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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 in 2015 and to as much as $13 in 2016. Using a variety of methods to analyze employment in all sectors paying below a specified real hourly wage rate, we conclude that the second wage increase to $13 reduced hours worked in low-wage jobs by 6-7 percent, while hourly wages in such jobs increased by 3 percent. Consequently, total payroll for such jobs decreased, implying that the Ordinance lowered the amount paid to workers in low-wage jobs by an average of $74 per month per job 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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1. Introduction

Neoclassical economic theory suggests that binding price floor policies, including minimum wages, should lead to a non-market equilibrium marked by excess supply and diminished demand. Some previous empirical studies have questioned the extent to which this prediction holds in the low wage labor market, with many estimates suggesting a negligible impact of higher minimum wages on employment. This paper uses rich administrative data on employment, earnings, and hours in Washington State to re-examine this prediction in the context of Seattle's minimum wage increases from $9.47 to as much as $11 in April 2015 and as much as $13 in January 2016. Seattle is among a set of localities that have instituted large local minimum wage increases in recent years as part of the "Fight for $15" movement (Greenhouse 2012; Rolf 2016). Our data allow us to examine the impacts of this large local increase on both the extensive and intensive margins. Employment losses associated with Seattle's mandated wage increases are large enough to have resulted in net reductions in payroll expenses – and total employee earnings – in the city's low-wage job market. Moreover, we find evidence of non-linear effects, as the rise to $11 per hour had an insignificant effect on employment, whereas the rise to $13 per hour resulted in a large drop in employment.

Basic models drastically oversimplify 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 often defined the relevant market by focusing on lower-wage industries, such as the restaurant sector, or on lower-productivity employees such as teenagers. Results of such studies cannot be generalized to the entire low-wage labor market and may yield attenuated estimates of the effect as they blend workers for whom the minimum wage is binding with workers for whom it is not. Moreover, prior studies commonly analyze only measures of "headcount" employment, ignoring the reality that most low-wage jobs are part-time in nature and the intensive margin may be a significant dimension of adjustment.

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, examining *both* headcount and hours-based measures of the quantity of labor. 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 estimates. Our analysis of restaurant employment at all wage levels, analogous to many prior studies, yields minimum wage employment impact estimates near zero. Point estimates are higher, though imprecisely estimated, when examining only low-wage jobs in the restaurant industry, and when examining total hours worked rather than employee headcount.

We further examine the impact of other methodological choices on our estimates. Some prior studies have drawn "control" cases from geographic regions immediately adjoining the "treatment" region (e.g., Dube, Lester, and Reich 2010). This could yield biased effect estimates to the extent that wages in adjacent regions adjust to the policy change in the treatment region. Indeed, cross border 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 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.

Many prior studies estimate employment elasticities by comparing the magnitude of estimated employment losses with the statutory increase in the minimum wage. Applying this method to our results yields elasticity estimates in line with earlier studies, if somewhat on the high side. We show, however, that the impact of Seattle's minimum wage increase on wage levels is *much smaller* than the statutory increase because 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 -0.9. The subsequent increase to as much as $13 yielded more substantial disemployment effects, with net elasticity point estimates closer to -2.6.[1]

While these findings imply that Seattle's minimum wage policy decreased 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 than the rise from $9.47 to $11 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 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 the subset of Washington State employers 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 90% 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 exhibit significantly different responses to the minimum wage, survey evidence indicates no differential response. Moreover, when we track workers longitudinally we find no evidence of an exodus from the employers included in our analysis to the excluded employers.

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] Our results are similar to those in Mastracci and Persky (2008) who evaluate an increase in Illinois' minimum wage. They find that while the state's minimum wage rose $1.35, "hourly pay for low-wage workers rose by only 15 cents on average" and "hours worked by low-wage workers fell by about two hours per week, resulting in lower weekly earnings," with the implied demand elasticity being in the range of "two to three" (p. 268).

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 living expenses. 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; Manning 2003), the possibility that higher wages intensify job search and thus improve employer-employee 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, along with a reliance of headcount-based measures of employment rather than hours-based measures, has figured less prominently.[2]

[2] One notable exception is the work of Belman, Wolfson, and Nawakitphaitoon (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).

2.1 What is the relevant labor market?

Previous literature has generally 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.[3] 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 argued 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 (Dube, Lester and Reich 2010, 2016; Addison, Blackburn and Cotti 2012, 2014; Neumark, Salas, and Wascher 2014; Allegretto, Dube, Reich, and Zipperer 2016; Totty 2017). 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 focused both on the federal and state level minimum wage hikes (Card 1992; Neumark and Wascher 1994, 1995, 2004, 2008, 2011; Allegretto, Dube, and Reich 2011; Neumark et al. 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 larger 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

[3] Exceptions include Neumark, Schweitzer, and Wascher 2004; Meer and West 2016; and Gopalan, Hamilton, Kalda, and Sovich 2017.

other hand, since some teens earn wages well above the minimum, including them in the sample would bias estimates of the impacts for that demographic group toward zero.

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.[4] 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, or if simultaneous economic shocks shift wages in a similar manner. 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, so long as minimum wage increases are not coincident with economic shocks that apply only to the implementing region, 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 on wages and

[4] This approach bears a strong resemblance to Cengiz, Dube, Lindner, and Zipperer (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. Their analysis focuses only on self-reported employment, not hours, and thus focuses only on the extensive and not the intensive margin.

employment of multiple minimum wages over time. The two-way fixed effect approach has come under criticism in recent years because of the geographic distribution of minimum wage adoption (Allegretto et al. 2016). States with higher minimum wages are concentrated in the Northeast and West coast, regions that have different 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 that 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, (Dube et al. 2010, 2016; Allegretto et al. 2011), the use and order of region-specific time trends (Addison et al. 2012, 2014), the use of a synthetic control to identify control regions with pre-trend employment levels similar to the treatment region (Neumark et al. 2014), and linear factor estimation (Totty 2017).[5]

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 et al. 2010, 2016; Allegretto et al. 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 minimum 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 et al. (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. Dube et al. (2016) rebut this claim, noting statistically significant larger mean absolute differences in covariates not related to the minimum

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

wage for noncontiguous counties compared to contiguous counties.

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 (2017) 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, similar 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 increased the minimum wage within Seattle's city boundaries to $15.[6] The phase-in rate differed by employer size, and offered 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.[7] 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.[8]

Most prior research has, by necessity, focused on increases at the federal (Card 1992; Katz and Krueger 1992; Belman and Wolfson 2010) or state (Card and Krueger 1994; Neumark and Wascher 1995; Dube et al. 2010, 2016; Meer and West 2016) level. Seattle's Ordinance

[6] $15 is high in the distribution of hourly wages in the U.S.; during 2012-14, 42.4% of U.S. workers earned less than this amount (Tung, Lathrop, and Sonn 2015).

[7] 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.

[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 the federal minimum wage over this time period were binding in Washington.

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 policies found small or no employment effects, results consistent with the bulk of the minimum wage literature (Potter 2006; Dube, Naidu, Reich 2007; Schmitt and Rosnick 2011).

For most of the phase-in period, the 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 – is considered a large business so long as the sum of employment at all franchises worldwide exceeds 500.

Seattle implemented its groundbreaking minimum wage 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 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 of 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 paid by the

[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.

hour, and either actual hours worked or 40 times the number of weeks worked for salaried employees.[10, 11]

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 employment counts, hours worked, 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.[16] As a result, we can estimate effects solely for low-wage jobs within all industries.[17]

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[10] ESD 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 – a pattern consistent with respondent rounding and consequently measurement error in CPS data. 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.

[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 are expressed in second 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 in 2015 dollars and observations with calculated wages above $500 if reported hours were below 10 in a calendar quarter. We also exclude observations reporting 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.

[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. Nonetheless, Cengiz et al. (2017) find that "wage distributions in the CPS and in the administrative data"…"on hourly wages from three U.S. states that collect this information (Minnesota, Washington, Oregon)"…"are quite similar both in the cross section as well over time" (p. 3).

[16] While the CPS merged outgoing rotation group data include self-reported hourly wage rates, as noted above respondent measurement error in hours would make analysis of the intensive margin problematic. Cengiz et al. (2017) use CPS data to study employment only, not hours.

[17] We exclude from the analysis services provided to private households, such as cooks, maids, nannies, gardeners etc. (NAICS code 814000) and services for the elderly and persons with disabilities (NAICS code 624120), because in both of these industries private households rather than businesses serve as employers. As a result, the data for these industries are often inconsistently reported, particularly for home caregivers reimbursed by Medicaid who are technically employed by the individual they care for but report their hours to a state agency.

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 (Dube et al. 2010, 2016; Addison et al. 2012, 2014; Neumark et al. 2014; Allegretto et al. 2016; Totty 2017).[18]

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.[19] 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 this outcome in our preferred specifications.

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. 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.

Firms with multiple locations have the option of establishing a separate UI account for each location, or having a common account for several locations. 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. [20] We

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

[19] 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.

[20] 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.

therefore exclude multi-site single-account businesses, which employed 29% of employees state-wide, from the analysis. Additionally, we are unable to geocode businesses with invalid addresses or those whose address is listed only as "statewide" or "unknown"; 9% of employees were employed by these businesses. The remaining firms included in the analysis are henceforth referred to as "locatable" businesses. As shown in Table 2, in Washington State as a whole, locatable businesses comprise 90% 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.[21]

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 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 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.[22]

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 altered 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.

[21] 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.

[22] In addition, larger firms are more likely to provide health benefits to their workers, and the Ordinance establishes a lower minimum wage for employers who contribute towards health benefits.

Survey evidence collected by our research team 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.[23] 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 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, 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 surrounding counties (described below) as the city's minimum wage increased. This result suggests that the Ordinance had no impact on gross flows into the non-locatable sector.[24] Our best inference, in summary, is that our data restriction to employment in locatable establishments is not likely to cause upward bias and, if anything, likely biases our employment results towards zero.

4.3 Preliminary visual analysis to identify a wage threshold

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 jobs (Neumark et al. 2004; Autor, Manning, and Smith 2016; Brochu, Green, Lemieux, and Townsend 2018). These cascading impacts may be caused by employers seeking to maintain differentiation between the wages paid to their least-skilled workers and those paid to workers

[23] 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.

[24] The basic impression conveyed by this figure is confirmed by synthetic control regression analysis, which finds no significant impact of the 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.

with higher skill or experience.[25] Our proxy for low-skilled employment 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.

Figure 2 presents plots of the wage distribution intended to identify potential fixed points. Panel A of Figure 2 shows the histogram of quarterly hours worked across ten-cent-wide wage bins, up to the $24.90-25.00 per hour level. Panel B shows the corresponding cumulative hours histogram. We begin with something of a falsification test, showing comparisons of these histograms for: the second quarter of 2012 versus the second quarter of 2013 (left column), which was a full year before the passage of the Ordinance. We then introduce identical plots comparing 2014.2 versus 2015.2 (center column), showing the changes concurrent with the $11 minimum wage; and 2015.2 versus 2016.2 (right column), showing the changes concurrent with the $13 minimum wage.

The left side of Panel A shows that the histogram of hours by wage for low-wage workers was roughly steady during the year prior to the passage of the Ordinance. Were this panel to reveal significant increases in real wages over time, when there was no increase in the real minimum wage, we might doubt the ability of this exercise to identify minimum wage impacts. In fact, these histograms are remarkably similar, with spikes generally corresponding to whole numbers (e.g. at $10, $11, and $12 per hour) and with the slight leftward shift of the spikes indicating nominal wage rigidity in the face of slight inflation. As shown in the left side of Panel B, the number of hours worked for wages under $15 was roughly the same in 2012.2 and 2013.2. By contrast, there is some evidence of growth in work paying moderately higher wage rates between $15 and $25. This pattern is consistent either with selective growth in employment opportunities for workers commanding moderately higher wages, or an upgrading of existing positions to higher wage levels.

The plots in the middle and right side of Panel A show clear direct impacts of Seattle's two minimum wage increases, with large spikes in the histogram of hours worked exactly at the levels specified by the minimum wage schedule (presented in Table 1 and shown in Figure 2 by the dotted vertical lines). In the right hand panel, the largest spike is observed at a wage of $12,

[25] For a detailed analysis of the effect of the Ordinance on firm behavior, see Jardim and van Inwegen (2018).

indicating that in our sample a larger number of hours were worked at the small-business minimum than the $13 minimum for larger businesses. Additionally, we see strong declines in the number of hours worked in Seattle for wages below these minimum wage thresholds. These results suggest that the Ordinance affected the distribution and that our data are of high quality.

These figures provide little evidence of cascading wage impacts beyond the range of $16-$19. Moreover, we do not see strong increases in hours worked for wages just above the statutory minimum wage levels. There are possible exceptions in the right hand panels, particularly a spike in hours worked at $15 per hour. Although the $15 minimum wage was not introduced for any business until 2017, both business owners and workers commonly misperceived that Seattle's law mandated a $15 minimum upon adoption (Romich et al. 2017). Nonetheless, the figures suggest no abnormal increases in the number of hours worked in the high teens or low 20s. If anything, growth in the number of positions paying between any wage rate under $25 looks anemic compared to the 2012-13 time period.

In our subsequent analysis, we select a preliminary, conservative threshold of $19 per hour (almost exactly twice the baseline minimum) as a starting point for our analysis, and $6 above the top statutory minimum wage rate in the period under study. Beyond this $19 per hour threshold, cascading effects are less likely to occur (Neumark et al. 2004).[26] We test sensitivity to this choice by evaluating impacts up to a $25 per hour threshold. As shown below, we do not find evidence of gains in hours between $19 and $25 per hour caused by the Ordinance. Thus, the evidence suggests that a low-wage threshold $6 above the top statutory minimum poses little risk of miscoding jobs as lost when they have really been promoted to higher wage levels.

The use of any fixed threshold to define the low-wage labor market is a problematic strategy to the extent that unrelated labor market trends are shifting equilibrium wages relative to the threshold, or causing overall growth or decline. The left-hand panels of Figure 2 suggest that such a pattern may have been underway before the minimum wage increased. Our analysis below rests on two strategies for addressing this threat. First, the City of Seattle will be compared to other geographic regions exhibiting similar labor market trends in the period leading up to the minimum wage increase. Appendix Figure 1 previews this strategy by plotting variants of Figure 2 for outlying areas of King County and the three urbanized counties

[26] Brochu et al. (2018) find a smaller range of cascading wages, with "spillover effects that go to about $2 above the minimum wage" (p. 27).

surrounding King County.[27] The second strategy emphasizes timing: minimum wage increases occur as discrete events rather than long-term trends.

5. Methodology: Causal identification strategy

We estimate the effect of the Ordinance on changes in employment and wages in Seattle relative to the second 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 the Ordinance was passed but before the first phase-in; this period is considered "post-treatment" in our analysis to assess the possibility of anticipatory effects.[28] The minimum wage reached as high as \$11 in the fourth through sixth quarters after baseline and as high as \$13 in the remaining quarters.

We analyze variation in year-over-year changes in each outcome, and then combine these estimates to derive the cumulative effect of the minimum wage. 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:

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

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).[29,30,31]

Y_{rt} is alternatively defined as low-wage workers' average wage (computed as the average hourly wages paid to low-wage workers weighted by their hours worked in a quarter), the sum of hours worked by low-wage workers, the total number of beginning-of-quarter jobs held by low-

[27] 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. We therefore exclude it from our analysis.
[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] Below we demonstrate that similar results are found using specifications that evaluate impacts on levels (Y_{rt}) and standardized levels.
[30] $t = -33$ corresponds to 2006.1, which is the earliest quarter for which our data permits computation of ΔY_{rt}, and thus the "pre-treatment" period that is evaluated includes quarterly observations beginning in 2006.1.
[31] In this paper, we use a repeated cross-sectional design. However, our data allow other methods to evaluate the impact of the minimum wage in Seattle. For example Jardim et al. (2018a) exploits the longitudinal links to evaluate the impact of the Ordinance on low-wage Seattle workers' earnings and job spell durations.

wage workers, or the total earnings paid to low-wage workers during the quarter, in region *r* and quarter t.[32]

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 et al. (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 3). 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.[33]

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

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

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

[32] Beginning-of-quarter jobs are defined as employer-employee spells which had non-zero earnings in two consecutive spells, and correspond to jobs spells which started before the current quarter and ended either in the current quarter or later. Beginning-of-quarter jobs is the best measure of the point-in-time employment which can be derived based on payroll data, and the resulting job counts are very close to those reported in Quarterly Census of Employment and Wage (QCEW).

[33] Our companion paper (Jardim et al., 2018b) examines this possibility of spillover and mechanisms for estimating spillovers in greater detail. In that paper, we empirically estimate the extent of labor market integration by evaluating the prevalence of pre-policy movements of low-wage workers between regions.

second phase-in occurred; and $q = 9$ corresponds to the third quarter of 2016, the last period included in our analysis. 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 four $\beta_q^{cum} = \beta_q$; for quarters five to eight, $\beta_q^{cum} = (1 + \beta_q)(1 + \beta_{q-4}) - 1$; and for quarter nine $\beta_9^{cum} = (1 + \beta_9)(1 + \beta_5)(1 + \beta_1) - 1$.[34] 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 method (Abadie and Gardeazabal 2003) and the interactive fixed effects method (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 (2017) 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:

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

[34] Note that since our estimate of β_9^{cum} is composed of a product containing three estimated coefficients (i.e., β_1, β_5, and β_9), it is likely to have a larger standard error than other cumulative change estimates (i.e., β_1^{cum},…, β_8^{cum}), and this contention is confirmed in the results shown below.

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:

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

subject to the constraints $\sum_r w_r = 1$ and $\forall r \; w_r \geq 0$.[35] Given a set of weights $\widehat{w_r}$, the impact of the Ordinance in quarter q is estimated as follows:

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

We allow weights across regions to be different for each outcome to improve the quality of the match in 2006-2014. Appendix Figure 2 shows that the set of regions in Washington, which receive a positive weight in synthetic control estimator is very similar for employment outcomes and payroll, but somewhat different for wage rates.[36]

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

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

[36] Pairwise correlations between synthetic control weights chosen for hours worked, number of jobs, and payroll are each larger than 0.75, while the correlations of the synthetic control weights chosen for wages with weights chosen for the other three outcomes is positive, but smaller (0.22, 0.24, and 0.12). 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.

Bai and Ng (2002).[37] We implement the interactive fixed effects estimator following Gobillon and Magnac (2016) who 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.[38] In principle, we could use different geographic units, such as counties, which are larger than PUMAs, or census tracts, which are much smaller than PUMAs. We have chosen PUMAs because they provide a good compromise in terms of geographic aggregation. On the one hand, PUMAs are generally smaller than counties and allow donors to come from areas of the state affected by similar economic trends in Seattle. On the other hand, PUMAs are quite large and less likely to be affected by idiosyncratic shocks.

