Center on Wage and Employment Dynamics

Chairs
Sylvia A. Allegretto
Michael Reich
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University of California, Berkeley

Seattle's Minimum Wage Experience 2015-16

By Michael Reich, Sylvia Allegretto, and Anna Godoey

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Michael Reich is a Professor at UC Berkeley and Co-Chair of the Center on Wage and Employment Dynamics (CWED) at UC Berkeley's Institute for Research on Labor and Employment. Dr. Sylvia Allegretto is a Research Economist and Co-Chair of CWED. Dr. Anna Godoey is a post-doctoral researcher at CWED. *We acknowledge support from the University of California and the Ford Foundation and we are grateful to Arindrajit Dube and Ben Zipperer for their sage advice. This report was prepared at the request of the Office of the Mayor of Seattle.*

ABSTRACT

This brief on Seattle's minimum wage experience represents the first in a series that CWED will be issuing on the effects of the current wave of minimum wage policies—those that range from $12 to $15. Upcoming CWED reports will present similar studies of Chicago, Oakland, San Francisco, San Jose and New York City, among others. The timing of these reports will depend in part upon when quality data become available. We focus here on Seattle because it was one of the early movers.

Seattle implemented the first phase of its minimum wage law on April 1, 2015, raising minimum wages from the statewide $9.47 to $10 or $11, depending upon business size, presence of tipped workers and employer provision of health insurance. The second phase began on January 1, 2016, further raising the minimum to four different levels, ranging from $10.50 to $13, again depending upon employer size, presence of tipped workers and provision of health insurance. The tip credit provision was introduced into a previously no tip credit environment. Any assessment of the impact of Seattle's minimum wage policy is complicated by this complex array of minimum wage rates. This complexity continues in 2017, when the range of the four Seattle minimum wages widened, from $11 to $15, and the state minimum wage increased to $11.

We analyze county and city-level data for 2009 to 2016 on all employees counted in the Quarterly Census of Employment and Wages and use the "synthetic control" method to rigorously identify the causal effects of Seattle's minimum wage policy upon wages and employment. Our study focuses on the Seattle food services industry. This industry is an intense user of minimum wage workers; if wage and employment effects occur, they should be detectable in this industry. We use county level data from other areas in Washington State and the rest of the U.S. to construct a synthetic control group that matches Seattle for a nearly six year period before the minimum wage policy was implemented. Our methods ensure that our synthetic control group meets accepted statistical standards, including not being contaminated by wage spillovers from Seattle. We scale our outcome measures so that they apply to all sectors, not just food services.

Our results show that wages in food services did increase—indicating the policy achieved its goal—and our estimates of the wage increases are in line with the lion's share of results in previous credible minimum wage studies. Wages increased much less among full-service restaurants, indicating that employers made use of the tip credit component of the law. Employment in food service, however, was not affected, even among the limited-service restaurants, many of them franchisees, for whom the policy was most binding. These findings extend our knowledge of minimum wage effects to policies as high as $13.

PART 1 INTRODUCTION

Minimum wage policy in the U.S. has entered a new wave of state and local activity, in response to over a decade of inaction at the federal level. As of June 2017, nine large cities and eight states have enacted minimum wage policies in the $12 to $15 range. San Francisco's minimum wage will increase to $14 on July 1, 2017 and to $15 on July 1, 2018. Seattle's 2017 minimum wage ranges from $11 to $15 and will reach $15 for all employers in 2021. Dozens of smaller cities and counties have also enacted wage standards in this range. These higher standards, which will be gradually phased in, already cover well over 20 percent of the U.S. workforce. And a substantial number of additional cities and states are poised to soon enact similar policies.

These minimum wage levels substantially exceed the previous peak in the federal minimum wages, which reached just under $10 (in today's dollars) in the late 1960s. These new policies will also raise pay substantially for a large share of the workforce—roughly 30 percent in most areas and as much as 40 to 50 percent of the workforce in some jurisdictions. By contrast, individual minimum wage increases in the period 1984-2014 increased pay for less than 10 percent of the workforce.[1]

Although minimum wage effects on employment have been much studied—and debated, this new wave of policy initiatives reaches levels that lie well beyond the reach of previous studies. To better inform public discussion, CWED is studying and will report on the effects of the new wave of minimum wage policies in as close to real time as is possible.

This brief represents the first of a number of reports that CWED plans to issue on this topic. Their timing and coverage will be determined by the phase-in schedules of each jurisdiction and the availability of sufficient post-policy data to make credible assessments. We begin with Seattle because it was one of the first movers in this new wave of minimum wage policies.

We begin by reviewing briefly how economists have studied minimum wage effects. Part 2 describes the Seattle policies; Part 3 describes our methods and findings. Appendix A provides our conceptual framework of how minimum wages affect an economy; Appendix B lists the counties that we use for our comparisons with Seattle.

Background: How economists study minimum wage effects on employment

Ever since George Stigler's pioneering 1946 essay, "The Economics of Minimum Wage Legislation," economists have used the familiar downward-sloping labor demand curve of Econ 101 as the conceptual framework to analyze the expected employment effects of minimum wages. In this framework, a higher wage floor implies that a smaller amount of labor will be demanded. The size of

[1] Nonetheless, $15 is insufficient, anywhere in the U.S., to allow a livable wage for households with children—even when supplemented by safety net programs such as food stamps or the Earned Income Tax Credit.

the disemployment effect depends upon how elastic labor demand is to wages. This elasticity is determined both by the slope of the demand curve and the relevant point on the line, since each point on a given labor demand curve represents a different elasticity. On a given curve, demand elasticities are smaller at lower wages and higher at higher wages. Stigler's framework thus leaves open the possibility that the wage gains of those receiving increases could be greater or smaller than the wage losses of those losing their jobs. Further, Stigler recognized that higher minimum wages could generate positive employment effects when employers possessed some power to set wages. Yet Stigler's analysis provided only a partial analysis based upon the effects of a minimum wage increase in a single industry. A more expanded analysis, which adds the effects of higher minimum wages upon worker purchasing power and consumer demand, finds that minimum wage effects upon employment can be positive or negative.[2]

Given these ambiguities in the theory's predictions, labor economists turned their attention to empirical studies to estimate the actual employment effects of minimum wages. Since the 1990s alone, economists have conducted hundreds of such studies (Bellman and Wolfson 2016). Some find a very small negative employment effect, while others find an effect that is difficult to distinguish from zero.

