RESEARCH REPORT
STUDY PAPER 258
OCTOBER 2025

Labor-Market Effects of Introducing the 8-Hour Workday

Marius F. Gunnesmo and Casper W. Hansen

THE ROCKWOOL FOUNDATION
RESEARCH

STUDY PAPER 258 OCTOBER 2025

Labor-Market Effects of Introducing the 8-Hour Workday

Published by:

Address:
The ROCKWOOL Foundation Research Unit
Ny Kongensgade 6
1472 Copenhagen, Denmark

Telephone +45 33 34 48 00
E-mail: kontakt@rff.dk
en.rockwoolfonden.dk/research/

October 2025

Labor-Market Effects of Introducing the 8-Hour Workday*

Marius F. Gunnesmo Casper W. Hansen†

Abstract

In 1919, the Danish craft and industrial sector permanently adopted the 8-hour workday, representing the largest reduction in working hours in the country's history. We collected quarterly data on hourly wages and employment from 1914 to 1931 across occupation groups, covering Copenhagen and the aggregate of the provinces in Denmark. By exploiting variation in percent work-time reductions across occupation groups and regions, we examine the income and employment effects of the reform. Our findings reveal only a compensating rise in hourly wages in Copenhagen, though this increase was insufficient to offset the decline in weekly earnings due to fewer working hours. Furthermore, we observe that the reduction in working hours was mitigated by new hires, particularly of unskilled workers. Overall, our results suggest that reductions in work hours were not (in any region) fully compensated by gains in hourly wages but tend to support the "work-sharing" hypothesis.

Keywords: work-time reduction, wage, earnings, employment, automatization.
JEL Codes: E24, J22, J31, N34

*__Acknowledgments__: We thank Magnus B. Rasmussen and Søren Kolstrup for comments and suggestions, as well as seminar participants at various locations. We also thank Jens B. Bjørst, Samuel Rana, and Victor Burrild for helpful research assistance. We gratefully acknowledge financial support from The ROCKWOOL Foundation on the research project titled "*Welfare and Economic Development: Lessons from the Origins of the Welfare State*", which this paper is part of.

†Gunnesmo: University of Copenhagen, mfg@econ.ku.dk; Hansen: University of Copenhagen and CEPR, casper.worm.hansen@econ.ku.dk;

1 Introduction

The impact of working hours on employment and income is a central topic not only in the economic literature (e.g., Hunt 1999; Crépon and Kramarz 2002; Fishback et al. 2024) but also in broader societal discussions. Recently, policymakers and the media have increasingly focused on proposals such as the 6-hour workday and the 4-day workweek. For example, the organization "4 Day Week Global" has been a prominent advocate, helping companies worldwide implement what it calls 4-day workweek trials. Notably, one of their key findings is that company revenues remained stable compared to the traditional 5-day workweek, suggesting that reduced working hours may be self-sustaining through increased worker productivity.[1] The demand for more leisure, shorter workweeks, and reforms to working hours is not a new phenomenon, but rather seems to have evolved alongside economic development, with significant changes occurring in the early 20th century in now-developed countries (e.g., Hazan 2009; Knutsen and Rasmussen 2023; Rasmussen 2024).

In this paper, we investigate the impact of working hours on labor-market outcomes, focusing on the introduction of the 8-hour workday. In the Danish craft and industrial sector, the implementation occurred through two central agreements in 1919. In January, the central organizations of workers and employers reached an agreement to limit the workday to a maximum of nine hours and a minimum of eight hours, depending on the occupation, while standardizing Saturday working hours to eight across all occupations. This first central agreement stated that workers were to receive compensation for the reduction in work hours. But wages were generally determined through negotiations at the level of the individual union, and not all unions had local negotiations before the next work-time reduction (Nielsen 1992). Four months later, in May 1919, the transition to the full 8-hour workday was implemented without any centrally agreed compensation. Together, the work-time reductions in 1919 (referred to as the 1919-WTR reform) shortened the workweek by up to 12 hours, marking the largest reduction in working hours in the country's history.[2]

We collected and digitized quarterly data on average hourly wages and employment from 1914 to 1931, disaggregated by occupation and region, distinguishing between the capital (Copenhagen) and the rest of the country (the Provinces), for the craft and industrial sectors.[3] Additionally, we gathered data on pre- and post-reform working hours, categorized

[1] See their arguments here: 4 Day Week Global

[2] Legislation was also implemented in February 1919 with the "Introduction of the 8-Hour Workday in Factories with 24-Hours Production." Although it included many exemptions, it may have reduced working hours for certain occupations. This suggests that our baseline estimates capture the transition to the 8-hour workday in 1919, driven by both the two negotiated labor-market reforms and the 1919 Factory Act.

[3] The main dataset covers industrial workers employed in firms affiliated with the national em-

by the same occupation groups and regions. Our research design compares key labor-market outcomes across occupation groups that experienced varying degrees of WTRs driven by variation in pre-reform working hours and the transition to the 8-hour workday. We employ a difference-in-differences (DiD) analysis within an event-study framework to assess the impacts on hourly wages, weekly earnings, and employment.[4]

In the baseline analysis, we focus on the period from 1914 to 1925 due to data limitations, but we also examine effects extending to 1931. The baseline empirical framework yields several key findings. First, in both regions, we observe that weekly earnings declined more in occupations with higher WTRs, allowing us to rule out the possibility that the reform was self-financing in terms of average earned income. However, for Copenhagen, we estimate an hourly wage elasticity of 0.4, suggesting that a one percent WTR led to only a 0.6 percent decrease in weekly earnings, providing evidence of a compensating effect through higher hourly wages in this labor market. In contrast, we do not observe a similar compensating effect in the combined labor market of the Provinces.

Second, we find evidence of work sharing, as employment increased more in occupation groups more affected by the WTR reform. Our event-study evidence shows this trend in both regions, though the DiD elasticities are estimated with less precision, leaving uncertainty as to whether more or less than full replacement occurred. Interestingly, in Copenhagen, the employment increase appears to be driven by firms hiring more unskilled and female workers. Third, beyond these classic labor market outcomes, we also digitized data on labor conflicts (strikes and lockouts) and measures of automation (firm-type-level horsepower usage). We find no evidence that the reform-intensity influenced labor conflicts, and firm types with a higher proportion of workers affected by the reform did not increase horsepower usage, implying that horsepower per-worker actually decreased, despite the fact that this was contemporarily argued to be an important margin of adjustment (Milhaud 1925).

Our work is related to research studying how contemporary WTR-reforms influenced labor-market outcomes in Germany and France. First, Hunt (1999) examines a weekly WTR of four hours, also negotiated by unions and employers (in the manufacturing sector) but in 1980s Germany. However, unlike our findings, she provides evidence of full wage compensation without work-sharing, but with employment losses. Second, Crépon and Kramarz

ployers' association (DA). Consequently, the provincial figures primarily reflect wages and employment in provincial towns, since rural industrial and craft firms were typically not members of DA.

[4]We cannot separate the labor market effects of the two agreements, as we only have pre-reform working hours measured in 1914. Therefore, our estimates should be interpreted as the combined effect of the transition to the 8-hour workday. We do not see this as a major issue, as the initial reduction in working time was set to be implemented over the subsequent collective agreement period, and the full transition to the 8-hour workday was agreed upon only a few months later.

(2002) document that a one-hour WTR per week in 1980s France resulted in employment losses. Third, Chemin and Wasmer (2009) investigate what they describe as one of the most significant regulatory shocks to the French labor market—the reduction of the workweek from 39 to 35 hours. However, they do not arrive at a definitive conclusion regarding the reform's impact on employment and income.[5] Compared to these studies of contemporary reforms, our historical evidence is based on a much larger WTR reform (12 hours vs. 4 and 1 hours), for a different time period, and we reach different conclusions regarding the effects of a shorter workweek on the labor market.

Our study also relates to Bengtsson and Molinder (2017) and Fishback et al. (2024) in terms of time period and research question. The first-mentioned paper examines the introduction of the 8-hour workday during the same period in Sweden. However, the nature of the reform differed, as Swedish workers were fully compensated by law for the reduction in all working hours, resulting in a significant increase in real hourly wages. The authors find evidence that the reform decreased employment in the traded manufacturing sector but increased employment in the non-traded manufacturing sector. In contrast, our study examines income and employment effects within a single sector, leveraging variation in working time regulations (WTRs) across more than 60 occupational groups, a reform that did not mandate full compensation for all reduced hours, and where wages ultimately were negotiated at the local union level. In addition, their findings are primarily based on time-series analysis across four sectors. The second-mentioned paper reaches conclusions similar to ours but focuses on a temporary 1933-WTR reform in the U.S., limiting their analysis to a shorter time horizon. We also contribute to the broader literature on labor scarcity, automation, and conflict (e.g., Schmick 2018; Molinder et al. 2022, 2021; Acemoglu 2010; Caprettini and Voth 2020; Voth et al. 2023).

There are also contemporary studies examining the labor market impacts of the 8-hour workday and work-time reductions more broadly, to which our work is related. For example, Milhaud (1925) presented case studies from various countries, suggesting that the 8-hour workday should be accompanied by changes in technology and production methods to ensure that increases in labor productivity offset the reduction in working hours. Closer to our context, a commission appointed by the Danish government in 1949 to evaluate the implications of further work-time reductions argued, based on time-series evidence for selected occupations over our study period, that the introduction of the 8-hour workday some

[5]Through cross-country comparisons, Du et al. (2013) argue that the introduction of the 35-hour workweek in France lowered unemployment rates and increased GDP per capita. In contrast, Estevão and Sá (2008), using firm-level data from France, show that while the shorter workweek increased labor turnover, it had no effect on overall employment.

30 years earlier likely led to large labor productivity increases (Arbejdsmarkedskommission 1952). A report from the U.S. Department of Labor from 1947 suggests that the 8-hour day and 40-hour week is optimal compared to longer work time, when taking into account costs and benefits from changes in labor productivity, absenteeism, and accidents (United States Department of Labor 1947). Common from such contemporary studies is that they build on case studies or descriptive evidence.

Finally, we also contribute to studies on productivity and working hours. Some studies document that longer working hours are associated with diminishing marginal productivity of labor. For example, Atack et al. (2003) show this for late nineteenth-century U.S. manufacturing, while Pencavel (2015, 2016) and Collewet and Sauermann (2017) provide more recent evidence from modern settings. Our findings for Copenhagen are in line with this evidence, suggesting that reductions in hours can raise productivity at the margin, although such effects appear limited to the capital and not the Provinces. Relatedly, Holt (2024) emphasizes that employer monopsony power during the Great Depression muted wage responses to productivity changes, highlighting that institutional and market conditions shape whether productivity gains from shorter hours are reflected in wages. In this way, our results add historical evidence to the ongoing debate on the productivity effects of working-time reductions.

We interpret our findings within the framework of the union–monopoly model proposed by Calmfors (1985), in which unions set wages while taking working hours as given. Our empirical setting aligns well with this framework. The 1919-WTR reform resulted from central negotiations between the national labor organization (DSF) and the main employers' association (DA). As a general principle, however, wages were not to be determined at the central level, but rather through negotiations at the level of the individual union (Nielsen 1992). Applying simple functional forms, the model demonstrates that if workers value consumption more than they dislike working, unions will raise the wage rate to compensate for the reduction in working hours, but not sufficiently to prevent a decline in weekly earnings. It further shows that if hours and workers are sufficiently substitutable, employment will increase. This mechanism may also account for our finding of larger employment effects among unskilled workers (in Copenhagen), as firms can more readily substitute workers for hours in occupations of this type.

The only model explanation for observing compensating wage effects solely in Copenhagen is differences in preferences over consumption and work. If workers in the Provinces value consumption as much as they dislike working, the model predicts no change in wages but still a positive effect on employment. While a preference-based explanation is difficult to falsify, an alternative—though not captured directly in the simple model—is that unions in

the capital had greater bargaining power, as reflected in higher unionization rates, which we document to be the case. A further measurement-based explanation is that the provincial aggregate combines many heterogeneous local labor markets, such that averaging may attenuate or even obscure positive occupation-level wage compensation, especially if the WTR measure is noisier in smaller areas.

2 Background

2.1 The historical Danish labor market

The September Settlement of 1899 (*Septemberforliget* in Danish) laid the foundation for the employer-employee bargaining model that also shaped our WTR reform. Nowadays referred to as the "constitution" of the Danish labor market, the settlement introduced several key principles. First, it established that crucial aspects of the working relationship, such as wages, working hours, overtime, and holidays, would be negotiated between employers and employees. Second, it safeguarded employers' rights to hire and fire, while securing workers' rights to organize and strike. Third, it required employers to recognize unions as legitimate representatives of workers for negotiation purposes. Finally, it stipulated that during the period of an agreed-upon contract, strikes and lockouts were prohibited. For a more detailed discussion of the September Settlement, see, for example, Madsen (1999).

Labor market agreements were determined through negotiations between employers and unions, with unions typically organized at the occupational level. Most unions in Denmark were affiliated with the national labor organization (*De Samvirkende Fagforbund*, DSF, in Danish), while many firms were members of the employer association (*Dansk Arbejdsgiverforening*, DA, in Danish). The degree of centralization in collective bargaining varied depending on the issue, but unions generally maintained that wage negotiations should be conducted at the local level, as they believed decentralized (occupation specific) bargaining would yield more favorable outcomes for workers (Nielsen 1992).

A individual union could negotiate with a single employer, a group of employers, or an employers' association (below the level of DA), and the character of local agreements therefore varied across regions and occupations. In 1915, we have detailed data on the nature of such agreements. There were 1,227 active agreements in the Danish craft and industrial sectors under consideration, covering 120,938 workers. Of these, 52,382 workers were covered by union-specific national agreements, 4,680 by regional agreements, 37,924 by union-specific local area agreements (e.g., at the town level), and 24,512 by agreements negotiated directly between a local union and an individual firm (Statistiske-Meddelelser

1916). According to the same sources, the duration of these agreements in 1915 ranged from less than one year to as long as five years. However, rising inflation in the years that followed led to a marked reduction in contract length, and by the late 1910s most agreements were renegotiated annually or biennially (Statistiske-Meddelelser 1927).

The Danish industrial labor market became increasingly unionized over the study period. In the Capital, unionization rates rose from about 45 percent in 1914 to nearly 80 percent in 1924, while in the Provinces the corresponding figures were 30 and 50 percent (Appendix Figure A.1). The figure also indicates a marked increase in unionization beginning in 1917, coinciding with a temporary policy reform discussed in Section 2.3. We also constructed occupation-specific unionization rates by area for 1916 by matching census-based counts of total workers to the corresponding union membership. The results show a clear pattern of systematically higher unionization rates in Copenhagen. Out of the 37 occupations where this was feasible, only six (e.g., watchmakers, stone industry workers, and turners) show higher rates in the Provinces (Appendix Table A.1).

2.2 The 1919-WTR reform

The length of a Danish workday has been declining since the 19th century. Figure 1 shows that the average workday (in the craft and industrial sector) declined from 11.4 hours in 1872 to around 9.5 hours prior to the 1919-WTR reform. Shortening the workweek and increasing leisure has been one of the key issues of the international labor movement, and the goal of an 8-hour workday was already manifested in the late 19th century.

The negotiations for the 8-hour workday serve as an example of more centralized bargaining between DSF and DA. The 1919-WTR reform followed a prolonged period of pressure from unions and local battles for working time reductions and wage increases. Danish bricklayers reportedly played a pivotal role in advocating for the 8-hour workday. After securing wage increases in 1918, only to see them quickly eroded by rising inflation, workers shifted their focus to reducing working time as an alternative. In April 1918, bricklayers in Copenhagen began leaving work early on Saturdays as a way to signal their demand for an "English workweek". Under this system, workers would begin their weekend at 12 PM on Saturdays, whereas the pre-WTR Danish workweek extended until 5 PM on Saturdays. The movement gradually expanded, drawing in more occupations and spreading across a larger geographic area, primarily within the construction sector. By the summer of 1918, however, the bricklayers' union that initiated the protests filed for bankruptcy after being heavily fined for organizing walkouts.

