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MINIMUM WAGE EFFECTS AND MONOPSONY EXPLANATIONS*

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We present the first causal analysis of recent large minimum wage increases, focusing on 47 large U.S. counties that reached \$15 or more by 2022q1. Using novel stacked county-level synthetic control estimators, we find substantial pay growth and no disemployment effects. Our research design allows us to reduce selection and attenuation effects—by excluding counties with local minimum wages or those with high wages. We then find significant and larger positive employment effects, as the monopsony model predicts. We go on to document the presence of monopsony in the restaurant industry. We show that minimum wages reduce restaurant workers' separation rates and that they caused McDonald's workers' wages to grow faster than the prices of Big Macs, suggesting the presence of monopsony power and positive economic profits. The fast food industry's monopsony power allowed it to accommodate large minimum wage increases and raise employment.

JEL Codes: B41, J23, J24, J31, J38, J42

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I. INTRODUCTION

Recent research studies have found very small or no negative employment effects of moderate minimum wage increases (Cengiz et al., 2019). Yet little evidence exists on the effects of very large policy increases. In this paper we exploit the near-doubling of minimum wages in California and New York State since 2013 to explore the effects of large increases in minimum wages—especially on middle- and lower-income labor markets that are thought to be more susceptible to disemployment effects. We find these minimum wage policies had large, *positive* and significant employment effects in these labor markets, just as a monopsony model predicts. We then show that employer power was present in these labor markets, reinforcing a monopsony explanation of our positive employment estimates.

In 2022 California's minimum wage reached $15 for all workers, the result of a series of increases from $8 that began in 2014. Over a similar 7.5 year period, New York State increased its minimum wage for fast food workers from $7.25 in 2013 to $15—in New York City on December 31, 2018 and in the rest of the state in 2021. California's increases comprise a gain of 87.5 percent; New York's comprise 107 percent. These magnitudes are unprecedented, even measured in constant dollars—56 percent in California and 72 percent in New York—and especially compared to the stagnant federal minimum wage level since 2009. The policies represent a dramatic departure from the incremental increases that characterized federal and state minimum wage policy in prior decades.

We focus on the fast food industry because its wage levels are among the lowest of any sizable industry. It also allows us to avoid issues related to tip credits for servers in full-service restaurants in New York and to permit including New York's upstate counties in our analysis. Our treatment sample consists of 47 larger counties in California and New York that reached $15 or higher by 2022q1. The control counties come from states that have not experienced a minimum wage change since the 2009 federal increase. The range of average pre-treatment earnings among the counties in our treated sample spans the nationwide distribution of county-level average earnings. As a result, our estimated minimum wage effects are likely generalizable to the U.S. as a whole.

Our primary analysis uses a stacked county-level synthetic control estimating strategy (Abadie and L'Hour, 2021; Dube and Zipperer, 2015; Wiltshire, 2022b). We normalize the data and correct for biases resulting from pairwise matching discrepancies and differences in pandemic responses between treated counties and control counties. Our county-level stacked synthetic control approach constitutes a novel strategy for estimating minimum wage effects. It directly accounts for local variation and estimates optimal counterfactuals for each treated county, making it well-suited to our context. It admits much more precise estimates than classic synthetic control estimation with a single treated unit.

Our county-level design allows us to address whether our positive employment estimates result from selection and attenuation effects, issues that have received relatively little attention in the literature. We exploit the fact that higher local minimum wages are more likely to be implemented where statewide policies have less bite. To account for such effects, we restrict our analysis to counties that did not locally choose to adopt higher local minimum wages and where statewide policies were more binding. We find that the near-doubling of the minimum wage caused significantly greater employment gains in these less-affluent counties.

Our primary specification estimates a highly significant earnings elasticity for fast food workers of +0.10, comparable to the Wursten and Reich (2023) fast food earnings elasticity of +0.15 estimated using a stacked event study on all national minimum wage events between 1990 and 2015. Our estimated employment elasticity of +0.08 is positive and significant according to even our conservative p-values. These wage and employment estimates imply an own-wage elasticity of +0.69, compared to an OWE of +0.41 for all workers in Cengiz et al. (2019) for their sample of minimum wage events between 1984 and 2016.

We then consider whether these positive estimated employment effects result from selection or attenuation biases. As Dube and Lindner (2021) document, cities that enact local minimum wage increases tend to already have higher wages. We therefore examine employment effects in sub-samples that *exclude*, in turn, areas with local minimum wages and areas with higher average wages. We then obtain *larger positive* and significant employment elasticity estimates that are persistent and robust. Indeed, in our sample of counties that excludes locales with minimum wages above the state-wide minima, our wage elasticities are similar but our 0.14 employment elasticity is higher than before. As a result, our estimated full-year own-wage elasticity for 2022 is 1.20; and reaches as high as 1.53 by the end of 2022—comparable to own-wage elasticities in Card (1992b); Card and Krueger (2000); Katz and Krueger (1992), and substantially higher than those found in more recent minimum wage studies. The larger positive employment estimates in these sub-samples suggest that selection and attenuation effects can obscure evidence of how minimum wages overcome monopsony power and raise employment.[1]

To connect our work to the previous literature, we also present difference-in-differences and synthetic difference-in differences estimates of our main results (Arkhangelsky et al., 2021; Callaway and Sant'Anna, 2021). In California, where \$15 minimum wages are legally binding for all workers, we additionally examine effects on all restaurant workers and teens. Our results for all restaurant workers show no evidence of disemployment effects. We find positive employment effects for teens that are consistent with the monopsony model.

To examine minimum wage effects on all workers, we again focus on California and develop a novel hourly wage bin-by-bin analysis, similar to that in Cengiz et al. (2019). To do so, we build on our stacked synthetic control estimation strategy, which is well-suited for studying repeated annual minimum wage increases following a long period of no change. We find that the job effects are concentrated almost entirely just below and above the new minimum wage levels, with no significant employment effects in high wage bins.

[1] As Online Appendix E reviews, early studies of minimum wage effects in fast food (Card, 1992b; Card and Krueger, 2000; Katz and Krueger, 1992) also found positive employment effects. More recently, Wiltshire (2022b) found positive employment effects in retail, especially where monopsony power is likely to be greater.

Our treatment period from 2013 to 2022q4 includes the rapid and severe pandemic-driven recession in March and April 2020 and the subsequent sharp economic recovery (Bureau of Labor Statistics, 2022). The negative shocks to low-wage employment caused by initial local pandemic-related responses were greater in our treated counties than in our controls, but plausibly independent of minimum wage effects. We therefore build on the bias-correction synthetic control literature (Abadie and L'Hour, 2021; Ben-Michael, Feller, and Rothstein, 2021; Doudchenko and Imbens, 2016) to develop and implement a method that mitigates bias related to heterogeneous pandemic responses, fitting a model only on control counties. Our uncorrected results over the entire treatment period show no evidence of disemployment effects, except during the height of the pandemic. Our pandemic correction shrinks continuously after 2020q4. Both our uncorrected and corrected estimates detect positive employment effects from 2021q3 on.

We end by exploring some possible monopsony mechanisms for this result. We first examine the effects of large minimum wage policies on separation rates of restaurant workers, teens and young workers. We then examine the effects of these policies on profit margins at a large sample of McDonald's restaurants. We find that minimum wages significantly reduced employee separation rates as well as McDonald's profit margins. These results strongly suggest our positive employment estimates are due to higher minimum wages overcoming local monopsony power.

The literature on large minimum wage increases in the U.S. is scant. The landmark paper by Cengiz et al. (2019) does not find significant disemployment effects in the highest decile of state minimum wage bites. 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. They find no disemployment effects even where the minimum-to-median wage ratio reaches as high as 82 percent. Our paper takes a more granular approach and finds significant positive employment effects.

Our paper makes four substantive contributions to the minimum wage literature. First, we evaluate the effects of large minimum wage that have little recent U.S. precedent, finding evidence of positive employment effects—contrary to predictions for competitive labor markets. Second, we leverage county-level data to consider potentially confounding effects from selection and attenuation biases. We find that subsetting the sample to focus on areas with larger bites results in higher positive employment effect estimates—a result even more at odds with predictions for competitive labor markets. Third, we find further evidence for monopsony labor market dynamics– in reduced employee separation rates following minimum wage increases. Fourth, we obtain estimates of the effects of minimum wages on profit margins. Taken together, these results indicate the presence of monopsony power in the fast food sector in local labor markets and provide evidence that minimum wages help overcome this employer power to the benefit of workers.

Our paper also makes three methodological contributions. First, we extend the sparse literature that leverages local variation with a stacked synthetic control method. Our stacked county-level synthetic control estimator provides more precise results than a statewide estimator, allowing us in effect to match Los Angeles to Montgomery and Atlanta, rather than only California to Alabama and Georgia. Second, we extend a bias-correction procedure for synthetic control methods to develop a novel means of separating pandemic-response confounds from minimum wage effects in stacked synthetic control estimates. Finally, we provide the first template for using stacked synthetic control results to estimate employment effects throughout the wage distribution.

The paper proceeds as follows. We discuss the policy environment and the monopsony puzzle in Section II. We discuss our data, analysis samples and descriptive statistics in Section III. In Section IV we explain the stacked synthetic control method and detail our extensions of it. In Section V we present our main results, our evidence for monopsony power and the distributional effects on all workers. We present and discuss the results of our robustness tests in Section VI and present evidence of monopsony in Section 7. We further discuss our results and offer conclusions in Section VIII. Appendix materials can be found in the Online Appendix.

II. HIGH MINIMUM WAGES AND THE MONOPSONY PUZZLE

II.A. High Minimum Wages: The Policy Environment

The federal minimum wage in the U.S. 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.

New York State's minimum wage for all workers began increasing on December 31, 2013. State law pre-empts New York localities from setting their own minimum wages. Nonetheless, responding to local conditions, in 2017 New York State created three minimum wage tiers: one for New York City; a second for the surrounding counties of Nassau, Suffolk and Westchester; and a third for upstate counties.[2] 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 (see Table I).

In July 2014, California began increasing its minimum wage for all workers, reaching $15 in 2022.[3] 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 rose more rapidly than in the state as a whole, and exceeded $16 by 2022. Table A.1 of the Online Appendix details the evolution of the minimum wage

[2]As Table I shows, though all three tiers were designated to eventually reach $15, by 2022 the minimum wage for all workers in the upstate counties remained lower—at $13.20.

[3]From 2023 on, California's minimum wage is indexed annually, capped at 3.5 percent per year. Additionally, the minimum wage for fast food workers is scheduled to increase to $25 on April 1, 2024. 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.

in the 34 California cities—across nine counties—that had local minimum wages, 17 of which had reached $15 or higher by 2020q1.[4]

Figure I presents 2013 county-wide average weekly earnings among the 47 largest counties in New York and California and in four other U.S. counties with minimum wages that reached at least $15 by 2022q1 (or contained cities with higher local floors). Figure I indicates that 2013 earnings were generally higher in the 14 counties with local minimum wages than in the remaining 33 counties (all of which are in California and New York). This correlation suggests that treatment selection is nonrandom, which could bias estimates of minimum wage effects.[5] Figure I also shows that the distribution of average earnings in the 33 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.

To further examine the representativeness of these counties, we show the share of all workers in fast food in each of the counties in Figure A.1. We use the same earnings ranking as in Figure I. 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.

II.B. The Range of these Policies Among Exposed Groups and Areas

To provide further context for the substantial scope of these policies, we deploy two commonly-applied minimum wage metrics: the ratio of the minimum wage to the median wage, and the fraction of workers earning less than the upcoming minimum wage (the ”bite”). Figure II displays these metrics for all restaurant workers in California (shown in blue), for a low-wage local labor market (Fresno, shown in green), for a high-wage local labor market (San Francisco, shown in red) and for teens 16 to 19 (shown in yellow).[6]

Panel A of Figure II shows how the minimum wage policies changed the ratio of the minimum wage to the median wage. For California this ratio increased from 44 percent in 2013 to 58 percent

[4]Since all the California cities fully index their minimum wage levels to inflation, minimum wage rates in 2023 (not shown in the table) were substantially higher.

[5]The 14 counties with local minimum wage counties are also high cost of living areas, a point often noted by local advocates of higher minimums.

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

in 2022.[7] 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.[8] 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 II displays how increasing California's minimum wage affected the proportion 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 II also plots these outcomes for the two most exposed subgroups: teens and restaurant workers. The bite for teens ranges between 45 and 60 percent, while the bite for restaurant workers ranges between roughly 40 and 50 percent. For both groups, the ratio of the minimum wage to the median wage hovers between 90 and 100 percent.[9] Figure II thus strongly indicates that these two subgroups are highly exposed to minimum wage policies[10] We plot exposure levels for each county visible in the CPS in Figure A.2.

II.C. The Monopsony Puzzle

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

[7] 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, as we show in Figure A.4.

[8] 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.

[9] 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 35 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.

[10] We discuss results for all restaurant workers in Section VI and for teens in Online Appendix C.

Mortensen, 1998). Binding minimum wage increases overcome the low-wage option by forcing the firm to pay the higher wage to all its employees: workers then face higher wages and accordingly supply greater quantities of labor, while the minimum wage becomes the new (flat) marginal cost of labor to the firm, inducing higher quantities of labor demanded.

Among the variety of monopsony models (see Manning (2021) for a review), the dynamic model that emphasizes search and matching frictions best fits what we know about the fast food restaurant industry. Fast food restaurants locate near their customers—and therefore near each other. Fast food workers thus usually have multiple feasible outside options. However, fast food exhibits the highest job vacancy and employee turnover rates of any industry. And Wursten and Reich (2023) find a minimum wage separation elasticity of -0.23 for fast food, implying a labor supply elasticity of +0.46, consistent with the mechanism in the dynamic monopsony model.[11]

However, invoking the presence of dynamic monopsony power to explain the puzzle of absent disemployment effects raises another puzzle: Binding minimum wage increases should, at least up to the competitive wage level, increase employment. Moreover, a labor supply elasticity of 0.46 implies a substantial monopsony-generated wage markdown, suggesting that the magnitude of the predicted positive employment effect of a minimum wage should also be substantial. Yet the most positive empirical estimates of minimum wage employment effects are often noisy zeros.[12] We refer to this discrepancy between theory and empirical estimation in the minimum wage literature as the monopsony puzzle.[13]

How might the monopsony puzzle be resolved? Data limitations constitute one possibility. Perhaps the restaurant sector constitutes too broad an industry to detect positive employment effects. The restaurant sector encompasses full-service restaurants at a variety of price and wage points as well as lower-wage fast food restaurants. This heterogeneity may attenuate observed employment effects.

We study the fast food industry because its pay levels are lower and less heterogeneous than restaurants as a whole. One might nonetheless be concerned that establishment-level data on fast food would be superior to the industry-level data that we use. Indeed, in recent decades the emergence of "fast-casual" restaurant chains and the proliferation of fast food ethnic varieties has increased the diversity of the industry. The major fast food chains today encompass a variety of food and ethnic varieties, with different price points.[14] Such diversity may add noise to estimated employment

[11]See also Kudlyak, Tasci, and Tuzemen (2023). Employers in less dense areas may possess more employer power, as in Wiltshire (2022b). Firm-specific non-wage amenities are less common in fast food and therefore less likely to be sources of employer power.

[12]Wiltshire (2022b) is a notable exception.

[13]The puzzle applies even when businesses with employer power raise their prices, as Ashenfelter and Jurajda (2022) found for McDonald's restaurants. Price increases raise the value of the marginal product of labor and shift the labor demand curve outward. If the labor market is imperfectly competitive, the new demand curve intersects with the marginal cost curve (up and) to the right of the previous point of intersection, thus *increasing* employment. In contrast, price increases in a perfectly competitive labor market would merely mitigate employment losses.

[14]Such as burritos, chicken, hamburgers, pizzas, sandwiches and tacos; and Chinese, Italian, Mexican, Thai and

effects.

This diversity does not imply that results using establishment or firm-level data are preferable to industry-level data. Studies of individual fast food chains (Card and Krueger, 1994; Katz and Krueger, 1992) can miss reallocation effects within fast food as a whole.[15] Industry-level effects thus may provide better measures of the net effects on the industry's workers.

A second explanation of the monopsony puzzle suggests that measurement error masks positive employment effects when studying smaller minimum wage increases. Using state-level data, Cengiz et al. (2019) do not find heterogeneity by state-level exposure. Using county-level data and a regression-based estimator, Godoey and Reich (2021) find somewhat more positive, but noisy, employment effects in more exposed counties. However, the minimum wage increases in these papers average just ten percent.

A third explanation, and the one we examine here, suggests that minimum wage effects may be masked by endogeneity in treatment selection and by attenuation effects. As Aeppli and Wilmers (2022) show, a state's average wage in 2012 predicts the magnitude of the state's minimum wage increases between 2012 and 2018. In other words, treatment selection at the state level is non-random. If employer power is weaker in higher wage areas, policy endogeneity could attenuate estimated employment effects when studying all state-level minimum wage events.

To overcome these obstacles to detecting monopsony employment effects, we examine effects a) of much larger minimum wage increases, b) using industry-level data on fast food, and c) in local areas that are neither inclined to select into higher minimum wages nor subject to the imposition of higher local minimum wages prompted by local economic conditions. We can therefore better control for policy endogeneity and attenuation bias and provide a cleaner test of whether high minimum wages generate positive employment effects.

III. SAMPLES AND DATA

III.A. Analysis Samples

To estimate effects that allow straightforward interpretation, we choose our analysis sample to balance the number of post treatment quarters in event time, through the fourth quarter after a $15 policy. California and New York State each have 30 quarters from the first minimum wage increase (2014q3 and 2014q1, respectively) to the first quarter of $15 for fast food (2022q1 and 2021q3, respectively). Adding another three quarters of data (taking California through 2022q4, and New

Vietnamese ethnic varieties. The In-N-Out Burger chain, which operates in multiple western states and employs 27,000 workers, pays higher wages and experiences lower employee turnover than does Burger King. Taco Bell and Chipotle both serve Mexican food, but at different price points.

[15] Examples of reallocation within ethnic varieties include customers switching from Taco Bell to Chipotle, or from McDonald's to Shake Shack. Such reallocation effects can explain why industry-level studies find much smaller wage effects than, say, the Ashenfelter and Jurajda (2022) paper on McDonald's.

York through 2022q2) yields 33 total post-treatment event quarters. We do not have as many post-treatment quarters through $15 for Chicago, Denver and the District of Columbia, requiring us to exclude these cities from our sample. We can then align the panel in event time.[16]

We focus on fast food workers because their wage rates are among the lowest of any sizable industry, because only fast food workers were covered by a $15 minimum wage in upstate New York, and to ensure comparability across areas that do and do not have tip credits.

Our treatment sample consists of 25 counties in California and 22 in New York. To reduce overfitting and reduce noise in the data (especially from sparsely populated counties) we restrict the California counties to those with at least 5,000 restaurant workers. We restrict the New York counties to those with at least 2,000 restaurant workers and consider appropriately sized control counties for those with fewer than 5,000 restaurant workers. These restrictions retain 95.6 percent of California employment and 86 percent of New York employment.[17]

Our untreated control counties consist of all the similarly sized counties with a $7.25 minimum wage throughout the pre-treatment and treatment periods. We have 123 large control counties for the 25 California treated counties and 11 large New York treated counties, and 150 mid-size control counties for the 11 mid-size New York treated counties.[18] Online Appendix Table A.2 lists the large donor pool counties. Table A.3 lists the mid-size donor pool counties. Columns 1 and 2 of Online Appendix Table A.4 present, as an example, the synthetic control weights for Los Angeles, one of our 47 treated counties (see Section IV). The weighting matrices for both outcomes are sparse and have several common donors, concentrated in Alabama, Georgia, the Carolinas and Texas.

Our analysis period begins in 2009q4, just after the last federal minimum wage increase, and ends in 2022q4, which is the most recent quarter with available QCEW data at the time of writing. For uniformity across the two states, our primary analysis ends in their f quarter with a $15 minimum wage.[19] We convert data for each county to event time and we end in event quarter 33, which is 2022q4 for California and 2022q2 for New York.

Our research design allows us to generate informative results for minimum wage effects at levels between $8 and $15, as well as at $15, using a sample that is representative of the U.S. as a whole. In principle, we could include counties in states with minimum wages below $15. However, our interest is in the effects of high minimum wages. Equally important, including only California and New York confers an important advantage for identification. Counties in both states (except in San Francisco and Santa Clara Counties) had long pre-treatment periods with no policy changes. The pre-treatment minimum wage trends in these counties are therefore identical to those in our donor pool– an important feature of our research design. Meanwhile, policy increases during our

[16] We additionally exclude Seattle because of its multi-tiered minimum wage structure.

[17] Relaxing the New York restriction did not add appreciably to the share of retained employment.

[18] In a robustness check using all restaurant workers in California, we add San Luis Obispo County to the other 25 large treated counties in California. The QCEW suppresses or is otherwise missing data for San Luis Obispo fast food workers, but not for restaurant workers.

[19] Several counties in the Bay Area and New York City reached a minimum wage higher than $15 by event quarter 33.

pre-treatment and treatment periods in all the other states with their own minimum wages either challenge the assumptions of our research design, possibly confounding our estimates, or were too low to be informative about effects of high minimum wages.

As we previously noted, Figure I shows that our treated counties are sufficiently diverse to capture most of the national variation in average county wages. Tulare, CA lies just below the (dashed gray) 10th percentile line. Four counties, three in the Bay Area and New York County (Manhattan) lie far above the 90th percentile line. The remainder of the treated counties are distributed uniformly in the 10th to 90th percentiles of U.S. county-level earnings.

To examine heterogeneity in the effects of minimum wages within our primary treated sample, we separately impose two sample restrictions. The first of these restrictions excludes the fourteen counties with a higher county-level minimum wage or a higher local minimum wage in at least one of its constituent localities.[20] The second restriction excludes the nine San Francisco Bay Area counties and nine New York City metro counties that comprise the high-income outliers in our sample.[21]

Our analysis of teens and restaurant workers draws entirely from California, where a $15 minimum wage is legally binding on all workers and employers and where there is no tip credit. The sample years are the same as for our study of fast food workers. Our control group for teens come from states without a minimum wage change since 2009. Our control group for restaurants is the same as in our fast food analysis.

III.B. 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.[22] 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 re-

[20]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), Queens and Richmond (Staten Island).

[21]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) Queens, and Richmond (Staten Island).

[22]Prior to 2012, the equivalent code is 722211.

search (Nadler et al. 2018) has demonstrated the small variation in quarterly hours in the QCEW.[23]

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.

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. Using the LAUS, we also calculate annual state-level unemployment rates for our state-level analyses.

4. Pandemic-response Shock Index. We use Google's Community Mobility Data as aggregated by Chetty et al. (2020) to construct an index of the effects of these local pandemic responses on economic activity in fast food restaurants. Google Mobility data uses location data from smartphones to track their owners in different locations before and after the onset of the pandemic. For each day of the week in each county, these data report the time individuals spent in a location that day relative to the median time spent that same weekday from January 6, 2020 to 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.[24] We discuss the evolution of each of these measures in our analysis sample in Online Appendix B. As we explain in Section IV, 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

[23]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.

[24]Google does not provide disaggregated data for fast food restaurants

(QWI) to estimate restaurant industry separation rates. The QWI report separation rates both for all workers and low-tenure workers– those 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, except that the QWI uses somewhat different data fuzzing and suppression algorithms.

6. McDonald's Price and Wage Data. Beginning in 2016, Ashenfelter and Jurajda have collected annual data on hourly wages and Big Mac prices for over 10,000 McDonald's locations in the U.S. See Ashenfelter and Jurajda (2020, 2022) for further details. We are grateful to the authors for sharing their updated data with us.

III.C. Raw Earnings Patterns

Observed earnings growth constitutes a necessary condition for the validity of any estimated employment effects. We therefore consider here trends in raw earnings for fast food workers throughout the pre- and post-treatment periods, using QCEW data. Figure III presents raw earnings data by state and size for the treated counties (large California counties, large New York counties, and mid-size New York counties) and the associated donor pool counties, along with the population-weighted average for each group. The earnings data are normalized to 100 in the final pre-treatment year for each state.

Three patterns appear in each of these plots. First, average earnings growth and seasonality in the treated counties and the donor counties were identical in the pre-treatment period, even without the application of a statistical control algorithm. Second, average weekly earnings began growing faster in the treated counties once the minimum wage began increasing; this divergence continued through 2019. Third, despite continued growth in the minimum wage in treated counties throughout the pandemic era (from 2020q1 onward), average weekly earnings in treated areas stopped diverging from those in the donor counties. Indeed, we observe some earnings convergence beginning in late 2021, suggesting more rapid earnings growth in donor pool counties.

This last pattern suggests that cross-state earnings differentials fell among low-wage (fast food) workers from 2021 onward. This compression is consistent with the exceptionally tight labor market conditions of this period. Using a broader array of indicators, Autor, McGrew and Dube (2023) also find such wage compression.

IV. METHODS

IV.A. Methods in the Minimum Wage Literature

Most modern minimum wage papers use regression-based difference-in-differences estimators.[25] These methods can be informative in cases with many treated units and large datasets. A smaller number of minimum wage studies use a synthetic control method. Two papers are most closely related to ours. Nadler et al. (2019) uses synthetic controls to study the effects of minimum wages in six cities through the end of 2016 and finds that minimum wage increases up to $13 increased pay of restaurant workers but did not reduce employment. However, the cities with local minimum wage policies may differ in important economic dimensions from the states that have set their own minimum wages (Dube and Lindner, 2021); and these policies may have different effects at $15 or $16 than at $12 or $13. Wiltshire (2022b) uses a stacked synthetic control method to study the local monopsony power of a major retailer, finding that federal minimum wage increases resulted in employment gains in places where the retailer operated.

We use synthetic control methods because in a linear factor model they do not rely on parallel mean outcome trends (Abadie, 2021) and they are informative in situations with as few as one treated unit, provided data are available for a sufficiently long pre-treatment period. Moreover, synthetic control methods allow us to be explicit and transparent about each estimated counterfactual and its similarity with the associated treated unit. Classic synthetic controls restrict weights to be non-negative, yielding estimates that are free of extrapolation beyond the support of the donor units (Abadie, Diamond, and Hainmueller, 2015; Ben-Michael, Feller, and Rothstein, 2022; Kellogg et al., 2021). And extensions in the past decade have made synthetic controls well-suited to handling policy environments in which treatment adoption is staggered and even when potential control/donor units ideally differ among treated units.[26] For these reasons, synthetic controls are well-suited to study the effects of a series of repeated annual policy events, such as the minimum wage increases in California and New York counties between 2014 and 2022.

IV.B. Synthetic Control Methods

1. Overview. Our "stacked" synthetic control estimator (Wiltshire, 2022a), is an event-period-specific weighted average of the individually-estimated synthetic control estimates of treatment effects for many units that received a binary, "absorbing" treatment.[27] This approach allows for more flexibility and specificity in donors. In our analysis, for instance, Los Angeles County is most comparable to the sprawling urban counties of the South, while Fresno County is most similar to more-rural areas in Texas, Georgia, and South Carolina.

[25] Prominent recent examples include Cengiz et al. (2019); Godoey and Reich (2021); Godoey et al. (Forthcoming); and Wursten and Reich (2023).

[26] For example, Abadie and L'Hour (2021); Acemoglu et al. (2016); Ben-Michael, Feller, and Rothstein (2022); Cavallo et al. (2013); Dube and Zipperer (2015); Kreif et al. (2016); Peri, Rury, and Wiltshire (Forthcoming); Wiltshire (2022a,b).

[27] A treatment is "absorbing" when any unit that receives the treatment remains treated (Sun and Abraham, 2021).

As we describe below, a stacked synthetic control approach also provides an opportunity to correct our estimates for potential bias associated with non-identically-distributed post-treatment period shocks, such as the effects of local pandemic responses on labor markets. And using stacked synthetic controls to estimate average effects over many treated units makes it viable to engage alternative modes of synthetic control inference.

For a given outcome of interest, our synthetic control estimator selects weights to best match an individual treated unit to a subset of untreated "donor pool" units along specified dimensions in the pre-treatment period. The resulting weighted average of donor pool unit outcomes is the synthetic control estimate of the counterfactual dynamic outcome path. Under fairly general assumptions and with a good pre-treatment "fit" between the treated unit and its synthetic control, the difference in the two dynamic outcome paths yields the estimated treatment effects. Abadie and L'Hour (2021) and Ben-Michael, Feller, and Rothstein (2021) further propose a correction procedure to adjust for bias resulting from pairwise matching discrepancies.

We estimate separate synthetic controls and paths of treatment effects for each treated county in California and New York and then stack and average these estimates using 2010 population levels as weights. To correct our estimates for the effects of confounds related to discrepancies in local pandemic responses, we adapt the bias-correction procedure as discussed below.

The literature on synthetic control inference methods remains active.[28] The most widely adopted approach, developed in Abadie, Diamond, and Hainmueller (2010, 2015), generates p-values based on the distributions of the ratios of the (root) mean squared prediction error (MSPE) as calculated by permuting treatment across untreated units.[29] For long post-treatment periods over which treatment intensity is increasing, RMSPE p-values for later periods are inherently conservative as they are calculated inclusive of estimates from all preceding post-treatment periods. Moreover, while two-sided inference may be appropriate for many contexts, two-sided RMSPE p-values may be substantially underpowered (Abadie, 2021).

One alternative, which also relies on the sample distribution of placebo averages, adapts the placebo-average-variance approach expounded in Arkhangelsky et al. (2021).[30] This approach assumes homoskedasticity across units and relies on a normal distribution of the estimand.[31]

We present both two-sided RMSPE p-values and 95 percent confidence intervals adapted from the placebo-average-variance approach to include only a single post-treatment period of interest. We

[28] See, for example, Abadie and L'Hour (2021); Ben-Michael, Feller, and Rothstein (2022); Cavallo et al. (2013); Chernozhukov, Wüthrich, and Zhu (2021); Doudchenko and Imbens (2016); Dube and Zipperer (2015); Ferman and Pinto (2017); Firpo and Possebom (2018); Hahn and Shi (2017).

[29] As Abadie (2021) notes, under the extreme assumption of truly random treatment this approach is simply randomization inference (Fisher, 1935).

[30] See also Conley and Taber (2011).

[31] In cases with many (M) treated units each placebo average will also be random draws of M donor pool units, thus the distribution is approximately normal by a central limit theorem. We thank Guido Imbens for a helpful observation on this point.

generally view the two-sided RMSPE p-values as conservative. We calculate 95 percent confidence intervals for our own-wage elasticity (OWE) estimates using the delta method and standard errors from the placebo-average-variance approach.

2. Stacked Synthetic Control Estimator. As the stacked synthetic control setup nests the classic case with a single treated unit, we expound here only the former. Formally, 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.[32] 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 a set of restrictions on the weights that help justify considering the estimated synthetic controls as valid counterfactual estimates. Specifically, given a set of weights $v_1^i,...,v_k^i$ that determine the relative importance of the k predictors,[33] 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:

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

[32] 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); Ferman and Pinto (2021), while also allowing estimation of effects in percentage changes.

