

# Effects of a \$20 Minimum Wage: Evidence from Granular Data on Wages, Employment and Prices\*

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April 1, 2026

## Abstract

On April 1, 2024, California implemented a \$20 hourly wage floor for workers in large chains in fast food restaurants and snack and non-alcoholic beverage bars. The new standard, which corresponds to 69 percent of the state's median full-time wage, surpasses all prior benchmarks in minimum wage policies and research. We use difference-in-differences (DiD) and triple difference (DDD) event study methods and granular data that permit distinguishing trends affecting the entire industry from those caused by the new minimum wage policy. Our granular pay data come from Glassdoor job postings and Square payroll data, granular mobility-based employment data from Advan, and granular prices scraped from over 2,000 restaurants in California and control states. We find that the policy increased average weekly wages for covered fast food workers by about 11 percent and did not reduce employment. Compared to controls, prices increased by 1.5 percent, equivalent to 6 cents for a \$4 item. Employers passed about 50 percent of the higher wage costs to consumers as higher prices, consistent with a monopsony model.

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\*We are grateful to the Institute for Research on Labor and Employment at UC Berkeley for research support, to Emma-Jane Morgan Burns, Sakshi Kolli, Alyssa Nguyen and Natania Wong for excellent research assistance and Ana Fox-Hodess for careful editing. Richard Blundell, David Card, Arindrajit Dube, Patrick Kline, Attila Lindner and Aaron Sojourner and participants at the AEA 2025 meetings, the IRLE seminar, the Berkeley Labor Seminar and the Institute for Fiscal Studies-provided helpful suggestions. We thank Daniel Zhao and Ara Khazarian for assistance with Glassdoor and Square data.

# 1 Introduction

We study the economic effects of a \$20 wage floor covering workers employed in large chains in California’s fast-food restaurants and snack and nonalcoholic beverage bars.<sup>1</sup> About 750,000 workers were employed in these two industries when the policy was implemented. This standard considerably exceeds the levels in previous minimum wage research and represents one of the highest minimum wage levels in the world.

The most prominent causal minimum wage studies examine much lower wage standards (Dube and Lindner, 2024). Moreover, previous studies are not equipped to estimate policies that apply only to large fast food chains. The empirical literature on price effects does not use detailed establishment-level price data. We fill these knowledge gaps using establishment-level wage and employment data for covered and uncovered large fast food chains and a novel set of detailed online prices that we scraped from a large representative sample of fast food and full service restaurants.

When the \$20 minimum wage was enacted, restaurant owners predicted that the cost of the policy would fall on franchisees, while price increases would increase the royalty payments they would have to make to the licensing franchisors (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.”<sup>2</sup> In response, California Governor Gavin Newsom cited descriptive BLS data showing consistent growth in year-over-year fast food employment.<sup>3</sup>

Given the interest in the effects of the novel \$20 sectoral wage floor and the paucity of relevant economic studies, we provide here estimates of the policy’s causal effects on wages, employment and prices. To do so, we use, private granular pay data from Glassdoor and Square, and granular Advan mobility data. We also collect a new dataset of restaurant prices at the individual location level. We additionally use QCEW and CES data for robustness tests. Utilizing appropriate control groups and modern causal identification methods, we capture the causal effects of the policy.

Our restaurant-level wage data on covered restaurants come from over 26,000 job reports in California and control states that were posted on Glassdoor’s internet job platform, before and after the policy. We find that wages increased about 12 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 of the policy. In contrast, the distribution of hourly wages did not change for workers in the same chains who were employed outside of California, nor for California full-service restaurant workers.

We estimate that roughly 70 percent of California’s fast-food industry employees are employed in establishments that belong to a chain with at least 60 locations nationwide—

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<sup>1</sup>Snack and nonalcoholic beverage bars include chains like Dunkin Donuts, Jamba Juice, Starbucks, and TCBY.

<sup>2</sup>See [www.placer.ai/anchor/articles/](http://www.placer.ai/anchor/articles/)

<sup>3</sup>See for example, [ktla.com/news/](http://ktla.com/news/); [www.foxnews.com/opinion/](http://www.foxnews.com/opinion/)

and thus, are covered by the minimum wage policy. We deploy Square payroll data on small businesses and Advan mobility data to identify effects on employment and average weekly earnings. Using the QCEW, we estimate that the policy increased average weekly earnings of covered workers by about 11 percent between 2024q1 and 2024q3, the same increase we estimate for hourly wages. These results together imply that the policy did not change weekly hours.

Regarding employment, we observe first 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. To better identify the causal effects of the policy in its first year, we therefore deseasonalize QCEW data by industry and state. Our preferred specification employs a triple-difference method that compares the deseasonalized fast food industry in California to control states as well as to trends in the full-service restaurant industry. We also compare covered fast food chains in California to smaller fast food chains that were not covered by the policy. After accounting for a small visible pre-trend and taking into account variation in pre-trends, as suggested by Rambachan and Roth (2023), our employment estimate centers around zero and is not statistically significant. In contrast, the earnings results exhibit a strong positive effect.

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 provides an economically meaningful measure of the policy effects. Our OWE estimate using our preferred specification is -0.117, remarkably close to the median OWE of -0.09 for recent restaurant and retail studies, as reported in the Dube and Zipperer (2024) meta-study of OWE estimates.

To study the policy’s causal effects on prices, we obtain reliable data on a granular level by repeatedly scraping menu prices from the Uber Eats platform, where consumers place pick-up and home delivery orders, before and after the policy’s effective date. We sample Uber Eats for a large representative sample of restaurants on the platform. To facilitate comparisons among menu items and chains, we restrict 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 then use difference-in-differences to compare 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 deploy 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.

Prices in the fast food industry increased by approximately 3.3 percent in California compared to control states in the three quarters following the policy. However, part of the increase can be attributed to broader trends in the restaurant industry. We detect a 1.8 percent increase in full-service restaurants that did not experience wage increases after the policy. Using triple difference estimate, we find that the policy led to short-run price increases in fast-food restaurants of about 2.9 percent. Prices decreased subsequently,

relative to prices in our control groups, resulting in an average increase of 1.5 percent three quarters after the policy. This increase amounts to around 6 cents for a \$4 hamburger. The pullback from the initial price increases may have resulted from excessive expectations of price increases, which came from policy opponents, leading to declines in traffic.<sup>4</sup> To provide some context, the price index for "food away from home" increased around 4.8 percent between April 2023 and April 2024, right before the \$20 wage standard went into effect.

To contextualize the price effect, we calculate price pass-throughs, the share of higher wage costs passed on to consumer prices. Pass-throughs are likely to be greater when the low-wage share of operating costs is higher and when demand elasticity is lower. The ratio of fast food payroll to revenue in the 2017 and 2022 Economic Censuses indicate that the labor share of operating costs is 0.3, higher than in other industries; demand for fast food is highly inelastic, according to Okrent and Alston (2012).

If a \$20 minimum wage causes average wages to increase about 9.8 percent, a 1.5 percent price increase implies a 50 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 full pass-through summary estimate in Dube and Lindner (2024).

However, a lower-than-full pass-through of minimum wages to prices is more common in the literature than is widely believed. Katz and Krueger (1992) and Card and Krueger (1994) obtained inconclusive effects of minimum wages on fast food. Two other papers, Basker and Khan (2016) and Ashenfelter and Jurajda (2022), report full pass-throughs. However, both papers assume unconventional labor shares in their calculations. The wage and price effects in Ashenfelter and Jurajda (2022) therefore imply a pass-through estimate of 0.69, close to estimates in this paper, while Wiltshire et al. (2024) use the same data and find a 0.55 pass-through. Moreover the 0.3 labor share and the estimates in Aaronson (2001) imply a pass-through of 1.77, which seems highly implausible.

In combination, the estimated positive effects on earnings, null employment effects, and a lower-than-full pass-through suggest that employers were able to adjust margins other than employment to absorb their increased wage costs. The presence of monopsonistic power wielded by restaurant chains explains this result. Multiple studies have demonstrated that many employers in low-wage labor markets possess the power to set wages lower than they would 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.<sup>5</sup>

Interestingly, the sectoral wage policy might generate potential benefits for franchisors, who have licensed franchisees to operate about 80 percent of fast food restaurants. If higher

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<sup>4</sup>In addition, several chains responded by introducing \$5 Value Meals, which the Washington Post estimated were worth over \$10.

<sup>5</sup>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.

labor costs cause prices to rise, restaurant revenues (but not profits) would likely increase, as Rao and Risch (2024) found using tax data.<sup>6</sup> The revenue increase, in turn, would raise the fees the franchisees are obligated to pay to their licensing franchisors.

Public interest in the policy has generated a number of studies. Using QCEW and CES data on the industry as a whole and event study methods, an early study by Reich and Sosinskiy (2024) (revised as Reich and Sosinskiy (2025)) detected significant pay increases, but no significant employment effects. Their data did not permit distinguishing covered and uncovered segments of the industry. Using survey data collected via social media, Schneider, Harknett, and Bruey (2024) found positive earnings of the policy and no employment effects. Their earnings effects are implausibly large, casting doubt on the representativeness and validity of their data. Using administrative payroll for large fast food chains from Equifax, Hamdi and Sovich (2025) find substantial wage increases, reductions in hires, and greater reductions in quits, leading to a small net increase in employment. Equifax data covers large employers (such as Starbucks and Subway) that do not franchise their individual stores, but omits the 80 percent of the fast food sector that is franchised. Reich and Sosinskiy (2024) (revised as Reich and Sosinskiy (2025)) additionally report small early price increases as a result of the policy.

Other papers (Clemens, Edwards, and Meer (2025) and Pandit (2026)) find significant negative employment effects. In both papers the negative effects occur almost entirely between the date the policy was enacted and the date of implementation. The results reported by Clemens, Edwards, and Meer (2025) are not robust to properly controlling for differential seasonality in California and controls areas. Pandit (2026) examines effects only in California areas without local minimum wages, thereby omitting effects in about half of the fast food industry. Also, Pandit (2026) looks at effects on only the top fifty fast food chains in the U.S. Our approach examines all chains with over 60 restaurants nationwide. Two other papers (Hartley and Vakil (2025) and Clemens et al. (2026)) find large price effects during the same pre-implementation period.

The results in four of these papers suggest that employers adjusted their staffing and price levels before implementation, in anticipation of the new policy and in order to smooth out shocks. Reductions in staffing levels and increases in prices before labor costs increase would contradict profit-maximization strategies. Moreover, the results in these papers indicate significant responses immediately after enactment, which is inconsistent with a smoothing hypothesis.

As far as we know, prior minimum wage policies have always been implemented after waiting periods of similar duration. Yet prior minimum wage studies have not detected anticipation effects. Indeed, employment and price changes prior to implementation could have resulted from other causes, such as different seasonal variation in California fast food relative to control groups, and changes in national immigration policy that may have

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

particularly affected California, reducing demand for fast food and increasing the price of food purchased by fast food restaurants. Our methodological approach and data allows us to control for such confounds. We do not find anticipation effects.

We discuss the policy’s background and coverage and our data in Section 2 and present our estimating methods in Section 3. We then discuss our findings on pay in Section 4, employment in Section 5 and price increases and pass-throughs in Section 6. Section 7 concludes. Online Appendix A reconciles our findings with those in Clemens, Edwards, and Meer (2025). We find their methods impart a spurious negative bias to their employment estimates.

## 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 nonalcoholic beverage bars, effective April 1, 2024. These two industries employed about 750,000 California workers in early 2024. To mitigate possible adverse 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 percent increase over the statewide minimum wage. It is also the highest minimum wage in the U.S. and higher in purchasing power parity terms than any minimum wages in Europe.<sup>7</sup> About 90 percent of non-managerial workers in the two industries were paid less than \$20 before the policy.<sup>8</sup> 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 \$4 overnight minimum wage increase was unprecedented in magnitude. However, the 25 percent overnight increase in the fast-food minimum wage was not. San Francisco’s citywide minimum wage, implemented in 2004, constituted 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

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<sup>7</sup>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.

