CHAPTER 3

FIRM BEHAVIOR TO DUAL MINIMUM WAGE POLICIES

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3.1 Introduction

In 2017, California enacted a state-level minimum wage policy that gradually raised the wage floor from \$10 to \$15 by 2023. The legislation created a tiered structure: firms with more than 25 employees were required to adopt the higher wage schedule sooner, while smaller firms followed a more gradual increase. Because smaller businesses often have fewer resources and less operational flexibility, they may struggle more than larger firms to absorb rising labor costs through productivity improvements or price adjustments (Chava et al., 2018). The policy phased in these increases over six years, culminating in 2023 when the dual wage structure was eliminated, and all firms became subject to a single minimum wage. This staggered implementation offers a unique opportunity to study firm behavior under a two-tier wage system and how smaller employers respond to wage pressure. This paper investigates how firms near the policy threshold adjust their labor inputs and production decisions. Specifically, we ask: "How do firms modify their labor input decisions in response to a dual minimum wage structure?"

The dual wage schedule creates incentives for firms to limit employment to remain below the threshold that triggers the higher wage. Unlike a uniform wage increase, this structure introduces both a kink and a notch: once a firm paying the minimum wage hires its 26^{th} worker, the higher wage applies retroactively to all current employees, causing a disproportionate increase in labor costs. As a result, firms face a sharp rise in payroll expenses upon crossing the 25-employee threshold. To avoid this, employers may limit their workforce size, potentially leading to a clustering of firms just below the cutoff. Additionally, the policy may provide a disincentive to increasing employment above the policy threshold to avoid paying higher wages. These employment dynamics are particularly relevant in California, where over 88% of firms employ fewer than 20 workers, making many businesses subject this policy feature (U.S. Census Bureau, 2019). The structure of the law may therefore influence hiring decisions and constrain firm growth.

To analyze these effects, we use the Longitudinal Business Database (LBD) from the U.S. Census Bureau, a restricted-use dataset that integrates survey and administrative records across all non-farm sectors with at least one employee. The LBD provides annual snapshots of employment by establishment, enabling us to track firms over time. We focus on the share of firms with employment levels just below the 26-employee cutoff and restrict our sample to single-establishment firms, which account for approximately 98% of observations. We segment results by industry, anticipating stronger effects in sectors with higher reliance on minimum-wage labor. We group firms by 2-digit NAICS codes according to their reliance on minimum-wage labor and estimate effects separately.

We implement a difference-in-differences (DiD) design using a two-way fixed effects model to estimate the policy's impact on the probability that a firm employs between 20 and 25 workers. Our main specification includes firm and year fixed effects, and the key variable is an indicator for whether the firm's employment falls just below the threshold. The treatment period runs from 2017 to 2019, while the pre-treatment period covers 2012 to 2016. We have 2020 and 2021, but exclude them to avoid measuring the effects of the COVID-19 pandemic. Our DiD approach relies on the parallel trends assumption: that in the absence of the policy, trends in firm size in California would have evolved similarly to those in other states. We assess this assumption using pre-policy event study plots and compare trends in California to trends in all other U.S. states. To further strengthen the design, we include zip-code-level demographic and economic controls—such as income and racial composition—interacted with year dummies to account for local, time-varying factors.

Because California is the only treated state, inference is challenging under standard assumptions. To address this, we perform a Fisher Randomization Test (FRT), re-estimating the treatment effect 51 times by assigning treatment to each U.S. state and Washington, D.C., one at a time. We compare California's estimate to the distribution of placebo effects. For statistical significance at the 95% level, California's effect must be among the top 2.5% of placebo estimates.

To leverage the longitudinal structure of the data, we begin by estimating event-study models that measure the dynamic effects of the policy on firm employment. This allows us to measure how firms' changes in employment are affected by the policy, particularly for firms near the policy threshold. In addition to employment, firms may adjust along other margins. In the second part of our analysis, we explore whether affected firms respond to the policy by reallocating payroll, either through hours adjustments or workforce composition changes. This broader set of outcomes captures a fuller range of firm responses and ensures our conclusions are not limited to solely employment changes.

To our knowledge, this is the first paper to analyze the effects of a firm-sizetriggered wage threshold on small businesses. Prior research has explored how small firms respond to uniform minimum wage hikes. For example, survey evidence shows small business owners are more likely to reduce employment in response to wage increases (Everett, 2014). Wursten and Reich (2023) found no employment effects for firms with fewer than 20 employees using a conventional state-by-state DiD framework. Chava et al. (2018) showed that national-level increases impose heavier burdens on small firms than large ones. Meanwhile, McPherson et al. (2022) studied California's SB3 law and found no overall effect on employment. Our study complements and extends these findings by focusing on a specific employment threshold and using variation within a single state.

This work also contributes to the literature on minimum wage effects using bunching methods. Cengiz et al. (2019) documented wage bunching at new minimums using a frequency distribution of worker wages across 138 wage hikes, finding no associated employment loss. When Azar et al. (2023) replicated these results using CPS data, they found greater wage elasticities in more concentrated labor markets (HHI ≤ 0.25). Our approach differs by analyzing bunching at the firm level—specifically in the distribution of firm employment counts. This enables us to estimate the number of "missing" or displaced workers via excess mass to the left of the threshold, providing a new perspective on how minimum wage policies can distort firm input decisions beyond wages and employment alone.

We find that firms in high and medium minimum-wage-use industries are more likely to remain just below the policy cutoff and less likely to be just above it—consistent with strategic avoidance of higher wage requirements. When zip-code-level covariates are added, the estimate for high-use industries being just below the threshold loses statistical significance, while the effects for avoiding the range just above the threshold become statistically significant in both high- and medium-use industries. Using the longitudinal structure of the data, we find that firms in California were more likely to expand their workforce prior to the policy's implementation, and less likely to do so afterward. These results are more consistent with firms just below the policy threshold restraining growth to avoid crossing it, rather than firms above the threshold downsizing in response to the policy.

Our FRT results show that California exhibits the largest positive treatment effect when covariates are excluded, indicating a strong response compared to placebo states. However, once controls are added, California's estimate shifts closer to the middle of the distribution. In both models, approximately 95% of placebo estimates cluster around zero, consistent with the null hypothesis and supporting the validity of our approach.

The remainder of this paper proceeds as follows: Section 2 provides background on the policy environment; Section 3 details the data; Section 4 outlines the empirical strategy; Section 5 presents the results; and Section 6 concludes.