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

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).

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

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

with some weights γ_i on the treated units such that $\gamma_i \geq 0 \;\forall\; i$ and $\sum_i^I \gamma_i = 1$.[34]

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.

Satisfying Condition (A) requires a treated unit and its synthetic control to have (i) a good pre-treatment fit for the outcome variable, and (ii) a good match on the predictor variables. It is also important to have (iii) a good match on the predictors between the treated unit and each of its donor units (Abadie, 2021). Abadie and L'Hour (2021) and Ben-Michael, Feller, and Rothstein (2021) propose a procedure to correct for possible bias in synthetic control estimates resulting from violations of (ii) and (iii), related to the outcome-residualization proposal of Doudchenko and Imbens (2016).[35]

Condition (B) may also need to be addressed—especially in our setting, since the pandemic began in 2020q1. Recent research has highlighted the differential local intensity and effects of changes in 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 higher minimum wages. In particular, pandemic restrictions in urban counties in California and New York were longer and more restrictive than elsewhere (Chetty et al., 2020).

We adopt the synthetic control bias-correction procedure to address (A), and then extend it to address (B) by treating each county's pandemic response as a predictor variable for which we

[34] Abadie and L'Hour (2021) and Abadie (2021) also propose a synthetic control bias-correction that attenuates bias from pairwise matching discrepancies in the values of the predictor variables between each treated unit and its synthetic control donors. We present these bias-corrected estimates as a robustness check.

[35] Abadie (2021) contains an excellent discussion of this bias-correction procedure.

failed to obtain a good fit. This approach does not use the pandemic response as a predictor in the synthetic control estimation; rather it removes its conditional effect on the outcome values.

We correct pandemic-related effects *after* estimating the synthetic control weights, by removing the pure effect of the *initial* local pandemic response on the outcome values. We do so by first estimating the average effect of the pandemic response on each period using *only* the donor pool units (none of which experienced a minimum wage increase). We then residualize the outcome values in that period for *all* (treated and donor pool) units using that estimated average pandemic-response 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.

Formally, consider a "standard" bias-correction procedure. 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 $\tilde{\hat{Y}}^N_{it} = \sum_{j=I+1}^{I+J} \hat{w}^i_j \tilde{Y}_{jt}$, which admits the bias-corrected synthetic control estimated gaps for each $\{i,t\}$, $\hat{\tau}_{BC_{it}} = \sum_{i=1}^{I} \gamma_i (\tilde{Y}_{it} - \tilde{\hat{Y}}^N_{it})$. We can then place these gaps in event time and use them to calculate the analog of Equation (4), corrected for bias arising from differences in predictor values.

To obtain estimates that also correct for the confounding effects of local pandemic-response intensity, we extend the bias-correction procedure: First, as before, for each i we obtain $\hat{\mathbf{W}}^i$ from synthetic control estimation on the uncorrected (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, $\tilde{Y}'_{zt} = Y_{zt} - \hat{\mu}^i_{0t}(\tilde{X}_z)$. Third, we apply $\hat{\mathbf{W}}^i$ to the $\tilde{Y}'_{jt}$ to calculate $\tilde{\hat{Y}}^{N\prime}_{it} = \sum_{j=I+1}^{I+J} \hat{w}^i_j \tilde{Y}'_{jt}$, yielding (in event time) the analog of Equation (4) corrected for bias arising from differences in predictor values *and* initial local pandemic policies and behavioral responses:

$$\tilde{\hat{\tau}}_{BC_e} = \sum_{i=1}^{I} \gamma_i \tilde{\tau}_{BC_{ie}} = \sum_{i=1}^{I} \gamma_i (\tilde{Y}'_{ie} - \tilde{\hat{Y}}^{N\prime}_{ie}) \tag{5}$$

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

A suitable comparison group is obviously key to any research design. Here we particularly want to ensure that the pandemic-exposure index is not incidentally controlling for differences between our treatment and control that have not already been accounted for by our predictor variables. A classic example would be "anticipation effects" (a confound which seems unlikely for the pandemic). More generally, we should expect that $E[\tilde{Y}'_{zt}] = E[\tilde{Y}_{zt}]$ for all $t < 2020q1$. Fortunately, this relationship is approximately true, as can be seen in Figure B.3 of the Online Appendix, which shows the difference in outcome values before and after the pandemic correction.

The second issue—attenuation bias from 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.

Except where noted, we conduct our estimates in event time and focus on the effect in event quarter 33, when most of our treated counties had a $15 minimum wage for a full year.[36] The predictor variables for all specifications include the outcome value and total employment (both normalized to the final pre-treatment quarter) in each quarter from 2009q4–2011q4, the averages of those same during that period, and the average unemployment rate during 2009–2011. This common specification for all our synthetic control analyses makes our estimates comparable across analyses and guards against specification searching. Note that our outcome values are levels expressed as a percentage of the local value in the final pre-treatment quarter for each treated unit, making our estimated effects the percent change in the outcome value relative to the final pre-treatment quarter (net of the change seen in the synthetic control). We estimate all treatment effects and p-values using the `allsynth` package for Stata (Wiltshire, 2022a) and a companion package released with this paper that facilitates the pandemic-correction procedure: `stackscpvals`.

IV.C. Own-wage Elasticities

In addition to our estimated effects of the minimum wage policies on employment and average earnings and their elasticities with respect to the minimum wage, we also report what we view as a superior measure: the own-wage elasticity (OWE). The OWE scales the employment effect to the magnitude of the treatment effect on average earnings. The OWE equals the ratio of the

[36] Some counties reached $15 earlier. Our results are robust to excluding these counties.

employment elasticity to the wage elasticity:

$$OWE = \frac{\left(\dfrac{\%\Delta\ Employment}{\%\Delta\ Minimum\ wage}\right)}{\left(\dfrac{\%\Delta\ Average\ wage}{\%\Delta\ Minimum\ wage}\right)} \tag{6}$$

See Online Appendix E for further discussion. We calculate and report the average OWE over the final four event quarters to account for variation in the quarterly data that affects ratios of estimated effects. We then calculate 95 percent confidence intervals using the delta method.

IV.D. Estimating Effects Throughout the Wage Distribution

Using CPS data for California, we conduct an analysis of the distributional effects of large minimum wage increases, similar to the relative wage bin-by-bin analysis in Cengiz et al. (2019). In our context, where minimum wages were increased every year in both treated states, we want to avoid the post-treatment period for one increase becoming the pre-treatment period for the next. We therefore do not use the DiD stacked event study estimator approach of these earlier studies. Instead we develop a bin-by-bin analysis using stacked synthetic controls matched by wage bin in the period before the first minimum wage increase in California.[37]

While Section P.A of the Print Appendix presents our detailed methods for estimating the effects, the basic process is straightforward. First, we use the synthetic control method to estimate effects on employment shares in nominal wage bins, matching on the first half of the pre-period, as in our main estimates. Second, we difference these estimates from their values four quarters earlier. Third, we stack the estimates based on the average share of workers in each nominal wage bin that falls in the same *relative* position to a minimum wage increase.[38]

IV.E. Regression-based Methods

We complement our main synthetic control analysis with analogous DiD and SDiD regressions. For our DiD analysis, 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 also estimate these outcomes using the synthetic difference-in-differences (SDiD) estimator (Arkhangelsky et al., 2021), implemented using the `sdid` Stata package (Clarke et al., 2023). Arkhangelsky et al. (2021) report that SDiD is competitive with or dominates classic synthetic

[37] We can conduct this analysis only for all workers in California because we use hourly wage data from the CPS and we cannot identify fast food workers as a separate group.

[38] For example, the \$0-\$0.99 relative wage bin includes, among other estimates, the \$10.00-\$10.99 wage bin from 2016q1-2016q4 because the minimum wage in 2016 was \$10. It also includes \$11.00-\$11.99 wage bin from 2017q1-2017q4, since the minimum wage in 2017 was \$11.

control methods, especially with a single treated unit. However, SDiD retains little apparent advantage over synthetic control methods once the data are centered on their pre-treatment mean as in Doudchenko and Imbens (2016) and Ferman and Pinto (2021)—a procedure equivalent to removing unit fixed effects. Our data normalization procedure similarly removes differences between treated and control units in the final pre-treatment period.

Section P.B of the Print Appendix contains further details on these regression-based methods.

V. RESULTS

We first present results for fast food workers using our pandemic-corrected stacked county-level synthetic control estimator for the entire treated sample. We then examine effects on fast food in counties where the minimum wage increases were less likely to reflect local conditions, and in lower-earnings counties where the bite was greater. We supplement this discussion with our estimated effects on teens. We then examine minimum wage employment effects throughout the wage distribution.Finally, we consider the effects of successive macro conditions that interacted with different policy environments in the treated and untreated counties to produce heterogeneous confounding shocks.

V.A. Effects on Pay and Employment in All Treated Counties

Panel A of Figure IV 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 47 treated counties. Event quarter 0 indicates the first quarter of treatment—2014q1 for New York counties, and 2014q3 for California counties. Event quarter 33 is the fourth quarter in which all of the treated counties had at least a $15 minimum wage that was binding for fast food workers—2022q2 for New York, and 2022q4 for California.

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

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

Panel A of Table II quantifies these estimated effects in event quarter 33. Average earnings increased by 8.82 percent; the placebo-variance-based 95 percent confidence intervals rule out an earnings elasticity with respect to the minimum wage below 0.05. The earnings elasticity of 0.10 is comparable to the 0.15 earnings elasticity for fast food workers in Wursten and Reich (2023) and to those in other minimum wage restaurant studies.The RMSPE-based *p*-value of 0.03 indicates the earnings estimate is highly significant.

Panel A of Table II also shows that the minimum wage policies increased employment by 7.33 percent in the treated counties. The placebo-variance-based 95 percent confidence intervals rule out an employment elasticity with respect to the minimum wage below 0.03. The RMSPE-based *p*-value of 0.08 suggests our positive estimated employment effect is significant. Our associated OWE estimate of +0.69 is smaller that in Card (1992b) and higher than the 0.41 OWE for all workers in Cengiz et al. (2019).[39]

However, as we demonstrate next, these results are likely biased downward by selection and attenuation effects that partly mask large and significant positive minimum wage effects on employment.

V.B. Detecting Selection and Attenuation Effects

A neglected issue in the literature concerns whether minimum wage policies are endogenous to employment outcomes. Would estimated employment effects be more negative if the minimum wage were applied to a broader population? Dube and Lindner (2021) point out that cities that enact higher minimum wages tend to already have higher wages, suggesting that minimum wages in these places have less bite, potentially attenuating estimates of a negative employment elasticity. The same pattern applies at the state level: states with higher minimum wages also tend to have higher average wages. The inverse of this concern—also not considered in the literature—is that if employers possess market power that suppresses wages and employment, selection and attenuation biases could mask *positive* employment effects of minimum wages.

Our setting includes localities with minimum wage policies that represent responses to local labor market conditions, as well as localities that had their increases imposed on them by state governments. Our sample also includes both high-wage and low-wage counties. Figure A.2 shows some of this variation. We therefore 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 that had a binding local minimum wage in at least one locality. We present local minimum wage schedules in Table I and Online Appendix Table A.1.[40] We display our results in Figure V and in Panel B of Table II. The statistically significant 8.44 percent earnings increase in Panel B is nearly identical to our earnings estimate in Panel A, as are the 95 percent confidence intervals and the p-values.

[39]The positive own-wage elasticity in Row A of Table II also holds for our OWE estimates by state and county-size, as we discuss in Section 6.1 below.

[40]The counties with no local minimum wages accounted for 53.6 percent of fast food employment in all 47 counties in 2013.

In contrast, the positive employment effect (12.9 percent) in Panel B is 57 percent larger than in Panel A. The narrow 95 percent placebo-based confidence interval of [0.08, 0.19] rules out an employment elasticity with respect to the minimum wage below 0.08. The RMSPE p-value of 0.08 allows us to reject the null of no employment effect. The positive own-wage elasticity over the four treated quarters at $15 (1.20) is comparable to those in Card (1992b); Card and Krueger (2000); Katz and Krueger (1992)—and higher than in the more recent minimum wage literature. The employment results suggest that local minimum wage laws have been enacted selectively in places where employers have less wage-setting power. Ignoring this effect can lead to underestimates of the employment benefits of minimum wage policies.

To test for the existence of attenuation, 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 I) 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 14 (30 percent) of our 47 treated counties—the five New York City counties and the nine constituent counties of the San Francisco Bay Area.

We present these results in Panel C of Table II and Figure A.3 of the Online Appendix. The statistically significant 10.26 percent increase in earnings is 1.44 percentage points higher than in Panel A. The 11.13 percent estimated employment effect is 3.8 percentage points higher than the 7.33 percent effect for the full treated sample, yielding an own-wage elasticity of 0.90, compared to 0.69 for the full sample in Panel A. The RMSPE p-value of 0.05 allows us to confidently reject the null hypothesis of no employment effect. Indeed, the placebo-variance-based 95 percent confidence intervals rule out an employment elasticity with respect to the minimum wage below 0.06.

We next compare our estimated employment elasticities of 0.14 and 0.12 (in rows B and C of Table II) to the predicted effects implied by labor supply elasticities that we derive from separation elasticities in other minimum wage studies. These separation elasticities imply a labor supply elasticity of $\varepsilon_{L_S} = 0.46$.[41] The first-order profit maximization condition for a monopsonist implies that the ratio of the wage to the VMP_L is $\frac{1}{1+\varepsilon_{L_S}}$. In a monopsonistic setting, then, $\varepsilon_{L_S} = 0.46$ implies a markdown of 32 percent below the perfectly competitive wage. Our estimated relative wage increase of up to 10 percent lies well below this markdown; it is therefore not surprising that we do not find any disemployment effects.

Indeed, the results suggest that $15 is well below the competitive wage. A minimum wage increase up to the competitive wage would move the industry up its labor supply schedule to the point where

[41] In Manning (2011), the labor supply elasticity to a firm or industry equals twice the separation elasticity. Using minimum wage events between 2000 and 2011 and a border county pair estimator, Dube, Lester, and Reich (2016) estimate separation elasticities of 0.23 for both restaurant workers and teens. Using minimum wage events from 1990 to 2019 and a stacked event study estimator, Wursten and Reich (2023) also estimate separation elasticities of 0.23 for restaurant workers and teens, and as well for fast food workers. These separation elasticity estimates therefore imply a labor supply elasticity of 0.46.

the markdown equals zero. If $\varepsilon_{L_S} = 0.46$, and if $\frac{dw}{w} = 0.32$, then the predicted percent change in employment is $\frac{dL}{L} = 0.32 \times 0.46 = 0.15$. This predicted employment elasticity is very close to our employment elasticity estimates in Rows B and C of Table II.

The estimates in Rows B and C of Table II 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.5 displays our estimated earnings and employment effects by quartile and by the presence of local minimum wage policies.

Unsurprisingly, the minimum wage effects on weekly earnings are considerably higher in the lowest earning quartile of treated counties than in the highest quartile for both the full sample of treated counties and the restricted sample with no local minimum wages. Consistent with a monopsony model, the point estimates for employment effects are also positive and larger in the lowest wage counties than in the highest wage counties. We discuss these results in detail in the Online Appendix.

The results in Tables II reject the notion that minimum wages have more deleterious employment effects in lower-wage counties. Instead, these results indicate the opposite– monopsony power is especially present in lower-wage labor markets.

As an additional exercise, we examine minimum wage effects for the sample of counties *with* local minimum wages. In these counties, state minimum wage changes have low bites and employers likely have less wage-setting power. This exercise involves some nuance: Two of these counties (San Francisco and Santa Clara) experienced minimum wage increases in our defined "pre-treatment" period (which changes the definition of event quarter 33), and these counties collectively lack a common post-treatment event (such as reaching $15) in a particular event quarter. We nonetheless make the same assumptions for this sample as we did for our sample without local minimum wages. We obtain almost identical estimated effects on earnings and a small and positive, but not significant, effect on employment in "event quarter 33" (elasticity = 0.278, p = 0.29). These results support our finding of no disemployment effects even in counties with lower bites and less employer power.

One might be concerned that our pandemic correction procedure has arbitrarily increased our employment estimates, particularly as the positive employment effects in Figures IV and V begin at the same time as the pandemic. This effect seems unlikely, as the parameterized impact of our pandemic index is estimated using only data from our donor counties. Moreover, the right panels of Online Appendix Figure B.3 show that the impact of our pandemic-related employment correction steadily decreased after the onset of the pandemic, while the employment effects in Figures IV and V steadily increased after the onset of the pandemic.[42]

[42] The pandemic correction by itself initially lifted employment by about 10 percent, diminishing thereafter to about 5 percent by event quarter 30. This declining effect makes sense and is reassuring. In the same quarters in Figure IV, employment initially increased by 5 percent, rising to 10 percent by q30. The continuing employment increase in

We also find positive employment effects among teens that begin well before the pandemic and continue throughout the treatment period. To economize on space, we detail our methods and most results for teens in Online Appendix C. Here we simply note that our synthetic control results, displayed in Figure C.4, and our DiD, SDiD and SC estimates—presented in Table C.2—all find positive employment effects, and the synthetic control OWE of .48 for teens is similar to our OWE of 0.55 for fast food workers.

In summary, our results suggest that selection and attenuation bias were not masking negative employment consequences of minimum wages. Indeed, selection and attenuation may have had the opposite effect—masking evidence of positive employment effects in lower-wage counties. Our county-level variation thus uncovers evidence for employer power that previous studies have not detected.

V.C. Distributional Effects on All Workers

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

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

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

The left panel of Figure VI presents results through 2019q4 and the right panel through 2022q2. The horizontal axis presents $1 wage bins, from $4 below the new minimum wage (-4) to $17 or more above the new minimum wage (17+). The bars in each wage bin indicate changes in the share of all jobs in that wage bin.[43] The handles indicate 95 percent confidence intervals.

event quarters 31 to 33, when the magnitude of the pandemic correction continues to shrink, further suggests that the correction does not itself account for our positive employment results.

[43]The shares are not constrained to sum to zero because they are estimated separately (from individual synthetic

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

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

In a similar spirit, we use our standard synthetic control technique to estimate the impact of minimum wages on tenth percentile and median wages. Figure A.4 shows that minimum wages lead to substantial increases in P10 wages, but did not affect P50 wages. Taken together, these results show that we are finding effects on wages where we expect minimum wages to cause them, and nowhere else.

V.D. Heterogeneity in Three Distinct Periods

During our policy period, changing macroeconomic conditions generated a series of positive and negative shocks in both treated and donor counties. In the decade preceding the pandemic, the recovery from the Great Recession lowered unemployment rates across the U.S. At the beginning of the pandemic, labor demand and supply both contracted sharply in every part of the U.S., with local variations that depended in part on the local incidence of the pandemic's first wave.

In the economic recovery period that followed, changes in local labor market conditions varied with the local incidence of the pandemic's subsequent waves, with local variation in the introduction and then relaxation of local pandemic restrictions and with local variation in federal recovery spending, behavioral responses to the pandemic, and shifts to working from home. Unprecedentedly large federal stimulus programs, including the 2020 CARES Act and the 2021 American Relief Plan, distributed pandemic relief funds using formulas that particularly reached low-wage households in low-wage states.[44] As a result, the stimulus programs generated a rapid national economic recovery

control estimates for each wage bin following each minimum wage increase), because synthetic California can differ for each wage bin-specific estimate, and because they are average effects (over contributing quarters, weighted by the percent size of the minimum wage change).

[44]These funds included lump-sum stimulus checks of $1,200 per adult and $600 per child in April 2020 and subsequent payments of $600 and $1,400 (https://www.usa.gov/covid-stimulus-checks); uniform enhancements of $600 weekly to unemployment benefits, implying median wage replacement rates of 145 percent nationally, and higher still in food service industries and in donor states (Ganong and Vavra, 2000); and relief funds of $150 billion issued to cities and counties on a per capita basis (https://www.nlc.org/covid-19-pandemic-response/american-rescue-plan-act/arpa-local-relief-frequently-asked-questions/ARPA-info). In normal times, UI replacement rates rarely exceed 50 percent and are generally lower in our donor states.

that exceptionally raised pay in low-wage jobs, particularly so in our donor counties.

Restrictions on entry to the U.S. that affected international immigration broadly and especially affected tourist gateways, such as New York City and San Francisco, also produced considerable variation in local labor market recoveries. And, of course, minimum wages continued to increase in treated counties both before and after the onset of the pandemic.

These patterns are evident even in raw earnings data. As previously discussed, Figure III shows raw earnings data—by state, normalized to the final pre-treatment quarter—for our various treated and control counties. Prior to the onset of the pandemic, fast food worker earnings in California and New York grew together with the steady increase of minimum wages, relative to those in the donor pool. After the period of initial pandemic response, this pattern reversed and fast food wages grew relatively faster in the donor pool areas.[45]

Our pandemic-corrected stacked synthetic control approach yields minimum wage treatment effect estimates that control for the confounding effects of the initial local restrictions and behavioral response. They also exhibit differential recoveries among low-wage labor markets, depending on the minimum wage policy environment.

In particular, and conditional on the initial local pandemic response, our results (in Figures IV and V) show that, after the onset of the pandemic, fast food earnings in untreated counties rose more sharply than in treated counties (in which minimum wages continued to grow). Meanwhile, fast food employment did not decline as much in treated counties as in untreated counties; it then grew faster in treated counties as minimum wages approached and reached $15.[46]

In conjunction with the raw earnings patterns evident in Figure III, these results suggest that labor supply to the fast food sector was better-sustained and recovered more quickly in treated counties, where the financial reward for working a fast food job was higher. At the same time, fast food employers in untreated counties rapidly increased wages in response to the labor shortage they faced.

VI. ROBUSTNESS TESTS

We present in this section multiple robustness tests of our results. We begin by considering the sensitivity of our results to our preferred estimating strategy. To do so, we present uncorrected estimates using the DiD, SDiD and stacked synthetic control estimators by treated state. This

[45] Figure 9 of Autor, Dube, and McGrew (2023) shows that 10th percentile wages grew faster in 2015 through 2019 in states with their own minimum wages than in the $7.25 states. Since the middle of 2020, 10th percentile wages have grown at about the same rate in both sets of states.

[46] These estimated effects are corrected for bias resulting from local pandemic-response shocks and pairwise matching discrepancies, allowing us to isolate the effect of the minimum wage increases on our outcomes apart from those confounds. We present uncorrected results in Figures B.4 and IV. These figures show the same pattern, though they are confounded by strong pandemic responses in treated counties.

comparison serves to contrast the uncorrected estimates with our preferred corrected ones.[47] We then present the results of our analysis when broadened to include all restaurant workers—though as New York state's $15 minimum wage policy was limited to fast food workers only, we restrict this analysis to all restaurant workers in California. Finally, we discuss other post-treatment policy changes that could confound our results; we conclude that they do not.

VI.A. Regression-based Estimates for Fast Food Workers (DiD and SDiD)

We repeat here our primary analysis, estimating average treatment effects on fast food workers in event quarter 33 using DiD (Callaway and Sant'Anna, 2021) and Synthetic DiD (Arkhangelsky et al., 2021) (SDiD) estimators. These estimators do not permit a straightforward aggregate estimation across our three county groups because the control units differ for the large treated counties and the mid-size treated counties. We therefore instead estimate results separately for each of three county groups: 25 large California counties, 11 large New York counties and 11 mid-size New York counties.

We present these estimates for employment—uncorrected for the pandemic response shocks—in Table III, along with our uncorrected stacked synthetic control estimates and our pandemic-corrected stacked synthetic control estimates.[48] As they are uncorrected, the SDiD and DiD results are directly comparable to the uncorrected stacked SCM estimate. Despite the variability of the sign on the employment estimates, the results for all outcomes are highly similar, regardless of the estimator, and the confidence intervals overlap across all methods. The DiD and SDiD confidence intervals overlap even with the pandemic-corrected SCM confidence intervals—although the employment point estimates for the latter are all positive.

Online Appendix Figure A.5 plots DiD results for each quarter for the three sets of counties. These results are not corrected for pandemic effects. Although the pre-trends are somewhat noisier than in our synthetic control figures, they do not move in a consistent direction prior to the minimum wage increases. The earnings results, shown on the left-hand side of Figure A.5, indicate steadily growing effects, with narrow confidence bands in the pre-pandemic period and somewhat wider ones since.

The DiD employment results, shown on the right-hand side of Figure A.5, indicate a slightly positive pre-pandemic employment effect in the California counties and an insignificant pre-pandemic effect in the New York Counties. Employment dips with the onset of the pandemic in all three sets of counties, but much more sharply in the New York counties than in California. During our third economic period—the economic recovery from the pandemic—employment recovers in all three

[47] 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 and house price growth, and to a state-level analysis using state-level QCEW data (although the latter is less-precisely estimated). The results of these supplementary robustness tests are available upon request.

[48] The earnings estimates (available upon request) are highly consistent across all estimators, jurisdictions, and geographies.

sets of counties (decreasing slightly in mid-sized NY counties in the last year). The confidence bands are broader than in our synthetic control results.

Overall then, the results in Table III and Figure A.5 support our finding of significant positive earnings effects and employment effects of high minimum wages.

VI.B. California Restaurant Workers

We next examine whether our primary results hold when we consider the wider group of all restaurant workers in California's counties.[49] We return here to our average county-level stacked synthetic control approach using QCEW data to consider the effects on all restaurant (NAICS 722) workers in California.[50] As with our primary results, we present the synthetic control weights for Los Angeles in Columns 3 and 4 of Online Appendix Table A.4, as an example. The weighting matrix for each outcome is sparse and they together have several donors in common, while the weighting matrix for each outcome also has several donors in common with its fast food analog. The donor counties are largely located in Alabama, Georgia, North Carolina, Pennsylvania, and Texas.

Figure A.6 plots the results, showing an excellent pre-treatment fit for both outcomes. The treatment effects, presented in Panel D of Table II, are similar to those for fast food workers, if slightly moderated: average earnings grew steadily from the time the minimum wage increases began until the beginning of the pandemic, and then flattened out to reach 5.73 percent higher in 2022q4 (with an RMSPE p-value of 0.01).

Employment, meanwhile, was flat throughout the post-treatment period. Although Figure A.6 shows a visible dip in employment of 7.52 percent in 2020q4, it is followed by an increase of 4.76 percent in 2022q4; the RMSPE p-values of 0.22 and 0.14 respectively suggest none of these results are statistically significant.[51]

The pandemic-era estimates may still be biased downward by pandemic confounds, as our pandemic index (the correction procedure) works well for fast food restaurants but does not account for the greater exposure of full service restaurants to pandemic lockdowns: California reimposed a stay-at-home order between early December 2020 until late January 2021, and California full-service restaurants were either barred from opening or prevented from operating at full capacity until June 2021.

[49] In Online Appendix C we use CPS data to analyze effects on teen workers.

[50] We include 26 California counties for this analysis, as San Luis Obispo County has complete data for NAICS 722.

[51] We also examined whether minimum wages led to labor-labor substitution in restaurants. American Community Survey data show that teens made up 22 percent of all restaurant workers in 2009, 18 percent in 2014 and 22 percent in 2019. Workers with a high school degree or less made up 57 percent of all restaurant workers in 2009, 55 percent in 2013 and 53 percent in 2019. These changes do not suggest that high minimum wages led employers to substitute adults for teens or workers with more education for workers with less.

VI.C. Other Policies

California, but not New York, adopted other policies during the treatment period that could confound our interpretation of minimum wage causal effects. Specifically, California adopted a generous Earned Income Tax Credit, expanded Medicaid and access to health care via the ACA and stepped up enforcement of minimum wage laws. Cal-EITC, which was first implemented in 2015, was claimed by over 4 million California taxpayers by 2020. However, the increases were too small to substantially increase the employment of single mothers. In 2020 California expanded ACA eligibility to non-citizens aged 19 to 26 and to households with incomes as high as 600 percent of the federal poverty level. The magnitudes of these changes and the research literature on the labor market effects of Medicaid and the ACA suggests that these California policies had at most a very small effect on the low-wage labor market.

California also enhanced minimum wage enforcement activities by increasing strategic inspections and penalties for noncompliance and by partnering with community organizations. While these changes successfully prevented compliance rates from falling, the U.S. Department of Labor similarly enhanced enforcement policies in our donor states during our treatment period.

In 2020 and 2021, the federal government spent an unprecedented 10 percent of GDP on pandemic-recovery programs. Our detailed analysis of these programs suggests they may have affected fast food employment only slightly more in our donor states than in our treatment states.

We discuss each of these policies and their research literature in detail in Online Appendix D. Our broad conclusion for each of these policies: their adoptions do not affect our results.

VII. EVIDENCE OF MONOPSONY

As we described in Section II.C, minimum wages can cause positive wage and employment effects in a monopsonistic labor market. In Section V, we found such effects. In this section, we consider whether monopsony characterizes the low-wage restaurant industry. If it does, we can be more confident that overcoming monopsony power explains why minimum wages generate positive wage and employment effects.

We first test for the existence of monopsony by examining whether minimum wages reduce restaurant workers' separation rates, as a monopsony model predicts. We then examine minimum wage effects on wages, prices and the mark-ups of prices over wages, using data on a single firm: McDonald's. A monopsony model predicts that mark-ups, which correspond to profit rates, will be lower when minimum wages increase.

VII.A. Effects on Employee Separation Rates

Using the Quarterly Workforce Indicators (QWI) dataset, we examine here the causal effects of minimum wages on workers' separation rates. As Manning (2011) showed, wage increases do not affect separation rates in competitive labor markets, but do reduce separation rates in monopsonistic labor markets. Indeed, in a monopsonistic equilibrium, the separation rate determines the elasticity of labor supply to a firm as well as wage and employment markdowns.

Like the QCEW, the QWI collects wage and employment data from employers. Unlike the QCEW, the QWI collects employer-based separation rate data among all workers and among workers with less than a full year of tenure with their current employer. Since the QWI data are available only to the four-digit level, we examine minimum wage effects on all restaurant workers, not just fast-food workers.[52] We use the same stacked county-level synthetic control estimator with the QWI that we used in Section 4 with the QCEW.

We again report results for samples that include and exclude treated counties with local minimum wages. County-industry separation rates can be highly seasonal, even compared to employment, and especially for low-tenure workers. We therefore de-seasonalize the separation rates in each county using the same approach as in Peri, Rury, and Wiltshire (Forthcoming). We then proceed using the same methods as with our primary estimates.

Separation rates constitute flows, while employment levels are stocks. As a result, the pandemic shock and its aftermath produced larger shifts in separation rates than in employment, even when using the same pandemic correction method that we used with the QCEW data. We therefore report results through 2019 as well as through 2022.

We first present results for restaurant workers and then for workers 14 to 18 and 19 to 21.

1. Restaurant Workers. The upper panels of Figure A.7 presents our results for restaurant workers in counties without local minimum wage.[53] We obtain fair pre-policy fits for the separation rate outcome. We detect significant negative effects on the separation rates of all restaurant workers in the pre-pandemic period among all restaurant workers and among low-tenure restaurant workers (those not employed at the same establishment one year previously) dropping 4.3 and 19.7 percent, respectively, in the final pre-pandemic quarter. Results in 2020 through 2022 are much noisier, as expected, but still point to a negative effect of -14.5 percent among low-tenure workers in the final observed quarter.

