Minimum Wage Effects and Monopsony Explanations*

JUSTIN C. WILTSHIRE
University of Victoria

CARL MCPHERSON
University of California, Berkeley

MICHAEL REICH
University of California, Berkeley

DENIS SOSINSKIY
University of California, Davis

Abstract

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.

*We are grateful to the Center on Wage and Employment Dynamics at UC Berkeley for research support, to Orley Ashenfelter and Stepan Jurajda for sharing their McDonald's data with us, and for helpful suggestions and comments from Michael Amior, David Autor, Eli Ben-Michael, Charles Brown, David Card, Gabriel Chodorow-Reich, Christina Chung, Arindrajit Dube, Guido Imbens, Ken Jacobs, Patrick Kline, Attila Lindner, Laurel Lucia, James Parrott, Steven Raphael, Jesse Rothstein, Geoff Schnorr, Anna Stansbury, David Weil, Jesse Wursten and participants in the UC Berkeley IRLE seminar, the WCEG Researchers Conference, the Berkeley Labor Lunch, LERA@ASSA 2023, the UVic Economics Brown Bag, CEA 2023, WEAI 2023, AEA 2024, and SoLE 2024. Funding for this research came from the UC Berkeley Institute for Research on Labor and Employment, including the dissertation fellowship (McPherson).

Email: wiltshire@uvic.ca, carl.mcpherson@berkeley.edu, mreich@econ.berkeley.edu, dsosinskiy@ucdavis.edu

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 2021b 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 (Manning, 2021b), or that pass-throughs to prices offset increased labor costs (Cooper, Luengo-Prado, and Parker, 2020). 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 adjudicate 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 and ensuring that 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 examine minimum wage effects on the fast-food industry in the 36 most populous counties of California and New York. Importantly, 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.[1]

Our primary estimation uses a stacked county-level synthetic control strategy (Wiltshire, 2024a,b). 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); Callaway and Sant'Anna (2021) and others, synthetic control estimates are not biased by staggered treatment adoption when treatment effects are heterogeneous, as is often the case with

[1] New York and California differ in their treatment of tip credits for servers in full-service restaurants.

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 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). Our analyses of other low-wage industries and of all workers in California (where the minimum wage was binding for all workers) also yield no evidence of negative employment effects. As part of these supplementary analyses, in Appendix C we present estimates from a synthetic-control equivalent of the wage-bin-by-bin approach 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. The point estimates remain statistically non-significant and do not suggest that our null employment 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. Other non-monopsony adjustment mechanisms, such as reductions in job amenities, are not likely to explain our results (Dube, Naidu, and Reich, 2022; Tong, 2024). A reduction in monopsonistic rents constitutes the 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 tight labor markets in recent years increased the elasticity of labor supply (Autor, Dube, and McGrew, 2023), flattening the labor supply schedule. Using a 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 findings are consistent with monopsony models based on search and matching frictions, but less so with spatial models because of the absence of substantial variation in spatial concentration in fast food.

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.

We discuss the policy environment in Section 2, our data in 3, and our methodological approach in Section 4. We present results on employment and earnings in Section 5, and adjustment 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 predicted employment effects under monopsony when labor supply and demand schedules change. 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.[2] 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.[3]

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, and in response 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.[4] 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 A.2 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.[5] Figure 1 also shows that the distribution of aver-

[2] 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.

[3] 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.

[4] As we show in Table A.2, 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.

[5] 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.

age 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.[6]

To further examine the representativeness of these counties, we display the share of all workers in fast food in each of the counties in Figure A.2, 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 a state EITC program, expanded Medicaid and greater coverage under the Affordable Care Act (ACA). Reich (2024) demonstrates that these changes were too small to confound our results. The paper also reviews how California increased its efforts to enforce wage and hours law and notes that the U.S. Department of Labor similarly increased enforcement of wage and hour laws in other states, especially those with especially weak enforcement agencies. It then examines the effects of the large federal programs instituted in 2020 and 2021 to stimulate recovery from the pandemic: three rounds of "stimulus checks," sent to most households, enhancements in unemployment insurance, food stamps, a temporary dependent care tax credit, aid to businesses (such as the Paycheck Protection Program) and to state and local government. As Reich (2024) details, these policies do not pose significant confounds for identifying the effects of minimum wage policies in California and New York.

2.3. *Exposure of Groups and Areas to the Minimum Wage Policies*

We provide here further indications of the substantial scope of the minimum wage policies, by deploying 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), for a high-wage local labor market (San Francisco), and for restaurant workers.[7]

[6] Figure 1, which used county-level QCEW average wages in all industries, is remarkably similar to the distribution of average QCEW county-level *fast food* wages in Figure A.1.

[7] Unlike the state wide minimum wage increases in California, the New York State minimum wage increase to $15 covered only fast food workers. Also, unlike California, New York allows a substantial tip credit for full service restaurant workers. . We omit New York from the figure because the CPS does not allow restricting to fast food workers.

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 around 45 percent from 2010-2013 to 58 percent in 2022.[8] 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.[9] 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 (Godoey 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 (Godoey 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 a highly exposed subgroup: 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.[10] 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.3 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.

Finally, we note that the two exposure measures in Figure 2 remained relatively stable during the treatment period. This stability is not surprising. In our bin-by-bin wage analyses, presented in Online Appendix C, each dollar increase in the minimum wage moved the mass of low-wage jobs

[8]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

[9]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.

[10]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.

one dollar higher. As a result, the proportion of workers earning less than the next minimum wage did not change over time.

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).[11] Our treated areas comprise 25 counties in California and 11 in New York, which in 2013 employed 94 percent of fast food workers in California and 67 percent in New York. Our donor pool includes 122 populous counties in states where the federal minimum wage has been binding since 2009. These include major urban centers such as Harris County, Texas (Houston), Davidson, Tennessee (Nashville) and Salt Lake County, Utah, as well as many smaller cities that meet the same threshold of at least 5,000 restaurant workers.[12]

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. We consider there the distributional effects of these policies on all workers. 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.[13] The second restriction excludes the high-income outliers in our sample: nine San Francisco Bay Area counties and nine New York City metro counties.[14]

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.

[11] See Section 4 for further details on synthetic controls.

[12] Table A.3 lists all the donor counties.

[13] 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.

[14] 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.

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.[15] 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.[16] For our restaurant-focused analysis, we restrict the QCEW data to private sector workers in the California restaurant industry (NAICS 722).

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.[17]

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.

[15] Several counties in the Bay Area and New York City reached a minimum wage higher than $15 by event quarter 33.
[16] Prior to 2012, the equivalent code is 722211.
[17] 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.

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 every 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 shock.[18] 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.[19] The authors generously sharing with us the county-level averages of these variables for all counties with at least five McDonald's restaurants, which we then restricted to the counties in our treated and donor pool samples.[20]

[18] Google does not provide disaggregated data for fast food restaurants. Since the severity of the shock is highly correlated over time, our index is not sensitive to the choice of particular weeks.

[19] See Ashenfelter and Jurajda (2020) for further details.

[20] Our sample for analysis of McDonald's restaurants consist of 31 treated counties in California and New York and 95 donor counties.

4. Methodology

4.1. Research Design and Estimating Strategy

The legislative phase-ins 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 begins with 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, 2024a,b). Like the new difference-in-difference methods of Cengiz et al. (2019); De Chaisemartin and d'Haultfoeuille (2020); Callaway and Sant'Anna (2021); Sun and Abraham (2021); and Borusyak, Jaravel, and Spiess (2022), 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 trends assumptions (Abadie, 2021; Abadie, Diamond, and Hainmueller, 2015).

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.[21] 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 (Peri, Rury, and Wiltshire, 2024; Wiltshire, 2024b) 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.

[21] Table A.4 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.

