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Nikolaos Prodromidis
University of Duisburg-Essen

Martin Karlsson
University of Duisburg-Essen and IZA

Daniel Kühnle
University of Duisburg-Essen and IZA

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IZA – Institute of Labor Economics

Schaumburg-Lippe-Straße 5–9 53113 Bonn, Germany	Phone: +49-228-3894-0 Email: publications@iza.org	www.iza.org

ABSTRACT

Working Hours and Workers' Health: Evidence from a National Experiment in Sweden

Despite the importance of regulating working hours for workers' health and maintaining labour productivity, the literature lacks credible causal estimates on the impact of reduced working hours. We provide new evidence for the causal effect of shorter workweeks on mortality using full population register data, exploiting a nationwide policy in Sweden that reduced the weekly working hours from 55 to 48 hours for certain occupations only in 1920. Using difference-in-differences and event-study models, we show that lower working hours decreased mortality by around 15% over the first six years. We identify several mechanisms behind this effect: the policy led to fewer workplace accidents, a decline in work-related disability, and a reduction in sick days taken by employees. Causal forest estimators indicate particularly strong effects for older workers. Our results imply that many lives could be saved worldwide by reducing long working hours for labour-intensive occupations.

JEL Classification: I18, I18, J10, J81, N14

Keywords: working hours, employment legislation, mortality, Sweden

Corresponding author:
Nikolaos Prodromidis
University of Duisburg-Essen
Forsthausweg 2
47057 Duisburg
Germany
E-mail: nikolaos.prodromidis@uni-due.de

1. Introduction

Regulating working hours has been one of the oldest concerns of employment legislation with the goals of protecting workers' health and maintaining their productivity. As early as of 1919, the International Labour Organization (ILO) passed its first convention that limited standard working hours to 48 hours per week. Despite a global convergence towards a 40-hour working week over the past decades, long working hours still remain a significant policy concern in many countries today, with about one in three workers regularly working more than 48 hours per week (e.g., see Messenger et al., 2022). This occurs in both emerging and established economies, with for instance, every second worker affected in India and every tenth worker affected in the UK and the US (International Labour Organisation, 2024, see also Appendix Figure A.1). Despite the importance of regulating working hours for health, the causal evidence for the effects of reduced working hours on workers' health is extremely limited.

Our paper offers a new perspective on this longstanding question. Specifically, we examine the causal effect of the introduction of the 8-hour working day in Sweden in 1920, which limited working hours to 48 hours per week, on mortality. This reform was one of the most pro-labour and sweeping legislative changes in Sweden's history, profoundly shaping the foundation of the modern Swedish welfare state. The policy led to a substantial and abrupt reduction in working hours, as a 10-hour workday was the norm prior to the reform, with average weekly working hours of around 55 hours. Crucially, both historical accounts and modern research suggest that earnings remained unchanged following the reform, allowing us to isolate the effect of reduced working hours from potential income effects. Notably, the policy was initially introduced as a nationwide trial for certain occupations in 1920 and was made permanent in 1926. The policy's scale was unprecedented for its time, affecting more than a quarter of Sweden's workforce.

We use high quality population-level data, linking all individuals from the 1910 census with the universe of death records, and exploit the introduction across eligible and non-eligible occupations within a difference-in-differences/event study framework to identify the causal effects of the policy. Our results show that the policy had an immediate impact on mortality, reducing it on average by around 15% in the first seven years following the policy's introduction. Because individuals may have changed occupations between the 1910 census and the policy's implementation in 1920, there is potential measurement error our reform exposure variable. As a result, our estimates represent conservative lower bounds. Addressing such measurement error, we find a reduction in mortality of approximately 30% over the first seven years. The effect is stronger for older individuals and is driven by reductions in workplace accidents, disability, and sick days. We further investigate whether these effects are driven by individuals with certain socio-demographic characteristics, vary by the skill level of their occupation, or differ by place of residence characteristics. We apply machine learning techniques and the causal forest estimator as proposed by Athey et al. (2019) to estimate Conditional Average Treatment Effects (CATEs) and analyze heterogeneous treatment effects. The causal forest analysis reveals that age is the key driver of heterogeneity, with older individuals benefiting more from the reform.

We comprehensively show that our estimates do not pick up other confounding trends related to occupations affected by the policy, as we estimate flat pre-trends throughout the years leading up to the reform, and we neither find any effects for a group of placebo workers not affected by the policy. We provide a specification curve with 192 specifications, varying the control variables, fixed effects, control group samples, and treatment definitions; we consistently estimate negative treatment effects for mortality ranging from around -16% to around -32%. For each of those specifications, we run a placebo analysis in the pre-treatment period, where we consistently find null results.

Three features of our study are particularly important. First, we focus on mortality rather than self-reported health measures or intermediate health behaviours. Providing new evidence for this outcome is important, as mortality is a fundamental health indicator. Moreover, examining mortality has several other distinct advantages that make it a particularly useful outcome. For one, mortality outcomes are recorded in official death records, which makes them highly reliable and highly representative, as everyone is included in these records. Unlike self-reported measures, mortality records are not influenced by individual perceptions and allow us to avoid biases stemming from measurement error, selective reporting, or social desirability.

Second, we provide evidence on the specific mechanisms through which the policy reduced mortality. One of the key challenges in estimating the effect of working hours on health is that changes in working hours typically also affect earnings, which can independently influence health outcomes. However, historical accounts and modern research indicate that earnings remained largely unchanged following the reform. This feature of our setting allows us to better isolate the effect of reduced working hours on health, independent of changes in earnings. To uncover the mechanisms, we additionally use a different data source at the occupational level that is available for 94 occupation categories, spanning the years 1918-1926 and including information on workplace accidents, sick days, and disability cases.

Third, our study benefits from a comprehensive data source and rich policy variation. We leverage the 1910 census, which includes all individuals registered in Sweden at the end of 1910, covering approximately 5.6 million people, and link this data to the universe of death certificates. Additionally, we analyze the effects of a reform that affected more than a quarter of Sweden's workforce, spanning a wide range of occupations and sectors, unlike previous studies that have primarily focused on public sector employees. The policy variation we examine—a reduction in working hours from 55 to 48 hours—is particularly relevant for understanding the potential impacts across different groups. These features provide an excellent foundation for studying heterogeneity, which, in turn, allows for assessing the generalizability and external validity of the findings in different contexts.

Our paper contributes to three distinct strands of literature. First, it extends research on the relationship between labour market conditions and mortality. A central focus of this literature is the impact of job loss, whether due to layoffs (Sullivan and Von Wachter, 2009), recession-induced unemployment (Ruhm, 2000, Finkelstein et al., 2024), or local market shocks from import competition (Pierce and Schott, 2020, Adda and Fawaz, 2020). However, these studies do not isolate the causal effect of working hours from other factors linked to job loss, such as income. A small literature exploits policy-induced reductions in working hours on short-term self-reported health and health behaviours, with mixed findings. While reforms in France (Berniell and Bietenbeck, 2020), Korea (Ahn, 2016), and Germany (Cygan-Rehm and Wunder, 2018) generally suggest improved health behaviours with fewer working hours, evidence from China (Hu et al., 2024) indicates adverse health effects despite no changes in earnings or nutrition. To the best of our knowledge, we provide the first study to causally estimate the effect of working hours on mortality for the universe of individuals using population-level data.

Secondly, we contribute to the literature on the determinants of the significant increase in life expectancy during the early 20th century. The literature has primarily focused on public health investments, such as hospitals (Hollingsworth et al., 2024), sanitation systems (Alsan and Goldin, 2019, Anderson et al., 2022), and campaigns to eradicate diseases like tuberculosis (Anderson et al., 2019, Egedesø et al., 2020). Improvements in nutrition and income also played a role (Costa, 2015), as did the discovery of medical innovations (Cutler et al., 2006), such as antibiotics (Jayachandran et al., 2010) and vaccines (Ager et al., 2023). While this literature typically focuses on early life stages, our paper examines mortality declines originating in the prime working years. As demographic transitions continue globally, attention is shifting toward reducing mortality among working-age adults.

Third, we contribute to a body of research outside the economics literature on the relationship between long working hours and mortality. A meta-analysis identifies long working hours as a risk factor for cardiovascular disease (Kivimäki et al., 2015). Other studies link long working hours to occupational injuries (Dembe et al., 2005, Matre et al., 2021) and depression (Bannai and Tamakoshi, 2014, Virtanen et al., 2018). However, the vast majority of these studies rely on cross-sectional survey data without addressing the endogeneity of working hours.

