Sectoral Wage-Setting and Prices in California*

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In 2024, California implemented an innovative sectoral minimum wage policy, setting a $20 hourly wage floor for workers in large chains in fast food restaurants and snack and non-alcoholic beverage bars. This standard represented 69% of the state's median full-time wage, surpassing alll prior benchmarks in minimum wage policy and research. We utilize administrative data on wages and employment, pay data from Glassdoor, and scrape prices from over 2,000 restaurants in California and control states to document the causal effects of the policy. Using the DID event study methods, we find that the policy increased average weekly wages for covered fast food workers by 12% without reducing employment. Compared to controls, prices increased by 2.1% two quarters after the policy, equivalent to 8 cents for a $4 item. Around 60% of the cost of higher wages was passed on to consumers as higher prices, consistent with a monopsony model.

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1. Introduction

In recent years, minimum wage literature has improved methodologically and expanded vastly to study the effects of numerous policies on various outcomes. Nevertheless, there is no consensus on the effect of minimum wages on prices. While a standard competitive labor market model predicts higher labor costs to be fully passed to prices, models with imperfect competition may predict lower price pass-through. Unlike for some other outcomes, data for prices is not readily available at a granular level, which poses empirical challenges in studying changes in prices. This study presents a new estimate for the price effects of minimum wages using a novel self-collected dataset of online prices in fast food restaurants.

Moreover, a wage floor of $20 studied in this paper constitutes the highest minimum wage of all previous economics studies. Thus, the effects on earnings and employment estimated in this study are the first for minimum wage at such a high level. This wage floor is considerably higher than the previous highest studied by Wiltshire et al. (2024), who obtained the causal effects of minimum wages as high as $15. Additionally, previous papers study the effects of federal or statewide changes. On the other hand, the policy considered in this study is sectoral. It applies to workers in California's fast-food restaurants and snack and nonalcoholic beverage bars.[1]

Notably, the $20 minimum wage came through the passage of AB 1228, which attracted considerable media attention. Some franchise owners stated that the agreement placed all the burden of the new minimum wage on the franchisees and that price increases would increase the royalty payments they would have to make to the licensing companies (Liedke, 2023). Numerous critical articles in the business press cited anecdotal evidence of substantial cuts in jobs and hours and reported that prices had increased "from single digits into the mid-teens." [2]. By contrast, in a September 18, 2024 opinion column in Fox News, California Governor Gavin Newsom cited BLS data showing consistent growth to date in year-over-year fast food employment. While other supporters agreed on the overall positive effects of the policy. [3]

Given the interest in the effects and novelty of the $20 sectoral wage floor and the absence of prior economic studies, we provide here an estimate of the *causal* effects on wages, employment, and prices in California fast food restaurants. To do so, we use administrative data on earnings and employment, private Glassdoor pay, and Square payroll data, and we collect a new dataset of restaurant prices at the individual location level. Utilizing appropriate control groups and modern causal identification methods, we capture the causal effect of the policy.

We use a novel dataset for restaurant wages, consisting of over 26,000 job reports posted on the Glassdoor internet job platform in California and control states before and after the policy. We find that wages increased about 10 percent two quarters after the implementation of the policy. We also observe a substantial upward shift in the distribution of wages in the fast-food industry at the time

[1] Snack and nonalcoholic beverage bars include chains like Dunkin Donuts, Jamba Juice, Starbucks, and TCBY.
[2] See anchor.placer.ai/the-anchor/...
[3] See for example, ktla.com/...; ktla.com/...; www.foxnews.com/...

of the policy. In contrast, the distribution of hourly wages did not change for workers in the same chains located outside of California, nor for California full-service restaurant workers.

We then employ two administrative data sets: BLS' Current Employment Statistics (CES) and the Quarterly Census of Employment and Wages (QCEW) to identify effects on employment and average weekly earnings. Our results suggest that average weekly earnings increased by 8.4 percent in the California fast-food industry. We estimate that roughly 70 percent of California's fast-food industry employees are employed in covered establishments (i.e., belong to a chain with at least 60 locations nationwide). Rescaling the estimate, we estimate that the policy increased the average weekly earnings of covered workers by about 12 percent between 2024q1 and 2024q3.

Turning to employment, we find that monthly seasonally unadjusted survey data does not exhibit a statistically significant change in California employment. Both utilized datasets suggest that fast food employment in California exhibits considerably less seasonality than in the U.S. as a whole and somewhat less seasonality than in our control states. Thus, we deseasonalize the QCEW data by industry and state to better identify the actual effect of the policy. Our preferred specification employs a triple-difference method that compares the fast food industry in California to control states and compares that to trends in the full-service restaurant industry. This preferred specification finds suggestive evidence of an employment decrease. However, after accounting for a small visible pre-trend and relaxing a parallel trend assumption as suggested by Rambachan and Roth (2023), the effect is statistically insignificant and is centered around zero. By contrast, the earnings results exhibit a strong positive effect even with a more relaxed assumption.

We also estimate own-wage elasticities (OWEs), which scale the percent change in employment to the percent change in wages. OWE allows a more straightforward comparison of minimum wage effects to previous studies and is an economically meaningful measure of the policy effects. Our preferred OWE estimate is -0.082. This OWE is remarkably close to the median OWE of -0.09 in the restaurant and retail studies reported in the Dube and Zipperer (2024) meta-study of OWE estimates.

Next, we turn to studying the policy's causal effects on prices. To obtain reliable data on a granular level, we repeatedly scraped menu prices before and after the policy's effective date. We collected price data from a large representative sample of restaurants on the Uber Eats platform, where consumers can place pick-up or delivery orders. To facilitate comparisons among menu items and chains, we restricted our sample to the burger-oriented segment of fast food. Our menu data include a panel of around 900 restaurants in California and 1,100 restaurants in states with binding federal minimum wages. We utilize a difference-in-differences method that compares price changes in fast food restaurants in California to those in states that have not experienced a minimum wage increase since 2009. In addition, we adopt a triple-difference estimator that leverages prices in full-service restaurants, which are not subject to the policy but share many characteristics with the fast food industry.

There are several papers studying the effect of minimum wages on prices. Studies vary greatly in methodology, geography, policy, time horizon, and, most importantly, data. The majority of the papers utilize surveys of establishments. These papers include Katz and Krueger (1992), CARD and KRUEGER (1994), and Ashenfelter and Jurajda (2022). Surveys pose an advantage of having prices on the establishment level. However, they are limited in scope, with most studies obtaining prices from around 300 locations while Ashenfelter and Jurajda (2022) obtain thousands of locations but only for one chain. Two other studies, Aaronson (2001) and Basker and Khan (2016), use city-level average prices for several items for 107 and 371 cities, respectively. Due to the aggregate level of such data, it is impossible to track increases by individual restaurants (or establishments). By contrast, our collected dataset has an advantage of granularity similar to surveys but is scaled to a national level and has more chains due to its digital nature.

We find that the policy led to short-run price increases in fast-food restaurants of about 6.6 percent. Prices decreased subsequently, relative to prices in our control groups, resulting in an overall increase of 2.1 percent two quarters after the policy. This increase amounts to around 8 cents for a $4 hamburger. The pullback in price increases may be related to excessive public expectations about price increases generated by policy opponents, leading to national declines in traffic.[4] Price changes vary considerably by chain. For McDonald's, the chain with the most locations in California and nationwide, prices of five main menu items changed at the same rate, six months after the policy, as prices in the control group of states. Our triple-difference estimator suggests that prices in our sample of large fast-food restaurant chains increased 1.1 percent, relative to the control groups. To provide some context, the price index for "food away from home" increased around 4.8 percent between April 2023 and April 2024, when the $20 wage standard went into effect.

To further quantify the price effect, we calculate price pass-throughs, the share of higher wages passed on to consumer prices. Labor costs constitute about 30 percent of operating costs in fast food. If a $20 minimum wage causes average wages to increase about 11 percent, a 2.1 percent price increase implies a 64 percent pass-through of higher labor costs to prices. This estimate is similar to the finding in Wiltshire et al. (2024) but lower than the commonly cited full pass-through documented by Dube and Lindner (2024). However, we argue that the lower-than-full pass-through of minimum wages to prices is more prevalent in the literature than is commonly believed.

Two seminal papers, Katz and Krueger (1992) and CARD and KRUEGER (1994), find inconclusive results suggesting that minimum wages did not drive price increases in fast-food restaurants in their sample. Two other papers, Basker and Khan (2016) and Ashenfelter and Jurajda (2022), report full pass-through. However, both papers assume unconventional labor shares in their calculations. Using the ratio of fast food payroll to revenue in the 2017 and 2022 Economic Censuses, labor share can be approximated as 0.3. Using wage and price effects from Ashenfelter and Jurajda (2022) and a 0.3 labor share of the cost, we obtain a pass-through estimate of 0.69, lower than one and close to estimates in this paper. Moreover, Wiltshire et al. (2024) uses the same data and finds 0.55 pass-through. Finally, using estimates from Aaronson (2001), and a Census estimate

[4] In addition, several chains responded by introducing $5 Value Meals, which the Washington Post estimated were worth over $10

of labor share costs implies a pass-through of 1.77, which seems highly unlikely. Therefore, a full pass-through of minimum wages to prices is an oversimplification of existing evidence and is inconsistent with the latest studies.

In combination, positive effects on earnings, null employment effects, and a lower-than-full pass-through suggest that employers could adjust margins other than employment to absorb some of their increased wage costs. This result can be reconciled in a framework of monopsonistic power restaurant chains possess. A substantial number of economic studies have demonstrated that many employers in low-wage labor markets possess the power to set wages lower than they would be if labor markets were perfectly competitive (for a recent review, see Manning (2021b)). In addition, Wiltshire et al. (2024) found evidence of employer wage-setting power in the fast food industry, specifically. Hence, fast-food restaurants can absorb some of the increased costs through reduced profit margins. [5]

Interestingly, the sectoral wage policy might generate potential benefits for franchisors, who have licensed franchisees to operate about eighty percent of fast food restaurants. If higher labor costs cause prices to rise, restaurant revenues (but not profits) would likely increase, as Rao and Risch (2024) found using tax data.[6] The revenue increase, in turn, would raise the fees the franchisees are obligated to pay to their licensing franchisors.

This paper proceeds as follows. We discuss the policy's background and coverage and our data in Section 2. Section 3 presents the methodology. Then, we discuss our findings on pay in Section 3, employment in Section 4, and price increases in Section 5. Section 6 concludes.

2. Policy and Data

2.1. Policy Background

AB 1228, enacted by the California legislature and signed into law on September 28, 2023, established a $20 minimum wage for workers in California's fast-food restaurants and snack and non-alcoholic beverage bars, effective April 1, 2024.[7] These two industries employed nearly 750,000 California workers in early 2024. To mitigate the effects on smaller businesses, the policy exempts chains that have fewer than 60 locations nationwide, as well as restaurants located inside airports, stadiums and convention centers. We estimate that about 70 percent of the workers in the two industries are covered by the policy.

