Online Appendix

for

Minimum Wage Effects and Monopsony Explanations

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A. Supplemental Analyses, Tables, and Figures

In this appendix section we provide supplemental analyses, tables and figures to those in the paper.

A.1. Regression-based estimators

In order to contextualize our synthetic control estimates in the larger minimum wage literature, we estimate wage and employment effects using a typical two-way fixed effects specification, as in the equation below:

$$Y_{ct} = \gamma_c + \lambda_t + \sum_{t \in T} \beta_t D_{ct} + \varepsilon_{ct} \tag{1}$$

where Y is the outcome of interest for county c and time t. The subscript t refers to quarters in the QCEW analysis. The set T contains all integers indexing t in event time, except for the period prior to the first minimum wage increase. D_{ct} is a treatment dummy equal to 1 if the county had a minimum wage increase and that increase has been implemented. The rest is standard: γ_s and λ_y are state and time-fixed effects.

We cluster standard errors at the state-level, and because of the small number of counties, we use the wild bootstrap procedure in callaway2020difference.[1] As with our synthetic control analysis, we weight by 2010 county population. Finally, we also estimate these outcomes for each of our jurisdiction/size groups using the synthetic difference-in-differences estimator (Arkhangelsky et al., 2021) with the same covariates as in our synthetic control, implemented using the `sdid` Stata package (Clarke et al., 2023). The delta method is then used to calculate the standard errors on the own-wage elasticities. These SDiD estimates are available upon request.

A.2. Employment Markdown Proof

Consider a monopsonistic labor market described by simple linear equations:

[1] We use the Callaway and Sant'Anna (2021) `csdid` Stata package to estimate our results for the convenience of calculating the standard errors. In our setting, however, their method calculates point estimates that differ from standard OLS. Since we estimate the event study on an "absorbing" treatment, with a never-taking control group, our results should not be affected by any of the recently-emphasized issues with dynamic DiD estimators. We allow room for heterogeneous treatment effects across different areas–such as if we pooled large New York and California counties. Nonetheless, since the pooled estimates accord with the synthetic control estimates, we report only the aggregated estimates.

$$\begin{aligned}
\text{Demand: } w &= a - dL \\
\text{Marginal Cost: } w &= sL \\
\text{Supply: } w &= 0.5sL
\end{aligned}$$

Consistent with Figure **??**, suppose that we enforce minimum wage w_1. Assuming that w_1 is below the competitive equilibrium, the change in employment levels $G_L = L_1 - L_M$ is given by:

$$G_L = \frac{2w_1}{s} - \frac{a}{s+d}$$

Thus, changes in the slope of the supply curve impact the employment gap such that:

$$\begin{aligned}
\frac{\partial G_L}{\partial s} &= \frac{a}{(s+d)^2} - \frac{2w_1}{s^2} \\
&= \frac{w_M}{s(s+d)} - \frac{2w_1}{s^2}
\end{aligned}$$

By assumption, $w_1 > w_M$, and, since s and d are positive, $s(s+d) > s^2$. Thus $\frac{\partial G_L}{\partial s} < 0$, and therefore, as the supply curve flattens out (s decreases), the observed employment effect G_L increases.

TABLE A.1
Local Minimum Wages in California

Local Area	2014	2015	2016	2017	2018	2019	2020	2021	2022
Alameda County									
Alameda	8.00	9.00	10.00	10.50	11.00	12.00	13.50	15.00	15.00
Berkeley	8.00	10.00	11.00	12.53	13.75	15.00	15.59	16.07	16.32
Emeryville	8.00	9.00	14.44	14.82	15.20	15.69	16.30	16.84	17.38
Fremont	8.00	9.00	10.00	10.50	11.00	12.00	13.50	15.00	15.00
Hayward	8.00	9.00	10.00	10.50	11.00	12.00	13.00	15.00	15.56
Oakland	8.00	9.00	12.55	12.86	13.23	13.80	14.14	14.36	15.06
San Leandro	8.00	9.00	10.00	10.50	12.00	13.00	14.00	15.00	15.00
Contra Costa County									
El Cerrito	8.00	9.00	10.00	12.25	13.60	15.00	15.37	15.61	16.37
Richmond	8.00	9.60	11.52	12.30	13.41	15.00	15.00	15.21	15.54
Los Angeles County									
Los Angeles	8.00	9.00	10.00	10.50	12.00	13.25	14.25	15.00	16.04
Malibu	8.00	9.00	10.00	10.50	12.00	13.25	14.25	15.00	16.04
Pasadena	8.00	9.00	10.00	10.50	12.00	13.25	14.25	15.00	17.10
Santa Monica	8.00	9.00	10.00	10.50	12.00	13.25	14.25	15.00	16.04
Unincorporated Areas	8.00	9.00	10.00	10.50	12.00	13.25	14.25	15.00	15.96
West Hollywood	8.00	9.00	10.00	10.50	12.00	13.25	14.25	15.00	16.54
Marin County									
Novato	8.00	9.00	10.00	10.50	11.00	12.00	13.00	15.24	15.77
San Diego County									
San Diego	8.00	9.75	10.50	11.50	11.50	12.00	13.00	14.00	15.00
San Mateo County									
Belmont	8.00	9.00	10.00	10.50	11.00	13.50	15.00	15.90	16.20
Redwood City	8.00	9.00	10.00	10.50	11.00	13.50	15.38	15.62	16.20
San Carlos	8.00	9.00	10.00	10.50	11.00	12.00	13.00	14.00	15.77
San Mateo	8.00	9.00	10.00	12.00	13.50	15.00	15.38	15.62	16.20
San Francisco County									
San Francisco	10.74	11.05	12.25	13.00	14.00	15.00	15.59	16.32	16.99
Santa Clara County									
Cupertino	8.00	9.00	10.00	12.00	13.50	15.00	15.35	15.65	16.40
East Palo Alto	8.00	9.00	10.00	10.50	11.00	12.00	13.00	15.00	15.60
Los Altos	8.00	9.00	10.00	12.00	13.50	15.00	15.40	15.65	16.40
Milpitas	8.00	9.00	10.00	10.50	12.00	13.50	15.00	15.40	15.65
Mountain View	8.00	9.00	11.00	13.00	15.00	15.65	16.05	16.30	17.10
Palo Alto	8.00	9.00	11.00	12.00	13.50	15.00	15.40	15.65	16.45
San Jose	10.15	10.30	10.30	10.50	13.50	15.00	15.25	15.45	16.20
Santa Clara	8.00	9.00	11.00	11.10	13.00	15.00	15.40	15.65	16.40
Sunnyvale	8.00	10.30	10.30	13.00	15.00	15.65	16.05	16.30	17.10
Sonoma County									
Petaluma	8.00	9.00	10.00	10.50	11.00	12.00	15.00	15.20	15.85
Santa Rosa	8.00	9.00	10.00	10.50	11.00	12.00	13.00	15.00	15.85
Sonoma	8.00	9.00	10.00	10.50	11.00	12.00	13.50	15.00	16.00

Note: This table shows the nominal minimum wage for employers with more than 25 employees at the beginning of each calendar year for every California locality with its own minimum wage law. Some localities, such as San Francisco, implement minimum wage changes on July 1. Sources: Vaghul and Zipperer (2021), the UC Berkeley Labor Center Local Minimum Wage Inventory and the authors' research.

TABLE A.2
Donor Pool Counties

Baldwin, AL	Durham, NC	Montgomery, TN	Milwaukee, WI
Jefferson, AL	Forsyth, NC	Rutherford, TN	Waukesha, WI
Madison, AL	Gaston, NC	Sevier, TN	
Mobile, AL	Guilford, NC	Shelby, TN	
Montgomery, AL	Mecklenburg, NC	Sullivan, TN	
Shelby, AL	New Hanover, NC	Williamson, TN	
Tuscaloosa, AL	Onslow, NC	Bell, TX	
Bibb, GA	Pitt, NC	Bexar, TX	
Chatham, GA	Wake, NC	Brazoria, TX	
Cherokee, GA	Cass, ND	Brazos, TX	
Clarke, GA	Cleveland, OK	Cameron, TX	
Clayton, GA	Oklahoma, OK	Collin, TX	
Cobb, GA	Tulsa, OK	Dallas, TX	
Dekalb, GA	Allegheny, PA	Denton, TX	
Henry, GA	Berks, PA	Ector, TX	
Muscogee, GA	Bucks, PA	El Paso, TX	
Ada, ID	Chester, PA	Fort Bend, TX	
Hamilton, IN	Cumberland, PA	Galveston, TX	
Hendricks, IN	Dauphin, PA	Gregg, TX	
Lake, IN	Delaware, PA	Harris, TX	
Marion, IN	Erie, PA	Hays, TX	
St Joseph, IN	Lancaster, PA	Hidalgo, TX	
Tippecanoe, IN	Lehigh, PA	Jefferson, TX	
Polk, IA	Luzerne, PA	Lubbock, TX	
Scott, IA	Montgomery, PA	Mclennan, TX	
Johnson, KS	Philadelphia, PA	Midland, TX	
Sedgwick, KS	Westmoreland, PA	Montgomery, TX	
East Baton Rouge, LA	York, PA	Nueces, TX	
Jefferson, LA	Anderson, SC	Potter, TX	
Lafayette, LA	Beaufort, SC	Smith, TX	
Orleans, LA	Charleston, SC	Tarrant, TX	
St Tammany, LA	Greenville, SC	Travis, TX	
Desoto, MS	Horry, SC	Webb, TX	
Harrison, MS	Lexington, SC	Williamson, TX	
Hillsborough, NH	Richland, SC	Davis, UT	
Rockingham, NH	Spartanburg, SC	Salt Lake, UT	
Alamance, NC	York, SC	Utah, UT	
Buncombe, NC	Davidson, TN	Weber, UT	
Cabarrus, NC	Hamilton, TN	Brown, WI	
Cumberland, NC	Knox, TN	Dane, WI	

Note: The large donor pool consists of the 122 counties with $\geq$ 5,000 restaurant workers in states that did not experience a minimum wage change since 2009, and which had a continuous data series.

