CWED

Center on Wage and Employment Dynamics

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**CWED Policy Report**

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# The New Wave of Local Minimum Wage Policies: Evidence from Six Cities

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# PART 1 INTRODUCTION

In recent years, a new wave of state and local activity has transformed minimum wage policy in the U.S. As of August 2018, ten large cities and seven states have enacted minimum wage policies in the $12 to $15 range.[1] Dozens of smaller cities and counties have also enacted wage standards in this range.[2] These higher minimum wages, which are being phased in gradually, will cover well over 20 percent of the U.S. workforce. With a substantial number of additional cities and states poised to soon enact similar policies, a large portion of the U.S. labor market will be held to a higher wage standard than has been typical over the past 50 years.

These minimum wage levels substantially exceed the previous peak in the federal minimum wage, which reached just under $10 (in today's dollars) in the late 1960s. As a result, the new policies will increase pay directly for 15 to 30 percent of the workforce in these cities and as much as 40 to 50 percent of the workforce in some industries and regions. By contrast, the federal and state minimum wage increases between 1984 and 2014 increased pay directly for less than eight percent of the applicable workforce.[3]

This report examines the effects of these new policies. Although minimum wage effects on employment have been much studied and debated, this new wave of higher minimum wages attains levels beyond the evidential reach of most previous studies. Moreover, city-level policies might have effects that differ from those of state and federal policies. Yet, most of the empirical studies of minimum wages focus on the state and federal-level policies. The literature on the effects of city-level minimum wages is much smaller. Our report helps fill these gaps.

To better inform public discussion as states and localities consider new wage standards, the Center on Wages and Employment Dynamics has initiated a series of reports studying the effects of this new wave of minimum wage policies. The timing and coverage of these reports will be determined by the phase-in schedules of the minimum wage in each jurisdiction, the availability of sufficient data after the policy change, and the availability of a sufficient sample of comparison groups.

Our first report in this series focused on Seattle, one of the first movers in this new wave.[4] Using a synthetic control method, this report obtained results consistent with the bulk of past research on the minimum wage. However, our results were at odds with the results from a University of Washington study on the Seattle policy (Jardim et al. 2017). Both studies stimulated considerable debate on the best methods and data for studying local minimum wage policies.

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[1] The ten large cities are Chicago, the District of Columbia, Los Angeles, Minneapolis, New York City, Oakland, Portland, San Francisco, San Jose and Seattle. The seven states are: Arizona, California, Colorado, Massachusetts, New York, Oregon and Washington. Nassau, Suffolk and Westchester Counties in New York and parts of Cook County in Illinois and Los Angeles and Santa Clara Counties in California also have minimum wages above $10.
[2] http://laborcenter.berkeley.edu/minimum-wage-living-wage-resources/inventory-of-us-city-and-county-minimum-wage-ordinances/
[3] Cooper (2017); Autor, Manning and Smith (2016).
[4] Reich, Allegretto and Godoey (2017).

This report advances the discussion of high local minimum wages by using both event study and synthetic control methods, and by expanding our analysis to the effects in six cities that were early movers: Chicago, District of Columbia, Oakland, San Francisco, San Jose and Seattle. At the end of 2016 (the last year in our analysis), citywide minimum wages exceeded $10 in all of these cities and had reached $13 in two—San Francisco and Seattle.

Similar to our first report, we focus here on the food services industry, a major employer of low wage workers. We extend our previous methods here, using both event study and synthetic control designs to assess the policies' effects. We report estimates that pool our data from all six cities as well as estimates that use the data for each city separately. Our various approaches yield broadly similar results. A 10 percent increase in the minimum wage increases earnings between 1.3 and 2.5 percent, depending on the model estimated. Moreover, we do not detect significant negative employment effects. These findings are similar to those in a recent state-of-the-art study of minimum wages up to $10 (Cengiz et al. 2018).

We apply a series of robustness tests to check whether our findings are influenced by contemporaneous changes in the cities that are not related to minimum wages. These tests include checks on the validity of our comparison groups—notably for whether they evolve in parallel to the cities before the policies went into effect. We also test for differences in outcomes between full and limited service restaurants, and whether our methods falsely detect effects in a high wage industry—professional services—or in comparison counties that did not experience a minimum wage increase. Results from these robustness tests support the conclusion that our overall findings do not reflect other changes taking place in the cities around the time the increases took effect.

The report proceeds as follows. Part 2 presents a brief review of recent minimum wage studies, especially in food services, and presents the minimum wage policies for each of the six cities. We describe the data we employ in our analyses in Part 3. Part 4 discusses our general evaluation strategy. We present the methods and results for our two evaluation approaches, event study and synthetic controls, in Parts 5 and 6, respectively. In Part 7 we conduct robustness and falsification tests. Lastly, we summarize the paper and conclude in Part 8. Appendix A provides a formal presentation of our methods and Appendix B provides additional results.

## Highlights

- We examine the effects of minimum wage policies in six large cities with high citywide minimum wages: Chicago, the District of Columbia, Oakland, San Francisco, San Jose and Seattle. At the end of 2016, the last period of our data availability, citywide minimum wages exceeded $10 in all of these cities and had reached $13 in two—San Francisco and Seattle.
- Recent research on minimum wages up to $10 has generally not found employment effects. Ours is the first comprehensive look at effects of minimum wages above $10.

- We use the U.S. Bureau of Labor Statistics' Quarterly Census of Employment and Wages (QCEW) administrative data for our analysis. The QCEW publishes a quarterly count of employment and wages reported by employers that belong to the Unemployment Insurance (UI) system, which covers more than 95 percent of all U.S. jobs.
- We focus on the food services industry, a major employer of the low-wage labor force.
- To measure the effects of the policies, we use two complementary statistical methods: Event study and synthetic control. Both methods isolate the causal effect of the local minimum wage policies by comparing the changes we observe in the six treated cities against a group of highly populated counties in metropolitan areas across the U.S.
- The six cities that implemented higher minimum wages have stronger private sector growth than the average comparison county. Simply comparing employment in the treated and comparison counties risks masking any true employment losses that may result from the higher minimum wages. Our analysis uses statistical methods that isolate the causal effect of the local minimum wage policies.
- Event study and synthetic control yield broadly similar results. On average across the six cities, we find that a 10 percent increase in the minimum wage increases earnings in the food services industry between 1.3 and 2.5 percent.
- We cannot detect significant negative employment effects. Our models estimate employment effects of a 10 percent increase in the minimum wage that range from a 0.3 percent decrease to a 1.1 percent increase, on average.
- Our conclusions are supported by robustness tests that check whether our findings are influenced by contemporary changes in the cities that are not related to minimum wages. For example, we test whether our event study and synthetic control methods detect earnings or employment effects in professional services, a high-wage industry that should not be affected. This falsification test passes in our event study models and for 11 out of 12 of the outcomes in our synthetic control analyses of each of the six cities separately.
- We will revisit these and other localities' minimum wage policies, which in many cases will reach $15, as they become more fully implemented.

# PART 2 BACKGROUND

## 2.1 How economists conceptualize minimum wage effects on employment

Modern economic analysis suggests that minimum wage increases can increase worker pay without necessarily reducing employment. This somewhat counterintuitive conclusion follows from a comprehensive analysis of the various channels through which workers, employers and consumers adjust to minimum wage changes. Some of these channels reduce employment, such as when automation increases and when labor demand falls because minimum wage-related price increases reduce product demand.

Other adjustment channels increase demand for workers. For example, higher wages reduce employee turnover, thereby cutting employers' recruitment and retention costs and increasing workers' tenure and experience. Positive employment effects can also arise when higher minimum wages draw working age adults into the labor force or induce them to increase their hours. If product demand is inelastic, higher product prices can provide a channel to pass on higher wage costs to consumers. Higher wages can also stimulate consumer demand and job creation.[5] Models that incorporate all these channels of adjustment suggest that a minimum wage's effect on employment can be positive or negative.[6]

## 2.2 Recent empirical studies of minimum wage employment effects

With these underlying ambiguities in the predictions of economic theory in mind, labor economists have focused on empirical studies to estimate the actual employment effects of minimum wages. Most of these studies focus on the low-paid groups that are most affected by a minimum wage—such as restaurant industry workers or teens. The most persuasive studies use causal identification strategies that draw upon advances in methods that have been labeled the "credibility revolution" in empirical economics (Angrist and Pischke 2010). As in trials of new drugs, an ideal causal identification strategy would randomly divide a population into two groups—one that receives a policy treatment, while the other does not. Causal effects of the policy are then measured by comparing the outcomes of interest in one group against the other. But such randomization is usually not possible in studying economic policies. Therefore, economists often study quasi-experimental situations, such as when one jurisdiction raises its minimum wage and a similar jurisdiction does not.

More specifically, many empirical minimum wage studies have drawn upon the quasi-experimental techniques pioneered by Card and Krueger (1994), which examined changes in fast food employment among counties along the New Jersey-Pennsylvania border following New Jersey's enactment of a state minimum wage increase. Subsequent studies applied Card and Krueger's approach to include

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[5] See Dube, Lester and Reich (2016) on employee turnover; Giuliano (2013) on teen labor force participation and Borgschulte and Cho (2018) on older adults' labor force participation; Allegretto and Reich (2018) on prices; and Cooper, Luengo-Prado and Parker (2017) on consumer demand.

[6] Reich, Montialoux, Allegretto and Jacobs (2017) provide a unified conceptual and quantitative account of these potential minimum wage effects for minimum wages up to $15.

large samples of minimum wage increases throughout the U.S. and to examine effects specifically on restaurant workers and teens.[7] This strand of research has consistently shown that higher minimum wages do measurably increase low-wage workers' pay.

Recent studies of restaurant workers have arrived at a consensus: They find little to no detectable negative effects of minimum wages on restaurant employment. This consensus is evident in the Allegretto et al. (2017) review of 17 estimates from five recent studies of the minimum wage's effect on wages and employment of restaurant workers.[8] These estimates indicate that a one percent increase in the minimum wage increases average earnings between 0.19 and 0.21 percent. In contrast, in these studies the percent change in restaurant employment from a one percent increase in the minimum wage is much smaller, ranging from -0.063 to 0.039, and often not statistically distinguishable from zero.

As mentioned above, Jardim et al. (2017) report negative effects of Seattle's minimum wage on low-wage employment, both overall and in the restaurant industry in particular. Critics of this study (notably Schmidt and Zipperer 2017), noticed that the Jardim et al. results imply that the minimum wage created large positive employment effects among very high-wage workers. This implausible finding casts doubt on whether their method successfully distinguished between the effects of Seattle's minimum wage policy and the effects of the employment boom that took off in Seattle at the same time. By raising pay throughout the wage distribution, the boom reduced the number of jobs in pay ranges that were also affected by the minimum wage increase. But a similar boom did not occur in Jardim et al.'s comparison group—other areas in Washington State. As we describe below, our comparison areas come from a much broader geographical area, including counties that were also booming. As a result, we are much less likely to find effects where none should occur.

Many restaurant studies, including ours, do not have data on hours of employed workers. But a new comprehensive study (Cengiz et al. 2018) of all of the 138 federal and state minimum wage increases since 1979 is able to estimate effects on total work hours. Cengiz et al. do not detect employment or hours changes, whether they examine all industries or restaurants only.[9] These results support our focus on employment outcomes here.

The policies examined in these studies include minimum wage policies that range up to $10, but not higher. What are the effects at higher levels? Most economists expect that extremely high minimum wages—such as at $30 or $50 per hour—would produce negative employment effects. But such high levels are not on the policy horizon. We examine here the effects of the highest minimum wages that have been implemented by the end of 2016. These range from $10 to $13.

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[7] See Dube, Lester and Reich (2010), Allegretto, Dube, and Reich (2011), or Allegretto, Dube, Reich, and Zipperer (2017).
[8] These five studies are Addison, Blackburn and Cotti (2014); Dube, Lester and Reich (2010); Dube, Lester and Reich (2016); Neumark, Salas and Wascher (2014); and an early version of Totty (2017). Neumark, Salas and Wascher contend that minimum wages have negative effects on teens; their evidence is critiqued by Allegretto et al. 2017.
[9] Cengiz et al. also do not find long-run employment effects of permanently higher minimum wages, such as the decade-long experience of Washington State's indexed minimum wage, and they do not find evidence that employers switch to more educated workers after a minimum wage increase.

In this report, we also leverage two tests that address a central issue in quasi-experimental studies: Has the researcher selected a valid comparison area for measuring the causal effects of the policy? The first test considers whether the outcomes of interest in the treated and comparison areas exhibit parallel trends during the years *before* the policy is implemented. When they do not, the researcher may find spurious effects of the policy before it is actually implemented; these effects are clearly not credible and indicate a problem in the research design. The second test considers whether the outcomes of interest in the treated and comparison areas would have trended in parallel *if the policy had never been enacted*. This test measures the outcomes after the policy was implemented among subgroups that should not be affected by the policy. In our context, the estimating method should not detect effects of a minimum wage increase in high-wage industries. Such "falsification tests" aim to show that the researchers have made sound assumptions to reach their conclusions, and that their choice of methods and data are effectively isolating the effects of the policy change.[10]

## 2.3 The six cities and their policies

### *Our six cities sample*

We study policy effects in the six large cities that have been the earliest movers in the new wave of local minimum wage policies: Chicago, the District of Columbia, Oakland, San Francisco, San Jose and Seattle.[11] The cities that have adopted higher minimum wage policies may differ from those that have not. Table 1 examines selected characteristics of our six cities in 2012, prior to any minimum wage increases, and in 2016, the last year of our analysis. We focus on how these six cities compare to each other and to the U.S. as a whole.

We begin with the state of the labor market in each city, and in the U.S. as a whole, in 2012, prior to the enactment of the new local minimum wage policies. As the first row of Table 1 shows, 2012 unemployment rates in the six cities ranged from a low of 5.7 percent in Seattle to 10.7 percent in Oakland. This range brackets the 8.1 percent unemployment in the U.S. as a whole, indicating that the unemployment rates in our cities were not outliers compared to the national labor market.

The post-Great Recession recovery continued to reduce unemployment rates in every one of the six cities and in the U.S. as a whole by 2016. As the second row of Table 1 shows, 2016 unemployment rates ranged from 3.3 percent in San Francisco to 6.4 percent in Chicago, compared to 4.9 percent in the U.S. as a whole. As in 2012, the 2016 range of local unemployment rates bracket the national level.

The changes from 2012 to 2016 indicate that labor market improvements in these cities were not exceptional, relative to the U.S. as a whole. Consistent with this interpretation, the ratio of the 2016

[10] Dube, Lester and Reich (2010) and Allegretto, Dube and Reich (2011) report that minimum wage studies spuriously find negative employment effects if they do not control adequately for regional differences. These issues are thoroughly reviewed by the Allegretto, Dube, Reich and Zipperer (2017) response to Neumark, Salas and Wascher (2014).
[11] Many other early mover cities, such as Emeryville CA, Flagstaff AZ, and Tacoma WA, are too small to analyze with available data. Other prominent cities with such policies, including Los Angeles, and New York City, began to implement their policies later. We will include such cities in future analyses.

unemployment to the 2012 rate (Table 1, row 3) ranges between 0.46 in Oakland and San Jose to 0.68 in the District of Columbia, again bracketing the national ratio of 0.60.

We turn next to the earnings of a median worker in each city in 2012 and 2016. As the fourth row of Table 1 reports, 2012 median annual earnings of all jobs in these cities ranged from nearly $41,000 in Chicago to over $62,000 in the District of Columbia. These median earnings levels were all higher than the $32,417 median annual earnings level in the U.S. as a whole. The six cities under study here are more affluent, on average, than the whole of the U.S.

**Table 1 Characteristics of the six cities**

| | Chicago | District of Columbia | Oakland | San Francisco | San Jose | Seattle | U.S. |
|---|---|---|---|---|---|---|---|
| Unemployment rate 2012[a] | 10.0 | 9.0 | 10.7 | 6.8 | 8.7 | 5.7 | 8.1 |
| Unemployment rate 2016[a] | 6.4 | 6.1 | 4.9 | 3.3 | 4.0 | 3.6 | 4.9 |
| 2016 rate as ratio of the 2012 rate | 0.64 | 0.68 | 0.46 | 0.49 | 0.46 | 0.63 | 0.60 |
| Median annual earnings, 2012[b] | $40,899 | $62,467 | $42,795 | $50,868 | $44,206 | $45,821 | $32,417 |
| Median annual earnings, 2016[b] | $43,397 | $68,353 | $47,995 | $60,655 | $50,223 | $51,976 | $35,815 |
| Percent change, 2012-2016, unadjusted for inflation | 6.1 | 9.4 | 12.2 | 19.2 | 13.6 | 13.4 | 10.5 |
| Median annual earnings: food prep & servers, 2010-12[c] | $17,434 | $21,289 | $16,986 | $21,306 | $16,245 | $17,236 | $13,090 |
| Median annual rent for a one-bedroom apartment, 2012[d] | $10,236 | $15,936 | $14,196 | $18,264 | $16,200 | $10,944 | na |
| Relative cost of living—all items, 2012[e] | 106.3 | 119.5 | 121.4 | 121.4 | 122.3 | 107.4 | 100 |
| Ratio of 2012 minimum wage to median hourly wage[f] | 0.38 | 0.26 | 0.35 | 0.40 | 0.34 | 0.37 | 0.42 |
| Ratio of 2016 minimum wage to median hourly wage[f] | 0.45 | 0.32 | 0.50 | 0.43 | 0.39 | 0.47 | 0.38 |
| Share of workers projected to receive a wage increase[g] | 0.31 | 0.14 | 0.27 | 0.23 | 0.19 | 0.29 | --- |

Notes: The share of workers projected to receive a wage increase refers to the share of each city's workers projected to receive a pay increase due to the minimum wage policy at full implementation, excluding employment effects or wage spillover effects. Sources: [a] U.S. Bureau of Labor Statistics. [b] City level estimates from the 2012 and 2016 American Community Survey (ACS), Table B08521: U.S. level estimates from Table B08121. [c] ACS 2010-2012, 3-year estimates, Table B24011: Occupation by median earnings in the past 12 months for the civilian employed population. [d] U.S. Department of Housing and Urban Development, Fair Market Rents, medians for metro areas. [e] U.S. Bureau of Economic Analysis, Regional Economic Statistics, regional price parities for metro areas. [f] Median hourly wages are median annual wages divided by average annual hours for each city, using the 2012 and 2016 ACS population files. [g] Chicago: https://www.cityofchicago.org/content/dam/city/depts/mayor/general/MinimumWageReport.pdf; Illinois Department of Employment Services, Local Area Statistics. District of Columbia: https://www.epi.org/publication/raising-the-d-c-minimum-wage/. Oakland: http://irle.berkeley.edu/files/2014/The-Impact-of-Oaklands-Proposed-City-Minimum-Wage-Law.pdf. San Francisco: ttp://irle.berkeley.edu/files/2014/San-Franciscos-Proposed-City-Minimum-Wage-Law.pdf. San Jose: http://irle.berkeley.edu/files/2012/Increasing-the-Minimum-Wage-in-San-Jose.pdf. Seattle: http://murray.seattle.gov/wp-content/uploads/2014/03/Evans-report-3_21_14-+-appdx.pdf.

Since minimum wage policies are unlikely to influence the median wage, changes in the median over time indicate the extent to which these economies experienced other, non-minimum-wage-based changes during this period. Median earnings rose in all six cities between 2012 and 2016 (when measured without correcting for inflation), ranging from 6.1 percent in Chicago to 19.2 percent in San Francisco (Table 1, sixth row). The 10.5 percent increase in national median earnings during these years falls well within the lower part of the six-city range, suggesting that some of our cities experienced especially high rates of pay increases for reasons other than a rising minimum wage.

Median earnings did grow particularly rapidly in San Francisco, San Jose and Seattle, each of which contains booming technology sectors. These patterns suggest the importance of testing whether the effects we measure may be attributable to other factors, such as tech booms, rather than minimum wage policies.

We turn next to pay levels in the food service industry. Not surprisingly, earnings in all six cities and in the U.S. are much lower for those working in food preparation and service related occupations. These patterns appear in the seventh row of Table 1. Annual earnings in these occupations range from just over $16,000 in San Jose to just over $21,000 in San Francisco and in the District of Columbia, compared to about $13,000 nationally. The low median earnings in these occupations support our focus on the food services sector.

As is well-known, living costs are higher in more affluent cities. The next rows of Table 1 display two measures of the costs of living in our cities—the median annual rent for a one-bedroom apartment and an index of the overall cost of living in each city relative to the national average. The U.S. Department of Housing and Urban Development's data on apartment rents, displayed in the eighth row, indicate that the annual cost of a one-bedroom apartment in 2012 ranged between $10,000 in Chicago and Seattle to over $18,000 in San Francisco.

Row nine of Table 1 shows the U.S. Bureau of Economic Analysis' overall cost of living indices for metro areas, relative to the U.S. average (which is set as 100). Overall living costs in the six cities are well above the average for the U.S. as a whole. Higher living costs are often cited as a motivation to increase local minimum wages. We do not pursue this issue here.

Minimum wages can also be measured *relative to local wage levels*. Rows 10 and 11 of Table 1 present the ratio of the minimum wage to the median wage for 2012 and 2016. The 2012 ratios in all six cities fall below the ratio for the U.S. As row 11 shows, in five of the six cities the 2016 ratios are higher than that for the U.S. as a whole (the District of Columbia is the exception). Nonetheless, the 2016 ratios remain well within the historical range of such ratios for federal and state minimum wages (0.27 to 0.67 since 1980, according to Zipperer and Evans 2014). In other words, the *relative* minimum wage levels are not as high as the *absolute* minimum wage levels might suggest.