4.2. Stacked Synthetic Control Estimator

For a given outcome of interest, our synthetic control estimator selects weights that best match an individual treated county to a subset of untreated "donor pool" counties according to specified dimensions ("predictors") from the pre-treatment period. For each of our synthetic control estimates the predictors include, from each quarter between 2009q4 and 2011q4, the values of the outcome variable and of total employment (both normalized to the final pre-treatment quarter), the average values of those variables over the same period, and the average unemployment rate over the period.[22] 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 general assumptions and with a good pre-treatment "fit" between the treated unit and its synthetic control, the difference between the two dynamic outcome paths yields the estimated treatment effects. We include no predictors between 2012q1 and the end of the pre-treatment period—this approach admits a test for the quality of the synthetic control matches prior to treatment: a good synthetic control will have a good pre-treatment fit despite not being matched on predictors in the two-ish years before treatment begins.

For inference, we present two sets of statistics. The first is the classic, and most widely-used approach, developed in Abadie, Diamond, and Hainmueller (2010, 2015). This approach generates p-values based on the distributions of the ratios of the (root) mean squared prediction error (MSPE) 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 homoskedasticity across units and asymptotic normality of the estimand.[23]

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, 2024a) and a companion package released with this paper that facilitates the pandemic-response bias correction procedure we detail below: `stackscpvals`.

[22] Raw fast-food wage and employment data for our treated counties and the donor pool counties show that wages and employment grew at about the same rate in both sets of counties in the pre-treatment period and somewhat faster in the treated counties in the treated period (results available upon request).

[23] In cases with many (say, M) treated units, each placebo average will comprise 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.

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 $i = 1,...,I$ are treated in calendar time $t = T_{0i}+1 \leq T$ (which can vary over i), and units $j = I+1,...,I+J$ are the subset of untreated units which comprise our donor pool (let $T_{0j} = T$). Let them collectively be indexed by $z = 1,...,I,I+1,...,I+J$. 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.[24] 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 $X_z = (X_{1,z},...,X_{k,z})'$ contains the values of these predictors for z, and the $k \times J$ matrix $\mathbf{X}_0 = [X_{I+1},...,X_{I+J}]$ 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:

$$\tau_{zt} = Y_{zt}^{Int} - Y_{zt}^N \tag{1}$$

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 τ_{it}. The synthetic control estimator for Y_{it}^N is:

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

We follow Abadie, Diamond, and Hainmueller (2010) and impose restrictions on the weights that help justify interpreting the estimated synthetic controls as valid counterfactual estimates. Specifically, given a set of weights $v_1^i,...,v_k^i$ that determine the relative importance of the k predictors ,[25] the synthetic control $\hat{\mathbf{W}}^i = (\hat{w}_{I+1}^i ... \hat{w}_{I+J}^i)'$ is selected that minimizes the distance between i and its donor pool units:

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

subject to $\sum_{j=I+1}^{I+J} w_j^i = 1$ and $w_j^i \geq 0 \; \forall \; j \in \{I+1,...,I+J\}$, 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).

[24] 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 Doudchenko and Imbens (2016) (see, also, Ferman and Pinto 2021), while also allowing estimation of effects in percentage changes.

[25] We use the regression-based method (Kaul et al., 2022) to select the v_h^i weights.

$\hat{\tau}_{it} = Y_{it} - \hat{Y}^N_{it} \; \forall \; \{i,t\}$ 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, $A\hat{T}T_e$, is:

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

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

Taking the above as the "classic" stacked synthetic control estimator, we then implement the synthetic control bias-correction procedure proposed by Abadie 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 $\hat{\mu}^i_{0t}(x)$, 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, $\tilde{Y}_{zt} = Y_{zt} - \hat{\mu}^i_{0t}(X_z)$. Third, we apply the estimated $\hat{\mathbf{W}}^i$ to the $\tilde{Y}_{jt} = Y_{jt} - \hat{\mu}^i_{0t}(X_j)$ to calculate $\hat{\tilde{Y}}^N_{it} = \sum_{j=I+1}^{I+J} \hat{w}^i_j \tilde{Y}_{jt}$, which admits the bias-corrected synthetic control estimated gaps for each $\{i,t\}$, $\hat{\tau}_{BC_{it}} = \sum_{i=1}^{I} \gamma_i (\tilde{Y}_{it} - \hat{\tilde{Y}}^N_{it})$. 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), in Section 7 we refer to these bias-corrected estimates as "uncorrected" estimates.

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 the pandemic and the heterogeneous responses to it. Recent research has highlighted the different 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, while 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 counties. None of our donor pool counties 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 $\tilde{X}_z = (X_{1,z}, ..., X_{k,z}, c_z)'$, 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, $\tilde{R}_{zt} = Y_{zt} - \hat{\mu}^i_{0t}(\tilde{X}_z)$. Third, we apply $\hat{\mathbf{W}}^i$ to the $\tilde{R}_{jt}$ to calculate $\tilde{\hat{R}}^N_{it} = \sum_{j=I+1}^{I+J} \hat{w}^i_j \tilde{R}_{jt}$, 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:

$$\tilde{\tilde{\tau}}_{BC_e} = \sum_{i=1}^{I} \gamma_i \tilde{\tau}_{BC_{ie}} = \sum_{i=1}^{I} \gamma_i (\tilde{R}_{ie} - \tilde{\hat{R}}^N_{ie}) \tag{5}$$

The resultant $\tilde{\tilde{\tau}}_{BC_e}$ 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 $E[\tilde{R}_{zt}] = E[\tilde{Y}_{zt}]$ 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 Callaway and Sant'Anna (2021) estimator 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 DiD and conduct a series of robustness tests.

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 1000 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.[26] This

[26]Tables A.5 and A.6 present balance tables for the predictors. They show excellent balances for each outcome for all predictors, with the minor exception of the average unemployment rate during 2009q4-2011q4. We do not view this exception as problematic: unemployment rates became comparable across treated and donor counties well before treatment, and then changed at similar rates in the treated and donor counties thereafter. Furthermore, our estimating strategy addresses this potential issue directly by correcting results for bias from pairwise matching discrepancies among

result is not mechanical, as we select our synthetic controls using predictors from only 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 1 quantifies these estimated effects in event quarter 21. Average earnings increased by 19 percent; the placebo-variance-based 95 percent confidence intervals rule out an earnings elasticity with respect to the minimum wage below 0.28. 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 under 0.01 indicates the earnings estimate is highly significant.

In contrast, our employment results are statistically non-significant. The confidence interval means that we cannot rule out employment elasticities from negative 0.07 to positive 0.03. This lower-bound is higher than that of Cengiz et al. (2019). It indicates that the estimates of no disemployment effects found in many recent studies remain even with the larger minimum wage increases we focus on.

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

If minimum wage policies are endogenous to employment outcomes, estimated employment effects may differ from those that would ensue if the minimum wage policies were applied to a broader population. 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 some minimum wage 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 some 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.

predictor values (Abadie, 2021). This one discrepancy thus should not confound our estimates.

Our sample also includes both high-wage and low-wage counties. (Figure A.3 displays 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 Tables A.1 and A.2 of the Online Appendix.[27] We present our results in Panel B of Table 1.

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 1 and Figure A.4 of the Online Appendix.

In both panels B and C, the employment estimates become more *positive*, but remain non-significant. 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 1 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.7 displays our estimated earnings and employment effects by quartile and by the presence of local minimum wage policies. When excluding counties with local minimum wages, the earnings effect estimates decline monotonically from the bottom to the top quartiles and are not significant for the top quartile, all as we would expect. In contrast, for the sample of all counties, the earnings effect estimates are not monotonic and the top quartile estimate is largest and statistically significant, which may indicate the full-sample estimates are influenced by including those high-income counties with local minimum wages. The results confirm that minimum wage effects on earnings are greater in counties with lower average earnings, while employment effects are not.

