IRLE Institute for Research on Labor and Employment

IRLE WORKING PAPER
#102-19
April 2019

Are Local Minimum Wages Too High?

Carl Nadler, Sylvia A. Allegretto, Anna Godøy and Michael Reich

Cite as: Carl Nadler, Sylvia A. Allegretto, Anna Godøy and Michael Reich (2019). "Are Local Minimum Wages Too High?" IRLE Working Paper No. 102-19.
http://irle.berkeley.edu/files/2019/04/Are-Local-Minimum-Wages-Too-High.pdf

http://irle.berkeley.edu/working-papers

Berkeley
UNIVERSITY OF CALIFORNIA

Are Local Minimum Wages Too High, and How Could We Even Know?*

Carl Nadler Sylvia Allegretto Anna Godoey Michael Reich
Institute for Research on Labor and Employment, UC Berkeley

April 17, 2019

Click here for latest version

Abstract

We measure the effects of six citywide minimum wages that ranged up to $13 in Chicago, the District of Columbia, Oakland, San Francisco, San Jose and Seattle, employing event study and synthetic control methods. Using aggregate data on average earnings and employment in the food services industry, we find significantly positive earnings increases and no significant employment losses. While such evidence suggests the policies raised the earnings of low-wage workers, as intended, a competing explanation is that the industry responds to wage increases by increasing their demand for more productive higher-wage workers, offsetting low-wage layoffs (i.e., labor-labor substitution). To tackle this key question, we present a theoretical framework that connects the responses estimated at the industry-level to the own- and cross-wage labor demand elasticities that summarize the total effect of the policies on workers. Using a calibration exercise, we find that the combination of average earnings gains and constant employment cannot be produced by labor-labor substitution unless there are also effects on hours. To test whether the minimum wage increases demand for higher-wage workers or reduces low-wage workers' hours, we examine the effects of California's recent state and local minimum wage policies on the food services industry. There we find no evidence of labor-labor substitution or hours responses. Thus, the most likely explanation for the responses we find in the cities is that the industry's demand for low-wage workers is inelastic, and the policies raised their earnings.

*Corresponding author: Carl Nadler, cnadler@berkeley.edu. Portions of this paper draw on a policy report, "The New Wave of Local Minimum Wage Policies: Evidence from Six Cities." We acknowledge support from the University of California, the Russell Sage Foundation, the Washington Center for Equitable Growth and the Ford Foundation. We are grateful to Orley Ashenfelter, David Card, David Cooper, Arindrajit Dube, Bruno Fermin, Sergio Firpo, Laura Giuliano, Dave Graham-Squire, Daniel Haanwinckel, Patrick Kline, Vítor Possebom, David Roodman, Jesse Rothstein, Raffaele Saggio, Ian Schmutte, David Silver, Jacob Vigdor, Chris Walters and Ben Zipperer for their expert advice, and for comments from participants at the Berkeley Labor Lunch, the joint American Economic Association-Labor and Employment Relations Association minimum wage panel at the 2018 Allied Social Science Associations annual meeting and a minimum wage panel at the 2018 Western Economic Association meetings. Uyanga Byambaa provided excellent research assistance. The Institute for Research on Labor and Employment connects world-class research with policy to improve workers' lives, communities, and society.

1 Introduction

Many U.S. studies on the labor market effects of minimum wage policies have focused on employment in industries in which a large fraction of workers are paid at or just above the minimum, such as restaurants. When it is found that the policies raise average earnings in the industry without causing statistically significant employment losses, the prevailing interpretation is that the policies raise low-wage workers' earnings (e.g., Card and Krueger, 1994, 2000, Dube et al., 2010, Allegretto et al., 2017). Since this evidence is based on changes in total employment, another explanation is that the industry responds to wage increases by reducing their demand for low-wage workers while simultaneously increasing their demand for more productive higher-wage workers—i.e., labor-labor substitution (e.g., Jardim et al., 2017, Neumark, 2018). Relatedly, if employers respond to wage increases by reducing hours (an intensive margin response) then evidence on employment alone is not sufficient to summarize the total effect.

The question of how we should interpret industry-level evidence is especially relevant for evaluating recent local minimum wages. For most of these localities, there are no publicly available data sources with direct information on wages, hours and employment. As a result, to evaluate them, analysts have to rely on data on average earnings and employment aggregated at the industry-level, such as that provided by the Bureau of Labor Statistics. Despite several decades of minimum wage research, it is also unclear what the effects of these local policies will be. Many of these minimum wages will reach $15 in the next few years, far above the wage floors studied in previous research of federal and state policies. As a result, there is concern that they could be counterproductive even among some minimum wage proponents (e.g., Krueger, 2015).

In this paper, we make three contributions to the minimum wage literature. First, we present a framework that connects the parameters estimated using aggregate industry-level data to the own- and cross-wage labor demand elasticities in a standard model of labor demand. We use this framework to show how labor-labor substitution affects the interpretation of industry-level evidence. Second, we provide a comprehensive assessment of the earnings and employment effects of six recent citywide policies in Chicago, the District of Columbia, Oakland, San Francisco, San Jose and Seattle, in which the new minimum wages ranged from $10 to $13. And third, we present new evidence on labor-labor substitution and hours effects in the food services industry, exploiting the recent changes in California's state and local minimum wage policies that have since 2014 raised the minimum wage from $8 to $10.50 statewide and to over $12 in a growing number of cities.

In the six cities, we find positive earnings responses in the food services industry that are remarkably close to those estimated in recent studies of state and federal minimum wage increases (e.g., Dube et al., 2010; Dube et al., 2016; Totty, 2017). Like these studies, we also do not detect any employment losses. Based on a calibration exercise informed by our theoretical framework, we show that labor-labor substitution alone cannot explain the combination of average earnings gains and constant employment. Intuitively, the reason is that when employers substitute more productive higher-wage workers for lower-wage workers, they necessarily reduce total employment demand. It is possible that a combination of labor-labor substitution and sharply reduced hours per worker can explain the pattern of results we see in the six cities. However, our analysis of California food services finds no evidence of either labor-labor substitution or hours effects. As a result, the most likely explanation for our findings is that total labor demand elasticity for employment and hours combined is inelastic, and the policies have raised the earnings of low-wage workers in the six cities.

Our theoretical analysis and calibration exercise is motivated by a recent critique raised by Jardim et al. (2017), who study the effects of Seattle's recent citywide minimum wage increases. Unlike previous U.S. studies, they use administrative microdata with information on quarterly earnings and hours worked by each employee in locatable establishments in the city. When they aggregate their data at the industry-level, they measure employment effects in food services close to zero. Yet, when they look at all workers earning under $19, they measure large negative employment effects. The effects are even larger when they look at total hours worked by these workers, suggesting that the minimum wage reduces demand along both the extensive and intensive margins. Comparing the different sets of results, they conclude that industry-level evidence based on aggregate earnings and employment data suffer from an "attenuation bias" and are unable to detect impacts on hours. Our theoretical framework suggests that this pattern of results—if they are accurate—can be reconciled only under some form of labor-labor substitution.

Motivated by this critique, we begin by carefully characterizing the conditions under which the responses estimated in industry-level studies—which we call industrywide elasticities—can be used to bound the total effects on low-wage workers. In a standard model of labor demand, these effects are summarized by the own-wage and cross-wage labor demand elasticities for employment and hours. We show that when there is no labor-labor substitution, cross-wage effects are zero, and small industrywide employment responses and positive earnings responses together imply that higher minimum wages increase the average earnings of low-wage workers net of any hours reductions.[1]

To quantify the effect of labor-labor substitution on this relationship, we focus on the extensive margin and consider a model in which production depends on only three inputs: a low-wage group whose wage is covered by the minimum wage, a high-wage group and capital. We show that the number of high-wage substitutes needed to replace the low-wage workers depends on three factors: the marginal rate of technical substitution of the low-wage group for the high-wage group, the ratio of the groups' weekly hours and the effect on capital. In industries such as food services, capital is considered to be another substitute for low-wage workers, and high-wage workers work longer hours on average. Therefore, the total increase in demand for high-wage workers will not fully offset the change in low-wage employment. As a result, small industrywide employment elasticities are inconsistent with large employment effects on low-wage workers in this case as well.[2]

[1] Neumark and Wascher (2008) show a similar relationship between group-level employment elasticities and the elasticity of labor demand (see also Neumark, 2018). To the best of our knowledge, our analysis of the industrywide elasticity of earnings is new.

[2] Brown et al. (1982) show a similar relationship between the employment responses estimated on de-

In the next part of the paper, we turn to our second objective, in which we measure the industrywide earnings and employment elasticities in the six cities. We focus on the food services industry, because of the large fraction of workers there who we estimate are covered under the policy, over 75 percent. We begin with an event study approach, comparing the treated cities to untreated metropolitan areas across the U.S. Our preferred model finds a statistically significant earnings elasticity of 0.19 and an insignificant employment elasticity of 0.04. The 90 percent confidence interval rules out negative employment elasticities lower than -0.05, indicating that employment losses industrywide have, if anything, been small.

Motivated in part by the contentious debates in the minimum wage literature over how to construct a valid counterfactual for employment in the absence of a minimum wage increase (e.g., Neumark et al., 2014, Allegretto et al., 2017), we show that these estimates are robust to an alternative approach using a synthetic control estimator (Abadie and Gardeazabal, 2003; Abadie et al., 2010).[3]

With the estimates of the industrywide elasticities in hand, we return to our original question and ask what they imply for low-wage workers. Using a calibration exercise based on our theoretical framework, we find the answer may depend on whether there is both labor-labor substitution and hours effects. In the absence of hours effects, our industrywide estimates rule out employment elasticities on the workers who are directly affected by the increase lower than -0.25, which implies that the policies raised low-wage workers' earnings. However, when there are also hours effects, higher-wage substitutes replace the output from not only low-wage workers' lost employment but also their hours. Therefore, if there are hours effects, it is possible that the employment gains among the substitutes could fully

mographic groups and the effects on those directly affected by the minimum wage. Although labor-labor substitution alone is unable to produce the combination of average earnings gains and constant employment, it can come close if the industry is able to substitute very low-wage workers with those whose market value is equal to the new minimum and who work similar hours. Importantly, however, this extreme form of labor-labor substitution cannot reconcile the pattern of results in Jardim et al. (2017). In this case, Jardim et al.'s preferred approach (based on all workers earning under $19) would not be able to detect the losses that they report.

[3] Previous studies of minimum wage policies using synthetic control estimators include Dube and Zipperer (2015), Jardim et al. (2017), Neumark et al. (2014) and Powell (2017).

offset the employment losses among low-wage workers.

To distinguish between these interpretations, in the third and final part of the paper we exploit California's recent changes in its state and local minimum wage policies and measure their combined influence on the demographics and hours worked among low-wage workers in the food services industry. We find no evidence that the policies caused the industry to replace the low-wage workers with those with higher qualifications or reduce their hours. With little empirical support for labor-labor substitution or hours effects, we conclude that the six cities' policies raised the earnings of low-wage workers in this industry.[4]

The rest of this paper is structured as follows. Section 2 provides a framework for interpreting the responses observed in industry-level studies. Section 3 describes the data we use in our analysis. Section 4 provides background on the six cities and their local minimum wage policies and reviews our analytic approach. Sections 5 and 6 report results from our event study and synthetic control analyses, respectively. Section 7 summarizes results from additional robustness and falsification tests. Section 8 reports the results from our calibration exercise. Section 9 presents evidence on labor-labor substitution and hours responses in the food services industry. Section 10 concludes.

2 A framework for interpreting industrywide responses

The goal of our analysis is to measure the effects of local minimum wage policies on workers whose wages are directly affected by the policies—who we refer to as minimum wage workers. As in previous studies, we focus on food services[5] and rely on industry-level data for each locality with information on average earnings and employment. This aggregation raises the question of how the responses that we observe at the industry-level relate to minimum wage workers. In particular, if we find the minimum wage raised earnings and did not reduce

[4]Our findings on labor-labor substitution are consistent with a growing number of studies that also find that minimum wages do not influence the composition of employment across different demographic groups (e.g., Dube et al., 2016, Cengiz et al., 2018, Giuliano, 2013).

[5]The full title of this industry is "food services and drinking places" (NAICS code 722).

employment at the industry-level, can we infer the employment effects on the industry's minimum wage workers were small enough such that on net the policy raised their earnings? Since industry-level responses are averages over workers who are and are not affected by the policy, one concern is that they will understate the actual effects of the policy, a form of "attenuation bias" (Jardim et al., 2017). This attenuation will be worse if increases in wages of one group of workers increases the demand for another, higher-wage group—i.e., labor-labor substitution (e.g., Neumark, 2018). Also, since we do not observe hours, we may not be able to tell if there are hours responses. To clarify these issues, in this section we present a framework for interpreting industry-level responses and show their relationship to the causal effects on minimum wage workers.

The causal estimands of industry-level analyses are the partial effects of the minimum wage on average earnings and employment. Typically, the outcomes and the minimum wage are specified in logs, so that these parameters are interpretable as elasticities with respect to the minimum wage. We call these estimands the *industrywide elasticities of earnings and employment with respect to the minimum wage*, or simply the industrywide elasticities.[6]

Assume that output in the industry is produced using labor and capital under a constant returns to scale production function. Also assume that the industry is competitive in both the factor and product markets, so that in equilibrium demand for each factor is set so that its marginal product is equal to an exogenously determined price. (In other words, the supply of labor and capital is perfectly elastic). Assume further that there are J groups of labor, indexed by i. The wage for each group is w_i. In equilibrium, the industry demands N_i workers of each group i for h_i hours a week.

Under these assumptions, we can decompose the industrywide elasticities into the effects of the minimum wage on different groups of workers. Taking the partial derivative of log employment with respect to the log minimum wage, we find that the industrywide employ-

[6] For example, in Dube et al. (2010), the main estimating equations take the form: $lnY_{it} = \epsilon lnMW_{it} + controls + \nu_{it}$, where Y_{it} is either average earnings or employment in restaurants, i indexes U.S. counties and t calendar quarter, and MW_{it} is the minimum wage. Using our terminology, the coefficient ϵ is then the industrywide elasticity of Y with respect to the minimum wage.

ment elasticity is a weighted average of group-specific employment elasticities, with weights that depend on the group's size:

$$\frac{\partial lnN}{\partial lnMW} = \sum_i \frac{N_i}{N} \frac{\partial lnN_i}{\partial lnMW_i} \tag{1}$$

Applying a similar set of steps to log average earnings, we find a more nuanced relationship between the industrywide earnings elasticity and the minimum wage's group-specific influence. Note that group i's total earnings in the industry is a product of their wage, hours and total employment: $Earn_i \equiv w_i h_i N_i$. Differentiating log average earnings and re-arranging terms, we find:

$$\begin{aligned} \frac{\partial lnEarn}{\partial lnMW} &= \sum_i eshr_i \left(\frac{\partial lnw_i}{\partial lnMW} + \frac{\partial lnh_i}{\partial lnMW} + \frac{\partial lnN_i}{\partial lnMW} \right) - \frac{\partial lnN}{\partial lnMW} \\ &= \sum_i \left[\underbrace{eshr_i \left(\frac{\partial lnw_i}{\partial lnMW} + \frac{\partial lnh_i}{\partial lnMW_i} \right)}_{\text{wage and hours effects}} + \underbrace{\left(eshr_i - \frac{N_i}{N} \right) \frac{\partial lnN_i}{\partial lnMW_i}}_{\text{composition effect}} \right] \end{aligned} \tag{2}$$

where $eshr_i$ is group i's share of earnings in the industry: $eshr_i \equiv \frac{w_i h_i N_i}{\sum_l w_l h_l N_l}$.

Equation (2) shows that the industrywide earnings elasticity is a weighted average of wage and hours responses as well as a composition effect. The sign of the composition effect depends on the difference between the group's share of earnings and the group's share of the workforce. When wages vary between groups, this term will be negative for low-wage workers and positive for groups higher in the wage distribution. In the context of a minimum wage increase, the composition effect captures the increase in average earnings that would result from layoffs.

For understanding the effects of the minimum wage on minimum wage workers, an important question is whether the increases in earnings caused by the new minimum are offset by hours and employment reductions—that is, whether the new minimum raises minimum wage workers' earnings on net. Given the notation above, this condition is $\frac{\partial lnw_i}{\partial lnMW_i} + \frac{\partial lnh_i}{\partial lnMW_i} + \frac{\partial lnN_i}{\partial lnMW_i} > 0$. If an increase in the minimum wage influences the demand only for groups whose wages are directly affected by the increase, then Equations (1) and (2) suggest that the industrywide elasticities are sufficient to answer this answer. If we find positive industrywide earnings elasticities and near zero industrywide employment elasticities—such as those reported by Dube et al. (2010, 2016), Addison et al. (2012), Totty (2017) and Jardim et al. (2017)—then the average employment effects on minimum wage workers must also be close to zero. In this case, there are no composition effects, and a positive industrywide earnings elasticity further implies that wage gains are larger than hours reductions.

The issue with this interpretation is that an increase in the wage of one group of workers influences the demand for other groups. In a standard model of labor demand, these adjustments are called cross-wage effects. If higher-wage workers are substitutes for lower-wage workers, then the cross-wage effects of a minimum wage increase on these groups are positive and will offset the reductions in the demand for the minimum wage workers. This process is called labor-labor substitution. In this case, the industrywide earnings and employment elasticities no longer reflect only the effects on minimum wage workers since the elasticities are averages over higher-wage groups as well.

To better understand how labor-labor substitution affects the interpretation of the industrywide elasticities, consider a stylized setting in which there are only two groups, a low-wage group, L, and a high-wage group, H. The increase in the minimum wage only affects the wage of the low-wage group. We also assume there are no hours responses.[7] The constant

[7] To incorporate employment and hours responses into our analysis of labor-labor substitution, we would need to specify separate prices for both the extensive and intensive margins of adjustment. We would also need to assume a labor aggregator function that specifies how each group's employment and hours enter into the production function. (See Hamermesh (1993) for an insightful discussion of these issues). We leave this to future research. Jardim et al. (2017) argue that hours responses are an important channel for understanding the total effects of minimum wage policies: They find larger negative effects of Seattle's minimum wage on total hours worked by low-wage workers than on their headcount employment. However, a comparison of Jardim et al.'s hours and employment estimates from their preferred synthetic control models reveals that the effect on employment accounts for about two thirds of the effect on total hours. They also find slightly larger effects on employment than total hours using their interactive fixed effects estimator (see Table 6).

returns to scale production function in this case is $F(h_L N_L, h_H N_H, K)$, where K is capital.

Under these assumptions, the total effects of a minimum wage increase follow the Hicks-Marshall rule of derived demand: For each group of workers, the total employment effect is the sum of output-constant effects—attributable to substitution between the different factors of production—and a scale effect (e.g., Hamermesh, 1993). In this case, the scale effect captures the reduction in industrywide employment attributable to the minimum wage's effect on production costs.

Let ϵ_{LL} denote the output-constant labor demand elasticity for the low-wage group with respect to their wage. Similarly, let ϵ_{HL} and ϵ_{KL} denote the output-constant cross-wage elasticities of labor demand for the high-wage group and capital, respectively, with respect to the low-wage group's wage.

To see how labor-labor substitution affects the interpretation of the industrywide employment elasticity, note that under cost minimization the output-constant elasticities obey:

$$F_L \epsilon_{LL} \frac{\partial lnw_L}{\partial lnMW} h_L L + F_H \epsilon_{HL} \frac{\partial lnw_L}{\partial lnMW} h_H H + F_K \epsilon_{KL} \frac{\partial lnw_L}{\partial lnMW} K = 0 \tag{3}$$

where F_L, F_H, and F_K are the marginal products of each factor. In other words, in order for an increase in the minimum wage to have no net effect on industry output, the change in output attributable to the change in demand for each factor individually must sum to zero.[8]

Equation (3) implies we can express the change in the demand for the high-wage group as a function of the change in the demand of the low-wage group and capital. Noting that under cost minimization the ratio of the marginal products is equal to the ratio of the factor prices, we find that the industrywide employment elasticity under labor-labor substitution

These comparisons suggest that most of the effect of the minimum wage in their analysis operates through the demand for low-wage workers' employment, not their weekly hours. In Section 9 we find no effects of California's state and local minimum wage policies on low-wage workers' weekly hours in the food services industry. We therefore believe that focusing on employment is likely to be a reasonable approximation for understanding how labor-labor substitution influences the interpretation of industrywide elasticities.

[8] To derive Equation (3), note that under cost minimization, output equals a constant $q \equiv F\left(h_L L^c(w,r,q), h_H H^c(w,r,q), K^c(w,r,q)\right)$, where $L^c(\cdot), H^c(\cdot), K^c(\cdot)$ denote the conditional factor demands, w is a vector of wages and r the price of capital. We then differentiate both sides of this identity with respect to the log minimum wage.

is:

$$\frac{\partial lnN}{\partial lnMW} = \frac{L}{N}\frac{\partial lnw_L}{\partial lnMW}\epsilon_{LL} + \frac{H}{N}\frac{\partial lnw_L}{\partial lnMW}\epsilon_{HL} - \frac{\partial lnw_L}{\partial lnMW}\eta s_L$$
$$= \frac{L}{N}\frac{\partial lnw_L}{\partial lnMW}\underbrace{\left(1 - \frac{w_L h_L}{w_H h_H}\right)\epsilon_{LL}}_{\text{labor-labor substitution}} - \underbrace{\frac{s_K}{s_H}\frac{H}{N}\frac{\partial lnw_L}{\partial lnMW}\epsilon_{KL}}_{\text{capital effect}} - \underbrace{\frac{\partial lnw_L}{\partial lnMW}\eta s_L}_{\text{scale effect}} \qquad (4)$$

where s_L, s_H and s_K are each factor's share of total costs and η is the elasticity of product demand. Under labor-labor substitution, the industrywide employment elasticity is a function of three components: (1) the output-constant effect on low-wage workers with an adjustment for labor-labor substitution, (2) the effect on the demand for capital, and (3) a scale effect.[9]

Equation (4) shows that the increase in the demand for high-wage workers after a minimum wage increase will not fully offset the reduction in low-wage employment for four reasons: First, in low-wage industries such as food services, capital is likely to also be another substitute for the low-wage group, so some of the output will be preserved via labor-capital substitution: $\epsilon_{KL} \geq 0$. Second, the amount of high-wage labor needed will depend on the marginal rate of technical substitution (MRTS) of the low-wage group for the high-wage group: $\frac{F_L}{F_H}$. Since under cost minimization the MRTS is equal to the ratio of the wages of the two groups, this ratio is less than one. Third, high-wage workers work longer hours on average than low-wage workers since high-wage workers are more likely to work full-time: $h_H \geq h_L$. Fourth, scale effects reduce the demand for both low- and high-wage workers. Therefore, if we find an increase in the minimum wage does not reduce industrywide employment, it implies the policy does not reduce demand for low-wage workers either.

Nevertheless, if the high-wage group of workers earn close to the new minimum wage

[9] Brown et al. (1982) show a similar relationship between (1) the employment elasticity with respect to the minimum wage for a group of workers in which only a subset are low-wage and earn a wage directly affected by the minimum and (2) the elasticity of employment for the low-wage subset. Their exposition abstracts from capital and scale effects and differences between hours worked by the different subgroups.

and work a similar number of hours per week as the low-wage group, it is possible that large reductions in the demand for low-wage workers' employment yield only small changes in total industrywide employment that may be difficult to distinguish statistically from no effect at all. Yet, this form of labor-labor substitution is less likely to be an issue in our setting, in which we measure the effects of large increases in the minimum wage. For example, suppose after Seattle's minimum wage increase from $9.47 to $13 the food services industry substitutes workers previously paid $9.47 with workers whose market wage is $15. For simplicity also suppose these are the only two types of workers and the $9.47 workers account for 25 percent of the industry (a similar share who we estimate worked at the pre-policy minimum in the six cities on average). According to Equation (4), in this case an output-constant labor demand elasticity of -1 for $9.47 workers would yield an industrywide employment elasticity equal to $\left(1 - \frac{9.47}{13}\right) \times -1 = -0.07$ before accounting for capital and scale effects. The 90 percent confidence intervals that we estimate in our event study analysis are able to rule out the modest employment losses implied by even this extreme form of labor-labor substitution.[10]

In summary, using a standard model of labor demand, we have shown that in the absence of labor-labor substitution, industrywide earnings and employment elasticities are averages over the effects on minimum wage workers' hours and employment. As a result, if we find an increase in the minimum wage raises earnings and has no effect on employment industrywide, it implies that the policy raises the earnings of minimum wage workers on average net of reductions in their employment or hours. Labor-labor substitution yields smaller industrywide employment elasticities. But, small industrywide employment elasticities are still inconsistent with highly elastic demand responses, especially if the substitute workers are earning wages well above the minimum or work longer hours than minimum wage workers.

[10]Interestingly, this extreme form of labor-labor substitution cannot reconcile the pattern of results in Jardim et al. (2017). In this case, Jardim et al.'s preferred approach (based on changes in employment and hours among workers earning under $19) would not be able to detect the losses that they report. We therefore consider this case to be very unlikely in light of the available evidence. We test for labor-labor substitution directly in Section 9.

To make our analysis of labor-labor substitution tractable, we have assumed the minimum wage does not reduce the demand for minimum wage workers' hours. If industries respond to wage increases by reducing hours as well employment, the total increase in the demand for high-wage substitutes could potentially fully offset the reduction in minimum wage worker employment. We present evidence in support of this assumption in Section 9, in which we study California's recent state and local minimum wage policies on the food services industries' use of low-wage labor and find no effects on hours. We also find no evidence of labor-labor substitution in California, indicating that industrywide earnings and employment responses reflect the effects on minimum wage workers directly.

3 Data

We study the effects of six large cities' local minimum wage policies in Chicago, the District of Columbia, Oakland, San Francisco, San Jose and Seattle. Our analysis relies mainly on two data sets. The first is the Outgoing Rotation Group of the Current Population Survey (CPS ORG).[11] This survey provides information on wages and hours of a randomly surveyed cross section of households in each state every month. We use these data, for instance, to measure the demographic attributes of food service workers who are living near the six cities.

To construct a measure of each CPS ORG respondent's wage, we follow the recent literature on the effects of the minimum wage on inequality (e.g., Autor et al., 2016): For workers who report they are paid by the hour, we take their hourly wage as given. For workers who report they are not paid by the hour, we measure their hourly wage by dividing their usual weekly earnings by the number of hours they worked in the previous week in their main job.[12] We also drop workers with allocated earnings responses, self-employed workers, and workers who report either zero hours or wages. Throughout our analysis, we use CPS ORG

[11] We obtain the CPS ORG files from the Center for Economic and Policy Research website: `http://ceprdata.org/cps-uniform-data-extracts/cps-outgoing-rotation-group/`.

[12] We use hours at their main job—and not all jobs as in Autor et al. (2016)—since we are interested in measuring the responses in food services specifically and hours at all jobs may include hours with employers in different industries.

weights when estimating averages or regression models.

A limitation of the CPS ORG data is that it does not provide geographic identifiers for most cities, only for larger metropolitan areas and counties. Even for these areas, the survey contains typically only a few hundred observations each year. As a result, it cannot be used to evaluate the effects of local minimum wages. For our analysis of the six citywide policies, we therefore turn to the Bureau of Labor Statistics' Quarterly Census of Employment and Wages (QCEW) administrative data. The QCEW publishes a quarterly count of employment and earnings 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.

We obtain QCEW data for all U.S. counties from the Bureau of Labor Statistics.[13] 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 obtain city-level QCEW tabulations from city or state agencies.

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 like the CPS ORG. 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. Fluctuations can also occur from changes in responses rates across establishments as well as 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]

[13]The QCEW data are available at `https://www.bls.gov/cew/datatoc.htm`.

[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

For each locality and industry, the QCEW reports quarterly employment levels based on counts of all filled jobs, whether full or part-time, temporary or permanent. 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.

4 Six local minimum wage policies

4.1 Background

The six cities we study were among the earliest movers in a new wave of local minimum wages across the U.S. 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. 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 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 three 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

sector employment. We conclude it is unlikely these reporting issues are biasing our results. Results are available upon request.

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.[15] In 2010, Chicago's minimum wage increased modestly along with Illinois's 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.

Although the six citywide policies varied in the speed and magnitude of their minimum wage increases, by the end of 2016 each of the cities had raised its wage floor to levels well above the level in most states in the past few decades. As a result, the policies covered about 21 percent of the private sector workforce in each city, on average. Nevertheless, some industries were affected more than others. Table 1 shows the distribution of private sector employment near the six cities before the local minimum wages went into effect, by industry. We tabulate the distribution for two groups, depending on whether or not the worker reports a wage above the level of the citywide minimum at the end of 2016.

Since the QCEW data do not provide direct information on hourly wages, we estimate the shares reported Table 1 using the CPS ORG, selecting workers living near their cities using the county and metropolitan area responses in the survey.[16] To ensure we have a large enough sample for each city, we pool the final four years before the citywide policy went into effect.

[15] Each of these three cities' policies allowed for subminimum wages, for example for tipped workers and for workers in small firms. Following the literature, we generally abstract from these subminimum wages in what follows.

[16] We select workers living in Chicago, San Jose and Seattle based on whether they report living in their respective metropolitan areas in the city's state. For Oakland and San Francisco, which are part of the same metropolitan area, we partition the sample depending on whether they live in San Francisco county. We select workers living in the District of Columbia using the separate state identifier for the District.

Table 1 shows that the workers covered by the citywide policies were heavily concentrated in only a few industries: food services, retail trade, and educational and health services. Together these industries employed over 60 percent of those covered. Among these industries, food services stands out not only for the number of minimum wage workers they employed, but also for the large share of the food service workforce covered by the policies, over 75 percent.

The large number of workers in food services who previously worked below the new citywide minimum wages supports our focus on this industry. As we showed in Section 2, in order for us to use industrywide responses to determine whether the policies increased minimum wage workers' net earnings, the number of minimum wage workers in the industry must be sufficiently large; otherwise, even small industrywide disemployment effects could be consistent with highly elastic labor demand elasticities.

