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MINIMUM WAGE INCREASES, WAGES, AND LOW-WAGE EMPLOYMENT: EVIDENCE FROM SEATTLE

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ABSTRACT

This paper evaluates the wage, employment, and hours effects of the first and second phase-in of the Seattle Minimum Wage Ordinance, which raised the minimum wage from $9.47 to as much as $11 per hour in 2015 and to as much as $13 per hour in 2016. Using a variety of methods to analyze employment in all sectors paying below a specified real hourly rate, we conclude that the second wage increase to $13 reduced hours worked in low-wage jobs by around 9 percent, while hourly wages in such jobs increased by around 3 percent. Consequently, total payroll fell for such jobs, implying that the minimum wage ordinance lowered low-wage employees' earnings by an average of $125 per month in 2016. Evidence attributes more modest effects to the first wage increase. We estimate an effect of zero when analyzing employment in the restaurant industry at all wage levels, comparable to many prior studies.

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Minimum Wage Increases, Wages, and Low-Wage Employment: Evidence from Seattle

1. Introduction

Economic theory suggests that binding price floor policies, including minimum wages, should lead to a disequilibrium marked by excess supply and diminished demand. Previous empirical studies have questioned the extent to which this prediction holds in the labor market, with many estimates suggesting a negligible impact of higher minimum wages on employment. This paper, using rich administrative data on employment, earnings and hours in Washington state, re-examines this prediction in the context of Seattle's minimum wage increases from $9.47 to as much as $11 per hour in April 2015 and as much as $13 per hour in January 2016. It reaches a markedly different conclusion: employment losses associated with Seattle's mandated wage increases are in fact large enough to have resulted in net reductions in payroll expenses – and total employee earnings – in the city's low-wage job market. The contrast between this conclusion and previous literature can be explained largely, if not entirely, by data limitations that we are able to circumvent. Most importantly, much of the literature examines the impact of minimum wage policies in datasets that do not actually reveal wages, and thus can neither focus precisely on low-wage employment nor examine impacts of policies on wages themselves.

Theory drastically oversimplifies the low-skilled labor market, often supposing that all participants possess homogeneous skill levels generating equivalent productivity on the job. In reality, minimum wages might be binding for the least-skilled, least-productive workers, but not for more experienced workers at the same firm. Empirically, it becomes challenging to identify the relevant market for which the prediction of reduced employment should apply, particularly when data do not permit direct observation of wages. Previous literature, discussed below, has typically defined the relevant market by focusing on lower-wage industries, such as the restaurant sector, or on lower-productivity employees such as teenagers.

This paper examines the impact of a minimum wage increase for employment across *all* categories of low-wage employees, spanning *all* industries and worker demographics. We do so by utilizing data collected for purposes of administering unemployment insurance by Washington's Employment Security Department (ESD). Washington is one of four states that collect quarterly hours data in addition to earnings, enabling the computation of realized hourly wages for the entire workforce. As we have the capacity to replicate earlier studies' focus on the restaurant industry, we can examine the extent to which use of a proxy variable for low-wage status, rather than actual low-wage jobs, biases effect estimates.

We further examine the impact of other methodological choices on our estimates. Prior studies have typically drawn "control" cases from geographic regions immediately adjoining the "treatment" region. This could yield biased effect estimates to the extent that control regions alter wages in response to the policy change in the treatment region. Indeed, in our analysis simple geographic difference-in-differences estimators fail a simple falsification test. We report results from synthetic control and interactive fixed effects methods that fare better on this test. We can also compare estimated employment effects to estimated wage effects, more accurately pinpointing the elasticity of employment with regard to wage increases occasioned by a rising price floor.

Our analysis of restaurant employment at all wage levels, analogous to many prior studies, yields minimum wage employment impact estimates near zero. Estimated employment effects are higher when examining only low-wage jobs in the restaurant industry, and when examining total hours worked rather than employee headcount. Even when analyzing low-wage employment across all sectors, employment elasticities as conventionally calculated lie within the range established in prior literature, if somewhat on the high side.

Our analysis reveals a major limitation of conventional elasticity computation methods, however. When comparing percent changes in employment to percent changes in wage, conventional methods must arrive at the percent change in wage by assumption rather than estimation, in some cases assuming that the percent change in wage equals the percent change in the statutory minimum. This is often a necessity, as analysis is performed using datasets that do not permit the estimation of policy impacts on wages themselves. We show that the impact of Seattle's minimum wage increase on wage levels is *much smaller* than the statutory increase, reflecting the fact that most affected low-wage workers were already earning more than the statutory minimum at baseline. Our estimates imply, then, that elasticities calculated using the statutory wage increase as a denominator are *substantially* underestimated. Our preferred estimates suggest that the rise from $9.47 to $11 produced disemployment effects that approximately offset wage effects, with elasticity point estimates around -1. The subsequent increase to as much as $13 yielded more substantial disemployment effects, with net elasticity point estimates closer to -3.[1]

While these findings imply that Seattle's minimum wage policy served to decrease total payroll expenses on low-wage employees, and by extension those employees' earnings, several caveats are in order. These estimates pertain to a minimum wage increase from what had been the nation's highest state minimum wage to an even higher level, and might not indicate the effects of more modest changes from lower initial levels. In fact, our finding of larger impacts of the rise from $11 to $13 per hour than the rise from $9.47 to $11 per hour suggests non-linearity in the response. Second, our data do not capture earnings in the informal sector, or by contractors, and minimum wage policies could conceivably lead employers and workers to shift towards these labor market arrangements. Some employers may have shifted jobs out of Seattle but kept them within the metropolitan area, in which case the job losses in Seattle overstate losses in the local labor market. Even without mobility responses by firms, reductions in payroll per employee may significantly exceed reductions in worker income to the extent that workers were able to find alternate employment in Seattle's rapidly growing suburbs.

Our analysis focuses on a subset of Washington State employers, those that definitively report workplace location for each of their employees. Because of this restriction, smaller single-site employers are over-represented in our sample; we include 89% of all business entities employing 63% of Washington's workforce. We discuss the ramifications of this restriction extensively below. While there may be concerns that larger businesses might exhibit significantly different responses to the minimum wage, survey evidence indicates no differential response and tracking workers longitudinally we find no evidence of an exodus of workers from the sector included in our analysis to the excluded sector.

Finally, the mechanisms activated by a local minimum wage ordinance might differ from those associated with a state or federal increase. It is reasonable to expect that policies implemented at a broader geographic scale offer fewer opportunities to reallocate employment in response.

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[1] Because we calculate elasticity by taking the ratio of the estimated effect on employment to estimated effect on hourly wages, these estimates are imprecise. For instance, the 95% confidence intervals for the elasticities associated with a $13 minimum wage range from -5.9 to -0.3.

We emphasize that any analysis of the welfare implications of a minimum wage increase must consider how income gains and losses distribute across the low-wage workforce. Some low-wage workers are household heads responsible for maintaining a family's standard of living. Others are secondary or tertiary earners whose income is less necessary for basic survival. Our study does not address which workers are better or worse off as a consequence of the minimum wage ordinance. Future analysis will combine employment records with other administrative data from Washington State to more fully address critical distributional questions.

2. Challenges in estimating the impact of minimum wage increases

Traditional competitive models of the labor market suggest that an increase in a binding minimum wage will cause reductions in employment. Any number of modifications to the standard model can raise doubts about this prediction. These include the presence of monopsony power (Bhaskar and To, 1999), the possibility that higher wages intensify job search and thus improve employee-employer match quality (Flinn, 2006), "efficiency wage" models that endogenize worker productivity (Rebitzer and Taylor, 1995), and the possibility that some low-wage workers exhibit symptoms of a "backward-bending" supply curve associated with a need to earn a subsistence income (Dessing, 2002). Even in the absence of these theoretical modifications, there has long been debate regarding the empirical magnitude of the theorized effect.

Over the course of the past 25 years, a robust literature has developed with researchers using a variety of strategies to estimate the effect of minimum wages on employment and other outcomes. While this literature has often generated significant debate over econometric specifications and data sources, the heavy reliance on proxies for low-wage employment in the absence of actual wage data has figured less prominently.[2]

2.1 What is the relevant labor market?

[2] One notable exception is the work of Belman and Wolfson (2015). They note: "Focusing on low-wage/low-income groups offers the advantage of providing more focused estimates of the effect of changes in minimum wage policies; employment and wage effects are less likely to be difficult to detect due to the inclusion of individuals unlikely to be affected by the minimum wage. Use of proxies for low wage/low income such as age, gender, and education are a step in this direction, but still potentially dilute the impact by the inclusion of unaffected individuals (p. 608)."

Previous literature has not examined the entire low-wage labor market but has focused instead on lower-wage industries such as the restaurant sector, or on stereotypically lower-productivity employees such as teenagers. Studies of the restaurant industry harken back to Card and Krueger (1994), which utilized a case study approach to estimate the employment effects of New Jersey's increase in its state minimum wage. The authors argue that fast-food restaurants are not just a leading employer of low-wage workers, but also display high rates of compliance with minimum-wage regulations. Many authors have subsequently chosen the restaurant and fast food industry to study federal and state level minimum wages (Addison, Blackburn and Cotti, 2012, 2014; Dube, Lester and Reich, 2010; Dube, Lester and Reich, 2016; Neumark, Salas and Wascher, 2014; Totty, 2015; Allegretto, Dube, Reich, and Zipperer 2016). Other authors have focused on retail (Kim and Taylor, 1995; Addison, Blackburn and Cotti, 2008).

Another strand of studies estimates the effect of minimum wages on teenagers. These studies argue that teenagers are typically at the bottom of the wage and earnings distribution and make up a large share of the low-wage workforce. Studies of minimum wage effects on teenagers have occurred at the federal and state level (Card, 1992; Allegretto, Dube, and Reich, 2011; Neumark and Wascher, 1994, 1995, 2004, 2008, 2011; Neumark, Salas, and Wascher, 2014).

Using restaurant or retail employees or teenagers as proxies for the entire low-wage labor market might lead to biased minimum wage effects. Intuitively, a sample mixing jobs directly affected by the minimum wage with others for which the price floor is irrelevant would generally skew estimated impacts towards zero. Isolating one industry, such as the fast food industry, may lead to downwardly biased wage and employment effects due to heterogeneity in wages in the industry (i.e., some workers whose wages are above the minimum wage will be misclassified as belonging to the "treatment" group). The estimates capture the minimum wage's net effects on all restaurant employees, not the effects on low-wage employees, which would likely be stronger. Similarly, using teenagers may lead to artificially large employment estimates as this group omits other low-wage workers, particularly those that have a stronger attachment to the labor force and are full-time full-year workers, for whom the wage-elasticity of demand may be smaller. On the other hand, since some teens earn wages well above the minimum, including them in the sample would lead to artificially low estimates of the impacts for that demographic group.

This discussion begs the question of what, exactly, should count as a low-wage job. An intuitive approach – and the one pursued in this analysis – focuses on jobs that pay below a certain (inflation-adjusted) hourly wage.[3] Analysis of employment at or below a specified wage threshold may overstate disemployment effects to the extent that minimum wage policy may cause some employers to raise wages of workers from below to above the threshold. A more purist approach would focus on jobs that entail any of a variety of tasks for which there are no specialized skill requirements, which any able-bodied person might perform. Practically, few if any employment datasets contain such information.

In theory, analysis of employment at or below a specific real wage level will be unproblematic if the wage distribution can be effectively partitioned into a component affected by minimum wage policy and an unaffected counterpart. Imagining a reaction function relating pre-policy to post-policy wages, the partition would be associated with a fixed point. It is not clear that any such fixed point exists. Our analyses below are informed by efforts to estimate reaction functions, which reveal little evidence of significant responses to the minimum wage above relatively low thresholds. We also report the results of sensitivity analyses that vary the threshold substantially.

2.2 Debates over methodology

While much of the previous literature has elided the difficult problem of identifying the relevant labor market by using simple industry or demographic proxies, there has been no shortage of debate over causal estimation strategy. The traditional approach uses variation in state-based minimum wages and estimates minimum wage-employment elasticities using a two-way fixed effect OLS regression (Neumark and Wascher, 2008). This approach assumes parallel pre-trends across treatment and control states and estimates the overall impact of minimum wages on wage and employment of multiple minimum wages over time. The two-way fixed effect approach has come under criticism in recent years because there are spatial patterns in minimum wage adoption (Allegretto, Dube, Lester and Reich, 2016). States with higher minimum wages are concentrated in the Northeast and West coast, regions that have different

[3] This approach bears a strong resemblance to Cengiz et al., (2017) who use pooled Current Population Survey data to study the impact of state-level minimum wage increases on employment at wages just above and below the newly imposed minimum between 1979 and 2016.

employment patterns from states in the South and parts of the Midwest. If this underlying regional pattern affects state employment trends differentially, then the parallel trends assumption of the two-way fixed effects model does not hold. Subsequently, difference-in-differences estimation strategies, which weight all states without a higher minimum wage equally as their control region, may negatively bias employment elasticity estimations.

To account for this issue, researchers have argued for a variety of specifications. These include: the use of local area controls, such as division-period fixed effects or a border discontinuity approach, (Allegretto, Dube and Reich, 2011; Dube, Lester and Reich, 2010; 2016; Allegretto, Dube, Lester, Reich, 2016), the use and order of region-specific time trends (Addison, Blackburn, Cotti, 2012, 2014), the use of a synthetic control to identify control regions with pre-trend employment levels similar to the treatment region (Neumark, Salas, and Wascher; 2014), and linear factor estimation (Totty, 2015).[4]

Local area control designs assume that neighboring counties or states within a census division region are more similar in trends and levels than regions further away. Researchers using local-area controls (Dube, Lester and Reich 2010, 2016; Allegretto, Dube, Reich, 2011) show strong and significant earnings elasticity estimates but insignificant employment elasticities near zero. While it is reasonable to think that nearby regions share many background characteristics with the treated region, a local area control design will yield biased estimates when policies have spillover effects in nearby areas, such as when businesses raise wages in response to a wage increase in a nearby jurisdiction.

The notion that nearby regions offer the best match on background characteristics is itself a matter of debate. Using a synthetic matching estimator approach, Neumark, Salas, and Wascher (2014) show that local areas are not picked as donors in the synthetic estimator of panel national data, and thus should not be used as the control region. Allegetto, Dube, Lester and Reich (2016) rebut this claim noting a recent paper found statistically significant larger mean absolute differences in covariates not related to the minimum wage for noncontiguous counties compared to contiguous counties (Dube, Lester and Reich, 2016).[5]

[4] In this study we do not replicate region-specific time trends due to the limited time-frame of our treatment group. However, this specification has become popular; see Dube, Lester and Reich (2010, 2016) and Addison, Blackburn and Cotti (2014) for use of linear and polynomial time trends in minimum wage estimation strategies.

[5] Covariates included log of overall private sector employment, log population, private-sector employment-to-population ratio, log of average private sector earnings, overall turnover rate and teen share of population.

A final strand of estimation has used linear factor estimation and interactive fixed effects, which relaxes the assumption of parallel trends in control and treatment regions by explicitly modelling unobserved regional trends. Totty (2015) utilizes Pesaran's (2006) common correlated effects estimators as a linear factor estimation. Pesaran's common correlated effects estimators do not estimate common factor and common factor loadings, like the interactive fixed effects estimator, but rather use cross-sectional averages of the dependent and independent variables as a proxy for factors. Totty also uses an interactive fixed effects estimator, identical to ours, which involves estimating the common factors and factor loadings across space and over time and finds insignificant and null employment effects of minimum wages.

3. Policy Context

In June 2014, the City of Seattle passed a minimum wage ordinance, which gradually increases the minimum wage within Seattle City boundaries to $15 an hour. The phase-in rate differs by employer size, and offers some differentiation for employers who pay tips or health benefits. The minimum wage rose from the state's $9.47 minimum to as high as $11 on April 1, 2015. The second phase-in period started on January 1, 2016, when the minimum wage reached $13 for large employers (see Table 1 for details). In this paper, we study the first and second phase-in periods of the Seattle Minimum Wage Ordinance (hereafter, the Ordinance) during which the minimum wage rose from $9.47 to $13 for large businesses – a 37.3% increase.[6] This ordinance, which at the time would have raised Seattle's minimum wage to the highest in the country, came toward the beginning of a wave of state and local minimum wage laws passed in 2012-2016.[7, 8]

[6] As of 2016, employers with fewer than 501 employees worldwide that provide health benefits or pay tips could pay a minimum wage of $10.50 if they contribute at least $1.50 towards tips and health benefits. Our data do not allow us to observe if a worker gets health benefits, but we do observe total compensation, which includes tips. We come back to this issue in greater detail when we discuss the data.

[7] Most prior research has, by necessity, focused on increases at the federal (Card 1992, Katz and Krueger 1992, Belman and Wolfson 2010) or state (Dube, Lester, Reich 2010; 2016, Card and Krueger 1994, Neumark and Wascher 1995, Meer and West 2016) level. This ordinance provides an opportunity to study the minimum wage on a smaller geographic area with an integrated labor market that could allow businesses and workers flexibility to relocate. Prior research on local minimum wage changes (Dube, Naidu, Reich 2007, Potter 2006, Schmitt and Rosnick 2011) have found small or no employment effects of the local wage policies, results consistent with the bulk of the minimum wage literature.

[8] During the years we study (2005 to 2016), the State of Washington had a state-specific minimum wage that was indexed to CPI-W (growing at an average annual rate of 2%) and was, on average, 30% higher than the federal Minimum Wage. As a result, none of the increases in federal minimum wage over this time period have been binding in Washington.

For most of the phase-in period, the minimum wage ordinance mandates higher wages for larger businesses, defined as those with more than 500 employees worldwide. For purposes of the ordinance, a franchised business – independently owned, but operated under contract with a parent company and reflecting the parent company brand – are considered large businesses so long as the sum of employment at all franchises worldwide exceeds 500.

Seattle's groundbreaking minimum wage was implemented in the context of a robust local economic boom. As the figures in Table 3 below indicate, overall employment expanded rapidly in Seattle over the two years following the ordinance's passage. Our methods will endeavor to separate this background trend from the impact of the ordinance itself.