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 2 we present average county-level employment estimates both with the bias-correction procedure and without

[27]The counties with no local minimum wages accounted for 45 percent of fast food employment in all 36 counties in 2013.

("classic").[28] The classic 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.

In Panel C of of Table 2 we present estimates from the Callaway and Sant'Anna (2021) dynamic difference-in-differences estimator. 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 again inconsistent with minimum wages having more deleterious employment effects in low-wage counties.

Table A.8 presents earnings effect estimates for each of the estimators described above (where the results in Panel A simply restate our primary results from Panels A and B of Column 1 in Table 1). We obtain positive elasticities between 0.27 and 0.35 for each estimator, demonstrating the robustness of our large, positive estimated earnings effects.

In Table A.9 we present results from varying the weighting scheme used to average the county-level estimates. Alongside results using our preferred 2010 population-based weighting scheme, we also present results using unit weights and using the pre-treatment fast-food shares of local employment. While several point estimates are slightly lower using the alternative weights, the elasticities remain remarkably similar for the earnings estimates and all producing zero employment effect estimates.[29]

We also check whether our results are generalizable to smaller counties or hold only in larger labor markets.[30] We consider a set of 11 treated counties in New York with between 2,000 and 5,000 restaurant employees. Including these counties brings total fast food employment coverage in New York to 81% of the total. The donor pool for these smaller counties is composed of 149 similarly sized counties that have not received a minimum wage increase since the most recent federal increase in 2009q3. The results, presented in Table A.10, are nearly identical to our large-county results, especially those from the subsample with no local minimum wages. The minimum wage increases caused mid-sized county fast food earnings to increase similarly to the large counties by the end of 2019, while employment remained unchanged.[31]

Lastly, we consider the effects of minimum wages across the wage distribution and on workers in other industries. We first demonstrate that the minimum wage increases sharply reduced the

[28] See Wiltshire (2024a) for further discussion on "classic" synthetic controls vs bias-corrected synthetic controls.

[29] Minimum wage growth is calculated using the same weights as the stacking procedure when calculating elasticities.

[30] Recall that our primary sample is composed of the 36 large counties in California and New York that have at least 5,000 restaurant employees, covering 94 percent and 67 percent of statewide fast food employment in California and New York, respectively.

[31] 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.

50-10 wage gap with no change in 50th percentile wages (Appendix Figure C.1), indicating these minimum wage increases positively impacted lower-wage workers without generating spurious or negative effects elsewhere in the distribution. These conclusions are reinforced by the results of our synthetic control version of the bin-by-bin approach from Cengiz et al. (2019), which shows a large movement of workers from just below to just above the new minimum wages (shown separately for each increase and also when aggregated), with positive spillovers on employment paid up to $5 above the new minimum wages and no impact on employment in the upper tail of the wage distribution (Appendix Figure C.2).

Finally, we consider impacts beyond the fast food industry using our stacked synthetic control and DiD estimating strategies. For full-service restaurants, we find similar results to our estimated effects on fast food workers. For retail trade and accommodation industries, and indeed for all workers as a whole, results indicate zero employment and earnings effects (Tables C.1 and C.2). We discuss these analyses and results in greater detail in Appendix C.

6. Adjustment Mechanisms and Evidence of Monopsony

In this section, we consider why our employment results through 2019 are so muted, focusing on reductions in employee turnover that reduce turnover costs, and on cost pass-throughs to prices. Using the Quarterly Workforce Indicators data, we find that minimum wages reduced employee separation rates, consistent with monopsony models. Reduced separation rates, in turn, imply savings in turnover costs. Using data from a national survey, we find that McDonald's restaurants passed on roughly half of their increased wage bill to consumers as higher prices. These two adjustment mechanisms leave room for a third: reduced monopsonistic rents. We also consider a number of non-monoposony adjustment mechanisms and conclude that they do not provide plausible explanations of our results.

6.1. Effects on Employee Separation Rates

We test for monopsony in our setting by considering changes in separation rates. As Manning (2021a) showed, wage increases would reduce employee turnover in a monopsonistic labor market, but not in a competitive one. Manning's search model is particularly apt for fast food because its annual employee turnover rates exceed 100 percent (Bebe, 2016).

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 through state unemployment insurance reporting systems. Unlike the QCEW, the QWI also 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 NAICS level, we examine minimum wage effects on all restaurant workers, rather than just fast-food workers.[32] County-industry separation rates can be highly

[32]The QCEW and the QWI use slightly different algorithms for data fuzzing and suppression. As a result, our QWI

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 (2024). We then proceed using the same stacked county-level synthetic control estimating strategy that we used to estimate our primary results (in Section 5).

Panels C and D of Table 3 present our estimated effects of these minimum wage policies on the separation rates of restaurant workers.[33] We find 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 13.12 and 35.78 percent, respectively, in the final pre-pandemic quarter.[34] These 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.

6.2. 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.[35] They did not collect any data on employment.

McDonald's dominates the burger chain segment of fast-food, with over 14,000 restaurants—most of them franchises—in the U.S. The chain primarily competes on price rather than quality, and enjoys higher profit margins than its fast-food competitors.[36] 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.[37]

---

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.

[33] As Figure A.5 shows, we obtain close pre-policy fits for the separation rate outcome.

[34] In 2013, separation rates in the donor counties equaled 20 percent for all workers and 11 percent for low-tenure workers. In other words, the declines we estimate represent a roughly 2.6 percentage point drop for all workers and a 3.9 percentage point drop for low-tenure workers in treated counties.

[35] We are grateful to Orley Ashenfelter and Stepan Jurajda for sharing their updated dataset with us.

[36] Cost pass-throughs might be higher for other fast food chains; however, we are not aware of any available data on this question.

[37] 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.

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 treated large counties in California and New York and 95 of our donor counties. This subsample is sufficient to identify plausible counterfactuals. Indeed, our earnings effect estimates are unchanged when we estimate them using only the subsample of treated and donor counties observed in the AJ data (Figure A.6).

1. McDonald's Hourly Wages Hourly wages increased on average about 22 percent in our 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 panels A-B of Table 3 and Figure A.7. 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 1) 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. Table 3 shows that prices in the treated counties did increase about 4 percent faster than in the donors from 2016 to 2019. 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 3, 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.[38] The combination of incomplete pass-through to prices and no disemployment effect calls for further explanation. We have already shown that separation rates fell, a response that points to the presence of monopsony power. A reduction in rents, therefore, is a likely margin of adjustment.

6.3. Alternative Adjustment Mechanisms

Our separation rate and employment results rule out the most common non-monopsony explanations for our results.[39]

[38] A labor share of 0.3 approximates the ratio of fast food payroll to revenue in the 2017 and 2022 Economic Censuses.

[39] Reduction in turnover, for instance, may save some costs. However, our estimated turnover declines are unlikely to offset all of the labor cost increase. If turnover represents 15 percent of labor costs (as in Dube, Lester, and Reich (2016)), and separations decline 13 percent, then labor costs decline by 1.95 percent. Accounting for price increases still

A perfectly competitive labor market cannot explain a simultaneous decline in separation rates and no disemployment effects from higher minimum wages. Alternatively, post-treatment shocks may have coincided with our minimum wage increases. We discuss numerous such confounds in Section 2, all of which are demonstrated to have been too insubstantial to affect our results (Reich, 2024). Moreover, for our results through 2019, any shocks would need to have increased labor demand or supply without changing employment. We are not aware of any shocks that meet this criterion.[40]

Alternatively, in a competitive model the return to capital declines when wages increase. In such a model, however, this would lead to less investment and reduced employment, which we do not find.

