

# Minimum Wage Effects and Monopsony Explanations\*

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We present the first causal analysis of a seven-year run-up of minimum wages to \$15. Using a novel stacked county-level synthetic control estimator and data on fast-food restaurants, we find substantial pay growth and no disemployment. Our results hold among lower-wage counties and counties without local minimum wages. Minimum wage increases reduce separation rates and raise wages faster than prices at McDonald’s stores; both findings imply a monopsonistic labor market with declining rents. In the tight post-pandemic labor market, when labor supply becomes more elastic, we find positive employment effects. These become larger and statistically significant after addressing pandemic-response confounds.

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## 1. Introduction

A substantial number of recent minimum wage studies have found very small or no negative employment effects of minimum wage policies. These results are consistent across a wide variety of research designs and estimating strategies (see Manning (2021) for a recent review). This literature is less clear about *why* there are no disemployment effects. The most common explanations suggest that monopsony power reduced wages below their competitive level, so that minimum wage increases eat into rents (Azar et al., 2023; Manning, 2021; Wiltshire, 2024), or that pass-throughs to prices (Ashenfelter and Jurajda, 2020, 2022; Cooper, Luengo-Prado, and Parker, 2020) offset increased labor costs. Others, such as Sorkin (2015), alternatively argue that the increases often studied are too small to detect employment effects, that inflation erodes the real value of the minimum wage and therefore minimizes potential negative effects, or that past analyses have been too short-term to allow firms to substitute capital for labor.

In this paper we exploit the implementation of two large state-level minimum wage increases to arbitrate among these potential explanations. The cumulative increases that we study are large—87.5 percent in California and 107 percent in New York—and in both cases were implemented over a 7.5 year period. These policies therefore differ substantially from traditional incrementalist minimum wage policies in the U.S., which historically implemented much smaller increases over similar event windows. The implementation schedules provided employers with considerable advance notice, the ramp up was long, and the gains in minimum wages have since been protected by indexing to inflation, motivating a “long-run” interpretation of our estimated effects, ensuring the treatment shocks were sufficiently large and rapid, even in real terms. These increases thus constitute important lower-bound tests of how high and rapidly minimum wages can increase without substantial negative employment effects.

We focus on the fast-food industry in the 36 most populous counties of California and New York. Interestingly, the range of average pre-treatment earnings among these counties spans the nationwide distribution of county-level average earnings. Insights gleaned from the increases in these two states may therefore be broadly applicable. We focus on fast food because its wage levels are among the lowest of any sizable industry and for easy comparison across states<sup>1</sup>.

Our primary estimation uses a stacked county-level synthetic control strategy (Wiltshire, 2022, 2024). For each of the 36 treated counties in our sample, we estimate a synthetic control from a donor pool of 122 counties in states that have not raised their minimum wages since the 2009 federal increase. We then stack and average these county-level estimates in event time. This approach provides more precise results than a statewide estimator—allowing us in effect to match, for example, Los Angeles to Montgomery, AL and Atlanta, GA, rather than only California to Alabama and Georgia. It also allows us to match to untreated counties far outside both treated states, reducing concerns about confounds from potential spillovers. Like the new difference-in-difference methods of Cengiz et al. (2019); De Chaisemartin and d’Haultfoeuille (2020); Callaway and Sant’Anna

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<sup>1</sup>New York and California differ in their treatment of tip credits for servers in full-service restaurants

(2021) and others, synthetic control estimates are not biased by staggered treatment adoption when treatment effects are heterogeneous, as is often the case with minimum wage policies. Unlike these regression-based estimators, the synthetic control approach provides interpretable estimates for each treated unit, is not subject to extrapolation bias, and does not depend on parallel pre-trends assumptions.

In 2019, the California minimum wage reached \$12 for all workers, while the fast food minimum wage in New York State reached \$12.75 (\$15 in New York City). Our results through 2019 are broadly similar to much of the existing literature that studies smaller minimum wages increases: we find substantial earnings increases and no significant disemployment effects. The same results obtain when we use a regression-based difference-in-differences approach that accommodates staggered treatment adoption (Callaway and Sant’Anna, 2021). The results also hold for all workers in California, where the minimum wage was binding for all workers. To show this, in Appendix C we use the synthetic-control equivalent of the wage-bin-by-bin estimates in Cengiz et al. (2019).

We then examine the potentially confounding effects of the recent proliferation of local minimum wage laws. We show that these laws reduce the bite of state-level laws, threatening the external validity of standard state-level analyses. We also address the related concern that higher minimum wage policies may have especially large disemployment effects in lower-wage labor markets. Leveraging the power of our county-level design, we omit by turn high-wage counties and counties with local minimum wage laws from our analysis sample. While the point estimates remain statistically non-significant, they provide no suggestion that our null employment effect estimates result from these potential confounds. Instead, they provide further evidence that our conclusions are robust, even when considering only lower-income labor markets.

We proceed by testing for the existence of monopsony in the fast-food industry. We first follow Manning (2011) and examine the effects of the same large minimum wage policies on separation rates of restaurant workers. Increasing minimum wages reduced separation rates, suggesting the presence of monopsony power.

Next, we examine another possible adjustment mechanism to minimum wage increases: pass-throughs to prices. Here we use price and wage data from a large national sample of McDonald’s restaurants collected by Ashenfelter and Jurajda (2022). We find that roughly half of the increase in the wage bill is passed on to consumers in the form of slightly higher prices. Price pass-throughs thus provide an important margin of adjustment to minimum wages, but do not fully account for the lack of disemployment effects. Our other results in this paper suggest that a reduction in monopsonistic rents constitutes the other most likely explanation.

To consider the employment effects up to the \$15 level, which were realized after the onset of the pandemic, we extend our analysis through 2022. Naive estimates that are uncorrected for pandemic-era confounds suggest that minimum wages were associated with sharp negative effects on employment, followed by *sharp positive* impacts. However, the large and sudden economic swings in 2020 make it important to conceptualize the correct counterfactual. Using publicly

available county-level cell phone tracking data (Chetty et al., 2020), we show that the initial local responses to the pandemic (from February through July 2020) of governments, businesses, and individuals—including lockdowns and personal isolation decisions—were greater in our treated counties than in our donor counties, temporarily depressing consumer demand and thus employment in the fast food sector.

To take this confound into account, we first develop a local pandemic-response index using the cell phone tracking data. We then propose and implement a novel extension of synthetic control bias-correction methods (Abadie and L’Hour, 2021; Ben-Michael, Feller, and Rothstein, 2021a). By construction, this correction is orthogonal to the effects of minimum wage policies, which we demonstrate using the pre-pandemic period. After correcting our estimated effects through 2022 for the bias caused by the heterogeneous local pandemic-responses, the positive employment estimates grow larger in magnitude and become statistically significant.

Finally, we consider why we find larger and significant positive employment effects after 2020 than earlier. A key reason is that tight labor markets in recent years increased the elasticity of labor supply (Autor, Dube, and McGrew, 2023), flattening the labor supply schedule. Using the standard monopsony model, we show that minimum wage increases generate larger positive employment effects when labor supply schedules are flatter, consistent with our findings.

Our paper adds to the minimum wage literature by showing that disemployment effects continue to be elusive, even with large and persistent minimum wage increases through \$15 per hour. We further add to this literature by demonstrating that price pass-throughs and reductions in monopsony rents each absorb minimum wage-related cost increases.

Our paper also contributes to a small set of papers that use local data to study minimum wage increases in the U.S. Godoey and Reich (2021), the paper closest to ours, exploits intra-state variation in median wages to examine the effects of recent minimum wage changes in low-wage counties, and find no disemployment effects even where the minimum-to-median wage ratio reaches as high as 82 percent. Azar et al. (2023) also use county-level QCEW data to study the fast food industry. In contrast to our approach, their event window consists only of minimum wage increases from 2010 to 2016—a period with relatively few minimum wage changes. We add to this literature by also examining a broader set of outcomes—notably, separation rates, prices and cost pass-throughs to prices. Compared to the previous literature (e.g. Dube and Lindner (2021)) we account for selection among localities with local minimum wages. And we draw upon Autor, Dube, and McGrew (2023)’s findings that labor supply elasticities increased substantially among low-wage workers after 2020.

The paper proceeds as follows. We discuss the policy environment in Section 2, our data in 3, and methodological approach in Section 4. We present results on employment and earnings in Section 5, and monopsony mechanisms—separations and partial price pass-throughs—in Section 6. We then consider the impact of the pandemic and conduct robustness checks in Section 7. Section 8 considers monopsony explanations of our results when labor supply schedules flatten. Section 9

concludes.

## 2. Policy Background

### 2.1. State and Local Legislation

The U.S. federal minimum wage last increased in 2009q3, to \$7.25. In the years following the Great Recession, state minimum wage increases were restricted to the few states that had already indexed their floors to inflation; thus California’s minimum wage remained at \$8 between 2008 and June 2014, while New York’s remained at \$7.25 between 2009q4 and the end of 2013.

In July 2014, California began increasing its minimum wage for all workers, reaching \$15 in 2022.<sup>2</sup> California minimum wage levels apply to all workers in all industries; and California allows localities to set their own minimum wages above the state level. San Francisco began doing so in 2004, followed by San Jose in 2013 and numerous other California cities in 2015. These local minimum wage policies were often substantially higher than the state level. For example, minimum wages in Los Angeles, San Francisco and San Jose exceeded \$16 by 2022. Table A.1 of the [Online Appendix](#) details the evolution of the minimum wage in the 34 California cities—across nine counties—that had local minimum wages, 17 of which had reached \$15 or higher by 2020q1.<sup>3</sup>

New York State’s minimum wage for all workers began increasing on December 31, 2013. New York State law pre-empts localities from setting their own minimum wages. Nonetheless, responding to local conditions, in 2017 New York State created three minimum wage tiers: one for New York City; a second for the surrounding counties of Nassau, Suffolk and Westchester; and a third for upstate counties.<sup>4</sup> In 2015 New York also began increasing minimum wages for fast food workers at a more rapid rate than for all workers—reaching \$15 in 2021q3—and even earlier in New York City. Table 1 shows the trajectory of these increases.

Figure 1 presents county-wide average weekly earnings in 2013 among the 36 largest counties in California and New York. Earnings were generally higher in the 13 counties with local minimum wages than in the remaining 23 counties, suggesting nonrandom treatment selection that could threaten the external validity of our estimates.<sup>5</sup> Figure 1 also shows that the distribution of average earnings in the 23 California and New York counties without local minimum wages is highly representative of the distribution of average county earnings faced by all U.S. workers.

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<sup>2</sup>From 2023 on, California’s minimum wage is indexed annually, capped at 3.5 percent per year. In 2016 and 2017, California set a \$1 lower minimum wage for employers with 25 workers or less. We ignore this differential, as Wursten and Reich (2023) show that effects on pay and employment for such businesses were the same as among all businesses.

<sup>3</sup>Since all the California cities fully index their minimum wage levels to inflation, their minimum wage rates in 2023 (not shown in the table) were substantially higher.

<sup>4</sup>As Table 1 shows, though all three tiers were designated to eventually reach \$15, in 2022 the minimum wage for all workers in the upstate counties remained lower—at \$13.20.

<sup>5</sup>The 13 counties with local minimum wage counties are also high cost of living areas, a point often noted by local advocates of higher minimums.

To further examine the representativeness of these counties, we show the share of all workers in fast food in each of the counties in Figure A.1, using the same earnings ranking as in Figure 1. The distribution of employment shares in fast food is only weakly correlated with 2013 wage levels, especially among counties that most resemble the rest of the U.S. Notably, the variance of these shares among the treated counties is low, with an inter-quartile range of 1.5 percentage points.

In summary, between 2014 and 2022 minimum wages in California and New York rose dramatically faster and higher than any U.S. minimum wage events in prior decades. Moreover, the distribution of pre-treatment county-level earnings in these two states is representative of labor markets across the U.S. These minimum wage policies thus present a unique opportunity to study the effects of large minimum wage increases on modern labor markets.

## 2.2. *Other Policies*

California passed several other policies in the post-treatment period, including Medicaid expansion and stricter enforcement of wage laws. Moreover, the U.S. Department of Labor also increased enforcement of wage and hour laws in states without substantial enforcement agencies. And the federal government implemented large federal programs designed for recovery from the economic effects of pandemic in the post-treatment period. As Reich (2024) details, these policies do not pose significant confounders for identifying the effects of minimum wage policies in California and New York.

## 2.3. *Impacts of Minimum Wage on Exposed Groups and Areas*

To provide further context for the substantial scope of these policies, we deploy two commonly-applied minimum wage metrics: the ratio of the minimum wage to the median wage and the fraction of workers earning less than the upcoming minimum wage (the "bite"). Figure 2 displays these metrics for all workers in California, for a low-wage local labor market (Fresno) and for a high-wage local labor market (San Francisco) and for restaurant workers.<sup>6</sup>

Panel A of Figure 2 shows how the minimum wage policies changed the ratio of the minimum wage to the median wage. For California this ratio increased from 44 percent in 2013 to 58 percent in 2022.<sup>7</sup> This variation lies within the range of the 138 state minimum wage increases studied by Cengiz et al. (2019); in their sample the highest minimum to median wage ratio is 59 percent.<sup>8</sup>

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<sup>6</sup>The sample size of the Current Population Survey is not sufficient for county-level analyses of fast food wages, nor are the SIC codes detailed enough. We therefore restrict this figure to restaurant workers in California, which does not have a tip credit.

<sup>7</sup>The 31 percent increase in the minimum-to-median wage ratio may seem low for a 87.5 percent increase in the minimum wage; however, median wages also grew by approximately 40 percent during this time period, in California and also in our control group states

<sup>8</sup>In most advanced countries with statutory minimum wages, the comparable ratio lies between .50 and .60 (OECD, 2022); in recent years the average ratio has increased toward the upper end of this range. The current ratio in the UK is .60, scheduled to increase to .66. France's ratio is .61, New Zealand's is .71.

However, some individual California counties lie well outside this range: in low-wage Fresno, the minimum wage to median wage ratio climbed as high as 80 percent, similar to ratios one would find in Alabama or Mississippi if the federal minimum wage were \$15 (Godoy and Reich, 2021). In high-wage San Francisco, which first raised its minimum wage to \$8.50 in 2004 (equivalent to about \$13 in 2022), the minimum wage to median wage ratio is much lower, about 30 percent.

Panel B of Figure 2 displays how California’s minimum wage increases affected the proportions of workers paid less than the new minimum wage. The statewide bite varied between 10 and 15 percent, while the bite in low-wage Fresno County reached as high as 35 percent. The bite of the state minimum wage in high-wage San Francisco was negligible, as expected, since the local minimum wage remained above the state minimum wage for this entire period. The variation in bites between Fresno and San Francisco is similar to the variation among all U.S. counties in 2005-2017 (Godoy and Reich, 2021). The high bite in Fresno and the low bite in San Francisco motivate our use of sub-samples to address potential selection and attenuation bias.

Each panel of Figure 2 also plots these outcomes for exposed subgroups: restaurant workers. The bite for restaurant workers ranges between roughly 40 and 50 percent and the ratio of the minimum wage to the median wage hovers between 90 and 100 percent.<sup>9</sup> Figure 2 thus strongly indicates that restaurant workers are highly exposed to minimum wage policies. Since their wages are lower, fast food workers are even more exposed.

Additionally, Figure A.2 uses CPS data to plot exposure levels in each of our treated counties in California (Panel A) and New York (Panel B). The horizontal axes measure median wages in the county two years prior to the first minimum wage increases. The vertical axes measure minimum wage bites in the county. Not surprisingly, exposure levels are higher in counties with lower pre-treatment median wages.

### 3. Samples and Data

#### 3.1. Analysis Samples

Our primary analysis sample consists of 36 populous counties in California and New York and the states where the federal minimum wage has been binding since 2009. We focus on counties with at least 5,000 restaurant workers, both to reduce measurement error and to provide an intuitive exogenous rule for trimming the set of control/donor pool counties, which is necessary to reduce bias and the likelihood of overfitting a synthetic control (Abadie, 2021; Abadie and Vives-i Bastida, 2022).<sup>10</sup> Our treated areas comprise 25 counties in California and 11 in New York, which in 2013 employed 94 percent of restaurant workers in California and 68 percent in New York. Our donor pool includes 122 populous counties in states where the federal minimum wage has been binding

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<sup>9</sup>An industry’s exposure to minimum wages depends both on its workers’ wage levels and on the labor share of operating costs. Labor costs account for about 30 percent of the restaurant industry’s operating costs, much higher than in retail, health care and most other industries that employ substantial numbers of low-wage workers.