The $20 standard constitutes a 25% increase over the state-wide minimum wage. It is also the highest minimum wage in the U.S. and higher in purchasing power parity terms than any minimum

[5]The minimum wage literature has also found that lower employee separation rates absorb some of the wage cost increases through savings in hiring and recruitment costs. We plan to investigate this adjustment channel in the future using Quarterly Workforce Indicators data on employee turnover

[6]Revenues increase when product demand is price-inelastic, as is the case for fast food Okrent and Alston (2012).

[7]www.dir.ca.gov/....

wages in Europe.[8] About 90 percent of non-managerial workers in the two industries were paid less than $20 before the policy.[9] A 90 percent bite is twice the amount in most previous policies. On the other hand, the real level of the $20 minimum wage in California in 2024 is about the same as the peak real value of the federal minimum wage, which was reached in 1968.

The 25 percent overnight increase in the fast-food minimum wage is not unprecedented. San Francisco's citywide minimum wage, implemented in 2004, represented a similar increase, as did San Jose's citywide minimum wage in 2013. Moreover, by 2024, minimum wages in 35 California localities ranged well above $16, reaching as high as $18.67 in San Francisco.

Minimum wage levels are often compared to the median wage of all workers and to the median wage of all full-time workers. Using the California CPS, we estimate that the $20 fast food wage represented 77 percent of the median wage of all California workers and 69 percent of all full-time California workers. Both ratios considerably exceed those for any European country (OECDStats) and those in the extant minimum wage research literature.

AB 1228 emerged from a negotiated agreement involving the governor, the legislature, the International Franchise Association (IFA) and the Service Employees International Union (SEIU) to replace an earlier law. AB 257, passed in September 2022, had established a Fast Food Council with the power to a) set a fast food minimum wage as high as $22, and b) set industry-wide working conditions standards (Egelko, 2023). The agreement that led to AB 1228 limited the initial minimum wage to $20 and eliminated the power of the Fast Food Council to set standards for working conditions, substituting instead a mandate only to recommend standards to the state legislature.[10]

2.2. The Policy's Coverage

As we have noted, fast food workers in small chains and in independent restaurants are not covered by the higher minimum wage. We first estimate the number of restaurants that are covered.

In 2024, California had 38,519 fast food restaurants.[11] According to data provided to us by Dataessential, 33,365 (86.6 percent) of these fast food restaurants were parts of chains and 5,154 (13.4 percent) were independent.[12] Moreover, 25,406 restaurants (76.1 percent) were in chains with more than 60 restaurants nationwide and 7,959 (23.9 percent) were in chains with between 2 and 59 restaurants nationwide. Fast food restaurants in large chains thus represented 66.0 percent of all California fast food restaurants.

[8] With the minor exceptions of the $20.29 minimum wage for large employers in two Seattle suburbs: Renton, WA, population 104,000 and Tukwila, WA, population 22,000.

[9] Estimated from percentile wages for fast food occupations reported by the BLS' Occupational and Employment Wage Survey, May 2023, California.

[10] At various stages, the proposed legislation also specified that franchisors were joint employers, along with the franchisees. This feature does not appear in the final bill.

[11] www.ibisworld.com/...

[12] Source: "Datassential sales intelligence solutions."

We next translate the number of restaurants that are covered into the number of workers who are covered. In the U.S. as a whole, an individual fast food employs on average 15.4 employees.[13] However, chain restaurants must meet minimum sales and employee thresholds established by the chains, while independents do not. The average chain restaurant workforce therefore is likely to be larger than the average among independents. If small and big chains both average 18 employees per restaurant and independent restaurants average 7 employees, then 94.4 percent of all California fast food employees are employed in fast food chains of all sizes and 71.8 percent (76.1x.944) of all California fast food employees are employed in chains with 60 or more restaurants.

2.3. Data

We first describe our wage and employment data and then provide a detailed description of our novel price data.

1. Wage and Employment Data

We use wage data from Glassdoor, a job-posting site, an undisclosed payroll services firm, and the QCEW. Our employment data come from the BLS' Current Establishment Survey and the QCEW.

The Glassdoor data includes wages on full-time and part-time fast food jobs. Notably, our Glassdoor data identifies employer names, allowing us to analyze wages for individual chains as well as groups of fast-food chains. Glassdoor uses a ”give and get” model: workers can search for jobs on Glassdoor if they share information about the pay and working conditions of their current– or most recent– job (see Chamberlain and Zhao (2019) and Chamberlain (2016)). Posts are thus voluntary and do not constitute a probability sample. Glassdoor users are more likely to be lower-paid and less experienced than the average fast-food worker. We can distinguish posted pay for a previous job from pay on a current job. Glassdoor data has been previously analyzed by a few economic papers including Karabarbounis and Pinto (2018) and Sockin (2022).

Our Square data includes monthly pay and hours data from 2022 to 2024 for 475,514 restaurant employees and 24,222 restaurants. Almost all of these restaurants are far too small to be covered by the fast food minimum wage. They therefore provide a useful control group.

We use the BLS' Current Employment Statistics (CES) to assess raw trends around the policy on employment. The CES reports monthly employment data separately for NAICS codes 722511 (full-service restaurants) and for the sum of 722513 (limited-service restaurants), 722514 (cafeterias) and 722515 (snack and nonalcoholic beverage bars), for both California and the U.S., but not for other states. We can, therefore, compare the employment trajectories by industry in California and the U.S. as a whole.[14] Importantly, the CES results reveal higher employment seasonality in

[13] www.ibisworld.com/...

[14] The CES surveys 119,000 businesses and government agencies, representing approximately 629,000 worksites throughout the U.S.

the U.S. than in California, suggesting the importance of de-seasonalizing the data by industry and state.

We use the QCEW county level data to assess causal effects of the policy on employment and weekly earnings. The QCEW provides a near-universe of establishment-level quarterly payroll and reports data for detailed six-digit NAICS industries. The QCEW thus allows us to separately examine changes in pay and employment in limited-service and full-service restaurants as well as in the snack and non-alcoholic beverage bar industry.[15]) As we mentioned above, seasonality might contaminate the estimates. Thus, we use the prior years of the data to remove seasonal fluctuations. Our procedure assumes the same seasonal trends in 2023-2024 as in 2017-2019 and 2022. We also assume a common trend within each industry-state combination (fast-food employment for all counties in California receives the same adjustment for each quarter).

Note, that this paper does not study the effects of minimum wage on average hours worked due to data availability. Hours worked are a related measure to employment. We refer to Allegretto, Dube, and Reich (2011) who study the effects of minimum wages on teen employment and do not detect adverse effects on hours worked.

2. Price Data

To collect price data, we used a web scraping algorithm to collect menu data from the platform of Uber Eats, a popular food delivery service used by almost all fast food chains.[16] To our knowledge, our data for multiple named restaurants and chains across the U.S. constitute the most comprehensive price dataset ever used in minimum wage research. The fast food industry comprises numerous ethnic cuisines as well as chains that specialize in different dishes– burgers, chicken dishes, pizza and others. To obtain the price data for comparable items, we focus on the largest segment of the industry– burger-oriented restaurants– and on their five most popular menu items: cheeseburgers, hamburgers, specialty items, fries and combo meals. Specialty dishes represent a chain's signature dish, such as a McDonald's Big Mac or a Burger King Whopper.

We collected menu prices from three categories of burger-oriented restaurants: the largest fast-food chains in California and the U.S.; the largest full-service restaurant chains serving "American cuisine;" and independent (non-chain) burger-oriented fast-food restaurants in California. Obtaining data for restaurants with comparable menus (or some overlapping items) allows us to examine prices by item, making comparisons more precise and excluding potential counterfactuals that affect the prices of certain meals. Our sample of burger restaurants (American cuisine) includes the largest U.S. fast-food and full-service chains, as measured by estimated market share and number of employees (companiesmarketcap.com).

For the cheeseburger, hamburger, fries and combo menu items, we search for a perfect text match

[15]The QCEW is commonly used to study the effects of minimum wages; examples include Dube, Lester, and Reich (2010), Godoey and Reich (2021), and Wiltshire et al. (2024)

[16]The In-N-Out Burger chain does not participate in food delivery.

to identify the item. If we do not identify a perfect match, we make choices manually: a hamburger represents a smaller, simpler burger without cheese; a cheeseburger is a smaller burger with cheese; fries are french fries of a default size; and a combo meal combines a default size specialty item, a side and a drink. We identify each chain's specialty item using its marketing materials and media. We also verify that they are comparable in size between the chains.

The $20 sectoral wage standard applies to chains in California that have at least 60 locations nationwide. Hence, the California locations of the largest burger-oriented fast-food chains provide our sample of the treated group, while locations outside of California for the same chains serve as a natural control group. To obtain the cleanest estimates of the effect of the policy, we limit our control group to restaurants in states without a binding state minimum wage since 2009. This selection ensures that our control group is not contaminated by minimum wage changes in other states. We identified the largest chains using published industry sources.[17] Table 1 lists our sample of chains and the corresponding number of restaurant locations.

We also collected prices for large national full-service restaurant chains, both in California and in our control states. These restaurant chains constitute a natural control group for our study. We include these chains also to examine wage spillovers from the fast food industry. While employers in the full-service industry are not covered by the new policy, their employees are similar to the the fast food industry workforce. Full-service restaurants thus might need to respond to wage pressures generated by the policy.

In addition to choosing the sample of restaurants, our first round of scraping used "search addresses" in the geographical areas of interest. To enable future county-level analyses, we defined the 25 largest counties in California as our treated locations; these counties cover 95 percent of fast food employment in the state. For controls, we chose the 95 most populous counties in states that do not have state-wide minimum wage policies. This selection of counties, which follows Wiltshire et al. (2024), is suitable for our difference-in-differences estimation.

For each county in our sample, we scraped menus in up to three of the most populated cities, depending on the county's population. For each city, we use the address of its City Hall as an input to the Uber Eats algorithm. The algorithm identifies up to six of the closest restaurants in each chain. For each restaurant, we scraped the full menu on the restaurant's page.

For the second and third waves of price data collection (the post-policy collection), we used the same search algorithm. In addition, we collect prices from any restaurants we missed in the first wave, thereby maximizing the number of restaurants in each time period in our data.

As Table 2 indicates, we obtained prices before the policy and in two periods after, from 810 fast-food California restaurants: including 205 McDonald's stores, and 779 fast-food restaurants in other states. Our sample of full- restaurants includes 77 California restaurants and 387 restaurants in other states. As Table 2 shows, the number of restaurants for each chain in our sample is highly

[17] www.qsrmagazine.com/...; www.businessinsider.com/...

correlated with the total number of restaurants of the chain in California and elsewhere.