3.2 Background

By the mid-2010s, raising the minimum wage had become a central issue in national economic policy, driven by concerns over income inequality and wage stagnation (Allegretto et al., 2011). The Fight for $15 movement, backed by labor unions and progressive activists, pushed for a $15 minimum wage, gaining traction in major cities and influencing state-level policies. By 2016, the issue had become a key point of debate in the presidential election. Prominent candidates supported raising the minimum wage, with some advocating for a nationwide $15 minimum (Dube, 2019; Fine et al., 2018). Meanwhile, Republican lawmakers and business groups cautioned that dramatic increases could lead to job losses, automation, and economic disruptions, particularly for small businesses. As a result, many state governments acted to bypass the political gridlock in Washington, D.C. California was among them, implementing a bill to gradually increase the minimum wage to $15 per hour.

3.2.1 Senate Bill No. 3

In 2016, California signed into law SB 3, which implemented steady minimum wage increases starting in 2017 until the state minimum wage reached $15 per hour in 2023. Figure 3.1 shows that California imposed a $10 per hour minimum wage on all firms, regardless of employment. The law was enacted in 2017, when firms with at least 26 employees experienced a 50-cent increase, while firms with fewer than 26 employees did not face an immediate change. From there, both groups experienced annual increases until the minimum wages converged in 2023. Figure 3.1 illustrates the evolution of the California state minimum wage over time. In 2016, all firms were subject to the same minimum wage. A gap persisted until 2023, when all firms were required to pay the same rate.

The law offers further clarification on how minimum wage requirements are applied. According to the State of California Department of Industrial Relations (DIR), an "employer" is defined as any person who directly or indirectly, or through an agent or any other person, employs or exercises control over the wages, hours, or working conditions of any worker (California Department of Industrial Relations, 2016b). This means that employer size—and therefore which wage schedule applies—is determined at the firm level rather than at the individual establishment level.

For example, a fast-food chain operating multiple locations, each with fewer than 26 employees, may still be required to pay the higher minimum wage if the total number of employees across all locations meets or exceeds 26. The situation becomes more complex in franchise relationships, such as those common in the fast-food industry. If each franchise location is legally independent—with separate ownership, payroll, and employment policies—it may be treated as a distinct employer. On the other hand, if franchise locations are centrally managed and operate under common control—especially in matters related to wages, hiring, and working conditions—the total number of employees may be aggregated to determine employer size. The DIR acknowledges that some cases are ambiguous, particularly when it is unclear whether multiple locations should be considered a single employer. In these instances, the DIR expects employers to make a reasonable and good-faith determination of their workforce size. Employers are encouraged to carefully assess their organizational structure and, if necessary, seek legal or regulatory guidance to ensure compliance.

Additionally, a business's workforce size may fluctuate within the year, which can affect minimum wage obligations. For example, if a firm's total workforce temporarily drops below 26 employees, it is unclear whether it would immediately qualify for the lower minimum wage or if it must maintain the smaller size for a defined period. Conversely, if a firm that previously had fewer than 26 employees expands and surpasses the threshold, it is likely required to raise wages accordingly. The DIR has not provided explicit guidance on short-term fluctuations, so businesses are expected to act in good faith and follow the highest applicable wage standard when in doubt.

3.2.2 Municipal Minimum Wages

Some municipalities in California have enacted local minimum wages that exceed the state-level standard. The UC Berkeley Labor Center provides an annual dataset of municipal minimum wage ordinances dating back to 2013, including additional conditions such as employment or revenue thresholds (UC Berkeley Labor Center, 2024). For example, the city of Los Angeles implemented a \$12.00 per hour minimum wage, surpassing the state-mandated rate at the time. Table 3.3 lists the 22 municipalities that adopted a higher minimum wage than the state standard in 2017 or 2018. In cities such as San Francisco, which implemented a \$13 per hour minimum wage, the local ordinance takes precedence over the state's dual wage structure, effectively nullifying its impact within those jurisdictions.

However, not all municipalities with higher minimum wages fully override the state's dual wage system. Certain cities, marked with a $*$

in Table 3.3, maintain the dual wage structure despite local laws. This means that in these municipalities, the distinction between firms with more than 25 employees and those with 25 or fewer remains relevant, potentially leading to different firm responses compared to cities where the local wage applies uniformly across all businesses. We discuss the implications of these differences and how we handle them in greater detail in the methodology section.

3.2.3 Small Firms

Small firms constitute a significant portion of the U.S. economy. According to the 2017 Census's Statistics of U.S. Businesses (U.S. Census Bureau, 2019), nearly 5.5 million firms employ fewer than 25 workers, with over 98% of these being single-establishment businesses—firms operating from a single physical location. Collectively, these small firms contribute just under $1 trillion in total payroll and employ approximately 24 million workers, accounting for about 15% of the nation's total payroll. Expanding the scope to include firms with fewer than 50 employees, these businesses employ 33 million workers and generate nearly $7 trillion in payroll, representing 26% of total U.S. employment.

Many of these firms operate in industries that rely heavily on minimum-wage labor, particularly Accommodation and Food Services (NAICS 72), as well as Professional, Scientific, and Technical Services (NAICS 54). California, home to a large share of small businesses, has approximately 690,000 firms with fewer than 25 employees, employing nearly 3 million workers. The state's small business landscape closely mirrors national trends, with firms under the 25-employee threshold accounting for roughly 20% of employment and 15% of California's total payroll (U.S. Census Bureau, 2019).

3.3 Data

3.3.1 Longitudinal Business Database

We use the Longitudinal Business Database (LBD) from the US Census to measure firm-level employment. This restricted-access microdata comprises annual surveys and administrative records. The data captures businesses across all non-farm industries with at least one employee. The Census implements multiple quality checks, such as comparing data points to similar programs, including County Business Patterns, Statistics of U.S. Businesses, and the U.S. Bureau of Labor Statistics' Business Employment Dynamics (BED) data (Jarmin and Miranda, 2002). The Census investigates any large discrepancies and fixes errors when discovered.

A primary goal of the database is to track year-to-year changes in employment (Chow et al., 2021). The LBD contains one measure of employment per year, taken during the pay period of March 12th. The choice to observe employment in March is because March is seen as a steady month of employment and is not subject to cyclical components of the economy (Jarmin and Miranda, 2002). Data are at the establishment level, and we aggregate to the firm level using a firm key. Our primary outcome is an indicator variable for whether the firm uses between 20 and 25 units of labor. The LBD contains industry codes for every firm. We expect firms that rely more on minimum-wage labor, such as those in hospitality, education, and medical services, to be more affected by this law. The NAICS codes also allow us to identify firms in industries that are unlikely to be affected by the minimum-wage policy (e.g., professional services, manufacturing, and transportation), as they employ minimal amounts of minimum-wage labor. Firms from these low-minimum-wage-usage industries are used in placebo regressions. If these placebo regressions yield precise zeros, this would provide stronger evidence that the effects observed in other industries are due to California's dual-minimum-wage policy.