<sup>10</sup>See Section 4 for further details on synthetic controls.

since 2009. These include major urban centers such as Harris County, Texas (Houston), Davidson, Tennessee (Nashville), Salt Lake County, Utah, as well as many smaller cities that meet the same threshold of at least 5,000 restaurant workers.<sup>11</sup>

For our primary analysis, we consider only fast food workers. Fast food workers have the lowest wages of any sizable industry, and so have an outsized importance in any minimum wage analysis. Cross-state variation in tip credits is less of a concern than if we look at all restaurant workers. In [Online Appendix C](#) we conduct several additional analyses focused exclusively on California, where the minimum wage applied to all workers. In those supplemental analyses we consider the distributional effects of these policies. The results support our fast food findings and demonstrate clearly that the effects of these minimum wage policies were felt only at the bottom end of the wage distribution. Since fast food workers as a group earn particularly low wages, they constitute the cleanest treatment group for our primary analysis.

When examining heterogeneity in the local effects of minimum wages within our primary treated sample, we separately impose two additional sample restrictions. The first restriction excludes the 13 counties with a higher county-level minimum wage or a higher local minimum wage in at least one of its constituent localities.<sup>12</sup> The second restriction excludes the high-income outliers in our sample: nine San Francisco Bay Area counties and nine New York City metro counties.<sup>13</sup>

Our analysis period begins in 2009q4, just after the last federal minimum wage increase. For our pre-pandemic analysis we end the treated period in event quarter 21, which is 2019q4 in California. We then extend the treated period through 2022q4, the most recent quarter of available QCEW data at the time of writing. We balance the treated county observations in event time. In our analysis through 2022, we therefore end the treated period in event quarter 33, which is the fourth quarter with a \$15 minimum wage for all treated counties without a local minimum wage.<sup>14</sup> Event quarter 33 is 2022q4 for counties in California and 2022q2 for counties in New York.

### 3.2. Datasets

1. *Quarterly Census of Employment and Wages.* We use the Bureau of Labor Statistics' Quarterly Census of Employment and Wages (QCEW) administrative data for our county-level and state-level analyses. The QCEW data covers about 95 percent of all U.S. payroll jobs. For our fast food analysis, we restrict the QCEW data to private sector workers in NAICS 722513.<sup>15</sup> For our restaurant-focused analysis, we restrict the QCEW data to private sector workers in the California restaurant industry (NAICS 722).

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<sup>11</sup>Table [A.2](#) lists all the donor counties.

<sup>12</sup>The excluded counties with a local minimum wage are: Alameda, Contra Costa, Los Angeles, Marin, San Diego, San Francisco, San Mateo, Santa Clara, Sonoma, Bronx, Kings (Brooklyn), New York (Manhattan), and Queens.

<sup>13</sup>The excluded counties from the Bay Area and New York City are: Alameda, Contra Costa, Marin, Napa, San Francisco, San Mateo, Santa Clara, Solano, Sonoma, Bronx, Kings (Brooklyn), New York (Manhattan), and Queens.

<sup>14</sup>Several counties in the Bay Area and New York City reached a minimum wage higher than \$15 by event quarter 33.

<sup>15</sup>Prior to 2012, the equivalent code is 722211.



Employers report payroll on a quarterly basis and employee headcounts monthly. To construct average weekly earnings, we compute the ratio of industry payroll to employment, divided by 13 (52 weeks / 4 quarters). We cannot distinguish whether changes in weekly earnings result from changes in hourly pay rates or changes in the number of quarterly hours. However, previous research (Nadler et al., 2019) has demonstrated a small variation in quarterly hours in the QCEW.<sup>16</sup>

Since the QCEW observes monthly employment, our employment measure averages employment over the three months in the quarter. The QCEW therefore over-weights full-time workers and those who worked the entire quarter. These groups are less likely to be minimum wage workers. As a result, the QCEW may under-estimate minimum wage effects on weekly earnings and employment.

2. *Current Population Survey.* Our data on hourly wage distributions come from the Current Population Survey (CPS) Outgoing Rotation Group (ORG) samples, beginning in 2009q4 and continuing through 2023q1. We make standard restrictions to the samples, such as excluding self-employed individuals and individuals who did not respond to the wage questions. We restrict the data to workers in the contiguous U.S. who reside in California, New York and the 20 states that did not experience any minimum wage changes since July 2009. CPS data refer to the previous week of the survey and are collected from a representative household sample. The CPS allows estimating effects on weekly hours and annual weeks worked and by demographic group, but the sample size limits its usefulness for data on most counties. In addition to the bites shown in Figure 2, we also use the CPS to show the effects on all workers in California in Appendix C.

3. *Unemployment Data.* As the unemployment rate is an important predictor of our outcomes of interest, we include it as a covariate in our analyses. We obtain annual county-level unemployment rates from the Bureau of Labor Statistics' Local Area Unemployment Statistics (LAUS) program. We also use the LAUS to calculate annual state-level unemployment rates for state-level supplementary analyses.

4. *Pandemic-response Index.* We use Google's Community Mobility Data as aggregated by Chetty et al. (2020) to construct an index of the effects of the local pandemic responses on economic activity in fast food restaurants. Google Mobility data uses location data from smartphones to track their owners in different locations before and after the onset of the pandemic. For each day of the week in each county, these data report the time individuals spent in a location that day relative to the median time spent that same weekday between January 6, 2020 and February 6, 2020.

In particular, we use the time spent at restaurants and retail and local smartphone data on time spent at workplaces from March to 15 to July 15, 2020, approximately the time of the initial pandemic

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<sup>16</sup>The period of pandemic-related restrictions constitutes an exception, as many restaurants restricted their business hours and many low-wage workers could only work part-time.

shock.<sup>17</sup> We discuss the evolution of each of these measures in our analysis sample in [Online Appendix B](#). As we explain in Section 4, we fit our model of how the pandemic affected wages and employment using only control counties, ensuring that minimum wage increases do not contaminate the index.

*5. Quarterly Workforce Indicators.* We use the Census Bureau’s Quarterly Workforce Indicators (QWI) to estimate restaurant industry separation rates. The QWI report separation rates both for all workers and for low-tenure workers who have been with their current employer for less than one year. QWI data consist of matched data from employers and data on employees in Census and other government surveys. The QWI’s coverage is similar to that of the QCEW, though the QWI uses somewhat different data fuzzing and suppression algorithms.

*6. McDonald’s Price and Wage Data.* Beginning in 2016, Ashenfelter and Jurajda (2022) have collected annual data on hourly wages and Big Mac prices for over 10,000 McDonald’s locations in the U.S.<sup>18</sup> We are grateful to the authors for generously sharing with us county-level averages of these variables for all counties with at least five McDonald’s restaurants, which we restrict to those counties in our treated and donor pool samples.<sup>19</sup>

## 4. Methodology

### *4.1. Research Design and Estimating Strategy*

The legislative phase-ins strategy to raise minimum wages to \$15 involved annual or near-annual increases through 2021. These increases began after a long period of no change between 2009q3 and the end of 2013 in New York, and between 2009q3 and the end of 2014q2 in California. They reached \$15 in 2022q1 in California and in 2021q3 in New York. This phase-in schedule admits a long pre-treatment period in every treated county prior to the first increase. Our research design leverages this long pre-treatment period and interprets treatment as the cumulative percentage increase in the minimum wage, implemented through a single phased-in policy over the full treatment period to that date. Thus the “absorbing” treatment (Sun and Abraham, 2021) through to any date is the beginning of the ramp up of minimum wages in 2014q1 in New York counties and in 2014q3 in California counties.

Our primary estimation strategy employs a bias-corrected stacked county-level synthetic control estimator (Wiltshire, 2022, 2024). Like the new difference-in-difference methods of Callaway and Sant’Anna (2021); Cengiz et al. (2019); Borusyak, Jaravel, and Spiess (2022); De Chaisemartin

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<sup>17</sup>Google does not provide disaggregated data for fast food restaurants. Severity of the shock is highly correlated over time, making our index not very sensitive to the choice of particular weeks.

<sup>18</sup>See, also, Ashenfelter and Jurajda (2020) for further details.

<sup>19</sup>Our primary sample for analysis of McDonald’s restaurants data consist of 31 treated counties in California and New York, and 95 donor counties.

and d’Haultfoeuille (2020); Sun and Abraham (2021), synthetic control estimates are not subject to bias from staggered treatment adoption when treatment effects are heterogeneous (as is often the case with minimum wage policies). Unlike these regression-based methods, the synthetic control approach provides interpretable estimates for each treated unit, is not subject to extrapolation bias and does not depend on parallel pre-trends assumptions (Abadie, Diamond, and Hainmueller, 2015; Abadie, 2021).

We estimate separate synthetic controls and paths of treatment effects for each of our 36 treated counties in California and New York. Each synthetic control constitutes an estimate of the counterfactual—as an optimally-matched weighted average of a subset of 122 untreated “donor pool” counties in states that did not experience a minimum wage change since 2009q3.<sup>20</sup> We correct the results for bias resulting from pairwise matching discrepancies (Abadie and L’Hour, 2021; Ben-Michael, Feller, and Rothstein, 2021b), and then stack and average the county estimates, using 2010 population levels as weights. We thereby obtain event-period-specific weighted averages of the individually-estimated synthetic control estimates of treatment effects.

We first estimate our results through 2019q4. As a robustness check, we also estimate the treatment effects using a similar research design and the Callaway and Sant’Anna (2021) DiD estimator, and find no meaningful difference. For our analysis through 2022, we propose an extension of the bias-correction procedure that ameliorates bias from heterogeneous local pandemic effects.

#### 4.2. *Stacked Synthetic Control Estimator*

For a given outcome of interest, our synthetic control estimator selects weights to best match an individual treated county to a subset of untreated “donor pool” counties along specified dimensions in the pre-treatment period. The predictor variables for all specifications include the outcome value and total employment (both normalized to the final pre-treatment quarter) in each quarter from 2009q4 to 2011q4, the averages of those same during that period, and the average unemployment rate during 2009–2011. This common specification for all our synthetic control analyses makes our estimates comparable across analyses and guards against specification searching. The resulting weighted average of donor pool unit outcomes provides the synthetic control estimate of the counterfactual dynamic outcome path. Under fairly general assumptions and with a good pre-treatment “fit” between the treated unit and its synthetic control, the difference in the two dynamic outcome paths yields the estimated treatment effects.

For inference, we present two sets of statistics. The first is the classic, and most widely-used approach, developed in Abadie, Diamond, and Hainmueller (2015, 2010). This approach generates  $p$ -values based on the distributions of the ratios of the (root) mean squared prediction error (MSPE)

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<sup>20</sup>Table A.3 provides an example of donor weights for Synthetic Los Angeles County. For average weekly earnings, the largest weights are obtained for Montgomery, AL; Hidalgo, TX; and Spartanburg, SC, each contributing around 16 to 22 percent of the total weight. The rest of the weights are distributed among 10 other counties; the sum of weights equals one, as constrained by the optimization.

calculated by permuting treatment across untreated units and then randomly sampling them to generate a sample distribution of placebo average treatment effects. For long post-treatment periods over which treatment intensity is increasing, RMSPE  $p$ -values for later periods are inherently conservative, as they include estimates from all preceding post-treatment periods. We therefore also include confidence intervals calculated by a procedure proposed in Arkhangelsky et al. (2021). This approach also relies on the sample distribution of placebo average treatment effects, and assumes both homoskedasticity across units and the asymptotic normality of the estimand.<sup>21</sup>

We further incorporate insights from the bias-correction literature for synthetic controls, intended to account for pairwise matching discrepancies in the pre-treatment period (Ben-Michael, Feller, and Rothstein (2021b); Abadie and L'Hour (2021)). We implement this procedure in our own estimates and we also extend it to correct for the impacts of the various local economic responses to the pandemic, described in more detail below. We estimate all treatment effects and  $p$ -values using the `allsynth` package for Stata (Wiltshire, 2022) and a companion package released with this paper that facilitates the pandemic-response bias correction procedure we detail below: `stackscpvals`.

We provide here a formal exposition of the stacked synthetic control estimator (which nests the classic synthetic control estimator with a single treated unit). We observe a total of  $I + J$  units. Units  $i = 1, \dots, I$  are treated in calendar time  $t = T_{0i} + 1 \leq T$  (which can vary over  $i$ ), and units  $j = I + 1, \dots, I + J$  are the subset of untreated units which comprise our donor pool (let  $T_{0j} = T$ ). Let them collectively be indexed by  $z = 1, \dots, I, I + 1, \dots, I + J$ . For every  $\{z, t\}$  we observe an outcome,  $Y_{zt}$ , which we normalize to 100 in  $t = T_{0i}$  for each  $i$  and its donor pool units.<sup>22</sup> For each  $z$  we observe  $k$  specified predictors of that outcome in the pre-treatment period, which can include linear combinations of the outcome variable and important covariates. The  $k \times 1$  vector  $X_z = (X_{1,z}, \dots, X_{k,z})'$  contains the values of these predictors for  $z$ , and the  $k \times J$  matrix  $\mathbf{X}_0 = [X_{I+1}, \dots, X_{I+J}]$  contains the values of the predictors for the donor pool.

Define  $Y_{zt}^N$  as the potential outcome if  $z$  does  $\{N\}$ ot receive an intervention, and for  $t > T_{0z}$  define  $Y_{zt}^{Int}$  as the potential outcome if  $z$  receives an  $\{Int\}$ ervention. For any  $\{z, t\}$ , the marginal treatment effect is:

$$\tau_{zt} = Y_{zt}^{Int} - Y_{zt}^N \quad (1)$$

Since we observe  $Y_{it}^{Int} = Y_{it}$  for each treated unit  $i = z \leq I$  in  $t > T_{0i}$ , we only need to estimate  $Y_{it}^N$  to estimate  $\tau_{it}$ . The synthetic control estimator for  $Y_{it}^N$  is:

<sup>21</sup>In cases with many ( $M$ ) treated units each placebo average will also be random draws of  $M$  donor pool units; thus the distribution is approximately normal by a central limit theorem. We thank Guido Imbens for a helpful observation on this point.

<sup>22</sup>We normalize separately for each treated unit, since donor pool units are often common for at least some or all  $i$ . This normalization effectively removes unit fixed effects from the data, similar to the demeaning approach proposed by Douchenko and Imbens (2016); Ferman and Pinto (2021), while also allowing estimation of effects in percentage changes.

$$\hat{Y}_{it}^N = \sum_{j=I+1}^{I+J} w_j^i Y_{jt} \quad (2)$$

We follow Abadie, Diamond, and Hainmueller (2010) and impose restrictions on the weights that help justify considering the estimated synthetic controls as valid counterfactual estimates. Specifically, given a set of weights  $v_1^i, \dots, v_k^i$  that determine the relative importance of the  $k$  predictors,<sup>23</sup> the synthetic control  $\hat{\mathbf{W}}^i = (\hat{w}_{I+1}^i \dots \hat{w}_{I+J}^i)'$  is selected that minimizes the distance between  $i$  and its donor pool units:

$$\left( \sum_{h=1}^k v_h^i (X_{h,i} - w_{I+1}^i X_{h,I+1} - \dots - w_{I+J}^i X_{h,I+J})^2 \right)^{1/2} \quad (3)$$

subject to  $\sum_{j=I+1}^{I+J} w_j^i = 1$  and  $w_j^i \geq 0 \forall j \in \{I+1, \dots, I+J\}$ , where the second constraint prevents extrapolation bias, and where both constraints together permit interpretation of the synthetic control as a weighted average of the outcome values of the *donor pool* units (Abadie, 2021).

$\hat{\tau}_{it} = Y_{it} - \hat{Y}_{it}^N \forall \{i, t\}$  follows from estimation of (2). Place the  $\hat{\tau}_{it}$  in event time  $\forall i, e \leq E$ , such that  $e(T_{0i} + 1) = 0 \forall i$ . The estimated average treatment effect on the treated in  $e$ ,  $\hat{ATT}_e$ , is:

$$\hat{\tau}_e = \sum_{i=1}^I \gamma_i \hat{\tau}_{ie} = \sum_{i=1}^I \gamma_i (Y_{ie} - \hat{Y}_{ie}^N) \quad (4)$$

with weights  $\gamma_i$  on the treated units such that  $\gamma_i \geq 0 \forall i$  and  $\sum_i \gamma_i = 1$ .