Almost all of these studies utilize a "difference-in differences" framework that has become standard in empirical economics (Angrist and Pischke 2009). This phrase refers to two sets of differences, each measuring changes in an outcome before and after a policy intervention, but in different areas, one that received the policy treatment and one that did not. The policy intervention in our case is a minimum wage change; the outcomes of interest are actual pay levels and employment among low-wage workers.

A key challenge in these studies is to identify a comparable area—or group—that did not experience the policy. We want to avoid control groups that are influenced by other changes, such as local economic conditions, that might be correlated with but not caused by minimum wage changes. Ideally, we would split the population randomly into two parts—a treatment group that would be given minimum wage increases, and a control group that would not. We could then be assured that differences in the outcomes between these two groups reflected only the causal effects of the treatment.

Of course, randomization is not feasible in the real world of minimum wage policies. Economists have therefore devised different strategies to ensure that our findings reflect causation and not correlation. The outcomes of differing minimum wage studies often vary simply because they use different methods and standards to define their comparison group.

In the past decade, the field of econometrics has made major advances—often known as the "credibility revolution"—that codify the best methodological practices in such studies (Angrist and

[2] We present a revised and expanded conceptual framework for analyzing minimum wages effects in Appendix A.

Pischke 2009). In particular, econometricians emphasize that a treatment and control study should pass three simple but very important tests:

1. The treatment and control groups should behave similarly in the pre-treatment period. This principle is often referred to as the parallel trends assumption. It is important to pass this test to rule out confounding factors that produce a biased causal estimate. The test is stronger when the pre-trend study period is much longer than the period of the post-trend time period.

2. The treatment should have a detectable effect on the treated group but not on the control group. That is, the minimum wage should have increased pay on the treated group by a detectable amount. Otherwise, there should be no expectation of a detectable effect on employment.

3. Groups that did not get a treatment should not exhibit any treatment effects. That is, minimum wages should not have any effects on high-paid groups or on areas that did not experience a minimum wage change. This principle is often examined by administering a "placebo" treatment to the control group.

CWED researchers and affiliates—and others—have reviewed many of the recent studies that obtain negative minimum wage effects. We find that these studies do not conform to one or more of the above three principles. When we deploy methods that do meet these principles—such as by comparing contiguous border county pairs that straddle a state line with a minimum wage difference, we find substantial wage effects but only very small or nonexistent negative employment effects.[3]

Some labor economists nonetheless continue to dispute whether adjoining areas make good comparison groups (Neumark, Salas and Wascher 2014). In response, we and other researchers have used a relatively new method to analyze minimum wage policies, called synthetic controls (Dube and Zipperer 2015; Allegretto, Dube, Reich and Zipperer 2017). This method, when properly deployed, is designed to generate the best control group possible by using an objective data-generated algorithm. We describe further and then use the synthetic control method in Part 3 of this report. Synthetic control methods, when not properly used, may not meet all of the three basic principles above. Under such conditions, they can give misleading results.

[3] See Allegretto, Dube, Reich and Zipper 2017 as well as Zipperer 2016 for examples.

PART 2 SEATTLE'S POLICY TIMETABLE AND COVERAGE

Table 1 displays Seattle's effective minimum wages from 2010 to 2022. We include the years from 2010 on as our study period begins then.

The citywide minimum wage law was enacted on June 20, 2014 and first implemented on April 1, 2015. As Table 1 shows, Seattle adopted a long phase-in policy, with a complex schedule. Two different minimum wages applied in 2015—$10 and $11, depending on size of employer, provision of medical benefits for employees and, for firms with 500 or fewer employees, whether employees receive tips. The law measures employer size using the firm's national employment, not employment just in Seattle, and it defined franchises as part of larger business entities for this purpose. These 2015 rate increases amount to increases of 5.6 percent and 16.2 percent, respectively, from the 2015 state minimum wage of $9.47.

Table 1 Seattle minimum wage timeline

	Large firms (500+)		Small firms (500 or fewer)	
Date	No health insurance	Health insurance	No health insurance, no tips	Health insurance /tips
January 1, 2010[a]	$8.55	$8.55	$8.55	$8.55
January 1, 2011[a]	$8.67	$8.67	$8.67	$8.67
January 1, 2012[a]	$9.04	$9.04	$9.04	$9.04
January 1, 2013[a]	$9.19	$9.19	$9.19	$9.19
January 1, 2014[a]	$9.32	$9.32	$9.32	$9.32
January 1, 2015[a]	$9.47	$9.47	$9.47	$9.47
April 1, 2015[b]	$11.00	$11.00	$11.00	$10.00
January 1, 2016	$13.00	$12.50	$12.00	$10.50
January 1, 2017	$15.00	$13.50	$13.00	$11.00
January 1, 2018	Indexed	$15.00	$14.00	$11.50
January 1, 2019	Indexed	Indexed	$15.00	$12.00
January 1, 2020	Indexed	Indexed	Indexed	$13.50
January 1, 2021	Indexed	Indexed	Indexed	$15.00
January 1, 2022	Indexed	Indexed	Indexed	Indexed

Notes: a.Seattle followed Washington State's minimum wage, which was indexed each year. b.Initiative 1433 went into effect on April 1, 2015. Employers of tipped workers receive a $1 tip credit in 2015 and a $2 tip credit in 2016. After the minimum wage reaches $15, it will be adjusted each year on January 1, based on the CPI for the Seattle-Tacoma-Bremerton Area.

