The Real Effects of Fair Workweek Laws on Work Schedules: Evidence from Chicago, Los Angeles, and Philadelphia

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Effective in eight jurisdictions and banned in four, Fair Workweek Laws (FWL) aim to increase the predictability and stability of work schedules. Among other requirements, these laws penalize employers for unilaterally adjusting work schedules without providing some pre-specified amount of advance notice. This penalty is proportional to the affected employee's wage, and is paid directly to the affected employee. In this paper, we study the effects of FWLs on worker schedules in Chicago, Los Angeles, and Philadelphia using administrative shift-level data covering multiple retailers. Consistent with their objectives of increasing work schedule predictability, we estimate significant increases in advance notice provided for covered employees. However, we estimate null effects on a series of variables that capture work schedule stability. Our analysis also rules out commonly raised concerns about FWLs, such as: (i) a reduction in scheduled work for employees, (ii) increased employee turnover, (iii) decreased employee hiring, (iv) an increased use of part-time employees, and (v) the possibility that these laws disproportionately benefit higher-wage workers, since penalties are a function of the underlying employee's wage. Overall, while FWLs are effective in reducing the number of short-notice shifts (but not completely), they are ineffective in increasing the stability of work schedules.

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Disclaimer: This paper makes use of proprietary administrative data on various companies. The conclusions drawn from the data are those of the researchers and do not reflect the views of any companies examined in this paper. These companies are not responsible for, had no role in, and were not involved in analyzing or developing the hypotheses or results reported herein. The corresponding author of this paper received financial compensation for consulting work from some suppliers to the companies examined in the paper. This author has not received compensation from the companies examined in the paper.

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1. Introduction

An increasing number of businesses are using "flexible" scheduling practices that provide employees with little notice of their schedules. Data from the American Time Use Survey by the US Bureau of Labor Statistics show that almost 20% of employees in 2019 received their schedules with less than one week's notice. Although these scheduling practices allow businesses to schedule employees only when they are needed and with the most up-to-date information about demand, they can also have negative effects on workers. Previous research has found that unpredictable work schedules are related to lower levels of employee well-being (Ananat et al. 2022; Ben-Ishai 2015; Harknett et al. 2021; Henly and Lambert 2014; Lambert et al. 2019a). Thus, while flexible scheduling practices may benefit businesses, they can make it difficult for workers to plan their lives around their employment.

In response, policymakers in several cities and states in the United States have passed legislation called Fair Workweek, Predictability Pay, or Predictable Scheduling laws ("FWLs") to protect workers from unpredictable and unstable work schedules. Among other requirements[1], these laws require employers to compensate employees who have their work schedules unilaterally adjusted without being provided sufficient advance notice. As of August, 2023, San Francisco, Emeryville (California), New York City, Philadelphia, Oregon (statewide), Seattle, Los Angeles, and Chicago have FWLs in effect. The popularity of FWLs is growing, with Congress and at least 10 other jurisdictions, including the states of Colorado and Massachusetts, having formally proposed FWLs in their legislatures[2].

However, despite the growing popularity of FWLs in the U.S., their impact on worker schedules remains poorly understood. To the best of our knowledge, no empirical papers across multiple disciplines interested in FWLs have explored their effects on work schedules, other than a handful that use survey data.[3] The rate at which FWLs are gaining popularity (for and against) may not be commensurate with the pace of research investigating their impact on work schedules. The lack of empirical evidence may also have contributed to differing policy responses; for example, while eight jurisdictions have adopted FWLs, four states have laws banning FWLs within their entire state.[4] In this paper, we aim to fill this void.

[1]These laws include all or some of the following requirements (depending on the jurisdiction): advance scheduling notice, good faith estimates of working hours (at the time of hire), stable scheduling, predictability pay, greater access to hours, and anti-retaliation measures. See Mitchell et al. (2021) and the citations therein for more details on these requirements along with a comparison between jurisdictions

[2]Beyond the aforementioned cities and states that have passed such laws, at least 10 other cities and states including Colorado (HB 23-1118), Connecticut (HB5353), Maine (HP 0973, LD 1345), Michigan (HB 5136), Minnesota (SF 0736), New Jersey (NJ S921), North Carolina (H 366), Rhode Island (H 7515), and Massachusetts (S 1236) have formally proposed FWLs for consideration. Finally, there is also interest at the federal level with the Schedules that Work Act (H.R. 6670).

[3]Two key concerns with surveys are nonrandom selection and recall bias (i.e., surveyed employees do not accurately recall the exact contents of their past work schedules).

[4]Arkansas, Tennessee, Iowa, and Georgia.

Using administrative data covering multiple retailers subject to FWLs in Chicago, Los Angeles, and Philadelphia, this paper addresses three key questions on the effects of FWLs on work schedules. First, we address whether FWLs achieve their goal of increasing work schedule predictability. While not having a widely agreed upon definition or metric, "schedule predictability" can be understood as how far in advance workers know about when, and how long, they are scheduled to work. The ability to increase work schedule predictability is a key selling point of FWLs: Policymakers and advocates of FWLs often argue that such laws will reduce the frequency and magnitude of unpredictable schedules for shift-based workers. For example, the Commissioner of the NYC Department of Consumer and Worker Protection, Lorelei Salas, advocated for New York City's FW law by stating that *"Fast food and retail workers endure unpredictable schedules and incomes that make it hard for them to create budgets, schedule child care, or pursue education or a second job."*.

However, because firms affected by FWLs fully retain the ability to schedule their employees on short notice by paying out the required penalty to the affected worker, it is unclear whether FWLs promote predictable schedules or increase worker compensation. In other words, FWLs ultimately give covered businesses two choices: Make work schedules predictable or bear the costs of providing unpredictable shifts. However, it is not *a priori* clear what covered firms will choose. For instance, firms that struggle to forecast demand accurately may continue to provide unpredictable schedules, giving them extra time to forecast demand better (despite the penalties). Therefore, FWLs may, in practice, behave similarly to minimum wage laws for some employers, rather than achieving their objectives of improving the predictability of work schedules. Further complicating this issue of the potential effectiveness of FWLs is the fact that only *unilteral* adjustments are penalized. This means that any adjustments agreed upon by the employer and the employee are not subject to penalties, potentially opening a loophole for employers to pressure workers into accepting short-notice schedule changes. Accordingly, there is an open question of whether FWLs can increase the predictability of work schedules.

Second, we address whether FWLs achieve their goal of increasing work schedule *stability*. Like schedule "predictability", there is also no widely agreed-upon definition or metric of work schedule stability. However, in the context of FWLs, a work schedule is often said to be stable if there are minimal week-to-week variations in scheduled hours and start times.

Although current FWLs contain no clauses related to work schedule stability, FWLs are often argued to improve stability indirectly by incentivizing employers to carefully plan schedules ahead of time to avoid penalties, leading to more consistent hours and start times for employees. For instance, one Los Angeles council member, who was a proponent of the LFW, said the following about the LFW: "*...these new regulations will provide employees – many of whom are people of color and live paycheck to paycheck – predictability, stability, and flexibility in their work schedules*".

Similarly, the FWL proposed in Massachusetts states that it "*...allows workers in restaurants, retail establishments, and the hospitality industry the chance to build stable lives for themselves and their families, because for many, erratic work schedules make stability almost impossible.*" However, predictability and stability are two independent characteristics of work schedules, with penalties only levied on employers for failing to provide adequate advance notice of work schedules (the former), and not for the variability or inconsistency in the number of hours, shifts, or start times from one week to the next (the latter). As such, to argue that FWLs will also increase work schedule stability requires one to assume that predictability and stability are strongly correlated features, which is currently not empirically supported. Accordingly, there is an open question of whether FWLs can also increase the stability of work schedules.

Third, because FWLs target a core function of businesses – labor scheduling and staffing – there is an open question of whether FWLs may potentially have broader, and potentially negative, effects on workers and stores. For example, meeting minutes introducing FWLs often express concerns that such laws may reduce employee hours and take-home pay, as asking employees to stay longer due to unanticipated spikes in demand becomes more costly. For instance, Committee Chair Rep. Judy Amabile, D-Boulder, a business owner who recently voted against Colorado's FWL remarked that she worried about "*...unintended consequences that, in the end, could hurt the very people the bill is intended to help, as well as damage to the restaurant industry, which she said is still facing the effects of COVID-19, including worker shortages.*" Other potential unintended consequences include a reduction in new hires, increased employee turnover, and employers substituting full-time workers with part-time workers (von Wilpert 2017; Yelowitz 2022). Consistent with this latter concern, Sonia Riggs (CEO of the Colorado Restaurant Association) objected to Colorado's FWL by arguing "*...99% of restaurants will likely limit plans for future growth, 98% are likely to schedule fewer workers per shift, 92% are likely to cut employee hours, 95% are likely to stop hiring individuals who need flexibility in scheduling such as students and single parents, 97% of restaurants are likely to increase menu prices to offset the compliance burden*".

Finally, an additional concern is that FWLs may inadvertently offer greater protection to higher-paid workers, whose schedules are costlier to alter on short notice due to adjustment penalties being a function of the affected worker's hourly wage. This feature of the law may cause employers to shift the unpredictability and instability of work schedules from higher-paid to lower-paid workers, with no changes in the frequency and magnitude of unpredictable and unstable work schedules at the store-level. Overall, an open question remains whether FWLs can have unintended consequences in the form of job loss, a reduction in hiring, increased employee turnover, reduced hours for workers, or shift unpredictable and unstable work schedules from higher-wage to lower-wage workers.

Overall, despite the growing popularity of FWLs in the United States, their real effects on worker schedules remain largely unknown, with three key open questions about their potential effects at the worker and store-levels. This lack of evidence not only makes it difficult to quantify the effectiveness and benefits of FWLs, but may also amplify the reservations that some business associations and politicians have expressed against these laws.[5] In this paper, we aim to fill this void.

Data. We study the effects of Chicago's, Los Angeles', and Philadelphia's Fair Workweek Laws (henceforth, CFW, LFW, and PFW) by examining shift-level data covering multiple retailers. Our data is both large and granular: They contain information on *every* employee shift, from when the shift is first generated by an AI tool (elaborated on in Section 2) to any subsequent modifications that managers make in light of new information that results in a new optimal labor plan (e.g., news about a demand shock). This dataset not only allows us to approximate the amount of advance notice that employees receive for their work schedules, but it also enables us to analyze how specific work schedule adjustments, such as short-notice shift additions or cancellations, change in response to FWLs.

We pair this data with human resource records that reveal employees' wages, gender, and full- or part-time status. This dataset is critical for two reasons. First, it enables us to identify the stores and employees subject to each FWL (i.e., based on store size and employee characteristics such as their wage), which mitigates measurement error in our analyses. Second, it allows us to conduct heterogeneous effects analyses, which are essential in addressing concerns that FWLs may disproportionately protect higher-paid employees, given that penalties are pegged to their hourly wages.

Main Results. We employ a difference-in-differences design to compare changes in a series of variables related to work schedules and labor utilization (outlined below) between employees and stores located within and outside Chicago (for the CFW), Los Angeles (for the LFW), and Philadelphia (for the PFW) around the effective date of each FWL. Our identification assumption is that without the CFW, LFW, and PFW, worker and store-level variables would have evolved along parallel trends. We note that all three FWLs were *fully* anticipated by stores and their managers, as these laws were passed more than a year before the effective dates of each FWL. We discuss our identification assumptions and several caveats and concerns with our identification strategy in Section 4. We also discuss why Los Angeles' FWL provides us the "cleanest" estimates on the effects of FWLs due to contemporaneous events that occurred with Chicago and Philadelphia's FWL (e.g., the COVID-19 Pandemic).

Using this approach, we obtain several results on the effects of the FWL in Chicago, Los Angeles,

[5] For example, Arkansas, Georgia, Iowa, and Tennessee have laws that prohibit cities in their respective states from passing FWLs.

and Philadelphia, which we summarize in Table 1. Overall, our results suggest that while FWLs can effectively increase the predictability of work schedules, they do not necessarily increase the stability of work schedules (based on three proxies of work schedule stability). We also partially rule out some concerns raised in the public discourse surrounding FWLs, such as: (i) their potential negative effects on scheduled work for covered employees, (ii) weaker protections for workers with relatively lower wages, as penalties are specific to each worker based on their hourly wage, (iii) their potential to increase employee turnover, and (iv) their potential to cause employers to shift from full-time to part-time workers. One of these unintended consequences exists for Philadelphia (e.g., Hiring). We discuss some of the potential reasons for these estimated effects below.

Table 1. Summary of Effects on Work Schedules and Employment

Outcome	City	Result
Shift Advance Notice (Predictability)	Chicago	Increase (35%)
(The amount of advance notice in days)	Los Angeles	Increase (5.9%)
	Philadelphia	Increase (5.6%)
Shift Stability Measures (Stability)	Chicago	Null
(Three measures of work schedule stability)	Los Angeles	Null
	Philadelphia	Null
Scheduled Labor for Workers	Chicago	Null
	Los Angeles	Null
	Philadelphia	Null
Heterogeneous Effects on Scheduled Labor by Worker Characteristics	Chicago	Decrease for males
(Gender, Part-time Status, Wage)	Los Angeles	Null
	Philadelphia	Null
Effects on Hiring	Chicago	Null
	Los Angeles	Null
	Philadelphia	Decrease (-12.4%)
Effects on Employee Turnover	Chicago	Null
	Los Angeles	Null
	Philadelphia	Null

Notes: The table above summarizes our results from Chicago, Los Angeles, and Philadelphia in our main samples (See Section 4). A "null" result implies that the effect is *not* statistically significant at the 5% level.

Limitations. While we offer the first non-survey based empirical evidence on the efficacy of FWLs in improving the predictability and stability of work schedules, our results are subject to a series of caveats and limitations concerning its internal and external validity. These limitations are critical when interpreting our results. First, our results for Chicago should be interpreted with considerable caution as Chicago's FWL was simultaneous with an increase in the city's minimum wage (from \$13 to \$14 per hour). We elaborate on the potential concerns associated with a contemporaneous minimum wage increase in Section 4. However, we note that only 16% of workers in our Chicago sample were paid the minimum wage before the CFW.

Second, the CFW's effective date of July 1st, 2020, coincided with the COVID-19 pandemic. This latter caveat is also relevant for our Philadelphia results, as the PFW became effective on April 1st, 2020. We discuss the potential challenges in interpreting the CFW and PFW's results due to the effects of the pandemic in Section 4. In brief, the main concern here is that the Pandemic resulted in increased demand and labor supply uncertainty, which may have also influenced our outcomes of interest (with an unknown sign or magnitude of effect). For instance, a source of bias that may make our Chicago and Philadelphia results more conservative is that due to demand uncertainty, managers were temporarily more willing to pay out penalties associated with scheduling employees on short notice. However, we cannot be definitive about the sign and magnitude of this particular effect, along with the potentially many scheduling-related variables affected by the Pandemic.

Despite this major contemporaneous event, we include the results from the CFW and PFW in our main results, as they may still offer some insights for policymakers in Chicago, Philadelphia, and elsewhere. For example, our results from the CFW and PFW may offer some insights into the potential effects of FWLs on stores that are subject to large demand and supply-sided uncertainties (e.g., stores that are experiencing high rates of turnover). These estimates may also be useful for policymakers in Chicago and Philadelphia interested in understanding the impacts of their respective FWLs within their cities. It is also important to note that any analyses of the FWLs in Chicago and Philadelphia are inescapable from the effects of the Pandemic.

Third, while our analysis uses novel administrative work schedule data that is significantly more granular and not subject to recall bias associated with surveys (elaborated on in the literature review below), several key limitations remain. For instance, we cannot definitively say whether an adjustment to a work schedule will trigger a penalty, even though there was less than k days' advance notice provided to the focal employee (e.g., 14 days in Los Angeles). This is because there are many instances where such short-notice adjustments are not penalized. For instance, short-notice adjustments across all three cities are not penalized if they are agreed upon bilaterally between the employer and the employee, which we cannot differentiate from unilateral changes to work schedules made by store managers. This is problematic because managers may shift from unilaterally giving some workers short notice (which is penalized) to scheduling other workers on short notice based on bilateral agreements (which is not penalized). Here, the data would show no changes to the aggregate magnitude of short-notice scheduling adjustments at the store-level. Accordingly, a comprehensive analysis would require data that reveals whether work schedule adjustments were made bilaterally or unilaterally between employees and managers.

Fourth, there are some issues with the external validity of our results based on our data sample. Specifically, all retailers in our sample are "large" (i.e., operate more than 300 stores) and are "sophisticated" as they use commercial AI tools to forecast demand and schedule labor. Accordingly, it is challenging to infer how "smaller" (but still large enough to be subject to FWLs) and "less sophisticated" retailers would respond based on our results. For instance, “larger" retailers may have more financial resources to be able to pay out penalties incurred by non-compliance with FWLs (in terms of making short-notice adjustments), and they may also possess advanced scheduling systems that minimize the need to make such adjustments in the first place. Smaller retailers may not have such luxury, which makes it difficult to say whether our results will also apply to such retailers.

Finally, our paper is also limited in scope as it examines only a partial list of outcomes that FWLs may impact. For example, while our paper considers the effects of FWLs on various measures of schedule predictability and stability, it does not explore their ultimate effects on worker well-being or satisfaction. Additionally, this paper does not examine the effects of FWLs on store operations or performance, which are crucial for assessing the welfare implications of such laws. We leave these analyses for future research.

Related Literature. This paper is related to several streams of literature from multiple disciplines that have either directly examined the effects of FWLs on work schedules or have used FWLs in their motivations to study the effects of work schedule policies on worker or store-level outcomes. The first stream relates to the former, which has used worker surveys to assess the impact of FWLs on work schedules in Seattle, Emeryville, and Oregon (Ananat et al. 2022; Haley-Lock and Schneider 2019; Harknett et al. 2021; Petrucci et al. 2022). The results in this set of papers have been mixed. Specifically, while the first three studies show some positive effects on worker schedules and employee well-being, the latter shows Oregon's FWL's limited efficacy in improving workers' schedules. An empirical challenge with these papers is that the results are wholly derived from employee surveys collected through advertisements or in-person interviews, making them susceptible to certain econometric issues, such as the representativeness and randomness of the survey respondents. There are also concerns about whether employees can accurately recall their work schedules, a challenge that is potentially exacerbated when their work schedules have been previously unstable and unpredictable. Finally, these results are subject to similar major limitations of our empirical results, described earlier in the introduction (e.g., identifying the exact number of short-notice adjustments their managers unilaterally made).

In contrast, our study provides empirical evidence based on shift-level data from administrative records covering all employees' shifts. This enables a more comprehensive analysis than previous studies that may have focused on a subset of workers or shifts or a limited number of outcomes. Furthermore, by examining the impact of FWLs at the shift-level, we can conduct a detailed analysis of how specific shifts (e.g., shifts for full-time workers versus part-time workers) are affected by the laws, including changes in the number of hours worked, shift timing, and work schedule predictability and stability.