4. Data

4.1 Basic description

We study the impact of the 2015 and 2016 minimum wage increases in Seattle using administrative employment data from Washington State covering the period 2005 through the third quarter of 2016. Washington's Employment Security Department collects quarterly payroll records for all workers who received wages in Washington and are covered by Unemployment Insurance (UI).[9,] Employers are required to report actual hours worked for employees whose hours are tracked (i.e. hourly workers), and report either actual hours worked or total number of hours, assuming a 40 hour work week for employees whose hours are not tracked (i.e. salaried workers).[10, 11]

[9] Most studies that analyze employment responses to minimum wage hikes in the US rely on data from the Quarterly Census of Employment and Wages, which in turn relies on information from the same data source as we do – payroll data on jobs covered by the UI program. As a result, our estimates will be comparable to many results in the literature.

[10] The Employment Security Department collects this information because eligibility for unemployment benefits in Washington is determined in part by an hours worked test. Comparison of the distribution of hours worked in the ESD data with the distribution of self-reported hours worked in the past week among Washington respondents to the CPS reveals some points of departure. In particular, self-reported data show more pronounced "spikes" at even numbers such as 40 hours per week. In general, given the statutory reporting requirement driven by benefits determination provisions, ESD considers the hours data reliable.

[11] Minnesota, Oregon, and Rhode Island are the other three states that collect data on hours.

This unique dataset allows us to measure the average hourly wage paid to each worker in each quarter by dividing total quarterly earnings by quarterly hours worked.[12, 13, 14] As such, we can identify jobs more likely affected by an increase in the minimum wage, and track trends in both employment counts and calculated average hourly wages.[15] Unlike the prior literature, we can plausibly identify low-wage jobs across industries and in all demographic groups, obviating the need for proxies based on those factors. As a result, we can estimate effects solely for low-wage jobs within all industries.

The ESD data contain industry (NAICS) codes, which permit us to estimate results using the restaurant industry proxy used in much of the prior literature (Addison, Blackburn and Cotti, 2012, 2014; Dube, Lester and Reich, 2010; Dube, Lester and Reich, 2016; Neumark, Salas and Wascher, 2014; Totty, 2015; Allegretto, Dube, Lester and Reich, 2016).[16]

We measure employment both as the number of jobs (headcount) and the number of hours worked during the quarter. Because the data provide information on all jobs that were on payroll during a quarter, including jobs which lasted only for a few weeks or even days, we follow prior studies in focusing on the number of beginning-of-quarter jobs, defined as a person-employer match which existed both in the current and previous quarter.[17] The hours worked measure includes all employment, regardless of whether a person-employer match persists for more than one quarter. Because the hours measure captures shifts in staffing on both the intensive and extensive margins, we focus on it in our preferred specifications.

[12] We convert nominal quarterly earnings into real quarterly earnings by dividing by the Consumer Price Index for Urban Wage Earners and Clerical Workers (CPI-W). All wage rates and earnings should thus be considered to be in 2nd quarter of 2015 dollars.

[13] The average wage may differ from the actual wage rate for workers who earn overtime pay, or have other forms of nonlinear compensation including commissions or tips. Workers may occasionally be paid in one quarter for work performed in another. In analysis below, we exclude observations with calculated wages below $9 or above $500 in 2015 dollars. We also exclude observations reporting under 10 or over 1,000 hours worked in a calendar quarter. These restrictions exclude 6.7% of all job/quarter observations.

[14] ESD requires employers to include all forms of monetary compensation paid to a worker, including tips, bonuses and severance payments. As such, for tipped employees we will observe total hourly compensation after adding tips, as long as employers have reported tipped income in full. Because of this data feature, appropriate minimum wage schedule for tipped workers employed by small businesses should include tip credit.

[15] The average hourly wage construct used here is not directly comparable to, say, the self-reported hourly wage in the CPS – in which respondents are instructed to exclude overtime, commissions, or tips. Results obtained through analysis of this average hourly wage measure may differ from those gleaned from self-reported wage studies to the extent that employers alter the use of overtime, tips, or commissions in response to the wage increase.

[16] Specifically, we examine employment and wages in the 3-digit NAICS code 722 "Food and Drinking Places".

[17] This definition is used by the Quarterly Workforce Indicators, based on the Longitudinal Employer Household Data (LEHD), and produces the total number of jobs comparable to the employment counts in the Quarterly Census of Employment and Wages.

The ESD data exclude jobs not covered by the UI program, such as contract employment generating IRS 1099 forms instead of W-2s, or jobs in the informal economy paid with cash. Our estimates may overstate actual reductions in employment opportunities if employers respond to the minimum wage by shifting some jobs under the table or outsourcing workers on payroll to contractor positions.

4.2 Limitation to geographically locatable employment

The data identify business entities as UI account holders. Firms with multiple locations have the option of establishing a separate account for each location, or a common account. Geographic identification in the data is at the account level. As such, we can uniquely identify business location only for single-site firms and those multi-site firms opting for separate accounts by location.[18, 19] We therefore exclude multi-site single-account businesses from the analysis, referring henceforth to the remaining firms as "locatable" businesses. As shown in Table 2, in Washington State as a whole, locatable businesses comprise 89% of firms, employ 62% of the entire workforce (which includes 2.7 million employees in an average quarter), and 63% of all employees paid under $19 per hour.[20]

Multi-site single-account or "non-locatable" firms may respond differently to local minimum wage laws for several reasons. These larger employers may be more likely to face higher mandated minimum wages under the Seattle ordinance. It is not possible to precisely determine which employers are subject to the large business phase-in schedule, as Washington data identify global employment only for those firms with no operations outside the state, do not identify which entities have operations outside the state, and do not indicate whether a business operates under a franchise agreement let alone the number of employees at all same-branded

[18] To determine the exact location of each business, we geocode mailing addresses to exact latitude and longitude coordinates. We then use these data to determine if a business is located within Seattle, and to place businesses into Public Use Microdata Areas within Washington State. A small number of employers use a post office box as a mailing address or have not reported a valid address; these are excluded from the analysis.

[19] Note that our analysis sample includes both independently-owned businesses and franchises where the owner owns a single location, but excludes corporations and restaurant and retail chains which own their branches and franchises whose owner owns multiple locations, unless these entities opt to establish separate UI accounts by location.

[20] Appendix Table 1 shows that the proportion of low-paid (under $19 per hour) employees included in the analysis falls close to the 63% benchmark in the accommodation and food service industry and the health care and social assistance industry. It exceeds the benchmark in manufacturing, educational services, and arts, entertainment and recreation. It falls short of the benchmark in the retail industry.

franchises. While it is reasonable to assume that multi-site employers are more likely to be large and thus subject to the higher wage mandate, it is by no means a perfect indicator.[21]

If it were a perfect indicator, basic economic theory suggests that excluded businesses should reduce employment faster than included businesses, as they face a higher mandated wage increase. Individual employees may exhibit some incentive to switch into employment at an excluded firm, but these job changes will be tempered by any adverse impact on labor demand.

This basic prediction could be tempered to the extent that excluded businesses exhibit a different labor demand elasticity relative to included businesses. On the one hand, firms with establishments inside and outside of the affected jurisdiction might more easily absorb the added labor costs from their affected locations, implying a less elastic response to a local wage mandate. On the other hand, such firms might have an easier time relocating work to their existing sites outside of the affected jurisdiction, implying a greater elasticity.

Survey evidence collected in Seattle at the time of the first minimum wage increase, and again one year later, suggests that multi-location firms were in fact more likely to plan and implement staff reductions.[22] Moreover, the ESD data can be used to track workers longitudinally, to check whether minimum wage increases are associated with an increased flow of workers from locatable jobs to non-locatable jobs. If the minimum wage ordinance were to cause an expansion of labor demand in the non-locatable sector, we might expect increased worker flows into this sector. As Figure 1 illustrates, we find that the rate of transition from locatable to non-locatable employment – tracking individual workers from one year to the next – shows no significant change in either Seattle or nearby regions as the city's minimum wage increased, suggesting no impact of the ordinance on gross flows into the non-locatable sector.[23]

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[21] In addition, larger firms are more likely to provide health benefits to their workers, and Seattle's minimum wage ordinance establishes a lower minimum wage for employers who contribute towards health benefits.

[22] The Seattle Minimum Wage Study conducted a stratified random-sample survey of over 500 Seattle business owners immediately before and a year after the Ordinance went into effect. In April 2015, multi-site employers were more likely to report intentions to reduce hours of their minimum wage employees (34% versus 24%) and more likely to report intentions to reduce employment (33% versus 26%). A one-year follow-up survey revealed that multi-location employers were more likely to report an actual reduction in full-time and part-time employees, with over half of multi-site respondents reporting a reduction in full-time employment (52%, against 45% for single-site firms). See Romich et al. (2017) for details on employer survey methodology.

[23] The basic impression conveyed by this figure is confirmed by synthetic control regression analysis, which finds no significant impact of the minimum wage ordinance on the probability that a low-wage individual employed at a locatable Seattle business in a baseline quarter is employed in the non-locatable sector anywhere in Washington State one year later.

Our best inference, in summary, is that our data restriction to geographically locatable employment likely biases our employment results towards zero.

4.3 Basic plots of the hourly wage distribution

Figure 2 shows the distribution of quarterly hours worked across one-dollar-wide wage bins, up to the $39-40 per hour level, in the 2nd quarter of 2014, when the minimum wage ordinance was passed, compared to the 2nd quarter of 2015, the quarter when $11 per hour minimum wage was implemented, and the 2nd quarter of 2016, one quarter after implementation of the $13 per hour minimum wage. After both minimum wage step-ups, we see strong declines in the share of Seattle's workers earning low wages, as well as increases in the hours worked in Seattle at higher wage levels. This change in the distribution could be due to the Ordinance, but might also reflect labor demand growth outpacing supply, which would prompt a similar rightward shift in the wage distribution. Indeed, the Seattle metropolitan area enjoyed a strong labor market during this time period, with unemployment rates well below the national average. As shown in Appendix Figure 1 for outlying King County and for surrounding Snohomish, Kitsap, and Pierce Counties, we see somewhat similar changes in the distributions of hours.[24] Our methods seek to differentiate the impacts of the ordinance from background labor market trends.

5. Methodology

5.1 Determining a threshold for low-wage employment analysis

As indicated in section 2 above, we focus our analysis on jobs with calculated hourly wages below a fixed (inflation-adjusted) threshold. This proxy for low-skilled employment will produce accurate estimates of the impact of minimum wage increases to the extent that a wage threshold accurately partitions the labor market into affected and unaffected components. It will overstate employment reductions if the threshold is set low enough that the minimum wage increase causes pay for some work to rise above it. This concern is particularly relevant given previous evidence of "cascading" impacts of minimum wage increases on slightly higher-paying

[24] Outlying King County is defined as the area of King County excluding the cities of Seattle and SeaTac. SeaTac lies between Seattle and Tacoma with an area of 10 square miles mostly containing the Seattle-Tacoma International Airport. In 2013, SeaTac passed a law raising its minimum wage to $15 per hour. We therefore exclude it from our analysis.

jobs (Neumark, Schwizer, and Wascher, 2004). It may understate proportional employment and wage effects if set too high, as effects on relevant jobs will be diluted by the inclusion of irrelevant positions in the analysis. Imagining a reaction function linking initial wages to post-increase wages, we aim to identify a fixed point above which there does not appear to be any impact.

To do this, we exploit the longitudinal links in ESD data to examine the pattern of wage increases experienced by individual workers at the discrete points when Seattle's minimum wage increased. To consider which workers' experiences are potentially relevant for this exercise, we select a preliminary threshold of $19 per hour, almost exactly twice the baseline minimum, a level beyond which cascading effects are less likely to occur (Neumark, Schwizer, and Wascher, 2004).[25] For employees in this category in a baseline quarter, we examine the full distribution of their hourly wages conditional on continued employment in a locatable Seattle firm one year later. We repeat this analysis with end quarters just before and after minimum wage increases to infer the impact of the minimum wage.[26]

Figure 3 presents four cumulative density functions, representing the results of this exercise for the periods ending just before and after Seattle's first and second minimum wage increases. The top panel shows densities which correspond to the time of the first minimum wage increase. Direct comparison of these densities reveals an expected consequence of the minimum wage increase: the cumulative density function visibly shifts to the right at the lowest wage levels, indicating that fewer tracked workers had wages below $11 after the first minimum wage increase, compared to workers tracked to a point just before the implementation date. Above $11 the two cumulative density functions quickly converge, indicating that the first minimum wage increase had little to no impact on the probability that a longitudinally tracked worker earned a wage greater than any threshold over $12. This is not to say that longitudinally tracked workers enjoyed no wage increases; indeed the cumulative density function shows that roughly 20% of the workers in this longitudinal sample moved from below $19 to above $19

[25] In the years before the minimum wage increase, a median Seattle worker earning the minimum wage worked about 1,040 hours per year (Klawitter, Long, and Plotnick, 2014). Using this figure, a family of two adults and one child with one adult working 1,040 hours at a wage of $19 per hour, would have a family income of $19,760, which is right above the official poverty threshold for such a family.

[26] This analytical strategy could be problematic to the extent there are significant anticipatory effects of minimum wage increases. Results below will indicate little to no evidence of anticipation effects associated with the Seattle minimum wage increases.

over one year. However, this probability appears equal before and after the minimum wage increase.

The bottom panel plots the pair of cumulative density functions which reveal the experiences of workers tracked just before and after the second minimum wage increase. Here, there is once again evidence of a rightward shift at the low end of the distribution, with the share of workers earning under $12, $13, or even $15 per hour dropping noticeably. The two cumulative densities overlap one another closely towards the right side of the chart. Once again, we infer that the minimum wage increase had no discernable impact on the probability that a longitudinally tracked worker earned a wage over any threshold higher than about $17.

Although the pairs of cumulative density functions plotted in Figure 3 overlap closely with one another above relatively modest thresholds, across-pair comparisons clearly show some rightward drift in the inflation-adjusted distribution, consistent with Seattle's overall pattern of robust employment growth. This rightward drift may be of little consequence to our analysis if it is also present in data for control regions. If it is not, this evidence shows that our best opportunity to cleanly identify minimum wage effects pertains to immediately apparent impacts.[27]

While the preponderance of evidence suggests that a low-wage threshold slightly above the statutory minimum poses little risk of miscoding jobs as lost when they have really been promoted to higher wage levels, in our preferred specifications we report findings based on a relatively conservative $19 threshold. In the analysis below, we evaluate impacts going up to a $25 threshold. As shown below, consistent with the results in Figure 3, we do not find evidence of gains in hours between $19 and $25 per hour caused by the Ordinance.

5.2 Causal identification strategy

We estimate the effect of the Ordinance on changes in employment and wages in Seattle relative to the 2nd quarter of 2014, when the Ordinance was passed. From this baseline period, we analyze effects over the next nine calendar quarters. The first three correspond to the period after

[27] Alternately, one could record the fact that over the period between early 2015 and early 2016 the probability of a worker earning under $19 remaining under $19 declined by about 2 percentage points, and consider this the result either of the minimum wage or exogenous increases in labor demand relative to supply. Under the assumption that 100% of the apparent drift can be attributed to the minimum wage, in spite of the fact that it occurs entirely across quarters where the minimum wage did not increase, this suggests our methods may overstate employment losses by about 2 percentage points.

the Ordinance was passed but before the first phase-in; this period is considered "post-treatment" in our analysis so that we can assess whether anticipatory effects ensued.[28] The minimum wage reached as high as \$11 per hour in the fourth through sixth quarters after baseline and as high as \$13 per hour in the remaining quarters. The "pre-treatment" period includes quarterly observations beginning in 2005.

Though we are interested in the cumulative effect of the minimum wage, we analyze variation in year-over-year changes in each outcome. This approach differences out seasonal fluctuations, and conforms to a standard time-series approach used in the prior literature. We define the year-over-year change in outcome Y as follows:

(1) $$\Delta Y_{rt} = Y_{rt}/\,Y_{r,t-4} - 1$$

where r denotes region (e.g. Seattle or comparison region), and t denotes quarter (with t ranging from -33 to 9, and $t = 0$ corresponding to the quarter during which the Ordinance was passed).

We begin with three candidate causal identification strategies. We will subject these strategies to a basic falsification test utilizing pre-treatment data before proceeding to the main analysis.

First, we consider a simple difference-in-differences specification, in which the outcomes of the treated region (Seattle in our case) are compared to the outcomes of a neighboring control region. We consider two different control regions. Comparison of Seattle to immediately surrounding King County can be thought of as equivalent to the contiguous county specification used by Dube, Lester and Reich (2010). Next, we compare growth rates in employment in Seattle to Snohomish, Kitsap, and Pierce Counties (SKP), which surround King County but do not share a border with Seattle (see Figure 4). Since a higher minimum wage might have a spillover effect on the parts of King County immediately adjacent to Seattle, we chose the counties which have similar local economic climates to Seattle's, but are not immediately adjacent to Seattle, as a candidate control region. We expect SKP to experience a smaller (if any) spillover effect of the Ordinance compared to King County, and thus yield a less biased estimate of its impact.[29]

[28] Alternatively, if one assumes that anticipatory effects are unlikely, then these three months can be considered policy leads and used to evaluate whether there is divergence in pre-implementation trends. As we show below, we do not find significant evidence of anticipation effects, which could, alternatively, be interpreted as lack of divergence in pre-implementation trends.

[29] Our companion paper (Jardim et al., 2017) examines this possibility of spillover and mechanisms for estimating spillovers in greater detail.

In both cases, we estimate the following difference-in-differences specification:

(2) $$\Delta Y_{rt} = \alpha_r + \psi_t + \sum_{q=1}^{9} \beta_q T_{rt} + \varepsilon_{rt},$$

where α_r is a region fixed effect, ψ_t is a period fixed effect, β_q is the treatment effect of the Ordinance in quarter $t = q$ (corresponding to the nine quarters after the Ordinance was passed), T_{rt} is an indicator that equals one for the treated region during which $t = q$, and ε_{rt} is an idiosyncratic shock.