Despite this setback, a shorter workday became a central demand in the subsequent

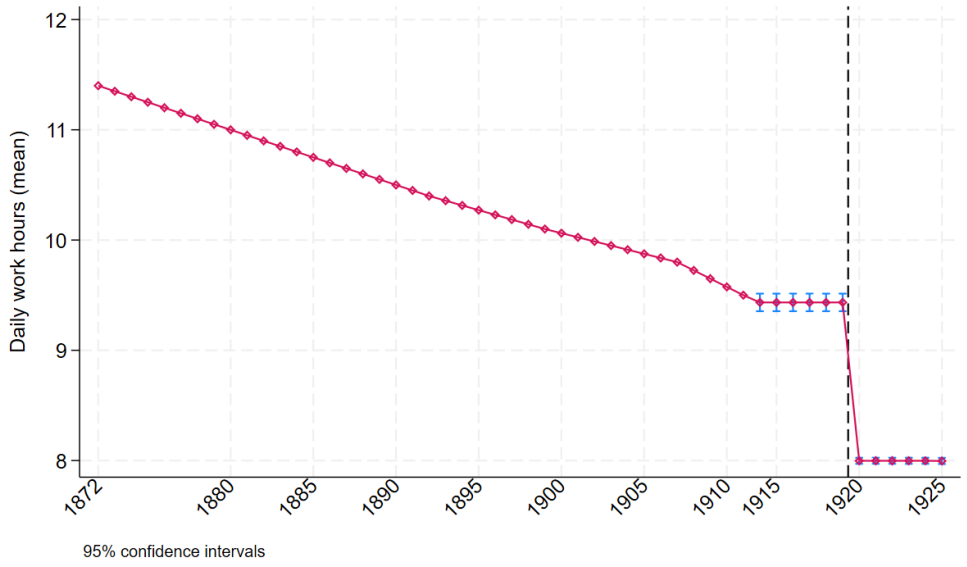
centralized bargaining round, and the transition to an 8-hour workday was implemented in two stages. On January 13, 1919, a collective agreement between DSF and DA established a reduction in working hours to a maximum of nine and a minimum of eight hours per day, with full wage compensation. Specifically, 10-hour workdays were reduced to nine hours, 9.5-hour workdays to 8.5 hours, and 8.5-hour workdays to eight hours. In addition, a standardized 8-hour workday was introduced for all occupations on Saturdays (DSF 1920).

These changes were to be phased in over the subsequent agreement period, and DSF insisted that local wage negotiations should not only incorporate the centrally agreed compensation for reduced hours but also proceed as regular bargaining rounds beyond this compensation (Nielsen 1992). However, dissatisfaction soon arose among workers who demanded the immediate introduction of the 8-hour workday for all. In April 1919, DSF therefore resolved to reopen negotiations, implying that the January agreement had only been implemented in certain local contexts (DSF 1920). The result was a new settlement in May 1919 between DSF and DA, which introduced the 8-hour workday across the entire craft and industrial sector, but without any additional centrally-agreed wage compensation. Employers were required to comply with the new working hours by January 1920, although certain exceptions were granted in specific occupations, such as drivers and coachmen (Statistiske-Meddelelser 1927).

Figure 1 shows that, on average, daily working hours across occupations decreased by approximately 1.5 hours per day, making this reform the most significant non-experimental work-time reduction (WTR) studied in the literature, to our knowledge. According to DSF (1920), only about 5% of unionized workers in the craft and industrial sectors had an 8-hour workday or less in 1918, but this figure had risen to 95% by January 1, 1920 due to the 1919-WTR reform. Workers in the agricultural sector were not included in the reform, and they did not experience any reductions in work hours until 30 years later.

Labor market conflict remained high in the years surrounding the adoption of the 8-hour workday, with lockouts becoming even more frequent in its aftermath. When the collective bargaining agreement that included the 8-hour workday expired, employers sought to reduce wages as Denmark entered a postwar recession, following a period of wartime profiteering during World War I (Statistiske-Meddelelser 1926b). Employers' efforts to cut wages and extend working hours—particularly in 1921, 1922, and 1925—resulted in some of the highest recorded levels of lost workdays due to strikes and lockouts in Danish history (Statistiske-Meddelelser 1926b). In 1921, wage reductions were employers' main focus, while in 1922, they pushed for both longer workdays and lower wages. They demanded a 15 percent wage cut and the termination of the 1919 working-time agreement. After prolonged conflicts across several industries, the 8-hour workday was preserved, but wages were ultimately reduced

Figure 1: The development of daily working hours



Notes: *Average hours worked in Denmark from 1872 to 1925. The dashed line represents the collective bargaining reform. We observe country-level average working hours between 1872 and 1911, but with gaps. We calculate the 1914-1925 values from occupation-level data. Hours are interpolated where data is missing: several intervals between 1872 and 1911, and between 1914 and 1919.*

nominally by 12 percent in the affected sectors. This 8-hour, 6-day work schedule remained in place for the next 40 years.

2.3 The Factory Act and other changes

In this subsection, we discuss other significant labor market changes during this period. Perhaps the most relevant for our study was the Factory Act of 1919, titled the "Introduction of the 8-Hour Workday in Factories with 24-Hours Production," which was passed by Parliament February 12, 1919. This law generally limited working hours to eight per day in factories operating around the clock. However, when shifts change during the day, workdays of up to 16 hours are permitted, provided that no individual exceeds a total of 160 working hours over any three consecutive weeks. The law did not require wage compensation if daily working hours were reduced for workers. As mentioned in the introduction, we cannot isolate the effect of this act from the two employer-employee negotiated WTR agreements. Consequently, our estimates reflect the transition to the 8-hour workday driven by these three fundamental changes in 1919.[6] It should also be noted that the national government, formed by the Social Liberals and the Social Democrats, indirectly pressured DSF and DA by signaling a willingness to impose working-time regulations through legislation. Since it was widely understood that worktime could be regulated by law if no agreement was reached, both DSF and DA had incentives to settle the issue through collective bargaining (Kolstrup 1972).

Another significant labor market reform, the "Extraordinary Assistance for the Unemployed, etc.," was enacted in 1917. Although intended as a temporary measure, it granted workers the right to unemployment benefits immediately upon joining an unemployment fund, abolishing the previous one-year qualifying period. The state subsidized the unemployment insurance funds to cover the additional costs of this change (e.g., Kolstrup 1998). While these funds were formally independent of unions, they were typically organized along the same occupational lines, which contributed to a rapid increase in unionization rates from 1917 onward (Appendix Figure A.1). This surge likely strengthened union bargaining power and may have led to higher wage demands, a pattern that appears consistent with aggregate trends (see Section 6.1). Although the policy preceded the 1919 WTR reform by one to two years, our robustness analysis controls for its effects.

[6]State employees (e.g., railway workers) changed to the 8-hour workday in March 1919, but this group workers are not part of our labor-market data.

2.4 International labor movement

Although this paper focuses on the Danish labor market, labor unions worldwide shared common political goals, particularly shorter workdays and higher wages. From the 1890s onward, the demand for an 8-hour workday became a central feature of International Workers' Day (May 1). Following the October Revolution in Russia in 1917, workers in many European countries intensified their efforts to achieve this goal (Cross 1985). Several European countries, including Sweden, France, and Germany, implemented an 8-hour workday or significant worktime reductions between 1918 and 1920, around the time of the Danish work-time reform (e.g., Milhaud 1925). This suggests that our study has strong historical external validity. In addition, it has been suggested that these international developments increased Danish employers' willingness to negotiate with unions over reductions in working hours (Kolstrup 1972).

3 Theoretical predictions

This section outlines theoretical predictions from a simple one-period union–monopoly model in which the union sets wages while taking working hours as given (Calmfors 1985). The aim is to fix ideas about the potential effects of the 1919-WTR reform on labor market outcomes. Our empirical setting fits naturally into this type of model: each occupation is represented by a corresponding union that sets wages within its sector, while the introduction of the 8-hour workday was the result of central negotiations between DSF and DA. As a first approximation, this reform can therefore be regarded as exogenous to the wage-setting decisions of individual unions.

Using $V_i = c_i^{\theta} h_i^{-\pi}$, where $\theta, \pi > 0$, and $\bar{V}_i = b_i$, where $b_i > 0$, to represent the utility of employed and unemployed workers in occupation i, where c_i is consumption and h_i hours worked, together with an isoelastic labor demand function given by $N_i = A_i w_i^{-\varepsilon} h_i^{-\gamma}$, where w_i is the hourly wage and $A_i, \varepsilon, \gamma > 0$, yields the following expression for the preferred wage rate of union i:[7]

$$\ln w_i = \frac{1}{\theta} \ln \varepsilon b_i - \frac{1}{\theta} \ln (\varepsilon - \theta) - \frac{\theta - \pi}{\theta} \ln h_i. \tag{1}$$

Therefore, when the preference weight on consumption exceeds that on the disutility of hours ($\pi < \theta$), the union raises the hourly wage to partly compensate for the income loss from reduced hours.

[7] So b_i can be interpreted as occupation specific unemployment benefits. We go through the model in more details in Appendix Section A.1.

The solution for income/consumption ($c_i = w_i h_i$), what we later refer to as weekly earnings, is given by:

$$\ln w_i h_i = \frac{1}{\theta} \ln \varepsilon b_i - \frac{1}{\theta} \ln (\varepsilon - \theta) - \left(\frac{\pi}{\theta}\right) \ln h_i, \tag{2}$$

which shows that the possible compensating effect from the increase in the wage rate is never strong enough to counteract a fall in earnings.[8]

Finally, the solution for employment is:

$$\ln N_i = \ln A_i - \frac{\varepsilon}{\theta} \ln \varepsilon b_i + \frac{\varepsilon}{\theta} \ln (\varepsilon - \theta) + \left(\varepsilon \frac{\theta - \pi}{\theta} - \gamma\right) \ln h_i. \tag{3}$$

Two opposing channels operate when hours h_i are reduced. First, if $\theta > \pi$, the union increases w, which lowers labor demand in proportion to the wage elasticity ε. Second, firms can substitute workers for hours; holding w_i constant, employment varies with elasticity $-\gamma$ in h_i, so a larger γ increases the scope for work sharing. In other words, the easier it is for firms to replace hours with additional workers, the more likely employment will rise as hours fall.

Eqs. (1)–(3) also inform the functional form of our baseline empirical specification. Moreover, under the assumption that the parameters θ and π are constant across occupation types, we can estimate the effect of hours on wages and income by including occupation fixed effects, which absorb the terms before hours in Eqs. (1) and (2). Estimating the effect on employment additionally requires that γ and ε do not vary across occupations, which we consider to be a stronger assumption. For example, it may be easier to substitute hours with additional workers in low-skilled occupations involving modular tasks and minimal training requirements, where the fixed hiring costs per worker are lower. Our heterogeneity analysis is designed to take these possible issues into account.

While we observe wages, weekly earnings, and employment by occupation, we do not observe firm output. The model nevertheless lets us infer production and productivity from these variables. In Appendix Section A.1, we show that with fixed capital and a production mapping that delivers isoelastic labor demand, total output is $Q_i = \kappa A \left[\frac{\varepsilon - \theta}{\varepsilon b_i}\right]^{\varepsilon/\theta} h_i^{1 + \frac{\varepsilon(\theta - \pi)}{\theta} - \gamma}$. Hence, if $\theta > \pi$ and hours–worker substitutability is at most unit elastic ($\gamma \leq 1$), a reduction in h_i lowers Q_i. Moreover, hourly productivity satisfies $\frac{Q_i}{N_i h_i} = \frac{\varepsilon}{\varepsilon - 1} w_i$, so it moves proportionally with the wage and rises when $\theta > \pi$. By contrast, output per worker is $\frac{Q_i}{N_i} = \frac{\varepsilon}{\varepsilon - 1} w_i h_i$, i.e., proportional to weekly earnings $w_i h_i$, and thus unambiguously declines when hours fall

[8] Calmfors (1985) demonstrates that this holds under general assumptions about the shape of utility function and labor demand function. So this theoretical prediction is fairly robust.

(since $w_i h_i$ falls when $\pi > 0$ as argued above).

4 Data and sample

This section describes the datasets collected for our main empirical analyses. Our primary source is quarterly data on hourly wages and employment for the period 1914–1931, which we digitized from Statistiske-Meddelelser (1927) and Statistiske-Meddelelser (1933). The data are reported at the level of what we term "occupational groups," and are disaggregated into two regions: Copenhagen (the capital) and the rest of the country, which we refer to as the "Provinces." The dataset covers industrial and craft workers employed in firms affiliated with the national employers' association (DA). As a result, the provincial figures primarily capture wages and employment numbers in provincial towns, since rural industrial and craft firms were typically not members of DA (Statistiske-Meddelelser 1927). For some occupation groups, where relevant, the data is further broken down by skill level and sex. The data is quarterly, with the exception of 1914, where it is only available annually. In 1915 and 1916, the first years with quarterly data, there are many missing observations, so our baseline panel begins in 1914, covering almost all occupations, with consistent quarterly data starting in 1917. To be included in our sample, we require that an occupation group is observed at least two times before the WTR reform in 1919.

In our baseline analysis, we focus on the period 1914–1925 because the data from 1926 to 1931 is drawn from a subsequent publication in the same series, which includes some changes in occupation names. To ensure consistency, we need to harmonize occupation group names across the two datasets. Although this is generally straightforward, certain groups, such as unskilled workers, are categorized with varying levels of detail, leading to the loss of some unskilled occupation groups when analyzing the full 1914–1931 period. Therefore, we treat the full-period analysis as a supplementary addition to the baseline analysis.

The data is originally sourced from payrolls submitted by employers (affiliated with DA) to *Statistics Denmark*, and thus occupation groups were categorized from the employer's perspective, specifically by industry and firm type. For example, in the food processing industry, workers were employed in various types of factories, and within these factories, there could be distinct groups such as skilled, unskilled, and female workers. As a result, one occupation group, in our sample, is "female workers in sugar factories", while another is "unskilled male workers in sugar factories". In bakeries, however, the only occupation group represented is the (male) "baker".[9] The number of workers included in these payrolls, as

[9] All occupational groups are described in Appendix Tables A.6-A.9, where we have also added HISCLASS codes from van Leeuwen and Maas (2011).

reported by employers, grew from approximately 60,000 in 1914 to 110,000 by 1920, which at that point represented about half of the industrial workforce, according to Statistiske-Meddelelser (1927). From 1920 to 1924, the number of workers, covered by the reported payrolls, "fluctuated" naturally around this level.

The same source provides data on the average number of daily working hours in 1914 (pre-WTR reform) and 1920 (post-WTR reform) at the same levels of aggregation as the wage and employment data. We calculate our treatment measure (WTR) as the percentage reduction in working hours based on these two observations. Nearly all occupations transitioned to the 8-hour workday in 1919, with the exception of drivers/coachmen ("Kusk"), who were exempt from the WTR reform.[10] According to Statistiske-Meddelelser (1927), there were only few changes in working hours from 1914 to 1918 due to World War I, so our treatment measure should accurately reflect the reduction in working hours resulting from the 1919 WTR-reform. However, in order to validate this, we obtained data on changes in work time in 1919 for a subset of around 40 union occupations from DSF (1920) which indeed suggests that working hours did not change much from 1914 to 1918. The average WTR in our main data (from 1914 to 1920) is 1.45 hours per day, whereas the 1919-data show an average WTR of 1.44 hours. As in Statistiske-Meddelelser (1927), we calculate weekly earnings by multiplying the number or work hours with hourly wages and the length of the working week (6 days). Appendix Tables A.2 and A.3 provide summary statistics by pre and post-reform periods and regions.[11]

The quarterly income and employment data forms the baseline of our labor market outcome dataset. However, we also digitized additional data to explore whether and how the 1919-WTR reform affected other labor market factors, specifically automation and labor-market conflicts. To measure automation, we collected data on the amount of horsepower used in production from the industrial censuses of 1914 and 1925 (Statistisk-Tabelværk 1917, 1929). This data is categorized by firm type within the industrial sector. Additionally, from (Statistisk-Tabelværk 1917), we obtained data on the number of occupations employed within different firm types, which we use as a cross-reference to assess each firm's exposure to the WTR reform. For example, if a particular firm type employed only one occupation that experienced a reduction from 10 to 8 working hours per day, this would imply that the firm type faced a 20 percent reduction in working hours. The horsepower data is available

[10]Due to lack of sufficient pre-1919-WTR reform data on earnings and employment, we do not include this occupation group (drivers) in the baseline sample. However, in our "no balance" sample we include it with the observed reduction in working hours from 10.5 (10.3 in the Provinces) to 9.3.

[11]We also obtained additional data on average working hours at the national level for the period 1872–1911 from the publication series "Beretning til De Samvirkende Fagforbunds Repræsentantskabsmøde," which was used to calculate the data points in Figure 1 up to 1914.

at the same level of geographic detail as the main dataset, covering both Copenhagen and the Provinces.

Finally, we digitized data on labor-market conflicts from Statistiske-Meddelelser (1917), Statistiske-Meddelelser (1922), and Statistiske-Meddelelser (1926b). These sources provide data on all strikes and lockouts by occupation (union) and employer, their exact date, location, and intensity (number of lost workdays and workers/employers involved). Since this data is more geographically detailed, we collapse them into our two main regions (Copenhagen and the Provinces).

5 Estimation strategy

We assess the impact of the 1919-WTR reform on labor market outcomes using an event-study framework. Our treatment variable is the percentage reduction in daily working hours in 1919. This is modeled as follows:

$$\ln Y_{it} = \sum_{t=1914}^{1925q4} \beta_t (WTR_i \times \mathbb{1}\{time = t\}) + \mu_i + \mu_t + \epsilon_{it}, \tag{4}$$

where Y_{it} is the outcome variable (hourly wages, weekly income, or employment) for occupation i at time t, which may vary by sex and skill level within certain occupations. We include fixed effects for occupation (μ_i) and time (μ_t), and split the sample by geographic region (i.e., Copenhagen and the Provinces). Standard errors are clustered at the occupational level.

