the 19 estimates, computed as the difference between the empirical variance and the average squared standard error of the effects. The between-events variance of 0.0014 implies that 95% of case studies would find an employment effect between -0.11 and 0.04 per affected salaried worker. Overall, the meta-analysis shows that expanding overtime coverage decreases salaried employment.

On the other hand, Panel (b) shows that most events caused an increase in hourly employment, consistent with employers reclassifying jobs from salaried to hourly. Although the average effect on hourly employment and the variance between-events are imprecisely estimated, the F-test rejects that all the estimates are zero. Summing together the salaried and hourly employment effects, Panel (c) finds that the majority of events have a negative employment effect, although there remains substantial heterogeneity between case studies. Some of this heterogeneity is attenuated in panel (d) when I include hourly workers in the definition of affected workers. Nevertheless, the 95% prediction interval in the last panel suggests that the employment effect lies within ±4%, centered around -0.2%.

Appendix J explores the heterogeneity across events in more detail. I document three results. First, while most event-study estimates for salaried employment satisfy the parallel trends assumption, pre-trends in hourly employment are most stable in California, Colorado, and Washington. In the remaining states, pre-existing trends are especially problematic when the policy changes are small because then the treatment effect would also be small and my estimates would primarily be capturing the pre-trend bias. This bias explains the large outlier estimate for New York 2016, which was the smallest policy change in the sample, raising the threshold by only $18.75. Nevertheless, the results remain unchanged if I restrict the stacked difference-in-difference regression to only the events for which the parallel trends assumption holds, or if I use a synthetic difference-in-difference design to match on pre-trends.

Second, I test whether policy parameters and state labor market conditions predict the heterogeneity in responses across events. I find that the magnitude of the new threshold is the strongest predictor of the bunching effect, suggesting that employers are less willing to cover high paying jobs for overtime. In contrast, neither the magnitude of the new threshold, the share of workers affected, the percent change in the threshold, nor the state unemployment rates predict the magnitude of the net employment effect.

Third, I find that the entire spillover effect among hourly jobs is driven by the seven events in California. Appendix K shows that the California-specific spillover effect cannot be explained by policy differences or by compensating differentials to appease reclassified workers. Although I am unable to explain why the spillover onto hourly jobs only occurs in California, I do find evidence to rule out common mechanisms raised in the minimum wage literature, such as the policy raising outside options, changing labor supply, or leading to up-skilling. Instead, I find suggestive evidence that internal fairness norms explain some, but not all, of the spillover effects.

V.e Extensions: Long-run Effects, Mechanisms, and Worker Welfare

The appendix contains several extensions of my main analysis. First, Appendix L uses California as a case study of the long-run effects of raising the overtime exemption threshold. Following firms from 2014 to 2017, I find that the reclassification effect accumulates with each successive threshold increase while the bunching mass shifts to the right. After three reforms, 36% of affected salaried jobs in California were reclassified as hourly. Meanwhile, I find no evidence of a decrease in employment. At the worker level, I find that incumbents' base pay increased by 4.5% over four years, with no evidence of slower nominal wage growth during intermittent periods between threshold changes. This suggests that the positive wage effect is persistent in the long-run. However, the effects are heterogeneous across workers. Some incumbents receive repeated salary increases as they are bunched after each successive reform, whereas others are bunched initially but reclassified following a later increase. Taken together, the clearest distinction in the long-run relative to the short-run is a larger substitution toward hourly employment for firms, and an accumulation of pay increases for workers.

Second, Appendix M uses the CPS data to examine the potential margins of response available to employers. I show that the majority of affected salaried workers report working no more than 40 hours per week, implying that the expected cost of the overtime premium is relatively small for most jobs. However, among workers who regularly work overtime, it is often substantially cheaper for firms to raise salaries above the exemption threshold than to directly pay the overtime premia or reduce hours. These descriptive patterns help rationalize the heterogeneity in firm responses documented in the main analysis: many workers remain salaried and below the threshold, while a subset of workers are bunched above the new cutoff.

Third, Appendix N provides suggestive evidence on whether raising the overtime exemption threshold is a net benefit for workers. To benchmark the policy against other labor market interventions, I estimate the employment elasticity with respect to earnings, analogous to the own-wage employment elasticity studied in the minimum wage literature. The point estimate implies that a 1 percent increase in affected workers' earnings reduces employment by about 0.55 percent. Although the estimate is more negative than common results in the minimum wage literature, the 95% confidence interval is fairly imprecise, ranging from -1.58 to 0.48. As an alternative measure of worker welfare, I examine the effect on separation rates and find that 0.3% of affected incumbents leave their firm in the month of the policy change. These findings suggest that at least some incumbents were made worse off either through involuntary job loss or because their jobs became less desirable. In contrast, the literature generally finds that increases in minimum wage reduce separations (Brochu and Green, 2013; Dube et al., 2016). Thus, although a complete normative assessment of the policy is beyond the scope of the paper, the employment-to-earnings elasticity and increase in separations suggest that expansions in overtime coverage may have less favorable effects on workers than comparable pay increases from the minimum wage.

VI 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 forecast 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, 2016a). 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 approximately 2.3% of affected workers are reclassified from salaried to hourly, and the policy appears to have noticeable spillover effects on the income of hourly workers. This reclassification effect also accumulates in the long-run, leading to more hourly jobs and fewer salaried ones.

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 consumption (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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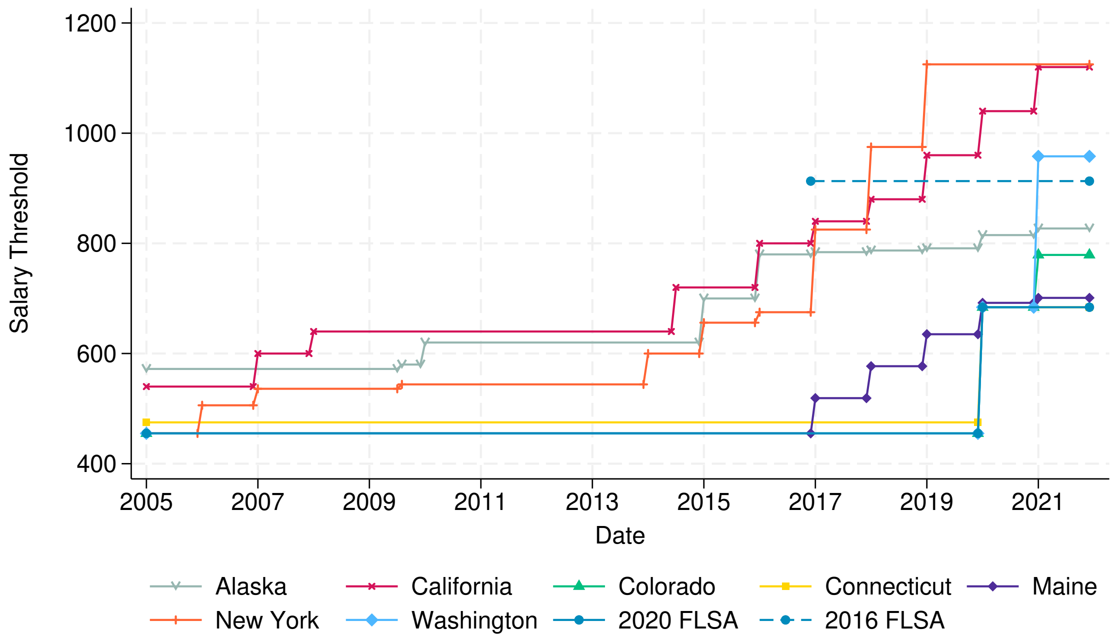
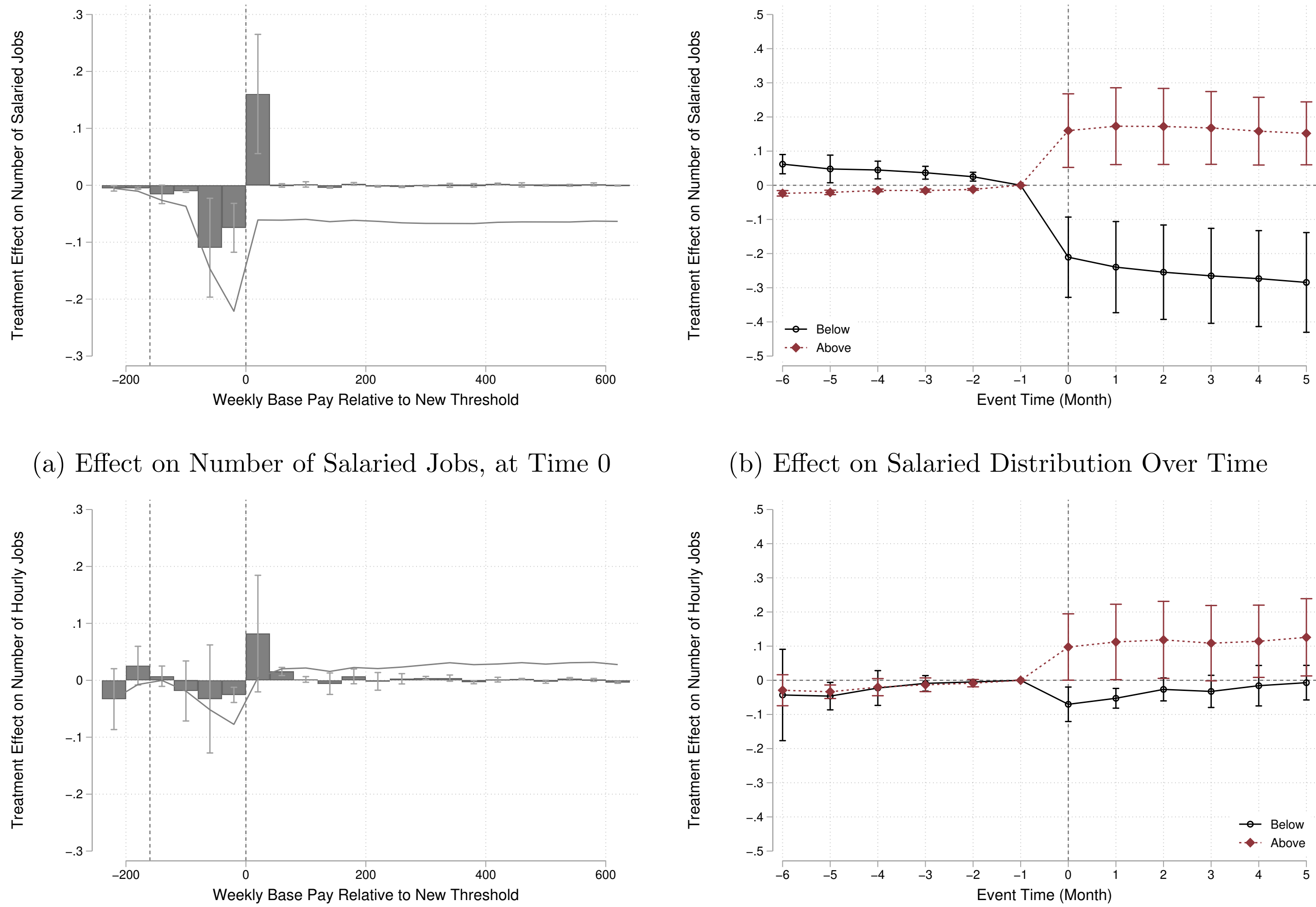


Figure 1
Variation in Overtime Exemption Thresholds Across States

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 vary 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

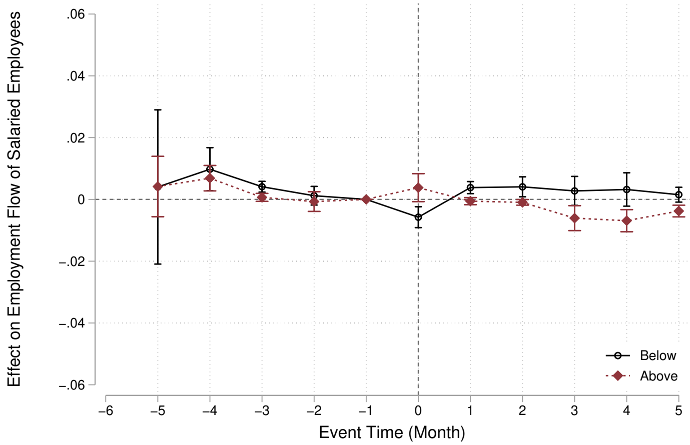
(c) Effect on Number of Hourly Jobs, at Time 0

(d) Effect on Hourly Distribution Over Time

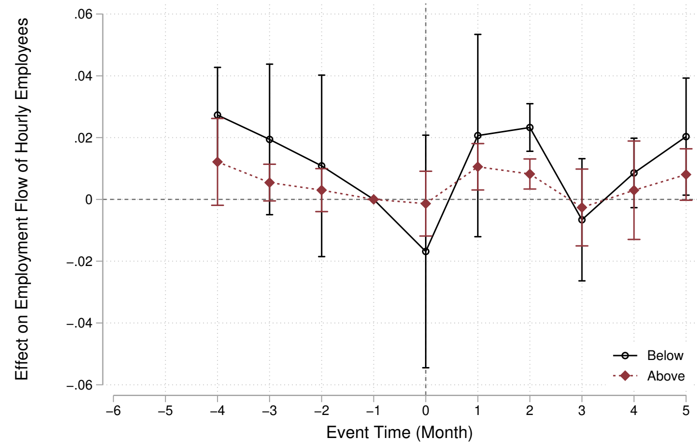
Figure 2

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

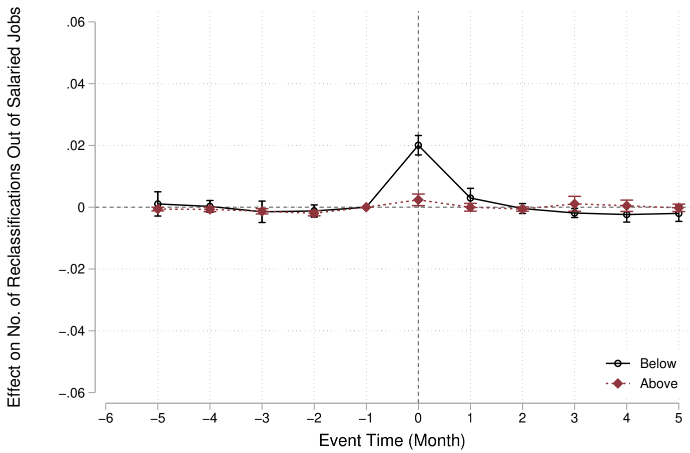
Notes. Panel (a) shows the event study estimates from equation 1. The height of each bar indicates the effect of raising the OT exemption threshold on the number of salaried jobs in each \$40 bin of base pay on the month that the new threshold becomes binding, divided by the baseline number of salaried 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.



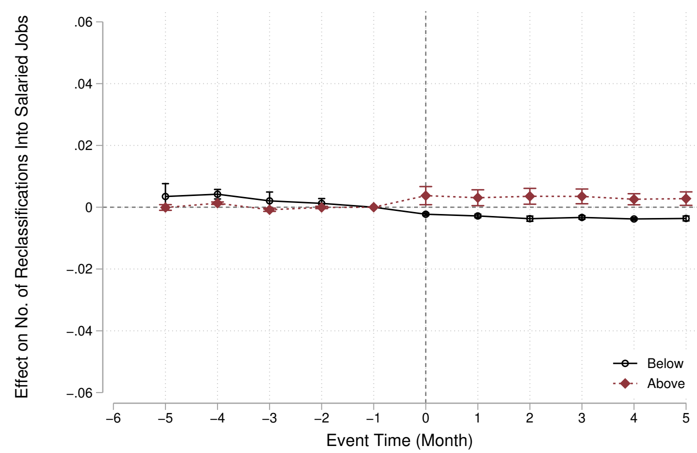
(a) Net Salaried Employment Flows



(b) Net Hourly Employment Flows



(c) Reclassification Flows Out of Salaried

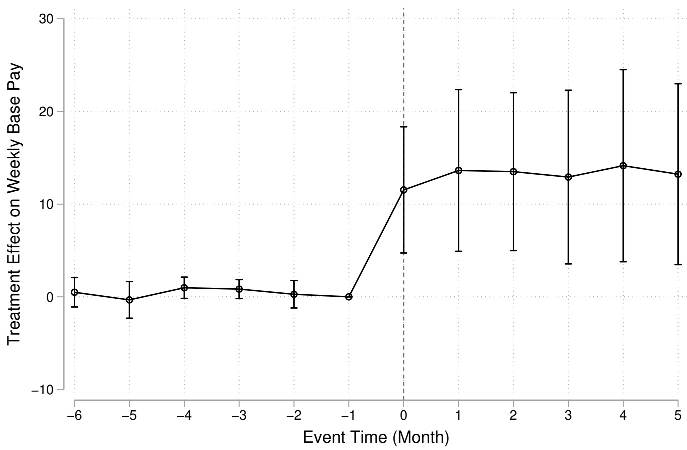


(d) Reclassification Flows Into Salaried

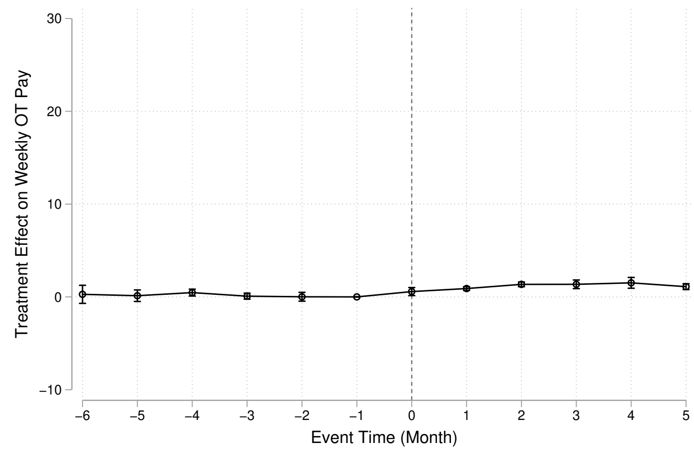
Figure 3

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

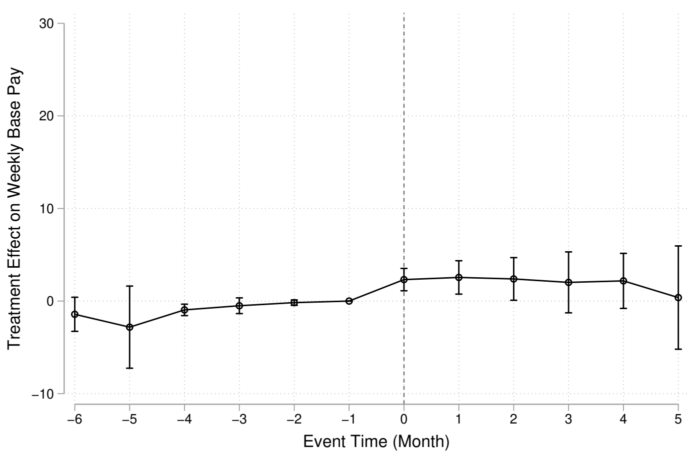
Notes. Panel (a) plots the effect of the state threshold changes on the net employment flow of salaried employees for each month since the threshold increased, scaled by the baseline number of salaried workers between the old and new thresholds. Panel (b) plots the analogous figure for net employment 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.



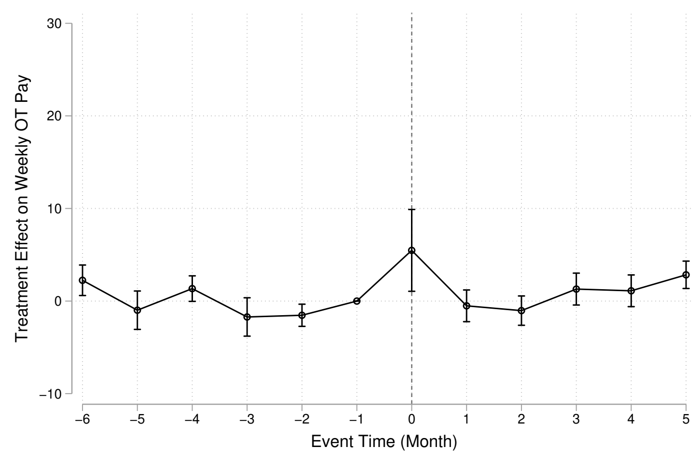
(a) Salaried: Weekly Base Pay



(b) Salaried: Weekly OT Pay



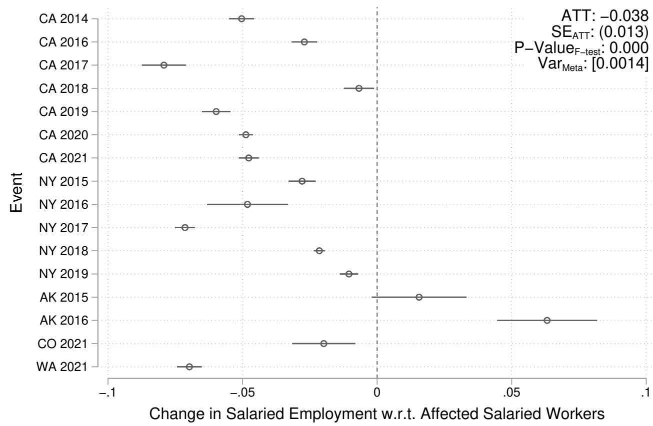
(c) Hourly: Weekly Base Pay



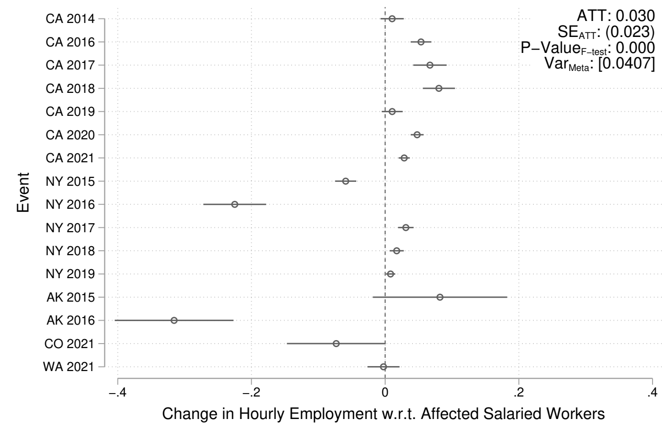
(d) Hourly: Weekly OT Pay

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

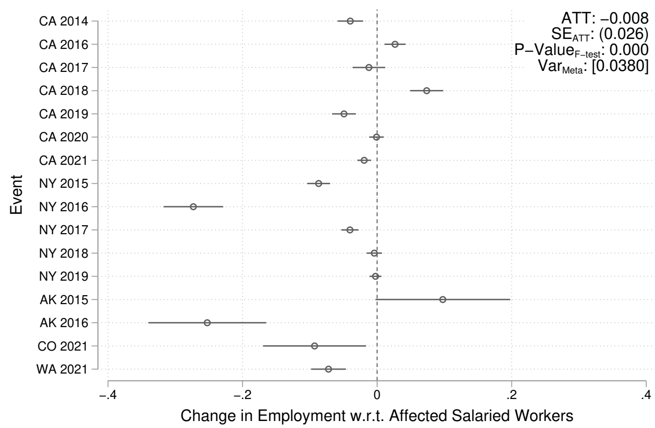
Notes. Panels (a)-(d) show the effect of raising the overtime exemption threshold on base pay and overtime pay, separately for salaried and hourly workers initially earning between the old and new thresholds. All estimates are computed from equation 2. Standard errors are clustered by state.



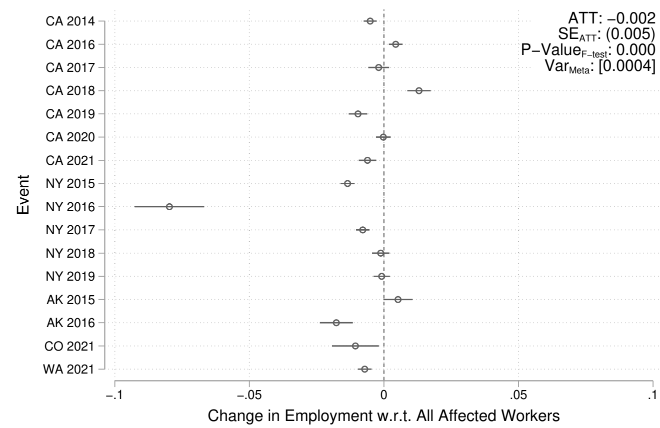
(a) Salaried Jobs



(b) Hourly Jobs



(c) All Jobs, per Affected Salaried Worker



(d) All Jobs, per Aff. Sal & Hr Worker

Figure 5

Event-Specific Employment Effects Scaled

Notes. The figure shows the difference-in-difference estimates from equation 1, separately for each event except the three from Maine. Panels (a)-(c) report changes in employment relative to the number of salaried workers between the old and new thresholds in the month before each policy change. Panel (d) divides the treatment effect by the total number of salaried and hourly workers in the affected pay interval. The bars around each estimate represent 95% confidence intervals. Standard errors are clustered by state. The top right of each figure reports a stacked difference-in-difference estimate that weights each event equally, the associated standard error, the p-value from an F-test that the estimates are jointly 0, and a variance of the estimates between events.

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

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A						
Salaried Jobs						
Below Threshold	-0.212***	-0.151***	-0.206***	-0.216***	-0.209***	
	(0.059)	(0.032)	(0.061)	(0.056)	(0.062)	
Above Threshold	0.160***	0.109***	0.159***	0.164***	0.153**	
	(0.055)	(0.030)	(0.056)	(0.054)	(0.062)	
Hourly Jobs						
Below Threshold	-0.070***	-0.041*	-0.027	-0.077***	-0.009	
	(0.026)	(0.023)	(0.022)	(0.023)	(0.017)	
Above Threshold	0.097*	0.065**	0.088**	0.102**	0.080*	
	(0.050)	(0.026)	(0.042)	(0.049)	(0.046)	
Panel B						
Emp. w.r.t. Aff. Salaried	-0.024	-0.019	0.014	-0.027	0.015	-0.024
	(0.024)	(0.013)	(0.019)	(0.025)	(0.035)	(0.024)
Emp. w.r.t. All Affected	-0.005	-0.004	0.003	-0.006	0.003	-0.005
	(0.005)	(0.003)	(0.004)	(0.005)	(0.008)	(0.005)
Number of Affected Salaried	1.076	1.076	1.097	1.150	1.198	1.076
Number of Affected Hourly	4.208	4.208	4.268	4.233	4.378	4.208
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, relative to the number of affected salaried jobs. Affected jobs are defined as salaried employees with base pay between the old and new thresholds on the month before 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 II
Event-Study Estimates of Employment Flow and Reclassification Effects

	Employment			Reclassification		
	Hires	Separations	Net	Into	Out of	Net
Salaried Below	-0.003***	0.003	-0.006***	-0.002***	0.021***	-0.023***
	(0.001)	(0.003)	(0.002)	(0.000)	(0.002)	(0.002)
Salaried Above	0.006***	0.002**	0.004**	0.004***	0.002**	0.001
	(0.001)	(0.001)	(0.002)	(0.001)	(0.001)	(0.001)
Hourly Below	-0.002	0.017*	-0.019	0.019***	0.002**	0.018***
	(0.011)	(0.010)	(0.020)	(0.002)	(0.001)	(0.002)
Hourly Above	0.003	0.005**	-0.001	0.005***	0.000	0.005***
	(0.005)	(0.002)	(0.005)	(0.001)	(0.000)	(0.001)
Cumulative	0.005	0.027*	-0.022	0.026***	0.025***	0.001
	(0.015)	(0.014)	(0.029)	(0.004)	(0.003)	(0.001)
Treatment Group						
Number of Affected Salaried	1.076	1.076	1.076	1.076	1.076	1.076
Number of Affected Hourly	4.208	4.208	4.208	4.208	4.208	4.208
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 relative to the average number of salaried jobs with base pay between the old and new thresholds in the month before the rule change (row 6). For comparison, row 7 reports the baseline number of hourly jobs within the same range of weekly base pay at the average employer. 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 III
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	11.867***	2.712***	7.392***	1.921***	9.454***	1.867***	11.824***	2.838***
	(3.658)	(0.774)	(1.853)	(0.369)	(2.519)	(0.415)	(3.834)	(0.861)
Weekly OT Pay	0.648**	5.620**	0.324**	4.684***	0.238*	1.381	0.642**	5.634**
	(0.252)	(2.262)	(0.128)	(1.531)	(0.128)	(1.009)	(0.264)	(2.498)
Weekly Total Pay	12.515***	8.331***	7.715***	6.605***	9.692***	3.248***	12.466***	8.472***
	(3.898)	(1.563)	(1.933)	(1.177)	(2.571)	(0.838)	(4.086)	(1.687)
% Δ Total Pay	0.014***	0.009***	0.009***	0.007***	0.011***	0.004***	0.014***	0.009***
	(0.004)	(0.002)	(0.002)	(0.001)	(0.003)	(0.001)	(0.005)	(0.002)
Log Total Pay	0.013***	0.008***	0.008***	0.006***	0.010***	0.003***	0.013***	0.008***
	(0.004)	(0.001)	(0.002)	(0.001)	(0.003)	(0.001)	(0.004)	(0.001)
Baseline Weekly Base Pay	868	812	868	812	875	818	871	818
Baseline Weekly OT Pay	7	78	7	78	10	85	8	80
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)	181,645	665,297	181,645	665,297	93,207	382,827	88,882	342,564
N (control)	2,296,506	8,908,133	2,296,506	8,908,133	1,430,870	6,508,487	1,153,747	4,160,458
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) divides 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.

Column (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 thresholds 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) drop 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.

