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Employment Effects of California's Fast Food Minimum Wage: Evidence from the QCEW and QWI *

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Abstract

California recently raised its minimum wage in the fast-food sector from $16.00 to $20.00 per hour. We present one-year and two-year fixed-horizon estimates of the effects of this policy on California's limited-service restaurant employment using data from the Quarterly Census of Employment and Wages (QCEW) and the Quarterly Workforce Indicators (QWI). After one year, our median estimates show that employment declines by 3 percent in the QCEW and 1.5 percent in the QWI relative to the counterfactual. After two years, both data sets show a decline of about 3.5 percent. We discuss evidence from several other recent papers on the employment effects of this policy.

*Author emails are jeffclemens@ucsd.edu, olivia.edwards@tamu.edu, jmeer@tamu.edu, and joshuanguyen@tamu.edu, respectively. Clemens thanks the Hoover Institution for its support in his capacity as an adjunct Senior Fellow. The authors used AI tools in order to create and edit LaTeX tables from output, edit figures, identify typos, improve clarity, assist in describing technical material (including details of table and figure notes), independently replicate estimates, and audit the draft for consistency. The authors reviewed and edited the content as needed and take full responsibility for the content of the publication. A replication package for reproducing the analyses in this paper can be found here: https://econweb.ucsd.edu/~j1clemens/replicationzips/CEMN_Employment_2026.zip.

1 Introduction

California's Assembly Bill 1228 (AB 1228) increased the minimum wage at limited-service restaurants with at least 60 locations nationwide from $16 to $20 per hour. Enacted in September 2023 and taking effect entirely in April 2024, it constitutes the largest one-time state-wide minimum wage increase in US history. We analyze the effects of AB 1228 on employment over one- and two-year time horizons using data from the Quarterly Census of Employment and Wages (QCEW) and from the Quarterly Workforce Indicators (QWI). In both data sets, our estimates include a simple difference-in-differences specification, a detrended difference-in-differences specification, a simple triple-difference specification (which nets out employment changes in higher wage industries), and a detrended triple-difference specification. On a one-year horizon, our median estimate using the QCEW data is that AB 1228 had an effect of about -3 percent on employment. The QWI data yield smaller estimates at a one-year horizon, with a median of just under -1.5 percent. On a two-year horizon, our median estimate using the QCEW data is -3.5 percent, and the median estimate in the QWI data is -3.5 percent, mirroring the QCEW. In QCEW data, the median one- and two-year estimates translate into job losses of roughly 18,000 and 20,000, respectively, whereas in the QWI data the one- and two-year estimates translate into job losses of 8,000 and 20,000, respectively.

Our analysis speaks to several dimensions of the emerging debate over AB 1228's employment effects. We confirm and extend the analysis of Clemens et al. (2025), who analyze one-year impacts using the QCEW. Analyses by Dube (2026) and Sosinskiy and Reich (2026) arrive at different conclusions and level a range of criticisms at the analysis in Clemens et al. (2025), which we address in this paper.

Dube (2026) contends that a synthetic difference-in-differences estimator is preferred to comparing California to the rest of the United States. His implementation creates a synthetic counterfactual that places its weight primarily on Idaho (61%) and Utah (22%). We briefly discuss why we view the use of synthetic difference-in-differences methods to be unnecessary in this setting, as California's fast food employment evolved similarly to that of the remainder of the United States between the pandemic and the enactment of AB 1228. Indeed, the implementation of these methods by Dube (2026) yields a synthetic counterfactual that differs more from California than does the rest of the United States during the pandemic itself. We refer readers who are interested in a more expansive discussion of the performance of both synthetic control and synthetic difference-in-differences methods in the AB 1228 context to Clemens et al. (2026b).[1]

[1] A key point of emphasis in Clemens et al. (2026b) is on the fact that synthetic control and synthetic

The analysis in Dube (2026) devotes substantial attention to the difference in the estimates obtained when using the QCEW versus the QWI at a one-year time horizon. Dube (2026) speculates that this difference is driven by one of several differences between the QCEW and QWI employment series and argues that the QWI employment series is preferable.[2] Regardless, the employment effects we estimate using the QCEW and QWI converge at the two-year time horizon. This casts doubt on Dube (2026)'s preferred understanding of the difference between the two series, while also rendering these differences inconsequential for the purpose of estimating AB 1228's employment effects.

Sosinskiy and Reich (2026) argue that the estimates in Clemens et al. (2025) are biased because its estimation strategy does not control for differences in the growth of California's population or gross state product. Sosinskiy and Reich (2026) add these variables as time-varying covariates, which is an approach that can introduce biases of its own in difference-in-differences estimation (Caetano et al., 2022). Further, Sosinskiy and Reich (2026) fail to provide readers with sufficient information to quantify the plausible magnitudes of the biases that could be introduced by differences in the trajectories of population and gross state product over the post-treatment period. We show that the potential biases are modest and would impact the estimates in opposing directions, such that they mitigate one another's net effects. This assessment is supported by the similarities between the difference-in-differences and triple-difference estimates in the present study and in Clemens et al. (2025).

Several additional papers have studied the effects of AB 1228 using alternative data sources and a range of outcomes. For example, Clemens et al. (2026a) document substantial pass-through of AB 1228 to California's restaurant prices. This is consistent with a broader literature on the effects of minimum wage increases (Aaronson, 2001; Renkin et al., 2022; Leung, 2021), including evidence from two additional papers on the effects of AB 1228 (Reich and Sosinskiy, 2024; Pandit, 2025). Clemens et al. (2026a) emphasize an insight from earlier work by Aaronson and French (2007) regarding the theoretical linkage between estimated employment and price pass-through effects; the key implication is that the estimated effects in Clemens et al. (2026a) and Clemens et al. (2025) are quantitatively and theoretically consistent. At least two other papers have estimated the employment effects of AB 1228 using alternative data sources. Hamdi and Sovich (2025) report declines in firm-level hiring that are offset by declines in employee turnover in an analysis of employment records from

difference-in-differences methods introduce a surprisingly large number of researcher degrees of freedom, and that the resulting estimates in this setting exhibit high variance across plausible specification choices.

[2]We find this preference puzzling given that the QWI does not include all states in a balanced panel. Additionally, the QWI employment concept preferred by Dube (2026) requires individuals to be employed in two consecutive quarters in order to be counted, and will thus undercount short-term employment relationships, which may be among those most sensitive to minimum wage increases.

Equifax that is restricted to the very largest national fast-food chains. Melo et al. (2026) find more negative employment effects than Hamdi and Sovich (2025), in particular for smaller impacted chains, using establishment-level data from the Your Economy Time Series.

The remainder of this paper proceeds as follows. Section 2 describes the data sets we analyze. Section 3 describes the estimators we implement. Section 4 presents our results and contrasts our estimators with the strategies adopted in other papers on AB 1228's employment effects. Section 5 concludes.

2 Data

Our analysis focuses on four years of data from the Quarterly Census of Employment and Wages (QCEW) and from the Quarterly Workforce Indicators (QWI). The QCEW data are released as a monthly series, which we analyze from September of 2021 (two years prior to AB 1228's enactment) through September of 2025. The QWI data are only available as quarterly series, and we analyze the equivalent period from Q3 2021 through Q3 2025. In both data sets, the data cover two calendar years preceding and two calendar years after the passage of AB 1228. Our post-treatment data include the two quarters between AB 1228's September 2023 passage and April 2024 implementation, as well as six quarters following the $20 fast food minimum wage's implementation.

We focus on two sectors: fast-food (NAICS 722513) and a constructed non-minimum-wage-intensive category defined as total private employment excluding food services / accommodation and retail (NAICS 44-45 and 72). For the initial time series tabulations, we construct a "Rest of U.S." series by subtracting California from national totals. Some of our analysis uses the underlying state-level aggregates. Along with analyzing the unadjusted data, we analyze data that are indexed relative to September 2023 or 2023Q3 (i.e., the month or quarter AB 1228 was enacted) and that are either detrended or both detrended and seasonally adjusted. When we remove state-specific pre-trends, we separately detrend each geography-by-industry series by measuring the annual growth in indexed employment between September 2021 (or 2021Q3) and September 2023 (or 2023Q3) and subtracting the resulting counterfactual trend from the observed series.[3] When we adjust additionally for seasonality, we regress detrended employment on month-of-year (or quarter-of-year) fixed effects using data from September 2021 to September 2023, separately for California and the

[3]Crucially, the trends themselves are estimated using pre-treatment data only so that they cannot be influenced by dynamic treatment effects. An expanding body of research recognizes that mistakenly allowing geography-specific trends to be influenced by dynamic treatment effects would result in bias (Meer and West, 2016).

Rest of U.S.[4] This choice reflects the clear differences in seasonality that appear in the unadjusted figures we present and discuss below. We obtain detrended and seasonally adjusted series through the latest available data by netting out the resulting seasonal adjustment factors.[5]

We present comparisons of fast-food employment in California and the Rest of the United States in Figures 1 and 2, which present data from the QCEW and QWI, respectively.[6] A number of other states enacted modest minimum wage increases of their own during this time period, an issue we address later with estimators that exclude these states from the control group. We begin with levels of employment in Panels 1a and 2a, then index both series to 100 in September 2023 (or 2023Q3) in Panels 1b and 2b.[7] We also show the series detrended as described above in Panels 1c and 2c and both detrended and adjusted for seasonality in Panels 1d and 2d.

A relative decline in fast food employment in California following the passage of AB 1228 is evident in the unadjusted levels of employment, in the indexed employment series, in the detrended series, and in the detrended and seasonally adjusted series in both the QCEW and QWI data. In Figure Panels 1b, 2b, 1c, 2c, 1d, and 2d, for which each series has been indexed relative to September 2023 (or 2023Q3), the magnitude of the relative decline through summer of 2025 is readily observed from the graphs as being on the order of 2 to 4 percent. In Panel 1a, the y-axis ranges were chosen such that the top of the range is about 11 percent higher than the bottom of the range for both the CA and Rest of U.S. series, such that similar movements in the level of each series correspond with an equivalent percent change. Here too, a difference on the order of 2-3 percent is visually apparent in the figure. The key implication of this set of figures is that indexing, detrending, and seasonally adjusting the data make the series more visually comparable but do not substantively alter the relative movements that underlie our estimates.

The QCEW employment counts include any worker on the payroll during the pay period containing the twelfth of the month, regardless of tenure. This is the comprehensive payroll

[4]We proceed in this order because removing the trend first ensures that the seasonal adjustment captures true cyclical fluctuations rather than the growth trends states experienced as employment recovered from its early pandemic lows.

[5]We are careful to estimate seasonal effects using pre-treatment data only. Additionally, the unadjusted figures we present and discuss below illustrate why it is relevant in this historical episode to detrend prior to estimating seasonal adjustment factors; seasonal adjustment would otherwise track the substantial employment growth associated with post-pandemic employment recoveries.

[6]For the QCEW, we analyze the 2026Q1 data release from Friday, Aug. 28, 2026, covering data through 2025Q4. QWI data are from the LEHD Quarterly Workforce Indicators, release R2026Q2 (V4.14.0).

[7]Subscripts are such that regions are indexed by j, industries by i, and time by t. Indexed employment in region $j \in \{\text{CA, Rest of US}\}$ in industry $i \in \{\text{Fast Food, Non-MW Intensive}\}$, at time t is thus $\text{Indexed Emp}_{i,j,t} = 100 \times \frac{\text{Emp}_{i,j,t}}{\text{Emp}_{i,j,\text{Sept 2023}}}$.

count the BLS uses to benchmark the Current Employment Statistics. The primary QWI count on which we focus, also emphasized by Dube (2026), represents beginning-of-quarter employment. This measure counts workers with positive UI earnings at the same employer in both quarter $t-1$ and quarter t. As we discuss in greater detail below, Dube (2026) emphasizes that, in contrast with the QCEW, the QWI measure will not double count positions that were filled by two individuals over the two-week reference period on which the QCEW count is based. We note, in turn, that the QWI will exclude the most marginal workers who are hired but let go prior to being observed in two consecutive quarters. Thus, it is a more stringent measure that may miss some of the very workers who are most likely to either be let go or to lose out on initial offers due to a higher minimum wage.

There are additional differences between the QCEW and QWI data that are relevant. Alaska and Michigan have incomplete QWI coverage during our analysis window, and therefore must be excluded from our QWI analyses. Additionally, the QWI data are reweighted to match aggregates from the QCEW, with that procedure changing (and applied retrospectively) in 2021 (McEntarfer et al., 2021).

3 Methods

In this section, we first present the basic features of the estimators we deploy to infer the effects of AB 1228 on employment. We then discuss the key identifying assumption on which these estimators rely, as well as inference issues that arise due to the fact that the policy change of interest is specific to California.