<sup>8</sup>Estimated from percentile wages for fast food occupations reported by the BLS’ Occupational and Employment Wage Survey, May 2023, California.

and 69 percent of all full-time California workers. Both ratios considerably exceed those for any European country (OECD Stats) 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 AB 257. This earlier law, passed in September 2022, had established a Fast Food Council with the power to set a fast food minimum wage as high as \$22 and to establish industry-wide mandatory standards for working conditions (Egelko, 2023). The law also required annual cost-of-living adjustments.

Following the passage of AB 257, the International Franchise Association successfully circulated a petition to place an initiative repealing the law on the November 2024 ballot. In response, in January 2023 a state court stayed the implementation of AB 257, pending the outcome of the vote on the repeal ballot measure. Negotiations then proceeded behind closed doors. A new agreement replacing AB 257 was announced on September 11, 2023, along with the withdrawal of the repeal initiative. The legislature passed AB 1228 on September 23, 2023, and the governor signed it on September 28, 2023.<sup>9</sup>

AB 1228 limited the initial fast food minimum wage to \$20, subject to annual cost-of-living increases, and eliminated the power of the Fast Food Council to set standards for working conditions. The new law instead empowered the council only to recommend such standards to the state legislature.<sup>10</sup>

The establishment of the California Fast Food Council and the creation of an industry-wide wage standard represent a further evolution of minimum wage policy in the U.S.: the creation of sectoral wage standards. Earlier sectoral policies had established living wage standards covering all workers at multiple airports in California, New York, and other states and industry-level wage standards for segments of the hotel industry in Emeryville, CA and Los Angeles. Following AB 1228, California enacted a \$25 minimum wage for the California health care industry and Los Angeles enacted a \$30 minimum wage for its tourism sector. In addition, Minnesota has established a \$27 wage standard for nursing home workers.

The Fast Food Council consists of nine members, two representatives of industry employer associations, two representatives of fast food restaurant owners, two representatives of labor organizations, two representatives of fast food workers and a chair, from state government. The members are all chosen by the governor. In its first two years, the Council held several meetings with stakeholders and deliberated whether to pass a cost-of-living increase in the wage standard for the covered employers. The chair of the council resigned in mid-2025; the Council has not met since, nor has it determined whether to enact the cost-of-living increase.

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<sup>9</sup>[leginfo.legislature.ca.gov/faces/](http://leginfo.legislature.ca.gov/faces/)

<sup>10</sup>At various stages, AB 1228 also specified that fast food franchisors were joint employers, along with the franchisees. This feature did not appear in the final bill.

## 2.2 The policy’s coverage

We first estimate the number of restaurants that are covered. As we have noted, fast food workers in small chains and in independent restaurants are not covered by the higher minimum wage. In 2024, 38,519 fast food restaurants were located in California.<sup>11</sup> 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.<sup>12</sup> 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.

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 restaurant employs on average 15.4 employees.<sup>13</sup> However, chain restaurants must meet minimum sales and employee thresholds established by the chains, while independents do not. The average number of workers at each chain restaurant therefore is likely to be larger than the average among independents. If, for example, 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 Proprietary wage data

We use wage data from Glassdoor, a job-posting site, and Square, which provides payroll services to small firms, as well as 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, it also identifies employer names and locations, 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.<sup>14</sup> Glassdoor data has been previously analyzed by a few economic papers, including Karabarounis 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

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<sup>11</sup>[www.ibisworld.com/us/industry/california/](http://www.ibisworld.com/us/industry/california/)

<sup>12</sup>[datessential.com/sales-intelligence/](http://datessential.com/sales-intelligence/)

<sup>13</sup>[www.ibisworld.com/](http://www.ibisworld.com/)

<sup>14</sup>We can distinguish posted pay for a previous job from pay on a current job.

small to be covered by the fast food minimum wage. They therefore provide a useful control group.

## 2.4 Administrative employment and wage data

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.<sup>15</sup> Importantly, the CES results reveal higher employment seasonality in 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.<sup>16</sup>) 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 2022-2024 as in 2017-2019. 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).<sup>17</sup>

## 2.5 Mobility data

Administrative employment data cannot differentiate between large chains covered by the new law and smaller chains or independent fast food restaurants that are not. We therefore additionally utilize mobility data that tracking locations of opted-in users' smartphones: the "Foot Traffic / Weekly Patterns" data provided by ADVAN Research.<sup>18</sup> The data covers the universe of establishments in the U.S. and reports the number of weekly visits to each location. Importantly, visits are disaggregated by "dwell" time, including visits longer

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<sup>15</sup>The CES surveys 119,000 businesses and government agencies, representing approximately 629,000 worksites throughout the U.S.

<sup>16</sup>The QCEW is commonly used to study the effects of minimum wages; examples include Dube, Lester, and Reich (2010), Godoy and Reich (2021), and Wiltshire et al. (2024)

<sup>17</sup>Available data do not permit studying minimum wage effects on average hours in fast food. However, our comparison of hourly and weekly wage effects provides suggestive information on hours effects. Allegretto, Dube, and Reich (2011) found that minimum wages did not have negative effects on hours worked by teens.

<sup>18</sup>This data was previously distributed and used by researchers as Safegraph Weekly Patterns.

than 240 minutes.<sup>19</sup> Since spending four hours as a customer in a fast food restaurant is highly unlikely, we use the number of visits longer than 240 minutes as a proxy for establishment-level employment.<sup>20</sup>

The Advan data includes restaurant names and their "brand." We can therefore identify restaurants that are part of a chain with 60 or more locations nationwide and therefore covered by the new minimum wage. Conversely, we can also identify independent restaurants and those that are part of smaller chains.

The data represents a sample of smartphone owners who have opted in to report their location. The sample might therefore not be representative of all cell phone users. In a difference-in-difference framework, however, we can obtain credible estimates of the effects of the minimum wage if coverage is constant within treatment and control groups. We find that coverage remains consistent starting in January 2023, thereby allowing us to observe a sufficient number of pre-policy periods. Moreover, Pandit (2026) finds that visits over 240 minutes are strongly correlated (0.97 or more) with QWI county-level employment numbers for our period of interest. Advan data thus allows us to obtain valid estimates of employment effects in covered chains.

## 2.6 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.<sup>21</sup> 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

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<sup>19</sup>Available bins are less than 5 minutes, 5-20 minutes, 21-60 minutes, 61-240 minutes and more than 240 minutes.

<sup>20</sup>Strictly speaking, the visits data provide counts of the number of employee-shifts, since we do not observe individual workers in the data.

<sup>21</sup>The In-N-Out Burger chain does not participate in food delivery.

chains, as measured by estimated market share and number of employees.<sup>22</sup>

For the cheeseburger, hamburger, fries and combo menu items, we first searched for a perfect text match 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 dish, and a drink. We identify each chain’s specialty item using its marketing materials and media. We also verify that the specialty items are comparable across 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.<sup>23</sup> Table 2 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

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<sup>22</sup>[companiesmarketcap.com/restaurant-chains/...](https://companiesmarketcap.com/restaurant-chains/)

<sup>23</sup>[www.qsrmagazine.com/](https://www.qsrmagazine.com/); [www.businessinsider.com/](https://www.businessinsider.com/)

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 service 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 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.<sup>24</sup> 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 difference methods throughout the paper. As described above, our three main data sources are defined at different levels. Thus, we modify our methods slightly to accommodate 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:

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

where  $Y_{ifs,t}$  is hourly pay for employer  $i$ , working in franchise  $f$  in a state  $s$  at event time  $t$ .  $\alpha_s$  and  $\tau_t$  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,  $\beta_t$  is a coefficient of interest representing a causal effect of the \$20 minimum wage  $t$  quarters after the implementation.

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<sup>24</sup>Massimo (2021) reports that prices are higher on Uber Eats than in restaurants.

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.

### 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 quarterly QCEW data for years 2022-2024. The model can be captured by the following equation:

$$\ln(Y_{i,t}) = \alpha_i + \tau_t + \sum_{k=-8}^3 \beta_k \times CA_i \times I\{t = k\} + \gamma X_{i,t} + \varepsilon_{i,t} \quad (2)$$

where  $\ln(Y_{c,t})$  is the natural log of an outcome of interest for county  $i$  at event time  $t$ . Outcomes of interest are the employment rate and average weekly earnings.  $\alpha_i$  and  $\tau_t$  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 to 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, the working-age population, and state GSP growth. Results are also robust to using yearly or quarterly county unemployment rates. Finally,  $\beta_t$  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 Figure 6 shows, California is less affected by seasonality than is the U.S. and our set of control states<sup>25</sup> 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 and subsequent recovery.<sup>26</sup>

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.

<sup>25</sup>In panel B, we include all control states for which CES data is available.

<sup>26</sup>The results are also robust to performing seasonal adjustment monthly, using earlier years (starting in 2014) and excluding 2023-2024.

Since previous studies find that minimum wage effects occur within nine months of a policy event, we estimate effects using three post-policy quarters.

Importantly, standard cluster-robust inference may be insufficiently conservative in our setting, as we have only one treatment state (cluster), California, and only one treatment event. Thus, traditional standard errors may not account for potential residual seasonality that we do not fully control, or for uncontrolled differences in demand for fast food between treatment and control states. We therefore employ the confidence interval estimating method developed by Rambachan and Roth (2023).<sup>27</sup> Their procedure allows us to relax the parallel-trends assumption and impose instead restrictions on the difference between post-treatment violations of parallel trends and pre-trends violations. Our preferred inference imposes the following assumption: post-treatment period-to-period trend deviation between treatment and control groups are at most as large as the largest pre-treatment deviation since 2023. That is, we employ Rambachan and Roth (2023) with  $\bar{M} = 1$  and using pre-periods starting in 2023.

### 3.3 Triple difference event study

To relax some assumptions of the difference-in-differences event study 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.

$$P_{ic,t} = \alpha_{ic} + \tau_t + \lambda_c + \sum_{k=-8}^3 \beta_k \times CA_i \times FastFood_c \times I\{k = t\} + \varepsilon_{ic,t} \quad (3)$$

The model builds on 4, adding a fast food fixed effect,  $\lambda_c$ , 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 and 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.<sup>28</sup> Similarly to the DiD, we apply the Rambachan and Roth (2023) inference procedure to triple difference estimates to provide an inference procedure with a more robust identification assumption.

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<sup>27</sup>Sometimes referred to as "honest DiD."

<sup>28</sup>Olden and Møen (2022) provides an in-depth discussion of the triple difference estimator.

### 3.4 Event study for employment using foot traffic data

We use a similar methodology to assess the effects on employment using Advan foot traffic data for the years 2023 to 2025. The model can be captured by the following equation:

$$\ln(Y_{is,t}) = \alpha_i + \tau_t + \sum_{k=-4}^5 \beta_k \times Treat_i \times I\{t = k\} + \gamma X_{s,t} + \varepsilon_{is,t} \quad (4)$$

where  $\ln(Y_{c,t})$  is the natural log of visits over 240 minutes in restaurant  $i$  in state  $s$  at event time  $t$ .  $Treat_i$  is an identifier equal to one if the establishment is subject to the policy (i.e., in California and is part of a chain with 60 or more locations nationwide).  $I\{t = k\}$  denotes an indicator equal to one for the relevant period.  $\varepsilon_{is,t}$  is a random error.  $X_{s,t}$  is a set of state-period-specific controls. Our controls include population and a seasonal adjustment factor obtained from QCEW.  $\beta_t$  is a coefficient of interest representing the causal effect of the \$20 minimum wage  $t$  quarters after implementation. We use the quarter before the policy as our reference period  $t = 0$ . Each consecutive  $t$  represents a one-quarter shift in time. We cluster standard errors by establishment level and weight results by state population from the 2010 Census.

### 3.5 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:

$$P_{ic,t} = \alpha_{ic} + \tau_t + \sum_{k=1,3} \beta_k \times CA_i \times I\{t = k\} + \varepsilon_{ic,t} \quad (5)$$

where  $P_{ic,t}$  is the price of an item in restaurant  $i$  of chain  $c$  at event time  $t$ .  $\alpha_{ic}$  and  $\tau_t$  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,  $\beta_t$  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.