These results, which are consistent with results using regression-based methods in Dube, Lester, and Reich (2010) 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.

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

[53]Note, that the results for the sample of all counties are very similar to those of the sample excluding counties with local minimum wage, so we present them in the appendix.

2. Teens 14–18 and Young Workers 19–21. The QWI provides data on teens 14 to 18 and on workers 19 to 21. Although very few 14 and 15-year-olds are employed, the 14 to 18 group is informative for teens who mainly live with their parents and who have not yet enrolled in higher education. The middle and bottoms panels of Figure A.7 present our results for the two age groups respectively. Separation rates of low-tenure workers declined sharply for both groups; the declines are not much affected by the pandemic and subsequent recovery. The estimates for the last observed quarter suggest statistically significant negative effects of -36.9 and -26.0 percent for teens and young workers, respectively. These results further suggest that minimum wage increases overcome monopsony power.

VII.B. Wage, Price and Mark-up Effects at McDonald's Restaurants

We examine here how minimum wages affect wages, prices and mark-ups of prices over wages 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. AJ collected these data around September 1 or so, in each of the seven years from 2016 to 2022.[54] They did not collect any data on employment.

Using all the minimum wage changes in their 2016 to 2020 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. 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 pre-treatment period (pre-2014), we apply a modified version of our county-level stacked synthetic control approach to the AJ data. Specifically, using the subset of counties with sufficient sample size in the AJ data, we compare treated counties in California and New York with their synthetic controls for fast-food industry earnings, using the weights we previously identified using QCEW data. This procedure assumes these synthetic controls are good counterfactual estimates for the McDonald's data. We then normalize each McDonald's outcome in each county to its 2016 level. The AJ data includes 31 of our large treated counties in the two states and 95 of our donor counties.[55] This approach plausibly derives a suitable counterfactual in the absence of McDonald's pre-treatment data. Indeed, using the QCEW data for just the subset of treated and donor counties observed in the AJ data does not at all affect our synthetic control weekly wage results (Figure A.8).

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

[55]We drop mid-sized New York counties and their corresponding donor pool as they are under-represented in the AJ data.

1. McDonald's Hourly Wages. Hourly wages increased on average about 50 percent in CA and NY counties from 2016 to 2022, as did the minimum wage. This wage increase relative to the donors implies a wage elasticity of 0.2. As a comparison, using QCEW industry data (reported in Table II), we obtain a wage elasticity of 0.18. Industry-level data on weekly earnings thus produce a very similar wage effect as establishment-level data on hourly earnings. However, our 0.2 wage elasticity is much smaller than the 0.7 wage elasticity reported by AJ (which ends in an earlier year).[56]

We display our synthetic control results in Figure A.9. Wages increased relative to the donors by about 20 percent from 2016 through 2019. The wage effect falls to about 11 percent higher than the donors from 2020 through 2022, possibly because of tighter labor markets in donor areas. This result is also consistent with our findings (8.82 percent) using the QCEW data, as shown in Table II . The overall wage effect is about 11 percent for all treated counties and about 15 percent when we exclude counties with local minimum wages.

2. McDonald's Prices. We consider here the extent of price pass-throughs at McDonald's stores from minimum wage increases. Price 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 less market power. Figure VIII. shows that price increases in the treated counties rose faster than in the donors, by about 4 percent from 2016 to 2019 and by about 2 percent from 2020 to 2022. Unweighted average prices show that the roughly 50 percent increase in the minimum wage resulted in an approximately 3 percent increase in prices—implying an elasticity of 0.06. This result is lower than AJ's price pass-through estimate of 0.14.

3. Mark-ups of Prices Over Wages. We discuss here how minimum wages affect the mark-up of wages to prices. In a sample of homogeneous stores of similar scale and technology, with similar expenses for non-labor inputs, mark-ups of prices over wages reflect profit margins. Since the technology and non-labor inputs used in McDonald's stores are relatively uniform across stores, as Ashenfelter has documented in several papers, capital-labor ratios and capital-output ratios are also uniform across their stores.[57] Hence our findings on mark-ups carry over to returns on capital invested.[58] Compared to the published minimum wage literature, these results thus constitute the

[56] Ashenfelter and Jurajda (2020) examines the ratio of average hourly wages to Big Mac prices, which they interpret as a measure of real wages.

[57] In addition to labor, the other main costs items for restaurants food and rent. Food price trends were likely very similar during this period in our treated and donor counties. It is likely that commercial retail rents increased more in the treated areas– and especially so in counties with their own local minimum wages. However, most commercial rent leases have multi-year terms, suggesting they likely changed more slowly than wages. (Unfortunately, good data for retail rents by county and year are not available.)

[58] Basu (2019) reviews recent literature on changes in mark-ups over costs. Much of the literature uses a constant return to scale assumption to reduce the estimation process to mark-ups over a single input—usually labor. Basu (2019)

one of the first modern studies of the effects of U.S. minimum wages on profits and profit rates.[59] That the results are consistent with a monopsony story is also remarkable.

As Figure A.10 shows, the average mark-up level in 2016 is 0.48 in our treated counties and 0.55 in our donors. Mark-ups of these magnitudes likely indicate the presence of monopsony power. In our treated counties, the mark-up of prices over wages was already smaller in 2016 in our treated counties than in our donors, indicating that minimum wage increases had already reduced employer power in our treated areas.

As Figure VIII shows, mark-ups in the treated counties, relative to the donors, fell steadily from 2016 through 2019; by about 14 percent. This squeeze of profit margins and profit rates is consistent with higher minimum wages in the treated areas overcoming monopsony power. Mark-ups in the treated areas recovered somewhat in 2020 to 2022, to about 6 percent less in the treated areas. Note also that mark-ups remained positive throughout 2016 to 2022, consistent with the absence of entry and exit effects in AJ.

4. Summary. Our synthetic control analyses of McDonald's store-level hourly wage data show that establishment-level data from a single large fast-food company produce similar wage effects as industry-level data on weekly earnings. We find only a very partial pass-through of wages to prices, suggesting that minimum wages reduce mark-ups of prices over wages. This result is consistent with the presence of monopsony power, but it is not likely in a competitive labor market. Our examination of the mark-ups provides the first test of minimum wage effects on profits in the U.S. The results confirm a prediction of the monopsony model: that minimum wage increases reduce monopsony profits.

VIII. DISCUSSION AND CONCLUSIONS

Our analysis of \$15 and higher minimum wage policies examines the effects of legislated nominal minimum wage levels and percentage increases that are considerably higher than any studied in the modern U.S. research literature. Our main sample consists of fast food workers in 47 large and mid-sized treated counties—25 in California and 22 in New York. These counties are representative of the U.S as a whole: the distribution of average county wages in 42 of these counties lies uniformly between the 10th and 90th percentiles of all U.S. counties, with only five outliers above the 90th percentile. This pattern implies our results are generalizable to jurisdictions across the U.S.

Using a stacked synthetic control method, we estimate separate minimum wage effects for each of our treated counties and then stack the county-level estimates in event time to construct a weighted average estimated effect. This strategy produces more precise estimates than does a traditional state-level synthetic control approach. It also offers advantages over regression-based estimators,

derives this condition formally.

[59]Using National Income and Products Accounts data on two-digit industries, Vergara (2022) also finds that minimum wages reduced mark-ups.

allowing us to correct for pandemic-related confounds. Using this estimating strategy, we develop a novel procedure that corrects for the confounding effects of heterogeneous pandemic-response shocks. We also develop a novel stacked synthetic control approach to estimate bin-by-bin effects of minimum wages throughout the wage distribution.

Using our full sample of 47 treated counties, our earnings and employment estimates are positive and significant. However, including counties that adopted higher local minimum wages plausibly introduces selection effects that confound these results. We re-estimate these effects after excluding counties with local minimum wages, and again after instead excluding high-wage counties that potentially introduce attenuation bias. In both cases, our earnings estimates remain stable, while our positive employment estimates increase in magnitude. Our employment estimates rule out elasticities less than +0.3, and our preferred own-wage elasticity estimate of +1.20 exceeds those in recent minimum wage studies.

We can compute the effect size implied by our elasticity estimates, using a simple back-of-the-envelope method. Fast food employment totaled about 532,000 workers in 2013 in our treated areas. Multiplying this amount by even our downward-biased 7.33 percent employment elasticity suggests that the California and New York minimum wage policies generated about 39,000 jobs in fast food alone.

We also find positive effects on teen pay, employment, hours and weekly earnings—effects which appear well before the pandemic's onset in 2020.

Our bin-by-bin analysis of minimum wage effects on all California jobs throughout the wage distribution supports our finding of no disemployment effects. The reduction in the number of jobs just below a new minimum wage is nearly exactly offset by the increase in the number of jobs just above the new standard. We do not find effects on high wage jobs, indicating that our findings do not result from uncontrolled confounders. These results for all workers, in addition to our results for full-service restaurants, teens and low-wage counties, show that the effects of high minimum wages were consistent even outside of the fast food industry.

Our finding of significant positive employment effects has precedents in the U.S. minimum wage literature. Katz and Krueger (1992) and Card and Krueger (2000), who used establishment-level data on fast food restaurant chains, also found substantial evidence of positive employment effects. Our examination of the effects of minimum wages on fast food and teen employee separation rates confirms previous studies and provides evidence of the presence of monopsony power. We examine data from over 10,000 McDonald's restaurants and obtain results consistent with pre-existing wage markdowns. Our analysis of price markups provides novel evidence for the U.S., showing that minimum wages reduce monopsony profits.

Compared to the existing U.S. minimum wage literature, our research design is better equipped to detect positive employment effects. We study policies that are higher in absolute levels, involve larger percentage increases, and create greater policy dispersion with counties in states that did not raise their minimum wages. We focus on effects in fast food and use county-level data instead of state-level data, which substantially expands precision as well as the right tail of observed minimum wage bites, as Godoey and Reich (2021) discovered. Importantly, we address and ameliorate bias from treatment selection and attenuation.

Our paper poses and addresses the question: Should we generally expect to find evidence of employer power and positive employment effects from minimum wages? Evidence from outside the minimum wage literature is consistent with our finding of employer power in fast food. For example, Lipsitz and Starr (2022) found that banning noncompete agreements increased the hourly pay of low-wage workers. And Lafontaine, Saattvic, and Slade (2023) found that the removal of no poaching clauses in some franchise contracts lifted wages 5 to 6 percent. These results, combined with the results of our bin-by-bin analysis for all workers, as well as our results for teens, suggest that our fast food results may apply in other industries as well.

In settings where employers possess wage-setting power, the monopsony model predicts that minimum wage increases up to an as yet undetermined level will increase employment; increases beyond that level could decrease employment. Our evidence demonstrates that the average effect of a large minimum wage increase to $15 in 2022 was not one of disemployment.

To conclude, our paper extends the range of causal evidence in the minimum wage literature and adds to the burgeoning literature that finds evidence of employer power in low-wage labor markets. We demonstrate that the rapid growth of minimum wages to high levels in California and New York increased earnings without reducing employment. Indeed, our evidence suggests that these minimum wage policies significantly increased employment. The evidence is strongest among counties without higher local minimum wages and whose pre-treatment earnings are representative of the rest of the country, suggesting our results may be extrapolated to counties across the U.S.

TABLE I

Minimum Wage Evolution in Areas with $15 Minimum Wages (2013-2022q1)

Locality	2013	2014	2015	2016	2017	2018	2019	2020	2021	2022
California (All Workers)	8.00	9.00*	9.00	10.00	10.50	10.50	12.00	13.00	14.00	15.00
Los Angeles (city)	8.00	9.00*	9.00	10.50*	12.00*	13.25*	14.25*	15.00*	15.00	16.04*
San Francisco	10.55	10.74	11.05/12.25†	13.00*	14.00*	15.00*	15.59*	16.07*	16.32*	16.99*
San Jose	10.00†	10.15	10.30	10.30	10.50/12*	13.50	15.00	15.25	15.45	16.20
Further California municipal minimum wages can be found in Table A.1 of the Online Appendix										
New York (All Workers)										
Nassau, Suffolk, Westchester ‡	7.25	8.00	8.75	9.00	10.00	11.00	12.00	13.00	14.00	15.00
New York City ‡	7.25	8.00	8.75	9.00	11.00	13.00	15.00	15.00	15.00	15.00
Rest of state ‡	7.25	8.00	8.75	9.00	9.70	10.40	11.10	11.80	12.50	13.20
New York (Fast Food Workers)										
New York City ‡	7.25	8.00	8.75	10.50	12.00	13.50	15.00	15.00	15.00	15.00
Rest of state ‡	7.25	8.00	8.75	9.75	10.75	11.75	12.75	13.75	14.50/15*	15.00
Other Areas (Fast Food Workers)										
Chicago	8.25	8.25	10.00*	10.50*	11.00*	12.00*	13.00*	14.00*	15.00*	15.40*
Denver	7.78	8.00	8.23	8.31	9.30	10.20	11.10	12.85	14.77	15.87
District of Columbia	8.25	9.50*	10.50*	11.50*	12.50*	13.25*	14.00*	15.00*	15.20*	16.10*
Seattle‡	9.19	9.32	11.00†	13.00	15.00	15.45	16.00	16.39	16.69	17.27

Notes: This table shows the history of minimum wages in U.S. areas that reached $15 by 2022q1. Some smaller locales, such as Flagstaff, Arizona are omitted. Table A.1 of the Online Appendix lists sub-state minimum wages in California, although we list some here as an example. Minimum wages are for the largest employer size category.
* Indicates the increase took effect in July; otherwise the increase occurred in January. Increases in New York were effective on December 31; they are entered as effective on January 1 of the following year. New York State increased its fast food minimum wage on December 31, 2020 and July 1, 2021. We include Cook County because we have not obtained QCEW data for Chicago.
† Seattle raised its minimum wage on April 1, 2015. Minimum wage level assumes employer does not provide medical benefits. San Francisco changed its minimum wage in January and May 2015. San Jose increased its minimum wage in March 2013.
‡ Multiple wage tiers depending on size of business and health insurance coverage
Sources: Vaghul and Zipperer (2021), UC Berkeley Labor Center Local Minimum Wage Inventory and the authors' research.

TABLE II

Average Effects Over Treated Counties

	Average Weekly Earnings	Employment	Own-wage Elasticity
Fast Food Workers			
A. All Treated Counties			
Treatment Effect	8.82	7.33	0.69
Elasticity	0.10	0.08	
Placebo-variance-based 95% CIs	[0.05, 0.14]	[0.03, 0.12]	[0.22, 1.17]
RMSPE-based *p*-value	0.03	0.08	
B. Excluding Counties with Local Minimum Wages			
Treatment Effect	8.44	12.87	1.20
Elasticity	0.09	0.14	
Placebo-variance-based 95% CIs	[0.04, 0.14]	[0.08, 0.19]	[0.45, 1.95]
RMSPE-based *p*-value	0.05	0.08	
C. Excluding Counties in the SF Bay Area and NYC			
Treatment Effect	10.26	11.13	0.90
Elasticity	0.11	0.12	
Placebo-variance-based 95% CIs	[0.05, 0.17]	[0.06, 0.18]	[0.34, 1.46]
RMSPE-based *p*-value	0.03	0.05	
Restaurant Workers			
D. All Treated California Counties			
Treatment Effect	5.73	4.76	0.61
Elasticity	0.07	0.05	
Placebo-variance-based 95% CIs	[0.02, 0.11]	[0.02, 0.09]	[0.10, 1.12]
RMSPE-based *p*-value	0.01	0.14	

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 our 36 large treated counties, the donor pool of control counties consists of the 122 counties with $\geq$ 5,000 employment in NAICS 722 in states that did not experience a minimum wage change since 2009. For our 11 mid-sized treated counties in New York, the donor pool of control counties consists of the 150 counties with NAICS 722 employment between 2,000 and 5,000 in states that did not experience a minimum wage change since 2009. We have a total of 47 treated counties in our primary sample of fast food workers: 25 large counties in California, plus 11 large and 11 mid-sized counties in New York. Our sample of restaurant workers is restricted to the large California counties and adds San Luis Obispo—a total of 26 counties. The large counties all have $\geq$ 5,000 employment in NAICS 722; the mid-sized counties all have between 2,000 and 5,000 employment in NAICS 722. Each treatment effect is the *average* estimated effect in the 33rd quarter after the minimum wage increase began in each jurisdiction, which in almost all cases is the first quarter with a local minimum wage of $15. For the stacked synthetic control estimates, each treatment effect is the *average* estimated difference between the (normalized to 2014q2 for California, and to 2013q4 for New York) outcome value in each treated county and its estimated synthetic control. The elasticity is calculated with respect to the treated-sample-specific average percent change in the minimum wage through event quarter 33. 95 percent confidence intervals of the elasticity are displayed in brackets and are estimated using the variance of the distribution of 100 sampled placebo average estimated effects based on estimated differences from in-space placebo treatment on the donor pool counties. Own-wage elasticity is reported as an average of estimates for each quarter of 2022. Respective confidence intervals are calculated using the delta method.

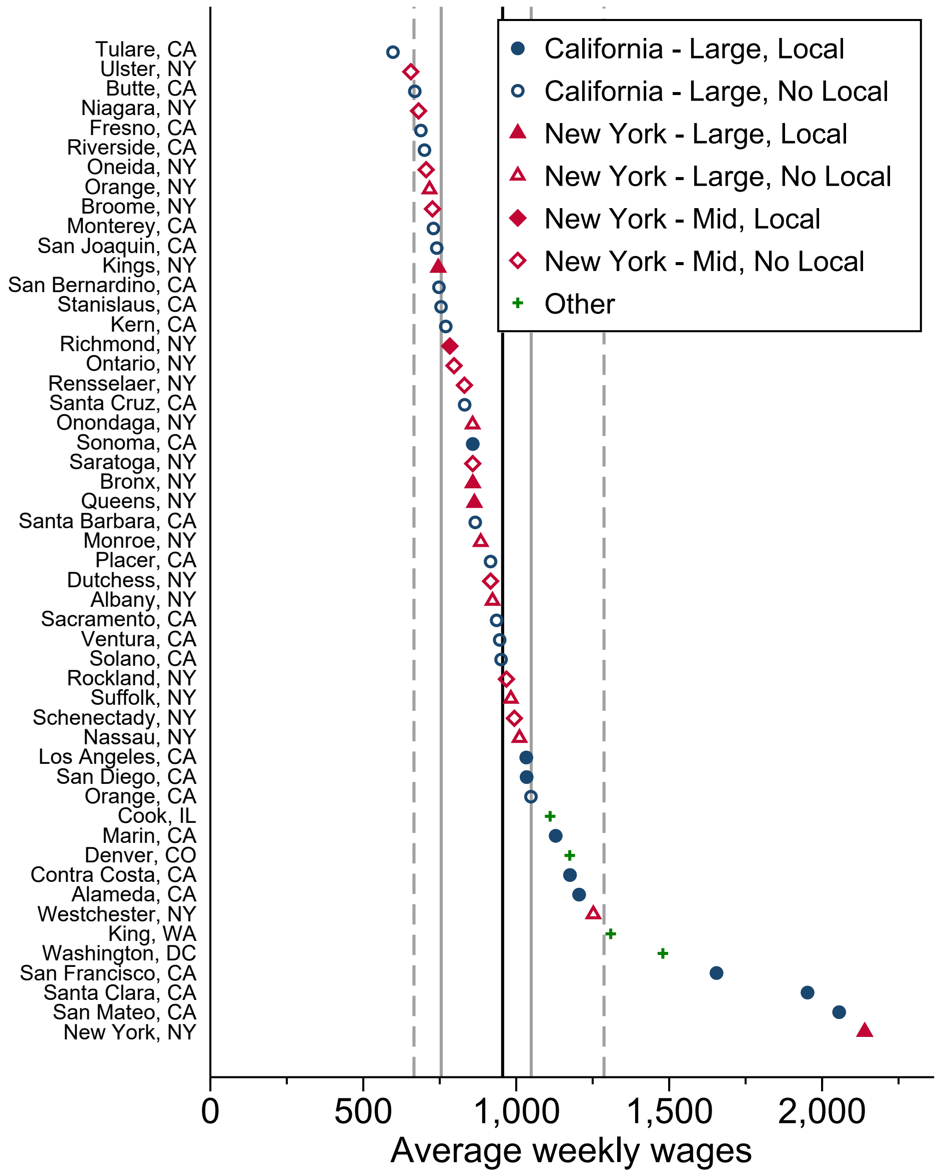
TABLE III

Employment Effects Over Treated Counties by Jurisdiction/size by Estimator

	California (large)		**New York (large)**		**New York (mid-sized)**	
	Full Sample	No Local MW Sample	Full Sample	No Local MW Sample	Full Sample	No Local MW Sample
SDiD						
A. Uncorrected						
Treatment Effect	2.98	6.64	-3.86	-2.11	-6.71	-7.41
Elasticity	0.03	0.08	-0.04	-0.02	-0.06	-0.07
Placebo 95% CIs	[-0.32, 0.39]	[-0.24, 0.39]	[-0.32, 0.24]	[-0.30, 0.26]	[-0.46, 0.33]	[-0.54, 0.40]
Placebo *p*-value	0.85	0.64	0.80	0.89	0.76	0.77
Difference-in-differences						
B. Uncorrected						
Treatment Effect	6.91	12.77	0.19	-0.58	-7.69	-9.46
Elasticity	0.08	0.15	0.002	-0.01	-0.07	-0.09
WBS CIs	[-0.05, 0.21]	[0.02, 0.30]	[-0.20, 0.21]	[-0.24, 0.23]	[-0.17, 0.03]	[-0.21, 0.02]
Stacked synthetic control						
C. Uncorrected						
Treatment Effect	6.78	9.98	-0.33	5.31	-2.89	-3.92
Elasticity	0.08	0.11	-0.00	0.05	-0.03	-0.04
Placebo 95% CIs	[0.02, 0.14]	[0.03, 0.20]	[-0.07, 0.07]	[-0.05, 0.15]	[-0.15, 0.09]	[-0.16, 0.09]
RMSPE *p*-value	0.29	0.22	0.15	0.40	0.29	0.28
D. Pandemic-corrected						
Treatment Effect	8.07	11.27	5.23	19.75	8.35	5.69
Elasticity	0.09	0.13	0.05	0.18	0.08	0.05
Placebo 95% CIs	[0.03, 0.16]	[0.06, 0.20]	[-0.03, 0.12]	[0.09, 0.28]	[-0.03, 0.19]	[-0.06, 0.16]
RMSPE *p*-value	0.15	0.12	0.43	0.02	0.48	0.73

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 25 large treated counties from California and the 11 large treated counties from New York, the donor pool consists of the 122 counties with $\geq$ 5,000 employment in NAICS 722 in states that did not experience a minimum wage change since 2009. For the 11 mid-sized treated counties in New York, the donor pool consists of the 150 counties with NAICS 722 employment between 2,000 and 5,000 in states that did not experience a minimum wage change since 2009. The large counties all have $\geq$ 5,000 employment in NAICS 722; the mid-sized counties all have between 2,000 and 5,000 employment in NAICS 722. The results are averaged in event time by jurisdiction/size over event quarter 33. The pandemic-correction procedure is specific to synthetic controls, so the SDiD and DiD estimates (Panels A and B) are *Uncorrected*, as are the synthetic control estimates in Panel C. The *Pandemic-corrected* synthetic control results in Panel D are corrected for bias due to pairwise matching discrepancies among the included predictor variables *and* pandemic confounds. 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).

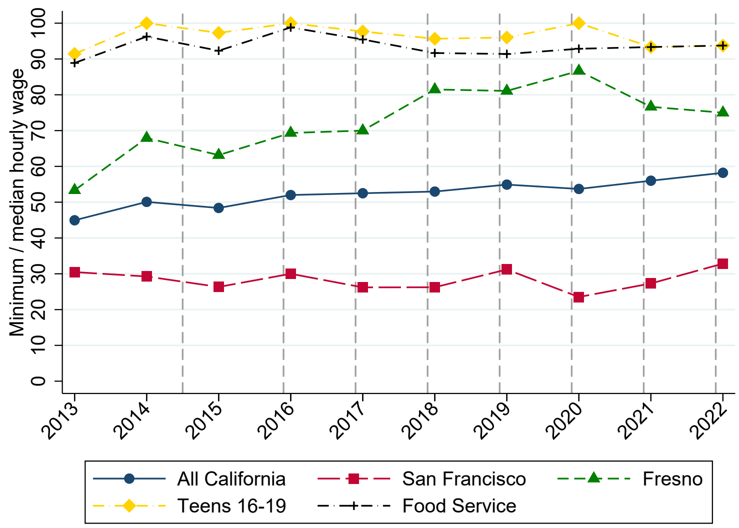
FIGURE I
Distribution of Average Wage by County



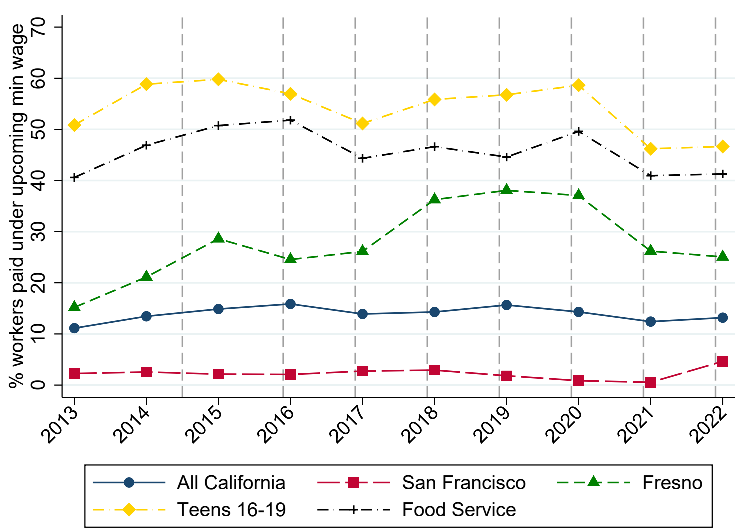
Notes: This figure shows the distribution of the employment-weighted average QCEW weekly wage across all quarters in 2013 for all industries in a given county. Treated counties are shown as individual points; their place in the national distribution is indicated by the vertical bars. The black bar shows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

FIGURE II
Reach of California Minimum Wages, 2014-2022

A. Ratio of Minimum Wage to Median Wage

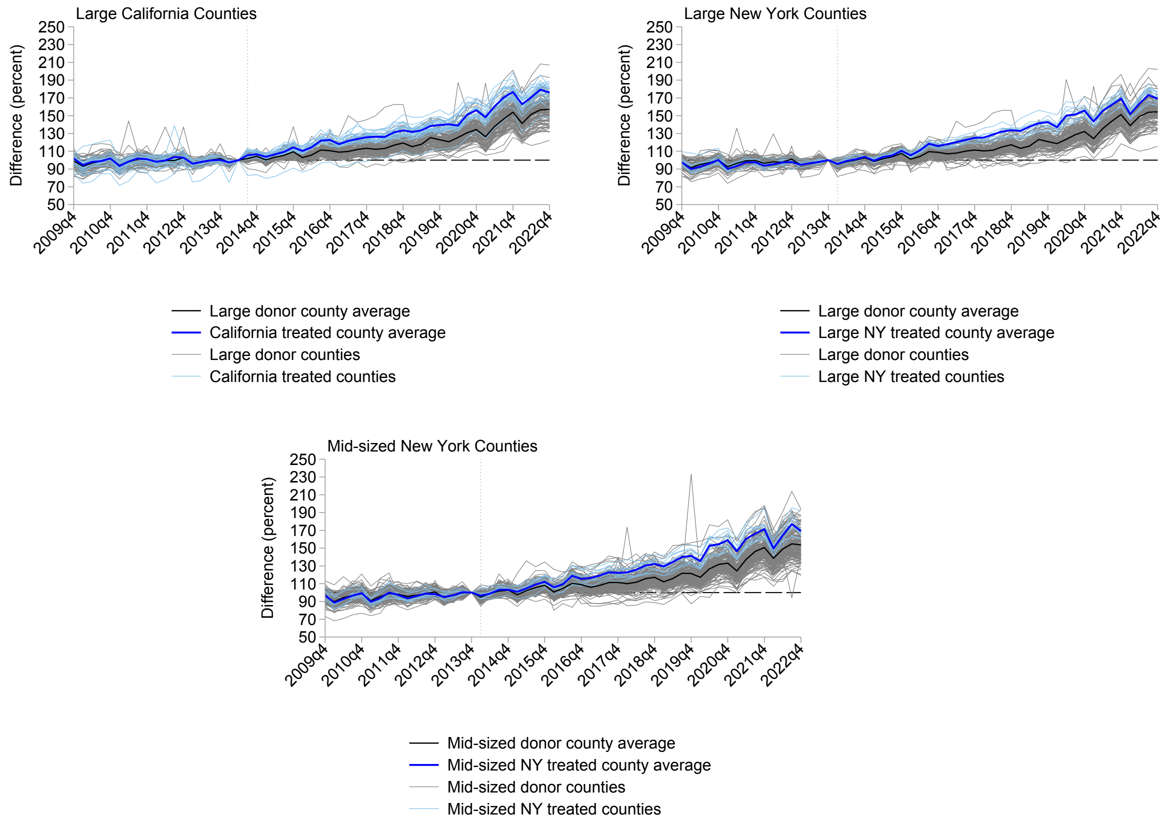


B. Fraction of Workers Earning Under the Upcoming Minimum Wage



Notes: This figure displays the reach of California's minimum wage levels. Panel A shows the ratio of the minimum wage to the median wage by year; Panel B shows the percent of workers earning wages under the upcoming minimum wage. These metrics are calculated using CPS data aggregated at the annual level. Teens are 16 to 19. Food service restricts the data to Census classification codes 8680 and 8690, which correspond to NAICS code 722. The gray vertical dashed lines indicate the timing of state-wide minimum wage increases, all of which are one nominal dollar, except for 2017 and 2018 ($0.50 each).