3.1 DD and DDD-style Estimators

We follow Clemens et al. (2025) in implementing a set of basic difference-in-differences and triple-difference style estimates. We focus on one-year and two-year changes in employment from September 2023 to September 2024 and September 2025 or from 2023Q3 to 2024Q3 and 2025Q3. This avoids the need to adjust for seasonality, which is a non-trivial source of variance due in part to the COVID-19 pandemic's influence on seasonal employment patterns. Our simplest estimator directly compares California to the rest of the United States over these one- and two-year time horizons and is thus:

$$\begin{aligned}\beta_{\text{DD,t}} = &[\text{Indexed Emp}_{\text{Fast Food, CA, t}} - \text{Indexed Emp}_{\text{Fast Food, Rest of US, t}}] \\ &- [\text{Indexed Emp}_{\text{Fast Food, CA, Sept 2023}} - \text{Indexed Emp}_{\text{Fast Food, Rest of US, Sept 2023}}], \quad (1)\end{aligned}$$

where t corresponds with either September 2024 (Q3 2024) or September 2025 (Q3 2025).

As extensions to our simplest estimator, we augment Equation 1 along two dimensions. First, we net out pre-treatment trend growth from September 2021 to September 2023 (or from 2021Q3 to 2023Q3) using the methods described in the previous section to generate a set of detrended difference-in-differences-style estimators. Second, we use non-minimum-wage-intensive industries as a within-state control group to construct a triple-difference estimator. The triple-difference estimator can be written as:

$$\begin{aligned}\beta_{\text{DDD}} = \Big[&[\text{Indexed Emp}_{\text{Fast Food, CA, t}} - \text{Indexed Emp}_{\text{Fast Food, Rest of US, t}}] \\ &- [\text{Indexed Emp}_{\text{Fast Food, CA, Sept 2023}} - \text{Indexed Emp}_{\text{Fast Food, Rest of US, Sept 2023}}]\Big] \\ &- \Big[[\text{Indexed Emp}_{\text{Non-MW Intensive, CA, t}} - \text{Indexed Emp}_{\text{Non-MW Intensive, Rest of US, t}}] \\ &- [\text{Indexed Emp}_{\text{Non-MW Intensive, CA, Sept 2023}} - \text{Indexed Emp}_{\text{Non-MW Intensive, Rest of US, Sept 2023}}]\Big]\end{aligned} \tag{2}$$

As noted in the previous section, the QCEW data are available on a monthly basis while the QWI data are available on a quarterly basis. This has a minor effect on the base period to which we index the data from each data set. We index the QCEW data to September 2023, the month in which AB 1228 was enacted, while we index the QWI data to 2023Q3. These are the last time periods in either dataset that ensure that all post-enactment impacts on employment are appropriately categorized as post-treatment effects rather than pre-treatment developments.

3.2 Discussion of Identification and Inference Assumptions

For estimates of Equations 1 and 2 to capture the causal effect of AB 1228, standard identification assumptions must hold. Unbiased estimation requires that employment in California's fast-food sector would have moved along a trend parallel to that of the estimator's counterfactual in the absence of AB 1228. Violations of this assumption can take two broad forms, one of which is testable and the other of which is not.

The testable form of parallel trends violation involves structural, pre-treatment divergence in the paths along which the treatment and control groups are trending. We test for such divergence in two ways. First, we visually inspect the simple time series for California relative to the rest of the United States and show that they moved in parallel over the two years preceding AB 1228's enactment. Second, we investigate whether we obtain meaningfully different estimates if we adjust our data for state-specific trends estimated using pre-treatment data. We show that we do not.

The untestable form of parallel trends violation involves shocks that occur contempora-

neously with or in the wake of treatment. By definition, such shocks are untestable precisely because they would emerge in the data in a way that is not distinguishable from the treatment effect itself. It is possible, however, to test for specific forms of such shocks. For example, California's fast food employment may have been impacted by shocks to other industries that were more prominent in California than in other states. The impact of such a shock can be investigated through our triple-difference estimator, which nets out any differential employment changes that occurred in California relative to other states in non-minimum wage intensive industries. In the next section, we present a detailed discussion of several possible shocks that have been suggested as sources of bias by other papers within the growing literature on AB 1228.

Inference in this setting is complicated by the fact that there is a single treated state. We adopt a standard randomization inference approach. We assign placebo treatment sequentially to each state other than California and estimate placebo treatment effects using the estimators described above. We then place the California estimate within the distribution of placebo estimates and compute a one-sided permutation p-value, $(b+1)/(m+1)$, where b is the number of placebo states whose estimates are more negative than California's and m is the number of placebo states. Counting California as one draw from the distribution ensures that the p-value is never exactly zero. We note that this form of randomization inference will tend to yield overly conservative inference (i.e., insufficiently small p-values) when the treated state exhibits less time series variability than other states, as tends to be the case when the treated state is a large and economically diverse state like California rather than a small and less economically-diversified state like North Dakota.

4 Results

In this section, we first present the sets of difference-in-differences and triple-difference estimates that arise from our preferred methodology over both one- and two-year time horizons. Next, we convert our employment effect estimates into elasticities. We then discuss the set of issues debated within the literature on AB 1228's effects, how our estimation strategy addresses these issues, and whether the estimation strategies deployed by other researchers can be viewed as resolutions to these issues.

4.1 Employment Effect Estimates

Figure 1 presents the monthly time trends for fast-food employment in California and the rest of the United States from September 2021 to September 2025 as reported in the Quarterly

Census of Employment and Wages. Figure 2 presents the equivalent quarterly series as reported in the Quarterly Workforce Indicators. The two figures reveal similar patterns, though somewhat more strikingly in the monthly data from the QCEW. The series track each other before AB 1228. After the September 2023 enactment of AB 1228, California's fast-food employment falls below the comparison and stays below through the end of the sample. Panel (b) from Figure 1, in which the series are indexed to September 2023 but not otherwise adjusted, provides a clear illustration that California declines, in relative terms, by about 3 percent through September 2024 and 3 percent through September 2025. When the series are adjusted for their post-pandemic, pre-AB 1228 trends, as in Panel 1c, a similar divergence remains clearly apparent. The same is true when the series are similarly adjusted for seasonal factors in Panel 1d.

The evidence from Panels c and d of Figures 1 and 2 will be particularly important when we discuss debates across estimation strategies within the literature. Sources of identifying variation can quickly become opaque when the underlying series are adjusted through complex seasonal adjustment and detrending procedures prior to estimation, which is why we present the adjustments on a step-by-step basis in Figures 1 and 2. Additionally, inferring post-pandemic trends and seasonality is inherently difficult due to the limited data available for this purpose. Consequently, our preferred estimates are at 12- and 24-month or 4- and 8-quarter time horizons, which render seasonal adjustment factors irrelevant. Figure A1 presents the same respective series from Figure 2 with the base period being 2023Q4, as in Dube (2026).

We now turn to our estimates of the one- and two-year employment effects of AB 1228 as inferred using QCEW and QWI data. We begin by walking through the 12-month differences we observe in the QCEW data, as reported in Table 1. The point estimates in Table 1 are complemented by Table 2, which provides the information required to infer the associated p-values. Table 1 replicates the estimates reported in Clemens et al. (2025), aside from minor changes arising from revisions to the QCEW data by the Bureau of Labor Statistics since the circulation of that paper. Consequently, our discussion of Table 1 draws liberally from the text of the earlier paper. We then present our extension of the QCEW data as well as our analysis of the QWI data.

The first four rows of column 5 in Table 1 illustrate that relative to September 2023, fast-food employment in California had declined by 2.74 percent, whereas employment in non-minimum-wage-intensive industries had increased by 0.48 percent as of September 2024. This contrasts with the rest of the United States, where fast-food restaurant employment had increased marginally (by 0.15 percent) while employment in all non-minimum-wage-intensive industries had risen by 1.18 percent. Our estimate of Equation 1, which compares California's

fast-food employment growth with fast-food employment growth elsewhere in the United States, is -2.89 percent, reflecting that California's fast-food employment contracted by 2.74 percent while fast-food employment elsewhere increased by 0.15 percent. We also compare California to a set of states that did not change their minimum wage after September 2023. These states experienced slightly larger increases (0.49 percent) in fast-food employment, possibly reflecting the impact of higher minimum wages on low-wage sectors in states that implemented increases. This leads to a larger estimate of -3.23 percent for Equation 1.

The estimates in Table 1 next check for the possibility that fast-food employment in California and the rest of the United States may have diverged from September 2023 to September 2024 because they were already trending differentially. We do so by comparing the trends in these variables from September 2021 to September 2023. The relevant cell entries in column 4 reveal that fast-food employment in California had, in fact, grown slightly more than fast-food employment in the rest of the United States prior to AB 1228's enactment (by 3.76 percent per year relative to 3.27 percent). Consequently, if the associated pre-treatment trends are extrapolated to produce a detrended estimate, we obtain a difference-in-differences estimate of -3.38 percent (-3.58 percent when California is only compared with states that enacted no minimum wage increases).

We next describe our triple-difference estimates, which net out any differential employment growth in California's non-minimum-wage-intensive industries relative to non-minimum-wage-intensive industries elsewhere in the country. Because employment in non-minimum-wage-intensive industries grew more slowly in California relative to the rest of the United States, our estimate of Equation 2 is moderately smaller than that of Equation 1, at -2.19 percent. The detrended version of Equation 2 comes in slightly larger, however, at -3.52 percent, reflecting that California's non-minimum-wage-intensive employment was on a slower growth trend than the rest of the United States prior to the implementation of its fast-food minimum wage. Once again, the estimates using non-changing states as a comparison are larger in absolute value, with a simple triple-difference estimate of -2.54 percent and a detrended triple-difference estimate of -3.90 percent. Figures A2, A3, and A4 present the employment trends for higher-wage industries in the QWI data and QCEW data respectively, with Figure A2 indexing the QWI data to 2023Q3 and Figure A3 indexing the QWI data to 2023Q4. Outside of a relative decline in California in the first quarter of 2023, the figures reveal similar patterns of high-wage industry employment in California and the rest of the U.S.

In summary, Table 1 presents eight difference-in-differences and triple-difference-style estimates of Equations 1 and 2 in the final four rows of columns 5 and 6, corresponding to simple differences and detrended differences, and for estimation using all states as controls

versus using only states that enacted no minimum wage increase as controls. The estimates range from -2.19 to -3.9 percent, with a median of -3.3 percent.

As discussed in Section 3, we assess the statistical strength of our estimates using placebo inference methods. For each of the eight specifications summarized in the previous paragraph, Table 2 lists the states for which the placebo estimate is more negative than the estimate for California. In the simple difference-in-differences specification (regardless of control group) and in the detrended difference-in-differences specification when focusing on control states that enacted no minimum wage increase, the estimate for California is the most negative among the set, implying strongly statistically significant effects. In the triple-difference estimates using all or non-increasing states as a control and the detrended difference-in-differences using all states, we calculate p-values less than 0.10, as only one to three states have more extreme negative values than California. For the remaining two specifications, which are detrended triple-difference models, the California point estimate yields a p-value of 0.12 to 0.13.[8]

Table 3 extends our analysis of the QCEW to encompass 24-month differences through September 2025. It is accompanied by Table 4, which presents the information required to infer p-values for the estimates in Table 3. At the 24-month time horizon, the estimates become more variable across specifications. The range across the 8 estimates is from -2.02 percent to -5.03 percent, and the median is -3.45 percent. Because states' 24-month changes are more variable nationwide, the statistical significance of these 24-month time horizon estimates is weaker despite being modestly larger, on average, in magnitude. Only the difference-in-differences comparisons of California to the non-increaser states (both simple and detrended) have p-values below 0.10, though all but one estimate has five or fewer states whose estimates are more negative than California.

We next turn to 4-quarter horizon estimates using the QWI data. Table 5 reports the estimates, which take 2023Q3 as the baseline relative to which employment effects are estimated. It is accompanied by Table 6, which presents the information required to infer p-values for the estimates in Table 5. These estimates are uniformly smaller than the 12-month QCEW estimates, and range from -0.18 percent to -2.45 percent with a median of -1.38 percent. Only one of the eight estimates is distinguishable from zero at the 0.10 level.[9] While these

[8] Across all specifications, the more extreme values come from five geographic units: Delaware, the District of Columbia, Idaho, Louisiana, and New York. All but Louisiana and New York fall in the bottom quartile of states by population, and Delaware and D.C. are among the very smallest. These states' smaller populations and fewer distinct labor markets likely contribute to greater underlying variance in their employment series. Because placebo inference relies on the distribution of these states' estimated effects, the presence of high-variance states generates a wide distribution relative to California's relevant counterfactual. This leads to overly conservative inference for California which, with its large population and many labor markets, likely has lower underlying variance in employment outcomes.

[9] Dube (2026)'s QWI estimates use changes relative to a baseline of 2023Q4 rather than 2023Q3. For

4-quarter estimates are uniformly smaller than the similarly timed QCEW estimates, we note an important discrepancy in Dube (2026). Dube (2026) asserts that his "replication of CEM's spec on QWI (Q3-Q3, log emp)" is a positive 1.5 percent (Row 2, Column 4, Table D2 in that paper). We attempt to replicate this estimate using the raw QWI employment counts, in Table A2, and show that the simple California-minus-Rest-of-US estimate for fast food is -0.85 percent. However, we are able to replicate Dube (2026)'s QCEW estimate in his Table D1 (Row 2, Column 4), which we show in Table A1, and which is similar to ours at -2.9 percent.