2. Institutional Background

In modern-day Sweden, as in most other developed countries, a typical working day is 8 hours long (Eurostat, 2024). In contrast, during the 19th and early 20th centuries, working hours were much longer, often exceeding 10 hours per day. One of the largest reductions in working time occurred in 1920, when a policy reform introduced the 8-hour workday for certain occupations. The reform stipulated an 8-hour work day and capped the weekly working time at 48 hours. Before the reform, in 1919, the average weekly working time was 55 hours (Socialstyrelsen och Kommerskollegium, 1925), so the reform provided eligible individuals with an additional 7 hours of leisure time each week.

Workers and their unions had for a long time demanded reduced working hours as one of their main goals (LO, 1922). Each year, May 1st demonstrations had advocated for the implementation of an 8-hour workday. Before the reform, workers typically rose at 6 a.m. and returned home by 8 p.m. due to unpaid meal breaks and commuting time, with Sundays being the sole exception (Arbetstidskommitén, 1919). Employers, however, opposed the reform, fearing increased production costs and loss of international competitiveness. Despite this resistance, the international political climate had shifted towards a more favourable environment for shorter workdays as reflected in the Treaty of Versailles, which named the adoption of the 48-hour workweek to be of "special and urgent importance" (Article 427), and the creation of the ILO that adopted the principle of the 8-hours days in its first Convention in 1919. This led to many European countries and trading partners adopting the 8-hours day in industry already in 1919, and by 1922, the 8-hours was general practice in industry throughout Europe.[1]

[1]The early adopters were Austria, Czechoslovakia, Denmark, France, Italy, the Netherlands, Norway, Poland, Portugal, Spain and Switzerland. Source: https://www.ilo.org/resource/article/

A state investigation published in 1919 provided strong arguments for the reform, highlighting the potential positive societal impacts (for details, see Arbetstidskommitén, 1919). The report emphasised how excessive working hours harmed workers' health and productivity, linking prolonged working hours to occupational diseases, increased sickness absences, and more work-related accidents. The investigation reported that the rate of accidents increased toward the end of the work shift and particularly highlighted that workers in industries with continuous operations and two-shift schedules faced especially strenuous conditions. The report further argued that technological innovation could maintain productivity by optimising work processes, cutting breaks, and increasing worker rest despite reduced working hours. Additionally, the report argued that the reform could potentially help to mitigate industrial conflicts and help Sweden maintain competitiveness as neighbouring countries considered similar policies.

Due to remaining concerns about economic risks, the Swedish government initially implemented the 8-hour workday as a three-year trial for selected occupations. Starting January 1, 1920, most workplaces were affected, except the ones with continuous operations who were given an extension until July 1, 1920 to allow the transition from two to three shifts. The law was of massive scale and encompassed the majority of workers in industry and construction, limiting the working hours of around 450,000 individuals, out of a salaried workforce of around 1.6 million people. An additional 350,000 individuals, mostly white-collar professionals and workers with national collective agreements, already worked 8 hours per day. Workers in agriculture and services were not affected and continued to work longer hours. Thus, half of the salaried workforce still continued to work more than 8 hours per day.

The first evaluation of the law was inconclusive and was complicated by the global economic downturn of 1921-1922. To comprehensively assess the reform, Sweden extended the trial period for three more years until 1926. The outcome was favourable, and in 1926, the eligible occupations from the trial permanently adopted the 8-hour workday (Socialstyrelsen, 1925). Legislation extending the 48-hour work week to other occupations was introduced later, in the 1940s. This included retail workers in 1942 and service workers in hotels, cafés, and restaurants in 1947. However, white-collar workers remained excluded from the 48-hour work week, as non-manual skilled workers historically tended to work fewer than 48 hours (SOU, 1968).

The anticipated high economic costs of the reforms, expected by employers to diminish

`convention-no-1-landmark-workers-rights`, last accessed 13/02/2024.

industrial production, did not materialise (Socialstyrelsen och Kommerskollegium, 1925). On the contrary, industrial production continued to grow and industrial productivity per worker continued to expand in Sweden throughout the period our study covers (Schön, 2012). With the exception of traded manufacturing, the reform also did not seem to adversely affect employment (Bengtsson and Molinder, 2017). Production was further automated, the capital-labour ratio was increased, and through organisational improvements like better work processes or reductions of breaks, a more efficient utilisation of labour was achieved (Socialstyrelsen och Kommerskollegium, 1925).

The reform greatly improved the quality of life for eligible workers. Despite working fewer hours, they maintained their salaries while enjoying more free time (Bengtsson and Molinder, 2017). Workers used this additional time for self-improvement and recreation. Participation in adult education courses increased, and sports like football, once reserved for the upper classes, became popular among workers who formed their own clubs. Involvement in social movements, such as the temperance movement, also grew. Additionally, many workers took up homesteading, renting small garden plots to grow their own produce, and spent more quality time with their families (Nystrom, 1924).

The policy did not coincide with any other major concurrent reforms aimed at workers. By 1920, Sweden had already established foundational social security and worker protection laws.

3. Data

Our analysis starts with the 1910 Swedish census, which covers the entire population of Sweden as of January 1, 1911, that we obtained through the Integrated Public Use Microdata Series (IPUMS). The census contains individual-level information on a wide range of variables related to employment, marital status, household characteristics, place of residence, and other demographic details. Importantly, it includes detailed occupation information that allows us to classify individuals into eligible and non-eligible occupations for the mandated 8-hours workday. In addition, we link individuals in the census to the Swedish death index, detailed below. We also merge socio-economic information on the parishes from statistical yearbooks on municipalities.

Treatment variable. Our primary sources for identifying eligible and non-eligible occupations include the original law, government evaluation reports from 1923 and 1925, and annual reports by the National Welfare Board (*Socialstyrelsen*). The 8-hour workday trial targeted workers in industry, manufacturing, and construction, excluding all other professions and introducing various restrictions. Our treatment group consists of individuals in occupations covered by the trial, while the control group includes blue-collar workers in forestry, service workers, and foremen, as well as professionals and white-collar workers.

To generate the treatment variable in the 1910 Census, we start by first manually classifying the largest occupations. We classified occupations with at least 300 individuals as eligible or non-eligible based on historical documents and a dictionary of historical occupations, covering 664 occupations and 73% of the sample. For the remaining occupations with unique or less common names, we implemented a binary text classification model, achieving an overall accuracy of 94%.

As a robustness check, we also determine the eligibility status based on HISCO occupational codes, and reach similar findings. However, we prefer to use the more detailed occupational descriptions in our main analysis as they provide a more precise classification that aligns with historical records.

Mortality. We use individual-level mortality data from the universe of death certificates issued in Sweden since 1911, provided by the Federation of Swedish Genealogical Societies. The dataset includes information on exact dates of birth and death, names, gender, and parish. We link the mortality data to the 1910 Census using probabilistic record linkage.

Sample selection criteria. The 1910 census includes consistent employment information only for males, so we restrict our sample to them. For our main analysis, we focus on males aged 21 to 55 years in 1911. We use the lower limit to ensure that these individuals had reached legal age and finished their vocational training at the time of the census and the upper bound to make sure they were still active in the labour market when the policy was implemented in 1920. In this historical context, the common retirement age for blue-collar workers was 64 years (Espeli, 2019). As a placebo group, we also include older individuals (aged 56-64 in 1911) who had likely gone into retirement in 1920 and thus no longer affected by the policy. In our preferred control group, we exclude farmers and high socioeconomic status (SES) entrepreneurs who own companies. We exclude farmers from our main analysis due to their higher occupational mobility, remote living conditions, and distinct 1918 pandemic exposure with lower mortality rates (Bengtsson et al., 2018). However, in our robustness checks, we show that including farmers or high SES individuals in the control group yields similar results.

Table 1 presents summary statistics for our baseline sample. On average, treated and control workers are quite similar. The final column reports the standardized differences, which are generally well below 0.1 and never exceed the usual benchmark of 0.2. This similarity in baseline observable characteristics provides evidence that the control group serves as a reasonable counterfactual for eligible treated workers. Note that balanced covariates are not our primary identification assumption (we make a parallel trends assumptions), but the observed balance provides initial evidence that the treated and control workers were similar at baseline. This similarity in observable characteristics strengthens the plausibility of the parallel trends assumption.