Finally, as Sorkin (2015) and Manning (2021a) have argued, employers could absorb minimum wage cost increases by substituting capital for unskilled labor, or they could make employees work harder or otherwise reduce non-wage job amenities. However, Ashenfelter and Jurajda (2020) do not detect effects on automation. Moreover, decreases in non-wage amenities should generate an *increase* in separation rates—the opposite of what we find. Additionally, recent work shows no impact of minimum wages on non-wage amenities: Dube, Naidu, and Reich (2022) examine the impact of Walmart's voluntary corporate minimum wage policies, while Tong (2024) uses the rich information on amenities in Glassdoor data to examine the impact of state-mandated minimum wage policies. Neither finds evidence that higher minimum wages lead to reduced non-wage amenities.

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.[41] Panel A of Figure 4 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).

leaves over 40 percent of increased labor costs unexplained.

[40] MaCurdy (2015) offers a simulation suggesting that minimum wage increases can raise demand for fast food. However, causal studies of the actual spending effects of minimum wages (Aaronson, Agarwal, and French (2012); Cooper, Luengo-Prado, and Parker (2020)) indicate that such effects are very small, if not undetectable, and not enough to pose a confound to our estimates.

[41] We focus on our employment, earnings, and separation results in this section. The pandemic correction procedure we describe here requires observations of the pre-treatment outcome values (a subset of the synthetic control predictors) for use as covariates in period-specific regressions used to estimate the impact of the pandemic response on the outcome of interest. As the McDonald's data are only available from 2016 onward, we do not have observations of pre-treatment McDonald's outcome values. We thus cannot apply the pandemic correction procedure to the McDonald's data.

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.[42]

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 4. This figures 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 5, 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.

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 5.

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

[42] California lifted its restaurant capacity restrictions fully on June 15, 2021.

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 4. 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 4. Among all treated counties, one year after reaching $15 per hour, these minimum wage policies increased employment in treated counties by 6.8 percent. The corresponding employment elasticity is 0.08, with placebo-variance confidence intervals from 0.03 to 0.12 and a RMSPE p-value of 0.06. These estimates increase to 12.87 percent ($\varepsilon = 0.15$, RMSPE $p = 0.03$) and 10.85 percent ($\varepsilon = 0.12$, RMSPE $p = 0.05$) in Panels B and C, for our non-local sample and our lower-income sample, respectively.

The results of this exercise lead to 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” [for pandemic-response bias] 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 under Changing Labor Supply and Demand Schedules

We discuss here the predictions of a monopsony model when labor supply and labor demand schedules change and argue that our empirical results line up with these predictions.

1. Changes in Labor Supply

In the Manning dynamic monopsony model, the elasticity of labor supply equals twice (the absolute value of) the separation rate elasticity. Labor market data on job-to-job changes indicate that the elasticity of separation rates increased after the pandemic, when labor markets tightened (Autor, Dube, and McGrew, 2023).[43]

Thus the pre-pandemic labor market is characterized by the "steep labor supply" equilibrium depicted in the left panel of Figure 6, in which we denote the competitive wage by w^*, the monopsony wage by w^M, and the minimum wage by w^1. Thus, we observe the employment level L^1 in our analysis. In this situation, the minimum wage would result in a large wage markdown, but a relatively small employment markdown (potentially too small to detect). In contrast, the post-pandemic labor market is characterized by the "flat labor supply" equilibrium in the right panel of Figure 6. We show in Appendix A.2 that the employment markdown can be expressed as the wage markdown divided by the slope of the labor supply schedule. Hence a flatter schedule (smaller slope) implies a greater employment markdown. We illustrate this outcome in the right panel of Figure 6. The flatter labor supply schedule that developed with the tight labor markets after the pandemic predicts smaller wage effects and larger positive employment effects than was the case before the pandemic. These are exactly our empirical results.

We expect larger positive minimum wage employment effects when monopsony power is lower. In a monopsonistic labor market, low wages and employment markdowns result from the rising marginal cost of hiring an additional worker, relative to the constant marginal cost of hiring an additional worker in a competitive labor market. As a result, these low-wage firms experience high levels of employee turnover and high job vacancy rates. Minimum wage increases relax these firms' labor supply constraints, allowing them to raise their employment levels. The employment increase will be greater when the elasticity of labor supply is greater—that is, when monopsony power is lower. This understanding that labor supply constrains employment contrasts with the common, but incorrect, intuition that higher wages will always lead employers to want to hire fewer workers.

Labor supply constraints to an entire low-wage industry also become relaxed in a tighter labor market, which particularly increases pay in low wage industries. As a result, such industries can attract more workers, both from the greater flow of workers into labor markets and from employed workers who become more willing to move from their existing jobs. In other words, when low-wage labor markets tighten, the elasticity of labor supply to low-wage firms increases. Autor, Dube and McGrew (2024) show that the elasticity of labor supply to low-wage firms approximately

[43] Autor, Dube, and McGrew (2023) estimate a quit elasticity of .663 for 2015 to 2019 and 1.075 for 2021 to 2022. In DLR (2016), Table 3, the estimated separation rate elasticity for 2001 to 2012 is .467. These estimates suggest that minimum wage separation elasticities are higher when the unemployment rate is lower. 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.

doubled with the tighter labor markets of the post-pandemic years.

The combination of a higher minimum wage and tighter labor markets then elicits an even greater increase in the quantity of workers available to, and employed by, low-wage firms.[44]

2. *Changes in Labor Demand*

Our pandemic correction already accounts for any labor demand shifts induced by the pandemic in the absence of minimum wages; and our synthetic control accounts for common trends in demand. Nonetheless, price-pass-throughs raise the value of the marginal product of labor, which implies an outward labor demand shift that is unique to our treated counties. By 2019, McDonald's Big Mac prices had increased about 3 percent in treated counties relative to donor counties. It seems plausible that this outward shift in labor demand continued through the pandemic. Under monopsony without minimum wages, this demand shift would result in larger employment gains.

However, if the minimum wage is binding and below the competitive wage, then employment is determined by the intersection of the minimum wage with the labor supply schedule. The labor demand schedule does not play a role, provided the new minimum wage remains below the competitive level. In other words, for the post-periods before and after the pandemic, changes in employment are solely based on shifts in the minimum wage and labor supply. Demand shocks should not influence changes in observed 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 in the fast-food restaurant industry—even in lower-wage counties or counties that did not choose to increase their minimum wages locally. We find similar results in other low-wage industries and among all low-wage workers in California.

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

[44] The economic context limits the extent of possible positive employment effects. The additional workers arrive from non-employment, from other industries and from other locations. Each of these sources is finite— they cannot expand without limit. Our positive employment effects occurred during the tightest labor market Moreover, in what has been the tightest labor market since the 1990s.

employee separation rates decrease after minimum wage increases, which should not occur in perfectly competitive labor markets. Second, we show that McDonald's passed only 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.

Our research design rules out several non-monopsony explanations of null employment effects that have been raised in the minimum wage literature—that the minimum wage increases had disemployment effects which were too small to detect, that the increases were slow enough to keep the real value constant, and that the analysis was too short-term to allow for labor-reducing capital adjustments. Other non-monopsony interpretations could in principle be consistent with our findings, but in practice appear inconsistent with the totality of evidence that we and others present. For example, a leftward shift of the labor supply schedule could explain our smaller estimated treatment effect on wages after the pandemic. However, such a shift would also imply a reduction in employment, not a zero or positive effect as we find. Minimum wages have also been accused of motivating employers to reduce non-wage amenities in place of reducing employment. However, recent research (Dube, Naidu, and Reich, 2022; Tong, 2024) has found no evidence that this is actually done, even by low-wage employers with monopsony power. Thus our results indicate that a monopsony explanation is necessary to fully account for the common absence of disemployment effects from the minimum wage literature.

We also demonstrate that responses to the pandemic reduced employment more 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 thus develop a straightforward method to ameliorate pandemic-response bias using smartphone location-tracking data. After making this correction, we show that minimum wages continued to have no disemployment effects. Indeed, we estimate positive employment effects after 2020. Lastly, we use a 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.