Appendix: For Online Publication

Appendix A. Additional figures and tables

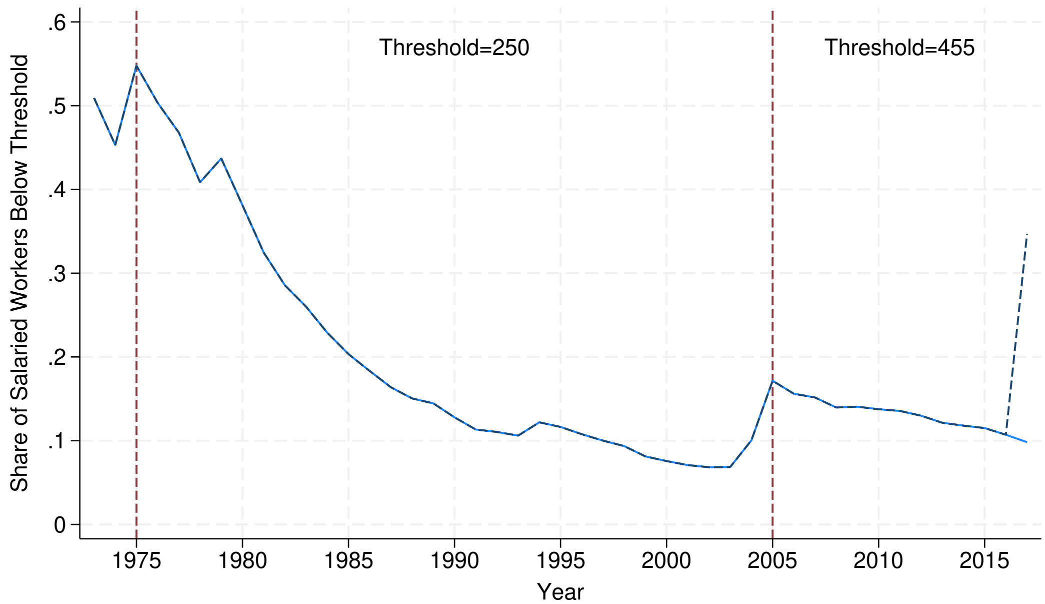


Figure A.1
Percent of Salaried Workers Earning Below the FLSA OT Exemption Threshold

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.

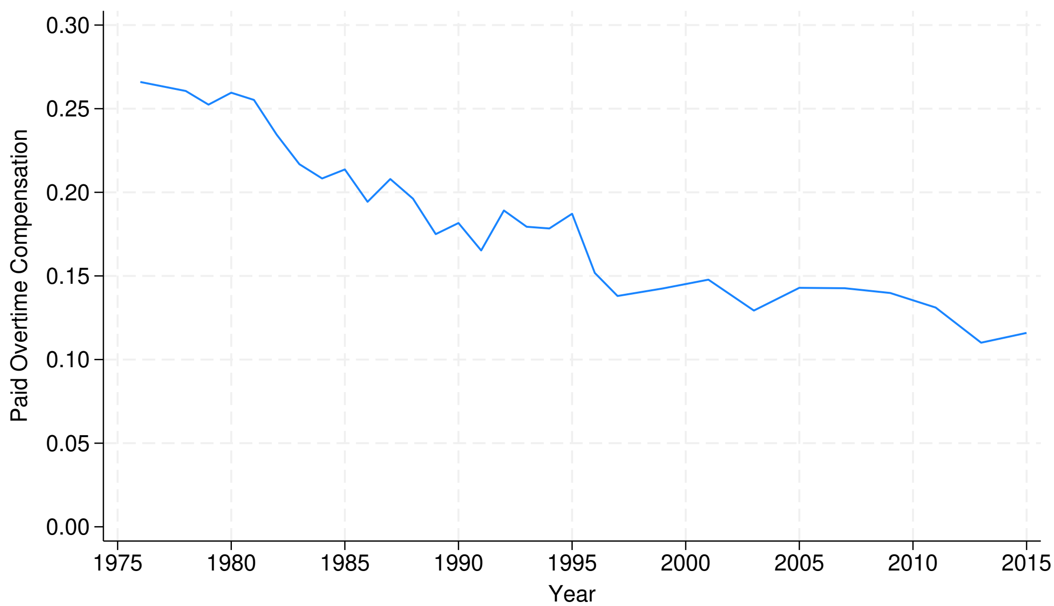
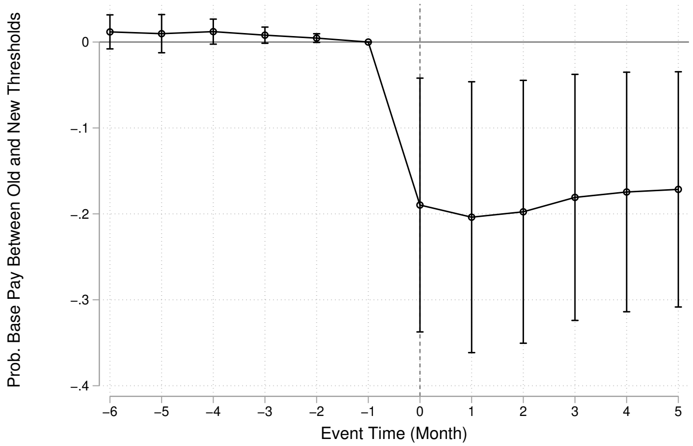
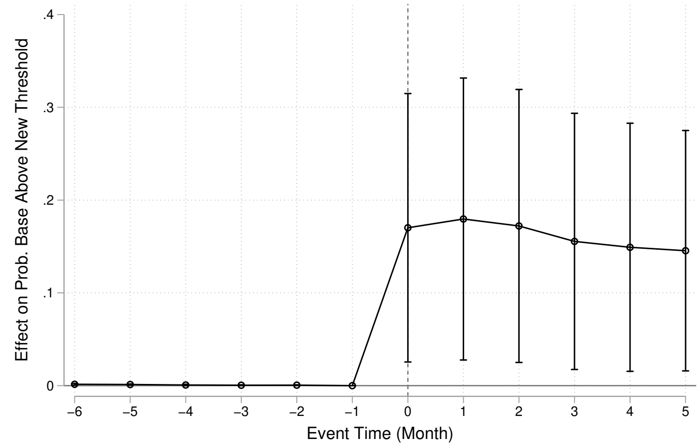


Figure A.2
Percent of Salaried Workers Eligible for Overtime

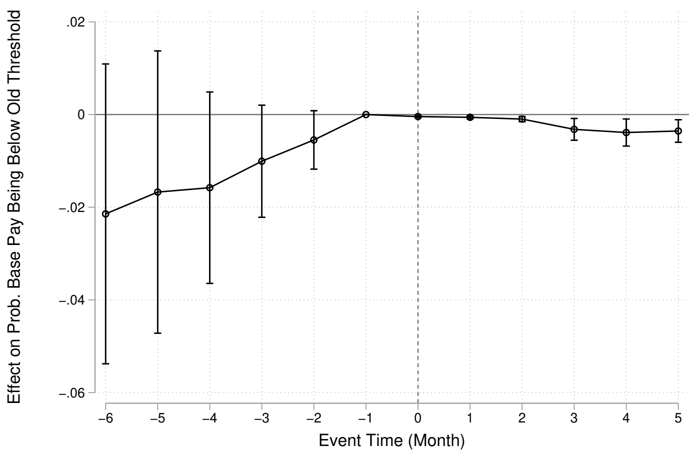
Notes. This figure shows the percent of salaried workers in the PSID who work at least 40 hours a week and respond yes to the question ”If you were to work more hours than usual during some week, would you get paid for those extra hours of work”.



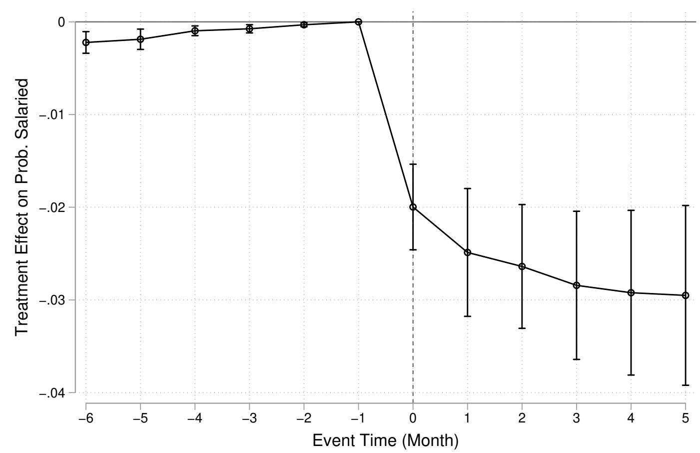
(a) Effect on Probability Within Thresholds



(b) Effect on Probability Above New Threshold



(c) Effect on Probability Below Old Threshold



(d) Effect on Probability Salaried

Figure A.3

Effect of Raising States' OT Exemption Thresholds on Incumbents' Place in the Salaried/Hourly Distributions

Notes. This figure plots the effects of raising the overtime exemption threshold on workers initially earning between the old and new thresholds. Panels (a)-(c) plot the effect on an indicator for still being salaried and earning within, above, and below those thresholds, respectively. Panel (d) plots the effect on an indicator for being salaried. For each estimate, I show the 95% confidence interval using standard errors clustered by state.

Table A.1
Effect of Raising the Overtime Exemption Threshold on the Pay Distribution After 5 Months

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A						
Salaried Jobs						
Below Threshold	-0.284***	-0.206***	-0.287***	-0.292***	-0.301***	
	(0.074)	(0.036)	(0.076)	(0.069)	(0.078)	
Above Threshold	0.152***	0.107***	0.140***	0.155***	0.134***	
	(0.047)	(0.025)	(0.045)	(0.046)	(0.046)	
Hourly Jobs						
Below Threshold	-0.007	0.015	0.044	0.004	0.102**	
	(0.026)	(0.033)	(0.032)	(0.022)	(0.047)	
Above Threshold	0.126**	0.074***	0.112**	0.130**	0.118**	
	(0.058)	(0.022)	(0.049)	(0.057)	(0.053)	
Panel B						
Emp. w.r.t. Aff. Salaried	-0.013	-0.010	0.009	-0.003	0.052	-0.014
	(0.024)	(0.029)	(0.020)	(0.018)	(0.041)	(0.025)
Emp. w.r.t. All Affected	-0.003	-0.002	0.002	-0.001	0.011	-0.003
	(0.005)	(0.006)	(0.004)	(0.004)	(0.009)	(0.005)
Number of Affected Salaried	1.076	1.076	1.097	1.150	1.198	1.076
Number of Affected Hourly	4.208	4.208	4.268	4.233	4.378	4.208
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, relative to the number of affected salaried jobs. Affected jobs are defined as salaried employees with base pay between the old and new thresholds on the month before 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 5 months after 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 A.2
Robustness to Method of Inference

	(1)	(2)	(3)
Panel A			
Salaried Jobs			
Below Threshold	-0.212	-0.141	-0.09
	[.151]	[.001]	[.123]
		(.037)	(.049)
Above Threshold	0.160	0.113	0.065
	[.118]	[.004]	[.143]
		(.034)	(.038)
Hourly Jobs			
Below Threshold	-0.070	-0.062	-0.004
	[.341]	[.001]	[.901]
		(.016)	(.031)
Above Threshold	0.097	0.063	0.029
	[.27]	[.009]	[.371]
		(.021)	(.029)
Panel B			
Emp. w.r.t. Aff. Salaried	-0.024	-0.027	0
	[.637]	[.007]	[.998]
		(.009)	(.026)
Emp. w.r.t. All Affected	-0.005	-.006	0
	[.637]	[.007]	[.998]
		(.002)	(.005)
Method	CGM (2008)	IM (2016)	IM-im2
Number of Affected Salaried	1.076	1.076	1.076
Number of Affected Hourly	4.208	4.208	4.208
Event-Bin-Month FE	Y	Y	Y
Event-Bin-State FE	Y	Y	Y

Notes. This table reports the employment effects of raising the overtime exemption threshold using three methods. Column (1) reports the stacked difference-in-difference estimate from equation (1), with p-values computed using wild clustered bootstrap. Column (2) reports estimates using the method by Ibragimov and Müller (2016) where I estimate a difference-in-difference separately for each event and then compute a t-test that the average coefficient is non-zero. Column (3) applies Ibragimov and Müller (2016), estimating a stacked difference-in-difference for each of the 6 treated states. P-values are reported in square brackets, and standard errors in round brackets.

Appendix B. Job Search and Bargaining Model

In this section, I present a job search and bargaining model that nests the labor demand and compensating differential models of overtime, while also incorporating key institutional features of the overtime exemption threshold.[19] The model builds on the framework of Carry (2022), which incorporates bargaining over hours and wages. The key addition I make to that model is the distinction between salaried and hourly jobs. Given that I use the model solely to illustrate how economic incentives change following overtime coverage, I simplify multiple features of the model by Carry (2022) to focus on the core mechanisms.

The model iterates through the following steps. First, employers decide how many vacancies to post. Second, unemployed workers search for jobs and randomly match with a vacancy. Third, the firm and worker bargain over the salaried/hourly classification and base pay of the job. A job is created when the firm and worker surplus are both positive. Fourth, the employer unilaterally sets the worker's hours.[20] Lastly, a share of jobs are exogenously destroyed. I describe each of these steps below.

B.a Preferences and Production Technologies

Employers. I assume there exists a technologically determined distribution of jobs that vary in productivity, hour constraints, and monitoring costs, drawn from the joint distribution $F(y, z, r)$.

Following Carry (2022), I assume firms' production function is given by the following form:

$$f(h|y,z) = \begin{cases} yh^{\alpha} \text{ if } h < z, \\ yz^{\alpha} \text{ otherwise} \end{cases} \tag{3}$$

where y is the productivity of the job and h is an endogenous outcome for the number of work hours. Each job has a maximum number of productive hours z, representing the number of hours necessary to complete a task, after which the marginal productivity drops to 0. I assume the parameter α is common to all jobs and captures returns to scale.

[19] The one feature of the labor demand model that I do not nest is the possibility of work-sharing because allowing production to depend non-linearly on the number of workers would make the bargaining solution far more complicated without improving the ability of the model to qualitatively fit the empirical results.

[20] I consider alternative ways of determining hours and pay classification in Appendix C.

The cost of employing a worker will depend on whether the job is salaried or hourly.

$$q(w,s,h) = \begin{cases} wh + 0.5w \cdot \max(h-40,0) \text{ if hourly} \\ s + 1.5\frac{s}{40} \cdot \max(h-40,0) \cdot 1_{s<\bar{s}} \text{ if salaried} \end{cases} \tag{4}$$

Hourly employees receive a wage w for each hour of work and earn a 50% pay premium for hours worked above 40. Salaried employees receive a fixed base pay s regardless of their hours and only qualify for an overtime of one-and-a-half times their implied wage if their base salary s is below a threshold $\bar{s}$.

Firms' profit is given by

$$\pi(s,w,h|y,z,r) = f(h|y,z) - q(w,s,h) - r \cdot 1_{s<\bar{s} \text{ or } hourly} \tag{5}$$

where r represents the cost of complying with the record-keeping requirements for non-exempt workers. When an employee is covered for overtime, the employer has to either record the exact hours the individual works each day, or set a fixed weekly schedule and show that the employee follows that schedule (U.S. Department of Labor, 2026). I assume that the administrative burden of tracking workers' hours or reducing their flexibility comes at a cost to employers, denoted by r.

Workers. There are N workers with types $i = 1 \cdots M$ that differ in their disutility of labor, parametrized by ϵ^i. The instantaneous utility of a worker of type i is given by

$$u(c,h) = c - \Phi(h,\epsilon^i) \tag{6}$$

where c is consumption and $\Phi(h,\epsilon^i)$ represents the disutility of working h hours. Workers' consumption is equal to their labor income if working and b if unemployed.

Job finding and job destruction. I assume that there is free entry of firms. To enter the market, a firm can post a vacancy for a cost k. Suppose there are v vacancies posted and u number of unemployed workers, then vacant jobs are matched at the rate $m(\theta)$ where $\theta = \frac{v}{u}$ represents labor market tightness. From the workers' perspective, unemployed workers meet employers at rate $\theta m(\theta)$. Jobs are destroyed exogenously at a rate μ.

B.b Determining Hours and Wages

When a firm and worker match, they observe the worker type and the characteristics of the job (y, z, r, ϵ^i). They then bargain over the base pay and salaried/hourly classification. The firm then chooses hours h to maximize its profits.

Hours. Using backward induction, suppose the firm takes a job's classification and base pay as given, and unilaterally sets workers' hours to maximize its profits $\pi(h|s, w, y, z, r)$. For hourly jobs, the first order conditions implies the solution

$$h^*(w|hourly) = \min\left[\left(\frac{y\alpha}{w + 0.5w1_{h>40}}\right)^{\frac{1}{1-\alpha}}, z\right] \tag{7}$$

The overtime premium in the denominator of the labor demand function generates bunching at 40 hours.

For salaried jobs, the profit-maximizing hours for the firm is given by

$$h^*(s|salaried) = \begin{cases} z \text{ if } s \geq \bar{s};\ \text{ or } z \leq 40 \\ \min\left[\left(\frac{40y\alpha}{1.5s}\right)^{\frac{1}{1-\alpha}}, z\right] \text{ otherwise} \end{cases} \tag{8}$$

In effect, the firm always makes the worker work z hours if they are not covered for overtime or if z is below 40 hours. However, for covered workers with $z > 40$, the firm would aim to equate the marginal productivity per hour of labor with the marginal cost from overtime, until the corner solution z where an additional hour has no effect on productivity. Overtime coverage is thus predicted to reduce some workers' hours who otherwise would work z.

Earnings and Salaried/Hourly Classification. Workers and employers are forward-looking, so they negotiate the base pay and classification of the job knowing that the firm will set hours according to equations (7) and (8). Bargaining over both these characteristics jointly is equivalent to bargaining the salary and wage for each classification first, and then picking the classification that maximizes the Nash product. For brevity, I illustrate the problem for just salaried jobs. Denote the worker's and firm's bargaining power by γ and $1-\gamma$, respectively. The optimal base pay for a salaried job solves the Nash bargaining problem

$$\max_s \left(W^i(y, z, r, \epsilon^i) - W_u^i\right)^\gamma J(y, z, r, \epsilon^i)^{1-\gamma} \tag{9}$$

where $W^i(y, z, r, \epsilon^i)$ is the value of employment to the worker, W_u is the value of unemployment for worker of type i, and $J(y,z,r,\epsilon^i)$ is the value of a job to the firm:

$$W^i(y,z,r,\epsilon^i) = q(s,h^*(s)) - \Phi(h^*(s),\epsilon^i) + \beta(1-\mu)W^i(y,z,r,\epsilon^i) + \beta\mu W_u^i \tag{10}$$

$$J(y,z,r,\epsilon^i) = f(h^*(s)|y,z) - q(s,h^*(s)) - r + \beta(1-\mu)J(y,z,r,\epsilon^i) \tag{11}$$

Intuitively, the distribution of (y,z,ϵ^i) generates a joint distribution of salaries and hours, whereas heterogeneity in r allows for jobs with the same hours and pay to have different salaried/hourly classifications. In standard job search models with bargaining, workers receive γ share of the total surplus, defined as $S^i = W^i + J - W_u^i$. However, that property only holds when the salary does not affect the amount of surplus. In this case though, s factors into the employer's hours decision, which affects both firms' production and workers' utility. Appendix C solves equation (9), showing that the solution balances a trade-off between distributing surplus according to the bargaining weight γ and maximizing total surplus.

B.c Job Creation

A match will result in employment if the bargaining solution gives both the worker and firm non-negative surplus. This job creation condition can be summarized by a cutoff rule such that a job is created if and only if y is greater than or equal to a threshold

$$\underline{y}(z,r,\epsilon^i) = \{y|\min[W^i(y,z,r,\epsilon^i), J(y,z,r,\epsilon^i)] = 0\} \tag{12}$$

This cutoff rule defines the value of unemployment as

$$W_u^i = b + \beta\theta m(\theta)\int_{y\geq\underline{y}(z,r,\epsilon^i)} W^i(y,z,r,\epsilon^i)dF + \beta(1-\theta m(\theta))W_u^i \tag{13}$$

The marginal value of posting a vacancy to the firm, denoted V, is similarly given by

$$V = -k + \beta m(\theta)\sum_{i=1}^{M}\sigma^i\Big[\int_{y\geq\underline{y}(z,r,\epsilon^i)} J(y,z,r,\epsilon^i)dF\Big] + \beta(1-m(\theta))V$$

where $\sigma^i = \frac{u^i}{\sum u^i}$ is the share of unemployed who are type i workers. Due to free entry, firms continue posting vacancies, v, until their expected profits are zero (i.e. $V = 0$). The zero-profit condition implies that

$$-k + \beta m(\theta) \sum_{i=1}^{M} \sigma^i \Big[\int_{y \geq \underline{y}(z,r,\epsilon^i)} J(y, z, r, \epsilon) dF \Big] = 0 \tag{14}$$

B.d Equilibrium

An equilibrium is defined by a vector $(\theta, v, \{u^i\}_i, \{W_u^i\}_i, \{\underline{y}\}_{z,r,i}, \{w, s, h\}_{y,z,r,i})$ that satisfies

1. The optimization conditions implied by equations (7) to (14)
2. Market clearing where the number of workers entering employment equals the number exiting for every worker type i: $u^i \theta m(\theta) \int_{y \geq \underline{y}(z,r,\epsilon^i)} dF = (N^i - u^i)\mu$

Appendix C. Comparative Statics

In this section, I use the job search and bargaining model in Appendix B to generate testable predictions of the effects of raising the overtime exemption threshold, $\bar{s}$. To do so, I solve the Nash bargaining problem in equation (9) for salaried workers exempt from overtime and show how the solution changes in response to the policy. The salary is chosen to solve

$$\max_{s} \left(W^i(y, z, r, \epsilon^i) - W^i_u\right)^{\gamma} J(y, z, r, \epsilon^i)^{1-\gamma} \tag{15}$$

Substituting in the value functions and taking logs, the problem is equivalent to

$$\max_{s} \gamma \log \left(\frac{q(s, h^*(s)) - \Phi(h^*(s), \epsilon) - (1-\beta)W_u}{1 - \beta(1-\mu)} \right) + (1-\gamma) \log \left(\frac{f(h^*(s)|y, z) - q(s, h^*(s)) - r}{1 - \beta(1-\mu)} \right)$$

First order conditions:

$$\gamma \frac{q_s + q_h h^*_s(s) - \Phi_h(h^*(s), \epsilon) h^*_s(s)}{q(s, h^*(s)) - \Phi(h^*(s), \epsilon) - (1-\beta)W_u} + (1-\gamma) \frac{[f_h(h^*(s)|y, z) - q_h] h^*_s(s) - q_s}{f(h^*(s)|y, z) - q(s, h^*(s)) - r} = 0 \tag{16}$$

C.a Baseline: Exempt from overtime.

Consider the solution for a salaried job whose base pay is above the overtime exemption threshold. By equation (8), the firm sets the worker's hours to the maximum productive hours, $h^*(s) = z$. This implies that $h^*_s(s) = 0$ so equation (16) simplifies to

$$\begin{aligned}
0 &= \gamma \frac{q_s}{q(s, h^*(s)) - \Phi(h, \epsilon) - (1-\beta)W_u} + (1-\gamma) \frac{-q_s}{f(h^*(s)|y, z) - q(s, h^*(s)) - r} \\
\Rightarrow 0 &= \gamma [f(h^*(s)|y, z) - q(s, h^*(s)) - r] - (1-\gamma)[q(s, h^*(s)) - \Phi(h, \epsilon) - (1-\beta)W_u] \\
\Rightarrow 0 &= \gamma [y z^{\alpha} - s - r] - (1-\gamma)[s - \Phi(z, \epsilon) - (1-\beta)W_u] \\
\Rightarrow s^* &= \gamma [y z^{\alpha} - r] + (1-\gamma)[\Phi(z, \epsilon) + (1-\beta)W_u] \qquad (17)
\end{aligned}$$

This is the classic Nash bargaining solution where the total surplus is fixed, and the worker receives γ share of the total surplus. Note that since workers are exempt from overtime at baseline, $r = 0$.

C.b Benchmark: Joint Bargaining Problem.

Before examining how overtime coverage affects the Nash bargaining solution, it is useful to first compare the baseline solution to one where workers and firms jointly bargain over both salary and hours:

$$\max_{s,h} \left(W^i(y,z,r,\epsilon^i) - W^i_u\right)^\gamma J(y,z,r,\epsilon^i)^{1-\gamma}$$

Substituting the value functions and taking logs, the problem is equivalent to

$$\max_{s,h} \gamma \log\left(\frac{q(s,h) - \Phi(h,\epsilon) - (1-\beta)W_u}{1-\beta(1-\mu)}\right) + (1-\gamma)\log\left(\frac{f(h|y,z) - q(s,h) - r}{1-\beta(1-\mu)}\right)$$

For someone exempt from overtime, the first order conditions are

$$[\text{FOC } s]: \quad \frac{\gamma}{W(y,z,\epsilon) - W_u} - \frac{1-\gamma}{J(y,z,\epsilon)} = 0 \tag{18}$$

$$[\text{FOC } h]: \quad \frac{\gamma \Phi_h(h,\epsilon)}{W(y,z,\epsilon) - W_u} - \frac{(1-\gamma)\frac{\partial f}{\partial h}}{J(y,z,\epsilon)} = 0 \tag{19}$$

Substituting equation (18) into (19), we have that hours are determined by

$$\Phi_h(h,\epsilon) = \frac{\partial f}{\partial h} \tag{20}$$

This condition sets the social marginal cost per hour of labor equal to the social marginal productivity, thereby maximizing the total surplus. Substituting in f, hours is given by

$$h^*_{joint} = \min\left[\left(\frac{y\alpha}{\Phi_h(h,\epsilon)}\right)^{\frac{1}{1-\alpha}}, z\right] \tag{21}$$

From the first order condition in equation (18), salaries are determined by:

$$s^*_{joint} = \gamma[yh^\alpha_{joint} - r] + (1-\gamma)[\Phi(h_{joint},\epsilon) + (1-\beta)W_u] \tag{22}$$

In other words, joint bargaining selects hours to maximize surplus and then the salary is used to distribute it. A feature of joint bargaining is that if there are no monitoring costs, the introduction of overtime has no impact on hours and net earnings since the same solution can be achieved with or without the policy. Both parties would simply agree to a lower base pay to offset the cost of overtime. On the other hand, if overtime coverage incurs a monitoring cost, then it would only enter the solution through r in equation (22). Thus, depending on the magnitude of r, the two parties may either agree to a lower salary since the monitoring cost reduced total surplus, or they may set $s = \bar{s}$ to keep the job exempt. If the monitoring cost is sufficiently large and the job originally had minimal firm surplus, then the match may also be destroyed. In any case, the optimal hours is still independent of the policy.

Relative to the solution to the joint bargaining problem, salaried jobs in the baseline model in Appendix C.a can be categorized into two groups. First, there are jobs where long hours are efficient, meaning z hours maximizes total surplus. These are cases where the worker has low disutility of labor ϵ, so even though the firm unilaterally sets hours to z, the outcome would have been the same if the pair jointly bargained over hours. Second, there are jobs that inefficiently work long hours. In this case, the surplus maximizing hours is given by the interior solution in equation (21), but since the firm does not internalize the cost of hours to the worker, they set hours at the corner solution. This outcome is inefficient because it does not maximize total surplus, so there are pairs of (s, h) that are Pareto improving. However, even though both parties are willing to trade lower salaries for shorter hours, such an outcome cannot be an equilibrium in the model as there is no enforcement mechanism that prevents the employer from reneging on the agreed upon hours after setting salaries. Moreover, since the inefficient outcome destroys surplus, some matches that would have otherwise resulted in a job under joint bargaining are not agreed upon.

C.c Salaried vs. Hourly Jobs

The two kinds of salaried jobs can be characterized further once I take into account which jobs are assigned as salaried or hourly. Two factors affect a job's salaried/hourly classification: the efficiency of long hours and the monitoring costs. First, all matches where z hours are sufficiently efficient are classified as salaried so that the firm would not cut hours.[21] Second, if long hours are sufficiently inefficient, then both parties would want the job to be hourly to discipline the firm from raising hours. Even in this case though, the job may nevertheless be salaried if the monitoring costs are prohibitively high. Thus, salaried jobs that have efficient and inefficient hours may both exist, but the latter must have high monitoring costs. The mechanism behind the response to expanding overtime coverage will differ between these two types of salaried jobs.

[21] For small inefficiencies where the optimal hours to the joint bargaining problem is near z, making the job hourly could actually backfire by causing firms to reduce hours too much. The comparative statics for jobs with small inefficiencies is thus similar to jobs where long hours are fully efficient.

C.d Policy Predictions

Consider an exempt salaried worker in the baseline model with base pay and weekly hours given by (s^*, z). Suppose the overtime exemption threshold increases from $\bar{s}_0$ to $\bar{s}_1$, and $s^* \in (\bar{s}_0, \bar{s}_1)$. This changes two incentives for the employer. First, long hours become more expensive because of the overtime premium. Second, employers incur a cost r from monitoring the worker's hours. What happens to the base pay, hours, and classification of the affected job? Equation (17) implies that workers with the same baseline salary may have different underlying values of (y, z, ϵ^i, r). As a result, the effects of raising the overtime exemption threshold can differ among workers with the same base pay.

1. **Case 1: Long hours are efficient (i.e. ϵ is small):** If workers have sufficiently low disutility from working, then it would be optimal for both parties to have long hours. In this case, overtime coverage actually makes the worker and firm worse off by incentivizing the employer to reduce hours, thereby shrinking the total surplus. In response, the bargaining solution may change in at least one of three ways:

   (a) **Bunching (z is large):** From equation (8), overtime coverage would reduce the worker's hours from z to $\left(\frac{40y\alpha}{1.5s}\right)^{\frac{1}{1-\alpha}}$, implying that the cost of the policy increases with the optimal hours z. For a large enough z, the Nash problem is maximized by raising the worker's salary above the overtime exemption threshold so that the worker continues to work the same hours, but receives a higher base salary. In other words, it is cheaper to raise salaries a little bit than to cut hours by a lot.

   Note that the model predicts two dimensions of selection into bunching. First, workers with higher baseline hours z are more likely to be bunched. As a result, the model predicts that the increase in overtime pay would be attenuated by the selection of high-hour workers into exemption status. Second, since the Nash problem was maximized at the original salary s^*, the penalty for bunching increases with the gap $\bar{s} - s^*$. Thus, the likelihood of bunching decreases with the distance to the new threshold.

   (b) **Cover for overtime (z is small):** If the worker's hours were already sufficiently close to 40, then the loss in surplus from cutting hours is small. In that case, the Nash bargaining solution would be to simply cover the worker for overtime and lower the worker's hours to $h^*(s) = \left(\frac{40y\alpha}{1.5s}\right)^{\frac{1}{1-\alpha}}$.