TABLE A.3
Donor Weights for Synthetic Los Angeles County

Positively-weighted Donor Counties	Average Weekly Earnings	Employment
Montgomery, AL	0.216	0.140
Tuscaloosa, AL	0.018	0
Cobb, GA	0.105	0.044
Muscogee, GA	0.035	0
Clayton, GA	0.046	0.065
Jefferson, LA	0	0.020
Orleans, LA	0	0.099
Harrison, MS	0	0
Alamance, NC	0	0.102
Forsyth, NC	0	0.174
Durham, NC	0	0.109
Gaston, NC	0.090	0
Wake, NC	0.046	0
Philadelphia, PA	0.034	0
Lexington, SC	0	0.045
Spartanburg, SC	0.160	0.012
Rutherford, TN	0	0.079
Brazos, TX	0.022	0
Cameron, TX	0.046	0.040
Hidalgo, TX	0.174	0
Hays, TX	0	0.063
Dallas, TX	0.008	0
Smith, TX	0	0.008

Note: Estimated using employment and payroll data from the QCEW, and local unemployment data from LAUS. The donor pool consists of the 122 donor pool counties with $\geq$ 5,000 employment in NAICS 722 in states that did not experience a minimum wage change since 2009. The treated county is Los Angeles County. We display only the donor pool counties with a strictly positive weight in synthetic Los Angeles (for fast food workers) for at least one outcome. Our synthetic control algorithm estimated these weights using data that was normalized to 2014q2.

TABLE A.4

Average Effects by County Earnings Quartile Through 2019

	Quartile 1	Quartile 2	Quartile 3	Quartile 4
Average Weekly Earnings				
A. Primary Sample of All Treated Counties				
Treatment Effect	19.41	17.00	12.22	19.75
Elasticity	0.39	0.35	0.25	0.27
Placebo-variance-based 95% CIs	[0.28, 0.50]	[0.25, 0.46]	[0.15, 0.34]	[0.19, 0.34]
RMSPE *p*-value	0.01	0.01	0.02	0.01
B. Excluding Counties with Local Minimum Wages				
Treatment Effect	19.15	15.09	12.79	16.31
Elasticity	0.38	0.30	0.26	0.34
Placebo-variance-based 95% CIs	[0.26, 0.51]	[0.19, 0.42]	[0.14, 0.38]	[0.21, 0.46]
RMSPE-based *p*-value	0.01	0.01	0.01	0.16
Employment				
C. Primary Sample of All Treated Counties				
Treatment Effect	-0.72	-3.61	0.18	-2.86
Elasticity	-0.01	-0.08	0.00	-0.04
Placebo-variance-based 95% CIs	[-0.14, 0.11]	[-0.21, 0.06]	[-0.12, 0.13]	[-0.12, 0.05]
RMSPE *p*-value	0.81	0.42	0.18	0.40
D. Excluding Counties with Local Minimum Wages				
Treatment Effect	1.47	-4.89	2.53	-0.31
Elasticity	0.03	-0.10	0.05	-0.01
Placebo-variance-based 95% CIs	[-0.13, 0.19]	[-0.26, 0.06]	[-0.11, 0.21]	[-0.20, 0.19]
RMSPE-based *p*-value	0.79	0.33	0.59	0.86

Note: Estimated using employment and payroll data from the QCEW and local unemployment data from LAUS. For the sample of fast-food workers, we have a total of 36 treated counties: 25 in California, plus 11 in New York. All treated counties have $\geq$ 5,000 employment in NAICS 722. The donor pool consists of the 122 counties with $\geq$ 5,000 employment in NAICS 722 in states that did not experience a minimum wage change since 2009. Each treatment effect is the *average* estimated effect in the 21st quarter after the minimum wage increase began in each jurisdiction. For the stacked synthetic control estimates, each treatment effect is the *average* estimated difference between the (normalized to 2014q2 for California, and to 2013q4 for New York) outcome value in each treated county and its estimated synthetic control. The elasticity is calculated with respect to the average population-weighted percentage change in the minimum wage among treated counties in each quartile through between 2013q4 and 2019q4. 95% confidence intervals of the elasticity are displayed in brackets and are estimated using the variance of the distribution of 100 sampled placebo average estimated effects based on estimated differences from in-space placebo treatment on the donor pool counties. The results are corrected for bias from matching discrepancies.

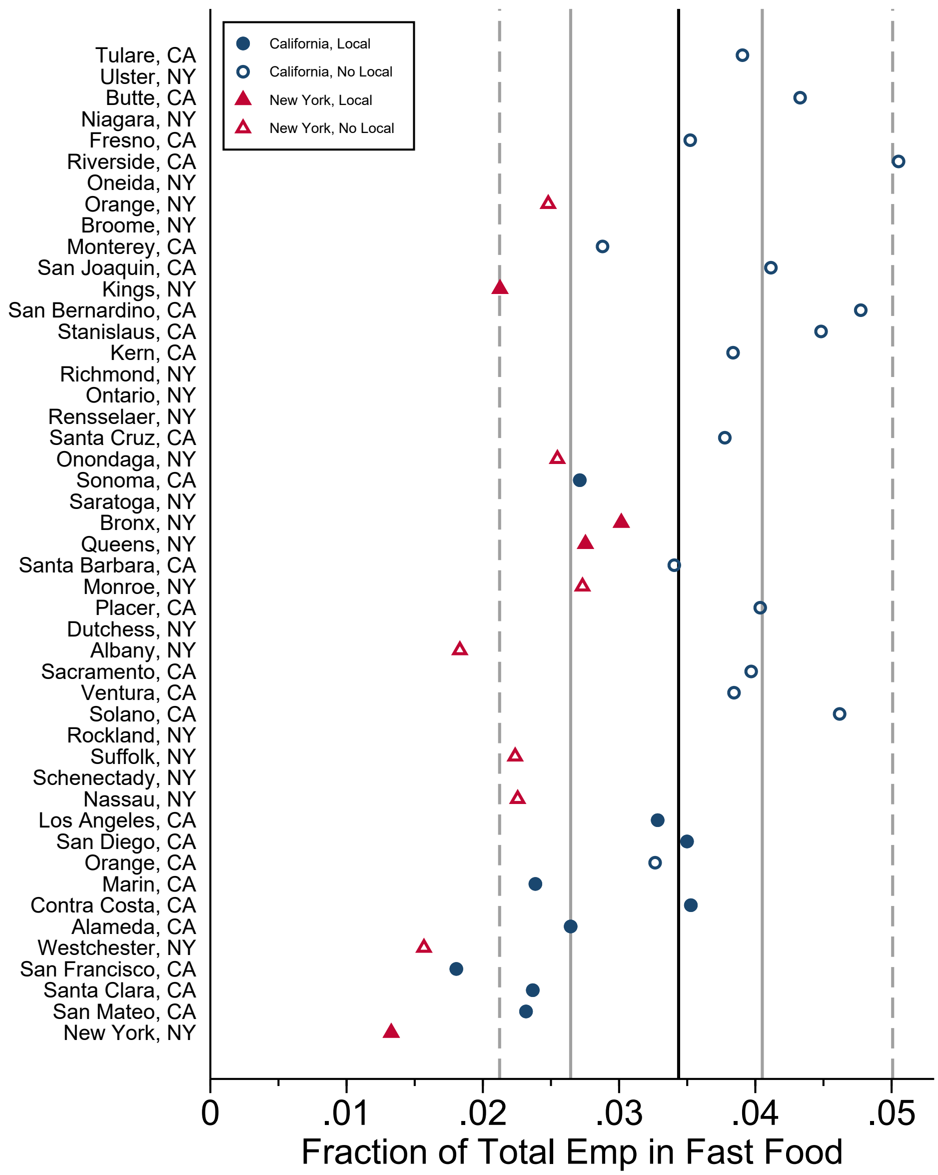
TABLE A.5

Average Effects For McDonald's Establishments Through 2022

	Average Hourly Wage	Price	Pass-Through
A. All Treated Counties			
Treatment Effect	11.31	1.78	0.52
Elasticity	0.21	0.03	
Placebo-variance-based 95% CIs	[0.10, 0.31]	[-.02, 0.08]	[-0.31, 1.36]
B. Excluding Counties With Local Minimum Wages			
Treatment Effect	15.14	4.45	0.98
Elasticity	0.30	0.09	
Placebo-variance-based 95% CIs	[0.16, 0.43]	[0.02, 0.16]	[0.07, 1.89]

Note: Estimated using McDonald's data from the **?**. McDonalds sub-sample includes 31 treated counties: 21 counties in California, plus 10 counties in New York. The treated counties all have $\geq$ 5,000 employment in NAICS 722. The donor pool consists of 95 counties with $\geq$ 5,000 employment in NAICS 722 in states that did not experience a minimum wage change since 2009. Treatment effects are the *average* estimated effects in 2022. Each treatment effect is the *average* estimated difference between the (normalized to 2016) outcome value in each treated county and its estimated synthetic control. The elasticity is calculated with respect to the treated-sample-specific average percent change in the minimum wage through the respective period. 95 percent confidence intervals of the elasticity are displayed in brackets and are estimated using the variance of the distribution of 100 sampled placebo average estimated effects based on estimated differences from in-space placebo treatment on the donor pool counties. Pass-through is calculated using synthetic control estimates and assuming 30% labor share. Associated 95 percent confidence intervals are obtained using delta method.