In equation (2), $q = 1$ corresponds to the third quarter of 2014, the first quarter after the Ordinance had been passed; $q = 4$ corresponds to the second quarter of 2015, when the first phase-in of the Ordinance occurred; $q = 7$ corresponds to the first quarter of 2016, when the second phase-in occurred; and $q = 9$ corresponds to the third quarter of 2016, the last period of data currently available. Since our interest is in the cumulative effect of the Ordinance on each outcome, we convert these coefficients into cumulative changes, using the following rules. For quarters one to three $\beta_q^{cum} = \beta_q$; for quarters four to eight, $\beta_q^{cum} = (1 + \beta_q)\left(1 + \beta_{q-4}\right) - 1$; and for quarter nine $\beta_9^{cum} = (1 + \beta_9)(1 + \beta_5)(1 + \beta_1) - 1$. We present all results in terms of cumulative changes, and adjust the standard errors accordingly using the delta method.

The model in Equation 2 is a standard two-way fixed effect specification used in the literature (Neumark and Wascher, 2008). As pointed out in Bertrand, Duflo, and Mullainathan (2004), local economic outcomes in this model are not independent from each other, because they come from the same region. We account for this correlation by calculating two-way clustered standard errors at the region and year level.

Difference-in-differences specifications assume that the treated and control region have the same trends in the absence of the policy (parallel trends assumption), and will generally fail to produce consistent treatment effect estimates if this assumption is not true. It is prudent to be especially cautious about the parallel trends assumption given that the greater Seattle region experienced rapid economic growth coming out of the Great Recession, and the pace of recovery could have varied in different sub-regions. As we show below, our two difference-in-differences specifications fail a falsification test, which suggests divergent trends between Seattle and Outlying King County and between Seattle and SKP.

To overcome this concern, we estimate the impact of the minimum wage using two methods which allow for flexible pre-policy trends in control and treated regions: the synthetic control estimator (Abadie and Gardeazabal, 2003) and the interactive fixed effects estimator (Bai, 2009). Both methods have been used in the regional policy evaluation literature and applied to the minimum wage as well (see Allegretto, Dube, Reich, and Zipperer (2013) for an application of synthetic control, and Totty (2015) for an application of interactive fixed effects).

Both methods assume that changes in employment in each region can be represented as a function of K unobserved linear factors plus the treatment effect:

(3) $$\Delta Y_{rt} = \sum_{k=1}^{K} \lambda_{rk}\mu_{tk} + \sum_{q=1}^{9} \beta_q T_{rt} + \varepsilon_{rt},$$

where μ_{tk} is an unobserved factor, common across all regions in each year-quarter, and λ_{rk} is a region-specific factor loading, constant across time.

The unobserved factors can be thought of as common economic shocks which affect all regions at the same time, such as an exchange rate shock, common demand shock, or changes in weather. Because the regions are allowed to have different sensitivity in response to these shocks, the treated and control regions are no longer required to have parallel trends.

Though both the synthetic control and interactive fixed effects estimators have the same underlying model, their implementation is quite different. The synthetic control estimator does not explicitly estimate the factors or factor loading, and uses pre-policy observations to find an optimal set of (weighted) control regions, which collectively match the pre-policy trend in the treated region. Denote Seattle by $r = 1$ and denote $r = 2,\dots,R$ all potential control regions. Then the weights for synthetic control can be found by minimizing forecasting error in the pre-policy period:

(4) $$\min_{w_r} \sum_{t=-33}^{0} \left(\Delta Y_{r=1,t} - \sum_{r=2}^{R} w_r \Delta Y_{rt}\right)^2,$$

subject to the constraints $\sum_r w_r = 1$ and $\forall r\ w_r \geq 0$.[30] Given a set of weights $\widehat{w_r}$, the impact of the Ordinance in quarter q is estimated as follows:

(5) $$\beta_q^{Synth} = \Delta Y_{r=1,q} - \sum_{r=2}^{R} \widehat{w}_r\, \Delta Y_{rq}\ .$$

We allow weights across regions to be different for each outcome to improve the quality of the match in 2005-2014. Appendix Figure 2 shows that the set of regions in Washington,

[30] We implement synthetic control estimator using the R programs provided by Gobillon and Magnac (2016).

which receive a positive weight in synthetic control estimator is very similar for employment outcomes and payroll, but somewhat different for wage rates.[31]

The interactive fixed effects approach estimates the factors and factor loadings in Equation 3 explicitly, by imposing normalization on the sum of the factors. Since the number of unobserved factors is not known, we estimate the model allowing for up to 30 unobserved factors, and pick the model with the optimal number of factors using the criterion developed in Bai and Ng (2002).[32] We implement the interactive fixed effects estimator following Gobillon and Magnac (2016) who have developed a publicly-available program to estimate the treatment effects in the regional policy evaluation context. Appendix Figure 3 shows the sensitivity of the interactive fixed effects estimates as a function of the number of factors used, as well as showing the choice of the optimal number of factors.We implement the synthetic control and interactive fixed effects estimators by approximating Seattle's economy using data on employment trends across Public Use Microdata Areas (PUMAs) in Washington State. A PUMA is a geographic unit defined by the U.S. Census Bureau with a population of approximately 100,000 people, designed to stay within county boundaries when possible.[33] We exclude King County PUMAs from analysis because of potential spillover effects. The remainder of Washington includes 40 PUMAs (see Figure 5), while Seattle is composed of five PUMAs.[34]

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[31] Pairwise correlations between synthetic control weights chosen for hours worked, number of jobs, and payroll are each larger than 0.85, while the correlations of the synthetic control weights chosen for wages with weights chosen for the other three outcomes is positive, but smaller (0.21, 0.22, and 0.22). Examination of the weights, depicted in Appendix Figure 2, suggest a basic intuitive story: the strong growth in employment in Seattle finds its closest parallels in outer suburban or exurban portions of the state, where rapid population growth drives expansion of local economies. The strongest resemblance to Seattle in terms of wages, by contrast, tends to be in closer-in suburban areas, including the satellite centers of Tacoma and Everett.

[32] The coefficients, β_q, can be identified if the number of factors is smaller than the number of periods in the data minus the number of coefficients to be estimated minus one. In our case, we cannot have more than 32 factors in the model (43 periods – 9 coefficients – 1). We use a global criterion IC2 developed by Bai and Ng (2002) to pick the optimal number of factors, and the optimal number of factors is always smaller than the maximum number of factors allowed by the model. We choose the optimal number of factors using criterion IC2 suggested in Bai and Ng (2002), as it was shown to have good performance in small samples.

[33] Twenty-seven of Washington's thirty-nine counties have fewer than 100,000 inhabitants, implying that they must share a PUMA with territory in at least one other county.

[34] Given Seattle's unique status as a city experiencing a tech-driven economic boom, there may be some concern that our restriction to Washington State forces us to use comparison regions that match poorly to the City's labor market dynamics. We present evidence on the quality of fit between treatment and control region below. Intuitively, we seek regions that match Seattle's dynamics in the low-wage labor market, and Appendix Figure 2 reveals that the high quality matches tend to be found in suburban or exurban regions of the state that are themselves experiencing growth, often associated with new construction and expansion of the residential population.

Though the synthetic control and interactive fixed effects estimators generally perform similarly in Monte Carlo simulations (Gobillon and Magnac, 2016), analytic standard errors for interactive fixed effects estimator have been established, while standard errors for the synthetic control estimator are usually obtained using placebo estimates. We provide the baseline standard errors for the synthetic control estimates using an approach of "placebo in space," suggested by Abadie, Diamond, and Hainmueller (2014). We implement it by randomly selecting 5 PUMAs in Washington State as "treated" and estimate the placebo impact for these PUMAs.[35] As in Gobillon and Magnac (2016), we implement 10,000 draws to obtain the standard errors. The standard deviation of these estimated placebo impacts is our estimate of the standard error.[36, 37]

6. Results

6.1 Simple first-difference analysis

Table 3 presents summary statistics on the number of jobs, total hours worked, average wages, and total payroll in Seattle's single-location establishments for all industries and for food and drinking places by wage level for the quarter the Ordinance was passed (t = 0, including June 2014), the first three quarters after the law was passed (t = 1, 2, or 3, July 2014-March 2015), and the first six quarters after the law was in force (t = 4, 5, 6, 7, 8, or 9, April 2015-September 2016). These statistics portray a general image of the Seattle labor force over this time period and should not be interpreted as estimates of the causal impact of the Ordinance.

As shown in Panel A of Table 3, comparing the baseline second quarter of 2014 to the second quarter of 2016, the number of jobs paying less than $13 per hour in all industries declined from 39,807 to 24,420 (a decline of 15,387 or 39%).[38] The decline is consistent with

[35] Note that Seattle spans 5 PUMAs, thus our placebo treatment region replicates Seattle's size.

[36] We have also estimated the standard errors based on a "placebo in time" approach. It is implemented by randomly picking a period when the Ordinance is implemented using the data before the actual Ordinance went in effect, and estimating a placebo effect for this period. We then take the standard deviation of these estimated placebo effects as estimate of the standard error. Standard errors using the "placebo in space" approach prove to be more conservative (i.e. larger) than the standard errors using a "placebo in time", so we report the former standard errors in our baseline estimate.

[37] Computing standard deviation of the placebo impact as a standard error of the estimated impact assumes that the distribution of placebo impacts converges to normal distribution as the number of permutations increases. We have compared inference based on this normality assumption with the inference based on 95% confidence intervals derived from the distribution of placebo impacts. The conclusions about the statistical significance based on these two procedures are very similar, and as such we report the standard errors in our estimation tables.

[38] Note that we are using the second quarter of 2016 to avoid issues with seasonality. Seattle's low-wage labor force tends to peak in the third quarter of each year during the summertime tourist season, and exhibits a trough in the winter months.

legislative intent, and the persistence of employment at wages below $13 can be explained by the fact that lower minima applied to small businesses and those offering health benefits.[39]

The reduction in employment at wages under $13 could reflect either movement of wage rates above this threshold or the elimination of jobs. Table 3 panel A shows that over the same two-year time period, the number of jobs paying less than $19 per hour fell from 92,959 to 88,431 (a decline of 4,528 or 4.8%).[40] Measuring hours worked at low wages rather than employee headcount, the table shows a 5.8 million hour reduction at wage rates under $13, and a 1.7 million hour (4.5%) reduction at wages under $19.

Over this same period, overall employment in Seattle expanded dramatically, by over 13% in headcount and 15% in hours. Table 3 makes clear that the entirety of this employment growth occurred in jobs paying over $19 per hour.[41] The impression of skewed growth – driven in part by rapid growth in the technology sector – extends to wage data.[42] Average hourly wages at jobs paying less than $19 rose from $14.14 to $15.01 (a 6.1% increase), while average hourly wages at all jobs surged from $36.93 to $44.04 (a 19.2% increase).[43]

Table 3 documents that payroll reductions attributable to declines in hours worked very nearly offset the observed wage increases for jobs paying under $19. Comparing "peak" third quarter statistics in 2014 and 2016, the sum total of wages paid at rates under $19 actually declines by over $6 million.[44] Similar comparisons of second quarter statistics reveal a comparably-sized increase.

Panel B of Table 3 restricts attention to Food and Drinking Places (NAICS industry 722), which, respectively, comprised 27%, 20%, and 10% of jobs in Seattle's locatable establishments

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[39] Low-wage employment could also reflect overestimation of hours by the employer, underreporting of tips, hours worked for wages paid in a different calendar quarter, or a subminimum wage set equal to 85% of the minimum for workers under 16 years old.

[40] Appendix Table 2 breaks down the changes in employment into more wage categories. The largest gains in employment occurred for jobs paying more than $40 per hour, which grew 32% between 2014.2 and 2016.2.

[41] The more detailed statistics in Appendix Table 2 show that net job growth in Seattle was positive for jobs paying over $25/hour but negative for jobs paying under $25. About 80% of net job growth can be attributed to jobs paying over $40/hour, and 95% to jobs paying over $30/hour.

[42] Quarterly Census of Employment and Wage (QCEW) data for King County indicate that between 2014 and the third quarter of 2016, the county added 94,000 jobs. The majority of these job gains can be attributed to four industries: non-store retail, information, professional/technical services, and construction. The food service industry added more than 10,000 jobs countywide over this same time period.

[43] The average hourly wage statistic at all wage levels includes a large number of salaried jobs in which hours may be imputed at 40 per week rather than tracked.

[44] At the same time, total quarterly wages paid at rates above $19 increased by $1.7 billion – implying a dramatic increase in inequality of earnings between low- and high-wage workers in Seattle.

paying less than $13, less than $19, and overall during the quarter the Ordinance was passed. Although this industry accounts for a minority of all low-wage employment, we highlight it for purposes of comparison with existing literature.

As in the full economy, growth in hours at restaurant jobs paying above $19 per hour exceeded growth in lower-paying restaurant jobs. At all wages, hours within this industry expanded by 12.9% while hours worked by low-wage employees in the restaurant industry was nearly unchanged, down 0.2% between the second quarter of 2014 and the second quarter of 2016. Wages in the restaurant sector grew comparably in the low-wage market and the full market: 12.1% growth in wages in jobs paying less than $19 per hour, and 13.6% growth in wages in all jobs.

6.2 Falsification tests

Previous analyses have raised concerns regarding the applicability of the parallel trends assumption in minimum wage evaluation. As noted above, the short duration of our post-treatment panel makes it infeasible to employ the traditional linear time-trend correction. For this reason, and to assess the performance of our proposed estimators, we conduct a simple falsification test by estimating the effects of a "placebo" law as if it were passed two years earlier (second quarter of 2012). We restrict this analysis to data spanning from the first quarter of 2005 to the third quarter of 2014. Table 4 presents the results.

We find strong evidence that total hours worked in jobs paying less than $19 per hour in Seattle diverged from both surrounding King County and SKP after second quarter 2012, as shown in columns 2 and 4. In both columns, all of the estimated pseudo-effects on hours are negative and significant, and would falsely suggest the placebo law caused a reduction in hours of 4.1% or 5.0%, respectively, in the average quarter following the second quarter of 2012. Given this divergent trend, we consider the two difference-in-differences estimators to have failed the falsification test and dispense with them henceforth.

In contrast, the synthetic control results shown in columns 5 and 6 behave well. In the average quarter following the placebo law, we find a 0.4% increase in wages and 0.1% increase in total hours. The pseudo-effects on wages, which are all positive, but mostly insignificant, are somewhat concerning – if these same positive pseudo-effects persist into the period that we study, we would be modestly overstating the effect of Seattle's minimum wage on wages, and thus understating elasticities of hours with respect to changes in wages.[45] The pseudo-effects on hours flip back-and-forth between positive and negative.

Finally, columns 7 and 8 show the estimates of the pseudo-effects using the interactive fixed effects specification. This specification finds no pseudo-effect on wages, while the pseudo-effects on hours are all negative, yet insignificant (with larger standard errors), and average -1.9%. If these same negative pseudo-effects on hours persist into the period that we study, we would be moderately overstating the negative effect of Seattle's minimum wage on hours. Consequently, we conclude that the synthetic control method is the most trustworthy, but include interactive fixed effect models below with the caveat that they may be prone to overstating negative employment impacts.

6.3 Examining the synthetic control match

Figure 6 plots the time series of year-over-year percentage changes in average wages, jobs, hours worked, and payroll for low-wage jobs in Seattle and the weighted average of PUMAs outside King County identified using the synthetic control algorithm.[46] In each panel, there is a very strong pre-policy match in trends between Seattle and the control region. As shown in Panel A, wage growth patterns in Seattle and control regions match closely, with growth rates matching to within a 0.5 percentage point tolerance except around 2009, where wage trends in the control region appear to anticipate those in the city.

Employment trends (panels B and C for jobs and hours, respectively) likewise match closely, with discrepancies below a 2-percentage point threshold except in the period around the Great Recession, where the control regions appear to enter and exit the slump slightly before the city itself. Total payroll growth also matches closely throughout the pre-policy period.

These graphs anticipate our causal effect estimates: in all cases, the post-ordinance period is marked by treatment-control divergences well outside the range observed in the pre-treatment period.

6.4 Causal effect estimates

[45] These positive wage effects are consistent with other evidence indicating robust labor demand in Seattle, including the cumulative density functions in Figure 2 above.

[46] Appendix Figure 4 shows a parallel analysis of the time series for Seattle compared to Outlying King County and SKP.

Table 5 presents our first estimates of the causal impact of the Ordinance for workers earning less than $19 per hour. Looking at both sets of results, we associate the first minimum wage increase, to $11, with wage effects of 1.4% to 1.9% (averaging 1.7%). The second increase, to $13, associates with a larger 2.8% to 3.6% wage effect (averaging 3.1%). A 3.1% increase in the wage of these workers corresponds to $0.44 per hour relative to the base average wage of $14.14.[47] We do not find strong evidence that wages rose in anticipation of enforcement during the three quarters following passage of the law. The small coefficients range from 0.3% to 0.7% and most are statistically insignificant.

These wage effect estimates appear modest in comparison to much of the existing literature. We note that the first-difference results presented in Table 3 themselves indicate modest increases in wages at the low end of the scale (under $19), about 4.5% during the first phase-in and 6.0% during the second. These estimates suggest that wages increased in the control region as well.[48] We further note that Table 3 indicates that the majority of low-wage jobs observed at baseline – 62% when defined as jobs paying under $19 per hour and weighted by hours – were not directly impacted by the minimum wage increase to $13. Any impacts on wages paid for jobs between $13 and $19 per hour at baseline would be "cascading" effects expected to be much smaller than the impact on lowest earners. Figure 3 above confirms that very little impact on the cumulative wage distribution of longitudinally tracked workers can be observed above relatively low thresholds. If we were to presume that our estimate reflects some sizable impact on jobs directly impacted by the increase and no cascading effects on other jobs under $19, the impact works out to a 7.9% wage increase, a level in line with existing literature.[49] Finally, we note that the measure of wages used here – average hourly wages – would by construction capture employer responses such as a reduction in the use of overtime. These would not be captured in, for example, self-reported CPS wage data.