Menu prices on delivery platforms may differ from in-store prices. Some restaurants may choose to post higher delivery-based prices either because they perceive delivery to be more price-inelastic or to compensate for commissions charged by delivery platforms.[18] To assess potential differences between Uber Eats menu prices and in-store prices, we collected prices directly from the websites of three fast food chains in our sample (Burger King, Wendy's and Carl's Jr). We can therefore examine whether prices on the restaurant's website differ from those on Uber Eats and whether the price effects differ using Uber Eats' prices. Since we did not detect any systematic differences in price increases, we do not include these results in this report.

3. Empirical Strategy

We employ a difference-in-differences and triple differences methods throughout the paper. As described above, our three main data sources are defined at different levels. Thus, we modify our methods slightly to accomodate each dataset. We describe each method below.

3.1. Event-Study Using Individual Hourly Wage Data

We use a difference-in-differences event-study specification to study the effect of the minimum wage on hourly pay using Glassdoor data. The data is a repeated cross-section at the worker level, with employer and state variables identified. The model can be captured by the following equation:

$$Y_{ifs,t} = \alpha_s + \tau_t + \gamma_{f,t} + \sum_{k=-5}^{2} \beta_k \times CA_i \times I\{t = k\} + \varepsilon_{ifs,t} \tag{1}$$

where $Y_{ifs,t}$ is log hourly pay for employer i, working in franchise f in a state s at event time t. α_s and τ_t are state and time fixed effects, respectively. We additionally augment the model by adding time-by-franchise fixed effect, $\gamma_{f,t}$. CA_i is an identifier equal to one for workers in California (subject to the policy), and $I\{t = k\}$ is an indicator equal one for the relevant period. $\varepsilon_{ifs,t}$ is a random error. Finally, β_t is a coefficient of interest representing a causal effect of the \$20 minimum wage t quarters after the implementation. We use the month before the policy as our reference period $t = 0$. Each consecutive t represents a quarter shift in time. We cluster standard errors at the state level. We weight results by state population according to 2010 Census.

The identifying assumption requires that California and non-California wages have parallel trends in the absence of the policy. We can assess the plausibility of the assumption by assessing pre-trends included in the event-study. Additionally, in our main results, we present a joint coefficient for all pre-policy periods, providing a joint test for pre-trends.

[18] Massimo (2021) reports that prices are higher on Uber Eats than in restaurants.

3.2. *Event-Study for County Average Weekly Wages and Employment*

We employ a difference-in-difference event-study specification to study the effects on the county employment rate and average weekly earnings using QCEW data. The model can be captured by the following equation:

$$Y_{i,t} = \alpha_i + \tau_t + \sum_{k=-3}^{1} \beta_k \times CA_i \times I\{t = k\} + \gamma X_i, t + \varepsilon_{i,t} \tag{2}$$

where $Y_{c,t}$ is an outcome of interest for county i at event time t. We use log employment rate and log average weekly earnings. α_i and τ_t are location-chain and time fixed effects, respectively. CA_i is an identifier equal to one if the county is in California (subject to the policy), and $I\{t = k\}$ denotes an indicator equal one for the relevant period. $\varepsilon_{i,t}$ is a random error. X_i, t is a set of county-period-specific controls. In the main specification, our controls include employment outside the restaurant industry, working-age population, and state GSP growth. Results are also robust to using yearly or quarterly county unemployment rates. Finally, β_t is a coefficient of interest representing the causal effect of the \$20 minimum wage t quarters after implementation. We use the month before the policy as our reference period $t = 0$. Each consecutive t represents a one quarter shift in time. We cluster standard errors by state level and weight results by county population from the 2010 Census.

Restaurant wages and employment vary by season. Moreover, as can be seen in Figure 3, California is more affected by seasonality than the U.S. and in the selected set of control states[19]. To de-seasonalize the data, we assume seasonal trends are defined by quarter for each industry and state. We then residualize outcomes of interest using quarterly QCEW data for the years 2010-2019 and 2023-2024, avoiding years directly affected by COVID-19[20].

The identifying assumption requires that California and non-California employment and wages have parallel trends in the absence of the policy. Considering five quarters before the policy allows us to empirically assess the assumption of prior parallel trends. However, QCEW reporting lags allow us to estimate effects in only two post-policy quarters.

As Figure A1 shows, the parallel trends assumption might be violated for the employment rate outcome. This violation might result from residual seasonality that we do not fully control, or from uncontrolled differences in demand for fast food between treatment and control states. We therefore employ a method of estimating confidence intervals developed by Rambachan and Roth (2023).[21] Their procedure allows us to relax the parallel-trends assumption and impose instead

[19] In panel B we include all control states for which CES data is available.

[20] The results are also robust to performing seasonal adjustment monthly, using earlier years (starting in 2014) and excluding 2023-2024.

[21] Sometimes referred to as ”honest DID”.

restrictions on the difference between post-treatment violations of parallel trends and pre-trends violations.

3.3. Difference-in-differences Model for Prices

To assess the causal effect of the minimum wage policy on restaurant prices, we conduct a similar difference-in-differences event study. The estimated model is:

$$P_{ic,t} = \alpha_{ic} + \tau_t + \sum_{k=1,2} \beta_k \times CA_i \times I\{t = k\} + \varepsilon_{ic,t} \tag{3}$$

where $P_{ic,t}$ is the price of an item in restaurant i of chain c at event time t. α_{ic} and τ_t are location-chain and time fixed effects, respectively. CA_i is an identifier equal to one if the location is in California (subject to the policy), and $I\{t = k\}$ equals one for the relevant period. $\varepsilon_{ic,t}$ is a random error. Finally, β_t is a coefficient of interest representing a causal effect of the $20 minimum wage t quarters after the implementation. We use the month before the policy as our reference period $t = 0$. Each consecutive t represents a quarter shift in time. We cluster standard errors by state and chain level.

We estimate price effects for each individual chain and pooling all the chains together. The individual chain estimates provide a price change by item for a particular chain (e.g., McDonald's or Burger King). The pooled estimate averages the price changes among all chains for each menu item. We weight the pooled results by the number of each chain's California locations. Such weighting provides the most representative price effect for the fast-food industry in California.

Our model assumes parallel trends before the policy: that within-chain price changes before the policy trend similarly in California locations as in other states. To select control groups without the policy that are most likely to trend similarly, we restrict our control group to locations in the same chains in the largest counties in states that never adopted a state-wide minimum wage policy. This control group closely follows the donor pool in Wiltshire et al. (2024), who find parallel pre-trends for fast food earnings and employment.

A threat to our identification strategy involves firms' anticipating the policy and beginning to adjust their prices before the policy became effective. Since we collected pre-policy data in mid-March 2024, our research design accounts for anticipation effects that occurred within two weeks of the policy's effective date. Although we cannot detect earlier anticipatory price changes, we do not expect those to be as common. An individual firm that raised its prices well before the effective date might find that other firms do not follow its lead. As a result, a strategy of increasing prices too early risks losing market share to firms that did not increase their prices. However, as the effective date of the policy approaches, the common shock to all firms makes it more likely that one firm's price increases will coincide with price increases by their competitors.

3.4. *Triple Differences Model for Prices*

To relax some assumptions of the difference-in-differences model for prices we described above, we also present estimates based on the triple difference-in-differences specification. In addition to comparisons between states, this model compares the fast food industry to the full service industry, which is not subject to the new policy. The model is described using the equation below.

$$P_{ic,t} = \alpha_{ic} + \tau_t + \lambda_c + \sum_{k=1,2} \beta_k \times CA_i \times FastFood_c \times I\{k=t\} + \varepsilon_{ic,t} \tag{4}$$

The model builds on 3, adding a fast-food fixed effect, λ_c, and adding an interaction term, $FastFood_c$, which equals one for fast food chains. The specification is equivalent to the difference between two difference-in-differences estimates. The specification leverages that fast food restaurants outside California and full service restaurants in California are not subject to the new policy.

Moreover, this approach provides the cleanest control group, relaxing the identifying assumption of parallel trends. This specification requires the difference between pre-policy fast-food prices and full-service prices in California to trend similarly to the difference between fast-food prices and full-service prices in control states. In other words, the parallel trend assumption is now concerned with trends in differentials rather than trends in levels, as in the difference-in-differences specification.[22] Similarly to the DiD, we apply the Rambachan and Roth (2023) inference procedure to triple differences estimates to account for a potential violation of parallel trends for the employment rate.

4. Effects on Pay

We present here our wage estimates, first using data on large national chains from Glassdoor, then using data on small restaurants from Square and finally using data from the QCEW on six-digit industries.

4.1. *Large National Chains*

Glassdoor job searchers post self-reported hourly base pay for current or past part-time and full-time jobs.[23] Job searchers enter their pay rate when they post information on the platform about their current or most recent job. Importantly, the posting date often occurs several months after the last job began, creating some ambiguity about the relevant time period. As a result, some posted pay rates refer to much earlier time periods. We return to this issue below.

[22] Olden and Møen (2022) provide an in-depth discussion of the triple difference estimator.

[23] We are grateful to Glassdoor for sharing their data with us. Our data excludes temporary employment and managerial positions.

We display the distribution of pre-policy and post-policy wages in the four panels of Figure 1. The two upper panels (A and B) display hourly wage distributions for fast food restaurants; the lower panels (C and D) do the same for full service restaurants. The two left panels (A and C) report wage distributions in California, while the two right panels (B and D) show wage distributions in our control states. In each panel, the blue lines show the pre-policy (pre-April 1) wage distributions and the red lines show the post-policy distributions. The pre-policy data consists of pay rates posted in 2024q1; the post-policy data consists of pay rates posted in 2024q2.[24]

In Panel A of Figure 1, we estimate that the distribution of pre-policy wages (blue line) in California fast food restaurants implies an average pre-policy wage of $17.13, with most jobs paying between $14 and $18. A considerable mass of jobs in the pre-policy period paid less than $16, which was the state minimum wage in 2024q1.

Do the Glassdoor jobs that paid less than $16 reflect noncompliance with the minimum wage or the Glassdoor reporting lags that we mentioned earlier? The franchises of fast food chains generally hire third-party payroll services (such as Paychex and others). To remain competitive, payroll service companies are highly averse to litigation; their software service thus very likely comply with the applicable minimum wage rates in 2024q1. The pre-policy job posts that reported paying less than $16 therefore probably reflect jobs that began earlier than they were posted on Glassdoor. Alternatively, some users may report a"take-home" rate rather than their full base wage.

The mass of pre-policy wages above $16 likely reflects higher local minimum wages, some of which reached $18.67 in 2024, as well as higher pay rates for more experienced workers, and higher pay scales at some chains than others. The small blue spike at $20 may also reflect the tendency of workers to report pay in round numbers.