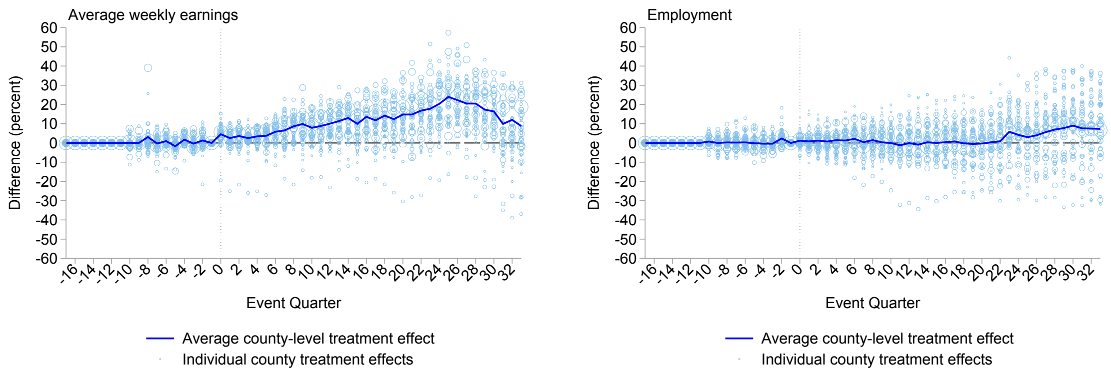
FIGURE III
Normalized Raw Average Weekly Earnings



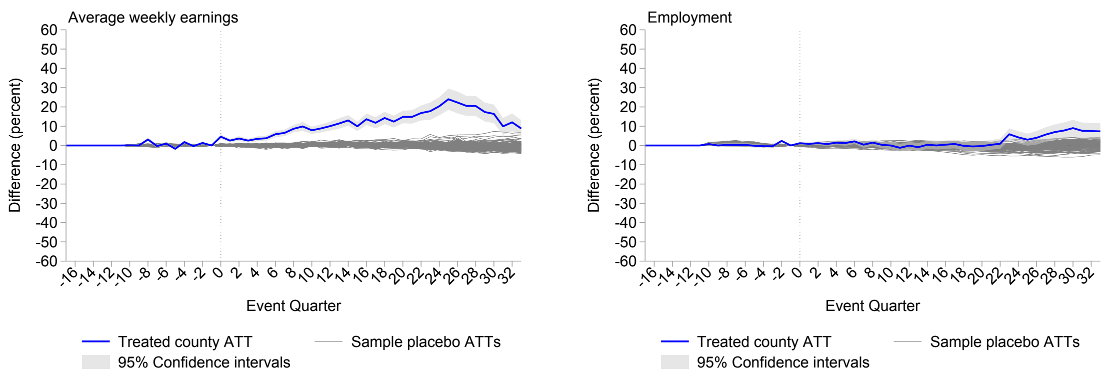
Note: Produced using employment and payroll data from the QCEW. For the large treated counties in California and New York, the donor pool consists of the 122 counties with $\geq$ 5,000 employment in NAICS 722 in states that did not experience a minimum wage change since 2009. For the mid-sized treated counties in New York, the donor pool consists of the 150 counties with NAICS 722 employment between 2,000 and 5,000 in states that did not experience a minimum wage change since 2009. In each plot the dark blue line shows average earnings in each quarter for the associated treated counties, normalized to 100 in the final quarter before the minimum wage began rising (large California counties normalized to 2014q2 in Panel A, large New York counties normalized to 2013q4 in Panel B, and mid-sized New York counties normalized to 2013q4 in Panel C) and the black line shows the average for the associated donor pool. The light blue lines show the individual treated-county normalized values, and the dark grey lines show the individual donor-pool-county normalized values. The vertical dotted line shows the initial period of treatment for the associated treated group of counties.

FIGURE IV
Treatment Effects in Full Sample of Treated Counties

A. Average and Individual Treated County Effects



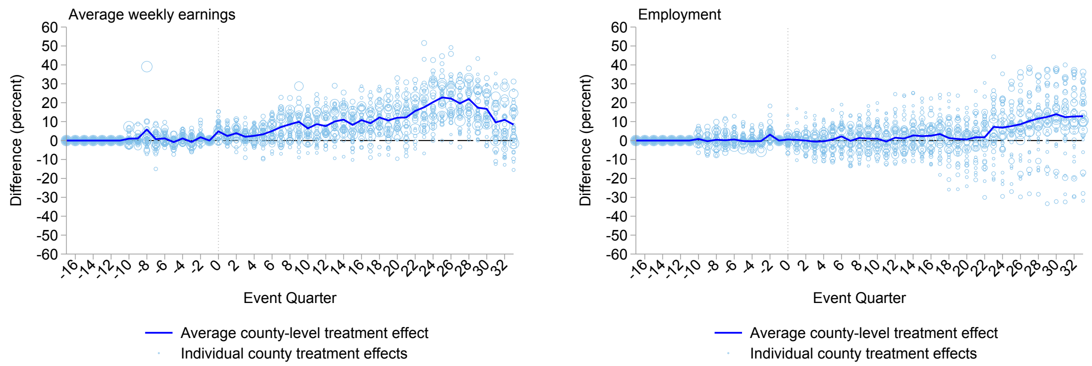
B. Average Effects in Treated Counties vs Sample Placebo Average Effects



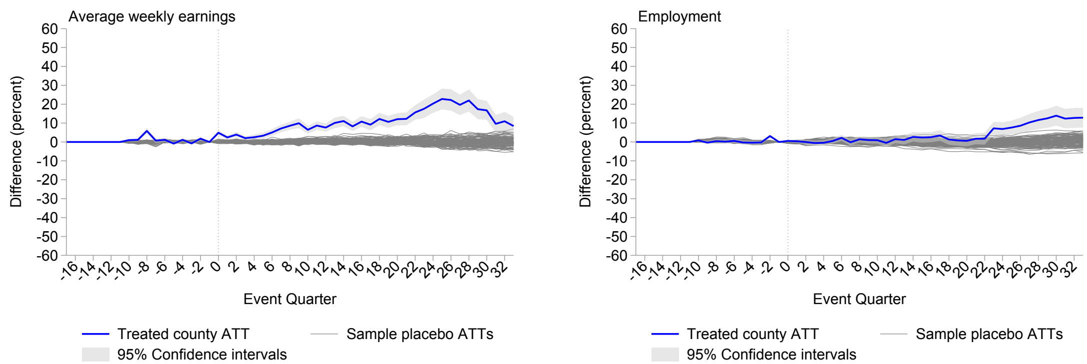
Note: Estimated using employment and payroll data from the QCEW, local unemployment data from LAUS, and Google Mobility data from Chetty et al. (2020). For our 36 large treated counties, the donor pool consists of the 122 counties with $\geq$ 5,000 employment in NAICS 722 in states that did not experience a minimum wage change since 2009. For our 11 mid-sized treated counties in New York, the donor pool consists of the 150 counties with NAICS 722 employment between 2,000 and 5,000 in states that did not experience a minimum wage change since 2009. We have a total of 47 treated counties: 25 large counties in California, plus 11 large and 11 mid-sized counties in New York. The large counties all have $\geq$ 5,000 employment in NAICS 722; the mid-sized counties all have between 2,000 and 5,000 employment in NAICS 722. 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 47 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 47 treated counties. The grey lines show 100 randomly sampled averages of 47 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 and pandemic-era confounds. The pandemic period began in event quarter 22 for California and event quarter 24 for New York.

FIGURE V
Treatment Effects in Counties *Without* Local Minimum Wages

A. Average and Individual Treated County Effects

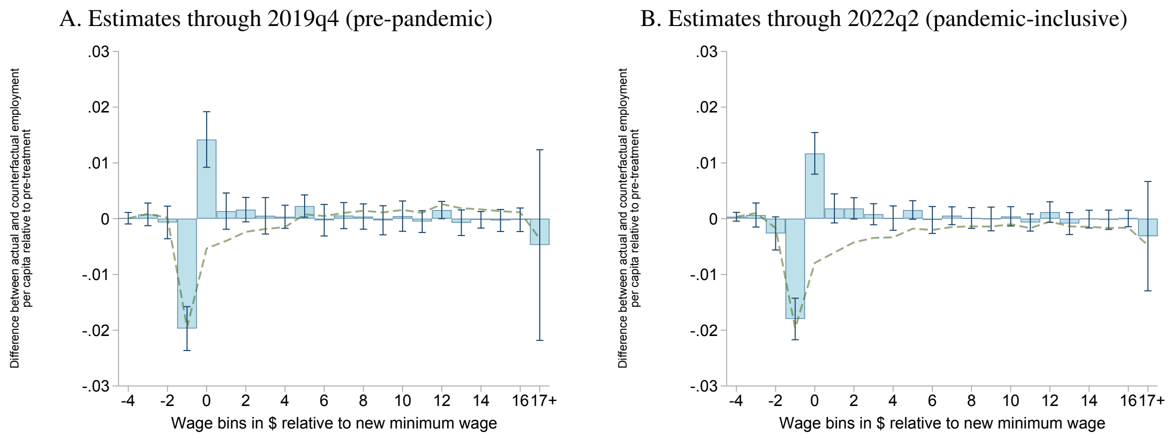


B. Average Effects in Treated Counties vs Sample Placebo Average Effects



Note: Estimated using employment and payroll data from the QCEW, local unemployment data from LAUS, and Google Mobility data from Chetty et al. (2020). For our 23 large treated counties, the donor pool consists of the 122 counties with $\geq$ 5,000 employment in NAICS 722 in states that did not experience a minimum wage change since 2009. For our 10 mid-sized treated counties, all in New York, the donor pool consists of the 150 counties with NAICS 722 employment between 2,000 and 5,000 in states that did not experience a minimum wage change since 2009. We have a total of 33 treated counties without local minimum wages: 16 large counties in California, plus 7 large and 10 mid-sized counties in New York. The large counties all have $\geq$ 5,000 employment in NAICS 722; the mid-sized counties all have between 2,000 and 5,000 employment in NAICS 722. We exclude from our primary sample those 14 treated counties where at least one municipality had a local minimum wage above the state policy. The y-axis shows the difference in each quarter between the (normalized to 2014q2 for California counties and to 2013q4 for New York treated counties) outcome variables and the associated estimated synthetic controls. In panel A, the solid blue line represents the average estimated effect across all 33 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 33 treated counties. The grey lines show 100 randomly sampled averages of 33 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 and pandemic-era confounds. The pandemic period began in event quarter 22 for California and event quarter 24 for New York.

FIGURE VI
Bin-by-Bin Estimates, All California Workers

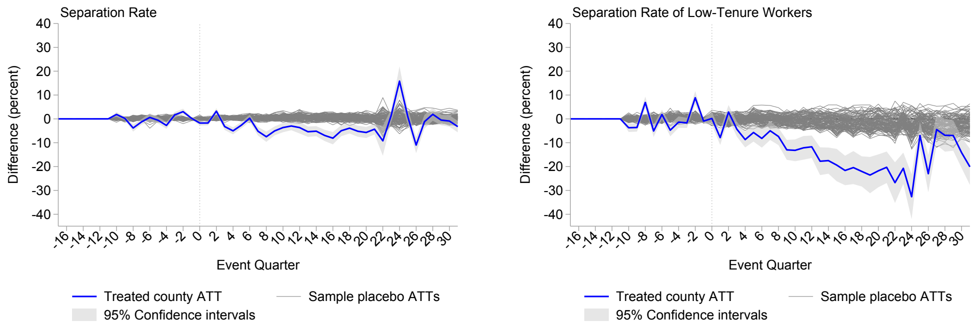


Notes: Estimated using employment and earnings data on all workers in the QCEW and local unemployment data from LAUS. The donor pool consists of 20 untreated/control states for the period ending in 2022q2. The plots show effect on the share of total employment in each relative wage bin (RWB. See Section IV and also Section P.A in the Print Appendix for details) in the year following California's minimum wage increases, for the pre-pandemic period indicated in Table P.1 (through 2019q4; on the left), and for the entire period including the pandemic (through 2022q2; on the right). The estimates consist of the combined average $1 wage bin estimates in the year following each minimum wage increase, differenced relative to the year preceding each minimum wage increase, and stacked by the relative wage bin—that is, relative to the minimum wage in that year—all weighted by the percent change in the minimum wage for each event. Handles show 95 percent confidence intervals based on the variance of 1000 draws with replacement of the placebo effects. The dashed green lines show the cumulative employment effects through the corresponding relative wage bin. The estimates are not corrected for bias from matching discrepancies or pandemic-era confounds.

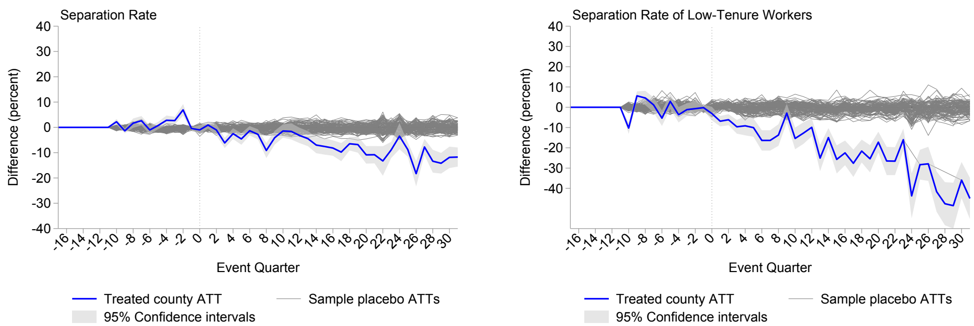
FIGURE VII

Average Effects On Separation Rates in Counties Without Local Minimum Wage

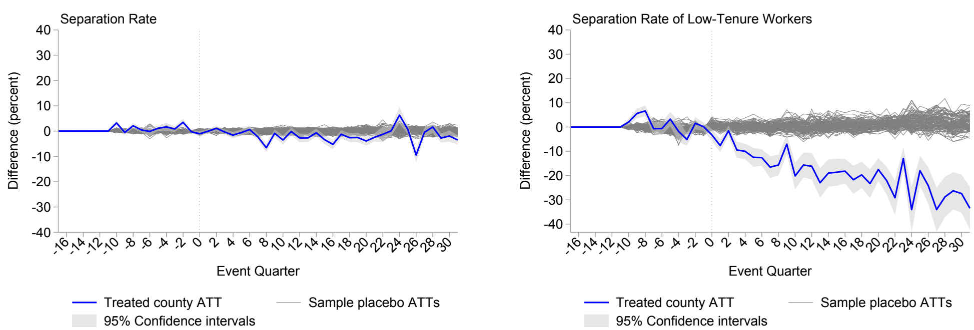
A. Restaurant Workers



B. Teen Workers



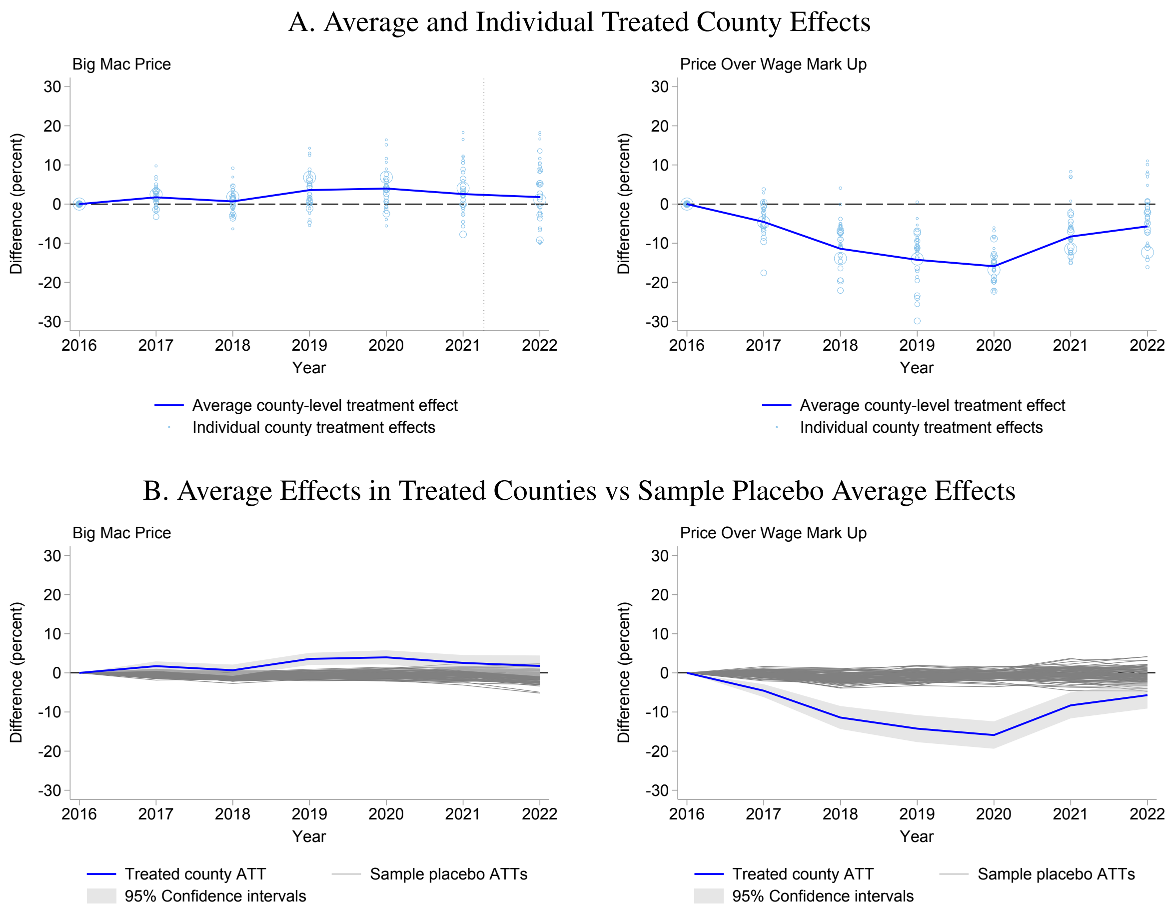
C. Young Workers



Note: Estimated using employment and separation data from the QWI, local unemployment data from LAUS, and Google Mobility data from Chetty et al. (2020). Samples of counties constructed using employment data from QCEW. Restaurant workers are identified as those employed in NAICS 7225; teen workers are aged 14-18; young workers are aged 19-21. Treated and donor pool counties are identical to the analysis presented in Figure 5. The y-axis shows the difference in each quarter between the (normalized to 2014q2 for California, and to 2013q4 for New York) outcome value and the associated estimated synthetic control. The solid blue line shows the average estimated effect across all 33 treated counties. The grey lines show 100 randomly sampled averages of 33 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 and pandemic-era confounds. The pandemic period began in event quarter 22 for California and event quarter 24 for New York.

FIGURE VIII

Estiated Effects On Big Mac Price And on Price-over-Wage Mark-up



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

PRINT APPENDIX

I. Further Methodological Details

P.A. Estimating effects throughout the wage distribution

We describe here our method for conducting an hourly wage bin-by-bin analysis of state-level effects on all Californian workers.[60] Using the CPS, we estimate separate synthetic controls for workers in each hourly wage bin in the four quarters following each discrete minimum wage increase, then stack and average the results by relative wage bin (see below for details). We restrict the data for each analysis as described in Section III. Our analysis is similar to the relative wage bin-by-bin analyses in Cengiz et al. (2019) and Wursten and Reich (2023). In our context, where minimum wages increased every year in both treated states, we want to avoid overlap between the post-treatment period for one increase and the pre-treatment period for the next. We therefore do not use the stacked event study (dynamic DiD) approach of these earlier studies. Instead, we develop a bin-by-bin analysis using stacked synthetic controls matched by wage bin in the period before the *first* minimum wage increase in California.

We develop this analysis in a series of steps: First, we use synthetic control analysis to estimate the effect on employment shares in many wage bins in each of our treatment quarters. Second, we then difference these estimates from their values four quarters previous, and stack the results for each wage bin in the four quarters following each minimum wage increase; this step allows us to estimate the average change in the share of workers in each *relative* wage bin–that is, those earning e.g. \$0.01 - \$1.00 less than the new minimum wage, \$0.00 - \$.99 more than the new minimum wage, and so on through the relative wage distribution from -\$4 through \$17+. Third, we average the effects by state for each relative wage bin.[61]

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

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

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

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

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

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

P.B. Regression-based estimators

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

$$Y_{ct} = \gamma_c + \lambda_t + \sum_{t \in T} \beta_t D_{ct} + \varepsilon_{ct} \tag{7}$$

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

We cluster standard errors at the state-level, and because of the small number of counties, we use the wild bootstrap procedure in Callaway and Sant'Anna (2021).[62] As with our synthetic control analysis, we weight by 2010 county population. Finally, we also estimate these outcomes for each of our jurisdiction/size groups using the synthetic difference-in-differences estimator (Arkhangelsky et al., 2021) with the same covariates as in our synthetic control, implemented using the `sdid` Stata package (Clarke et al., 2023). The delta method is then used to calculate the standard errors on the own-wage elasticities.

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

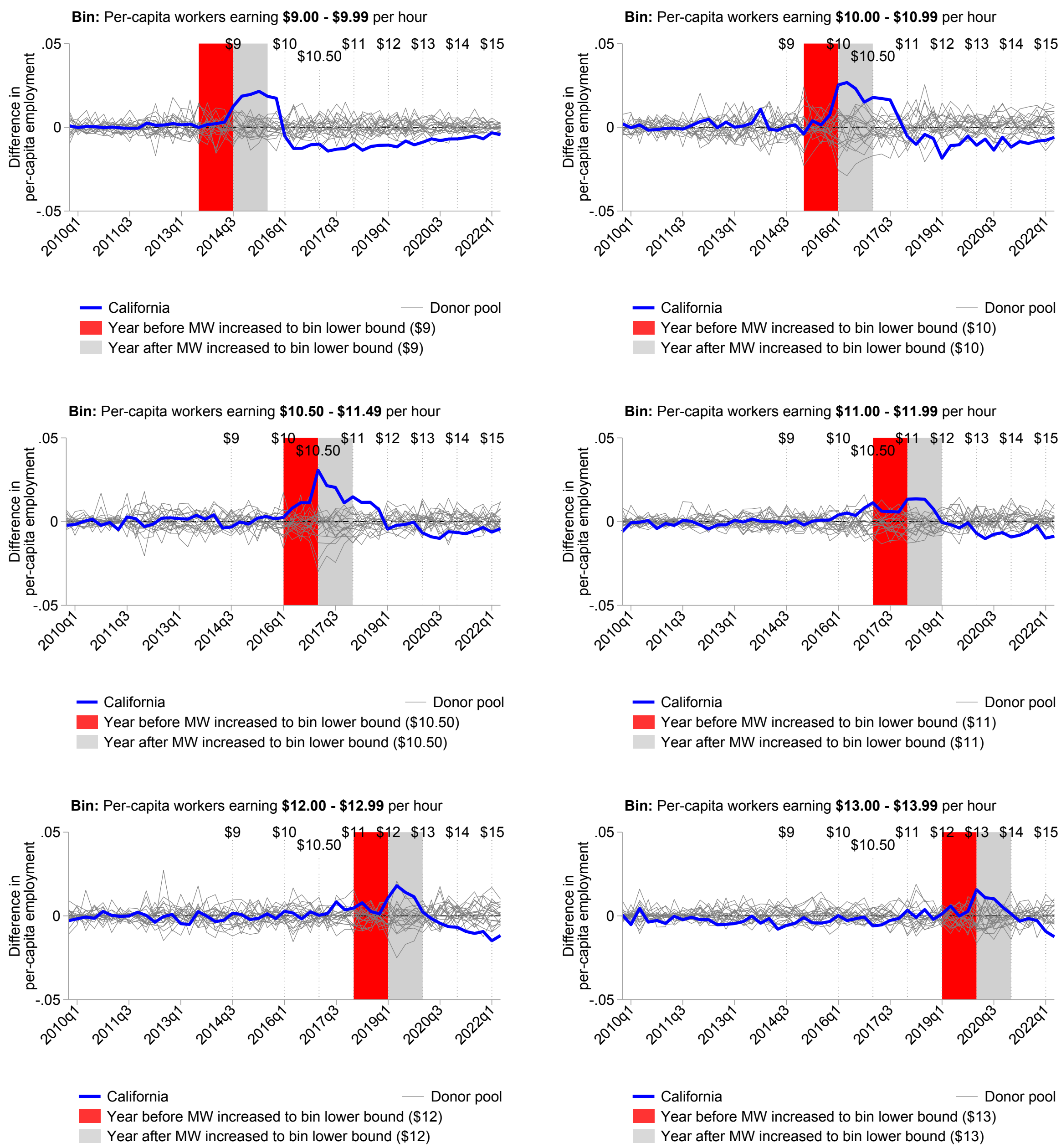
TABLE P.1
Contributing Elements to Relative Wage Bin $0–$0.99

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

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

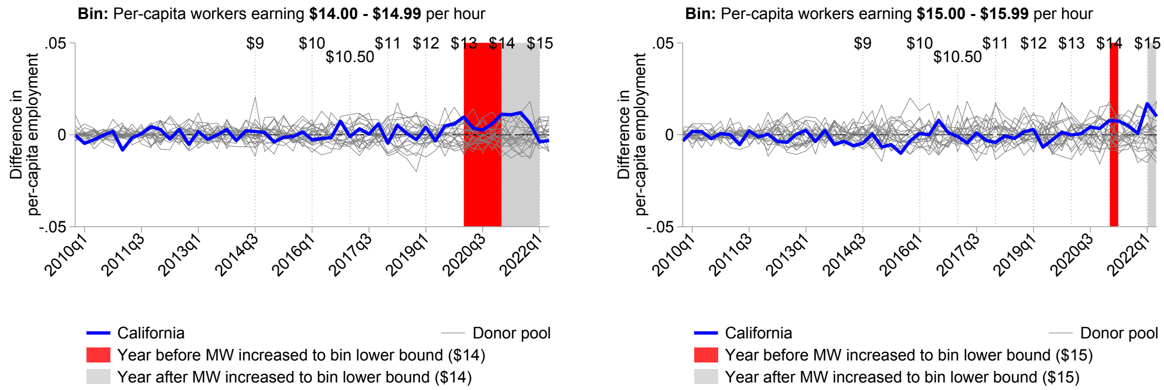
FIGURE P.1

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



Note: Continues on next page.

FIGURE P.1 – Cont'd.
Change in Wagebin-specific Employment per capita, Relative Wage Bin \$0 – \$0.99



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

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Online Appendix

for

Minimum Wage Effects and Monopsony Explanations

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A. Supplemental Tables and Figures

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

We also discuss in further detail the results by quartile in Table A.5. Specifically, Table A.5 displays our estimated earnings (Panels A and B) and employment (Panels C and D) effects by quartile and by the presence of local minimum wage policies. The first quartile denotes the counties with the lowest average earnings and the fourth quartile denotes those with the highest earnings.[1]

Unsurprisingly, the minimum wage effects on weekly earnings are considerably higher in the lowest wage quartile: 15.07 percent in Panel A of Table A.5 versus 8.82 percent in Panel A of Table II for the full sample of treated counties, and 15.00 percent in Panel B of Table A.5 versus 8.44 percent in Panel B of Table II for the sample of counties with no local minimum wages.

Consistent with a monopsony model, the point estimates for employment effects are positive and larger in the lowest wage counties than in the highest wage counties: 14.38 percent in the lowest quartile in Panel C of Table A.5, versus 6.84 percent in the highest quartile of the full sample in Panel C of Table A.5; and 14.93 percent in the lowest quartile versus 12.5 percent in the highest quartile in Panel D of Table A.5.[2] The employment effects by quartile are also uniformly larger in counties without a local minimum wage, suggesting the same selection effects as in the sub-sample estimates in Table II.

The patterns in Panel C of Table A.5 also suggest some attenuation bias. The lower bounds of the 95 percent confidence intervals on the employment elasticities in the two lowest earnings quartiles are comfortably positive: +0.07 and +0.03, respectively. The confidence intervals for employment effects in the two highest earnings quartiles in Table A.5 are not: -0.07 and -0.02, respectively.

While we find large, positive employment estimates in lower-wage quartiles, the small sample sizes for each quartile limit the statistical power and thus the confidence we can place on the estimates. This can be seen in the conservative RMSPE p-values. Moreover, the overlapping confidence intervals across the quartiles in

[1] Average county earnings vary considerably among the quartiles. For example, the lowest quartile includes Fresno County, CA, with average weekly earnings of $668, while San Francisco, with an average weekly earnings of $1,665, lies in the highest quartile. These wage differences are much greater than differences in living costs: the BEA's 2014 regional price parity index (which is normalized at 100.0 for the entire U.S.) stood at 97.1 for the Fresno metro and 112.6 for the San Francisco metro.

[2] Using exposure probabilities, Cengiz et al. (2019) test for heterogeneity among three unequal bins– the top decile, the bottom half and the rest. They find larger earnings effects in the bottom bin but do not detect differences in employment effects.

the table do not entirely rule out similar employment effects in each quartile. As the estimates in Table II are based on a well-motivated research design and larger samples, while the quartiles in Table A.5 reflect arbitrary divisions that result in smaller samples, we place greater weight on the employment results in Table II.

Still, together with the results in Table II, these results strongly suggest that, counter to the prevailing "wisdom", minimum wages do not have more deleterious employment effects in lower-wage counties. In fact, the opposite is true—a result that is most consistent with monopsony power being particularly present where wages are lowest.

TABLE A.1
Local Minimum Wages in California

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

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

TABLE A.2

Large Donor Pool Counties

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

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

TABLE A.3

Mid-sized Donor Pool Counties

Etowah, AL	Douglas, KS	Orange, NC	Angelina, TX
Lauderdale, AL	Riley, KS	Randolph, NC	Bowie, TX
Lee, AL	Shawnee, KS	Rowan, NC	Comal, TX
Marshall, AL	Wyandotte, KS	Union, NC	Ellis, TX
Bulloch, GA	Boone, KY	Wayne, NC	Grayson, TX
Carroll, GA	Boyd, KY	Wilson, NC	Guadalupe, TX
Columbia, GA	Campbell, KY	Burleigh, ND	Johnson, TX
Coweta, GA	Daviess, KY	Grand Forks, ND	Kaufman, TX
Dougherty, GA	Hardin, KY	Ward, ND	Nacogdoches, TX
Douglas, GA	Kenton, KY	Canadian, OK	Parker, TX
Fayette, GA	Mccracken, KY	Comanche, OK	Randall, TX
Floyd, GA	Madison, KY	Payne, OK	Rockwall, TX
Forsyth, GA	Warren, KY	Beaver, PA	Taylor, TX
Glynn, GA	Ascension, LA	Blair, PA	Tom Green, TX
Hall, GA	Bossier, LA	Butler, PA	Victoria, TX
Houston, GA	Livingston, LA	Cambria, PA	Wichita, TX
Lowndes, GA	Ouachita, LA	Centre, PA	Cache, UT
Rockdale, GA	Rapides, LA	Fayette, PA	Washington, UT
Whitfield, GA	Tangipahoa, LA	Franklin, PA	Eau Claire, WI
Bannock, ID	Terrebonne, LA	Lebanon, PA	Fond Du Lac, WI
Bonneville, ID	Forrest, MS	Lycoming, PA	Kenosha, WI
Canyon, ID	Jackson, MS	Mercer, PA	La Crosse, WI
Kootenai, ID	Lafayette, MS	Monroe, PA	Marathon, WI
Twin Falls, ID	Lamar, MS	Northampton, PA	Ozaukee, WI
Bartholomew, IN	Lee, MS	Washington, PA	Racine, WI
Clark, IN	Madison, MS	Berkeley, SC	Rock, WI
Delaware, IN	Rankin, MS	Dorchester, SC	St Croix, WI
Elkhart, IN	Grafton, NH	Florence, SC	Sauk, WI
Howard, IN	Merrimack, NH	Orangeburg, SC	Sheboygan, WI
Laporte, IN	Brunswick, NC	Pickens, SC	Walworth, WI
Madison, IN	Carteret, NC	Sumter, SC	Washington, WI
Monroe, IN	Catawba, NC	Anderson, TN	Laramie, WY
Porter, IN	Cleveland, NC	Blount, TN	Natrona, WY
Vigo, IN	Craven, NC	Bradley, TN	
Wayne, IN	Davidson, NC	Madison, TN	
Black Hawk, IA	Henderson, NC	Maury, TN	
Dallas, IA	Iredell, NC	Sumner, TN	
Story, IA	Johnston, NC	Washington, TN	
Woodbury, IA	Nash, NC	Wilson, TN	

Note: The mid-sized donor pool consists of the 150 counties with between 2,000 and 4,999 restaurant workers in states that did not experience a minimum wage change since 2009, and which had a continuous data series.