Taking the above as the “classic” stacked synthetic control estimator, we then implement the synthetic control bias-correction procedure proposed by Abadie (2021); Abadie and L’Hour (2021) and Ben-Michael, Feller, and Rothstein (2021a), as follows. First, for each treated unit  $i$  we obtain  $\hat{\mathbf{W}}^i$  from synthetic control estimation on the uncorrected (normalized) outcome values,  $Y_{it}$ . Second, for each  $i$  we estimate  $\hat{\mu}_{0t}^i(x)$ , which is a predictor of  $Y_{it}$  given  $X_i = x$ , by regressing each  $Y_t$  on the complete set of predictor variables, *using only the donor pool units* for  $i$ . This procedure allows us to calculate the residualized outcome values,  $\tilde{Y}_{zt} = Y_{zt} - \hat{\mu}_{0t}^i(X_z)$ . Third, we apply the estimated  $\hat{\mathbf{W}}^i$  to the  $\tilde{Y}_{jt} = Y_{jt} - \hat{\mu}_{0t}^i(X_j)$  to calculate  $\tilde{Y}_{it}^N = \sum_{j=I+1}^{I+J} \hat{w}_j^i \tilde{Y}_{jt}$ , which admits the bias-corrected synthetic control estimated gaps for each  $\{i, t\}$ ,  $\hat{\tau}_{BCit} = \sum_{i=1}^I \gamma_i (\tilde{Y}_{it} - \tilde{Y}_{it}^N)$ . We can then place these gaps in event time and use them to calculate the analog of Equation (4), corrected for bias arising from pairwise differences in predictor values.

To contrast these estimates with the pandemic-response-bias-corrected estimates (described next), we refer to these bias-corrected estimates as “uncorrected” estimates.

<sup>23</sup>We use the regression-based method (Kaul et al., 2022) to select the  $v_h^i$  weights.

### 4.3. Correcting for Differences in Local Pandemic Responses.

The synthetic control method yields relatively unbiased treatment effect estimates under a linear factor model, given a sufficient number of pre-treatment periods and a donor pool that is selected to contain only viable control units, and provided that (A) we obtain a good pre-treatment fit between each treated unit and its synthetic control for all predictor variables; and (B) there are no confounding shocks in the treated period that affect the treated units and donor pool units differently.

Condition (A) is likely satisfied in our setting, as is Condition (B) through 2019. After 2019, our estimates are complicated by the heterogeneous spread of pandemic and the heterogeneous responses to it. Recent research has highlighted the differential local intensity and effects of changes in government, consumer and worker behavior in response to the pandemic, as well as the associated shift to working from home (Alexander and Karger, 2021; Goolsbee and Syverson, 2021). These behavioral changes exhibit spatial heterogeneity that correlates geographically with, but is not caused by, higher minimum wages in California and New York. In particular, pandemic restrictions in urban counties in California and New York were longer and more restrictive than elsewhere; the shift to working from home exhibits a similar heterogeneity (Chetty et al., 2020).

We correct for pandemic-response bias *after* estimating the synthetic control weights, by removing the pure effect of the *initial* local pandemic response on the outcome values. We control only for the initial weeks of the pandemic, when the shock was most plausibly exogenous.

We do so by first regressing employment on the pandemic index in each quarter, using *only* the donor pool units. None of our donor states experienced a minimum wage increase, mechanically preventing this measure from being confounded by a response to minimum wage changes. We then residualize the outcome values in that period for *all* (treated and donor pool) units using that estimated average pandemic effect and the intensity of the local pandemic response, which was systematically greater in our treated counties. Provided the minimum wage changes experienced by the treated group had no causal effect on the intensity of the initial local pandemic response, the resulting “pandemic-corrected” estimate is unconfounded by differences in local pandemic policies or behavioral responses, while still capturing the full impact of the minimum wage increases.

More formally, consider this method as an extension of the bias-correction procedure detailed above: First, as before, for each  $i$  we obtain  $\hat{\mathbf{W}}^i$  from synthetic control estimation on the truly uncorrected (raw, normalized) outcome values,  $Y_{it}$ , using the original set of predictors. Second, we add our pandemic-intensity index  $c_z$  for each county to the set of predictor variables, yielding  $\tilde{X}_z = (X_{1,z}, \dots, X_{k,z}, c_z)'$ , then regress each  $Y_t$  on the complete set of predictors *plus* the pandemic-exposure index, *using only the donor pool units*. This allows us to calculate the residualized outcome values,  $\tilde{Y}'_{zt} = Y_{zt} - \hat{\mu}_{0t}^i(\tilde{X}_z)$ . Third, we apply  $\hat{\mathbf{W}}^i$  to the  $\tilde{Y}'_{jt}$  to calculate  $\tilde{Y}'_{it} = \sum_{j=l+1}^{l+J} \hat{w}_j^i \tilde{Y}'_{jt}$ , yielding (in event time) the analog of Equation (4) corrected for bias arising from differences in predictor values *and* initial local pandemic policies and behavioral responses:

$$\tilde{\tau}_{BC_e} = \sum_{i=1}^I \gamma_i \tilde{\tau}_{BC_{ie}} = \sum_{i=1}^I \gamma_i (\tilde{Y}'_{ie} - \tilde{Y}^{Nt}_{ie}) \quad (5)$$

The resultant  $\tilde{\tau}_{BC_e}$  can be interpreted as the causal effect of the minimum wage under the same assumptions as those for the standard synthetic control bias-corrected estimator and the additional requirement that minimum wage changes did not have a causal effect on the pandemic exposure index. More specifically, we need: (1) a suitable comparison group and (2) no reverse causality.

A suitable comparison group is obviously key to any research design. Here we particularly want to ensure that the pandemic-exposure index is not incidentally controlling for differences between our treatment and control that have not already been accounted for by our predictor variables. A classic example would be “anticipation effects” (a confound which seems unlikely for the pandemic). More generally, we should expect that  $E[\tilde{Y}'_{zt}] = E[\tilde{Y}_{zt}]$  for all  $t < 2020q1$ . Fortunately, this relationship is approximately true, as can be seen in Panel B of Figure B.1 of the [Online Appendix](#), which shows the difference in outcome values before and after the pandemic correction.

The second issue, reverse causality, is mechanically shut down by our estimation procedure because we estimate the *coefficients* in the bias-correction regression using only data from donor pool counties, which all have identical and unchanging minimum wages. This approach still allows high minimum wages to worsen the effects of pandemic shocks. If, for instance, areas with higher minimum wages were unable to respond as flexibly to the pandemic and employment fell as a result, we would still expect to see that evidence in the estimated gaps. Our approach effectively prevents unintentionally controlling for part of the true effect of the minimum wage when we are trying to control only for pandemic-related effects.

#### 4.4. Regression-based Estimator

We complement our main synthetic control analysis with the analogous DiD regression. We use a standard design with county and quarter-fixed effects and our donor pool counties, which are all “never-takers”, as our controls. The coefficients of interest are the interaction between quarter dummies and a binary treatment indicator. We use the method suggested by Callaway and Sant’Anna (2021), and estimate standard errors using a wild bootstrap.

## 5. Earnings and Employment Results

We present in this section our earnings and employment estimates for fast food workers for the period through 2019. We begin with our estimate in all treated counties and then examine local variations in minimum wage effects. Finally, we present our results using an alternative DiD method.

### 5.1. *Effects on Earnings and Employment in All Treated Counties*

Panel A of Figure 3 plots the effects of minimum wage increases on fast food weekly earnings (left panel) and employment (right panel). Each blue circle indicates the estimated gap in a treated county in any given quarter, with the relative 2010 county population indicated by the size of the circle. The solid blue line represents the dynamic population-weighted average estimated effect across all 36 treated counties. Event quarter 0 indicates the first quarter of treatment—2014q1 for New York counties and 2014q3 for California counties. Event quarter 2019q2 in New York and 2019q4 in California are the final periods before the pandemic.

In Panel B of Figure 3, the solid blue line again displays the average effect, while the dark gray lines show the sample distribution of 100 randomly sampled placebo average estimated effects. The light grey bands around the blue line indicate the 95 percent confidence intervals in each period, based on the variance of the sample distribution of placebo averages.

The wage and employment outcomes in Panel A of Figure 3 each display very good pre-treatment fits in the vast majority of treated counties and an excellent pre-treatment fit on average. This result is not mechanical, since we select our synthetic controls using matching variables only in the first half of the pre-treatment period. Panel B indicates that the minimum wage increases caused substantial and significantly higher earnings for fast food workers, without any evidence of negative effects on fast food employment.

Panel A of Table 2 quantifies these estimated effects in event quarter 21. Average earnings increased by 17 percent; the placebo-variance-based 95 percent confidence intervals rule out an earnings elasticity with respect to the minimum wage below 0.23. Unsurprisingly, this elasticity is higher than the earnings estimates in minimum wage studies that focus on all restaurant workers, such as Cengiz et al. (2019). The RMSPE-based  $p$ -value of 0.01 indicates the earnings estimate is highly significant.

In contrast, our employment results are statistically insignificant. The confidence interval means that we cannot rule out employment elasticities from positive 0.03 to negative 0.08. This lower-bound is higher than that of Cengiz et al. (2019). It indicates that the finding of no disemployment effects in many recent studies remains at higher minimum wage increases.

This average effect combines the impacts from areas such as Manhattan and San Francisco that are much more expensive and have higher wages than most of the U.S.—as well as more rural areas that are lower-wage, such as Tulare, CA or Orange, NY. Including these high-wage areas may mask negative effects in lower-wage areas. To test external validity, we next exclude areas with local minimum wage policies or areas with very high-wages .

### 5.2. *Local Variation in Minimum Wage Effects*

Minimum wage policies may be endogenous to employment outcomes. In that case, estimated employment effects might be more negative if the minimum wage were applied to a broader pop-



ulation. As Dube and Lindner (2021) point out, cities that enact higher minimum wages tend to already have higher wages, suggesting that minimum wages in these places have less bite. The same pattern applies at the state level: states with higher minimum wages also tend to have higher average wages. These facts have been suggested to explain why studies fail to detect negative employment effects in the lowest wage areas. The simplest version of this argument claims that estimates are attenuated relative to an elasticity that might predict the impact of minimum wages on untreated areas. The inverse of this concern—not considered in the literature—is equally valid. If employers possess market power that suppresses wages and employment, selection and attenuation biases could mask *positive* employment effects of minimum wages.

Our setting includes localities that raised their minimum wages in response to local labor market conditions, as well as localities that had increases imposed on them by state governments. Our sample also includes both high-wage and low-wage counties. Figure A.2 shows some of this variation. We therefore can test both the effects of selection into local minimum wage laws and potential attenuation bias due to smaller bites.

To test for selection effects, we re-estimate our results *excluding* counties with a binding local minimum wage in at least one of its local entities. We present local minimum wage schedules in Table 1 and [Online Appendix Table A.1](#)<sup>24</sup> We display our results in Panel B of Table 2.

In Panel C, we re-estimate our results using a treated sample that excludes the four counties with average earnings above the 90th percentile (San Francisco, Santa Clara, New York (Manhattan), and San Mateo— see Figure 1) and their surrounding counties. This approach accommodates potential spillovers from the high-income counties that boost wages and mitigate the bite of minimum wages in the surrounding counties. The restriction excludes the four large New York City counties and the nine large constituent counties of the San Francisco Bay Area; or 14 (74 percent) of our 36 treated counties— We present these results in Panel C of Table 2 and Figure A.3 of the [Online Appendix](#).

In both panels B and C, the employment estimates become more *positive*, but remain insignificant. This result is inconsistent with high-wages or selection on labor market characteristics masking disemployment effects. However, the increasingly less-negative own-wage elasticities (OWEs) are more consistent with a monopsony explanation.

The estimates in Panels B and C of Table 2 do not directly confront the overlap noted in Dube and Lindner (2021) between counties that chose to increase their local minimum wages and those that have high average wages. To address this issue, we also estimate our results by earnings quartile. [Online Appendix Table A.4](#) displays our estimated earnings and employment effects by quartile and by the presence of local minimum wage policies. The results confirm that minimum wage effects on wages are greater in counties with lower earnings, while employment effects are not.

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<sup>24</sup>The counties with no local minimum wages accounted for 45 percent of fast food employment in all 36 counties in 2013.

### 5.3. *Alternative Specifications*

We next present multiple robustness tests of our employment estimates. We begin by considering the sensitivity to our preferred estimating strategy. To do so, in Panels A and B of Table 3 we present average county-level estimates with and without the bias-correction procedure.<sup>25</sup> The uncorrected stacked synthetic control estimates in Panel B are more positive than our preferred bias-corrected estimates in Panel A. However, both methods yield confidence intervals that do not rule out zero employment effects in either the full sample or the sample excluding counties with local minimum wages.

Finally, in Panel C of of Table 3 we present estimates from a dynamic difference-in-differences estimator (Callaway and Sant’Anna (2021)). The results show positive but statistically non-significant effects on employment. The estimates for the no-local minimum wage sample show a more positive estimate on employment, despite coming from the lower-income counties that did not pass minimum wage legislation. This pattern is not consistent with minimum wages having more deleterious employment effects in low-wage counties.<sup>26</sup>

## 6. Detecting the Presence of Monopsony

In this section, we consider why our employment results through 2019 are so muted, focusing on two potential adjustment margins: the reduction of monopsonistic rents and cost pass-throughs to prices. These explanations are perhaps the two most commonly offered in previous work on minimum wages; other theorized adjustment margins, such as firm exit or automation, do not appear to be relevant in the case of fast food (Ashenfelter and Jurajda, 2020) despite increased adoption of online ordering and self-service kiosks.

We find that minimum wages reduced employee separation rates, consistent with monopsony models. Using data from a national survey, we also find evidence that McDonald’s restaurants passed on roughly 50 percent of their increased wage bill to consumers as higher prices. This partial pass-through of the increased labor costs leaves room for a monopsony power explanation.

### 6.1. *Monopsonistic Employment Markdowns*

In monopsony models, firm-level labor supply schedules slope upward, workers face limited outside options and a monopsonistic firm pays wages below the level that would obtain in a competitive labor market—at the cost of being unable to hire as many workers as it wants at these subpar wages. The firm finds it as profitable to pay lower wages—and accept subpar employment levels and higher employee turnover costs—as to raise wages to attract new workers, which would necessitate raising wages for its incumbent workers (Burdett and Mortensen, 1998). Binding minimum

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<sup>25</sup>See Wiltshire (2022) for further discussion on “classic” synthetic controls vs bias-corrected synthetic controls.

<sup>26</sup>In results not shown here, our estimated effects are also broadly robust to using different pre-treatment years to calculate donor weights, to alternative covariate specifications (such as including GDP or house price growth), and to a state-level analysis using state-level QCEW data. These results are available upon request.

wage increases overcome the low-wage option by forcing the firm to pay the higher wage to all its employees: workers then face higher wages and accordingly supply greater quantities of labor, while the minimum wage becomes the new (flat) marginal cost of labor to the firm, inducing higher quantities of labor demanded.

Among the variety of monopsony models (see Manning (2021) for a review), the dynamic model that emphasizes search and matching frictions best fits the fast food restaurant industry. Fast food restaurants locate near their customers—and therefore near each other. Fast food workers thus usually have multiple feasible outside options. Moreover, fast food exhibits the highest job vacancy and employee turnover rates of any industry.

As Manning (2011) showed, wage increases do not affect separation rates in competitive labor markets, but they do reduce separation rates in monopsonistic labor markets. Indeed, in a monopsonistic equilibrium, the separation rate determines the elasticity of labor supply to a firm as well as wage and employment markdowns. A natural first test of the existence of monopsony is thus whether minimum wages reduce restaurant workers’ separation rates, as the dynamic monopsony model predicts.

## 6.2. *Effects on Employee Separation Rates*

Using the Quarterly Workforce Indicators (QWI) dataset, we examine here the causal effects of minimum wages on workers’ separation rates.

Like the QCEW, the QWI collects wage and employment data from employers. Unlike the QCEW, the QWI collects employer-based separation rate data among all workers and among workers with less than a full year of tenure with their current employer. Since the QWI data are available only to the four-digit level, we examine minimum wage effects on all restaurant workers, not just fast-food workers.<sup>27</sup> County-industry separation rates can be highly seasonal, even compared to employment, and especially for low-tenure workers. We therefore de-seasonalize the separation rates in each county using the same approach as in Peri, Rury, and Wiltshire (Forthcoming). We then proceed using the same methods as with our primary estimates. We use the same stacked county-level synthetic control estimator with the QWI that we used in Section 5 with the QCEW.

Figure 4 presents our results for separation rates of restaurant workers.<sup>28</sup> We obtain close pre-policy fits for the separation rate outcome. Results suggest significant negative effects on the separation rates of all restaurant workers in the pre-pandemic period, both among all restaurant workers and among low-tenure restaurant workers (those not employed at the same establishment one month previously), dropping 4.3 and 19.7 percent, respectively, in the final pre-pandemic quarter. These

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<sup>27</sup>The QCEW and the QWI use slightly different algorithms for data fuzzing and suppression. As a result, our QWI samples of treated and donor counties differ slightly from our QCEW samples. Nonetheless, the patterns in our QWI estimated earnings and employment effects, not shown here, are broadly consistent with our QCEW results.

