

# Evaluating the Impacts of the Seattle Secure Scheduling Ordinance

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## Introduction

To evaluate the impacts of Seattle’s Secure Scheduling legislation on the work schedule experiences of Seattle workers, we surveyed a set of workers paid by the hour and employed at businesses covered by the Secure Scheduling Ordinance. We collected pre-implementation, baseline survey data from Seattle workers in the Spring of 2017. We then collected follow-up survey data from Seattle workers between Fall of 2017 and Spring of 2018, after the law had gone into effect.

For the short-term follow-up period covered in this report, our goal was to generate rigorous estimates of the impacts of the Secure Scheduling Ordinance on workers’ reports of their work schedules. To accomplish this goal, it is essential to understand how work schedules might have changed over time even in the absence of the Secure Scheduling Ordinance. Therefore, we also collected survey data from workers employed by the same set of businesses in comparison cities that did not have any scheduling regulations in place. The data from comparison cities provides the best available gauge of whether and how scheduling conditions would have changed in the absence of the Seattle ordinance, and allow us to isolate any effects of the law from general trends in work schedules unrelated to the law.

This report describes the experiences of 755 Seattle workers before the Secure Scheduling Ordinance took effect as well as the experiences of 624 Seattle workers after the Secure Scheduling Ordinance was in place for a short period. We compare the experiences of Seattle workers to that of 5,402 workers in comparison cities in the baseline period and 7,328 workers in comparison cities in the follow-up period. We use this survey data to estimate the impact that the Secure Scheduling Ordinance had on several dimensions of the work schedule experiences reported by the workers themselves.

Our findings over the short-term follow-up period include:

- The Secure Scheduling Ordinance significantly increased schedule predictability. For Seattle workers covered by the Ordinance, the share of workers receiving at least two weeks’ advance notice of their work schedules increased by 9.3 percentage points (a 20 percent increase compared with baseline).
- The Secure Scheduling Ordinance also increased predictability pay for schedule changes by about 7 percentage points (more than doubling the percentage of Seattle workers receiving pay for schedule changes compared with baseline).

Over the short-term follow-up period, our study did not detect changes on other dimensions of work schedules such as the experience of on-call, clopening, canceled shifts, or number of weekly work hours.

Worker awareness of the Secure Scheduling Ordinance increased between baseline and follow-up but many workers reported they were not aware of the Ordinance. About 44% of Seattle workers overall and 35% of workers who speak a language other than English at home reported that they had heard about Seattle’s “Secure Scheduling Ordinance” in the follow-up survey.

## Research Questions

Seattle’s Secure Scheduling legislation aims to increase schedule predictability by requiring two weeks of advance notice, to increase stability of schedules by requiring employers to pay predictability pay for schedule changes, to increase the amount of rest in between shifts by requiring extra pay for closely spaced shifts, and to increase access to sufficient work hours by requiring existing workers to be offered more hours before new hires are made. In the short-term follow-up period examined in this report, we focus on impacts on a set of scheduling outcomes that align with these provisions.

This report addresses the following set of research questions:

- Does secure scheduling legislation lead to an increase in the proportion of workers who receive two weeks' notice of their work schedule?
- Does the legislation reduce on-call shifts, clopening shifts, and last-minute shift cancellations and schedule changes?
- Does the legislation increase the receipt of pay to compensate workers for on-call schedules, schedule changes, shift cancellations, or clopening shifts?
- Does the law increase "access to hours" by increasing offers of additional work hours before new workers are hired?
- Does the legislation affect the usual number of weekly work hours?

If the Secure Scheduling Ordinance improves schedule predictability, stability, the right to rest, and access to hours, over a longer term follow-up period the Ordinance may have impacts on worker health and well-being and benefits for workers' children. Future reports with a longer follow-up period will examine some of these longer-term outcomes.

## Data and Methods

### Data Collection

Because no existing data sources contain the measures of work scheduling that are necessary to evaluate the Secure Scheduling Ordinance (which we will refer to as the "SSO"), it was necessary to collect new data but collecting data from a large sample of covered workers in Seattle was challenging. There was no readily available contact list for covered workers from which to sample. To identify and recruit samples of covered workers before and after the SSO went into effect, our research team compiled a list of covered employers, then harnessed the advertising infrastructure of Facebook and Instagram to deliver targeted advertisements to workers employed by particular retail, food service, or casual dining establishments known to be covered by the new scheduling regulations. These advertisements invited workers to our online survey, which was designed to capture key scheduling outcomes.

Although the survey recruitment approach means we are unlikely to reach workers without any internet access, a recent study finds a high level of digital saturation in Seattle. Across City Council districts between 93 and 97 percent of households have internet access and between 92 and 95 percent of households have smartphones or mobile phones (City of Seattle, 2018). Details on the Facebook targeted advertising platform and process are explained in Supplementary Materials II-A.

### Identifying Seattle Covered Workers and Comparison Workers

We focus our data collection and limit our sample to workers paid by the hour and to workers employed by a large retail or food service establishment that fall under the coverage of the SSO for any of their locations in Seattle City limits. We identify workers employed in Seattle city limits or in one of the designated comparison city locations through a combination of information collected from survey reports and a process of geocoding.

For survey respondents in Washington State, our survey instrument asked workers to self-report whether their workplace was within Seattle city limits. Those who responded affirmatively and whose employers were of the size and type that would be covered by the SSO were identified as covered by the SSO. We determine this coverage by cross-referencing the named employer against a list of covered employers assembled from City Business Records, Hoovers data, and data from the Reference U.S. Database.

Our comparison group consists of workers who are paid hourly and employed at the same set of employers as the Seattle treatment group but live in a city other than Seattle. We also limit the comparison sample to large cities that have a minimum wage higher than the Federal minimum wage, as is the case in Seattle. In this way, we are comparing Seattle to cities with similarly progressive labor policy environments and we are comparing among workers who all have access to minimum wages higher than the Federal minimum wage.

For our comparison sample, we only include metropolitan areas with at least 1 million people, so we are comparing Seattle workers to other large urban areas. The comparison cities include Baltimore, Boston, Buffalo, Chicago, Cleveland, Columbus, Denver, Detroit, Hartford, Jacksonville, Los Angeles, Miami, Minneapolis-St. Paul, Orlando, Phoenix, Portland, Providence, Riverside, Rochester, Sacramento, San Diego, St. Louis, Tampa, and Washington D.C.

<b>Box 1. Identifying Seattle Covered Worker and Comparison Workers</b>	
<b>Definition of Secure Scheduling Ordinance Coverage</b>	<b>Survey Measures</b>
<p><b>Employee coverage:</b> Hourly employees who work, or report to work, at a fixed point of sale location in Seattle for 50% of the services provided to the employer.</p> <p><b>Employer coverage:</b> Retail and food services establishments with 500+ employees worldwide; and Full service restaurants with 500+ employees and 40+ full-service restaurant locations worldwide.</p>	<ul style="list-style-type: none"> <li>■ What is the name of your main employer?</li> <li>■ Are you paid by the hour at [EMPLOYER NAME]?</li> <li>■ Is your [EMPLOYER NAME] workplace located in Seattle city limits?</li> </ul> <p><i>Assessed based on employer name and Reference USA database</i></p>

We intentionally do not include in the comparison sample any of the small number of cities that already had scheduling laws in place in 2017 or 2018.