4.2 Analytic approach

We measure the causal influence of the six local minimum wage policies on average earnings and employment in food services by comparing the changes we observe in the six treated cities against a group of untreated counties in metropolitan areas across the U.S using two complementary methods, event study and synthetic control. Each assumes that the comparison counties would have trended in parallel with the six cities in the absence of the policies once we account for certain differences between the two groups.

The event study model accounts for the differences between the six cities and the comparison counties' food services industries by controlling for observable changes in other parts of the local economies. It then measures the industrywide elasticities by exploiting variation over time in the average level of the minimum wage induced by the phase in of the local policies. A virtue of this approach is that we can assess its identifying assumptions by testing for any influence of the policies before they went into effect. Nevertheless, when we find significant pre-trends, identification then relies on parametric trend controls, which assume that the rate of divergence between the treated and untreated groups is constant over time.

We therefore also estimate the effects of the minimum wage policies using synthetic control, which has been shown to be robust to unobserved time-varying heterogeneity to the extent that these differences manifest themselves in the outcomes during the pre-policy period. The synthetic control method also yields city-specific estimates of the policies that we leverage to measure the industrywide elasticities using variation in the minimum wage policies between the treated cities. Yet, the synthetic control estimator itself can be biased if the trajectory of earnings and employment in the cities in the absence of the policies cannot be represented as a weighted average of the untreated comparison counties.

In light of the tradeoffs between the two approaches, we present results from both analyses without favoring one over the other. In fact, we will find that they yield similar estimates of the industrywide earnings and employment elasticities, suggesting that our findings are robust to the different assumptions that underlie these methods.

4.3 Comparison counties

To construct our comparison group, 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. Our sample of comparison counties include those that are in states that either did not increase their minimum wage between 2009q4 and 2016q4, or—like San Francisco and Seattle—index their minimum to inflation each year but otherwise had no other changes.

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 2009.[17] By restricting our comparison group to only counties

[17] 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/`

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.

Figure 3 maps the resulting 159 comparison counties across the United States. The 99 counties in our sample that did not experience any increases in their state minimum wage are located throughout the South as well as parts of the Midwest and Northeast. The other 60 counties that are in states that index their minimum wage to inflation each year are located primarily in Florida, Ohio and Washington, and also include parts of Arizona, Colorado and Missouri.

Table 2 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 populations and employment, pay lower wages, and experienced slower growth in the aftermath of the Great Recession. The third and fourth rows in Panel A report that average earnings across all sectors in the six cities were (1385.4/860.9 =) 1.6 times higher than the comparison counties and employed over twice as many workers, on average. Average earnings and employment of food services exhibit similar differences between the cities and the comparison counties. These differences in compensation reflect differences in previous minimum wage policies, as well as living costs and other underlying economic conditions.

Table 2 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 Section 7.)

The differences we find between the six cities and their comparison counties suggest that

`cbsa-msa-fips-ssa-county-crosswalk.html` (last accessed January 24, 2018).

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 2 reports earnings and employment changes from 2009 through 2012 for both sets of areas. Food services employment grew about ($\frac{11.7}{7.0} - 1 \times 100 \approx$) 70 percent faster in the six cities than in the comparison counties but experienced similar growth in average earnings during this period. Table 2 also shows that the overall private sector employment grew about 37 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 Sections 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.

5 Event study analysis

5.1 Methods

We first measure the effect of local minimum wage policies using an event study framework. An event study generalizes the difference-in-differences approach by measuring the effect of the policies both before and after they were implemented. Estimates of the influence of the policies before they went into effect can be used to test the models' identifying assumptions. In this analysis, we leverage two sources of variation over time to estimate the earnings and employment responses to the policies: (1) The initial *jump* in the minimum wages when the policies first went into effect, and (2) the subsequent *phase in*.

We begin with a fully nonparametric model that measures the effect of each city's policy separately at each quarter around the time of implementation. 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, our study period. We also introduce notation to mark the beginning of the new minimum wage policy for each city, t_i^0. Quarter t_i^0 is when the city's minimum wage started to increase. For example, for Seattle, t_i^0 is 2015q2, when the minimum wage increased from \$9.47 to \$11; for the District of Columbia, t_i^0 is 2014q3, when the minimum wage increased from \$8.25 to \$9.50.

To estimate the nonparametric event study models, we fit regression models that take the form:

$$Y_{it} = \sum_{s=-13}^{6} 1\left(t - t_i^0 = s\right)\gamma_{is} + X_{it}'\beta_t + \mu_i + \delta_t + u_{it} \tag{5}$$

where Y_{it} is our outcome of interest (e.g., log food service average earnings or employment) for locality i in quarter t, μ_i is a locality effect, and δ_t is a quarterly time effect. The locality effect μ_i controls for factors, such as geography or local labor market institutions, that influence the overall level of the outcome in the locality and do not vary over our period of study. The time effect δ_t captures factors, such as the business cycle or inflation, that influence the level of the outcome in quarter t and are common to all cities and counties in our sample. The variable X_{it} is a vector of control variables. We allow the influence of these control variables to vary over time by interacting them with time effects (indicated by the t subscript on the coefficient, β_t). We discuss these control variables in more detail below. The event time dummy variables $1(t - t_i^0 = s)$ indicate city i's position at quarter t relative to the quarter when the policy went into effect. We set these dummy variables to zero for the untreated comparison counties.

The event study coefficient γ_{is} in Equation (5) measures the difference between the observed outcome Y_{it} and a regression-based counterfactual that is constructed from the combined influence of the control variables as well as the locality and time effects: $\gamma_{is} = Y_{it} - X_{it}'\beta_t - \mu_i - \delta_t$. This counterfactual represents what we would expect to observe in city i but for the new minimum wage policy.

The full set of event study coefficients are not identified, because the model includes separate locality effects for each city. Following convention, we normalize the event study coefficients so that the difference between the observed outcome and its counterfactual equals zero one quarter before the policy went into effect. To do this, we exclude the event time dummy $1\left(t - t_i^0 = -1\right)$ from the model. For example, for an outcome measured in logs, if we estimate γ_{is} to be 0.05 it means the outcome grew about 5 percent in city i, s quarters after the start of the citywide policy, relative to what we would expect based on the regression model.

Each estimated event time coefficient γ_{is} is based only on a single city-quarter observation in the sample: This estimate equals what would otherwise be the regression residual for the city-quarter observation if the set of city-specific event time dummies were excluded from the model. As a result, even when the regression recovers unbiased estimates of the causal effects, the estimates will be noisy. We therefore consider alternative parameterizations of Equation (5) to measure the overall effect of the policies that average over the changes in the cities during our period of study.

Based on the policies' phase in schedules, we adopt a simple trend break model for estimating the average effects of the minimum wage policies.[18] As motivation, Figure 4 plots the event study coefficients from estimating Equation (5) on the six cities' minimum wage policies, measured in logs. The horizontal axis counts the quarters since the cities implemented their policies, $t - t_i^0$—which we call "event time." The vertical dotted line at zero therefore marks the beginning of the citywide policies, when $t - t_i^0 = 0$.

Each circle in Figure 4 plots an estimate of the event time coefficient γ_{is}. In this model, the event time coefficients measure the difference between the city's log minimum wage and the regression-based counterfactual, relative to one quarter before the citywide policies went into effect. Since 60 of the comparison counties, and two of the treated cities, have minimum wages that are indexed to inflation, the counterfactual minimum wage rises modestly over time. As a result, the event time coefficients to the left of the zero line generally lie either

[18] Recent studies to use trend break models to measure the effects of either policies or other events over time include Deryugina (2017) and Lafortune et al. (2018).

below or above zero, depending on whether or not the city's minimum wage was also indexed to inflation, respectively. Over time, the event time coefficients trend toward zero as they reach their level at the end of the pre-policy period.

When the policies go into effect, Figure 4 depicts a jump in each of the cities' minimum wages, ranging from 7 percent in San Francisco to 19 percent in Chicago. As the minimum wages phase in, the increases continue. The figure shows that six quarters after the start of the policies, at $t - t_i^0 = 6$, the cities' minimum wages range from 16 to 44 percent above their pre-policy level.[19]

The pattern depicted in Figure 4 suggests we can capture the growing influence of the citywide policies as the minimum wages phase in with a regression model of the form:

$$Y_{it} = 1\left(t \geq t_i^0\right)\theta^{jump} + 1\left(t \geq t_i^0\right)\left(t - t_i^0\right)\theta^{phasein} + X_{it}'\beta_t + \mu_i + \delta_t + u_{it} \tag{6}$$

Equation (6) is a difference-in-differences model with an additional parameter to capture the influence of the phase ins. The coefficient θ^{jump} measures the average shift in the outcome at event time zero. The coefficient $\theta^{phasein}$ measures a slope of a linear trend that runs through the subsequent quarters. A positive slope indicates that the influence of the policies increases over time.

Assuming that we can model the dynamic effects of the policies with this parameterization, estimates of θ^{jump} and $\theta^{phasein}$ are unbiased if—after adjusting for the control variables—the six cities on average would have trended in parallel with the untreated comparison counties but for the policies.

We can relax the parallel trends assumption if we assume that the treated and untreated groups diverge at a constant rate during the study period. To do so, we add a term to Equation (6) that controls for a linear trend in event time:

[19] Technically, Figure 4 shows that at $t - t_i^0 = 6$ the minimum wages range from 16 to 44 log points—not percent—above their pre-policy level. Although differences between variables in logs are approximately equal to the percent difference, this relationship does not hold when the difference in logs is sufficiently large. Going forward, we refer to effects measured in log point units as percent differences, for simplicity.

$$
\begin{aligned}
Y_{it} = \left(t - t_i^0 + 1\right)\theta^{pretrend} + 1\left(t \geq t_i^0\right)\theta^{jump} + 1\left(t \geq t_i^0\right)\left(t - t_i^0\right)\theta^{phasein} \\
+ X_{it}'\beta_t + \mu_i + \delta_t + u_{it}
\end{aligned}
\tag{7}
$$

In Equation (7), $\theta^{pretrend}$ measures the slope of a linear trend that runs throughout the study period.[20] Since the model also includes a break in this trend at event time zero—captured by the parameter $\theta^{phasein}$—the regression estimates $\theta^{pretrend}$ using only variation during the pre-policy period. The model then estimates the causal effects of the policy using variation after the policies went into effect that cannot be explained by this linear trend.

The trend break model also admits a test of the parallel trends assumption. If a t-test finds that $\theta^{pretrend}$ is nonzero then we conclude that the six cities and the comparison counties do not trend in parallel during the pre-policy period, and models that do not control for the pre-trend directly are biased. For example, a positive slope indicates that the six cities on average grow at a faster rate than the comparison counties. Without further adjustment, the model will understate any true losses in the outcome attributable to the minimum wage.

Even after we control for the linear trend term $(t - t_i^0 + 1)$, estimates of the causal effects of the policies will be biased if the six cities diverge from the comparison counties at a rate that changes during the study period. Although it is impossible to test whether the rate of divergence is constant, we can assess the likelihood of this assumption by comparing the estimated pre-trend against the city-level event time coefficients that we find when we estimate Equation (5).

We plot the estimates of trend break model in Equation (7) for the cities' log minimum wages in Figure 4. As expected, the pre-trend, jump, and phase in that we estimate closely

[20]The model identifies the coefficient $\theta^{pretrend}$ off of the linear trend term $\left(t - t_i^0 + 1\right)$. This variable marks the position of city i in quarter t relative to the end of the city's pre-policy period, $t - t_i^0 = -1$. By centering the linear trend term around this quarter, we constrain the pre-trend to equal zero at the end of the pre-policy period. This constraint is consistent with how we normalize the city-level event time coefficients in the nonparametric model (Equation (5)), and allows us to check the validity of this parameterization by comparing the estimated pre-trend to the event time coefficients.

follow the pattern depicted by the city-level event time coefficients. The figure shows no pre-trend on average during the pre-policy period, followed by a 13 percent jump in the minimum wage at event time zero. Afterward, the model finds that the minimum wage grows about 3 percent on average each quarter.

We expect to find a similar jump and phase in pattern to that shown in Figure 4 from regressions of earnings and employment, to the extent that the citywide policies have an effect on these outcomes. Moreover, if the influence over time can be summarized by an industrywide elasticity with respect to the minimum wage, the magnitude of the effect each quarter will be proportional to the increase in the average minimum wage at that point.

We estimate the industrywide elasticities using an instrumental variables framework that leverages the variation in the minimum wage over time recovered by the event study. The first stage is the model specified in Equation (7) in which the dependent variable is the log minimum wage in the locality (depicted in Figure 4). The second stage model is:

$$Y_{it} = \epsilon \widehat{lnMW}_{it} + \left(t - t_i^0 + 1\right) \phi^{pretrend} + X_{it}' \lambda_t + \zeta_i + \psi_t + \nu_{it} \tag{8}$$

where $\widehat{lnMW}_{it}$ is the log minimum wage predicted from the first stage model. Since Y_{it} is measured in logs, the coefficient ϵ is the industrywide elasticity of the outcome with respect to the minimum wage. Since Equation (8) includes the same set of trends, control variables, and locality and time effects as in Equation (7), the instrumental variables regression identifies the elasticity only from the changes in the minimum wage on average over time, as estimated by the trend break model.

In addition to the locality and time effects, we include two control variables in our regression models: (1) the average earnings in the total private sector, excluding food services, and (2) total employment in the total private sector, excluding food services. Each variable is measured quarterly at the locality level (either city or county) and comes from the QCEW. For simplicity, we call these variables private sector earnings and private sector employment, respectively. These variables control for changes in local labor market conditions over time.

We exclude food services, because it is the focus of our analysis. Including it in our control variables could bias our estimates toward zero by controlling for changes to the cities that would be attributable to the minimum wage.[21] Previous studies of state level minimum wage policies include similar variables (e.g., Addison et al., 2015, Allegretto et al., 2017, Meer and West, 2016). Each variable is measured in logs.[22]

As shown in Equations (5)–(8), we allow the influence of these control variables to change over our study period by interacting each one with a full set of quarterly time dummies. We focus on this particular specification, because we find it is the best predictor of the six cities' outcomes during the pre-policy period. Alternative specifications yield larger mean squared errors. Since the parameters of our trend break model are identified off of the regression-adjusted variation in the cities' outcomes, this specification yields more precise estimates of the effects of the policies and reduces the sensitivity of the trends to outliers. For the same reason, this specification also reduces the variability of the city-specific coefficients from the nonparametric model, which helps us assess the validity of the parametric assumptions underlying the trend break model when we compare the city-specific coefficients to the fitted trends. Nevertheless, we find that the industrywide earnings and employment elasticities are not sensitive to which specification we adopt, including those that do not include any control variables at all other than the locality and time effects.[23]

We perform hypothesis tests and construct confidence intervals under two alternative assumptions about how the data are correlated. We first cluster the data at the city and county levels, the level at which the policies were enacted. However, if the data between

[21] In Section C.2 we perform a falsification test in which we re-run our event study models on earnings and employment in professional services. For that analysis our control variables are total private sector earnings and employment, excluding both food services *and* professional services.

[22] We do not include population, because we find the six cities' population on average trends in parallel with the comparison counties' throughout the event study window. As a result, our results are not sensitive to whether or not we control for population. See Appendix Figure A1. In addition, we find that controlling for population generally reduces the model's explanatory power, as we discuss below.

[23] To determine which specification minimizes the city-level mean squared error, we use a cross-validation procedure: First we estimate each model on the comparison county sample only, excluding the treated cities. We then compute goodness of fit statistics on the held-back city sample over the pre-policy period. See Appendix B for a full description of this procedure, including estimates of the industrywide elasticities from alternative models.

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. With these tradeoffs in mind, we report the results both ways: clustering at either (1) the city and county or (2) the state level.[24]

Cluster-robust standard errors control for correlations in the regression error terms 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 (including the District of Columbia). We correct for the small number of treated 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 et al., 2008).[25] The 90 percent confidence intervals we report contain 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.

We fit the event study models specified in Equations (5)–(8) over quarters 2009q4 through 2016q4. Each locality in the sample is either one of the six treated cities or one of the untreated comparison counties that we described in Section 4.3. The six treated cities appear in the sample only during event time quarters -13 through 6 so that we estimate the pre-trend and phase in slope coefficients over the same period that we use to measure the nonparametric event time coefficients.[26]

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

[26]We focus on the final 13 quarters of the pre-policy period because it is the longest period before the policies went into effect that we have data on all six cities. We end our event study at quarter 6 because quarter 0 through 6 is the longest period after the policies went into effect that we have data on all the cities except for Chicago. The QCEW extract we received from the local agencies for Chicago ends in 2016q2.

5.2 Results

We now turn to the results of our event study analysis. We begin with a discussion of the estimates of the trend break model for average earnings and employment in food services. We then report the industrywide elasticities with respect to the minimum wage.

Figure 5 plots the event study results for average earnings in food services, measured in logs. As in Figure 4—which plots the results for the minimum wage—each circle depicts an estimate of a city-specific event time coefficient from the nonparametric model specified in Equation (5). The lines show the pre-trend and dynamic effects as estimated by the trend break model. The gray dashed line plots the pre-trend implied by the estimate of the slope coefficient $\theta^{pretrend}$ in Equation (7). The solid and dashed red lines depict the effects implied by the jump and phase in coefficients, θ^{jump} and $\theta^{phasein}$, respectively.

The city-specific coefficients depicted in Figure 5 show a similar jump and phase in pattern to the one we find in Figure 4, albeit these city-specific coefficients are estimated with less precision. We find a small upward trend in the cities during the quarters leading up to the citywide policies. The trend break model estimates that this pre-trend rises about 0.1 percent each quarter but is statistically insignificant. When the policies go into effect, we find earnings increase about 2 percent on average. Earnings then continue to rise about 0.6 percent each quarter over the next six quarters.

Panel A of Table 3 reports coefficients from the trend break model. Column 1 shows the results for log earnings when we do not control for a pre-trend. This model corresponds to the specification in Equation (6). Column 2 adds the pre-trend and corresponds to Equation (7). Both models measure similar jump and phase ins from the citywide policies. Tests of the joint significance of the two coefficients based on the wild bootstrap procedure—reported in the row labeled "p, total event effect = 0"—find they are significant at the 10 percent level when we cluster at the city and county level. Clustering instead at the state level yields larger p-values that are just above the 10 percent threshold. Together, these results indicate

The data for the other five cities extends through 2016q4.

that the citywide policies raised the earnings of food service workers in a pattern consistent with the phase in of the minimum wages over time.

In contrast to the results for earnings, the event study analysis of employment finds little influence of the citywide policies. Figure 6 plots the estimates from the nonparametric and trend break models. Here the upward trend during the pre-policy period is more pronounced, growing on average 0.4 percent each quarter.

The city-specific event time coefficients closely follow the pre-trend line estimated by the trend break model, suggesting that it is valid to assume that the rate of divergence between the treated cities and untreated comparison counties is constant. Under this assumption, we can measure the effect of the policies on employment after controlling for the linear trend. Using this preferred specification, we estimate a small, statistically insignificant 0.8 percent increase attributable to the policies, reported in column 4 of Table 3. Unlike the effect we find for earnings, this effect does not grow as the minimum wages phase in. These results indicate that the minimum wage had no effect on food service employment on average.[27]

We estimate the industrywide earnings and employment elasticities of the minimum wage by instrumental variables. Under this approach, the trend break model reported in Panel A of Table 3 is the "reduced form." Figure 4 depicts the "first stage," in which we estimate the jump and phase in coefficients to be 0.13 and 0.03, respectively.

If the dynamic effects of the citywide policies can be accurately represented by an industrywide elasticity with respect to the minimum wage, then we would expect the trend break coefficients in the reduced form to be proportional to their counterparts in the first stage. For earnings, this relationship approximately holds: The ratio of the reduced form and first stage jump coefficients imply an elasticity of $\left(\frac{0.02}{0.13} \approx\right) 0.17$; the ratio of the phase

[27]This conclusion is not sensitive to any outliers. Figure 6 shows that one city diverged from the others during the quarters leading up to the increases. This city, which is Oakland, then experienced a sharp 5 percent increase in employment at event time zero. For reasons we discuss in detail in Section 6.2 it is unlikely this increase is attributable to the minimum wage policy. Similar to the results reported in Table 3, when we drop Oakland from the model that controls for the pre-trend, we measure a jump of 0.3 percent and a phase in coefficient of zero. Hypothesis tests find that these coefficients are jointly insignificant regardless of how we cluster.

in coefficients, an elasticity of 0.21. In contrast, the ratios that we find when we insert the coefficients from the employment model do not match as closely. In this case, the ratio of the jump coefficients imply an elasticity of 0.06; the ratio of the phase in coefficients, an elasticity of 0.01. This mismatch is further evidence that the citywide policies had little influence on food service employment.

Panel B of Table 3 shows the industrywide earnings and employment elasticities using the instrumental variables approach. Columns 1 and 2 report earnings elasticities between 0.19 and 0.22, depending on whether or not we control for the pre-trend. These elasticities imply that a 10 percent increase in the minimum wage raises food service earnings about 2 percent, and are similar to those found in previous studies. Hypothesis tests using the wild bootstrap find that these estimates are statistically significant at conventional levels. The employment elasticities that we measure are sensitive to whether or not we control for a linear trend, consistent with the statistically significant pre-trend uncovered by the trend break model. In our preferred specification that controls for the trend, we find an elasticity of 0.04 that is statistically insignificant.

Panel B Table 3 also reports 90 percent confidence intervals that we estimate using the wild bootstrap. The confidence intervals from the earnings models cover a wide range of positive elasticities, especially when we cluster at the state level. The confidence intervals for employment are more narrow. In the specification that controls for the trend (column 4), the confidence intervals rule out elasticities lower than -0.04 when we cluster at the city and county level and elasticities lower than -0.05 when we cluster at the state level. These confidence intervals indicate that that the event study employment elasticities are precise enough to rule out all but small employment losses attributable to the citywide policies.

6 Synthetic control analysis

6.1 Methods

In this section, we use the synthetic control approach to measure the effect of the six cities' minimum wage policies (Abadie and Gardeazabal, 2003; Abadie et al., 2010). This design directly compares each treated city's earnings and employment after the citywide policies went into effect against those experienced by a "synthetic city." 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 the subsequent quarters thus reflects the effects of the policy.

Below we discuss the methods we use to estimate the synthetic controls. Using this approach, we 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. We develop a measure for the industrywide elasticities with the respect to the minimum based on this relationship. Unlike the elasticities measured using the event study framework in the previous section—based on the average variation in the minimum wage over time—this approach leverages differences in the size of the minimum wage increases between the treated cities.

Following the literature, we select the subset of untreated comparison counties over which we estimate each treated city's weights—the *donor pool*—to those that could plausibly approximate the city's low-wage labor market in the absence of the citywide policies (Abadie et al., 2015). As we discussed in Section 4.1, San Francisco's and Seattle's minimum wage was indexed to inflation before their citywide policies. Since we expect this inflation adjustment to influence the distribution of wages in low-wage industries, we restrict San Francisco's and Seattle's donor pools to counties in states that also index their minimum wage to inflation. For the other four cities, we restrict their donor pools to counties in states with no increase in the level of the minimum wage between 2009q4 and 2016q4.[28]

Table 4 summarizes how we set up our synthetic control analysis for each city. As a result of how we select our donor pools, we partition the 159 untreated comparison counties we used in our event study analysis into two groups depending on whether or not they index their minimum wage to inflation. The second row reports that San Francisco and Seattle have 60 counties in their donor pools, and the other four cities have 99.[29]

Table 4 also shows the time periods during which we measure the effects of each city's policy. Unlike our event study analysis, which measured the effects over a 20 quarter window, our synthetic control analysis is based on the entire time series. We call the quarters before the city implemented the policy the *pre-policy period*, and quarters afterward the *evaluation period*.[30]

As a summary of each city's local minimum wage policy, Table 4 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.

[28]Ideally, the untreated comparison counties in a treated city's donor pool would follow the same minimum wage that the city would have followed between 2009q4 and 2016q4 in the absence of the citywide policy. In practice, constructing such a donor pool is infeasible, because in this counterfactual, the binding minimum wage would be set by the state. In contrast, the binding minimum wage for most of the comparison counties in our sample is the federal and therefore lower. We therefore select counties for each city's donor pool that follow a similar counterfactual policy as the city (either indexing or not) and experience no change in this policy during the period of study. This selection criteria is unlikely to bias our analysis, because our outcomes of interest measure changes in food service earnings and employment relative to their pre-policy levels (as we discuss below). These outcomes are more likely to be influenced by changes in the minimum wage than by its over all level.

[29]As we discussed in Section 4.3, each untreated comparison county is in a metropolitan area with an estimated population of at least 200,000 in 2009. We have also run our synthetic control analysis on donor pools based on counties with an estimated population of at least 100,000 and at least 300,000. The results from these synthetic controls are very similar to what we report in this section. See Appendix Tables A2 and A3.

[30]The beginning of each city's pre-policy period is 2009q4, except for Chicago, which experienced an increase in its state minimum wage in the beginning of 2010. Chicago's evaluation period ends two quarters earlier than the other cities, because the QCEW extract we received for Chicago from the local agency only has data through 2016q2.

Table 4 reports that the average minimum wage increase ranges from 11.5 percent in San Francisco to 35.5 percent in Oakland.

We construct separate synthetic controls for each city and 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 subset of untreated comparison counties in city i's donor pool.

For each city i and outcome r, the synthetic control estimator finds the weights that minimize the pre-policy period mean squared prediction error (MSPE) of the outcome between the actual and synthetic city:[31]

$$\left(w^*_{i1r}, ..., w^*_{iJ(i)r}\right) \in \underset{\vec{w}_i \in \mathcal{W}}{\arg\min} \sum_{S(i) \leq t < t^0(i)} \left(Y_{irt} - \sum_{j \in J(i)} w_{ijr} Y_{jrt} \right)^2 \tag{9}$$

where $S(i)$ denotes the first quarter of city i's pre-policy period, as reported in Table 4. (For example, for Seattle, $S(i)$ is 2009q4.) Following the notation we introduced for our event study analysis, $t^0(i)$ marks the beginning of city i's evaluation period, and Y_{irt} is the outcome of interest. $\vec{w}_i$ is a vector of county weights for city i, and $\mathcal{W}$ is the set of non-negative weights that sum to one.

Formally, each synthetic control $\hat{Y}^{synth}_{irt}$ is a weighted average of counties in the city's donor pool using the weights that solve Equation (9): $\hat{Y}^{synth}_{irt} \equiv \sum_{j \in J(i)} w^*_{ijr} Y_{jrt}$. Unlike the event study-based counterfactual that we measure in Section 5, $\hat{Y}^{synth}_{irt}$ is tuned for each city to match as closely as the possible the actual city's trend during the pre-policy period. This tuning yields estimates of the effect of the city's policy that are more precise than what we find using the event study approach, which enables us to measure earnings and employment responses based on between-treated city variation in the policies.

To measure the effect of the policy for a given city $\hat{\alpha}_{ir}$, we average over the difference between the outcome's actual and synthetic values over the evaluation period:

[31] To find the county weights, we use the user-written package synth in Stata (Abadie et al., 2011).

$$\hat{\alpha}_{ir} \equiv \frac{1}{T(i) - t^0(i) + 1} \sum_{t^0(i) \leq t \leq T(i)} \left(Y_{irt} - \hat{Y}_{irt}^{synth} \right) \tag{10}$$

where $T(i)$ denotes the final quarter of city i's evaluation period, as reported in Table 4.

Under the assumption that, but for the policy, the outcome of interest can be represented by an interactive fixed effects model, the synthetic control estimator $\hat{\alpha}_{ir}$ is approximately unbiased if the number of pre-policy periods is large relative to the scale of the transitory shocks (Abadie et al., 2010).[32] Synthetic control thus relaxes the parallel trends assumption that we impose in our event study framework. However, this property of the synthetic control estimator only holds when the underlying weights generate a perfect match between the synthetic and actual city during the pre-policy period. When they do not, the estimator will be biased (Ben-Michael et al., 2018; Ferman and Pinto, 2017b; Gobillon and Magnac, 2016).

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 2, 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, Ferman and Pinto (2017b) find that this transformation improves the method's accuracy even in cases where it would not be necessary to

[32]Building on the framework we introduced in Section 5, the interactive fixed effects model is $Y_{it} = X_{it}'\beta_t + \phi_t'\eta_i + \epsilon_{it}$, where ϕ_t is a vector of factors that vary over time, and η_i is a vector of unobserved locality effects. For our event study model, we assume interactive fixed effects term is equal to a linear combination of locality and time effects: $\phi_t'\eta_i = \mu_i + \delta_t$.

construct a close match.

To measure the quality of the pre-policy match between the actual and synthetic city, we report Ferman and Pinto's (2017b) pseudo R^2 statistic. For each city i and outcome r, the pseudo R^2 is:

$$\text{Pseudo } R_{ir}^2 \equiv 1 - \frac{\sum_{S(i) \leq t < t^0(i)} \left(Y_{irt} - \hat{Y}_{irt}^{synth}\right)^2}{\sum_{S(i) \leq t < t^0(i)} \left(Y_{irt} - \bar{Y}_{irt}^{pre}\right)^2} \tag{11}$$

where $\bar{Y}_{irt}^{pre}$ is the average of the outcome during the pre-policy period. When the match is perfect, the pre-policy pseudo R^2 equals one. Imperfect matches are associated with lower values, and extremely poor matches can yield negative values.

For food service earnings and employment, we find that the synthetic controls closely match the actual observations during the pre-policy period, yielding pseudo R^2 values above 0.95 in most cases. Nevertheless, in some cities we estimate lower quality matches, especially when we analyze other sectors. Unfortunately, the synthetic control literature does not indicate what quality of match is needed for the estimator to be reliable. For this reason, we develop a test that assesses the bias in our synthetic control estimates similar in spirit to our test of the parallel trends assumption in our event study analysis. (We describe this test in Section 7).