4. Empirical Strategy

Estimating the causal effects of reducing working hours on mortality brings certain empirical challenges. First, working hours could potentially be correlated with several unobservables that also influence health. Second, reverse causality might lead to working hours being determined by health instead. Third, since changes in working hours typically lead to changes in earnings, it is difficult to isolate the working hours effect from a potential earnings effect. Importantly, our method allows us to exclude these issues from our estimates, as it leverages the policy-induced variation in working hours to eliminate bias from non-random selection and reverse causality. Additionally, earnings were kept constant, as demonstrated by Bengtsson and Molinder (2017) and empirically validated by us in Appendix Table A.1.

We exploit the introduction of the policy as a natural experiment to estimate the effect of reducing working hours on mortality within a difference-in-differences/event-study design. Intuitively, all our methods boil down to comparing mortality differences between workers whose occupations were covered by the reform and those that were not, before and after the policy was implemented. As our setting involves a single treatment date, it avoids the issue of negative weights encountered in staggered treatment designs (Goodman-Bacon, 2021).

To estimate the short-term effects of the policy, we focus on the period from 1911 to 1926, when the law was introduced and eventually made permanent. We use a balanced panel that spans the years 1911 to 1926 and construct a yearly mortality indicator based on the exact date of death. The main model we estimate is:

$$y_{it} = \alpha + \beta T_{o_{(i)}} \mathbf{1}\left(t \geq 1920\right) + \phi_t + \delta_{o_{(i)}} + \gamma_{p_{(i)}} + \epsilon_{it} \tag{1}$$

where i indexes the individual and t denotes the year. The outcome variable y_{it} is a binary indicator equal to 1 if individual i died in year t , and 0 otherwise. The treatment indicator $T_{o(i)}$ takes the value 1 if the occupation of individual i was covered by the reform, and 0 otherwise. We also include fixed effects for year ϕ_t, occupation $\delta_{o(i)}$, and parish $\gamma_{p(i)}$ to account for time-specific shocks and time-invariant characteristics of occupations and parishes. The coefficient β gives us the DID estimate for the average treatment effect on the treated (ATT) on mortality, and standard errors are clustered at the occupation level.

An advantage of using a balanced dataset without compositional changes is that it minimizes concerns about the dynamic selection of workers into treatment. Such selection would require workers to strategically choose their profession based on a policy that was unanticipated and occurred much later in time. Another advantage is that we do not need to calculate an updated population at risk, which makes the analysis less sensitive to dynamic mortality selection. We also estimate an alternative version of Equation 1 , aggregating the post-reform years into a single period.

Our identification strategy captures the causal impact of the 8-hour policy on mortality assuming that mortality trajectories would have evolved in parallel across eligible and non-eligible groups in the absence of the reform. To assess the validity of the parallel trends assumption and the dynamics of the treatment effect, we estimate the following event-study specification:

$$y_{it} = \alpha + \phi_t + \delta_{o(i)} + \gamma_{p(i)} + \sum_{k=-9}^{6} \delta_k \mathbf{1}\left(k = t - 1920\right) T_{o(i)} + \epsilon_{it} \tag{2}$$

Here, k indexes event years relative to the policy introduction in 1920 ($k = -1$ serves as the omitted baseline year). Our baseline model covers the time period from 1911 to 1926, when the law was made permanent. The coefficients δ_k capture the dynamic treatment effects of the reform for each event year k, while all other variables and fixed effects are as described for the DiD specification. Observations are drawn from a balanced panel of individuals, ensuring that each individual is observed annually throughout the 1911–1926 period to avoid dynamic mortality selection. As before, we cluster the standard errors at the occupation level.

Our analysis spans the pre- and post-reform period from 1911 to 1926, ensuring sufficient pre-treatment observations to validate the parallel trends assumption. For robustness, we extend the model to include parish-by-year fixed effects, controlling for potential confounding shocks at the parish-year level.

5. Results

5.1. Main results

We begin our analysis by descriptively plotting the raw mortality rates of workers aged 21-55 in 1910, shown in Panel A of Figure 1, where the vertical line indicates the introduction of the 8 hours policy in 1920. The figure shows that the mortality rates of treatment and control workers start from the same baseline rate of around 0.7% in 1911 and then develop in parallel for the nine years leading up to the policy. We observe a notable general spike in 1918 due to the influenza epidemic which struck in that year, although it affected both treatment and control workers at similar rates. Importantly, the fact that we observe the same level of mortality and hardly any differential changes between the two groups during the pre-reform period indicates that we have identified a plausible counterfactual. Following the introduction of the policy, we observe an immediate divergence in mortality of the two groups, driven by a reduction in deaths of treated workers. This descriptive evidence already hints at the beneficial effects the policy had for the health of workers.

To estimate the causal effect of the policy, we move on to the event study estimates as in eq. (2) and shown in Panel B of Figure 1. The approach requires that the eligible and non-eligible groups exhibit parallel trends in their outcomes in the absence of the law. As suggested by the descriptives, we find no evidence for any systematic pre-trends in the years leading up to the policy, which supports the parallel trends assumption. With the introduction of the policy, we observe an immediate and sharp decline in mortality. In the first year of the policy, treated workers are less likely to die (-0.14 percentage points, pp) with a baseline probability of dying of around 1% (see Panel A). Thus, the policy reduces the probability of dying by 14% relative to the baseline. The effect slightly increases and persists over time, stabilising at around -0.18 pp each year.

Using the difference-in-differences model from eq. (1), we estimate the average annual treatment effect on the treated over the seven-year period following the policy's introduction. The estimates in Column 1 of Table 3 indicate a reduction in average post-reform mortality of approximately 0.15 percentage points. Given a male labour force of around 1 million workers per year, a back-of-the-envelope calculation based on our estimates suggests that the policy saved approximately 1,500 lives annually. Using the alternative outcome, which aggregates post-reform years into a single period, we estimate a cumulative effect of the reform over the 1920–1926 period, indicating a mortality reduction of 1.12 percentage points.

5.2. *Mechanisms*

Next, we explore the mechanisms underlying our main effects. Previous literature has suggested that strenuous working conditions increase the risk of death from heart disease or accidents (Pega et al., 2021). To investigate these mechanisms further, we additionally use a different data source at the occupational level that is available for 94 occupation categories, spanning the years 1918-1926 and including information on workplace accidents, sick days, and disability cases.

In Figure 2, Panel A, we show that the reform led to a substantial reduction in workplace accidents by approximately 10 per 1,000 employees per year following the policy's introduction. Panel B shows a reduction in serious accidents causing disability by 0.5 per 1,000 workers. Similarly, Panel C presents the estimates for sick days, where we also observe significant reductions following the reform. These findings from this data source corroborate our main analysis and indicate that the reform led to a substantial reduction in workplace-related accidents and sickness of covered workers.

5.3. *Treatment Heterogeneity and Causal Forests*

We analyse potential heterogeneities in the aggregate causal treatment effect across individuals during the period from 1911 to 1926. To estimate conditional average treatment effects (CATEs), we use machine learning, specifically causal forest estimators, as outlined by Athey et al. (2019). This method is well-suited to our setting due to the combination of a high-dimensional set of covariates, sufficient statistical power provided by population census data, and the potential variability of the policy effects across subgroups. Our forest construction adheres to principles of honesty, balancedness, random feature splitting, and sub-sampling without replacement (Chernozhukov et al., 2024).

Since our paper uses a difference-in-differences design, we construct a panel of individuals for two periods—pre- and post-reform—and create a binary indicator for mortality (1911-1926). We then compute the first difference and apply a causal forest in a manner similar to Britto et al. (2022) based on an uncofoundness (Athey and Wager, 2021) and overlap assumptions (Athey and Wager, 2019). Sufficient overlap implies that we cannot predict an individual's treatment status based on their covariates. Figure A.4 in Appendix provides evidence supporting this assumption in our context. We estimate CATEs based on baseline demographic characteristics, whether the occupation is skilled or unskilled, as well as local socioeconomic conditions. We categorize occupations as skilled or unskilled using the HISCLASS measure, which reports the required skill level (Van Leeuwen and Maas, 2011).