Consider now the post-policy (red) line in Panel A of Figure 1. The red line shows that pay rates bunched at $20 after the sectoral policy went into effect. The red line in Panel A of Figure 1 also shows some continued bunching at $16. This hump may again reflect reporting lags, rather than noncompliance with the wage standard. Even with some imperfect reporting, the probability of being paid $20 or higher increased threefold.

Glassdoor is an internet platform on which workers search for jobs and share information about their pay rates and other working conditions. To gain access to job postings workers must answer a questionnaire regarding their current or previous position. Online job posts can suffer from self-selection and measurement error. We expect Glassdoor jobs to be more representative of lower wage workers within each industry. Thus, the pay of workers considering switching jobs are more likely to appear on Glassdoor.

We therefore regard the Glassdoor data as informative for assessing the effect of the policy on *targeted* workers: those earning less than $20 per hour before the policy. We find that in the quarter prior to the new minimum wage implementation, the average salary of workers earning less than

[24]We also inspected, but do not report here, pay trends in 2023; the pre-trends are parallel.

the new minimum wage was $16.16.

Panel B of Figure **??** presents pre and post wage distributions for the same restaurant chains that make up Panel A, but are located in our control group of states– those with $7.25 minimum wages. The mass of pre-policy wages in this panel bunches at round numbers– $10, $12 and $15, indicating that the $7.25 federal standard is not binding in those states. As we expect, the post-policy wage distribution is identical to the pre-policy distribution, reinforcing our confidence in the results in Panel A.

Our results in Panel A are also supported by the results for full-service restaurants, displayed in Panels C and D. Here the pre and post policy wage distributions are nearly identical, demonstrating that the sectoral wage standard had little effect on pay in most full-service restaurant jobs. Nonetheless, the small red spike at $20 indicates wage spillovers for about 10 percent of full-service restaurant jobs.

Figure 2 presents event study results on the effect of the minimum wage on hourly pay. In Panel A, for fast food, pay increased 7.0 percent one quarter after the implementation of the policy. The increase persists two quarters after the *event*, to 10.1 percent. This result translates into a $1.68 increase, from a baseline of $16.68.

Including the quarters before the policy allows us to assess pre-trends empirically. In Panel A all estimates before the policy are small in magnitude. All but one are statistically indistinguishable from zero, suggesting that the parallel trend assumption is satisfied for California and the control state fast food restaurants.

Panel B of Figure 2 presents results for the snack and non-alcoholic bars industry. These results suggest a positive effect on hourly wages of 8 to 13 percent, similar in magnitude to our fast food estimates. However, for this industry, we observe some pre-trends in pre-treatment quarters. We therefore interpret our estimates for the snack and non-alcoholic beverage bars industry only as suggestive evidence.

Finally, Panel C of Figure 2 presents results for the hourly pay of workers in the full-service industry. Both post-treatment quarters have small and insignificant estimates. Pre-trends for this industry are large in magnitude and statistically significant in 2024q3-q4. The noise in the data partly reflects the smaller sample of workers and smaller number of firms in the data. This result suggests the absence of any spillovers to the wages of workers in full-service restaurants.

4.2. Small restaurants

Figure 4 displays our main results for the small restaurants that use Square payroll services. Pay in small California fast food restaurants averaged just under $19 at the time of the policy. The figure suggests that pay and employment did not change at the time of the policy. The pre-trends are parallel to the control groups– fast food restaurants outside of California and full-service restaurants inside and outside of California– validating the use of small restaurants as a control for estimating effects on large restaurant chains.

4.3. QCEW event-study earnings effect estimates

We discuss here our event study estimates of the effects of the policy on average weekly earnings, using QCEW data. Columns (1) and (4) of Table 1 present results for average weekly wages using DiD and triple differences specifications, respectively. The post-policy coefficients are positive and economically meaningful. Across two specifications, the effect for the first quarter ranges between 4.4 percent to 6.1 percent. The two second quarter estimates are remarkably close– 8.4 and 8.5 percent, respectively. An increase of 8.4 percent in weekly earnings constitutes an increase of about $45, from a pre-period average of $535. As Panels A and C of Figure A1 show, all the pre-treatment quarter estimates are small and most are statistically indistinguishable from zero in both specifications. These results provide evidence of parallel pre-trends.

4.4. Reconciling The Wage Results

Our Glassdoor data finds a 10.1 percent wage effect, while our QCEW wage effect is 8.4 percent. As we mentioned above, in the Glassdoor data we are able to identify workers who were employed by restaurants directly affected by the policy– those with more than 60 locations nationwide. Meanwhile, the QCEW estimate includes both firms with over 60 locations nationwide that are covered by the policy and those employed in smaller firms and thus excluded from the $20 minimum wage. As we show above, pay of employees in smaller chains was not affected by the policy. Thus, our 8.4 percent estimated wage increase represents a lower bound on the overall effect of covered workers. To obtain a better estimate, we scale our results using our estimated 71.8 percent share of workers within the industry who are covered by the policy– which we discussed in Section 2. This rescaling implies an earnings effect of 11.7 percent for covered workers, remarkably close to the Glassdoor estimate in the same quarter.

5. Employment Effects

5.1. Fast Food Employment Trends

We present employment trends from 2023m1 through 2024m12 for fast food and full service restaurants in Figure 3. The data come from the BLS' monthly Current Establishment Survey (CES).[25] In both panels, the two solid lines refer to California restaurants; in Panel A, the two dashed lines refer to restaurants in the U.S.; in Panel the dashed lines represent the eight states with $7.25 minimum wages that are reported in the CES. The red lines refer to fast food and other; the blue lines refer to full service. To view these lines on the same graph, we use the left vertical axis to measure California employment and the right vertical axis to measure U.S. employment.

[25] CES data on fast food and related employment is available for California and for a limited sample of other states. No other state or locality raised its minimum wage on or near April 1, 2024.

The figure shows much more seasonal volatility in the U.S. than in California, and somewhat more seasonal volatility in a subset of control states. In both 2023 and 2024, employment grew faster in the U.S. from January to June and then fell more rapidly in the second half of each year. We therefore deseasonalize the QCEW employment data before performing event studies.

After accounting for seasonal variation, the figure suggests no to minimal immediate change in employment due to the policy. The trajectory of fast food employment between March and May 2024 resembles the trajectory in the same months in 2023 and a reduction in employment later in the year also appears in the year before the policy. Hence, the raw trends suggest that employment was not significantly affected by the minimum wage policy.

Similarly, comparing the two blue lines (full service, California and full service, U.S.) in Figure 3 and accounting for seasonality, there is seemingly no effect on employment in the full service industry. This is expected, since full service restaurants are not covered by the policy. Additionally, spillover effects on pay are absent, as discussed in Section 3. The evidence of zero employment effect in fast food industry is reinforced by comparing the two industries. The evolution and volatility of fast food employment closely mimics employment in the full service industry, which was not affected by the policy.

5.2. *Employment Effects*

We now turn to discussing more comprehensive causal estimates of the employment effect using the event-study design described in Section 3 and de-seasonalized QCEW data.

We present these results in columns (2) and (5) of Table **??**. The DiD coefficient in the first post-policy quarter is -0.6 percent, but statistically indistinguishable from zero using the Rambachan and Roth (2023) procedure. In the second post-policy quarter, the effect is -1.2 percent and is also not statistically different from zero. The triple-differences specification coefficients are 0.2 and -0.7 percent in each quarter, respectively. Both coefficients are indistinguishable from zero.

The confidence intervals in Table **??** use an "honest DID" procedure developed by Rambachan and Roth (2023). The procedure requires researchers to choose a parameter $\bar{M}$, which equals the ratio of the hypothetical post-policy trend to the pre-trend. We choose $\bar{M} = 1$, which assumes The trend difference between the treatment and control groups that is unrelated to the policy is the same pre- and post-policy.

To assess the robustness of our results, we conduct the sensitivity analyses suggested in Rambachan and Roth (2023). We present these results for DiD in Figure A2. Each panel displays (in blue) the 95 percent confidence intervals as $\bar{M}$ varies from 0 to 1.4. The original confidence interval appears in red.

Panels A and C show that the effect on log wages is significant at every level of $\bar{M}$ up to 1.4. This result suggests that we can reject a null hypothesis as long as the post-treatment violation of parallel trends is no more than 1.4 times the worst pre-treatment violation of parallel trends. This conservative assumption is implausible. On the other hand, at realistic values of $\bar{M} = 1$ and less the wage effect is statistically different from zero.

The effect on log employment in Panel B implies a statistically insignificant effect in 2024q2 even at the strictest assumption of parallel trends (i.e., original). For 2024q3, the employment effect is statistically insignificant at any $\bar{M} \geq 0.2$. This result suggests that the employment effect is indistinguishable from zero as long as the post-treatment violation of parallel trends is at least 0.2 times the worst pre-treatment violation of parallel trends.

In Figure A3, we present sensitivity tests for the triple-differences specification. The results are similar to those for DiD. As expected, for each given $\bar{M}$ triple-difference CI is wider than its DiD counterpart. Nevertheless, the log wage effect is statistically significant up to $\bar{M} = 1.2$ in 2024q2 and up to $\bar{M} = 0.6$ in 2024q3. In contrast, the log employment effect is not statistically different from zero at $\bar{M} = 0.2$ and above.

Overall, we interpret our results as a null employment effect. This result is consistent with the recent literature on employment effects of minimum wages in the restaurant industry, such as Wiltshire et al. (2024) and Dube et al. (2024). The absence of a significant disemployment effect is consistent with the presence of monopsony power (see Manning (2021a). A minimum wage increase enables firms to hire the same number of employees or more. This monopsony interpretation pertains only so long as the minimum wage remains below the marginal revenue product of labor. Whether that level is reached with a given minimum wage in a given industry is an empirical question. According to our employment results, the $20 minimum wage is still below the marginal revenue product of labor in California's fast food industry.

Rescaling the estimates to be representative of the whole industry, our point estimates suggest an employment effect from -1.7 to 0.28 percent. Together with the wage estimates, these suggest an own-wage elasticity (OWE) between -0.14 and 0.03. However, as discussed above, employment estimates are statistically indistinguishable from zero when using robust inference, Thus we consider OWE estimate to be around zero.

6. Effects on Prices and Price Pass-throughs

6.1. Price Levels Before the Policy

Table A1 displays pre-policy prices for five main menu items in each of nine burger-oriented fast food chains in our sample. These prices are for California stores only. The chain are arrayed according to the number of stores in California. The top three rows display menu item price averages across all nine chains, lower-price chains and higher-price chains, respectively. All averages across chains are weighted by the number of California stores in each chain.

The larger chains tend to have lower prices across all five menu items than do the smaller ones. Prices for each item are relatively low and similar among the largest four chains: McDonald's, Burger King, Jack in the Box and Wendy's. Prices may be lower because of scale economies in operations, and/or because they target a different segment of fast-food consumers. On the other hand, price variation within a chain, measured by the standard deviation of prices and reported in parentheses in Table A1, is greater in these four chains, indicating that franchisees have some leeway to choose prices, or that Uber Eats' menus vary by geography from in-store menu prices.