Data Availability

Code replicating the tables and figures in this article can be found in Wiltshire et al. (2024) in the Harvard Dataverse, https://doi.org/10.7910/DVN/5HN0VT.

References

Aaronson, Daniel, Sumit Agarwal, and Eric French. 2012. "The spending and debt response to minimum wage hikes." American Economic Review 102 (7):3111–3139.

Abadie, Alberto. 2021. "Using Synthetic Controls: Feasibility, Data Requirements, and Methodological Aspects." Journal of Economic Literature 59 (2):391–425.

Abadie, Alberto, Alexis Diamond, and Jens Hainmueller. 2010. "Synthetic Control Methods for Comparative Case Studies: Estimating the Effect of California's Tobacco Control Program." Journal of the American Statistical Association 105 (490):493–505.

———. 2015. "Comparative Politics and the Synthetic Control Method." American Journal of Political Science 59 (2):495–510.

Abadie, Alberto and Jérémy L'Hour. 2021. "A Penalized Synthetic Control Estimator for Disaggregated Data." Journal of the American Statistical Association 116 (536):1817–1834.

Abadie, Alberto and Jaume Vives-i Bastida. 2022. "Synthetic controls in action." Working paper .

Alexander, Diane and Ezra Karger. 2021. "Do Stay-at-Home Orders Cause People to Stay at Home? Effects of Stay-at-Home Orders on Consumer Behavior." Review of Economics and Statistics :1–25.

Arkhangelsky, Dmitry, Susan Athey, David A Hirshberg, Guido W Imbens, and Stefan Wager. 2021. "Synthetic Difference-in-Differences." American Economic Review 111 (12):4088–4118.

Ashenfelter, Orley and S Jurajda. 2020. "How Low Are US Wage Rates? A McWage Comparison"." Unpublished manuscript, Princeton University .

Ashenfelter, Orley and Štěpán Jurajda. 2022. "Minimum Wages, Wages, and Price Pass-Through: The Case of McDonald's Restaurants." Journal of Labor Economics 40 (S1):S179–S201.

Autor, David, Arindrajit Dube, and Annie McGrew. 2023. "The Unexpected Compression: Competition at Work in the Low Wage Labor Market." NBER Working Paper 31010 .

Azar, José, Emiliano Huet-Vaughn, Ioana Marinescu, Bledi Taska, and Till Von Wachter. 2023. "Minimum Wage Employment Effects and Labor Market Concentration." Review of Economics Studies :rdad091.

Bassier, Ihsaan, Arindrajit Dube, and Suresh Naidu. 2022. "Monopsony in movers: The elasticity of labor supply to firm wage policies." Journal of Human Resources 57 (S):S50–s86.

Bastian, Jacob. 2024. "Research Note on Elasticities, Work Incentives and Recent Childcare Tax Credit Proposals." Working paper.

Bebe, Imelda A. 2016. Employee turnover intention in the US fast food industry. Walden University.

Ben-Michael, Eli, Avi Feller, and Jesse Rothstein. 2021a. "The Augmented Synthetic Control Method." Journal of the American Statistical Association 0 (ja):1–34. URL https://doi.org/10.1080/01621459.2021.1929245.

———. 2021b. "The Augmented Synthetic Control Method." Journal of the American Statistical Association 116 (536):1789–1803.

Borusyak, Kirill, Xavier Jaravel, and Jann Spiess. 2022. "Revisiting Event Study Designs: Robust and Efficient Estimation." cemmap Working Paper CWP11/22 .

Callaway, Brantly and Pedro HC Sant'Anna. 2021. "Difference-in-Differences with Multiple Time Periods." Journal of Econometrics 225 (2):200–230.

Cengiz, Doruk, Arindrajit Dube, Attila Lindner, and Ben Zipperer. 2019. "The Effect of Minimum Wages on Low-Wage Jobs." Quarterly Journal of Economics 134 (3):1405–1454.

Chetty, Raj, John N Friedman, Nathaniel Hendren, Michael Stepner et al. 2020. "The Economic Impacts of COVID-19: Evidence from a New Public Database Built Using Private Sector Data." NBER Working Paper 27431 .

Cooper, Daniel, María José Luengo-Prado, and Jonathan A Parker. 2020. "The local aggregate effects of minimum wage increases." Journal of Money, Credit and Banking 52 (1):5–35.

De Chaisemartin, Clément and Xavier d'Haultfoeuille. 2020. "Two-way fixed effects estimators with heterogeneous treatment effects." American Economic Review 110 (9):2964–96.

Doudchenko, Nikolay and Guido Imbens. 2016. "Balancing, Regression, Difference-in-Differences and Synthetic Control Methods: A Synthesis." NBER Working Paper 22791 .

Dube, Arindrajit, T William Lester, and Michael Reich. 2016. "Minimum wage shocks, employment flows, and labor market frictions." Journal of Labor Economics 34 (3):663–704.

Dube, Arindrajit and Attila Lindner. 2021. "City Limits: What Do Local-Area Minimum Wages Do?" Journal of Economic Perspectives 35 (1):27–50.

Dube, Arindrajit, Suresh Naidu, and Adam D Reich. 2022. "Power and dignity in the low-wage labor market: Theory and evidence from wal-mart workers." NBER WOrking Paper 30441 .

Ferman, Bruno and Cristine Pinto. 2021. "Synthetic controls with imperfect pre-treatment fit." arXiv preprint arXiv:1911.08521v2 .

Gailliot, Annette, Kristen Harknett, Daniel Schneider, and Ben Zipperer. 2022. "Company Wage Tracker." Working paper, Economic Policy Institute. https://www.epi.org/company-wage-tracker/.

Godoey, Anna and Michael Reich. 2021. "Are Minimum Wage Effects Greater in Low-Wage Areas?" Industrial Relations: A Journal of Economy and Society 60 (1):36–83.

Goolsbee, Austan and Chad Syverson. 2021. "Fear, lockdown, and diversion: Comparing drivers of pandemic economic decline 2020." Journal of Public Economics 193:104311.

Hirsch, Boris, Elke J Jahn, and Claus Schnabel. 2018. "Do employers have more monopsony power in slack labor markets?" ILR Review 71 (3):676–704.

Kaul, Ashok, Stefan Klößner, Gregor Pfeifer, and Manuel Schieler. 2022. "Standard Synthetic Control Methods: The Case of Using All Preintervention Outcomes Together With Covariates." Journal of Business & Economic Statistics 40 (3):1362–1376.

MaCurdy, Thomas. 2015. "How effective is the minimum wage at supporting the poor?" Journal of Political Economy 123 (2):497–545.

Manning, Alan. 2011. "Imperfect Competition in the Labor Market." In Handbook of Labor Economics, vol. 4. Elsevier, 973–1041.

———. 2021a. "The elusive employment effect of the minimum wage." Journal of Economic Perspectives 35 (1):3–26.

———. 2021b. "Monopsony in Labor markets: A Review." ILR Review 74 (1):3–26.

Nadler, Carl, Sylvia Allegretto, Anna Godoey, and Michael Reich. 2019. "Are Local Minimum Wages Too High, and How Could We Even Know?" IRLE Working Paper .

OECD. 2022. "Minimum wages in times of rising inflation." Organization for Economic Co-operation and Development Working Paper .

Peri, G., D. Rury, and J. C. Wiltshire. 2024. "The Economic Impact of Migrants from Hurricane Maria." Journal of Human Resources 59:1795–1829.

Reich, Michael. 2024. "Potential Policy Confounds of Minimum Wage Employment Estimates." Published note, Center on Wage and Employment Dynamics.

Sokolova, Anna and Todd Sorensen. 2021. "Monopsony in labor markets: A meta-analysis." ILR Review 74 (1):27–55.