The treatment variable, WTR_i, is constructed as the log change in daily working hours ($\ln h_{i,1914} - \ln h_{i,1920}$), reflecting the percent reduction due to the 1919-WTR reform.[12] Figure 2 illustrates the total distribution of this treatment variable (Panel (a)) and how it varies by sub-industry and region (Panel (b)). We see that the most affected occupational groups experienced a WTR reduction of about 25%.

Since both the treatment variable and the outcomes are in log form, as the theoretical model also suggest they should be, the coefficients of the event-study dummies (β) can be interpreted as elasticities relative to the omitted reference period (the last quarter of 1918). Since we do not observe each work-time reduction by occupational group but only the total reduction in 1919—proxied by the change in working hours from 1914 to 1920 (i.e., WTR_i)—we can estimate only the combined effect of the 1919-WTR reform on labor market outcomes. We do not regard this as being problematic since the first WTR had little to no time to impact labor market outcomes, before the full transition was agreed upon.

[12] Thus, compared to the theoretical equivalent in Eq. (1), the sign is just reversed.

We will also report Difference-in-Differences (DiD) estimates, where time fixed effects are replaced by an indicator variable set to one after the introduction of the 8-hour workday in the first quarter of 1919. Within this DiD framework, we will explore heterogeneity in key occupational characteristics; namely sex and skill level.

6 Results

6.1 Labor market trends

The primary adjustment to mitigate the negative income effect of reduced working hours is through hourly wages. Therefore, before presenting the labor market effects of the 1919-WTR reform, we first examine trends in real hourly wages (in 1914 DKK) based on the reform's magnitude. Figure 3 shows the average of this outcome with 95% confidence intervals for the Copenhagen labor market in Panels (a) and (b) and the Provincial labor market in Panels (c) and (d) over the entire baseline sample period from 1914 to 1925. The samples are split based on the number of hours worked prior to the reform.

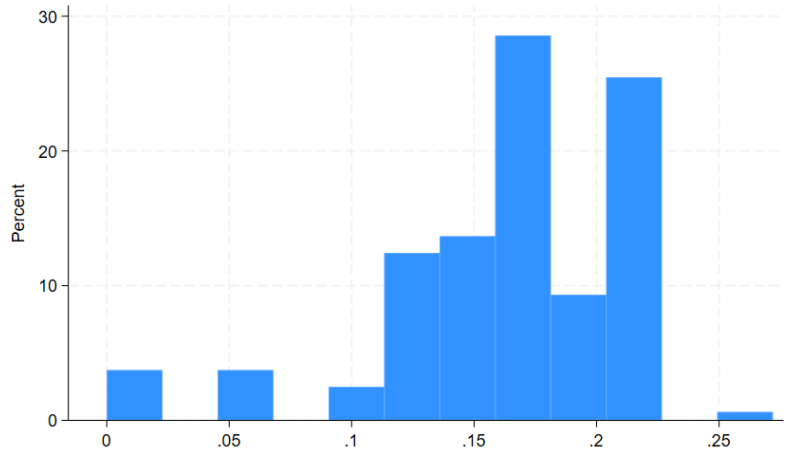
In Panel (a), we observe that occupational groups with more than 9.5 pre-reform hours experience an increase in the real hourly wage from around 0.4 to 0.6 kr. (both in 1914 DKK) following the introduction of the 8-hour workday in 1919, suggesting a potential compensating effect on weekly earnings. Indeed, similar evidence was later used by the 1949 Work-Time Commission to argue that the 8-hour workday in Denmark likely led to substantial increases in labor productivity (Arbejdsmarkedskommission 1952).

However, this interpretation overlooks other time-varying confounding factors. If occupations less affected by the 1919-WTR reform also experienced a rise in the real hourly wage after 1919 (as seen in Panel (b)), it becomes impossible to draw definitive conclusions about the causal relationship between working hours and hourly wages based solely on these aggregate trends. Similar patterns are observed in the Provincial labor market, as shown in Panels (c) and (d). In addition, we see similar patterns if we extend the sample period to 1931 (Appendix Figure A.2) and for weekly earnings up to 1925 (Appendix Figure A.3), where we also observe that average weekly earnings increased from the pre to the post-WTR-period, despite this major reduction in daily working hours.

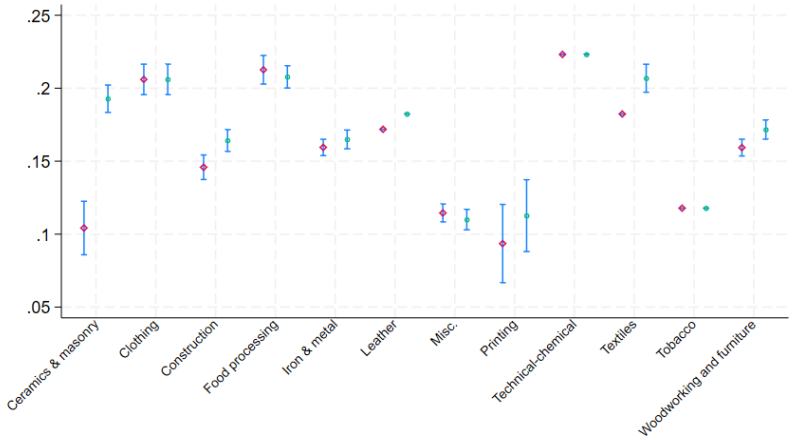
What potential time-related confounding factors existed around 1919? As argued in Hansen (1977), unions affiliated with DSF faced significant pressure from the revolutionary labor movement called the Syndicalists (*Syndikalister* in Danish). The Syndicalists advocated direct action and pushed for workers' control in the management of workplaces. Pressure on DSF intensified following the storming of the Copenhagen Stock Exchange in

Figure 2: The distribution of the 1919 WTR-reform

(a) All occupations



(b) By sub-industry and region

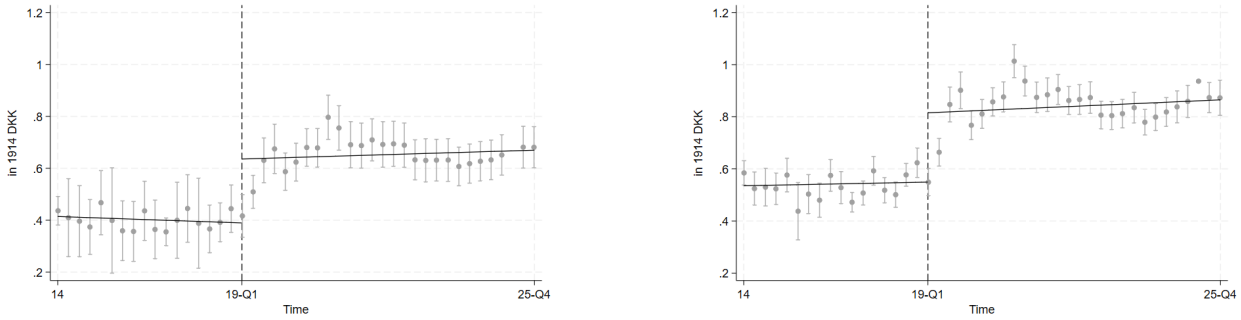


Notes: *This figure illustrates the distribution of the treatment measure, represented as the weekly work-time reduction (WTR) in percentage terms. Specifically, WTR is defined as the logarithmic change in daily hours between 1914 and 1919. Panel (a) shows the WTR across all occupations and regions, while Panel (b) breaks it down by region and industry type. In Panel (b), red dots represent the averages for Copenhagen, and green dots represent those for the Provinces. The whiskers are 95-percent confidence bands.*

Figure 3: Trends in real hourly wages by pre-hours and region, 1914-1925

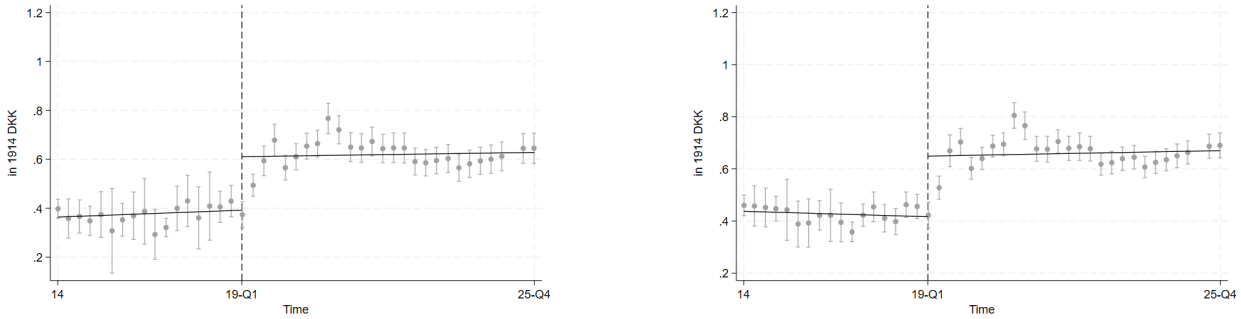
(a) Copenhagen, > 9.5 pre-hours

(b) Copenhagen, < 9.5 pre-hours



(c) The Provinces, > 9.5 pre-hours

(d) The Provinces, < 9.5 pre-hours



Notes: *This figure presents the development real hourly wages for the years 1914-1925. Panels (a) and (b) are for Copenhagen, where we have split the occupational groups by the number of hours worked prior to the 1919-WTR reform (more or less than 9.5 hours). Panels (c) and (d) report the corresponding development for the Provinces. The vertical dashed lines separate the pre- and post 1919-WTR reform periods. Hourly wages are reported in Danish kroner (kr.) in constant 1914 prices.*

January 1918. A group of Syndicalists, frustrated with economic hardship and inspired by revolutionary movements abroad, launched an aggressive demonstration that escalated into an occupation of the trading floor. In general, the Syndicalists influenced more moderate unions into adopting a more combative stance in negotiations to maintain their legitimacy among workers. In addition, Skoglund (2025) documents that unionization had a positive effect on wages in Sweden during the same period. As shown in Appendix Figure A.1, unionization rates in Denmark were also rising over the course of our study period, suggesting that similar mechanisms may have contributed to an upward pressure on wages in the Danish context.

6.2 Wages, earnings, and employment

This subsection presents our main findings on the effects of introducing the 8-hour workday on earnings and employment. Due to missing observations between 1915 and 1917, the sample includes data from 1914 and then resumes in the first quarter of 1917. We require each occupation group to be observed at least twice before the reform, with robustness to these choices demonstrated later. Figure 4 shows the event-study estimates for hourly wages and weekly earnings, with Panels (a) and (c) displaying results for Copenhagen and Panel (b) and (d) for the Provinces. First, we observe that, in both regions, there are no distinct pre-reform trend differences in hourly wages and weekly earnings across occupation groups that were affected differently by the 1919-WTR reform. While earnings may fluctuate across these groups in certain quarters, these changes do not follow any systematic pre-trend.[13]

We find that hourly wages increase, relative to the omitted reference quarter in 1918, but only in Copenhagen. Given the definition of the treatment (WTR), the estimates can be interpreted as the percentage change in hourly wages resulting from a one-percent reduction in work hours (i.e., an elasticity). A positive elasticity indicates partial compensation, while an elasticity of one implies full compensation for the reduction in work hours, leaving weekly earnings unaffected. The red dashed line in the figure indicates what the magnitude of the coefficients should be for full compensation. The point estimate for Q1-1919 indicates an elasticity of close to 0.5, with point estimates for the subsequent post-treatment quarters showing a similar or slightly larger magnitude. This evidence suggests some compensation through an increase in hourly wages in the Copenhagen labor market. This is mirrored in Panel (c) where the outcome is weekly earnings, which combines the loss in working hours and the increase hourly wages. The estimated elasticities are negative in many post-treatment quarters, but not equal to negative one, which would be evidence of full loss in earnings form the reduction in working time.

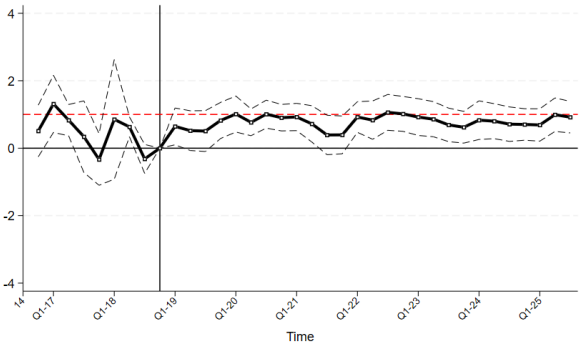
In the Provinces, the hourly wage elasticity remains close to zero and statistically insignificant in the quarters immediately following the work-time reduction, eventually turning negative toward the end of the post-treatment period (Panel (b)). As a result, the weekly earnings elasticity, shown in Panel (d), stays consistently at or below negative one, indicating no evidence of compensation in the aggregate labor market of all the provinces.

Figure 5 presents the corresponding event-study estimates for employment. Once again, the pre-reform trends appear relatively stable. However, there is a notable increase in employment in the Provinces during 1917, likely due to the opening of new export markets

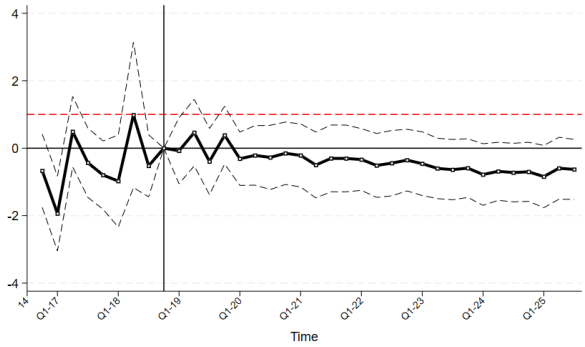
[13]Regarding pre-reform balance, we find that in Copenhagen, occupational groups with higher 1919-WTRs had lower hourly wages, lower weekly earnings, and fewer workers. In contrast, we do not observe these pre-treatment differences in the Provinces (Appendix Table A.4 and A.5).

Figure 4: Wage and weekly earnings effects

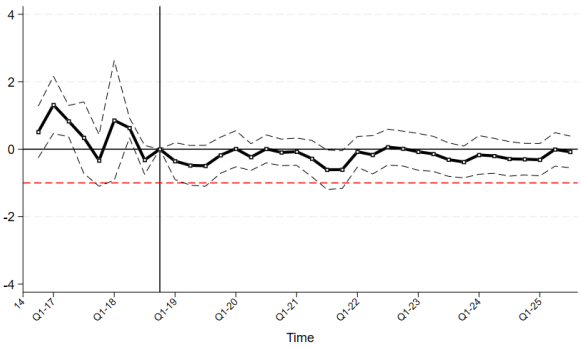
(a) Hourly wages: Copenhagen



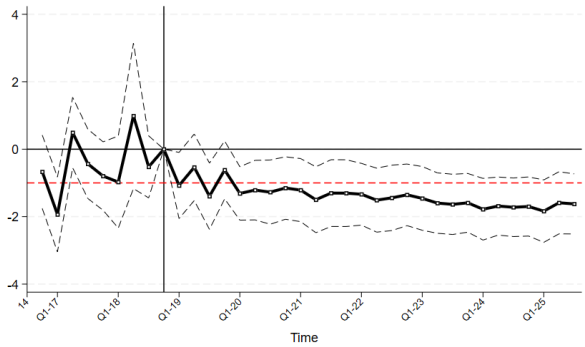
(b) Hourly wages: Provinces



(c) Weekly earnings: Copenhagen



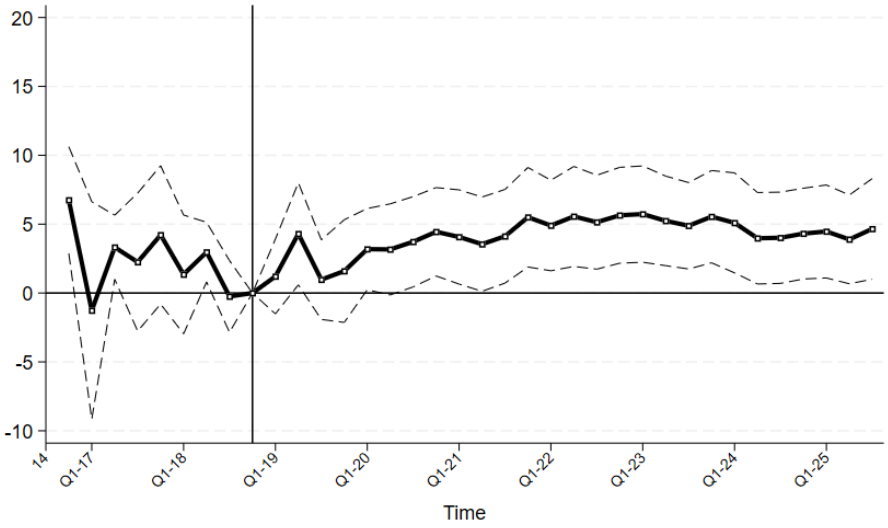
(d) Weekly earnings: The Provinces



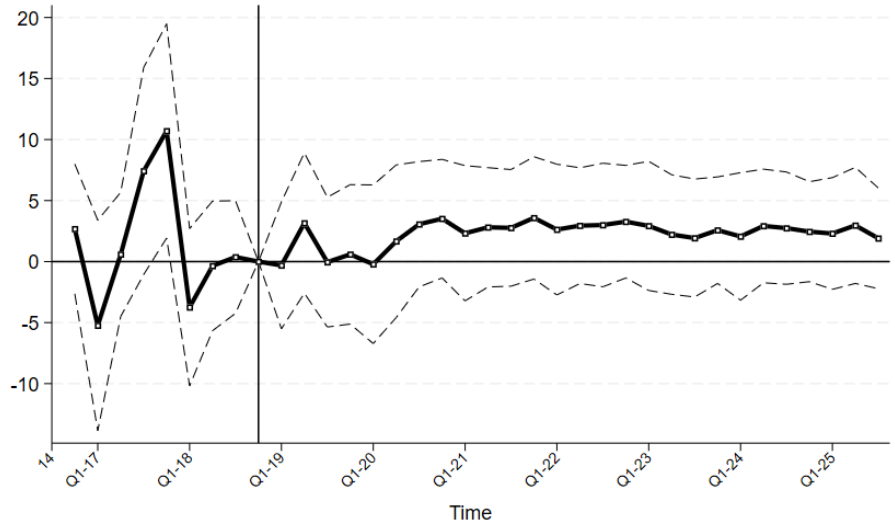
Notes: *This figure presents the event-study results from estimating Eq. (4), where the outcome variable is the log of real hourly wages and the log of real weekly earnings. Panels (a) and (c) display the estimates for Copenhagen, while Panel (b) and (d) show the estimates for the Provinces. Both samples include controls for occupation-group and time fixed effects. The sample period begins in 1914, with quarterly data from 1917 to the second quarter of 1925. To be included, occupation groups must have at least two pre-reform observations. The reference period is the last quarter of 1918. Standard errors are clustered at the occupation-group level, and the dashed black curves represent 95% confidence intervals. The red dashed horizontal lines in Panels (a) and (b) indicate what the size of the estimate should be for full compensation in hourly wages (=1), while the red dashed horizontal lines in Panels (c) and (d) indicates the size of the estimates for weekly earnings if there was no compensation (=-1).*