We exclude King County PUMAs from the analysis because of potential spillover effects. The remainder of Washington includes 40 PUMAs (see Figure 4), while Seattle is composed of five PUMAs.[39] In the interactive fixed effects estimation we allow each Seattle PUMA to be a separate unit of observation, and estimate a common coefficient for the Seattle PUMAs in each treated period (i.e. nine coefficients in total). In the synthetic control estimation, we first calculate a weighted average of the growth rates of the five Seattle PUMAs, weighted by hours worked in each PUMA four quarters ago, and then estimate the effect of the minimum wage on this weighted average growth rate, treating it as one unit. Though the coefficients which

[37] 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 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. In our application, the optimal number of factors is always smaller than the maximum number of factors allowed by the model.

[38] 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.

[39] 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 high quality of fit between treatment and control region below. Intuitively, we seek regions that match Seattle's dynamics in the low-wage labor market. 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.

we estimate by both methods correspond to the effect on the year-over-year growth rate in each outcome, we report the estimates of the cumulative effect of the minimum wage since the passage of the Ordinance, which we calculate in the same way as in the case of the difference-in-differences method.

We calculate the standard errors for the interactive fixed effects coefficients based on the assumption of the independent and identically distributed idiosyncratic shocks ε_{rt}, i.e. we assume that any correlation in shocks across regions has been captured by the common factors. We report *p*-values for the null hypothesis that each cumulative effect equals zero based on standard errors calculated using the delta method.

Inference for the synthetic control method is based on placebo-in-space permutations, as is customary in the literature (Abadie, Diamond, and Hainmueller 2014; Firpo and Possebom 2017).[40] We estimate the effect of placebo treatments introduced in the second quarter of 2014, i.e. in the quarter when Ordinance was passed, in all possible combinations of five contiguous PUMAs in Washington State excluding King County.[41] This allows us to locate the coefficients estimated in our main analysis within a distribution of coefficients obtained in settings where we expect no actual treatment effect. For the cumulative effect in each period, we report the *p*-value for the null hypothesis of no effect, calculated as the share of placebo estimates which were larger in absolute value than the estimated effect in Seattle, i.e. $p\left(\hat{\beta}_q^{cum}\right) = \frac{1}{J}\sum_j 1\{\left|\hat{\beta}_{j,q}^{cum}\right| > \left|\hat{\beta}_q^{cum}\right|\}$, where j indexes the possible combinations of five contiguous PUMAs, $\hat{\beta}_q^{cum}$ is the estimated treatment effect in Seattle, and $\hat{\beta}_{j,q}^{cum}$ is the estimated placebo effect in region j. There are 2,994 possible combinations of five contiguous PUMAs in WA outside of King County, so the smallest possible *p*-value for each coefficient is 1/2,994 = 0.0003.[42]

Finally, we also calculate confidence intervals for the estimates of the effect of the minimum wage on employment, which we obtain by inverting the test statistic (Imbens and

[40] The synthetic control method does not yield conventional standard error estimates.

[41] Note that Seattle spans 5 PUMAs, thus our placebo treatment region replicates Seattle's size. Further, we require that the 5 PUMAs randomly selected as placebo "treated" be contiguous. By making the placebo-treated region contiguous, we replicate the contiguous nature of Seattle, and thus account for the possibility of common regional shocks.

[42] Abadie, Diamond, and Hainmueller (2010) report the *p*-value based on the same procedure which we use, while Abadie et al. (2014) and Firpo and Possebom (2017) recommend dividing the estimate by the pre-policy MSPE for each region, and calculating the *p*-value based on the rank of this statistic. We have calculated the *p*-values using their method as well. Conclusions about the statistical significance based on these two procedures are very similar.

Rubin 2015). For each estimated coefficient we calculate the range of estimated effects which cannot be rejected at the 5-percent significance level, i.e. we find β_q^* such that $\left|\hat{\beta}_q^{cum} - \beta_q^*\right| < |\beta|_{0.95,q}$, where $|\beta|_{0.95,q}$ is the 95th percentile of the absolute values of the placebo estimates.[43] We compute 90-percent and 50-percent confidence intervals analogously. In our presentation of the results, we present $\hat{\beta}_q^{cum}$ and $p(\hat{\beta}_q^{cum})$ in tables and show confidence intervals in figures.

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 locatable 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 38,013 to 25,053 (a decline of 12,960 or 34%).[44] The decline is consistent with legislative intent, while the persistence of employment at wages below \$13 reflects the lower minima applied to small businesses and those offering health benefits.[45]

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 90,757 to

[43] Because we have 2,994 possible combinations of the contiguous PUMAs, we are able to use 95.02338% confidence level for our estimates (149/2,944*100% = 95.02338%).
[44] 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.
[45] 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, a subminimum wage set equal to 85 percent of the minimum for workers under 16 years old, situations where Seattle-based employers hire employees to work outside the city limits, or noncompliance with the ordinance.

89,188 (a decline of 1,569 or 1.7%).[46] Measuring hours worked at low wages rather than employee headcount shows a 5.1 million hour reduction at wages under $13, and a 1.0 million hour (2.7%) reduction at wages under $19.

Over this same period, overall employment in Seattle expanded dramatically, by over 14.6% in headcount and 15.8% in hours. Table 3 makes clear that the entirety of this employment growth occurred in jobs paying over $19 per hour.[47] The impression of skewed growth – driven in part by rapid growth in the technology sector – extends to wage data.[48] Average hourly wages at jobs paying less than $19 per hour rose from $14.19 to $15.00 (a 6.4% increase), while average hourly wages at all jobs surged from $38.48 to $47.09 (a 22.4% increase).[49, 50]

Table 3 documents that payroll reductions attributable to declines in hours worked substantially cut into the observed wage increases for jobs paying under $19 per hour; the sum total of earnings paid at wages under $19 increased only slightly (2.9%) from 517 to 532 million dollars between second quarter of 2014 and the second quarter of 2016, and the gain is even smaller (0.4%) when comparing "peak" third quarter statistics in 2014 and 2016.[51]

Panel B of Table 3 restricts attention to Food and Drinking Places (NAICS industry 722), which, respectively, comprised 32%, 24%, and 11% of jobs in Seattle's locatable establishments paying less than $13 per hour, less than $19 per hour, and overall during the quarter when the

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[46] 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 second quarter of 2014 and the second quarter of 2016.

[47] The more detailed statistics in Appendix Table 2 show that net job growth in Seattle was 25% for jobs paying over $25 per hour but only 3% for jobs paying under $25. About 66% of net job growth can be attributed to jobs paying over $40 per hour, and 81% to jobs paying over $30 per hour.

[48] 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.

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

[50] The median hourly wage, weighted by hours, which is not shown in Table 3, was $25.81 in the second quarter of 2014. Note that the ratio of the $13 minimum wage and this median wage (what is known as the "Kaitz Index," Kaitz (1970)) is 0.504. Given the high wages in Seattle, this level of the Kaitz Index is not particularly high. For comparison, the Kaitz Index for the US federal minimum wage was 0.371 in 2014 (Cooper, Mishel, and Schmitt 2015).

[51] In contrast, between the second quarter of 2014 and the second quarter of 2016, total quarterly earnings paid at wage rates above $19 increased by $2.1 billion (46.2%) – implying a dramatic increase in inequality of earnings between low- and high-wage workers in Seattle.

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 12.6% while hours worked by low-wage employees in the restaurant industry was nearly unchanged, up 0.8% 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: 11.6% growth in wages in jobs paying less than \$19 per hour, and 12.5% 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 (e.g., Allegretto et al. 2016). 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 4.4% or 5.6%, 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 and interactive fixed effects results shown in columns 5-8 behave well, with only 2 of the 36 estimated coefficients being significant at the two-tailed 90-percent confidence level, and none being significant at the 95-percent level. Thus, these methods "pass" the falsification test.[52] Given the general lack of significance of these estimated pseudo

[52] Across both methods, all but one of the estimated pseudo-effects on hours are negative and average -1.7% and -1.6%, respectively. If these same negative pseudo-effects on hours persist into the period that we study, we would moderately overstate the negative effect of Seattle's minimum wage on hours. As will be seen below, these negative coefficients are not consistently observed in the first three quarters of "post" data, between adoption of the

effects, we consider the synthetic control and interactive fixed effects to be reliable means of estimating causal impacts. In the tables below, we show estimates from both methods.

6.3 Examining the synthetic control match

Before turning to the estimates of the effect of the Ordinance, we examine the quality of the match for our preferred method, synthetic control, between Seattle and synthetic Seattle in 2006.1 – 2014.2, i.e. during the periods used to select the synthetic weights.

Figure 5 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.[53] In each panel, trends in Seattle and the control region track closely through 2014. As shown in Panel A, wage growth patterns match 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 region appears to enter the slump slightly before Seattle, and during the recovery in 2011, during which Seattle's growth in low-wage jobs briefly exceeded the control region. Total payroll growth also matches closely throughout the pre-policy period. Previewing our main results, each of these time series shows a stark divergence between Seattle and the synthetic control region once the minimum wage begins to rise.

Figure 6 repeats this analysis with separate trend lines for each PUMA in Washington outside King County. This figure shows that Seattle's pre-Ordinance year-over-year percentage changes in wages, hours, jobs, and payroll lie within the convex hull of these other PUMAs. Further, this figure shows that while wages rose faster in Seattle than most of the other PUMAs post-Ordinance, Seattle experienced nearly the largest declines in hours, jobs, and payroll.[54]

Ordinance and the first wage phase-in. For wages, there is less cause for concern as in the average quarter following the placebo law, estimated pseudo-effects are much smaller, +0.5% and -0.2%, respectively.

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

[54] Appendix Figures 5 and 6 compares Seattle to these same PUMAs, but shows *levels* of each outcome rather than year-over-year percentage changes. Note that since Seattle contains five PUMAs, we have divided Seattle's jobs, hours, and payroll by five to ensure comparability of magnitudes. Seattle's average wage paid to workers earning less than $19 per hour is generally near or at the top of the distribution of other PUMAs, while its jobs, hours, and payroll are well within the convex hull of the other PUMAs. In Appendix Figures 7 and 8, we repeat the analysis with *standardized levels* as well to make Seattle more comparable to other PUMAs. For this analysis, we

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

Table 5 presents our first estimates of the causal impact of the Ordinance for workers earning less than $19 per hour.[55] Looking at both sets of results, we associate the first minimum wage increase, to $11, with wage effects of 1.1% to 2.2% (averaging 1.7%). The second increase, to $13, associates with a larger 3.0% to 3.4% wage effect (averaging 3.2%). A 3.2% increase in the wage of these workers corresponds to $0.45 per hour relative to the base average wage of $14.19.[56] We do not find evidence that wages rose or fell in anticipation of enforcement during the three quarters following passage of the law using the synthetic control method, with coefficients ranging from 0.2% to 0.3%, while the interactive fixed effects specification shows some evidence of wages rising post-passage, but pre-enforcement, ranging from 0.5% to 0.9%, and significant for the second and third quarters after passage.

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 per hour), about 3.2% during the first phase-in and 5.8% during the second. These estimates suggest that wages increased in the control region as well – a pattern clearly observed in Figure 5 panel A.[57] We further note that Table 3 indicates that the majority of jobs (58%) and hours (63%) paying less than $19/hour

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standardize each time series by first computing the difference between the region's outcome level in year-quarter t and the region's mean level of the outcome and then dividing this difference by the region's standard deviation of the level of the outcome. The patterns we observe in both levels and standardized levels are similar to those we observe in year-over-year percentage changes, with Seattle's wages rising faster than Synthetic Seattle's wages, and with hours, jobs, and payroll in Seattle lagging behind Synthetic Seattle. These conclusions are further demonstrated in Appendix Table 3 which compares the results from our growth rates specification (which will be discussed in Tables 5 and 6) to specifications using levels and standardized levels and shows that the results are generally robust.

[55] We have chosen $19 per hour as a conservative threshold for our estimates. We discuss sensitivity of the estimates to the choice of the wage threshold below in detail.

[56] Estimated wage impacts are larger when the low-wage threshold is lowered from $19 per hour (see Figure 7 for estimated effect on wages using lower thresholds). This result is consistent with the Ordinance having sizable effects on the lowest-paid workers and smaller cascading impacts on workers with initial wages closer to $19. Alternatively, a smaller wage effect for larger wage thresholds is consistent with an attenuation bias when we pool affected and unaffected workers.

[57] 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.

at baseline were not directly impacted by the minimum wage increase to $13. Any impacts on wages for jobs paying between $13 and $19 per hour at baseline would be "cascading" effects expected to be much smaller than the impact on lowest earners. 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 per hour, the impact works out to an 11.0% wage increase, a level in line with existing literature.[58] 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 effects on hours ranging between +0.8% and -2.7% (averaging -0.8%) during the three quarters when the minimum wage was $11. By contrast, the subsequent minimum wage increase to $13 associates with larger, significant hours reductions between 4.6% and 9.2% (averaging 6.9%). Columns 3 and 4 present a parallel analysis for jobs, with similar results: statistically weak evidence of reductions in the first phase-in period (averaging -2.5%) followed by larger generally significant impacts in the second (averaging -5.9%).[59] The adverse effects on hours in the final three quarters are 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 -5.9% average job estimate by the 90,757 jobs paying less than $19 per hour at baseline suggests that the Ordinance caused the elimination of 5,340 low-wage jobs at locatable establishments compared to the scenario in which the minimum wage does not increase. Since Seattle's locatable establishments lost about 3,000 low-wage jobs between the second quarter of 2014 and the third quarter of 2016 (Table 3), our estimates suggest that in the absence of the policy change locatable establishments in Seattle would have added 2,350 low-wage jobs. Scaled

[58] 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 11% on a minimum wage increase of 37% would imply an elasticity 0.29. We note, moreover, that the highest $13 minimum did not apply to small business or businesses providing health benefits. In particular, the histogram of hours worked at different wage levels in Seattle in the second quarter of 2016 demonstrated the largest spike at $12 rather than at $13 per hour (see Figure 2, Panel A).

[59] Note that we measure jobs as person-employer matches which existed both in the previous and the current quarter, which corresponds to the number of jobs at the beginning of each quarter, but we measure hours worked in any jobs during the whole quarter. Due to this discrepancy in definitions, there is likely to be a one-quarter lag in the detection of an effect if the employment effect occurs through reduced hiring rates, which would be reasonable to expect in high-turnover industries. This might explain why we see a jump in the estimated effects on jobs between 2016.1 and 2016.2

up linearly to account for multi-site single-account firms, job losses would amount to roughly 8,400.[60]

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 would be 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. The point estimates exhibit a sudden change in the first quarter of 2016 and then remain at this more negative level. 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, which is less likely.

To probe this issue further, Figures 7-9 illustrate the sensitivity of the estimated effects on wages and hours (based on the synthetic control method) using different thresholds ranging from jobs paying less than \$12 per hour to jobs paying less than \$25 per hour.[61] In Figure 7, we show the effect on wage growth. Not surprisingly, as we raise the threshold towards \$25 per hour, the estimated effect on wage growth diminishes. This pattern is what we would expect since, as we raise the threshold, the jobs that are added into the sample are less affected, or even unaffected, by cascading effects. That is, the estimated effect is attenuated as we add in more unaffected jobs.

Figure 8 shows the estimated effects on hours given different thresholds, and Figure 9 illustrates these same results, but multiplies the estimated coefficients by the baseline number of hours worked in jobs paying below the threshold. For the effects of raising the minimum wage to \$11, shown in the top-3 panels, the estimated impacts become insignificant once the threshold rises to around \$16-17 per hour. 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 at the 5-percent level

[60] We cannot ascertain whether the effect on locatable establishments 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.

[61] More specifically, we evaluate jobs paying the highest minimum wage in Seattle in that quarter plus \$1 (i.e., \$12 for 2015.2-2015.4 and \$14 for 2016.1 to 2016.3) up to \$25 per hour.

as we raise the threshold all of the way to $25 per hour for 2016.1, and close to significant at all thresholds for 2016.2 and 2016.3.[62] These results suggest that estimated employment effects do not completely converge to zero, and rather reflect an actual reduction in low-wage employment that cannot be explained by workers upgrading their wage above $19.[63] As shown in Figure 9, the estimated absolute change in total hours concurrent with the increase in the minimum wage to $13 is 3.0 million hours per quarter when the threshold is set at $19 per hour, and this point estimate varies little as we increase the threshold to $25 per hour. Confidence intervals widen as we increase the threshold – we are, in essence, looking for the same needle (i.e., the same 3.0-million-hour decline) in a larger haystack as we increase the threshold. Nonetheless, note that 3.0 million fewer hours worked is within the 50-percent confidence interval for each quarter 2016.1 to 2016.3 given a threshold of $19 per hour or a threshold of $25 per hour. Thus, we conclude that the estimated employment losses associated with the second phase-in reflect an actual reduction in hours worked by low-wage workers, rather than a jump of wages over the selected wage threshold. We return to this issue in Section 6.6.

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. Table 7 confirms this expectation using regression specifications examining the impact on payroll for jobs paying less than $19 per hour. Although results are not statistically significant, point estimates suggest payroll declines of 1.3% to 3.9% (averaging 3.0%) 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 $62 million on an annual basis.[64]

6.5 Elasticity estimates

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[62] Recall, as previously noted, that confidence intervals for the final quarter, 2016.3, are wider than for 2016.1 and 2016.2 as β_9^{cum} is composed of a product containing three estimated coefficients (i.e., β_1, β_5, and β_9), whereas β_7^{cum} and β_8^{cum} are each only composed of a product containing two estimated coefficients.

[63] If there is an absolute loss in hours worked in low-wage jobs, then the estimated effect would not converge to zero, no matter how high we raised the threshold.

[64] Simple calculations based on preceding results suggest an effect of comparable magnitude. Hours results suggest a 6.9% decline in hours, which on a base of $517 million paid in the baseline quarter amounts to a $142 million less in annual payroll. Wage results suggest a 3.2% boost to earnings, which amounts to a $70 million increase in annual payroll (again assuming 6.9% fewer hours). Combining these results yields a net annual loss of $72 million.

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.[65] We also compute measures of statistical uncertainty for these elasticities since they are the ratio of two estimates.[66, 67] During the first phase-in, when the minimum wage was $11, estimated elasticities range from -0.32 to -1.74 (averaging -0.88). Notably, we cannot reject elasticity = -1 with 95-percent confidence, which is consistent with our finding in Table 7 that we could not reject zero effect on payroll. Additionally, we cannot reject elasticity = 0, which is consistent with our finding in Table 6 that we could not reject zero effect on hours. The relatively modest estimated wage and hours impacts of the first phase-in create considerable statistical uncertainty regarding the associated elasticity estimate.