Sorkin, Isaac. 2015. "Are there long-run effects of the minimum wage?" Review of economic dynamics 18 (2):306–333.

Sun, Liyang and Sarah Abraham. 2021. "Estimating Dynamic Treatment Effects in Event Studies with Heterogeneous Treatment Effects." Journal of Econometrics 225 (2):175–199.

Tong, Di. 2024. "Beyond Wage and Employment: Do Minimum Wage Mandates Affect Management Practices." Working paper, MIT.

Webber, Douglas A. 2022. "Labor market competition and employment adjustment over the business cycle." Journal of Human Resources 57 (S):S87–S110.

Wiltshire, Justin C. 2024a. "allsynth: (Stacked) Synthetic Control Bias-Correction Utilities for Stata." Working paper .

———. 2024b. "Walmart Supercenters and Monospony Power: How a Large, Low-Wage Employer Impacts Local Labor Markets." Working paper .

Wiltshire, Justin C., Carl McPherson, Michael Reich, and Denis Sosinskiy. 2024. "Replication Data for: Minimum Wage Effects and Monopsony Explanations." https://doi.org/10.7910/DVN/5HN0VT, Harvard Dataverse.

Wursten, Jesse and Michael Reich. 2023. "Small Businesses and the Minimum Wage." IRLE Working Paper .

TABLE 1
Average Effects Through 2019

	Average Weekly Earnings	Employment	Own-wage Elasticity
A. All Treated Counties			
Treatment Effect (%)	19.21	-1.11	-0.06
Elasticity	0.33	-0.02	
Placebo-variance-based 95% CIs	[0.28, 0.38]	[-0.07, 0.03]	[-0.21, 0.10]
RMSPE-based *p*-value	0.00	0.49	
B. Excluding Counties with Local Minimum Wages			
Treatment Effect (%)	15.45	0.29	0.02
Elasticity	0.31	0.01	
Placebo-variance-based 95% CIs	[0.25, 0.38]	[-0.07, 0.08]	[-0.22, 0.26]
RMSPE-based *p*-value	0.04	0.52	
C. Excluding Counties in the SF Bay Area and NYC			
Treatment Effect (%)	15.88	-0.22	-0.01
Elasticity	0.27	-0.00	
Placebo-variance-based 95% CIs	[0.22, 0.33]	[-0.07, 0.06]	[-0.24, 0.21]
RMSPE-based *p*-value	0.01	0.69	

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 1000 sampled placebo average estimated effects based on estimated differences from in-space placebo treatment on the donor pool counties.

TABLE 2

Average Employment Effects by Estimator Through 2019

	Full Sample	No Local MW Sample
A. Stacked synthetic control (Bias-corrected)		
Treatment Effect (%)	-1.11	0.29
Elasticity	-0.02	0.01
Placebo-variance-based 95% CIs	[-0.07, 0.03]	[-0.07, 0.08]
RMSPE-based p-value	0.49	0.52
B. Stacked synthetic control (classic)		
Treatment Effect (%)	1.89	2.92
Elasticity	0.03	0.06
Placebo-variance-based 95% CIs	[-0.02, 0.08]	[-0.02, 0.13]
RMSPE-based p-value	0.26	0.20
C. Difference-in-differences		
Treatment Effect (%)	4.07	4.53
Elasticity	0.07	0.09
WBS CIs	[-0.25, 0.39]	[-0.29, 0.48]

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. Panel A presents our preferred ("bias-corrected") synthetic control estimates. Panel B presents "classic" synthetic control estimates (uncorrected for bias from pairwise matching discrepancies). The bias-correction procedure is specific to synthetic controls, so the DiD estimates presented in Panel C are also uncorrected for pairwise matching discrepancies. 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 3
Average Effects For Additional Outcomes Through 2019

	McDonald's Establishments		
	Average Hourly Wage	Price	Pass-Through
A. All Treated Counties			
Treatment Effect (%)	21.65	3.57	0.55
Elasticity	0.75	0.12	
Placebo-variance-based 95% CIs	[0.67, 0.83]	[0.07, 0.17]	[0.32, 0.78]
B. Excluding Counties With Local Minimum Wages			
Treatment Effect (%)	16.36	2.78	0.57
Elasticity	0.71	0.12	
Placebo-variance-based 95% CIs	[0.60, 0.82]	[0.05, 0.19]	[0.21, 0.92]

	Separation Rates Of Restaurant Workers	
	All Workers	Low-Tenure Workers
C. All Treated Counties		
Treatment Effect (%)	-13.12	-35.78
Elasticity	-0.22	-0.61
Placebo-variance-based 95% CIs	[-0.26, -0.19]	[-0.73, -0.50]
D. Excluding Counties With Local Minimum Wages		
Treatment Effect (%)	-12.73	-38.41
Elasticity	-0.26	-0.78
Placebo-variance-based 95% CIs	[-0.33, -0.20]	[-0.94, -0.62]

Note: Panels A-B are estimated using McDonald's data from the Ashenfelter and Jurajda (2020). Mcdonald's subsample 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. Panels C-D are 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. 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 1000 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 4
Average Effects Through 2022

	Average Weekly Earnings	Employment	Own-wage Elasticity
A. All Treated Counties			
Treatment Effect (%)	9.61	6.79	0.71
Elasticity	0.11	0.08	
Placebo-variance-based 95% CIs	[0.06, 0.15]	[0.03, 0.12]	[0.18, 1.24]
RMSPE-based *p*-value	0.02	0.06	
B. Excluding Counties with Local Minimum Wages			
Treatment Effect (%)	9.58	12.87	1.34
Elasticity	0.11	0.15	
Placebo-variance-based 95% CIs	[0.05, 0.17]	[0.09, 0.21]	[0.44, 2.24]
RMSPE-based *p*-value	0.11	0.03	
C. Excluding Counties in the SF Bay Area and NYC			
Treatment Effect (%)	11.30	10.85	0.96
Elasticity	0.13	0.12	
Placebo-variance-based 95% CIs	[0.07, 0.18]	[0.06, 0.18]	[0.33, 1.58]
RMSPE-based *p*-value	0.04	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 1000 sampled placebo average estimated effects based on estimated differences from in-space placebo treatment on the donor pool counties.

Figure Legends

FIGURE 1

Title: Distribution of Average Wage by County

Note: 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 solid thick bar shows the employment-weighted mean for all U.S. counties. The solid thin bars show the 25th and 75th percentiles. The dashed gray bars show the 10th and 90th percentiles. Markers for California are circles, while markers for New York are triangles. Markers for counties with local minimum wages are solid; markers for counties without them are hollow.

FIGURE 2

Title: Reach of California Minimum Wages, 2014-2022

Note: 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 vertical dashed lines indicate precede 2014, the first year affected by state-level minimum wage increases.

FIGURE 3

Title: Treatment Effects in Full Sample of Treated Counties Through 2019

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 1000 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

Title: Employment Level And Average Effects Through 2022

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 1000 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 5

Title: The Evolution Of Mobility Since COVID By Establishment Type

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 light gray is the time period captured by our pandemic index.