<sup>28</sup>The results for the sample of counties without local minimum wages are very similar to those of the sample excluding counties with local minimum wage, and are available upon request.

results, which are consistent with results using regression-based methods in Dube, Lester, and Reich (2016) and Wursten and Reich (2023), indicate the presence of monopsony in restaurant labor markets and the capacity of minimum wage increases to overcome monopsony power. However, monopsonistic labor markets do not preclude other responses to minimum wage increases, such as cost pass-throughs to prices. We discuss this adjustment mechanism next.

### 6.3. *Cost pass-through to prices at McDonald's Restaurants*

This subsection examines how minimum wages affect wages and prices at McDonald's restaurants. To do so, we use the Ashenfelter and Jurajda (2020) (hereafter, AJ) store-level dataset. AJ collected data on average hourly wages of front-line workers and Big Mac prices at over 10,000 McDonald's locations in the U.S., around September 1, in each of the seven years from 2016 to 2022.<sup>29</sup> They did not collect any data on employment.

McDonald's dominates the burger chain segment of fast-food, with over 14,000 stores, most of them franchises, in the U.S. The chain competes on price rather than quality and enjoys higher profit margins than its fast-food competitors.<sup>30</sup>

Using all the minimum wage changes in their 2016 to 2020 McDonald's store sample and difference-in-difference methods on county-level data, AJ found that minimum wages increased average wages and prices. Their wage elasticity with respect to the minimum wage is about 0.7, their estimate of the price elasticity with respect to wages is 0.2 and their price elasticity with respect to minimum wages is 0.14.<sup>31</sup>

Since we do not have McDonald's data for the entire pre-treatment period (2009 to 2014), we apply a modified version of our county-level stacked synthetic control approach. Specifically, using only the subset of counties with sufficient sample size in the AJ data, we normalize each McDonald's outcome in each county to its 2016 level and then apply the synthetic control donor county weights we previously estimated using QCEW average earnings data. This procedure assumes the average earnings synthetic controls are good counterfactual estimates for the McDonald's data. The AJ data includes 31 of our large treated counties in California and New York and 95 of our donor counties. This subsample is sufficient to identify plausible counterfactuals. Indeed, our weekly wage results are unchanged when we use the QCEW data for the subset of treated and donor counties observed in the AJ data (Figure A.4).

#### 1. *McDonald's Hourly Wages* Hourly wages increased on average about 22 percent in our Cali-

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<sup>29</sup>We are grateful to Orley Ashenfelter and Stepan Jurajda for sharing their updated dataset with us.

<sup>30</sup><https://www.foodindustry.com/articles/what-are-the-profit-margins-in-the-fast-food-business/>

<sup>31</sup>AJ did not detect any effects on the adoption of labor-saving (touch screen ordering) technology or store entry and exit rates. They also did not detect any effects on concentration of franchise ownership or any effects of ownership concentration on wages and prices.

California and New York counties from 2016 to 2019, while the minimum wage increased about 29 percent. These increases imply a treatment elasticity of 0.75, as shown in Table 4 and Figure A.5. This result is similar to AJ's estimate of 0.7 using similar data for a different set of minimum wage changes and a difference-in-differences strategy; it is much higher than our all-fast-food estimate (reported in Table 2) of 0.29. The higher treatment effect at McDonald's is consistent with evidence that McDonald's wage rates are lower than in the industry as a whole (Gailliot et al., 2022).

*2. McDonald's Prices* We consider here the extent of cost pass-throughs to prices at McDonald's stores. Cost pass-throughs are likely to be greater when product demand is inelastic—which is the case for the fast-food industry—when labor costs comprise a higher portion of costs (as is again the case for fast food) and when employers in an industry possess more labor market power. Figure 5 shows that prices in the treated counties did increase faster than in the donors, about 4 percent from 2016 to 2019. As we show in Table 4, these results imply a price elasticity of 0.12, again similar to AJ's price pass-through estimate of 0.14. The results in Table 4, when multiplied by the 0.3 labor share of costs in fast food, suggest that McDonald's passed about half of the minimum wage-related labor cost increases to prices in the period through 2019, in the full and no-local samples.<sup>32</sup>

The combination of incomplete pass-through to prices and no disemployment effect calls for further explanation. The presence of monopsony power provides a prime candidate, especially as we already showed that minimum wages reduce separation rates.

*3. Ratio of McDonald's Prices to Wages* We discuss here how minimum wages affect the ratio of prices to wages at McDonald's. As Ashenfelter and Jurajda (2020) documented, technology, scale and non-labor inputs are relatively uniform across McDonald's stores. The effects of minimum wages on the ratio of store prices to store wages thus suggests the extent to which McDonald's stores absorbed increased labor costs in the form of reduced rents.

Figure A.10 shows that the McDonald's price-wage ratio in 2016 was 1.48 in our treated counties and 1.55 in our donors. Figure 5 shows that the price-wage ratio in the treated counties fell steadily from 2016 through 2019—about 14 percent relative to in the donors. This finding suggests that minimum wages reduced rents in the treated areas.

*4. Summary* Our synthetic control analyses of McDonald's store-level hourly wages show that industry-level and establishment-level data reveal similar effects on earnings. We find a partial pass-through of wages to prices, suggesting room for a monopsony explanation of how McDonald's stores absorb the higher wage costs of minimum wage increases. Finally, we find that minimum wage increases reduce the ratio of prices to wages, suggesting that the policies reduce monopsony rents.

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<sup>32</sup>A labor share of 0.3 approximates the ratio of fast food payroll to revenue in the 2017 and 2022 Economic Censuses.

## 7. The Pandemic and its Aftermath

We next consider the impact of minimum wage changes during and after the pandemic, when minimum wages increased to \$15/hour.<sup>33</sup> Panel A of Figure 6 shows how fast food employment evolved, on average, in the treated counties of California and New York versus in the donor pool counties, through the end of 2022. This figure simply displays the normalized raw employment data averaged by sub-sample. After growing at very similar rates in each sub-sample throughout the pre-treatment period, fast food employment began to grow faster in treated counties in 2017 (after the relevant minimum wage reached at least \$10.50 in every treated county), continuing to grow through the end of 2019 (the end of the pre-pandemic period).

Pandemic-induced lockdowns and personal isolation decisions then caused sharp employment contractions in all counties, especially in the treated counties. Following the end of pandemic restrictions the employment gap again becomes positive and continues to grow through the end of 2022.<sup>34</sup>

With these patterns in mind, we first use our stacked synthetic control approach to estimate minimum wage effects under the naive assumption that the pandemic equally affected treated and donor pool counties (on average). We show these results in the left-hand plot in Panel B of Figure 6. This figure extends our pre-pandemic estimates through event quarter 33 (when all treated counties had been exposed to four quarters of a \$15 minimum wage. After falling sharply in 2020Q2, the average gap in treated county employment quickly rebounded and turned positive by mid-2021, reaching a (non-significant) estimated employment increase of 2.2 percent (see, also, column (1) of Table B.1 in the [Online Appendix](#)).

However, local legislative and behavioral responses to the pandemic exhibited substantial spatial heterogeneity (Alexander and Karger, 2021; Goolsbee and Syverson, 2021), making untenable the assumption of equal average pandemic confounds among treated and donor pool counties. We demonstrate as much in Figure 7, using smartphone location-tracking data (Chetty et al., 2020) to show the effect of the pandemic and subsequent developments on time spent in restaurants and retail (Panel A), and time spent in workplaces (Panel B), relative to early 2020. Time spent in restaurants and retail fell between 30 and 50 percent in the donor states, and between 50 and 70 percent in California and New York.

While time spent in retail and restaurants recovered substantially by 2021 in the donor areas, it remained below pre-pandemic levels in California and New York as late as the last week of available data in 2022. Time spent in the workplace fell by a similar amount to time spent in restaurant and retail in the donor states, and again fell more sharply in California and New York than in any donor state in 2020. Unlike time spent in restaurants and retail, time spent in the workplace in the donor states has not recovered to pre-pandemic levels, perhaps representing a permanent shift.

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<sup>33</sup>We focus on our main employment and earnings results in this section. Since we cannot apply the pandemic-correction method we described here to our limited McDonald's data, we presented results only through 2019 in Section 6

<sup>34</sup>California lifted its restaurant capacity restrictions fully on June 15, 2021.

Since these unanticipated shocks impacted our treatment and control counties differently, we implement a novel procedure to correct the estimates of bias resulting from discrepancies in local pandemic responses between each treated county and the donor pool counties. Our measure of local pandemic responses—an index which incorporates the effects of *county-level* responses of government, businesses, and individuals to the pandemic—is informed directly by actual behavior during the shaded period of Figure 7.

Our pandemic-response-bias correction, which extends the synthetic control bias-correction procedure of Abadie and L’Hour (2021) and Ben-Michael, Feller, and Rothstein (2021a), proceeds in five steps. 1. For a particular treated county we estimate the synthetic control weights. 2. Using only the donor pool counties, we regress the outcomes in each period on the pandemic-response index and the other predictors. 3. We use the estimated coefficients from these regressions to predict the outcomes in the donor counties *and* the treated county (free of any impact of the minimum wage). 4. We calculate the difference between the actual and predicted outcomes in each donor pool county and the treated county, yielding residuals that are free of the unconfounded impact of the pandemic. (They are not confounded by the minimum wage policies because we estimate the pandemic-response coefficient using only the donor counties.) 5. We calculate the difference between the treated county residuals and the weighted average of the donor pool county residuals, using the weights we estimated in the first step.

The result of these five steps provides the pandemic-corrected estimated minimum wage effect in the treated county. We repeat this entire process for every treated county, then finally stack and average the pandemic-corrected estimated effects in event time, yielding our estimated pandemic-corrected average treatment effects. Section 4 describes this procedure in detail (see, also Appendix B).

We present the pandemic-corrected estimated effects of minimum wages on employment in the right-hand plot of Panel B of Figure 6. Accounting for the effects of the local pandemic responses changes the trajectory of fast food employment in our treated counties relative to our control counties. As shown in Figure B.1, the pandemic correction does not affect the pre-pandemic estimates (demonstrating that the pandemic-response index is only spuriously geographically correlated with the treated counties). However, the pandemic-corrected employment effect grows, on average, in our treated counties, relative to their synthetic controls, once the pandemic begins. We quantify these estimated effects in Panel A of Table 5. Among all treated counties, one year after reaching \$15 per hour, these minimum wage policies increased employment in treated counties by 7.3 percent. The corresponding employment elasticity is +0.08, with placebo-variance confidence intervals from 0.03 to 0.13 and a RMSPE  $p$ -value of 0.08. These estimates increase to 13.6 percent ( $\epsilon = 0.15$ ) and 11.5 percent ( $\epsilon = 0.13$ ) in Panels B and C, for our non-local sample and our lower-income sample, respectively.

The results of this exercise lead us to draw three conclusions. First, even before accounting for the confounding effects of differential local responses to the pandemic, we do not find evidence that \$15 minimum wage policies cause disemployment. (Indeed, the uncorrected point estimates are

all positive). Second, although the local responses to the pandemic (and the substantial negative consequence for fast food employment) were much stronger in the treated counties (in California and New York) than in the donor pool counties, the apparent brief negative shock to employment from minimum wage policies disappears after accounting for the confounding effect of the local pandemic response. Third, accounting for differential local pandemic responses suggests that minimum wage policies through \$15 significantly *increased* fast food employment in treated counties.

## 8. Employment Effects with Varying Labor Supply Elasticities

As we have noted, minimum wage researchers have often invoked monopsony to explain minimal employment findings. However, the monopsony model is compatible with a variety of positive and negative employment outcomes. This variety is not recognized when the assumed counterfactual consists only of the equilibrium wage and employment levels of a competitive labor market. Our discussion in this section here adds to these discussions, by also using a different baseline: the monopsony wage and employment levels that obtain in the absence of a minimum wage. We do so because of the evidence for monopsony that we discussed in Section 6: (1) employee separation rates decline when minimum wages increase, as suggested by a search-and-matching frictions monopsony model; and (2) McDonald's stores passed about half of their minimum wage-related higher labor costs to consumer prices, leaving substantial room for a decline in monopsony power to explain the absorption of a substantial portion of the cost increases.

We then examine whether a monopsony model can explain our finding in Section 7, of smaller wage and larger positive employment effects of minimum wages for the period through 2022. We build here on the work of Autor, Dube, and McGrew (2023), who showed that labor supply elasticities of low wage workers increase substantially reduced when labor markets are very tight, as has been the case since 2020. In other words, we examine monopsony effects on wages and employment when labor supply schedules are much flatter. We show that a relatively steep labor supply schedule is consistent with sizable positive effects on wages and relatively small effects on employment. In contrast, the flatter labor supply schedule that developed with the tight labor markets after the pandemic is consistent with smaller wage effects and larger positive employment effects than was the case before the pandemic.

Finally, we examine the effects of a partial cost pass-through to prices in the context of monopsony. In this model an increase in prices increases the marginal revenue product of labor and thus shifts the labor demand curve to the right. In our case, the price increase is only 3 percent, implying that the effects on employment will also be small.

### 8.1. *A typology of monopsony employment effects*

Beginning with Card and Krueger (1994) and continuing at least through Manning (2021), minimum wage researchers have repeatedly appealed to the presence of monopsony to explain the absence of negative employment effects. However, the monopsony model is consistent with a



range of positive and negative employment outcomes. We show this variety of outcomes using the left panel of Figure 8. In the left panel, the competitive equilibrium is denoted by  $(w^*, L^*)$ . We represent the going wage without a minimum wage law and under monopsony power as  $w^M$ , with a corresponding employment level of  $L^M$ . Compared to  $(w^*, L^*)$ , both wage and employment are marked down:  $w^M < w^*$  and  $L^M < L^*$ . The baselines in the three cases below therefore consist not just in the competitive equilibrium  $(w^*, L^*)$ , but also in the monopsony equilibrium  $(w^M, L^M)$ .

*Case 1:* The implementation of a minimum wage,  $\underline{w}^1$ , raises the equilibrium wage but remains below the competitive wage:  $w^M < \underline{w}^1 < w^*$ . Here  $L^M < \underline{L}^1 < L^*$ . So  $\underline{w}^1$  raises wages and employment, relative to no minimum wage, but markdowns remain, relative to the competitive equilibrium. In this plot, the labor supply schedule in the left panel is relatively steep (low labor supply elasticity). As a result, the wage markdown is relatively large and the employment markdown is relatively small, perhaps too small to detect. This pattern is consistent with the findings of numerous minimum wage studies, including ours, for the period through 2019.

*Case 2:* A larger minimum wage,  $\underline{w}^2 > \underline{w}^1$ , raises the equilibrium wage above  $w^*$ :  $\underline{w}^2 > w^*$ . The new wage is above the competitive wage and the new employment level is below  $L^*$ . Nonetheless employment,  $\underline{L}^2$ , is higher than the baseline level,  $L^M$ .

*Case 3:* A minimum wage,  $\underline{w}^3$ , that is higher than  $\underline{w}^2$ , reduces employment relative to the baseline level  $L^M$ .

This typology of cases suggests that monopsony models are consistent with studies that find a small positive employment effect, no employment effect, or a small negative employment effect, even when the wage effects are clearly positive.

## 8.2. Wage and employment effects when labor supply elasticities increase

We turn next to the right panel of Figure 8.<sup>35</sup> Here, the labor supply elasticity is much higher than in the left panel, and the labor supply schedule is much flatter. The schedule in this panel is thus consistent with two key findings in Autor, Dube, and McGrew (2023). After 2020, wages rose more among lower-paid workers than among higher-paid workers; the elasticity of labor supply among low-paid workers also increased substantially after 2020.<sup>36</sup> Autor, Dube, and McGrew (2023)

<sup>35</sup>We represent similar minimum wage increases in both panels, from  $w^M$  to  $\underline{w}^1$  and from  $\underline{w}^1$  to  $\underline{w}^2$ , because the annual minimum wage increases from 2013q4 2019q4 and from 2020 to 2022 were similar. From 2013q4 to 2019q4, the minimum wage increased from \$7.25 in NY and \$8 in CA to \$12 in both states, an average increase of just over 50 percent over six years. The increases between 2020 and 2022, from \$12 to \$15, represent an increase of 25 percent over three years. The annual increases are thus nearly the same in both periods.