   Due to the overtime premium, the salary s no longer only affects how surplus is shared, but also the total amount of surplus available. To see this, rearranging equation (16), I get the following first order condition:

$$\frac{\gamma J(y,z,r,\epsilon^i)}{(1-\gamma)[W(y,z,r,\epsilon^i)-W_u]} = \frac{q_s + q_h h_s^*(s) - f_h(h|y,z)h_s^*(s)}{q_s + q_h h_s^*(s) - \Phi_h(h,\epsilon)h_s^*(s)} \tag{23}$$

The left-hand side captures the effect of s on the relative surplus between firms and workers, whereas the right hand side captures the effect on total surplus through the salary's indirect effect on firms' choice of hours (i.e. $h_s^*(s)$).

The baseline salaries in equation (17) are equivalent to setting the right term equal to 1. Whether s will adjust upward or downward relative to baseline depends on the marginal effect of hours on the total surplus. For jobs where long hours are efficient, the productivity of an extra hour (i.e. f_h) is large relative to the disutility per hour (i.e. Φ_h). This makes the right term greater than 1 since labor demand is downward sloping (i.e. $h_s^*(s) < 0$). To offset the increase on the right hand side, the left term needs to be made more positive by increasing the surplus of the firm via a reduction in the salary s. Thus, overtime coverage cuts workers' hours, leading to less surplus and a lower base salary.

(c) **Job loss:** For jobs on the margin of employment, the cost of bunching or the cost of reducing hours can cut the firm's surplus below zero, resulting in the match dissolving. In short, if a job is efficiently working long hours, it is costly to tax those hours.

2. **Case 2: Long hours are inefficient (i.e. ϵ is large):** Some jobs are working more hours than socially efficient. In this scenario, it would be Pareto improving to cut hours and salaries, but this outcome is not an incentive compatible equilibrium. Overtime coverage can act as an enforcement mechanism to bring hours closer to the surplus maximizing value. However, if overtime coverage results in a higher Nash product, the worker and firm would have already agreed on making the job hourly during the bargaining phase. As discussed in Appendix C.c, there exists salaried jobs where long hours are inefficient because it would be too costly to monitor their hours. Thus, covering these workers for overtime would actually destroy surplus, leading to the same predictions as case 1, except the responses are driven not through hours distortion, but by the administrative cost of monitoring workers' hours.

Thus, increasing the overtime exemption threshold has heterogeneous effects, leading to winners and losers. First, the clear winners are workers who get bunched above the new threshold. These are workers who tend to work long hours or already earned close to the threshold. Second, individuals who remain below the threshold and are now covered for overtime experience opposing effects. Although their base pay may fall, that is compensated by an increase in overtime pay and a reduction in hours. Lastly, the clear losers are matches that would have resulted in a job if not for the policy. These are positions where both bunching and covering workers for overtime are too expensive to generate positive surplus.

C.e Extension 1: Employer Sets Salaried/Hourly Status

Under the current assumptions of the model, overtime coverage for salaried workers cannot be justified on efficiency grounds. It either distorts efficient long hours or incurs a large monitoring cost. Thus, the only rationale for the policy is a preference for redistribution from employers to workers.

The efficiency argument for overtime changes if employers have market power over workers' salaried/hourly status. Suppose that instead of bargaining over both base pay and classification, the firm decides the classification first and the pair then bargains over base pay. The model is otherwise identical, except the firm selects the classification that maximizes its own surplus rather than total surplus. The qualitative predictions of Cases 1 and 2 persist: firms still prefer salaried jobs when long hours are efficient or when monitoring costs are high, and overtime coverage produces the same distortions described above.

However, this setting generates a third case in which overtime coverage for salaried workers can be efficiency-improving. Even when long hours are socially inefficient and monitoring costs are low, the firm may classify the job as salaried to extract a greater share of the surplus by pushing hours beyond the level the worker would accept under joint bargaining. Expanding overtime coverage to such jobs eliminates this incentive, thereby making the firm indifferent between classifying the job as salaried or hourly. Since match surplus decreases in the first two cases and increases in this last case, the employment effects of overtime coverage are ambiguous.

A policy-relevant example of this third case is the circumvention of overtime for hourly workers. Suppose no jobs are covered for overtime. Since firms set hours and do not internalize workers' disutility of labor, jobs may have inefficiently long hours. Introducing an overtime premium for hourly jobs can mitigate this distortion. Unlike for salaried jobs, the policy may be socially efficient because hourly jobs already incur a monitoring cost so Case 2 is no longer a concern. However, the premium also reduces the firm's surplus by raising the marginal cost of labor and reducing hours. Employers could avoid this cost by reclassifying hourly jobs as salaried. Extending overtime coverage to salaried workers prevents this response and preserves the efficiency gains from overtime coverage for hourly workers.

C.f Extension 2: Stochastic Consumer Demand

In the current framework, if both salaried and hourly jobs are covered for overtime, then the firm and worker are indifferent between the two classifications because they lead to the same outcomes with $w = \frac{s}{40}$ when $h \geq 40$. If we assume that worker classification is randomly assigned when indifferent, then the model predicts that some of the workers who gain overtime coverage may be reclassified to hourly.

To generate definitive predictions on worker's salaried/hourly status, the model can be extended to allow for z to be stochastic. In that case, when a firm and worker match, they observe the job characteristics (y, r, ϵ^i) and a distribution of productive hours $G(z)$. They then bargain over the base pay and classification based on the *expected* surplus of the job. In each subsequent period, a specific z_t is realized and the firm unilaterally sets the actual hours h_t.

In this framework, firms face an asymmetric downside risk when both types of jobs are covered for overtime. If demand is high ($z > 40$), then they have to pay an overtime premium whether the job is salaried or hourly. However, if demand is low ($z < 40$), then the cost of an hourly job shrinks with h, but a salaried job has a fixed cost of s. Thus, given $w = \frac{s}{40}$, a firm would strictly prefer an hourly job to mitigate risk given their concave production function. In contrast, since I have defined the worker's utility to be risk neutral, they are indifferent between either classification. As a result, expanding overtime coverage for salaried workers can generate reclassification effects if consumer demand is stochastic.[22]

[22]If workers were risk averse, whether jobs are reclassified will depend on the firm and worker's relative risk preferences.

Appendix D. Defining the Compensation Variables

D.a Overtime Pay

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

First, firms are not required to input a value into the "OT earnings" field. Although the payroll 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 E.3 plots the probability that a salaried worker receives overtime as a function of their weekly base pay, and finds a clear discontinuity at the overtime exemption threshold.

D.b Computing Weekly Measure of Income

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

Appendix Table D.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.

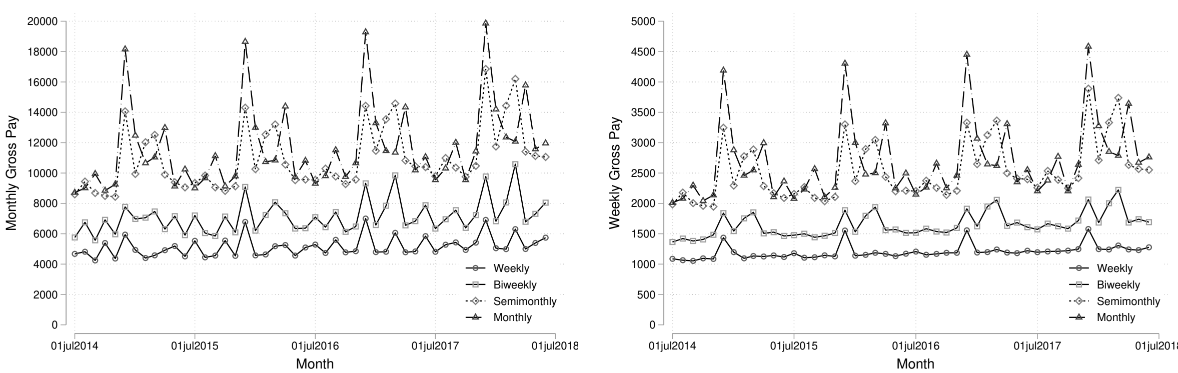
To derive workers' weekly base pay from their standard rate of pay, I follow the rules set by the Department of Labor and scale each worker's standard rate of pay by their pay frequency (i.e. $\frac{\text{standard pay}}{\text{week}} = \frac{\text{standard pay}}{\text{paycheck}} \cdot \frac{\text{paycheck}}{\text{week}}$).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

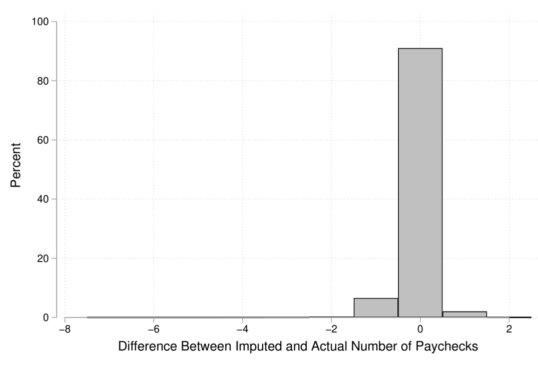
1. Compute the average gross pay across all workers of the same pay frequency within each firm-month.

2. Within each year, for each firm-frequency, compute the median of the 12 average monthly gross pays computed in step 1.

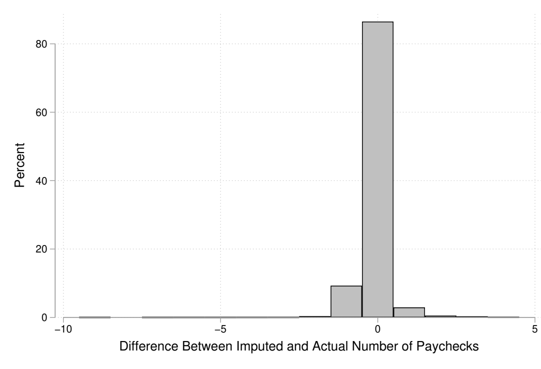
3. Record biweekly workers as receiving 3 paychecks in months where the average gross pay in their firm-frequency exceeds 1.25 times the firm's median gross pay in that year, and 2 otherwise.

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 D.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 D.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 D.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 E. Descriptive Statistics

E.a Firm and Worker Characteristics

In this section, I describe the characteristics of the firms and workers in the payroll data. Figure E.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.

Table E.1 describes in more detail the characteristics of firms and workers affected by the increases in the overtime exemption thresholds. In column (1), I record the size distribution, industry mix, and worker composition among firms in the treated states. I find that the sample consists 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 salaried 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.

Columns (3) and (4) repeat the summary statistics for firms in the states that did not raise their overtime exemption thresholds. I include 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 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 the robustness of my results to keeping only firms that operate in both treated and control states.

The descriptive statistics show that, on average, an increase in the overtime exemption threshold only affects 1.7% of the labor force. Although the share of workers exposed to a single policy appears small, none of the treated states only implement a single threshold increase. Instead, they gradually raise the threshold over multiple years to make it easier for employers to adjust. To measure the cumulative exposure to the state policies, Figure E.2 plots the distribution of salaried workers' weekly base pay for each treated state in the month before their first policy change. The vertical bars show the overtime exemption thresholds in effect at the start and end of my study period. The figure shows that for some states, the cumulative increase in the overtime exemption thresholds is fairly large. For instance, between 2014 and 2021, the threshold in California increased from the 5th percentile of the salaried income distribution to the 26th percentile. Even if I adjust the 2021 threshold for inflation, about 16% of salaried jobs are directly affected by the regulation. Moreover, given that all six states are still in the process of increasing their overtime exemption thresholds at the end of my study period, the share of workers exposed to the policy will continue to grow over time. For example, Washington and Colorado only started their multi-year threshold changes in 2021. By 2026, Washington's threshold had more than doubled from \$684 to \$1,541, and Colorado's increased from \$684 to \$1,111. Thus, a significant share of salaried workers in the treated states are exposed to increases in the overtime exemption thresholds.

Table E.2 compares the characteristics of workers in the treated interval between treated and control states, and to the rest of the sample more generally. Comparing columns (1) and (2), I find that salaried workers with weekly base pays between the old and new thresholds are similar across treated and control states. About 55% of workers targeted by the expansions in overtime coverage are women, the average tenure is 7 years and about a third of workers are married. In contrast, the full sample of workers in treated states tend to have higher average salaries, are more likely male, and have one fewer year of tenure. Overall, the restriction of workers to within a similar salary range matches worker characteristics along multiple dimensions between treated and control states.

E.b Cross-sectional Evidence of Compliance

Next, I explore whether employers comply with the overtime rules. Figure E.3 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.

E.c External Validity

In this section, I explore the extent to which the results estimated using the payroll data may be representative of the labor force as a whole. Given that the data provider offers its clients with advice on payroll management and labor law compliance, the response of these firms may differ from employers outside the sample. However, the company is not the only payroll service provider in the United States.

Appendix Table E.3 lists the largest payroll software providers and their respective customer base. Together, business reports from ADP, Quickbooks, Paychex, and Paycom indicate that 40% of the entire US labor force work for a firm that uses one of these four software providers. That does not even include clients of companies like Workday, Dayforce, and Oracle that combine human capital management with payroll. The pervasiveness of such payroll software provides suggestive evidence that the results of the study are externally valid for a large share of workers.

Although many firms subscribe to a payroll software, they are not perfectly representative of the U.S. labor market. I note three characteristics of such firms based on the analysis by Grigsby et al. (2021), which benchmarked ADP's payroll data to external sources. First, ADP's data over-samples medium size firms with 50-4999 employees and under-samples firms with <50 or 5000+ employees relative to the Census Business Dynamics Statistics. Intuitively, mom-and-pop stores do not need a sophisticated software to organize their payroll, whereas extremely large employers benefit from sufficient economies of scale to justify creating an in-house software. Second, despite differences in firm size, the age, gender, tenure, and pay frequency distribution of workers in the data closely match that of the Current Population Survey. Third, average wages also evolve similarly over time between the two datasets. Even within-worker, the distribution of annual earnings changes in the ADP data matches the mean, standard deviation, and skewness of the same distribution using Social Security Records, albeit it under-estimates the kurtosis. Based on these benchmarks, the payroll data is likely representative of workers in the U.S., but undercounts very small and very large firms.

One potential way in which firms without payroll software may respond differently is in the speed of adoption. I find that employers in the payroll data adjust salaries and hourly/salary classification in the exact month of the policy change. This immediate effect may be the result of assistance provided by the software itself. Whether or not firms without the software would respond as quickly is difficult to test because few datasets have a large enough sample size to detect effects at a monthly frequency. However, existing evidence from the minimum wage literature suggests that employers do respond quite swiftly to labor market regulations. For instance, using administrative UI records from Washington state, Jardim et al. (2022) show clear bunching in the wage distribution at the minimum wage in the quarter after it increases (they do not look at the quarter of implementation). Similarly, using data from the Quarterly Census of Employment and Wages, Dube et al. (2010) finds that raising the minimum wage increases earnings immediately in the quarter of the policy. Given these results, it is not implausible that out-of-sample firms may also respond quickly to changes in the overtime exemption threshold.

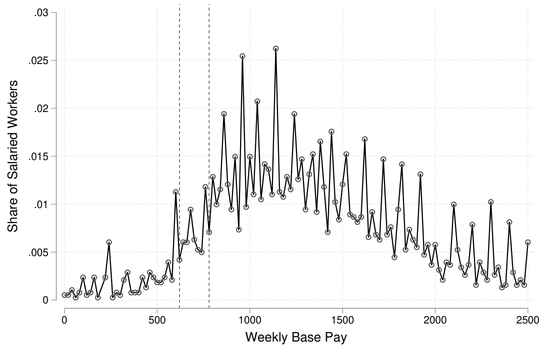
Appendix Figure E.1
Distribution of Share Directly Affected by the State Overtime Policy

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.

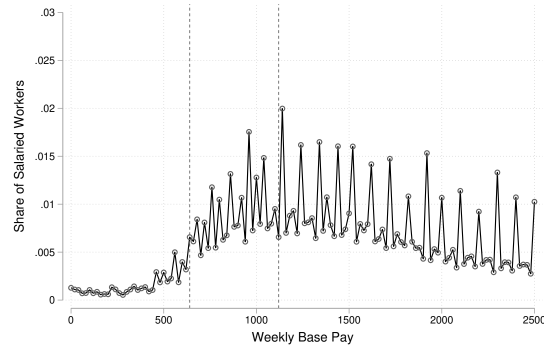
Appendix Table E.1
Firms Affected by Changes in the Overtime Exemption Threshold

	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: $\geq$ 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	$\geq$ 1 aff. worker	All	$\geq$ 1 aff. worker

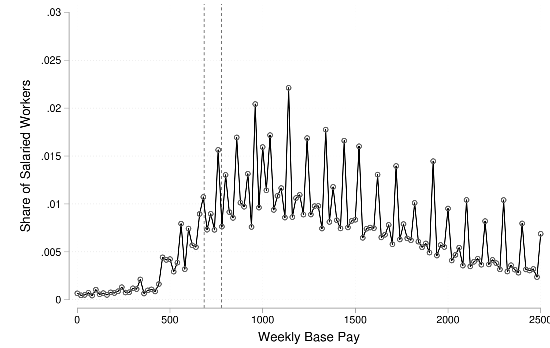
Notes. The table reports the characteristics of firms in the baseline month prior to each threshold change, separately for all firms and for only firms that employed at least one salaried worker directly affected by the reform. Columns (1)-(2) report these statistics for firms in states that raised their 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.



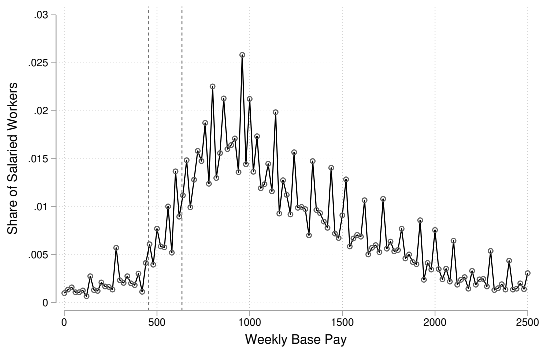
(a) Alaska, 2014



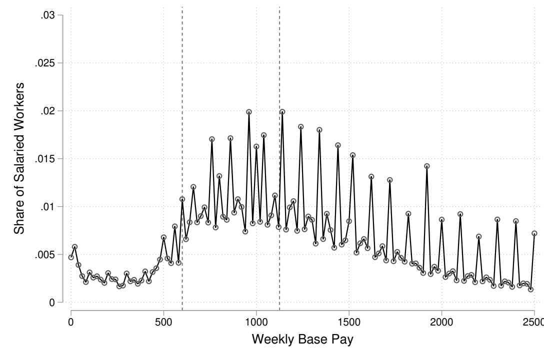
(b) California, 2014



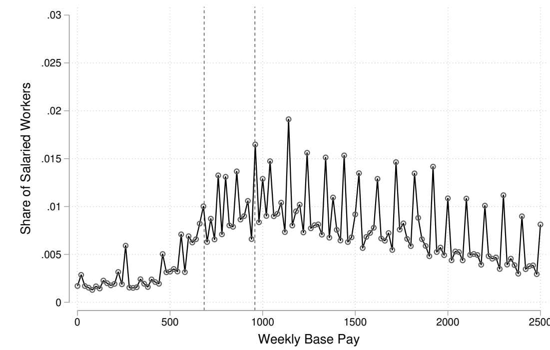
(c) Colorado, 2020



(d) Maine, 2016



(e) New York, 2014



(f) Washington, 2020

Appendix Figure E.2
Distribution of Weekly Base Pay in Month Before First State-Specific Policy Change

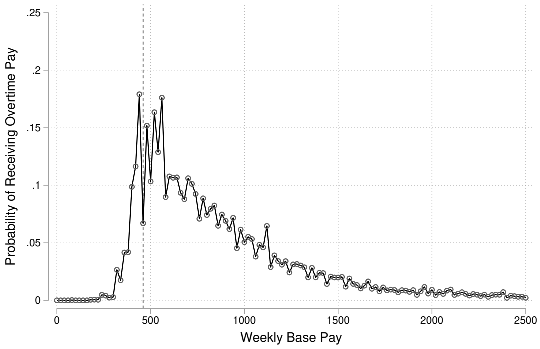
Notes. This figure plots the distribution of weekly base pay for salaried workers in each of the treated states, in the baseline month prior to their first threshold increase. The dotted vertical lines plot the overtime exemption threshold in effect at the beginning and end of my study period.

Appendix Table E.2

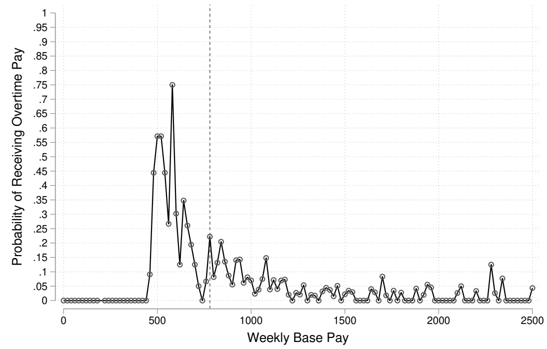
Workers Affected by Changes in the Overtime Exemption Threshold

	(1) Treated	(2) Control	(3) All
Weekly Base Pay	868.135	833.564	1192.272
Male	0.448	0.452	0.540
Tenure (Years)	7.180	7.142	6.070
Married	0.320	0.365	0.351
N	183,191	2,311,790	24,502,643

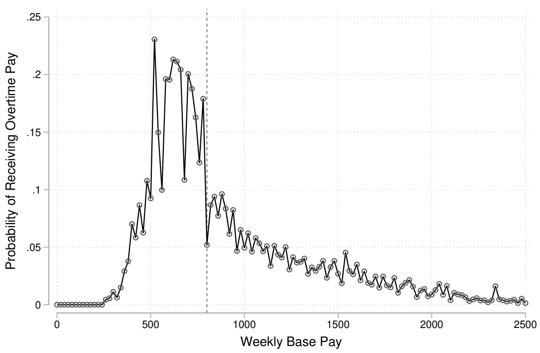
Notes. The table reports the characteristics of workers in the baseline month prior to each threshold change. Columns (1) and (2) restrict the sample to salaried workers with weekly base pay between the old and new thresholds in the treated and control states, respectively. Column (3) keeps all salaried and hourly workers in the treated states.



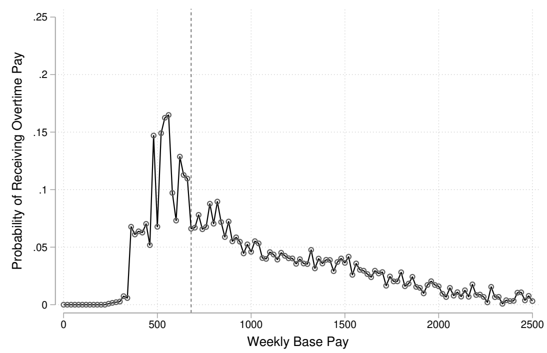
(a) FLSA states, April 2016



(b) Alaska, April 2016



(c) California, April 2016



(d) New York, April 2016

Appendix Figure E.3
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.

Appendix Table E.3
Payroll and HR Software Companies: User Statistics and Sources

Company	**Number of Users**	**Source**
ADP	∼1 in 6 US workers	ADP Corporate Overview
QuickBooks Payroll	17.6 million US workers (FY22)	Intuit Fact Sheet
Paychex	∼1 in 11 US private sector workers	Paychex Company Info
Paycom	6.8 million workers	Paycom FY2023 Results
Paylocity	Undisclosed; similar market cap as Paycom	Paylocity Website
Workday	65 million global users	Workday Blog

Notes. This table reports the approximate number of users of the major U.S. payroll service providers. The third column provides a hyperlink to a source for each report.

Appendix F. Robustness to Functional Form

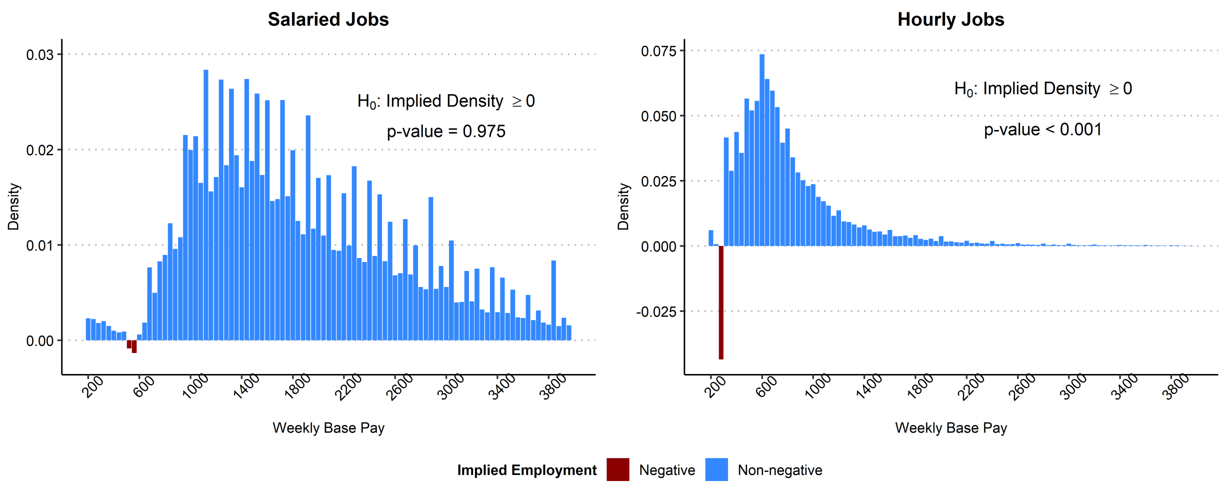
In this section, I test whether the parallel trends in weekly base pay is sensitive to the functional form. Specifically, Roth and Sant'Anna (2023) show that the parallel trends of an outcome y is invariant to all strictly monotonic transformations $g(\cdot)$ if and only if the parallel trends assumption holds for the entire CDF of the untreated potential outcomes. To empirically test this condition, they show that if the assumption is true, then

$$f_{Y_{i1}(0)|D_i=1}(y) = f_{Y_{i1}(0)|D_i=0}(y) + f_{Y_{i0}(0)|D_i=1}(y) - f_{Y_{i0}(0)|D_i=0}(y) \ \geq 0 \text{ for all } y \qquad (24)$$

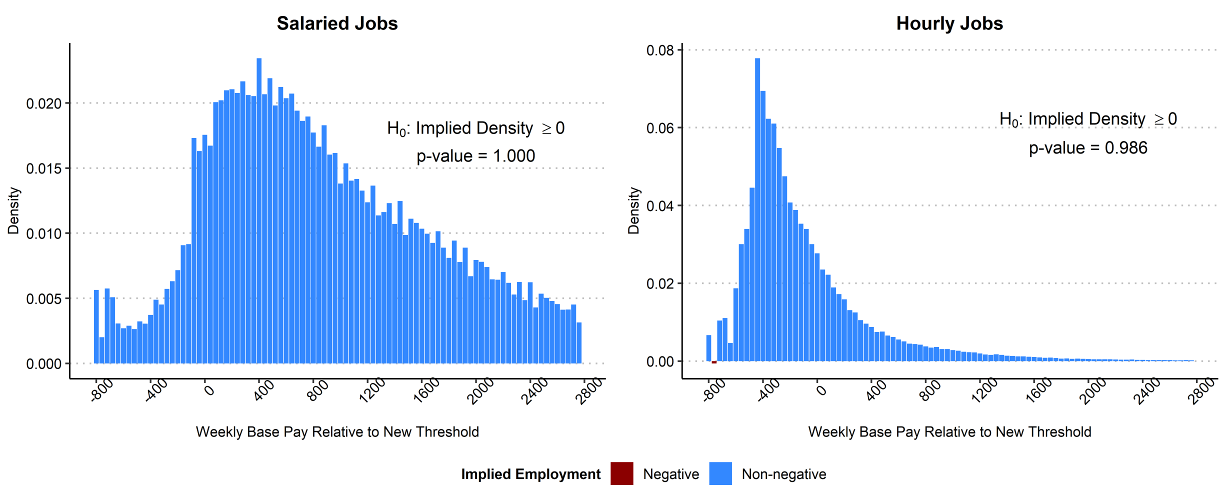
where $f_{Y_{it}(0)|D_i=d}(y)$ is the probability density of $Y_{it}(0)|D_i$ at y. The left hand side is the counterfactual distribution of y for the treatment group had they not been treated. Following Roth and Sant'Anna (2023), I estimate this distribution using the sample analog on the right hand side of equation (24), which uses the change in the control group to model the counterfactual growth over time.

In Figure F.1, I plot the implied counterfactual distribution of weekly base pay. Panel (a) computes the distribution for the six states that ever increased their overtime exemption thresholds between January 2014 and June 2021. The left panel shows that the implied density for salaried jobs is negative for weekly base pays between \$520 to \$600. This implies that the share of employment in these wage bins decreased more in the control states from Jan 2014 to June 2021 than the baseline shares in the treated states. However, I cannot statistically reject that the density is nonnegative for all values of weekly base pay using bootstrapped standard errors. In comparison, the right panel exhibits a sharp negative density in the left tail of the income distribution for hourly workers. This is to be expected since the treated states tend to have higher minimum wages and therefore fewer low wage workers at baseline. As a result, I reject that the parallel trends assumption holds throughout the entire distribution of hourly jobs during my sample period.

Given functional form concerns at the left tail of the distribution, my empirical strategy restricts the sample to jobs paying above the minimum wage. It is clear from panel (a) that the implied distribution of such jobs would pass the parallel trends test. As an alternative test, panel (b) plots the implied distribution using my stacked difference-in-difference sample, where I calculate changes in the distribution over the 12 month event window and center the distribution at the new overtime exemption threshold. The distribution is thus an average of the distribution across events, weighted by the employment of each event-state. In this case, I cannot reject the null hypothesis that parallel trends is satisfied throughout the relevant part of the pay distribution for both salaried and hourly jobs.