More broadly, this paper is related to research on the impact of labor scheduling practices on employee outcomes (Dickson et al. 2018; Henly and Lambert 2014; Jang et al. 2012; Lambert 2008; Lambert et al. 2019b). Among these papers, a closely related paper to ours is Lambert et al. (2019b), which conducted experimental analyses using surveys to understand managers' experiences of posting schedules in advance to evaluate the ability of employees to anticipate their work schedules. Their core finding is that while schedules were posted further in advance (4 weeks prior), their intervention did not improve schedule "anticipation" (i.e., how closely work schedules aligned with the expectations of the employee). One potential reason for this null result may be that their intervention allowed scheduling adjustments after the posting of schedules, which would trigger penalties under FWLs if made within a certain number of days before the scheduled shift[6]. Due to data limitations, however, it is unclear from their paper how often managers adjust employee schedules by adding, canceling, or editing shift characteristics once they are posted. The absence of data that tracks both the generation of employee schedules and subsequent adjustments, which we have, is necessary to fully assess the effects of advance notice and schedule anticipation.

The second stream of research that this paper relates to is the literature in Operations Management on the connection between scheduling practices and worker/firm performance (Fisher et al. 2021; Kamalahmadi et al. 2021; Netessine et al. 2010; Perdikaki et al. 2012). Within this stream of work, our paper is closely related to Kesavan et al. (2022) and Lu et al. (2022), as their results are highly relevant to the objectives of FWLs. Regarding the former, Kesavan et al. (2022) shows, using a field experiment, that "responsible" scheduling practices affecting four dimensions of schedules (inconsistency, unpredictability, inadequacy, and lack of employee control) can have positive effects on store productivity. Lu et al. (2022) show, in the context of cashiers processing transactions in a grocery store, that consistency in hour-of-the-day and day-of-the-week can increase cashier productivity by 0.95% and 1.63%, respectively.

Finally, our paper is also closely related to Kwon and Raman (2023a), which studies the effects of FWLs in Chicago, Los Angeles, and Philadelphia on store performance. In contrast to our paper, they focus on the interplay between flexible scheduling practices, store performance (proxied by labor productivity), and scheduling adjustment costs (which are increased by FWLs). We view our results as complementary to theirs, as our paper focuses on the effects of FWLs primarily from the worker's perspective by directly assessing the efficacy of FWLs in improving the predictability and stability of work schedules. Our paper also addresses various concerns about some of the unintended (negative) consequences of FWLs that have been expressed in public and academic discourse (e.g. Mitchell et al. (2021); Yelowitz (2022)).

[6]Note that the number of days required for advance notice depends on the particular jurisdiction.

2. Background and Setting

Below, we provide a brief description of the CFW, LFW, and the PFW. Our primary goals are to discuss: (i) which industries and workers are covered by each FWL, and (ii) the specific scheduling practices that are penalized. Full details of each FWL are provided on the city of Chicago's website[7], the city of Los Angeles' website[8], and the city of Philadelphia's website[9]. A comparison between the three FWLs is also provided below. We also refer the reader to a comprehensive summary and discussion of FW-related laws by Mitchell et al. (2021). We conclude the section by discussing the focal retailers underlying our data sample.

TABLE 2. A Comparison of the FWLs in Chicago, Philadelphia, and Los Angeles

Conditions	**Philadelphia**	**Chicago**	**Los Angeles**
Effective Date	April 1, 2020	July 1, 2020	April 1, 2023.
Covered Industries	Retail (2 others)	Retail (6 others)	Retail.
Employer Size	250 employees	100 employees	300 employees
Covered Employees	Any	$W_i \leq$\$30.80 $\sum W_i \leq$ \$59,161.50/year	$W_i \geq$Min Wage. $\geq$ 2 Hours/Week
Good Faith Estimates	Yes	Yes	Yes
Advance Notice (T^*)	10 Days	10 Days.	14 Days.
Changing Start Time ($1 \leq T \leq T^*$)	W_i	W_i	W_i
Adding Shift ($1 \leq T \leq T^*$)	W_i	W_i	W_i
Deleting Shift ($1 \leq T \leq T^*$)	$\frac{1}{2} \times (L^{\text{Old}}) \times W_i$	W_i	W_i
Extending Shift ($1 \leq T \leq T^*$)	W_i	W_i	W_i
Reducing Shift ($1 \leq T \leq T^*$)	$\frac{1}{2} \times (L^{\text{New}} - L^{\text{Old}}) \times W_i$	W_i	W_i
Reducing Shift ($T < 1$)	N/A	$\frac{1}{2} \times (L^{\text{New}} - L^{\text{Old}}) \times W_i$	$\frac{1}{2} \times (L^{\text{New}} - L^{\text{Old}}) \times W_i$
Deleting Shift ($T < 1$)	N/A	$\frac{1}{2} \times (L^{\text{Old}}) \times W_i$	$\frac{1}{2} \times (L^{\text{Old}}) \times W_i$
Clopening/Right to Rest	\$40	$1.25\,W_i$	$1.5\ W_i$

Notes: The table above summarizes key differences of the FWLs from Chicago, Los Angeles, and Philadelphia. The following are the meanings of the variables above: (1) T^* is the amount of advance notice required by each FWL, (2) W_i is the hourly wage rate of the affected employee, L^{New} L^{Old} is the new and old duration of the adjusted shift, respectively.

2.1. Chicago's Fair Workweek Law

Chicago's Fair Workweek Ordinance (CFW) took effect on July 1, 2020. The CFW requires employers in seven industries - building services, healthcare, hotels, manufacturing, restaurants, retail, and warehouse services - to be subject to the CFW law. To be covered by the CFW, employees must: (1) perform work as an employee (not a contractor) or as a temporary worker, (2) spend the majority of their working time within city boundaries, and (3) earn an annual salary of less than or equal to \$50,000, or \$26 per hour (if hourly). To be subject to the CFW, employers must: (1) employ 100 or more employees globally (non-profit employers must employ 250 or more), with at least 50

[7] https://www.chicago.gov/city/en/depts/bacp/supp_info/fairworkweek.html
https://www.chicago.gov/content/dam/city/depts/bacp/OSL/20200518fwwshortFAQ77.pdf

[8] https://wagesla.lacity.org/sites/g/files/wph1941/files/2023-03/Fair%20Work%20Week%20Ordinance.pdf

[9] https://www.phila.gov/documents/fair-workweek-resources/

employees being covered by the CFW, and (2) be primarily engaged in a covered industry. The CFW is expected to cover a significant number of workers in Chicago, as a census conducted in 2019 showed that approximately 438,790 people over 16 work in the covered industries (Mitchell et al. 2021).

There are four major clauses of the CFW law. First, employers must provide a written estimate of the days and hours of work before or after employment (referred to as providing "good faith estimates"). Second, modifying, adding, or canceling employee shifts in a work schedule must be done with at least ten days' notice of the work period. This requirement increased to 14 days' notice on July 1, 2022. Modifications can include changes to shift start time or duration. Employers who fail to comply with these stipulations must pay an additional hour for every affected shift. This additional pay is called “predictability pay". The penalty increases when canceling or reducing shifts with less than 24 hours notice. Employees must receive 50% of the pay associated with lost hours, and the same penalties for the 10-day threshold apply when adding or modifying shifts. Lastly, workers can decline hours that occur less than 10 hours after the end of a previous day's shift and must be paid 1.25 times their regular pay for working back-to-back shifts. Finally, employers may ask managers to provide flexible schedules. Although these requests do not need to be accommodated, employers are prohibited from retaliating against workers for making such requests.

2.2. Los Angeles' Fair Workweek Law

Los Angeles' Fair Work Week Ordinance (LFW) took effect on April 1, 2023. In contrast to the CFW, the LFW affects businesses only in the retail industry. To be covered, a business needs to satisfy all of the following three requirements: (1) have more than 300 employees globally, (2) have a NAICS code within the retail trade categories and subcategories (44 through 45), and (3) have control over the wages, hours, or working conditions of any employee (this includes indirect control through a staffing agency). The LFW protects employees that satisfy the following criteria: (1) work at least two hours in Los Angeles for an employer covered by the LFW, (2) qualify for California's minimum wage law, (3) primarily work in retail operations. Like the CFW, the LFW covers full-time, part-time, and seasonal workers.

There are three major clauses of the LFW. First, employers must provide a good-faith estimate of the number of hours for new hires. Second, employers must provide 14 days' advance notice before the first day of employees' work schedules. Third, the LFW requires employers to provide at least 10 hours of rest between shifts. Violating this requirement requires the employer to pay the affected employee at 1.5 times their regular wage rate.

2.3. Philadelphia's Fair Workweek Law

Philadelphia's Fair Workweek Ordinance (PFW) took effect on April 1, 2020. The PFW affects businesses operating in retail, hospitality, and food services establishments with 250 or more employees and 30 or more locations worldwide. To be covered by the PFW, employees must: (1) perform work within the geographical boundaries of the City of Philadelphia, (2) work for an employer in the retail, hospitality, or food services industry, and (3) work for an employer that has 250 or more employees and 30 or more locations worldwide.

The PFW has several key provisions. First, employers must provide a written, good faith estimate of the employee's work schedule at the time of hire. This includes the average number of work hours employees can expect each week. Second, employers must give employees at least 10 days' advance notice of their work schedules. This requirement will increase to 14 days' notice beginning January 1, 2021. If changes are made to an employee's schedule after this notice period without the employee's consent, the employer must pay "predictability pay" as compensation. Third, if an employer adds time to an employee's shift, cancels or subtracts hours from a shift, or changes the date or time of a shift with no loss of hours with less than 10 days' notice but more than 24 hours' notice, the employer must pay the employee one hour of predictability pay. If changes are made with less than 24 hours' notice, the employer must pay the employee no less than half of the employee's hourly wage for any scheduled hours the employee does not work. Finally, employees have the right to rest by declining any work hours scheduled less than nine hours after the end of the previous day's shift. However, if an employee agrees to work a shift that begins less than nine hours after the end of their previous shift, they must be compensated with $40 for each such shift.

2.4. Focal Retailers

Our data encompass multiple independent retail chains. By "independent", we refer to chains not part of a larger conglomerate or franchise system, such as Trader Joe's within ALDI. These retailers operate across various sub-industries (indicated by the first two NAICS digits). Examples of these sub-industries include groceries, stationery, and durable goods. We have received comprehensive and unredacted data for all stores and all retailers, thus encompassing every store and every employee scheduled to work.

Given that all stores in our sample belong to the same respective chain, store operations are predominantly homogeneous at the chain level. This homogeneity is reflected in the similarity of employee tasks and the range of products and services offered by stores within a company. The organizational structure is consistent across all stores, featuring a single store manager responsible for overseeing operations, including labor scheduling, with additional management roles focused on supervising and training frontline employees.

Our combined sample includes over 6,500 brick-and-mortar stores, spanning over 2,500 cities across the United States. Collectively, our focal retailers operate at least 20 stores in Chicago, at least 35 stores in Los Angeles, and at least 50 stores in Philadelphia. Although we have exhaustive data on all stores across our three retailers (including those in Chicago and Los Angeles), we report a minimum number of stores (by stating "at least" x stores) in these three cities to maintain confidentiality. We acknowledge that our inability to disclose specific sub-industries and exact sample sizes limits the external validity of our study.

A crucial point about our focal retail chains is their classification as both "large" and "sophisticated" retailers. The label "large" is based on the number of store locations, with all companies operating at least 400 stores. The "sophisticated" designation stems from the utilization of commercial machine-learning algorithms to facilitate labor scheduling processes. These algorithms are designed to predict demand in short intervals (15 minutes) and align labor with desired service levels as specified by corporate officers.

The fact that our focal retailers are large and sophisticated has several implications for the external validity of our results. First, larger companies may have more financial assets to pay adjustment penalties and a well-developed understanding of their labor needs for store operations. Second, sophisticated retailers that use AI tools to schedule labor may be better equipped to adjust work schedules compliant with FWLs. Their advanced algorithms can rapidly adapt to regulatory changes and efficiently match labor schedules with real-time demand predictions. Consequently, such retailers might face fewer challenges in adhering to these laws than less technologically advanced companies. Furthermore, their machine-learning driven scheduling processes could potentially minimize labor-related inefficiencies, ensuring that employees are scheduled optimally while respecting the constraints of the fair workweek regulations. Thus, when interpreting the outcomes of our study, it is essential to consider the unique capabilities and resources of large, sophisticated retailers compared to smaller or less technologically equipped counterparts.

The fact that our retailers utilize AI tools to schedule labor may also have implications for the internal validity of our results. A key concern is whether any estimated effects are based on the actions and behaviors of store managers in affected cities, or due to changes in the AI tool's behavior. We address this concern in three ways. First, we asked our data providers whether there were major changes to the AI tool during the passing of the CFW, LFW, and PFW. For example, we asked whether there were material changes to the program's visual interface (to show when a scheduling action would trigger a penalty potentially) or whether there were material changes to the demand forecasting or labor scheduling processes. We were not informed of any[10]. Second, we

[10]An earlier version of this paper mentioned that some "constraints" were introduced. These were backend changes

empirically examine the behavior of the AI tool (e.g., its demand forecasts and the timing of schedule generation) as robustness checks to rule out changes in the underlying AI tool as a primary driver of our results. Third, our main analysis focuses on the actions and behaviors of managers. As we describe in Section 5, we have access to an exceptionally granular dataset that tracks *every* action that a store manager makes to work schedules. Among other variables, we know (with time stamps) what shifts were adjusted, when they were adjusted, and how much advance notice was provided for each adjustment. Overall, our main analyses focus on the store manager's behavior, rather than the behavior of the underlying AI tool.

3. Conceptual Framework and Hypothesis Development

In this section, we discuss and develop our hypotheses on the potential effects of FWLs on worker schedules. We primarily focus this discussion on the potential issues of compliance with FWLs. While compliance is a nontrivial issue with all major public policy, it is particularly important in our context because FWLs aim to achieve their objectives through penalties, and not by explicitly prohibiting certain scheduling actions. This approach makes the concept of compliance more nuanced: Firms may comply with the letter of the law (e.g., by paying out required penalties), but not comply with the overall spirit of the law, which is to improve the predictability and stability of work schedules. Differentiating these two types of compliance is crucial for policymakers to understand the efficacy of FWLs. For example, it allows policymakers to understand whether FWLs will effectively increase the predictability or stability of work schedules or increase the wages of covered employees.

To differentiate these two types of compliance, we introduce and define two types of compliance that firms may exhibit concerning FWLs: "technical" and "practical". The former relates to the classic definition of compliance, where employers follow the explicit rules and regulations set by the FWLs, such as paying the mandated penalties for making adjustments to work schedules without providing sufficient advance notice. The latter relates to employers adapting their scheduling practices to align with the law's underlying intent of ensuring stable and predictable work hours for employees. Section 3.1 discusses why some covered firms may not technically or practically comply with FWLs.

In addition, given practical compliance, we discuss several additional hypotheses on the potential consequences of FWLs. First, we discuss how FWLs may reduce scheduled labor for covered workers due to the predictability pay penalties (e.g., adding a short-notice shift based on new information about demand is more costly). Second, we discuss why FWLs may have unintended consequences by shifting unpredictable and volatile work schedules from higher- to lower-wage workers. Specifically, a key concern with FWLs is that because adjustment penalties are tied to the employee's wages, they may offer weaker protections for workers with lower wages, but they may also transfer unpredictable

that tracked predictability pay penalties that were sent to payroll. We do not have access to such data.

and volatile work schedules from higher- to lower-wage workers. Here, we note that FWLs may inadvertently exacerbate existing inequalities if wages are systematically different based on worker characteristics such as gender, age, or other demographic factors. Finally, we discuss how the requirements to provide new employees with "good faith estimates" on how much work they will be scheduled may hinder the ability of covered stores to hire new workers.

3.1. Firm Compliance

The first-order question regarding the efficacy of FWLs is whether firms comply with the law. In our context, compliance can take on two different definitions. The first definition relates to whether businesses subject to FWLs follow the specific details of the law discussed in Section 2.1. We hereafter refer to this definition as "technical compliance." For example, firms subject to the CFW must compensate employees for modifying, adding, or canceling shifts without providing at least 10 days' notice (based on the first day of the work schedule). Accordingly, an example of technical non-compliance with the CFW is when covered firms do not provide at least 10 days' notice *and* also do not pay out the required predictability pay penalties. Recall that employers have recourse against firms not technically compliant with the CFW by filing a complaint with the city or taking civil action against the firm directly (with city resources). A similar resource policy exists in Los Angeles and Philadelphia.

A second definition of compliance is also critical when evaluating the efficacy of FWLs. This definition of compliance relates to whether businesses modify their scheduling practices to promote predictable schedules. Hereafter, we refer to this definition of compliance as "practical compliance". Although the stated goals of FWLs (such as those in Chicago, Los Angeles, and Philadelphia) aim to provide employees with more predictable and stable schedules, it is not clear whether the penalties imposed by the law are large enough to cause employers to alter their scheduling practices. Said differently, the feasible set of work schedules (concerning predictability) does not change for covered employers. Accordingly, while the potential for additional compensation stemming from predictability pay penalties certainly benefits employees, it is essential to understand, from a policy evaluation standpoint, whether FWLs achieve one of their core objectives of promoting stable and predictable schedules.

Concerning technical compliance, economic analyses of regulatory compliance are commonly analyzed from canonical models of crime (Becker 1968; Stigler 1970), which argue that regulatory noncompliance is a decreasing function of the penalty and the probability of being caught. Theoretically, given the significant penalties for non-compliance, coupled with the numerous resources provided to covered workers to report violations, the likelihood of firms adhering to FWLs may be high. The FWLs in Chicago, Los Angeles, and Philadelphia equip covered workers with multiple avenues to file complaints for infractions. For instance, workers in Chicago can dial 311 (City Services) or submit a complaint directly to the Office of Labor Standards via the City of Chicago's official website. In addition, anti-retaliatory measures are in place to shield workers from backlash should they decide to come forward with a complaint.

Empirically, however, the likelihood of technical compliance may not be as high as expected. This is evidenced by the numerous high-profile class-action lawsuits filed by workers claiming violations of FWLs, as seen in cases against major corporations such as Walmart, Ruth's Steak House, Target, and Chipotle in Philadelphia and New York City. In our specific context, we have preliminary evidence of technical compliance among our focal companies. These companies utilize commercial AI tools to construct worker schedules across all their stores, and have reportedly incorporated some backend features into the AI tool that track adjustments that are potentially subject to penalties. Accordingly, while we do not know (based on the data) whether every scheduling adjustment qualifying for compensation (during the post-FWL period) was indeed compensated, the modifications made to the AI tool's features by corporate officers suggest a move toward technical compliance.

Concerning our second definition of compliance, it is also not obvious ex-ante whether stores that previously scheduled their employees with less than the required amount of advance notice will begin to provide their employees with more advance notice. As mentioned above, FWLs do not affect the feasible set of work schedules as long as the required penalties are paid. Consequently, affected stores can continue to follow their status quo while paying penalties. For example, one could imagine that businesses with deep pockets that rely on flexible scheduling practices triggering compensation may continue to follow their status quo closely. However, there are some doubts about this possibility, because labor is a significant cost for retailers (Fisher and Raman 2010; Fisher et al. 2021; Netessine et al. 2010), and paying one hour's additional wage is a 12.5% increase in wage expenditure for an 8-hour shift, which is a considerable increase.

