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Minimum-Wage Increases and Low-Wage Employment: Evidence from Seattle†

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Seattle raised its minimum wage to as much as $11 in 2015 and as much as $13 in 2016. We use Washington State administrative data to conduct two complementary analyses of its impact. Relative to outlying regions of the state identified by the synthetic control method, aggregate employment at wages less than twice the original minimum—measured by total hours worked—declined. A portion of this reduction reflects jobs transitioning to wages above the threshold; the aggregate analysis likely overstates employment effects. Longitudinal analysis of individual Seattle workers matched to counterparts in outlying regions reveals no change in the probability of continued employment but significant reductions in hours, particularly for less experienced workers. Job turnover declined, as did hiring of new

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workers into low-wage jobs. Analyses suggest aggregate employment elasticities in the range of -0.2 *to* -2.0*, concentrated on the intensive margin in the short run and largest among inexperienced workers.* (*JEL* J22, J23, J24, J31, J38, R23)

Arguments for and against minimum-wage increases often invoke significantly different narratives regarding the low-wage labor market. Opponents cite neoclassical economic theory, which suggests that binding price-floor policies, including minimum wages, should lead to a nonmarket equilibrium marked by excess supply and diminished demand. Further, opponents see low-wage jobs as temporary phenomena, offered to workers with little experience or skill who rapidly attain both as they spend time on the job. To proponents, by contrast, low-wage jobs are not avenues of advancement but dead ends, both inequitable and inefficient to the extent that they reflect employers' monopsony power.[1]

This paper joins a lengthy and vibrant empirical literature on the effects of the minimum wage, analyzing the impact of two increases implemented in the city of Seattle on April 1, 2015 and January 1, 2016.[2] The analysis uses administrative employment microdata from Washington State, one of only four states that collect hours data in the course of administering the unemployment insurance program. These data enable us to expand on prior studies in three critical respects. First, we are able to exactly identify low-wage workers rather than rely on industry-based proxies that leave the majority of the low-wage workforce unstudied.[3] Second, we study employment effects of the minimum wage on both the extensive and intensive margins. Third, we study individuals tracked longitudinally and present results for workers stratified by experience.[4]

[1] Prior conceptual arguments challenging the neoclassical prediction incorporate the presence of monopsony power (Bhaskar and To 1999; Manning 2003), the possibility that higher wages intensify job searching 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).

[2] Most prior research has focused on increases at the federal level (Card 1992; Katz and Krueger 1992; Belman and Wolfson 2010) or state level (Card and Krueger 1994; Neumark and Wascher 1995; Dube, Lester, and Reich 2010, 2016; Meer and West 2016). Prior research on local minimum-wage policies has generally found little to no impact on employment, consistent with the bulk of the minimum-wage literature (Potter 2006; Dube, Naidu, Reich 2007; Schmitt and Rosnick 2011).

[3] The canonical proxy for the low-wage labor market is the restaurant industry, studied by Card and Krueger (1994); Dube, Lester, and Reich (2010, 2016); Addison, Blackburn, and Cotti (2012, 2014); Neumark, Salas, and Wascher (2014); Allegretto et al. (2017); and Totty (2017). Kim and Taylor (1995) and Addison, Blackburn, and Cotti (2008) focus on the retail industry. Card (1992); Neumark and Wascher (1994, 1995, 2004, 2008, 2011); Allegretto, Dube, and Reich (2011); and Neumark, Salas, and Wascher (2014) study teenage workers, effectively using worker age as a proxy for low-wage status. Exceptions to the tendency to use proxies for low-wage status include Linneman (1982); Currie and Fallick (1996); Neumark, Schweitzer, and Wascher (2004); Meer and West (2016); Gopalan et al. (2020); and Clemens and Wither (2019). One notable exception is 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 status 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).

[4] Many prior studies of the minimum wage use either aggregated or repeat-cross-sectional data that cannot distinguish among workers on different trajectories (e.g., Addison, Blackburn, and Cotti 2008; Allegretto et al. 2013; Card and Krueger 1994; Cengiz et al. 2018, 2019; Dube, Naidu, and Reich 2007; Dube, Lester, and Reich

When analyzing aggregate labor market trends, our main estimates rely on the synthetic control method, focusing primarily on jobs paying up to twice the initial minimum wage, adjusted for inflation.[5] When analyzing individual outcomes, we rely on a nearest-neighbor matching model linking low-wage Seattle workers to counterparts in outlying parts of the state. In both analyses, our results are vulnerable to the concern that Seattle's unprecedented labor market boom has no true analogue anywhere in Washington State. We argue that the likely bias imparted on our estimates differs across models, and thus they bound the true effect.

Our results indicate that Seattle's minimum-wage increases had significant positive effects on hourly wages among least-paid workers. Both aggregate and micro-level analyses suggest that these wage increases were accompanied by employment reductions on the intensive margin. Although our implied elasticity estimates are not tightly bound, the lower bound for the elasticity is more negative than in much prior literature. At the micro level, workers initially employed at low wages in Seattle show modest but statistically significant reductions in hours worked across all Washington jobs, even as they show no reduction in the probability of remaining employed. These hours effects are strongest in the calendar quarter immediately following a wage increase and dissipate over time, leading to a net gain in workers' earnings by \$10–12 per week by the end of the period studied. Analysis of aggregate data, while confirming a pattern of immediate hours reductions following wage increases, shows a pattern of delayed impacts on measures of headcount employment and no statistically significant impact on aggregate payroll. Aggregate data also reveal a slowdown in the rate of new entry into Seattle's low-wage labor market.

Together, these results suggest that Seattle employers responded to the minimum-wage increases in the short run by reducing hours per worker rather than headcount. As ordinary job separations occur in succeeding months, the probability that a departing worker is replaced declines, and continuing workers see some restoration of their work hours.

We show that less experienced workers suffered a larger proportionate reduction in hours compared to more experienced workers. We also show that the minimum-wage increase reduced both turnover and the rate of new worker entry into the low-wage labor market. These findings on differential impact of the minimum wage on less experienced versus more experienced workers suggest a means of reconciling the results of the aggregate and micro analyses and carry important implications

2010; Meer and West 2016; Neumark and Wascher 1995; Reich, Allegretto, and Godoey 2017). Among studies that do track workers over time, data limitations generally preclude parsing income effects into wage and hour impacts or analyzing effects in time intervals shorter than one year (Rinz and Voorheis 2018; Clemens and Wither 2019; Stewart 2004; Currie and Fallick 1996). The mixed aggregate and micro-level analysis in this paper parallels the methodology used by Yagan (2019) to study the impact of the Great Recession on employment trajectories.

[5] Minimum-wage increases could reduce the aggregate number of jobs in the market either by eliminating them or causing them to pay higher wages. We conduct sensitivity analyses to assess this problem but face the fundamental problem of not knowing which, among newly created jobs, would have paid below the threshold in the absence of a minimum-wage increase. The focus on jobs paying at or below a threshold wage bears some resemblance to Cengiz et al. (2018, 2019), 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. Because their analysis examines self-reported employment but not self-reported hours, its findings pertain only to the extensive margin.

for ongoing debates on the minimum wage. They suggest that the competing narratives outlined above are both true—that Seattle's minimum wage delivered higher earnings to workers with little evidence of a strong upward labor market trajectory, while reducing employment opportunities for those without experience.

We caution that these findings should be read as a story of what happened in Seattle in 2015 and 2016, rather than a more general story of what any local minimum-wage increase would cause. We similarly caution against generalizing to state or federal policy changes, in particular because mechanisms of response to a local minimum-wage ordinance—business relocation and outsourcing work to uncovered jurisdictions—are much costlier endeavors in response to a state or federal law.

I. Policy Context

In June 2014, the City of Seattle passed a minimum-wage ordinance mandating a phased increase to \$15 per hour, followed by inflation indexing.[6] Table 1 shows the statutory phase-in rate as a function of employer size, whether workers receive health benefits, and whether workers receive tips.[7] The minimum wage rose from the state's \$9.47 minimum to as high as \$11 on April 1, 2015 and again to as high as \$13 on January 1, 2016.[8] 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 percent increase. During this period, the minimum wage in Washington State remained stable at \$9.47 per hour, which makes the low-wage labor market in Washington State outside of Seattle a natural comparison group.[9]

Seattle implemented its groundbreaking minimum wage in the context of a robust local economic boom. Overall employment expanded nearly 15 percent in Seattle over the two years following the ordinance's passage. Identifying the impact of Seattle's minimum-wage increase requires us to select counterfactual regions with similar underlying labor market conditions. It is reasonable to be concerned that no such regions exist. Our analysis shows that several regions of Washington State match trends in Seattle's low-wage labor market between 2005 and 2014.

[6] At the time the ordinance was passed, \$15 was high in the distribution of hourly wages. During 2012–14, 42.4 percent of US workers earned less than this amount (Tung, Lathrop, and Sonn 2015).

[7] The ordinance considers a franchised business—independently owned but operated under contract with a parent company and reflecting the parent company brand—a large business so long as the sum of employment at all franchises worldwide exceeds 500. While in theory the differentiation of employers by size introduces an opportunity to identify impacts with differencing or even a regression-discontinuity design, Washington data do not reveal a business's global employee headcount nor its affiliation with a franchise network. Our data do not allow us to observe whether a worker receives health benefits, but we do observe total cash compensation, which should include tips so long as employers fully comply with reporting requirements. We come back to this issue in greater detail when we discuss the data.

[8] During the years we study (2005–2016), the State of Washington had a state-specific minimum wage that was indexed to CPI-W and was, on average, 30 percent 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.

[9] In November 2016, Washington State voters passed Initiative 1433, implementing an increase in the state's minimum wage to \$13.50 by 2020, with the first increase—to \$11—in 2017. This significant shock to the labor market in our control region complicates analysis of the effects of Seattle's minimum wage after 2016 and explains why we choose to conclude our analysis before the completion of the Seattle phase-in.

TABLE 1—MINIMUM WAGE SCHEDULE 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	$13.50	$13.00	$11.00
January 1, 2018	$15.45	$15.00	$14.00	$11.50
January 1, 2019	$16.00	$16.00	$15.00	$12.00
January 1, 2020	$16.39[d]	$16.39[d]	$15.75[e]	$13.50
January 1, 2021				$15.00[f]

[a] A large employer employs 501 or more employees worldwide, including all franchises associated with a common corporate parent or a network of franchises.
[b] Employers who pay toward medical benefits, provided the employee takes up the benefits and the sum of hourly compensation and imputed benefit value exceeds the minimum wage.
[c] Employers who pay toward medical benefits and/or employees who are paid tips. Total minimum hourly compensation (including tips and benefits taken up by the employee) must exceed the minimum wage.
[d] In subsequent years the minimum wage is indexed to inflation using the CPI-W for the Seattle-Tacoma-Bremerton Area.
[e] The minimum wage for small employers not providing tips or health benefits converges with the large-employer minimum on January 1, 2021.
[f] The minimum wage for small employers with benefits or tips will converge with other employers by 2025.

II. Data

We use administrative employment data from Washington State covering the period 2005 through the third quarter of 2016. Washington's Employment Security Department (ESD) collects quarterly payroll records for all workers who receive wages in Washington and are covered by unemployment insurance (UI).[10] Employers are required to report actual hours worked for employees paid by the hour and either actual hours worked or 40 times the number of weeks worked for salaried employees.

[10] Most studies that analyze employment responses to minimum-wage hikes in the United States rely on data from the Quarterly Census of Employment and Wages, which in turn relies on information from the same source as we do—payroll data on jobs covered by the UI program. The ESD data contain industry (NAICS) codes, which permit us to estimate results for the restaurant industry, as in much of the prior literature. For the aggregate analysis, we exclude services provided to private households—such as maids, nannies, and gardeners (NAICS code 814000)—and services for the elderly and disabled (NAICS code 624120), because in these industries, private households rather than businesses serve as employers. As a result, data for these industries are often inconsistently reported. 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. Our estimates may overstate actual reductions in employment opportunities if employers respond by shifting some jobs under the table or shifting work from employees to contractors.

The ESD collects this information from employers because eligibility for UI benefits in Washington is determined in part by an hours-worked test. Specifically, employees become eligible for benefits once they have worked a total of 680 hours for their employer.[11]

Beyond allowing us to study quarterly hours worked as an outcome, this dataset lets us measure the average hourly wage paid to each worker in each quarter by dividing total quarterly earnings by quarterly hours worked.[12] All wage rates and earnings are expressed in second-quarter-of-2015 dollars using the Consumer Price Index for Urban Wage Earners and Clerical Workers (CPI-W).

Employer reports of hours worked may not measure employee work effort perfectly. There is a concern, in particular, that increasing the minimum wage might increase the incentives for "wage theft"—requiring employees to begin work before a shift officially starts, to continue after it ends, or to work through a legally required rest period. While we have no means of detecting wage theft in our data, we note that the City of Seattle enacted a wage-theft ordinance imposing civil penalties beginning in 2015. The city's Office of Labor Standards reported opening 45 wage-theft investigations over a five-month period in 2015; this tally declined to 34 investigations for the same five months in 2016. Any increased incentives for employers to engage in wage theft following the 2016 minimum-wage increase may have been muted by the wage-theft ordinance.

The data identify business entities as UI account holders and include the mailing address of record for each account. To determine the exact location of each business, we geocode mailing addresses to exact latitude and longitude coordinates. We then use these coordinates to determine whether a business is located within the city

[11] 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 Current Population Survey (CPS) reveals some points of departure. 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 errors in CPS data. Given the statutory reporting requirement driven by benefits-determination provisions, ESD considers the hours data reliable. Minnesota, Oregon, and Rhode Island are the only other states that collect data on hours.

[12] 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 are occasionally paid in one quarter for work performed in another. For the aggregate analysis, we exclude job/quarter observations with calculated wages below \$9 in 2015 dollars and job/quarter observations with calculated wages above \$500 if reported hours were below 10. We also exclude job/quarter observations reporting over 1,000 hours worked. These restrictions exclude 6.7 percent of all job/quarter observations. Our micro-level analysis excludes those workers who had an apparently flawed calculated wage in the baseline or prior two quarters. Calculated wages are considered flawed if they were below \$8, above \$500 (if reported hours were below 10 in the quarter), missing due to the worker having observed earnings but zero reported hours during the quarter, or if the worker was reported as implausibly working more than 2,190 hours across all jobs in the quarter. These restrictions exclude 4.0 percent of workers.

The average hourly wage construct used here is not directly comparable to the self-reported hourly wage in the CPS, in which respondents are instructed to exclude overtime, commissions, or tips. Results obtained with our average hourly wage measure may differ from those based on 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. (2018) find that "wage distributions in the CPS and in the administrative data […] on hourly wages from three US states that collect this information (Minnesota, Washington, Oregon) […] are quite similar both in the cross section as well over time" (p. 3).

ESD requires employers to include all forms of monetary compensation, including tips, bonuses, and severance payments. As such, for tipped employees, we observe total hourly compensation—including tips—as long as employers have reported tipped income in full. ESD does not collect data on fringe benefits. To the extent firms cut back on benefits when required to pay a higher cash minimum wage, we underestimate effects on compensation.

of Seattle and to place businesses into Public Use Microdata Areas elsewhere in Washington State.[13]

Firms operating in multiple Washington locations have the option of establishing a separate ESD account for each location or reporting all employment to a single account. As such, we can uniquely identify business location only for single-site firms reporting valid addresses and those multisite firms opting for separate accounts by location.[14] Rather than include these firms with uninformative or potentially erroneous geocodes, we exclude them from the aggregate analysis.[15] Henceforth we refer to the remaining firms as "locatable." As shown in Table 2, in Washington State as a whole, locatable firms comprise 90 percent of firms and employ 62 percent of the entire workforce, and 63 percent of all employees are paid under \$19 per hour.[16]

Figure 1 illustrates that the rate of transition of longitudinally tracked individual workers from locatable to nonlocatable employment shows no substantive 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 nonlocatable sector.[17]

[13] We assume that each business employs workers at its mailing address. The ordinance applies to all work performed in the city, regardless of the employer's business location. For example, a restaurant located outside the city employing a delivery driver making a trip into the city is, in theory, required to pay the Seattle minimum wage for the duration of time spent inside the city limits. To the extent that our methods inaccurately identify work location, we expect our results to be affected by attenuation bias.

[14] Our aggregate analysis sample includes both independently owned businesses and franchises where the owner owns a single location, but it excludes corporations and restaurant and retail chains that own their branches and franchises whose owners operate multiple locations, unless these entities opt to establish separate ESD accounts by location.

[15] Nonlocatable businesses may exhibit differential responses to minimum-wage increases. To the extent that they are more likely to face the faster phase-in schedule for large businesses, basic economic theory suggests that multisite firms should reduce employment more than locatable firms that are included in the aggregate analysis. Note, however, that some multisite firms may employ fewer than 500 workers. In addition, larger firms are more likely to provide health benefits to their workers, and the ordinance establishes a lower minimum wage for firms that contribute toward health benefits. Note as well that some locatable firms may employ fewer than 500 workers but face the more rapid phase-in schedule because they are linked to a franchise network or have significant out-of-state employment.