(a) Raw Distribution, Jan 2014 to June 2021



(b) Stacked DID, Event time -6 to +5

Appendix Figure F.1
Test of Sensitivity to Functional Form (Roth and Sant'Anna, 2023)

Notes. This figure shows the Roth and Sant'Anna (2023) test of whether the parallel trends assumption holds throughout the CDF of untreated potential outcomes, separately for salaried and hourly jobs. Panel (a) implements the test by computing the treated states' implied distribution of weekly base pay in June 2021, using the change in control states' distribution since January 2014. Panel (b) repeats the test in a stacked difference-in-difference design, restricting the distribution to between -800 and +2,400 of the new overtime exemption threshold for each event.

Appendix G. Interactions with the Minimum Wage

This Appendix examines potential interaction effects between states' overtime exemption thresholds and their minimum wages.

G.a Do MW Increases Affect the OT Estimates?

Figure G.1 plots the event study estimates comparing states that raised their exemption thresholds to states that did not. In contrast to the stability of salaried employment in the left tail of the distribution, the figure shows significant activity among hourly jobs, primarily driven by the minimum wage. In general, these contemporaneous changes in the minimum wage are concentrated in the far left tail of the distribution, away from the bounds affected by the change in the overtime exemption threshold. Nevertheless, they may bias my estimate if its spillover effects extend up the distribution. For example, I observe a small increase in the number of hourly workers \$160-200 below the new threshold in Panel (c) of Figure 2.

I adopt two strategies to avoid any possible contamination with the minimum wage. First, the estimates in the main text only aggregate the estimates within -\$160 to +\$80 of the new overtime exemption threshold when computing the employment effects. Second, Appendix Figure G.2 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.[23] As expected, I no longer observe a change in the left tail of the hourly workers' distribution. Nevertheless, the sum of my estimates is very similar to the baseline results with the full sample. This suggests that my preferred estimate of the employment effect is not confounded by changes in the minimum wage.

Although increases in the minimum wage may not directly interact with jobs targeted by the expansions in overtime coverage, the two policies can still interact through general equilibrium responses. For example, prior research has found that increases in the minimum wage leads to higher prices, with pass-through rates often exceeding 100% (Dube and Lindner, 2024). These aggregate price increases from the minimum wage could potentially make it easier for firms to also raise prices in response to the cost of overtime, thereby muting the employment response.

One way to isolate the effect of raising the overtime exemption threshold is to focus on only states that did not change their minimum wage at the same time. Two events satisfy this criterion: Colorado and Washington only adjusted their minimum wage for inflation by

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

$0.30 and $0.19, respectively, in 2021.[24] The event-specific analysis in Figure 5 finds that these two events had small negative employment effects, but not significantly different from the other state policy changes.

As an alternative approach to isolate the effect of increasing the overtime exemption threshold from general equilibrium responses to the minimum wage, Table G.1 replicates the firm-level analysis from the main text, but restricts the control group in each event to only states that increased their minimum wage by at least the same amount as the treated state. In general, I find similar magnitude effects to when I used the full sample, though the employment estimates are slightly more positive in these specifications. Taken together, the results suggest that accounting for the effects of minimum wage changes do not significantly alter my findings.

G.b Does the MW Prevent Decreases in Base Pay?

Although changes in the minimum wage may not affect my estimates, the *level* of the minimum wage could potentially affect their interpretation. In particular, employers are legally required to pay salary workers a weekly base pay such that their implied hourly wage, $\frac{\text{weekly base pay}}{\text{weekly hours}}$, is no less than the minimum wage (U.S. Department of Labor, 2016b). If affected workers tend to earn close to the minimum wage, then their base pay cannot decrease to offset the cost of the overtime premium.

Using the Current Population Survey, Figure G.3 plots the distribution of implied hourly wages for all treated salaried workers.[25] The median treated worker in the CPS data earned $23 per hour, which is comparable to the nominal median personal income in 2024 assuming a 40 hour workweek. Given this high wage, I find that only 1% of workers were earning below their state minimum wage. The wage distribution thus suggests that expansions in the overtime exemption threshold tend to target fairly well-paid jobs.

Although the minimum wage is not binding for the majority of salaried workers, it may be binding for individuals working above 40 hours per week. Mechanically, individuals who work more hours have a lower hourly wage since I have restricted weekly earnings to be between the old and new overtime exemption thresholds.

Focusing specifically on the 23% of workers who report working over 40 hours last week, Figure G.4 plots the difference between workers' implied wage and their state minimum wage. In red, I find that even for this group of individuals who work long hours, only 4% earn below the minimum wage. However, the green bars indicate that a large share of workers

[24] Although Washington sets its threshold as a multiple of the minimum wage, the 2021 reform was specifically a change in the multiplier rather than a change in the minimum wage.

[25] See Appendix I for a description of the data cleaning process.

would violate minimum wage laws if firms were to reduce their base pay to fully offset the cost of paying overtime. In particular, I calculate the salary s_{new} that would achieve the same weekly earnings and hours as at baseline once I account for the overtime premium: $s_{old} = s_{new} + 1.5\frac{s_{new}}{40}(h_{old} - 40)$. The implied hourly wage would fall below the minimum wage for 32% of affected salaried workers who work over 40 hours per week.

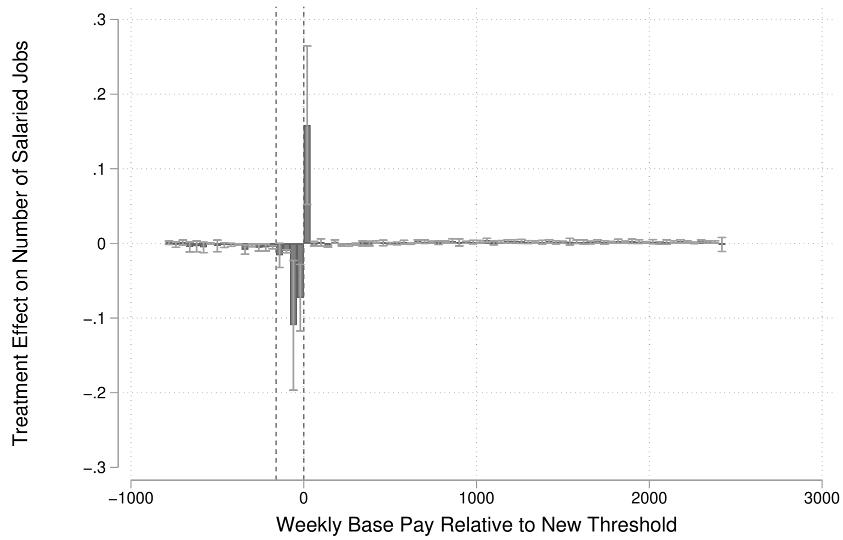
Even if the overtime premium cannot be fully offset by a decrease in weekly base pay, employers can still implement a partial offset by lowering base pays until the implied hourly wage is at the minimum wage. However, I find no evidence of such partial wage adjustments. Appendix Figure G.5 plots the estimates of equation (1) recentered to zero at the old overtime exemption threshold. The large drop in the number of salaried workers at the old threshold is only compensated for by increases to the right of the distribution. In fact, I find a very small decrease in salaried employment to the left of the old threshold. Among hourly jobs, I find a gradual increase in employment below the old threshold, but this does not appear to be coming from jobs above the threshold.

Overall, I find that the minimum wage is a binding constraint for about a third of salaried workers who work over 40 hours a week. Employers would not be able to fully offset the cost of the overtime premium for these workers by reducing their weekly base pay without violating minimum wage laws. Conversely, that means employers are able to cut salaries for the majority of salaried workers. Also, even if the minimum wage prevents firms from fully negating the cost of overtime, they can still partially offset the cost by cutting hourly wages to exactly the minimum wage. Despite having the option to cut workers' weekly base pay, I find no evidence of such behavior in the payroll data.

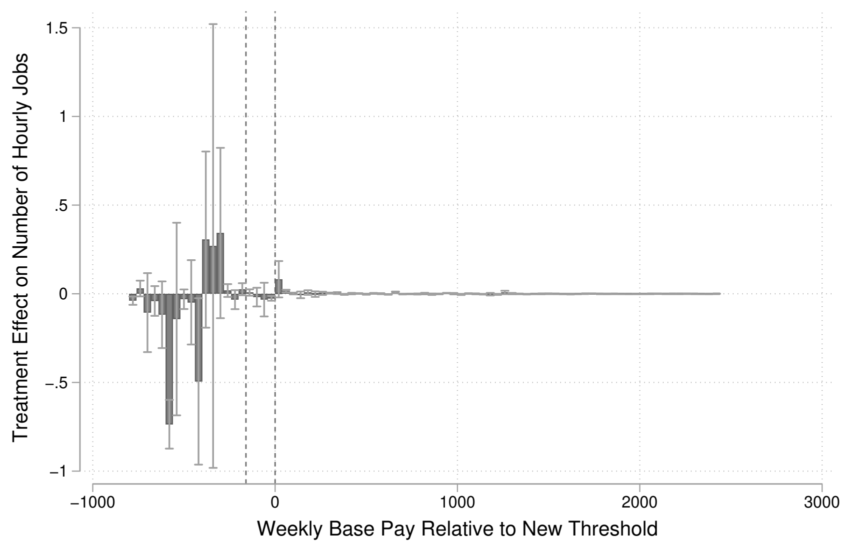
G.c Does the OT Threshold Explain MW Spillover Effects?

Previous studies find that increases in the minimum wage tend to have positive spillover effects higher up the income distribution. Given that in some states, the overtime exemption threshold is a multiple of the minimum wage, one potential explanation for the spillover effect is that it simply reflects the bunching response identified in this paper. To test this hypothesis, Figure G.6 plots the effect of minimum wage changes on the distribution of weekly base pay, where each $40 bin is equivalent to a $1 wage increase for hourly workers. The estimation procedure is similar to the strategy used in the main text with three minor changes. First, I only use the 17 events where increases in the minimum wage mechanically led to an increase in the overtime exemption threshold. Second, weekly base pay is centered around the new minimum wage for each event, rather than the new overtime exemption threshold. Third, following Cengiz et al. (2019), I scale the estimates by the total number of workers at baseline, rather than just the number of affected workers.

Panel (a) finds that raising the minimum wage leads to a clear bunching mass in the left tail of the income distribution among hourly jobs. Consistent with Cengiz et al. (2019), I find statistically significant spillover effects up to \$3/hour above the new minimum wage. In contrast, Panel (b) finds that the effects of raising the overtime exemption threshold do not manifest until \$4/hour above the minimum wage. Moreover, the effects of expanding overtime coverage would actually *shrink* estimates of the spillover effects in the left tail of the income distribution. It is not until \$13 above the new minimum wage that I observe the bunching mass from the overtime policies. Thus, changes in the overtime exemption threshold can explain none of the positive wage effects immediately above the new minimum wage.



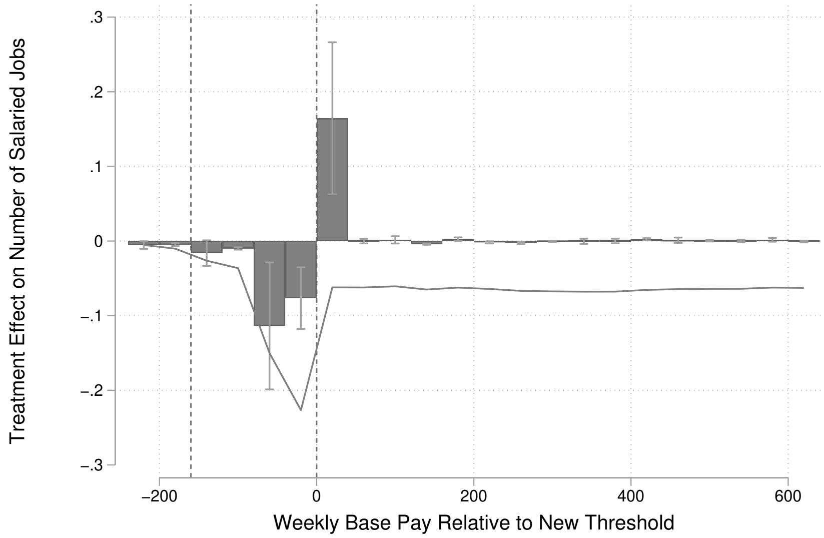
(a) Effect on Number of Salaried Jobs, at Time 0



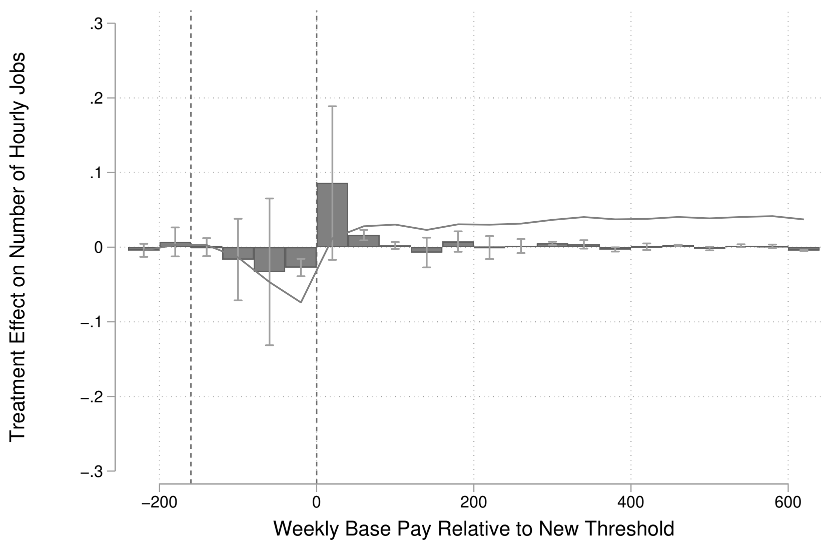
(b) Effect on Number of Hourly Jobs, at Time 0

Appendix Figure G.1
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.



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



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

Appendix Figure G.2
Effect of Raising States' OT Exemption Thresholds on the Distribution of Base, No Interaction with the Minimum Wage

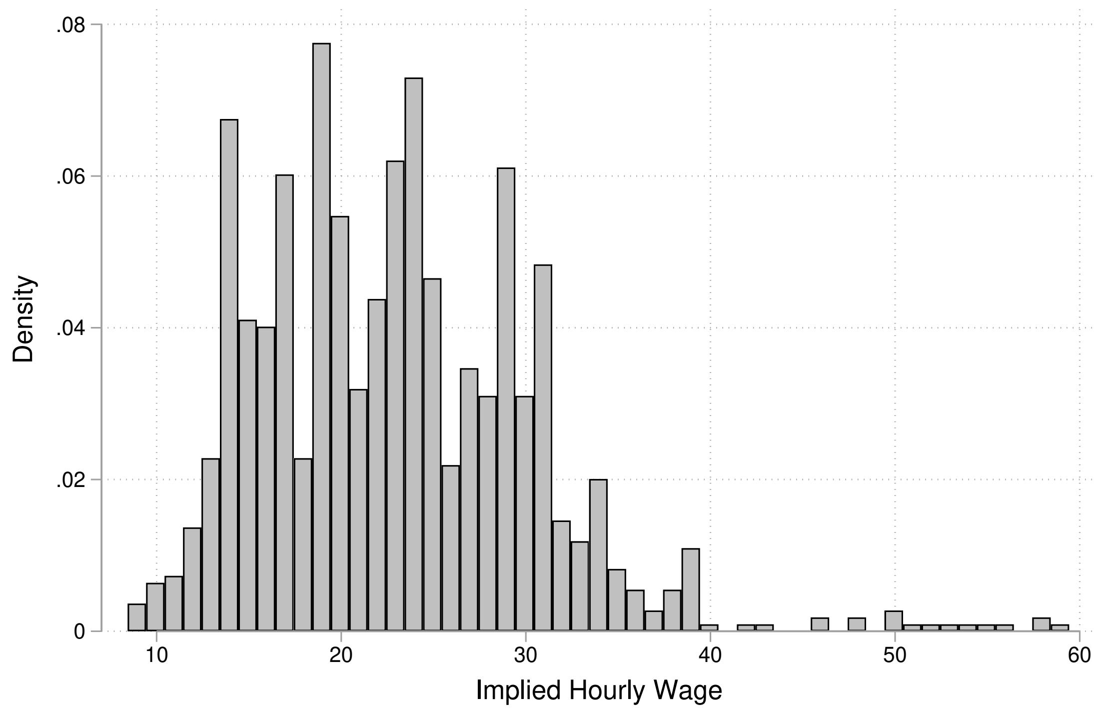
Notes. The figures show the event study estimates from equation 1, separately for salaried and hourly jobs. The sample is restricted to the 14 events where the old overtime exemption threshold is at least \$200 weekly base pay above a new minimum wage. The height of each bar indicates the effect of raising 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.

Appendix Table G.1
Effect of Raising the Overtime Exemption Threshold on the Pay Distribution, Control Group Restricted to States that Raised the Minimum Wage

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A						
Salaried Jobs						
Below Threshold	-0.219***	-0.206***	-0.238***	-0.237***	-0.291***	
	(0.051)	(0.032)	(0.073)	(0.047)	(0.018)	
Above Threshold	0.179***	0.173***	0.204***	0.194***	0.257***	
	(0.044)	(0.024)	(0.068)	(0.040)	(0.011)	
Hourly Jobs						
Below Threshold	-0.069***	-0.045**	-0.042	-0.068***	0.019	
	(0.026)	(0.020)	(0.036)	(0.015)	(0.016)	
Above Threshold	0.105***	0.105***	0.103**	0.115***	0.159***	
	(0.035)	(0.020)	(0.044)	(0.034)	(0.030)	
Panel B						
Emp. w.r.t. Aff. Salaried	-0.005	0.027***	0.028	0.004	0.144***	-0.005
	(0.031)	(0.010)	(0.062)	(0.025)	(0.039)	(0.031)
Emp. w.r.t. All Affected	-0.001	0.005**	0.006	0.001	0.031***	-0.001
	(0.006)	(0.002)	(0.013)	(0.005)	(0.008)	(0.006)
Number of Affected Salaried	1.076	1.076	1.097	1.150	1.198	1.076
Number of Affected Hourly	4.208	4.208	4.268	4.233	4.378	4.208
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

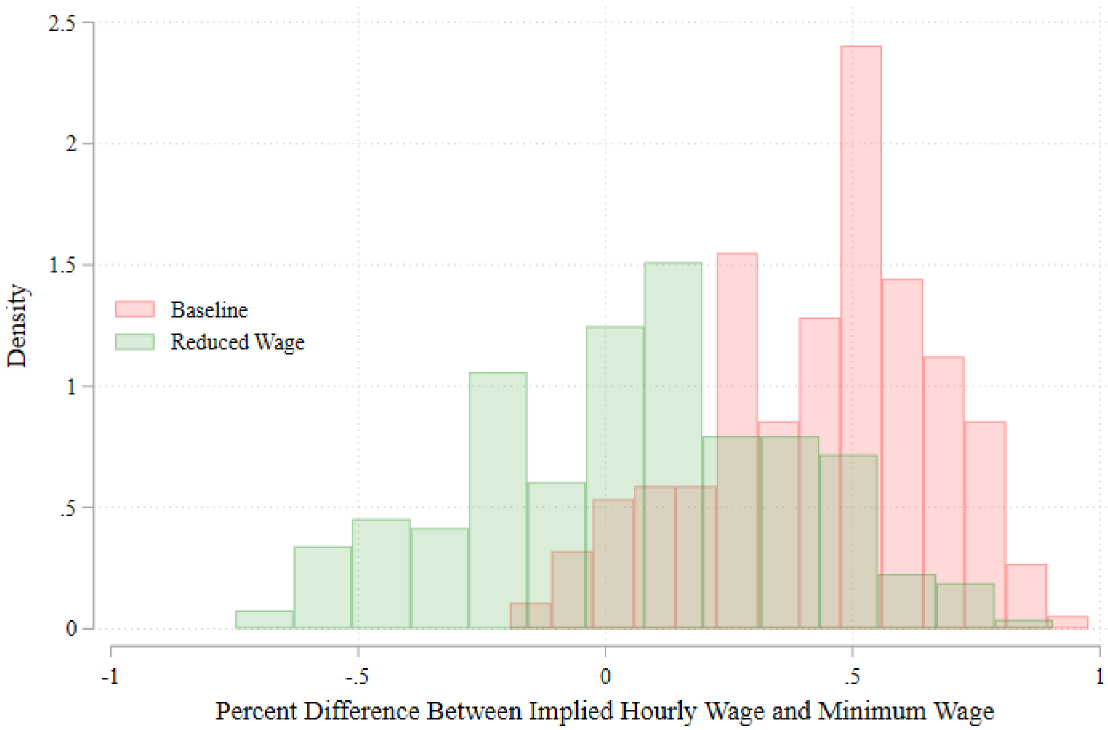
Notes. Rows (1) and (2) report the effect of raising the OT exemption threshold on the number of salaried jobs below and above the new threshold, respectively, scaled by the number of affected salaried jobs. Affected jobs are defined as salaried employees with base pay between the old and new thresholds on the month before 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. 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.



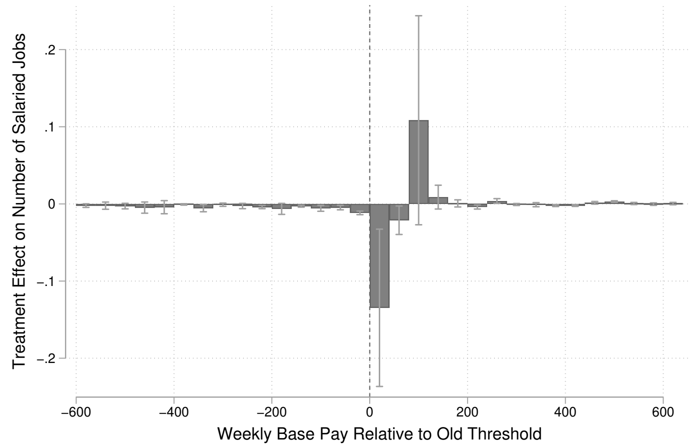
Appendix Figure G.3
Implied Hourly Wage

Notes. This figure plots the distribution of implied wages among salaried workers in the CPS who reported weekly earnings between the old and new overtime exemption thresholds in the year before a policy change.

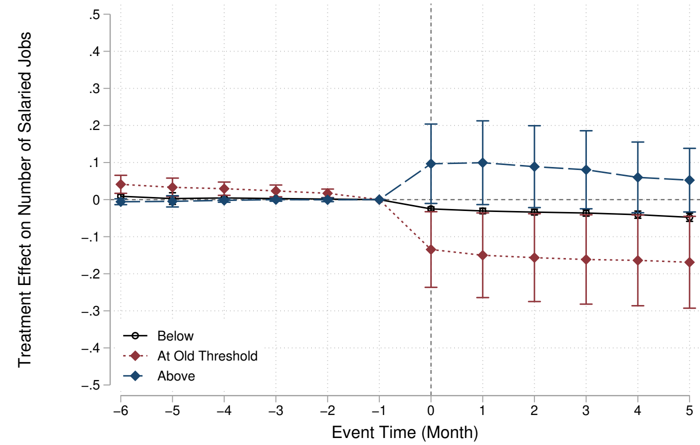


Appendix Figure G.4
Distribution of Wages Relative to Minimum Wage

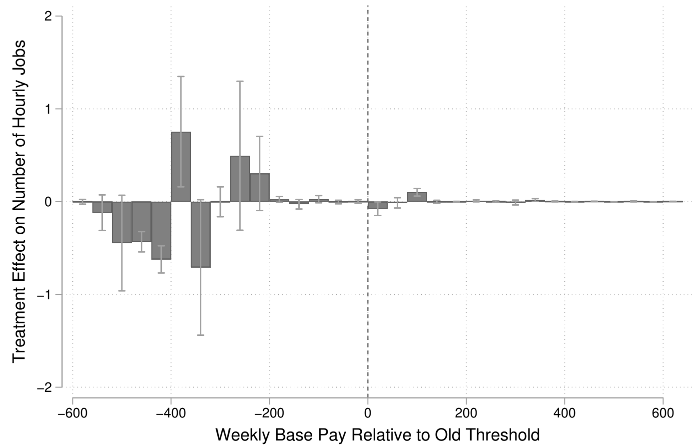
Notes. This figure plots the distribution of workers' wages relative to the minimum wage. The red bars calculate workers' current implied wage given their weekly earnings and hours. The green bars plot the implied wage from a reduction in weekly base pay that keeps earnings and hours constant after accounting for the overtime premium. The sample is restricted to treated salaried workers who report working over 40 hours last week.



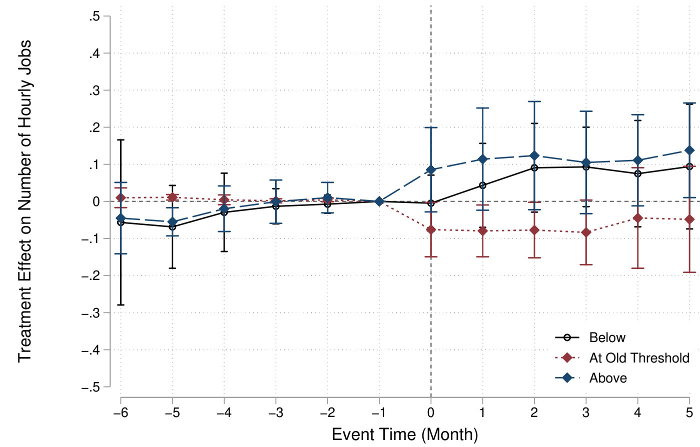
(a) Effect on Number of Salaried Jobs, at Time 0



(b) Effect on Salaried Distribution Over Time



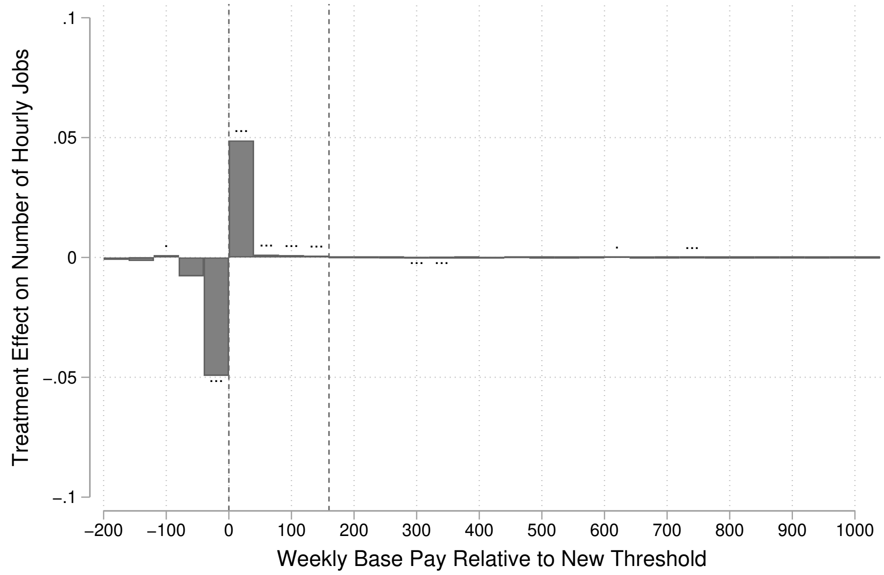
(c) Effect on Number of Hourly Jobs, at Time 0



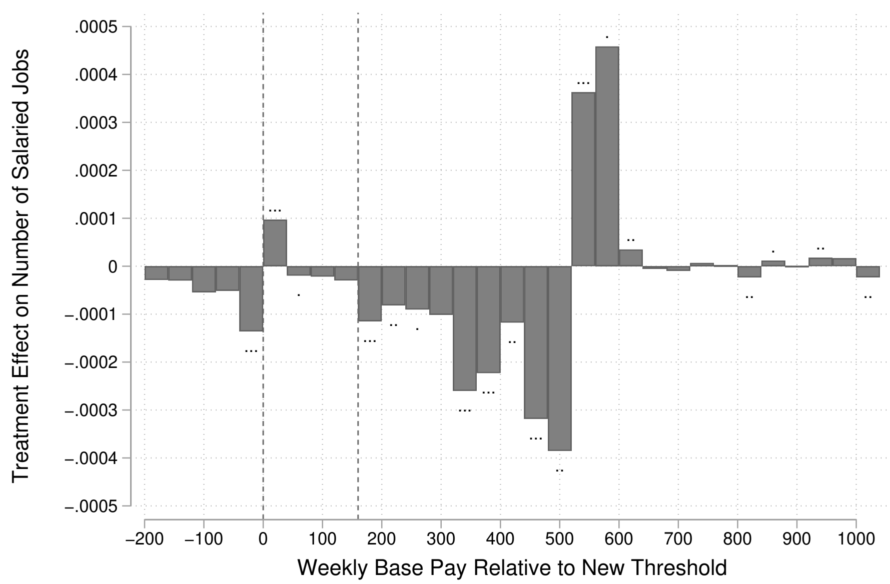
(d) Effect on Hourly Distribution Over Time

Appendix Figure G.5
Effect of Raising States' OT Exemption Thresholds on the Frequency Distribution of Base Pay, Centered at Old Threshold

Notes. This figure plots the estimates from equation 1, with the bins of base pay centered at the old overtime exemption threshold for each event. In panels (a) and (c), the height of each bar indicates the effect of raising the OT exemption threshold on the number of salaried and hourly jobs, respectively, 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. Panels (b) and (d) show the sum of the estimates over time, separately for bins within \$160 below the old threshold, within \$40 above the old threshold, and within \$40 to \$200 above the old threshold. For each estimate, I show the 95% confidence interval using standard errors clustered by state.



(a) Hourly Jobs



(b) Salaried Jobs

Appendix Figure G.6
Effect of Increasing the Minimum Wage on the Distribution of Weekly Base Pay

Notes. This figure plots stacked difference-in-difference estimates of the effect of the minimum wage changes in California, New York, Alaska, and Maine on the distribution of weekly base pay in the month of the policy change. Each estimate is scaled by the total number of workers in the treated states in the month before the reform. The bins are centered so that the new minimum wage for each event is 0. The left vertical dashed line is at the new minimum wage and the right vertical dashed line is at \$3/hour above the new minimum wage, where Cengiz et al. (2019) finds spillover effects. P-values: ·10%, · · 5%, · · ·1%, significance level.