### 3.6 Triple difference model for prices

Similarly to equation 3 above, we estimate price effects using the triple difference-in-differences specification described in equation 5:

$$P_{ic,t} = \alpha_{ic} + \tau_t + \lambda_c + \sum_{k=1,3} \beta_k \times CA_i \times FastFood_c \times I\{k = t\} + \varepsilon_{ic,t} \quad (6)$$

This specification builds on equation 5, adding an industry fixed effect.

## 4 Effects on pay

We present here our wage estimates, first using data for 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.<sup>29</sup> 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.

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

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<sup>29</sup>We are grateful to Glassdoor for sharing their data with us. Our data excludes temporary employment and managerial positions.

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.<sup>30</sup>

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 the new minimum wage was \$16.16.

Panel B of Figure 1 presents pre- and post-policy wage distributions for the same restaurant chains that make up Panel A, but are located in our control group of states-

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<sup>30</sup>We also inspected, but do not report here, pay trends in 2023; the pre-trends are parallel.

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. 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 3 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 the absence of spillover effects: pay and employment did not change in the small restaurants that were not covered by 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. Panels A and C of Figure 4 present results for average weekly wages using DiD and DDD specifications. The pre-treatment quarter estimates are small and most are statistically indistinguishable from zero in both specifications, indicating parallel pre-trends. Earnings increase immediately after the implementation of the policy. Columns (1) and (4) of Table 1 present comparable results. We find an increase of 6.1 to 7.3 percent in the third quarter after implementation. An increase of 7.3 percent in weekly earnings constitutes an increase of about \$40, from a pre-period average of \$535.

### 4.4 Reconciling the wage results

Our Glassdoor data finds a 10.1 percent wage effect in the second post-treatment quarter, while our preferred QCEW wage effect is 8.0 percent in the same quarter and 7.3 one quarter later. 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 QCEW 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 11.1 percent for covered workers two quarters after implementation, remarkably close to the Glassdoor estimate in the same quarter. In the last evaluated quarter, the QCEW effect rescales to 10.2 percent.

## 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 6. The data come from the BLS monthly Current Establishment Survey (CES).<sup>31</sup> 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 B 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

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<sup>31</sup>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.

on the same graph, we use the left vertical axis to measure California employment and the right vertical axis to measure U.S. employment.

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 6 and accounting for seasonality, there is seemingly no effect on employment in the full service industry. This result 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 estimates

We discuss here our comprehensive causal estimates of employment effects, 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 1 and in Panels B and D of Figure 4. The DiD coefficient in the first post-policy quarter is -0.8 percent, but statistically indistinguishable from zero using the Rambachan and Roth (2023) procedure. In the second and third post-policy quarter, the effect is -1.1 and -1.2 percent, respectively. Both confidence intervals include zero suggesting that coefficients are not statistically different from zero. The triple-difference specification coefficients are 0.1 and -0.9 and -0.9 percent in each quarter, respectively. Similar to DiD, all coefficients are indistinguishable from zero. Overall, the QCEW estimates suggest an employment effect that is statistically and economically not meaningful.

Taking into account our wage estimates and applying the Delta method, we estimate the own-wage elasticity (OWE) between -0.198 and 0.013, depending on the period. Confidence intervals for all OWEs include zero so are indistinguishable from zero. All in all, a small negative to zero OWE is consistent with the literature (see Dube and Zipperer (2024)).

The confidence intervals in Table 1 use an "honest DID" procedure developed by Rambachan and Roth (2023). The standard parallel trends test first examines whether the observed pre-trends are parallel. If they are, the standard approach makes an implausible

assumption: that the hypothetical post-treatment trends are also parallel. Rambachan and Roth relax this strict assumption, allowing hypothetical trends to differ from each other, based on observed pre-trends and a specified 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 same pre-policy and post-policy differences in trends between the treatment and control group (that are unrelated to the policy) .

To assess the robustness of these results, we conduct the sensitivity analysis suggested in Rambachan and Roth (2023), which allows  $\bar{M}$  to vary below and above 1. We present these results for DiD in Figure A1. 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, C, and E show that the effect on log wages is significant at every level of  $\bar{M}$  up to 1. Other than in the last quarter of the data, the wage effect is significant at the conservative parameters  $\bar{M}$  of 1.2-1.4. These results suggest that we can reject a null hypothesis for the first two quarters, provided that the post-policy trends are not more than 1.4 times the worst pre-treatment violation of parallel trends. This worst-case scenario, however, is implausible. On the other hand, at realistic values of  $\bar{M} = 1$  and less, the wage effect is statistically different from zero in every period.

The effect on log employment in Panel B implies a statistically significant effect in 2024q2 at the strictest assumption of parallel trends. The effect becomes insignificant only at the relatively low bound parameter of 0.6. For 2024q3 and 2024q4, the employment effect is statistically insignificant at any  $\bar{M} \geq 0.4$ . This result suggests that the employment effect is indistinguishable from zero as long as the post-treatment violation of parallel trends is at least 0.4 times the worst pre-treatment violation of parallel trends, starting in 2023. For example, if the largest deviation from parallel trends prior to treatment is 1 percent, the employment effect is significant as long as the post-treatment trend is less than or equal to 0.4 percent.

Figure A2 presents sensitivity tests for the triple-difference specification. The results are similar to those for DiD. As expected, for each given  $\bar{M}$ , the triple-difference confidence interval is wider than its DiD counterpart. Nonetheless, the wage effect is statistically significant for  $\bar{M}$  up to 1.4 in every quarter. In contrast, the employment effect is not statistically different from zero in 2024q2, even at the most strict assumption; and it is insignificant for  $\bar{M} = 0.8$  and  $\bar{M} = 0.6$ , respectively, in the following two quarters.

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 also consistent with the presence of monopsony power (see Manning (2021a)). A minimum wage increase enables firms to hire and retain 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, a \$20 minimum wage is still below the marginal revenue product of

labor in California’s fast food industry.

### 5.3 Sensitivity to methodological choices

Our employment findings contrast sharply with a recent paper by Clemens, Edwards, and Meer (2025) (hereafter CEM), which also uses QCEW data and event study methods to evaluate the employment effect of California’s \$20 fast food minimum wage. CEM’s main finding suggests a statistically significant and negative 3.2 percent employment effect, while our paper finds a smaller and statistically insignificant effect of -1.2 percent. Both papers use DiD and DDD event study approaches. However, the two papers make different detailed methodological choices. Appendix A discusses the detailed methodological differences that underlie the two sets of results and conducts robustness tests of their results and ours. We summarize that discussion here.

First, CEM do not control for California’s slower population growth, which adversely affected employment and demand for fast food and full-service restaurants. Second, they use noisier monthly data. Third, they choose a nonstandard reference period—the date the policy was enacted, which preceded its implementation by five months. Half of their detected employment decline occurred in five months prior to the implementation date. When we use less noisy quarterly data and control for population and GDP changes, we do not detect any significant employment effects in the five months between CEM’s reference date and the actual implementation date. Finally, CEM’s DDD specification does not adequately account for confounding changes that affected related industries, such as full service restaurants.

CEM’s methodological choices impart substantial negative biases to their employment estimates, while ours are free of such biases. Moreover, our “honest DiD” inference method—following Rambachan and Roth (2023), is robust to relaxing the implausible assumption of perfectly parallel trends. We therefore stand by our employment result: the \$20 minimum wage did not significantly affect fast food employment.

### 5.4 Effects on employment in covered chains

Next, we discuss estimates of employment effects in covered chains using ADVAN traffic data. This analyses allow to assess the employment effects in restaurants directly affected by the policy. We present results in Figure 5 with four alternative specifications.

First, in Panel A we present a difference-in-differences event study comparing covered fast food restaurants in California to uncovered (small) fast food restaurants in California. The estimates suggest parallel pre-trends between these two groups during the year before the policy. Employment changes in the two groups were then similar for the first three quarters after the policy. Beginning in 2025q1, employment rose faster in covered chains, suggesting an estimated 8 percent increase in the first two quarters of 2025. If the policy created spillover wage effects on small fast food restaurant chains in California, we would

be overestimating its direct effect on covered chains. However, our estimated wage effects using Glassdoor and Square data do not detect any spillover effects.<sup>32</sup>

In Panel B, we present a similar difference-in-difference event study with a broader control group. In this specification, the control group consists of non-covered fast food restaurants in California and all fast food restaurants in control states. These results further guard against bias from potential spillover effects. The two groups trended very similarly prior to the policy. In the first post-policy quarter, employment stayed the same in two groups. The negative estimates in the next two quarters suggest that employment grew somewhat slower in affected chains. However, the estimates converge to zero in 2025, suggesting no overall employment effect.

Next, in the bottom two panels of Figure 5 we present triple-difference specifications. Both estimates compare fast food to full service restaurants in California and in control states, but Panel D includes only large chains (more than 60 locations). Panel D thus estimates the effect on directly affected fast food chains by comparing them to large full service chains in California and control states. Results in both panels suggest an employment effect indistinguishable from zero. While in Panel C the estimates are small in magnitude, estimates in the Panel D are large and positive. However, these estimates are much noisier and exhibit a positive pre-trend in the last pre-policy period.

Overall, the results using ADVAN traffic data suggest no negative employment effect of the \$20 minimum wage. There is also suggestive evidence of a positive effect in large chains directly affected by the policy.

## 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 chains are arrayed according to the number of stores in California. The top three rows display menu item price averages across all nine chains, among five lower-price chains and four 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 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.<sup>33</sup> 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. This pattern suggests that franchisees possess

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<sup>32</sup>Using Experian payroll data, Hamdi and Sovich (2025) also do not detect any spillover effects.

<sup>33</sup>Prices may be lower because of scale economies in operations, and/or because they target a different segment of fast-food consumers.

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 7 compares recent restaurant price index changes in two major California metros with recent restaurant price changes in the U.S. as a whole. These price indices come from the large surveys that BLS conducts to construct the monthly Consumer Price Index.<sup>34</sup> In the twelve months before the sectoral wage policy, 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.

Table 3 reports our difference-in-differences estimates of price changes for fast food and full service restaurants. These results, multiplied by 100, can be interpreted as percent changes. On average, fast food prices increased 3.5 to 5.3 percent, depending on the menu item, one quarter after the policy was implemented. Price changes were more prominent for higher-price chains.

The price effects are smaller two quarters after the policy than after one quarter. For hamburgers, the cumulative price effect after two quarters is 0.8 percent and is only marginally statistically significant. Specialty burgers and combo prices increased on average by 1.6 percent and 1.4 percent, respectively. On average, prices increased 2.0 percent. Similarly to the one-quarter results, prices increased more as a result of the policy for higher-price chains. Cumulative price increases in quarter four of 2024, three quarters after policy implementation, averaged 3.6 percent.

Finally, averaged over the three post-policy quarters, fast food prices increased approximately 3.3 in California fast food chains, compared to price changes in the control states.

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, our difference-in-difference estimates for fast food do not control for common shocks, such as changes in the prices of other inputs, that may have affected the broader industry.

As the bottom panel of Table 3 shows, prices for the average of main items in our full-service restaurants increased 1.5 after the first quarter and 1.6 percent after two quarters. By contrast, in the third quarter, prices increased by 2.5 percent. Notably, hamburger prices increased 15.2 percent, suggesting a potential common shock to input prices around the time of the policy. This shock suggests that a triple-difference approach might constitute a preferred method of estimating price effects in fast food.

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<sup>34</sup>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.

We therefore turn in Table 4 to our triple-difference results. 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 the average of main items are 2.9, 0.4, and 1.1 percent in the first, second and third quarters, respectively. The average post-policy price increase in full service restaurants is 1.8 percent.<sup>35</sup>

Hence, our findings suggest that some of the price increases in fast food resulted from broader trends in the restaurant industry. After taking these into account, the fast food price increase in 2024 attributable to the minimum wage policy is 1.5 percent.

### 6.3 Cost pass-through to prices

We consider here the extent to which restaurants passed on increases in labor costs to consumers in higher prices-or absorbed the costs as reduced profits. According to recent Economic Censuses, labor costs represent about 30 percent of a fast food restaurant's operating costs. An average post-policy increase in wages of 8.75 percent could therefore be fully absorbed by a price increase of 2.63 ( $8.75 \times 0.3$ ) percent, without any reduction in profits. Our difference-in-difference estimated price increase of 3.6 percent thus indicates that about 137 percent of the cost increases were borne by consumers. However, our preferred triple-difference estimates, which control for common shocks in the food industry, suggest a pass-through of about 50 percent ( $1.47/9.81 \times 0.3$ ).