This prediction could be altered to the extent that excluded firms exhibit a different labor demand elasticity relative to included ones. 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. Yet such firms might find it easier to adopt labor-saving technologies or to relocate work to their existing sites outside of the affected jurisdiction, implying a greater elasticity.

Survey evidence collected at the time of the first minimum-wage increase and again one year later suggests that multilocation firms were in fact more likely to plan and implement staff reductions. 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 (Romich et al. 2020). In April 2015, multisite employers were more likely to report intentions to reduce the hours of their minimum-wage employees (34 percent versus 24 percent) and more likely to report intentions to reduce employment (33 percent versus 26 percent). A one-year follow-up survey revealed that 52 percent of multilocation employers reported an actual reduction in full-time and part-time employees, compared to 45 percent for single-site firms.

[16] Online Appendix Table A1 shows that the proportion of low-paid (under \$19 per hour) employees included in the aggregate analysis falls close to the 63 percent 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.

[17] 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 nonlocatable sector anywhere in Washington State one year later.

TABLE 2—CHARACTERISTICS OF FIRMS AND ESTABLISHMENTS INCLUDED IN AND EXCLUDED FROM THE AGGREGATE ANALYSIS

		Excluded from analysis			
Characteristic	Included in analysis	Nonlocatable multisite businesses	Nonlocatable single-site businesses	Total	Share included
Number of firms	123,132	1,345	12,277	13,622	90.04 percent
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 percent
Number of employees paid < \$19 per hour	715,808	325,320	87,395	412,715	63.43 percent
Employees/firm	13	279	19	58	
Standard deviation of employees/firm	160	1,610	328	706	
Employees/establishment	13	Unknown	19	Unknown	
Standard deviation 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:I and 2016:III. "Excluded from Analysis" includes two categories of firms: multilocation firms (flagged as such in UI data) and single-location firms (which operate statewide or whose physical location could not be determined).

Source: UI records from WA

FIGURE 1. RATES OF TRANSITION FROM LOCATABLE TO NONLOCATABLE EMPLOYMENT
P(NONLOCATABLE JOB IN t | LOCATABLE AND PAID UNDER \$19 PER HOUR IN $t-4$, EMPLOYED IN WA IN t)

Notes: Nonlocatable jobs are defined as those in a nonlocatable business anywhere in Washington State. Hourly wages are inflation-adjusted to the second quarter of 2015 using the CPI-W. A worker's initial location in Seattle or surrounding counties is determined by the location of the worker's employment in $t-4$. These workers are then tracked longitudinally to t.

Source: UI records from WA

III. Methodology: Aggregate Analysis

Our aggregate analysis considers whether the overall level of employment in Seattle's low-wage labor market declined because of the ordinance. As our data identify hours worked and hourly wage rates, the aggregate analysis considers both traditional headcount measures of employment and aggregate hours worked, defining the low-wage labor market as jobs paying under a specific hourly threshold, for example, $19 per hour.

A. *Outcomes*

We evaluate the impact of the ordinance on four outcomes: the number of beginning-of-quarter jobs (headcount) in low-wage jobs in Seattle, the number of hours worked during a quarter for low wages in Seattle, the average wage paid to low-wage jobs in Seattle, and the total amount paid to low-wage workers in Seattle (i.e., payroll).[18] We define low-wage workers' average wage as the average hourly wages paid to low-wage workers weighted by their hours worked in a quarter.

We also use the methods described in this section to study patterns of new entry into Seattle's low-wage labor market. We define new entrants as workers who appear in the ESD data after at least 20 quarters, or 5 years, when they were not observed. We use this definition rather than attributing the first appearance of a worker in the data to new entry because we have no data prior to the first quarter of 2005. Absent this restriction, a new entrant in 2006 would be selected very differently from one in 2016.[19]

B. *Defining the Low-Wage Labor Market*

Segmenting the low-wage labor market as jobs paying below a certain threshold creates concerns that analysis of a minimum-wage increase might misclassify jobs as lost when in fact their wages were raised above the threshold. This concern is particularly relevant given evidence of cascading impacts of minimum-wage increases on slightly higher-paying jobs (Neumark, Schweitzer, and Wascher 2004; Autor, Manning, and Smith 2016; Brochu et al. 2018). Cascades can be caused by employers seeking to maintain differentials between the wages paid to their least-skilled workers and those paid to workers with higher skill or experience. However, if we set the threshold for low-skilled employment too high to avoid this issue, we may understate proportional employment and wage effects, as effects on relevant jobs will be diluted by the inclusion of irrelevant jobs. Imagining a reaction function linking initial wages to post-increase wages, we aim to identify a fixed point that partitions the labor market into segments affected and unaffected by the minimum wage.

[18] Because the data provide information about all individuals on payroll during a quarter, including those working only for a few weeks or even days, we follow prior studies in tallying beginning-of-quarter jobs, defined as person-employer matches that existed in both the current and previous quarter. This definition is used by the Quarterly Workforce Indicators, based on the Longitudinal Employer Household Data, and produces the total number of jobs comparable to the employment counts in the Quarterly Census of Employment and Wages. The hours-worked measure includes all employment, regardless of whether a person-employer match persists for more than one quarter.

[19] Although our analysis makes use of data for the period 2005–2016, we have access to individual employment data beginning in 2000, allowing us to define the "new worker" variable consistently for the full time period. While individual workers can be traced back into the pre-2005 data, firms cannot.

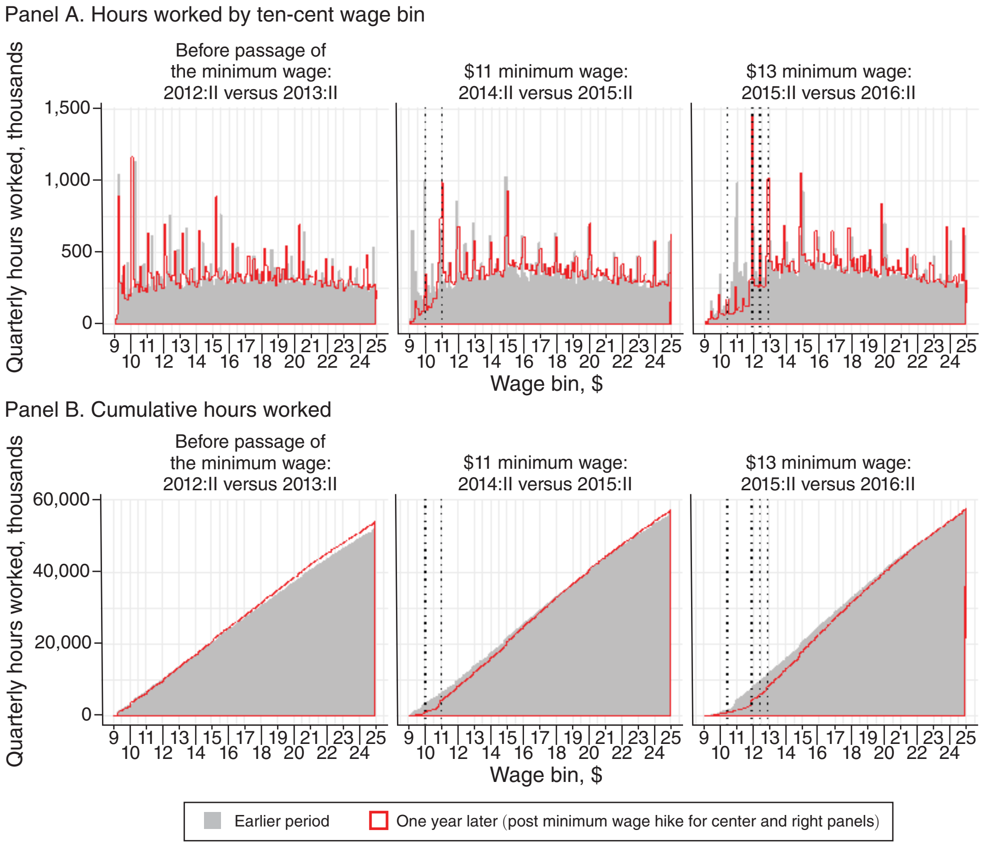

Figure 2. Changes in the Distribution of Quarterly Hours Worked in Seattle

Notes: Sample: Workers at single-location firms. Wages have been adjusted for inflation using CPI-W. Dotted lines show the minimum-wage schedules. Jobs which pay less than the minimum wage likely correspond to trainees, teenage workers, and workers with disability who can be paid only 85 percent of the minimum wage. In addition, some of these observations occur due to measurement errors in hours.

Source: UI records from WA

Figure 2 presents plots of the wage distribution intended to identify potential fixed points. Panel A shows histograms of quarterly hours worked across ten-cent-wide wage bins, up to $24.90–25.00 per hour. Panel B shows the corresponding cumulative plots. We begin on the left with an informal falsification test by comparing the wage distribution, weighted by hours, at two snapshots before the actual increase in Seattle's minimum wage. We then introduce identical plots comparing 2014:II versus 2015:II (center column), showing the changes following the $11 minimum wage; and comparing 2015:II versus 2016:II (right column), showing the changes following the $13 minimum wage.

The left side of panel A shows very minor shifts in the wage distribution during the year before the passage of the ordinance. The distribution shows a spike at the state's inflation-adjusted minimum wage; this spike shrinks slightly over the year.

The remainder of the histogram shows spikes corresponding to round values of hourly wages. These spikes drift slightly to the left, indicating the erosion in value of nominal-dollar wages due to inflation. The cumulative hours plot in panel B shows evidence of modest growth in hours worked, most evident at the higher end of the distribution, above $15 per hour. If this plot revealed more 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 or conclude that market-driven wage increases were likely to swamp any effects of the ordinance.

The plots in the middle and right side of panel A show clear, direct impacts of Seattle's two minimum-wage increases. In each case, spikes below the new minimum-wage values shrink dramatically while spikes at the ordinance's minima increase. In the right-hand panel, the largest spike is observed at $12, corresponding to a larger volume of hours in our sample at the lowest minimum wage imposed on small businesses compared to larger businesses, which had a $13 minimum wage. Additionally, Panel B shows 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.

To select a threshold for the low-wage labor market segment, we are looking for a wage level above which there is no evidence of growing spikes in the wage distribution. These graphs provide little evidence of cascading wage impacts above the $11 level in the middle panel or above the $15 level in the right panel.[20] The figures suggest no abnormal increases in the number of hours worked at wages in the high teens or low twenties. If anything, growth in the number of positions paying between $15 and $25 looks anemic compared to the 2012–13 time period.

We select a preliminary, conservative threshold of $19 per hour as a starting point for our analysis, which is almost exactly twice the baseline minimum wage and $6 above the top statutory minimum in the period under study. Beyond this $19 per hour threshold, cascading effects are less likely to occur (Brochu et al. 2018; Neumark, Schweitzer, and Wascher 2004). In online Appendix A, we test sensitivity to this choice by evaluating impacts up to a $25 per hour threshold.

Use of any fixed threshold to define the low-wage labor market is problematic if unrelated labor market trends are shifting equilibrium wages relative to this threshold. The left-hand panels of Figure 2 suggest that such a pattern may have been underway before the minimum wage increased. Our analysis rests on two strategies for addressing this threat. The first strategy compares Seattle to other geographic regions with similar labor market trends in the period leading up to the minimum-wage increase. The second strategy emphasizes the timing: minimum-wage increases occur as discrete events rather than gradual shifts, while underlying trends are gradual.

[20] Although the $15 minimum wage was not required 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. 2020).

C. *Causal Identification Strategy*

Our identification strategy uses two methods allowing 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).[21]

Both methods assume that changes in an aggregate outcome Y_{rt} in each region r for quarter t can be represented as a function of K unobserved linear factors plus the treatment effect:

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

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. In equation (1), $q = 1$ corresponds to the third quarter of 2014, the first quarter after the ordinance had been passed; $q = 4$ corresponds to the second quarter of 2015, when the first phase-in of the ordinance occurred; $q = 7$ corresponds to the first quarter of 2016, when the second phase-in occurred; and $q = 9$ corresponds to the third quarter of 2016, the last period included in our analysis. Note that we normalize time such that the Minimum Wage Ordinance is passed at $t = 0$, which implies that our first quarter of observed data corresponds to $t = -32$.

The unobserved factors can be thought of as common economic shocks that affect all regions at the same time, such as an exchange rate shock, changes in weather, or a common demand shock. Because the regions are allowed to have different sensitivities to these shocks, the treated and control regions are no longer required to have parallel trends. Rather, the weighted average of the control regions supplants as a "counterfactual" region.

Our analysis shows effects on levels of Y and year-over-year percentage changes in Y (i.e., $\Delta Y_{rt} = (Y_{rt} - Y_{r,t-4})/Y_{r,t-4})$), which differences out seasonal fluctuations. Both outcomes present similar results.[22]

Though both the synthetic control and interactive fixed effects estimators have the same underlying model, they are implemented quite differently. 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 $r = 2, \ldots, R$ all potential control regions. Then the weights for

[21] Both have been used in the regional policy evaluation literature and applied to the minimum wage. (See Allegretto et al. [2013] for an application of synthetic control, and Totty [2017] for an application of interactive fixed effects.) For a more thorough discussion of causal inference techniques in existing literature, see online Appendix B.

[22] To facilitate comparison between the "Levels" and "Growth Rates" results, we divide the "Levels" coefficients by the outcome's level in the baseline quarter, 2014:II. Additionally, for all outcomes except for mean wages the level is further divided by five as Seattle, which contains five PUMAs, is approximately five times the size of the prospective control regions. We additionally estimate effects on the natural log of Y and standardized Y (i.e., the level of each outcome minus its pre-policy mean divided by its per policy standard deviation within each region), and these outcomes produced qualitatively similar results.

the synthetic control can be found by minimizing forecasting error in the pre-policy period,

$$\min_{w_r} \sum_{t=-32}^{0} \left(Y_{r=1,t} - \sum_{r=2}^{R} w_r Y_{rt} \right)^2, \tag{2}$$

subject to the constraints $\sum_r w_r = 1$ and $\forall r \, w_r \geq 0$.[23, 24] Given a set of weights $\hat{w}_r$, the impact of the ordinance in quarter q is estimated as follows:

$$\beta_q^{Synth} = Y_{r=1,q} - \sum_{r=2}^{R} \hat{w}_r Y_{rq}. \tag{3}$$

We allow weights across regions to vary by outcome in order to improve the quality of the match in the pre-policy period.[25] As a robustness check, we create a single weight for region r to be used across all four outcomes. We construct this common weight by first standardizing each of the outcomes, taking the average across standardized outcomes, then applying the same procedure described above for the outcome-specific weights, i.e., solving equation (2) with the averaged outcome.

The interactive fixed effects approach estimates region fixed effects, time fixed effects, and the factors and factor loadings in (1) explicitly, by imposing normalization on the sum of the factors. Since the number of unobserved factors is not known, we allow for up to 30 of them and pick the model with the optimal number of factors using the criterion in Bai and Ng (2002).[26] We implement the interactive fixed effects estimator following Gobillon and Magnac (2016), who developed a publicly available program to estimate treatment effects in the regional policy evaluation context.

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

[24] Note that our approach to identifying the best set of weights, w_r, relies solely on the pre-policy time series of the dependent variable. An alternative approach in some of the literature using synthetic control methods is to include other characteristics of the region that predict Y_{rt}. For example, Abadie, Diamond, and Hainmueller (2010) use "average retail price of cigarettes, per capita state personal income (logged), the percentage of the population age 15–24, and per capita beer consumption" (p. 499) averaged over the period 1980 to 1988 and per capita cigarette sales in three pre-policy years (1975, 1980, and 1988) as predictors of state-level per capita cigarette sales during the pre-policy period of 1970 to 1988. We have not taken this approach because we lack a set of variables that are measured quarterly at the PUMA level and are likely to be strong predictors of Y_{rt}—for example, the unemployment rate (which is reported at the core-based statistical area or county level, but not available at the lower granularity) or the demographic composition of the region PUMAs (which is reported at annual frequency in the American Community Survey).

[25] Online Appendix Figure A1 shows that the set of regions in Washington that receive a positive weight in the synthetic control estimator is very similar for employment outcomes and payroll but somewhat different for wage rates. 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 suggests 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 the expansion of local economies. The strongest resemblance to Seattle in terms of wages, by contrast, tends to be in closer suburban areas, including the satellite centers of Tacoma and Everett.

[26] 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. Online Appendix Figure A2 shows the sensitivity of the interactive fixed effects estimates as a function of the number of factors and the choice of the optimal number of factors.

D. *Geography*

We implement both estimators using data on employment trends across Public Use Microdata Areas (PUMAs) in Washington State. A PUMA is a geographic unit defined by the United States Census Bureau with a population of approximately 100,000 people.[27] We exclude King County PUMAs outside of Seattle from the analysis to avoid potential spillover effects. The remainder of Washington includes 40 PUMAs, while Seattle is composed of 5.[28] 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 Y_{rt} for 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 level of Y, treating it as one unit.[29]

E. *Inference*

We calculate standard errors for the interactive fixed effects coefficients based on the assumption of independent and identically distributed idiosyncratic shocks ε_{rt} and 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.