<sup>36</sup>For workers with a high school or less education and who are under 40, Autor, Dube, and McGrew (2023) estimate a quit elasticity of .663 for 2015 to 2019 and 1.075 for 2021 to 2022. Other studies that find flattening labor supply schedules when unemployment rates are low include Hirsch, Jahn, and Schnabel (2018), Bassier, Dube, and Naidu (2022) and Webber (2022). Bastian (2024)'s review of recent labor supply elasticity estimates in the EITC literature finds that the labor supply elasticity for the EITC-eligible population falls in a similar range, .35 to .50. Sokolova and Sorensen (2021) provide a meta-analysis of labor supply elasticity estimates.

further show that this flattening of the labor supply schedule results from a very tight labor market. Under such conditions, workers' outside options improve and employers' monopsony power falls.

Positive employment effects of monopsony are greater when the labor supply elasticity is greater. Compared to the outcome in the left panel, the wage markdown in the right panel is smaller and the employment markdown is larger.<sup>37</sup> The substantial increase in the labor supply elasticity that occurred after 2020 thus predicts smaller wage effects and larger positive employment effects, relative to the results through 2019. This is exactly what we find.

We note also that a tighter labor market, with workers moving up the job ladder, is also consistent with an inward shift in labor supply curve for "low-rung" industries like fast food. If counterfactual wages are increasing, this shift provides a complementary reason for our finding of smaller wage effects post-pandemic.

### 8.3. *Employment effects of cost pass-throughs to prices*

Finally, we turn to employment effects when firm pass their higher wage costs to consumer prices. Higher prices, which increase the marginal revenue product of labor, can be modeled by a rightward shift of the labor demand curves in Figure 8. The labor share of fast-food operating costs is about 0.3 and the minimum wage elasticity of average wages is about 0.2. A full pass-through to prices, therefore, implies only a 0.06 shift of the demand curve. A fifty percent pass-through implies only a 0.03 shift. These shifts imply greater wage and employment markdowns, but evidently, the differences will be very small. We conclude that a fifty percent pass-through would not have detectable effects on employment.

## 9. Conclusions

Our analysis of \$15 and higher minimum wage policies examines the effects of legislated minimum wage levels and percentage increases that are considerably higher than any studied in the modern U.S. research literature. Our main treated sample consists of fast food workers in 36 large counties—25 in California and 11 in New York. These counties are representative of the U.S as a whole: the distribution of pre-treatment average county wages spans the set faced by U.S. workers. This pattern implies our results are generalizable to jurisdictions across the U.S.

Using a stacked synthetic control estimation strategy, we show that these higher minimum wages did not cause disemployment effects—even in lower-wage counties or counties that did not choose to increase their minimum wages locally.

We also discuss how firms could accommodate such higher minimum wages without causing disemployment. We present two results consistent with a monopsony framework. First, we show that employee separation rates decrease after minimum wage increases, which should not occur in perfectly competitive labor markets Manning (2011). Second, we show that McDonald's passed only

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<sup>37</sup>See Appendix A for a brief algebraic proof.

half of the costs of these policies through to customers in higher prices. This partial pass-through leaves room for the policies to be absorbed in reduced employer rents, which is also inconsistent with models of perfect competition.

We find that employment responses to the pandemic were stronger in California and New York than in donor states. This pattern confounds simple synthetic control and DiD estimates of minimum wage effects during 2020 to 2022. We develop a straightforward method to ameliorate this bias using publicly available smartphone location-tracking data. After making this correction, we show that minimum wages continued to have no disemployment effects. Lastly, we use the standard monopsony model to show that a flattening of the labor supply schedule, which occurred after 2020, generates larger positive employment effects. Indeed, we estimate larger positive employment effects after 2020, consistent with such a flattening of the labor supply schedule.

TABLE 1  
Minimum Wage Evolution in California And New York (2013-2022q1)

Locality	2013	2014	2015	2016	2017	2018	2019	2020	2021	2022
<b>California (All Workers)</b>	8.00	9.00*	9.00	10.00	10.50	10.50	12.00	13.00	14.00	15.00
Los Angeles (city)	8.00	9.00*	9.00	10.50*	12.00*	13.25*	14.25*	15.00*	15.00	16.04*
San Francisco	10.55	10.74	11.05/12.25†	13.00*	14.00*	15.00*	15.59*	16.07*	16.32*	16.99*
San Jose	10.00†	10.15	10.30	10.30	10.50/12*	13.50	15.00	15.25	15.45	16.20
<i>Further California municipal minimum wages can be found in Table A.1 of the <a href="#">Online Appendix</a></i>										
<b>New York (All Workers)</b>										
Nassau, Suffolk, Westchester ‡	7.25	8.00	8.75	9.00	10.00	11.00	12.00	13.00	14.00	15.00
New York City ‡	7.25	8.00	8.75	9.00	11.00	13.00	15.00	15.00	15.00	15.00
Rest of state ‡	7.25	8.00	8.75	9.00	9.70	10.40	11.10	11.80	12.50	13.20
<b>New York (Fast Food Workers)</b>										
New York City ‡	7.25	8.00	8.75	10.50	12.00	13.50	15.00	15.00	15.00	15.00
Rest of state ‡	7.25	8.00	8.75	9.75	10.75	11.75	12.75	13.75	14.50/15*	15.00

*Notes:* This table shows the history of minimum wages in California and New York that reached \$15 by 2022q1. Table A.1 of the [Online Appendix](#) lists sub-state minimum wages in California, although we list some here as an example. Minimum wages are for the largest employer size category.

\* Indicates the increase took effect in July; otherwise the increase occurred in January. Increases in New York were effective on December 31; they are entered as effective on January 1 of the following year. New York State increased its fast food minimum wage on December 31, 2020 and July 1, 2021.

† San Francisco changed its minimum wage in January and May 2015. San Jose increased its minimum wage in March 2013.

‡ Multiple wage tiers depending on size of business and health insurance coverage.

*Sources:* Vaghul and Zipperer (2021), UC Berkeley Labor Center Local Minimum Wage Inventory and the authors' research.

TABLE 2  
Average Effects Through 2019

	Average Weekly Earnings	Employment	Own-wage Elasticity
<i>A. All Treated Counties</i>			
Treatment Effect	17.01	-1.43	-0.08
Elasticity	0.29	-0.02	
Placebo-variance-based 95% CIs	[0.23, 0.36]	[-0.08, 0.03]	[-0.27, 0.10]
Placebo-variance-based <i>p</i> -values	0.00	0.37	
RMSPE-based <i>p</i> -value	0.01	0.44	
<i>B. Excluding Counties with Local Minimum Wages</i>			
Treatment Effect	15.82	-0.30	-0.02
Elasticity	0.32	-0.01	
Placebo-variance-based 95% CIs	[ 0.24, 0.40]	[-0.09, 0.08]	[-0.28, 0.24]
Placebo-variance-based <i>p</i> -values	0.00	0.89	
RMSPE-based <i>p</i> -value	0.03	0.45	
<i>C. Excluding Counties in the SF Bay Area and NYC</i>			
Treatment Effect	14.93	0.61	0.04
Elasticity	0.26	0.01	
Placebo-variance-based 95% CIs	[0.19, 0.32]	[-0.05, 0.07]	[-0.18, 0.26]
Placebo-variance-based <i>p</i> -values	0.00	0.71	
RMSPE-based <i>p</i> -value	0.02	0.50	

*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 treated-sample-specific average percent change in the minimum wage through event quarter 21. 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.

TABLE 3  
Average Employment Effects by Estimator Through 2019

	Full Sample	No Local MW Sample
<i>A. Stacked synthetic control (Bias-corrected)</i>		
Treatment Effect	-1.43	-0.30
Elasticity	-0.02	-0.00
Placebo 95% CIs	[-0.08, 0.03]	[-0.09, 0.08 ]
RMSPE <i>p</i> -value	0.37	0.89
<i>B. Stacked synthetic control (uncorrected)</i>		
Treatment Effect	0.24	2.73
Elasticity	0.00	0.06
Placebo 95% CIs	[-0.05, 0.06]	[-0.04, 0.15 ]
RMSPE <i>p</i> -value	0.89	0.24
<i>C. Difference-in-differences</i>		
Treatment Effect	3.83	9.04
Elasticity	0.07	0.18
WBS CIs	[-0.47, 0.60]	[-0.31, 0.68]

*Note:* Estimated using employment and payroll data from the QCEW and local unemployment data from LAUS. *Full Sample* includes 36 treated counties: 25 in California, plus 11 in New York. For the *No Local MW* sample we restrict the sample to 23 treated counties without local minimum wages: 16 in California, plus 7 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 results are averaged in event time by jurisdiction over event quarter 21. The bias-correction procedure is specific to synthetic controls, so the DiD estimates (Panel C) are *Uncorrected*, as are the synthetic control estimates in Panel A. The *Bias-corrected* synthetic control results in Panel B are corrected for bias due to pairwise matching discrepancies among the included predictor variables. Placebo confidence intervals are calculated based on Arkhangelsky et al. (2021), RMSPE *p*-values are calculated based on Abadie, Diamond, and Hainmueller (2015). Wild bootstrap standard errors (WBS) are clustered at the state level and calculated using the procedure from Callaway and Sant’Anna (2021).

TABLE 4  
Average Effects For McDonald's Establishments Through 2019

	Average Hourly Wage	Price	Pass-Through
<i>A. All Treated Counties</i>			
Treatment Effect	21.65	3.57	0.55
Elasticity	0.75	0.12	
Placebo-variance-based 95% CIs	[0.59, 0.90]	[0.07, 0.18]	[0.28, 0.82]
<i>B. Excluding Counties With Local Minimum Wages</i>			
Treatment Effect	16.36	2.78	0.57
Elasticity	0.71	0.12	
Placebo-variance-based 95% CIs	[0.54, 0.88]	[0.05, 0.20]	[0.19, 0.95]

*Note:* Estimated using McDonald's data from the Ashenfelter and Jurajda (2020). 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 2019 for panels. 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.

TABLE 5  
Average Effects Through 2022

	Average Weekly Earnings	Employment	Own-wage Elasticity
<i>A. All Treated Counties</i>			
Treatment Effect	8.82	7.33	0.83
Elasticity	0.10	0.08	
Placebo-variance-based 95% CIs	[0.05, 0.15]	[0.04, 0.13]	[0.21, 1.46]
RMSPE-based <i>p</i> -value	0.03	0.08	
<i>B. Excluding Counties with Local Minimum Wages</i>			
Treatment Effect	8.80	13.60	1.55
Elasticity	0.10	0.16	
Placebo-variance-based 95% CIs	[0.04, 0.17]	[0.09, 0.23]	[0.41, 2.69]
RMSPE-based <i>p</i> -value	0.14	0.04	
<i>C. Excluding Counties in the SF Bay Area and NYC</i>			
Treatment Effect	10.60	11.47	1.08
Elasticity	0.12	0.13	
Placebo-variance-based 95% CIs	[0.07, 0.17]	[0.06, 0.19]	[0.36, 1.81]
RMSPE-based <i>p</i> -value	0.02	0.05	

*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 first 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.

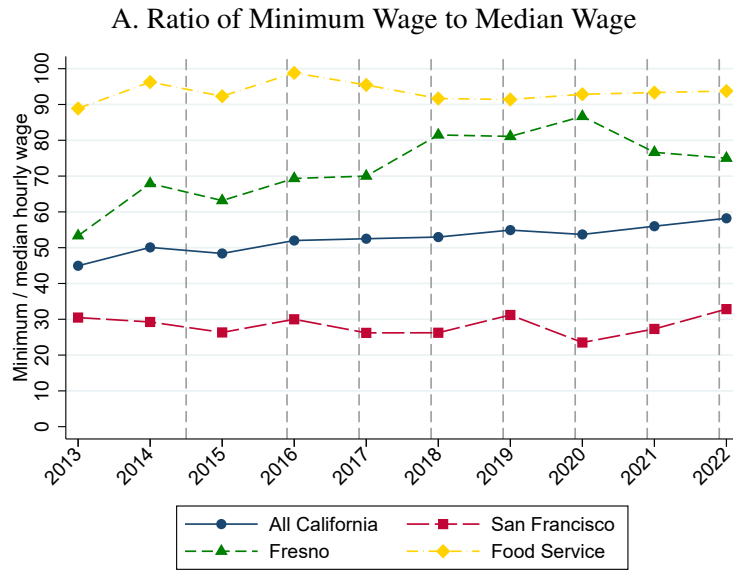


FIGURE 1  
Distribution of Average Wage by County

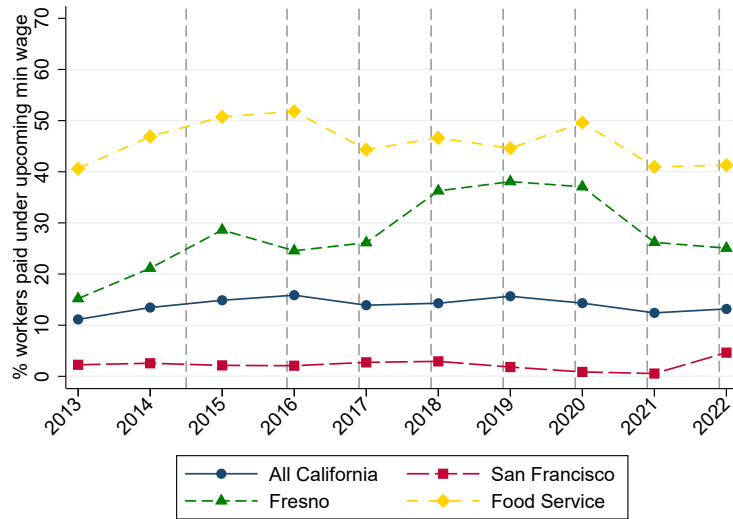


Notes: This figure shows the distribution of the employment-weighted average QCEW weekly wage across all quarters in 2013 for all industries in a given county. Treated counties are shown as individual points; their place in the national distribution is indicated by the vertical bars. The black bar shows 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 2  
Reach of California Minimum Wages, 2014-2022



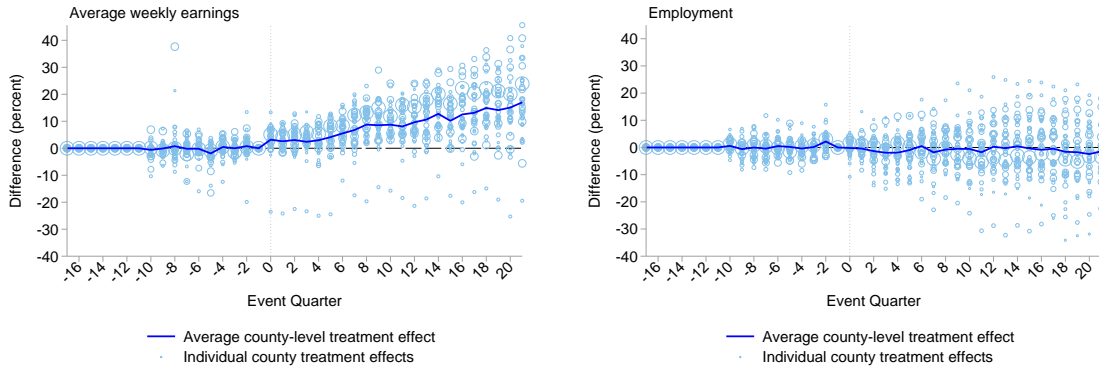
B. Fraction of Workers Earning Under the Upcoming Minimum Wage



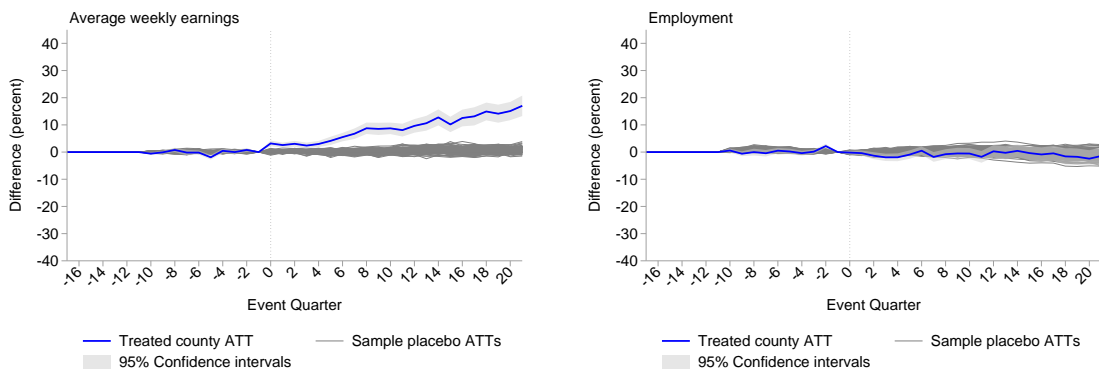
Notes: This figure displays the reach of California’s minimum wage levels. Panel A shows the ratio of the minimum wage to the median wage by year; Panel B shows the percent of workers earning wages under the upcoming minimum wage. These metrics are calculated using CPS data aggregated at the annual level. Food service restricts the data to Census classification codes 8680 and 8690, which correspond to NAICS code 722. The gray vertical dashed lines indicate the timing of state-wide minimum wage increases, all of which are one nominal dollar, except for 2017 and 2018 (\$0.50 each).