A pass-through of about 50 percent of increased payroll to higher consumer prices is consistent with recent studies (Ashenfelter and Jurajda 2021; Wiltshire et al. forthcoming) that use more granular price data than earlier papers, which reported approximately full pass-throughs (Dube and Lindner 2024).

The price increases probably translated into higher restaurant revenues, given the highly inelastic demand for fast food (Okrent and Alston, 2012). Such higher revenues hold implications for franchisee payments to franchisors. 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 paid greater fees to their parent companies, even as their own profits were reduced.

## 7 Conclusions

On April 1, 2024, California established a \$20 sectoral wage standard for fast food and related workers in large chains, covering about 500,000 workers in the state. The statewide minimum wage for all other workers remained \$16. At the time, the \$20 standard was

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<sup>35</sup>As with the previous results, prices in higher-price chains are affected more on average in both studied periods. Their average price increases are 5.1, 2.2, and 3.1 percent, respectively.

higher than any other minimum wage in the world. Prior studies examine minimum wages up to only \$12; only one examines the effects of \$15 minimum wages.

To study the policy, we use novel Glassdoor wage data for fastfood and full service restaurants in California and in a control group of states without a minimum wage increase since 2009, ADVAN establishment-level data on every restaurant in the U.S., 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 wage policy, we deploy difference-in-differences event study methods that control for policy coverage, 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 11 percent. Nonetheless, the policy did not reduce employment. It did increase fast food prices by about 1.5 percent, or about 6 cents for a \$4 item, implying a price pass-through of 0.5 and a reduction in restaurant profits. Restaurant revenues likely increased; the royalty fees that restaurant operators pay to franchisors therefore probably 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) Wage	(2) EPop	(3) OWE	(4) Wage	(5) EPop	(6) OWE
2024q2	0.051 [0.031, 0.073]	-0.008 [-0.017, 0.005]	-0.155 [-0.566, 0.256]	0.059 [0.039, 0.081]	0.001 [-0.005, 0.007]	0.013 [-0.345, 0.371]
2024q3	0.077 [0.041, 0.127]	-0.011 [-0.031, 0.013]	-0.142 [-0.700, 0.416]	0.080 [0.037, 0.127]	-0.009 [-0.023, 0.003]	-0.116 [-0.682, 0.450]
2024q4	0.061 [0.003, 0.127]	-0.012 [-0.043, 0.023]	-0.198 [-1.217, 0.821]	0.073 [0.021, 0.135]	-0.009 [-0.029, 0.009]	-0.117 [-0.903, 0.669]
N	1,764	1,764		3,480	3,480	
County FE	X	X		X	X	
Quarter FE	X	X		X	X	
Industry-qtr FE				X	X	

*Note:* Columns (1)-(2) are estimated using Equation 5, while columns (4)-(5) using Equation 6. 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
<b>Fast-Food</b>				
McDonald's	1,221	12,308	224	307
Jack in the Box	942	1,251	201	105
Carl's Jr and Hardee's	647	1,990	96	6
Burger King	534	6,193	163	159
Wendy's	297	5,711	133	139
The Habit	258	109	132	10
Five Guys	123	1,377	95	117
Sonic	82	3,430	33	111
Shake Shack	60	290	36	53
Total			1,113	1,007
<b>Full-Service</b>				
Denny's	358	996	137	128
Applebee's	106	1,430	72	116
Buffalo Wild Wings	99	1,199	69	128
Red Robin	57	438	47	70
Outback Steakhouse	44	632	36	116
Total			361	558

*Note:* Table depicts a number of restaurants by chain in California and other states. Columns 1-2 show the overall number of locations. 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 or, if not available, the best approximation from other resources is used.

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

	(1) Hamburger	(2) Specialty Burger	(3) Combo	(4) Average
<i>A. All fast-food</i>				
2024Q2	0.053*** (0.002)	0.043*** (0.003)	0.035*** (0.002)	0.044*** (0.003)
2024Q3	0.008* (0.004)	0.016*** (0.002)	0.014*** (0.004)	0.020*** (0.002)
2024Q4	0.034*** (0.007)	0.031*** (0.004)	0.034*** (0.007)	0.036*** (0.004)
<i>B. Lower-price chains</i>				
2024Q2	0.053*** (0.002)	0.041*** (0.003)	0.035*** (0.002)	0.042*** (0.003)
2024Q3	0.006 (0.005)	0.014*** (0.003)	0.014*** (0.004)	0.019*** (0.002)
2024Q4	0.033*** (0.007)	0.029*** (0.004)	0.034*** (0.007)	0.035*** (0.004)
<i>C. Higher-price chains</i>				
2024Q2	0.057*** (0.003)	0.065*** (0.002)	–	0.066*** (0.002)
2024Q3	0.039*** (0.002)	0.038*** (0.002)	–	0.038*** (0.002)
2024Q4	0.061*** (0.010)	0.052*** (0.004)	–	0.056*** (0.004)
<i>D. Full service</i>				
2024Q2	-0.070*** (0.007)	0.016*** (0.001)	–	0.015*** (0.002)
2024Q3	0.024*** (0.004)	0.018*** (0.002)	–	0.016*** (0.001)
2024Q4	0.152*** (0.005)	0.035*** (0.003)	–	0.025*** (0.005)

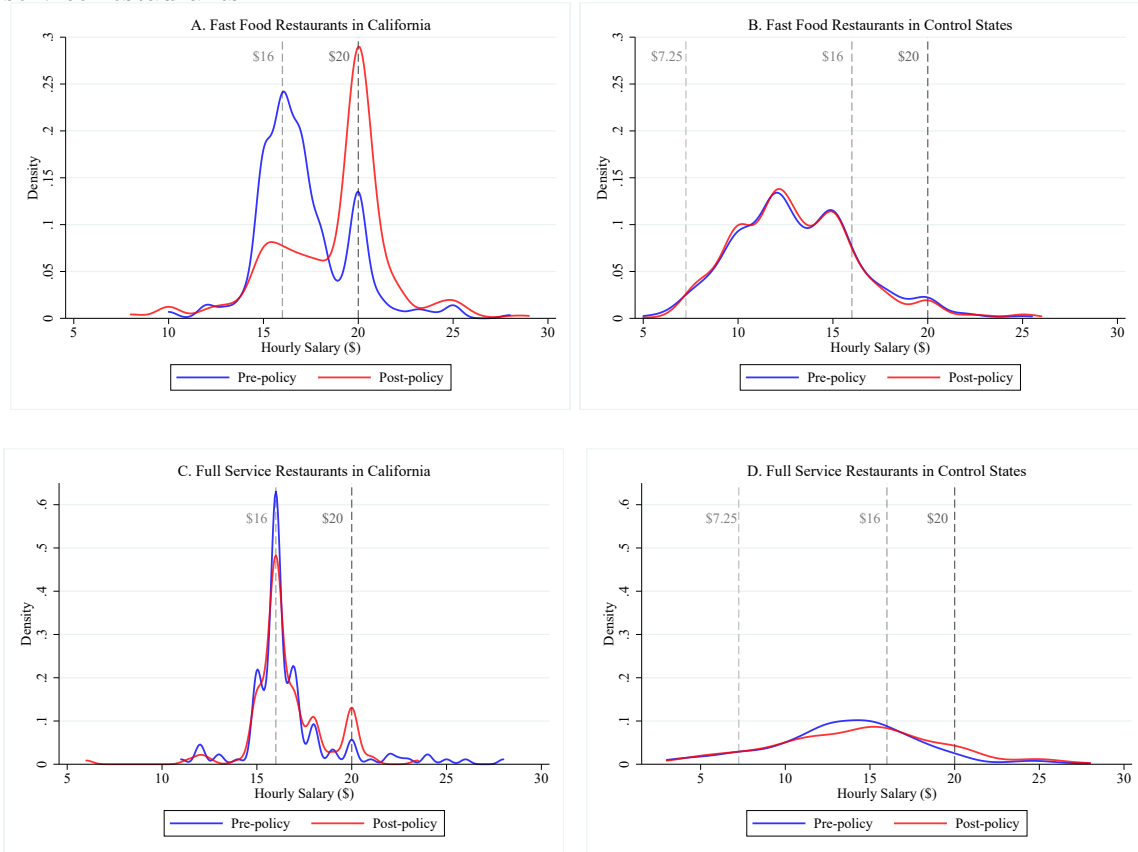
*Note:* Estimated using Equation 5. Each outcome is a log of the stated variable. The last row represents the average effect weighted by the number of locations in California. Missing cells represent variables not captured by the data-collection algorithm. Statistical significance is marked as follows: \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

Table 4: Triple difference log price effect by item

	(1) Hamburger	(2) Specialty	(3) Average of
<i>A. All fast-food</i>			
2024Q2	0.123*** (0.007)	0.027*** (0.003)	0.029*** (0.002)
2024Q3	-0.016** (0.007)	-0.002 (0.003)	0.004* (0.002)
2024Q4	-0.119*** (0.012)	-0.004 (0.005)	0.011 (0.008)
<i>B. Lower-price chains</i>			
2024Q2	0.123*** (0.006)	0.025*** (0.003)	0.027*** (0.002)
2024Q3	-0.018** (0.007)	-0.004 (0.003)	0.002 (0.002)
2024Q4	-0.119*** (0.012)	-0.005 (0.006)	0.010 (0.008)
<i>C. Higher-price chains</i>			
2024Q2	0.127*** (0.010)	0.049*** (0.002)	0.051*** (0.003)
2024Q3	0.015*** (0.005)	0.020*** (0.002)	0.022*** (0.002)
2024Q4	-0.092*** (0.013)	0.017*** (0.004)	0.031*** (0.006)

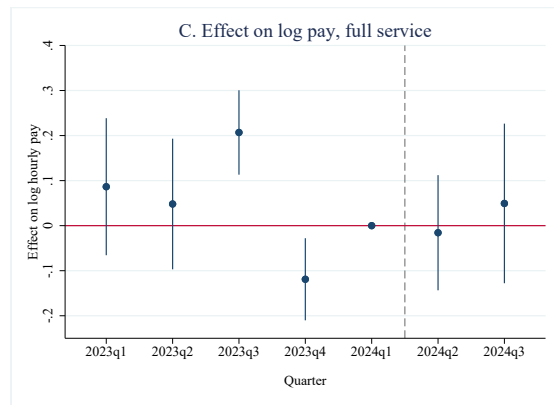
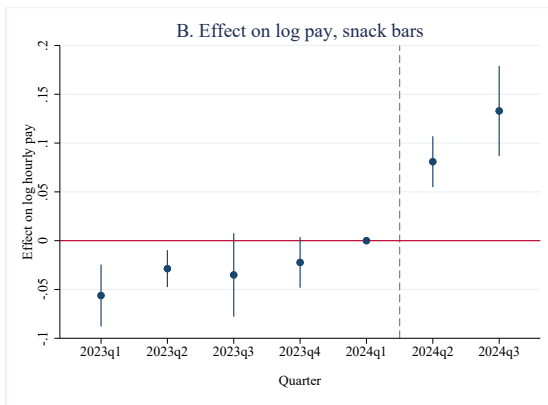
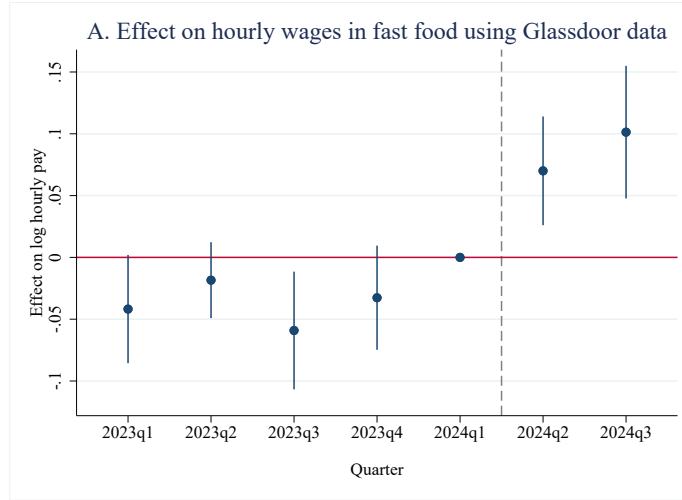
*Note:* Estimated using Equation 5. Each outcome is a log of the stated variable. The last row represents the average effect weighted by the number of locations in California. Missing cells represent variables not captured by the data-collection algorithm. Statistical significance is marked as follows: \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