Overall, while there are some high-profile examples of a lack of technical compliance (and hence practical compliance), it remains unclear whether firms that technically comply also comply in practice, presenting an obstacle for policymakers to understand whether FWLs increase the predictability and stability of work schedules, or increase employees' wages. Accordingly, this paper's key objective and contribution is to shed light on the effects of FWLs on businesses that have generally complied with FWLs at the corporate level.

3.2. Efficacy of FWLs on Improving Work Schedules

Given practical compliance (which also implies technical compliance), there are open questions regarding the effects of FWLs that have yet to be empirically examined across multiple literatures. The lack of empirical evidence has hindered the evaluation of the efficacy of FWLs in achieving their intended objectives, as well as understanding their broader implications on the workforce and employers.

First, what are the effects of FWLs on work schedules and employment? That is, do FWLs increase schedule predictability by causing managers to provide more advance notice for their employees' work schedules? Moreover, are schedule stability and predictability sufficiently correlated that FWLs also cause an increase in work schedule stability? Finally, do FWLs have unintended negative consequences on work schedules or employment, such as job loss? We discuss our hypotheses on these outcomes below.

Effects on Schedule Predictability. We begin with our hypotheses on how FWLs affect schedule predictability (i.e., how far in advance employees know their work schedules). We reiterate that FWLs place no explicit restrictions on short-notice schedules, as long as additional compensation is provided to the focal employee. This "choice-based" mechanism results in three competing hypotheses. Our first hypothesis is possibly the most obvious and natural (based on the imposed penalties). Specifically, we hypothesize that, on average, the amount of advance notice will increase for covered workers. This hypothesis is based on the fact that a 12.5% increase in wages for a short-notice adjustment (for an 8-hour shift) is difficult to absorb for retailers, who often operate on thin profit margins and persistently look for ways to cut labor costs.

Our second hypothesis is that, on average, FWLs will have no discernible impact on schedule predictability. This hypothesis is based on two possibilities. First, some employers may already be providing a sufficient level of advance notice to their employees' work schedules, and therefore, FWLs will not prompt any significant changes in this regard. Second, employers may find it optimal to continue to schedule their employees with short notice schedules while absorbing the required penalties. Consequently, the effects of FWLs on schedule predictability may depend on the existing scheduling practices of covered employers and their willingness or ability to absorb the associated costs of providing short-notice schedules. This latter point raises a potential issue of whether the imposed penalties are sufficiently large to cause an improvement in the predictability of work schedules in the focal jurisdiction.

Finally, our third hypothesis is that, on average, FWLs will hurt schedule predictability. This hypothesis is based on the possibility that covered employers may respond to the increased cost of short-notice adjustments by resorting to alternative strategies. One such strategy could involve hiring new employees to handle short-notice shifts rather than providing more advance notice to existing workers. These workers may be hired with the expectation that they would have to agree upon short-notice schedule adjustments. This could reduce schedule predictability for the existing workforce due to the influx of new employees with different scheduling needs and availability. Consequently, the predictability of the schedule at the *store-level* may decline.

Overall, the three hypotheses outlined above add some nuance to the discussion on how FWLs may affect schedule predictability. It is not necessarily obvious or guaranteed that schedule predictability will increase due to FWLs. The ultimate impact on schedule predictability will likely vary by employer. It will also depend on various factors, including existing scheduling practices, financial capacity, and strategic response to the new regulations. It may also be the case that the imposed penalties are not sufficiently high to cause an increase in the predictability of work schedules. As such, it is important to consider the broader context in which these laws are implemented and the specific circumstances of covered employers when evaluating the potential impact of FWLs on schedule predictability.

Effects on Schedule Stability. Next, we focus on the potential effects of FWLs on schedule stability, which is defined as the degree to which employees' work schedules remain consistent over time. We first reiterate that while increases in schedule stability are often mentioned as a positive effect of FWLs, they are not explicitly mandated or delineated within the provisions of FWLs. We also note that there is no widely agreed-upon definition of schedule stability, and that this lack of a clear conceptual framework may not provide the right incentives for employers to make schedules more stable. Based on this fact, we discuss three hypotheses of how FWLs affect schedule stability.

Our first hypothesis posits that, on average, FWLs will increase schedule stability for covered workers. This hypothesis is grounded in the expectation that schedule stability and predictability are sufficiently correlated that, if FWLs successfully increase the predictability of work schedules, they will also increase the stability of work schedules. Anecdotal evidence based on conversations with our data providers provides some impetus for this hypothesis: By having fewer short notice shift changes (due to the predictability pay penalties), managers are less likely to add shifts that cause significant week-to-week schedule deviations for employees. However, we note that beyond such anecdotal conversations, we are unaware of any empirical or theoretical papers suggesting that these two features of work schedules (i.e., predictability and stability) are correlated.

Our second hypothesis posits that, on average, FWLs will not significantly impact schedule stability. This is potentially the most natural hypothesis, as schedule stability is not directly encoded into the clauses of FWLs. Consequently, employers may emphasize adhering to the explicit requirements of the law, such as providing additional compensation for short-notice schedules, without necessarily prioritizing the stability of their workers' schedules. This could result in a situation where workers receive additional pay for unpredictability, but do not experience an improvement in the consistency of their schedules over time. Another potential reason for a null effect is based on the fact that there are no well-defined, universally accepted definitions or measurements of schedule stability.

This lack of a clear conceptual framework could lead to variability in how employers interpret and address schedule stability. For example, some managers may believe that schedule stability means avoiding cancellations or additions of shifts on short notice. In contrast, others may interpret it as maintaining consistent weekly work hours for their employees over time.

Finally, our third hypothesis is that schedule stability will, on average, decline. There are several mechanisms for this potential effect. First, while FWLs may result in employers providing more advance notice for shifts, they may also introduce more week-to-week variation in employees' schedules to maintain operational flexibility. Employers may need to respond to fluctuations in demand, changes in the availability of other employees, or other unforeseen operational requirements. To maintain flexibility while also complying with FWLs, employers may opt to vary the start times, duration, and days of shifts, resulting in more inconsistent schedules from week to week. Second, providing advance notice may give employers more time to optimize their scheduling practices to minimize labor costs. As a result, they might schedule shifts to meet demand more precisely, but this can also result in more variability in employees' schedules. For example, suppose an employer realizes that demand is higher on Tuesdays than on Mondays. In that case, they may adjust schedules accordingly, leading employees to work different days (i.e., Tuesday over Monday) from one week to the next. Third, by providing more advance notice, employers may also be able to more effectively use part-time or temporary workers to fill gaps in the schedule. This can result in full-time employees experiencing more variability in their schedules, as they may be scheduled for fewer shifts to accommodate the part-time or temporary workers.

Overall, the complex and nuanced nature of FWLs makes it challenging to straightforwardly predict their impact on work schedule predictability and stability. Our six hypotheses highlight the range of potential outcomes for covered workers, which can vary significantly depending on existing scheduling practices and how employers adapt to the new regulations. Although some employers may improve both predictability (and stability) to avoid penalties, others may find alternative ways to comply with the law that do not necessarily improve the predictability and stability of workers' schedules, such as hiring new employees with expectations that accommodate more short-notice shifts or irregular hours, thereby circumventing the need for schedule adjustments that would trigger predictability pay.

3.3. Potential Unintended Consequences on Work Schedules

Next, based on prior literature (e.g., Yelowitz (2022)) and some of the public discourse surrounding FWLs, we now discuss several hypotheses concerning the potential unintended consequences of these laws on both employers and employees, which may not be explicitly addressed in policy documents or might be overlooked when considering the overall impact of the legislation. Some of these unintended consequences include reduced hours and shifts (for workers), reduced hiring, or job loss. To this end, we first note the potential motivations for employers to craft work schedules in a way that is unpredictable or unstable. A large body of literature, using modeling techniques and a variety of queuing and demand forecasting models, shows that flexible scheduling practices are the outcome of firms attempting to adjust their labor supply to match volatile demand (See Bhandari et al. (2008); Fisher and Raman (2010); Fisher et al. (2021); Gans et al. (2003); Gurvich et al. (2008), and the citations therein). In addition to these theoretical papers, a growing empirical literature in Operations Management draws a link between flexible scheduling practices and worker/store performance (See Kamalahmadi et al. (2021); Kwon and Raman (2022, 2023b); Lu et al. (2022) and the citations therein).

The main intuition provided by the collective empirical and theoretical work supporting flexible scheduling practices is that delaying the provision of schedules, or making short-notice scheduling adjustments conditioned on more "fresh" information about demand, can help reduce wage expenditure on low-demand days and increase output on high demand days. In other words, the expected marginal product of labor is a function of the manager's information about demand and employee productivity, which may increase as the time between the date of schedule generation and the scheduled work date decreases. To be concrete, a convenience store manager in Philadelphia may schedule more labor on short notice if the weather forecast predicts a sunny and warm weekend, which typically leads to increased foot traffic and demand, while conversely cutting shifts if, say, a snowstorm is expected and fewer customers are likely to visit the store.

Based on these insights, the scheduling adjustment costs imposed by FWLs may have several negative consequences on worker schedules of businesses that practically comply. One possibility is a loss of scheduled labor (and hence, wages). This possibility is borne out of the fact that because canceling shifts upon the revelation of new information of a negative demand shock becomes more costly, fewer shifts are scheduled upfront. In addition to this possibility, because adding shifts based on the revelation of new information of a positive demand shock also becomes more costly, fewer shifts are added later on[11]. Overall, combining both possibilities may result in less work (and hence less wages) provided for employees. Furthermore, in extreme cases, job loss may also result if managers begin to schedule and utilize considerably less labor due to the imposed adjustment costs. Indeed, a lower amount of work is one commonly expressed by opponents of FWLs (von Wilpert 2017).

Overall, while the concerns above have been frequently discussed in public discourse, they have not been empirically examined using granular shift-level scheduling data. Previous studies that

[11]We note that because bilaterally agreed-upon adjustments to shifts made with less than 10 days' notice are not penalized, it is likely that shift cancellations made unilaterally by managers to conserve on wages are more likely to decrease than adjustments that increase labor minutes, which employees may be more willing to voluntarily accept (due to the increase in their wages).

evaluated FWLs have primarily relied on survey data, which may suffer from non-representativeness. For instance, it may not include employees who have lost their jobs due to reduced labor demand. Therefore, our analysis using administrative scheduling data (described in Section 5) provides a more comprehensive understanding of the effects of FWLs on worker schedules and employment.

3.4. Potential Unintended Consequences on the Composition of Labor

Another potential unintended consequence of FWLs is that businesses may substitute full-time workers with part-time ones. There are two potential reasons for this shift. First, employers may try to utilize workers not covered by FWLs (for example, temporary workers in Chicago must have worked at least 420 hours within an 18-month period to be covered by the CFW). Second, employers may try to offset the increased costs of scheduling adjustments by using part-time workers who are usually not provided with non-monetary forms of compensation, such as health insurance or sick pay.

Supporting these hypotheses is a closely related paper by Yelowitz (2022), which presents evidence using the 2014-2022 Current Population Survey that the composition of employees working part-time in jurisdictions with FWLs increased by approximately 9.2 percentage points. Furthermore, he finds that approximately two-thirds of this shift comes from individuals reporting to be part-time involuntarily, and that very little" of this shift is explained by non-economic reasons such as "childcare problems", "family or personal obligations", or "in school or training".

Another paper that supports the hypothesis that employers may strategically respond to FWLs by substituting full-time workers with part-time workers to partially offset the predictability pay penalties with non-wage benefits is Yu et al. (2022). They show that increases in the minimum wage result in a decrease in labor hours for full-time workers. The authors argue that this outcome may result from employers attempting to offset the increase in wage expenditure by paying out fewer benefits to workers.

3.5. Potential Unintended Consequences on Hiring and Turnover

Another concern with the FWLs is its potential effects on hiring. There are two reasons why it may affect hiring. First, as discussed in Section 2.1, some FWLs require covered businesses to provide workers, when they are first hired, with a written estimate of the days and hours of work they are expected to work. Therefore, businesses that cannot accurately forecast demand (or operate in industries where demand is difficult to forecast) may be unable to provide their new hires with "good faith" estimates of how much work they will receive. Second, as we noted earlier, managers may substitute full-time workers with part-time ones (to lower the payment of non-wage benefits), which may result in a reduction in full-time hires and an increase in part-time hires. Third, because employers must offer additional hours to existing employees before hiring new ones, this could also limit opportunities for new job seekers.

In addition to hiring, FWLs may also affect employee turnover for several reasons. First, some employees may be dissatisfied with the changes to their work schedules, if, for example, schedule predictability increases but schedule stability decreases (a hypothesis discussed above). Second, some employees may also be dissatisfied if the unintended consequences of FWLs reduce labor hours, as we have hypothesized. This decrease in hours could also reduce some workers' wages, potentially driving them to seek employment elsewhere. Consequently, combining these factors could potentially increase employee turnover, as workers may become dissatisfied with unpredictable or insufficient work hours and decide to leave their current positions in search of more stable and predictable employment opportunities.

3.6. Importance of Heterogeneous Effects

Finally, we discuss the importance of heterogeneous effects of FWLs. Given that the predictability pay penalties in FWLs are based on the underlying employee's wage rate, there is potential for significant variation in the impact of these laws across the distribution of workers based on their wage rates. For instance, higher-wage workers may experience a smaller relative impact from the predictability pay penalties than lower-wage workers. Specifically, employers facing the increased costs associated with schedule adjustments may choose to avoid adjusting higher-wage workers' schedules and instead adjust those of lower-wage workers to minimize the financial impact of these penalties. This could potentially exacerbate existing income inequalities, as lower-wage workers might be more severely affected by the penalties imposed by FWLs. Furthermore, these dynamics may contribute to a shift in the workforce composition, with employers potentially favoring lower-wage workers to avoid the high costs of predictability pay for higher-wage employees. The resulting labor market stratification could have long-term implications for workers' income mobility and overall economic inequality.

In addition, there may also be heterogeneous effects based on sex, particularly given the existing disparities in wages and employment opportunities between men and women. If employers preferentially adjust the schedules of lower-wage workers to minimize the financial impact of predictability pay penalties, and if women are disproportionately represented in lower-wage jobs, then women may be more severely affected by the penalties imposed by FWLs. This could potentially exacerbate existing gender inequalities in the workforce. Furthermore, if FWLs lead to a reduction in full-time hires, as discussed earlier, and if women are more likely to occupy part-time positions, this could further entrench gender disparities in employment. Overall, it is essential to consider these potential gendered impacts when evaluating the overall effects of FWLs and to conduct further empirical research to understand the full range of consequences these laws may have on different demographic groups within the workforce.

4. Empirical Methodology

We now turn to our empirical methodology to test the hypotheses outlined in the previous section. The main empirical analysis of the paper studies whether stores affected by the CFW, LFW, and PFW ("treatment group") began to schedule their employees with more predictable and stable work schedules than stores not impacted by the CFW, LFW, and PFW ("control group"). To this end, we examine a series of variables that describe the labor scheduling decisions of managers from several perspectives (e.g., at the shift, worker, and store levels). Below, we describe our empirical specifications, the outcomes of interest, and how we construct our sample.

First, we examine the effects of the three FWLs on how managers adjust work schedules after they are algorithmically generated by the AI tool, which is used across all stores belonging to the retailers underlying our analysis. We start with this analysis to directly focus on the actions and behaviors of managers in Chicago, Los Angeles, and Philadelphia. We use Figure 1 to illustrate this point. Consider a store manager in Chicago, say. Before the CFW, the manager on date t can freely adjust work schedules on date t, $t+1$,...,$t+n$. The only costs store managers bear for adjustments is the potential disutility of short-notice work schedules for employers along with its potential negative effects on worker productivity and schedule adherence (Kamalahmadi et al. 2021; Kwon and Raman 2023b; Lu et al. 2022).

However, after the CFW, unilaterally adjusting work schedules on dates t through $t+k$ (where $k = 10$ for the CFW) becomes more costly. Specifically, unilaterally made adjustments (e.g., removing a shift outright) after this threshold date requires a predictability pay penalty that is fully rebated back to the affected employee. Consequently, affected stores that also practically comply would make fewer unilateral adjustments, on average, between dates t and $t+k$, and potentially make more unilateral adjustments to work schedules from $t+k+1$ onward. And ultimately, stores that practically comply may increase the amount of advance notice for their employees' work schedules. We note here that we do not observe whether a scheduling adjustment was made uniterally or bilaterally between managers and employers. We, therefore, focus on the distributional effects of schedule adjustments across different time horizons relative to the work date to infer the impacts of the FWLs on scheduling behavior.

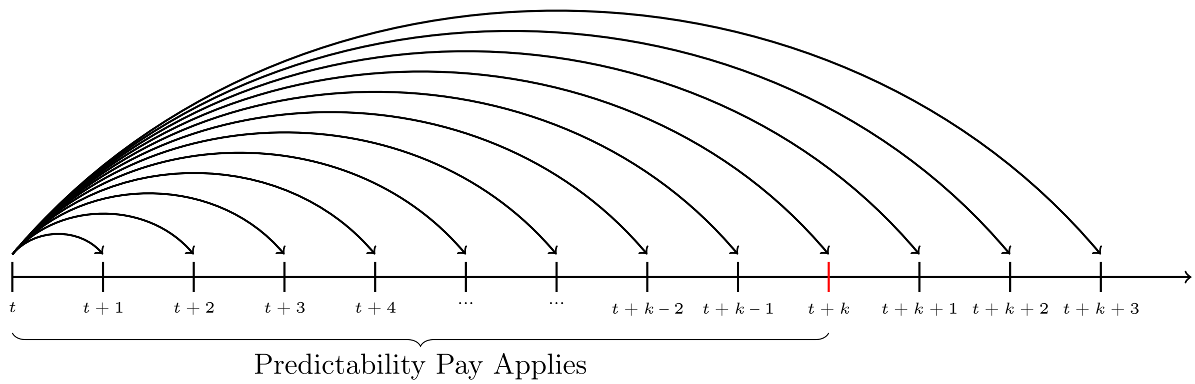


FIGURE 1. An Illustration of the Dynamic Scheduling Adjustment Process

Notes: The figure above highlights the dynamic scheduling process across stores in our sample. On date t, managers can make edits to work schedules on dates $t+1$ $t+2$, and so on. The red line indicates the threshold date for when predictability pay penalties no longer apply. For instance, $k = 10$ for the CFW and PFW, but $k = 14$ for the LFW (based on the work period). Accordingly, unilateral edits made to work schedules on nodes before $t+k$ trigger predictability pay for affected workers.

To test these hypotheses, we estimate regressions of the following form:

$$y_{i,t} = \beta X_{i,t} + \alpha_i + \lambda_t + \boldsymbol{\gamma}\boldsymbol{\Pi}_{i,t} + \varepsilon_{i,t} \tag{1}$$

where $y_{i,t}$ captures the magnitude of shifts scheduled more than k days away for store i during week t. We also study the effects on the magnitude of shifts scheduled less than or equal to k days away. For controls, $\boldsymbol{\Pi}_{i,t}$ is a vector of control variables (total number of shifts, labor minutes, and number of operating days within the week), α_i is a store fixed effect, λ_t is a company-specific time fixed effect (year-week)[12], and $\varepsilon_{i,t}$ is an error term. Our independent variable of interest is $X_{i,t}$, which is a dummy variable that equals one after the enactment of the CFW, LFW, and PFW, for stores located in Chicago, Los Angeles, and Philadelphia respectively[13]. Under a prallel-trends assumption (among other assumptions that we discuss below), β captures the causal effect of the FWL on scheduling adjustments. Finally, we cluster standard errors at both the store (the level of the shock) and year-week levels[14].