FIGURE A.1
Distribution of Fraction of Employment in Fast Food by County

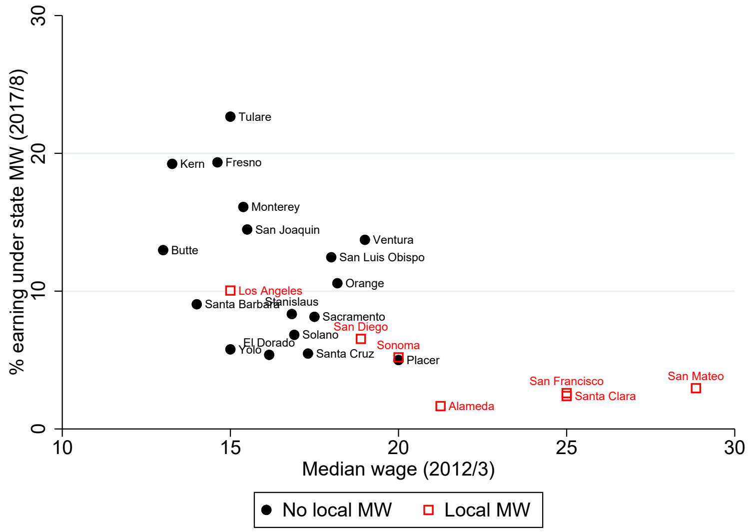


Notes: This figure shows the distribution of the employment-weighted average QCEW employment in fast food as a fraction of all employment across all quarters in 2013 in a given county. Treated counties are shown as individual points; their place in the national distribution is indicated by the vertical bars. The black bar shows the employment-weighted mean for all U.S. counties. The solid gray bars show the 25th and 75th percentiles. The dashed gray bars show the 10th and 90th percentiles. Markers for counties with local minimum wages are solid; markers for counties without them are hollow.

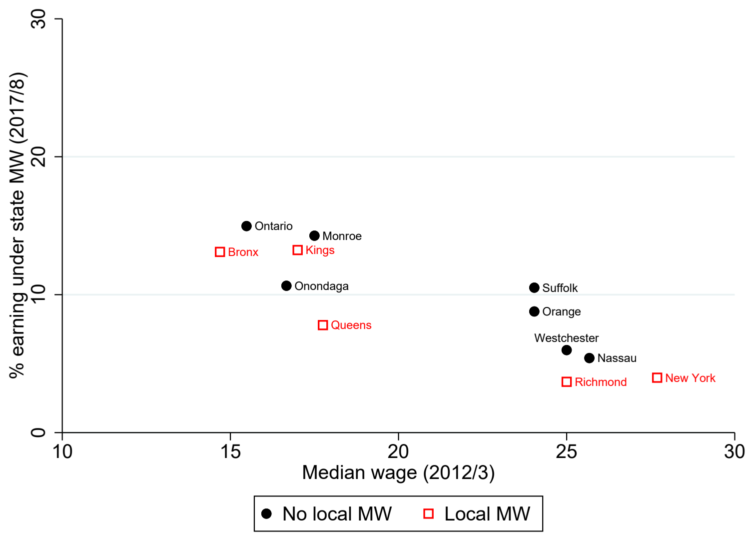
FIGURE A.2

County-level Exposure to State Minimum Wages

A. California



B. New York

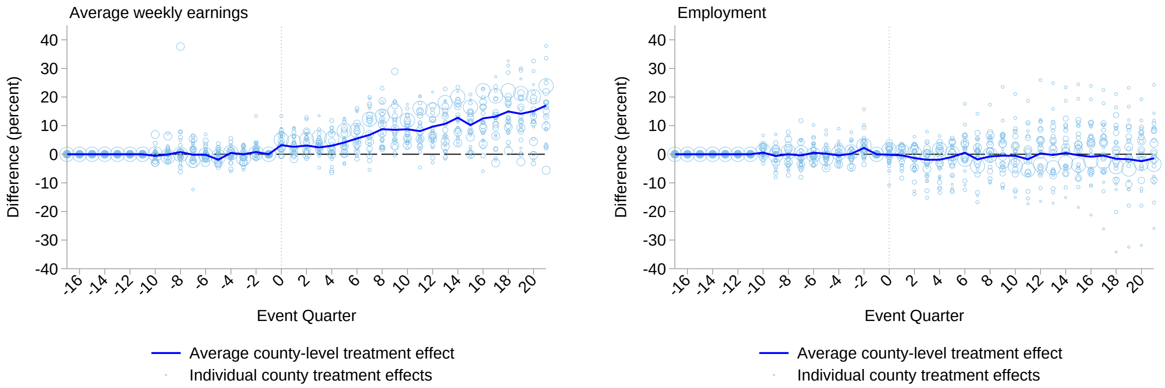


Notes: The figures above plot two measures of county-level exposure to minimum wages. The horizontal axes show the median wage in the two years before the first minimum wage increase. The vertical axes show the average percent earning under the upcoming minimum wage in 2017 and 2018. Years were pooled to capture more counties, since the CPS suppresses those with idiosyncratically small numbers of respondents in a given year. Santa Clara county is suppressed in all years so that data on its CBSA can be released without revealing information on relatively sparsely populated San Benito County. The information on Santa Clara therefore reflects the CBSA and not the county.

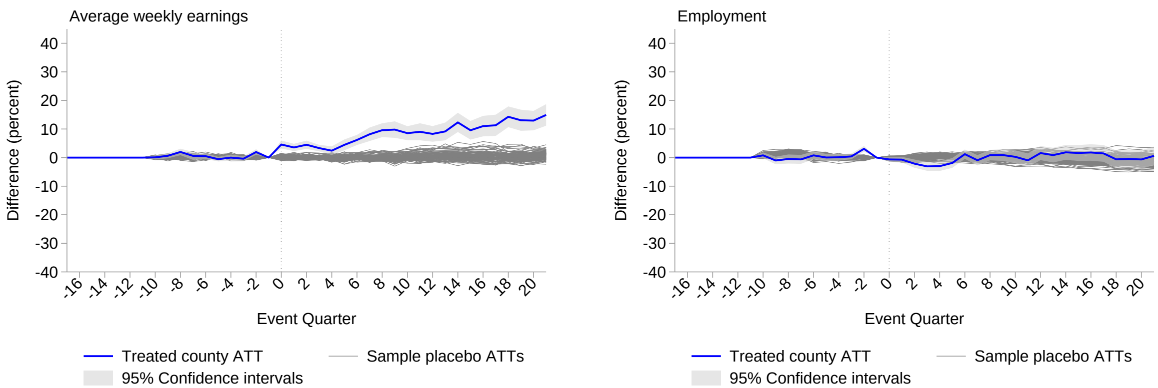
FIGURE A.3

Treatment Effects Excluding Counties in the SF Bay Area and NYC Through 2019

A. Average and Individual Treated County Effects



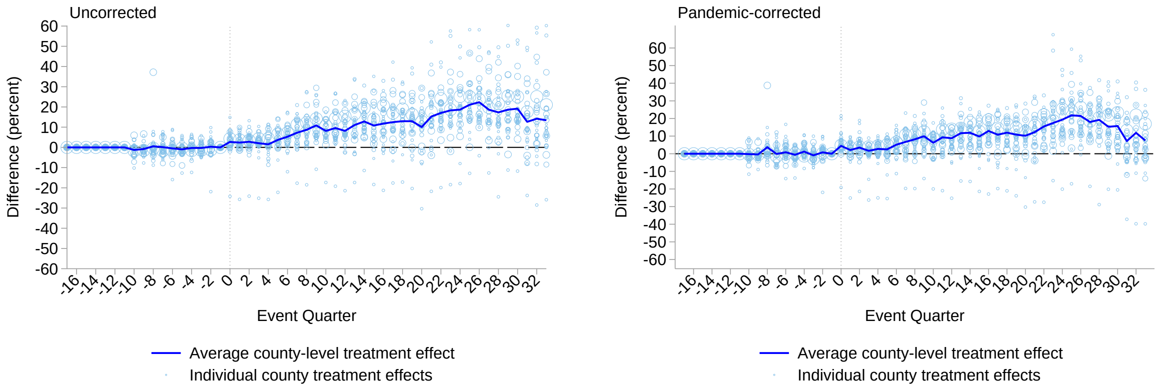
B. Average Effects in Treated Counties vs Sample Placebo Average Effects