Estimated elasticities for the period after the minimum wage increased to $13 range from -2.15 to -2.94 (averaging -2.63). Point estimates of elasticities imply that, within Seattle, low-wage workers lost more than $2 in forgone employment opportunities for every $1 gained from higher hourly wages. While the estimates of these elasticities are still noisy, we can reject the hypothesis that the elasticity equals zero (consistent with Table 6) for 2016.1 and 2016.2 and nearly for 2016.3. We cannot reject the hypothesis that the elasticity equals -1 with 95-percent confidence. In Figure 10, we show the sensitivity of these estimated elasticities using different thresholds. These very large elasticities do not appear to be artifacts of setting the threshold at $19 per hour. The upper panels of Figure 10 show the conventional 95-percent confidence

[65] 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 hours fall more than wages rise, the results more likely reflect a decline in labor demand. Worker interview data collected by members of our research team suggest that the proportion of low-wage workers opting to voluntarily reduce hours as a result of wage increases is nonzero, but small.

[66] We compute the 95-percent confidence interval for the estimated elasticities based on the permutation inference, taking into account the correlation between estimated effect of the minimum wage on employment and wages within PUMAs. We include a pair of estimates $(\beta^{Hours}, \beta^{Wages})$ into the 95-percent confidence set if after subtracting these estimates from the observed outcomes in Seattle we cannot reject a zero effect on *both* outcomes in Seattle after the passage of the minimum wage at the 5-percent significance level based on the permutation inference. After that, we estimate the confidence interval for employment elasticity by calculating elasticity as $\beta^{Hours}/\beta^{Wages}$ for all pairs of $(\beta^{Hours}, \beta^{Wages})$ which belong to the confidence set.

[67] Note that our estimates of the "demand elasticity" might not map onto any particular labor demand curve as we are blending workers at the lowest wage levels with workers at more modest wage levels (e.g., those with wages below $15 compared to those between $18 and $19). As such, it is best to think our estimates as weighted average elasticity for workers with wages below $19.

intervals (which get quite wide for higher thresholds due to lower estimated effects on wages at higher wage thresholds), whereas the bottom panels zoom-in on the 50-percent confidence intervals (which, arguably, might be more valuable information for policymakers). As shown more clearly in the lower part of Figure 10, the estimated elasticities are very close to -3 when the threshold is set anywhere between $16 and $25 per hour.[68] At most thresholds, an elasticity of -1 is not within the 50-percent confidence intervals – the preponderance of the evidence suggests that hours fell more than wages rose in Seattle's low-wage jobs.

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 per hour appear to have resulted in net earnings losses in Seattle for these workers.

6.6 A final assessment of "rightward shift"

As noted above, our analytical strategy may be confounded by contemporaneous trends or shocks that shift the wage distribution to the right, reducing the number of hours worked below any fixed threshold even when there is no actual reduction in hours worked overall. Evidence to this point – the cumulative density functions in Figure 2, the coefficient estimates indicating immediate impacts in a time period marked by slack labor demand – does not generally support the "rightward shift" hypothesis. Nonetheless, we can examine the issue more closely, decomposing the year-over-year growth rates of hours worked as follows:

(6)
$$\frac{h_t - h_{t-4}}{h_{t-4}} = \frac{h_t(hires)}{h_{t-4}} - \frac{h_{t-4}(separations)}{h_{t-4}} + \frac{\Delta h_{t,t-4}(job\ stayers)}{h_{t-4}} + \frac{h_t(wage\ fell\ below\ \$19\ threhsold)}{h_{t-4}} - \frac{h_t(wage\ rose\ above\ \$19\ threshold)}{h_{t-4}} + \frac{h_t(missing\ wage\ in\ t-4)}{h_{t-4}} - \frac{h_{t-4}(missing\ wage\ in\ t)}{h_{t-4}}$$

We denote by h_t quarterly hours worked in jobs paying less than $19 per hour in the period t. We define as $hires$ those jobs which started between $t-4$ and t and paid less than $19 in period t. Similarly, we define as $separations$ those jobs which ceased to exist between $t-4$

[68] 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 per hour 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.

and t and paid less than \$19 per hour in period $t-4$. We define $job\ stayers$ as those jobs which existed in both $t-4$ and t, and which paid less than \$19 in both $t-4$ and t. $wage\ fell\ below$ \$19 $threhsold$ is defined as those jobs which existed in both $t-4$ and t, but received a wage cut from \$19 and above in $t-4$ to below \$19 in t. Similarly, $wage\ rose\ above$ \$19 $threshold$ are defined as jobs which existed in both $t-4$ and t, but received a wage raise from below \$19 in $t-4$ to \$19 and above in t. Finally, the last two terms capture hours changes by those with missing wages in one of the two periods.[69] If the Ordinance caused an increase in the fifth term of Equation 6, $\frac{h_t(wage\ rose\ above\ \$19\ threshold)}{h_{t-4}}$, then we would be overestimating the adverse effects on hours by not considering the hours worked by individuals whose wages rose above the threshold.

To conduct a decomposition of the total estimated effect, and to specifically evaluate the fifth term, we compute each term for Seattle, and we apply the same weights used by the synthetic control method estimates to produce the results in the first column of Table 6 to compute a control group estimate. The results for the fifth term are shown in Appendix Figure 9. This figure shows that Seattle had a higher rate of wages rising above the \$19 threshold than Synthetic Seattle in every quarter during and after the end of the Great Recession.[70] In the quarters prior to passage of the Ordinance, this gap between Seattle and Synthetic Seattle was a relatively steady amount of roughly 2.0%. That is to say, the likelihood of a low-wage worker receiving an increase to a rate above \$19 was consistently 2 percentage points higher in Seattle than in Synthetic Seattle. It would be reasonable to expect this persistent gap to have continued in the absence of the Ordinance. Indeed, this 2.0% gap continued for the first six quarters after passage, but appears to have widened to 3.0% once the minimum wage rose to \$13.[71] These

[69] We set to missing wages of any jobs which reported more than 1,000 hours worked in a quarter, reported 0 hours worked, wages higher than \$500 and fewer than 10 hours worked, or wages less than \$9 in 2015.2 dollars.
[70] Seattle also had persistently higher rates of hires of workers earning less than \$19 per hour than Synthetic Seattle (i.e., the first term in Equation 6 was persistently positive) during the entire pre-Ordinance period, averaging about 5%, and had a persistently higher rate of separations (i.e., the second term) of about 2%-3%. The offsetting combination of the first, second, and sixth terms of Equation 6 produced the tight fit of growth in hours worked in Seattle and Synthetic Seattle shown in Panel B of Figure 5.
[71] The vast majority of the overall change in growth of hours worked for wages under \$19 came from a large decline in the first term of Equation 6 (i.e., growth rate of hours worked from newly hired workers earning less than \$19 per hour), which dropped from a pre-Ordinance average of about +5% to about -2% in the quarters following the increase of the minimum wage to \$13. While a more gradual trend in this difference might suggest a phenomenon where jobs in Seattle were gradually transitioning to higher wages at the point of hire, the sudden difference coincident with the minimum wage increase suggests a simple reduction in hiring.

point estimates suggest that we might be overestimating the adverse effects on hours worked by around 1.0 percentage point. Making this adjustment, we would conclude that the second phase-in of the Ordinance to $13 per hour caused an average of a 5.9% decline in hours (rather than 6.9%). Even accepting this adjustment results in an estimate of the decline in hours that is of larger magnitude than the estimated positive effect on wages (i.e., 3.2%), suggesting that the amount paid to low-wage workers fell.

6.7 Reconciling these estimates with prior methods

Most prior studies compute employment elasticities by dividing regression-estimated percentage changes in employment by the percentage change in the statutory minimum wage (e.g., Sabia 2009; Belman and Wolfson 2010; Allegretto et al. 2011). 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 the second-to-last column of Table 8, we use the conventional approach for computing employment elasticities and find estimates in the range of -0.04 to -0.25 (averaging -0.15). This range is high but not outside of the envelope of estimates found in prior literature (Belman and Wolfson 2014).[72] 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 that focused on restaurant industry employment (e.g., Card and Krueger's (1994) seminal examination of fast food employment in New Jersey and Pennsylvania in response to New Jersey's increase in its minimum wage). As previously noted when discussing Table 3, only 32% of all jobs paid less than $13 per hour at baseline in Seattle were in NAICS industry 722 (Food Services and Drinking Places). Moreover, only 37% of jobs within NAICS 722 were paid less than $13 per hour at baseline; most were not directly affected by either wage increase. This

[72] 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.

raises concerns that analyses of restaurant employment at all wage levels suffer from attenuation bias.

The first three columns of Table 9 repeat the main synthetic control results findings from Table 5 and Table 6, and are included as a point of reference. The middle three columns of Table 9 evaluate impacts on all jobs in the restaurant industry, which is a common form of analysis in the prior literature (e.g., Reich, Allegretto, and Godoey 2017). Wages paid to Seattle's restaurant workers increased substantially and significantly relative to Synthetic Seattle after passage of the law. Estimates of employment effects, whether measured in hours or beginning-of-quarter jobs, are statistically insignificant. These findings, which confirm Reich et al.'s (2017) analysis of the Seattle restaurant industry and many prior studies, demonstrate the severity of attenuation bias using this methodology. In this case at least, industry-based proxies for low wage employment yield unreliable estimates of minimum wage impacts.

The last three columns of Table 9 restrict the analysis to restaurant employment in jobs that pay less than $19 per hour, and thus are more directly comparable to the estimates in the first three columns. Here, employment impacts in particular are imprecisely estimated, reflective of a relatively poor pre-policy fit between Seattle and the synthetic control region. Wage effects are fairly precise and substantial, with the $13 wage associated with a 6.6% boost. Point estimates indicate that the same minimum wage increase reduced hours by 10-11%. An analysis of low-wage jobs in the restaurant industry, rather than all jobs in the restaurant industry, yields conclusions comparable to analysis of the entire low-wage job market.

Analysis of the Seattle restaurant industry must be tempered by a caution regarding pre-policy trends. First, note that it is suspicious that estimated effects of the Ordinance on wages for the entire restaurant industry are larger than those for the low-wage restaurant industry. This result suggests that wages were rising faster for jobs paying over $19 per hour than for low-wage restaurant jobs, and suggests that there may be a secular trend underlying these results. Indeed, compared to the low-wage labor market results, which show wage effect increases timed precisely with phase-in points, wages in Seattle's restaurant industry appear to accelerate more smoothly away from the synthetic control region.[73] A falsification test examining the nine-

[73] A Seattle restaurant owner, in private communication, suggested that trends toward increased wages in UI system data may reflect changes in reporting patterns by employers. To comply with the Ordinance, employers may be requiring tipped workers to report – and pay taxes on – a higher proportion of their tip income.

quarter period beginning in 2012 reveals additional acceleration of wages in Seattle relative to the control region.

In summary, utilizing methods more consistent with some prominent prior studies allows us to replicate their findings of no, or minor, employment effects. These methods reflect data limitations, however, that our analysis can circumvent. These methods also appear to be particularly unreliable in Seattle given pre-policy trends specific to the restaurant industry. We conclude that the stark differences between our findings and these studies 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 and the adoption of high minimum wages in several states, cities, and 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" (p. 110). This paper extends the literature in a number of ways, one of which evaluates effects of two consecutive large local minimum wage increases.

Beyond basic causal inference challenges, many 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. Prior work also focuses on binary measures of employment as an outcome, a crude metric given the overwhelmingly part-time nature of low-wage work. 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 6.9% during the three quarters when the minimum wage was $13, resulting in a loss of around 3 million hours worked per calendar quarter and more than 5,000 jobs. These estimates are robust to cutoffs other than $19 per hour.[74] A 3.2% increase in wages in jobs that paid less than $19 per hour coupled with a 6.9% loss in hours yields a labor demand elasticity of roughly -2.6, 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 -2.6 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 or delegated to consumers. 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 3.2%. Using data in Table 3, we compute that the average low-wage employee was paid $1,900 per month. The reduction in hours would cost the average employee $130 per month, while the wage increase would recoup only $56 of this loss, leaving a net loss of $74 per month, which is sizable for a low-wage worker.

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[74] 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 UI 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.

The estimates may be much larger than those reported in prior minimum wages studies for three reasons. First, it is reasonable to expect that labor demand elasticity would generally be larger for a small, open economy such as Seattle than for a state or the nation – although it should be noted that analysis of Seattle's experience using methods conventional in the literature yield elasticity estimates comparable to that literature.

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 establishments 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.[75]

Further, we lack data on contractor jobs with income reported on 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

[75] If we ignore our survey evidence and suppose that non-locatable 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, – 1.67, as locatable businesses employ 63% of the low-wage workforce.

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 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).

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	$15.45	$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 In subsequent years, 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: Multi-site businesses	Excluded from Analysis: Non-locatable single-site businesses	Excluded from Analysis: Total	Share Included
Number of Firms	123,132	1,345	12,277	13,622	90.04%
Number of Establishments (i.e., Sites)	126,248	Unknown	12,501	Unknown	
Total Number of Employees	1,676,653	767,348	240,237	1,007,585	62.46%
Number of Employees paid <$19/hour	715,808	325,320	87,395	412,715	63.43%
Employees / Firm	13	279	19	58	
St. Dev. of Employees / Firm	160	1610	328	706	
Employees / Establishment	13	Unknown	19	Unknown	
St. Dev. of Employees / Establishment	153	Unknown	282	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 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.

Table 3: Employment Statistics for Seattle's Locatable Establishments

Quarter	Quarters After Passage/ Enforcement	Number of Jobs			Total Hours (thousands)			Average Wage			Total Payroll ($mlns.)		
		Hourly wage rates:			Hourly wage rates:			Hourly wage rates:			Hourly wage rates:		
		Under $13	Under $19	All	Under $13	Under $19	All	Under $13	Under $19	All	Under $13	Under $19	All
Panel A: All Industries													
2014.2	0	38,013	90,757	293,257	13,468	36,451	129,237	11.15	14.19	38.48	150	517	4,973
2014.3	1	38,906	92,845	301,480	13,868	37,570	131,767	11.15	14.19	39.38	155	533	5,189
2014.4	2	33,949	87,779	304,121	11,352	34,563	135,127	11.25	14.41	42.80	128	498	5,783
2015.1	3	33,438	88,758	305,704	10,704	33,244	131,372	11.27	14.46	42.89	121	481	5,634
2015.2	4/1	33,380	90,526	312,350	11,534	36,248	138,208	11.48	14.53	40.22	132	527	5,558
2015.3	5/2	32,363	91,407	321,551	10,960	36,453	141,658	11.54	14.62	41.72	126	533	5,909
2015.4	6/3	28,516	85,190	321,295	9,278	33,882	146,018	11.62	14.78	44.16	108	501	6,448
2016.1	7/4	23,292	85,618	323,436	7,092	32,105	139,914	11.80	15.02	48.11	84	482	6,732
2016.2	8/5	25,053	89,188	336,177	8,297	35,467	149,675	11.87	15.00	47.09	98	532	7,048
2016.3	9/6	23,896	87,753	340,755	7,998	35,614	153,544	11.87	15.03	46.69	95	535	7,170
Panel B: Food and Drinking Places (NAICS 722)													
2014.2	0	12,149	22,087	33,130	4,317	8,207	11,949	10.99	13.10	17.80	47	108	213
2014.3	1	12,323	22,955	34,924	4,389	8,694	12,799	10.98	13.20	18.03	48	115	231
2014.4	2	11,243	22,805	35,469	3,757	8,286	12,528	11.09	13.48	18.95	42	112	237
2015.1	3	11,109	22,923	35,576	3,534	7,930	12,031	11.13	13.55	19.00	39	107	229
2015.2	4/1	10,334	22,607	35,715	3,540	8,399	12,783	11.42	13.77	18.75	40	116	240
2015.3	5/2	9,675	23,181	37,274	3,345	8,826	13,695	11.54	14.01	19.15	39	124	262
2015.4	6/3	8,704	23,144	37,990	2,836	8,584	13,609	11.60	14.26	20.23	33	122	275
2016.1	7/4	6,703	22,308	37,190	1,958	7,695	12,458	11.87	14.61	20.71	23	112	258
2016.2	8/5	6,958	22,093	37,518	2,236	8,268	13,451	11.95	14.63	20.01	27	121	269
2016.3	9/6	6,726	22,221	38,261	2,224	8,819	14,504	11.89	14.70	20.25	26	130	294

Notes: Data derived from administrative employment records obtained from the Washington Employment Security Department. Non-locatable employers (i.e., multi-location single-account firms and single-location firms which operate statewide or whose location could not be determined) 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		Difference-in-Differences between Seattle and: Snohomish, Kitsap, and Pierce Counties		Synthetic Control: Washington excluding King County		Interactive Fixed Effects: Washington excluding King County	
		Wage	Hours	Wage	Hours	Wage	Hours	Wage	Hours
2012.3	1	0.002***	-0.047***	-0.003*	-0.016***	0.003	-0.025*	-0.003	-0.009
		[0.000]	[0.000]	[0.092]	[0.005]	[0.417]	[0.076]	[0.384]	[0.326]
2012.4	2	-0.001	-0.037***	-0.002	-0.043***	0.003	-0.024	-0.001	-0.018
		[0.356]	[0.000]	[0.261]	[0.000]	[0.357]	[0.398]	[0.641]	[0.418]
2013.1	3	0.003***	-0.040***	0.001	-0.035***	0.002	-0.007	0.001	-0.022
		[0.000]	[0.000]	[0.418]	[0.000]	[0.526]	[0.826]	[0.658]	[0.541]
2013.2	4/1	0.003***	-0.022***	0.005***	-0.039***	0.002	-0.007	0.000	-0.005
		[0.000]	[0.000]	[0.002]	[0.000]	[0.615]	[0.828]	[0.908]	[0.900]
2013.3	5/2	0.005***	-0.067***	-0.001	-0.068***	0.006	-0.028	-0.005	-0.026
		[0.000]	[0.000]	[0.851]	[0.000]	[0.305]	[0.358]	[0.251]	[0.504]
2013.4	6/3	0.004***	-0.071***	-0.003	-0.105***	0.006	-0.039	-0.003	-0.034
		[0.004]	[0.000]	[0.281]	[0.000]	[0.186]	[0.411]	[0.504]	[0.487]
2014.1	7/4	0.004***	-0.033***	0.003	-0.054***	0.006	0.008	-0.004	-0.008
		[0.006]	[0.000]	[0.435]	[0.000]	[0.185]	[0.844]	[0.325]	[0.848]
2014.2	8/5	0.006***	-0.030***	0.006*	-0.064***	0.008*	-0.009	-0.001	-0.006
		[0.000]	[0.000]	[0.055]	[0.000]	[0.097]	[0.800]	[0.857]	[0.882]
2014.3	9/6	0.006***	-0.046***	0.002	-0.078***	0.011	-0.020	-0.005	-0.014
		[0.004]	[0.000]	[0.686]	[0.000]	[0.192]	[0.633]	[0.365]	[0.749]
Average		0.004	-0.044	0.001	-0.056	0.005	-0.017	-0.002	-0.016
R2		0.961	0.985	0.826	0.966			0.800	0.981
Pre-Policy RMSPE						0.003	0.013		
Obs.		68	68	68	68	1,530	1,530	1,530	1,530