[12]We let the time fixed effect vary based on each company to capture seasonal demand fluctuations (based on the types of products that are sold at each company, changes in company policy, or changes in the AI tool.

[13]Thus it is the interaction of two dummy variables. The first is a dummy variable that equals 1 for stores located in the associated FWL jurisdiction (and 0 otherwise), and the second is a dummy variable that equals 1 in the post-FWL period (and 0 otherwise).

[14]This allows for arbitrary correlations in the error term between (1) observations of the same stores across different weeks, and (2) observations of different stores in the same week. Our results are robust to alternative standard errors, including single clustering (either dimension) or using White standard errors.

In addition to the effects on the magnitude of adjustments that trigger predictability pay, we also use Equation 1 to estimate the effect of the CFW, LFW, and PFW on a series of other scheduling-related variables that were discussed in our hypothesis development section, such as: (i) total scheduled labor minutes, (ii) total scheduled shifts, (iii), the average length of shifts, and (iii), new employee hires and employee turnover.

Our motivations for examining these results were provided in Section 3. In particular, these outcomes relate to concerns about FWLs in public discourse. For example, we are interested in whether impacted stores reduce scheduled labor, as its expected marginal product may be lower when scheduled further in advance. We are also interested in whether impacted stores strategically adjust from using full-time workers to part-time workers to offset the additional costs from predictability pay penalties by paying fewer benefits to their employees.

In addition to examining store-level effects, we also examine the effects at the worker-level. Specifically, we modify Equation 1 in the following ways. First, we make the unit of observation from the store-level to the worker-level. We use j to index each employee. In addition to the included fixed effects (store and company $\times$ time), we include worker fixed effects. Specifically, we include worker $\times$ store fixed effects to account for the fact that a single worker may be scheduled across multiple stores (some of which may be in the treatment group, whereas others may be in the control group).

Sample. We make three notes about our sample to estimate Equation 1. First, due to several key differences between the CFW, LFW, and PFW (described earlier in Section 2), we estimate Equation 1 separately for the CFW, LFW, and PFW. For instance, the advance notice required in Chicago is 10 days, but it is 14 days in Los Angeles. Differences such as these make treatment considerably different between jurisdictions. Additionally, as elaborated below, the CFW and PFW were contemporaneous with a variety of contemporaneous shocks that make the identification of a causal effect much more challenging than the LFW. Namely, the CFW and PFW were contemporaneous with the COVID-19 Pandemic, and the CFW was additionally contemporaneous with a minimum wage increase from \$13 to \$14 per hour. Accordingly, examining the effects of the CFW, LFW, and FW separately and then comparing and contrasting the results may be more effective in understanding the true effects of FWLs. We further elaborate on the econometric challenges associated with our estimates below.

Second, we estimate Equation 1 using a sample that contains observations 10 weeks before and 20 weeks after the enactment of the CFW, LFW, and PFW. The pre-FWL period is essential for examining any changes in our dependent variables, particularly if stores in the treatment group experimented with scheduling practices in anticipation of the new laws. Here, we also note that the 20-week post-period (5 months) may not be sufficient to characterize a "long-run effect" of FWLs.

Third, our main regressions use all stores in Chicago, Los Angeles, and Philadelphia as the "treatment group" in the analyses for the CFW, LFW, and PFW, respectively. For our control group, we use all stores that do not operate in a jurisdiction with an FWL. For example, in our CFW sample, we drop all stores in New York City, Evanston, San Francisco, Seattle, Oregon (statewide), Emeryville (CA), Los Angeles, and Philadelphia. Our retailers frequently use this approach when evaluating the effects of major public policy shocks. We also prefer this approach because of the added precision of our estimates from having a larger sample. Finally, this approach gives the least amount of discretion in constructing the sample, as there are many competing ways to construct suitable control groups. Nevertheless, in Section 7.2, we show that our results are quantitatively and qualitatively similar when we construct a more restrictive set of control stores and workers constructed from matching. We use several matching approaches. Specifically, some of our matches are constructed at the store-level (e.g., using size-related variables), while others are matched at the worker-level (e.g., wage, labor minutes, and gender).

Finally, we also show that our results are quantitatively and qualitatively similar when focusing only on the set of workers that have been "continuously employed" before the effective dates of the FWL. We define "continuously employed" based on the number of shifts and observations the employee has before the effective date of the FWL. Here, the question that we ask when using this sample is whether the effects of the FWL differ for the existing workforce versus new employees, who may have been hired with different contracts or expectations to reflect changes to managerial scheduling practices stemming from the FWL. Analyzing this subsample is critical given that FWLs may have an affect on hiring practices and the composition of new employees (See Section 3.5).

Identifying Assumptions. Our identifying assumption is that scheduling-related variables for workers and stores in our treatment and control groups would have evolved along parallel trends in the absence of the CFW, LFW, or PFW (depending on the sample). We say upfront that, for at least two reasons, this assumption probably does not hold in the context of the CFW and PFW. First, the CFW was contemporaneous with an increase in the minimum wage in Chicago from $13 to $14 per hour, which affected approximately 15% of our workers in the CFW sample.

Second, the effective dates of the CFW and PFW (July 1, 2020 and April 1, 2020, respectively) were contemporaneous with the COVID-19 Pandemic. The connection between these two contemporaneous shocks and labor scheduling decisions is straightforward. Here, the concern is that a contemporaneous negative demand shock stemming from the COVID-19 Pandemic may be driving our results. Additionally, uncertainty stemming from demand may have also had unobservable effects on the labor supply of stores. We add some nuance to this concern in the following ways. First, based on our difference-in-differences setup, the Pandemic's effect must have differed systematically across treatment and control cities during the periods of our study to bias our estimates. It is unclear whether Philadelphia and Chicago had particular "severe" or "different" pandemic-related effects than the other 1,900 cities in our sample. To support this point, we show that the demand forecasts made by the AI tool do not differentially evolve around the passing of the CFW and PFW. Nevertheless, we cannot definitively characterize the interaction effects between the Pandemic and the FWL, which strongly limits the implications of the estimates from Chicago and Philadelphia.

However, despite the econometric challenges associated with the CFW and the PFW, we include them in our main analyses (but separately) for two reasons. First, as we mentioned previously, the results from Chicago and Philadelphia may provide a unique opportunity to understand the potential effects of FWLs under increased demand and volatility in labor supply. These estimates may be useful for some readers who have private insights about the effects of the Pandemic on store operations.

Second, these estimates may still be valuable to policymakers in Chicago and Philadelphia who are interested in understanding how their FWLs have been affecting employment and worker schedules. The COVID-19 Pandemic was ubiquitous and affected all retailers in Chicago and Philadelphia (potentially to different degrees). Therefore, any approach in evaluating the effects of the FWLs during this period in Chicago and Philadelphia would inherently face the same econometric challenges.

Event Study Models. To provide supporting evidence for our assumption of parallel trends, we estimate an event study model that allows the effect of treatment to vary across year-week groups of observations. In addition, this model also allows us to examine the potential dynamics of these effects, which allows us to investigate their persistence over time, albeit with a limited 20-week post-FWL sample. To this end, we estimate variants of the following model:

$$y_{i,t} = \sum_{\substack{\underline{T} \leq k \leq \overline{T} \\ k \neq -1}} \beta_k X^k_{i,t} + \alpha_i + \gamma_j + \lambda_t + \boldsymbol{\gamma} \Pi_{i,t} + \varepsilon_{i,t} \tag{2}$$

In the above specification, all variables are the same as in Equation 1 except for $X^k_{,t}$, which is now a relative time to treatment indicator variable. Specifically, this variable $X^k_{i,t}$ equals 1 whenever $t = k$ for $k \in [\underline{T}, \overline{T}]$ only for stores in the treatment group. Here, $\underline{T}$ and $\overline{T}$ equal 10 and 20 weeks, respectively. In this specification, β_k measures the impact of the FW law in period k, and we normalize $\beta_{-1} = 0$, which is the week before the FW law coming into effect. We again double cluster standard errors at the store and year-week levels for statistical inference on the β_k estimates.

5. Data and Summary Statistics

This section provides an overview of our data sources and presents summary statistics. Our data extracts digitized work schedules and scheduling adjustments recorded by an AI tool across our retailer. We use three key datasets in our analyses, which we describe below.

5.1. Work Schedule Data

Our first dataset contains the work schedules of *all* shift-based employees across stores. The unit of observation of this data component is at the store-worker-shift-version level, and reveals the shift's start time, duration, and task. We refer to the aforementioned components of each shift as "characteristics".

We observe up to three versions of every shift observation. Characteristics may change across versions, and shifts may be deleted or added outright between versions. We describe these versions below:

- Version 1 ("V1"): This version is algorithmically generated by the AI tool. Employees typically do not observe this version.
- Version 2 ("V2"): This version is ultimately distributed to employees and can incorporate "overrides" that managers make to V1 work schedules.
- Version 3 ("V3"): This version records the shift and its characteristics at the beginning of the week in which the shift is scheduled[15]. Version 3 rarely differs from Version 2, with differences occurring in less than 1.8% of shift observations (likely due to unanticipated employee turnover or unavailability). This version is sent to payroll for an estimate of wage costs.

We use this shift-level dataset to aggregate to the worker-week and store-week levels for our analyses. Because V3 reflects the "finalized" version of work schedules used for payroll estimates, whenever we talk about labor minutes or shifts without mentioning the associated version, we are referring to V3.

5.2. Schedule Adjustments Data

The second data component contains records of *all* shift-level adjustments that are made by store managers Because every action is time-stamped, we know exactly when an adjustment was made, who it was made for, what shift it was applied to, and how much advance notice was provided. There are many potential adjustments (with some variation across customers). The key adjustments applying to all retailers that we focus on are shift: (1) additions, (2) cancellations, (3) time additions,

[15] For example, if the shift is scheduled on a Tuesday, this version would capture the characteristics of the shift on Monday

and (4) time reductions.

We note that while there are only three versions for each shift, multiple changes may be made within a single version. For example, a manager may have changed the start time of a V1 shift, only to undo this change before it is ultimately distributed to employees. Additionally, a manager may have made multiple adjustments to a V2 shift, such as changing its duration and task. Overall, this dataset enables us to track the complete *evolution* of each shift, from when it is first generated by a scheduling algorithm to when it is distributed to employees and sent to payroll.

This dataset is critical because it allows us to approximate the amount of advance notice provided to employees for each shift. While we have exact timestamps, we describe the amount of advance notice computed from this dataset as only being approximate because it is entirely possible that a manager first told an employee about an impending change to their work schedule, but only found time to record this change in the AI tool days later. Similarly, a manager may have changed a work schedule but chose not to publish it to their employees until a few days later. Accordingly, our measures of “advance notice" in our analyses are only approximate.

5.3. Human-Resource Records

Our final dataset contains human resource (hereafter, "HR") records describing all store employees. For each employee, we observe their status (part-time versus full-time), wage, hire date, termination date (if relevant), what tasks they can work on, and their availability to work on each day of the week. We observe multiple observations for an employee if, for example, their wage changed, if they previously worked at the company, or were promoted.

This dataset is essential when examining the effects of FWLs on worker schedules for several reasons. First, it allows us to identify all employees subject to the CFW, LFW, and PFW. For instance, only employees in Chicago who earn no more than $56,381.35 per year in salary or no more than $29.35 per hour. Understanding employees' hourly wages is also critical when assessing the costs of scheduling adjustments made without 10 days' notice. We also use this data to test whether there are heterogeneous effects on workers based on their wages, as higher (resp. lower) wage workers may be subject to relatively fewer (resp. greater) short-notice shift adjustments. Finally, this dataset is particularly relevant for Chicago, as we can determine what percentage of employees experienced the contemporaneous minimum wage from $13 to $14 per hour.

The HR dataset is also crucial for accurately determining whether a firm is subject to the CFW, LFW, or the PFW, as it allows us to measure the total number of stores and employees, including those in Chicago, Los Angeles, and Philadelphia. As explained in Section 2.1, for-profit firms are only subject to the CFW if they employ at least 100 employees, with at least 50 being covered by the CFW. For the LFW, firms must employ more than 300 employees globally. Finally, for the PFW, firms are only subject if they have at least 250 employees worldwide and at least 30 locations worldwide. Overall, merely knowing the number of locations a firm operates is insufficient to determine whether it is subject to the CFW, LFW, and PFW if the staffing roster and employees' hourly wages are unknown. Therefore, the comprehensiveness and granularity of our data allow us to accurately determine whether a firm or store is subject to an FWL.

5.4. Final Sample and Summary Statistics

Table 3. Summary Statistics

Panel	Variable	N	Mean	SD	Median
A: Chicago	Scheduled Minutes (V1)	216,110	9.861	0.526	9.725
A: Chicago	Scheduled Minutes (V2)	216,110	9.924	0.532	9.817
A: Chicago	Scheduled Minutes (V3)	216,110	9.924	0.534	9.819
A: Chicago	Scheduled Shifts (V3)	216,110	3.846	0.542	3.761
A: Chicago	Demand Forecast	216,110	9.903	0.564	9.770
A: Chicago	Operating Duration (Minutes)	216,110	8.668	0.263	8.616
A: Chicago	Advance Notice (Days)	216,110	12.468	4.950	11.897
B: Los Angeles	Scheduled Minutes (V1)	209,801	9.859	0.559	9.701
B: Los Angeles	Scheduled Minutes (V2)	209,801	9.930	0.583	9.762
B: Los Angeles	Scheduled Minutes (V3)	209,801	9.935	0.585	9.767
B: Los Angeles	Scheduled Shifts (V3)	209,801	3.842	0.609	3.714
B: Los Angeles	Demand Forecast	209,801	9.902	0.566	9.722
B: Los Angeles	Operating Duration (Minutes)	209,801	8.696	0.278	8.616
B: Los Angeles	Advance Notice (Days)	209,801	16.666	5.200	16.481
C: Philadelphia	Scheduled Minutes (V1)	217,382	9.869	0.525	9.733
C: Philadelphia	Scheduled Minutes (V2)	217,382	9.908	0.534	9.800
C: Philadelphia	Scheduled Minutes (V3)	217,382	9.907	0.536	9.800
C: Philadelphia	Scheduled Shifts (V3)	217,382	3.839	0.544	3.738
C: Philadelphia	Demand Forecast	217,382	9.910	0.510	9.769
C: Philadelphia	Operating Duration (Minutes)	217,382	8.674	0.262	8.616
C: Philadelphia	Advance Notice (Days)	217,382	11.920	5.193	10.486

Notes: The above table presents summary statistics at the store-week level. The definitions for each schedule version (i.e. V1) are presented in Section 5.1. All variables above are log transformed except for "Advance Notice (Days)".

We now review the final sample for the CFW, LFW, and PFW used in our analyses. Summary statistics for each sample are provided in Table 3. We comment on a few of the summary statistics for each sample below. We present only lower bounds on figures concerning sample size to mask the identities of the retailers underlying our sample.

For the CFW sample:

1. There are at least 20 stores in Chicago in our sample, employing more than 400 employees.
2. There are at least 4,000 stores in the control group, which employs more than 100,000 employees.

For the LFW:

1. There are at least 35 stores in Los Angeles in our sample, employing more than 450 employees.
2. There are at least 4,000 stores in the control group, which employs more than 100,000 employees.

For the PFW:

1. There are at least 55 stores in Philadelphia in our sample, employing more than 2,000 employees.
2. There are at least 5,000 stores in the control group, which employs more than 100,000 employees.

Again, we note that only lower bounds on the sample size are provided to protect the anonymity of the underlying retailers. However, our data contains full and uncensored data from all retailers. We acknowledge the potential limitations with internal and external validity caused by this omission. Additionally, we note that stores and workers in the "control group" are different across the CFW, LFW, and the PFW, because the focal sample drops stores in other cities and states that have ever enacted an FWL (e.g.,in the CFW sample, stores and workers in Los Angeles and Philadelphia). Finally, we note the high imbalance between the number of treated versus control groups (both stores and workers). While we prefer to keep all control stores in our analyses to increase the precision of our estimates[16], we show that our results are quantitatively and qualitatively similar when constructing matched control groups (See Section 7.2).

6. Main Results

6.1. Effects on Schedule Adjustments

We begin our discussion of our results by examining the effects of the CFW, LFW, and PFW on the frequency and patterns of scheduling adjustments made by managers. To do so, we estimate Equation 1 using three dependent variables: (1) the number of adjustments made to shifts scheduled less than or equal to k days in the future, (2) the number of adjustments made to shifts scheduled more than k days in the future, and (3) the total number of adjustments. Here, $k = 10$ for Chicago and Philadelphia, and $k = 14$ for Los Angeles). Recall that an "adjustment" can be a shift addition, deletion, or a change to any shift "characteristic" (e.g., duration). A shift can be subject to multiple adjustments. We refer the reader back to Figure 1 to visualize how on any given date t managers can make adjustments to work schedules on days $t + 1$, $t + 2$,...$t + k$, $t + k + 1$, etc. Given this setup, we investigate whether the scheduling horizon of managers in impacted cities, on average, shifted from dates $[t, t + k]$ to $[t + k + 1, \infty)$.

We start with this analysis to determine whether the penalties associated with short-notice adjustments imposed by the CFW, LFW, and PFW were large enough to invoke affected stores to provide their employees with more advance notice. This analysis also allows us to focus directly on the individual scheduling actions made by managers, rather than jumping ahead to examine any potential aggregate effects these adjustments may have on workers and stores. The richness of our

[16] In addition, comparing a cluster of stores or workers that are affected by a policy shock with the average performance of stores not affected by the policy shock seems to be the approach that is most familiar and most frequently used by our data providers (based on conversations).

data (in particular, the dataset that logs the actions taken by every manager), allows us to observe the precise actions (e.g., shift deletion) of every manager. Each action is also timestamped, which allows us to make some inferences about whether that particular action would trigger a predictability pay penalty[17]. Overall, this approach allows us to provide micro-level evidence on the effects of the CFW, LFW, and PFW on managerial scheduling behavior.

While there are many potential "adjustments" that shifts can be subject to (See Section 5.2), we focus our attention on five key statistics that capture management scheduling decisions after the AI tool generates the first batch of schedules: (1) total adjustments, (2) total shift additions, (3) total shift cancellations, (4) total shift time extensions, and (5) total shift time reductions. We further break these up into two variables based on whether the specific action affects a shift from dates $[t, t+k]$ or $[t+k+1, \infty)$, i.e., whether they are potentially subject to a predictability pay penalty or not.

We begin by studying the effects of the three FWLs on total adjustments. Table 4 presents these results, which are displayed in three panels (Panel A for Chicago, Panel B for Los Angeles, and Panel C for Philadelphia). Across all panels, the dependent variables are: (1) the total adjustments made to shifts that are scheduled less than or equal to k days away, (2) the total adjustments made to shifts that are scheduled more than k days away, and (3) total adjustments. All specifications include a series of fixed effects and control variables including: (1) Store fixed effects, (2) Company $\times$ year-week fixed effects, (3) (log) operating duration in minutes, (4) (log) total labor minutes, (5) (log) number of shifts, and (6) (log) number of operating days.

