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LABOR MARKET EFFECTS OF CALIFORNIA'S $20 FAST-FOOD MINIMUM WAGE

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ABSTRACT

California's AB 1228 raised the minimum wage for large fast-food chains to $20 per hour in April 2024—roughly 77 percent of the state's median hourly wage, the highest wage floor for fast-food workers in the U.S. Using QCEW data through 2025Q3, I estimate that the policy raised fast-food wages by about 7 percent. A conventional difference-in-differences yields an employment own-wage elasticity (OWE) of −0.19; synthetic difference-in-differences, which reweights controls to match California's pretreatment trajectory, shrinks the OWE to −0.04. Newly available QWI data through 2024Q4 yield estimates that are on average more positive. Across 32 QCEW and QWI specifications, the OWE ranges from −0.29 to +0.26, bracketing the median OWE of −0.02 I compute across 26 post-2010 state minimum-wage events despite AB 1228's much larger bite. The QWI also reveals a sharp reduction in the separation rate, with own-wage elasticities of −1.7 to −4.2—several times the restaurant-sector benchmark in Dube, Lester, and Reich (2016) and consistent with a monopsonistic quit-reduction channel. Wage and separation-rate effects concentrate among large employers covered by AB 1228, with limited spillovers. The fall in separations also helps reconcile the somewhat more negative QCEW employment estimates.

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1 Introduction

California's Assembly Bill 1228 raised the minimum wage for fast-food chain workers—those employed by firms with 60 or more U.S. locations—to $20 per hour effective April 1, 2024. At roughly 77 percent of the state's median hourly wage (a Kaitz ratio of 0.77), this is the highest minimum wage affecting fast-food workers in the United States. Independent restaurants and full-service establishments remained covered by California's general $16 minimum. The partial coverage creates an unusually demanding test of minimum-wage effects: consumers can substitute toward uncovered competitors, and employers face incentives to restructure operations across covered and uncovered categories. These features—high bite combined with partial coverage—make AB 1228 a sharp test of disemployment effects from a high minimum wage.

I use two sources of employment data. The Quarterly Census of Employment and Wages (QCEW) is a state-industry establishment count built from UI records on workers receiving pay during the pay period containing the 12th of each month, and runs through 2025Q3. I additionally use the LEHD Quarterly Workforce Indicators (QWI), which link a worker's UI wage records across employers to construct a beginning-of-quarter point-in-time employment stock and a rich set of worker-flow margins, through 2024Q4. The 5- and 6-digit NAICS QWI files needed to identify fast food were released by the Census Bureau only in 2026, and were not used in prior studies. The two measures respond differently to within-quarter turnover, an asymmetry I discuss in detail in §4.3.

Two recent papers, Clemens, Edwards, and Meer (2025) and Sosinskiy and Reich (2026), study AB 1228 in QCEW alone and reach somewhat different conclusions on the policy effect on employment. CEM find a median own-wage elasticity (OWE) of -0.40, while SR find a preferred -0.12 estimate. CEM's -0.40 sits exactly at the boundary between what Dube and Lindner (2024) classify as "small" (OWE less negative than -0.4) and "medium negative" (OWE between -0.4 and -0.8), while SR's -0.12 falls comfortably inside the "small" range. The gap between the two is informative. A central driver is the choice of within-state comparison sector for triple-differencing (DDD): CEM use all non-fast-food employment; SR use full-service restaurants. My analysis of the QCEW data reproduces this sensitivity. A DDD using full-service restaurants yields an OWE of -0.05; using all non-fast-food employment yields -0.29; an intermediate low-wage non-fast-food composite yields -0.20, close to the DD estimate of -0.19.

A priori, it is difficult to know which of these internal controls is more sensible. A DDD comparison using non-fast-food employment may be overly broad. At the same time, a DDD using full-service restaurants may be more exposed to possible spillover effects. And use

of DDD additionally imposes a parallel-trends restriction on the chosen (internal control) sector. A yet deeper problem is that California's fast-food employment rate followed a highly nonlinear path over the prior decade: well below the control-state average in the mid-2010s, converging gradually through 2019, rebounding sharply in 2021–2022 following the pandemic, and plateauing near parity by 2023 (Figure 1). Importantly, none of the three DDDs eliminates this trajectory in the extended pre-period (Figure 3).[1]

I take a different approach to the pre-trend problem with synthetic difference-in-differences (SDID): it reweights control states and pre-treatment quarters to match California's trajectory directly, without selecting an internal comparison sector. The resulting counterfactual tracks California through 2015–2023 (Figure 2), and the employment effect is small (-0.04, $p = 0.50$). Applying the same synthetic reweighting to each DDD outcome ("SDDD") drives the three OWEs to $+0.01$, -0.03, and $+0.01$—the comparison-sector sensitivity dissolves once the pre-treatment trajectory is matched.[2]

The QWI estimates are more positive still. Across 16 QWI specifications, wage effects range from $+5.0$ to $+8.8$ percent and employment effects from -0.6 to $+1.7$ percent; 12 of 16 employment estimates are non-negative, none individually distinguishable from zero, and the implied OWE ranges from -0.10 to $+0.26$. The headline DD OWE is -0.04 and the SDID OWE is $+0.13$. The DDD-versus-DDD sensitivity that drives the CEM–SR gap inthe QCEW data is less pronounced in the QWI data.

The QWI data also show that quarterly separations in California fast food fall sharply by 13 to 25 log points across the 16 QWI specifications, implying separation OWEs of -1.7 to -4.2. These are roughly 1.5 to 4 times the restaurant-sector benchmark of -1.1 in Dube, Lester, and Reich (2016). This pattern is consistent with a monopsonistic quit-reduction channel in which higher wages lower turnover and thereby mute the employment response. Disaggregating by firm size sharpens the picture further: wage effects are 1.1 to 2.4 percent in the smallest size bin, where AB 1228 does not bind, and 10.4 to 11.8 percent in the 500+ bin most clearly covered, with separation-rate declines concentrated in the same bin. The policy bites where it is designed to bite, with limited spillovers.

[1] CEM address the dynamics by fitting a linear trend over September 2021–September 2023 and extrapolating forward; SR construct Rambachan and Roth (2023) bounds anchored on the largest pre-2024 deviation. Both are disciplined responses, but a short-window linear extrapolation across a nonlinear decade is fragile, and the identifying power of the pre-treatment bound depends on the short window. A separate, important, innovation of SR is cell-phone visit data that yields small employment effects in within-California comparisons.

[2] This carries a methodological point beyond the AB 1228 case. In multi-state panels, idiosyncratic pre-trend violations in any one treated state average out, and pooled DiD has been reasonably stable in the post-2000 minimum-wage literature (Cengiz et al., 2019). In a single-treated-unit case study there is no averaging: the one state's nonlinear path *is* the estimate, which is exactly why synthetic-control-based methods are valuable here.

Importantly, the fall in separations also helps reconcile the somewhat divergent employment estimates from the QCEW and QWI. Because QCEW counts every worker paid during the pay period containing the 12th of the month, falling within-period turnover mechanically shrinks the QCEW headcount even when the true point-in-time stock is unchanging; a back-of-the-envelope calculation and direct empirical validation indicate that this single channel can account for most of the -0.98 log-point QCEW-vs-QWI gap I estimate in California fast food (Appendix B).

Across the 32 QCEW and QWI estimates assembled here, no specification yields an OWE more negative than -0.29, and the range $[-0.29, +0.26]$ sits comfortably inside the band that Dube and Lindner (2024) classify as "small." Here is another useful benchmark: across 26 post-2010 state minimum-wage increases that I compute for fast food (Section 5), the QCEW long-difference median OWE is -0.02 with an interquartile range of $[-0.34, +1.25]$, and across 28 U.S. restaurant-sector studies the median is -0.15. AB 1228's post-period Kaitz is roughly $1.5\times$ the average post-2010 state event, yet its OWE is no more negative. Even at the most negative end of my range, the wage gains substantially exceeded the employment decline, so total fast-food earnings rose. California's $20 fast-food minimum wage—set at a level many predicted would produce large job losses—raised pay for low-wage workers with modest to no employment costs.

2 Data and Method

2.1 Data

I use state-level BLS Quarterly Census of Employment and Wages (QCEW), covering 2015Q1 through 2025Q3, and state-level Census Bureau Longitudinal Employer-Household Dynamics (LEHD) Quarterly Workforce Indicators (QWI), R2026Q1 release (release date January 2026), covering 2015Q1 through 2024Q4.[3] The treated sector is NAICS 722513 (fast-food restaurants, formally "limited-service"). State population estimates from the Census Bureau—splicing the 2010–2019 and 2020–2025 intercensal vintages—are interpolated to quarterly frequency, linear in logs and anchored at Q3 (July 1), to construct employment-to-population rates.[4]

[3]The QWI extract was pulled through the Census API for NAICS 722513, 722511, 72, and 00, plus NAICS 22, 42, 44–45, 48–49, and 56 for the low-wage non-FF composite used in §2.3; the matched-state sample covers CA and the 20 federal-MW controls.

[4]Annual Census estimates are dated July 1. Interpolating in logs imposes a constant percentage growth rate between anchors. The two Census bases may produce a small level shift at the 2019–2020 boundary; the log-linear interpolation distributes this smoothly over four quarters.

Both QCEW and QWI are built primarily from state unemployment-insurance (UI) records covering nearly all private-sector wage and salary employment. QCEW reports state-industry monthly employment representing "the number of covered workers who worked during, or received pay for, the pay period that included the 12th day of the month";[5] my quarterly QCEW employment is the within-quarter average of the three monthly counts. QWI reports `Emp`, an "estimate of the total number of jobs on the first day of the reference quarter"[6]—operationalized as workers with positive UI earnings at the same employer in both the current and the previous quarter. Wages are average weekly wages for QCEW (total quarterly wages divided by employment times 13) and `EarnBeg` for QWI (average monthly earnings of the beginning-of-quarter stock).

The two measures respond differently to within-quarter turnover. The QWI count requires presence in consecutive quarters and is essentially invariant to within-pay-period turnover; the QCEW count, by contrast, captures every worker paid during the pay period containing the 12th, so a single position filled in succession by two different workers in that pay period is counted twice. When turnover is high, the QCEW count thus mechanically exceeds the true point-in-time headcount the measure is intended to capture (Appendix B quantifies the implications). This asymmetry matters specifically for the AB 1228 setting, because the policy itself produced a large reduction in California fast-food turnover (§4); the implication for reading the QCEW vs. QWI employment estimates is developed in §4.3 and Appendix B.[7]

Over my full sample (CA plus 20 controls, 2015Q1–2024Q4), the two sources agree closely on fast-food employment: the raw correlation of log per-capita 722513 employment is 0.99 (state-demeaned 0.87), and the correlation of 4-quarter log changes is 0.83. However, as I show in Appendix B, the two employment estimates diverge more when there is higher turnover.

2.2 Control group

I compare California to up to 20 states whose binding minimum wage for fast-food employment is the federal floor of $7.25 and remained at that level through the most recent state-published rate: AL, GA, IA, ID, IN, KS, KY, LA, MS, NC, ND, NH, OK, PA, SC,

[5]BLS Handbook of Methods, `https://www.bls.gov/opub/hom/cew/concepts.htm#scope-and-exclusions`.

[6]Census Bureau, *QWI 101*, `https://lehd.ces.census.gov/doc/QWI_101.pdf`.

[7]Another difference is that QCEW protects confidentiality by suppressing cells that fail disclosure thresholds (typically fewer than three reporting establishments, or one dominant employer), which yields more missing values at finer NAICS detail and in small states. In contrast, QWI applies multiplicative noise infusion to cell values for confidentiality, with very few cells suppressed. For fast food in the 21-state sample, neither source has any suppressed or missing cells.

TN, TX, UT, WI, and WY. Four other non-increaser states with rates above $7.25—AR ($11.00), MA ($15.00), NM ($12.00), and WV ($8.75)—are excluded. This guards against possibly lagged effects of recent minimum wage hikes contaminating the control sample.

2.3 Difference-in-differences estimation

I estimate population-weighted event-study DD regressions:

$$y_{st} = \sum_{\tau \neq 0} \beta_\tau \cdot \mathbf{1}[s = \text{CA}] \cdot \mathbf{1}[t - t^* = \tau] + \alpha_s + \gamma_t + \varepsilon_{st} \tag{1}$$

where y_{st} is the seasonally adjusted log outcome for state s in quarter t, $t^* = 2023\text{Q4}$ is the omitted base period, and α_s, γ_t are state and quarter fixed effects. AB 1228 took effect April 1, 2024, so using 2023Q4 as the base leaves room for any pre-implementation anticipation in 2024Q1 to be absorbed into the post-period rather than the base; this places my base between Clemens, Edwards, and Meer (2025)'s September 2023 and Sosinskiy and Reich (2026)'s 2024Q1. As a robustness alternative to the single-quarter base, Panel B in Tables 1 and 2 subtracts the average of seven pre-period event-study coefficients (2022Q1–2023Q3) from the post-period average. Panel B reduces sensitivity to idiosyncratic variation in any one base-period quarter and is invariant to anticipation that may have leaked into 2023Q4 itself; both panels exclude 2024Q1 from the post-period average as well as from the pre-treament period, treating it as a transition quarter. COVID quarters (2020Q1–2021Q4) are excluded from all regressions. The post-period runs from 2024Q2 through 2025Q3 (six quarters). The OWE is the ratio of the averaged post-period employment effect to the averaged post-period wage effect; the OWE standard error is computed by the delta method, $\sqrt{(\sigma_e/w)^2 + (\text{OWE} \cdot \sigma_w/w)^2}$, using the Ferman–Pinto SEs of the wage and employment effects as inputs (the FP procedure is described in §2.5).