Because the synthetic control method does not yield conventional standard error estimates, inference is based on placebo-in-space permutations, as is customary in the literature (Abadie, Diamond, and Hainmueller 2015; Firpo and Possebom 2018). We estimate the effect of placebo treatments introduced in the quarter when the ordinance was passed, in all possible combinations of five contiguous PUMAs in Washington State excluding King County.[30] This placebo exercise gives us the distribution of coefficients 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 that were larger in absolute value than the estimated effect in Seattle, i.e., $p(\hat{\beta}_q) = (1/J)\sum_j 1\{|\hat{\beta}_{j,q}| \geq |\hat{\beta}_q|\}$, where j indexes the possible combinations of five contiguous PUMAs, $\hat{\beta}_q$ is the

[27] In principle, we could use different geographic units such as counties, which are typically larger than PUMAs, or census tracts, which are much smaller. We chose 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 than tracts to be affected by idiosyncratic shocks.

[28] For a map of Washington State PUMAs, see online Appendix Figure A3.

[29] When we evaluate year-over-year percentage changes in Y to compute the cumulative effect of the ordinance on each outcome from the baseline quarter, we convert the 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, $\beta_9^{cum} = (1+\beta_9)(1+\beta_5)(1+\beta_1) - 1$. We present all results in terms of cumulative changes and adjust the standard errors accordingly using the delta method. Since our estimate of β_9^{cum} is 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., $\beta_1^{cum}, \ldots, \beta_8^{cum}$).

[30] Since Seattle spans five PUMAs, our placebo treatment region replicates Seattle's size. We require that the five PUMAs randomly selected as placebo treated PUMAs be contiguous, which replicates the contiguous nature of Seattle and thus accounts for the possibility of common regional shocks.

estimated treatment effect in Seattle, and $\hat{\beta}_{j,q}$ is the estimated placebo effect in region j. There are 2,994 possible combinations of five contiguous PUMAs in Washington State outside of King County, so the smallest possible p-value for each coefficient is $1/2{,}994 = 0.0003$.[31,32]

Finally, we calculate confidence intervals for the estimates, which we obtain by inverting the test statistic (Imbens and Rubin 2015). For each estimated coefficient, we calculate the range of estimated effects that cannot be rejected at the 5 percent significance level; i.e., we find β_q^* such that $\left|\hat{\beta}_q - \beta_q^*\right| < |\beta|_{0.95,q}$, where $|\beta|_{0.95,q}$ is the ninety-fifth percentile of the absolute values of the placebo estimates.[33] We compute 90 percent and 50 percent confidence intervals analogously. In our presentation of the results, we present $\hat{\beta}_q$ and $p\left(\hat{\beta}_q\right)$ in tables and show confidence intervals in figures.

F. *Examining the Synthetic Control Match*

Figure 3 plots the time series of 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 method. In each panel, trends in Seattle and the control region track closely through 2014. As shown in Panel A, the difference between wages in Seattle and in the weighted average of PUMAs chosen by the synthetic control method is below \$0.13 in all quarters prior to the Ordinance's passage. Employment trends (panels B and C for jobs and hours, respectively) and payroll (panel D) likewise match closely, with discrepancies below 3 percent except in the period around the Great Recession, where the control region appears to enter the slump slightly before Seattle. The quality of the match between treatment and control regions thus appears high.[34]

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

[31] Abadie, Diamond, and Hainmueller (2010) report the p-value based on the same procedure that we use, while Abadie, Diamond, and Hainmueller (2015) and Firpo and Possebom (2018) 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 calculated the p-values using their method, as well. Conclusions about statistical significance based on these two procedures are very similar.

[32] During the pre-policy period, Seattle was subject to more year-over-year wage growth stability than other portions of the state. Specifically, Seattle would lie at the fifth percentile of the distribution of wage shocks among the set of five contiguous PUMA groupings. Since Seattle experiences smaller wage shocks, our inference procedure will produce overly conservative standard errors. (We thank an anonymous referee for this insight). For other outcomes (hours, jobs, and payroll), Seattle's outcome shocks lie closer to the median among these contiguous PUMA groupings.

[33] Because we have 2,994 possible combinations of the contiguous PUMAs, we are able to use a 95.02338 percent confidence level for our estimates. (Due to the finite number of possible placebo combinations, we can calculate confidence interval in increments of $1/2{,}994$, or 0.0334 percent.)

[34] Online Appendix Figure A4 repeats this analysis with separate trend lines for each PUMA in Washington outside of King County. Since Seattle contains five PUMAs, we 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. Online Appendix Figures A5 and A6 repeat this analysis for year-over-year percentage changes of each outcome rather than levels. Online Appendix Figure A6 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. As noted above, this implies that the year-over-year change models will offer an improvement of pre-policy fit relative to the level models.

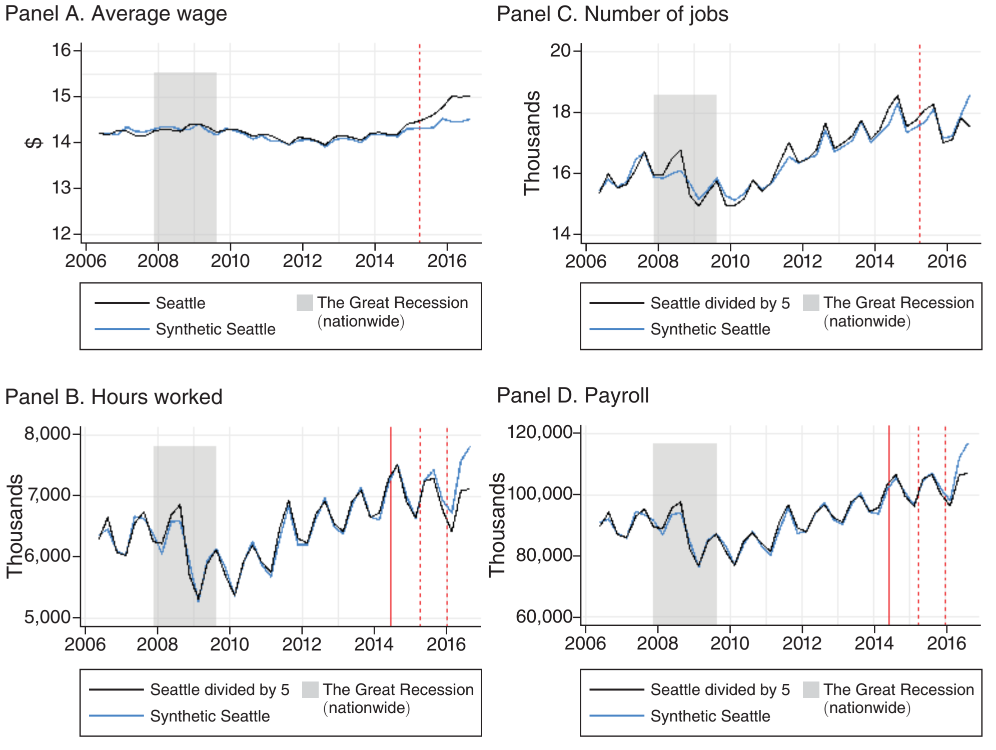

Figure 3. Levels of Employment, Wages, and Payroll in Seattle Compared to Synthetic Seattle in Jobs Paying Less than $19 per Hour

Notes: Sample: Workers at locatable firms. Wages have been adjusted for inflation using CPI-W. Estimates for all jobs paying < $19 in all industries. We implement the synthetic control estimator using the R programs provided by Gobillon and Magnac (2016). Outcomes are measured in levels.

Source: UI records from WA

range observed in the pre-treatment period. Each of these figures show that while wages rose faster in Seattle than most of the other PUMAs post-ordinance, Seattle experienced nearly the largest declines in hours and jobs and its payroll lagged.

G. *Assessing a Key Threat to Validity: Seattle's Boom*

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. The cumulative density functions in Figure 2 do not generally support the rightward-shift hypothesis. We note, moreover, that rightward shift of the wage distribution is unlikely to appear suddenly in our data, particularly in the winter quarter, which is a natural seasonal trough in Seattle's labor market.

We examine the issue more closely in two ways. First, we apply our synthetic control method to evaluate whether there appear to be impacts on wages and hours above $19 per hour. Second, we conduct a 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 the first quarter of 2005 to the third quarter of 2014. As we show below, our synthetic control and interactive fixed effects specifications pass falsification tests.

IV. Methodology: Micro-Level Analysis

The key limitation of our aggregate analysis can be illustrated with a simple example. Suppose an employee begins work in the baseline period, earning a wage slightly below the threshold for low-wage employment, and subsequently earns a modest raise to a wage level above the threshold. The aggregate analysis would consider this a lost job when clearly it is not. This limitation could be circumvented by beginning with a sample of low-wage earners and tracking them longitudinally, regardless of the evolution of their hourly wage.

Were an exogenous shock to the City of Seattle to systematically push workers above the threshold at a faster rate than those in a comparison region, our aggregate methodology would return a negative coefficient. A longitudinal analysis, by contrast, would return a positive estimate. In the presence of such shocks, these two techniques may bound the true impact of any contemporaneously implemented policies. Longitudinal analysis also allows us to study effect heterogeneity in a manner not supported in the aggregate analysis.

To pursue this second analytic technique, we must first choose the criteria for determining whether a worker was exposed to the Seattle minimum-wage increases. We then require a strategy for estimating the counterfactual for these treated workers.

A. *Outcomes*

We focus on four outcomes analogous to those in the aggregate analysis: hourly wages, probability of continued employment, hours worked, and earnings. Individuals who do not appear in Washington UI records for a given quarter are excluded from the hourly wage analysis but coded as not employed, working zero hours, and earning zero dollars. To parse the findings on employment and hours, we also examine the probability that workers remain employed by their primary baseline employer, job turnover, and hours worked outside the city of Seattle.

B. *Defining Treated and Control Workers*

Seattle's minimum wage is imposed on employers rather than workers, which creates a challenge for assigning treatment status to individual workers. The ordinance covers work done within the city's boundaries, defined by the physical location of the employer or by the workplace, if the work is done outside of the employer's premises.

In theory, the set of workers directly impacted by the ordinance would consist of those individuals who would have been hired at a rate below the local minimum wage if the policy had not been adopted. This set of individuals is not observable in our data. We can reasonably approximate it by selecting the individuals working in the city for hourly wages below the minimum wage immediately before the ordinance took effect, but this raises a series of concerns. The set of individuals working for low wages may be a selected sample if employers raised wages in anticipation of policy implementation. The set of individuals working for low wages in location *i* at time *t* will generally differ from those who would be working for low wages in the same location at later times for any of several reasons: attrition from the labor market, maturation to higher wage levels, mean reversion following negative shocks (Ashenfelter 1978), transition to a form of employment not captured in ESD data, or transition to employment in other locations.[35]

In Section V, we assess whether wages appear to increase in anticipation of the first statutory increase in April 2015. We will conclude that any anticipatory effects are small. As such, our analysis focuses on a set of treated individuals observed working for wages below \$11 exclusively in locatable Seattle businesses in the first calendar quarter of 2015.[36] In logging their post-increase employment outcomes, we count all employment, whether in Seattle, outside the city, or geographically unlocatable in Washington State. These workers have spent an average of 5.5 quarters working for their current employer and 20.4 quarters in the state's labor force.[37] They average fewer than 20 work hours per week, with only 7 percent of the sample reaching the hours threshold corresponding to full-time work for the entire quarter (520 hours).

Our control group should, in theory, consist of a set of workers who were not exposed to the Seattle minimum wage but otherwise faced similar labor market conditions. We defined as potential control group workers in Washington State who received all of their earnings from locatable employers outside of King County in the relevant baseline quarter.[38]

[35] Theory and evidence suggest that we should expect the lowest-earning employees at any point in time to experience wage increases (Murphy and Welch 1990; Smith and Vavrichek 1992; Long 1999; Carrington and Fallick 2001; Even and Macpherson 2003). Washington employees outside King County earning under \$11 per hour in the first quarter of 2015 earned an average raise of \$1.76 conditional on continued employment by the fourth quarter, even though the statutory minimum remained the same (Jardim et al. 2018). This mean is skewed by positive outliers; most workers earning under \$11 continued to earn under \$11 at the conclusion of the calendar year. Some portion of the mean increase may also reflect selective attrition by lower-earning workers. Note that these statistics are not directly comparable to those in Smith and Vavrichek (1992) and Long (1999), as in those analyses only workers earning exactly the minimum wage are studied. Mean reversion following negative shocks is also supported in the data, which show a higher mean hourly wage in the fourth quarter of 2014 relative to the first quarter of 2015. Given our definition of hourly wages as the ratio of quarterly earnings to quarterly hours, a reduction in overtime hours or bonus pay would appear as a negative shock to hourly wages.

[36] Use of a baseline employment date earlier than the first quarter of 2015 introduces concerns related to the transient nature of much low-wage work. A high percentage of low-wage employment spells observed in the third or fourth calendar quarters are seasonal, tied to either Seattle's summer tourism economy or holiday season retail. The earlier the start date, the lower the percentage of studied workers who could reasonably be expected to still be employed for low wages in Seattle after April 1, 2015. In our working paper (Jardim et al. 2018), we report results for a second cohort of workers, i.e., those who had 100 percent of their baseline quarter (2015:IV) employment in locatable firms in Seattle and earned at least \$8 but less than \$13 per hour in that quarter. We find similar results for this cohort.

[37] Note that both the duration of current spell and time since entry variables are truncated at 41 quarters, implying that these measures understate the true means. The mean time since entry masks considerable variation in spell duration. About one-third of low-wage job spells initiating in a given quarter will persist four quarters; a sixth will last eight quarters.

[38] We drop workers employed at baseline by an employer in King County outside of Seattle, as these workers are at heightened risk of transitioning into the Seattle labor market after April 1, 2015. They may also be affected by policy spillovers.

Table 3—Number of Workers Included in and Excluded from the Trajectories Analysis

Sample	Number of workers
Workers earning less than \$11 per hour in baseline quarter (2014:I)	367,312
Excluded from analysis because of baseline employment at:	
Nonlocatable multisite employer	128,201
Nonlocatable single-site employer	18,313
Employers not solely in Seattle nor solely outside of King County	42,205
Included in analysis (treated + pool of potential control workers)	178,593

Note: The number excluded reflects those excluded after dropping workers for conditions shown in prior rows.

Table 3 describes restrictions we place on the sample. First, we exclude workers whose baseline employment was nonlocatable because they worked in a multisite, single-account business or a business without a valid address. These establishments employed 40 percent of low-wage workers. We also exclude workers employed at baseline by both a Seattle employer and an employer outside of Seattle. Finally, we exclude workers reporting any hours worked in King County outside Seattle in the baseline quarter.

As our set of treated individuals may not continue to work for low wages in Seattle after the baseline period and control workers may transition into Seattle employment, our analysis can be thought of in an "intent to treat" (ITT) framework. Just as null estimated ITT effects can be the result of widespread noncompliance rather than an ineffective treatment in a randomized trial setting, null effects in this analysis could result if the "dosage" of exposure to low-wage employment in Seattle shows little difference between the treated and control groups.

C. *Causal Inference Strategy*

We use a combination of matching and difference-in-differences methods to identify causal effects.[39] Rather than using the entire analysis sample of non-Seattle workers as controls, we apply a nearest-neighbor matching strategy to minimize observed treatment-control differences in baseline characteristics. Matching methods are often criticized on the grounds that narrowing observable differences between treated and control observations can actually exacerbate unobserved differences. These concerns are amplified in scenarios where individuals faced a personal choice regarding whether to obtain the treatment. In this case, selecting control observations with no employment in King County during a baseline period before the treatment was implemented mitigates the concern. As

[39] Difference-in-differences matching estimators are introduced and discussed in Heckman, Ichimura, and Todd (1997); Heckman et al. (1998); Smith and Todd (2005); and Stuart et al. (2014).

we discuss below, to address concerns regarding residual mismatch on unobservables, we difference results between a treated and untreated cohort.

For each treated worker, we identify the nearest neighbor from the pool of potential control workers without replacement.[40] We match exactly workers' employment status and whether they were first observed in Washington State data in the three quarters prior to each minimum-wage hike: the baseline quarter as well as the two prior quarters. We continuously match workers on quarterly hours worked in all jobs in the baseline quarter and each of the two prior quarters, hourly wages (conditional on employment) in these quarters, having earnings from more than one employer in a quarter (conditional on employment), the number of quarters a worker has been linked to their current primary employer, and the number of quarters since the worker first appeared in Washington State data.[41] We use Mahalanobis (1936) distance, D_{ij}, to measure the distance between individual i and individual j, defined as

$$D_{ij} = (X_i - X_j)'\Sigma^{-1}(X_i - X_j), \tag{4}$$

where Σ is the sample-covariance matrix of the covariates, X, in the pool of potential control workers.

Table 4 compares pre-policy covariates for treated workers, the pool of potential control workers (i.e., all workers in locatable low-wage jobs in Washington employed outside of King County at baseline), and the control workers chosen as nearest neighbors. As a measure of balance, we present the normalized differences in covariates between treated and control workers. Even prior to matching, normalized differences between the treated workers and the pool of potential control workers are not typically large and have a mean absolute value of 0.08. Seattle's workers in low-wage jobs earned higher hourly wages than potential control workers, yet they tended to work fewer hours at baseline and in the quarters before baseline and had a lower probability of having earnings from multiple employers than potential control workers.