For Chicago (Panel A), we find sizeable effects for our first two variables, but a null aggregate effect. Specifically, we find a 78.9% decrease in adjustments made to shifts that are scheduled less than or equal to k days away. Additionally, we find a 112% increase in adjustments made to shifts that are scheduled more than k days away. Both estimates are statistically significant at the 1% level. Overall, we find strong evidence that, on average, the scheduling horizon of managers in Chicago changed from $[t, t+k]$ to $[t+k+1, \infty)$.

Next, Panel B of Table 4 presents the results for the LFW, which became effective on April 1, 2023. In contrast to our Chicago results, we find a 14.6% increase in the total number of adjustments made to shifts scheduled less than or equal to k days away. However, this estimate is only significant at the 10% level. Next, similar to our Chicago results, we find a 38.3% increase in adjustments to shifts scheduled more than k days away. Overall, just like the CFW (but with smaller magnitudes), we find that the LFW was effective in increasing the number of adjustments within the $[t+k+1, \infty)$ period. However, we find an increase in total adjustments (31.4%), and we also find an increase in

[17] We cannot say for sure whether an action would trigger a predictability pay penalty because these penalties do not apply if the action stems from a bilateral agreement between the manager and the focal employee.

adjustments within the $[t, t+k]$ window, which was not found in Chicago.

Finally, Panel C of Table 4 presents our results for the PFW, which became effective on April 1, 2020. The results here are significantly different than our Chicago and Los Angeles results. Specifically, we find no statistically significant changes to total adjustments or potentially penalized adjustments within the $[t, t+k]$ period. However, we find a sizeable decrease in non-penalized adjustments (i.e., adjustments made in the $[t+k+1, \infty)$ period) of 14.5%.

While the results above are informative of how each FWL affected the aggregate number of shift adjustments during the penalty and non-penalty periods, they are not informative about the changes to the *type* of adjustments (e.g., shift additions versus shift cancellations). Understanding the effects of FWLs on specific types of scheduling adjustments is crucial to fully grasp the full impact of FWLs. Specifically, shift additions, cancellations, and start time changes each have distinct implications for employee income stability and work-life balance, and these nuances are vital to comprehensively evaluate the effectiveness of such laws.

To this end, we now examine the effects of the three FWLs on shift-level: (1) additions, (2) deletions, (3) time extensions, and (4) time reductions. Each adjustment is also broken up by whether they are potentially subject to penalties based on the amount of advance notice provided. We present these results in Table 5, where Panels A, B, and C present the results for Chicago, Los Angeles, and Philadelphia, respectively.

For Chicago (Panel A), we find a reduction in all four types of adjustments in the penalty period, and an increase in all four types of adjustments in the non-penalty period. For instance, we find that shift additions in the penalty period decreased by 60.1%, but increased in the non-penalty period by 81.3%. Similarly, we find that shift deletions decreased by 34.6% in the penalty period, but increased by 59.9% in the non-penalty period. We also find similar offsetting results for shift extensions and shift reductions. For Los Angeles (Panel B), we find mostly null effects besides a reduction in penalized shift additions (a reduction of 15.5%). However, we find an increase in time extensions and time reductions in the penalty period of 9.0% and 8.8%, respectively. Finally, for Philadelphia (Panel C), we find a significant reduction in: (1) non-penalized shift additions, (2) penalized shift deletions, (3) non-penalized shift deletions. In contrast, we find a significant increase in: (1) penalized shift additions, and (2) penalized shift time reductions.

Overall, our analysis indicates that the penalties associated with the Fair Workweek Laws (FWL) were sufficiently large to incite real changes in scheduling practices. This is evident from the fact that, on average, the scheduling horizon of managers shifted from the penalty period $[t, t+k]$ to the non-penalty period $[t+k+1, \infty)$. It is important to note that these shifts are not simply the result of changes in an AI scheduling tool, but are direct consequences of managerial actions in response to the FWL. Additionally, our findings do not support the notion that the "loophole" of bilateral changes between managers and employees was prevalent enough to nullify the effects of the FWL. While we observed significant changes in scheduling practices, it is also worth noting that there are substantial differences in the effects across cities, which may be due to contemporaneous shocks or the type of underlying retailer in Chicago and Philadelphia. The variations observed across cities open up new avenues for research, particularly in understanding the reasons behind these differences and how they may impact workers in the long term.

TABLE 4. Effects on Schedule Stability

Panel A: Effects in Chicago

	log(Adj. <= 10) (1)	log(Adj.> 10) (2)	log(All Adj,) (3)
Chicago × Post-CFW	-0.789***	1.12***	0.044
	(0.094)	(0.100)	(0.080)
Control Variables	Yes	Yes	Yes
Observations	216,219	216,219	216,219
Adjusted R^2	0.677	0.743	0.395
Company-Store FEs	✓	✓	✓
Company-Year-Week FEs	✓	✓	✓

Panel B: Effects in Los Angeles

	log(Adj. <=14) (1)	log(Adj. > 14) (2)	log(All Adj,) (3)
Los Angeles × Post-LFW	0.103	0.394***	0.335***
	(0.092)	(0.064)	(0.090)
Control Variables	Yes	Yes	Yes
Raw Mean	40.35	111.17	151.53
Observations	144,545	144,545	144,545
Adjusted R^2	0.491	0.681	0.443
Company-Store FEs	✓	✓	✓
Company-Year-Week FEs	✓	✓	✓

Panel C: Effects in Philadelphia

	log(Adj. <= 10) (1)	log(Adj. > 10) (2)	log(All Adj,) (3)
Philadelphia × Post-PFW	-0.031	-0.145***	-0.066
	(0.052)	(0.035)	(0.042)
Control Variables	Yes	Yes	Yes
Observations	217,491	217,491	217,491
Adjusted R^2	0.687	0.713	0.390
Company-Store FEs	✓	✓	✓
Company-Year-Week FEs	✓	✓	✓

Notes: The estimates above correspond to fixed effects regression where the (log) number of schedule adjustments (based on the amount of advance notice) are regressed on a binary indicator variable that equals 1 for stores in an FWL jurisdiction (Chicago in Panel A, Los Angeles in Panel B, and Philadelphia in Panel C) after the effective date of the respective FWL. The dependent variables are as follows (all variables are log-transformed): (1) log(Adj. $\leq x$) is the number of adjustments made to shifts that are scheduled less than or equal to x days later, (2) log(Adj. $> x$) is the number of adjustments made to shifts that are scheduled more than x days later, (3) log(All Adj.) is the total number of adjustments. Recall that an "adjustment" is an addition, deletion, or edit to a shift characteristic. A shift can be subject to multiple adjustments. The store-date level counts (of the specific adjustments) are aggregated to the store-week levels. All specifications include store and company-year-week fixed effects. All specifications include the following control variables (at the store-week level): (1) (log) operating duration in minutes, (2) (log) total labor minutes, (3) (log) number of shifts, (4) (log) number of operating days. Standard errors that are double clustered at the store and year-week levels are presented in parentheses. *, **, and *** imply that coefficients are significant at the 10%, 5%, and 1%, respectively.

Table 5. Effects on Schedule Adjustments

Panel A: Effects in Chicago

	Add (0) (1)	Add (1) (2)	Delete (0) (3)	Delete (1) (4)	Extend (0) (5)	Extend (1) (6)	Reduce (0) (7)	Reduce (1) (8)
Chicago × Post-CFW	-0.601***	0.813***	-0.346***	0.599***	-0.509***	0.372***	-0.459***	0.385***
	(0.083)	(0.076)	(0.080)	(0.072)	(0.076)	(0.071)	(0.083)	(0.076)
Control Variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	216,219	216,219	216,219	216,219	216,219	216,219	216,219	216,219
Adjusted R^2	0.638	0.706	0.535	0.688	0.631	0.686	0.619	0.685
Company-Store FEs	✓	✓	✓	✓	✓	✓	✓	✓
Company-Year-Week FEs	✓	✓	✓	✓	✓	✓	✓	✓

Panel B: Effects in Los Angeles

	Add (0) (1)	Add (1) (2)	Delete (0) (3)	Delete (1) (4)	Extend (0) (5)	Extend (1) (6)	Reduce (0) (7)	Reduce (1) (8)
Los Angeles × Post-LFW	-0.155***	-0.070	0.059	0.013	0.090**	0.041	0.088***	-0.027
	(0.038)	(0.050)	(0.045)	(0.032)	(0.038)	(0.041)	(0.034)	(0.037)
Control Variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	209,906	209,906	209,906	209,906	209,906	209,906	209,906	209,906
Adjusted R^2	0.658	0.789	0.574	0.736	0.640	0.738	0.637	0.747
Company-Store FEs	✓	✓	✓	✓	✓	✓	✓	✓
Company-Year-Week FEs	✓	✓	✓	✓	✓	✓	✓	✓

Panel C: Effects in Philadelphia

	Add (0) (1)	Add (1) (2)	Delete (0) (3)	Delete (1) (4)	Extend (0) (5)	Extend (1) (6)	Reduce (0) (7)	Reduce (1) (8)
Philadelphia × Post-PFW	-0.034	-0.103***	-0.415***	-0.272***	0.098***	-0.076*	0.194***	0.068
	(0.046)	(0.035)	(0.052)	(0.052)	(0.037)	(0.042)	(0.039)	(0.046)
Control Variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	217,491	217,491	217,491	217,491	217,491	217,491	217,491	217,491
Adjusted R^2	0.648	0.678	0.548	0.681	0.635	0.678	0.646	0.669
Company-Store FEs	✓	✓	✓	✓	✓	✓	✓	✓
Company-Year-Week FEs	✓	✓	✓	✓	✓	✓	✓	✓

Notes: The estimates above correspond to fixed effects regression where the (log) number of a specific schedule adjustment is regressed on a binary indicator variable that equals 1 for stores in an FWL jurisdiction (Chicago in Panel A, Los Angeles in Panel B, and Philadelphia in Panel C) after the effective date of the respective FWL. The dependent variables are as follows (all variables are log-transformed): (1) Add is the number of shift additions, (2) Delete is the number of shift deletions, (3) Extend is the number of shift duration extensions, (4) Reduce is the number of shift duration reductions. The (0) and (1) indicate whether the specific adjustment was made with less or more than than k days' notice, respectively. The store-date level counts (of the specific adjustments) are aggregated to the store-week levels. All specifications include store and company-year-week fixed effects. All specifications include the following control variables (at the store-week level): (1) (log) operating duration in minutes, (2) (log) total labor minutes, (3) (log) number of shifts, (4) (log) number of operating days. Standard errors that are double clustered at the store and year-week levels are presented in parentheses. *, **, and *** imply that coefficients are significant at the 10, 5, and 1%, respectively.

Finally, we present evidence for the parallel-trends assumption for our previous analyses by presenting estimates from our dynamic model in Equation 2. Specifically, Panel A, B, and C of Figure 2 present the event study estimates associated with Column (2) of Table 4, i.e., the total number of non-penalized adjustments. Crucially, we find that the pre-trend estimates for all cities to be small and statistically insignificant. In terms of dynamics, we find that in Chicago, the magnitude of the effective size is relatively persistent throughout the post-period. In contrast, the effects in Los Angeles appear to grow through the post-period only to exhibit a considerable drop in the 15th week. Given the observed drop in the effect size in Los Angeles in the 15th week, it becomes difficult to draw firm conclusions about the long-term impacts of the FWLs based on the available data. More weeks of post-intervention data would be needed to better understand their enduring effects, and to ascertain whether the observed trends stabilize or continue to fluctuate over time. Finally, we find mostly null effects in Philadelphia (consistent with the associated PFW estimates in Panel C of Table 4). However, we showed earlier in Table 5 that major changes to specific scheduling actions (e.g. penalized shift deletions) were affected by the PFW.

Table 6. Effects on Shift Advance Notice

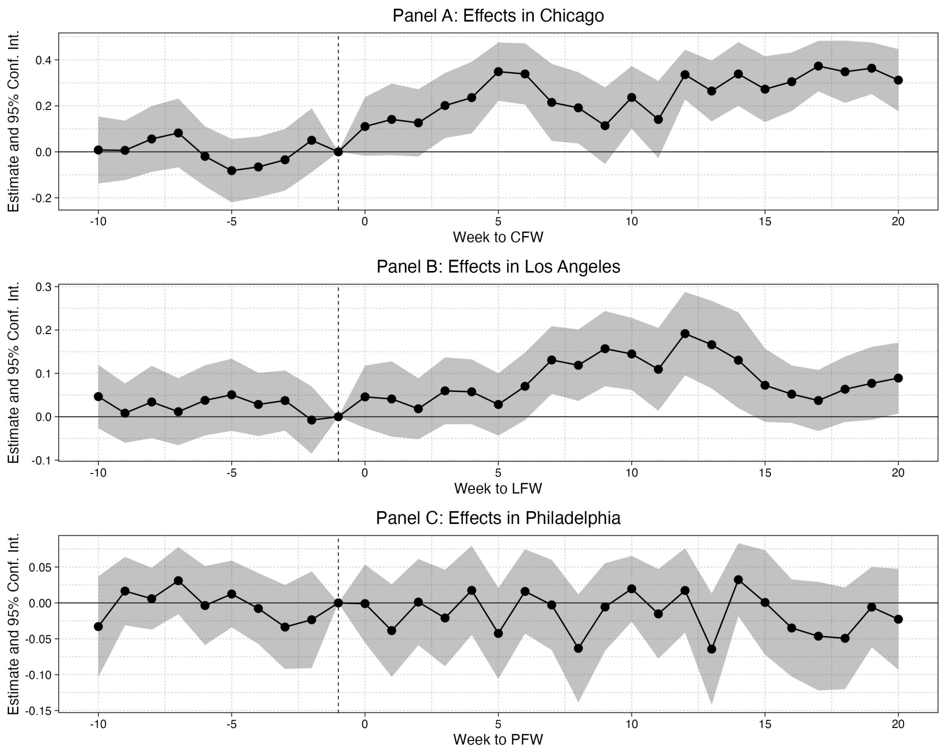
	log(Notice) (1)	Notice (2)	log(Notice) (3)	Notice (4)	log(Notice) (5)	Notice (6)
Chicago × Post-CFW	0.350***	3.77***				
	(0.026)	(0.314)				
Los Angeles × Post-LFW			0.059***	0.865***		
			(0.017)	(0.157)		
Philadelphia × Post-PFW					0.056***	0.930***
					(0.003)	(0.053)
Observations	12,291,387	12,291,975	12,853,503	12,853,861	12,148,012	12,148,844
Adjusted R^2	0.604	0.696	0.525	0.575	0.599	0.705
Mean of dep. var.	2.52	14.4	2.79	18.0	2.48	14.0
Company-Store-Employee FEs	✓	✓	✓	✓	✓	✓
Company-Year-Week FEs	✓	✓	✓	✓	✓	✓

Notes: The estimates above correspond to fixed effects regression where advance notice (log in odd-numbered columns, levels in even-numbered columns) is regressed on a binary indicator variable that equals 1 for stores in an FWL jurisdiction (Chicago in Columns 1–2, Los Angeles in Columns 3–4, and Philadelphia in Columns 5–6) after the effective date of the respective FWL. The unit of observation is at the store-year-week-shift level. All specifications include store-employee and company-year-week fixed effects. Standard errors that are double clustered at the store and year-week levels are presented in parentheses. *, **, and *** imply that coefficients are significant at the 10%, 5%, and 1%, respectively.

6.2. Effects on Advance Notice

While the results in the previous section showed that the scheduling horizon of managers in Chicago, Los Angeles, and Philadelphia were affected by their respective FWLs, they do not clearly show how these adjustments ultimately affected the predictability of work schedules. This is particularly true in Los Angeles, where penalized shift additions declined by 15.5%, but penalized shift time

FIGURE 2. Event Study Plots for Scheduling Adjustments in Chicago and Los Angeles



The above panels present event-study coefficients associated with Equation 2 for store-level outcomes. Across all panels, each point represents the coefficient associated with β_k, which is the effect of the FWL (Chicago in Panel A, Los Angeles in Panel B, and, Philadelphia in Column C) on the outcome of interest, relative to one week before the FWL (indicated by the dashed vertical line). Weeks are defined based on the focal date of July 1st, 2020, i.e. "Week 0" contains the dates between July 1st, 2020 and July 7th, 2020 (inclusive). All specifications include the following control variables (at the store-week level): (1) (log) operating duration in minutes, (2) (log) total labor minutes, (3) (log) number of shifts, (4) (log) number of operating days. Error bands are 95% confidence intervals that are constructed using standard errors that are double clustered at the store and company-year-week levels.

extensions increased by 9%. Accordingly, the natural question here is, on average, how did the three FWLs affect the amount of advance notice provided to employees?

We investigate this question by estimating fixed-effects regressions at the shift-level, where the dependent variable is the amount of advance notice provided for the shift. Columns (1) and (2) in Table 6 present the results for the CFW. The first column shows that the amount of advance notice increased by 34.8%. Column (2) shows the results in terms of levels. Specifically, we find that the CFW caused an increase in advance notice of 3.74 days. Next, Columns (3) and (4) present analogous results for the LFW. Here, we find that the LFW increased advance notice by 5.4%. In levels, this turns out to be 0.802 days. Finally, Columns (5) and (6) present the results for the PFW. Here, we find an increase of 5.6%, which in levels, turns out to be 0.930 days. All estimates presented in this table are statistically significant at the 1% level.

What explains the large difference in magnitudes? Comparing the shift-level regression estimates from Chicago, Los Angeles, and Philadelphia, we find that the effect of the CFW to be significantly larger than the LFW and PFW, despite a less stringent threshold of $k = 10$ (over $k = 14$ in Los Angeles). Specifically, we find that in in both levels and logs, the effects in Chicago are more than four times larger than the effects in Los Angeles.

There are many potential reasons for this difference. Here, we are only able to speculate. For instance, the difference could be driven by the different compositions of stores in the three cities (across our three retailers), which may reflect varying operational dynamics and managerial behaviors. Specifically, Chicago's store composition across our three retailers may include stores with more complex scheduling demands, thereby having a higher baseline need of scheduling adjustments. Therefore, when faced with the CFW, managers in these stores may have found it more advantageous or necessary to extend the scheduling horizon to comply with the regulation while managing their scheduling needs. Additionally, during the investigation period, Chicago experienced a simultaneous increase in the minimum wage, which may have further tightened operational budgets, nudging managers to plan schedules more meticulously to manage labor costs. Finally, the unfolding of the COVID-19 pandemic during the period under which the CFW was analyzed in Chicago could have further influenced the observed outcomes. The pandemic may have led to heightened uncertainty and operational challenges, potentially prompting managers to plan schedules further in advance to navigate these complexities. However, the pandemic was also relevant in Philadelphia, where the effects are much smaller.

In addition to the previously mentioned factors, another reason for the significant observed differences between our Chicago and Los Angeles samples could be the pre-existing scheduling practices in these cities before the effective date of their respective FWLs. It is plausible that stores in Los Angeles were already scheduling labor in a manner that largely complied with the FWL, hence requiring fewer adjustments post-enactment. On the other hand, stores in Chicago may have had scheduling practices that were more misaligned with the new FWL requirements, necessitating more substantial adjustments to comply with the law. This disparity in initial compliance could contribute to the larger effect size observed in Chicago compared to Los Angeles.