FIGURE 3  
Treatment Effects in Full Sample of Treated Counties Through 2019

A. Average and County-level Treatment Effects



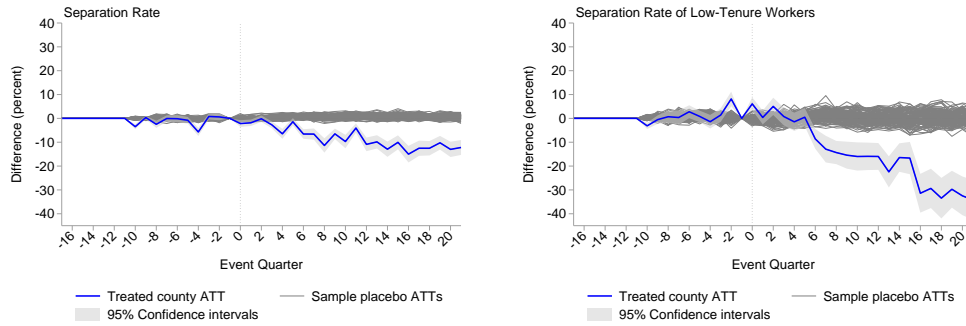
B. Average Effects in Treated Counties vs Sample Placebo Average Effects



*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.

FIGURE 4  
Average Effects On Separation Rates Of Restaurant Workers Through 2019

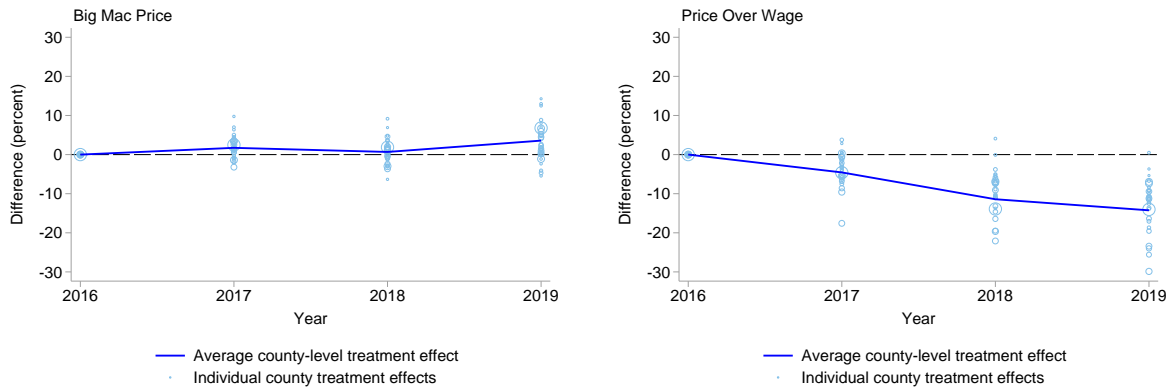
All Treated Counties



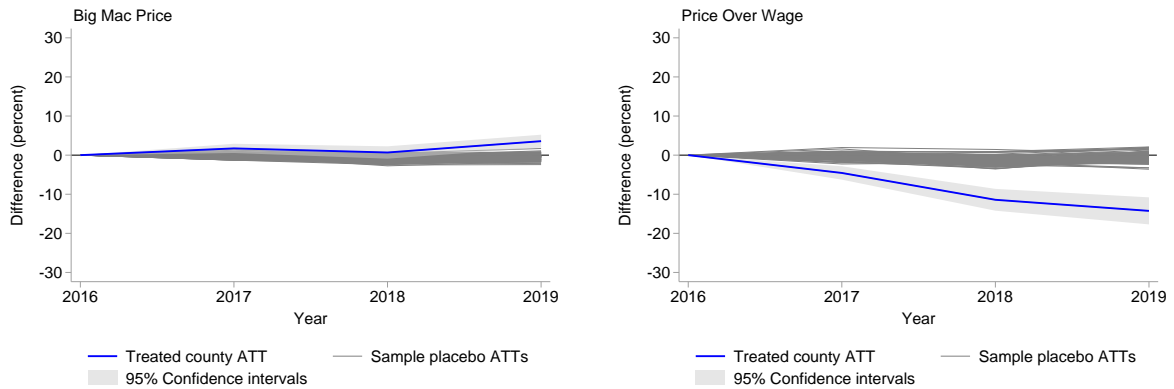
*Note:* Estimated using employment and separation data from the QWI, and local unemployment data from LAUS. Samples of counties constructed using employment data from QCEW. Restaurant workers are identified as those employed in NAICS 7225. Treated and donor pool counties are identical to the analysis presented in Figure 3. 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. The solid blue line shows the average estimated effect across all treated counties. The grey lines show 100 randomly sampled averages of 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 5  
 Treatment Effects On Big Mac Price And  
 On Price-Over-Wage Through 2019

A. Average and Individual Treated County Effects



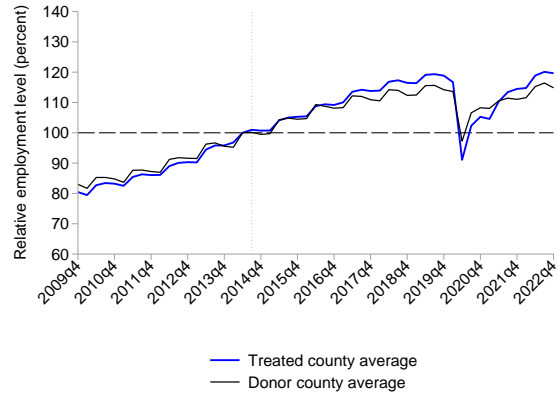
B. Average Effects in Treated Counties vs Sample Placebo Average Effects



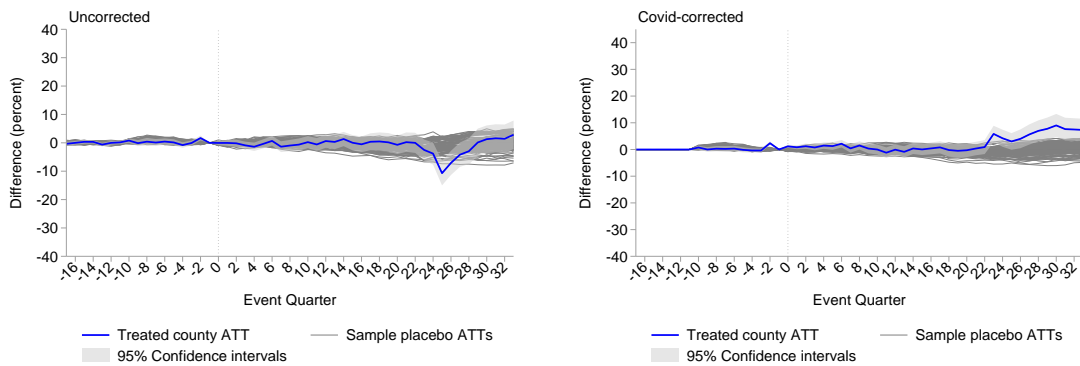
*Note:* Estimated using McDonald’s data from Ashenfelter and Jurajda (2020). McDonalds sub-sample includes 31 treated counties: 21 counties in California, plus 10 counties in New York. The treated counties all have  $\geq 5,000$  employment in NAICS 722. The donor pool consists of 95 counties with  $\geq 5,000$  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.

FIGURE 6  
 Employment Level And Average Effects Through 2022

A. Employment In Treated and Donor Counties (2014Q2 = 100)



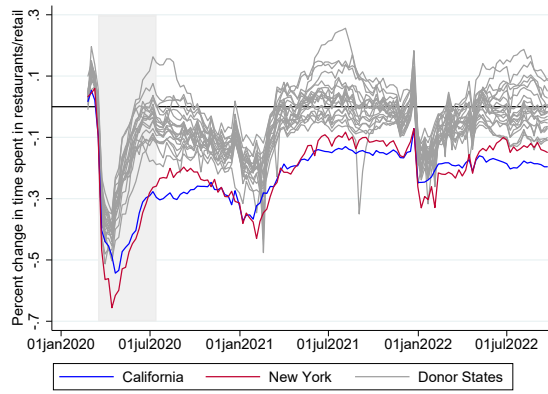
B. Average Effect in Treated Counties With And Without Pandemic Correction



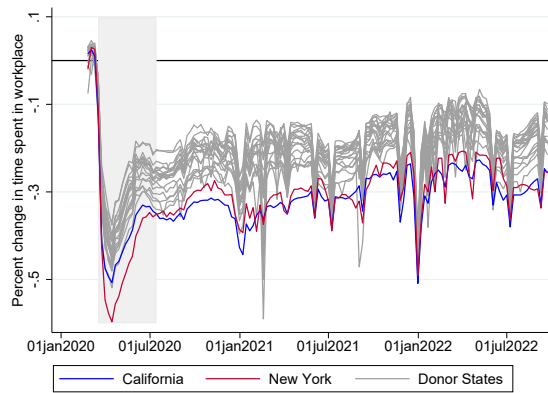
*Note:* Panel A shows the average raw employment levels for treated and donor pool counties, respectively, normalized to 100 in 2014q2. Panel B shows the uncorrected estimated average effect, while Panel C shows the Covid-corrected estimated average effect. In Panels B and C the solid blue line shows the average estimated effect across all treated counties; the grey lines show 100 randomly sampled averages of 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 *Covid-corrected* results are corrected for bias from matching discrepancies and the pandemic effect.

**FIGURE 7**  
**The Evolution Of Mobility Since COVID By Establishment Type**

**A. Change In Time Spent in Restaurants and Retail Establishments**

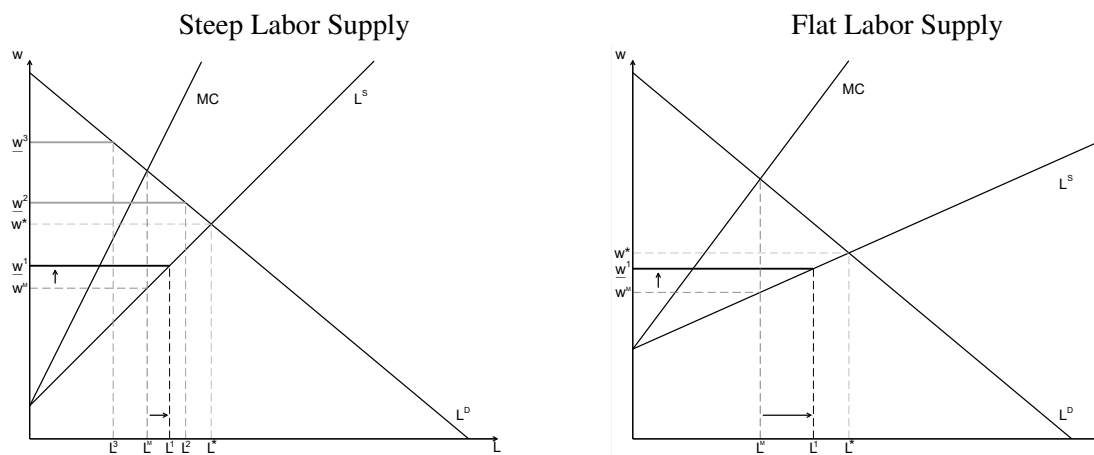


**B. Change In Time Spent in Workplace Location**



*Note:* Panel A shows the percent change in time spent in restaurants and retail for California and New York versus Donor States. Panel B depicts the percent change in time spent at the workplace for California and New York versus Donor States. Time spent in retail and restaurants comes from the Google Mobility data. The area shaded in gray is the time period captured by our pandemic index.

FIGURE 8  
Monopsony And Minimum Wages



Note: In each plot  $L^D$  denotes labor demand,  $L^S$  – labor supply, and  $MC$  – marginal cost function of the monopsony.  $L^*$ ,  $w^*$  is competitive employment and wage, respectively;  $L^M$ ,  $w^M$  is equilibrium under monopsony without regulation; while  $L^i$ ,  $w^i$  for  $i = 1, 2, 3$  are equilibria under monopsony with the minimum wage level set at  $w^i$ .



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# Online Appendix

for

## *Minimum Wage Effects and Monopsony Explanations*

Justin C. Wiltshire, Carl McPherson, Michael Reich, and Denis Sosinskiy

### 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:

$$Y_{ct} = \gamma_c + \lambda_t + \sum_{t \in T} \beta_t D_{ct} + \varepsilon_{ct} \quad (6)$$

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:  $\gamma_s$  and  $\lambda_y$  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`.<sup>38</sup> 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:

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<sup>38</sup>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 8, 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
<b>Alameda County</b>									
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
<b>Contra Costa County</b>									
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
<b>Los Angeles County</b>									
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
<b>Marin County</b>									
Novato	8.00	9.00	10.00	10.50	11.00	12.00	13.00	15.24	15.77
<b>San Diego County</b>									
San Diego	8.00	9.75	10.50	11.50	11.50	12.00	13.00	14.00	15.00
<b>San Mateo County</b>									
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
<b>San Francisco County</b>									
San Francisco	10.74	11.05	12.25	13.00	14.00	15.00	15.59	16.32	16.99
<b>Santa Clara County</b>									
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
<b>Sonoma County</b>									
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
<b>Average Weekly Earnings</b>				
<i>A. Primary Sample of All Treated Counties</i>				
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 <i>p</i> -value	0.01	0.01	0.02	0.01
<i>B. Excluding Counties with Local Minimum Wages</i>				
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 <i>p</i> -value	0.01	0.01	0.01	0.16
<b>Employment</b>				
<i>C. Primary Sample of All Treated Counties</i>				
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 <i>p</i> -value	0.81	0.42	0.18	0.40
<i>D. Excluding Counties with Local Minimum Wages</i>				
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 <i>p</i> -value	0.79	0.33	0.59	0.86

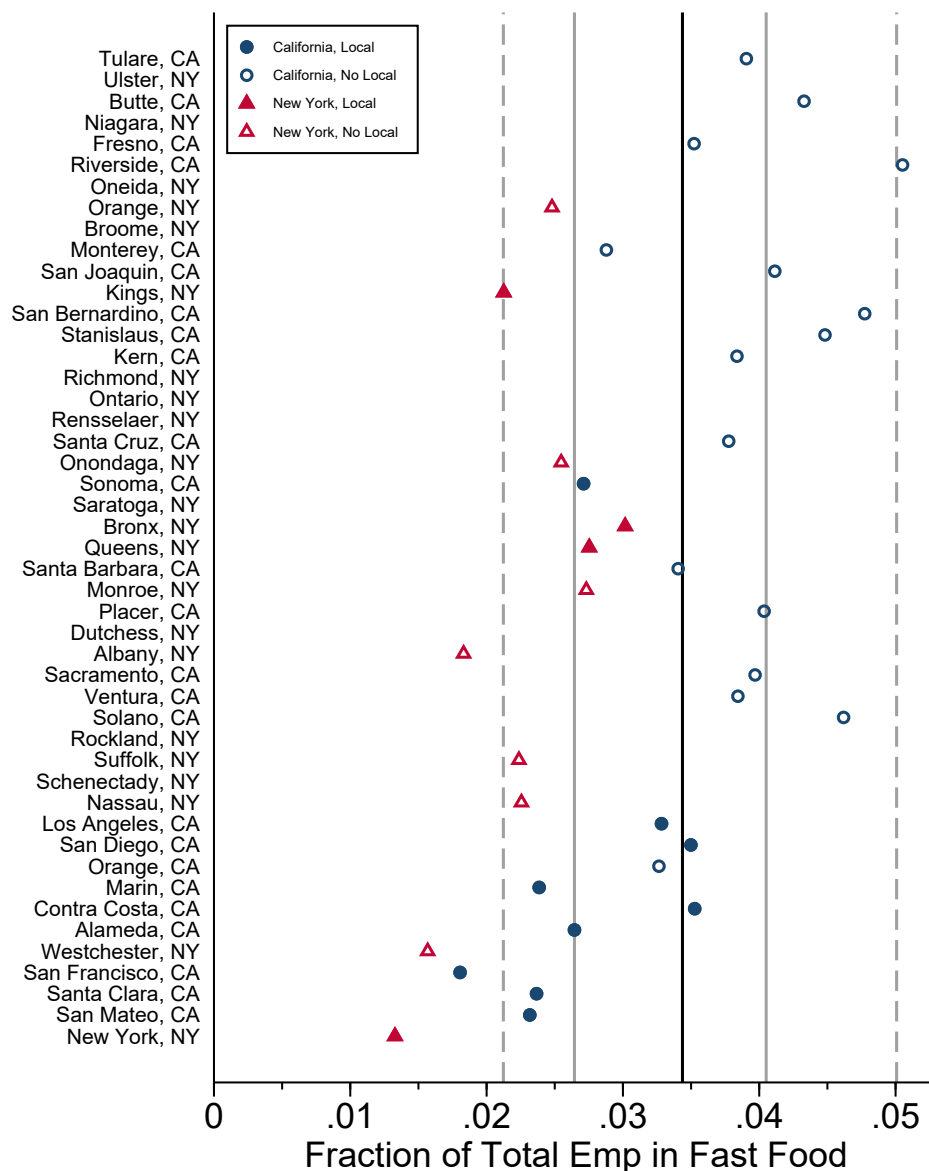
*Note:* Estimated using employment and payroll data from the QCEW and local unemployment data from LAUS. For the sample of fast-food workers, we have a total of 36 treated counties: 25 in California, plus 11 in New York. All treated counties have  $\geq 5,000$  employment in NAICS 722. The donor pool 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
<i>A. All Treated Counties</i>			
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]
<i>B. Excluding Counties With Local Minimum Wages</i>			
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]