After matching, most normalized differences disappear or become barely perceptible. By construction we achieve perfect balance on the variables used for exact matching. We achieve near-perfect balance for discrete variables, for which we potentially allow imperfect matches.

Match quality is somewhat worse for pre-baseline hourly wages, both before and after matching. Though there are virtually no wage differences between Seattle workers and the matched controls in the first quarter of 2015, there are some modest discrepancies in wages in the two quarters before baseline; Seattle workers were paid 25 and 48 cents more per hour than their matched Washington State counterparts in the last two quarters of 2014. The "Ashenfelter dips" exhibited by Seattle

[40] Abadie and Spiess (2021) recommend nearest neighbor without replacement so as to derive valid standard errors. Alternate models using nearest-neighbor matching with replacement and one to four matches yield point estimates similar to those presented here. There are tradeoffs in the choice of the number of matches. While increasing the number of neighbors allows for a more stable control group and reduces the variance of the estimates, it comes at the expense of allowing lower-quality matches into the sample.

[41] These duration measures are left-censored for workers whose employment history extends before 2005.

TABLE 4—BALANCE BETWEEN TREATED WORKERS, POTENTIAL CONTROL WORKERS, AND MATCHED CONTROL WORKERS IN THE TRAJECTORIES ANALYSIS

	Treated workers		Pool of potential control workers			Matched control workers		
	Mean	(SD)	Mean	(SD)	Normalized diff. from treated	Mean	(SD)	Normalized diff. from treated
Variables matched exactly:								
Employed in 2015:I	1.00		1.00		NA	1.00		NA
Employed in 2014:IV	0.79		0.83		−0.10	0.79		0.00
Employed in 2014:III	0.72		0.76		−0.10	0.72		0.00
New entrant in 2015:I	0.08		0.07		0.06	0.08		0.00
New entrant in 2014:IV	0.05		0.05		0.03	0.05		0.00
New entrant in 2014:III	0.05		0.05		0.01	0.05		0.00
Variables matched non-exactly:								
Job tenure at baseline (2015:I) employer	5.6	(8.0)	5.6	(7.8)	0.00	5.5	(8.0)	0.01
Potential experience at baseline (2015:I)	20.4	(15.4)	21.4	(15.3)	−0.07	20.4	(15.5)	0.00
Hours worked in 2015:I	240	(199)	261	(179)	−0.12	237	(191)	0.01
Hours worked in 2014:IV	227	(207)	255	(205)	−0.14	225	(202)	0.01
Hours worked in 2014:III	216	(211)	251	(227)	−0.16	214	(207)	0.01
Wage in 2015:I	\$10.06	(\$0.51)	\$10.01	(\$0.47)	0.11	\$10.06	(\$0.50)	0.00
Wage in 2014:IV (conditional on employment)	\$11.09	(\$6.24)	\$10.85	(\$6.68)	0.04	\$10.84	(\$5.64)	0.03
Wage in 2014:III (conditional on employment)	\$11.46	(\$12.48)	\$11.19	(\$11.72)	0.02	\$10.98	(\$11.45)	0.03
Earnings from >1 employer in 2015:I	0.048		0.084		−0.13	0.048		0.00
Earnings from >1 one employer in 2014:IV	0.112		0.131		−0.05	0.112		0.00
Earnings from >1 one employer in 2014:III	0.116		0.155		−0.11	0.116		0.00
Mean of absolute values					0.08			0.01
Observations	14,409		164,184			14,409		

workers appear slightly steeper than those seen elsewhere in Washington. These differences amount to 0.03 standard deviations, which is small in an absolute sense.

The basic causal estimate we present is the difference between the mean outcomes of treatment and control workers in quarter q following an increase in Seattle's minimum wage (with q ranging from 1 to 6). This difference can be represented as follows:

$$\frac{1}{N_{Treated}} \sum_{i=1}^{N_{Treated}} \left[Y_{iq}\right] - \frac{1}{N_{Treated}} \sum_{i=N_{Treated}+1}^{2N_{Treated}} \left[Y_{iq}\right], \tag{5}$$

with the observations sorted by treatment status such that treated observations are indexed from $i = 1$ to $i = N_{Treated}$ and their matched control observations are indexed from $i = N_{Treated} + 1$ to $i = 2N_{Treated}$.

Because we match on several continuous covariates, the matching estimator that compares each observation to its neighbors may be biased (Abadie and Imbens 2011). We follow Abadie and Imbens (2011) and implement bias correction by running a regression of the outcome of interest on the continuous covariates using the sample of the treated observations to obtain $\hat{\beta}_1$ and repeating with the sample of control observations to obtain $\hat{\beta}_0$. We then compute the bias-corrected difference between the mean outcomes of treatment and control workers in quarter q as follows:

$$(6)\quad \frac{1}{N_{Treated}}\sum_{i=1}^{N_{Treated}}\left[Y_{iq}+\left(X_i\hat{\beta}_0-X_{i+N_{Treated}}\hat{\beta}_0\right)\right] - \frac{1}{N_{Treated}}\sum_{i=N_{Treated}+1}^{2N_{Treated}}\left[Y_{iq}+\left(X_i\hat{\beta}_1-X_{i-N_{Treated}}\hat{\beta}_1\right)\right].$$

We refine this basic causal estimate by taking a second difference between the bias-corrected difference for quarter q (after the minimum-wage hike) and the bias-corrected difference for the baseline quarter, $q = 0$ (i.e., the quarter before the minimum-wage hike).

While our procedure ensures that treated and control workers match closely on pre-treatment characteristics, control workers may face very different local labor market conditions. They may also differ on unobserved dimensions. In particular, the concentration of college students in the low-wage workforce may be larger in Seattle relative to outlying Washington State given the relative concentration of higher education institutions in the city.[42]

To address this concern, we refine our estimates by estimating the effect of a pseudo-minimum-wage ordinance on pre-policy data. We begin by drawing a sample of workers observed in the first quarter of 2012, defining pseudo-treated workers as those who earn less than \$11 per hour (in 2015:II dollars) and who have 100 percent of their earnings in locatable firms in Seattle.[43] We match these workers to pseudo-control workers drawn from Washington State outside King County in the same time period. We then follow them for six quarters after a pseudo-ordinance, 2012:II–2013:III, estimating a twice-differenced effect for this pseudo-cohort using the exact same methodology as described above.

Ultimately, we present a thrice-differenced estimator equal to the difference between the true DD estimate and the pseudo-DD estimate computed for the 2012 cohort. This approach makes our methodology robust to differences in labor market conditions between treated workers and matched controls, so long as the nature

[42] Online Appendix Figure A7 provides evidence of important divergence in labor market dynamics at the top end of the wage distribution between pseudo-treated workers and matched controls drawn from Washington State outside of King County. The figure shows quantiles from the hourly wage distribution in the fourth quarter of 2012 for workers who were earning less than \$11 per hour in the first quarter of 2012. At most quantiles, up to the seventy-fifth percentile, hourly wages for Seattle workers are quite similar to their matched controls, consistent with the assumption of parallel trends post-pseudo-treatment. The ninetieth and ninety-ninth percentiles, however, show Seattle workers well ahead of their counterparts. At these percentiles matched control workers have hourly wages of roughly \$17 and \$27, respectively, while Seattle workers at the same percentiles see wages of roughly \$19 and \$31. It appears that the upper tail of the distribution reflects individuals who accelerate rapidly out of the low-wage labor market because, for example, they complete an educational degree or training program and transition to higher-skilled work. These opportunities may be more plentiful in Seattle, which is home to approximately one-tenth of Washington's population but more than one-sixth of the state's colleges, according to the United States Department of Education. Among the city's 13 postsecondary institutions is the state's largest by enrollment, the University of Washington.

[43] Note that we chose 2012:I as a starting point because (a) it is sufficiently early that when followed for six quarters (i.e., to 2013:III) it is still pre-passage of the ordinance; (b) by beginning in a first quarter, we are comparing workers employed at the same calendar quarter as the real cohort 1, which is followed from 2015:I; and (c) it is sufficiently after the Great Recession of 2007:IV to 2009:II (NBER 2019) that we can reasonably assume that labor market outcomes for this pseudo-treated cohort are a counterfactual for the actually treated cohort.

of these differences remained stable between cohorts, except for the effect of the Seattle's minimum-wage increase on treated workers.

D. *Effect Heterogeneity*

This methodology, by necessity, omits a potentially important component of the workforce impacted by the Seattle minimum-wage ordinance: those not yet in the labor force at the beginning of the baseline period. While we cannot use our data to study the trajectories of those whose labor-force entry might have been delayed or eliminated as a consequence of the ordinance, we do have the capacity to study whether impacts varied significantly among workers with varying levels of experience at baseline.

To assess this potential heterogeneity, we split workers into two groups based on the sum of hours worked in the baseline and prior two quarters. We use a threshold of 582 hours, which is the median number of hours worked by Seattle workers in the nine-month period encompassing the baseline and prior two quarters.[44] We apply these same thresholds to control workers and workers in the pseudo-cohort.[45]

For each subsample, we compute the DDD estimates described above. Finally, we difference the results for less experienced workers with more experienced workers to produce a DDDD estimate.

E. *Inference*

Following Abadie and Spiess (2016), we use a nonparametric block bootstrap that resamples matched pairs of treatment and control workers. We produce 1,000 block-bootstrapped samples for each point estimate.

V. Results: Aggregate Analysis

A. *Simple First and Second Differences*

Tables 5a, 5b, 5c, and 5d present, respectively, summary statistics on the number of jobs, total hours worked, average wages, and total payroll in Washington's locatable establishments for all industries and for food and drinking places by wage level for the quarter the ordinance was passed ($t = 0$, April–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 compared with the rest of the state over this period and are not estimates of the ordinance's causal impact.

[44] Using a past-hours measure for experience will lead us to label a worker with a large number of hours over a short time span as "experienced." While we are agnostic as to whether this is appropriate, in alternate specifications we stratified workers by number of quarters since first observed in the ESD data and obtained equivalent results.

[45] Online Appendix Figure A8 plots the distribution of hours worked by treatment and matched control workers and illustrates that there are many workers in low-wage jobs with very low hours and a more modest group—roughly one in ten—working full-time (1,560 hours) or more for these three quarters.

Table 5a—Employment Statistics for Washington's Locatable Establishments

Quarter	Quarters after passage/ enforcement	Number of jobs		
		Wages under \$13	Wages under \$19	All
Panel A. Seattle				
2014:II	0	38,013	90,757	293,257
2014:III	1	38,906	92,845	301,480
2014:IV	2	33,949	87,779	304,121
2015:I	3	33,438	88,758	305,704
2015:II	4/1	33,380	90,526	312,350
2015:III	5/2	32,363	91,407	321,551
2015:IV	6/3	28,516	85,190	321,295
2016:I	7/4	23,292	85,618	323,436
2016:II	8/5	25,053	89,188	336,177
2016:III	9/6	23,896	87,753	340,755
Panel B. Washington State, excluding Seattle				
2014:II	0	384,871	759,967	1,690,641
2014:III	1	407,189	778,728	1,745,154
2014:IV	2	363,477	752,479	1,743,840
2015:I	3	364,759	743,421	1,710,429
2015:II	4/1	364,390	760,044	1,745,159
2015:III	5/2	375,648	771,202	1,809,506
2015:IV	6/3	338,312	743,801	1,785,712
2016:I	7/4	336,045	730,912	1,738,194
2016:II	8/5	346,153	761,930	1,801,923
2016:III	9/6	348,872	753,513	1,810,138

Notes: Data derived from administrative employment records obtained from the Washington Employment Security Department. Nonlocatable employers (i.e., multilocation, single-account firms and single-location firms that operate statewide or whose location could not be determined) are excluded.

The clearest signal of policy impact can be seen in the quarterly data on (inflation-adjusted) average wages for jobs paying under \$13 or under \$19 per hour, presented in Table 5c. The first and second phase-ins are associated with a 21 and 18 cent rise, respectively, in the average wage in jobs paying up to \$13. This significantly exceeds the quarter-to-quarter increase observed at any other point in time. The comparable time series for Washington State outside Seattle shows no increase whatsoever at the same points in time.

The evidence on employment patterns is harder to parse, partially because of the strong seasonality of low-wage employment and partially because Seattle and the remainder of Washington State exhibit similar patterns. As Table 5a shows, comparing the baseline second quarter of 2014 to the second quarter of 2016, the number of Seattle jobs paying less than \$13 per hour in all industries declined from 38,013 to 25,053 (a decline of 12,960, or 34 percent).[46] It isn't immediately clear how to interpret this decline, in part because Table 5b shows that employment in these

[46] We use 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 summer tourist season, and exhibits a trough in the winter months. Subminimum-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.

Table 5b—Employment Statistics for Washington's Locatable Establishments

Quarter	Quarters after passage/ enforcement	Total hours (thousands) Wages under $13	Wages under $19	All
Panel A. Seattle				
2014:II	0	13,468	36,451	129,237
2014:III	1	13,868	37,570	131,767
2014:IV	2	11,352	34,563	135,127
2015:I	3	10,704	33,244	131,372
2015:II	4/1	11,534	36,248	138,208
2015:III	5/2	10,960	36,453	141,658
2015:IV	6/3	9,278	33,882	146,018
2016:I	7/4	7,092	32,105	139,914
2016:II	8/5	8,297	35,467	149,675
2016:III	9/6	7,998	35,614	153,544
Panel B. Washington State, excluding Seattle				
2014:II	0	151,734	317,788	744,596
2014:III	1	157,656	325,913	751,898
2014:IV	2	128,537	297,671	758,199
2015:I	3	124,334	283,317	719,080
2015:II	4/1	141,841	316,924	769,817
2015:III	5/2	141,835	320,277	775,253
2015:IV	6/3	118,879	295,523	785,670
2016:I	7/4	114,832	281,240	735,968
2016:II	8/5	132,364	316,939	794,792
2016:III	9/6	131,066	317,357	796,075

Notes: Data derived from administrative employment records obtained from the Washington Employment Security Department. Nonlocatable employers (i.e., multilocation, single-account firms and single-location firms that operate statewide or whose location could not be determined) are excluded.

lowest-paying jobs shrank by 10 percent elsewhere in Washington State over the same period.

The reduction in employment at wages under $13 could reflect either the movement of wage rates above this threshold or the elimination of jobs. Table 5a shows that over the same two-year time period, the number of Seattle jobs paying less than $19 per hour fell from 90,757 to 89,188 (a decline of 1,569, or 1.7 percent).[47] Table 5b shows that employment at equivalent wages rose slightly in the remainder of Washington over the same time period. Measuring hours worked at low wages rather than employee head count yields similar conclusions: employment at the lowest wages shrank both in Seattle and elsewhere, but raising the threshold to $19 uncovers slight differences in trends. Between Spring 2014 and Spring 2016, hours worked at wages under $19 fell 2.7 percent in Seattle compared to 0.3 percent in the remainder of Washington.

Over this same period, total employment in Seattle expanded dramatically, by over 14.6 percent in head count and 15.8 percent in hours. Tables 5a and 5b clearly

[47] Online Appendix Table A2 breaks down the changes in employment into more wage categories. The largest gains in Seattle employment occurred for jobs paying more than $40 per hour, which grew 32 percent between the second quarter of 2014 and the second quarter of 2016.

TABLE 5C—EMPLOYMENT STATISTICS FOR WASHINGTON'S LOCATABLE ESTABLISHMENTS

		Average wage ($)		
Quarter	Quarters after passage/ enforcement	Wages under $13	Wages under $19	All
Panel A. Seattle				
2014:II	0	11.15	14.19	38.48
2014:III	1	11.15	14.19	39.38
2014:IV	2	11.25	14.41	42.80
2015:I	3	11.27	14.46	42.89
2015:II	4/1	11.48	14.53	40.22
2015:III	5/2	11.54	14.62	41.72
2015:IV	6/3	11.62	14.78	44.16
2016:I	7/4	11.80	15.02	48.11
2016:II	8/5	11.87	15.00	47.09
2016:III	9/6	11.87	15.03	46.69
Panel B. Washington State, excluding Seattle				
2014:II	0	10.83	13.50	28.66
2014:III	1	10.90	13.49	33.25
2014:IV	2	11.03	13.80	31.21
2015:I	3	11.01	13.76	31.44
2015:II	4/1	11.00	13.70	29.57
2015:III	5/2	11.06	13.72	33.66
2015:IV	6/3	11.14	13.98	31.78
2016:I	7/4	11.10	13.92	32.35
2016:II	8/5	11.13	13.91	30.24
2016:III	9/6	11.15	13.92	34.43

Notes: Data derived from administrative employment records obtained from the Washington Employment Security Department. Nonlocatable employers (i.e., multilocation, single-account firms and single-location firms that operate statewide or whose location could not be determined) are excluded.

show that the entirety of this growth occurred in jobs paying over $19 per hour.[48] The impression of skewed growth—driven in part by rapid growth in the technology sector—extends to wage data. Table 5c documents that average hourly wages at jobs paying less than $19 per hour rose from $14.19 to $15.00 (a 5.7 percent increase), while average hourly wages at all jobs surged from $38.48 to $47.09 (a 22.4 percent increase).[49]

Table 5d documents that declines in hours worked substantially offset the observed wage increases for jobs paying under $19 per hour. Total earnings paid at wages under $19 increased only slightly (2.9 percent), from 517 to 532 million dollars, between the second quarter of 2014 and the second quarter of 2016, barely distinguishable from the 2.7 percent payroll growth observed in jobs in the same wage range in the remainder of Washington State.[50]

[48] The more detailed statistics in online Appendix Table A2 show that net job growth in Seattle was 25 percent for jobs paying over $25 per hour but only 3 percent for jobs paying under $25. About 66 percent of net job growth can be attributed to jobs paying over $40 per hour and 81 percent to jobs paying over $30 per hour.