The predicted CATEs are always negative, indicating a consistently negative effect of reduced working hours on mortality. In Figure A.5 in Appendix, we present the distribution of the CATEs in our sample, which ranges from -0.003 to -0.021. We estimate a metric for the frequency with which each covariate was used to split the data as in Athey and Wager (2019). We find that age was the most frequently used covariate in the causal forest, accounting for 70% of the splits. Age is followed by living in an urban area and working in an unskilled occupation, each of which accounts for approximately 5% of the sample splits.

To further explore heterogeneities, we split the sample based on the median value of the Conditional Average Treatment Effects (CATEs) across the sample. We define two groups: the most affected group and the least affected group. The most affected group consists of individuals with the highest mortality reductions. In Table 2, we present the means for the low-effect and high-effect groups, along with their standardized differences. Overall, our results show that the single most striking difference between these two groups is age. The 50% of individuals with the highest mortality reductions are, on average, 16 years older than the remaining individuals. We do not observe significant differences in occupation characteristics (e.g., unskilled labor) or parish-level measures between the two groups. Additionally, being married and having a child were factors associated with higher treatment effects.

In Figure A.6, we present how the effect varies across age deciles. We observe an increasing reduction in mortality among older individuals. Given that certain diseases and conditions, such as cardiovascular diseases, usually manifest after a certain age, it was more critical for these individuals to receive a reduction in working hours.

5.4. Robustness

To probe the robustness of our main results, we ran a series of robustness checks that we jointly plot as a specification curve in Figure 3. The upper part of the figure displays our main DID estimates from equation 1 for the period 1911-1926 for various specifications. We vary the following dimension for the different specifications. First, we omit or add individual level control variables from the 1910 census, such as age, marital status, family size, as well as an individual's parish of birth. Second, we vary the inclusion of time, parish, and occupation level fixed effect; we use occupation fixed effects at the two-digit level to ensure that we have vaiation in treatment status within each occupation group. Third, we add farmers to our control group to evaluate potential spillover effects from treated to untreated workers. Fourth, we focus on individuals aged 21 and older in 1910 to ensure that they are less likely to switch occupations following 1910, and 48 or younger in 1910 to ensure that they had not reached the official retirement age of 64 in 1920. Fifth, our estimates capture the effect of treatment eligibility measured nine years before the reform. Thus, a potential challenge arises from individuals switching occupations during this period. To address measurement error in the treatment variable, we measure the share of individuals in each occupation who changed eligibility status between 1900 and 1910. In this historical context, such changes primarily reflect career progression, such as becoming a foreman within the same sector. We find that career advancement drives most eligibility changes. Appendix Figure A.3 presents estimates of different eligibility shift shares, ranging from zero (no eligibility changes within the occupation) to the full sample. As a robustness check, we drop individuals in the upper half of the occupational mobility distribution to focus on below-median occupational mobility where we expect less measurement error. Finally, we also use an alternative treatment assignment based on the occupation HISCO codes.

A crucial assumption for our identification strategy is the parallel trends assumption. This assumption could be threatened if workers in different occupations have inherently different health risks due to the nature of their work, pre-existing working conditions, or socio-economic factors that could affect mortality trends differently. To check the parallel trends assumption for each specification, the bottom panel of Figure 3 presents the coefficients corresponding to the same specification, but restricts the observations to the pre-reform period between 1911 and 1919. We use the years from 1911-1915 as the "pre" period and 1916-1919 as the "post" period. If workers eligible for the reform were on different mortality trends prior to the reform compared to non-eligible workers, we would see statistically significant differences.

Importantly, the top panel of Figure 3 clearly shows that our main estimate is highly robust to these specification changes and that all estimates are negative and statistically significant. In line with the pre-coefficients from the event study analysis, the bottom panel shows that all placebo estimates are close to zero and statistically insignificant, indicating no differential pretrends and supporting the parallel trends assumption. We find that the placebo estimates associated with the second treatment assignment are shifted slightly upwards, but are still statistically insignificant. The upper panel clearly shows that, regardless of the specification choice, we always estimate negative and statistically significant treatment effects. To the left, we observe a set of larger negative estimates that stem from individuals with low occupational mobility; for this group, we estimate the largest negative effects, which range from -0.32pp to -0.27pp, in line with attenuation bias due to occupation misclassification. The smallest negative effect on the right of the curve still points to a mortality reduction of around 0.17pp. As the figure shows very transparently, the estimates are highly robust to any of the specification checks we perform. This provides us with confidence that the effects we find are of high internal validity. Moreover, the main estimate we interpret lies in the middle of the curve and likely underestimates the true effect.

Lastly, we investigate whether our findings potentially capture effects due to changes in unemployment that correlate with the reform and impact mortality. Unfortunately, unemployment data are not available for this historical setting. Instead, we estimate a measure of the employment share in each industry and exploit within-industry variation in treatment exposure. We estimate the equation with employment share as the outcome, and in Figure A.7, we find no evidence of employment shifts following the reform. This suggests that eligible workers in these industries were not differentially impacted compared to non-eligible workers in the same industries, further supporting the validity of our findings.

6. Conclusion

The regulation of working hours is one of the oldest concerns in employment legislation due to its importance for protecting workers' physical and mental health and maintaining labour productivity. Our study provides new causal evidence on the impact of reduced working hours on worker mortality, utilising a unique natural experiment from Sweden. By exploiting the nationwide introduction of an 8-hour workday for selected occupation as a quasi-experimental setting, we contribute new evidence using high quality data from the universe of death records. The use of full-population register data enables us to provide precise estimates, capturing the full scope of mortality impacts across a diverse set of occupations.

Our findings demonstrate that the implementation of the 8-hour workday reduced average annual mortality rates among affected workers by around 15% over the first seven years. These results are particularly pronounced among older workers and largely driven by decreases in deaths from workplace accidents, disability cases, and sick days. The robustness of our results is supported by an extensive series of robustness checks, including an analysis of parallel trends prior to the policy and a specification curve analysis. The absence of similar effects among placebo groups further strengthens the validity of our causal inference.

Our findings have important policy implications. We provide new evidence for a policy relevant margin, as about 1 in 3 workers even today work more than 48 hours per week significant (e.g., see Messenger et al., 2022). Our study suggests that reducing working hours could be an important strategy to improve worker health and reduce mortality, particularly in labour-intensive sectors.