Our final analysis sample includes 755 covered workers in Seattle at baseline and 624 covered workers in Seattle following implementation. Our baseline sample size was larger than at follow-up because of the availability of supplementary funds at baseline. The sample characteristics and rate of response to survey recruitment advertisements were similar at baseline and follow-up.

Because rates of turnover in retail and food service sectors are high, our follow-up survey sample includes a combination of some workers who were surveyed at baseline and again in the short-term follow-up period, and some new workers who were only surveyed during the follow-up period. Of the 624 workers surveyed at follow-up, 146 were also interviewed at baseline. Our comparison sample includes 5,402 workers at baseline and 7,328 workers following implementation. Of those, 576 were also interviewed at baseline.

With these sample sizes, we can calculate what is known as the “minimum detectable effect size,” which is the smallest-sized policy impact that our study has the statistical power to detect. Given our sample sizes and research design, our study has the statistical power to detect an impact of the SSO that is 5.5 percentage points or larger in size. For instance, if the SSO increased advance notice of work schedules by 6 percentage points, we would be reasonably confident that the SSO had a positive effect. If, however, the SSO increased advance notice by 3 percentage points, we would not rule out the possibility that the impact occurred by chance.

The data for this study are from a non-probability sample and as such may differ from the broader population of Seattle workers covered by the Secure Scheduling Ordinance. Similarly, the comparison sample may differ from the broader population of workers in those comparison cities. To address the possibility of systematic differences between the workers in our sample and the broader population on observed characteristics, we construct and apply survey weights that align the characteristics of our sample with the broader population of workers represented in the American Community Survey. We describe the construction of these weights in Supplementary Materials II-B, along with our approach for addressing missing values.

## Measuring Secure Scheduling

To design the survey questionnaire, we drew on, and when necessary adapted, measures from validated survey instruments. We also consulted with Seattle’s Office of Labor Standards and City Council members on our questionnaire before beginning our data collection in the Spring of 2017. After collecting baseline data and presenting our baseline report to stakeholders in Seattle, we made some additions to our questionnaire to capture whether workers reacted positively or negatively to scheduling changes.

The resulting survey questions are specifically tailored to align with the provisions of the SSO and phrased in such a way as to be easily understandable and to be reasonably easy to answer in terms of recall and specificity.

**ADVANCE NOTICE.** We ask respondents the amount of advance notice they are given of their work schedule: “How far in advance do you usually know what days and hours you will need to work at [EMPLOYER NAME]?” We coded their responses into categories of “received 2 or more weeks’ notice of work schedule”, or “received less than 2 weeks’ notice.”

**ON-CALL SHIFT.** Workers were asked if they worked an on-call shift in the past month: “In the past month or so, have you ever been asked to be “on-call” for work at [EMPLOYER NAME]? By “on-call”, we mean you have

to be available to work, and you find out if you are needed to work just a few hours before your shift.” Those workers who did work an on-call shift were then asked if they were called in for work and, if they were not, were asked whether or not they were compensated for their time. We use this information to code two separate outcomes: one measures whether a worker was asked to be on-call, and the other measures whether a worker was asked to be on-call but then did not work and did not get paid.

**CANCELLED SHIFT.** Workers were also asked if they ever experienced a shift cancellation in the past month: “In the past month or so, did your employer ever cancel one of your scheduled shifts at [EMPLOYER NAME]?”. Those that did were asked if they received compensation for the cancelled shift. We then analyze a three-category outcome: no shift cancellation, shift cancellation with pay, and shift cancellation without pay.

**CLOPENING SHIFT.** A clopening shift refers to a back-to-back closing then opening shift without sufficient time to rest in between. Workers were asked: “In the past month or so, have you ever worked a closing shift and then worked the very next opening shift with less than 11 hours off in between your shifts at [EMPLOYER NAME]? This is sometimes called “clopening.” Those that did were asked if they received compensation for this short duration between shifts. We examine whether workers ever worked a clopening shift in the prior month and whether they worked a clopening shift without receiving extra compensation for the closely spaced shifts.

**SHIFT TIMING CHANGES.** Workers were asked if they ever experienced a change in the timing of their shift in the past month: “In the past month or so, did your employer ever change the timing or the length of your scheduled shift at [EMPLOYER NAME]? For example, your employer asked you to come in early or late, or asked you to leave early or to stay later than the hours you were originally scheduled for.” Workers who had experienced such a change were then asked whether they received any extra compensation for the change. We analyze a three-category outcome: no change in the timing of shifts, shift changed with pay, and shift changed without pay.

**PART-TIME WORKERS OFFERED MORE HOURS BEFORE NEW HIRES.** To capture potential impacts of the access to hours provision, workers were asked whether part-time workers were first offered additional hours before new workers were hired by their employer in a set of two questions: “In the past month or so, has your employer hired any new employees to do work that is similar to the job you do at [EMPLOYER NAME]?”. Then, “Did your employer offer current employees more hours first, before hiring the new employee or employees at [EMPLOYER NAME]?”. We then examine an outcome measuring whether new workers were hired without offering hours to existing workers first.

**USUAL WEEKLY WORK HOURS.** Workers were asked to report on their usual weekly work hours: “How many hours per week do you usually work at [EMPLOYER NAME]?”. The SSO does not regulate usual weekly work hours, but some SSO provisions could have an indirect effect on usual weekly hours.

For the preceding outcome measures, we collected data for all workers – in Seattle and comparison cities, before and after the SSO went into effect. A few additional outcomes were measured for select workers, as explained below.

**KNOWLEDGE OF SSO.** We asked Seattle workers only about their knowledge of the Secure Scheduling Ordinance before and after implementation. The question asked: “Have you heard anything about the following recent Seattle ordinances?” and included the “Secure Scheduling Ordinance.” Workers could respond “yes” or “no.”

**SCHEDULE CHANGE OVER PAST FEW MONTHS.** In the follow-up survey only, workers in Seattle and comparison cities were asked to report on how their schedule may have changed recently: “Over the past few months, would you say that overall your work schedule at [EMPLOYER NAME] has gotten better, stayed the same, or gotten worse?”

**HOURS CHANGE OVER PAST FEW MONTHS.** In the follow-up survey only, we also asked workers to report on how their number of hours may have changed recently: “Over the past few months, has your number of work hours at [EMPLOYER NAME] increased, stayed the same, or decreased?” Although this measure does not distinguish between changes driven by employers and workers, the majority of workers in our survey reported a desire for more weekly work hours.