We use placebo tests to infer the statistical significance of our estimates (Abadie et al., 2010). For a given null hypothesis about the true effect of the policy in a city—such as the policy had no effect whatsoever—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. To construct confidence intervals, we follow an extension of this procedure proposed by Firpo and Possebom (Forthcoming).[33]

[33]This placebo test is an extension of Fisher's Exact Hypothesis Test (Fisher, 1971; Rosenbaum, 2002). This method tests the sharp null hypothesis that the true effect of the citywide policy equals a specific value, for each locality and each time period. (In contrast, the inference methods for our event study analysis test a null hypothesis about the average effect of the policy.) The placebo test is only valid under the strong assumption that the choice of which locality receives a citywide minimum wage policy among our six cities and comparison counties is random (e.g., Firpo and Possebom, Forthcoming). Recent econometric

For performing hypothesis testings, we compute a test statistic constructed from the absolute value of the effect that we measure in each city. To then test the null hypothesis of no effect whatsoever 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, $\hat{\alpha}_{ijr}$, assuming the same pre-policy and evaluation periods as city i. (2) For each comparison county, compute its test statistic. (3) Compute the p-value from the number of comparison counties with a larger value of the test statistic than city i:

$$p_{ir} \equiv \frac{1 + \sum_{j \in J(i)} 1\left(|\hat{\alpha}_{ijr}| \geq |\hat{\alpha}_{ir}|\right)}{1 + |J(i)|} \tag{12}$$

To construct confidence intervals, we invert the test statistic following a procedure outlined in Firpo and Possebom (Forthcoming). The 90 percent confidence intervals we report then include each minimum wage effect for which the associated null hypothesis is not rejected by our inference procedure at the 10 percent level.[34]

We leverage the variation in the magnitude of the minimum wage increases across the cities to estimate industrywide earnings and employment elasticities with respect to the minimum wage. Under the assumption that these elasticities are similar across the cities, we expect the effect in each city to be commensurate with the average increase in the city's minimum wage. We estimate the elasticity with respect to the minimum wage as the solution to a least squares problem based on this relationship:

$$\widehat{\epsilon}_r^{synth} \equiv \arg\min_e \sum_{i \in 6 \text{ cities}} \left(\hat{\alpha}_{ir} - e \times \partial lnMW_i\right)^2 \tag{13}$$

where $\partial lnMW_i$ denotes the average minimum wage increase we observe in city i. This elasticity estimate is the slope of the line of best fit between the six cities' effects and their

studies indicate that statistical tests based on this placebo-based approach are likely to be biased in more general settings (e.g., Ferman and Pinto, 2017a; Ben-Michael et al., 2018). 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.

[34]We thank Vítor Possebom for sharing their R code for constructing confidence intervals.

average minimum wage increases.

For Chicago, the District of Columbia, Oakland and San Jose, the average minimum wage increase is the increase we report in Table 4. 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.[35]

6.2 Results

We begin by reporting results on food services earnings and employment for each city. We then use the variation in the average minimum wage increases between the cities to measure the industrywide elasticities.

Figures 7 and 8 display our synthetic control results separately for each city. The left column of each figure shows the results for food service earnings. We find that the synthetic controls closely match the actual evolution of earnings in each of the cities during the pre-policy period. After the citywide policies go into effect (marked by the vertical dotted line), actual earnings in most cities rise above their synthetic counterparts, indicating an influence of the citywide policies on earnings. We find smaller differences in Chicago and the District of Columbia. One explanation for this finding is that these two cities experienced smaller increases in the minimum wage compared to the other cities.[36]

Table 5 displays the estimated effects of the minimum wage increases in each city. We report the results for earnings in Panel A. The earnings effects range from about 1.3 percent

[35]Since we use synthetic control weights to calculate the average minimum wage 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. For example, 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 to be 24.1 percent using either the earnings or employment-based weights.

[36]An additional explanation for the smaller effects is that Chicago and the District of Columbia have a lower sub-minimum wage for servers, the "tipped wage." 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. Since restaurant owners can allocate tips toward paying for at least some of the minimum wage increase, we expect the effect of the minimum wage to be smaller in these cities. (Seattle's citywide policy also introduced a lower tipped wage, but only for small employers.)

for Chicago to about 10 percent for Oakland. The row in Table 5 labeled "Pre-policy pseudo R^2" reports Ferman and Pinto's (2017b) pseudo R^2 statistic. The pre-policy pseudo R^2 for the six cities ranges from 0.85 for Oakland to 0.99 for San Jose. These values are consistent with the close matches depicted in Figures 7 and 8.

The right column of Figures 7 and 8 shows the synthetic control results for employment. Here, we find a close match between the actual and synthetic time series that extends through the evaluation period. This pattern indicates that the policies had little influence on employment. We report the employment effects in Panel B of Table 5. These effects 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.

For each estimated effect, Table 5 also reports its statistical significance and the 90 percent confidence interval. 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 estimate are statistically significant, except for Oakland, where we observe a significant positive effect.[37]

To examine the extent to which the range of effects that we find may be attributable to differences in the cities' minimum wage increases, Figure 9 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 is commensurate with the size of that city's average minimum wage increase.

The positive relationship depicted in Figure 9 is consistent with an industrywide earnings elasticity with respect to the minimum wage that is similar to what we found in our event study analysis. To compute the 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

[37]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 Figure 7), 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.

9 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 elasticity implied by the line. Since this ratio is also the line's slope, we can use the slope to measure the elasticity. Using this approach, we find an industrywide earnings elasticity of 0.25.

Figure 10 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 industrywide employment elasticity of 0.07. This estimate is sensitive to whether or not we include the large positive employment effect we measure in Oakland. Dropping this data point, the elasticity is close to zero. We interpret Figure 10 as showing little relationship between the size of the minimum wage increases and employment changes.

Over all, the industrywide elasticities that are implied by our synthetic control estimates are very close to what we found in our event study analysis. The consistency of these findings suggests that the results are not sensitive to the different assumptions that underlie these methods.

7 Robustness and falsification tests

We next peform additional analyses that test the robustness of our findings from the six cities. We summarize these results below. Full results are reported in the appendix.

Sensitivity of event study results to model specification

First, to check the sensitivity of our event study results to model specification, we estimate the industrywide earnings and employment elasticities under alternative models, including those in which we do not include any control variables other than locality and time effects. Over all, we find little variation in the elasticities, especially once we control for the linear trend. For example, the employment elasticities estimated in models that control for the trend range from -0.02 to 0.04. The similarity of the results across models suggests our conclusions are unlikely to be biased by model misspecification.[38]

Test of parallel trends in synthetic controls

We next present a simple test 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. Our 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*. 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.

The results of this analysis are reported beneath the heading "Test of parallel trends assumption" in Table 5. 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 industrywide earnings and employment elasticities depicted in Figures 9

[38]We present these results, along with the selection criteria we use to choose our preferred specification, in Appendix B.

and 10 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 elasticities to what we find above: An earnings elasticity of 0.21 and an employment elasticity of 0.09. We conclude that it is unlikely that these pre-trends are actually biasing our results.

Full and limited service restaurants

One lesson from the framework we presented in Section 2 is that industrywide responses will be larger in sectors in which minimum wage workers make up a larger share of the workforce. To test this prediction, we re-run our analyses of food services for two sub-sectors, full and limited service restaurants. As we saw in Table 2, average earnings in limited service restaurants are lower than in full service restaurants. We should therefore find larger effects in limited than in full service restaurants.[39]

Panels A and B of Figure 11 show the results of the synthetic control for full and limited service restaurants, respectively. 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 negative employment elasticities in either sector. Event study results are similar.[40] These results support our conclusion from analyzing food services industrywide: the local minimum wage policies raised earnings in the industry without reducing employment.

Professional services (falsification exercise)

We next 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

[39]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.

[40]See Appendix C.1 for a full discussion of these results.

on professional services—a high-wage industry that should not be affected by changes in minimum wage policy.[41] 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.

The results from our synthetic control analysis are depicted in Panel C of Figure 11. Event study results are similar. Reassuringly, we do not generally find significant earnings or employment effects in professional services: Out of the 16 tests we perform (four event study models and 12 synthetic controls), we find significant effects in only one case. This analysis indicates 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.[42]

8 Interpretation under different forms of labor-labor substitution

The industrywide elasticities we measure in the six cities are in line with those reported in previous studies of the food services industry, including Dube et al. (2010, 2016), Allegretto et al. (2017) and Totty (2017). As described in the previous sections, we measure a statistically significant earnings elasticity between 0.19 and 0.25 and a positive but insignificant employment elasticity between 0.04 and 0.08.

[41] This industry is also called the Professional, Scientific, and Technical Services sector (NAICS code 54). This sector includes lawyers, accountants and management consultants, among other highly educated professions. Table 1 reports that less than 3 percent of workers in the six cities in this industry were covered by the new local minimum wage policies during our study period.

[42] See Appendix C.2 for a full discussion of these results.

In this section, we ask whether these industrywide responses reflect the effects on minimum wage workers, or if they may be attributable to labor-labor substitution. To do this, we draw on the framework we developed in Section 2. We first benchmark our estimates against the industrywide elasticities we would expect to observe in the absence of any employment effects. We then simulate the industrywide earnings and employment elasticities we would expect to observe while varying the level of substitutability between high- and low-wage workers. Throughout, we assume the minimum wage does not affect workers' hours (we verify this assumption in the next section). If the policies raised minimum wage workers' earnings, then the estimated industrywide elasticities will match the simulated elasticities only when the employment effects on minimum wage workers is smaller in magnitude than the direct effect on their wages: $\frac{\partial lnw_i}{\partial lnMW_i} + \frac{\partial lnN_i}{\partial lnMW_i} > 0$.

As we showed in Section 2, the industrywide elasticities are weighted averages over the effects of the minimum wage on different groups of workers:

$$\frac{\partial lnN}{\partial lnMW} = \sum_i \frac{N_i}{N} \frac{\partial lnN_i}{\partial lnMW_i} \tag{14}$$

$$\frac{\partial lnEarn}{\partial lnMW} = \sum_i eshr_i \left(\frac{\partial lnw_i}{\partial lnMW} + \frac{\partial lnh_i}{\partial lnMW_i} \right) + \left(eshr_i - \frac{N_i}{N} \right) \frac{\partial lnN_i}{\partial lnMW_i} \tag{15}$$

In the absence of employment or hours effects, the industrywide employment elasticity is zero, and Equation (15) implies the earnings elasticity is equal to the sum of each group's share of earnings times the effect of the policy on their wage. This sum represents the expected increase in earnings if each worker who would be paid below the new minimum received a wage increase under full compliance with the policy.

As a first step then in simulating the industrywide earnings elasticity we would observe in the absence of any employment or hours effects, Figure 12 shows the pre-policy distribution of employment and earnings in the food services industries near the six cities. To estimate the shares depicted in this figure, we select the food service workers identified in our crosstabulation of the six cities' workforce in Section 4.1 using data from the CPS ORG. We then center each worker's reported wage around their city's pre-policy minimum wage and group together workers in 50 cent increments. Finally, we estimate each wage group's share of total food service employment and total food service weekly earnings, averaging over the six cities.[43]

Figure 12 shows that before the local policies went into effect, about 26 percent of food service workers reported a wage at or just below the prevailing minimum wage. These workers accounted for about 17 percent of total industry earnings. The large minimum wage increases induced by the local policies during our study period raised the minimum wage by nearly three dollars, on average. Figure 12 shows that about 74 percent of workers earned less than three dollars above the minimum wage, a similar fraction that we estimated would be covered by the new policies by the end of our study period (reported in Table 1). These workers account for over half of the earnings in the food services industry.

To simulate the industrywide earnings elasticity in the absence of any employment or hours effects, we assume the distribution of employment and earnings depicted in Figure 12 would stay constant but for the policies. We then evaluate Equation (15) assuming a 20 percent increase in the minimum wage, an amount close to the average increase that we observe in the six cities in our event study and synthetic control analyses.[44]

In the absence of employment or hours effects, we find a 20 percent minimum wage increase would be expected to yield an industrywide earnings elasticity of 0.26, just above

[43]For workers who are not paid by the hour, weekly earnings is equal to their reported usual weekly earnings. For workers paid by the hour, weekly earnings is equal to their hourly wage times the number of hours they worked at their main job in the previous week. Our measure of weekly earnings for hourly workers thus excludes compensation from tips, which we assume are not affected by minimum wage policies. We assume hourly workers in Chicago and the District of Columbia who report wages within 50 cents of their tipped wage actually earn the hourly minimum wage. (The other four cities did not have a tipped wage before the local policies went into effect). This adjustment assumes that employers always pay tipped workers a wage equal to the minimum wage even when their earnings with tips fall short, as required by law.

[44]To determine which groups of workers are affected by the 20 percent minimum wage increase, we need to center the employment and earnings distributions depicted in Figure 12 around a minimum wage that would be enforced in the absence of the local policies. The average pre-policy minimum wage in the six cities is $8.84 (see Table 4). Since we group together workers in 50 cent wage bins, we center the distributions around $9. We find similar results in our simulations if we center the distributions around $8 or $10 instead. Results available upon request.

the range that we estimate in our event study and synthetic control analyses. Since we also find no industrywide employment response, the similarity between this value and what we estimate in the six cities suggests hours and employment effects are small and that the local policies raised minimum wage workers' earnings.

To simulate the earnings and employment elasticities under labor-labor substitution, we assume food services output industrywide is produced under a nested constant elasticity of substitution (CES) production function that depends on capital and different groups of labor. As in Figure 12, labor is grouped according to its pre-policy wage. The labor groups are then nested into a labor composite. Building on the notation we introduced in Section 2, we assume that industrywide production function takes the Cobb-Douglas form:

$$F\left(h_1 N_1, ..., h_J N_J, K\right) = Ag\left(\vec{N}\right)^{\alpha} K^{1-\alpha} \tag{16}$$

where A measures total factor productivity, $\vec{N}$ is a vector of the employment of each of the labor groups and $\alpha \in (0,1)$ is labor's share of income in the industry. The labor aggregate $g\left(\vec{N}\right)$ is defined as:

$$g\left(\vec{N}\right) = \left(\sum_i \beta_i (h_i N_i)^{\frac{\sigma-1}{\sigma}}\right)^{\frac{\sigma}{\sigma-1}} \tag{17}$$

where β_i are group-specific productivity levels. An attractive feature of the nested CES production function specified in Equations (16) and (17) is that the elasticity of substitution between any two labor groups is equal to σ.

The labor aggregate specified in Equation (17) allows for multiple wage-based groups. In contrast, the framework we presented in Section 2 assumed there were only two labor groups, high and low. While useful for expositional purposes, the two group model would be unable to reproduce the distributions depicted in Figure 12, in which most food service workers earn within a few dollars of the minimum wage, but nearly 10 percent earn wages more than 10 dollars above the prevailing minimum. Nested CES production functions similar to the above are widely used in the labor and macro literatures on wage inequality (e.g., Bound and Johnson, 1992; Katz and Murphy, 1992; Krusell et al., 2000; Card and Lemieux, 2001; Borjas, 2003; Ottaviano and Peri, 2012).

To assess how labor-labor substitution affects the relationship between the industrywide elasticities and the effects on minimum wage workers, we simulate the effects of the 20 percent minimum wage increase using the model above under different values for σ. To perform this simulation, we assume there are no scale effects and hold capital fixed, so that all between-factor substitution occurs through labor. As we discussed in Section 2, the total employment effect of the policy on any group of workers is a combination of output-constant substitution effects and a scale effect. Since labor-capital substitution and scale effects imply larger industrywide employment effects for any given output-constant effects on minimum wage workers, the industrywide elasticities that we simulate are an upper bound for what we would expect to observe in the presence of these additional channels of adjustment.

Under these assumptions, an increase in the minimum wage to m implies a group-specific elasticity of employment with respect to the minimum wage of:

$$\frac{\partial lnN_i}{\partial lnMW} = \begin{cases} \underbrace{\sigma \frac{\partial lnw_i}{\partial lnMW}(eshr_i - 1)}_{\text{own-wage effect}} + \underbrace{\sigma \sum_{\substack{k \neq i \\ w_k < m}} \frac{\partial lnw_k}{\partial lnMW} eshr_k}_{\text{cross-wage effect}} & \text{if } w_i < m \\ \sigma \sum_{w_k < m} \frac{\partial lnw_k}{\partial lnMW} eshr_k & \text{otherwise} \end{cases} \tag{18}$$

Equation (18) shows that the group-specific employment elasticity under labor-labor substitution is a combination of an output-constant own-wage effect and cross-wage effects. In the absence of labor-capital substitution or scale effects, the negative employment effects of the minimum wage are attributable solely to labor-labor substitution. As a result, there is a direct relationship between the elasticity of substitution and the employment elasticity.

One can show that Equation (18) implies that the employment effects of the minimum wage are weakly increasing in the group's wage, w_i. Therefore, the largest employment reductions are borne on the lowest-wage workers who would otherwise earn the original minimum wage. Since these workers also receive the full effect of the increase on their wages ($\frac{\partial lnw_i}{\partial lnMW} = 1$), if their group-specific employment elasticity is smaller in magnitude than 1, then the increase raises their earnings and the earnings of *all* groups whose wages are directly affected by the increase. For this reason, we focus on the employment elasticity of this group in our presentation of the simulation results below.

Equation (18) implies that the industrywide employment elasticity is:

$$\frac{\partial lnN}{\partial lnMW} = \sigma \sum_{w_i<m} \frac{\partial lnw_i}{\partial lnMW} \left(eshr_i - \frac{N_i}{N} \right) \tag{19}$$

Therefore, to simulate the relationship between the group-specific employment effects of the minimum wage increase and the industrywide elasticities, we plug into Equations (18) and (19) the shares depicted in Figure 12 for the groups whose wages are directly affected by the increase.

Table 6 shows the results of this calibration exercise. Each row reports the employment effects we would expect to observe for a given value of the elasticity of substitution, σ. The top row shows what we would expect to find if there was no labor-labor substitution, which corresponds to the case in which the increase has no employment effects, discussed above. Column 2 reports the employment elasticity with respect to the minimum wage for the lowest-wage workers in the industry, a lower bound for the group-specific elasticity of any other group. Columns 3 and 4 report the industrywide earnings and employment elasticities, respectively.

As expected, Table 6 shows that models that allow for more substitution between labor yield larger, more negative group-specific and industrywide employment elasticities. Since minimum wage workers are only a subset of the total food service workforce and some of the employment losses among low-wage workers are offset by increases among higher-wage workers, the industrywide employment elasticity is only about a fifth of the size of the employment elasticity among the lowest-wage workers.

Nevertheless, Table 6 indicates that the industrywide earnings and employment elasticities that we find in the six cities are inconsistent with large negative employment effects on minimum wage workers under labor-labor substitution. Recall that the 90 percent confidence intervals in our event study analysis rule out industrywide employment elasticities lower than -0.05 (Table 3). Comparing the lower bound from this confidence interval to the simulated elasticities in Table 6, we find this industrywide elasticity implies an employment elasticity among the lowest-wage workers of -0.25. Since this elasticity is smaller than 1 in magnitude, our industrywide results imply that the local policies raised the earnings of all groups whose wages are affected by the policies net of any reductions in their employment.

Equation (19) also implies we can use the industrywide employment elasticity to calibrate the elasticity of substitution. Comparing our industrywide elasticities to those in Table 6, we find they imply an elasticity of substitution no greater than 0.3. These results indicate the minimum wage has little influence on the demand for higher-wage workers.

In summary, the simulations support the conclusions of the analysis we presented in Section 2. The combination of average earnings gains and constant employment that we find in the cities industrywide cannot be explained large employment losses on minimum wage workers offset by gains among higher-wage workers. In fact, our industrywide elasticities suggest cross-wage effects are small and that the local policies had little influence on labor-labor substitution. Nevertheless, to analyze the effect of the local policies under labor-labor substitution, we assumed they had no effect on minimum wage workers' hours. As we noted in Section 2, if there are hours effects, it is possible the total number of higher-wage workers hired after the increase could fully offset the employment losses among low-wage workers. In the next section, we assess the empirical support for labor-labor substitution and hours effects by examining the effects of California's recent state and local policies on its food services industry.

9 Evidence on labor-labor substitution and hours responses in food services

To measure the effect of the minimum wage on labor-labor substitution and hours, we exploit California's recent state and local minimum wage policies. We begin by reviewing California's recent minimum wage history as it relates to the distribution of wages in the food services industry. We then estimate the combined influence of these policies on the qualifications and hours worked among low-wage workers. Panel A of Figure 13 shows that in 2010 about 30 percent food service workers earned the state minimum wage, \$8. To create this plot, we bin wages in 50 cent intervals, grouping together those below one dollar or above \$20. We then estimate the fraction of workers employed in food services in each bin using data from the CPS ORG.

For comparison, the figure also shows the distribution in the 14 states where the minimum wage was set at the federal level during this period.[45] We find a similar number reporting at or just below the binding minimum wage in these states, \$7.25. Another 12 percent report a wage between \$2 and \$2.50. This group is reporting the "tipped wage" of \$2.13 that restaurants can choose to pay their tipped workers as long as the workers' hourly earnings are higher than the minimum wage once tips are included. In contrast, the minimum in California applies to all workers regardless of tips. Despite these differences, in both California and the other states, about 75 percent workers report a wage no more than two dollars above the binding wage floor. Only a small minority report earning \$20 or higher.

Panel B of Figure 13 shows the remarkable influence California's state and local policies have had on the distribution of the wages in this industry. Between 2010 and 2016, the statewide minimum wage increased to \$10—first to \$9 in July of 2014, then to \$10 in the beginning of 2016. In 2017, California then introduced a modestly higher \$10.50 minimum

[45] The 14 states are: Alabama, Georgia, Indiana, Kansas, Kentucky, Louisiana, Mississippi, North Carolina, South Carolina, Tennessee, Texas, Utah, Virginia and Wyoming. Each year, the CPS ORG contains about 500 responses that meet our sample restrictions from food service workers living in California and 1,700 responses from those in these other states.

wage for firms with more than 25 employees. During this period, 21 cities also began enforcing their own minimum wage policies. Similar to the state, many of these policies set higher minimums for firms with more than 25 employees. Depending on which local minimum we apply, we estimate that the share of food service workers employed in cities with a minimum at \$12 or above reached 10 to 24 percent in 2017.[46] Comparing Panels A and B, we find that the share of workers earning between \$10 and \$15 in California increased over 180 percent during this period but only 61 percent in the states at the federal minimum wage.

Although we find a large increase in the share of workers earning between \$10 and \$15 from 2010 to 2017, we find little evidence that the policies influenced the share earning amounts above \$15. When we compare Panels A and B of 13, we find this share increased about 50 percent in both California and the other states. The similar growth between these groups during this period indicates that the policies did not incentivize the industry to substitute away from minimum wage workers and hire more workers at these higher wages.

Over all, the large rightward shift in the wage distribution in California compared to other states suggests that, as a result of the higher state and local minimum wages, many food service workers who previously would have earned a wage at or near \$8 in 2010, received a higher wage in 2017. On the other hand, since these distributions are estimated on samples drawn from cross sections of the labor force each year, it is also possible that the industry in 2017 hired a different group of workers than it did in 2010—workers that would earn a higher wage than \$8 regardless of the minimum wage policy.

To distinguish between these two explanations, we test whether the 2017 food service workers in California have attributes that would earn them a higher wage in the labor market

[46] To estimate the number of food service employees affected by these local minimum wage policies, we draw on data on city-level food service employment in the 2012 Economic Census, Table EC1272A1: Accommodation and Food Services: Geographic Area Series: Summary Statistics for the U.S., States, Metro Areas, Counties, and Places 2012. We assume a city's share of food service employment does not change between 2012 and 2016. When counting the total share of the workforce above \$12, we weight each city depending on the number of months during the year that have a minimum wage above \$12. For example, a city that raises their minimum wage to \$12 in July would have a weight of 0.5 that year. We acquire a list of California's local minimum wage policies from the UC Berkeley Labor Center's "Inventory of U.S. City and County Minimum Wage Ordinances": `http://laborcenter.berkeley.edu/minimum-wage-living-wage-resources/inventory-of-us-city-and-county-minimum-wage-ordinances/` (last accessed October 2018).

than those in 2010. To do so, we measure the wage each worker would be expected to earn based on their observable characteristics. If food services respond to the higher minimum wage policies by substituting towards workers with higher qualifications, then this expected wage will increase over time as the new minimum wages go into effect. Alternatively, if food services do not adjust its hiring practices, then this expected wage will not change.

We measure each food server's expected wage using a Mincer regression of log hourly wages on interactions of educational attainment and potential labor market experience, separately for men and women. We also include in the model other demographic attributes such as race, Hispanic ethnicity and whether the respondent was born in a foreign country. We estimate the model on the 2010 CPS ORG data including all 50 states and the District of Columbia.[47]

Figure 14 compares the evolution of actual wages to that predicted by the Mincer regression. We focus on workers earning at or below $16 because we are interested in whether the industry changed how it hired workers close to the minimum wage. Moreover, we saw little change in the share earning above $16 in California between 2010 and 2017 compared to the states at the federal minimum. (We confirm this difference between California and the other states is not statistically significant below).[48]

Figure 14 shows that, on average, actual wages in California were about 12 percent higher than in other states in 2010 (the solid black and red lines, respectively), consistent with the

[47] More specifically, we include four dummies for educational attainment (high school dropout, high school, some college and bachelors or higher). We omit the high school dummy and interact the other three with: race and Hispanic ethnicity dummies, a quartic in potential experience, marital status, a dummy for citizenship and a dummy for foreign born. We also include dummy variables for ages 16, 17 and 18, each interacted with the race and Hispanic ethnicity dummies. Potential experience equals age minus 18 for those with a high school degree or less, age minus 20 for those with some college and age minus 22 for those with a bachelors or higher. We also include state effects. We windsorize wages at $5.77 and $75 (the 1 percent and 99 percentiles of the distribution nationwide in 2010). We drop from the regression sample workers who are predominantly paid in tips since they may report hourly wages at the tipped wage. When estimated, the regressions yield an R^2 of 45 percent for men and 40 percent for women.

[48] We report summary statistics for this sample in Appendix Table A1. When measuring average wages in states at the federal minimum wage, we replace wages reported near the tipped wage with the federal minimum wage. This adjustment assumes that employers always pay tipped workers a wage equal to the minimum wage, even when their earnings with tips fall short, as required by law. To do this, we replace wages reported by hourly workers between $1 and $3.50 to $7.25.

higher minimum in California. Despite this difference in levels, before California increased its minimum wage, wages in California trended nearly in parallel with the other states. California wages departed from this trend starting in 2014, increasing each year with the rising state minimum wage and growing number of local policies.

We find a similar difference in 2010 between the dashed lines that depict the predicted wages from the Mincer regression for California and the other states. However, unlike actual wages, predicted wages were stable over the sample period. This pattern suggests the increases in actual earnings were attributable to minimum wage workers earning higher wages, not changes in the composition of who the industry employed.[49]

The similarity in California's and other states' wage growth between 2010 and 2013 (both actual and predicted) suggests we can measure the causal effects of the state's minimum wage policies with a difference-in-differences approach, in which we compare the wage growth between 2014 and 2017 to a counterfactual based on the changes we observe in the other states. To measure the effects, we fit regression models of the form:

$$Y_{ist} = \sum_{\tau=2011}^{2017} 1\left(i \in CA\right)_{ist} \pi_{\tau} + \kappa_s + \varphi_t + \omega_{ist} \tag{20}$$

where Y_{ist} is the outcome of interest (e.g., actual or predicted log hourly wages) of respondent i living in state s in year t. The coefficients κ_s and φ_t are state and year effects, respectively. The variable $1\left(i \in CA\right)_{ist}$ is a dummy variable for whether the respondent lives in California, which we interact with each calendar year.

In this model, the coefficient of interest is π_τ, which measures the difference between California and what we observe in the states at the federal minimum wage in year τ. We omit the California dummy variable for year 2010 so that the coefficient is interpretable

[49]One noticeable pattern in Figure 14 is that predicted wages were about 30 percent higher than actual wages on average in both California and the other states. This gap is attributable to workers in food services that, according to the Mincer regression, would be expected to earn a higher wage if employed in another industry. This gap is consistent with previous findings on inter-industry wage differentials (e.g., Krueger and Summers, 1988) as well as more recent evidence on between-firm wage differences (e.g., Abowd et al., 1999; Card et al., 2013; Song et al., Forthcoming).

as this difference relative to this year. This coefficient coincides with the causal effect of California's minimum wage policies if California and the other states would have trended in parallel in the absence of the policies. To assess whether this assumption holds, we test for effects between 2011 and 2013, before California raised its minimum wage to $9 and the rise of the new local policies.[50]

Table 7 shows the effects of California's state and local minimum wage policies using this approach. Columns 1 through 3 summarize the changes in the state and local minimum wage policies between 2010 and 2017. Column 4 reports the results from a regression of whether the worker earns under $16. This model tests whether the policies incentivized the food services industry to replace low-wage workers with workers who earn above $16. Consistent with evidence depicted in Figures 13, we find that California's employment of worker's earning less than $16 did not change significantly between 2010 and 2017.

We report the effects on the actual wages of low-wage workers earning below $16 in column 5 of Table 7. We find that leading up to the minimum wage increases, wages in California for this group fell modestly relative to the other states. The p-value reported in the bottom row tests the hypothesis that the changes estimated at years 2011, 2012 and 2013 are all zero. This test finds that these pre-increase changes are not statistically significant, indicating that California and the other states trended together before the increase to $9 in 2014. As we saw in Figure 13, wages in California then rose each year beginning in 2014. We find that by 2017 that minimum wage policy increased wages by 12 percent and that this increase is statistically significant.