Four different mandated wage standards were introduced on January 1, 2016, varying from $10.50 to $13, again depending upon employer size, provision of medical benefits and, for firms with fewer than 500 employees, whether the employees received tips. These increases ranged from 5 percent to 22

percent. The state minimum wage did not increase in 2016, even though it is indexed each year, as the CPI was unchanged. All Seattle employers will face at least a $15 minimum wage in 2021.

On January 1, 2017, the minimum wage range among Seattle employers became even wider, extending from $11 to $15. Meanwhile, a statewide November 2016 ballot initiative raised the state minimum wage to $11 in 2017, to be increasing further to $13.50 by 2020.

Seattle's complex schedule, which does not appear in other $15 citywide minimum wage ordinances, makes it difficult to compute an average minimum wage effect for each year, as we lack data on how many employees fall under each of the four categories. Our data also do not permit us to discern whether individual employers actually adopted the minimum that applied to them, nor whether employees responded to these differences by moving to employers that had to pay higher minimums.

These are important issues, in part because Seattle's franchise businesses, which employ about six percent of all private sector workers, according to the International Franchise Association (IFA), contested their inclusion in the large employer category. Many of the franchises are limited-service restaurants (think fast food chains) and many of the franchisees own multiple stores. The IFA sued the city, arguing that it was unfair to include these businesses among large employers just because their franchisor employed 500 employees or more throughout the U.S. Despite losing in lower courts, the franchises' minimum wage requirements remained uncertain until May 2016, when the U.S. Supreme Court refused to hear the case (Reuters May 2, 2016).

The Seattle policy instituted an allowable subminimum wage (lower than the regular minimum wage) to be paid to workers who customarily and regularly receive tips—such as wait staff and bartenders. The sub-wage hinges on a tip credit provision—the amount of the wage bill that an employer can pass on to customers in the form of tips. This provision effectively limited the minimum cash wage for restaurant servers to $10 in 2015 and 2016, giving employers a tip credit of $1 in 2015 and $2 in 2016.

This introduction of a tip credit for employers, aka a subminimum wage for tipped workers, into a previously non-tip credit policy environment in Seattle is extremely rare, perhaps unique. Previous research using panel data has shown that cash wages are indeed lower in states with greater tip credits without creating more employment (Allegretto and Nadler 2015). Our data permits us to distinguish differences in wage and employment effects between limited- and full-service restaurants. Since limited-service restaurants by definition rarely employ tipped servers, we may be able to observe the effects of introducing a tip credit on employer-provided pay in Seattle.

PART 3 SYNTHETIC CONTROL ANALYSES

Data and Methods

Data

We use the Bureau of Labor Statistics' Quarterly Census on Employment and Wages (QCEW) administrative data for our analysis. The QCEW tabulates employment and wages of all business establishments that belong to the Unemployment Insurance (UI) system. The UI system covers about 97 percent of all wage and salary civilian employment. We obtained QCEW data from 2009q4 through 2016q1, for all counties in the U.S., from the website of the U.S. Bureau of Labor Statistics. We obtained Seattle city-level QCEW tabulations from Seattle's Office of Economic and Financial Analysis.

The coverage of the QCEW is thus much more complete than household or employer surveys. But like all datasets, it is not perfect. QCEW data can be noisy for areas smaller than a county, insofar as businesses change location or their name. Moreover, some multi-site businesses report payroll and head counts separately for each of their locations, while others consolidate their data and provide information as if their business operated only at a single location. Moreover, the Bureau of Labor Statistics recently began to organize data spatially by geocodes (exact addresses), rather than by zip codes. Postal zip codes do not exactly match city boundaries. In some cities these changes affected both how multi-unit businesses report their results and whether some businesses were located in the city. Our tests find that the statistical noise level in the city-level Seattle QCEW data was very low.

Finally, QCEW data do not include independent contractors, such as Uber and Lyft drivers. The number of such workers has grown in Seattle in recent years, and faster than in other areas of the U.S. (Seattle Minimum Wage Team 2016b). This growth is unrelated to minimum wage policy and thus should not affect our analysis.

Outcomes

Our main outcomes of interest are average weekly wages (reported quarterly) and employment (reported monthly).[4] We construct the average weekly wage variable using the ratio of total industry payroll to employment; it thus reflects both the hourly wage paid to workers and the number of hours worked every week. Employers who react to the minimum wage increase by reducing employee hours will thus impart a negative effect on our wage measure. In the presence of negative effects on hours, our estimated effects on wages represent a lower bound on the true wage effect. However, studies that have hours data (including Seattle Minimum Wage Team 2016a, b), find a very small hours effect.

[4] We obtain the average weekly wage by dividing total payroll by average employment and then dividing by 13 weeks for a quarterly measure. Monthly employment counts only filled jobs, whether full or part-time, temporary or permanent, by place of work on the twelfth of the month.