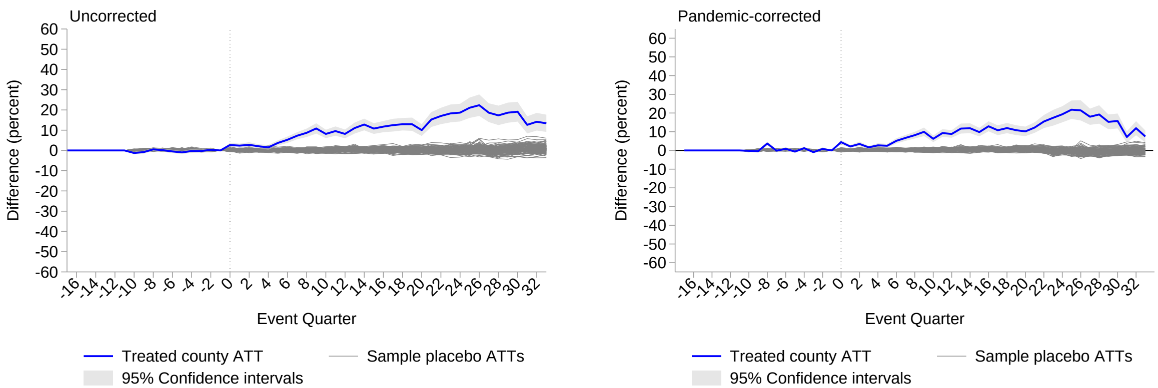
Note: Estimated using employment and payroll data from the QCEW and local unemployment data from LAUS. We have a total of 22 treated counties: 16 in California, plus 6 in New York. All treated counties have $\geq$ 5,000 employment in NAICS 722. The donor pool consists of the 122 counties with $\geq$ 5,000 employment in NAICS 722 in states that did not experience a minimum wage change since 2009. We exclude from our primary sample 14 treated counties in the San Francisco Bay Area and New York City. The y-axis shows the difference in each quarter between the (normalized to 2014q2 for California, and to 2013q4 for New York) outcome value and the associated estimated synthetic control. In panel A, the solid blue line represents the average estimated effect across all 36 treated counties, weighted by 2010 population, and the light blue circles show the individual estimated effects for each contributing county in each time period; the size of the circle represents the relative 2010 population. In Panel B, the solid blue line shows the average estimated effect across all 36 treated counties. The grey lines show 100 randomly sampled averages of 36 placebo treatment effects, estimated for each treated unit by permuting treatment "in-space" across each of the donor pool counties and then taking the difference between the outcome path of the placebo treated unit and that of its synthetic control. The results are averaged in event time, with event-quarter 0 indicating the first quarter of treatment, shown by the vertical dotted line. The results are corrected for bias from matching discrepancies.

FIGURE A.4
Effect on Average Weekly Earnings Using the Sub-sample Of Counties Through 2022

A. Average and Individual Treated County Effects



B. Average Effects in Treated Counties vs Sample Placebo Average Effects

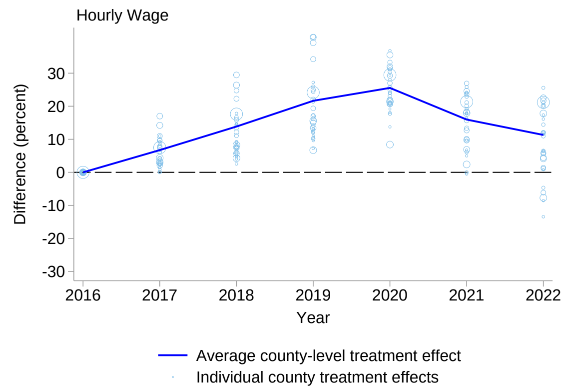


Note: Estimated using employment and payroll data from the QCEW, and local unemployment data from LAUS. McDonalds sub-sample includes 31 treated counties: 21 counties in California, plus 10 counties in New York. The treated counties all have ≥ 5,000 employment in NAICS 722. The donor pool consists of 95 counties with ≥ 5,000 employment in NAICS 722 in states that did not experience a minimum wage change since 2009. The y-axis shows the difference in each quarter between the (normalized to 2014q2 for California, and to 2013q4 for New York) outcome value and the associated estimated synthetic control. In panel A, the solid blue line represents the average estimated effect across all 31 treated counties, weighted by 2010 population, and the light blue circles show the individual estimated effects for each contributing county in each time period; the size of the circle represents the relative 2010 population. In Panel B, the solid blue line shows the average estimated effect across all 31 treated counties. The grey lines show 100 randomly sampled averages of 31 placebo treatment effects, estimated for each treated unit by permuting treatment "in-space" across each of the donor pool counties and then taking the difference between the outcome path of the placebo treated unit and that of its synthetic control. The results are averaged in event time, with event-quarter 0 indicating the first quarter of treatment, shown by the vertical dotted line. The results on the left side are corrected for bias from matching discrepancies. The results on the right are additionally corrected using pandemic index. The pandemic period began in event quarter 22 for California and event quarter 24 for New York.

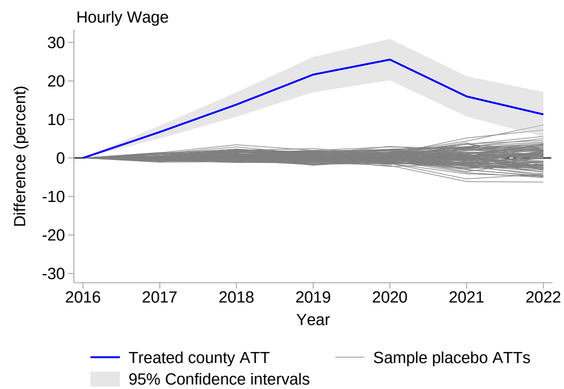
FIGURE A.5

Effect On McDonald's Hourly Wages Through 2022

A. Average and Individual Treated County Effects



B. Average Effects in Treated Counties vs Sample Placebo Average Effects



Note: Estimated using McDonald's data from the **?**. McDonalds sub-sample includes 31 treated counties: 21 counties in California, plus 10 counties in New York. The treated counties all have $\geq$ 5,000 employment in NAICS 722. The donor pool consists of 95 counties with $\geq$ 5,000 employment in NAICS 722 in states that did not experience a minimum wage change since 2009. The y-axis shows the difference in each quarter between the (normalized to 2016) outcome value and the associated estimated synthetic control. In panel A, the solid blue line represents the average estimated effect across all 31 treated counties, weighted by 2010 population, and the light blue circles show the individual estimated effects for each contributing county in each time period; the size of the circle represents the relative 2010 population. In Panel B, the solid blue line shows the average estimated effect across all 31 treated counties. The grey lines show 100 randomly sampled averages of 31 placebo treatment effects, estimated for each treated unit by permuting treatment "in-space" across each of the donor pool counties and then taking the difference between the outcome path of the placebo-treated unit and that of its synthetic control. The results are averaged by year, starting in 2016, with the year 2014 (not included in the graph) being the year of treatment. The results are not corrected for bias from matching discrepancies or pandemic index. The pandemic period began in the year 2020.

B. Pandemic Confounds and Correction

B.1. Pandemic-response Index

We construct the pandemic-response index primarily to understand the trajectory of earnings and employment during the years in which the pandemic was at its height. In every period from 2021q3 on, our estimates match in sign. For the intervening quarter, our goal is to choose a parsimonious way to control, at the county level, for the myriad effects of the pandemic and the response of governments, businesses and individuals. One might, for instance, control for the length of the lockdown in each county, but lockdowns and stay-at-home orders did not create the same limits on restaurant capacity in every county, nor were the statutes enforced with equal zeal. Even a complete and detailed legal account would not capture differences in the severity and timing of infections, vaccine take-up, or the cultural response to the disease. Accordingly, we opt to control for what people actually did, which implicitly accounts for the full set of exogenous shocks just described.

We build our index using local smartphone data from Google on time spent at restaurants and retail stores and local smartphone data on time spent at workplaces.[2] Comparisons of Figure B.2 to Figure **??** emphasize why using these time-spent measures is superior to using other potential candidates like Covid cases or deaths. Figure B.2 shows the the correlation between time spent in restaurants and retail versus Covid cases (Panel A) and deaths (Panel B) for California and New York relative to our donor states. The cases and deaths appear above the horizontal axis, since they are increasing; time spent appears below the horizontal axis since they are decreasing. (State-level case and death data come from the New York Times Covid database, available on GitHub). The area shaded in gray is the time period captured by our pandemic index.

In our index period, New York City is the first epicenter of Covid cases and deaths. California cases and death remained relatively low on a national scale, but, reacting to the national Covid situation, California locked down, and saw a steep decline in time spent at resuarants and retail. The same pattern is evident during the spike in cases in late 2021 and early 2022 (due to the Delta and Omicron variants).

Time spent at restaurants is affected by the pandemic-generated shift to takeout and restaurant delivery. Time spent might therefore not capture actual spending on restaurant meals. However, the shift to takeout and delivery entails reduced demand for waitstaff in full-service restaurants. As Dalton, Dey and Loewenstein (2022) document, reductions in foot traffic and time spent at restaurants did reduce restaurant employment.

We next consider the relevant time period for the pandemic index. We want to choose a period that captures the differential effects of the pandemic while minimizing over-fitting and the odds that other events begin to leak in. Panel A of Figure **??** indicates that the daily differences in time reduction between the treated and donor states varied considerably in 2020 through 2022. However, most of the inter-county variation is captured in the March 15 to July 15, 2020 window.