We conclude the presentation of our basic difference-in-differences and triple-difference estimates with 8-quarter fixed horizon estimates using the QWI data. The estimates are reported in Table 7 and are accompanied in Table 8 by the information required to infer p-values. The magnitudes range from -1.54 percent to -5.55 percent with a median of -3.52 percent. All but two estimates – those for the non-detrended triple-differences specifications – have p-values below 0.10. We conclude by noting that the magnitudes for the two-year QWI estimates have converged with the two-year QCEW estimates.

4.2 Elasticity Estimates

To convert our estimated employment impacts into own-wage elasticities, we present the QCEW's average weekly wage series for limited service restaurants in California versus the rest of the United States in Figure A5.[10] The series move roughly in parallel from the fall of 2021 through the fall of 2023. Then, following the enactment of AB 1228, average weekly wages in California's fast food sector sharply rise relative to fast-food wages elsewhere. The differential rise from 2023Q3 to 2024Q3 is roughly 8 percent, while the differential rise through 2025Q3 is just over 6 percent. This modest decrease may reflect a catching up in wages in other states in light of the fact that California's fast food minimum wage was not increased a second time after AB 1228's enactment. These estimated differential increases are consistent with those estimates in other work.[11]

the sake of direct comparison, we produce estimates that mirror this choice in Table A3 and differ only marginally from the estimates that use 2023Q3 as the baseline. The estimates range from -0.59 percent to -2.47 percent with a median of -1.43 percent. None of the eight estimates are significant at the 0.10 level. The information required to infer p-values on these estimates can be found in Table A4.

[10]These measure average weekly earnings, not hourly wages. Reductions in hours worked among those maintaining employment, as in Jardim et al. (2022), would mean a larger own-wage elasticity when measuring the overall employment response. Changes in the composition of the workforce can impact the interpretation of these estimates as well (Clemens et al., 2021).

[11]At the one-year time horizon, the full set of estimators that we consider for employment effect estimates yields broadly similar estimates of wage elasticities. At the two-year time horizon, however, triple-difference style estimates yield notably smaller estimates due to a fast pace of wage growth in California's non-minimum-wage-intensive industries. We note that signs of this movement can also be seen in the QCEW wage effect

At the one-year time horizon, our median estimated employment decline of roughly 3 percent in the QCEW translates into an own-wage elasticity of -0.40. The one-year estimated employment decline in the QWI translates into an elasticity of -0.17. At a two-year horizon, the employment decline of 3.5 percent, as estimated in both data sets, translates into an elasticity of just over -0.5. The magnitude of the latter elasticity reflects in part that the employment decline had marginally increased and in part that the wage effect had attenuated due to faster wage growth elsewhere around the country over the second year following the one-time increase in California's fast food minimum wage.

4.3 Identification Challenges Raised Within the Literature on AB 1228

We now turn to a discussion of the identification threats that have been proposed by Dube (2026) and by Sosinskiy and Reich (2026). We proceed by discussing how we think about the potential biases they entail, whether these biases are addressed by our analyses, and whether the proposed solutions in Dube (2026) or Sosinskiy and Reich (2026) are credible alternatives.

4.3.1 Empirical Choices

We begin by briefly addressing the issues of seasonal adjustment and detrending. Sosinskiy and Reich (2026) write that "the results reported by Clemens, Edwards, and Meer (2025) are not robust to properly controlling for differential seasonality in California and controls *[sic]* areas." We find this puzzling, as our emphasis on one- and two-year fixed horizon changes is motivated in large part by a desire to obviate the relevance of seasonal adjustment, which otherwise imposes subtle challenges due to the COVID-19 pandemic's effects on seasonal patterns in the restaurant sector. Because of those changes, we prefer this approach over an alternative like that adopted by Dube (2026), which relies on seasonal adjustment patterns estimated using pre-pandemic data. The key point is that seasonality adjustments appear to have modest relevance to the results in any of the papers on AB 1228's effects and that our analysis in particular is structured to render them irrelevant. Procedures to account for pre-treatment trends, by contrast, do impact our estimates. Even here, however, both our sets of robustness checks and our transparent sequence of time series figures reveal that

estimates in Dube (2026), as two of his triple-difference estimators exhibit a shrinking wage estimate in early 2025. Additionally, we note that detrended estimates are at risk of being driven artificially (in our view) by the striking late pandemic movement we observe in late 2021. Our reading of the wage data is that the simple difference-in-differences style estimate comparing wages in California's fast food sector to the fast food sector elsewhere is the most reliable estimate across the specifications we consider.

detrending and seasonally adjusting should not be expected to have substantial impacts on our results, and indeed they do not. Any claims to the effect that accounting for state-specific time trends meaningfully impacts estimates in this setting should be treated with caution.

A second claim by Sosinskiy and Reich (2026) is that Clemens et al. (2025) "use noisier monthly data." On this point, we note first that the monthly data we analyze are from the comprehensive administrative records of the QCEW and are not exposed to concerns of sampling variability. Variations in the QCEW data's month-to-month employment readings reflect, to the best of any available information, actual variations in the most comprehensive administrative employment census in the United States. Further, our time series figures reveal the month-to-month movements in both the California and rest-of-United-States fast-food employment series to be smooth, and that our analyses of monthly versus quarterly employment data cannot plausibly explain meaningful variations in results across studies. Indeed, we find the QCEW's relatively smooth monthly employment patterns quite helpful for visualizing seasonal patterns in the data.

A third point by Sosinskiy and Reich (2026) relates to our choice of reference period. Sosinskiy and Reich (2026) argue that Clemens et al. (2025) "choose a nonstandard reference period-the date the policy was enacted, which preceded its implementation by five months." Meanwhile, Dube (2026) applies a base period of 2023Q4 (and excludes a "transition" quarter of 2024Q1). Both of these alternatives risk biasing estimates towards zero by folding post-enactment data into the pre-treatment period. Sosinskiy and Reich (2026) contend that the time of treatment should be the date at which the minimum wage increase was implemented rather than the date at which it was enacted. But it is at odds with reasonable theories of firm behavior to disregard the possibility of a response to a major policy change over the time period between its enactment and implementation. Models of forward-looking firms allow for anticipatory effects (Bond and van Reenen, 2007) and, indeed, expectations about future conditions are a standard feature of the determinants of demand.

A final issue we address here relates to Dube (2026)'s comparison of results in the QWI data relative to the QCEW data. Differences in the estimates obtained using the QCEW versus the QWI can be attributed to several sources. Dube (2026)'s preferred interpretation is that differences in employment effects estimated at the one-year time horizon are driven by the possibility that the QCEW's reference window can result in double counting of jobs in cases in which two workers hold the same position over the span of two weeks. We note that the difference could just as plausibly arise from the QWI's undercount of jobs in cases where marginally employed individuals fail to be counted by the QWI because they were not employed in consecutive quarters. The QWI's incomplete reporting and reweighting to match the QCEW may also play a role. All that said, as shown above, the employment effects we

estimate in the QCEW and QWI converged at a two-year time horizon. This suggests that transient noise rather than substantive differences in the underlying employment concepts is likely to explain the difference observed at the 1-year horizon.

4.3.2 Accounting for Macroeconomic Differences with Covariates and Within-State Comparisons

A more interesting set of empirical questions involves macroeconomic shocks that may have differentially impacted California relative to other states. Methods for quantifying and addressing such shocks raise several econometric considerations. These include the implications of controlling for time-varying covariates, the use of within-state comparison groups, and the use of reweighting strategies like synthetic control and synthetic difference-in-differences methods. We address these in turn.

With respect to macroeconomic conditions, Sosinskiy and Reich (2026) write that Clemens et al. (2025) "do not control for California's slower population growth" and emphasize that differences between their results and the results in Clemens et al. (2025) arise from the decision to "control for population and GDP changes." Regarding the relevance of population and GDP growth, we first consider the potential magnitudes of biases that could arise from these variations in macroeconomic conditions. Census population data reveal that from July 1, 2023 to July 1, 2025, California's population grew by 1 percent less than the country as a whole (U.S. Census Bureau, 2026). Gross State Product, by contrast, grew by about 2 percentage points more in California than in the rest of the country between 2023 and 2025 (U.S. Bureau of Economic Analysis, 2026a,b). Together, this implies a roughly 3 percent greater growth in California's per capita Gross State Product relative to the rest of the United States. Available evidence points to an income elasticity of demand for limited service restaurant services around 0.3 to 0.6.[12] This approximation suggests that California's slower population growth and faster growth in per capita incomes may roughly offset one another with respect to their implications for limited service restaurant demand.[13]

[12] Hoffer et al. (2017) directly estimate an income elasticity of 0.28 for fast food. An alternative is to infer the relevant elasticity from separate estimates of the income elasticity of overall food expenditure and the share of incremental increases in food expenditure that goes to spending at limited service restaurants. Combining estimates from Dong and Stewart (2021), who estimate the first parameter to be 0.34 for non-SNAP households, with estimates from Okrent and Kumcu (2016), who estimate the second parameter to be 1.93, yields an income elasticity of demand for limited service restaurants of roughly 0.66.

[13] Restaurant expenditures as a share of income are quite stable over time, suggesting that a 1 percent increase in per capita income will tend to produce a 1 percent increase in demand for restaurant services writ large (U.S. Department of Agriculture, Economic Research Service, 2026). If income elasticities for limited services restaurants are smaller than income elasticities for full service restaurants, as seems plausible given that the higher-end restaurants may be modeled as "luxury" goods, then the income elasticity of demand for limited service restaurants may be expected to take values on the order of 0.5.

Further, there are a number of ways in which the inclusion of time-varying covariates can introduce bias in difference-in-differences specifications when they are affected by treatment (Caetano et al., 2022). Both Clemens et al. (2025) and this paper implement a reasonable approach for accounting for such biases, namely, the triple-difference estimator in Equation 2. The triple-difference estimates subtract off changes in non-minimum-wage-intensive industries, adjusting for state-level shocks under the assumption that, without AB 1228, the differences between those industries and the fast food industry would have evolved similarly in California and the control states.[14] We find that adjusting for the evolution of employment in these industries has a modest effect on the magnitude and no effect on the qualitative conclusions.[15]

The choice of the full-service restaurant sector as a comparison sector is consequential in Sosinskiy and Reich (2026)'s analysis. This is a likely invalid control group because full-service restaurants compete with limited-service restaurants in both the labor and product markets and may thus be impacted by AB 1228 through a number of channels. Sosinskiy and Reich (2026) nonetheless treat the changes in full-service restaurant employment that are estimated in Clemens et al. (2025) as though they are *ipso facto* evidence of bias. They proceed by using full-service restaurants as a within-state control group for the purpose of triple-difference estimation, which is econometrically invalid due to the spillover channels in product and labor market competition. Regarding these spillovers, we note that evidence from Clemens et al. (2026a) is strongly suggestive that AB 1228 led prices to rise in both the limited- and full-service restaurant sectors. Indeed, a calibration exercise in Clemens et al. (2026a) shows that the restaurant sector-wide price effects they find are quantitatively consistent with precisely the blend of fast-food and full-service restaurant employment effects estimated in Clemens et al. (2025).

We note an additional issue that arises from the direct incorporation of population data into analyses of the sort undertaken by Dube (2026) and by Sosinskiy and Reich (2026). The issue is that the available state and county population data are updated at an annual

[14] In October 2023, California passed Senate Bill 525, which increased minimum wages for certain health care workers. Initial implementation was delayed until October 2024 by Senate Bill 159. The higher initial wages and the length of time to be fully phased in differ by type of facility, with at least 11 different schedules specified (California Department of Industrial Relations, Division of Labor Standards Enforcement, 2026). Using back-of-the-envelope estimates from Lucia et al. (2024) on the number of affected workers, about 3% of the non-minimum-wage-intensive workforce would be impacted. The effect on the overall control group is unlikely to impact the validity of the triple-difference estimates.

[15] Sosinskiy and Reich (2026) dismiss this approach by claiming it "does not adequately account for confounding changes that affected related industries," but then reference full-service restaurants – which are not included in our control industries. In Appendix A.2, they argue that non-minimum-wage-intensive industries evolve on different trends in California and the rest of the country. They do not quantify the extent to which this might affect the results. We explicitly account for this possibility in the detrended triple-differences specifications.

frequency, whereas the employment data at the center of these analyses are either monthly or quarterly. Dube (2026) reports that the population data "are interpolated to quarterly frequency... to construct employment-to-population rates," which are his primary outcome. The population controls deployed by Sosinskiy and Reich (2026) appear to be the unadjusted annual levels. The Dube (2026) approach thus involves dividing the administrative employment census data by interpolated (and thus error-prone) population figures, whereas the Sosinskiy and Reich (2026) analysis uses a covariate that jumps artificially at an annual frequency. Neumark and Rodriguez-Lopez (2026) and Monras (2019) make the additional point that population itself may be endogenously impacted by minimum wage increases, for example if the minimum wage's effects on prices and employment opportunities contribute to out-migration.