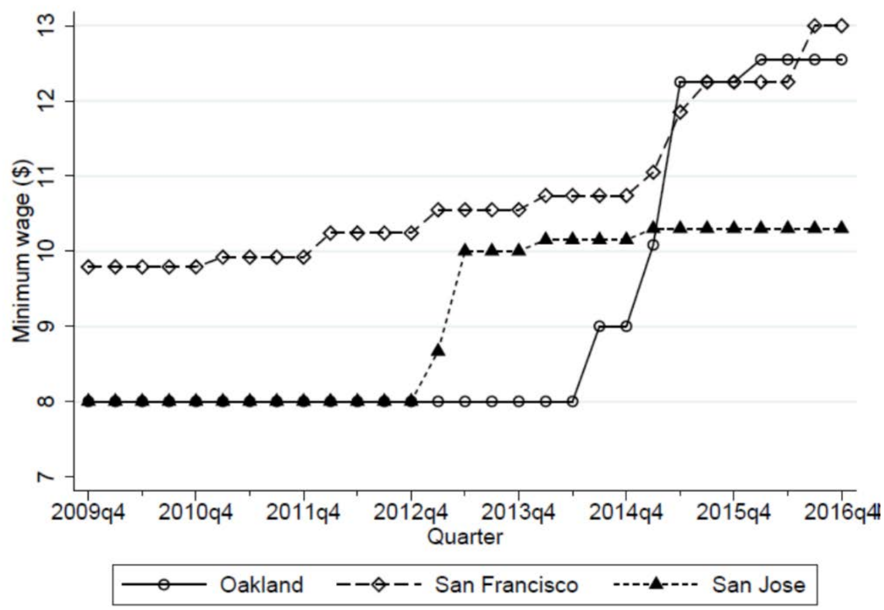
The last row of Table 1 displays the *percentage of each city's workers* who will ultimately receive pay increases directly because of the city's policy. This percentage, often referred to as the policy's bite, provides an intuitive measure of the scope of each city's policy. The bites, which range between 14 and 31 percent, are each well above the eight percent maximum bite for all the federal and state minimum wage increases between 1979 and 2014 (Autor, Manning and Smith 2016).

To summarize, Table 1 indicates that the six cities on the whole were experiencing employment and income growth during the policy implementation period and that their median earnings and living costs were higher than the national average. Nevertheless, prior to the new minimum wage policies, food service workers in each city earned much less than other workers. The policies so far have raised the minimum wage levels relative to median wages, but not above the ratios in previous U.S. experience. On the other hand, at full implementation, the new wave of policies will increase pay for higher proportions of each city's workforce, relative to our previous experience of federal and state minimum wage policies.

## *The minimum wage policies*

The six cities adopted minimum wage policies at varying levels and with varying rates of implementation. Figure 1 displays the evolution of the minimum wage during our study period in the three California cities in our sample, Oakland, San Francisco and San Jose. Figure 2 does the same for Chicago, the District of Columbia and Seattle. As the two figures show, these six cities implemented thirteen minimum wage increases during our study period (not counting inflation adjustments).

**Figure 1 Minimum wage policies: Oakland, San Francisco, San Jose**



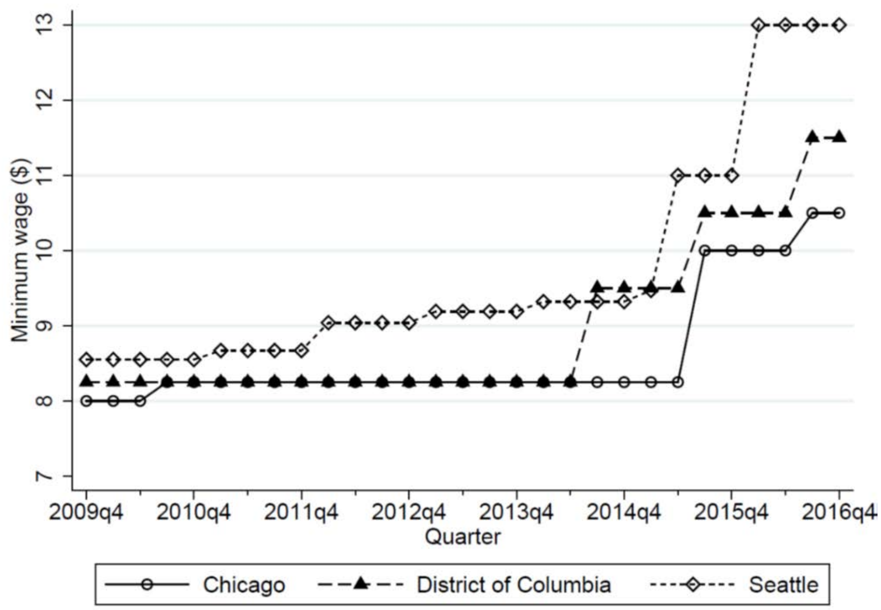

Notes: The evolution of the minimum wage in Oakland, San Francisco and San Jose. When the minimum wage increases in the middle of a quarter, the figure plots the average minimum wage over the months within the quarter. For cities that allow for subminimum wages, such as for tipped workers and workers in small firms, we use the highest minimum wage in effect.

Among the California cities, San Francisco had the highest local minimum wage in 2012, $10.24, which increased annually with regional inflation until 2015. The city then raised its minimum wage to $12.25 in May of 2015 and to $13 in July of 2016. San Francisco's minimum wage thus increased a total of 27 percent during our study period. San Francisco was also the first city in the U.S. to establish a $13 minimum wage for all workers in a city.

Oakland and San Jose both began our study period at the $8 California minimum wage. Each city then increased its minimum wage rapidly. Oakland's minimum wage increased from $8 to $9 in the first quarter of 2014 (2014q3), as a result of the California statewide minimum wage increase. The city's minimum wage then rose from $9 to $12.25 in a single step in 2015q2—an overnight increase of 36 percent and a total increase of 53 percent over two quarters. Oakland then indexed its minimum wage to regional inflation beginning in 2015. San Jose's minimum wage rose overnight by 25 percent, from $8 to $10 in March of 2013. The city then indexed the minimum wage to regional inflation beginning in 2015, resulting in an overall increase of 29 percent by 2016.

Figure 2 displays the evolution of local minimum wages in our three other cities: Chicago, the District of Columbia and Seattle.[12] In 2010, Chicago's minimum wage increased modestly along with Illinois' statewide minimum wage change, from $8 to $8.25. The city level then increased to $10 in 2015q3 and to $10.50 in 2016q3. The overall increase was thus 27 percent. Meanwhile, the District of Columbia's minimum wage rose from $8.25 to $9.50 in 2014q3, then to $10.50 in 2015q3 and to $11.50 in 2016q3. The District's minimum wage overall increase was thus 39 percent. Finally, Seattle's minimum wage rose from $9.47 to $11 in 2015q2 and then to $13 in 2016q1, or a 37 percent increase in total.

**Figure 2 Minimum wage policies: Chicago, District of Columbia and Seattle**




Notes: The evolution of the minimum wage in Chicago, the District of Columbia and Seattle. When the minimum wage increases in the middle of a quarter, the figure plots the average minimum wage over the months within the quarter. For cities that allow for subminimum wages, such as for tipped workers and workers in small firms, we use the highest minimum wage in effect.

Summarizing to this point, minimum wages in these six cities varied in their initial levels and in the speed and magnitude of their increases. San Francisco began at the highest initial level. Oakland experienced both the most rapid and largest increase (53 percent).

In our evaluation, we will use the percent increase in a city's minimum wage level, adjusted for the length of its phase-in period.

[12] Each of these three cities' policies allowed for subminimum wages, for example for tipped workers and for workers in small firms. To simplify our discussion and our analysis, we ignore these subminimum wages in what follows.

# PART 3 DATA

We use the U.S. Bureau of Labor Statistics' Quarterly Census of Employment and Wages (QCEW) administrative data for our analysis. The QCEW publishes a quarterly count of employment and wages reported by employers that belong to the Unemployment Insurance (UI) system, which covers more than 95 percent of all U.S. jobs. The data are aggregated at the county level and are available by detailed industry. The QCEW is frequently used in minimum wage and other labor market studies.

We obtained QCEW data for all U.S. counties from the QCEW website of the U.S. Bureau of Labor Statistics. Two cities in our study, the District of Columbia and San Francisco, are coterminous with their counties. The four other cities are located within larger counties. To analyze these cities, we obtained city-level QCEW tabulations from city or state agencies.[13]

As in administrative datasets generally, the employment and earnings figures reported in the QCEW are not prone to the sampling errors that are inherent in household surveys. Nevertheless, the QCEW data can be noisy—i.e., they can fluctuate significantly from one period to the next—especially for areas smaller than a county. This noise can be generated when businesses change location, name, or their industry code. In addition, large fluctuations can occur when multi-site businesses change whether they report their employment and earnings figures separately for each location or decide to consolidate their data and report as a single, multi-site business.[14]

For our earnings analyses we use the QCEW average weekly wage, which is constructed as the ratio of total industry payroll to employment, divided by 13 (52 weeks / 4 quarters). Since this variable reflects both the hourly wage paid to workers and the number of hours worked every week, we refer to this variable as average weekly earnings, or, simply, average earnings.

The rich local data in the QCEW makes it the only public dataset available for studies of local minimum wage policies in multiple locations.[15] The sample size of the Current Population Survey—another commonly used public dataset—is too small for use at the city level. The American Community Survey contains enough observations, but its annual frequency is insufficient for minimum wage analysis.

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[13] Quarterly employment represents aggregate counts of all filled jobs, whether full or part-time, temporary or permanent. The QCEW reports establishment-based monthly employment levels for the pay periods that include the twelfth of the month.

[14] To check whether these reporting issues may bias our results, we have examined whether the variables in our analysis are noisier in Chicago, Oakland, San Jose and Seattle (the four cities that are not coterminous with their counties) than in the counties that we include in the cities' comparison groups. We measure the level of noise in each variable by its standard deviation during the period before the minimum wage policy went into effect. (We first de-trend each variable before computing its standard deviation to distinguish noise from overall growth across the localities). We find that the amount of noise in the variables in these cities are typically within the range observed in other counties, even those with comparable levels of private sector employment. We conclude it is unlikely these reporting issues are biasing our results. Results are available upon request.

[15] Although a few states have made the microdata underlying the QCEW available to selected researchers, many states are not able to do so for legal reasons. The Quarterly Workforce Indicators (QWI), a product of the U.S. Census Bureau, provides similar county-level data as the QCEW and also contains limited demographic information. Officials at Census were not able to provide us with city-level QWI data.

# PART 4 RESEARCH DESIGN

## 4.1 Outcomes

We study the effects of the six cities' minimum wage policies on workers employed in the food services and drinking places industry—hereafter referred to as food services.[16] Composed mainly of restaurants and bars, the food services industry is a major employer of low-wage workers, employing 8 percent of the workforce in 2016 and paying the median worker $9.96 per hour.[17] As such, wages in this industry are strongly influenced by minimum wage policies: Recent analysis indicates that 67.8 percent of food services workers would be affected by the Raise the Wage Act of 2017, a proposal to raise the federal minimum wage to $15 per hour by 2024 (Cooper 2017).

For workers in food services, we measure the effects of the minimum wage on quarterly aggregates of average weekly earnings and total employment, as reported in the QCEW data. The measures include all workers in the industry, even those who are not affected by the minimum wage. As a result of this aggregation, the effects we measure are a weighted average of effects among workers with potentially different responses to the policy. We discuss how this aggregation affects the interpretation of our results in Part 8.

## 4.2 Evaluation strategy

To identify the causal effects of local minimum wage policies, the methods we use must be able to distinguish changes attributable to the policies from other factors that influence the evolution of average earnings and employment over time. To do so, we consider each city as a separate quasi-experiment and measure the effect of the policies by comparing the changes in earnings and employment in each of the six cities against the changes that we observe in other localities across the U.S. This approach is often called the "difference-in-differences" method, because if we observed only one city with a local minimum wage policy and only one comparison locality, and if our data included only two points in time (one before and one after the increase), the estimated effect would be the difference between the change in the city and the change in the comparison locality.

Ideally, for each "treated" city in our study that passed a local minimum wage policy, we would have data on an "untreated" comparison locality that had no minimum wage change. Moreover, the comparison locality and the city would exhibit trends in employment and weekly earnings that would be parallel but for the effect of the policy. In this study, we use two complementary methods—event study and synthetic control—that approximate this ideal scenario under different assumptions. Both methods isolate the causal effect of the local minimum wage policies by comparing the changes we observe in the six treated cities against a group of untreated comparison counties across the United States.

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[16] The food services industry is NAICS 722. Its full title is "food services and drinking places."
[17] Bureau of Labor Statistics, Occupational Employment Statistics, May 2016.

To construct our comparison groups, we include counties that had no change in their minimum wage policy during our period of study. To maximize the number of counties we can include, we begin our period of study in 2009q4, one quarter after the federal minimum wage increased to $7.25 per hour. For the District of Columbia, Oakland and San Jose, we include counties that had no minimum wage increase between 2009q4 and 2016q4. For San Francisco and Seattle, which previously indexed their minimum wage to inflation, we include counties in states that also indexed their minimum wage and had no other minimum wage increases between 2009q4 and 2016q4. For Chicago (whose state-level minimum wage increased to $8.25 in 2010q3), we include counties that had no minimum wage increase between 2010q4 and 2016q4.

In addition to requiring each county in a comparison group to have no change in its minimum wage policy, we only include counties in a metropolitan area with an estimated population of at least 200,000 in 2009q4.[18] By restricting our comparison group to only counties meeting these criteria, we are able to distinguish the effects of the policies from other changes that occurred to other heavily populated, metropolitan areas during the same period.

Table 2 reports the number of comparison areas we use to measure the effect of the local minimum wage policies and provides additional information on our research design. We have 99 counties in the comparison groups for the District of Columbia, Oakland, and San Jose; 60 counties for San Francisco and Seattle; and 113 counties for Chicago.

**Table 2 Policy evaluation context, by city**

| | Chicago | District of Columbia | Oakland | San Francisco | San Jose | Seattle |
|---|---|---|---|---|---|---|
| Comparison group MW policy[a] | No increases | No increases | No increases | Indexed to inflation | No increases | Indexed to inflation |
| Counties in comparison group | 113 | 99 | 99 | 60 | 99 | 60 |
| Pre-policy period[b] | 2010q3–2015q2 | 2009q4–2014q2 | 2009q4–2014q2 | 2009q4–2015q1 | 2009q4–2012q4 | 2009q4–2015q1 |
| Pre-policy MW[c] | $8.25 | $8.25 | $8.00 | $11.05 | $8.00 | $9.47 |
| Evaluation period[d] | 2015q3–2016q2 | 2014q3–2016q4 | 2014q3–2016q4 | 2015q2–2016q4 | 2013q1–2016q4 | 2015q2–2016q4 |
| Average MW over evaluation period | $10.00 | $10.30 | $11.50 | $12.41 | $10.10 | $12.14 |
| Average MW increase[e] | 19.2% | 21.9% | 35.5% | 11.5% | 23.3% | 24.5% |

Notes: [a] Indicates whether the comparison group includes counties that either (1) have no minimum wage increases between the pre-policy and evaluation periods (No increases) or (2) includes counties that index their minimum wage to inflation (Indexed to inflation). [b] The quarters before the minimum wage increase that we use in our analysis. [c] The minimum wage in the city at the end of the pre-policy period. [d] The quarters after the minimum wage increase that we use to measure the effect of the policy on earnings and employment. [e] Average log minimum wage during the evaluation period minus the log minimum wage at the end of the pre-policy period.

The comparison counties for Chicago, the District of Columbia, Oakland, and San Jose are located throughout the South as well as parts of the Midwest and Northeast. The comparison counties for San

[18] Counties are in a metropolitan area if they lie in a Census Core-based statistical area (CBSA). To determine whether a county lies in a CBSA, we use CMS's "SSA to FIPS CBSA and MSA County Crosswalk" for fiscal year 2015. These data are released by the NBER: http://www.nber.org/data/cbsa-msa-fips-ssa-county-crosswalk.html (last accessed January 24, 2018).

Francisco and Seattle are located primarily in Florida, Ohio and Washington, and also include parts of Arizona, Colorado and Missouri.[19]

The new policies raised the level of the local minimum wage in the six cities by different magnitudes and at different speeds, as illustrated in Figures 1 and 2 above. In our analysis, we abstract from these differences in implementation and measure the average effect of the local policies from the quarter the city first increased its local minimum wage. That is, we evaluate each city's local policy as if it were a single event. We call the quarters before the city implemented the policy the *pre-policy* period, and quarters afterward the *evaluation* period. Table 2 reports each city's pre-policy and evaluation period.

As a summary of each city's local minimum wage policy, Table 2 also reports what we call the *average increase in the minimum wage*. This variable measures each city's percent increase in the minimum wage during the evaluation period relative to its pre-policy level and incorporates changes in the minimum wage due to phase-ins and cost-of-living adjustments. We compute the average increase in the minimum wage by subtracting the log of the pre-policy minimum wage from the average log minimum wage during the evaluation period. Table 2 reports that the average minimum wage increase ranges from 11.5 percent in San Francisco to 35.5 percent in Oakland. In general, we expect to find the largest earnings increases (and, potentially, the largest employment effects) in the cities with the largest minimum wage increases.

Table 3 presents an array of descriptive statistics for the six cities and the comparison counties. Compared to the six cities, the comparison counties on average have smaller private sectors, pay lower wages, and experienced slower growth in the aftermath of the Great Recession. The second row in Panel A, labeled "total earnings, private sector," shows the total earnings of all private sector workers—a measure of the size of the local economy—during 2012. Total private sector earnings in the six cities were $\left(\frac{8728.9}{2748.6}=\right)$ 3.2 times larger than the comparison counties, on average. Average earnings and employment of food services exhibit smaller but nonetheless important differences between the cities and the comparison counties. Relative to food services in the comparison counties, food services in the six cities employed over twice as many workers, who earned on average about 1.4 times more each quarter. These differences in compensation reflect differences in previous minimum wage policies, as well as living costs and other underlying economic conditions.

Table 3 also reports the average earnings and employment of workers in two food service sub-sectors, full service and limited service restaurants. Similar to what we find for food services as a whole, more workers are employed in restaurants in the six cities than in the comparison counties; on average these workers earn more as well. In both the six cities and the comparison counties, workers in limited service restaurants earn on average about 80 percent of those in full service restaurants. As a result, we expect the cities' local minimum wage laws to have a larger effect on limited than full service restaurants. (We return to this prediction in Part 7.)

[19] See Appendix Figure 1 for a map of the comparison counties for each city.

## Table 3 Average characteristics of our six cities and comparison counties

| | Six cities[a] (1) | Comparison counties[b] (2) |
|---|---|---|
| **Panel A: Annual average, 2012[c]** | | |
| Population (1000s) | 1033.2 | 584.4 |
| Total earnings, private sector (Millions) | $8,728.9 | $2,748.6 |
| Food services | | |
| Average weekly earnings | $409.4 | $298.9 |
| Employment (1000s) | 44.7 | 20.8 |
| Full service restaurants | | |
| Average weekly earnings | $441.3 | $321.6 |
| Employment (1000s) | 23.2 | 10.1 |
| Limited service restaurants | | |
| Average weekly earnings | $342.9 | $259.3 |
| Employment (1000s) | 11.6 | 7.5 |
| Professional services | | |
| Average weekly earnings | $2,131.0 | $1,273.4 |
| Employment (1000s) | 72.0 | 17.1 |
| **Panel B: Percent change, 2009–2012[d]** | | |
| Population | 2.8 | 3.5 |
| Total earnings, private sector | 13.4 | 10.9 |
| Food services | | |
| Average weekly earnings | 6.6 | 6.9 |
| Employment | 11.7 | 6.9 |
| Full service restaurants | | |
| Average weekly earnings | 7.0 | 7.8 |
| Employment | 13.9 | 6.6 |
| Limited service restaurants | | |
| Average weekly earnings | 4.6 | 5.2 |
| Employment | 8.5 | 7.2 |
| Professional services | | |
| Average weekly earnings | 10.0 | 7.5 |
| Employment | 8.9 | 4.6 |
| Observations | 6 | 173 |

Notes: [a] Averages across Chicago, Oakland, San Francisco, San Jose, Seattle and the District of Columbia. [b] Averages across all untreated counties in the comparison groups. [c] Sample means during 2012. We compute the mean of each variable by averaging over the quarterly observations in the group indicated by the column heading. [d] Percent change in the sample means between 2009 and 2012.

The differences we find between the six cities and their comparison counties suggest that even if the cities had not increased their minimum wages, average earnings and employment of workers in the comparison counties would not have followed the same trend as in the six cities. To test this important issue, we examine whether they evolved similarly during the years preceding the minimum wage increase. Panel B of Table 3 reports earnings and employment changes from 2009 through 2012 for both sets of areas. Food services employment grew about $\left(\frac{11.7}{6.9} - 1 \times 100 \approx\right)$ 70 percent faster in the six cities than in the comparison counties but experienced similar growth in average earnings during this period. Table 3 also shows that the overall private sector grew about twenty percent faster in the six cities.

Together, the differences between the six cities and the comparison counties suggest that simple comparisons between these groups alone would not accurately isolate the true causal effect of the local minimum wage policies. In Parts 5 and 6, we describe and use two statistical methods—event study and synthetic control—to evaluate the causal effect of the policies despite these underlying differences.

Both statistical methods assume that each city's food services industry during the years after the minimum wage increases can be modeled accurately using information from the years preceding the increase. This assumption may not hold. For example, Seattle contains the headquarters of Amazon, whose rapid expansion substantially reshaped Seattle's economy just as the city implemented its minimum wage policy (Tu, Lerman and Gates 2017).

To test this assumption, we conduct a falsification test to ensure that the effects we measure are not driven by contemporaneous changes that are unrelated to the minimum wage policies. Specifically, we test for effects on professional services, a high wage industry. Panel A of Table 3 reports that in 2012, professional service workers in the six cities earned on average over $2,000 per week, more than five times as much as food service workers earned. Thus, the earnings and employment levels of professional service workers should not be influenced by minimum wage laws, but they would be influenced by more general changes to the local labor market. If our analysis of the professional services industry does not reveal any significant earnings or employment effects, the effects we measure in food services are less likely to reflect contemporaneous changes that are not policy-related.

### 4.3 Measuring Earnings and Employment Elasticities

To benchmark our estimated effects to those from previous studies, we report our estimated earnings and employment effects as elasticities. The *earnings elasticity with respect to the minimum wage* equals the percent change in average earnings from each one percent increase in the minimum wage. The *employment elasticity with respect to the minimum wage* equals the percent change in employment from a one percent increase in the minimum wage. Throughout our analysis, we measure outcomes such as average earnings and employment in logs so that the effects are interpretable as percent increases. To compute elasticities, we then scale these effects by the average increase in the minimum wage.