Notes: Estimates for all jobs paying < $19 in all industries. Cumulative effect since 2012.2 is reported. Dependent variable in all regressions is year-over-year growth rate in each outcome. P-value for a two-tailed test of the hypothesis that the coefficient equals to zero are reported in square brackets. P-values are calculated based on robust standard errors for difference-in-differences; based on permutation inference for synthetic control, and based on i.i.d. standard errors for interactive fixed effects. RMSPE shows the root mean squared prediction error for the Synthetic Controls' pre-policy predictions of year-over-year growth. The number of observations used in the difference-in-differences specifications equals the number of regions (2, treatment and control region) times the number of quarters included in this analysis (34). 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: Effect on Wages of Low-Wage Jobs

Quarter	Quarters After Passage / Enforcement	Synthetic Control	Interactive Fixed Effects
2014.3	1	0.002	0.005
		[0.585]	[0.101]
2014.4	2	0.003	0.008***
		[0.465]	[0.013]
2015.1	3	0.002	0.009***
		[0.598]	[0.004]
2015.2	4/1	0.011**	0.016***
		[0.029]	[0.000]
2015.3	5/2	0.016***	0.022***
		[0.006]	[0.000]
2015.4	6/3	0.019***	0.019***
		[0.000]	[0.000]
2016.1	7/4	0.030***	0.032***
		[0.000]	[0.000]
2016.2	8/5	0.031***	0.031***
		[0.000]	[0.000]
2016.3	9/6	0.033***	0.034***
		[0.000]	[0.000]
R2			0.781
Pre-Policy RMSPE		0.003	
Obs.		1,890	1,890

Notes: Estimates for all jobs paying < $19 in all industries. Cumulative effect since 2014.2 is reported. Dependent variable in all regressions is year-over-year growth rate in average wages. P-value for a two-tailed test of the hypothesis that the coefficient equals to zero are reported in square brackets. P-values are calculated based on permutation inference for synthetic control, and based on i.i.d. standard errors for interactive fixed effects. RMSPE shows the root mean squared prediction error for the Synthetic Controls' pre-policy predictions of year-over-year growth. 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 6: Effect on Low-Wage Employment

Quarter	Quarters After Passage / Enforcement	Hours: Synthetic Control	Hours: Interactive Fixed Effects	Jobs: Synthetic Control	Jobs: Interactive Fixed Effects
2014.3	1	0.002	0.005	0.002	-0.003
		[0.916]	[0.766]	[0.924]	[0.842]
2014.4	2	0.006	0.000	-0.002	-0.014
		[0.713]	[0.975]	[0.892]	[0.357]
2015.1	3	-0.018	-0.015	0.007	-0.005
		[0.336]	[0.349]	[0.659]	[0.724]
2015.2	4/1	-0.006	-0.008	-0.010	-0.024
		[0.756]	[0.594]	[0.549]	[0.107]
2015.3	5/2	-0.027	-0.008	-0.011	-0.026
		[0.356]	[0.715]	[0.576]	[0.223]
2015.4	6/3	-0.006	0.008	-0.033	-0.035
		[0.894]	[0.735]	[0.391]	[0.109]
2016.1	7/4	-0.087***	-0.057***	-0.038	-0.032
		[0.005]	[0.014]	[0.293]	[0.146]
2016.2	8/5	-0.066***	-0.046*	-0.052*	-0.071***
		[0.022]	[0.052]	[0.076]	[0.001]
2016.3	9/6	-0.092*	-0.064***	-0.072*	-0.088***
		[0.051]	[0.023]	[0.067]	[0.001]
R2			0.791		0.718
Pre-Policy RMSPE		0.013		0.013	
Obs.		1,890	1,890	1,890	1,890

Notes: Estimates for all jobs paying < $19 in all industries. Cumulative effect since 2014.2 is reported. Dependent variable in all regressions is year-over-year growth rate in quarterly hours worked and in the number of beginning-of-quarter jobs. P-value for a two-tailed test of the hypothesis that the coefficient equals to zero are reported in square brackets. P-values are calculated based on permutation inference for synthetic control, and based on i.i.d. standard errors for interactive fixed effects. RMSPE shows the root mean squared prediction error for the Synthetic Controls' pre-policy predictions of year-over-year growth. 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 7: Effect on Payroll for Low-Wage Jobs

Quarter	Quarters After Passage / Enforcement	Synthetic Control	Interactive Fixed Effects
2014.3	1	-0.001	0.014
		[0.946]	[0.301]
2014.4	2	0.012	0.012
		[0.479]	[0.404]
2015.1	3	-0.004	-0.006
		[0.836]	[0.698]
2015.2	4/1	0.017	0.01
		[0.399]	[0.486]
2015.3	5/2	0.006	0.015
		[0.847]	[0.478]
2015.4	6/3	0.025	0.023
		[0.614]	[0.286]
2016.1	7/4	-0.032	-0.035
		[0.416]	[0.149]
2016.2	8/5	-0.013	-0.024
		[0.739]	[0.352]
2016.3	9/6	-0.037	-0.039
		[0.519]	[0.176]
R2			0.825
Pre-Policy RMSPE		0.012	
Obs.		1,890	1,890

Notes: Estimates for all jobs paying < $19 in all industries. Cumulative effect since 2014.2 is reported. Dependent variable in all regressions is year-over-year growth rate in quarterly payroll. P-value for a two-tailed test of the hypothesis that the coefficient equals to zero are reported in square brackets. P-values are calculated based on permutation inference for synthetic control, and based on i.i.d. standard errors for interactive fixed effects. RMSPE shows the root mean squared prediction error for the Synthetic Controls' pre-policy predictions of year-over-year growth. 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 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	-0.58	(-48.88, 31.04)	-0.04	(-0.27, 0.20)
2015.3	5/2	-1.74	(-18.45, 6.51)	-0.17	(-0.52, 0.18)
2015.4	6/3	-0.32	(-7.79, 6.51)	-0.04	(-0.48, 0.41)
2016.1	7/4	-2.94	(-7.83, -0.59)	-0.23	(-0.41, -0.06)
2016.2	8/5	-2.15	(-6.38, -0.16)	-0.18	(-0.34, -0.02)
2016.3	9/5	-2.81	(-10.20, 0.02)	-0.25	(-0.50, 0.00)

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.

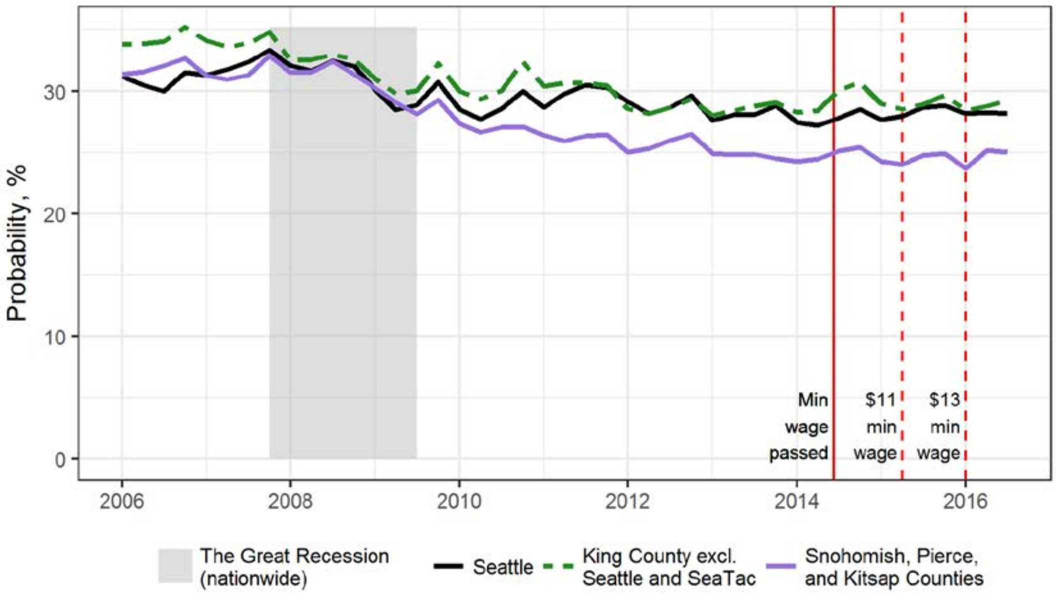
Table 9 : Effect of Restricting Analysis to Food Service and Drinking Places

		All Industries			Restaurant Industry (NAICS 722)					
	Quarters	Wages Under $19			All Wage Levels			Wages Under $19		
Quarter	After Passage / Enforcement	Wages	Hours	Jobs	Wages	Hours	Jobs	Wages	Hours	Jobs
2014.3	1	0.002	0.002	0.002	0.024**	0.003	0.035*	0.004	-0.012	0.023
		[0.585]	[0.916]	[0.924]	[0.036]	[0.862]	[0.095]	[0.354]	[0.623]	[0.247]
2014.4	2	0.003	0.006	-0.002	0.043***	0.039	0.065**	0.013*	0.029	0.035
		[0.465]	[0.713]	[0.892]	[0.000]	[0.107]	[0.042]	[0.067]	[0.315]	[0.289]
2015.1	3	0.002	-0.018	0.007	0.020***	-0.020	0.028	0.010**	-0.043	0.004
		[0.598]	[0.336]	[0.659]	[0.017]	[0.624]	[0.364]	[0.037]	[0.286]	[0.89]
2015.2	4/1	0.011**	-0.006	-0.010	0.025***	-0.041	-0.015	0.027***	-0.064*	-0.054
		[0.029]	[0.756]	[0.549]	[0.000]	[0.213]	[0.632]	[0.000]	[0.057]	[0.119]
2015.3	5/2	0.016***	-0.027	-0.011	0.047***	-0.032	0.009	0.032***	-0.071*	-0.028
		[0.006]	[0.356]	[0.576]	[0.000]	[0.438]	[0.814]	[0.000]	[0.086]	[0.479]
2015.4	6/3	0.019***	-0.006	-0.033	0.078***	-0.049	-0.032	0.036***	-0.106**	-0.097**
		[0.000]	[0.894]	[0.391]	[0.000]	[0.361]	[0.511]	[0.000]	[0.043]	[0.042]
2016.1	7/4	0.030***	-0.087***	-0.038	0.094***	-0.045	-0.014	0.066***	-0.121**	-0.104*
		[0.000]	[0.005]	[0.293]	[0.000]	[0.465]	[0.793]	[0.000]	[0.039]	[0.069]
2016.2	8/5	0.031***	-0.066***	-0.052*	0.069***	-0.034	-0.015	0.068***	-0.112	-0.118*
		[0.000]	[0.022]	[0.076]	[0.000]	[0.701]	[0.800]	[0.000]	[0.15]	[0.072]
2016.3	9/6	0.033***	-0.092*	-0.072*	0.081***	0.001	0.020	0.064***	-0.090	-0.078
		[0.000]	[0.051]	[0.067]	[0.000]	[0.988]	[0.763]	[0.000]	[0.147]	[0.109]
Pre-Policy RMSPE		0.003	0.013	0.013	0.012	0.040	0.057	0.009	0.048	0.062
Obs		1,890	1,890	1,890	1,890	1,890	1,890	1,890	1,890	1,890

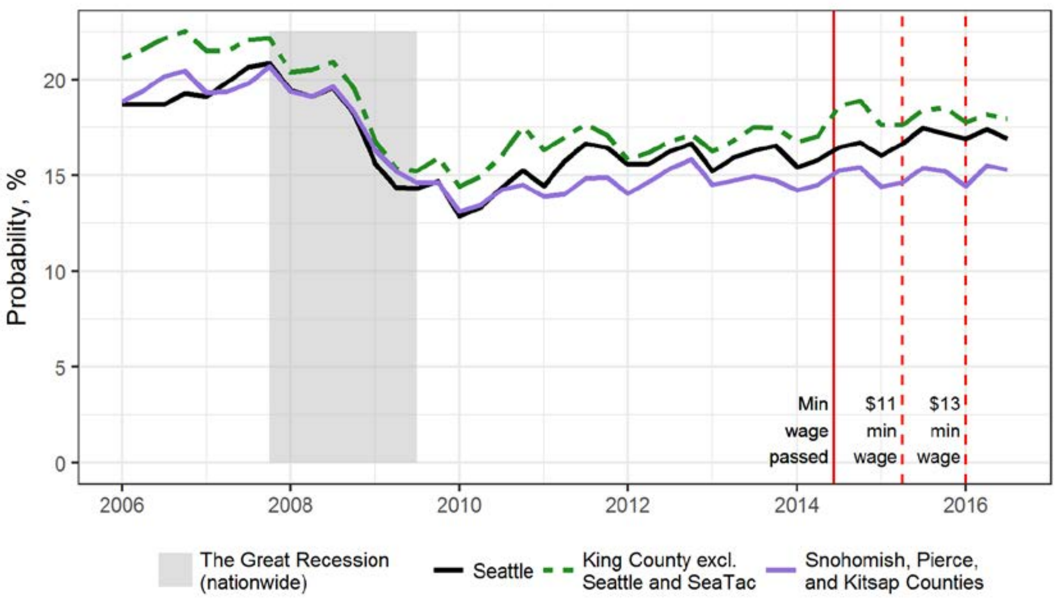
Notes: NAICS 722 = Food services and drinking places. Estimates using Synthetic Control reported. Cumulative effect since 2014.2 is reported. Dependent variable in all regressions is year-over-year growth rate in each outcome. P-value for a two-tailed test of the hypothesis that the coefficient equals to zero are reported in square brackets. P-values are calculated based on permutation. RMSPE shows the root mean squared prediction error for the Synthetic Controls' pre-policy predictions of year-over-year growth. 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

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

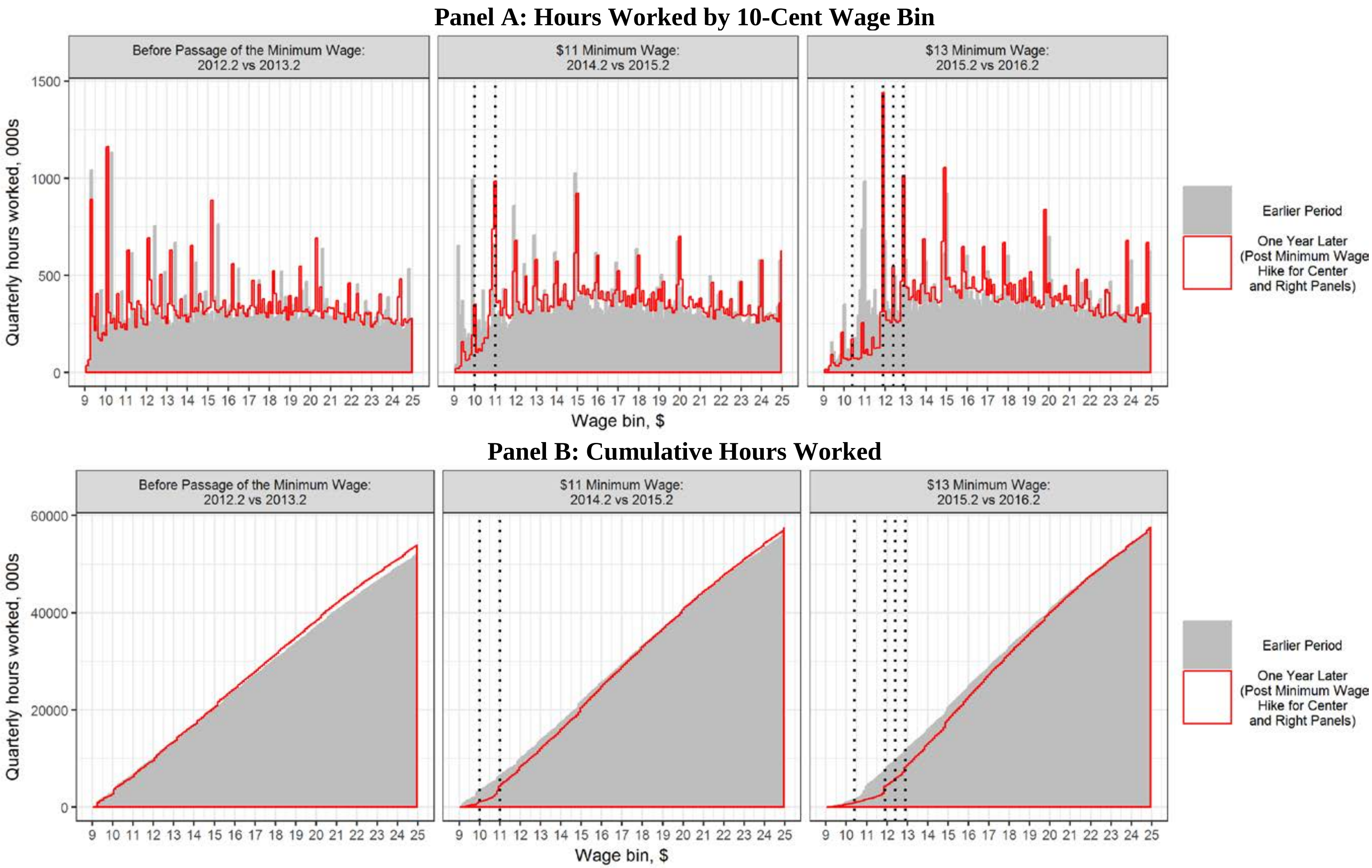


Panel B. *P*(non-locatable job in t | locatable and paid under $19/hour in t-4) by initial location



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

Figure 2: Changes in the Distribution of Quarterly Hours Worked 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. Dashed lines correspond to the minimum wage thresholds as given by the schedules shown in Table 1.