Box 2 summarizes the key provisions of the SSO and the survey measure designed to capture the worker experience related to each provision. As shown in the table, the provisions of the law were nuanced. Although the survey measures were aligned as closely as possible with the provisions, it was not possible to capture all the nuances and there were some slight differences between the survey measures and the provisions of

the law. For clopening shifts, the SSO requires extra pay when back-to-back closing and opening shifts are scheduled with less than 10 hours of rest in between, but the survey measure asked about when these shifts occurred with less than 11 hours of rest in between. In a more nuanced example, the access to hours provision stipulates that available hours should be posted for a minimum of 3 days. The survey measure does not capture the number of days that available shifts were posted, because this level of detail would be difficult for workers to accurately report.

<b>Box 2. Secure Scheduling Provisions and Survey Measures</b>	
<b>Secure Scheduling Provisions</b>	<b>Survey Measures</b>
<b>Advance notice of work schedule</b>	
Employers must post employees' work schedules 14 days in advance.	How far in advance do you usually know what days and hours you will need to work at [EMPLOYER NAME]?
<b>Right to rest between work shifts</b>	
Employers cannot schedule a closing and opening shift (i.e. "clopening") separated by less than 10 hours unless an employee requests, or consents, to such hours.	In the past month or so, have you ever worked a closing shift and then worked the very next opening shift with less than 11 hours off in between your shifts at [EMPLOYER NAME]? This is sometimes called "clopening."
Regardless of request or consent, employers must always pay time-and-a-half for the hours separated by less than 10 hours.	The last time this happened, how much were you paid for these shifts?
<b>Compensation for work schedule changes</b>	
Additional hours: If an employer adds hours to the employee's schedule after it is posted, the employer must pay the employee one additional hour of pay at the scheduled rate. For each employer-addition of less than one hour, the employer may pro-rate the additional compensation due.	In the past month or so, did your employer ever cancel one of your scheduled shifts at [EMPLOYER NAME]?  The last time this happened, how far in advance did you find out about the shift being canceled at [EMPLOYER NAME]?  The last time this happened, how much were you paid for the cancelled shift?
Subtracted hours: If an employee is scheduled for a shift and then sent home early, the employer must pay the employee for half of the hours not worked.	In the past month or so, did your employer ever change the timing or the length of your scheduled shift at [EMPLOYER NAME]? For example, your employer asked you to come in early or late, or asked you to leave early or to stay later than the hours you were originally scheduled for.
Grace Period: Additions or Subtractions of less than 15 minutes do not incur additional compensation.	The last time this happened, how far in advance did you find out about your shift getting changed?
On-Call Protections: If an employee is scheduled for an on-call shift and is not called-in, the employer must pay the employee for half of the hours not worked.	The last time this happened, how much were you paid?  In the past month or so, have you ever been asked to be "on-call" for work at [EMPLOYER NAME], but then your employer did not need you to work?  The last time this happened, how much were you paid for being "on-call"?
<b>Access to hours for existing employees</b>	
Before hiring external employees, employers must offer additional hours of work to existing employees, subject to certain exceptions. Employers must post notice of additional hours for three days and allow existing employees two days to consider job offers.	In the past month or so, has your employer hired any new employees to do work that is similar to the job you do at [EMPLOYER NAME]?  Did your employer offer current employees more hours first, before hiring the new employee or employees at [EMPLOYER NAME]?

Source: <http://www.seattle.gov/laborstandards/ordinances/secure-scheduling>

## Analytic Methods

The data collection approach of surveying workers before and after the Ordinance took effect, in Seattle and in comparison places, allows us to estimate the Ordinance's effects using a difference-in-differences approach.

The difference-in-differences approach estimates the change over time experienced by Seattle covered workers, before and after the SSO took effect. But the approach goes a step farther, and also considers the change over time experienced by workers elsewhere – in comparison cities – over the same period.

Finally, by comparing the change experienced by Seattle workers to the change experienced by workers in comparison cities, the difference-in-differences method determines whether the Seattle covered workers experienced significantly greater changes in outcomes. The difference between the change experienced by Seattle workers and the change experienced by workers in comparison cities is the estimate of the impact of the SSO.

We estimate these difference-in-differences models using the common statistical approach of linear multivariate regression analysis. For our outcome variables that are dichotomous – such as received two weeks' advance notice of work schedule (or not), experience on-call work (or not), and worked a clopening shift (or not) – we estimate linear probability models. These models are recommended for regression models like ours that include interaction terms and also have an easier interpretation than alternative approaches such as logit or probit models. For continuous outcome measures, such as usual weekly work hours, we use ordinary least squares (OLS) regression as is appropriate for continuous outcomes. For our outcomes that capture three separate categories – such as shift canceled with pay, shift canceled without pay, or shift not canceled – we use multinomial logistic regression models, then estimate predicted values for each group of workers.

As is conventional in regression analysis, we take into account and control for a set of demographic characteristics composed of age, race/ethnicity, gender, educational attainment, school enrollment, marital status, and presence of children in the household. We also control for managerial status and industry subsector (retail apparel, cafe, casual dining, department or big box store, fast food, grocery, hardware, health and beauty, and miscellaneous retail).

## Sample Characteristics

In Table 1, we describe the characteristics of our sample of workers in Seattle and Comparison cities. Just over half of Seattle workers are between 18 and 29 years old and just over half identify as White. About 4 percent of Seattle workers describe themselves as Black or African-American, about 16 percent as Hispanic, about 14 percent Asian, and about 12 percent as other or multi-racial. Just over half the sample are female. In terms of educational attainment, roughly one-third of Seattle workers have no more than a high school degree, more than a third have some college education, and less than a third have a college degree. About one-third are enrolled in school. Almost half of Seattle workers are married or living with a partner, and about one-quarter are parents. The Seattle sample includes some managers, but all of the managers included in the sample are paid hourly and subject to the SSO. The Seattle workers span several industry subsectors including retail apparel, cafes, fast food, casual dining, and grocery stores.

In Table 1 and in all of our subsequent analyses, we apply survey weights, derived from the American Community Survey, which are designed to make our sample align with the characteristics of the broader populations of retail and food service workers employed in Seattle. These survey weights are described further in Supplementary Materials II-B.

As shown in Table 1, the Seattle and Comparison samples are fairly closely aligned on their characteristics but there are some differences, for instance, in the share of workers in different sectors of the retail or food service industry. The regression models take into account and control for these differences when generating impact estimates.