## PART 5 EVENT STUDY ANALYSIS

### 5.1 Method

We first measure the effect of local minimum wage policies using an event study model. An event study generalizes the difference-in-differences approach by measuring the effect of a policy at each quarter around the time of implementation. This approach allows us to test whether the treated six cities and the untreated comparison counties trended together during the pre-policy period. In addition, the event study approach provides a convenient way to pool our results, incorporating variation in the timing of the minimum wage policies across the six cities.

We estimate the event study model using linear regression. Since we have a number of comparison counties for each city, regression allows us to account for observable differences between the groups by including control variables in the model. The event study model then measures the effect of the policy separate from changes in earnings and employment caused by changes in the control variables.

We first establish the conceptual framework of an event study by depicting the results for employment in food services from a model without any control variables in Figure 3. The vertical line at time zero represents the quarter that the minimum wage policies were implemented for each city. For example, for Seattle, quarter zero represents 2015q2, when the minimum wage increased from $9.47 to $11; for the District of Columbia, zero represents 2014q3, when the minimum wage increased from $8.25 to $9.50. Negative values (to the left of the zero line) represent the quarters leading up to the end of the cities' pre-policy periods, and positive values (to the right of the zero line) represent quarters following the beginning of the evaluation periods, as reported in Table 2.

Figure 3 plots the parameters (i.e., the estimated effects) from the event study model and forms the basis for our estimates of the causal effect of the policies. Specifically, each point measures, during a given quarter, the difference between employment in each city and employment in its comparison counties (averaged over all six cities). We normalize the parameters so that this difference equals zero one quarter before the policy went into effect—that is, at -1 on the horizontal axis. The rising trend of the points between -13 and -2 on the horizontal axis indicate stronger employment growth in the six cities relative to the comparison counties in the three years preceding the increase. The points between 0 and 6 on the horizontal axis indicate employment growth in the six cities after the minimum wage increased, relative to employment growth in the comparison counties. For example, point 6 indicates that, seven quarters into the evaluation period, employment in the six cities grew on average 6.2 percent more than in the comparison counties.

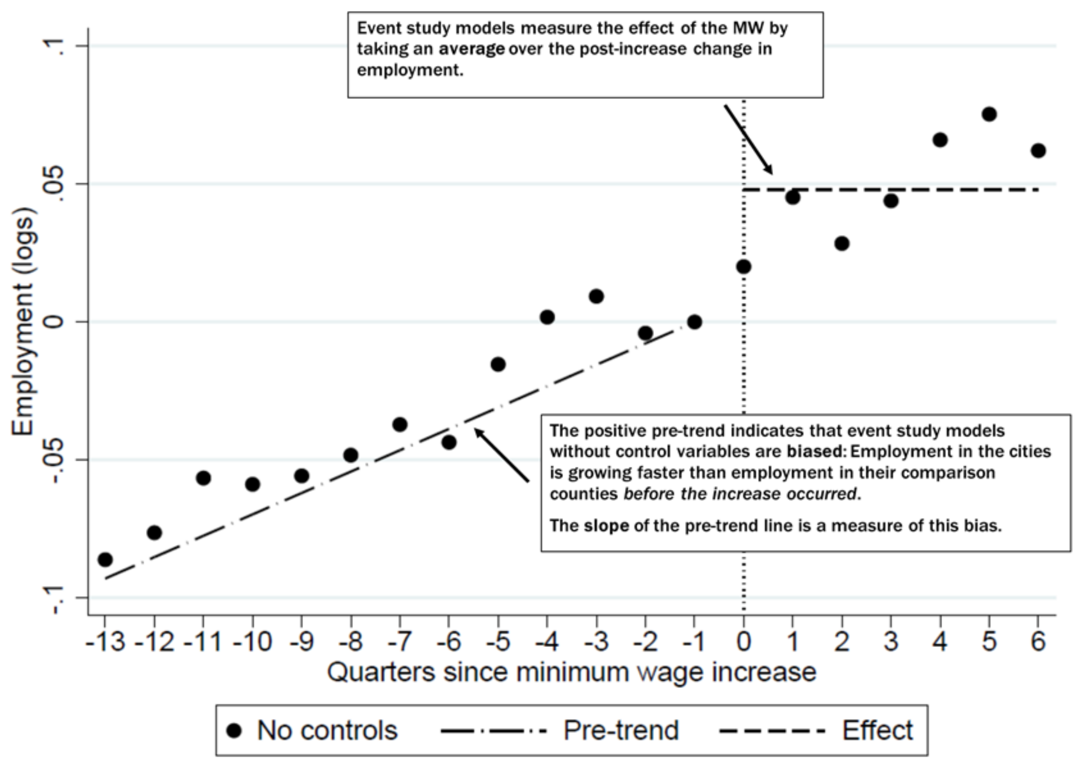
The event study model measures the effect of the policy by taking a weighted average over the points on the horizontal axis between 0 and 6.[20] The horizontal dashed line plots the estimated effect and shows that the model finds the minimum wage *increased* employment about 4.8 percent in our six city sample. However, this interpretation is accurate only if the comparison counties would have trended similarly to the six cities if the minimum wage had *not* increased.

The point at -13 indicates that 13 quarters prior to the minimum wage's implementation, the difference in the cities' employment relative to employment in the counties was 8.6 percent lower than it was 1 quarter before the minimum wage was increased. In other words, during the three years preceding the end of the pre-policy period, employment in the six cities grew on average 8.6 percent

[20] The weights used in the average are based on, for example, the number of cities with information on employment at each point during the evaluation period.

more than in the comparison counties. Employment in the six cities thus did not trend with the comparison counties during the years preceding the increase. It is therefore unlikely that the points following the increase represent only the effect of the minimum wage policy.

**Figure 3 Event study methodology, employment**




Notes: This figure plots coefficients from an event study of food services employment, measured in logs. We estimate the coefficients using an event study model that compares each city against the untreated counties in its comparison group. The model is normalized such that each coefficient represents the change in log employment relative to the end of the pre-policy period (point -1 on the horizontal axis).

The higher employment growth in the cities during the pre-policy period suggests that, without any control variables, the event-study-based measure of the minimum wage effect is biased against finding employment losses. To test for the presence of this bias directly, we measure the slope of a line based on the points between -13 and -2 on the horizontal axis. Our statistical test finds a non-zero slope, indicating that the comparison counties do not trend in parallel with the six cities. Following previous studies that use event study methods, we call this a test of the *parallel trends assumption*, and we call the differential growth between the groups during the pre-policy period a *pre-trend*.[21]

The different trends in average earnings and employment revealed by our event study analysis between the six cities and the comparison counties may be attributable to other differences between the two groups, such as population growth. We account for these differences by including them as control variables in the regression models. The parameters of the event study model for each of the

[21] An alternative test of the parallel trends assumption examines (jointly) whether each of the points between -13 and -2 on the horizontal axis is zero. We are unable to perform this test because our sample includes only six cities that enacted a local minimum wage policy, and these cities are located in only four states (including the District of Columbia).

quarters then indicate only the differences between the cities and untreated counties that cannot be explained by the control variables. Moreover, we can test whether the pre-policy differences form a pre-trend. As we have already suggested, if the test—controlling for differences in these other factors—does not find a pre-trend, we can infer that the six cities and comparison counties are likely to trend together during the evaluation period, and the model with control variables will better isolate the effect of the minimum wage policies.

We include two control variables in our models. The first measures the population of the city or county, as estimated annually by the U.S. Census Bureau. The second variable measures the total payroll of all private sector workers in the city or county, which approximates the size of the local economy.[22] By controlling for different growth rates in the treated cities, we reduce possible biases in the event study estimation. Previous studies of state level minimum wage policies include similar variables.[23]

In our event study analysis, we perform hypothesis tests and construct confidence intervals under two alternative assumptions about how the data are correlated. Under the first, we assume that the data are grouped into 179 "clusters": the six treated cities and the 173 untreated counties. Under the second, we assume that the data are grouped into only 28 clusters, one for each state in our sample (including the District of Columbia). We group the data into clusters to control for correlations in the data within the group—either city and county or state—over time. The number of clusters used is likely to affect the standard error and confidence interval calculations.

We first cluster the data at the city and county levels, the level at which the policies were enacted. However, if the data between cities and counties in the same state are correlated, it may be more appropriate to cluster at the state level. In this case, clustering at the city and county level may lead us to overstate the statistical significance from our tests. On the other hand, clustering the data at the state level could risk understating the statistical significance, if clustering at the city and county level is more appropriate.[24] With these tradeoffs in mind, we report the results both ways: clustering at either (1) the city and county or (2) the state level.

We compute p-values for the results of our hypothesis tests. Each p-value measures the likelihood that the event study model would yield the estimated effect if the true effect were zero. A p-value of less than 10 percent indicates a statistically significant effect. We also report confidence intervals that denote the range of effects that we cannot reject at a 10 percent significance level.[25]

[22] We include annual averages of this variable during the years 2007, 2008, and the first three quarters of 2009. These averages control for growth in average earnings or employment that would be associated with the size of their local economy during the years immediately preceding and following the Great Recession.
[23] See, for example, Allegretto et al. (2017), Addison et al. (2014), and Meer and West (2016).
[24] The correct clustering level can depend upon the context, such as the timing of policies within a state or the spatial size of the relevant labor market. High-wage labor markets, such as professional services, are spatially bigger than low-wage labor markets, such as food services. Rather than attempting to determine which clustering level is correct for our case, we provide results for both clustering assumptions. See Abadie et al. (2017) for a recent discussion of these issues.
[25] We compute p-values and confidence intervals using a "wild bootstrap" (Cameron, Gelbach and Miller 2008). Previous studies indicate that conventional approaches for conducting hypothesis tests with clustered data may be biased when the

## 5.2 Event study results

We now turn to the results of our event study analysis. First, we present a graphical depiction of our results on food services for earnings and employment. We then delve further into how our model specifications performed and more information on our results.

Figure 4 plots parameters estimated by two separate event study specifications of average earnings. The dashed line labeled "No controls," plots the growth in the six cities relative to the untreated comparison counties without any adjustment for differences in population or private sector size.[26] The line's position at -13 indicates that average earnings in food services grew about 4.4 percent during the final 13 quarters of the pre-policy period. This pre-policy growth in earnings suggests that earnings increases after the policy (between 0 and 6) are partly attributable to other factors that would increase earnings regardless of the higher minimum wage.

The solid line in Figure 4 labeled "Controls" plots the earnings growth in the six cities relative to the comparison counties after accounting for population growth and differences in private sector size (measured by the total earnings paid to private sector workers between 2007 and 2009q3). This model finds only a 3.7 percent growth during the three-year pre-policy period, which is then followed by a sudden 3.2 percent jump in earnings in the first two quarters of the evaluation period. During the next five quarters, earnings continue to rise, averaging a 5.1 percent increase.

Overall, the results indicate—after we account for changes attributable to population growth and the size of the local economy—that the minimum wage policies increased earnings about 4 percent. More rapid growth in earnings began within a quarter of the increase in the minimum wage, suggesting the earning growth is at least partly attributable to the policy. Nevertheless, the modest pre-trend that remains even after we add control variables to the model suggests this increase in earnings may overstate the true effect of the policy. We return to this issue when discussing Table 4.

Unlike the event study results for average earnings, the results for food service employment—displayed in Figure 5—suggest that the growth in employment in the six cities during the evaluation period is attributable to factors such as population growth, not to the increase in the local minimum wage. The line labeled "No controls" corresponds to the points depicted in Figure 3 and shows that employment grew about 8.6 percent relative to the comparison counties during the three years preceding the minimum wage increase. This trend continued during the evaluation period, suggesting little influence of the minimum wage policy.

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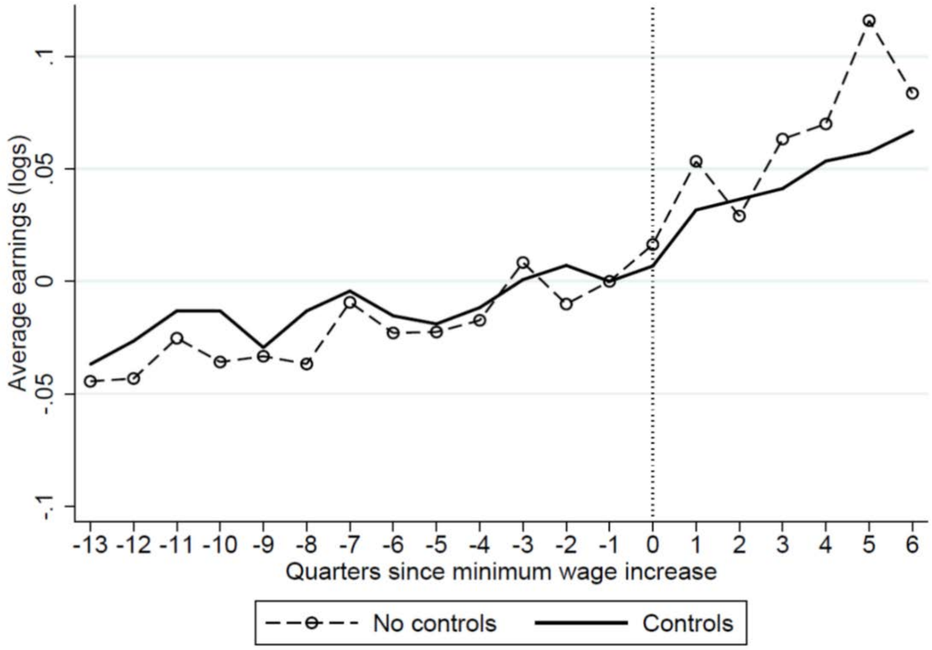
number of clusters affected by the policy is small. Depending on how we cluster, we have either six treated city clusters or four treated state clusters. We use the wild bootstrap because studies have shown it to be more robust in settings with small numbers of clusters (e.g., Cameron and Miller 2015). See Appendix A.1 for more information on how we apply the wild bootstrap.

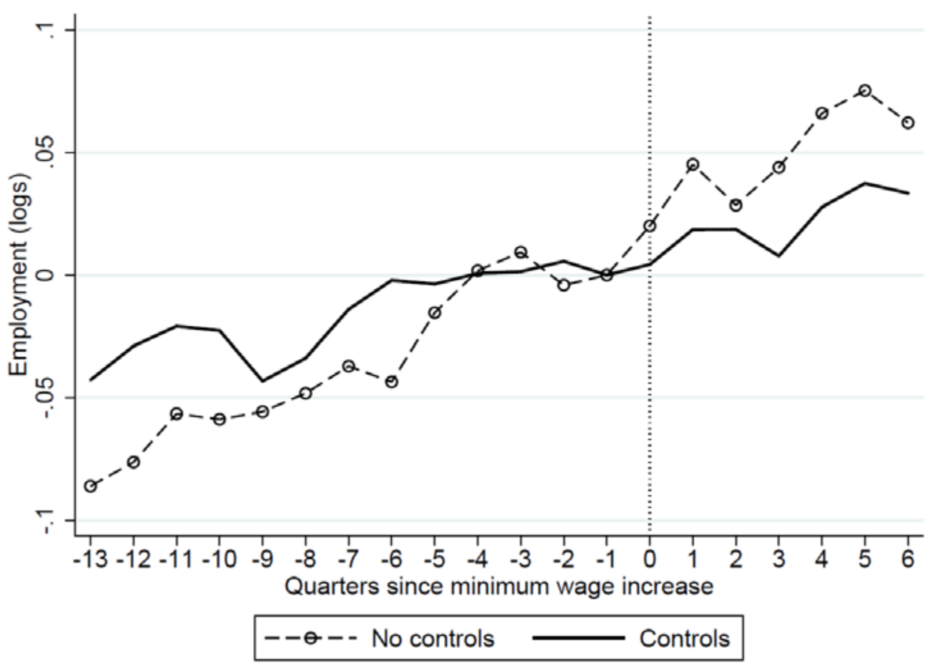
[26] The line in Figure 4 labeled "No controls" corresponds to an event study model that includes comparison group-specific calendar time effects and county effects. By including these variables in the model, the parameters plotted in Figure 4 represent the growth, on average, in earnings in each treated city relative to its comparison counties. See Appendix A.1 for more information on how we specify the event study model.

**Figure 4 Event study earnings estimates**




Notes: This figure plots coefficients from event studies of average earnings in food services, measured in logs. Models are normalized such that each coefficient represents the difference in log average earnings relative to the end of the pre-policy period (point -1 on the horizontal axis). The line labeled "No controls" reports coefficients from an event study model that compares each city against the untreated counties in its comparison group. The line labeled "Controls" reports coefficients from a model that controls for differences in population and private sector size across localities.

**Figure 5 Event study employment estimates**




Notes: This figure plots coefficients from event studies of food service employment, measured in logs. Models are normalized such that each coefficient represents the difference in log employment relative to the end of the pre-policy period (point -1 on the horizontal axis). The line labeled "No controls" reports coefficients from an event study model that compares each city against the untreated counties in its comparison group. The line labeled "Controls" reports coefficients from a model that controls for differences in population and private sector size across localities.

The solid line labeled "Controls" shows the employment change in the six cities relative to the comparison counties that cannot be explained by growth in the overall population or differences in private sector size (captured by our two control variables). Once we include these variables in the model, we find employment in the six cities grew only 4.3 percent during the pre-policy period. The attenuation in pre-policy growth between the models with and without controls indicates that $\left(\frac{8.6-4.3}{8.6} \times 100 =\right)$ 50 percent of the difference between the six cities and the comparison counties can be accounted for by the control variables alone.

During the last year and a half of the pre-policy period (quarters -7 through -2), the solid-line labeled "Controls" in Figure 5 is close to zero, indicating—after accounting for differences in population growth and private sector size—that employment grew in parallel between the six cities and the comparison counties right before the minimum wage increase. After the minimum wage increase, employment in the six cities departs from this trend and increases modestly relative to the comparison counties. This pattern suggests, if anything, that the minimum wage caused employment to expand.

Overall, Figures 4 and 5 suggest that raising the minimum wage had a clear effect on workers' earnings, but little, if any, effect on employment.

We now turn to Table 4, which displays our average estimated effects of the policies, presents our results as elasticities and offers insight into how well our specifications address the parallel trends criterion.

Columns 1-3 report the earnings effects with and without population and private sector size controls. Consistent with the graphical analysis in Figure 4, we find that, once we control for population and private sector size, the minimum wage policies increase average earnings about 4 percent (column 2). This increase is statistically significant (at the 5 percent level when we cluster at the city and county level and at the 10 percent level when we cluster at the state level), indicating it is very unlikely that this increase would have occurred without the policy.[27]

Columns 4-6 in Table 4 report the employment effects. The statistical significance of the smaller 2.1 percent increase reported in column 5 for employment depends on how we cluster the data. When we cluster at the city and county level, this effect is significant at the 5 percent level. But when we cluster at the state level, the effect is not significant. Regardless of how we cluster, the positive employment effect indicates the minimum wage did not lead to employment losses, consistent with the results depicted in Figure 5.

The rows labeled "P-value" under the "Test of parallel trends assumption" show the results of our statistical tests of whether the six cities and the comparison counties trended together during the quarters preceding the minimum wage increases. P-values below 0.1 indicate that the six cities and comparison counties do not trend together. Models that do not include control variables (columns 1

[27] We compute p-values and estimate 90 percent confidence intervals using a wild bootstrap procedure (Cameron, Gelbach, and Miller 2008). See Appendix A.1 for more information.

and 4) have significant pre-trends for both earnings and employment in food services. These results imply that the positive minimum wage effects estimated from the 'no controls' specifications are overstated. However, once we include control variables in our specifications (columns 2 and 5), we do not find any significant pre-trends—the reported p-values are greater than 0.1 regardless of how we cluster the data. The differences in the test results in models with and without control variables are consistent with the attenuation between the lines labeled "Controls" and "No controls" in the pre-policy growth plotted in Figures 4 and 5. Together, these results suggest that the event-study-based effects reported in columns 2 and 4 are measured without bias.

**Table 4 Event study results**

| | Food services | | | | | |
|---|---|---|---|---|---|---|
| | Avg. earnings (logs) | | | Employment (logs) | | |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Effect of MW increase | 0.060**† | 0.040**† | 0.022**† | 0.048**†† | 0.021** | -0.005 |
| P-value (179 city and county clusters) | 0.004 | 0.026 | 0.014 | 0.002 | 0.034 | 0.501 |
| 90% CI (179 city and county clusters) | [0.043,0.079] | [0.015,0.060] | [0.014,0.030] | [0.013,0.090] | [0.006,0.056] | [-0.020,0.007] |
| P-value (28 state clusters) | 0.075 | 0.059 | 0.062 | 0.046 | 0.129 | 0.631 |
| 90% CI (28 state clusters) | [0.023,0.088] | [0.017,0.089] | [0.012,0.037] | [0.007,0.078] | [-0.003,0.071] | [-0.021,0.009] |
| Elasticity with respect to the MW[a] | 0.288**† | 0.212**†† | 0.131**†† | 0.227**† | 0.111** | -0.029 |
| P-value (179 city and county clusters) | 0.000 | 0.044 | 0.006 | 0.001 | 0.040 | 0.507 |
| 90% CI (179 city and county clusters) | [0.202,0.377] | [0.045,0.400] | [0.083,0.185] | [0.099,0.396] | [0.024,0.230] | [-0.119,0.039] |
| P-value (28 state clusters) | 0.057 | 0.048 | 0.026 | 0.051 | 0.180 | 0.654 |
| 90% CI (28 state clusters) | [0.118,0.423] | [0.080,0.305] | [0.075,0.198] | [0.037,0.372] | [-0.180,0.224] | [-0.128,0.064] |
| Test of parallel trends assumption | | | | | | |
| P-value (179 city and county clusters)[b] | 0.019 | 0.157 | – | 0.026 | 0.116 | – |
| P-value (28 state clusters)[c] | 0.084 | 0.228 | – | 0.077 | 0.231 | – |
| Controls for population, private sector[d] | No | Yes | Yes | No | Yes | Yes |
| Control for trend | No | No | Yes | No | No | Yes |
| Observations | 5132 | 5132 | 5132 | 5132 | 5132 | 5132 |

Notes: Significance tests and confidence intervals are based on a wild bootstrap using the empirical t-distribution, clustered at either the (1) city and county or (2) state level. **indicates significance at the 5 percent level when we cluster at the city and county level, *indicates significance at the 10 percent level. †† indicates significance at the 5 percent level when we cluster at the state level, † indicates significance at the 10 percent level. Each regression is estimated on a sample of 179 cities and counties in 28 states. All models include comparison group X quarter effects. [a] See footnote 28 for how we calculate the elasticity. [b] The p-value from testing whether a pre-trend (based on the estimated pre-policy effects of the minimum wage) has a slope of zero when we cluster at the city and county level. A p-value less than 0.1 indicates that we reject the parallel trends assumption. [c] The p-value from testing whether a pre-trend (based on the estimated pre-policy effects of the minimum wage) has a slope of zero when we cluster at the state level. [d] Reports whether the event study model includes control variables for population and private sector size.