We focus our analysis on the food service/restaurant industry because it is the most intensive employer of the minimum wage workforce. We examine wages both to determine if there is a treatment effect (which assures us we are analyzing an affected industry) and to quantitatively estimate the increase in worker pay. We report employment and wage outcomes for the major industry category of Food Services and Drinking Places, the combined subsectors of Full Service (FSR) and Limited Service Restaurants (LSR), and separately for the two latter industries.[5]

Wage increases and employment effects in food services are likely to be larger than in other industries, precisely because it has the highest proportion of low-wage workers affected by the minimum wage policy. Therefore, as is standard in minimum wage research, we express our outcome measures as elasticities rather than as absolute changes. Minimum wage elasticities measure the percent change in an outcome, such as actual wages or employment, for a one percent change in the minimum wage. We also report the labor demand elasticity, which is the ratio of the employment elasticity to the wage elasticity. With these scaling, that results from the food services industry are comparable to results for all minimum wage jobs.

Methods

We evaluate the causal effects of minimum wages on wages and employment by using synthetic control estimation. While we can observe wages and employment directly in Seattle, we cannot observe how wages and employment would have evolved if Seattle had not implemented its minimum wage policies. To evaluate the policy empirically, we estimate a counterfactual—what would have happened in a counterfactual or "Synthetic" Seattle, made up of a weighted average of donor counties, that did not raise their minimum wage standards. More precisely, the synthetic control method estimates the counterfactual outcomes by constructing an optimally-weighted average of counties in non-treated areas that track pay and employment trends in pre-treatment Seattle.[6] The data-driven nature of this procedure reduces the role of subjective judgment by the researchers in determining the appropriate control region.

We specify a pool of potential donor counties that have similar population size, and which come only from states that, like Washington, index their minimum wages each year, but did not experience any other changes to the minimum wage during the study period. We are thus careful to ensure (unlike Neumark, Salas and Wascher 2014) that our pool of synthetic donor counties is not contaminated by minimum wage increases.

As Appendix B shows, the synthetic control algorithm picks mainly donor counties that are outside Washington State. This result contrasts with previous studies (Dube and Zipperer 2015), which may reflect idiosyncrasies of the Seattle area. In particular, other areas of Washington (outside of King

[5] Food Services and drinking places (NAICS 722), Full Service Restaurants (NAICS 722110 pre-2011, 722511 in 2011+) and Limited Service Restaurants (NAICS 722211 pre-2011, 722513 in 2011+).

[6] A more formal discussion of the synthetic control methods used in these studies will be available in a forthcoming working paper. For insight and intuition regarding this method, see Abadie et al. 2010.

County) are quite dissimilar to Seattle itself. In any case, the large distance between Seattle and the most highly-weighted donors ensures that wage spillovers from Seattle do not contaminate our synthetic control. We are also careful to construct independent synthetic controls for each outcome.

We use as long a period as possible to construct the synthetic control for the time period that runs up close to, but not right at, the minimum wage increase (the "learning" period). We then test to ensure that we can actually obtain a good synthetic Seattle by a) examining the goodness of fit for the outcomes during the learning period and b) testing the goodness of fit for quarters that fall between the learning period and when the treatment is introduced.

We then estimate minimum wage effects by comparing post-treatment outcomes in Seattle with post-treatment outcomes in our Synthetic Seattle. For each outcome, we calculate point estimates as the difference between the outcome in Seattle and Synthetic Seattle, averaged over the post-treatment period and relative to the average outcome in Synthetic Seattle. We then calculate elasticities by scaling the point estimates using the corresponding minimum wage changes.

To assess the statistical significance of these effects, we follow the usual approach in the literature, estimating a series of placebo models for untreated donors. By construction, there have been no changes in minimum wage policies in the donor counties, so any apparent effect on wages or employment are caused by random variation. By looking at the share of donor counties that show apparent wage or employment effects greater than that in Seattle, we obtain an indication of the statistical significance of the estimated effects. For each estimate, we construct the percentile rank statistic as the rank of the estimated treatment effect divided by the number of donors +1. If $p<0.025$ or $p>0.975$, the estimated effect is significant at the 5 percent level.

Key findings

Wage effects

Figure 1 below presents our synthetic control results for the wage effect of the Seattle minimum wage law. Our data begin in 2009q4 and end in 2016q1. The dashed vertical line represents the time of implementation of the first phase of the policy—in April 2015. The second phase began in January 2016. The data have been seasonally corrected using standard procedures.