Figure 5: Employment effects

(a) Copenhagen



(b) The Provinces



Notes: *This figure presents the event-study results from estimating Eq. (4), where the outcome variable is the log employment numbers. Panel (a) displays the estimates for Copenhagen, while Panel (b) shows the estimates for the Provinces. Both samples include controls for occupation-group and time fixed effects. The sample period begins in 1914, with quarterly data from 1917 to the second quarter of 1925. To be included, occupation groups must have at least two pre-reform observations. The reference period is the last quarter of 1918. Standard errors are clustered at the occupation-group level, and the dashed lines represent 95% confidence intervals.*

during WWI driven by the warring countries (Statistiske-Meddelelser 1921). By 1918, however, employment trends across occupation groups converge, even in the Provinces.

The post-reform estimates are all positive, although only borderline statistically significant in some quarters for Copenhagen and in most quarters for the Provinces. Positive estimates suggest that firms hired additional workers to compensate for the reduction in working hours resulting from the reform. While the point estimates indicate full or even more than full replacement, we cannot rule out partial replacement, as all confidence intervals include values below one. Nevertheless, this evidence is consistent with the reform inducing work sharing.

As the event-study evidence suggests that the 1919-WTR reform (if anything) led to a level shift in outcomes, rather than a trend-break, we now present DiD estimates that quantify the average effect of the 1919-WTR reform.[14] Table 1 presents the estimates for hourly wages (Columns (1) and (2)), weekly earnings (Columns (3) and (4)), and employment (Columns (5) and (6)). In Copenhagen, a one percent WTR reduction leads to an average increase in hourly wages of 0.42%, which means that weekly earnings decrease by 0.58%.[15] In the Provinces, there is no significant effect on hourly wages and weekly earnings declined by 1.28% indicating no compensating effect. Columns (5) and (6) show employment elasticities of 1.47 for Copenhagen and 0.95 for the Provinces, though only the former is borderline statistically significant. Whether full replacement of the lost hours occurred or not, these results suggest the presence of work-sharing.

Within the framework of the model in Section 3, one explanation for compensating wage effects only in Copenhagen is differences in preferences over consumption and work. If workers in the Provinces value consumption as much as they dislike working, the model predicts no change in wages, negative effects on weekly earnings, and a positive effect on employment, as we observe in the case of the Provinces. Beyond the model, unions in Copenhagen had greater bargaining power, as reflected in higher unionization rates (Appendix Figure A.1 and Appendix Table A.1), which can increase wage demands after a work-time reduction. Finally, one measurement-based explanation is that the provincial aggregate combines many heterogeneous local labor markets, such that averaging may attenuate or even obscure positive occupation-level wage compensation, especially if the WTR measure is noisier in smaller towns, and we do estimate generally the model for the Provinces with less precision.

[14] If the treatment effects are increasing over time, DiD estimates can be misleading (Goodman-Bacon 2021).

[15] At first glance, the significant negative effect on weekly earnings may seem at odds with the post-reform event estimates in Figure 4. However, if we restrict the pre-reform estimates to average zero—rather than omitting the quarter prior to the reform, as the DiD estimation effectively does—we indeed observe negative post-reform event estimates (Appendix Figure A.4).

Table 1: Effects on hourly wage, weekly earnings and employment

	(1) Wages Hourly	(2) Wages Hourly	(3) Earnings Weekly	(4) Earnings Weekly	(5) Workers	(6) Workers
post × WTR	0.42***	-0.28	-0.58***	-1.28***	1.47*	0.95
	(0.13)	(0.27)	(0.13)	(0.27)	(0.87)	(1.08)
Region	CPH	Provin	CPH	Provin	CPH	Provin
Occupation FE	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes
N × T	2175	2054	2175	2054	2157	2015
N	67	66	67	66	67	65

Notes: *This table report presents difference-in-differences (DiD) estimates from estimating Eq. (4), where the work-time reduction in percent (WTR) is interacted with a post-reform indicator rather than time fixed effects. All regressions include controls for time and occupation-group fixed effects. In columns (1) and (2), the dependent variable is logged hourly wages; in columns (3) and (4), it is logged weekly earnings; and in columns (5) and (6), it is logged employment. Odd-numbered columns use data from Copenhagen, while even-numbered columns use data from provincial areas. Standard errors are clustered at the occupation-group level. ***, **, and * indicate significance at the 1, 5, and 10% level.*

6.2.1 Robustness

We next present robustness checks of the baseline DiD estimates for our three main outcomes—hourly wages, weekly earnings, and employment. The results are reported in Appendix Figures A.5–A.7, where each row corresponds to estimates from a modified version of the baseline specification; for reference, row 1 reproduces the baseline estimate.

First, in the baseline specification we require that each occupation group is observed at least twice in the pre-reform period. If we instead relax this requirement and include all available observations, or if we also drop our baseline interpolation across consecutive quarters (e.g., 1917-Q1 to 1918-Q1), the results for hourly wages, weekly earnings, and employment remain unchanged (rows 2 and 3).

Second, the estimated effects are not driven by secular changes within broader occupational classes. We demonstrate this by controlling for female and unskilled occupations through year fixed effects (rows 4 and 5). As an additional check, we assign each occupation group in the sample a HISCLASS code following van Leeuwen and Maas (2011) and control for this by year fixed effects (row 6).

Third, the balance tables (Appendix Tables A.4 and A.5) reveal initial level differences correlated with the magnitude of the 1919 WTR reform. Although our DiD framework accounts for such level differences, they could potentially interact with other industry-specific changes. To address this concern, we interact these initial levels with the year fixed effects and control for them, confirming that the results are unaffected (row 7).

Fourth, prior research on Sweden during the same period documents that unionization rates influenced wages (Skoglund 2025). Our descriptive evidence likewise suggests that unionization rates were higher in Copenhagen than in the Provinces, which may help explain the regional differences in wage effects. A natural way to assess this within our regression framework would be to control for pre-1919 WTR unionization rates by occupation group. However, this is not feasible, as constructing such rates requires a double crosswalk. Specifically, we only observe union membership counts at the union level; to obtain rates, these must first be combined with data on the number of workers in each occupation, which substantially reduces coverage (first crosswalk). These rates would then need to be mapped into our main dataset (second crosswalk), leaving too few observations to conduct meaningful regressions.

However, we can examine whether the 1917 temporary labor market reform, the "Extraordinary Assistance for the Unemployed, etc.," which arguably increased unionization rates more in some occupations than in others, confounds our results. To capture the intensity of the reform, we measure the growth in union membership between 1917 and 1919 and classify unions into above- and below-median growth groups. This approach circumvents the first crosswalk, but once the second crosswalk is applied, the number of occupation groups is still reduced to approximately 47–49. For Copenhagen, adding this control does not alter the conclusions. For the Provinces, the effect on hourly wages becomes positive but only borderline statistically significant, which may suggest that differences in unionization rates contribute to the wage findings. However, it is difficult to draw strong conclusions, as this result could also be driven by sample selection: when we restrict the sample in the same way but exclude the control, we obtain nearly identical estimates.

Fifth, as mentioned, we have digitized data on wages, weekly earnings, and employment up to 1931, whereas our baseline analysis ends in 1924 because extending the sample results in the attrition of some occupation groups. Nevertheless, when we disregard this issue and extend the sample period, the conclusions remain essentially unchanged, suggesting that the short-run effects did not reverse (or mute) in the latter part of the 1920s (row 9).

6.2.2 Heterogeneity

According to our model, the effect of working hours on wages can be identified under the assumption that preferences over consumption and leisure are homogeneous across occupations within a region. Identifying the effect on employment requires not only this assumption but also the stronger condition that the degree of substitutability between hours and workers remains constant across occupations. In the following, we relax these baseline assumptions and examine heterogeneity across skill groups, as this represents a dimension along which the substitutability between hours and workers (γ in our model) is particularly likely to differ and a characteristic that we can identify in our dataset.

In particular, in Statistiske-Meddelelser (1927), which provides the labor market data, the occupational titles explicitly indicate whether workers are unskilled male workers or female workers. For example, entries include categories such as "unskilled workers in chocolate factories" or "female workers in chocolate factories" (Appendix Table A.6). We exploit this information to construct indicator variables for these two broader unskilled-occupation type groups and interact them with our main treatment measure. The results are presented in Table 2. Across both regions and groups, we find that hourly wages increased more in these occupations relative to other more high-skilled ones (e.g. skilled craftsman), implying that these groups experienced a smaller decline in weekly earnings following the introduction of the eight-hour workday (Columns (1)–(4)). Yet, we note that in the Provinces the hourly wage still decreased, but less so for unskilled and female workers. As shown in Columns (5) and (6), evidence of an increase in employment is found only for the two groups in Copenhagen.

One possible explanation for these patterns, consistent with our model, is that unskilled workers place a relatively higher value on consumption compared to their disutility from work ($\theta > \pi$) than other occupational groups. In addition, in Copenhagen the degree of substitutability between hours and unskilled workers (i.e., a high γ) may have offset the reduction in labor demand resulting from the increase in wage rates.

6.3 Automation

One way by which firms could adjust to the reduction in work hours per worker could be via automation (mechanization). In fact, Milhaud (1925) provides numerous quotes from different countries suggesting automation was necessary to prevent the 8-hour workday from slowing production. This point was also mentioned some 20 years earlier (Commision 1902, p.765):

"*It is true also that the higher the wages and fewer the hours the greater is*

Table 2: Heterogeneity in unskilled and female workers.

	(1) Wages Hourly	(2) Wages Hourly	(3) Earnings Weekly	(4) Earnings Weekly	(5) Workers	(6) Workers
post × WTR	0.21*	-0.64**	-0.79***	-1.64***	0.31	1.48
	(0.12)	(0.27)	(0.12)	(0.27)	(1.00)	(1.07)
post × WTR × unskilled	0.31***	0.39***	0.31***	0.39***	1.30**	-0.42
	(0.09)	(0.12)	(0.09)	(0.12)	(0.64)	(0.64)
post × WTR × female	0.13	0.31*	0.13	0.31*	1.24*	-0.68
	(0.13)	(0.16)	(0.13)	(0.16)	(0.72)	(0.67)
Region	CPH	PRV	CPH	PRV	CPH	PRV
Occupation FE	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes
N × T	2175	2054	2175	2054	2157	2015
N	67	66	67	66	67	65

Notes: *This table reports heterogeneity of treatment effects across occupation groups. Specifically, we include a an additional term, consisting of the DiD variable interacted with dummy variables for whether an occupation consists of unskilled male workers (row 2) or female workers (row 3), who also tended to be unskilled. Columns (1) and (2) report the hourly wage effects of the different groups. Columns (3) and (4) report the effect on weekly earnings Columns (5) and (6) report employment estimates. Odd numbered columns report estimates for Copenhagen, while even-numbered columns report estimates from the Provinces.*

the pressure upon the employer to substitute labor-saving devices and to be more careful in his selection of high-grade workmen."

We investigate the automation hypothesis using the amount horse powers used in production by firm type. This data is from the industrial censuses in 1914 and 1925, and so we do not have the possibility to check pre-trends across the WTR distribution. We use information on the distribution of occupation groups in 1914 to measure exposure to the WTR reform at the firm-type level.

Table 3 presents the DiD estimates. We show the effects on the logged number of horsepower and logged horsepower per worker, with the number of workers derived from the same data source as the horsepower data (i.e., an alternative source to the baseline employment data, which came from the employer payroll lists). We find no evidence of increased horsepower usage (automation/mechanization) in firm types more affected by the 1919-WTR reform based on their pre-WTR employment structure. This holds true for both regions (Columns (1) and (2)), and we observe that horsepower per worker decreased as more affected firm types hired additional workers (Columns (3) and (4)). This also serves as a robustness check for the baseline employment effects, as the data here come from a different source than the employer payroll lists used in the baseline.

6.4 Labor-market conflict

Finally, we examine how the 1919-WTR reform affected labor market conflicts. The 8-hour workday had been a key demand of the Danish and international labor movements since the late 19th century, so one might expect fewer conflicts (i.e., strikes) once it was achieved. However, the historical account indicates that employers disliked the 8-hour workday and attempted to roll it back in the early 1920s, though this ultimately did not happen.

We have detailed data on the exact date, location, and type of conflict (strike or lockout). This data is organized by union type, and we cross-reference it with our occupation groups (from the employers payroll lists) to measure conflicts within our baseline categories. As conflicts are relatively rare events at the occupation group level, we aggregate the data annually and create a dummy variable indicating whether an occupation group was involved in a strike or lockout in a given year, using this as our outcome variable for conflict.

Because this conflict data is available for several years prior to the reform, we present the results as an event study in Figure 6. We do not find any evidence of differential pre-trends across the WTR distribution in either region or for either type of conflict. Additionally, there is little to suggest that the 1919-WTR reform significantly reduced or increased conflicts in

Table 3: Effects on automation

	(1) HP	(2) HP	(3) $\frac{\text{HP}}{\text{Workers}}$	(4) $\frac{\text{HP}}{\text{Workers}}$	(5) Workers	(6) Workers
post × WTR	-1.33	-0.83	-5.21***	-5.19***	3.88**	4.58**
	(1.49)	(1.58)	(1.46)	(1.79)	(1.64)	(1.85)
Region	CPH	Provin	CPH	Provin	CPH	Provin
Occupation FE	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes
N × T	168	156	168	154	168	156
N	84	78	84	77	84	78

Notes: *This table report presents difference-in-differences (DiD) estimates where the outcomes are measured from the industrial census in 1914 and 1925 at the firm-type level. The treatment (WTR) measures how exposed the firm was the to work-time reduction in percent. All regressions include controls for year and firm-type fixed effects. In columns (1) and (2), the dependent variable is logged horse power used in production; in columns (3) and (4), it is logged horse power per worker; and in columns (5) and (6), it is logged employment measured in the industrial census by firm type. Odd-numbered columns use data from Copenhagen, while even-numbered columns use data from the Provinces. Standard errors are clustered at the firm-type level. ***, **, and * indicate significance at the 1, 5, and 10% level.*

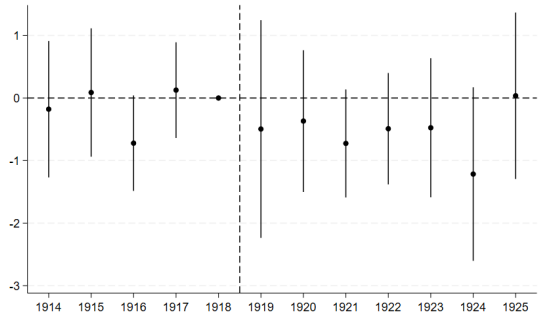
the two labor markets. We obtain similar findings when examining the number of lost workdays per worker due to the two types of conflicts (Appendix Figure A.8).