We provide evidence for this possibility by examining the average amount of advance notice provided to employees in impacted stores during the pre-FWL period. In Chicago, Los Angeles, and Philadelphia, the average amount of advance notice (standard deviation in parantheses) for treated stores was 7.60 (3.73), 12.87 (5.25), and 22.39 (5.39), respectively. Clearly, the "bite" of the FWL will be strongest for stores with historically low levels of advance notice, as was the case in Chicago. The Chicago stores had a pre-FWL advance notice average of 7.60 days, well below the threshold set by the CFW. In contrast, Los Angeles and Philadelphia stores had pre-FWL averages of 12.87 and 22.39 days, respectively, already complying with or exceeding their FWL requirements. This latter analysis highlights the importance of context when evaluating policy impacts. While the FWLs are designed to improve worker stability and work-life balance, their effects are modulated by pre-existing conditions, and their effectiveness needs to be evaluated in light of these initial conditions.

6.3. Effects on Schedule Stability

The results in the previous subsection showed a significant increase in advance notice of shifts in Chicago, with smaller increases in Los Angeles and Philadelphia. These results potentially suggest that employee shifts became more *predictable* due to the implementation of the three FWLs. However, in addition to schedule predictability, another explicit objective of FWLs is to increase schedule *stability*. Although there is no widely accepted definition or metrics of schedule stability, it generally refers to the consistency of work schedules over time. For instance, it is difficult to say that an employee who works alternating Mondays and alternates between starting work at 8AM and 10AM across weeks has a “stable" schedule. While they may be statistically correlated, schedule predictability and schedule stability are independent concepts. Using the example above, a work schedule that alternates start times between 8AM and 10AM can still be “predictable" if the focal worker is provided, say, 2 months advance notice for those shifts.

The importance of schedule stability in improving worker well-being is highlighted in Lu et al. (2022), as well as in the literature cited therein. One key reason for this is that schedules with high variance in start times can disrupt workers' circadian rhythms, negatively impacting their health and well-being. Furthermore, high schedule variability can make it more difficult for workers to make commitments outside of work, such as pursuing further education or coordinating childcare arrangements.

However, despite the implicit objective to improve schedule stability, it is unclear whether the FWLs can or have had such an effect. This is simply because there are no clear stipulations in any of the passed FWLs (including the CFW, LFW, and PFW) that require employers to make schedules more stable. Further exacerbating this issue is the lack of a clear, widely accepted definition for schedule stability. Therefore, an implicit assumption in the policy discussion surrounding FWLs is that increasing schedule predictability in the form of increasing the amount of advance notice provided to employees for their shifts can somehow also increase the stability of their work schedules.

We test this assumption using our data. To do so, we operationalize measures of schedule stability by following Lu et al. (2022) and Kwon and Raman (2023b) to construct day-of-the-week and hour-of-the-day schedule stability measures. Specifically, we construct three variables that aim to proxy for schedule stability in our shift-level dataset:

1. **Day-of-the-week Inconsistency**: Shifts scheduled on a day of the week (e.g. Monday) that the employee did not work the prior week. The employee must also have worked at least one

shift in the previous week (which eliminates new employees or employees who were on leave). We refer to this variable as N^*.

2. **Start-time Inconsistency (Version A)**: Shifts scheduled on a day of the week that the employee did work the previous week, but with a start time that differs by more than one hour. We refer to this variable as $\tilde{N}$.
3. **Start-time Inconsistency (Version B)** Shifts that are scheduled on back-to-back days for an employee, but with a start time that differs by more than one hour. We refer to this variable as $\hat{N}$.

Using our shift-level data, we identify shifts that exhibit day-of-the-week inconsistency or either of the two versions of start-time inconsistency. We then aggregate the data to the worker-week level by summing the respective counts of these measures. In addition to these schedule inconsistency metrics, we also look at the number of “clopening" shifts, which provide less than or equal to 10 hours of rest time between shifts. For Chicago, employees who agree to work clopening shifts in writing hours must be compensated 1.25x their regular pay rate for any hours worked less than 10 hours following the end of a previous shift. They also have the right to deny these shifts without retaliation. For Los Angeles, this premium pay is 1.5x.

We now discuss and present our results. For the CFW, Panel A of Table 7 presents estimates associated with using the (log) value of one plus N^*, $\tilde{N}$, $\hat{N}$, and clopen shifts as dependent variables in Equation 1, while controlling for the (log) number of total scheduled shifts. Here, we find null effects for our first three measures, which suggests that while the CFW was effective in increasing the amount of advance notice for covered workers, it was ineffective in increasing the stability of their work schedules. In addition, the effect on clopening shifts is statistically significant (at the 10% level), but has a point estimate that is very small (-0.009). We also note that the frequency of clopening shifts is already quite small in the sample, with its raw count having a mean of 0.046 in our worker-week sample.

We note that this result on schedule stability is inconsistent with the stated objectives of the CFW. For example, in her press release surrounding the CFW, then Mayor of Chicago Lori Lightfoot wrote about "*the burden [that the] lack of scheduling stability places on a family*" in the motivation to pass the CFW. Additionally, the Chair of the Workforce Committee and 10th Ward Alderman Susan Sadlowski Garza wrote that the CFW "will give hundreds of thousands of workers Chicago more certainty and stability". Accordingly, while the intent behind the CFW as articulated by city officials was to provide more certainty and stability to workers, our data does not yet reflect a significant change in schedule stability post-enactment.

We find very similar (null) effects in Los Angeles and Philadelphia, presented in Panels B and C of Table 7, respectively. Specifically, we find none of the estimates to be significant at the 5%

level. Overall, contrary to the explicit objectives of FWLs, there is no evidence that increasing work schedule predictability through adjustment penalties can also increase work schedule stability.

TABLE 7. Effects on Schedule Stability

Panel A: Effects in Chicago

	$\log(1+N^*)$ (1)	$\log(1+\tilde{N})$ (2)	$\log(1+\hat{N})$ (3)	log(No. 1 + Clopen) (4)
Chicago × Post-CFW	-0.032*	0.005	-0.001	-0.009**
	(0.018)	(0.021)	(0.025)	(0.004)
Control Variables	Yes	Yes	Yes	Yes
Observations	2,541,766	2,541,766	2,541,766	2,541,766
Adjusted R^2	0.297	0.409	0.539	0.316
Store-Company-Worker FEs	✓	✓	✓	✓
Year-Week-Company FEs	✓	✓	✓	✓

Panel B: Effects in Los Angeles

	$\log(1+N^*)$ (1)	$\log(1+\tilde{N})$ (2)	$\log(1+\hat{N})$ (3)	log(No. 1 + Clopen) (4)
Los Angeles × Post-LFW	0.014	0.007	-0.012	-0.016*
	(0.021)	(0.026)	(0.020)	(0.009)
Control Variables	Yes	Yes	Yes	Yes
Observations	2,610,487	2,610,487	2,610,487	2,610,487
Adjusted R^2	0.360	0.418	0.543	0.350
Store-Company-Worker FEs	✓	✓	✓	✓
Year-Week-Company FEs	✓	✓	✓	✓

Panel C: Effects in Philadelphia

	$\log(1+N^*)$ (1)	$\log(1+\tilde{N})$ (2)	$\log(1+\hat{N})$ (3)	log(No. 1 + Clopen) (4)
Philadelphia × Post-PFW	0.014	-0.022*	0.003	0.002
	(0.011)	(0.011)	(0.011)	(0.004)
Control Variables	Yes	Yes	Yes	Yes
Observations	2,644,372	2,644,372	2,644,372	2,644,372
Adjusted R^2	0.305	0.399	0.525	0.253
Store-Company-Worker FEs	✓	✓	✓	✓
Year-Week-Company FEs	✓	✓	✓	✓

Notes: The estimates above correspond to fixed effects regression where the dependent variables (listed above the column numbers) are regressed on a binary indicator variable that equals 1 for employees working in an FWL jurisdiction after its effective date, and 0 otherwise (Chicago in Panel A, Los Angeles in Panel B, and Philadelphia in Panel C). The dependent variables are: (1) N^* is the number of shifts that an employee works in a given week that falls on a week of the day (e.g., Monday) that they did not work the previous week, (2) $\tilde{N}$ is the number of shifts that an employee works in a given week that falls on a week of the day that they did work the previous week, but has a start time that differs by more than 60 minutes (in absolute value), (3) $\hat{N}$ is the number of back-to-back shifts that an employee works in a given week that has a start time that differs by more than 60 minutes (in absolute value), and (4) Clopen is the number of "clopening shifts", which are shifts that are scheduled on back to back days with less than 10 hours notice (only counting the shift on the latter day). The unit of observation is at the worker-week level. All specifications include the (log) number of shifts. All specifications include employee, store and year-week fixed effects. Standard errors that are double clustered at the store and company-year-week levels are presented in parentheses. *, **, and *** imply that coefficients are significant at the 10, 5, and 1%, respectively.

6.4. Effects on Scheduled Labor

In the previous two subsections, we studied whether the CFW, LFW, and PFW successfully achieved some of their explicit objectives (limited to a post-period of 20 weeks). Specifically, we showed that the three FWLs: (i) were effective in increasing the time horizon of scheduling adjustments made by store managers, which ultimately increased the amount of advance notice of work schedules provided to employees, but (ii) were largely ineffective in increasing the stability of work schedules. These two results address the first empirical question raised in Section 3.

We now address the second question raised in Section 3, which raised the question of whether FWLs can have unintended negative consequences, such as reduced scheduled labor minutes for affected workers. That is, we now examine the effects on outcomes that are *not* explicitly mentioned or addressed by either the CFW, LFW, and PFW but may be linked due to theory and concerns raised in public policy debates (discussed in Section 3).

We first investigate whether FWLs can have negative unintended consequences in the form of a reduction in scheduled shifts and hours for workers in affected stores. To do so, we estimate the DiD and event study models (Equations 1 and 2) at the worker-week level, where the (log) number of labor minutes and shifts are our two dependent variables. The estimates for this analysis are presented in Table 8. Here, we examine the effect of the CFW (Columns 1–2), LFW (Columns 3–4), and PFW (Columns 5–6) on total labor minutes and total shifts using Equation 1. In contrast to some of the concerns related to job loss or a reduction in scheduled hours for workers discussed in Section 3, we do not find evidence that the CFW resulted in a reduction in labor for impacted workers. Specifically, estimates of the effects on total labor minutes and shifts in Chicago and Los Angeles are small, precisely estimated, and statistically insignificant at the 10% level.

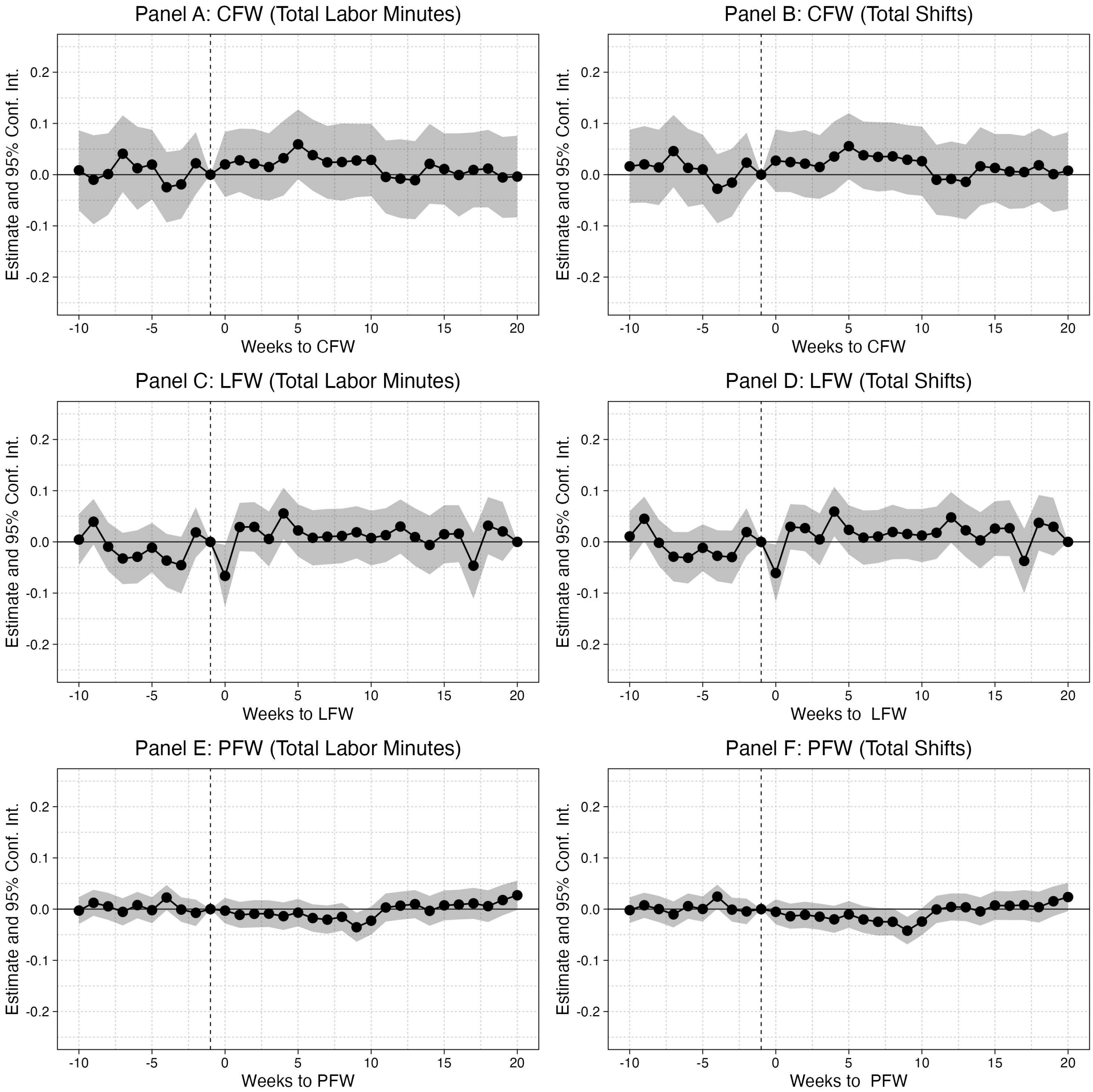
TABLE 8. Effect of CFW, LFW, and PFW on Scheduled Labor for Workers

	Chicago		Los Angeles		Philadelphia	
	log(Mins.) (1)	log(Shifts) (2)	log(Mins.) (3)	log(Shifts) (4)	log(Mins.) (5)	log(Shifts) (6)
Chicago × Post-CFW	0.012 (0.015)	0.009 (0.016)				
Los Angeles × Post-LFW			0.021 (0.020)	0.021 (0.020)		
Philadelphia × Post-PFW					-0.007 (0.009)	-0.010 (0.008)
Control Variables	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2,539,530	2,539,530	2,609,137	2,609,137	2,642,018	2,642,018
Adjusted R^2	0.628	0.477	0.737	0.621	0.638	0.491
Mean of dep. var.	7.40	1.32	7.39	1.32	7.37	1.31
Store-Company-Worker FEs	✓	✓	✓	✓	✓	✓
Year-Week-Company FEs	✓	✓	✓	✓	✓	✓

Notes: The estimates above correspond to fixed effects regression where the dependent variables (listed above the column numbers) are regressed on a binary indicator variable that equals 1 for stores in an FWL jurisdiction after its effective date, and 0 otherwise (Chicago in Columns 1–2, Los Angeles in Columns 3–4, and Philadelphia in Columns 5–6). The unit of observation is at the worker-week level. All specifications include store and company-year-week fixed effects. All specifications include the (log) of the total number of shifts and scheduled labor minutes, at the store-level, as control variables. Standard errors that are double clustered at the store and year-week levels are presented in parentheses. *, **, and *** imply that coefficients are significant at the 10, 5, and 1%, respectively.

Finally, to provide evidence for the parallel trends assumption and allow for possible dynamic treatment effects, we estimate Equation 2 when using the total number of minutes and shifts for workers in our sample (in all three cities). Figure 3 displays the coefficients associated with each β_k in Equation 2 for our two outcomes in Chicago (Panels A and B) and Los Angeles (Panels C and D). For both cities, our findings show no evidence of differential trends across the outcomes, with minimal fluctuations in the coefficients during the pre-FW period, and the 95% confidence intervals always include 0. Finally, examining the dynamics of our estimated effects (β_k), we note that our estimated null effects on the total amount of scheduled labor and shifts to be persistent throughout the entire post-FW period.

FIGURE 3. Event Study Plots for Worker-level Outcomes in Chicago



The above panels present event-study coefficients associated with Equation 2 for store-level outcomes. Across all panels, each point represents the coefficient associated with β_k, which is the effect of the respective FWL (Chicago in Panels A–B, Los Angeles in Panels C–D, and Philadelphia in Panels E–F) on the outcome of interest, relative to one week before the FWL (indicated by the dashed vertical line). Weeks are defined based on the focal date of July 1st, 2020, i.e. "Week 0" contains the dates between July 1st, 2020 and July 7th, 2020 (inclusive). Error bands are 95% confidence intervals that are constructed using standard errors that are double clustered at the store and company-year-week levels.

Overall, the results from this subsection are inconsistent with commonly raised concerns that FWLs can have negative unintended consequences in the form of a reduction in scheduled labor minutes or shifts for workers in impacted stores. We showed the lack of such effects in the full sample (in Chicago, Los Angeles, and Philadelphia), and later in subsamples (Section 7.2). In the subsection below, we also show that these null effects are

6.5. Heterogeneous Effects on Scheduled Labor by Worker Characteristics

In this subsection, we study heterogeneous effects based on worker characteristics such as their status (part-time versus full-time) and wage. We do so to test two hypotheses about the potential effects of the CFW. For the former, there is recent evidence from Yelowitz (2022) that FWLs may increase the utilization of part-time workers. For the latter, we test the hypothesis that because adjustment penalties are tied to employees' wages, FWLs may have differential effects on high- versus low-wage workers. That is, it is more expensive to adjust a high-wage worker's schedule than a low-wage worker's schedule without providing sufficient notice. Here, the concern is that protections from FWLs may be concentrated on higher-wage workers, or that short-notice shift adjustments may be passed from higher-wage to lower-wage workers.

To investigate these possibilities, we test for heterogeneous effects using a triple difference-in-differences specification that interacts the $X_{i,t}$ variable in Equation 1 with three employee charactertics (separately): (1) Gender, (2) Wage, and (3) Part-Time Status.

Tables 9 and 10 present our estimates of Equation 1 when examining heterogeneous effects on advance notice and scheduled labor (by including an interaction term), respectively. Concerning the former, we find mixed evidence of heterogeneous effects. Specifically, in Chicago (Columns 1–2), we find no evidence of heterogeneous effects, as all interaction terms are statistically insignificant at the 10% level. However, in Los Angeles, we find that advance notice is larger for males (7.3%, significant at the 10%level), whereas the amount of advance notice is smaller for part-time workers. These latter results suggest that part-time workers are more likely to receive shorter advance notice than their full-time counterparts. This finding is consistent with the hypothesis that FWLs can lead to increased utilization of part-time workers in the revelation of new information about demand or changes in labor supply, potentially as a way for employers to have more flexibility in scheduling without incurring penalties. However, we find opposite effects in Philadelphia, with part-time workers receiving more advance notice (4.2%, significant at the 1% level).