The earnings elasticity implied by our event study results is consistent with earlier studies of state-level minimum wage policies reviewed in Part 2. To calculate the earnings and employment elasticities, we scale the estimated coefficients reported in Table 4, row 1 by the average increase in the minimum wage across the six cities during the evaluation period (quarters 0 through 6).[28]

[28] To calculate the earnings and employment elasticities, we fit two-stage least squares models. The elasticities these models yield is equivalent to dividing the estimated coefficients reported in Table 4, row 1, by an event study model-based measure of the average increase in the minimum wage across the six cities. The model without population and private sector control variables finds city minimum wages increased 21.0 percent on average; the model with controls finds minimum wages increased 19.1 percent. To then compute p-values and confidence intervals we apply a wild bootstrap

Column 2 of Table 4 reports that the 4 percent earnings effect implies an earnings elasticity of 0.21, shown in the sixth row, labeled "elasticity with respect to the MW." This elasticity can be interpreted to mean that—on average across the six cities—for every 10 percent increase in the minimum wage, food service earnings rose by 2.1 percent. The 2.1 percent employment effect reported in Column 5 implies a positive employment elasticity of 0.11. Although the earnings elasticity is consistent with earlier studies, the estimated employment elasticity is higher—and more positive—and may be attributable to the modest pre-trend that remains even after we include the population and private sector control variables in the model.

To assess the influence of the modest pre-trends that remain, we add to our event study models an adjustment for a linear trend. Intuitively, these models measure the effect of the minimum wage policy after first removing the changes in earnings and employment that would be expected based on the average quarterly growth during the pre-policy and evaluation periods.[29]

The results from including a linear trend in the event study models for earnings and employment are reported in Table 4, columns 3 and 6, respectively. As expected, the earnings and employment elasticities in these models are smaller than what we find in models that only control for population growth and private sector size. Nevertheless, the conclusions are similar. The earnings elasticity of 0.13 is similar to previous studies of restaurant workers and is statistically significant. The employment elasticity, though negative at -0.029, is small and statistically insignificant.

Although our estimated earnings and employment elasticities are similar to the consensus of estimates in previous restaurant studies, the confidence intervals depend somewhat on how we cluster the data; some are not precise enough to rule out meaningful employment effects. The eighth and tenth rows of Table 4 report the confidence interval for each elasticity at the city and county level and state level, respectively.[30] These intervals denote the range of elasticities that we cannot reject at a 10 percent significance level. Column 2 reports that the confidence interval from the model that controls for population growth and private sector size. When we cluster at the city and county level, the confidence interval rules out elasticities smaller than 0.05 or larger than 0.40. When we cluster at the state level, the confidence interval rules out elasticities smaller than 0.08 or larger than 0.31. When we add an adjustment for a linear trend (column 3), the model yields an interval that largely overlaps with that in column 2.

Column 5 reports the confidence intervals for the estimated employment elasticity in the event study specification that includes control variables. When we cluster at the city and county level, the confidence interval rules out elasticities lower than 0.02. But when we cluster at the state level, the confidence interval rules out negative elasticities lower than -0.18. The inclusion of a linear trend

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procedure directly to the elasticities estimated by the two-stage least squares model. These p-values and confidence intervals for the elasticities can differ from those we compute for the estimated coefficients reported in Table 4, row 1. See Appendix A.1 for more information on the two-stage least squares procedure and the wild bootstrap procedure.

[29] See Appendix A.1 for a formal description of the event study models that control for a linear trend.

[30] See Appendix A.1 for a description of the wild bootstrap procedure we use to compute p-values and confidence intervals for the elasticity estimates.

narrows the confidence interval. For example, column 6 reports that once we control for any linear trend, the confidence interval, when we cluster at the state level, rules out elasticities lower than -0.13 or higher than 0.06.

In summary, our event study analysis finds that the local minimum wage policies significantly increased food service earnings by about 4 percent and does not find any significant negative effects on employment. The validity of these findings rests on the parallel trends assumption: Had it not been for the minimum wage policies, earnings and employment in the treated cities would have followed the path of the average outcomes in the untreated comparison counties, at least conditional on the control variables of our models.

In the next part, we implement a synthetic control analysis that takes a different approach to measure the effects of the minimum wage policies. Using synthetic control, we can estimate minimum wage effects for each city separately. Individual cities that experienced larger increases in their local minimum wage should experience larger increases in their earnings, and, therefore, potentially larger reductions in employment. Results from the two methods together offer a compelling assessment of the minimum wage effects in the six cities.

# PART 6 SYNTHETIC CONTROL ANALYSIS

## 6.1 Method

In this part we use the synthetic control approach to measure the effect of the six cities' minimum wage policies.[31] The synthetic control design directly compares each treated city's earnings and employment during the evaluation period against those experienced by a "synthetic city." Intuitively, each synthetic city is constructed via an optimally-weighted average of untreated comparison counties so that the synthetic city tracks, as closely as possible, the average earnings and employment trends of the actual treated city during the pre-policy period. Any divergence between the actual and synthetic trends during the evaluation period thus reflects the effects of the policy.

The synthetic control for each city comprises a weighted average of untreated counties in the city's comparison group. Each actual city has a separate synthetic control for each outcome we measure (e.g., food service average earnings and food service employment). A computer algorithm selects a group of counties each weighted so that the synthetic city matches as closely as possible the actual city's trend in the outcome of interest during the pre-policy period.[32] Since the synthetic control trends with the city before the minimum wage went into effect, we expect that the synthetic would continue to trend with the city *but for* the policy—thus isolating the causal effect of the policy in the evaluation

[31] The synthetic control method was introduced by Abadie and Gardeazabal (2003) and Abadie, Diamond, and Hainmueller (2010).
[32] See Appendix A.2 for a formal description of how we apply synthetic control.

period. The data-driven nature of this procedure reduces the role of researchers' subjective judgments in determining the appropriate comparison group.[33]

To illustrate our synthetic control method, we begin with an example—our city analysis for Seattle. We then present results for each of the six cities in tabular form, followed by a graphical representation that depicts how we pool the city-specific results to measure earnings and employment elasticities.

Figures 6 and 7 illustrate how we use the synthetic control method to measure the effects of the local minimum wage in Seattle on earnings and employment in food services, respectively.[34] Figure 6 plots average earnings in food services between 2009q3 and 2016q3.[35] The solid line labeled "Seattle" shows Seattle's actual earnings during this period. The line labeled "Synthetic" shows Seattle's synthetic control, constructed to track Seattle's actual earnings through 2015q1, the end of the pre-policy period. The vertical dashed line marks the beginning of the evaluation period, after Seattle's minimum wage began to take effect.

The synthetic control results, depicted in Figure 6 for Seattle's food services, show that earnings in the synthetic and actual match one another closely during the pre-policy period. Although the synthetic constructed from the comparison counties is selected to match the actual during this period, the algorithm does not guarantee finding a close fit. To do so, the algorithm must find a weighted average that not only follows the same upward trend as Seattle's but also a similar seasonal pattern.[36]

To measure the quality of the pre-policy period match, we use a goodness of fit statistic introduced by Ferman and Pinto (2017a), called a pseudo R-squared.[37] When the match is perfect, the pre-policy pseudo R-squared equals one. Imperfect matches are associated with lower values of the pseudo R-squared, and extremely poor matches can yield negative values. The pseudo R-squared of the match depicted in Figure 6 is 0.98. The close fit suggests the synthetic is influenced by similar determinants as Seattle; it would have continued to follow Seattle's trend if the minimum wage had not increased.

To measure the effect of the policy, we compute the average difference between the actual and the synthetic control over the evaluation period. As Figure 6 shows, after the minimum wage increased from \$9.47 to \$11, average earnings in Seattle depart from the trend predicted by its synthetic control, indicating a positive effect of the policy. Over the evaluation period, actual Seattle earnings average 4.4 percent higher than in synthetic Seattle.

[33] As discussed in Part 4, to be included in our sample of untreated comparison counties, each county must be part of a metropolitan area with a population of at least 200,000 in 2009q4. Similar outcomes result if we include counties with a population of at least 100,000 in 2009q4 or only those with populations greater than 300,000. See Appendix Tables 1 and 2.

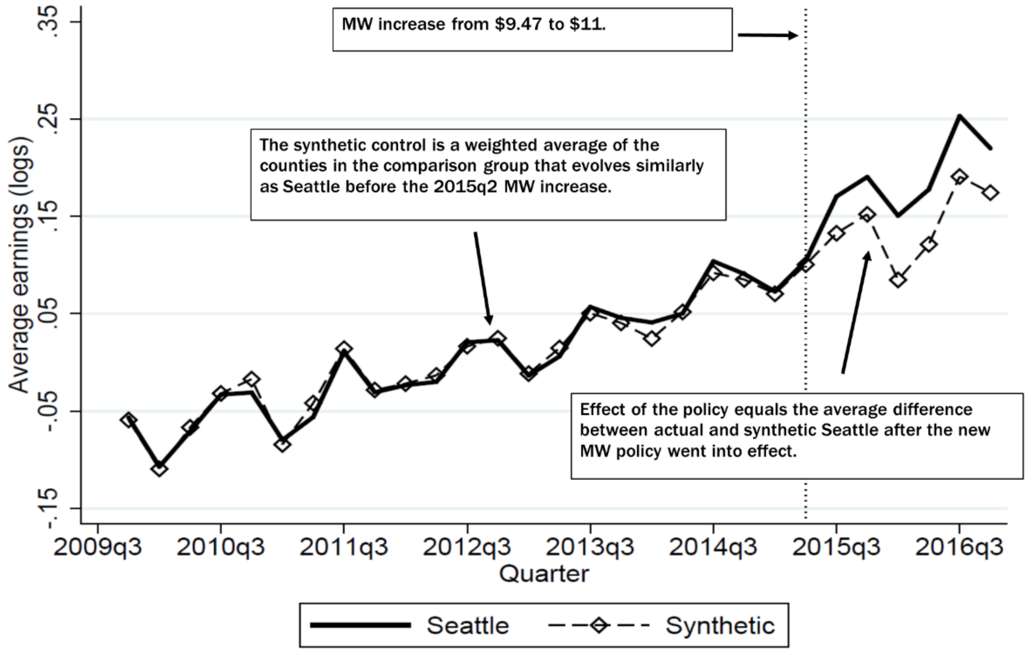
[34] Appendix Figures 2 and 3 show graphs for each of the six cities similar to Figures 6 and 7.

[35] We normalize the time series of log average earnings depicted in Figure 6 by subtracting from each quarter Seattle's average value during the pre-policy period. We perform this normalization for each city and comparison county in our sample, on each outcome we study. See Appendix A.2 for more information.

[36] We report the weights we use to construct synthetic control for each of the six cities in Appendix Table 3.

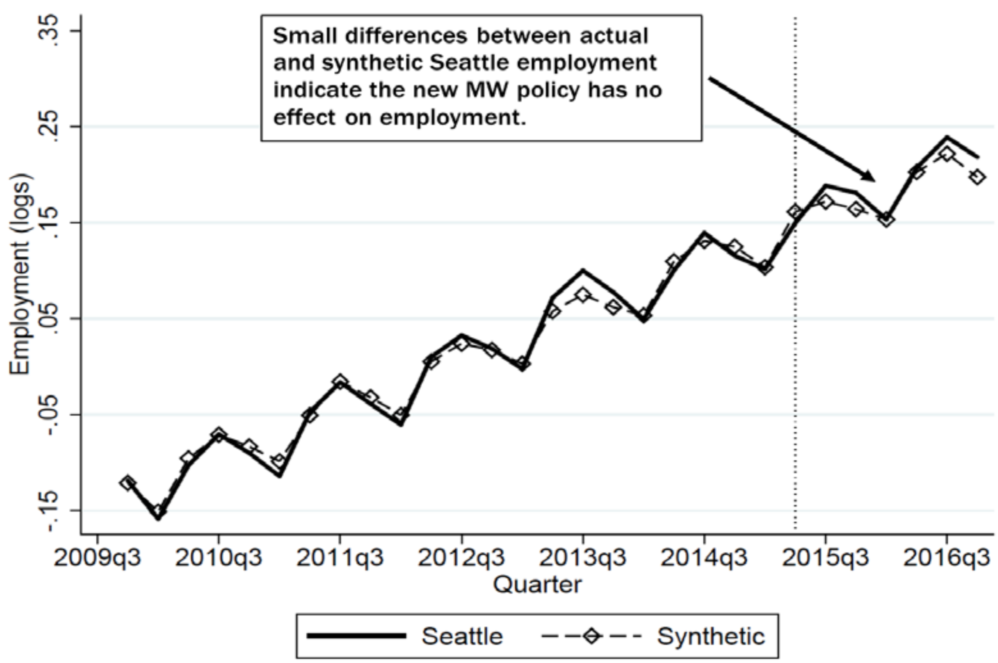
[37] See Appendix A.2 for a formal description of this statistic.

## Figure 6 Seattle synthetic control earnings analysis




Notes: This figure plots average earnings in food services in Seattle and its synthetic control. Average earnings is measured in logs and is centered around its pre-policy average.

## Figure 7 Seattle synthetic control employment analysis




Notes: This figure plots employment in food services in Seattle and its synthetic control. Employment is measured in logs and is centered around its pre-policy average.

Figure 7 displays the synthetic control analysis for Seattle's food service employment. Similar to Figure 6, actual and synthetic Seattle match one another closely during the pre-policy period. In contrast to earnings, actual and synthetic employment levels continue to match one another after the minimum wage is increased. The average difference between actual and synthetic Seattle during the evaluation period is only 0.9 percent, indicating the policy had little, if any, effect on employment.

We follow similar steps to measure the effects in each of the remaining five cities.

## 6.2 Synthetic control results

We begin by reporting results on food services earnings and employment for each city. To benchmark these individual city effects against those in previous minimum wage studies, we then estimate the earnings and employment elasticities for all six cities combined, using a procedure that we discuss in more detail below.

Table 5 displays our synthetic control results separately for each city. Average earnings results are reported in panel A of the table and employment results in panel B. The earnings effects range from about 2 percent for Chicago to about 10 percent for Oakland. The average earnings effect across the six cities is 5.8 percent—comparable to the effect we estimated using the event study model with control variables (4 percent). The row in Table 5 labeled "Pre-policy pseudo R-squared" reports Ferman and Pinto's (2017a) pseudo R-squared statistic. The pre-policy pseudo R-squared for the six cities ranges from 0.853 for Oakland to 0.999 for San Jose. These values indicate that the synthetic control algorithm was able to construct reasonably close matches for each of the cities.

The employment effects reported in panel B are uniformly smaller in magnitude than the earnings effects in each of the six cities, ranging from -1.2 percent in the District of Columbia to 7.0 percent in Oakland. The average effect on the cities' food service employment, 1.1 percent, is also similar to the estimate found by the event study model (2.1 percent). The pseudo R-squared statistics for employment in these cities indicate a close pre-policy period match in each of the cities, ranging from 0.886 for San Jose to 0.997 for Chicago.

The consistency of our synthetic control findings with those in the event study suggests that the results are not sensitive to the different assumptions that underlie these methods.

For each estimated effect, Table 5 also reports its statistical significance and the 90 percent confidence interval.[38] Although the earnings effects are all positive, the effects are statistically significant for only four cities: Oakland, San Francisco, San Jose and Seattle. None of the smaller employment effects we

[38] We test for statistical significance and measure confidence intervals using a placebo test-based approach described by Firpo and Possebom (2017). Recent econometric studies indicate that statistical tests based on placebo test-based approach may be biased (e.g., Ferman and Pinto 2017b). Unfortunately, the econometrics literature on synthetic control inference has not settled on a solution to this issue. As a result, we interpret the statistical tests we report as only suggestive. See Appendix A.2 for more information.

estimate are statistically significant, except for Oakland, where we observe a significant *positive* effect.[39]

## Table 5 Synthetic control results, by city

| | Chicago (1) | District of Columbia (2) | Oakland (3) | San Francisco (4) | San Jose (5) | Seattle (6) |
|---|---|---|---|---|---|---|
| **Panel A: Average earnings (logs)** | | | | | | |
| Effect of MW increase | 0.017 | 0.020 | 0.099** | 0.063** | 0.105** | 0.044** |
| P-value[a] | 0.237 | 0.270 | 0.020 | 0.033 | 0.020 | 0.033 |
| 90% CI | [-0.007,0.043] | [-0.021,0.060] | [0.058,0.139] | [0.041,0.088] | [0.059,0.150] | [0.022,0.068] |
| Elasticity with respect to the MW[b] | 0.087 | 0.090 | 0.278** | 0.559** | 0.449** | 0.184** |
| 90% CI | [-0.038,0.222] | [-0.094,0.274] | [0.164,0.392] | [0.363,0.778] | [0.253,0.645] | [0.092,0.283] |
| Tests of parallel trends assumption: | | | | | | |
| Effect during final pre-policy year[c] | -0.005 | 0.002 | 0.028** | 0.015* | -0.001 | 0.008 |
| P-value, effect during final pre-policy year[d] | 0.412 | 0.750 | 0.020 | 0.098 | 0.680 | 0.262 |
| Mean effect, comparison group[e] | -0.001 | 0.003 | 0.001 | -0.001 | 0.000 | 0.000 |
| Pre-policy pseudo R-squared | 0.972 | 0.925 | 0.853 | 0.951 | 0.999 | 0.983 |
| **Panel B: Employment (logs)** | | | | | | |
| Effect of MW increase | -0.010 | -0.012 | 0.070** | 0.009 | -0.002 | 0.009 |
| P-value[a] | 0.518 | 0.560 | 0.020 | 0.590 | 0.930 | 0.623 |
| 90% CI | [-0.042,0.022] | [-0.054,0.030] | [0.029,0.112] | [-0.049,0.070] | [-0.060,0.056] | [-0.049,0.069] |
| Elasticity with respect to the MW[b] | -0.050 | -0.056 | 0.198** | 0.085 | -0.010 | 0.038 |
| 90% CI | [-0.216,0.116] | [-0.247,0.135] | [0.081,0.316] | [-0.439,0.627] | [-0.258,0.240] | [-0.203,0.287] |
| Tests of parallel trends assumption: | | | | | | |
| Effect during final pre-policy year[c] | -0.002 | -0.002 | -0.011 | 0.022 | 0.020** | -0.003 |
| P-value, effect during final pre-policy year[d] | 0.702 | 0.650 | 0.140 | 0.131 | 0.030 | 0.738 |
| Mean effect, comparison group[e] | -0.002 | -0.001 | -0.002 | -0.001 | -0.003 | -0.001 |
| Pre-policy pseudo R-squared | 0.997 | 0.989 | 0.949 | 0.979 | 0.886 | 0.988 |
| Counties in comparison group | 113 | 99 | 99 | 60 | 99 | 60 |
| Pre-policy periods | 20 | 19 | 19 | 22 | 13 | 22 |

Notes: ** indicates significance at the 5 percent level. * indicates significance at the 10 percent level. Significance tests and confidence intervals are based on placebo tests. [a] The p-value from testing whether the effect is equal to zero. [b] The elasticity with respect to the minimum wage. To find the elasticity, we divide the estimated effect on the indicated outcome by the city's average minimum wage increase. The average minimum wage increase is the average log minimum wage during the evaluation period minus the log pre-policy minimum wage (see Table 2). For San Francisco and Seattle, which previously indexed their minimum wage to inflation, we additionally adjust for the expected increase in the indexed minimum wage due by subtracting the average minimum wage increase that we observe in their synthetic control. [c] The effect of the minimum wage if computed during the year before the increase occurs. We measure this effect using a synthetic control that we estimate using all pre-policy quarters except for the final year. [d] The p-value from testing whether the effect during the final pre-policy year is equal to zero. [e] The average effect of the minimum wage increase in the counties in the comparison group. The results indicate whether our estimated model finds effects where it should not.

[39] It is unlikely that the 7 percent increase in Oakland's employment is attributable to its new minimum wage policy. The employment effect we measure in Oakland is attributable to a positive spike in 2014q3 (see Appendix Figure 2), three quarters before the minimum wage increase to $12.25. It is unlikely that the local policy induced this change. It is also unlikely that this increase was induced by the increase in the California minimum wage from $8 to $9 in 2014q3, since the time series for food service earnings does not depict an increase in that quarter.

The effects we measure in the six cities imply a range of earnings and employment elasticities (reported on the fourth rows of panels A and B of Table 5). We compute each elasticity by dividing each city's effect by its average minimum wage increase, as reported in Table 2.[40] The earnings elasticities with respect to minimum wage range from 0.09 for Chicago to 0.56 for San Francisco. The employment elasticities range from -0.05 for Chicago to 0.20 for Oakland.

The range of estimated earnings and employment elasticities across the cities speaks, in part, to the limitations of any individual case study to uncover the true effect of a policy. However, we can obtain more reliable estimates by pooling the results from all six cities together and taking into account the variation in minimum wage increases induced by the cities' local policies. When we do so, in Figures 8 and 9, the pooled earnings and employment elasticities are similar to those found in our event study analysis and indicate little influence of the minimum wage on employment.

Figure 8 plots the earnings effects (from the first row of Table 5) against the average increase in each city's minimum wage. The graph reveals that the size of the earnings effect in each city (shown on the vertical axis) is commensurate with the size of that city's average minimum wage increase (shown on the horizontal axis).

To compute the earnings elasticity, we first obtain the line of best fit between the six cities' earnings effects and their average minimum wage increase. The dashed line in Figure 8 marks the predicted percent change in earnings for a given percent change in the minimum wage. The ratio of these two percent changes equals the earnings elasticity implied by the line. Since this ratio is also the line's slope, we can use the slope to measure the elasticity.[41] Using this approach, we find an earnings elasticity of 0.25.