7 Concluding remarks

In this paper, we analyze the largest work-time reduction (WTR) reform in Danish history, focusing on its impact on earnings, employment, and labor market dynamics for the period 1914 to 1925. Our findings reveal that weekly earnings decline more in occupation groups with greater reductions in work hours. In the labor market of Copenhagen, we find that one percent reduction in work hours led to a 0.6 percent decrease in weekly earnings, implying a statistically significant compensating hourly wage increase of 0.4 percent. In the Provinces, weekly earnings appear to decline in direct proportion to work hours, or put differently we cannot reject any effects on hourly wages. Our evidence also suggests that firms responded to the introduction of the 8-hour workday by hiring new workers. In a union–monopoly model, these findings can be explained by workers valuing consumption more than they dislike working (in Copenhagen) and that workers, in general, are substitutable with hours.

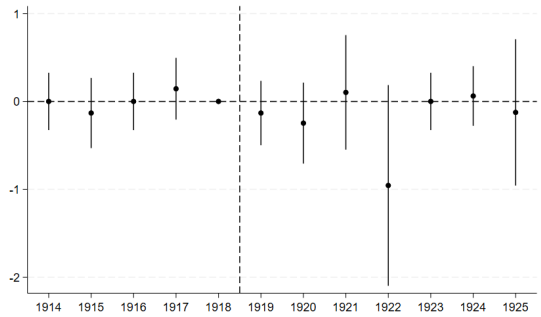
Figure 6: Effects on labor market conflicts

(a) Strikes in Copenhagen

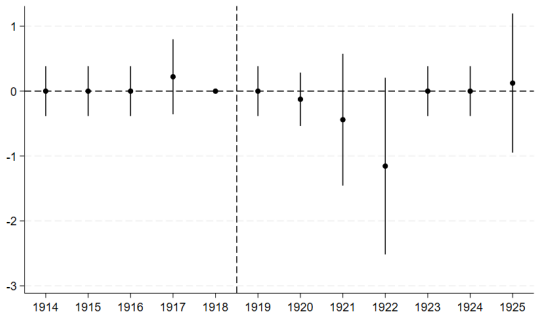


(b) Strikes in the Provinces

(c) Lockouts in Copenhagen



(d) Lockouts in the Provinces



Notes: *This figure reports event-study estimates when the outcomes are conflict in the labor market. Treatment (WTR) here is defined at the union occupation level and the model has been collapsed such that the data is annual. The outcome in Panels (a) and (b) is an indicator variable equal to one if there was a strike in a given occupation, while in Panels (c) and (d) the outcome is an indicator variable equal to one if there was a lockout. All regressions control for year and occupation fixed effects.*

Our heterogeneity analysis shows that hourly wages for unskilled workers increased more in Copenhagen, and in the Provinces their decline was smaller relative to other occupational groups. According to our model, these patterns may reflect differences in preferences across skill groups. We further find that unskilled workers experienced larger employment gains in Copenhagen, which could suggest that unskilled labor is more easily substitutable for hours in the capital.

What are the implications of these insights? Most importantly, consistent with our model predictions, there is no historical evidence from this reform to support the notion that introducing a 4-day workweek today would be without cost, contrary to some policymakers' and reports' suggestions. Additionally, our evidence indicates that shortening the workweek could increase employment opportunities for marginal groups in the labor market. However, it is important to remember that the 1919-WTR reform was implemented during a period characterized by an industrial labor supply surplus and relatively high unemployment rates nationwide.

References

Acemoglu, Daron, "When does labor scarcity encourage innovation?," *Journal of Political Economy*, 2010, *118* (6), 1037–1078.

Arbejdsmarkedskommission, *Virkningerne af en Arbejdstidsnedsættelse i Danmark*, Universitets-Bogtrykkeri, København, 1952.

Atack, Jeremy, Fred Bateman, and Robert A Margo, "Productivity in Manufacturing and the Length of the Working Day: Evidence from the 1880 Census of Manufactures," *Explorations in Economic History*, 2003, *40* (2), 170–194.

Bengtsson, Erik and Jakob Molinder, "The economic effects of the 1920 eight-hour working day reform in Sweden," *Scandinavian Economic History Review*, 2017, *65* (2), 149–168.

Calmfors, Lars, "Work sharing, employment and wages," *European Economic Review*, 1985, *27* (3), 293–309.

Caprettini, Bruno and Hans-Joachim Voth, "Rage against the machines: Labor-saving technology and unrest in industrializing England," *American Economic Review: Insights*, 2020, *2* (3), 305–320.

Chemin, Matthieu and Etienne Wasmer, "Using Alsace-Moselle local laws to build a difference-in-differences estimation strategy of the employment effects of the 35-hour workweek regulation in France," *Journal of Labor economics*, 2009, *27* (4), 487–524.

Collewet, Marion and Jan Sauermann, "Working hours and productivity," *Labour economics*, 2017, *47*, 96–106.

Commision, Industrial, *Final Report of the Industrial Commission*, Vol. XIX, Washington: Government Printing Office, 1902. Prepared in accordance with an act of Congress approved June 18, 1898.

Crépon, Bruno and Francis Kramarz, "Employed 40 hours or not employed 39: Lessons from the 1982 mandatory reduction of the workweek," *Journal of Political Economy*, 2002, *110* (6), 1355–1389.

Cross, Gary, "Les trois huits: labor movements, international reform, and the origins of the eight-hour day, 1919-1924," *French Historical Studies*, 1985, *14* (2), 240–268.

DSF, *Beretning om De samvirkende fagforbunds virksomhed, 1. April 1919 - 31. Marts 1920*, F.E. Bording (V. Petersen), 1920.

Du, Zaichao, Hua Yin, and Lin Zhang, "The macroeconomic effects of the 35-h workweek regulation in France," *The BE Journal of Macroeconomics*, 2013, *13* (1), 881–901.

Estevão, Marcello and Filipa Sá, "The 35-hour workweek in France: Straightjacket or welfare improvement?," *Economic Policy*, 2008, *23* (55), 418–463.

Fishback, Price, Chris Vickers, and Nicolas L Ziebarth, "The Labor Market Effects of Workweek Restrictions: Evidence from the Great Depression," *American Economic Journal: Macroeconomics*, 2024, *16* (4), 77–113.

Goodman-Bacon, Andrew, "Difference-in-differences with variation in treatment timing," *Journal of econometrics*, 2021, *225* (2), 254–277.

Hansen, Svend Aage, *Økonomisk vækst i Danmark, Bind II*, Akademisk Forlag, 1977.

Hazan, Moshe, "Longevity and lifetime labor supply: Evidence and implications," *Econometrica*, 2009, *77* (6), 1829–1863.

Holmlund, Bertil and Per Lundborg, "Unemployment insurance and union wage setting," *The Scandinavian Journal of Economics*, 1988, pp. 161–172.

Holt, Andrew Chase, "Monopsony power in the United States: Evidence from the great depression," *Explorations in Economic History*, 2024, *92*, 101570.

Hunt, Jennifer, "Has work-sharing worked in Germany?," *The Quarterly Journal of Economics*, 1999, *114* (1), 117–148.

Knutsen, Carl H. and Magnus B Rasmussen, "Imperial Labor Standards: Working Time Regulation in a World of Empires," *Working paper*, 2023.

Kolstrup, Søren, "8-timers dagen: Kravets formulering og dets gennemførelse i Danmark," *Erhvervshistorisk Årbog*, 1972, *23*, 7–59.

_ , *Velfærdsstatens rødder: Fra kommunesocialisme til folkepension* number 38. In 'SFAH skriftserie.', København: Selskabet til Forskning i Arbejderbevægelsens Historie, 1998.

Madsen, Jørgen Steen, *Septemberforliget og den danske model: baggrund, indhold og udvikling 1899-1999*, Dansk Arbejdsgiverforening, 1999.

Milhaud, Edgard, "The Results of the Adoption of the Eight-Hour Day: The Eight-Hour Day and Technical Progress," *Int'l Lab. Rev.*, 1925, *12*, 820.

Molinder, Jakob, Tobias Karlsson, and Kerstin Enflo, "More power to the people: electricity adoption, technological change, and labor conflict," *The Journal of Economic History*, 2021, *81* (2), 481–512.

_ , _ , and _ , "Social democracy and the decline of strikes," *Explorations in Economic History*, 2022, *83*, 101420.

Nielsen, Poul Vitus, "De tog, De fik, De Otte Timer: Arbejdsgivere og arbejdere, socialdemokrater og syndikalister i kampen om 8 timers arbejdsdagen," *Årbog for arbejderbevægelsens historie*, 1992, pp. 263–312.

of Labor Statistics United States Department of Labor, Bureau, *Hours of Work and Output*, Vol. 917 of *Bulletin*, U.S. Government Printing Office, 1947.

Pencavel, John, "The productivity of working hours," *The Economic Journal*, 2015, *125* (589), 2052–2076.

_ , "Recovery from work and the productivity of working hours," *Economica*, 2016, *83* (332), 545–563.

Rasmussen, M, "The great standardization: Working hours around the world since 1789," *Labor History*, 2024.

Schmick, Ethan, "Collective action and the origins of the American labor movement," *The Journal of Economic History*, 2018, *78* (3), 744–784.

Skoglund, William, "Union wage effects in Sweden: Evidence from the interwar period," *Explorations in Economic History*, 2025, *96*, 101655.

Statistisk-Tabelværk, Danmarks Statistik, *Danmarks Haandværk og Industri Ifølge Tællingen den 26. Maj 1914*, Det Statistsike Departement, København, 1917.

_ , *Erhvervs- og Indtæktsforholdene ved Folketællingen d. 1. Februar 1916*, Det Statistsike Departement, København, 1920.

_ , *Erhvervstællingen 1925*, Det Statistsike Departement, København, 1929.

Statistiske-Meddelelser, Danmarks Statistik, *Arbejdsoverenskomster i Danmark, den 1. august 1915*, Det Statistsike Departement, København, 1916.

_ , *Strejker og Lockouter, i Danmark 1911-15*, Det Statistsike Departement, København, 1917.

_ , *Arbejdsløsheden i Aarene 1915-1919*, Det Statistsike Departement, København, 1921.

_ , *Strejker og Lockouter, i Danmark 1916-20*, Det Statistsike Departement, København, 1922.

_ , *Arbejdsløsheden i Aarene 1920-1924*, Det Statistsike Departement, København, 1926.

_ , *Strejker og Lockouter, i Danmark 1921-25*, Det Statistsike Departement, København, 1926.

_ , *Arbejdslønnen i Industrien M.V. i Danmark 1914-25*, Det Statistsike Departement, København, 1927.

_ , *Arbejdslønnen i Industrien M.V. i Danmark 1926-31*, Det Statistsike Departement, København, 1933.

van Leeuwen, Marco H. D. and Ineke Maas, *HISCLASS: A Historical International Social Class Scheme*, Leuven: Leuven University Press, 2011.

Voth, Hans-Joachim, Bruno Caprettini, and Alex Trew, "Fighting for growth: Labor scarcity and technological progress during the British industrial revolution," *CEPR Discussion Papers*, 2023, (17881).

A Online Appendix

A.1 Model

This appendix section goes through the monopoly-union model of Calmfors (1985) in more details compared to the main text. We first set up the general model and hereafter, under simple functional forms, obtain closed-form predictions for wages, weekly earnings, and employment as functions of hours, which are the ones presented in the main text. These clarify under which conditions hours cuts can trigger an increase in the wage rate, that weekly earnings must fall, and when work-sharing occurs (i.e., employment increases).

First, as also argued in the main text, our empirical setting maps naturally into Calmfors (1985)'s model. Each occupation is represented by a corresponding union that sets wages within its sector, while the introduction of the 8-hour workday resulted from central negotiations between DSF and DA. As a first approximation, this reform can therefore be treated as exogenous to wage-setting decisions of the individual union.

In the model, the utility function of an employed worker is given as:

$$V = V(c, h), \tag{A.1}$$

where w is the wage, h hours (per day), and so wh is the total labor income, which is used for consumption, c (i.e., $c = wh$). It is assumed that $V_c > 0, V_{cc} < 0$, $V_h < 0$, $V_{hh} < 0$ such that workers enjoy consumption and dislikes working.

An unemployed worker gets the following utility:

$$\bar{V} = V(b, 0) \tag{A.2}$$

where b denotes the unemployment benefit, which in the Danish labor market was (and still is) administered by so-called unemployment insurance funds formally independent of unions. In practice, however, this was not the case ("Ghent style"). This feature could be incorporated into the model following Holmlund and Lundborg (1988) but doing so would not add further insights into how working hours affect wages and employment. The derivatives are: $\bar{V}_b > 0$ and $\bar{V}_{bb} < 0$.

Assume that N is the number of employed workes and M is the total number of workers (thus, $M - N$ are unemployed). In this case, expected utility is given by:

$$U = \frac{N}{M} V(c, h) + \frac{M - N}{M} V(b, 0),$$

since we are not in the framework of Holmlund and Lundborg (1988), we normalize $M = 1$ (and so $N \leq 1$) without loss of generality:

$$U = N \times V(c, h) + (1 - N) \times V(b, 0). \tag{A.3}$$

The union sets w, whereas the firm determines the level of employment, taking the wage rate and hours per day as given. The labor demand function is represented as:

$$N = N(w, h), \tag{A.4}$$

where $N_w < 0$, meaning that an increase in the wage rate lowers labor demand. A higher numerical value implies that the union is less aggressive in its wage setting. Calmfors argues that N_h is ambiguous. He writes: "*It is thus not in general obvious that a reduction of working time will increase employment even at an unchanged wage*". Nevertheless, it seems most plausible to us that, other things being equal $N_h < 0$; that is, working hours and workers are to some extent substitutes. Calmfors also considers the perhaps more extreme case where they are perfect substitutes (or more precisely, firms are indifferent between workers and hours), showing that $N_h = -N/h$. Introducing a fixed cost for firm of hiring workers, as in Hunt (1999), implies that they are less than perfect substitutes.

The trade unions preferred wages rate is found by inserting Eq. (A.4) into Eq. (A.3) and choosing w that maximizes expected utility:

$$\max_w U = N(w,h) \times V(c,h) - N(w,h) \times \bar{V}(b,0) + \bar{V}(b,0). \tag{A.5}$$

The first order conditions is:

$$\frac{\partial U}{\partial w} = N_w(V - \bar{V}) + NhV_c = 0. \tag{A.6}$$

Calmfors (1985) shows that the effect of hours on wages is generally ambiguous. However, if labor demand is less elastic, it becomes more likely that unions will respond to an exogenous reduction in hours by pushing up wages, for example. He also shows that if the initial number of hours worked is below the level the trade union considers "optimal," an exogenous reduction in hours will raise the union's preferred wage rate.[16]

Next, we apply simple functional forms to the model, allowing for closed-form solutions for wages, income, and employment. These solutions help motivate the baseline empirical specification and provide theoretical predictions. We use the following functional forms for the utility and demand functions:

$$V = c^{\theta} h^{-\pi}, \text{ where } \theta, \pi > 0 \tag{A.7}$$

$$N = A w^{-\varepsilon} h^{-\gamma}, \text{ where } A, \varepsilon, \gamma > 0 \tag{A.8}$$

$$\bar{V} = b, \text{ where } b > 0. \tag{A.9}$$

In the utility specification, hours worked h enter with disutility (workers prefer leisure). Below, we also consider a time-constraint version where the day is normalized to 1 so leisure equals $(1 - h)$. We assume an isoelastic labor-demand curve $N = A\, w^{-\varepsilon} h^{-\gamma}$, where ε is the wage elasticity of labor demand, γ governs how hours shift labor demand (with $\gamma = 1$ implying firms are indifferent between adjusting hours and heads), and A is a productivity/scale parameter. Finally, unemployment benefits are taken as exogenous and fixed at b.

Inserting Eqs. (A.7)-(A.9) into the first-order condition in Eq. (A.6) gives closed-form solutions:

[16] The optimal hours are obtained by allowing the union to choose h in Eq. A.5.

$$w = \left(\frac{\varepsilon b}{(\varepsilon - \theta) h^{\theta - \pi}} \right)^{\frac{1}{\theta}} \Leftrightarrow$$
$$\ln w = \frac{1}{\theta} \ln \varepsilon b - \frac{1}{\theta} \ln (\varepsilon - \theta) - \frac{\theta - \pi}{\theta} \ln h, \tag{A.10}$$

So if $\pi < \theta$, reducing hours ($\downarrow h$) raises w. Intuition: a cut in hours lowers consumption but increases leisure (or decreases work hours); when the preference weight on consumption exceeds that on hours' disutility ($\pi < \theta$), the income loss dominates the leisure gain, so the union raises the hourly wage to partly offset it.