Appendix H. Distribution of Hours and Interactions with the ACA

This section examines the distribution of hours for hourly workers. Similar to overtime pay, the variable for hours is aggregated over all paychecks in a month, and I convert it to an average weekly value following the procedure in Appendix D. Figure H.1 plots the distribution of hourly workers' hours in the month before an increase in the overtime exemption threshold. Despite the imprecision from converting monthly hours to an average weekly value, panel (a) shows that about 17% of hourly workers average 40 hours per week, more than twice the number of workers at any other specific hour. Panel (b) likewise finds bunching at multiples of 40 hours, where the spike in monthly hours at 200 and 240 reflect workers who are paid weekly and biweekly, respectively, and received an extra paycheck in the month.

Aside from the costs of complying with the overtime regulation, another factor that may affect employers' response is interaction with the Affordable Care Act. Under the ACA, firms with at least 50 full-time employees are required to offer affordable health insurance to all their full-time workers, which the IRS defines as "an employee employed on average at least 30 hours of service per week, or 130 hours of service per month," including both base and overtime hours (Internal Revenue Service, 2025). Thus, employers have incentives to keep workers' average hours below 30 per week.[26]

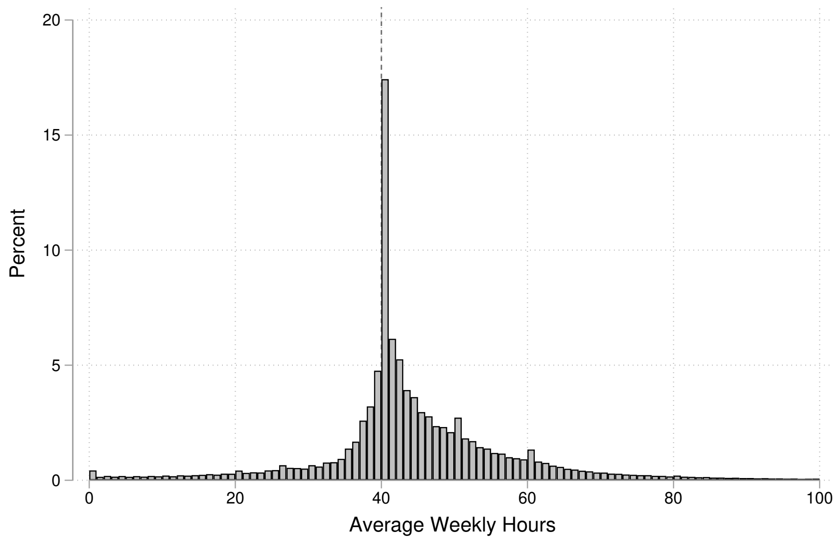
Theoretically, the ACA could exacerbate the hours response to expansions in overtime coverage. For example, consider an exempt salaried employee working 50 hours per week. To exclude that worker from ACA requirements, the firm must reduce hours by at least 20. However, if the firm was already cutting hours to 40 to avoid overtime costs, the marginal adjustment to avoid the ACA falls to just 10 additional hours, making the two policy incentives complementary.

In practice though, my analysis suggests that the ACA is unlikely to interact with the overtime exemption threshold. First, Figure H.1 finds no evidence of bunching at either the 30 weekly hours or 130 monthly hours cutoffs. Using weekly level data from another payroll provider, Goff (2020) likewise finds no bunching at 30 hours per week relative to other round numbers.[27] Second, Figure M.1 finds that only 6% of salaried workers targeted

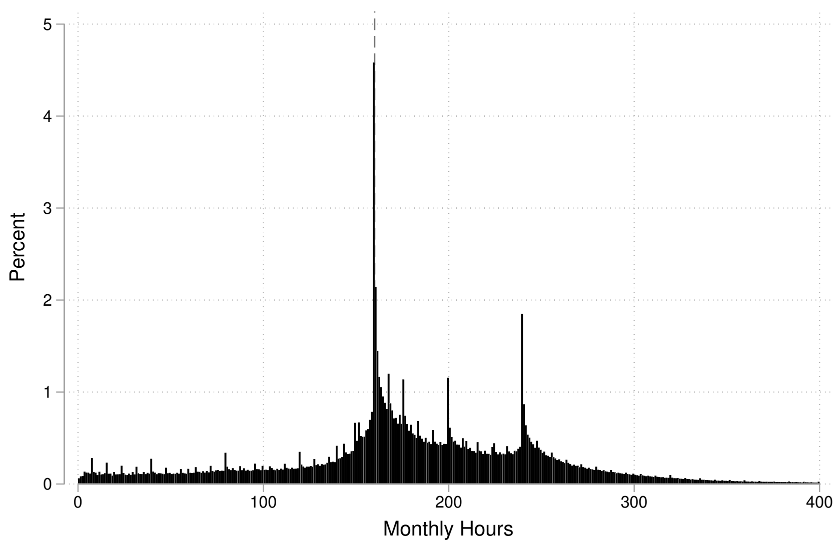
[26] Employers have some discretion on the time frame over which they would like to calculate average weekly or average monthly hours, ranging from once per month to once a year. Regardless of the time frame used to calculate average hours, the incentive remains for employers to cut work hours.

[27] However, he does find bunching at 24 and 32 hours due to individuals taking an 8-hour day or two off work. I do not find similar bunching since I am unable to observe exact weekly hours in the payroll data. Instead, I divide total monthly hours by the number of weeks worked, resulting in a smoother distribution. For example, if someone works 8-hour days, took 1 day off in the month, and worked 4 weeks, then their average weekly hours would be $\frac{4*40-8}{4} = 38$.

by the overtime policies report working less than 30 hours per week. This suggests that even for the popul ... to the ACA hours criteria.



(a) Average Weekly Hours



(b) Monthly Hours

Appendix Figure H.1
Distribution of Hours Among Hourly Workers

Notes. The figure shows the distribution of hours among hourly workers with base pays between the old and new overtime exemption threshold in the month before a policy change. Monthly hours are summed over all paychecks received in the month. Average weekly hours is imputed from monthly hours following the methodology in Appendix D. Each bin is a one hour increment. The dotted vertical line is at 40 hours per week for Panel (a) and 160 hours per month for Panel (b).

Appendix I. Effect on Hours: Evidence from CPS Data

In this section, I supplement the analysis in the main text using data from the CPS Outgoing Rotation Survey. Although the CPS lacks the precision and sample size of the payroll data, an advantage of the CPS is that it asks respondents the number of hours they worked in the previous week. I leverage this variable, along with information on respondents' salaried/hourly status and weekly earnings, to examine the effect of raising the overtime exemption threshold on workers' hours.

In particular, I use workers' responses at their first Outgoing Rotation Group (ORG) interview to assign treatment status. The CPS surveys each respondent for four consecutive months, then leaves them out of the sample for eight months, and then surveys them again for four more months. The ORG, which is administered on the fourth and sixteenth months, asks respondents about their weekly earnings and salary/hourly status. For each policy change, I define the treatment group as workers who report "no" when asked if they are paid hourly and report baseline weekly earnings between the old and new salary thresholds. As a placebo group, I also use salaried workers earning between the new threshold and \$100 above it. To avoid imputation error, I only keep observations for whom neither their weekly earnings, weekly hours, or salaried/hourly status are imputed.

Similar to the analysis in the main text, I estimate a difference-in-difference design comparing workers in treated states to those in control states, restricting the sample to workers of similar salary ranges. In particular, I estimate the following regression:

$$\log(Y_{it}) = \sum_{q=-8}^{3} \beta_q I_{sq} + \alpha_{iv} + \delta_{tv} + \varepsilon_{it} \tag{25}$$

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 (δ_{tv}) fixed effects. The treatment interacted with event-time indicators (I_{sq}) are aggregated at the quarterly level, q. Although the payroll data covers eight years from 2014 to 2021, the CPS has a much longer history. For statistical power, I expand my sample period to include all 49 state overtime threshold changes from 2008 to 2026. I cluster estimates at the state level and weight observations using the CPS's sampling weights.

By controlling for individual fixed effects, my preferred specification follows the same individuals over time. For outcomes that are only observed in the Outgoing Rotation Group survey, namely weekly earnings and pay classification, each individual contributes at most two observations separated by one year. As a result, β_q in the regression represents the change in workers' outcomes relative to their response one year earlier. Since individuals appear in the data with a one-year gap, four quarters must be omitted for Equation (25) to be identified. I set quarters -8 to -5 as the baseline reference periods so that the estimates are defined for periods -4 to $+3$. In contrast, since the CPS asks respondents their weekly hours in every month of the survey, I only omit the baseline period $t = -1$ to maximize the sample size.

Figure I.1 presents the estimates of equation 25 for four outcome variables. First, as a validity check on the quality of the data, I replicate my earlier analysis of the income and reclassification effects of expanding overtime coverage for salaried workers. Panel (a) shows that treated and control states had fairly similar earnings growth in the year prior to the reform. Average weekly earnings then increases immediately following the expansion in overtime coverage, albeit the estimates tend to fluctuate over time given the small sample size. In panel (b), I find a clear decrease in the probability that a worker remains salaried following the policy change. The successful replication of the income and reclassification effects, although imprecisely estimated, builds confidence that the earlier results in the payroll data are externally valid.[28]

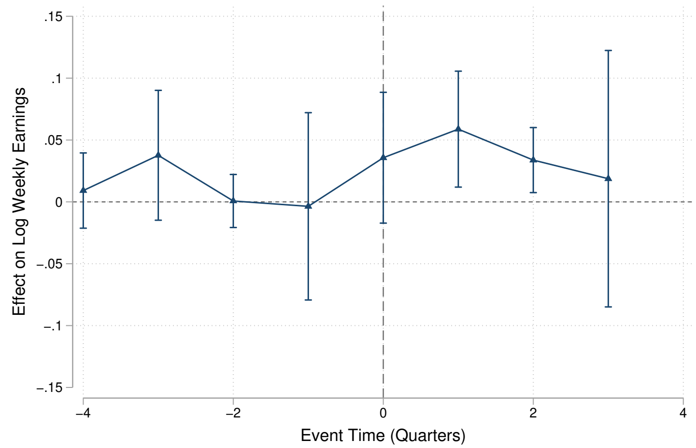
After confirming that the CPS data is able to identify workers affected by the overtime reforms, I next estimate the effect on workers' hours. Panel (c) suggests that raising the overtime exemption threshold did not significantly impact workers' hours. In principle, the positive earnings effect and null hours effect suggest that workers' implied hourly wage increased (i.e. $\frac{\text{Weekly Earnings}}{\text{Weekly Hours}}$). However, the estimated impact on hourly wage in panel (d) are too imprecise to draw meaningful conclusions. Overall, I find no evidence that increasing the overtime exemption threshold raised workers' hours, but the CPS data does not have enough observations to determine the impact on workers' wages.

Table I.1 assesses the robustness of my analysis of the CPS data. First, the estimates in column (1) are analogous to Figure I.1, except I aggregate the dynamic effects using a simple indicator for the treated states post-reform. Consistent with the figure, I find a 2.5% (s.e. 0.7%) increase in weekly earnings among the treatment group and no change in workers' hours or wages. As a placebo check, Panel (b) estimates the difference-in-difference for workers earning up to \$100 per week above the new threshold, and finds no significant effects on earnings. The 95% confidence interval of the hours effect rules out hours changes below or above 1.2%. That suggests that hours did not rise significantly enough to offset the 2.5% increase in workers' earnings. However, the estimated effect on workers' hourly wage is fairly imprecise, with the 95% confidence interval ranging from -1.4% to +2.9% in the

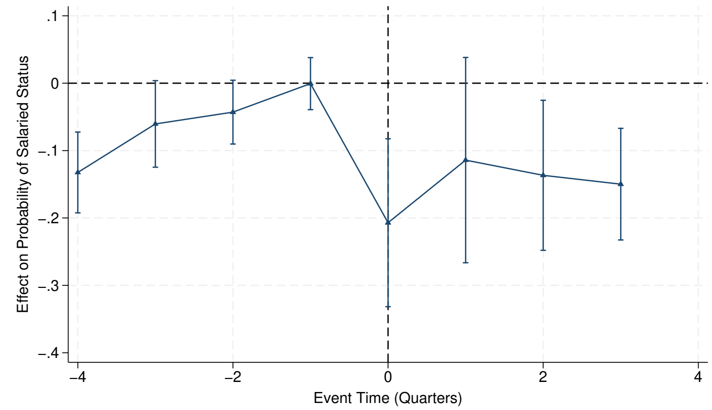
[28]Given the relatively small sample size of the CPS, I do not have enough statistical power to replicate the bunching results.

baseline specification. Although I cannot rule out zero effects on hourly wages, I also cannot rule out that hourly wages increased by the same amount as weekly earnings.

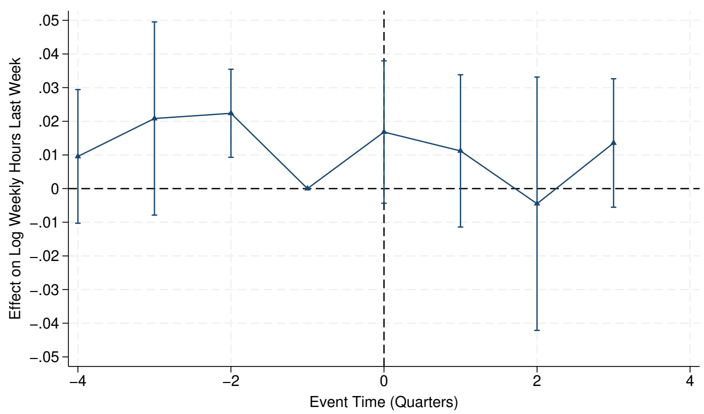
Column (2) adds demographic controls for gender, race, marital status, age, and age-squared. Lastly, columns (3) and (4) add industry and occupation specific month fixed effects, respectively. These stricter specifications account for industry and occupation specific time trends so that I am comparing workers in similar jobs. I find positive earnings effects across all four specifications, whereas the hours effects fluctuate around zero. Meanwhile, the placebo test finds primarily null effects, suggesting that the earnings effect is not simply capturing broader trend differences between the treatment and control groups. Taken together, the evidence suggests that raising the overtime exemption threshold increases workers' earnings, without significantly impacting workers' hours, though the net effect on hourly wages is imprecisely estimated.



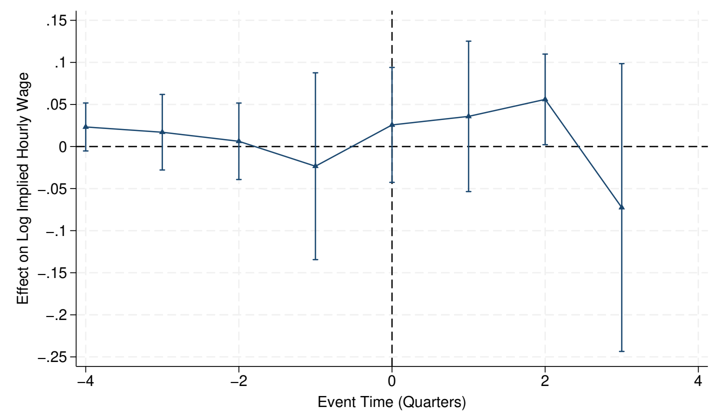
(a) Log Weekly Earnings



(b) Salaried Status



(c) Log Weekly Hours



(d) Log Wage

Appendix Figure I.1
Difference-in-Difference Estimates of the Income, Reclassification, and Hours Effects using the CPS

Notes. This figure shows the estimates of equation 25, which compares the outcomes of CPS respondents in states that raised the overtime exemption threshold to respondents in states that did not.

Appendix Table I.1
Effect of Raising the OT Exemption Threshold on Workers' Earnings and Hours

	(1)	(2)	(3)	(4)
Panel A: Treatment (n=78,666)				
Log Weekly Earnings	0.025***	0.025***	0.036**	0.067**
	(0.007)	(0.007)	(0.017)	(0.031)
Log Weekly Hours	-0.000	-0.001	-0.004	-0.003
	(0.006)	(0.006)	(0.010)	(0.009)
Log Hourly Wage	0.007	0.008	0.004	0.076
	(0.011)	(0.011)	(0.026)	(0.050)
Panel B: Placebo (n=109,110)				
Log Weekly Earnings	0.005	0.005	0.021*	0.007
	(0.005)	(0.005)	(0.011)	(0.015)
Log Weekly Hours	-0.005	-0.005	-0.000	-0.010*
	(0.006)	(0.006)	(0.008)	(0.005)
Log Hourly Wage	0.007	0.007	0.016	0.014
	(0.015)	(0.015)	(0.026)	(0.027)
Event-Individual	Y	Y	Y	Y
Event-Month	Y	Y		
Event-Month-Industry			Y	
Event-Month-Occupation				Y
Controls		Y	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 three outcome variables (i.e. log weekly earnings, log weekly hours, and log hourly wage) 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 4 digit industry-event-month fixed effects. Column (4) adds 4 digit occupation-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 used to identify the earnings effect. All robust standard errors in parentheses are clustered by state. *10%,** 5%,*** 1% significance level.

Appendix J. Analysis by Event

In this section, I analyze each of the 19 state events separately to examine how the labor market effects of expanding overtime coverage vary across different settings. Appendix Figure J.1 replicates the event-specific employment effects, equivalent to Figure 5, but keeps the three events in Maine. This section investigates the cause of the outlier estimates and the predictors of the employment elasticity across the 19 events.

J.a Salaried Jobs

My first goal is to understand why the estimates in New York 2016 and Maine 2017 are much more negative than the estimates from the other policies. I begin by examining the distributional effects and the pre-trends for each event, separately for salaried and hourly employment.

Figure J.2 plots the employment effect along the distribution of weekly base pay for salaried jobs, by event. Each estimate is scaled by the number of salaried workers between the old and new thresholds in the month before its respective policy change. Similar to the results from the stacked regression analysis, I find clear bunching above the new overtime exemption threshold in all but 3 events. Two of the instances where there is no bunching coincide with the events for which I have the fewest observations. For instance, I have only 672 firms in Alaska 2015, of which only 47 have at least one salaried worker in the treated interval. The average firm has 0.17 affected salaried workers, translating to a total of 115 treated workers across the entire state for that event. Given the small number of treated firms and workers, the response in Alaska may be diluted by other idiosyncratic shocks. On the other hand, it is a mystery why there was no bunching in response to the policy in Maine 2018 when firms bunched workers in 2017 and 2019.[29] Besides the three outlier cases, all the other events led to a bunching mass in the salary distribution, albeit the share of treated workers who are bunched differs substantially across events, even within the same state over time.

To show that the cross-sectional estimates are not driven by other contemporaneous shocks, I test the parallel trends assumption in Figure J.3. For most events, I find that the number of workers in the treated states are trending similarly as those in the control states prior to the policy change. However, there are a few exceptions. First, there appear to be

[29] Examining the raw data, I can rule out firms decreasing workers' salaries in 2018. The bunching at the old threshold is coming from even lower paying jobs whereas the decreased mass above the new threshold can be accounted for by growth to the right of the distribution.

some anticipation effects in response to the 2018 policy in New York. For consistency, I do not adjust for this anticipatory response in my estimates.[30] Second, the parallel trends assumption fails for my analysis of Alaska. Third, despite having similar pre-trends to other states, the labor market response to the policy changes in Maine appears to differ drastically from those of the other events. Although the cross-sectional estimates in Figure J.2 were as of the first month that the policy went into effect, I find that the bulk of the response to the event in Maine 2017 was delayed by 3 months. Moreover, that response is larger in magnitude than the bunching effect from any other event in the sample. Despite these discrepancies in Alaska and Maine, I largely find similar responses across policies in California, New York, Colorado, and Washington.

J.b Hourly Jobs

In contrast to salaried jobs, there is substantially more variability in the response of hourly jobs across the 19 events. Repeating my analysis for hourly workers, Figure J.4 finds that the entire bunching of hourly jobs is driven by California. There is no clear pattern to the estimates from the remaining events. I explore the spillover effects onto hourly jobs in more detail in Appendix K.

The cross-sectional estimates provide some insight into why ME 2017 and NY 2016 had such large negative employment effects in Figure J.1. First, the event in Maine 2017 is contaminated by the minimum wage. As described in the main text, the minimum wage increased to only \$95 below the old overtime exemption threshold, leading to the large spike in the left tail of the distribution in Figure J.4. This spike from the minimum wage is an order of magnitude larger than the estimates from any of the other events and can lead to spillover effects onto the segment of the income distribution affected by the change in the overtime exemption threshold.

Second, the 2016 policy in New York was the smallest out of all the events, increasing the threshold by only \$18.75. Suppose my estimator captures both the treatment effect of the policy and a small degree of bias. For a large policy, the magnitude of the treatment effect makes the bias quantitatively negligible, whereas for a small policy, I need a flawless counterfactual control group to parse out the treatment effect. As a result, the negative employment estimate from the 2016 New York policy may simply reflect other idiosyncratic shocks to hourly employment, hence why employment falls so drastically even outside the salary intervals affected by the policy.

[30] Accounting for the anticipation response in the stacked difference-in-difference would largely not change results since it simply moves the baseline period from $t = -1$ to $t = -2$ in Figure 2.

To test whether selection bias may be affecting some of the outlier results, Figure J.5 shows that the parallel trends test fails for about half the events, particularly in New York, Alaska, and Maine. Salaried employment did not face a similar issue because those tend to be fairly stable jobs, so both the treatment and control groups exhibit little variation in employment from month-to-month. However, given the high turnover of hourly jobs, a simple difference-in-difference design fails to adequately match the fluctuations in hourly employment in many cases. Nevertheless, Table J.1 shows that the main results of the stacked difference-in-difference design hold even when I restrict the sample to California, Colorado, and Washington, the 9 events that do not violate the parallel trends assumption. Moreover, after dropping the noisy events, the estimates of the employment effect are much more precise, though their direction varies with the specification.

J.c Synthetic Control

Given that the simple difference-in-difference design does not credibly identify the causal effect on hourly jobs for many of the events, I use a synthetic difference-in-difference estimator as an alternative identification strategy (Arkhangelsky et al., 2021). This method essentially weights the control states in each event to match the pre-trends of the treated state. Since the synthetic control method was designed to match pre-trends for a single outcome, rather than for an entire distribution, I only estimate a net employment effect, localized around the treated salary range. For inference, the synthetic difference-in-difference computes standard errors using placebo regressions, similar to the method proposed by Conley and Taber (2011).

Figure J.6 plots the synthetic difference-in-difference estimates for each event. Although the confidence intervals from the synthetic design are far larger than those from my previous regressions, the results are fairly similar. Panels (a) and (b) again show a decrease in salaried employment and an increase in hourly employment across nearly all the events. Even after matching on pre-trends, panels (c) and (d) find that 12 of the 16 reported estimates for the change in employment point in the same direction as before.

J.d Predicting Heterogeneous Response

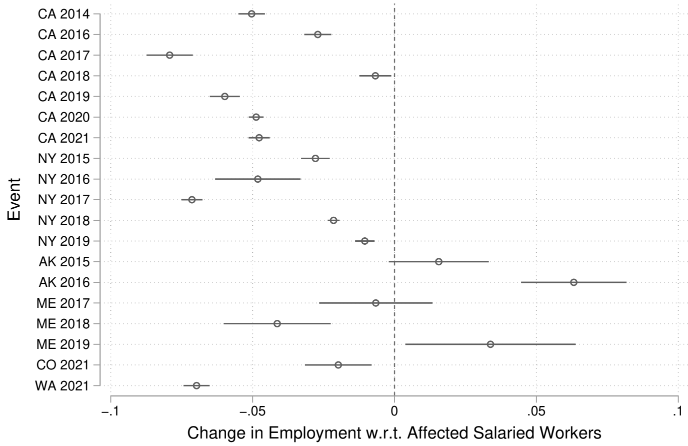
Next, I test whether the heterogeneity in employment effects across events can be explained by the characteristics of the policies or the labor market conditions during the period of the event. In particular, I regress the estimates of the treatment effects on a series of potential predictors:

$$Y_v = \beta_0 + \beta_1 X_v + \varepsilon_v \tag{26}$$

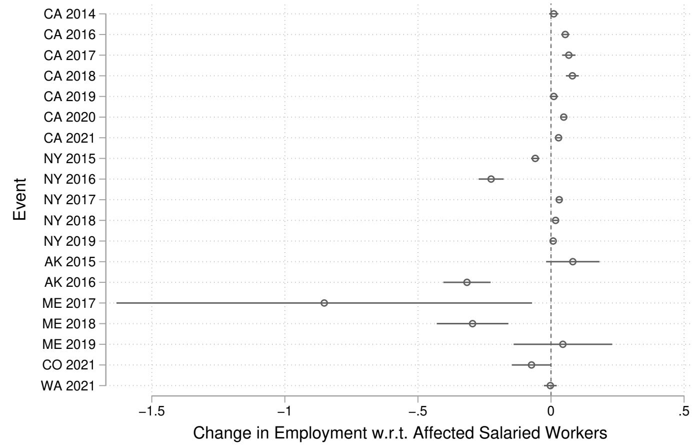
where Y_v is either the magnitude of the bunching mass or the cumulative employment effect for event v and X_v is a characteristic of the policy or state during that event. Estimates are clustered at the state level.

Figure J.7 plots the line of best fit between the size of the bunching mass and six explanatory variables. The first panel finds that the magnitude of the new threshold explains 34% of the variation in the share of salaried workers who get bunched above the new overtime exemption threshold. The second panel shows that the value of the new threshold relative to the median weekly earnings in that state-year, computed using the CPS, gives a similar correlation. Even though the overtime exemption threshold is often a multiple of states' minimum wages, the third panel finds that the minimum wage does a poorer job explaining the variation in the bunching mass. In the fourth and fifth panels, I similarly find no significant relationship between the bunching mass and the share of workers affected or the percent increase in the overtime exemption threshold. In the final panel, I find a positive correlation between a state's unemployment rate and the size of the bunching mass, albeit the R-squared is weaker than that of the first panel. Overall, the policy parameter that best explains the magnitude of the bunching response is the nominal value of the new threshold.

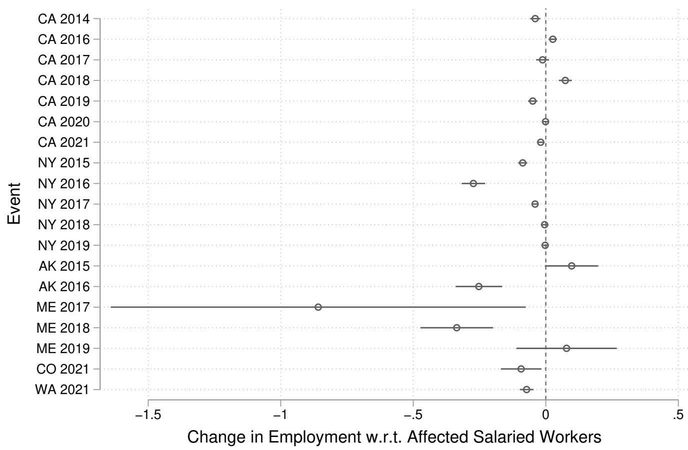
Figure J.8 repeats the analysis using the net change in employment as the outcome variable. Similar to the previous case, I find that the magnitude of the new threshold explains the largest share of the variation in the employment response. I also find a statistically significant correlation between the employment effect and the state unemployment rate. However, the positive correlations across the 6 panel are heavily impacted by the two outlier events with large negative employment effects: New York 2016 and Maine 2017. To correct for these outliers, Table J.2 weights each event's estimate by the inverse of its standard error so that less precise estimates have a weaker impact on the slope. Although the magnitude of the new threshold and the unemployment continue to be positively correlated with the size of the bunching mass, they are not predictive of the employment effects.