Table 6 shows employment impacts for jobs paying less than $19 per hour. As shown in columns 1 and 2, relative to the baseline quarter (2014.2), we estimate statistically insignificant

[47] Estimated wage impacts are larger when the low-wage threshold is lowered from $19. This is consistent with the minimum wage ordinance having sizable effects on the lowest-paid workers and smaller cascading impacts on workers with initial wages closer to $19.

[48] Data from the Bureau of Labor Statistics' Current Employment Statistics indicate that seasonally adjusted average hourly earnings for all employees increased about 5.5% nationwide from June 2014 to September 2016.

[49] Belman and Wolfson (2014) point to elasticities of wages paid to statutory minimum wage increases in the range of 0.2 to 0.5. An effect of 7.9% on a minimum wage increase of 37% would imply an elasticity just over 0.2. We note, moreover, that the full $13 minimum did not apply to small business or businesses providing health benefits.

hours reductions between 0.9% and 3.4% (averaging 1.9%) during the three quarters when the minimum wage was $11 per hour. By contrast, the subsequent minimum wage increase to $13 associates with larger, significant hours reductions between 7.9% and 10.6% (averaging 9.4%). Columns 3 and 4 present a parallel analysis for jobs, with qualitatively similar results: statistically weak evidence of reductions in the first phase-in period followed by larger significant impacts in the second. The adverse effects on hours in the final three quarters are proportionately greater than the effects on jobs, suggesting that employers are not only reducing the number of low-wage jobs, but also reducing the hours of retained employees. Multiplying the -6.8% average job estimate by the 92,959 jobs paying less than $19 per hour at baseline suggests that the Ordinance caused the elimination of 6,317 low-wage jobs at locatable firms.[50] Scaled up linearly to account for multi-site single-account firms, job losses would amount to roughly 10,000.[51]

As noted above, there is some concern that our methodology might yield negative estimates in scenarios where increasing labor demand is leading to a rightward shift in the overall wage distribution, pushing a growing number of jobs above any given threshold. We note that the results in Table 6 are consistent with this "rightward shift" hypothesis only under a specific and unusual set of circumstances. In the synthetic control estimates for hours, for example, we observe no significant negative coefficients through the end of 2015 – in fact, the point estimates for the first and last quarters of 2015 are nearly identical. The point estimate exhibits a sudden change in the first quarter of 2016 and then remains at this more negative level without exhibiting any further trend. A confounding rightward shift would have had to occur precisely at the beginning of 2016 – in the winter, the trough period of Seattle's seasonal economy. Figure 3 shows no evidence of such a precisely-timed rightward shift among continuously employed workers tracked longitudinally.

To probe this issue further, Figure 7 illustrates the sensitivity of the estimated effect on hours using different thresholds ranging from jobs paying less than $11 to jobs paying less than $25. For the effect of raising the minimum wage to $11 per hour, shown in the top panel, the

[50] If we base this calculation on just the synthetic control estimates, we would conclude that the Ordinance led to 5,133 fewer jobs paying less than $19 per hour.
[51] We cannot ascertain whether the effect on locatable firms should extrapolate to multi-site single-account firms. As noted above, survey evidence suggests that multi-location firms were more likely to have reported reducing staffing in the wake of minimum wage increases.

estimated impacts become insignificant once the threshold rises to around $17. It appears that any "loss" in hours at lower thresholds likely reflects a cascade of workers to higher wage levels. In contrast, as shown in the bottom panel, the negative estimated effects of the second phase-in to $13 are significant as we raise the threshold all of the way to $25 per hour. Thus, there is no evidence to suggest that the estimated employment losses associated with the second phase-in reflect a similar cascading phenomenon.

Figure 8 illustrates these same results, but multiplies the estimated coefficients by the baseline number of hours worked in jobs paying below the threshold. These results show the estimated absolute change in total hours. We find that during the second phase-in period low-wage hours fell by 3.5 million hours per quarter when the threshold is set at $19 per hour, and this result remains as we increase the threshold to $25 per hour.[52]

Because the estimated magnitude of employment losses exceeds the magnitude of wage gains in the second phase-in period, we would expect a decline in total payroll for jobs paying under $13 per hour relative to baseline. Indeed, we observe this decline in first-differences when comparing "peak" calendar quarters, as shown in Table 3 above. Table 7 confirms this inference in regression specifications examining the impact on payroll for jobs paying less than $19 per hour. Although results are not consistently significant, point estimates suggest payroll declines of 4.0% to 7.6% (averaging 5.8%) during the second phase-in period. This implies that the minimum wage increase to $13 from the baseline level of $9.47 reduced income paid to low-wage employees of locatable Seattle businesses by roughly $120 million on an annual basis.[53]

Note that the largest and only statistically significant payroll estimate corresponds to the first quarter of 2016. This result is notable, as the first quarter tends to be a time of slack demand for low-wage labor (after Christmas and before the summer tourist season) – in effect, Seattle suffers a mini recession every winter. This result could be a harbinger of the effects of the minimum wage in a full recession, or in a less robust local economy, as wages will have less ability to decrease to equilibrate the low-wage labor market.[54]

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[52] Confidence intervals widen as we increase the threshold – we are, in essence, looking for the same needle (i.e., the same 3.5-million-hour decline) in a larger haystack as we increase the threshold.

[53] Simple calculations based on preceding results suggest an effect of comparable magnitude. Wage results suggest a 3% boost to earnings, which on a base of about $530 million paid in the baseline quarter amounts to a $16 million increase in payroll. Employment declines of 3.5 million hours per quarter, valued at $9.47 per hour, equate to a loss of $132 million – and a net loss of $116 million – on an annual basis.

[54] See Clemens (2015), Clemens and Wither (2016), and Clemens and Strain (2017) for evidence of the effects of the Great Recession on impacts of minimum wage increases.

6.5 Elasticity estimates

Column 1 of Table 8 shows our estimate of the elasticity of labor demand with respect to changes in wages computed as the ratio of our estimated effect on hours to our estimated effect on wages, using the synthetic control method, for the six quarters after the Ordinance was enforced.[55] We also compute measures of statistical uncertainty for these elasticities since they are the ratio of two estimates.[56] During the first phase-in, when the minimum wage was $11 per hour, estimated elasticities range from -0.97 to -1.80 (averaging -1.31). Notably, we cannot reject elasticity = -1 with 95% confidence, which is consistent with our finding in Table 7 that we could not reject zero effect on payroll, and we cannot reject elasticity = 0, which is consistent with our finding in Table 6 that we could not reject zero effect on hours. These findings are not artifacts of setting the threshold at $19 per hour. As shown in the upper part of Figure 9, the estimated elasticities range between -1 and 0 when the threshold is set anywhere between $17 and $25 per hour. In summary, the relatively modest estimated wage and hours impacts of the first phase-in create considerable statistical uncertainty regarding the associated elasticity estimate.

After the minimum wage increased to $13 per hour, we find much larger estimated elasticities ranging from -2.66 to -3.46 (averaging -2.98). During these three quarters, we can reject the hypothesis that the elasticity equals zero (consistent with Table 5), and we can reject the hypothesis that the elasticity equals -1 in the first quarter of 2016, consistent with the significant decline in payroll during this quarter shown in Table 6. Point estimates of elasticities imply that, within Seattle, low-wage workers lost $3 from lost employment opportunities for every $1 they gain due to higher hourly wages. These very large elasticities are not artifacts of setting the threshold at $19 per hour. As shown in the lower part of Figure 9, the estimated

[55] One might think that the decline in hours worked was due to a voluntary cut in hours, and thus interpret our findings as showing a labor supply elasticity in the region where the labor supply curve is "backwards bending." While there may be some voluntary reductions in hours by some workers, it would be unreasonable to expect such workers to reduce their hours so far that their total earnings declined. Given that we find that hours fall more than wages rise, the results are more likely to reflect a decline in labor demand.

[56] We computed standard errors for the estimates elasticities using the delta method, taking into account the correlation between estimated effect of the minimum wage on employment and wages.

elasticities are very close to -3 when the threshold is set anywhere between $17 and $25 per hour.[57]

The larger elasticities in the second phase-in period relative to the first suggest that total earnings paid to low-wage workers in Seattle might be maximized with a statutory minimum wage somewhere in the range of $9.47 to $11. By contrast, increases beyond $11 appear to have resulted in net earnings losses in Seattle for these workers.

6.6 Reconciling these estimates with prior work

Most prior studies compute employment elasticities by dividing regression-estimated percentage changes in employment by the percentage change in the statutory minimum wage. Applied in this case, this method would use a denominator of 16.2% (i.e., ($11-$9.47)/$9.47) for the first phase-in period, and 37.3% ($13-$9.47)/$9.47) for the second. The conventional method clearly overstates the actual impact on wages given that many affected workers' wages are above the old minimum but below the new. This method is also unsuitable for evaluating the impacts on workers who began over the new minimum wage but are nonetheless affected by cascading wage increases (defined as the range of either $11 or $13 to $19 per hour). In column 2 of Table 8, we use the conventional approach for computing employment elasticities and find estimates in the range of -0.08 to -0.28 (averaging -0.20). This range is high but not outside of the envelope of estimates found in prior literature (see Appendix Table 3).[58] Thus, computing the elasticity based on the Ordinance's impact on *actual* average wages suggests that the conventional method yields substantial underestimates.

We conclude our analysis by attempting to reconcile our results with prior studies focused on restaurant industry employment. In Table 9, we walk our results back to a sample and outcome that is similar to Card and Krueger's (1994) examination of fast food employment in New Jersey and Pennsylvania in response to New Jersey's increase in its minimum wage. The traditional focus on restaurant employment reflects its common perception as a canonical low-wage industry, and the general absence of data resources allowing a more precise analysis of jobs

[57] While it may be argued that our wage effects combine a large effect on the lowest-paid workers with near-zero impacts on those paid above $13 at baseline, this only implies an overestimated elasticity for the least-paid workers if the employment effects are somehow concentrated among higher-paid workers. Our evidence does not support this conjecture.

[58] Estimates on the high end are plausible because theory suggests that labor demand elasticity would generally be larger for a small, open economy such as Seattle than for a state or the nation.

paying low wages. In 46 of 50 states, there is no data resource allowing the systematic computation of average hourly wage rates for the entire UI-covered workforce.

Column 1 of Table 9 repeats the main results findings from column 1 of Table 6, and is included as a point of reference. Moving from column 1 to column 5 of Table 9, we make one change at a time to evaluate the sensitivity of our results to various modeling choices. In column 2, we use the same specification as in column 1, but restrict the analysis to hours in low-wage jobs in Food Services and Drinking Places (NAICS industry 722). The results are quite comparable to those in column 1 for all industries. We find significant declines in hours worked by low-wage restaurant workers in two of the last three quarters when the wage increased to $13 per hour, and this reduction averages -10.1%. Moving from column 2 to 3, we switch the focus to headcount employment, the outcome used in most prior literature. Again, these results are quite comparable suggesting that nearly all of the reduction in hours worked by low-wage restaurant workers is coming from a reduction in jobs rather than a reduction in hours worked by those who have such jobs.

In columns 4 and 5, we shift from examining low-wage jobs to *all* jobs in the restaurant industry. Here we see a *dramatic change*: the effects on all jobs (hours in all jobs) are insignificant in all quarters and averages +0.4% (-0.8%) in the last three quarters.[59] Thus, by using the imprecise proxy of all jobs in a stereotypically low-wage industry, prior literature may have substantially underestimated the impact of minimum wage increases on the target population.

In summary, utilizing methods more consistent with prior literature allows us to almost perfectly replicate the conventional findings of no, or minor, employment effects. These methods reflect data limitations, however, that our analysis can circumvent. We conclude that the stark differences between our findings and most prior literature reflect in no small part the impact of data limitations on prior work.

7. Conclusion

There is widespread interest in understanding the effects of large minimum wage increases, particularly given efforts in the US to raise the federal minimum wage to $15 per hour

[59] The finding of a more negative effect on all hours than on all jobs in Food and Drinking Places is consistent with Neumark and Wascher's (2000) critique of Card and Krueger (1994).

and the adoption of high minimum wages in several states, cities and foreign countries in the past few years. There is good reason to believe that increasing the minimum wage above some level is likely to cause greater employment losses than increases at lower levels. Wolfers (2016) argues that labor economists need to "get closer to understanding the optimal level of the minimum wage" (p. 108) and that "(i)t would be best if analysts could estimate the marginal treatment effect at each level of the minimum wage level" (p. 110). This paper extends the literature in a number of ways, one of which is by evaluating effects of two consecutive large local minimum wage increases.

Beyond basic causal inference challenges, prior studies have analyzed minimum wage effects using data resources that do not permit the direct observation of hourly wages. In those situations, researchers resort to using proxies for low-wage workers by examining particular industries that employ higher concentrations of low-wage labor or by restricting the analysis to teenagers. This paper demonstrates that such strategies likely misstate the true impact of minimum wage policies on opportunities for low-skilled workers. Our finding of zero impact on headcount employment in the restaurant industry echoes many prior studies. Our findings also demonstrate, however, that this estimation strategy yields results starkly different from methods based on direct analysis of low-wage employment.

Our preferred estimates suggest that the Seattle Minimum Wage Ordinance caused hours worked by low-skilled workers (i.e., those earning under $19 per hour) to fall by 9.4% during the three quarters when the minimum wage was $13 per hour, resulting in a loss of 3.5 million hours worked per calendar quarter. Alternative estimates show the number of low-wage jobs declined by 6.8%, which represents a loss of more than 5,000 jobs. These estimates are robust to cutoffs other than $19.[60] A 3.1% increase in wages in jobs that paid less than $19 coupled with a 9.4%

[60] The finding of significant employment losses, particularly after the second minimum wage increase in 2016, may seem incongruent with unemployment statistics for the City of Seattle, which suggest very low numbers of unemployed individuals seeking work. The Bureau of Labor Statistics' Local Area Unemployment Statistics program estimates city-level unemployment statistics on the basis of unemployment insurance claims, data from other government surveys such as the Current Population Survey, and statistical modeling. The unemployment statistics pertain to the residents of a city, not individuals employed in a city (indeed, unemployed workers are employed in no city). Our analysis pertains instead to individuals employed in Seattle.

In Washington State, workers are eligible for UI benefits only after they have accumulated 680 hours of work. In low-wage, high-turnover businesses, the proportion of separated workers who reach this threshold may be low. Further, longitudinal analysis of ESD data suggest that reduced employment largely impacts new entrants to the labor force, rather than experienced workers. New entrants are not eligible for UI benefits and thus cannot generate claims. These unemployed new entrants might be captured in the CPS, but with a relatively small sample size these estimates are subject to significant noise and are smoothed considerably.

loss in hours yields a labor demand elasticity of roughly -3.0, and this large elasticity estimate is robust to other cutoffs.

These results suggest a fundamental rethinking of the nature of low-wage work. Prior elasticity estimates in the range of zero to -0.2 suggest there are few suitable substitutes for low-wage employees, that firms faced with labor cost increases have little option but to raise their wage bill. Seattle data show – even in simple first differences – that payroll expenses on workers earning under $19 per hour either rose minimally or fell as the minimum wage increased from $9.47 to $13 in just over nine months. An elasticity of -3 suggests that low-wage labor is a more substitutable, expendable factor of production. The work of least-paid workers might be performed more efficiently by more skilled and experienced workers commanding a higher wage. This work could, in some circumstances, be automated. In other circumstances, employers may conclude that the work of least-paid workers need not be done at all.

Importantly, the lost income associated with the hours reductions exceeds the gain associated with the net wage increase of 3.1%. Using data in Table 3, we compute that the average low-wage employee was paid $1,897 per month. The reduction in hours would cost the average employee $179 per month, while the wage increase would recoup only $54 of this loss, leaving a net loss of $125 per month (6.6%), which is sizable for a low-wage worker.

The estimates may be much larger than those reported in prior minimum wages studies for three reasons. First, theory suggests that labor demand elasticity would generally be larger for a small, open economy such as Seattle than for a state or the nation. Yet, there is evidence to suggest that our results are not simply divergent from the literature due to this issue. Note that Seattle data produce an effect estimate of zero when we adopt the traditional approach of studying restaurant employment at all wage levels.

Second, rather than using the statutory change in the minimum wage as the denominator in an elasticity computation, we use the change in actual wage rates for low-skill workers, which we can estimate from the Washington data. Because the actual change is necessarily smaller than the statutory change, the arithmetic of elasticity computation leads to larger estimated elasticities than those derived using conventional methods of computing the elasticity of demand for low-skill workers with respect to the statutory change in minimum wage.

Third, we analyze the impact of raising the minimum wage to a significantly higher level than what has been analyzed in most prior work. Deflating by the Personal Consumption

Expenditures price index, the real value of the federal minimum wage has never reached the $13 level studied in our analysis. Theory suggests that the impact of raising the minimum wage depends critically on the starting point; Seattle started from the nation's highest state minimum wage, and our own evidence indicates that the effects differed dramatically from the first phase-in period to the second.

A few cautions should be noted. Our analysis includes only firms reporting employment at specific locations, as we cannot properly locate employment for multi-location firms that do not report employment separately by location. It may be the case that the labor demand elasticity of locatable firms is larger than that of multi-site firms who do not report employment at specific locations. Yet, as discussed above, multi-site firms that we surveyed were more likely to self-report cuts in employment than smaller firms.[61]

Further, we lack data on contractor jobs which get 1099 forms instead of W-2s and on jobs in the informal economy paid with cash. If the Ordinance prompted an increase in low-wage workers being paid as contractors or under the table, our results would overstate the effect on jobs and hours worked. However, such a move would not be without consequence for the workers, who would lose protections from the Unemployment Insurance and Worker's Compensation systems and not receive credit toward future Social Security benefits for such earnings (though they would not have to pay the full amount of taxes for Social Security and Medicare).

In addition, some employers may have shifted jobs out of Seattle but kept them within the metropolitan area, in which case the job losses in Seattle overstate losses in the local labor market. Reductions in payroll attributable to the minimum wage may exceed reductions in income for the affected workers, to the extent they were able to take advantage of relocated opportunities in the metropolitan area. Finally, the long-run effects of Seattle's minimum wage increases may be substantially greater, particularly since subsequent changes beyond a final increase to $15 per hour will be indexed to inflation, unlike most of the minimum wage increases that have been studied in the literature, which have quickly eroded in real terms (Wolfers, 2016).