Our index using data on retail and restaurants turns out to match well the patterns of fast food employment.The decline in retail employment was more moderate than in restaurants. Foot traffic data reported in Yang, Liu, and Chen (2020) confirm that the decline in fast foods was more moderate than in restaurants as a whole. National QCEW data also show the different effects on full service and limited service restaurants. In April 2020 employment in full service restaurants had declined to 37 percent of the February 2020 level;

[2] These smartphone data are broadly representative of the U.S. population as a whole. Google does not attach demographic information that could assess the representativeness of its mobility data. Nonetheless, other pandemic studies similarly use data collected from smartphones such as SafeGraph or PlaceIQ (Chen and Pope, 2020; Couture et al., 2022). These papers conclude that, while poorer and older adults are slightly under-represented in smartphone datasets, the data are nonetheless broadly representative of the general population and represent a particularly good match for within-county demographics and for labor force participants. This feature makes them well-suited for capturing spatial and temporal variation.

it then recovered by July 2020 to 73 percent of the February 2020 level. Meanwhile, employment in fast food restaurants in April 2020 had declined to 77 percent of its February level; by July 2020 it recovered to 93 percent of the February level. Finally, retail employment in April 2020 fell to 83.7 percent of its February 2020 level and then recovered by July 2020 to 95.7 of its February 2020 level.

These trends somewhat offset each other. Expressed as a proportion of retail and restaurant employment, fast food employment rose from 17.7 percent in February 2020 to 18.8 percent in April 2020 and then fell to 18.3 percent in July 2020. In other words, changes in fast food employment were similar to those for restaurant/retail as a whole.[3]

The decline in time spent at all workplaces was more moderate than the time spent in restaurants/retail. Taken together, these considerations suggest taking the simple average of the restaurant/retail and workplaces indices to proxy for relevant local pandemic confounds affecting fast food restaurants.

The map in Panel E of Figure B.3 displays the variation of the pandemic index across our treated and donor areas. The map suggests that while the pandemic affected both treated and donor counties, the effects were greater in treated counties. In other words, the pandemic confounds our minimum wage estimates.

B.2. Pandemic-response Bias Correction

We discussed our novel approach to correcting for the confounding effect of the pandemic response shocks in Section **??**.[4] Figure B.1 plots $\tilde{Y}'_{zt} - \tilde{Y}_{zt}$ in event time by outcome, for the donor pool (which, by construction, average zero except for the impact of population weighting) in Panel A, and for the treated counties in Panel B. A necessary condition for the validity of our pandemic correction procedure is that $E[\tilde{Y}'_{zt}] = E[\tilde{Y}_{zt}]$ for all $t < 2020q1$. Visual inspection of Panel B shows there is no difference between $\tilde{Y}'_{zt}$ and $\tilde{Y}_{zt}$, on average, before event quarter 22, which coincides with 2020q1 in California. The confounding effects of the pandemic shock on our treated counties can then be seen from event quarter 22 onward, most dramatically in the event quarters coinciding with the calendar 2020 quarters, then continually dissipating without fully disappearing by event quarter 33.

Figure B.4 presents our estimates corrected only for bias from matching discrepancies on predictor variables, which we refer to as our "uncorrected" estimates to differentiate them from our "pandemic-corrected" estimates. These are the estimated effects absent our pandemic-correction procedure (the values of which are differenced out of the pandemic-corrected estimates to yield Panel B of B.1). They make clear that, absent the pandemic correction, average earnings were not much different, but employment briefly but sharply disproportionately fell in our treated counties, on average, during the depths of the pandemic before rapidly recovering to yield a positive, non-significant estimated effect. The larger, statistically significant positive estimated effect on employment is then clearly obtained by correcting for the bias generated by heterogeneous local pandemic responses, which again is seen dissipating but not fully disappearing by event quarter 33 in Panel B of Figure B.1.

Finally, Table B.1 presents our event quarter 33 employment estimates using various measures to correct for the local pandemic response. Column (1) presents the uncorrected estimates, column (2) the estimates corrected using our pandemic-response index, column (3) the estimates corrected for the average local change

[3]Full-service restaurant employment declined much more steeply than fast food employment during the pandemic.

[4]More precisely, as we describe in Section 4.C, we first regress employment on the pandemic index using only the donor counties. We use the resultant estimated effects of the index to predict the local outcome values in the absence of the pandemic. We then apply the pre-estimated synthetic control weights to the pandemic-corrected donor pool outcome values, then difference out the resultant pandemic-corrected synthetic control outcome values from the associated treated county pandemic corrected outcome values. The result is a pandemic-corrected estimate that we use to measure the effects of the minimum wage increases without the confound of different initial local pandemic responses.

in time spent in retail stores and restaurants, and column (4) the estimates corrected for the average local change in time spent in the workplace. All the estimates are positive, and all estimates corrected for local pandemic response are larger in magnitude than the uncorrected ones. However, by far the largest estimate is that corrected using only the change in time spent in retail stores and restaurants, in column (3). This measure is obviously appropriate, given our focus on the fast food industry. However, we construct our index by averaging the measures used in columns (3) and (4), as it seems economically relevant to incorporate the effect of working from home on consumer demand in central city fast food restaurants.

TABLE B.1

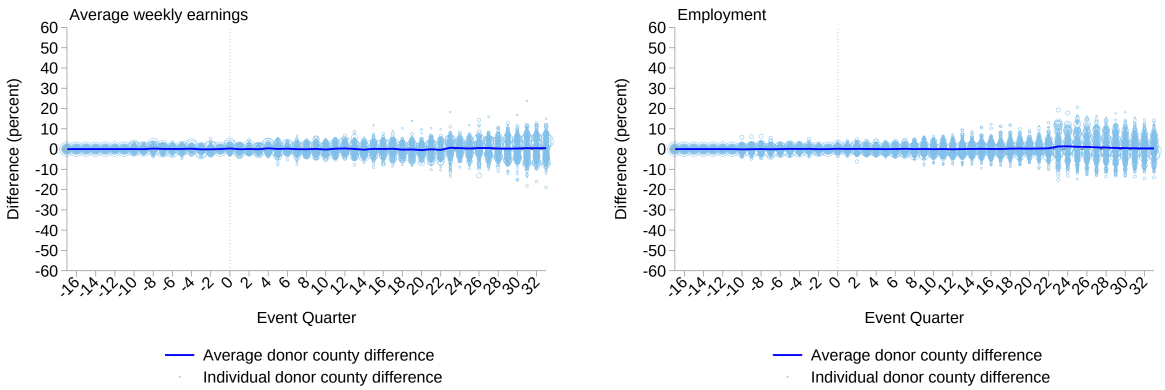
Average Employment Effects Corrected by Local Pandemic-response Measure

	Local Pandemic-response Measure			
	None	Pandemic-response Index	Time in Retail & Restaurants	Time in Workplace
All Treated Counties				
Treatment Effect	2.22	7.33	10.80	2.95
Elasticity	0.02	0.08	0.12	0.03
Placebo-variance-based 95% CIs	[-0.03, 0.07]	[0.04, 0.13]	[0.07, 0.17]	[-0.02, 0.08]
RMSPE-based p-value	0.12	0.08	0.01	0.45

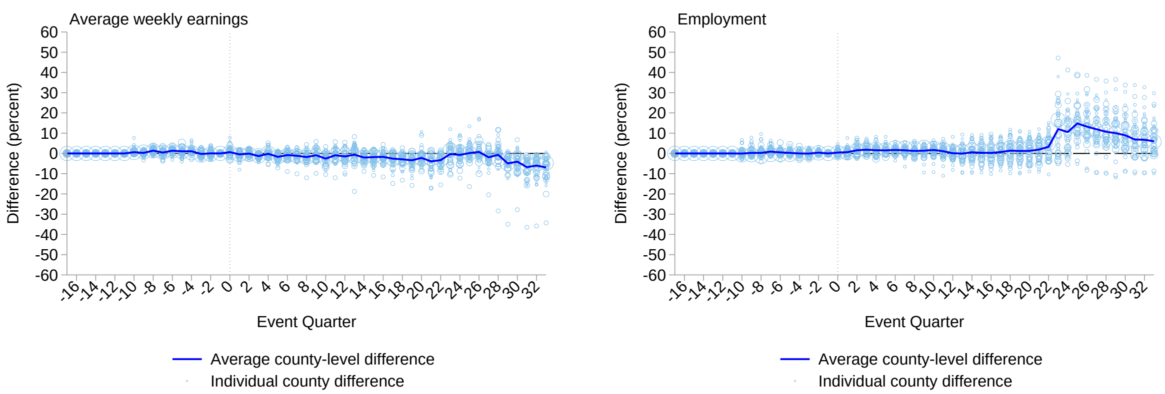
Note: Estimated using employment and payroll data from the QCEW, local unemployment data from LAUS, and Google Mobility data from Chetty et al. (2020). For the sample of fast-food workers, we have a total of 36 treated counties: 25 in California, plus 11 in New York. All treated counties have $\geq$ 5,000 employment in NAICS 722. The donor pool consists of the 122 counties with $\geq$ 5,000 employment in NAICS 722 in states that did not experience a minimum wage change since 2009. Each treatment effect is the *average* estimated effect in the 33rd quarter after the minimum wage increase began in each jurisdiction, which in almost all cases is the fourth quarter with a local minimum wage of $15. For the stacked synthetic control estimates, each treatment effect is the *average* estimated difference between the (normalized to 2014q2 for California, and to 2013q4 for New York) outcome value in each treated county and its estimated synthetic control. The elasticity is calculated with respect to the treated-sample-specific average percent change in the minimum wage through event quarter 33. 95 percent confidence intervals of the elasticity are displayed in brackets and are estimated using the variance of the distribution of 100 sampled placebo average estimated effects based on estimated differences from in-space placebo treatment on the donor pool counties. The first column results are corrected only for bias due to matching discrepancies. The remaining columns from left to right, respectively, are additionally corrected for the pandemic-response index, change in time spent in retail and restaurant establishments, and change in time spent in the workplace. The pandemic-response index is an average between latter two.