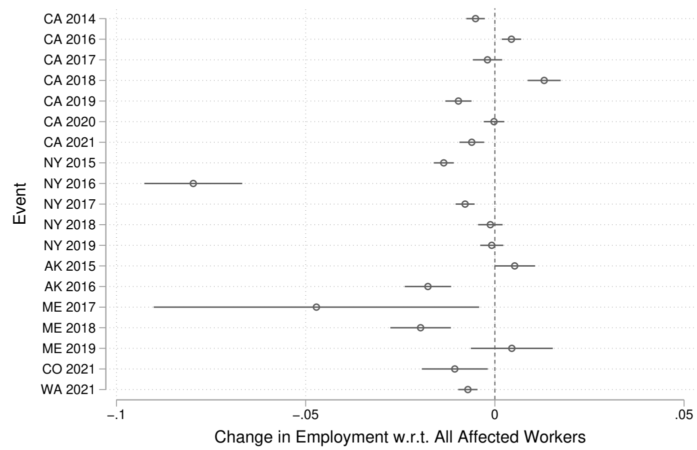
(a) Salaried Jobs



(b) Hourly Jobs



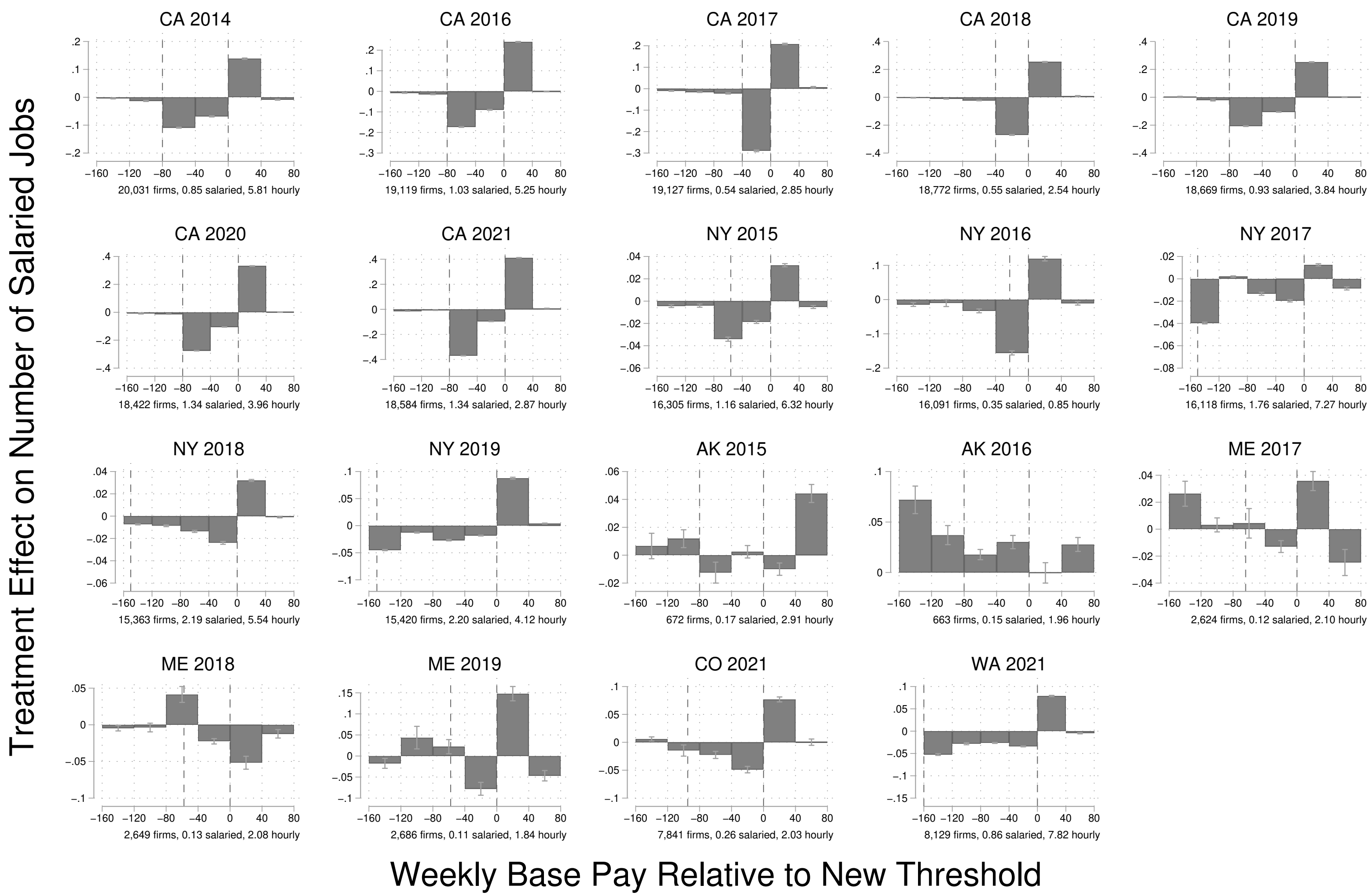
(c) All Jobs, per Affected Salaried Worker



(d) All Jobs, per Aff. Sal & Hr Worker

Appendix Figure J.1
Event-Specific Employment Effects Scaled, All 19 Events

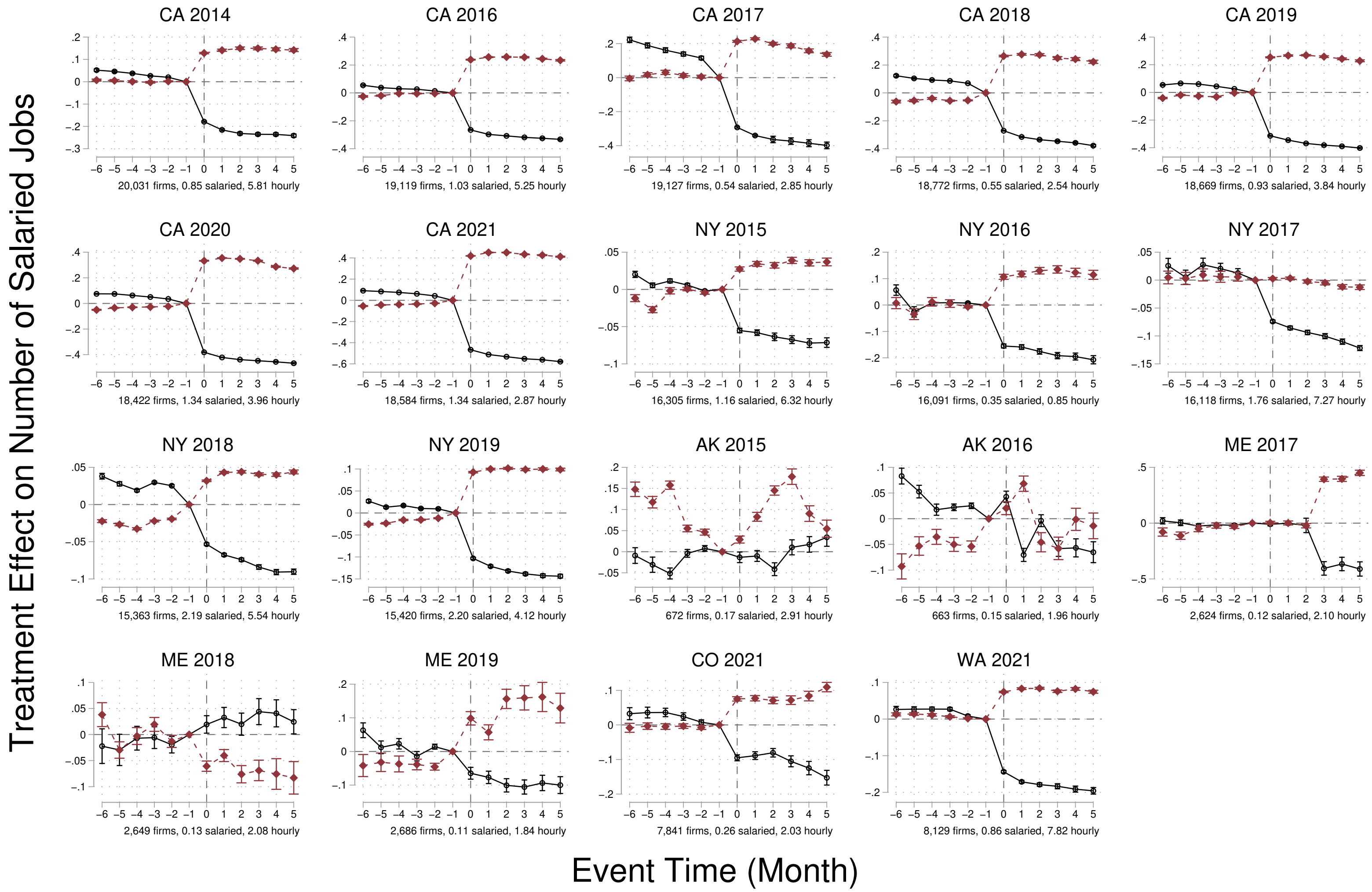
Notes. The figure shows the difference-in-difference estimates from equation 1, separately for all 19 events. Panels (a)-(c) report the change in employment per salaried worker between the old and new thresholds in the month before each policy change. Panel (d) divides the treatment effect by the total number of salaried and hourly in the affected pay interval. The bars around each estimate represent 95% confidence intervals. Standard errors are clustered by state.



Appendix Figure J.2

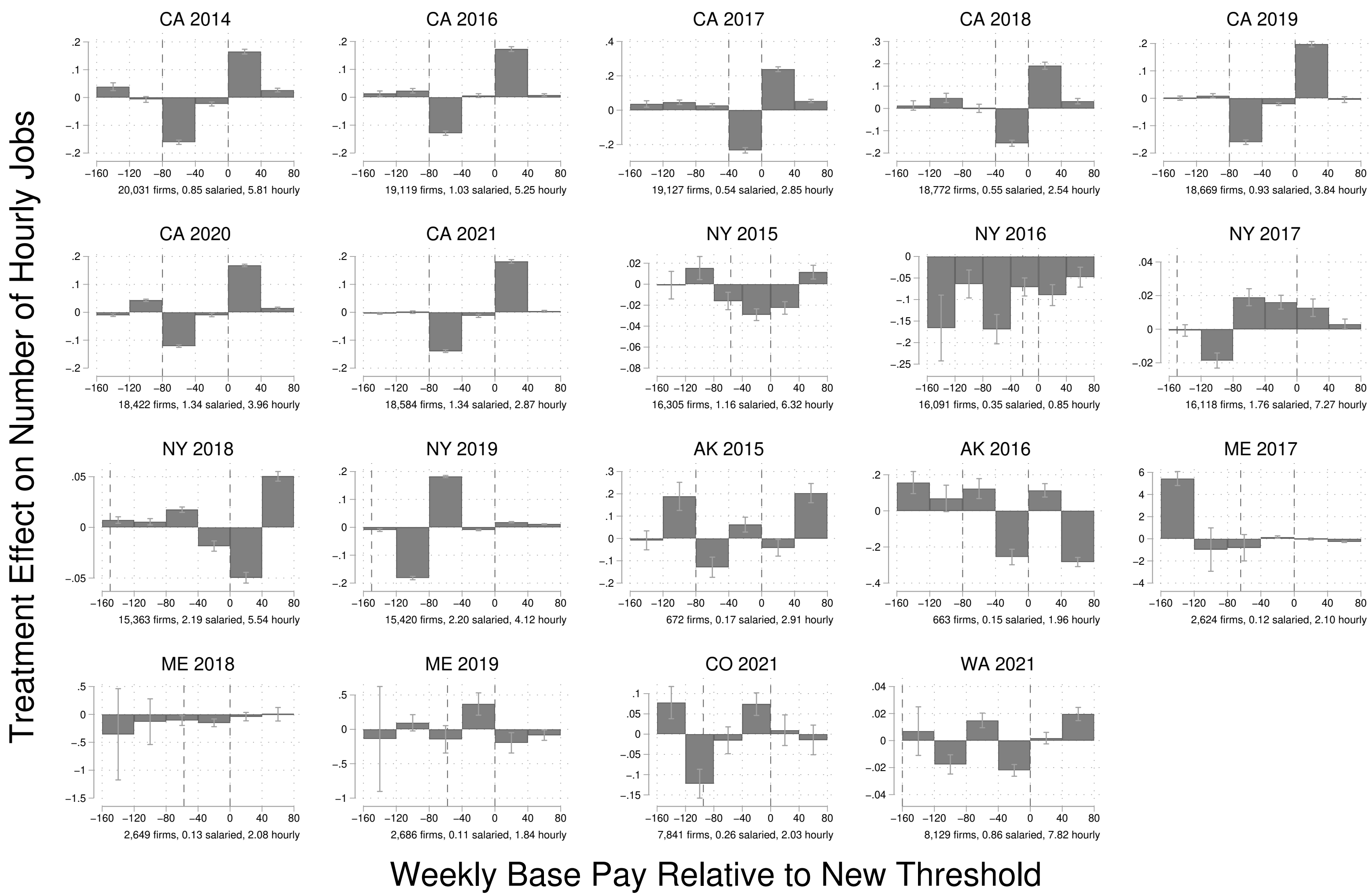
Effect of Increasing the Overtime Exemption Threshold on Salary Distribution, by Event

Notes. The figure shows the effect of each event on the distribution of weekly base pay in the month the policy went into effect. The outcome variable is the number of salaried jobs, divided by the number of salaried workers between the old and new overtime exemption threshold in the month before the policy change. Reported under each panel are the total number of firms in the treated state and the average number of treated workers per firm. Standard errors are clustered by state.



Appendix Figure J.3
Effect of Increasing the Overtime Exemption Threshold on Salary Distribution, by Event

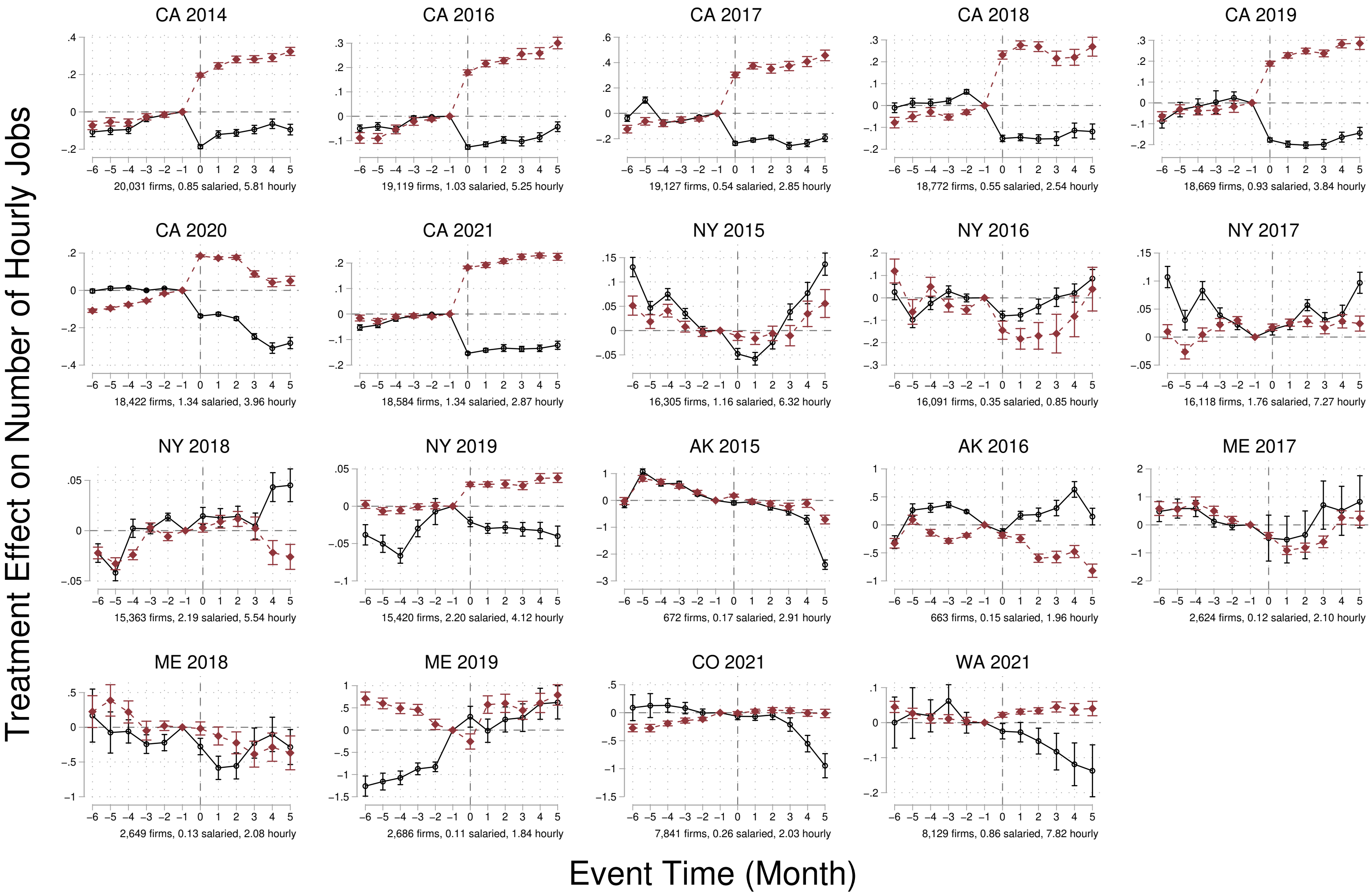
Notes. The figure shows the effect of each event on the number of salaried jobs below and above the new overtime exemption threshold in the 6 months before and after the event. Estimates are scaled by the number of salaried workers between the old and new overtime exemption threshold in the month before the policy change. Reported under each panel are the total number of firms in the treated state and the average number of treated workers per firm. Standard errors are clustered by state.



Appendix Figure J.4

Effect of Increasing the Overtime Exemption Threshold on Hourly Distribution, by Event

Notes. The figure shows the effect of each event on the distribution of weekly base pay in the month the policy went into effect. The outcome variable is the number of hourly jobs, divided by the number of salaried workers between the old and new overtime exemption threshold in the month before the policy change. Reported under each panel are the total number of firms in the treated state and the average number of treated workers per firm. Standard errors are clustered by state.



Appendix Figure J.5

Effect of Increasing the Overtime Exemption Threshold on Hourly Distribution, by Event

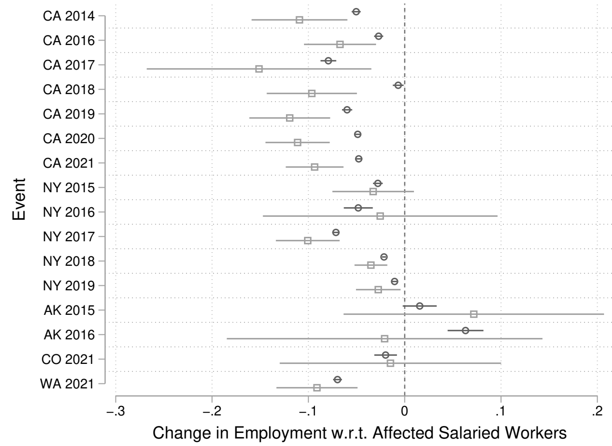
Notes. The figure shows the effect of each event on the number of salaried jobs below and above the new overtime exemption threshold in the 6 months before and after the event. Estimates are scaled by the number of salaried workers between the old and new overtime exemption threshold in the month before the policy change. Reported under each panel are the total number of firms in the treated state and the average number of treated workers per firm. Standard errors are clustered by state.

Appendix Table J.1
Effect of Raising the Overtime Exemption Threshold on the Pay Distribution: Restricted to CA, CO, WA

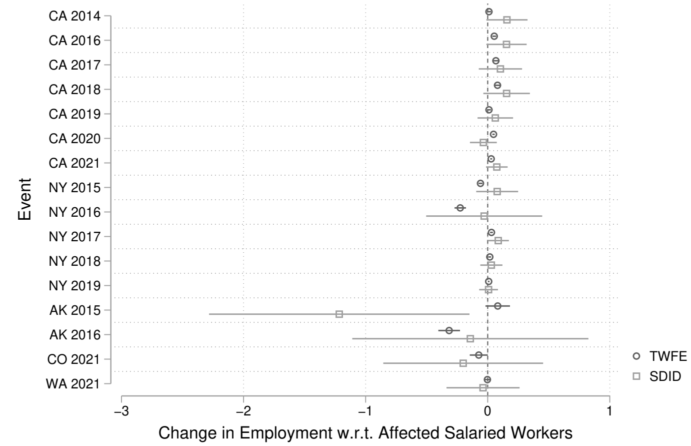
	(1)	(2)	(3)	(4)	(5)
Panel A					
Salaried Jobs					
Below Threshold	-.334***	-.275***	-.341***	-.408***	
	(.034)	(.031)	(.05)	(.097)	
Above Threshold	.266***	.221***	.277***	.346***	
	(.029)	(.024)	(.043)	(.08)	
Hourly Jobs					
Below Threshold	-.119***	-.116***	-.072***	-.032***	
	(.015)	(.015)	(.011)	(.02)	
Above Threshold	.185***	.155***	.171***	.213***	
	(.023)	(.021)	(.029)	(.042)	
Panel B					
Emp. w.r.t. Aff. Salaried	-.002	-.016***	.035*	.119***	-.002
	(.01)	(.005)	(.018)	(.027)	(.01)
Emp. w.r.t. All Affected	0	-.003***	.006*	.022***	0
	(.002)	(.001)	(.003)	(.005)	(.002)
Number of Affected Salaried	.898	.898	.887	.887	.898
Number of Affected Hourly	4.013	4.013	4.02	4.02	4.013
Event-Bin-Month FE	Y	Y	Y	Y	
Event-Bin-State FE	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)	148,694	148,694	108,577	108,577	148,694
Number of event-firms (control)	2,425,512	2,425,512	1,331,135	1,331,135	2,425,512

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 thresholds on the month before 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.

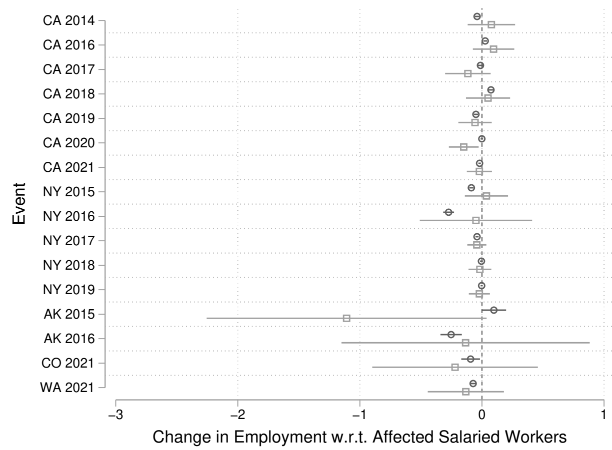
Column (1) reports the effects of the state threshold increases on the exact month of the rule change, estimated using equation 1. Column (2) estimates the effects using within-census division variation. Column (3) uses variation within firms that operate in both the treatment and control groups. Column (4) estimates a saturated model. Column (5) 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.



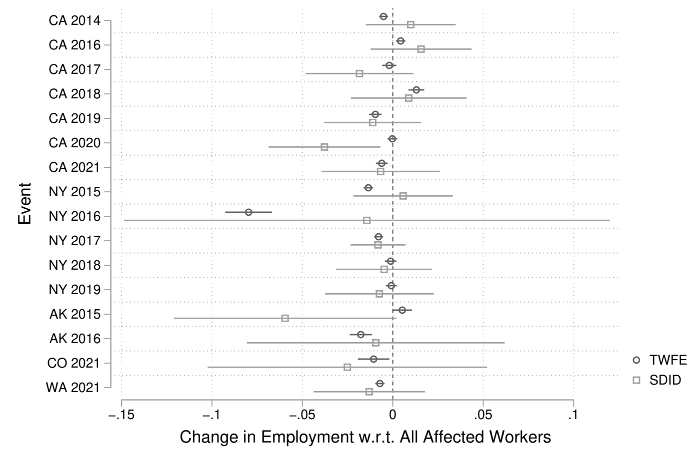
(a) Salaried Jobs



(b) Hourly Jobs



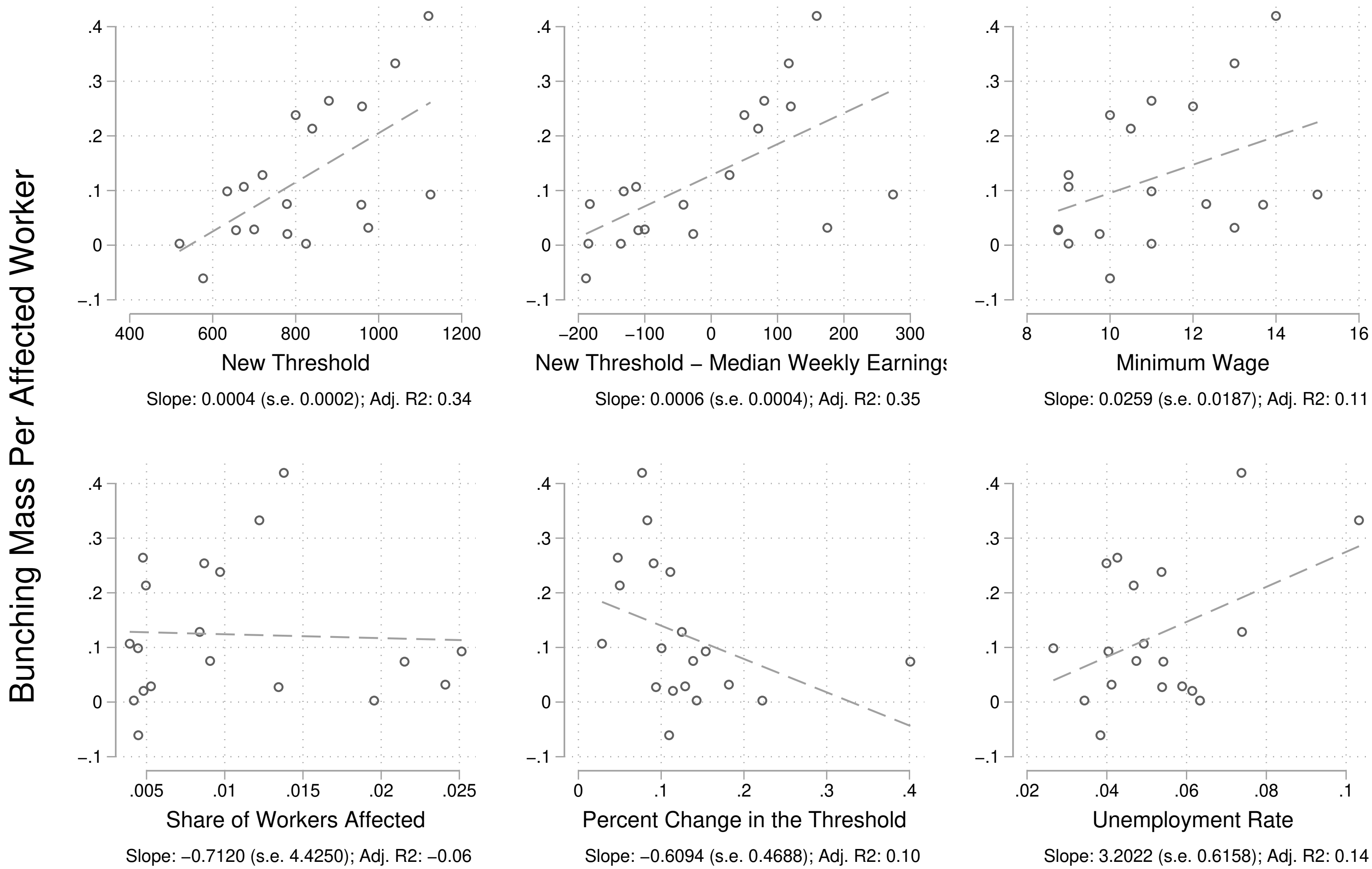
(c) All Jobs, per Affected Salaried Worker



(d) All Jobs, per Aff. Sal & Hr Worker

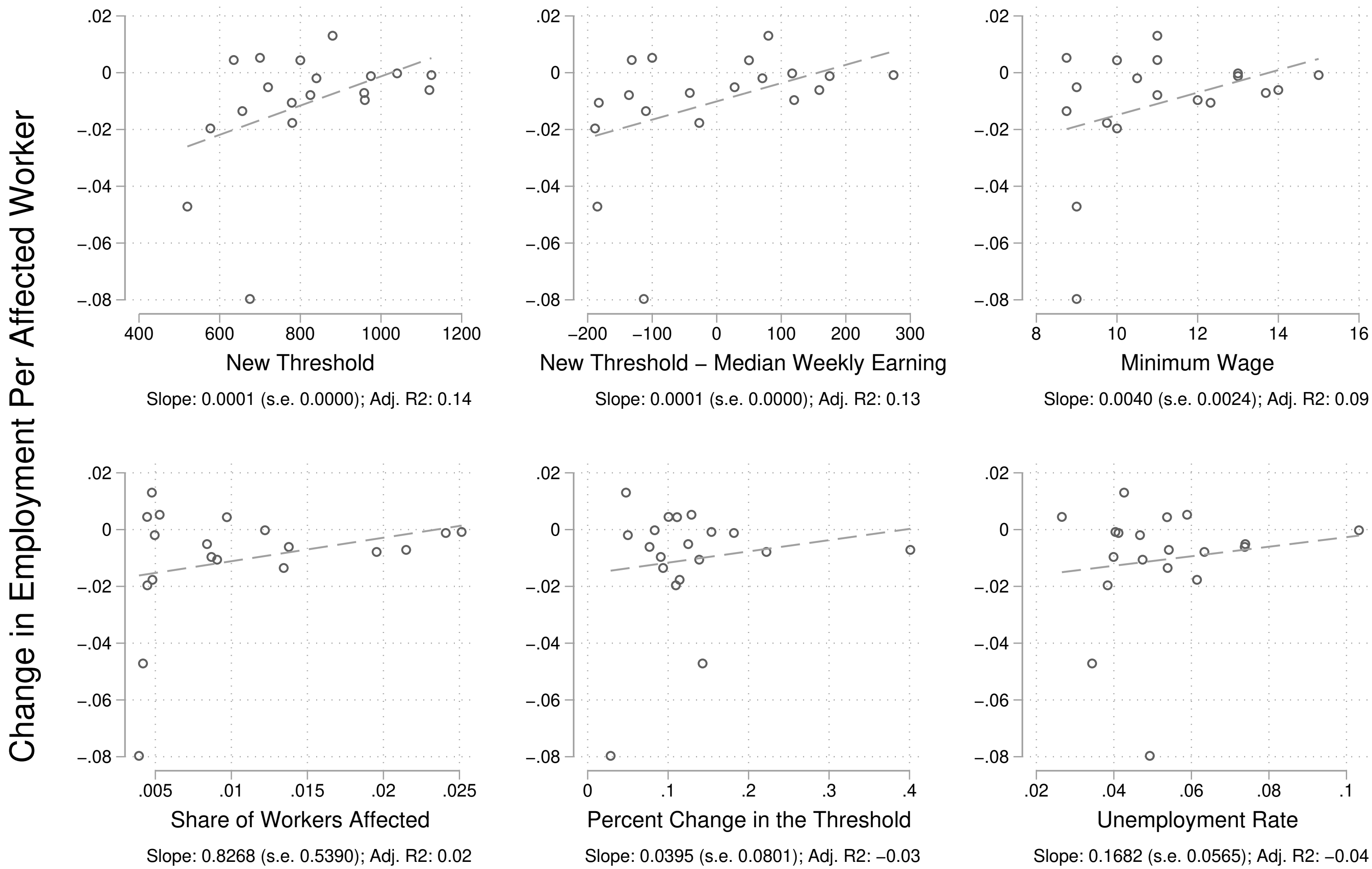
Appendix Figure J.6
Synthetic Control Estimates of Employment Effect

Notes. The figure shows the net employment effect of each event, estimated using both a regular and a synthetic difference-in-difference design. Standard errors for the regular design are clustered at the state level and the standard errors for the synthetic design are computed using 1,000 placebo regressions.



Appendix Figure J.7
Predicting the Share of Affected Workers Bunched

Notes. This figure reports the correlation between the share of affected workers bunched above the new overtime exemption threshold and various characteristics of the policy or state at the time of implementation. Hours and unemployment rate are calculated using the CPS. Each dot represents a state-year specific policy change.



Appendix Figure J.8
Predicting the Change in Employment

Notes. This figure reports the correlation between the estimate of the change in employment per affected worker and various characteristics of the policy or state at the time of implementation. Hours and unemployment rate are calculated using the CPS. Each dot represents a state-year specific policy change.