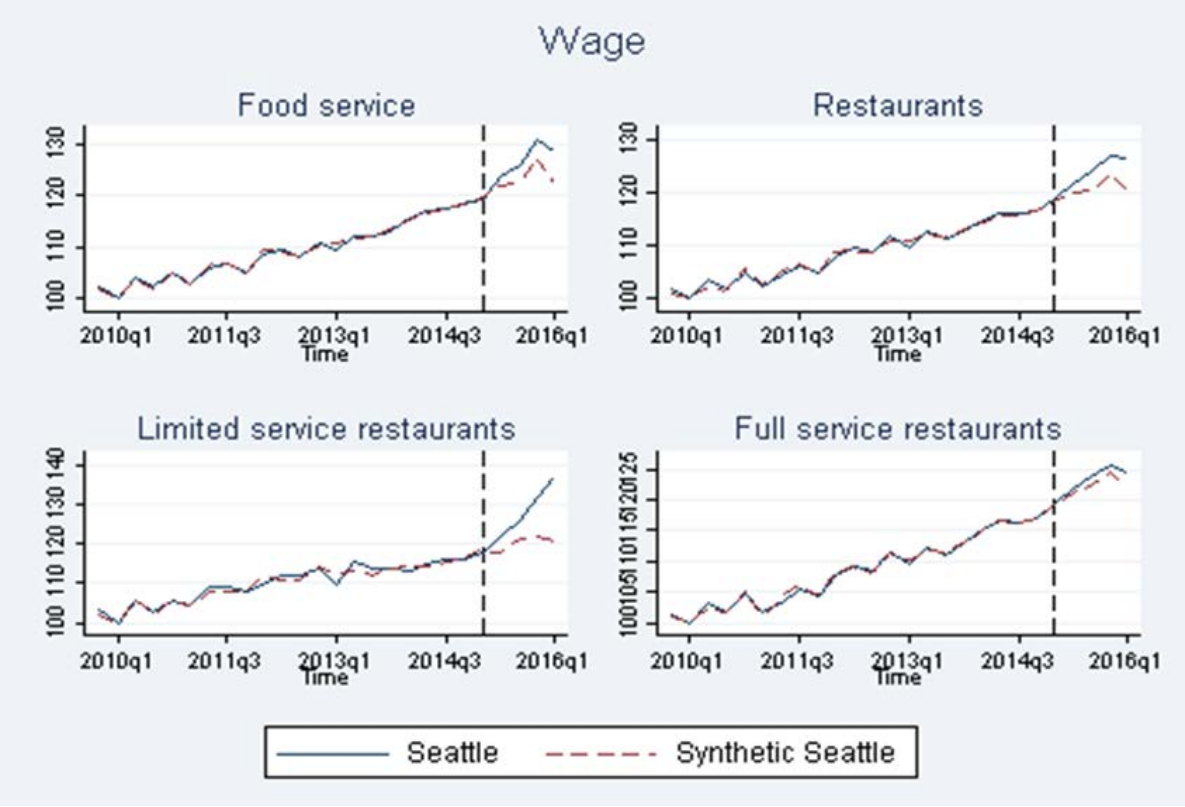
As the figure shows, wages in Synthetic Seattle track wages in Seattle remarkably well, and over the entire pre-treatment period.[7] This finding indicates that our application of the synthetic control method strongly passes the parallel trends requirement. These results thereby satisfy the first of the three credible causal identification conditions we laid out in the beginning of this brief.

[7] The synthetic control method is not appropriate if the researcher cannot obtain close fits in the pre-treatment period. This is often the case. For copious such examples, see Donohue, Aneja and Weber 2017. Researchers who do not display these time paths raise questions about their ability to come up with a synthetic cohort with a good fit.

After the treatment begins, wages in each of the industry groupings increase faster in Seattle than in Synthetic Seattle. This result supports the presence of a wage effect, indicating that the treatment did what it was supposed to do. This finding satisfies the second condition for a credible causal identification.

Importantly, wages increase substantially more in limited service restaurants than in the overall food service industry. And wages in full-service restaurants barely increase relative to Synthetic Seattle. The larger wage increase among limited-service restaurants, many of which are part of franchise chains, suggests widespread compliance with the law, despite the opposition of the International Franchise Association. On the other hand, the very small wage increase among full-service restaurants suggests that these employers made great use of the tipped wage credit.

Figure 1 Wage outcomes, Seattle and Synthetic Seattle

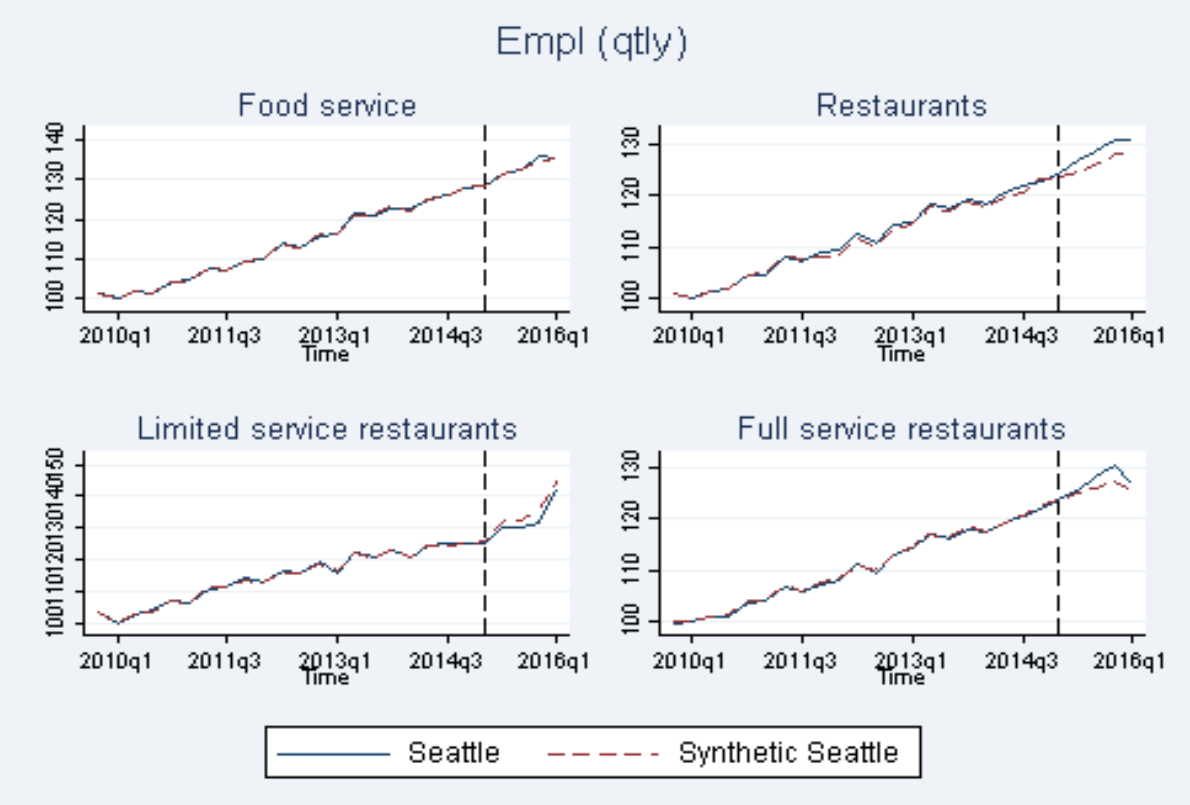


Notes: City-level QCWED data for Seattle. County-level QCEW data for the donors that make up Synthetic Seattle. See Appendix B for a list of donors. The vertical dashed line refers to April 1, 2015, the implementation date of the first phase. The second increase occurred on January 1, 2016.