We expect the policy to have the largest effect in industries with a high concentration of minimum wage workers. Among these, the Accommodation and Food Services sector (NAICS 72) stands out as the most reliant on minimum wage labor as 8.4% of paid employees make minimum wage or less (U.S. Bureau of Labor Statistics, 2021). This industry includes restaurants, bars, and catering services, where a substantial share of employees earn wages near the minimum. Industries with moderate exposure to minimum wage labor include Construction (NAICS 23), Educational Services (NAICS 61), and Health Care (NAICS 62), where a nontrivial portion of the workforce earns close to the minimum wage. By contrast, industries such as Manufacturing (31-33) and Professional, Scientific, and Technical Services (NAICS 54) employ relatively few minimum wage workers and are therefore expected to be minimally affected by the policy. We use these low-exposure industries as placebo groups in our empirical strategy to validate the identification assumptions.

3.3.2 Sampling

To disseminate results from this project, the analysis must comply with the Federal Statistical Research Data Center (FSRDC) Disclosure Avoidance Methods handbook.* In particular, future dissemination becomes increasingly difficult—and potentially prohibited—if implicit samples are created. In this context, an implicit sample refers to releasing results for a strict subset of firms that have already appeared in a previously disclosed sample.

To avoid this issue and ensure compliance, we constructed a narrower analytic sample. First, we restricted the data to a balanced panel, retaining only firms that appear in every year from 2012 through 2019. Second, we limited the sample to single-establishment firms. This restriction serves two purposes: (1) it prevents fu-

*This document is publicly available on the Census Bureau's website. The version referenced here was last accessed at `https://www.census.gov/content/dam/Census/programs-surveys/sipp/methodology/FSRDC-Disclosure-Avoidance-Methods-Handbook.pdf` on 5/21/2025.

ture creation of implicit subsets; and (2) it addresses ambiguity in how the minimum wage law applies to multi-establishment firms, which could lead to inconsistent compliance behavior. Importantly, among firms with fewer than 50 employees—the population most relevant to this study—the vast majority are single-establishment entities, making this restriction unlikely to meaningfully alter the generalizability of our results.

3.3.3 Zip-Code Characteristics

We include geographic controls to account for variation in firm characteristics at the ZIP code level. Specifically, we use five-year average data between 2012 to 2016 from the American Community Survey (ACS) to construct time-invariant ZIP code covariates. Because these characteristics do not vary over time, we interact them with year fixed effects to allow for differential trends across areas. We exclude any controls measured in the post-policy period to avoid endogeneity arising from policy-induced changes in local demographics. The set of controls includes the percent of the ZIP code population that is male, married, has some college education, holds a bachelor's degree, is below the federal poverty level, and is unemployed. We also include median household income, median housing value, and the Gini inequality index.

Table 3.4 presents the mean covariate values for ZIP codes in California compared to those in the rest of the United States. The values are generally similar across regions. For instance, the percentages of individuals who are unemployed, in the labor force, or living below the federal poverty level differ by no more than five percentage points. However, California shows a higher level of educational attainment, with 62% of residents holding a college degree compared to 53% in the rest of the country. Additionally, California exhibits significantly higher income levels and housing prices.

3.4 Empirical Strategy

3.4.1 Two-Way Fixed Effect Model

This exercise aims to estimate the extent to which employers are willing to downsize to avoid the higher minimum wage. To do so, we implement a difference-in-differences identification strategy to estimate excess bunching. The model is as follows:

$$y_{ist} = \delta_i + \tau_t + \beta D_{it} + \epsilon_{ist} \tag{3.1}$$

The outcome variable is denoted as y_{ist}. One example is an indicator variable equal to 1 if firm i has between 20 and 25 units of labor in state s and year t, though we consider multiple outcomes throughout the analysis. Firm and year fixed effects are represented by δ and τ, respectively. The firm fixed effects absorb variation in the outcome that is constant over time within a given firm, while the year fixed effects absorb variation that is constant across states within a given year. The parameter of interest is β, which captures the effect of the minimum wage policy on the probability that a firm operates at a specific level of employment. If the policy encourages firms to remain just below the threshold to reduce labor costs, we expect estimates of β to be positive when the outcome equals 1 for firms just below the threshold, and negative when the outcome equals 1 for firms just above it.

We establish specific criteria for firms to be included in our sample for the primary analysis. First, we restrict the analysis to firms with 17 to 34 employees, based on the assumption that a firm with 50 employees would not find it optimal to cut its workforce in half to qualify for a lower wage. Second, we conduct the analysis at the 2-digit NAICS level, recognizing that some industries—particularly accommodations—depend more heavily on low-wage labor than others. Third, we construct a balanced sample by including only firms that exist for the entire duration of the study, acknowledging evidence that minimum wage changes can influence firm entry and exit. Finally, we limit the sample to single-establishment firms, which account for approximately 98% of the dataset, to eliminate potential compliance uncertainties among multi-establishment employers. Firms that meet these criteria serve as the baseline sample, and we conduct robustness analyses to assess the impact of these restrictions on our results.

3.4.2 Timing

California is the only state that adopted the treatment policy in the data, so we do not have to consider the complexities of staggered rollout. Therefore, a standard two-way fixed effect model yields estimates with straightforward interpretations. Equation 3.1 captures the average treatment effect across treatment groups and years. The model implicitly assumes that the effect is homogeneous across treatment units and time. The assumption across treatment units is trivial given that there is only 1 treatment unit. Then, β captures the average effect for all years in the post period.

3.4.3 Parallel Trends

The next step in justifying the implementation of a DiD identification strategy is to provide evidence supporting the parallel trend assumption. The outcome does not need to be the same between treatment and control in levels, but in trends. In this case, the parallel trend assumption is that the change in the proportion of firms with employment just under the threshold is the same in California as in the rest of the country. To provide visual evidence of the parallel trend assumption, we present event study plots that come from

$$y_{ist} = \delta_i + \tau_t + \sum_{k \neq -1} \alpha_k D_{t=k} + X_i \gamma + \upsilon_{ist} \tag{3.2}$$

In the event study figures, we plot the set of α_t point estimates and 95% confidence intervals. For evidence that the β point estimate from Equation 3.1 captures a treatment effect, the figure should show that the difference is steady in the pre-period, then expands in the post-period. The event study is a way to visually see if the treatment and control means are moving in a similar direction pre-policy.