Appendix Table J.2
Predicting the Bunching and Employment Effects

	(1)	(2)	(3)
Panel A: Share Bunched			
New Threshold ($100)	0.046*		0.039
	(0.023)		(0.023)
Unemployment Rate		3.467**	1.631
		(0.896)	(1.242)
Panel B: Share Unemployed			
New Threshold ($100)	0.004		0.005
	(0.003)		(0.002)
Unemployment Rate		0.087	-0.066
		(0.055)	(0.086)
Weight by Inverse SE.	X	X	X
N	19	19	19

Notes. This table reports estimates from equation (26) where I regress the share of bunched workers (Panel A) or the change in employment per affected worker (Panel B) on the magnitude of the new threshold and the state unemployment rate. Observations are weighted by the inverse of the standard errors for each state-event. Standard errors are clustered by state.

Appendix K. Exploring Spillover Effects

This section examines the spillover effects of increasing the overtime exemption threshold onto jobs not directly targeted by the policy. In particular, the analysis in the main text found significant bunching in the weekly base pay of hourly workers right above the new threshold, even though their overtime eligibility is independent of the cutoff. In Appendix J, I find that this spillover effect onto hourly jobs is driven entirely by the response to policy changes in California. This section considers multiple hypotheses for why this spillover effect occurred.

First, the bunching of hourly workers is likely not due to policy differences between California and other states. Regardless of variation in state policies, hourly workers are nearly always covered for overtime under Federal law. The only exceptions are computer employees, outside sales employees, teachers, and "practitioners of law or medicine" (U.S. Department of Labor, 2019, 2024). Of these occupations, only computer employees need to meet a wage threshold to be exempt from overtime, whereas the others are always exempt. However, the threshold to exempt hourly computer employees far exceeds the usual one for salaried workers. For example, in 2021, when California's overtime exemption threshold was \$1,120 per week, the exemption threshold for hourly computer employees was \$47.48 per hour, equivalent to \$1,899.20 per week on a 40 hour schedule (California Department of Industrial Relations, 2026). Thus, there is no policy incentive for employers to bunch hourly workers at the overtime exemption threshold.

Second, I show that the bunching of hourly jobs is not due to the reclassification of salaried jobs. One possibility is that workers prefer being a salaried employee, so reclassifying them to hourly requires firms to pay a compensating differential. To test this hypothesis, I replicate my analysis on the distribution of employment, but I define workers' salaried/hourly status by their classification in the month before the policy change. Thus, any changes to the distribution is driven only by wage growth or employment flows. Even after removing reclassification effects, Figure K.1 finds that there is still clear bunching above the new overtime exemption threshold among hourly jobs. Note that unlike Figure 2, I find no net increase in hourly jobs suggesting that in the short run, the entire increase in hourly employment is driven by reclassification.

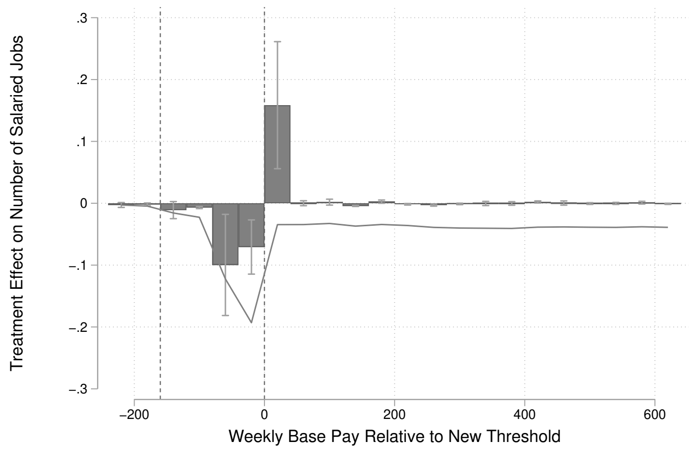
Third, I find evidence of broader spillover effects across the income distribution. Figure K.2 plots difference-in-difference estimates comparing workers in treated to control states, separately by their weekly base pay in the month before the policy change. Panel (a) shows that the largest increase in weekly base pay accrues to workers earning \$80 below the new overtime exemption threshold, consistent with the bunching effect. However, salaried workers earning well below the old threshold also experience around 0.5% increase in their salaries. In contrast, there are no positive spillover effects above the new overtime exemption threshold. Turning to hourly jobs, Panel (b) finds that the largest wage increase is driven by the minimum wage in the left tail of the income distribution. Nevertheless, there is also a clear increase in the wages of hourly workers earning $80 per week below the new threshold, similar to the effect for salaried workers.

Aside from spillovers in wages, I also find spillover effects in reclassifications. Panel (c) shows that jobs paid below the old threshold and even some jobs above the new one are less likely to be salaried after the policy change. Although jobs below the threshold were already covered for overtime, the reclassification makes it so that they would receive less pay if they worked fewer hours in a week. This would thus require more granular monitoring of workers' hours instead of say, fixing a strict 40-hour week and providing overtime for any additional hours. On the other hand, panel (d) finds precise null effects on the salaried/hourly classification of incumbent hourly workers. The spillover in reclassifications is suggestive that rather than reclassifying individuals, employers may wait to reclassify entire job titles once enough workers are covered for overtime.

The spillover effect along the pay distribution is reminiscent of studies of the minimum wage. That literature has proposed multiple theories for why the minimum wage can increase pay up the distribution such as 1) internal fairness concerns, 2) increased outside options, 3) reductions in other amenities leading to a reduction in labor supply, or 4) substitution for higher skilled workers (See Dube and Lindner (2024) for a review). Although empirical evidence on these mechanisms varies, in my setting, hypotheses (2)-(4) fail to explain the asymmetry of spillover effects among salaried workers and the preciseness of the spillover effects among hourly jobs. If the policy raised outside options or moved labor supply, it would have had a more uniform effect throughout the distribution. Moreover, my analysis focused on stayers, so the results also cannot be explained by substitution between workers of different skill levels. Rather, the asymmetry in Panel (a) of Figure K.2 is consistent with theories of inequality aversion where workers compare themselves to individuals who earn more than them (Card et al., 2012).

However, fairness concerns alone also do not fully explain the bunching among hourly jobs. In Table K.1, I estimate the distributional impacts of raising the overtime exemption threshold by firm characteristics. If hourly workers receive a pay increase for equity reasons, then there should be no spillover effects in firms with no salaried workers. In contrast, columns (1) and (2) find that although the share of hourly workers who get bunched is twice as large at firms with at least 1 treated salaried worker, it is nevertheless statistically significant at firms that have no salaried employees between the old and new thresholds.

On the other hand, columns (3) and (4) compare firms with and without any hourly workers. Employers with hourly workers may be more willing to cover salaried employees for overtime because they already have the infrastructure in place to track workers' hours. I instead find the opposite result. Firms with hourly workers are more likely to raise the earnings of both salaried and hourly employees. However, employers with no hourly employees also tend to be much smaller firms. Columns (5) and (6) separate firms into those with above and below median number of employees and similarly find that large firms are more likely to bunch workers' weekly base pay. It is also among larger firms that I find stronger spillover effects. Columns (7) and (8) find that the bunching of hourly jobs persists even if I drop the 1% largest firms in the data, suggesting that it is not simply driven by a few outlier firms disproportionately affecting the results. Overall, the heterogeneity analysis across the income distribution and across firms suggest that morale concerns may play some role in explaining the spillovers, but so does behavior specific to large firms.

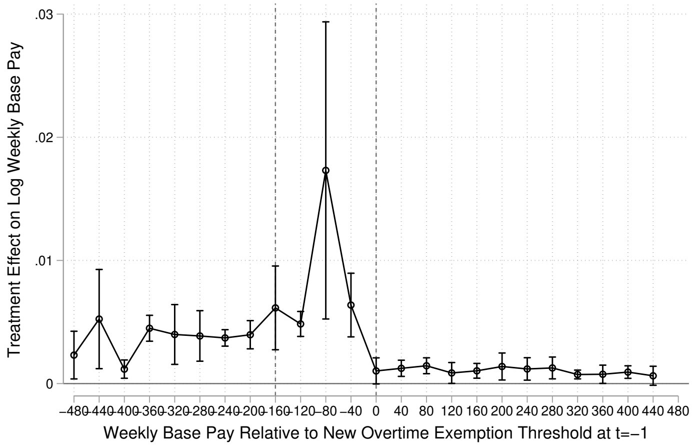


(a) Salaried

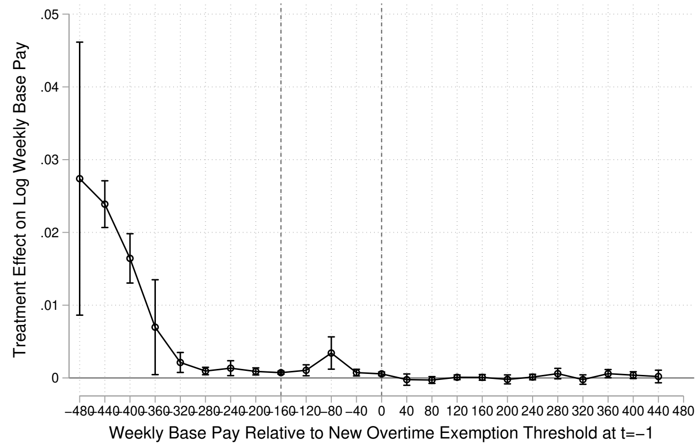
(b) Hourly

Appendix Figure K.1
Effect on Salaried and Hourly Employment, Fixing Salaried/Hourly Status at t=-1

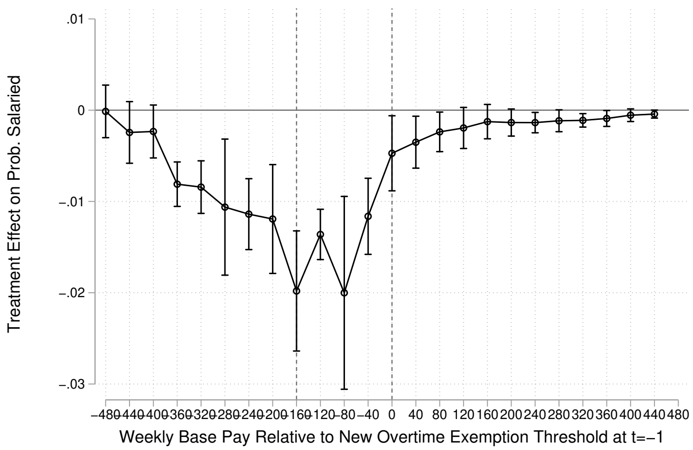
Notes. The figure plots estimates of equation (1) at the month of the policy change, and salaried/hourly classification is time-invariant and fixed at the month before the policy change. Standard errors are clustered by state.



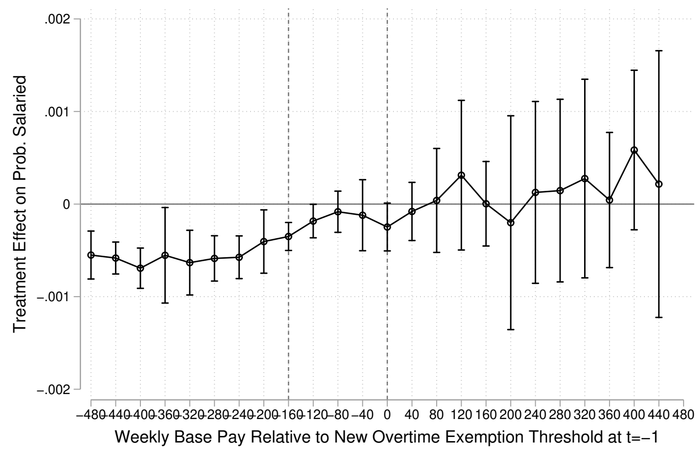
(a) Log Base Pay, Salaried



(b) Log Base Pay, Hourly



(c) Prob. Salaried, cond. Salaried at Baseline



(d) Prob. Salaried, cond. Hourly at Baseline

Appendix Figure K.2

Income and Reclassification Effects, by Workers' Baseline Characteristics

Notes. The figure plots estimates of equation (2) separately by workers' salaried/hourly status and weekly base pay in the month before an increase in the overtime exemption threshold. The dotted vertical line at 0 represents the new overtime exemption threshold, and the line at -160 represents the lowest overtime exemption threshold across the state policies, with the exception of Washington 2021. Standard errors are clustered by state.

Appendix Table K.1
Heterogeneous Effect of Raising the Overtime Exemption Threshold

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A								
Salaried Jobs								
Below Threshold	-0.109***	-0.001***	-0.221***	-0.042***	-0.218***	-0.047***	-0.201***	-0.260**
	(0.033)	(0.000)	(0.057)	(0.008)	(0.061)	(0.017)	(0.055)	(0.109)
Above Threshold	0.087***	-0.001***	0.170***	0.020**	0.165***	0.050*	0.144***	0.247***
	(0.031)	(0.000)	(0.053)	(0.010)	(0.055)	(0.026)	(0.053)	(0.091)
Hourly Jobs								
Below Threshold	-0.027**	-0.013***	-0.081***	0.070	-0.079**	0.106	-0.063***	-0.060
	(0.013)	(0.003)	(0.029)	(0.056)	(0.031)	(0.094)	(0.017)	(0.086)
Above Threshold	0.031*	0.018**	0.102**	0.015	0.100**	0.041	0.077**	0.217*
	(0.018)	(0.008)	(0.049)	(0.021)	(0.050)	(0.037)	(0.039)	(0.125)
Panel B								
Emp. w.r.t. Aff. Salaried	-0.017***	0.004	-0.029	0.063	-0.032*	0.150	-0.043*	0.144***
	(0.005)	(0.008)	(0.021)	(0.082)	(0.019)	(0.140)	(0.023)	(0.028)
Emp. w.r.t. All Affected	-0.001***	0.000	-0.006	0.063	-0.006	0.055	-0.010**	0.019***
	(0.000)	(0.000)	(0.004)	(0.082)	(0.004)	(0.051)	(0.005)	(0.004)
Treatment Group Heterogeneity	Treated > 0	No Treated	Hourly > 0	No Hourly	Big Firms	Small Firms	Size$\leq p99$	Size=p100
Number of Affected Salaried	4.850	0.000	1.414	0.248	2.081	0.096	0.933	5.125
Number of Affected Hourly	8.906	2.869	5.926	0.000	8.356	0.164	3.199	32.857
Event-Bin-Month FE	Y	Y	Y	Y	Y	Y	Y	Y
Event-Bin-State FE	Y	Y	Y	Y	Y	Y	Y	Y
Number of event-firms	9,823,904	55,609,520	40,490,960	24,942,464	31,357,400	34,076,024	61,142,892	4,290,532

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. 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.

Columns (1) and (2) reports estimates separately by whether or not the firm had any salaried workers between the old and new thresholds in the month before the policy change. Columns (3) and (4) separate the sample by whether the firm had any hourly workers at baseline. Columns (5) and (6) separate the sample by firms above and below median employment size. Columns (7) and (8) separate the sample by firms below and above the 99th percentile of the size distribution, respectively. Except for columns (1) and (2), estimates in rows (1)-(5) are scaled by the number of affected salaried jobs, defined as salaried employees with base pay between the old and new thresholds on the month before the rule change. Estimates in column (1) and (2) are scaled by the number of hourly workers in rows (1)-(5), and by the total number of workers in row (6). Standard errors are clustered by state. *10%,** 5%,*** 1% significance level.

Appendix L. Long-run Effects

The analysis in the main text presents results up to 6 months after a change in the overtime exemption threshold. This restriction was made so that when a state gradually increases its threshold over multiple years, the event windows for these policies do not overlap. Although the stacked difference-in-differences design avoids contamination across policies, it can only capture the short-run impact of expansions in overtime coverage. Real reductions in base pay through decreased nominal wage growth or renegotiations may take time. Moreover, firms' response to a policy change may differ as the overtime exemption threshold increases. In this section, I present evidence on the long-run effects of raising the overtime exemption threshold.

L.a Effect on New Hires

One way of predicting the long-run impacts of the policy is to examine the effect on flows rather than stocks. Although Section V.b showed that incumbent workers experienced pay increases, this positive earnings effect may not persist in the long-run if firms are able to hire new workers with lower base pays to offset the cost of overtime. The baseline turnover rate is about 2.7% per month on average for salaried workers in the treated interval. Thus, 5 months is sufficient time for firms to replace about 13.5% of the initial group of incumbent workers affected by the policy. Estimating the effects on new hires would therefore be informative about whether employers are adjusting behavior to reach a new equilibrium.

Appendix Figure L.1 reports estimates of equation (1) over time, with the outcome being the number of hires above and below the new overtime exemption threshold. The number of new hires below the new thresholds appears to have a downward pre-trend without a strong trend-break after the policy goes into effect. However, the number of hires above the new threshold exhibits a sudden increase in the month of the reform that persists for at least 5 months after the threshold increased. This indicates that firms continue to bunch new hires above the threshold. Thus, at least in the short-run, there is no evidence of wage cuts for new hires to offset the cost of overtime. However, a challenge with extrapolating the long-run effects from the stacked difference-in-difference estimates is that they do not distinguish between the first time a state increases its threshold and the latter subsequent threshold increases that may have compounding effects.

L.b Case Study of California

To directly measure the long-run labor market response to expansions in overtime coverage for salaried workers, I implement a case study of the policy changes in California from 2014 to 2017. California is a useful case study for three reasons. First, it has an 18-month gap between the reform in July 2014 and January 2016, whereas most other states only have a one year gap between successive policies. This gap gives me a relatively longer time period to study the response to a single threshold increase. Second, Appendix J showed that the difference-in-difference estimates for California have relatively flatter pre-trends compared to other states. Third, California's policies happened far enough in the past that I can study the cumulative impact of multiple threshold changes. Together, these features make California a credible setting to estimate both the medium-run effect of threshold changes that occur in isolation and the aggregate effects of gradual state reforms.

Firm-level Effects: To study the cumulative effects of the policies in California, I construct a balanced panel of firms from January 2014 to December 2017. Creating a balanced panel avoids compositional effects as the businesses using payroll processor's software change over time. I stop the panel in 2017 to avoid losing too many observations while still being able to capture three policy changes. Next, I follow the same difference-in-difference strategy as the main text comparing outcomes in California to all other states, excluding New York, Alaska, and Maine. The key distinction is that I do not treat the three policies in California as independent events. Rather, I use June 2014 as the baseline month and examine how outcomes evolve as each policy is added.

Figure L.2 plots the effect on the distribution of salaried jobs over time. Examining the panels chronologically, I find no significant changes to the distribution between January and June 2014, suggesting that the parallel pre-trend assumption is satisfied. After the first threshold increase in July 2014, I observe a large decrease in employment between the old and new thresholds, and a bunching mass right above it, as expected from the results in the main text. However, one and a half years later in December 2015, the missing mass has grown considerably, whereas the bunching mass has shrunk. With each subsequent increase to the overtime exemption threshold in January 2016 and 2017, the bunching mass moves further to the right whereas the missing mass continues to accumulate. By the end of the sample, there is a large missing mass between the original threshold in January 2014 and the binding threshold in December 2017. In contrast to the stacked difference-in-difference results, none of the missing mass in the long-run is accounted for by bunching above the cutoff.

Instead, I find evidence that employers are hiring more hourly jobs in lieu of salaried ones.

Appendix Figure L.3 plots the number of salaried and hourly jobs over time, separately by the segments of the income distribution affected by each policy. In panel (a), I show that employment was fairly stable in the months prior to the first policy change. Firms then shifted workers from the \$640-720 weekly base pay range to above the \$720 overtime exemption threshold in July 2014. The estimates remain fairly stable for the first six months after that, consistent with the stacked difference-in-difference estimates. However, an insight from the long-run analysis is that after six months, firms start decreasing the number of salaried jobs both above and below the \$720 threshold. By December 2015, the bunching mass all but disappears. This pattern of a sharp bunching mass after a policy change, followed by a gradual decrease in salaried employment repeats itself with each threshold increase. As a result, the three policies lead to a significant drop in salaried employment over the four years.

On the other hand, panel (b) finds a persistent increase in hourly employment. Similar to salaried jobs, I observe employers raising hourly workers' weekly base pay above the overtime exemption threshold with each policy reform. In contrast to salaried jobs though, the number of hourly jobs continue to grow over time throughout the intervals affected by the threshold changes. The estimates are consistent with employers bunching workers in the short run because it is difficult to monitor workers' hours, but investing in monitoring capital in the long run to reclassify jobs as hourly. For each reform, I find that the increase in hourly employment occurs both above and below the new threshold, suggesting that employers are not simply replacing bunched salaried workers with lower paid hourly workers. Rather, the increases in weekly base pay from bunching persists among hourly jobs.

Summing the effect on both salaried and hourly employment, Figure L.4 plots the net change in employment along the pay distribution as of the last month of my sample. Panel (a) shows that the largest change in the distribution is due to changes in the minimum wage. Aside from the state minimum wage, cities like San Francisco increased the minimum wage to \$14 per hour, which is equivalent to a weekly base pay of \$560. The highest minimum wage in California at the time was Emeryville at a rate of \$15.20 per hour (i.e. \$608 per week). To focus on the segment of the income distribution targeted by the overtime exemption thresholds, panel (b) drops the left tail of the distribution. I find that there is a net increase in jobs between the 2014 and 2017 thresholds, as well as a bunching mass right above the last threshold. Thus, the increase in hourly employment more than offsets the decrease in salaried jobs.

The replacement of salaried jobs with hourly jobs to the right of the last threshold suggests that investments in monitoring worker' hours may lead firms to reclassify entire occupations, rather than individual workers. However, a cautious interpretation is that there may be other confounding shocks in the right tail of the distribution. For instance, Figure L.3 finds that hourly employment in the $840-920 bin was rising two years before that range was targeted by any of the policy changes. Nevertheless, even if there was no rise in employment at the right of the distribution, the estimates still suggest that employment increased by 0.031 (s.e. 0.013) jobs per salaried and hourly worker between the overtime exemption thresholds in 2014 and 2017.

Given that there was no decrease in employment, it is reasonable to assume that the entire missing mass of salaried jobs is driven by either bunching above the threshold or reclassifications. Under that assumption, I calculate

$$\frac{\text{Missing Mass Minus Bunching Mass at End of Sample}}{\text{Number of Salaried Workers Between First and Last Thresholds at Baseline}}$$

as the share of jobs reclassified relative to baseline. In total, I find that 36% of affected salaried jobs in California were reclassified. This may seem large considering that the analysis in the main text found that only 2% of directly affected workers are reclassified on average after a single threshold increase. However, the long-run reclassification effect also accumulates the spillover effects onto lower paying jobs as shown in Appendix K, and changes in hiring patterns as firms replace salaried workers who leave with new hourly workers.

Worker-level Effects: Next, I test whether the increase in workers' earnings persist in the long run. On the one hand, firms could stop raising workers' salaries after bunching them above the threshold, letting inflation erode their salary over time. On the other hand, workers bunched above the threshold in one wave of the policy would be treated again in the next wave. The long-run wage effects thus depend on two factors: whether firms slow down nominal wage growth and how many times firms are willing to bunch the same worker as the threshold moves to the right.

Using the worker-level panel, I compute difference-in-differences estimates comparing incumbent workers in California to other states. I restrict the sample to workers with a base pay between $640 and $720 per week in June 2014, the salary range affected by the first policy change in California. Although I restrict the sample to a balanced panel of firms, I make no restriction on workers staying at their baseline firms over time.

Figure L.5 plots the difference-in-differences estimates over time for multiple outcomes. First, panel (a) shows that incumbent workers' salaries increased each time the state raised its overtime exemption threshold. California workers' pay increased by 4.5% over 4 years as the threshold increased from $640 to $840. Even during periods without a threshold increase, such as during the 1.5 year gap from July 2014 to January 2016, wages in California did not grow slower than in other states. The positive wage effects of raising the overtime exemption threshold are therefore not eroded by inflation in the long-run. However, the amount by which incomes increase diminishes with each threshold change, implying that a smaller share of the initial cohort of workers are being bunched after each reform.

Aside from receiving a raise, I find that workers who got bunched after one policy may also be reclassified after the second. Panel (b) shows that 4% of incumbents were reclassified after the first policy change in July 2014. However the probability that an incumbent is paid by salary drops again in January 2016.[31] Since reclassification after the second reform requires having survived the first as a salaried worker, this pattern implies that some workers who received a pay increase from the first reform were subsequently reclassified following the second.

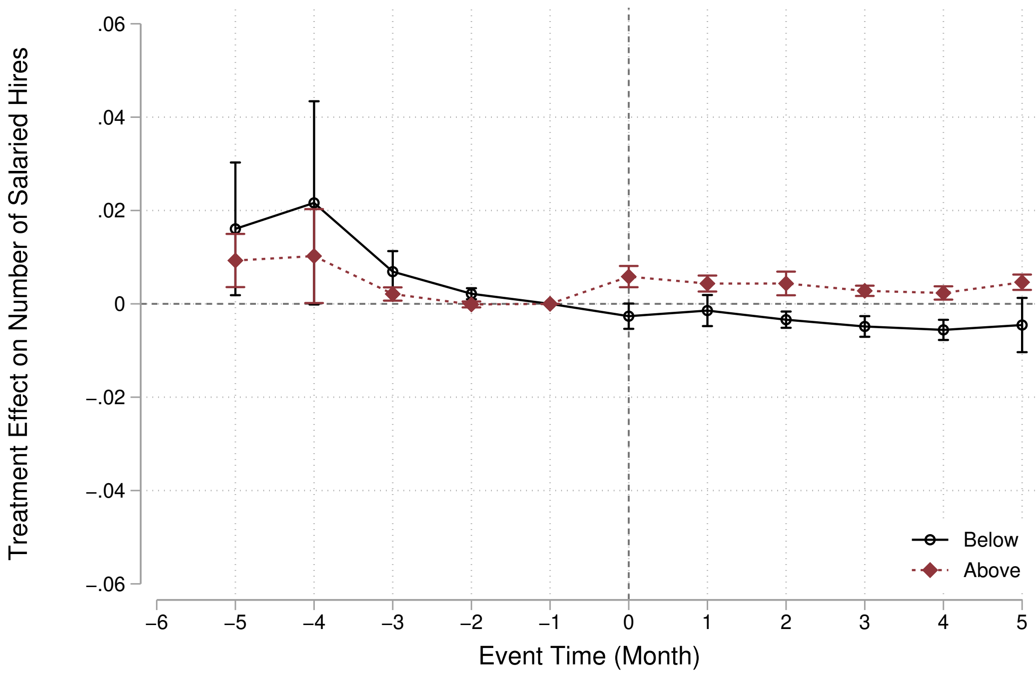
To provide more direct evidence, Panel (c) plots the probability that a worker is hourly, conditional on whether they earn a salary just above the threshold in the month of a policy change. For instance, the line labeled "Bunched 01/2016" refers to workers who earned up to $20 weekly base pay above the threshold in January 2016. Since the figure conditions on post-treatment outcomes, the time series should not be interpreted as causal. Nevertheless, the discontinuous jumps at the policy dates are informative. The control group exhibits a smooth, gradual shift in classification except for the Federal policy shock in December 2016. In comparison, about 5% of incumbents in California who were never bunched are reclassified in July 2014. Among those who were bunched that month, there is a sharp rise in hourly status after the next policy change. This sequence suggests that as each threshold increase renders bunching unprofitable, firms reclassify previously bunched workers rather than raising their salaries further.

An alternative option to bunching or reclassifying workers is for firms to simply let the worker go. However, Panel (d) finds no increase in separation rates during the months of the policy change. Incumbents in California exhibit more seasonality than other states, with separations rising over the summer and falling afterward each year. Beyond that, separations rise gradually but stabilize at 1.5% about a year after the first policy change. The response in separations therefore appears concentrated around the initial reform rather than recurring with each subsequent one.

In summary, the California case study yields four findings on the long-run effects of raising the overtime exemption threshold. First, the reclassification effect grows over time. The number of salaried jobs in the targeted income range declines gradually while hourly

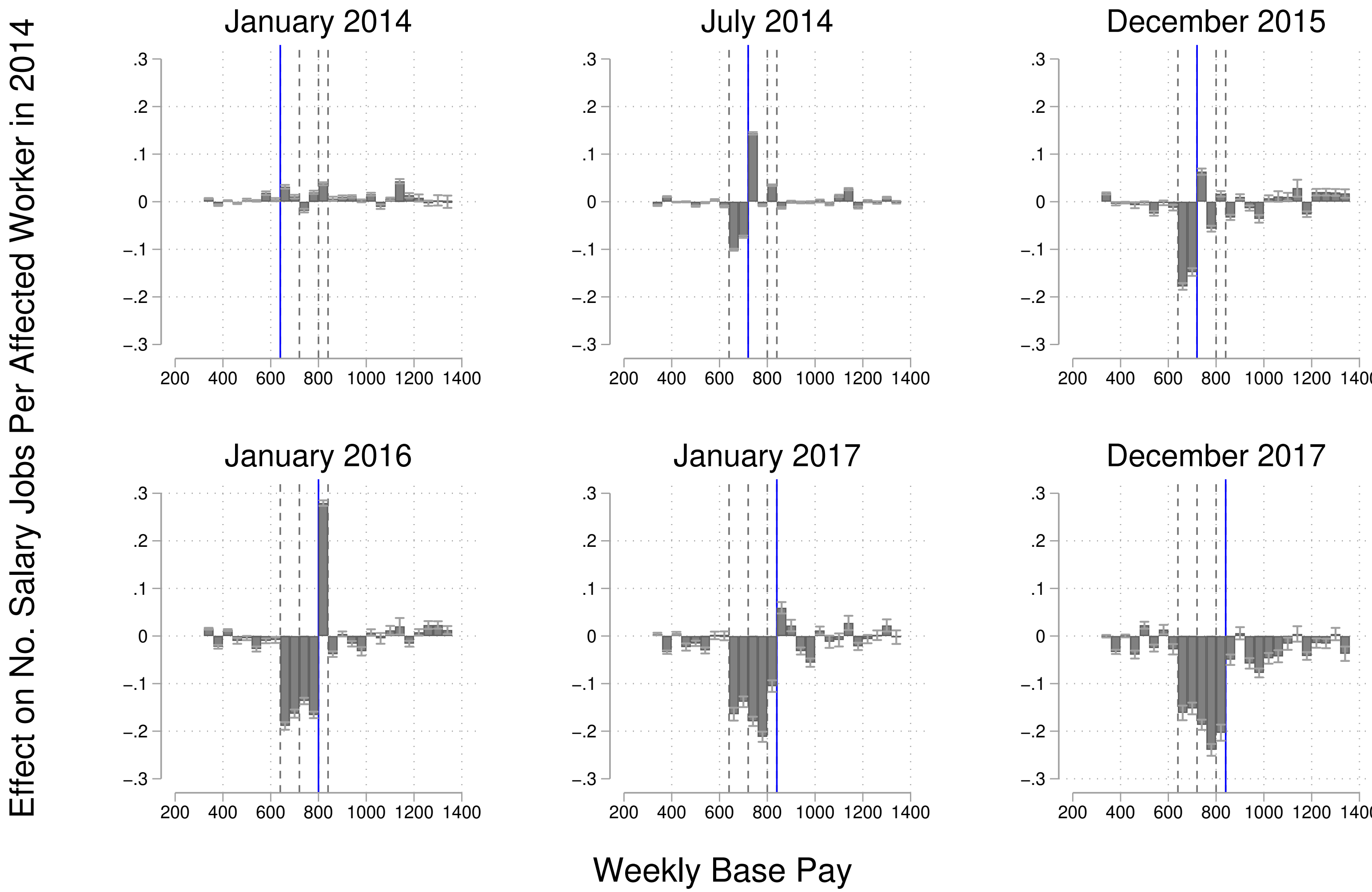
[31] The estimates for the 2017 policy are confounded by a large federal reform that was supposed to raise the national overtime exemption threshold to $913 per week in December 2016 (Quach, 2025). Although that policy was never binding, it nevertheless led to mass reclassification of workers, which had a disproportionately smaller effect in California since many of the incumbents were already reclassified as hourly from the state's earlier policies.

employment rises. Second, California experienced a net employment increase over the four years. Third, the positive wage effects are not eroded by slower nominal wage growth between reforms. Fourth, the within-worker response varies across successive policies: some workers are bunched repeatedly as the threshold rises, while others are reclassified after being bunched once. Separation rates, by contrast, respond primarily to the initial threshold increase and not to subsequent policy changes.