[49] The median hourly wage, weighted by hours (not shown in Table 5), was $25.81 in the second quarter of 2014. The ratio of the $13 minimum wage to the median (i.e., the "Kaitz Index," Kaitz [1970]) is 0.504. For comparison, the Kaitz Index for the US federal minimum wage was 0.371 in 2014 (Cooper, Mishel, and Schmitt 2015).

[50] Online Appendix Table A3 shows the growth in jobs by industry during the year before the ordinance was passed (i.e., 2013:II to 2014:II). Seattle experienced 5.2 percent growth in jobs paying less than $19 per hour during this year and more than half of this growth was in two industries, "Administrative and Support and Waste

Table 5d—Employment Statistics for Washington's Locatable Establishments

Quarter	Quarters after passage/ enforcement	Total payroll ($millions)		
		Wages under $13	Wages under $19	All
Panel A. Seattle				
2014:II	0	150	517	4,973
2014:III	1	155	533	5,189
2014:IV	2	128	498	5,783
2015:I	3	121	481	5,634
2015:II	4/1	132	527	5,558
2015:III	5/2	126	533	5,909
2015:IV	6/3	108	501	6,448
2016:I	7/4	84	482	6,732
2016:II	8/5	98	532	7,048
2016:III	9/6	95	535	7,170
Panel B. Washington state, excluding Seattle				
2014:II	0	1,643	4,291	21,337
2014:III	1	1,718	4,397	24,998
2014:IV	2	1,418	4,109	23,662
2015:I	3	1,369	3,897	22,606
2015:II	4/1	1,560	4,342	22,764
2015:III	5/2	1,568	4,395	26,092
2015:IV	6/3	1,324	4,132	24,972
2016:I	7/4	1,275	3,915	23,807
2016:II	8/5	1,474	4,409	24,035
2016:III	9/6	1,462	4,417	27,405

Notes: Data derived from administrative employment records obtained from the Washington Employment Security Department. Nonlocatable employers (i.e., multilocation, single-account firms and single-location firms that operate statewide or whose location could not be determined) are excluded.

B. *Causal Effect Estimates*

Table 6a, 6b, 6c, and 6d present, respectively, our estimates of the causal impact of the ordinance on wages, hours, jobs, and payroll for workers earning less than $19 per hour, using three methodologies: synthetic control in levels, synthetic control in first differences, and interacted fixed effects in first differences. We harmonized estimates across methods to measure relative impacts. Levels estimates are often estimated less precisely than corresponding models in first differencing. We present both here, but carry forward with the more precise first-differenced models.

We find some evidence of anticipation effects in the period immediately before the first wage increase. Two specifications, the synthetic control method in levels and the interactive fixed effects, suggest that wages paid in Seattle's low-wage labor market rose by just under a percentage point in the six months prior to implementation, four to nine months after the ordinance's passage. We will return to this finding in the discussion of our micro-level analysis below.

Management and Remediation Services" and "Accommodation and Food Services." Synthetic Seattle saw 3.8 percent growth in jobs paying less than $19 per hour during this year and more than half of this growth was in the same two industries plus "Manufacturing." Seattle experienced similar growth in higher-paying jobs, up 5.7 percent, of which more than half of this growth came from two industries, "Retail Trade" and "Finance and Insurance." More than half of the growth in higher-paying jobs in Synthetic Seattle came from "Retail Trade," "Educational Services," and "Construction." (Note, our data-sharing agreement precludes us from evaluating the impacts of specific firms, e.g., Amazon, on these industry job tallies).

Table 6a—Estimated Effects on Levels and Growth Rates in Wages Using Synthetic Control and Interactive Fixed Effects Methods

Quarter	Quarters after passage/enforcement	Wages SC levels	Wages SC growth rates	Wages IFE growth rates
2014:III	1	0.003	0.002	0.005
		[0.391]	[0.585]	[0.101]
2014:IV	2	0.008	0.003	0.008
		[0.024]	[0.465]	[0.013]
2015:I	3	0.009	0.002	0.009
		[0.008]	[0.598]	[0.004]
2015:II	4/1	0.015	0.011	0.016
		[0.002]	[0.029]	[0.000]
2015:III	5/2	0.020	0.016	0.022
		[0.013]	[0.006]	[0.000]
2015:IV	6/3	0.018	0.019	0.019
		[0.000]	[0.000]	[0.000]
2016:I	7/4	0.039	0.030	0.032
		[0.000]	[0.000]	[0.000]
2016:II	8/5	0.038	0.031	0.031
		[0.000]	[0.000]	[0.000]
2016:III	9/6	0.036	0.033	0.034
		[0.000]	[0.000]	[0.000]
R^2				0.781
Pre-policy RMSPE		0.004	0.003	
Observations		1,890	1,890	1,890

Notes: Estimates for all jobs paying $<$\$19 in all industries. Cumulative effect since 2014:II is reported. "Levels" results present the coefficient divided by the level in the baseline quarter, 2014:II. The dependent variable in each "levels" specification is the level of the outcome divided by the level in the baseline quarter, 2014:II, and for all outcomes except for wages the level is further divided by five as Seattle contains five PUMAs. The dependent variable in each "growth rates" specification is the year-over-year percentage change in that outcome. p-values for a two-tailed test of the hypothesis that the coefficient equals zero are reported in square brackets. p-values are calculated based on permutation. RMSPE shows the root-mean-square prediction error for the synthetic controls' pre-policy predictions (and divided by the the level in the baseline quarter, 2014:II, for the "levels" specifications). 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 (42). However, note that some of these PUMAs receive zero weight in the synthetic control results.

Across methodologies, we associate the first minimum-wage increase, to \$11, with wage effects of 1.1 percent to 2.2 percent (averaging 1.7 percent). The second increase, to \$13, is associated with a larger 3.0 percent to 3.9 percent wage effect (averaging 3.4 percent), or roughly \$0.48 per hour relative to the base average wage of \$14.19.

These estimates of wage effects appear modest in comparison to much of the existing literature. We note that the first-difference results in Table 5c show somewhat larger increases in wages at the low end of the scale (under \$19 per hour), about 3.2 percent during the first phase-in and 5.8 percent during the second. The smaller causal estimates suggest that wages increased in the control region as well—a pattern clearly observed in Figure 3, panel A. Tables 5a and 5b also indi-

Table 6b—Estimated Effects on Levels and Growth Rates in Hours Using Synthetic Control and Interactive Fixed Effects Methods

Quarter	Quarters after passage/ enforcement	Hours		
		SC levels	SC growth rates	IFE growth rates
2014:III	1	−0.004	0.002	0.005
		[0.760]	[0.916]	[0.766]
2014:IV	2	−0.013	0.006	0.000
		[0.333]	[0.713]	[0.975]
2015:I	3	0.000	−0.018	−0.015
		[0.987]	[0.336]	[0.349]
2015:II	4/1	−0.003	−0.006	−0.008
		[0.892]	[0.756]	[0.594]
2015:III	5/2	−0.019	−0.027	−0.008
		[0.406]	[0.356]	[0.715]
2015:IV	6/3	−0.021	−0.006	0.008
		[0.564]	[0.894]	[0.735]
2016:I	7/4	−0.048	−0.087	−0.057
		[0.051]	[0.005]	[0.014]
2016:II	8/5	−0.071	−0.066	−0.046
		[0.101]	[0.022]	[0.052]
2016:III	9/6	−0.099	−0.092	−0.064
		[0.029]	[0.051]	[0.023]
R^2				0.791
Pre-policy RMSPE		0.014	0.013	
Observations		1,890	1,890	1,890

Notes: Estimates for all jobs paying < \$19 in all industries. Cumulative effect since 2014:II is reported. "Levels" results present the coefficient divided by the level in the baseline quarter, 2014:II. The dependent variable in each "levels" specification is the level of the outcome divided by the level in the baseline quarter, 2014:II, and for all outcomes except for wages the level is further divided by five as Seattle contains five PUMAs. The dependent variable in each "growth rates" specification is the year-over-year percentage change in that outcome. *p*-values for a two-tailed test of the hypothesis that the coefficient equals zero are reported in square brackets. *p*-values are calculated based on permutation. RMSPE shows the root-mean-square prediction error for the synthetic controls' pre-policy predictions (and divided by the the level in the baseline quarter, 2014:II, for the "levels" specifications). 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 (42). However, note that some of these PUMAs receive zero weight in the synthetic control results.

cate that the majority of jobs (58 percent) and hours (63 percent) paying less than \$19 per hour 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 the 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 a 9 percent wage increase, a level in line with existing literature.[51] Finally, we note that the measure of wages used

[51] 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 9 percent on a minimum-wage increase of 37 percent would imply an elasticity of

Table 6c—Estimated Effects on Levels and Growth Rates in Employment Using Synthetic Control and Interactive Fixed Effects Methods

Quarter	Quarters after passage/ enforcement	Jobs SC levels	Jobs SC growth rates	Jobs IFE growth rates
2014:III	1	0.011	0.002	−0.003
		[0.321]	[0.924]	[0.842]
2014:IV	2	0.011	−0.002	−0.014
		[0.462]	[0.892]	[0.357]
2015:I	3	0.012	0.007	−0.005
		[0.568]	[0.659]	[0.724]
2015:II	4/1	0.022	−0.010	−0.024
		[0.251]	[0.549]	[0.107]
2015:III	5/2	0.007	−0.011	−0.026
		[0.469]	[0.576]	[0.223]
2015:IV	6/3	−0.009	−0.033	−0.035
		[0.785]	[0.391]	[0.109]
2016:I	7/4	−0.012	−0.038	−0.032
		[0.660]	[0.293]	[0.146]
2016:II	8/5	−0.011	−0.052	−0.071
		[0.709]	[0.076]	[0.001]
2016:III	9/6	−0.063	−0.072	−0.088
		[0.027]	[0.067]	[0.001]
R^2				0.718
Pre-policy RMSPE		0.013	0.013	
Observations		1,890	1,890	1,890

Notes: Estimates for all jobs paying $<$\$19 in all industries. Cumulative effect since 2014:II is reported. “Levels” results present the coefficient divided by the level in the baseline quarter, 2014:II. The dependent variable in each “levels” specification is the level of the outcome divided by the level in the baseline quarter, 2014:II, and for all outcomes except for wages the level is further divided by five as Seattle contains five PUMAs. The dependent variable in each “growth rates” specification is the year-over-year percentage change in that outcome. *p*-values for a two-tailed test of the hypothesis that the coefficient equals zero are reported in square brackets. *p*-values are calculated based on permutation. RMSPE shows the root-mean-square prediction error for the synthetic controls’ pre-policy predictions (and divided by the the level in the baseline quarter, 2014:II, for the “levels” specifications). 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 (42). However, note that some of these PUMAs receive zero weight in the synthetic control results.

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 6b shows statistically insignificant effects on hours both during the pre-implementation period and the three quarters when the minimum wage was \$11. Point estimates for the \$11 period range between +0.8 percent and −2.7 percent (averaging −1.0 percent) and show no consistent time trend within the period

0.25. We note, moreover, that the highest \$13 minimum did not apply to small businesses or to 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).

Table 6d—Estimated Effects on Levels and Growth Rates in Payroll Using Synthetic Control and Interactive Fixed Effects Methods

Quarter	Quarters after passage/ enforcement	Payroll		
		SC levels	SC growth rates	IFE growth rates
2014:III	1	0.006	−0.001	0.014
		[0.643]	[0.946]	[0.301]
2014:IV	2	−0.012	0.012	0.012
		[0.425]	[0.479]	[0.404]
2015:I	3	−0.004	−0.004	−0.006
		[0.789]	[0.836]	[0.698]
2015:II	4/1	0.004	0.017	0.010
		[0.850]	[0.399]	[0.486]
2015:III	5/2	−0.006	0.006	0.015
		[0.797]	[0.847]	[0.478]
2015:IV	6/3	−0.016	0.025	0.023
		[0.623]	[0.614]	[0.286]
2016:I	7/4	−0.021	−0.032	−0.035
		[0.300]	[0.416]	[0.149]
2016:II	8/5	−0.060	−0.013	−0.024
		[0.128]	[0.739]	[0.352]
2016:III	9/6	−0.100	−0.037	−0.039
		[0.037]	[0.519]	[0.176]
R^2				0.825
Pre-policy RMSPE		0.014	0.012	
Observations		1,890	1,890	1,890

Notes: Estimates for all jobs paying < \$19 in all industries. Cumulative effect since 2014:II is reported. "Levels" results present the coefficient divided by the level in the baseline quarter, 2014:II. The dependent variable in each "levels" specification is the level of the outcome divided by the level in the baseline quarter, 2014:II, and for all outcomes except for wages the level is further divided by five as Seattle contains five PUMAs. The dependent variable in each "growth rates" specification is the year-over-year percentage change in that outcome. *p*-values for a two-tailed test of the hypothesis that the coefficient equals zero are reported in square brackets. *p*-values are calculated based on permutation. RMSPE shows the root-mean-square prediction error for the synthetic controls' pre-policy predictions (and divided by the the level in the baseline quarter, 2014:II, for the "levels" specifications). 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 (42). However, note that some of these PUMAs receive zero weight in the synthetic control results.

across methodologies. By contrast, the subsequent minimum-wage increase to \$13 associates with larger, mostly significant hours reductions between 4.6 percent and 9.9 percent (averaging −7.0 percent). The significant effects appear immediately upon implementation of the \$13 minimum wage and show no consistent time trend within that period across methodologies.

Table 6c presents parallel findings for jobs, with contrasting results. Similarly to the effect on hours, there is no statistically significant evidence of an impact either in the pre-implementation period or during the \$11 minimum wage. While five of nine coefficients are statistically significant in the \$13 minimum-wage period, none of the three methodologies show evidence of an instantaneous impact. In fact, point estimates for the first quarter of 2016 are quite similar to those of the fourth quarter of 2015. Significant effects emerge by the third quarter of 2016 across methodologies.

The contrast in results across specifications measuring employment by hours and head count poses a puzzle. We note that our methodology implies that an instantaneous cutback in new hiring would be observed with a one-quarter time lag here, as a job does not count until it enters its second quarter of existence. At the same time, the scope for hiring reductions in the winter months is limited, since the first calendar quarter represents the trough in seasonal employment patterns. In an environment with low baseline hiring rates, employers might respond to the minimum-wage increase by reducing hours for existing workers, without laying off a significant number of workers. Over time, ordinary attrition coupled with reduced hiring lead to a reduction in employee headcount, producing the jobs result pattern seen here. For this explanation to carry weight, persistent workers would need to see some restoration of their hours over time. We look for this evidence in the micro-level analysis and find the pattern to hold.

The employment point estimates for the third quarter of 2016 average -0.074. Multiplying this number by the 90,757 jobs paying less than \$19 per hour at baseline suggests that the ordinance eliminated more than 6,700 low-wage jobs at locatable establishments in that quarter compared to the counterfactual. The point estimates for lost hours in that same quarter are more negative, suggesting either that the missing jobs included greater than average hours or that persisting workers experienced declines on the intensive margin.[52]

Because the estimated magnitude of employment losses exceeds the magnitude of wage gains in the second phase-in period, we would expect a decline in total payroll for jobs paying under \$13 per hour. Table 6d in fact shows mostly insignificant effects, though the point estimates are consistently negative.[53,54]

C. *Assessing Threats to Validity*

Our methodology might yield spurious negative estimates if increasing labor demand leads to a rightward shift in the overall wage distribution, pushing a growing number of jobs above any fixed wage threshold. While noting that this concern requires for a shock to labor demand to coincide with winter 2016, given the pattern

[52] Since Seattle's locatable establishments experienced an observed reduction of about 3,000 low-wage jobs between the second quarter of 2014 and the third quarter of 2016 (Table 5), our estimates suggest that in the absence of the policy change, locatable establishments in Seattle would have added more than 3,700 low-wage jobs over that period.

[53] In a robustness check of the synthetic control method (online Appendix Table A4), we replace the outcome-specific weights for control PUMAs with a common set of weights for PUMAs (found by first standardizing each of the outcomes, creating a stacked dataset, and estimating weights for the stacked data). Though this common-weights approach generates less appropriate weighting when evaluating any specific outcome, the results are robust to this change. Notably, we find significant effects on wages that coincide with the minimum-wage step-ups and negative and mostly significant effects on hours, averaging +3.8 percent and -6.8 percent, respectively, in the last three quarters. Finally, note that the pre-policy root-mean-square prediction error is consistently higher using the common weights than the outcome-specific weights, which is as expected as the common weights are not designed to minimize this error for any specific outcome and thus are inferior when evaluating effects on any specific outcome.