To test whether this wage increase reflects raises for low-wage workers, and not changes in the qualifications of the workers themselves, we re-run the model, replacing the actual log wage with that predicted by the Mincer regression. Column 6 of Table 7 reports the results. Here we find a statistical significant upward trend in the lead up to the minimum

[50]As we discussed in Section 4.1, before 2013, the only citywide minimum wage in California was in San Francisco, which we estimate covered about 5 percent of food service workers. Although San Jose put into place its citywide policy in 2013, we estimate this policy covered only another 2.5 percent, and therefore would be unlikely to have much influence on the statewide wage distribution.

wage increase. Although actual wages changed little during this period, predicted wages rose over 5 percent. This trend then reverses, declining each year with the increase in the minimum wage. By 2017, predicted wages were only about 1 percent above their 2010 level. This difference is not statistically different from zero.

Taken together, the pattern of rising actual wages and, if anything, falling predicted wages indicates that the state and local minimum wage increases between 2014 and 2017 did not incentivize food services to substitute away from low-wage workers towards those with higher qualifications. As a robustness check, we test whether the minimum wage increases coincided with any changes in the attributes of food service workers earning below $16. We find no changes in educational attainment or potential experience. We also find no declines in the employment shares of demographic groups associated with lower wages, including teenagers, women, African-Americans or Hispanics. Although we do find a large reduction in the fraction born in other countries, this change is first detected in 2013, one year before California's minimum wage increase, and is then stable through 2017 despite the growing influence of the minimum wage. We thus attribute this change to pre-increase changes in migration patterns and not the policies. Over all, we do not find any evidence for labor-labor substitution in food services.[51]

Finally, we test for whether the higher minimum wages triggered reductions in the hours worked in the previous week by workers earning less than $16. We report these results in column 7. We find no statistically significant reductions in hours between 2010 and 2017. Although we detect a marginally significant increase of 7 percent in 2014 (the first year the minimum wage increased), the fluctuating pattern year to year over all indicates that the policies did not incentivize firms to reduce hours.[52]

[51] We report these additional results in Appendix Table A4. In addition to the change in the fraction born in other countries, we also detect transitory declines in the share of food service workers who are teenagers and African-Americans. These declines occur in the middle of the study period (2014 for teenagers, 2016 for African-Americans) and then increase in 2017 back to a value close to their 2010 level and are no longer statistically significant. Since these fluctuations do not match the growing influence of the policies on actual wages during this period, we attribute these short-lived reductions to sampling error.

[52] We have also tested for any changes in part-time workers, measured by whether the worker reports fewer than 25 hours worked in the previous week. We find not effects on this measure as well. Results are available

In summary, we find no evidence that higher state and local minimum wages influence the food services industry to substitute away from minimum wage workers or reduce their hours. Combined with the evidence from our calibration exercise in the previous section, these results indicate that the most likely explanation for the industrywide responses we estimate in the six cities is that the local policies have raised the earnings of minimum wage workers.

10 Conclusion

Many studies on the labor market effects of the minimum wage have focused on employment in industries in which a large fraction of workers are paid at or just above the minimum. New evidence from Seattle's recent citywide policy suggests that these designs may be unreliable (Jardim et al., 2017). Intuitively, since the evidence in industry-level studies is based on changes in aggregate employment, the small effects that they find may be explained by production processes in which firms respond to increases in the minimum wage by substituting minimum wage workers for more productive, higher-wage workers (i.e., labor-labor substitution) (Neumark, 2018). Relatedly, if employers respond to wage increases by reducing hours then evidence on employment alone is not sufficient to summarize the total effect.

In this paper, we address this key issue for the interpretation of industry-level employment effects. Using a theoretical framework based on a standard model of labor demand, we show that labor-labor substitution alone is generally insufficient to explain the combination of average earnings gains and constant employment often found in industry-level studies (e.g., Dube et al., 2010; Dube et al., 2016; Totty, 2017). Intuitively, when employers substitute more productive higher-wage workers for lower-wage workers, they necessarily reduce total employment demand.

We use this framework to help us interpret the effects of six local minimum wage policies that we estimate in Chicago, the District of Columbia, Oakland, San Francisco, San Jose

upon request.

and Seattle, in which the new minimum wages ranged from \$10 to \$13. We focus on the food services industry. To measure the effects, we employ event study and synthetic control methods using aggregate data on average earnings and employment. Similar to previous studies of state and federal policies, we find significantly positive earnings increases and no significant employment losses. These estimates are robust to a battery of specification and falsification tests.

Using a calibration exercise that draws on our theoretical framework, we find that labor-labor substitution is unable to produce the small industrywide employment effects contained in our confidence intervals if there are no effects on hours. To test whether there is any empirical support for either labor-labor substitution or hours responses in the food services industry, we examine the effects of California's recent state and local minimum wage policies. There we find no evidence of labor-labor substitution or hours effects. The most likely explanation for the effects we estimate is that the six cities' local policies raised the average earnings of low-wage workers in this industry.

Numerous cities across the U.S. are in the process of raising local minimum wages, some to as high as \$15 per hour. Yet, for most of these localities, there are no publicly available data sources with direct information on wages, hours and employment. As a result, to evaluate them, analysts have to rely on data on average earnings and employment aggregated at the industry-level. This paper provides a new approach for assessing whether the evidence produced by these studies can be explained by different forms of labor-labor substitution. Our analysis of California's state and local minimum wage policies suggests that this evidence is likely to be sufficient to bound the total effect of the policies on low-wage workers, especially in food services.

References

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

_ , _ , and _ , "SYNTH: Stata Module to Implement Synthetic Control Methods for Comparative Case Studies," Statistical Software Components, Boston College Department of Economics October 2011.

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

_ and Javier Gardeazabal, "The Economic Costs of Conflict: A Case Study of the Basque Country," *American Economic Review*, 2003, *93* (1), 113–132.

_ , Susan Athey, Guido W. Imbens, and Jeffrey Wooldridge, "When Should You Adjust Standard Errors for Clustering?," Working Paper 24003, National Bureau of Economic Research November 2017.

Abowd, John M., Francis Kramarz, and David N. Margolis, "High Wage Workers and High Wage Firms," *Econometrica*, 1999, *67* (2), 251–333.

Addison, John T., McKinley L. Blackburn, and Chad D. Cotti, "The Effect of Minimum Wages on Labour Market Outcomes: County-Level Estimates from the Restaurant-and-Bar Sector," *British Journal of Industrial Relations*, 2012, *50* (3), 412–435.

_ , _ , and _ , "On the Robustness of Minimum Wage Effects: Geographically-Disparate Trends and Job Growth Equations," *IZA Journal of Labor Economics*, Dec 2015, *4* (24).

Allegretto, Sylvia, Arindrajit Dube, Michael Reich, and Ben Zipperer, "Credible Research Designs for Minimum Wage Studies: A Response to Neumark, Salas, and Wascher," *ILR Review*, 2017, *70* (3), 559–592.

Autor, David H., Alan Manning, and Christopher L. Smith, "The Contribution of the Minimum Wage to US Wage Inequality Over Three Decades: A Reassessment," *American Economic Journal: Applied Economics*, January 2016, *8* (1), 58–99.

Ben-Michael, Eli, Avi Feller, and Jesse Rothstein, "New Perspectives on the Synthetic Control Method," Working Paper 2018.

Borjas, George J., "The Labor Demand Curve is Downward Sloping: Reexamining the Impact of Immigration on the Labor Market," *The Quarterly Journal of Economics*, 2003, *118* (4), 1335–1374.

Bound, John and George Johnson, "Changes in the Structure of Wages in the 1980's: An Evaluation of Alternative Explanations," *American Economic Review*, 1992, *82* (3), 371–392.

Brown, Charles, Curtis Gilroy, and Andrew Kohen, "The Effect of the Minimum Wage on Employment and Unemployment," *Journal of Economic literature*, 1982, *20* (2), 487–528.

Cameron, A Colin and Douglas L. Miller, "A Practitioner's Guide to Cluster-Robust Inference," *Journal of Human Resources*, 2015, *50* (2), 317–372.

Cameron, A. Colin, Jonah B. Gelbach, and Douglas L. Miller, "Bootstrap-Based Improvements for Inference with Clustered Errors," *The Review of Economics and Statistics*, 2008, *90* (3), 414–427.

Card, David and Alan B. Krueger, "Minimum Wages and Employment: A Case-Study of the Fast-Food Industry in New Jersey and Pennsylvania," *American Economic Review*, 1994, *84* (4), 772–793.

_ **and** _ , “Minimum Wages and Employment: A Case Study of the Fast-Food Industry in New Jersey and Pennsylvania: Reply,” *American Economic Review*, 2000, *90* (5), 1397–1420.

_ **and Thomas Lemieux**, “Can Falling Supply Explain the Rising Return to College for Younger Men? A Cohort-based Analysis,” *The Quarterly Journal of Economics*, 2001, *116* (2), 705–746.

_ **, Jörg Heining, and Patrick Kline**, “Workplace Heterogeneity and the Rise of West German Wage Inequality,” *The Quarterly Journal of Economics*, 2013, *128* (3), 967–1015.

Cengiz, Doruk, Arindrajit Dube, Attila Lindner, and Ben Zipperer, “The Effect of Minimum Wages on Low-Wage Jobs: Evidence from the United States Using a Bunching Estimator,” CEP Discussion Papers dp1531, Centre for Economic Performance, LSE February 2018.

Deryugina, Tatyana, “The Fiscal Cost of Hurricanes: Distaster Aid versus Social Insurance,” *American Economic Journal: Applied Economics*, 2017, *9* (3), 168–198.

Dube, Arindrajit and Ben Zipperer, “Pooling Multiple Case Studies using Synthetic Controls: An Application to Minimum Wage Policies,” Working Paper 2015.

_ **, T. William Lester, and Michael Reich**, “Minimum Wage Effects Across State Borders: Estimates Using Contiguous Counties,” *The Review of Economics and Statistics*, 2010, *92* (4), 945–964.

_ **,** _ **, and** _ , “Minimum Wage Shocks, Employment Flows, and Labor Market Frictions,” *Journal of Labor Economics*, 7 2016, *34* (3), 663–704.

Ferman, Bruno and Christine Pinto, “Placebo Tests for Synthetic Controls,” 2017. Working paper.

_ **and** _ , “Revisiting the Synthetic Control Estimator,” 2017. Working Paper.

Firpo, Sergio and Vítor Possebom, "Synthetic Control Method: Inference, Sensitivity Analysis and Confidence Sets," *Journal of Causal Inference*, Forthcoming.

Fisher, Ronald A., *The Design of Experiments*, eighth edition ed., Hafner Publishing Company, 1971.

Giuliano, Laura, "Minimum Wage Effects on Employment, Substitution, and the Teenage Labor Supply: Evidence from Personnel Data," *Journal of Labor Economics*, 2013, *31* (1), 155–194.

Gobillon, Laurent and Thierry Magnac, "Regional Policy Evaluation: Interactive Fixed Effects and Synthetic Controls," *The Review of Economics and Statistics*, 2016, *98* (3), 535–551.

Hamermesh, Daniel S., *Labor Demand*, Princeton University Press, 1993.

Jardim, Ekaterina, Mark C Long, Robert Plotnick, Emma van Inwegen, Jacob Vigdor, and Hilary Wething, "Minimum Wage Increases, Wages, and Low-Wage Employment: Evidence from Seattle," Working Paper 23532, National Bureau of Economic Research June 2017.

Katz, Lawrence F. and Kevin M. Murphy, "Changes in Relative Wages, 1963–1987: Supply and Demand Factors," *The Quarterly Journal of Economics*, 1992, *107* (1), 35–78.

Krueger, Alan B., "The Minimum Wage: How Much Is Too Much?," *The New York Times*, October 2015.

_ **and Lawrence H. Summers**, "Efficiency Wages and the Inter-industry Wage Structure," *Econometrica*, 1988, *56* (2), 259–293.

Krusell, Per, Lee E. Ohanian, José-Víctor Ríos-Rull, and Giovanni L. Violante, "Capital-skill Complementarity and Inequality: A Macroeconomic Analysis," *Econometrica*, 2000, *68* (5), 1029–1053.

Lafortune, Julien, Jesse Rothstein, and Diane Whitmore Schanzenbach, "School Finance Reform and the Distribution of Student Achievement," *American Economic Journal: Applied Economics*, 2018, *10* (2), 1–26.

Meer, Jonathan and Jeremy West, "Effects Of The Minimum Wage On Employment Dynamics," *Journal of Human Resources*, 2016, *51* (2), 500–522.

Neumark, David, "The Econometrics and Economics of the Employment Effects of Minimum Wages: Getting from Known Unknowns to Known Knowns," Working Paper 25043, National Bureau of Economic Research September 2018.

_ **and William Wascher**, *Minimum Wages*, Cambridge, MA: The MIT Press, 2008.

_ **, J. M. Ian Salas, and William Wascher**, "Revisiting the Minimum Wage—Employment Debate: Throwing Out the Baby with the Bathwater?," *ILR Review*, 2014, *67* (3_suppl), 608–648.

Ottaviano, Gianmarco I. P. and Giovanni Peri, "Rethinking The Effect Of Immigration On Wages," *Journal of the European Economic Association*, 2012, *10* (1), 152–197.

Powell, David, "Synthetic Control Estimation Beyond Case Studies: Does the Minimum Wage Reduce Employment," RAND Labor & Population Working Paper WR-1142 2017.

Roodman, David, "BOOTTEST: Stata Module to Provide Fast Execution of the Wild Bootstrap with Null Imposed," Statistical Software Components, Boston College Department of Economics December 2015.

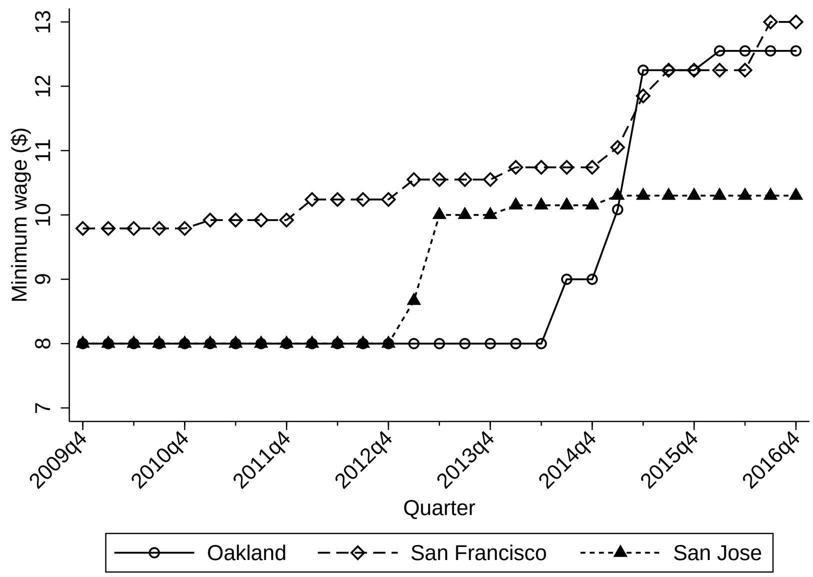
Rosenbaum, Paul R., *Observation Studies*, second edition ed., Springer Science, 2002.

Song, Jae, David J. Price, Fatih Guvenen, Nicholas Bloom, and Till von Wachter, "Firming Up Inequality," *Quarterly Journal Economics*, Forthcoming.

Totty, Evan, "The Effect of Minimum Wages on Employment: A Factor Model Approach," *Economic Inquiry*, 2017, *55* (4), 1712–1737.

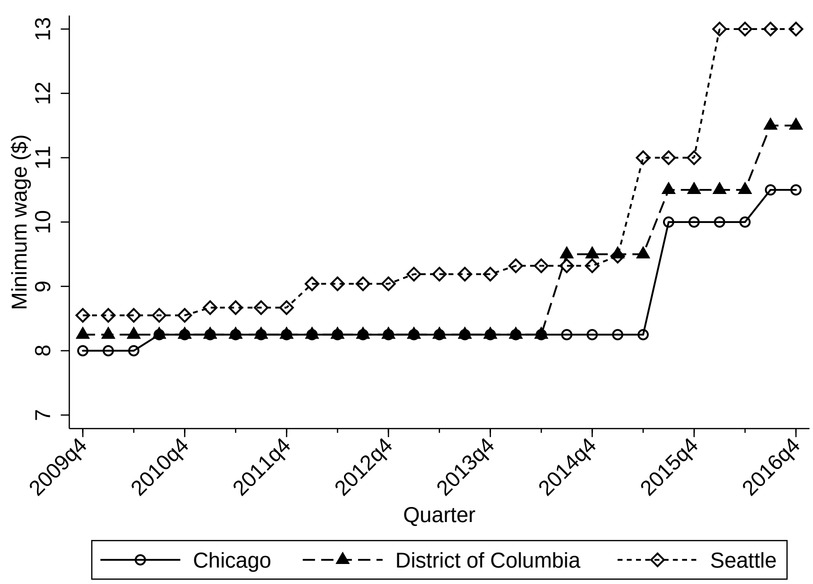
Figures

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



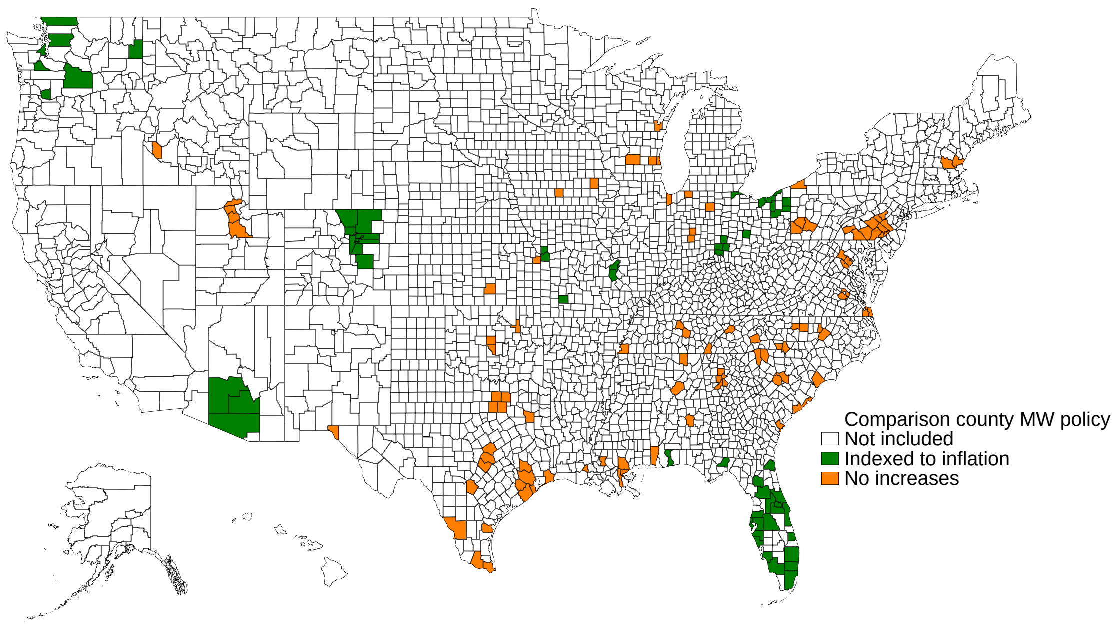
Notes: When the minimum wage increases in the middle of a quarter, the figure plots the average minimum wage over the months within the quarter. For cities that allow for subminimum wages, such as for tipped workers and workers in small firms, we use the highest minimum wage in effect.

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



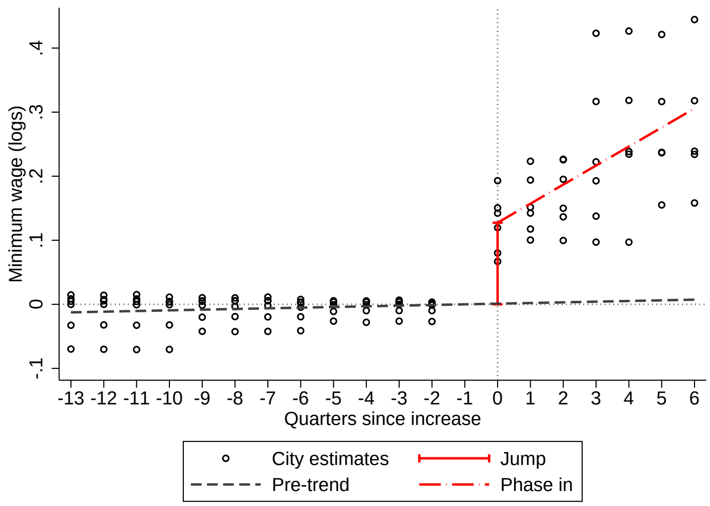
Notes: See notes to Figure 1.

Figure 3: Map of comparison counties



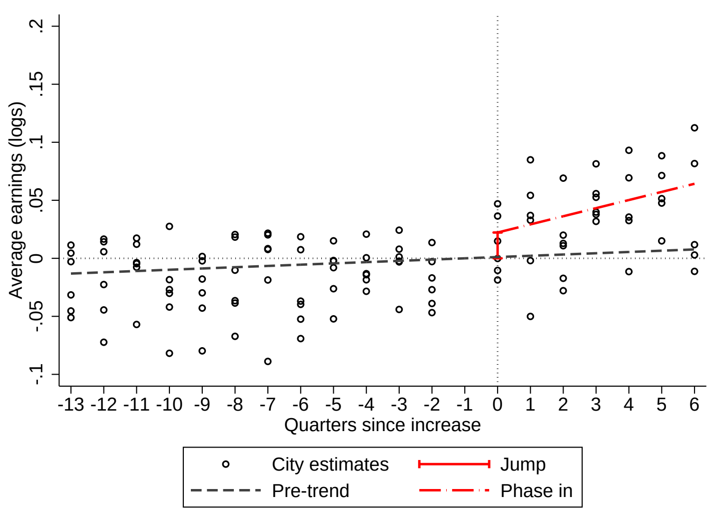
Notes: This map shows the untreated comparison counties that we use in our event study and synthetic control analyses. We include counties that (1) had no change in their minimum wage during our period of study, and (2) are in a metropolitan area with an estimated population of at least 200,000 in 2009q4.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

Figure 4: Event study minimum wage estimates



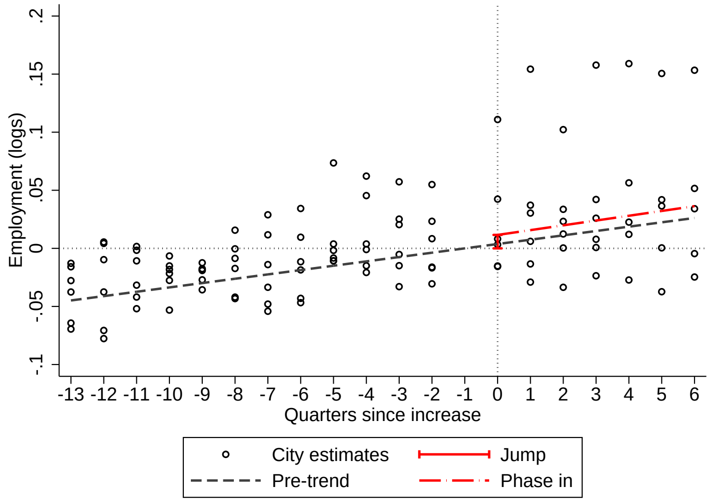
Notes: This figure plots estimates from event studies of the minimum wage policy, measured in logs. Models are normalized such that each estimate represents the difference in the log minimum wage relative to the end of the pre-policy period (point -1 on the horizontal axis). The circles labeled "city estimates" plot coefficients from the nonparametric model. The lines labeled "pre-trend," "jump," and "phase in" plot the pre-trend and dynamic effects of the citywide policies that we estimate using the trend break model. See Section 5.1 for more information.

Figure 5: Event study earnings estimates



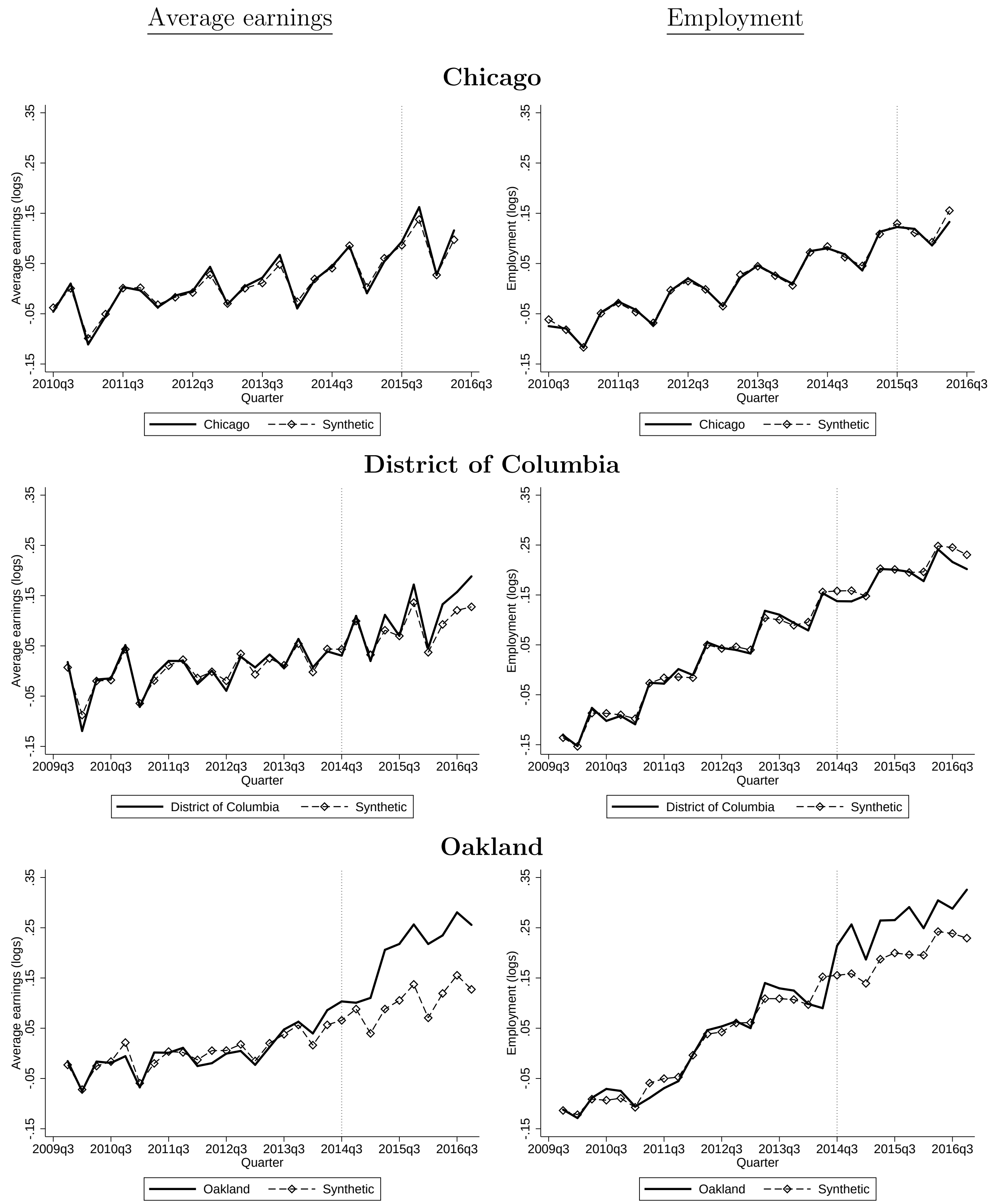
Notes: This figure plots estimates from event studies of average earnings in food services, measured in logs. See notes to Figure 4 for more information.

Figure 6: Event study employment estimates



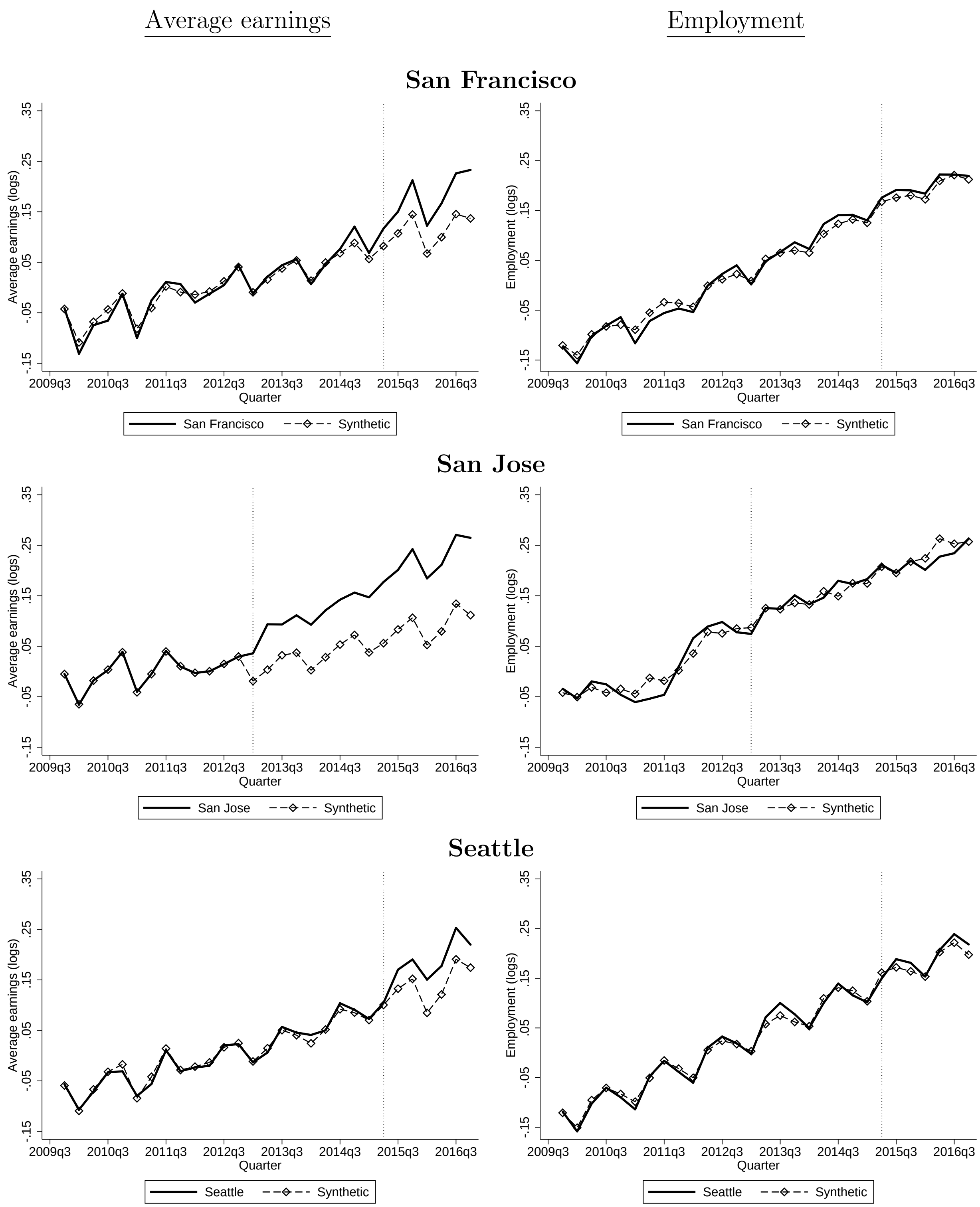
Notes: This figure plots estimates from event studies of employment in food services, measured in logs. See notes to Figure 4 for more information.