6.2. Price Changes

Panel B of Figure 5 compares recent restaurant price index changes in two major California metros with recent restaurant price changes in the U.S. as as whole. The price indices come from the large surveys undertaken by the BLS to construct the monthly Consumer Price Index. [26]

Table 3 reports our difference-in-differences estimates of price changes for fast food and full service. These results multiplied by 100 can be interpreted as percent changes. On average, fast food prices increased 3,8 to 6,8 percent, depending on the menu item, one quarter after the policy was implemented. Price changes were more prominent for higher-price chains. Interestingly, the price of a specialty burger increased only by 3.8 percent. This pattern might indicate the importance of pricing of "signature" items (such as a Big Mac or Whopper) for marketing purposes and to keep customers from switching to competitors.

The price effects are smaller two quarters after the policy than after one quarter. For hamburgers, the price effect is statistically indistinguishable from zero. Specialty burgers and combo prices increased on average by only 0.8 and 1.5 percent, respectively. On average, prices increased 2.1 percent. Similarly to the one quarter results, prices increased more as a result of the policy for higher-price chains.

The bottom two rows of Table 3 displays price changes for full service restaurants. We do not expect to see direct effects of the policy on full service restaurants, since they are not covered by the policy. However, some spillover effects are possible. The estimated price effects for average of main items in our full service restaurants are small in magnitude and show a 0.9 percent increase. We obtained the same result after two quarters. However, hamburger prices increased 3.9 percent, suggesting potential spillovers or a common shock to input prices around the time of the policy. Overall, the results suggest no to minimal spillover effects among full service restaurants.

As additional evidence, we present results in Table 4 for our strongest identifying specification, triple-differences. Since the model uses data for both fast food and full service restaurants, we can study only a subset of menu items that are present in both types of restaurants. Our estimated price increases for average of main items are 5.6 and 1.1 percent in the first and second quarters, respectively. As with the previous results, prices in higher-price chains are affected more on average

[26] Unfortunately, the BLS does not report separate price indices for fast food and full service restaurants. According to BLS, restaurant prices increased 5.2 percent during 2023 and at an annual rate of 2.8 percent in 2024q1.

In the twelve months before the sectoral wage policy began, the California and U.S. lines are parallel, indicating similar changes in restaurant prices. After April 1, California restaurant prices increase faster than do restaurant prices in the U.S. as a whole.

in both studied periods. The average price increase is 6 percent in the first quarter and 2.7 in the second.[27]

Finally, Table A2 presents difference-in-difference price effect estimates by fast food chain for the latest available quarter after the policy. The smallest increases are observed at McDonald's, ranging from -1.4 to 0.3 percent. We observe the largest changes at Carl's Jr, ranging from -21.4 percent for an average of main menu items to -9.1 percent for hamburger.[28] Price effects were lower for each chain's specialty item (becoming negative for some), validating our decision to collect data on these items. Price increases may vary by menu item and chain for a number of reasons, including varying labor intensity by item and chain, heterogeneous wage increases by chain, and different price-setting power of chains.

6.3. Cost Pass-through to Prices

We consider here the extent to which increases in labor costs were passed on to consumers in higher prices or were absorbed by restaurants in reduced profits. Since labor costs represent about 30 percent of a fast food restaurant's operating costs, an increase in wages of 11.8 percent could be fully absorbed by a price increase of 3.54 (11.8x0.3) percent, without any reduction in profits. Our preferred estimated price increase of 2.1 percent thus indicates that about 59 percent of the cost increases were borne by consumers. With a Glassdoor estimated wage increase of 10.1 percent, the cost pass-through is 69 percent. Thus, we estimate the pass-through to prices is between 60 and 70 pecent. This result is in line with the recent literature on minimum wage effects on prices. A few recent papers find partial pass-through of minimum wages to prices, possibly due to monopsony power. By contrast, many papers find about a full pass-through.

The initial price increase of 6.6 percent in the first quarter after the policy suggests an extremely high pass-through of around 400 percent. This result might indicate that fast-food restaurants overestimated the cost of the policy and increased their prices well beyond levels that would offset higher costs. Hence, it is important to examine whether further price adjustments occurred over a longer period.

The price increases probably translated into higher restaurant revenues, since the price increases were likely greater than a potential reduction in consumer demand. Franchise licenses granted by a chain's parent company to individual restaurant owners call for a royalty fee to be paid to the parent company. The fee is usually a fixed percentage of the restaurant's revenue. Restaurant owners may thus have to pay a greater amount to the parent companies. This finding suggests that the policy might have benefited franchisors by increasing their franchise fees.

[27] Note that standard errors are higher by construction in triple difference estimates, which reduces the statistical significance of some estimates.

[28] This negative estimate does not imply that hamburger prices fell in California. A negative estimate can occur when prices rise more in the control states than in California.

7. Conclusions

On April 1, 2024, California established a $20 sectoral wage standard for fast food and related workers in large chains. The statewide minimum wage for all other workers remained $16. The $20 sectoral standard, which covers about 750,000 workers, is higher than any minimum wage in the world. Although the bulk of recent minimum wage studies find minimal employment effects, many of these studies examine minimum wages up to only $12; only one examines the effects of $15 minimum wages.

In this report, we use novel Glassdoor wage data for fast food and full service restaurants in California and in a control group of states without a minimum wage increase since 2009, BLS data on employment in fast food and full service restaurants in California and the U.S., and novel scraped data on menu prices in burger-oriented fast food and full service restaurants in California and in states without a minimum wage increase since 2009. To identify the causal effects of the sectoral wage policy, we deploy difference-in-differences event study methods that control for changes in other states and in full service restaurants.

We find that the sectoral wage standard raised average pay of non-managerial fast food workers by about 10 to 12 percent. At the same time, the policy did not reduce employment. It increased fast food prices by about 2.1 percent, or about 8 cents for a $4 item, two quarters after the policy. Therefore, we estimate a price pass-through of 0.6 to 0.7. Restaurant revenues likely increased; the royalty fees restaurant operators pay to franchisors therefore rose as well.

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Tables and Figures

Table 1
MW effects on average weekly earnings, employment rate and own-wage elasticity in fast food using QCEW

	DID			DDD		
	(1)	(2)	(3)	(4)	(5)	(6)
	Wages	EPop	OWE	Wages	EPop	OWE
Q2*Treat	0.044	-0.006	-0.136	0.061	0.002	0.033
	[0.009, 0.067]	[-0.21, 0.014]		[0.011, 0.105]	[-0.171, 0.173]	
Q3*Treat	0.084	-0.012	-0.143	0.085	-0.007	-0.082
	[0.015, 0.135]	[-0.036, 0.025]		[-0.021, 0.169]	[-0.349, 0.339]	
N	1,029	1,029		2,044	2,044	
County FE	X	X		X	X	
Quarter FE	X	X		X	X	
Industry-by-qtr FE				X	X	
Controls	X	X		X	X	

Note: Columns (1)-(2) are estimated using Equation 3, while columns (4)-(5) using Equation 4. Columns (3) and (6) are derived using delta method. All columns include county and quarter-fixed effects. Columns (4)-(5) additionally use time-by-industry fixed effects. Standard errors are clustered at the state level. 95% confidence intervals, reported in parentheses, are obtained using Rambachan and Roth (2023) procedure with a bound parameter $\bar{M} = 1$. All regressions use as controls the outcome of interest outside the restaurant industry, county population, and quarterly state-wide GDP growth .

Table 2
Number of restaurants overall and in our sample, by chain and location

	Overall		Sample	
	California	Non-California	California	Non-California
A. Fast-Food				
McDonald's*	1,221	12,308	205	133
Jack in the Box*	942	1,251	178	81
Carl's Jr and Hardee's	647	1,990	73	32
Burger King*	534	6,193	126	141
Wendy's	297	5,711	125	190
The Habit*	258	109	47	10
Five Guys	123	1,377	13	74
Sonic	82	3,430	12	70
Shake Shack	60	290	31	48
Total			810	779
B. Full-Service				
Denny's	358	996	38	131
Applebee's*	106	1,430	18	93
Buffalo Wild Wings	99	1,199	7	71
Red Robin*	57	438	8	47
Outback Steakhouse	44	632	6	63
Total			77	387

Note: Table depicts a number of restaurants by chain in California and other states. Columns 1-2 show the overall number of locations in Spring 2024. Columns 3-4 show the number of locations in the collected data. All locations in the sample are present in each wave of data collection. Numbers are taken from the company's resources. The company does not report a number of locations publicly, so best approximation is taken from the other resources.

Table 3
Difference-in-differences log price effect by item and group

	(1) Hamburger	(2) Specialty burger	(3) Combo	(4) Average of main items
A. All fast food				
Q2	0.068***	0.055***	0.038***	0.066***
	(0.006)	(0.003)	(0.001)	(0.005)
Q3	0.002	0.008***	0.015***	0.021***
	(0.004)	(0.002)	(0.003)	(0.005)
B. Lower-price chains				
Q2	0.069***	0.055***	0.036***	0.066***
	(0.006)	(0.004)	(0.001)	(0.005)
Q3	0.000	0.007***	0.015***	0.020***
	(0.004)	(0.002)	(0.003)	(0.006)
C. Higher-price chains				
Q2	0.051***	0.061***	–	0.069***
	(0.002)	(0.001)		(0.001)
Q3	0.035***	0.034***	–	0.036***
	(0.003)	(0.002)		(0.002)
C. Full service restaurants				
Q2	-0.057***	0.008***	–	0.009***
	(0.008)	(0.002)		(0.002)
Q3	0.039***	0.007***	–	0.009***
	(0.006)	(0.003)		(0.002)