[54] As a second robustness check (online Appendix Table A5) of the synthetic control method, in constructing weights for the PUMAs, we exclude values of the dependent variable during the four quarters prior to the ordinance's passage. That is, we match based on correspondence of the dependent variable in Seattle and the dependent variables in PUMAs outside King County through 2013:II. The results are similar to the main results in Table 6.

of our hours results, we examine this hypothesis in two ways.[55] First, we reverse the analysis and evaluate whether there are effects of the ordinance on jobs paying more than $19 per hour. If we successfully cleave the market into an affected and an unaffected group, then we would not expect to identify any effects for the group that we label as unaffected. Tables 7b, 7c, and 7d show the result of extending our synthetic control methodology to the study of geographically locatable jobs paying over $19 per hour, stratifying the labor market into segments where hourly wages range between $19 and $25, between $25 and $40, and over $40, respectively. For convenience, Table 7a collects the comparable results from the above analysis of jobs paying under $19 per hour.

Table 7b, studying the labor market tranche between $19 and $25, reports 0 significant coefficients out of 27 estimates. There is no evidence that the imposition of the minimum wage altered the wages, hours, or aggregate employment tallies in this segment. The pre-policy fit for synthetic control analysis, as measured by the RMSPE, is roughly equivalent to our main results.

Table 7c, analyzing jobs paying between $25 and $40, shows 6 significant coefficients among 27 estimated. The significant coefficients are all positive, suggesting that labor demand in Seattle accelerated ahead of the synthetic match region in the period under study. There is no consistent pattern, however, with regard to timing. Once again, the pre-policy fit between Seattle and its synthetic match is reasonably similar to our main analysis. The results suggest some impact of the "Seattle boom" as an ongoing phenomenon not precisely tied to the first quarter of 2016.

The highest-paying segment studied here, which would equate to annual salary ranges of $80,000 and above for full-time, year-round work, poses a methodological challenge. While Seattle's low-wage labor market has close analogues elsewhere in Washington State, leading to a close synthetic control match, Seattle's high-wage labor market is distinct. The RMSPE value for the wage-effect model is eleven times larger for high-wage employment than for low-wage employment; RMSPE for hours and jobs are more than twice their respective benchmarks.

With this caveat in mind, results in Table 7d show a concerning pattern: statistically significant wage effects appear suddenly in the first quarter of 2016, with magnitudes five times greater than what we observe in the original low-wage segment of the market. Large, statistically significant, and positive hours effects also appear at the same time.[56] In the jobs specification, we continue to see positive, significant results, but the timing is not as precise.

One possible reading of this evidence is that the minimum-wage ordinance had cascading impacts up to and above the $40 per hour level, more than four times the original minimum wage, causing a worker who might have earned $40 per hour in the absence of the ordinance to earn more than $45 instead. This interpretation, though consistent with these particular estimates, is difficult to reconcile with prior

[55] Online Appendix A reports on additional sensitivity tests varying the low-wage employment threshold from $12 to $25. In general, our results are not sensitive to altering this threshold.

[56] This pattern is also observed in online Appendix Figure A11, which presents finer detail than Tables 7b, 7c, and 7d, examining results for employment disaggregated into hourly wage bins of width $1.

Table 7a—Estimated Effects by Wage Range

Quarter	Quarters after passage/enforcement	Wages under $19		
		Wages	Hours	Jobs
2014:III	1	0.002	0.002	0.002
		[0.585]	[0.916]	[0.924]
2014:IV	2	0.003	0.006	−0.002
		[0.465]	[0.713]	[0.892]
2015:I	3	0.002	−0.018	0.007
		[0.598]	[0.336]	[0.659]
2015:II	4/1	0.011	−0.006	−0.010
		[0.029]	[0.756]	[0.549]
2015:III	5/2	0.016	−0.027	−0.011
		[0.006]	[0.356]	[0.576]
2015:IV	6/3	0.019	−0.006	−0.033
		[0.000]	[0.894]	[0.391]
2016:I	7/4	0.030	−0.087	−0.038
		[0.000]	[0.005]	[0.293]
2016:II	8/5	0.031	−0.066	−0.052
		[0.000]	[0.022]	[0.076]
2016:III	9/6	0.033	−0.092	−0.072
		[0.000]	[0.051]	[0.067]
Pre-policy RMSPE		0.003	0.013	0.013
Observations		1,890	1,890	1,890

Notes: Estimates using synthetic control reported. Cumulative effect since 2014:II is reported. Dependent variable in all regressions is year-over-year growth rate in each outcome. *p*-values for a two-tailed test of the hypothesis that the coefficient equals zero are reported in square brackets. *p*-values are calculated based on permutation. RMSPE shows the root-mean-square 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 (42). However, note that some of these PUMAs receive zero weight in the synthetic control results.

estimates suggesting that cascading effects extend only to around twice the original minimum wage.

An alternate reading is that this evidence shows an unrelated positive shock to highly skilled labor demand that roughly coincided with the implementation of the $13 minimum wage. This shock may have had secondary impacts on the low-wage labor market to the extent that newly recruited high-skilled workers raised demand for labor in the lower-paid service sector. Such a simultaneous shock might lead us to misstate the impact of the ordinance, with the sign of the bias indeterminate and depending on whether the secondary impulse to low-wage labor demand pushed market wages above $19 per hour. Online Appendix C presents evidence based on longitudinal tracking of jobs, which suggests any such effect would cause us to overstate our hours-reduction effects in the main analysis by about one percentage point.

Our second test of the rightward-shift hypothesis evaluates a pseudo-passage of a minimum-wage ordinance two years prior to the actual passage, when the Seattle boom was by many measures already underway. Online Appendix Table A6 shows that both the synthetic control and interactive fixed-effects specifications pass

Table 7b—Estimated Effects by Wage Range

Quarter	Quarters after passage/enforcement	Wages $19–25		
		Wages	Hours	Jobs
2014:III	1	−0.001	0.009	0.007
		[0.692]	[0.782]	[0.799]
2014:IV	2	−0.001	−0.005	0.007
		[0.577]	[0.841]	[0.829]
2015:I	3	−0.001	−0.036	−0.030
		[0.447]	[0.171]	[0.285]
2015:II	4/1	0.001	−0.009	−0.020
		[0.573]	[0.737]	[0.360]
2015:III	5/2	−0.001	0.021	−0.001
		[0.361]	[0.565]	[0.985]
2015:IV	6/3	−0.003	−0.015	−0.022
		[0.176]	[0.786]	[0.623]
2016:I	7/4	−0.002	−0.057	−0.041
		[0.466]	[0.317]	[0.570]
2016:II	8/5	−0.001	−0.026	−0.044
		[0.636]	[0.703]	[0.379]
2016:III	9/6	−0.002	0.044	−0.001
		[0.170]	[0.551]	[0.992]
Pre-policy RMSPE		0.000	0.014	0.015
Observations		1,890	1,890	1,890

Notes: Estimates using synthetic control reported. Cumulative effect since 2014:II is reported. Dependent variable in all regressions is year-over-year growth rate in each outcome. *p*-values for a two-tailed test of the hypothesis that the coefficient equals zero are reported in square brackets. *p*-values are calculated based on permutation. RMSPE shows the root-mean-square 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 (42). However, note that some of these PUMAs receive zero weight in the synthetic control results.

this falsification test. Only 2 of the 36 estimated coefficients are significant at the two-tailed 90-percent confidence level, and none are significant at the 95-percent level.[57]

In summary, these tests of threats to validity present mixed evidence. Our methods pass a falsification test when applied to an earlier period still within the post-2010 Seattle boom. Online Appendix A shows they are not sensitive to the choice of the wage threshold. Our longitudinal decomposition analysis in online Appendix C indicates that the increased rate of transition from below to above the threshold

[57] Across both methods, all but one of the estimated pseudo-effects on hours are negative and average −1.7 percent and −1.6 percent, 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. However, as shown in Table 6b, these negative coefficients are not consistently observed in the first three quarters of post-ordinance data, between adoption of the 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 percent and −0.2 percent, respectively.

Table 7c—Estimated Effects by Wage Range

Quarter	Quarters after passage/enforcement	Wages $25–40		
		Wages	Hours	Jobs
2014:III	1	0.003	0.025	0.028
		[0.301]	[0.413]	[0.281]
2014:IV	2	0.003	0.000	0.014
		[0.140]	[0.995]	[0.687]
2015:I	3	0.005	0.004	0.054
		[0.010]	[0.826]	[0.049]
2015:II	4/1	0.002	0.023	−0.029
		[0.464]	[0.456]	[0.266]
2015:III	5/2	0.004	0.048	0.034
		[0.142]	[0.062]	[0.312]
2015:IV	6/3	0.006	0.000	0.026
		[0.060]	[1.000]	[0.500]
2016:I	7/4	0.007	0.001	0.068
		[0.026]	[0.987]	[0.005]
2016:II	8/5	0.002	0.002	0.028
		[0.413]	[0.972]	[0.516]
2016:III	9/6	0.003	0.028	0.071
		[0.301]	[0.561]	[0.015]
Pre-policy RMSPE		0.001	0.024	0.017
Observations		1,890	1,890	1,890

Notes: Estimates using synthetic control reported. Cumulative effect since 2014:II is reported. Dependent variable in all regressions is year-over-year growth rate in each outcome. *p*-values for a two-tailed test of the hypothesis that the coefficient equals zero are reported in square brackets. *p*-values are calculated based on permutation. RMSPE shows the root-mean-square 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 (42). However, note that some of these PUMAs receive zero weight in the synthetic control results.

wage can explain about one-seventh of our estimated hours impact. This longitudinal analysis of continuing jobs cannot account for new workforce entrants who earned above $19 per hour but would have earned below that level in the counterfactual scenario. Finally, synthetic control analysis of the highest-paying jobs in Seattle points to a discrete jump in employment coincident with the minimum-wage increase to $13 per hour.

These last pieces of evidence, in particular, suggest that our aggregate analysis may overstate hours losses due to the minimum-wage increase. The same sources of bias will work in the opposite direction in our micro-level analysis below: to the extent that the Seattle boom spuriously pushes wages up, we run the risk of attributing the effects on longitudinally tracked workers to the minimum-wage ordinance.

D. *Comparison with Restaurant Industry Analysis*

Online Appendix D presents supplemental analysis comparing the results presented in Tables 6a, 6b, 6c, and 6d with an industry-specific analysis focusing on

TABLE 7D—ESTIMATED EFFECTS BY WAGE RANGE

		Wages above $40		
Quarter	Quarters after passage/ enforcement	Wages	Hours	Jobs
2014:III	1	0.029	0.046	0.070
		[0.247]	[0.316]	[0.107]
2014:IV	2	0.032	0.037	0.061
		[0.044]	[0.332]	[0.072]
2015:I	3	−0.003	0.037	0.075
		[0.860]	[0.458]	[0.050]
2015:II	4/1	0.019	0.018	0.060
		[0.324]	[0.722]	[0.178]
2015:III	5/2	0.040	0.072	0.094
		[0.109]	[0.128]	[0.053]
2015:IV	6/3	−0.002	0.049	0.069
		[0.926]	[0.265]	[0.130]
2016:I	7/4	0.144	0.115	0.087
		[0.000]	[0.039]	[0.151]
2016:II	8/5	0.206	0.088	0.103
		[0.000]	[0.130]	[0.036]
2016:III	9/6	0.151	0.197	0.173
		[0.000]	[0.000]	[0.052]
Pre-policy RMSPE		0.033	0.032	0.030
Observations		1,890	1,890	1,890

Notes: Estimates using synthetic control reported. Cumulative effect since 2014:II is reported. Dependent variable in all regressions is year-over-year growth rate in each outcome. *p*-values for a two-tailed test of the hypothesis that the coefficient equals zero are reported in square brackets. *p*-values are calculated based on permutation. RMSPE shows the root-mean-square 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 (42). However, note that some of these PUMAs receive zero weight in the synthetic control results.

restaurants, analogous to Reich, Allegretto, and Godoey (2017). Synthetic control models suggest that restaurant industry employment at all wage levels did not significantly change following Seattle's minimum-wage increases, consistent with the conclusions of Reich, Allegretto, and Godoey (2017). Restricting attention to employment at wage levels under $19, however, reveals significant 10–11 percent reductions in hours alongside wage increases of 6.6 percent.

VI. Results: Micro-Level Analysis

A. *Basic Findings*

Figure 4 presents the results of the micro-level analysis, i.e., the longitudinal impacts of the ordinance on outcomes for workers employed at baseline. Online Appendix Table A7 provides the quantitative findings that underlie the figure. The first row of panel A of the table shows that workers employed in Seattle in the first quarter of 2015 at hourly wages under $11 had an average wage of $10.06. Following

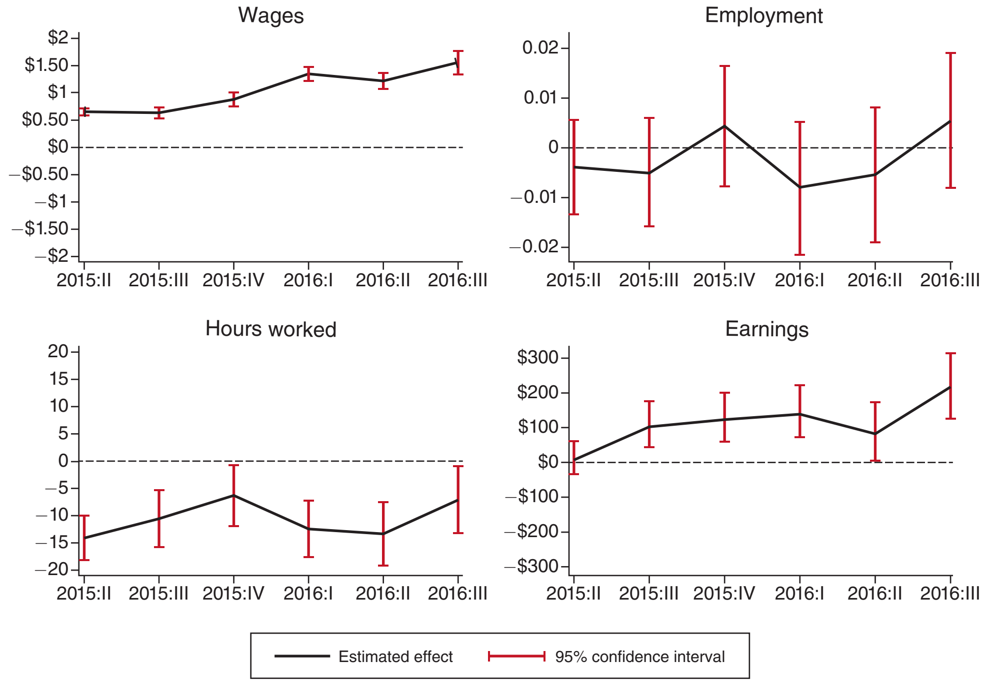

Figure 4. Estimated Effect of the Seattle Minimum Wage Ordinance on Wages, Employment, Hours Worked, and Earnings Conditional on Employment in 2015:I Paying Less Than $11 Per Hour

Notes: Sample: Workers at locatable firms. Wages have been adjusted for inflation using CPI-W. This figure presents the difference-in-differences-in-differences impact estimates with details provided in online Appendix Table A7.

Source: UI records from WA

this cohort forward, we see that by the third quarter of 2016 their average wage, conditional on employment, had risen to $15.08. The remainder of Panel A deduces what portion of this substantial wage increase can be attributed to the ordinance. This rapid gain could be due to mean reversion, the natural experience-earnings relationship, or underlying labor market trends. We expect these alternate mechanisms to be similarly salient among our matched controls. The second row of Panel A shows that the average wages of matched control-group workers increased from $10.06 at baseline to $12.84. The third row shows that the bias-corrected treatment-control difference in wages is $2.30 in 2016:III, whereas the bias-corrected difference at baseline was negligible ($0.00, rounded). The fourth row of Panel A nets off the negligible baseline difference to produce a DD estimate of $2.31.

Results for the pseudo-treated cohort assess whether we can attribute the DD estimated effect to the ordinance rather than "business as usual" differences in labor market dynamics between the city and outlying Washington State. We estimate that wages of the pseudo-treated cohort—i.e., workers employed in Seattle in 2012:I and earning an inflation-adjusted wage under $11—rose $0.76 relative to matched controls by the third quarter of 2013. This result suggests that labor market outcomes for low-wage Seattle workers are likely to diverge from their matched control workers outside of King County even in the absence of policy change.[58] We assume that this \$0.76 increase in wages provides a counterfactual for the DD estimate we would have found in Seattle for the treated cohort 1 (i.e., workers employed in Seattle in the first quarter of 2015 and earning a wage less than \$11) if Seattle had not passed the ordinance.