All outcomes are seasonally adjusted before estimation; as I will show below, California fast food has markedly different seasonality patterns than the control states.[8] For each state and industry, I compute a 2×4 centered moving average over the pre-COVID, pre-treatment period (2015Q1–2019Q4) to extract the local trend, take deviations of the actual series from this trend, and average the deviations by quarter-of-year to obtain four seasonal factors (normalized to sum to zero). These constant factors are then subtracted from the full series. By estimating seasonality as deviations from a moving-average trend rather than from a regression with year fixed effects, the method correctly separates seasonal variation

[8]Both Clemens, Edwards, and Meer (2025) and Sosinskiy and Reich (2026) also seasonally adjust their series.

from trend growth. Seasonal adjustment does not make a substantial difference to regression estimates with the full post-treatment period, but does make a difference for visualization in the event studies.[9]

I also estimate three DDD specifications in which the outcome is the log difference between fast-food restaurants and a comparison sector, $y_{st} = \ln Y_{st}^{\text{FF}} - \ln Y_{st}^{\text{comp}}$:

A. *Full-service restaurants* (NAICS 722511)—the within-restaurant-industry control used by Sosinskiy and Reich (2026).

B. *Low-wage non-fast-food*—a composite of non-fast-food private industries comprising accommodation and food services excluding fast food (NAICS 72 − 722513), the retail and transportation supersector (1021), and administrative/waste services (56), covering roughly 5.4 million California workers at a composite average wage of $1,195 per week.[10]

C. *All non-fast-food private* (NAICS 10 − 722513)—the broadest comparison, closest to the approach in Clemens, Edwards, and Meer (2025).

2.4 Synthetic difference-in-differences

I apply the SDID estimator of Arkhangelsky et al. (2021) to the DD outcomes and to each of the three DDD differenced outcomes (labeling the latter "SDDD"). The SDID jointly solves for unit weights $\hat{\omega}$ (on control states) and time weights $\hat{\lambda}$ (on pre-treatment quarters):

$$\hat{\tau}_{\text{SDID}} = \left(\bar{y}_{\text{CA,post}} - \bar{y}_{\text{CA,pre}}^{\hat{\lambda}}\right) - \left(\bar{y}_{\text{ctrl,post}}^{\hat{\omega}} - \bar{y}_{\text{ctrl,pre}}^{\hat{\lambda},\hat{\omega}}\right). \tag{2}$$

Each is the solution to a regularized least-squares problem with a simplex constraint (nonnegative weights summing to one) and an ADHS-recommended ridge penalty $\hat{\zeta}$; Appendix C gives the full specification. The pre-treatment period spans 2015Q1–2023Q4, excluding 2020Q1–2021Q4 (28 quarters). I derive $\hat{\omega}$ and $\hat{\lambda}$ from the *employment* outcome and apply those same weights when estimating the wage effect. This choice reflects the structure of the

[9]The quarter fixed effects in the event-study regression absorb the *average* seasonal pattern across states, but not state-specific seasonality. If California's fast-food sector has a different seasonal cycle from the control states—plausible given differences in tourism, weather, and labor market composition—the DD event-study coefficients will retain residual seasonal variation. Pre-deseasonalizing by state removes this. The seasonal adjustment does not affect the DDD estimates, since within-state differencing already cancels state-specific seasonality, but it substantially affects the DD.

[10]NAICS 1021 is a BLS supersector bundling retail trade (44–45), transportation/warehousing (48–49), wholesale trade (42), and utilities (22). The composite uses only NAICS codes with complete data across all control states and quarters, avoiding the suppression issues that arise with narrower industry codes in small states.

research design: the control states—all at the federal $7.25 minimum—had systematically lower fast-food wages than California throughout the pre-period, because California's own minimum wage was already $10–$16 before AB 1228. No weighted combination of federal-minimum-wage states can match California's wage *level*, and fitting wage-specific weights produces a synthetic control driven by a single small state. Matching on the employment trajectory instead—where the cross-state variation is more comparable—and then observing what happened to wages for the employment-matched states produces more stable and interpretable estimates of both margins.[11]

The SDID applied to the raw DD outcomes is my preferred specification: it constructs a data-driven counterfactual that matches California's pre-treatment employment trajectory without requiring a choice of within-state comparison sector. The SDDD variants—which apply the same synthetic reweighting to the DDD differenced outcomes—serve as robustness checks.

2.5 Inference

With a single treated state, cluster-robust standard errors do not yield valid inference. Therefore, I use Ferman–Pinto permutation-based standard errors (Ferman and Pinto, 2019) as implemented in Cengiz et al. (2019). For each of the 20 control states j, I re-estimate the specification treating state j as "California" and compute the same post-period average effect. Each placebo effect is scaled by $\sqrt{\text{pop}_j/\text{pop}_{\text{CA}}}$ to correct for heteroscedasticity across states of different sizes; the FP standard error is the standard deviation of the scaled placebos. The same approach is used for the SDID and SDDD specifications: for each placebo state, the full SDID is re-estimated (including recomputation of the unit and time weights), and the resulting placebo treatment effect is scaled identically.

3 Results

3.1 The identification challenge in levels

Figure 1 illustrates the basic identification challenge using QCEW data. California's fast-food employment rate was roughly 12 per 1,000 population in 2015—well below the control-state average of 14. The two series did not converge along a simple trend: California narrowed

[11] When separate wage weights are used, the SDID places 100 percent of the wage weight on New Hampshire—the one federal-MW state whose fast-food wage trajectory most resembles California's. The employment-derived weights instead distribute weight across Idaho, Utah, Pennsylvania, Texas, and Georgia, producing a more diversified and robust synthetic control.

the gap gradually from 2015 through 2019, rebounded rapidly in 2021–2022 after the pandemic drop, and then plateaued through 2023, where it held until AB 1228. This nonlinear catch-up predates the policy by years and reflects California-specific dynamics—such as demographic trends, sectoral composition, differential COVID recovery—that have nothing to do with the fast-food policy. After implementation, California's employment rate declines modestly while the control average stays flat. Whether this small divergence is the policy's effect, a tipping of the 2023 plateau into mild decline, or a rollback of the post-COVID rebound is difficult to tell from the DD alone. The figure also clearly shows that California fast-food employment has historically had much more dampened seasonality than the control states. This motivates the use of de-seasonalized data.

Figure 2 shows the SDID's solution: a synthetic control—a weighted combination of Idaho (61%), Utah (22%), Pennsylvania (7%), Texas (7%), and Georgia (4%)—that tracks California's pre-treatment trajectory much more closely. The synthetic control nearly overlaps with California throughout 2015–2023, and there is no visible divergence after implementation.

3.2 Wage and employment estimates from QCEW

The wage effect is large and robust across all specifications and both panels: +7.5 to +7.6 percent in the conventional estimates and +6.4 to +7.5 percent in the synthetic variants, all with FP $p < 0.001$. Wage coefficients in Figure 3 (left column) show a sharp increase of 6–8 percent at implementation in every one of the eight specifications, with no meaningful pre-trends near the base period.

Employment effects vary with identification strategy. Figure 3 presents the event-study coefficients for all eight specifications with QCEW data, with conventional DD/DDD coefficients (navy circles) and SDID/SDDD counterparts (red squares) overlaid in each cell. In the DD series for fast-food employment (top-right panel), the convergence pattern from Figure 1 appears as a rising pre-trend: coefficients climb from −10 percent in 2015 to zero by 2023 and decline by about 2 percent after implementation, leaving the post-treatment "effect" difficult to distinguish from the pre-existing trend. The same pattern, attenuated, appears in the all-non-fast-food DDD (bottom row); differencing against broad private employment does not remove the California-specific dynamic. The full-service DDD (second row) eliminates the DD employment effect entirely, hovering near zero in both pre- and post-periods; the low-wage non-fast-food DDD (third row) is intermediate, with DD employment effects of −1 to −2 percent.

The SDID/SDDD series (red squares) tells a different story. Pre-treatment period variation is dramatically compressed in every cell: the SDID employment series in the top row

shows no visible break at implementation, and the SDDD series in the lower three rows show flat pre-trends and small, insignificant post-treatment effects. The SDID approach constructs a counterfactual that tracks California more closely, absorbing the state-specific trends that contaminate the conventional DD estimates.

Table 1 reports the averaged post-period effects. The left columns show conventional DD/DDD results; the right columns show their SDID/SDDD counterparts. Panel A reports the post-period average relative to the 2023Q4 base; Panel B subtracts the average of the seven pre-period coefficients from 2022Q1 through 2023Q3, netting out pre-existing level differences. The broader pre-period averaging window in Panel B reduces sensitivity to the choice of a specific base-period quarter.

Quantitatively, the baseline DD yields an employment effect of -1.4 percent (FP $p < 0.001$, OWE $= -0.19$). Among the DDD specifications, the all-non-FF DDD is most negative at -2.2 percent ($p < 0.001$, OWE $= -0.29$); the full-service DDD reduces the effect to -0.4 percent ($p = 0.40$, OWE $= -0.05$), statistically indistinguishable from zero; and the low-wage DDD falls in between at -1.5 percent ($p < 0.001$, OWE $= -0.20$). Every synthetic specification produces a smaller employment effect than its conventional counterpart, and none is statistically significant; the preferred SDID yields -0.3 percent ($p = 0.50$, OWE $= -0.04$).

Panel B pushes employment effects closer to zero across most specifications. The DD OWE is essentially unchanged at -0.19; the full-service DDD moves from -0.05 to $+0.04$; the low-wage DDD from -0.20 to -0.07; the all-non-fast-food DDD from -0.29 to -0.22; and the preferred SDID from -0.04 to -0.00.

3.3 Wage and employment estimates from QWI

Using QWI data through 2024Q4, I re-estimate the same DD and SDID designs, plus three DDD analogs. The three QWI DDDs are constructed to mirror the QCEW DDDs of Table 1: full-service restaurants (NAICS 722511); a low-wage non-fast-food composite (NAICS 72 − 722513, plus NAICS 22, 42, 44–45, 48–49, and 56)[12]; and all non-fast-food private employment (00 − 722513). Table 2 reports 16 QWI estimates (four comparison-sector specifications × DD and SDID × the two averaging-window panels of Table 1).

On wages, the QWI results align closely with the QCEW findings: wages rise by +5.0 to +8.8 percent across the 16 specifications. Employment estimates, however, are systematically more positive in QWI than in QCEW—−0.6 to +1.7 percent (12 of 16 non-negative, none

[12]The QWI low-wage non-FF composite uses the same supersector elements as the QCEW composite (NAICS 72 − 722513, NAICS 1021, and NAICS 56), spelled out at the 2-digit level since QWI does not publish the BLS supersector aggregates.

individually distinguishable from zero), with the implied OWE ranging from −0.10 (main DD, Panel B) to +0.26 (SDDD vs. full-service, Panel B). Across all four comparison-sector specifications, the QWI Panel A OWEs (range −0.07 to +0.22, five of eight non-negative) sit above the corresponding QCEW Panel A OWEs (range −0.29 to +0.01).[13] As in the QCEW (§3), Panel B is more positive than Panel A because netting out CA's persistently below-control pre-treatment gap absorbs more of the convergence trend.

Combining the 16 QCEW estimates in Table 1 with the 16 QWI estimates in Table 2 yields a 32-specification OWE range of −0.29 to +0.26. The systematic QWI–QCEW gap on the employment margin is not driven by the different post-treatment windows: re-running QCEW on the matched 2024Q2–2024Q4 window leaves the QCEW numbers essentially unchanged (Appendix Table B1; DD OWE = −0.18, SDID = −0.03). I take up the QCEW–QWI comparison and reconcile the gap in §4.3, after presenting the QWI worker-flow evidence.

4 Additional Evidence from QWI: Firm-Size Heterogeneity and Worker Flows

QWI adds two margins the QCEW cannot measure: firm-size breakdowns and worker-flow statistics (separations). Both speak directly to whether the wage change is concentrated where AB 1228 binds and to the mechanism by which employment adjusts to a higher wage floor.

4.1 Firm-size gradient

AB 1228's coverage threshold—chains with 60 or more establishments nationwide—implies that a large share of workers at small fast-food firms are uncovered. Wage effects should therefore be more pronounced in large firms. Table 4 reports DD and SDID estimates separately by QWI firm-size bin (employment-based; bins ≤19, 20–49, 50–249, 250–499, 500+ employees).[14]

[13]The QCEW and QWI SDIDs produce very similar synthetic controls: both place nearly all unit weight on Idaho, Utah, Pennsylvania, Texas, and Georgia. On QCEW the weights are 0.61/0.22/0.07/0.07/0.04; on QWI they are 0.51/0.17/0.10/0.14/0.03, with a small 0.04 weight on Mississippi and the remaining control states receiving essentially zero weight in both cases.

[14]QWI firm-size bins are based on total *firm* employment, not establishment count. Chains with 60+ establishments typically have thousands of workers, so they fall into the 500+ bin. Independent franchisees of covered chains are reported under their own firm, so a single-location franchisee of a large chain would still appear in a smaller firm-size bin; this attenuates the measured gradient but does not eliminate it. The 250–499 bin is the smallest in California fast food and has the noisiest estimates.

The wage gradient is sharp and fairly monotone: both estimators place the smallest-bin wage effect between +1.1 and +2.4 percent and the 500+-bin effect between +10.4 and +11.8 percent, with the middle bins in between. The wage gradient is strong evidence that the estimated effects reflect the policy's coverage structure and not a generic California-specific post-2024 shock. Figure 6 displays the pattern graphically.