California may have different legislation or economic conditions, such that the parallel trend assumption does not hold. There may be reasons to think that the parallel trend assumption is more realistic once we control for state-level characteristics. We include the following Zip Code-level variables from the Census American Community Survey described in Subsection 3.3.3 . These variables are included in the model by interacting every covariate with the set of year dummies, allowing for the effect of the covariates to change over time. Data are 2016 values, so they are pre-treatment for the closest year before the policy went into effect. The minimum wage policy potentially affected future values of the covariates, so we do not include any post-treatment covariates in the regression. The positivity condition is satisfied in the context, meaning there are covariates for the control units with similar values as covariates for the treated units. This condition is important in using standard inference procedures (Khan and Tamer, 2010).

A necessary condition to interpret the results as the causal effect of the minimum wage law is there are no other shocks that happened in 2017 that confound the policy. For example, if California also passed a law that imposed other standards on firms around the threshold, such as healthcare coverage or paid time off, we would no longer be solely identifying the effect of the minimum wage policy, but the joint effect of all the policies. To the best of our knowledge, firms in California are not subjected to policies that would affect firms with 25 employees differently than firms with 26 employees. Therefore, we attribute any evidence in the event study and TWFE models to the minimum wage policy.

3.4.4 Clustering

The current specification analyzes at the firm level. However, labor markets are highly localized, meaning that nearby firms may experience similar economic conditions, demand fluctuations, or policy impacts Henning and Eriksson (2020); Storper (2018). To account for these localized effects, we cluster standard errors at the zip-code level, ensuring that inference is robust to spatial correlations in employment responses. This approach allows for correlations in unobserved terms among firms within the same zip code while assuming independence between firms in different zip codes. Firm fixed effects absorb state fixed effects, controlling for any state-level policies that may systematically affect firms within a state. Clustering at the state level would unnecessarily inflate the standard errors because, even within California, local labor markets—such as those in Sacramento and San Francisco—remain distinct despite state-level policies. State-level clustering would risk inflating standard errors, making zip-code level clustering the more appropriate choice.

3.4.5 Randomization Inference

Standard DiD methods assume a large number of treatment and control clusters for the central limit theorem to apply (Roth et al., 2022). With only one treated cluster, our estimator may be poorly approximated by the central limit theorem because the average of the shocks will generally not be approximately normally distributed (Roth and Sant'Anna, 2023). To account for the small number of treatment clusters, we run a Fisher Randomization Test (FRTs) to validate the DiD estimates. We measure employment in all 50 states plus Washington, D.C, making 51 total groups. We run the DiD estimator 51 times, each time assigning treatment to a different group, to obtain a distribution of 51 treatment effects and corresponding standard errors to compute t statistics. We use this distribution of t statistics for inference. To reject the null hypothesis that $t = 0$ at a 95% confidence level using a two-tail test, California's point estimate needs to be in the top 2.5% of estimates. Given there are 51 estimates, California needs the largest t to reject the null. However, if we conduct a one-tail test at the 90% confidence level, $\beta_{California}$ would have to be one of the 5 largest. Given no other state has a policy that treats firms around the threshold differently, we expect the other treatment effects to be statistically indistinguishable from zero. The treatment effects from other states being precise nulls provides evidence that any large effects in $t^{California}$ is actually measuring the effect of the policy.

3.5 Results

3.5.1 Parallel Trends

The validity of the estimates presented in Table 3.5 rests on the assumption that firms employing between 20 and 25 workers in California followed similar pre-trends to those in other states. To assess this, we estimate Equation 3.2, which captures year-by-year differences in employment levels. We report only the signs and significance levels of each coefficient, α. Estimates are normalized to 2016—the year before the policy takes effect—so under the parallel trends assumption, all pre-2017 coefficients should be statistically indistinguishable from zero.

Table 3.5 presents the year-treatment interaction terms from Equation 3.2. Panel A reports results without covariates; Panel B includes covariates. The sample sizes reflect the number of firms observed in each year of the data. The sample is fully balanced, so the same firms appear in every year.

The results for high-usage firms are consistent across models estimated with and without covariates. The point estimates are mostly insignificant in the pre-period, except for a moderately significant estimate in 2013 when estimated without covariates. This suggests that the differences between treatment and control groups in the pre-period are statistically indistinguishable from zero, supporting the parallel trends assumption.

For medium-usage firms, point estimates are consistently positive across both specifications, which aligns with a bunching interpretation, suggesting that some firms may have strategically increased employment to just above the 20-worker threshold in anticipation of or in response to the policy. This behavior would be consistent with firms adjusting to maintain eligibility or compliance under the new regulatory environment. However, the lack of statistical significance across all periods weakens the evidentiary basis for this claim. While the direction of the estimates suggests a potential behavioral response, the imprecision of the estimates means we cannot distinguish this pattern from random variation.

Low-usage firms exhibit a different pattern. In the pre-period, point estimates are consistently negative, with one significant estimate in 2012. This raises concerns about whether the parallel trends assumption holds for this group. Accordingly, we place less interpretive weight on the low-use group and focus on the high- and medium-use firms, where the identifying assumptions appear more credible.

Table 3.5 presents the year-by-treatment interaction terms from Equation 3.2, where the outcome is an indicator variable equal to 1 if a firm has between 20 and 25 employees. This exercise is designed to detect "missing mass"—that is, firms that might otherwise fall in this part of the employment distribution but are affected by the policy.

For high-use industries, the event study estimates are qualitatively similar regardless of whether covariates are included. The coefficients during the pre-period are initially negative but turn positive just before the policy is implemented; however, none of these pre-period estimates are statistically significant. In the post-period, all coefficients have the expected positive sign, suggesting firms are more likely to be operating just below the threshold, though only one year (2018) is statistically significant when covariates are excluded, and one year is marginally significant when covariates are included.

The pattern for medium-use industries is similar. Pre-period estimates are generally positive and not statistically significant, except for one marginally significant coefficient when covariates are included. Post-policy estimates are uniformly positive, with significance increasing in later years—2018 and especially 2019—when covariates are excluded.

For low-use industries, post-period coefficients are a mix of positive and negative signs. However, several of these are statistically significant, particularly in 2018 and 2019, suggesting some degree of behavioral response even among firms less exposed to the policy.