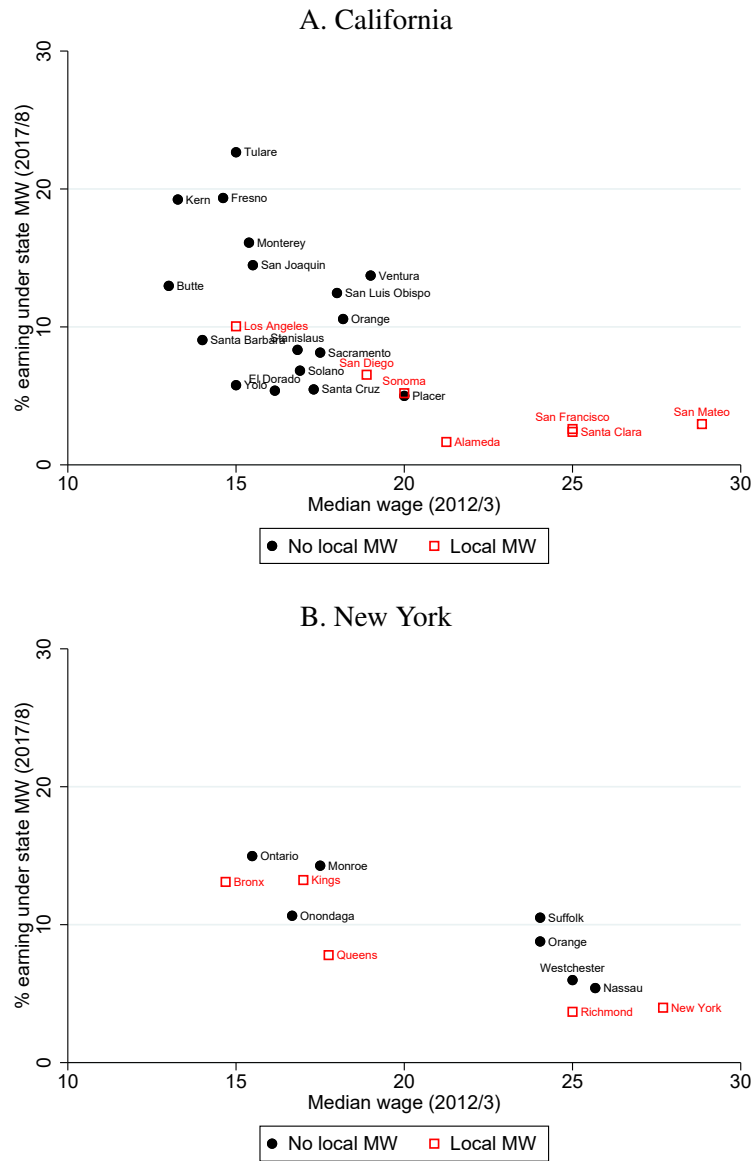
*Note:* Estimated using McDonald's data from the Ashenfelter and Jurajda (2020). McDonalds sub-sample includes 31 treated counties: 21 counties in California, plus 10 counties in New York. The treated counties all have  $\geq 5,000$  employment in NAICS 722. The donor pool consists of 95 counties with  $\geq 5,000$  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

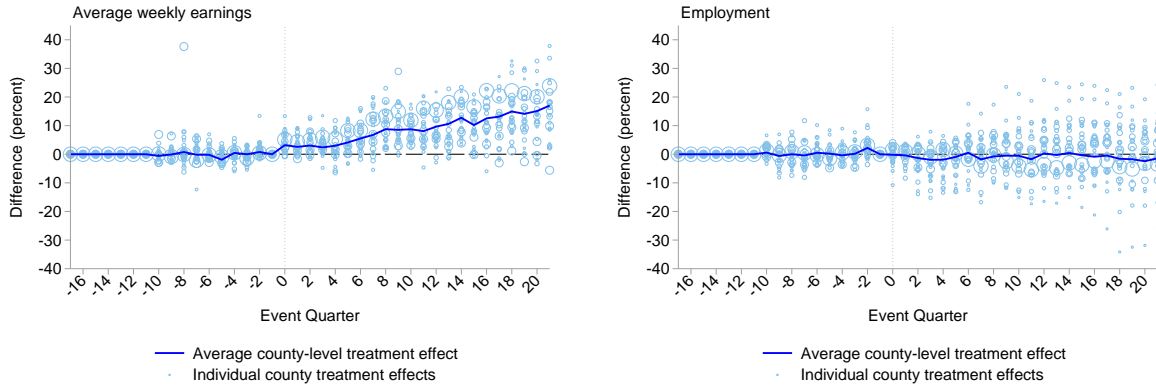


Notes: The figures above plot two measures of county-level exposure to minimum wages. The horizontal axes show the median wage in the two years before the first minimum wage increase. The vertical axes show the average percent earning under the upcoming minimum wage in 2017 and 2018. Years were pooled 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.

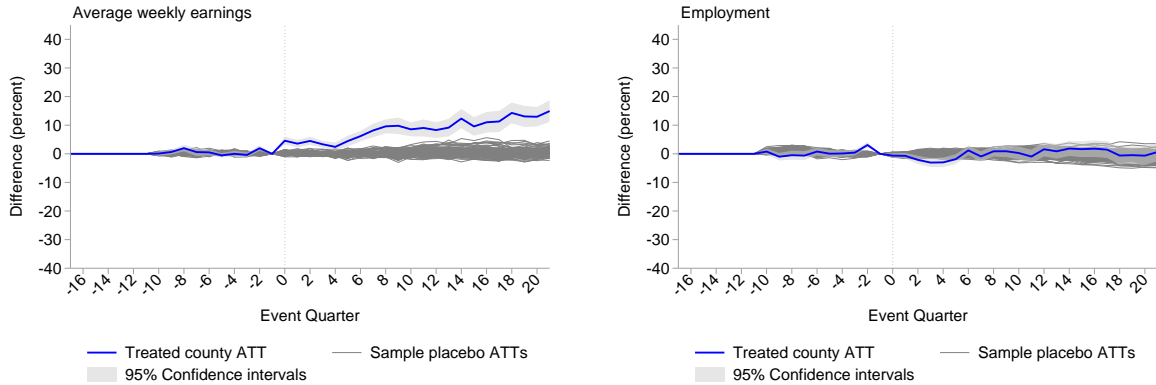
FIGURE A.3

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

A. Average and Individual Treated County Effects



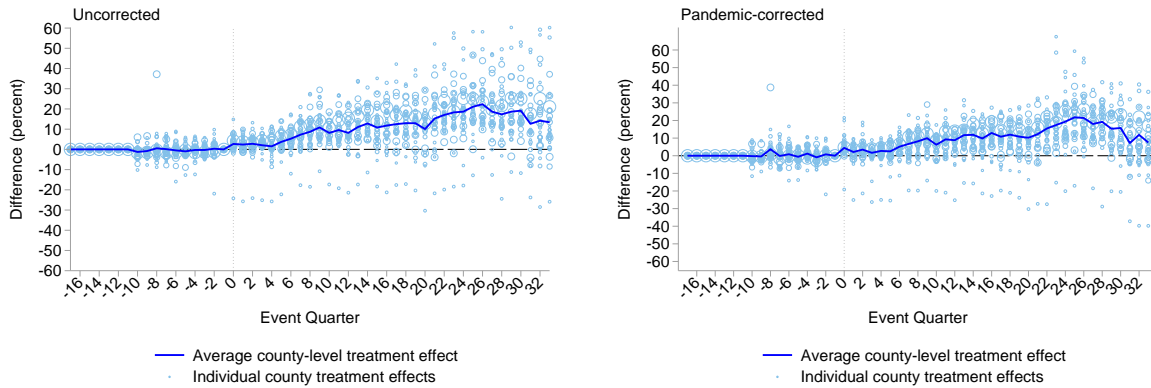
B. Average Effects in Treated Counties vs Sample Placebo Average Effects



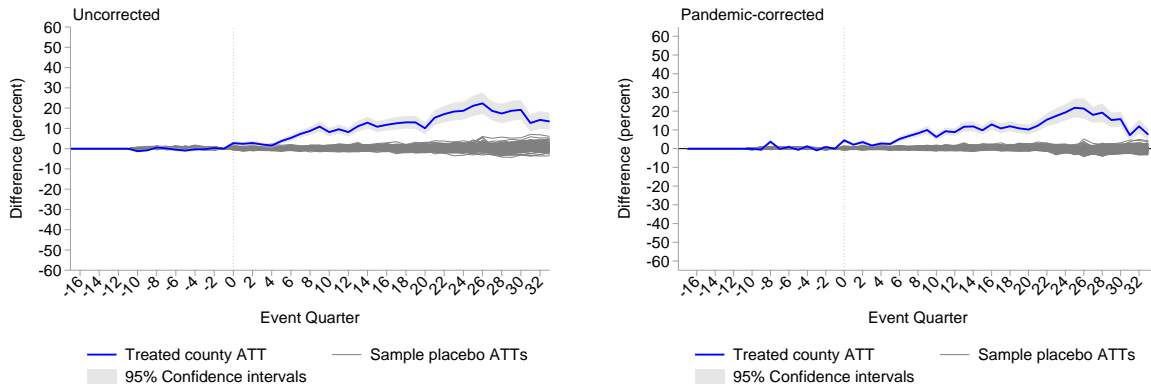
*Note:* Estimated using employment and payroll data from the QCEW and local unemployment data from LAUS. We have a total of 22 treated counties: 16 in California, plus 6 in New York. All treated counties have  $\geq 5,000$  employment in NAICS 722. The donor pool consists of the 122 counties with  $\geq 5,000$  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



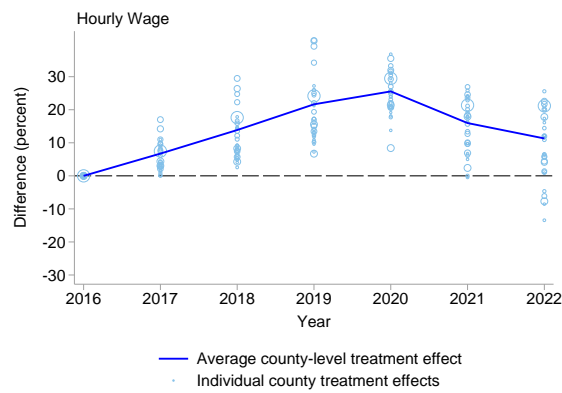
B. Average Effects in Treated Counties vs Sample Placebo Average Effects



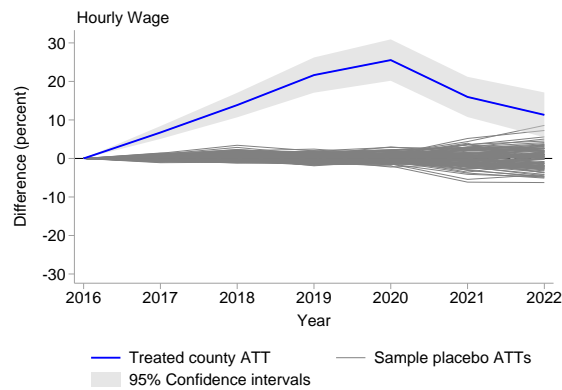
*Note:* Estimated using employment and payroll data from the QCEW, and local unemployment data from LAUS. McDonalds sub-sample includes 31 treated counties: 21 counties in California, plus 10 counties in New York. The treated counties all have  $\geq 5,000$  employment in NAICS 722. The donor pool consists 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 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 Ashenfelter and Jurajda (2020). 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.<sup>39</sup> Comparisons of Figure B.2 to Figure 7 emphasize why using these time-spent measures is superior to using other potential candidates like Covid cases or deaths. Figure B.2 shows 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 7 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;

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<sup>39</sup>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.<sup>40</sup>

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.<sup>41</sup> 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

<sup>40</sup>Full-service restaurant employment declined much more steeply than fast food employment during the pandemic.

<sup>41</sup>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.

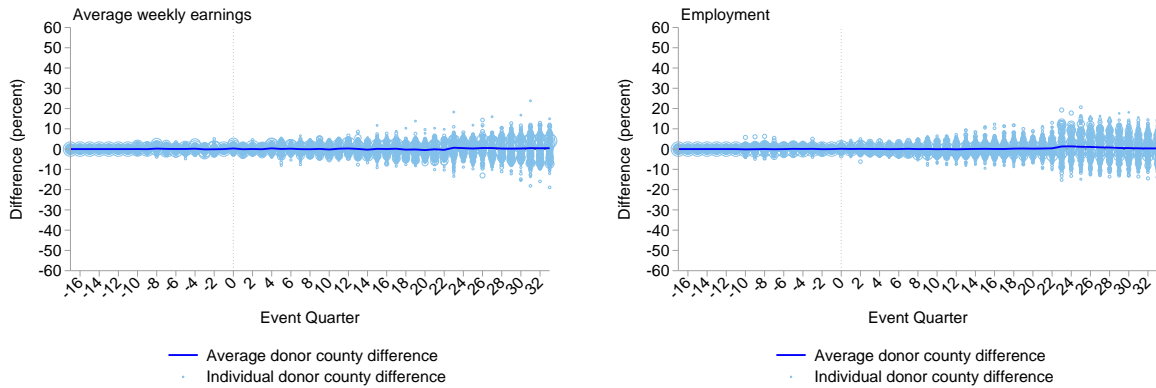
TABLE B.1  
Average Employment Effects Corrected by Local Pandemic-response Measure

	<i>Local Pandemic-response Measure</i>			
	None	Pandemic-response Index	Time in Retail & Restaurants	Time in Workplace
<i>All Treated Counties</i>				
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 <i>p</i> -value	0.12	0.08	0.01	0.45

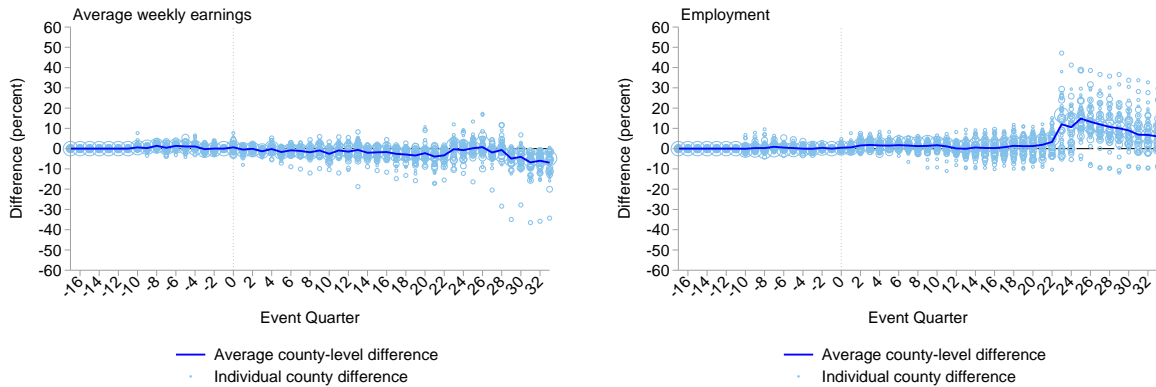
*Note:* Estimated using employment and payroll data from the QCEW, local unemployment data from LAUS, and Google Mobility data from Chetty et al. (2020). For the sample of fast-food workers, we have a total of 36 treated counties: 25 in California, plus 11 in New York. All treated counties have  $\geq 5,000$  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