The small positive wage effect in the QWI 0–19 bin (+2.4 percent, Panel A) is best read as partial coverage rather than cross-firm spillover: QWI firm-size is defined by total national firm employment, so many single-location franchisees of 60+ chain brands—which *are* covered by AB 1228—appear in the 0–19 bin regardless of their local size. A mix of roughly one-quarter covered franchisees paying the covered-chain wage and three-quarters uncovered independents paying the control wage reproduces the observed +2.4-percent bin average. Consistent with this reading, the wage effect also does not propagate to full-service restaurants: the DDD-vs.-full-service wage coefficient (+7.6 percent) is statistically indistinguishable from the fast-food DD wage effect (+7.5 percent), implying a near-zero full-service wage response (Table 1).

The absence of measurable wage spillover to truly uncovered firms is consistent with Derenoncourt and Weil (2025), who find that voluntary minimum wage policies by large retailers generate small or negligible cross-employer wage spillovers. They argue that oligopsony models with local labor market concentration—specifically Berger, Herkenhoff, and Mongey (2022)—predict substantially larger strategic spillovers than what is observed empirically. These results fit that pattern.

Turning to employment, effects in the 500+ bin (where the policy binds) are small and insignificant under both estimators (−1.8 to −1.9 percent). In the two smallest bins, the SDID employment estimates are positive, though DD estimates are smaller.[15] The 250–499 bin has the fewest fast-food workers in California and the widest confidence intervals.

4.2 Worker flows

The quarterly separation rate in California fast food fell by 12.7 log points in the main DD and 19.0 log points in the SDID (Appendix Figure A1; Table 2; FP $p < 0.001$ in both), with the range across all 16 QWI specifications spanning 13 to 25 log points—a roughly 3–6 percentage-point absolute decline against baseline quarterly rates of 25–30 percent. Pre-treatment separation rates in California were persistently 10–15 log points *above* control states; they crossed zero at AB 1228 implementation and continued falling through 2024Q4. Like the wage gradient, the separation-rate response is monotone in firm size: a 3 to 8 log-

[15] One note of caution in interpreting bin-level employment effects is that firms can cross bin thresholds as their employment changes, so a reduction in a firm's size can boost employment in a lower bin

point drop among the smallest firms rises to 20 to 26 log points in the 500+ bin (Table 4; Figure 6).

Reductions in separations in response to a minimum wage increase are consistent with a monopsonistic model in which an upward-sloping firm-level labor supply curve means higher wages reduce quits (Dube, Lester, and Reich, 2016; Dube and Lindner, 2024). The resulting lower turnover lowers vacancy, training, and recruiting costs and mitigates the employment response to a higher wage floor. Firms that had to raise wages experienced lower quits, consistent with the wage and separation gradients. An alternative story in which the effect reflects CA-specific secular changes in low-wage labor market conditions cannot generate this firm-size pattern.

The implied own-wage elasticity of separations is -1.67 in the main DD ($-12.74/7.62$) and -3.39 in the SDID ($-18.97/5.60$); across all 16 QWI specifications (Table 2), the separation OWE ranges from -1.67 (main DD, Panel A) to -4.20 (DDD vs. all-non-FF, DD, Panel B). Dube, Lester, and Reich (2016), using cross-state minimum wage variation, estimate a separation OWE of approximately -1.1 for restaurant workers; the AB 1228 estimates are roughly 1.5 to 4 times larger in absolute value. The gap reflects AB 1228's partial coverage. A typical minimum wage raises wages at all firms drawing from a given low-wage labor market, so the change in *relative* wages—what governs quits—is smaller than the gross wage change. Because AB 1228's wage effects are confined to covered chains, with no measurable response at uncovered full-service restaurants or the broader low-wage labor market (Table 1, DDD coefficients), the policy raises the relative wages at affected establishments more than a standard minimum wage hike would. The relevant benchmark is therefore closer to firm-specific quit elasticities estimated from matched employer-employee data, which cluster in the range -1 to -3 (Bassier, Dube, and Naidu, 2022; Derenoncourt and Weil, 2025)—broadly consistent with the magnitudes I estimate.

4.3 Comparing QCEW and QWI

The two data sources tell a consistent story on wages but differ somewhat on the employment margin. QWI Panel-A employment OWEs range from -0.07 to $+0.22$ across the eight Panel-A specifications; QCEW Panel-A OWEs (Table 1) range from -0.29 to $+0.01$ over the same eight design cells. The wedge is consistent with the QCEW double-counting mechanism described in §2.1: the 13–25 log-point reduction in separations documented above mechanically deflates the QCEW headcount, even when the true point-in-time stock is flat. Appendix B provides the back-of-the-envelope simulation and a direct reconciliation regression that can nearly close the gap.

A second pattern is that within each data source, SDID delivers more positive and tighter estimates than DD: it compresses pre-treatment variation by reweighting controls to match California's pre-trajectory, and the SDDD variants converge across comparison sectors. The QCEW is a transparent measure of jobs in normal times, but in this setting the policy itself shifts what QCEW measures. I therefore view the QWI stock measure—unaffected by within-quarter turnover—as the more informative estimate in this context. The two together bracket the employment effect, but the QWI evidence suggests it is very likely closer to zero than the QCEW headline alone implies.

5 Interpretation and Context

5.1 Interpreting the range of employment estimates

Across the 32 estimates in Table 1 and Table 2, the OWE ranges from −0.29 to +0.26. The headline finding—a small or zero employment effect—is robust across both data sources and across DD, SDID, SDDD, and DDD designs.

The most negative estimate—the QCEW all-non-fast-food DDD (−0.29 in Panel A)—is a plausible upper bound in absolute value. First, the QCEW double-counting mechanism imparts a negative bias to QCEW employment estimates when separations fall (§4.3). Second, the other DDDs using economically closer controls suggest a smaller response. While a spillover is a reasonable concern *a priori*, there is a near-zero wage response in full-service restaurants. A negative employment spillover without a corresponding wage spillover is difficult to rationalize.

Within each data source, SDID compresses the cross-specification range relative to DD. For QCEW, the four SDID/SDDD Panel-A OWEs span −0.04 to +0.01; on QWI they span +0.11 to +0.22. The QCEW SDID is −0.04 and the QWI SDID is +0.13; the substantive lesson is the convergence across designs, not the precise value of any single point estimate. Across DD/SDID and across QCEW/QWI, the employment effect is mostly small, and it is more positive in specifications and datasets that are more reliable in this context.

Appendix D decomposes the gap between Clemens, Edwards, and Meer's reported point estimates and my preferred SDID step by step, applying six methodological modifications in parallel to four starting CEM specifications (Table D1). Removing linear detrending alone shrinks the DDD OWE from −0.43 to −0.29 and the DD OWE from −0.42 to −0.36; synthetic-control weighting and window extension are the next-largest single contributors in the DD path; normalizing job counts by state population also matters, but only for the DD path (worth ≈ 0.09 in OWE) since population cancels out in the DDD formulation. The

four parallel paths converge by the final row to two distinct preferred estimates: SDDD vs. all-non-FF at OWE +0.01 and SDID at −0.04, both far less negative than CEM's reported −0.49. Perhaps most importantly, Table D2 shows that none of the core CEM specifications yield OWEs more negative than −0.24 when repeated using QWI data, and most of the estimates are very close to zero or positive.

5.2 Comparison with the literature and post-2010 state events

Table 3 places the estimates from this paper alongside two prior studies, a complementary long-difference DID exercise on the post-2010 state MW events assembled here (Figure 4), and the broader U.S. restaurant-sector literature. How large is an OWE of −0.04? The Own-Wage Elasticity Repository, which I maintain with Ben Zipperer, collects every U.S. and European minimum wage study that reports both wage and employment estimates from which an OWE can be computed.[16] Across the 28 U.S. restaurant-sector studies in the repository, the median OWE is −0.15, with the bulk of estimates falling between 0 and −0.4. The preferred SDID estimate of −0.04 is near the middle of this distribution—squarely within the range Dube and Lindner (2024) characterize as "small."

What does "small" mean in welfare terms? The OWE is a sufficient statistic for the first-order effect of a minimum wage on total labor earnings in the covered sector. If the wage rises by $\Delta w/w$ and employment falls by $\text{OWE} \times \Delta w/w$, then total earnings change by approximately $(1 + \text{OWE}) \times \Delta w/w$. For an OWE of −0.04 and a wage effect of 7 percent, total sector earnings rise by $(1-0.04)\times 7 \approx 6.7$ percent. Even for the most negative estimate (OWE = −0.29), total earnings would rise by $(1 - 0.29) \times 7 \approx 5.0$ percent. As long as the OWE exceeds −1—that is, as long as employment falls by less than wages rise—low-wage workers as a group are better off.[17] Across the 32 QCEW and QWI specifications, the 95-percent CI lower bound on the OWE is no more negative than −0.41 except for one (imprecise) QCEW DDD-vs-all-non-FF specification; for the preferred QCEW SDID the lower bound is roughly −0.2 (FP standard errors). The policy substantially raised earnings for fast-food workers while reducing employment by, at most, a modest amount.

It's useful to benchmark these numbers to estimates from recent state-wide minimum wages using a standard difference-in-difference approach. The distribution of state-level estimates are reported in the middle block of Table 3 and plotted in Figure 4. For each of 26

[16] See `https://economic.github.io/owe`. The repository currently covers over 50 studies across multiple sectors and countries.

[17] This calculation captures the first-order effect on the wage bill and does not account for distributional considerations—the workers who lose jobs are worse off even if the group as a whole gains—or for general equilibrium effects such as price adjustments and consumer surplus changes. Clemens et al. (2025) and Sosinskiy and Reich (2026) provide evidence on the price margin.

states with a first ≥5 percent MW increase after 2010, I compute a single long-difference DID—Δlog(wage) and Δlog(emp/pop) from a three-year pre period ending the year before the state's first major increase to a three-year post period 2023–2025—differenced against the 20 federal-$7.25 non-increaser states.[18] I run the exercise twice—on QCEW (26 states) and on QWI (25 states)—using identical pre/post year-windows and a QWI extract that I extended back to 2011.[19] Figure 4 additionally plots both data sources' AB 1228 estimates—DD and SDID separately—and CA's general-MW trajectory in each source, so the figure brings all three data-source comparisons into a single view. The figure normalizes the wage and employment effects by Δlog(MW), so panels (a) and (b) plot the minimum-wage elasticity of fast-food wages and the minimum-wage elasticity of fast-food employment; panel (c) is the OWE (the ratio of the two). California's $20 fast-food minimum pushes the Kaitz for covered workers up to roughly 0.77, well above the 0.42–0.62 range spanned by the state events.

Two very different patterns emerge. On *wages*, AB 1228's MW elasticity (0.31–0.34 in QCEW; 0.25–0.34 in QWI) lies above the 90th percentile of the state-event distribution in both sources, whose QCEW median is 0.15 with IQR $[0.12, 0.22]$—the unusually high Kaitz is matched by an unusually large wage response. On *employment*, by contrast, AB 1228's MW elasticity is essentially zero (DD and SDID estimates between -0.06 and $+0.04$ in both sources) and statistically indistinguishable from most of the state-event distribution. Together, these patterns place the AB 1228 OWE near the median of the state-event OWE distribution in each source: -0.02 in QCEW (26 events, IQR $[-0.34, +1.25]$) and $+0.02$ in QWI (25 events, IQR $[-0.41, +0.92]$). The four AB 1228 OWE estimates (-0.19 QCEW DD, -0.04 QCEW SDID, -0.04 QWI DD, $+0.13$ QWI SDID) all fall comfortably inside both interquartile ranges. AB 1228 produces an unusually large wage response without an unusual employment response, even at a Kaitz well above the levels seen in past state events—and despite partial coverage providing ample scope for substitution toward uncovered establishments.

A third feature of the state-event panel concerns the separation-rate response (Appendix Figure A2). Across the QWI state events, the median $\Delta \log(\text{sep})$ is -5.1 log points (median separation OWE -0.45), well above AB 1228's -12.7 to -19.0 log points (separation OWE -1.7 to -3.4). The AB 1228 separation response is 2.5–4× the typical state-MW response, consistent with partial coverage raising the *relative* wage at covered firms by more than a

[18] Pre-period windows that would otherwise include 2020–2021 drop those COVID years and shift back to keep three valid years. Kaitz is computed as state MW (post-period) divided by the OEWS all-occupations state median hourly wage (2023).

[19] Alaska is absent from the QWI panel because it withdrew from the LEHD/QWI partnership in 2016.

uniform MW change does.[20]

6 Conclusion

Across 32 QCEW and QWI specifications, California's $20 fast-food minimum wage raised wages by roughly 7 percent and produced an own-wage elasticity of employment in the range -0.29 to $+0.26$. The QCEW DD yields an OWE of -0.19 and the preferred SDID -0.04. The QWI employment estimates are on average more positive, with 12 of 16 specifications non-negative and the implied OWE between -0.10 and $+0.26$. Because QCEW's establishment-month count mechanically scales with within-quarter turnover, and the policy-induced fall in separations leads to a QCEW–QWI gap, I view the QWI `Emp` stock as the more credible measure of jobs in this setting.

The QWI data deliver an independent and striking finding: the quarterly separation rate fell sharply, with an own-wage elasticity between -1.7 and -4.2—consistent with the quit-reduction margin emphasized in monopsony models, which absorbs part of the cost of higher wages. Both the wage and separation responses are concentrated in the 500+-employee firm-size bin AB 1228 covers (Section 4). That the policy's footprint maps onto its statutory coverage—rather than spreading diffusely across the low-wage labor market—argues that the estimates capture AB 1228 itself rather than a generic California-specific shock, and confirms that cross-employer wage spillovers are limited (Derenoncourt and Weil, 2025).