If one adds an index for occupation type and time, this equation motivates our baseline empirical specification.

$$\ln w_{it} = \beta \ln h_{it} + \mu_i + \mu_t + \epsilon_{it} \tag{A.11}$$

where $\beta = -(\theta - \pi)/\theta$ and $\mu_i = \frac{1}{\theta} \ln \varepsilon_i b_i - \frac{1}{\theta} \ln (\varepsilon_i - \theta)$, thus our framework allows for differences in the wage elasticity and unemployment benefits across occupation types by controlling for occupation fixed effects.

Next, the closed-form solution for total income is given by:

$$wh = \left(\frac{\varepsilon b}{(\varepsilon - \theta) h^{\theta - \pi}} \right)^{\frac{1}{\theta}} h \Leftrightarrow$$
$$\ln wh = \frac{1}{\theta} \ln \varepsilon b - \frac{1}{\theta} \ln (\varepsilon - \theta) - \left(\frac{\pi}{\theta} \right) \ln h, \tag{A.12}$$

which makes it clear that the possible compensating effect from above is never strong enough to counteract a fall in total income, since $\theta, \pi > 0$ by assumption.

Finally, the closed-form solution for employment is given by:

$$N = A w^{-\varepsilon} h^{-\gamma} = A \left(\frac{\varepsilon b}{(\theta - \varepsilon) h^{\theta - \pi}} \right)^{\frac{-\varepsilon}{\theta}} h^{-\gamma}$$
$$\ln N = \ln A - \frac{\varepsilon}{\theta} \ln \varepsilon b + \frac{\varepsilon}{\theta} \ln (\varepsilon - \theta) + \left(\varepsilon \frac{\theta - \pi}{\theta} - \gamma \right) \ln h. \tag{A.13}$$

Two opposing channels operate when hours h fall. First, the **wage channel**: if $\theta > \pi$, the union raises w, which depresses labor demand roughly in proportion to the wage elasticity ε. Second, the **substitution channel**: firms can substitute workers for hours; at a given w, employment moves with elasticity $-\gamma$ in h, so a larger γ makes "work sharing" more likely. Combining these yields the condition for employment to rise when hours fall:

$$\gamma \; > \; \varepsilon \, \frac{\theta - \pi}{\theta}.$$

This also explains possible heterogeneity across occupations. Where it is *easier* to replace hours with additional workers (higher γ)—for example, low-skilled jobs with modular

tasks and short training—employment is more likely to increase after a WTR. Conversely, sizable *per-worker fixed costs* (e.g., task-specific capital) make firms substitute less toward workers (lower γ), dampening the employment response. Finally, product-market conditions interact with this: in local non-tradables and larger establishments (plausibly smaller ε), the threshold $\varepsilon(\theta-\pi)/\theta$ is lower, so the same substitution capacity translates into a larger employment increase.

If one instead uses an alternative utility function, where time is constrained (and here we simply normalize to one) as follows:

$$V = c^{\theta}\,(1-h)^{1-\theta},$$

we obtain the following solutions for wage, income, and employment:

$$w = \left[\frac{\varepsilon b}{\varepsilon-\theta}\right]^{\frac{1}{\theta}} h^{-1}(1-h)^{\frac{-(1-\theta)}{\theta}}, \tag{A.14}$$

$$wh = w = \left[\frac{\varepsilon b}{\varepsilon-\theta}\right]^{\frac{1}{\theta}} (1-h)^{\frac{-(1-\theta)}{\theta}}, \tag{A.15}$$

$$N = A\left[\frac{\varepsilon-\theta}{\varepsilon b}\right]^{\frac{\varepsilon}{\theta}} h^{\varepsilon-\gamma}(1-h)^{\frac{\varepsilon(1-\theta)}{\theta}}. \tag{A.16}$$

where it can be demonstrated that wages rise when hours fall iff $h<\theta$. In other words, if baseline hours are a smaller share than the consumption weight (θ) cutting hours raises the marginal value of income enough that the union pushes up w. Total earnings always fall as hours fall, and employment increases iff:

$$\gamma > \varepsilon - \varepsilon\frac{1-\theta}{\theta}\frac{h}{1-h}$$

So according to this model, it is easier to see work-sharing when demand is less elastic (smaller ε), swapping heads for hours is easy (larger γ), and baseline h is not too high. In other words, this model predicts that initial level of hours also matter, not only the WTR.

A.1.1 Why partial wage compensation in Copenhagen but not in the Provinces?

Within the same Calmfors (1985) framework, different channels can rationalize stronger wage compensation to hours cuts in Copenhagen than in the Provinces:

(i) Baseline-hours channel:

Under the time-constraint utility $V(c,h) = c^{\theta}(1-h)^{1-\theta}$,

$$\frac{\partial \ln w}{\partial \ln h} = -1 + \frac{1-\theta}{\theta}\,\frac{h}{1-h},$$

so wages rise when hours fall (partial compensation) iff $h<\theta$. If pre-reform hours were lower in Copenhagen than in the rest of the country, the same WTR mechanically generates

stronger compensation there.

(ii) Composition/aggregation channel:

With $V = c^{\theta} h^{-\pi}$, the occupation-level slope is $\beta_i = -(\theta_i - \pi_i)/\theta_i$. A region's estimated response is a *weighted average* of β_i across occupations. The Provinces aggregates many heterogeneous local markets; averaging can attenuate or even mask positive occupation-level compensation, especially if $\Delta \ln h$ is noisier in small towns.

(iii) Local elasticity channel:

If labor demand is locally translog,

$$\ln N = \alpha - \varepsilon \ln w - \gamma \ln h - \tfrac{\tau}{2}(\ln w)^2 - \sigma(\ln w)(\ln h), \quad \varepsilon_{\text{eff}} = \varepsilon + \tau \ln w + \sigma \ln h,$$

the union FOC is unchanged except $\varepsilon \to \varepsilon_{\text{eff}}$. A standard linearization yields

$$\frac{\partial \ln w}{\partial \ln h} = \frac{-(\theta - \pi) \; + \; \sigma\left(\frac{1}{\varepsilon_{\text{eff}}} - \frac{1}{\varepsilon_{\text{eff}} - \theta}\right)}{\theta \; - \; \tau\left(\frac{1}{\varepsilon_{\text{eff}}} - \frac{1}{\varepsilon_{\text{eff}} - \theta}\right)}.$$

If Copenhagen has smaller ε_{eff} (thicker markets, more non-tradables/market power, and *higher unionization/coordination*), the fraction is more negative, i.e., stronger compensation in Copenhagen.

One can interpret Copenhagen's higher unionization as lowering the *effective* wage elasticity of labor demand faced by unions, i.e. ε_{eff} in the local-elasticity expression. Thus union density amplifies partial compensation (smaller ε_{eff} makes $\partial \ln w / \partial \ln h$ more negative).

In sum, differences in baseline hours (h), occupational mix and aggregation, and the *local* wage elasticity of labor demand (ε_{eff}), with unionization feeding into ε_{eff}, provide theory-consistent reasons for observing clear partial compensation in Copenhagen but not in the Provinces.

A.1.2 Productivity effects

We now study productivity effect from the adjustments in the labor market. Let effective labor be $E = h^{\psi} N$ with $\psi > 0$, and short-run production be:

$$Q \;=\; \kappa \, E^{\alpha} K^{\,1-\alpha}, \qquad 0 < \alpha < 1, \; \kappa > 0, \; K \text{ fixed.} \tag{A.17}$$

The firm maximizes profit by choosing the number of workers (N) and capital (K) is fixed in the short:

$$\max_{N} \;\; \pi \;=\; \kappa \, (h^{\psi} N)^{\alpha} K^{\,1-\alpha} \;-\; w \, h \, N. \tag{A.18}$$

First order condition is:

$$\frac{\partial \pi}{\partial N} \;=\; \kappa \, \alpha \, E^{\alpha-1} K^{\,1-\alpha} \, h^{\psi} \;-\; w \, h \;=\; 0 \;\iff\; \kappa \, \alpha \, K^{\,1-\alpha} \, E^{\alpha-1} \;=\; w \, h^{\,1-\psi}. \tag{A.19}$$

Next, solve for effective labor E from (A.19):

$$E \;=\; \left(\kappa\,\alpha\,K^{\,1-\alpha}\right)^{\frac{1}{1-\alpha}}\;\left(w\,h^{\,1-\psi}\right)^{-\frac{1}{1-\alpha}}. \tag{A.20}$$

Define the short-run wage elasticity and the productivity/scaling parameter:

$$\varepsilon \;\equiv\; \frac{1}{1-\alpha}, \qquad A \;\equiv\; \left(\kappa\,\alpha\,K^{\,1-\alpha}\right)^{\varepsilon}. \tag{A.21}$$

Since $N = E/h^{\psi}$, combining (A.20) and (A.21) yields

$$N(w,h) \;=\; A\,w^{-\varepsilon}\,h^{-\left[\varepsilon(1-\psi)+\psi\right]} \;\equiv\; A\,w^{-\varepsilon}\,h^{-\gamma}, \tag{A.22}$$

where $\gamma = \varepsilon - \psi(\varepsilon - 1)$. When $\psi = 1$ (effective labor $E = hN$), $\gamma = 1$. If $0 < \psi < 1$ (diminishing returns to per-worker hours), then $1 < \gamma \leq \varepsilon$, so shorter shifts call for *more than one-for-one* extra heads. If $\psi > 1$, this would imply that $\gamma < 1$.

Now we have the same labor demand function as we used solving the union problem, and we can express effective labor and production as functions of working hours and the remaing parameters of the model:

$$E = (\kappa\alpha K^{\,1-\alpha})^{\varepsilon}\left[\frac{\varepsilon-\theta}{\varepsilon b}\right]^{\frac{\varepsilon}{\theta}}\;h^{\,\varepsilon\left(\psi-\frac{\pi}{\theta}\right)}, \tag{A.23}$$

$$Q = \kappa^{\varepsilon}\,\alpha^{\,\varepsilon-1}\,K\;\left[\frac{\varepsilon-\theta}{\varepsilon b}\right]^{\frac{\varepsilon-1}{\theta}}\;h^{\,(\varepsilon-1)\left(\psi-\frac{\pi}{\theta}\right)}. \tag{A.24}$$

With $\varepsilon > 1$ and $\theta > \pi$, if $\psi > 1$ (equivalently $\gamma < 1$), then a reduction in hours lowers both effective labor E and total output Q. The reason is that $\psi > 1$ captures that hours and workers are less than unit-elastic substitutes (e.g., due to per worker fixed hiring costs). When hours are reduced, firms do not expand workers one-for-one, so effective labor falls; and because $\theta > \pi$, the union raises the hourly wage, further restraining employment.

Finally, we can derive to often used productivity measures

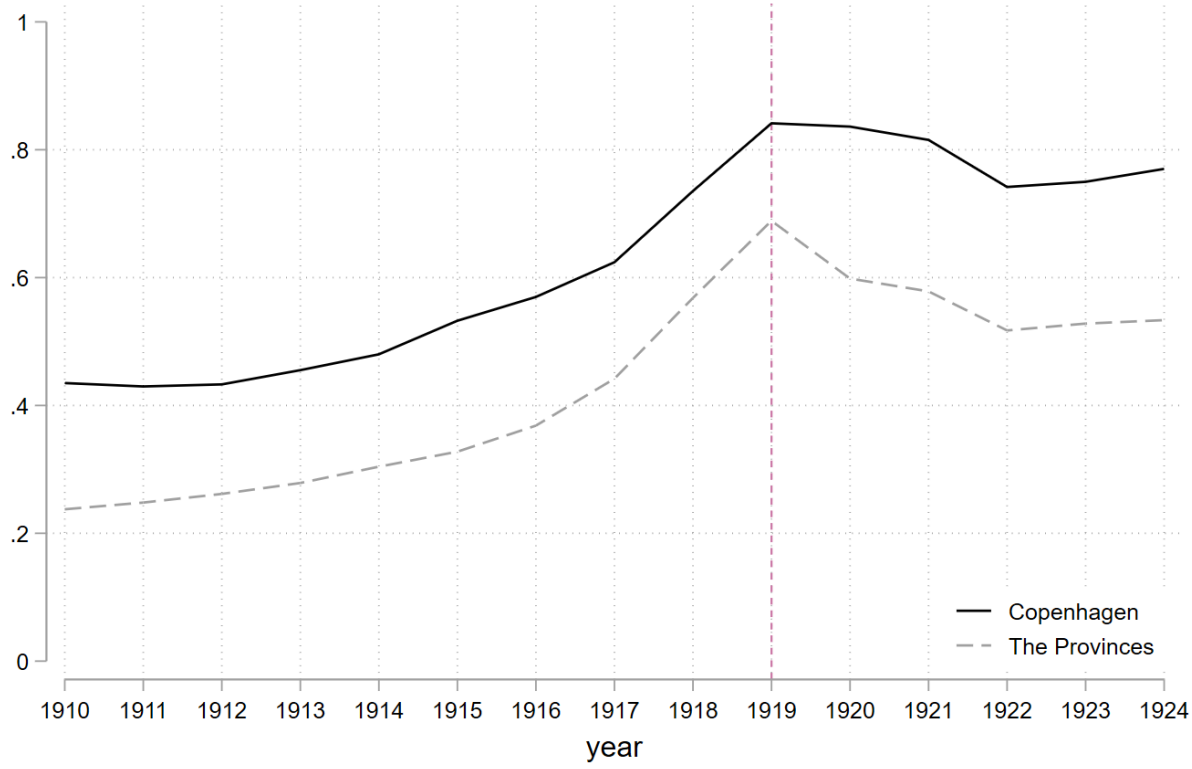
$$\frac{Q}{Nh} = \frac{\varepsilon-1}{\varepsilon}w = \frac{\varepsilon}{\varepsilon-1}\;\left[\frac{\varepsilon b}{(\varepsilon-\theta)\,h^{\,\theta-\pi}}\right]^{1/\theta} \tag{A.25}$$

$$\frac{Q}{N} = \frac{\varepsilon-1}{\varepsilon}wh = \frac{\varepsilon}{\varepsilon-1}\;\left[\frac{\varepsilon b}{\varepsilon-\theta}\right]^{1/\theta}\;h^{\,\pi/\theta} \tag{A.26}$$

These show that when hours decrease output per hour increase in proportion to the wage rate (if $\theta > \pi$), while out per worker decrease, mirroring the loss in weakly earnings.

A.2 Additional figures

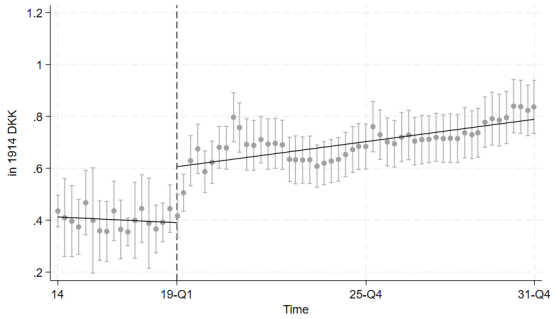
Figure A.1: Unionization rates by region



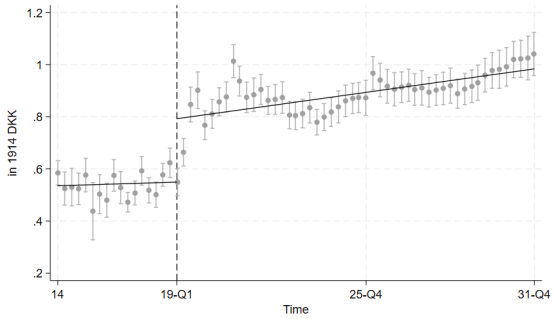
Notes: *This figure shows the development of unionization rates by area (Copenhagen and the Provinces). The numerator is the number of union members, for which we have annual data. The denominator is the number of employees in the industrial sector, observed in 1906, 1914, and 1925 from the industrial censuses and linearly interpolated for the intervening years. We exclude unions for office-, trade-, and agricultural workers, since these groups are not covered in the industrial censuses. The black solid line represents the Capital region, while the gray dashed line represents the Provinces. Union membership data are from Statistiske-Meddelelser (1921) and Statistiske-Meddelelser (1926a).*