Employment effects

Figure 2 displays our synthetic control results for employment. Once again, each of the four industry groupings show a close fit between employment in Seattle and employment in Synthetic Seattle over the entire pre-treatment period. Post-treatment employment gains are slightly greater in Seattle than in Synthetic Seattle for all restaurants and among full-service restaurants, and slightly smaller among limited-service restaurants.

Figure 2 Employment trends, Seattle and Synthetic Seattle



Notes: City-level QCWED data for Seattle. County-level QCEW data for the donors that make up Synthetic Seattle. See Appendix B for a list of donors. The vertical dashed line refers to April 1, 2015, the implementation date of the first phase. The second increase occurred on January 1, 2016.

Wage and employment elasticities

Table 2 presents our estimated wage and employment elasticities for each of the four industry groups. The percentile rank statistic in the last column provides a measure of the statistical significance of the estimate. Percentile ranks above .975 and below .025 indicate conventional statistical significance—at the ten percent level. Percentile ranks between these two progressively indicate lower levels of statistical significance.

The estimated wage elasticities in the top panel of Table 2 for food services, all restaurants and limited service restaurants all fall within the range of previous studies and all are highly significant. The wage elasticity of 0.229 for limited service restaurants is nearly identical to our findings in Allegretto et al. (2017). The 0.036 wage elasticity for full-service restaurants is very small and less precisely estimated. These results suggest that full-service restaurants made use of the tip credit to limit the wage increases they would otherwise have paid.

These estimated wage results are subject to a standard caveat. Wages in Seattle may have diverged from Synthetic Seattle just when the minimum wage was implemented for reasons that have little to do with the minimum wage. For example, Seattle's economy may have entered an especially boom period at that time (Tu, Lerman and Gates 2017). We will be able to test this issue by including additional controls in our regressions in future years, as additional quarters of data become available.

The bottom panel of Table 2 displays the employment elasticities. Three of the elasticities are positive, implying a positive effect on employment and one is negative. All are very small and none are precisely estimated, implying that they are not significantly different from zero. All of them are similar to employment elasticities in previous research (such as Allegretto et al. (2017).

Table 2 Estimated wage and employment elasticities

Dependent variable	Industry	Elasticity	Percentile rank statistic
Wage	Food services & drinking places	.098**	.985
	Restaurants (all)	.098**	.984
	Limited service restaurants	.229**	.987
	Full service restaurants	.036	.946
Employment	Food services & drinking places	.010	.538
	Restaurants (all)	.058	.739
	Limited service restaurants	-.060	.333
	Full service restaurants	.045	.704

Notes: Statistical significance levels: ***1 percent, **5 percent, *10 percent. To calculate elasticities, we use the fastest phase-in schedule in Table 1 (employees of large firms who are not covered by employer-sponsored health insurance).

Labor demand elasticities

Although our estimated employment elasticities are not statistically significant from zero, for completeness we present here their equivalents when scaled as labor demand elasticities. Estimated labor demand elasticities in low-wage labor markets in other studies generally center on -0.3. Should they be any different for Seattle? The industries most affected by minimum wages provide local services (in economists' terms, they are not tradeables). Moreover, Seattle is large enough that most of the consumption by Seattle residents occurs within the city's boundaries.

We compute labor demand elasticities for each of our four industry groupings by taking the ratio of the employment elasticity to the wage elasticity, using the results in Table 2. The labor demand elasticities are 0.102 for food services and drinking places, 0.592 for all restaurants, -0.262 for limited-service restaurants, and 1.25 for full-service restaurants. These results vary in part because our estimated wage increases vary by industry and in part because our employment effects vary by industry. However, we do not place much weight on these results as they are measured very imprecisely.

Placebo tests

We turn next to examining how our donor counties, which did not receive the minimum wage treatment, respond when they are given a "placebo" minimum wage treatment. The synthetic control algorithm conducts this test separately for each donor county.[8] Recall that the purpose of these tests is to validate the statistical significance of the results reported in Figures 1 and 2 and Table 2.

Figure 3 displays the placebo results with thin gray lines, one for each donor county. (The vertical lines in Figure 3 are located one quarter after the first minimum wage implementation; we will correct this in a future version.) The gray lines trace the difference between the outcomes of interest for each donor, relative to its "synthetic area." Since these donor counties did not actually receive a minimum wage treatment, we expect considerable random variation in the large post-treatment outcomes. If the post-treatment individual gray lines diverge considerably from each other, we are observing random variation—the absence of a treatment effect.

Figure 3 also displays the results for Seattle (using the thicker orange line), relative to Synthetic Seattle. The orange lines that lie well within the envelope of the numerous gray lines indicate that the orange line could just reflect random variation. If an orange line hugs or reaches outside the envelope

[8] The starting point for these placebo graphs consists of all the potential donors with data available for all periods for the industry subcategory. The potential donors were counties in states that indexed minimum wages but had no other minimum wage events. We estimated two versions: (1) ranking the Seattle result relative to all potential donors; (2) ranking the Seattle results against donors with a "good" pre- intervention fit (RMSPE<2 times that of Seattle). This second criterion excludes potential donors for whom we were unable to construct a good-fitting synthetic control. The placebo graphs illustrate the second approach. Although the second approach excludes some potential donors, potentially reducing significance levels, the actual significance levels are not materially different.

of gray lines, we have additional support that the Seattle results reflect a statistically significant treatment.