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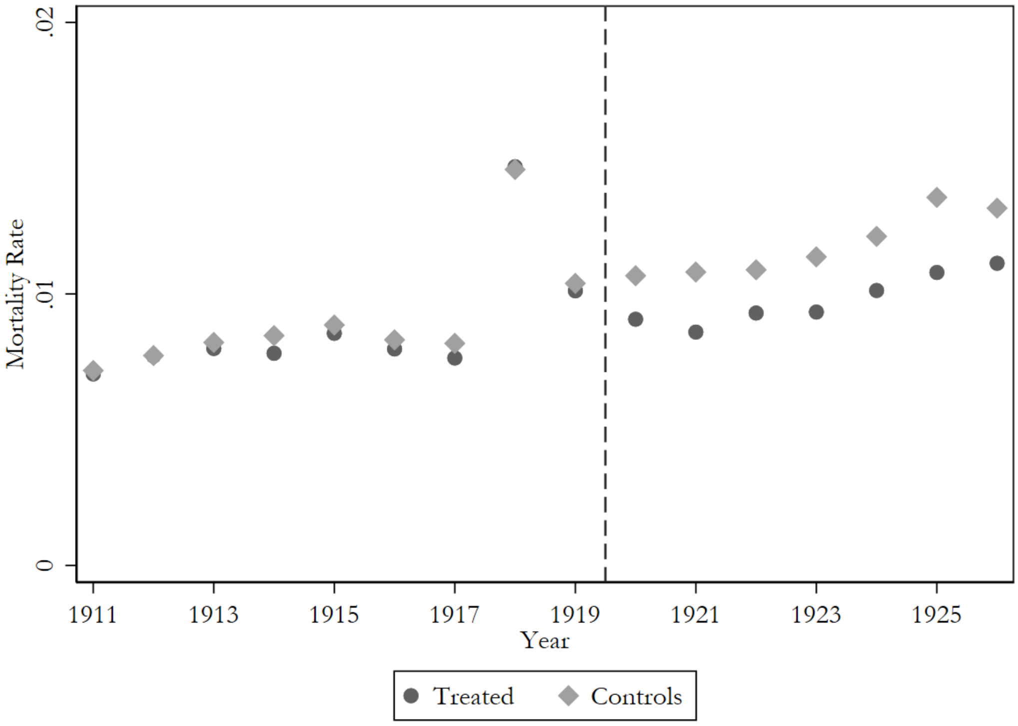
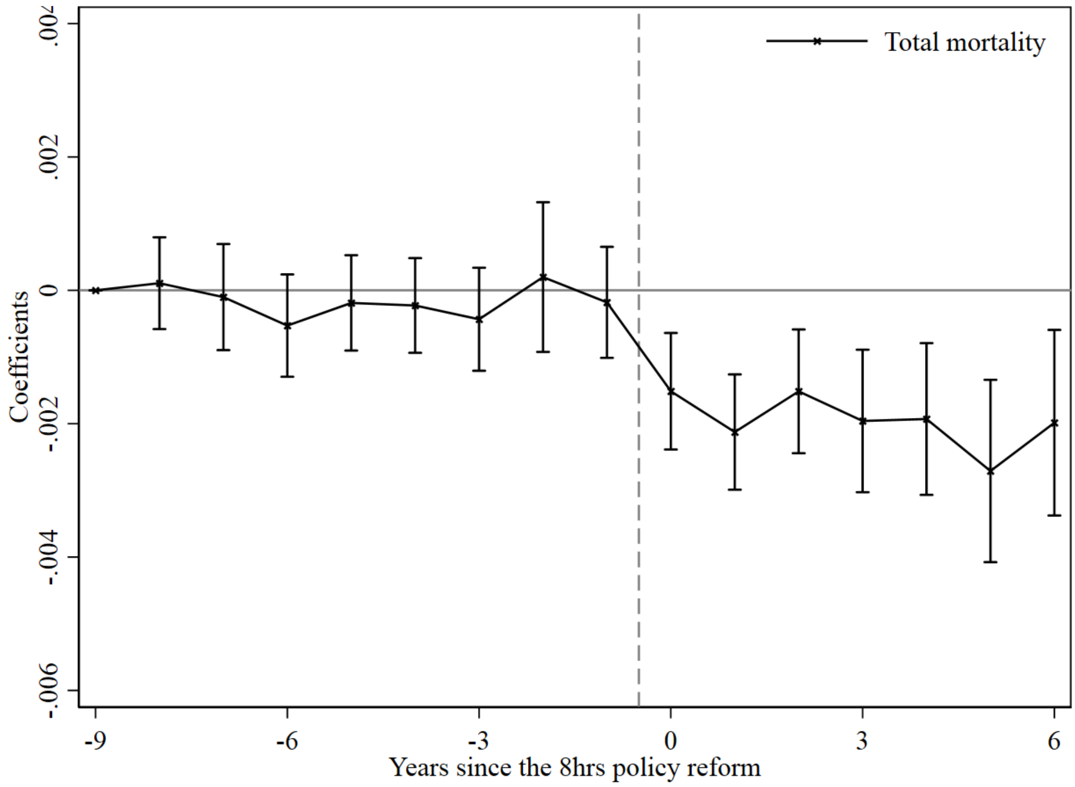
Figure 1: Effect of policy on mortality

A: Descriptive time series



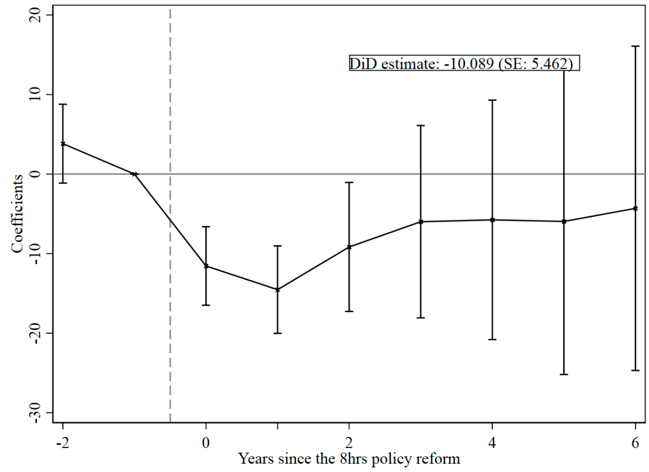
B: Event study estimates



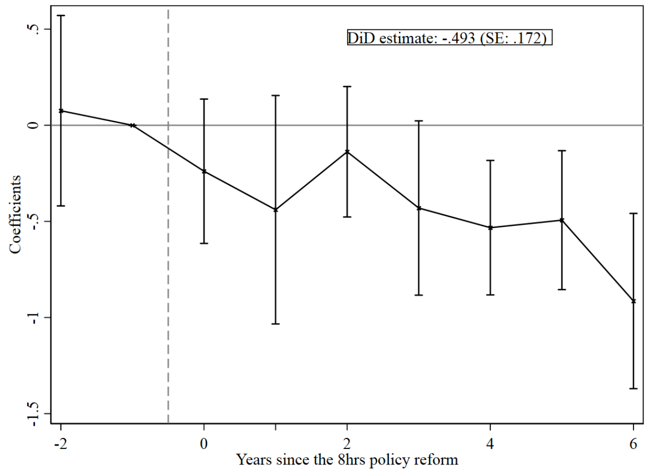
Notes: Panel A shows raw descriptive mortality rates for workers aged 21 to 55 years in 1910, separately by treatment status. Source: 1910 census and Swedish Death Book, own calculations. Panel B shows event study estimates for the effect of the introduction of the policy on mortality. Specification includes occupation, year and parish fixed effects. Source: 1910 census and Swedish Death Book, own calculations

Figure 2: Event-Study: Accidents and Sick Days

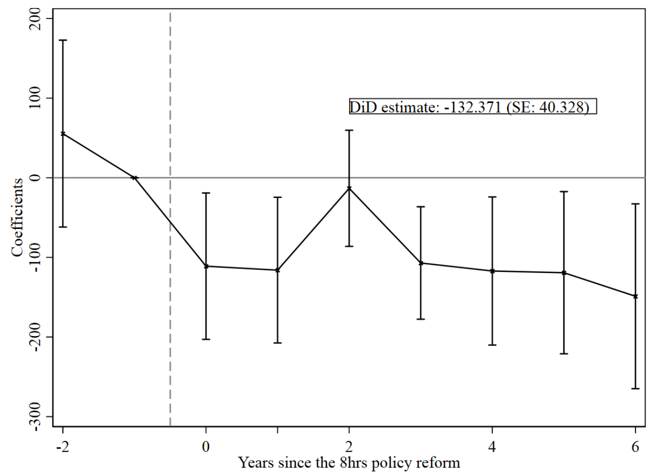
(a) Workplace Accidents per 1,000 Employees