Figure 7: Synthetic control analysis of Chicago, District of Columbia and Oakland



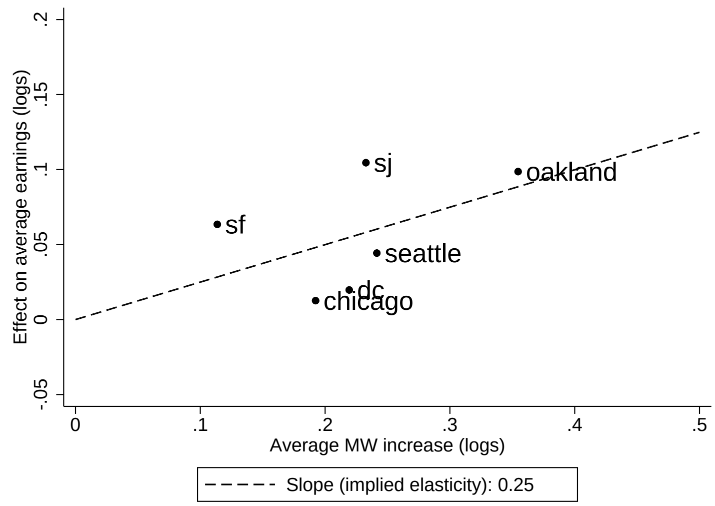
Notes: This figure plots average earnings and employment in food services in Chicago, the District of Columbia and Oakland and their synthetic controls. Each outcome is measured in logs and is centered around its pre-policy average. The vertical dotted line marks the beginning of the evaluation period.

Figure 8: Synthetic control analysis of San Francisco, San Jose and Seattle



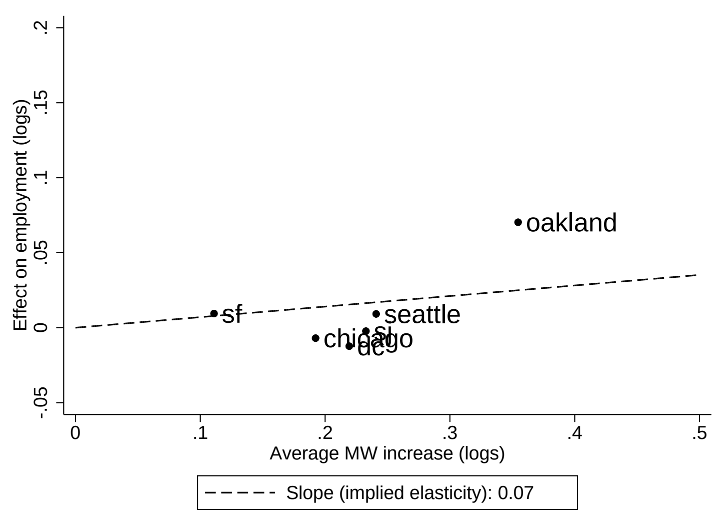
Notes: This figure plots average earnings and employment in food services in San Francisco, San Jose and Seattle and their synthetic controls. Each outcome is measured in logs and is centered around its pre-policy average. The vertical dotted line marks the beginning of the evaluation period.

Figure 9: 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 4). 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 industrywide elasticity of average earnings in food services with respect to the minimum wage.

Figure 10: Synthetic control employment estimates

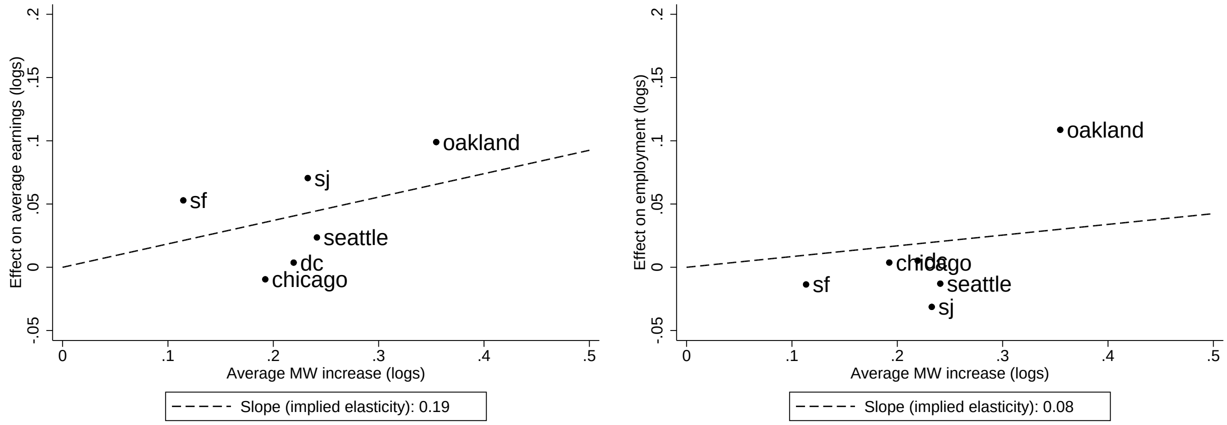


Notes: This figure plots each city's estimated employment effect of its local minimum wage policy against the city's average minimum wage increase. The slope of the dashed line is a measure of the industrywide elasticity of employment in food services with respect to the minimum wage. See Figure 9 for more information.

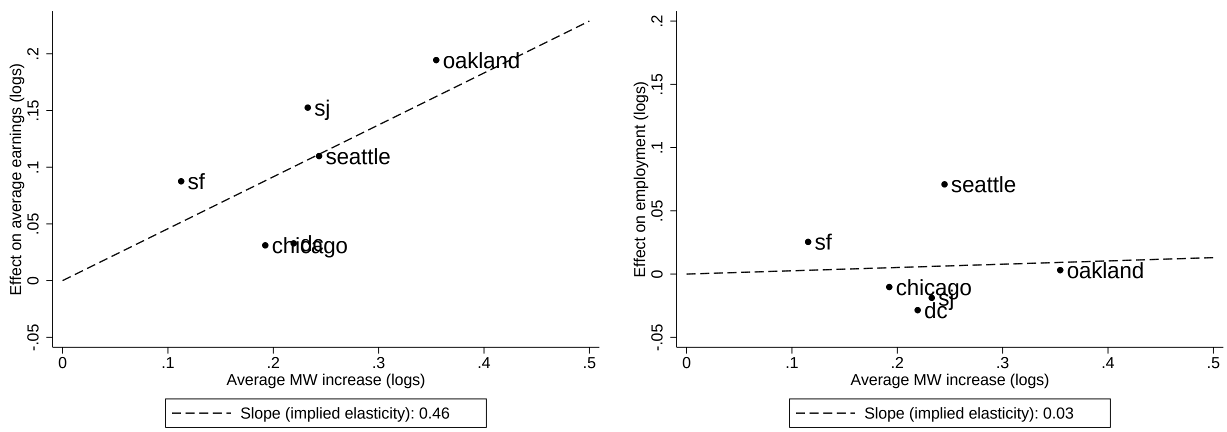
Figure 11: Synthetic control estimates
full and limited service restaurants and professional services

Average earnings | Employment

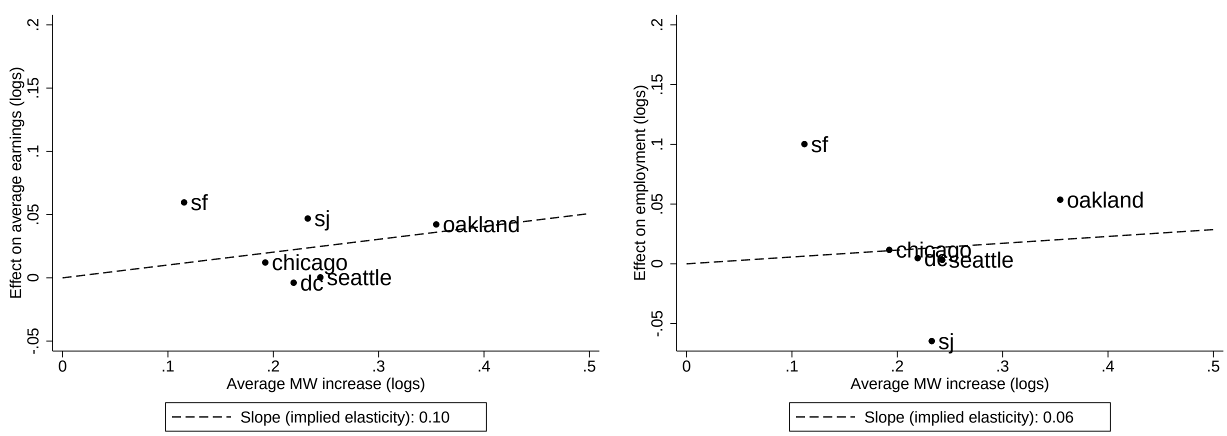
Panel A: Full service restaurants



Panel B: Limited service restaurants

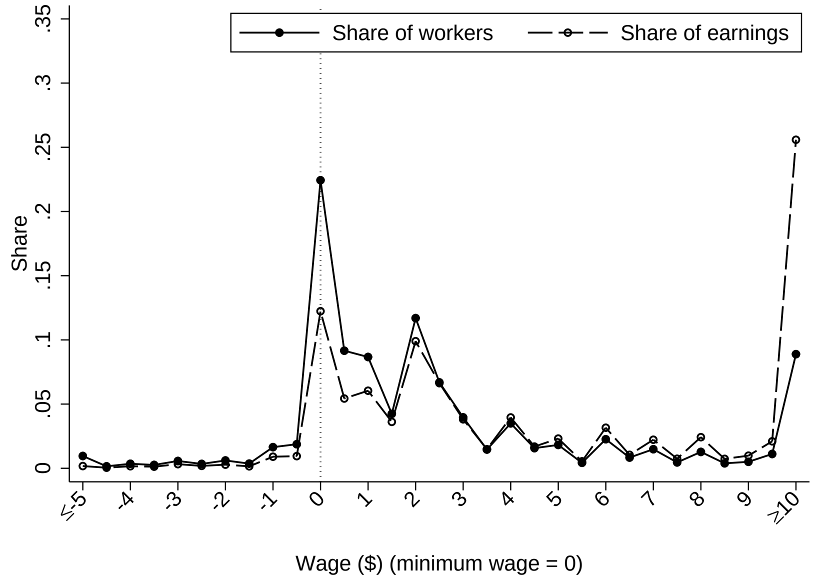


Panel C: Professional services



Notes: These figures plot each city's estimated effects of its local minimum wage policy against the city's average minimum wage increase. See note to Figure 9 for more information.

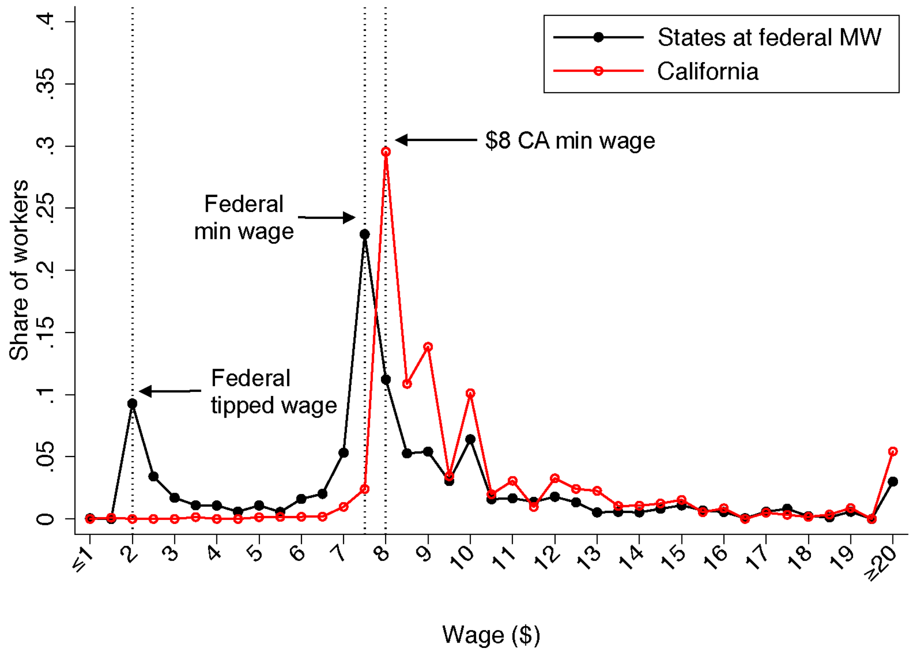
Figure 12: Employment and earnings shares in the six cities before the local policies



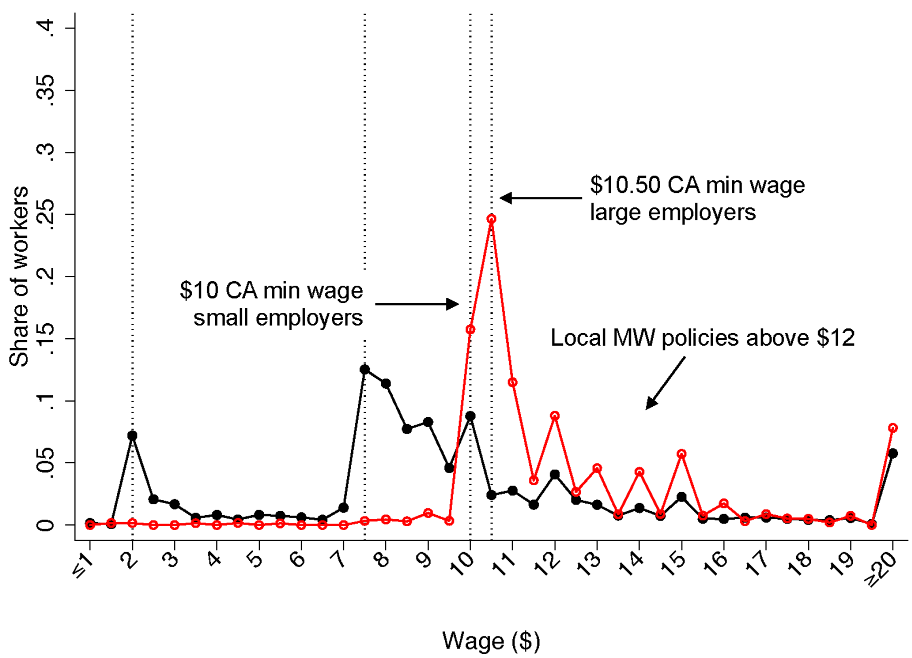
<u>Notes</u>: This figure shows the distribution of employment and earnings among food service workers living near Chicago, the District of Columbia, Oakland, San Francisco, San Jose and Seattle. Wages are centered around the pre-policy minimum wage. To estimate shares, we first bin together the difference between the wage and the city's pre-policy minimum wage in fifty cent increments. We then group together all workers who report earning less than $5 or more than $10 above the minimum. We estimate these distributions using data from the CPS ORG, pooling four years of pre-policy data for each city. Hourly workers in Chicago and the District of Columbia who report wages within 50 cents of their cities' tipped wage are assumed to earn the minimum wage. There are 1,606 workers in this sample. See Section 8 for more information.

Figure 13: Distribution of wages in food services, 2010 and 2017

Panel A: 2010

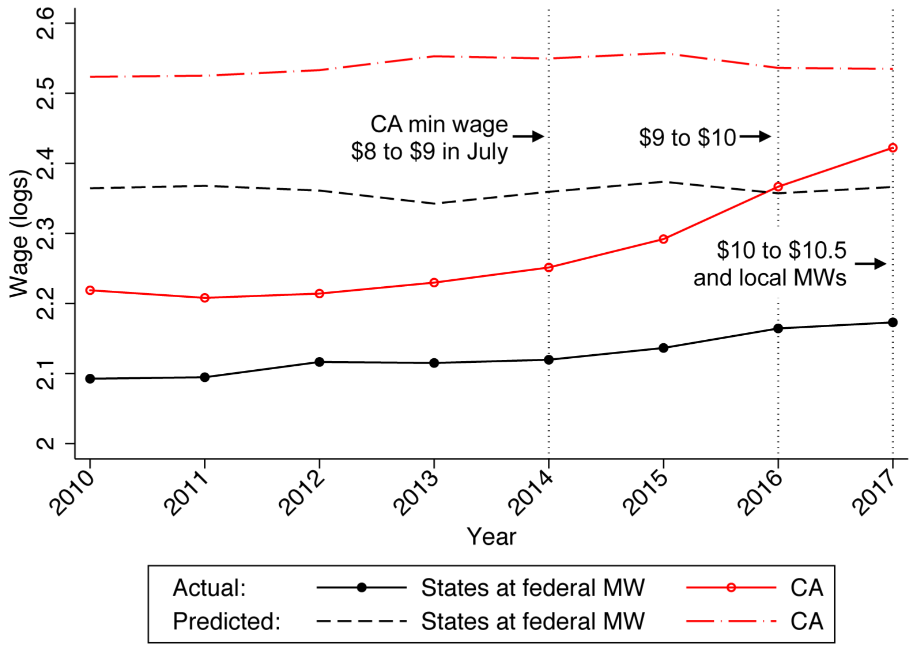


Panel B: 2017



Notes: This figure plots the distribution of wages of workers employed in food services in California and states in which the minimum wage is set at the federal level. Wages are binned around 50 cent intervals. Data on wages comes from the CPS ORG. See Section 9 for more information.

Figure 14: Actual vs. predicted wages of food service workers earning under $16



Notes: Plots actual and predicted log wages paid to workers employed in food services in California and states in which the minimum wage is set at the federal level. Vertical lines mark years in which the California minimum wage increased statewide. Data on wages come from the CPS ORG. We predict log wages from a regression model estimated on the 2010 CPS ORG. See Section 9 for more information.

Tables

Table 1: Distribution of private sector employment in the six cities before the local policies

	Earning less than citywide MW		
Industry	No	Yes	Total
	(1)	(2)	(3)
Construction			
Row percentage (%)	88.0	12.0	100.0
Column percentage (%)	5.0	2.5	4.5
Educational and health services			
	85.7	14.3	100.0
	18.0	11.3	16.6
Financial activities			
	94.2	5.8	100.0
	9.0	2.1	7.6
Food services			
	24.2	75.8	100.0
	2.6	30.3	8.4
Information			
	92.3	7.7	100.0
	5.2	1.6	4.5
Leisure and hospitality, excl. food			
	68.0	32.0	100.0
	3.1	5.5	3.6
Manufacturing			
	89.8	10.2	100.0
	12.9	5.5	11.4
Professional services			
	97.7	2.3	100.0
	19.5	1.7	15.8
Retail trade			
	59.2	40.8	100.0
	7.9	20.6	10.6
All other industries			
	76.8	23.2	100.0
	16.7	18.9	17.1
Total			
	79.0	21.0	100.0
	100.0	100.0	100.0

Notes: Table reports the distribution of private sector employment in six cities before the local minimum wages went into effect, by industry. The six cities are Chicago, the District of Columbia, Oakland, San Francisco, San Jose and Seattle. We report the distribution for two groups: workers who report wages higher than the level of the citywide minimum wage at the end of the sample period (column 1) and those who report wages at or lower than the citywide minimum wage (column 2). Column 3 reports the distribution for both groups combined. We estimate these distributions using data from the CPS ORG, pooling four years of pre-policy data for each city. Sample size is 21,486 observations. See Section 4.1 for more information.

Table 2: Average characteristics of our six cities and comparison counties

	Six cities (1)	Comparison counties (2)
Panel A: Annual average, 2012		
Population (1000s)	1033.2	589.8
Total private sector		
Average earnings ($)	1385.4	860.9
Employment (1000s)	490.9	227.3
Food services		
Average earnings ($)	409.4	298.6
Employment (1000s)	44.7	21.1
Full service restaurants		
Average earnings ($)	441.3	321.3
Employment (1000s)	23.2	10.3
Limited service restaurants		
Average earnings ($)	342.9	259.2
Employment (1000s)	11.6	7.6
Professional services		
Average earnings ($)	2131.0	1270.8
Employment (1000s)	72.0	17.6
Panel B: Percent change, 2009–2012		
Population (%)	2.8	3.6
Total private sector		
Average earnings (%)	9.8	7.5
Employment (%)	4.8	3.5
Food services		
Average earnings (%)	6.6	6.9
Employment (%)	11.7	7.0
Full service restaurants		
Average earnings (%)	7.0	7.8
Employment (%)	13.9	6.8
Limited service restaurants		
Average earnings (%)	4.6	5.2
Employment (%)	8.5	7.1
Professional services		
Average earnings (%)	10.0	7.4
Employment (%)	8.9	4.6
Observations	6	159

Notes: Table reports descriptive statistics for the six cities (column 1) and the untreated comparison counties (column 2). The six cities are Chicago, the District of Columbia, Oakland, San Francisco, San Jose, and Seattle. Panel A reports sample means during 2012. For each variable, we compute its mean by averaging over the quarterly observations in the group indicated by the column heading. Panel B reports the percent change in the sample means between 2009 and 2012.

Table 3: Event study results

	Food services			
	Earnings (logs)		Employment (logs)	
	(1)	(2)	(3)	(4)
Panel A: Point estimates of trend break model[a]				
Jump	0.029	0.021	0.033	0.008
	(0.009)	(0.009)	(0.015)	(0.007)
Phase in	0.007	0.006	0.004	0.000
	(0.004)	(0.003)	(0.002)	(0.002)
p, total event effect = 0 (city clusters)[b]	0.054	0.066	0.122	0.583
p, total event effect = 0 (state clusters)	0.133	0.119	0.175	0.709
Pre-trend	—	0.001	—	0.004
	—	(0.001)	—	(0.001)
p, pre-trend = 0 (city clusters)	—	0.310	—	0.030
p, pre-trend = 0 (state clusters)	—	0.338	—	0.072
Observations	4728	4728	4728	4728
Adjusted R^2	0.79	0.79	0.86	0.86
Panel B: Industrywide elasticity with respect to the minimum wage[c]				
Elasticity	0.224	0.186	0.199	0.039
	(0.053)	(0.048)	(0.067)	(0.036)
p, elasticity = 0 (city clusters)	0.005	0.010	0.020	0.359
90% CI (city clusters)	[0.113,0.321]	[0.068,0.289]	[0.069,0.342]	[-0.041,0.104]
p, elasticity = 0 (state clusters)	0.029	0.074	0.060	0.363
90% CI (state clusters)	[0.036,0.327]	[0.017,0.282]	[0.035,0.286]	[-0.048,0.113]
Control for trend[d]	No	Yes	No	Yes

Notes: Standard errors in parentheses, clustered at the city and county level. 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. All models include quarterly calendar time effects and city and county effects, and control for private sector earnings and employment. See Section 5.1 for more information. [a] Reports estimates of models specified in Equations (6) and (7). [b] p-values for total event effect test the hypothesis that the coefficients θ^{jump} and $\theta^{phasein}$ are both zero. [c] Earnings and employment elasticities are estimated by instrumental variables.
[d] Reports whether the model controls for a linear trend in event time.

Table 4: Synthetic control setup, by city

	Chicago (1)	District of Columbia (2)	Oakland (3)	San Francisco (4)	San Jose (5)	Seattle (6)
Donor pool MW policy[a]	No increases	No increases	No increases	Indexed to inflation	No increases	Indexed to inflation
Counties in donor pool	99	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 synthetic control donor pool includes comparison counties their either (1) have no minimum wage increases between 2009q4–2016q4 (No increases), or (2) includes counties that index their minimum wage to inflation (Indexed to inflation). [b] The pre-policy period are the quarters before the minimum wage increase that we use in our analysis. [c] The minimum wage enforced in the city at the end of the pre-policy period. [d] The evaluation period are the quarters after the minimum wage increase that we use to measure the effect of the policy on earnings and employment. [e] The average log minimum wage during the evaluation period minus the log minimum wage at the end of the pre-policy period.

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)						
Industrywide elasticity with respect to the minimum wage: 0.25						
Effect of MW increase	0.013	0.020	0.099	0.063	0.105	0.044
p, effect = 0	0.390	0.270	0.020	0.033	0.020	0.033
90% CI	[-0.012,0.037]	[-0.021,0.060]	[0.058,0.139]	[0.041,0.088]	[0.059,0.150]	[0.022,0.068]
Test of parallel trends assumption:						
Effect during final pre-policy year[a]	-0.007	0.002	0.028	0.015	-0.001	0.008
p, effect during final pre-policy year = 0	0.260	0.750	0.020	0.098	0.680	0.262
Pre-policy pseudo R^2	0.963	0.925	0.853	0.951	0.999	0.983
Panel B: Employment (logs)						
Industrywide elasticity with respect to the minimum wage: 0.07						
Effect of MW increase	-0.007	-0.012	0.070	0.009	-0.002	0.009
p, effect = 0	0.670	0.560	0.020	0.590	0.930	0.623
90% CI	[-0.040,0.027]	[-0.054,0.030]	[0.029,0.112]	[-0.049,0.070]	[-0.060,0.056]	[-0.049,0.069]
Test of parallel trends assumption:						
Effect during final pre-policy year[a]	-0.001	-0.002	-0.011	0.022	0.020	-0.003
p, effect during final pre-policy year = 0	0.850	0.650	0.140	0.131	0.030	0.738
Pre-policy pseudo R^2	0.993	0.989	0.949	0.979	0.886	0.988
Counties in donor pool	99	99	99	60	99	60
Pre-policy periods	20	19	19	22	13	22

Notes: Reports synthetic control results for food service earnings and employment. Significance tests and confidence intervals are based on placebo tests. We base the industrywide elasticities with respect to the minimum wage on the fitted relationship between the estimated effects and the average increase in the minimum wage. See Section 6.1 for more information. [a] 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.

Table 6: Simulations of elasticities with respect to the minimum wage under labor-labor substitution

Elasticity of Substitution	Employment elasticity of workers at old MW	Industrywide elasticity	
		Earnings	Employment
(1)	(2)	(3)	(4)
0.00	0.00	0.24	0.00
0.25	-0.19	0.28	-0.04
0.50	-0.38	0.33	-0.09
0.75	-0.57	0.37	-0.13
1.00	-0.76	0.41	-0.17
1.25	-0.95	0.46	-0.22
1.50	-1.14	0.50	-0.26
1.75	-1.33	0.54	-0.30
2.00	-1.52	0.59	-0.35

Notes: Table presents simulated elasticities with respect to the minimum wage that we would observe following a 20 percent increase in the minimum wage. Simulations are based on the actual pre-policy distribution of wages and earnings among food service workers living near the six cities. Each row reports the effects the we would observe based on a different value for the elasticity of substitution, reported in column 1. Column 2 reports the employment elasticity of workers who would be paid at the old minimum wage in the absence of the increase. Values smaller than 1 in magnitude indicate that the minimum wage increase raises these workers' earnings net of employment losses. Columns 3 and 4 report the industrywide earnings and employment elasticities, respectively. See Section 8 for more information.

Table 7: Effects of California's statewide and local minimum wages on food service workers

	California Minimum wage (1)	Locals MWs[a] (2)	Percent with MW $\geq$\$12[b] (3)	Earning under \$16 (4)	Workers earning under \$16: Log(wage) (5)	Workers earning under \$16: Predicted log(wage) (6)	Workers earning under \$16: Log(hours) (7)
CA×Year 2011	\$8	1	0	-0.021	-0.012	0.004	0.031
				(0.020)	(0.016)	(0.021)	(0.039)
CA×Year 2012	\$8	1	0	-0.007	-0.028	0.014	0.058
				(0.018)	(0.017)	(0.021)	(0.037)
CA×Year 2013	\$8	2	0	0.008	-0.011	0.052	0.039
				(0.019)	(0.016)	(0.021)	(0.038)
CA×Year 2014	\$8 to \$9 in July	2	0	0.017	0.006	0.033	0.070
				(0.019)	(0.023)	(0.022)	(0.037)
CA×Year 2015	\$9	8	3	0.019	0.029	0.028	0.054
				(0.019)	(0.017)	(0.022)	(0.037)
CA×Year 2016	\$10	16	6	-0.009	0.076	0.023	0.016
				(0.020)	(0.020)	(0.021)	(0.038)
CA×Year 2017	\$10 or \$10.50	21	10–24	0.001	0.124	0.013	0.049
				(0.020)	(0.015)	(0.022)	(0.039)
CA				-0.042	0.133	0.198	-0.074
				(0.016)	(0.017)	(0.020)	(0.035)
Observations				18090	16651	16651	16651
Outcome mean				0.922	2.168	2.411	3.315
p, pre-increase effects = 0[d]				0.506	0.428	0.043	0.474

Notes: Table reports results from analysis of the effects of California's statewide and local minimum wage policies using the CPS ORG. Regression sample is composed of all workers employed in food services in California and states at the federal minimum wage during years 2010–2017. Regression also includes state and calendar year effects. Robust standard errors in parentheses. See Section 9 for more information.