Next, Table 10 presents our estimates when examining heterogeneous effects on scheduled labor. Here, the results are more consistent than on advance notice. Here, we find no evidence of *any* heterogeneous effects across all three cities based on part-time status or wage rate. The one exception here is a negative effect for males in Chicago, who appear to be scheduled for less work than their female counterparts.

A key takeaway from this subsection is that the effects of the FWLs do not appear to vary based on employee wages. And it is not as if the workers in our sample exhibit small variations in their hourly wage. The coefficient of variation (mean over standard deviation) in hourly wage rates in our Chicago, Los Angeles, and Philadelphia sample are 2.61, 0.77, and 3.81, respectively. As mentioned previously, a plausible concern associated with Fair Workweek laws is that they may inadvertently confer benefits only to employees with relatively larger wages because adjustment penalties are directly tied to employees' wages. This point is also relevant to our discussion and analyses as a simultaneous increase in the minimum wage accompanied the CFW. Accordingly, the null heterogeneous effects associated with employees' wages do not indicate that relative wages among employees in our sample are an important determinant of our observed results.

Overall, the mixed heterogeneous effects based on worker characteristics such as gender, and part-time status, as discussed above, are made further challenging to interpret in light of other contemporaneous events. For instance, the increase in the minimum wage in Chicago, and the COVID-19 pandemic in both Chicago and Philadelphia, represent significant exogenous shocks that could potentially confound our results. Specifically, the minimum wage increase in Chicago may have affected the relationship between wages and scheduling practices. At the same time, the COVID-19 pandemic may also have had wide-ranging effects on worker schedules due to changes in demand and labor supply. These contemporaneous events make it difficult to cleanly identify the true heterogeneous effects of the FWLs in our sample. Therefore, while our results provide evidence of differential effects based on worker characteristics, we urge some caution in interpreting these results as conclusive. Further research is required to disentangle the complex interplay of FWLs, worker characteristics, and external shocks to fully understand the impacts of these policies.

Table 9. Heterogeneous Effects on Workers' Schedules (Advance Notice)

	Chicago		Los Angeles		Philadelphia	
	log(Notice) (1)	Notice (2)	log(Notice) (3)	Notice (4)	log(Notice) (5)	Notice (6)
Chicago × Post-CFW	0.364***	3.85***				
	(0.068)	(0.865)				
Chicago × Post-CFW × log(Wage Rate)	0.007	0.106				
	(0.011)	(0.131)				
Chicago × Post-CFW × Male	-0.090	-0.881				
	(0.057)	(0.687)				
Chicago × Post-CFW × Part-Time Worker	0.069	0.582				
	(0.055)	(0.673)				
Los Angeles × Post-LFW			0.005	0.388		
			(0.041)	(0.486)		
Los Angeles × Post-LFW × log(Wage Rate)			0.011	0.209		
			(0.013)	(0.179)		
Los Angeles × Post-LFW × Male			0.073*	0.668*		
			(0.038)	(0.380)		
Los Angeles × Post-LFW × Part-Time Worker			-0.080**	-0.887**		
			(0.033)	(0.403)		
Philadelphia × Post-PFW					-0.041	0.016
					(0.034)	(0.394)
Philadelphia × Post-PFW × log(Wage Rate)					0.023**	0.188
					(0.011)	(0.129)
Philadelphia × Post-PFW × Male					-0.011	-0.141
					(0.009)	(0.119)
Philadelphia × Post-PFW × Part-Time Worker					0.042***	0.528***
					(0.011)	(0.143)
Observations	11,084,001	11,084,366	10,874,562	10,874,659	11,102,108	11,102,755
Adjusted R^2	0.603	0.696	0.537	0.584	0.601	0.708
Company-Store-Employee FEs	✓	✓	✓	✓	✓	✓
Company-Year-Week FEs	✓	✓	✓	✓	✓	✓

Notes: The estimates above correspond to fixed effects regression where the dependent variables (advance notice) are regressed on a binary indicator variable that equals 1 for stores in an FWL jurisdiction after its effective date, and 0 otherwise (Chicago in Columns 1–2, Los Angeles in Columns 3–4, and Philadelphia in Columns 5–6). The unit of observation is at the worker-week level. All specifications include store-employee and company-year-week fixed effects. Standard errors that are double clustered at the store and year-week levels are presented in parentheses. *, **, and *** imply that coefficients are significant at the 10, 5, and 1%, respectively.

TABLE 10. Heterogeneous Effects on Workers' Schedules (Scheduled Labor)

	Chicago		Los Angeles		Philadelphia	
	log(Mins.) (1)	log(Shifts) (2)	log(Mins.) (3)	log(Shifts) (4)	log(Mins.) (5)	log(Shifts) (6)
Chicago × Post-CFW	0.039	0.043				
	(0.028)	(0.030)				
Chicago × Post-CFW × log(Wage Rate)	-0.006	-0.010				
	(0.008)	(0.007)				
Chicago × Post-CFW × Male	-0.064***	-0.061**				
	(0.020)	(0.029)				
Chicago × Post-CFW × Part-Time Worker	0.050	0.047				
	(0.053)	(0.053)				
Los Angeles × Post-LFW			0.031	0.011		
			(0.033)	(0.037)		
Los Angeles × Post-LFW × log(Wage Rate)			-0.003	-0.0004		
			(0.022)	(0.020)		
Los Angeles × Post-LFW × Male			-0.028	-0.003		
			(0.042)	(0.046)		
Los Angeles × Post-LFW × Part-Time Worker			0.054	0.069		
			(0.051)	(0.048)		
Philadelphia × Post-PFW					-0.018	-0.016
					(0.028)	(0.025)
Philadelphia × Post-PFW × log(Wage Rate)					0.018	0.014
					(0.011)	(0.010)
Philadelphia × Post-PFW × Male					0.020	0.008
					(0.013)	(0.012)
Philadelphia × Post-PFW × Part-Time Worker					-0.038	-0.024
					(0.042)	(0.039)
Control Variables	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2,402,762	2,402,762	2,388,334	2,388,334	2,525,806	2,525,806
Adjusted R^2	0.617	0.468	0.730	0.608	0.619	0.475
Store-Company-Worker FEs	✓	✓	✓	✓	✓	✓
Year-Week-Company FEs	✓	✓	✓	✓	✓	✓

Notes: The estimates above correspond to fixed effects regression where the dependent variables (log number of labor minutes or shifts) are regressed on a binary indicator variable that equals 1 for stores in an FWL jurisdiction after its effective date, and 0 otherwise (Chicago in Columns 1–2, Los Angeles in Columns 3–4, and Philadelphia in Columns 5–6). The unit of observation is at the worker-week level. All specifications include store and company-year-week fixed effects. All specifications include the (log) of the total number of shifts and scheduled labor minutes, at the store-level, as control variables. Standard errors that are double clustered at the store and year-week levels are presented in parentheses. *, **, and *** imply that coefficients are significant at the 10, 5, and 1%, respectively.

6.6. Effects on Employee Hiring and Turnover

Next, we examine two additional hypothesized unintended consequences of FW-related laws: employee turnover and employee hiring. For the former, the hypothesized mechanism here is that some employees are “on-call" employees who are utilized only during periods with high demand volatility, and are thus scheduled with short notice. As these employees become more expensive to schedule on short notice (due to the FW law), one hypothesis is that these employees are let go. For the latter, because employers are now required to provide “good faith" estimates of how much work will be available, one hypothesis is that hiring only occurs when employers are more confident of how much work they can guarantee for new hires.

TABLE 11. Effects on Hiring and Turnover

	Chicago		Los Angeles		Philadelphia	
	Hires (1)	Turnover (2)	Hires (3)	Turnover (4)	Hires (5)	Turnover (6)
Chicago × Post-CFW	-0.066	0.011				
	(0.057)	(0.030)				
Los Angeles × Post-LFW			-0.007	-0.011		
			(0.019)	(0.013)		
Philadelphia × Post-PFW					-0.124***	0.020
					(0.044)	(0.028)
Control Variables	Yes	Yes	Yes	Yes	Yes	Yes
Observations	216,219	216,219	209,906	209,906	217,491	217,491
Adjusted R^2	0.092	0.128	0.105	0.113	0.091	0.136
Store-Company FEs	✓	✓	✓	✓	✓	✓
Year-Week-Company FEs	✓	✓	✓	✓	✓	✓

Notes: The estimates above correspond to fixed effects regression where the dependent variables (listed above the column numbers) are regressed on a binary indicator variable that equals 1 for Chicago stores after the calendar week that includes July 1, 2020, and zero otherwise. For dependent variables, Column (1) uses the (log) of one plus the number of full-time hires, Column (2) uses the (log) of one plus the number of part-time hires, Column (3) uses uses the (log) of one plus the number of full-time employee turnover, and Column (4) uses the (log) of one plus the number of part-time employee turnover. The unit of observation is at the store-week level. All specifications include the following control variables (at the store-week level): (1) (log) operating duration in minutes, (2) (log) total labor minutes, (3) (log) number of shifts, (4) (log) number of operating days. All specifications include store and year-week fixed effects. Standard errors that are double clustered at the store and year-week levels are presented in parentheses. *, **, and *** imply that coefficients are significant at the 10, 5, and 1%, respectively.

Table 11 presents the estimates on store hiring and turnover (broken up by part-time and full-time status), respectively. Across all four columns, we find null effects on hiring (full-time, and part-time) and turnover (full-time and part-time). Note that a key limitation with the effects of turnover is that we do not know whether the turnover/termination was voluntary or involuntary (i.e., the employee was fired or left voluntarily). Null effects are also found when using Poisson fixed effects regressions and using the actual counts in the dependent variables (and not the log transform, where a value of 1 is added to the count). Overall, these latter results rule out concerns that FWLs are associated with job loss (potentially due to reduced scheduled labor, which has already been ruled out) and difficulties in hiring.

7. Robustness Checks

In this section, we perform several additional analyses to show the robustness of our results. These analyses focus on re-estimating our main specifications using alternate subsamples.

7.1. Results For Continuously Employed Employees

One potential issue with our sample is that there may be workers that are hired or fired during the pre- and post-FWL period. We note from Section 6.6 that the CFW and LFW did not have a significant effect on employee hiring and turnover. However, this does not imply the absence of hiring or turnover in our treated stores. Importantly, these new hires may have been hired specifically in response to the demands or working conditions created by the Fair Workweek laws, and their scheduling experiences may not reflect the broader impact of these laws on schedule stability for the existing workforce. That is, if managers hire workers with the clauses of the FWL in mind (e.g., good faith estimates of the number of hours worked), they may allocate schedules to these new hires during the pre-FWL period in a manner that aligns with the anticipated FWL regulations. This approach could potentially affect our estimates as these schedules may not exhibit the full extent of variability or instability that might have been observed otherwise, given that they were crafted with the forthcoming FWL compliance in mind.

Overall, focusing on this group allows us to address a more refined question on the effects of FWLs: "Do FWLs improve the work schedules of *current* employees in impacted stores, or do they simply improve the work schedules of new hires?" To answer this question, we restrict our attention in our worker sample to those that have: (i) worked at least one shift every pre-FW week, and (ii) worked at least one shift during the post-FW period. Note that because our sample is at the year $\times$ worker levels, working at least one shift in a week means that the focal worker is non-missing throughout the entire "pre" period in our worker panel. We refer to employees in this particular subsample as those who have been "continuously employed" before the effective date of the FWL. This subsample provides an opportunity to evaluate the effects of FWL on a stable group of workers, minimizing the confounding effects from hiring or firing processes.

Table A1 presents our results on scheduled labor for this subsample of workers. Comparing our results with Table 8, we find *all* estimates in our subsample of continuously employed workers to be smaller. Additionally, estimates are all statistically insignificant at the 10% level. We also note here that there is a considerable drop in sample sizes across the CFW, LFW, and PFW, samples, which may suggest non-negligible rates of turnover, hiring, and the utilization of workers that have inconsistent number of scheduled hours and shifts across weeks. Specifically, the sample sizes drop from 2.5 to 2.1 million in Chicago, 2.6 to 2.2 million in Los Angeles, and 2.6 to 2.2 million in Philadelphia. This drop in sample size underscores the potential importance of analyzing the results separately for continuously employed workers.

Next, Table A2 presents the results for the amount of advance notice (at shift-level) provided to continuously employed workers. These results are also very similar in both magnitudes and statistical significance with its counterpart that uses all control workers (Table 6). Next, Table A3 presents the results for heterogeneous effects based on worker characteristics, which is the counterpart of Table 10. One key difference here is that effect sizes vary based on employees' wages in Los Angeles and Philadelphia. Specifically, the effect sizes appear to be decreasing in wages in Los Angeles. The results for the latter are partially explainable by the fact that stores in Philadelphia cut down on part-time labor (Panel C of Table **??**). However, we do not see changes in the ratio between full and part-time labor in Los Angeles, which may imply that for continuously employed workers, there may be some unintended consequences in the form of reduced scheduled labor for higher-paid employees. One hypothesis here is that employers in Los Angeles may be compensating for increased labor costs associated with FWL penalties by reducing hours for those who earn more. Consequently, this shift in scheduling for higher-paid employees may reflect an attempt to manage costs.

Overall, the primary takeaway from this particular analysis is that evaluating the efficacy of FWLs may require separate analyses of "continuously employed" workers (defined above), new employees, and employees who are irregularly scheduled before the effective date of the FWL. Because FWLs are often introduced and passed by on their potential provisions for *existing* workers, it may be necessary to disentangle the effects on different subgroups of workers. While the data suggests variations in the effect sizes across cities and worker wage brackets, the underlying reasons for these discrepancies must be further investigated. The preliminary evidence from Los Angeles suggests possible adaptations by employers to manage increased labor costs, a finding that warrants a deeper exploration. We leave this exploration to future research.

7.2. Results Using Matched Samples

Our main analysis used all stores (and their respective workers) *not* operating in a jurisdiction with an FWL. For example, in our Chicago sample, we dropped all stores operating in Los Angeles, Philadelphia, and all other jurisdictions[18] with an FWL.

In this section, we additionally consider a more "restrictive" sample that we construct by matching on store and worker characteristics. Depending on the unit of observation, we match either at the worker or store-level. At the store-level, we match by taking the following steps:

[18] These include San Francisco, Oregon (statewide), New York City, Emeryville (CA), and Seattle.

1. We take the average of the following variables in the pre-FWL period for each store in the focal sample: (i) Labor minutes, (ii) Shifts, (iii) Advance Notice, (iv) Operating Duration. We also take the count of the total number of operating days.
2. We use nearest-neighbor matching on the above variables (using a probit link). We match without replacement, and we exact match on the focal company (i.e., a single treated store is matched to a single control store within the same company).

At the worker-level, we match by taking the following steps:

1. We take the average of the following variables in the pre-FWL period for each worker in the focal sample: (i) Labor minutes, (ii) Shifts, (iii) Hourly Wage, (iv) Advance Notice.
2. We use nearest-neighbor matching on the above variables (using a probit link). We match without replacement, and we exact match on company, full-time status, and gender (i.e., a single treated worker is matched to a single control worker within the sample company of the same recorded gender and full-time status).

Tables A8-A10 present the results when matching at the worker-level. Here, the essence of our results from using the full sample is unchanged. Specifically, treated workers: (i) Do not experience a reduction in scheduled labor (Table A9), (ii) Experience an increase in advance notice (Table A8), (iii) Do not experience an increase in schedule stability (Table A10). We note that very similar magnitudes and statistical significance are obtained when matching at the store-level, and then using all workers that belong to treated and matched controls from these stores.

Finally, Tables A5- A7 presents the results when matching at the store-level. Again, the essence of our results from using the full sample is unchanged. Specifically, treated stores: (i) Do not experience an increase in turnover (Table A5), (ii) Do not experience a reduction in hiring, except for the PFW (Table A5), (iii) Do not experience a reduction in scheduled labor, except for the PFW (Table A7), and (iv) Reduce making work schedule adjustments within the respective FWL's threshold date, which ultimately increases the advance notice provided at the shift-level (Table A6).

8. Conclusion and Discussion of Limitations

This paper presents the first non-survey-based causal evidence of the impact of Fair Workweek (FW) laws on worker schedules and employment. We address a critically important question left unanswered in the literature: whether FWLs achieve their stated objectives of increasing schedule predictability and stability for shift-based workers. The key concern that makes this question non-trivial is that employers do not lose the ability to schedule their employees on short notice. Rather, they are penalized for making unilateral adjustments to work schedules. Accordingly, understanding whether employers modify their scheduling practices to improve schedules or simply provide additional compensation to employees is of first-order importance for policymakers. It is difficult to consider FWLs a complete success if large firms continue to frequently schedule their employees on short notice and only pay out the required penalties.

Our paper stands out in multiple literatures, such as sociology and operations management, due to its empirical analyses that directly focus on evaluating the effects of an FW-related law. While various past studies have claimed to address the effectiveness of FWLs with their empirical analyses, they do so either indirectly (by examining related scheduling practice adjustments, (e.g., Kesavan et al. (2022); Lu et al. (2022)) or with econometric concerns based on their utilization of employee surveys (e.g., Harknett et al. (2021)). Therefore, our research is the first to directly examine the effects of an FW-related law on worker schedules and employment using administrative scheduling data that is both granular (i.e., at the shift-level) and comprehensive (i.e., providing rich details on the amount of advance notice provided along with employee-related covariates).

The results of this study have important implications for policymakers, labor advocates, and business leaders in assessing the potential benefits of FWLs. The findings indicate that FWLs increase the amount of advance notice provided to employees without negatively affecting the number of scheduled shifts and work minutes allocated to them. Moreover, there is no evidence that impacted stores decreased labor utilization or employee headcount, or faced increased difficulties in hiring new employees. Additionally, the study finds no evidence that FWLs lead to increased employee turnover or inequitable scheduling practices due to adjustment penalties being a function of employees' wages. Therefore, the study suggests that the CFW has effectively improved employees' work schedules without the unintended consequences often cited in the literature and public policy debates.

It is important to acknowledge that this study has several limitations. First, the generalizability of our findings to other jurisdictions and industries may be limited since we only examine two large retail chains within one jurisdiction. However, the fact that the companies we studied are well-resourced and large suggests that our results may hold for smaller companies as well (who have fewer financial resources to pay out adjustment penalties for short-notice shift adjustments). Second, our analysis focuses exclusively on the effects of FWLs on workers and does not consider their effects on business performance. As a result, we cannot make definitive statements about the impact of the CFW on employers and businesses. Finally, while our analysis of the 20-week post-period likely captures short-, medium-, and long-run effects from the perspective of retail managers, longer horizons may be necessary to fully understand the dynamics of our estimated treatment effects. These limitations highlight the need for further research on the effects of FWLs in different settings and industries.

Overall, this paper provides some insights into the impact of FWLs on worker schedules. However, additional research is necessary to fully understand the longer-term effects and broader implications of these regulations, including their effects on employers and business performance. By providing a more comprehensive understanding of the effects of FWLs, future research can inform policymakers and other stakeholders about the potential costs and benefits of these regulations, enabling them to make more informed decisions.