<b>Table 1. Weighted Descriptives for Seattle and Comparison Group Before and After SSO</b>				
	<b>Seattle</b>		<b>Comparison</b>	
	<b>Pre (Spring 2017)</b> %	<b>Post (Spring 2018)</b> %	<b>Pre (Spring 2017)</b> %	<b>Post (Spring 2018)</b> %
<b>Age</b>				
18-29 years of age	53.3	52.1	60.3	47.9
30-39	21.0	22.4	21.2	20.0
40-49	13.8	13.2	13.0	14.0
50+	12.0	12.2	5.5	18.1
<b>Race</b>				
White	52.3	56.0	52.8	54.5
Black	3.5	4.1	3.9	3.4
Hispanic	15.9	16.4	20.9	19.8
Asian	15.4	12.0	9.9	9.4
Other or multi-racial	12.8	11.4	12.5	12.9
<b>Gender</b>				
Female	54.8	55.7	55.4	56.5
<b>Education</b>				
High School or Less	33.0	33.5	35.9	34.7
Some college	39.3	42.2	43.2	41.7
BA+	27.7	24.3	20.8	23.6
Enrolled in school	30.5	34.2	40.6	32.4
<b>Marital and Parental Status</b>				
Married	24.9	24.4	20.4	27.3
Living with partner	23.5	23.8	22.4	22.1
Not living with partner	51.7	51.8	57.2	50.7
Has kids	25.5	27.1	24.3	32.1
<b>Managerial status</b>				
Is a manager	18.6	15.6	20.7	19.1
<b>Industry Subsector</b>				
Apparel	14.4	14.0	5.5	10.7
Cafe	14.5	13.2	8.7	5.4
Casual Dining	5.5	3.0	4.7	4.7
Dept/Super Store	9.1	11.1	12.4	14.2
Fast Food	12.9	14.5	34.4	27.1
Grocery	31.0	28.9	16.1	14.8
Hardware/Paint	3.6	6.9	3.9	6.6
Health/Beauty	2.3	1.0	4.3	6.3
Misc. Retail	6.9	7.3	10.0	10.2
<b>N</b>	755	624	5,402	7,328

## Results

The results from our difference-in-differences analyses are presented in Tables 2 through 8. Each of these tables presents adjusted means for Seattle workers before and after the SSO took effect and for a comparison group of workers employed in cities with minimum wage ordinances before and after the SSO took effect in Seattle. The estimated impact of the Secure Scheduling Ordinance is also shown. All of these results are weighted to the characteristics of the broader population of Seattle workers represented in the American Community Survey.

### Advance Notice

Table 2 displays the impact estimate for the outcome “two weeks advanced notice.” The SSO increased the proportion of workers receiving at least two weeks’ advanced notice of their work schedules by 9.3 percentage points. This 9.3 percentage point increase reflects a 9.1 percentage-point increase in advance notice experienced by Seattle workers, combined with a 0.2 percentage-point decrease in advance notice experienced in the comparison cities. (Given that 46 percent of Seattle workers had at least two weeks’ notice at baseline, this effect represents a 20% increase in receipt of advance notice.)

<b>Table 2. Two Weeks’ Notice of Work Schedule</b>			
	<b>Pre-SSO (Spring 2017)</b>	<b>Post-SSO (Spring 2018)</b>	<b>Post-Pre Difference</b>
Seattle Workers	45.7	54.8	9.1
Workers Employed in Comparison Cities with Minimum Wage Ordinances	45.7	45.5	-0.2
<b>Estimated Impact of SSO</b>			<b>9.3**</b>

Notes: \*\* p<0.01. Pre-SSO and Post-SSO columns display adjusted mean values derived from difference-in-differences regression models that control for age, race/ethnicity, gender, education, school enrollment, parental status, managerial status, and industry subsector.

### On-Call Shifts

In the top panel of Table 3, we see that 25 percent of Seattle workers reported an on-call shift at baseline and a similar proportion (24 percent) reported an on-call shift at follow-up. On-call shifts were a bit more common in comparison cities both before and after the SSO took effect. In the lower panel of the table, we see that about 8 or 9 percent of workers were asked to keep their schedule open for an on-call shift but did not work or receive any pay. There was little change over time, and little difference between Seattle and the comparison cities.

<b>Table 3. On-Call Shifts and On-Call Shifts without Work or Pay</b>			
	<b>Pre-SSO (Spring 2017)</b>	<b>Post-SSO (Spring 2018)</b>	<b>Post-Pre Difference</b>
<b>On-call shift</b>			
Seattle Workers	25.1	24.0	-1.1
Workers Employed in Comparison Cities with Minimum Wage Ordinances	26.7	25.8	-0.9
<b>Estimated Impact of SSO</b>			<b>-0.2</b>
<b>On-call shift and not called or paid</b>			
Seattle Workers	8.1	9.0	0.9
Workers Employed in Comparison Cities with Minimum Wage Ordinances	9.3	8.6	-0.7
<b>Estimated Impact of SSO</b>			<b>1.6</b>

Notes: Pre-SSO and Post-SSO columns display adjusted mean values derived from difference-in-differences regression models that control for age, race/ethnicity, gender, education, school enrollment, parental status, managerial status, and industry subsector.

## Shift Changes

Table 4 presents results for employer-initiated shift changes, with and without extra pay. The SSO did not have a significant impact on shift changes overall, with the share of workers who did not experience a change increasing in both Seattle and comparison cities by similar amounts. However, the law did increase the chance that a worker received extra compensation for an employer-initiated shift change. The SSO increased the likelihood of compensation for shift changes by 7 percentage points. This represents more than doubling the share of workers who received extra compensation for a shift change.

Among those workers who experienced a change to their schedule, we asked how they felt about the change. A small majority (57%) reported feeling neutral (not happy or unhappy) about the change, and the rest were about evenly split between reporting being happy about the change (23%) or unhappy about the change (20%).

<b>Table 4. Shift Timing Change with and without Extra pay</b>			
	<b>Pre-SSO (Spring 2017)</b>	<b>Post-SSO (Spring 2017)</b>	<b>Post-Pre Difference</b>
<b>No shift timing change</b>			
Seattle Workers	24.9	33.2	8.3
Workers Employed in Comparison Cities with Minimum Wage Ordinances	26.2	33.7	7.6
<b>Estimated Impact of SSO</b>			<b>0.7</b>
<b>Shift timing change with pay</b>			
Seattle Workers	6.1	14.2	8.1
Workers Employed in Comparison Cities	5.8	6.7	0.9
<b>Estimated Impact of SSO</b>			<b>7.2**</b>
<b>Shift timing change without pay</b>			
Seattle Workers	69.0	52.6	-16.4
Workers Employed in Comparison Cities	68.1	59.6	-8.4
<b>Estimated Impact of SSO</b>			<b>-7.9*</b>

Notes: \*\* p=0.01; \* p<0.05. Pre-SSO and Post-SSO columns display adjusted mean values derived from difference-in-differences regression models that control for age, race/ethnicity, gender, education, school enrollment, parental status, managerial status, and industry subsector.

## Cloping Shifts

Table 5 displays impact estimates for cloping shifts overall and cloping shifts without extra pay. In comparison cities, cloping shifts increased over the follow-up period, but no such increase was experienced by Seattle workers. As a result, the SSO reduced cloping shifts by almost 3 percentage points and reduced cloping shifts worked without extra compensation by 4 percentage points, but neither of these impacts was statistically significant. Here, we see a hint of evidence that the SSO was having some of its intended effects on cloping shifts, but our sample size does not provide enough statistical power to be confident about this positive effect.