[61] If we ignore our survey evidence and suppose that multi-site firms' wage impact was the same as reported here but their hours impact was zero, the elasticity would still be high compared to earlier work – around -1.9 (as single-site businesses employ 62% of the workforce).

One cannot assume our specific findings generalize to minimum wage policies set by other localities or at the federal or state level. The impacts of minimum wage policies established by other local governments likely depend on the industrial structure, characteristics of the local labor force, and other features of the local and regional economy.

Last, there may be important forms of effect heterogeneity across workers. Some workers may well have experienced significant wage increases with no reduction in hours; others may have encountered significantly greater difficulty in securing any work at all. From a welfare perspective, it is critical to understand how this heterogeneity plays out across low-skilled workers in varying life circumstances. Such an exploration is beyond the scope of this paper, which uses a data resource that identifies no pertinent information about individual workers. Future work will take advantage of linkages across administrative data resources within Washington State to understand how the minimum wage affects workers in varying demographic categories, or with a history of reliance on means-tested transfer programs.

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Tables and Figures

Table 1: Minimum Wage Schedule in Seattle under the Seattle Minimum Wage Ordinance

	Large Employers[a]		Small Employers	
Effective Date	No benefits	With benefits[b]	No benefits or tips	Benefits or tips[c]
	Before Seattle Ordinance			
January 1, 2015	\$9.47	\$9.47	\$9.47	\$9.47
	After Ordinance			
April 1, 2015	\$11.00	\$11.00	\$11.00	\$10.00
January 1, 2016	\$13.00	\$12.50	\$12.00	\$10.50
January 1, 2017	\$15.00[d]	\$13.50	\$13.00	\$11.00
January 1, 2018		\$15.00[e]	\$14.00	\$11.50
January 1, 2019			\$15.00[f]	\$12.00
January 1, 2020				\$13.50
January 1, 2021				\$15.00[g]

Notes:

a A large employer employs 501 or more employees worldwide, including all franchises associated with a franchise or a network of franchises.
b Employers who pay towards medical benefits.
c Employers who pay toward medical benefits and/or employees who are paid tips.
Total minimum hourly compensations (including tips and benefits) is the same as for small employers who do not pay towards medical benefits and/or tips.
d For large employers, in the years after the minimum wage reaches \$15.00 it is indexed to inflation using the CPI-W for Seattle-Tacoma-Bremerton Area.
e Starting January 1, 2019, payment by the employer of medical benefits for employees no longer affects the hourly minimum wage paid by a large employer.
f After the minimum hourly compensation for small employers reaches \$15 it goes up to \$15.75 until January 1, 2021 when it converges with the minimum wage schedule for large employers.
g The minimum wage for small employers with benefits or tips will converge with other employers by 2025.

Table 2: Characteristics of Included and Excluded Firms, Washington State

	Included in Analysis	Excluded from Analysis	Share Included
Number of Firms	123,180	14,917	89.2%
Number of Establishments (i.e., Sites)	140,451	Unknown	
Total Number of Employees	1,672,448	1,019,875	62.1%
Number of Employees paid <$19/hour	725,231	425,023	63.0%
Employees / Firm	14	68	
Employees / Establishment	12	Unknown	

Notes: Firms are defined as entities with unique federal tax Employer Identification Numbers. Statistics are computed for the average quarter between 2005.1 and 2016.3. “Excluded from Analysis” includes firms whose location could not be determined.

Table 3: Employment Statistics for Seattle's Locatable Establishments

Quarter	Quarters After Passage/ Enforcement	Number of Jobs: Under $13	Number of Jobs: Under $19	Number of Jobs: All	Total Hours (thousands): Under $13	Total Hours (thousands): Under $19	Total Hours (thousands): All	Average Wage: Under $13	Average Wage: Under $19	Average Wage: All	Total Payroll ($mlns.): Under $13	Total Payroll ($mlns.): Under $19	Total Payroll ($mlns.): All
		Hourly wage rates:			Hourly wage rates:			Hourly wage rates:			Hourly wage rates:		
Panel A: All Industries													
2014.2	0	39,807	92,959	292,640	14,117	37,408	130,007	11.14	14.14	36.93	157	529	4,802
2014.3	1	40,706	94,913	300,892	14,527	38,565	132,604	11.15	14.15	37.76	162	546	5,007
2014.4	2	35,421	89,598	303,089	11,999	35,589	136,012	11.27	14.37	39.78	135	511	5,410
2015.1	3	35,085	90,813	305,229	11,335	34,269	132,275	11.28	14.41	40.61	128	494	5,371
2015.2	4/1	35,075	92,668	311,886	12,174	37,270	139,197	11.47	14.48	38.52	140	540	5,362
2015.3	5/2	33,959	93,382	320,807	11,589	37,472	142,638	11.54	14.58	39.83	134	546	5,681
2015.4	6/3	30,002	87,067	320,195	9,924	34,943	146,960	11.64	14.74	41.73	116	515	6,133
2016.1	7/4	24,662	87,122	321,360	7,645	33,031	140,429	11.82	14.97	43.90	90	494	6,164
2016.2	8/5	24,420	88,431	331,927	8,315	35,681	149,514	11.87	15.01	44.04	99	535	6,584
2016.3	9/6	23,232	86,842	336,517	8,046	35,867	153,603	11.87	15.03	43.60	96	539	6,697
Panel B: Food and Drinking Places (NAICS 722)													
2014.2	0	10,614	18,788	28,276	3,707	6,772	9,941	10.96	12.99	17.53	41	88	174
2014.3	1	10,825	19,581	29,815	3,792	7,229	10,763	10.94	13.10	17.82	41	95	192
2014.4	2	9,778	19,278	30,237	3,253	6,857	10,458	11.05	13.35	18.54	36	92	194
2015.1	3	9,682	19,493	30,505	3,044	6,567	10,100	11.08	13.44	18.62	34	88	188
2015.2	4/1	9,006	19,122	30,500	3,025	6,874	10,629	11.38	13.67	18.65	34	94	198
2015.3	5/2	8,376	19,622	31,895	2,843	7,282	11,500	11.47	13.94	19.09	33	101	219
2015.4	6/3	7,566	19,550	32,439	2,461	7,107	11,398	11.54	14.15	19.74	28	101	225
2016.1	7/4	5,869	18,651	31,469	1,730	6,307	10,396	11.83	14.54	20.07	20	92	209
2016.2	8/5	6,155	18,504	31,980	1,983	6,756	11,222	11.90	14.56	19.92	24	98	224
2016.3	9/6	6,050	18,542	32,402	2,034	7,236	12,088	11.85	14.59	20.11	24	106	243

Note: Data derived from administrative employment records obtained from the Washington Employment Security Department. Non-locatable employers (i.e., multi-site single-account firms) are excluded.

Table 4: Falsification Test: Pseudo-Effect of Placebo Law Passed in 2012

Quarter	Quarters after (pseudo) Passage/ Enforcement	Difference-in-Differences between Seattle and: Outlying King County Wage	Hours	Snohomish, Kitsap, and Pierce Counties Wage	Hours	Synthetic Control Washington excluding King County Wage	Hours	Interactive Fixed Effects Washington excluding King County Wage	Hours
2012.3	1	0.001*	-0.044***	-0.003**	-0.014***	0.001	-0.014	-0.002	-0.012
		(0.001)	(0.004)	(0.002)	(0.006)	(0.003)	(0.015)	(0.003)	(0.013)
2012.4	2	-0.002***	-0.033***	-0.003*	-0.038***	0.001	-0.018	-0.001	-0.022
		(0.001)	(0.004)	(0.002)	(0.006)	(0.003)	(0.021)	(0.003)	(0.014)
2013.1	3	0.002***	-0.034***	0.001	-0.028***	0.001	-0.002	0.000	-0.017
		(0.001)	(0.004)	(0.002)	(0.006)	(0.003)	(0.020)	(0.003)	(0.038)
2013.2	4/1	0.003***	-0.022***	0.005***	-0.036***	0.001	0.004	0.001	-0.016
		(0.001)	(0.004)	(0.002)	(0.006)	(0.003)	(0.026)	(0.003)	(0.038)
2013.3	5/2	0.003***	-0.063***	-0.002	-0.063***	0.004	-0.006	-0.002	-0.024
		(0.001)	(0.007)	(0.003)	(0.012)	(0.005)	(0.022)	(0.004)	(0.041)
2013.4	6/3	0.003**	-0.069***	-0.006*	-0.095***	0.006	-0.009	0.000	-0.034
		(0.001)	(0.007)	(0.003)	(0.012)	(0.004)	(0.033)	(0.004)	(0.049)
2014.1	7/4	0.003**	-0.031***	0.001	-0.047***	0.005	0.028	-0.001	-0.008
		(0.001)	(0.007)	(0.003)	(0.012)	(0.004)	(0.029)	(0.004)	(0.053)
2014.2	8/5	0.006***	-0.031***	0.004	-0.059***	0.008***	0.014	0.003	-0.024
		(0.001)	(0.007)	(0.003)	(0.012)	(0.004)	(0.031)	(0.004)	(0.055)
2014.3	9/6	0.004**	-0.046***	-0.001	-0.073***	0.010*	0.013	0.000	-0.019
		(0.002)	(0.011)	(0.005)	(0.017)	(0.005)	(0.031)	(0.005)	(0.081)
Average		0.003	-0.041	0.000	-0.050	0.004	0.001	0.000	-0.019
Obs.		68	68	68	68	1,530	1,530	1,530	1,530

Notes: Standard errors in parentheses. Clustered standard errors reported for difference-in-differences; permutation inference standard errors are reported for synthetic control, iid standard errors are reported for interactive fixed effects. Estimates for all jobs paying < $19 in all industries. The number of observations used in the synthetic control and interactive fixed effects specifications equals the number of PUMAs (45) times the number of quarters included in this analysis (34). However, note that some of these PUMAs receive zero weight in the synthetic control results.***, **, and * denote statistically significance using a two-tailed test with $p \leq 0.01$, 0.05, and 0.10, respectively.

Table 5: Main Results: Effect on Wages of Low-Wage Jobs

Quarter	Quarters after Passage/ Enforcement	Synthetic Control	Interactive FE
2014.3	1	0.003 (0.003)	0.003 (0.003)
2014.4	2	0.003 (0.003)	0.006** (0.003)
2015.1	3	0.005 (0.004)	0.007*** (0.003)
2015.2	4/1	0.014*** (0.004)	0.014*** (0.003)
2015.3	5/2	0.019*** (0.005)	0.019*** (0.004)
2015.4	6/3	0.018*** (0.004)	0.018*** (0.004)
2016.1	7/4	0.031*** (0.005)	0.028*** (0.005)
2016.2	8/5	0.033*** (0.006)	0.029*** (0.005)
2016.3	9/6	0.036*** (0.007)	0.031*** (0.006)

Notes: n=1,890. Standard errors in parentheses. Permutation inference standard errors are reported for synthetic control, while iid standard errors are reported for interactive fixed effects. Estimates for all jobs paying < $19 in all industries, where the control region is defined as the state of Washington excluding King County. The number of observations equals the number of PUMAs (45) times the number of quarters included in this analysis (42). However, note that some of these PUMAs receive zero weight in the synthetic control results.

***, **, and * denote statistically significance using a two-tailed test with $p \leq 0.01$, 0.05, and 0.10, respectively.

Table 6: Main Results: Effect on Low-Wage Employment

Quarter	Quarters since Passage/ Enforcement	Hours SC	Hours IFE	Jobs SC	Jobs IFE
2014.3	1	0.008	0.004	0.004	-0.006
		(0.018)	(0.013)	(0.017)	(0.015)
2014.4	2	0.003	-0.001	-0.010	-0.023
		(0.018)	(0.013)	(0.021)	(0.015)
2015.1	3	-0.023	-0.018	0.000	-0.013
		(0.018)	(0.013)	(0.023)	(0.015)
2015.2	4/1	-0.013	-0.014	-0.014	-0.032**
		(0.019)	(0.014)	(0.019)	(0.015)
2015.3	5/2	-0.034	-0.022	-0.019	-0.035*
		(0.025)	(0.020)	(0.021)	(0.021)
2015.4	6/3	-0.021	-0.009	-0.045	-0.048***
		(0.033)	(0.019)	(0.029)	(0.020)
2016.1	7/4	-0.106***	-0.090***	-0.051*	-0.053***
		(0.031)	(0.024)	(0.028)	(0.021)
2016.2	8/5	-0.087***	-0.079***	-0.052*	-0.083***
		(0.031)	(0.027)	(0.028)	(0.020)
2016.3	9/6	-0.102***	-0.100***	-0.063*	-0.106***
		(0.042)	(0.034)	(0.036)	(0.024)

Notes: Standard errors in parentheses. Permutation inference standard errors are reported for synthetic control, while iid standard errors are reported for interactive fixed effects. N=1,890. Estimates for all jobs paying < $19 in all industries, where the control region is defined as the state of Washington excluding King County. The number of observations equals the number of PUMAs (45) times the number of quarters included in this analysis (42). However, note that some of these PUMAs receive zero weight in the synthetic control results.

***, **, and * denote statistically significance using a two-tailed test with $p \leq 0.01$, 0.05, and 0.10, respectively.

Table 7: Main Results: Effect on Payroll for Low-Wage Jobs

Quarter	Quarters since passage/ enforcement	Synthetic Control	Interactive Fixed Effects
2014.3	1	0.011 (0.018)	0.010 (0.013)
2014.4	2	0.008 (0.018)	0.003 (0.013)
2015.1	3	-0.016 (0.019)	-0.014 (0.014)
2015.2	4/1	0.002 (0.019)	0.002 (0.014)
2015.3	5/2	-0.013 (0.025)	0.004 (0.020)
2015.4	6/3	-0.002 (0.034)	0.011 (0.019)
2016.1	7/4	-0.076*** (0.034)	-0.054* (0.029)
2016.2	8/5	-0.053 (0.032)	-0.040 (0.031)
2016.3	9/6	-0.065 (0.044)	-0.060 (0.038)

Notes: n=1,890. Standard errors in parentheses. Permutation inference standard errors are reported for synthetic control, while iid standard errors are reported for interactive fixed effects. Estimates for all jobs paying < $19 in all industries, where the control region is defined as the state of Washington excluding King County. The number of observations equals the number of PUMAs (45) times the number of quarters included in this analysis (42). However, note that some of these PUMAs receive zero weight in the synthetic control results.

***, **, and * denote statistically significance using a two-tailed test with $p \leq 0.01$, 0.05, and 0.10, respectively.

Table 8: Estimates of the Elasticity of Labor Demand with respect to Minimum Wages

Quarter	Quarters after Passage/ Enforcement	Denominator is synthetic control estimated wage effect		Denominator is statutory increase in minimum wage	
		Point Estimate	95% Conf. Int.	Point Estimate	95% Conf. Int.
2015.2	4/1	-0.97	(-3.75, 1.81)	-0.08	(-0.32, 0.15)
2015.3	5/2	-1.80	(-4.49, 0.90)	-0.21	(-0.51, 0.09)
2015.4	6/3	-1.16	(-4.81, 2.50)	-0.13	(-0.53, 0.27)
2016.1	7/4	-3.46	(-5.87, -1.04)	-0.28	(-0.45, -0.12)
2016.2	8/5	-2.66	(-4.79, -0.54)	-0.23	(-0.40, -0.07)
2016.3	9/6	-2.82	(-5.38, -0.27)	-0.27	(-0.50, -0.05)

Notes: Confidence interval based on permutation inference. Estimates for all jobs paying < \$19 in all industries, where the control region is defined as the state of Washington excluding King County. % Δ Min. Wage is defined as (\$11 - \$9.47)/\$9.47 for quarters 1-3 after enforcement, and as (\$13 - \$9.47)/\$9.47 for quarters 4-6 after enforcement.

Table 9 : Effect of Restricting Analysis to Food Service and Drinking Places

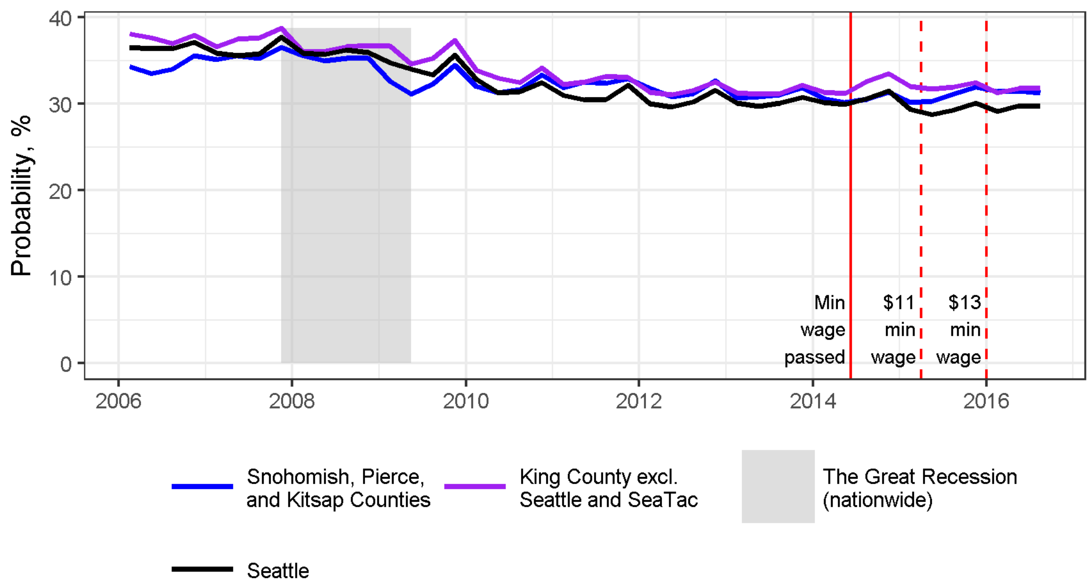
Quarter	Quarter since Passage/ Enforcement	All industries: Wages under $19: Hours	Restaurant Industry (NAICS 722): Wages under $19: Hours	Restaurant Industry (NAICS 722): Wages under $19: Jobs	Restaurant Industry (NAICS 722): All wage levels: Jobs	Restaurant Industry (NAICS 722): All wage levels: Hours
2014.3	1	0.008	-0.008	0.039	0.038	-0.008
		(0.018)	(0.030)	(0.030)	(0.029)	(0.029)
2014.4	2	0.003	-0.008	-0.006	0.035	0.009
		(0.018)	(0.031)	(0.038)	(0.037)	(0.030)
2015.1	3	-0.023	-0.022	-0.005	-0.001	-0.008
		(0.018)	(0.043)	(0.039)	(0.038)	(0.039)
2015.2	4/1	-0.013	-0.040	-0.033	0.008	-0.003
		(0.019)	(0.038)	(0.038)	(0.036)	(0.038)
2015.3	5/2	-0.034	-0.071	-0.019	0.031	-0.027
		(0.025)	(0.050)	(0.049)	(0.051)	(0.052)
2015.4	6/3	-0.021	-0.036	-0.077*	0.002	0.023
		(0.033)	(0.054)	(0.047)	(0.048)	(0.056)
2016.1	7/4	-0.106***	-0.101*	-0.110**	-0.016	-0.005
		(0.031)	(0.059)	(0.052)	(0.057)	(0.069)
2016.2	8/5	-0.087***	-0.099*	-0.122**	0.031	0.006
		(0.031)	(0.060)	(0.058)	(0.066)	(0.070)
2016.3	9/6	-0.102***	-0.102	-0.105*	-0.004	-0.024
		(0.042)	(0.066)	(0.056)	(0.067)	(0.078)

Notes: n=1,890. Standard errors in parentheses. Permutation inference standard errors are reported for synthetic control. The control region is defined as the state of Washington excluding King County. Estimates using Synthetic Control reported. NAICS 722 = Food services and drinking places. The number of observations equals the number of PUMAs (45) times the number of quarters included in this analysis (42). However, note that some of these PUMAs receive zero weight in the synthetic control results.