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Appendix A. Additional Results

A.1. Effects on Continuously Employed Workers

We present below the results on workers that have been "continuously employed" before the effective day of each FWL. We define a "continuously employed" as one that has worked at least one shift during every pre-FWL week (i.e., has no non-missing observation during the entire "pre" period of the worker-panel).

TABLE A1. Effect of CFW, LFW, and PFW on Scheduled Labor for Continuously Employed Workers

	Chicago		Los Angeles		Philadelphia	
	log(Mins.) (1)	log(Shifts) (2)	log(Mins.) (3)	log(Shifts) (4)	log(Mins.) (5)	log(Shifts) (6)
Chicago × Post-CFW	0.004	0.002				
	(0.024)	(0.022)				
Los Angeles × Post-LFW			0.009	0.011		
			(0.023)	(0.024)		
Philadelphia × Post-PFW					-0.003	-0.005
					(0.010)	(0.009)
Control Variables	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,956,238	1,956,238	2,008,594	2,008,594	2,086,618	2,086,618
Adjusted R^2	0.593	0.411	0.730	0.596	0.622	0.443
Store-Company-Worker FEs	✓	✓	✓	✓	✓	✓
Year-Week-Company FEs	✓	✓	✓	✓	✓	✓

Notes: The estimates above correspond to fixed effects regression where the dependent variables (listed above the column numbers) are regressed on a binary indicator variable that equals 1 for stores in an FWL jurisdiction after its effective date, and 0 otherwise (Chicago in Columns 1–2, Los Angeles in Columns 3–4, and Philadelphia in Columns 5–6). The unit of observation is at the worker-week level. All specifications include store and company-year-week fixed effects. All specifications include the (log) of the total number of shifts and scheduled labor minutes, at the store-level, as control variables. Standard errors that are double clustered at the store and year-week levels are presented in parentheses. *, **, and *** imply that coefficients are significant at the 10, 5, and 1%, respectively.

Table A2. Effects on Shift Advance Notice for Continuously Employed Workers

	log(Notice) (1)	Notice (2)	log(Notice) (3)	Notice (4)	log(Notice) (5)	Notice (6)
Chicago × Post-CFW	0.401***	4.40***				
	(0.031)	(0.383)				
Los Angeles × Post-LFW			0.059***	0.735***		
			(0.020)	(0.188)		
Philadelphia × Post-PFW					0.041***	0.774***
					(0.004)	(0.056)
Observations	8,318,858	8,319,048	8,331,386	8,331,438	8,765,116	8,765,537
Adjusted R^2	0.614	0.707	0.551	0.596	0.605	0.712
Mean of dep. var.	2.52	14.4	2.82	18.4	2.49	14.3
Company-Store-Employee FEs	✓	✓	✓	✓	✓	✓
Company-Year-Week FEs	✓	✓	✓	✓	✓	✓

Notes: The estimates above correspond to fixed effects regression where advance notice (log in odd numbered columns, levels in even numbered columns) is regressed on a binary indicator variable that equals 1 for stores in an FWL jurisdiction (Chicago in Columns 1–2, Los Angeles in Columns 3–4, and Philadelphia in Columns 5–6) after the effective date of the respective FWL. The unit of observation is at the store-year-week-shift level. All specifications include store-employee and company-year-week fixed effects. Standard errors that are double clustered at the store and year-week levels are presented in parentheses. *, **, and *** imply that coefficients are significant at the 10%, 5%, and 1%, respectively.

Table A3. Effect of CFW, LFW, and PFW on Scheduled Labor for Continuously Employed Workers

	Chicago		Los Angeles		Philadelphia	
	log(Mins.) (1)	log(Shifts) (2)	log(Mins.) (3)	log(Shifts) (4)	log(Mins.) (5)	log(Shifts) (6)
Chicago × Post-CFW	-0.0008	-0.005				
	(0.046)	(0.048)				
Chicago × Post-CFW × log(Wage Rate)	-0.006	-0.013				
	(0.010)	(0.010)				
Chicago × Post-CFW × Male	-0.036	-0.014				
	(0.043)	(0.044)				
Chicago × Post-CFW × Part-Time Worker	0.143*	0.122*				
	(0.072)	(0.069)				
Los Angeles × Post-LFW			-0.008	-0.012		
			(0.041)	(0.039)		
Los Angeles × Post-LFW × log(Wage Rate)			-0.040**	-0.030**		
			(0.016)	(0.015)		
Los Angeles × Post-LFW × Male			-0.004	-0.0006		
			(0.053)	(0.051)		
Los Angeles × Post-LFW × Part-Time Worker			0.074	0.054		
			(0.057)	(0.056)		
Philadelphia × Post-PFW					-0.038	-0.024
					(0.038)	(0.036)
Philadelphia × Post-PFW × log(Wage Rate)					0.033*	0.029*
					(0.018)	(0.017)
Philadelphia × Post-PFW × Male					-0.016	-0.019
					(0.017)	(0.015)
Philadelphia × Post-PFW × Part-Time Worker					-0.022	-0.029*
					(0.017)	(0.015)
Control Variables	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R^2	0.606	0.439	0.667	0.605	0.505	0.418
Store-Company-Worker FEs	✓	✓	✓	✓	✓	✓
Year-Week-Company FEs	✓	✓	✓	✓	✓	✓

Notes: The estimates above correspond to fixed effects regression where the dependent variables (log number of labor minutes or shifts) are regressed on a binary indicator variable that equals 1 for stores in an FWL jurisdiction after its effective date, and 0 otherwise (Chicago in Columns 1–2, Los Angeles in Columns 3–4, and Philadelphia in Columns 5–6). The unit of observation is at the worker-week level. All specifications include store and company-year-week fixed effects. All specifications include the (log) of the total number of shifts and scheduled labor minutes, at the store-level, as control variables. Standard errors that are double clustered at the store and year-week levels are presented in parentheses. *, **, and *** imply that coefficients are significant at the 10, 5, and 1%, respectively.

Table A4. Effects on Schedule Stability

Panel A: Effects in Chicago

	$\log(1+N^*)$ (1)	$\log(1+\tilde{N})$ (2)	$\log(1+\hat{N})$ (3)	log(No. 1 + Clopen) (4)
Chicago × Post-CFW	-0.031	0.003	0.002	-0.009*
	(0.020)	(0.024)	(0.028)	(0.005)
Control Variables	Yes	Yes	Yes	Yes
Observations	2,100,494	2,100,494	2,100,494	2,100,494
Adjusted R^2	0.297	0.412	0.544	0.326
Store-Company-Worker FEs	✓	✓	✓	✓
Year-Week-Company FEs	✓	✓	✓	✓

Panel B: Effects in Los Angeles

	$\log(1+N^*)$ (1)	$\log(1+\tilde{N})$ (2)	$\log(1+\hat{N})$ (3)	log(No. 1 + Clopen) (4)
Los Angeles × Post-LFW	0.009	0.005	-0.016	-0.019*
	(0.022)	(0.029)	(0.022)	(0.010)
Control Variables	Yes	Yes	Yes	Yes
Observations	2,203,718	2,203,718	2,203,718	2,203,718
Adjusted R^2	0.370	0.426	0.555	0.362
Store-Company-Worker FEs	✓	✓	✓	✓
Year-Week-Company FEs	✓	✓	✓	✓

Panel C: Effects in Philadelphia

	$\log(1+N^*)$ (1)	$\log(1+\tilde{N})$ (2)	$\log(1+\hat{N})$ (3)	log(No. 1 + Clopen) (4)
Philadelphia × Post-PFW	0.016	-0.021*	0.004	0.002
	(0.011)	(0.012)	(0.011)	(0.004)
Control Variables	Yes	Yes	Yes	Yes
Observations	2,263,351	2,263,351	2,263,351	2,263,351
Adjusted R^2	0.306	0.405	0.533	0.260
Store-Company-Worker FEs	✓	✓	✓	✓
Year-Week-Company FEs	✓	✓	✓	✓

Notes: The estimates above correspond to fixed effects regression where the dependent variables (listed above the column numbers) are regressed on a binary indicator variable that equals 1 for employees working at Chicago stores after the calendar week that includes July 1, 2020, and zero otherwise. The dependent variables are: (1) N^* is the number of shifts that an employee works in a given week that falls on a week of the day (e.g., Monday) that they did not work the previous week, (2) $\tilde{N}$ is the number of shifts that an employee works in a given week that falls on a week of the day that they did work the previous week, but has a start time that differs by more than 60 minutes (in absolute value), and (3) $\hat{N}$ is the number of back-to-back shifts that an employee works in a given week that has a start time that differs by more than 60 minutes (in absolute value). The unit of observation is at the worker-week level. All specifications include employee, store and year-week fixed effects. Standard errors that are double clustered at the store and year-week levels are presented in parentheses. *, **, and *** imply that coefficients are significant at the 10, 5, and 1%, respectively.

A.2. Effects using Matched Sample (Matches Constructed at the Store-Level)

TABLE A5. Effects on Hiring and Turnover (Matched Store Sample)

	Chicago		Los Angeles		Philadelphia	
	Hires (1)	Turnover (2)	Hires (3)	Turnover (4)	Hires (5)	Turnover (6)
Chicago × Post-CFW	-0.082	0.024				
	(0.079)	(0.047)				
Los Angeles × Post-LFW			0.019	-0.006		
			(0.021)	(0.017)		
Philadelphia × Post-PFW					-0.144***	-0.011
					(0.048)	(0.037)
Control Variables	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R^2	0.069	0.084	0.109	0.107	0.122	0.118
Store-Company FEs	✓	✓	✓	✓	✓	✓
Year-Week-Company FEs	✓	✓	✓	✓	✓	✓

Notes: The estimates above correspond to fixed effects regression where the dependent variables (listed above the column numbers) are regressed on a binary indicator variable that equals 1 for Chicago stores after the calendar week that includes July 1, 2020, and zero otherwise. For dependent variables, Column (1) uses the (log) of one plus the number of full-time hires, Column (2) uses the (log) of one plus the number of part-time hires, Column (3) uses uses the (log) of one plus the number of full-time employee turnover, and Column (4) uses the (log) of one plus the number of part-time employee turnover. The unit of observation is at the store-week level. All specifications include the following control variables (at the store-week level): (1) (log) operating duration in minutes, (2) (log) total labor minutes, (3) (log) number of shifts, (4) (log) number of operating days. All specifications include store and year-week fixed effects. Standard errors that are double clustered at the store and year-week levels are presented in parentheses. *, **, and *** imply that coefficients are significant at the 10, 5, and 1%, respectively.

TABLE A6. Effects on Shift Advance Notice (Matched Store Sample)

	log(Notice) (1)	Notice (2)	log(Notice) (3)	Notice (4)	log(Notice) (5)	Notice (6)
Chicago × Post-CFW	0.222***	2.93***				
	(0.032)	(0.341)				
Los Angeles × Post-LFW			0.050*	0.791***		
			(0.025)	(0.238)		
Philadelphia × Post-PFW					0.042***	0.715***
					(0.006)	(0.086)
Adjusted R^2	0.483	0.554	0.389	0.465	0.445	0.466
Mean of dep. var.	2.26	11.0	2.40	12.7	3.01	21.9
Company-Store-Employee FEs	✓	✓	✓	✓	✓	✓
Company-Year-Week FEs	✓	✓	✓	✓	✓	✓

Notes: The estimates above correspond to fixed effects regression where the dependent variables (listed above the column numbers) are regressed on a binary indicator variable that equals 1 for employees working at Chicago stores after the calendar week that includes July 1, 2020, and zero otherwise. All specifications include employee, store and year-week fixed effects. Standard errors that are double clustered at the store and year-week levels are presented in parentheses. *, **, and *** imply that coefficients are significant at the 10, 5, and 1%, respectively.

Table A7. Store-Level Effects on Labor Utilization

Panel A: Effects in Chicago

	log(Mins.) (1)	log(Shifts) (2)	log(Mins./Shifts) (3)	log(PT Mins.) (4)
Chicago × Post-CFW	0.086	0.060	0.025*	0.254
	(0.054)	(0.056)	(0.015)	(0.184)
Control Variables	Yes	Yes	Yes	Yes
Adjusted R^2	0.813	0.805	0.731	0.578
Store-Company FEs	✓	✓	✓	✓
Year-Week-Company FEs	✓	✓	✓	✓

Panel B: Effects in Los Angeles

	log(Mins.) (1)	log(Shifts) (2)	log(Mins./Shifts) (3)	log(PT Mins.) (4)
Los Angeles × Post-LFW	-0.032	-0.043	0.011	0.357
	(0.034)	(0.035)	(0.007)	(0.469)
Control Variables	Yes	Yes	Yes	Yes
Adjusted R^2	0.751	0.757	0.584	0.806
Store-Company FEs	✓	✓	✓	✓
Year-Week-Company FEs	✓	✓	✓	✓

Panel C: Effects in Philadelphia

	log(Mins.) (1)	log(Shifts) (2)	log(Mins./Shifts) (3)	log(PT Mins.) (4)
Philadelphia × Post-PFW	-0.140***	-0.143***	0.003	-0.211***
	(0.022)	(0.021)	(0.005)	(0.063)
Control Variables	Yes	Yes	Yes	Yes
Adjusted R^2	0.915	0.920	0.756	0.877
Store-Company FEs	✓	✓	✓	✓
Year-Week-Company FEs	✓	✓	✓	✓

Notes: The estimates above correspond to fixed effects regression where the dependent variables (listed above the column numbers) are regressed on a binary indicator variable that equals 1 for stores operating in an FWL jurisdiction after its effective date, and 0 otherwise (Chicago in Panel A, Los Angeles in Panel B, and Philadelphia in Panel C). All specifications include the following control variables (at the store-week level): (1) (log) operating duration in minutes, and (2) (log) number of operating days. Standard errors that are double clustered at the store and company-year-week levels are presented in parentheses. *, **, and *** imply that coefficients are significant at the 10, 5, and 1%, respectively.

A.3. Effects using Matched Sample (Matches Constructed at the Worker-Level)

TABLE A8. Effects on Shift Advance Notice (Matched Worker Sample)

	log(Notice) (1)	Notice (2)	log(Notice) (3)	Notice (4)	log(Notice) (5)	Notice (6)
Chicago × Post-CFW	0.317***	3.47***				
	(0.025)	(0.264)				
Los Angeles × Post-LFW			0.026	0.701***		
			(0.022)	(0.239)		
Philadelphia × Post-PFW					0.027***	0.535***
					(0.005)	(0.072)
Adjusted R^2	0.469	0.520	0.485	0.536	0.492	0.485
Mean of dep. var.	2.32	11.5	2.53	14.3	3.03	22.1
Company-Store-Employee FEs	✓	✓	✓	✓	✓	✓
Company-Year-Week FEs	✓	✓	✓	✓	✓	✓

Notes: The estimates above correspond to fixed effects regression where the dependent variables (listed above the column numbers) are regressed on a binary indicator variable that equals 1 for employees working at Chicago stores after the calendar week that includes July 1, 2020, and zero otherwise. All specifications include employee, store and year-week fixed effects. Standard errors that are double clustered at the store and year-week levels are presented in parentheses. *, **, and *** imply that coefficients are significant at the 10, 5, and 1%, respectively.

TABLE A9. Effects on Scheduled Labor (Matched Worker Sample)

	Chicago		Los Angeles		Philadelphia	
	log(Mins.) (1)	log(Shifts) (2)	log(Mins.) (3)	log(Shifts) (4)	log(Mins.) (5)	log(Shifts) (6)
Chicago × Post-CFW	0.013	-0.0007				
	(0.021)	(0.019)				
Los Angeles × Post-LFW			0.021	0.021		
			(0.026)	(0.026)		
Philadelphia × Post-PFW					0.012	0.010
					(0.009)	(0.008)
Control Variables	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R^2	0.660	0.514	0.686	0.583	0.543	0.451
Store-Company-Worker FEs	✓	✓	✓	✓	✓	✓
Year-Week-Company FEs	✓	✓	✓	✓	✓	✓

Notes: The estimates above correspond to fixed effects regression where the dependent variables (listed above the column numbers) are regressed on a binary indicator variable that equals 1 for employees working at Chicago stores after the calendar week that includes July 1, 2020, and zero otherwise. All specifications include employee, store and year-week fixed effects. Standard errors that are double clustered at the store and year-week levels are presented in parentheses. *, **, and *** imply that coefficients are significant at the 10, 5, and 1%, respectively.

Table A10. Effects on Schedule Stability (Matched Worker Sample)

Panel A: Effects in Chicago

	$\log(1+N^*)$ (1)	$\log(1+\tilde{N})$ (2)	$\log(1+\hat{N})$ (3)	log(No. 1 + Clopen) (4)
Chicago × Post-CFW	-0.011	0.013	-0.003	-0.009
	(0.021)	(0.023)	(0.026)	(0.006)
Control Variables	Yes	Yes	Yes	Yes
Adjusted R^2	0.274	0.401	0.537	0.306
Store-Company-Worker FEs	✓	✓	✓	✓
Year-Week-Company FEs	✓	✓	✓	✓

Panel B: Effects in Los Angeles

	$\log(1+N^*)$ (1)	$\log(1+\tilde{N})$ (2)	$\log(1+\hat{N})$ (3)	log(No. 1 + Clopen) (4)
Los Angeles × Post-LFW	0.031	0.002	-0.030	-0.032**
	(0.031)	(0.035)	(0.031)	(0.013)
Control Variables	Yes	Yes	Yes	Yes
Adjusted R^2	0.327	0.363	0.556	0.367
Store-Company-Worker FEs	✓	✓	✓	✓
Year-Week-Company FEs	✓	✓	✓	✓

Panel C: Effects in Philadelphia

	$\log(1+N^*)$ (1)	$\log(1+\tilde{N})$ (2)	$\log(1+\hat{N})$ (3)	log(No. 1 + Clopen) (4)
Philadelphia × Post-PFW	0.013	-0.020*	0.000	0.006
	(0.012)	(0.012)	(0.011)	(0.004)
Control Variables	Yes	Yes	Yes	Yes
Adjusted R^2	0.307	0.380	0.429	0.182
Store-Company-Worker FEs	✓	✓	✓	✓
Year-Week-Company FEs	✓	✓	✓	✓

Notes: The estimates above correspond to fixed effects regression where the dependent variables (listed above the column numbers) are regressed on a binary indicator variable that equals 1 for employees working at Chicago stores after the calendar week that includes July 1, 2020, and zero otherwise. The dependent variables are: (1) N^* is the number of shifts that an employee works in a given week that falls on a week of the day (e.g., Monday) that they did not work the previous week, (2) $\tilde{N}$ is the number of shifts that an employee works in a given week that falls on a week of the day that they did work the previous week, but has a start time that differs by more than 60 minutes (in absolute value), and (3) $\hat{N}$ is the number of back-to-back shifts that an employee works in a given week that has a start time that differs by more than 60 minutes (in absolute value). The unit of observation is at the worker-week level. All specifications include employee, store and year-week fixed effects. Standard errors that are double clustered at the store and year-week levels are presented in parentheses. *, **, and *** imply that coefficients are significant at the 10, 5, and 1%, respectively.