<b>Table 5. Clopening Shift and Clopening Shift without Extra Pay</b>			
	<b>Pre-SSO (Spring 2017)</b>	<b>Post-SSO (Spring 2018)</b>	<b>Post-Pre Difference</b>
<b>Clopening shift</b>			
Seattle Workers	37.9	36.0	-1.9
Workers Employed in Comparison Cities with Minimum Wage Ordinances	44.0	44.8	0.7
<b>Estimated Impact of SSO</b>			<b>-2.6</b>
<b>Clopening shift without extra pay</b>			
Seattle Workers	34.1	30.7	-3.4
Workers Employed in Comparison Cities with Minimum Wage Ordinances	41.5	42.1	0.6
<b>Estimated Impact of SSO</b>			<b>-4.0</b>

Notes: Pre-SSO and Post-SSO columns display adjusted mean values derived from difference-in-differences regression models that control for age, race/ethnicity, gender, education, school enrollment, parental status, managerial status, and industry subsector.

### Shift Cancellations

Table 6 examines shift cancellations. The percentage of Seattle workers that experienced a shift cancellation was around 15 percent before the SSO and about 18 percent after the SSO. The percentages were similar for workers in comparison cities (17 and 16 percent, respectively). Seattle workers experienced a slight uptick in shift cancellations while workers in comparison cities experienced a slight decline, but neither of these results is statistically significant at conventional levels. However, the percent of workers who experienced a shift cancellation with pay increased from 1% to 3.3%, a significant increase of 2.2 percentage points relative to comparison cities.

Among those workers who experienced a shift cancellation, a plurality (46%) reported feeling unhappy about the cancellation. Another 43% were neutral feeling about the change (not happy or unhappy), and just 11% reported being happy that their shift was cancelled.

<b>Table 6. Shift Cancellation with and without Extra pay</b>			
	<b>Pre-SSO (Spring 2017)</b>	<b>Post-SSO (Spring 2017)</b>	<b>Post-Pre Difference</b>
<b>Canceled shift</b>			
Seattle Workers	85.1	81.7	-3.4
Workers Employed in Comparison Cities with Minimum Wage Ordinances	82.5	84.4	1.9
<b>Estimated Impact of SSO</b>			<b>-5.3+</b>
<b>Canceled shift with pay</b>			
Seattle Workers	1.0	3.3	2.3
Workers Employed in Comparison Cities	0.9	0.9	0.1
<b>Estimated Impact of SSO</b>			<b>2.2*</b>
<b>Canceled shift without pay</b>			
Seattle Workers	13.9	15.0	1.1
Workers Employed in Comparison Cities	16.7	14.7	-2.0
<b>Estimated Impact of SSO</b>			<b>3.1</b>

Notes: \* p<0.05; + p<0.10. Pre-SSO and Post-SSO columns display adjusted mean values derived from difference-in-differences regression models that control for age, race/ethnicity, gender, education, school enrollment, parental status, managerial status, and industry subsector.

## Access to Hours

In Table 7 and 8, we examine two outcomes related to the access to hours provisions of the SSO: (1) whether new workers were hired before offering part-time workers more hours and (2) workers' usual weekly work hours. In Seattle and comparison cities before the SSO went into effect, about 40 percent of workers reported that their employer hired new workers before offering more hours to existing part-time workers. Over the follow-up, this percent slightly declined in Seattle and slightly increased elsewhere for an estimated 4 percentage point impact (not statistically significant). In Table 8, we see consistency over time and across place in usual weekly work hours, with workers reporting 31 or 32 hours of work per week on average. That is, we find no evidence of any positive or adverse effects of the SSO on usual work hours.

<b>Table 7. Hired Part-Time Workers without Offering Hours to Existing Part Time Workers</b>			
	<b>Pre-SSO (Spring 2017)</b>	<b>Post-SSO (Spring 2018)</b>	<b>Post-Pre Difference</b>
Seattle Workers	40.4	39.5	-0.9
Workers Employed in Comparison Cities with Minimum Wage Ordinances	39.1	42.5	3.4
<b>Estimated Impact of SSO</b>			<b>-4.2</b>

Notes: Pre-SSO and Post-SSO columns display adjusted mean values derived from difference-in-differences regression models that control for age, race/ethnicity, gender, education, school enrollment, parental status, managerial status, and industry subsector.

<b>Table 8. Usual Weekly Work Hours</b>			
	<b>Pre-SSO (Spring 2017)</b>	<b>Post-SSO (Spring 2018)</b>	<b>Post-Pre Difference</b>
Seattle Workers	31.5	31.4	-0.1
Workers Employed in Comparison Cities with Minimum Wage Ordinances	31.0	31.7	0.7
<b>Estimated Impact of SSO</b>			<b>-0.8</b>

As a robustness check, we also estimate SSO impacts using alternative comparison groups and with and without survey weights in Supplementary Materials II-C. For the most part, these results are consistent with those presented above. The impacts on two weeks' advance notice are the largest and the most robust impact of the Secure Scheduling Ordinance and range from an increase of 7 to 10 percentage points across comparison groups, with and without survey weights. Therefore, we find strong evidence that the Secure Scheduling Ordinance significantly increased the proportion of workers receiving at least two weeks' notice of their work schedule.

## Knowledge of the SSO and Perceived Schedule Changes

Table 9 presents some additional descriptive information on workers' knowledge of the Ordinance and workers' perceptions of how their schedules had changed over time. We find that knowledge of the Secure Scheduling Ordinance increased from 40 to 44 percent between the baseline and follow-up survey. The Ordinance was less likely to be familiar to workers who speak a language other than English for whom knowledge increased from 33 to 35 percent over the follow-up period. For those who reported being aware of the ordinance, the survey followed-up with a question that asked how they learned about the SSO. In the follow-up survey, these workers reported learning about the ordinance from their managers (40%), postings at work (36%), the media (39%), co-workers (23%), friends or family (13%), and the Office of Labor Standards (7%). These were also the most common sources of information about the SSO in the baseline survey.

In the follow-up survey, we asked workers whether their work schedules have gotten better or worse over the past few months. Table 9 tabulates responses for workers who had been working for the same employer for at least one year to capture changes in schedules from conditions changing rather than from taking a different job. In Seattle and in comparison cities, about 18-19 percent of workers report that their schedules had gotten better over the past few months. Seattle workers were slightly more likely to say that their schedule had stayed the same - 63 percent in Seattle and 61 percent in comparison cities - and slightly less likely to say their schedule had gotten worse- 19 percent in Seattle and 22 percent in comparison cities.

<b>Table 9. Additional Descriptives for Seattle and Comparison Group Before and After SSO</b>				
	<b>Seattle</b>		<b>Comparison</b>	
	<b>Pre (Spring 2017)</b>	<b>Post (Spring 2018)</b>	<b>Pre (Spring 2017)</b>	<b>Post (Spring 2018)</b>
	<b>%</b>	<b>%</b>	<b>%</b>	<b>%</b>
Knowledge of SSO	40.2	43.8	--	--
Knowledge of SSO among those who speak language other than English at home	33.3	35.2	--	--
<b>How did schedule change in past few months?<sup>1</sup></b>				
Got better	--	18.6	--	17.6
Stayed the same	--	62.9	--	60.7
Got worse	--	18.5	--	21.7
<b>How has number of work hours changed in past few months?<sup>1</sup></b>				
Increased	--	16.7	--	16.3
Stayed the same	--	62.7	--	62.1
Decreased	--	20.7	--	21.6

<sup>1</sup>Among workers employed at the same employer for one year or longer at time of follow-up survey.