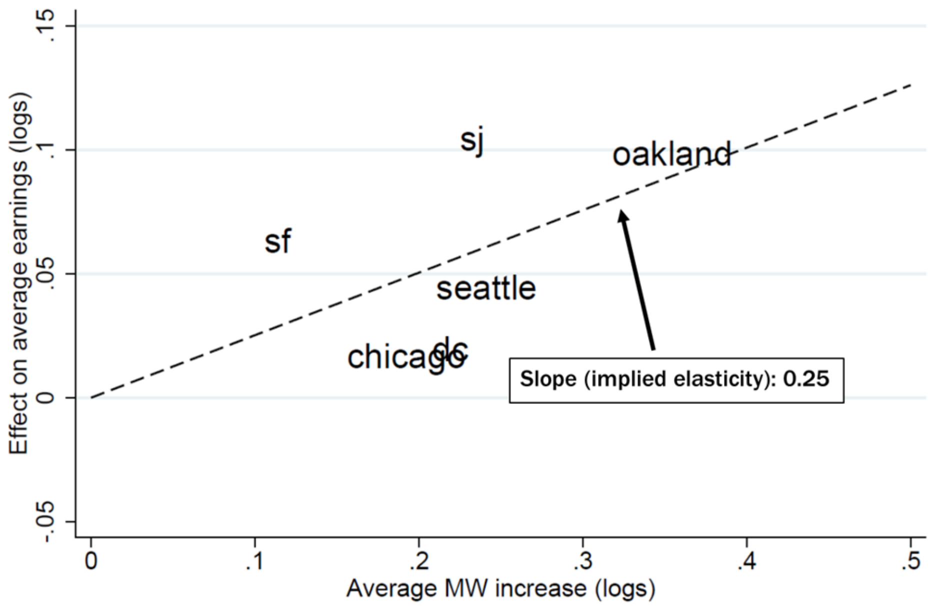
In other words, averaging across the six cities, every 10 percent increase in the minimum wage caused a 2.5 percent increase in food service worker earnings. This elasticity is similar to what we found in our event study analysis (0.21) as well as those found in previous restaurant studies.

Figure 9 displays our estimated synthetic control effects on food service employment. The dashed line, which plots the line of the best fit between the cities' employment effects and their average minimum wage increase, indicates an employment elasticity of 0.07.

This employment elasticity is more positive than elasticities reported in previous minimum wage studies. However, the effect is very small. We interpret Figure 9 as not showing a clear relationship between the size of the minimum wage increases and employment changes.

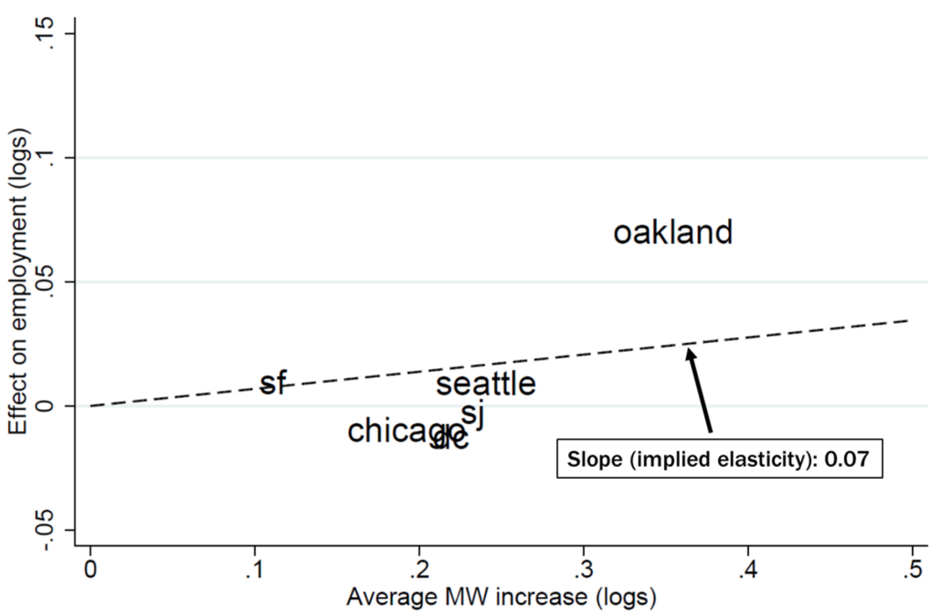
[40] San Francisco and Seattle previously indexed their minimum wage to inflation. We adjust their average minimum wage increase for the expected increase in the minimum wage due to indexing by subtracting the average minimum wage increase that we observe in their synthetic control. See Appendix A.2 for more information.
[41] See Appendix A.2 for a formal description of this approach for combining synthetic control effects to measure earnings and employment elasticities with respect to the minimum wage.

## Figure 8 Synthetic control earnings estimates




Notes: This figure plots each city's estimated earnings effect of its local minimum wage policy against the city's average minimum wage increase. Average earnings are measured in logs. The average minimum wage increase is the average log minimum wage during the evaluation period minus the log pre-policy minimum wage (see Table 2). For San Francisco and Seattle, which previously indexed their minimum wage to inflation, we additionally adjust for the expected increase in the minimum wage due to indexing by subtracting the average minimum wage increase that we observe in their synthetic control. The dashed line plots the fitted relationship between the estimated effect on earnings and the average minimum wage increase from a regression without a constant. The slope of the dashed line is a measure of the elasticity of average earnings in food services with respect to the minimum wage.

## Figure 9 Synthetic control employment estimates




Notes: This figure plots each city's estimated employment effect of its local minimum wage policy against the city's average minimum wage increase. Employment is measured in logs. The average minimum wage increase is the average log minimum wage during the evaluation period minus the log pre-policy minimum wage (see Table 2). For San Francisco and Seattle, which previously indexed their minimum wage to inflation, we additionally adjust for the expected increase in the minimum wage due to indexing by subtracting the average minimum wage increase that we observe in their synthetic control. The dashed line plots the fitted relationship between the estimated effect on employment and the average minimum wage increase from a regression without a constant. The slope of the dashed line is a measure of the elasticity of employment in food services with respect to the minimum wage.

## 6.3 Tests of parallel trends in synthetic control

In this section, we present two simple tests of whether the synthetic control method accurately constructs a weighted average of the comparison counties that would have trended in parallel with the cities but for the minimum wage policies. We then assess the sensitivity of our findings to violations of the parallel trends assumption by re-estimating the earnings and employment elasticities after we exclude the cities in which we find the assumption may not hold.

Our first test uses synthetic controls to measure the effect of the minimum wage policy for each county included in a city's comparison group. Since these untreated comparison counties experienced no change in their minimum wage policies during the evaluation period, we should not detect any effect of the minimum wage. To perform this test, we find synthetic controls for each county in each city's comparison group—as if the untreated county had adopted the city's minimum wage policy. That is, we run the synthetic control algorithm to look for changes in the county's average food service earnings and employment using the city's pre-policy and evaluation periods. We then measure the effect of the policy in the county as before, computing the average difference between the actual and synthetic counties' outcomes during the evaluation period.[42]

We report the results of this test in Table 5. The rows labeled "Mean effect, comparison group" show, for food service earnings and employment in each city, the estimated effects averaged over all the city's comparison counties. These effects average close to zero, ranging from -0.3 to 0.3 percent. They are also much smaller than any of the earnings effects that we measure in the cities. These results therefore suggest that, in the absence of the minimum wage policy, the synthetic cities would have otherwise trended with the actual cities.

Our second test is similar in spirit to our test of the parallel trends assumption in our event study analysis: For each city and the two outcomes of interest (earnings and employment), we test for any effects of the minimum wage policies during the final year of the pre-policy period. Since the new minimum wage policy had not yet gone into effect, we should not find any differences during this year between a city's actual earnings and employment and the earnings and employment in its synthetic control.

To perform this test, we re-run the synthetic control algorithm, but instead of setting the algorithm to find a synthetic control that matches all pre-policy quarters, we set the algorithm to find a synthetic control that matches all pre-policy quarters *except for the final year*.[43] By excluding the final pre-policy year, we leave open whether the (new) synthetic controls will trend with the city's actual outcomes during this year. If we then find that the new synthetic control matches the city's earnings and employment during this year, we can be more confident that it would have continued to do so during the evaluation period.

[42] To perform this test, we use the "placebo" synthetic controls that we estimate for each comparison county for testing statistical significance and constructing confidence intervals. See Appendix A.2 for more information.
[43] See Appendix A.2 for a formal description of this test.

The rows labeled, “Effect during the final pre-policy year” and “P-value, effect during final pre-policy year” in Table 5 reports the results from this test. These rows report our estimated earnings and employment effects during the final pre-policy year and their statistical significance for each city and outcome of interest. Out of the 12 tests performed, the synthetic control analysis passes in all but three cases. The test fails only for Oakland and San Francisco earnings, which rose significantly relative to their synthetic controls by 2.8 and 1.5 percent, respectively, before the increase, and for San Jose employment, which rose by 2 percent before the increase.

These pre-trends suggest that the effects measured in these three cases during the evaluation period may be positively biased. That is, our estimates may understate true earnings or employment losses. To assess the extent to which this bias may be influencing our results, we re-estimate the pooled earnings and employment elasticities depicted in Figures 8 and 9 after excluding the cities in which we find significant pre-trends (Oakland and San Francisco for earnings; San Jose for employment). Doing so yields very similar pooled elasticities to what we find above: An earnings elasticity of 0.22 and an employment elasticity of 0.08. We conclude that it is unlikely that these pre-trends are actually biasing our results.

In summary, we have extended the test of the parallel trends assumption that we used in our event study analysis to our synthetic control analysis. We find the synthetic control approach correctly does not detect effects of the policy in the untreated counties we include in our comparison groups on average, although it does detect positive effects before the policy went into effect in three out of the 12 cases tested. When we drop these three cases and re-estimate the pooled earnings and employment elasticities, our results do not change, suggesting that these pre-trends are not biasing our results.

# PART 7 ROBUSTNESS AND FALSIFICATION TESTS

We now turn to additional analyses that test the robustness of our main findings. First, we re-run our event study and synthetic control analyses of food services for two sub-sectors, full and limited service restaurants. We find larger earnings effects for workers in limited service than full service restaurants, consistent with the larger share of workers in limited service restaurants directly affected by minimum wage policies. We do not find significantly negative employment effects in either sector.

Second, we re-run our event study and synthetic control analysis for professional services, an industry that, at an aggregate-level, should not be influenced by minimum wage policies but would be influenced by more general changes to the local labor market. Reassuringly, we do not generally find significant earnings or employment effects in professional services: Out of the 16 tests we perform (4 event study models and 12 synthetic controls), we find significant effects in only one case.

Together, these analyses indicate that our methods for measuring the causal effects of the cities’ minimum wage policies are not confounded by other changes that occurred in the cities around the time the higher minimum wages were implemented.

## 7.1 Full service and limited service restaurants

We re-run our event study and synthetic control analyses for the full service and limited service restaurant sub-sectors. This exercise tests whether the effects of the minimum wage are stronger in the sector with a larger share of workers who are affected by the policies. Average earnings in limited service restaurants are lower than in full service restaurants.[44] We should therefore find larger effects in limited than in full service restaurants.

Table 6 reports the results of our event study analysis on full and limited service restaurants. Figure 10 plots the earnings and employment effects we measure using synthetic control in each sub-sector.[45] In both event study and synthetic control, the earnings elasticities are larger in the limited service than those in the full-service sector—as we would expect. In the event study model that includes our population and private sector size control variables (columns 1 and 5), the earnings elasticity is 0.37 for limited service and 0.18 for full service restaurants.

The earnings and employment elasticities with respect to the minimum wage in full service restaurants are similar to those for food services overall, reflecting the large share of food service employment in full service restaurants. The results from synthetic control (Figure 10) are consistent with these findings: 0.46 for limited service and 0.19 for full service restaurants.[46]

Despite the larger earnings effects of the minimum wage in limited service restaurants, both event study and synthetic control methods find that the policies have little effect on limited service employment: With our population and private sector control variables, the event study-based employment elasticity equals 0.05 and the synthetic control-based employment elasticity is 0.02. None of our event study models or city-level synthetic control analyses find negative effects on limited service employment that are statistically significant.[47]

In contrast to our results for food services overall, the results from our tests of the parallel trends assumptions in these two subsectors are sensitive to how we cluster the data. In our event study models for full service restaurant employment and limited service restaurant earnings (columns 3 and 5 of Table 6), the parallel trends tests pass only when we cluster at the state level.

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[44] Limited service restaurants are also more likely to be influenced by minimum wage policies because the minimum wages for tipped workers is set lower than the minimum wage in some cities in our sample. In these cities, restaurants can choose to pay their tipped workers the lower tipped wage as long as the workers' hourly earnings are higher than the minimum wage once tips are included. During our period of study, Chicago and the District of Columbia, have tipped wages that are lower than the local minimum wages for all employers, and Seattle introduced lower tipped wages for small employers.

[45] We report the synthetic control estimates for full service restaurants and limited service restaurants in Appendix Tables 4 and 5, respectively.

[46] Synthetic control finds that effects on earnings are larger in limited service than full service restaurants in each of the six cities. See Appendix Tables 4 and 5.

[47] We report p-values for the city-level synthetic control results in Appendix Table 5.

## Table 6 Event study results, full and limited service restaurants

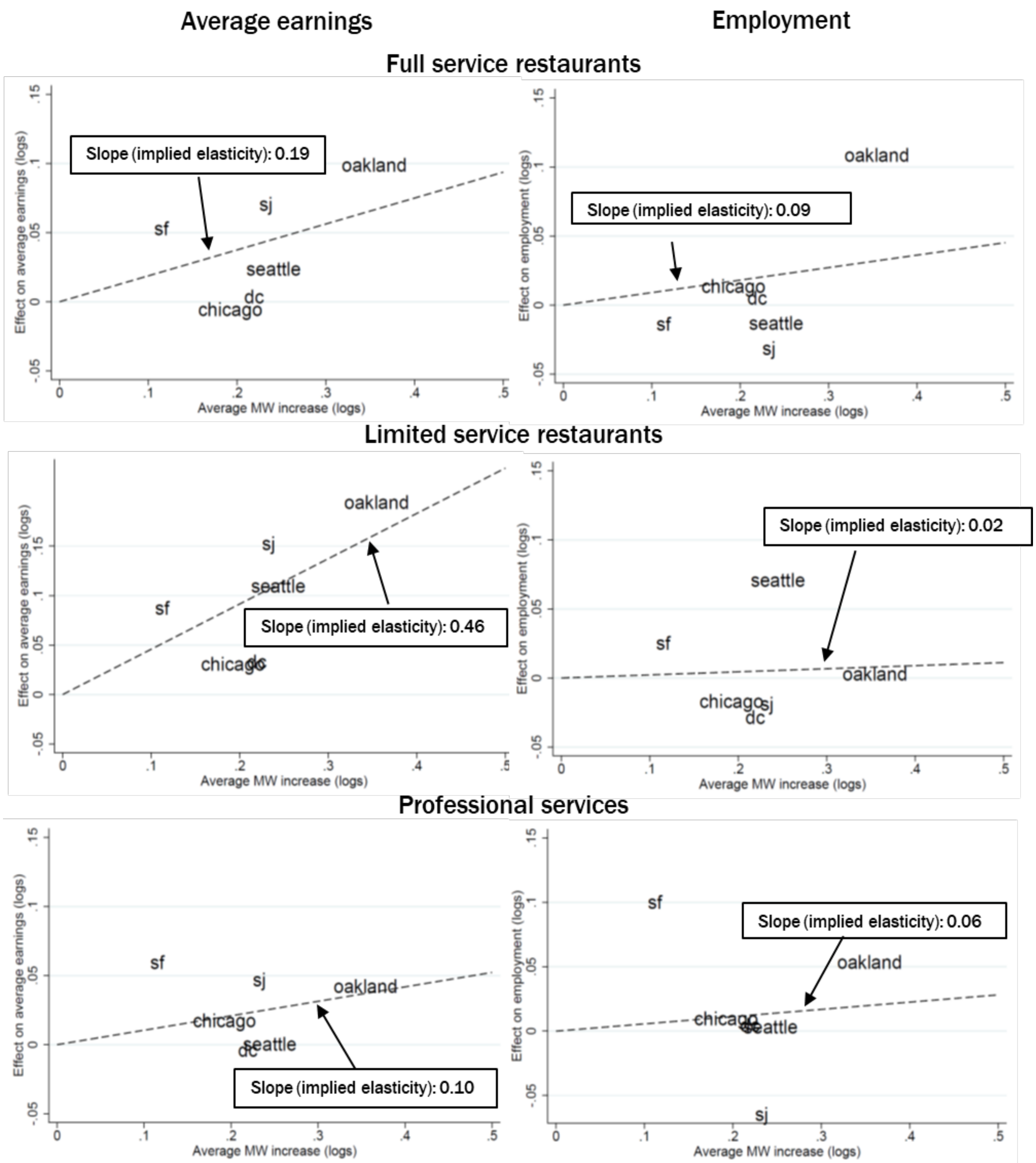
| | Full service restaurants | | | | Limited service restaurants | | | |
|---|---|---|---|---|---|---|---|---|
| | Average earnings (logs) | | Employment (logs) | | Average earnings (logs) | | Employment (logs) | |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Effect of MW increase | 0.035** | 0.023*† | 0.011 | -0.018 | 0.071**†† | 0.042**†† | 0.010 | 0.025* |
| P-value (179 city and county clusters) | 0.028 | 0.050 | 0.266 | 0.148 | 0.018 | 0.012 | 0.618 | 0.079 |
| 90% CI (179 city and county clusters) | [0.011,0.065] | [0.010,0.033] | [-0.006,0.040] | [-0.047,0.003] | [0.027,0.110] | [0.028,0.054] | [-0.022,0.057] | [0.003,0.041] |
| P-value (28 state clusters) | 0.114 | 0.093 | 0.407 | 0.429 | 0.035 | 0.030 | 0.653 | 0.125 |
| 90% CI (28 state clusters) | [-0.020,0.097] | [0.006,0.035] | [-0.021,0.063] | [-0.044,0.010] | [0.016,0.189] | [0.029,0.059] | [-0.017,0.061] | [-0.006,0.047] |
| Elasticity with respect to the MW | 0.184 | 0.139* | 0.060 | -0.108 | 0.373**†† | 0.252**†† | 0.054 | 0.153*† |
| P-value (179 city and county clusters) | 0.116 | 0.072 | 0.283 | 0.158 | 0.030 | 0.017 | 0.604 | 0.081 |
| 90% CI (179 city and county clusters) | [-0.022,0.449] | [0.041,0.225] | [-0.032,0.173] | [-0.256,0.020] | [0.100,0.671] | [0.133,0.367] | [-0.115,0.272] | [0.019,0.252] |
| P-value (28 state clusters) | 0.158 | 0.117 | 0.419 | 0.525 | 0.041 | 0.023 | 0.634 | 0.098 |
| 90% CI (28 state clusters) | [-0.185,0.304] | [-0.024,0.202] | [-0.111,0.237] | [-0.265,0.103] | [0.068,0.561] | [0.161,0.311] | [-0.084,0.319] | [0.002,0.254] |
| Test of parallel trends assumption | | | | | | | | |
| P-value, (179 city and county clusters) | 0.240 | — | 0.096 | — | 0.042 | — | 0.269 | — |
| P-value, (28 state clusters) | 0.353 | — | 0.172 | — | 0.138 | — | 0.214 | — |
| Controls for population, private sector | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Control for trend | No | Yes | No | Yes | No | Yes | No | Yes |
| Observations[a] | 5134 | 5134 | 5134 | 5134 | 5134 | 5134 | 5134 | 5134 |

Notes: Significance tests and confidence intervals are based on a wild bootstrap using the empirical t-distribution, clustered at either the (1) city and county or (2) state level. ** indicates significance at the 5 percent level when we cluster at the city and county level, * indicates significance at the 10 percent level. †† indicates significance at the 5 percent level when we cluster at the state level, † indicates significance at the 10 percent level. [a] The difference in observation sizes between these models and those reported in Table 4 for food services is due to two counties in the comparison group for Chicago that are missing observations in 2009 or 2016q4. Unlike the counties we include in the comparison group for the other cities, the counties in the Chicago comparison group are required to be balanced over the quarters 2010q3–2016q2 only. The counties in the other comparison groups are balanced over the quarters 2009q4–2016q4. See Table 4 for additional notes.

Nevertheless, it is unlikely the pre-trends detected in these models are driving our results: Adding a control for a linear trend (columns 4 and 6) yields lower but qualitatively similar elasticities. Column 4 reports an insignificant employment elasticity in full service restaurants (-0.11 compared to 0.06 without the trend). Column 6 reports a positive earnings elasticity in limited service restaurants (0.25 compared to 0.37 without the trend) and is statistically significant regardless of how we cluster.[48]

[48] We have also re-run the parallel trends tests that we described in Part 6.3 for the synthetic control analysis of full service and limited service restaurants. These results are reported in Appendix Tables 4 and 5. The results for full service restaurants are similar to what we found for food services overall, failing in 3 out of the 12 cases tested. Dropping the city-outcome pairs in which we find significant pre-trends yield similar elasticities to what we find when we include all cities (0.11 for earnings, 0.13 for employment). The synthetic control analysis for limited service restaurants passes all 12 of the parallel trends tests performed.

## Figure 10 Synthetic control estimates, full and limited service restaurants, and professional services




Notes: These figures plot each city's estimated effects of its local minimum wage policy against the city's average minimum wage increase. Average earnings and employment are measured in logs. The average minimum wage increase is the average log minimum wage during the evaluation period minus the log pre-policy minimum wage (see Table 2). For San Francisco and Seattle, which previously indexed their minimum wage to inflation, we additionally adjust for the expected increase in the minimum wage due to indexing by subtracting the average minimum wage increase that we observe in their synthetic control. The dashed line plots the fitted relationship between the estimated effect on earnings and the average minimum wage increase from a regression without a constant. The slope of the dashed line is a measure of the elasticity of either earnings or employment in the respective industry with respect to the minimum wage.

In summary, full service and limited service results are consistent with our findings for food services overall. The cities' minimum wage policies had greater earnings effects in limited service than full service restaurants. We do not detect significantly negative employment effects in either sector.