3.5.2 Main Estimates

Table 3.7 presents the results from Equation 3.1, estimated separately for each group of firms. Each row corresponds to a different industry-use category (high, medium, low), and each estimate comes from a separate regression. Column (1) reports the total sample size for each specification (number of firms multiplied by the number of years). Column (2) shows estimates for "bunching"—firms with 20–25 employees (left of the policy threshold)—while Column (3) captures the "missing mass"—firms with 26–30 employees (just above the threshold). Panel A reports estimates without covariates; Panel B includes covariates.

Our findings on missing mass align with the results on bunching. In Panel A, high- and medium-use industries have positive coefficients in Column (2), suggesting that firms in these industries are more likely to operate just below the 26-employee threshold compared to similar firms in other states. Only the estimate for high-use firms is statistically significant, allowing us to reject the null hypothesis of no treatment effect. In Column (3), estimates for missing mass are negative, consistent with the idea that firms are either avoiding growing beyond the threshold or decreasing their employment to below the policy threshold. However, none of these estimates are statistically significant without covariates.

Including covariates (Panel B) does not change the direction of the estimates, but it substantially alters their statistical significance. The previously significant bunching effect for high-use firms loses significance, while the missing mass estimates for high- and medium-use firms become statistically significant. This shift raises concerns about the robustness of the estimates to model specification.

The changes in significance when covariates are added could result from two possible mechanisms: (1) the point estimates attenuate toward zero, and/or (2) the standard errors increase, reducing statistical power. Since statistical significance is jointly determined by both the magnitude of the estimate and its standard error, these patterns warrant further scrutiny.

Including covariates is standard practice in difference-in-differences settings. However, in our case, the use of zip-code-level controls may introduce problems. Labor markets can be highly localized, and zip codes often contain over 100,000 people and thousands of firms (World Population Review, 2024). As a result, the zip-code-level variables may be too coarse. The zip code was the most granular level of variation available to us.

3.5.3 Randomization Inference

As discussed in Section 3.4.3, our difference-in-differences (DiD) estimates are sensitive to outcomes in California, which is the only treated unit. To assess the robustness of these estimates, we conduct an event study and implement randomization inference using Equation 3.2. This allows us to test placebo regressions and evaluate whether our findings are likely driven by random variation.

In our study, California is the sole treated unit, while the other 49 states and

Washington, D.C., serve as controls. Because the treatment is concentrated in a single unit, the estimates are highly sensitive to trends specific to California. To address this concern, we apply a randomization inference procedure: we re-estimate Equation 3.1 50 times, each time assigning the treatment to one control unit while holding the rest as controls. This placebo design generates a distribution of treatment effects under the null hypothesis of no policy impact, helping us evaluate whether California's estimate is unusually large we focus this analysis on accommodation firms due to the accommodation industry's large use of minimum wage labor.

The specification used for randomization inference is shown in Equation 3.3:

$$y_{ist} = \delta_i^j + \tau_t^j + \beta^j D_{it}^j + \epsilon_{ist}^j \tag{3.3}$$

Equation 3.3 mirrors our main DiD model, with superscript j indicating the unit assigned to treatment. To compare California's result with the distribution of placebo estimates, we calculate studentized treatment effects using Equation 3.4:

$$\hat{t}^j = \frac{\hat{\beta}^j}{\widehat{SE}^j} \sim t_{50} \tag{3.4}$$

$\widehat{SE}^j$ is the standard error the corresponding $\hat{\beta}^j$. As in Equation 3.1, the standard error is hetroskedistic robust and clustered at the Zip Code level. Equation 3.4 yields a standardized measure of how far each estimate is from zero in units of its standard error. If the policy truly only affects California, we expect placebo estimates to be centered around zero.

Table 3.8 reports the t-statistics from this exercise. Column (1) shows results without covariates; Column (2) includes zip code–level controls. California's estimate without controls is highly significant and matches the findings in Table 3.7. With controls, the estimate remains positive and large but is no longer statistically significant at conventional levels.

Table 3.9 summarizes the distribution of placebo estimates by sign and significance. In Column (1), which excludes controls, California is the only unit with a highly significant positive estimate, placing it in the top 1.96% of placebo results. This allows us to reject the null hypothesis of no effect at the 5% level. Among the remaining 50 regressions, 94% are either statistically insignificant or only moderately significant—close to the 95% expected under the null. The signs of the estimates are evenly split: 47% are positive and 53% negative.

Column (2) shows results with covariates included. The distribution remains largely similar, with five of eight sign/significance bins unchanged. California's estimate is still positive but no longer statistically distinguishable from zero. Due to data constraints, we cannot determine whether this change is due to a shift in the point estimate or an increase in the standard error. However, we note that California's treatment effect, conditional on zip code characteristics, remains larger than at least half of the placebo estimates. At this level, the effect is not statistically distinguishable from zero at any conventional confidence level. Only three units—Washington, D.C., Washington State, and Idaho—have positive estimates that are significant or highly significant. This is under 6% of all estimates, which aligns with expectations under a true null that $\beta = 0$. In future versions of the project, we will release the full numeric results to allow for a more detailed discussion of the California estimate.

3.5.4 Mechanisms

We find that the dual minimum wage incentivizes high-minimum-wage firms in California to bunch below the employment threshold. While we find suggestive evidence of a missing mass of high-minimum-wage firms above the bunching threshold, our results were not statistically significant. To explain our results, we explore several employment dynamics of high-minimum-wage firms in Table 3.10. That is, we explore the ways firms may be manipulating their employment due to the policy.

We construct four variables to explore firm employment decisions:

1. *Under Threshold:* A dummy variable equal to 1 if a firm utilizes labor above the cutoff in year t-1, but then utilizes labor below the cutoff in year t
2. *Over Threshold:* A dummy variable equal to 1 if a firm utilizes labor below the cutoff in year t-1, but then utilizes labor above the cutoff in year t
3. *Difference:* The difference in employment in years t-1 and t
4. *Increase:* A dummy variable equal to 1 if a firm increases their employment from years t-1 to t

We then estimate our event-study specification, replacing the firm-employment variable with the constructed variable of interest, for firms in high-minimum-wage industries utilizing between 13-38 units in the pre-policy year.

We find no evidence that bunching is induced by a firm reducing employment from above to below the threshold, as the post-policy *Under Threshold* coefficients are negative and insignificant. Rather, bunching seems to be induced by stymied firm growth. Firm employment growth (*Difference variable*) sharply falls in the year before the policy, which may be an anticipatory response to increased costs. While employment continues to grow for California firms, it does not return to pre-policy rates. The *Increase* results convey the same story. These results suggest that bunching comes from firms that were already below the threshold choosing to not expand employment, rather than firms already above the threshold reducing employment, and are consistent with our main results.