4.3.3 Accounting for Macroeconomic Differences with Synthetic Controls

An alternative approach to accounting for differences in macroeconomic trends across states is to implement synthetic control or synthetic difference-in-differences methods. This is the primary approach taken by Dube (2026). Whether a synthetic control group successfully resolves the problem it is intended to address depends on key steps in implementation. Here we briefly discuss flaws in the counterfactual constructed using the approach taken in Dube (2026). In Clemens et al. (2026b), we provide a more extensive analysis of the sensitivity of synthetic control and synthetic difference-in-differences methods, including a set of unconventional choices in the implementation of these methods in Dube (2026).[16]

The implementation of the SDID method in Dube (2026) is said to be motivated by the "deeper problem... that California's fast-food employment rate followed a highly nonlinear path over the prior decade." Surprisingly, however, given the COVID-19 pandemic's critical role in shaping that path, Dube (2026) excludes the pandemic itself from his procedure. The result is that the procedure adopts a synthetic control group that differs substantially from California over that crucial period. As described by Dube (2026), his constructed synthetic control is "a weighted combination of Idaho (61%), Utah (22%), Pennsylvania (7%), Texas (7%), and Georgia (4%)." Readers familiar with the geography of pandemic-era employment variations may be surprised at the composition of this counterfactual, as Idaho, Utah, and Texas, in particular, had substantially different pandemic-era employment trajectories than

[16]Briefly, the estimates in Dube (2026) are toward the right tail of a large distribution of estimates created from a systematic examination of reasonable researcher choices regarding the construction of synthetic controls. Further, the empirical procedure described in Appendix C of Dube (2026) deviates in a number of dimensions from the conventional implementation of the SDID method as described in Arkhangelsky et al. (2021) and implemented in existing R and Stata packages. While some of these deviations are deliberate and explained choices, others appear to be errors. We show in Clemens et al. (2026b) that these deviations lead to estimates that differ from the conventional implementations of both SDID and synthetic control methods.

did California. Indeed, when Dube (2026) writes that "The resulting counterfactual tracks California through 2015–2023 (Figure 3)," the figure in question excludes the pandemic and immediate post-pandemic recovery. We fill in this blank in Figure A6. Using the outcome variable preferred by Dube (2026), the figure shows the substantial divergence between California and the synthetic control group constructed by Dube (2026) during the pandemic.

The scatterplot in Figure A7 focuses on variations in states' early-pandemic shocks and subsequent recoveries. Each x-axis point captures a state's fast-food employment changes over the year ending in the early pandemic low of 2020Q2, while the y-axis captures the recovery over the subsequent 3 years, which end shortly before AB 1228's enactment. The scatterplot illustrates that the initial shock and subsequent recovery are strongly correlated; employment changes from 2020Q2 through 2023Q2 are strongly predicted by the depths from which states had to recover. This is why it is important for a good control group to closely match the magnitude of California's employment decline at the pandemic's onset. Note that the green circle marking the average for the "Rest of the US" is quite close to California in this regard. By contrast, Dube (2026)'s synthetic control construction differs substantially from California's pandemic-era experience, driven in large part by the highly unusual experiences of Idaho and Utah. Rather than resolving plausibly relevant differences between California and the rest of the United States, synthetic control construction in Dube (2026) exacerbates them.

A final point is that the scatterplot in Figure A7 highlights the perils of adjusting for pre-existing trends in this context. Typical trend adjustments assume that trends estimated using pre-treatment data will continue. Figure A7, however, illustrates the important role of employment rebounds in describing cross-state variations in fast-food employment in the years preceding AB 1228's enactment. Steep rebound trajectories should not be interpreted as a long-run ongoing trend. When a state's employment has fully rebounded, its growth rate may flatten substantially. Considerations of this sort drive our preference for showing estimates across a range of transparent specifications, some of which incorporate pre-trend adjustments and others of which do not. And where we do adopt pre-trend adjustments, we purposefully estimate those trends on a relatively narrow period leading up to AB 1228's enactment so that they are driven as little as possible by the most tumultuous portion of the pandemic during which the rebound phenomenon would have been most severe.

5 Conclusion

Our analysis in this paper has explored the effects of California's $20 fast-food minimum wage on employment using data from the QCEW and QWI. At a one-year time horizon, we show that estimates generated by these data sets differ, with an employment decline of roughly 3 percent having emerged in the QCEW data and of roughly 1.5 percent in the QWI data. At a two-year time horizon, the estimates from the QCEW and QWI converge to a median decline of 3.5 percent. These two-year estimates translate into own-wage employment elasticities of slightly more negative than -0.5 and imply roughly 20,000 fewer fast-food jobs in California relative to a counterfactual in which AB 1228 was not enacted.

We provide a detailed discussion of econometric issues that are the subject of debate within the growing literature on the California fast-food minimum wage's effects. We close by emphasizing that to establish a transparent benchmark estimate of a policy change's effects, there is no replacement for unadjusted data displays like the employment time series we present in panel A of Figures 1 and 2. Our most basic estimates, the dynamic path they follow, and the absence of divergent pre-treatment trends are all captured by these initial figures. The sequence of adjustments we apply in Panels b, c, and d of Figures 1 and 2 illustrates further that detrending and seasonal adjustment are not plausible as explanations for altering the conclusion to which the simple time series figures point.

A similar lesson applies to debates over sources of econometric bias. A mere conjecture of bias should not, by itself, move readers' priors away from the most transparent possible reading of the evidence. The magnitude of conjectured biases should be quantified, and evidence should be brought directly to bear on the question of whether a proposed resolution to a conjectured bias actually resolves the problem it is intended to resolve. As argued above, the biases conjectured by Dube (2026) and by Sosinskiy and Reich (2026) are modest when quantified and, in any event, appear to be exacerbated rather than resolved by the econometric solutions they propose.

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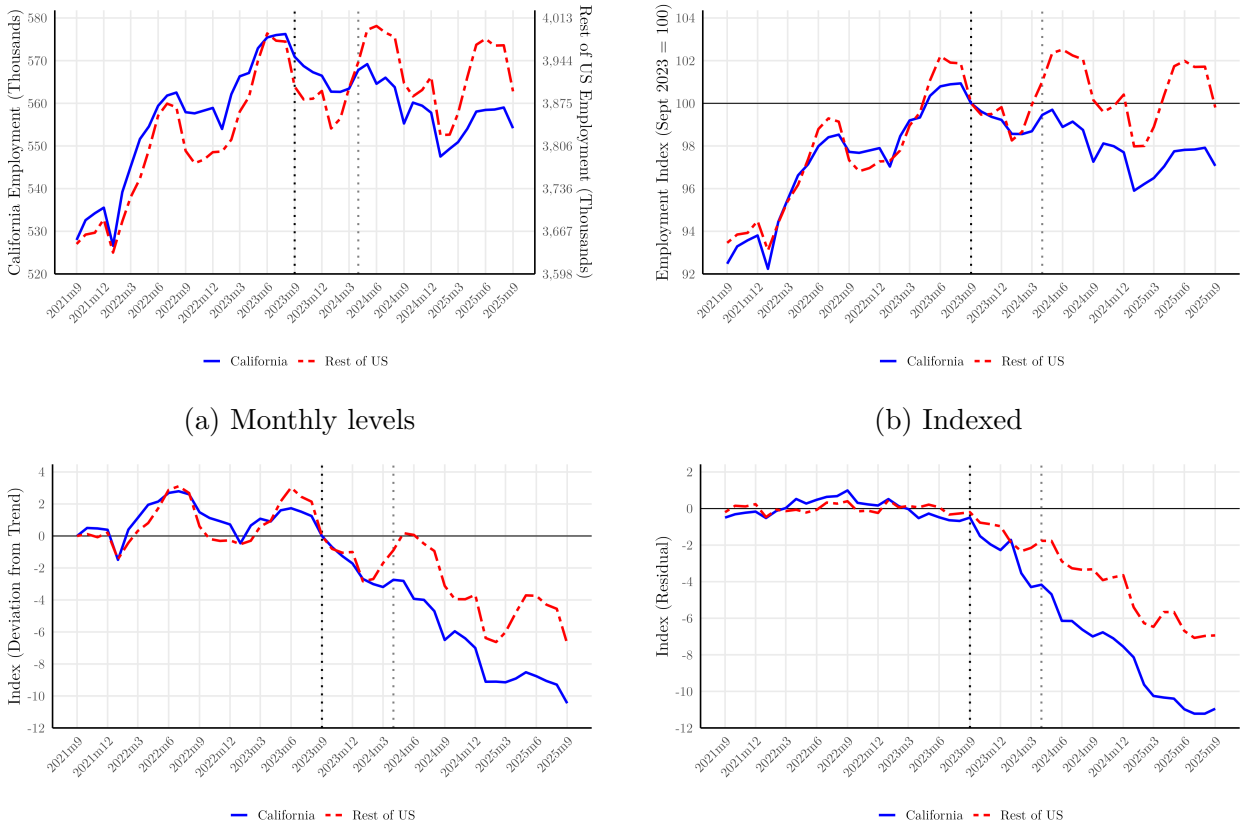
Tables and Figures



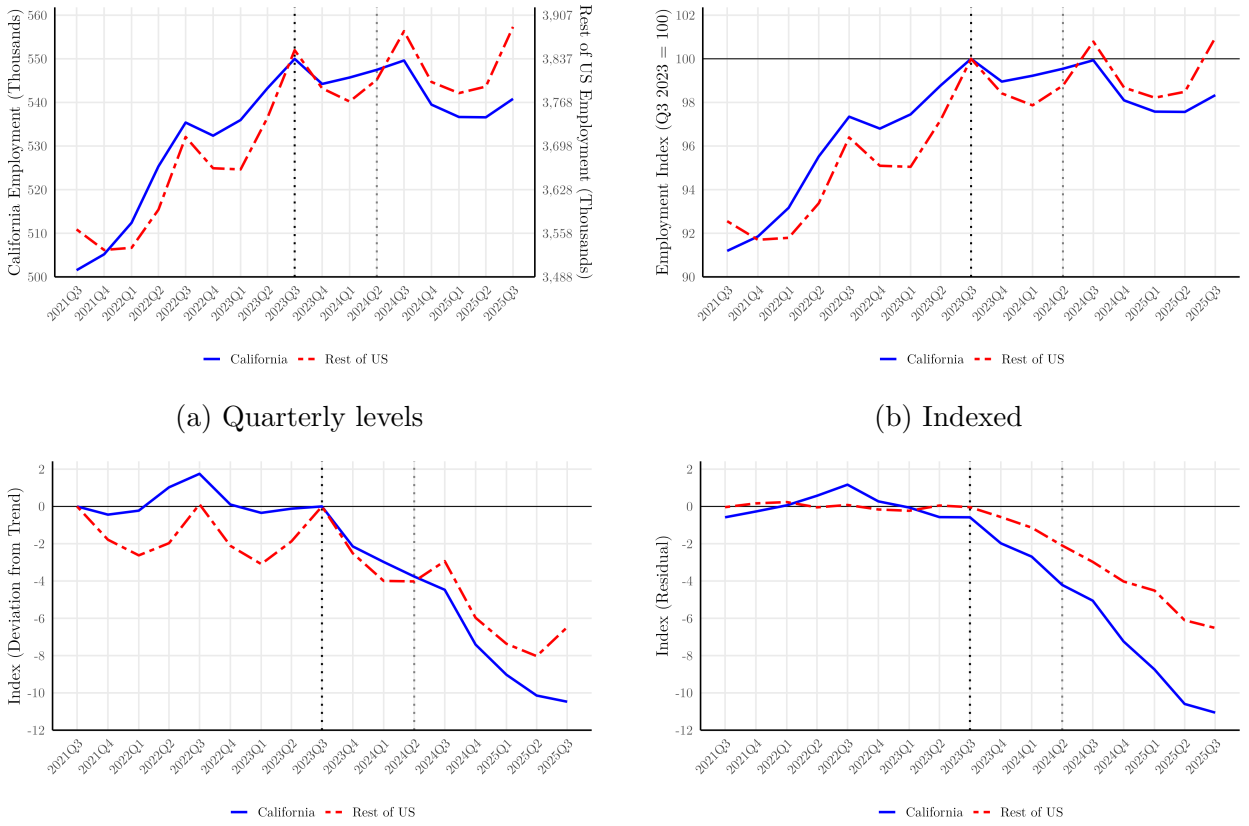
(a) Monthly levels

(b) Indexed

(c) Indexed and detrended

(d) Indexed, detrended, and deseasoned

Figure 1: **Fast-food employment trends in California versus the Rest of the United States (September 2021–September 2025).** This figure illustrates monthly fast-food employment in California and the Rest of the U.S. around the enactment of AB 1228 in September 2023, using QCEW data for NAICS 722513. Panel (a) displays raw employment levels in each region, with California on the left axis and the Rest of the U.S. on the right axis. Panel (b) indexes both series to 100 at September 2023 to emphasize relative changes following AB 1228's enactment. Panel (c) presents the series after removing the growth trend calculated over the 24 months prior to September 2023. Panel (d) adjusts the detrended series for seasonal effects using state-by-month-of-year fixed effects, which are also estimated using data over the 24 months prior to September 2023. Vertical dotted lines mark the September 28, 2023 enactment of AB 1228 (black) and the April 1, 2024 effective date (grey).