(b) Disability Caused at Work per 1,000 Employees

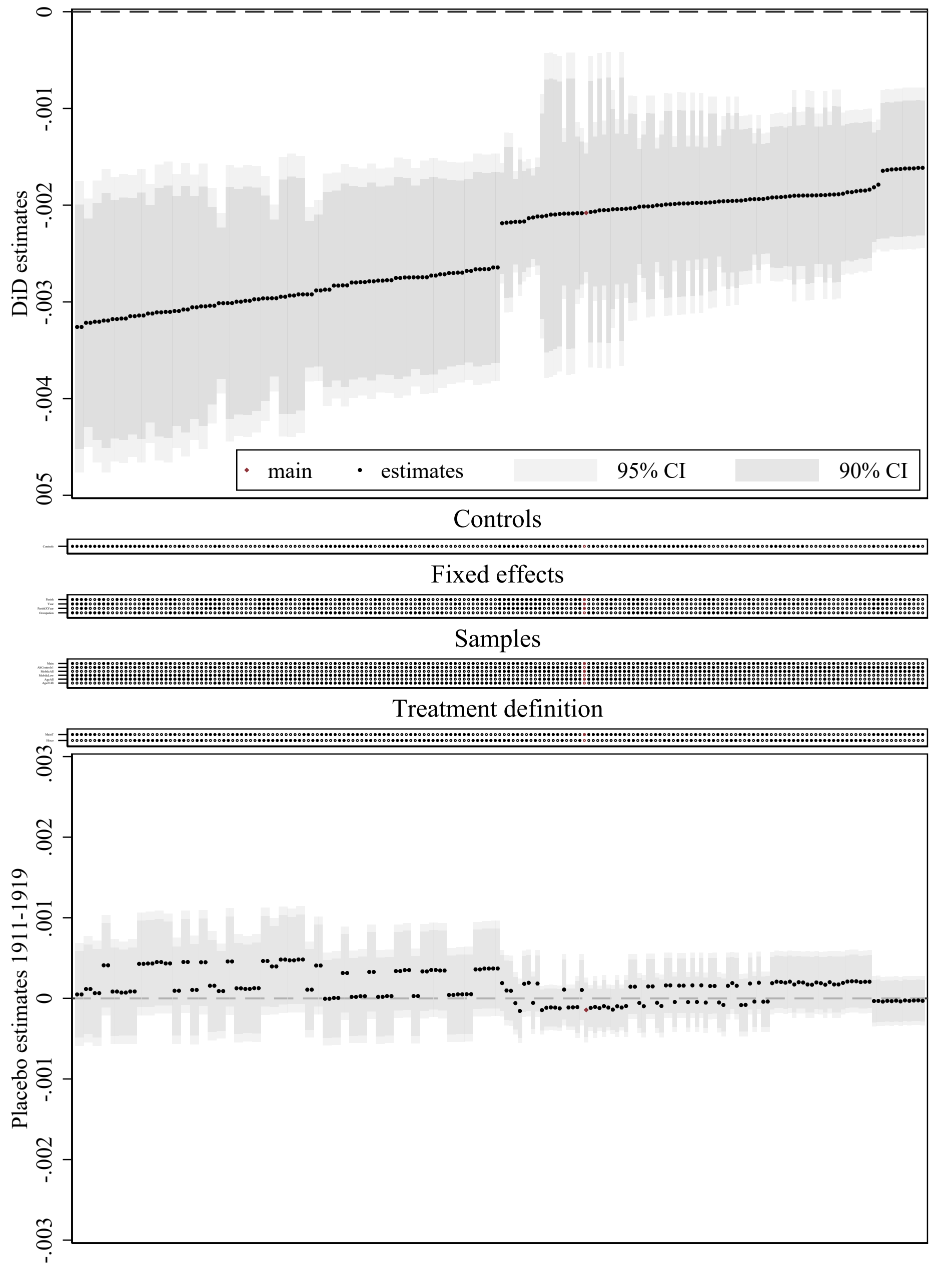


(c) Sick Days per 1,000 Employees



Notes: The graphs display event-study estimates of the 8-hour reform's eligibility effect on measures of occupational accidents. The year 1919 serves as the reference category. The x-axis represents the number of years since the implementation of the 8-hour law in 1920, and the y-axis shows the estimated coefficients. Figure A uses the number of accidents that caused disability per labor force as the dependent variable, Figure B shows the number of workplace accidents per labor force, and Figure C presents the number of sick days per labor force. All dependent variables are estimated per 1,000 individuals. The sample includes 845 observations from 94 occupation categories spanning 1918 to 1926. The regression includes fixed effects for year and occupation category. The sample consists only of male workers at firms employing at least 5 individuals. Observations prior to 1918 are excluded, as this is the first year following the 1916 act that introduced compulsory accident insurance, for which authorities began producing statistics. We show the 95 percent confidence intervals, and standard errors are clustered at the occupation category level. Source: Riksförsäkringsanstalten (1918-1926).

Figure 3: Specification curve



Notes: This figure presents the specification curve for our analysis. Each dot in the top panel depicts a point estimate from a different specification for the main DID estimates for the period 1911-1926. The dots vertically aligned below in the bottom panel show the point estimates from a placebo regression for the same specification but the pre-reform years 1911-1919. A total of 192 specifications were run for each panel. The dots in the middle panel indicate which control variables were used and for which sample. Standard errors are always clustered at the occupation level.

Table 1: Summary statistics, by eligibility status

	Controls (1)	**Treated** (2)	**Standardised difference (T-C)**
Demographic Characteristics			
Age	35.414	35.169	0.025
Unskilled Worker	0.198	0.149	0.130
Married	0.580	0.637	-0.118
Has a Child	0.498	0.571	-0.148
Living Alone	0.151	0.128	0.066
Patronymic Surname	0.427	0.515	-0.177
High SES Surname	0.238	0.226	0.028
Parish-Level Covariates			
Taxable Income (per capita)	351	385	-0.067
Poorhouse Rate	0.007	0.008	-0.075
Population Density	5.686	3.630	0.146
Train Station	0.398	0.401	-0.006
Average Temperature (°C)	5.529	5.676	-0.104
Average Rainfall (mm)	1.556	1.562	-0.040
Hospital	0.450	0.388	0.125
People's House	0.532	0.522	0.020
Union	0.747	0.749	-0.006
Religious Movement	0.705	0.671	0.073
Temperance Movement	0.871	0.850	0.061
Urban	0.419	0.350	0.142
Observations	157,922	242,745	

Notes: Table shows summary statistics for controls and treated groups by eligibility status. *Source:* 1910 Census, own calculations.

Table 2: Summary of Variables with Mean Values and Standardized Differences

Variable	**Least Affected**	**More Affected**	**Standardized Difference**
Individual Characteristics			
Age	27.167	43.328	-3.123
Married	0.422	0.806	-0.858
Unskilled Worker	0.190	0.144	0.125
Has Child	0.354	0.729	-0.811
Parish-Level			
Urban	0.384	0.370	0.028
Hospital in Parish	0.425	0.401	0.048
Temperance Movement in Parish	0.873	0.844	0.084
Union in Parish	0.774	0.723	0.117
People's Home in Parish	0.547	0.513	0.069

Note: This table divides individuals based on the median value of the Conditional Average Treatment Effects (CATE) of the 8-hour reform and presents covariate means by group and standardized differences. CATEs are calculated using generalized random forests. Our causal forest consists of 2,000 causal trees, with a minimum of 3,000 observations per leaf. Column 2 presents the covariate means for the group with higher treatment effects, Column 3 for the group with lower treatment effects, and Column 4 shows the standardized difference.

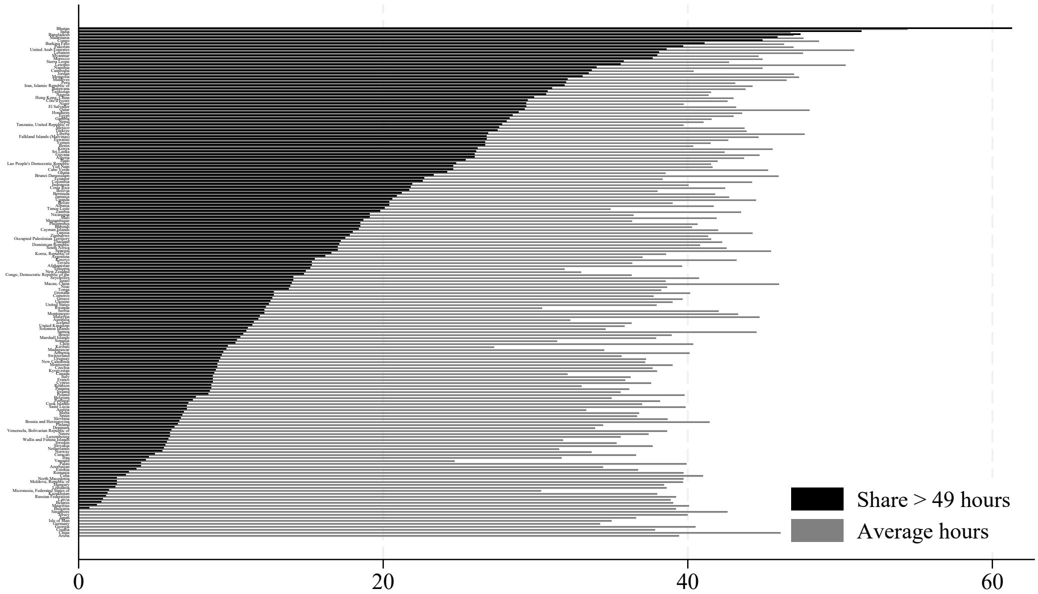
Table 3: Difference-in-Differences: Effect of 8-Hour Reform on Mortality

	(1)	(2)
DiD estimate	-0.0015	-0.0119
	(0.0003)	(0.0030)
	[0.0000]	[0.0001]
N	6,410,672	4,006,670
Mean	0.01	0.01
Individuals	400,667	400,667
Year FE	Yes	Yes
Parish FE	Yes	Yes
Occupation FE	Yes	Yes

Notes: This table presents the effect of the 8-hour reform estimated using a difference-in-differences regression. The unit of observation is the individual, covering the period 1911–1926. The dependent variable is a binary indicator of whether the individual died in a given year. The regressions include year, occupation, and parish fixed effects. Standard errors, clustered by occupation, are reported in parentheses, with p-values shown in brackets.