As further evidence that bunching is induced by stymied employment growth we study firm payroll changes. Specifically, we study how firms utilizing between 26 to 30 units of labor in 2016 change their payroll or payroll per employee using our event-study specification (with payroll outcomes) in Table 3.11. We find no evidence that treated firms change payroll per employee, consistent with the story that firms right above the employment threshold are not responding to the policy. We do find evidence of total payroll increases for Californian firms, but this result is likely due to minimum wage increases in general. Ideally we would also present similar results for firms utilizing 21-25 units of labor in 2016, but these estimates have not been cleared for release by the RDC at the moment.

3.6 Conclusion

This paper provides new empirical evidence on how small firms respond to a dual minimum wage structure that introduces a sharp employment threshold. Our findings suggest that California's minimum wage policy, which imposes higher wage requirements on firms with 26 or more employees, creates incentives for firms to bunch just below this threshold. Using administrative microdata and a difference-in-differences approach, we detect distortions in firm behavior that do not arise under traditional minimum wage frameworks. Notably, we find that the law primarily affects the accommodation industry—a sector with a high proportion of minimum-wage workers. The effects appear to become stronger over time, which may be from it taking time for firms to respond or thing annually increasing minimum wage driving more firms to change labor.

This study contributes to the broader literature on labor demand elasticity and firm responses to wage regulation. While most prior research has examined the employment effects of uniform minimum wage increases, we show that discrete policy thresholds based on firm size can induce firm-level behavioral responses. The observed bunching just below the 26-employee cutoff suggests that firms may be substituting away from labor to avoid increased costs.

Future dissemination of this work will include numerical results to complement the current analysis. While signs and statistical significance offer a basic summary, they limit our ability to describe the underlying dynamics. For example, although the event study results show that pre-treatment trends are largely insignificant, we cannot fully rule out the presence of noisy, non-zero patterns. Including point estimates will allow us to discuss magnitudes and provide a more rigorous assessment of the timing and strength of treatment effects. Additionally, we will be able to quantify how the inclusion of covariates shifts results, clarifying whether significance changes are driven by altered point estimates or standard errors.A commonly desired parameter in minimum wage research is the wage elasticity of labor supply. While the policy-induced wage change is mechanically known, we require numerical results to measure the aggregate labor market response and construct this elasticity estimate.

Future extensions of this paper will examine the policy's impact on firm entry and exit. Prior work has shown that minimum wage increases can affect business survival and market dynamics (Aaronson et al., 2013; Luca and Luca, 2019). In our current setup, we focused on the intensive margin by restricting the sample to firms that existed throughout the entire sample period. Expanding the analysis to include firm births and deaths will allow us to explore the policy's effects on firm creation, growth trajectories, and shutdowns.

This paper highlights the consequences of a unique minimum wage policy design and shows that firm behavior can be shaped not only by the level of the wage floor but also by how that policy is structured across firm level employment.As the minimum wage debate continues, policymakers must balance the goal of raising wages for low-income workers with the potential for unintended distortions in the labor market caused by sharp policy thresholds. Ongoing extensions of this will further quantify the magnitude of these effects and examine their broader implications for firm dynamics and aggregate employment.

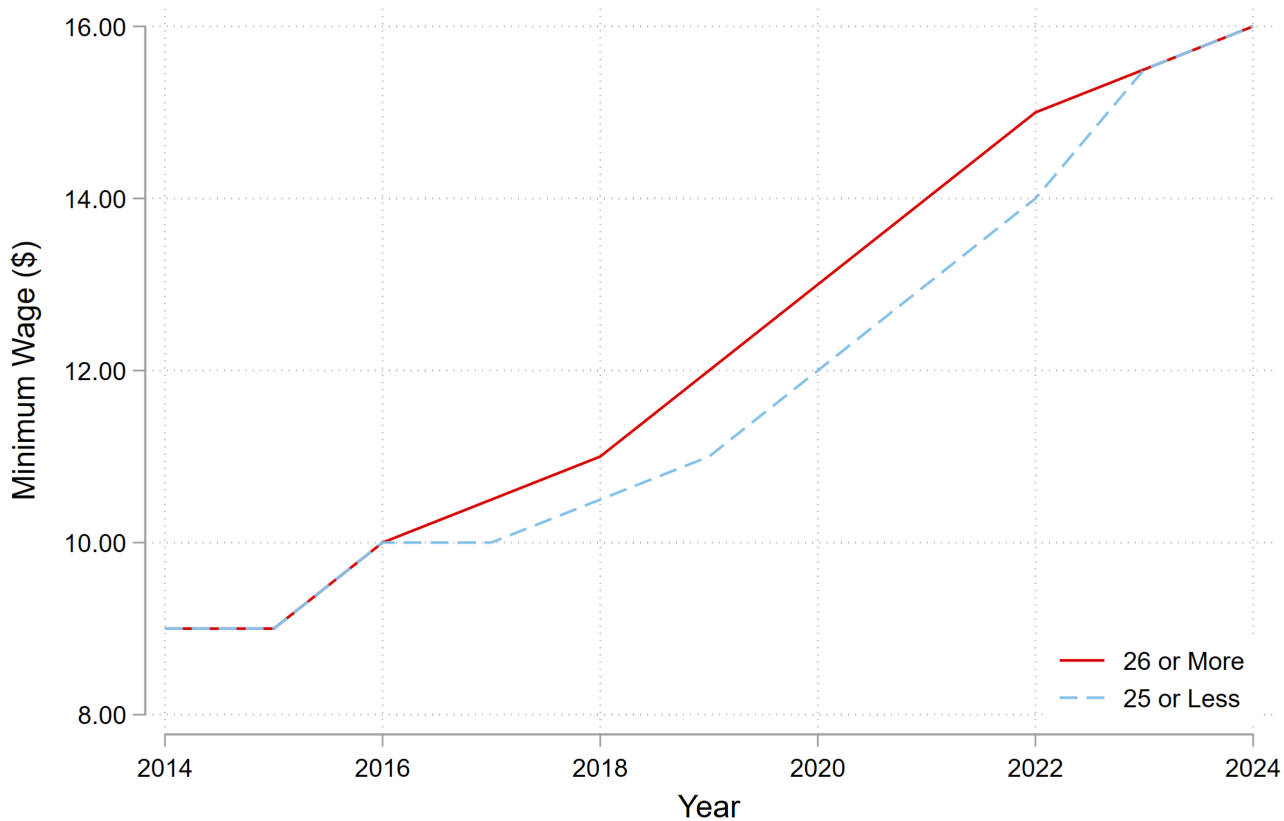

Figure 3.1. Policy Rollout: Minimum Wage by Year

TABLE 3.1

SUMMARY STATISTICS OF FIRMS BY SUBGROUP

	(1) All	(2) High	(3) Medium	(4) Low
Employment	–	-*	-	+*
Payroll	+***	+***	+***	+***
Average Earnings	+***	+***	+***	+***
Firm Age	-***	-***	-***	-***
% High Use	-***			
% Medium Use	-***			
% Low Use	+***			
N Treatment	39,000	7,900	11,500	19,500
N Control	358,000	77,000	112,000	170,000