(a) Quarterly levels

(b) Indexed

(c) Indexed and detrended

(d) Indexed, detrended, and deseasoned

Figure 2: **Fast-food employment trends in California versus the Rest of the United States, QWI (2021Q3–2025Q3).** This figure illustrates quarterly fast-food employment in California and the Rest of the U.S. around the enactment of AB 1228 in September 2023, using QWI beginning-of-quarter employment (*Emp*) for NAICS 722513. The Rest of the U.S. is a balanced sample dropping Alaska and Michigan due to incomplete QWI coverage. Panel (a) displays raw employment levels in each region, with California on the left axis and the Rest of the U.S. on the right axis. Panel (b) indexes both series to 100 at 2023Q3 to emphasize relative changes following AB 1228's enactment. Panel (c) presents the series after removing the growth trend calculated over the 8 quarters prior to 2023Q3. Panel (d) adjusts the detrended series for seasonal effects using quarter-of-year fixed effects, which are also estimated using data over the 8 quarters prior to 2023Q3. Vertical dotted lines mark the September 28, 2023 enactment of AB 1228 (black) and the April 1, 2024 effective date (grey).

Table 1: **Indexed Fast-Food Employment Outcomes and Difference-in-Differences Estimates (QCEW, 1-Year).**

		(1)	(2)	(3)	(4)	(5)	(6)
Panel A							
State	**Industry**	**Sept. 2021**	**Sept. 2023**	**Sept. 2024**	**Change from 21–23 (annual)**	**Change from 23–24**	**Detrended (23–24) − (21–23)**
California	Fast Food (FF)	92.48	100.00	97.26	3.76	−2.74	−6.50
California	Higher Wage Ind. (HWD)	95.03	100.00	100.48	2.48	0.48	−2.01
Rest of US	Fast Food (FF)	93.46	100.00	100.15	3.27	0.15	−3.12
Rest of US	Higher Wage Ind. (HWD)	93.33	100.00	101.18	3.33	1.18	−2.15
Non-Increasers	Fast Food (FF)	93.18	100.00	100.49	3.41	0.49	−2.92
Non-Increasers	Higher Wage Ind. (HWD)	93.01	100.00	101.17	3.49	1.17	−2.32
Panel B							
Comparison					**Pre-Trend DD or DDD**	**Simple DD or DDD**	**Detrended DD or DDD**
HWD	CA minus Rest of US				−0.85	−0.70	0.14
	CA minus Non-Increasers				−1.01	−0.69	0.31
FF	CA minus Rest of US				0.49	−2.89**	−3.38*
	CA minus Non-Increasers				0.35	−3.23**	−3.58**
FF and HWD	CA minus Rest of US				1.34	−2.19*	−3.52
(DDD)	CA minus Non-Increasers				1.36	−2.54*	−3.90

* $p < .1$; ** $p < .05$; *** $p < .01$

Note: Using data from the QCEW, this table presents the full set of cell entries that underlie estimates of equations 1 and 2 as presented in Section 3. "Change from 21–23 (annual)" = $(\text{Index}_{2023} - \text{Index}_{2021})/2$. The "Detrended" column = (col 5) − (col 4), and analogously for the DD and DDD estimates in Panel B. September 2023 is 100 for every industry in every state by the mechanics of the index construction. The sample is a balanced panel that drops Alaska and Michigan to match the QWI sample; Non-Increasers is the 22 states that did not raise their minimum wage during the sample period. Significance levels are calculated using leave-one-out placebo inference (one-sided), where the p-value is $(b+1)/(m+1)$, with b the number of placebo states whose estimates are more negative than California's and m the number of placebo states, as described in Section 3. The full sets of placebo treatment states with estimates more negative than the California estimate are reported in Table 2.

Table 2: **QCEW Fast-Food Employment Placebo Estimates by Estimation Type, September 2024 (1-Year).**

Estimation Type	**CA Est.**	***p*-value**	**States More Negative than CA**	**# Plac.**
DD (All states)	−2.889	0.0204	—	48
DD (Non-increasers)	−3.231	0.0435	—	22
DDD (All states)	−2.186	0.0816	Delaware, Idaho, New York	48
DDD (Non-increasers)	−2.537	0.0870	Idaho	22
Detrended DD (All states)	−3.379	0.0612	Delaware, District of Columbia	48
Detrended DD (Non-increasers)	−3.581	0.0435	—	22
Detrended DDD (All states)	−3.524	0.1224	Delaware, District of Columbia, Idaho, Louisiana, New York	48
Detrended DDD (Non-increasers)	−3.895	0.1304	Idaho, Louisiana	22

Note: Placebo distributions for QCEW fast-food (NAICS 722513) September-to-September employment over the one-year window (September 2023 base, September 2024 post-period) difference-in-differences and triple-difference estimates. The DDD specifications use total private employment net of accommodation and food services (NAICS 72) and retail trade (NAICS 44–45) as the higher-wage within-state control (HWD). The "All states" pool is Rest of US (excluding Alaska and Michigan so as to match the QWI sample) and excluding California, the treatment state. The "Non-increasers" pool consists of the 22 states that did not raise their minimum wages during the sample period. The p-value is $(b+1)/(m+1)$, where b is the number of placebo states with estimates more negative than California and m is the number of placebo states, so that California counts as one draw from the placebo distribution. Placebo estimates are computed by leave-one-out aggregation (when state j is the placebo, each control aggregate excludes j from raw counts before re-indexing to its own base).

Table 3: **Indexed Fast-Food Employment Outcomes and Difference-in-Differences Estimates (QCEW, 2-Year).**

		(1)	(2)	(3)	(4)	(5)	(6)
Panel A							
State	**Industry**	**Sept. 2021**	**Sept. 2023**	**Sept. 2025**	**Change from 21–23 (annual)**	**Change from 23–25**	**Detrended (23–25) − 2×(21–23)**
California	Fast Food (FF)	92.48	100.00	97.07	3.76	−2.93	−10.46
California	Higher Wage Ind. (HWD)	95.03	100.00	100.75	2.48	0.75	−4.22
Rest of US	Fast Food (FF)	93.46	100.00	99.80	3.27	−0.20	−6.74
Rest of US	Higher Wage Ind. (HWD)	93.33	100.00	101.47	3.33	1.47	−5.20
Non-Increasers	Fast Food (FF)	93.18	100.00	100.25	3.41	0.25	−6.58
Non-Increasers	Higher Wage Ind. (HWD)	93.01	100.00	101.62	3.49	1.62	−5.37
Panel B							
Comparison					**Pre-Trend DD or DDD**	**Simple DD or DDD**	**Detrended DD or DDD**
HWD	CA minus Rest of US				−0.85	−0.72	0.98
	CA minus Non-Increasers				−1.01	−0.87	1.15
FF	CA minus Rest of US				0.49	−2.74	−3.72
	CA minus Non-Increasers				0.35	−3.18**	−3.88*
FF and HWD (DDD)	CA minus Rest of US				1.34	−2.02	−4.69
	CA minus Non-Increasers				1.36	−2.31	−5.03

* $p < .1$; ** $p < .05$; *** $p < .01$

Note: Using data from the QCEW, this table extends Table 1 to the two-year horizon (September 2025). "Change from 21–23 (annual)" = $(\text{Index}_{2023} - \text{Index}_{2021})/2$. The "Detrended" column applies twice the annual pre-trend to the two-year change: (col 5) − 2×(col 4), and analogously in Panel B. The Pre-Trend column in Panel B reports the annual pre-trend, so its entries match those in Table 1. The sample is a balanced panel that drops Alaska and Michigan to match the QWI sample; Non-Increasers is the 22 states that did not raise their minimum wage during the sample period. Significance levels are calculated using leave-one-out placebo inference (one-sided), where the p-value is $(b+1)/(m+1)$, with b the number of placebo states whose estimates are more negative than California's and m the number of placebo states, as described in Section 3. The full sets of placebo treatment states with estimates more negative than the California estimate are reported in Table 4.

Table 4: **QCEW Fast-Food Employment Placebo Estimates by Estimation Type, September 2025 (2-Year).**

Estimation Type	**CA Est.**	***p*-value**	**States More Negative than CA**	**# Plac.**
DD (All states)	−2.736	0.1020	Delaware, District of Columbia, Florida, Montana	48
DD (Non-increasers)	−3.180	0.0435	—	22
DDD (All states)	−2.018	0.1633	Delaware, Florida, Idaho, Montana, New York, South Carolina, Utah	48
DDD (Non-increasers)	−2.310	0.1739	Idaho, South Carolina, Utah	22
Detrended DD (All states)	−3.717	0.1224	Delaware, District of Columbia, Florida, Louisiana, Montana	48
Detrended DD (Non-increasers)	−3.879	0.0870	Louisiana	22
Detrended DDD (All states)	−4.694	0.1224	Delaware, District of Columbia, Idaho, Louisiana, Utah	48
Detrended DDD (Non-increasers)	−5.026	0.1739	Idaho, Louisiana, Utah	22

Note: Placebo distributions for QCEW fast-food (NAICS 722513) September-to-September employment over the two-year window (September 2023 base, September 2025 post-period) difference-in-differences and triple-difference estimates. The DDD specifications use total private employment net of accommodation and food services (NAICS 72) and retail trade (NAICS 44–45) as the higher-wage within-state control (HWD). The "All states" pool is Rest of US (excluding Alaska and Michigan so as to match the QWI sample) and excluding California, the treatment state. The "Non-increasers" pool are the 22 states that did not raise their minimum wages during the sample period. The p-value is $(b+1)/(m+1)$, where b is the number of placebo states with estimates more negative than California and m is the number of placebo states, so that California counts as one draw from the placebo distribution. Placebo estimates are computed by leave-one-out aggregation: when state j is the placebo, each control aggregate excludes j from raw counts before re-indexing to its own base.

Table 5: **Indexed Fast-Food Employment Outcomes and Difference-in-Differences Estimates (QWI, Q3 2023 Base).**

		(1)	(2)	(3)	(4)	(5)	(6)
Panel A							
State	**Industry**	**Q3 2021**	**Q3 2023**	**Q3 2024**	**Change from 21–23 (annual)**	**Change from 23–24**	**Detrended (23–24) − (21–23)**
California	Fast Food (FF)	91.19	100.00	99.93	4.40	−0.07	−4.47
California	Higher Wage Ind. (HWD)	94.91	100.00	100.59	2.54	0.59	−1.95
Rest of US	Fast Food (FF)	92.55	100.00	100.79	3.72	0.79	−2.93
Rest of US	Higher Wage Ind. (HWD)	93.12	100.00	101.27	3.44	1.27	−2.17
Non-Increasers	Fast Food (FF)	92.80	100.00	101.15	3.60	1.15	−2.45
Non-Increasers	Higher Wage Ind. (HWD)	92.80	100.00	101.22	3.60	1.22	−2.38
Panel B							
Comparison					**Pre-Trend DD or DDD**	**Simple DD or DDD**	**Detrended DD or DDD**
HWD	CA minus Rest of US				−0.89	−0.68	0.22
	CA minus Non-Increasers				−1.05	−0.62	0.43
FF	CA minus Rest of US				0.68	−0.86	−1.54
	CA minus Non-Increasers				0.80	−1.22	−2.02*
FF and HWD (DDD)	CA minus Rest of US				1.58	−0.18	−1.75
	CA minus Non-Increasers				1.86	−0.59	−2.45

* $p < .1$; ** $p < .05$; *** $p < .01$

Note: Using data from the QWI, this table presents indexed beginning-of-quarter fast-food employment (Q3 2023 = 100) and the corresponding difference-in-differences and triple-difference estimates of equations 1 and 2. "Change from 21–23 (annual)" = $(\text{Index}_{2023} - \text{Index}_{2021})/2$. The "Detrended" column = (col 5) − (col 4), and analogously for the DD and DDD estimates in Panel B. The sample is a balanced panel that drops Alaska and Michigan due to incomplete QWI coverage; Non-Increasers is the 22 states that did not raise their minimum wage during the sample period. Significance levels are calculated using leave-one-out placebo inference (one-sided), where the p-value is $(b+1)/(m+1)$, with b the number of placebo states whose estimates are more negative than California's and m the number of placebo states, as described in Section 3. The full sets of placebo treatment states with estimates more negative than the California estimate are reported in Table 6.