Figure 1: Glassdoor hourly wage distribution pre- and post-policy for fast food and full service restaurants



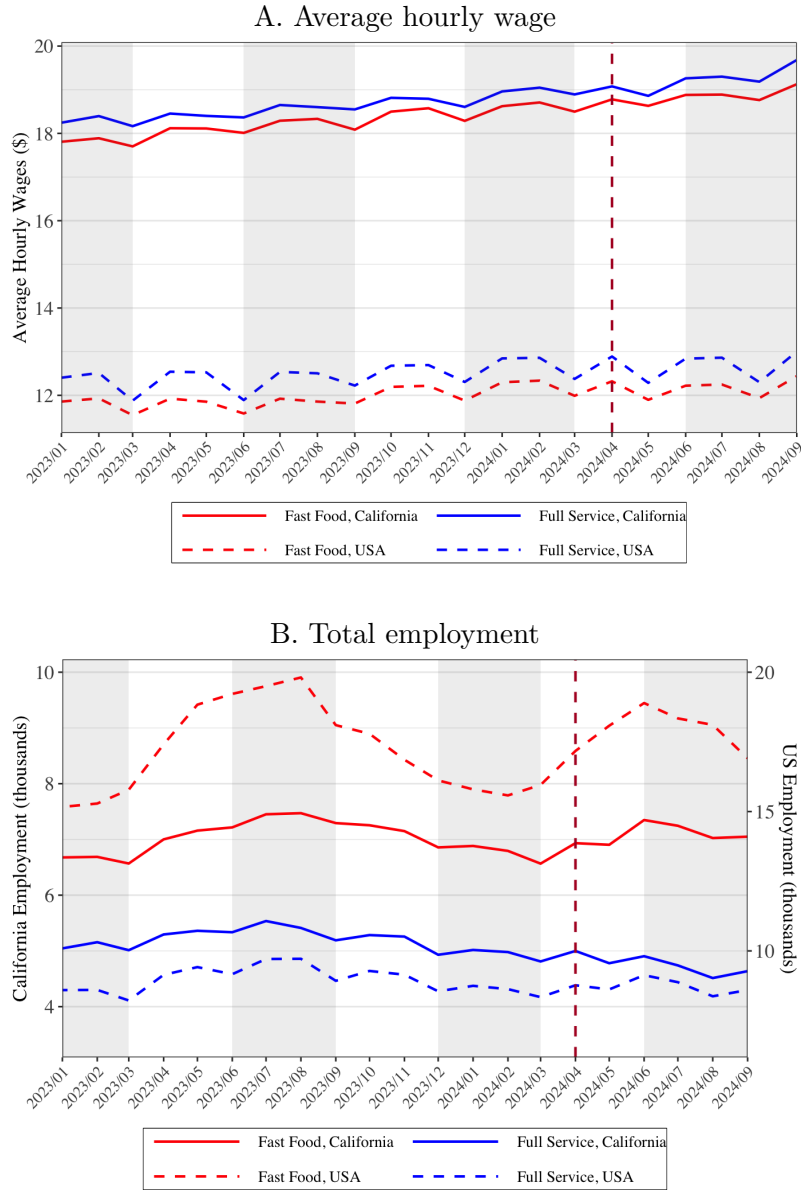
*Note:* Constructed using data provided by Glassdoor. The pre-policy period includes wages for 2024Q1; the post-policy includes wages for 2024Q2. Panels A and B include reported wages in the "Restaurants & Cafes" industry for fast food restaurants in our price data and the top 20 fast-food chains– ranked 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



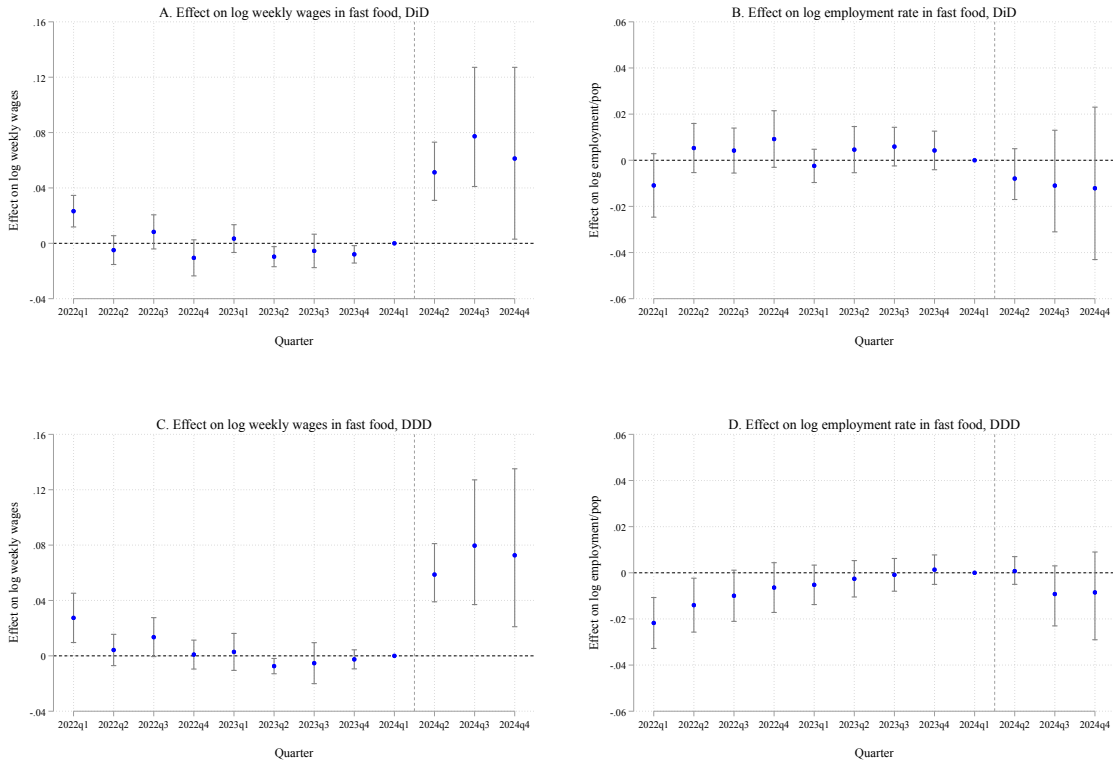
*Note:* Estimated using Equation 1 and Glassdoor data. The outcome in each panel is log state-firm average of unadjusted base hourly pay reported by workers. All estimates are relative to 2024q1, the last quarter before treatment. Panel A uses wages reported for a sample of identified restaurants in 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 percent confidence intervals. Standard errors are clustered at the state level.

Figure 3: Small restaurants average hourly wage and employment in California and the U.S.



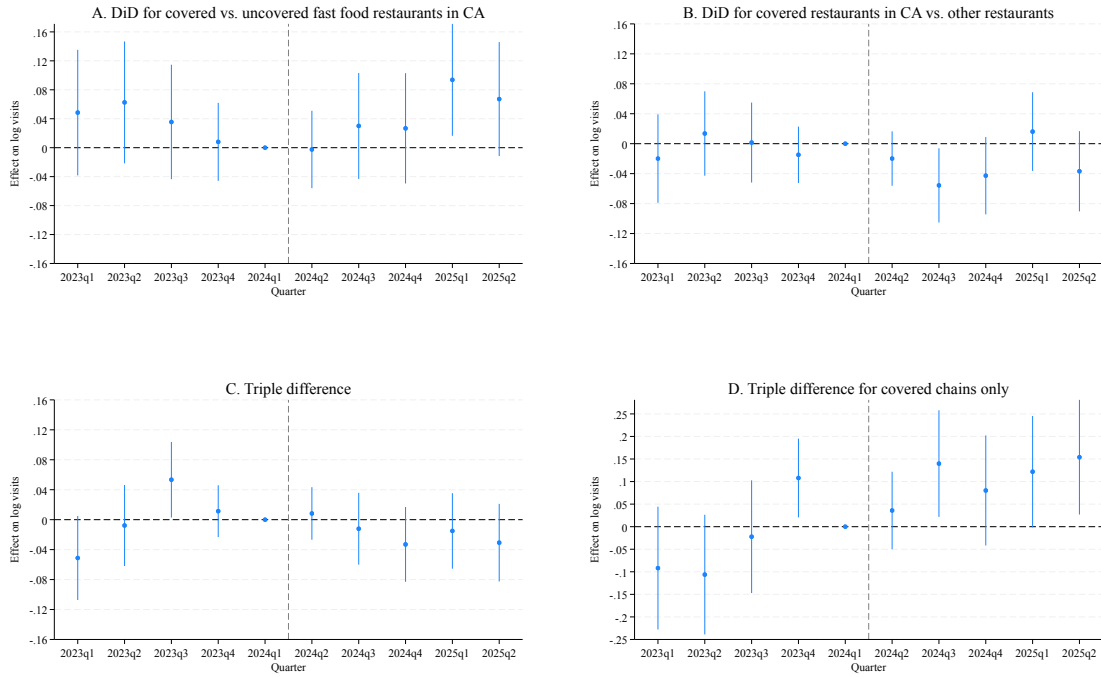
*Note:* Constructed using data from a popular point of sale and payroll software provider. Panel A shows monthly average hourly wage in dollars. Panel B shows employment in thousands. The left y-axis ranges from 3 to 10. The right y-axis has a different scale and a range of 9 to 20. The vertical dashed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

Figure 4: DiD and triple difference event study results using seasonally adjusted QCEW



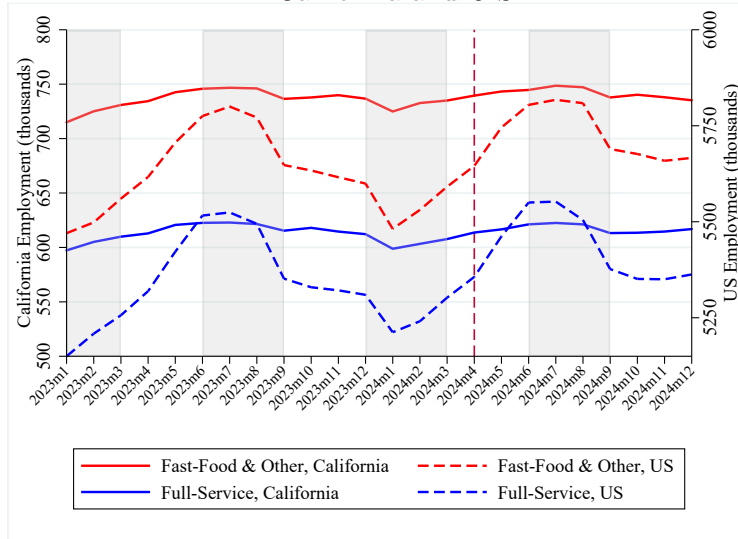
*Note:* Panels A and B are estimated using Equation 5, while panels C and D– using Equation 6. All regressions use seasonally adjusted QCEW data. The outcome in each panel on the left is the log of the county average weekly wage. In the right panels, the outcome is the log of the county employment divided 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 the state-wide change in GDP as controls. Panels A and C additionally use the county population as a control. 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 estimated using Rombachan and Roth (2023) with a bound parameter  $\bar{M}$ . Standard errors are clustered at the state level.