TABLE A.4
Donor Weights for Synthetic Los Angeles County

Positively-weighted Donor Counties	Fast Food Workers: Average Weekly Earnings	Fast Food Workers: Employment	Restaurant Workers: Average Weekly Earnings	Restaurant Workers: Employment
Montgomery, AL	0.216	0.140	0.071	0
Madison, AL	0	0	0	0.108
Tuscaloosa, AL	0.018	0	0	0.112
Cobb, GA	0.105	0.044	0	0
Muscogee, GA	0.035	0	0	0
Clarke, GA	0	0	0	0.117
Clayton, GA	0.046	0.065	0.098	0.025
Jefferson, LA	0	0.020	0	0.007
Orleans, LA	0	0.099	0	0
Harrison, MS	0	0	0	0.006
Alamance, NC	0	0.102	0	0
Forsyth, NC	0	0.174	0	0
Durham, NC	0	0.109	0.021	0.231
Gaston, NC	0.090	0	0	0
Wake, NC	0.046	0	0	0
Cabarrus, NC	0	0	0.193	0
Westmoreland, PA	0	0	0.018	0
Philadelphia, PA	0.034	0	0.125	0
Anderson, SC	0	0	0.115	0
Richland, SC	0	0	0	0.077
Lexington, SC	0	0.045	0.014	0
Spartanburg, SC	0.160	0.012	0.007	0
Davidson, TN	0	0	0.049	0
Rutherford, TN	0	0.079	0	0
Brazos, TX	0.022	0	0	0
Cameron, TX	0.046	0.040	0.179	0.101
Hidalgo, TX	0.174	0	0.087	0.064
Fort Bend, TX	0	0	0	0.129
Hays, TX	0	0.063	0	0
Dallas, TX	0.008	0	0	0
Jefferson, TX	0	0	0.014	0
Smith, TX	0	0.008	0	0.005
Brown, WI	0	0	0.009	0
Milwaukee, WI	0	0	0	0.018

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

TABLE A.5

Average Effects by County Earnings Quartile

	Quartile 1	Quartile 2	Quartile 3	Quartile 4
Average Weekly Earnings				
A. Primary Sample of All Treated Counties				
Treatment Effect	15.07	9.76	9.70	6.10
Elasticity	0.17	0.10	0.10	0.07
Placebo-variance-based 95% CIs	[0.08, 0.25]	[0.02, 0.17]	[0.03, 0.16]	[0.004, 0.14]
RMSPE *p*-value	0.01	0.05	0.03	0.10
B. Excluding Counties with Local Minimum Wages				
Treatment Effect	15.00	10.02	11.62	1.50
Elasticity	0.16	0.11	0.12	0.02
Placebo-variance-based 95% CIs	[0.07, 0.25]	[0.02, 0.21]	[0.03, 0.21]	[-0.06, 0.09]
RMSPE-based *p*-value	0.01	0.01	0.04	0.81
Employment				
C. Primary Sample of All Treated Counties				
Treatment Effect	14.38	10.77	0.69	6.84
Elasticity	0.16	0.11	0.01	0.08
Placebo-variance-based 95% CIs	[0.07, 0.25]	[0.03, 0.18]	[-0.07, 0.08]	[-0.02, 0.18]
RMSPE *p*-value	0.22	0.15	0.54	0.29
D. Excluding Counties with Local Minimum Wages				
Treatment Effect	14.93	15.37	8.65	12.5
Elasticity	0.16	0.17	0.09	0.13
Placebo-variance-based 95% CIs	[0.05, 0.28]	[0.06, 0.29]	[-0.02, 0.20]	[0.02, 0.23]
RMSPE-based *p*-value	0.26	0.38	0.46	0.18

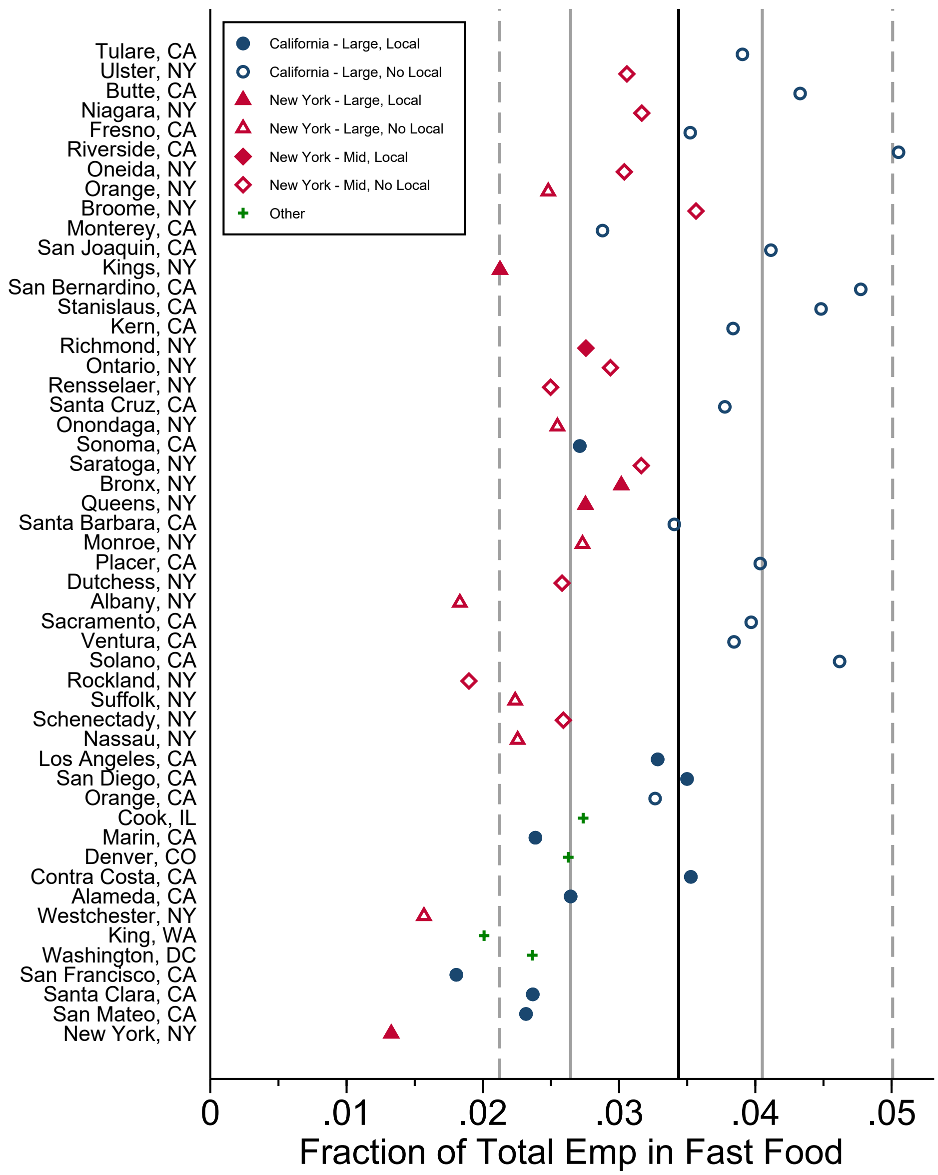
Note: Estimated using employment and payroll data from the QCEW, local unemployment data from LAUS, and Google Mobility data from Chetty et al. (2020). For our 37 large treated counties, the donor pool consists of the 122 counties with $\geq$ 5,000 employment in NAICS 722 in states that did not experience a minimum wage change since 2009. For our 11 mid-sized treated counties in New York, the donor pool consists of the 150 counties with NAICS 722 employment between 2,000 and 5,000 in states that did not experience a minimum wage change since 2009. We have a total of 47 treated counties in our primary sample: 25 large counties in California, plus 11 large and 11 mid-sized counties in New York. We have a total of 33 counties in our sample with no local minimum wages: 16 large counties in California, plus 7 large and 10 mid-sized counties in New York. The large counties all have $\geq$ 5,000 employment in NAICS 722; the mid-sized counties all have between 2,000 and 5,000 employment in NAICS 722. Treated counties are broken into quartiles according to average weekly earnings. Each treatment effect is the *average* estimated difference—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—between the (normalized to 2014q2 for California, and to 2013q4 for New York) outcome value in each treated county and its estimated synthetic control. The elasticity is calculated with respect to the average population-weighted percentage change in the minimum wage among treated counties in each quartile through between 2013q4 and 2022q4. 95% confidence intervals of the elasticity are displayed in brackets and are estimated using the variance of the distribution of 100 sampled placebo average estimated effects based on estimated differences from in-space placebo treatment on the donor pool counties.

TABLE A.6
Average County Effects For McDonald's Establishments

	Average Hourly Wage	Price	Mark-Up
Pre-Covid Effect (2016-2020)			
C. All Treated Counties			
Treatment Effect	25.54	3.97	-15.89
Elasticity	0.51	0.08	-0.32
Placebo-variance-based 95% CIs	[0.40, 0.62]	[0.04, 0.12]	[-0.39, -0.25]
D. Excluding Counties With Local Minimum Wages			
Treatment Effect	24.14	3.86	-15.28
Elasticity	0.48	0.08	-0.31
Placebo-variance-based 95% CIs	[0.38, 0.59]	[0.04, 0.012]	[-0.38, -0.23]
Whole Period Effect (2016-2022)			
A. All Treated Counties			
Treatment Effect	11.3	1.78	-5.69
Elasticity	0.23	0.04	-0.11
Placebo-variance-based 95% CIs	[0.11, 0.34]	[-.02, 0.09]	[-0.18, -0.05]
B. Excluding Counties With Local Minimum Wages			
Treatment Effect	15.1	4.45	-5.87
Elasticity	0.30	0.09	-0.12
Placebo-variance-based 95% CIs	[0.17, 0.43]	[0.02, 0.16]	[-0.18, -0.05]

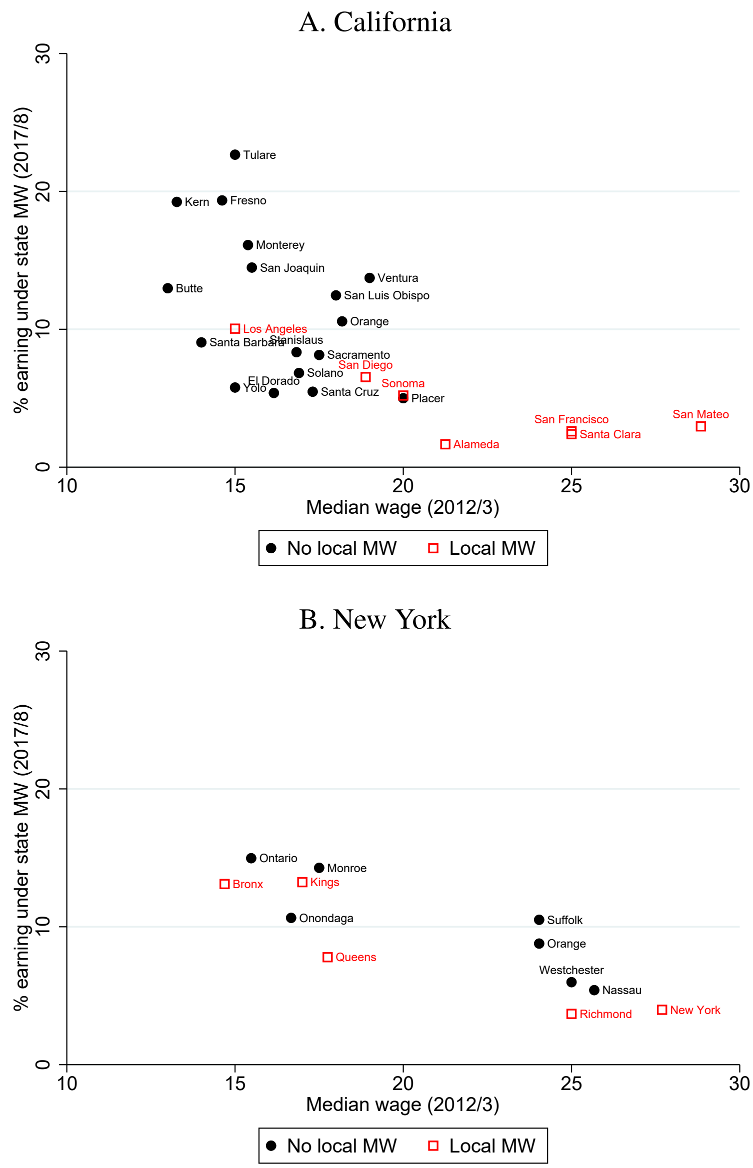
Note: Estimated using McDonald's data from the Ashenfelter and Jurajda (2020). Sub-sample includes 31 large treated counties, and the donor pool consists of 95 counties with $\geq$ 5,000 employment in NAICS 722 in states that did not experience a minimum wage change since 2009. We have a total of 21 large counties in California, plus 10 large counties in New York. The large counties all have $\geq$ 5,000 employment in NAICS 722. Each treatment effect is the *average* estimated effect in the 31st 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 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 event quarter 31. 95 percent confidence intervals of the elasticity are displayed in brackets and are estimated using the variance of the distribution of 100 sampled placebo average estimated effects based on estimated differences from in-space placebo treatment on the donor pool counties.

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



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

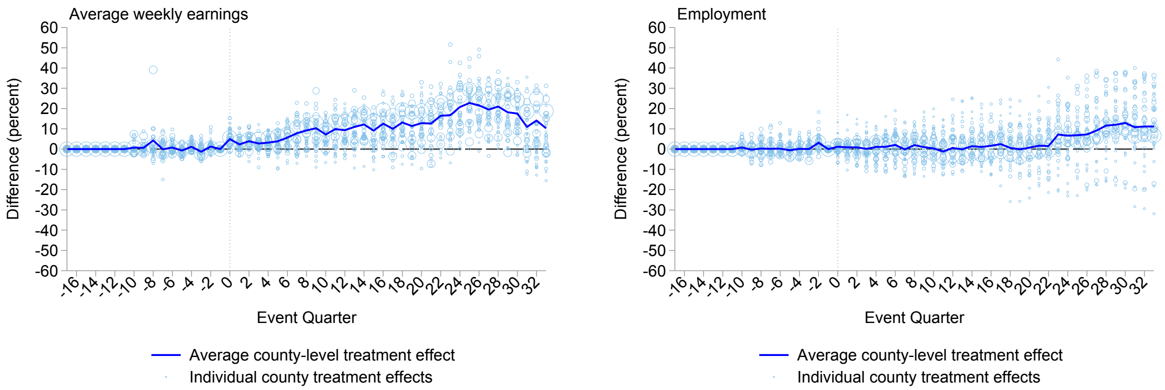
FIGURE A.2
County-level Exposure to State Minimum Wages



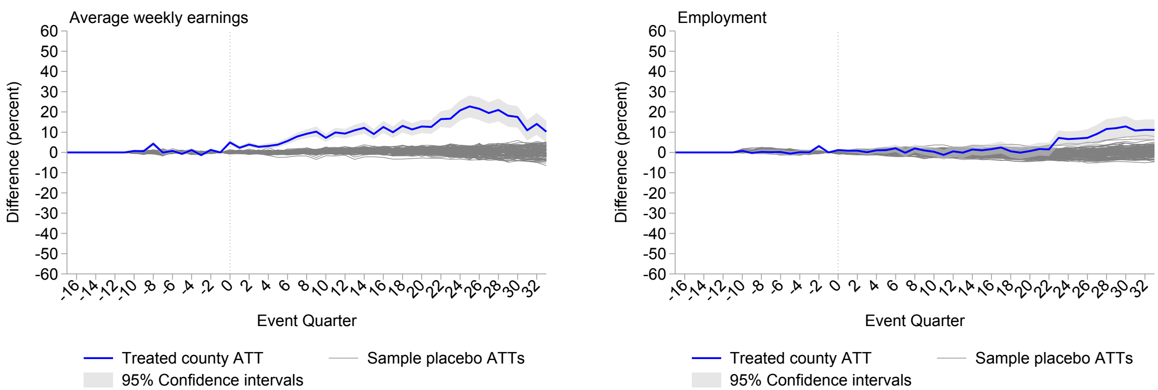
Notes: The figures above plot two measures of county-level exposure to minimum wages. The horizontal axes show the median wage in the two years before the first minimum wage increase. The vertical axes show the average percent earning under the upcoming minimum wage in 2017 and 2018. Years were pooled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

FIGURE A.3
Effects Excluding Counties in the SF Bay Area and NYC

A. Average and Individual Treated County Effects

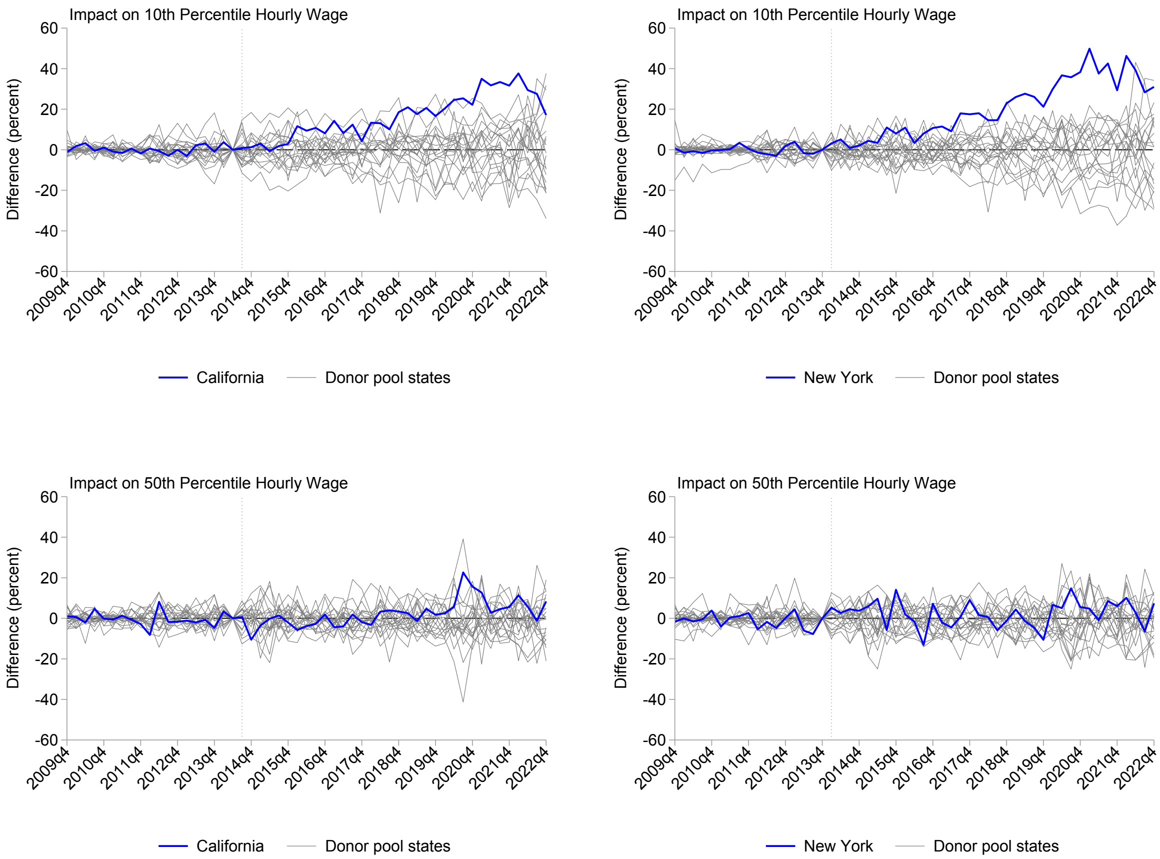


B. Average Effects in Treated Counties vs Sample Placebo Average Effects



Note: Estimated using employment and payroll data from the QCEW, local unemployment data from LAUS, and Google Mobility data from Chetty et al. (2020). For our 23 large treated counties, the donor pool consists of the 123 counties with $\geq$ 5,000 employment in NAICS 722 in states that did not experience a minimum wage change since 2009. For our 10 mid-sized treated counties, all in New York, the donor pool consists of the 150 counties with NAICS 722 employment between 2,000 and 5,000 in states that did not experience a minimum wage change since 2009. We have a total of 33 treated counties without local minimum wages: 16 large counties in California, plus 7 large and 10 mid-sized counties in New York. The large counties all have $\geq$ 5,000 employment in NAICS 722; the mid-sized counties all have between 2,000 and 5,000 employment in NAICS 722. We exclude from our primary sample those 14 treated counties in the San Francisco Bay Area and New York City. The y-axis shows the difference in each quarter between the (normalized to 2014q2 for California counties and to 2013q4 for New York treated counties) outcome variables and the associated estimated synthetic controls. In panel A, the solid blue line represents the average estimated effect across all 33 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 33 treated counties. The grey lines show 100 randomly sampled averages of 33 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 and pandemic-era confounds. The pandemic period began in event quarter 22 for California and event quarter 24 for New York.

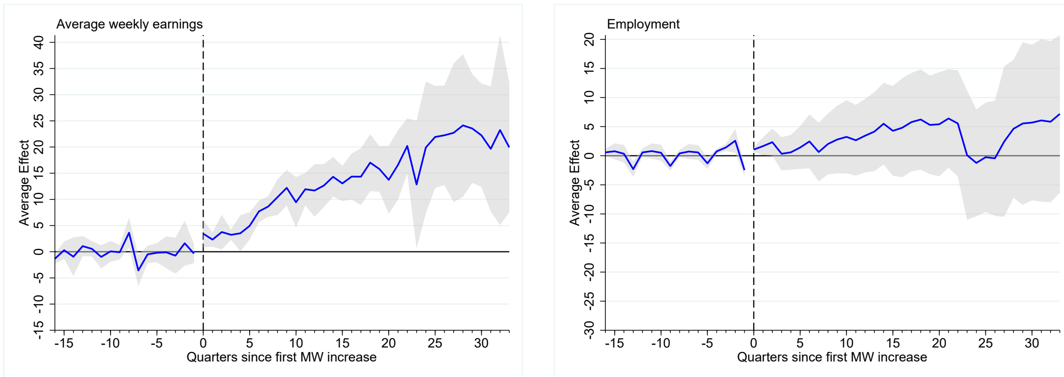
FIGURE A.4
Minimum Wage Impacts on 10th and 50th Percentile Hourly Wage, All Workers, California and New York



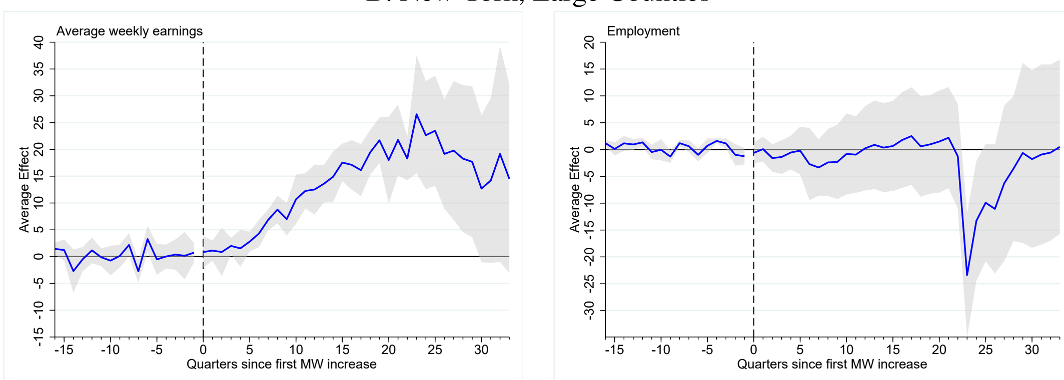
Note: Estimated using employment and earnings data on all workers in the CPS and local unemployment data from LAUS. The donor pool consists of 20 untreated/control states for the period ending in 2022q4. The top and bottm panels' the y-axis shows the estimated difference in each quarter for, respectively, the normalized 10th and 50th percentile hourly wage between each state and its estimated synthetic control for California (left) and New York (right). The vertical dotted line indicates the first quarter of treatment.

FIGURE A.5
Differences-in-differences estimates by size and state

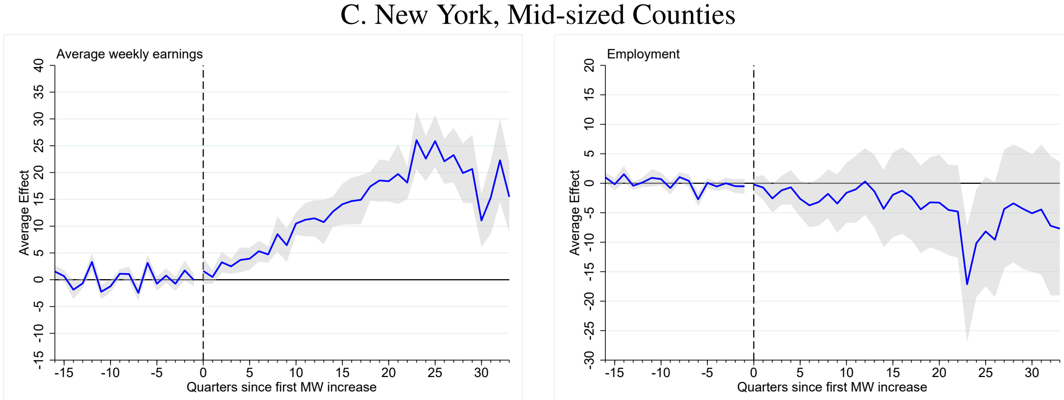
A. California



B. New York, Large Counties



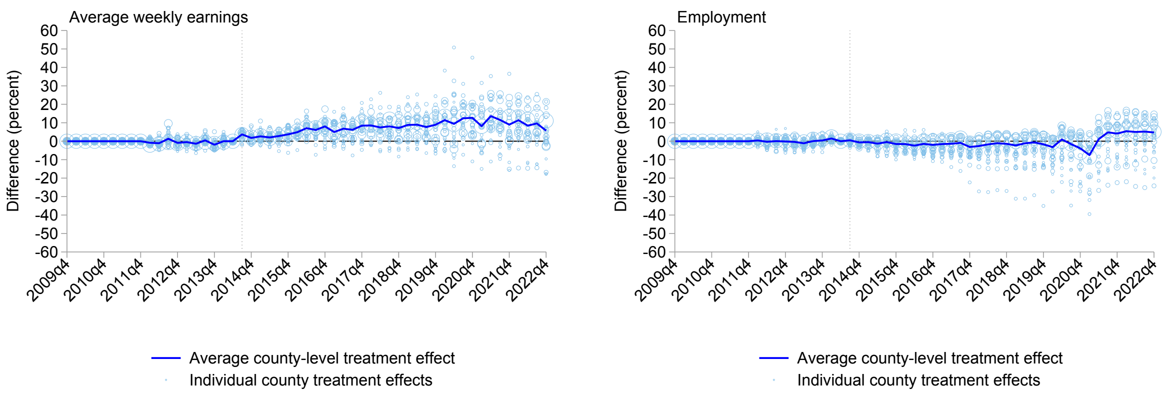
C. New York, Mid-sized Counties



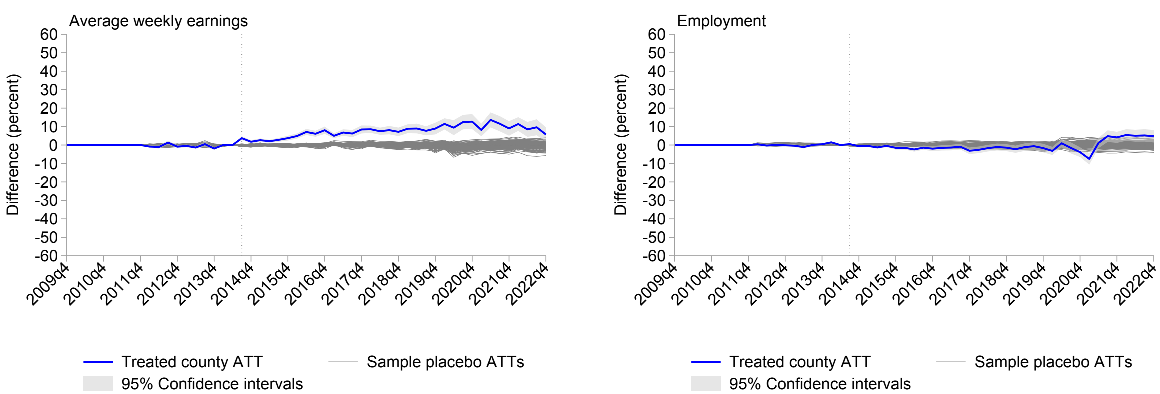
Note: The figures above plot the point estimates and confidence intervals for the β_t in Equation 6 calculated using the estimator described in Callaway and Sant'Anna (2021). Following Callaway and Sant'Anna (2021), the pre-period coefficients are reported relative to the immediately proceeding period, and the post-period coefficients relative to the last pre-treatment quarter. Standard errors, displayed in gray, are 95% confidence intervals estimated using wild bootstrap. The pandemic period began in event quarter 22 for California and event quarter 24 for New York.