Table 6: **QWI Fast-Food Employment Placebo Estimates by Estimation Type, All Ages (Q3 2023 Base).**

Estimation Type	CA Est.	p-value	States More Negative than CA	# Plac.
DD (All states)	−0.856	0.3673	Arkansas, Delaware, District of Columbia, Florida, Idaho, Illinois, Indiana, Louisiana, Maryland, Massachusetts, Montana, New Mexico, New York, Oregon, South Carolina, Washington, West Virginia	48
DD (Non-increasers)	−1.216	0.3043	Arkansas, Idaho, Indiana, Louisiana, South Carolina, West Virginia	22
DDD (All states)	−0.178	0.4082	Arkansas, Delaware, District of Columbia, Florida, Idaho, Indiana, Kansas, Louisiana, Maryland, Massachusetts, Montana, New Mexico, New York, South Carolina, South Dakota, Virginia, Washington, West Virginia, Wyoming	48
DDD (Non-increasers)	−0.591	0.3913	Arkansas, Idaho, Indiana, Kansas, Louisiana, South Carolina, West Virginia, Wyoming	22
Detrended DD (All states)	−1.537	0.2245	Delaware, District of Columbia, Florida, Massachusetts, Montana, New Mexico, New York, Oregon, South Carolina, Washington	48
Detrended DD (Non-increasers)	−2.017	0.0870	South Carolina	22
Detrended DDD (All states)	−1.754	0.2449	Delaware, District of Columbia, Idaho, Louisiana, Massachusetts, Montana, New York, Oregon, South Carolina, Virginia, West Virginia	48
Detrended DDD (Non-increasers)	−2.446	0.2174	Idaho, Louisiana, South Carolina, West Virginia	22

Note: Placebo distributions for QWI fast-food (NAICS 722513) beginning-of-quarter employment (Q3 2023 base, Q3 2024 post-period) difference-in-differences and triple-difference estimates. The DDD specifications use total private employment net of accommodation and food services (NAICS 72) and retail trade (NAICS 44–45) as the higher-wage within-state control (HWD). The "All states" pool is Rest of US (dropping Alaska and Michigan for incomplete QWI coverage) and excluding California; the "Non-increasers" pool is the 22 states that did not raise their minimum wage during the sample period. The p-value is $(b+1)/(m+1)$, where b is the number of placebo states with estimates more negative than California and m is the number of placebo states, so that California counts as one draw from the placebo distribution. Placebo estimates are computed by leave-one-out aggregation: when state j is the placebo, each control aggregate excludes j from raw counts before re-indexing to its own base.

Table 7: **Indexed Fast-Food Employment Outcomes and Difference-in-Differences Estimates (QWI, Q3 2023 Base, 2-Year).**

		(1)	(2)	(3)	(4)	(5)	(6)
Panel A							
State	**Industry**	**Q3 2021**	**Q3 2023**	**Q3 2025**	**Change from 21–23 (annual)**	**Change from 23–25**	**Detrended (23–25) − 2×(21–23)**
California	Fast Food (FF)	91.19	100.00	98.33	4.40	−1.67	−10.47
California	Higher Wage Ind. (HWD)	94.91	100.00	100.90	2.54	0.90	−4.19
Rest of US	Fast Food (FF)	92.55	100.00	100.97	3.72	0.97	−6.48
Rest of US	Higher Wage Ind. (HWD)	93.12	100.00	101.99	3.44	1.99	−4.89
Non-Increasers	Fast Food (FF)	92.80	100.00	101.37	3.60	1.37	−5.83
Non-Increasers	Higher Wage Ind. (HWD)	92.80	100.00	102.10	3.60	2.10	−5.10
Panel B							
Comparison					**Pre-Trend DD or DDD**	**Simple DD or DDD**	**Detrended DD or DDD**
HWD	CA minus Rest of US				−0.89	−1.09	0.70
	CA minus Non-Increasers				−1.05	−1.20	0.90
FF	CA minus Rest of US				0.68	−2.63⋆	−4.00⋆
	CA minus Non-Increasers				0.80	−3.04⋆⋆	−4.64⋆⋆
FF and HWD (DDD)	CA minus Rest of US				1.58	−1.54	−4.69⋆
	CA minus Non-Increasers				1.86	−1.83	−5.55⋆

⋆ $p < .1$; ⋆⋆ $p < .05$; ⋆⋆⋆ $p < .01$

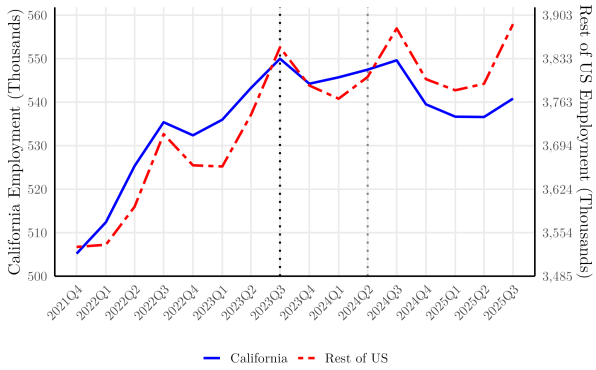
Note: Using data from the QWI, this table presents indexed beginning-of-quarter fast-food employment (Q3 2023 = 100) and the corresponding difference-in-differences and triple-difference estimates of equations 1 and 2. "Change from 21–23 (annual)" = $(\text{Index}_{2023} - \text{Index}_{2021})/2$. The "Detrended" column = (col 5) − 2×(col 4), and analogously for the DD and DDD estimates in Panel B. The sample is a balanced panel that drops Alaska and Michigan due to incomplete QWI coverage; Non-Increasers is the 22 states that did not raise their minimum wage during the sample period. Significance levels are calculated using leave-one-out placebo inference (one-sided), where the p-value is $(b+1)/(m+1)$, with b the number of placebo states whose estimates are more negative than California's and m the number of placebo states, as described in Section 3. The full sets of placebo treatment states with estimates more negative than the California estimate are reported in Table 8.

Table 8: **QWI Fast-Food Employment Placebo Estimates by Estimation Type, All Ages (Q3 2023 Base, 2-Year).**

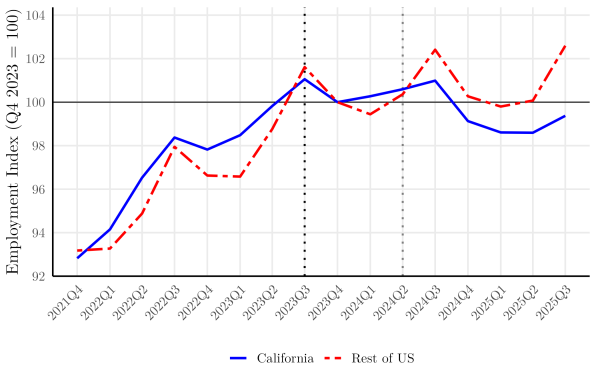
Estimation Type	**CA Est.**	***p*-value**	**States More Negative than CA**	**# Plac.**
DD (All states)	−2.633	0.0816	District of Columbia, Montana, Washington	48
DD (Non-increasers)	−3.038	0.0435	—	22
DDD (All states)	−1.542	0.2653	Arkansas, Delaware, Florida, Idaho, Louisiana, Montana, New Mexico, New York, South Carolina, Utah, Washington, West Virginia	48
DDD (Non-increasers)	−1.835	0.3043	Arkansas, Idaho, Louisiana, South Carolina, Utah, West Virginia	22
Detrended DD (All states)	−3.996	0.0816	Delaware, District of Columbia, Massachusetts	48
Detrended DD (Non-increasers)	−4.642	0.0435	—	22
Detrended DDD (All states)	−4.693	0.0816	Delaware, District of Columbia, Louisiana	48
Detrended DDD (Non-increasers)	−5.545	0.0870	Louisiana	22

Note: Placebo distributions for QWI fast-food (NAICS 722513) beginning-of-quarter employment over the two-year window (Q3 2023 base, Q3 2025 post-period) difference-in-differences and triple-difference estimates. The DDD specifications use total private employment net of accommodation and food services (NAICS 72) and retail trade (NAICS 44–45) as the higher-wage within-state control (HWD). The "All states" pool is Rest of US (dropping Alaska and Michigan for incomplete QWI coverage) and excluding California; the "Non-increasers" pool is the 22 states that did not raise their minimum wage during the sample period. The p-value is $(b+1)/(m+1)$, where b is the number of placebo states with estimates more negative than California and m is the number of placebo states, so that California counts as one draw from the placebo distribution. Placebo estimates are computed by leave-one-out aggregation: when state j is the placebo, each control aggregate excludes j from raw counts before re-indexing to its own base.

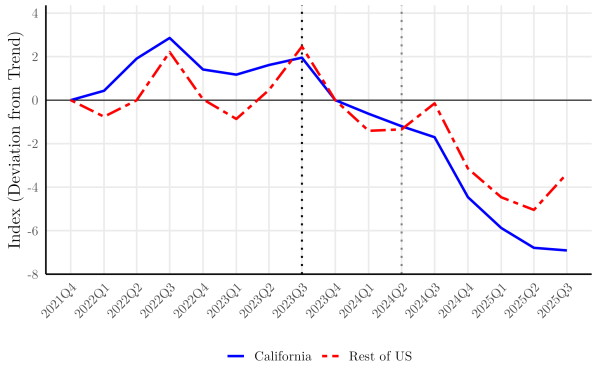
A Additional Tables and Figures



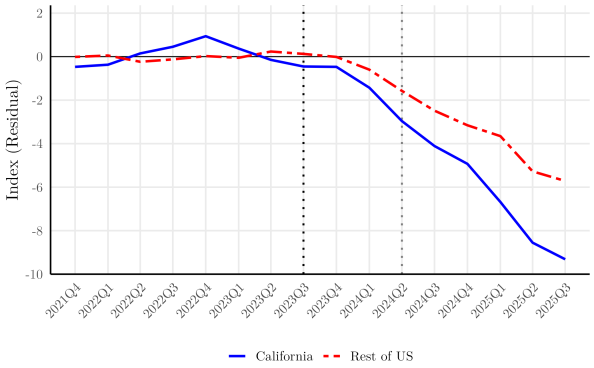
(a) Quarterly levels



(b) Indexed



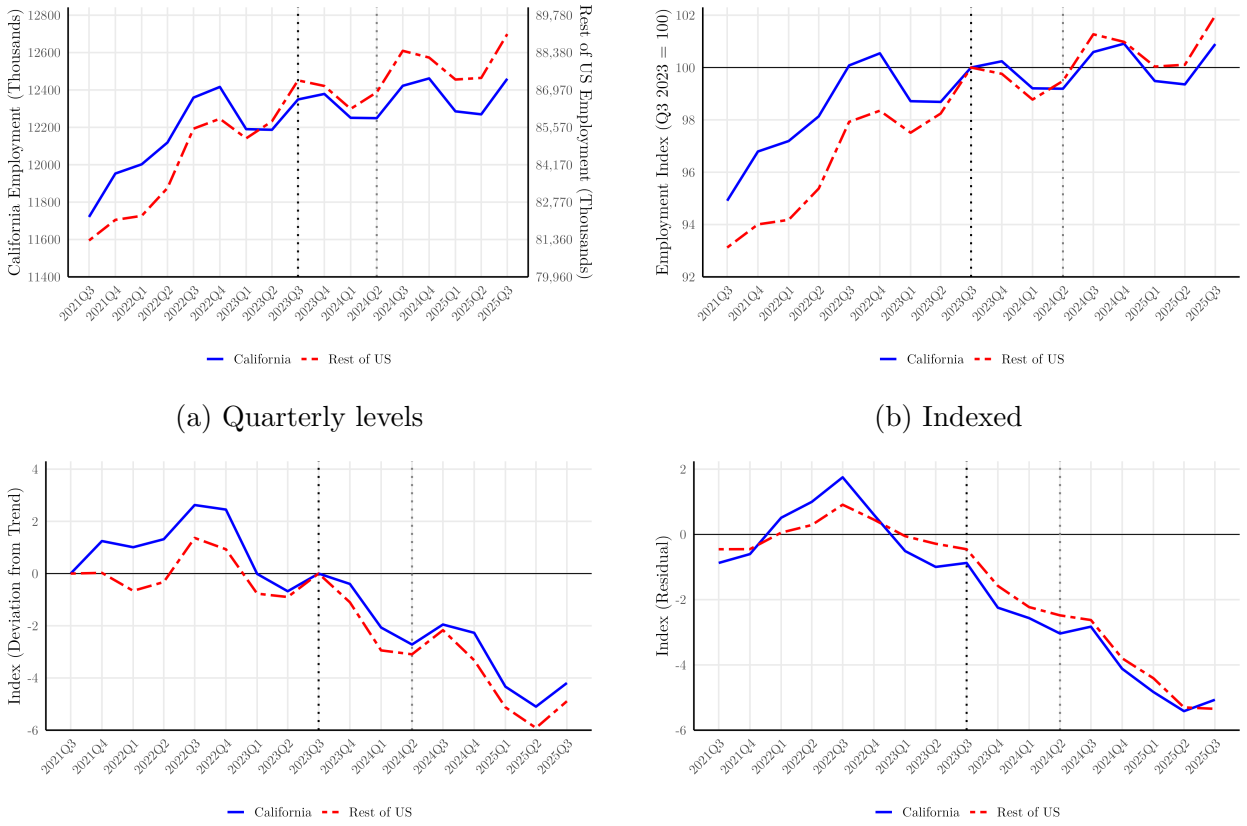
(c) Indexed and detrended



(d) Indexed, detrended, and deseasoned

Figure A1: **Fast-food employment trends, California versus the Rest of the United States, Q4 2023 base (QWI).**

Notes: As Figure 2, but indexed to 2023Q4 = 100 (the base period used by Dube, 2026) and detrended over the eight quarters from 2021Q4 to 2023Q4. California (blue solid) and Rest of US (red dashed), measured as beginning-of-quarter employment (*Emp*) in NAICS 722513 from the QWI. The Rest of US aggregate is the balanced sample dropping Alaska and Michigan due to incomplete QWI coverage. Vertical dotted lines mark the September 28, 2023 enactment of AB 1228 (black) and the April 1, 2024 effective date (grey).