Figure 3: Difference-in-Differences Regions
(Seattle, Outlying King County, and Snohomish, Kitsap, and Pierce Counties)

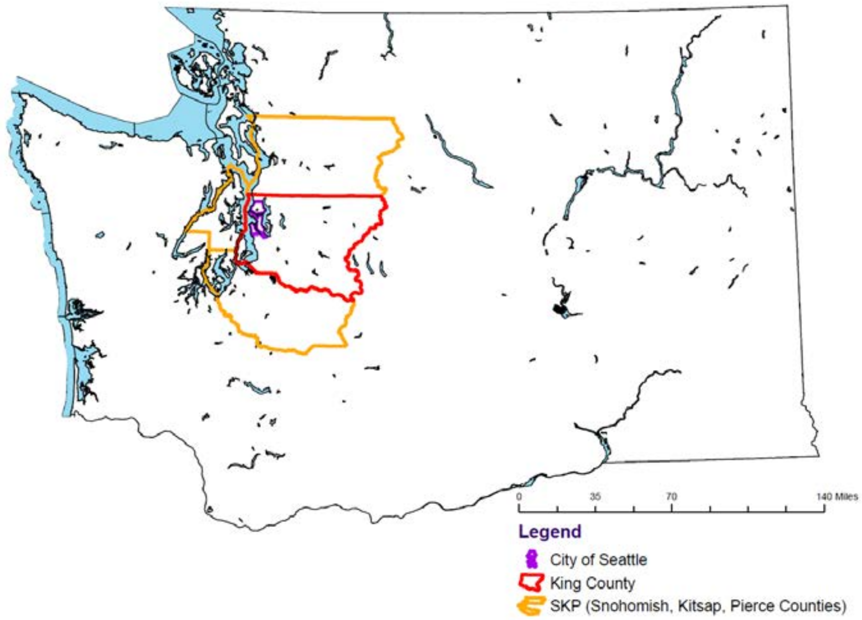


Figure 4: Synthetic Control and Interactive Fixed Effects Regions
(Seattle and Public Use Microdata Areas Outside King County)

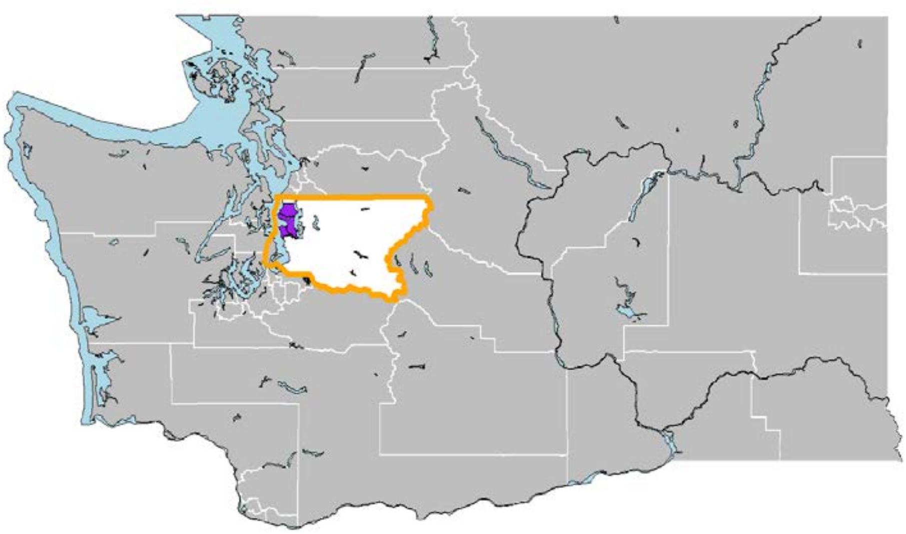
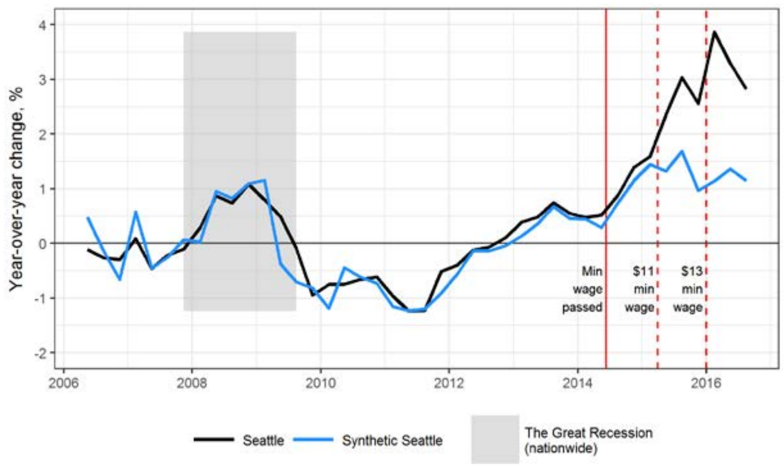
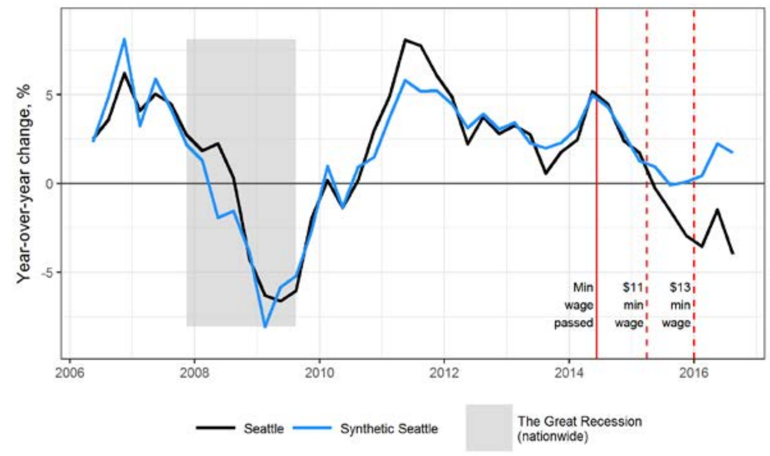


Figure 5: Employment, Wages, and Payroll in Seattle Compared to Synthetic Seattle in Jobs Paying Less than $19 Per Hour

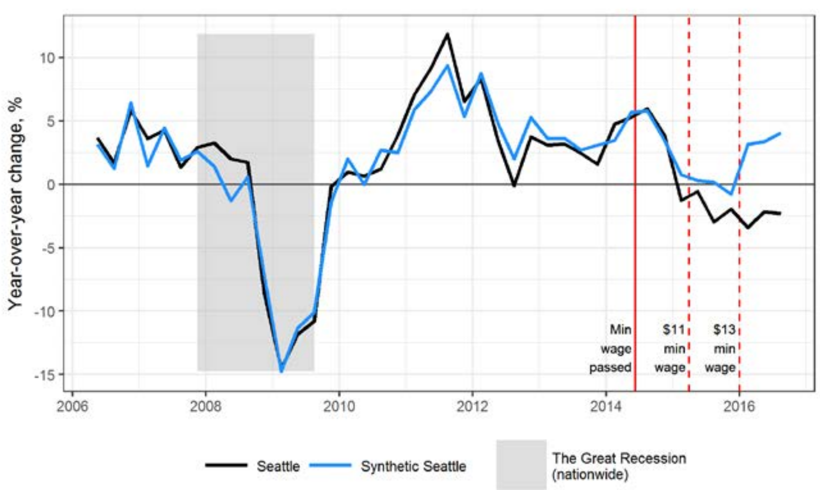
Panel A: Average Wage



Panel C: Number of Jobs



Panel B: Hours Worked



Panel D: Payroll

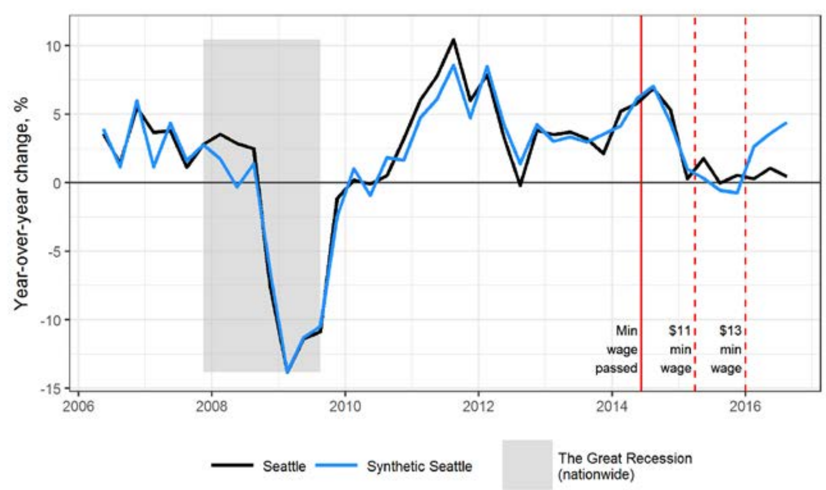


Figure 6: Employment, Wages, and Payroll in Seattle Compared to PUMAs outside of King County in Jobs Paying Less than $19 Per Hour

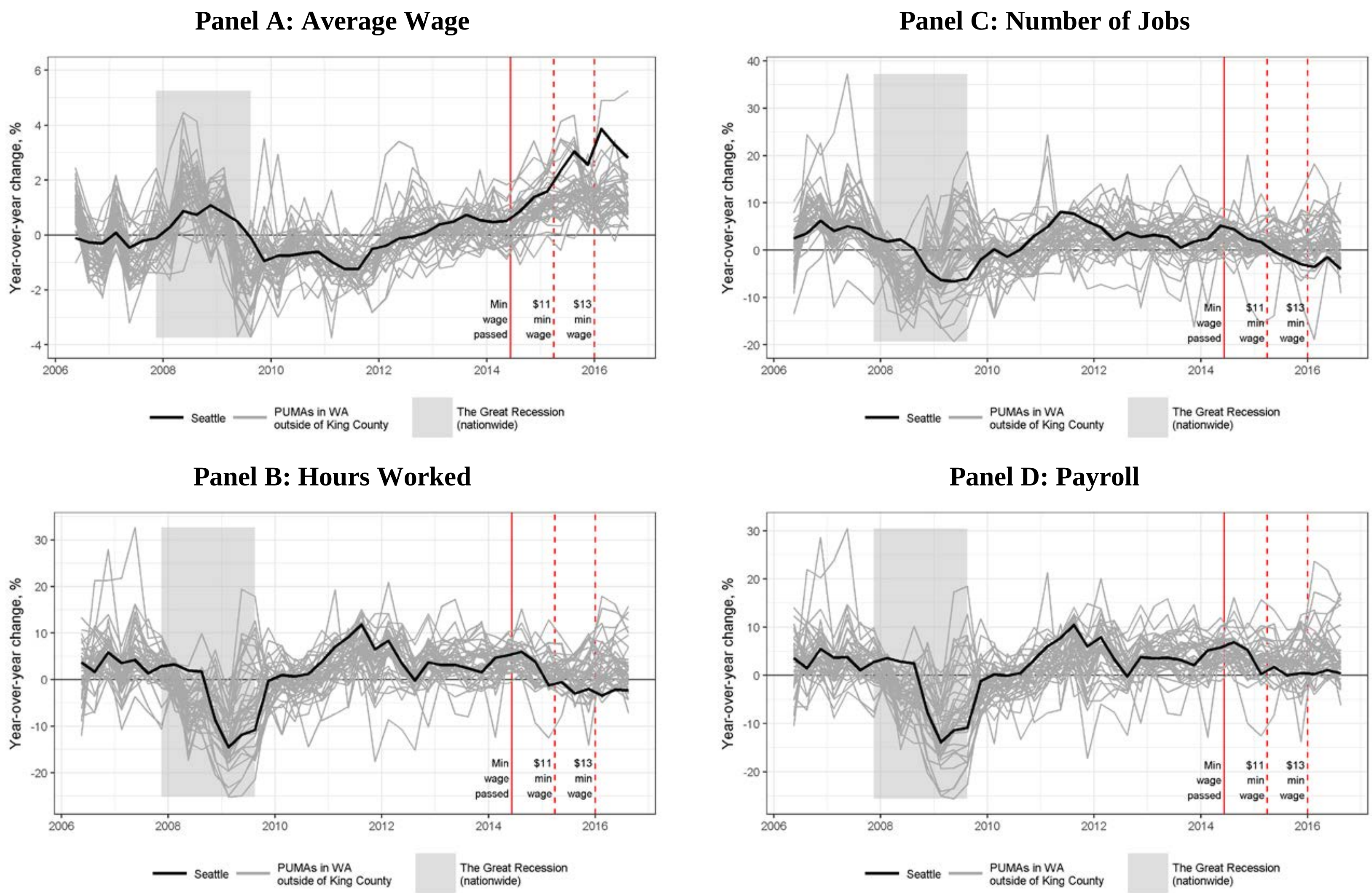


Figure 7: Sensitivity of the Estimated Percentage Change in Wages Using Different Wage Thresholds

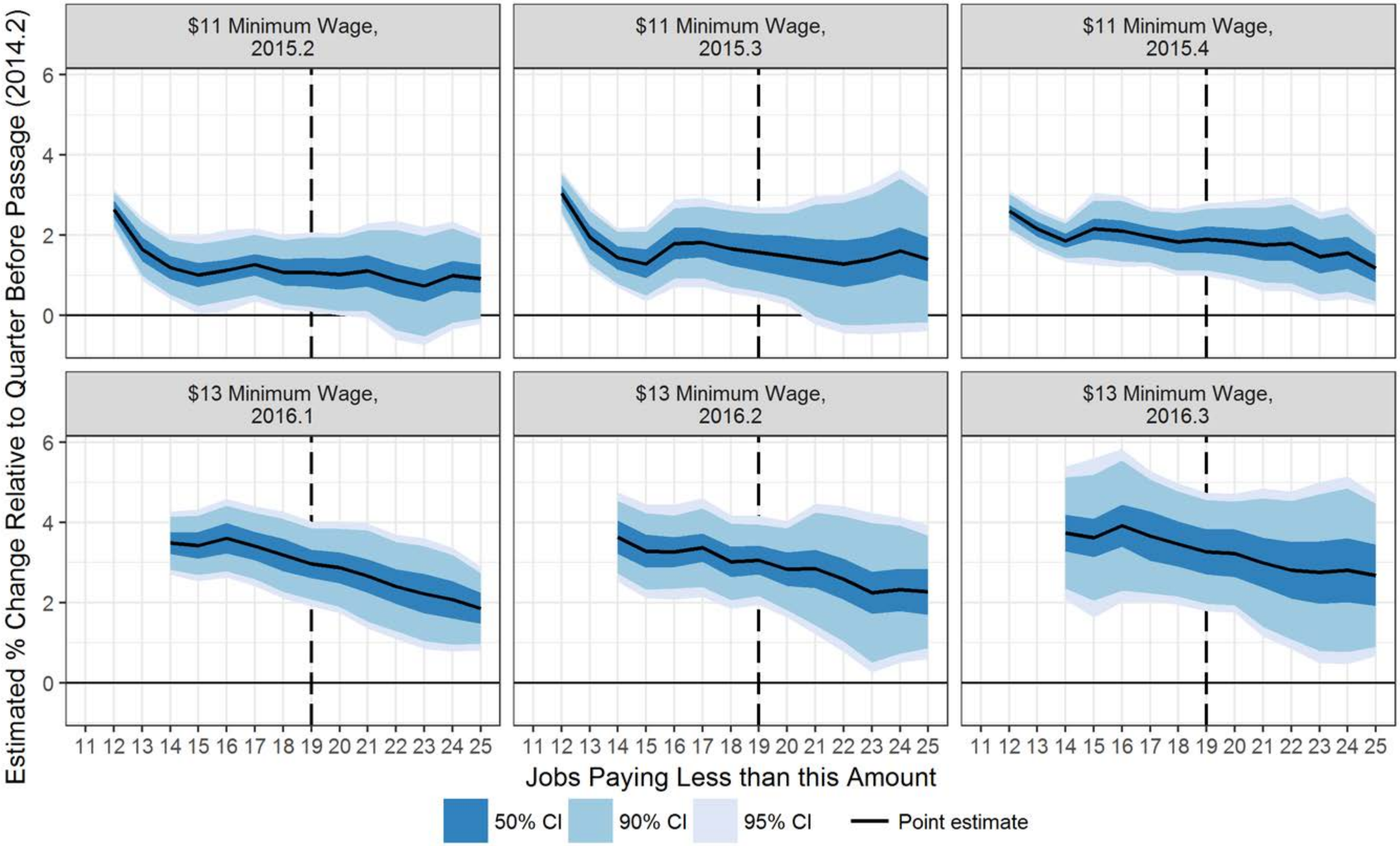
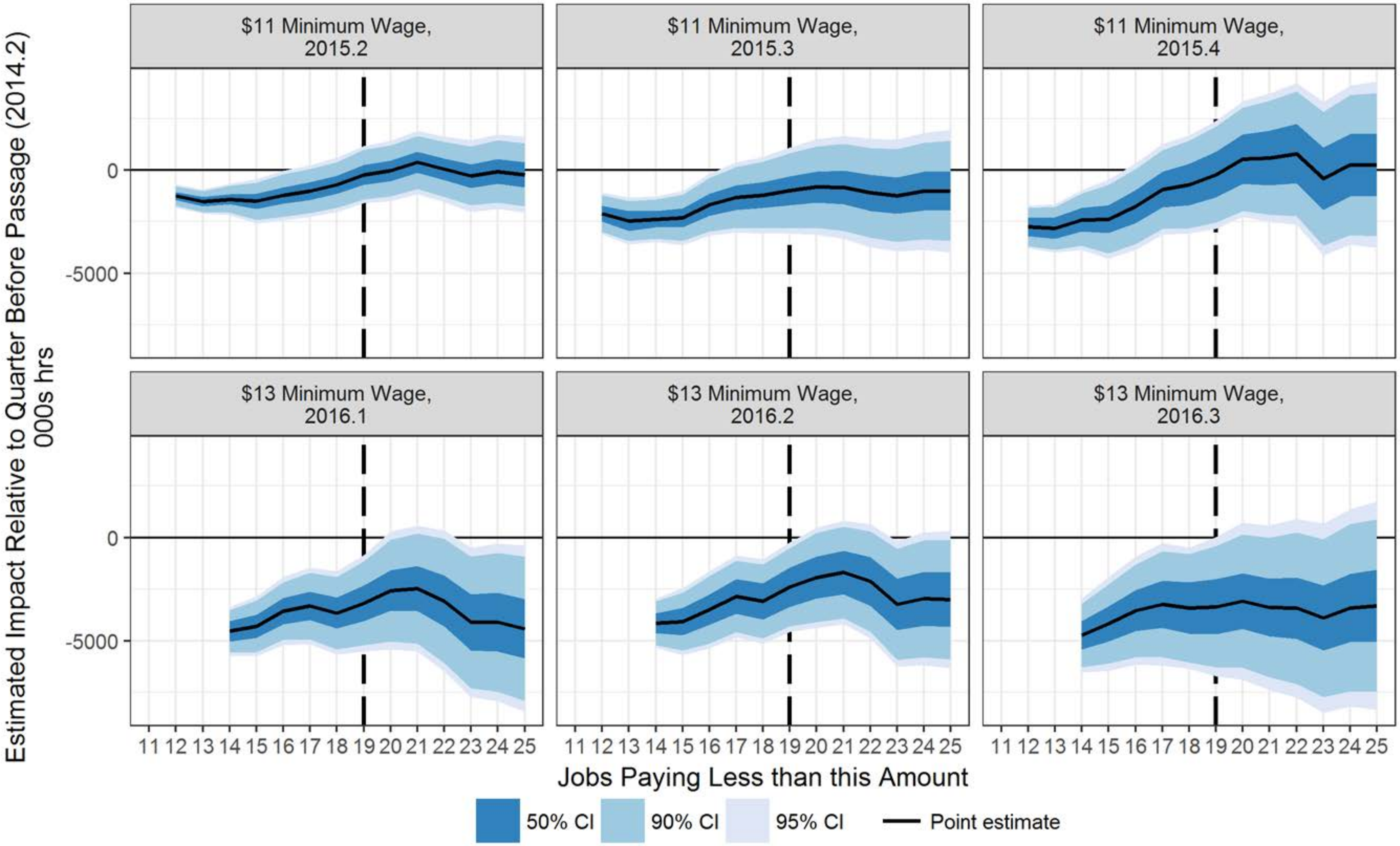