FIGURE A.6
Effects for California Restaurant Workers

A. Average State and Individual County Effects



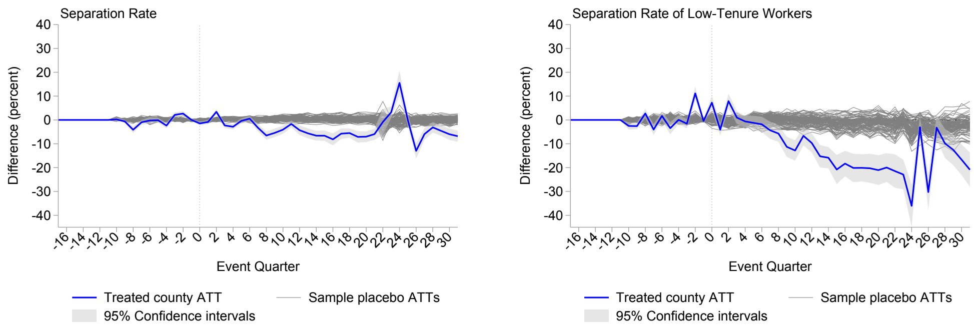
B. Average Effects in California Counties vs Sample Placebo Average Effects



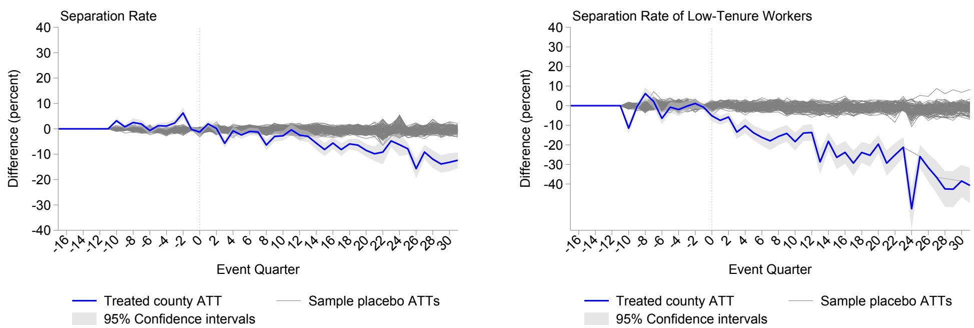
Note: Estimated using employment and payroll data from the QCEW and local unemployment data from LAUS, and Google Mobility data from Chetty et al. (2020). The donor pool consists of the 123 counties with $\geq$ 5,000 employment in NAICS 722 in states that did not experience a minimum wage change since 2009. We have 26 treated California counties (the 25 from our fast food analysis plus San Luis Obispo)—all of those with $\geq$ 5,000 employment in NAICS 722. The y-axis shows the difference in each quarter between the (normalized to 2014q2) outcome value and the associated estimated synthetic control. In panel A, the solid blue line represents the average estimated effect across all 26 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 26 treated counties. The grey lines show 100 randomly sampled averages of 26 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 vertical dotted line indicates the first quarter of treatment.

FIGURE A.7
Average Effects On Separation Rates in Treated Counties vs Sample Placebo Average Effects

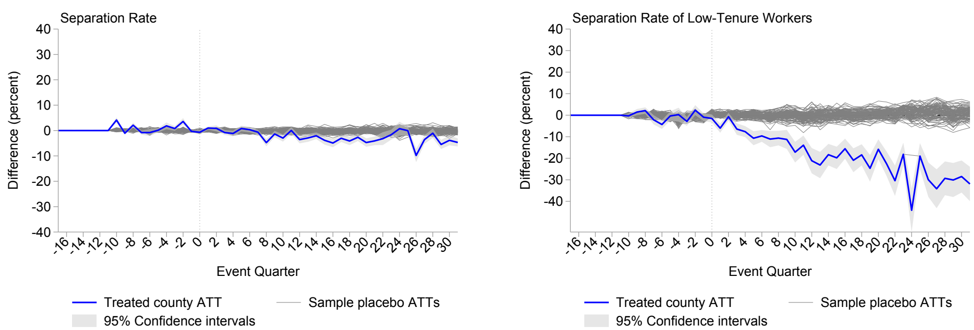
A. Restaurant Workers



B. Teen Workers



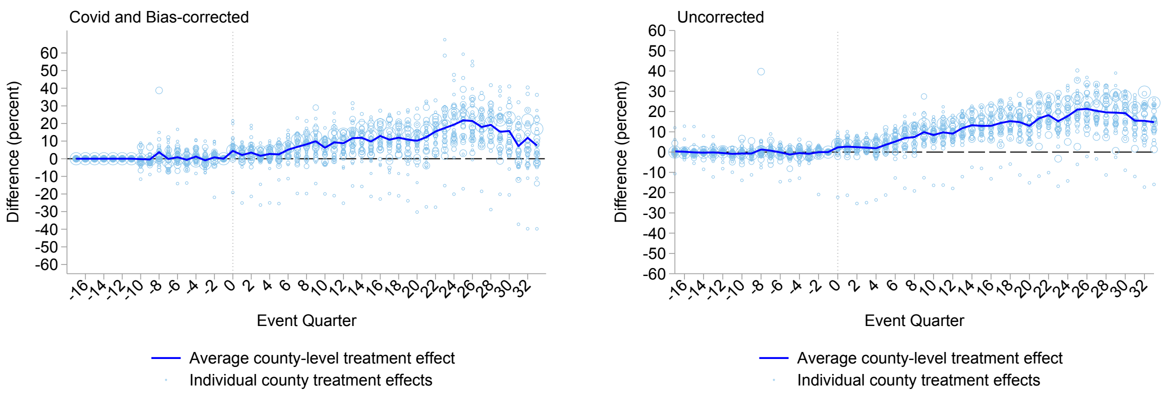
C. Young Workers



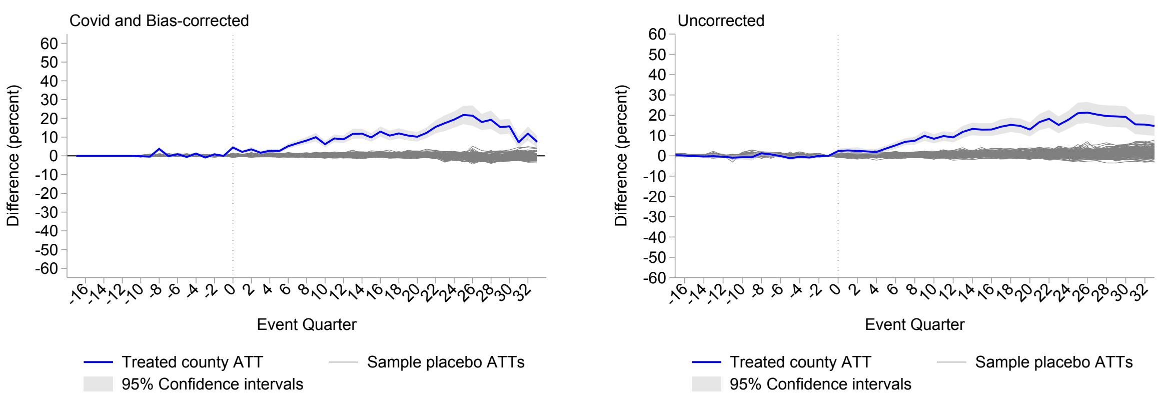
Note: Estimated using employment and separation data from QWI, local unemployment data from LAUS, and Google Mobility data from Chetty et al. (2020). Samples of counties constructed using employment data from QCEW. Restaurant workers are identified as those employed in NAICS 7225; teen workers are aged 14-18; young workers are aged 19-21. Treated and donor pool counties are identical to the analysis presented in Figure 4. The y-axis shows the difference in each quarter between the (normalized to 2014q2 for California, and to 2013q4 for New York) outcome value and the associated estimated synthetic control. The solid blue line shows the average estimated effect across all 47 treated counties. The grey lines show 100 randomly sampled averages of 47 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 and pandemic-era confounds. The pandemic period began in event quarter 22 for California and event quarter 24 for New York.

FIGURE A.8
Effect on Average Weekly Earnings Using hte McDonald's Sub-sample Of Counties

A. Average and Individual Treated County Effects



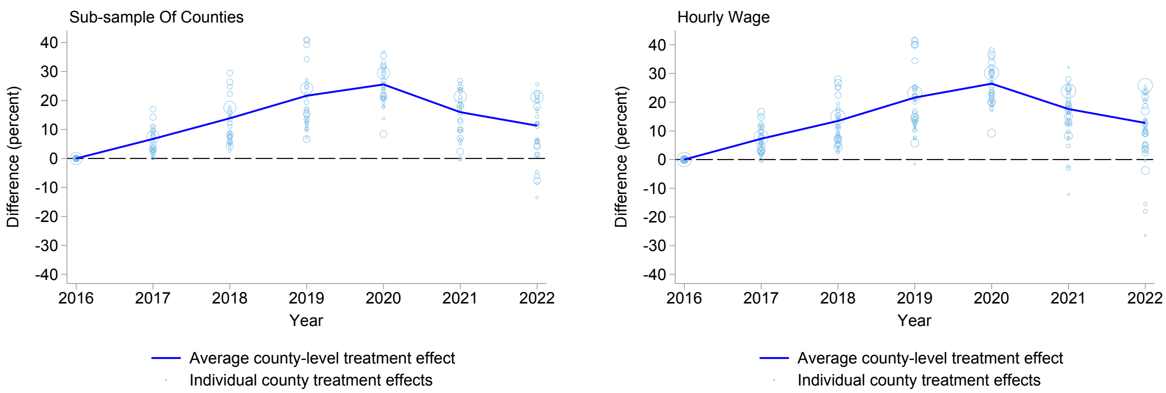
B. Average Effects in Treated Counties vs Sample Placebo Average Effects



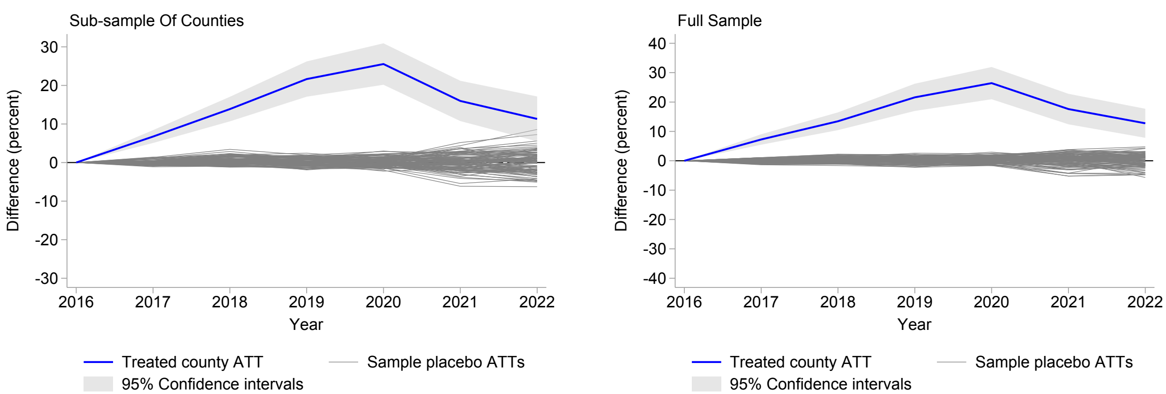
Note: Estimated using employment and payroll data from the QCEW, local unemployment data from LAUS. Sample includes 31 large treated counties, the donor pool consists of 95 counties with $\geq$ 5,000 employment in NAICS 722 in states that did not experience a minimum wage change since 2009. We have a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

FIGURE A.9
Effect On McDonald's Hourly Wages

A. Average and Individual Treated County Effects

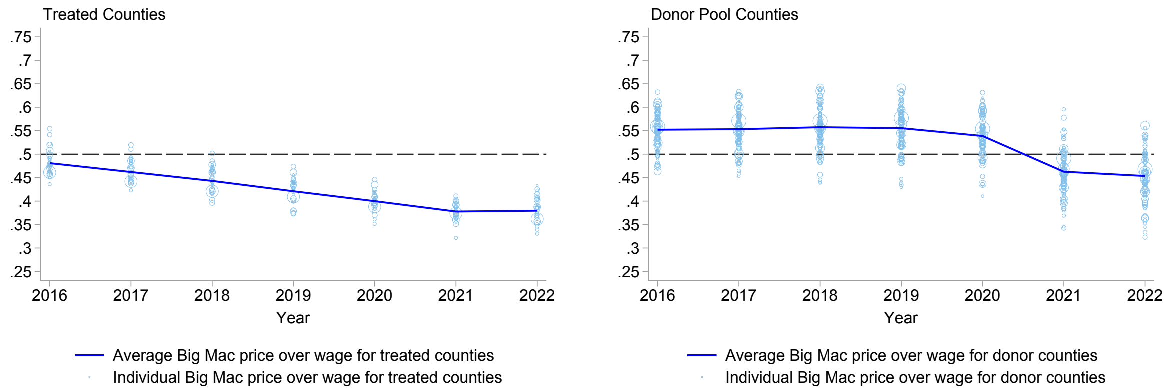


B. Average Effects in Treated Counties vs Sample Placebo Average Effects



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

FIGURE A.10
Average and Individual Raw Price-over-Wage Markups by Treatment Status



Note: Produced using McDonald's data from Ashenfelter and Jurajda (2020). The sample includes 31 large treated counties, and 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. We have a total of 21 large counties in California, plus 10 large counties in New York. The large counties all have $\geq$ 5,000 employment in NAICS 722. The y-axis shows the average ratio of the price of Big Mac divided by the average hourly wages at McDonalds' establishments by county by year. The solid blue line represents the average markup across all 31 treated counties, weighted by 2010 population, and the light blue circles show the individual levels for each contributing county in each time period; the size of the circle represents the relative 2010 population. The results are averaged by year, starting in 2016, with the year 2014 (not included in the graph) being the year of treatment.

B. Pandemic Confounds and Correction

B.A. Pandemic-response index

We construct the pandemic response index primarily to understand the trajectory of earnings and employment during the years in which the pandemic was at its height. In every period from 2021q3 on, our estimates match in sign.

It is difficult to choose a parsimonious way to control for the myriad effects of the pandemic and the government response on a county-by-county level. One might, for instance, control for the length of the lockdown in each county, but lockdowns and stay-at-home orders did not create the same limits on restaurant capacity in every county, nor were the statutes enforced with equal zeal. Even a complete and detailed legal account would not capture differences in the severity and timing of infections, vaccine take-up, or the cultural response to the disease. Accordingly, we opt to control for what people actually did, which implicitly accounts for the full set of exogenous shocks just described.

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

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

Figure B.1 shows the evolution of the retail and restaurants and workplace sub-indices over time. Panels A and C display the indices for California (blue line), New York (red line) and our control states (gray lines). Panels B and D display these indices for California (blue), New York (red), the District of Columbia (green), Cook County, IL (yellow) and King County, WA (blue).

In Panels A and C, the reductions in time spent at restaurants and retail and at workplaces are shown to be similar in New York and California and greater than in almost all control states. The reduction is greater for time in restaurants/retail than in time in workplaces. This difference suggests that an index that summarizes the differences in both indices may be superior to using only one. Panel B shows a greater reduction in time spent in restaurants in the District of Columbia relative to the other areas than does the comparable reduced times at workplaces shown in Panel D. The patterns in Panels B and D thus also suggest creating a pandemic shock index using weights for both the restaurant/retail and workplace times. We note also that the shift to working from home continued after the height of the pandemic, especially in denser cities such as New York City and San Francisco (Barrera, Bloom and Davis 2023) and that higher rates of working from home reduced demand for restaurant labor (Dey et al. 2021). Unfortunately, we are not aware of working from home data for most of the other treated and donor counties in our samples.

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

The decline in retail employment was more moderate than in restaurants. Foot traffic data reported in Yang, Liu, and Chen (2020) confirm that the decline in fast foods was more moderate than in restaurants as a whole. National QCEW data also show the different effects on full service and limited service restaurants. In April 2020 employment in full service restaurants had declined to 37 percent of the February 2020 level; it then recovered by July 2020 to 73 percent of the February 2020 level. Meanwhile, employment in fast food restaurants in April 2020 had declined to 77 percent of its February level; by July 2020 it recovered to 93 percent of the February level. Finally, retail employment in April 2020 fell to 83.7 percent of its February 2020 level and then recovered by July 2020 to 95.7 of its February 2020 level.

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

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

The map in Panel E of Figure B.1 displays the variation of the pandemic index across our treated and donor areas. The map suggests that while the pandemic affected both treated and donor counties, the effects were greater in treated counties. In other words, the pandemic confounds our minimum wage estimates.

B.B. Correlation of Pandemic Index with COVID cases and deaths

Figure B.2 shows the the correlation between time spent in restaurants and retail versus Covid cases (Panel A) and deaths (Panel B) for California and New York relative to our donor states. The cases and deaths appear above the horizontal axis, since they are increasing; time spent appears below the horizontal axis since they are decreasing. (State-level case and death data come from the New York Times Covid database, available on GitHub). The area shaded in gray is the time period captured by our pandemic index.

Figure B.2 demonstrates why time spent provides a better measure than Covid cases or deaths. In our index period, New York City is the first epicenter of Covid cases and deaths. California cases and death remained relatively low on a national scale because California a) imposed lockdowns early and b) had stockpiled adequate supplies of protective equipment, ventilators and hospital beds. The same pattern is evident during the spike in cases in late 2021 and early 2022 (due to the Delta and Omicron variants).

B.C. Pandemic-bias correction

We discuss our novel approach to correcting for the confounding effect of the pandemic response shocks in Section **??**. Here, we present Figure B.3, which plots $\tilde{Y}'_{zt} - \tilde{Y}_{zt}$ in event time by outcome, for the donor pool and for the treated counties. A necessary condition for the validity of our pandemic correction procedure is that $E[\tilde{Y}'_{zt}] = E[\tilde{Y}_{zt}]$ for all $t < 2020q1$. Visual inspection of Figure B.3 shows there is no difference between $\tilde{Y}'_{zt}$ and $\tilde{Y}_{zt}$, on average, before event quarter 22, which coincides with 2020q1 in California. The confounding effects of the pandemic shock on our treated counties can then be seen from event quarter 22 onward.

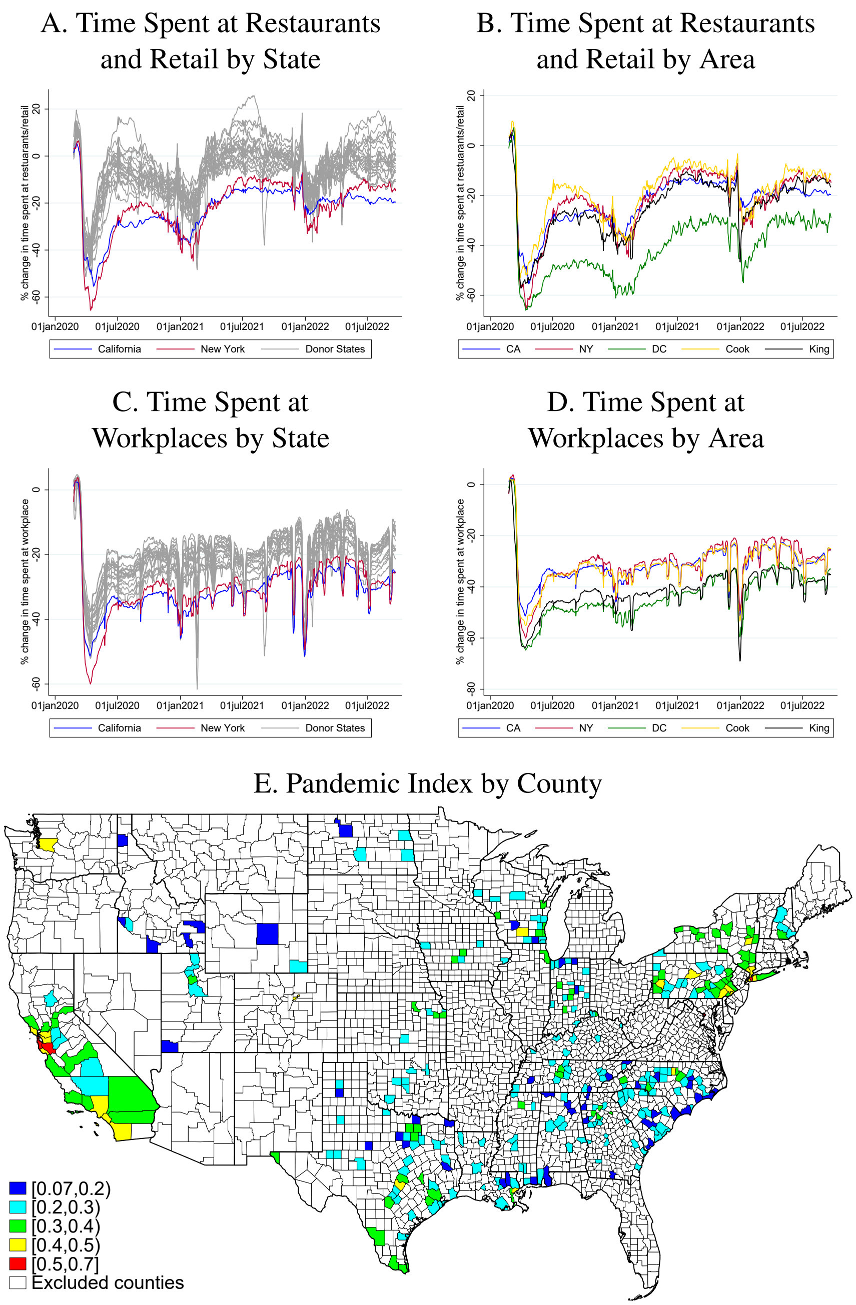
[3] Full-service restaurant employment declined much more steeply than fast food employment during the pandemic.

Figures B.4 and B.5 present our uncorrected results and our estimates corrected for bias from pairwise matching discrepancies on predictor variables. As our pandemic-corrected estimates also correct for bias from pairwise matching discrepancies on predictors, these figures (in conjunction with Figure IV) provide different approaches to observing the confounding effects of the pandemic shock. Both figures make clear that employment decreased more sharply in treated counties during the pandemic. Comparing Figure B.4 to Figure B.5, we see very little bias resulting from pairwise matching discrepancies. The differences between Figure B.5 and Figure IV then entirely result from the pandemic correction. They show that after correcting for the stronger pandemic shocks in the treated counties, the minimum wage increases boosted employment in treated counties.

Figure B.5 shows that, after correcting for the pandemic response shocks, earnings fall slightly in treated counties, relative to their synthetic controls. We explore this erosion of the minimum wage effects on earnings in Section V.E. Unusually tight low-wage labor markets and relief programs (stimulus checks and expanded unemployment insurance benefits) caused wages to rise more than inflation and therefore more than minimum wage increases in donor counties, thereby reducing the bite of the minimum wage. Meanwhile, earnings in low-paid jobs also increased rapidly and more than inflation in donor states.

Finally, Rows C and D of Table III presents our quantified employment estimates of how our combined pairwise matching discrepancy-correction and pandemic-correction procedures affect our earnings and employment results. The table presents results separately for large California counties, large New York counties and mid-size New York counties. The corrections do not change our earnings elasticities (available upon request), while they make our employment elasticities positive and permit us to rule out employment elasticities more negative than -0.04 in even the most conservative scenario (lower bound of the 95% confidence interval on employment elasticity in mid-sized New York counties with no local minimum wages), and to rule out employment elasticities below +0.06 in California.

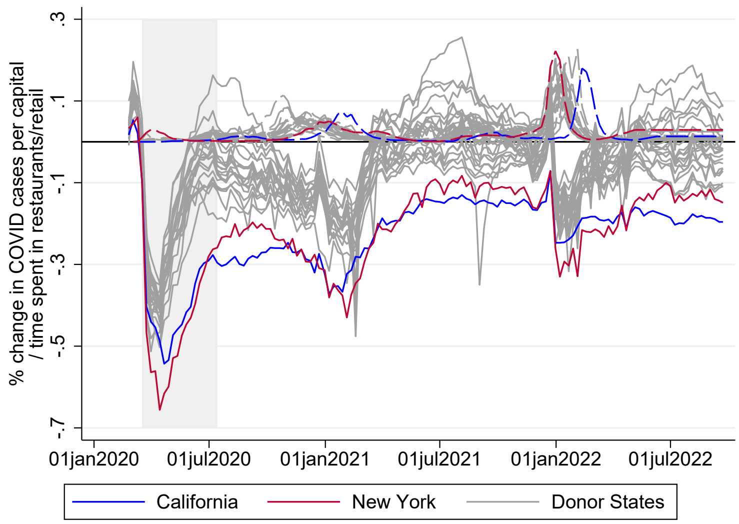
FIGURE B.1
Pandemic Analysis



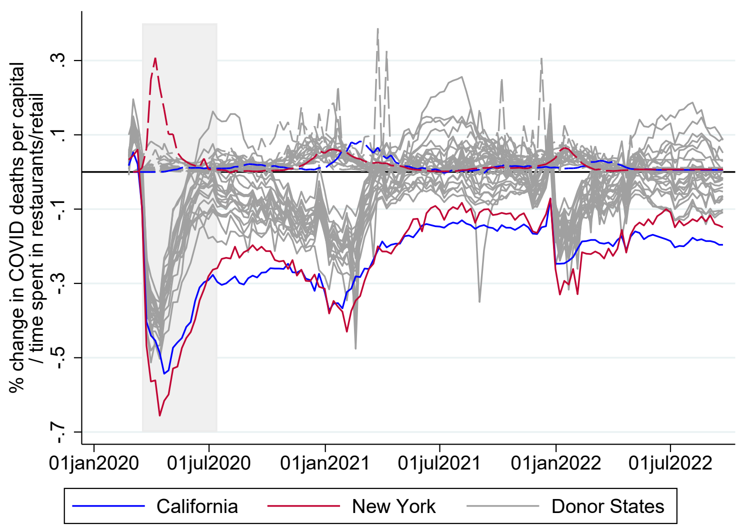
Source: Data on time spent in locations comes from Chetty et al. (2020), which is available by state, county, and city. The pandemic index is described in Section 2.3. A higher value of the index entails a greater impact. Among donor counties, the index has a mean of 0.26 and a standard deviation of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

FIGURE B.2
COVID Cases and Deaths compared to Time Spent in Restaurants and Retail

A. Comparison of COVID Cases to Time Spent in Restaurants and Retail



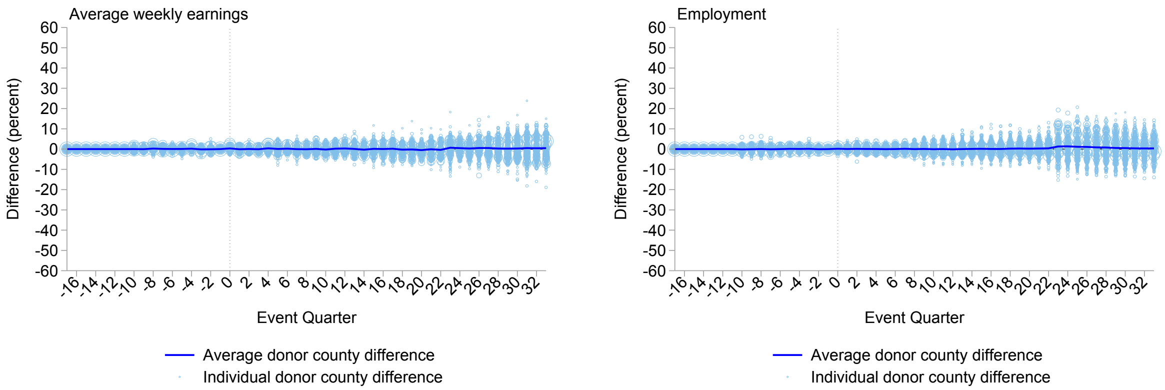
B. Comparison of COVID Deaths to Time Spent in Restaurants and Retail



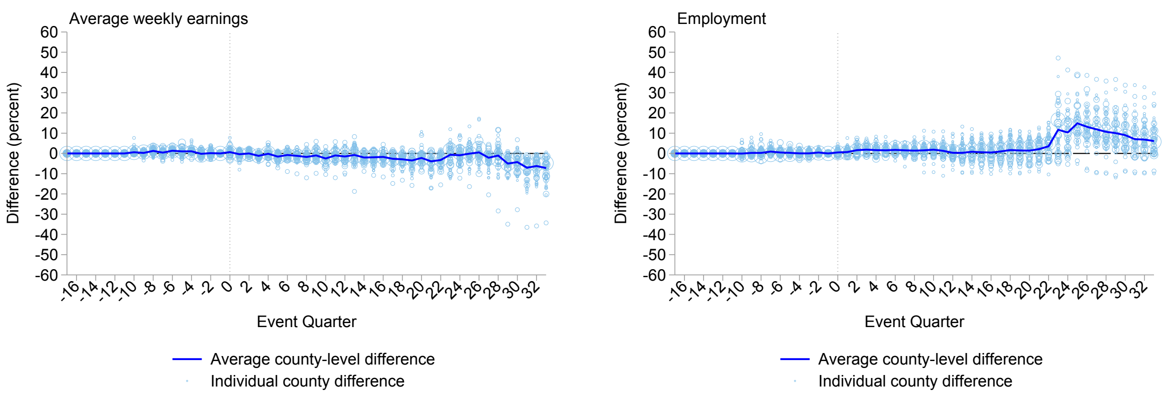
Notes: This figure displays the the correlation between time spent in restaurants and retail vs COVID cases (Panel A) and COVID deaths (Panel B) for California and New York vs our Donor States. The cases and deaths appear above the horizontal axis as dashed lines and time spent appears as solid lines. State-level cases and deaths come from the New York Times. Time spent in retail and restaurants comes from the Google Mobility data. The area shaded in gray is the time period captured by our pandemic index.

FIGURE B.3
Pandemic-corrected Minus Bias-corrected Outcome Values

A. Average and Individual *Donor Pool* Counties



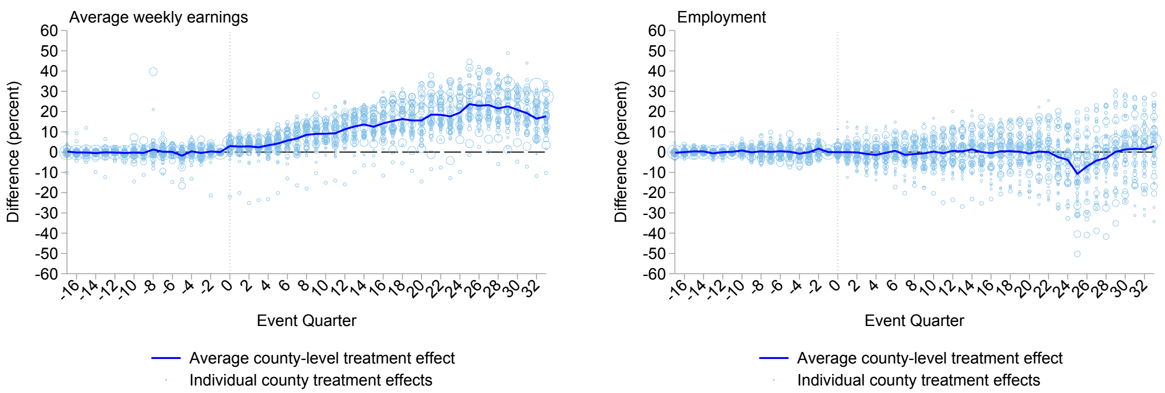
B. Average and Individual *Treated* Counties



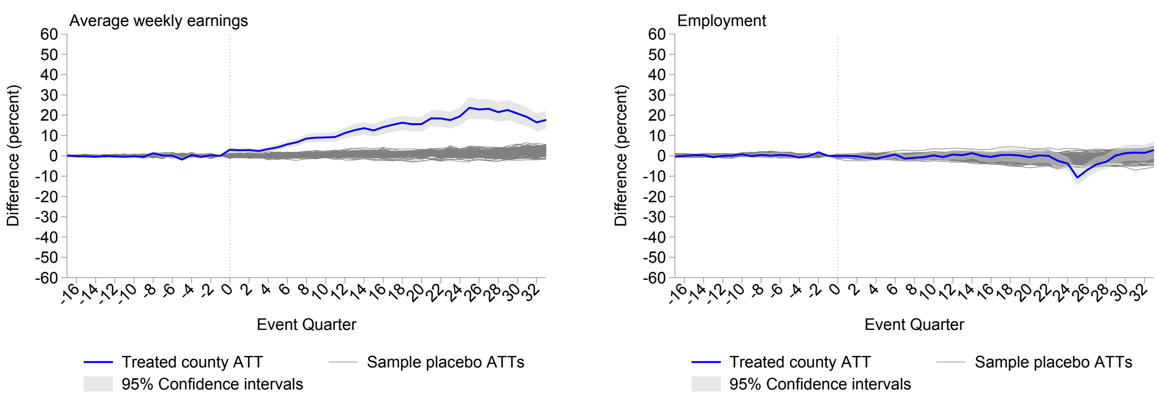
Note: Estimated using employment and payroll data from the QCEW, local unemployment data from LAUS, and Google Mobility data from Chetty et al. (2020). There are a total of 47 treated counties: 25 large counties in California, plus 11 large and 11 mid-sized counties in New York. The large counties all have $\geq$ 5,000 employment in NAICS 722; the mid-sized counties all have between 2,000 and 5,000 employment in NAICS 722. There are a total of 123 large donor pool counties, with $\geq$ 5,000 employment in NAICS 722 in states that did not experience a minimum wage change since 2009, and 150 mid-sized treated counties, with NAICS 722 employment between 2,000 and 5,000 in states that did not experience a minimum wage change since 2009. For the bias correction, in each period we regress the outcome on the full set of predictor variables *using the donor pool only*, then predict residualized outcome values for all counties (treated and donor pool). For the pandemic correction we do the same but add the pandemic-exposure index to the set of regressors in the residualization process. The y-axis shows the difference in each quarter between the (normalized to the associated final pre-treatment period) pandemic-corrected outcome and the associated bias-corrected outcome. Panel A shows these values individually (blue circles) and on average (solid blue line) for the donor pool counties. Panel B shows the same but for the treated counties. The results are placed in event time, with event-quarter 0 indicating the first quarter of treatment (or placebo treatment, for the donor pool), shown by the vertical dotted line. The pandemic period began in event quarter 22 for California and event quarter 24 for New York.