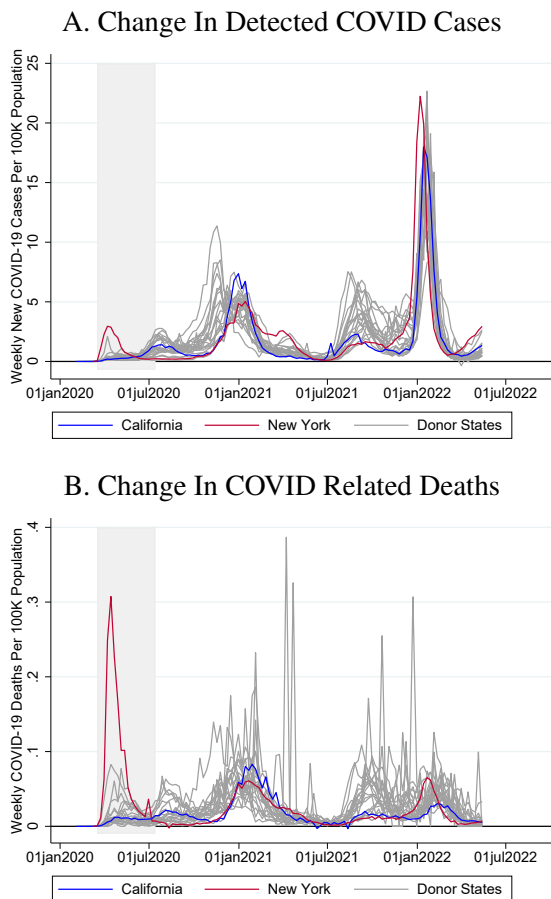


B. Average and Individual *Treated* Counties



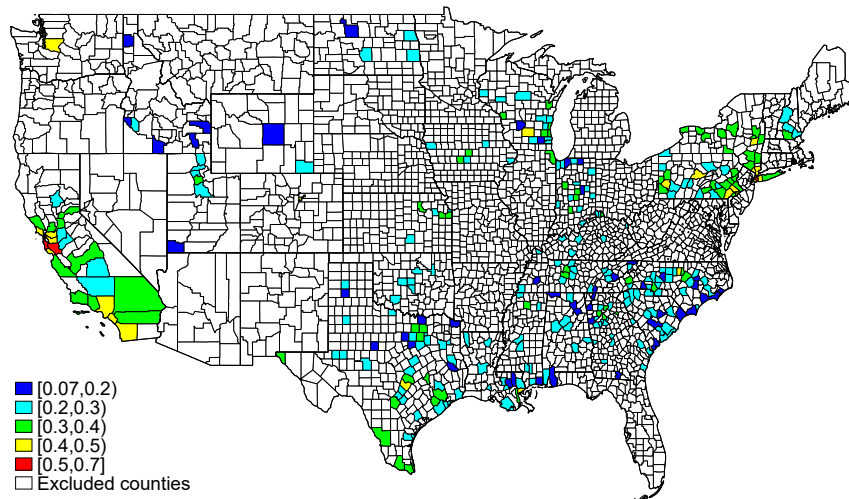
*Note:* Estimated using employment and payroll data from the QCEW, local unemployment data from LAUS, and Google Mobility data from Chetty et al. (2020). We have a total of 36 treated counties: 25 in California, plus 11 in New York. All treated counties have  $\geq 5,000$  employment in NAICS 722. The 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



*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.

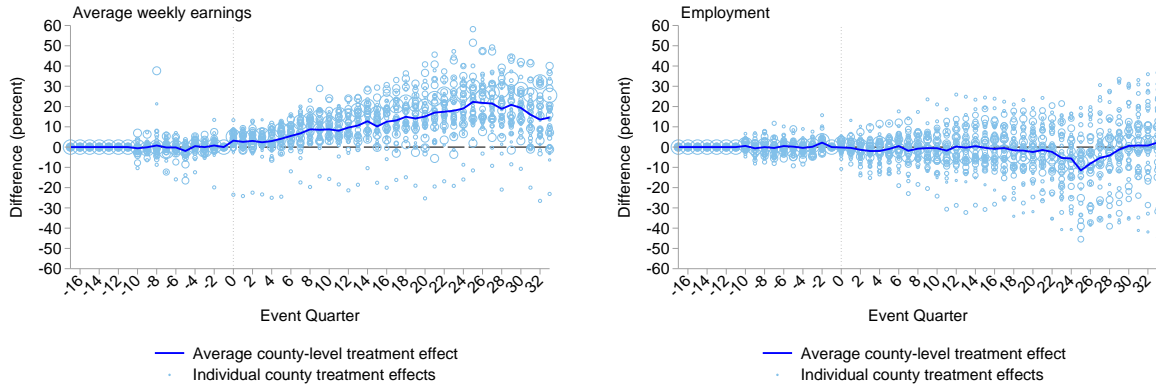
FIGURE B.3  
Pandemic Index by County



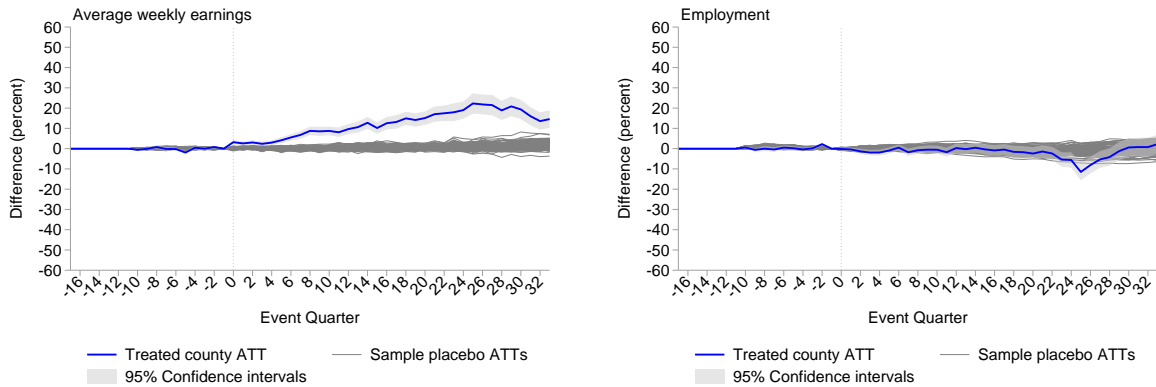
*Source:* Data on time spent in locations comes from Chetty et al. (2020), which is available by state, county, and city. The pandemic index is described in Section 2.3. A higher value of the index entails a greater impact. Among donor counties, the index has a mean of 0.26 and a standard deviation of 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.<sup>42</sup> 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.

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<sup>42</sup>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.<sup>43</sup> 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 3. 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 - \$0.99 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.<sup>44</sup>

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).

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<sup>43</sup>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.

<sup>44</sup>We weight the contributions from each minimum wage increase by the percentage change in the minimum wage with the increase represented.

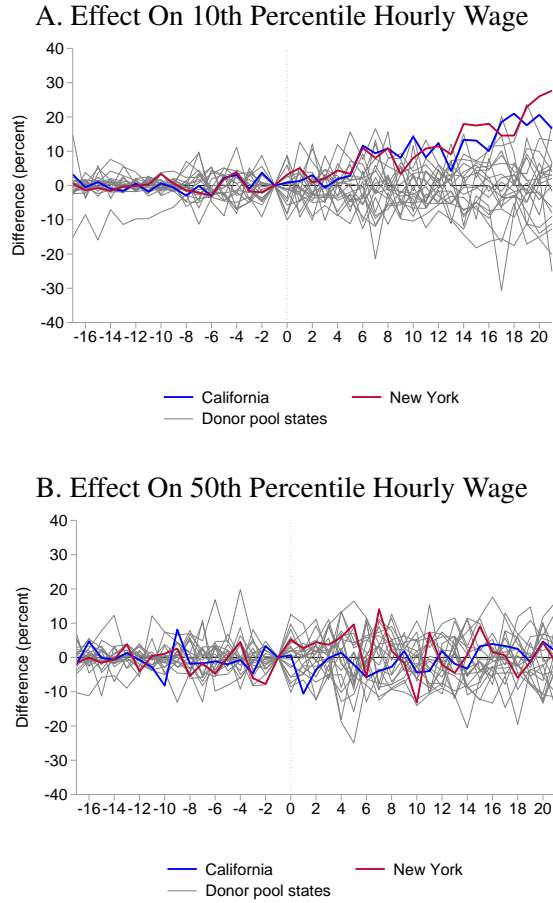
TABLE C.1  
Contributing Elements to Relative Wage Bin \$0–\$0.99

Relative Wage Bin (RWB)	Wage Bin (WB)	Contributing Quarters
<i>Pre-COVID period</i>		
\$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
<i>COVID period</i>		
\$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).

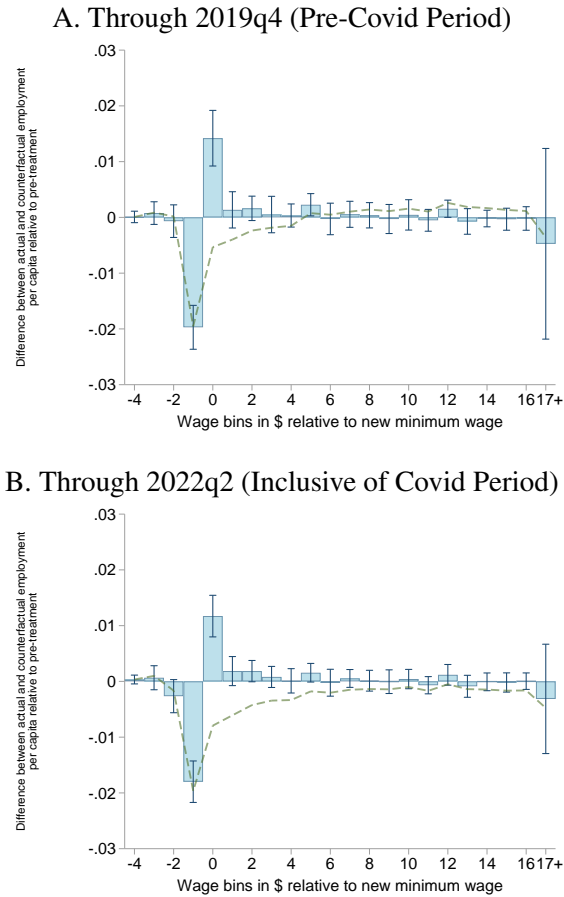
Figures

FIGURE C.1  
Average Effects on 10th and 50th Percentile Hourly Wage For All Workers Through 2019



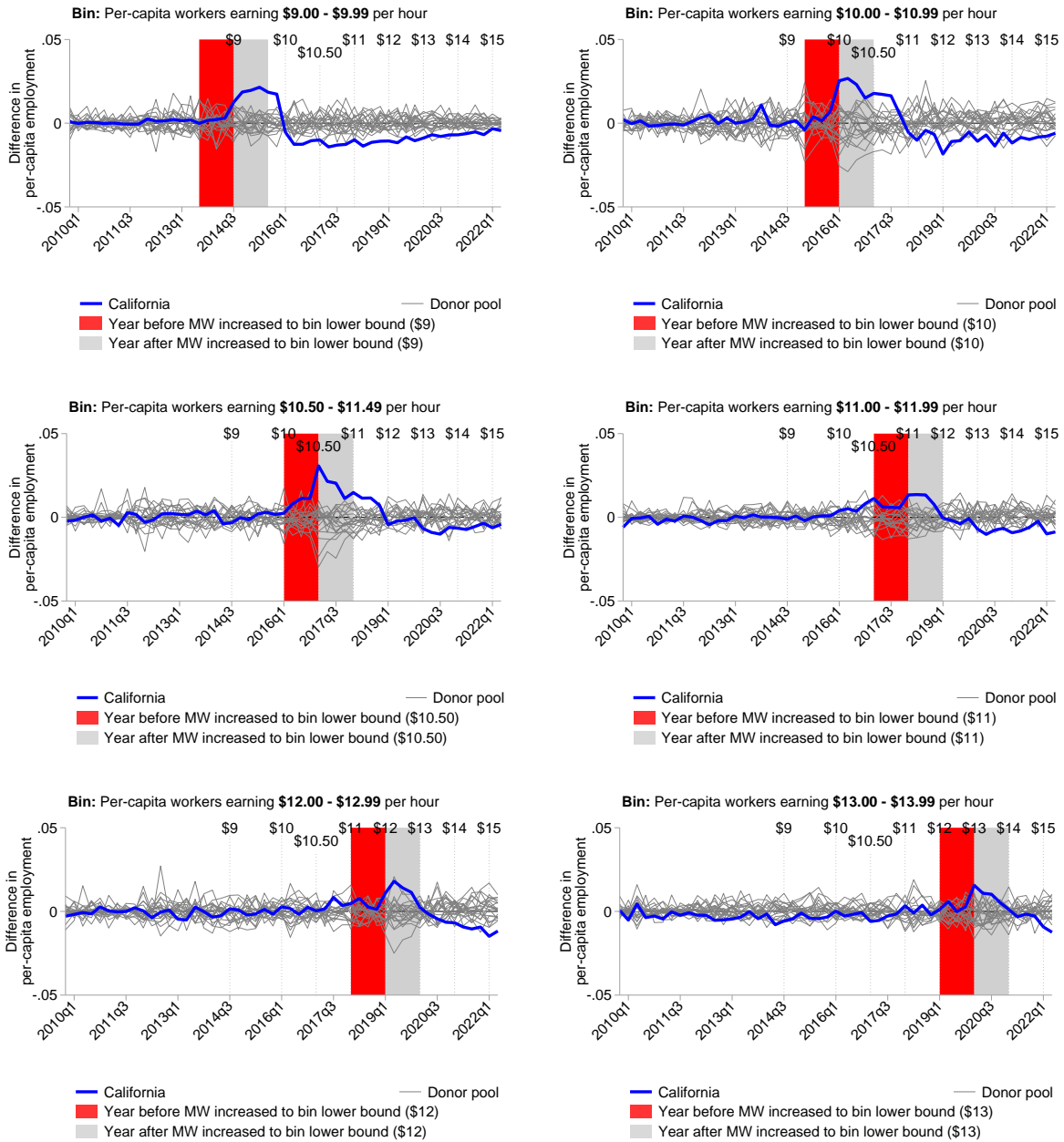
*Note:* Estimated using employment and earnings data on all workers in the CPS and local unemployment data from LAUS. The donor pool consists of 20 untreated/control states for the period ending in event quarter 21. The top and bottom panels' the y-axis shows the estimated difference in each quarter 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



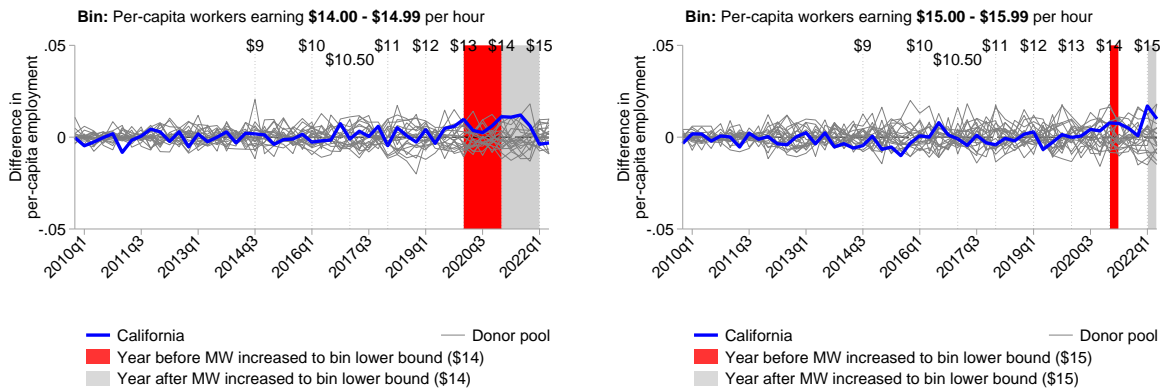
*Notes:* Effect on the share of total employment in each wage bin in the year following California’s minimum wage increases, for the pre-Covid period indicated in Table C.1 (through 2019q4), and for the entire period inclusive of the Covid period (through 2022q2). For each wage bin, we first use synthetic 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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