*Notes: Data comes from the Census's Longitudinal Business Database. Observations are all firms in the year 2026. Column 1 uses all firms. Column 2 uses firms classified by Naics 72. Column 3 uses firms classified as Naics . Column 4 uses firms from Naics. Estimates represent the sign for the variable for the state of California minus the variable for the rest of the United States. Sample sizes are rounded. Significance is denoted by * $p < 0.1$, ** $p < 0.05$, and *** $p < 0.01$.*

TABLE 3.2

SUMMARY STATISTICS OF FIRMS BY SUBGROUP

	(1) All	(2) High	(3) Medium	(4) Low
Employment	–	-*	-	+*
Payroll	+***	+***	+***	+***
Average Earnings	+***	+***	+***	+***
Firm Age	-***	-***	-***	-***
% High Use	-***			
% Medium Use	-***			
% Low Use	+***			
N Treatment	39,000	7,900	11,500	19,500
N Control	358,000	77,000	112,000	170,000

*Notes: Data comes from the Census's Longitudinal Business Database. Observations are all firms in the year 2026. Column 1 uses all firms. Column 2 uses firms classified as high minimum wage using industries. Column 3 uses firms classified as medium minimum wage using industries. Column 4 uses firms classified as low minimum wage using industries. Estimates represent the sign for the variable for the state of California minus the variable for the rest of the United States. Sample sizes are rounded. Significance is denoted by * $p < 0.1$, ** $p < 0.05$, and *** $p < 0.01$.*

TABLE 3.3

CITIES WITH HIGHER MINIMUM WAGES

(1) Place	(2) 2017 Minimum Wage	(3) 2018 Minimum Wage
Berkeley	$12.53	$13.75
Cupertino	NA	$12.00
El Cerrito	$11.60	$12.25
Emeryville	$13.00	$14.00
Long Beach	NA	$10.50
Los Altos	NA	$12.00
Los Angeles∗	$10.50	$12.00
Los Angeles County∗	$10.50	$12.00
Malibu∗	$10.50	$12.00
Mountain View	$11.00	$13.00
Oakland	$12.55	$12.86
Palo Alto	$11.00	$12.00
Pasadena∗	$10.50	$12.00
Richmond	$11.52	$12.30
San Diego	$10.50	$11.50
San Francisco	$13.00	$14.00
San Jose	$10.30	$10.50
San Leandro	NA	$12.00
San Mateo	NA	$12.00
Santa Clara	$11.00	$11.10
Santa Monica∗	$10.50	$12.00
Sunnyvale	$11.00	$13.00

*Notes: Data comes from the UC Berkeley Labor Center's detailed inventory of municipal minimum wages. Figure 2 lists the municipalities in the state of California that have a higher minimum wage than the state's minimum wage of 11. We exclude these cities from the sample because the state's dual minimum wage structure will not bind. Cities with an * have a higher minimum wage structure than the state's but still impose the dual structure at the 26-employee threshold.*

TABLE 3.4

SUMMARY STATISTICS: ZIP CODE CHARACTERISTICS

	California (1)	Rest of US (2)
Percent Male	0.51	0.50
Percent Married	0.50	0.54
Percent Unemployed	0.10	0.07
Percent in Labor Force	0.59	0.60
Percent with Some College	0.62	0.53
Percent with College	0.30	0.23
Percent Below Federal Poverty Line	0.17	0.15
Median Household Income ($1000)	65.72	54.07
Median Housing Value ($1000)	448.28	164.67
GINI Index	0.43	0.41
Observations	1,763	31,357

Notes: Data comes from the 2012-2016 5 year average ACS dataset. Each observation is a Zip-Code-level variable. Column 1 is every zip code in California. Column 2 is every zip code in the rest of the United States.

TABLE 3.5

EVENT STUDY ESTIMATES, LEFT OF CUTOFF

	(1) N	(2) 2012	(3) 2013	(4) 2014	(5) 2015	(6) 2017	(7) 2018	(8) 2019
Panel A: No Covariates								
High	84,500	-	-*	-	-	+	+**	+
Medium	123,000	-	+	+	+	+	+	+
Low	189,000	-***	-	-	-	-	-**	-
Panel B: With Covariates								
High	84,500	-	-	+	+	+	+*	+
Medium	123,000	+	+	+	+	+	+	+
Low	189,000	-*	-	-	-	-**	-***	-**

Notes: Data come from the U.S. Census Bureau's Longitudinal Business Database (LBD). Each row reports coefficients from a single event study regression estimated using Equation 3.2, where the outcome is a binary indicator equal to one if the firm has between 20 and 25 full-time equivalent employees. Panel B includes controls for ZIP code-level characteristics from the American Community Survey (ACS) 2012–2017 five-year estimates, interacted with year fixed effects. Firm groups (High, Medium, Low) reflect relative reliance on minimum wage labor. Standard errors are robust to heteroskedasticity and clustered at the ZIP code level. Statistical significance is denoted as follows: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

TABLE 3.6

EVENT STUDY, RIGHT OF CUTOFF

	(1) N	(2) 2012	(3) 2013	(4) 2014	(5) 2015	(6) 2017	(7) 2018	(8) 2019
Panel A: No Covariates								
High	84,500	-	-	-	+	-	-	-
Medium	123,000	-	+	+	-	-	-**	-***
Low	189,000	-	-	-	-	+	-	-***
Panel B: Covariates								
High	845,00	-	-	+	+	-	-	-*
Medium	123,000	+	+*	+	-	-	-**	-
Low	189,000	+	-	-	-	+	-	-*

*Notes: Data come from the U.S. Census Bureau's Longitudinal Business Database (LBD). Each row reports coefficients from a single event study regression estimated using Equation 3.2, where the outcome is a binary indicator equal to one if the firm has between 26 and 30 full-time equivalent employees. Panel B includes controls for ZIP code-level characteristics from the American Community Survey (ACS) 2012–2017 five-year estimates, interacted with year fixed effects. Firm groups (High, Medium, Low) reflect relative reliance on minimum wage labor.Standard errors are robust to heteroskedasticity and clustered at the ZIP code level. Statistical significance is denoted as follows: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.*