FIGURE B.1
Pandemic-corrected Minus Uncorrected Effects Through 2022

A. Average and Individual *Donor Pool* Counties



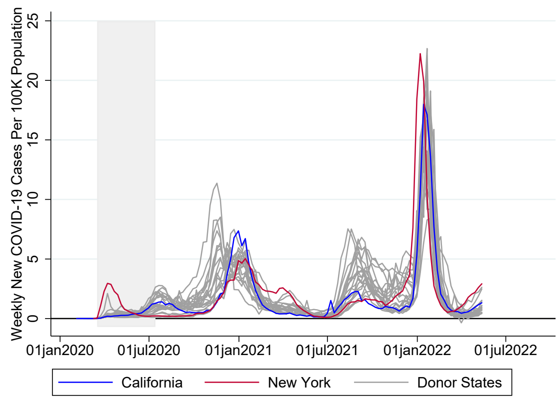
B. Average and Individual *Treated* Counties



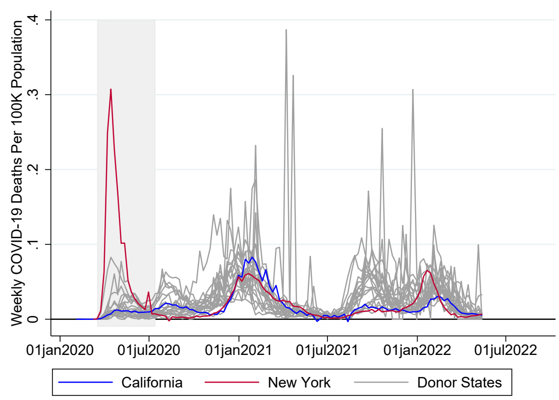
Note: Estimated using employment and payroll data from the QCEW, local unemployment data from LAUS, and Google Mobility data from Chetty et al. (2020). We have a total of 36 treated counties: 25 in California, plus 11 in New York. All treated counties have $\geq$ 5,000 employment in NAICS 722. The donor pool consists of the 122 counties with $\geq$ 5,000 employment in NAICS 722 in states that did not experience a minimum wage change since 2009. For the bias correction, in each period we regress the outcome on the full set of predictor variables *using the donor pool only*, then predict residualized outcome values for all counties (treated and donor pool). For the pandemic correction, we do the same but add the pandemic-exposure index to the set of regressors in the decidualization process. The y-axis shows the difference in each quarter between the (normalized to the associated final pre-treatment period) pandemic-corrected outcome and the associated bias-corrected outcome. Panel A shows these values individually (blue circles) and on average (solid blue line) for the donor pool counties. Panel B shows the same but for the treated counties. The results are placed in event time, with event-quarter 0 indicating the first quarter of treatment (or placebo treatment, for the donor pool), shown by the vertical dotted line. The pandemic period began in event quarter 22 for California and event quarter 24 for New York.

FIGURE B.2
Change In COVID Cases and COVID-related Deaths

A. Change In Detected COVID Cases

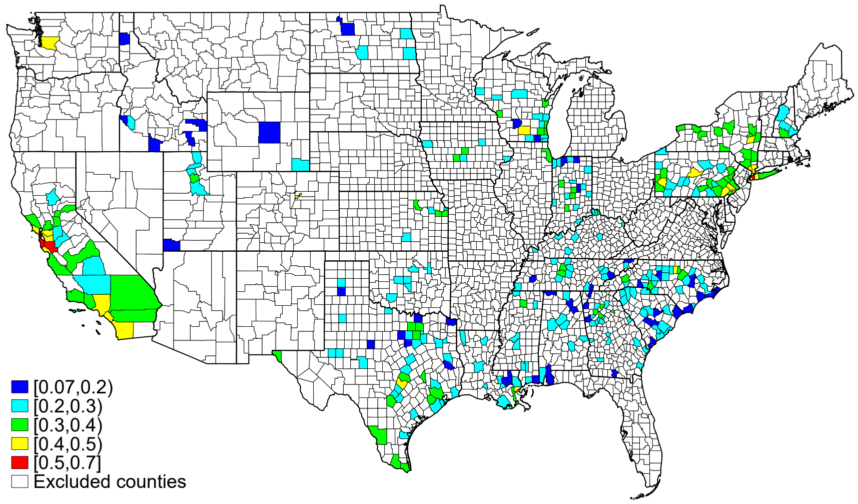


B. Change In COVID Related Deaths



Notes: Panel A displays the new detected weekly COVID cases for California and New York versus Donor States. Panel B shows the change in registered deaths caused by COVID for California and New York versus our Donor States. State-level cases and deaths data come from the New York Times. The area shaded in gray is the time period captured by our pandemic index.

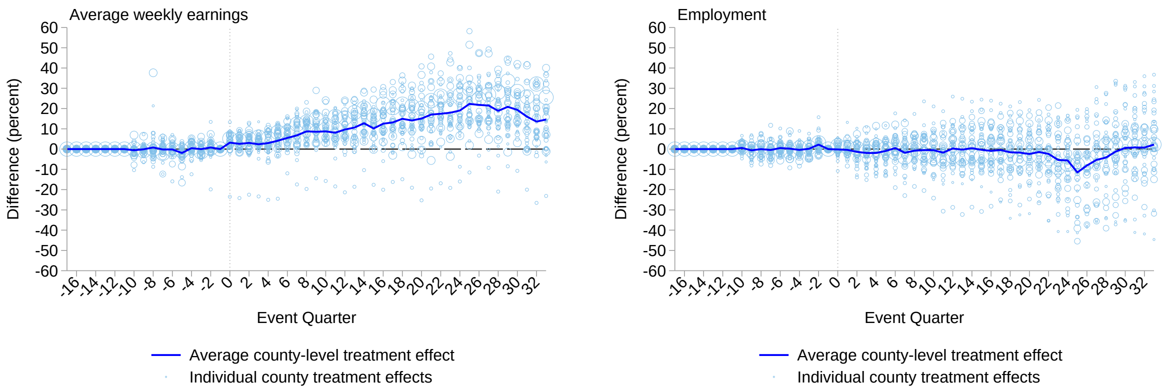
FIGURE B.3
Pandemic Index by County



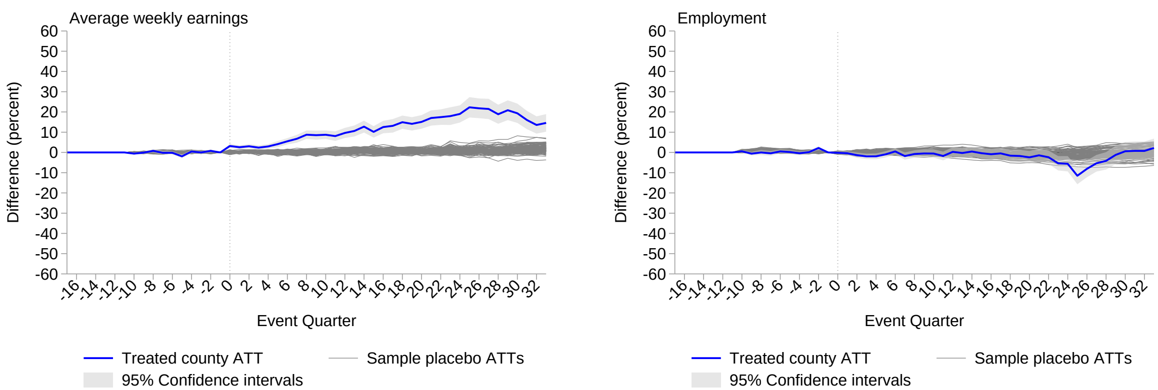
Source: Data on time spent in locations comes from Chetty et al. (2020), which is available by state, county, and city. The pandemic index is described in Section 2.3. A higher value of the index entails a greater impact. Among donor counties, the index has a mean of 0.26 and a standard deviation of 0.06. In all counties, the mean is 0.23 with a standard deviation of 0.09.

FIGURE B.4
Uncorrected Effects Through 2022

A. Average and Individual Treated County Effects



B. Average Effects in Treated Counties vs Sample Placebo Average Effects



Note: Estimated using employment and payroll data from the QCEW and local unemployment data from LAUS. We have a total of 36 treated counties: 25 in California, plus 11 in New York. All treated counties have $\geq$ 5,000 employment in NAICS 722. The donor pool consists of the 122 counties with $\geq$ 5,000 employment in NAICS 722 in states that did not experience a minimum wage change since 2009. The y-axis shows the difference in each quarter between the (normalized to 2014q2 for California, and to 2013q4 for New York) outcome value and the associated estimated synthetic control. In panel A, the solid blue line represents the average estimated effect across all 36 treated counties, weighted by 2010 population, and the light blue circles show the individual estimated effects for each contributing county in each time period; the size of the circle represents the relative 2010 population. In Panel B, the solid blue line shows the average estimated effect across all 36 treated counties. The grey lines show 100 randomly sampled averages of 36 placebo treatment effects, estimated for each treated unit by permuting treatment "in-space" across each of the donor pool counties and then taking the difference between the outcome path of the placebo treated unit and that of its synthetic control. The results are averaged in event time, with event-quarter 0 indicating the first quarter of treatment, shown by the vertical dotted line. The results are corrected for bias from matching discrepancies.