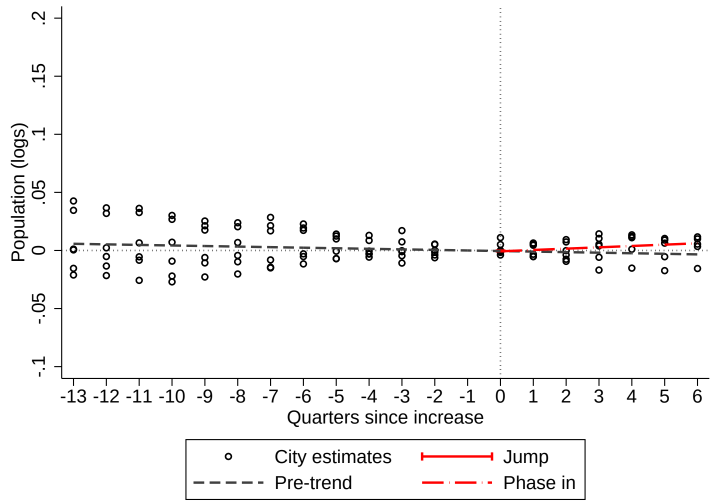
[a] Reports number of cities in California with a local minimum wage policy. [b] Reports the share of food service workers in cities with a local minimum wage policy above \$12. We estimate the share using the data on food service employment by city from the 2012 Economic Census. The range in 2017 is based on the different minimum wage policies some cities set for small and large employers.

[c] Outcome is the log hourly wage predicted from a Mincer regression estimated on the 2010 CPS ORG. [d] Reports the p-value for the hypothesis that the interactions between California and the year effects for years 2011, 2012 and 2013 are all zero.

Appendices

A Additional exhibits

Figure A1: Event study population estimates



Notes: This figure plots estimates from event studies of annual population, measured in logs. Models are normalized such that each estimate represents the difference in log population relative to the end of the pre-policy period (point -1 on the horizontal axis). The circles labeled "city estimates" plot coefficients from the nonparametric model. The lines labeled "pre-trend," "jump," and "phase in" plot the pre-trend and dynamic effects of the citywide policies that we estimate using the trend break model. See Section 5.1 for more information.

Table A1: Summary statistics of food service workers earning under $16

	California		States at federal MW	
	2010	2017	2010	2017
	(1)	(2)	(3)	(4)
Wage (logs)	2.219	2.422	2.093	2.173
Predicted wage (logs)	2.524	2.535	2.364	2.366
Female	0.505	0.549	0.575	0.548
Teenager	0.099	0.118	0.139	0.176
Potential experience	11.632	11.147	10.846	11.150
Less than high school	0.264	0.217	0.270	0.241
High school	0.351	0.362	0.368	0.385
Some college	0.315	0.342	0.301	0.311
Bachelors or higher	0.070	0.080	0.062	0.063
White	0.308	0.236	0.548	0.526
Black	0.031	0.042	0.172	0.167
Hispanic	0.536	0.545	0.241	0.264
Asian	0.119	0.163	0.031	0.039
Other race	0.007	0.015	0.008	0.005
Foreign born	0.480	0.360	0.169	0.181
Citizen	0.643	0.727	0.865	0.861
Hours last week at main job	28.538	30.090	30.405	30.657
Hours$\leq$ 25	0.428	0.355	0.375	0.354
Paid by the hour	0.929	0.957	0.875	0.898
Tipped worker	0.303	0.289	0.339	0.280
Observations	503	499	1541	1563

Notes: Table reports summary statistics of food service workers earning under $16 in California and states at the federal minimum wage during years 2010–2017. Data is from the CPS ORG. The predicted log wage is the log hourly wage predicted from a Mincer regression estimated on the 2010 CPS ORG. See Section 9 for more information.

Table A2: 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	0.024	0.012	0.094	0.067	0.098	0.050
p, effect = 0	0.163	0.545	0.014	0.023	0.010	0.023
90% CI	[-0.008,0.057]	[-0.026,0.050]	[0.055,0.132]	[0.044,0.093]	[0.055,0.142]	[0.027,0.078]
Test of parallel trends assumption:						
Effect during final pre-policy year[a]	0.000	-0.005	0.019	0.018	0.000	0.012
p, effect during final pre-policy year = 0	0.880	0.378	0.057	0.045	0.976	0.170
Pre-policy pseudo R^2	0.977	0.951	0.933	0.958	1.000	0.986
Counties in donor pool	208	208	208	87	208	87
Pre-policy periods	20	19	19	22	13	22
Panel B: Comparison counties, population greater than 200,000 (in paper)						
Effect of MW increase	0.013	0.020	0.099	0.063	0.105	0.044
p, effect = 0	0.390	0.270	0.020	0.033	0.020	0.033
90% CI	[-0.012,0.037]	[-0.021,0.060]	[0.058,0.139]	[0.041,0.088]	[0.059,0.150]	[0.022,0.068]
Test of parallel trends assumption:						
Effect during final pre-policy year[a]	-0.007	0.002	0.028	0.015	-0.001	0.008
p, effect during final pre-policy year = 0	0.260	0.750	0.020	0.098	0.680	0.262
Pre-policy pseudo R^2	0.963	0.925	0.853	0.951	0.999	0.983
Counties in donor pool	99	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, effect = 0	0.217	0.348	0.029	0.045	0.029	0.045
90% CI	[-0.011,0.044]	[-0.027,0.059]	[0.058,0.144]	[0.022,0.077]	[0.055,0.139]	[0.018,0.066]
Test of parallel trends assumption:						
Effect during final pre-policy year[a]	-0.001	0.002	0.030	0.014	-0.006	0.005
p, effect during final pre-policy year = 0	0.928	0.739	0.014	0.068	0.261	0.432
Pre-policy pseudo R^2	0.947	0.916	0.835	0.940	0.982	0.979
Counties in donor pool	68	68	68	43	68	43
Pre-policy periods	20	19	19	22	13	22

Notes: Reports synthetic control results for food service earnings. Significance tests and confidence intervals are based on placebo tests.
[a] 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.

Table A3: 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	0.024	0.012	0.094	0.067	0.098	0.050
p, effect = 0	0.163	0.545	0.014	0.023	0.010	0.023
90% CI	[-0.008,0.057]	[-0.026,0.050]	[0.055,0.132]	[0.044,0.093]	[0.055,0.142]	[0.027,0.078]
Test of parallel trends assumption:						
Effect during final pre-policy year[a]	0.000	-0.005	0.019	0.018	0.000	0.012
p, effect during final pre-policy year = 0	0.880	0.378	0.057	0.045	0.976	0.170
Pre-policy pseudo R^2	0.977	0.951	0.933	0.958	1.000	0.986
Counties in donor pool	208	208	208	87	208	87
Pre-policy periods	20	19	19	22	13	22
Panel B: Comparison counties, population greater than 200,000 (in paper)						
Effect of MW increase	0.013	0.020	0.099	0.063	0.105	0.044
p, effect = 0	0.390	0.270	0.020	0.033	0.020	0.033
90% CI	[-0.012,0.037]	[-0.021,0.060]	[0.058,0.139]	[0.041,0.088]	[0.059,0.150]	[0.022,0.068]
Test of parallel trends assumption:						
Effect during final pre-policy year[a]	-0.007	0.002	0.028	0.015	-0.001	0.008
p, effect during final pre-policy year = 0	0.260	0.750	0.020	0.098	0.680	0.262
Pre-policy pseudo R^2	0.963	0.925	0.853	0.951	0.999	0.983
Counties in donor pool	99	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, effect = 0	0.217	0.348	0.029	0.045	0.029	0.045
90% CI	[-0.011,0.044]	[-0.027,0.059]	[0.058,0.144]	[0.022,0.077]	[0.055,0.139]	[0.018,0.066]
Test of parallel trends assumption:						
Effect during final pre-policy year[a]	-0.001	0.002	0.030	0.014	-0.006	0.005
p, effect during final pre-policy year = 0	0.928	0.739	0.014	0.068	0.261	0.432
Pre-policy pseudo R^2	0.947	0.916	0.835	0.940	0.982	0.979
Counties in donor pool	68	68	68	43	68	43
Pre-policy periods	20	19	19	22	13	22

Notes: Reports synthetic control results for food service employment. Significance tests and confidence intervals are based on placebo tests.
[a] 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.

Table A4: Effects of California's statewide and local minimum wages on food service workers, additional results

	Workers earning under \$16									
	High school or higher (1)	Bachelors or higher (2)	Potential experience (3)	Teenager (4)	Female (5)	White (6)	Black (7)	Asian (8)	Hispanic (9)	Foreign born (10)
CA×Year 2011	0.000	-0.004	-0.557	-0.019	0.058	-0.015	-0.019	0.005	0.035	-0.038
	(0.033)	(0.018)	(0.872)	(0.022)	(0.037)	(0.035)	(0.019)	(0.022)	(0.035)	(0.035)
CA×Year 2012	-0.033	0.008	0.101	-0.027	0.025	-0.044	-0.013	0.009	0.052	-0.044
	(0.032)	(0.018)	(0.860)	(0.022)	(0.037)	(0.033)	(0.018)	(0.022)	(0.035)	(0.035)
CA×Year 2013	0.001	0.023	0.740	-0.037	0.001	0.038	-0.027	-0.033	0.020	-0.104
	(0.032)	(0.018)	(0.886)	(0.022)	(0.037)	(0.034)	(0.018)	(0.021)	(0.035)	(0.035)
CA×Year 2014	0.018	0.015	0.031	-0.028	-0.001	0.061	-0.031	-0.020	-0.017	-0.137
	(0.032)	(0.019)	(0.909)	(0.023)	(0.038)	(0.035)	(0.021)	(0.022)	(0.036)	(0.035)
CA×Year 2015	0.035	0.006	0.133	-0.029	-0.008	0.009	-0.058	0.023	0.023	-0.075
	(0.032)	(0.019)	(0.922)	(0.023)	(0.038)	(0.035)	(0.019)	(0.023)	(0.036)	(0.035)
CA×Year 2016	0.013	-0.009	0.775	-0.046	0.044	0.035	-0.036	0.004	-0.011	-0.102
	(0.032)	(0.019)	(0.908)	(0.023)	(0.038)	(0.035)	(0.019)	(0.023)	(0.036)	(0.035)
CA×Year 2017	0.013	0.008	-0.772	-0.017	0.068	-0.057	0.006	0.037	0.004	-0.123
	(0.032)	(0.019)	(0.911)	(0.024)	(0.038)	(0.034)	(0.020)	(0.024)	(0.036)	(0.035)
CA	-0.004	0.028	1.469	-0.033	-0.124	-0.272	-0.293	0.086	0.486	0.404
	(0.030)	(0.016)	(0.798)	(0.023)	(0.035)	(0.034)	(0.026)	(0.018)	(0.027)	(0.028)
Observations	16651	16651	16651	16651	16651	16651	16651	16651	16651	16651
Outcome mean	0.754	0.062	10.930	0.135	0.547	0.441	0.147	0.060	0.346	0.245
p, pre-increase effects = 0	0.644	0.458	0.513	0.378	0.359	0.082	0.524	0.150	0.487	0.027

Notes: Table reports results from analysis of the effects of California's statewide and local minimum wage policies using the CPS ORG. Regression sample is composed of all workers employed in food services in California and states at the federal minimum wage during years 2010–2017. Regression also includes state and calendar year effects. Robust standard errors in parentheses. The row labeled "p, pre-increase effects = 0" reports the p-value for the hypothesis that the interactions between California and the year effects for years 2011, 2012 and 2013 are all zero. See notes to Table 7 for more information.

B Event study model specification and sensitivity

B.1 Model selection

In this section, we describe the cross-validation procedure we use to choose our preferred event study specification. Our objective is to formulate a model for the six cities' earnings and employment in the absence of the citywide policies that will provide a suitable counterfactual for comparing outcomes after the policies go into effect. We measure minimum wage effects on these outcomes in four sectors: food services, full service restaurants, limited service restaurants and professional services. Although we do not observe the counterfactuals of interest after the policies went into effect, we do observe them beforehand. A natural approach then is to compare alternative models based on their goodness of fit on the six city sample during the pre-policy period.

Ideally, we would fit flexible models for each outcome using only the treated city sample. This approach would ensure that the relationships that we estimate between the control variables and the outcomes would be calibrated to maximize the goodness of fit on the population of interest. Unfortunately, with only six treated cities in the panel, there are too few degrees of freedom to estimate these models. When we increase the degrees of freedom by including untreated comparison counties in the regression sample, we introduce the risk that the relationships we measure using the comparison counties will not be representative of the six cities. (For example, we find in Table 2 that the six cities are on average higher paying and more populous than the comparison counties.) We increase this risk when we make the model more complex—for example, by allowing the relationships to change over time—because we increase the chance of overfitting.

To find the specification that has the best explanatory power on the six city sample during the pre-policy period, we consider a wide class of models. In a first step, we fit each model on the untreated comparison county sample only. We then validate each model by calculating R^2 statistics on the held-back treated city sample during the final 13 quarters of the pre-policy period—the same 13 quarters included in our event study window. We call the R^2 statistic that we calculate on the city sample the *city* R^2.[53]

In addition to private sector earnings and employment, we also consider models that include two other variables. The first is city or county population, 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), averaged

[53] Formally, for each outcome and model, we calculate the city R^2 statistic:

$$\textit{city } R^2 \equiv 1 - \frac{\sum_{i \in 6 \text{ cities}} \sum_{e=-13}^{-1} \left(Y_{ite} - \hat{Y}_{ite}\right)^2}{\sum_{i \in 6 \text{ cities}} \sum_{e=-13}^{-1} \left(Y_{ite} - \bar{Y}_i^{pre}\right)^2}$$

where e indexes event time relative to the beginning of the city i's evaluation period ($e(i,t) = t - t_i^0$), Y_{ite} is the outcome of interest, $\hat{Y}_{ite}$ is the model's prediction and $\bar{Y}_i^{pre}$ is the outcome for city i averaged over event time quarters -13 through -1. Note that since each model includes a locality effect α_i, before computing the prediction we need to first estimate an effect for each treated city. We do so by setting each locality effect so that it solves its respective least squares first order condition. For example, if the model is $Y_{ite} = \alpha_i + \beta X_{ite} + u_{ite}$, then the estimate for city i is $\hat{\alpha}_i = \bar{Y}_i^{pre} - \hat{\beta}\bar{X}_i^{pre}$.

over the quarters 2007q1–2009q3.[54] This period immediately precedes the quarters included in our regression sample (2009q4–2016q4). This variable comes from the QCEW. We consider these variables, because they are less likely to be influenced by the cities' minimum wage policies than contemporaneous private sector earnings or employment (even after we exclude food services from their calculation). We measure both population and private sector size in logs.

Table B1 reports the city R^2 statistics. Each row reports results from a model that includes some combination of our four control variables (private sector earnings, private sector employment, population and 2007q1–2009q3 private sector size). All models also control for locality and quarterly calendar time effects.[55]

We also estimate different parameterizations of these variables. For example, the second row reports statistics from a model that constrains the influence of log population to be linear and common across all quarters. The third row allows for a nonlinear relationship by adding to the model a third order polynomial of log population. The fourth row instead allows for the influence of population to be seasonal, interacting log population with quarter of year effects. Finally, the fifth row allows the influence of population to vary each quarter, interacting log population with a full set of time effects.

For each of the eight outcomes, the city R^2 statistics from the models that allow for more complex relationships of population are either similar to or worse than what we find in the second row. The reduction in explanatory power indicates that models that allow for more complex effects of population yield noisier predictions. In general, when we add noise, the parameters of the trend break model will be less precisely estimated. We therefore prefer models that on average have higher explanatory power. Column 10 reports the average city R^2 over all eight outcomes. Here we find that models that control for population perform worse than those that control only for locality and time effects (row 1). We therefore do not include population as a control variable in our preferred event study model.[56]

Rows 8 through 11 of Table B1 illustrate the tradeoffs of using more flexible models to predict counterfactual outcomes when the model is estimated on a sample composed mostly of untreated units. It also shows the limitations of using more conventional approaches to validate a specification. Regressions that model the relationship with private sector earnings as a third order polynomial yield an average city R^2 of 0.41 (row 9), 30 percent lower than what we find when we model the relationship as linear (row 8). The difference in explanatory power is particularly dramatic for average earnings in limited service restaurants, in which the nonlinear model yields a *negative* city R^2. Importantly, the reduction in fit among the city sample is not evident from the regression results estimated on the comparison counties, which find a statistically significant nonlinear relationship. In this case, validation based

[54] We have also considered models that allow for private sector size to have a separate influence during each year 2007, 2008 and 2009q1–2009q3. These models typically perform worse than models that control for private sector size averaged over the entire 2007q1–2009q3 period. Results available upon request.

[55] We have also estimated models that allow for separate calendar time effects for the counties in states that index their minimum wage to inflation. These models perform modestly worse on average than those that allow only for common time effects. Results available upon request.

[56] Adding population does not increase the models' explanatory power, because the six cities on average trend in parallel with the comparison counties throughout the event study window. See Appendix Figure A1.

on t-tests would lead one to include variables that would ultimately reduce the explanatory power for the population of interest.[57]

In contrast, models that allow the influence of private sector earnings to vary over time typically perform better than models that assume the influence is constant. Table B1 reports that models that allow the influence to vary (row 11) yield an average city R^2 that is about 10 percent higher on average than those that do not (row 8). This improvement in explanatory power is not revealed by a comparison of adjusted R^2 from the regression results (not shown), which find only a one percentage point difference.

We find that the model with the greatest explanatory power is the one that controls for private sector earnings and employment and allows the influence of these variables to vary over time. This model, reported in row 19 of Table B1, yields an average city R^2 of 0.67, 13 percent higher than the city R^2 of the model does not include any control variables other than locality and time effects. Models that also control for population and 2007q1–2009q3 private sector size perform worse on average.[58]

In summary, we find that regression models that control for private sector earnings and employment and allow their influence to vary over time have greater explanatory power than alternatives that either include other control variables or allow for other nonlinear relationships. The improvement in explanatory power that we find when we measure the goodness of fit directly on the six city sample is not indicated from the regression results themselves. This disagreement reveals the limitations of using more conventional methods, such as t-tests and the adjusted R^2, to validate models used in event studies and underscores the importance of reporting the goodness of fit on the population of interest.

B.2 Sensitivity analysis

As a robustness check on our main findings from our event study analysis, we estimate the industrywide earnings and employment elasticities under each of the alternative specifications we considered in our model selection process. If we find that our results are similar across models, it suggests are conclusions are less likely to be biased by model misspecification.

Table B2 reports the industrywide earnings and employment elasticities that we estimate in food services under the alternative specifications. Over all, we find little variation in the elasticities, especially once we control for the linear trend. Column 3 reports that the earnings elasticities in these models range from 0.15 to 0.22, similar to what we report in the main text (reported in row 19). Likewise, the employment elasticities estimated in models that control for the trend are all very small and range from -0.02 to 0.04. All models that do not control for the trend yield larger, positive employment elasticities.

Tables B3 and B4 report the industrywide elasticities in full service restaurants and

[57]The t-statistic on each parameter of the third order polynomial of private sector earnings is about 3.5 in magnitude. The large reduction in fit that we find for earnings in limited service restaurant is robust to whether or not we include the six cities in the regression sample: Even with the six cities in the sample, the city R^2 for the nonlinear model is only 0.12. We also find a significant nonlinear relationship when we include the cities. In this case, the t-statistic on each parameter is between 2.1 and 2.2 in magnitude.

[58]We also find that the model reported in row 19 of Table B1 has the greatest explanatory power if we estimate the regressions on samples that include the six treated cities' pre-policy periods. In this case, the average city R^2 is 0.72.

limited service restaurants, respectively. As in Section 7, we find larger earnings elasticities in limited than full service restaurants, consistent with the larger number of minimum wage workers in that sector. We also find no support for negative employment effects. Over all, the employment elasticities range from -0.01 to 0.25.

Finally, Table B5 reports the results for professional services, building on the falsification exercise we discussed in Section 7. Interestingly, here we find the earnings elasticity is sensitive across specifications, even when we control for the linear trend, which in contrast yields generally stable results for food services and its sub-sectors. The earnings elasticity from the model that controls for the linear trend, 0.12, is the highest among the models. This sensitivity to how we specify the model suggests the earnings elasticity in our preferred model is more likely to be driven by idiosyncratic modeling assumptions than contemporaneous changes in the six cities' economies that could bias our analysis of the food services industry.

Over all, we find the earnings and employment effects we report in the main text for food services and its sub-sectors are robust to how we specify our model, suggesting it is unlikely our results are biased by model misspecification. In contrast, estimates for professional services, especially for earnings, are more variable, supporting our conclusion that the effects we measure in food services are not biased by contemporaneous non-policy related changes around the time the local policies went into effect.

Table B1: Summary of goodness of fit statistics from alternative event study model specifications

Specification	K	Earnings (logs)				Employment (logs)				*Avg, cols 2–9*
		Food	FSR	LSR	Prof	Food	FSR	LSR	Prof	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	*(10)*
(1) None	28	0.63	0.50	0.49	0.73	0.73	0.67	0.50	0.49	*0.59*
(2) Pop	29	0.63	0.50	0.49	0.73	0.76	0.70	0.52	0.43	*0.59*
(3) Poly(Pop)	31	0.63	0.50	0.48	0.73	0.76	0.71	0.49	0.42	*0.59*
(4) PopXQtr	32	0.57	0.43	0.41	0.77	0.75	0.69	0.51	0.43	*0.57*
(5) PopXTime	57	0.56	0.42	0.41	0.77	0.75	0.69	0.51	0.43	*0.57*
(6) Pop+2007-09 PrivateXTime	57	0.62	0.52	0.38	0.82	0.76	0.69	0.54	0.44	*0.60*
(7) (Pop+2007-09 Private)XTime	85	0.63	0.54	0.33	0.83	0.74	0.66	0.56	0.43	*0.59*
(8) Earnings	29	0.61	0.48	0.49	0.79	0.73	0.67	0.50	0.49	*0.59*
(9) Poly(Earnings)	31	0.49	0.48	-0.72	0.79	0.73	0.68	0.39	0.43	*0.41*
(10) EarningsXQtr	32	0.65	0.66	0.47	0.85	0.73	0.69	0.50	0.53	*0.63*
(11) EarningsXTime	57	0.69	0.66	0.43	0.86	0.75	0.69	0.52	0.62	*0.65*
(12) Employment	29	0.63	0.49	0.50	0.73	0.78	0.71	0.46	0.61	*0.61*
(13) Poly(Employment)	31	0.62	0.48	0.44	0.73	0.79	0.72	0.42	0.64	*0.61*
(14) EmploymentXQtr	32	0.62	0.51	0.44	0.79	0.79	0.71	0.46	0.63	*0.62*
(15) EmploymentXTime	57	0.62	0.49	0.43	0.79	0.79	0.72	0.45	0.65	*0.62*
(16) Earnings+Employment	30	0.61	0.48	0.49	0.79	0.79	0.71	0.50	0.61	*0.62*
(17) (Earnings+Employment)XQtr	36	0.63	0.62	0.46	0.86	0.79	0.72	0.50	0.67	*0.66*
(18) (Earnings+Employment+2007-09 Private)XQtr	39	0.63	0.62	0.45	0.84	0.79	0.72	0.50	0.66	*0.65*
(19) **(preferred model)** (Earnings+Employment)XTime	86	0.67	0.64	0.41	0.87	0.80	0.72	0.51	0.74	*0.67*
(20) (Earnings+Employment+2007-09 Private)XTime	114	0.66	0.62	0.38	0.85	0.81	0.72	0.54	0.74	*0.67*
(21) Pop+Earnings+Employment	31	0.59	0.47	0.48	0.79	0.79	0.71	0.54	0.53	*0.61*
(22) (Pop+Earnings+Employment)XQtr	40	0.57	0.55	0.42	0.86	0.80	0.73	0.54	0.58	*0.63*
(23) (Pop+Earnings+Employment+2007-09 Private)XQtr	43	0.56	0.53	0.42	0.84	0.80	0.72	0.54	0.58	*0.62*
(24) (Pop+Earnings+Employment)XTime	115	0.60	0.56	0.37	0.86	0.79	0.70	0.57	0.63	*0.63*
(25) (Pop+Earnings+Employment+2007-09 Private)XTime	143	0.59	0.53	0.33	0.84	0.79	0.70	0.58	0.65	*0.63*
Summary of city R^2 statistics										
(26) Minimum		0.49	0.42	-0.72	0.73	0.73	0.66	0.39	0.42	*0.41*
(27) p25		0.59	0.48	0.41	0.77	0.75	0.69	0.50	0.44	*0.59*
(28) p50		0.62	0.51	0.43	0.79	0.79	0.71	0.51	0.58	*0.62*
(29) p75		0.63	0.56	0.48	0.85	0.79	0.72	0.54	0.64	*0.63*
(30) Maximum		0.69	0.66	0.50	0.87	0.81	0.73	0.58	0.74	*0.67*

Notes: Table shows city R^2 statistics from alternative event study model specifications. All models include calendar time and locality effects. Column 1 reports the number of total parameters in the model (excluding the locality effects). Each cell in columns 2–9 reports an R^2 statistic that we measure on the six cities before the citywide policies went into effect. We estimate each regression model on the untreated comparison counties only (excluding the six cities). Column headers indicate the following: "Food" food services, "FSR" full service restaurants, "LSR" limited service restaurants, "Prof": professional services. The variable "Pop" is population, "2007–09 Private" 2007–09 average private sector earnings, "Earnings" private sector earnings, "Employment" private sector employment. See Section B for more information.

Table B2: Industrywide elasticities from different event study specifications, food services

		Elasticity with respect to the MW			
		Earnings		Employment	
	Control for trend?	No	Yes	No	Yes
	Avg city R^2	(1)	(2)	(3)	(4)
(1) None	0.59	0.28	0.21	0.22	0.00
(2) Pop	0.59	0.28	0.21	0.17	0.00
(3) Poly(Pop)	0.59	0.28	0.21	0.15	0.00
(4) PopXQtr	0.57	0.28	0.21	0.17	0.00
(5) PopXTime	0.57	0.29	0.22	0.16	0.01
(6) Pop+2007-09 PrivateXTime	0.60	0.28	0.23	0.17	0.00
(7) (Pop+2007-09 Private)XTime	0.59	0.26	0.22	0.19	-0.02
(8) Earnings	0.59	0.27	0.21	0.22	0.01
(9) Poly(Earnings)	0.41	0.28	0.21	0.19	0.00
(10) EarningsXQtr	0.63	0.25	0.20	0.21	0.00
(11) EarningsXTime	0.65	0.23	0.19	0.20	0.02
(12) Employment	0.61	0.28	0.21	0.18	0.01
(13) Poly(Employment)	0.61	0.27	0.21	0.16	0.01
(14) EmploymentXQtr	0.62	0.28	0.21	0.18	0.01
(15) EmploymentXTime	0.62	0.28	0.23	0.19	0.02
(16) Earnings+Employment	0.62	0.27	0.21	0.21	0.01
(17) (Earnings+Employment)XQtr	0.66	0.25	0.20	0.20	0.01
(18) (Earnings+Employment+2007-09 Private)XQtr	0.65	0.25	0.20	0.20	0.01
(19) **(preferred model)** (Earnings+Employment)Xtime	0.67	0.22	0.19	0.20	0.04
(20) (Earnings+Employment+2007-09 Private)XTime	0.67	0.23	0.19	0.20	0.04
(21) Pop+Earnings+Employment	0.61	0.27	0.21	0.19	0.01
(22) (Pop+Earnings+Employment)XQtr	0.63	0.25	0.20	0.18	0.00
(23) (Pop+Earnings+Employment+2007-09 Private)XQtr	0.62	0.25	0.20	0.18	0.00
(24) (Pop+Earnings+Employment)XTime	0.63	0.24	0.20	0.19	0.01
(25) (Pop+Earnings+Employment+2007-09 Private)XTime	0.63	0.24	0.20	0.19	0.02
Summary of elasticities					
(26) Minimum		0.22	0.19	0.15	-0.02
(27) p25		0.25	0.20	0.17	0.00
(28) p50		0.27	0.21	0.19	0.01
(29) p75		0.28	0.21	0.20	0.01
(30) Maximum		0.29	0.23	0.22	0.04

Notes: Reports industrywide elasticities with respect to the minimum wage from alternative model specifications. The column labeled "City avg R^2" reports the average city R^2 from Table B1, column 10. Row 19 is reported in Table 3. See notes to Tables B1 and 3 for more information.

Table B3: Industrywide elasticities from different event study specifications, full service restaurants

		Elasticity with respect to the MW			
		Earnings		Employment	
	Control for trend?	No	Yes	No	Yes
	Avg city R^2	(1)	(2)	(3)	(4)
(1) None	0.59	0.20	0.19	0.25	0.01
(2) Pop	0.59	0.20	0.19	0.20	0.01
(3) Poly(Pop)	0.59	0.20	0.19	0.18	0.01
(4) PopXQtr	0.57	0.20	0.19	0.20	0.01
(5) PopXTime	0.57	0.21	0.19	0.20	0.02
(6) Pop+2007-09 PrivateXTime	0.60	0.20	0.19	0.20	0.01
(7) (Pop+2007-09 Private)XTime	0.59	0.18	0.16	0.22	-0.01
(8) Earnings	0.59	0.20	0.19	0.25	0.01
(9) Poly(Earnings)	0.41	0.20	0.19	0.22	0.01
(10) EarningsXQtr	0.63	0.18	0.17	0.24	0.01
(11) EarningsXTime	0.65	0.14	0.15	0.22	0.02
(12) Employment	0.61	0.19	0.19	0.22	0.01
(13) Poly(Employment)	0.61	0.19	0.19	0.20	0.01
(14) EmploymentXQtr	0.62	0.19	0.19	0.22	0.01
(15) EmploymentXTime	0.62	0.20	0.19	0.22	0.03
(16) Earnings+Employment	0.62	0.19	0.19	0.24	0.02
(17) (Earnings+Employment)XQtr	0.66	0.18	0.18	0.24	0.02
(18) (Earnings+Employment+2007-09 Private)XQtr	0.65	0.18	0.18	0.24	0.02
(19) **(preferred model)** (Earnings+Employment)Xtime	0.67	0.13	0.14	0.22	0.04
(20) (Earnings+Employment+2007-09 Private)XTime	0.67	0.14	0.15	0.23	0.05
(21) Pop+Earnings+Employment	0.61	0.20	0.19	0.22	0.02
(22) (Pop+Earnings+Employment)XQtr	0.63	0.18	0.18	0.21	0.01
(23) (Pop+Earnings+Employment+2007-09 Private)XQtr	0.62	0.18	0.17	0.21	0.01
(24) (Pop+Earnings+Employment)XTime	0.63	0.14	0.14	0.20	0.00
(25) (Pop+Earnings+Employment+2007-09 Private)XTime	0.63	0.13	0.14	0.21	0.02
Summary of elasticities					
(26) Minimum		0.13	0.14	0.18	-0.01
(27) p25		0.18	0.17	0.20	0.01
(28) p50		0.19	0.19	0.22	0.01
(29) p75		0.20	0.19	0.23	0.02
(30) Maximum		0.21	0.19	0.25	0.05

Notes: Reports industrywide elasticities with respect to the minimum wage from alternative model specifications. See notes to Table B2 for more information.