Figure 8: Sensitivity of the Estimated Percentage Change in Cumulative Hours Worked Using Different Wage Thresholds

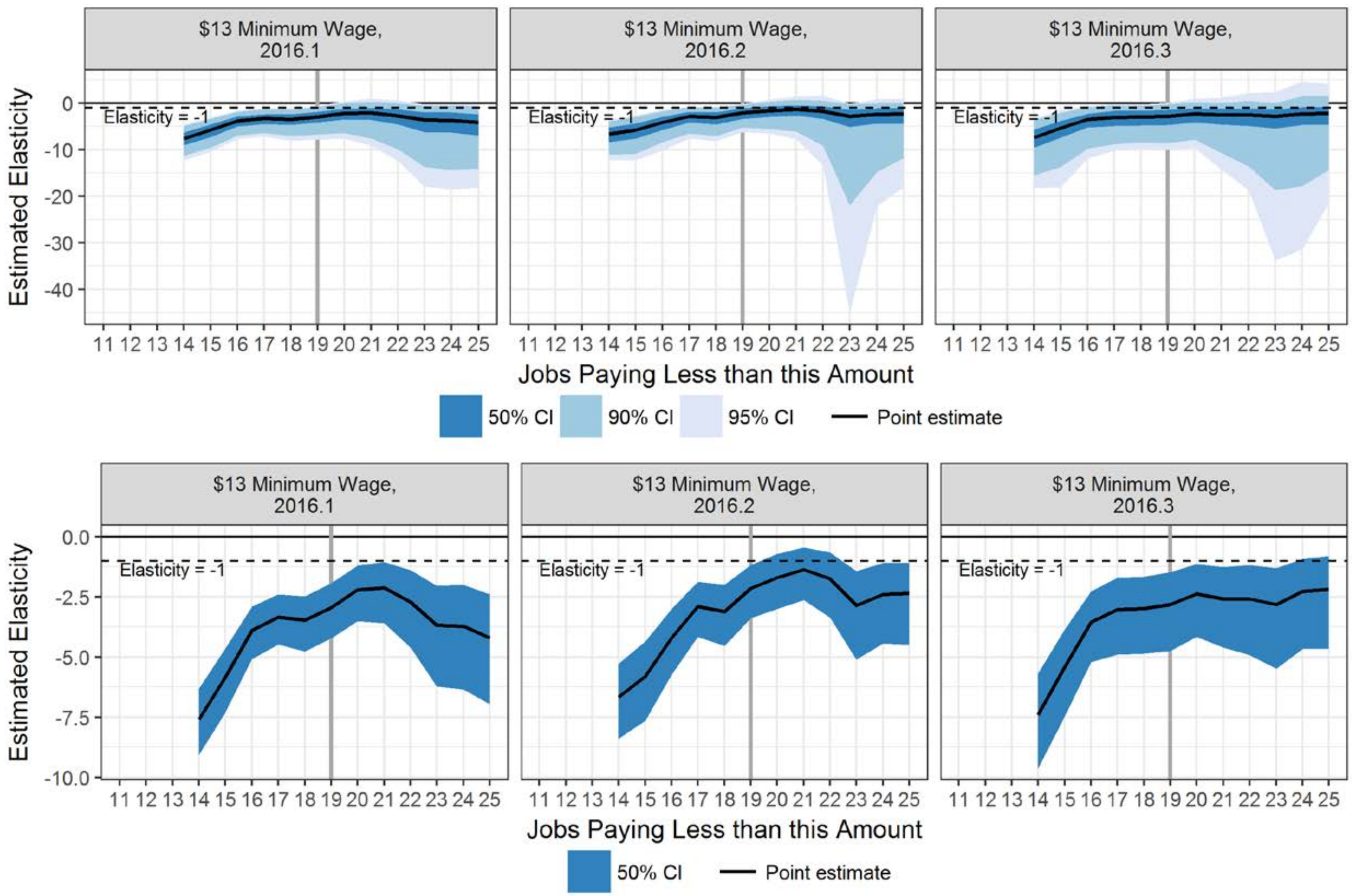
Notes: Point estimates using the synthetic control method are shown by the lines, while 50-, 90-, and 95-percent confidence intervals centered around these estimates are shown by the shaded regions.

Figure 9: Sensitivity of the Estimated Level Change in Cumulative Hours Worked Using Different Wage Thresholds



Notes: Point estimates using the synthetic control method are shown by the lines, while 50-, 90-, and 95-percent confidence intervals centered around these estimates are shown by the shaded regions.

Figure 10: 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 50-, 90-, and 95-percent confidence intervals centered around these estimates are shown by the shaded regions. The lower panels show the same estimates as the upper panels with a different scale on the y-axis to clearly show the point estimates and the 50-percent confidence interval.

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	62,412	19,922	75.5%	52,001	16,913	75.1%
Mining, Quarrying, and Oil and Gas Extraction	1,672	885.3478	65.0%	324	97	77.8%
Utilities	6,903	7,512	47.9%	693	313	69.0%
Construction	132,064	19,420	87.2%	32,255	3,503	90.2%
Manufacturing	148,163	129,881	53.3%	61,907	20,061	75.5%
Wholesale Trade	74,819	45,185	62.3%	26,800	14,736	64.5%
Retail Trade	137,500	175,024	44.0%	86,998	116,205	42.9%
Transportation and Warehousing	47,772	47,329	50.3%	18,169	10,142	64.1%
Information	73,490	31,685	69.8%	7,714	6,817	53.1%
Finance and Insurance	36,823	59,111	38.4%	9,446	16,701	36.2%
Real Estate and Rental and Leasing	32,184	14,242	69.3%	16,260	6,986	70.1%
Professional, Scientific, and Technical Services	118,649	33,067	78.1%	22,762	6,360	78.1%
Management of Companies and Enterprises	3,896	3,801	55.3%	471	1,138	29.7%
Administrative and Support and Waste Management and Remediation Services	98,437	53,451	64.6%	49,645	34,242	59.0%
Educational Services	182,502	64,196	74.0%	59,582	16,298	78.0%
Health Care and Social Assistance	189,124	130,104	59.2%	82,314	53,030	60.8%
Arts, Entertainment, and Recreation	51,797	8,654	85.7%	33,060	5,117	86.6%
Accommodation and Food Services	134,570	80,558	62.4%	107,948	60,987	63.8%
Other Services (except Public Administration)	60,077	19,842	75.1%	31,743	13,151	70.7%
Public Administration	83,764	63,704	56.8%	15,686	9,911	61.3%
Total	1,676,653	1,007,585	62.4%	715,808	412,715	63.4%

Notes: Firms are defined by federal tax Employer Identification Numbers. Statistics are computed for the average quarter between 2005.1 and 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 by Industry and Wage Level

Quarter	Quarters After Passage / Enforcement	Number of Jobs Paying: 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	38,013	52,744	44,357	28,049	22,039	20,480	87,575
2014.3	1	38,906	53,939	44,108	27,642	21,873	20,166	94,846
2014.4	2	33,949	53,830	43,614	29,146	23,091	21,030	99,461
2015.1	3	33,438	55,320	43,484	29,068	23,259	21,050	100,085
2015.2	4/1	33,380	57,146	45,719	30,263	24,079	19,392	102,371
2015.3	5/2	32,363	59,044	45,385	30,350	24,052	21,604	108,753
2015.4	6/3	28,516	56,674	44,776	30,795	24,318	22,626	113,590
2016.1	7/4	23,292	62,326	46,117	31,004	24,803	22,374	113,520
2016.2	8/5	25,053	64,135	49,771	32,443	25,876	23,120	115,779
2016.3	9/6	23,896	63,857	49,451	31,550	25,051	23,297	123,653
Panel B: Washington State (including Seattle)								
2014.2	0	422,884	427,840	309,291	175,158	131,078	109,641	408,006
2014.3	1	446,095	425,478	309,742	178,272	131,138	105,776	450,133
2014.4	2	397,426	442,832	314,298	190,231	140,163	115,250	447,761
2015.1	3	398,197	433,982	305,534	185,980	137,259	114,680	440,501
2015.2	4/1	397,770	452,800	318,444	187,502	138,373	110,959	451,661
2015.3	5/2	408,011	454,598	317,983	193,151	140,689	112,596	504,029
2015.4	6/3	366,828	462,163	320,651	197,784	145,847	119,156	494,578
2016.1	7/4	359,337	457,193	315,716	194,563	143,536	117,523	473,762
2016.2	8/5	371,206	479,912	340,516	193,851	145,315	119,073	488,227
2016.3	9/6	372,768	468,498	330,602	193,105	142,883	115,260	527,777

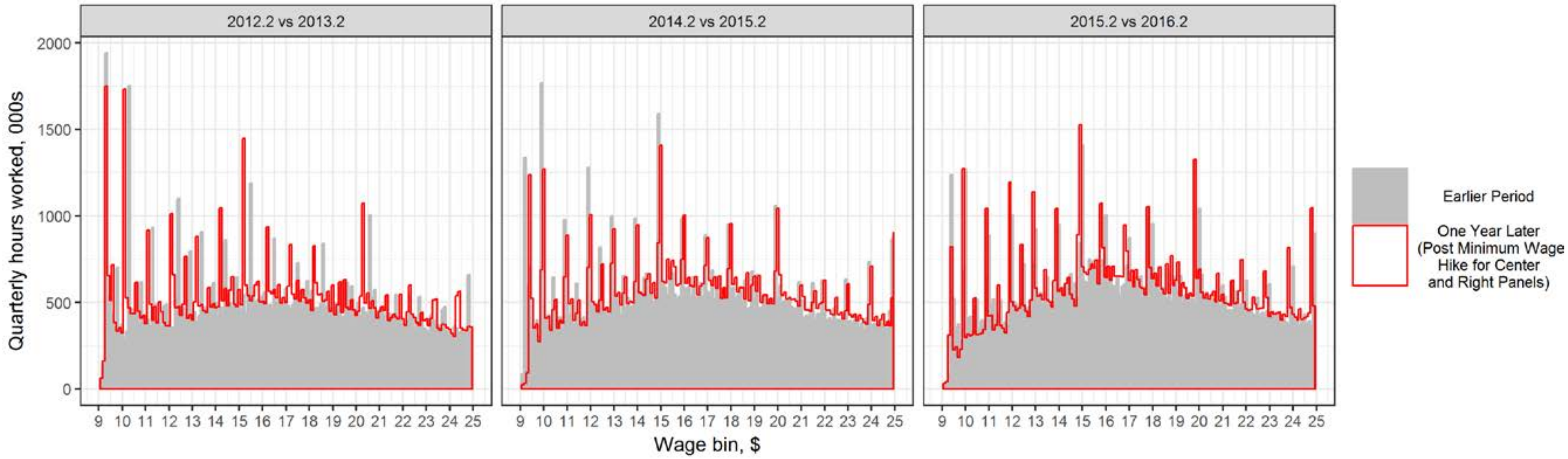
Appendix Table 3: Comparison of Estimated Effect based on Growth Rates vs. Levels of Outcomes

Quarter	Quarters After Passage / Enforcement	Wages			Hours			Jobs		
		Growth rates	Levels	Standard. levels	Growth rates	Levels	Standard. levels	Growth rates	Levels	Standard. levels
2014.3	1	0.002	0.003	0.005**	0.002	-0.004	0.022	0.002	0.011	0.011
		[0.585]	[0.391]	[0.036]	[0.916]	[0.76]	[0.201]	[0.924]	[0.321]	[0.624]
2014.4	2	0.003	0.008***	0.01***	0.006	-0.013	-0.009	-0.002	0.011	0.019
		[0.465]	[0.024]	[0.000]	[0.713]	[0.333]	[0.618]	[0.892]	[0.462]	[0.312]
2015.1	3	0.002	0.009***	0.013***	-0.018	0.000	-0.005	0.007	0.012	0.022
		[0.598]	[0.008]	[0.000]	[0.336]	[0.987]	[0.818]	[0.659]	[0.568]	[0.415]
2015.2	4/1	0.011**	0.015***	0.021***	-0.006	-0.003	-0.015	-0.010	0.022	0.028
		[0.029]	[0.002]	[0.000]	[0.756]	[0.892]	[0.467]	[0.549]	[0.251]	[0.34]
2015.3	5/2	0.016***	0.020***	0.026***	-0.027	-0.019	-0.016	-0.011	0.007	0.013
		[0.006]	[0.013]	[0.000]	[0.356]	[0.406]	[0.452]	[0.576]	[0.469]	[0.505]
2015.4	6/3	0.019***	0.018***	0.029***	-0.006	-0.021	-0.019	-0.033	-0.009	0.004
		[0.000]	[0.000]	[0.000]	[0.894]	[0.564]	[0.597]	[0.391]	[0.785]	[0.924]
2016.1	7/4	0.03***	0.039***	0.048***	-0.087***	-0.048*	-0.055**	-0.038	-0.012	-0.004
		[0.000]	[0.000]	[0.000]	[0.005]	[0.051]	[0.045]	[0.293]	[0.660]	[0.903]
2016.2	8/5	0.031***	0.038***	0.049***	-0.066***	-0.071	-0.089**	-0.052*	-0.011	-0.002
		[0.000]	[0.000]	[0.000]	[0.022]	[0.101]	[0.036]	[0.076]	[0.709]	[0.959]
2016.3	9/6	0.033***	0.036***	0.049***	-0.092*	-0.099**	-0.112***	-0.072*	-0.063**	-0.047
		[0.000]	[0.000]	[0.000]	[0.051]	[0.029]	[0.015]	[0.067]	[0.027]	[0.26]
Pre-Policy RMSPE		0.003	0.061	0.277	0.013	99856	0.202	0.013	241	0.259
Obs		1,890	1,890	1,890	1,890	1,890	1,890	1,890	1,890	1,890

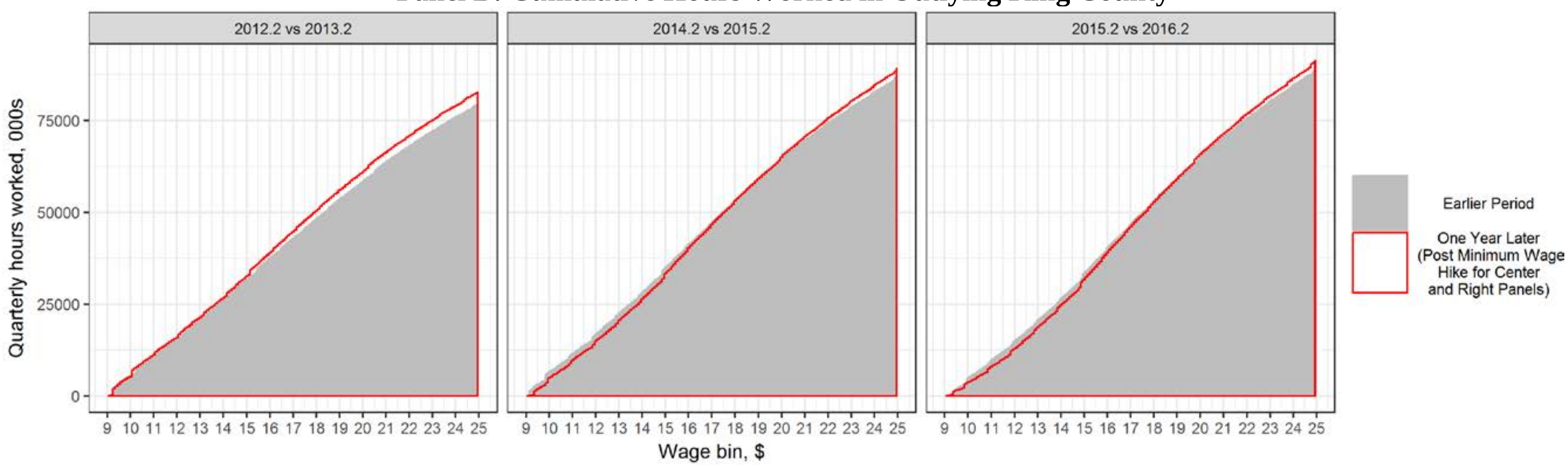
Notes: Estimates for all jobs paying < $19 in all industries. Estimates using Synthetic Control reported. Cumulative effect since 2014.2 is reported. Dependent variable in growth rates specification is year-over-year growth rate in each outcome. Dependent variable in levels specifications is the level of each outcome divided by five, except for mean wages. Dependent variable in standardized levels specification is the level of each outcome minus its pre-policy mean divided by its per-policy standard deviation. P-value for a two-tailed test of the hypothesis that the coefficient equals to zero are reported in square brackets. P-values are calculated based on permutation. RMSPE shows the root mean squared prediction error for the Synthetic Controls' pre-policy predictions. The number of observations used in the synthetic control specification 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

Appendix Figure 1: Changes in the Distribution of Quarterly Hours Worked in Outlying King County and Snohomish, Pierce, and Kitsap Counties.