## 7.2 Professional services

In this section, we check whether our results might be biased by other contemporaneous changes in the six cities' local labor markets. To do so, we run our event study and synthetic control analyses on professional services (Table 7)—a high-wage industry that should not be affected by changes in minimum wage policy. For example, if our estimated positive earnings effects in low-wage food services are driven by an expanding tech sector, then we should find positive earnings effects in high-wage industries like professional services as well. The expansion of the high-paying tech sector would put upward pressure on average earnings in professional services by increasing the overall demand for highly educated workers. On the other hand, if our methods are effectively accounting for such contemporaneous changes, we should not find any significant earnings or employment effects in professional services.

**Table 7 Event study results, professional services**

| | Professional services | | | |
|---|---|---|---|---|
| | Average earnings (logs) | | Employment (logs) | |
| | (1) | (2) | (3) | (4) |
| Effect of MW increase | 0.014 | 0.020 | 0.021 | -0.006 |
| P-value (179 city and county clusters) | 0.716 | 0.460 | 0.298 | 0.375 |
| 90% CI (179 city and county clusters) | [-0.079,0.109] | [-0.031,0.064] | [-0.013,0.055] | [-0.018,0.007] |
| P-value (28 state clusters) | 0.516 | 0.657 | 0.705 | 0.389 |
| 90% CI (28 state clusters) | [-0.025,0.040] | [-0.053,0.054] | [-0.114,0.061] | [-0.029,0.011] |
| Elasticity with respect to the MW | 0.072 | 0.119 | 0.111 | -0.038 |
| P-value (179 city and county clusters) | 0.705 | 0.464 | 0.333 | 0.370 |
| 90% CI (179 city and county clusters) | [-0.467,0.515] | [-0.159,0.330] | [-0.085,0.341] | [-0.113,0.038] |
| P-value (28 state clusters) | 0.548 | 0.675 | 0.749 | 0.381 |
| 90% CI (28 state clusters) | [-0.383,0.180] | [-0.907,0.345] | [-0.790,0.345] | [-0.148,0.055] |
| Test of parallel trends assumption | | | | |
| P-value (179 city and county clusters) | 0.185 | — | 0.384 | — |
| P-value (28 state clusters) | 0.223 | — | 0.376 | — |
| Controls for population, private sector | Yes | Yes | Yes | Yes |
| Control for trend | No | Yes | No | Yes |
| Observations[a] | 5133 | 5133 | 5133 | 5133 |

Notes: Significance tests and confidence intervals are based on a wild bootstrap using the empirical t-distribution, clustered at either the (1) city and county or (2) state level. ** indicates significance at the 5 percent level when we cluster at the city and county level, * indicates significance at the 10 percent level. †† indicates significance at the 5 percent level when we cluster at the state level, † indicates significance at the 10 percent level. [a] The difference in observation sizes between these models and those reported in Table 4 for food services is due to two counties in the comparison group for Chicago that are missing observations in 2009 or 2016q4. Unlike the counties we include in the comparison group for the other cities, the counties in the Chicago comparison group are required to be balanced over the quarters 2010q3–2016q2 only. The counties in the other comparison groups are balanced over the quarters 2009q4–2016q4. See Table 4 for additional notes.

Table 7 reports the results for professional services from our event study models. None of our event study models find significant earnings or employment effects of the minimum wage policies. For example, the event study model that controls for population and private sector size (column 1) finds an earnings elasticity with respect to the minimum wage of 0.072—about a third of the size of the earnings elasticity in food services, and it is not statistically significant.

The bottom row of Figure 10 plots the earnings and employment effects we measure using synthetic control. Overall, the effects are consistent with what we find using our event study model. Of the 12 tests (two for each city), we estimate a significant effect in only one—professional employment in San Francisco.[49] This result, which is large and positive and significant at the 10 percent level, is not inconsistent with our other findings for professional services. By construction of the statistical tests we employ, we would expect to find significant results about 10 percent of the time, even if there was no actual correlation between the policy and the outcomes we are testing.

Taken together with our event study analysis, these results indicate that it is unlikely that our estimated food services effects result from other post-increase changes in the six cities' local labor markets.

The results from our robustness tests indicate that our estimated effects on food services are indeed attributable to the cities' local minimum wage policies. Consistent with a minimum wage effect, both event study and synthetic control-based methods find larger effects in the lower paying limited service restaurants and—with only one exception—detect no significant effects in the high-paying professional services industry. Together, these results indicate our food services estimates are unlikely to be driven by contemporaneous changes in the cities that are not minimum wage policy-related.

# PART 8 DISCUSSION

Numerous cities across the U.S. are in the process of raising local minimum wages, some to as high as $15 per hour. These policies have already attained minimum wage levels that are well above previous peaks in the U.S. We study the effects of the new polices on earnings and employment by examining the effects of policy changes in six large cities—Chicago, the District of Columbia, Oakland, San Francisco, San Jose and Seattle—that resulted in a total of thirteen minimum wage increases during the period we study. These cities comprise the earliest movers in the new wave of higher local minimum wage policies.

Minimum wages in our six cities ranged from just above $10 to $13 at the end of 2016, the last period for which our data are available. As in earlier case studies of individual cities and national minimum wage studies, we focus on the food services industry, the largest and most intense user of low-wage

[49] See column 4 of Appendix Table 6.

labor. We isolate the causal effects of the minimum wage changes using both event study and synthetic control methods.

Compared to earlier local case studies, we draw from a wider variety of untreated comparison counties to conduct our analysis. We examine the results for each city as well as pooled estimates that draw from all six cities together. We use the Quarterly Census of Employment and Wages because it has sufficiently finely-grained quarterly data at the local level, unlike other datasets, such as the Current Population Survey or the American Community Survey.

We find that minimum wages in the $10 to $13 range have statistically significant positive effects on earnings. At the individual city level, our estimated wage increases are proportional to the size of the minimum wage increases. On average across the six cities, we find that a 10 percent increase in the minimum wage increases earnings in the food services industry between 1.3 and 2.5 percent. This result is very similar to the estimates in previous studies of minimum wage levels up to $10.

In addition to our findings of positive effects on earnings, we do not detect negative significant employment effects in any of the individual cities, or when pooling them together. Our results from the event study and synthetic control approaches are remarkably similar. They are also consistent with the consensus of estimates in previous studies of restaurant workers and with studies of minimum wage policies with similar minimum wage to median wage ratios. However, our pooled employment confidence intervals are somewhat broader than in previous studies. This imprecision may result from the limited number of events in our study compared to studies of state and federal increases.

Our robustness exercises include tests for parallel pre-trends in both our event studies and synthetic control analyses, and for differences in the magnitude of the minimum wage's effect between full and limited service restaurants. Combining these results and those for professional services, our tests rule out non-parallel pre-trends in 49 of 56 cases. We find larger earnings effects in limited service restaurants than in full-service ones—consistent with lower wages in limited service—but no detectable employment effects in either industry.

We also conduct falsification tests using the high-paid professional services industry and placebo tests on untreated areas. The falsification test using professional services passes in all four of our event study models and in 11 of the 12 cases from our synthetic control analysis of the six cities separately. We also do not find earnings or employment effects in the untreated comparison counties. The results of these exercises indicate that our findings are not likely to result from changes unrelated to the minimum wage that may have occurred around the time of the policy's implementation.

Our study has some limitations. Our study does not examine effects in other low-wage industries, which could differ from those in the food service industry. Another limitation arises from the nature of our data. The QCEW reports industry average weekly wages, which include both high-wage and low-wage workers within an industry. Estimated effects on average earnings thus reflect the combination of changes in hourly wages, as well as potential changes in hours and composition effects. The employment measure aggregates potentially differing effects on low wage and higher wage workers.

This effect can be important insofar as employers respond to minimum wage increases by substituting higher-skilled workers for lower-skilled ones. Including higher-wage workers who are not affected could attenuate both our estimated wage effects and our estimated employment effects. As noted earlier, Cengiz et al. (2018) do not detect effects on hours or on the substitution of more educated workers for lower educated ones. Their findings support our conclusion that these six citywide policies did not result in significant reductions in employment.

Our findings suggest that the low-wage community as a whole clearly benefited from minimum wage policies in the $10 to $13 range, particularly if labor-labor substitution effects are minimal. Even if there are negative effects on employment, the low-wage community still can gain more from pay increases than it might lose from any employment losses. Using the findings in Table 5, the ratio of the employment effects to the earnings effects in our cities ranges between -0.62 (District of Columbia) to +0.71 (Oakland). The simple average of these ratios is -0.03. These results are consistent with the estimated 0.14 ratio for all industries, and -0.01 for restaurants only, in Cengiz et al. (2018) and the -0.1 ratio for low-skilled workers found in a recent meta-analysis of 105 labor market studies (Lichter et al. 2015). Thus, our employment estimates are consistent with the conclusion that the low wage community gained on net from these policies.

In closing, our estimated wage and employment effects of minimum wages up to $13 are consistent with the emerging consensus of estimates of minimum wages up to $10. This result is not entirely surprising. While we have emphasized that recent local policy increases up to $13 range well above previous *absolute* minimum wage levels, the policies remain within the range of previous *relative* minimum wages—that is, within the previous range of ratios of state minimum wages to state median hourly wages.

The cities in our sample continue to increase their minimum wages beyond the end of 2016. Other large cities are also in the process of implementing similar policies. We look forward to studying the effects of these policies as they are implemented and suitable data become available.

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# APPENDICES

## Appendix A: Formal methods

### A.1 Event study

In our event study analysis, we measure the effect of local minimum wage policies and test for pre-trends using four sets of regression models. The first set, depicted in Figures 3, 4 and 5, measures the effect of increasing the minimum wage during each quarter around the first increase in each city. Because these models do not make any assumptions on how the effects vary over time, we can inspect the estimates for pre-trends by measuring any influence of the policies before they actually went into effect. The second set of models measures the effect of increasing the minimum wage overall. To do this, we calculate the average effect of the minimum wage policy during the first seven quarters of each city's evaluation period. The third set measures the effect of increasing the minimum wage after adjusting for a linear trend that runs through the pre-policy and evaluation periods. Finally, the fourth set of models tests for pre-trends directly and measures the slope of a linear trend during the pre-policy period. Within each model set, we fit separate specifications with and without the population and private sector control variables we described in Part 5.1. Below we formally present each of the regression models we estimate. We also describe how we infer statistical significance and construct confidence intervals.

#### *Regression models*

Let $i$ index each of the localities in our analysis: the six treated cities and their untreated comparison counties. Let $t$ index quarters 2009q4 through 2016q4.

Each county is a member of at least one of the six cities' comparison groups. As described in Part 4.2, there are three comparison groups over all: For the District of Columbia, Oakland and San Jose, we include in the comparison group counties that had no minimum wage increase between 2009q4 and 2016q4. For San Francisco and Seattle, which previously indexed their minimum wage to inflation, we include counties in states that also index their minimum wage to inflation and that have no other minimum wage increases between 2009q4 and 2016q4. For Chicago (whose state-level minimum wage increased to \$8.25 in 2010q3), we include counties that had no minimum wage increase between 2010q4 and 2016q4. As reported in Table 2, there are 99 comparison counties for Oakland, San Jose, and the District of Columbia; 60 counties for San Francisco and Seattle; and 113 counties for Chicago.

Let $k \in \{1,2,3\}$ index the three comparison groups. The dummy variable $D_{ik}$ denotes whether locality $i$ is a member of comparison group $k$. Since we use these dummy variables in the regression model to control for comparison group-specific time effects, we set $D_{ik}$ so that each city is a member of its own comparison group. For example, for Oakland, San Jose, and the District of Columbia, $D_{i1} = 1$. The untreated comparison counties can be members of more than one comparison group.

In addition, we introduce notation to mark the timing of the minimum wage policy for each city, $t_i^0$. Quarter $t_i^0$ is when the city's minimum wage started to increase. For each city, let $e(i,t)$ (for "event time") index the quarters around this point for each city: $e(i,t) = t - t_i^0$. In other words, $e(i,t)$ counts the quarters -13 through 6 that we display on the horizontal axes of Figures 3, 4 and 5.

In Figures 3, 4 and 5 we present results from models that take the form:

$$Y_{it} = \sum_{s=-13}^{-2} 1(e(i,t) = s)\beta_s + \sum_{s=0}^{6} 1(e(i,t) = s)\beta_s + \mu_i + u_{it} \quad (1)$$

where $Y_{it}$ is our outcome of interest (e.g., log food service average earnings or employment) for locality $i$ in quarter $t$, and $\mu_i$ is a locality effect. The locality effect $\mu_i$ controls for factors that influence the average level of the outcome in the locality during the period of study. For the untreated comparison counties, the dummy variables $1(e(i,t) = s)$ always equal zero.

Since the event time variable, $e(i,t)$, equals zero for each of the comparison counties, the event time coefficients $\beta_s$ in Equation (1) measure the average level of the outcome across the six cities during quarter $s$ that cannot be explained by the other variables in the model. We normalize $\beta_s$ by excluding from the model the indicator for the final quarter of the pre-policy period ($s = -1$). As a result, each $\beta_s$ measures the difference between the outcome's six city average during quarter $s$ and the average at the end of the pre-policy period.

We fit two separate specifications of Equation (1), labeled in Figures 3, 4 and 5 as "No Controls" and "Controls." Each specification makes a different assumption regarding the year-specific locality component, $u_{it}$, in Equation (1):

$$u_{it} = \sum_{k=1}^{3} D_{ik}\delta_{kt} + \omega_{it} \qquad \text{(No Controls)}$$
$$u_{it} = \sum_{k=1}^{3} D_{ik}\delta_{kt} + X_{it}'\gamma_t + \nu_{it} \qquad \text{(Controls)}$$

The coefficient $\delta_{kt}$ is a comparison group-specific quarter effect that captures factors that influence the level of all cities and counties in comparison group $k$ in quarter $t$. The variable $X_{it}$ is a vector that contains the population and private sector control variables for locality $i$ in quarter $t$: annual population and the total earnings of all private sector workers during years 2007, 2008, and the first three quarters of 2009. We allow the influence of these control variables to vary over time by interacting them with quarter effects (indicated by the $t$ subscript on the coefficient, $\gamma_t$). The variables $\omega_{it}$, and $\nu_{it}$, are ordinary least squares error terms that are, by construction, uncorrelated with the other explanatory variables in the model.

Each event time coefficient $\beta_s$ measures the difference between the average across the six cities in quarter $s$ and the average at the end of the pre-policy period. In the No Controls and Controls models, $\beta_s$ measures only the difference that cannot be explained by the other variables in the model.

To measure the average effect of the minimum wage policies in the six cities, we modify Equation (1) so that the effects of the policy are constant between quarters 0 and 6.

$$Y_{it} = \sum_{s=-13}^{-2} 1(e(i,t) = s)\beta_s + 1(0 \leq e(i,t) \leq 6)\theta + \mu_i + u_{it}' \tag{2}$$

The coefficient $\theta$ in Equation (2) measures the average change in the outcome, $Y_{it}$, across the six cities during the first seven quarters of the evaluation period that cannot be explained by the other variables in the model. This change coincides with the average causal effect of the local minimum wage policies on the cities if the quarter when the policies go into effect is uncorrelated with locality-specific factors that are not accounted for by our control variables, locality effects, or comparison group-specific quarter effects. We report the estimates of $\theta$ in Tables 4 and 6 in the rows labeled "Effect of MW increase."

To additionally adjust the effect of the policies that we measure for a linear trend that runs through the pre-policy and evaluation periods, we modify Equation (2) to control directly for the trend:

$$Y_{it} = (e(i,t) + 1)\varphi + 1(0 \leq e(i,t) \leq 6)\theta + \mu_i + u_{it}'' \tag{3}$$

The coefficient $\varphi$ in Equation (3) measures the slope of the linear trend between event time quarters -13 and 6. When comparison-group specific quarter effects, $\delta_{kt}$, are included in the model, this trend measures the extent to which the outcome increased at a faster (or slower) rate in the six cities relative to their comparison counties during the pre-policy period and evaluation periods. The coefficient $\theta$ then measures only the average change in the outcome, $Y_{it}$, that cannot be explained by this trend (or the other variables in the model). In Tables 4, 6 and 7, we indicate whether the estimates of $\theta$ control for a linear trend in the row labeled "Control for trend."

To test the parallel trends assumption, we modify Equation (1) so that the event time coefficients before quarter 0 form a linear trend that crosses the horizontal axis at quarter -1:

$$Y_{it} = [1(e(i,t) < -1) \times (e(i,t) + 1)]\rho + \sum_{s=0}^{6} 1(e(i,t) = s)\beta_s + \mu_i + u_{it}''' \tag{4}$$

The coefficient $\rho$ in Equation (4) measures the slope of the linear trend between event time quarters -13 and -1. In contrast to the coefficient $\varphi$ in Equation (3), $\rho$ measures the rate the outcome increased in the six cities relative to their comparison counties during the pre-policy period only. If our statistical test finds that $\rho$ is not zero, it indicates that the comparison counties do not trend in parallel with the six cities. We report the p-value of this test in Tables 4, 6 and 7 in the rows labeled "P-value, slope of pre-policy trend equals zero."

We fit the event study models specified in Equations (1) – (4) by ordinary least squares over quarters 2009q4 through 2016q4. Each locality in the sample is either one of the six treated cities or is an untreated county in one of the city's comparison groups. The six treated cities appear in the sample only during event time quarters -13 through 6. We drop comparison counties that are missing employment or average earnings information during the quarters spanned by the pre-policy and evaluation periods in one of the following industries and sub-sectors: food services, full service restaurants, limited service restaurants, retail, or professional services.

### *Inference*

In our event study analysis, we cluster our standard errors at the either the (1) city and county or (2) state level. These standard errors control for correlations in the error terms in Equations (1) – (4) within clusters, under the assumption that the number of clusters is sufficiently large. In our application, however, this assumption may not hold. Depending on how we cluster, we have either six treated city clusters or four treated state clusters. As a result of the small number of treated clusters, the clustered standard errors we estimate will likely overstate the statistical significance of the minimum wage effects and pre-trends (Cameron and Miller 2015).

To perform hypothesis tests and construct confidence intervals, we correct for the small number of clusters by following a recommendation of Cameron and Miller (2015). In particular, we report p-values from a wild bootstrap using the empirical t-distribution, clustered at either the city and county or state level (Cameron, Gelbach and Miller 2008).[50] The 90 percent confidence interval we report contains the set of values that are not rejected at the 10 percent level—that is, those values for which hypothesis tests yield p-values greater than or equal to 0.1.

### *Computing earnings and employment elasticities*

The row labeled "Elasticity with respect to the MW" in Tables 4 and 7 reports the earnings and employment elasticities implied by our event study-based estimates. To compute these elasticities, we estimate a two-stage least squares model in which we use the evaluation period indicator, $1(0 \leq e(i,t) \leq 6)$, as an instrument for the log minimum wage. The first stage is the model specified in Equation (2) in which we replace the dependent variable with the log minimum wage in the locality, $logMW$:

$$logMW_{it} = \sum_{s=-13}^{-2} 1(e(i,t) = s)\beta_s + 1(0 \leq e(i,t) \leq 6)\theta + \mu_i + \eta_{it} \qquad (5a)$$

where $\eta_{it}$ is an error term. The coefficient $\theta$ in Equation (5a) is an event study-based measure of the average increase in the minimum wage across the six cities. The second stage model is:

[50] We perform the wild bootstrap using the user-written package BOOTTEST in Stata (Roodman 2015).

$$Y_{it} = \sum_{s=-13}^{-2} 1(e(i,t) = s)\beta_s + \epsilon \widehat{logMW}_{it} + \mu_i + u_{it}'''' \qquad (5b)$$

where $\widehat{logMW}$ is the log minimum wage predicted from the first stage model. The coefficient $\epsilon$ is the elasticity of the outcome $Y$ (either earnings or employment) with respect to the minimum wage.

We then compute p-values and confidence intervals by applying the wild bootstrap procedure to the estimate of $\epsilon$.

By construction, the coefficient $\epsilon$ in Equation (5b) is equal to the elasticity one would find by dividing the estimates of the effect on average earnings and employment (reported in the row labeled "Effect of MW increase") by the estimate of $\theta$ in Equation (5a) (the average increase in the minimum wage). In the models without controls, we measure the average minimum wage increase to be 21.0 percent. In models with controls, we measure the average minimum wage increase to be 19.1 percent. In the models with controls that also allow for a linear trend, we measure the average minimum wage increase to be 16.6 percent.

## A.2 Synthetic control

We use the synthetic control method to measure the effect of the local minimum wage policies in each of the six cities separately. We then pool the cities' estimates for each outcome to find the implied elasticity with respect to the minimum wage. In this section, we explain formally how we employ the synthetic control method to perform this analysis. We describe the placebo tests we perform to infer statistical significance and construct confidence intervals, and as well as how we measure the quality of the synthetic control match. We also provide more information on how we test the parallel trends assumption, as reported in Part 6.3.

### *Constructing the synthetic control*

The synthetic control for each city comprises a weighted average of counties in the city's comparison group. We construct synthetic controls for each outcome of interest (e.g., log food service average earnings, employment).

Let $w_{ijr}^*$ denote the weight city $i$'s synthetic control places on county $j$ for outcome $r$. Let $J(i)$ denote the set of untreated comparison counties for city $i$. Let $\underline{T}^{pre}(i)$ and $\overline{T}^{pre}(i)$ denote first and last quarters of city $i$'s pre-policy period, respectively, as reported in Table 2. (For example, for Seattle, $\underline{T}^{pre}(i)$ is 2009q4 and $\overline{T}^{pre}(i)$ is 2015q1.)

For each city $i$ and outcome $r$, the synthetic control estimator finds the weights $(w_{i1r}^*, \ldots, w_{iJ(i)r}^*)$ that minimize the pre-policy period mean squared prediction error (MSPE) of the outcome between the actual and synthetic city:

$$\left(w^*_{i1r}, \dots, w^*_{iJ(i)r}\right) \in \operatorname{argmin}_{\overrightarrow{w_i} \in \mathcal{W}} \sum_{\underline{T}^{pre}(i) \le t \le \overline{T}^{pre}(i)} \left( Y_{irt} - \sum_{j \in J(i)} w_{ijr} Y_{jrt} \right)^2 \tag{6}$$

where $\vec{w}_i$ is a vector of county weights for city $i$, and $\mathcal{W}$ is the set of non-negative weights that sum to one.