We also asked workers whether the number of weekly work hours had increased or decreased over the past few months. A similar percentage of workers in Seattle and comparison cities reported that their work hours had increased over the past few months (16-17 percent), that their hours had stayed the same (62-63 percent), and their hours had decreased (21-22 percent). The access to hours provision of the Secure Schedule Ordinance was expected to increase weekly work hours for workers who wanted more hours, but this descriptive tabulation and the results in Table 8 do not find evidence that the Ordinance increased work hours in the short-term follow-up.

## Discussion

In 2017, Seattle joined San Francisco and became the second large U.S. city to pass legislation aimed at increasing the predictability of work schedules for hourly workers employed in the retail and food service sectors. The U.S. Department of Labor and the City of Seattle funded an impact evaluation to document the effects of the legislation on workers' schedules and hours. These impact results represent the effects of the legislation over a short-term follow-up period. A full accounting of the effects of the law would benefit from longer-term follow-up data.

Over the short-term follow-up period between 3 and 12 months after the Secure Scheduling Ordinance took effect, the law had a significant impact on increasing advance notice of work schedules. The legislation increased the proportion of workers that had at least two weeks' advance notice of their schedule by 9.3 percentage points, which represents a 20 percent increase compared with the period prior to the law going into effect. In this way, the Secure Scheduling Ordinance had its intended effect.

The Secure Scheduling Ordinance also had a positive impact on workers receiving extra compensation when their schedules were changed with little notice.

In other areas, we did not observe significant impacts of the Secure Scheduling Ordinance. We did not see large or significant changes in on-call shifts, clopening shifts, cancelled shifts, or shifts changes. We also did not see changes in usual weekly work hours.

In the short term, the impact study results show that these laws can significantly increase advance notice and increase compensation for last-minute schedule changes. Longer-term follow-up will reveal whether in Seattle these laws also eventually led to additional impacts such as a reduction in on-call or clopening shifts or an increase in work hours and whether the law affected other outcomes related to worker wellbeing.

## Supplementary Materials

### II-A. Data Collection Methodology

The lack of existing research on this set of work scheduling practices stems from a lack of available data. First, large-scale existing data sets do not generally measure schedule instability (PSID, ACS, CPS, NLSY79). Second, the few data sets that measure scheduling (NLSY97 for 2014 -2018 and GSS in 2016-2018) have insufficient power to study samples of retail workers, let alone those in a particular city such as Seattle. Finally, administrative data, such as UI records, that are sufficiently large to permit focusing on affected workers in particular cities, lack data on scheduling and outcomes of interest.

To address this gap in existing data, we use an innovative method of collecting web-based surveys from a population of low-wage service-sector workers. Our insight is that service sector workers can be effectively recruited to surveys through audience-targeted advertising on Facebook. Acting as an “advertiser,” we purchase and place ads in the newsfeeds of Facebook users who report working at the large retail and food establishments covered by the Ordinance, both in Seattle and in comparison cities. We used this approach to collect baseline and 1-year follow-up data in Seattle and comparison cities. Our project leverages the tools of “big data” to collect low-cost web-based surveys.

Using Facebook to collect survey data departs from traditional probability sampling and some have raised reasonable questions about such approaches (Groves, 2011; Smith, 2013). One potential concern arises from the sampling frame of Facebook users. In the recent past, both internet access and Facebook use has been confined to relatively narrow subgroups of the population, which tended to have relatively high socioeconomic status. However, internet access is now widespread in the United States among working aged adults. Recent estimates from the American Community Survey find that between 90-94% of working aged adults have a computer at home and 80-84% have broadband internet access at home (Ryan and Lewis, 2017). Among those who use the internet, the very large majority are active on Facebook – 79% overall and 86% of those 18-49 (Greenwood et al., 2016). The result is that 81% of Americans age 18-49 are now active on Facebook, far in excess of the percent of this population with landlines. Further, although people of color and low-income strata are less likely to have home computers and broadband access (Ryan and Lewis, 2017), Facebook use is nevertheless not especially stratified by demographic characteristics (Greenwood et al., 2016). In addition, unlike some online platforms, Facebook goes to some length to verify that each user account is associated with a unique identifiable person (Facebook, 2017).

Facebook has two other important advantages over both phone and address-based sampling. First, unlike phone and address based sampling, the Facebook profile is a portable and durable means of contact. Respondents can be reached by Facebook for survey recruitment whether at home or work, whether they have moved or have a long residential tenure, whether they change phone numbers or lose service. This represents a distinct advantage over conventional sampling frames.

Second, Facebook collects detailed data on the attributes of users that can be used by advertisers to target their campaigns quite precisely. Indeed, this capability is at the heart of Facebook’s business model. These attributes include standard demographics such as age and gender, locational attributes, interests, as well as information on schooling and employment. This last field permits us to deliver advertisements that are targeted to users who work at specific firms. Given the goal of assembling a data set that includes large samples of workers at each of a large number of firms, this targeting capability is very valuable.

Acting as an “advertiser,” we use Facebook’s audience targeting tools to purchase and place survey recruitment advertisements in the newsfeeds of Facebook users who work at specific companies. Each advertisement was targeted to employees of a specific company (or family of consumer-facing brands), in the 18-50 age range, who were located in the United States. The availability of targeting by employer name was a key feature that made this data collection approach viable for our research purposes.

A key advantage of this approach is that we cast a wide net in recruiting covered workers and comparison workers to our survey. Our sample is not selected on the basis of having existing relationships with community, business, or advocacy groups. We also avoid the potential for bias that might arise from recruiting workers in or near their place of employment where either the sample composition could be biased or reports could be biased by concerns over employer knowledge of participation.