(a) Quarterly levels

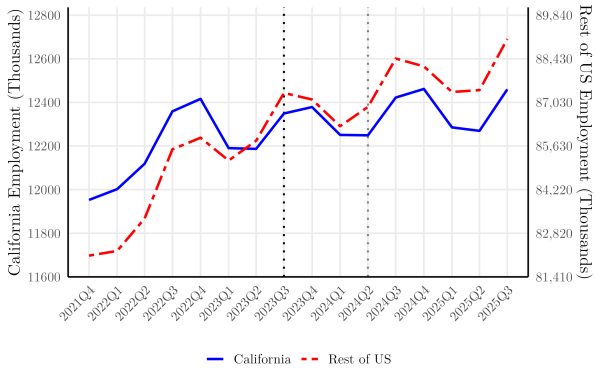
(b) Indexed

(c) Indexed and detrended

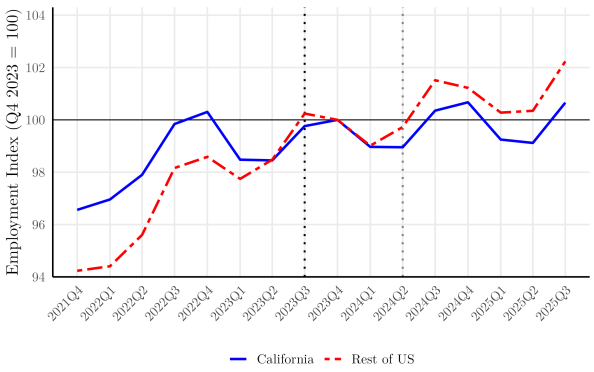
(d) Indexed, detrended, and deseasoned

Figure A2: **Higher-wage industry employment trends, California versus the Rest of the United States (QWI, Q3 2023 base).**

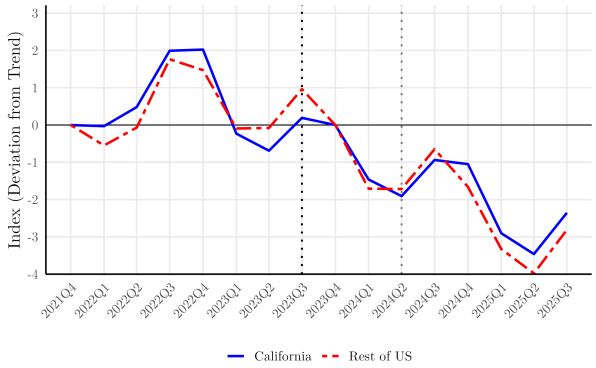
Notes: California (blue solid) and Rest of US (red dashed), measured as beginning-of-quarter employment (*Emp*) in the higher-wage industries aggregate (total private employment less accommodation/food services [NAICS 72] and retail trade [NAICS 44–45]) from the QWI. The Rest of US aggregate is the balanced sample dropping Alaska and Michigan due to incomplete QWI coverage. Panel (a) shows raw quarterly levels (California on the left axis, Rest of US on the right). Panels (b)–(d) index each series so that 2023Q3 = 100. Panel (c) subtracts a growth trend estimated over the eight quarters from 2021Q3 to 2023Q3. Panel (d) additionally subtracts a quarter-of-year seasonal component. Vertical dotted lines mark the September 28, 2023 enactment of AB 1228 (black) and the April 1, 2024 effective date (grey).



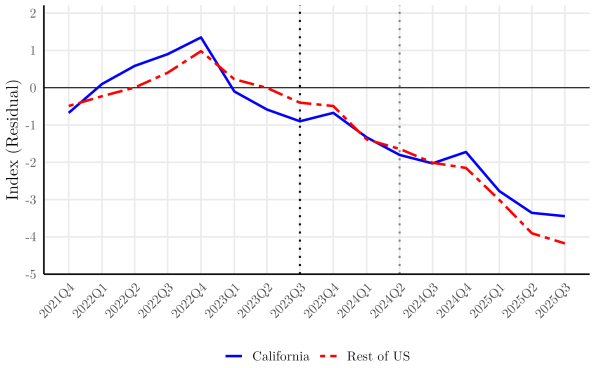
(a) Quarterly levels



(b) Indexed



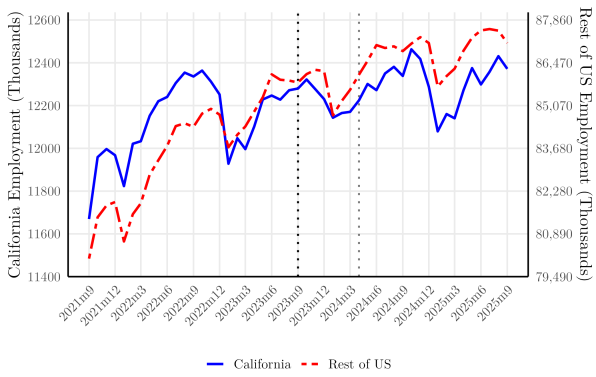
(c) Indexed and detrended



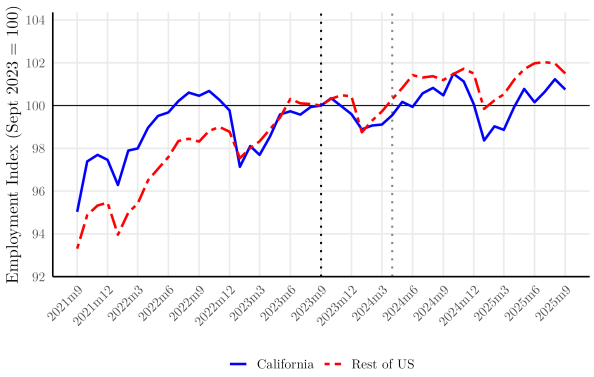
(d) Indexed, detrended, and deseasoned

Figure A3: **Higher-wage industry employment trends, California versus the Rest of the United States (QWI, Q4 2023 base).**

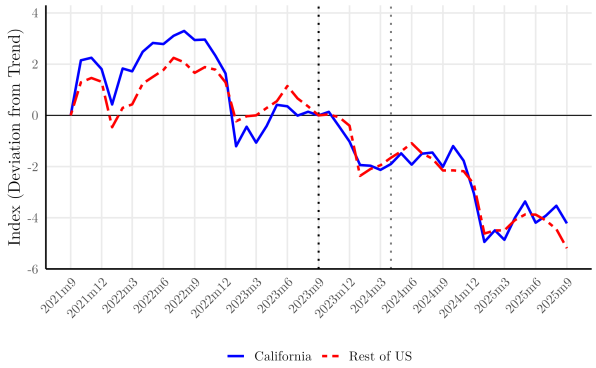
Notes: As Figure A2, but indexed to 2023Q4 = 100 (the base period used by Dube, 2026) and detrended over the eight quarters from 2021Q4 to 2023Q4. California (blue solid) and Rest of US (red dashed), measured as beginning-of-quarter employment (*Emp*) in the higher-wage industries aggregate (total private employment less accommodation/food services [NAICS 72] and retail trade [NAICS 44–45]) from the QWI. The Rest of US aggregate is the balanced sample dropping Alaska and Michigan. Vertical dotted lines mark the September 28, 2023 enactment of AB 1228 (black) and the April 1, 2024 effective date (grey).



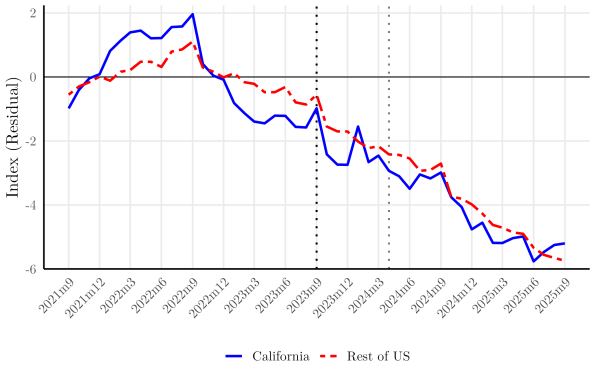
(a) Monthly levels



(b) Indexed



(c) Indexed and detrended



(d) Indexed, detrended, and deseasoned

Figure A4: **Higher-wage industry employment trends, California versus the Rest of the United States (QCEW, September 2021–September 2025).**

Notes: California (blue solid) and Rest of US (red dashed), measured as total monthly employment in the higher-wage industries aggregate (total private employment less accommodation/food services [NAICS 72] and retail trade [NAICS 44–45]) from the QCEW. Panel (a) shows raw monthly levels (California on the left axis, Rest of US on the right). Panels (b)–(d) index each series so that September 2023 = 100. Panel (c) subtracts a growth trend estimated over the 24 months from September 2021 to September 2023. Panel (d) additionally subtracts a month-of-year seasonal component estimated over the same pre-period. Vertical dotted lines mark the September 28, 2023 enactment of AB 1228 (black) and the April 1, 2024 effective date (grey).

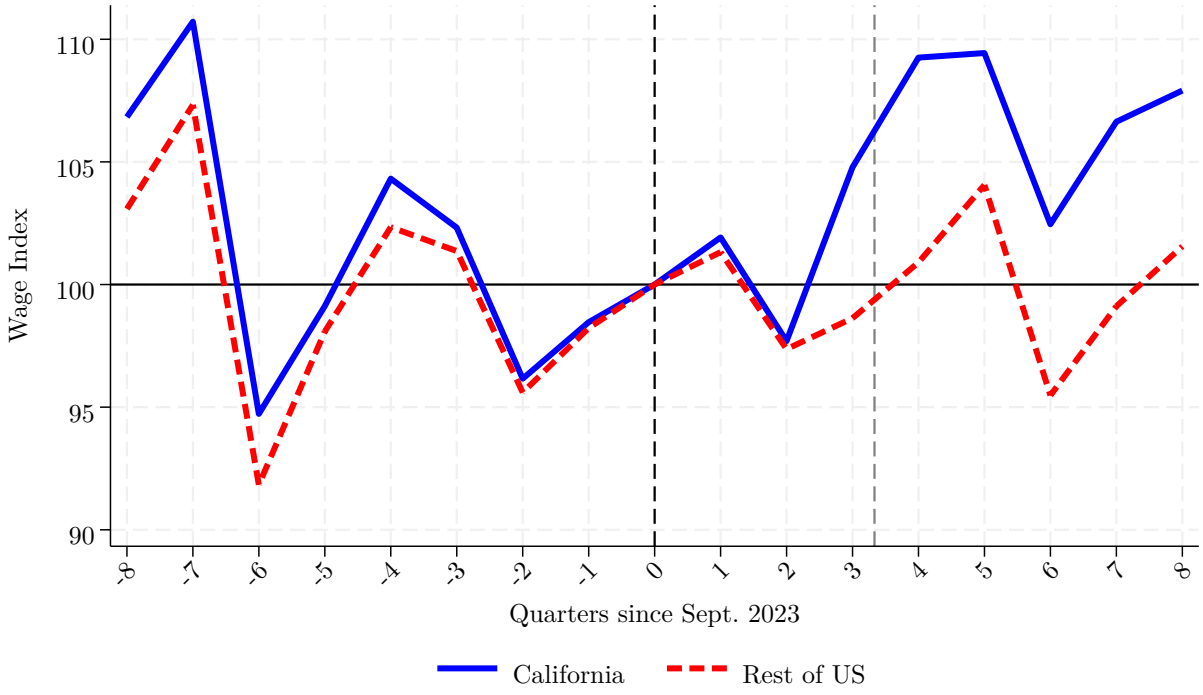
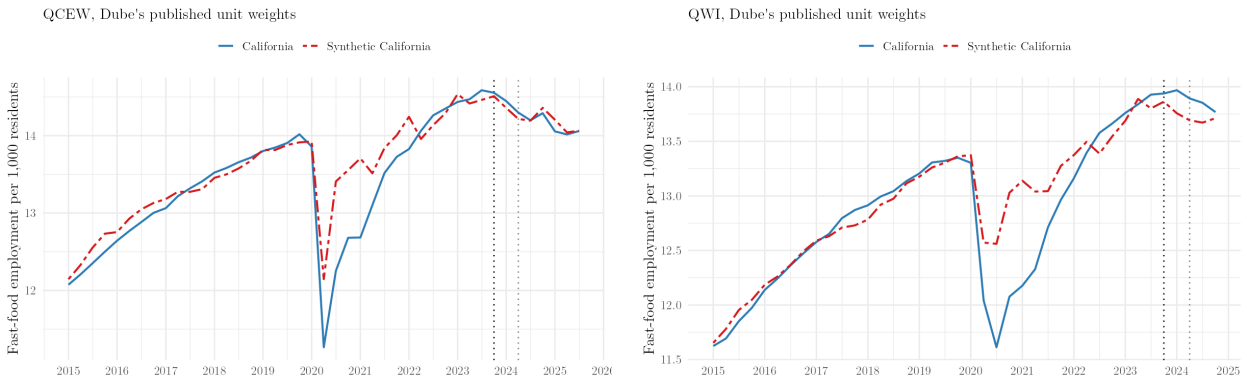


Figure A5: **Indexed Fast-Food Weekly Wages, California versus the Rest of the United States (QCEW, 2021Q3–2025Q3).**

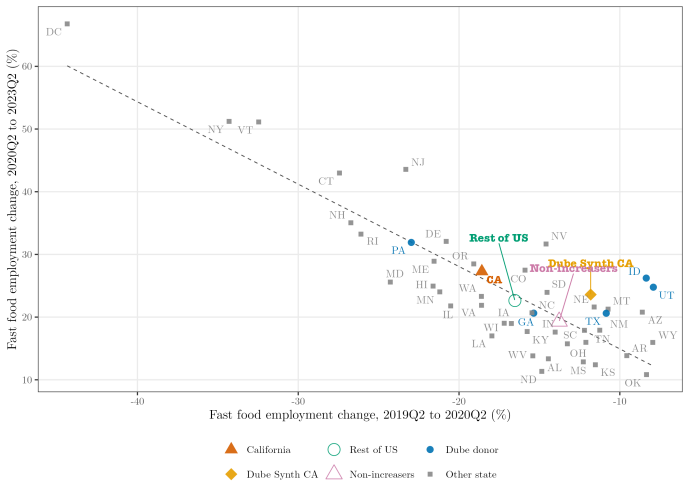
Notes: This figure shows the (real) average weekly wage index for fast food (NAICS 722513) using QCEW data. The figure shows time series data after indexing the weekly wages to 100 at 2023Q3. The sample is a balanced panel that drops Alaska and Michigan to match the QWI sample. Nominal weekly wage values are deflated using CPI-U. Vertical dotted lines mark the September 28, 2023 enactment of AB 1228 (black) and the April 1, 2024 effective date (grey).