Panel A: Hours Worked by 10-Cent Wage Bin in Outlying King County

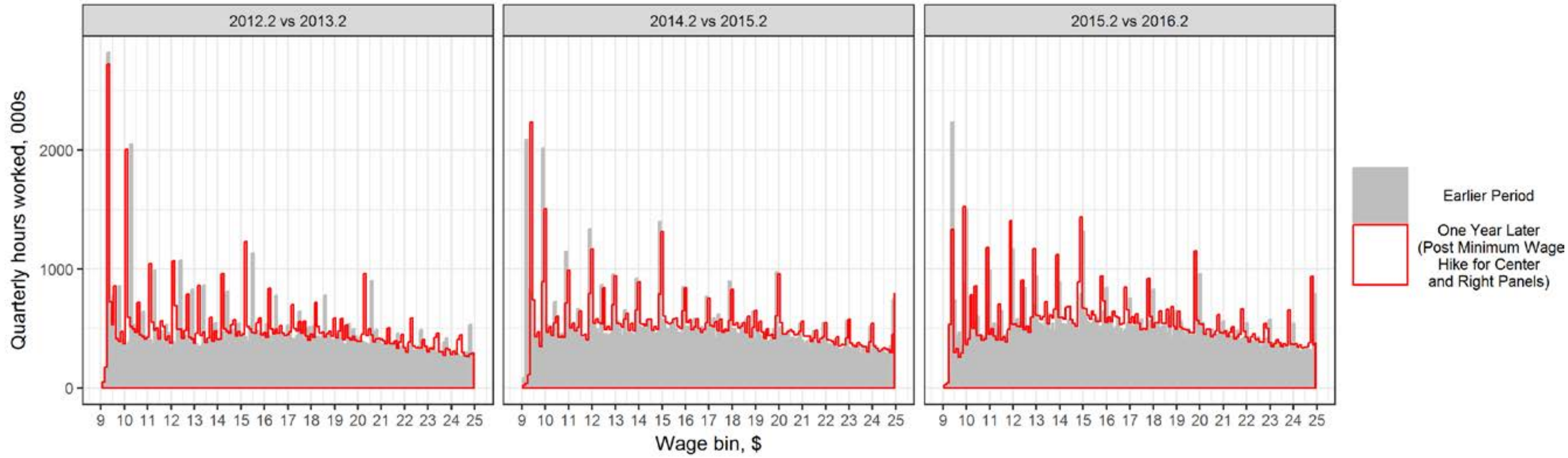


Panel B: Cumulative Hours Worked in Outlying King County

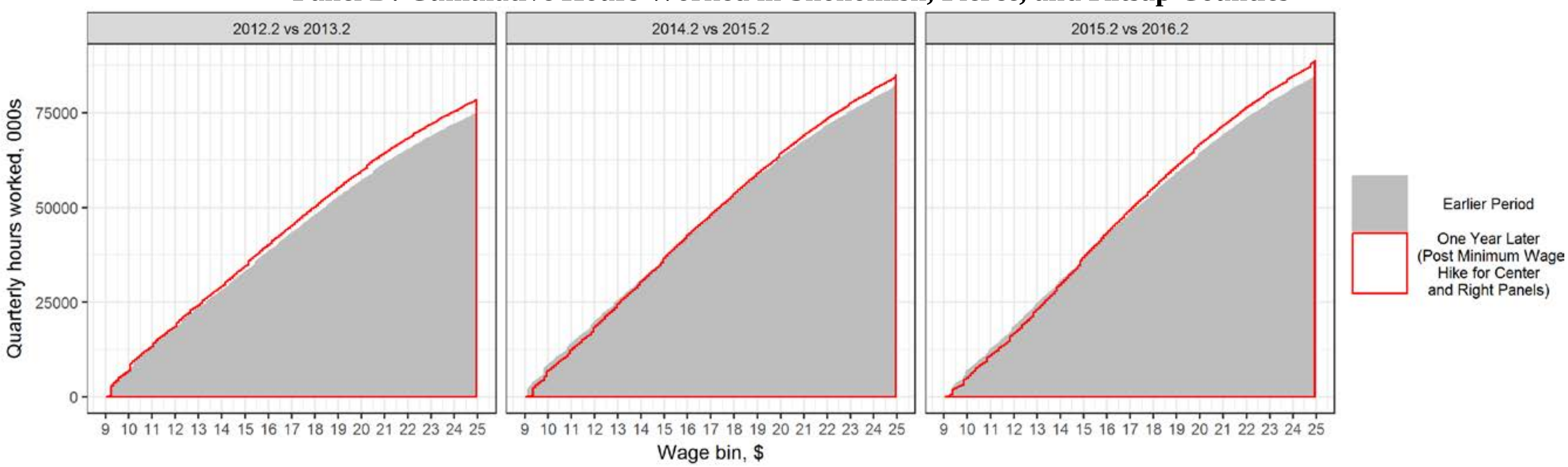


Appendix Figure 1 Continued on Next Page

Panel C: Hours Worked by 10-Cent Wage Bin in Snohomish, Pierce, and Kitsap Counties

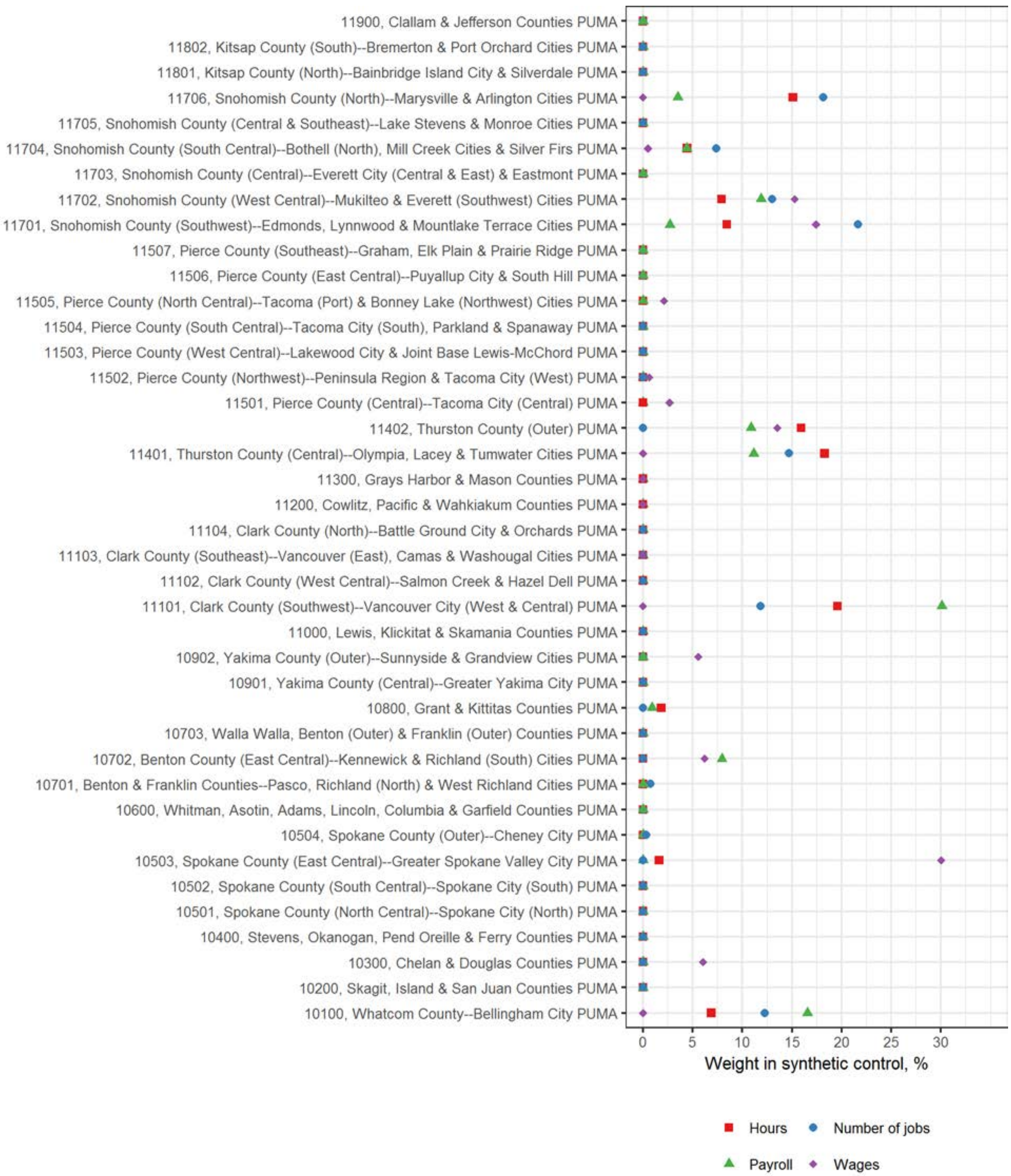


Panel D: Cumulative Hours Worked in Snohomish, Pierce, and Kitsap Counties



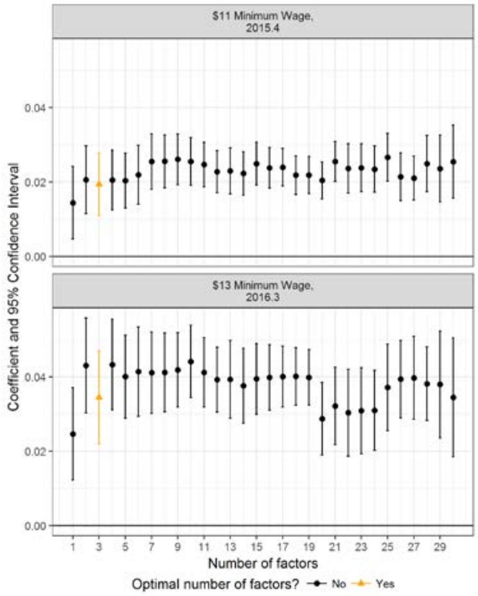
Notes: Authors calculations based on UI records from State of WA using the sample of jobs in locatable employers in Outlying King County (i.e., King County excluding Seattle and SeaTac) and Snohomish, Pierce, and Kitsap Counties. Wage rates and earnings are expressed in constant prices of 2015 Q2

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

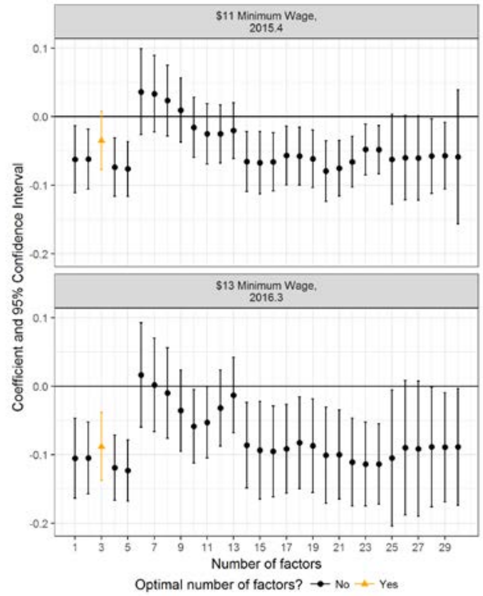


Appendix Figure 3: Sensitivity of the Interactive Fixed Effects Estimates to the Number of Factors Used

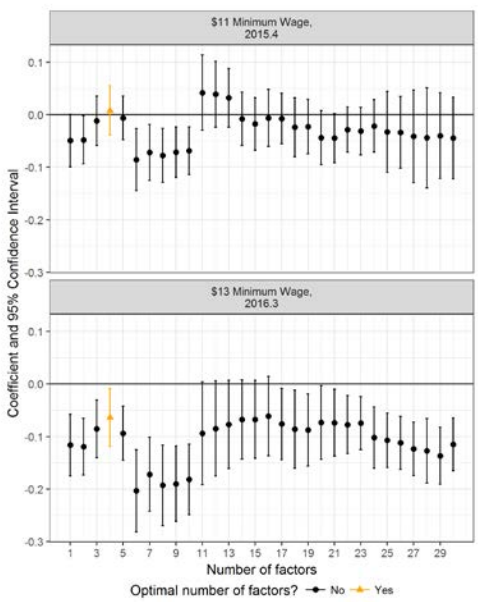
Panel A: Average Wage, Jobs paying <$19 per hour



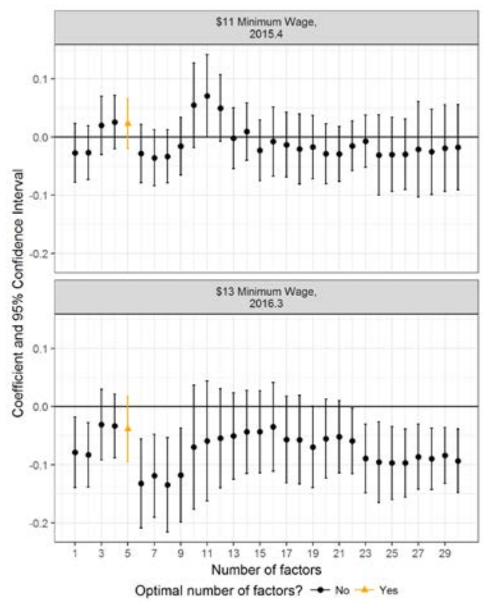
Panel C: Number of Jobs, Jobs paying <$19 per hour



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

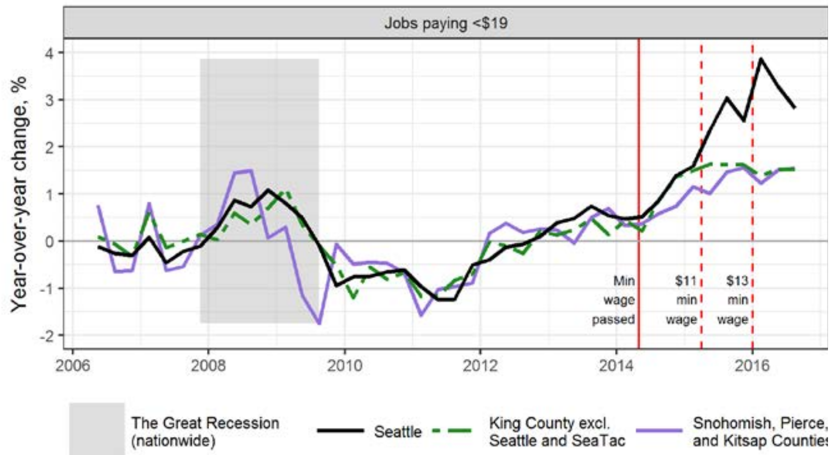


Panel D: Payroll, Jobs paying <$19 per hour

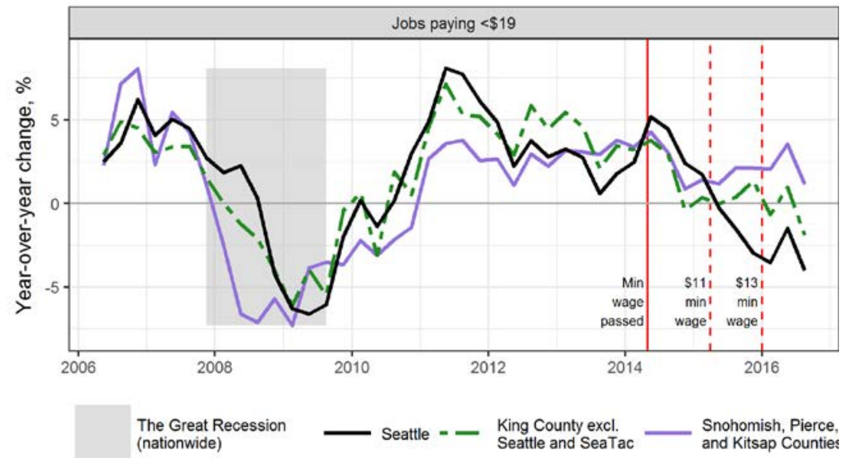


Appendix Figure 4: Year-over-year Growth Rates in Employment, Wages, and Payroll in Seattle Compared to Outlying King County and Snohomish, Kitsap, and Pierce Counties

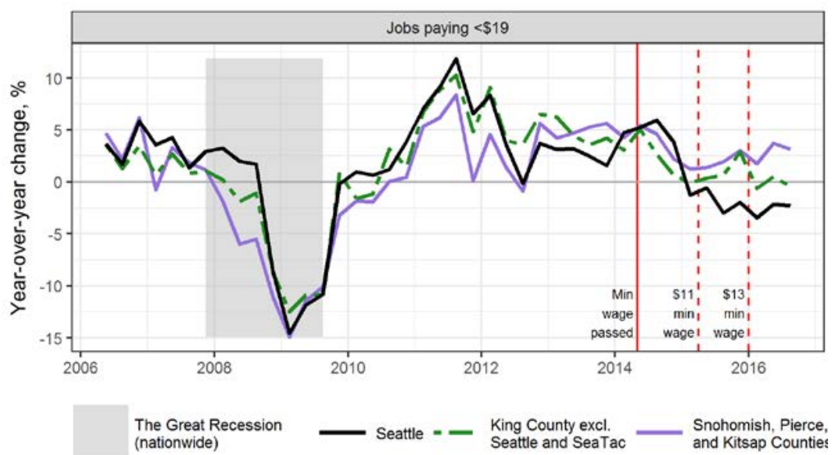
Panel A: Average Wage



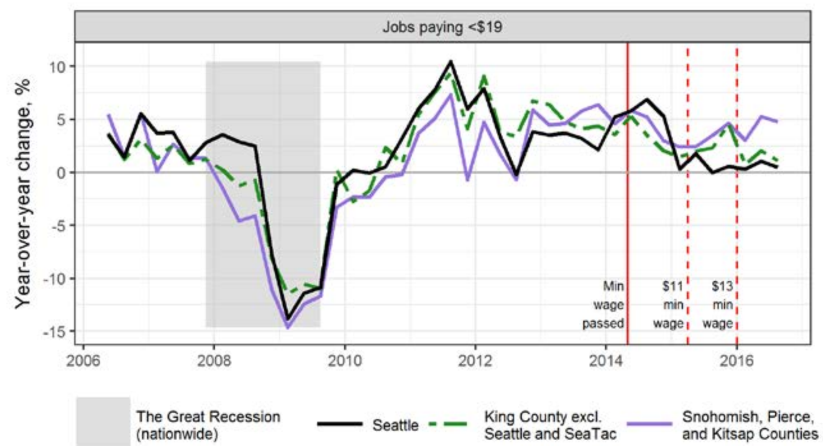
Panel C: Number of Jobs



Panel B: Hours Worked

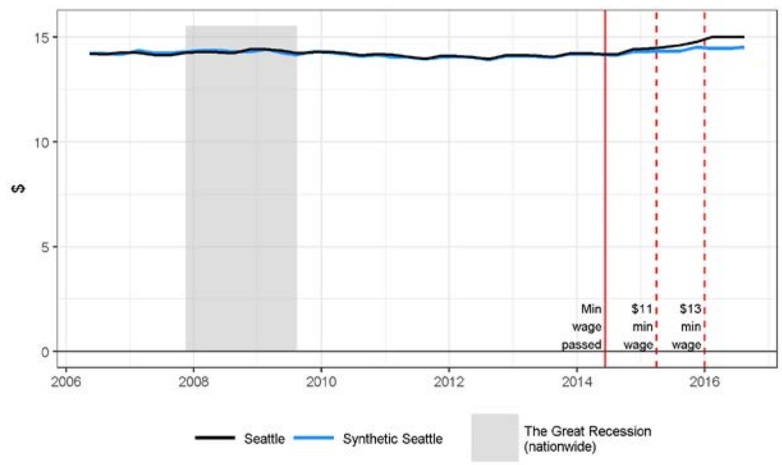


Panel D: Payroll

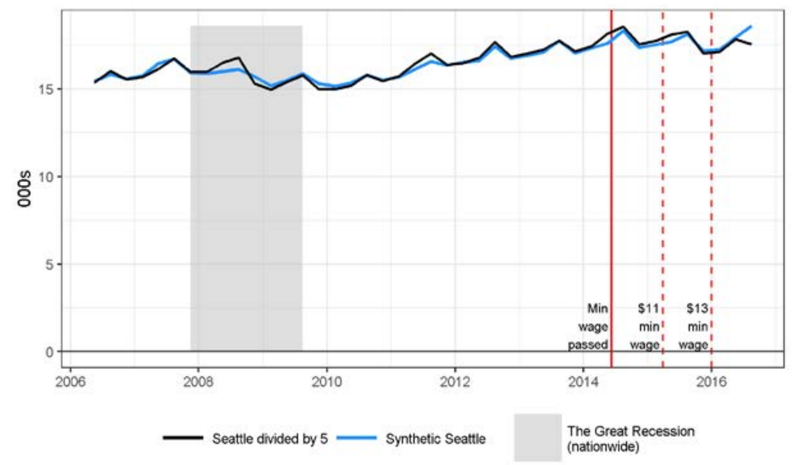


Appendix Figure 5: *Levels of* Employment, Wages, and Payroll in Seattle Compared to Synthetic Seattle in Jobs Paying Less than $19 Per Hour