The synthetic control, $\hat{Y}_{irt}$, is then the weighted average of counties in the city's comparison group using the weights we find when we solve Equation (6):[51]

$$\hat{Y}_{irt} = \sum_{j \in J(i)} w^*_{ijr} Y_{jrt} \tag{7}$$

To measure the effect of the policy in each city, we average the difference between the outcome's actual and synthetic values over the evaluation period. We report these estimates in the rows labeled "Effect of MW increase" in Table 5.

To improve the match between the actual outcomes and the synthetic controls, we normalize each city's time series by subtracting from each quarter the city's average value during the pre-policy period. We perform the same normalization on the outcomes for each of the comparison counties as well, subtracting from each quarter the county's average value during the pre-policy period. As a result, we find synthetic controls that match the cities' *trends*, not their level. We perform this normalization for two reasons. First, if we did not, it would be very difficult for the synthetic control algorithm to construct a weighted average of the comparison counties that matches some cities' outcomes during the pre-policy period: As shown in Table 3, outcomes like earnings and employment are generally much higher in the six cities than in other parts of the country because of underlying differences in living costs and other economic conditions. Second, a recent study (Ferman and Pinto 2017a) on the statistical properties of the synthetic control method finds that this transformation improves the method's accuracy even in cases where it would not be necessary to construct a close match.

## *Measuring the quality of the match*

To measure the quality of the pre-policy match between the actual and synthetic city, we report Ferman and Pinto's (2017a) pseudo R-squared statistic. For each city $i$ and outcome $r$, the pseudo R-squared is:

$$\tilde{R}_{ir} = 1 - \frac{\sum_{\underline{T}^{pre}(i) \le t \le \overline{T}^{pre}(i)} \left(Y_{irt} - \hat{Y}_{irt}\right)^2}{\sum_{\underline{T}^{pre}(i) \le t \le \overline{T}^{pre}(i)} \left(Y_{irt} - \overline{Y}^{pre}_{irt}\right)^2} \tag{8}$$

[51] To find the county weights, we use the user-written package synth in Stata (Abadie, Diamond, and Hainmueller 2014).

where $\bar{Y}_{irt}^{pre}$ is the average of the outcome during the pre-policy period. $\tilde{R}_{ir} = 1$ indicates a perfect match, and low values (including negative ones) indicate the match is poor. We report Ferman and Pinto's pseudo R-squared statistic for each city in Table 5.

## *Inference*

We use placebo tests to infer the statistical significance of our estimates. For a given null hypothesis about the true effect of the policy in a city—such as the policy had no effect—this approach assesses how likely the effect we observe could have occurred under the null by comparing it against synthetic control estimates in each of the comparison counties. Abadie, Diamond and Hainmueller (2010) originally proposed this method for performing inference. To construct confidence intervals, we follow an extension of Abadie, Diamond, and Hainmueller's procedure proposed by Firpo and Possebom (2017).[52]

To determine whether an estimate is statistically significant, we compute a test statistic constructed from the absolute value of the effect that we measure in each city. Let $N^{pre}(i)$ denote the number of pre-policy period quarters available in our sample for city $i$, and let $N^{eval}(i)$ denote the number of evaluation period quarters available. The test statistic for city $i$ is then:

$$S_{ir} \equiv \left| \frac{1}{N^{eval}(i)} \sum_{\underline{T}^{eval}(i) \leq t \leq \overline{T}^{eval}(i)} \left( Y_{irt} - \hat{Y}_{irt} \right) \right| \tag{9}$$

where $\underline{T}^{eval}(i)$ and $\overline{T}^{eval}(i)$ denote first and last quarters of city $i$'s evaluation period, respectively.

To then test the null hypothesis of no effect of the city $i$'s local minimum wage on outcome $r$, we perform the following steps: (1) Estimate the synthetic control for each of city $i$'s comparison counties, assuming the same pre-policy and evaluation periods as city $i$. (2) For each comparison county, compute its test statistic, $S$. (3) Compute the p-value from the number of comparison counties with a larger $S$ than city $i$:

$$p_{ir} \equiv \frac{1 + \sum_{j \in J(i)} 1\left(S_{jr} \geq S_{ir}\right)}{1 + |J(i)|} \tag{10}$$

To construct confidence intervals, we invert the test statistic, $S$, following a procedure outlined in Firpo and Possebom (2017). The 90 percent confidence intervals we report in Table 5 then include all

[52] Recent econometric studies indicate that statistical tests based on placebo test-based approach may be biased (e.g., Ferman and Pinto 2017b). Unfortunately, the econometrics literature on synthetic control inference has not settled on a solution to this issue. As a result, we interpret the statistical tests we report as only suggestive.

minimum wage effects whose associated null hypothesis are not rejected by our inference procedure at the 10 percent level.[53]

### *Computing earnings and employment elasticities*

To estimate the pooled earnings and employment elasticities we report in Figures 8, 9 and 10, we first obtain the line of best fit between the six cities' effects and their average minimum wage increases. We then measure the elasticity using the slope of this line.

Let $\partial lnMW_i$ denote the average minimum wage increase we observe in city $i$. Let $\hat{\alpha}_{ir}$ denote the synthetic control-based estimate of outcome $r$ in city $i$.[54] Our estimator of the elasticity is based on the expectation that, assuming the elasticity with respect to the minimum wage is constant across cities, the effect of the minimum wage policy in each city will be commensurate with the average increase in the city's minimum wage: $\hat{\alpha}_{ir} \approx \epsilon_r \times \partial lnMW_i$.

We estimate the elasticity with respect to the minimum wage as the solution to the least squares problem based on this relationship:

$$\hat{\epsilon}_r \equiv \text{argmin}_e \sum_{i \in 6\ cities} (\hat{\alpha}_{ir} - e \times \partial lnMW_i)^2 \tag{11}$$

For outcome $r$, the elasticity with respect to the minimum wage is then:

$$\hat{\epsilon}_r = \frac{\sum_{i \in 6\ cities} \hat{\alpha}_{ir} \times \partial lnMW_i}{\sum_{i \in 6\ cities} \partial lnMW_i^2} \tag{12}$$

For Chicago, the District of Columbia, Oakland and San Jose, the average minimum wage increase, $\partial lnMW_i$, is the increase we report in Table 2: We measure their increase by subtracting the city's log minimum wage at the end of the pre-policy period from the average log minimum wage during the evaluation period. For San Francisco and Seattle, which previously indexed their minimum wage to inflation, we adjust this difference for the expected increase in the minimum wage due to indexing by subtracting the average minimum wage increase that we observe in their synthetic control:

$$\partial lnMW_i = \frac{1}{N^{eval}(i)} \sum_{\underline{T}^{eval}(i) \leq t \leq \overline{T}^{eval}(i)} \left(lnMW_{it} - \widehat{lnMW}_{it}\right) - \left(lnMW_{i\overline{T}^{pre}(i)} - \widehat{lnMW}_{i\overline{T}^{pre}(i)}\right) \tag{13}$$

where $\widehat{lnMW}_{it}$ is the average log minimum wage using the synthetic control weights: $\widehat{lnMW}_{it} \equiv \sum_{j \in J(i)} w^*_{ijr} lnMW_{jt}$ . Since we use synthetic control weights to calculate the average minimum wage

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53 We thank Vítor Possebom for sharing their R code for constructing confidence intervals.

54 Formally, $\hat{\alpha}_{ir} \equiv \frac{1}{N^{eval}(i)} \sum_{\underline{T}^{eval}(i) \leq t \leq \overline{T}^{eval}(i)} \left(Y_{irt} - \hat{Y}_{irt}\right)$.

increase in San Francisco and Seattle, and we estimate different weights for each outcome, the average increase differs slightly depending on the outcome we are analyzing.[55]

## *Testing the parallel trends assumption*

We perform two tests for whether the synthetic control method accurately constructs a weighted average of untreated comparison counties that would have trended with the cities but for the new local minimum wage policies. In the first, we use synthetic control to measure the effect of the minimum wage policy for each untreated county included in a city's comparison group. Since these comparison counties experience no change in their minimum wage policies during the evaluation period, we should not measure any effect of the minimum wage.

To perform this test, we rely on the synthetic control estimates we use in our placebo tests to infer statistical significance. Let $\hat{\alpha}_{ijr}$ denote the synthetic control-based estimate of city $i$'s comparison county $j$ for outcome $r$. The rows labeled "Mean effect, comparison group" in Table 5 reports the average of these estimates over all of city $i$'s comparison counties: $\frac{1}{|J(i)|}\sum_{j\in J(i)}\hat{\alpha}_{ijr}$.

For our second test of the parallel trends assumption, we test for any effects of the minimum wage policies during the final year of the pre-policy period. Since the new minimum wage policy had not yet gone into effect, there should be no difference during this year between the city's actual food service average earnings and employment and their synthetic controls'.

To perform this test, we re-run synthetic control for each city and each outcome, but, instead of finding county weights to optimize the match based on all pre-policy quarters, we find county weights to optimize the match based on all pre-policy quarters except for the final year. In other words, we drop the final four quarters from the optimization problem specified in Equation (6):

$$\left(\widetilde{w}_{i1r}^{*},\dots,\widetilde{w}_{iJ(i)r}^{*}\right)\in \operatorname{argmin}_{\overrightarrow{w_i}\in\mathcal{W}}\sum_{\underline{T}^{pre}(i)\le t\le \overline{T}^{pre}(i)-3}\left(Y_{irt}-\sum_{j\in J(i)}w_{ijr}Y_{jrt}\right)^2 \tag{14}$$

We then compute the effect of the policy during the final pre-policy year by taking the average of the difference between the outcome's actual and synthetic values over the final four quarters. The row labeled "Effect during final pre-policy year" in Table 5 reports the six cities' estimates during this period. The row labeled "P-value, effect during final pre-policy year" reports the associated p-value. To perform inference, we follow the same procedure described above, but we substitute the final pre-policy year for the evaluation period. The test statistic for outcome $r$ in city $i$ in this case is:

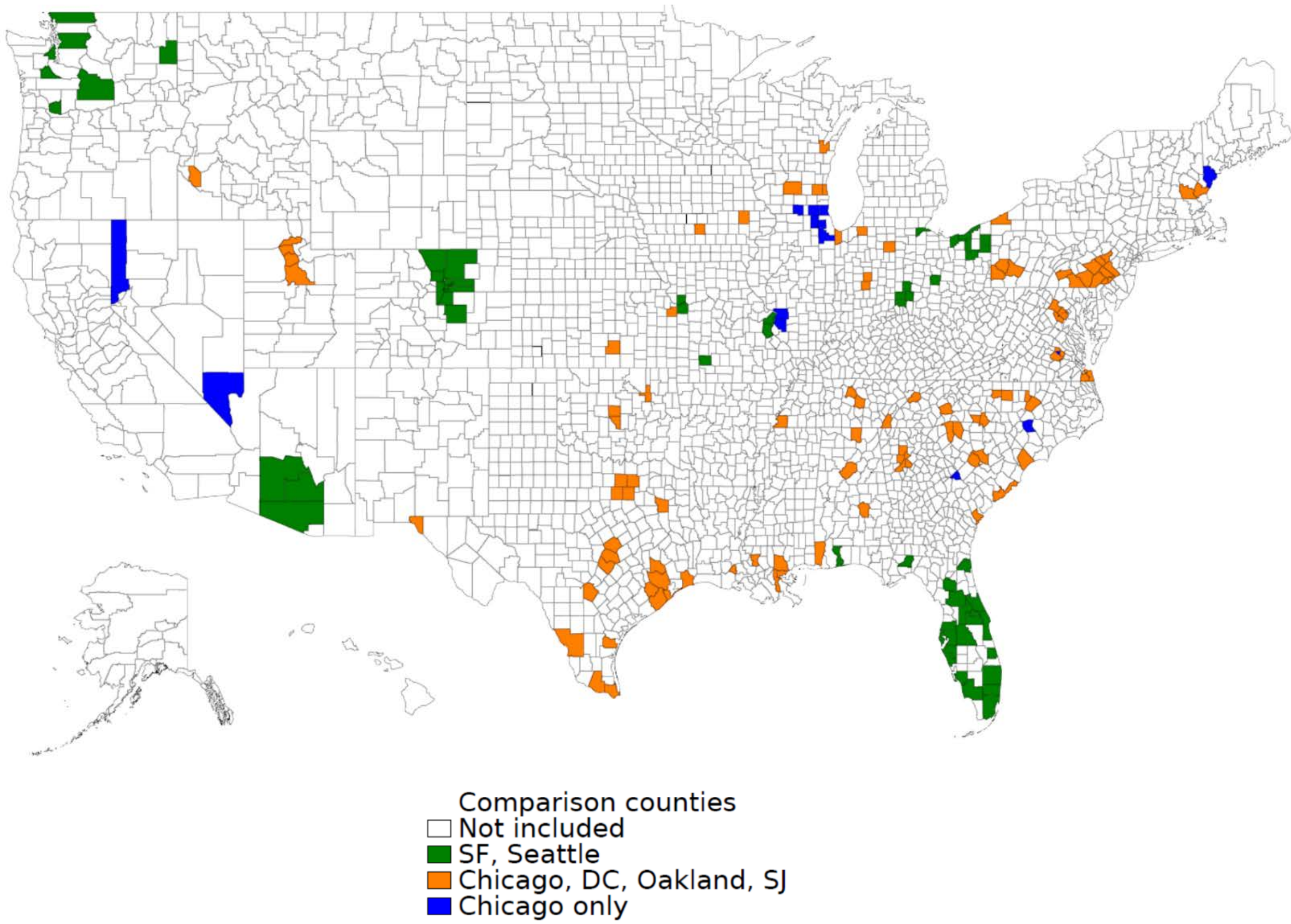
[55] In San Francisco, we estimate the average minimum wage increase is 11.4 percent using the food service earnings-based weights and 11.1 percent using the employment-based weights. In Seattle, we estimate the average minimum wage increase is 24.1 percent using either the earnings or employment-based weights.

$$\tilde{S}_{ir} \equiv \left| \frac{1}{4} \sum_{\underline{T}^{pre}(i)-3 \leq t \leq \overline{T}^{pre}(i)} \left( Y_{irt} - \hat{Y}_{irt} \right) \right| \tag{15}$$

# Appendix B: Additional exhibits

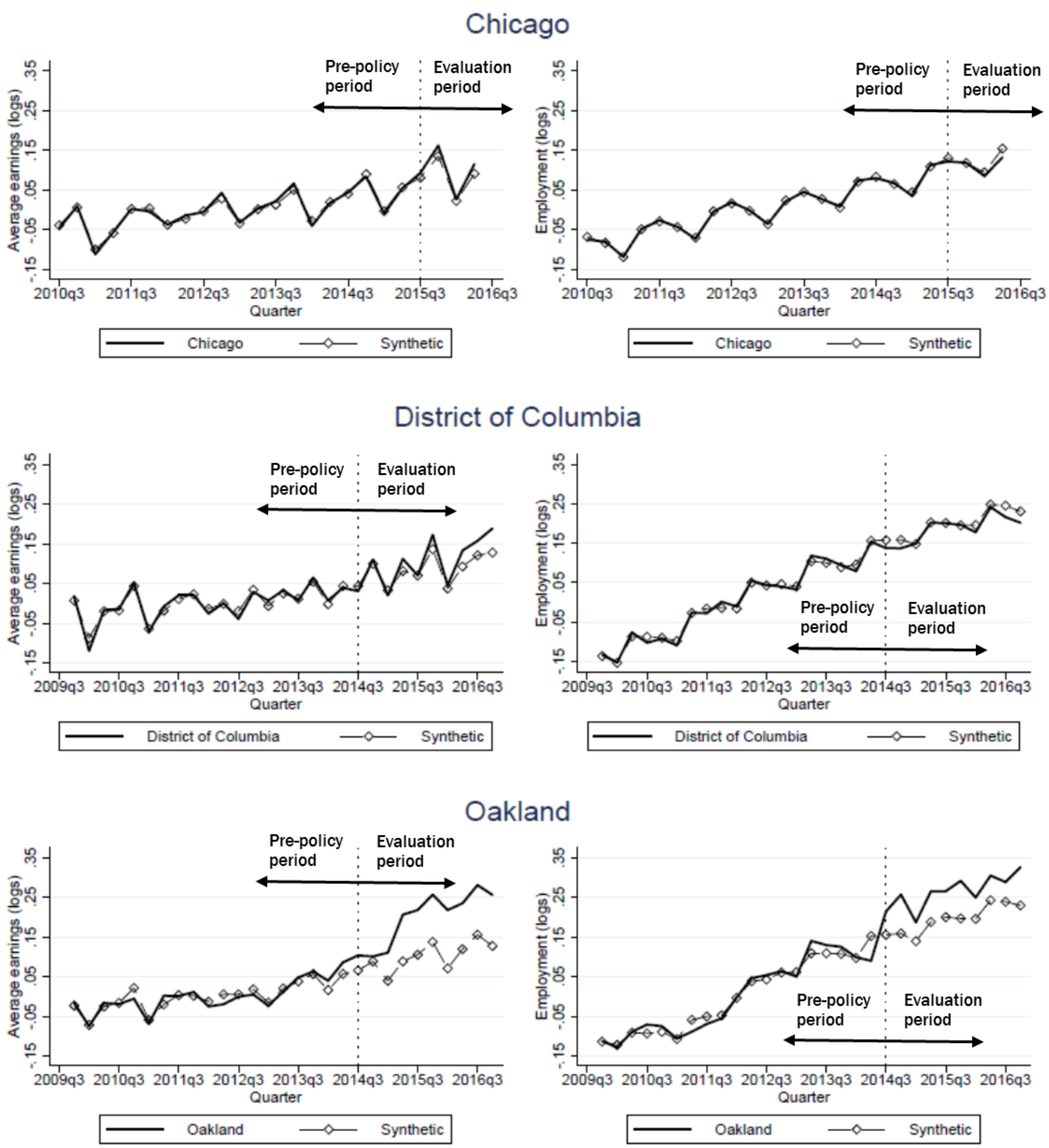
## *Figures*

**Appendix Figure 1 Map of comparison counties**

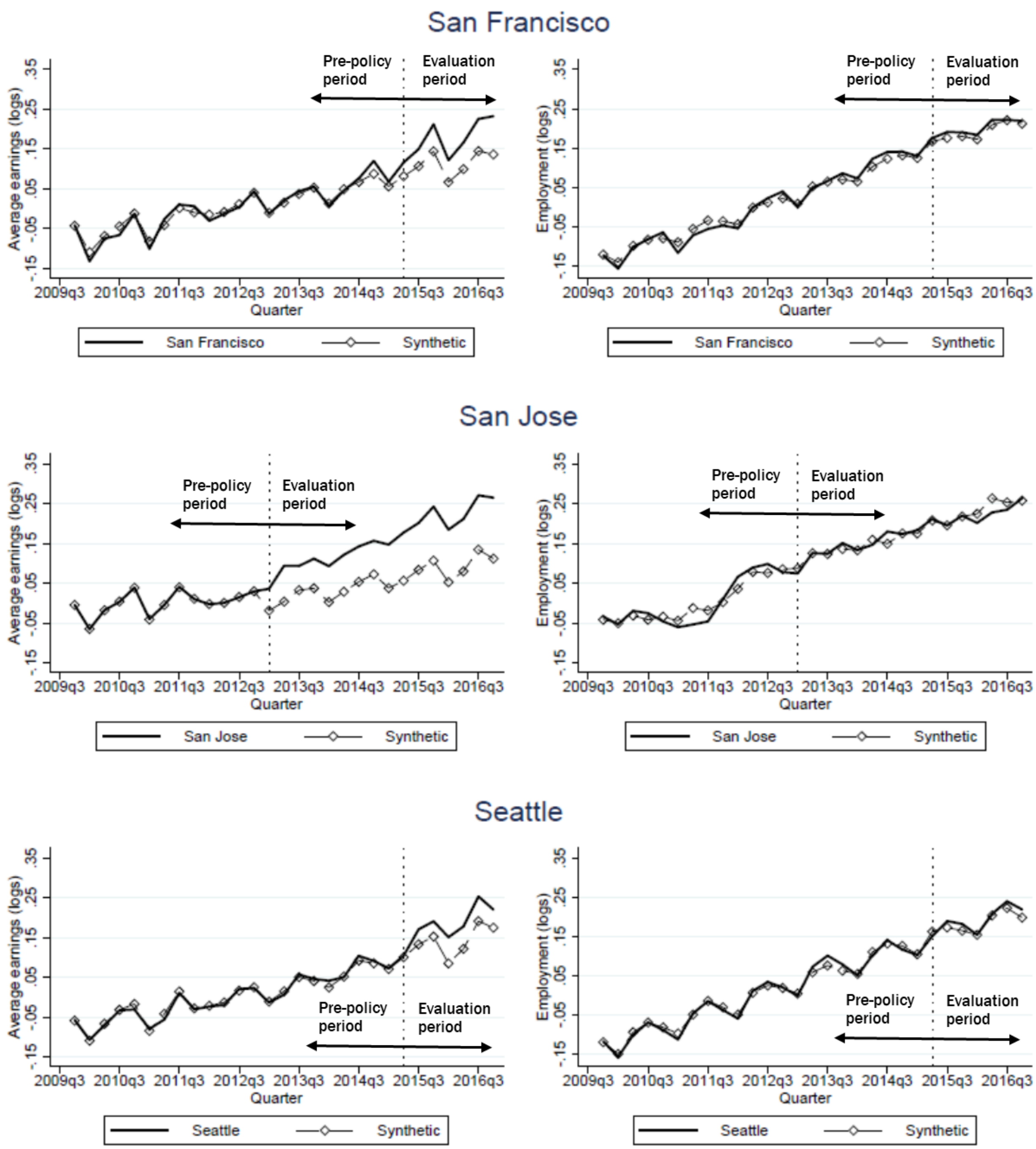



Notes: This map shows the untreated comparison counties that we use in our event study and synthetic control analyses. We include counties that (1) had no change in their minimum wage policy during our period of study, and (2) are in a metropolitan area with an estimated population of at least 200,000 in 2009q4. For the District of Columbia, Oakland and San Jose, we include counties that had no minimum wage increase between 2009q4 and 2016q4. For San Francisco and Seattle, which previously indexed their minimum wage to inflation, we include counties in states that also indexed their minimum wage and had no other minimum wage increases between 2009q4 and 2016q4. For Chicago (whose state-level minimum wage increased to $8.25 in 2010q3), we include counties that had no minimum wage increase between 2010q4 and 2016q4.