The DDD estimates in the last three rows of panel A show our best estimate of the causal effect of Seattle's minimum wage. We conclude that the first minimum-wage increase, to \$11, caused these workers' wages to rise between \$0.63 and \$0.88, and the second increase caused an increase between \$1.22 and \$1.54. The top-left panel in Figure 4 presents the DDD results in graphical form. Relative to the baseline average wage of \$10.06, this represents an increase of up to 15 percent, which is substantially greater than the wage effects observed in our aggregate analysis. The difference in magnitudes likely reflects differences in intensity of treatment: the aggregate analysis examines all jobs paying under \$19, whereas this exercise tracked workers who began with a wage under \$11.

Panel B of online Appendix Table A7 examines an indicator for continued employment at any wage level anywhere in Washington State and shows, broadly speaking, no effect. The DDD estimates are statistically insignificant (Figure 4, top right). This finding contrasts with our aggregate analysis, which found significant declines in overall employment, albeit with a time lag.

Panel C of online Appendix Table A7 indicates that Seattle workers worked significantly fewer hours per quarter as a result of the minimum-wage hikes. The DDD results show significant declines in hours worked in all six quarters, ranging from -6.3 to -14.1 hours per quarter, or 30 to 60 minutes per week (Figure 4, lower left).

These results again contrast with those of the aggregate analysis. Relative to the baseline mean of 239.4 hours per quarter, point estimates suggest hours reductions of up to 6 percent. While this magnitude is somewhat smaller than the 7 percent reduction implied by the aggregate analysis, the effects differ importantly in terms of timing. The aggregate analysis indicated no significant hours impacts with the first minimum-wage increase, followed by escalating impacts after the second. The micro-level analysis shows hours impacts that spike in the quarter of implementation for both the first and second phase-in quarters, then recede.

This is exactly the pattern forecasted in Section VB. It suggests that firms react to minimum-wage increases by scaling back hours per employee rather than laying off workers, a reaction that, among other things, may shield firms from impacts on their unemployment insurance experience rating. As attrition occurs—we note that online Appendix Table A7 documents "normal" attrition rates exceeding 20 percent over 6 months—persistent workers witness some restoration of their lost hours. This restoration may not be apparent in aggregate data because it may be accompanied by a slowdown in hiring rates.

Together, panels A and C suggest that treated incumbent workers experienced potentially offsetting effects—an increase in hourly wages coupled with a decline in hours worked. Panel D assesses how these opposing forces affected their quarterly

[58] Referencing online Appendix Figure 7, the spurious effect in the pseudo-cohort reflects the significant divergence in post-treatment hourly wages at the upper tail of the distribution.

earnings. The DDD estimate suggests that the Seattle minimum wage led to a gain in earnings of \$221, or about 9 percent of baseline earnings, in the third quarter of 2016 (Figure 4, lower right). For the three quarters of 2016, we estimate an average gain in earnings of \$153 per quarter or \$12 per week. As would be expected given the pattern of hours losses described above, earnings effects are relatively muted in the first quarter following phase-in but rise over time with the restoration of hours.

Overall, this analysis indicates that the ordinance caused hourly wage increases that were partly, and temporarily, offset by hours reductions among incumbent workers. Reconciliation of this evidence with the larger negative effects shown in Section VB hinges critically on the effect of the ordinance on workers yet to be hired as of the baseline period. While there are challenges in inferring effects on individuals who may never actually enter our database, we can draw some inferences by stratifying the sample of the entrants we observe.

B. *Heterogeneity in Effects Based on Prior Experience*

Table 8 presents evidence in support of the hypothesis that the ordinance was more beneficial to experienced workers, defined as those with working hours above the median in the nine months prior to the first minimum-wage increase.[59] Before reviewing results, it is important to note baseline differences in these two subsets of the low-wage workforce. The more experienced workers posted slightly higher average wages coupled with much greater hours worked: 367 hours over the first quarter of 2015, against 109 for the less experienced workers. This implies a substantial difference in baseline quarter earnings, \$3,716 against \$1,091. The more experienced group exhibits substantially stronger labor-force attachment.

We find that experienced workers saw their wages rise a bit more than less experienced workers, conditional on employment. For the three quarters of 2016, the estimated DDD estimates range from \$1.23 to \$1.90 for more experienced workers, versus \$1.06 to \$1.18 for less experienced workers. This disparity is statistically significant for three of the six quarters evaluated and each of these three estimates favor more experienced workers.

The relatively modest impacts on the probability of employment observed in online Appendix Table A7 persist when we split the sample by experience. There is evidence suggestive of greater disemployment among the less experienced population. Averaging across the estimates for 2016, we find a DDD effect of -1.0 percentage points for the less experienced, and $+0.5$ percentage points for the more experienced. Two of the six DDD estimates are negative and significant at the 10 percent level.

Both experience groups witnessed significant declines in hours worked. The DDD estimates for 2016 range from -8.1 to -12.5 and average -10.5 for the less experienced, a near 10 percent reduction relative to baseline hours. More experienced workers saw hours reductions over the same period between -8.8 to -16.1, averaging -13.4 or a 3.7 percent reduction relative to baseline. Compared to more

[59] Results are substantively unchanged if we instead define an experienced worker as one who first appeared in the Washington ESD data more than the median number of quarters prior to 2015:I.

TABLE 8—HETEROGENEITY IN ESTIMATED EFFECTS OF THE SEATTLE MINIMUM WAGE ORDINANCE BY HOURS WORKED IN BASELINE AND PRIOR TWO QUARTERS

	Baseline mean	Estimate	2015:II	2015:III	2015:IV	2016:I	2016:II	2016:III
Panel A. Effect on wages								
Workers with low experience	$10.00	DDD	$0.67	$0.44	$0.74	$1.06	$1.18	$1.11
		(s.e.)	($0.06)	($0.10)	($0.13)	($0.12)	($0.14)	($0.18)
		p-value	0.000	0.000	0.000	0.000	0.000	0.000
Workers with high experience	$10.13	DDD	$0.66	$0.81	$0.97	$1.55	$1.23	$1.90
		(s.e.)	($0.05)	($0.08)	($0.09)	($0.09)	($0.10)	($0.15)
		p-value	0.000	0.000	0.000	0.000	0.000	0.000
Low–high experience		DDD	$0.02	−$0.36	−$0.22	−$0.49	−$0.06	−$0.79
		(s.e.)	($0.08)	($0.13)	($0.16)	($0.15)	($0.17)	($0.24)
		p-value	0.856	0.004	0.153	0.001	0.747	0.001
Panel B. Effect on employment								
Workers with low experience	1.000	DDD	−0.013	−0.016	−0.002	−0.014	−0.010	−0.005
		(s.e.)	(0.008)	(0.009)	(0.010)	(0.011)	(0.010)	(0.010)
		p-value	0.128	0.079	0.840	0.190	0.313	0.611
Workers with high experience	1.000	DDD	0.004	0.004	0.010	−0.002	0.000	0.016
		(s.e.)	(0.005)	(0.007)	(0.008)	(0.009)	(0.009)	(0.009)
		p-value	0.422	0.504	0.195	0.789	0.991	0.076
Low–high experience		DDD	−0.017	−0.021	−0.012	−0.012	−0.011	−0.021
		(s.e.)	(0.010)	(0.011)	(0.013)	(0.014)	(0.014)	(0.014)
		p-value	0.089	0.065	0.349	0.388	0.449	0.128
Panel C. Effect on quarterly hours worked								
Workers with low experience	108.6	DDD	−15.7	−13.1	−11.1	−10.8	−12.5	−8.1
		(s.e.)	(3.1)	(3.8)	(3.9)	(3.7)	(4.0)	(4.2)
		p-value	0.000	0.001	0.004	0.003	0.002	0.055
Workers with high experience	367.1	DDD	−13.3	−9.8	−3.4	−15.3	−16.1	−8.8
		(s.e.)	(3.0)	(3.8)	(4.0)	(4.1)	(4.5)	(4.6)
		p-value	0.000	0.011	0.405	0.000	0.000	0.055
Low–high experience		DDD	−2.4	−3.3	−7.8	4.4	3.6	0.7
		(s.e.)	(4.2)	(5.5)	(5.8)	(5.6)	(6.1)	(6.2)
		p-value	0.572	0.546	0.178	0.424	0.559	0.907
Panel D. Effect on quarterly earnings								
Workers with low experience	$1,091	DDD	−$76	−$64	−$65	−$6	−$36	$35
		(s.e.)	($37)	($49)	($55)	($55)	($61)	($69)
		p-value	0.038	0.190	0.234	0.919	0.555	0.608
Workers with high experience	$3,716	DDD	$115	$280	$306	$293	$201	$395
		(s.e.)	($35)	($50)	($55)	($56)	($63)	($72)
		p-value	0.001	0.000	0.000	0.000	0.001	0.000
Low–high experience		DDD	−$191	−$344	−$371	−$299	−$237	−$359
		(s.e.)	($51)	($70)	($78)	($77)	($86)	($98)
		p-value	0.000	0.000	0.000	0.000	0.006	0.000

Notes: Sample: Workers at locatable firms. Wages have been adjusted for inflation using CPI-W. "Low experience" is defined as working less than the median number of hours worked by Seattle workers in the baseline and prior two quarters (i.e., less than 582 hours). Of those categorized as having low experience, 33 percent had no hours worked during the two quarters before baseline. Treated workers are defined as those employed in 2015:I in locatable establishments in Seattle, not employed elsewhere in the state, and earning < $11 per hour. Control workers are defined as those employed in 2015:I in locatable establishments in Washington State but not employed in King County and earning < $11 per hour. Each treated worker is matched to their nearest neighbor control worker without replacement. The control sample is exactly matched in employment status in 2015:I, 2014:IV, and 2014:III and on an indicator for worker first observed in WA in 2015:I, 2014:IV, or 2014:III. Matching using Mahalanobis distance is based on wage rate, hours worked, tenure at the primary job, number of quarters since first observed in WA, and indicators for having earnings from more than one job in 2015:I, 2014:IV, and 2014:III. The pseudo-treated cohort is constructed analogously, but beginning from 2012:I. Estimators were bias-adjusted using wage rate, hours worked, tenure at the primary job, and number of quarters since first observed in WA in the baseline quarter and prior two quarters.

Source: UI records from WA

experienced workers, less experienced workers saw a smaller wage increase and lost nearly three times more hours in percentage terms, relative to baseline.[60]

Panel D presents the most striking findings. For less experienced workers, the gain in hourly wages was offset by the decline in hours, yielding small and insignificant net impacts on earnings. By contrast, for more experienced workers, all six DDD estimates for effects on earnings are positive and significant. For the three quarters of 2016, the DDD estimates average −\$2 per quarter for the less experienced workers and \$296 per quarter (or 8 percent) for the more experienced workers.[61]

C. *Employment outside Seattle and Turnover*

The contrasting findings of the aggregate and micro-level analyses could be reconciled in multiple ways. First, longitudinal analysis by necessity excludes workers who enter Seattle's low-wage workforce after the baseline period or who never enter at all. Second, individual workers may be making up for lost work in Seattle by adding employment outside the city limits. Our decision to include all Washington State employment and earnings might mask steeper employment and hours declines in the city.

The top panel of Table 9 explores this second mechanism of adjustment. In the full sample we do not find an increase in hours worked outside the city. We do, however, find significant increases in hours worked outside of Seattle among more experienced workers, with 3 of 6 estimated coefficients being statistically significant at the 10 percent level. Averaged across quarters, the effect amounts to about 5 hours per quarter, or 24 minutes per week. Taken in the context of our results on overall hours worked, these results indicate that experienced workers offset reductions in hours in Seattle by finding work outside the city. While the effect is modest in magnitude, it implies that a non-negligible portion of the weekly earnings increases accruing to more experienced workers can be attributed to increased work outside the city.[62]

By contrast, point estimates suggest less experienced workers in low-wage jobs decreased their hours worked outside the city by around four hours per quarter. The

[60] This larger percentage reduction in hours for less experienced workers is consistent with the incentives created by the UI system's tax on employers based on their experience rating, as previously noted. This result is also consistent with other explanations, e.g., standard last-in, first-out seniority rules in layoffs and the lower productivity of less experienced workers.

[61] Online Appendix Table A8 provides further detail, dividing workers into deciles on the basis of hours worked in the three quarters prior to each minimum-wage increase. Table 8's general conclusion—that roughly half of all workers enjoyed significant earnings increases, while the less experienced half effectively broke even—continues to hold. Individuals in the highest decile, who worked at least 1,471 hours in the nine-month base period, or an average of 38 hours per week, show particularly noteworthy impacts. These workers saw a combination of significantly higher earnings and reduced hours. It is conceivable that the observed hours reductions reflect voluntary cutbacks, given the high level of effort observed at baseline and the fact that these workers retain enough hours to boost their weekly earnings by as much as \$50 in some quarters.

[62] Valuing hours outside the city at the state minimum wage of \$9.47, the results suggest that about one-sixth of the earnings gains for experienced workers reflect increased work outside the city. From another perspective, computing an elasticity of labor demand for experienced low-wage workers using the Table 9 results would suggest a value around −0.25. (For experienced workers, a roughly 15 percent boost to wages was offset by a 3.7 percent reduction in hours.) Focusing more specifically on hours reduced by geographically locatable Seattle-based businesses, the elasticity estimate would be closer to −0.33.

Table 9—Mechanisms of Labor Adjustment: Employment outside Seattle and Reduced Turnover inside Seattle

	Estimate	2015:II	2015:III	2015:IV	2016:I	2016:II	2016:III
Panel A. Effect on hours worked outside Seattle							
All Workers	DDD	−3.3	0.8	−0.1	−7.1	−5.7	−2.3
	(s.e.)	(1.8)	(2.5)	(2.5)	(2.6)	(2.9)	(3.2)
	p-value	0.068	0.758	0.982	0.007	0.049	0.473
Workers with low experience	DDD	−6.9	−4.6	−2.2	−4.0	−4.3	−2.8
	(s.e.)	(2.6)	(3.2)	(3.4)	(3.3)	(3.7)	(4.0)
	p-value	0.007	0.158	0.525	0.230	0.247	0.473
Workers with high experience	DDD	3.9	10.7	8.2	−1.0	1.4	7.6
	(s.e.)	(2.5)	(3.4)	(3.7)	(3.7)	(4.2)	(4.5)
	p-value	0.122	0.001	0.029	0.778	0.734	0.090
Low–high experience	DDD	−10.7	−15.3	−10.3	−3.0	−5.7	−10.4
	(s.e.)	(3.6)	(4.7)	(5.1)	(5.1)	(5.8)	(6.1)
	p-value	0.003	0.001	0.041	0.562	0.325	0.087
Panel B. Effect on employed by baseline employer, conditional on employment							
All workers	DDD	0.005	0.013	0.031	0.039	0.023	0.036
	(s.e.)	(0.005)	(0.007)	(0.008)	(0.008)	(0.009)	(0.009)
	p-value	0.382	0.072	0.000	0.000	0.009	0.000
Workers with low experience	DDD	0.008	0.010	0.026	0.037	0.015	0.027
	(s.e.)	(0.009)	(0.012)	(0.013)	(0.014)	(0.014)	(0.013)
	p-value	0.333	0.400	0.056	0.007	0.293	0.042
Workers with high experience	DDD	0.004	0.017	0.036	0.042	0.031	0.044
	(s.e.)	(0.007)	(0.009)	(0.010)	(0.011)	(0.011)	(0.011)
	p-value	0.559	0.065	0.000	0.000	0.005	0.000
Low–high experience	DDD	0.005	−0.006	−0.010	−0.005	−0.017	−0.017
	(s.e.)	(0.011)	(0.015)	(0.017)	(0.018)	(0.018)	(0.018)
	p-value	0.681	0.668	0.542	0.762	0.352	0.346

Notes: Sample: Workers at locatable firms. Wages have been adjusted for inflation using CPI-W. Low experience is defined as working less than the median number of hours worked by Seattle workers in the baseline and prior two quarters (i.e., less than 582 hours). Of those categorized as having low experience, 33 percent had no hours worked during the two quarters before baseline. Treated workers are defined as those employed in 2015:I in locatable establishments in Seattle, not employed elsewhere in the state, and earning < \$11 per hour. Control workers are defined as those employed in 2015:I in locatable establishments in Washington State but not employed in King County and earning < \$11 per hour. Each treated worker is matched to their nearest neighbor control worker, without replacement. The control sample is exactly matched in employment status in 2015:I, 2014:IV, and 2014:III, and on an indicator for worker first observed in WA in 2015:I, 2014:IV, or 2014:III. Matching using Mahalanobis distance is based on wage rate, hours worked, tenure at the primary job, number of quarters since first observed in WA, and indicators for having earnings from more than one job in 2015:I, 2014:IV, and 2014:III. The pseudo-treated cohort is constructed analogously, but beginning from 2012:I. Estimators were bias-adjusted using wage rate, hours worked, tenure at the primary job, and number of quarters since first observed in WA in the baseline quarter and prior two quarters.

Source: UI records from WA

differences in estimated effects between more and less experienced workers are statistically significant at the 10 percent level in 4 of the 6 post-enforcement quarters.