FIGURE B.4
Effects *Uncorrected* for Matching Discrepancies or Pandemic Confounds

A. Average and Individual Treated County Effects



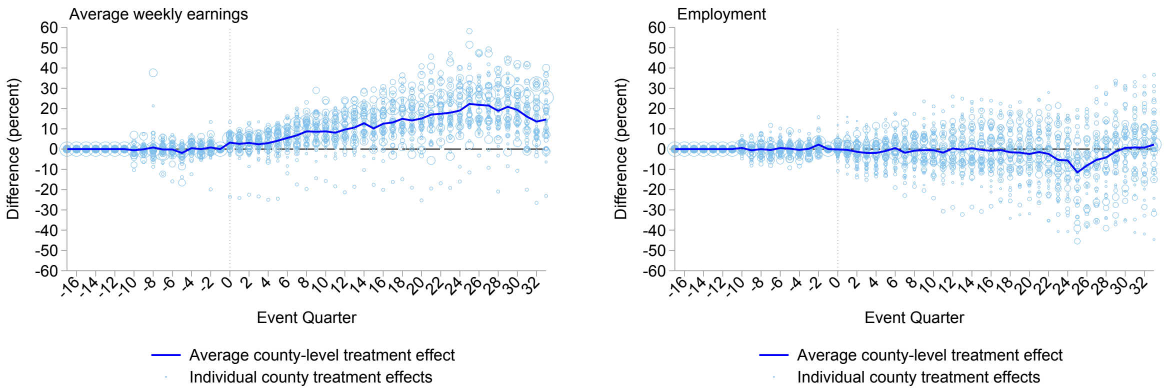
B. Average Effects in Treated Counties vs Sample Placebo Average Effects



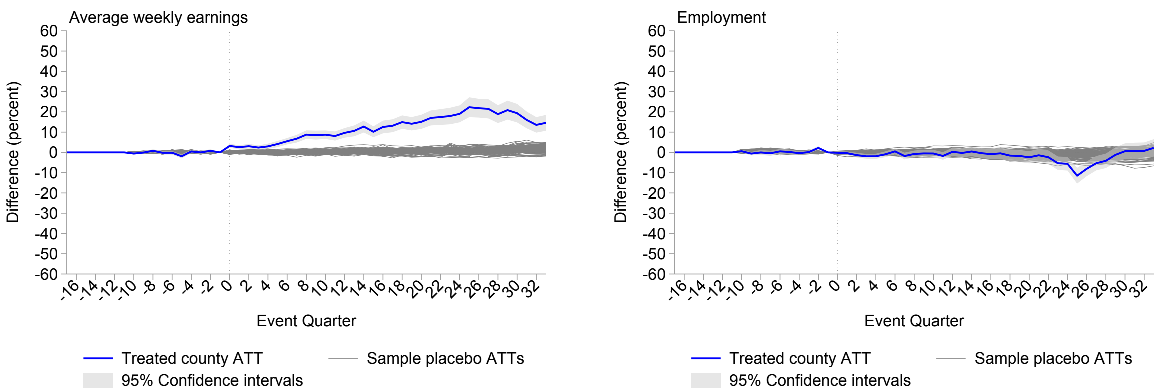
Note: Estimated using employment and payroll data from the QCEW, local unemployment data from LAUS. For our 36 large treated counties, the donor pool consists of the 123 counties with $\geq$ 5,000 employment in NAICS 722 in states that did not experience a minimum wage change since 2009. For our 11 mid-sized treated counties in New York, the donor pool consists of the 150 counties with NAICS 722 employment between 2,000 and 5,000 in states that did not experience a minimum wage change since 2009. We have a total of 25 large counties in California, plus 11 large and 11 mid-sized counties in New York. The large counties all have $\geq$ 5,000 employment in NAICS 722; the mid-sized counties all have between 2,000 and 5,000 employment in NAICS 722. 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 47 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 47 treated counties. The grey lines show 100 randomly sampled averages of 47 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 not corrected for bias from matching discrepancies or pandemic-era confounds. The pandemic period began in event quarter 22 for California and event quarter 24 for New York.

FIGURE B.5
Effects Corrected for Matching Discrepancies *only*

A. Average and Individual Treated County Effects



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



Note: Estimated using employment and payroll data from the QCEW, local unemployment data from LAUS, and Google Mobility data from Chetty et al. (2020). For our 36 large treated counties, the donor pool consists of the 123 counties with $\geq$ 5,000 employment in NAICS 722 in states that did not experience a minimum wage change since 2009. For our 11 mid-sized treated counties in New York, the donor pool consists of the 150 counties with NAICS 722 employment between 2,000 and 5,000 in states that did not experience a minimum wage change since 2009. We have a total of 47 treated counties: 25 large counties in California, plus 11 large and 11 mid-sized counties in New York. The large counties all have $\geq$ 5,000 employment in NAICS 722; the mid-sized counties all have between 2,000 and 5,000 employment in NAICS 722. 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 47 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 47 treated counties. The grey lines show 100 randomly sampled averages of 47 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 only. The pandemic period began in event quarter 22 for California and event quarter 24 for New York.

C. Teens

We consider here the effects of minimum wage increases on teen workers. We use Current Population Survey (CPS) data, which unlike the QCEW, allows us to identify teen workers and to observe their hourly wages and hours worked, in addition to employment status and weekly pay. However, the CPS does not allow us to identify the county in which an individual works. For this reason, we aggregate CPS microdata to the state-by-quarter level and conduct a state-level analysis. This setting mandates a classic synthetic control approach, with a single treated state (California) and 20 donor pool states, yielding less precise estimates than when we use county-level data. (Table C.1 shows the donor weights for our synthetic control analysis.)

We also apply regression-based estimators to teens, in a manner similar to our fast food regression-based estimates: with state and year fixed effects and no controls and with asymptotic standard errors clustered at the state level. Since these regressions use individual teen data, we apply sampling weights from the CPS.

C.A. Smoothing CPS Teen Data

The small number of teens 16 to 19 by state and quarter in the CPS and the seasonality of teen employment limit the statistical power of the CPS data. In some states our variables are quite noisy, partly because of small samples, partly because of seasonal variation and partly because respondents in the CPS are adults, not the teens themselves.[4] To address the noise in the teen data, we interpolate a few cells with missing data and adjust for seasonal variation by smoothing the outcome variables. Specifically, we predict outcome values iteratively (three times) by using OLS to regress each outcome on its values in each of the four immediately-preceding quarters, such that the smoothing takes effect in 2010q4.[5] This procedure delays any trend changes in the smoothed series by four quarters.

We use smoothing only for our CPS teen synthetic control results, but not for our CPS teen regression-based analysis. Figure C.2 compares the unsmoothed and smoothed outcomes with their respective estimated synthetic control outcomes. The benefit of smoothing is evident; the signal is much clearer in the smoothed series. Reassuringly, smoothing does not change our point estimates significantly.[6] Thus we conclude that smoothing reduces noise without changing our findings.

C.B. Teen Results

We discuss here our results for teen workers in California using our smoothed, aggregated state-by-quarter CPS data. These estimates are *not* corrected for pandemic confounds because they use state-level CPS data; our bias correction procedure relies on local variation in pandemic responses.

Panel B of Figure C.2 and Figure C.4 (in the main paper) present our main synthetic control results. We obtain good pre-period fits for hourly wages and employment. As with restaurant workers, the gap between California and synthetic California remains close to zero throughout the pre-period (although the limited CPS sample size makes these estimates noisier). As Figure C.4 shows, teen hourly wages, hours worked, employment and average weekly earnings all increased during the treatment period, relative to the donor pool, and well before the onset of the pandemic.[7]

[4] We also examined effects on teen workers using the much larger samples of the American Community Survey. However, the ACS teen earnings and employment data are noisier than in the CPS, more than eliminating the advantage of the larger sample.

[5] Following Cengiz et al. (2019), we also applied QCEW industry weights to the CPS. This procedure did not reduce noise, so we dropped using these weights from our methods.

[6] Note that the first four quarters of the smoothed series remain unsmoothed as we exclude earlier data from the smoothing process.

[7] The apparent delay in the hourly wages increase in Figure C.4 results from the smoothing process we applied to this data.

Figure C.3 shows our annual estimates using the Callaway and Sant'Anna DiD estimator. The results for average hourly wages and average weekly wages are significantly positive and similar to our main results for using the synthetic control estimator. The employment and hours estimates are positive, but very noisy.

Table C.2 displays results for the regression-based estimators and the synthetic control estimator. The outcomes here are hourly wages, employment, the own-wage elasticity, hours and weekly earnings. We present the respective point estimates for effect sizes and elasticities, the p-values appropriate to each estimator and the 95 percent confidence intervals for the regression-based estimators.[8]

The DiD estimated effect of the minimum wage shows teen average hourly earnings increased by 21 percent, while the SDiD and synthetic control estimator effects are 30.5 percent and 30.4 percent, respectively (with p= 0.01 and p=0.05, respectively). The synthetic control estimated effect on teen employment is positive and sizable (14.6 percent) and significant: p = 0.1. This positive employment estimate compares with an estimated employment effect of 12 percent using the SDiD estimator (p = 0.28) and 1.83 percent using DiD. The confidence intervals for the DiD employment elasticities rule out effects below -0.02, while those on the SDiD estimator rule out effects below -0.11. The synthetic control-estimated own-wage elasticity is 0.48, compared to 0.41 in Cengiz et al. (2019). Our results also suggest significant increases in teen hours worked and weekly pay. The synthetic control estimated effect on hours is 13.1 percent (p = 0.05), while the effect on weekly earnings is 76.9 percent (p = 0.1).

To summarize, the positive estimated effects are contrary to older studies that found negative or insignificant teen employment and hours elasticities. They make clear that teen workers greatly benefited from the minimum wage increases.

[8] We do not estimate placebo-variance-based confidence intervals for the synthetic control estimates as the required normality assumption likely does not hold with a single treated unit.

TABLE C.1
Donor Weights for Synthetic California, Teen Workers

Donor States	Average Hourly Wage	Average Weekly Earnings	Average Weekly Hours	Employment
Alabama	0	0	0.330	0.422
Georgia	0	0	0.288	0
Idaho	0.069	0.173	0	0
Indiana	0	0	0	0
Iowa	0.013	0	0	0
Kansas	0	0	0	0
Kentucky	0.356	0.111	0	0
Louisiana	0	0.127	0.277	0.353
Mississippi	0	0.118	0.106	0
New Hampshire	0	0	0	0.205
North Carolina	0	0	0	0
North Dakota	0	0	0	0.020
Oklahoma	0	0.263	0	0
Pennsylvania	0	0	0	0
South Carolina	0.039	0	0	0
Tennessee	0	0	0	0
Texas	0.173	0	0	0
Utah	0.350	0.208	0	0
Wisconsin	0	0	0	0
Wyoming	0	0	0	0

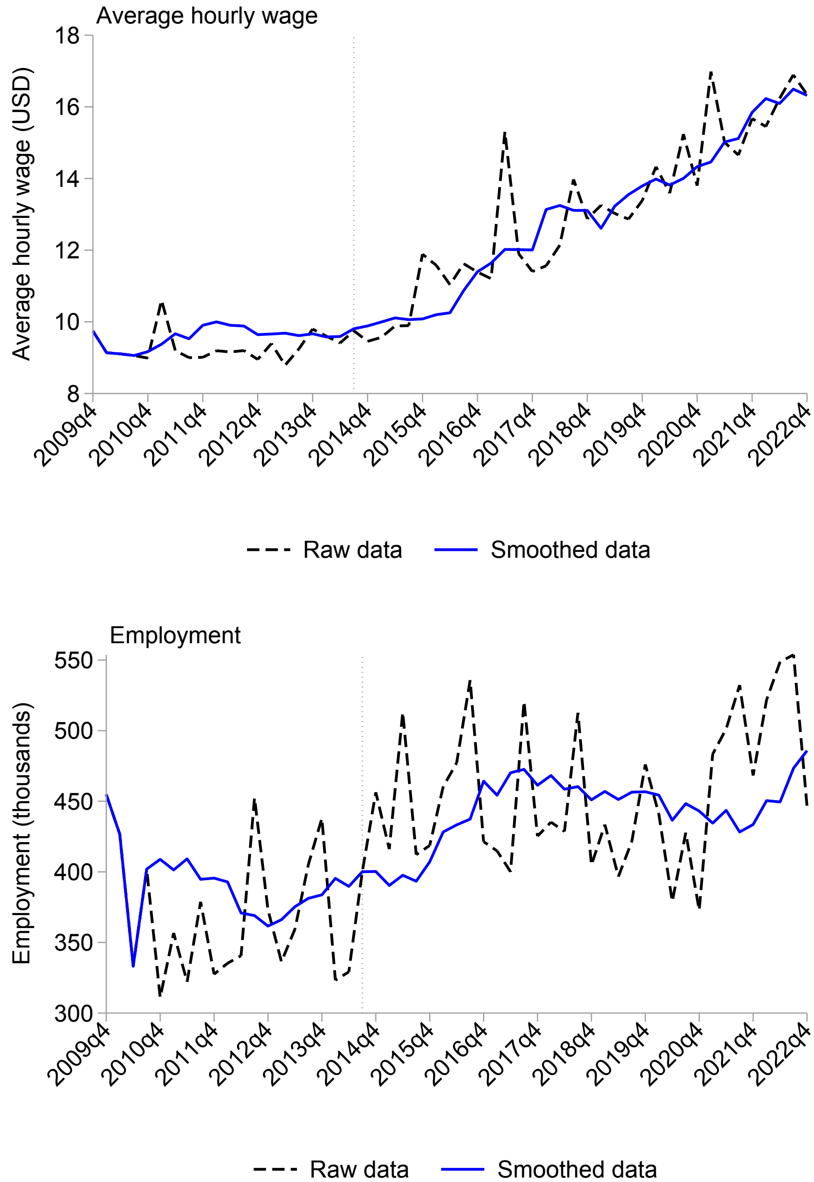
Note: Estimated using wage, hours, and employment data from the CPS, and local unemployment data from LAUS. The donor pool consists of the 20 states that did not experience a minimum wage event through 2022q1. California is the treated state. Each outcome variable is smoothed and normalized to 2014q2. The predictor variables include the (smoothed, normalized) outcome value in each quarter from 2009q4–2011q4, their average over the same period, and the average unemployment rate in 2009–2011.

TABLE C.2 Effects for Teen Workers in California, State-level Data

	Hourly Wages	Employment	Own-wage Elasticity	Hours	Weekly Earnings
Difference-in-differences					
Treatment Effect	21.03	1.83	0.12	8.25	36.49
Elasticity	0.24	0.02		0.09	0.42
CIs	[0.19, 0.29]	[-0.02, 0.06]		[0.02, 0.17]	[0.31, 0.52]
SDiD					
Treatment Effect	26.95	21.10	0.78	30.14	62.01
Elasticity	0.31	0.24		0.34	0.71
Placebo 95% CIs	[-0.02, 0.63]	[0.00, 0.48]		[0.08, 0.61]	[0.17, 1.25]
Placebo *p*-value	0.06	0.05		0.01	0.01
Synthetic Control					
Treatment Effect	24.99	25.23	1.01	31.95	74.61
Elasticity	0.29	0.29		0.37	0.85
RMSPE *p*-value	0.05	0.10		0.10	0.05

Note: Estimated using employment and earnings data on all workers 16–19 in the CPS and local unemployment data from LAUS. The donor pool consists of the 20 states without minimum wage events during the treatment period. The difference-in-differences treatment effects are estimated using annualized microdata and reflect the 2021 estimate, and the confidence intervals are clustered at the state level and are asymptotic. The SDiD and synthetic control treatment effects are estimated off of data smoothed and normalized to 2014q2, and reflect the estimates in 2022q1. The elasticities reflect the ratio of the estimated treatment effect and the 87.5% increase in California's minimum wage between 2014q2 and 2022q1. The placebo 95% CIs on the elasticities and *p*-value for the SDiD results are calculated using standard errors estimated using the placebo-variance approach. The RMSPE *p*-values for the synthetic control results are estimated from the *RMSPE*-ranking of the average estimated treatment effect, relative to the distribution of estimated placebo effects, which are based on estimated differences from in-space placebo treatments on the donor pool counties. The RMPSE *p*-values for hourly pay and average weekly earnings are one-sided as this inferential approach is particularly underpowered with one treated state and 20 donor pool states. The RMSPE *p*-values for hours and employment are two-sided. The own-wage elasticity is the ratio of the estimated employment effect to the estimated earnings effect.

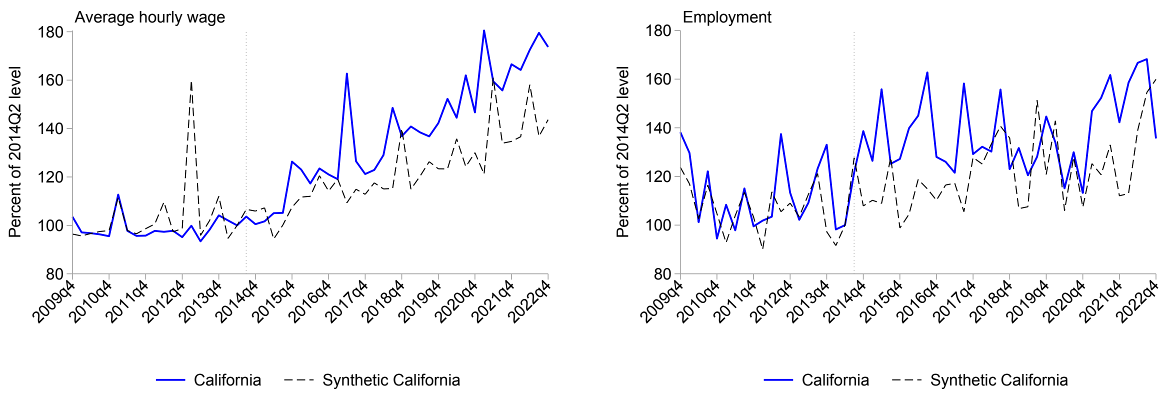
FIGURE C.1
Raw and Smoothed State-level Data, Teen Workers



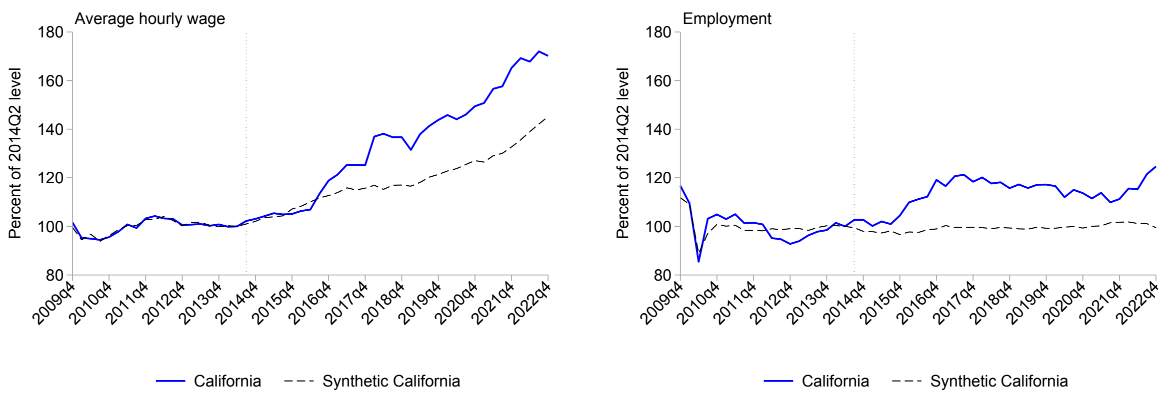
Note: Hourly wages and employment for California workers 16–19, raw and smoothed CPS data series. We smooth the outcome variables by predicting them iteratively (three times) for each state using a simple OLS regression on their values during each of the four immediately-preceding quarters, such that the smoothing takes effect in 2010q4. This procedure addresses the noise that is evident in the raw series (especially in the donor pool states, which have fewer observations of workers who are 16–19), while also adjusting for seasonal variation.

FIGURE C.2
California v Synthetic California Teen Workers, Unsmoothed and Smoothed State-level Data

A. Unsmoothed Data

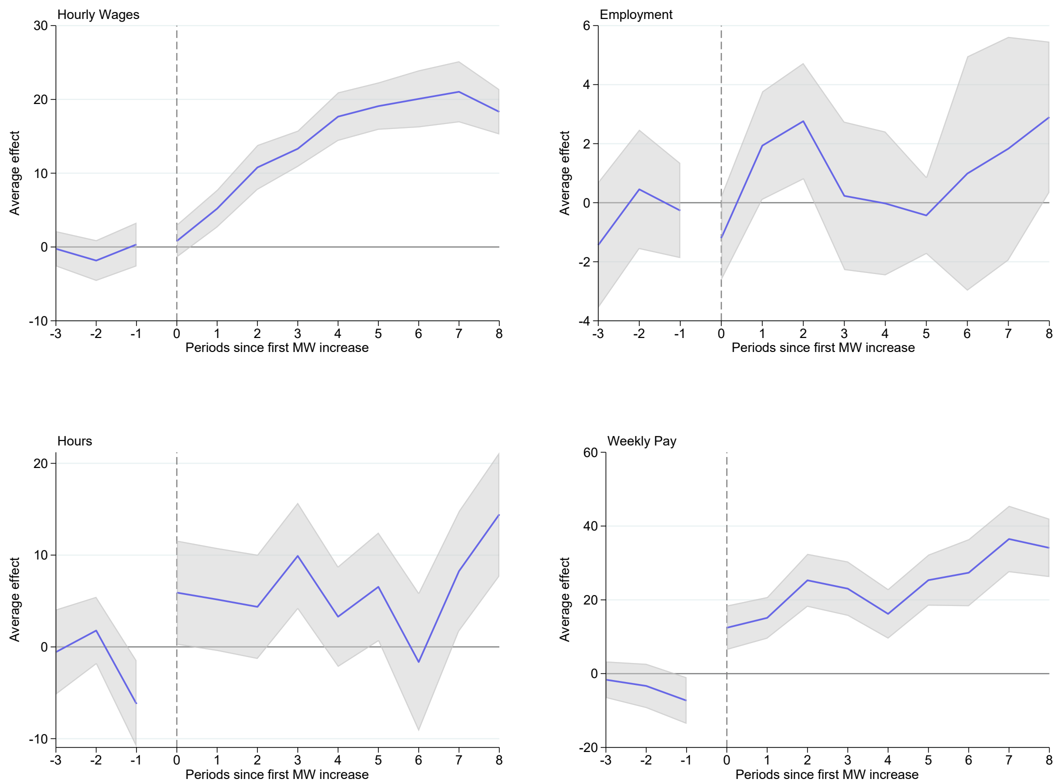


B. Smoothed Data



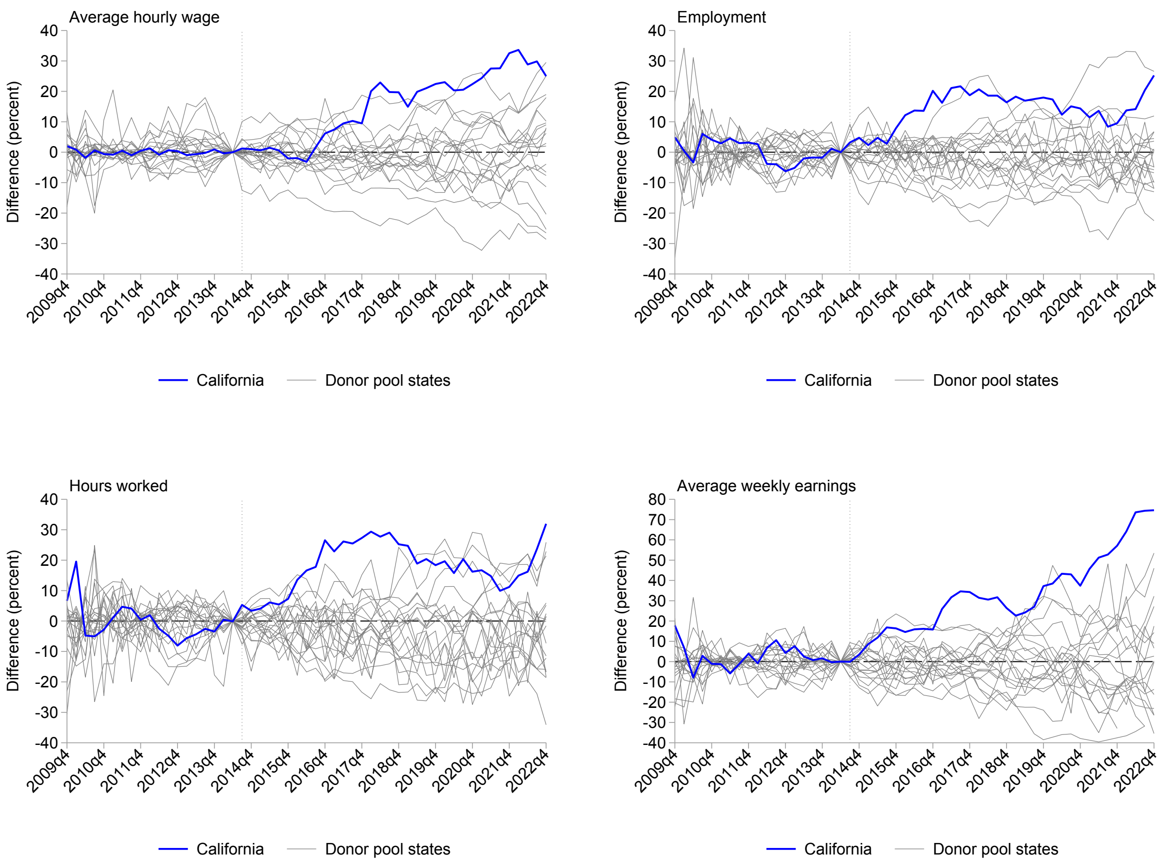
Note: Estimated using unsmoothed and smoothed employment and earnings data for workers 16–19 in the Current Population Survey (CPS), and local unemployment data from LAUS. The donor pool consists of the 20 states that had no minimum wage events between 2009 and 2022q2. The y-axis shows the (normalized to 2014q2) outcome variable value in each quarter for California and for its estimated synthetic control. The solid blue line is the value for California, while the dashed black line is the the value for synthetic California. Panel A shows these results using unsmoothed data; Panel B shows the results using smoothed data. Smoothing consisted of predicting outcomes iteratively (three times) by state using OLS regression on their values during each of the four immediately-preceding quarters, such that the smoothing takes effect in 2010q4.

FIGURE C.3
Teen Workers, Difference-in-differences



Notes: These figures show annual coefficients using the Callaway and Sant'Anna (2021) estimator for employment, log of hourly earnings, log of hours and log of weekly pay for teens 16-19. The coefficients in the post-period represent the difference in percent changes between California and control states, relative to 2013. The coefficients in the pre-period are relative to the previous year. Standard errors are clustered at the state level and estimated using a wild bootstrap.

FIGURE C.4
Estimates for Teen Workers Using State-level Data



Note: Estimated using employment and earnings data on workers aged 16–19 in the CPS and local unemployment data from LAUS. The donor pool consists of 20 untreated/control states for the period ending in 2022q2. The y-axis shows the estimated difference in each quarter between the (smoothed, normalized to 2014q2) outcome value in California and its estimated synthetic control. The solid blue line is the estimated difference (effect) for California, while the grey lines show the estimated differences from in-space placebo treatments on the donor pool states. The vertical dotted line indicates the first quarter of treatment.

D. Policy Confounders

As we have noted, a credible synthetic control method must check for confounding events that differently affected the treatment and control groups during the treatment period. California, but not New York, did indeed institute policies during the treatment period that were not matched in our donor pool. Such factors could confound our identification of minimum wages as the cause of changes in pay and employment in low-wage jobs. We discuss potential policy confounds here.

Perry (2017) identifies 51 individual policies that California adopted by 2016 that were not implemented in most other states, and that could have affected the state's economic growth. To make the analysis tractable, Perry sorted these policies into five broad groups: enhancements of workers' rights (mainly minimum wage increases and greater enforcement activity), increases in taxation at high incomes, enhancements to the state's safety net, improvements in infrastructure and housing, and environmental policies. He then combined these groups into a single weighted index and used the synthetic control method to examine their combined effect. Perry finds that they did not reduce economic growth in California between 2011 and 2016, relative to a control group.

Perry's results suggest that the state's policies do not confound our results. However, his study period ends in 2016 and his outcome of interest—state economic growth—differs from ours. Moreover, the amalgamation of tax, infrastructure, housing and environmental policies with labor market policies limits the relevance of his study for our purposes here. We therefore examine here whether a subset of Perry's policy list qualify as potential confounds that threaten our identification of the effects of high minimum wages in California.