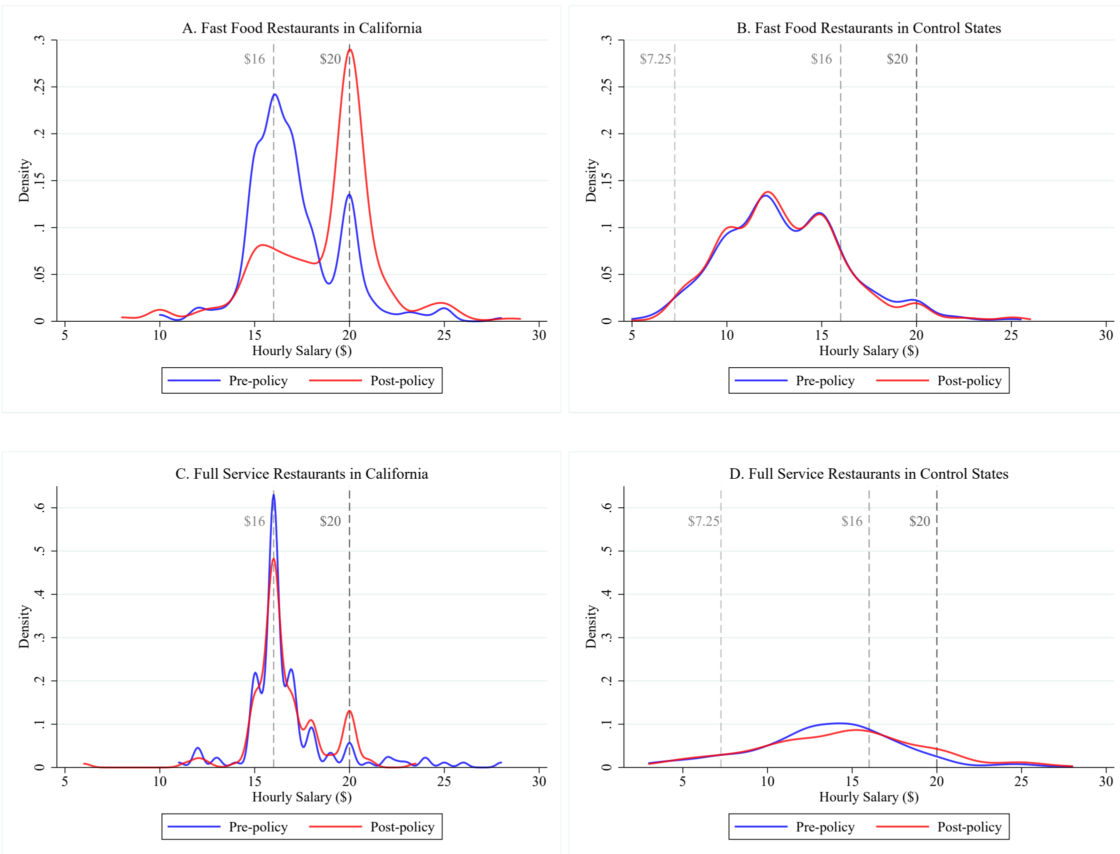
Note: Estimated using Equation 3. Each outcome is a log price of the stated item. Treatment effects are weighted by the number of locations in California of a given franchise. Each specification includes restaurant and time fixed effects. Missing cells represent variables that do not have enough data for estimation. Standard errors are clustered at the state level. Statistical significance is marked as follows: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 4
Triple difference log price effects by item and group

	(1) Hamburger	(2) Specialty burger	(3) Average of main items
A. All fast-food			
Q2	0.125***	0.047***	0.056***
	(0.012)	(0.005)	(0.005)
Q3	-0.037***	0.001	0.011**
	(0.008)	(0.004)	(0.005)
B. Lower-price chains			
Q2	0.126***	0.047***	0.056***
	(0.012)	(0.005)	(0.006)
Q3	-0.039***	-0.000	0.011*
	(0.009)	(0.004)	(0.005)
C. Higher-price chains			
Q2	0.108***	0.053***	0.060***
	(0.009)	(0.003)	(0.002)
Q3	-0.005	0.027***	0.027***
	(0.004)	(0.005)	(0.003)

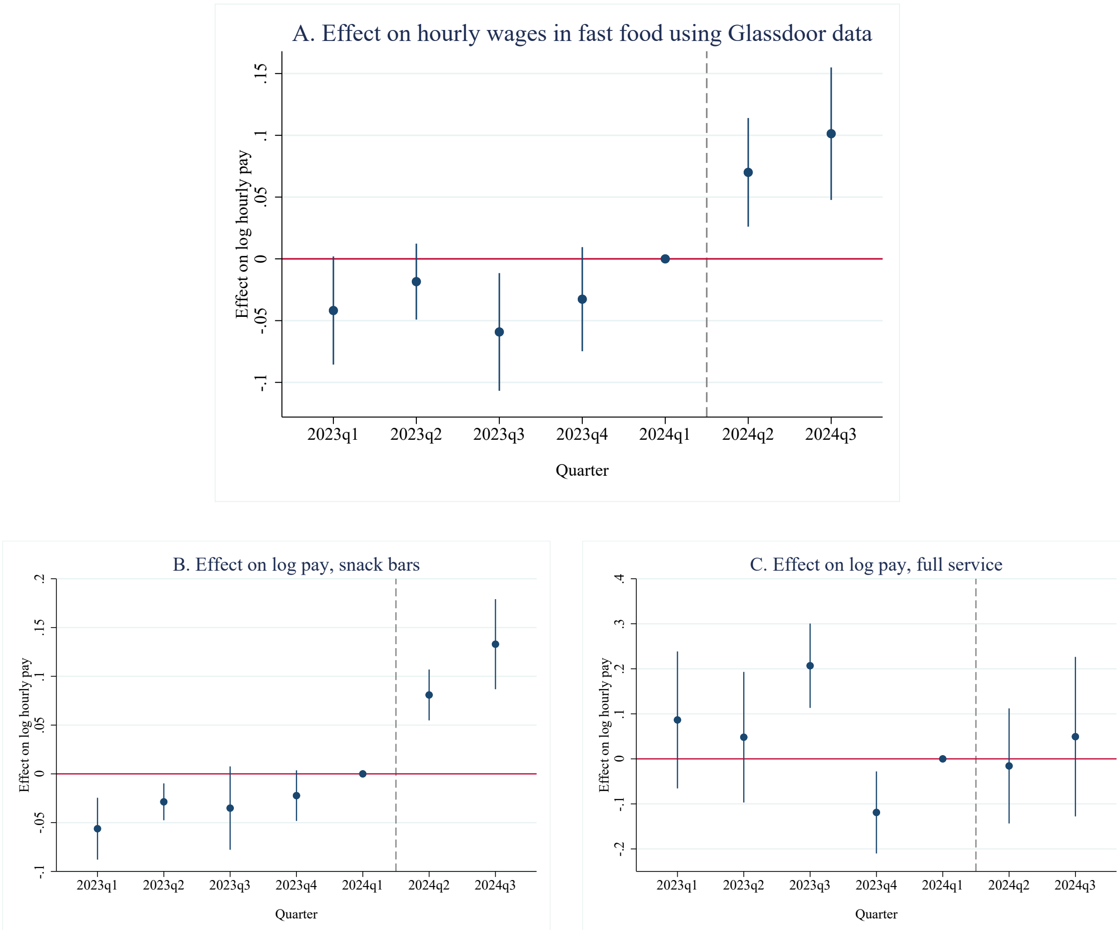
Note: Estimated using Equation 3. Each outcome is a log of the stated variable. The last row represents the average effect weighted by the number of locations in California. Missing cells represent variables not captured by the data-collection algorithm. Statistical significance is marked as follows: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Figure 1:
Glassdoor Hourly Wage Distribution Pre- and Post-policy for Fast Food and Full Service Restaurants



Note: Constructed using data provided by Glassdoor. The pre-policy period includes wages for 2024Q1; the post-policy includes wages for 2024Q2. Panels A and B include reported wages in the "Restaurants & Cafes" industry for fast food restaurants in our price data and the top 20 fast-food chains– ranked by the number of salaries reported in the period of interest in California. Panels C and D include wages for full service restaurants in our price data and the top 25 full service chains ranked by the number of salaries reported in the period of interest in California. Distributions are constructed using kernel density approximation. Excludes managerial and sales occupations. The vertical dashed lines represent the pre-policy California minimum wage, $16, and the new minimum wage for fast food in California, $20.

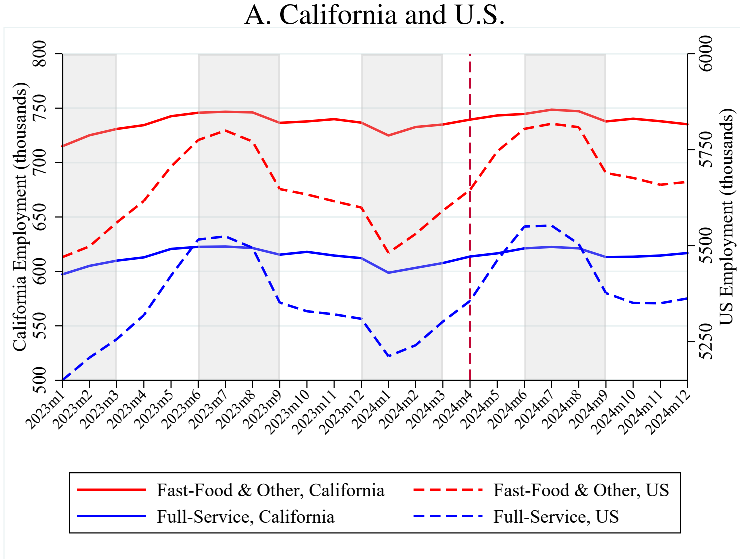
Figure 2:
Effects on Glassdoor hourly pay by industry



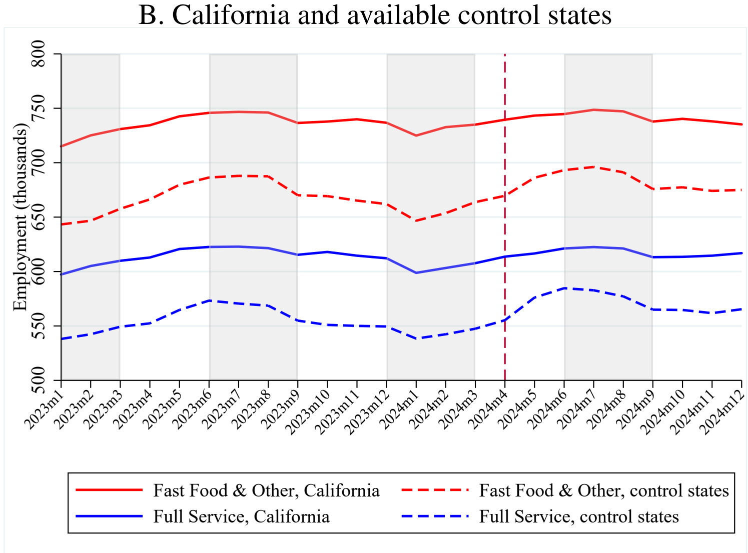
*Note:*Estimated using Equation 1 and Glassdoor data. The outcome in each panel is log state-firm average of unadjusted base hourly pay reported by workers. All estimates are relative to 2024q1, the last quarter before treatment. Panel A uses wages reported for a sample of identified restaurants in NAICS 722513; Panel B shows 722515; and Panel C shows 722511. The gray dashed line represents the time the policy was implemented. The dots show estimates weighted by number of firms' locations in California. Lines show 95 pecent confidence intervals. Standard errors are clustered at the state level.

Figure 3:
Fast-Food and Full Sevice Employment in California and U.S.