TABLE 3.7

TWFE ESTIAMTES

	(1)	(2)	(3)
	N	Left of Cutoff	Right of Cutoff
Panel A: No Covaraites			
High	678,000	+***	-
Medium	987,000	+	-
Low	1,514,000	-	-
Panel B: Covaraites			
High	678,000	+	-**
Medium	987,000	+	-***
Low	1,514,000	-***	-

*Notes: Data come from the U.S. Census Bureau's Longitudinal Business Database (LBD). Each estimate reflects the sign and significance level from a separate two-way fixed effects regression as specified in Equation 3.1. The dependent variable in Column 2 is an indicator equal to one if the firm has between 20 and 25 full-time equivalent employees; in Column 3, it is an indicator equal to one if the firm has between 26 and 30 full-time equivalent employees. Panel B includes ZIP code-level controls from the American Community Survey (ACS) 2012–2017 five-year estimates, interacted with year fixed effects. Each row represents a different group of firms categorized by their reliance on minimum wage labor (High, Medium, Low). Standard errors are robust to heteroskedasticity and clustered at the ZIP code level. Statistical significance is denoted by * $p < 0.10$, ** $p < 0.05$, and *** $p < 0.01$.*

TABLE 3.8

RANDOMIZATION INFERENCE

	(1) No Covariates	(2) Covariates
Alabama	-	-
Alaska	-	-
Arizona	+	+
Arkansas	-	-
California	+***	+
Colorado	+	+
Connecticut	-	-
DC	+**	+***
Delaware	-	-
Florida	+*	+*
Georgia	-*	-*
Hawaii	+	+
Idaho	+**	+**
Illinois	-	+
Indiana	-***	-***
Iowa	+	+
Kansas	-	-
Kentucky	+	+
Louisiana	-*	-*
Maine	+	+
Maryland	-*	-
Massachusetts	+	+
Michigan	-	-
Minnesota	+	+
Mississippi	+	+
Missouri	+	+
Montana	-	-

Continued on next page

TABLE 3.8 CONTINUED

Nebraska	-	-
Nevada	-	-
New Hampshire	+	+
New Jersey	+	+
New Mexico	+	+
New York	-	-
North Carolina	+	+
North Dakota	-	-
Ohio	-	-
Oklahoma	-	-
Oregon	+	+
Pennsylvania	-	-
Rhode Island	-	-
South Carolina	+	+
South Dakota	-	-
Tennessee	+	+
Texas	-	-
Utah	-	-
Vermont	+	+
Virginia	-	-
Washington	$+^{**}$	$+^{**}$
West Virginia	-	-
Wisconsin	-	-
Wyoming	+	+

*Notes: Data come from the U.S. Census Bureau's Longitudinal Business Database (LBD) in the Accommodation and Food Services industry (NAICS 72). Each estimate reflects the sign and significance level from a separate two-way fixed effects regression as specified in Equation 3.1. The dependent variable in all regressions is an indicator equal to one if the firm has between 20 and 25 full-time equivalent employees. Column 1 estimates the baseline specification. Column 2 includes ZIP code-level controls from the American Community Survey (ACS) 2012–2017 five-year estimates, interacted with year fixed effects. Each row represents a different group assigned as the sole treated group. Statistical significance is denoted by * $p < 0.10$, ** $p < 0.05$, and *** $p < 0.01$. Standard errors are clustered at the ZIP code level.*

TABLE 3.9

RI FULL RESULTS

	Number of States	
	(1)	(2)
T Statistic	No Covariates	Covariates
Negative and Highly Significant	1	1
Negative and Insignificant	23	23
Negative and Moderately Significant	3	2
Negative and Significant	0	0
Positive and Highly Significant	1	1
Positive and Insignificant	19	21
Positive and Moderately Significant	1	1
Positive and Significant	3	2

Notes: Table 3.9 tabulates the results from Table 3.8. Positive and negative refer to the sign of the t statistic. Insignificant corresponds to $\mid t \mid < 1.65$*, moderately significant corresponds to* $1.65 \leq \mid t \mid < 1.96$*, significant corresponds to* $1.96 \leq \mid t \mid < 2.58$*, and highly significant corresponds to* $2.58 \leq \mid t$

TABLE 3.10

EVENT STUDY, MECHANISMS

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	N	2012	2013	2014	2015	2017	2018	2019
Panel A: No Covariates								
Under Threshold	84,500	-***	-	-	-	-	-	
Over Threshold	84,500	+	+***	+***	+	+	+	
Difference	84,500	+**	+***	+***	+**	+	+	
Increase	84,500	+***	+***	+***	+	-	-	
Covariates								
Under Threshold	84,500	-	-	-	-	-	-	
Over Threshold	84,500	+	+**	+**	+	-	+	
Difference	84,500	+	+***	+***	+**	+	+**	
Increase	84,500	+	+**	+	-	-	-	

*Notes: Data come from the U.S. Census Bureau's Longitudinal Business Database (LBD). The sample is all firms in the Accommodations and Services Industry (NAICS 72). Each estimate reflects the sign and significance level from a separate two-way fixed effects regression as specified in Equation 3.1. Each row represents a different outcome specified in 3.5.4. Panel B includes ZIP code-level controls from the American Community Survey (ACS) 2012–2017 five-year estimates, interacted with year fixed effects. Standard errors are robust to heteroskedasticity and clustered at the ZIP code level. Statistical significance is denoted by * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.*

TABLE 3.11

EVENT STUDY, PAYROLL

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	N	2012	2013	2014	2015	2016	2017	2018	2019
No Covariates									
Payroll Per Employee	24,500	+	+	+	-***	-	+	+	
Payroll	24,500	-***	-***	-***	-***	+	+**	+***	
Covariates									
Payroll Per Employee	24,500	-	-	-	-	-	+	+	
Payroll	24,500	-*	-***	-**	-	-	-	+	

Notes: Data come from the U.S. Census Bureau's Longitudinal Business Database (LBD). The sample is all firms in the Accommodations and Services Industry (NAICS 72). Each estimate reflects the sign and significance level from a separate two-way fixed effects regression as specified in Equation 3.1. Each row represents a different outcome specified in 3.5.4. Panel B includes ZIP code-level controls from the American Community Survey (ACS) 2012–2017 five-year estimates, interacted with year fixed effects. Standard errors are robust to heteroskedasticity and clustered at the ZIP code level. Statistical significance is denoted by * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.