Facebook provides several options for the “marketing objective” of the campaign. Our default approach, selected after consultation with advertising specialists at Facebook, is to set the campaign objective as “traffic,”

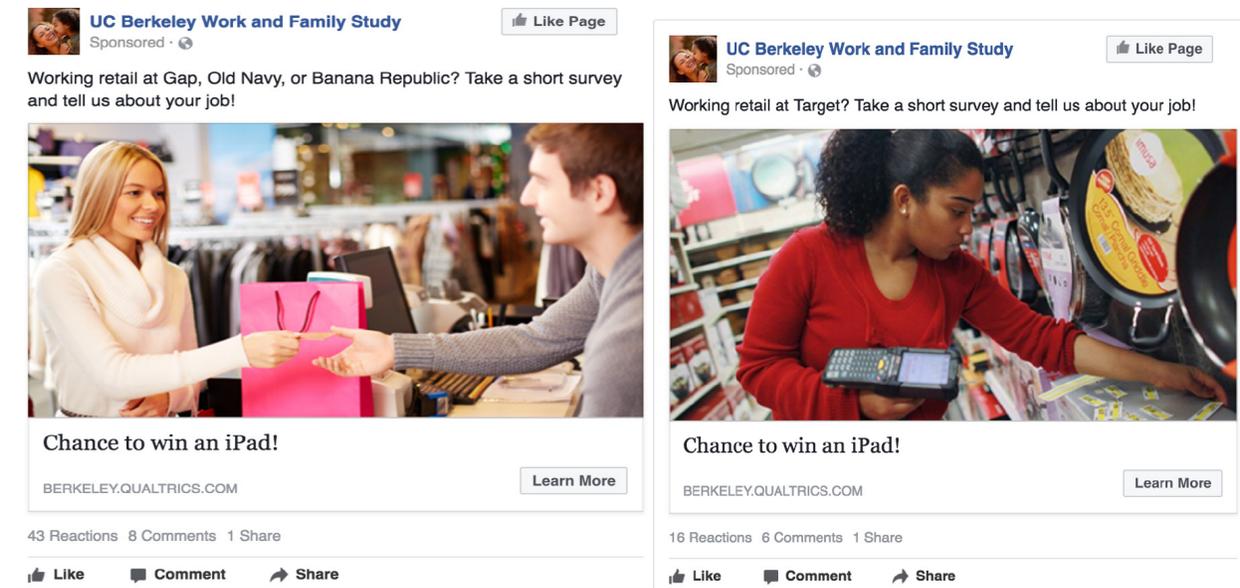
which equates with the goal of having Facebook users click the link embedded in the advertisement that takes them to our online survey.

Advertisements appearing on Facebook must follow a fairly standardized design, but there are options within that framework. For instance, while every advertisement must link to a Facebook page, include a headline and advertisement text, an image, and may include a link to an external webpage, advertisers have substantial discretion in crafting the advertising text, in choosing the content of the image, and in using a single image as opposed to a carousel, a video, a slideshow, or a collection.

We used a simple template for all of our advertisements. Every advertisement included a single image drawn from licensed stock photography available at no charge on the Facebook advertising page. We selected images that seemed to most closely approximate an employee of the target company at work, matching on store environment and color and style of employees' uniforms. Every advertisement linked to an "[Author's University] Work & Family Study" Facebook page that itself included very little additional content. For the data reported on in our main analysis, every advertisement used the "headline" field to offer users the opportunity to enter a drawing for an Apple iPad or to receive an electronic gift card of a modest incentive amount. Finally, again for the data in our main analysis, every advertisement used the advertisement text field to include a standard recruitment message. This message took the form of "Working at <targeted employer>? Take a short survey and tell us about your job!"

Finally, Facebook offers various options for advertisement placement. Advertisers may opt to have their advertisements appear on Facebook (in the newsfeed and/or in the right-hand column on desktop), on Instagram, or on partner networks. All of our campaigns were placed on Facebook in the newsfeed and on Instagram. In essence, Facebook serves as both the sampling frame and the recruitment channel.

Figure 1 presents some sample advertisements we have used to recruit workers to our survey. Users who click on the advertisement are routed to an electronic survey hosted by the firm Qualtrics. The survey can be accessed on desktop or mobile. Users are asked to consent and then begin the survey. Survey incentives include a drawing for an iPad, and \$5, \$10, and \$15 gift cards. These were updated in progressive order to attract respondents who were not recruited through prior advertisements.



### Supplementary Materials Figure 1

To collect re-interview data, we sent follow-up survey invitations to those who had responded to our baseline survey in Seattle and in comparison cities and who had provided valid email or phone contact information. These re-interview invitations offered the baseline respondents a \$5 gift card for completing a follow-up survey. Workers who did not respond to the initial invitation were sent reminders, and then were later offered incentives of \$10 or \$15 for completing a follow-up survey if they had not yet responded. Those invited to re-interview were given the option to opt-out of future text or email contacts, and anyone who opted-out was removed from the contact list.

The re-interview survey instrument asked workers if they were still working for the same employer. If they were, the survey collected updated information about their work schedules and work conditions. If they were no longer working for the same employer, the survey collected an update on the current employment status, and, when relevant, on their new job. Because not all baseline survey respondents provided valid contact information and only a portion of those invited to re-interview accepted the invitation, only about 20% of our follow-up sample in Seattle was in the baseline sample. The rest were new respondents. For the comparison sample, only a small share of about 4% were re-interviews and the remainder were new respondents.

## **II-B. Survey Weighting Methodology and Missing Data Imputation**

Our approach to survey data collection departs from traditional probability sampling methods. One possible source of bias arises from our sampling frame of Facebook users. However, recent estimates show that approximately 80% of Americans age 18-50 are active on Facebook (Perrin, 2015). Thus, the sampling frame is now on par with coverage of telephone-based methods (Christian et al., 2010).

A second source of bias arises from non-random non-response to the recruitment advertisement. To correct, we use a set of existing post-stratification and weighting methods. An emerging body of work in statistics and computational social science has demonstrated that nonprobability samples drawn from non-traditional platforms, in combination with statistical adjustment, yield similar distributions of outcomes and estimates of relationships as probability-based samples. This work has drawn data from Xbox users (Wang et al., 2015), Mechanical Turk (Goel et al., 2015; Mullinix et al., 2016), and Pollfish (Goel et al., 2015).

We use a similar approach to re-weight our data to recover the demographic characteristics of the employees covered by the SSO. We know of no data set that provides detailed demographic characteristics of workers at the specifically covered firms who work within Seattle city limits. The American Community Survey does permit us to home in on the population of employees in the covered industries and who work on an hourly basis. In addition to the large firms covered by the SSO, this population also includes those at smaller firms. The more significant problem is that publicly available Census products do not allow us to identify ACS respondents who work in Seattle City limits, only in the larger entity of King County. We overcome this problem by purchasing a custom Census tabulation that provides the demographic characteristics of workers in covered industries and occupations whose place of work is within Seattle City limits.

We use these tabulations from the American Community Survey to construct survey weights. When we apply these weights to our survey data, the attributes of our survey sample closely resemble those of the broader population of Seattle workers. Supplemental Table 1 presents the characteristics of Seattle workers derived from ACS custom tabulations alongside descriptives of our Seattle survey sample with and without survey weights applied.

We also apply these weights to each of our comparison groups. This ensures similarity on these relevant demographic characteristics between the two populations. Further, by weighting both the pre-treatment (baseline) and post-treatment (follow-up) samples to the same demographic benchmark, we reduce the potential for changes in the demographic composition of workers over time to bias the estimates.

The construction of survey weights was performed using the `calibrate` package in R. The descriptive statistics shown in Table 1 demonstrate that once the weights are applied, the demographic characteristics of Seattle and the Comparison samples are closely aligned.

In a separate working paper we describe a set of additional tests we performed to assess data validity, including comparisons of our non-probability survey data with probability samples collected by the Current Population Survey and the National Longitudinal Survey of Youth. These comparisons show that our survey data is more similar to either of these source than these two sources are to one another. This working paper also describes test of bias on unobservables that did not reveal evidence of bias (Schneider and Harknett 2019).