Figure 5: Effects on visits longer than 4 hours using Advan mobility data

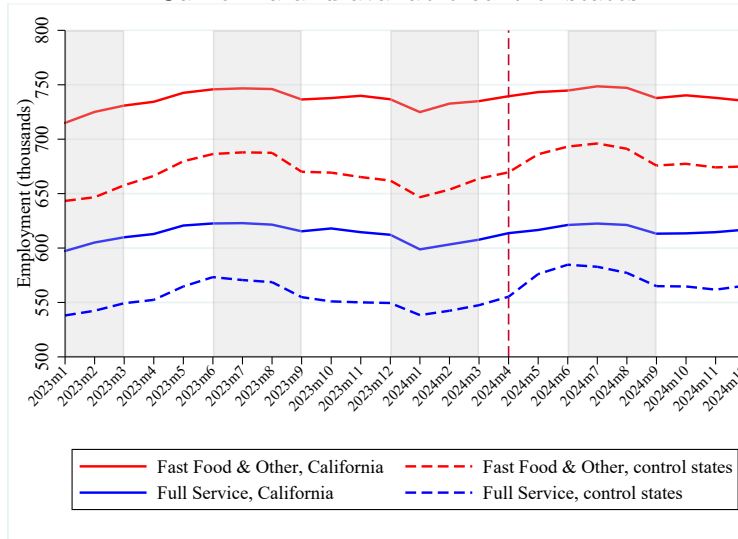


*Note:* Estimated using Advan foot traffic data. The outcome in each panel is the log of number of visits over 240 minutes. Panels A and B are estimated using a difference-in-difference specification, while panels C and D— using the triple difference. All event studies include establishment and quarter-fixed effects. Panels C and D additionally use time-by-industry fixed effects. All regressions use seasonality factor obtained from QCEW as a control. Panels A and C additionally use the state population as a control. All estimates are relative to 2024q1, the last quarter before treatment. The gray dashed line represents policy implementation. Dots show estimates weighted by state population in 2010. Lines show 95% confidence intervals. Standard errors are clustered at the establishment level.

Figure 6: Fast food and full service employment in California and U.S.  
 A. California and U.S.

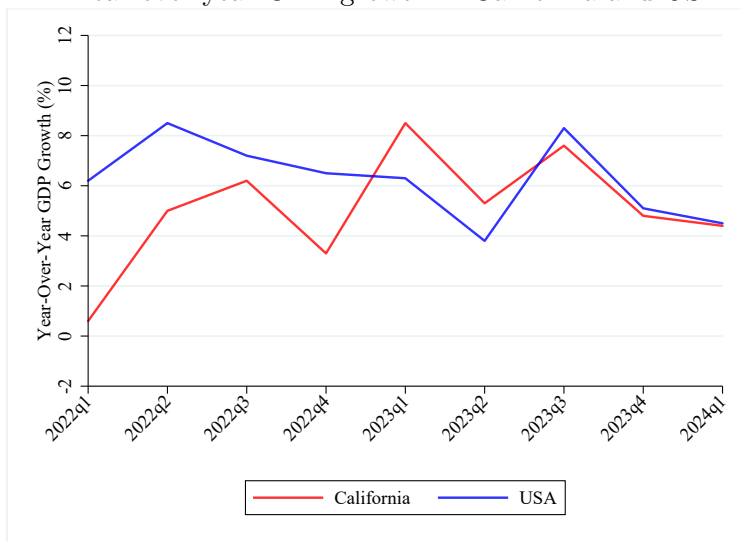


B. California and available control states

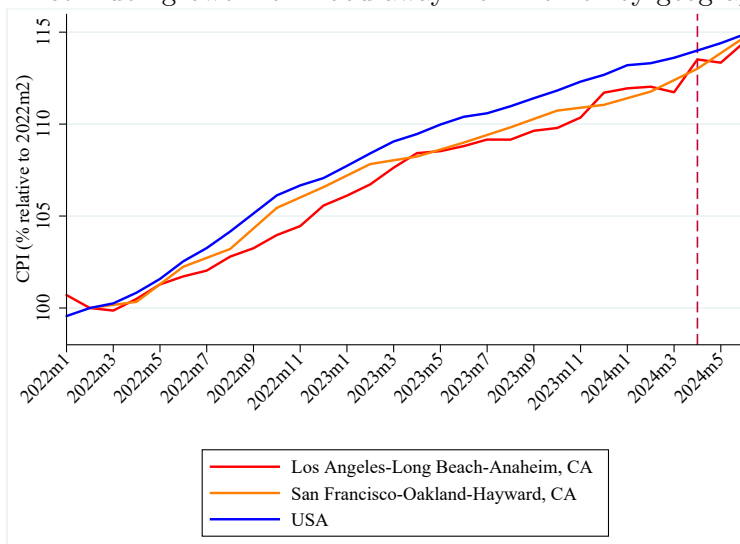


*Note:*Constructed using BLS Current Employment Statistics data (CES). "Fast-food & Other" include NAICS codes 722513, 722514, 722515. CES does not provide state-level data for the fast-food industry individually. Full-service is NAICS 722511. The vertical dashed line is the month the new policy was 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 7: GDP and price growth in California and USA  
 A. Year-over-year GDP growth in California and USA



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

### Appendix A Reconciling employment estimates

A recent paper, Clemens, Edwards, and Meer (2025) (hereafter CEM) also uses QCEW data and event study methods to evaluate the employment effect of California’s \$20 fast food minimum wage. CEM’s main finding suggests a statistically significant and negative 3.2 percent employment effect, while our paper finds a smaller and statistically insignificant effect of -1.2 percent (-1.3 percent using an additional quarter of data). Both papers use DiD and DDD event study approaches. However, the two paper’s detailed methodological choices differ in nine dimensions: choice of reference date, using monthly or quarterly data, choice of controls and control group, using state-level or county-level data, choice of employment measure, seasonal adjustment method, whether to de-trend the data, how to account for possible confounding pre-trends and choice of controls for their DDDs.

Our examination of these differences identifies four that we find to be the most problematic: CEM choose an implausible policy reference date— five months before the policy’s implementation, when half of their employment decline occurs; CEM use monthly data which is noisier; CEM do not control for California’s slower population growth, which adversely affected employment and demand for fast food and full-service restaurants; and CEM’s DDD specification does not adequately account for confounding changes.<sup>36</sup> CEM’s choices impart substantial negative biases to their employment estimates. Our methods are not contaminated by such biases. Moreover, our “honest” inference is robust to relaxing the implausible assumption— that pre-trends are perfectly parallel. We therefore stand by our results— the \$20 minimum wage has not significantly affected fast-food employment.

We discuss here these different methodological choices and conduct a series of robustness tests to examine the sensitivity of their results to their choices. We explain why each of our choices is more appropriate than those OF CEM. Table A3 presents the results of our robustness tests.

#### A.1 Anticipation effects, reference dates and monthly data

When minimum wage policies are enacted, they provide some lead time to allow employers and workers to prepare for the minimum wage increases. CEM claim that employers anticipate that adverse effects of higher minimum wages will begin before their actual implementation and therefore begin to reduce their workforces before the new standard takes effect. On the other hand, previous minimum wage studies have not found credible causal evidence of such anticipation effects (see the discussion in Dube et al. 2025). CEM therefore choose Setember 2023, the month the policy was enacted, as their reference

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<sup>36</sup>Additionally, unlike CEM, we do not detrend the data, as detrending is not a standard procedure in minimum wage studies. Instead, we use years unaffected by COVID-19 for our seasonal adjustments.

month. Our paper chooses the reference date that virtually all minimum wage papers use—the last month or quarter before its implementation (in our case March 2024 or 2024q1).

CEM’s choice to use the date the bill was signed as the beginning of an anticipation period is problematic. The state legislature passed the final version of the bill on September 14. But the governor had brokered an agreement the previous June with both legislative houses concerning minor amendments to an earlier version of the bill. On the other hand, one could argue that many employers did not pay attention to the new law until after it was enacted. The appropriate anticipation reference date could plausibly be as early as July or August of 2023 and as late as October or November.

Importantly, the reference date in an event study will not affect the employment estimate if all the pre-trends are exactly zero. In practice, it is implausible that all pre-periods will exhibit a zero deviation from a reference period. Thus, shifting the reference period from one date to another may affect the estimate. In our preferred specification, the point estimate is -1.2 when we use implementation as the reference date and -1.8 when we use the enactment date. For CEM, the comparable estimates are around -1.7 and -3.9, respectively.<sup>37</sup>

Three patterns emerge from these comparisons. First, almost 60 percent of CEM’s estimated employment effect occurs during the anticipation phase. Second, CEM’s estimated employment effect after implementation is of much smaller magnitude, much closer to our estimate, and is potentially not statistically significant. Third, the anticipation effect is virtually non-existent in our preferred specification.

To investigate this issue further, we implement our preferred and several other specifications using CEM’s reference period. We present these results using quarterly data in columns 1 to 3 of Table A3 and monthly data in columns 4 and 5. Our preferred specification, shown in column 1, rows 1-2, finds minimal anticipatory effects of -0.2 and -0.6 percent, respectively. The anticipatory effect becomes somewhat more negative if we use monthly data (column 4) and even larger in magnitude if we do not account for population and GDP changes (column 2). Finally, the strongest estimated effect between enactment and implementation appears when we use the specification closest to CEM (column 5). Recall, moreover, that none of the pre-implementation effects are significant when we use our preferred inference procedure.

Overall, we interpret these results as suggesting the absence of any anticipatory effect. And comparing columns 1 and 2 suggests that slower population growth—or lower GDP growth in California—account for substantial parts of the employment decrease in the descriptive data, in the five months after enactment.

Additionally, CEM’s anticipatory effect is more pronounced when we use monthly data. CEM’s Figure A3, Panel C (which we reproduce in Panel B of figure A4) suggests why. In every quarter, fast food employment rises in the first month of the quarter and then falls

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<sup>37</sup>We can deduce this from the Figure A3, Panel C of CEM. The estimate for policy implementation as a reference date is a difference between the effect in period 7 and period 15.

in the following two months. This pattern likely reflects measurement noise in the monthly data, which are frequently reported with lags, rather than real employment changes. This pattern is especially evident in periods 4 to 6 (January to March 2024). We conclude that using quarterly data better accounts for noisy patterns in monthly data. Moreover, the QCEW provides wage data only on a quarterly basis, making it difficult to relate wage effects to monthly employment results. The half of CEM’s negative employment estimate that occurs before implementation (period 6) results from noise in the monthly data rather than real employment effects.

## A.2 Employment measure and controls

To measure employment effects, we use the employment rate—the employment level divided by the working age population, while CEM use the employment level. The employment rate accounts for changes in employment levels related to overall population changes that are unrelated to minimum wage policies, but which nonetheless affect the demand for fast food and the supply of fast-food workers.

Columns 1 and 2 in Table A3 illustrate how the choice of employment measure and inclusion of controls affects the employment estimate. Column 1 represents our preferred specification which uses employment rate as an outcome and includes controls for local economy (employment in non-restaurant industries and change in GDP). These results suggest a small pre-implementation effect that is statistically indistinguishable from zero. The estimates become more negative post-implementation, but are still statistically insignificant. The final quarter estimated effect is -1.8 percent, with a third of the change occurring before implementation.

By contrast, Column 2, which uses employment level as the outcome and does not include controls, produces more negative effects. The last quarter effect is -2.7 percent, with around 40 percent of the effect occurring before implementation. None of these estimates are statistically significant, when we use our preferred inference method. Comparing these results to the ones in Column 1 suggests confounding factors, unrelated to the minimum wage policy, might contaminate estimates if they are not accounted for.

Indeed, as Figure A3 shows, between 2022 and 2024 California experienced slower population growth than the rest of the US. Additionally, Panel A of the Figure 7 shows that California GDP and the GDP of other states did not always grow at the same rate during the studied period. Incorporating these factors in the estimation is essential to isolate the causal effect of the minimum wage policy.

Both papers also implement a DDD event study design, which by construction absorbs state- or country-wide economic and demographic factors. However, CEM use “non-minimum-wage-intensive industries” as an additional margin of comparison. This control group is unlikely to satisfy any plausible version of a parallel pre-trend assumption. Figure A4 of CEM shows significant pre-trends, suggesting that the groups exhibit different trends. In contrast, full-service restaurants provide a natural comparison group that is

subject to many similar factors as fast-service restaurants, but are not covered by policy. We find insignificant employment effects using our triple difference estimator.