There is a methodological takeaway as well. With a single treated state, conventional DD and DDD designs offer no cross-unit averaging to wash out idiosyncratic trends, so the treated state's pre-treatment trajectory feeds directly into the estimate. California's fast-food employment followed a markedly nonlinear pre-treatment path, and the cross-study variation in published OWEs traces to how each design handles that path—a sensitivity Appendix D decomposes step by step. Synthetic-control–based methods, which reweight controls and pre-treatment periods to match the treated unit's trajectory directly, are well suited to this case-study setting.

California's $20 fast-food minimum represents an unusually demanding test. With a Kaitz of roughly 0.77 for covered workers, it is well above the 0.42–0.62 range spanned by post-2010 state events. Combined with partial coverage, this gives consumers and employers ample scope to substitute toward uncovered competitors. The wage response was unusually large, commensurate with the unusually high Kaitz, while the QWI employment response

[20]The smaller separation response in the state events also suggests that the QCEW/QWI gap from turnover reduction is less of a problem there than for AB 1228; and indeed the OWE from the common 25 state events using QWI and QCEW are nearly the same at $+0.02$ and $+0.03$, respectively (IQRs $[-0.41, +0.92]$ and $[-0.31, +1.25]$).

sits near zero—close to the median across post-2010 state minimum-wage events. Overall, the sectoral standard in California appears to have worked largely as intended.

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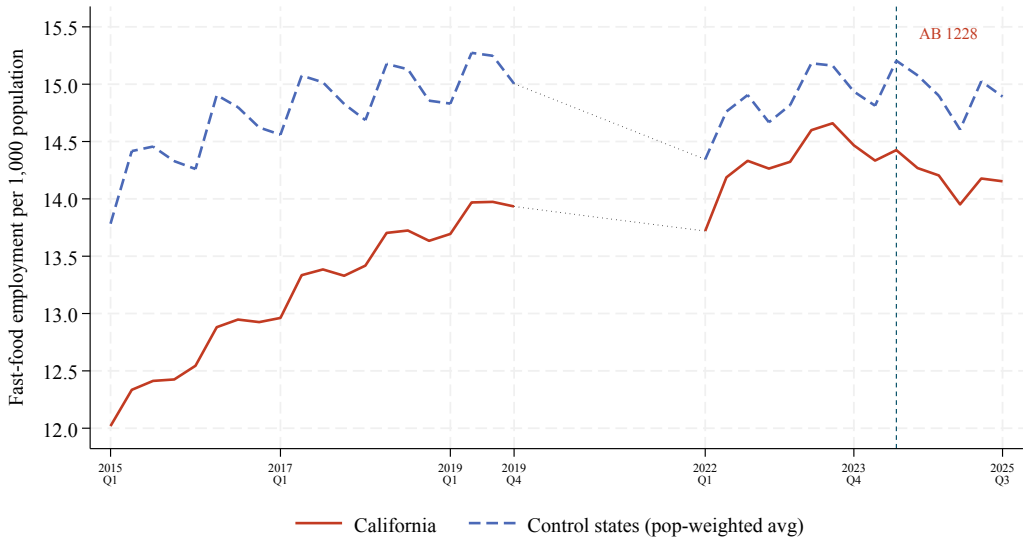

Figure 1: Fast-food employment rates: California vs. control states

Notes: Fast-food restaurant (NAICS 722513) employment per 1,000 population. Solid line: California. Dashed line: population-weighted average of 20 federal-$7.25 states. Data for 2020Q1–2021Q4 omitted (dotted connector). Vertical dashed line marks AB 1228 implementation (2024Q2).

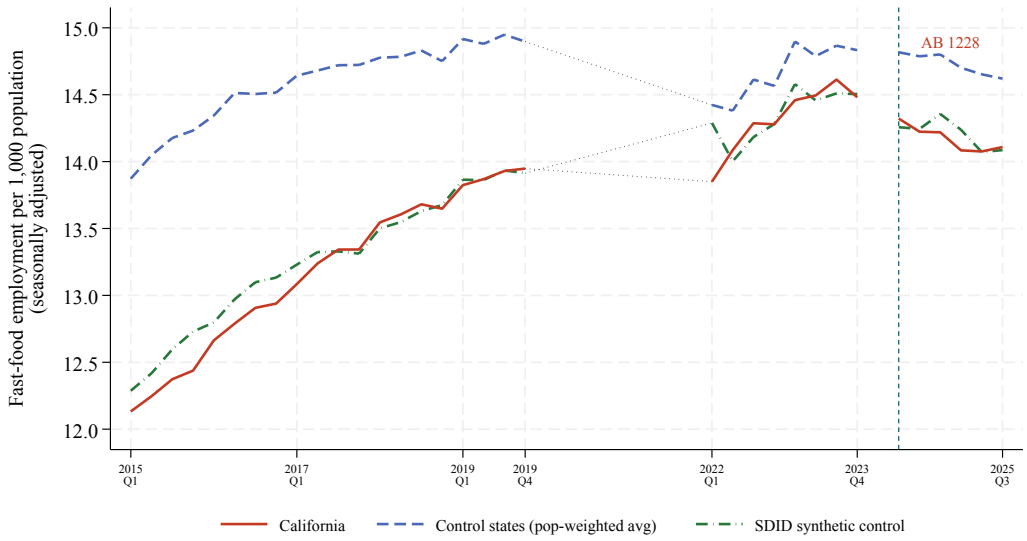

Figure 2: Seasonally adjusted fast-food employment rates: California vs. SDID synthetic control

Notes: Seasonally adjusted fast-food restaurant employment per 1,000 population. Seasonal factors estimated by state from 2015–2019 data. Solid line: California. Dashed line: population-weighted control-state average. Dash-dot line: SDID synthetic control—a weighted combination of control states (primarily Idaho, Utah, Pennsylvania, Texas, and Georgia) whose employment trajectory is optimized to match California's pre-treatment path. Vertical dashed line marks AB 1228 implementation (2024Q2).

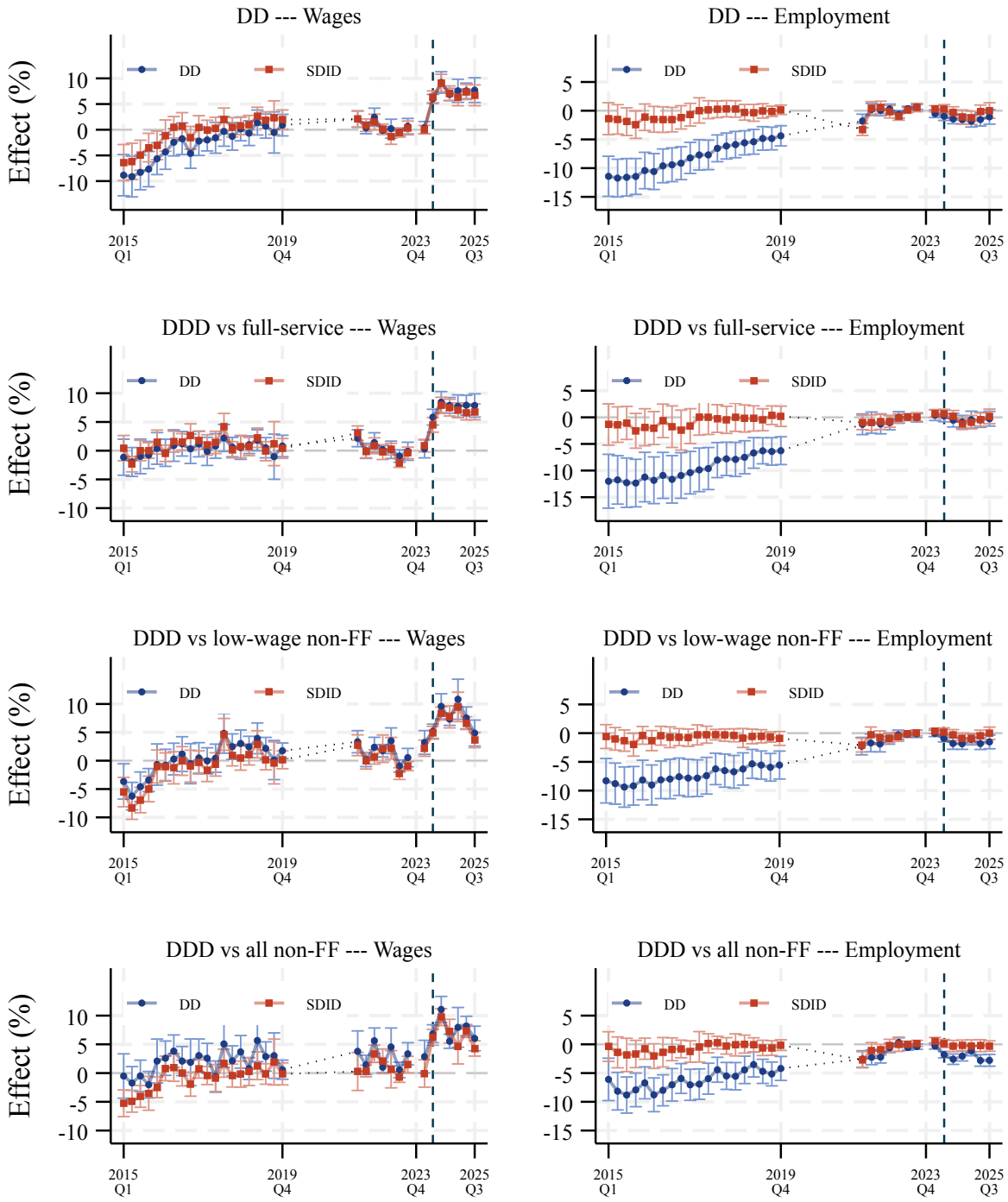

Figure 3: Event-study estimates: wages and employment, DD and SDID overlaid

Figure 3, continued.

Notes: Population-weighted event-study coefficients with DD (navy circles) and SDID (red squares) overlaid in each cell. SDID/SDDD coefficients are California minus the synthetic control, normalized to the $\hat{\lambda}$-weighted pre-treatment average (Arkhangelsky et al., 2021); unit and time weights are derived from log employment-per-capita and applied to both outcomes, with pre-treatment fitting excluding COVID quarters (2020Q1–2021Q4). Left column: log wage. Right column: log employment-to-population. Rows: fast food only (top), DDD vs. full-service, DDD vs. low-wage non-fast-food composite, DDD vs. all non-fast-food private (bottom). COVID quarters (2020Q1–2021Q4) omitted; the light dotted segment across the gap is a visual connector, not a fitted value. Error bars: 95% CIs using Ferman–Pinto SEs. Base: 2023Q4; vertical dashed line marks 2024Q2 (AB 1228 implementation). Sample: California vs. 20 federal-$7.25 states, 2015Q1–2025Q3.

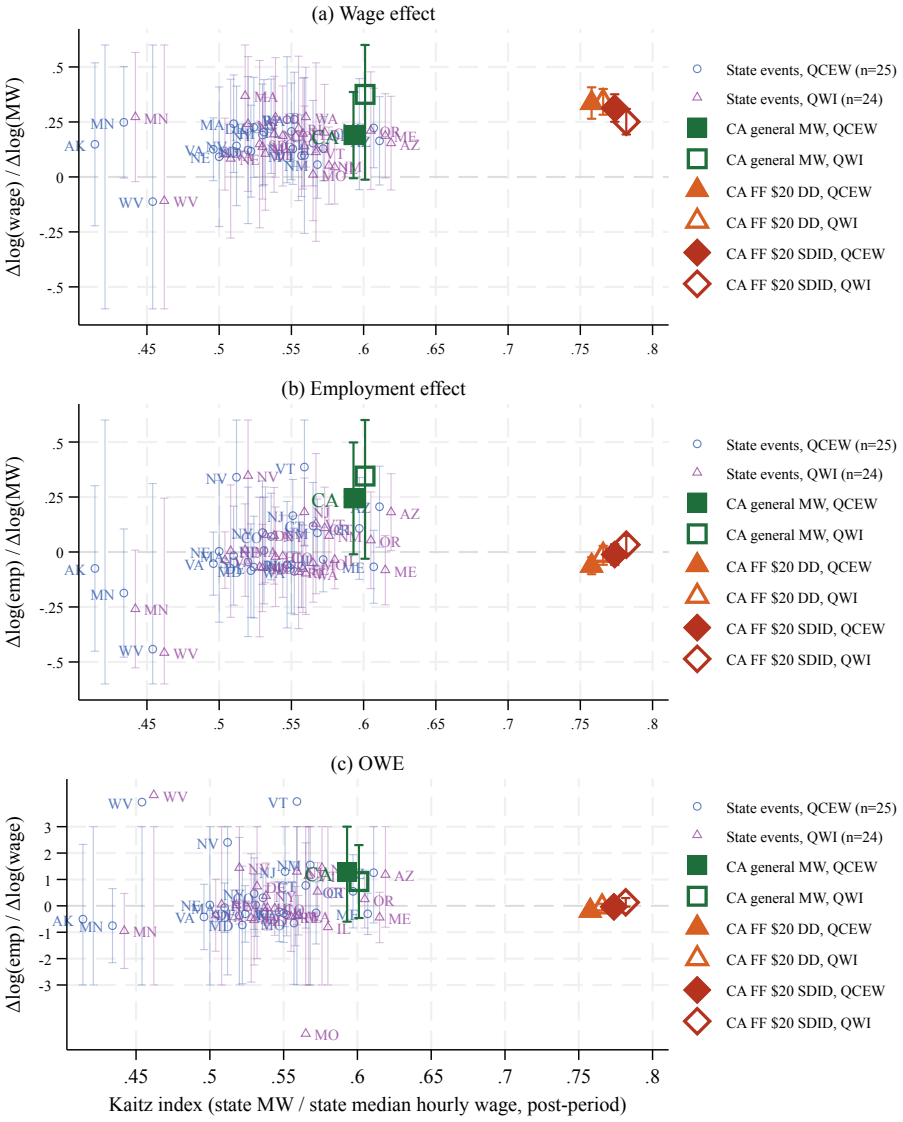

Figure 4: MW effects by Kaitz index: Long-difference DID on state events, QCEW and QWI