Table B4: Industrywide elasticities from different event study specifications, limited service restaurants

		Elasticity with respect to the MW			
		Earnings		Employment	
	Control for trend?	No	Yes	No	Yes
	Avg city R^2	(1)	(2)	(3)	(4)
(1) None	0.59	0.42	0.35	0.07	0.14
(2) Pop	0.59	0.42	0.35	0.01	0.13
(3) Poly(Pop)	0.59	0.42	0.35	-0.01	0.13
(4) PopXQtr	0.57	0.42	0.35	0.01	0.13
(5) PopXTime	0.57	0.43	0.36	0.00	0.13
(6) Pop+2007-09 PrivateXTime	0.60	0.45	0.36	0.01	0.12
(7) (Pop+2007-09 Private)XTime	0.59	0.43	0.34	0.03	0.10
(8) Earnings	0.59	0.40	0.34	0.08	0.14
(9) Poly(Earnings)	0.41	0.45	0.36	0.05	0.13
(10) EarningsXQtr	0.63	0.39	0.34	0.07	0.13
(11) EarningsXTime	0.65	0.40	0.33	0.07	0.18
(12) Employment	0.61	0.41	0.35	0.03	0.14
(13) Poly(Employment)	0.61	0.43	0.35	0.01	0.14
(14) EmploymentXQtr	0.62	0.41	0.35	0.03	0.14
(15) EmploymentXTime	0.62	0.44	0.36	0.03	0.14
(16) Earnings+Employment	0.62	0.40	0.34	0.06	0.15
(17) (Earnings+Employment)XQtr	0.66	0.39	0.34	0.06	0.15
(18) (Earnings+Employment+2007-09 Private)XQtr	0.65	0.39	0.34	0.06	0.15
(19) **(preferred model)** (Earnings+Employment)Xtime	0.67	0.39	0.32	0.08	0.21
(20) (Earnings+Employment+2007-09 Private)XTime	0.67	0.40	0.31	0.08	0.21
(21) Pop+Earnings+Employment	0.61	0.41	0.35	0.03	0.14
(22) (Pop+Earnings+Employment)XQtr	0.63	0.40	0.34	0.03	0.14
(23) (Pop+Earnings+Employment+2007-09 Private)XQtr	0.62	0.40	0.34	0.03	0.14
(24) (Pop+Earnings+Employment)XTime	0.63	0.40	0.32	0.05	0.18
(25) (Pop+Earnings+Employment+2007-09 Private)XTime	0.63	0.40	0.31	0.05	0.18
Summary of elasticities					
(26) Minimum		0.39	0.31	-0.01	0.10
(27) p25		0.40	0.34	0.02	0.13
(28) p50		0.41	0.34	0.03	0.14
(29) p75		0.43	0.35	0.07	0.15
(30) Maximum		0.45	0.36	0.08	0.21

Notes: Reports industrywide elasticities with respect to the minimum wage from alternative model specifications. See notes to Table B2 for more information.

Table B5: Industrywide elasticities from different event study specifications, professional services

		Elasticity with respect to the MW			
		Earnings		Employment	
	Control for trend?	No	Yes	No	Yes
	Avg city R^2	(1)	(2)	(3)	(4)
(1) None	0.59	0.10	0.05	0.13	-0.05
(2) Pop	0.59	0.10	0.05	0.06	-0.06
(3) Poly(Pop)	0.59	0.10	0.05	0.05	-0.06
(4) PopXQtr	0.57	0.10	0.06	0.06	-0.06
(5) PopXTime	0.57	0.11	0.06	0.06	-0.06
(6) Pop+2007-09 PrivateXTime	0.60	0.12	0.08	0.04	-0.08
(7) (Pop+2007-09 Private)XTime	0.59	0.12	0.06	0.02	-0.10
(8) Earnings	0.59	0.02	0.03	0.10	-0.06
(9) Poly(Earnings)	0.41	0.04	0.03	0.09	-0.06
(10) EarningsXQtr	0.63	0.07	0.07	0.05	-0.10
(11) EarningsXTime	0.65	0.13	0.12	-0.08	-0.11
(12) Employment	0.61	0.10	0.05	0.10	-0.06
(13) Poly(Employment)	0.61	0.12	0.05	0.06	-0.06
(14) EmploymentXQtr	0.62	0.10	0.06	0.10	-0.07
(15) EmploymentXTime	0.62	0.11	0.07	0.08	-0.06
(16) Earnings+Employment	0.62	0.02	0.03	0.10	-0.06
(17) (Earnings+Employment)XQtr	0.66	0.07	0.07	0.06	-0.09
(18) (Earnings+Employment+2007-09 Private)XQtr	0.65	0.07	0.06	0.06	-0.09
(19) **(preferred model)** (Earnings+Employment)Xtime	0.67	0.13	0.12	-0.09	-0.12
(20) (Earnings+Employment+2007-09 Private)XTime	0.67	0.13	0.10	-0.08	-0.14
(21) Pop+Earnings+Employment	0.61	0.02	0.03	0.07	-0.06
(22) (Pop+Earnings+Employment)XQtr	0.63	0.07	0.07	0.02	-0.09
(23) (Pop+Earnings+Employment+2007-09 Private)XQtr	0.62	0.07	0.06	0.03	-0.09
(24) (Pop+Earnings+Employment)XTime	0.63	0.13	0.11	-0.09	-0.13
(25) (Pop+Earnings+Employment+2007-09 Private)XTime	0.63	0.13	0.09	-0.08	-0.15
Summary of elasticities					
(26) Minimum		0.02	0.03	-0.09	-0.15
(27) p25		0.07	0.05	0.02	-0.10
(28) p50		0.10	0.06	0.06	-0.07
(29) p75		0.12	0.07	0.08	-0.06
(30) Maximum		0.13	0.12	0.13	-0.05

Notes: Reports industrywide elasticities with respect to the minimum wage from alternative model specifications. See notes to Table B2 for more information.

C Analysis of full and limited service restaurants and professional services

C.1 Full service and limited service restaurants

This section presents results from other sectors. First, 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. We should therefore find larger effects in limited than in full service restaurants.[59]

Figures C1 and C2 show the earnings and employment effects we measure using event study and synthetic control, respectively, in each sub-sector. We plot the results for full service restaurants in Panel A and limited service in Panel B. In both event study and synthetic control, the earnings effects are larger for limited than full service restaurants—as we would expect. Tables C1 and C2 report the industrywide elasticities from our event study analysis on full and limited service restaurants, respectively. In the trend break model that controls for a linear event time trend (column 2), the industrywide earnings elasticity is 0.32 for limited and 0.15 for full service restaurants. Figure C2 shows that the earnings elasticities recovered by synthetic control show a similar pattern.[60]

Despite the larger earnings effects of the minimum wage in limited service restaurants, both event study and synthetic control find that the policies have not reduced employment in either sector. In fact, if anything, estimates from our trend break model indicate a positive effect. However, we do not find this effect in our synthetic control analysis, in which we measure an elasticity of employment close to zero.[61]

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.

[59]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.

[60]Synthetic control also finds that the effects on earnings are larger for limited than full service restaurants in each of the six cities. See Figures C3–C6.

[61]Similar to our results for food services over all, these estimates are robust to how we specify our event study model (see Appendix B). At the city-level, the synthetic control analyses also do not detect any statistically significant negative effects on limited service employment. We report p-values for the city-level synthetic control results for full and limited service restaurants in Tables C4 and C5. We have also re-run the parallel trends tests that we described in Section 7 for the synthetic control analysis of full service and limited service restaurants. 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.

C.2 Professional services

Next, 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—a high-wage industry that should not be affected by changes in minimum wage policy.[62] 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.

Panel C of Figure C1 shows the results for professional services from our event study models. Although the trend break model measures a positive break in the trend for earnings—suggesting a positive bias in our earnings estimates—the city-specific estimates show that this break appears to be identified off of a single city. Consistent with this interpretation, hypothesis tests reported in Table C3 find that the industrywide elasticity implied by the trend break model is statistically insignificant regardless of how we cluster. Our event study model also does not find any significant effects for employment in professional services.[63]

Evidence from synthetic controls confirm that our analysis is unlikely to be biased by contemporaneous changes in the six cities' local labor markets. These results are shown in Panel C of Figure C2 and Table C6. Of the 12 tests (two for each city), we estimate a significant effect in only one—professional employment in San Francisco. 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.

Over all, the results from our analysis of other sectors 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.

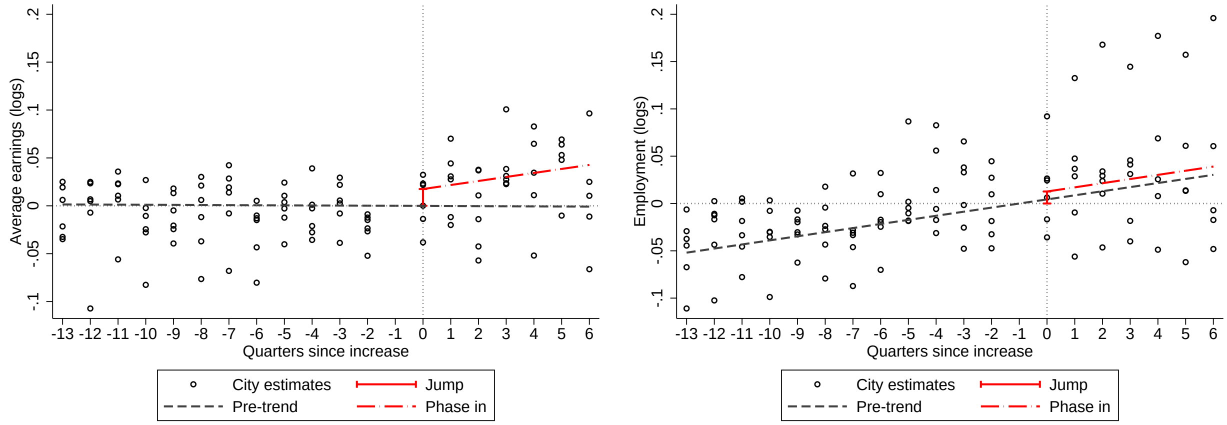
[62] This industry is also called the Professional, Scientific, and Technical Services sector (NAICS code 54). This sector includes lawyers, accountants and management consultants, among other highly educated professions. Table 1 reports that less than 3 percent of workers in the six cities in this industry were covered by the new local minimum wage policies during our study period.

[63] We also find that these elasticities are not robust to how we specify our event study model. In Appendix B, we find the industrywide elasticities for professional services are very sensitive to which control variables we include, especially for earnings. This pattern supports our conclusion that the event study results for professional services are not detecting any changes around the time the local policies went into effect that would bias our estimates in food services.

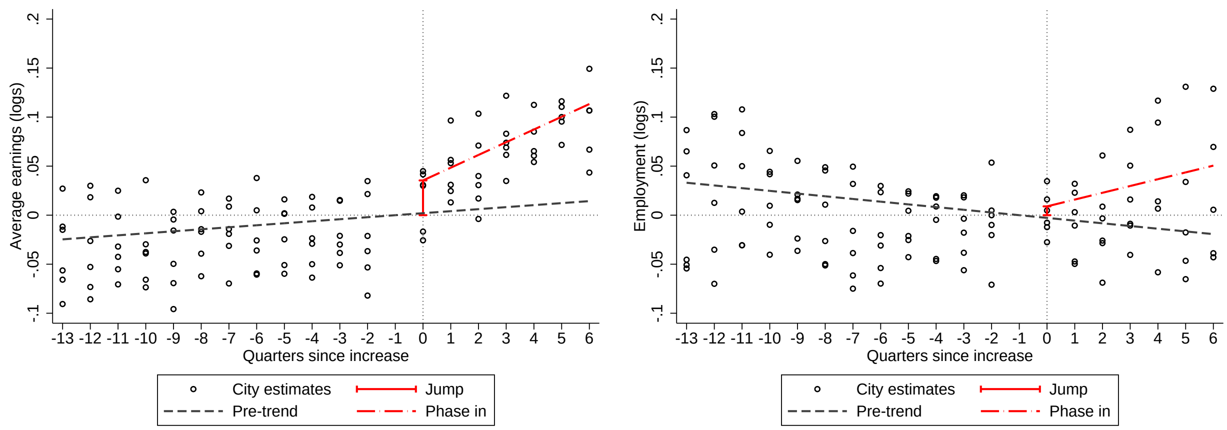
Figure C1: Event study estimates
full and limited service restaurants and professional services

Average earnings | Employment

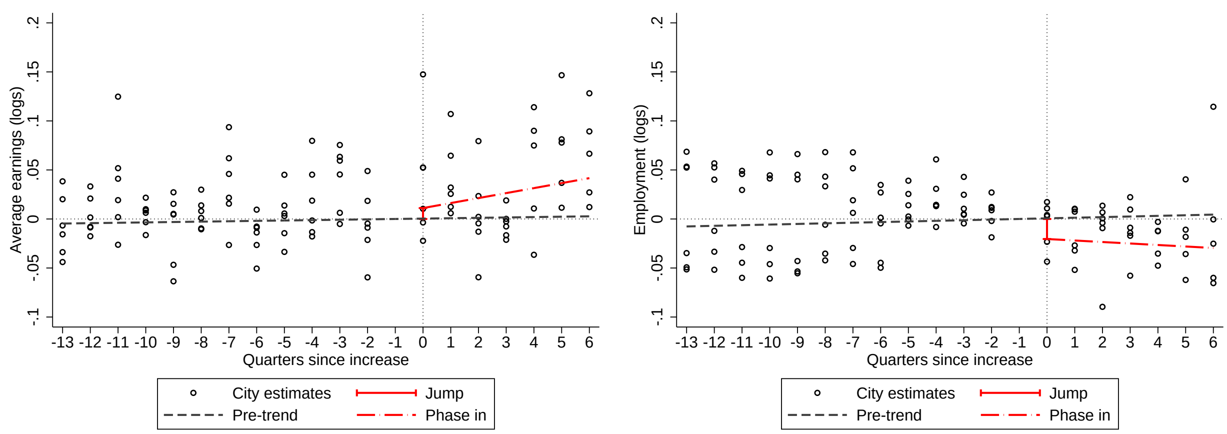
Panel A: Full service restaurants



Panel B: Limited service restaurants

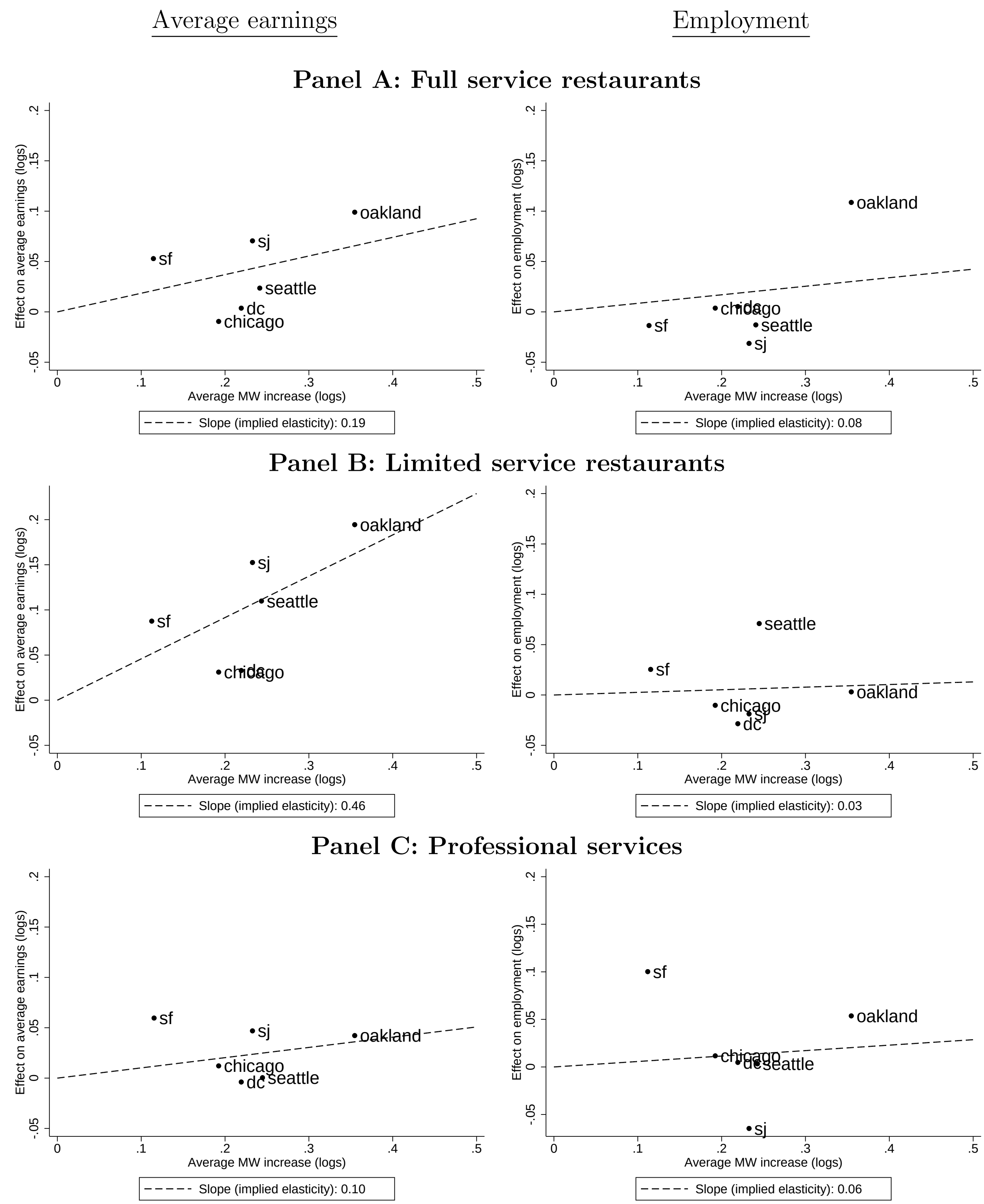


Panel C: Professional services



Notes: This figure plots estimates from event studies. See notes to Figure 4 for more information.

Figure C2: Synthetic control estimates
full and limited service restaurants and professional services

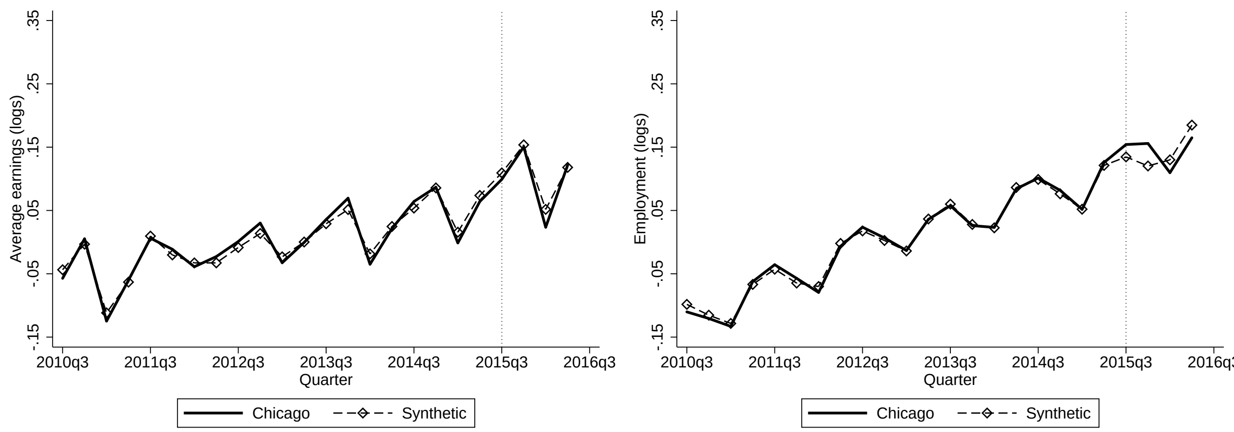


Notes: These figures plot each city's estimated effects of its local minimum wage policy against the city's average minimum wage increase. See note to Figure 9 for more information.

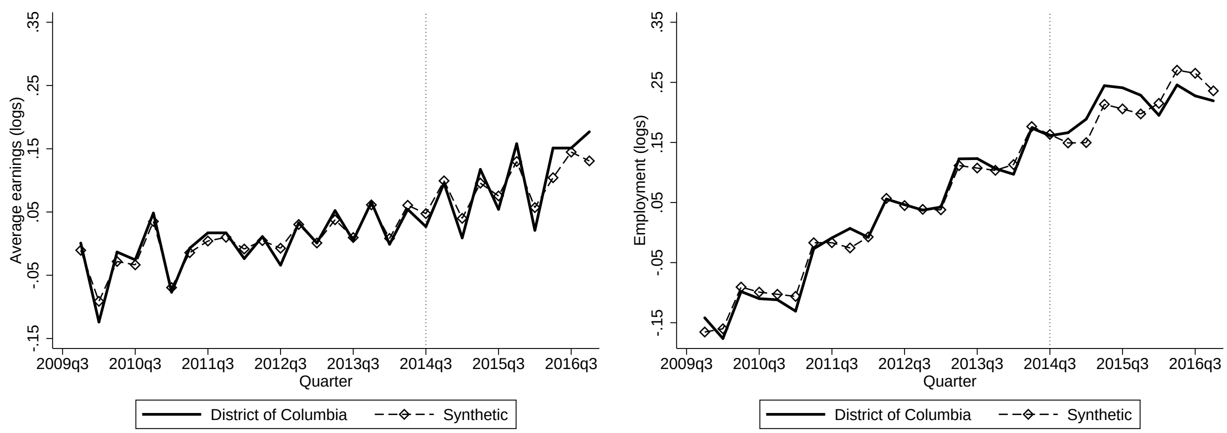
Figure C3: Synthetic control analysis of full service restaurants
Chicago, District of Columbia and Oakland

Average earnings | Employment

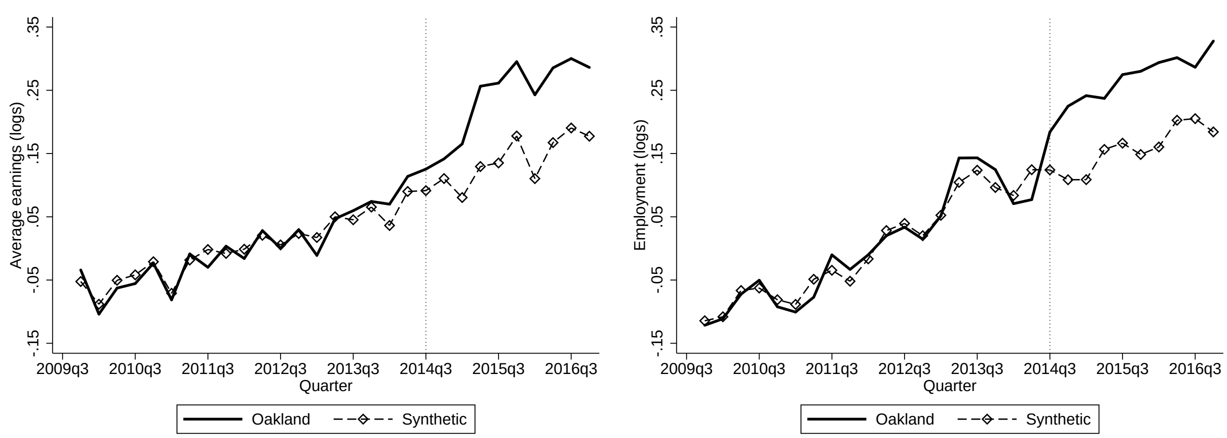
Chicago



District of Columbia

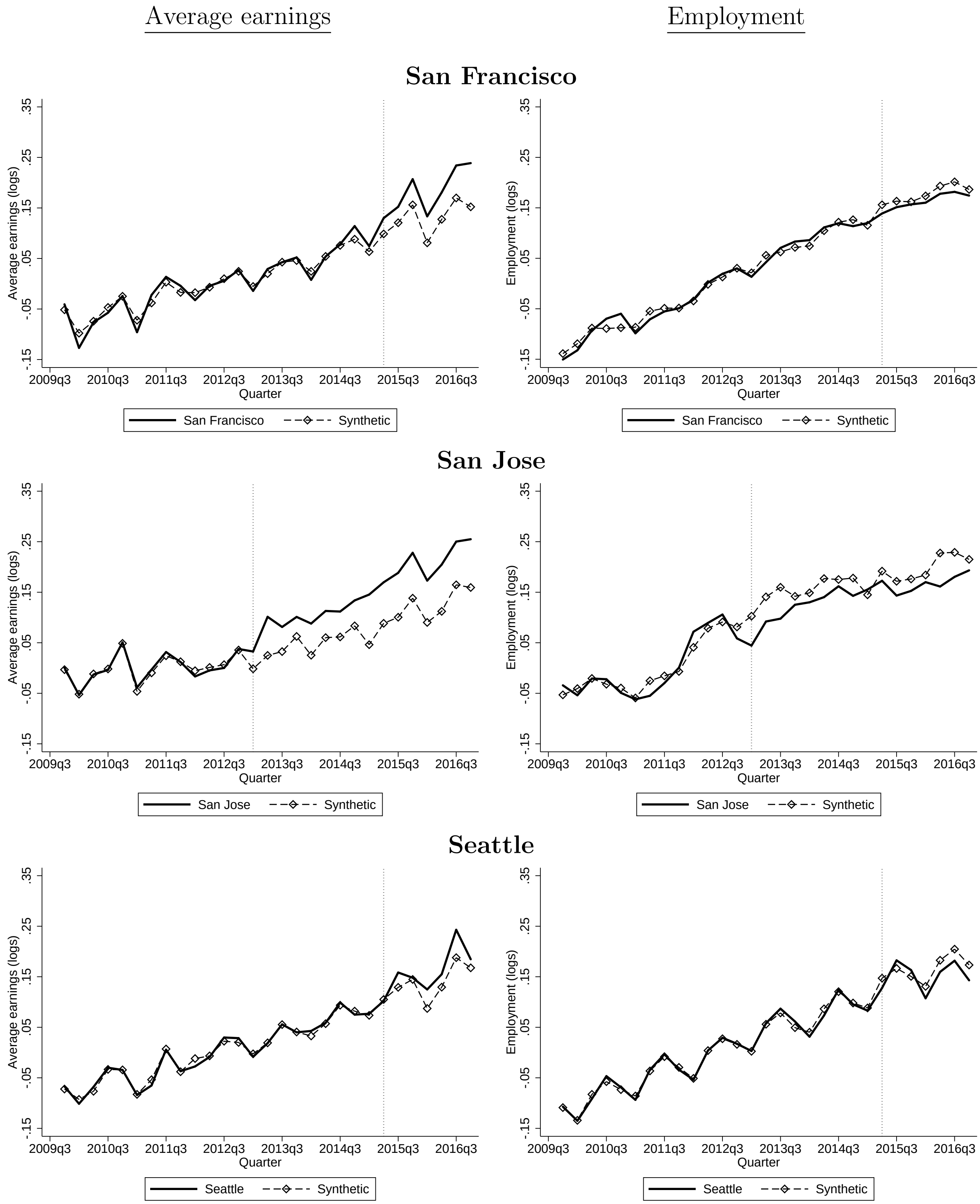


Oakland



Notes: This figure plots average earnings and employment in full service restaurants in Chicago, the District of Columbia and Oakland and their synthetic controls. Each outcome is measured in logs and is centered around its pre-policy average. The vertical dotted line marks the beginning of the evaluation period.

Figure C4: Synthetic control analysis of full service restaurants
San Francisco, San Jose and Seattle

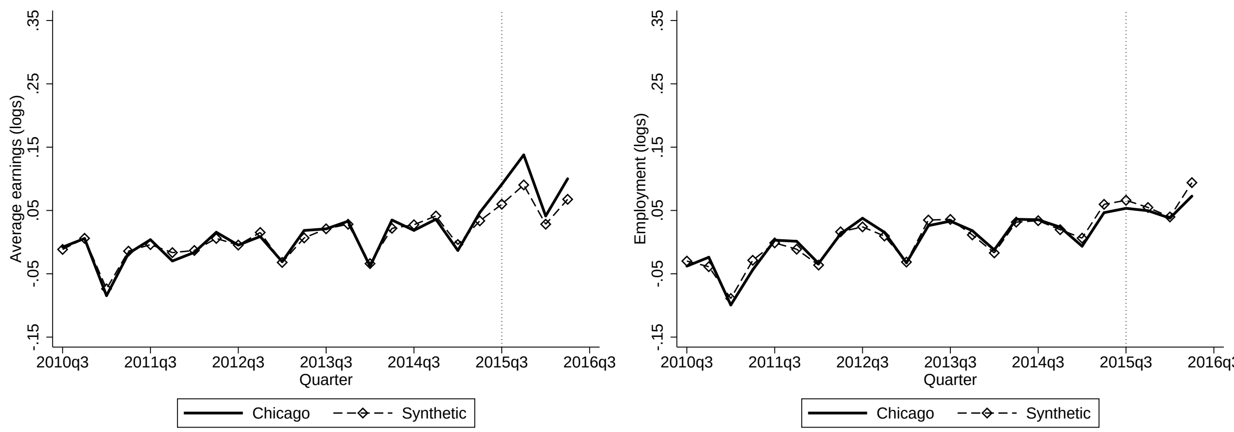


Notes: This figure plots average earnings and employment in full service restaurants in San Francisco, San Jose and Seattle and their synthetic controls. Each outcome is measured in logs and is centered around its pre-policy average. The vertical dotted line marks the beginning of the evaluation period.