A. California and U.S.

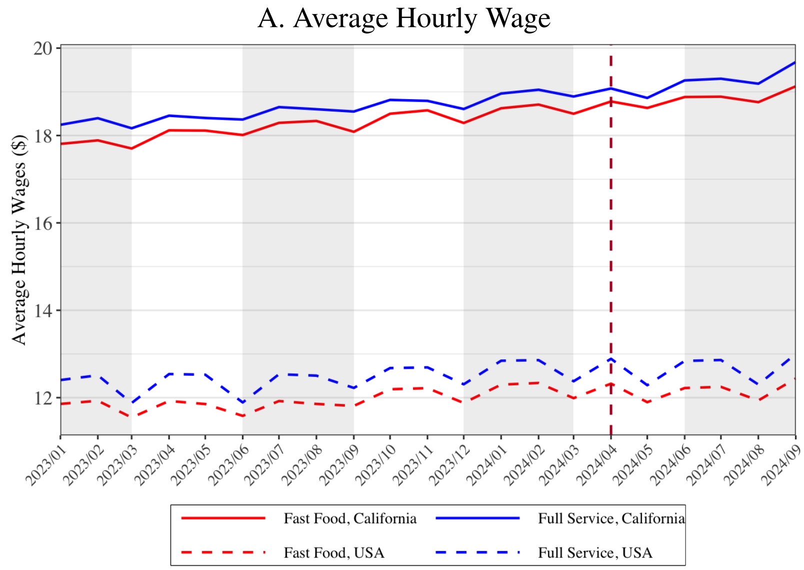


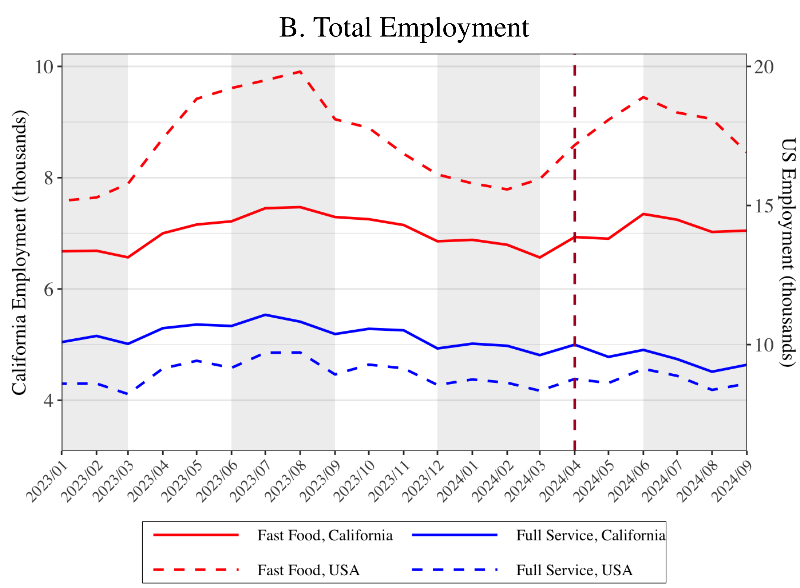
B. California and available control states



*Note:*Constructed using BLS Current Employment Statistics data (CES). "Fast-food & Other" include NAICS codes 722513, 722514, 722515. CES does not provide state-level data for the fast-food industry individually. Full-service is NAICS 722511. The vertical dashed line is the month the new policy was introduced. Shading represents quarters. In Panel A, The left y-axis shows employment in thousands of workers and ranges from 550 to 800. The right y-axis also shows employment in thousands of workers but has a different scale and a range of 4,750 to 6,250. In Panel B, control state groups include all states available in CES data with binding federal minimum wage. There are eight such states available: AL, ID, IN, NH, OK, PA, UT, WI.

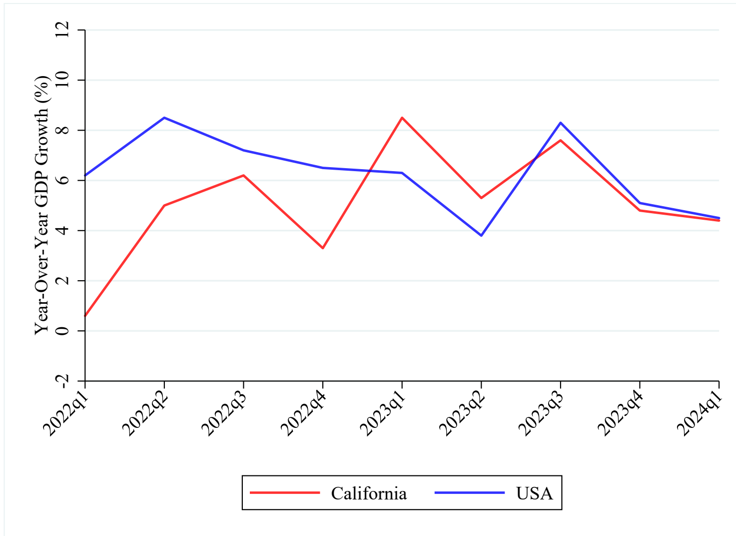
Figure 4:
Small restaurants average hourly wage and employment in California and the U.S.

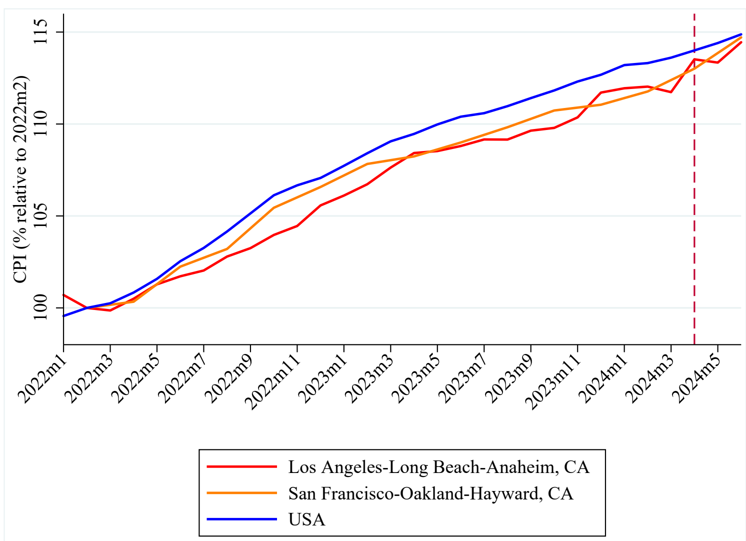




Note: Constructed using data from a popular point of sale and payroll software provider. Panel A shows monthly average hourly wage in dollars. Panel B shows employment in thousands. The left y-axis ranges from 3 to 10. The right y-axis has a different scale and a range of 9 to 20. The vertical dashed line is the month the new policy was introduced. Shading represents quarters.

B. Price Index Growth for ”Food Away From Home” by Geography





Note: Panel A is constructed using GDP data, U.S. Bureau of Economic Analysis. Panel B is constructed using BLS Consumer Price Index data. The price index is normalized to 100 in 2022m2. The vertical dashed line represents the introduction of the new policy. The vertical dashed line is the month the new policy was introduced.

Appendix Tables and Figures

Table A1
Average pre-policy fast food prices in California

	(1) Hamburger	(2) Specialty burger	(3) Combo	(4) Average of main items
A. By group				
All Chains	3.91	7.56	13.21	6.10
	(1.27)	(1.28)	(1.64)	(3.07)
Lower-price Chains	3.74	7.49	13.10	6.00
	(0.97)	(1.21)	(1.52)	(3.02)
Higher-price Chains	7.57	9.71	16.96	9.82
	(1.10)	(1.59)	(0.59)	(2.28)
B. By chain				
McDonald's	3.43	7.52	12.75	7.29
	(0.51)	(0.83)	(1.51)	(2.67)
Jack in the Box	–	6.58	13.09	5.96
		(0.75)	(1.20)	(1.64)
Carl's Jr and Hardee's	–	7.93	14.18	0.83
		(0.98)	(0.00)	(1.28)
Burger King	5.30	9.46	15.23	7.09
	(0.76)	(0.84)	(1.08)	(3.16)
Wendy's	2.91	8.12	–	2.53
	(0.54)	(0.86)		(0.57)
The Habit	7.03	9.15	16.96	10.90
	(0.21)	(0.30)	(0.59)	(1.01)
Five Guys	10.05	14.17	–	7.00
	(0.92)	(1.11)		(0.59)
Shake Shack	8.93	9.53	–	5.17
	(0.13)	(0.13)		(0.39)

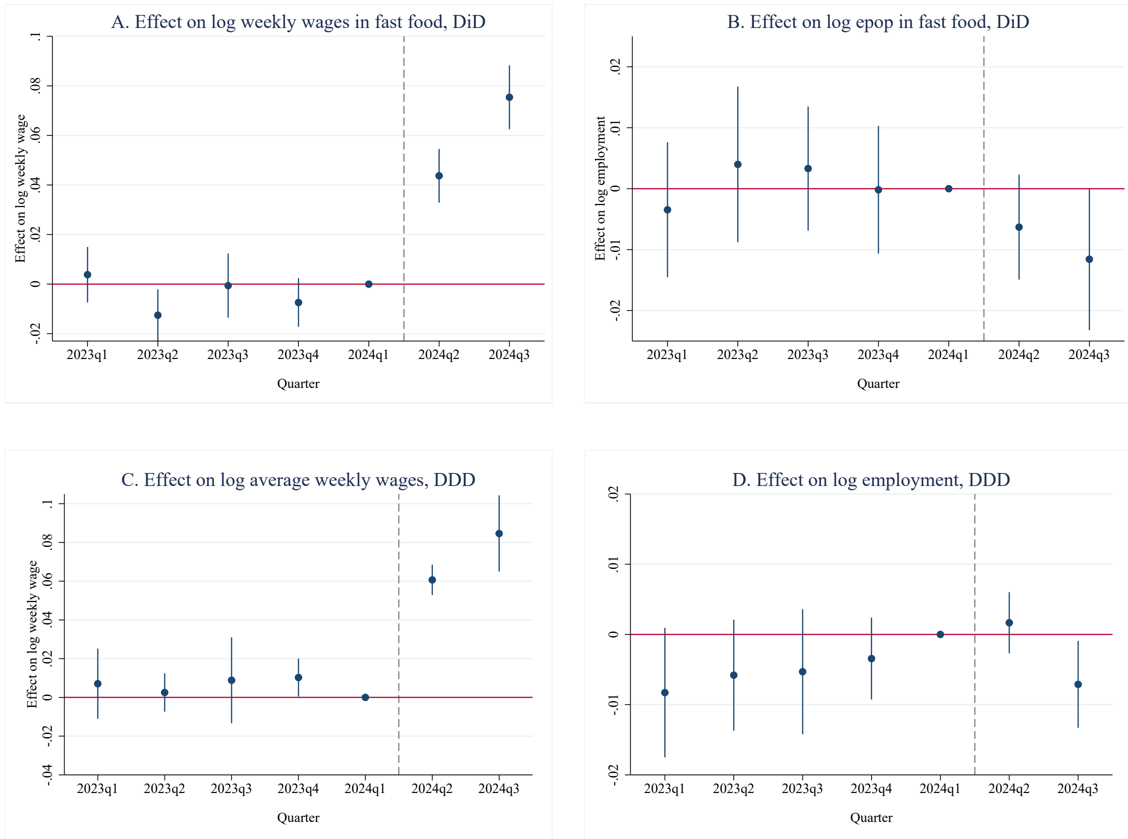
Note: This table reports average prices of selected items in the collected data by group and by fast food chain. Last column reports avergae of five items defined by authors: cheeseburger, hamburger, specialty burger, combo, and fries. Panel A is weighted by number of locations in California of each chain. All averages are reported in U.S. dollars. Standard deviations are reported in parentheses. Missing cells represent items either not included in the chain's menu or not captured by the data-collection algorithm. Items for each franchise are selected manually if no perfect match by name is found. "Higher-price chains" include The Habit, Five Guys, and Shake Shack. "Lower-price chains" include all other chains in the sample.