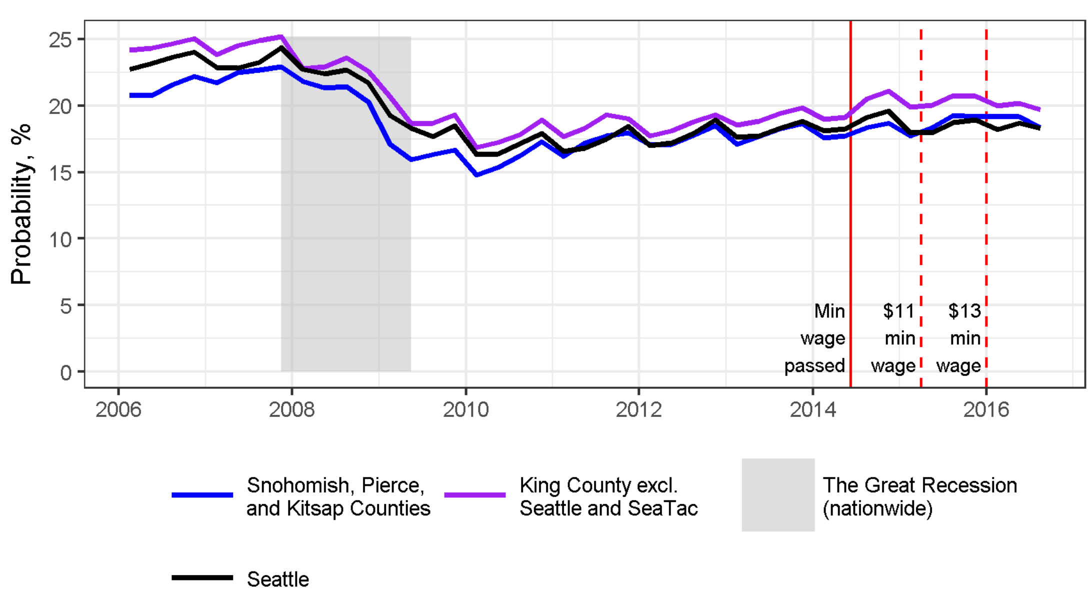
***, **, and * denote statistically significance using a two-tailed test with $p \leq 0.01$, 0.05, and 0.10, respectively.

Figure 1: Rates of Transition from Locatable to Non-Locatable Employment

Panel A. *P*(non-locatable job in t | locatable and paid under $19/hour in t-4, employed in WA in t) by initial location

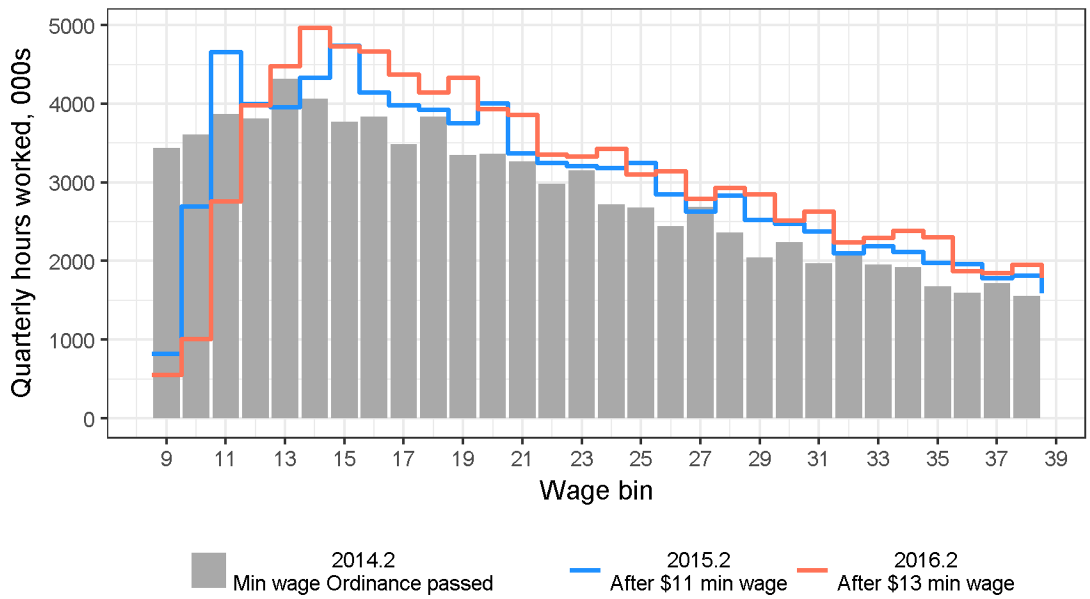


Pane B. *P*(non-locatable job in t | locatable and paid under $19/hour in t-4) by initial location



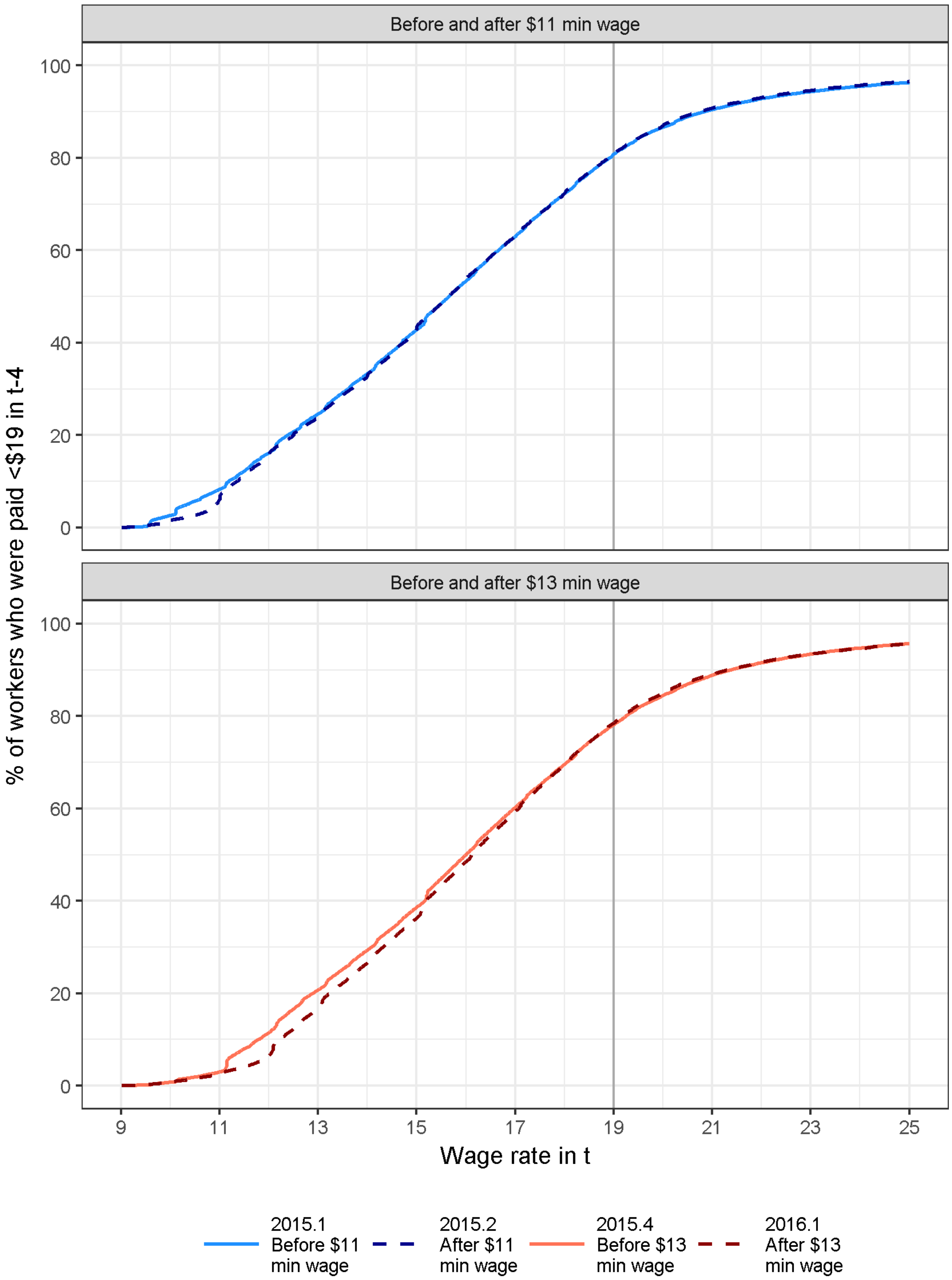
Notes: Non-locatable jobs are defined as those in a non-locatable business anywhere in Washington State. Hourly wages are inflation-adjusted to the 2nd quarter of 2015 using CPI-W.

Figure 2: Changes in the Wage Distribution in Seattle



Notes: Authors calculations based on UI records from State of WA using the sample of jobs in locatable employers in Seattle. Wage rates and earnings are expressed in constant prices of 2015 Q2.

Figure 3: Cumulative Density Function for Wages of Low-wage Workers



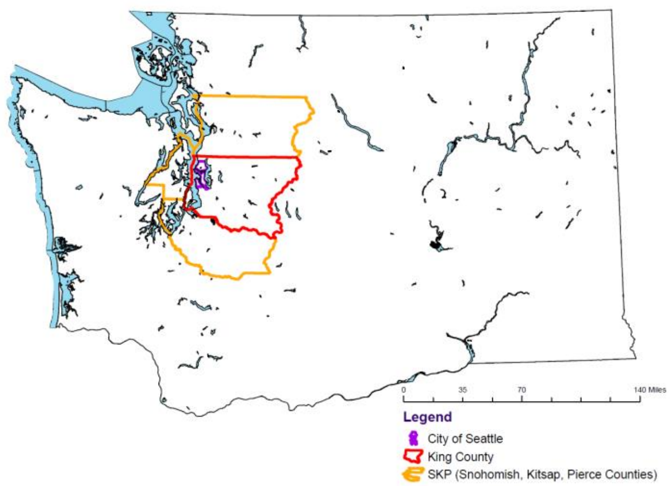
Notes: Workers who were employed in Seattle by locatable establishments in periods t and t-4, and paid less than $19 in t-4.

Figure 4: Geography of Seattle and King, Snohomish, Kitsap, and Pierce Counties

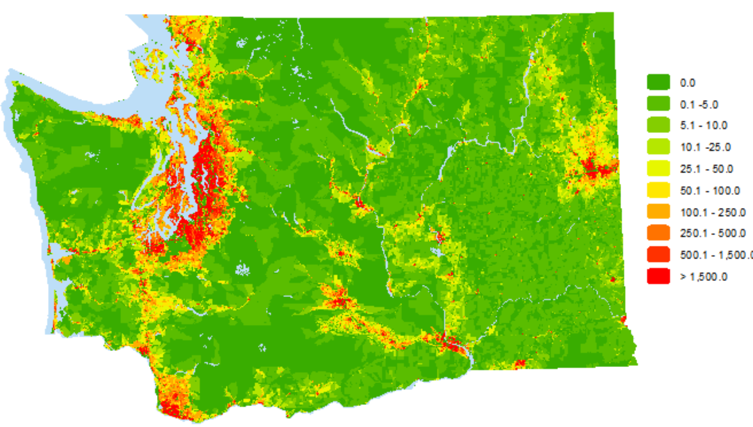
Panel A: Seattle's Water Boundaries

Source: https://www.google.com/maps/

Panel B: Difference-in-Differences Regions



Panel C: Population Density by Census Block, 2010



Source: http://www.ofm.wa.gov/pop/census2010/pl/maps/map05.asp

Figure 5: Geography of Washington's PUMAs

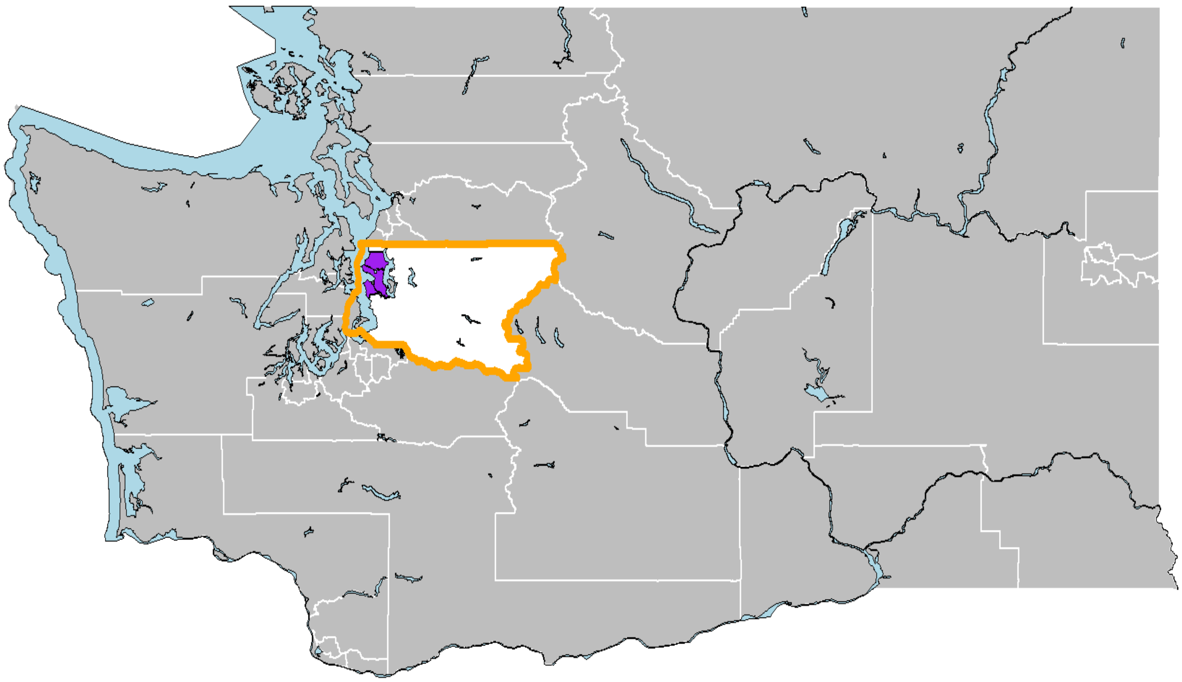


Figure 6: Employment, Wages, and Payroll in Seattle Compared to Synthetic Seattle in Jobs Paying Less than $19 Per Hour