Figure A.2: Trends in real hourly wages by pre-hours and region, 1914-1931

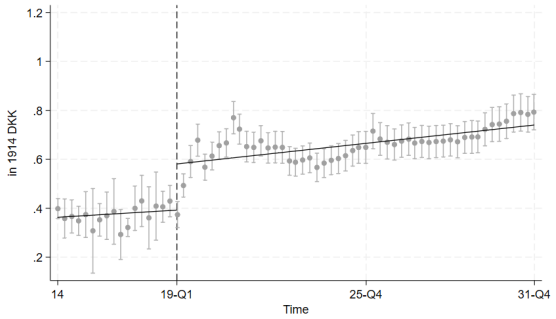
(a) Copenhagen, > 9.5 pre-hours



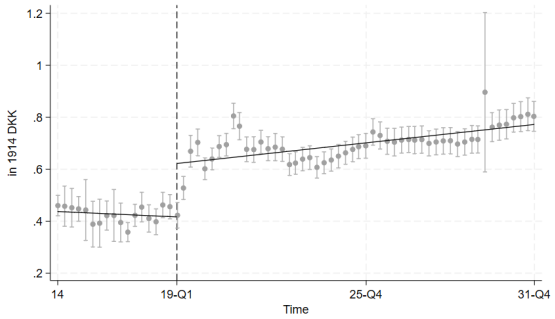
(b) Copenhagen, < 9.5 pre-hours



(c) The Provinces, > 9.5 pre-hours



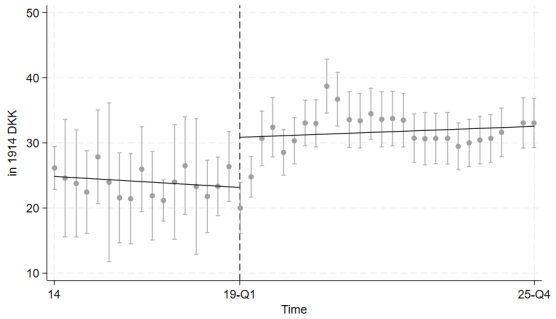
(d) The Provinces, < 9.5 pre-hours



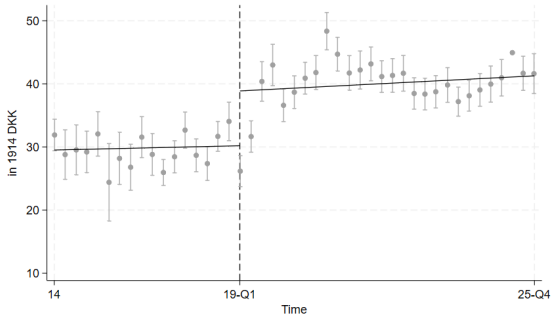
Notes: *This figure presents the development real hourly wages for the years 1914-1931. Panels (a) and (b) are for Copenhagen, where we have split the occupational groups by the number of hours worked prior to the 1919-WTR reform (more or less than 9.5 hours). Panels (c) and (d) report the corresponding development for the Provinces. The vertical dashed lines separate the pre- and post 1919-WTR reform periods. Hourly wages are reported in Danish kroner (kr.) in constant 1914 prices.*

Figure A.3: Trends in real weekly earnings by pre-hours and region, 1914-1925

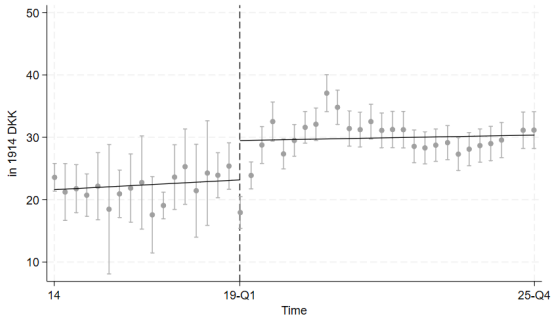
(a) Copenhagen, $>$ 9.5 pre-hours



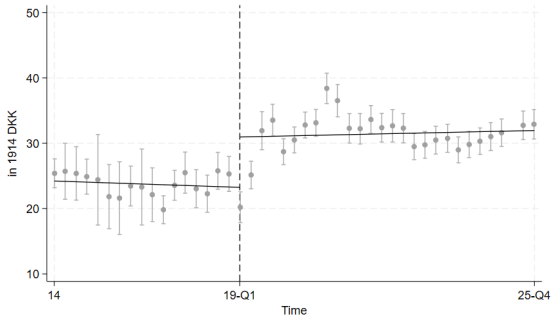
(b) Copenhagen, $<$ 9.5 pre-hours



(c) the Provinces, $>$ 9.5 pre-hours



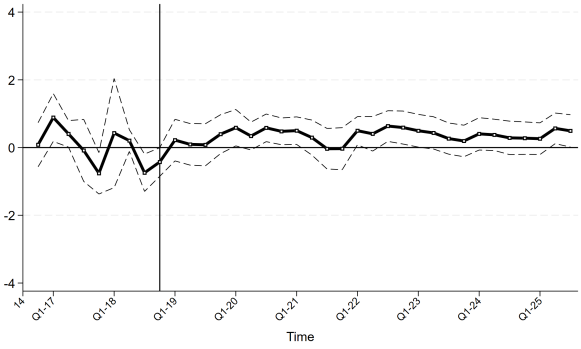
(d) the Provinces, $<$ 9.5 pre-hours



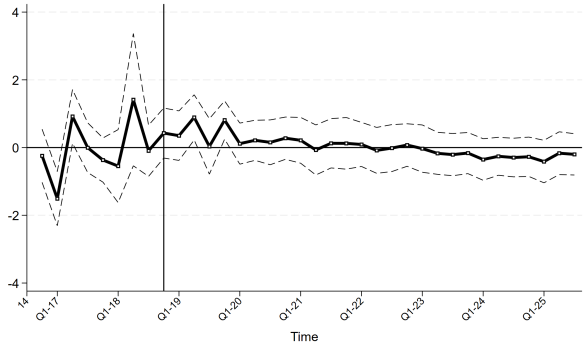
Notes: *This figure presents the development real weekly earnings for the years 1914-1925. Panels (a) and (b) are for Copenhagen, where we have split the occupational groups by the number of hours worked prior to the 1919-WTR reform (more or less than 9.5 hours). Panels (c) and (d) report the corresponding development for the Provinces. The vertical dashed lines separate the pre- and post 1919-WTR reform periods. Earnings are reported in Danish kroner (kr.) in constant 1914 prices.*

Figure A.4: Income effects when restricting the average of pre-reform coefficients to be zero

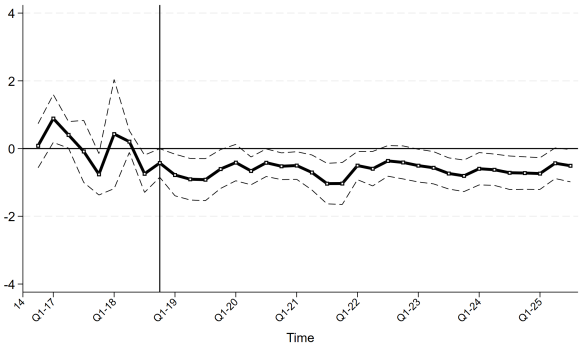
(a) Hourly wages: Copenhagen



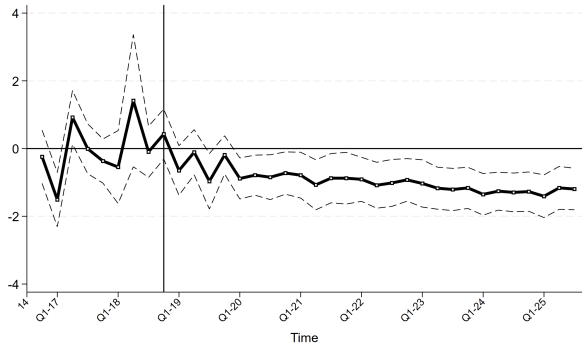
(b) Hourly wages: the Provinces



(c) Weekly earnings: Copenhagen

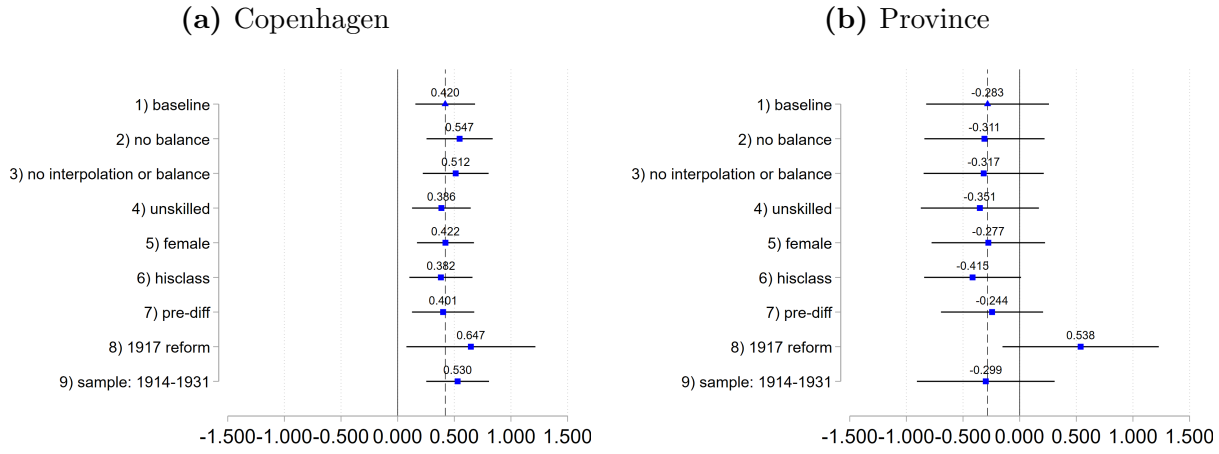


(d) Weekly earnings: The Provinces



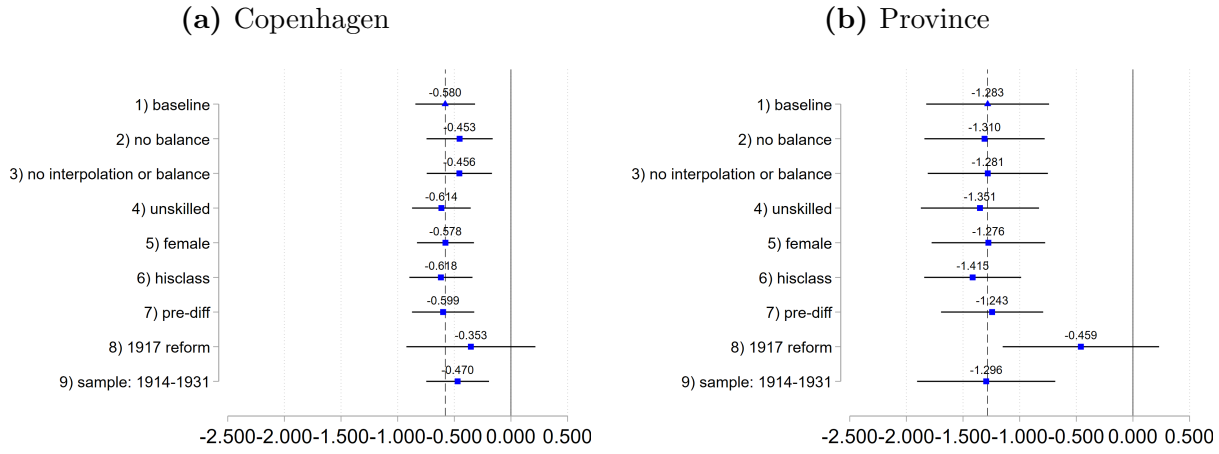
Notes: *This figure presents the same as Figure 4, but instead of omitting the quarter before the reform as the reference period, we restrict the average of the pre-treatment coefficients to be zero, which is what DiD estimation does.*

Figure A.5: Robustness: Hourly wages



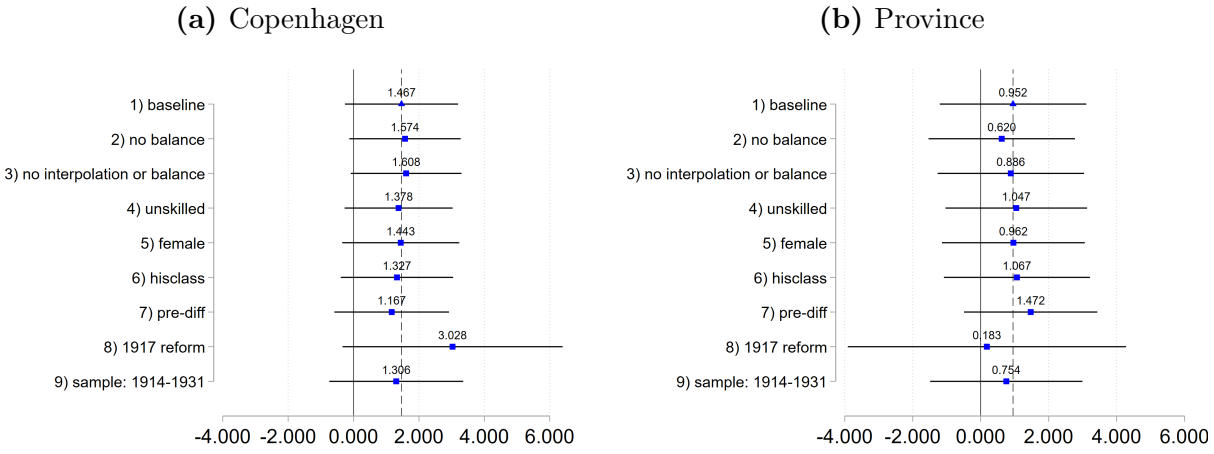
Notes: *This figure presents DiD estimates of our model under a number of robustness checks. Panel (a) shows results for Copenhagen and Panel (b) shows results for the Provinces. The top row shows the baseline estimate. 'No balance' includes all possible observations (no balancing requirement). 'No interpolation or balance' shows the estimate with no interpolation of the outcome variable, but also with no balancing requirement. The 'unskilled'-specification includes a control variable for unskilled male workers interacted with time FEs. 'Female' controls for female workers interacted with time FEs. 'Pre-diffs' controls for logged earnings and employment in 1914 interacted with time FEs. 'Hisclass' controls for the HISCLASS ranking of the occupation, interacted with time FEs. '1917 reform' controls for a 1917 reform which led to a large increase in union memberships, interacted with time FEs. 'Sample: 1914-1931' shows the estimate for the extended sample (1914-1931).*

Figure A.6: Robustness: Weekly earnings



Notes: *This figure presents DiD estimates of our model under a number of robustness checks. Panel (a) shows results for Copenhagen and Panel (b) shows results for the Provinces. The top row shows the baseline estimate. 'No balance' includes all possible observations (no balancing requirement). 'No interpolation or balance' shows the estimate with no interpolation of the outcome variable, but also with no balancing requirement. The 'unskilled'-specification includes a control variable for unskilled male workers interacted with time FEs. 'Female' controls for female workers interacted with time FEs. 'Pre-diffs' controls for logged earnings and employment in 1914 interacted with time FEs. 'Hisclass' controls for the HISCLASS ranking of the occupation, interacted with time FEs. '1917 reform' controls for a 1917 reform which led to a large increase in union memberships, interacted with time FEs. 'Sample: 1914-1931' shows the estimate for the extended sample (1914-1931).*

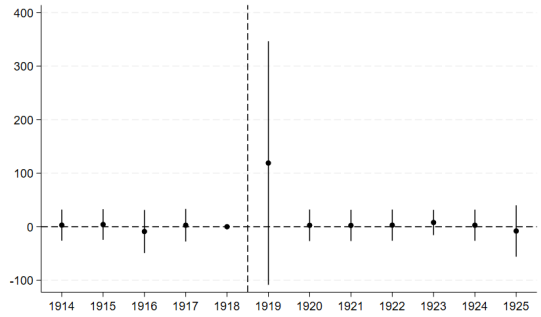
Figure A.7: Robustness: Employment



Notes: *This figure presents DiD estimates of our model under a number of robustness checks. Panel (a) shows results for Copenhagen and Panel (b) shows results for the Provinces. The top row shows the baseline estimate. 'No balance' includes all possible observations (no balancing requirement). 'No interpolation or balance' shows the estimate with no interpolation of the outcome variable, but also with no balancing requirement. The 'unskilled'-specification includes a control variable for unskilled male workers interacted with time FEs. 'Female' controls for female workers interacted with time FEs. 'Pre-diffs' controls for logged earnings and employment in 1914 interacted with time FEs. 'Hisclass' controls for the HISCLASS ranking of the occupation, interacted with time FEs. '1917 reform' controls for a 1917 reform which led to a large increase in union memberships, interacted with time FEs. 'Sample: 1914-1931' shows the estimate for the extended sample (1914-1931).*

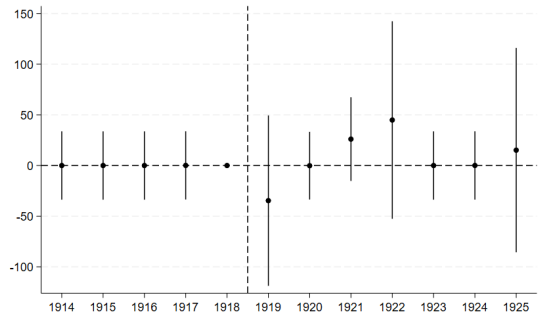
Figure A.8: Effects on labor market conflicts