In the upper panel of Figure 3, the gray lines diverge during the placebo treatment period, consistent with random variation and no observed treatment effect. For all food services and for all restaurants, this panel also shows a substantial difference between the Seattle results (the thick orange line) and the set of individual donor placebo results (the thin gray lines), indicating that the wage effect is not likely the result of random variation. These results satisfy the three basic principles articulated by the credibility revolution in econometrics.

The upper panel of Figure 3 shows a particularly large and significant effect on wages in limited-service restaurants (note the compression of the vertical axis in this industry's figure). This result is consistent with lower initial pay in limited-service restaurants than in the rest of the industry and with substantial compliance among fast-food restaurants, whether franchises or company-owned.[9] The orange line in the full-service sector is not so steep, indicating smaller and statistically insignificant pay increases, consistent with the results in Table 2. These results are also consistent with the establishment of a tip credit for employers in this industry.

The lower panel of Figure 3 displays the equivalent results for the employment outcomes. Again, the placebo test lines diverge considerably in the post-placebo treatment period, indicating the absence of a treatment effect on employment when there was no treatment. The thick orange line now falls within the enveloped of individual gray lines for food services and for all restaurants.

The orange line is closer to the bottom envelope of the placebo results for limited-service restaurants in the first treatment phase and then bounces back in the second phase.[10] In both periods, it remains within the envelope, indicating that the observed outcome could reflect random variation. The orange line for full-service restaurant employment rises within the top of the placebo envelope in the first phase and bounces back toward zero in the second phase. These results confirm the finding in Table 2: the employment effects in limited- and full-service restaurants are not statistically different from zero.

[9] Ji and Weil (2015) find that franchised outlets of fast food restaurants exhibit much lower compliance rates with minimum wages than do company-owned outlets.
[10] This effect looks larger than it is because the vertical axis is elongated, relative to the other outcomes.

Figure 3 Placebo graphs for wages and employment

Note: The vertical dashed line in this Figure refers to one quarter after the implementation of the first phase. The vertical axis in the limited services figure is elongated relative to those in the other three figures, exaggerating the actual deviations from zero. Placebos where RMSPE<2 times that of Seattle are reported.

SUMMARY

The evidence collected here suggests that minimum wages in Seattle up to $13 per hour raised wages for low-paid workers without causing disemployment. Each ten percent minimum wage increase in Seattle raised pay by nearly one percent in food services overall and by 2.3 percent in limited-service restaurants. The pay increase in full-serve restaurants was much smaller and not statistically significant, consistent in part with higher pay in full-service restaurants and the establishment of a tip credit policy. Employment effects in food services, in restaurants, in limited-service restaurants and in full-service restaurants were not statistically distinguishable from zero. These results are all consistent with previous studies that credibly examine the causal effects of minimum wages.

These findings of no significant disemployment effect of minimum wages up to $13 significantly extend the minimum wage range studied in the previous literature. Of course, unobserved factors, such as Seattle's hot labor market compared to that in Synthetic Seattle (Tu, Lerman and Gates 2017), may have positively affected Seattle's low-wage employment during this period. We will monitor this possibility as the city's $15 policy continues to phase in. And Seattle makes up just one case study; examination of a wider set of cities may lead to different conclusions. Our future reports will throw further light on this possibility.

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APPENDIX A

Why minimum wage increases produce little to no employment effects

CWED researchers and other labor economists have challenged the Stigler downwardly-sloping labor demand framework and developed an alternative framework that considers how minimum wages affect an entire economy (Reich, Allegretto and Montialoux 2017). We refer to this alternative framework as the CWED minimum wage model. It contains five components:

1. Building upon Stigler's insight that employers may possess some wage-setting power, we recognize that employers can choose whether to set low wages and experience high turnover costs or set higher wages and face lower turnover costs. This formulation follows modern search theories of the labor market. Wage rates are indeed inversely related to employee turnover rates, often exceeding 100 percent per year in low-wage industries. Wage-setting power in low-wage labor markets then becomes the norm and not the exception (as Stigler had expected). Our previous empirical work confirms that raising minimum wages does significantly reduce the high rate of employee turnover in low-wage industries (Dube, Lester and Reich 2016). We estimate that the reduced costs of recruiting and retaining workers absorb about 15 percent of the increased payroll costs.

2. Raising wages directly increases worker productivity somewhat, even in low-skilled jobs. A recent study by Burda, Genadek and Hamermesh (2016) confirms this relationship. Increased productivity may arise directly because workers are more experienced or motivated or more likely to receive employer-based training.

3. Higher minimum wages can lead to increased substitution of technology for labor. However, the magnitude of this effect is smaller than is commonly recognized—especially in low-paid service occupations that remain difficult to routinize, such as restaurant food preparation, childcare and eldercare, driving emergency vehicles and janitorial work. Technology has transformed more routinized work mainly because the cost of technology has fallen so sharply, while wages have remained stagnant.