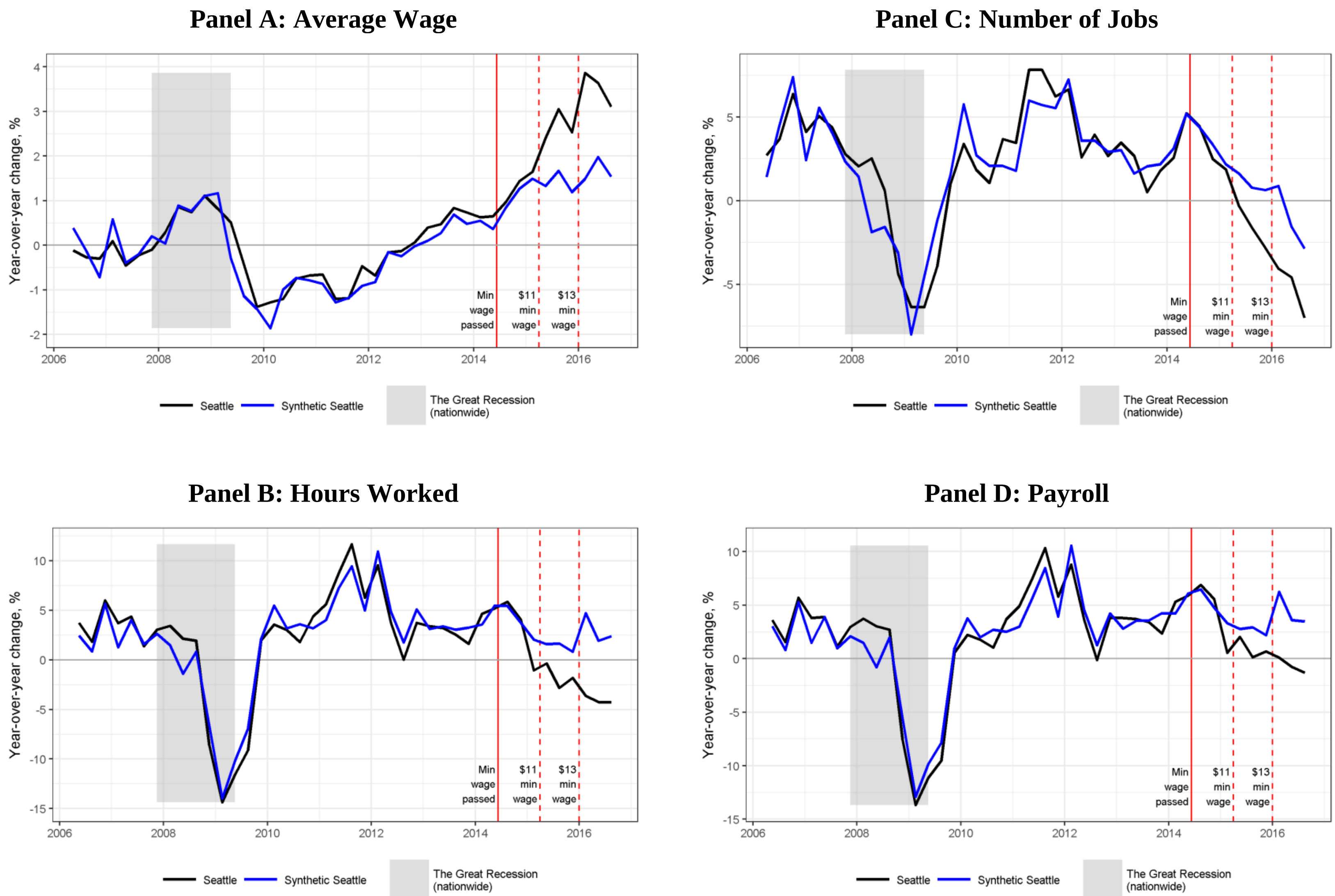
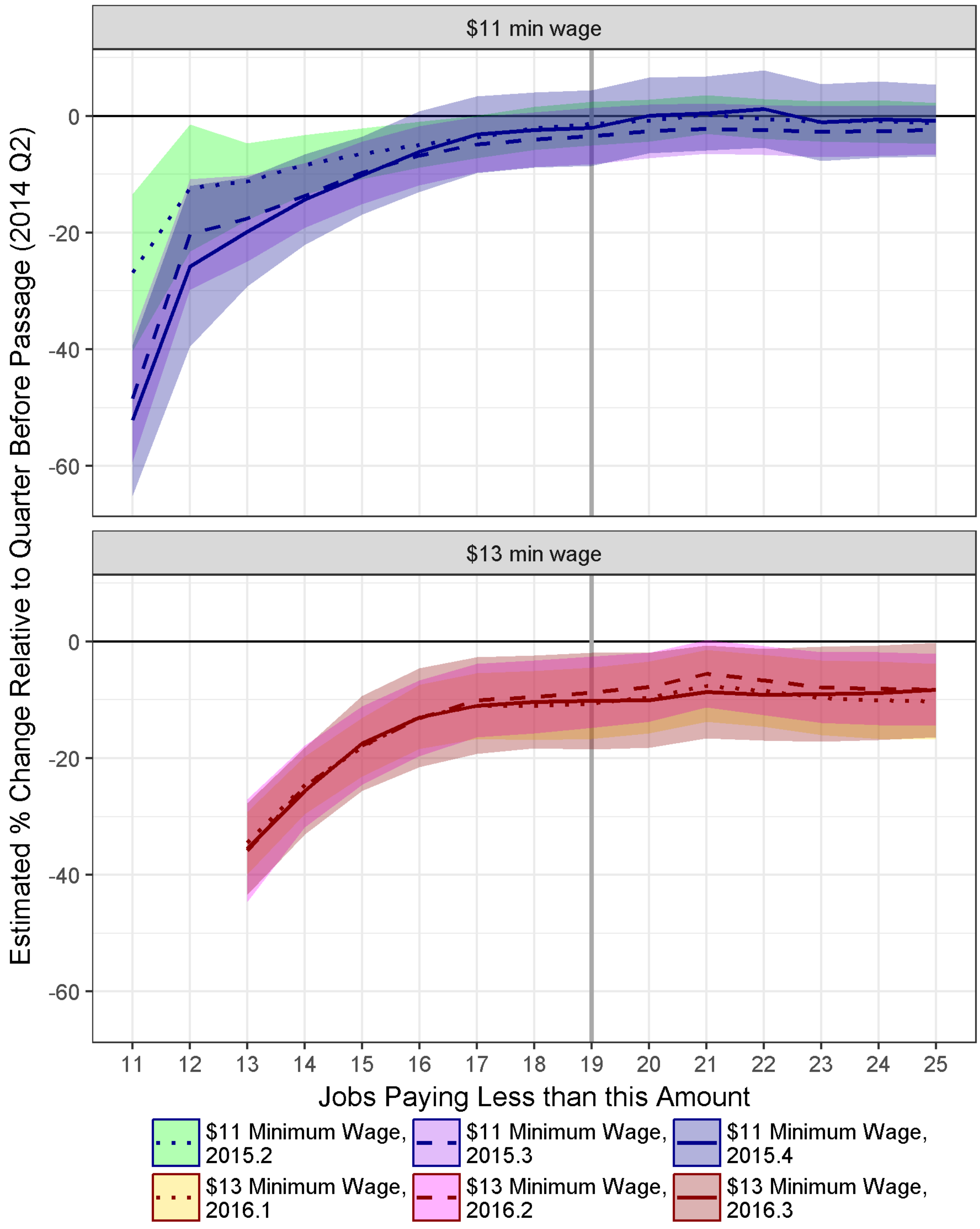
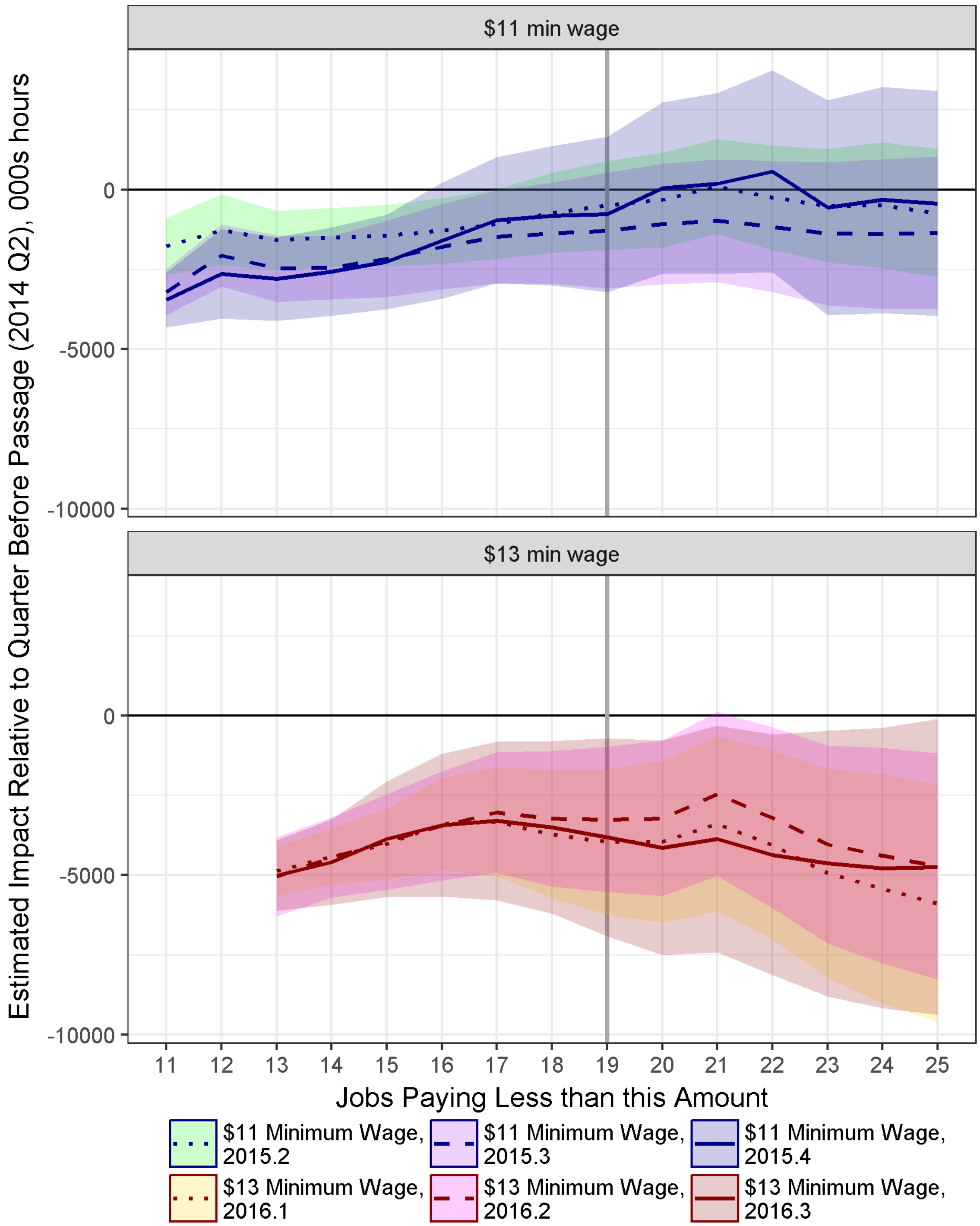


Figure 7: Sensitivity of the Estimated Effects on Percentage Change in Hours Worked Using Different Thresholds



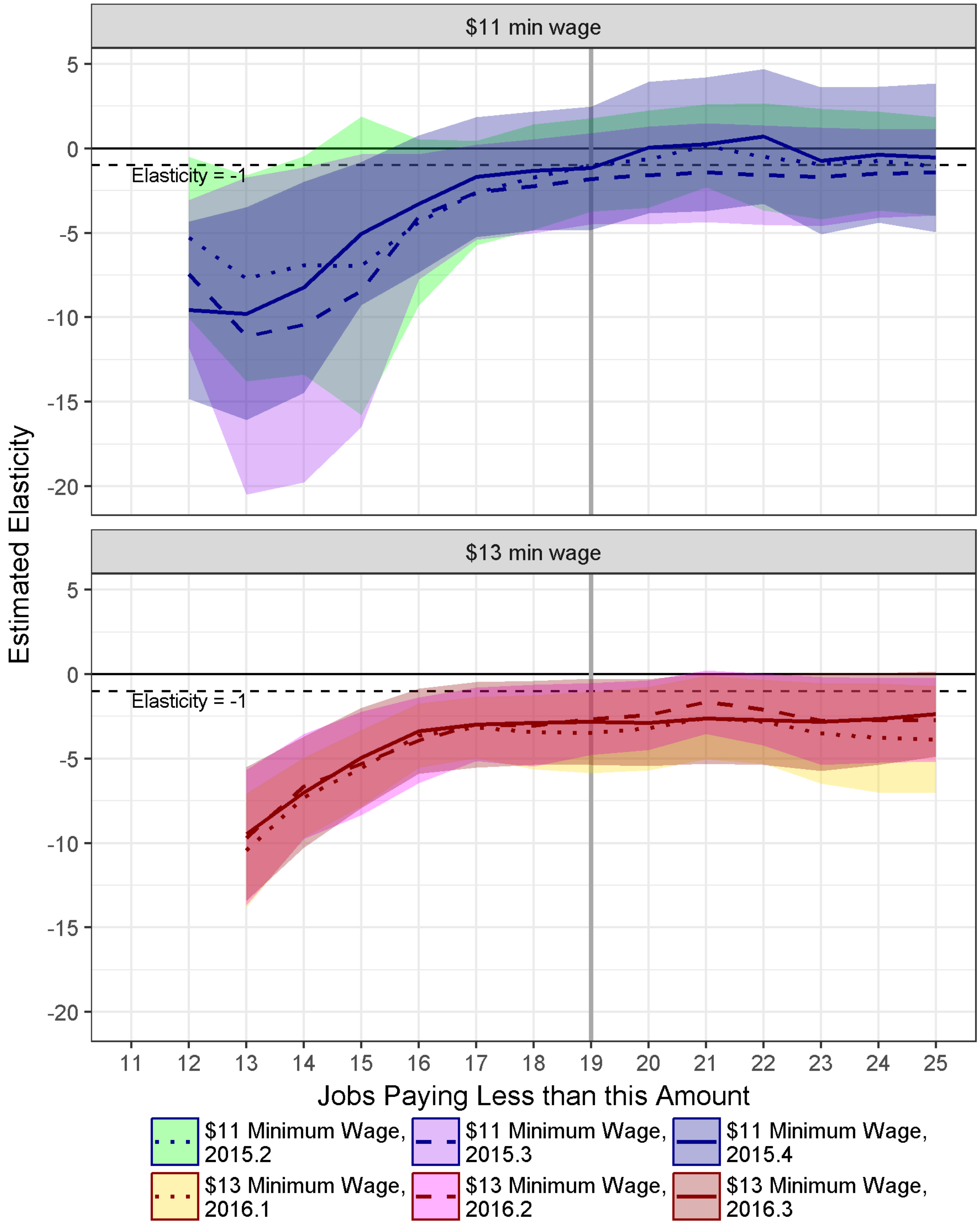
Notes: Point estimates using the synthetic control method are shown by the lines, while 95% confidence intervals centered around these estimates are shown by the shaded regions.

Figure 8: Sensitivity of the Estimated Effects on Total Hours Worked Using Different Thresholds



Notes: Point estimates using the synthetic control method are shown by the lines, while 95% confidence intervals centered around these estimates are shown by the shaded regions.

Figure 9: Sensitivity of the Estimated Elasticity of Labor Demand With Respect to Wages Using Different Thresholds



Notes: Point estimates using the synthetic control method are shown by the lines, while 95% confidence intervals centered around these estimates are shown by the shaded regions.

On-Line Appendix Tables and Figures

Appendix Table 1: Number of Jobs in Seattle's Locatable Establishments, by Industry and Wage Level

Industry (NAICS Sector)	Total Number of Employees			Number of Employees paid <$19 per hour		
	Included in Analysis	Excluded from Analysis	Share Included	Included in Analysis	Excluded from Analysis	Share Included
Agriculture, Forestry, Fishing and Hunting	60,714	20,065	75.2%	50,650	17,053	74.8%
Mining, Quarrying, and Oil and Gas Extraction	1,677	857	66.2%	325	91	78.1%
Utilities	6,777	7,513	47.4%	670	320	67.7%
Construction	130,621	19,380	87.1%	31,720	3,546	89.9%
Manufacturing	146,599	130,360	52.9%	61,200	20,323	75.1%
Wholesale Trade	74,148	45,109	62.2%	26,516	14,746	64.3%
Retail Trade	135,748	173,901	43.8%	85,816	115,401	42.6%
Transportation and Warehousing	47,059	46,900	50.1%	17,915	10,082	64.0%
Information	72,647	31,425	69.8%	7,617	6,734	53.1%
Finance and Insurance	36,354	58,924	38.2%	9,335	16,697	35.9%
Real Estate and Rental and Leasing	31,130	14,672	68.0%	15,741	7,163	68.7%
Professional, Scientific, and Technical Services	117,455	32,765	78.2%	22,423	6,229	78.3%
Management of Companies and Enterprises	3,832	3,798	50.2%	458	1,142	28.6%
Administrative and Support and Waste Management and Remediation Services	96,906	51,992	65.1%	48,732	33,148	59.5%
Educational Services	179,519	62,173	74.3%	57,383	15,665	78.6%
Health Care and Social Assistance	212,455	143,618	59.7%	106,209	66,186	61.6%
Arts, Entertainment, and Recreation	49,248	9,025	84.5%	31,737	5,273	85.8%
Accommodation and Food Services	132,324	79,971	62.3%	106,242	60,561	63.7%
Other Services (except Public Administration)	58,944	19,379	75.3%	31,243	12,882	70.8%
Public Administration	78,291	68,002	53.5%	13,295	11,746	53.1%
Total	1,672,448	1,019,875	62.1%	725,231	425,023	63.0%

Notes: Firms are defined by federal tax Employer Identification Numbers. Statistics are computed for the average quarter between 2005.1 to 2016.3. "Excluded from Analysis" includes two categories of firms: (1) Multi-location firms (flagged as such in UI data), and (2) Single-location firms which operate statewide or whose location could not be determined.