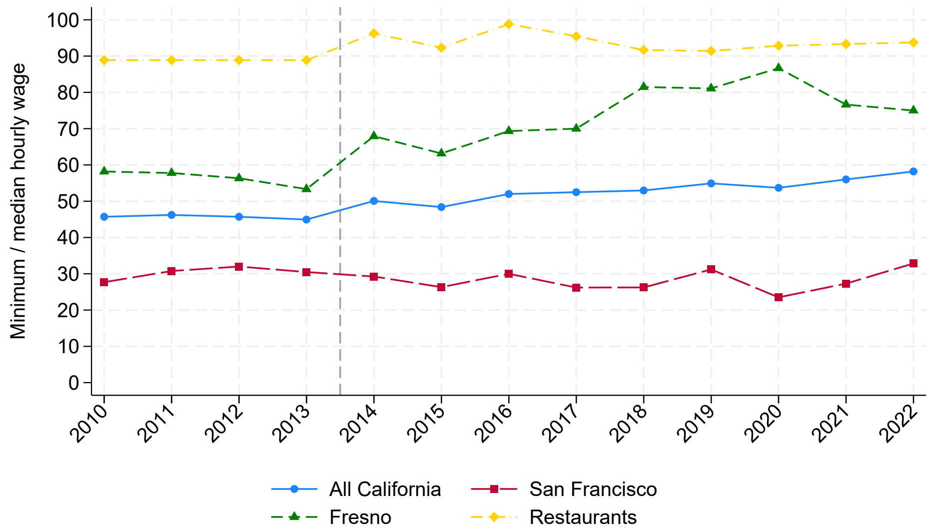
FIGURE 6

Title: Monopsony And Minimum Wages

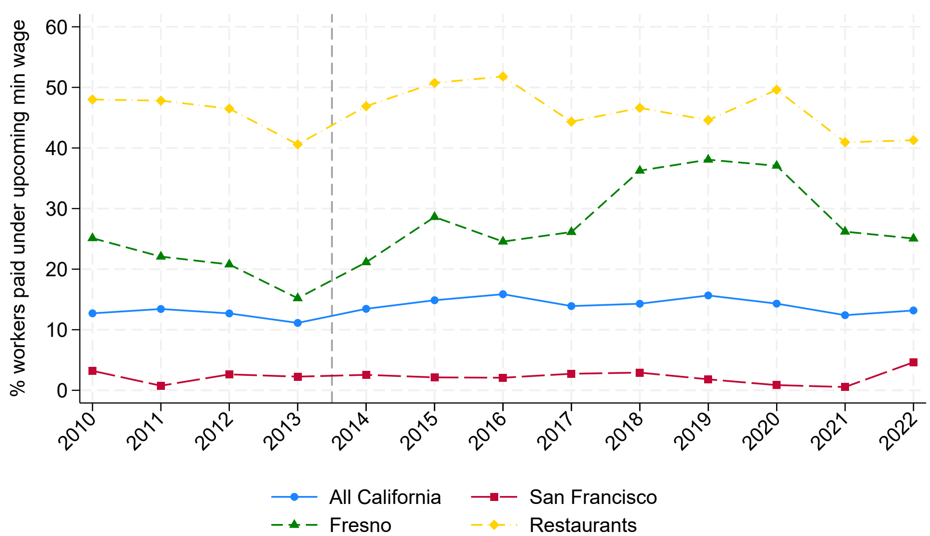
Note: In each plot L^D denotes labor demand, L^S – labor supply, and MC – marginal cost function of the monopsony. L^*, w^* is competitive employment and wage, respectively; L^M, w^M is equilibrium under monopsony without regulation; while $\underline{\mathrm{L}}^i$, $\underline{\mathrm{w}}^i$ for $i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

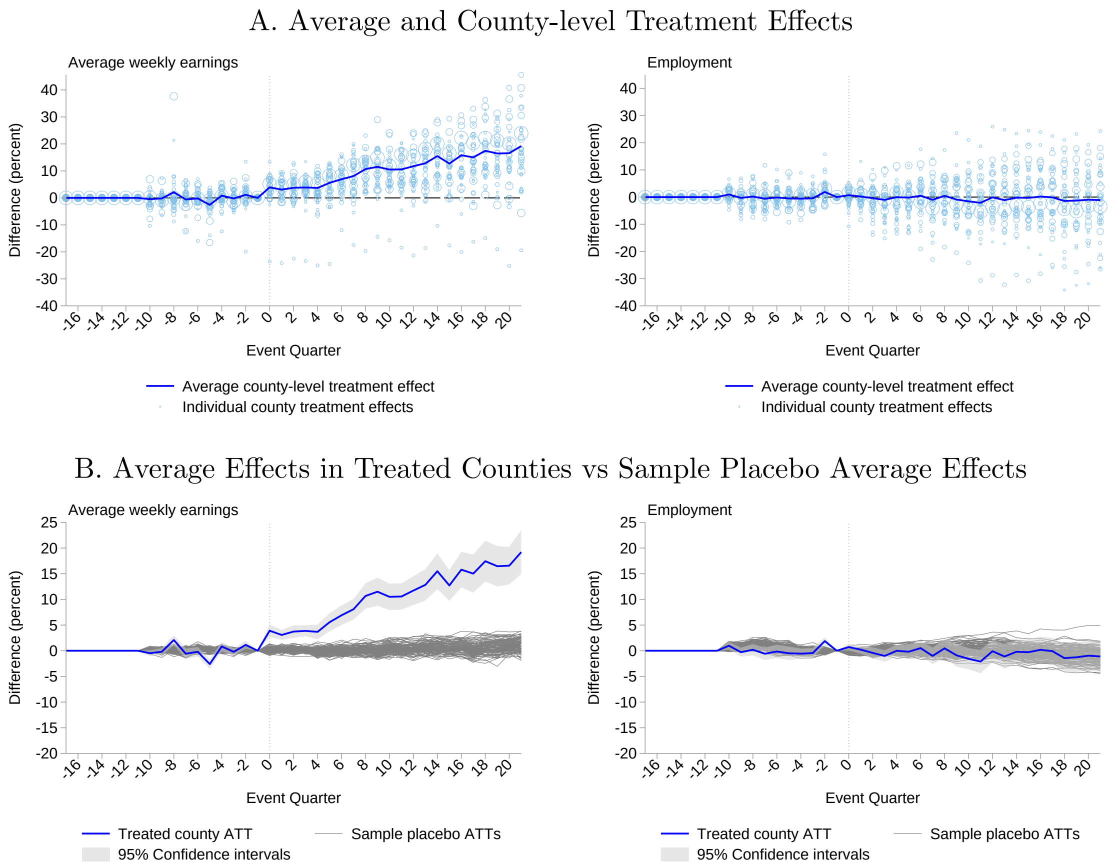

Tulare, CA
Butte, CA
Fresno, CA
Riverside, CA
Orange, NY
Monterey, CA
San Joaquin, CA
Kings, NY
San Bernardino, CA
Stanislaus, CA
Kern, CA
Santa Cruz, CA
Onondaga, NY
Sonoma, CA
Bronx, NY
Queens, NY
Santa Barbara, CA
Monroe, NY
Placer, CA
Albany, NY
Sacramento, CA
Ventura, CA
Solano, CA
Suffolk, NY
Nassau, NY
Los Angeles, CA
San Diego, CA
Orange, CA
Marin, CA
Contra Costa, CA
Alameda, CA
Westchester, NY
San Francisco, CA
Santa Clara, CA
San Mateo, CA
New York, NY
California, Local
California, No Local
New York, Local
New York, No Local
0
500
1,000
1,500
2,000
Average weekly wages