The bottom panel of Table 9 explores whether workers were more likely to remain employed by their baseline employer, conditional on being employed. Given the higher minimum wage, employers have an incentive to retain their employees, assuming either that labor productivity is an increasing function of tenure or that new hires impose one-time recruitment or training costs. Higher wages may also increase incentives for workers to continue working for their current employer.

We find evidence consistent with this hypothesis. Workers are 0.5 to 3.9 percentage points more likely to be employed at their baseline employer conditional on being employed. These effects are significant at the 10 percent level in 5 of the 6 cases evaluated. Estimated turnover effects tend to increase over time. After 6 quarters, roughly half of our matched controls and placebo cohorts continued to be employed by their baseline employer, indicating a turnover rate of 50 percent. The 3.6 percentage point reduction in turnover found in the sixth quarter of treatment thus amounts to a 7 percent drop in turnover. We find no significant heterogeneity in this effect by worker experience. These results confirm recent empirical studies on minimum wages' impacts on job turnover that used proxies for the low-wage labor market (Dube, Lester, and Reich 2016; Gittings and Schmutte 2016).

VII. Results: Aggregate Analysis of Labor Market Entry

The micro-level analysis focuses on individuals employed at baseline. To be included in the treatment group, these workers must have had some work experience by the time Seattle's minimum wage increased, and that work experience may set them apart from inexperienced workers attempting to enter the Seattle low-wage labor market. As demonstrated in Table 8, more and less experienced workers already exhibit important differences in estimated policy effects. Job seekers who lack any labor market experience in Washington are invisible to us, as they do not appear in administrative records. We can infer their trajectories by studying aggregate statistics on the number of new entrants into the Seattle low-wage labor market.

Figure 5 presents evidence on new entrants, defined as workers paid under $15 per hour, inflation-adjusted, who had not been employed in Washington State in the prior five years.[63] We show four-quarter moving averages of the raw number of new entrants to eliminate strong seasonality in labor market entry and normalize the time series by multiplying by 100 and dividing by the four-quarter moving average in the second quarter of 2014 (i.e., we set second quarter of 2014, the quarter before passage of the ordinance, to 100). We again use synthetic control methods to construct the counterfactual and estimate the casual impact of the ordinance.

During the baseline period, both Seattle and synthetic Seattle show signs of accelerating growth in the low-wage labor market, with the number of new entrants growing at a comparable rate. The two trends diverge after the baseline period. Seattle transitions from a period of growth to a period of stasis or modest decline once the minimum-wage increase takes effect, despite its booming economy. Synthetic Seattle continues to see growth in new entrants. By the conclusion of the period under study, the number of new low-wage labor market entrants in Seattle lies about 10 percent below the level in synthetic Seattle.[64]

Combining the results of the aggregate and micro-level analysis suggests an important lesson: the ordinance appears to have delivered higher pay to longer-tenured

[63] Use of a moving window to identify new entrants is preferable to identifying new entrants as those with no prior appearance in the data, as this second selection criterion becomes systematically more stringent over time.

[64] Online Appendix Figure A12 graphically illustrates the statistical significance of this decline in low-wage labor market entry in Seattle by placing the difference between Seattle and synthetic Seattle (thick line) in the distribution of the differences between 2,994 other sets of 5 contiguous PUMAs and their respective synthetic counterfactuals (thin lines). The estimated effects of the ordinance on entrants is negative in all quarters after the minimum wage is enforced and significant at the 10 percent level in 2 of the 6 quarters.

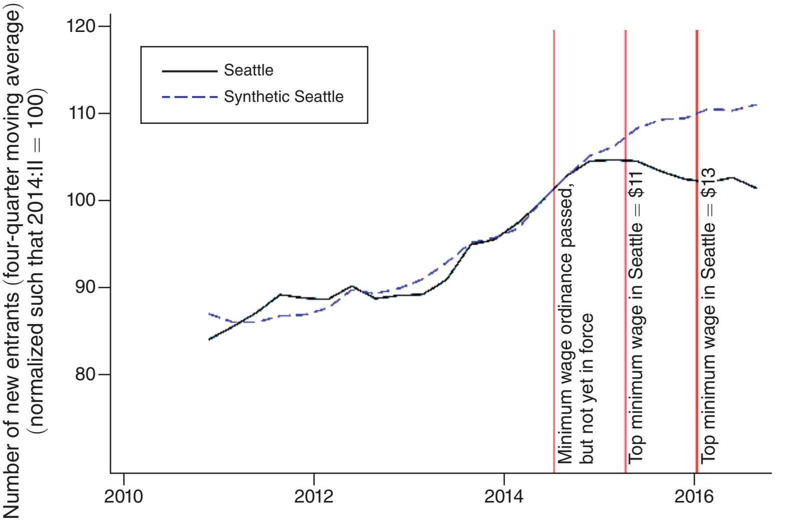

Figure 5. Relative Decline in New Low-wage Labor Market Entrants in Seattle

Notes: Sample: Workers at locatable firms. Wages have been adjusted for inflation using CPI-W. New entrants are defined as workers paid under $15 per hour, inflation-adjusted, who had not been employed in Washington State in the prior five years. Synthetic Seattle is a weighted average of time series of new entrants for other Washington PUMAs outside of King County.

Source: UI records from WA

incumbent workers at the cost of reduced earnings and opportunities for those with no prior work experience.

VIII. Discussion: The Elasticity of Labor Demand

Many prior studies of the employment effects of the minimum wage summarize their results with a demand elasticity estimate: the percent change in employment brought about by the observed or imputed percent change in wage. The analysis above raises several caveats about summarizing employment effects with a single elasticity. Estimates may vary importantly for workers with varying characteristics, in varying industries, along the extensive and intensive margins, in the short-run versus the medium-run, and depending on exactly how high and how fast the minimum wage is raised. Aaronson (2001); Aaronson and French (2007); Aaronson and Phelan (2017); and Harasztosi and Lindner (2019) have shown that the disemployment effect of the minimum wage for continuing firms is determined by firms' ability to raise consumer prices without facing reductions in sales (i.e., consumer price elasticity) and by the ability to substitute low-wage labor with capital (i.e., automation).

Beyond these dimensions of variation, the elasticity may depend on the geographic scope of the jurisdiction implementing the policy and the economic conditions therein. Our study cannot hope to illuminate all the dimensions of variation in the labor demand elasticity in the low-wage labor market, particularly this last one.

The results outlined in Sections V and VI nonetheless illuminate several important patterns.

Defining the unit of labor as an hour worked, elasticities can be computed as the ratio of (relative) hours effects to (relative) wage effects.[65] Panel A of Table 10 records elasticity estimates quarter by quarter, based on the results of the micro-level analysis.[66] Consistent with the diminishing hours effects observed in online Appendix Table A7, the intensive-margin elasticity estimate is largest in absolute value in the first quarter of implementation, at −0.91. By the sixth quarter of implementation, the implied elasticity has dropped to −0.19 in this selected subset of the labor market.

Panels B and C, based on Table 8, show considerably larger intensive-margin demand elasticities among less experienced workers. The implied elasticities in the first quarter of implementation are −0.56 for more experienced workers and −2.14 for less experienced workers. The differences persist even as elasticities for both groups decline through the six quarters.

The pattern of large but declining elasticities in the micro-level analysis contrasts strongly with the results of the aggregate synthetic control models. Panel D of Table 10 shows estimates of the elasticity of labor demand derived from the year-over-year change models in Section V.[67] We also compute measures of statistical uncertainty for these elasticities, since they are the ratio of two estimates.[68,69]

During the first phase-in, when the minimum wage was $11, estimated elasticities are estimated very imprecisely, reflecting the small magnitude and uncertainty of the numerator and denominator estimates. Estimated elasticities for the period after the minimum wage increased to $13 range from −2.15 to −2.94 (averaging −2.63), with no monotonic trend.[70] While the estimates of these elasticities are noisy, we can reject the hypothesis that the elasticity equals zero (consistent with Table 6) for the first quarter of 2016 and the second quarter of 2016 and nearly for the third quarter of 2016.

Table 10 also reports elasticity measures using the more conventional method in the minimum-wage literature, the ratio of employment impacts to the statutory

[65] As estimated employment effects are near zero in the micro-level analysis, labor demand elasticities treating jobs as the unit of labor are likewise near zero.

[66] To compute 95 percent confidence intervals for these elasticity estimates, we take 10,000 bootstrapped samples; compute estimated impacts on hours, wages, and the resulting elasticity for each bootstrapped sample; then report the 2.5th and 97.5th percentiles of the distribution of these 10,000 elasticities.

[67] 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 backward bending. Worker interview data collected by our research team suggest that the proportion of low-wage workers opting to voluntarily reduce hours as a result of wage increases is small.

[68] 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 $\left(\beta^{Hours}, \beta^{Wages}\right)$ in 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 $\left(\beta^{Hours}, \beta^{Wages}\right)$ that belong to the confidence set.

[69] Our estimates of the demand elasticity might not map onto any particular labor demand curve, as we are blending workers with wages below $15 with workers at more modest wage levels, between $15 and $19. As such, it is best to think our estimate is a weighted average elasticity for workers with wages below $19.

[70] Our results are similar to those in Mastracci and Persky (2008).

Table 10—Estimates of the Elasticity of Labor Demand with Respect to Minimum Wages

		Denominator is synthetic control estimated wage effect		Denominator is statutory increase in minimum wage	
Quarter	Quarters after passage/ enforcement	Point estimate	95 percent CI	Point estimate	95 percent CI
Panel A. Micro-level analysis, all workers					
2015:II	4/1	−0.91	(−0.64, −1.22)	−0.36	(−0.47, −0.26)
2015:III	5/2	−0.70	(−0.34, −1.12)	−0.27	(−0.41, −0.14)
2015:IV	6/3	−0.30	(−0.03, −0.59)	−0.16	(−0.31, −0.02)
2016:I	7/4	−0.39	(−0.21, −0.57)	−0.14	(−0.20, −0.08)
2016:II	8/5	−0.46	(−0.25, −0.69)	−0.15	(−0.22, −0.08)
2016:III	9/6	−0.19	(−0.02, −0.38)	−0.08	(−0.15, −0.01)
Panel B. Micro-level analysis, more experienced workers					
2015:II	4/1	−0.56	(−0.31, −0.82)	−0.22	(−0.32, −0.12)
2015:III	5/2	−0.34	(−0.08, −0.61)	−0.17	(−0.29, −0.04)
2015:IV	6/3	−0.11	(0.13, −0.33)	−0.06	(−0.19, 0.08)
2016:I	7/4	−0.27	(−0.13, −0.42)	−0.11	(−0.17, −0.05)
2016:II	8/5	−0.36	(−0.16, −0.58)	−0.12	(−0.18, −0.05)
2016:III	9/6	−0.13	(0.01, −0.27)	−0.06	(−0.13, 0.00)
Panel C. Micro-level analysis, less experienced workers					
2015:II	4/1	−2.14	(−3.16, −1.30)	−0.89	(−1.24, −0.55)
2015:III	5/2	−2.73	(−5.60, −1.07)	−0.75	(−1.18, −0.32)
2015:IV	6/3	−1.38	(−2.79, −0.42)	−0.63	(−1.08, −0.20)
2016:I	7/4	−0.94	(−1.76, −0.29)	−0.27	(−0.46, −0.09)
2016:II	8/5	−0.98	(−1.80, −0.34)	−0.31	(−0.51, −0.11)
2016:III	9/6	−0.67	(−1.56, 0.03)	−0.20	(−0.41, 0.01)
Panel D. Aggregate analysis					
2015:II	4/1	−0.58	(−48.88, 31.04)	−0.04	(−0.27, 0.20)
2015:III	5/2	−1.74	(−18.45, 6.51)	−0.17	(−0.52, 0.18)
2015:IV	6/3	−0.32	(−7.79, 6.51)	−0.04	(−0.48, 0.41)
2016:I	7/4	−2.94	(−7.83, −0.59)	−0.23	(−0.41, −0.06)
2016:II	8/5	−2.15	(−6.38, −0.16)	−0.18	(−0.34, −0.02)
2016:III	9/6	−2.81	(−10.20, 0.02)	−0.25	(−0.50, 0.00)

Notes: Sample: Workers at locatable firms. Wages have been adjusted for inflation using CPI-W. Synthetic control estimated wage effect based on year-over-year percentage change specification. 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. For the elasticity with respect to the statutory increase in minimum wage, $\%\Delta$ 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.

Source: UI records from WA

increase in the minimum wage. As the statutory increase is considerably larger than the estimated wage effect, these estimates are much closer to zero. In fact, the aggregate analysis elasticities estimated in this manner lie within the envelope of estimates found in prior literature (Belman and Wolfson 2014). They are also estimated more precisely, as the denominator is a constant rather than a statistical estimate.

In summary, elasticity estimates derived from this analysis range quite broadly. The largest estimates pertain to our aggregate analysis in the second phase-in period. They may be biased upward to the extent we have not solved the rightward shift problem. We obtain elasticity estimates nearly as large, exceeding −2, for less-experienced workers tracked longitudinally in the first phase-in period. In these analyses, the intensive-margin elasticity estimates for the full sample of workers employed in Seattle at baseline range from −1 immediately following the first phase-in to −0.2 in the second phase-in. If our methods fail to solve the rightward-shift concern, these elasticities may be underestimated depending on the relative impacts on wages and employment. They also fail to account for the ordinance's impact on workers not yet employed at baseline.

IX. Conclusion

Administrative data from Washington, one of four states that collect hours worked data through unemployment insurance quarterly reports, allow considerable insight into the impacts of Seattle's minimum-wage increases in 2015 and 2016. While different methods of analyzing these data produce divergent pictures of the net impact on the city's low-wage labor market, several consistent patterns emerge.

Our estimated labor demand elasticities are larger than much of the previous literature. We obtain the largest estimates, on the order of −2, in our aggregate analysis. These estimates rely on the assumption that the minimum wage did not have cascading impacts above 200 percent of the starting minimum, and that there was no contemporaneous Seattle-specific labor market shock shifting employment above a similar threshold. Our analysis raises some concerns that this assumption may not hold, leading us to overstate the elasticity. The smallest estimates, on the order of −0.2, appear in our longitudinal analysis of workers employed at baseline 15–18 months after the first minimum-wage increase. These estimates will be biased toward zero to the extent they ignore effects on the arrival rate of new workers into the low-wage labor market. While the implied bounds of our analysis, between −0.2 and −2, are admittedly large, they lie outside the 0 to −0.2 reported in most of the prior literature.

There are five reasons why these estimates exceed those in prior studies. First, we use the estimated impact of the minimum wage on wages as the denominator. Prior studies use the statutory increase in the minimum wage, which in our case is much larger. Second, we study an increase in a geographically compact region from which firm exit may be a more likely response. Third, we analyze the impact of raising the minimum wage to a high nominal level. Deflating by the Personal Consumption Expenditures price index, the real value of the federal minimum wage has never reached the $13 level studied here. Fourth, we directly study low-wage employment rather than potentially noisy proxies based on industry or worker age. Fifth, our measure of hours worked allows us to analyze both the intensive and extensive margins of employment. The analysis shows an important distinction between these two, particularly in the short run.

Longer-tenured workers appear to derive the greatest benefit from minimum-wage increases. Stratifying our sample based on total hours worked over a nine-month baseline period shows that more experienced workers saw stronger wage effects and smaller negative-hours effects. The intensive-margin elasticities for experienced workers are about one-third of the magnitude of those for less-experienced workers. This pattern can be read as supporting both proponents and opponents of minimum-wage increases: higher wages appear to deliver bigger paychecks to incumbent workers who earn low wages with limited upward mobility, but they also appear to curtail opportunities for new labor market entrants.

Rather than lay off workers in the wake of a minimum-wage increase, firms respond by cutting back hours per employee in the short run. As natural attrition occurs, our data show some evidence of a decline in the likelihood of hiring new workers coupled with a partial restoration of hours worked among remaining employees. This pattern suggests that efforts to study the impact of the minimum wage will tend to find stronger extensive margin impacts several months after an increase, a pattern apparent in our data.

A few cautions are in order. Our aggregate analysis includes only firms reporting employment at specific locations, as we cannot properly locate employment for multilocation firms that do not report employment separately by location. Our micro-level analysis includes only workers employed at such businesses at baseline. While survey evidence suggests little distinction between single-site and multisite firms, their labor demand elasticities may differ.

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. Such a move would not be without consequence for the workers, who would lose unemployment insurance, worker's compensation, and perhaps health insurance coverage.

Some employers may have shifted work just outside of Seattle, in which case the aggregate 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 that they were able to take advantage of relocated opportunities in the metropolitan area. We note that experienced low-wage workers, tracked longitudinally, exhibit some tendency to shift their work efforts outside of the city.

One cannot assume our specific findings generalize to minimum-wage policies set by other localities or at the federal or state level. Nor can one assume that what we found in Seattle through 2016 will continue. The impacts of minimum-wage policies likely depend on the industrial structure, characteristics of the local labor force, and other features of the local and regional economy. It may be that in Seattle, a hub of high-tech industry, firms are more inclined to adopt labor-saving technology in response to a large low-wage cost shock, which would have larger adverse effects than in other localities.

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