In this appendix we consider three policies that California enacted and/or expanded during our treatment period that could have affected pay and employment among low-wage workers, thereby potentially confounding our minimum wage estimates. These policies are 1) the enactment of a California Earned Income Tax Credit (Cal-EITC) in 2015 and its subsequent expansion; 2) the expansions of Medi-Cal– California's Medicaid program—and coverage of the American Care Act (ACA) in California during the treatment period; and 3) enhanced enforcement of minimum wage laws.[9]

The synthetic control method does not allow turning such programs on during the treatment period, limiting our ability to control for potential confounds. To assess whether these programs are potential confounds, we examine the magnitudes of the programs and draw on previous research on their labor market effects.

During the pandemic California also enacted substantial temporary policies to protect workers' rights and provided frontline ("hero") workers with additional pay, supplemental paid sick leave and a stimulus package to help the state's economy recover from the pandemic's effects. California's economy may have been more affected than our donor pool states by the pandemic itself and the concomitant shift to working from home. These factors are too recent to allow evaluation in this appendix. We therefore consider only the pre-pandemic years here.

D.A. Cal-EITC

The federal EITC, created in 1975 and expanded multiple times since, supplements wages of employed workers in low-income households. It was designed to create an incentive for eligible taxpayers, mainly women with children, to join the labor force. The theoretical impact of the EITC, however, may be negative, positive or zero, because of conflicting income and substitution effects in the "phase-in" region. With the

[9]In 2015 California instituted paid sick leave mandate for all employees, paid for by a tax on employers. The cost amounts to between one and two percent of pay, capped at 30 hours per month. Various Covid-related policies–state stimulus, hero pay, lockdowns and restrictions that were stronger and longer in CA than elsewhere– could also have affected employment and wage growth. However, none would affect our pre-2020 results.

notable exception of Kleven (2019), the empirical evidence suggests that the EITC has had modest positive labor supply effects. Beyond its potential employment effects, the EITC may also create an incentive for employers to pay lower wages (Rothstein and Zipperer, 2020).

Beginning in 1986, states began to enact their own EITCs, usually as a percentage add-on to the federal EITC. By 2019, 28 states and the District of Columbia had created their own EITC programs and many have expanded their EITCs over time (Bogdanos, 2019). California created its own EITC program, dubbed Cal-EITC, in 2015, providing an add-on of up to 85 percent of the federal EITC for eligible recipients. Unlike in the federal and other state EITC programs, Cal-EITC focuses its benefits especially on households in deep poverty—those earning less than fifty percent of the federal poverty level. As a result, the California program has a steeper phase-out range and therefore provides substantial benefits to a smaller percentage of poor households than do the other EITC programs. Thus, taxpayers with children qualified only if they had an earned income of less than $13,870. Taxpayers without children qualified if their earned income was less than $6,580; their maximum credit, for taxpayers with less than $3,250 in earned income, was $214. The earnings eligibility limits were extended in 2017, 2018 and 2019. Adults 18 to 24 with no children were added in 2018. The number of refunds and their average size has thus grown modestly in recent years. In our CPS sample, hours worked per week average about 38 hours; at the current minimum wage of $15, single filers would be ineligible for Cal-EITC benefits if they worked 12 or more weeks a year.

In 2020, the Cal-EITC was claimed on 4.15 million tax returns. About 75 percent of these taxpaying units had no children and received an average refund of $105. Such a small amount is unlikely to have a measurable effect on our estimated minimum wage earnings and employment effects. The average refund on the approximately 1.1 million taxpaying units with children was about $450 (Franchise Tax Board, 2022). The most recent careful study of the EITC, by Whitmore Schanzenbach and Strain (2021), found that every $1,000 increase in the federal EITC led to a 2.1 percent increase in employment among single mothers. According to these results, the addition of the Cal-EITC would therefore have increased employment among single mothers in California by about one percentage point. Single mothers comprised an average of 10.1 percent of workers getting minimum wage workers over 1997 to 2019 (Godoey and Reich, 2021). Consequently, the employment effect of the Cal-EITC was perhaps 0.1 percentage points, not enough to affect our finding of no employment effects of the minimum wage.

Moreover, our finding of positive earnings and employment effects among teens would not have been affected by the Cal-EITC. Teens can typically be claimed as dependents, making them ineligible for EITC benefits, and likely unaffected by any state-level EITC change.

D.B. Medi-Cal Expansion and ACA Expansion

California's legacy Medicaid program has long covered a higher proportion of the state's population, relative to other states. California's ACA-related Medicaid expansion, implemented on January 1, 2014, increased Medi-Cal coverage from about one-fifth of the state's population in 2013 to about one-third in 2016, and then remained at that level through 2021 (McConville, 2021). ACA-related Medi-Cal widened differences with our donor states. Among the 18 donor states with positive weights, only five adopted ACA-Medicaid expansion during our treatment period.

California also expanded health care coverage during our treatment period. In January 2020, young adults 19-26 became eligible for Medi-Cal regardless of immigration status. The numbers affected were small, since many already had access in the state through its DACA programs. California has recently increased subsidies through Covered California-—the state's health insurance marketplace—-by raising the eligibility ceiling for households from 400 percent of the federal poverty level to 600 percent. It also increased subsidies for households between 20 and 400 percent of the federal poverty level. However, these expansions likely affected a very small percentage of low-wage earners.

By providing health care or health insurance that was not linked to employment, California's Medi-Cal and ACA expansions could have raised reservation wages and reduced labor supply in the state. A substantial literature examines this possibility. Guth, Garfield, and Rudowitz (2020) provides a recent comprehensive review of studies conducted between 2014 and 2020. Earlier research, based on the Oregon lottery experiment (Baicker et al., 2014), had found that Medicaid expansion had no effects on employment rates or pay.

Two studies stand out in Guth et al.'s review. Using a standard difference-in-differences method, Heim, Lurie, and Simon (2015) found that the extension of ACA coverage to young adults 19 to 25 had no measurable effects on their labor market outcomes. Using QCEW data, Peng, Guo, and Meyerhoefer (2020) compared pairs of bordering counties in expansion and non-expansion states. These authors found that Medicaid expansion was associated with a transitory employment decrease of 1.2 percent one year later; this effect did not persist two years later and Medicaid expansions had no wage effects at any point. These and other studies reviewed by Guth et al. suggest that California's health policies had small, if any, effects on the state's low-wage labor market.

D.C. Changes in compliance and enforcement

Higher minimum wages increase the incentives for employers not to comply with the law. In response, some states and localities have enhanced their enforcement activities when they increase their minimum wages. Comparing state-level enforcement, Galvin (2016) finds such a pattern for the period up to 2010.[10] We examine here the enhanced enforcement activities in California that accompanied the minimum wage increases and compare these to contemporaneous changes in federal enforcement activity in our donor states.

1. Compliance and enforcement changes

Employer compliance in California did not fall, despite the state's minimum wage increases from $8 to $15. Figure D.1 reports the percent of workers in the lowest pay quartile in California and in the donor states who reported wages less than their state's minimum wage. The yellow line shows the percentage of California's low wage workers who were paid less than the state's minimum wage for small employers.[11] The gray lines show the percent of low-paid workers earning less than their state minimum wage in the donor states. The California noncompliance percentage remained stable between 2009 and 2022, varying between 3 and 5 percent.[12] Noncompliance declined somewhat in the donor states, from about 7 percent to 3 percent. This decline occurred while nominal entry wages continued to rise in those states, while nominal minimum wages did not.

Why did compliance not fall in California? Beginning in 2011, the state enhanced its detection efforts and penalty policies (Bureau of Field Enforcement, 2020).[13] The state progressively enacted greater financial and criminal penalties for minimum wage violations, including for retaliations against immigrant workers. BoFE also streamlined the collection of back wages by using estimated rather than litigated damages ("liquidated damages") and reduced violations sooner by issuing injunctions to violators. As an incentive

[10]Galvin (2016) also finds that only more substantial penalties deter noncompliance.

[11]Between 2016 and 2022 the state mandated a lower minimum wage for businesses with 25 or fewer employees.

[12]Also using the CPS, Eastern Research Group (2014) reported that noncompliance rates were similar in California and New York.

[13]The following account and data relies on the 2020 report of the California Bureau of Field Enforcement (BoFE), an arm of the California Labor Commissioner.

for employers to resolve cases more quickly, California also began to impose a ten percent interest rate on liquidated damages.

BoFE also changed its monitoring strategy. At first, it simply increased the number of workplace inspections, but not its detection strategy. BoFE then shifted its detection strategy, from a complaint-driven model to a pro-active model that focused inspection activity on known violating industries, such as agriculture, apparel, car washes and restaurants. As a result, BoFE performed fewer inspections, but increased the number of violations per inspection, from 50 percent in 2009/10 to 86 percent in 2016-17 to 148 percent in 2017-18, 207 percent in 2018-19 and 160 percent in 2019-20. Assessed wages per inspection grew even more dramatically, from $1,402 in 2009-10 to $33,971 in 2018-19 and $82,616 in 2019-20. The state's enforcement actions, measured by the number of citations issued and the dollar value of assessed penalties, began to increase in 2011 and rose sharply in 2017.

The California Labor Commissioner's office also developed partnerships with community groups and industry associations. These partnerships led to improved awareness of minimum wage laws in exposed communities, generated better information about noncomplying employers and enhanced BoFE's ability to interview workers in trusted locations outside the workplace. BoFE also began to conduct audits of a company's entire payroll records, thereby moving from the investigation of individual cases to company-wide patterns; and it increasingly published news releases about egregious violations, thereby deterring noncompliance by other employers.[14]

On the other hand, the Labor Commissioner's capacity to hold hearings on wage claims grew much more slowly than the growth in monitoring activity and the number of wage claims. Between 2017 and 2021, the time to an initial wage claim hearing averaged 505 days, well beyond the mandated 120 day limit. The proportion of back wages that were paid fully to workers also fell, to 14 percent within five years after a worker won a wage theft claim (Kuang, Jeanne and Lazo, Alejandro, 2022).

The state's activities were supplemented by local enforcement offices in the large California cities with local minimum wages, notably Los Angeles, San Francisco and San Jose.[15] San Francisco's pioneering Office of Labor Standards and Enforcement (OLSE), created in 2000, worked with a substantial number of community-based organizations to educate the public about minimum wage standards and to encourage reports of violations. As a result, many of the most non-compliant industries in the city—including restaurants and retail—also had the highest complaint rates (Fine and Shepherd, 2021). OLSE also pioneered the practice of auditing payroll records for all workers when a single worker issued a meritorious complaint.

2. Federal enforcement changes

California and New York each employ over 100 wage violation inpectors. On the other hand, six of the states in our pool do not employ any enforcement personnel and nine others employ less than ten, and five donor states employ between ten and 99 enforcement personnel (Levine, Marianne, 2018). The burden of enforcement in these states thus falls on the federal government, specifically on the U.S. Department of Labor's Wage and Hours Division (WHD).

Beginning with Ashenfelter and Smith (1979) and continuing to Stansbury (2021), multiple research studies have examined the efficacy of the penalties in the Fair Labor Standards Act and WHD's enforcement activities. These studies have found a limited deterrent effect on minimum wage and overtime violations. This result is not surprising, as the number of workplace inspectors funded by Congress are two orders of

[14] Weil (2010) shows that such publicity create substantial deterrent effects on nearby employers.

[15] Gerstein (2020) and Gerstein and Gong (2022) shows that state and local enforcement activities also have grown in other parts of the U.S., but not in many of the states that make up synthetic California.

magnitude lower than they were when WHD first began operations. But in about 2009 and accelerating in 2014, WHD began to enhance its enforcement activities. These enhancements included hiring more inspectors, creating a closer relationship with the Department's legal arm (Office of the Solicitor) and prioritizing industries with high rates of subminimum wages and low complaint rates.

Recognizing that worker complaints in some industries might be constrained by retaliation fears, WHD reduced the percent of inspections that were complaint-driven from 80 percent to 50 percent.[16] WHD also increased its outreach to worker and community-based groups.[17]

By shifting from complaint-driven methods to these strategic methods, WHD increased the percent of its investigations that found violations from 35 percent in 2009 to 51 percent in 2016 (Weil, 2018). A U.S. Government Accounting Office study found that the dollar value of WHD's assessed back wages increased 75 percent from 2010 to 2019 and that investigations continued at the same rate after budget and personnel cuts in 2017.

WHD prioritizes it enforcement efforts in states with weak enforcement activities of their own. Thus, the South, which is well represented in our donor pool, accounted for 38 percent of WHD investigations (Government Accountability Office, 2020). However, WHD does not publish sufficiently detailed data to permit determining the proportion of its investigations in our donor states.

3. Conclusion on enforcement

The narrative above suggests that minimum wage enforcement activity increased in both California and in our donor pool. It is likely that heightened enforcement efforts deterred greater noncompliance in California. Federal enforcement activity also increased during our treatment period, particularly in our donor states. Although WHD publishes summary statistics on its enforcement efforts, it does not make the microdata available to researchers. We therefore surmise, but not cannot test, that changes in overall enforcement activity were similar in California and in synthetic California.

D.D. Federal pandemic recovery programs

We consider here whether the federal recovery programs instituted after the onset of the Covid pandemic constitute potential confounds for the results in this paper.

In February and March 2020, the Covid pandemic-related recession generated unusually severe economic dislocations in all areas of the U.S., rivaling in magnitude the worst year of the Great Depression. Restaurant spending, for example, fell by about 40 percent in a matter of weeks. In response, in 2020 and 2021 Congress enacted a series of large economic recovery programs with over $5 trillion in new spending. These programs amounted to 10 percent of GDP in 2020 and 11 percent in 2021, compared to less than 3 percent of GDP for the American Recovery Act, enacted after the Great Recession. About one-third of the funds went directly to households and individuals, one-third to businesses and one-third to state and local governments and some health providers (Edelberg et al. 2022). This unprecedented expansive fiscal policy, together with the onset of vaccines and the retreat of the virus itself, generated a rapid recovery from early 2020.

Taken together, the sheer magnitude of the programs helped the aggregate economy recover from the deep shock of the pandemic. If, however, the distribution of the spending varied systematically between high- and low-income states, the programs might have generated differential effects on restaurants and teens in our treated and donor areas. We review here each of the major recovery programs with this issue in mind.

[16]https://www.dol.gov/agencies/whd/data/charts/low-wage-high-violation-industries
[17]https://www.dol.gov/agencies/whd/data/charts/outreach

1. Direct assistance to people

The programs that provided direct assistance to people included relief payments (Economic Impact Payments, or EIPs) sent directly to households ($814 billion); enhanced unemployment insurance ($674 billion) benefits; and two enhancements of entitlement programs—higher SNAP benefits and a temporary refundable child and dependent care tax credit (CDCTC). These four programs provided new government spending of over $1.4 trillion between April 2020 and the end of 2021. The smaller programs that provided direct assistance to people included rental assistance, enhanced Medicaid spending and suspension of debt payments on federally backed mortgage and student loans.

Economic impact payments: $866 billion. These payments (often referred to as stimulus checks) were disbursed in three rounds: $300 billion in April 2020, $166 billion in January 2021 and $400 billion in March 2021, with 476 million payments in total. Checks were issued to households, including those without any employed workers–retirees, unemployed, students and others.

The IRS began to distribute the first round of 162 million EIP checks on April 15, 2020 and completed disbursements in July, 2020. Households received a maximum of $1,200 per adult and $500 per child; thus a household with two adults and two children would have received a check for $3,400. Among households, receiving checks, 55 percent were for $1,200 and 15 percent were for $2,400.The benefit amounts phased out with income at a five percent rate, beginning at $75,000 for single filers and $150,000 for married filers, and declining to zero for households with incomes above $300,000. The second round of EIP checks, distributed beginning on December 29, 2020, provided half the amounts of the first round for each adult and $600 per child. The third round, which began in April 2021, mandated larger amounts than the first round, but the phase-out began at much lower income levels.

Gelman and Stevens (2022) report that the three successive EIPs averaged 38, 27 and 63 percent, respectively, of recipients' median monthly income levels. About 95 percent of married-couple households with incomes up to $100,000 reported receiving EIPs. Among households with less than $25,000 in income, recipiency rates were only slightly lower—89 percent; many of these households were unbanked and receipt required additional steps, leading to lower take-up rates. Gelman and Stevens (2022)'s summary of studies of the EIPs indicate large effects on maintaining consumer spending as well as on building household balances.

The mandated EIP check amounts were identical in low and high minimum wage states. And EIP recipiency rates at lower income levels were likely also quite similar in both sets of states. Since average incomes are lower in our donor states than in our treatment states, the EIPs could therefore have had greater economic effects in our donors, potentially confounding our results.

The general economic effects of the EIPs could have been greater in lower-income states for two reasons: a) fewer and smaller phase-outs, and b) higher ratios of EIP disbursements to average incomes.

To examine the number of phase-outs, we reviewed data on the first EIP round, using IRS data on the number of checks issued in every state. We calculated the number of checks issued per capita in our two treated states and in four of our largest donor states: GA, MS, PA and TX. Given the phase-out levels and the higher incomes in CA and NY, we expected that the number of EIP checks per capita would be higher in the donor states than in the treated states. The number of checks per capita turned out to be similar in the donors and in CA and NY: CA 43.0; NYS 48.4; GA 44.1; MS 48.5; PA 48.3; TX 42.3.

First-round EIPs per capita ranged from $7,148 in CA and $7,640 in NY to $7,405 in GA, $8,238 in MS, $8,168 in PA and $7,204 in TX. The variation in spending per capita as a ratio of median household income was somewhat greater, ranging from 8.8 percent in CA and 10.5 percent in NY to 12 percent in GA, 17.7

percent in MS, 11.3 percent in PA and 10.7 percent in TX. These differences suggest somewhat larger EIP effects in our donor states.

However, according to Coibion, Gorodnichenko, and Weber (2020), about 60 percent of the first round of EIPs went to savings or paying down debt. About 10 percent of spending was on food, health/beauty aids, and household products. The amount spent on fast food was therefore likely less than three percent, or about \$40 for a check of \$1,200 and a differential of about \$8 between high- and low-income states. We therefore conclude that the confounding effects on fast food employment were negligible.

Unemployment insurance (UI) additions: A \$600 flat rate unemployment benefit supplement begin in April 2020 and ended in July 2020; subsequently, \$300 supplements began in August 2020 and ended in December 2020. These higher benefits added \$439 billion in new government spending; extensions of UI benefit durations cost an additional \$84 billion. (pandemicoversight.gov). The fixed dollar amount of the supplements likely represented much greater percentage increases in benefits in our donor states, which typically had relatively low benefit ceilings, than in CA and NY, whose ceilings were close to the average among all states.

However, the labor market effects of UI spending depend not only on benefit levels, but also on UI recipiency, duration and exhaustion rates. Bell et al. (2023) reviews disparities by state in UI recipiency, duration and exhaustion rates in 2020 and 2021. Recipiency and duration rates in CA and NY are well above those in most of our donors, while UI exhaustion rates were lower in CA and NY. These differences are large enough to offset the proportionally greater effect of the \$600 and \$300 benefit level enhancements in our donor states.

The effects of the enhanced UI programs thus were not likely to constitute a confound of our results.

Enhancements to SNAP: The Families First Coronavirus Response Act (March 18, 2020) authorized an Emergency Allotment (EA) for SNAP. As a result, SNAP spending increased 75 percent in March 2020, with further increases in 2021 to 120 percent of February 2020 levels. Maximum SNAP benefit levels were increased by 15 percent from January 2021 to September 2021.

Bell et al. (2023) report that the EA allowed states to increase benefits for all recipients to the maximum benefit level, but it did not increase maximum benefit levels. The suspended SNAP phaseouts helped those with somewhat higher incomes in the phase-out area, but it did not help those with lower incomes, who were already receiving the maximum benefit.

Participation in SNAP increased 15 percent between March and May 2020, partly because of higher take-up and partly because income losses increased the numbers eligible for the program. Increases in SNAP participation were larger in counties with bigger county-level employment shocks (2020q1 to 2020q2). But SNAP benefit spending increased more in counties with smaller negative shocks.

The size and distribution of new SNAP spending therefore does not suggest a confound of our results.

Child and dependent care tax credit (CDCTC): Beginning in July 2021 and ending on December 15, 2021, the CDCTC provided payments to working families with formal child and dependent care expenses. The refund amounts were capped at 50 percent of actual expenses and gradually phased out for families with adjusted gross incomes above \$112,500. The CDCTC reached an estimated 14 percent of all families with children. The actual monthly amounts, received by 35 million families, averaged about \$270. Researchers have found that the CDCTC substantially reduced child poverty during the six months of its existence.

The amount of the credit, the limited number of families receiving, and its short timeline suggest that it does not constitute a significant confound of our results.

2. Assistance to businesses

Programs intended to assist businesses included: the Paycheck Protection Program (PPP, $524 billion), Economic Injury Disaster Loans (EIDL; $318 billion), the Employee Retention Tax Credit ($70 billion); a Payroll Support Program for the airline industry ($28.6 billion) and a Restaurant Revitalization Fund ($28 billion). We summarize below how spending varied among each of these five programs.

Paycheck protection program (PPP): $524 billion in potentially forgivable loans between April and August 2020. We have examined data on PPP data by state and industry. *Economic Injury Disaster Loan Advances* (EIDL): $194 billion in non-forgivable long-term loan advances through November 2020 and additional $124 billion in 2021. We have examined state-level data on this program. Employee Retention Tax Credit: $70 billion claimed (refundable) for wages through 2021q1 and $31B afterward. The small size of this program suggests it would not confound our results. Airline fund: $28.6 billion intended for employee compensation. The small size of this program also makes it an unlikely confound. Restaurant Revitalization Fund: $28 billion.

The state-level data for these five programs suggest that differences in the proportional amounts spent in each state were not large enough to create a confound for our results.

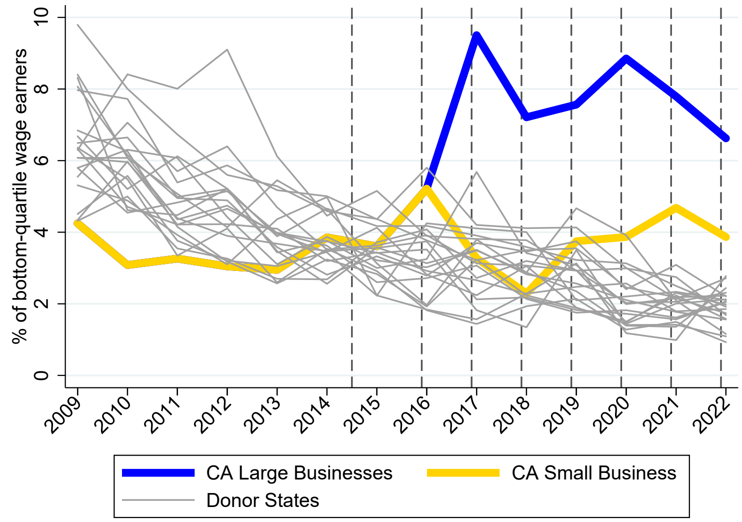
3. Aid to state and local government

The American Rescue Plan (2021) authorized $350 billion in relief aid to state and local governments. These were disbursed on a per capita basis and were therefore likely greater as a percentage of income in the donor states. However, very little of this spending would have gone directly to restaurants. The differential indirect effects on fast food restaurants therefore were likely small.

4. Conclusion

The pandemic recovery programs constituted about $5 trillion in new federal spending in 2020 and 2021. Our best estimate suggests that any confounds were nonetheless likely very small.

FIGURE D.1
Fraction of Workers Earning Less Than the Minimum Wage



Notes: This figure shows the fraction earning less than the minimum wage in California and donor pool states. The blue line shows the fraction earning less than the minimum wage assuming all businesses in California are "large businesses" (those with 26 or more employees) and thus subject to a higher minimum wage, while the yellow line shows the same line assuming all businesses are small. Excludes self-employed workers and workers with imputed responses. Hourly wages are calculated for salaried workers.

E. Own-Wage Elasticities in Early Minimum Wage Studies

In the minimum wage research literature, the most reported measure of the effect on employment is the employment elasticity– the percent change in employment divided by the percent change in the minimum wage. However, employment elasticities are likely to vary among different groups of workers: such as all workers, teens, black workers, restaurant workers, single mothers, workers with low education, etc. The variation results from differences in the intensity of treatment—or the bite of the minimum wage. In other words, each of these groups are likely to experience differing wage elasticities—the percent change in the wage divided by the percent change in the minimum wage. A better measure, the own-wage elasticity (OWE), scales the employment effect to the magnitude of the treatment effect on wages. The own-wage elasticity (OWE) equals the ratio of the employment elasticity to the wage elasticity:

$$OWE = \frac{\left(\frac{\%\Delta Employment}{\%\Delta Minimum\ Wage}\right)}{\left(\frac{\%\Delta Average\ Wage}{\%\Delta Minimum\ Wage}\right)}$$

The OWE thus can also be expressed as the ratio of the percent change in employment to the percent change in the wage. The OWE constitutes a preferred measure of the effects of minimum wage policies. Three decades ago, the trio of David Card, Lawrence Katz and Alan Krueger published five minimum wage studies of fast food and teen workers. Each used a rudimentary difference-in-differences method, yet obtained results that implied quite different OWEs. Two were positive but not significant, while three were positive and significant and ranged from 1.3 to 2.5 (see Table E.1 for the summary). The papers that found large and significant positive employment effects each interpreted their results modestly, as finding no negative employment effects.

E.A. Katz, Larry and Alan Krueger 1992. "The Effect of the Minimum Wage on the Fast Food Industry."

Katz and Krueger (1992) used telephone directories to create a universe of all Burger King, Kentucky Fried Chicken and Wendy's restaurants in metro areas of Texas. They then conducted a telephone survey of these chains before and after the federal minimum wage increased 10.4 percent, from 3.80to4.25, on April 1, 1991. Their Table 3 shows that the federal increase was highly binding on these Texas restaurants: the average starting wage increased 9.1 percent. Their Table 5, reproduced below, reports OLS and TSLS regressions in which percent change in employment (full-time equivalents, or FTE) is the dependent variable and percent change in starting wage is the independent variable of interest, along with controls for the restaurant chain and seven Texas regions. The coefficient on employment (FTE) is thus the OWE. They find OWEs for fast-food restaurant workers ranging from 1.73 to 1.85 (p = .07) for employment and 2.48 for FTE (p = .03).

E.B. Card, David 1992a. "Using Regional Variation in Wages to Estimate the Effects of the Federal Minimum Wage."

? studies the effects on teen employment of the April 1, 1990 federal minimum wage increase from \$3.35 to \$3.80, or 13.4 percent. Using CPS data on all 50 states and D.C., Card examines teen wage and employment growth over the last three quarters of 1989 and 1990. As Card states, in Table (reproduced below) the ratio of the entry in row 1, column 4 to the entry in row 1, column 1 provides an estimate of the elasticity of employment (aka OWE). The teen OWE is therefore .02/.15 = .133. Using the delta method, Online Appendix A.4 of Harasztosi and Lindner (2019) calculated a standard error of 0.57 for this OWE.

E.C. Card, David 1992b. "Do Minimum Wages Reduce Employment? A Case Study of California 1987-89."

Card (1992b) examines the effects of the July 1988 California minimum wage increase from $3.35 to 4.25$, or 26.9 percent. Using the CPS, he compares wages and employment rates of teens in California and a comparison area (Arizona, Florida, Georgia, New Mexico, and Dallas-Fort Worth) before and after the minimum wage increase. Teen employment rates exhibited similar pre-trends in California and the comparison area. In Table 4 (reproduced below) the diff-in-diff change in log wages is 0.10, or 10 percent. The diff-in-diff in the employment rate is 5.6 percent, on a base of a 42.0 percent employment rate; the employment elasticity is therefore 5.6/42.0 = .133. The OWE for California teens is therefore .133/.10, or 1.33. The Harasztosi and Lindner (2019) standard error for this OWE is 0.49. These teen results are unchanged when Card (1992b) controls for four age categories, gender, four ethnicities, sample month, four major California cities and the individual comparison states. Card finds a small but not significant negative employment effect among retail (which then included restaurant) workers. As he points out, however, prior to the minimum wage increase, retail employment trended lower in California than in the comparison area.

E.D. Card, David and Alan Krueger (1994, 2000). " Minimum Wages and Employment: A Case Study of the Fast-Food Industry in New Jersey and Pennsylvania."

Card and Krueger (1994) and Card and Krueger (2000) are the famous New Jersey-Pennsylvania fast food studies. The 1994 paper used survey data on the largest fast-food chains that were collected by the authors. The purpose was to study the effects of New Jersey's April 1, 1992 minimum wage increase from $4.25 to $5, a 17.6 percent increase. The 2000 paper uses monthly BLS ES-202 data (which has since evolved into the QCEW program); Card and Krueger also report results when the October 1, 1996 federal minimum wage raised Pennsylvania's minimum wage from $4.25 to $4.75 (an increase of 11.8 percent) that did not affect the New Jersey level.

Card and Krueger (1994) find small but not significant increases in employment in the treated group (restaurants in New Jersey). Using regression analysis, Card and Krueger (2000) (Table 2) also find positive but not significant effects of the 1992 New Jersey increases. However, they find a positive and highly significant employment effect when the treated group consisted of Pennsylvania restaurants near the New Jersey border. Although they do not report the coefficient on employment, an eyeball examination of their Figure 2 (reproduced below) shows that the employment increase in Pennsylvania by September 1997 was roughly 15 to 20 percent higher than in New Jersey.[18]

Card and Krueger (2000) do not report effects of the minimum wage increases on wages. The Ashenfelter and Jurajda (2022) study of McDonald's restaurants estimated a .7 percent wage elasticity. Using this elasticity implies a wage increase in Pennsylvania of an $0.7 \times 11.8 = 8.19$ percent wage increase. If the percent difference in employment is 15 percent, then the implied OWE for fast-food workers would be 15/8.19, or 1.83.

[18] The authors write (p. 1407): "The results in Figure 2 clearly indicate greater employment growth in Pennsylvania than in New Jersey following the 1996 minimum-wage increase." In the next paragraph, they write: "To more formally test the relationship between relative employment trends and the minimum wage using the data in Figure 2, we estimated a regression in which the dependent variable was the difference in log employment between New Jersey and Pennsylvania each month, and the independent variables were the log of the minimum wage in New Jersey relative to that in Pennsylvania and a linear time trend. For the 7-county sample, this regression yielded a positive coefficient with a t-ratio of 5.2 on the minimum wage. Although we would not necessarily interpret this evidence as suggesting that a higher minimum wage causes employment to rise, we see little evidence in these data that the relative value of the minimum wage reduced relative employment in the fast-food industry during the 1990's."

TABLE E.1
Summary of Estimated OWEs in Early studies

Study	Target Sample	OWE Estimate
Katz and Krueger (1992)	Fast-food restaurants, Texas	1.7 to 2.5
Card (1992a)	Teens, all states	.13
Card (1992b)	Teens, California	1.33
Card and Krueger (1994)	Restaurants, New Jersey	.003
Card and Krueger (2000)	Restaurants, Pennsylvania	1.83

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