A. Ratio of Minimum Wage to Median Wage

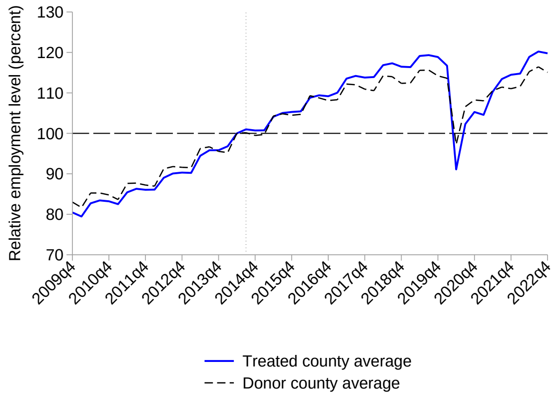


B. Fraction of Workers Earning Under the Upcoming Minimum Wage

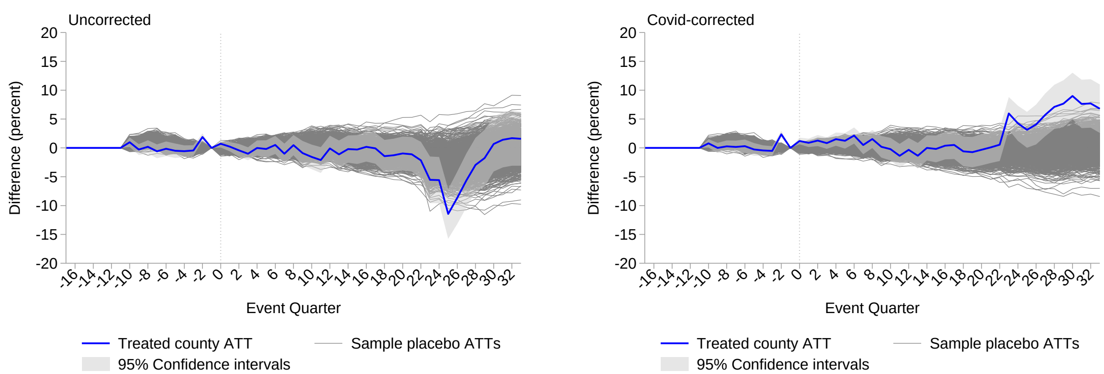



A. Average and County-level Treatment Effects
Average weekly earnings
Employment
Difference (percent)
Event Quarter
Average county-level treatment effect
Individual county treatment effects
B. Average Effects in Treated Counties vs Sample Placebo Average Effects
Average weekly earnings
Employment
Difference (percent)
Event Quarter
Treated county ATT
Sample placebo ATTs
95% Confidence intervals

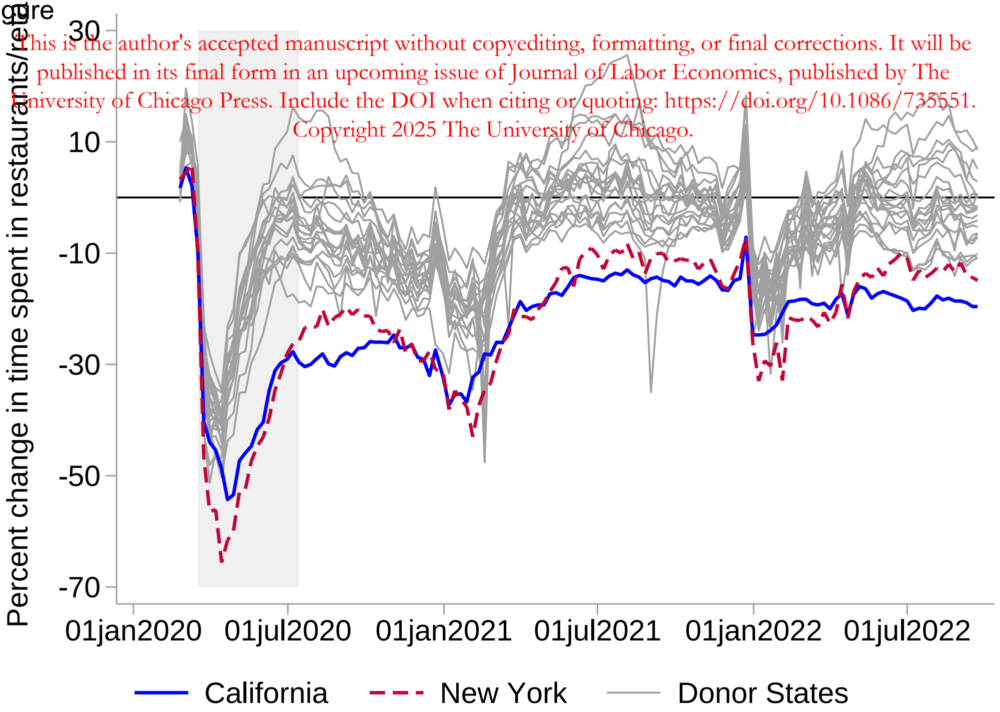
A. Employment In Treated and Donor Counties (2014Q2 = 100)



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

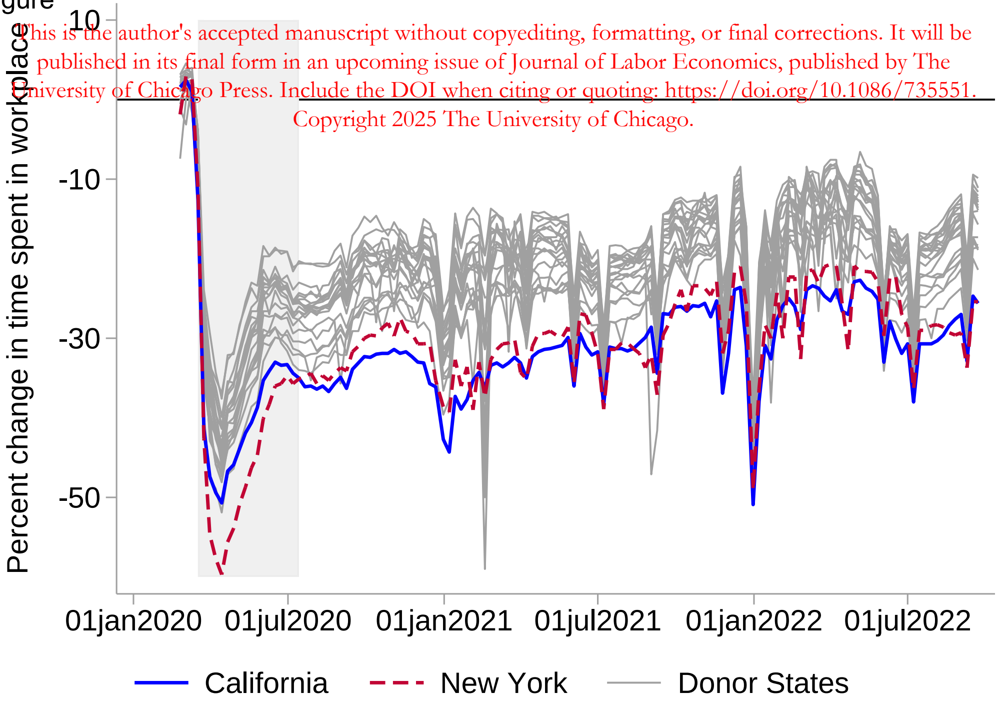


Figure



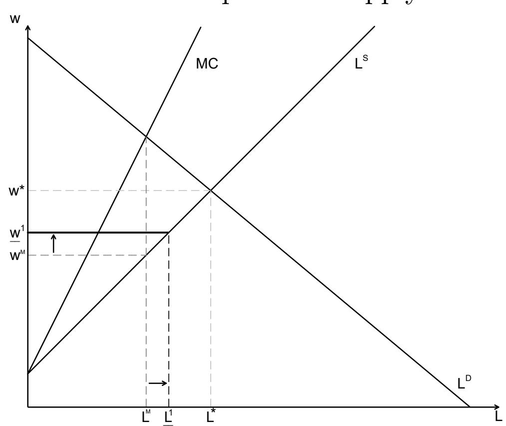
Percent change in time spent in restaurants/retail
30
10
-10
-30
-50
-70
01jan2020
01jul2020
01jan2021
01jul2021
01jan2022
01jul2022
California
New York
Donor States

Figure



Percent change in time spent in workplace
10
-10
-30
-50
01jan2020
01jul2020
01jan2021
01jul2021
01jan2022
01jul2022
California
New York
Donor States

Steep Labor Supply



Flat Labor Supply