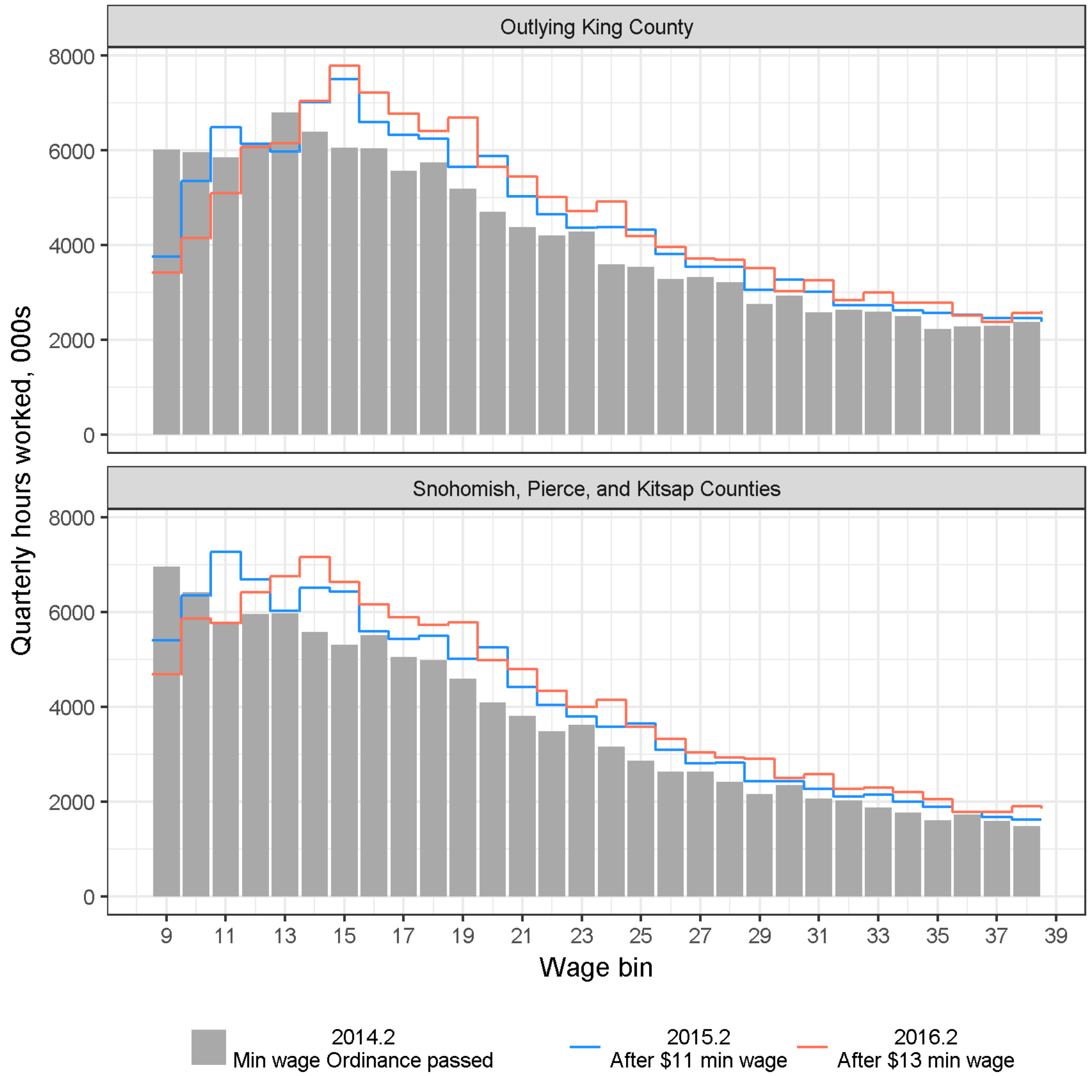
Appendix Table 2: Number of Jobs in Seattle's Locatable Establishments, by Wage Level

	Quarters After:	Number of jobs, absolute value						
		Jobs paying						
Quarter	Passage / Enforcement	Under $13	$13 to $19	$19 to $25	$25 to $30	$30 to $35	$35 to $40	$40 and above
Panel A: Seattle								
2014.2	**0**	39,807	53,152	44,076	27,793	21,848	20,016	85,948
2014.3	**1**	40,706	54,207	43,795	27,375	21,683	19,908	93,218
2014.4	**2**	35,421	54,177	43,494	28,947	22,920	20,685	97,445
2015.1	**3**	35,085	55,728	43,341	28,919	23,102	20,891	98,163
2015.2	**4/1**	35,075	57,593	45,609	30,085	23,920	19,192	100,412
2015.3	**5/2**	33,959	59,423	45,208	30,140	23,889	21,355	106,833
2015.4	**6/3**	30,002	57,065	44,548	30,547	24,154	22,310	111,569
2016.1	**7/4**	24,662	62,460	45,794	30,730	24,585	22,158	110,971
2016.2	**8/5**	24,420	64,011	49,437	32,155	25,670	22,800	113,434
2016.3	**9/6**	23,232	63,610	49,047	31,277	24,816	23,059	121,476
Panel B: Washington State (including Seattle)								
2014.2	**0**	458,807	434,216	307,615	174,202	130,385	108,336	401,680
2014.3	**1**	481,075	431,208	307,262	177,187	130,441	104,748	440,004
2014.4	**2**	431,551	451,306	312,764	188,893	139,294	114,271	439,626
2015.1	**3**	433,749	441,660	304,120	184,817	136,687	113,934	432,791
2015.2	**4/1**	434,072	461,186	317,136	186,442	137,569	110,101	444,056
2015.3	**5/2**	441,220	461,944	315,665	191,594	139,622	111,502	492,744
2015.4	**6/3**	400,306	472,108	319,016	196,468	144,892	118,198	486,026
2016.1	**7/4**	392,573	470,059	314,359	193,384	142,870	116,854	464,950
2016.2	**8/5**	370,939	478,860	338,816	192,767	144,546	118,098	480,613
2016.3	**9/6**	370,333	466,528	327,986	191,790	141,932	114,350	516,659

Appendix Table 3: Elasticity Estimates from Selected Literature

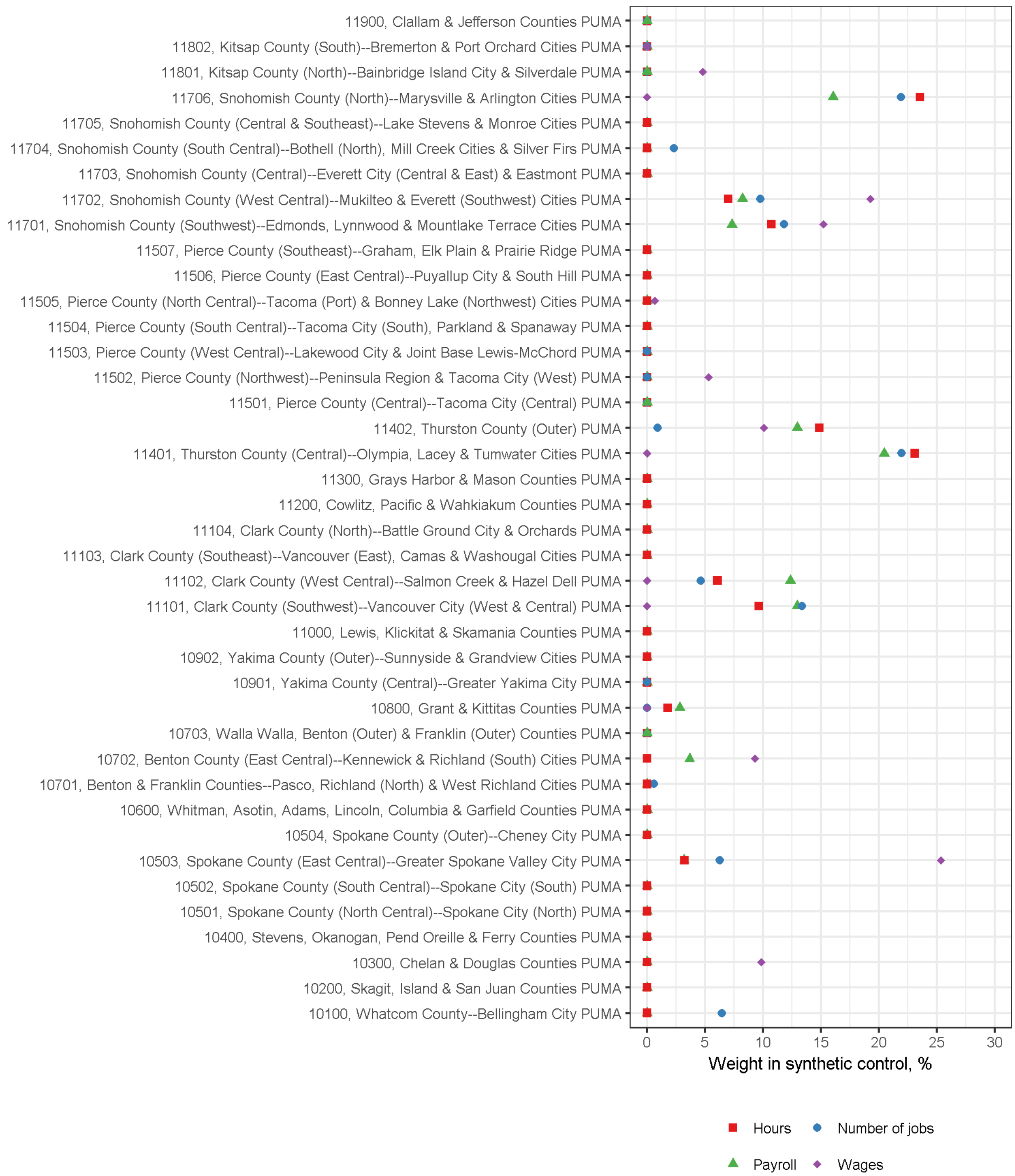
Level of Government	Industry and Outcome	Years	Method	Elasticity
State	Restaurant Employment All Jobs	1990-2010	Interactive FE	-0.04
			Common Correlated Effects-Pooled Estimator	-0.01
			Common Correlated Effects-Mean Group Estimator	-0.01
State	Restaurant Employment All Jobs	2000-2011	DnD (State and Time FE)	-0.12
			Synthetic Matching Estimator	-0.06
State	Restaurant Employment All Jobs	1990-2006	DnD (Census division-by-period fixed effects and County FE)	-0.02
			+ State linear trend	-0.04
			Contiguous Border County Pair Sample (County and Quarter FE)	-0.11
			Contiguous Border County Pair Sample (County-pair × period FE)	0.02
State	Restaurant Employment All Jobs	2000-2011	DnD (County and Quarter FE)	-0.07
			DnD (Contiguous County-Pair Quarter FE + County FE)	-0.02
State	Restaurant Employment All Jobs	1990-2005	DnD (County and Quarter Fixed Effects)	-0.10
			+ Linear County Trends	-0.01
			+ Quadratic County Trends	-0.05
			+ Cubic County Trends	-0.04
			+ Quartic County Trends	-0.06
			+ Fifth-order County Trends	-0.05
		1990-2012	DnD (County and Quarter FE)	0.00
			+ Linear County Trends	-0.04
			+ Quadratic County Trends	-0.02
			+ Cubic County Trends	-0.04
			+ Quartic County Trends	-0.02
			+ Fifth-order County Trends	-0.01
State	Restaurant Employment All Jobs	1990-2014	DnD relative to All Counties (County and Quarter FE)	-0.24
			DnD Contiguous Border County Pair with (County and Quarter FE)	-0.18
			DnD Contiguous Border County Pair with (County-pair × Quarter FE)	0.02
			Unweighted Average	-0.05
			Unweighted Standard Deviation	0.06

Appendix Figure 1: Changes in the Wage Distribution in Outlying King County and Snohomish, Pierce, and Kitsap Counties.



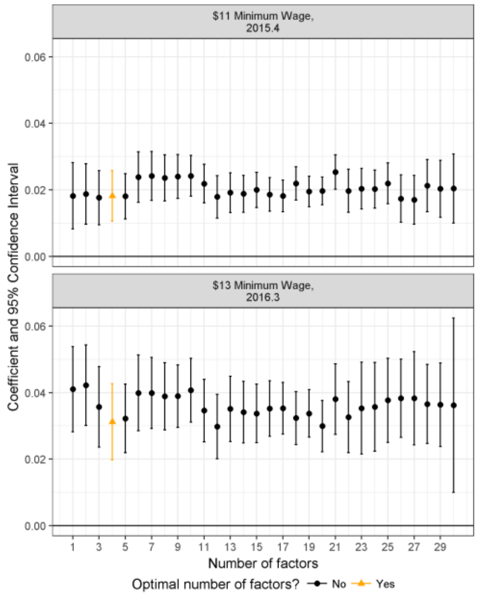
Notes: Authors calculations based on UI records from State of WA using the sample of jobs in locatable employers. Wage rates and earnings are expressed in constant prices of 2015 Q2.

Appendix Figure 2: Weights Chosen by Synthetic Control Estimator, by Outcome.

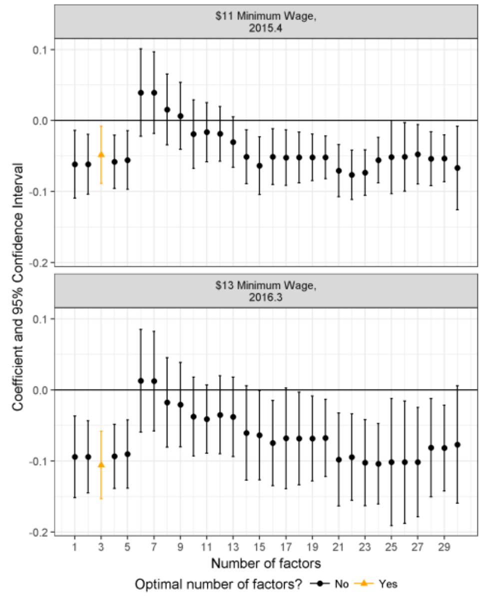


Appendix Figure 3: Sensitivity of the Interactive Fixed Effects Estimates to the Number of Factors Used

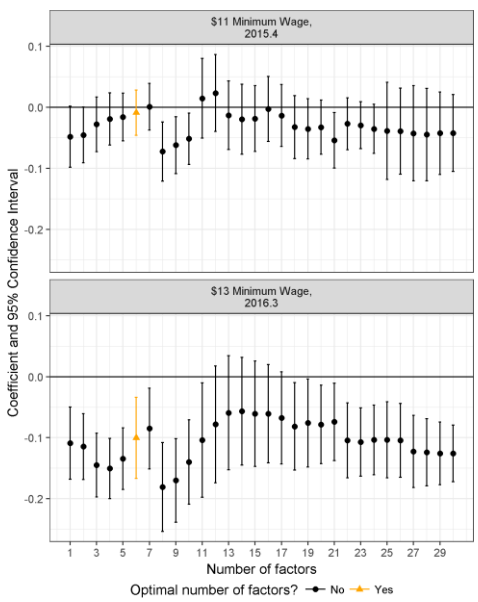
Panel A: Average Wage, Jobs paying <$19 per hour



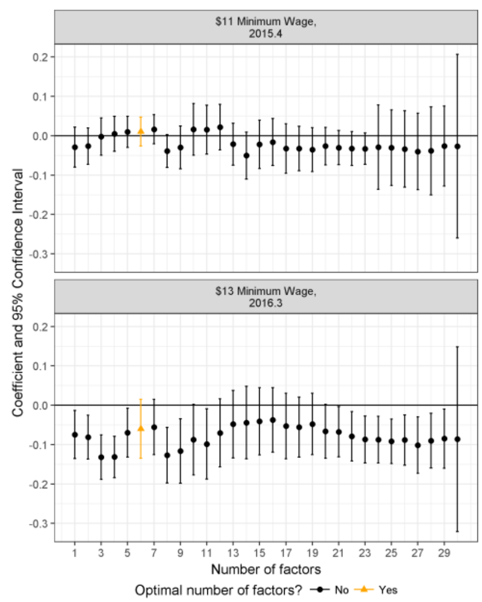
Panel C: Number of Jobs, Jobs paying <$19 per hour



Panel B: Hours Worked, Jobs paying <$19 per hour

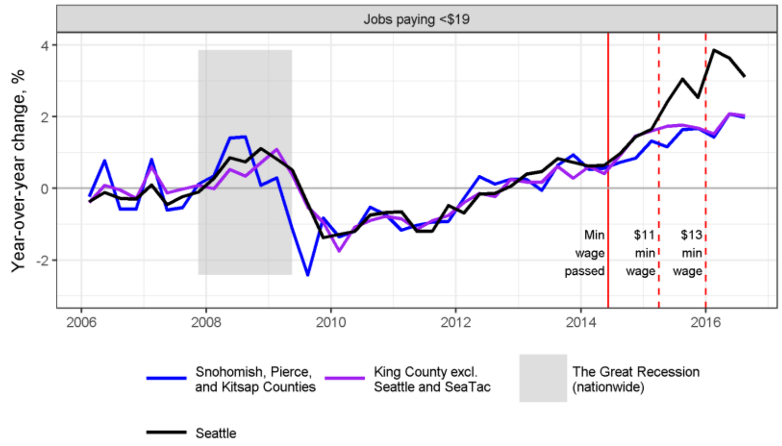


Panel D: Payroll, Jobs paying <$19 per hour

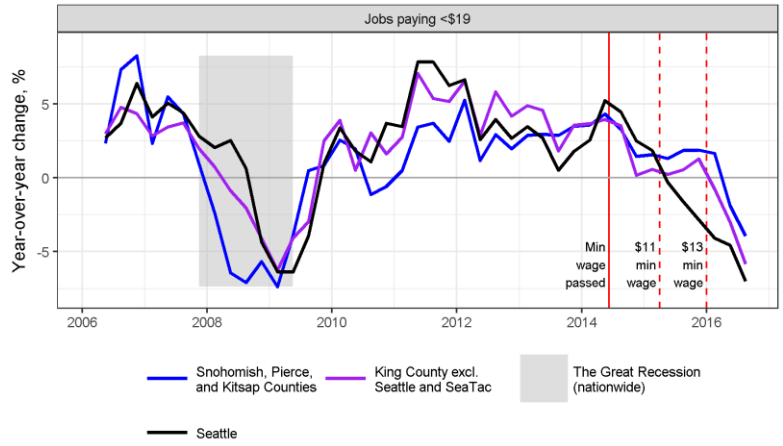


Appendix Figure 4: Employment, Wages, and Payroll in Seattle Compared to Outlying King County and Snohomish, Kitsap, and Pierce Counties

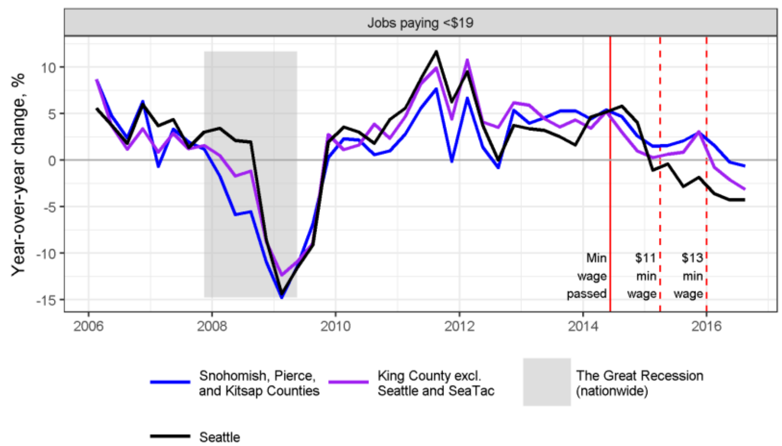
Panel A: Average Wage



Panel C: Number of Jobs



Panel B: Hours Worked



Panel D: Payroll