Table A2
Difference-in-differences log price effect by item and chain, 2024Q3

	(1) Hamburger	(2) Specialty burger	(3) Combo	(4) Average of main items
McDonald's	-0.014*	-0.012**	0.003	-0.002
	(0.007)	(0.005)	(0.005)	(0.004)
Jack in the Box	–	0.032***	0.025***	0.028***
		(0.008)	(0.005)	(0.006)
Carl's Jr and Hardee's	–	-0.091	–	-0.214***
		(0.125)		(0.033)
Burger King	0.008	-0.010	0.037***	0.008
	(0.012)	(0.011)	(0.009)	(0.010)
Wendy's	0.036***	0.023**	–	0.102***
	(0.010)	(0.010)		(0.020)
The Habit	0.046***	0.047***	0.043***	0.045***
	(0.007)	(0.006)	(0.005)	(0.005)
Five Guys	0.003	0.002	–	0.002
	(0.005)	(0.005)		(0.004)
Shake Shack	-0.022***	-0.014***	–	-0.009***
	(0.003)	(0.002)		(0.002)

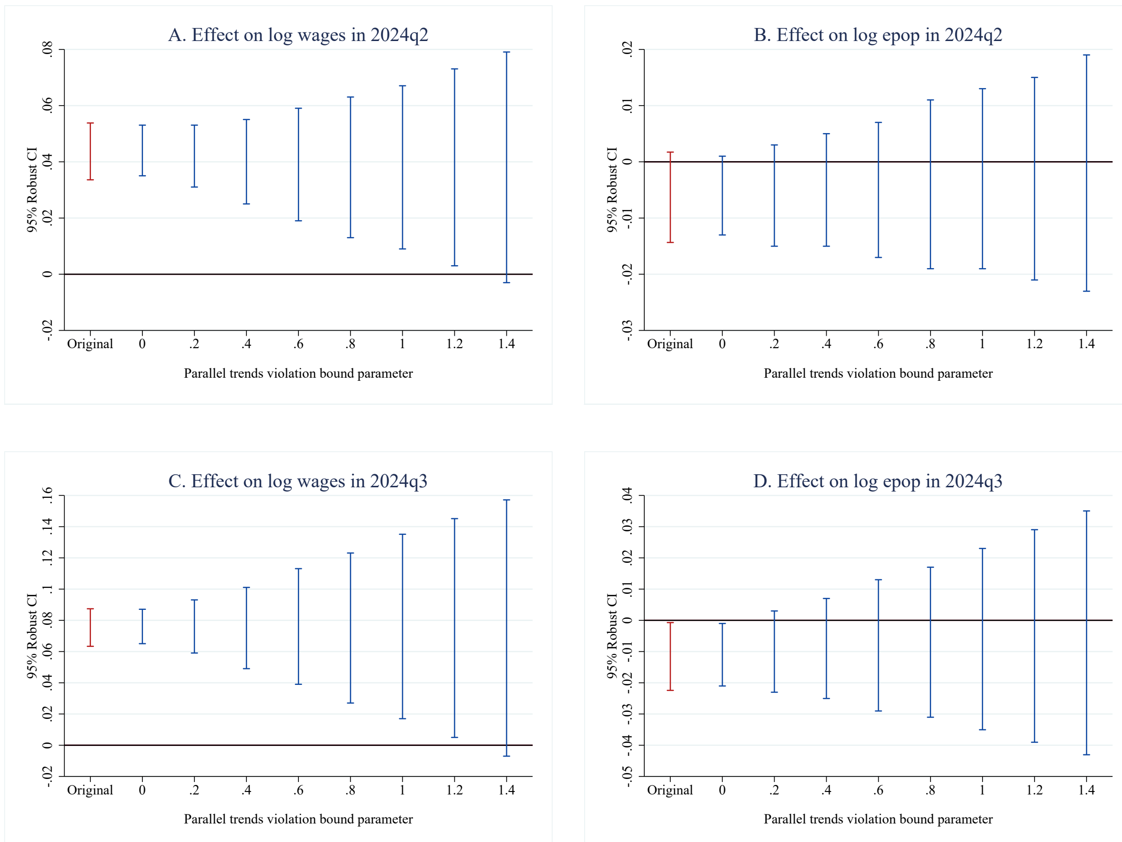
Note: Estimated using Equation 3. Each outcome is a log of the stated variable. The last row represents the average effect weighted by the number of locations in California. Missing cells represent variables not captured by the data-collection algorithm. Statistical significance is marked as follows: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Figure A1:
DiD and Triple Differences event study results using seasonally adjusted QCEW



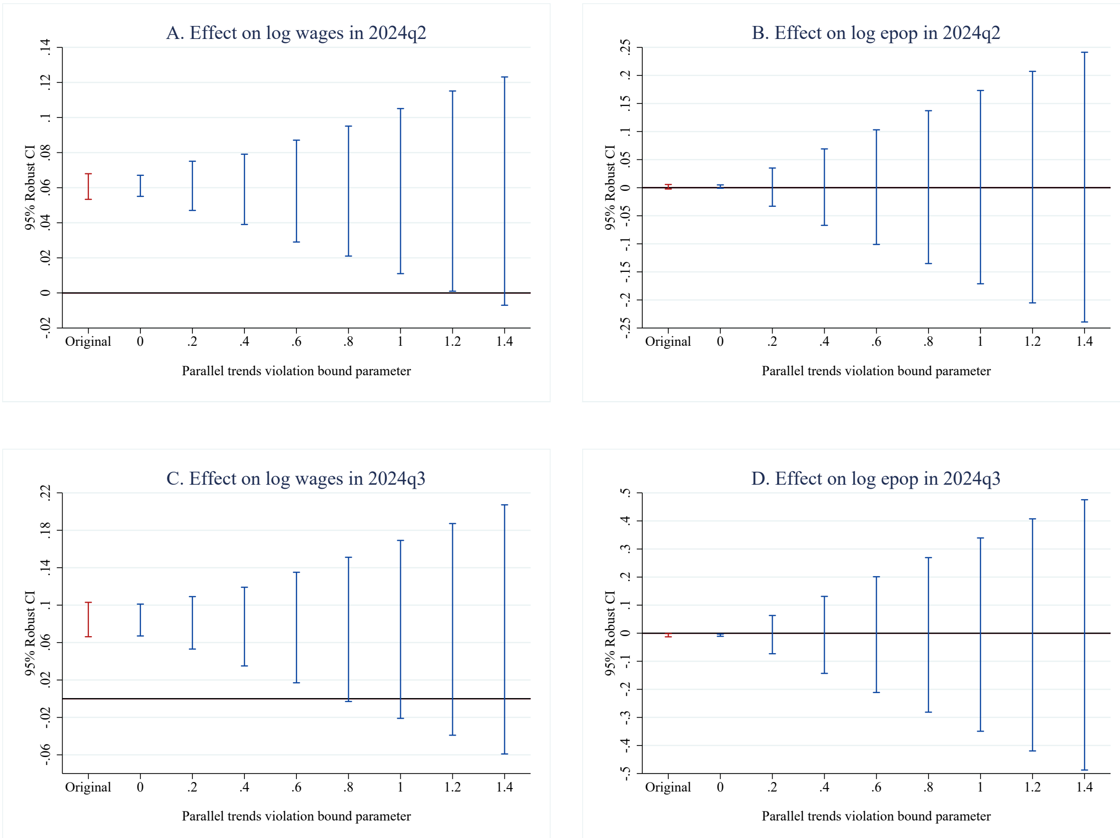
Note: Panels A and B are estimated using Equation 3, while panels C and D– using Equation 4. All regressions use seasonally adjusted QCEW data. The outcome in each panel on the left is the log of the county average weekly wage. In the right panels, the outcome is the log of the county employment divided by the working-age population. All event studies include county and quarter-fixed effects. Panels C and D additionally use time-by-industry fixed effects. All regressions use outcomes outside of the industry of interest and county population as controls. All estimates are relative to 2024q1, the last quarter before treatment. The gray dashed line represents policy implementation. Dots show estimates weighted by county population in 2010. Lines show 95% confidence intervals. Standard errors are clustered at the state level.

Figure A2:
Rambachan and Roth (2023) robust CIs for difference-in-differences using seasonally adjusted QCEW



Note: All panels are estimated using Equation 3 and Rambachan and Roth (2023) confidence interval procedure. Horizontal axis depicts parameter of choice $\bar{M}$, while the vertical axis shows the estimated effect. The red line shows the original CI, while blue lines show CIs for varying bound parameters, $\bar{M}$. All regressions use seasonally adjusted QCEW data. The outcome in each panel on the left is the log of the county average weekly wage. In the right panels, the outcome is the log of the county employment divided by the working-age population. All event studies include county and quarter-fixed effects. All regressions use outcomes outside of the industry of interest and county population as controls. All estimates are relative to 2024q1, the last quarter before treatment. Lines show 95% confidence intervals. Standard errors are clustered at the state level.

Figure A3:
Rambachan and Roth (2023) robust CIs for Triple-differences using seasonally adjusted QCEW



Note: All panels are estimated using Equation 4 and Rambachan and Roth (2023) confidence interval procedure. Horizontal axis depicts parameter of choice $\bar{M}$, while the vertical axis shows the estimated effect. The red line shows the original CI, while blue lines show CIs for varying bound parameters, $\bar{M}$. All regressions use seasonally adjusted QCEW data. The outcome in each panel on the left is the log of the county average weekly wage. In the right panels, the outcome is the log of the county employment divided by the working-age population. All event studies include county, quarter, and time-by-industry fixed effects. All regressions use outcome outside of the restaurant industry and the county population as controls. All estimates are relative to 2024q1, the last quarter before treatment. Lines show 95% confidence intervals. Standard errors are clustered at the state level.