Because our survey was self-administered, some respondents break off before the end of the survey.

We organized our survey instrument so that the module on work scheduling outcomes was near the start of the survey to minimize missing data on scheduling experiences. Imputations were run using all observations of respondents who were in the Seattle sample or comparison groups at baseline or at follow-up. Missing variables were imputed both for questions that were seen but skipped and for questions that were unanswered due to survey attrition. Ten imputed data sets were generated using the 'Amelia' package in R (Honaker, King, and Blackwell 2011).

<b>Supplemental Table 1. American Community Survey (ACS) and Unweighted and Weighted Descriptives</b>			
	<b>ACS %</b>	<b>Seattle Unweighted %</b>	<b>Seattle Weighted %</b>
<b>Age</b>			
18-29 years of age	44.8	58.5	52.8
30-39	24.5	18.6	21.6
40-49	15.6	11.6	13.5
50+	15.0	11.3	12.1
<b>Race</b>			
White	57.9	57.3	54.0
Black	4.3	3.3	3.8
Hispanic	16.2	13.8	16.1
Asian	14.0	8.8	13.9
Other or multi-racial	7.7	16.8	12.2
<b>Gender</b>			
Female	45.4	65.5	55.2
<b>Education</b>			
High School or Less	33.0	33.9	33.3
Some college	39.2	42.7	40.6
BA+	27.8	23.5	26.2
Enrolled in school	--	33.3	32.2
<b>Marital and Parental Status</b>			
Married	--	23.1	24.7
Living with partner	--	25.1	23.6
Not living with partner	--	51.7	51.7
Has kids	18.4	28.7	26.2
<b>Language</b>			
English Second Language*	29.1	21.6	31.3
Speaks English less than "well"	8.6	--	--
Spanish-Speaker with Limited English	4.4	--	--
Chinese-Speaker with Limited English	2.3	--	--
Other language with Limited English	2.1	--	--
<b>N</b>	<b>49,980</b>	<b>1,379</b>	<b>1,379</b>

\* Refers to ACS category "speaks a language other than English at home" (but does not necessarily imply limited proficiency)

## II-C. Results for Alternative Comparison Groups

In our main results, as described above, our comparison group is comprised of large U.S. cities that have a minimum wage that is higher than the Federal minimum wage. We also estimate impacts for three alternative comparison groups and find results consistent with those we presented, including a significant increase in advance notice between 8 and 10 percentage points.

Supplemental Table 2 presents results for four separate comparison groups:

1. **Minimum Wage** is the comparison group presented in the report, consisting of large cities with a minimum wage higher than the Federal minimum.
2. **Scheduling Cities** includes hourly workers who were surveyed in metropolitan areas of at least 1 million people that are either soon to consider work scheduling legislation or who have recently considered such legislation, but narrowly failed to pass it. We discern this information from correspondence with city legislators and other actors in the policy space. We argue that these cities constitute an important comparison group because they likely share some of the same unobservables that select Seattle into passing scheduling legislation, but have not yet enacted the treatment. These CBSAs are Chicago, Philadelphia, Los Angeles, Minneapolis, and Washington DC.
3. **Scheduling Cities or States** expands the group of Scheduling Cities to also include metropolitan areas in states that are either soon to consider state-level scheduling legislation or who have recently considered such legislation, but narrowly failed to pass it. These cities are Chicago, Philadelphia, Los Angeles, Minneapolis, and Washington DC, Portland, San Diego, Boston, Sacramento, and Hartford.
4. **Near Seattle** includes hourly workers who were surveyed in the Seattle CBSA, are employed at the same set of retail and food establishments as the Seattle treatment group, but work outside of Seattle city limits and thus are not covered by the Ordinance.

In Supplemental Table 2, we first reproduce the DiD estimate from Main Tables 2-7. We then show the same estimate when using each of the alternative three comparison groups in the subsequent columns. In the top panel of Table 2, we present results with survey weights applied, and in the bottom panel we present unweighted results. Overall, the impact estimates are largely consistent across comparison groups and with and without survey weights. One exception is that estimated impacts on shift timing changes are larger for the Near Seattle comparison group than for the other comparison groups. When Seattle workers covered by the SSO are compared to their nearby counterparts working in the area but outside of Seattle city limits, we estimate a significant 8 percentage point decline in shift changes, and a 13 percentage point decrease in shift changes without extra pay. This provides some evidence that the SSO reduced schedule changes and increased compensation when last-minute schedule changes did occur.

<b>Supplemental Table 2. Estimated Impacts of SSO with Alternative Comparison Groups</b>				
	<b>Minimum Wage n=14,109</b>	<b>Scheduling Cities n=7,654</b>	<b>Scheduling Cities or States n=10,557</b>	<b>Near Seattle n=4,247</b>
<b>Weighted</b>				
Two Weeks' Notice	9.3**	10.0**	8.2*	8.6*
On-Call	0.0	-2.0	-0.2	-2.8
On-Call without Work or Pay	1.5	-0.3	0.7	-0.6
No Shift Timing Change	0.7	-0.5	-0.2	8.3*
Shift Change with Pay	7.2**	7.1**	6.9**	4.8
Shift Change without Pay	-7.9*	-6.6	-6.6	-13.1**
Cloping Shift	-2.6	-2.7	-1.9	-5.6
Cloping without Extra Pay	-3.9	-4.0	-3.4	-6.6+
No Cancelled Shift	-5.3+	-5.4+	-5.6*	-1.0
Cancelled Shift with Pay	2.2*	2.3+	2.4*	1.3
Cancelled Shift without Pay	3.1	3.1	3.2	-0.3
Offer hire	-4.2	-4.6	-4.6	-1.3
Usual hours	-0.8	-0.9	-0.7	-0.8
<b>Unweighted</b>				
Two Weeks' Notice	7.9**	9.0**	7.3*	7.4*
On-Call	0.0	-1.0	0.5	-2.0
On-Call without Work or Pay	1.4	-0.1	0.8	-0.1
No Shift Timing Change	1.0	-0.4	0.0	6.1
Shift Change with Pay	6.4***	6.5**	6.3**	5.1*
Shift Change without Pay	-7.4*	-6.1	-6.4	-11.2**
Cloping Shift	-2.5	-2.4	-1.4	-4.7
Cloping without Extra Pay	-4.8	-4.6	-3.8	-6.7*
No Cancelled Shift	-5.0+	-5.6*	-5.4*	-0.8
Cancelled Shift with Pay	2.3*	2.5*	2.5*	1.5
Cancelled Shift without Pay	2.7	3.1	2.9	-0.7
Offer hire	-2.0	-2.0	-2.4	1.0
Usual hours	-0.5	-0.7	-0.5	-0.6

Notes: \*\*p<.01; \* p<.05; + p<.10. Estimates from difference-in-differences regression models. Impacts are the interaction between Seattle treatment group and post-SSO period.

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