**Appendix Figure 2 Synthetic control analysis of Chicago, District of Columbia and Oakland**



**Appendix Figure 3 Synthetic control analysis of San Francisco, San Jose and Seattle**



## *Tables*

### Appendix Table 1 Synthetic control earnings results using different population thresholds

| | Chicago (1) | District of Columbia (2) | Oakland (3) | San Francisco (4) | San Jose (5) | Seattle (6) |
|---|---|---|---|---|---|---|
| **Panel A: Comparison counties, population greater than 100,000** | | | | | | |
| Effect of MW increase | 0.017 | 0.012 | 0.094** | 0.067** | 0.098** | 0.050** |
| P-value | 0.299 | 0.545 | 0.014 | 0.023 | 0.010 | 0.023 |
| 90% CI | [-0.014,0.047] | [-0.026,0.050] | [0.055,0.132] | [0.044,0.093] | [0.055,0.142] | [0.027,0.078] |
| Tests of parallel trends assumption: | | | | | | |
| Effect during final pre-policy year | -0.002 | -0.005 | 0.019* | 0.018** | 0.000 | 0.012 |
| P-value, effect during final pre-policy year | 0.632 | 0.378 | 0.057 | 0.045 | 0.976 | 0.170 |
| Mean effect, comparison group | 0.002 | 0.003 | 0.002 | -0.001 | 0.001 | 0.000 |
| Pre-policy pseudo R-squared | 0.983 | 0.951 | 0.933 | 0.958 | 1.000 | 0.986 |
| Counties in comparison group | 233 | 208 | 208 | 87 | 208 | 87 |
| Pre-policy periods | 20 | 19 | 19 | 22 | 13 | 22 |
| **Panel B: Comparison counties, population greater than 200,000 (in report)** | | | | | | |
| Effect of MW increase | 0.017 | 0.020 | 0.099** | 0.063** | 0.105** | 0.044** |
| P-value | 0.237 | 0.270 | 0.020 | 0.033 | 0.020 | 0.033 |
| 90% CI | [-0.007,0.043] | [-0.021,0.060] | [0.058,0.139] | [0.041,0.088] | [0.059,0.150] | [0.022,0.068] |
| Tests of parallel trends assumption: | | | | | | |
| Effect during final pre-policy year | -0.005 | 0.002 | 0.028** | 0.015* | -0.001 | 0.008 |
| P-value, effect during final pre-policy year | 0.412 | 0.750 | 0.020 | 0.098 | 0.680 | 0.262 |
| Mean effect, comparison group | -0.001 | 0.003 | 0.001 | -0.001 | 0.000 | 0.000 |
| Pre-policy pseudo R-squared | 0.972 | 0.925 | 0.853 | 0.951 | 0.999 | 0.983 |
| Counties in comparison group | 113 | 99 | 99 | 60 | 99 | 60 |
| Pre-policy periods | 20 | 19 | 19 | 22 | 13 | 22 |
| **Panel C: Comparison counties, population greater than 300,000** | | | | | | |
| Effect of MW increase | 0.016 | 0.016 | 0.101** | 0.046** | 0.096** | 0.041** |
| P-value | 0.208 | 0.348 | 0.029 | 0.045 | 0.029 | 0.045 |
| 90% CI | [-0.008,0.043] | [-0.027,0.059] | [0.058,0.144] | [0.022,0.077] | [0.055,0.139] | [0.018,0.066] |
| Tests of parallel trends assumption: | | | | | | |
| Effect during final pre-policy year | -0.001 | 0.002 | 0.030** | 0.014* | -0.006 | 0.005 |
| P-value, effect during final pre-policy year | 0.896 | 0.739 | 0.014 | 0.068 | 0.261 | 0.432 |
| Mean effect, comparison group | -0.002 | 0.003 | 0.001 | -0.004 | 0.003 | -0.004 |
| Pre-policy pseudo R-squared | 0.948 | 0.916 | 0.835 | 0.940 | 0.982 | 0.979 |
| Counties in comparison group | 76 | 68 | 68 | 43 | 68 | 43 |
| Pre-policy periods | 20 | 19 | 19 | 22 | 13 | 22 |

Notes: ** indicates significance at the 5 percent level. * indicates significance at the 10 percent level. Significance tests and confidence intervals are based on placebo tests. See Table 5 for additional notes.

## Appendix Table 2 Synthetic control employment results using different population thresholds

| | Chicago (1) | District of Columbia (2) | Oakland (3) | San Francisco (4) | San Jose (5) | Seattle (6) |
|---|---|---|---|---|---|---|
| **Panel A: Comparison counties, population greater than 100,000** | | | | | | |
| Effect of MW increase | -0.003 | -0.007 | 0.114** | 0.028 | -0.009 | 0.011 |
| P-value | 0.880 | 0.813 | 0.019 | 0.295 | 0.856 | 0.705 |
| 90% CI | [-0.042,0.037] | [-0.065,0.051] | [0.056,0.170] | [-0.025,0.099] | [-0.098,0.080] | [-0.047,0.074] |
| Tests of parallel trends assumption: | | | | | | |
| Effect during final pre-policy year | 0.000 | -0.005 | -0.011 | 0.023 | 0.010** | 0.005 |
| P-value, effect during final pre-policy year | 0.983 | 0.431 | 0.148 | 0.114 | 0.048 | 0.591 |
| Mean effect, comparison group | 0.000 | -0.003 | -0.003 | -0.001 | -0.009 | -0.001 |
| Pre-policy pseudo R-squared | 0.999 | 0.993 | 0.975 | 0.984 | 0.951 | 0.991 |
| Counties in comparison group | 233 | 208 | 208 | 87 | 208 | 87 |
| Pre-policy periods | 20 | 19 | 19 | 22 | 13 | 22 |
| **Panel B: Comparison counties, population greater than 200,000 (in report)** | | | | | | |
| Effect of MW increase | -0.010 | -0.012 | 0.070** | 0.009 | -0.002 | 0.009 |
| P-value | 0.518 | 0.560 | 0.020 | 0.590 | 0.930 | 0.623 |
| 90% CI | [-0.042,0.022] | [-0.054,0.030] | [0.029,0.112] | [-0.049,0.070] | [-0.060,0.056] | [-0.049,0.069] |
| Tests of parallel trends assumption: | | | | | | |
| Effect during final pre-policy year | -0.002 | -0.002 | -0.011 | 0.022 | 0.020** | -0.003 |
| P-value, effect during final pre-policy year | 0.702 | 0.650 | 0.140 | 0.131 | 0.030 | 0.738 |
| Mean effect, comparison group | -0.002 | -0.001 | -0.002 | -0.001 | -0.003 | -0.001 |
| Pre-policy pseudo R-squared | 0.997 | 0.989 | 0.949 | 0.979 | 0.886 | 0.988 |
| Counties in comparison group | 113 | 99 | 99 | 60 | 99 | 60 |
| Pre-policy periods | 20 | 19 | 19 | 22 | 13 | 22 |
| **Panel C: Comparison counties, population greater than 300,000** | | | | | | |
| Effect of MW increase | -0.004 | 0.009 | 0.069** | 0.004 | -0.015 | 0.009 |
| P-value | 0.792 | 0.710 | 0.029 | 0.795 | 0.609 | 0.682 |
| 90% CI | [-0.041,0.032] | [-0.035,0.053] | [0.025,0.113] | [-0.058,0.065] | [-0.077,0.047] | [-0.053,0.072] |
| Tests of parallel trends assumption: | | | | | | |
| Effect during final pre-policy year | 0.002 | -0.002 | -0.012 | 0.015 | 0.018** | -0.006 |
| P-value, effect during final pre-policy year | 0.675 | 0.754 | 0.116 | 0.205 | 0.043 | 0.545 |
| Mean effect, comparison group | -0.001 | -0.001 | -0.002 | 0.003 | -0.004 | 0.002 |
| Pre-policy pseudo R-squared | 0.990 | 0.987 | 0.948 | 0.973 | 0.855 | 0.979 |
| Counties in comparison group | 76 | 68 | 68 | 43 | 68 | 43 |
| Pre-policy periods | 20 | 19 | 19 | 22 | 13 | 22 |

Notes: ** indicates significance at the 5 percent level. * indicates significance at the 10 percent level. Significance tests and confidence intervals are based on placebo tests. See Table 5 for additional notes.

## Appendix Table 3 Synthetic control weights

| City | County | Earnings | County | Employment |
|---|---|---|---|---|
| Chicago | Philadelphia County, Pennsylvania | 0.600 | Hamilton County, Indiana | 0.193 |
| | Charleston County, South Carolina | 0.204 | Richmond city, Virginia | 0.186 |
| | York County, Maine | 0.113 | Dane County, Wisconsin | 0.089 |
| | Davidson County, Tennessee | 0.044 | Mecklenburg County, North Carolina | 0.086 |
| | Fulton County, Georgia | 0.016 | Bell County, Texas | 0.079 |
| | Rutherford County, Tennessee | 0.014 | Will County, Illinois | 0.077 |
| | Lexington County, South Carolina | 0.010 | Smith County, Texas | 0.063 |
| | | | York County, Maine | 0.048 |
| | | | Northampton County, Pennsylvania | 0.048 |
| | | | Cleveland County, Oklahoma | 0.039 |
| | | | Webb County, Texas | 0.038 |
| | | | Montgomery County, Pennsylvania | 0.030 |
| | | | Cumberland County, Pennsylvania | 0.023 |
| | | | Collin County, Texas | 0.001 |
| District of Columbia | Chester County, Pennsylvania | 0.323 | Smith County, Texas | 0.231 |
| | Arlington County, Virginia | 0.283 | Fort Bend County, Texas | 0.227 |
| | Charleston County, South Carolina | 0.128 | Philadelphia County, Pennsylvania | 0.206 |
| | Loudoun County, Virginia | 0.118 | Brazoria County, Texas | 0.124 |
| | Orleans Parish, Louisiana | 0.107 | Orleans Parish, Louisiana | 0.120 |
| | Bucks County, Pennsylvania | 0.041 | Horry County, South Carolina | 0.084 |
| | | | Chatham County, Georgia | 0.008 |
| Oakland | Weber County, Utah | 0.217 | Davidson County, Tennessee | 0.403 |
| | Charleston County, South Carolina | 0.215 | Henrico County, Virginia | 0.353 |
| | Loudoun County, Virginia | 0.147 | Fort Bend County, Texas | 0.240 |
| | Smith County, Texas | 0.128 | Rockingham County, New Hampshire | 0.005 |
| | Knox County, Tennessee | 0.128 | | |
| | Washington County, Pennsylvania | 0.090 | | |
| | Galveston County, Texas | 0.042 | | |
| | Brazoria County, Texas | 0.024 | | |
| | Sedgwick County, Kansas | 0.008 | | |
| San Francisco | Mahoning County, Ohio | 0.407 | Denver County, Colorado | 0.475 |
| | Lorain County, Ohio | 0.251 | Larimer County, Colorado | 0.309 |
| | Boulder County, Colorado | 0.191 | Lee County, Florida | 0.098 |
| | Larimer County, Colorado | 0.117 | Leon County, Florida | 0.092 |
| | Hamilton County, Ohio | 0.021 | Miami-Dade County, Florida | 0.027 |
| | Jefferson County, Missouri | 0.012 | | |
| San Jose | Hidalgo County, Texas | 0.157 | Spartanburg County, South Carolina | 0.436 |
| | Westmoreland County, Pennsylvania | 0.154 | Brazoria County, Texas | 0.233 |
| | Sedgwick County, Kansas | 0.141 | Davidson County, Tennessee | 0.200 |
| | Loudoun County, Virginia | 0.099 | Fort Bend County, Texas | 0.132 |
| | Horry County, South Carolina | 0.097 | | |
| | Gaston County, North Carolina | 0.095 | | |
| | Brown County, Wisconsin | 0.093 | | |
| | Jefferson Parish, Louisiana | 0.065 | | |
| | Dallas County, Texas | 0.034 | | |
| | Jefferson County, Texas | 0.028 | | |
| | Fairfax County, Virginia | 0.019 | | |
| | Spartanburg County, South Carolina | 0.018 | | |
| Seattle | Boulder County, Colorado | 0.547 | Larimer County, Colorado | 0.407 |
| | Trumbull County, Ohio | 0.148 | Denver County, Colorado | 0.373 |
| | Larimer County, Colorado | 0.139 | Miami-Dade County, Florida | 0.133 |
| | Cuyahoga County, Ohio | 0.076 | Escambia County, Florida | 0.087 |
| | Kitsap County, Washington | 0.075 | | |
| | Lake County, Florida | 0.015 | | |

Notes: This table displays the comparison county weights we use to estimate the synthetic controls for average earnings and employment in food services. We exclude comparison counties that receive zero weight for both the average earnings and employment synthetic controls. The weights we use to estimate the synthetic control for other outcomes are available upon request.

## Appendix Table 4 Synthetic control results for full service restaurants

| | Chicago (1) | District of Columbia (2) | Oakland (3) | San Francisco (4) | San Jose (5) | Seattle (6) |
|---|---|---|---|---|---|---|
| **Panel A: Average earnings (logs)** | | | | | | |
| Effect of MW increase | -0.006 | 0.004 | 0.099** | 0.053** | 0.071** | 0.024 |
| P-value | 0.667 | 0.890 | 0.010 | 0.049 | 0.030 | 0.213 |
| 90% CI | [-0.038,0.027] | [-0.041,0.048] | [0.057,0.143] | [0.015,0.090] | [0.028,0.113] | [-0.014,0.061] |
| Elasticity with respect to the MW | -0.029 | 0.017 | 0.279** | 0.462** | 0.303** | 0.098 |
| 90% CI | [-0.197,0.139] | [-0.186,0.219] | [0.161,0.403] | [0.135,0.788] | [0.121,0.485] | [-0.056,0.252] |
| Test of parallel trends assumption: | | | | | | |
| Effect during final pre-policy year | -0.005 | -0.006 | 0.029** | 0.016* | -0.006 | 0.000 |
| P-value, effect during final pre-policy year | 0.456 | 0.420 | 0.020 | 0.082 | 0.220 | 1.000 |
| Mean effect, comparison group | -0.001 | 0.000 | -0.002 | -0.001 | -0.002 | -0.001 |
| Pre-policy pseudo R-squared | 0.957 | 0.912 | 0.911 | 0.949 | 0.961 | 0.987 |
| **Panel B: Employment (logs)** | | | | | | |
| Effect of MW increase | 0.014 | 0.005 | 0.109** | -0.014 | -0.031 | -0.013 |
| P-value | 0.579 | 0.890 | 0.020 | 0.705 | 0.470 | 0.705 |
| 90% CI | [-0.027,0.054] | [-0.049,0.060] | [0.054,0.163] | [-0.080,0.053] | [-0.107,0.048] | [-0.079,0.054] |
| Elasticity with respect to the MW | 0.071 | 0.024 | 0.306** | -0.120 | -0.135 | -0.054 |
| 90% CI | [-0.139,0.281] | [-0.226,0.274] | [0.152,0.461] | [-0.705,0.468] | [-0.460,0.207] | [-0.330,0.222] |
| Test of parallel trends assumption: | | | | | | |
| Effect during final pre-policy year | 0.002 | -0.003 | -0.010 | -0.003 | 0.013* | -0.008 |
| P-value, effect during final pre-policy year | 0.763 | 0.670 | 0.320 | 0.869 | 0.090 | 0.557 |
| Mean effect, comparison group | -0.003 | 0.001 | 0.001 | -0.001 | -0.003 | -0.001 |
| Pre-policy pseudo R-squared | 0.998 | 0.981 | 0.943 | 0.982 | 0.915 | 0.992 |
| Counties in comparison group | 113 | 99 | 99 | 60 | 99 | 60 |
| Pre-policy periods | 20 | 19 | 19 | 22 | 13 | 22 |

Notes: ** indicates significance at the 5 percent level. * indicates significance at the 10 percent level. Significance tests and confidence intervals are based on placebo tests. See Table 5 for additional notes.

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## Appendix Table 5 Synthetic control results for limited service restaurants

| | Chicago (1) | District of Columbia (2) | Oakland (3) | San Francisco (4) | San Jose (5) | Seattle (6) |
|---|---|---|---|---|---|---|
| **Panel A: Average earnings (logs)** | | | | | | |
| Effect of MW increase | 0.031 | 0.033 | 0.194** | 0.088** | 0.153** | 0.110** |
| P-value | 0.158 | 0.290 | 0.020 | 0.016 | 0.030 | 0.016 |
| 90% CI | [-0.005,0.066] | [-0.039,0.104] | [0.122,0.267] | [0.044,0.129] | [0.076,0.229] | [0.071,0.151] |
| Elasticity with respect to the MW | 0.160 | 0.150 | 0.548** | 0.778** | 0.655** | 0.451** |
| 90% CI | [-0.023,0.343] | [-0.176,0.476] | [0.345,0.752] | [0.389,1.144] | [0.328,0.982] | [0.291,0.621] |
| Test of parallel trends assumption: | | | | | | |
| Effect during final pre-policy year | -0.001 | 0.000 | 0.016 | 0.011 | -0.004 | 0.009 |
| P-value, effect during final pre-policy year | 0.877 | 1.000 | 0.130 | 0.328 | 0.370 | 0.393 |
| Mean effect, comparison group | -0.002 | 0.009 | 0.009 | -0.003 | 0.003 | -0.002 |
| Pre-policy pseudo R-squared | 0.934 | 0.921 | 0.714 | 0.946 | 0.909 | 0.928 |
| **Panel B: Employment (logs)** | | | | | | |
| Effect of MW increase | -0.017 | -0.029 | 0.003 | 0.025 | -0.019 | 0.071* |
| P-value | 0.535 | 0.460 | 0.920 | 0.426 | 0.750 | 0.098 |
| 90% CI | [-0.066,0.032] | [-0.091,0.047] | [-0.062,0.069] | [-0.044,0.095] | [-0.126,0.098] | [0.002,0.140] |
| Elasticity with respect to the MW | -0.087 | -0.130 | 0.009 | 0.220 | -0.081 | 0.290* |
| 90% CI | [-0.344,0.169] | [-0.413,0.214] | [-0.176,0.194] | [-0.380,0.821] | [-0.540,0.422] | [0.008,0.572] |
| Test of parallel trends assumption: | | | | | | |
| Effect during final pre-policy year | -0.013 | -0.009 | 0.012 | 0.023 | 0.009 | -0.006 |
| P-value, effect during final pre-policy year | 0.219 | 0.280 | 0.190 | 0.148 | 0.130 | 0.590 |
| Mean effect, comparison group | -0.001 | -0.001 | -0.001 | -0.003 | -0.009 | -0.003 |
| Pre-policy pseudo R-squared | 0.940 | 0.960 | 0.938 | 0.370 | 0.786 | 0.988 |
| Counties in comparison group | 113 | 99 | 99 | 60 | 99 | 60 |
| Pre-policy periods | 20 | 19 | 19 | 22 | 13 | 22 |

Notes: ** indicates significance at the 5 percent level. * indicates significance at the 10 percent level. Significance tests and confidence intervals are based on placebo tests. See Table 5 for additional notes.

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## Appendix Table 6 Synthetic control results for professional services

| | Chicago (1) | District of Columbia (2) | Oakland (3) | San Francisco (4) | San Jose (5) | Seattle (6) |
|---|---|---|---|---|---|---|
| **Panel A: Average earnings (logs)** | | | | | | |
| Effect of MW increase | 0.017 | -0.004 | 0.042 | 0.060 | 0.047 | 0.000 |
| P-value | 0.482 | 0.900 | 0.180 | 0.197 | 0.180 | 1.000 |
| 90% CI | [-0.035,0.070] | [-0.057,0.050] | [-0.011,0.096] | [-0.013,0.174] | [-0.021,0.115] | [-0.072,0.085] |
| Elasticity with respect to the MW | 0.090 | -0.018 | 0.119 | 0.517 | 0.202 | 0.002 |
| 90% CI | [-0.184,0.363] | [-0.261,0.226] | [-0.032,0.270] | [-0.111,1.507] | [-0.090,0.493] | [-0.295,0.348] |
| Test of parallel trends assumption: | | | | | | |
| Effect during final pre-policy year | -0.005 | -0.003 | -0.013 | 0.016 | 0.012 | 0.000 |
| P-value, effect during final pre-policy year | 0.570 | 0.740 | 0.340 | 0.295 | 0.200 | 1.000 |
| Mean effect, comparison group | 0.003 | 0.000 | 0.000 | 0.001 | 0.002 | 0.002 |
| Pre-policy pseudo R-squared | 0.996 | 0.999 | 0.977 | 0.860 | 0.982 | 0.969 |
| **Panel B: Employment (logs)** | | | | | | |
| Effect of MW increase | 0.010 | 0.005 | 0.054 | 0.100* | -0.065 | 0.003 |
| P-value | 0.693 | 0.850 | 0.280 | 0.066 | 0.480 | 0.951 |
| 90% CI | [-0.048,0.068] | [-0.076,0.086] | [-0.024,0.137] | [0.023,0.196] | [-0.229,0.100] | [-0.080,0.087] |
| Elasticity with respect to the MW | 0.051 | 0.022 | 0.151 | 0.896* | -0.278 | 0.014 |
| 90% CI | [-0.251,0.352] | [-0.349,0.393] | [-0.067,0.388] | [0.208,1.753] | [-0.985,0.429] | [-0.331,0.358] |
| Test of parallel trends assumption: | | | | | | |
| Effect during final pre-policy year | 0.000 | 0.001 | -0.010 | 0.052* | 0.000 | 0.003 |
| P-value, effect during final pre-policy year | 0.982 | 0.750 | 0.270 | 0.098 | 0.520 | 0.787 |
| Mean effect, comparison group | 0.003 | 0.000 | -0.002 | -0.001 | -0.019 | 0.003 |
| Pre-policy pseudo R-squared | 0.997 | 0.986 | 0.430 | 0.959 | 0.969 | 0.995 |
| Counties in comparison group | 113 | 99 | 99 | 60 | 99 | 60 |
| Pre-policy periods | 20 | 19 | 19 | 22 | 13 | 22 |

Notes: ** indicates significance at the 5 percent level. * indicates significance at the 10 percent level. Significance tests and confidence intervals are based on placebo tests. See Table 5 for additional notes.

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