Notes: Long-difference DID for fast-food (NAICS 722513) on three sets of events, each computed in QCEW and QWI: post-2010 state MW events (filled = QCEW, $n = 26$; hollow = QWI, $n = 24$; AK/DC drops explained in §5), CA's general-MW trajectory, and AB 1228's CA fast-food $20 effect (DD and SDID). Both sources use identical pre/post year-windows; the QWI panel adds a 2011–2014 supplement. Effects are scaled by $\Delta \log(\text{MW})$ at each event, so (a) is the MW elasticity of fast-food wages, (b) is the MW elasticity of fast-food employment, and (c) their ratio (OWE). State-event pre window: three years ending the year before the state's first ≥5% increase (2020–2021 dropped if in window); post window: 2023–2025 (QCEW), 2023–2024 (QWI). CA general MW: pre 2011–2013, post 2021–2023 (QCEW) / 2021–2022 (QWI). For CA FF, $\Delta \log(\text{MW}) = \log(20/16) = 0.223$ and Kaitz ≈ 0.77 uses the $20 fast-food rate. Kaitz = state MW (post) ÷ OEWS state median hourly wage (2023). Controls: 20 federal-$7.25 non-increaser states; 95% CIs from placebo SE. CIs are clipped at ±0.6 in (a) and (b) and ±3 in (c); MN, NV, VT, WV in both sources, AK in QCEW, and MO in QWI exceed these bounds.

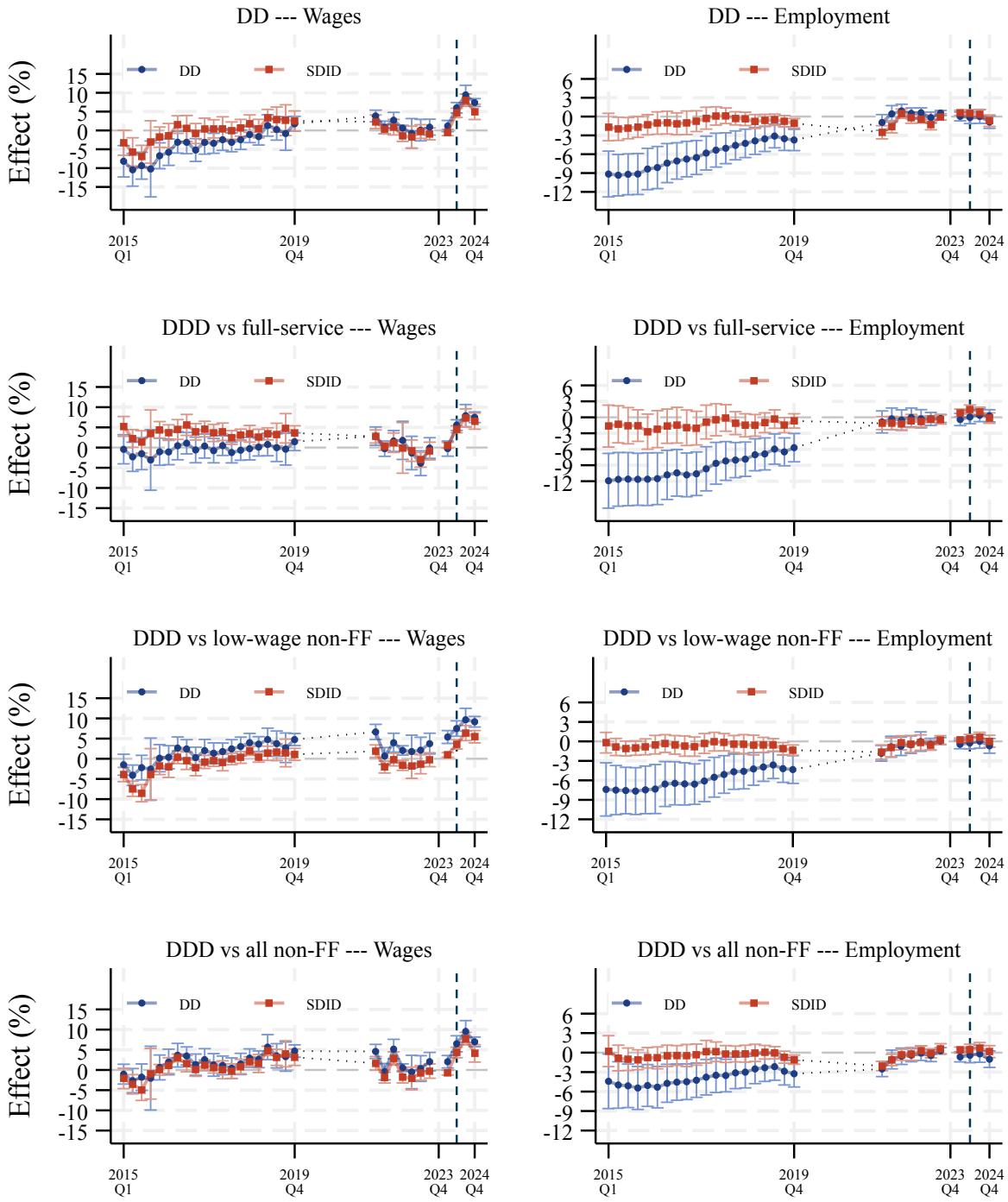

Figure 5: QWI event-study estimates: wages and employment, DD and SDID overlaid

Figure 5, continued.

Notes: Population-weighted QWI event-study coefficients with DD (navy circles) and SDID (red squares) overlaid in each cell. Layout parallels Figure 3: rows are estimator specification (top to bottom: DD, DDD vs. full-service, DDD vs. low-wage non-FF composite, DDD vs. all non-FF private); columns are wages (log EarnBeg) and employment (log Emp/pop). The corresponding separation-rate event studies appear in Appendix Figure A1. SDID/SDDD unit and time weights derived from log Emp/pop and applied to both outcomes; pre-treatment fitting excludes COVID quarters (2020Q1–2021Q4); the light dotted segment across the gap is a visual connector. Base: 2023Q4 (set missing in SDID/SDDD so the line breaks at the normalization point); vertical dashed line marks 2024Q2 (AB 1228 implementation). 95% CIs use Ferman–Pinto SEs. Sample: California vs. 20 federal-$7.25 states, NAICS 722513, all firm sizes, all ages, 2015Q1–2024Q4.

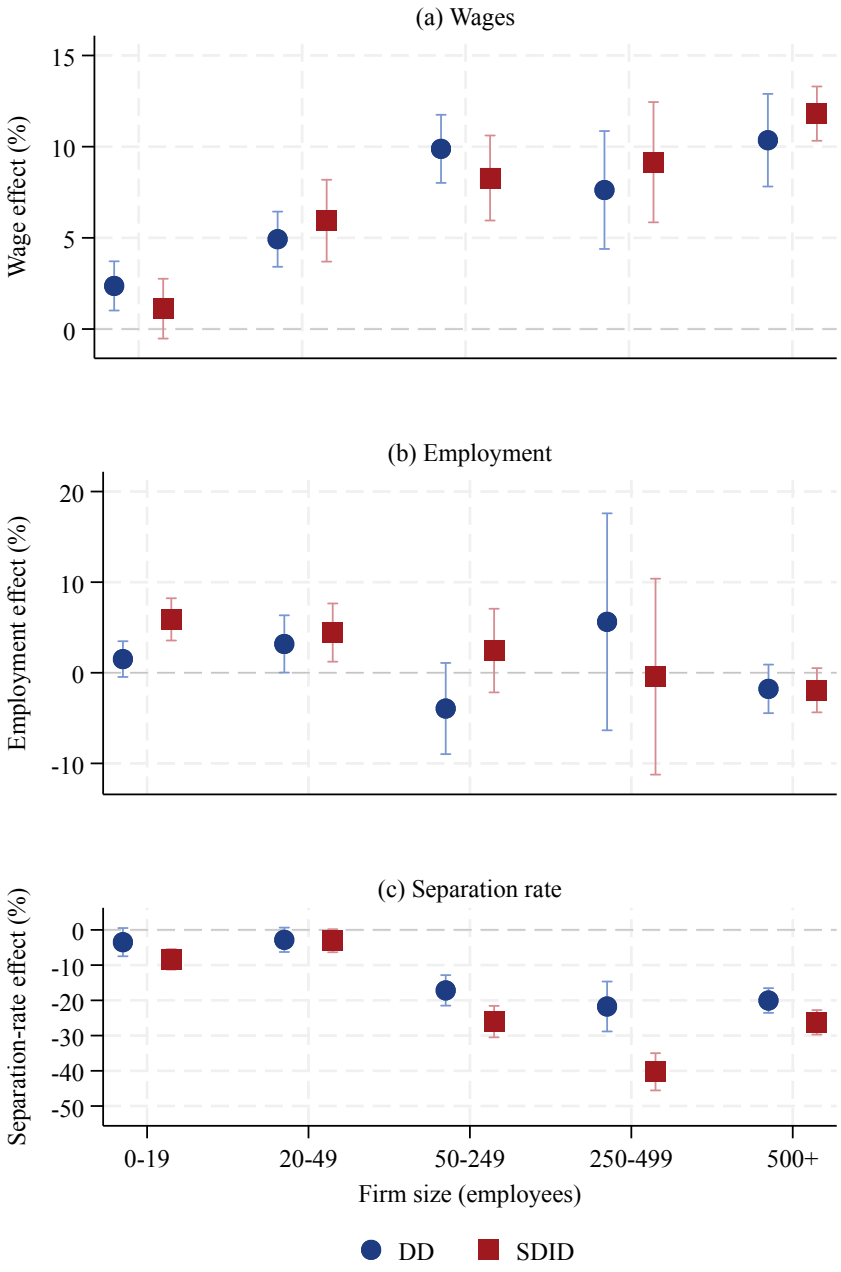

Figure 6: QWI firm-size heterogeneity: DD and SDID estimates by firm-size bin

Notes: Post-treatment average (2024Q2–Q4) relative to 2023Q4 base, by QWI firm-size bin. Panel (a): wages (log EarnBeg). Panel (b): employment rate (log Emp/pop). Panel (c): separation rate (log Sep/Emp). Circles: DD. Squares: SDID. SDID unit and time weights derived from log Emp within each bin for the wage and employment estimates; the separation rate uses own-weights. 95% CIs use Ferman–Pinto SEs. Sample: California vs. 20 federal-$7.25 states, NAICS 722513, 2015Q1–2024Q4 (QWI).

Table 1: Post-period average effects: California vs. control states

	DD, DDD			Synthetic DD, DDD		
	Wage (%)	Emp (%)	OWE	Wage (%)	Emp (%)	OWE
Panel A: Post-period average relative to 2023Q4 base						
Fast food	7.50	−1.39	−0.19	6.99	−0.26	−0.04
	[0.81]	[0.44]	(0.06)	[0.71]	[0.54]	(0.08)
vs Full-service	7.61	−0.38	−0.05	6.91	+0.07	+0.01
	[0.80]	[0.58]	(0.08)	[0.54]	[0.58]	(0.08)
vs Low-wage non-FF	7.55	−1.50	−0.20	7.48	−0.23	−0.03
	[0.96]	[0.43]	(0.06)	[0.65]	[0.46]	(0.06)
vs All non-FF private	7.61	−2.19	−0.29	6.36	+0.04	+0.01
	[0.90]	[0.39]	(0.06)	[0.71]	[0.49]	(0.08)
Panel B: Post-period average relative to pre-period average (2022Q1–2023Q3) base						
Fast food	6.75	−1.28	−0.19	6.71	−0.02	−0.00
	[0.61]	[0.60]	(0.09)	[0.60]	[0.66]	(0.10)
vs Full-service	7.19	+0.28	+0.04	6.53	+0.35	+0.05
	[0.52]	[0.77]	(0.11)	[0.57]	[0.74]	(0.11)
vs Low-wage non-FF	6.00	−0.44	−0.07	6.13	+0.32	+0.05
	[0.65]	[0.71]	(0.12)	[0.52]	[0.63]	(0.10)
vs All non-FF private	4.70	−1.02	−0.22	5.46	+0.57	+0.10
	[0.57]	[0.63]	(0.14)	[0.53]	[0.66]	(0.12)

Notes: Sample: California vs. 20 federal-$7.25 states, NAICS 722513 (fast-food restaurants), 2015Q1–2025Q3 (QCEW). Wage: log average weekly wage (total quarterly wages / employment / 13). Emp: log Emp/pop. Panel A: post-average (2024Q2–2025Q3, six quarters) minus 2023Q4 base. Panel B: post-average minus the 2022Q1–2023Q3 pre-period average (seven quarters). DDD outcomes are log differences between fast food and the indicated within-state comparison sector: full-service restaurants (NAICS 722511); low-wage non-fast-food composite (NAICS 72 − 722513, plus the BLS 1021 supersector aggregate and NAICS 56; uses only codes with complete data across all control states); all private employment net of fast food (NAICS 10 − 722513). Left columns: population-weighted DD/DDD event-study regressions. Right columns: SDID/SDDD following Arkhangelsky et al. (2021), excluding 2020–2021 from pre-treatment fitting; unit and time weights derived from employment and applied to both outcomes. Ferman–Pinto SEs in brackets; OWE SEs in parentheses (see §2.3 for the SE construction). Panel B FP SEs are approximate (computed under independence of pre and post placebo averages). Pre-COVID seasonal adjustment via 2×4 centered moving-average decomposition.