(a) Strike days per worker in Copenhagen

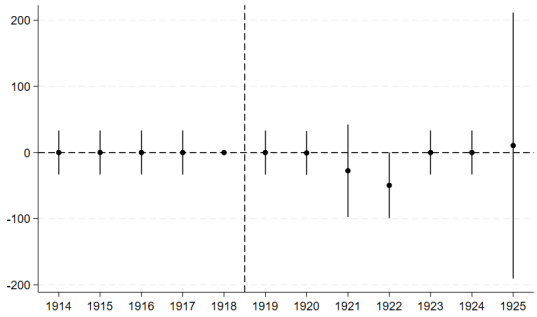


(b) Strike days per worker in the Provinces

(c) Lockout days per worker in Copenhagen



(d) Lockout days per worker in the Provinces



Notes: *This figure reports event-study estimates when the outcomes are different types of labor-market conflict. Treatment (WTR) here is defined at the union occupation level and the model has been collapsed such that the data is annual. The outcomes in Panels (a) and (b) is the number of strike days per worker in an occupation group, while in Panels (c) and (d) the outcome is the number of lockout days per worker. All regressions control for year and occupation fixed effects.*

A.3 Additional tables

Table A.1: Unionization rates in the Capital and the Provinces in 1916

Union (Danish)	Union (English)	Unionization CPH (%)	Unionization PRV (%)
Bagerne og Konditorerne	Bakers and Confectioners	36.4	26.4
Billedskærere	Woodcarvers	25.5	15.2
Blikkenslagere	Plumbers	50.2	26.8
Bogbinderiarbejdere	Bookbinders	77.6	28.3
Bogtrykfaget	Printers	74.4	64.4
Brolæggerne	Pavers	59.6	18.4
Bryggeriarbejdere	Brewery Workers	98.8	68.3
Bødkerne	Coopers	71.9	35.6
Drejerfaget	Turners	24.1	24.7
Elektrikere	Electricians	36.3	35.8
Faglærte Jern- og Metalarbejdere	Skilled Iron and Metal Workers	80.7	35.5
Glasarbejdere	Glass Workers	55.8	20.8
Guld-, Sølv-, og Elektropletarb	Gold, Silver, and Electroplating Workers	33.1	27.4
Gørtler- og Metalarbejdere	Brass and Metal Workers	31.1	16.6
Handskemagere	Glove Makers	22.5	12.0
Hattearbejdere	Hat Workers	74.2	68.3
Karetmagerne	Coach Builders	80.0	21.9
Korkskærere og Sorterersker	Cork Cutters and Sorters	85.1	44.6
Kurvemagere	Basket Makers	51.2	–
Læder- og Skindarbejdere	Leather and Skin Workers	27.9	22.6
Malersvende	Painters	49.8	35.5
Murersvende	Bricklayers	72.2	37.8
Mølleriarbejdere	Mill Workers	64.9	12.4
Papirindustriarbejdere	Paper Industry Workers	24.3	82.7
Rebslageriarbejdere	Rope Makers	14.6	44.9
Sadelmagere og Tapetserere	Saddlers and Upholsterers	55.9	14.8
Skibstømrere	Ship Carpenters	80.8	57.2
Skorstensfejersvende	Chimney Sweeps	55.2	15.6
Skotøjsarbejdere	Footwear Workers	47.4	11.4
Skræddere	Tailors	20.0	11.6
Slagter- og Kødindustriarbejdere	Butchers and Meat Industry Workers	43.9	35.8
Snedkerfaget	Cabinetmakers	53.1	27.7
Stenindustriarbejdere	Stone Industry Workers	14.9	23.3
Stukkatører	Stucco Workers	54.3	13.7
Sukkervarearbejdere	Confectionery Workers	54.9	71.4
Tømrerfaget	Carpenters	63.7	34.1
Urmagere	Watchmakers	8.0	10.0

Notes: *This table shows the 1916 unionization rates of different occupations by area, where data exists. We get the number of union members in an occupation from unemployment data reported by the unions (Statistiske-Meddelelser 1921), which we scale by the total number of workers in the same occupation, gathered from the 1916 population census (Statistisk-Tabelværk 1920).*

Table A.2: Summary statistics for Copenhagen

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	period 1					period 2				
	N	mean	p25	p50	p75	N	mean	p25	p50	p75
Nominal wages, Hourly	385	82.80	59	83	103	1,790	173.7	137	174	207
Real wages, Hourly	385	51.09	40	52.30	61	1,790	79.70	64.00	81.15	96.33
Nominal earnings, Weekly	385	4,614	3,360	4,625	5,747	1,790	8,322	6,624	8,352	9,936
Real earnings, Weekly	385	2,849	2,243	2,916	3,421	1,790	3,819	3,116	3,900	4,597
Workers	384	554.2	88	201	747.2	1,790	535.8	75	190	643
Work time, Daily	385	9.341	9.100	9.300	9.500	1,790	7.969	8	8	8

Notes: *This table reports summary statistics for Copenhagen before and after the reform. Real and nominal wages and earnings are all in levels and reported as Danish cents. The number of workers are also in levels. Daily work time is reported as hours. The table shows the number of observations, mean and the 25th, 50th, and 75th percentiles, in the period before and after the reform.*

Table A.3: Summary statistics for the Provinces

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	period 1					period 2				
	N	mean	p25	p50	p75	N	mean	p25	p50	p75
Nominal wages, Hourly	301	64.92	47	63	81	1,753	140.6	116	142	168
Real wages, Hourly	301	40.81	34.15	41.62	49.09	1,753	64.48	54.09	67.28	76.33
Nominal earnings, Weekly	301	3,700	2,700	3,610	4,666	1,753	6,747	5,568	6,816	8,050
Real earnings, Weekly	301	2,325	1,926	2,399	2,765	1,753	3,095	2,622	3,218	3,652
Workers	295	648.5	40	180	730	1,753	603.5	57	179	730
Work time, Daily	301	9.511	9.300	9.500	10	1,753	7.976	8	8	8

Notes: *This table reports summary statistics for the Provinces before and after the reform. Real and nominal wages and earnings are all in levels, and reported as Danish cents. The number of workers are also in levels. Daily work time is reported as hours. The table shows the number of observations, mean and the 25th, 50th, and 75th percentiles, in the period before and after the reform.*

Table A.4: Balance table for Copenhagen

	(1) WTR	(2) WTR	(3) WTR	(4) WTR
Earnings, Weekly	-0.04*			-0.03
	(0.02)			(0.02)
Wages, Hourly		-0.05***		
		(0.02)		
Workers			-0.01**	-0.01**
			(0.00)	(0.00)
Constant	0.46***	0.39***	0.22***	0.45***
	(0.16)	(0.08)	(0.02)	(0.16)
Region	CPH	CPH	CPH	CPH
N	67	67	67	67

Notes: *This balance table presents the results of a cross-sectional regression analysis, where we have the treatment variable (WTR) as the outcome variable. As the independent variables, we have the average weekly earnings (column 1), hourly wages (column 2), and employment (column 3) over the entire pre-period. We also run a regression in which we include both weekly earnings and employment as independent variables (column 4). The table shows regression results for Copenhagen.*

Table A.5: Balance table for the Provinces

	(1) WTR	(2) WTR	(3) WTR	(4) WTR
Earnings, Weekly	0.02			0.02
	(0.02)			(0.02)
Wages, Hourly		-0.00		
		(0.02)		
Workers			0.00	0.00
			(0.00)	(0.00)
Constant	0.04	0.19**	0.17***	0.04
	(0.15)	(0.08)	(0.02)	(0.15)
Region	Provin	Provin	Provin	Provin
N	66	66	65	66

Notes: *This balance table presents the results of a cross-sectional regression analysis, where we have the treatment variable (WTR) as the outcome variable. As the independent variables, we have the average weekly earnings (column 1), hourly wages (column 2), and employment (column 3) over the entire pre-period. We also run a regression in which we include both weekly earnings and employment as independent variables (column 4). The table shows regression results for the Provinces.*

Table A.6: Occupation groups with HISCLASS

Industry (Danish)	Industry (English)	Occupation (Danish)	Occupation (English)	HISCLASS
Beklædning	Clothing	**Skotøjsarbejdere, Kvinder**	**Footwear Workers, Women**	9
		Skotøjsarbejdere, Mænd	**Footwear Workers, Men**	9
		Skræddersvende i Konfektionsindustrien	Tailors in the Ready-Made Clothing Industry	7
		Syersker i Konfektionsindustrien	Seamstresses in the Ready-Made Clothing Industry	9
		Syersker, andre	**Seamstresses, Other**	9
		Ugesvende i bestilt Skrædderi	**Journeymen in Custom Tailoring**	7
Bygning	Construction	Arbejdsmænd hos Tømrerne	Laborers for Carpenters	9
		Blikkenslagersvende	**Tinsmiths, Journeymen**	7
		Bygningssnedkersvende	**Building Joiners, Journeymen**	7
		Glarmestersvende	Glaziers, Journeymen	7
		Malersvende	**Painters, Journeymen**	7
		Murarbejdsmænd	**Bricklaying Laborers**	9
		Murersvende	**Bricklayers, Journeymen**	7
		Stukkatørsvende	**Stucco Workers, Journeymen**	7
		Tømrersvende	**Carpenters, Journeymen**	7
Forskelligt	Misc.	Fyrbødere	Stokers	11
		Kuske	Coachmen	8
Grafisk Industri	Printing	**Bogbinderier, Kvinder**	**Bookbinding, Women**	9
		Bogbinderier, Svende	**Bookbinding, Journeymen**	7
		Bogtrykkerier, Kvinder	**Printing, Women**	11
		Bogtrykkerier, ufaglærte Arbejdere	**Printing, Unskilled Workers**	11
		Kemigrafer	Chemical Engravers	7
		Litografiske Etabl., Kvinder	**Lithographic Establishments, Women**	11
		Litografiske Etabl., ufaglærte Arbejdere	**Lithographic Establishments, Unskilled Workers**	11
		Papirfabrikker, Arbejdsmænd	**Paper Mills, Laborers**	11
		Papirfabrikker, Kvinder	**Paper Mills, Women**	11
		Typografer	**Typographers**	7
		Æskejomfruer	Box Factory "Maidens"	11

Notes: *This table lists the various occupation groups, with their Danish names in the third column and English translations in the fourth column, along with their corresponding (sub)industries in the first two columns. The fifth column contains our best effort to provide the occupation with a HISCLASS rating from (van Leeuwen and Maas 2011), where a lower number indicates a profession with higher social status (and often also skill). Occupations in bold are included in the baseline sample. Occupations in regular text are part of the unbalanced sample, along with those in bold.*

Table A.7: Occupation groups (cntd) with HISCLASS

Industry (Danish)	Industry (English)	Occupation (Danish)	Occupation (English)	HISCLASS
Jern og Metal	Iron & metal	**Arbejdsmænd i Jernindustrien**	**Laborers in the Iron Industry**	11
		Blikkenslagersvende	**Tinsmiths, Journeymen**	7
		Elektrikere	**Electricians**	7
		Formere	**Molders**	7
		Gørtlere	**Metalworkers**	7
		Kobbersmede	**Coppersmiths**	7
		Kvinder i Jernindustrien	**Women in the Iron Industry**	11
		Malersvende	**Painters, Journeymen**	7
		Metalslibere	**Metal Polishers**	7
		Metaltrykkere	**Metal Spinners**	7
		Skibstømrere	**Ship Carpenters**	7
		Smede og Maskinarbejdere	**Smiths and Machine Workers**	7
		Træarbejdere	**Woodworkers**	7
Ler, Sten og Glas	Ceramics & masonry	**Brolæggersvende**	**Paving Workers, Journeymen**	7
		Cementfabrikker, Arbejdsmænd	**Cement Factories, Laborers**	11
		Grus og Skærvefabrikker, Arbejdsmænd	**Gravel and Quarry Factories, Laborers**	11
		Jord- og Betonarbejdere	**Earth and Concrete Workers**	11
		Keramisk Industri, Kvinder	Ceramic Industry, Women	11
		Keramisk Industri, faglærte Arbejdere	Ceramic Industry, Skilled Workers	7
		Keramisk Industri, ufaglærte Arbejdere	Ceramic Industry, Unskilled Workers	11
		Stenhuggerier, Svende	**Stone Carving, Journeymen**	7
		Stenhuggerier, ufaglærte Arbejdere	Stone Carving, Unskilled Workers	11
		Teglværker, Arbejdsmænd	Brickworks, Laborers	11
		Teglværker, Kvinder	Brickworks, Women	11
Læderindustrien	Leather	**Garverier, Svende**	**Tanneries, Journeymen**	7
		Garverier, ufaglærte Arbejdere	**Tanneries, Unskilled Workers**	11

Notes: *This table lists the various occupation groups, with their Danish names in the third column and English translations in the fourth column, along with their corresponding (sub)industries in the first two columns. The fifth column contains our best effort to provide the occupation with a HISCLASS rating from (van Leeuwen and Maas 2011), where a lower number indicates a profession with higher social status (and often also skill). Occupations in bold are included in the baseline sample. Occupations in regular text are part of the unbalanced sample, along with those in bold.*

Table A.8: Occupation groups (cntd) with HISCLASS

Industry (Danish)	Industry (English)	Occupation (Danish)	Occupation (English)	HISCLASS
Næringsmiddel	Food processing	**Chokoladefabrikker Kvinder**	**Chocolate Factories, Women**	11
		Chokoladefabrikker ufaglærte Arbejdere	**Chocolate Factories, Unskilled Workers**	11
		Chokoladefabrikker, faglærte Arbejdere	**Chocolate Factories, Skilled Workers**	7
		Handelsmøller, Svende	**Trade Mills, Journeymen**	7
		Margarinefabrikker, Arbejdsmænd	**Margarine Factories, Laborers**	11
		Margarinefabrikker, Kvinder	**Margarine Factories, Women**	11
		Spritfabrikker, Arbejdere	**Distilleries, Workers**	11
		Spritfabrikker, Kvinder	**Distilleries, Women**	11
		Sukkerfabrikker, Kvinder	**Sugar Factories, Women**	11
		Sukkerfabrikker, ufaglærte Arbejdere	**Sugar Factories, Unskilled Workers**	11
Teknisk-kemisk	Technical-chemical	**Oliemøller, Arbejdsmænd**	**Oil Mills, Laborers**	11
		Svovlsyrefabrikker, Arbejdsmænd	**Sulfuric Acid Factories, Laborers**	11
		Tændstikfabrikker, Arbejdsmænd	Matchstick Factories, Laborers	11
		Tændstikfabrikker, Kvinder	Matchstick Factories, Women	11
Tekstilindustrien	Textiles	**Rebslagerier, Kvinder**	**Rope Factories, Women**	11
		Rebslagerier, Svende	**Rope Factories, Journeymen**	7
		Rebslagerier, ufaglærte Arbejdere	**Rope Factories, Unskilled Workers**	11
		Tekstilfabrikker, Kvinder	Textile Factories, Women	11
		Tekstilfabrikker, Mænd	Textile Factories, Men	11

Notes: *This table lists the various occupation groups, with their Danish names in the third column and English translations in the fourth column, along with their corresponding (sub)industries in the first two columns. The fifth column contains our best effort to provide the occupation with a HISCLASS rating from (van Leeuwen and Maas 2011), where a lower number indicates a profession with higher social status (and often also skill). Occupations in bold are included in the baseline sample. Occupations in regular text are part of the unbalanced sample, along with those in bold.*

Table A.9: Occupation groups (cntd) with HISCLASS

Industry (Danish)	Industry (English)	Occupation (Danish)	Occupation (English)	HISCLASS
Tobak	Tobacco	**Cigarfabrikker m. v., Kvinder**	**Cigar Factories, etc., Women**	11
		Cigarfabrikker m. v., faglærte mandl. Arb.	**Cigar Factories, etc., Skilled Male Workers**	7
		Cigarfabrikker m. v., ufaglærte mandl. Arb.	**Cigar Factories, etc., Unskilled Male Workers**	11
Træ og Møbel	Woodworking & furniture	Billedskærersvende	Wood Carvers, Journeymen	7
		Bødkersvende	Coopers, Journeymen	7
		Børstenbindersvende	Brush Makers, Journeymen	7
		Drejersvende	Turners, Journeymen	7
		Forgyldere	Gilders	7
		Karetmagersvende	Coach Makers, Journeymen	7
		Maskinsnedkere	**Machine Joiners**	7
		Møbelsnedkersvende	**Furniture Joiners, Journeymen**	7
		Pianoarbejdere	Piano Workers	7
		Sadelmager- og Tapetserersvende	**Saddlers and Upholsterers, Journeymen**	7
		Træindustriarbejdere, Kvinder	Wood Industry Workers, Women	11
		Træindustriarbejdere, ufaglærte	**Wood Industry Workers, Unskilled**	11

Notes: *This table lists the various occupation groups, with their Danish names in the third column and English translations in the fourth column, along with their corresponding (sub)industries in the first two columns. The fifth column contains our best effort to provide the occupation with a HISCLASS rating from (van Leeuwen and Maas 2011), where a lower number indicates a profession with higher social status (and often also skill). Occupations in bold are included in the baseline sample. Occupations in regular text are part of the unbalanced sample, along with those in bold.*