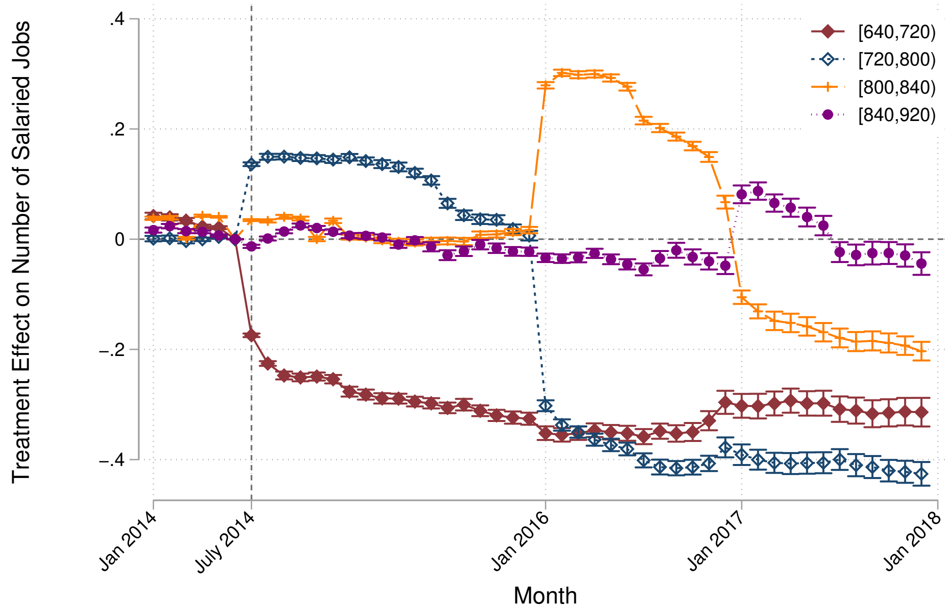
Appendix Figure L.1
Effect on the Number of Salaried Hires Over Time

Notes. This figure plots the effect of raising the overtime exemption threshold on the number of hires above and below the new overtime exemption threshold. All estimates are scaled by the average number of salaried workers between the old and new overtime exemption thresholds at baseline.

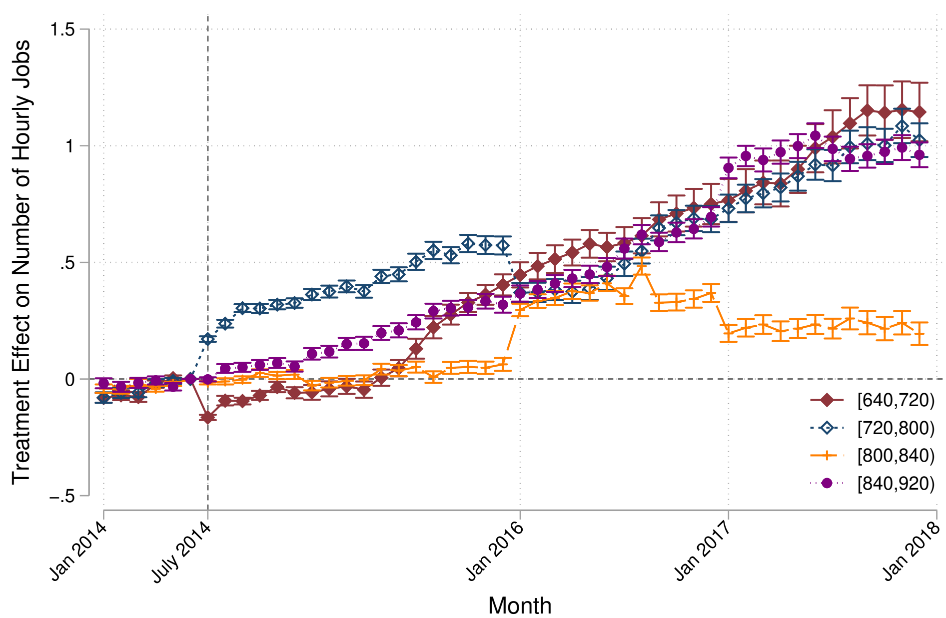


Appendix Figure L.2
Effect on Distribution of Salaried Jobs in California, Relative to June 2014

Notes. This figure plots difference-in-difference estimates, comparing the distribution of salaried jobs over time in California relative to other states from January 2014 to December 2017. All estimates are scaled by the number of salaried workers directly affected by the threshold increase in July 2014. The x-axis is partitioned into $40 bins of weekly base pay. The vertical dashed lines are at overtime exemption thresholds that ever go into effect during the four-year period. The solid blue line is the binding overtime exemption threshold for the month labeled.



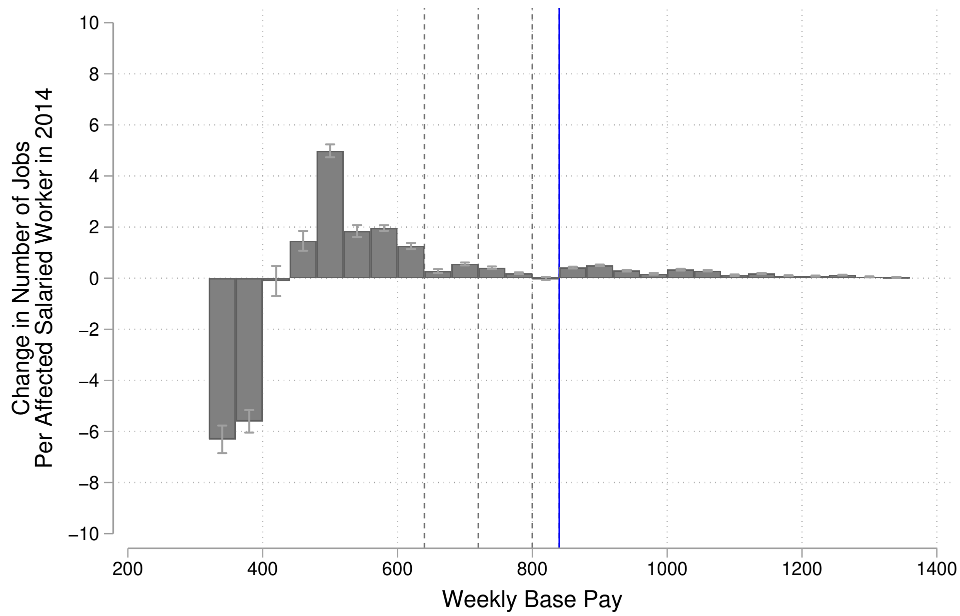
(a) Salaried Jobs



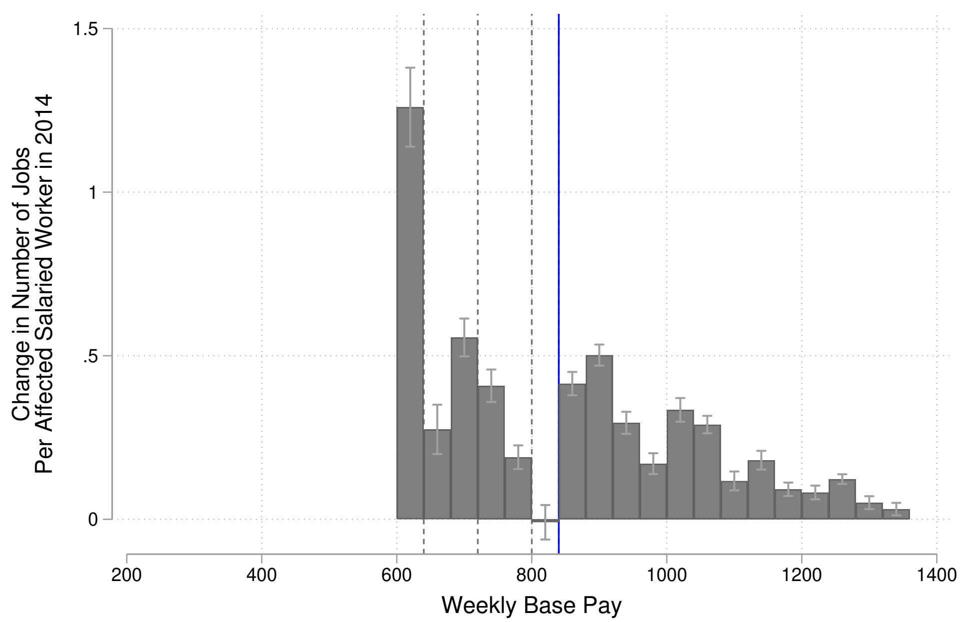
(b) Hourly Jobs

Appendix Figure L.3
Cumulative Effect on Salaried and Hourly Jobs in California

Notes. This figure plots the cumulative effect of raising the overtime exemption threshold on the number of salaried and hourly jobs over time along segments of the distribution of weekly base pay. All estimates are scaled by the number of salaried workers affected by the policy in July 2014.



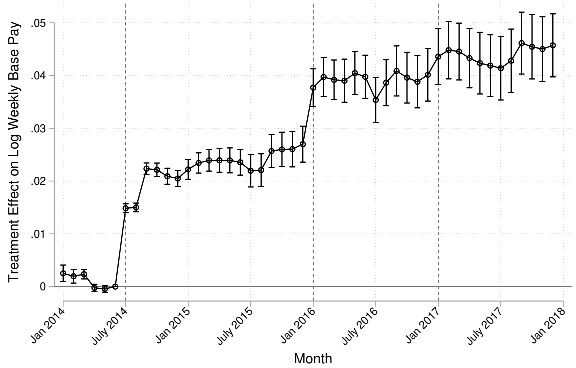
(a) Zoomed Out



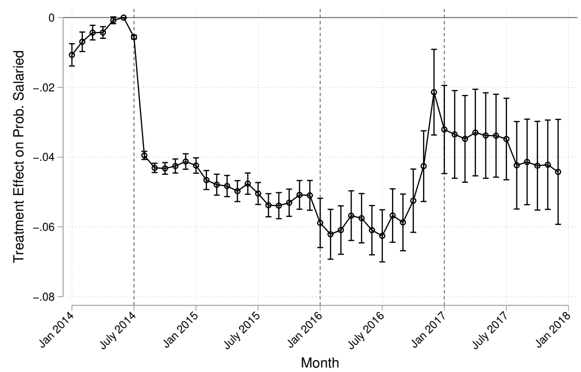
(b) Zoomed In

Appendix Figure L.4
Cumulative Effect on Distribution of Jobs in California as of December 2017

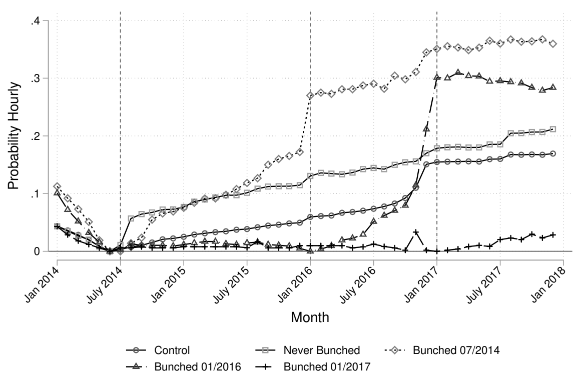
Notes. This figure plots difference-in-differences estimates comparing the distribution of all jobs (i.e. salaried + hourly) between California and other states from June 2014 to December 2017. All estimates are scaled by the number of salaried workers affected by the policy in July 2014.



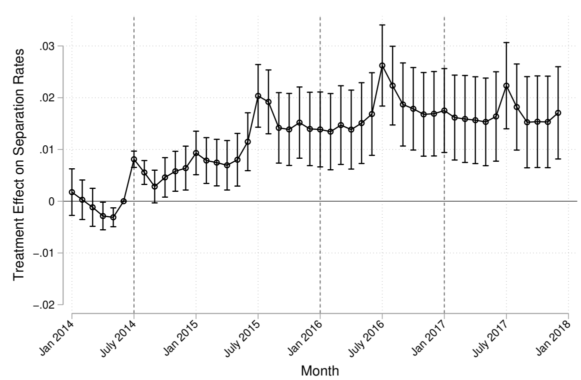
(a) Log Base Pay



(b) Salaried Status



(c) Share Hourly



(d) Separation

Appendix Figure L.5
Long-run Effect on Workers' Outcomes

Notes. This figure plots difference-in-differences estimates comparing salaried workers directly affected by an increase to the overtime exemption threshold to similarly paid workers in other states. Panels (a), (b), and (d) plots the base pay, salaried classification, and separation rates, respectively. Panel (c) plots the share of workers who are hourly, conditional on whether they were bunched after each threshold increase.

Appendix M. Potential Margins of Adjustment: Hours vs. Wages

In this section, I describe the characteristics of workers affected by increases in the overtime exemption thresholds and use these statistics to contextualize the possible responses to the expansions in overtime coverage.

Following the model predictions in Appendix C, the labor market response to expansions in overtime coverage depend on the baseline characteristics of workers. If workers seldom work more than 40 hours per week, then the expected cost of overtime is low and it would be cheaper for firms to cover these workers for overtime. On the other hand, if workers tend to work long hours, then employers may prefer to keep them exempt from overtime by raising their base salaries above the overtime exemption threshold.

To quantify the trade-offs faced by employers, I compare the cost of covering workers for overtime versus the cost of raising their salary above the new threshold. To start, I examine the hours distribution of treated workers in the Current Population Survey to measure the share of workers who report working over 40 hours per week prior to the policy change. For statistical power, I expand my sample period to include all 49 major state overtime threshold changes from January 2008 to 2026. I drop events where the threshold increased by less than $30.

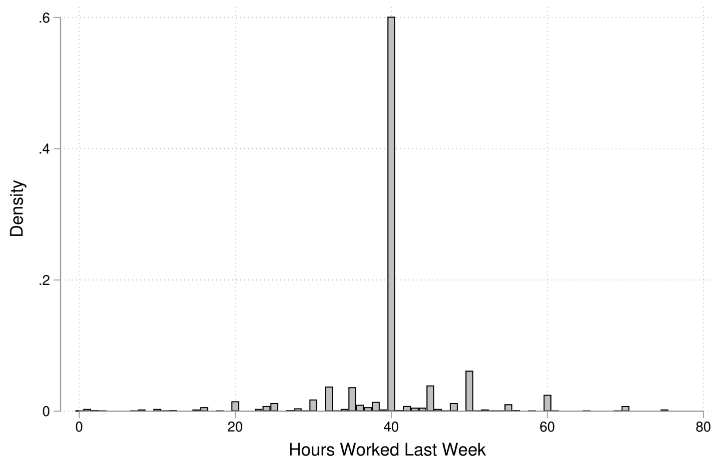
Figure M.1 plots the distribution of weekly hours that individuals report working in the week prior to the survey. I observe a large bunching mass of respondents working exactly 40 hours per week. In fact, 77% of workers report working no more than 40 hours per week. If monitoring costs are low for employees who do not work overtime (e.g. firms can set a fixed 9am-5pm schedule at little cost), then the prevalence of short hours suggest that it is fairly costless to cover the majority of workers for overtime. Consistent with this interpretation, my analysis of the payroll data found that only around 21% of salaried jobs between the old and new overtime exemption thresholds were in the missing mass (see Table I). The remainder stayed below the threshold and became covered for overtime.

Employers have three potential margins of adjustment for individuals who work over 40 hours per week, aside from laying off the worker. They can cut their hourly wage, cut their hours, or raise their salary above the overtime exemption threshold. I discussed the possibility of reducing salaries and the interaction with the minimum wage in Appendix G. In this section, I consider the latter two options. To understand the tradeoff between cutting hours and raising salaries, Figure M.2 plots average weekly base pay, relative to the new overtime exemption threshold, as a function of workers' hours. I find that individuals who work the longest hours also earn the closest to the new threshold, meaning they are the cheapest to bunch. However, few workers report working 80+ hours per week and there is no clear relationship between earnings and hours in the remainder of the hours distribution.

Even for more moderate weekly hours though, it may still make financial sense for firms to bunch workers. Conditional on working above 40 hours per week, the average respondent reported working 52 hours. If employers simply covered the average worker for overtime, the employee's payroll would increase by 45% (i.e. $\frac{s+1.5\frac{s}{40}(52-40)}{s} = 1.45$). In fact, the cost of each hour above 40 is equivalent to 3.75% of workers' weekly base pay. Figure M.3 simulates the cost of raising workers' salaries above the overtime exemption threshold or paying them overtime. For 98.6% of workers, it is cheaper to raise their salaries than to keep hours constant and simply pay the overtime premium.

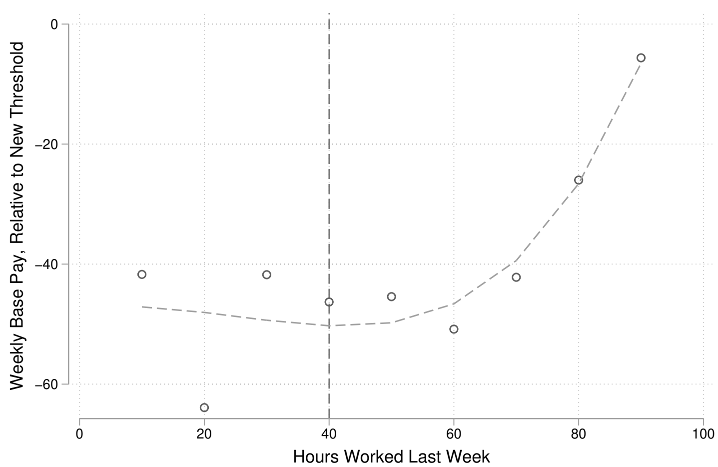
Next, consider whether it is more profitable to bunch weekly base pay at the new threshold while keeping hours constant or to bunch hours at exactly 40 while keeping salaries constant. As a benchmark, I compute the implied cost per hour of bunching workers: $\frac{\text{new threshold - weekly base pay}}{\text{weekly hours - 40}}$. Comparing this implied cost with each worker's implied hourly wage (i.e. $\frac{\text{weekly base pay}}{\text{weekly hours}}$), Figure M.4 finds that for the vast majority of individuals, the labor cost per hour from bunching workers' salary is far lower than the existing hourly wage. On average, the implied cost of bunching is only 35% of the current wage. Thus, there is a large discount to bunching workers relative to cutting their hours.

Taken together, the descriptive evidence in this section helps explain the distribution of responses in the main text. About 77% of affected salaried workers do not work over 40 hours a week, consistent with the share of workers I find that remain salaried and below the overtime exemption threshold. For the remainder, it is often more cost effective to raise salaries above the threshold than to pay the overtime premium or to cut hours, consistent with the large bunching effect I observe in the payroll data.



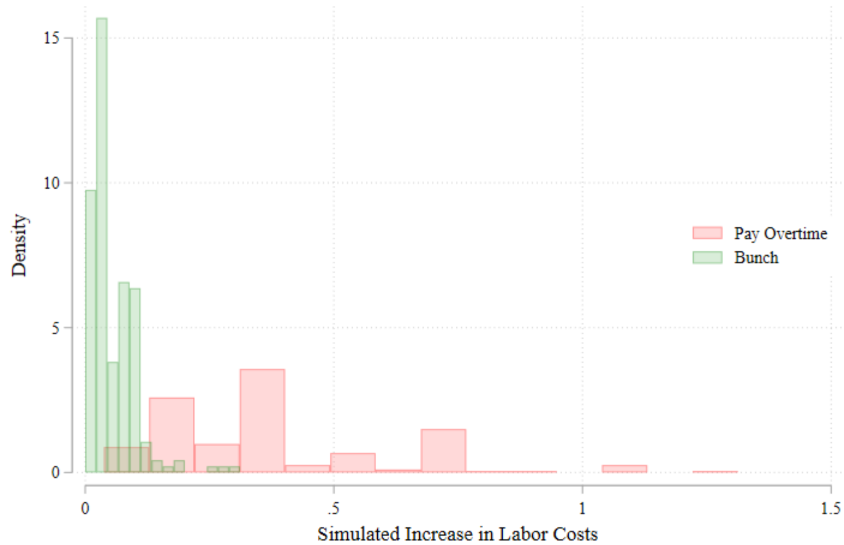
Appendix Figure M.1
Distribution of Weekly Hours

Notes. This figure plots the distribution of hours worked in the week before the survey among salaried workers in the CPS who reported weekly earnings between the old and new overtime exemption thresholds in the year before a policy change.



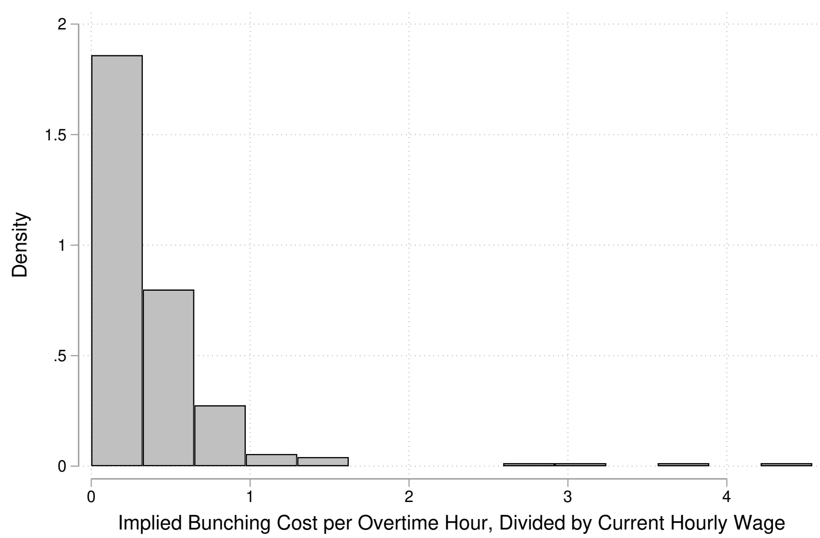
Appendix Figure M.2
Weekly Base Pay by Hours Worked

Notes. This figure plots the average weekly base pay of salaried workers in the CPS who earn between the old and new overtime exemption thresholds in the year before a policy change. The dashed vertical line is at 40 hours per week. The dashed line is a quadratic fit.



Appendix Figure M.3
Cost of Bunching vs. Paying Overtime

Notes. This figure plots the distribution of the percent change in workers' earnings if the worker either receives overtime for hours worked above 40 in a week, or gets a raise to the new overtime exemption threshold. The sample is restricted to salaried workers who report working over 40 hours last week.



Appendix Figure M.4
Distribution of Implied Bunching Cost per Hour of Overtime

Notes. This figure plots the distribution of the implied cost of bunching workers' salaries above the overtime exemption threshold, $\frac{\text{new threshold - weekly base pay}}{\text{weekly hours - 40}}$, divided by their current hourly wage. The sample is restricted to salaried workers who report working over 40 hours last week.

Appendix N. Policy Implications

N.a Elasticity

To benchmark the costs and benefits of raising the overtime exemption threshold relative to other labor market policies, Table N.1 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. Given that the policy has significant spillover effects onto hourly jobs, I include both types of workers in my calculations.

Column (1) suggests that for each percent increase in affected salaried workers' earnings, the number of affected jobs falls by 0.55% (s.e. 0.53%). The remaining columns of Table N.1 show that the point estimate of the elasticity ranges from -0.65 to 1.1 depending on the specification. 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 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 median 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 more negative elasticities than in the minimum wage literature, though my estimates are fairly noisy.

N.b Separations

A limitation of the employment elasticity with respect to workers' earnings is that the denominator conflates changes in hours with changes in the implied hourly wage. The CPS analysis in Appendix I suggests that hours did not sufficiently increase to fully offset the costs of the pay increase, but the estimates are fairly imprecise. Moreover, even a modest increase in hours could lead to a net decrease in worker welfare if they have convex work disutilities.

As an alternative measure of workers' valuation of their jobs, I examine the effect of the policy on separation rates. Unlike the worker-level analysis in the main text, I no longer restrict the sample to a balanced panel of workers since I am interested in the effect on worker exits. Moreover, I define treatment status using workers' salaries 6 months prior to an expansion in overtime coverage, rather than 1 month before. By using an earlier reference period, I no longer require workers to be employed exactly one month before the policy change, which allows me to test for parallel trends in separation rates leading up to the reform.

Figure N.1 plots the estimates from four difference-in-difference regressions. First, to show that the earlier reference period still captures the relevant group affected by the policy changes, Panel (a) shows that the weekly base pay of workers in the treated states increased at the exact month of the reform relative to workers in the control states. Note that since I am no longer restricting the sample to stayers, the figure shows that the estimates of the income effect in the main text are robust to allowing for composition changes. Second, panel (b) finds that despite an initial pre-trend, separations increased at precisely the month of the policy change. The outcome in this regression is an indicator variable for whether workers are still employed in the same firm that they started in 6 months before the reform. The estimates suggest that about 0.3% of incumbents left after the policy change, consistent with the firm-level estimates from Table II.

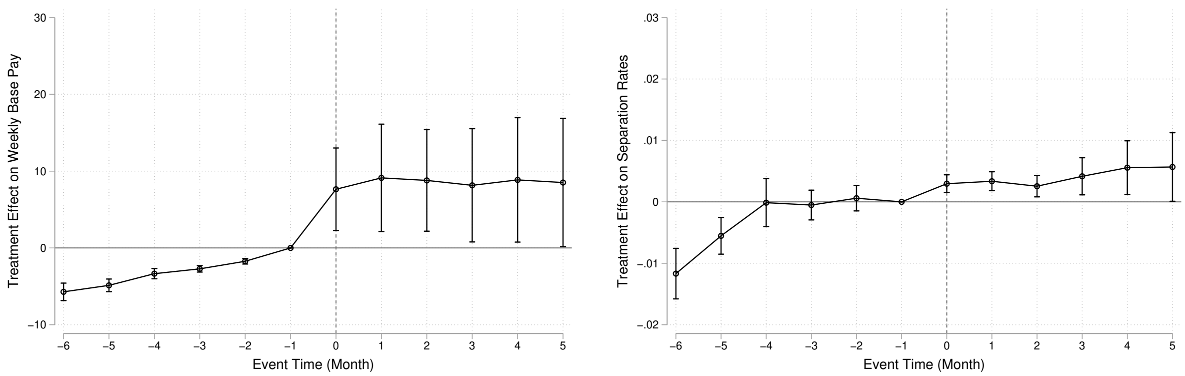
In general, it is unclear from the data why separation rates increased. On the one hand, the increase in separations could be demand-driven due to the rise in labor costs. On the other hand, it could be supply-driven if workers who received a raise are asked to work more hours, or workers are dissatisfied with being reclassified. In either case though, it is clear that at least some incumbents are worse off, either from involuntarily losing their job or choosing to leave because the job is no longer as desirable. These results stand in contrast to findings from the minimum wage literature, which shows that increases to the minimum wage decrease separations (Brochu and Green, 2013; Dube et al., 2016).

Appendix Table N.1
Ratio of Employment and Income Effects

	(1)	(2)	(3)	(4)	(5)	(6)
% Δ Emp. w.r.t. All Affected	-.005	-.004	.003	-.006	.003	-.005
	(.005)	(.003)	(.004)	(.005)	(.007)	(.005)
% Δ All Income	.009***	.006***	.004***	.009***	.003***	.009***
	(.001)	(.001)	(0)	(.001)	(.001)	(.001)
Elasticity w.r.t. Income	-.546	-.584	.721	-.653	1.143	-.561
	(.525)	(.37)	(.894)	(.583)	(2.651)	(.549)
Number of Affected Salaried	1.076	1.076	1.097	1.15	1.198	1.076
Number of Affected Hourly	4.208	4.208	4.268	4.233	4.378	4.208
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)	2,188,793	2,188,793	1,337,287	1,752,879	1,076,424	2,188,793
Number of event-workers (sal + hr)	10,525,474	10,525,474	7,306,454	7,217,860	4,976,799	10,525,474

Notes. Row (1) reports the change in employment scaled by the number of affected workers, defined as salaried and hourly workers earning between the old and new overtime exemption thresholds in the month before the policy change. Row (2) reports the average percent change in income of affected workers. Row (3) reports the ratio of the estimates in rows (1) and (2).

Column (1) reports the employment effect from column (1) of Table I, the average income effect from columns (1) and (2) of Table III, 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.



(a) Base Pay, Treated

(b) Separations, Treated

Appendix Figure N.1
Effect on Pay and Separations of Incumbents

Notes. This figure plots difference-in-difference estimates comparing salaried workers in states that raised their overtime exemption threshold to similar workers in control states. The sample is restricted to workers employed 6 months prior to the date of the policy earning between the old and new thresholds.