Table 2: QWI estimates: California fast food vs. 20 federal-minimum-wage states

	DD, DDD				Synthetic DD, DDD			
	Wage (%)	Emp (%)	Sep (%)	OWE	Wage (%)	Emp (%)	Sep (%)	OWE
Panel A: Post-period average (2024Q2–2024Q4) relative to 2023Q4 base								
Main (FF vs. controls)	+7.62	−0.34	−12.74	−0.04	+5.60	+0.74	−18.97	+0.13
	[0.67]	[0.49]	[1.17]	(0.06)	[0.66]	[0.46]	[1.30]	(0.08)
vs Full-service (722511)	+7.00	+0.25	−13.08	+0.04	+6.75	+1.49	−17.67	+0.22
	[0.71]	[0.57]	[1.13]	(0.08)	[0.59]	[0.72]	[3.37]	(0.11)
vs Low-wage non-FF	+8.77	−0.30	−15.37	−0.03	+6.35	+0.70	−17.95	+0.11
	[0.87]	[0.50]	[1.07]	(0.06)	[0.52]	[0.42]	[0.71]	(0.07)
vs All non-FF private	+7.67	−0.54	−18.84	−0.07	+5.01	+0.77	−15.71	+0.15
	[0.73]	[0.61]	[1.14]	(0.08)	[0.38]	[0.50]	[0.97]	(0.10)
Panel B: Post-period average minus pre-period average (2022Q1–2023Q3) base								
Main (FF vs. controls)	+6.44	−0.62	−22.49	−0.10	+6.00	+0.87	−21.71	+0.15
	[0.59]	[0.47]	[1.58]	(0.07)	[0.61]	[0.49]	[1.81]	(0.08)
vs Full-service (722511)	+6.91	+0.59	−19.80	+0.09	+6.23	+1.65	−23.14	+0.26
	[0.75]	[0.88]	[2.61]	(0.13)	[0.76]	[0.85]	[4.38]	(0.14)
vs Low-wage non-FF	+5.77	+0.31	−22.47	+0.05	+5.90	+0.97	−19.21	+0.16
	[0.58]	[0.62]	[1.37]	(0.11)	[0.47]	[0.52]	[0.75]	(0.09)
vs All non-FF private	+5.99	+0.14	−25.12	+0.02	+5.72	+0.98	−18.67	+0.17
	[0.64]	[0.55]	[1.44]	(0.09)	[0.57]	[0.50]	[1.00]	(0.09)

Notes: Post-treatment period: 2024Q2–2024Q4 (QWI ends 2024Q4). Panel A: post-average minus 2023Q4 base. Panel B: post-average minus the 2022Q1–2023Q3 pre-period average (seven quarters). Sample: California vs. 20 federal-$7.25 states, NAICS 722513 (fast-food restaurants), all firm sizes, all ages, 2015Q1–2024Q4. Wage: log EarnBeg. Emp: log Emp/pop. Sep: log Sep/Emp. DDD outcomes are log differences between fast-food and the indicated within-state comparison sector: full-service restaurants (NAICS 722511); low-wage non-fast-food composite (NAICS 72 − 722513, plus NAICS 22, 42, 44–45, 48–49, and 56, parallel to the QCEW Low-wage non-FF composite in Table 1); all private employment net of fast food (NAICS 00 minus 722513). SDID/SDDD weights: emp-derived for wage and employment; own-weights for the separation rate. Ferman–Pinto SEs in brackets; OWE SEs in parentheses (see §2.3 for the SE construction). Pre-COVID seasonal adjustment via 2×4 centered moving-average decomposition.

Table 3: Own-wage elasticities in context

	Wage (%)	Emp (%)	OWE
California AB 1228 fast-food minimum wage ($20, April 2024)			
This paper: range across 32 QCEW + QWI estimates: −0.29 *to* +0.26			
QCEW SDID, Panel A (preferred)	+7.0	−0.3	−0.04
QCEW DD, Panel A (conventional)	+7.5	−1.4	−0.19
QCEW most negative: DD all non-FF, Panel A	+7.6	−2.2	−0.29
QCEW most positive: SDDD all non-FF, Panel B	+5.5	+0.6	+0.10
QWI main DD, Panel A	+7.6	−0.3	−0.04
QWI main SDID, Panel A	+5.6	+0.7	+0.13
QWI most negative: main DD, Panel B	+6.4	−0.6	−0.10
QWI most positive: SDDD vs. Full-service, Panel B	+6.2	+1.7	+0.26
Clemens, Edwards, and Meer (2025) (median across specs)	+8.0	−3.2	−0.40
Sosinskiy and Reich (2026) (preferred DDD)	+7.1	−0.9	−0.12
Post-2010 state MW events, this paper (Figure 4)			
QCEW long-difference, median across 26 states	–	–	−0.02
Interquartile range	–	–	[−0.34, +1.25]
QWI long-difference, median across 25 states	–	–	+0.02
Interquartile range	–	–	[−0.41, +0.92]
U.S. restaurant-sector literature (OWE Repository)			
Median (28 studies)	–	–	−0.15
Interquartile range	–	–	[−0.04, −0.30]

Notes: OWE = own-wage elasticity of employment (%Δ employment / %Δ wage). This-paper QCEW estimates: population-weighted, post-period averages (2024Q2–2025Q3), Panel A 2023Q4 base, Panel B subtracts pre-period average 2022Q1–2023Q3 (Table 1). QWI estimates: post-period averages (2024Q2–2024Q4), same Panel A/B conventions (Table 2). Post-2010 state MW events: one long-difference OWE per state with a first post-2010 MW increase ≥5% plus CA's general-MW trajectory. Pre/post windows, sample selection, and the AK/DC exclusion follow Figure 4. Literature: the OWE Repository, maintained by the author and Ben Zipperer (`https://economic.github.io/owe`); median and IQR across 28 U.S. restaurant-sector studies. Clemens, Edwards, and Meer (2025): OWE implied from median employment effect across specifications (−3.2/8.0 = −0.40); their unadjusted DD gives −0.34. Sosinskiy and Reich (2026): preferred DDD OWE; wage and employment effects are DDD averages across post-implementation quarters (individually rounded).

Table 4: QWI firm-size heterogeneity: post-treatment average effects

	DD			SDID		
Firm size (employees)	Wage (%)	Emp (%)	Sep (%)	Wage (%)	Emp (%)	Sep (%)
≤ 19	+2.36	+1.51	−3.49	+1.12	+5.89	−8.37
	[0.69]	[1.01]	[2.04]	[0.84]	[1.19]	[1.48]
20–49	+4.93	+3.18	−2.82	+5.94	+4.44	−3.08
	[0.77]	[1.61]	[1.77]	[1.14]	[1.64]	[1.67]
50–249	+9.88	−3.95	−17.17	+8.28	+2.45	−26.04
	[0.95]	[2.57]	[2.21]	[1.19]	[2.35]	[2.28]
250–499	+7.62	+5.62	−21.74	+9.15	−0.43	−40.28
	[1.65]	[6.11]	[3.61]	[1.68]	[5.52]	[2.69]
500+	+10.35	−1.78	−20.05	+11.81	−1.93	−26.23
	[1.30]	[1.37]	[1.79]	[0.76]	[1.25]	[1.77]

Notes: Post-treatment average (2024Q2–2024Q4) relative to 2023Q4 base, separately for each QWI firm-size bin. Wage: log EarnBeg (average monthly earnings of beginning-of-quarter employees). Emp: log Emp/population. Sep: log Sep/Emp. Within each bin, SDID unit and time weights are derived from log Emp and applied to the wage and employment estimates (preserving OWE as a ratio on the same synthetic California); the separation rate uses own-weights. Ferman–Pinto SEs in brackets. Pre-COVID seasonal adjustment within each bin using 2×4 centered moving-average decomposition.

Appendix A. Additional figures

This appendix contains two figures referenced from the main text. Figure A1 is the QWI separation-rate event study underlying the worker-flow discussion in §4. Figure A2 plots the MW elasticity of the separation rate, $\Delta \log(\text{Sep/Emp})/\Delta \log(\text{MW})$, on the same Kaitz axis as Figure 4 panels (a)–(b)—a separation-rate analog of the wage and employment elasticities reported there.

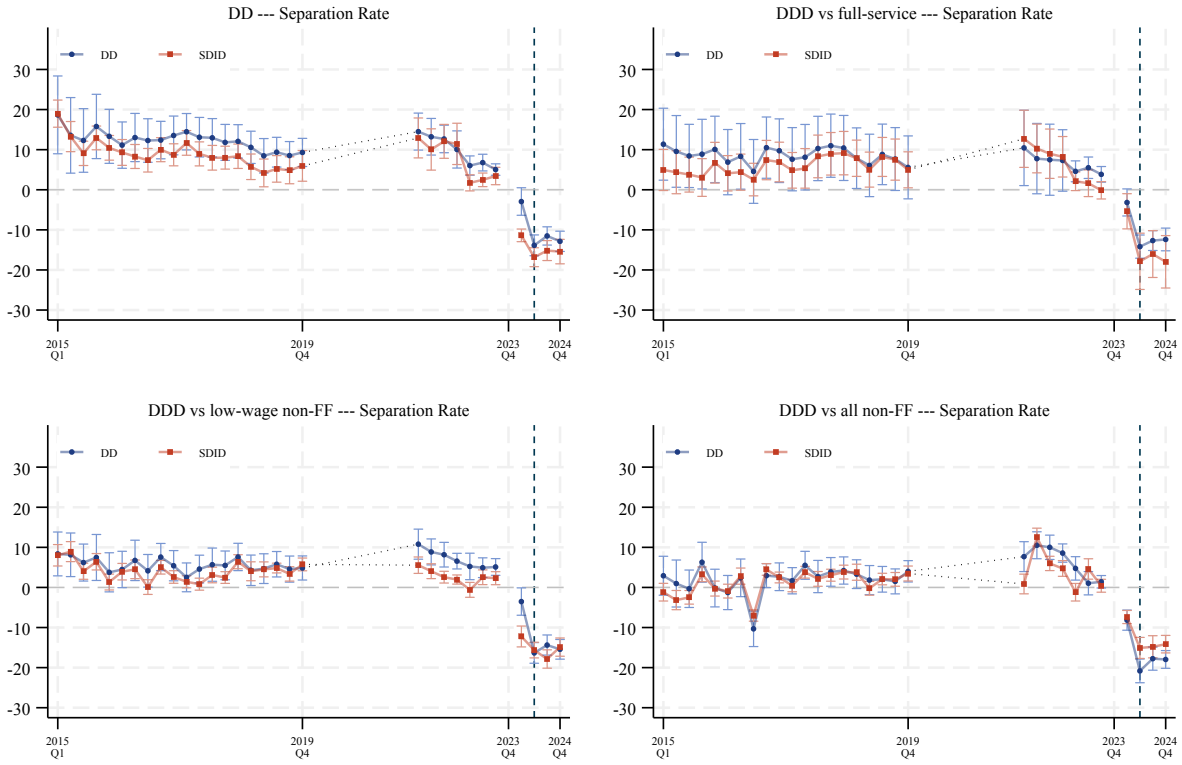

Figure A1: QWI event-study estimates: separation rate, DD and SDID overlaid

Notes: Population-weighted QWI event-study coefficients on log Sep/Emp (the separation rate as a share of beginning-of-quarter employment) with DD (navy circles) and SDID (red squares) overlaid in each cell. Layout mirrors the four DD/DDD specifications in Figure 5: top-left, DD; top-right, DDD vs. full-service restaurants; bottom-left, DDD vs. low-wage non-FF composite; bottom-right, DDD vs. all non-FF private. Unit and time weights for SDID are derived from the separation-rate outcome itself (own-weights). Pre-treatment fitting excludes 2020Q1–2021Q4; the dotted segment across the gap is a visual connector. Base: 2023Q4; vertical dashed line marks 2024Q2 (AB 1228 implementation). 95% CIs use Ferman–Pinto SEs. Sample: California vs. 20 federal-$7.25 states, NAICS 722513, all firm sizes, all ages, 2015Q1–2024Q4.