### A.3 Seasonal adjustment

Both our paper and CEM use a similar seasonal adjustment procedure that relies on observed seasonality. The main difference between the two papers lies in the periods each uses to correct for seasonal affects. Our method uses data from 2010 to 2024, but excluding 2020-2022, which were affected by Covid and the subsequent economic recovery. CEM use only the two years prior to the policy’s enactment– from September 2021 to September 2023. Their choice is problematic because the evolution of employment in those years was heavily affected by the continuing recovery from the pandemic. Those years thus do not reflect typical seasonal patterns. Nevertheless, our results are robust to using CEM’s time period for seasonal adjustment (SA). Column 3 shows that changing the SA procedure only slightly affects the estimates, relative to our preferred specification in Column 1.

### A.4 Robust inference

CEM mention that the standard cluster inference produces confidence intervals that are insufficiently conservative. Hence, we apply our inference procedure to the specification closest to theirs. Column 5 of Table A3 closely replicates CEM’s methods and results: It uses employment rather than the employment-to-population ratio, does not control for employment in non-restaurant industries or changes in GDP, uses the period from September 2021 to September 2023 for seasonal adjustment, and uses monthly employment levels. The resulting coefficient for the last month is similar to CEM’s (-3 percent versus -3.9 percent).

Figure A5 presents sensitivity analyses of the bound parameter we use for inference in our preferred employment result (Panel A) and in the specification closest to CEM (Panel B). As we discussed above, in our preferred specification the employment effect becomes statistically insignificant even at a low value of  $\bar{M} = 0.4$ . Similarly, the specification closest to CEM produces insignificant employment effect at  $\bar{M} = 0.4$  and above.<sup>38</sup> This estimate makes a generous and implausible assumption– that post-treatment trends are at most 40 percent of observed pre-trends. We conclude that the employment effect is not distinguishable from zero in the specification that produces the most negative point estimate.

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<sup>38</sup>Monthly data produce wider confidence intervals, since more post-treatment periods are included.

Table A1: Average pre-policy fast food prices in California

	(1) Hamburger	(2) Specialty burger	(3) Combo	(4) Average of main items
<i>A. By group</i>				
All Chains	3.91 (1.27)	7.56 (1.28)	13.21 (1.64)	6.10 (3.07)
Lower-price Chains	3.74 (0.97)	7.49 (1.21)	13.10 (1.52)	6.00 (3.02)
Higher-price Chains	7.57 (1.10)	9.71 (1.59)	16.96 (0.59)	9.82 (2.28)
<i>B. By chain</i>				
McDonald's	3.43 (0.51)	7.52 (0.83)	12.75 (1.51)	7.29 (2.67)
Jack in the Box	–	6.58 (0.75)	13.09 (1.20)	5.96 (1.64)
Carl's Jr and Hardee's	–	7.93 (0.98)	14.18 (0.00)	0.83 (1.28)
Burger King	5.30 (0.76)	9.46 (0.84)	15.23 (1.08)	7.09 (3.16)
Wendy's	2.91 (0.54)	8.12 (0.86)	–	2.53 (0.57)
The Habit	7.03 (0.21)	9.15 (0.30)	16.96 (0.59)	10.90 (1.01)
Five Guys	10.05 (0.92)	14.17 (1.11)	–	7.00 (0.59)
Shake Shack	8.93 (0.13)	9.53 (0.13)	–	5.17 (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 average of five items defined by authors: cheeseburger, hamburger, specialty burger, combo, and fries. Panel A is weighted by the 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* (0.007)	-0.012** (0.005)	0.003 (0.005)	-0.002 (0.004)
Jack in the Box	–	0.032*** (0.008)	0.025*** (0.005)	0.028*** (0.006)
Carl's Jr and Hardee's	–	-0.091 (0.125)	–	-0.214*** (0.033)
Burger King	0.008 (0.012)	-0.010 (0.011)	0.037*** (0.009)	0.008 (0.010)
Wendy's	0.036*** (0.010)	0.023** (0.010)	–	0.102*** (0.020)
The Habit	0.046*** (0.007)	0.047*** (0.006)	0.043*** (0.005)	0.045*** (0.005)
Five Guys	0.003 (0.005)	0.002 (0.005)	–	0.002 (0.004)
Shake Shack	-0.022*** (0.003)	-0.014*** (0.002)	–	-0.009*** (0.002)

*Note:* Estimated using Equation 5. 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: \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$



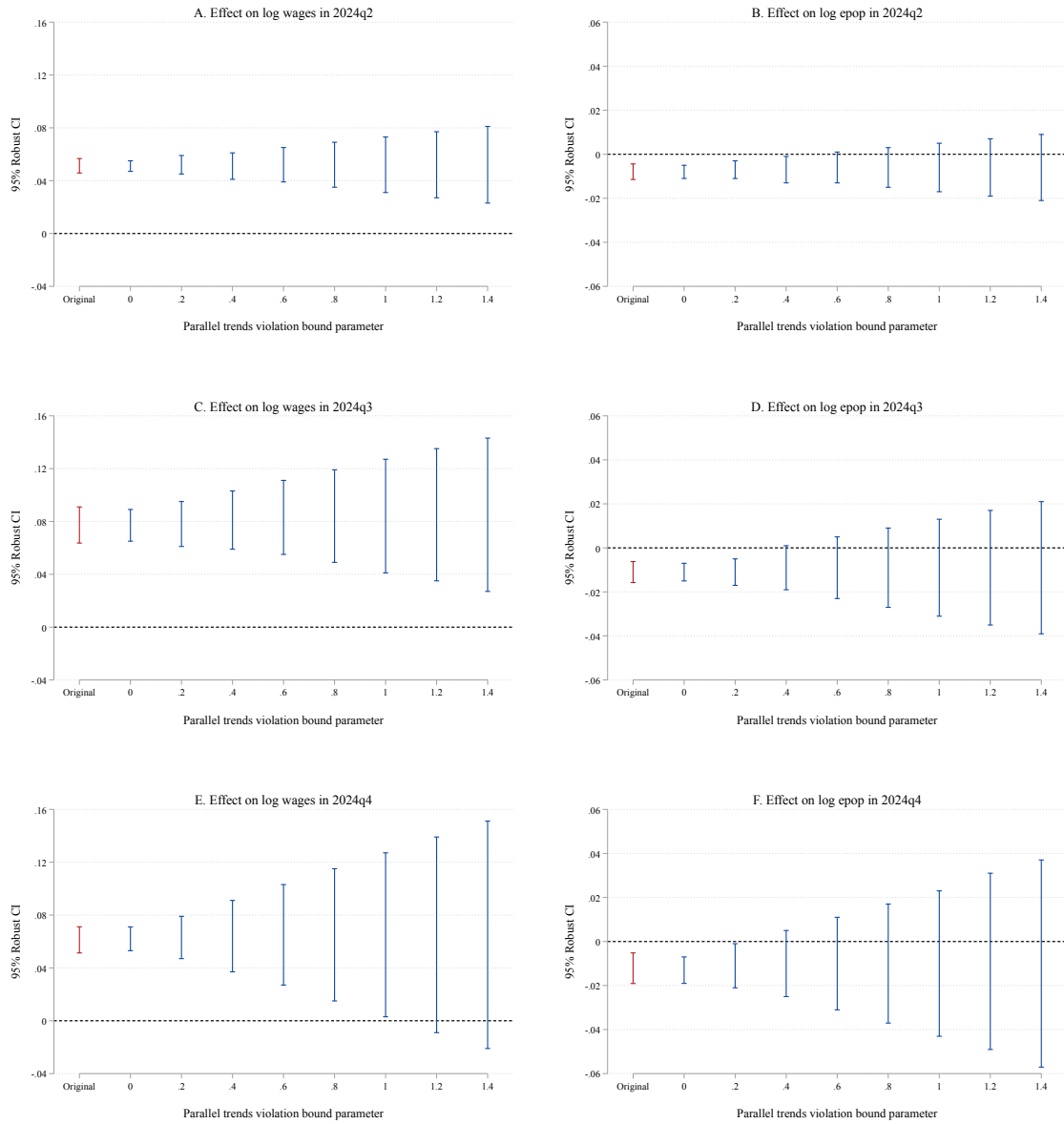
Table A3: Robustness of MW effects on employment rate in fast food using QCEW with reference period set as September 2023 (before policy enactment)

	Quarterly			Monthly	
	(1) Preferred specification	(2) Employment as outcome	(3) SA based on 2021-2023	(4) Preferred specification	(5) CEM
<b>Pre-implementation</b>					
2023q4	-0.002 [-0.019, 0.017]	-0.004 [-0.019, 0.013]	-0.004 [-0.015, 0.007]	-0.001 [-0.029, 0.027]	-0.005 [-0.021, 0.009]
2024q1	-0.006 [-0.045, 0.027]	-0.010 [-0.045, 0.023]	-0.002 [-0.029, 0.019]	-0.008 [-0.065, 0.049]	-0.014 [-0.045, 0.019]
<b>Post-implementation</b>					
2024q2	-0.014 [-0.069, 0.037]	-0.019 [-0.071, 0.031]	-0.019 [-0.053, 0.009]	-0.017 [-0.101, 0.069]	-0.026 [-0.071, 0.025]
2024q3	-0.017 [-0.089, 0.051]	-0.024 [-0.093, 0.041]	-0.017 [-0.061, 0.023]	-0.021 [-0.137, 0.091]	-0.028 [-0.091, 0.037]
2024q4	-0.018 [-0.109, 0.069]	-0.027 [-0.113, 0.057]	-0.020 [-0.075, 0.031]	-0.020 [-0.159, 0.125]	-0.030 [-0.111, 0.051]
N	1,764	1,764	1,764	5,292	5,292
County FE	X	X	X	X	X
Quarter FE	X	X	X	X	X
Controls	X		X	X	

*Note:* All columns are estimated using Equation 5. The final pre-treatment period is 2023q3 for quarterly specifications and September 2023 for monthly. Column (1) represents our preferred specification: employment-to-population ratio as an outcome, controls for non-restaurant employment and county GDP growth, seasonal adjustment based on 2010-2019 and quarterly data. Column (2) uses employment as an outcome and does not include controls for local economy or population. Column (3) uses September 2021 to September 2023 for seasonal adjustment. Column (4) uses our preferred specification but monthly QCEW data. Column (5) combines all choices in columns (2)-(4) and is the closest to CEM's preferred DiD specification. All columns include county and time-fixed effects. Columns (4)-(5) depict estimates for the last month of a respective quarter. 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  $\bar{M} = 1$ .