C. All Workers in California

C.1. Distributional Effects on All Workers

We present here the results of our distributional analysis of the effects of the minimum wage increases on all workers. We restrict this analysis to California, as New York State's $15 minimum wage policy applied only to fast food workers and New York employers receive a credit for tipped workers in full service restaurants. Employers can thus pay these workers a sub-minimum wage.

We begin by using our standard synthetic control technique to estimate the impact of minimum wages on tenth percentile and median wages. Figure C.1 shows that minimum wages lead to substantial increases in P10 wages, but did not affect P50 wages.

We then take this analysis farther by constructing a figure similar to those in the bin-by-bin analysis of Cengiz et al. (2019). To do so, we first aggregate CPS microdata to hourly-wage bins by state and quarter. We then aggregate differences among synthetic control estimated effects on each wage bin following each minimum wage increase (as described below) to summarize the effects of all our minimum wage changes on the share of jobs in $1 wage bins throughout the wage distribution. These estimates are *not* corrected for pandemic confounds because they are conducted using *state-level* CPS data, while our pandemic-response correction procedure relies on *county-level* variation in pandemic responses. (Section C.2, below, details this bin-by-bin estimation procedure.)

Our bin-by-bin analysis reveals, in the year following each minimum wage increase, the average decline in jobs just below the new minimum wage and the average increase in jobs just above the new minimum wages, as well as whether our synthetic control methods find effects on higher-wage jobs. Effects on high-wage jobs, where they are not expected, would indicate the presence of confounding shocks, implying that we have poorly identified the causal effects of the minimum wage policies.

Panel A of Figure C.2 presents results through 2019q4 and Panel B through 2022q2. The horizontal axis presents $1 wage bins, from $4 below the new minimum wage (-4) to $17 or more above the new minimum wage (17+). The bars in each wage bin indicate changes in the share of all jobs in that wage bin.[5] The handles indicate 95 percent confidence intervals.

The large negative bars just below the new minimum wage indicate the large share of jobs that were bunched below the new minimum wage and the decline in the share of such jobs after the implementation of the new minimum wage. The large positive bars just above the new minimum indicate that the policy was effective in increasing hourly wages in accordance with the new standard. The positive bar just above the new minimum wage is of the same magnitude as the negative bar just below the new minimum wage. These similar magnitudes indicate that the number of new jobs is roughly equal to the decline in the number of old jobs.

The bars are much smaller at higher wage levels. The small bars (and their confidence intervals) in the higher bins together indicate that we do not find minimum wage employment effects at wage levels where we expect not to find any. This finding provides important confirmation that our methods identify only minimum wage effects and not other economic shocks.

Taken together, these results show that we are finding effects on wages where we expect minimum wages to cause them, and nowhere else.

[5] The shares are not constrained to sum to zero because they are estimated separately (from individual synthetic control estimates for each wage bin following each minimum wage increase), because synthetic California can differ for each wage bin-specific estimate, and because they are average effects (over contributing quarters, weighted by the percent size of the minimum wage change).

C.2. Estimating effects throughout the wage distribution

We describe here our method for conducting an hourly wage bin-by-bin analysis of state-level effects on all Californian workers.[6] Using the CPS, we estimate separate synthetic controls for workers in each hourly wage bin in the four quarters following each discrete minimum wage increase, then stack and average the results by relative wage bin (see below for details). We restrict the data for each analysis as described in Section **??**. Our analysis is similar to the relative wage bin-by-bin analyses in Harasztosi and Lindner (2019), Cengiz et al. (2019) and Wursten and Reich (2023). In our context, where minimum wages increased every year in both treated states, we want to avoid overlap between the post-treatment period for one increase and the pre-treatment period for the next. We therefore do not use the stacked event study (dynamic DiD) approach of these earlier studies. Instead, we develop a bin-by-bin analysis using stacked synthetic controls matched by wage bin in the period before the *first* minimum wage increase in California.

We develop this analysis in a series of steps: First, we use synthetic control analysis to estimate the effect on employment shares in many wage bins in each of our treatment quarters. Second, we then difference these estimates from their values four quarters previous, and stack the results for each wage bin in the four quarters following each minimum wage increase; this step allows us to estimate the average change in the share of workers in each *relative* wage bin–that is, those earning e.g. $0.01 - $1.00 less than the new minimum wage, $0.00 - $.99 more than the new minimum wage, and so on through the relative wage distribution from -$4 through $17+. Third, we average the effects by state for each relative wage bin.[7]

More specifically, we use hourly wage bin data calculated from the CPS ORG to estimate the effects of California's minimum wage increases on the frequency distribution of hourly wages. This process involves multiple steps. For each one-dollar wage bin $\{\$5 - \$5.99\}$ through $\{\$31 - \$31.99\}$, as well as our top-coded bins, we observe the share of total state-wide employment in that bin for each state $\times$ quarter in our sample. We then estimate, for each of these bins, a synthetic control and treatment effects on the employment share in that bin resulting from treatment beginning in 2014q3, when California's minimum wage began rising. We then take the estimated treatment effects for each bin-specific estimate and difference them from the estimates for the same bin, from four quarters before the most recent minimum wage increase. This difference is the change in the employment share for each wage bin in the four quarters following the minimum wage increase.

In order to combine all of these impact estimates, we assign our estimated bin effects to one-dollar *relative* wage bins (RWBs) from -$4 to +$16 around each new minimum wage in California over our period of interest, as well as the RWB that is +$17 or more than each new minimum wage level.

With relative wage bin $0 – $0.99 serving as an example, Table C.1 details the contributing elements and time periods, and Figure C.3 visualizes the contributing estimates.

We stack these estimates for all relative wage bins and all donor pool states plus California, then calculate a weighted average effect in each relative wage bin for each state using the percent change in the minimum wage for each event as weights. We estimate confidence intervals using the variance of 1,000 draws with replacement of the weighted average placebo effects (in the donor pool states).

[6] We focus on California for this exercise as, unlike New York, California's minimum wage increases covered all workers and did not provide for tip credits.

[7] We weight the contributions from each minimum wage increase by the percentage change in the minimum wage with the increase represented.

Tables

TABLE C.1
Contributing Elements to Relative Wage Bin $0–$0.99

Relative Wage Bin (RWB)	Wage Bin (WB)	Contributing Quarters
Pre-COVID period		
$0 – $0.99	$9.00 — $9.99	2014q3 — 2015q2
$0 – $0.99	$10.00 — $10.99	2016q1 — 2016q4
$0 – $0.99	$10.50 — $11.49	2017q1 — 2017q4
$0 – $0.99	$11.00 — $11.99	2018q1 — 2018q4
$0 – $0.99	$12.00 — $12.99	2019q1 — 2019q4
COVID period		
$0 – $0.99	$13.00 — $13.99	2020q1 — 2020q4
$0 – $0.99	$14.00 — $14.99	2021q1 — 2021q4
$0 – $0.99	$15.00 — $15.99	2022q1 — 2022q2

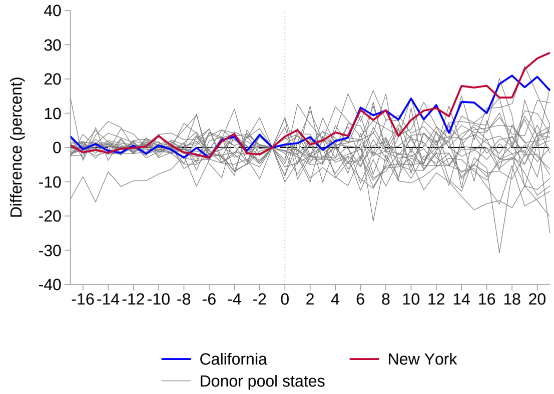
Note: Displays the quarters from each set of $1 wage bin-specific estimates that contribute to the $0–$0.99 relative wage bin (the wage bin earning between each new minimum wage and up to $0.99 more in the year following each minimum wage increase).