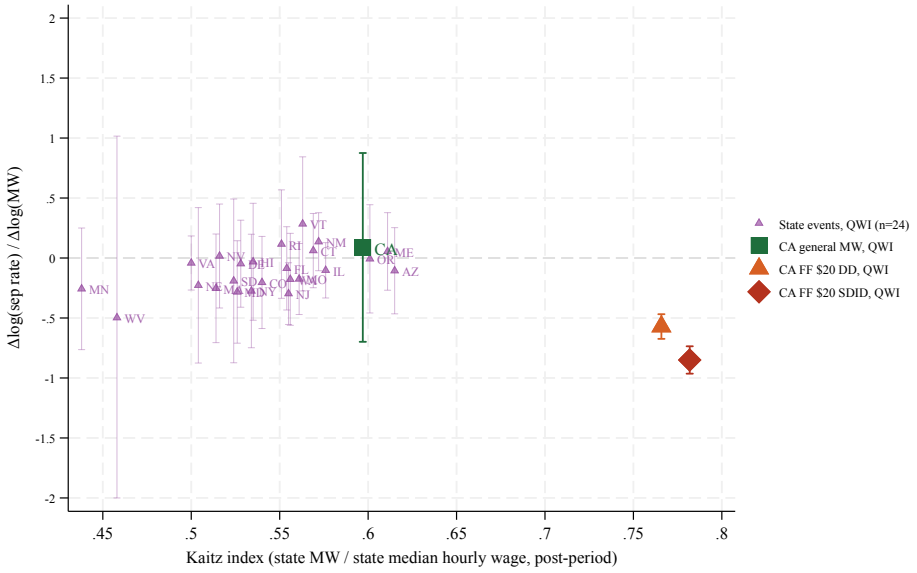

Figure A2: Separation-rate MW elasticity by Kaitz index, QWI

Notes: Y-axis: $\Delta \log(\text{Sep/Emp})/\Delta \log(\text{MW})$, the MW elasticity of the separation rate, parallel to Figure 4 panels (a)–(b). QWI only (QCEW does not report separations). Markers, windows, controls, and CI conventions match Figure 4; CIs are clipped at ±2.

Appendix B. Reconciling the QCEW and QWI based estimates

The headline DD employment estimates from QCEW and QWI differ: −1.4 percent in QCEW (Table 1, Panel A) versus −0.3 percent in QWI (Table 2, Panel A), implying OWEs of −0.19 and −0.04. The two data sources also use different post-windows: QCEW runs through 2025Q3, QWI ends in 2024Q4. Table B1 addresses the window concern by re-running the QCEW specifications on the QWI-matched 2024Q2–2024Q4 post-window. The matched QCEW estimates are essentially unchanged from the full-window numbers in Table 1: the DD Panel A OWE moves from −0.19 to −0.18, the preferred SDID from −0.04 to −0.03, and Panel B SDID estimates straddle zero in both versions. Wage effects shift modestly upward (DD/DDD +7.4 to +7.8 vs. +7.5 to +7.6; SDID/SDDD +7.1 to +7.6 vs. +6.4 to +7.5), reflecting continued wage accumulation through 2025. The window difference therefore explains little of the QCEW-vs-QWI gap, which must instead reflect how the two data constructs measure employment.

QCEW counts every worker on the payroll during the pay period containing the 12th of each month: a position filled by two workers via mid-period turnover is counted twice. QWI's beginning-of-quarter `Emp`, by contrast, counts only workers with positive earnings at the same employer in both the previous and current quarter; it is unaffected by within-pay-period turnover. When quits fall, the QCEW double-counting shrinks and the QCEW headcount declines mechanically, even when the true number of jobs is unchanged. The QWI count is essentially invariant to within-quarter turnover in steady state. The two constructs therefore respond asymmetrically to the policy-induced fall in turnover documented in Section 4, pushing the QCEW DD employment estimate more negative than the QWI DD estimate.

A simple model quantifies the predicted magnitude. Consider a fast-food employer with 100 stable positions and a quarterly separation rate s. Assume turnover events are uniformly distributed across the 90-day quarter and pay periods are biweekly—the standard schedule for hourly restaurant workers. The expected number of turnover events at a given position during a 14-day pay period is $s \times 14/90 \approx 0.156\,s$, so the expected QCEW headcount is $100 \times (1 + 0.156\,s)$. For QWI, every worker present at the start of q also had earnings in $q-1$ in steady state, so $\texttt{Emp}_q \approx 100$ regardless of s. The log-level QCEW-vs-QWI gap is therefore approximately $0.156\,s$ in log points. Pre-AB 1228 California fast-food separation rates were near 0.28 per quarter (Section 4), implying a +4.3 log-point baseline gap. The 13 to 25 log-point reduction in s documented in Table 2 brings s down to 0.22–0.25, lowering the gap to +3.3 to +3.8 log points. The model therefore predicts a −0.5 to −0.9 log-point

movement in the QCEW-vs-QWI ratio for California fast food after AB 1228, from the QCEW double-counting channel alone.

Two empirical exercises confirm this prediction. First, a direct DD on the log of the QCEW-to-QWI employment ratio as the outcome—population-weighted, with state and quarter fixed effects, and the same pre-COVID seasonal adjustment and 2023Q4 base as Table B1—yields a coefficient of −0.98 log points (cluster SE 0.16) for fast food and +0.06 log points (SE 0.09) for non-fast-food private employment (NAICS 10 net of fast food in QCEW; NAICS 00 net of fast food in QWI). The fast-food-specific decline rules out a generic CA-vs-rest-of-country data-source artifact, and its magnitude—at the upper end of the model's −0.5 to −0.9 range—suggests the QCEW double-counting channel can plausibly account for essentially the entire observed gap.

Second, a regression relates the log QCEW-to-QWI employment ratio directly to the log separation rate. Pooling the two restaurant industries (NAICS 722513 and NAICS 722511) across CA + 20 federal-MW controls and pre-treatment quarters (2015Q1–2024Q1, excluding 2020Q1–2021Q4), with state-by-industry and quarter fixed effects, yields an elasticity of +0.040 (state-clustered SE 0.010); Figure B1 shows the binned-scatter version. The model's analytical slope $d\log(\text{QCEW}/\text{QWI}) \,/\, d\log s = 0.156\, s$ evaluates to $0.156 \times 0.28 \approx 0.044$ at the average pre-period separation rate—well within sampling error of the empirical estimate. Applied to the 13 to 25 log-point separation-rate cut, the empirical elasticity implies a 0.5 to 1.0 log-point reduction in the QCEW-vs-QWI gap, bracketing both the model's prediction and the direct DD estimate.

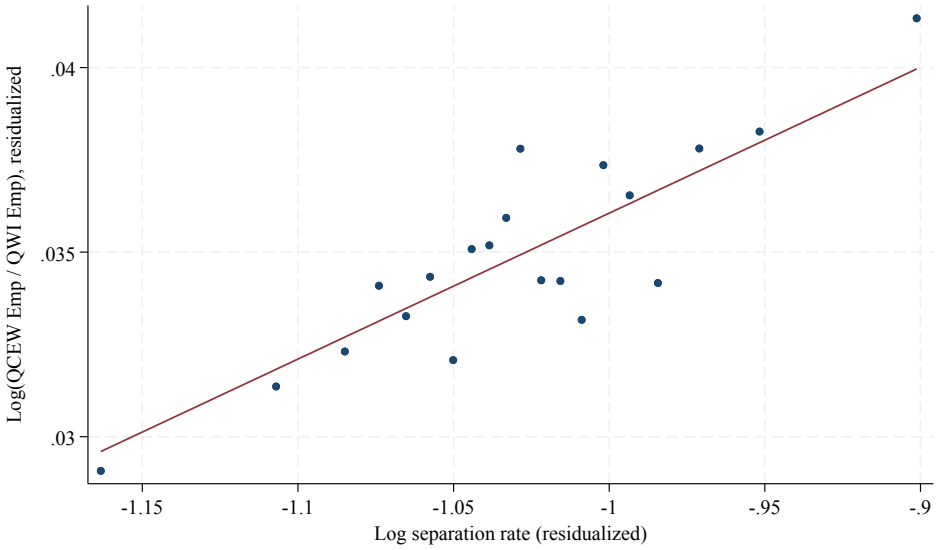

Figure B1: Binned scatter: log(QCEW Emp/QWI Emp) vs. log(separation rate), residualized

Notes: Twenty equal-frequency bins on the residualized x-axis. Sample: pooled NAICS 722513 (fast food) and NAICS 722511 (full-service), CA + 20 federal-MW non-increaser controls, pre-treatment quarters 2015Q1–2024Q1 excluding 2020Q1–2021Q4 (N = 1,218 state-industry-quarter observations). Both axes are residualized on state-by-industry and quarter fixed effects; sample means are added back so the axis values are interpretable. The fitted line corresponds to the regression $\log(\text{QCEW Emp/QWI Emp}) = \beta \log(\text{separation rate}) + \alpha_{si} + \gamma_t + \varepsilon_{sit}$, with $\hat{\beta} = +0.040$ (state-clustered SE 0.010). The relationship is the within state-industry $\times$ within quarter co-movement of the QCEW-vs-QWI employment gap with the separation rate.

Table B1: QCEW estimates with QWI-matched post-window (2024Q2–2024Q4)

	DD, DDD			Synthetic DD, DDD		
	Wage (%)	Emp (%)	OWE	Wage (%)	Emp (%)	OWE
Panel A: Post-period average (2024Q2–2024Q4) relative to 2023Q4 base						
Fast food	+7.37	−1.32	−0.18	+7.13	−0.21	−0.03
	[0.64]	[0.37]	(0.05)	[0.63]	[0.49]	(0.07)
vs Full-service	+7.39	−0.38	−0.05	+7.09	+0.20	+0.03
	[0.73]	[0.54]	(0.07)	[0.52]	[0.56]	(0.08)
vs Low-wage non-FF	+7.36	−1.52	−0.21	+7.55	−0.16	−0.02
	[0.69]	[0.40]	(0.06)	[0.58]	[0.43]	(0.06)
vs All non-FF private	+7.83	−2.14	−0.27	+7.37	+0.09	+0.01
	[0.70]	[0.37]	(0.05)	[0.48]	[0.43]	(0.06)
Panel B: Post-period average minus pre-period average (2022Q1–2023Q3) base						
Fast food	+6.63	−1.21	−0.18	+7.07	+0.05	+0.01
	[0.52]	[0.54]	(0.08)	[0.63]	[0.63]	(0.09)
vs Full-service	+6.97	+0.28	+0.04	+6.53	+0.48	+0.07
	[0.47]	[0.73]	(0.11)	[0.52]	[0.70]	(0.11)
vs Low-wage non-FF	+5.82	−0.47	−0.08	+6.46	+0.36	+0.05
	[0.47]	[0.68]	(0.12)	[0.57]	[0.55]	(0.08)
vs All non-FF private	+4.92	−0.96	−0.20	+6.82	+0.55	+0.08
	[0.59]	[0.60]	(0.12)	[0.76]	[0.58]	(0.09)

Notes: Same sample, panel construction, seasonal adjustment, and estimators as Table 1, but with the post-treatment period restricted to 2024Q2–2024Q4 (three quarters) to match the QWI extract's coverage in Table 2. Ferman–Pinto SEs in brackets; OWE SEs in parentheses (see §2.3 for the SE construction). Panel B FP SEs are approximate (computed under independence of pre and post placebo averages).

Appendix C. SDID estimation details and robustness

All SDID and SDDD estimates in the paper are produced by my own Python implementation of the Arkhangelsky et al. (2021) estimator (`scripts/sdid_mine.py`). The Python module solves the two quadratic programs below by SLSQP and exposes options for the regularization parameter, separate-vs-shared unit/time weights, and a ridge-without-simplex variant. Row 7 of Table C1 validates the implementation against the Clarke–Pailañir Stata package (Clarke and Pailañir, 2022).

Solving for $\hat{\omega}$, $\hat{\lambda}$, and $\hat{\zeta}$

The SDID estimator uses two sets of weights, chosen jointly by regularized least-squares with simplex constraints. Let Y^{co} denote the $N_c \times T_{\text{pre}}$ matrix of control-unit outcomes and y^{CA} the treated-unit pre-period outcome vector. Let $\bar{Y}^{\text{co,post}}$ be the vector of post-period averages for controls.

The *unit weights* $\hat{\omega} \in \Delta^{N_c}$ (weights on the 20 control states) solve

$$\hat{\omega} \;=\; \arg\min_{\omega \in \Delta^{N_c}} \left\| y^{\text{CA}} - \omega^{\top} Y^{\text{co}} \right\|_2^2 \;+\; \hat{\zeta}^{\,2}\, N_c\, \|\omega\|_2^2,$$

subject to $\omega \geq 0$ and $\sum_i \omega_i = 1$. These are synthetic-control weights with an L_2 (ridge) penalty that dampens concentration on any single state.

The *time weights* $\hat{\lambda} \in \Delta^{T_{\text{pre}}}$ (weights on pre-treatment quarters) solve

$$\hat{\lambda} \;=\; \arg\min_{\lambda \in \Delta^{T_{\text{pre}}}} \left\| \bar{Y}^{\text{co,post}} - Y^{\text{co}} \lambda \right\|_2^2 \;+\; \hat{\zeta}^{\,2}\, T_{\text{pre}}\, \|\lambda\|_2^2,$$

subject to $\lambda \geq 0$ and $\sum_t \lambda_t = 1$. Pre-treatment quarters with control dynamics close to the post-period receive more weight; this is the main feature distinguishing SDID from plain synthetic control.

The *regularization parameter* $\hat{\zeta}$ is the ADHS data-driven choice

$$\hat{\zeta} \;=\; (N_{\text{tr}}\, T_{\text{post}})^{1/4} \cdot \hat{\sigma},$$

where $\hat{\sigma}$ is the empirical standard deviation of the first differences of the two-way-demeaned control panel $Y^{\text{co}} - \bar{Y}^{\text{co}}_{\text{unit}} - \bar{Y}^{\text{co}}_{\text{time}} + \bar{Y}^{\text{co}}$. In my single-treated-unit setting, $N_{\text{tr}} = 1$. I derive $\hat{\omega}$, $\hat{\lambda}$, and $\hat{\zeta}$ from the log-employment-per-capita panel and apply the same weights to the wage outcome, as described in §2.4. All QP problems are solved by SLSQP.