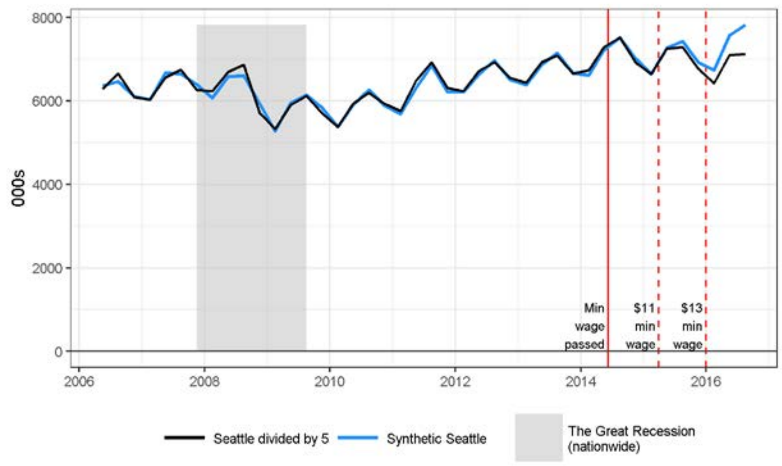
Panel A: Average Wage



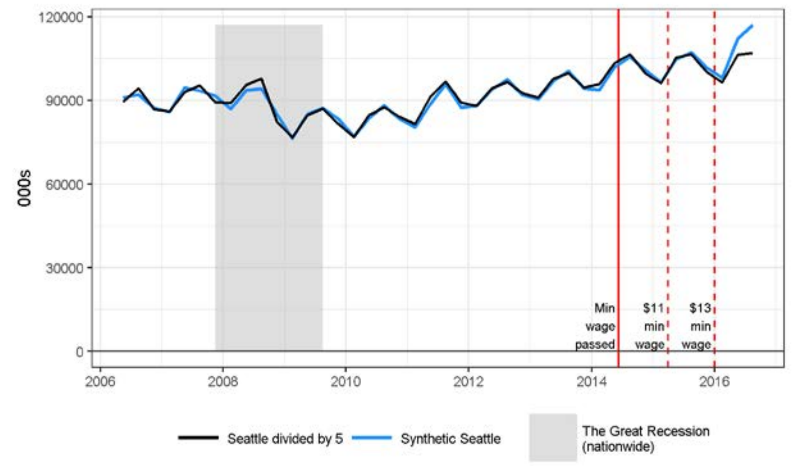
Panel C: Number of Jobs



Panel B: Hours Worked

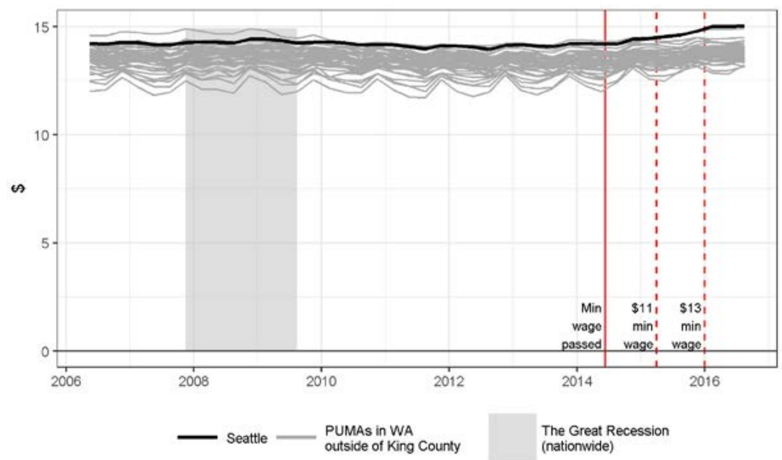


Panel D: Payroll

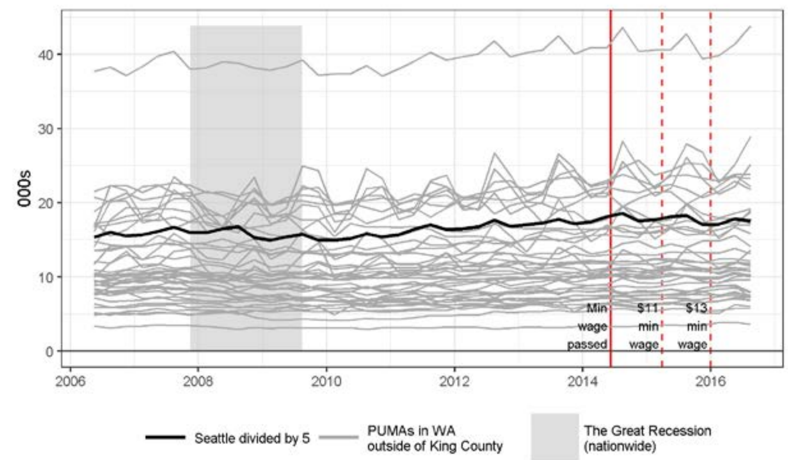


Appendix Figure 6: ***Levels of*** **Employment, Wages, and Payroll in Seattle Compared to PUMAs Outside of King County in Jobs Paying Less than $19 Per Hour**

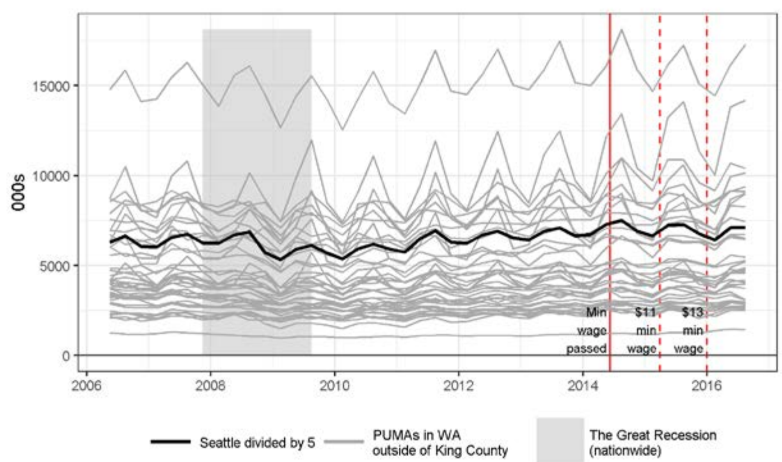
Panel A: Average Wage



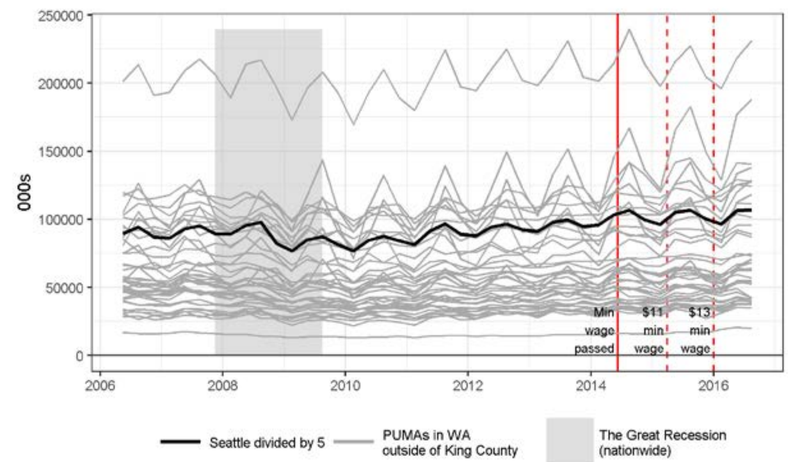
Panel C: Number of Jobs



Panel B: Hours Worked

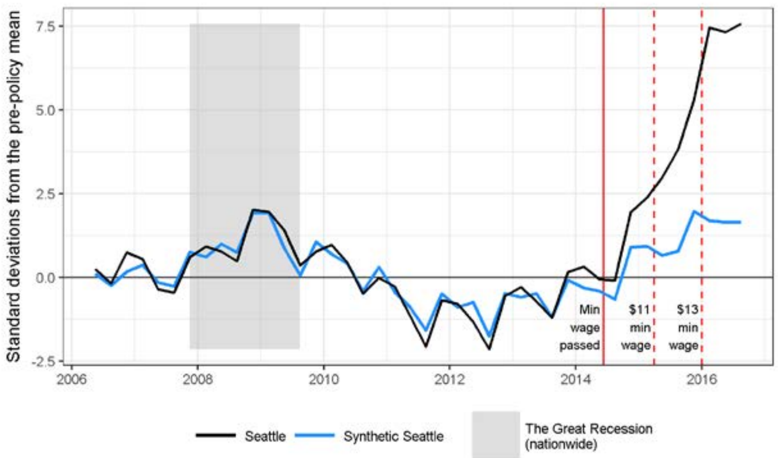


Panel D: Payroll

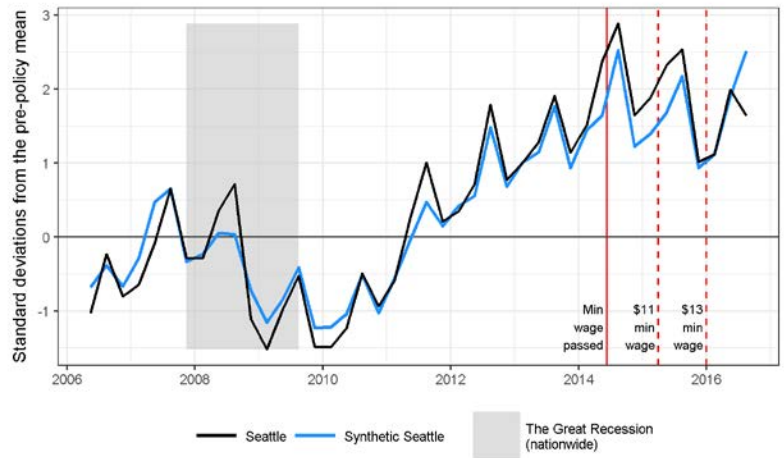


Appendix Figure 7: ***Standardized Levels of*** **Employment, Wages, and Payroll in Seattle Compared to Synthetic Seattle in Jobs Paying Less than $19 Per Hour**

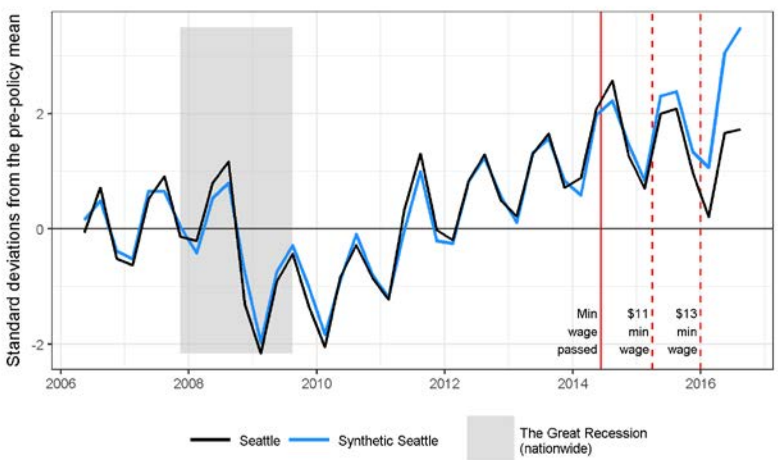
Panel A: Average Wage



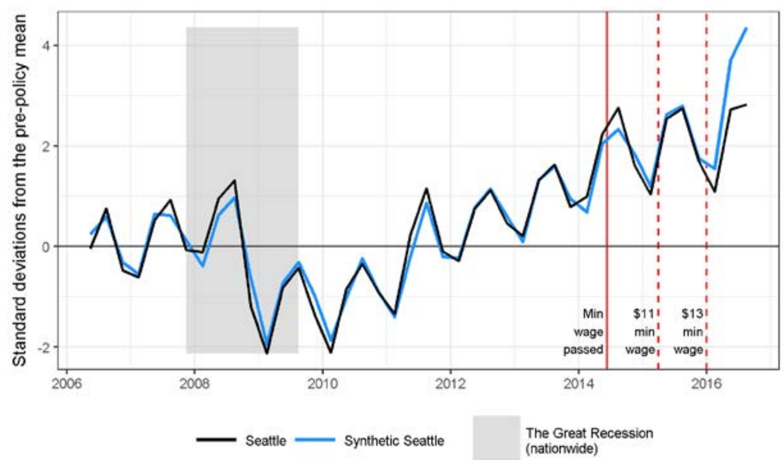
Panel C: Number of Jobs



Panel B: Hours Worked

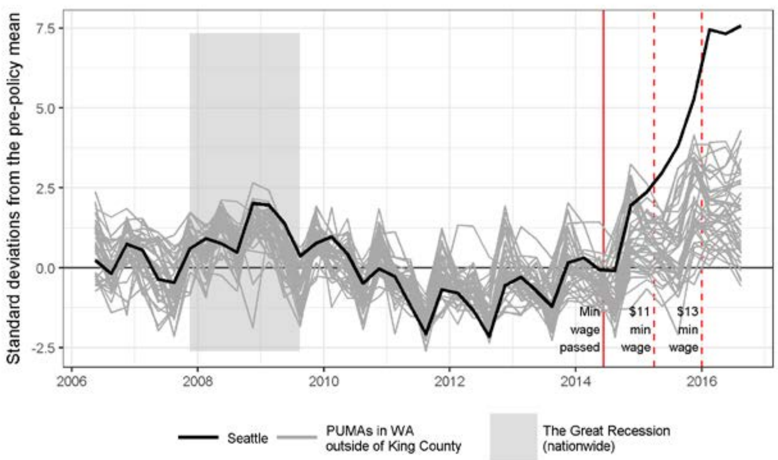


Panel D: Payroll

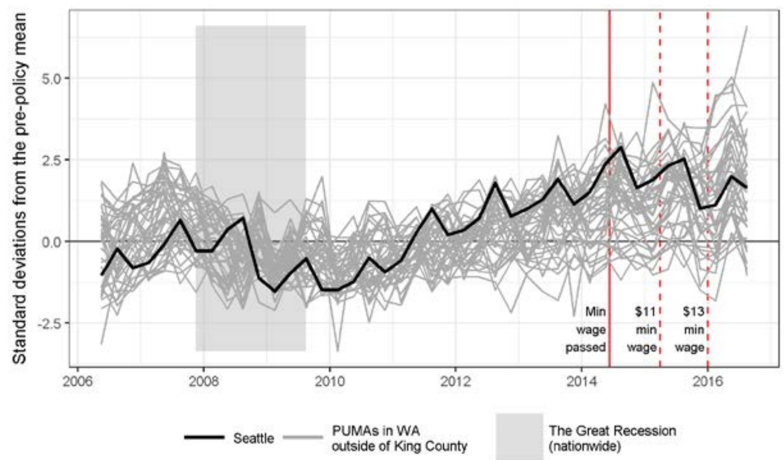


Appendix Figure 8: *Standardized level of* Employment, Wages, and Payroll in Seattle Compared to PUMAs Outside of King County in Jobs Paying Less than $19 Per Hour

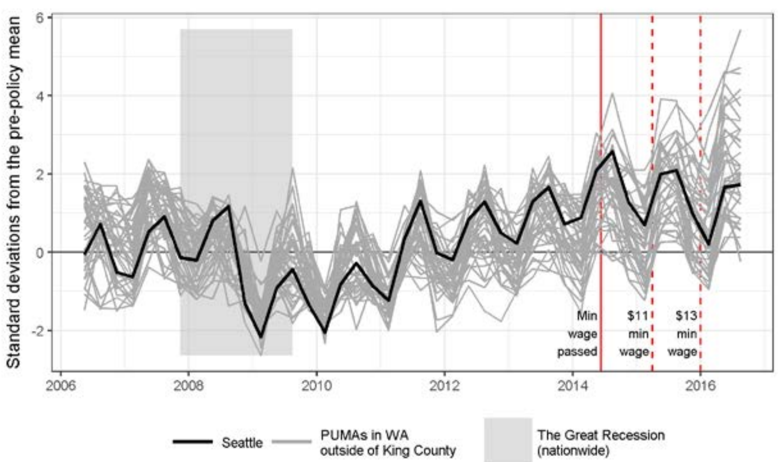
Panel A: Average Wage



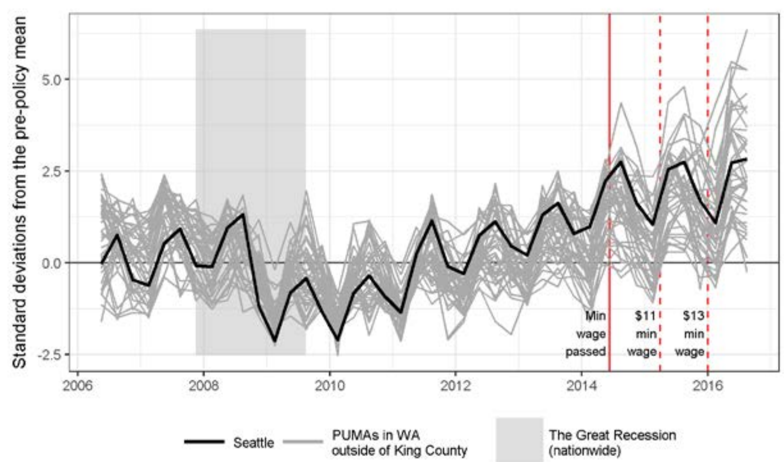
Panel C: Number of Jobs



Panel B: Hours Worked



Panel D: Payroll



Appendix Figure 9: Decomposition of the Effect on Hours Worked: Contribution of Wages Rising Above the $19 Threshold