4. Higher costs due to minimum wages will be passed on in higher prices and reduce the scale of output, thereby reducing labor demand. This effect is also much smaller than is usually recognized, for five reasons. First, some workers in affected industries are already well-paid and will not get increases. Second, the pay of workers getting increases does not bunch entirely at the old minimum wage—it ranges across the entire range to just above the new minimum wage. As a result, actual wage increases are about 20-25 percent of the statutory increase. Third, labor consists of only about 30 percent of operating costs in the affected industries. Fourth, prices increases are limited to the industries that most employ minimum wage workers. Fifth, consumer demand in these industries is relatively inelastic to changes in

prices, so the effect on sales and on demand for workers is even smaller than the effects on prices.

5. Minimum wage increases raise take-home pay primarily among workers who have high propensities to spend on consumer goods. This increased consumption increases the demand for labor in the entire consumer goods sector. When larger numbers of workers will get pay increases, the magnitude of this effect grows in relative importance to the others above.

Each of these components affects employment, some in a negative direction and others in a positive direction. Adding them together generates the net effect on employment. Our CWED team has used parameters from various literatures and the Implan Input-Output model to calibrate our model. We have already estimated the model for $15 minimum wage policies in New York State, California, San Jose and Fresno County. We have in progress a study of the effects of a federal $15 policy on the U.S. and on Mississippi. All of these enacted or proposed policies would phase in over five to seven years. $15 in 2024 is the equivalent of $12.50 to $13 today.

These studies all suggest that a $15 minimum wage policy would substantially raise pay for millions of workers and their families with only negligible net effects on employment. Of course, much bigger increases, such a $50 minimum wage, would not have the same effects and indeed would require building an entirely different model.

APPENDIX B: DONOR COUNTIES AND WEIGHTS

Appendix Table B1: Wages

Food service	Boulder County, Colorado	.537
	Pickaway County, Ohio	.105
	Charlotte County, Florida	.100
	Carroll County, Ohio	.062
	Coconino County, Arizona	.061
	Clear Creek County, Colorado	.041
	Park County, Colorado	.031
	St. Louis County, Missouri	.023
	Lafayette County, Missouri	.016
	Pend Oreille County, Washington	.008
	Larimer County, Colorado	.007
	Trumbull County, Ohio	.006
	Stevens County, Washington	.004
Restaurants	Larimer County, Colorado	.310
	Kitsap County, Washington	.157
	Missoula County, Montana	.132
	Charlotte County, Florida	.128
	St. Johns County, Florida	.071
	Medina County, Ohio	.061
	Trumbull County, Ohio	.056
	Union County, Ohio	.036
	Jefferson County, Colorado	.025
	Sarasota County, Florida	.024
Limited service	Walla Walla County, Washington	.165
	Jefferson County, Colorado	.165
	Stevens County, Washington	.147
	Union County, Ohio	.125
	Cochise County, Arizona	.094
	Douglas County, Colorado	.073
	Missoula County, Montana	.066
	Delaware County, Ohio	.059
	Benton County, Washington	.055
	Charlotte County, Florida	.025
	Chelan County, Washington	.024
	Clay County, Florida	.002
Full service restaurants	Skagit County, Washington	.276
	Platte County, Missouri	.147
	Spokane County, Washington	.133
	Yavapai County, Arizona	.119
	Larimer County, Colorado	.100
	Pinal County, Arizona	.080
	Whatcom County, Washington	.051
	Portage County, Ohio	.037
	Lafayette County, Missouri	.020
	Teller County, Colorado	.011
	Santa Rosa County, Florida	.010
	Cass County, Missouri	.008
	Park County, Colorado	.008

Appendix Table B2: Employment

Food service	Lee County, Florida	.257
	Delaware County, Ohio	.143
	Nassau County, Florida	.081
	Denver County, Colorado	.075
	Jefferson County, Ohio	.074
	Flagler County, Florida	.069
	El Paso County, Colorado	.060
	Osceola County, Florida	.059
	Walla Walla County, Washington	.033
	Allen County, Ohio	.032
	Newton County, Missouri	.032
	Carbon County, Montana	.029
	Collier County, Florida	.029
	Buchanan County, Missouri	.017
	Highlands County, Florida	.006
	DeKalb County, Missouri	.003
	Park County, Colorado	.001
Restaurants	Lee County, Florida	.225
	Lorain County, Ohio	.193
	Newton County, Missouri	.148
	Platte County, Missouri	.109
	Jasper County, Missouri	.079
	Brevard County, Florida	.076
	Carbon County, Montana	.051
	Gulf County, Florida	.020
	Hernando County, Florida	.020
	Asotin County, Washington	.015
	Lafayette County, Missouri	.013
	Gadsden County, Florida	.012
	Teller County, Colorado	.010
	Sumter County, Florida	.009
	Park County, Colorado	.009
	Cochise County, Arizona	.006
	Clear Creek County, Colorado	.002
	Carroll County, Ohio	.002
	Pickaway County, Ohio	.001
Limited service	Pinal County, Arizona	.295
	Jasper County, Missouri	.161
	Bay County, Florida	.088
	Polk County, Florida	.058
	Sumter County, Florida	.052
	Snohomish County, Washington	.046
	Fulton County, Ohio	.044
	Santa Rosa County, Florida	.043
	Walton County, Florida	.04
	Geauga County, Ohio	.038
	Flagler County, Florida	.024
	St. Johns County, Florida	.023
	Citrus County, Florida	.021
	Collier County, Florida	.013
	Asotin County, Washington	.013
	Franklin County, Washington	.011

	Charlotte County, Florida	.011
	Brevard County, Florida	.011
	Yavapai County, Arizona	.008
Full service restaurants	Denver County, Colorado	.156
	Lee County, Florida	.133
	Allen County, Ohio	.110

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Center on Wage and Employment Dynamics
University of California, Berkeley
2521 Channing Way #5555
Berkeley, CA 94720-5555
(510) 643-8140
http://www.irle.berkeley.edu/cwed