Robustness to tuning choices

Table C1 reports the QCEW SDID (CA vs. 20 federal-MW controls, DD specification, Panel A) under the default tuning and six alternatives. Rows 2–6 vary the SDID tuning: three perturbations of $\hat{\zeta}$ (rows 2–4), the standard ADHS convention that fits separate weights to wages vs. employment (row 5), and a ridge-regression alternative that drops the simplex constraint while retaining the L_2 penalty (row 6). Row 7 is an external-software validation: the same panel passed through the Stata user-written `sdid` command of Clarke and Pailañir (2022), which independently implements the ADHS estimator.

Across all seven implementations, the wage effect lies between +5.2% and +8.0%, the employment effect between −0.05% and −0.79%, and the implied OWE between −0.098 and −0.007. Removing the ridge penalty entirely (row 2) leaves the OWE essentially at zero (−0.007); doubling it (row 4) leaves the OWE within 0.01 of baseline. Dropping the simplex constraint (row 6) produces the most negative OWE, −0.098, but the resulting unit weights are no longer interpretable as a convex combination of control states. The Stata implementation (row 7), using an independent code base and an independent optimizer, returns OWE −0.068—quantitatively close to the paper's headline SDID of −0.037 (row 1).

Table C1: SDID robustness to tuning: QCEW fast food, Panel A

	Specification	Wage (%)	Emp (%)	OWE
1	Baseline (default tuning: ADHS $\hat{\zeta}$, emp-derived $\hat{\omega}, \hat{\lambda}$, simplex)	+6.99	−0.26	−0.037
2	$\hat{\zeta} = 0$ (no ridge; pure synthetic control)	+7.08	−0.05	−0.007
3	$\hat{\zeta} \times 0.5$	+6.80	−0.36	−0.054
4	$\hat{\zeta} \times 2$	+6.99	−0.24	−0.034
5	Standard ADHS: separate weights fit to wage and emp	+5.24	−0.26	−0.049
6	No simplex constraint (ridge synthetic control)	+8.04	−0.79	−0.098
7	Stata `sdid` package (Clarke and Pailañir, 2022), external validation	+6.85	−0.46	−0.068

Notes: All rows estimated on the same seasonally adjusted panel, CA vs. 20 federal-MW controls, 2015Q1–2023Q4 pre-period excluding 2020Q1–2021Q4, 2024Q2–2025Q3 post. Row 1 is the default-tuning baseline used throughout the main paper; rows 2–6 vary one tuning choice at a time; row 7 is the Stata user-written `sdid` command of Clarke and Pailañir (2022) on the same panel (100 placebo replications, placebo variance, seed 12345). Emp-derived weights in rows 1–4, 6, and 7 apply the $\hat{\omega}, \hat{\lambda}$ from log Emp/pop to the log wage outcome; row 5 fits separate weights for each outcome (the ADHS standard convention).

Appendix D. Reconciling CEM and SDID estimates

This appendix assesses how Clemens, Edwards, and Meer (2025)'s reported OWE estimates respond to small modifications of their methodological choices, and how the same specifications perform when applied to QWI. Tables D1 (QCEW) and D2 (QWI) share the same structure. Four supercolumns summarize CEM's Table 1 Panel B: *(1)* DDD vs. all-non-FF detrended (CEM's most-negative spec, OWE −0.49), *(2)* the same DDD without detrending (−0.32), *(3)* DD detrended (−0.40, CEM's headline), and *(4)* DD without detrending (−0.34). Two of CEM's own choices—whether to detrend and which comparison group—already produce a 0.17-unit internal range. Row 2 of each table is my replication; rows 3–8 apply six further modifications in sequence: drop linear detrending; population normalization; multi-quarter pre/post averaging windows; my pre-COVID 2×4 CMA seasonality; my 20 fed-MW non-increaser controls; synthetic-control weighting (SDID).

Which CEM choices matter, on QCEW. Four of the six modifications make first-order contributions; two are essentially irrelevant. (i) *Detrending*: removing CEM's compound-monthly-growth linear detrending shrinks the DDD OWE by 0.14 in col (1) (−0.43 → −0.29) and the DD OWE by 0.06 in col (3) (−0.42 → −0.36). (ii) *Population normalization*: replacing log(emp) with log(emp/pop) contributes 0.07 to the DD OWE (col (3): −0.36 → −0.29); it is mechanically irrelevant for DDD because population cancels in the within-state difference. The DD effect arises because California's population grew about 0.7 percentage points slower than CEM's donor pool over Sept-to-Sept, and log(emp) attributes that demographic differential to the policy. (iii) *Averaging window*: replacing CEM's single-month Sept-to-Sept comparison with multi-quarter averages shrinks the DDD OWE by 0.13 (−0.29 → −0.16) and the DD OWE by 0.07 (−0.29 → −0.22); the single-month snapshot overweights idiosyncratic variation in Sept 2024 that the average smooths out. (iv) *Synthetic-control weighting*: SDID/SDDD moves the DDD OWE from −0.14 to +0.01 and the DD OWE from −0.20 to −0.04. Two further modifications make essentially no difference: my pre-COVID seasonality contributes ≈ 0.02 to each path; the donor-pool swap leaves the DDD OWE unchanged (CEM's non-increaser set already approximates mine) and produces only a small wage adjustment for the DD path.

How CEM's specifications perform on QWI. Applying CEM's specifications unchanged to QWI rather than QCEW (Table D2, row 2) substantially reduces the absolute OWE in every column: DDD detrended moves from −0.49 on QCEW to −0.24 on QWI, DDD un-detrended from −0.32 to −0.06, DD detrended from −0.40 to −0.11, and DD un-

detrended from −0.34 to +0.19. The data-source switch alone, holding the specification fixed, accounts for roughly half of the gap to my preferred estimates—reflecting the QCEW double-counting mechanism described in §4.3, which biases QCEW estimates downward when separations fall, as they did under AB 1228. Within QWI, the row-by-row decomposition runs the same way: detrending and SDID are again the largest contributors; population normalization and multi-quarter windows matter less because the row 2 starting OWEs are already smaller in absolute value. By row 8 the four supercolumns collapse to two preferred estimates: SDDD-vs-all-non-FF at +0.15 and SDID at +0.13. On both data sources, then, the gap between CEM's most-negative estimate (−0.49) and my preferred SDID traces to the same small set of methodological choices—detrending, DDD-with-all-non-FF, population normalization (DD path), and synthetic-control weighting—together with the data-source switch, which contributes a comparable share that no methodological refinement on QCEW alone can address.

Table D2 repeats the decomposition on QWI; row 1 carries CEM's QCEW-reported numbers for reference (CEM did not separately estimate on QWI), and row 2 is my QWI replication of CEM's specs (Q3-to-Q3, the quarterly analog of Sept-to-Sept). Subsequent rows apply the same six modifications as Table D1.

Table D1: Reconciling Clemens, Edwards, and Meer (2025) and SDID employment elasticities (QCEW)

		(1) DDD vs all non-FF, detrended			*(2) DDD vs all non-FF*			*(3) DD, detrended*			*(4) DD*		
	Specification step	W (%)	E (%)	OWE	W (%)	E (%)	OWE	W (%)	E (%)	OWE	W (%)	E (%)	OWE
1	Clemens, Edwards, and Meer (2025) reported (their Table 1)	+8.0	−3.9	−0.49	+8.0	−2.6	−0.32	+8.0	−3.2	−0.40	+8.0	−2.7	−0.34
2	My replication of CEM's spec (CEM seasonality, CEM controls, Sept-to-Sept)	+8.5	−3.6	−0.43	+8.5	−2.5	−0.29	+8.0	−3.4	−0.42	+8.0	−2.9	−0.36
3	+ Drop linear detrending	+8.5	−2.5	−0.29	+8.5	−2.5	−0.29	+8.0	−2.9	−0.36	+8.0	−2.9	−0.36
4	+ Population normalization, log(emp/pop)	+8.5	−2.5	−0.29	+8.5	−2.5	−0.29	+8.0	−2.3	−0.29	+8.0	−2.3	−0.29
5	+ My multi-quarter pre/post windows	+6.7	−1.0	−0.16	+6.7	−1.0	−0.16	+5.5	−1.2	−0.22	+5.5	−1.2	−0.22
6	+ My pre-COVID 2×4 CMA seasonality	+6.7	−0.9	−0.14	+6.7	−0.9	−0.14	+5.5	−1.1	−0.20	+5.5	−1.1	−0.20
7	+ My 20 fed-MW non-increaser controls	+6.7	−0.9	−0.14	+6.7	−0.9	−0.14	+6.7	−1.3	−0.20	+6.7	−1.3	−0.20
8	+ Synthetic-control weighting (SDID/SDDD)	+6.4	+0.0	+0.01	+6.4	+0.0	+0.01	+7.0	−0.3	−0.04	+7.0	−0.3	−0.04

Notes: Each supercolumn is a starting CEM specification, modified row by row toward my preferred specification. Row 1 reproduces Clemens, Edwards, and Meer (2025)'s Table 1 Panel B figures. Cols (1)–(2) use CEM's Non-Increaser controls; cols (3)–(4) use CEM's Rest-of-US controls. The W (%) column reports the FF wage DiD against the same controls (CEM's convention); CEM does not decompose wages by control group, so row 1's wage cell is their reported ≈ 8 percent. Rows 2–7 are simple DiDs on monthly QCEW for NAICS 722513 (FF) and NAICS 10 (NONFF = NAICS 10 − 722513). Detrending follows Clemens, Edwards, and Meer (2025): project levels forward by compound monthly growth from Sept 2021 to Sept 2023, subtract the projection, apply seasonality. Rows 3–8 apply six methodological modifications in sequence: (3) drop detrending; (4) population normalization, log(emp/pop); (5) my multi-quarter pre/post windows (pre = 2022Q1–2023Q3, post = 2024Q2–2025Q3); (6) my pre-COVID 2×4 CMA seasonality; (7) my 20 fed-MW non-increaser controls; (8) synthetic-control weighting. Where a modification leaves a cell unchanged by construction—row 4 on cols (1)–(2) (population cancels in the DDD within-state difference), row 7 on cols (1)–(2) (my fed-MW set already approximates CEM's non-increasers)—the cell repeats the row above. Row 8 jumps to the event-study with state and quarter fixed effects from Table 1 Panel A: cols (1)–(2) use the ddd_C SDDD; cols (3)–(4) use the DD SDID.

Table D2: Reconciling Clemens, Edwards, and Meer (2025) and SDID employment elasticities (QWI)

		(1) DDD vs all non-FF, detrended			*(2) DDD vs all non-FF*			*(3) DD, detrended*			*(4) DD*		
	Specification step	W (%)	E (%)	OWE	W (%)	E (%)	OWE	W (%)	E (%)	OWE	W (%)	E (%)	OWE
1	Clemens, Edwards, and Meer (2025) reported on QCEW (their Table 1; for reference)	+8.0	−3.9	−0.49	+8.0	−2.6	−0.32	+8.0	−3.2	−0.40	+8.0	−2.7	−0.34
2	My replication of CEM's spec on QWI (Q3-Q3, log emp)	+8.6	−2.0	−0.24	+8.6	−0.5	−0.06	+7.8	−0.9	−0.11	+7.8	+1.5	+0.19
3	+ Drop linear detrending	+8.6	−0.5	−0.06	+8.6	−0.5	−0.06	+7.8	+1.5	+0.19	+7.8	+1.5	+0.19
4	+ Population normalization, log(emp/pop)	+8.6	−0.5	−0.06	+8.6	−0.5	−0.06	+7.8	+2.0	+0.26	+7.8	+2.0	+0.26
5	+ My multi-quarter pre/post windows	+5.8	−0.3	−0.04	+5.8	−0.3	−0.04	+5.5	+1.6	+0.28	+5.5	+1.6	+0.28
6	+ My pre-COVID 2×4 CMA seasonality	+5.8	−0.3	−0.04	+5.8	−0.3	−0.04	+5.5	+1.6	+0.28	+5.5	+1.6	+0.28
7	+ My 20 fed-MW non-increaser controls	+5.8	−0.3	−0.04	+5.8	−0.3	−0.04	+5.8	−0.9	−0.15	+5.8	−0.9	−0.15
8	+ Synthetic-control weighting (SDID/SDDD)	+5.0	+0.8	+0.15	+5.0	+0.8	+0.15	+5.6	+0.7	+0.13	+5.6	+0.7	+0.13

Notes: Same row and supercolumn structure as Table D1, applied to QWI quarterly data. Wage variable: QWI `EarnBeg` (avg monthly earnings of beginning-of-quarter employees). Employment: QWI `Emp`. Row 1 carries CEM's QCEW-reported numbers (from Clemens, Edwards, and Meer (2025) Table 1) for direct reference; CEM did not separately estimate on QWI. Row 2 is my QWI replication of CEM's specifications: Q3-to-Q3 comparison (2023Q3 vs. 2024Q3, the quarterly analog of CEM's Sept-to-Sept), detrended via compound quarterly growth from 2021Q3 to 2023Q3 projected to 2024Q3. Rows 3–8 apply the same six methodological modifications as Table D1, with the QWI multi-quarter post window 2024Q2–2024Q4 (vs. 2024Q2–2025Q3 in QCEW). Where a modification leaves a cell unchanged by construction (row 4 on DDD cols, row 7 on cols where my non-increaser set already approximates CEM's), the cell repeats the row above. Row 8 jumps to the event-study with state and quarter fixed effects from Table 2 Panel A: cols (1)–(2) use the ddd_C SDDD; cols (3)–(4) use the QWI-spec SDID.