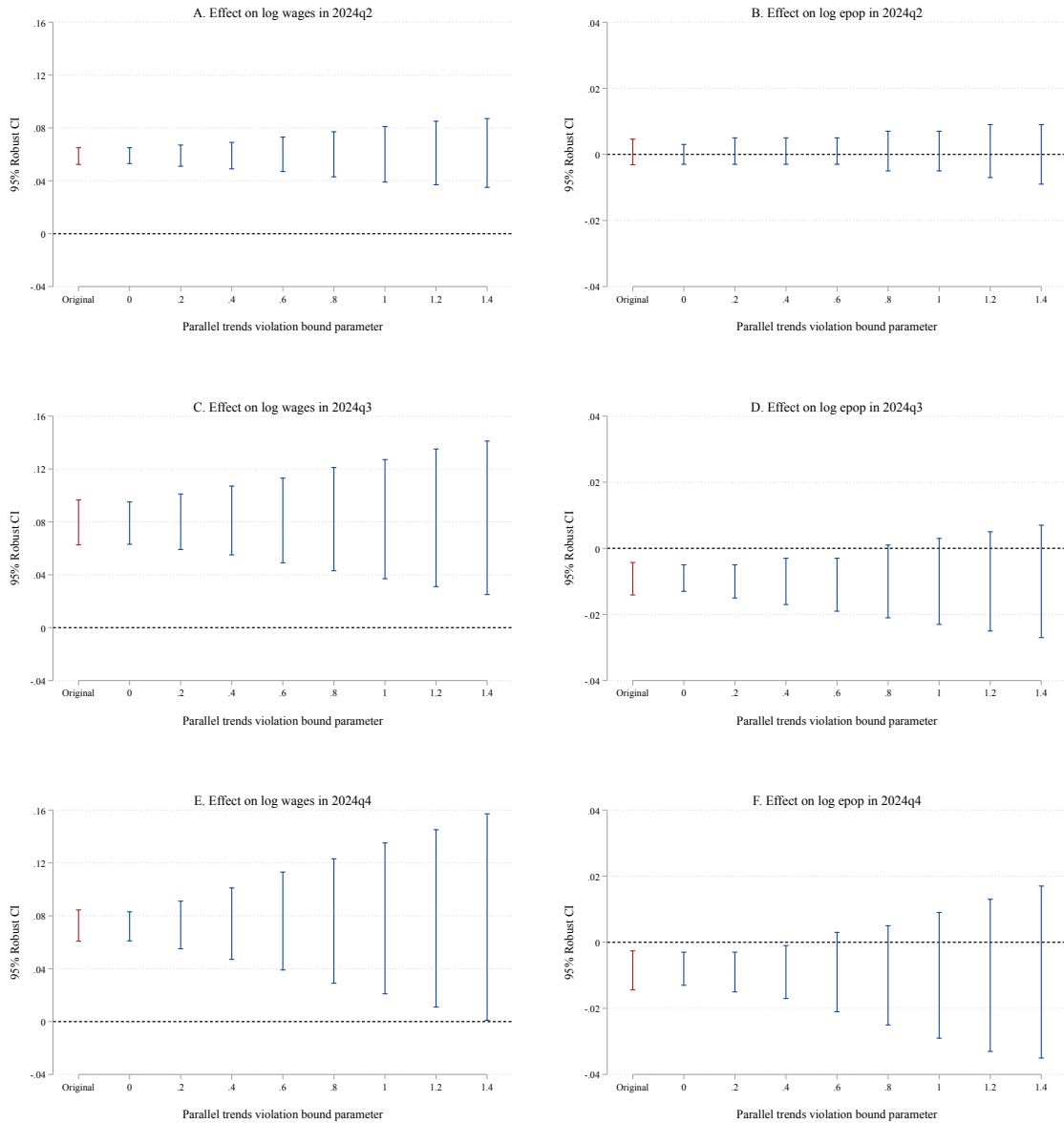


Figure A1: Rambachan and Roth (2023) robust CIs for difference-in-differences using seasonally adjusted QCEW



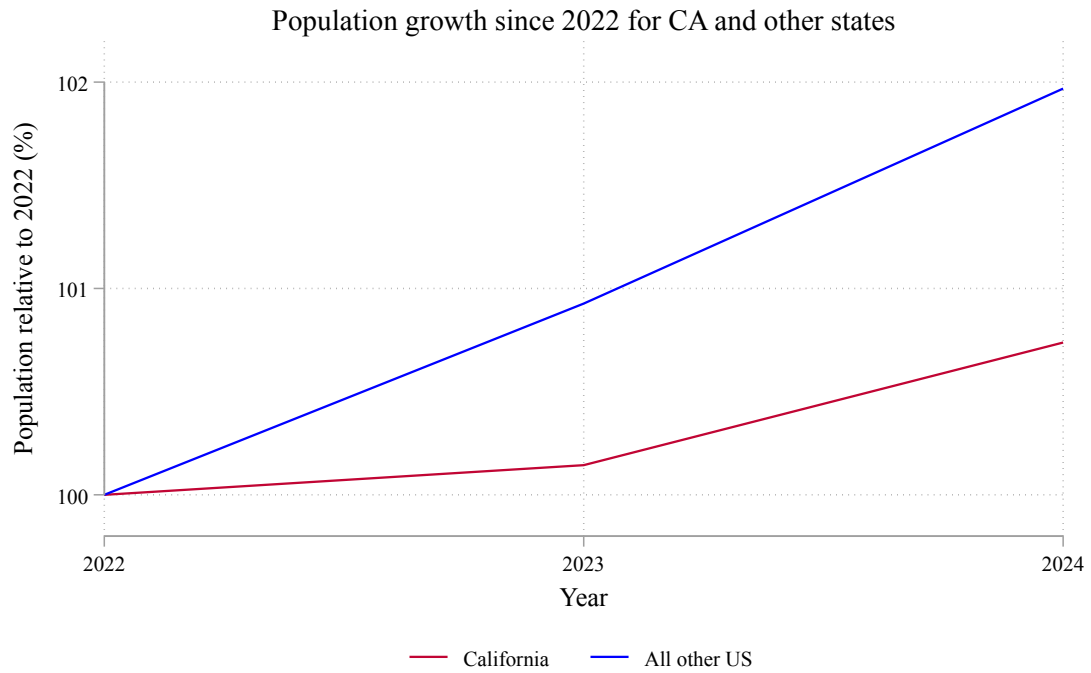
*Note:* All panels are estimated using Equation 5 and Rambachan and Roth (2023) confidence interval procedure. The horizontal axis depicts the parameter of choice  $\bar{M}$ , while the vertical axis shows the estimated effect. The red line shows the original CI, while blue lines show CIs for varying 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 state-wide change in GDP as controls. Panels A, C, and E additionally use county population as control. 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 A2: Rambachan and Roth (2023) robust CIs for triple-difference using seasonally adjusted QCEW



*Note:* All panels are estimated using Equation 6 and Rambachan and Roth (2023) confidence interval procedure. The horizontal axis depicts the 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 outcomes outside of the industry of interest and state-wide change in GDP as controls. Panels A, C, and E additionally use county population as control. All estimates are relative to 2024q1, the last quarter before treatment. Lines show 95% confidence intervals. Standard errors are clustered at the state level.

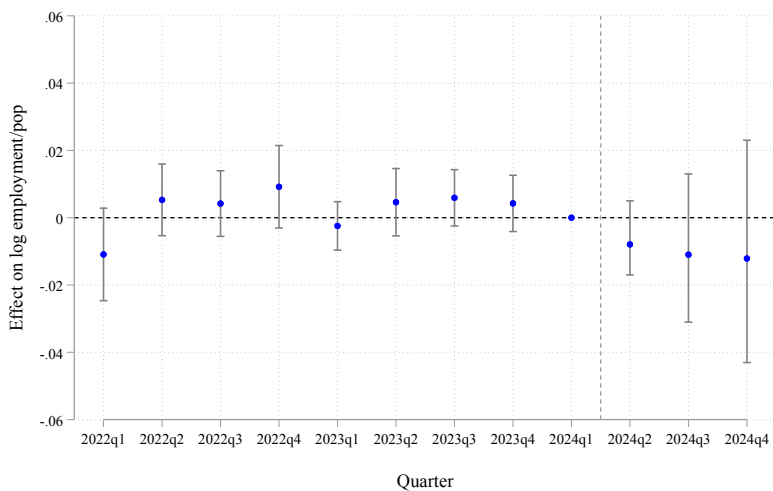
Figure A3: Yearly population growth in California and the rest of the US 2022-2024



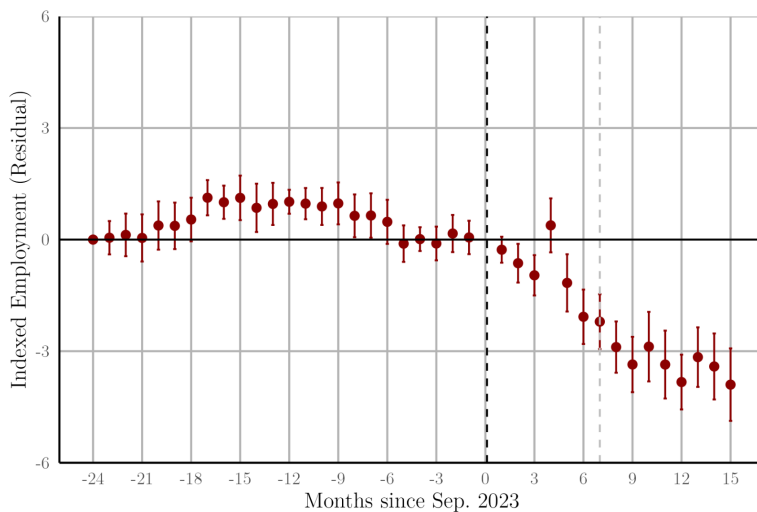
*Note:* Constructed using Census yearly population estimates for years 2022 to 2024. Each year's population is estimated as of July of the respective year.

Figure A4: DiD event-study for employment effects in fast food using QCEW Preferred specification and CEM

A. Preferred specification

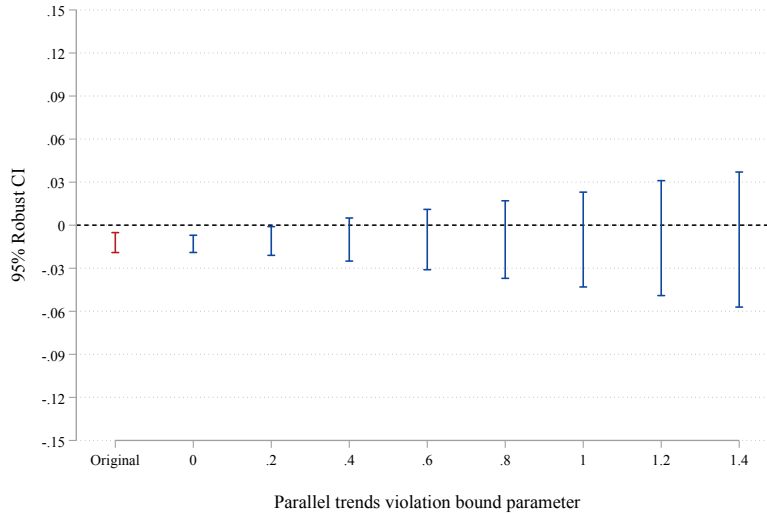


B. Figure A3, Panel C of Clemens, Edwards, Meer (2025)

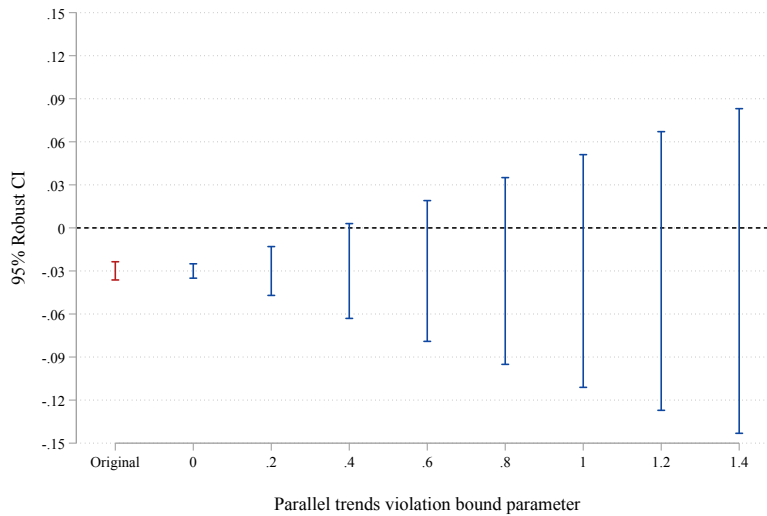


*Note:* Panel A presents our preferred specification. Panel B, which we reproduce from CEM, uses monthly QCEW data indexed to 100 in September 2023. Their zero event time point corresponds to September 2023 (the AB 1228 announcement). CEM de-trend the data for each geography-by-industry series by removing the compound monthly growth trend over September 2021–September 2023; they adjust for seasonality by subtracting month-of-year fixed-effects estimated on September 2021–September 2023 data. Their confidence intervals use cluster robust standard errors, which they regard as insufficiently conservative.

Figure A5: Rambachan and Roth (2023) robust CIs for DiD specifications  
A. Preferred specification



B. CEM specification



*Note:* All panels are estimated using Equation 5 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 Panel A is employment-to-population ratio; in panel B – employment. Both panels include county and time fixed effects. Panel A uses outcome outside of the restaurant industry and the state-wide quarterly GDP growth. Panel A uses quarterly data while Panel B uses monthly data. Estimates in Panel A are relative to 2023q3; in Panel B – relative to September 2023. Lines show 95% confidence intervals. Standard errors are clustered at the state level.