Appendix (For Online Publication)

Figure A.1: Average hours and prevalence of excessive working time



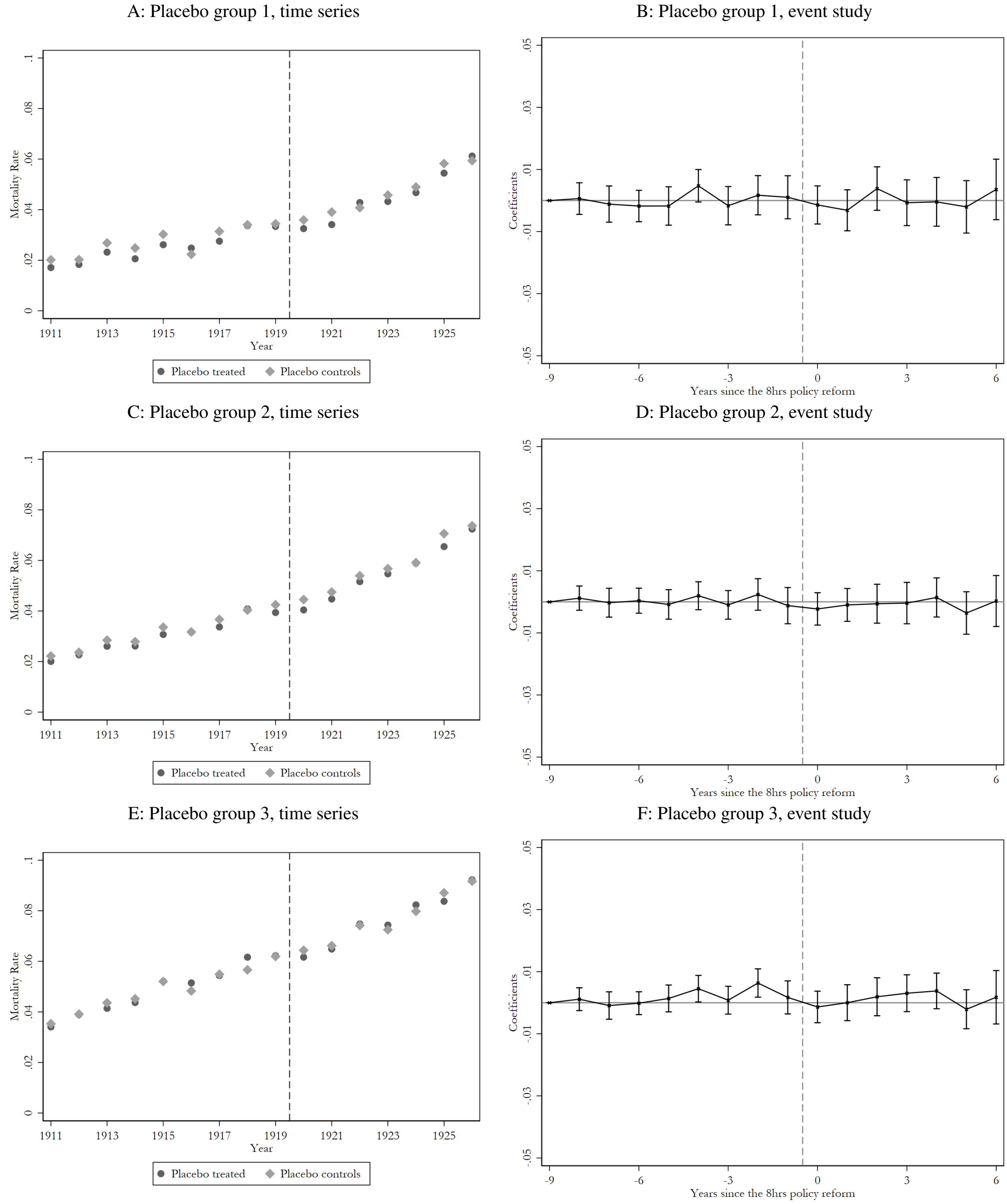
Notes: Figures shows Average hours per week per employed person (light gray bars) and share of employed working 49 or more hours per week separately (dark gray bars), separately by country. Source: ILO, `https://ilostat.ilo.org/topics/working-time/`, own figure.

Table A.1: Placebo Regression: Effect of 8-Hour Reform on Earnings

	Log(earnings)
DiD estimate	0.044
	(0.107)
	[0.680]
Observations	622
Year Fixed Effects	Yes
Parish Fixed Effects	Yes

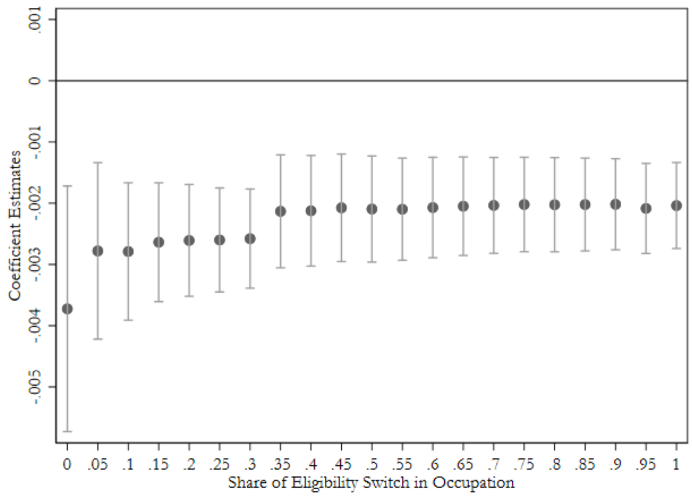
Notes: This table displays the effect of the 8-hour reform using a difference-in-differences regression. The unit of observation is a combination of occupation and year. The dependent variable is the log of earnings for different occupations. The earnings data has been assembled using various sources. Earnings information for industries has been derived from Social Messages and is available through the Historical Labour Database (HILD) dataset. Information concerning the control group occupations includes technical staff, office staff, and retail staff from Social Messages. Regressions include year and occupation fixed effects. Standard errors, clustered by occupation, are shown in parentheses, and p-values are in brackets. The sample covers the period 1916–1926.

Figure A.2: Mortality, placebo group



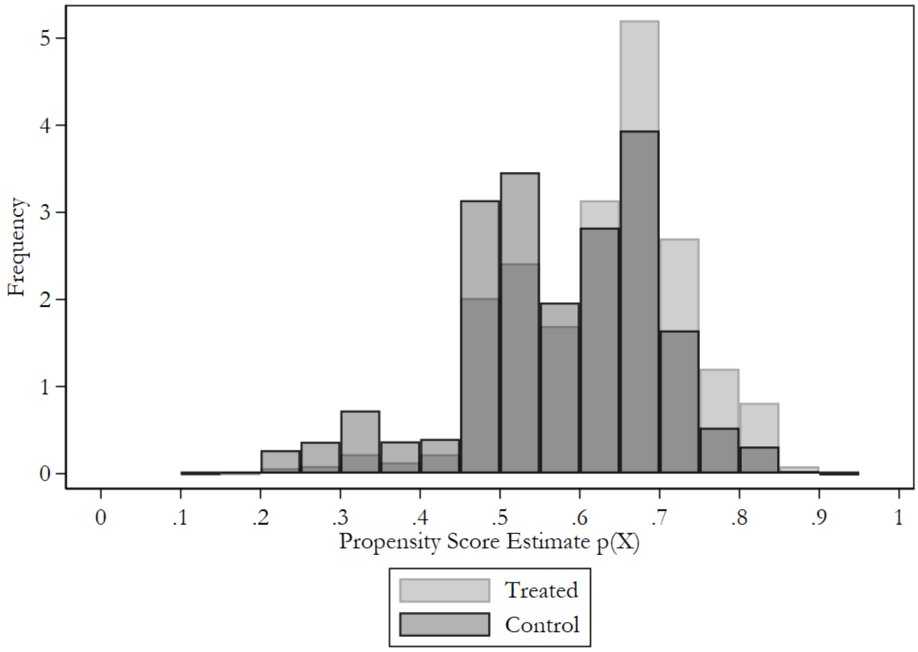
Notes: Figures show raw descriptive mortality rates and event study estimates for workers aged 55 and above in 1910. Same treatment and control group definition by occupation as in main analysis, but for workers not affected by the reform because they were too old. Panel A uses workers aged 56-59 in 1910, Panel B workers aged 56-64 in 1910, and Panel C uses workers aged 56 and older in 1910. Source: 1910 census and Swedish Death Book, own calculations.