Figures

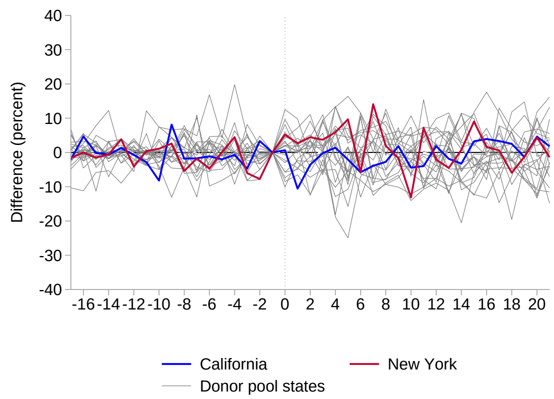
FIGURE C.1

Average Effects on 10th and 50th Percentile Hourly Wage For All Workers Through 2019

A. Effect On 10th Percentile Hourly Wage



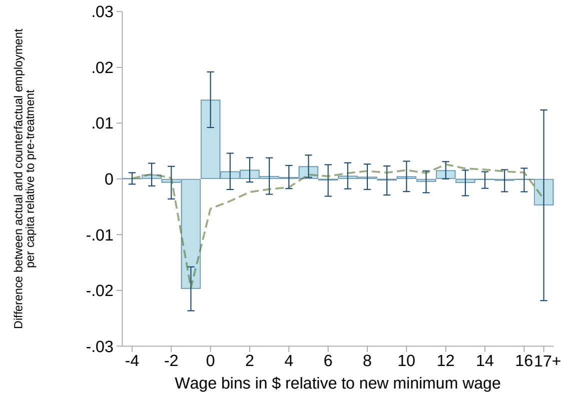
B. Effect On 50th Percentile Hourly Wage



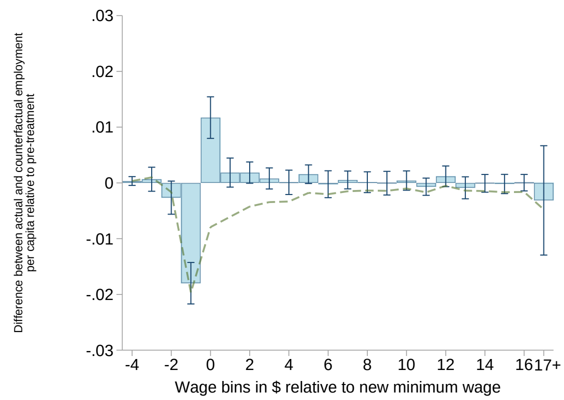
Note: Estimated using employment and earnings data on all workers in the CPS and local unemployment data from LAUS. The donor pool consists of 20 untreated/control states for the period ending in event quarter 21. The top and bottom panels' the y-axis shows the estimated difference in each quarter for, respectively, the normalized 10th and 50th percentile hourly wage between each state and its estimated synthetic control for California (blue) and New York (red). The vertical dotted line indicates the first quarter of treatment.

FIGURE C.2
Bin-by-Bin Effects Using State-level Data, All Workers

A. Through 2019q4 (Pre-Covid Period)

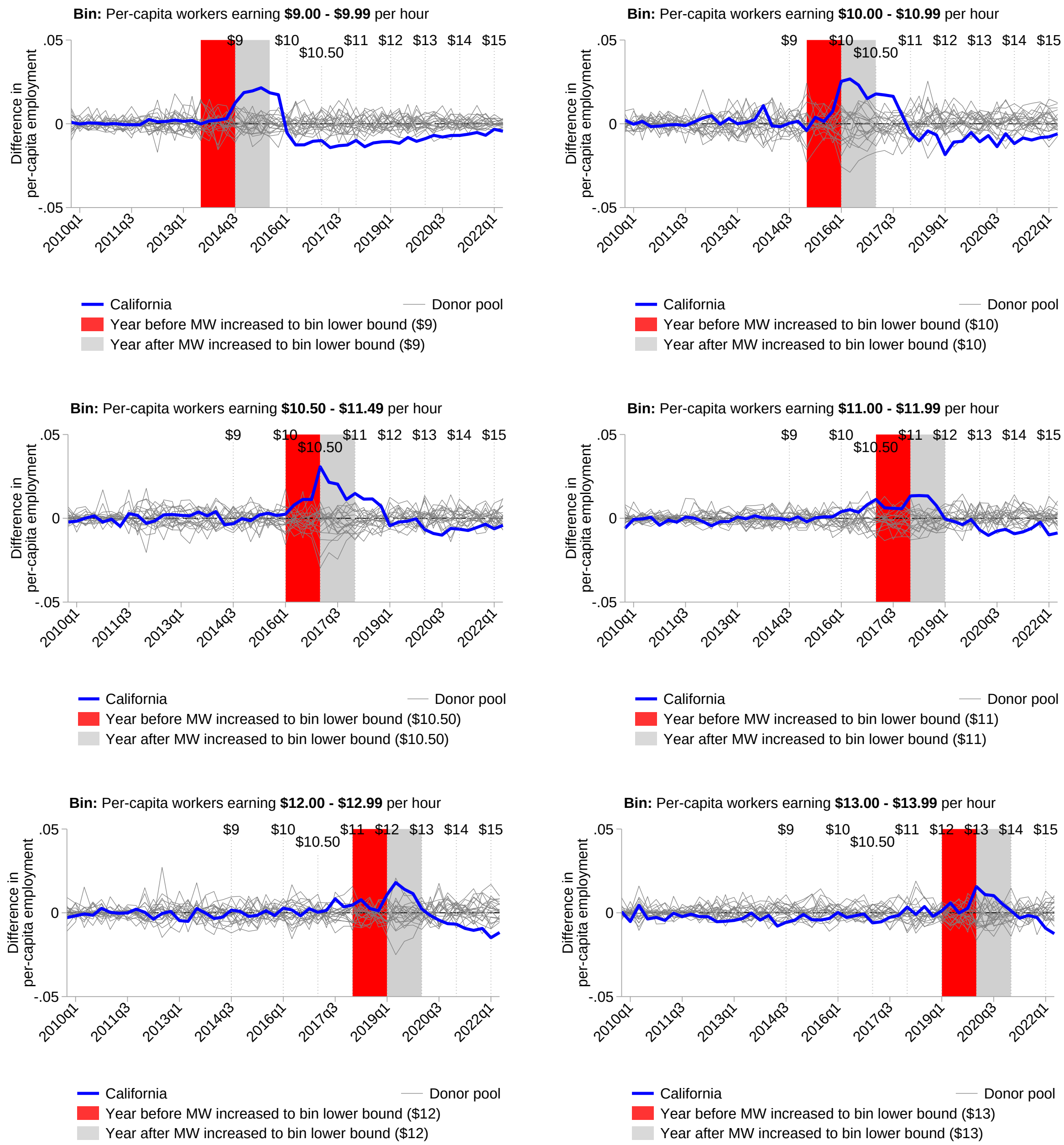


B. Through 2022q2 (Inclusive of Covid Period)



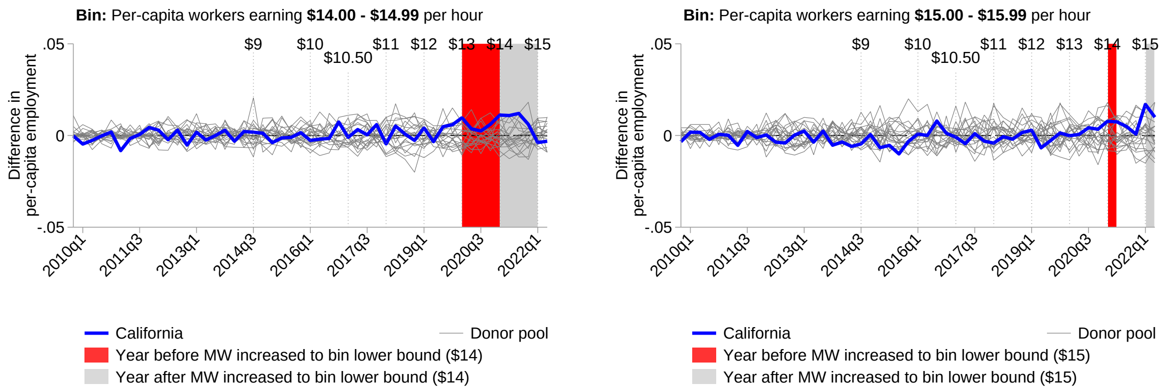
Notes: Effect on the share of total employment in each wage bin in the year following California's minimum wage increases, for the pre-Covid period indicated in Table C.1 (through 2019q4), and for the entire period inclusive of the Covid period (through 2022q2). For each wage bin, we first use synthetic control analysis and CPS data to estimate effects (for California) and placebo effects (for each donor pool state). We then difference these estimates relative to the year preceding each minimum wage increase and stack them by relative wage bins (RWBs) – relative to the minimum wage in that year. Finally, we estimate RWB-specific effects using OLS regression weighted by the inverse of the RMSPE *p*-values. Handles show 95 percent confidence intervals based on robust standard errors.

FIGURE C.3
Change in Wagebin-specific Employment per capita, Relative Wage Bin $0 – $0.99



Note: Continues on next page.

FIGURE C.3 – Cont'd.
Change in Wagebin-specific Employment per capita, Relative Wage Bin $0 – $0.99



Note: Continued from previous page. Estimated using employment and earnings data on workers aged 16–19 in the CPS and local unemployment data from LAUS. Shows the wagebin-specific synthetic control estimated effects of the California minimum wage increases on the share of employment in each $1 wage bin that contributes to the relative wage bin $0–$0.99 (for our pre-pandemic bin-by-bin analysis, we only consider the wage bins and quarters indicated in the pre-Covid period in Table C.1. For the pandemic-inclusive bin-by-bin analysis, we consider all the wage bins and quarters indicated in Table C.1). The donor pool consists of 20 untreated/control states for the period ending in 2022q2. The y-axis shows the estimated difference in each quarter between the (smoothed, normalized to 2014q2) outcome value in California and its estimated synthetic control. The solid blue line is the estimated difference (effect) for California, while the grey lines show the estimated differences from in-space placebo treatments on the donor pool states. For each $1 wage bin, the grey-shaded area indicates the quarters in the year immediately following the minimum wage increase that set the minimum wage to be the lower bound of that $1 wage bin, while the red-shaded area indicates the quarters in the year immediately preceding that minimum wage increase. For each state, the estimates in the red-shaded area are differenced-out of the estimates four quarters later, in the grey-shaded area, then divided by the average employment-population ratio in the year preceding treatment, to calculate the estimated effect of each minimum wage increase on the share of employment in each $1 wage bin.

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