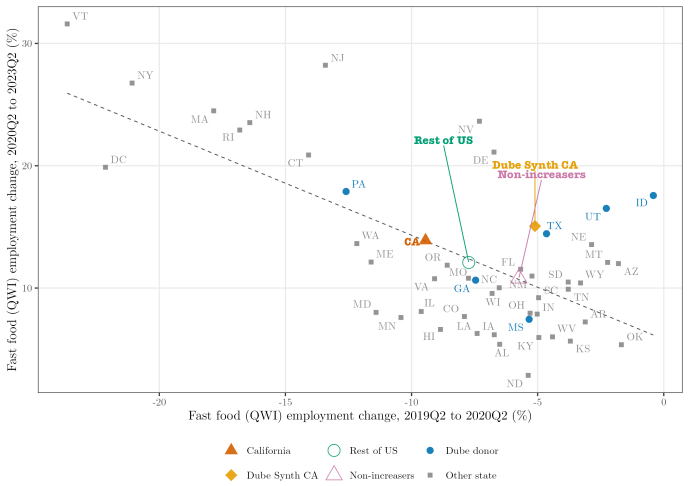
(a) QCEW (b) QWI

Figure A6: **California and Synthetic California Under Dube (2026)'s Published Unit Weights.**

Notes: Seasonally adjusted fast-food employment per *1000* residents, in levels, corresponding to Figure 3 in Dube (2026). The synthetic control applies weights from his Footnote 14. Dotted verticals mark the 2023Q4 base and the April 2024 effective date.



(a) QCEW



(b) QWI

Figure A7: **COVID shock and recovery in state fast-food employment.**

Notes: Each marker plots a state's percentage change in fast-food (NAICS 722513) employment during the COVID shock (2019Q2 to 2020Q2, x-axis) against the subsequent recovery (2020Q2 to 2023Q2, y-axis). The recovery window ends at 2023Q2 to capture cross-state variation in post-pandemic dynamics prior to any AB 1228 anticipation. Panel (a) uses QCEW data (within-quarter average of the three monthly employment counts, private ownership); Panel (b) uses QWI Emp (beginning-of-quarter point-in-time employment stock) from the R2026Q1 release. Sample: California plus the balanced panel of jurisdictions remaining after dropping Alaska and Michigan. "Rest of US" is the employment-weighted aggregate across all non-California jurisdictions in the Panel. "Non-increasers" is the same aggregate computed over the 22 non-increaser states (Dube (2026)'s 20 federal-$7.25 states plus Arkansas and West Virginia). "Synth CA" applies Dube's (NBER WP 35171, footnote 14) SDID unit weights to each donor state's (shock, recovery) coordinates: in QCEW, 0.61 ID / 0.22 UT / 0.07 PA / 0.07 TX / 0.04 GA; in QWI, 0.51 ID / 0.17 UT / 0.10 PA / 0.14 TX / 0.03 GA / 0.04 MS. "Dube donor" marks his 20 federal-$7.25 donor states individually. The dashed line is an OLS fit across the individual state observations, excluding California and the three aggregate points.

Table A1: **Fast-Food Employment Change in QCEW from Raw Counts**

	(1)	(2)	(3)	(4)	(5)
Panel A: Raw Fast-Food Employment					
Region	**Sept. 2023 Emp.**	**Sept. 2024 Emp.**	**Indexed (Sept 23 = 100)**	**Pct. Change**	**Log Change**
California	570,909	555,273	97.26	−2.74	−2.78
Rest of US (bal. panel)	3,901,843	3,907,713	100.15	0.15	0.15
Non-Increasers (22)	1,974,813	1,984,533	100.49	0.49	0.49
$7.25 states (Dube 20)	1,902,294	1,912,189	100.52	0.52	0.52
Panel B: Basic Sept.-to-Sept. DD					
Comparison			**DD: Indexed (pp)**	**DD: Log (× 100)**	**Dube Reported**
CA − Rest of US (bal. panel)			−2.89	−2.93	−2.9
CA − Non-Increasers (22)			−3.23	−3.27	—
CA − $7.25 states (Dube 20)			−3.26	−3.30	—

Note: Using data from the QCEW, this table reports raw September fast-food employment (NAICS 722513) summed across the states in each region in September 2023 and September 2024, and the basic September-to-September difference-in-differences. The "Indexed" column sets September 2023 = 100; "Pct. Change" is column (3) minus 100; "Log Change" is the log difference times 100. The "Dube Reported" column gives the result from Table D1 Row 2 Column (4) in Dube (2026), a September-to-September difference-in-differences against Rest-of-US controls without detrending. Dube (2026) does not report a difference-in-differences estimate with Non-Increaser or $7.25-state control groups, so those cells are blank. The sample is a balanced panel that drops Alaska and Michigan to match the QWI sample. Non-Increasers are the 22 states that did not raise their minimum wage during the sample period, used in Clemens et al. (2025). The $7.25 states are the 20 states at the federal minimum wage throughout the sample, Dube (2026)'s control group.

Table A2: **Fast-Food Employment Change in QWI from Raw Counts**

	(1)	(2)	(3)	(4)	(5)
Panel A: Raw Fast-Food Employment					
Region	**Q3 2023 Emp.**	**Q3 2024 Emp.**	**Indexed (Q3 23 = 100)**	**Pct. Change**	**Log Change**
California	549,980	549,612	99.93	−0.07	−0.07
Rest of US (bal. panel)	3,851,097	3,881,467	100.79	0.79	0.79
Non-Increasers (22)	1,939,899	1,962,184	101.15	1.15	1.14
$7.25 states (Dube 20)	1,867,203	1,890,328	101.24	1.24	1.23
Panel B: Basic Q3-to-Q3 DD					
Comparison			**DD: Indexed (pp)**	**DD: Log (× 100)**	**Dube Reported**
CA − Rest of US (bal. panel)			−0.86	−0.85	+1.50
CA − Non-Increasers (22)			−1.22	−1.21	—
CA − $7.25 states (Dube 20)			−1.31	−1.30	—

Note: Using data from the QWI, this table reports raw Q3 fast-food employment (NAICS 722513) summed across the states in each region in Q3 2023 and Q3 2024, and the basic Q3-to-Q3 difference-in-differences. The "Indexed" column sets Q3 2023 = 100; "Pct. Change" is column (3) minus 100; "Log Change" is the log difference times 100. The "Dube Reported" column gives the result from Table D2 Row 2 Column (4) in Dube (2026), a Q3-to-Q3 difference-in-differences against Rest-of-US controls without detrending. Dube (2026) does not report a difference-in-differences estimate with Non-Increaser or $7.25-state control groups, so those cells are blank. The sample is a balanced panel without Alaska and Michigan due to incomplete QWI coverage. Non-Increasers are the 22 states that did not raise their minimum wage during the sample period, used in Clemens et al. (2025). The $7.25 states are the 20 states at the federal minimum wage throughout the sample, Dube (2026)'s control group.

Table A3: **Indexed Fast-Food Employment Outcomes and Difference-in-Differences Estimates (QWI, Q4 2023 Base).**

		(1)	(2)	(3)	(4)	(5)	(6)
Panel A							
State	**Industry**	**Q4 2021**	**Q4 2023**	**Q4 2024**	**Change from 21–23 (annual)**	**Change from 23–24**	**Detrended (23–24) − (21–23)**
California	Fast Food (FF)	92.82	100.00	99.13	3.59	−0.87	−4.46
California	Higher Wage Ind. (HWD)	96.56	100.00	100.67	1.72	0.67	−1.05
Rest of US	Fast Food (FF)	93.18	100.00	100.27	3.41	0.27	−3.14
Rest of US	Higher Wage Ind. (HWD)	94.23	100.00	101.22	2.88	1.22	−1.66
Non-Increasers	Fast Food (FF)	92.78	100.00	100.69	3.61	0.69	−2.93
Non-Increasers	Higher Wage Ind. (HWD)	93.88	100.00	101.08	3.06	1.08	−1.98
Panel B							
Comparison					**Pre-Trend DD or DDD**	**Simple DD or DDD**	**Detrended DD or DDD**
HWD	CA minus Rest of US				−1.16	−0.55	0.61
	CA minus Non-Increasers				−1.34	−0.40	0.94
FF	CA minus Rest of US				0.18	−1.14	−1.32
	CA minus Non-Increasers				−0.02	−1.56	−1.53
FF and HWD (DDD)	CA minus Rest of US				1.34	−0.59	−1.93
	CA minus Non-Increasers				1.32	−1.15	−2.47

$^{\star}\, p < .1$; $^{\star\star}\, p < .05$; $^{\star\star\star}\, p < .01$

Note: Using data from the QWI, this table presents indexed beginning-of-quarter fast-food employment (Q4 2023 = 100) and the corresponding difference-in-differences and triple-difference estimates of equations 1 and 2. "Change from 21–23 (annual)" = $(\text{Index}_{2023} - \text{Index}_{2021})/2$. The "Detrended" column = (col 5) − (col 4), and analogously for the DD and DDD estimates in Panel B. The sample is a balanced panel that drops Alaska and Michigan due to incomplete QWI coverage; Non-Increasers is the 22 states that did not raise their minimum wage during the sample period. Significance levels are calculated using leave-one-out placebo inference (one-sided), where the p-value is $(b+1)/(m+1)$, with b the number of placebo states whose estimates are more negative than California's and m the number of placebo states, as described in Section 3. The full sets of placebo treatment states with estimates more negative than the California estimate are reported in Table A4.

Table A4: **QWI Fast-Food Employment Placebo Estimates by Estimation Type (Q4 2023 Base).**

Estimation Type	**CA Est.**	***p*-value**	**States More Negative than CA**	**# Plac.**
DD (All states)	−1.139	0.2245	Colorado, Delaware, District of Columbia, Florida, Indiana, Louisiana, New York, Utah, Washington, West Virginia	48
DD (Non-increasers)	−1.557	0.2174	Indiana, Louisiana, Utah, West Virginia	22
DDD (All states)	−0.587	0.3061	Arkansas, Colorado, Delaware, District of Columbia, Florida, Idaho, Louisiana, Maryland, Montana, New York, Oregon, Utah, Washington, West Virginia	48
DDD (Non-increasers)	−1.152	0.3043	Arkansas, Idaho, Louisiana, South Carolina, Utah, West Virginia	22
Detrended DD (All states)	−1.317	0.1837	Colorado, Delaware, District of Columbia, Florida, Massachusetts, Oregon, Utah, West Virginia	48
Detrended DD (Non-increasers)	−1.534	0.2174	Louisiana, North Carolina, Utah, West Virginia	22
Detrended DDD (All states)	−1.928	0.1837	Colorado, Delaware, District of Columbia, Louisiana, Massachusetts, New York, Oregon, Utah	48
Detrended DDD (Non-increasers)	−2.471	0.1304	Louisiana, Utah	22

Note: Placebo distributions for QWI fast-food (NAICS 722513) beginning-of-quarter employment (Q4 2023 base, Q4 2024 post-period) difference-in-differences and triple-difference estimates. The DDD specifications use total private employment net of accommodation and food services (NAICS 72) and retail trade (NAICS 44–45) as the higher-wage within-state control (HWD). The "All states" pool is Rest of US (dropping Alaska and Michigan for incomplete QWI coverage) and excluding California; the "Non-increasers" pool is the 22 states that did not raise their minimum wage during the sample period. The p-value is $(b+1)/(m+1)$, where b is the number of placebo states with estimates more negative than California and m is the number of placebo states, so that California counts as one draw from the placebo distribution. Placebo estimates are computed by leave-one-out aggregation: when state j is the placebo, each control aggregate excludes j from raw counts before re-indexing to its own base.