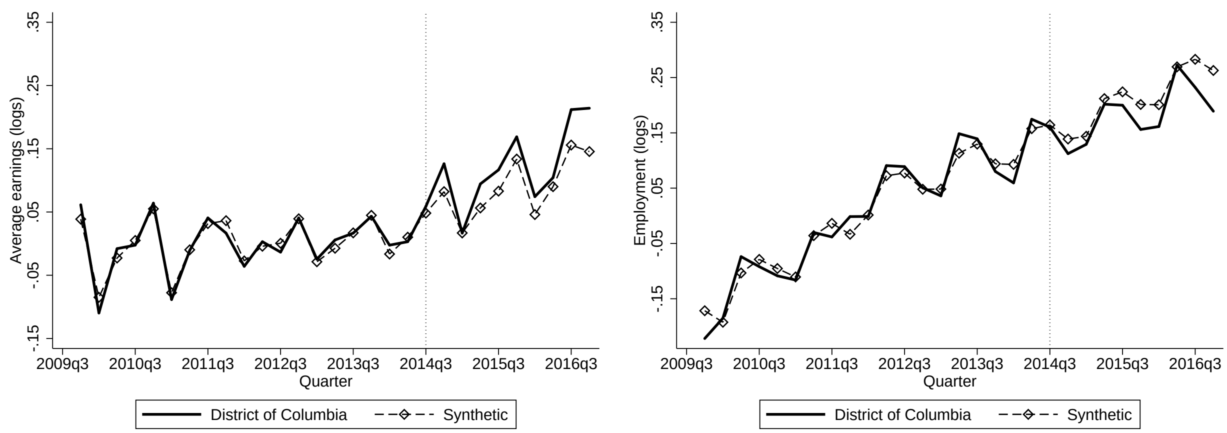
Figure C5: Synthetic control analysis of limited service restaurants
Chicago, District of Columbia and Oakland

Average earnings | Employment

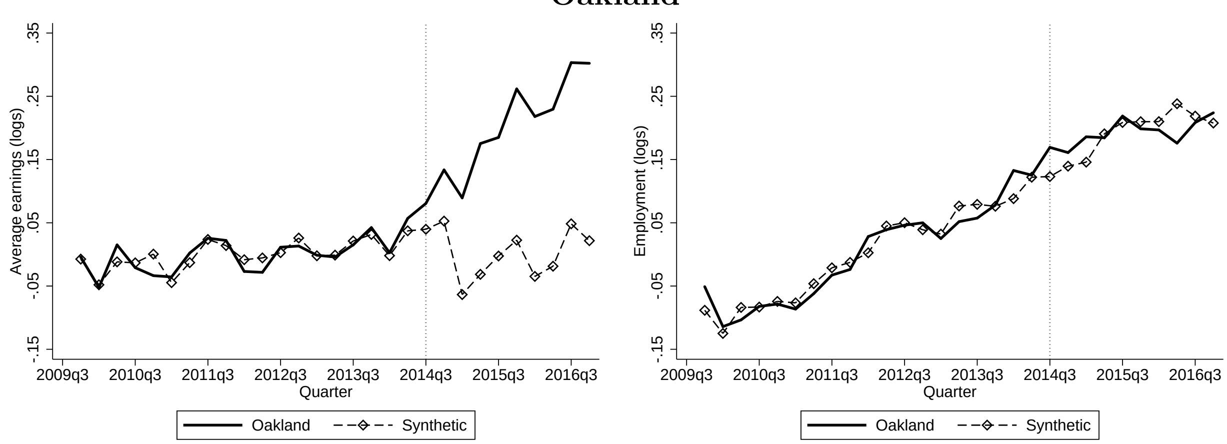
Chicago



District of Columbia

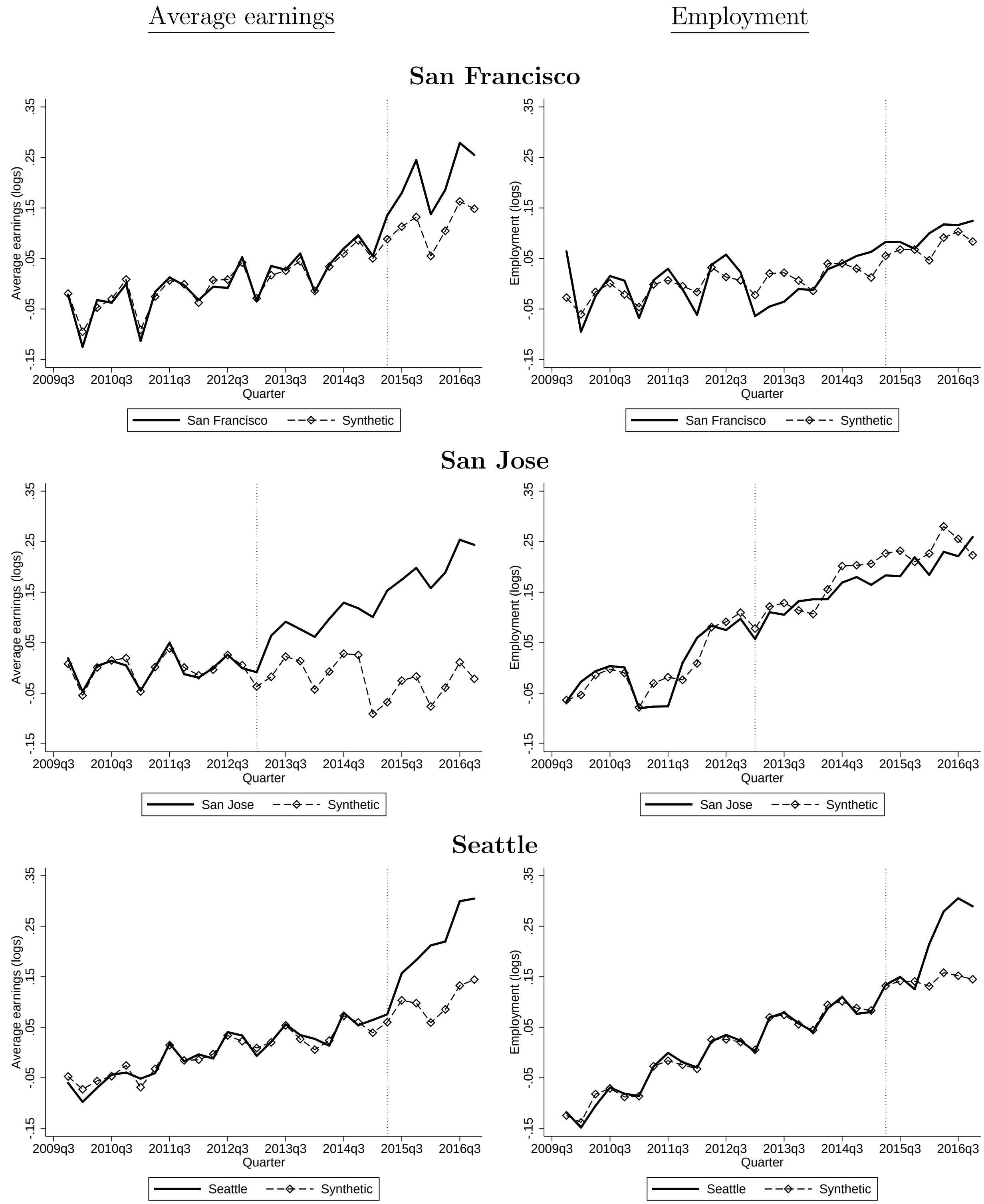


Oakland



Notes: This figure plots average earnings and employment in limited service restaurants in Chicago, the District of Columbia and Oakland and their synthetic controls. Each outcome is measured in logs and is centered around its pre-policy average. The vertical dotted line marks the beginning of the evaluation period.

Figure C6: Synthetic control analysis of limited service restaurants
San Francisco, San Jose and Seattle

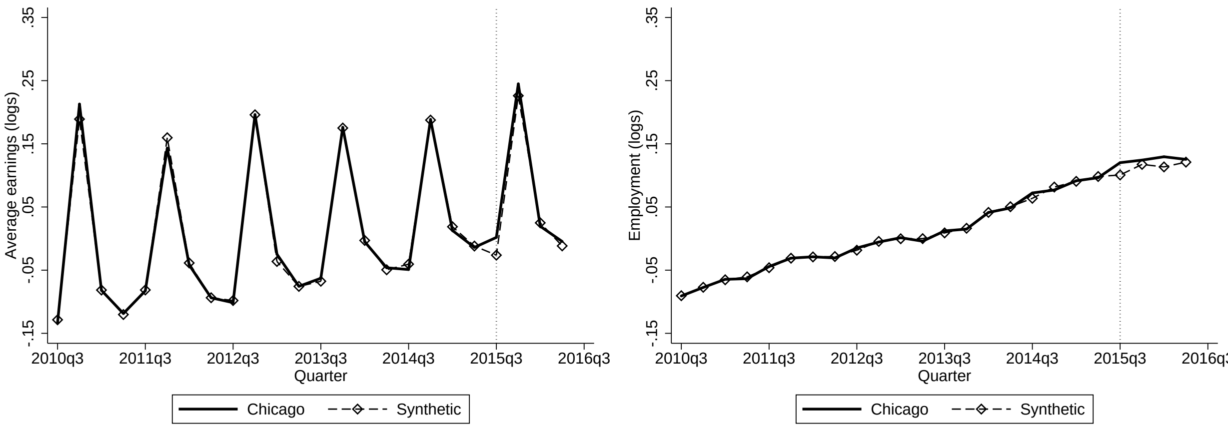


Notes: This figure plots average earnings and employment in limited services restaurants in San Francisco, San Jose and Seattle and their synthetic controls. Each outcome is measured in logs and is centered around its pre-policy average. The vertical dotted line marks the beginning of the evaluation period.

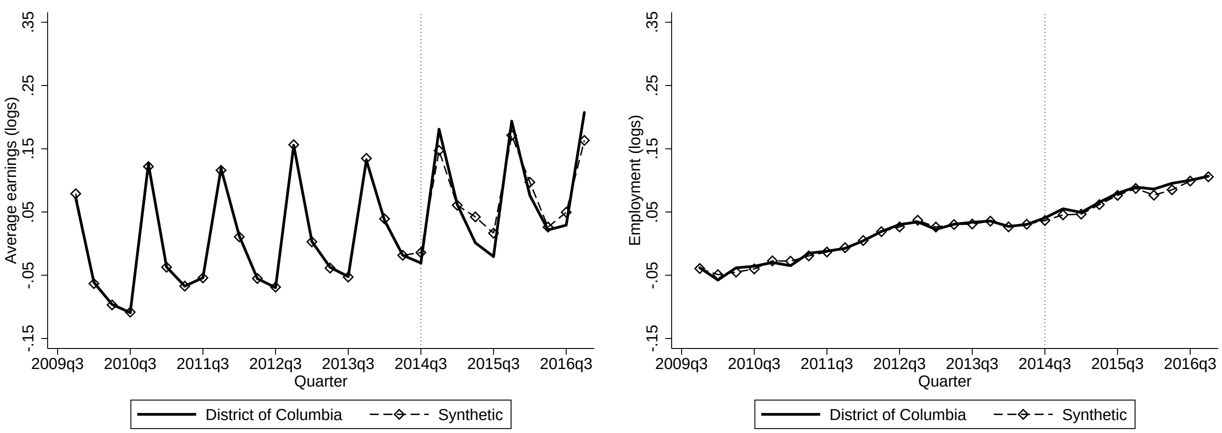
Figure C7: Synthetic control analysis of professional services
Chicago, District of Columbia and Oakland

Average earnings | Employment

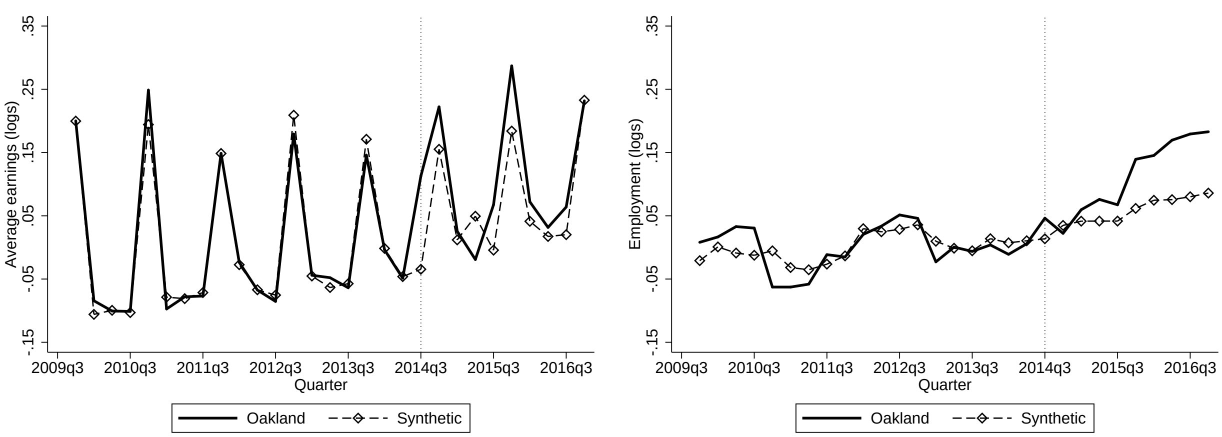
Chicago



District of Columbia

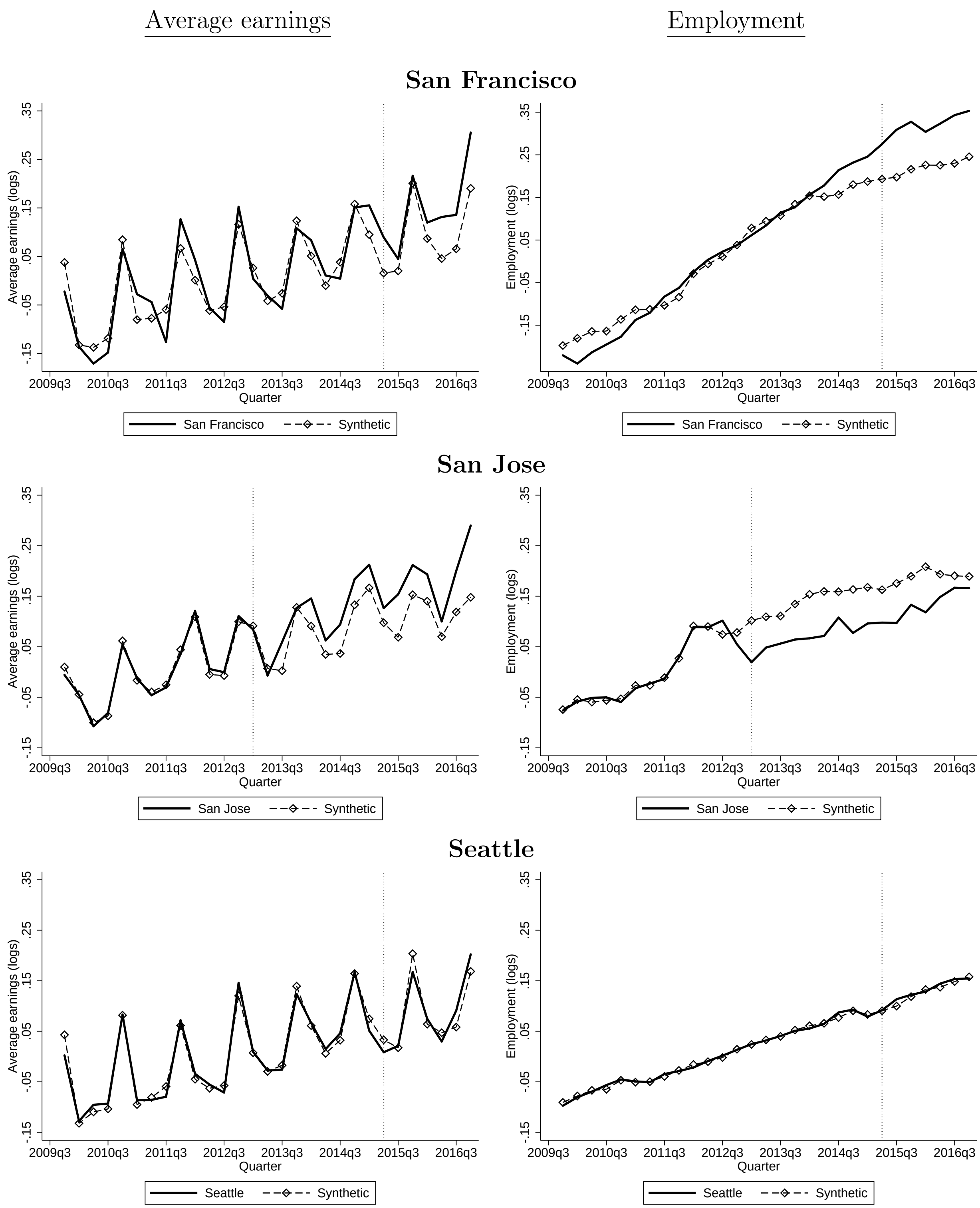


Oakland



Notes: This figure plots average earnings and employment in professional services in Chicago, the District of Columbia and Oakland and their synthetic controls. Each outcome is measured in logs and is centered around its pre-policy average. The vertical dotted line marks the beginning of the evaluation period.

Figure C8: Synthetic control analysis of professional services
San Francisco, San Jose and Seattle



Notes: This figure plots average earnings and employment in professional services in San Francisco, San Jose and Seattle and their synthetic controls. Each outcome is measured in logs and is centered around its pre-policy average. The vertical dotted line marks the beginning of the evaluation period.

Table C1: Event study results for full service restaurants

	Full service restaurants			
	Earnings (logs)		Employment (logs)	
	(1)	(2)	(3)	(4)
Panel A: Point estimates of trend break model[a]				
Jump	0.017	0.018	0.038	0.008
	(0.007)	(0.009)	(0.015)	(0.009)
Phase in	0.004	0.004	0.004	0.000
	(0.004)	(0.003)	(0.003)	(0.003)
p, total event effect = 0 (city clusters)[b]	0.212	0.241	0.156	0.701
p, total event effect = 0 (state clusters)	0.497	0.082	0.034	0.510
Pre-trend	—	0.000	—	0.004
	—	(0.001)	—	(0.001)
p, pre-trend = 0 (city clusters)	—	0.932	—	0.024
p, pre-trend = 0 (state clusters)	—	0.811	—	0.060
Observations	4728	4728	4728	4728
Adjusted R^2	0.82	0.82	0.73	0.73
Panel B: Industrywide elasticity with respect to the minimum wage[c]				
Elasticity	0.133	0.145	0.222	0.035
	(0.073)	(0.056)	(0.090)	(0.062)
p, elasticity = 0 (city clusters)	0.117	0.080	0.060	0.704
90% CI (city clusters)	[-0.006,0.309]	[0.004,0.257]	[0.028,0.419]	[-0.082,0.166]
p, elasticity = 0 (state clusters)	0.528	0.365	0.134	0.559
90% CI (state clusters)	[-0.115,0.260]	[-0.085,0.253]	[-0.018,0.384]	[-0.117,0.135]
Control for trend[d]	No	Yes	No	Yes

Notes: Standard errors in parentheses, clustered at the city and county level. 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. All models include quarterly calendar time effects and city and county effects, and control for private sector earnings and employment. See Section 5.1 for more information. [a] Reports estimates of models specified in Equations (6) and (7). [b] p-values for total event effect test the hypothesis that the coefficients θ^{jump} and $\theta^{phasein}$ are both zero. [c] Earnings and employment elasticities are estimated by instrumental variables. [d] Reports whether the model controls for a linear trend in event time.

Table C2: Event study results for limited service restaurants

	Limited service restaurants			
	Earnings (logs)		Employment (logs)	
	(1)	(2)	(3)	(4)
Panel A: Point estimates of trend break model[a]				
Jump	0.047	0.033	-0.007	0.012
	(0.017)	(0.012)	(0.016)	(0.006)
Phase in	0.013	0.011	0.007	0.010
	(0.004)	(0.004)	(0.004)	(0.005)
p, total event effect = 0 (city clusters)[b]	0.022	0.020	0.447	0.090
p, total event effect = 0 (state clusters)	0.102	0.050	0.432	0.178
Pre-trend	—	0.002	—	-0.003
	—	(0.001)	—	(0.002)
p, pre-trend = 0 (city clusters)	—	0.135	—	0.297
p, pre-trend = 0 (state clusters)	—	0.200	—	0.128
Observations	4728	4728	4728	4728
Adjusted R^2	0.54	0.54	0.82	0.82
Panel B: Industrywide elasticity with respect to the minimum wage[c]				
Elasticity	0.391	0.321	0.076	0.212
	(0.098)	(0.070)	(0.080)	(0.079)
p, elasticity = 0 (city clusters)	0.010	0.008	0.389	0.041
90% CI (city clusters)	[0.224,0.570]	[0.185,0.445]	[-0.108,0.231]	[0.049,0.393]
p, elasticity = 0 (state clusters)	0.037	0.028	0.326	0.108
90% CI (state clusters)	[0.120,0.639]	[0.135,0.481]	[-0.102,0.224]	[-0.006,0.464]
Control for trend[d]	No	Yes	No	Yes

Notes: Standard errors in parentheses, clustered at the city and county level. 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. All models include quarterly calendar time effects and city and county effects, and control for private sector earnings and employment. See Section 5.1 for more information. [a] Reports estimates of models specified in Equations (6) and (7). [b] p-values for total event effect test the hypothesis that the coefficients θ^{jump} and $\theta^{phasein}$ are both zero. [c] Earnings and employment elasticities are estimated by instrumental variables. [d] Reports whether the model controls for a linear trend in event time.

Table C3: Event study results for professional services

	Professional services			
	Earnings (logs)		Employment (logs)	
	(1)	(2)	(3)	(4)
Panel A: Point estimates of trend break model[a]				
Jump	0.013	0.011	-0.017	-0.021
	(0.013)	(0.015)	(0.018)	(0.013)
Phase in	0.005	0.005	-0.002	-0.002
	(0.003)	(0.002)	(0.004)	(0.006)
p, total event effect = 0 (city clusters)[b]	0.276	0.251	0.646	0.532
p, total event effect = 0 (state clusters)	0.413	0.386	0.745	0.421
Pre-trend	—	0.000	—	0.001
	—	(0.001)	—	(0.003)
p, pre-trend = 0 (city clusters)	—	0.780	—	0.817
p, pre-trend = 0 (state clusters)	—	0.658	—	0.781
Observations	4728	4728	4728	4728
Adjusted R^2	0.75	0.75	0.60	0.60
Panel B: Industrywide elasticity with respect to the minimum wage[c]				
Elasticity	0.132	0.123	-0.092	-0.122
	(0.072)	(0.072)	(0.083)	(0.122)
p, elasticity = 0 (city clusters)	0.167	0.173	0.313	0.421
90% CI (city clusters)	[-0.035,0.296]	[-0.021,0.287]	[-0.240,0.073]	[-0.341,0.174]
p, elasticity = 0 (state clusters)	0.622	0.484	0.324	0.132
90% CI (state clusters)	[-0.066,0.270]	[-0.113,0.233]	[-0.301,0.158]	[-0.243,0.042]
Control for trend[d]	No	Yes	No	Yes

Notes: Standard errors in parentheses, clustered at the city and county level. 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. All models include quarterly calendar time effects and city and county effects, and control for private sector earnings and employment. See Section 5.1 for more information. [a] Reports estimates of models specified in Equations (6) and (7). [b] p-values for total event effect test the hypothesis that the coefficients θ^{jump} and $\theta^{phasein}$ are both zero. [c] Earnings and employment elasticities are estimated by instrumental variables. [d] Reports whether the model controls for a linear trend in event time.

Table C4: 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)						
Industrywide elasticity with respect to the minimum wage: 0.19						
Effect of MW increase	-0.010	0.004	0.099	0.053	0.071	0.024
p, effect = 0	0.510	0.890	0.010	0.049	0.030	0.213
90% CI	[-0.045,0.026]	[-0.041,0.048]	[0.057,0.143]	[0.015,0.090]	[0.028,0.113]	[-0.014,0.061]
Test of parallel trends assumption:						
Effect during final pre-policy year[a]	-0.006	-0.006	0.029	0.016	-0.006	0.000
p, effect during final pre-policy year = 0	0.430	0.420	0.020	0.082	0.220	1.000
Pre-policy pseudo R^2	0.955	0.912	0.911	0.949	0.961	0.987
Panel B: Employment (logs)						
Industrywide elasticity with respect to the minimum wage: 0.08						
Effect of MW increase	0.004	0.005	0.109	-0.014	-0.031	-0.013
p, effect = 0	0.880	0.890	0.020	0.705	0.470	0.705
90% CI	[-0.043,0.050]	[-0.049,0.060]	[0.054,0.163]	[-0.080,0.053]	[-0.107,0.048]	[-0.079,0.054]
Test of parallel trends assumption:						
Effect during final pre-policy year[a]	0.005	-0.003	-0.010	-0.003	0.013	-0.008
p, effect during final pre-policy year = 0	0.510	0.670	0.320	0.869	0.090	0.557
Pre-policy pseudo R^2	0.995	0.981	0.943	0.982	0.915	0.992
Counties in donor pool	99	99	99	60	99	60
Pre-policy periods	20	19	19	22	13	22

Notes: Reports synthetic control results for full service restaurant earnings and employment. Significance tests and confidence intervals are based on placebo tests. We base the industrywide elasticities with respect to the minimum wage on the fitted relationship between the estimated effects and the average increase in the minimum wage. See Section 6.1 for more information. [a] 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.

Table C5: 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)						
Industrywide elasticity with respect to the minimum wage: 0.46						
Effect of MW increase	0.031	0.033	0.194	0.088	0.153	0.110
p, effect = 0	0.140	0.290	0.020	0.016	0.030	0.016
90% CI	[-0.005,0.067]	[-0.039,0.104]	[0.122,0.267]	[0.044,0.129]	[0.076,0.229]	[0.071,0.151]
Test of parallel trends assumption:						
Effect during final pre-policy year[a]	-0.001	0.000	0.016	0.011	-0.004	0.009
p, effect during final pre-policy year = 0	0.940	1.000	0.130	0.328	0.370	0.393
Pre-policy pseudo R^2	0.933	0.921	0.714	0.946	0.909	0.928
Panel B: Employment (logs)						
Industrywide elasticity with respect to the minimum wage: 0.03						
Effect of MW increase	-0.010	-0.029	0.003	0.025	-0.019	0.071
p, effect = 0	0.680	0.460	0.920	0.426	0.750	0.098
90% CI	[-0.056,0.035]	[-0.091,0.047]	[-0.062,0.069]	[-0.044,0.095]	[-0.126,0.098]	[0.002,0.140]
Test of parallel trends assumption:						
Effect during final pre-policy year[a]	-0.012	-0.009	0.012	0.023	0.009	-0.006
p, effect during final pre-policy year = 0	0.230	0.280	0.190	0.148	0.130	0.590
Pre-policy pseudo R^2	0.939	0.960	0.938	0.370	0.786	0.988
Counties in donor pool	99	99	99	60	99	60
Pre-policy periods	20	19	19	22	13	22

Notes: Reports synthetic control results for limited service restaurant earnings and employment. Significance tests and confidence intervals are based on placebo tests. We base the industrywide elasticities with respect to the minimum wage on the fitted relationship between the estimated effects and the average increase in the minimum wage. See Section 6.1 for more information. [a] 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.

Table C6: 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)						
Industrywide elasticity with respect to the minimum wage: 0.10						
Effect of MW increase	0.012	-0.004	0.042	0.060	0.047	0.000
p, effect = 0	0.500	0.900	0.180	0.197	0.180	1.000
90% CI	[-0.040,0.064]	[-0.057,0.050]	[-0.011,0.096]	[-0.013,0.174]	[-0.021,0.115]	[-0.072,0.085]
Test of parallel trends assumption:						
Effect during final pre-policy year[a]	-0.008	-0.003	-0.013	0.016	0.012	0.000
p, effect during final pre-policy year = 0	0.410	0.740	0.340	0.295	0.200	1.000
Pre-policy pseudo R^2	0.995	0.999	0.977	0.860	0.982	0.969
Panel B: Employment (logs)						
Industrywide elasticity with respect to the minimum wage: 0.06						
Effect of MW increase	0.012	0.005	0.054	0.100	-0.065	0.003
p, effect = 0	0.670	0.850	0.280	0.066	0.480	0.951
90% CI	[-0.047,0.071]	[-0.076,0.086]	[-0.024,0.137]	[0.023,0.196]	[-0.229,0.100]	[-0.080,0.087]
Test of parallel trends assumption:						
Effect during final pre-policy year[a]	0.000	0.001	-0.010	0.052	0.000	0.003
p, effect during final pre-policy year = 0	0.980	0.750	0.270	0.098	0.520	0.787
Pre-policy pseudo R^2	0.997	0.986	0.430	0.959	0.969	0.995
Counties in donor pool	99	99	99	60	99	60
Pre-policy periods	20	19	19	22	13	22

Notes: Reports synthetic control results for professional service earnings and employment. Significance tests and confidence intervals are based on placebo tests. We base the industrywide elasticities with respect to the minimum wage on the fitted relationship between the estimated effects and the average increase in the minimum wage. See Section 6.1 for more information. [a] 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.