Figure A.3: Aggregate Difference-in-Difference: Eligibility Switchers 1900-1910



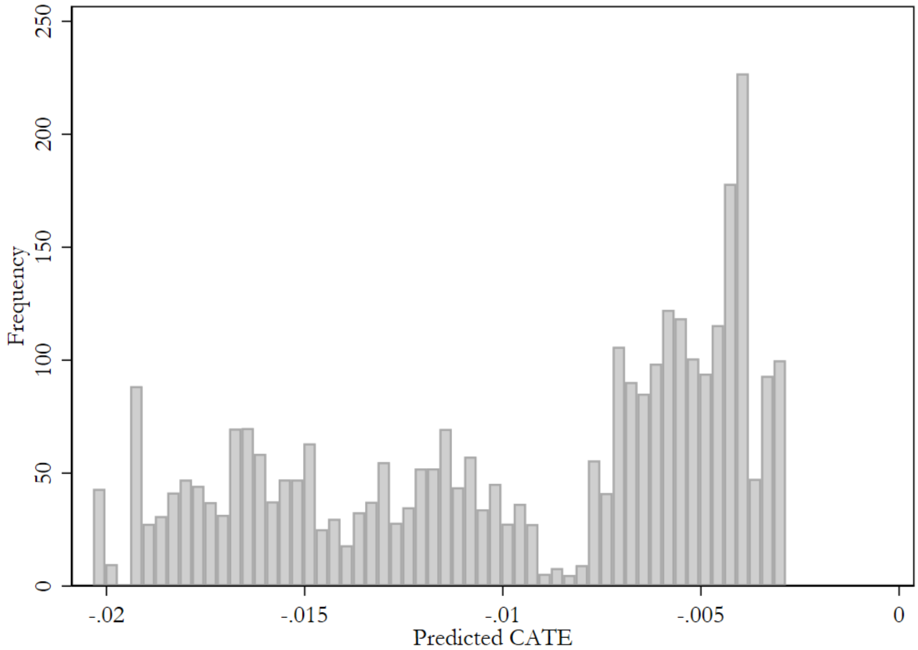
Notes: This figure presents the aggregate difference-in-difference effects of the 8-hour reforms for different subsamples, based on the share of eligibility switching between 1900 and 1910. A share near zero indicates occupations made up of individuals who rarely switched to other occupations with different eligibility. Occupations with a higher share are those where it was more common for individuals to change both occupations and eligibility status.

Figure A.4: Causal Forests: Propensity Score



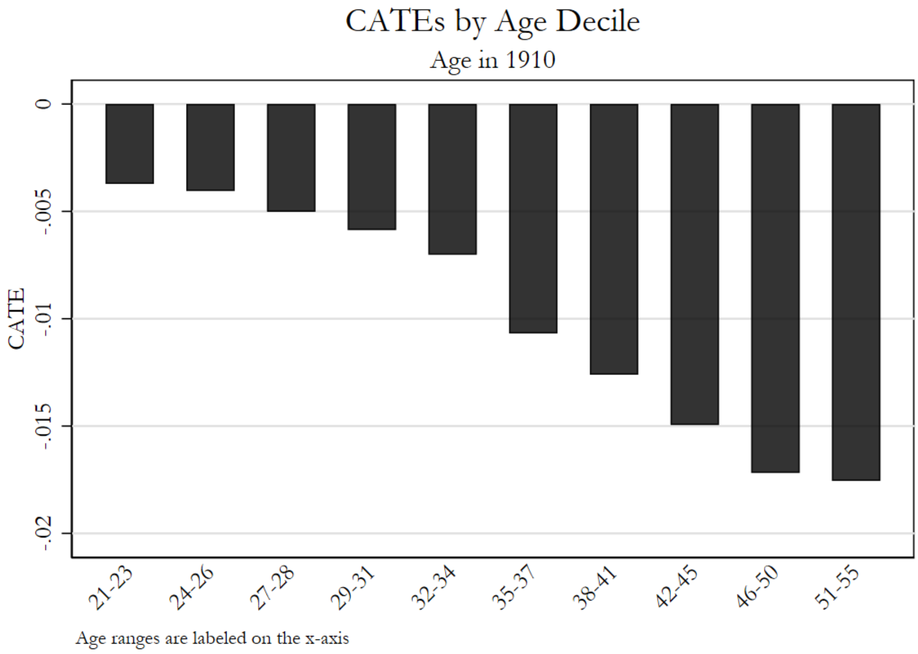
Notes: This histogram shows the estimated propensity scores for individuals regarding the reform, based on the following covariates: Age, Unskilled Job, Marital Status, Urban Status, Own Children, and the presence of the following variables in the parish: Hospital , People's Home, Labour Union and Temperance Movement.

Figure A.5: Causal Forests: Distribution CATEs



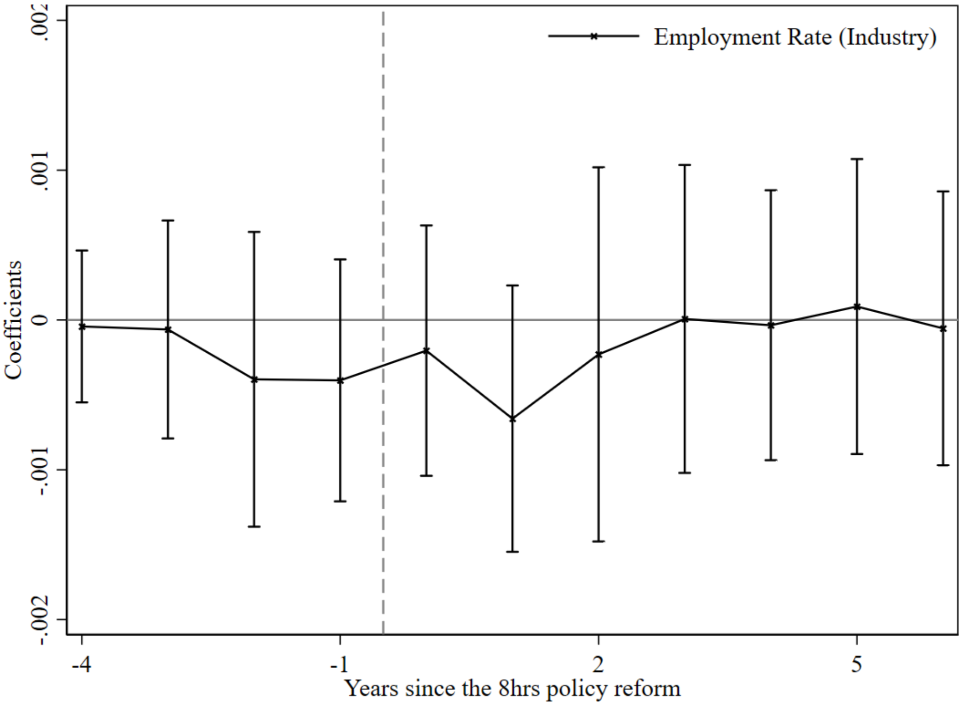
Notes: This figure shows the distribution of the estimated individual CATEs across the entire sample. CATEs are calculated using generalized random forests. Our causal forest consists of 2,000 causal trees, with a minimum of 3,000 observations per leaf.

Figure A.6: Causal Forests: Distribution CATE of reform by age



Notes: This figure presents the mean predicted Conditional Average Treatment Effects (CATEs) across decile bins of individual age in the 1910 Census. The CATEs are estimated using generalized random forests. Our causal forest consists of 2,000 causal trees, with a minimum of 3,000 observations per leaf.

Figure A.7: Effect of Policy on Employment



*Notes:*This figure shows difference-in-difference estimates of the reform's impact on the worker's employment share (individuals in industry i / total industrial workforce). We exploit within-industry variation in treatment exposure. Non-eligible individuals are administrative personnel, while eligible individuals are workers. Source: Industri / Kommerskollegium. 1915-1926